

Why Do Term Limits Polarize State Legislatures?*

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Abstract

Term limits have dramatically reshaped many features of legislative politics, yet the general equilibrium ideological effects of these policies remain unclear. Pairing new roll-call based candidate ideology scalings and local newspaper data with a difference-in-differences design for 1992-2022, this article traces the causal chain of term limits' effects on legislative polarization across the candidate pipeline. I find that newspaper coverage of legislative elections declines sharply as tenured incumbents are termed out, translating into diminished voter knowledge about their state legislators. Consequently, term limits systematically attenuate traditional electoral returns to moderation in general elections. As the benefits of ideological moderation decline, I find that the pool of primary and general election office-seekers polarize and, ultimately, election winners become mechanically more extreme in term-limited states. These findings help explain why term limits polarize state legislatures and illustrate how both declining news coverage and the scarcity of moderate candidates contribute to legislative polarization.

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*“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, former editor, The Sacramento Bee

1 Introduction

Few institutional reforms to American legislatures have received more scholarly attention than legislative term limits. A rich literature investigates how term limits affect important political outcomes including the incumbency advantage (Ansolabehere and Snyder Jr., 2004; Fowler and Hall, 2014), electoral competition (Cain, Hanley, and Kousser, 2006; Maskett and Lewis, 2007), inter-branch power distribution (Carey et al., 2006), and legislative productivity (Fouirnaies and Hall, 2022), among others.² Despite their far-reaching political consequences and theoretical implications, however, there is limited empirical evidence on how term limits shape legislative polarization, candidate entry, and electoral selection. To address this gap, building on Olson and Rogowski (2020) I conduct the first general equilibrium analysis of the ideological effects of legislative term limits—analyzing candidates at all stages of the electoral pipeline—and evaluate their contribution to rising polarization. Understanding these ideological effects is important for at least two reasons. First, as support for term limits grows among both the public and legislators, my analysis highlights their unintended consequences while informing future policy design.³ Second, this paper addresses widespread concern about polarization across American legislative landscapes, underscoring how weakened news environments and the dwindling supply of moderate candidates drive polarization.

¹Schrag (1998), pp. 13.

²The legislative term limits literature is too vast to catalogue here in its entirety. See Mooney (2009) for a survey of research on the political effects of term limits.

³A March 2023 survey, for example, reported that 83% of registered voters supported term limits for members of Congress (Kull, 2023). In parallel, at least seven constitutional amendments have been introduced in the 118th Congress that would limit congressional tenures (Congressional Research Service, 2023), while voters in North Dakota in 2022 overwhelmingly voted to enact term limits for governor and state legislators (MacPherson, 2022).

In this paper, I first estimate the effect of term limits on the ideological composition of both incumbents (e.g., Olson and Rogowski, 2020) and non-incumbents using a difference-in-differences design combined with a new roll-call based candidate ideology scaling for the years 1992-2022.⁴ I find that term limits increase ideological polarization across the electoral pipeline, including among primary and general election office-seekers in addition to sitting legislators. Second, drawing on an extensive local newspaper corpus, survey responses, and election returns data, I evaluate how term limits affect the ideological composition of the legislative candidate pool. I show that, consistent with evidence on incumbent press advantages (Arnold, 2004; Cook, 2010; Robinson, 1981; Vinson, 2003), aggregate newspaper coverage of state legislators declines by 24% on average following the introduction of term limits, and this shock translates into diminished voter knowledge about legislative politics. Finally, as a result, I document that the traditional electoral returns to ideological moderation in general elections are reduced by nearly half in term-limited states. These findings suggest that, by reducing news coverage of legislative elections, term limits attenuate voter knowledge, allowing more-extreme candidates to run with weakened threat of electoral sanction.

This paper builds most directly on recent work on legislative term limits by Olson and Rogowski (2020) and Fourniaies and Hall (2022). Leveraging a state-level difference-in-differences design, Olson and Rogowski (2020) show that term limits increase polarization in state legislative roll-call voting patterns. In contrast, Fourniaies and Hall (2022), who use a legislator-level difference-in-differences, 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. My analysis reconciles these competing perspectives by illustrating how, even as individual legislators' ideology remains constant, state legislatures may polarize as the pool of legislative candidates becomes increasingly extreme.

⁴Since the ideological composition of office-seekers is instrumental in shaping legislative polarization (Hall, 2015; Thomsen, 2017), my analysis of the full candidate pool contributes an important new perspective on term-limit-induced legislative polarization.

My research also relates to a growing 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; Ferraz and Finan, 2011; Fourinaies and Hall, 2022; List and Sturm, 2006) and complements work on professionalization and partisan control in state legislatures (Fiorina, 1994, 1996; Meinke and Hasecke, 2003).⁵ More broadly, this paper connects to a rich literature on political polarization across legislative landscapes (Handan-Nader, Myers, and Hall, 2022; Layman, Carsey, and Horowitz, 2006; McCarty, Poole, and Rosenthal, 2006; Poole and Rosenthal, 1984; Shor and McCarty, 2011, 2022) and electoral selection (Ansolabehere, Snyder, and Stewart, 2001; Canes-Wrone, Brady, and Cogan, 2002; Canes-Wrone and Kistner, 2022; Hall, 2019; Rogers, 2017; Utych, 2020).

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 trace the effect of term limits on legislative newspaper coverage and voter knowledge about legislative politics. Section four investigates how term limits affect electoral selection. In the fifth section, I focus on candidate supply, documenting the polarizing effects of term limits across all stages of legislative elections, culminating in a more extreme set of incumbent state legislators. Finally, section six discusses implications of the findings and concludes.

2 Empirical Strategy

Over the past three decades, twenty-two states have enacted legislative term limits.⁶ 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

⁵Other related studies of term-limit induced ideological extremity 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. Finally, Masket and Shor (2015) find that the partisan-lead replacement of termed-out legislators in Nebraska accelerated that state’s polarization, even in the absence of traditional partisan affiliations.

⁶Legislative term limits imposed by voters in six states were nullified by court or legislative action. Sixteen states currently have legislative term limits in effect.

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.

allow ballot initiatives, only four—Alaska, Illinois, Mississippi, and North Dakota—have not imposed term limits at some time. Hence, from a design perspective, term limits represent a plausibly independent shock to the electoral system.

To implement my study, I build three key datasets on state legislative elections, each of which I describe in detail below. Cumulatively, these data cover 49 states for the years 1992-2022, ensuring comprehensive coverage of candidates at all stages of the election pipeline.⁷ Table 1 summarizes the relevant characteristics of the term-limited states that enter my analysis.

⁷In accord with existing work, I exclude non-partisan Nebraska from the analysis. In 2022 voters in North Dakota approved a ballot initiative that limits legislators to eight years in each chamber. Since these limits were not retroactive and legislators have not yet been termed-out, North Dakota does not appear in Table 1. Finally, I also focus on Democratic and Republican candidates.

2.1 Predicting Legislative Roll-Call Using Hall-Snyder Scores

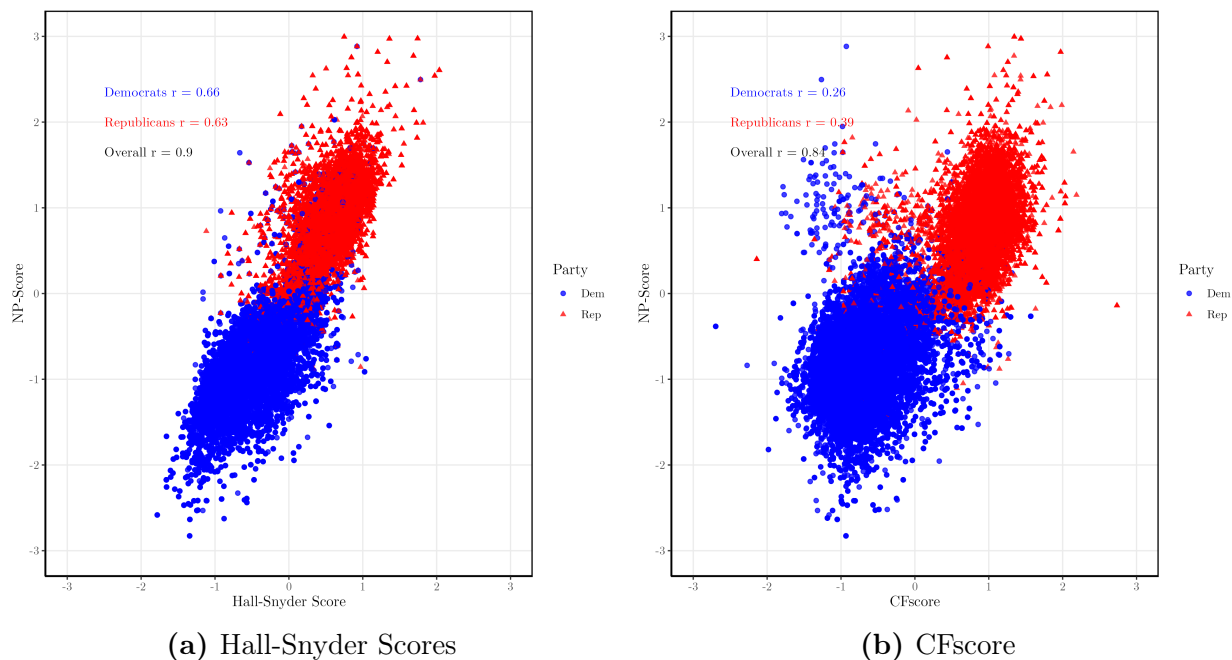
Due to data limitations, existing empirical work on term limits focuses exclusively on incumbent legislators’ ideology. As Hall (2019) illustrates, designs focused on incumbent legislators miss a key source of polarization from the candidate pipeline. Hence, an ideal measure of ideology for the study of term limits would capture how candidates would cast roll-call votes if elected to office. Unfortunately, existing candidate ideology scalings are not optimized to measure state legislative roll-call behavior.⁸ For example, 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 supervised 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 scores, rather than state legislative NP-Scores. Hence, to accurately measure legislative polarization, a new ideology scaling is required.

In this paper, to measure the roll-call ideology of state legislative candidates I follow the scaling method proposed for congressional candidates by Hall and Snyder (2015) and recently applied to state legislative candidates by Handan-Nader, Myers, and Hall (2022). This method scales candidates using the contribution-weighted ideology of their donors. Specifically, donors’ ideological positions are estimated as the contribution-weighted NP-Score (Shor and McCarty, 2011) of incumbents to which they contribute. Subsequently, candidates are scaled as the contribution-weighted average of their donors, but where contributions to candidate i are excluded when calculating i ’s scaling. This paper extends the scalings produced by Handan-Nader, Myers, and Hall (2022) (henceforth referred to as Hall-Snyder Scores) to the universe of state legislative candidates for the period 1992-2022.⁹ Appendix B provides

⁸Shor 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.

⁹Handan-Nader, Myers, and Hall (2022) focus on candidates running in single member districts, account-

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.



complete details on the scaling process.

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 ($r = 0.66$ for Democrats and $r = 0.73$ for Republicans) and significantly higher than the within-party correlations between CFscores and NP-Scores (c.f., Figure 1(b)). Both within-party and overall correlations are higher than congressional Hall-Snyder Scores (Hall, 2015; Hall and Snyder, 2015). These results are consistent with a larger set of validation exercises performed in Handan-Nader, Myers, and Hall (2022).¹⁰ Finally, while I prefer Hall-Snyder Scores for their ability to differentiate candidates within-party, I show in Appendix

ing for 83% of all state legislative candidates since 1992. I construct scalings for the remaining candidates.

¹⁰Handan-Nader, Myers, and Hall (2022) use an optimal cutting-point procedure to calculate the percent of legislative votes for the years 2010-2022 that are classified correctly by NP-Scores, Hall-Snyder Scores, CFscores, and legislator party. They find that state legislative Hall-Snyder Scores correctly predict 91.0% of roll-call votes (APRE=.700) which is second only to NP-Scores (92.8%, APRE=.769). Since NP-Scores are calculated directly from roll-call votes while Hall-Snyder Scores are constructed independently of roll-call data, the similarity of classification rates underscores the validity of the Hall-Snyder scaling method in the context of state legislatures.

F.2 that my findings are substantively the same when I use CFscores.

2.2 Assembling Election Returns and Legislative News Coverage Data

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, 2023). These data contain the universe of state legislative general elections held during the period of my study.¹¹ 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 Stromberg, 2010) and inform electoral returns to moderation (Canes-Wrone and Kistner, 2023). To evaluate this possibility, I build a corpus of local and regional newspaper coverage of state legislators.¹² 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 1992-2022. Using these data, I measure legislator news exposure by counting the number of articles written about each legislator in every election year. Appendix C.1 outlines this process in detail. Overall, these data contain 13.7 million articles about sitting 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 2010-2022. These responses test individuals' knowledge of partisan control in their home legislature and Congress.

After merging the Hall-Snyder scalings with the general election returns and news coverage data, my combined dataset features a total of 224,015 candidate-year observations.

¹¹I exclude special elections from the analysis. Inclusion of this small subset of elections does not affect my conclusions.

¹²Since state legislative elections are typically highly localized, it is essential to analyze local and regional—rather than national—coverage.

2.3 Difference-in-Differences Design

My results throughout this paper are identified by a difference-in-differences design for the years 1992-2022.¹³ The unit of analysis is either the state, legislator, or legislator-newspaper. Here, I focus on the state-level specification, although the results generalize directly to the other two settings. Specifically, I model

$$Y_{st} = \beta_0 + \beta_1 \textit{Term Limited}_{st} + \Omega X_{st} + \alpha_s + \delta_t + \epsilon_{st} \quad (1)$$

where Y_{st} is an outcome (either legislative polarization or a measure of news coverage) in state s in year t , X_{st} is a vector of controls, and α_s and δ_t are state and year fixed effects, respectively. The term limits variable, $\textit{Term Limited}_{st}$, indicates whether state s in time t had term limits in effect.¹⁴ The error term, ϵ_{st} , is clustered at the state level. This specification allows me to compare outcomes within states over time.

My difference-in-differences design requires a parallel trends assumption. This assumption dictates that polarization or news trends in control states (i.e., states that never enacted legislative term limits) approximate 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 (“pre-trends”) 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.

¹³This modeling strategy was first implemented by Olson and Rogowski (2020).

¹⁴Since term limits alter legislative incentives before becoming binding, an alternate definition might operationalize $\textit{Term Limited}_{st}$ 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 $\textit{Term Limited}_{st}$ based on implementation date will likely serve to attenuate coefficient estimates, making this analysis particularly rigorous. When years of impact differ between a state’s upper and lower chamber, I code treatment as beginning on the first year of impact.

Table 2 – Effect of Term Limits on State Legislative News Coverage. Local and regional newspapers write fewer articles about state legislators following the implementation of term limits.

	Articles About Legislator			
	(1)	(2)	(3)	(4)
Term Limited	-12.310 (7.056)	-9.974 (3.793)	-1.214 (0.382)	-1.087 (0.571)
N	118,755	118,755	510,600	510,600
Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes
Newspaper-District FEs	No	No	Yes	Yes

Note: In columns one and two, the unit of analysis is the legislator-year. In columns three and four, the unit of analysis is the legislator-newspaper-year. Across all columns, the outcome is the number of references to state legislative candidates 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.

3 News Coverage and Voter Knowledge in Term-Limited States

My analysis begins with the legislative news environment. Previous research suggests that candidates running in races that receive sparse news coverage 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 this section, I explore the first link in this process: whether the legislative news environment changes post-term limits.

3.1 Newspaper Coverage of State Legislators

A rich literature documents that local news media devote more coverage to incumbents and senior members of Congress than challengers and junior members (Arnold, 2004; Cook, 2010; Robinson, 1981; Vinson, 2003). These coverage differentials may arise because local reporters,

who often lack the resources and time to initiate and write political articles, rely on legislators to alert them to important stories and provide relevant information (Kaniss, 1991; Paletz and Entnam, 1981). Further, more-experienced legislators may have stronger journalistic connections, allowing them to garner more coverage than their more-junior counterparts (Arnold, 2004).

In Appendix C.1, I test whether the patterns described above hold in state legislatures. Mirroring the literature on Congress, Appendix Figure C.1 shows that the average number of articles written about state legislators is increasing in legislator tenure, although this relationship levels off after approximately 10 years of experience. For example, in my data, an average of 39 articles are written about freshmen legislators while 67 are written about legislators with 10 years of experience, or an approximately 72% increase. Hence, by reducing the number of tenured incumbents and weakening relationships with the press corps, term limits may indirectly reduce the number of articles written about legislative politics and legislative news coverage in general.¹⁵

To evaluate this possibility, Table 2 reports the difference-in-differences estimate of the effect of term limits on newspaper coverage of state legislators. My specification mirrors Equation 1 where s is the individual legislator or legislator-newspaper.¹⁶ First, in columns one and two, the unit of analysis is the legislative district, and I use year and district fixed effects to account for differential newspaper coverage. On average, newspapers write 51 articles about an individual state legislator every election cycle. Column one indicates that term limits are associated with 12 fewer articles written about each legislator, or a 24% decline in coverage. Column reports a similar 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. Second, to further validate the analysis, columns three and four of Table 2 analyze the same relationship at the legislator-newspaper level and include

¹⁵This possibility is clearly reflected in a Michigan statehouse reporter’s reflections on legislative term limits: “partly because of term limits...there are fewer long-lasting relationships between the media and the elected officials” (quoted in Cooper and Johnson, 2006, 23).

¹⁶Observe that the treatment, $Term\ Limited_{st}$, is identical for all legislators in a given state-year.

newspaper-by-district and year fixed effects. I again find that, following the implementation of term limits, newspapers write fewer articles about the sitting state legislator. The average newspaper writes 11 articles about state legislators, so columns three and four translate into a more-modest 10% decline in coverage after term limits.

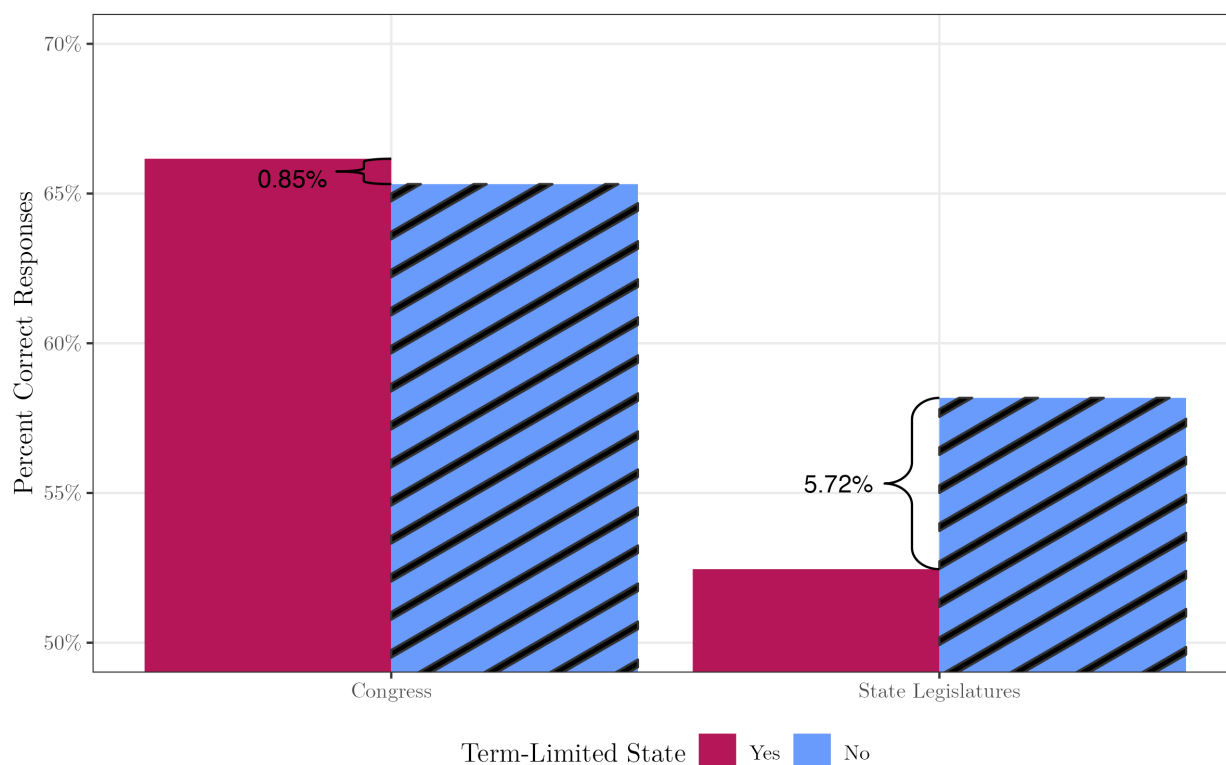
3.2 Voter Knowledge in Term-Limited States

Having documented that term limits reduce legislative news coverage and diminish 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 on 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 2010-2022, I impute the correct response and compute the share of respondents who answer correctly. Although limited in yearly coverage and question scope, 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 identical questions about Congress—an institution not subject to term limits—as a comparable untreated outcome. Figure 2 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 left two bars in Figure 2 illustrate that respondents in term-limited states are nearly indistinguishable from their non-term-limited

Figure 2 – 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) have reduced political knowledge about their home state legislatures.



counterparts when asked about Congress (difference of 0.85 percentage points), suggesting these two groups of respondents have similar baseline levels of political knowledge. The right two bars in Figure 2, however, illustrate that respondents who live in term-limited states correctly identified the party in power at a level that is nearly 6 percentage points (pp.) lower than respondents who reside in non-term-limited states. Using the correct response rate for questions about Congress as the control condition, my informal difference-in-difference estimate of the effect of term limits on voter knowledge about state politics is 6.57 pp. (5.72 pp. – 0.85 pp.).¹⁷ In Appendix D.1, I show that these differences hold after stratifying response rates by legislative chamber, year, and chamber-year.

¹⁷I refer to this estimate as an “informal” difference-in-differences because the estimand is identified by cross-sectional—rather than time-series—variation.

In this section, I have demonstrated that term limits—by removing incumbents who receive disproportionate newspaper coverage—attenuate press coverage of legislative elections. I further presented evidence that these informational shocks translate into diminished voter knowledge about their state legislatures. In the next section, I test whether voters are less able to select moderate candidates as a result of the weakened informational environment in term-limited states.

4 Electoral Selection

Voters in state legislative general elections have a well-documented preference for moderate candidates (Handan-Nader, Myers, and Hall, 2022; Rogers, 2017). I now test whether changes to the legislative news environment alter electoral selection in term-limited states.

4.1 Midpoint Design

To assess the role term-limited states’ electorates play in producing increased legislative 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) to my setting. Consequently, I estimate a baseline equation of the form

$$Y_{dct} = \beta_0 + \beta_1 \text{Midpoint}_{dct} + \beta_2 \text{Distance}_{dct} + \Omega X_{dct} + \alpha_d + \delta_t + \epsilon_{dct} \quad (2)$$

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 .¹⁸ *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

¹⁸Since 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.

error term, ϵ_{dct} , is clustered by district d .¹⁹

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* under term limits. 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) suggests 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 electoral selection differs between term-limited and non-term-limited states by adding the terms $\beta_3 \textit{Term Limited}_{dct} + \beta_4 \textit{Midpoint}_{dct} \cdot \textit{Term Limited}_{dct}$ to Equation 2, where *Term Limited* indicates whether term limits are binding.²⁰ For this study, the key coefficient is β_4 , which captures the change in *Midpoint* following the adoption of term limits.

4.2 Electoral Selection Results

Table 3 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-

¹⁹The midpoint model requires the ideology of districts' median 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.

²⁰The full equation is then

$$Y_{dct} = \beta_0 + \beta_1 \textit{Midpoint}_{dct} + \beta_2 \textit{Distance}_{dct} + \beta_3 \textit{Term Limited}_{dct} + \beta_4 \textit{Midpoint}_{dct} \cdot \textit{Term Limited}_{dct} + \Omega X_{dct} + \alpha_d + \delta_t + \epsilon_{dct}.$$

Table 3 – Term Limits Attenuate 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 halved in term-limited states.

	Dem Vote Share				Dem Win			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Midpoint	0.052 (0.015)	0.222 (0.023)	0.077 (0.019)	0.075 (0.019)	0.482 (0.075)	1.158 (0.105)	0.621 (0.092)	0.621 (0.094)
Term Limits		0.046 (0.017)	0.042 (0.014)	0.036 (0.016)		0.267 (0.073)	0.255 (0.064)	0.254 (0.088)
Midpoint · Term Limits		-0.073 (0.033)	-0.061 (0.028)	-0.057 (0.028)		-0.386 (0.144)	-0.347 (0.126)	-0.347 (0.136)
Distance · Term Limits				0.010 (0.020)				0.001 (0.100)
Distance	0.083 (0.011)	0.082 (0.013)	0.081 (0.011)	0.077 (0.014)	0.283 (0.054)	0.268 (0.061)	0.273 (0.054)	0.272 (0.070)
Dem Contributions	0.033 (0.001)		0.033 (0.001)	0.033 (0.001)	0.118 (0.004)		0.118 (0.004)	0.118 (0.004)
Rep Contributions	-0.028 (0.001)		-0.028 (0.001)	-0.028 (0.001)	-0.118 (0.005)		-0.118 (0.005)	-0.118 (0.005)
N	13,821	13,821	13,821	13,821	13,821	13,821	13,821	13,821
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.

term-limited state-years. To estimate this difference, the remaining columns in Table 3 interact *Midpoint* with *Term Limits*, an indicator for the presence of legislative term limits defined in Section 5. 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 3 are negative and highly significant, indicating that extreme ideological positions are penalized at a lower rate in term-limited states. Columns two and six indicate that term limits reduce by half the advantage to mod-

eration 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 3 predict that term limits reduce the electoral return to moderation in general elections by approximately 50%.

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 E.1.²¹ As Figure E.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 E.1, I reestimate the midpoint model using CFscores. My substantive conclusions are unchanged using this alternative scaling.

In sum, Table 3 establishes a key new finding: term limits systematically attenuate the traditional advantage of more-moderate candidates in contested general elections. These results are robust across alternative ideological scalings and modeling specifications.

5 Term Limits Increase Polarization Across the Electoral Pipeline

The results presented thus far indicate that electoral returns to moderation disappear in term-limited state legislatures. As these returns fall, we may expect the supply of moderate candidates to decline in parallel. 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.

²¹Figures were created using the R package *interflex*.

The analysis proceeds in two stages. First, I evaluate how term limits affect polarization among the supply of legislative candidates, including the pool of primary and general election candidates. Second, I assess whether this polarization of the candidate pool translates into increased extremity among incumbent legislators.²²

5.1 Effect of Term Limits on Candidate Supply

To evaluate how term limits affect the supply of legislative office-seekers, I begin by analyzing how term limits affect polarization across the candidate pipeline—including primary and general election candidates. For a given set of candidates or legislators, I define legislative polarization, Y_{st} , as the difference between the median Republican and Democratic candidates’ ideology scalings in state s in year t . Across 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 5.1.1 and Appendix F.3, I show my results are robust to a variety of alternative specifications.

Table 4 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 third of one standard deviation of the distribution of polarization. For more context, the *Term Limited* coefficient in Table 4 is equivalent

²²While Olson and Rogowski (2020) initially studied this second estimand, I revisit their analysis with the benefit of expanded legislative ideology data.

Table 4 – 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	0.13 (0.05)	0.10 (0.05)	0.12 (0.05)	0.13 (0.06)
Term Limited, $t + 1$			0.01 (0.03)	
Term Limited, $t + 2$			-0.01 (0.05)	
Log(Leg Prof)		-0.21 (0.05)	-0.18 (0.05)	-0.10 (0.04)
Divided Government		0.01 (0.01)	0.01 (0.01)	0.00 (0.01)
Party Competetiveness		-0.58 (0.18)	-0.46 (0.13)	-0.18 (0.09)
N	611	611	523	611
Outcome Standard Deviation	.31	.31	.31	.31
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..

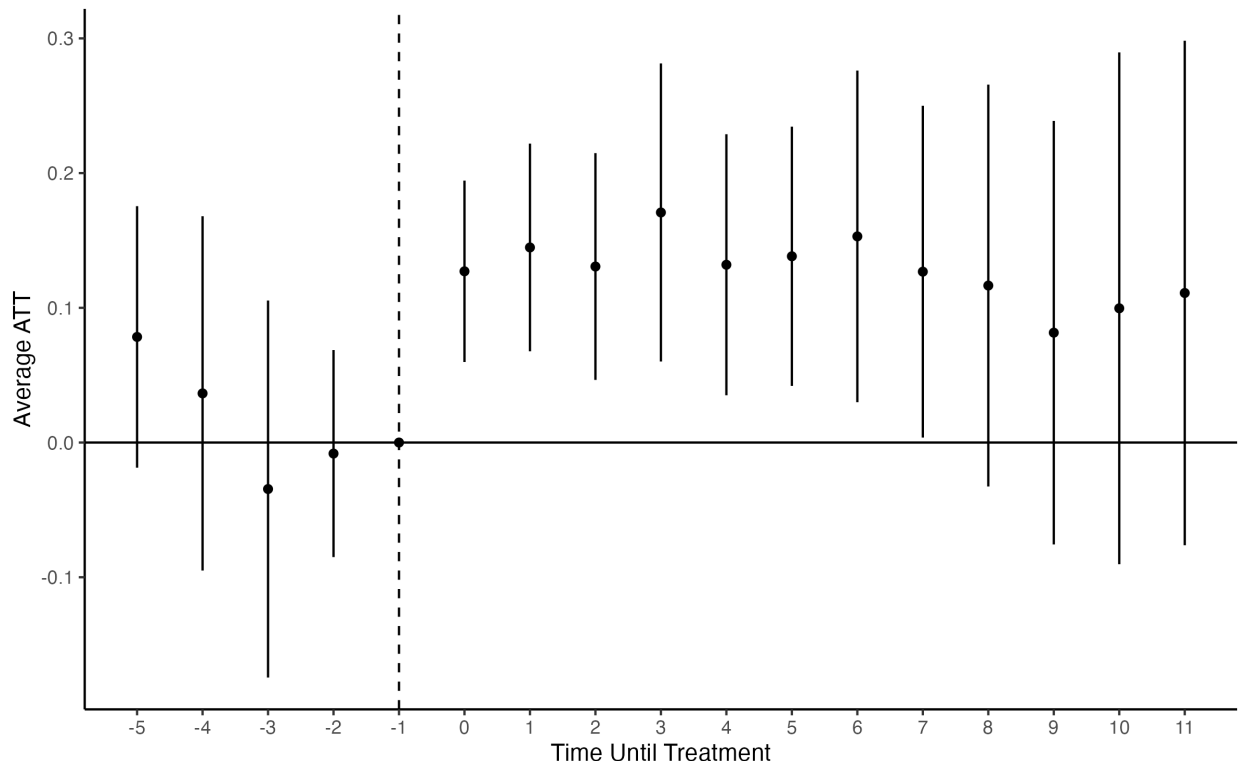
to approximately 30% of the aggregate increase in polarization observed among incumbent state legislators for the years 1992-2022.²³

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 the lead coefficients are small and insignificant.

Finally, in column four I include a state-specific linear time trend to absorb any unobserved 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

²³See Appendix Figure A.1 for details on aggregate legislative polarization.

Figure 3 – 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.

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 3 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 and no evidence of pre-trending.

So far, I have shown that term limits increase polarization across the pool of candidates

who run for office. Are these primary election compositional effects consequential for general election voters? In Appendix Table F.1, I examine the effect of term limits on polarization among general election candidates. I find nearly identical effects for general election candidates as I did for the aggregate pool of candidates. The similarity of these effects suggests that the polarization of the candidate pool is the key driver of term-limit-induced legislative polarization.

5.1.1 Robustness of Main Results

To establish the robustness of these results, 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 identified 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 heterogeneous treatment effects and staggered treatment adoption. The results of this exercise—reported in Appendix Figure F.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 F.2 replicates my results using Bonica (2014) CFscores to measure legislative polarization. CFscores—available for roughly 51% of all state legislative candidates between 1992-2022—use an unsupervised machine learning algorithm to predict candidate ideal points using campaign finance data. Second, to minimize concerns about strategic donations, Appendix Table F.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.

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 F.4 shows the inclusion of these controls does not alter my conclusions.

Finally, I evaluate three sources of effect heterogeneity that are suggested by the literature: heterogeneity by party, district composition, and legislative chamber. In the interest of space, I report and analyze the results in the appendix. In short, I find no evidence that the effects reported above are driven by asymmetric polarization among the Republican party, that term limits differentially polarize either upper or lower legislative chambers, or that term-limit-induced polarization is driven by the replacement of moderate Democrats with more-extreme Republicans in the South.

Table 5 – Differential Polarization of Term-Limited Legislatures. Term 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	0.20 (0.08)	0.21 (0.08)	0.11 (0.04)	0.09 (0.05)
Log(Leg Prof)		0.11 (0.08)		-0.17 (0.04)
Divided Government		-0.02 (0.02)		0.01 (0.01)
Party Competetiveness		-0.29 (0.23)		-0.60 (0.18)
N	725	725	610	610
Standard Deviation	.5	.5	.29	.29
Year FEs	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Years	1993-2018	1993-2018	1992-2022	1992-2022

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

5.2 Effect of Term Limits on Incumbent Legislators

As the most prominent evidence of legislative polarization, I now focus on the incumbent legislators that make up the end of the electoral pipeline. Table 5 presents my overall estimates of the effects of term limits on polarization among sitting legislators. 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 5 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, throughout this paper I have measured 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 at-

tenuated, results in columns three and four, underscoring the potentially conservative nature of my new measure of polarization and increasing the credibility of my Hall-Snyder scalings. In addition, the coefficient estimates of the effect of term limits on polarization among incumbents (Table 5 columns three and four) closely match estimates for the candidate pool (Table 4) and general election candidates (Table F.1), suggesting that term limits primarily polarize state legislatures by attracting a more-extreme pool of candidates. That is, as the pool of primary and general election candidates polarize, the set of sitting legislators mechanically becomes more extreme.

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 5 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 F.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 Fourinaies and Hall (2022) and suggest that state legislators “die with their ideological boots on” (Poole, 2007, p. 435).

Overall, this section has established that a decline in the legislative news environment combines with weakened mechanisms of electoral selection to produce meaningfully elevated polarization across the electoral pipeline. I have further shown that this effect is comparable between the candidate pool and incumbents, demonstrating that the key driver of term-limit-induced polarization is increasing extremity among the pool of legislative office-seekers.

6 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 changes in the legislative candidate pool and declines in voter knowledge drive legislative polarization.

Why do term limits polarize state legislatures? My analysis establishes a candidate-supply-centric pathway through which term limits polarize state legislatures. I find that, in line with research on the incumbent media advantage, newspaper coverage of legislative elections declines following the implementation of term limits. Newspaper coverage of legislative elections declines by between 28% and 32% on average post-term limits. I further demonstrate that these shocks to the legislative news environment translate into weakened voter knowledge about legislative politics. 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.

As voter knowledge about legislative politics declines, I find that the electoral return to ideological moderation in general elections is significantly reduced in term-limited states, reducing candidates' cost to extremity and signaling voters' inability to penalize extremist candidates. The result is a more-extreme candidate pool. I demonstrate that term limits increase polarization across the pool of candidates who run for legislative office and that these results are robust to a variety of scaling and estimation techniques. The culmination of this process is a mechanically more extreme set of general election winners and sitting legislators.

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. Weakened political news environments and the dwindling supply of moderate candidates are not particular to term limits or state legislatures. My results underscore the importance of attracting an ideologically representative sample of legislative candidates and strengthening the legislative news environment.

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Appendix

Contents

A	Summary Statistics and Data Descriptions	33
	A.1 Key Variable Summaries	33
	A.2 Aggregate Legislative Polarization	34
B	Scaling Candidates Using Hall-Snyder Scores	35
C	Robustness Checks for Legislative News Environment	37
	C.1 State Legislative News Coverage Collection Procedures	37
	C.2 Legislator News Coverage Over Career	39
D	Robustness Checks for Voter Knowledge	40
	D.1 CES Correct Response Rates Stratified by Chamber and Year	40
E	Robustness Checks for Electoral Selection	41
	E.1 Marginal Effects Plots	41
	E.2 Electoral Selection Using CFscores	42
F	Robustness Checks for Polarizing Effects of Term Limits	43
	F.1 Heterogenous Treatment Effect Robust Estimate of the Effect of Term Limits on Candidate Pool Polarization.	43
	F.2 Effect of Term Limits on General Election Candidate Polarization	44
	F.3 Difference-in-Differences Estimates Using CFscores	45
	F.4 Difference-in-Differences Estimates Using Alternative Hall-Snyder Scores	45
	F.5 Alternate Control Specifications	46
	F.6 Incumbents' Ideological Response to Term Limits	47
G	Heterogeneity Analyses	48
	G.1 Polarization Simulation	51

A Summary Statistics and Data Descriptions

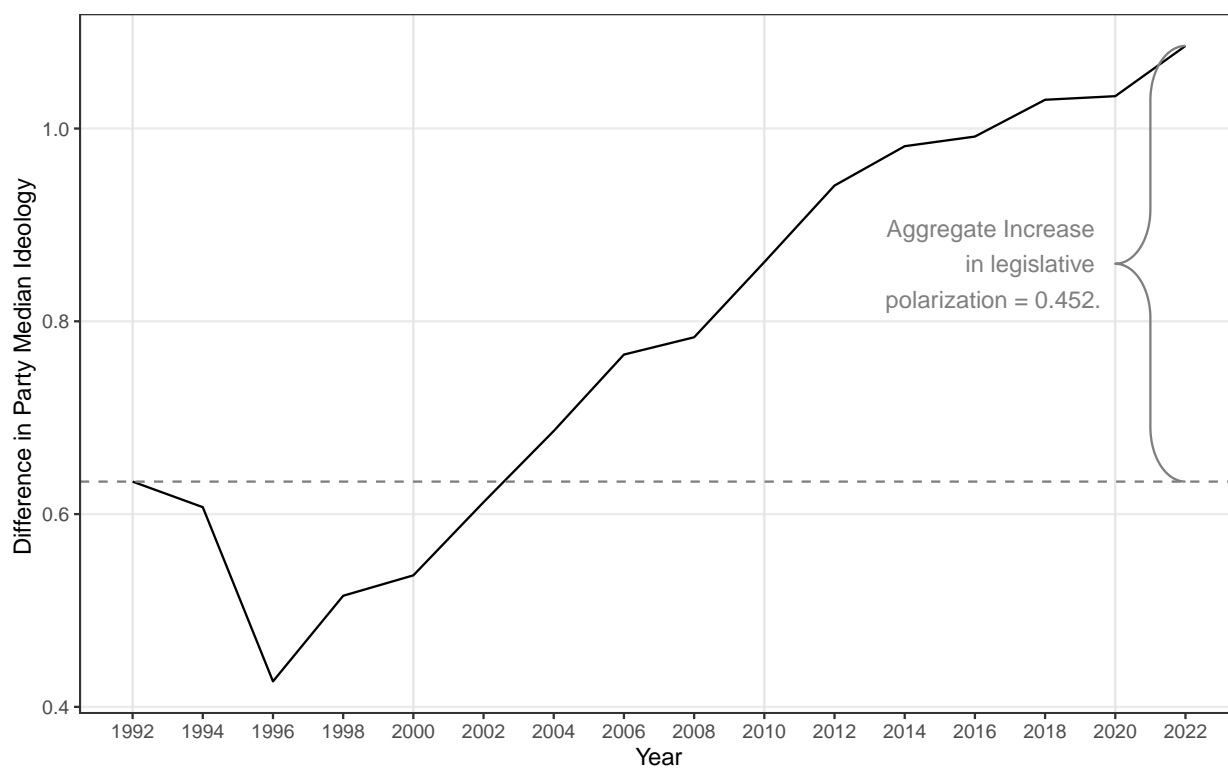
A.1 Key Variable Summaries

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	0.58
Divided Gov.	0.46	0	0	1	0.5
Party Competition	0.15	0.12	0	0.44	0.1
Polarization (pipeline)	0.54	0.46	-0.094	1.6	0.31
Polarization (general election)	0.54	0.46	-0.092	1.6	0.31
Polarization (incumbents)	0.51	0.45	-0.092	1.6	0.29
Dem. Vote Share	0.52	0.5	0	1	0.34
Dem. Win	0.5	1	0	1	0.5

A.2 Aggregate Legislative Polarization

Figure A.1 – Aggregate Legislative Polarization, Incumbents 1992-2022 (Hall-Snyder Scores). This figure plots the level of legislative polarization across all 98 chambers included in this analysis. Polarization is measured as the difference in party median ideologies. Legislative polarization has increased from .63 to 1.08 between the years 1992 and 2022.



B Scaling Candidates Using Hall-Snyder Scores

Hall-Snyder Scalings require 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 27,629 incumbent state legislators between 1993 and 2020.²⁴ 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) via state-specific OLS projections. Second, I draw on campaign contributions to state legislative candidates from the National Institute on Money in Politics. These data consist of over 50 million transactions between 1989 and 2022.

Estimation of candidate ideology proceeds in two stages.²⁵ First, I estimate the ideology of all state legislative donors as the average contribution-weighted ideology of the incumbents to which a donor contributes.²⁶ 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.²⁷ Subsequently, I estimate each candidate’s ideology as

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

²⁴The most-recent version of this data was downloaded from <https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/NWSYOS>. See Shor and McCarty (2022) for an overview of these updated scalings.

²⁵I adopt the notation of Hall and Snyder (2015) in this section.

²⁶I include individual and interest group donors in this process.

²⁷This 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., $Donor\ Ideology_j = \frac{\sum_i NPScore_i Contribution_{ij}}{\sum_i Contribution_{ij}}$).

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.²⁸ To reduce noise in these estimates, I exclude donors who donated to fewer than ten distinct candidates and candidates who received donations from fewer than ten distinct donors.²⁹

²⁸To allay concerns about strategic donations, in Appendix Table F.3 I replicate my main analyses using scalings that exclude donations to candidates after they become incumbents. My substantive conclusions remain unchanged.

²⁹The inclusion of these restrictions do not alter my substantive conclusions.

C Robustness Checks for Legislative News Environment

C.1 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 1992-2022, all of which are digitized. Table C.1 reports the number of newspapers and newsprint pages that enter this corpus by state.

Using these data, I construct a measure of individual legislators’ newspaper coverage in election years, following the text-as-data approach of Hopkins (2018). To do so, I search the news corpus for references to sitting state legislators in every state’s legislative election year between 1992 and 2022.³⁰ I define references as any article that mentions a legislator’s combined first and last name (e.g., “FIRST LAST”) or the legislator’s last name with an appropriate prefix (e.g., “state senator LAST”). To reduce the risk of false positive results, I restrict this search to newspapers within the candidate’s home state. Finally, I aggregate counts of these references to the legislator or legislator-newspaper level. The result is a measure of how often individual newspapers reference state legislators.³¹

³⁰While 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.

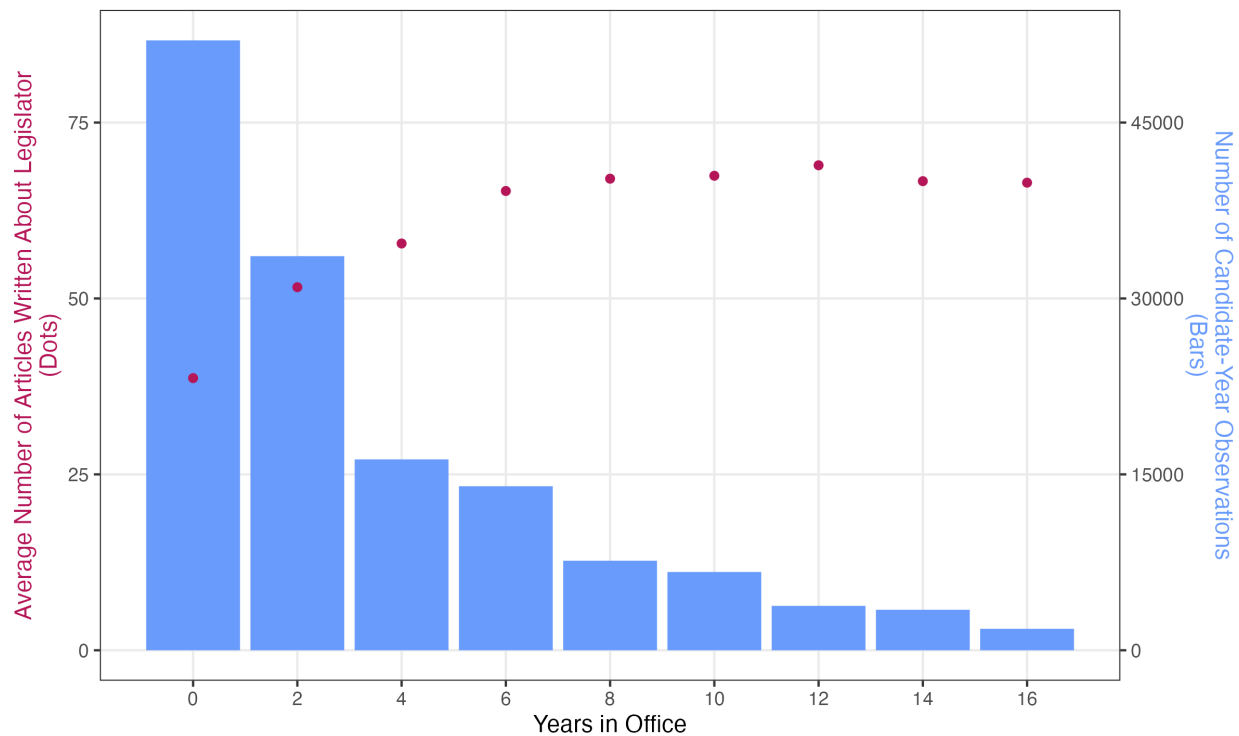
³¹Recent work draws on machine learning methods to classify the content of news articles (e.g., Cagé, Hervé, and Viaud, 2020; Turkel et al., 2021), enabling researchers to evaluate the quality in addition to quantity of new coverage. These methods, while beyond the scope of this article, could prove fruitful in future analyses of legislative news coverage.

Table C.1 – Number of Newspapers and Pages in Corpus by State, 1992-2022.
This table reports the number of newspapers and total newspaper pages included in the news corpus by state.

State	Number of:		State	Number of:	
	Newspapers	Pages		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			

C.2 Legislator News Coverage Over Career

Figure C.1 – Number of Articles Written About Legislators By Tenure in Office. The average number of newspaper articles written about sitting legislators (pink dots) is strongly increasing in legislator tenure. Tenure-related newspaper coverage gains are largest in legislators first 10 years in office, which constitutes the vast majority of observations that enter my analysis (see blue bars).

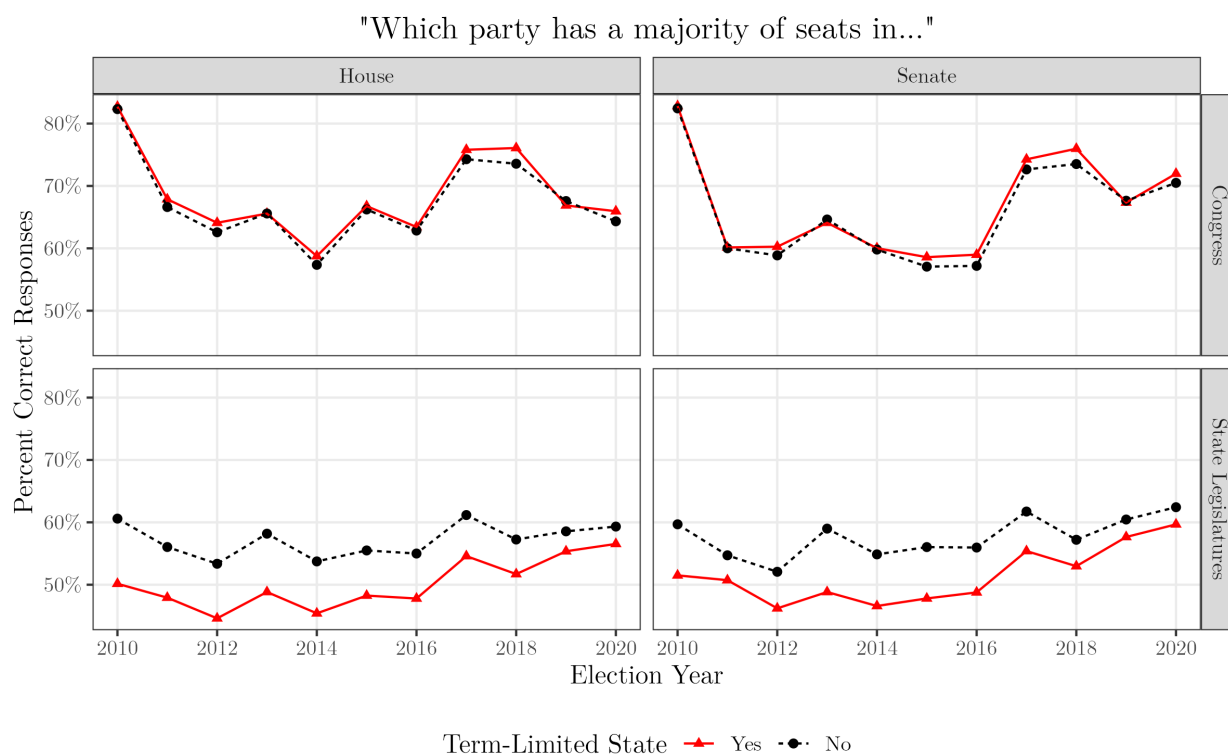


Note: Figure omits tenures greater than 16 years for clarity of presentation.

D Robustness Checks for Voter Knowledge

D.1 CES Correct Response Rates Stratified by Chamber and Year

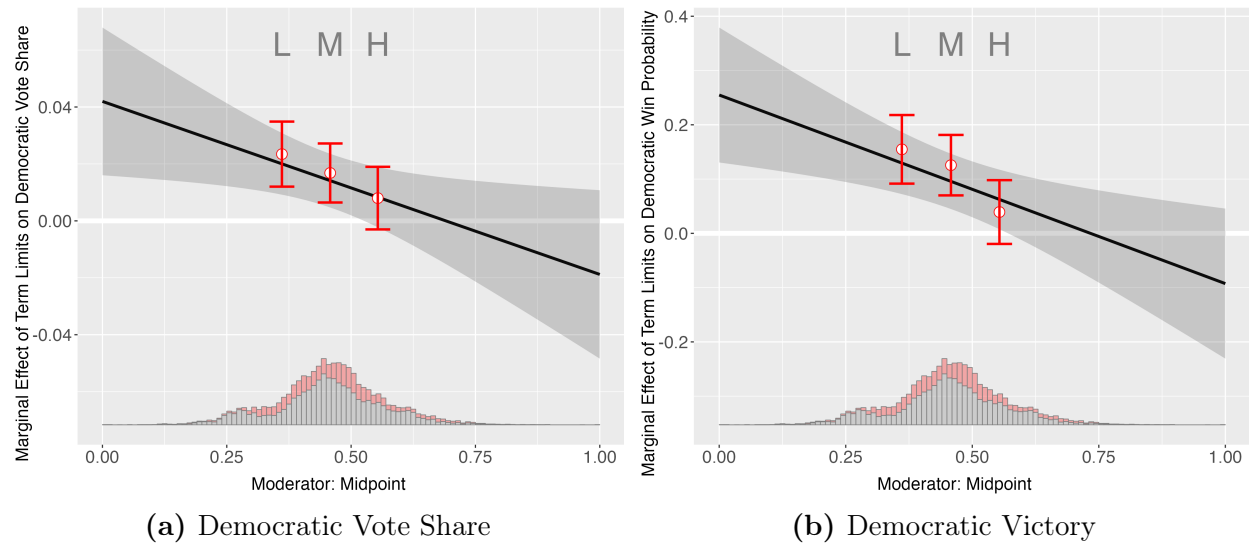
Figure D.1 – Percent Correct Responses to CES Voter Knowledge Questions. This figure depicts average response accuracy rates to four CES questions about party control of Congress and respondents' home state legislatures. Respondents in term-limited states (whose knowledge about federal elections matches their non-term-limited counterparts) exhibit reduced political knowledge about their home state legislatures.



E Robustness Checks for Electoral Selection

E.1 Marginal Effects Plots

Figure E.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 3 columns 3 and 7.



E.2 Electoral Selection Using CFscores

Table E.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.

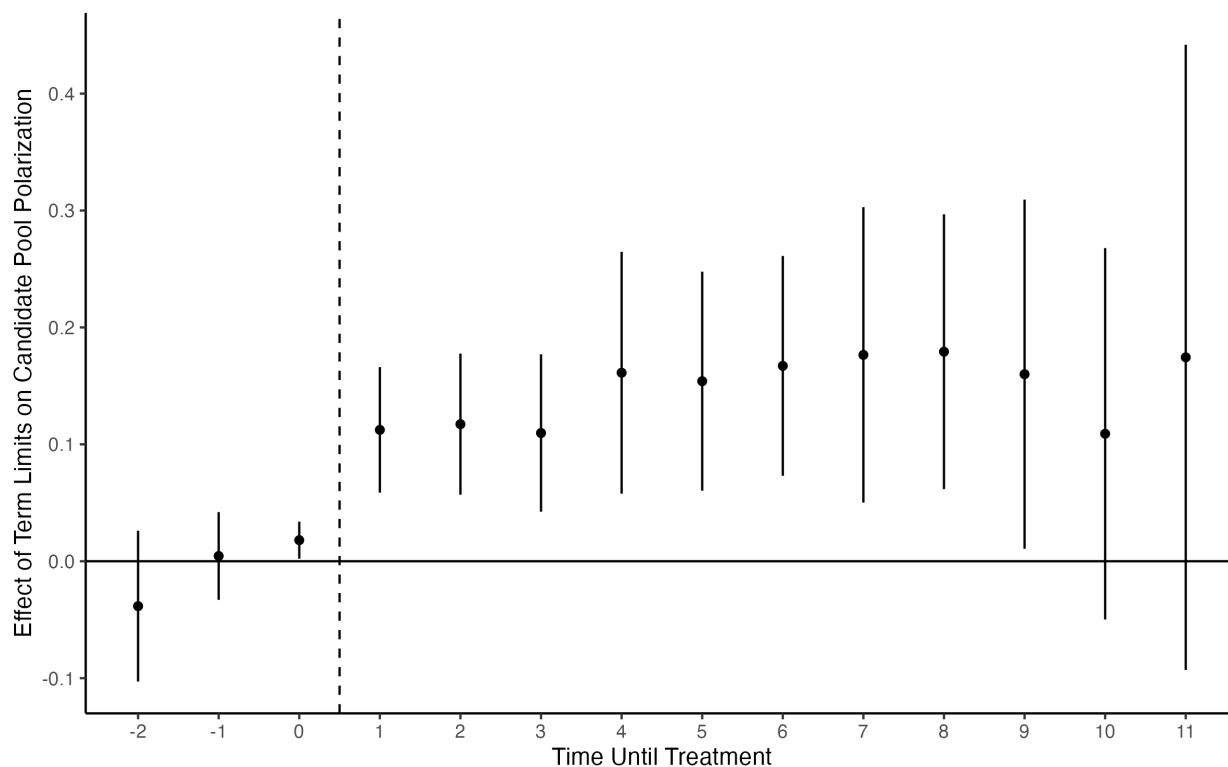
	Dem Vote Share				Dem Win			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Midpoint	0.298 (0.019)	0.564 (0.023)	0.337 (0.021)	0.337 (0.021)	1.238 (0.102)	2.209 (0.119)	1.401 (0.116)	1.401 (0.116)
Term Limits		0.048 (0.026)	0.092 (0.022)	0.093 (0.023)		0.247 (0.131)	0.411 (0.117)	0.402 (0.123)
Midpoint · Term Limits		-0.056 (0.044)	-0.129 (0.038)	-0.129 (0.038)		-0.258 (0.217)	-0.532 (0.196)	-0.533 (0.196)
Distance · Term Limits				-0.003 (0.026)				0.024 (0.124)
Distance	0.023 (0.014)	0.017 (0.016)	0.022 (0.014)	0.024 (0.016)	-0.058 (0.065)	-0.089 (0.070)	-0.064 (0.065)	-0.074 (0.081)
Dem Contributions	0.029 (0.001)		0.029 (0.001)	0.029 (0.001)	0.102 (0.005)		0.103 (0.005)	0.103 (0.005)
Rep Contributions	-0.027 (0.001)		-0.027 (0.001)	-0.027 (0.001)	-0.105 (0.006)		-0.105 (0.006)	-0.105 (0.006)
N	9,711	9,711	9,711	9,711	9,711	9,711	9,711	9,711
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.

F Robustness Checks for Polarizing Effects of Term Limits

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

Figure F.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.



F.2 Effect of Term Limits on General Election Candidate Polarization

Table F.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	0.13 (0.05)	0.11 (0.06)	0.13 (0.04)	0.13 (0.07)
Term Limited, $t + 1$			0.04 (0.04)	
Term Limited, $t + 2$			-0.06 (0.06)	
Log(Leg Prof)		-0.21 (0.05)	-0.14 (0.05)	-0.09 (0.04)
Divided Government		0.01 (0.01)	0.01 (0.01)	0.00 (0.01)
Party Competetiveness		-0.56 (0.18)	-0.42 (0.14)	-0.19 (0.10)
N	611	611	479	611
Outcome Standard Deviation	.31	.31	.31	.31
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.

F.3 Difference-in-Differences Estimates Using CFscores

Table F.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 5, 4, and F.1)

	Candidate Pool Polarization	General Election Polarization	Incumbent Polarization
	(1)	(2)	(3)
Term Limits	0.12 (0.06)	0.11 (0.06)	0.09 (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.

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

Table F.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	0.06 (0.07)	0.06 (0.07)	0.10 (0.08)
N	586	585	582
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.

F.5 Alternate Control Specifications

Table F.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	0.15 (0.04)	0.16 (0.04)	0.11 (0.03)
Log(Leg Prof)	-0.15 (0.05)	-0.14 (0.04)	-0.12 (0.03)
Divided Government	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Party Competitiveness	-0.43 (0.13)	-0.43 (0.13)	-0.44 (0.12)
Log(Population)	-0.01 (0.15)	-0.01 (0.15)	0.14 (0.14)
Unemployment Rate	0.00 (0.00)	0.00 (0.01)	-0.00 (0.00)
Per Capita Income	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Gini	-0.78 (0.35)	-0.79 (0.34)	-0.53 (0.31)
Democratic Governor	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Percent Population Foreign Born	-0.02 (0.03)	-0.02 (0.03)	-0.02 (0.03)
N	483	483	483
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.

F.6 Incumbents' Ideological Response to Term Limits

Table F.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	-0.0063 (0.0063)	-0.0034 (0.0058)
N	58,457	58,376
Standard Deviation	.33	.33
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.

G Heterogeneity Analyses

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.

Table G.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	0.04 (0.03)	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.02 (0.03)	0.05 (0.03)
Log(Leg Prof)	-0.07 (0.06)	-0.01 (0.04)	-0.07 (0.06)	-0.01 (0.04)	-0.07 (0.04)	-0.02 (0.04)
Divided Government	0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	0.01 (0.01)	-0.00 (0.01)	0.01 (0.01)
Party Competetiveness	-0.18 (0.13)	0.01 (0.08)	-0.19 (0.13)	0.03 (0.08)	-0.25 (0.13)	0.03 (0.09)
N	616	614	616	614	615	614
Standard Deviation	.3	.2	.3	.19	.3	.2
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.

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 G.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.

Table G.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	0.05 (0.03)	0.06 (0.03)	0.05 (0.04)
Safe for Dems	-0.01 (0.03)	-0.01 (0.03)	0.00 (0.03)
Safe for Reps	-0.02 (0.03)	-0.02 (0.03)	-0.01 (0.03)
Term Limited · Safe for Dems	-0.06 (0.08)	-0.08 (0.08)	-0.08 (0.09)
Term Limited · Safe for Reps	0.03 (0.05)	0.03 (0.05)	0.04 (0.05)
N	77,278	71,974	64,502
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.

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.”³² In Table G.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 5.1. 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.

Table G.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 (0.06)	0.12 (0.04)	0.10 (0.06)	0.14 (0.04)	0.09 (0.05)	0.10 (0.04)
Log(Leg Prof)	-0.20 (0.04)	-0.23 (0.08)	-0.20 (0.05)	-0.23 (0.08)	-0.16 (0.04)	-0.16 (0.05)
Divided Government	-0.00 (0.01)	0.02 (0.02)	-0.01 (0.02)	0.02 (0.02)	-0.00 (0.01)	0.02 (0.01)
Party Competetiveness	-0.58 (0.21)	-0.47 (0.20)	-0.57 (0.21)	-0.52 (0.19)	-0.64 (0.19)	-0.59 (0.19)
N	594	550	593	550	590	541
Standard Deviation	.31	.32	.31	.32	.3	.3
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 subsetting by legislative chamber. Robust standard errors are clustered by state in parentheses.

Finally, term limits’ effect may vary by legislative chamber. In Table G.3 I estimate

³²A 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.”

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.

G.1 Polarization Simulation

Figure G.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.

