

Stare Decisis and the Electoral Connection: Do Retention Systems Affect Judges' Deference to Precedent?*

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Abstract

Using data from nearly 5,000 votes cast by more than 400 judges in courts of last resort from all fifty states, we investigate whether there is a relationship between a state's judicial retention method and the likelihood that a judge votes to join a precedent-overturning majority. We find that relative to judges retained by institutions such as judicial commissions or state legislatures, those retained via either partisan or retention elections are significantly more likely to join majorities that overturn precedent. Most of this effect is due to behavior in high-profile cases that garner media attention. We find little evidence that an impending election moderates these effects. Finally, we find no evidence that judges retained via non-partisan elections treat precedent differently than their institutionally retained colleagues.

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Introduction

A majority of American states has long favored some level of voter input with respect to whether judges should continue their service. In some, voters decide whether to retain judges via a yes/no vote, while in others, judges run in more typical elections against opponents.¹ In a substantial number of states, voters have no input on judicial retention at all; judges in some serve until they reach a mandatory retirement age, while in others they serve defined terms and are re-appointed by some body of state government such as a legislature or judicial selection committee. Given the wide range of methods employed, there is no shortage of debate regarding the optimal method of judicial retention. Since 2010, at least five states have discussed (or made) significant changes to the manner in which judicial elections should be funded and/or conducted, often on the basis that proposed changes would make the judiciary more capable, independent, or democratic. These claims are frequently grounded on empirical evidence, since over the same period, the relationship between selection and/or retention systems and a range of outcomes has been a growing topic of scholarly research (e.g., [Brace, Yates and Boyea 2012](#); [Caldarone, Canes-Wrone and Clark 2009](#); [Choi, Gulati and Posner 2010](#); [Goelzhauser and Cann 2014](#); [Hall 2001, 2007](#); [Bonneau and Hall 2009](#); [Yates, Tankersley and Brace 2010](#)).

Existing studies in this area have made great advances toward understanding the manner in which selection and/or retention systems affect judicial behavior, but at least one question has escaped substantial scrutiny: Does knowing that voters (as opposed to government institutions) will decide whether they will continue their service affect judges' willingness to break with established precedent? This question transcends academic debates. Claims of "activism" have become commonplace in judicial politics, and are particularly salient among voters on the right ([Gibson and Caldeira 2009](#)). While judges' adherence to precedent is not the only criterion by which they might be deemed "activist,"

¹As of 2019, twenty-two states employ some type of judicial election in which judges face an opponent and voters choose between them. In fourteen of these, judicial elections are officially non-partisan.

it is certainly one important factor. Given the apparent prominence of judicial restraint in public opinion about judges' performance, it is important to understand what link, if any, exists between the manner in which a judge retains her seat and her willingness to deviate from *stare decisis*.

There are clear theoretical reasons to expect that such a link does exist, though the direction of the relationship is not immediately clear. It could be that voter-retained judges are more likely to support overturning precedent than institutionally retained courts; if public opinion is not consistent with existing precedent, elected judges may vote in-line with the former for fear of invoking the wrath of a vengeful electorate. However, there are also compelling arguments to expect the opposite result: Judges who never face voters are possibly less vulnerable to attacks that they are engaged in activist behavior. They may therefore feel freer to depart from precedent, even when doing so is unpopular. Or, perhaps neither of these scenarios is true, and the norms of precedent adherence transcend electoral incentives.

Despite the potential of retention systems to shape the manner in which courts treat precedent, to the best of our knowledge, this question has not been empirically answered in existing work. We evaluate judge-level evidence from *State Supreme Court Data Project* (SSCDP) data from nearly 5,000 votes cast by more than 400 judges in courts of last resort from all fifty states. We seek to determine whether there is a relationship between judicial retention method and judges' propensity to join a majority that overturns precedent. We find that relative to institutionally retained judges (those whose continued service is not decided by voters), justices who are retained via partisan or retention elections are significantly more likely to join majorities overturning established precedent. We find little evidence that an impending election moderates these effects. However, case salience is an important moderator of this relationship, as most of the effects are driven by shifts in high-salience cases. We find no evidence that judges retained via non-partisan elections treat precedent differently than their institutionally retained colleagues.

Our findings contribute to a growing literature that examines the relationship between selection and/or retention systems and judge-level behavior (Hall 1992, 1995, 2014b; Huber and Gordon 2004). The paper proceeds as follows: In the next section, we describe existing literature linking retention systems with judicial outcomes, and detail reasons to expect that voter-retained judges might be more (or less) deferential to precedent than institutionally retained ones. Next, we describe the data and methods we use to answer our question, followed by a description of the findings. We close with a discussion of the results.

1 Judges, Precedent, and the Electoral Connection

Judges typically face more behavioral constraints than legislators. For instance, while legislative majorities can at least initiate debate on policy that suits their preferences, judges do not even have total control over the questions on which they will rule. Moreover, *stare decisis* further restrains judges in important respects. Even when their preferences might render them predisposed to ruling in a certain way on particular policy matters, the vitality of a precedent (that is, the inherent legal authority therein) provides a powerful force that can condition a judge's ruling (Hansford and Spriggs 2006). The manner in which judges treat precedent is therefore a key component of their behavior.

Indeed, deference to precedent is an important element in a conception of judges as political actors who deploy their court's institutional power to create policy that aligns with their preferred outcomes (Segal and Spaeth 2002). For instance, Hansford and Spriggs (2006) find that the United States Supreme Court treats precedent in such a fashion as to shift policy toward its justices' preferences. Yet, federal judges enjoy lifetime appointments in good behavior, and are therefore immune from any immediate electoral repercussions from their decisions. In contrast, state judges almost always serve defined terms, and many must stand for some kind of popular election if they wish to continue

service. We believe this creates different incentives for judges seeking reappointment, and warrants further investigation of state judges' behavior in key areas.

One such area is treatment of precedent. Despite the centrality of *stare decisis* in their work—coupled with the clear potential of popular retention to shape the manner in which they approach difficult decisions—the question of whether voter-retained judges are more (or less) likely to depart from precedent is almost entirely unanswered. Recent work has found no difference between appointed and elected courts with respect to the U.S. Supreme Court subsequently reversing their decisions (Owens et al. 2015), which suggests that regardless of selection system, state court justices are equally committed to making good law. However, we are aware of no empirical studies that seek to determine whether the method by which judges are retained shapes their willingness to join majorities that overturn their *own* previous rulings.

Our central goal is therefore to determine whether the manner in which judges are retained for an additional term on a state court of last resort affects the probability that they will depart from precedent. Goelzhauser (2018) cautions that the myriad systems that states employ can lead to questionable classifications, and advises both careful theorizing and transparency when it comes to comparing outcomes in various judicial systems.² For instance, in Alabama and seven other states, judges run in partisan elections akin to most legislative elections in the United States. In fourteen states, judges run in non-partisan elections. However, while any sitting judge must run for re-election in these states, she may have obtained her seat through a range of mid-term appointment mechanisms, including being a candidate of the governor's sole choice (as in Alabama) or one whom the governor selects from a commission's list (many other states).³ Thus, how a judge obtains her position does not always indicate how she will keep it.

²For a thorough description of selection systems, see Chapter 2 in Hall (2014a)

³Similarly, in California the governor appoints Supreme Court candidates, who are then confirmed by an independent commission. After serving a twelve-year term, California Supreme Court judges face a retention election in which voters simply cast a vote on whether to retain them or not. In other states, a commission offers the governor a list of qualified candidates from which one is chosen, and a similar retention election is held after the term expires.

In undertaking this inquiry, we therefore focus on the method of judicial *retention* that states employ, as opposed to their system of judicial *selection*. A judge who knows she will meet an opponent in an open election—in which her decisions may be publicly criticized via advertising or social media—might approach overruling precedent differently than a judge who must re-submit to a legislative confirmation that may largely escape public notice. Legislators may themselves be susceptible to claims about a given judge’s “activism,” but a judge might reasonably expect the opportunity to explain the nuance of any given decision in an institutional setting. In short, we expect the audience that judges must please to secure reappointment to be a potentially important force shaping judges’ orientation to *stare decisis*.

We therefore place states in four categories based on the mechanism by which judges are retained—regardless of how they are selected.⁴ In our classification system, “lifetime appointment” judges are selected by the executive or legislative branch (or some combination thereof), and continue to serve in good behavior with no scheduled evaluation by either voters or other branches of the state government. We designate judges as being subject to “institutional retention” if after being initially selected, they are subject to scheduled subsequent re-evaluation or confirmation after some period of time, by some part of the state government. There are a range of institutional retention practices among the nine states we place in this category. In some states, judges re-apply to a nominating commission at the end of their term; in others, the state legislature re-confirms them—sometimes in cooperation with the executive. Nonetheless, we group judges in this category because while on the bench, judges know that at some point they must convince an elite audience—however comprised—of their capability in order to continue their service. For our purposes, this knowledge is arguably more important than *which* body of elites will be making the decision.

We group judges for whom the method of retention is an election into three categories.

⁴We describe this grouping in more detail below, and in Table 1.

“Voter retained” judges are those who face uncontested retention elections at the end of their terms. “Partisan elected” and “Non-partisan elected” judges run in more typical elections against opponents, either with or without partisan affiliation. Because voters ultimately decide their fate, these judges might see their retention prospects as endangered if they stray too far from public opinion, or if they leave themselves vulnerable to popular appeals about their “toughness” or “activism.” Such charges could be levied by opponents and/or interest groups, regardless of the election type.

That said, we believe it is important to distinguish between the various kinds of elections that judges face. For instance, [Schaffner, Streb and Wright \(2001\)](#) find that voters fall back on incumbency in the absence of party labels in elections for a range of offices; this may make judges running in non-partisan elections more confident in their reelection than partisan judges—and therefore, more similar to the institutionally retained group. [Caldarone, Canes-Wrone and Clark 2009](#) also demonstrate important differences between non-partisan and partisan judges; among them, non-partisan judges’ rulings hew more closely to public opinion than those of partisan ones. So while we posit that an electoral connection may guide judges in part as they consider whether to overrule existing precedent, we allow for the possibility that different election types may impart disparate effects on how judges treat precedent.

The “electoral connection” necessarily assumes analogues between the behavior of judges and legislators. Political science has long observed the linkage between elections and legislative behavior, recognizing that representatives tend to take public opinion into account before voting on highly visible bills because they want to avoid angering voters in their next election (e.g. [Hogan 2008](#); [Mayhew 1974](#); [Miller and Stokes 1963](#)). It therefore seems reasonable to expect judges to behave differently if they believe that certain actions may prove difficult to explain to voters. Indeed, one of the major arguments in support of having judicial elections at all is to hold the judiciary accountable to public sentiment. For instance, [Posner \(2005\)](#) refers to an election as a “performance review”

whereby voters periodically assess the quality of a judge's work, writing that "a judge who defies public opinion is not only a judge unlikely to be reelected; he is, it can be argued, however paradoxically, a bad, even a usurpative, judge."

It may not be immediately clear however that the electoral connection is the powerful force for judges that it appears to be for legislators, since judicial elections are often thought of as low-salience affairs in which few people vote (Hall and Aspin 1987). Yet, high-quality challengers spending large sums are capable of lowering information costs, as are partisan labels that facilitate easier choices for voters; in actuality, salient, competitive judicial elections are hardly uncommon (Baum and Klein 2007; Cann and Wilhelm 2011; Dubois 2008; Hall 2001; Bonneau and Hall 2009; Kritzer 2011). Furthermore, there is growing evidence that voters can find sufficient information to make an informed choice via the material disseminated during a judicial election campaign. The increasingly expensive and competitive environment of judicial elections in many states over the last decade has led to increased levels of voter participation, and voters may be more knowledgeable of the judiciary in such conditions (Hall 2007; Bonneau and Hall 2009). Voters also appear to be willing to use what they learn from elections in their preference formation; for instance, Hall (2001) found that incumbent judges can be more vulnerable to defeat in elections when a state's murder rate is high. Similarly, voters have demonstrated a relatively high level of sophistication in making choices between judicial candidates (Baum and Klein 2007; Hall 2001). The frame of mind for elected judges therefore seems straightforward: If they want to keep their seats, they would do well not to anger voters.

We should therefore expect judges whose continued service depends on voters' judgment to respond both to public opinion and electoral incentives. In the former area, previous research has found public opinion to be a powerful force in shaping judicial behavior; indeed, even at the United States Supreme Court (Casillas, Enns and Wohlfarth 2011). At the state level, existing work has found apparent links between selection sys-

tem and case outcomes. For instance, elected judges appear more likely to follow public opinion when deciding a range of cases, including those dealing with capital punishment (Brace and Boyea 2007, 2008) and abortion issues (e.g. Brace, Hall and Langer 2001; Caldarone, Canes-Wrone and Clark 2009), as well as in cases that receive more media coverage (Cann and Wilhelm 2011).

Moreover, elected judges' willingness to serve a demanding public appears to transcend case disposition. While Goelzhauser and Cann (2014) found no differences in opinion clarity across selection systems, elected courts handle certain types of cases more efficiently (Goelzhauser 2012), even while writing significantly more opinions than judges who do not run in elections (Choi, Gulati and Posner 2010). In total, these findings are consistent with a self-interested elected judiciary dispatching cases with an eye on serving constituents, akin to an elected legislator. Indeed, the legislator is the point of comparison for many voters, who see judges' activity as similar to legislating—and who therefore see elections as a legitimizing force for the judiciary as an institution (e.g. Gibson 2012).

Much like the effect of impending elections on legislative behavior (e.g., Miller and Stokes 1963), judges may therefore feel constrained in certain circumstances if they know they will answer to voters in the near future, and there is evidence that this is the case. Huber and Gordon (2004) find that trial judges are more likely to render punitive sentences nearer their re-election, out of a recognition that the public is more likely to notice (and take issue with) a light sentence than one that is too long (Roberts and Edwards 1989; Roberts and Doob 1990). Similarly, Hall (1992) finds that in states with short terms, even liberal judges are less likely to dissent when a majority upholds the death penalty (a popular position among the public at the time), presumably out of fear of an opponent portraying them as lacking a “tough-on-crime” approach. Subsequent research found that electoral factors such as proximity to Election Day and previous vote margins also affect judges' voting behavior in capital cases (Hall 1995). Finally, Hall (2014b) finds that the relationship between electoral context and judges' voting in capital cases is di-

minished significantly when judges know they will not be running for re-election due to term limits. Thus, there is growing evidence that judges likely make decisions with an eye on the circumstance of their next election. It stands to reason that whether to depart from existing precedent will be part of this calculus.

With respect to how judicial retention systems may influence justices' behavior when it comes to how they treat precedent, there are at least three possibilities. The first is that there is no link between retention systems and observance of *stare decisis* at all. Perhaps all judges view themselves as primarily accountable to their peers, the legal community, and their role in the profession, which might make them favor deference to existing opinions—regardless of how they will retain their position (e.g. [Gibson 1978](#); [Posner 2005](#)). Adherence to precedent is, after all, the default setting guiding the disposition of a case; there is perhaps no stronger norm in the legal community. Justices tend to see adherence to precedent as contributing to a stable, predictable legal system. United States Supreme Court Justice Stephen Breyer communicated this viewpoint in his majority opinion in *Randall v. Sorrell* (548 U.S. 230), writing, “*Stare decisis*...avoids the instability and unfairness that accompany disruption of settled legal expectations. For this reason, the rule of law demands that adhering to our prior case law be the norm. Departure from precedent is exceptional and requires special justification.” Thus, it is possible that electoral incentives are not sufficiently strong to overcome the inherent power of precedent in the minds of many judges.

The second possibility is that elected judges are less willing to overrule precedent, on average, than those who do not directly face voters. The latter certainly enjoy more political independence, leaving them freer to overturn the legal precedents they find irrelevant, erroneous, or disagreeable without fear of being characterized as radical by a future election challenger. Indeed, the “judicial activism” attack is frequently employed in elections when courts break from established precedent in opinions with which one party or the other disagrees: A Lexis-Nexis search we conducted in early 2017 indicated

that the term had been used in election-related news stories more than 5,000 times since 2010. A healthy majority of Americans can provide an approximate definition of “judicial activism,” though Republicans are more likely both to express an opinion of “activism” and to reject it as a desirable trait in a judge (Gibson and Caldeira 2009). As such, the term is likely a valuable heuristic in communicating to voters that a judge is “liberal” in the sense that she is willing to break from existing law and legislate from the bench. Indeed, in 2015 Gallup data suggested that trust in the judicial branch had fallen to an all-time low, with most of the drop driven by Republican respondents (Jones 2015). Elected judges might therefore be reluctant to overturn precedent to avoid the “activist” label during their next election.

Third, it is also theoretically plausible that we may find elected judges are *more* likely to overrule existing legal precedents. As noted above, there is ample evidence that elected judges are more responsive to the opinion of the mass public. As such, judges who periodically face voters may be more willing to craft legal conclusions that reflect “the will of the people,” even if public opinion is at odds with judicial precedent. Particularly on legal questions in fluid areas on which the public’s thinking has evolved over a period of time, elected courts may therefore be particularly susceptible to overturning (or qualifying) their previous decisions to stay in the good graces of the voters who allow them to remain on the bench. Similarly, as institutionally retained judges’ authority is less directly rooted in the democratic process, they may be wary of allegations of judicial overreach and defer to elected legislators to remedy unpopular or outdated decisions. Furthermore, if, as some suggest, institutionally retained judges are more qualified and/or capable than elected judges (Posner 1993, 2005), the former may also be more skillful at integrating precedents into their decisions while obtaining their desired result.

It stands to reason that retaining their position is a strong motive for most judges, which should encourage them to avoid action that invokes the wrath of voters. This shapes expectations for the potential of judicial selection systems to impact judges’ will-

ingness to overturn precedent. Perhaps this means that judges will prioritize the public will over the doctrine of *stare decisis*, or perhaps it means they will err on the side of restraint out of fear that their actions will be interpreted as an overreach. Below, we therefore engage a straightforward question: Do judges whose continued tenure depends on voters treat precedent differently than those who are institutionally retained?

Data and Method

We employ data from the *State Supreme Court Data Project* (Brace and Hall 2009, hereafter SSCDP), which contains a wealth of information from more than 20,000 decisions in state courts of last resort between 1995 and 1998. Given the potential for high levels of public and/or media scrutiny to affect judicial behavior (Cann and Wilhelm 2011), we begin by including in our data the 413 cases in the SSCDP that Vining and Wilhem (2010; 2011) found to be “salient;” that is, the opinion was covered in a state’s largest newspaper on the day after the opinion was released. We add to these cases data from a random sample of 500 additional, non-salient cases that we drew from the remainder of the SSCDP. Thus, we begin with basic information from more than 900 cases, slightly less than half of which were salient. For a state-by-state count of case-votes, see Appendix Table A2.

For each of the cases in our sample, we downloaded the court’s opinion from the Lexis database and used it to code new variables reflecting a number of attributes. We classified a majority as overturning a precedent if the opinion explicitly contradicts a previous holding.⁵ Overturning precedent is likely the clearest measure of a majority’s willingness to follow *stare decisis* in a given case, as it necessarily directs future courts to rule in accordance with the precedent established in the new case—and to ignore the old one. As is clear from Justice Breyer’s thoughts presented in the previous section, many judges see overruling precedent as a legally disruptive act, and doing so is probably the typical

⁵We use the terms “overturning” and “overruling” interchangeably.

basis by which treatment of precedent would expose a judge to criticism on the basis of her “activism.” We therefore created a dichotomous variable for whether the court overruled existing precedent in its opinion.⁶ To determine if precedent was overruled, we employed the following search terms: “overruled,” “overruling,” “overrule,” and “decline.” We then backwards-Shepardized to determine how Lexis coded the case. Since we are interested in how courts treat existing precedent, we exclude first-impression cases from all analysis.

After coding case-level information, we re-shaped the SSCDP data to the judge-level. Each observation in our data therefore reflects a “case-vote,” or how a given judge voted to dispose of a given case. In total, our initial case sample includes nearly 5,000 votes cast by about 440 judges from courts of last resort in all 50 states. Importantly, our coding scheme for the treatment of precedent allows us to determine whether the *majority* in a given case voted to overrule precedent. Shifting the unit to each individual judge’s vote therefore imparts some advantages over a court-level analysis, as well as some disadvantages. With respect to the former, a growing body of work recognizes that much of the relationship between electoral context and judicial behavior is best observed at the judge-level (Hall 1992, 1995, 2014b; Huber and Gordon 2004). For instance, we might expect voter-retained judges to be more conscious of how opponents might characterize decisions with voters, particularly when re-election draws near. Thus, focusing on the behavior of an individual judge—as opposed to the entire court—allows for a finer-grained measure of the link between elections and treatment of precedent.

However, the manner in which an individual judge votes, coded with respect to the majority’s treatment of precedent in a case, may not fully capture the judge’s views on whether a precedent should be overturned. Judges do not always write concurring or dissenting opinions, and so it is sometimes impossible to glean their views on precedent from the manner in which they voted to dispose of a case. If, for instance, a majority of

⁶We also coded for whether the case involved statutory interpretation, which functions as one of our control variables (see below).

the court adheres to precedent in a given case, it is not obvious that a dissenting judge's vote should be interpreted as an intent to deviate, as there are numerous other reasons why that judge might not join the majority. Since our only clear signal with respect to precedent treatment is the majority's handling of precedent in its opinion, our judge-level binary dependent variables take the value of one if the judge *joins a majority* to overrule precedent, and zero otherwise.

We designate five independent variables with particular importance to our analysis, indicating judges who faced different retention systems during the period covered by SSCDP data who fall into the classifications we describe above: Lifetime Appointment, Institutional Retention, Voter Retention, Non-Partisan Elections, and Partisan Elections. Judges with a "lifetime appointment" are those in three states who serve in good behavior, without the need for re-confirmation at end of a fixed term.⁷ "Institutionally retained" judges are those in nine states whose continued service will be decided by a body other than voters, such as a legislature, governor, or judicial appointment committee. In another sixteen states—regardless of how they are initially appointed—voters are asked whether to retain judges in uncontested elections via a yes/no vote after a fixed period of time; we refer to these judges as facing "voter retention." Finally, twenty-two states follow a contested election model in which candidates run against opponents. In fourteen of these states, the elections are not officially partisan, although some states allow party endorsements or for candidates to make ballot statements signaling their ideological position. Nevertheless, we distinguish these officially non-partisan elections from those in another eight states, where judicial candidates affiliate with political parties. We coded indicator variables for each of three types of system, which serve as the key independent variables in most of our analysis. The states in each classification can be found in Table 1.

⁷We include Massachusetts and New Hampshire in this category. Service on the high courts of these states is not truly "for life," as both have a mandatory retirement age, but judges are not subject to re-confirmation during the tenure of their service.

Table 1: State Retention Classifications, 1994-1998

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

There are other factors at both the case- and judge-level that might affect how precedent is treated beyond how a judge retains her place on the bench; for instance, a case's notoriety and/or complexity also have implications for precedent. The SSCDP features several case-level covariates relevant for our study. Generally, we expect a judge's propensity to deviate from precedent to be negatively related to the complexity of the case. Thus, we anticipate a negative correlation between overturning precedent and both an indicator for whether there were multiple parties to a case and the total legal issues decided in the case.⁸ In contrast, we expect more visible and/or consequential cases to be more likely to result in a judge voting to overrule precedent. As such, we expect a positive correlation between precedent overrule and indicators for whether amicus briefs were filed, whether

⁸The number of legal issues demonstrates considerable right skew. We therefore transform this variable by adding one to each value and then taking the natural log.

a case comes from a criminal court, and whether the state government is a party, as well as the variables we coded for case salience (see Vining and Wilhem (2010; 2011)) and whether the case involved a statutory interpretation.

At the judge-level, we employ controls for the judge's length of service. Judges who are more senior may feel more confident in their interpretation. Alternatively, perhaps longer service increases the likelihood that a judge encounters precedent that she had a hand in establishing. Bonica and Woodruff (2014) note the year in which each judge entered the court. We use this information to create a "length of service" variable, which is the year in which the judge took her seat subtracted from the year in which a case was decided.⁹

We also account for retirements. The intuition here is straightforward: If judges know that they will not be facing re-confirmation or an election, they might shift the manner in which they vote regarding precedent. We define a judge as having "retired" if she willfully left the bench—either at or before the end of her term.¹⁰ Though we cannot know for sure when a judge decides to retire, we expect that the typical judge gives retirement some thought before leaving. We therefore code cases as having been decided by a retiring judge if they were disposed within one year of a judge ultimately leaving by choice.

The purpose of our investigation is to determine whether, compared to judges who do not need voter approval to continue in their positions, those who must be either re-elected or voter-retained are differently willing to join a majority that overturns a precedent. To that end, one additional methodological consideration remains. While grouping courts (and judges) by retention method is a useful means of comparison, the judicial selection system in a given state may itself be correlated with factors such as political culture or mass (or elite) ideology, which could also influence judges' judicial outlook. Judges, after all, are products of their states' political culture. Or, perhaps judges in certain systems—

⁹We log this variable in the same fashion as the number of legal issues, for the same reason.

¹⁰We do not include judges who were defeated, who died, or who were elected/elevated to other positions as having retired because we believe that self-knowledge of the impending departure is an important theoretical element that must be captured.

by virtue of some underlying variable such as crime rates and/or laws, racial demographics, or dominant industries—are simply more likely to adjudicate case law that requires a particular disposition to precedent. If either of these is true, any results we find could be the result of spurious inference. We believe it is therefore prudent to compare the distribution of observed covariates between judges who face voters and those who do not, with respect to the aspects of the cases on which they vote, the courts on which they serve, and the judges themselves.

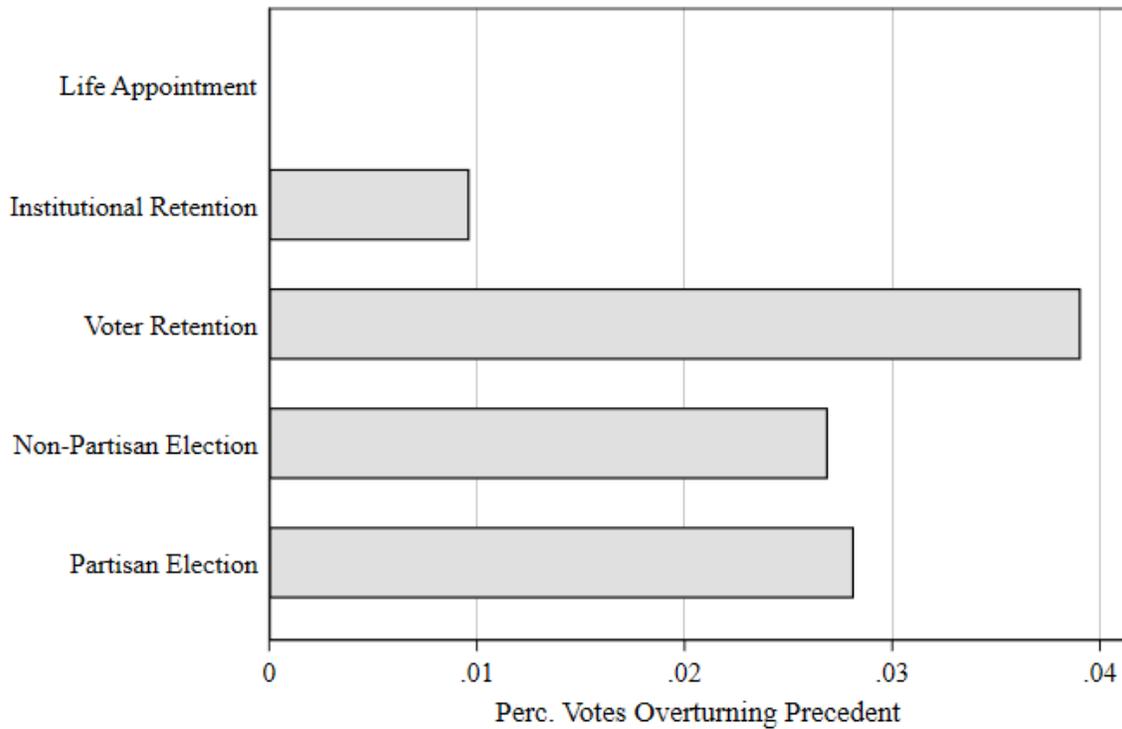
To overcome this problem, we borrow from the language of experiments in designating three “treatment” groups: judges retained via partisan, non-partisan, and retention elections. Our aim is to compare each of these groups against a “control” group of judges who do not require voter approval to continue in their positions. Preliminary analysis sheds some light on how the control group should be constructed. Figure 1 depicts the percentage of votes that justices in each type of system cast to join a majority decision that overruled precedent in cases of first impression. There are two trends apparent.

First, Figure 1 reveals a clear disparity in disposition toward precedent between judges who face voters and those who do not. For instance, the rate at which institutionally retained judges vote to overrule precedent is less than half the rate for any category of judge that is retained by voters in some way. Second, the gap between elected and life-appointed judges is even greater because in our sample, we found no votes cast to overrule a precedent by the latter. While preliminary evidence therefore suggests that retention system might shape judges’ willingness to overturn precedent, this lack of variation among life appointees poses some methodological difficulties since we cannot perform statistical tests on judges in this category.¹¹

We therefore construct all control groups wholly from institutionally retained judges, which we believe is appropriate for a number of reasons. Institutionally retained judges

¹¹While we do believe that Figure 1 should be taken as evidence that lifetime appointees are exceptionally deferential to precedent, we also do not believe that lifetime appointees have *never* voted to overrule precedent. The estimate in Figure 1 clearly results from sampling error and might be expected with rare events.

Figure 1: Percentage of Judge-Votes to Overturn Precedent in Cases of First Impression, by Retention Method, Full Sample



Note: Weighted estimates, correcting for oversampling of salient cases.

serve distinct terms with a mechanism of retention, which gives their term a natural “cycle” that mirrors those of elected judges. However, they differ from judges in our three elected groups because they never appear on a ballot of any kind, and instead face an elite body such as a state legislature or judicial commission. Finally, all three lifetime-appointment states are in New England, whose distinct political culture may render it suboptimal for comparison to a broader state pool.

This last point is a reminder that as noted above, covariate imbalance between voter- and institutionally retained judges would pose potential problems for inference about the effect of the retention systems, since there would be a higher likelihood that the “treatment” variables are related in some way to other covariates (Ho et al. 2007; Sekhon 2009). We therefore seek covariate balance before proceeding with the analysis. To that end,

we pre-processed the judge-vote dataset with a genetic matching algorithm intended to improve the balance of the observed covariates between treatment and control groups (On pre-processing, see [Ho et al. 2007](#). On genetic matching and search algorithms, see [Sekhon 2009](#); [Diamond and Sekhon 2013](#); [Sekhon and Mebane 1998](#)). The algorithm seeks optimal balance via the construction of matched pairs consisting of one institutionally retained “control” judge and one “treated” judge retained via partisan, non-partisan, or retention elections, and supplies weights that can be used in subsequent analysis. We conducted separate matches for each treated group.

We allow the matching algorithm to operate without restriction on thirteen theoretically relevant observable covariates. These include the variables described above: indicators for whether the case was salient, involved multiple parties, *amicus* briefs, and/or the state as a party, was criminal in focus, and/or required statutory interpretation. We also match on the log of legal issues involved in the case, the judge’s length of service, the years remaining on her term at the time of the decision, and whether she retired within a year of the decision. We also include Bonica and Woodruff’s (2014) common-space measure of judge ideology (CFscore), as well as the size of the court and its professionalization score ([Squire 2008](#)). We seek improved balance on all of these measures.¹²

Table 2 contains a number of balance measures for all covariates in both groups, before and after conducting the matches. The figures in Table 2 indicate that there are indeed some apparent differences between the treatment and control judges (or the cases on which they ruled) in our sample. For instance, compared to cases heard by institutionally retained judges, elected judges heard a higher proportion of salient cases, but their cases were typically less likely to involve *amicus* briefs or multiple parties. Moreover, the control group is notably more conservative (as measured by CFscore) than were the elected or voter-retained judges. On the whole however, the cell entries in Table 2 show

¹²Matching should also mitigate concerns about endogeneity stemming from the inclusion of case salience as a control variable, since it could be that cases are more likely to be covered in local media if they overturn precedent. We achieve balance on the salience measure across groups.

Table 2: Mean Values of Observable Covariates and Balance Assessment, Pre- and Post-Matching

Partisan Elections								
	Means: Unmatched Data		Means: Matched Data		Mean Diff.	Percent Improvement		
	Treatment	Control	Treatment	Control		eQQ Med	eQQ Mean	eQQ Max
Case-Level:								
Salient Case	0.533	0.238	0.533	0.477	81.1	0	7.1	0
Amicus Briefs Were Filed	0.129	0.182	0.129	0.164	33.6	0	67.9	0
Multiple Parties	0.281	0.414	0.281	0.277	97.2	0	84.2	0
Criminal Case	0.279	0.251	0.279	0.251	-0.8	0	10.3	0
State is a Party	0.159	0.148	0.159	0.124	-215.7	0	-87.0	0
Statutory Interpretation	0.531	0.539	0.531	0.529	78.3	0	-348.7	0
Log Total Issues	0.286	0.219	0.286	0.256	54.1	0	26.2	0
Judge-Level:								
CF-Score	0.065	-0.602	0.065	-0.032	85.4	42.7	43.7	36.5
Service Length	9.15	8.27	9.15	8.47	22.7	0	0.8	0
Years Until Term Expires	4.19	4.38	4.19	4.23	80.0	0	-61.4	-50.0
Judge Retired	0.253	0.212	0.253	0.221	21.8	0	18.4	0
Court-Level:								
Size	7.83	6.67	7.83	6.75	7.6	0	3.7	0
Squire Index	0.625	0.636	0.625	0.650	-116.2	-26.0	-11.1	-7.9
Non-Partisan Elections								
	Means: Unmatched Data		Means: Matched Data		Mean Diff.	Percent Improvement		
	Treatment	Control	Treatment	Control		eQQ Med	eQQ Mean	eQQ Max
Case-Level:								
Salient Case	0.344	0.238	0.344	0.344	100	0	2.6	0
Amicus Briefs Were Filed	0.123	0.182	0.123	0.119	93.3	0	87.7	0
Multiple Parties	0.265	0.414	0.265	0.261	97.4	0	35.4	0
Criminal Case	0.314	0.251	0.3135	0.3088	92.5	0	53.6	0
State is a Party	0.154	0.148	0.154	0.131	-301.2	0	36.2	0
Statutory Interpretation	0.535	0.539	0.535	0.546	-142.7	0	-219.0	0
Log Total Issues	0.321	0.219	0.321	0.293	72.4	0	29.1	0
Judge-Level:								
CF-Score	-0.127	-0.602	-0.127	-0.212	82.1	31.2	37.1	7.7
Service Length	7.43	8.27	7.43	7.11	62.4	0	-8.7	0
Years Until Term Expires	3.28	4.38	3.28	3.40	87.2	0	38.5	0
Judge Retired	0.191	0.212	0.191	0.100	-345.9	0	82.6	0
Court-Level:								
Size	7.17	6.67	7.17	6.92	51.0	0	39.0	0
Squire Index	0.578	0.636	.578	.561	71.5	62.8	25.4	17.4
Retention Elections								
	Means: Unmatched Data		Means: Matched Data		Mean Diff.	Percent Improvement		
	Treatment	Control	Treatment	Control		eQQ Med	eQQ Mean	eQQ Max
Case-Level:								
Salient Case	0.493	0.238	0.493	0.483	96.1	0	17	0
Amicus Briefs Were Filed	0.195	0.182	0.195	0.248	-302.2	0	-53.6	0
Multiple Parties	0.315	0.414	0.315	0.334	81.5	0	-1.4	0
Criminal Case	0.351	0.251	0.351	0.351	100	0	-11.6	0
State is a Party	0.191	0.148	0.191	0.191	100	0	45.5	0
Statutory Interpretation	0.523	0.539	0.523	0.53	57.9	0	20.4	0
Log Total Issues	0.382	0.219	0.382	0.335	70.9	0	3.8	0
Judge-Level:								
CF-Score	-0.126	-0.602	-0.126	-0.183	88.1	47.5	36.7	3.2
Service Length	8.6	8.27	8.6	8.46	59.5	-100	-45.9	0
Years Until Term Expires	2.92	4.38	2.92	3.18	81.8	0	56.6	50
Judge Retired	0.224	0.212	0.224	0.171	-320.5	0	100	100
Court-Level:								
Size	6.66	6.67	6.66	6.55	-3500	0	19.9	0
Squire Index	0.657	0.636	0.657	0.645	43.5	3.7	5.7	0

that genetic matching results in superior overall balance in all three judge groupings, and all “treated” case-votes were successfully matched.¹³

The four leftmost columns of Table 2 depict sample mean differences between treatment and control groups, both before and after matching; entries in the four right-most columns reflect the percentage improvement in a range of differences between the groups. The absolute value of the mean difference is smaller after matching for all but a few covariates for each group, and for most variables the difference is substantially improved. Our matching exercise should raise confidence that unobserved covariates do not drive any apparent results; we therefore apply weights derived from the matching in our comparison of appointed and elected/retained judges.¹⁴

Results

Table 3 contains results of logistic regression models, which we construct using weights obtained from genetic matching. We conducted separate matching exercises for judges elected in non-partisan and partisan elections, and also for retention-plan judges, using institutionally retained judges as a “control” group in each case.¹⁵ As such, we model the three groups separately, and the independent variables of interest are “treatment” indicators for elected (non-partisan and partisan) and retention-plan judges, respectively. Thus, the coefficients on these indicators reflect the effect of partisan elections, non-partisan elections, and retention elections on judges’ propensity to join a majority that overturns precedent, relative to those who are institutionally retained.

¹³Given that the control group is much smaller than the treatment groups (i.e., there are fewer votes by institutionally retained judges than each grouping of elected judges), we necessarily match with replacement. The matching algorithm attempts to pair as many “treated” units as possible with a control unit, pruning unmatched controls. There were 1,061 case-votes cast by partisan judges, 1,276 case-votes cast by non-partisan judges, and 1,319 case-votes cast by retention-plan judges. The practical effect of this matching exercise is an oversampling of control counties that compared favorably with the treatment group, which accounts for the weights employed in the analysis described below.

¹⁴We calculated these weights with the MatchIt package in R (Ho et al. 2011).

¹⁵We do not compare judges with lifetime appointments to institutionally retained judges, since as Figure 1 indicates, there are no cases of judges in the former group voting to overrule precedent in our sample.

Table 3: Logistic Regression Models: Determinants of Voting to Join Majority Overruling Precedent

	Partisan Elections			Non-Partisan Elections			Retention Plan		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Judge is Elected in Partisan Election	2.76*	2.73*	1.43	-	-	-	-	-	-
	(0.95)	(0.98)	(1.80)						
Judge is Elected in Non-Partisan Election	-	-	-	-0.24	-0.24	-0.03	-	-	-
				(0.35)	(0.36)	(0.74)			
Judge Faces Voter Retention	-	-	-	-	-	-	1.56*	1.61*	1.83*
							(0.45)	(0.46)	(0.80)
Partisan Elections X Term Yrs.	-	-	0.34	-	-	-	-	-	-
			(0.48)						
Non-Partisan Elections X Term Yrs.	-	-	-	-	-	-0.03	-	-	-
						(0.19)			
Retention Elections X Term Yrs.	-	-	-	-	-	-	-	-	-0.07
									(0.21)
Years Until Term Expires		0.09*	-0.25		-0.01	0.02		-0.05	0.03
		(0.04)	(0.48)		(0.06)	(0.17)		(0.04)	(0.20)
Salient Case		2.10*	2.10*		-1.00*	-0.99*		0.70*	0.70*
		(0.37)	(0.37)		(0.41)	(0.41)		(0.23)	(0.23)
Amicus Briefs Were Filed		-0.13	-0.16		1.74*	1.75*		0.12	0.12
		(0.42)	(0.42)		(0.46)	(0.46)		(0.25)	(0.25)
Multiple Parties to the Case		-1.17*	-1.17*		-0.06	-0.06		-0.39	-0.38
		(0.39)	(0.39)		(0.39)	(0.39)		(0.24)	(0.24)
Criminal Case		3.19*	3.17*		-0.54	-0.54		-0.98*	-0.98*
		(0.50)	(0.50)		(0.57)	(0.57)		(0.46)	(0.46)
State Government is a Party		-0.50	-0.51		0.78*	0.78*		0.87*	0.87*
		(0.34)	(0.34)		(0.36)	(0.36)		(0.22)	(0.22)
Case Requires Statutory Interpretation		1.91*	1.91*		0.97*	0.97*		0.95*	0.95*
		(0.35)	(0.35)		(0.35)	(0.35)		(0.24)	(0.24)
Log Total Issues Decided in the Case		-2.19*	-2.19*		0.94*	0.93*		0.21	0.21
		(0.55)	(0.55)		(0.44)	(0.44)		(0.38)	(0.38)
Judge Retired	-	-0.11	-0.11	-	-0.28	-0.27	-	-0.29	-0.31
		(0.31)	(0.31)		(0.42)	(0.43)		(0.28)	(0.28)
Constant	-5.35*	-10.76*	-7.36*	-3.22*	-3.16*	-4.24*	-4.03*	-5.53*	-5.04*
	(0.95)	(1.31)	(1.89)	(0.31)	(0.66)	(0.76)	(0.44)	(0.62)	(0.83)
Observations	1,299	1,299	1,299	1,555	1,555	1,555	1,617	1,617	1,617
Log Likelihood	-275.5	-210.8	-210.5	-219.9	-199.0	-199.0	-387.7	-353.7	-353.7
Wald Chi-Squared	22.96	152.5	153.0	0.434	42.08	42.10	18.79	86.71	86.83

* p<0.05. Robust standard errors in parentheses. Weights derived from genetic matching. Institutionally retained judges serve as the “control” group in all cases.

For each grouping, we report the results of three logistic model specifications in Table 3. The first is a bivariate model that returns the effect of being a judge retained via partisan, non-partisan, or retention elections on a judge’s propensity to join a precedent-overturning majority. The second (full) specification employs a range of theoretically relevant controls. The third (interacted) model adds an interaction between the relevant treatment indicator and the number of years remaining in a judge’s term; that is, the time

remaining until she is scheduled to go through some type of reappointment process (for institutionally retained judges) or run in the next election.

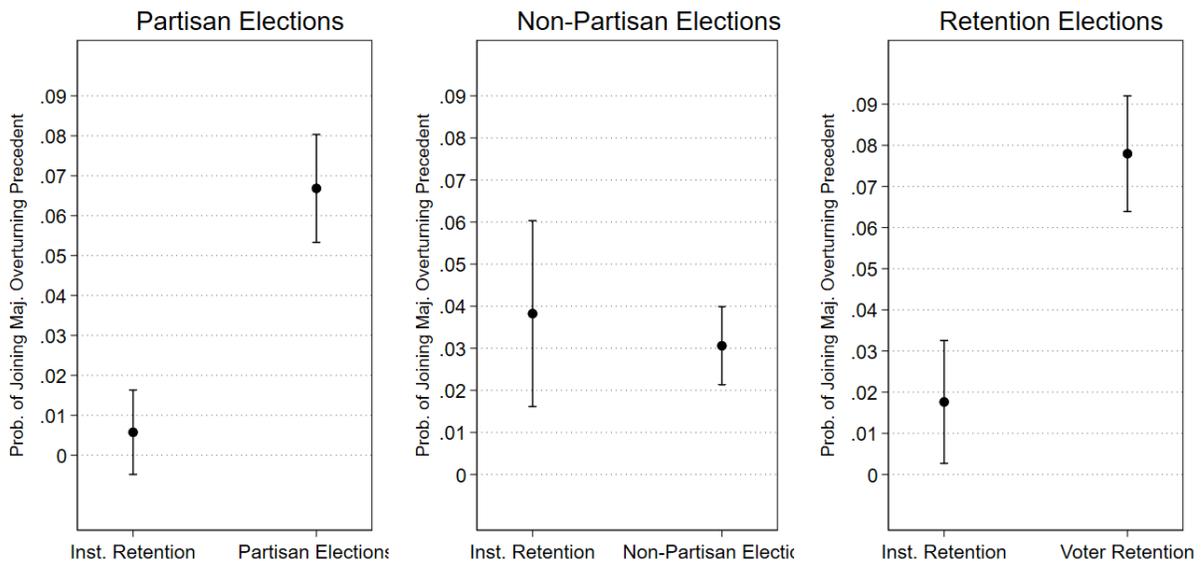
Before considering predicted effects, we first consider the coefficients of interest. There are three notable trends with respect to the election indicators. First, the models in Table 3 yield little evidence of effects for judges retained via non-partisan elections; the coefficient in both Models 4 and 5 is substantively small and does not approach statistical significance. There is more evidence for an effect on joining a precedent-overturning majority among judges who run in retention elections, however, as both the bivariate (7) and full (8) models return a positive, statistically significant, and substantively meaningful coefficient. Finally, the effect of partisan elections appears stronger than that of the other two systems; indeed, the coefficient for partisan elections is positive and significant in both Models 1 and 2, and is nearly twice the size of the coefficients for retention elections. The models therefore indicate that relative to institutionally retained judges, those retained via partisan and retention elections are more likely to join a majority that overrules precedent, with particularly large effects for the former.

Since it can be difficult to derive substantive effects from logistic models, for the remainder of our analysis we employ the models in Table 3 to calculate predicted probabilities of a judge in each type of retention system joining a precedent-overturning majority in a number of theoretically relevant contexts. Figure 2 depicts the predicted probability (and 95% confidence intervals, derived from the full models) that judges vote to overrule precedent, for judges retained via partisan (Table 3, Model 2), nonpartisan (4), and retention (6) elections, as well as from the institutionally retained judges who were selected as a control group in each matching exercise.¹⁶

Figure 2 underscores that voting to overrule precedent is a rare event for judges regardless of retention mechanism, but is particularly so for institutionally retained judges, whose predicted probability of overruling precedent ranges from about .01 to just under

¹⁶In calculating these probabilities, we hold all control variables at their means.

Figure 2: Results from Full Models of Matched Data: Predicted Probability of Overruling Precedent



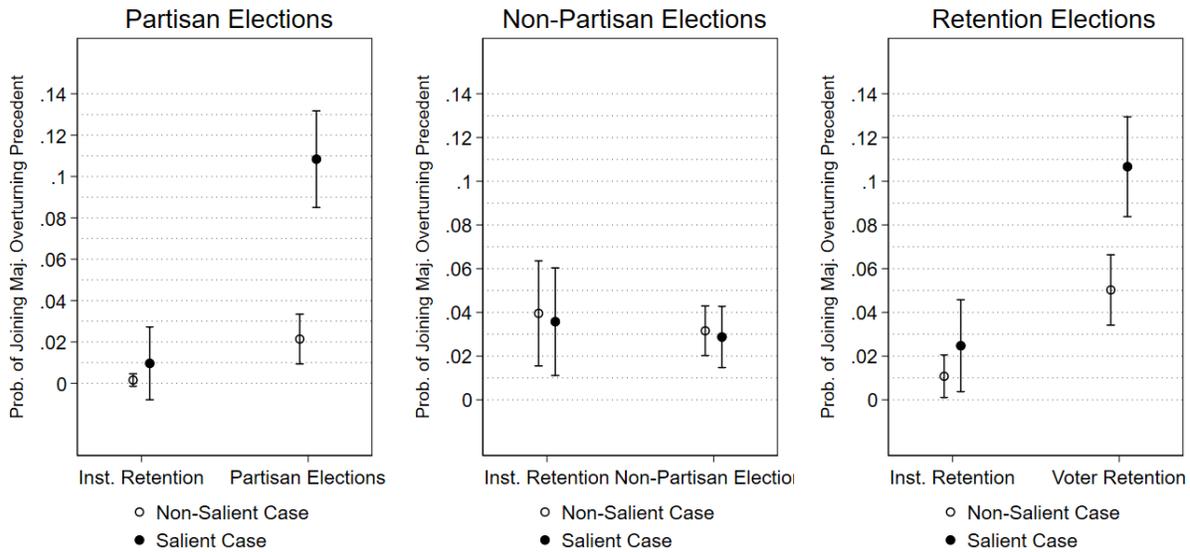
Note: Probabilities derived from Models 2, 5, and 8 in Table 3. Levels of all control variables are held at their mean levels. Matched institutionally retained judges serve as the comparison group in all cases.

.04 in the three panes.¹⁷ The predicted probability of departing from precedent is about .07 for judges facing partisan elections, and .08 for judges facing retention elections. Considering the point estimates for the “control” group of institutionally retained judges, this amounts to a predicted increase in the probability of voting to overturn precedent of about six points in each case.

In contrast, there is little difference in propensity to overturn precedent between institutionally retained judges and those facing non-partisan elections. Indeed, at just over .03, the predicted probability for judges in non-partisan election states is slightly *less* than the point estimate for its control group, though as the coefficient for Model 4 in Table 3 indicates, the difference does not approach statistical significance. Overall then, holding all control variables in the full models at their means, we observe large and statistically

¹⁷We matched with replacement, and unmatched controls are parsed from the data. Each model in Table 3 therefore relies on a different “control group” and employs different weights, which explains the distinct point estimate for institutionally retained judges in each pane of Figure 2.

Figure 3: Results from Models of Matched Data: Predicted Probability of Overruling Precedent, by Case Salience



Note: Probabilities derived from Models 2, 5, and 8 in Table 3. Levels of all control variables (other than case salience) are held at their mean levels. Matched institutionally retained judges serve as the comparison group in all cases.

significant effects of partisan and retention elections, but no effects in non-partisan elections.

We next consider whether case salience—that is, whether the case was covered in the largest newspaper in the state on the day following the decision (see [Vining and Wilhelm \(2010, 2011\)](#))—moderates the relationship between retention system and judges’ propensity to overturn precedent. As noted above, we are particularly interested in the potential for case salience to moderate the relationship between selection system and adherence to precedent, largely because we expect an “electoral connection” to be most likely in cases that are more visible to the public. Indeed, the coefficient for case salience is large and statistically significant in all models in Table 3, which underscores the need for further exploration.

We examine the implications of case salience more fully in Figure 3, which plots

predicted probabilities from the full specifications indicating how likely judges in each group are to join a precedent-overturning majority, calculated separately for high- and low-salience cases. Once again, we find little evidence that judges facing non-partisan elections differ much from their institutionally retained colleagues; as the middle pane of Figure 3 demonstrates, neither retention elections nor case salience seems to affect the probability of a judge voting to overturn precedent. In all four subgroups, the predicted probability of overruling precedent ranges within approximately one point.

In the right pane of Figure 3 however, we see that judges facing retention elections were significantly more likely than institutionally retained ones to overrule precedent in salient cases, but not in non-salient ones. For instance, the probability of voter-retained judges joining a precedent-overturning majority in a salient case is about .11—six points higher than that of institutionally retained judges ruling on salient cases. In non-salient cases, the gap between voter- and institutionally retained judges is only about 1.5 points, and is not statistically significant.

We observe an even more pronounced pattern when we compare judges facing partisan election to those who are institutionally retained. In the left pane of Figure 3, we see the predicted probability of voting to overturn precedent is about .11 among judges facing partisan elections. This marks an increase of roughly nine points over their propensity to depart from precedent in non-salient cases, and is ten points above institutionally appointed judges' predicted probability (.01) of voting to overrule precedent in salient cases.¹⁸

In sum, our models return clear findings for all three groups, relative to their comparison groups. For judges retained via non-partisan elections, there is practically no evidence of a different approach to precedent than judges who do not face voters, regardless of case salience. However, we observe large effects among judges retained via both retention and partisan elections, who appear to be significantly more likely to overrule

¹⁸There are no cases among institutionally retained judges who were matched to those facing partisan elections in which the majority overturned precedent.

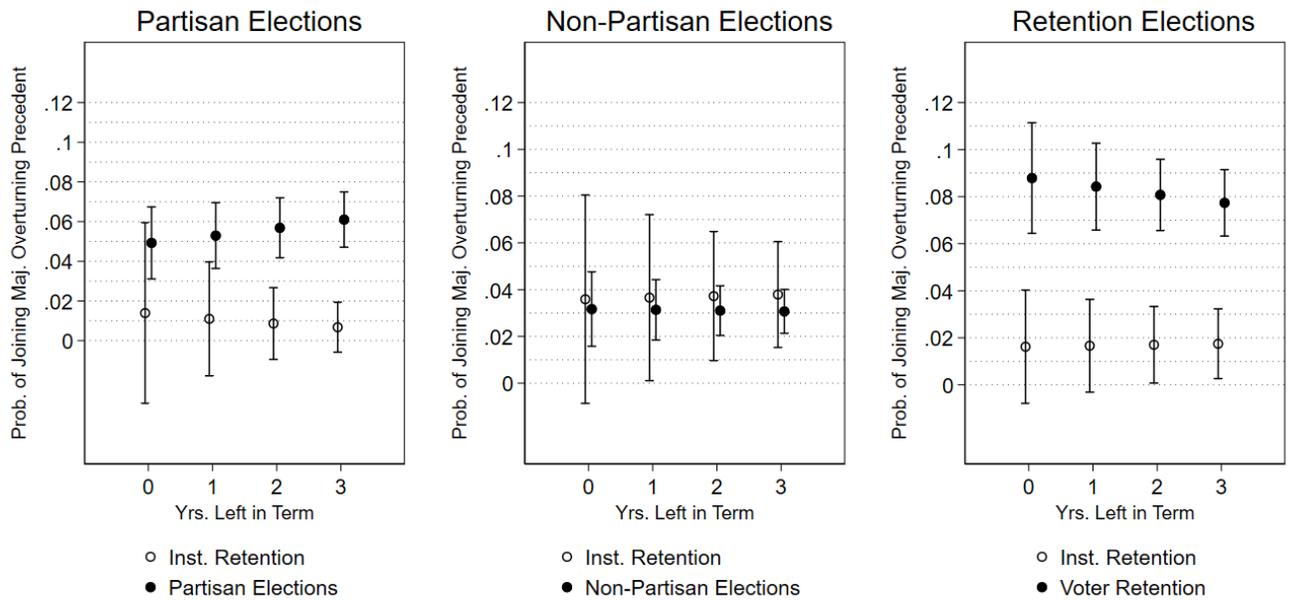
precedent. In both groups, most of this effect is driven by judges' votes in salient cases. Put another way, in states with partisan and retention elections, judges demonstrate a heightened willingness to break from *stare decisis*—but only in cases that garner public attention.

Election Effects

A natural next question is whether electoral conditions further influence the behavior of non-appointed judges. For instance, judges who are contemplating a vote on a given case might be more (or less) willing to overturn precedent as their next election nears, in an effort to either respond to public opinion or avoid charges of “judicial activism.” We engage that possibility with Models 3, 6, and 9 in Table 3. These models feature interactions between the election indicator and the number of years remaining in a judge's term at the time the case was decided. The “time in term” variable effectively captures the amount of time between the judge ruling on the case and the next decision to retain her, to be rendered by either voters (in the case of elected judges) or some institutional body. The interaction allows us to determine whether voter-retained judges are differently likely to overrule precedent near the end of their term.

Figure 4 depicts the predicted probabilities derived from these interactions. Unsurprisingly given our results so far, there is practically no difference between judges facing non-partisan elections and their matched control group at any point as their terms approach the final year. A more visible difference is apparent between voter-retained judges and their matched control group. As their term draws to a close, the predicted probability that retention-election judges join a precedent-overruling majority increases slightly, rising by about one point over the final three years of the judge's term. Moreover, this probability remains significantly higher than institutionally retained judges—whose predicted probability remains constant across time remaining on their term—at all points depicted in Figure 4.

Figure 4: Predicted Probability of Joining Majority Overturning Precedent, By Time Left in Term



Note: Probabilities derived from Models 3, 6, and 9 in Table 3. Levels of all control variables are held at their mean levels. Matched institutionally retained judges serve as the comparison group in all cases.

We observe a similar pattern when comparing judges retained via partisan elections with their matched control. The left pane of Figure 4 demonstrates that the predicted probability of partisan judges voting to overrule precedent declines by slightly more than one point over the final three years of their term, while the prediction for their control group remains relatively stable. This could be taken as evidence that judges in partisan elections are *slightly* less likely to overrule precedent as their re-election nears, though it is important to note that given the confidence intervals about the predictions, nothing in Figure 4 is definitive with respect to time trends among either partisan or retention-election judges.

We examine time trends in greater depth in Figure 5, which depicts predicted probabilities over the time remaining in a judge’s term separately for salient and non-salient cases. The middle pane of Figure 5 is broadly consistent with the trend we report above

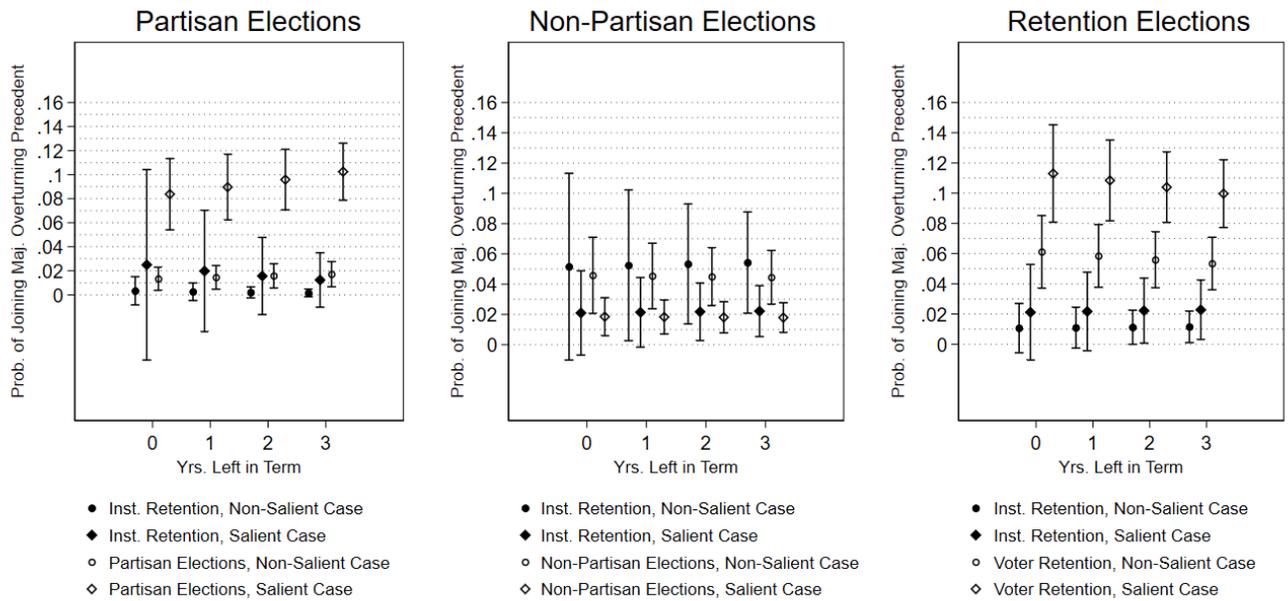
for non-partisan judges: The relationship between retention system and treatment of precedent is time-insensitive, regardless of case salience. However, salience is an important factor for judges facing both partisan and retention elections. Judges facing retention elections (right pane) are more likely than institutionally retained judges to overrule precedent in both salient and non-salient cases, regardless of the time remaining in their term. Moreover, in both salience categories, we see the same one-point increase in retention election judges' probability of overruling precedent as is visible in Figure 4 above.

Among partisan judges (left pane), nearly all of the differences we observed in Figure 4 appear to be driven by salient cases. Compared to institutionally retained judges, partisan ones are equally likely to overrule precedent in non-salient cases, and the trend over time for non-salient cases is flat. This contrasts with the pattern among salient cases; the predicted probability that partisan judges will vote to overturn precedent in those cases falls from .102 three years from the election to .083 in an election year—though as with the increase we observe for retention election judges, given the confidence intervals, we hesitate to draw definitive conclusions about over-time trends.

Conclusion

We conducted what is to our knowledge the first evaluation of the linkage between judicial retention systems and *stare decisis* in state courts of last resort. Our models of judges' votes to join majorities overruling precedent yield statistically significant, substantively meaningful differences. We find evidence for a relatively strong relationship between both partisan and retention elections and judges' propensity to overturn existing precedent: Compared to institutionally retained judges, those retained via partisan or retention elections are significantly more likely to join a majority that overturns precedent. Case salience is also an important moderator of these effects; while clear differences in the predicted probability of overruling precedent are apparent between “treated” and “con-

Figure 5: Predicted Probability of Joining Majority Overturning Precedent, By Time to Election and Case Salience



“control” groups in salient cases, we find little difference in non-salient ones. Nor does the proximity of the next election appear to moderate these effects; judges facing partisan and retention elections treat precedent similarly in each of the final four years of their term, regardless of case salience. Finally, our analysis yields no detectable differences in treatment of precedent between institutionally retained judges and those retained via non-partisan elections.

In total, our findings indicate that media coverage of a case shifts incentives for judges retained via partisan and retention elections, who become more likely to depart from precedent in rendering their decisions in high-salience cases. This is consistent with the idea of an electoral connection between voters and those judges. If judges are cognizant of factors such as popular will or political conditions when making their decisions, we would expect these effects to be particularly acute when judges rule in salient cases—which receive the most media coverage and public attention.

We believe these results are important in advancing our understanding of how reten-

tion systems affect judicial behavior. Our findings suggest that retention systems create incentives that affect the manner in which judges approach their work. Indeed, even precedent—one of the most well-established norms in the judiciary—is subject to disparate treatment by judges who face voters in partisan and/or retention elections. This is an important addition to the ever-expanding literature examining linkages between judicial selection and/or retention systems and judges' behavior (e.g., [Brace, Yates and Boyea 2012](#); [Caldarone, Canes-Wrone and Clark 2009](#); [Choi, Gulati and Posner 2010](#); [Goelzhauser and Cann 2014](#); [Hall 2001, 2007](#); [Bonneau and Hall 2009](#); [Yates, Tankersley and Brace 2010](#)). That said, we stress that ours is an early foray into understanding the manner in which judges treat precedent. To be sure, our analysis does not take up some questions, and our results suggest others. As such, we believe that future work should consider other several important aspects of how state judges treat precedent.

First, while this paper focuses on the method of judicial *retention* in a given state, there are also reasons to believe that judicial *selection* could affect the manner in which judges treat precedent. For instance, it is possible that judges in merit selection systems—who often go through vetting by an independent commission before being appointed—may be differently deferential to precedent than those who won their seats in contested elections. Future research should extend our analysis of retention effects to determine how selection impacts judges' treatment of precedent.

Second, future work should also more closely explore the differences between judges retained via non-partisan elections and the two other retention mechanisms we identify. Though our analysis clearly indicates that non-partisan judges *are* distinct from partisan and retention plan judges in that they do not differ from institutionally retained judges in their treatment of precedent, it is not immediately clear why. It could be that public opinion favors adherence to precedent, and non-partisan judges are more likely to tack closer to the public's preference. This would be generally consistent with previous work finding that non-partisan judges are more responsive to the public will ([Caldarone,](#)

Canes-Wrone and Clark 2009). Alternatively, non-partisan judges might treat precedent similarly to institutionally retained ones out of a recognition that voters in non-partisan elections weigh incumbency heavily in their decisions (e.g., Schaffner, Streb and Wright 2001), which might make their views on the likelihood of further service similar to judges who must obtain legislative re-confirmation. Regardless, we urge future scholarship on state court precedents to extend and replicate our results.

Third, our present inability to definitely say why non-partisan judges are unique among judges facing voters underscores that there remains much unknown about the mechanism between retention systems and how judges rule. Given data limitations, we cannot say *why* partisan and retention-elected judges are more willing to join majorities that overrule precedent. The SSCDP data are quite robust with respect to capturing aspects of each case, but they cannot tell us everything we might want to know. For instance, it could be that elected judges see their institutions as more volatile than institutionally retained ones do. If they perceive higher turnover among their colleagues, they may be more willing to depart from the norm of *stare decisis* if a case comes to them at an opportune time.

Similarly, the SSCDP data do not reveal public opinion in a given state, on a given issue, at the time of the decision. It is certainly possible that judges in the partisan and retention election groups deviate from precedent at a higher rate in order to bring their legal interpretation in-line with public opinion. If this is the case, then our findings are consistent with other previous work suggesting that public opinion influences judicial behavior in elected courts (e.g., Brace and Boyea 2007; Brace, Hall and Langer 2001; Caldarone, Canes-Wrone and Clark 2009; Cann and Wilhelm 2011). An elected judiciary deviating from precedent in order to hew to changing popular sentiment is also in-line with other work that has argued that elected judges produce more democratic outcomes (e.g. Bonneau and Hall 2009; Gibson 2012). Future work should therefore take up the question of whether these judges break with precedent in an effort to render a decision

more consistent with the public will. That said, it is of course debatable whether stable “public opinion” exists for the disposition of a great many cases on which state courts rule, which should also be taken into account in future work in this area.

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Online Appendix: Stare Decisis and the Electoral Connection

Results From Unmatched Data

We present results from logistic regression models of unmatched data in Table A1. With respect to the independent variables of interest, there are two notable differences. First, the coefficient for non-partisan elections is positive and significant in Model 1. Second, the coefficient for partisan elections fails to achieve statistical significance in the full specification of Model 2 ($p=.062$). That said, there is little in Table A1 that gives rise to serious questions about the findings we report in the main paper, though we believe that for the reasons described there, the models of matched data result in superior estimates of the effect of retention system on judges' treatment of precedent.

Summary of Data by State

The data that drive some main results of our paper rely in part on a random sample of 500 non-salient cases from the *State Supreme Court Data Project*. As such, it is possible that sampling bias could drive our results. For instance, an oversampling of cases from one state could result in that state impacting our findings. While our matching strategy should mitigate this threat, here we consider whether our sample could affect our results in some way.

In Table A2, we depict the number of case-votes in our overall data. The distribution of case-votes is approximately normal across states. However, a visual inspection of the case-votes in Table A2 suggests that two states merit further inspection: Alabama and Nebraska. These two states not only comprise the largest number of case-votes in our sample, but also exhibit the highest rates of votes to overrule precedent. So, it is possible that they exert leverage on our findings.

Table A1: Logistic Regression Models of Unmatched Data: Determinants of Voting to Join Majority Overruling Precedent

	(1)	(2)
Judge is Elected in Partisan Election	1.09*	0.95
	(0.51)	(0.51)
Judge is Elected in Non-Partisan Election	1.04*	0.91
	(0.50)	(0.53)
Judge Faces Voter Retention	1.43*	1.17*
	(0.48)	(0.51)
Years Until Term Expires		-0.11*
		(0.04)
Salient Case		0.95*
		(0.18)
Amicus Briefs Were Filed		1.14*
		(0.35)
Multiple Parties to the Case		-1.04*
		(0.41)
Criminal Case		1.10*
		(0.46)
State Government is a Party		0.53
		(0.30)
Case Requires Statutory Interpretation		0.52*
		(0.23)
Log Total Issues Decided in the Case		-0.16
		(0.37)
Judge Retired		-0.14
		(0.30)
Constant	-4.63*	-4.90*
	(0.45)	(0.56)
Observations	4,664	4,601
Log Likelihood	-605.9	-553.0
Wald Chi-Squared	9.327	221.6

* $p < 0.05$. Robust standard errors in parentheses. Weights reflect oversampling of salient cases. Institutionally retained judges serve as the referent group.

We checked for this possibility by re-running the models in Table A1, omitting first Alabama, then Nebraska, then both. None of these changes affected the direction or substantive size of the independent variables of interest in the models contained in Table A1 of the paper, though the coefficient for partisan elections does achieve statistical significance when Alabama is omitted.

Table A2: Judge-Votes by State

State	Num. Votes Overall	Num. Votes to Overrule
AL	467	8.12%
NE	348	6.05%
CA	231	4.02%
ME	208	3.62%
OH	200	3.48%
TX	188	3.27%
WI	186	3.23%
NV	182	3.17%
CO	171	2.97%
OK	156	2.71%
LA	154	2.68%
WV	154	2.68%
FL	143	2.49%
RI	143	2.49%
NH	136	2.37%
KS	133	2.31%
SC	133	2.31%
AR	131	2.28%
MA	131	2.28%
MS	126	2.19%
CT	120	2.09%
IL	118	2.05%
NY	117	2.03%
GA	114	1.98%
NJ	112	1.95%
PA	112	1.95%
WA	97	1.69%
IA	90	1.57%
AK	88	1.53%
SD	79	1.37%
MN	76	1.32%
TN	73	1.27%
MD	70	1.22%
VA	70	1.22%
OR	63	1.1%
ND	60	1.04%
MT	57	0.99%
IN	56	0.97%
MO	56	0.97%
NC	55	0.96%
KY	51	0.89%
MI	49	0.85%
UT	45	0.78%
ID	42	0.73%
VT	40	0.7%
WY	38	0.66%
NM	25	0.43%
AZ	21	0.37%
HI	20	0.35%
DE	15	0.26%

Additional Analysis: Leveraging Retirements

We looked for election effects by leveraging judge retirements in our matched groups. Figure A1 displays predicted probabilities of overruling precedent for judges in each treatment group from the models in Table 3, depicted by their retirement status. Those probabilities yield little reason to suspect significant difference-in-differences by system and retirement status. Once again, the predicted probabilities for non-partisan judges are similar across both retention system and retirement status. For judges retained via partisan and retention elections, the treatment of precedent is also quite similar among retiring and non-retiring judges. Our retirement measure is hardly a fine one when it comes to capturing any dynamic that may affect judges' treatment of precedent once they decide to leave the bench. That said, it yields no evidence that individual judges treat precedent any differently once they have made the decision to forego reappointment. Analysis that accounts for case salience or years remaining on a judge's term reveals largely the same pattern as that in Figure A1

Figure A1: Predicted Probability of Joining Majority Overturning Precedent for Retiring and Non-Retiring Judges, by Retention System

