Electoral Proximity and Issue-Specific Responsiveness

Michael Pomirchy*

Abstract

Do elections increase responsiveness of legislators to their constituents? Previous studies that examine the effect of electoral proximity have been unable to hold the roll-call agenda constant and control for differences in unobserved covariates between legislators. This paper utilizes a natural experiment in four state legislatures—Arkansas, Illinois, Florida, and Texas—where term length was randomly assigned. This design compares the responsiveness to constituency opinion of those randomly assigned to a two-year term to those assigned a four-year term on different issue areas, like the economy, environment, and crime. I find no evidence for an electoral proximity effect on responsiveness. In addition, in the Illinois State Senate, the causal effect of electoral proximity on responsiveness is measured on several individual roll-call votes, including the legalization of medical marijuana and gay marriage.

*Postdoctoral Research Associate, Institution for Social and Policy Studies, Yale University. Email: michael.pomirchy@yale.edu, Phone: (917) 733-2628, Mailing Address: 77 Prospect Street, New Haven, CT 06520. The author would like to thank Brandice Canes-Wrone, Martin Gilens, John Kastellec, Michael Kistner, Dean Knox, Jonathan Mummolo, and Rocío Titiunik for helpful comments and feedback.

1 Introduction

The notion that elections induce greater responsiveness in roll-call voting carries a strong hold in our public discourse. In discussing Senator Susan Collins's vote against confirming Justice Amy Coney Barrett, for instance, her opponent in the race accused her of making a "political calculation" to appear moderate in the run-up to the 2020 elections.[1] Indeed, journalists noted that Collins's Senate seat in Maine was electorally competitive.[2] In voting against a humanitarian aid bill that Speaker Pelosi proposed in 2019, the *New York Times* reported that moderate Democrats "were wary of supporting a bill that provided less money for ICE that could later be used against them in their re-election campaigns to portray them as weak on immigration enforcement."[3] In line with these cases, scholars have shown that electoral timing can affect incumbents' behavior in office (Anzia 2011; Canes-Wrone 2015). This is often attributed to voters' "recency bias," in that they are more likely to respond to events that transpire close to an election (e.g., Achen and Bartels 2016; Arceneaux et al. 2016; Healy and Lenz 2014; Huber, Hill and Lenz 2012), or to the idea that voters are unlikely to know at election time if the enacted policy is the right one (Canes-Wrone, Herron and Shotts 2001).

However, empirically estimating the effect of electoral timing on specific roll-call votes in a causally identifiable way can be challenging. There are two approaches that scholars often use to measure this effect. The first is to compare legislators with different time horizons (e.g., Ahuja 1994; Bernstein 1991; Shepsle et al. 2009). In the U.S. Senate, for instance, one-third of all Senators go up for reelection every two years; as such, one can perform a cross-sectional analysis and compare legislators with varying levels of electoral proximity.

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[1] Christian Science Monitor, "Collins no-vote on Barrett a mixed signal for shifting ME voters," October 28, 2020

[2] Newsweek, "Susan Collins, Lone GOP Senator to Vote Against Amy Coney Barrett, in Danger of Losing Senate Seat," October 28, 2020

[3] NY Times, "House Passes Senate Border Bill in Striking Defeat for Pelosi," June 27, 2019

However, one issue here is that Senators from different cohorts may differ across relevant covariates, like party affiliation and electoral competitiveness, because of “partisan waves” or year-specific trends. For instance, according to the Cook Report, more senators were elected in the 2008 cohort in “toss-up” or “purple” states than in 2010; in the 2013-2014 Congress, both electoral proximity and the competitiveness of their seats might drive the 2008 cohort to be more responsive to their voters than the 2010 cohort.[4] The second approach is to look across time, within a politician’s term (e.g., Bernhard and Sala 2006; Canes-Wrone 2006; Elling 1982; Levitt 1996; Thomas 1985; Warshaw 2016; Wood and Andersson 1998). For example, one can compare an incumbent’s performance in the last two years of her term with her performance in the first two years. With this design, though, measurement issues arise like controlling for changes in the state of the world or holding the issue category constant. In addition, one cannot necessarily rule out the alternative theory that greater legislative responsiveness to the voters closer to election time is a result of more incumbent experience and “learning-by-doing,” instead of greater sanctioning by the voters.

In this paper, I utilize a natural experiment in four state legislatures—Arkansas, Florida, Illinois, and Texas—where term length was randomly assigned at the beginning of each decade, to resolve these issues. After reapportionment and redistricting, all state legislators in these states go up for reelection and either serve a two-year or a four-year term. This natural experiment was first used in Titiunik (2009) and has been exploited in later work (Gaines, Nokken and Groebe 2012; Titiunik 2016; Titiunik and Feher 2017).[5] This nice causal setting has still been relatively under-utilized by scholars, however, despite its potential to help rigorously address various research questions. Using survey data that gauges constituent opinion on specific policy items, this paper is (to my knowledge) the first study to directly

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[4] According to the Cook Report, there were 10 toss-up states in the 2008 Senate elections, while there were only seven toss-up states in 2010.

[5] This prior work explores outcomes like legislative effort (Titiunik 2009; Titiunik and Feher 2017) and campaign expenditures (Gaines, Nokken and Groebe 2012). Titiunik (2016) also looks at responsiveness by using gubernatorial vote share, but there is no analysis of survey data or individual roll-call votes.

assess the causal effect of electoral proximity on legislative responsiveness on individual roll-call votes (in addition to responsiveness on broader issue areas).

To test the conventional wisdom about the electoral proximity effect described earlier, this paper checks to see if this effect manifests consistently across roll-call votes and across issue areas. Over $2,000$ roll-call votes are collected from each of the four states in each relevant session and classified into three categories—economic/spending, environmental, and crime-related legislation. Ideal points are estimated for each legislator within each issue category (and across all votes), and responsiveness is then measured by assessing the association between these ideal points and constituency opinion estimates. In this initial analysis, there is no evidence of an electoral proximity effect, and the results are similar when looking across issue areas.

Scholars might be concerned that a lack of evidence for electoral proximity using scaled measures of ideology might not translate to the same result using individual items; indeed, recent work has argued that legislators who represent constituents poorly on scaled measures of ideology may represent them well on specific issues (Ahler and Broockman 2018; Broockman 2016). To address this concern, I utilize several surveys of Illinois registered voters taken before and during the 2013-2014 legislative session which asked respondents their opinions on several items taken up in the Illinois State Senate. Using these surveys, one can measure the association between legislators' roll-call votes and the percentage of constituency support on a given bill. These surveys have the advantage that they ask questions contemporaneously with the votes cast by Illinois Senators on these measures. Moreover, the survey questions touch on roll-call votes that are quite salient. Perhaps most notably, in 2013, Illinois became the 15th state to legalize same-sex marriage. They also voted to legalize marijuana for medical purposes, blocked restrictions on gun ammunition, passed a comprehensive pension reform package, and became the last state in the union to allow concealed-carry weapons. On these roll-call votes, surprisingly, I continue to find little evidence that shorter terms induce greater responsiveness.

A remaining question is whether the electoral proximity effect might manifest in some state legislatures rather than others. State legislatures differ across many relevant characteristics, like levels of professionalization. For instance, Arkansas is a part-time legislature that ranks relatively low on commonly used professionalized scores (e.g., Squire 2007), whereas Illinois and Florida typically rank much higher. When states offer larger salaries and greater perks, it is reasonable to expect greater electoral proximity effects because legislators might place higher value on winning reelection in these cases. However, this paper finds that the electoral proximity effect is similar across the four states, both in terms of magnitude and sign. Moreover, the effect sizes do not appear to be correlated with differences in professionalization.

One additional limitation of this analysis is that two of the four states under consideration impose term limits that only count four-year terms. Specifically, in Arkansas and Florida, in the time period considered in this analysis, incumbents can no longer run again (or in the case of Arkansas, they have to wait two years) if they have served two four-year terms. To address this, three approaches are adopted. First, the results in Arkansas and Florida are contrasted with the results in Illinois and Texas. Second, term-limited status is controlled for. Lastly, the analyses are run excluding observations that are term-limited. Across these approaches, the results remain substantively unchanged.

Overall, these results may be informative for thinking about the role of elections at the state legislative level. More generally, with the caveats described above in mind, this result provides another data point for a body of literature supporting the notion that elections do not encourage greater incumbent responsiveness (e.g., Cohen 1997; Titiunik 2016; Wood and Lee 2009). While there are many possible explanations for the null findings here, the results are compatible with prior work that shows weak sanctioning effects at the state level relative to the federal level (e.g., Rogers 2017).

2 Related Literature

This paper primarily contributes to three strands of literature: (1) responsiveness; (2) state legislative representation; and (3) electoral proximity. In the first strand, scholars generally find evidence that politicians are responsive to their constituencies (e.g., Erikson, MacKuen and Stimson 2002; Stimson, MacKuen and Erikson 1995), in the sense that there is a strong relationship between policy and opinion; for a review of the large literature on responsiveness, see Canes-Wrone (2015). There is some debate about whether responsiveness is conditional, particularly with regards to whether policies cater to the affluent and the well-connected (Bartels 2016; Gilens 2012; Soroka and Wlezien 2008), and whether electoral institutions play a role in the political success of under-represented groups (e.g., Abott and Magazinnik 2020). Moreover, sub-constituencies, like copartisans, may receive more attention from politicians, as famously noted by Fenno (1978) and substantiated by later work (e.g., Kastellec et al. 2015; Lax, Phillips and Zelizer 2019). Finally, politicians may respond to their constituencies through communication to the bureaucracy (Lowande, Ritchie and Lauterbach 2019; Ritchie 2018). This paper contributes to this line of research by leveraging causal variation to identify an important predictor of responsiveness—electoral proximity—and analyzing issue-level differences in the effect of this predictor.

Similar to work examining national-level policy, early work comparing state ideology with policy outcomes suggests that there is a strong association between the two (Erikson, Wright and McIver 1993). More recent work has confirmed this finding (Caughey and Warshaw 2018). However, others have pointed out that while there is great responsiveness at the state level, there are often mismatches between the majority view and state policy (Lax and Phillips 2012), biases in policy (Simonovits, Guess and Nagler 2019), and incumbents tend to systematically overestimate their constituents' support for more conservative policies (Broockman and Skovron 2018). Moreover, there is a relatively modest impact of ideological distance between legislators and constituencies on electoral support (Rogers 2017), which some might attribute to the growing nationalization of state and local elections (Hopkins 2018; Rogers 2016). Fouirnaies and Hall (2021) show that electoral incentives at the state legislative level lead to greater effort, but not necessarily more moderate roll-call voting records. Finally, other relevant studies find that term limits increase polarization in state legislatures (Olson and Rogowski 2020), and divided government leads to legislative delay (Kirkland and Phillips 2018). The results presented in this paper contribute to this literature by examining the role of another institution that exhibits variation at the state level—term length—and its effect on legislative behavior.

Lastly, scholars have developed ample evidence for a positive electoral proximity effect, when examining U.S. presidents (Canes-Wrone 2006), Senators (Ahuja 1994; Bernhard and Sala 2006; Bernstein 1991; Elling 1982; Levitt 1996; Lindstadt and Wielen 2011; Shepsle et al. 2009; Thomas 1985; Warshaw 2016), and judges (Huber and Gordon 2004). Some scholars have also presented some contrary or null evidence (e.g., Cohen 1997; Fowler and Hall 2018; Healy and Malhotra 2009); in addition, others have suggested that presidents' rhetorical positions become less centrist in reelection years (e.g., Wood and Lee 2009). Moreover, Bouton et al. (2021) demonstrate electoral proximity effects in the context of the U.S. Senate on some issue areas, like the environment and gun control laws, but not other issues, like reproductive rights. The approach taken in this paper differs from these studies in that one can control for observed and unobserved covariates across incumbents in assessing the effect of electoral proximity.

Prior studies have obtained causal estimates of the effect of electoral proximity on various measures of legislative behavior. Bo and Rossi (2011) exploited a similar natural experiment in Argentina and found that shorter terms discourage effort. As mentioned before, Titiunik (2009) originally analyzed the natural experiment in Arkansas, Illinois, and Texas and focused on measures of effort as well, which include the number of bills sponsored and the number of bills introduced. A follow-up study in Arkansas analyzed term limits (Titiunik and Feher 2017). The closest study to the current one is Titiunik (2016), which examined legislative responsiveness as an outcome in this setting by using gubernatorial vote share. This paper builds on this by looking at individual roll-call votes (and responsiveness on broader issue categories), using survey data. In addition, the data is extended to include a new case, Florida, and the random assignments conducted in 2012.

3 Theoretical Expectations

Before beginning the analysis, it is important to lay out the theoretical expectations for this design. As incumbents largely aim to seek reelection (Arnold 1990; Mayhew 2004), elections are ways of incentivizing incumbents to do better in office, either by passing bills that their constituents might like, delivering funding to their district, or providing constituency service. Theories of accountability often discuss the "sanctioning" mechanism of elections (Barro 1973; Fearon 1999; Ferejohn 1986), in which incumbents are motivated by reputational concerns to choose the voter's preferred policy. Classic theories of retrospective voting suggest that voters use policy outcomes to inform their reelection decisions (Fiorina 1981); indeed, evidence suggests that voters punish dissonant incumbents at the polls (Ansolabehere and Jones 2010; Canes-Wrone, Brady and Cogan 2002).

Generally, the literature offers several rationales for why electoral proximity may induce greater responsiveness. First, in light of evidence that voters are more attentive to events that occur soon before an election (Achen and Bartels 2016; Healy and Lenz 2014; Huber, Hill and Lenz 2012; Sarafidis 2007), it is likely that legislators are incentivized to appeal to voters closer to the end of their term.[6] Because voters are paying more attention, incumbents should be more likely to exert effort or take positions that are in accord with their constituents

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[6] It should be noted that other studies have documented the absence of any voter recency effects (e.g., Fowler and Hall 2018).

(Arceneaux et al. 2016; Lindstadt and Wielen 2011). Relatedly, other scholars have noted the tendency of attribute substitution (Kahneman 2003); individuals might simplify the retrospective voting problem to an assessment of recent incumbent performance (Healy and Lenz 2014). Finally, another reason to expect a positive electoral proximity effect is that when the policy consequences of bills have a large probability of being realized by the voter (e.g., at the beginning of the incumbent's term), the incumbent may have greater incentive to exercise "policy leadership" instead of pandering to the voters (Canes-Wrone, Herron and Shotts 2001).

The electoral proximity effect could run the other way, however. One might expect that there is a tradeoff between campaigning and investing in policy expertise. If an election is nearby, incumbents may be incentivized to focus on fundraising and responding to potential contributors instead of advancing a particular policy agenda. Furthermore, Bo and Rossi (2011) discuss a potential "payback horizon" effect such that if incumbents are likely to reap rewards from policy efforts only in the long-term, those who are up for reelection soon may be discouraged from engaging in such policy efforts. Moreover, Besley (2006) considers a model in which there exists a lag in the realization of policy outcomes for the voter. In this case, there is greater responsiveness from incumbents with more time left since the policy outcome will be more likely to be revealed by election time. Finally, some literature has posited that closer to election time, incumbents might move in the direction of their likely opponents, regardless of the preferences of their constituency (Bernstein 1991; Thomas 1985).

Lastly, the electoral proximity effect may be conditional on the salience of the issues legislators are voting on. Politicians may perceive the consequences from deviating from constituency preferences to be more dire when legislating on issues that are important to the constituency (Arnold 1990). At the congressional level, it has been shown that legislators are more likely to be thrown out of office due to dissonant votes on more salient issues (Highton 2019; Hutchings 1998). However, at the state level, while governors might receive a lot of media coverage, it is possible that state legislators and their actions may garner less attention. Indeed, there are greater electoral consequences for dissonant legislative behavior at the federal level than at the state level (Rogers 2017). Thus, actions taken by state legislators that are less salient should depress our expectations for any effect of electoral proximity on responsiveness.

4 Natural Experiment

In Arkansas, Florida, Illinois, and Texas, the State Senate randomizes the term length—either two years or four years—of incoming state legislators. This procedure takes place every decade after the Census is conducted and Senate districts are reapportioned. All seats in the Senate go up for reelection at the next election date; in the time periods considered in this paper, this means either in 2002 or in 2012. In Arkansas, Illinois, and Texas, this procedure regularly transpires at the start of each decade, whereas in Florida, this has only happened in 2012.

In these states, state legislators are randomly assigned an electoral calendar for the next decade. Table 1 describes these possible assignments, which one can refer to, for short, as 2-4-4, 4-4-2, and 4-2-4, respectively, to indicate the length and order of assigned terms across the decade. In Arkansas, Florida, and Texas, the legislators are either assigned to the 2-4-4 or 4-4-2 calendars,[7] whereas in Illinois, the legislators can be assigned to either of these two calendars or to the 4-2-4 calendar.[8] This implies that in the former three states, there is approximately a one-half probability of serving a two-year term to start with, whereas in

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[7] The Texas Tribune has reported that "one by one, senators walked up to the front of the chamber and picked an envelope, each with a piece of paper inside a capsule. The papers were numbered 1-31. Senators who picked an even number will serve a two-year term. Senators who picked an odd number got a four-year term" (Batheja 2013). In Arkansas, the state Constitution actually mandates that the drawing of lots take place at the beginning of the legislative session, following reapportionment.

[8] In 2012, for instance, the State Journal-Register reports that "Secretary of State Jesse White today drew lots to determine the length of terms in state Senate districts that will be won in general elections over the next 10 years" (Schoenburg 2012).

Table 1: Possible Term Assignments (C is only possible in Illinois)

<table>
<tr><th></th><th>T</th><th>T + 1</th><th>T + 2</th><th>T + 3</th><th>T + 4</th></tr>
<tr><td>A</td><td>2-year term</td><td colspan="2">4-year term</td><td colspan="2">4-year term</td></tr>
<tr><td>B</td><td colspan="2">4-year term</td><td colspan="2">4-year term</td><td>2-year term</td></tr>
<tr><td>C</td><td colspan="2">4-year term</td><td>2-year term</td><td colspan="2">4-year term</td></tr>
</table>

Illinois, there is approximately a one-third probability.

There are two important aspects to this design. First, the random assignment was applied to legislators who were elected in the same election cycle. Thus, there are no differences arising from "partisan waves" or year-specific trends between legislators across Senate calendars. Secondly, and perhaps more interestingly, the term lengths (with the exception of Florida) were assigned after the legislators had filed for candidacy. In Arkansas and Texas, the random assignment occurred at the beginning of the legislative session after the election. In Illinois, the random assignment occurred after the filing deadlines for majority party candidates but before the election.

The one exception is Florida, however, where the random assignment took place before the filing deadlines. In 2012, after a redistricting map was ruled unconstitutional by the State Supreme Court, the State Senate chose to use lottery balls to randomly assign district numbers.[9] In Florida, all odd-numbered districts received four-year terms at the beginning of the decade, and all even-numbered districts received two-year terms to start with, so randomizing the district number (where there are 40 districts) is akin to randomizing term length and assigning a four-year term with probability one-half. Since the term length associated with a given district was known prior to candidate filing deadlines, it is possible that there may be some selection bias here. For instance, state legislators who are less congruent with their constituency's preferences might have chosen to run in a district with a four-year term than one with a two-year term.[10] Covariate balance tests provided in the Supplemental Materials suggest, however, that this bias is likely to be small.

5 Estimation and Data

5.1 Dependent Variable

The dependent variable is legislator roll-call voting. All legislators' roll-call votes were collected for each of the four states in each of the relevant sessions and coded into three categories: spending/taxes (the term "Economic" is used to label this group), those relating to the environment ("Environment"), and those that pertain to crime or restrictions on guns ("Crime"). These issue categories were chosen because there is a sufficient number of bills/roll-call votes that touch on these dimensions. Legislator ideal points are then estimated using the Bayesian IRT (Clinton, Jackman and Rivers 2004).

The number of non-unanimous roll-call votes that belong in each issue category, for each state-session, is listed in Table 2. Note that in most of the relevant legislative sessions,

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[9] In 2016, the Court ordered the Senate to redo their district numbers again and randomly assign district numbers to different districts. Given that this legislative session is too recent to do analysis on, the only legislative sessions that are ultimately included in the analysis are the ones that start in 2013 and 2015.

[10] On the other hand, given that Florida (and Arkansas, as well) imposes term limits which only count four-year terms and not two-year terms, a two-year term may be more desirable to state Senators who want to pursue a career with greater longevity.

	Issue	**Number of Votes**		**Number of Votes**
AR			IL	
2003-2004	Economic	147	2003-2004	591
	Environment	21		131
	Crime	61		149
	Total	302		1087
2013-2014	Economic	85	2013-2014	106
	Environment	26		48
	Crime	54		38
	Total	274		666
FL			TX	
2003-2004	Economic		2003-2004	217
	Environment			55
	Crime			80
	Total			530
2013-2014	Economic	112	2013-2014	147
	Environment	45		21
	Crime	71		111
	Total	748		754

Table 2: Number of Non-Unanimous Votes by Issue, State, and Session

a large majority of votes are unanimous and get discarded in the process of estimating ideal points. Some examples of bills that are "Economic" include bills on appropriations, economic development, and taxes. Bills that are classified into the "Environment" category pertain to water management, wildlife, conservation, and environmental protection. Finally, crime-related bills include gun restrictions, penalties for illegal drug use, and rules for convicted felons.

In the Supplemental Materials, I show how these ideal point estimates correspond with existing ones, notably those from Shor and McCarty (2011). Shor and McCarty (2011) estimate ideal points for state legislators that are consistent across states and across time, but these ideal points do not contain any ideological drift (i.e., they are time-invariant). To get a more fine-grained estimate of the electoral proximity effect, one should ideally treat the roll-call votes in the immediate session following the random assignment as distinct from those later in an incumbent's term. As such, I estimate a new set of ideal points for each legislator for each legislative session. I also construct a separate set of ideal point estimates that are consistent across states, bridging legislators across chambers using the National Political Action Test (NPAT) surveys; results using these NPAT scores are shown in the Supplemental Materials.

5.2 Independent Variable

Constituency preferences are measured using the district-level ideal points constructed by Tausanovitch and Warshaw (2013). They use multilevel regression and post-stratification (MRP) on pooled Cooperative Congressional Election Study (CCES) surveys that together contain at least 100,000 respondents to estimate mean ideal points for each state legislative district in the country. They have constructed these estimates for the last two decades.

In the first stage of analyses using scaled issue-specific measures of ideology, I also construct issue-specific scaled opinion measures using the CCES data. For both relevant decades, survey responses to various survey items pertaining to the three issue categories are pooled together and aggregated up to the state legislative district level using multi-level modeling. For the 2003-2004 legislative session, the CCES 2006-2012 surveys are pooled together. Given the slight disjuncture in time periods, I provide all results using the Tausanovitch and Warshaw (2013) scores in the Supplemental Materials. For the 2013-2014 session, responses to the 2013-2018 CCES surveys are pooled together.

In the second stage of analyses, I look primarily at individual roll-call votes taken in the Illinois State Senate. The actual roll call votes of individual legislators were secured through the online databases kept by the Illinois General Assembly. The measures of constituency opinion on these roll call votes come from the Paul Simon Public Policy Institute in Illinois, which has conducted several surveys between 2008 and 2016 that ask registered voters in Illinois about state-level proposals and policies, many of which have been voted on in the

Illinois State Senate. There are questions on same-sex marriage, medical marijuana, and various gun control policies.

Finally, to obtain values for relevant covariates, I used state legislative election data from ICPSR's State Legislative Election Returns (1967-2010) data. More recent election data was retrieved from each of the Secretaries of State's historical archives.

6 Empirical Strategy

To measure whether or not incumbents are more responsive closer to the end of their term, I examine the effect of term length on the relationship between voting behavior and constituency preferences. In particular, Table 3 presents a zoomed-in version of periods T and $T+1$. In the main analysis, I compare legislators in groups A and B in time period T. In the time frame that I am considering, T corresponds to the 2003-2004 and 2013-2014 legislative sessions.

I use the following empirical specification, where i denotes the legislator and t denotes the time period:

$$\begin{aligned} Legislator\,Ideal\,Point_{it} &= \alpha + \beta_1 ConstituencyIdealPoint_{it} + \beta_2 TwoYear_{it} \\ &+ \beta_3 ConstituencyIdealPoint_{it} \times TwoYear_{it} + \beta_4 Republican_{it} + \mu_t + \eta_s + \epsilon_{it} \quad (1) \end{aligned}$$

$TwoYear$ is an indicator variable equal to 1 if the legislator had been assigned to a two-year term, *Republican* is an indicator variable equal to 1 if the legislator is Republican, μ_t denotes a year fixed effect, and η_s denotes a state fixed effect. Summary statistics for the variables that are used in the empirical specification are available in Table A.7. I include controls in the main specification to attain greater precision, but results that omit all controls can be found in the Supplemental Materials. Both sets of results are substantively similar.

Table 3: Term Assignments (Periods T and T+1)

<table>
<tr><th></th><th>T</th><th>T + 1</th></tr>
<tr><td>A</td><td>2-year term</td><td>4-year term</td></tr>
<tr><td>B</td><td colspan="2">4-year term</td></tr>
</table>

In equation (1), the level of responsiveness of state legislators assigned a four-year term is captured by β_1, and the level of responsiveness of state legislators assigned a two-year term is $\beta_1 + \beta_3$. Thus, the coefficient of interest is β_3. If this coefficient is positive, then legislators assigned a two-year term are more responsive to their constituents.

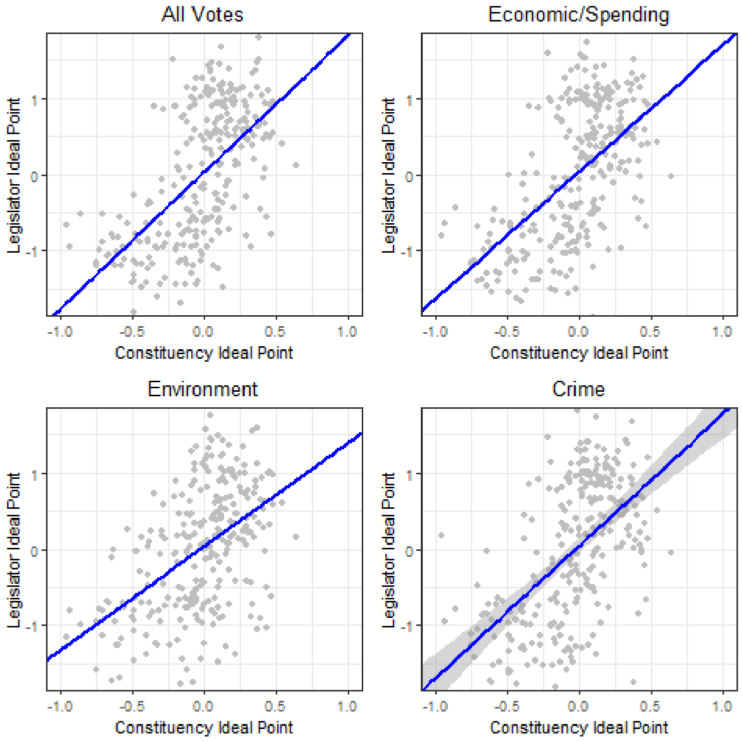
7 Empirical Results

7.1 Descriptive Statistics

The association between legislator ideal points and constituency ideal points is plotted in Figure 1 for all votes and for each issue category. Unsurprisingly, state legislators tend to vote more conservatively in more conservative districts. This association is positive and statistically significant ($p < 0.001$, using two-tailed tests), and this is true across issue categories.

In Figure 2, legislator ideal points (pooling all votes) are plotted against constituency ideal points in each state, and one can reach a similar conclusion at the state level that those representing conservative districts have more conservative roll-call voting records. This

Figure 1: Relationship Between Roll-Call Voting and Opinion



All Votes

Economic/Spending

Environment

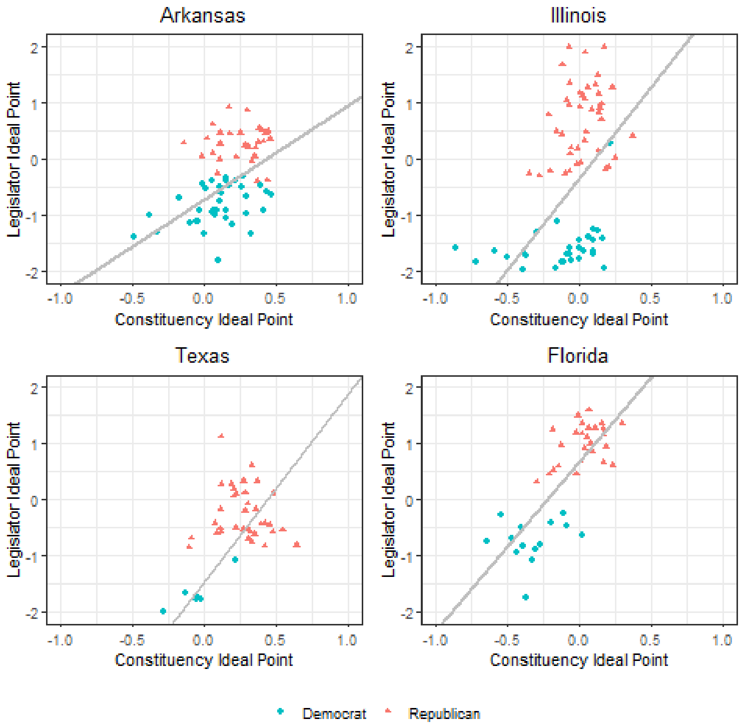
Crime

is consistent with past research on legislative responsiveness (Erikson, Wright and McIver 1993; Lax and Phillips 2012). The analogous plots featuring issue-specific ideal points by state can be found in the Supplemental Materials. In each state, Republicans have more conservative roll-call voting records than Democrats. Moreover, there is evidence of within-party responsiveness; Republicans (Democrats) in more conservative districts have higher ideal point scores than other Republicans (Democrats).

7.2 Electoral Proximity

To measure the electoral proximity effect, I compare the levels of responsiveness between legislators assigned a two-year term and those assigned a four-year term. As noted above, responsiveness is measured as the correlation between legislator roll-call voting and constituency preferences. As such, I call the magnitude of this correlation the "level of respon-

Figure 2: Relationship Between Constituency Opinion and Roll-Call Voting By State



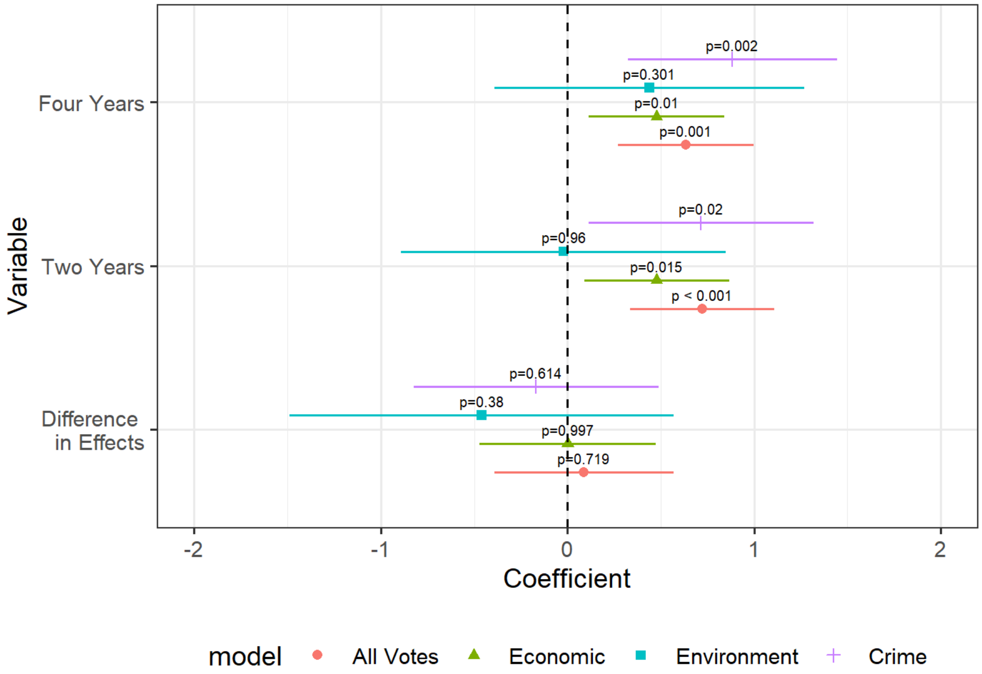
siveness."

Figure 3 shows the levels of responsiveness for each treatment condition, and in Table C.8, the specification in equation (1) is implemented. In Figure 3, the first row shows the level of responsiveness (on each issue and across all votes) for legislators assigned a four year term. The second row shows the same for legislators assigned a two-year term. The last row measures the difference between the first and second rows. The results are clear on evaluating the level of responsiveness for legislators assigned to either a four-year or a two-year term, looking at all votes and economic-related votes: the sign of the coefficient is positive and statistically significant ($p < 0.001$ in both cases, using two-tailed tests), suggesting that there is a positive relationship, regardless of treatment assignment, between roll-call voting and constituency opinion. However, there is no statistically significant difference in responsiveness between two-year legislators and four-year legislators, and this is true across issue categories. These findings suggest that term length does not have an impact on how legislators respond to their constituencies.

These results add to an existing literature that shows no effect of electoral incentives on roll-call voting behavior (e.g., Cohen 1997; Titiunik 2009; Wood and Lee 2009). More generally, however, one can speculate about why reducing term length does not lead to greater responsiveness. In particular, much of the prior literature in this area conceives of electoral proximity effects as a function of voters' punishment strategies (e.g., Canes-Wrone 2006). Scholars often suppose that legislators moderate their roll-call voting later in their term in order to avoid any electoral repercussions. At first glance, a null finding in this setting suggests that the sanctioning mechanism at the state legislative level may not be particularly strong. Indeed, existing studies indicate that state legislators are not punished to the same degree as legislators at the federal level (Rogers 2017), and in general, state elections have been nationalized (Hopkins 2018; Rogers 2016), such that electoral fortunes are somewhat disconnected from individual legislative behavior at subnational levels. Both these empirical trends would point to a small effect of electoral proximity on responsiveness.

However, it is possible there are other mechanisms at play here. For instance, one might surmise that state legislators have congruent preferences with their constituents on most issues, and so term length might not have an effect because legislators already adopt their constituents' positions, independent of electoral pressures. Alternatively, one might imagine that the benefits from winning reelection are sufficiently small such that even if legislators thought they were going to lose reelection by deviating from constituency preferences, the utility they accrue from voting for their preferred policy (relative to the popular policy) reigns supreme.

Figure 3: Electoral Proximity Effect

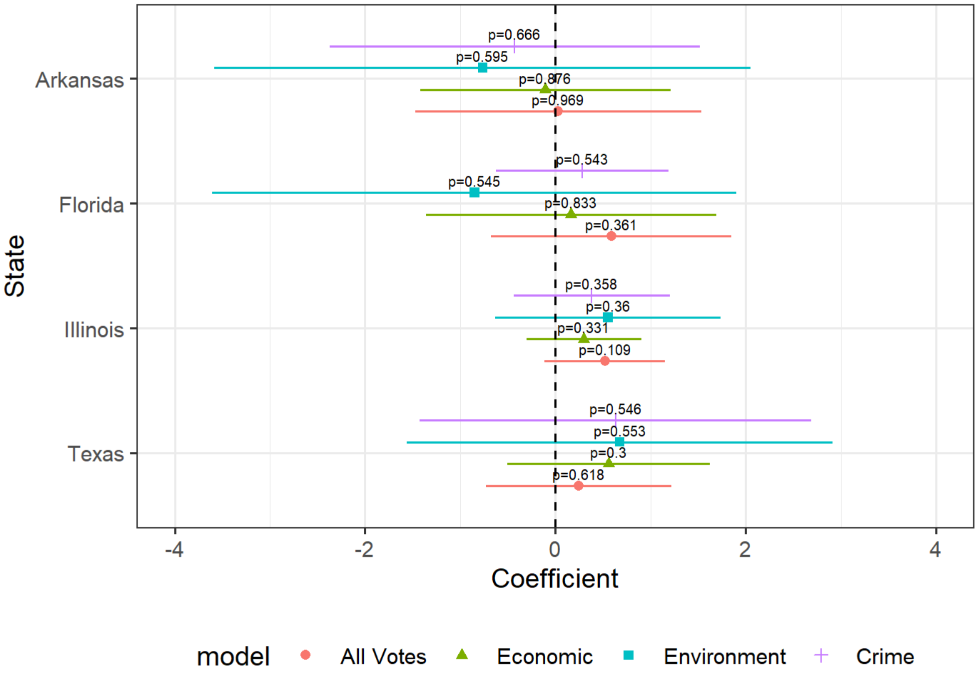


7.3 State-Specific Results

Figure 4 shows the results separately for each state. Across states and across issue categories, the effect is not statistically distinguishable from zero. The effects that yield the largest magnitude are the environment dimension in Texas and the crime dimension in Illinois. Across all states, with some exceptions, the electoral proximity effect is fairly close to zero.

These results suggest that factors like professionalization and similar characteristics are likely not positively associated with the effect of electoral proximity. For instance, Arkansas is a part-time legislature and ranks fairly low in commonly used professionalization scores (Squire 2007), whereas Florida and Illinois rank among the highest in all 50 states. Despite this, however, the electoral proximity effect is pretty similar across all of the states in our sample.

Figure 4: Electoral Proximity Effect by State



8 Roll-Call Votes in the Illinois State Senate

The previous analysis carries some drawbacks, which can be described twofold. First, the survey items used in the constituency opinion measures (e.g., those from Tausanovitch and Warshaw 2013) do not necessarily belong to the same dimension as the legislative roll-call votes that are scaled. The policy content may differ between the survey items on the CCES and the votes taken in these state legislatures. Second, Broockman (2016) suggests that a lack of representation/congruence using scaled measures may not necessarily translate to poor representation on specific issues. As such, if we looked at individual roll-call votes, we might get substantively different results.

In light of these drawbacks, this section examines the causal effect of electoral proximity on responsiveness on specific roll-call votes taken in the Illinois State Senate in the 2013-2014 legislative session. The Paul Simon Public Policy Institute conducts several surveys of registered voters in Illinois with questions that pertain to issues that state legislators voted on. These questions are typically phrased as follows: "There have been a number of proposals to address... Do you favor or oppose...?" In the 2013-2014 session, there are survey items on legalizing medical marijuana, restricting high-capacity ammunition clips, and passing a comprehensive pension reform package. In this analysis, any survey items corresponding to unanimous (or near-unanimous) Senate roll-call votes or strictly party-line votes are omitted.

There are two main advantages to these surveys. First, the questions address the same or similar policy content as the corresponding bill, so constituency preferences and roll-call votes can be measured on the same dimension. Second, these surveys have geographic indicators that provide researchers some information to derive (albeit with some error) the legislative district the respondent resides in. Using the respondents' zip code and some uniformity assumptions (e.g., Tausanovitch and Warshaw 2013), I map individuals to specific legislative districts.[11] One disadvantage here is that because we are looking at a more narrow setting, we cannot get a sense of how the electoral proximity effect on specific roll-call votes varies over time or across states. External validity concerns, which are arguably already present in the main analysis, are exacerbated here. Thus, one should be careful in attempting to generalize from these results to other settings or other points in time. Moreover, issues of statistical power arise here as well, which means that researchers should be cautious in interpreting the estimates.

As mentioned in the introduction, the Illinois State Senate had considered several important issues during the 2013-2014 legislative session. Given the state of Illinois's public finances, one important issue that came up for consideration was the pension reform bill that reconfigured cost-of-living adjustments and changed employee contribution rates. There was

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[11] For instance, if 40 percent of a zip code is located in the 5th district and 60 percent in the 6th district, then I assume the probability that a respondent living in that zip code resides in the 5th district is 40 percent (and 60 percent for the 6th district).

extensive discussion within the legislature on this bill, and many proposals were considered, including Senator Daniel Biss's bill (SB35), which proposed applying cost-of-living-adjustment (COLA) increases only to the first 25,000 dollars of pension income. Biss's proposal was rejected by the Senate, and instead, SB1, a compromise between SB35 and other proposals, was ultimately passed.

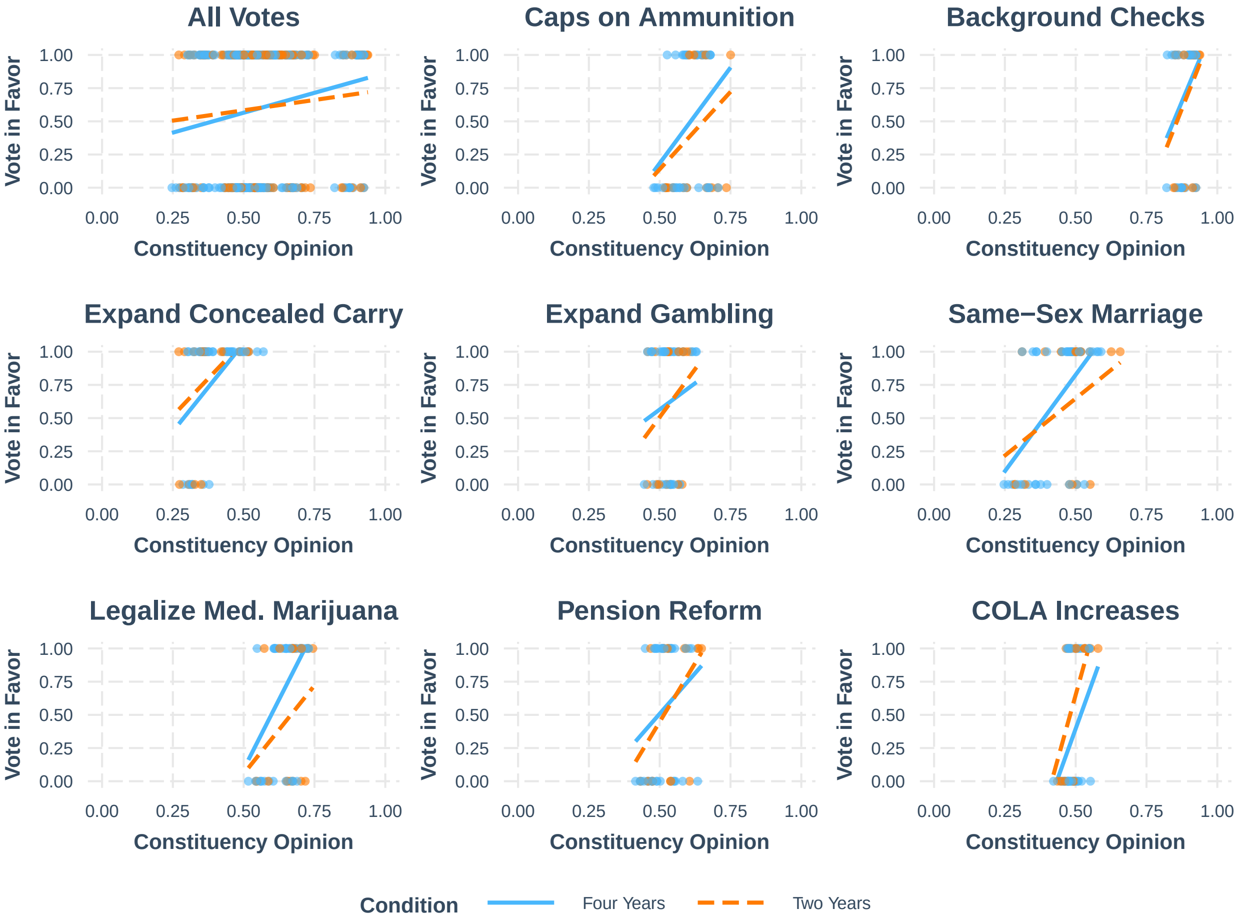
In addition, the Illinois State Senate had considered several bills that imposed gun limitations. Before the session started, the Seventh Circuit Court of Appeals had struck down the concealed carry ban that Illinois had implemented. As such, in 2013, the Illinois legislature became the last state to pass a concealed-carry law. They also expanded background checks and voted down a bill that would have banned high-capacity ammunition clips containing more than 10 bullets. Unrelatedly, the Illinois legislature also legalized medical marijuana, and the Senate passed a bill expanding casinos and gambling (but this latter measure was ultimately voted down in the House). Finally, Illinois became the 15th state to legalize same-sex marriage during this legislative session.

Table 4 describes the roll-call votes that are analyzed here and the corresponding survey data. The first column indicates the name of the bill, the second column gives the date of the Senate roll call (with the vote margin in parentheses), the third column gives the date (or range of dates) of the poll(s) that is/are used to measure constituency preferences, the fourth column provides the actual wording of the survey question, and the last column indicates the total sample size of the survey(s) used. As one can see, while there is variation in the specificity of the survey questions, which is somewhat correlated with issue complexity (e.g., the pension reform question is extremely detailed, whereas the ammunition question is more brief), they are reasonably tailored to the content of the bill, and the surveys are conducted pretty close to (often before, but not always) the time of the roll-call vote. For two of the items (background checks and concealed carry), I use policy questions from the CCES, since they are incidentally very related to the content of two of the major bills that were voted on

Table 4: Individual Roll-Call Votes

Bill	**Roll-Call Date**	**Poll Date**	**Survey Question**	**Sample Size**
Legalizing medical marijuana (HB1)	5/17/2013 (35-21)	January 2013	Some in Springfield have proposed that the state should make it legal for people with certain health issues to be prescribed small amounts of marijuana. Generally speaking, do you favor or oppose legalized medical marijuana in Illinois?	600
Banning high-capacity ammunition clips (HB1346)	5/31/2013 (28-31)	January 2013	Do you favor or oppose banning high-capacity ammunition clips that can contain more than 10 bullets?	600
Apply COLA increases to first $25K (SB35)	3/20/2013 (23-30)	January 2013	As you may have heard, the state of Illinois has an unfunded pension liability of about 96 billion dollars...I'm going to read some proposals that state officials have made to fix the pension liability. For each, please tell me whether you favor or oppose that proposal: A proposal to apply cost-of-living increases only to the first $25,000 of retirees' pensions	600
Expanding legalized gambling (SB1739)	5/2/2013 (32-20)	January 2013-February 2014	Do you favor or oppose a proposal expanding legalized gambling in the state?	1,539
Legalizing same-sex marriage (SB10)	2/15/2013 (34-21)	January 2013 - September 2014	Which of the following three statements comes closest to your position on the legal rights of gay and lesbian couples in Illinois?: Gay and lesbian couples should be allowed to legally marry; Gay and lesbian couples should be allowed to form civil unions, which would give them some legal rights; OR There should be no legal recognition of relationships between gay and lesbian couples?	1,539
Comprehensive pension reform (SB1)	12/3/2013 (30-24)	February 2014	Last year the legislature passed and the governor signed a pension reform bill. It is designed to save Illinois' under-funded public employee pension system 160 billion dollars over 30 years, and would eventually fully fund the system. It would decrease the amount workers pay into the program, but would also cut cost-of-living increases for state retirees. Generally speaking, do you approve or disapprove of the new law?	1,001
Concealed carry (HB0183)	5/31/2013 (45-12)	2014 (CCES)	On the issue of gun regulation, are you for or against each of the following proposals?: Make it easier for people to obtain concealed-carry permit	2,383 (disaggregated)
Background checks (HB1189)	5/31/2013 (41-15)	2014 (CCES)	On the issue of gun regulation, are you for or against each of the following proposals?: Background checks for all sales, including at gun shows and over the Internet	2,383 (disaggregated)

Figure 5: Electoral Proximity Effect on Individual Roll-Call Votes



in this legislative session.

There is also variation in the sample size for the questions. Obviously, researchers desire larger survey samples to obtain more precise estimates of opinion at the district level. Since there are 59 State Senate districts, the sample sizes listed in Table 4 suggest that we observe between 10 and 35 observations per district across issues. To leverage all of the survey data in estimating constituency preferences, I use multi-level regression and post-stratification (MRP), which is a common method in the literature for opinion estimation (e.g., Gelman and Hill 2006; Warshaw and Rodden 2012). The methodological details are presented in the

Appendix, and the results from disaggregating the surveys, which are substantively similar to those using the MRP estimates, are shown there as well.

Table 5: Responsiveness on Specific Roll-Call Votes

	Dependent variable:			
	Vote in Favor			
	Pooled	Gay Marriage	Pension Reform	Background Checks
Const. Opinion	2.233 (0.452)	2.544 (0.944)	2.015 (1.908)	4.446 (2.971)
Two-Year	0.108 (0.176)	0.327 (0.639)	-0.707 (1.493)	-0.297 (4.254)
Const. Opinion * Two-Year	-0.228 (0.281)	-0.985 (1.352)	1.262 (1.623)	0.205 (5.017)
Observations	442	55	54	58
R^2	0.055	0.469	0.019	0.257
Adjusted R^2	0.049	0.438	−0.040	0.216

In Figure 5, the results for each bill are shown visually, and the regression tables are shown in Table 5 and Table 6. I do not detect a statistically significant effect for any of the bills. Focusing just on the magnitudes of the coefficients, the bill to limit COLA increases to a certain amount of pension income carries the largest (positive) effect among the votes considered in this analysis. Moreover, if we redefine the dependent variable as a binary variable equal to one if the legislator votes with the majority (or the median) of her district, we can express the results here more concretely. The bill-specific results with this redefinition of the dependent variable are available in Table G.19, and they show similar results. In particular, across all votes, there is no difference between two-year and four-year legislators across roll-call votes.

9 Notes on Design

In this section, I detail some caveats to the design in this paper and how I address them.

First, two of the states in this design—Arkansas and Florida—have term-limit statutes that only count four-year terms. This means that legislators can only run for office again

Table 6: Responsiveness on Specific Roll-Call Votes (cont'd)

	Dependent variable:				
			Vote in Favor		
	Amm. Clips	Gambling	Med. Marij.	COLA Incr.	Conc. Carry
Constituency Opinion	2.090 (2.082)	1.411 (2.190)	3.138 (1.901)	2.190 (2.763)	2.518 (0.888)
Two-Year	0.060 (1.379)	-0.522 (1.807)	0.520 (1.719)	-0.519 (2.574)	0.188 (0.188)
Constituency Opinion * Two-Year	-0.094 (2.359)	1.320 (3.267)	-0.934 (2.490)	1.621 (4.554)	-0.351 (1.426)
Observations	56	56	52	52	59
R^2	0.219	0.224	0.143	0.035	0.119
Adjusted R^2	0.174	0.179	0.089	−0.026	0.071

if they have not already served two four-year terms.[12] In the time period of this analysis, these term-limit statutes complicate the design because it is unclear whether those serving four-year terms will be less responsive because these incumbents are more likely to be term-limited or because they have more time left until their next election. This issue is addressed in three ways. The two states that *do not* have term limits (i.e., Illinois and Texas) are examined separately to see if results are substantively different in those states. Next, I try controlling for term-limit status in the main specification. Finally, again using all states, the term-limited legislators are thrown out. The latter two approaches are presented in Table D.14 and Table D.15. Across these three approaches, the results are not substantively different.

Another caveat is that the opinion and legislator ideology estimates used in the analysis contain some measurement error. When estimating constituency preferences, this issue is exacerbated when dealing with small samples of survey respondents. To account for this, I use a similar method to some prior studies (Kastellec et al. 2015; Lax, Phillips and Zelizer 2019), which is sometimes referred to as *propagated uncertainty*, which estimates the marginal distribution of a parameter vector while integrating over the measurement error in the variables. This method involves three steps. First, one samples the variables $\mathbf{X}$ that are measured with

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[12] In the case of Arkansas, this statute had been struck down in 2014 by the voters and replaced with a rule that instead allows legislators to serve until they have completed a certain number of years in office.

error from a distribution $p(\mathbf{X}|\mathbf{Z})$ (conditional on the data Z and a measurement model). Second, conditional on the covariates, one estimates the parameter vector $\hat{\beta}$ and its associated variance-covariance matrix $\hat{\mathbf{V}}$. Finally, one samples a parameter vector $\tilde{\beta}$ from a multivariate normal distribution with mean vector $\hat{\beta}$ and variance-covariance matrix $\hat{\mathbf{V}}$. Using this method, the main regression analyses in the paper account for the additional measurement error that arises from opinion and ideal point estimation. The results unadjusted for any measurement error can be found in the Supplemental Materials.

One caveat for the analysis of individual roll-call votes is that only votes that are specifically associated with survey questions that were asked at the time are used. Thus, this somewhat limits the universe of roll-call votes that one could use to measure the electoral proximity effect in this context. The likely effect is that the estimates are biased upwards. Survey questions often ask questions on policies that are salient to the media and to voters; given this, since the electoral proximity effect is likely to be higher on issues that are more salient, focusing on just these particular roll-call votes will probably paint a "rosier" picture of the effect of term length than if we hypothetically had access to a wider range of roll-call votes. Of course, one could argue that the salience of state legislative roll-call votes is low across the board, in which case one does not have to worry about biases in the selection of votes. However, if we suppose that there are biases here, since the results below suggest a small or negative effect of electoral proximity on responsiveness across roll-call votes, this truncation of the universe of possible roll-call votes likely does not significantly alter our substantive conclusions.

Finally, some aspects of the randomization require more detail and assumptions. In Illinois, in particular, the randomization is conducted at the level of groups, not legislators. Districts are divided into three groups, and these groups are then allotted to one of the three treatment conditions described in Table 1. Thus, we have to make an additional assumption here that the assignment of districts to groups is random as well. The covariate balance tests provide indirect support for this to the extent that in Illinois, the districts assigned to receive the four-year term do not look different across relevant covariates than the districts assigned a two-year term.

10 Conclusion

This paper exploits a randomization of term length in four states, where one group of legislators is assigned a two-year term and the other is assigned a four-year term. In the main analysis, I measure the difference in responsiveness, or the association between constituency opinion and legislative roll-call voting, between the treatment conditions. This test is performed by collecting over 2,000 roll-call votes and estimating ideal points for each legislator in each relevant legislative session. These votes are then coded into different categories, like economic, environmental, and crime-related, to assess responsiveness on broader issue areas. This design carries several advantages. In particular, one can control for observed and unobserved covariates between legislators. In addition, differences in electoral timing are not affected by "partisan waves" or year-specific trends, and the roll-call agenda is fixed, such that variation in roll-call voting across treatment conditions is measured on the same set of bills. Another neat feature of the design is that the random assignment took place after candidates filed for candidacy (except in Florida), limiting opportunities for legislators to "select" into treatment conditions.

This paper then estimates the causal effect of electoral proximity on responsiveness on individual roll-call votes. To perform this analysis, survey data is collected on several specific measures that legislators in the Illinois State Senate voted on in the 2013-2014 legislative session. These items included many highly salient bills like the legalization of same-sex marriage, the expansion of background checks, and the reform of the retirement system. These survey data allow us to measure constituency opinion at relatively granular levels, enabling an analysis of the relationship between roll-call votes and support for the bill in a given district. Across these empirical tests, this paper finds no evidence for an electoral proximity effect. Thus, this analysis provides another data point for the argument that elections do not impact legislative roll-call voting behavior, which prior studies have corroborated (e.g., Cohen 1997; Titiunik 2016; Wood and Lee 2009).

Since the sample sizes here are relatively small, some caution should be exercised in interpreting the main results. To address concerns that the effect of term length might manifest differently in some states rather than others, results are broken down at the state level. This paper shows that the effect of electoral proximity does not vary much across states. In particular, the effect sizes do not appear to be correlated with potentially important state-level variables like professionalization.

One interesting question that can be posed here is what degree these results can speak to previous findings in the literature that identify effects of electoral timing on roll-call voting or responsiveness. On the one hand, the design put forward in this paper has strong internal validity. Many relevant confounders, like incumbent quality, are held constant, and we can rule out some alternative explanations, like a shifting roll-call agenda or "learning-by-doing." On the other hand, the degree to which these results are generalizable to the national level is an open question. Actors at the state legislative level are arguably less visible than U.S. Senators, for instance. As such, it might be the case that electoral proximity has a smaller effect at the state legislative level than at other levels of government. Therefore, these considerations might limit how much we can extrapolate from these results to political actors at higher levels of government.

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Supplemental Materials

A Difference in Covariates

The difference in covariates between the legislators who were originally assigned a four-year term and the legislators who were originally assigned a two-year term can be found in Figure A.6. The covariates that I include here are the legislator's ideal point (constructed using all roll-call votes), the constituency ideal points from Tausanovitch and Warshaw (2013), the margin of victory for the incumbent in the most recent election, the presidential vote share for the district that the incumbent represents (in the most recent presidential election), the number of general elections that the incumbent won, how many years the legislator has served, the number of four-year terms served, and party affiliation.

To check whether or not the randomization worked, in the sense that relevant covariates are on average the same across calendars, I conduct covariate balance tests to see if those originally assigned a four-year term were any more or less different in observed covariates than those originally assigned a two-year term. For each variable, I regressed the assignment of a two-year term or a four-year term at the beginning of each decade on the corresponding covariate, with state-level fixed effects. Across all these covariate tests, one cannot reject the null hypothesis that those assigned a four-year term are different than those assigned a two-year term.[13]

In addition, summary statistics are available in Table A.7. These variables are used in the empirical specifications for the main results in this paper.

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[13] Note that the presidential vote share variable is equal to the 2008 presidential vote share for observations in years before 2010 and the 2012 presidential vote share for observations after 2010. There are missing values for this variable for Arkansas's 2008 presidential vote share.

Figure A.6: Difference Between Treated and Control Groups (all significance tests are two-tailed)

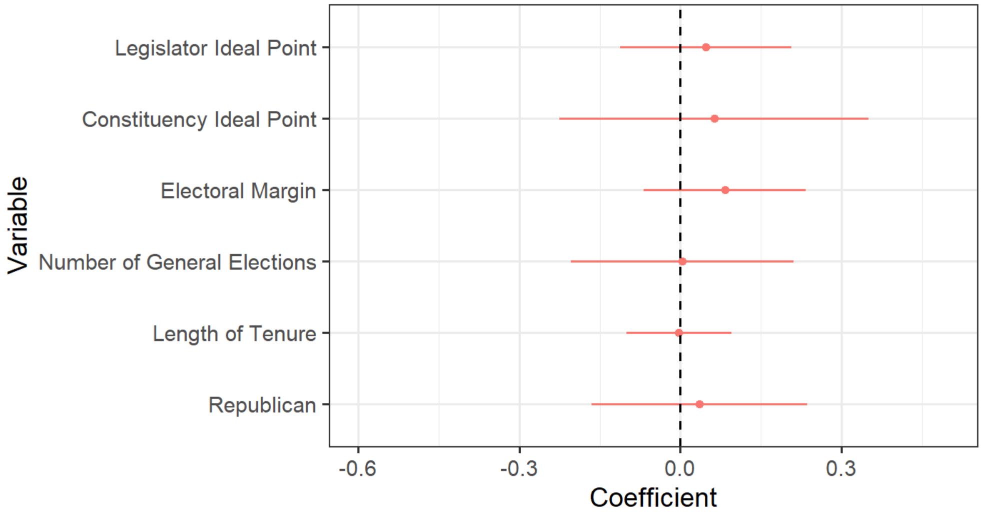
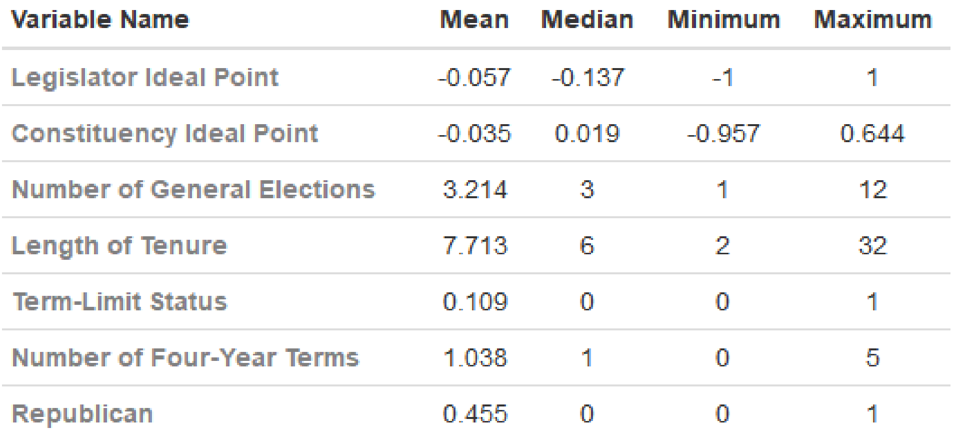
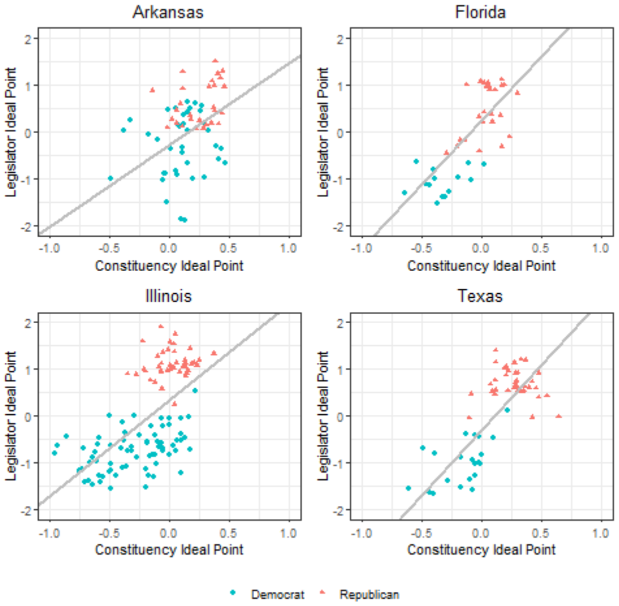


Table A.7: Summary Statistics



Variable Name	Mean	Median	Minimum	Maximum
Legislator Ideal Point	-0.057	-0.137	-1	1
Constituency Ideal Point	-0.035	0.019	-0.957	0.644
Number of General Elections	3.214	3	1	12
Length of Tenure	7.713	6	2	32
Term-Limit Status	0.109	0	0	1
Number of Four-Year Terms	1.038	1	0	5
Republican	0.455	0	0	1

Figure B.7: Responsiveness by State (Economic)



B Descriptive Statistics on Ideal Points

In Figure B.7, the association between constituency opinion and the legislator ideal points constructed from economy-related roll-call votes is shown. The same conclusion can be drawn as for the analogous graph presented in the body of the paper. In each state, there is a strong positive and statistically significant relationship between opinion and ideology. The same is true for the ideal points constructed on crime-related roll-call votes (in Figure B.8 and environment-related votes (in Figure B.9), with Florida being an exception on the environment issue.

Moreover, the relationship between the Shor and McCarty scores and the ideal points for each issue category constructed in this analysis is plotted in Figure B.10. The ideal points that I estimated are on the y-axis, and the Shor and McCarty ideal points are on the x-axis. In each panel of the figure, there is a positive and statistically significant association between the two sets of ideal points. In all cases, the correlation is larger than 0.6, with the ideal points constructed from all votes and economic-related votes having the largest association.

Figure B.8: Responsiveness by State (Crime)

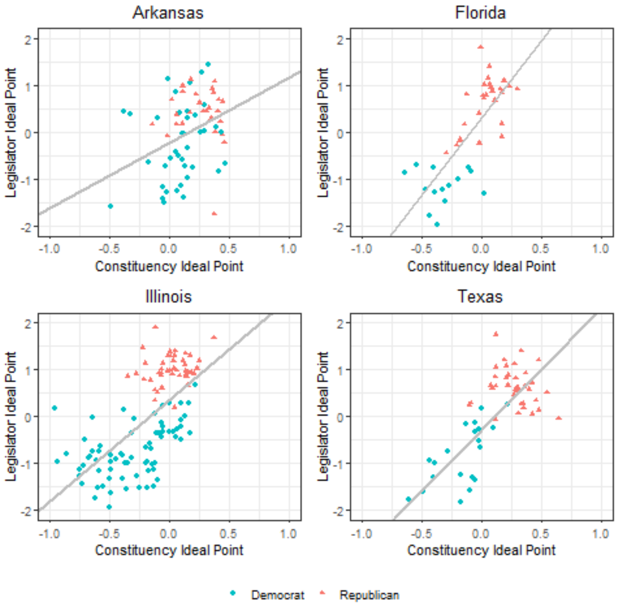


Figure B.9: Responsiveness by State (Environment)

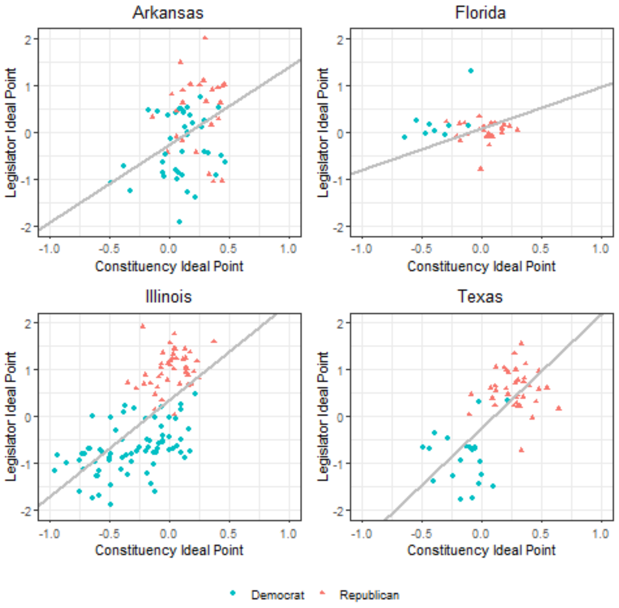
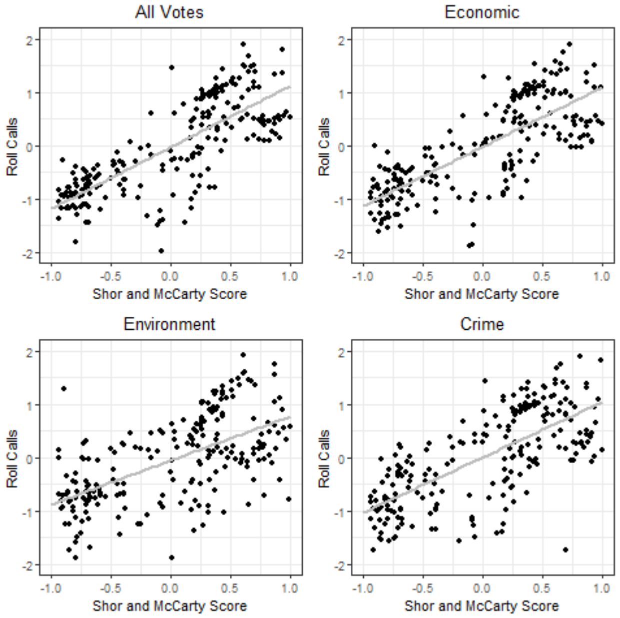


Figure B.10: Comparison of Ideal Points



C Alternative Specifications and Sensitivity Checks

In this section, I show how the main results (which are shown in Table C.8) in the paper are robust to various alternative specifications. In Table C.9, the results are shown omitting any control variables. Table C.10 shows the results using the Tausanovitch and Warshaw (2013) scores instead of the issue-specific public opinion measures, and Table C.11 shows the results using NPAT legislator ideology scores. Table C.12 shows the results unadjusted for any measurement error. Finally, Table shows the results using a measure of public opinion for the 2013-2014 legislative session that only uses the CCES surveys from 2008 to 2012 (in order to create an ex ante measure).

D Term Limits

Table D.14 shows the results when controlling for term-limit status. Moreover, Table D.15 shows the results when omitting term-limited legislators. Across all of these results, the

Table C.8: Electoral Proximity Effect (time period T)

	Dependent variable:			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	0.991 (0.231)	0.986 (0.270)	0.786 (0.285)	0.867 (0.379)
	p < 0.001	p < 0.001	p=0.358	p=0.111
Two Years	-0.112 (0.092)	-0.077 (0.096)	-0.007 (0.115)	0.189 (0.146)
	p=0.218	p=0.243	p=0.932	p=0.175
Con. Ideal Point * Two Years	-0.060 (0.284)	-0.213 (0.315)	-0.257 (0.343)	0.098 (0.458)
	p=0.171	p=0.197	p=0.618	p=0.599
Observations	288	288	288	288
R^2	0.750	0.678	0.476	0.604
Adjusted R^2	0.743	0.669	0.461	0.593

Note: All significance tests are two-tailed

Table C.9: Electoral Proximity Effect (omitting controls)

	Dependent variable:			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	2.254^{***} (0.314)	4.224^{***} (0.628)	1.779^{***} (0.561)	1.989^{***} (0.547)
	p¡0.001	p¡0.001	p=0.002	p¡0.001
Two Years	0.008 (0.144)	0.076 (0.155)	0.101 (0.126)	0.276^{*} (0.162)
	p=0.958	p=0.626	p=0.421	p=0.089
Con. Ideal Point * Two Years	0.085 (0.478)	0.511 (0.980)	−0.107 (0.836)	−0.184 (0.799)
	p=0.859	p=0.602	p=0.899	p=0.818
Observations	288	288	288	288
R^2	0.250	0.231	0.061	0.087
Adjusted R^2	0.242	0.223	0.051	0.077

Note: All significance tests are two-tailed

Table C.10: Electoral Proximity Effect (TW scores)

	Dependent variable:			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	2.254 (0.314)	1.916 (0.316)	1.557 (0.250)	1.430 (0.320)
	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
Two Years	0.008 (0.144)	−0.017 (0.145)	0.028 (0.115)	0.211 (0.147)
	$p = 0.958$	$p = 0.906$	$p = 0.806$	$p = 0.153$
Con. Ideal Point * Two Years	0.085 (0.478)	−0.051 (0.480)	−0.122 (0.380)	0.178 (0.488)
	$p = 0.859$	$p = 0.917$	$p = 0.749$	$p = 0.716$
Observations	288	288	288	288
R^2	0.250	0.184	0.187	0.133
Adjusted R^2	0.242	0.175	0.178	0.124
Note:		All significance tests are two-tailed		

Table C.11: Electoral Proximity Effect (NPAT scores)

	Dependent variable:			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	1.834 (0.217)	2.359 (0.501)	1.883 (0.520)	1.922 (0.401)
	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
Two Years	0.062 (0.099)	0.076 (0.124)	−0.032 (0.117)	0.057 (0.119)
	$p = 0.532$	$p = 0.539$	$p = 0.782$	$p = 0.629$
Con. Ideal Point * Two Years	0.071 (0.330)	0.377 (0.783)	0.600 (0.776)	0.311 (0.586)
	$p = 0.830$	$p = 0.631$	$p = 0.441$	$p = 0.596$
Observations	288	288	288	288
R^2	0.320	0.133	0.101	0.154
Adjusted R^2	0.313	0.124	0.091	0.145
Note:		All significance tests are two-tailed		

Table C.12: Electoral Proximity Effect (Unadjusted Results)

	Dependent variable:			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	0.801 (0.164)	1.000 (0.281)	0.742 (0.450)	1.237 (0.328)
	p ¡ 0.001	p ¡ 0.001	p = 0.101	p ¡ 0.001
Two Years	−0.097 (0.061)	−0.052 (0.071)	0.017 (0.093)	−0.035 (0.079)
	p = 0.114	p = 0.467	p = 0.853	p = 0.656
Con. Ideal Point * Two Years	−0.115 (0.201)	−0.420 (0.362)	−0.329 (0.635)	−0.117 (0.415)
	p = 0.569	p = 0.247	p = 0.606	p = 0.779
Observations	288	288	288	288
R^2	0.750	0.671	0.453	0.593
Adjusted R^2	0.743	0.661	0.438	0.581
Note:			All significance tests are two-tailed	

Table C.13: Electoral Proximity Effect (using pre-treatment opinion)

	Dependent variable:		
	Legislator Ideal Point		
	Economic	Environment	Crime
	(1)	(2)	(3)
Constituency Ideal Point	1.581 (0.322)	−0.638 (0.623)	4.568 (1.402)
	$p < 0.001$	p = 0.307	p = 0.002
Two Years	−0.159 (0.102)	0.019 (0.099)	0.111 (0.137)
	p = 0.119	p = 0.850	p = 0.422
Constituency Ideal Point * Two Years	−0.483 (0.330)	0.600 (0.830)	−2.329 (1.776)
	p = 0.145	p = 0.471	p = 0.191
Observations	288	288	288
R^2	0.737	0.510	0.338
Adjusted R^2	0.730	0.496	0.319
Note:		All significance tests are two-tailed	

same substantive conclusion arises which is that there is not much evidence for an electoral proximity effect.

Table D.14: Electoral Proximity Effect (controlling for term-limit status)

	Dependent variable:			
	Legislator Ideal Point			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	0.999 (0.233)	2.655 (0.433)	0.742 (0.551)	1.333 (0.642)
	$p < 0.001$	$p < 0.001$	$p = 0.180$	$p = 0.039$
Two Years	−0.105 (0.089)	−0.107 (0.093)	0.030 (0.095)	0.217 (0.146)
	$p = 0.238$	$p = 0.253$	$p = 0.750$	$p = 0.138$
Con. Ideal Point * Two Years	−0.060 (0.285)	−0.850 (0.567)	−0.437 (0.612)	−0.469 (0.699)
	$p = 0.834$	$p = 0.136$	$p = 0.476$	$p = 0.503$
Observations	288	288	288	288
R^2	0.741	0.752	0.513	0.322
Adjusted R^2	0.732	0.744	0.497	0.300

Note: All significance tests are two-tailed

Table D.15: Electoral Proximity Effect (omitting term-limited legislators)

	Dependent variable:			
	Legislator Ideal Point			
	All Votes	Economic	Environment	Crime
	(1)	(2)	(3)	(4)
Constituency Ideal Point	1.095 (0.242)	2.674 (0.437)	1.225 (0.563)	1.281 (0.675)
	$p < 0.001$	$p < 0.001$	$p = 0.031$	$p = 0.059$
Two Years	−0.109 (0.090)	−0.113 (0.094)	0.043 (0.094)	0.213 (0.149)
	$p = 0.223$	$p = 0.230$	$p = 0.651$	$p = 0.154$
Con. Ideal Point * Two Years	−0.145 (0.293)	−0.905 (0.573)	−0.800 (0.613)	−0.426 (0.723)
	$p = 0.621$	$p = 0.116$	$p = 0.194$	$p = 0.556$
Observations	276	276	276	276
R^2	0.742	0.754	0.536	0.312
Adjusted R^2	0.734	0.747	0.522	0.291

Note: All significance tests are two-tailed

E Opinion Estimation

Multi-level regression and post-stratification (MRP) is used to measure public opinion at the constituency level. This has two steps. In the first step, using survey data, respondents' support for a particular policy is regressed on various individual-level demographic characteristics, specifically gender, education, and race, and a district-level intercept, which is itself modeled as a function of constituency-level predictors, including the proportion of veterans, median income, the percent of same-sex couples, and presidential vote share. Given the results of the multi-level regression, we calculate predicted probabilities for each demographic-geographic type in our specification and weight these predicted probabilities by their recorded value in the Census.

Denote support for a given policy by Y_i for a given individual i. This value is either 1 if the individual supports the policy or 0 if the individual opposes it.[14] The individual-level predictors are race ("White," "Black," "Hispanic," and "Other"), education ("No HS," "High school graduate," "Some college," and "College graduate,"), and gender ("Female" and "Male"). Formally, we use the following specification:

$$Pr(Y_i = 1) = logit^{-1}(\beta^0 + \beta^{female} * female_i + \alpha^{race}_{k[i]} + \alpha^{educ}_{l[i]} + \alpha^{district}_{j[i]} + \alpha^{poll}_{p[i]})$$

where k denotes the category of race that respondent i falls into, l denotes the category of education i belongs to, j denotes the district that i resides in, and p denotes the poll that i is responding to. The district intercepts are modeled as a function of district-level predictors:

$$\alpha^{district}_j \sim N(\beta^{med.income} * med.income_j + \beta^{veteran.prop} * veteran.prop_j + \beta^{same.sex.prop} * same.sex.prop_j + \beta^{presidential.vote.share} * presidential.vote.share_j, \sigma^2_{state})$$

---

[14] Respondents who said don't know or that they hadn't heard enough are counted as missing.

To clarify, the variance of the state coefficient is constant across all states. Furthermore, the following individual-level and geographic-level coefficients are modeled as follows:

$$\alpha_k^{race} \sim N(0, \sigma_{race}^2) \qquad \text{for} \quad k = 1, ..., 4$$
$$\alpha_l^{educ} \sim N(0, \sigma_{educ}^2) \qquad \text{for} \quad l = 1, ..., 4$$
$$\alpha_p^{poll} \sim N(0, \sigma_{poll}^2) \qquad \text{for} \quad p \in \mathbb{R}_+$$
$$\alpha_j^{district} \sim N(0, \sigma_{state}^2) \qquad \text{for} \quad j = 1, ..., 59$$

Using these results, we calculated the predicted probability of supporting the policy for each demographic-geographic type and used Census data to post-stratify. Given 59 districts, 2 gender categories, 4 race groups, and 4 education groups, we have $59 * 2 * 4 * 4 = 1,888$ demographic-geographic types. Using the model estimated above for respondent preferences, we calculated predicted probabilities for each of these $1,888$ categories.[15]

We weight these probabilities by the recorded population level listed in the Census. Thus, if d denotes a particular district, $\hat{\theta}_j$ is the predicted probability for a given cell j, N_j is the Census population size for cell j, and $\hat{y}_d$ is the proportion of individuals supporting a given policy for district d, then

$$\hat{y}_d = \frac{\sum_{j \in d} N_j \hat{\theta}_j}{\sum_{j \in d} N_j}$$

F Disaggregated Estimates (Individual Roll-Call Votes)

The results presented in the paper used estimates of constituency opinion in Illinois derived from multi-level regression and post-stratification. In this section, I show the corresponding estimates when disaggregating the surveys. Figure F.11 shows that, consistent with prior

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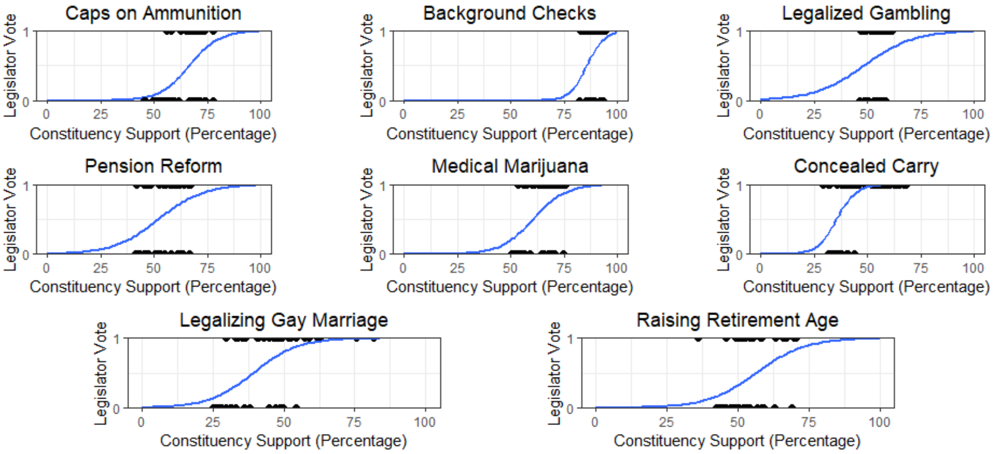
[15] For the poll coefficients, we take the average of the intercepts.

Figure F.11: Responsiveness on Individual Roll-Call Votes



literature, there is a positive association between legislators' roll-call votes and constituency opinion.

Table F.16 shows the electoral proximity effect on the gay marriage, pension reform, and concealed carry bills. Table F.17 shows the effect for medical marijuana, background checks, and raising the retirement age. Finally, Table F.18 presents the results for expanded gambling and the caps on ammunition. Across all eight of these bills, there is no evidence of electoral proximity, though the caps on ammunition bill comes closest to achieving significance. When looking at just the signs of the relevant coefficients, they are mostly positive.

G Additional Descriptive Statistics/Results on Individual Roll-Call Votes

In this section, I describe the data on the individual roll-call votes in the Illinois State Senate in more depth and show some additional results. First, Figure G.12 shows the distribution of constituency opinion on these roll-call votes. Second, Table G.19 compares two-year and four-year legislators, redefining the dependent variable as equal to one if the legislator votes with the median of her district. Table G.20 shows the proportion of legislators that vote

Table F.16: Responsiveness on Specific Roll-Call Votes

	Dependent variable:		
		Vote in Favor	
	Gay Marriage	Pension Reform	Concealed Carry
Constituency Opinion	0.015 (0.006)	0.015 (0.007)	0.016 (0.007)
	p = 0.012	p = 0.033	p = 0.023
Two-Year	−0.124 (0.679)	0.912 (0.713)	0.269 (0.472)
	p = 0.856	p = 0.207	p = 0.571
Constituency Opinion * Two-Year	0.001 (0.011)	−0.016 (0.013)	−0.007 (0.012)
	p = 0.948	p = 0.207	p = 0.541
Observations	55	54	57
R^2	0.160	0.091	0.107
Adjusted R^2	0.110	0.036	0.057
Residual Std. Error	0.462 (df = 51)	0.492 (df = 50)	0.399 (df = 53)
F Statistic	3.232** (df = 3; 51)	1.663 (df = 3; 50)	2.124 (df = 3; 53)
Note:			All significance tests are two-tailed

Table F.17: Responsiveness on Specific Roll-Call Votes

	Dependent variable:		
		Vote in Favor	
	Medical Marijuana	Background Checks	Raising Retirement Age
Constituency Opinion	0.006 (0.005)	0.022 (0.016)	0.001 (0.004)
	p = 0.270	p = 0.174	p = 0.867
Two-Year	−0.237 (0.703)	−0.951 (2.387)	0.156 (0.410)
	p = 0.738	p = 0.692	p = 0.706
Constituency Opinion * Two-Year	0.00004 (0.010)	0.010 (0.027)	0.002 (0.006)
	p = 0.998	p = 0.695	p = 0.717
Observations	56	56	52
R^2	0.074	0.075	0.081
Adjusted R^2	0.021	0.021	0.024
Residual Std. Error	0.483 (df = 52)	0.442 (df = 52)	0.493 (df = 48)
F Statistic	1.391 (df = 3; 52)	1.399 (df = 3; 52)	1.420 (df = 3; 48)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table F.18: Responsiveness on Specific Roll-Call Votes

	Dependent variable:	
	Vote in Favor	
	Gambling	Caps on Ammunition
Constituency Opinion	0.0004 (0.007)	−0.0003 (0.005)
	p = 0.948	p = 0.957
Two-Year	−0.282 (0.647)	−0.859 (0.546)
	p = 0.665	p = 0.121
Constituency Opinion * Two-Year	0.005 (0.012)	0.012 (0.008)
	p = 0.654	p = 0.139
Observations	52	59
R^2	0.007	0.059
Adjusted R^2	−0.055	0.008
Residual Std. Error	0.505 (df = 48)	0.500 (df = 55)
F Statistic	0.113 (df = 3; 48)	1.154 (df = 3; 55)
Note:		*p<0.1; **p<0.05; ***p<0.01

with the majority of their district and the difference between the congruence of two-year and four-year legislators. Finally, the crosstabs of individual roll-call votes are presented in Table G.21 that show the proportion of legislators who vote with their district median and the proportion of legislators who do not.

H Difference-in-Differences Design

As a robustness check, one can make use of the 2005-2006 and 2015-2016 legislative sessions and run a regression with district and session fixed effects. In order to carry out this analysis, I collect the roll-call votes in these sessions and calculate ideal points for each legislator. These ideal points are plotted against constituency opinion in Figure H.13 in order to visualize the data.

As part of the difference-in-differences design, I run two tests here. The first test includes all observations in 2005-2006 and 2015-2016 and controls for district fixed effects and period fixed effects. This is summarized in the first column of Table H.22. The second column subsets the data in the 05-06 and 15-16 sessions to only include legislators who

Figure G.12: Distribution of Constituency Opinion on Individual Roll-Call Votes

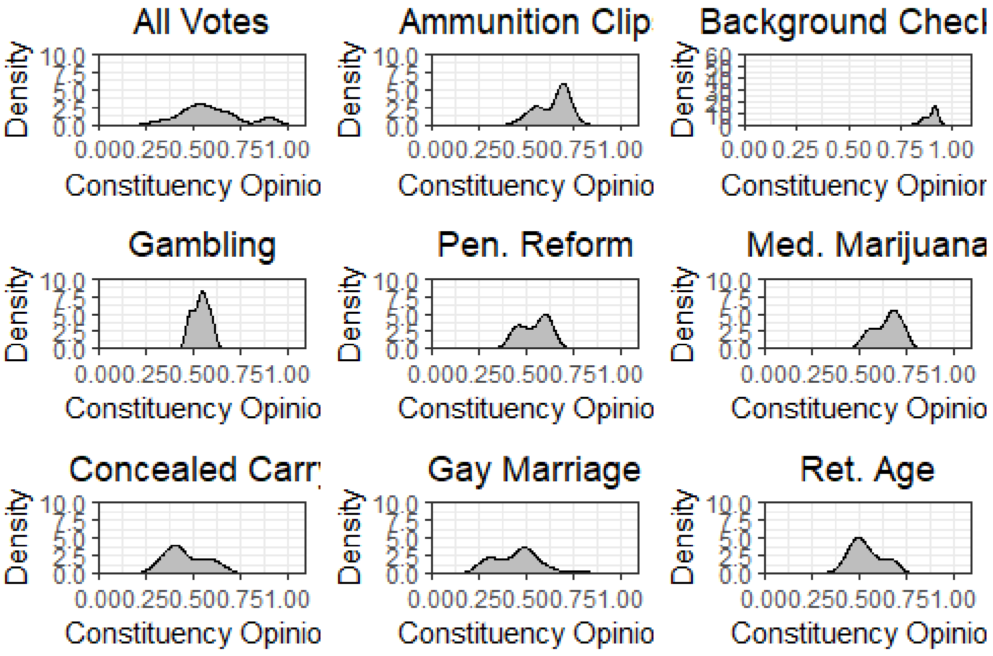


Table G.19: Responsiveness on Individual Roll-Call Votes

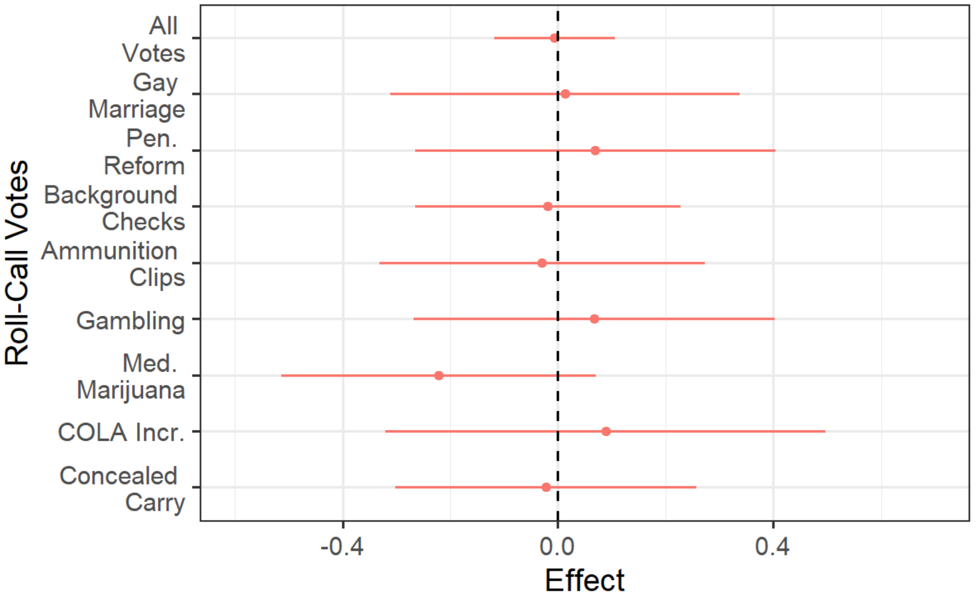
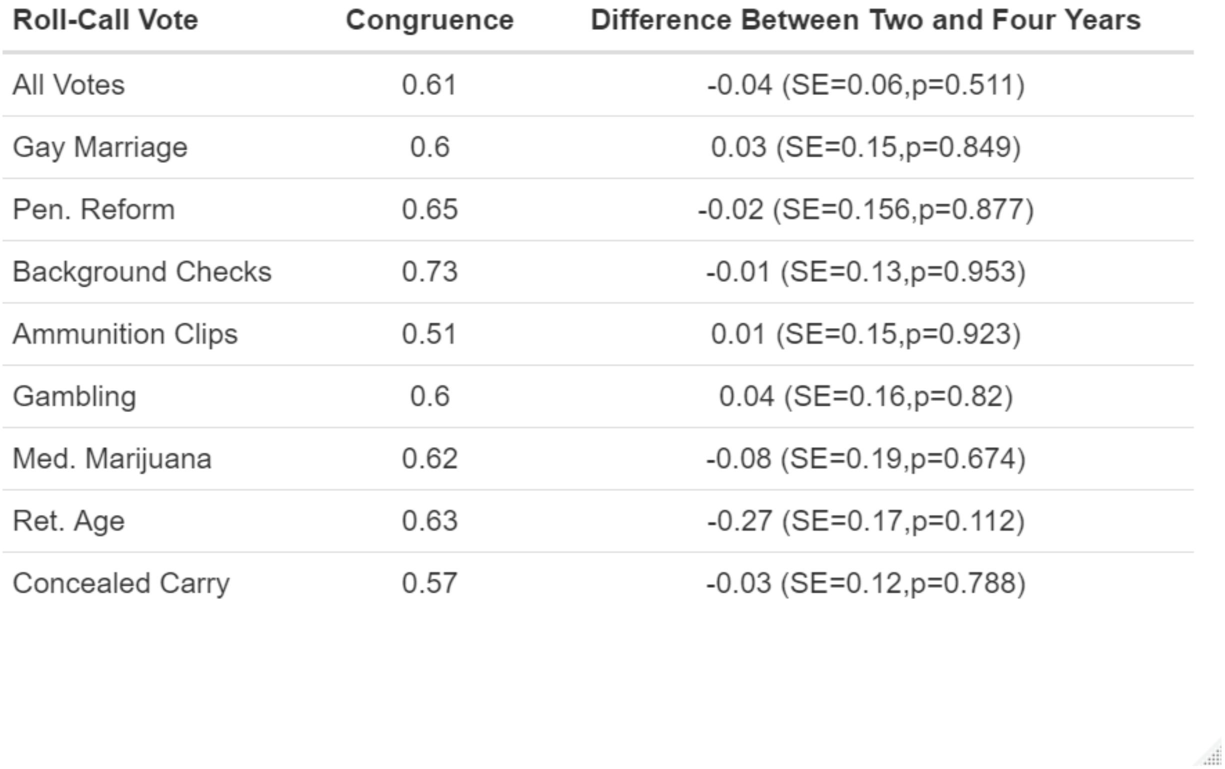


Table G.20: Congruence and Treatment Effects (all significance tests are two-tailed)



Roll-Call Vote	Congruence	Difference Between Two and Four Years
All Votes	0.61	-0.04 (SE=0.06,p=0.511)
Gay Marriage	0.6	0.03 (SE=0.15,p=0.849)
Pen. Reform	0.65	-0.02 (SE=0.156,p=0.877)
Background Checks	0.73	-0.01 (SE=0.13,p=0.953)
Ammunition Clips	0.51	0.01 (SE=0.15,p=0.923)
Gambling	0.6	0.04 (SE=0.16,p=0.82)
Med. Marijuana	0.62	-0.08 (SE=0.19,p=0.674)
Ret. Age	0.63	-0.27 (SE=0.17,p=0.112)
Concealed Carry	0.57	-0.03 (SE=0.12,p=0.788)

Table G.21: Crosstabs of Individual Roll-Call Votes

Legislator Votes

District Median

All Votes

	Oppose	Favor
Favor	0.24	0.45
Oppose	0.14	0.13

Ammo Clips

	Oppose	Favor
Favor	0.49	0.46
Oppose	0.05	0

Background Checks

	Oppose	Favor
Favor	0.27	0.73
Oppose	0	0

Conc. Carry

	Oppose	Favor
Favor	0	0.35
Oppose	0.21	0.43

Gambling

	Oppose	Favor
Favor	0.26	0.47
Oppose	0.1	0.12

Gay Marriage

	Oppose	Favor
Favor	0.03	0.24
Oppose	0.33	0.35

Med. Marijuana

	Oppose	Favor
Favor	0.38	0.62
Oppose	0	0

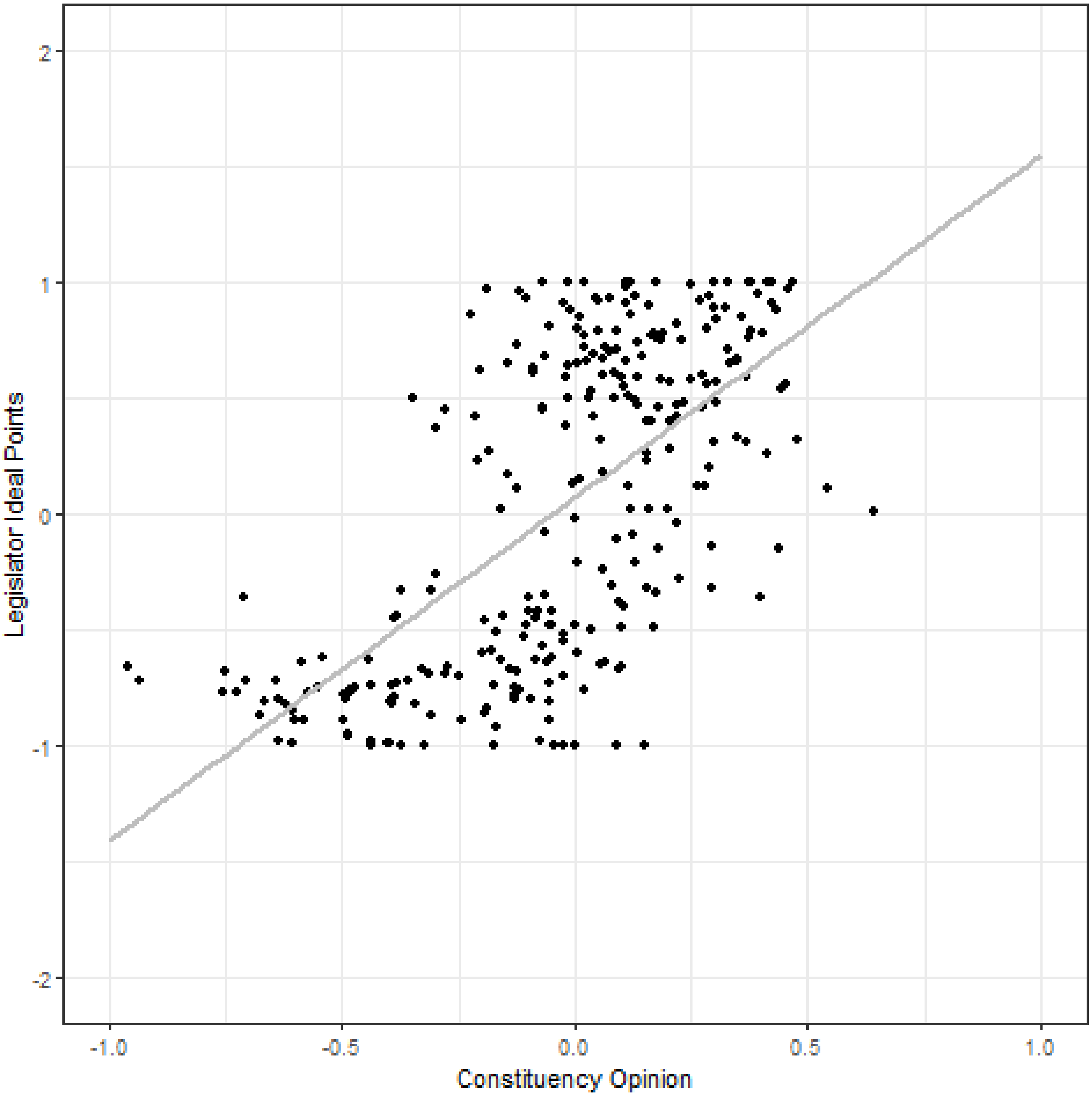
Pen. Reform

	Oppose	Favor
Favor	0.23	0.42
Oppose	0.19	0.11

Ret. Age

	Oppose	Favor
Favor	0.25	0.31
Oppose	0.28	0.09

Figure H.13: Relationship Between Roll-Call Voting and Constituency Opinion (2005-2006 and 2015-2016 sessions)



survived until this session. Legislator fixed effects are included in this latter test. One should note that these tests are solely meant as robustness checks; in particular, the second test carries post-treatment biases and should be interpreted with caution. In neither test are legislators with two years left until the end of their term more responsive to constituency opinion than legislators with four years left.

Table H.22: Differences-in-Differences Design

	Dependent variable:	
	Legislator Ideal Point	
	(1)	(2)
Constituency Ideal Point	0.263 (0.131)	−0.079 (0.190)
	p = 0.046	p = 0.678
Two Years	−0.001 (0.020)	0.014 (0.016)
	p = 0.947	p = 0.354
Con. Ideal Point * Two Years	0.002 (0.068)	0.034 (0.051)
	p = 0.981	p = 0.514
Observations	558	543
R^2	0.924	0.969
Adjusted R^2	0.891	0.941

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$