Why Do Term Limits Polarize State Legislatures?[*]

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Abstract

How do term limits affect polarization in state legislatures? Pairing novel roll-call based candidate ideology scalings with a difference-in-differences design for 1992-2020, I conduct the first comprehensive study of the ideological effects of term limits in state legislatures. I find that term limits increase polarization across the candidate pipeline, from the pool of primary election candidates to eventual general election winners. This effect is strongest in more-professionalized legislatures, where term limits devalue office the most, and is accompanied by a sharp decline in newspaper coverage of legislative elections, thereby limiting candidates' cost of extremism. As a result, term limits systematically erase the traditional advantage of moderate candidates in general elections and correlate with diminished voter knowledge about, and ability to hold accountable, their state legislators. These findings help explain why term limits polarize state legislatures and illustrate how the devaluation of office and elections contribute to legislative polarization.

"Rather than bringing on the 'Citizen Legislature' promised by some of its advocates, term limits have generated even more partisanship [among state legislators]." [1]

— Peter Schrag, editor, The Sacramento Bee

1 Introduction

Political polarization has reached historic levels across American legislative landscapes, generating widespread concern about diminished legislative productivity, efficiency, and effectiveness (e.g., Binder, 1999; Koger, 2010; Mann and Ornstein, 2012; McCarty, 2007; Sinclair, 2006). Despite extensive theoretical guidance, it is challenging to identify the roots of legislative polarization, particularly as it relates to candidate entry and voter engagement. By altering the value of office and salience of elections, state legislative term limits provide an ideal setting to evaluate how both candidate supply and electoral selection contribute to polarization. In this article, I conduct a general equilibrium analysis of the ideological effects of legislative term limits. As I detail below, my analysis is the first to fully characterize the ideological effects of term limits and their contribution to rising polarization.

While foundational, existing research on term limits and polarization focuses exclusively on incumbent legislators (e.g., Olson and Rogowski, 2020). Since the ideological composition of office-seekers is instrumental in shaping legislative polarization (Hall, 2019), it is essential to consider how term limits affect the ideological composition of primary and general election candidates in addition to their impact on incumbents. To address this gap, I construct a novel roll-call based candidate ideology scaling following Hall and Snyder (2015) and pair it with a difference-in-differences design to trace the effects of term limits across the electoral pipeline. In the process, I assess the basic prediction that, since term limits lower the value of legislative office, they may yield a larger mix of extremist relative to moderate candidates running for office (Hall, 2019).[2]

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[1] Schrag (1998), pp. 13.

[2] More broadly, my research relates to a rich literature that uses term limits to study how electoral incentives affect incumbent behavior (e.g., Alt, Bueno de Mesquita, and Rose, 2011; Besley and Case, 1995;

Analyzing these data, I find that legislative term limits increase polarization across the candidate pipeline–including the pool of primary and general election candidates in addition to eventual race winners–but do not cause individual legislators to become more extreme. Consistent with candidate supply-side theories of legislative polarization (Besley and Coate, 1997; Hall, 2019; Osborne and Slivinski, 1996), I show that this polarizing effect is greatest in more-professionalized legislatures where the value of office falls the greatest with the onset of term limits.

Term limits may also affect polarization through shifts in electoral selection and voter engagement, although existing empirical work has not considered this possibility. Combining comprehensive data on general election returns for 1992-2020 with the design of Ansolabehere, Snyder, and Stewart (2001), I document that the traditional electoral returns to ideological moderation are erased in term-limited states. To evaluate whether this effect is shaped by the legislative news environment and voter knowledge, I build a corpus of state legislative news coverage for nearly 6,700 local and regional newspapers spanning the years 1992-2022 and draw on responses to the Cooperative Election Study (CES).

Drawing on the news coverage corpus, I find that newspaper coverage of state legislative elections declines by 9% to 21% on average following the introduction of term limits. These shocks to the legislative news environment allow more-extreme candidates to run with diminished threat of electoral sanction. Finally, I show that post-term-limit shocks in the legislative news environment translate into diminished voter knowledge about their state legislature. Voters in term-limited states are approximately 10 percentage points less likely to be able to identify the party in control of their home state legislature than their non-term-limited counterparts. These findings suggest that, by reducing the news coverage of legislative elections, term limits attenuate voter knowledge, allowing more-extreme candidates to run with weakened threat of electoral sanction.

Ferraz and Finan, 2011; Fouirnaies and Hall, 2022; List and Sturm, 2006). My analysis also complements work on professionalization and partisan control in state legislatures (Fiorina, 1994, 1996; Meinke and Hasecke, 2003).

This article builds on a rich literature on the behavioral and institutional effects of legislative term limits. Outcomes of interests include legislative productivity (Fouirnaies and Hall, 2022), fiscal policy (Erler, 2007; Johnson and Crain, 2004; Lewis, 2012), women and minority groups' representation (Carroll and Jenkins, 2001; Casellas, 2010; Moncrief, Powell, and Storey, 2007; Robert, 1996), bills' policy complexity (Kousser, 2006), voter turnout (Nalder, 2007), electoral competition (Daniel and Lott, 1997; Masket and Lewis, 2007), inter-branch power distribution (Apollonio and La Raja, 2006; Cain and Kousser, 2004; Carey et al., 2006; Kousser, 2006; Miller, Nicholson-Crotty, and Nicholson-Crotty, 2011), and the incumbency advantage (Fowler, 2014; Rogers, 2014).

A few important studies in the term limits literature also explore the effects of term limits on individual legislator and aggregate legislative ideology. Fouirnaies and Hall (2022) examine how term limits' removal of electoral incentives affect the behavior of individual legislators. They conclude that legislators who can no longer run for reelection do not systematically alter their ideology, but strategically shirk legislative duties such as casting roll-call votes and participating on committees. In contrast, Olson and Rogowski (2020) study how term limits affect polarization within legislative bodies, finding that term limits increase polarization in state legislative roll-call voting records.[3] Olson and Rogowski (2020) and Fouirnaies and Hall's (2022) conclusions are not necessarily mutually exclusive. Even if individuals' ideology remains constant, state legislative polarization may increase if term limits cause the pool of legislative candidates to polarize. Investigating this possibility is the first task of this article. Second, while Olson and Rogowski suggest that term-limit-included polarization is driven by an increased role of parties, we know little about how term limits cause polarization. The second portion of this article tackles this task.

Finally, my research follows in a burgeoning literature on ideological extremity and polarization. Recent work documents sustained growth in political polarization in Congress

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[3]Other related studies report null results in the context of the Arkansas State Senate (Titiunik and Feher, 2018) and California legislature (Cain and Kousser, 2004). Wright (2007) also finds no effect of term limits on roll-call voting for the 1999-2000 legislative session.

(Layman, Carsey, and Horowitz, 2006; McCarty, Poole, and Rosenthal, 2006; Poole and Rosenthal, 1984), state legislatures (Handan-Nader, Myers, and Hall, 2022; Shor and McCarty, 2011, 2022), and federal courts (Bonica and Sen, 2021). Explanations for this phenomena are diverse, including primary election format (Brady, Han, and Pope, 2007; Kaufmann, Gimpel, and Hoffman, 2003), campaign finance rules (Barber, 2016), legislator pay (Hall, 2019), and the costs of running for office (Hall, 2019). Related work seeks to understand whether legislative elections favor moderate or extreme candidates (Ansolabehere, Snyder, and Stewart, 2001; Canes-Wrone, Brady, and Cogan, 2002; Canes-Wrone and Kistner, 2022; Hall, 2019; Rogers, 2017; Utych, 2020). Due to endogeneity, it is challenging to estimate the causal effects of policy on polarization and electoral selection. Hence, plausibly exogenously implemented state legislative term limits provide an ideal setting to study the drivers of polarization and direction of electoral selection.

The remainder of this paper is organized as follows. Section two outlines my solution to the methodological challenges of studying candidate pool ideology and introduces new datasets on state legislative candidate ideology and news coverage. In Section three, I focus on candidate supply, documenting the polarizing effects of term limits across all stages of legislative election. Section four investigates how term limits affect legislative polarization and demonstrates that this effect is driven in part by declining voter knowledge about legislative candidates. Finally, Section five discusses implications of the findings and concludes.

2 Empirical Strategy

To implement my study, I build three key datasets on state legislative elections. Cumulatively, these data cover 49 states for the years 1992-2020, ensuring comprehensive coverage of candidates at all stages of the election pipeline. In accord with existing work, I exclude non-partisan Nebraska from the analysis and focus on Democratic and Republican candidates. Table 1 summarizes the relevant characteristics of the term-limited states that enter

Table 1 – Summary of Term-Limited States Included in Analysis.

State	Year Enacted	Year Binding	Type	Term Limit Lower Chamber	Term Limit Upper Chamber
AR	1992	1998	$\begin{cases}\text{Lifetime} & t < 2020 \\ \text{Consecutive} & t \geq 2020\end{cases}$	$\begin{cases}6 & t < 2014 \\ 16 & t \in [2014, 2020) \\ 12 & t \geq 2020\end{cases}$	$\begin{cases}8 & t < 2014 \\ 16 & t \in [2014, 2020) \\ 12 & t \geq 2020\end{cases}$
AZ	1992	2000	Consecutive	8	8
CA	1990	1996	Lifetime	$\begin{cases}6 & t < 2012 \\ 12 & t \geq 2012\end{cases}$	$\begin{cases}8 & t < 2012 \\ 12 & t \geq 2012\end{cases}$
CO	1990	1998	Consecutive	8	8
FL	1992	2000	Consecutive	8	8
LA	1995	2007	Consecutive	12	12
ME	1993	1996	Consecutive	8	8
MI	1992	1998	Lifetime	6	8
MO	1992	2002	Lifetime	8	8
MT	1992	2000	Consecutive	8	8
NV	1996	2010	Lifetime	12	12
OH	1992	2000	Consecutive	8	8
OK	1990	2004	Lifetime	12	12
SD	1992	2000	Consecutive	8	8

Note: *Year Enacted* refers to year term limit legislation became law. *Year Binding* refers to first year in which incumbents are no longer eligible to run for re-election. Source: the National Conference of State Legislatures.

my analysis.

2.1   Predicting Legislative Roll-Call Using Hall-Snyder Scores

Due to data limitations, existing empirical work on term limits focuses on incumbent legislators' ideology. As Hall (2019) illustrates, designs focused on incumbents miss a key source of polarization from the candidate pipeline. Hence, an ideal measure of ideology for the study of term limits captures how candidates would cast roll-call votes if elected to office. However, existing candidate ideology scalings are not optimized to measure state legislative roll-call behavior.[4] Bonica's (2014) CFscores, which use unsupervised machine learning to predict legislator ideal points from campaign contributions, have low within-party correlations with roll-call based ideology measures (Hill and Huber, 2017; Tausanovitch and Warshaw, 2017). For contexts where high within-party correlation is important, Bonica (2018) develops super-

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[4]Shor and McCarty's (2011) NP-Scores, which are derived directly from legislative roll-call data, are only available for the subset of state legislative candidates who become sitting legislators.

vised scalings that explicitly predict legislative roll-call behavior. However, these DW-DIME scores do not cover most state legislative candidates and are trained using congressional DW-NOMINATE–rather than state legislative NP–Scores. Hence, to accurately measure legislative polarization, the purpose of this section is to build an ideology scaling that explicitly predicts state legislative roll-call.

To measure the roll-call ideology of state legislative candidates, I follow the method used by Hall and Snyder (2015) in the context of congressional elections and impute candidates' ideology as the contribution-weighted ideology of their donors. This method requires two inputs: a target measure of ideology and data on campaign donations. I use Shor and McCarty's (2011) NP-Scores as the target measure of ideology which are available for 24,716 incumbent state legislators between 1993 and 2018. NP-Scores are derived from a two-step process in which incumbents' within-state ideology is measured using their roll-call record. These scalings are then mapped to a common national issue space using legislator responses to the Project Vote Smart National Political Awareness Test (NPAT). Second, I draw on campaign contributions to state legislative candidates from the National Institute on Money in Politics. These data consist of over 43 million transactions between 1989 and 2020.

Estimation of candidate ideology proceeds in two stages. First, I estimate the ideology of all state legislative donors as the average contribution-weighted ideology of the incumbents to which a donor contributes.[5] More formally, let $Contribution_{ij}$ be the donation amount from donor j to candidate i and $NPScore_i$ be incumbent i's NP-Score. Then donor j's revealed ideological preference is given by

$$Donor\ Ideology_{-i,j} = \frac{\sum_{w \neq i} NPScore_w\ Contribution_{wj}}{\sum_{w \neq i} Contribution_{wj}},$$

where I leave out candidate i when estimating donor j's ideology to avoid a feedback loop.[6]

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[5]I include individual and interest group donors in this process.

[6]This method yields a separate donor scaling for every candidate-donor pair. All subsequent results replicate when including candidate i in donor j's ideology (i.e., $Donor\ Ideology_j = \frac{\sum_i NPScore_i\ Contribution_{ij}}{\sum_i Contribution_{ij}}$).

Figure 1 – **Correlation Between NP-Scores and Hall-Snyder Scores and CFscores.**
Hall-Snyder Scores correlate well with NP-Scores, even within party, and perform better than
CFscores.

(a) Hall-Snyder Scores

(b) CFscore
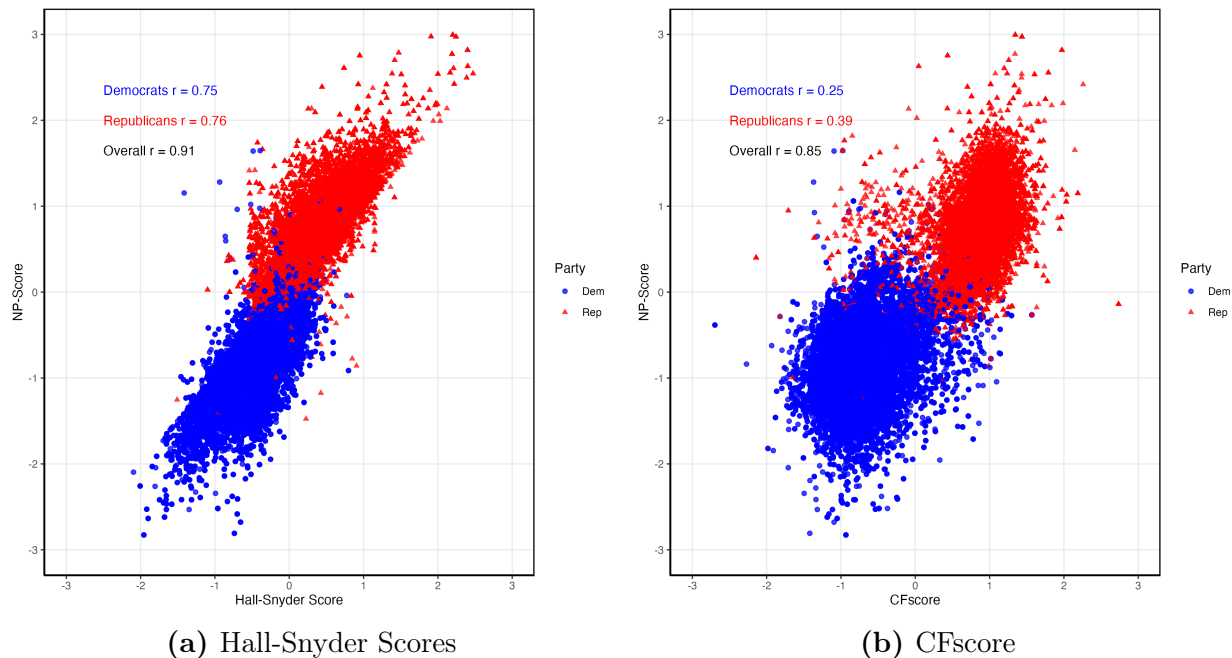
Subsequently, I estimate each candidate's ideology as

$$Cand\ Ideology_i = \frac{\sum_j Donor\ Ideology_{-i,j}\ Contribution_{ij}}{\sum_j Contribution_{ij}}.$$

In keeping with Hall and Thompson (2018) and Hall (2015, 2019), I use primary and general election donations to all candidates to scale donors and then use only primary donations to scale candidates.[7] Given the comparatively small number of donors per state-legislative race, I include all donors and candidates in the scaling process, regardless of donor pool size.

As a validation exercise, Figure 1(a) presents the correlation between state legislative Hall-Snyder Scores and NP-Scores for every available incumbent-year. The correlations are high within party ($r = 0.75$ for Democrats and $r = 0.76$ for Republicans) and approximately double the within-party correlations between CFscores and NP-Scores (c.f., Figure 1(b)).

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[7]To allay concerns about strategic donations, in Appendix Table B.3 I replicate my main analyses using scalings that exclude donations to candidates after they become incumbents. My substantive conclusions remain unchanged.

Both within-party and overall correlations are higher than congressional Hall-Snyder Scores (Hall, 2015; Hall and Snyder, 2015).

2.2   Assembling Election Returns and Legislative News Coverage

Next, in order to evaluate how term limits affect electoral selection, I obtain general election returns from the State Legislative Election Returns dataset (SLERs) (Klarner, 2021). These data contain the universe of state legislative general elections held during the period of my study.[8] To this dataset I add indicators for candidate-level and chamber-level term limits using data from the National Conference of State Legislatures (NCSL).

Second, news coverage of elections may shape voter knowledge (Snyder and Strömberg, 2010) and inform electoral returns to moderation (Canes-Wrone and Kistner, 2022). To evaluate this possibility, I build a corpus of newspaper coverage of state legislative elections. I collect these data from Newspapers.com, an online database containing 596 million newspaper pages for nearly 6,700 local and regional U.S. newspapers for the years 1992-2020. Drawing on these data, I construct measures of news coverage of state legislatures as a whole and of individual incumbent legislators. The former captures state-level newspaper references to legislative bodies (e.g., "Wisconsin State Legislature") while the latter captures newspaper-level references to individual legislators. I outline this process in detail in Appendix A.4. Overall, these data contain 17.6 million references to state legislatures and 13.7 million references to incumbent legislators.

Finally, to explore the consequences of shocks to the legislative news environment, I build a dataset of voter knowledge about state legislative politics using nearly 500,000 responses to the CES from 2010-2020. These responses test individuals' knowledge of partisan control in their home legislature and Congress.

After merging my Hall-Snyder scalings with the general election returns and news coverage data, my combined dataset features 68,317 distinct general election candidates and

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[8]I exclude special and runoff elections. Inclusion of this small subset of elections does not affect my substantive conclusions.

107,314 primary election observations across 87,800 races for a total of 194,369 candidate-year observations. See Appendix Table A.2 for a state-by-year matrix of my data's coverage.

3 Term Limits Increase Polarization Across the Electoral Pipeline

In this section, I use my data on state legislative ideology to estimate the effect of legislative term limits on candidate-pool and incumbent polarization. The analysis proceeds in four stages. First, I consider the effect of term limits on state-level polarization among sitting legislators. While Olson and Rogowski (2020) first studied this estimand, I revisit their analysis with the benefit of expanded legislative ideology data. Second, I explore how polarization among primary and general election candidates is shaped by term limits. Third, I segment the ideological effects of term limits by party and chamber. In the final section, I test whether term-limit-induced polarization is driven by the devaluation of legislative office.

3.1 State-Level Difference-in-Differences Design

Over the past three decades, twenty-one states have enacted legislative term limits.[9] Often deeply unpopular among contemporary legislators, these movements have relied almost exclusively on the ballot initiative process to impose term limits. In fact, of the states that allow ballot initiatives, only four–Alaska, Illinois, Mississippi, and North Dakota–have not imposed term limits at some time. Hence, for the purposes of this article, term limits represent an independent shock to the electoral system, candidates' expected tenure, and the importance of elections.

Leveraging the quasi-exogenous imposition of term limits, in this section I employ a

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[9]Legislative term limits imposed by voters in six states were nullified by court or legislative action. Fifteen states currently have legislative term limits in effect.

state-level difference-in-differences design for the years 1992-2020.[10] Specifically, I model

$$Y_{st} = \beta_0 + \beta_1 \, Term \ Limited_{st} + \Omega X_{st} + \alpha_s + \delta_t + \epsilon_{st} \qquad (1)$$

where Y_{st} is the level of partisan polarization in state s in year t, $Term \ Limited_{st}$ indicates whether state s in time t had term limits in effect, X_{st} is a vector of controls, and α_s and δ_t are state and year fixed effects, respectively. The error term, ϵ_{st}, is clustered at the state level. This specification allows me to compare levels of polarization within states over time.

For a given set of candidates or legislators, I define partisan polarization, Y_{st}, as the difference between the median Republican and Democratic candidates' ideology scalings in state s in year t. The term limits variable, $Term \ Limited_{st}$, indicates state-years for which term limits are in effect.[11] When years of impact differ between a state's upper and lower chamber, I code treatment as beginning on the first year of impact.[12]

My difference-in-differences design requires a parallel trends assumption. This assumption dictates that polarization trends in control states (i.e., states that never enacted legislative term limits) approximate polarization trends in treatment states (i.e., states that eventually implemented term limits) absent treatment. In conjunction with each analysis, I test for violations of this assumption and find no evidence of pre-existing trends ("pretrends") in polarization between treatment and control states. Combined with knowledge of the quasi-random implementation of state legislative term limits, the following results may meaningfully be interpreted as the ideological effect of term limits.

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[10]This modeling strategy and control specifications was first implemented by Olson and Rogowski (2020).

[11]Since term limits alter legislative incentives before becoming binding, an alternate definition might operationalize $Term \ Limited_{st}$ according to term limits' dates of enactment. Unfortunately, campaign finance data limitations preclude this possibility. Existing research by Keele, Malhotra, and McCubbins (2013), however, find similar results using enactment and implementation dates. Further, as Olson and Rogowski (2020) note, defining $Term \ Limited_{st}$ based on implementation date will likely serve to attenuate coefficient estimates, making this analysis particularly rigorous.

[12]In Appendix Table D.3, I estimate Equation 1 separately for each chamber while accounting for differential treatment years. The results are nearly identical.

Table 2 – Differential Polarization of Term-Limited Legislatures. dsTerm Limits increase polarization among incumbent state legislators. These results hold for NP-Score and Hall-Snyder Score measures of polarization.

	Polarization (NP-Scores)		Polarization (Hall-Snyder Scores)	
	(1)	(2)	(3)	(4)
Term Limits	0.19	0.20	0.15	0.13
	(0.07)	(0.07)	(0.07)	(0.07)
Log(Leg Prof)		0.08		0.03
		(0.07)		(0.09)
Divided Government		-0.01		0.04
		(0.02)		(0.02)
Party Competetiveness		-0.33		-0.51
		(0.22)		(0.21)
N	679	679	590	590
Standard Deviation	.49	.49	.36	.36
Year FEs	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Years	1993-2018	1993-2018	1992-2020	1992-2020

Note: The outcome is the difference in party median NP-Scores and Hall-Snyder Scores. Standard errors are clustered by state in parentheses.

3.2 Effect of Term Limits on Incumbent Legislators

I begin by focusing on incumbent legislators. Table 2 presents my overall estimates of the effects of term limits on polarization among sitting legislators. For all specifications I present a univariate model and, to guard against the possibility of attributing non-static state features to the effect of term limits, a model with state governance controls. The battery of controls was first introduced in Olson and Rogowski (2020). The variable *Legislative Professionalism* (Squire, 2017) combines information on legislator salary, session length, and staffing resources to quantify legislator engagement in policy making. *Divided Government* indicates whether one party simultaneously controls the governorship, lower and upper legislative chambers. Finally, *Party Competitiveness* measures the absolute two-party difference in control of legislative seats. In Section 3.3.2 and Appendix B.3, I show my results are robust to a variety of alternative specifications.

Using Shor and McCarty (2011) NP-Scores to measure polarization, Olson and Rogowski (2020) show that term limits increase incumbent partisan polarization. With the benefit of additional data, I provide an updated estimate of Equation 1 in the first and second columns of Table 2 using NP-Scores. The results of this analysis closely match those of Olson and Rogowski (2020).

Since NP-Scores are not available for non-incumbents, for the remainder of this paper I measure polarization using Hall-Snyder Scores. To validate these scalings, I re-estimate Equation 1 in columns three and four using Hall-Snyder Scores. I find similar, if slightly attenuated, results in columns three and four, underscoring the potentially conservative nature of my new measure of polarization and increasing the credibility of my extensions.

Since Hall-Snyder Scores are, by construction, static over the course of a legislator's career, it is possible that the effects observed in Table 2 obscure within-legislator changes in ideology in response to term limits. To evaluate this possibility, I build dynamic Hall-Snyder scores by grouping donations by candidate-year. Using these dynamic scalings, Appendix Table B.5 tests whether incumbents alter their ideological positions in the absence of electoral incentives by regressing candidate extremity on *Term Limited*–an indicator for whether an incumbent is serving in her final term before being termed out of office. This analysis makes within-incumbent ideology comparisons between legislators who are subject to binding term limits and those who are not. Across all specifications, I estimate a precise null effect, indicating that incumbents do not systematically alter their ideological platforms in the absence of electoral incentives. These findings match Fouirnaies and Hall (2022) and suggest that state legislators "die with their ideological boots on" (Poole, 2007, p. 435).

In sum, while term limits polarize the group of incumbent legislators, they do not systematically shift individual legislators' ideology. This apparent contradiction underscores the necessity of analyzing the pool of candidates who run for office.

3.3 Effect of Term Limits on Candidate Supply

While the dynamics of incumbents' ideology directly shape legislative polarization, recent work illustrates that it is essential to also consider the indirect effect of the complete candidate pool on legislative polarization. For example, in the context of U.S. House of Representatives elections, Hall (2019) shows that the vast majority of polarization originates from ideological extremity that is built into the pool of candidates who run for office. As the simulations in Appendix Figure A.1 show, the same is true of state legislatures. Regardless of whether voters consistently choose the most extreme candidate or most moderate candidate, American state legislatures will feature extraordinary levels of polarization. This result underscores the importance of studying how term limits affect the ideology of the complete legislative candidate pipeline, not only incumbents.

3.3.1 Main Results

To capture the previously unobserved effect of term limits on candidate pool polarization, I expand my analysis to the full candidate pipeline–including primary and general election winners and losers and incumbents. Table 3 reports the difference-in-differences estimate of the effect of term limits on polarization using Equation 1. Columns one and two estimate the effect of term limits on candidate-pool polarization with and without controls, respectively. The point estimates for *Term Limited* in these columns indicate that state legislative term limits increase legislative polarization by approximately one quarter of one standard deviation of the distribution of polarization. This change is equivalent to a one-time 36% increase in average polarization.

Since term limits became binding multiple elections after they were passed into law, it is important to rule out anticipatory effects. Column three tests for violations of the parallel trends assumption by including two leads of the term limits variable. If term limits become binding in time t, then they should have no effect on polarization in any future times. This is exactly what I find in column three, where the overall effect of term limits remains and

Table 3 – Effect of Term Limits on Candidate Pool Polarization. Term limits increase polarization across the pool of legislative office seekers.

|	Candidate Pool Polarization			
	(1)	(2)	(3)	(4)
Term Limited	0.14	0.13	0.15	0.13
	(0.07)	(0.07)	(0.06)	(0.04)
Term Limited, $t+1$			-0.02	
			(0.05)	
Term Limited, $t+2$			0.03	
			(0.03)	
Log(Leg Prof)		0.06	0.01	0.06
		(0.06)	(0.05)	(0.08)
Divided Government		0.04	0.01	0.02
		(0.02)	(0.02)	(0.02)
Party Competetiveness		-0.46	-0.41	-0.31
		(0.24)	(0.16)	(0.13)
N	590	590	449	590
Outcome Standard Deviation	.4	.4	.4	.4
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State Specific Linear Time Trend	No	No	No	Yes

Note: In all columns the outcome is the difference in party median Hall-Snyder Scores. Standard errors are clustered by state in parentheses..

the lead coefficients are small and insignificant.

Finally, in column four I include a state-specific linear time trend to absorb any un-observed state-level time trends that are associated with the timing of polarization and implementation of term limits. My results remain unchanged following the inclusion of this trend.

The difference-in-differences model provides a static estimate of the effect of term limits on polarization. To obtain a time-varying treatment effect, and test for violations of the parallel trends assumption, I also run an event study. Figure 2 displays coefficients from an event study of candidate pool political polarization with 95% confidence intervals. The p-value for the omnibus Wald test of zero pre-event effects is .923, while the p-value for zero post-event effects is $< .001$. Hence, while the results are necessarily noisy, I find credible evidence of universally heightened polarization among candidate pools in term-limited states

Figure 2 – Effect of Term Limits on Candidate Pool Polarization. This figure plots the average effect of term limits on legislative candidate pool polarization using a dynamic two-way fixed effect estimator. State legislative polarization increases significantly in the years following term limits' implementation.
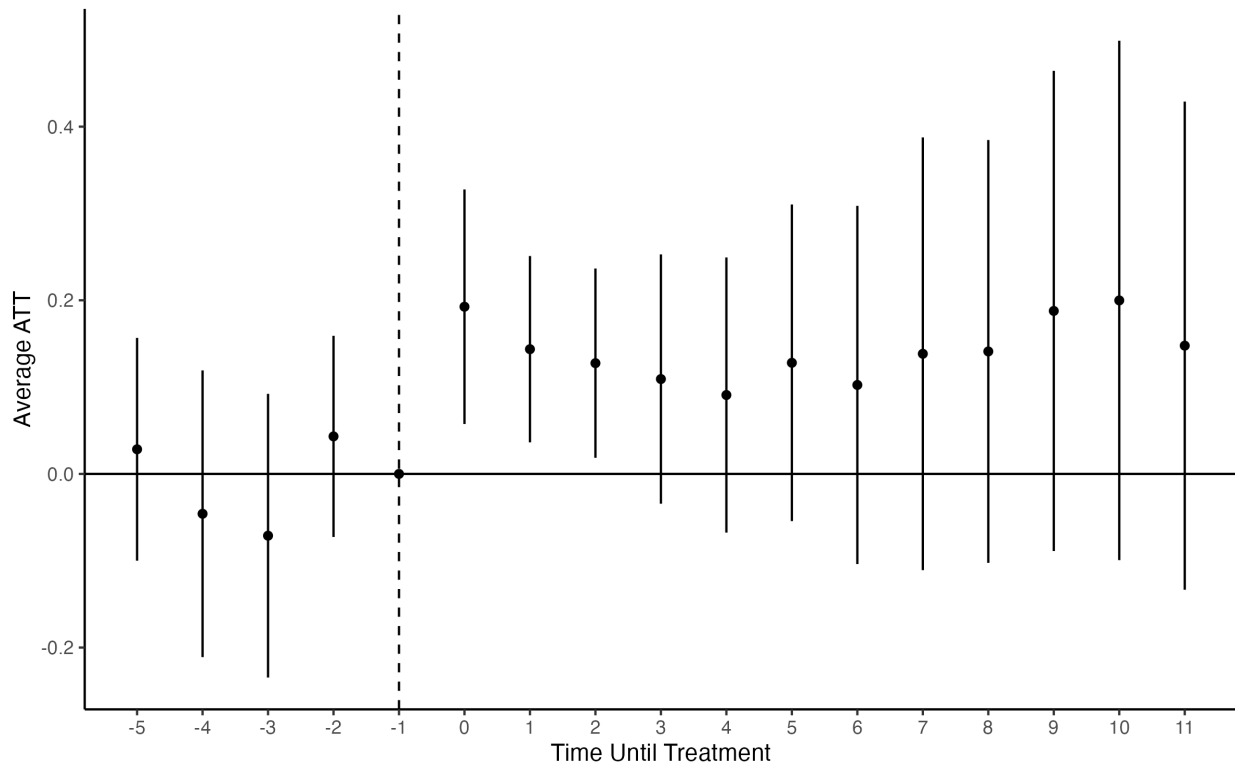
Note: Periods containing two or fewer states are aggregated into a single endpoint. Baseline is t_{-1}. Robust standard errors are clustered by state.

and no evidence of pre-trending.

3.3.2 Robustness of Main Results

To establish the robustness of my main findings, I conduct a variety of analyses using alternative estimation specifications and external measures of polarization. My results are robust across these estimates. I report the results of these analyses in the appendix in the interest of space.

In the standard difference-in-differences framework employed in Equation 1, Goodman-Bacon (2021) shows that β_1 is the weighted average of all possible two-group/two-period difference-in-difference estimates. Hence, the effect of term limits on polarization is identi-

fied by comparing i) treated states with untreated states as controls, ii) early-adopting states with late-adopting states as controls, and iii) late-adopting states with early-adopting states as controls. The third group of differences makes "forbidden comparisons" and, in the presence of staggered adoption and heterogenous treatment effects, does not yield a traditional ATT estimate. In the extreme, state-periods could receive negative weights and produce an ATT that is incorrectly signed (de Chaisemartin and D'Haultfœuille, 2020). While the bias due to heterogenous treatment effects is less concerning in the presence of numerous never treated units (a case likely satisfied by the 35 states that never termed-out legislators) and ignorability (also likely satisfied by the initiative-led process of term limit implementation), I nevertheless reestimate my baseline results using the heterogenous treatment effect robust estimator proposed by Liu, Wang, and Xu (2022). This estimator imputes counterfactual control outcomes for post-treatment periods using pre-treatment data and calculates an equally-weighted treatment effect as the difference between states' observed and imputed post-treatment outcomes. By excluding "forbidden comparisons" and enforcing equal weighting, the imputation estimator yields unbiased treatment effect estimates under heterogenous treatment effects and staggered treatment adoption. The results of this exercise–reported in Appendix Figure B.1–closely mirror those of the dynamic two-way fixed effects specification.

Second, to ensure my results are not a scaling artifact, I reestimate my difference-in-differences design using two alternative ideology scalings. Appendix Table B.2 replicates my results using Bonica (2014) CFscores to measure legislative polarization. CFscores–available for roughly 51% of all state legislative candidates between 1992-2020–use an unsupervised machine learning algorithm to predict candidate ideal points using campaign finance data. Second, to minimize concerns about strategic donations, Appendix Table B.3 employs alternative Hall-Snyder Scores which exclude donations to candidates after they become incumbents. My conclusions hold for all levels of analysis using CFscore and alternative Hall-Snyder Score measures of polarization. The effect size–approximately one quarter of one standard deviation–is extremely similar to that of my main findings. Since my goal is to

measure legislative roll-call polarization, and CFscores are not optimized to predict roll-call (Hill and Huber, 2017; Tausanovitch and Warshaw, 2017), I prefer Hall-Snyder Scores and use CFscores as a general robustness check.

Third, I reestimate my models after including a wider set of time-varying state factors that could be associated with legislative polarization, including population, unemployment rate, per capita income, income inequality, Democratic control of the governorship, and immigration. Appendix Table B.4 shows the inclusion of these controls does not alter my conclusions.

3.3.3 Effect Heterogeneity

The ideological ramifications of legislative term limits may vary according to a variety of institutional and electoral factors. In this section, I evaluate three sources of heterogeneity suggested by the literature–heterogeneity by party, district composition, and legislative chamber. In the interest of space, I report the results of these analyses in the appendix.

A growing body of research explores the prevalence of asymmetric polarization in American elections. In the standard account, scholars argue that ideological polarization is disproportionately driven by rising Republican extremity (e.g Grossmann and Hopkins 2016; McCarty 2007). Recent evidence at the state level, reaches different conclusions. Olson and Rogowski (2020) find no evidence of asymmetric polarization among incumbent legislators in term-limited states. Handan-Nader, Myers, and Hall (2022) also find little evidence of asymmetric polarization among the pool of general election candidates, but illustrate that Democratic primaries favor extremists at a higher rate than Republican primaries.

In Appendix Table D.1, I re-estimate Equation 1, defining Y_{st} separately for Democrats and Republicans. Unsurprisingly, I find that term limits are associated with an increase in extremity among Democratic candidates (columns one and three) and Republican candidates (columns two and four) compared to non-term-limited candidates. These findings hold across all stages of the electoral pipeline–from primary and general elections to incumbents. Across

party, my estimates are extremely similar and provide no evidence of asymmetric polarization at any stage of the electoral pipeline.

Second, Hall (2014) finds that, in their early years, term limits disproportionately removed senior Democratic legislators and replaced them with Republican legislators. If these new Republican legislators were more extreme than their Democratic predecessors–a possibility especially likely in the South where district composition trended to the right–term limits may have simply accelerated legislative polarization that would have occurred anyways. Under this interpretation, term limits would have increased legislative polarization by hastening the replacement of moderate Democrats with more-extreme Republicans.

To test this hypothesis, I classify legislative districts as "Competitive," "Safe for Republicans," or "Safe for Democrats."[13] In Table D.2, I re-estimate Equation 1 after adding interactions between *Term Limited* and indicators for district safety. Across all stages of the electoral pipeline, I find that the polarizing effect of term limits is strongest in competitive legislative districts, mirroring the results of Section 3.3. Further, I find no significant differences in the effect of term limits on polarization between safe Democratic and safe Republican districts. Hence, term limits appear to have a compositional effect beyond that of accelerating inevitable candidate replacement.

Finally, term limits' effect may vary by legislative chamber. In Appendix Table D.3 I estimate Equation 1 separately for state legislative lower and upper legislative chambers. The effect sizes are consistent both between chambers and with the overall aggregate effect.

Overall, the findings presented in this section suggest a more-nuanced picture of the ideological effects of legislative term limits. Instead of causing individual incumbents to become more extreme, term limits increase ideological extremity across the full candidate pipeline. In the next section I investigate the drivers of this global ideological shift.

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[13]A district is classified as "Safe for Democrats" if Democratic candidates captured greater than 60% of all legislative votes cast within a district over the course of a complete districting cycle. "Safe for Republicans" districts are defined analogously. Districts that are not safe for either party are considered "Competitive."

4 How Term Limits Polarize State Legislatures

Taken as a whole, my results illustrate that term limits increase legislative polarization by altering the ideological composition of candidates who run for office. In this section, I explore four interrelated explanations for how term limits alter the ideological distribution of the legislative candidate pool.

First, I draw on theoretical work that outlines how a decline in the expected benefits from holding office will increase candidate divergence (Besley and Coate, 1997; Osborne and Slivinski, 1996). To the extent that they devalue legislative office (Meinke and Hasecke, 2003), term limits may also alter the ideological composition of state legislatures. I evaluate this implication using Squire (2017) data on legislative professionalization.

Second, candidates who run in races that receive sparse news coverage may face diminished electoral consequences for ideological extremity (Canes-Wrone and Kistner, 2023; Hall, 2015). Hence, a shock to the legislative news environment could minimize candidates' cost to ideological extremism, thereby increasing candidate pool polarization. In the second section, I explore whether the legislative news environment changes post-term limits. News coverage may decline if, for example, newspaper editors reallocate coverage to higher-stakes national election coverage or voters pay less attention to legislative news.

Shifts in news coverage can only shape legislative polarization if they alter electoral selection. In the third section, I investigate whether the strength of voters' electoral selection changes following the implementation of term limits. Any systematic shifts in how voters in term-limited states evaluate and elect extreme candidates will mechanically shape polarization within the candidate pool.

Finally, in the fourth section I directly assess how term limits shape voter knowledge of legislative elections.

4.1 Professionalization and the Benefits of Holding Legislative Office

The citizen-candidate model–developed by Besley and Coate (1997) and Osborne and Slivinski (1996)–relates the net value of holding elected office to citizens' decisions to run for office. The key insight of the model is that, as the benefits from holding office decline (or, equivalently, the costs of running for office increase), two-candidate races become increasingly divergent. This is because extremist candidates are more concerned by the ideological positions of their opponents than are their moderate counterparts, yielding a pool of extremist candidates more willing to pay the costs of running for office.

In addition to intangible benefits such as prestige, there are numerous benefits to holding legislative office, including the opportunity to shape policy, direct staff, and ability to draw a government salary. Given a strong incumbency advantage in state legislatures (Hirano and Snyder, 2009), legislative candidates–if elected–can expect to derive extended benefits from holding office. By reducing expected tenure and opportunity to shape policy outcomes, term limits curb legislators' expected benefits from holding office (Carey et al., 2006; Fiorina, 1994, 1996; Meinke and Hasecke, 2003). As Hall (2019) shows in the context of the U.S. House, the result of this devaluation of office–in line with the citizen-candidate model–is a more-polarized candidate pool.

The benefits of holding legislative office are not, of course, the same across states, especially in term-limited states. For example, the base salary for California state legislators in 2021 was $119,702 compared to $13,957 for legislators in South Dakota. Legislators in more-professionalized legislatures thus risk losing larger benefits as a result of term limits than do legislators in less-professionalized states. Hence, if the devaluation of office drives candidate pool polarization, we should observe a stronger effect of term limits on polarization in more-professionalized legislatures.

I evaluate this implication in Table 4 by interacting *Term Limits* with a measure of legislative professionalization from Squire (2017). The interaction term is positive across all

Table 4 – Term Limits and Legislative Professionalization. The polarizing effect of term limits may be greater in more-professionalized state legislatures.

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limits	0.25	0.26	0.42
	(0.23)	(0.24)	(0.21)
Log(Leg Prof)	0.01	0.00	-0.03
	(0.05)	(0.05)	(0.07)
Log(Leg Prof) · Term Limits	0.09	0.11	0.20
	(0.14)	(0.15)	(0.13)
Controls	Yes	Yes	Yes
N	433	433	433
Outcome Standard Deviation	.4	.39	.36
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes

Note: In all columns the outcome is the difference in party median Hall-Snyder Scores. Standard errors are clustered by state in parentheses.

stages of the electoral pipeline, indicating that the effect of term limits is strongest among legislatures that are more-professionalized. These results mirror work by Olson and Rogowski (2020) for incumbent candidates.

4.2   News Coverage of Legislative Elections

News coverage of state legislative elections may also shape the ideological composition of office-seekers. If legislative election news coverage declines following the implementation of term limits, candidates' cost of extremism may also fall since voters would be less able to differentiate between more-extreme and less-extreme candidates (Canes-Wrone and Kistner, 2023).

Term limits may alter news coverage by, for example, reducing the power of the legislative branch over policy outcomes (e.g., Carey et al., 2006) and the importance of legislative election outcomes. As political activity is increasingly controlled by actors other than state legislators, there is less need for journalists to cover legislative activity. To evaluate this

Table 5 – Effect of Term Limits on State Legislative News Coverage. Local and regional newspapers write fewer articles about state legislatures and incumbent state legislators following the implementation of term limits.

	News Coverage Ratio		Incumbent Newspaper References	
	(1)	(2)	(3)	(4)
Term Limited	-0.015	-0.015	-101.778	-84.481
	(0.008)	(0.008)	(50.629)	(39.081)
N	651	651	7,438	7,438
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	No	No
Newspaper FEs	No	No	Yes	Yes

Note: In columns one and two the unit of anaysis is the state-year and the outcome is the ratio of occurrences of each state's combined legislative body name (i.e. "state legislature," "general assembly," or "general court") to occurrences of "congress" in local and regional newspapers. In columns three and four the unit of analysis is the newspaper-year and the outcome is the total number of references to the incumbent legislator in local and regional newspapers. Robust standard errors clustered by state in parentheses. Controls are total population, share of seats up for election, and number of news-related interest groups.

possibility, I build a corpus of local and regional newspaper coverage of state legislatures and legislators.[14] I collect this data from Newspapers.com, drawing on 596 million pages of newsprint for 6,627 local and regional U.S. newspapers. Appendix A.4 documents this process in detail.

My analysis of legislative news coverage proceeds in two stages. I first examine aggregate coverage of state legislatures before pivoting and studying coverage of individual incumbent legislators. The former captures state-level newspaper references to state legislative bodies while the latter captures newspaper-level references to individual legislators.

To evaluate the effect of term limits on aggregate legislative news coverage, I count newspaper references to states' combined legislative bodies (e.g., "Wisconsin State Assembly"). In the same newspapers, I also collect all references to "Congress." Overall, my data include some 754,000 newspaper references to state legislatures and 6.6 million newspaper references

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[14]Since state legislative elections are typically highly localized, it is essential to analyze local and regional–rather than national–coverage.

to Congress. To account for varying newspaper availability and article length, I construct a news coverage ratio as ratio of references to state legislatures divided by references to Congress in each state-year.

A decrease in the news coverage ratio indicates that congressional news coverage is becoming more prominent vis-á-vis state legislative news coverage. On average, the news coverage ratio is .15, indicating that for every one reference to a state legislature there are approximately seven references to Congress. In columns one and two of Table 5, I use a difference-in-differences design to test whether term limits affect aggregate legislative news coverage. Column one reports the simple difference-in-differences estimate while column two reports the estimate after controlling for the share of state legislative seats up for election, total population, and number of news-related interest groups active in the state. Across both models, I find that term limits decrease state legislative news coverage relative to congressional news coverage. This estimate translates into an 10% reduction in aggregate news coverage of state legislatures.

Second, I analyze the effect of term limits on individual newspapers' coverage of incumbent legislators. To do so, I build a newspaper-year-level dataset of articles written about individual incumbent state legislators. Overall, these data contain 4 million newspaper articles written about incumbents. Using these data, columns three and four of Table 5 report the difference-in-differences estimate of the effect of term limits on individual newspapers' coverage of incumbents. Within newspaper, I estimate that newspapers write between 84 and 101 fewer articles about incumbent state legislators following the implementation of term limits. This is relative to a pre-term-limits median of approximately 574 articles, or a 15 to 17% decline in coverage.

4.3   Electoral Selection

Overall, I find that news coverage of state legislatures declines following the implementation of term limits. In this section, I evaluate whether these changes in the news environment

affect voters' well-documented preference for moderates in general elections (Handan-Nader, Myers, and Hall, 2022; Rogers, 2017).

4.3.1 Midpoint Design

To assess the role term-limited states' electorates' play in producing increased partisan polarization, I compare the ideology of competing Democratic and Republican general election candidates and predict their electoral returns to changes in ideological platform. To do so, I adopt the midpoint method of Ansolabehere, Snyder, and Stewart (2001). Consequently, I estimate an equation of the form

$$Y_{dct} = \beta_0 + \beta_1 Midpoint_{dct} + \beta_2 Distance_{dct} + \Omega X_{dct} + \alpha_d + \delta_t + \epsilon_{dct} \tag{2}$$

where Y_{dct} is either the Democratic candidate's general election vote share or a victory indicator in district d in chamber c in year t.[15] $Midpoint$ and $Distance$ are the midpoint and distance between Democratic and Republican candidates, respectively. Finally, X_{dct} is an optional vector of controls, α_d and δ_t are district and year fixed effects, respectively, and the error term, ϵ_{dct}, is clustered by district d.[16]

The coefficient of interest is β_1, the estimated electoral return for the Democratic candidate arising from a rightward (i.e., positive) shift in $Midpoint$. Previous research on Congress (Ansolabehere, Snyder, and Stewart, 2001; Canes-Wrone, Brady, and Cogan, 2002; Hall, 2015, 2019; Hall and Thompson, 2018) and state legislatures (Handan-Nader, Myers, and Hall, 2022; Rogers, 2017) suggest that β_1 is positive, indicating that, on average, general election voters reward moderate candidates at the ballot box. After replicating existing findings, I test whether β_1 differs between term-limited and non-term-limited states.

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[15]Since this design requires competition between one Democratic and one Republican candidate, I restrict my sample to elections in contested single-member districts when using the midpoint model.

[16]The midpoint model requires the ideology of districts' mean voter to be held constant. Ansolabehere, Snyder, and Stewart (2001) use presidential vote share for this purpose. Because presidential vote share is not consistently available at the level of state legislative districts, I employ district-regime fixed effects to hold the median voter constant.

Table 6 – Term Limits Erase Traditional Electoral Returns to Moderation in Contested General Elections. These models report the expected general election electoral returns and change in win probability resulting from a liberal (i.e., positive) shift in candidate ideology. Returns to moderation are erased in term-limited states.

	Dem Vote Share				Dem Win			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Midpoint	0.03	0.10	0.05	0.05	0.22	0.42	0.29	0.29
	(0.01)	(0.02)	(0.01)	(0.01)	(0.05)	(0.07)	(0.07)	(0.07)
Term Limits		0.05	0.03	0.03		0.22	0.13	0.11
		(0.01)	(0.01)	(0.01)		(0.06)	(0.05)	(0.05)
Midpoint · Term Limits		-0.08	-0.05	-0.06		-0.36	-0.17	-0.20
		(0.03)	(0.02)	(0.02)		(0.12)	(0.10)	(0.10)
Distance · Term Limits				0.02				0.10
				(0.01)				(0.06)
Distance	-0.03	-0.07	-0.03	-0.04	-0.22	-0.35	-0.22	-0.26
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(0.03)	(0.05)
Dem Contributions	0.03		0.03	0.03	0.09		0.09	0.09
	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)
Rep Contributions	-0.03		-0.03	-0.03	-0.08		-0.08	-0.08
	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)
N	22,929	26,919	22,929	22,929	22,929	26,919	22,929	22,929
Standard Deviation	.14	.14	.14	.14	.5	.5	.5	.5
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The outcome is either Democratic vote share or a Democratic win indicator. Robust standard errors are clustered by district in parentheses. Midpoint and Distance variables are scaled to run from 0 to 1. The sample is limited to contested general elections in single member districts.

4.3.2 Electoral Selection Results

Table 6 reports my estimates of midpoint model (i.e., Equation 2). The baseline models reported in columns one and five provide compelling evidence that general election candidates are punished by voters for ideological extremity. That is, more-extreme candidates receive, on average, lower vote shares and are less likely to win in state legislative elections. As noted above, these estimates mirror existing work on accountability in congressional and state legislative elections.

I am interested in the difference in *Midpoint* coefficients between term-limited and non-term-limited state-years. To estimate this difference, the remaining columns in Table 6

interact *Midpoint* with *Term Limits*, an indicator for the presence of legislative term limits defined in Section 3. If voters in term-limited states reward ideological extremity at a higher rate than their peers in non-term-limited states, the interaction term would be negative. Conversely, if, relative to non-term-limited states, voters in term-limited states punish candidates for ideological extremism more, the interaction term would be positive.

The interaction terms across all models in Table 6 are negative, indicating that extreme ideological positions are penalized at a lower rate in term-limited states. Columns two and six indicate that term limits nearly erase the advantage to moderation in general elections. In columns three and seven, I add in controls for Democratic and Republican campaign contributions. Finally, to allow for a more-flexible effect of candidate distance, I interact *Term Limits* with *Distance* in columns four and eight. As is apparent, these effects are both robust to different specifications and decidedly large. In fact, the majority of models in Table 6 predict that term limits erase the electoral return to moderation in general elections.

Hainmueller, Mummolo, and Xu (2019) show that multiplicative interaction models–such as the midpoint model–often tend to erroneously assume linearity in effect and common support of the moderating variable. In response, I report the diagnostic measures proposed by Hainmueller, Mummolo, and Xu (2019) in Appendix Figure C.1.[17] As Figure C.1 illustrates, this interaction effect is linear and there is common support of the predictors across term-limited and non-term-limited states. Hence, the assumptions of the multiplicative interaction model appear to hold. As a second robustness check, in Appendix Table C.1, I reestimate the midpoint model using CFscores scores. My substantive conclusions are unchanged using this alternative scaling.

The midpoint model employed above assumes that voters evaluate incumbent and challengers' ideological positions uniformly. Recent empirical (Woon and Pope, 2008; Peskowitz, 2019; Canes-Wrone and Kistner, 2022) and theoretical (Snyder and Ting, 2003) work suggests that voters' evaluation of a candidate's ideology may depend on the candidate's incumbency

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[17]Figures were created using the R package *interflex*.

status. Canes-Wrone and Kistner (2022), for example, illustrate that congressional account-ability has only reliably declined among challengers since 1980. Following Canes-Wrone and Kistner (2022), in Appendix Table C.2 I relax the uniform evaluation assumption of Equation 2 by allowing incumbent and challenger ideology to have independent effects on electoral outcomes. Across all specifications, the interactions between candidate ideology and term limits are negative and significant, indicating that candidates receive smaller electoral rewards for moderating their ideological positions. This effect is similar for incumbents and challengers.

In sum, Table 6 establishes a key new finding: term limits erase the traditional advantage of more-moderate candidates in general elections. The analysis of legislative news coverage in Section 4.2 suggests that this effect is driven by reduced voter knowledge about candidates. That is, if voters are unable to identify moderate vs. extremist candidates, we should expect electoral returns to moderation to disappear in term-limited states. I explore this voter knowledge pathway in the next section.

4.4   Voter Knowledge in Term-Limited States

Finally, having documented that term limits reduce legislative news coverage and erase the traditional electoral return to moderation, I evaluate whether these changes affect voter knowledge about legislative candidates.

To identify the informational effects of term limits on voters, I build a dataset of voter knowledge of legislative elections using data from the Cooperative Election Survey (CES), a national stratified survey administered to more than 50,000 people per year. Every year since 2010, the CES has asked its respondents "Which party has a majority of seats in ..." the U.S. House and Senate and the respondents' state upper and lower legislative chambers. Respondents must choose between "Republicans", "Democrats", "Neither", or "Not sure." For every state-year from 2010-2020, I impute the correct response and compute the share of respondents who answer correctly. Although limited in yearly coverage and question scope,

Figure 3 – Percent Correct Responses to CES Voter Knowledge Questions. This figure depicts average response accuracy rates to four CES questions about party control of Congress and respondents' home state legislatures. Respondents in term-limited states (whose knowledge about federal elections matches their non-term-limited counterparts) exhibit reduced political knowledge about their home state legislatures.
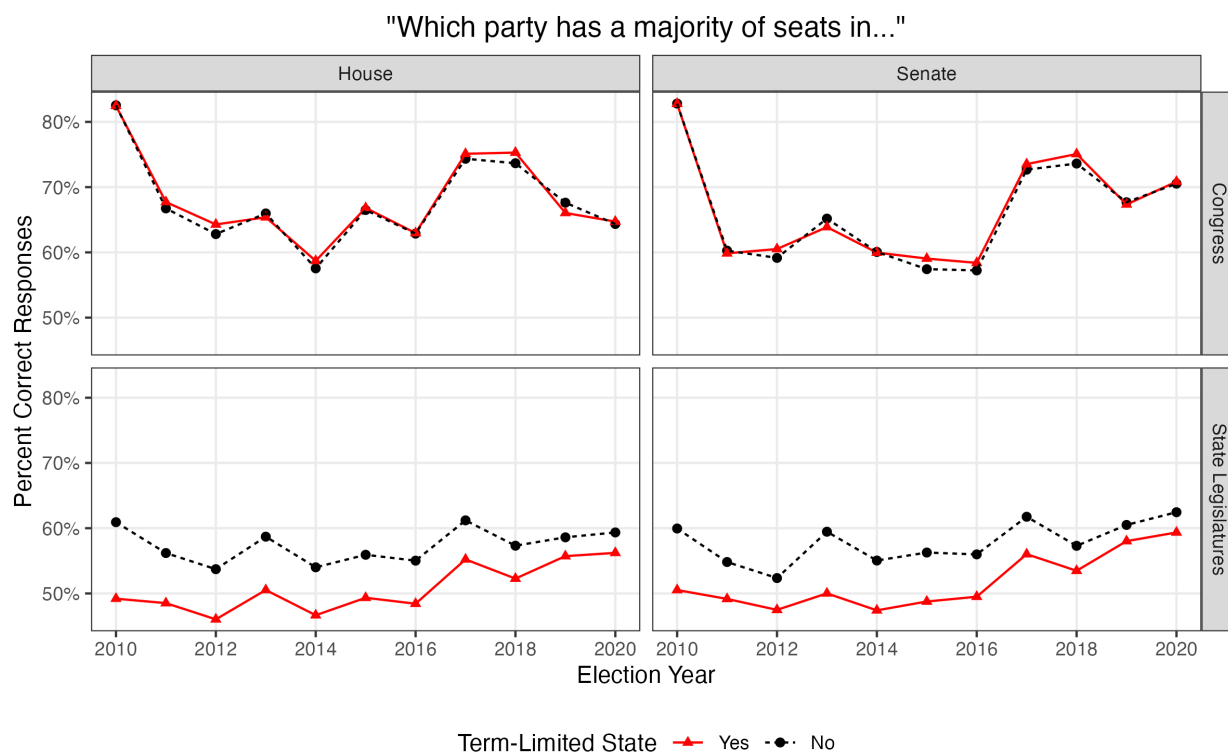
in the absence of other historical survey data on state legislative elections, this is the best available measure of voters' knowledge of legislative activities.

Since the CES data begins in 2010, I am unable to obtain a formal difference-in-differences estimate of the effect of term limits on correct response rate. Instead, I use the same individuals' response rate for questions about Congress–an institution not subject to term limits–as a comparable untreated outcome. Figure 3 plots the percent of correct responses for questions about Congress and state legislatures stratified by whether the respondent resided in a term-limited state. Predictably, respondents know more about Congress than they know about their state legislature. More interestingly, the first row of Figure 3 illustrates that respondents in term-limited states are indistinguishable from their non-term-limited

counterparts when asked about Congress, suggesting these two groups of respondents have similar baseline levels of political knowledge. The second row, however, illustrates that respondents who live in term-limited states correctly identified the party in power at a level that is approximately 10 percentage points lower than respondents who reside in non-term-limited states. Although I am unable to formally test this difference, the fact that these survey respondents had identical baseline levels of political knowledge suggests that term limits act to reduce voter knowledge.

These results suggest that term-limit-induced polarization is partially driven by a lack of voter knowledge about legislative candidates' ideological platforms. When news coverage declines, voters are less able to select moderate candidates, yielding a more-extreme legislature.

5   Conclusion

Over the past three decades, term limits have had an important impact on American state legislatures. Despite extensive research on the effects of legislative term limits on critical political outcomes and legislator incentives, we know little about how term limits affect the ideological composition of state legislatures. This article introduces new data on candidate ideology and legislative news coverage to demonstrate how term-limit-induced devaluation of office and declines in voter knowledge drive legislative polarization.

My analysis suggests that term limits shape legislative polarization through two key pathways. First, term limits alter the ideological composition of the legislative candidate pool, creating a more extreme group of primary and general election candidates in addition to incumbents. Hence, one way term limits appear to drive polarization is by reshaping the ideological composition of the universe of candidates who run for office. I find that this effect is strongest in more-professionalized legislatures where term limits devalue office the most. These results are broadly consistent with citizen-candidate models in which the devaluation

of office causes moderate candidates to exit the political arena (Osborne and Slivinski, 1996; Besley and Coate, 1997; Alesina, 1988).

On the other hand, term limits also systematically affect electoral selection. I find that term limits erase the traditional electoral return to moderation in general elections. Although term limits do not directly constrain voters, I show that news coverage of legislative elections declines following the implementation of term limits. In response, survey data demonstrate that voter knowledge about state legislatures is also lower in term-limited states. Overall, these results suggest that–as a virtue of shortened expected tenure and diminished legislative power–term limits devalue legislative elections, generating reduced news coverage and voter interest. These trends serve to reduce candidate costs to extremity and limit voters' ability to penalize extremist candidates. Further, I find no evidence that term limits induce individual legislators to become more extreme in the absence of electoral incentives.

My results–and the study of state legislative polarization in general–are important for at least three reasons. First, state legislatures are increasingly consequential policy-making bodies. Many of today's most controversial political issues–including abortion rights, voting access, and election certification–originate and are decided in statehouses. If term limits alter the ideological composition of state legislatures, they will also impact a host of essential policy outcomes.

State legislatures are also a key source of future members of Congress. By one count, nearly half of the members of the 117th Congress were former state legislators (Manning, 2022). Thus, policies that affect the composition of state legislatures are certain to shape policy-making and polarization at the federal level (Hall, 2019; Thomsen, 2014).

Finally, my findings have important implications for the study of legislative polarization at large. The devaluation of office and elections is not particular to term limits or state legislatures. My results underscore the importance of attracting an ideologically representative sample of legislative candidates–by maximizing net benefits of running for political office–and incentivizing voter engagement in electoral politics.

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Appendix

Contents

A Summary Statistics and Data Descriptions

A.1 Key Variable Summaries

Table A.1 – Summary Statistics for Key Variables

| Variable	Mean	Median	Min	Max	Std. Dev.
Term Limits	0.2	0	0	1	0.4
Log(Legislative Prof.)	-1.8	-1.7	-3.6	-0.46	0.59
Divided Gov.	0.47	0	0	1	0.5
Party Competition	0.14	0.11	0.0025	0.44	0.098
Polarization (pipeline)	0.83	0.78	0.066	2.3	0.4
Polarization (general election)	0.83	0.78	0.066	2.2	0.39
Polarization (incumbents)	0.78	0.73	0.077	2.1	0.36
Dem. Vote Share	0.52	0.5	0	1	0.34
Dem. Win	0.51	1	0	1	0.5

Table A.2 – Data Coverage Matrix

A.2 Election Data Coverage

state	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	Total
AK	123/102		37/86		129/90		55/80		62/82		50/88		52/88		47/84		40/87		24/78		133/100		106/90		105/76		125/88		111/77	1199/1296
AL			238/206				177/222				189/212				168/207				173/203				152/195				172/204			1269/1449
AR	82/165		96/160		73/155		106/170		92/156		106/180		71/149		81/149		60/140		103/155		73/199		57/155		45/145		46/165		48/164	1139/2407
AZ	137/134		118/122		144/137		104/128		153/137		202/135		175/134		186/145		185/147		220/141		171/134		190/143		163/131		216/150		162/144	2526/2062
CA	291/192		205/196		218/193		185/191		182/197		164/186		149/192		201/188		157/191		295/192		270/194		262/193		258/193		140/187		160/189	3137/2874
CO	25/149		35/146		32/143		51/145		42/144		158/139		154/146		169/149		177/157		171/155		176/158		160/147		175/151		179/155		172/155	1876/2239
CT	100/365		54/347		14/327		33/323		17/302		22/316		24/299		26/304		37/302		41/327		32/334		39/321		24/325		48/349		20/338	531/4879
DE	28/101		29/77		26/84		17/72		17/81		35/95		21/77		30/81		11/85		26/86		46/93		24/80		19/76		44/85		28/78	401/1251
FL	267/250		149/211		111/211		139/192		230/219		169/217		97/183		134/195		122/208		165/212		179/232		99/193		211/233		156/237		180/254	2408/3247
GA	288/318		176/312		439/339		443/336		407/318		455/333		493/340		384/308		370/291		416/314		398/288		364/283		305/280		435/325		467/349	5840/4734
HI	115/108		129/109		104/112		105/124		95/113		98/136		43/119		60/116		57/96		104/126		93/123		128/108		139/96		140/85		136/99	1546/1670
IA	131/229		77/197		57/224		42/201		33/211		101/222		57/205		26/199		56/221		61/209		100/217		211/188		231/210		255/214		253/215	1691/3162
ID	83/161		51/151		84/162		67/148		82/151		138/179		89/161		64/163		63/163		73/161		78/183		62/160		59/167		76/163		65/158	1134/2431
IL	330/317		156/234		125/266		104/238		68/204		153/278		59/209		71/239		70/229		100/219		343/253		227/191		254/218		299/239		253/205	2612/3539
IN	122/217		100/210		77/209		55/195		33/192		85/186		71/192		96/207		115/208		100/215		268/208		215/189		238/205		264/211		252/209	2091/3053
KS	123/299		108/211		112/279		69/190		113/265		88/180		171/252		61/200		88/274		77/200		399/280		245/202		353/275		232/188		334/273	2573/3568
KY	90/165		112/165		135/190		118/185		58/165		70/164		88/181		58/172		58/156		77/185		84/172		60/178		92/189		122/220		89/187	1311/2674
LA				0/275				0/239				0/233				0/277				0/223				0/222				16/248		16/1717
MA	62/312		141/286		76/262		130/258		64/258		122/263		77/310		80/254		75/236		124/294		23/266		26/277		33/247		342/260		281/242	1656/4025
MD			609/329				456/307				518/314				512/313				502/317				516/308				523/305			3636/2193
ME	143/334		139/336		93/354		38/327		349/323		374/341		402/362		391/363		379/350		398/361		397/355		369/347		378/344		370/343		320/328	4540/5168
MI	281/207		311/295		209/220		410/295		149/218		370/295		256/218		310/291		325/210		494/293		361/218		525/296		418/222		571/296		395/219	5385/3793
MN	131/390		91/256		74/390		38/260		43/384		192/390		37/266		41/395		59/272		54/396		56/394		42/259		81/393		75/267		123/395	1137/5107
MO	214/287		168/292		169/296		353/289		361/274		488/309		390/291		375/296		357/277		412/282		393/269		332/269		336/255		435/313		373/270	5156/4269
MS				150/246				190/223				340/250				274/238				273/240				261/231				322/213		1810/1641
MT	63/219		108/228		58/208		81/193		92/222		117/216		134/216		124/229		134/224		161/217		283/226		292/230		283/222		255/210		267/205	2452/3265
NC	176/279		175/262		156/287		99/265		130/272		235/272		175/252		114/254		114/259		184/284		222/266		131/257		133/262		173/337		132/326	2349/4134
ND	193/236		189/226		193/227		182/229		95/146		93/141		121/133		88/132		90/134		121/124		140/137		124/120		135/133		133/127		130/123	2027/2368
NE	90/50		48/44		48/45		46/42		47/41		48/44		55/38		82/47		71/47		51/39		0/48		0/46		0/46		0/43		0/45	586/665
NH	749/712		486/692		667/721		659/686		549/682		669/747		712/770		724/762		698/771		517/790		57/774		59/759		64/760		55/777		59/826	6724/11229
NJ		97/232		2/158		85/231		0/158		90/231		96/234		0/154		86/228		5/159		88/238		89/236		1/158		192/235		181/158		1012/2810
NM	26/161		53/105		89/178		54/101		82/172		47/100		62/156		35/97		77/155		50/103		228/166		125/102		188/154		118/102		239/191	1473/2043
NV	116/101		78/97		67/95		51/95		56/91		16/95		85/96		61/85		59/100		126/103		87/96		79/87		101/93		84/89		75/86	1141/1409
NY	53/388		22/372		15/377		13/369		6/354		27/358		17/347		22/357		27/356		26/375		41/345		124/337		128/336		102/342		154/350	777/5363
OH	118/223		91/221		72/219		108/219		310/212		149/214		104/202		156/220		117/208		162/215		263/216		272/210		266/200		325/223		264/207	2777/3209
OK	155/186		163/184		142/217		102/194		108/198		100/189		242/206		135/187		102/192		99/179		108/169		125/173		202/213		319/216		122/174	2224/2877
OR	94/134		88/137		99/135		88/129		65/131		67/133		75/131		34/138		42/120		43/146		59/135		151/124		136/125		102/130		179/141	1322/1989
PA	285/425		197/377		194/390		116/360		92/349		118/366		108/345		252/383		159/346		159/364		420/346		412/332		354/345		465/372		452/376	3783/5476
RI	155/238		95/230		45/227		53/197		46/202		68/169		64/184		47/168		67/172		112/184		196/161		171/143		153/146		192/157		173/149	1637/2727
SC	148/235		89/165		121/239		46/159		124/234		104/159		94/221		94/155		169/223		91/159		80/207		63/154		145/209		103/171		149/251	1620/2941
SD	82/191		34/178		47/193		43/170		72/178		78/170		70/173		73/190		90/203		38/166		83/169		155/155		69/171		61/194		85/160	1080/2661
TN	303/179		276/175		270/175		210/159		211/160		283/180		235/176		254/169		220/160		239/183		269/173		219/162		247/171		292/202		162/162	3690/2586
TX	375/253		333/248		314/229		283/225		265/212		381/260		294/229		346/243		302/242		307/231		380/247		308/220		330/222		408/272		384/273	5010/3606
UT	16/162		42/160		25/142		32/158		34/162		25/146		20/150		26/159		10/174		16/162		34/161		14/158		20/147		40/160		11/149	365/2350
VA		12/155		12/226		17/138		22/195		23/150		36/189		45/136		36/188		35/153		41/191		23/143		49/189		64/160		77/227		492/2440
VT	211/318		179/306		166/313		167/300		187/313		148/287		134/299		126/276		129/286		121/287		175/234		182/245		210/262		195/253		214/280	2544/4259
WA	185/236		148/223		133/226		84/207		95/221		66/202		89/230		46/210		75/218		107/213		95/215		53/213		44/195		213/220		57/220	1490/3249
WI	140/186		97/184		98/194		66/184		69/183		90/175		115/179		80/189		122/191		137/199		283/197		238/177		219/173		224/190		264/205	2242/2806
WV	258/196		269/189		272/200		220/180		122/185		123/195		86/227		42/204		265/186		269/193		277/194		281/215		0/227		301/222		293/213	3078/3026
WY	177/155		38/114		37/114		33/109		28/112		49/108		59/108		50/101		37/105		77/101		138/95		143/100		177/130		137/103		153/96	1333/1651
Total	7334/10265	109/387	6627/10051	164/905	5829/9994	102/369	6131/9806	212/815	5535/9426	113/381	7448/10084	472/906	6126/9442	45/290	6582/9973	396/931	6037/9372	40/312	7423/10138	402/892	7991/9647	112/379	8132/9491	311/800	7521/9373	256/395	9562/10161	596/846	8240/9756	109848/155587

Note: Table shows counts of all unique legislators in my analysis dataset by state and year of election. Cell tuples denote number of observed primary candidates (first) and general candidates (second).

A.3 Polarization Simulation

Figure A.1 – Observed Versus Simulated Polarization in State Legislatures. This graph contrasts observed polarization with the theoretical maximum (minimum) polarization present if voters selected the most extreme (moderate) candidate in every election. Legislative polarization remains high regardless of voters' choices.
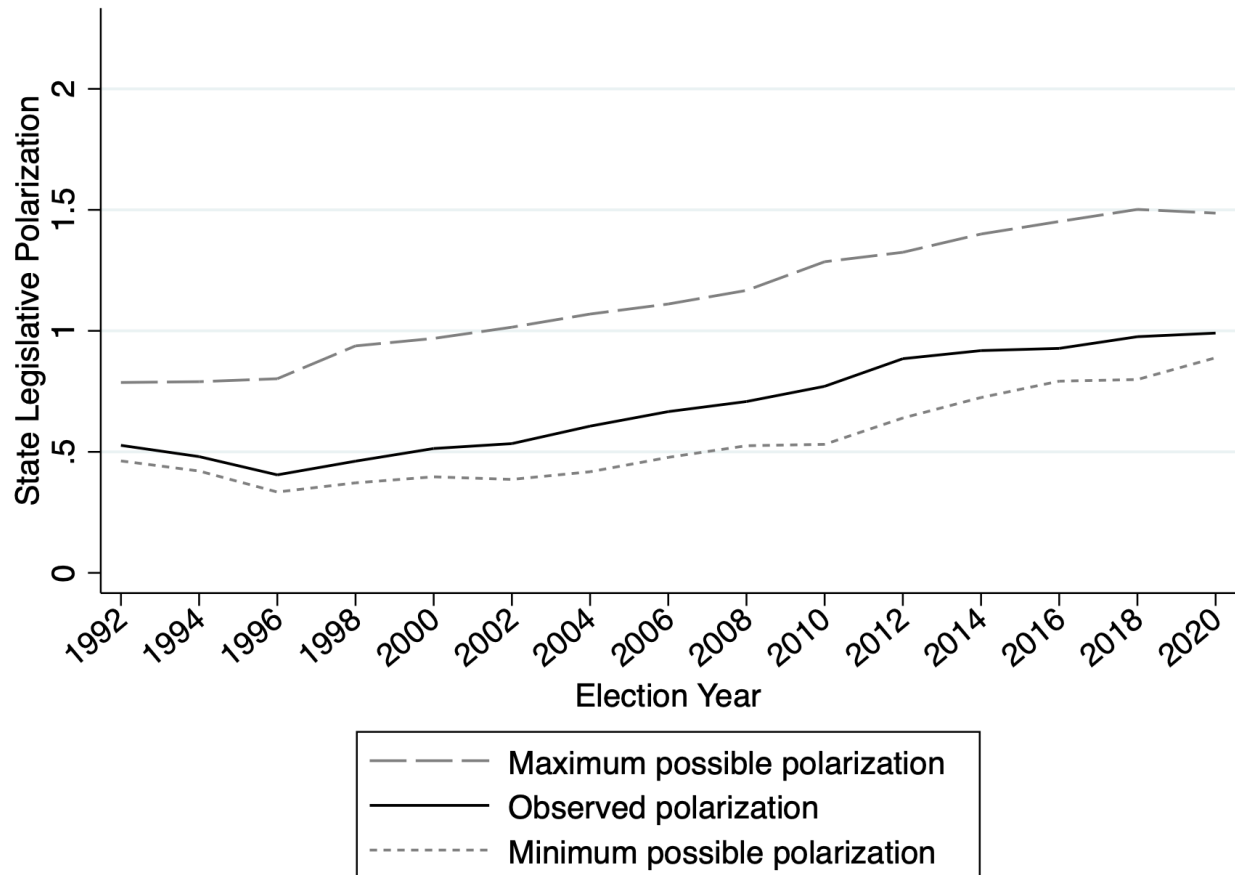
A.4 State Legislative News Coverage Collection Procedures

Newspapers.com hosts a vast repository of local and regional U.S. newspapers. As of October, 2022, the website contained 596 million pages of newsprint for 6,627 newspapers from 1992-2020, all of which are digitized. Table A.3 reports the number of newspapers and newsprint pages that enter this corpus by state. I construct two measures of state legislative news coverage: one of state legislature as a whole and one of individual incumbent legislators. My text-as-data approach matches Hopkins (2018).

First, to quantify aggregate new coverage of state legislatures, I search the Newspapers.com corpus for references to states' combined legislative bodies. For example, I search for references to the "Wisconsin State Legislature," "Virginia General Assembly," "Massachusetts General Court," and analogous bodies. I use the names of states' combined legislative bodies–rather than the names of their individual chambers–so that I do not mistakenly capture references to the U.S. House of Representatives or the U.S. Senate. Due to newspaper closures and a differing numbers of articles in each state-year, I normalize state legislative news coverage by references to "Congress" (and appropriate variations). Hence, my outcome is the ratio of references to state legislatives to references to Congress in each state-year.[18]

Second, a more-compelling design would examine the effect of term limits on the legislative news coverage of candidates in individual newspapers. To do so, I search the news corpus for references to incumbent state legislators in every state's legislative election years between 1992 and 2020.[19] To reduce the risk of false positive results, I restrict this search to newspapers within the candidates' states. Then, I aggregate counts of these references to the newspaper-year level. The result is a measure of how often individual newspapers reference

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[18]Recent work draws on machine learning methods to classify the content of news articles (e.g., Cagé, Hervé, and Viaud, 2020; Turkel et al., 2021), enabling researchers to evaluate the quality in addition to quantity of new coverage. These methods, while beyond the scope of this article, could prove fruitful in future analyses of legislative news coverage.

[19]While news coverage of challengers and open seat candidates is undoubtedly important, due to the time-intensive nature of this task I focus this analysis on incumbents.

state legislative incumbent legislators.

Table A.3 – Number of Newspapers and Pages in Corpus by State, 1992-2020.
This table reports the number of newspapers and total newspaper pages included in the news corpus by state.

State	Number of: Newspapers	Pages	State	Number of: Newspapers	Pages
AK	3	66,691	MT	21	6,812,752
AL	135	5,098,801	NC	407	11,720,065
AR	91	410,563	ND	2	996,250
AZ	14	9,875,798	NH	5	1,335,795
CA	161	42,289,744	NJ	87	18,170,278
CO	4	2,290,591	NM	30	5,631,452
CT	6	6,366,246	NV	6	2,805,647
DE	43	2,051,678	NY	50	47,997,616
FL	36	46,088,784	OH	69	24,636,316
GA	21	7,984,385	OK	771	4,007,204
HI	8	5,660,341	OR	13	4,000,690
IA	66	13,291,514	PA	144	35,638,641
ID	5	1,827,678	RI	1	136
IL	171	21,325,574	SC	34	11,212,620
IN	86	23,344,230	SD	30	2,710,751
KS	1,393	6,077,830	TN	157	12,118,670
KY	77	11,470,247	TX	68	30,182,854
LA	52	8,479,238	UT	108	5,607,505
MA	14	7,838,017	VA	17	5,981,645
MD	19	7,507,883	VT	196	3,756,570
ME	14	6,688,821	WA	27	12,736,124
MI	24	9,246,845	WI	103	20,197,122
MN	12	4,842,590	WV	8	449
MO	397	12,194,798	WY	5	1,349,823
MS	136	6,751,528			

B Robustness Checks for Polarizing Effects of Term Limits

B.1 Heterogenous Treatment Effect Robust Estimate of the Effect of Term Limits on Candidate Pool Polarization.

Figure B.1 – Effect of Term Limits on Candidate Pool Polarization (HTE Robust). This figure reports the treatment effect of term limits on candidate pool legislative polarization the Liu, Wang, and Xu (2022) imputation estimator. Periods containing two or fewer states are aggregated into a single endpoint. State legislative polarization increases significantly in the years following term limits' implementation. Robust standard errors are clustered by state.
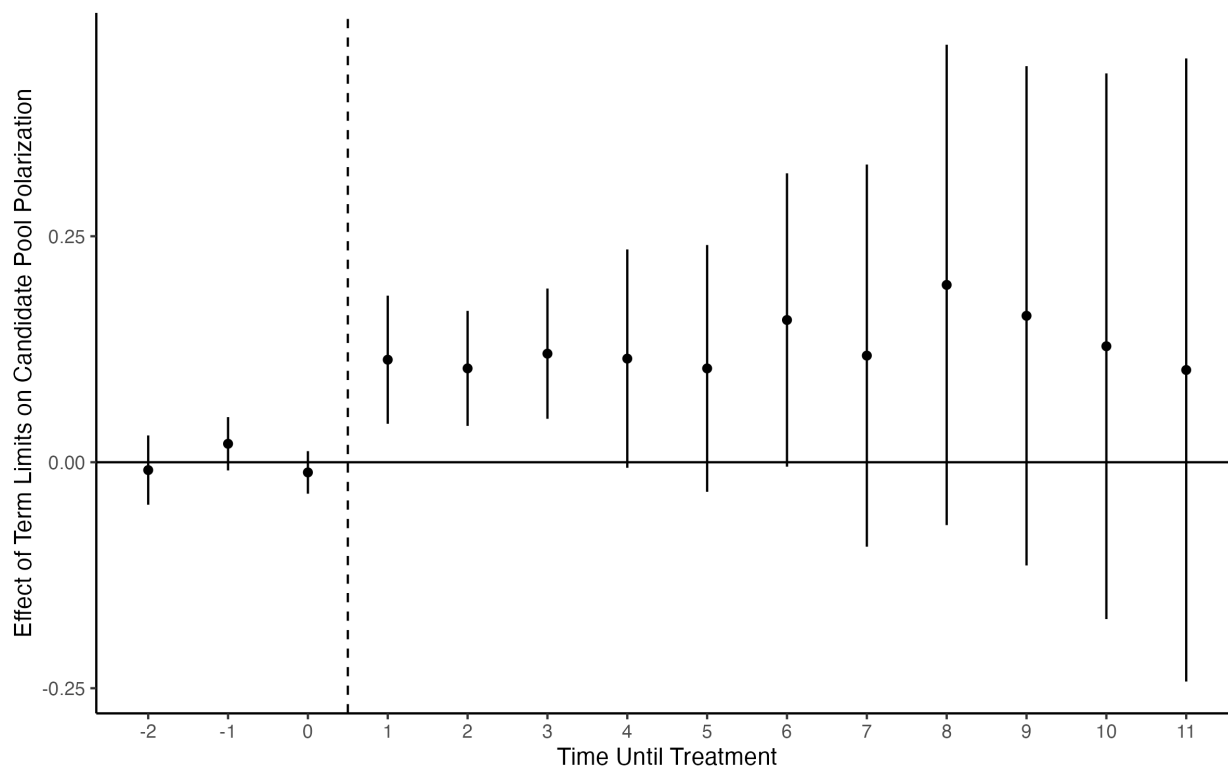
B.2 Effect of Term Limits on General Election Candidate Polarization

Table B.1 – Effect of Term Limits on General Election Candidate Polarization.
Term limits increase polarization among state legislative general election candidates.

	Polarization Among General Election Candidates			
	(1)	(2)	(3)	(4)
Term Limited	0.12	0.11	0.15	0.12
	(0.08)	(0.08)	(0.07)	(0.05)
Term Limited, $t+1$			-0.02	
			(0.06)	
Term Limited, $t+2$			0.01	
			(0.03)	
Log(Leg Prof)		0.05	0.01	0.05
		(0.06)	(0.05)	(0.08)
Divided Government		0.04	0.01	0.02
		(0.02)	(0.02)	(0.02)
Party Competetiveness		-0.44	-0.44	-0.30
		(0.23)	(0.16)	(0.13)
N	590	590	449	590
Outcome Standard Deviation	.39	.39	.39	.39
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State Specific Linear Time Trend	No	No	No	Yes

Note: In all columns the outcome is the difference in party median Hall-Snyder Scores. Standard errors are clustered by state in parentheses.

B.3 Difference-in-Differences Estimates Using CFscores

Table B.2 – Effect of Term Limits on Polarization Using CFscores. Term limits increase CFscore polarization across the electoral pipeline. These estimates are similar to Hall-Snyder Score measures of polarization (c.f., Tables 2, 3, and B.1)

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limits	0.12	0.11	0.09
	(0.06)	(0.06)	(0.06)
N	500	500	499
Outcome Standard Deviation	.4	.41	.4
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes

Note: In all columns the outcome is the difference in party median Dynamic CFscores. Standard errors are clustered by state in parentheses.

B.4 Difference-in-Differences Estimates Using Alternative Hall-Snyder Scores

Table B.3 – Effect of Term Limits on Polarization Using Alternative Hall-Snyder Scores. In this table, Hall-Snyder Scores are constructed after excluding donations to incumbents. Estimated effects are similar to those reported in the main paper.

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limits	0.26	0.26	0.32
	(0.10)	(0.11)	(0.10)
N	498	498	497
Outcome Standard Deviation	.4	.41	.4
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes

Note: In all columns the outcome is the difference in party median alternative Hall-Snyder Scores. Standard errors are clustered by state in parentheses.

B.5 Alternate Control Specifications

Table B.4 – Effect of Term Limits on Polarization: Additional State Covariate Specification. The addition of time-varying district and state controls does not alter my conclusions. Results are nearly identical when controls are added one at a time.

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limits	0.16	0.15	0.17
	(0.07)	(0.07)	(0.06)
Log(Leg Prof)	0.04	0.04	0.01
	(0.06)	(0.06)	(0.08)
Divided Government	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)
Party Competitiveness	-0.43	-0.45	-0.45
	(0.19)	(0.19)	(0.17)
Log(Population)	0.29	0.23	0.23
	(0.26)	(0.26)	(0.23)
Unemployment Rate	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)
Per Capita Income	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
Gini	-0.86	-0.80	-0.91
	(0.39)	(0.38)	(0.31)
Democratic Governor	-0.03	-0.02	-0.01
	(0.02)	(0.02)	(0.02)
Percent Population Foreign Born	-0.00	-0.02	-0.02
	(0.04)	(0.04)	(0.04)
N	499	499	499
Year FEs	Yes	Yes	Yes
State FEs	Yes	Yes	Yes

Note: In all columns the outcome is the difference in party median Hall-Snyder Scores. Standard errors are clustered by state in parentheses.

B.6 Incumbents' Ideological Response to Term Limits

Table B.5 – Effect of Term Limits on Within-Legislator Ideology. Legislators serving their final term before being termed out do not meaningfully change their ideology.

	Absolute Value of Incumbent Ideology	
	(1)	(2)
Term Limited	-0.0007	-0.0010
	(0.0062)	(0.0064)
N	54,266	54,233
Standard Deviation	.36	.36
Legislator FEs	Y	Y
Chamber-by-Year FEs	Y	N
Chamber-by-Party-by-Year FEs	N	Y

Note: Outcome is the absolute value of candidates' dynamic Hall-Snyder Scores. *Term Limited* indicates when legislators are serving in their final term before being termed-out of office. Robust standard errors clustered by legislator in parentheses.

C Robustness Checks for Electoral Selection

C.1 Marginal Effects Plots

Figure C.1 – Marginal Effects Plot for General Election Electoral Returns. This figure plots the marginal effect of term limits on Democratic vote share and win probability in general elections. Error bars and bands represent 95% confidence intervals. Model specifications match Table 6 columns 3 and 7.

(a) Democratic Vote Share   (b) Democratic Victory
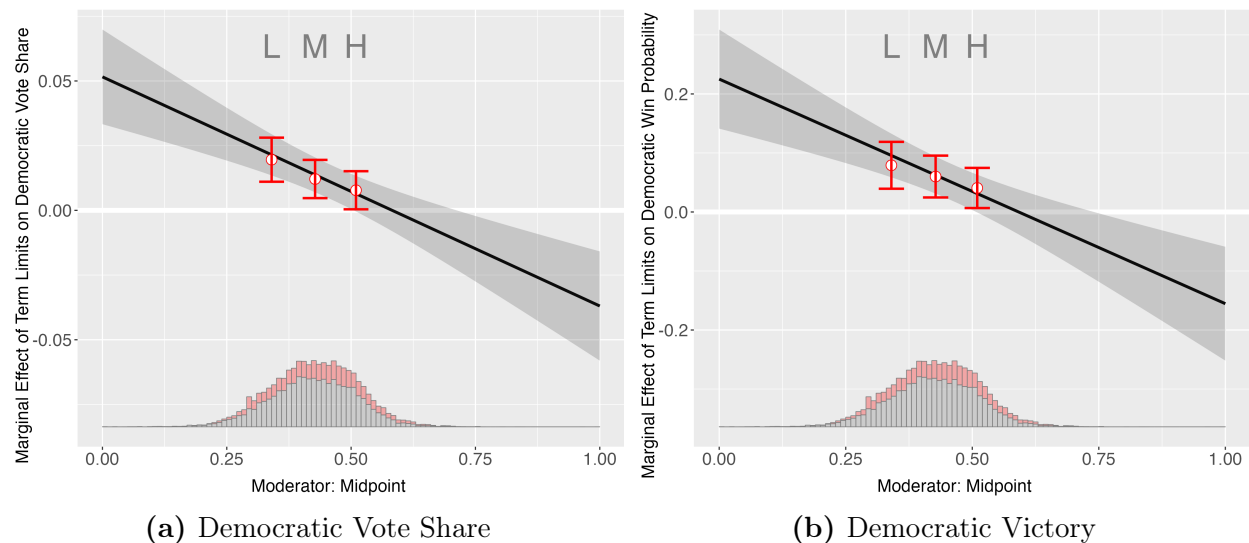
C.2 Electoral Selection Using CFscore Ideal Points

Table C.1 – General Election Electoral Selection, CFscores. These models report the expected general election electoral returns and change in win probability resulting from a liberal (i.e., positive) shift in candidate CFscore ideology.

	Dem Vote Share				Dem Win			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Midpoint	0.22	0.52	0.25	0.25	0.94	1.80	1.06	1.06
	(0.01)	(0.02)	(0.01)	(0.01)	(0.05)	(0.06)	(0.05)	(0.05)
Term Limits		0.08	0.08	0.08		0.29	0.28	0.25
		(0.02)	(0.01)	(0.02)		(0.07)	(0.06)	(0.07)
Midpoint · Term Limits		-0.14	-0.13	-0.13		-0.48	-0.45	-0.43
		(0.03)	(0.02)	(0.02)		(0.11)	(0.10)	(0.10)
Distance · Term Limits				-0.00				0.07
				(0.02)				(0.10)
Distance	-0.05	-0.13	-0.05	-0.05	-0.26	-0.49	-0.26	-0.28
	(0.01)	(0.02)	(0.01)	(0.01)	(0.05)	(0.06)	(0.05)	(0.06)
Dem Contributions	0.61		0.60	0.60	1.61		1.60	1.60
	(0.01)		(0.01)	(0.01)	(0.05)		(0.05)	(0.05)
Rep Contributions	-0.68		-0.68	-0.68	-1.94		-1.94	-1.94
	(0.01)		(0.01)	(0.01)	(0.05)		(0.05)	(0.05)
N	22,603	22,603	22,603	22,603	22,603	22,603	22,603	22,603
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Outcomes are reported in column headers. Robust standard errors clustered by district in parentheses. Midpoint and Distance are scaled to run from 0 to 1. Sample is limited to contested general elections in single member districts.

C.3 Differential General Election Accountability

Table C.2 – Differential General Election Accountability Model. This table estimates electoral returns to moderation in general elections separately for incumbents and challengers. Both incumbents and challengers receive electoral boosts from extremity in term-limited states.

	Dem Vote Share		Dem Win	
	(1)	(2)	(3)	(4)
Term Limited	0.016	0.011	0.049	0.035
	(0.005)	(0.005)	(0.018)	(0.018)
Incumbent Ideology	0.060	0.061	0.171	0.173
	(0.003)	(0.004)	(0.014)	(0.015)
Incumbent Ideology · Term Limits	-0.031	-0.027	-0.090	-0.084
	(0.006)	(0.006)	(0.021)	(0.021)
Challenger Ideology	0.012	0.013	0.040	0.040
	(0.002)	(0.003)	(0.008)	(0.008)
Challenger Ideology · Term Limits	-0.011	-0.011	-0.021	-0.026
	(0.004)	(0.004)	(0.013)	(0.014)
N	15,538	15,128	15,538	15,128
Year FEs	Yes	No	Yes	No
District FEs	Yes	Yes	Yes	Yes

Note: Outcomes and scalings are reported in column headers. Robust standard errors clustered by district in parentheses. Sample is limited to contested general elections with one incumbent in single member districts.

D   Effect Heterogeneity

D.1   Asymmetric Polarization

Table D.1 – **Asymmetric Polarization in the Candidate Pipeline.** Democratic and Republican legislative candidates and incumbents do not differentially polarize in response to term limits.

	Candidate Pool Polarization		General Election Polarization		Incumbent Polarization	
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limited	0.07	0.06	0.07	0.05	0.08	0.06
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Log(Leg Prof)	-0.02	0.04	-0.02	0.04	-0.04	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Divided Government	0.02	0.00	0.02	0.00	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Party Competetiveness	-0.18	-0.30	-0.15	-0.31	-0.17	-0.25
	(0.11)	(0.15)	(0.10)	(0.15)	(0.09)	(0.14)
N	590	590	590	590	590	590
Standard Deviation	.29	.23	.29	.22	.3	.22
Party	Dems	Reps	Dems	Reps	Dems	Reps
Year FEs	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y

Note: In all columns the outcome is the difference in party median Hall-Snyder Scores. Outcome is the absolute value of Hall-Snyder Scores aggregated by party. Standard errors clustered by state in parentheses.

D.2 District Safety

Table D.2 – Effect of Term Limits on Candidate Extremity by District Composition. The effect of term limits on polarization does not meaningfully vary by district partisan composition.

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limited	0.04	0.05	0.05
	(0.03)	(0.04)	(0.03)
Safe for Dems	0.02	0.02	0.05
	(0.03)	(0.03)	(0.04)
Safe for Reps	0.01	0.02	0.02
	(0.02)	(0.02)	(0.03)
Term Limited · Safe for Dems	-0.02	-0.03	-0.02
	(0.05)	(0.05)	(0.08)
Term Limited · Safe for Reps	0.03	0.02	0.06
	(0.03)	(0.03)	(0.05)
N	126,645	105,737	72,251
State FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Note: Outcome is the absolute value of legislators' Hall-Snyder Scores. Omitted category is competitive districts. Results are reported separately for candidate groupings listed in column headers. Robust standard errors clustered by state in parentheses.

D.3 Term Limits' Effects by Chamber

Table D.3 – Effect of Term Limits on Polarization by Chamber. The effect of term limits on polarization is similar between legislative upper and lower chambers.

	Candidate Pool Polarization		General Election Polarization		Incumbent Polarization	
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limited	0.09	0.15	0.09	0.16	0.11	0.20
	(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.05)
Log(Leg Prof)	0.05	-0.06	0.05	-0.06	0.03	-0.03
	(0.07)	(0.06)	(0.07)	(0.06)	(0.09)	(0.08)
Divided Government	0.04	0.03	0.05	0.02	0.04	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Party Competetiveness	-0.55	-0.35	-0.51	-0.28	-0.61	-0.30
	(0.26)	(0.25)	(0.25)	(0.25)	(0.22)	(0.21)
N	582	549	582	549	582	538
Standard Deviation	.4	.4	.39	.39	.37	.36
Party	House	Senate	House	Senate	House	Senate
Year FEs	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y

Note: In all columns the outcome is the difference in party Hall-Snyder Scores subsetted by legislative chamber. Robust standard errors are clustered by state in parentheses.