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Is There Such a Thing as Too Much Democracy? :

The Downsides of Democratizing State Courts

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Abstract of the Dissertation

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Much of the empirical work on judicial selection at the state level has failed to uncover any negative consequences of selecting judges in competitive elections. This dissertation examines three previously unconsidered potential consequences of using elections to select and reselect judges. Specifically, this dissertation examines whether voters in judicial elections are influenced by factors beyond the control of individual judges, whether elected judges are systematically less affected by the facts of the case than are appointed judges, and whether the practice of electing judges is harmful to minority rights. This dissertation uncovers evidence that suggests, while many of these consequences may not be true of elected judges generally, there may be some truth to them for judges selected and reselected in low-information judicial elections.

For my amazing husband,

Zach,

who was still my boyfriend when I began this journey,

for always believing in me,

even when I didn't believe in myself

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Chapter 1: Introduction

"That inflexible and uniform adherence to the rights of the constitution and of individuals, which we perceive to be indispensable in the courts of justice, can certainly not be expected from judges who hold their offices by a temporary commission. Periodical appointments, however regulated, or by whosoever made, would in some ways be fatal to their necessary independence." Alexander Hamilton 1788 "The Federalist No. 78." *The Federalist Papers*. Bantam Dell (p. 478).

With these words Alexander Hamilton eloquently defended the new proposed Constitution, and the principle of judicial independence ingrained in it, from those who argued that the appointment process and life tenure of judges would keep them too far removed from the people (Alexander Hamilton 1788 "The Federalist No. 78." *The Federalist Papers*). While Hamilton's views prevailed at the federal level when the Constitution was ratified in 1789, this debate between the relative importance of the competing values of judicial independence and democratic accountability has still not been settled in the states. States are considerably freer than the federal government to adjust their method of judicial selection, and many have done so in response to changing values among the public. Thus in the 1820s when a popular movement arose that called for the extension of democracy, many states substituted judicial appointments with judicial elections (Dubois 1980). Amidst the celebrations over the democratization of the judiciary, however, were those who decried the loss of judicial independence, that they saw as necessary for the courts to act as an unbiased interpreter of the laws (Landes and Posner 1975). Scholars have already demonstrated numerous instances in which the practice of electing judges has affected the subsequent behavior of the judge (Vines 1965; Hall 1987, 1992, 2001 (b); Drechsel 1987; Baum 1994; Brace and Hall 1995; Cann 2007; Brace and Boyea 2008). So far, however, scholars have largely framed these findings in a positive light, arguing that judicial elections have enhanced the democratic accountability of the courts (Hojnacki and Baum 1992; Hall 2001 (a); Hall and Bonneau 2006). Much less attention has been paid to the costs of using

elections to select judges (for an exception see the work on legitimacy (Cann and Yates (2008), Gibson 2008 (a), Gibson 2008 (b), Gibson and Caldeira 2012). This dissertation examines three previously unconsidered costs of electing judges and presents new empirical evidence that may speak to the drawbacks of employing low-information judicial elections.

The second chapter of this dissertation reexamines the data on judicial elections and questions whether or not there really is democratic accountability in these elections. For evidence of accountability in judicial elections, scholars have noted that judicial elections are influenced by the murder rate of the state (Hall 2001 (a)), as well as other normatively appropriate criteria, such as bar poll ratings and endorsements (Sheldon 1977; Volcansek 1981; Dubois 1984). These findings that voters are using election-specific information to base their choices on, however, are inconsistent with the other large body of literature, which finds that voters are often very poorly informed about judicial elections (Johnson, Shaefer, and McKnight 1978; Lovrich Jr. and Sheldon 1983; Rottman and Schotland 2001; Baum 2003), and in many instances cannot even recall any of the candidates' names (Lovrich Jr. and Sheldon 1983). Additionally, scholars have presented evidence that voters' choices are based, in part, on thoroughly irrelevant information, such as the arrangement of the candidates on the ballot (Volcansek 1981). In the second chapter of this dissertation, I argue that the persistent findings that voters lack information about judicial elections (Johnson, Shaefer, and McKnight 1978; Lovrich Jr. and Sheldon 1983; Rottman and Schotland 2001; Baum 2003) combined with the low visibility (Volcansek 1981; Iyengar 2002; Baum 2003; Frederick and Streb 2008) and other unique features (Peters 2009; Bonneau and Cann 2011) of judicial elections, leads voters to be influenced by information that is unrelated to judicial elections. This may include things such as the popularity of the executive and the state of the economy. In my analysis, I find some suggestive evidence that voters do consider state-level

GDP and national unemployment when evaluating performance incumbent and in-party incumbent performance respectively judicial elections. I also find evidence that voters look to presidential popularity when evaluating the performance of in-party incumbents. Both of these findings provide tentative support for Chapter Two's argument that judicial elections do not actually enhance democratic accountability, by showing that voters, who often have low levels of information about factors relevant to judicial elections (Johnson, Shaefer, and McKnight 1978; Lovrich Jr. and Sheldon 1983; Rottman and Schotland 2001; Baum 2003), may vote instead, at least partially, on the basis of factors that are irrelevant to judicial elections.

The second part of this dissertation considers new consequences of the well-documented finding that judges who are elected are influenced by public opinion.[1] Scholars who have researched the impact of public opinion on elected judges have, for the most part, not raised any concerns about what the implications of elected judges' reliance on public opinion are (for an exception see Hume 2013). I argue, however, that there are likely to be at least two important consequences of having judges that are overly responsive to the public. The first is that judges who are too busy paying attention to what the public might think of their decisions, may be too preoccupied to fully consider the facts of the case. I consider this possibility in Chapter Three by looking at death penalty cases and examining whether the impact of the case facts differs between appointed and elected judges. I find only tentative support for this hypothesis. I do not find that elected judges, in general, are less affected by the facts of the case than are appointed judges. I do, however, find that judges reselected in low-information nonpartisan and retention elections may be influenced less by the sum of the case facts when compared to appointed judges

[1] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

and judges reselected in partisan elections. This raises at least some questions about how the impact of public opinion combined with low-information elections can affect an elected judge's ability to consider the relevant case facts when making decisions.

The second way in which the reliance of elected judges on public opinion may be detrimental is the possibility that elected judges will consider only the public opinion of majorities, while subsequently overlooking and thus failing to protect the interests of minorities. This is a likely possibility because elected judges are concerned with the opinion of the public, mostly because they need their support to win their next election and retain their seat on the bench (Bright and Kennan 1995; Bright 2000; Baum 2003). In order to win the election, however, a judge needs the support of a majority of voters. Thus when judges consider public opinion when deciding cases with an eye towards their next election, they are likely to consider only the opinion of the majority who can keep them in office. This means that the opinion and interests of minorities are not likely to be given a fair hearing whenever judges are elected (Hume 2013). Using the same dataset of death penalty decisions and separate estimates of public opinion for blacks and non-blacks, I find that once again the difference may be more fairly said to exist between judges reselected in low-information elections and judges who are either appointed or reelected in high-information partisan elections.

In this dissertation, I consider three new consequences of selecting judges in competitive elections. Firstly, I examine whether voters in judicial elections are actually making choices based on the performance of the judge. I find that while voters do seem to react to some normatively appropriate factors when making decisions in judicial elections, they are also influenced by the performance of the state economy and the popularity of the president. This indicates that voters are also considering broad national trends, that should theoretically have no

effect on judicial elections, because judges cannot directly control either the economy or the performance of the executive. These findings call in to question one of the biggest arguments in support of judicial elections, namely the argument that judicial elections increase democratic accountability. Next, I examine whether the tendency of elected judges to rely on public opinion makes them less likely to be influenced by the facts of the case. In Chapter Three, I do not find support for the argument that elected judges, in general, are affected less by the case facts when compared to appointed judges. However, I do find that appointed judges and judges reselected in partisan elections are influenced more by the facts of the case than are judges reselected in nonpartisan or retention elections. This indicates that judges who are appointed or elected in high-information elections, are both more likely to be swayed by the facts of the case than are judges reselected in nonpartisan or retention elections. Lastly, in the fourth chapter of this dissertation, I investigate whether the practice of electing judges is harmful to minority rights. In this chapter, I undercover some evidence that suggests reselecting judges in low-information elections may be harmful to minority interests. Thus even if this dissertation does not serve as a definitive indictment of judicial elections, it should at the very least raise some important questions and concerns about how the practice of electing judges in low-information elections may negatively impact society.

Chapter Two: A Reexamination of Democratic Accountability in Judicial Elections.

Scholars have long been interested in assessing what factors influence judicial elections. The resulting body of literature that has developed around this question has identified many important determinants of a judicial candidate's vote share. Of these factors, many of them are the things that one would normatively expect and want to influence judicial elections, such as candidate quality (Hall and Bonneau 2006), party (Squire and Smith 1988; Schaffner, Streb, and Wright 2001; Baum 2003) or bar ratings and endorsements (Sheldon 1977; Volcansek 1981; Dubois 1984). Many scholars have pointed to this evidence to argue that elections are a good and valid selection method for judges (Hall 2001 (a)). Other scholars, however, who have taken a less optimistic view of judicial elections, have found that judicial elections can also be affected by factors that one would normatively prefer not to influence elections, such as ballot placement (Volcansek 1981), local "friend and neighbor" effects (Aspin and Hall 1987; Thielmann 1993), or the candidate's race or sex (Uhlman 1977; Dubois 1979, 1984; Alozie 1990; Ifill 1998; Hall 2001 (a); Bratton and Spill 2002; Frederick and Streb 2008; Streb and Frederick 2009). Overwhelmingly, however, the evidence in the literature has been dominated by studies espousing the former view over the latter (see for example Hojnacki and Baum 1992; Hall 2001 (a); Hall and Bonneau 2006). Consequently, the consensus that seems to be emerging in the literature supports the idea that judicial elections are influenced by normatively desirable factors, and that the selection of judges by elections can actually be seen as enhancing the democratic accountability of the courts (Hojnacki and Baum 1992; Hall 2001 (a); Hall and Bonneau 2006). This, however, conflicts with numerous studies that have found voters often know too little about judicial candidates to make informed choices (Johnson, Shaefer, and McKnight 1978; Lovrich Jr.

and Sheldon 1983; Rottman and Schotland 2001; Baum 2003). This lack of information, I argue, leads voters to be influenced by several normatively undesirable factors. This includes two factors that scholars have not previously considered, the state of the economy and the popularity of the current president or state governor. Both of these factors, while arguably relevant to other elections, should be unrelated to judicial elections as judges have little control over either the economy or the administration of the executive branch. This chapter finds some evidence that state GDP, the national unemployment rate, and presidential approval may affect incumbent vote share. If these findings can be strengthened in future studies, then they would challenge the conventional wisdom that judicial elections are democracy enhancing, by showing that voters are systematically influenced by factors that are clearly beyond the control of the individual judges.

Judicial Elections Can Be Influenced by Normatively Desirable Factors

Scholars have uncovered some evidence that voters in judicial elections can be influenced by factors that are, in a normative sense, appropriate for voters to consult when making decisions in judicial contests (Dubois 1984; Hojnakci and Baum 1992; Hall 2001 (a)). For instance, Hojnakci and Baum (1992) found that voters in the 1986 and 1988 Ohio Supreme Court elections paid attention to cues given out by local unions. Unions in these two elections were particularly likely to organize in support of the Democratic judicial candidates. Thus the findings of Hojnakci and Baum (1992) that union members were significantly more likely than non-union members to vote for Democratic candidates in these elections, provides evidence that voters were at least partially considering relevant cues when casting their ballots. In another example of voters responding to election issues, Hall (2001 (a)) finds that voters penalize incumbent judges for an increase in the murder rate of the state. This is a normatively appropriate criterion for

voters to consider to the extent that one believes individual judges can influence the state's murder rate by sentencing criminals harshly to deter future crime.

Endorsements and bar poll ratings are another relevant cue in judicial elections, because they can provide voters with important information regarding candidate quality. Scholars have found some evidence that these factors can have a small impact on judicial contests (Sheldon 1977; Volcansek 1981; Lovrich Jr. and Sheldon 1983; Dubois 1984). Volcansek (1981), for example, finds that both whether or not a candidate received an endorsement from a major newspaper and the candidate's bar rating contributed to explaining a candidate's overall vote share. Dubois (1984) finds that these factors are important in both primary and runoff judicial elections. The effectiveness of bar activities, however, can be influenced by several institutional features of the election system (Sheldon 1977). For instance, bar groups have been found to be more effective in partisan rather than nonpartisan elections (Sheldon 1977). In addition, the effectiveness of bar activities is strengthened as the level of two-party competition in the state decreases (Sheldon 1977). The impact of bar polls and endorsements also varies with the audience (Lovrich Jr. and Sheldon 1983). Voters who are already more informed about the election, are more likely to report that they considered these cues when making their decisions (Lovrich Jr. and Sheldon 1983).

Another important and normatively relevant cue for voters to consider is the party identification of the candidates. Baum (2003) suggests that voters in judicial elections may be particularly likely to rely on party cues, and thus the judicial candidates of the dominant party in the state may be significantly advantaged. The relationship between party and voting, however, is conditioned on several institutional features of the election (Dubois 1979; Squire and Smith 1988; Schaffner, Streb, and Wright 2001). Specifically, party has been shown to be more

important when the candidate's party is included on the ballot (Dubois 1979; Schaffner, Streb, and Wright 2001) or directly provided to voters (Squire and Smith 1988).

Related to party is the ideology of the judicial candidates. Hall (2001 (a)) studies elections to state courts of last resort and computes the ideological distance between the incumbent judge and the electorate. She finds that incumbent judges in nonpartisan election states perform worse when they are more ideologically distant from the electorate they represent. This provides some evidence that voters are responding to normatively appropriate factors and removing judges when they drift too far from the preferences of their constituency (Hall 2001 (a)).

Why Judicial Elections Are Likely Influenced by Normatively Undesirable Factors

Although scholars have presented some evidence that judicial elections can sometimes be influenced by normatively desirable factors (Sheldon 1977; Volcansek 1981; Lovrich Jr. and Sheldon 1983; Dubois 1984; Hojnakci and Baum 1992; Hall 2001 (a)), there are many reasons to doubt that these factors are always systematically important. This is because there are many unique features of judicial elections that make it difficult for voters to base their decision on normatively appropriate criteria (Johnson, Shaefer, and McKnight 1978; Volcansek 1981; Baum 2003; Scott 2009; Bonneau and Cann 2011). One feature that make judicial elections different from most other elections is the low visibility of most judicial races (Volcansek 1981; Iyengar 2002; Baum 2003; Frederick and Streb 2008). Judicial elections rarely capture the attention of the media, and consequently, voters often have little to no information to base their choices on (Iyengar 2002; Baum 2003). In fact, Baum (2003) suggests that most, if not all, of the

information voters have about judicial elections is information that they have learned from the ballot itself.

Another unique feature of judicial elections is that in some states the party identification of the candidates is not listed on the ballot. This deprives voters of one of the most important pieces of information about the candidates, and may lead voters to consider other less relevant information that can be gleamed from the ballot instead, such as the race, ethnicity, or party identification implied by the candidate's name (Dubois 1979; Baum 1984; Schaffner, Streb, and Wright 2001; Hall 2001 (a); Klein and Baum 2001; Rock and Baum 2010). Schaffner, Streb, and Wright (2001) study all types of nonpartisan elections and find that removing party information from the ballot does reduce the impact of partisanship on vote choice, while simultaneously increasing the impact of incumbency. Thus the net effect of this is to reduce voter's attention to important cues, while increasing their attention to arguably less important ones (Schaffner, Streb, and Wright 2001). This effect can be mitigated among more informed voters (Baum 1984), more visible elections (Rock and Baum 2010) or if partisan information about the candidates is explicitly made available to voters, despite the nonpartisan nature of the election (Squire and Smith 1988). Additionally, Hall (2001 (a)) finds that minority candidates for judicial office are slightly disadvantaged when they must run in nonpartisan elections, which suggests that, absent the party cue on the ballot, voters are likely to resort to making decisions based on inappropriate factors. One final feature of nonpartisan elections that affects voters' ability to make decisions in these types of elections, is the finding that candidates in nonpartisan elections are typically not able to raise or spend as much money as candidates in partisan elections (Bonneau 2007). Thus nonpartisan elections reduce the information available to voters, not only by removing one of the

most important cues from the ballot, but also by limiting the ability of the candidates to get other information out to voters (Bonneau 2007).

Judicial elections are also different from most other elections in that the candidates are often quite limited in the electoral activities they may partake in (Briffault 2004; Peters 2009; Bonneau and Cann 2011). While these restrictions were adopted with the purpose of rescuing the legitimacy of the courts, they also have the effect of severely limiting what information is available to voters (Peters 2009).[2] Bonneau and Cann (2011) find that when states adopt stricter fundraising requirements for judicial elections, both incumbents and challengers are limited in the amount of money they are able to raise and spend. As spending money is one of the primary ways that candidates can make information available to voters, these fundraising restrictions directly limit how much voters learn about the election (Bonneau and Cann 2011). Furthermore, when voters learn less about the candidates they may be more likely to pay attention to easily available, but subpar, voting cues, such as relying solely on incumbency (Bonneau and Cann 2011).

The lack of voter information in these elections is real a problem (Johnson, Shafer, and McKnight 1978; Lovrich Jr. and Sheldon 1983; Rottman and Schotland 2001). Johnson, Shafer, and McKnight (1978), who study voters in Texas, find that 85 percent could not even name one judicial candidates running for the Court of Appeals and even more still failed to recall the name of any candidate running for a lower court seat. In a similar study, Lovrich Jr. and Sheldon (1983) find that less than half of judicial voters could identify a single candidate. These recall statistics are lower than they are for both the House and the Senate, suggesting that the lack of information in judicial races is significantly worse than it is for other offices (Johnson, Shafer,

[2] Following the case of *Republican Party of Minnesota v. White*, however, which invalidate certain speech restrictions these state codes generally became more permissive (Briffault 2004).

and McKnight 1978). Additionally, only one of five voters surveyed by Lovrich Jr. and Sheldon (1983) self-reported having enough information about the election to feel like they were capable of making an informed decision.

Judicial Elections Can Be Influenced by Normatively Undesirable Factors

The evidence that voters overwhelmingly lack information about judicial contests (Johnson, Shaefer, and McKnight 1978; Lovrich and Sheldon 1983) leads directly to the expectation that voters will be influenced by normatively undesirable factors. This is because if voters do not have enough appropriately relevant information, they will be forced to make a choice based on the little information they do have, which is likely to be less relevant and perhaps normatively inappropriate for voters to rely on (Dubois 1979; Schaffner, Streb, and Wright 2001; Hall 2001 (a); Iyengar 2002). Volcansek (1981), for instance, finds that the order in which the candidates are placed on the ballot in trial court elections, something which is determined by the first letter of the candidate's last name, can have significant electoral effects. Additionally, Dubois (1979) finds that when candidates have names that have historically been identified with the political party opposite of them, party is more weakly correlated with the vote. This results from individuals incorrectly guessing the party of the candidate (Dubois 1979).

The lack of information in judicial elections can also increase the importance of candidate characteristics to the outcome (Uhlman 1977; Dubois 1979; 1984; Alozie 1990; Ifill 1998; Hall 2001 (a); Frederick and Streb 2008; Streb and Frederick 2009). This may be a result of the fact that even without any information about the candidates, voters can often infer their gender and race based on the candidates' names shown on the ballot (Dubois 1979). Thus even if voters do not bring any prior knowledge of the candidates to the voting booth, they can still be

influenced by these factors (Dubois 1979). Dubois (1984) studies judicial elections in California and finds that although there is no direct effect of the candidate's race or sex on primary elections, female candidates do perform significantly worse in runoff elections. Additionally, Bratton and Spill (2002) find that women are more likely to be appointed to the bench than elected, and that this is truer when the appointing governor of the state is a Democrat. Frederick and Streb (2008), however, find that the effect of being female is actually slightly positive in intermediate appellate court elections. They attribute this to voters relying on the stereotype that women are inherently more trustworthy than are men (Frederick and Streb 2008). Although these studies are in disagreement over the direction of the effect, they all find that sex can be important in judicial elections (Dubois 1984; Alozie 1990; Bratton and Spill 2002; Frederick and Streb 2008; Streb and Frederick 2009). This contrasts to the normative ideal of all candidates being judged solely on their merits. In addition to the effects of gender, Hall (2001 (a) finds that minority candidates are disadvantaged in nonpartisan elections. Minority candidates are also significantly disadvantaged in at large compared to district level judicial elections (Ifill 1998). Additionally, one study found that of all the black judges in the sample, only six percent of those judges were elected. This was much lower than the comparable number for white judges (Uhlman 1977). Thus voters in judicial elections are relying on both sex (Dubois 1984; Alozie 1990; Bratton and Spill 2002; Frederick and Streb 2008; Streb and Frederick 2009) and race (Uhlman 1977; Ifill 1998; Hall 2001 (a)) to evaluate judicial candidates, neither of which are ideal factors for voters to consider.

Local "friend and neighbor" effects can also be seen in judicial elections, probably as a result of the dearth of other information available to voters (Aspin and Hall 1987; Thielemann 1993). Friend and neighbor effects occur when a candidate gains an advantage as a result of

being known to the area (Thielemann 1993). Thielemann (1993) finds that local candidates are likely to be advantaged, not only electorally, but also in their fundraising ability. Aspin and Hall (1987), however, find that the local effect can be either positive or negative. Thus local voters may be more likely to vote for familiar candidates if they have a positive view of them, but they may also be more likely to vote against candidates if they possess negative information about them (Aspin and Hall 1987). These effects show that local voters have more information than the rest of the electorate, and consequently, may act differently (Aspin and Hall 1987; Thielemann 1993).

Low levels of information about judicial elections is a problem, not only because it can increase the importance of irrelevant factors to the vote, but also because it can decrease the importance of relevant election-specific factors. (Johnson, Shaefer, and McKnight 1978; Streb and Frederick 2009; Peters 2009). In their survey of Texas voters, Johnson, Shaefer, and McKnight (1978) find that in addition to failing to recall the candidates' names, voters were also relatively uninformed with respect to the fact that a candidate for the Texas Supreme Court was in the process of being disbarred. Thus most voters in this instance were clearly lacking information that should have been relevant to their decision (Johnson, Shaefer, and McKnight 1978). In another example, Streb and Frederick (2009) find no effect of facing a quality challenger on the vote share of incumbent intermediate appellate court judges. This finding may not be limited to intermediate appellate courts either. Peters (2009) also finds no relationship between challenger quality and incumbent performance when studying state Supreme Court elections, however, Hall and Bonneau (2006) do. If there is no effect of quality challengers on an incumbent's vote share, this suggests that voters do not have enough information to respond to the quality of the candidates (Streb and Frederick 2009; Peters 2009).

Previously Unconsidered Normatively Inappropriate Factors that Voters May Also Rely On

Scholars have found some evidence that the lack of available information in judicial elections induces voters to consider normatively undesirable factors when making decisions in these contests (Uhlman 1977; Dubois 1979; 1984; Aspin and Hall 1987; Alozie 1990; Thielemann 1993; Ifill 1998; Hall 2001 (a); Frederick and Streb 2008; Streb and Frederick 2009). Scholars, however, have thus far failed to consider all of the normatively undesirable factors that voters may rely on. One such factor that has been overlooked so far, with respect to judicial elections, is the popularity of the president or the state governor. The popularity of the president has already been shown to affect congressional (Abramowitz 1985; Gronke, Koch, and Wilson 2003), gubernatorial (Simon, Ostrom Jr., and Marra 1991; Svoboda 1995; Niemi, Stanley, and Vogel 1995; Atkeson and Partin 1995), and even state legislative elections (Campbell 1986; Simon, Ostrom Jr., and Marra 1991). Meanwhile, the popularity of the incumbent governor has been shown to affect other gubernatorial races (King 2001), as well as midterm state legislative races (Campbell 1986) and support for judicial confirmations (Squire and Smith 1988). Thus voters may also consider the popularity of the president or governor when deciding how to cast their ballots in judicial elections. In fact, it may be especially likely that voters will consider the president or governor's popularity in judicial elections compared to other elections, because information about judicial elections is considerably harder to come by (Johnson, Shaefer, and McKnight 1978; Lovrich Jr. and Sheldon 1983; Rottman and Schotland 2001; Baum 2003). This leads to the first hypothesis of this chapter, which is that the approval rating of the president and state governor will have a significant impact on judicial elections. This impact may be felt in several ways. The first possibility is that all incumbents, regardless of their party, will benefit when the president or state governor is more popular and punished when

they are less popular. The second possibility is that this effect may be felt only when a candidate shares the party of the executive.

Before moving on to what other factors mays affect judicial elections, it is worth taking a moment to discuss whether or not it is actually undesirable for voters in judicial elections to make their choices by considering the popularity of the current state governor or president. In some respects, this question is essentially a normative question, and as such, there is no definite answer. I, however, argue that it is undesirable for voters to consider the popularity of the current president or state governor, because the performance of the executive is irrelevant to how a potential judge will perform once on the bench. In some states, where the partisan identification of judges is known, a candidate may share a party affiliation with the current executive. In these cases, there may be some reasons to consider the two candidates jointly, however, the executive and legislative branch perform very different functions. Additionally, a well-performing executive, is in no way guarantee of a well-performing judge. In states where the party identification of the judge is unknown, there is even less of a reason for voters to rationally consider the performance of the executive when voting in judicial elections.

One other factor that may influence judicial elections, even though it should be unrelated to them, and has been thus far overlooked by judicial scholars, is the performance of the economy. Scholars have shown that the economy affects voting in both national (Fiorina 1978; Kinder and Kiewiet 1979, 1981; Radcliff 1994; Norpoth 2001; Nadeau and Lewis-Beck 2001) and state and local elections (Svoboda 1995; Niemi, Stanley, and Vogel 1995). Thus it is possible that the economy will also have an effect on judicial elections. In fact, this may be particularly likely in judicial elections, because most voters in these contests do not have much election-specific information to consider instead (Johnson, Shaefer, and McKnight 1978; Lovrich

Jr. and Sheldon 1983; Rottman and Schotland 2001; Baum 2003). This leads to the second hypothesis of this chapter, which is that the performance of the economy will have a significant impact on judicial elections. As in the case of executive popularity, the impact of the economy may manifest itself in several ways. First, it is possible that all incumbents will be judged on the basis of the national economy. It is also possible, however, that it will be candidates of the president's party only who are judged on the basis of the national economy. The local economy of the state may also be important. Thus either incumbents or candidates of the president's party may be judged on the basis of the state economy as well.

As with executive popularity, the argument that voters in judicial elections should not be influenced by the state of the economy is also somewhat open to interpretation. There is a possibility that judicial decisions could affect broad economic trends. I, however, argue that there are several reason why incumbent judges should not be evaluated on the basis of economic performance. Firstly, the argument that judges can affect economic performance may be somewhat plausible at the state level but certainly cannot be said to extend to the national level. Decisions handed down by state judges are limited to their state borders, and consequently, they cannot be expected to influence national conditions. Thus even if one argues that there may be some basis for voters to consider the economic performance of the state when voting in judicial elections, national economic performance should be unrelated.

I go further, however, and also argue that it is irrelevant for voters to consider state-level economic performance as well. I believe it is undesirable for voters to evaluate judicial candidates based on the performance of the state economy, because, while courts can sometimes make policy, they are still inherently reactive institutions. Courts cannot do anything unless a dispute is first brought before them (Rosenberg 2008). Additionally, even after a case has been

brought before them, judges are constrained, to some extent, by existing law and precedent when making decisions (Rosenberg 2008). Furthermore, even putting aside these two issues and assuming that the court does attempt to independently shape economic policy, any impact the court can actually have on the state of the economy will surely pale in comparison to the effects brought about by the executive and legislative branch. This also raises the question of whether the courts can actually bring about any significant effects without the cooperation of the other two branches (Rosenberg 2008). Rosenberg (2008) studies this question extensively with regards to the ability of the courts to bring about social change. He finds for the most part the courts can only be effective agents for social change when they are supported by the other two branches. His conclusions suggest that courts, acting alone, are likely to have very little effects (Rosenberg 2008). Thus voters wishing to bring about economic change are probably in the best position to do so by exercising their right to vote for their state legislators and governors, not judges.

Data and Methods

Much of the data necessary to test these hypotheses can be found in Bonneau's Judicial Elections Dataset.[3] This dataset includes a wide array of race, candidate, and state-level variables on general elections to the state courts of last resort that occurred between 1980 and 2010. The analysis that follows in this chapter examines elections that occurred between 1990 and 2007. The main dependent variable in this chapter is incumbent vote share. For retention elections, this is simply the percentage of yes votes cast. Because vote share is measured as a percentage, it is theoretically bound between 0 and 100. For the purposes of this analysis, however, I drop all cases (143) where the incumbent's vote share is equal to 100 percent. I do this because when an incumbent receives 100 percent of the vote this is a vote signal that voters did not have a

[3] The author would like to sincerely thank Chris Bonneau for sharing his data with her.

meaningful choice to make. Thus these elections should be considered fundamentally different from the other elections included in the analysis. After dropping these cases, I have 513 cases remaining. Incumbent vote share in these remaining cases ranges from a low of 23 percent to a high of 90 percent.

Two of the main independent variables for this analysis are the popularity of the current president and state governor. The ideal measure of presidential approval would be a measure that is both state and year specific. Fortunately, Pacheco (2014) has made publicly available a dataset that uses the method of multilevel regression, imputation, and post stratification (MRP) to produce estimates of presidential approval for each state that covers the period of years under study here. Pacheco's (2014) MRP estimates are based on an aggregation of polls conducted by CBS/NYT that measure the proportion of people who approve of the president's job performance. Because these are mostly state-level elections[4], however, it is very possible that is the approval of the current state governor, more so than approval of the president, which matters most to voters. Data on gubernatorial approval are available from The Cooperative Project of the University of Rochester, The University of North Carolina Chapel Hill, and the George Washington University.[5] This dataset contains information about different polls that were taken, which measured gubernatorial job approval. To aggregate this data together for each state, I calculate the average of the governor's approval across all of the polls available for that year. I expect both of these variables, presidential and gubernatorial approval, to have a positive sign on them in the models that follow, as a more popular president or governor should be an asset.

[4] A few of the state's included use district level elections for State Supreme Court justices.

[5] Wohlfarth, Patrick. 2010. "U.S. Presidents, 1945-2009 Database." The Cooperative Project of the University of Rochester, the University of North Carolina Chapel Hill, and the George Washington University, http://www.unc.edu/~beyle/jars.html. Accessed May 2014.

The other main independent variable in this chapter is the performance of the economy. I consider the possibility that both the state and national economy will affect judicial elections. I also consider a variety of possible economic indicators including GDP, unemployment, and consumer sentiment. My measures of state and national GDP come from the Bureau of Economic Analysis.[6] National GDP is measured in billions of dollars and state GDP is measured with Per Capital Real GDP in chained 2005 (1997) dollars[7]. I theorize that the level of GDP will be an important cue for voters in judicial elections. It is also, however, possible that voters will be more attuned to GDP growth, rather than the measure of GDP itself. To account for this possibility I gather data on the percent change in GDP from the previous period for both the national and state level from the Bureau of Economic Analysis. These measures reflect GDP growth from the previous year. The second economic indicator I consider is the unemployment rate. Data on the national and state levels of unemployment comes from the Bureau of Labor Statistics.[8] I measure unemployment both at its average level for the year, and like GDP, also consider how it changes from the previous year. The final economic indicator I consider is a more subjective measure based on people's perceptions of the economy rather than on any objective indicators. This measure is an MRP estimate of state-year level consumer sentiment. It was constructed by Pacheco (2014) based on the number of people who rated the state of the national economy positively on CBS/NYT polls.

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[6] "National Economic Accounts-Gross Domestic Product (GDP)." Bureau of Economic Analysis- United States Department of Commerce. http://www.bea.gov/national/index.htm#gdp. Accessed May 2015. "Regional Economic Accounts- Gross Domestic Product (GDP)." Bureau of Economic Analysis- United States Department of Commerce. http://www.bea.gov/regional/index.htm. Accessed May 2015.

[7] Earlier years were measured in chained 1997 dollars.

[8] "Database, Tables, & Calculators by Subject-Unemployment." Bureau of Labor Statistics- United States Department of Labor. http://www.bls.gov/data/#unemployment. Accessed May 2015.

There are two possible ways in which the economy and executive approval may affect judicial elections. The first possibility is that it will be incumbents who are rewarded or punished based on these factors. To test this theory, I simply enter the variables as they are into the model and look to see if any of them significantly affects incumbent vote share. The second possibility is that voters will attribute the blame or rewards for these factors solely to in-party members. I test for this possibility by identifying, among the incumbents, the ones who are members of the in-party in any given year. Then I reverse code the key independent variables for out-party members. These variables are then coded in a way where positive factors are expected to help in-party members and hurt out-party members. To properly identify in-party members I use the party of the president for variables measured at the national level and the party of the state governor for variables measured at the state level.[9]

In the analysis, it will also be necessary to control for a variety of other factors, which have been demonstrated to affect judicial elections. These factors may include things at the level of the state, race, or candidate. At the national and state level it may be important to control for the murder rate. Hall (2001 (a)) has shown that incumbents are punished when the murder rate of the state increases. She argues that this is evidence of democratic accountability, because voters are holding incumbents responsible for something at the state level over which they may exercise some control over (Hall 2001 (a)). In my analysis, I consider both the national yearly murder rate, which is available from The Death Penalty Information Center,[10] and the state-yearly

[9] Incumbents whose party matches the party of the president or state governor in that year are considered in-party members. All other incumbents, including those for whose party is unknown or missing are considered out-party members.

[10] "Murder Rates Nationally and by State." 2014. Death Penalty Information Center. http://www.deathpenaltyinfo.org/murder-rates-nationally-and-state.

murder rate, available in the Judicial Elections Initiative Data.[11] Hall (2001 (a)) argues that a negative relationship between the state murder rate and incumbent vote share provides evidence of democratic accountability in judicial elections. If there is also a negative relationship between the national murder rate and incumbent vote share, however, then this may weaken the claim of democratic accountability, because judges should not be punished by a rational electorate for the murder rate of the nation, when they can only influence conditions in their own state.

Other factors that are important to control for include characteristics of the race, such as the type of election used. States have many different election rules. Most judicial elections, however, can be classified into one of three main types, partisan, nonpartisan, or retention elections. Partisan elections are probably the most similar to the type of familiar elections that are commonly held for other offices. They are contested elections where the party identification of the candidates is listed on the ballot. Nonpartisan elections are similar, except for the fact that the party identification of the candidates does not appear on the ballot. Lastly and most different are retention elections. These are elections that are not contested. Instead, the public simply votes either yes to keep the incumbent judge in office or no to remove the incumbent judge from the bench. Election type can affect vote share, because scholars have found that partisan elections are more competitive (Hall 2001 (a); Bonneau 2007) and have higher turnout (Dubois 1979; Hall 2007; Schaffner, Streb, and Wright 2001; Streb and Frederick 2011; Hall and Bonneau 2013) than either of the other two types of elections. They are also more likely to attract quality challengers than nonpartisan elections (Bonneau and Hall 2003). Controlling for election type can be accomplished by including dummy variables, available in Bonneau's data, for partisan and nonpartisan elections in the analysis. In this scenario, retention elections become the omitted

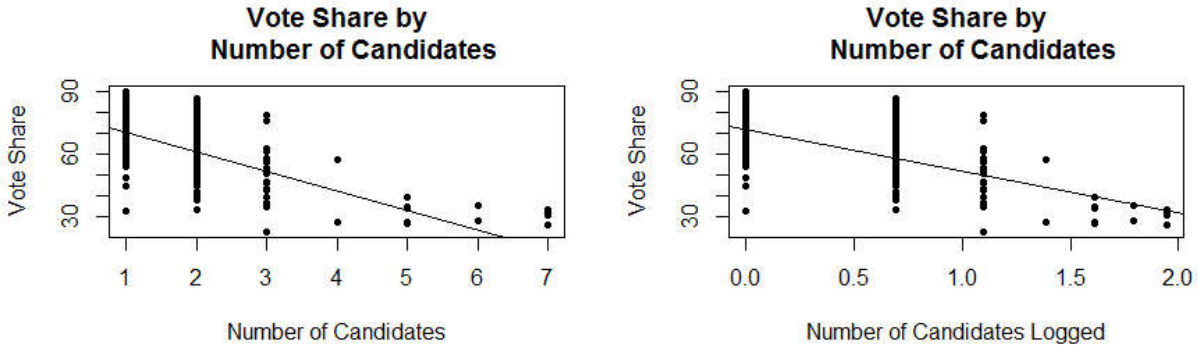
[11] Martin, Andrew et al. 2012. "The Judicial Elections Data Initiative." The Center for Empirical Research in the Law at the Washington University of St. Louis, http://jedi.wustl.edu/index.php. Accessed 2014.

category. Because partisan and nonpartisan elections should be more competitive than retention elections (Hall 2001 (a); Bonneau 2007), a negative sign is expected on both of these dummy variables. Judicial elections also differ according to whether the candidates run statewide or in districts (Hall 2001 (a)). Thus I also include a variable, district, that is coded 1 for district level elections and 0 for state-level elections. This controls for any differences that may arise between judges selected by these two different constituencies. Because Hall (2001 (a)) has previously found that candidates do better in district elections, I expect the sign on this variable to be positive.

Another important feature of the race is the number of candidates running. Unlike in congressional elections, where there is almost never more than one candidate from each major party, the number of candidates in judicial elections is considerably more variable. In retention elections for example, there is always only one candidate per seat. In contested elections, however, it is not uncommon for there to be three or more candidates vying for the same judicial seat. In the set of elections I analyze, the number of candidates in a race ranges from 1 to 7. As the number of candidates in a race increases, the vote share of the incumbent candidate can naturally be expected to decline. This is simply a result of voters having more options. To control for this phenomenon, I include a variable that measures the number of candidates in the race. The data for this variable comes from Bonneau's dataset, and the expected sign on this variable is negative. A linear relationship between the number of candidates in the race and an incumbent's vote share, however, may not be the most appropriate model. This is because the drop in expected vote share when going from 1 candidate to 2 candidates may be a lot bigger than the drop in expected vote share when going from 5 candidates to 6 candidates. To investigate the relationship between incumbent vote share and the number of candidates in the race more

carefully, I construct a scatterplot between these two variables, which is displayed in Figure 2.0. I also consider the Ramsey RESET statistic for an OLS model of the number of candidates on incumbent vote share. Both the scatterplot, where the points at the higher number of candidates fall above the regression line, and the significant Ramsey RESET statistic (13.4, p=0.00) suggest that a linear model may not be the most accurate way to depict this relationship. To better capture the true relationship between the number of candidates and an incumbent's vote share, I take the log of the number of candidates. The scatterplot between this transformed variable and incumbent vote share, also displayed in Figure 2.0, shows a better fit between the points and the regression line. Additionally, the Ramsey RESET statistic (1.78, p=0.17) is no longer significant. These results both suggest that the transformed variable is more appropriate. Thus in the models that follow I will use the log of the number of candidates in the race. In the appendix to this chapter, however, I also provide results where this variable has not been logged for comparison purposes.

Figure 2.0: Vote Share by Number of Candidates and Number of Candidates Logged



In addition to the number of candidates running, the quality of these candidates is also important. Scholars have presented evidence that quality candidates perform better electorally

(Hall and Bonneau 2006; Bonneau 2007). Thus another important characteristic of the race to control for is whether or not the incumbent is facing a quality challenger. For the purpose of this analysis, challengers will be considered quality challengers if they have ever previously been a judge. This is measured with a dummy variable, collected by Bonneau, which takes on the value of 1 if there is at least one quality challenger in the race and a value of 0 otherwise. This variable is expected to have a negative relationship with incumbent vote share, as the evidence in the literature so far indicates that quality challengers do receive more votes (Hall and Bonneau 2006; Bonneau 2007).

It is also important to control for characteristics of the candidates, such as the candidate's race and sex, both of which have been shown to influence judicial elections (Uhlman 1977; Dubois 1979; 1984; Alozie 1990; Ifill 1998; Hall 2001 (a); Frederick and Streb 2008; Streb and Frederick 2009). To control for the candidate's sex, I include a dummy variable from Bonneau's data that takes on the value of 1 if the candidate is female and 0 otherwise. The sign on this variable is expected to be positive, as some studies have shown that females are seen as more trustworthy and thus may actually have an advantage in judicial elections (Frederick and Streb 2008). Similarly, to control for race I include a dummy variable, also from Bonneau's data, which takes on the value of 1 if the candidate is of a minority race and 0 otherwise. The expected sign on this variable is negative, as studies haves shown that minorities are slightly disadvantaged in judicial elections (Uhlman 1977; Ifill 1998; Hall 2001 (a)). For completeness, I also include variables that measure whether or not any of the challengers in the race are minorities or female. This is important because if race or sex matters, then running against a female or a minority should also affect vote share. These factors are accounted for by including dummy variables that are coded 1 if the incumbent is facing a minority or female challenger

respectively. The expected sign on these variables are negative for female challengers and positive for minority challengers. Although these variables are not the main subject of the analysis, it is worth considering whether or not they have any significant effects. If there are this would be additional evidence that voters are relying on inappropriate criteria to select judges, because candidates' races or sexes should have very little bearing on their qualifications for office.

There is also reason to expect vote share to vary depending on incumbency status. Although all of the candidates in this analysis are incumbents, the nature of their incumbency can affect the extent to which it advantages them. For example, Hall (2001 (a)) has suggested that incumbents who were appointed to the bench initially, and have not yet faced an election, may not enjoy the full advantages of incumbency that normally accrues to judicial candidates who have already successfully faced elections. To account for this possibility, I include a variable from Bonneau's data that takes on the value of 1 if the incumbent was initially appointed and has not yet ran for election and 0 otherwise. The expected sign on this variable is negative.

Candidates also differ based on their party affiliation. There is some reason to expect that candidates who are affiliated with one of the two major parties will perform differently than candidates whose party affiliation is unknown, who are unaffiliated, or who are affiliated with one of the minor parties, because party actors will be more invested in these races (Bonneau 2005). To control for this, I include in the model a dummy variable for major party that is coded 1 for known candidates of a major party and 0 otherwise. Candidate's party affiliation is taken from Bonneau's dataset and the expected sign on this variable is positive.

Lastly, it may also be important to control for the amount of money spent by the candidate. Scholars have found some evidence that spending by the candidates can be influential

in judicial races (Hall and Bonneau 2006; Bonneau 2007; Frederick and Streb 2008; Streb and Frederick 2009; Bonneau and Cann 2011). Thus to control for this, I measure the difference between the spending of the incumbent and his or her opponents. The expected sign on this variable is positive, because candidates are expected to perform better when they have a spending advantage over their challengers. Data on candidate spending are also available in the Bonneau dataset. It is missing for many elections, however, so I consider models both with and without this variable included. A summary of all of the variables to be used in this chapter's analyses can be found in Table 2.1.

Table 2.1 Summary of Variables

Statistic	N	Mean	St. Dev.	Min	Max
Vote Share	513	65.1	12.7	23.1	89.9
Number of Candidates logged	513	0.3	0.4	0	1.95
Natl GDP in thousands of billions (Inc. coded)	513	9.9	2.8	6.0	14.7
Natl GDP in thousands of billions (In-Party coded)	513	-5.8	8.6	-14.7	14.7
Natl GDP Growth (Inc. coded)	513	3.0	1.4	-0.3	4.8
Natl GDP Growth (In-Party Coded)	513	-1.8	2.8	-4.5	4.8
State GDP in thousands of billions (Inc. coded)	513	33.3	8.0	17.4	59.7
State GDP in thousands of billions (In Party Coded)	513	19.3	28.3	-43.5	59.7
State GDP Growth (Inc. coded)	513	1.9	2.4	-6.2	8.7
State GDP Growth (In-Party coded)	513	1.2	2.8	-6.2	8.7
Natl Unemployment (Inc. coded)	513	5.4	0.9	4.0	7.5
Natl Unemployment (In-Party coded)	513	-3.3	4.4	-7.5	7.5
Natl Unemployment Change (Inc. coded)	513	0.04	0.7	-0.8	1.2
Natl Unemployment Change (In-Party coded)	513	-0.1	0.6	-1.2	1.2
State Unemployment (Inc. coded)	513	5.0	1.3	0.4	9.0
State Unemployment (In-Party coded)	513	2.7	4.4	-7.6	9.0
State Unemployment Change (Inc. Coded)	513	-0.02	0.6	-1.6	2.3
State Unemployment Change (In-Party coded)	513	-0.01	0.6	-1.6	2.3
Pres. Approval (Inc. coded)	511	0.6	0.1	0.2	0.8
Pres. Approval (In-Party coded)	511	0.3	0.5	-0.8	0.8
Gov. Approval (Inc. Coded)	406	53.0	14.8	2	81

Gov. Approval (In-Party coded)	406	26.8	48.0	-80	81
Consumer Sentiment (Inc. coded	509	0.6	0.2	0.1	0.9
Consumer Sentiment (In-Party coded	509	0.3	0.5	-0.9	0.9
Natl Murder Rate (Inc. coded)	513	6.9	1.6	5.4	9.8
State Murder Rate (In-Party coded)	513	3.1	6.2	-14.1	17.5
Difference in Spending in thousands	201	223.3	524.7	-1,646.7	3,378.4

Statistic	N	No	%	Yes	%
Appointed Incumbent	513	0	61%	1	39%
Quality Challenger	513	0	79%	1	21%
Major Party	513	0	64%	1	36%
Partisan	513	0	82%	1	18%
Nonpartisan	513	0	77%	1	23%
Retention	513	0	41%	1	59%
Female	509	0	77%	1	23%
Female Challenger	503	0	91%	1	9%
Minority	490	0	87%	1	13%
Minority Challenger	482	0	98%	1	2%
District	513	0	88%	1	12%

Analysis

I begin my analysis by first looking at exploratory plots and OLS models between the independent variables I expect to influence judicial elections (executive popularity, economic indicators) and incumbent vote share. The first relationship I consider is the relationship between GDP and vote share. I expect this relationship to be positive, because voters are likely to reward higher GDP or greater GDP growth. Figure 2.1 displays the relationship between incumbent vote share and GDP, which is incumbent-coded or in other words in its original form. The plots suggest a positive relationship between GDP and incumbent vote share, however, the points are not very tightly clustered around the line. This suggests that if such a relationship exists, it is most likely very weak. The state-level plots suggest a stronger relationship than the national plots. Figure 2.2 reproduces the same plots, but this time with the GDP measures in-party coded

or in other words multiplied by negative one for out-party members. This time the relationship between the measures of national GDP and vote share appear to be negative, while the plots between the state measures and vote share continue to suggest a weak positive relationship.

Figure 2.1 Incumbent Vote Share by GDP

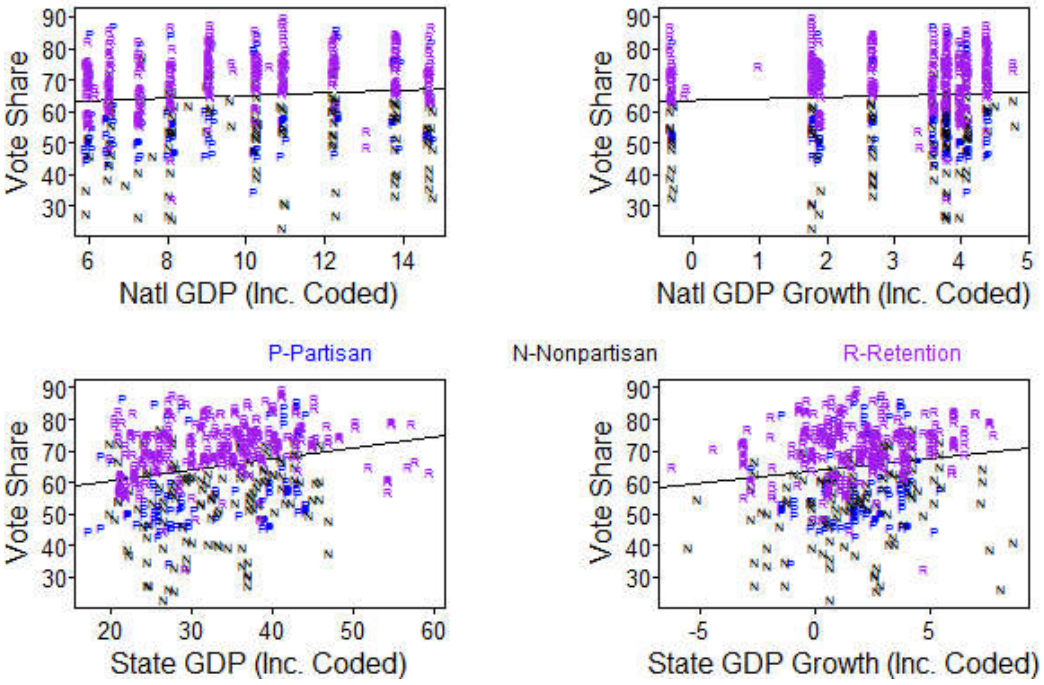
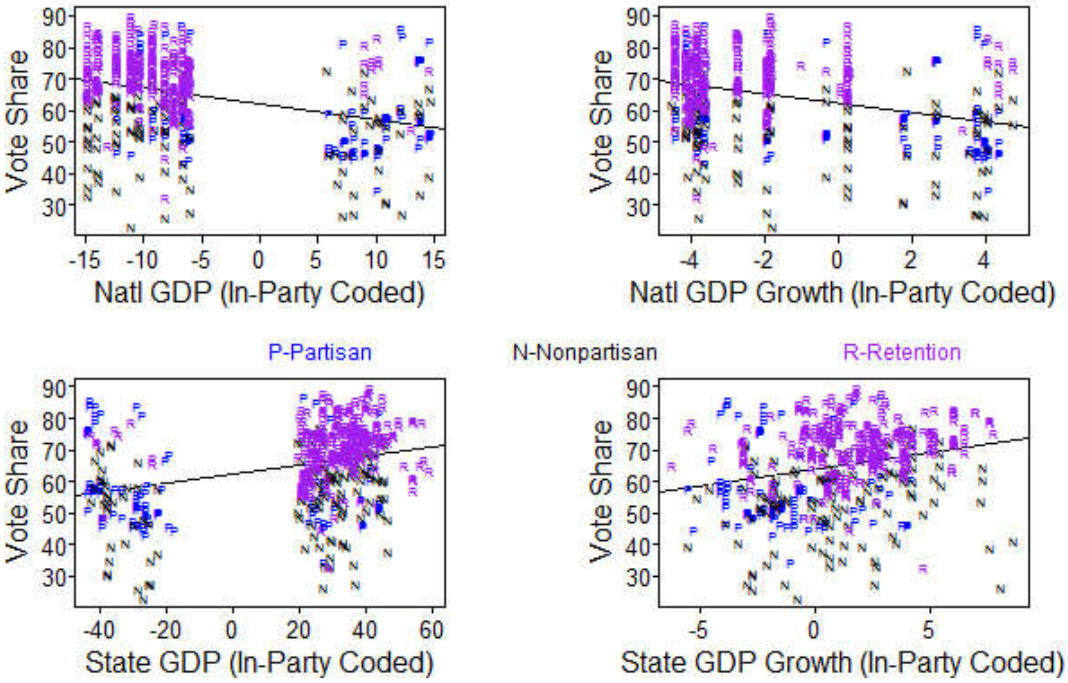


Figure 2.2 Incumbent Vote Share by GDP



To look at these relationships a little more systematically, I use OLS and control for the other variables that we know from previous research also affect candidate vote share. However, there is clustering inherent in the data, with elections being clustered by state. Clustered data are problematic, because it leads to correlation between the error terms. This violates the traditional OLS assumption of independent error terms and can lead to estimated standard errors that are too small (Finch, Bolin, and Kelley 2014). To correct for this problem, I use clustered standard errors (Croux, Dhaene, and Hoorelbeke 2003). I chose this approach over a multi-level model, the other main approach for dealing with clustered data, because clustered standard errors are computationally simpler, require fewer assumptions, and do not affect the coefficient estimates. To ensure that my results are not dependent on my choice to use clustered standard errors rather than a multilevel model, I present multi-level models in the appendix for comparison purposes. (Primo, Jacobsmeier, and Milyo 2006). In the models that follow, however, I present standard

errors, which are clustered by state. I cluster by state, rather than the more specific state-year combination, because when using clustered standard errors, a researcher must assume that the observations are not correlated between clusters (Primo, Jacobsmeier, and Milyo 2006). Clustering by state-year then would imply that cases within a state in different years are independent ((Primo, Jacobsmeier, and Milyo 2006). Given the many state-specific factors that may affect judicial elections across years, this is not an assumption that I feel comfortable making. Therefore, I take the more cautious approach and cluster my errors more broadly by state.

I present the results for the GDP estimates in Table 2.1. Models 1-4 estimate the effect of the GDP variables on incumbent vote share with all of the controls minus the difference-in-spending measure. In models 5-8, I repeat models 1-4, but also in add the difference-in-spending measure. Because this variable is not available for all races, including this variable has the effect of reducing the sample size from about 480 to 190. Looking at the estimated coefficients in these models, one can see that several measures of GDP have the incorrect sign on them. Some of the in-party-coded measures of GDP are even negative and significant. There is a positive and significant effect, however, of incumbent-coded State GDP growth. Although, possibly as a result of the reduced sample size, this variable fails to maintain its statistical significance in the model with the spending measure included. Interestingly, incumbent-coded state GDP is significant only in the model where spending is also included. These results may suggest a relationship between state GDP and incumbent vote share, however, the evidence is tenuous at most.

Table 2.2 GDP OLS Results with Clustered Standard Errors

	Incumbent Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	82.24***	78.67***	65.41***	79.92***	79.92***	82.56***	53.68***	82.56***
	(5.98)	(2.07)	(12.12)	(1.99)	(11.65)	(6.41)	(13.89)	(5.36)
Appointed Incumbent (-)	-0.42	-0.58	-0.31	-0.45	-1.29	-2.01	-0.91	-1.38
	(0.96)	(0.92)	(0.95)	(1.00)	(1.64)	(1.28)	(2.05)	(1.87)
Quality Challenger (-)	-7.86***	-7.86***	-7.78***	-7.98***	-7.90***	-7.80***	-7.91***	-8.08***
	(2.01)	(1.90)	(1.80)	(1.61)	(2.10)	(2.17)	(2.04)	(1.96)
Number of Candidates (-)	-19.53***	-20.63***	-19.93***	-19.47***	-19.94***	-20.85***	-20.00***	-20.05***
	(2.71)	(2.30)	(2.20)	(2.48)	(2.68)	(2.21)	(2.11)	(2.40)
Major Party (+)	3.78	5.15*	2.16	2.02	3.01	3.70	1.61	0.83
	(2.13)	(2.17)	(2.16)	(1.93)	(2.65)	(2.64)	(3.06)	(2.40)
Nonpartisan (-)	2.52	3.17	3.16	2.39	-2.63	-2.95	-1.46	-1.20
	(2.71)	(2.62)	(2.63)	(2.58)	(3.78)	(3.15)	(3.08)	(3.46)
Partisan (-)	7.21*	7.50**	6.69*	5.36				
	(3.03)	(2.83)	(2.79)	(2.76)				
District (+)	4.21	4.60	4.66	4.26	-2.09	-0.75	1.79	-2.17
	(4.00)	(3.94)	(3.87)	(3.91)	(2.26)	(2.17)	(2.64)	(2.22)
Female (+)	0.72	0.75	0.50	0.66	0.77	0.88	0.37	0.75
	(0.60)	(0.61)	(0.60)	(0.58)	(0.92)	(0.94)	(0.86)	(0.81)
Female Challenger (-)	-4.48**	-4.42**	-4.50***	-4.26**	-5.66***	-5.11***	-5.52***	-5.27***
	(1.60)	(1.43)	(1.15)	(1.32)	(1.53)	(1.48)	(1.13)	(1.22)
Minority (-)	-0.79	-0.74	-0.92	-0.91	-0.92	-0.37	-1.06	-0.92
	(1.16)	(1.09)	(1.13)	(1.19)	(1.55)	(1.31)	(1.63)	(1.83)
Minority Challenger (+)	6.05**	7.10***	4.65**	4.71*	6.64***	8.05***	5.21***	5.45***
	(2.13)	(1.76)	(1.79)	(1.94)	(1.58)	(1.18)	(1.11)	(1.22)
Natl Murder Rate (-)	-0.82	-0.75**	0.30	-0.72**	0.11	-0.23	1.75*	-0.19
	(0.52)	(0.28)	(0.68)	(0.25)	(0.88)	(0.60)	(0.68)	(0.52)
State Murder Rate (-)	-0.89**	-0.85**	-0.82*	-0.78*	-0.86	-0.85*	-0.72	-0.70
	(0.32)	(0.32)	(0.33)	(0.32)	(0.45)	(0.40)	(0.47)	(0.44)
Difference in Spending (+)					0.004***	0.004***	0.004***	0.004***
					(0.001)	(0.001)	(0.001)	(0.001)
Natl GDP Inc. Coded (+)	-0.21				0.14			
	(0.30)				(0.62)			
Natl GDP In-Party Coded (+)	-0.16*				-0.16			

	(0.06)				(0.09)			
Natl GDP Growth Inc. Coded (+)		-0.06				0.28		
		(0.32)				(0.56)		
Natl GDP Growth In-Party Coded (+)		-0.88***				-0.95***		
		(0.17)				(0.19)		
State GDP Inc. Coded (+)			0.25				0.47*	
			(0.20)				(0.22)	
State GDP In-Party Coded (+)			-0.01				-0.01	
			(0.03)				(0.03)	
State GDP Growth Inc. Coded (+)				0.71***				0.48
				(0.15)				(0.29)
State GDP Growth In-Party Coded (+)				-0.39*				-0.26
				(0.18)				(0.30)
N	481	481	481	481	191	191	191	191
Adjusted R^2	0.59	0.61	0.60	0.60	0.46	0.50	0.47	0.46

*p < .05; **p < .01; ***p < .001

The next economic variable I consider is unemployment. Plots of incumbent vote share versus unemployment are presented in Figure 2.3 (Incumbent Coded) and Figure 2.4 (In-Party coded). The plots in Figure 2.3 suggest that the relationship between unemployment and incumbents is mostly flat, with the exception of state unemployment, where the relationship may be slightly negative. The lines in the plots in Figure 2.4 are also somewhat flat, with the exception of in-party-coded national unemployment and incumbent vote share.

Figure 2.3 Incumbent Vote Share by Unemployment

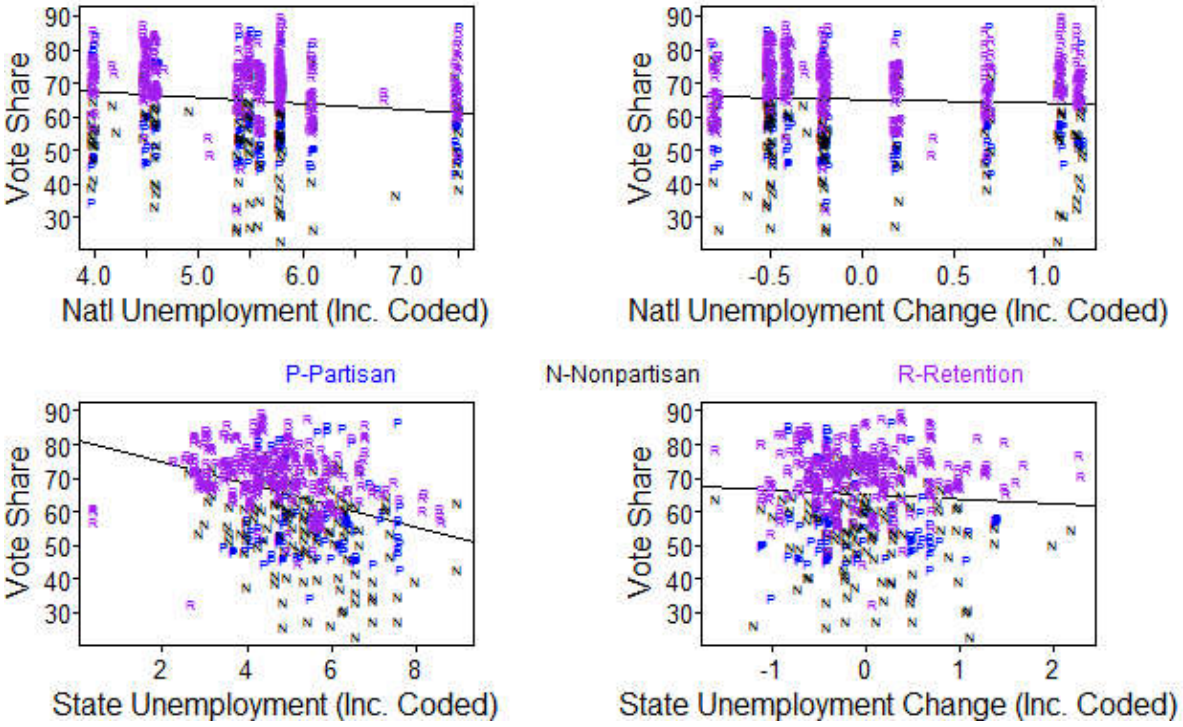
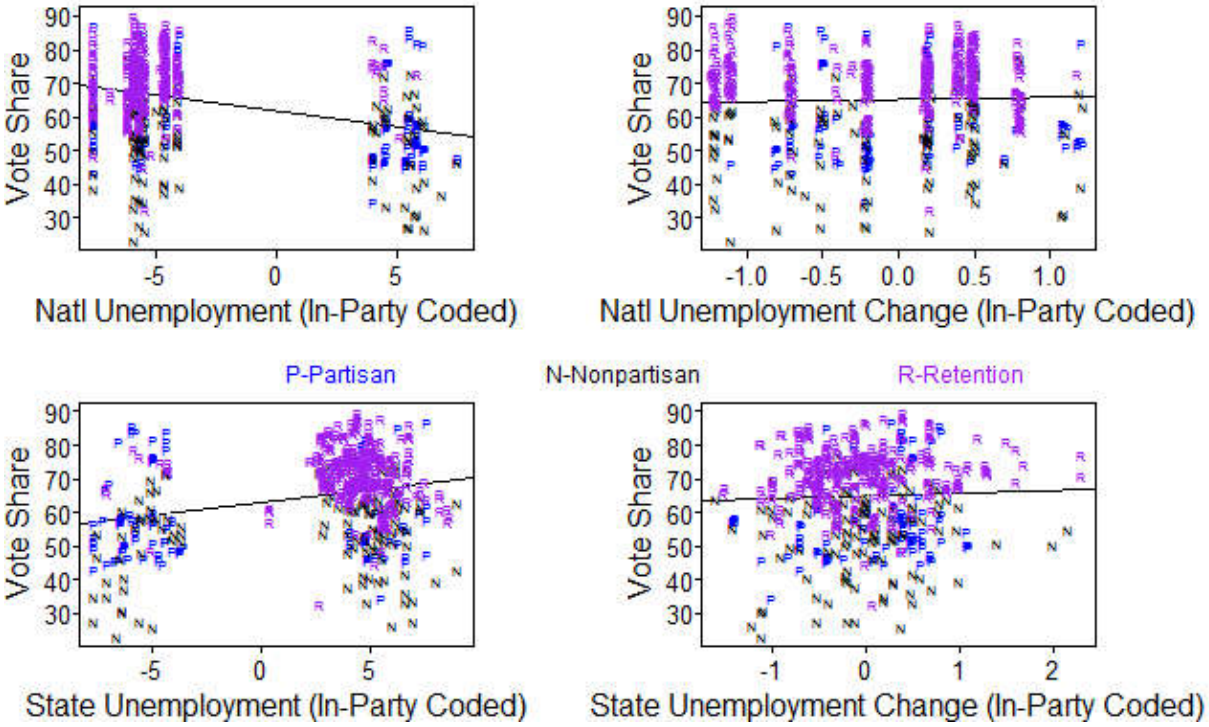


Figure 2.4 Incumbent Vote Share by Unemployment



The OLS results for the unemployment measures, presented in Table 2.3, confirm a significant negative effect of in-party coded national unemployment. This effect is significant in both the models with and without the difference-in-spending variable included. The other unemployment coefficients are either insignificant or significant in the wrong direction. Nevertheless, the significant negative effect of in-party-coded national unemployment suggests that national conditions matter more, and that it is in-party members, rather than incumbents, who receive the lion's share of the blame or credit for the unemployment level. Again the evidence here is only suggestive. However, even weak evidence that voters may be reacting to national conditions, suggests that voters are not responding to normatively appropriate criteria when making decisions in these contests. This is because, while some may argue that judicial decisions can have some incidental economic effects that voters could legitimately be responding to, it is more unlikely that state courts can affect national economic trends, such as the national unemployment rate.

Table 2.3 Unemployment OLS Results with Clustered Standard Errors

	Incumbent Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	80.48***	79.77***	79.75***	80.02***	84.31***	82.28***	82.36***	82.75***
	(2.93)	(1.96)	(2.33)	(1.96)	(7.14)	(5.60)	(7.00)	(5.89)
Appointed Incumbent (-)	-0.37	-0.31	-0.17	-0.22	-1.40	-1.36	-0.67	-1.03
	(0.96)	(0.95)	(0.99)	(0.97)	(1.68)	(1.47)	(1.99)	(1.73)
Quality Challenger (-)	-7.68***	-8.13***	-7.85***	-7.57***	-7.77***	-8.36***	-8.04***	-7.82***
	(1.97)	(1.72)	(1.93)	(1.74)	(2.19)	(1.85)	(2.18)	(2.01)
Number of Candidates (-)	-19.52***	-20.70***	-19.65***	-19.03***	-20.07***	-21.40***	-19.99***	-19.76***
	(2.64)	(2.86)	(2.35)	(2.71)	(2.54)	(2.78)	(2.38)	(2.73)
Major Party (+)	4.15*	2.53	2.69	2.41	3.24	0.65	2.02	1.14
	(2.09)	(2.07)	(2.15)	(2.05)	(2.51)	(2.19)	(2.92)	(2.52)
Nonpartisan (-)	2.48	3.58	2.63	1.90	-2.64	-1.72	-1.85	-1.70
	(2.69)	(2.71)	(2.64)	(2.76)	(3.74)	(3.42)	(3.55)	(3.65)

Partisan (-)	7.23*	7.27*	6.82*	5.66				
	(2.99)	(2.85)	(3.07)	(3.07)				
District (+)	4.28	4.37	4.12	4.02	-1.78	-1.85	-2.77	-2.44
	(3.90)	(3.93)	(3.95)	(3.88)	(2.22)	(2.33)	(2.05)	(2.23)
Female (+)	0.72	0.63	0.59	0.62	0.83	0.85	0.69	0.67
	(0.60)	(0.61)	(0.59)	(0.58)	(0.93)	(0.98)	(0.97)	(0.86)
Female Challenger (-)	-4.42**	-4.55**	-4.58***	-4.48**	-5.38***	-5.53***	-5.73***	-5.51***
	(1.43)	(1.47)	(1.37)	(1.41)	(1.36)	(1.51)	(1.41)	(1.30)
Minority (-)	-0.77	-0.87	-0.86	-0.71	-0.52	-0.92	-0.98	-0.89
	(1.16)	(1.18)	(1.19)	(1.20)	(1.65)	(1.66)	(1.88)	(1.69)
Minority Challenger (+)	6.61**	5.40**	5.01*	4.76*	7.32***	6.56***	5.77***	5.56***
	(2.33)	(1.68)	(2.01)	(1.90)	(1.67)	(1.06)	(1.47)	(1.27)
Natl Murder Rate (-)	-0.65	-0.58*	-0.62*	-0.58*	-0.06	0.29	-0.12	-0.09
	(0.37)	(0.28)	(0.28)	(0.26)	(0.65)	(0.52)	(0.50)	(0.52)
State Murder Rate (-)	-0.87**	-0.83*	-0.86**	-0.87**	-0.84	-0.73	-0.73	-0.76
	(0.32)	(0.32)	(0.33)	(0.32)	(0.43)	(0.43)	(0.43)	(0.43)
Difference in Spending (+)					0.004***	0.004***	0.004***	0.004***
					(0.001)	(0.001)	(0.001)	(0.001)
Natl Unemployment Inc. Coded (-)	-0.41				-0.46			
	(0.51)				(0.66)			
Natl Unemployment In-Party Coded (-)	-0.40**				-0.38*			
	(0.15)				(0.15)			
Natl Unemployment Change Inc. Coded (-)		0.31				-0.75		
		(1.01)				(1.36)		
Natl Unemployment Change In-Party Coded (-)		1.44				3.37*		
		(0.84)				(1.39)		
State Unemployment Inc. Coded (-)			0.02				0.02	
			(0.38)				(0.58)	
State Unemployment In-Party Coded (-)			0.08				0.13	
			(0.15)				(0.20)	
State Unemployment Change Inc. Coded (-)				-0.80				-0.79
				(1.02)				(1.60)

State Unemployment Change In-Party Coded (-)				1.47*				0.63
				(0.72)				(0.83)
N	481	481	481	481	191	191	191	191
Adjusted R^2	0.60	0.59	0.59	0.59	0.47	0.48	0.45	0.45

*p < .05; **p < .01; ***p < .001

The last economic variable I consider is the MRP measure of consumer sentiment. This variable is expected to have a positive relationship with incumbent vote share, because voters should reward incumbents when they feel more positively about the economy and punish them when these perceptions turn more negative. The plots of consumer sentiment versus incumbent vote share are displayed in Figure 2.5. Both the incumbent-coded measure and the in-party-coded measure suggest a positive relationship between incumbent vote share and consumer sentiment. The points on the plot, however, do not cluster very tightly around the line. This suggests that if a positive relationship does exist, it is not a very strong one. The OLS results, displayed in Table. 2.4, confirm these initial impressions. Both measures of consumer sentiment are estimated to have a positive impact on incumbent vote, but in none of the models are these variables significant at conventional levels of statistical significance.

Figure 2.5 Incumbent Vote Share by Consumer Sentiment

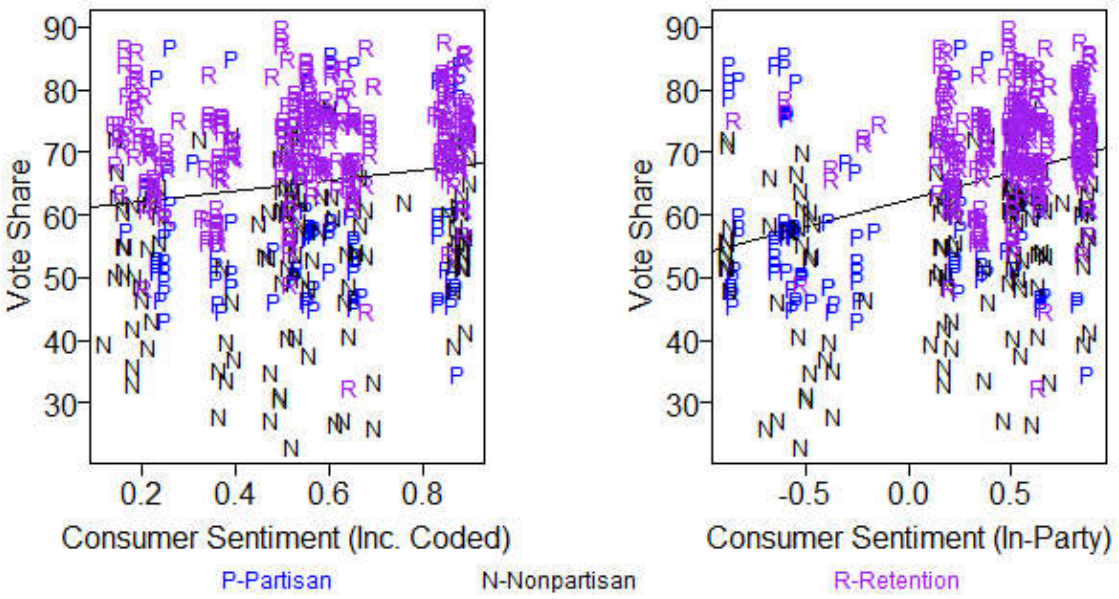


Table 2.4 Consumer Sentiment OLS Results with Clustered Standard Errors

	Incumbent Vote Share	
	(1)	(2)
Constant	78.76***	81.80***
	(2.39)	(6.51)
Appointed Incumbent (-)	-0.32	-1.05
	(1.01)	(1.99)
Quality Challenger (-)	-7.81***	-7.97***
	(1.91)	(2.14)
Number of Candidates (-)	-19.76***	-20.20***
	(2.33)	(2.38)
Major Party (+)	2.29	1.23
	(2.29)	(3.15)
Nonpartisan (-)	2.77	-1.84
	(2.62)	(3.53)
Partisan (-)	6.82*	
	(3.02)	
District (+)	4.17	-2.42
	(3.95)	(2.18)

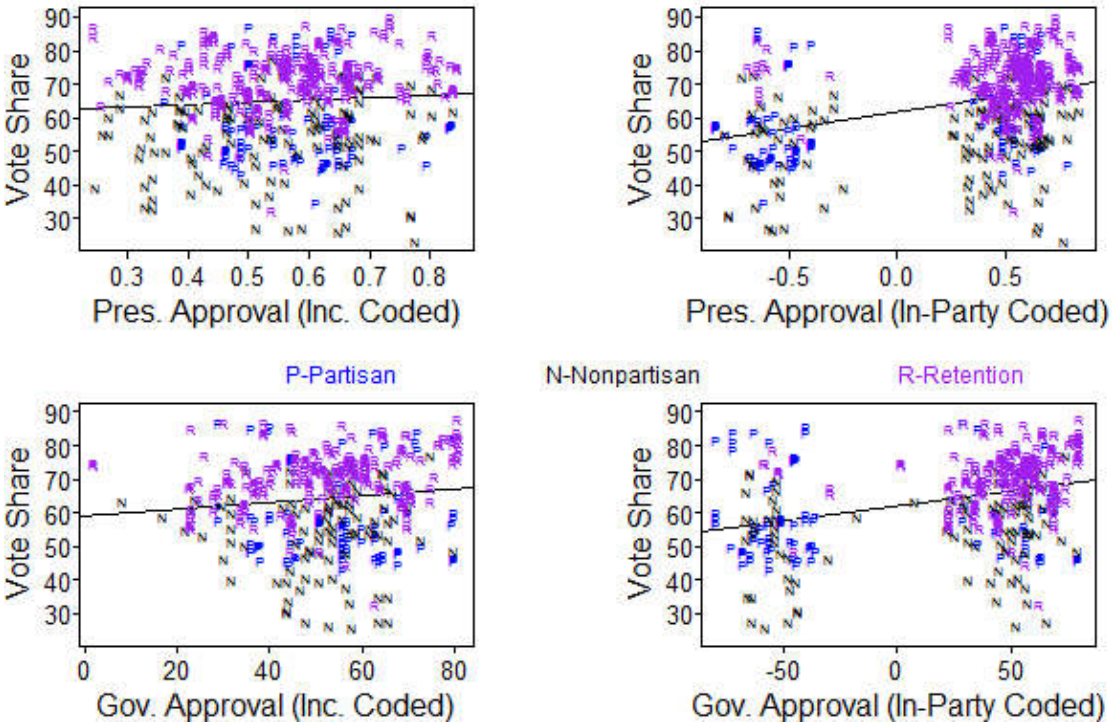
Female (+)	0.61	0.72
	(0.59)	(0.94)
Female Challenger (-)	-4.47***	-5.48***
	(1.31)	(1.29)
Minority (-)	-0.82	-0.81
	(1.21)	(1.85)
Minority Challenger (+)	5.11**	5.87***
	(1.92)	(1.26)
Natl Murder Rate (-)	-0.53	0.004
	(0.31)	(0.51)
State Murder Rate (-)	-0.85**	-0.76
	(0.32)	(0.43)
Difference in Spending		0.004***
		(0.001)
Consumer Sentiment Inc. Coded (+)	1.22	1.36
	(2.10)	(2.70)
Consumer Sentiment In-Party Coded (+)	0.19	0.22
	(1.95)	(2.60)
N	478	191
Adjusted R^2	0.59	0.45

*p < .05; **p < .01; ***p < .001

Aside from economic issues, I also expect voters to consider the popularity of the executive when voting in judicial elections. To investigate these relationships, I plot the MRP measure of presidential approval and the measure of gubernatorial approval against incumbent vote share. I again use measures that are coded both for incumbents and for in-party members. Although it is more likely that voter's evaluations of candidates will depend on their view of the current executive only when the two share a party, it is also possible that voters will view all incumbents as culpable for current conditions. In this case their evaluations of incumbent judges may also depend on their view of the executive. Plots of incumbent vote share versus executive

approval, displayed in Figure 2.6, suggest that, as expected, there is a stronger relationship between executive approval and incumbent performance for in-party members.

Figure 2.6 Incumbent Vote Share by Executive Approval



The OLS results for executive approval and incumbent vote share, illustrated in Table 2.5, show a strong positive relationship between in-party-coded presidential approval and incumbent vote share, both in the model with and without the difference-in-spending variable included. The relationship between in-party-coded gubernatorial approval and incumbent vote share is also estimated to be positive, without the spending variable included, but the coefficient is much smaller and fails to reach statistical significance. Again the fact that it is a national level variable, as opposed to a state-level variable, provides further support for my argument that voters are making decision in judicial elections based on factors that are of very little to no

relevance to the actual election at hand. While some state governors may play a role in appointing judges initially, either temporarily to fill a vacancy or in the case of some retention elections before the first election, a situation where approval of the governor may have some relevance to the election, the president has no role to play on state courts. Thus voters' opinions of the president should be unrelated to incumbent performance, if voters were only responding to normatively appropriate criteria.

Table 2.5 Executive Approval OLS Results with Clustered Standard Errors

	Incumbent Vote Share			
	(1)	(2)	(3)	(4)
Constant	79.23***	80.64***	82.32***	85.57***
	(2.58)	(3.39)	(5.13)	(6.03)
Appointed Incumbent (-)	-0.40	-0.49	-1.56	-2.96
	(0.95)	(0.97)	(1.61)	(1.77)
Quality Challenger (-)	-7.72***	-7.67***	-7.77***	-7.86***
	(1.96)	(1.96)	(1.43)	(1.62)
Number of Candidates (-)	-19.59***	-18.56***	-20.27***	-19.41***
	(2.69)	(2.53)	(2.89)	(3.18)
Major Party (+)	4.40*	1.38	3.24	0.02
	(2.23)	(2.40)	(2.20)	(2.55)
Nonpartisan (-)	2.55	3.73	-2.90	-3.05
	(2.75)	(2.75)	(2.07)	(2.41)
Partisan (-)	7.33*	7.87**		
	(3.00)	(2.98)		
District (+)	4.35	3.36	-1.54	-0.64
	(3.97)	(3.49)	(2.44)	(2.88)
Female (+)	0.75	0.65	1.04	0.72
	(0.59)	(0.73)	(1.68)	(1.84)
Female Challenger (-)	-4.61**	-5.35***	-5.35**	-5.43**
	(1.53)	(0.97)	(1.89)	(2.05)
Minority (-)	-0.70	-1.00	-0.20	-0.46
	(1.11)	(1.56)	(1.97)	(2.23)

Minority Challenger (+)	6.83**	7.55***	7.41*	7.16
	(2.35)	(0.96)	(3.25)	(3.84)
Natl Murder Rate (-)	-0.69*	-0.80*	-0.17	-0.07
	(0.29)	(0.33)	(0.56)	(0.61)
State Murder Rate (-)	-0.87**	-1.02***	-0.88*	-1.05**
	(0.32)	(0.25)	(0.34)	(0.38)
Difference in Spending (+)			0.004**	0.003*
			(0.001)	(0.002)
Pres. Approval Inc. Coded (+)	-1.38		1.48	
	(2.55)		(5.57)	
Pres. Approval In-Party Coded (+)	4.11**		4.12**	
	(1.26)		(1.42)	
Gov. Approval Inc. Coded (+)		0.03		0.03
		(0.04)		(0.06)
Gov. Approval In-Party Coded (+)		0.0004		-0.004
		(0.02)		(0.02)
N	479	380	191	161
Adjusted R^2	0.60	0.61	0.47	0.48

*p < .05; **p < .01; ***p < .001

Before moving on, it is also worth thinking about the implication of the effects of some of the other variables in the model. Although these variables were included primarily as control variables, they may also partially inform the central question of this chapter about whether or not judicial elections increase democratic accountability. The state murder rate is significantly negative across most model specifications. Hall (2001 (a)) argues this provides evidence of democratic accountability in judicial elections, and it does, at least to the extent one believes judges can deter future crime by sentencing criminals harshly. The national murder rate, however, is also significant in many models. This detracts from the argument for democratic accountability, because judges have little power to control events happening beyond their state borders.

I also find strong effects of the race and sex of challengers. Incumbents facing a female challenger perform worse, whereas facing a minority challenger can lead to a higher vote share for the incumbent. Although the effects of the incumbents' race and sex themselves are not estimated to be significant, which may be due to the fact that these candidates are already incumbents and have all of the advantages that come along with that, the fact that the race or sex of the challenger matters, shows that voters are considering these factors when casting their ballots in judicial elections. These factors are normatively undesirable for voters to consider, because candidates' race or sex should be unrelated to their fitness for office.

The other control variables in the model largely performed as expected. The estimated effect of being an appointed incumbent is for the part consistently negative, although it is only significant in some of the different model specifications. Not surprisingly, the quality of the challenger matter as well. Incumbents who face quality challengers perform worse than incumbents facing more inexperienced challengers. Incumbents also perform worse as the number of candidates in the race increases. For the most part, incumbents who run on the ticket of a major party or those who run in district-wide elections, do better than minor party candidates or candidates who run in statewide elections. Election type also matters, however, these variables are signed in the wrong direction. This may be a result of controlling for the logged number of candidates in the race. When the number of candidates is not logged, these variables are properly signed (see appendix). When included, the spending variable is estimated to have a positive effect, which indicates that, as expected, incumbents with a spending advantage over their challengers do perform better.

These results provide suggestive evidence that voters are influenced by the economy and the popularity of the president when voting in judicial elections. For example, the results suggest

that incumbent judges may be rewarded when state GDP is high. Additionally, I found some evidence that in-party incumbents are also judged based on the national unemployment rate and the popularity of the current president. These results, combined with the findings that the national murder rate and the race or sex of an incumbent's challenger all may matter, suggests that voters are, at least partially, influenced by factors that should be normatively unrelated to judicial elections.

Discussion and Conclusions

The results of this chapter suggest several interesting conclusions. Firstly, it appears that there are relationships between the performance of incumbent judicial candidates and some facets of the economy. Though not all of the economic variables I hypothesized would affect judicial elections had the expected effects, it does appear like there might be a relationship between state GDP, national unemployment, and incumbent performance. Thus voters may consider the performance of the economy in some ways when voting in judicial elections. There is also a strong relationship between presidential approval and the performance of in-party incumbents. As neither economic performance nor the popularity of the executive should be relevant to judicial elections, the findings that these factors can matter in judicial elections casts doubt on the argument that judicial elections increase democratic accountability.

I also found that that there was a relationship between incumbent vote share and the national murder rate. As the relationship between incumbent vote share and the state murder rate is often cited as evidence that there is democratic accountability in judicial elections (Hall 2001(a)), the evidence of a relationship with the national murder rate, which incumbents have even less power to control, casts further doubts on the argument that judicial elections increase

democratic accountability in any meaningful way. Similarly, the significant effects of a challenger's race and sex also point to the conclusion that voters may not be considering the most relevant factors when making choices in these elections.

These results provide some empirical evidence that voters in judicial elections are responding to certain factors that are not theoretically relevant. Future work on this topic could determine whether the weak relationships found in this chapter are affected by the type of election used, the kind of campaign regulations enforced, or the amount of information included on the ballot. In particular, whether or not the incumbent is identified on the ballot may be important, because with the low levels of information that are typical of judicial elections, it is possible that voters do not even know this much. Answering these questions would probably require a larger dataset of judicial elections. Extending one's study to include elections to lower courts or even primary elections as well, could also be a fruitful way to learn more about how these relationships work. Another interesting next step for further research on this question, might involve actually talking to the people who vote in these elections and measuring their knowledge of the race or hearing directly from the voters what things they are thinking about when they cast their ballots on Election Day.

Chapter 3- Improper Influences on Judicial Decision Making

In studies of the impact of selection method on judicial decision making scholars have documented, in various ways, the influence of public opinion and constituency pressures on judges that are chosen in competitive elections.[12] Many authors have interpreted these findings in a positive light, arguing that the switch to judicial elections in many states has successfully increased the representativeness of those courts (Hall 1992, 1995; Caldarone, Canes-Wrone, and Park 2009; Canes-Wrone, Clark and Park 2010; Cann and Wilhelm 2011). The full implications of these findings, however, have not yet been fully drawn out. For example, scholars have thus far failed to consider what factors judges must overlook in order to incorporate public opinion into their decision making calculus. It is likely that judges will have to overlook some factors, which may include the law and case facts, if they are going to give increased weight to other factors, such as public opinion. This is because judges are human beings, and human beings possess a limited cognitive capacity (Simon 1985). To put it simply, judges, like all humans, face cognitive limitations when making decisions (Simon 1985). Consequently, it is unreasonable to expect that judges will be influenced by all of the potentially relevant factors when voting on cases. Because judges who are appointed do not need to consider the preferences of their constituency in order to retain their seat on the bench, the need to weigh the importance of the case facts and the law against public opinion is a problem unique to elected judges. Assuming that judges are motivated by their desire to remain on the bench (Pozen 2008; Cann and Wilhelm 2011), it is expected that elected judges will systematically favor constituency concerns over

[12] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013, Canes-Wrone, Clark, and Kelly 2014.

legal matters when making case decisions, because neglecting the wishes of one's constituency can directly threaten the judge's reelection prospects (Bright and Kennan 1995; Bright 2000), whereas, ignoring case facts can lead to, at most, a reversal by a higher court. The findings of this chapter, however, mostly fail to support this hypothesis. Nevertheless, they do suggest two alternative hypotheses that should be evaluated by further testing.

Judges Can Systematically Be Influenced by the Facts of the Case

The debate about the extent to which judges and justices in the United States are influenced by the law versus their own personal political attitudes has a long history in political science. The idea that judges would be influenced by the law, and the law alone, was for a while the predominate view of the field (Segal and Spaeth 2002, 48). This impression, however, relied more on normative arguments, i.e., judges would be influenced by the law because judges believed they were supposed to make decisions based solely on the law (Kahn 1999), than on any empirical findings. Once scholars found ways to empirically evaluate these propositions, however, their findings began to raise serious doubts about how much judges' decisions were actually affected by the law and legal factors (Segal and Speath 1996, 2002; Segal and Howard 2002). This was partially due to the inability of scholars to demonstrate a systematic effect of the law on the justices' decisions (Segal and Speath 1996, 2002; Segal and Howard 2002), and also a result of the findings by Segal and Spaeth (2002) that the decisions of the justices, at least those on the Supreme Court, could be predicted very accurately by the justices' ideology. These findings, as well as similar others, revolutionized the field by challenging the accepted view, and ultimately, led to a more nuanced understanding of judicial decision making, which sees both the

law and the personal political opinions of the judges as being relevant to their decision making (Segal and Speath 2002).

Elected Judges Are Also Influenced by Public Opinion and Constituency Concerns

State court judges who have to be reelected in order to retain their seat on the bench also have to consider public opinion in addition to the law and their personal opinions. This is an especially important consideration, because if elected judges decide too many cases in a way that upsets the public they may be putting the continuation of their legal career in jeopardy (Bright and Kennan 1995; Bright 2000; Baum 2003). Such was the fate of the three Supreme Court judges in California who were removed by the voters in response to their decisions to overturn a series of death sentences (Bright and Kennan 1995). Furthermore, as judicial campaigns are progressively becoming more expensive and visible races, the importance of any one or series of "wrong" decisions is likely to increase (Baum 2003). This is true even with respect to decisions in low-salience cases, because the media are likely to inform people of any and all unpopular decisions made by the court (Windett, Hall, and Harden 2013). This has led some judges to be extra attentive to any media coverage of their decisions (Drechsel 1987). Additionally, the greater tendency of judicial candidates to use negative attack advertising in their campaigns (Iyengar 2002; Hall and Bonneau 2013) also heightens the pressure for judges to avoid casting unpopular votes that could potentially become the subject of these types of ads. Thus, if anything, the pressure for elected judges to consider public opinion when casting votes in cases is probably rapidly increasing. Recently, scholars have begun to document the myriad of ways in which these pressures directly affect the decision making of elected judges.[13]

[13] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and

One line of research has looked at how electoral concerns and constituency pressures may impact a judge's probability of issuing a dissenting opinion (Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Boyea 2007, 2010; Vining Jr. and Wilhelm 2011). Canon and Jaros (1970) argue that elected judges have a greater need to write dissenting opinions, because dissenting can be a way for a judge to appease a large and diverse constituency. Looking at a sample of cases from 1961 to 1970, they do find that judges who are selected in partisan elections dissent more frequently than do judges who are selected by appointment or merit plan systems (Canon and Jaros 1970). Dissent by elected judges decreases, however, as the public becomes more ideologically extreme (Boyea 2007). Similarly, Brace and Hall (1993) find that as the level of partisan competition in the state increases, elected judges, but not appointed judges, are more likely to dissent in death penalty cases. This provides further support for the theory that elected judges may have a greater need to dissent as their constituency gets more diverse (Brace and Hall 1993).

Different types of cases, however, may alter the relationship between selection method and the tendency to dissent. Dissent by elected judges for example, has been found to be more common in salient cases (Vining Jr. and Wilhelm 2011), but may also be less likely in salient cases if the dissenting position is considered to be unpopular with the public (Hall 1987, 1992). This is likely because when judges dissent they are unable to justify their decisions to the electorate solely on legal grounds, as a result of taking the minority position on the court (Hall 1992). In interviews with elected Louisiana Supreme Court judges, Hall (1987) finds that at least one judge admitted to suppressing dissents in death penalty cases to avoid upsetting the public. In a more systematic study, Brace and Hall (1993) find that this behavior might be fairly

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Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013; Canes-Wrone, Clark, and Kelly 2014.

common. They find that elected judges are less likely to issue a liberal dissent in death penalty cases when there are aggravating factors present. This is likely a result of the judges' fears that a liberal dissent in these types of cases could be interpreted by the electorate as a failure to be tough on crime (Brace and Hall 1993). Furthermore, the more electorally vulnerable the judges are the more likely they are to forego dissents and side with the majority in these types of cases (Hall 1992). Additionally, Boyea (2010) finds that in torts cases, elected judges are actually less likely to dissent when compared to judges who are appointed. This is true even for elected judges who have accumulated a great deal of seniority, which is a factor that usually increases one's willingness to dissent (Boyea 2010).

Constituency pressures have also been shown to directly affect the content of the decisions issued by the court.[14] This is particularly true in highly salient cases, such as death penalty cases (Hall 1995; Brace and Hall 1995, 1997; Brace and Boyea 2008; Canes-Wrone, Clark, and Kelly 2014). The direction of constituency pressure is almost uniformly in favor of upholding these sentences, because voters are often more concerned with punishing criminals then with providing adequate safeguards for their constitutional rights (Bright and Kennan 1995; Howard, Graves, and Flowers 2006). An example of judges reacting to this type of pressure can be seen in California, where, in response to the removal of the three judges for overturning death sentences too frequently, the California state supreme court subsequently began to uphold death sentences approximately 97 percent of the time (Bright and Kennan 1995). Additionally, there is some evidence that individual California judges, who faced a serious campaign against them on the basis of their liberal death penalty decisions but managed to hold onto their seats, did

[14] Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013; Canes-Wrone, Clark, and Kelly 2014.

consequently, issue fewer liberal death penalty decisions in their subsequent terms (Traut and Emmert 1998). More generally, Brace and Boyea (2008) find that the probability of a judge voting to uphold a death sentence is influenced by public support for the death penalty. The tendency of elected judges to uphold death sentences in response to electoral considerations does vary as a result of institutional or personal factors (Hall 1995; Brace and Hall 1995). Hall (1995), for instance, finds that judges who are more electorally vulnerable, i.e., judges that were elected in a competitive election or who are in the last two years of their term, were significantly less likely to overturn death penalty sentences than were more electorally secure judges.

The finding that judges react to public opinion in death penalty cases has also been shown to apply more broadly to all types of criminal cases, even those of lesser salience (Cauthen and Peters 2003; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007). For example, in more conservative states, courts with elected judges have been found to issue less liberal pro-defendant decisions in all types of criminal cases (Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Bright 2000). This can occur even in the most egregious of cases where elected judges refused to find for the defendant in an ineffective consul case, even after evidence had been provided to show that the defendant's lawyer routinely fell asleep during the proceedings (Bright 2000). The electoral concerns of the judges and their desire to secure convictions can permeate every aspect of the case, not just the final decisions (Gibson 1980; Bright and Kennan 1995; Bright 2000; Huber and Gordon 2004; Nelson 2014). Elected judges, for instance, may be both less likely to grant a change of venue request (Bright and Kennan 1995) and more likely to appoint poor quality lawyers to represent alleged criminals, because either granting the request or appointing a more qualified lawyer could reduce the chances of a conviction (Bright and Kennan 1995; Bright 2000). Likewise, elected judges are more likely to

ground their opinions in federal, rather than state, law, because state law can be more, but not less, protective of criminal rights (Nelson 2014). Sentencing behavior in criminal cases is also affected by constituency concerns (Gibson 1980; Huber and Gordon 2004, Gordon and Huber 2007). This is likely because the electorate will react poorly to any perceived under punishment of criminals (Huber and Gordon 2004). Consequently, judges have been shown to, on average, hand down longer jail sentences for criminals when crime is perceived to be a more important problem by the electorate (Gibson 1980) and as the time until the judge's next election decreases (Gordon and Huber 2004). This effect is heightened for judges selected in partisan elections compared to judges selected in retention elections (Huber and Gordon 2007).

Well perhaps most notable in criminal and death penalty cases, the influence of constituency pressure on decision making can be seen in other types of cases as well, including abortion cases (Brace, Hall, and Langer 2001; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010).The overall liberalism of the state for example, affects whether or not state supreme courts decide to hear cases challenging abortion restrictions, and this is truer of elected courts than of appointed courts (Brace, Hall, and Langer 2001). Additionally, Canes-Wrone, Clark, and Park (2010) find that state-level public support for abortion significantly affects a judge's probability of voting to uphold abortion restrictions.

Furthermore, several scholars have demonstrated an effect of the general partisanship and ideology of the state on the overall tendency of the court to issue liberal or conservative decisions in many types of cases (Hoekstra 2005; Shepherd 2009; Cann and Wilhelm 2011, Windett, Hall, and Harden 2013). Taking a broad approach, Windett, Hall, and Harden (2013) demonstrate that changes in public mood are reflected by the changing ideal points of elected state court judges. This phenomenon can also be observed with respect to specific subsets of

cases as well. Hoekstra (2005) for example, concludes that elected judges make more liberal decisions in wage and hour legislation cases as their constituency gets more progressive. Additionally, Shepherd (2009) examines many types of different cases, and finds that when the electorate becomes more Republican, a judge is more likely to take the traditional Republican position in cases. For example, elected judges in an increasingly Republican state are more likely to decide cases in favor of businesses, employers, doctors, and the state over individuals, employees, patients, and criminals respectively (Shepherd 2009). LeRoy (2010) extends the findings with regard to employer-employee cases to elected judges more broadly, and finds that employers win significantly more often whenever judges are elected compared to appointed. He attributes this finding to the history of employers contributing to judges' reelection campaigns (LeRoy 2010).

Thus in addition to having to consider public opinion directly, judges who must stand for reelection may also have to give special consideration to the opinions of their campaign contributors (Rottman and Schotland 2001; Cann 2007; Williams and Ditslear 2007; Pozen 2008). This is likely to be even more important recently, as the average cost of a judicial campaign has risen significantly (Bonneau 2004), and spending by challengers and incumbents has been showed to have some effect on the final election results (Bonneau 2007; Bonneau and Cann 2011). There is somewhat mixed evidence, however, with regard to the extent to which scholars have been able to empirically demonstrate an effect of campaign contributions on judicial decision making (Williams and Ditslear 2007). Williams and Ditslear (2007) for instance, find that only a small percentage of judges are systematically influenced by donations. Their study, however, was conducted in Wisconsin, which is a state that provides partial public funding of judicial elections (Williams and Ditslear 2007). Thus their results may not be

generalizable to other states where the impetus is on the judges to fully fund their own campaigns (Williams and Ditslear 2007). Cann (2007), who studies the effect of campaign contributions on judges in Georgia, finds that campaign contributions do have significant effects on judicial decision making. Specifically, he finds that the probability of a judge making a liberal decision is affected by whether or not liberal groups contributed more money to the judge's reelection campaign than did conservative groups (Cann 2007).

In in a variety of ways scholars have amply demonstrated that elected judges are systematically influenced by constituency pressures and electoral concerns when making case decisions.[15] What scholars have overlooked so far, however, is how this affects a judge's ability to consult other relevant factors, like case facts and the law, when making decisions. It is likely that a judge will not be able to fully consider both the law and public opinion, because research in political psychology has shown that the attention span of humans is quite limited (Simon 1985). Most humans for instance, only possess the capacity to store approximately six pieces of information in their short term memory at a time (Simon 1985). Consequently, decision makers are often unable to seriously consider every important component of a decision (Simon 1985). For elected judges, their desire to be reelected (Pozen 2008; Cann and Wilhelm 2011) is probably paramount to their desire to be appropriately influenced by the law. Thus it is likely that they will consider public opinion more seriously than the facts of the case when making decisions. Appointed judges, on the other hand, are usually more insulated from public opinion and may experience less turnover than elected judges (Dudley 1997). Thus appointed judges should be freer than elected judges to be swayed by the facts of the case. This leads to the main

[15] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013; Canes-Wrone, Clark, and Kelly 2014.

hypothesis of this chapter, which is that elected judges will be systematically less likely to be influenced by the facts of the case than will appointed judges.

Data and Methods 1

In order to test the hypothesis that the decision making of elected judges is affected less by the facts of the case than are the decisions made by appointed judges, it is necessary first to obtain a sample of case decisions from state supreme courts on the same topic so that the facts of the case can be properly coded and accounted for. Death penalty cases are well-suited to a study that involves a fact pattern analysis, because several studies have established that the votes of state supreme court judges are affected by several case facts concerning the type of crime committed and the characteristics of the victim (Brace and Hall 1997; Canes-Wrone, Clark, and Kelly 2014). Death penalty cases are also a particularly good case type to study, because death penalty cases are highly salient cases on which public opinion has been demonstrated to affect the voting behavior of the judges (Hall 1995; Brace and Hall 1995, 1997; Brace and Boyea 2008; Canes-Wrone, Clark, and Kelly 2014).

Canes-Wrone, Clark, and Kelly (2014) have previously assembled a dataset of death penalty decisions decided by state courts of last resort between 1980 and 2006. It is their data that I use in this chapter.[16] In brief the dataset, which is described in detail in Canes-Wrone, Clark, and Kelly (2014), includes 12,777 judges' votes in 2078 death penalty cases from 26 different states.[17] For states with less than 100 death penalty cases heard during that time period,

[16] The author would like to sincerely thank Tom Clark for generously sharing the data with her.

[17] Some states that have election rules that are not comparable to at least two other states are excluded, as are states that use district elections. For more details see Canes-Wrone, Clark, and Kelly 2014.

all of the cases are included in the dataset. For states with 100 or more cases, 100 were randomly selected to be included in the dataset.[18]

My dependent variable for this analysis is the vote of the individual state supreme court judge in each case. This is a binary variable, which is coded 0 if the judge votes to uphold the death sentence and 1 if the judge votes to intervene. The judges' votes in these cases are available in the Canes-Wrone, Clark, and Kelly (2014) data. Of the total votes in the dataset, judges vote to intervene approximately 27 percent of the time.

My main hypothesis concerns the extent to which elected versus appointed judges are influenced by the facts of the case. Therefore, it is necessary to include the facts of the case in the model. I use all of the case facts variables that are available for the full sample of cases in the Canes-Wrone, Clark, and Kelly (2014) dataset.[19] This includes variables that measure the characteristics of the victim, as well as the presence of any aggravating circumstances. Aggravating circumstances involve crimes that were committed in addition to the murder, such as rape or robbery. The presence of either rape or robbery is measured by dichotomous variables that are coded 1 if that crime was committed by the defendant in the case and 0 otherwise. In total rape occurred in about 24 percent of all cases and robbery in 44 percent. The presence of either of these aggravating factors should make it less likely that a judge will vote to overturn the death sentence of the defendant (Brace and Hall 1997; Canes-Wrone, Clark, and Kelly 2014), thus I expect a negative sign on these variables.

The second set of case fact variables included in the Canes-Wrone, Clark, and Kelly (2014) dataset concerns the characteristics of the victim(s). It has been previously shown that

[18] If a state changed its method of judicial selection during the years studied, then up to 100 cases were allowed before and after the switch. For more details, see Canes-Wrone, Clark, and Kelly 2014.

[19] Variables for the race of the defendant and the race of the victim are only available for a subset of the dataset and I therefore I do not include these variables in any of my models.

judges are more reluctant to overturn death sentences for people who are convicted of killing females or police officers (Brace and Hall 1997; Canes-Wrone, Clark, and Kelly 2014). The variables female and cop killed take on the value of 1 if a female or cop respectively was a victim in the case and 0 otherwise. In total females were the victim in 58 percent of the cases in the dataset and cops in 5. The sign on both of these variables is also expected to be negative, because judges should be more reluctant to overturn sentences for criminals who select these types of victims. Another important victim characteristic is the number of victims. When there are multiple victims, the crime may be seen as even more heinous, and the imposition of the death penalty may be more likely Canes-Wrone, Clark, and Kelly 2014). The variable multiple victims measures whether or not there is more than one victim in the case. It is coded 1 for cases with multiple victims and 0 for cases with a single victim. 33 percent of cases in the dataset involved multiple victims. I expect the sign on this variable to be negative. Judges should be less likely to overturn death sentences when there are multiple victims involved.

Because my goal is to compare how judges reselected by different methods respond to the cases facts, I also need an indicator for how judges are reselected. For this I use Canes-Wrone, Clark, and Kelly's (2014) classification system, which codes judicial selection systems into four separate categories: judges who are reappointed, either by the execute, legislature, or some combination thereof, judges who are retained by retention elections, judges who are retained by nonpartisan elections, and judges who are retained in partisan elections.[20] Table 3.1 contains a summary of the variables to be used in the analysis.

[20] Judicial selection methods were coded by Canes-Wrone, Clark, and Kelly (2014) based on the current and historical data on judicial selection in in the states maintained by the American Judicature Society.

Table 3.1 Summary of Variables

Statistic	N	No	%	Yes	%
Vote	12,819	0	73%	1	27%
Appointed	13,269	0	90%	1	10%
Partisan	13,269	0	69%	1	31%
Nonpartisan	13,269	0	75%	1	25%
Retention	13,269	0	66%	1	34%
Cop Killed	13,269	0	95%	1	5%
Rape	13,269	0	76%	1	24%
Rob	13,269	0	56%	1	44%
Multiple Victims	13,269	0	67%	1	33%
Female Victims	13,269	0	42%	1	58%

Analysis 1

One way to ascertain how much different types of judges are affected by the case facts is to model judges' votes using all of the previously described case facts variables separately for judges reselected in each of the four main types of reselection methods: appointment, partisan, nonpartisan, and retention elections. Then I can compare the performance of the different models. If appointed judges are, as predicted, the most responsive to the case facts, then I would expect the model for appointed judges to correctly predict the outcome better than the models for the other types of judges. My dependent variable, judicial votes, is dichotomous. I, therefore, use logit models in the analysis that follows. The four different models are presented in Table 3.2. I am again dealing with clustered data, with death penalty cases clustered in the states in which they were heard. Because there are many factors within a state that can be expected to be same across cases, i.e., the judges who hear the case, the laws governing the use of the death penalty, etc., it is unreasonable to assume that the errors within a state are uncorrelated. To correct for this problem, I present standard errors that are clustered by state.

Table 3.2 Case Facts Logit Results with Clustered Standard Errors

	Vote			
	(1)	(2)	(3)	(4)
	Appointed	Partisan	Nonpartisan	Retention
Constant	-0.67	-0.53**	-0.97***	-0.53*
	(0.64)	(0.20)	(0.19)	(0.27)
Killed a Cop (-)	-0.51	-0.87***	-0.32	-0.19
	(0.30)	(0.22)	(0.43)	(0.19)
Rape (-)	-0.48***	-0.42**	-0.20	0.01
	(0.13)	(0.14)	(0.13)	(0.25)
Robbery (-)	-0.26	-0.38**	-0.19	-0.25
	(0.33)	(0.12)	(0.22)	(0.16)
Multiple Victims (-)	-0.01	-0.60***	-0.15	-0.09
	(0.11)	(0.14)	(0.11)	(0.14)
Female Victim (-)	-0.02	-0.15	0.07	-0.25
	(0.13)	(0.18)	(0.17)	(0.20)
Percent Modal Category	0.728	0.742	0.752	0.694
Expected % Correctly Predicted Null Model	0.604	0.617	0.627	0.575
Percent Correctly Predicted	0.728	0.742	0.752	0.694
Expected % Correctly Predicted	0.608	0.626	0.628	0.577
Expected Proportionate Reduction in Error	0.009	0.025	0.003	0.006
N	1,280	4,028	3,203	4,266

*p < .05; **p < .01; ***p < .001

Before discussing model fit, it is worth briefly considering the effects of the case facts in the models. Recall that the prediction was for each of the case facts to have a negative effect. With only two exceptions this prediction is borne out by the data. Most of the case facts variables are estimated to have a negative effect across all four models. Looking more closely at the results, however, it is clear that many of these variables failed to attain statistical significance. In models 3 and 4, for nonpartisan and retention judges respectively, not a single case fact variable is significant. In model 1, for appointed judges, the only significant effect is the negative coefficient on rape, which suggests that appointed judges are less likely to overturn death

sentences when the criminal also committed rape in addition to murder. The story of partisan elections, in Model 2, however, is much different. In partisan elections all of the case fact variables, except for the female victim variable, are statistically significant. These results suggest that the impact of the case facts is the largest among judges retained by partisan elections followed by judges who are appointed.

A better indicator of how well the case facts explain judicial decision-making, however, is to look at how well each of the models does at predicting judges' votes. A typical measure for this is to compare the percent of cases correctly predicted by the model. This is usually done by assuming that any predicted value of .5 or above predicts an outcome of 1 and any predicted value of below .5 predicts an outcome of 0 (Herron 1999). Herron (1999), however, argues that this method is flawed, because it fails to account for the extra uncertainty involved in assuming that a predicted value of 0.56 predicts an outcome of a 1. Instead Herron (1999) recommends researchers report the Expected Percent Correctly Predicted (EPCP), which takes this uncertainty into account. It may also be useful to compare the EPCP of the fitted model to the performance of the null model (Herron 1999). All of these summary statistics for the four models are presented at the bottom of Table 3.2. For each of the models the EPCP is only slightly lower than the Percent Modal Category (PMC). This is a signal that knowledge of the case facts might not contribute to better predictions of judicial votes. Clearly a lot of other factors are involved here as well. The model for partisan elections, however, has the largest Expected Proportionate Reduction in Error statistic (EPRE). This suggests that including the case facts contributes the most in the model for partisan elections. The model for appointed judges has the next largest EPRE. Together these results imply that the impact of the case facts may be the largest among judges reselected in partisan elections, with the second largest impact for judges who are

appointed and reappointed to the bench. Confidence intervals around the EPRE, obtained through parametric bootstrapping, however, overlap for all of these models. Thus these results should be interpreted cautiously.

That the results of this analysis found judges retained by partisan elections may actually be affected the most by the case facts is puzzling. Upon further reflection, however, two different explanations may serve to explain this unexpected finding. The first possibility is that perhaps death penalty cases are a unique subset of cases where public opinion may actually reinforce the importance of the case facts. This is conceivable because, similarly to judges, the public is also likely to be particularly outraged when aggravating factors are present. Thus judges have more reason to fear public backlash if they vote to commute a sentence in a case where aggravating factors are present. This self-reinforcing relationship between public opinion and the case facts may not exist in other cases, such as in search and seizure cases, where a layperson is unlikely to appreciate the difference between searches occurring in a car versus a home, etc.

The other potential explanation for why the expected effects were not found is that I have made the incorrect comparisons. Canes-Wrone, Clark, and Kelly (2014) find support for the argument that judges selected in nonpartisan or retention elections may actually have the strongest incentives to consider public opinion. They theorize that is because, without a party label on the ballot, voters have less information, and are more likely to make decisions that are based solely on the judge's record. This implies that the party labels in partisan elections actually protect judges from public scrutiny of their record by giving voters an easier cue on which to base their votes (Canes-Wrone, Clark, and Kelly 2014). The logical inference from this idea is that judges reselected in partisan elections may actually be more comparable to judges reselected in appointment systems, at least in terms of the pressure they feel to directly consider public

opinion (Canes-Wrone, Clark, and Kelly 2014). If the difference in the willingness to consider the facts of the case is posited to result from the pressure on judges to consider public opinion, then the expected differences may be more fairly hypothesized to exist between judges facing less pressure, such as judges who are appointed or reelected in high-information partisan elections, and judges facing a lot of pressure, such those who are reselected in low-information nonpartisan or retention elections. The results of the above analysis, which finds the smallest impact of the case facts on judges reselected in nonpartisan or retention elections, may provide some support for this alternative explanation.

Data and Methods 2

The previous analysis compared the predictive power of models that used the facts of the case to predict judicial votes separately for judges reselected by each of the four main reselection methods. This analysis found suggestive evidence that the case facts model performed the best among judges who are reselected in partisan elections followed by judges who are appointed. This analysis, however, considered only the impact of the case facts and did not control for any other factors, which we would also expect to affect judicial decision making. The low EPRE's of the models clearly suggest that other factors are important as well. Therefore, another way to evaluate this hypothesis, is by considering how the impact of the case facts varies by reselection method in full models that also incorporate controls for other factors that are hypothesized to affect judicial votes in death penalty cases.

Specifically, to test my main hypothesis that the influence of the case facts will vary by reselection method, I rerun the previous models but add in controls for other factors that previous research suggests should affect judicial decision making. To make the comparison about the how

the impact of case facts varies between different reselection methods more clear, however, I create a new variable that measures the total number of case facts present, rather than including each individual fact by itself. This measure ranges from 0 to 5. Judges should be less likely to overturn death sentences in cases where more of these factors are present. Thus I expect a negative sign on this variable. My hypothesis, that appointed judges will be the most reactive to the case facts, will be supported if the sum-of-case-facts variable has the largest effect in the model for appointed judges. An alternative way of testing this hypothesis would be to run a model on the combined dataset and include interactions between the sum-of-case-facts variable and the different indicators of judicial reselection. I choose to present separate models instead, because the interpretation is clearer, but present an interactive model in the appendix that comes to largely similar conclusions.

In these models, I introduce controls for other variables that may also affect judicial decision making. This includes many attributes of the judges themselves, which could increase their willingness to cast liberal or conservative votes in death penalty cases. Specifically, these factors include the judge's party affiliation, proximity to reelection, and ability to seek another term (Canes-Wrone, Clark, and Kelly 2014). The Canes-Wrone, Clark, and Kelly (2014) dataset contains several variables that allow for these factors to be controlled for in the model. The variable, party, is coded 1 for Democrat judges and 0 otherwise. Judge's party affiliations were determined by consulting election results, judicial biographies, and newspapers. For some judges, for whom no party affiliation could be determined, the party of the appointing governor was used in place.[21] About 71 percent of the votes in the dataset were cast by Democratic judges. Because Democratic judges are assumed to be less supportive of the death penalty than Republicans (Canes-Wrone, Clark, and Kelly 2014), I expect a positive sign on this variable.

[21] For more details, see Canes-Wrone, Clark, and Kelly 2014.

Another important judicial characteristic is a judge's closeness to and ability to seek reelection. A judge's proximity to reelection or reappointment is measured by the variable, electoral proximity, that is coded 1 if the judge is within two years or less from their next reselection and 0 otherwise. In total about 44 percent of the votes in the dataset are cast by judges who will face their next reselection within two years or less. I expect the sign on this variable to be negative, as judges have been shown to be less willing to overturn death sentences when their next election is closer (Hall, 1995; Canes-Wrone, Clark, and Kelly 2014). Related to the issue of electoral proximity, is the question of whether the judge can seek reelection at all. Judges who cannot seek reelection may act differently, because they no longer have to answer to the electorate (Hall 2014; Canes-Wrone, Clark, and Kelly 2014). To control for this, the Canes-Wrone, Clark, and Kelly (2014) dataset contains the variables retire and lame duck, which measure whether or not the judge is facing a mandatory retirement and whether or not they have lost their election or are not seeking reelection for other reasons respectively. In the Canes-Wrone, Clark, and Kelly (2014) dataset these variables are also coded to account for party effects. Therefore, Republican judges with lame duck status or who are facing mandatory retirement are coded 1, while Democrats in these same circumstances are coded -1. All judges who are either not facing a mandatory retirement or who are not lame ducks receive a code of 0 on these variables. Rather than constraining these effects to be asymmetric for the parties as Canes-Wrone, Clark, and Kelly (2014) did, however, I instead transform these variables, creating separate dummy variables for Republican and Democratic judges who are lame ducks or who are retiring.[22] I expect a positive sign on the Democratic lame duck and retirement variables and a negative sign on the corresponding Republican variables. These assumptions are based on the

[22] This variable is omitted from the model for appointed judges and the model for judges reselected in retention elections, because these none of these judges are lame ducks in those datasets.

idea that Democrats, absent electoral pressure, will be more likely to vote to overturn death sentences, whereas, Republicans in similar circumstances will be more likely to uphold (Canes-Wrone, Clark, and Kelly (2014).

I include two other control variables in the model. These are controls for public opinion and Supreme Court precedent. It is important to control directly for public opinion, because numerous scholars have demonstrated that public opinion directly affects the decision making of elected judges.[23] To control for differences in state-level support for the death penalty, I use the variable, public opinion, from the Canes-Wrone, Clark, and Kelly (2014) dataset. To create this variable Canes-Wrone, Clark, and Kelly (2014) used the method of MRP to produce state-level estimates of support for the death penalty from national polls.[24] The state-specific estimates in the dataset range from a low of 55 to a high of 96, with a mean of 77. The expected sign on this variable is negative, because judges should be less likely to overturn death sentences as public support for the death penalty increases (Hall 1995; Brace and Hall 1995, 1997; Brace and Boyea 2008; Canes-Wrone, Clark, and Kelly 2014). The last variable, Supreme Court precedent, is a dummy variable from the Canes-Wrone, Clark, and Kelly (2014) dataset. It is coded 1 for cases that are based directly on recent Supreme Court cases that declared some aspect of the death penalty unconstitutional. This occurred in only about 2 percent of cases in the dataset.[25] The expected sign on this variable is positive, as judges are expected to be influenced by recent

[23] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

[24] For more details on how this variable was created see Canes-Wrone, Clark, and Kelly (2014).

[25] None of the cases in the dataset that were heard by appointed judges fit this definition so this variable is omitted from that model.

precedents. A summary of the new variables to be included in the full models that follow are displayed in Table 3.3.

Table 3.3 Summary of Variables

Statistic	**N**	**Mean**	**St. Dev.**	**Min**	**Max**
Sum Facts	13,269	1.641	0.962	0	5
Public Opinion	13,269	0.767	0.062	0.546	0.963
Statistic	**N**	**No**	**%**	**Yes**	**%**
Next Election	13,265	0	56%	1	44%
SCOTUS	13,269	0	98%	1	2%
Dem Retire	13,265	0	7%	1	93%
Rep Retire	13,265	0	4%	1	96%
Dem Lame Duck	13,265	0	1%	1	99%
Rep Lame Duck	13,265	0	0.3%	1	99.7%
Statistic	**N**	**Rep**	**%**	**Dem**	**%**
Party	12,939	0	29%	1	71%

Analysis 2

The dependent variable in these analyses continues to be the votes of the individual state supreme court judges. Because these votes are still clustered by state, I continue to present standard errors that are clustered by state in the analysis that follows. Again, however, in the appendix to this chapter, I also present multilevel model results that allows for state and year random intercepts for comparison purposes. The results of the multilevel model, like the results reported in Table 3.4, show that the effect of the case facts is negative across all models. The multilevel results, however, show that the effects of the case facts may matter the least for appointed judges.

Table 3.4 Case Facts with Controls Logit Results with Clustered Standard Errors

	Vote			
	(1) Appointed	(2) Partisan	(3) Nonpartisan	(4) Retention
Constant	1.99	1.70	-1.46	-0.36
	(4.07)	(1.74)	(1.70)	(1.56)
Sum Facts (-)	-0.13	-0.32***	-0.08	-0.13
	(0.06)	(0.03)	(0.08)	(0.09)
Next Election (-)	0.17	-0.25	-0.09	-0.04
	(0.18)	(0.19)	(0.09)	(0.13)
Dem Retire (+)	1.30	0.15	-1.35***	-0.24
	(0.84)	(0.24)	(0.24)	(0.15)
Rep Retire (-)	-0.48	-1.24***	-1.89***	-0.17
	(1.10)	(0.10)	(0.47)	(0.32)
Party (+)	0.33	0.14	0.26	0.97***
	(0.17)	(0.38)	(0.18)	(0.19)
Dem Lame Duck (+)		0.54	-0.51	
		(0.51)	(0.47)	
Rep Lame Duck (-)		0.001	-0.11	
		(0.41)	(0.59)	
SCOTUS (+)		1.31	17.80***	3.78***
		(1.31)	(0.91)	(0.37)
Public Opinion (-)	-4.62	-3.12	0.34	-1.10
	(6.24)	(2.33)	(1.99)	(1.95)

*p < .05; **p < .01; ***p < .001

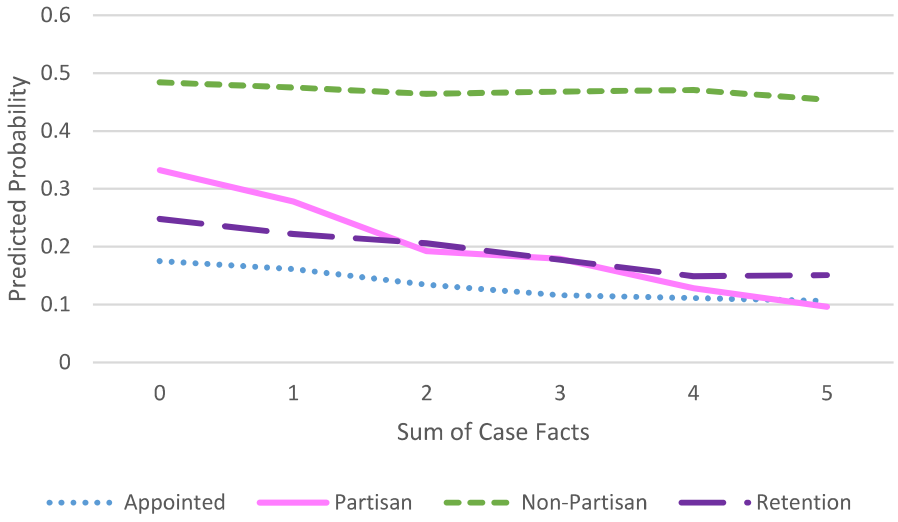
Turning to the OLS results with clustered standard errors, displayed in Table 3.4, we can see that across all four different models the sum-of-case-facts variable is always negative, as expected. This indicates that judges are consistently less likely to vote to overturn death sentences when more of the individual case facts are present. These results are in accordance with previous studies that have found that the presence of aggravating factors or specific victims in a case makes imposition of the death penalty more likely (Brace and Hall 1997; Canes-Wrone,

Clark, and Kelly 2014). The sum-of-case-facts variable is significant, however, only in the model for partisan elections. This indicates that, contrary my original expectations, the impact of the case facts may be the largest when judges are reselected in partisan elections.

To illustrate the magnitude of these effects, I use the Zelig package (Imai, King, and Lau 2007, 2008, 2016) to generate predicted probabilities for different permutations of the sum-of-case-facts variable in the models for the different reselection methods. Specifically, for each of the models in Table 3.4, I set all of the independent variables to 0, except for the public opinion variable, which is held at its overall mean[26], and then calculate the predicted probability of a judge voting to overturn a death sentence. I do this separately for each different reselection method model. These predicted probabilities are displayed in Figure 3.1. The probability of voting to overturn is the highest for nonpartisan elections and the lowest for appointed. The line for all of the models, however, is negative, which indicates that, regardless of reselection method, when the sum of case facts increases the probability of voting to overturn decreases. The impact clearly differs by reselection method, however. This can be seen with regards to the slope of the lines, which shows the impact of increasing the number of case facts present on the probability of voting to overturn. Looking at the Figure, it is clear that the steepest drop in the probability of voting to overturn death sentences as the number of case facts present increases, is seen in partisan elections. The next steepest drop appears to occur in retention elections followed by appointed and then nonpartisan elections, although these lines are much more flat overall. This confirms the earlier findings of this chapter that the case facts actually seem to matter the most when judges are reselected in partisan elections.

[26] For comparability purposes this is held at the mean of the combined data.

Figure 3.1 Predicted Probability of Voting to Overturn Death Sentences



Predicted probability of a judge voting to overturn generated with Zelig. All other independent variables set to 0, except for public opinion, which is set to the mean.

Turning to the control variables, we can see that most of these variables are correctly signed, although often fail to achieve statistical significance in one or more of the models. For example, there is some evidence that party matters. Democratic judges are consistently found to be more likely to overturn death sentences than are Republican judges across all four models. However, this effect is only significant when judges are reselected in retention elections.

Discussion and Conclusion

The motivation behind this chapter was to examine the potential consequences of the well-documented finding that the behavior of elected judges is influenced by public opinion.[27]

[27] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013, Canes-Wrone, Clark, and Kelly 2014.

Well many scholars have tended to emphasize the positive implications of this finding, such as the increased democratic responsiveness of the judges (Hall 1992, 1995; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark and Park 2010; Cann and Wilhelm 2011), other more negative potential implications have largely been left unexplored. Specifically, I argue that as a result of the strong incentives that exist to consider public opinion in election systems, these judges will be less willing to also consider the facts of the case than will appointed judges, who face no similar incentives to consider public opinion directly. I tested and failed to find support for this theory using an extensive dataset on death penalty decisions decided by state courts of last resort. The lack of support for my main hypothesis forced me to reconsider the premise upon which this chapter was based. Upon further reflection two possible explanations for these unexpected results presented themselves. One possible explanation centered on the argument that the pressure for judges to consider public opinion is stronger in retention and nonpartisan elections than partisan elections, because voters are more likely to make decisions based on a judge's voting record when party labels are missing from the ballot (Canes-Wrone, Clark, and Kelly 2014). I found some evidence to support this alternative explanation, by finding that the predictive power of the case facts model is higher in the partisan and appointed models than it is in models for judges reselected in nonpartisan or retention elections. The subsequent analysis, however, only found support for increased responsive among judges reselected in partisan elections.

The other possible explanation for the failure to find support for my original hypothesis, is the possibility that death penalty cases are a unique type of case where public opinion may actually reinforce the importance of the case facts. This is plausible, because if people wants to accuse a judge of being soft on crime, they can gain mileage by publicizing a decision of the

judge to overturn a death sentence in a case where aggravating factors were present. This type of relationship may not exist in all types of cases. In search and seizure cases for example, several important case facts relate to the place where the search occurred. It is hard to imagine the public caring about or differentiating between searches that took place in one's home compared to one's car. Thus it is possible that were I to repeat this analysis using search and seizure cases in place of death penalty cases the expected differences in the importance of the case facts between elected and appointed judges would emerge. Further research should test this theory on search and seizure cases, as well as other types of cases to see if the results found here generalize. This would provide important evidence to help understand the relationship between reselection method and responsiveness to the case facts.

Chapter Four: Do Elected Judges Favor Majorities at the Expense of Minorities

In the 1820's a wave of democratic sentiment swept through the United States that fundamentally altered the political landscape. Courts, long criticized as undemocratic for appointing judges, were a popular target of reformers. This resulted in judicial appointments being replaced by judicial elections in many states (Dubois 1980). While this undeniably made the courts more democratic, it is also raised the question of who would protect minority rights. In a democracy, tension always exists between the basic principle of majority rule and minority rights. If majorities always rule, then danger lies in the risk that they might become overbearing and usurp the rights of minorities (Madison 1788 (a) (b)). The courts, because of their independence, have traditionally been seen as the last, best defense of minority rights. Requiring judges to stand for popular election, however, may severely compromise their independence, thus inhibiting their ability to protect minorities. This raises the question that is the central focus of this chapter: do elected judges to a greater extent than appointed judges favor the majority in their decision making at the expense of minorities.

The idea that judicial elections may negatively impact minorities is not a new idea. Practically ever since states began using elections to select and reselect judges, scholars have been interested in the experience of minorities under these systems (Alozie 1990; Ifill 1998; Hurwitz and Lanier 2003; Holmes and Emrey 2006). Scholarly attention, however, has largely been confined to examining how elections affect the ability of minorities to attain seats on the bench (Alozie 1990; Ifill 1998; Hurwitz and Lanier 2003; Holmes and Emrey 2006). Much less attention has been focused on how elections impact the decision making of courts on issues important to minorities (for an exception see Vines 1965 or Hume 2013). Because judges who are retained by elections need to maintain the support of a majority of the voters in order to keep

their seats on the bench, it is likely that the decisions they make will be biased towards the outcome favored by the majority. This possibility has been named the "majoritarian difficulty" by Croley (1995). Scholars have already demonstrated that the decision making of elected judges is affected by public opinion.[28] It follows logically then, that if elected judges are influenced by public opinion, it will be the public opinion of the majority that they will be the most responsive to. This means that the opinions of minorities are likely to be overlooked by elected judges (Hume 2013). Appointed judges, on the other hand, do not need to rely on the support of the public to retain their seats on the bench. Thus appointed judges are likely to be much more responsive to minority opinions than are elected judges (Hume 2013). Even if true, this dichotomy may not, at first, seem like a drawback of judicial elections to everyone. After all, the doctrine of majority rules is often considered to be a core component of democracy. As far back as the founding of the Constitution, however, the framers were worried about the potential for a tyranny of the majority (see for example James Madison 1788 "The Federalist Paper No. 10" *The Federalist Papers* and James Madison 1788 "The Federalist Paper No. 51" *The Federalist Papers*). The judicial branch, which has the power to rule on the constitutionality of laws passed by the legislature, is the one branch that is uniquely suited to protecting the interests of minorities. If elections impair the ability of judges to play this role, however, then minorities are left without protection, because the executive and legislative branch are already, by design, fairly majoritarian institutions (Hume 2013). The findings of this chapter suggest that the distinction may be more fairly said to exist between appointed judges or judges reselected in high-information partisan elections and judges reselected in low-information elections, such as

[28] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

nonpartisan or retention elections. In particular, the results suggest that the former group may be more responsive to the opinion of minorities than the latter. The practical implications of this finding then may be that, if elections are going to be used to select and reselect judges, high-information elections, such as partisan elections, will protect minority interests better than will other low-information elections.

Elected Judges Are Influenced by Public Opinion

Not surprisingly, many scholars have found that when judges have to run in competitive elections their behavior is influenced by the views of their constituents.[29] In a frank interview with the Louisiana Supreme Court Justices, one judge admitted to Hall (1987) that he purposefully suppressed dissents in death penalty cases, because his views on the death penalty were out of step with the views of his constituents. Later research has showed that elected judges do systematically dissent less when the dissenting position is relatively unpopular (Hall 1987, 1992; Brace and Hall 1993). Case decisions made by elected judges are also influenced by public opinion,[30] most notably in death penalty cases (Hall 1995; Brace and Hall 1995, 1997; Brace and Boyea 2008). The influence of public opinion on the case decisions of elected judges is likely a result of their fear of defeat if they make decisions that upset voters (Bright and Kennan 1995; Bright 2000; Baum 2003; Hume 2013). The experience of judges in California who were defeated by the voters for issuing liberal death penalty decisions (Bright and Kennan 1995) or

[29]Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

[30] Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013, Hume 2013.

the judges in Iowa who were defeated after ruling in favor of same-sex marriage (Hume 2013) suggests that these fears are real.

The Influence of Public Opinion on Judicial Decision Making is to the Detriment of Minorities

As the cases in California and Iowa show, when judges must stand for election the public does react by voting out judges whose decisions they dislike (Bright and Kennan 1995; Hume 2013). This ability of the majority to remove officeholders whom they disagree with is often considered to be a core component of democracy. This view, however, ignores the fact that the courts were not intended to be majoritarian democratic institutions. Rather the courts were given independence in order to allow them to fulfill their role in protecting the rights of both majorities and minorities (see of example Alexander Hamilton 1788 "The Federalist No. 78." *The Federalist Papers*). When judges must stand for election, however, and thus need the support of a majority to stay on the bench, what becomes of the interests of minorities? Surprisingly little research that has examined the influence of public opinion on the decision making of elected judges has explored how this phenomenon impacts the rights of minorities (for an exception see Vines 1965 or Hume 2013).

The impact of elected judges on minority rights, however, has been partially studied by Vines (1965). Vines examines how southern state supreme courts decided civil rights cases during the period from 1954 to 1963. Specifically, he compares the performance of the courts in Virginia and South Carolina, whose legislatures select the judges, to other southern courts, where judges are selected in popular elections. He finds that South Carolina decided approximately 28 percent of their cases in favor of minorities. This is close to the average for states in the deep south, but significantly higher than in a state like Alabama, where only seven percent of cases

were decided in favor of minorities. In Virginia almost half of the civil rights cases were decided in favor of minorities. This is substantially higher than the average for the peripheral south. Vines (1965) argues that these differences may be explained by many factors, but that method of judicial selection likely played a role (Vines 1965). Vines' (1965) study, while informative, was limited to just southern states and conducted only over a short time span. A more systematic analysis is, therefore, needed to understand the true effects of electing judges on minority rights.

Hume (2013) is one of the few scholars who has explicitly considered this question more systematically. Studying the issue of same-sex marriage, Hume (2013) investigates how the method of judicial selection employed by the state supreme court impacts the direction of same-sex-marriage litigation. One of his first findings is that litigants are much more reluctant to bring same-sex-marriage cases before courts whose judges are retained by competitive elections. He attributes this to strategic behavior on the part of the litigants, who are understandably weary of bringing cases before courts whose decisions are not likely to be favorable (Hume 2013). This finding suggests that minorities themselves recognize that courts whose members rely on majority support for the continuation of their careers are likely either unable or unwilling to risk angering the majority and ruling in their favor. Thus members of the minority in these states either eschew litigation or chose to bring their cases in the more independent federal court system instead (Hume 2013).

Further research by Hume (2013) shows that there is some truth to minority litigants' fears that their grievances will not get a fair hearing in courts where judges have to run in competitive elections. Hume (2013) expects that judges who are selected by elections will be less likely than judges retained by reappointment to rule in favor of same-sex marriage, because at the time that these judges were being asked to consider the question, majority opinion was still

clearly against same-sex marriage. Thus elected judges feared losing their jobs if they ruled in support of same-sex marriage. His findings largely confirm these expectations. In fact, Hume (2013) finds that none of the pro same-sex-marriage opinions came from state courts whose judges are forced to run in competitive elections. Additionally, only two pro same-sex-marriage decisions came from courts whose members are retained in retention elections. Thus the majority of the seven pro same-sex-marriage decisions handled in the states, were handed down in courts whose members are reappointed, and thus not dependent on majority support (Hume 2013). These numbers, however, also reflect the previously mentioned behavior of strategic litigants, who, for the most part, were more reluctant to bring cases in courts whose members are chosen in elections (Hume 2013). In a more systematic event history analysis, however, Hume (2013) continues to find that using elections to select judges is significantly and negatively related to the probability that the state court will rule in favor of same-sex marriage.

A myriad of research has shown that when judges are elected they are influenced by public opinion.[31] What little research that exists that has considered how this affects minorities, has suggested that the interests of minorities are better protected when judges are appointed instead of elected (Vines 1965, Hume 2013). This makes sense because if elected judges are dependent on the support a majority of voters to keep their seats on the bench, then they are going to naturally avoid making decisions in favor of minorities if doing so risks upsetting the majority. Thus the main hypothesis of this chapter is that elected judges will be less sympathetic to the interests of minorities than appointed judges.

[31] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

Data and Methods:

One way to test this theory is suggested by the work of Gilens (2012). Gilens (2012) tests the possibility that the decision making of public officials is disproportionally responsive to the opinion of the rich at the expense of the opinions of the poor and middle class. He evaluates this by combining a large number of national surveys and then disaggregating the opinion of the public on various issues by income and education. Next, he models the probability that a policy is adopted based, in part, on the support for the policy of the rich and lower classes. He finds that only the opinions of the rich significantly affect the probability of policy adoption. Thus he concludes that policy makers are disproportionally responsive to the opinions of the rich (Gilens 2012). While Gilens (2012) is more concerned with responsiveness by the executive and legislative branch, his basic methodology can be more or less adopted to examine whether elected courts are disproportionally responsive to majorities at the expense of minorities. I propose to do this by using estimates of black and nonblack support for the death penalty in each state and examining how these opinions affect the probability of a judge casting a liberal vote in a death penalty case. I theorize that the decision making of elected judges will be affected less by black opinion on the death penalty than the decision making of appointed judges, because of elected judges' need to cater to majority opinion.

I can test this theory using the same dataset of death penalty decisions that I utilized in Chapter Three. Instead of using state-level estimates of public opinion that combines the opinions of blacks and whites, however, I need separate estimates for black and white opinion. Fortunately, Shirley and Gelman (2015)[32] have already done a lot of work in estimating support among various groups for the death penalty. Specifically, they combine questions on support for

[32] Whose estimates also formed the basis of the public opinion estimates utilized by Canes-Wrone, Clark, and Kelly (2014).

the death penalty, asked either by Gallup or on the General Social Survey, and then use a multilevel logistic regression to estimate support for the death penalty for different permutations of state, year, race, education, and age. Because these estimates are separate for each different possible combination, I need to do a little work on these estimates so they are usable for my purposes. Essentially, I want to create separate estimates for blacks and non-blacks for each state-year that is as reflective of the that population as possible. To do this I gather data from the U.S Census Bureau[33] that allows me to calculate within each state the percent of people falling into each age, race, and sex demographic bucket for which individual estimates of support on the death penalty exist for. Because I cannot also find this data by education, I first average the estimates across the different education groups. Next, I adjust these estimates for each of the remaining demographic buckets, by multiplying them by the percent of the state population they make up in that year. Finally, I sum these estimates up for both blacks and non-blacks to arrive at my final state and year specific estimates.

Table 4.1 Summary of Variables

Statistic	Mean	St. Dev.	Min	Max
Black Support	0.539	0.122	0.205	0.911
Non-Black Support	0.808	0.069	0.519	0.965

A summary of these estimates is provided in Table 4.1 and a graph of the estimates for both black and non-black opinion by state and year are displayed in Figure 4.1. Comparing between the two graphs, it appears that blacks are overall less supportive of the death penalty

[33] "Population Estimates-1980s State Tables." The United States Census Bureau. http://www.census.gov/popest/data/historical/1980s/state.html. Accessed March 2016. "Population Estimates- Age by Sex by Race by Hispanic Origin." The United States Census Bureau. http://www.census.gov/popest/data/state/asrh/1990s/st_detail.html. Accessed March 2016. "Population Estimates-Intercensal Estimates." The United States Census Bureau. http://www.census.gov/popest/data/intercensal/index.html. Accessed March 2016.

than are non-blacks. This dichotomy can also be seen in Figure 4.2, which compares the overall median level of support for blacks with the overall median for non-blacks. The fact that there are strong differences in black and white opinion on the death penalty is good for the purposes of this study. If opinion did not differ between blacks and whites it would be hard to assess the separate influence of each on the decision making of judges. Since opinion does differ, however, testing for the separate influence of black and white opinion on appointed versus elected judges should be a fairly straightforward task. To do this I estimate the same models from Chapter Three for death penalty decision making, except I include variables for both black and non-black support for the death penalty rather than the general public support variable included in Chapter Three. Similarly to Chapter Three, I present estimates from separate models here, but also run an interactive model, displayed in the appendix, that largely comes to the same conclusions as the separate models discussed here.

Figure 4.1: Death Penalty Opinion by Race, State, and Year

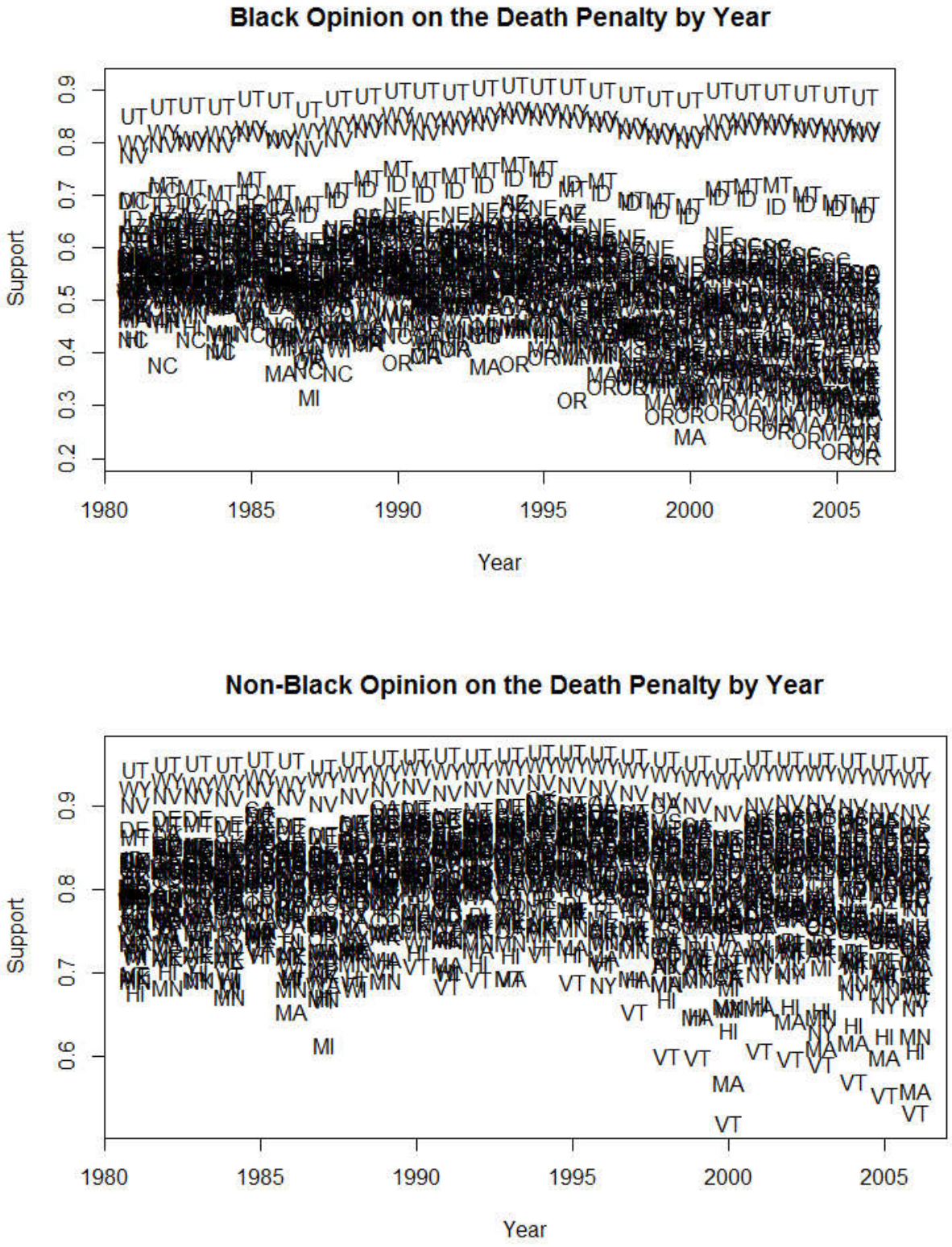
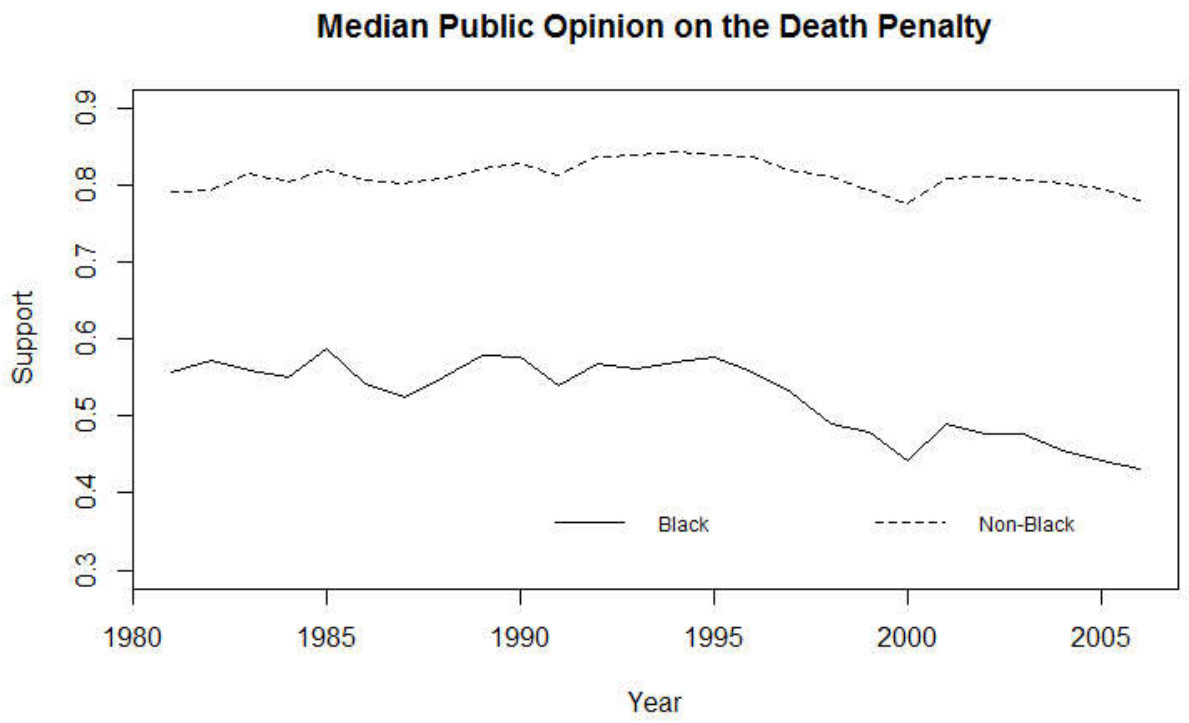


Figure 4.2: Median Public Opinion on the Death Penalty over Time



Analysis:

The results of these models are displayed in Table 4.2. As in the previous chapter is it important to account for the clustering in the data. To do this I continue to present standard errors that are clustered by state. Also, as in the other chapters, I present multilevel results with random intercepts for states and years in the appendix. The results from the multilevel model are fairly similar to the OLS results presented here, with the exception of the model for retention elections. One of the first things to note about the OLS models, is that there is a negative, although not always statistically significant, effect of non-black public opinion on the decision making of judges reselected by some type of election. This indicates that as non-black opinion becomes more supportive of the death penalty, elected judges are less likely to vote to overturn death sentences. This finding is expected, as several studies have demonstrated that elected judges are

affected by public opinion when making decision in death penalty cases (Hall 1995; Brace and Hall 1995, 1997; Brace and Boyea 2008). What is more surprising, however, is that the relationship between black opinion and judicial decision making in the model for judges reselected by nonpartisan or retention elections is estimated to be positive. This suggests that as blacks become more supportive of the death penalty, judges reselected by these methods are paradoxically more likely to vote to overturn death sentences. This implies that when judges reselected by those systems look to public opinion to influence their decision making, it is the public opinion of the majority, and not the minority, whom they are primarily paying attention to. The coefficient on black opinion for judges who are either reappointed or reselected in partisan elections, however, is negative. This suggests that judges in these states are influenced to some extent by black opinion. My original hypothesis for this chapter was that all elected judges would be influenced more by majority opinion than by minority opinion. Looking at the coefficients, there is some support for this hypothesis as applied to judges reselected in nonpartisan and retention elections. This is because, in these models, there is a negative effect of non-black opinion, but actually a positive and significant effect of black opinion. Judges reselected in partisan elections, however, appear to be responsive to both black and non-black opinion, while the results show that appointed judges may be influenced only by black opinion. These results suggest that the problem of judges favoring majority opinion over minority opinion may be the most acute in cases where judges are reselected in either nonpartisan or retention elections.

Table 4.2 Black and Non-Black Public Opinion Logit Results with Clustered Standard Errors

	Vote			
	(1) Appointed	(2) Partisan	(3) Nonpartisan	(4) Retention
Constant	-6.50	2.76*	6.17***	3.19
	(12.43)	(1.33)	(1.65)	(2.66)
Sum Facts (-)	-0.08	-0.30***	-0.12	-0.16
	(0.05)	(0.04)	(0.08)	(0.10)
Next Election (-)	0.17	-0.09	-0.13	-0.02
	(0.19)	(0.11)	(0.08)	(0.13)
Dem Retire (+)	1.26	0.14	-1.79***	-0.36*
	(0.83)	(0.27)	(0.17)	(0.18)
Rep Retire (-)	-0.37	-1.29***	-1.91***	-0.22
	(1.01)	(0.11)	(0.46)	(0.31)
Party (+)	0.24	0.16	0.46**	1.10***
	(0.22)	(0.36)	(0.14)	(0.22)
Dem Lame Duck (+)		0.30	-0.93*	
		(0.49)	(0.45)	
Rep Lame Duck (-)		-0.41	0.29	
		(0.39)	(0.47)	
SCOTUS (+)		3.55***	17.40***	3.71***
		(0.11)	(0.87)	(0.40)
Non-Black Opinion (-)	12.53	-2.89	-10.31***	-7.04
	(22.02)	(2.00)	(2.06)	(3.64)
Black Opinion (-)	-9.42	-2.13	2.35***	2.37**
	(11.78)	(1.20)	(0.44)	(0.86)
N	1,216	3,797	3,009	4,107

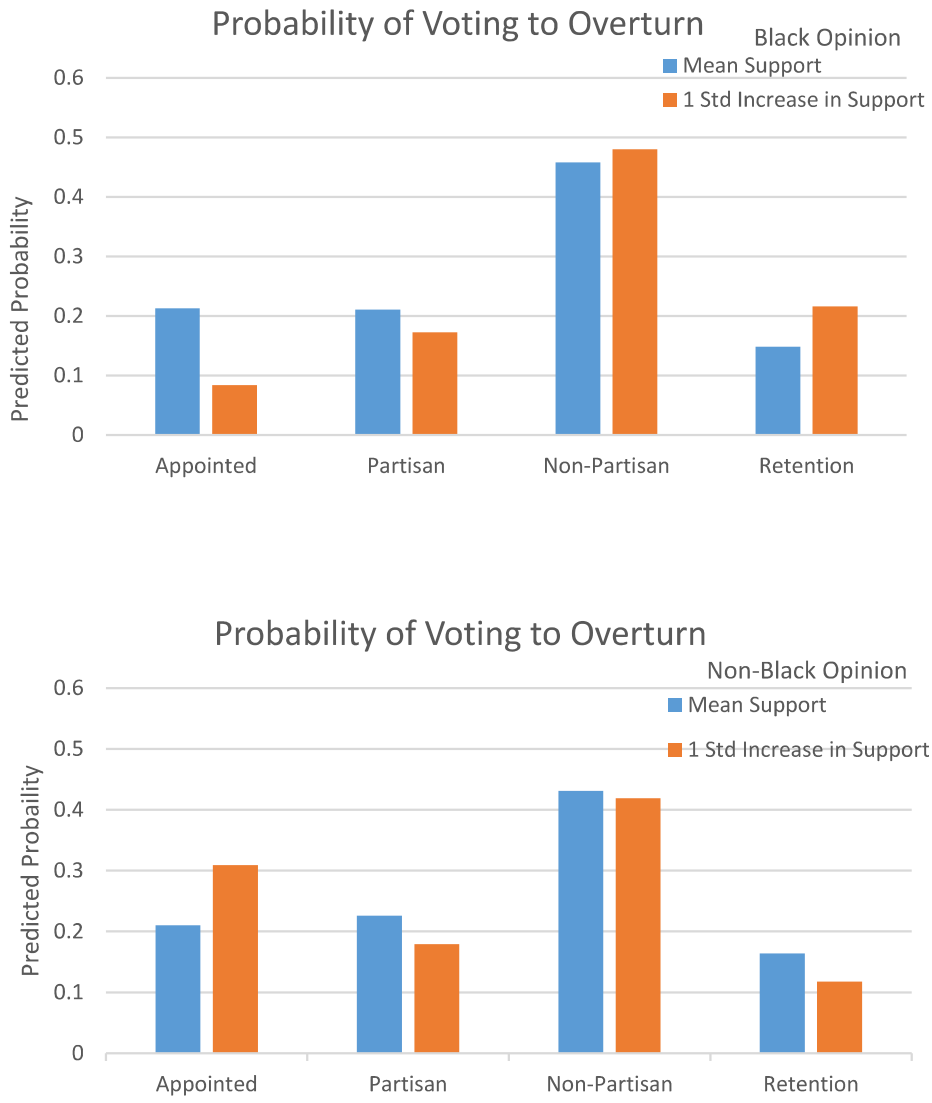
*p < .05; **p < .01; ***p < .001

To illustrate more clearly how the impact of black and nonblack opinion on the probability of voting to overturn death sentences varies by reselection method, I once again use Zelig (Imai, King, and Lau 2007, 2008, 2016) to generate predicted probabilities of a judge voting to overturn. The predicted probabilities are based on the models in Table 4.2. I set all of

the variables to 0, except for the sum of case facts, which is held at 2. To examine the impact of public opinion, I first estimate the predicted probability of voting to overturn when public opinion is held at its mean. Then, I do this again after simulating a one standard deviation increase in support.[34] I repeat this process separately for black and non-black opinion. Since I am increasing support for the death penalty, if judges are responsive to public opinion, we should see a drop in their probability of voting to overturn between the two different public opinion scenarios. The results, which are displayed in Figure 4.3, show that once again the probability of voting to overturn is highest in nonpartisan elections. Looking at the impact of public opinion, however, shows that judges reselected in either partisan elections or by reappointment do show the expected drop in probability of voting to overturn when black support for the death penalty increases. This drop is steeper among appointed judges, however, suggesting that, as hypothesized, appointed judges may be the most responsive to minority opinion. The non-responsiveness of judges selected in nonpartisan and retention elections can be seen in the fact that when black opinion because one standard deviation more supportive of the death penalty the probability of a judge voting to overturn actually increases.

[34] I use the mean public opinion among the combined dataset so the comparison is not affected by different levels of public support among the four different types of reselection systems.

Figure 4.3 Predicted Probability of Voting to Overturn Death Sentences



Predicted probability of a judge voting to overturn generated with Zelig. All other independent variables set to 0, except for the sum of case facts, which is set to 2 and the other public opinion variable, which is held at its mean.

Discussion:

The results from this chapter provide some evidence that the decision making of judges reselected in nonpartisan or retention elections is only responsive to the opinion of majorities and not minorities. This is a seriously troubling finding, because the opinion of minorities seems to

have less influence than the opinion of majorities in these systems. While I originally believed that this would occur whenever judges are reselected in elections, this expectation was not borne out by the data. Instead, I found that this seems to occur only when judges are reselected in low-information nonpartisan or retention elections. As with the findings from the previous chapter, this suggests that when voters have less information to base their opinion on they are more likely to consider the judges' record. Thus the pressure for judges to vote in accordance with majority opinion may actually be the greatest in nonpartisan and retention elections (Canes-Wrone, Clark, and Kelly 2014). Future studies should see if these tendencies hold true across different cases types. It could also be interesting to examine how the differential turnout rates of minorities in judicial elections may also affect these relationships.

Chapter Five: Conclusions

This dissertation embarked on the task of identifying and testing three new potential consequences of using elections to select judges. Much of the existing literature on judicial elections views judicial elections positively, often crediting them with increasing democratic accountability (Hojnacki and Baum 1992; Hall 2001 (a); Hall and Bonneau 2006). There has been very little evidence to show that there may also be some downsides that accompany the practice of electing judges. The aim of this dissertation was to change that. Thus in this dissertation I identified three new theoretical ways in which judicial elections may be harmful to society, and set out to empirically evaluate and rigorously test these theories. I found some support for each of the theories tested in this dissertation, however, in most cases, I found that the real differences may be said more fairly to exist between judges who are appointed or reselected in high-information partisan elections and judges reselected in low-information nonpartisan or retention elections. Thus although these results do not support a general indictment of judicial elections, they at least suggest that low-information judicial elections may have some important negative consequences. Bearing this in mind, I think it is certainly worth conducting more research and thinking more carefully about the concerns raised in this dissertation, with respect to low-information judicial elections. This is especially true, whenever policy makers are evaluating the best method in which to select and reselect judges.

The first-posited consequence of using elections to select judges was the idea that voters in judicial elections would be poorly informed, and consequently, would make decisions based on factors that are irrelevant to judicial elections. This would challenge one of the main arguments in support of judicial elections, namely the argument that judicial elections can enhance democratic accountability. In Chapter Two of this dissertation, I used a large dataset of

judicial elections, occurring between 1990-2007, to extensively test for the influence of irrelevant factors on incumbent vote share. I found that certain economic factors may be related to the performance of incumbent judicial candidates. Specifically, I found that some incumbent judges may be punished or rewarded on the basis of state GDP (all incumbents) and national unemployment (in-party incumbents). These economic factors should not theoretically be related to judicial elections, because courts should have very little to do with economic performance. The existence of such relationships suggests that voters may be responding to broader societal trends, rather than to more appropriate election-specific information. I also unrecovered a relationship between presidential approval and the vote share of in-party incumbents. This finding also indicates that voters may be responding to broader trends in party performance, rather than to any factors that are specific and relevant to judicial elections. Together these findings challenge the argument that electing judges increases democratic accountability, because often the factors that voters are basing their choices on may be completely unrelated to judicial elections.

The second consequence of using elections to reselect judges that I tested, was the idea that judges who are elected will be more likely to pay attention to public opinion, and consequently, will have less cognitive resources leftover to also consider the facts of the case in their decision making. This is undesirable, because there is widespread agreement that judges should pay attention to the law and case facts when they make decisions (Segal and Spaeth 1993, 2002; Pozen 2008). In Chapter Three, I tested this theory with a dataset of judicial decision making in death penalty cases. I did not find, as expected, that the presence of aggregating factors, in general, had a larger effect on appointed judges when compared to elected judges. Instead, I found that the predictive power of a case facts model was the greatest among judges

reselected in partisan elections, followed by judges who are appointed. While this evidence may not serve as a general indictment of elected judges, it does caution against certain types of elections. Electing judges in contests where voters have particularly little information, such as in nonpartisan elections, can increase pressure on these judges to follow public opinion (Canes-Wrone, Clark, and Kelly 2014). This may negatively impact their ability to also pay attention the facts of the case when making decisions.

The last consequence of electing judges that I tested, was the theory that elected judges would be harmful to minority rights. It has been well documented that elected judges are influenced by the public when they make decisions.[35] This fact is not often viewed negatively. I, however, argue that the electoral need to secure a majority of the votes, may lead judges to be concerned with only the public opinion of the majority. I evaluated this theory by testing whether minority opinion has a differential impact on appointed judges versus judges reselected by elections. Again looking at judicial decision making in death penalty cases, I found that the impact of black public opinion on the death penalty was the greatest when judges were appointed or reselected in partisan elections. Thus once again, the results of this chapter caution only against using low-information elections to select and reselect judges.

Overall, this dissertation provides some evidence that there may be consequences of using certain types of elections to select and reelect judges. Firstly, I find that voters in judicial elections may not be responding to normatively appropriate criteria. I also find that when judges are reselected in low-information elections, they may not pay as much attention to the facts of the case then they otherwise might. Lastly, I find some evidence that the practice of electing

[35] Canon and Jaros 1970; Hall 1987, 1992; Brace and Hall 1993; Hall 1995; Brace and Hall 1995, 1997; Brace, Hall, and Langer 2001; Cauthen and Peters 2003; Hoekstra 2005; Howard, Graves, and Flowers 2006; Savchak and Barghothi 2007; Boyea 2007, 2010; Brace and Boyea 2008; Shepherd 2009; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2010; Cann and Wilhelm 2011; Windett, Hall, and Harden 2013; Hume 2013.

judges incentivizes judges reelected in low-information elections to consider the opinions and interests of the majority, potentially to the detriment of the minority. These three consequences that I identify in this dissertation may just be the beginning. Future research on how the practice of electing and reelecting judges in low-information elections impacts society should continue. Considering that states may always adjust their method of judicial selection and reselection, it is critically important to continue trying to understand the consequences of all of these different methods of judicial selection, so that policy makers may make the best informed choices about method is right for judges in their state.

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Appendix to Chapter 2

To make sure that the results presented earlier were not unduly affected by my decision to control for the logged number of candidates in the race, rather than the actual number of candidates in the race, I reproduce the tables presented in Chapter Two without transforming this variable. As can be seen below the substantive results for the variables of interest are quite similar. The main difference between these models and the models presented earlier in the chapter are the signs on the control variables for partisan and nonpartisan elections. These variables are positive in the models presented earlier but negative here. This suggests that transforming this variable may have caused the signs to flip on these two control variables.

Table A2.1 GDP OLS Results with Clustered Standard Errors

	Incumbent Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	88.27***	84.23***	71.04***	85.31***	78.63***	79.01***	51.87***	79.45***
	(6.01)	(2.12)	(12.19)	(2.02)	(11.40)	(6.57)	(14.26)	(5.63)
Appointed Incumbent (-)	-0.44	-0.58	-0.32	-0.47	-1.31	-2.01	-0.94	-1.42
	(0.97)	(0.94)	(0.96)	(1.01)	(1.68)	(1.32)	(2.12)	(1.90)
Quality Challenger (-)	-8.05***	-8.06***	-7.98***	-8.17***	-8.17***	-8.05***	-8.16***	-8.32***
	(1.99)	(1.87)	(1.77)	(1.57)	(2.11)	(2.18)	(2.03)	(1.96)
Number of Candidates (-)	-5.57***	-5.80***	-5.66***	-5.57***	-5.76***	-5.93***	-5.76***	-5.80***
	(0.69)	(0.69)	(0.64)	(0.64)	(0.63)	(0.63)	(0.55)	(0.57)
Major Party (+)	3.78	4.96*	2.08	1.99	2.86	3.36	1.38	0.76
	(2.15)	(2.19)	(2.17)	(1.93)	(2.76)	(2.76)	(3.15)	(2.45)
Nonpartisan (-)	-5.48*	-5.37*	-5.03*	-5.58*	-2.05	-2.39	-0.87	-0.55
	(2.34)	(2.39)	(2.29)	(2.19)	(3.66)	(3.12)	(3.01)	(3.32)
Partisan (-)	-1.12	-1.39	-1.87	-3.00				
	(3.55)	(3.16)	(2.98)	(3.07)				
District (+)	4.05	4.44	4.51	4.11	-2.62	-1.33	1.09	-2.66
	(4.01)	(3.95)	(3.87)	(3.91)	(2.31)	(2.27)	(2.54)	(2.24)
Female (+)	0.80	0.82	0.58	0.73	0.98	1.07	0.60	0.92
	(0.60)	(0.61)	(0.60)	(0.58)	(0.93)	(0.93)	(0.85)	(0.80)

Female Challenger (-)	-4.58**	-4.59***	-4.63***	-4.35***	-5.74***	-5.28***	-5.64***	-5.35***
	(1.49)	(1.32)	(1.09)	(1.25)	(1.49)	(1.44)	(1.18)	(1.23)
Minority (-)	-0.91	-0.87	-1.05	-1.05	-1.08	-0.58	-1.24	-1.15
	(1.18)	(1.11)	(1.15)	(1.21)	(1.60)	(1.37)	(1.69)	(1.89)
Minority Challenger (+)	6.42*	7.37***	4.98*	5.03*	7.05***	8.41***	5.60***	5.81***
	(2.51)	(2.14)	(2.17)	(2.15)	(2.06)	(1.72)	(1.66)	(1.57)
Natl Murder Rate (-)	-0.86	-0.74**	0.30	-0.73**	-0.05	-0.26	1.65*	-0.25
	(0.52)	(0.28)	(0.69)	(0.25)	(0.86)	(0.59)	(0.68)	(0.52)
State Murder Rate (-)	-0.87**	-0.83**	-0.79*	-0.75*	-0.77	-0.76	-0.63	-0.59
	(0.32)	(0.32)	(0.33)	(0.32)	(0.45)	(0.41)	(0.47)	(0.44)
Difference in Spending (+)					0.004***	0.004***	0.004**	0.004**
					(0.001)	(0.001)	(0.001)	(0.001)
Natl GDP Inc. Coded (+)	-0.24				0.05			
	(0.30)				(0.62)			
Natl GDP In-Party Coded (+)	-0.16*				-0.15			
	(0.06)				(0.09)			
Natl GDP Growth Inc. Coded (+)		-0.001				0.40		
		(0.32)				(0.55)		
Natl GDP Growth In-Party Coded (+)		-0.85***				-0.91***		
		(0.17)				(0.19)		
State GDP Inc. Coded (+)			0.25				0.45*	
			(0.20)				(0.22)	
State GDP In-Party Coded (+)			-0.01				-0.01	
			(0.03)				(0.04)	
State GDP Growth Inc. Coded (+)				0.74***				0.56
				(0.15)				(0.30)
State GDP Growth In-Party Coded (+)				-0.40*				-0.26
				(0.18)				(0.30)
N	481	481	481	481	191	191	191	191
Adjusted R^2	0.59	0.61	0.60	0.60	0.46	0.50	0.47	0.45

*p < .05; **p < .01; ***p < .001

Table A2.2 Unemployment OLS Results with Clustered Standard Errors

	Incumbent Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	86.03***	85.49***	85.36***	85.26***	81.53***	79.00***	79.69***	79.78***
	(2.81)	(1.95)	(2.31)	(1.97)	(7.41)	(5.95)	(7.08)	(6.22)
Appointed Incumbent (-)	-0.39	-0.32	-0.19	-0.23	-1.41	-1.38	-0.74	-1.02
	(0.97)	(0.96)	(1.00)	(0.98)	(1.71)	(1.46)	(2.04)	(1.74)
Quality Challenger (-)	-7.87***	-8.29***	-8.04***	-7.76***	-8.01***	-8.58***	-8.27***	-8.06***
	(1.94)	(1.67)	(1.91)	(1.71)	(2.19)	(1.82)	(2.18)	(2.01)
Number of Candidates (-)	-5.56***	-5.83***	-5.57***	-5.39***	-5.79***	-6.13***	-5.73***	-5.69***
	(0.69)	(0.76)	(0.66)	(0.71)	(0.60)	(0.68)	(0.60)	(0.64)
Major Party (+)	4.13	2.44	2.60	2.36	3.14	0.48	1.80	1.01
	(2.11)	(2.08)	(2.17)	(2.07)	(2.61)	(2.25)	(3.06)	(2.59)
Nonpartisan (-)	-5.52*	-5.01*	-5.41*	-5.93*	-2.07	-1.17	-1.26	-1.14
	(2.36)	(2.26)	(2.36)	(2.30)	(3.61)	(3.27)	(3.40)	(3.50)
Partisan (-)	-1.11	-1.63	-1.63	-2.51				
	(3.53)	(3.26)	(3.43)	(3.36)				
District (+)	4.14	4.20	3.95	3.88	-2.29	-2.40	-3.16	-2.98
	(3.91)	(3.94)	(3.97)	(3.90)	(2.28)	(2.34)	(2.09)	(2.27)
Female (+)	0.79	0.70	0.67	0.68	1.02	1.04	0.89	0.85
	(0.60)	(0.60)	(0.59)	(0.58)	(0.92)	(0.96)	(0.96)	(0.86)
Female Challenger (-)	-4.53***	-4.70***	-4.71***	-4.62***	-5.49***	-5.67***	-5.83***	-5.63***
	(1.32)	(1.37)	(1.26)	(1.33)	(1.33)	(1.48)	(1.38)	(1.27)
Minority (-)	-0.89	-1.00	-0.99	-0.83	-0.70	-1.14	-1.17	-1.11
	(1.17)	(1.20)	(1.21)	(1.23)	(1.69)	(1.71)	(1.91)	(1.74)
Minority Challenger (+)	6.97*	5.71**	5.32*	5.05*	7.73***	6.98***	6.12**	5.92**
	(2.71)	(1.93)	(2.34)	(2.26)	(2.14)	(1.39)	(1.90)	(1.80)
Natl Murder Rate (-)	-0.62	-0.58*	-0.60*	-0.57*	-0.03	0.26	-0.11	-0.13
	(0.37)	(0.28)	(0.29)	(0.26)	(0.65)	(0.53)	(0.51)	(0.52)
State Murder Rate (-)	-0.84**	-0.80*	-0.82*	-0.85**	-0.76	-0.65	-0.63	-0.67
	(0.32)	(0.32)	(0.33)	(0.32)	(0.44)	(0.43)	(0.43)	(0.43)
Difference in Spending (+)					0.004***	0.004***	0.004***	0.004***
					(0.001)	(0.001)	(0.001)	(0.001)
Natl Unemployment Inc. Coded (-)	-0.47				-0.59			
	(0.51)				(0.64)			
Natl Unemployment In-Party Coded (-)	-0.41**				-0.38*			

	(0.15)				(0.15)			
Natl Unemployment Change Inc. Coded (-)		0.11				-1.00		
		(1.04)				(1.39)		
Natl Unemployment Change In-Party Coded (-)		1.26				3.27*		
		(0.86)				(1.44)		
State Unemployment Inc. Coded (-)			-0.05				-0.12	
			(0.39)				(0.61)	
State Unemployment In-Party Coded (-)			0.07				0.11	
			(0.16)				(0.22)	
State Unemployment change Inc. Coded (-)				-0.86				-0.94
				(1.02)				(1.57)
State Unemployment Change In-Party Coded (-)				1.48*				0.53
				(0.73)				(0.88)
N	481	481	481	481	191	191	191	191
Adjusted R^2	0.60	0.59	0.59	0.59	0.47	0.47	0.45	0.45

*p < .05; **p < .01; ***p < .001

Table A2.3 Consumer Sentiment OLS Results with Clustered Standard Errors

	Incumbent Vote Share	
	(1)	(2)
Constant	83.97***	78.06***
	(2.49)	(6.75)
Appointed Incumbent (-)	-0.34	-1.10
	(1.03)	(2.04)
Quality Challenger (-)	-8.00***	-8.20***
	(1.88)	(2.13)
Number of Candidates (-)	-5.62***	-5.83***
	(0.64)	(0.58)
Major Party (+)	2.23	1.07
	(2.28)	(3.22)
Nonpartisan (-)	-5.33*	-1.26
	(2.31)	(3.40)
Partisan (-)	-1.64	
	(3.36)	
District (+)	4.03	-2.92
	(3.96)	(2.23)
Female (+)	0.68	0.92
	(0.59)	(0.93)
Female Challenger (-)	-4.58***	-5.56***
	(1.20)	(1.25)
Minority (-)	-0.94	-0.98
	(1.23)	(1.89)
Minority Challenger (+)	5.44*	6.29***
	(2.25)	(1.73)
Natl Murder Rate (-)	-0.50	0.02
	(0.32)	(0.52)
State Murder Rate (-)	-0.83*	-0.67
	(0.32)	(0.43)
Difference in Spending		0.004**
		(0.001)
Consumer Sentiment Inc. Coded (+)	1.50	2.04
	(2.17)	(2.73)
Consumer Sentiment In-Party Coded (+)	0.18	0.20
	(1.96)	(2.62)
N	478	191

Adjusted R^2	0.59	0.45

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A2.4 Executive Approval OLS Results with Clustered Standard Errors

	Incumbent Vote Share			
	(1)	(2)	(3)	(4)
Constant	84.41***	85.93***	78.47***	83.11***
	(2.47)	(3.27)	(5.03)	(5.89)
Appointed Incumbent (-)	-0.41	-0.57	-1.58	-3.16
	(0.97)	(0.98)	(1.62)	(1.76)
Quality Challenger (-)	-7.91***	-7.90***	-8.03***	-8.16***
	(1.93)	(1.89)	(1.44)	(1.61)
Number of Candidates (-)	-5.56***	-5.43***	-5.86***	-5.73***
	(0.70)	(0.52)	(0.85)	(0.91)
Major Party (+)	4.30	1.36	3.02	-0.11
	(2.26)	(2.40)	(2.20)	(2.52)
Nonpartisan (-)	-5.48*	-3.72	-2.25	-2.64
	(2.38)	(2.21)	(2.10)	(2.41)
Partisan (-)	-1.08	0.20		
	(3.46)	(3.27)		
District (+)	4.19	3.25	-2.11	-1.03
	(3.98)	(3.51)	(2.46)	(2.86)
Female (+)	0.82	0.68	1.26	0.82
	(0.59)	(0.73)	(1.68)	(1.82)
Female Challenger (-)	-4.73***	-5.24***	-5.41**	-5.31*
	(1.41)	(0.89)	(1.90)	(2.03)
Minority (-)	-0.83	-1.05	-0.37	-0.52
	(1.13)	(1.57)	(1.98)	(2.21)
Minority Challenger (+)	7.09**	8.56***	7.68*	8.27*
	(2.70)	(1.12)	(3.27)	(3.84)
Natl Murder Rate (-)	-0.68*	-0.81*	-0.19	-0.13
	(0.29)	(0.33)	(0.56)	(0.60)
State Murder Rate (-)	-0.84**	-0.99***	-0.78*	-0.97*
	(0.32)	(0.25)	(0.35)	(0.38)
Difference in Spending (+)			0.004**	0.003
			(0.001)	(0.002)

Pres. Approval Inc. Coded (+)	-0.93		2.74	
	(2.63)		(5.62)	
Pres. Approval In-Party Coded (+)	4.04**		4.00**	
	(1.29)		(1.42)	
Gov. Approval Inc. Coded (+)		0.03		0.03
		(0.04)		(0.06)
Gov. Approval In-Party Coded (+)		0.001		-0.004
		(0.02)		(0.02)
N	479	380	191	161
Adjusted R^2	0.60	0.61	0.47	0.49

*p < .05; **p < .01; ***p < .001

I also present the results of multilevel models in this appendix that can be compared to the results in Chapter 2 obtained by using clustered standards errors. With a multilevel model, rather than accounting for clustering by state or year by adjusting the standard errors, I instead, include random intercepts for states and years into the model directly. Due to sample size considerations, I do not attempt to replicate the results of the models that include the spending variable with multilevel models. Comparing the multilevel results to the main results presented earlier reveals that the effects of state GDP is not significant in the multilevel model, even though it was significant using the clustered standard error approach. Given that the evidence for a relationship between state GDP and incumbent vote share was already fairly tenuous, this does not really challenge the conclusions of Chapter 2. The multilevel model results for unemployment, consumer sentiment, and executive approval are fairly similar to the results from the models with clustered standard errors, and the broad conclusions about the effect of these variables remains the same. There may be a relationship between national unemployment and in-party incumbent vote share, as well as presidential approval and in-party incumbent vote share.

Table A2.5 GDP MLM Results

	Incumbent Vote Share			
	(1)	(2)	(3)	(4)
Constant	89.20***	82.50***	70.71***	83.45***
	(6.94)	(2.13)	(8.35)	(2.30)
Appointed Incumbent (-)	-1.06	-1.17	-0.74	-0.96
	(0.65)	(0.65)	(0.66)	(0.66)
Quality Challenger (-)	-7.79***	-7.84***	-7.81***	-7.77***
	(0.98)	(0.97)	(0.99)	(1.00)
Number of Candidates (-)	-23.62***	-24.42***	-24.42***	-24.43***
	(2.60)	(2.56)	(2.61)	(2.61)
Major Party (+)	0.95	1.74	-0.23	-0.57
	(1.35)	(1.34)	(1.32)	(1.26)
Nonpartisan (-)	1.03	1.48	2.00	1.75
	(2.54)	(2.50)	(2.56)	(2.55)
Partisan (-)	2.78	3.45	2.91	2.19
	(2.98)	(2.95)	(3.02)	(3.03)
District (+)	3.68	3.85	3.29	3.21
	(2.69)	(2.63)	(2.76)	(2.69)
Female (+)	0.85	0.84	0.73	0.79
	(0.72)	(0.71)	(0.72)	(0.72)
Female Challenger (-)	-5.17***	-5.14***	-5.28***	-5.01***
	(1.25)	(1.24)	(1.27)	(1.27)
Minority (-)	-0.77	-0.68	-0.99	-0.94
	(0.93)	(0.92)	(0.93)	(0.94)
Minority Challenger (+)	4.49*	5.11*	3.55	3.48
	(2.15)	(2.15)	(2.16)	(2.17)
Natl Murder Rate (-)	-1.16*	-0.85**	0.07	-0.80**
	(0.58)	(0.28)	(0.59)	(0.31)
State Murder Rate (-)	-0.80***	-0.75**	-0.69**	-0.66**
	(0.24)	(0.23)	(0.24)	(0.24)
Natl GDP Inc. Coded (+)	-0.38			
	(0.32)			
Natl GDP In-Party Coded (+)	-0.16***			
	(0.05)			
Natl GDP Growth Inc. Coded (+)		0.07		
		(0.26)		
Natl GDP Growth In-Party Coded (+)		-0.65***		

		(0.14)		
State GDP Inc. Coded (+)			0.20	
			(0.13)	
State GDP In-Party Coded (+)			0.02	
			(0.02)	
State GDP Growth Inc. Coded (+)				0.34
				(0.19)
State GDP Growth In-Party Coded (+)				-0.15
				(0.17)
State Std.	6.37	6.15	6.15	6.32
Year Std.	1.11	0.58	0.58	0.9
N	481	481	481	481

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A2.6 Unemployment MLM Results

	Incumbent Vote Share			
	(1)	(2)	(3)	(4)
Constant	85.08***	83.52***	83.11***	83.31***
	(2.74)	(2.35)	(2.73)	(2.49)
Appointed Incumbent (-)	-1.01	-0.95	-0.69	-0.86
	(0.65)	(0.66)	(0.66)	(0.66)
Quality Challenger (-)	-7.68***	-7.87***	-7.83***	-7.61***
	(0.98)	(1.00)	(0.99)	(1.00)
Number of Candidates (-)	-23.28***	-25.28***	-23.68***	-23.31***
	(2.60)	(2.68)	(2.64)	(2.68)
Major Party (+)	1.21	-0.53	-0.01	-0.67
	(1.34)	(1.27)	(1.31)	(1.26)
Nonpartisan (-)	0.91	2.44	1.41	0.79
	(2.53)	(2.60)	(2.56)	(2.61)
Partisan (-)	2.86	3.15	2.47	1.67
	(2.97)	(3.03)	(3.00)	(3.04)
District (+)	3.89	3.29	2.74	3.09
	(2.67)	(2.71)	(2.73)	(2.69)
Female (+)	0.82	0.78	0.73	0.75
	(0.71)	(0.72)	(0.72)	(0.72)
Female Challenger (-)	-5.10***	-5.21***	-5.31***	-4.92***
	(1.25)	(1.26)	(1.26)	(1.27)
Minority (-)	-0.68	-0.92	-0.97	-0.75
	(0.93)	(0.93)	(0.93)	(0.94)
Minority Challenger (+)	4.88*	3.99	3.34	3.28
	(2.16)	(2.17)	(2.17)	(2.18)
Natl Murder Rate (-)	-0.61	-0.71*	-0.68	-0.71*
	(0.37)	(0.31)	(0.35)	(0.33)
State Murder Rate (-)	-0.73**	-0.70**	-0.65**	-0.69**
	(0.23)	(0.24)	(0.24)	(0.24)
Natl Unemployment Inc. Coded (-)	-0.80			
	(0.56)			
Natl Unemployment In-Party Coded (-)	-0.37***			
	(0.10)			
Natl Unemployment Change Inc. Coded (-)		-0.23		
		(0.73)		

Natl Unemployment Change In-Party Coded (-)		0.95		
		(0.59)		
State Unemployment Inc. Coded (-)			-0.22	
			(0.37)	
State Unemployment In-Party Coded (-)			0.19*	
			(0.09)	
State Unemployment Change Inc. Coded (-)				-0.87
				(0.68)
State Unemployment Change In-Party Coded (-)				1.05
				(0.60)
State Std.	6.29	6.41	6.41	6.33
Year Std.	0.87	0.97	0.97	1.16
N	481	481	481	481

*p < .05; **p < .01; ***p < .001

Table A2.7 Consumer Sentiment MLM Results

	Incumbent Vote Share
Constant	80.57***
	(3.15)
Appointed Incumbent (-)	-0.76
	(0.67)
Quality Challenger (-)	-7.77***
	(1.00)
Number of Candidates (-)	-24.56***
	(2.62)
Major Party (+)	-0.35
	(1.31)
Nonpartisan (-)	1.95
	(2.57)
Partisan (-)	2.88
	(3.05)
District (+)	3.11
	(2.78)
Female (+)	0.76
	(0.72)
Female Challenger (-)	-5.27***
	(1.27)
Minority (-)	-0.94
	(0.93)
Minority Challenger (+)	3.49
	(2.16)
Natl Murder Rate (-)	-0.54
	(0.35)
State Murder Rate (-)	-0.74**
	(0.24)
Consumer Sentiment Inc. Coded (+)	2.31
	(2.19)
Consumer Sentiment In-Party Coded (+)	1.31
	(0.84)
State Std.	6.7
Year Std.	1.08
N	478

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A2.8 Executive Approval MLM Results

	Incumbent Vote Share	
	(1)	(2)
Constant	81.56***	80.95***
	(2.99)	(3.14)
Appointed Incumbent (-)	-0.99	-1.36
	(0.65)	(0.78)
Quality Challenger (-)	-7.69***	-7.66***
	(0.98)	(1.16)
Number of Candidates (-)	-23.33***	-22.06***
	(2.59)	(2.88)
Major Party (+)	1.40	-0.13
	(1.34)	(1.57)
Nonpartisan (-)	0.92	3.16
	(2.53)	(2.83)
Partisan (-)	2.86	4.43
	(2.96)	(3.22)
District (+)	3.86	3.62
	(2.67)	(2.50)
Female (+)	0.86	0.74
	(0.71)	(0.86)
Female Challenger (-)	-5.24***	-5.02***
	(1.25)	(1.45)
Minority (-)	-0.66	-0.49
	(0.92)	(1.10)
Minority Challenger (+)	5.01*	5.10
	(2.17)	(2.72)
Natl Murder Rate (-)	-0.83*	-0.64
	(0.32)	(0.33)
State Murder Rate (-)	-0.73**	-0.81**
	(0.23)	(0.25)
Pres. Approval Inc. Coded (+)	1.16	
	(3.20)	
Pres. Approval In-Party Coded (+)	3.55***	

	(0.87)	
Gov. Approval Inc. Coded (+)		0.03
		(0.03)
Gov. Approval In-Party Coded (+)		0.004
		(0.01)
State Std.	6.28	4.94
Year Std.	1.07	0.75
N	479	380

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Appendix to Chapter 3:

In this chapter, I tested the hypothesis that the impact of the case facts would vary by reselection method. I did this by running the model on subsets of the data for each different reselection method and comparing the results and predicted probabilities across models. An alternative way to test for the differential impact of the case facts across reselection methods, is to look at an interactive model. Thus in the model below I interact the indicator for judges reselected in partisan, nonpartisan, and retention elections with the sum-of-case-facts variable. These interactions compare the effect of the case facts in those systems to a system in which judges are appointed. Negative coefficients indicate that the sum of the case facts matters more in that system when compared to a system in which judges are appointed. In the model below, the interaction for both judges reselected in partisan and retention elections is estimated to be negative, although it is only significant in the former. This indicates that judges reselected in these systems may be influenced more by the case facts than judges who are reappointed. This is contrary to the hypothesis of this chapter, which posited that the case facts would matter the most when judges are reappointed, but in line with the results from the chapter which showed the steepest drop in the probability of voting to overturn occurred when judges are selected in partisan, followed by retention elections.

Table A3.1 Interactive Logit Results with Clustered Standard Errors

	Vote
Constant	-0.24
	(0.91)
Next Election (-)	-0.11
	(0.09)
Dem Retire (+)	0.12
	(0.22)
Rep Retire (-)	-0.69*

	(0.31)
Party (+)	0.49**
	(0.17)
Dem Lame Duck (+)	0.22
	(0.50)
Rep Lame Duck (-)	0.08
	(0.30)
SCOTUS (+)	2.22*
	(0.94)
Public Opinion (-)	-1.16
	(1.02)
Sum Facts (-)	-0.14**
	(0.04)
Partisan (-)	0.07
	(0.56)
Nonpartisan (-)	-0.18
	(0.57)
Retention (-)	0.29
	(0.60)
Partisan*Sum Facts (+)	-0.19***
	(0.06)
Nonpartisan*Sum Facts (+)	0.05
	(0.09)
Retention*Sum Facts (+)	-0.001
	(0.10)
N	12,495

*p < .05; **p < .01; ***p < .001

In this chapter I also used clustered standard errors to account for the clustering between the observations. An alternative strategy for dealing with clustered observations is to use a multilevel model. In this appendix, I present multilevel models with random intercepts for different states and different years. The results of the multilevel models are somewhat similar to the results presented in the chapter obtained using OLS with clustered standard errors, because

the effect of the cases facts is negative across all of the models. The difference, however, is that in the multilevel models this coefficient is significant in all of the models except for the model where judges are reappointed. This is contrary to expectations, which suggested that the effect of the case facts would matter the most when judges are appointed.

Table A3.2 MLM Logit Results

	Vote			
	(1)	(2)	(3)	(4)
	Appointed	Partisan	Nonpartisan	Retention
Constant	7.33**	3.78**	-2.83	-0.60
	(2.52)	(1.43)	(1.50)	(1.13)
Sum Facts (-)	-0.08	-0.30***	-0.16**	-0.17***
	(0.09)	(0.04)	(0.05)	(0.04)
Next Election (-)	0.01	-0.08	-0.14	-0.09
	(0.18)	(0.08)	(0.10)	(0.08)
Dem Retire (+)	0.02	0.21	-0.68	-0.45***
	(0.21)	(0.17)	(0.46)	(0.12)
Rep Retire (-)	-1.58	-0.78	-1.78**	-0.21
	(0.81)	(0.54)	(0.66)	(0.19)
Party (+)	0.40	0.34*	0.24*	0.88***
	(0.27)	(0.17)	(0.12)	(0.10)
Dem Lame Duck (+)		0.43	-0.35	
		(0.33)	(0.67)	
Rep Lame Duck (-)		0.28	-0.64	
		(0.69)	(0.69)	
SCOTUS (+)		2.03***	22.03	3.71***
		(0.25)	(128.00)	(0.64)
Public Opinion (-)	-10.50**	-6.32***	2.50	-0.54
	(3.32)	(1.88)	(1.83)	(1.41)
State Std.	0.89	0.55	0.55	0.42
Year Std.	1.77	0.56	0.56	0.49
N	1,255	4,026	3,022	4,151

*p < .05; **p < .01; ***p < .001

Appendix to Chapter 4

An alternative strategy to the sub-setting of the data based on reselection method used in the main chapter is to run a model on the combined data and examine the interactions between the public opinion variables and the different reselection indicators. In the model below, I interact reselection indicators for partisan, nonpartisan, and retention elections with the black and non-black public opinion variables. A negative coefficient on these interactions would mean that public opinion has a stronger effect in these systems compared to judges who are reappointed, and a positive coefficient would indicate the opposite. The positive coefficients on the interactions between partisan, nonpartisan, and retention elections with the black-public-opinion variable suggests that black opinion matters more when judges are appointed. The negative interactions between these selection indicators and non-black opinion suggests that non-black opinion matters less when judges are appointed.

Table A4.1 Interactive Logit Results with Clustered Standard Errors

	Vote
Constant	-7.93
	(13.06)
Sum Facts (-)	-0.19***
	(0.05)
Next Election (-)	-0.06
	(0.07)
Dem Retire (+)	0.04
	(0.23)
Rep Retire (-)	-0.65*
	(0.31)
Party (+)	0.63***
	(0.17)
Dem Lame Duck (+)	-0.09

	(0.51)
Rep Lame Duck (-)	0.15
	(0.32)
SCOTUS (+)	4.02***
	(0.45)
Non-Black Opinion (-)	13.70
	(22.79)
Partisan (-)	9.78
	(13.22)
Nonpartisan (-)	14.08
	(13.22)
Retention (-)	10.84
	(13.43)
Black Opinion (-)	-8.13
	(11.33)
Partisan*Non-Black Opinion (-)	-15.91
	(22.98)
Nonpartisan*Non-Black Opinion (-)	-24.11
	(22.93)
Retention*Non-Black Opinion (-)	-19.61
	(23.21)
Partisan*Black Opinion (+)	5.54
	(11.37)
Nonpartisan*Black Opinion (+)	10.50
	(11.35)
Retention*Black Opinion (+)	9.98
	(11.38)
N	12,170

*p < .05; **p < .01; ***p < .001

For comparison purposes, I also ran the models in this chapter as multilevel models that allow for random intercepts by state and year. These models were estimated using the bobyqa optimizer in R, as the default settings gave convergence warnings. The results from these models differ slightly from the results presented in the chapter. The biggest difference between these

results and the results presented earlier, is that the estimate of the effect of black opinion in retention elections is estimated to be negative here. This suggests that it may just be judges reselected in nonpartisan elections who favor majority opinion over minority opinion.

Table A4.2 MLM Logit Results

	Vote			
	(1)	(2)	(3)	(4)
	Appointed	Partisan	Nonpartisan	Retention
Constant	-0.71	-0.21	3.40	-1.58
	(5.85)	(2.13)	(2.79)	(1.66)
Sum Facts (-)	-0.004	-0.27***	-0.16**	-0.19***
	(0.09)	(0.04)	(0.05)	(0.04)
Next Election (-)	-0.02	-0.06	-0.14	-0.10
	(0.19)	(0.09)	(0.10)	(0.08)
Dem Retire (+)	-0.04	0.22	-0.79	-0.45***
	(0.21)	(0.17)	(0.49)	(0.13)
Rep Retire (-)	-1.87*	-0.73	-1.63*	-0.19
	(0.90)	(0.55)	(0.65)	(0.20)
Party (+)	0.38	0.29	0.23	0.89***
	(0.28)	(0.17)	(0.12)	(0.10)
Dem Lame Duck (+)		0.06	-0.43	
		(0.36)	(0.67)	
Rep Lame Duck (-)		-0.05	-0.54	
		(0.70)	(0.69)	
SCOTUS (+)		3.77***	17.77	3.65***
		(0.46)	(21.95)	(0.64)
Non-Black Opinion (-)	11.84	2.59	-7.78*	1.60
	(10.57)	(2.79)	(3.91)	(2.55)
Black Opinion (-)	-17.27**	-5.68***	4.24*	-1.34
	(6.11)	(1.12)	(1.65)	(1.26)
State Std.	0.92	0.59	0.59	0.37
Year Std.	2.14	0.36	0.36	0.49
N	1,216	3,797	3,009	4,107

*p < .05; **p < .01; ***p < .001