

# OUT OF OFFICE, OUT OF STEP? ELECTORAL INCENTIVES AND STRATEGIC MODERATION IN THE U.S. HOUSE OF REPRESENTATIVES<sup>†</sup>

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## Abstract

This paper proposes a novel identification strategy to separate electoral incentives from selection effects on incumbent politicians' policy choices. Exploiting the unique institutional setup of lame-duck sessions in the U.S. Congress, where lame-duck incumbents who lost reelection vote on the same issues as their reelected colleagues, I use a close election regression discontinuity design to leverage quasi-random assignment of reelection-seeking representatives to lame-duck status, which is orthogonal to voter preferences and incumbents' type. Comparing within-incumbent changes in roll call voting of barely unseated lame ducks to narrowly reelected co-partisans serving the same congressional term, I find that lame ducks revert to more extreme positions with lame-duck Democrats (Republicans) voting more liberally (conservatively). Consistent with lame ducks' loss of re-election incentives driving the result, the effect of lame-duck status on roll call extremism is more pronounced among electorally vulnerable incumbents, representatives of districts with a large proportion of swing voters, and retirement-aged lame ducks who are less likely to rerun in the future. I also consider but ultimately dismiss several other mechanisms including emotional backlash, logrolling motives, party control, and selective abstention. Although quantitatively dominated by selection effects, electoral incentives to moderate positions are large enough to reduce polarization by 21–26% and to flip high-stakes legislative outcomes under plausible counterfactual scenarios motivated by ongoing debates over U.S. constitutional design.

**Keywords:** Electoral Incentives, Accountability, Polarization, Legislator Behavior, RDD

**JEL Classification Codes:** D72, P0, J45

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*As it is essential to liberty that the government in general should have a common interest with the people, so it is particularly essential that the branch of it under consideration should have an immediate dependence on, and an intimate sympathy with, the people. Frequent elections are unquestionably the only policy by which this dependence and sympathy can be effectually secured.*

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— James [Madison](#) ([1788] 2009), Federalist No. 52

*I'm just telling the truth now. I don't have to run for office again, so I can just, you know, let her rip.*

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— Barack [Obama](#) (2014), Speech in Austin, Texas

## 1 Introduction

As the founders of the U.S. Constitution noted, there are two fundamentally different roles of elections in ensuring that politicians act on behalf of the people. One is the *selection* of high-quality types of politicians “who possess most wisdom to discern, and most virtue to pursue, the common good of the society”, the other lies in “keeping them virtuous whilst they continue to hold their public trust” ([Madison](#), [1788] 2009, Federalist No. 57). On the one hand, regular elections sort out politicians who are ex-ante incongruent with voters’ interests. On the other hand, once elected, re-election concerns provide officeholders with *incentives* to maintain ex-post responsiveness to voters’ interests. The notion that elections discipline officeholders is a core principle of representative democracy. Yet, whether electoral incentives effectively constrain politicians’ policy choices is a theoretically ambiguous and empirically unsettled question.

Reelection constraints feature prominently in the theoretical literature on political agency problems starting with [Barro](#) (1973) and [Ferejohn](#) (1986).<sup>1</sup> The absence of high-powered wage incentives in the public sector makes politicians’ career concerns the most important incentive scheme to ensure accountability of elected officials ([Tirole](#), 1994; [Diermeier et al.](#), 2005). Electoral incentives created by the threat of being thrown out of office can motivate politicians to represent the interest of voters, which by a Downsian logic would lead to more moderate policies that please the median voter ([Downs](#), 1957). However, the effectiveness of electoral incentives in disciplining incumbents’ policy choices hinges on politicians’ ability to credibly commit to policy platforms that differ from their personal preferences ([Alesina](#), 1988; [Osborne and Slivinski](#), 1996; [Besley and Coate](#), 1997), and voters’ ability to recognize and punish politicians for policies that do not align

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<sup>1</sup>See [Duggan and Martinelli](#) (2017) for a review of the theoretical political agency literature.

with their interests (e.g., [Miller and Stokes, 1963](#)).<sup>2</sup>

While structural estimates of political agency models find large effects of reelection concerns ([Sieg and Yoon, 2017](#); [Aruoba et al., 2019](#); [Iaryczower et al., 2022](#)), we lack credibly identified quasi-experimental evidence that supports these results. The standard strategy to examine the effect of electoral incentives on incumbent behavior has been to compare reelection-seeking politicians with lame-duck incumbents who serve in their final term and are, therefore, free from reelection concerns. However, separating electoral incentives from non-random selection into lame-duck status is challenging. Observational studies of U.S. Congress members typically find that legislators' roll call voting does not change after they announce retirement (see [Lott and Davis \(1992\)](#) for a review of the "congressional shirking" literature). These non-findings are consistent with other accounts of representatives' ideological rigidity ([Lee et al., 2004](#); [Poole, 2007](#)), but also with downward biased estimates due to anticipation effects as incumbents likely decide on retirement before announcing it. Although self-selection into lame-duck status is most evident when (perhaps strategic) retirement is under perfect control of the politician, selection effects also hamper the identification of incentive effects in studies exploiting constitutional term limits as an "exogenous" determinant of lame-duck status (e.g., [Besley and Case, 1995](#); [List and Sturm, 2006](#); [Alt et al., 2011](#); [Ferraz and Finan, 2011](#)). If voters use elections to *select* politicians, lame-duck incumbents who survived enough reelection bids to hit the term limit are not just more experienced but also better types with preferences ex-ante aligned with voters'. Improving on cross-incumbent comparisons, [Fouirnaies and Hall \(2022\)](#) employ a difference-in-differences strategy that keeps individual politicians' unobservable type fixed by comparing within-incumbent changes of termed-out U.S. state legislators to counterfactual changes among reelection-eligible members serving in the same chamber. While showing that termed-out lame ducks exert less legislative effort (floor attendance, bill sponsorship, committee service), [Fouirnaies and Hall \(2022\)](#) find a precisely estimated null effect of electoral incentives on roll call voting, which is identified as a causal parameter on a selected sample of legislators who were reelected sufficiently many times to hit the term limit.<sup>3</sup>

This paper proposes a novel empirical strategy that directly addresses the non-random selection of incumbent politicians into lame-duck status. Specifically, I consider contemporary lame-duck sessions in the U.S. House of Representatives. Congressional lame-duck sessions occur in the two

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<sup>2</sup>Consistent with the Downsian notion of rational ignorance ([Downs, 1957](#)), survey research documents that voters in the United States are fairly uninformed about their representatives' policy actions and congressional politics more generally, and that they would vote differently if they were more informed ([Bartels, 1996](#); [Delli Carpini and Keeter, 1996](#); [Fowler and Margolis, 2014](#); [Ansolabehere and Kuriwaki, 2022](#)). Electoral incentives to take moderate positions may also break down if candidates can target information to their core constituency ([Glaeser et al., 2005](#)), or if extremist voters with more intense preferences invest more in costly information acquisition than rationally inattentive moderates ([Matějka and Tabellini, 2021](#)). Even if all voters are informed about politicians' policy choices, they could be unable to commit to effective punishment mechanisms if facing a trade-off between selection and control (e.g., [Banks and Sundaram, 1993](#); [Fearon, 1999](#); [Daley and Snowberg, 2011](#)).

<sup>3</sup>[Aidt and Shvets \(2012\)](#) use a similar design to estimate last-term effects on U.S. state legislators' pork barrel spending. [Lopes da Fonseca \(2020\)](#) also uses within-mayor variation to study the effect of term limits on local public finance in Portugal.

months between the general elections in November and the January inauguration when newly elected members are seated. During this transition period, the House convenes in its old composition, including lame-duck officials who retire from office or lost their reelection bids. As lame ducks retain all their powers during this period, this unique institutional setup allows observing both reelected incumbents and lame ducks, who are stripped of reelection concerns, voting on the same issues.

To identify the impact of electoral incentives on incumbents' roll call voting position net of selection effects, I employ a regression-discontinuity strategy to exploit as good as random assignment of reelection-seeking incumbents to lame-duck status by close elections. In practice, I compare the within-incumbent change in W-NOMINATE scores from regular to lame-duck sessions of representatives who narrowly lost their reelection bid to barely reelected members of the *same party* serving in the *same congressional term*. Focusing on within-incumbent changes, the difference-in-discontinuity design flexibly controls for incumbents' type, while quasi-random assignment by toss-up elections prevents self-selection of incumbents into lame-duck status. Restricting attention to co-partisans in the same term (by conditioning on a full set of *party*  $\times$  *congress* fixed effects) ensures the comparability of W-NOMINATE scores between lame ducks and bare election winners, and rules out differences in roll-call voting resulting from majority status and agenda control. The key identification assumption underlying the *difference-in-discontinuity* design is that bare election winners and narrow losers follow parallel trends (Grembi et al., 2016). In addition to providing evidence that bare election winners and narrow losers are similar in pre-determined characteristics, I validate this assumption by showing the absence of pre-trends at the cutoff in the year leading up to the general elections.

I find substantial effects of lame-duck status on legislators' roll-call voting records. Consistent with electoral incentives constraining incumbents to compromise toward more moderate policy positions, narrowly ousted incumbents shift to more extreme positions after elections. This effect is driven by both lame-duck Democrats voting more liberally and lame-duck Republicans taking more conservative positions than their co-partisans reelected to the next Congress. Estimated effects of lame-duck status on incumbents' roll call voting imply a shift toward more extreme positions by 0.1–0.12 units on the W-NOMINATE scale, which ranges from -1 (very liberal) to +1 (very conservative). The effect magnitude thus equals 5% of the distance between the most conservative Republican and the most liberal Democrat in my sample. Evaluated in terms of pre-election W-NOMINATE scores of incumbents in the 116<sup>th</sup> Congress, a shift by 0.1 points corresponds approximately to one-half the within-party interquartile range or to the average distance between representatives and their own party's median.

Next, I provide three pieces of evidence showing that lame ducks' loss of electoral incentives to moderate is the driving mechanism behind their reversion to more extreme voting positions. First, effects are larger among electorally more vulnerable incumbents whose electoral constraints were

*ex-ante* more binding, whereas relatively safe incumbents with a high *predicted* margin of victory do not react to electoral loss. Second, the effect of lame-duck status is stronger on incumbents who sought reelection in swing districts with a high proportion of independent voters that could be swayed by strategic policy moderation. Yet, there is no significant positional shift by lame ducks representing highly partisan districts where most voters identify as Democrats or Republicans.<sup>4</sup> Third, I estimate the largest effects on roll call voting positions of retirement-aged lame ducks who are also the least likely to rerun for elected office in the future. By contrast, the effects of lame-duck status on both roll call extremism and the propensity to rerun are smaller for early- and mid-career politicians who may still capitalize on more moderate positions to get reelected in the upcoming electoral cycle.

I also evaluate, but ultimately dismiss, several alternative mechanisms including emotional backlash, logrolling motives, party control, and selective abstention. First, lame ducks could be aggrieved due to electoral defeat and take more extreme positions in defiance of voters who did not re-elect them. The theoretical and empirical literature on emotional cues predicts that emotional backlash is caused by *unexpected* loss (Hart and Moore, 2008; Fehr et al., 2011; Card and Dahl, 2011; Eren and Mocan, 2018). However, lame-duck status has a larger impact on incumbents with a smaller *predicted* margin of victory, i.e., on those who lost *expectedly*. Second, policy-seeking legislators who trade votes across party lines for bipartisan support of their own bills could lose their logrolling motives once ousted from office. Inconsistent with this channel, I do not find any differential effect of lame-duck status on roll call voting of legislatively more or less active incumbents. Third, rather than losing accountability to voters, lame ducks may be less reliant on party leadership and vote more extremely because the latter loses control over departing members. Yet, I show that lame-duck status does not affect incumbents' loyalty to party leadership, as measured by the fraction of votes cast in line with their party's whip. Last, I consider the possibility that the removal of re-election concerns causes lame ducks' roll call extremism indirectly by reducing incentives to exert effort and participate in floor votes. If lame ducks only attend roll call votes on issues they care about and preference intensity is correlated with extremism, a more extreme roll call voting record could be the byproduct of selective abstention rather than the direct consequence of removing incentives to moderate strategically. To explore this channel, I conduct a mediation analysis. I first document that removing electoral incentives indeed causes an increase in lame ducks' absenteeism by 4.5 percentage points. However, conditioning on the change in incumbents' abstention rate does not affect the estimated effect of lame-duck status on roll call extremism, suggesting that my results reflect a genuine change in position-taking rather than a side effect of reduced effort.

Additional heterogeneity analyses show that electoral incentives' moderating effect on roll call

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<sup>4</sup>Importantly, incumbents' predicted margin of victory and districts' share of independent voters are uncorrelated in the relevant subsample of close elections decided by vote share margins of less than 5%, indicating that the measures for incumbent vulnerability and swing districts proxy for conceptually different underlyings.

voting is remarkably stable over time and across very different political constellations. Effects of similar size are present during the first (2009-2013) and the second Obama Administration (2013-2017) as well as the otherwise norm-breaking Presidency of Donald Trump (2017-2021). Effects are not specific to the current, prospective, or post-electoral changes in the majority status of House members' parties, and extend to lame ducks away from the cutoff who lose by vote share margins larger than zero. Further supporting the external validity of my findings, House roll calls held in post-electoral lame-duck sessions highly resemble proposals voted upon in pre-electoral regular sessions regarding policy areas, several measures of bills' importance, and ex-post closeness determining pivotality of individual legislators' votes.

I close the paper by quantifying the impact of electoral incentives on substantive representation and legislative outcomes. To assess the relevance of electoral incentives relative to selection effects for substantive representation at the district level, I use a regression discontinuity design that holds district characteristics and voter preferences fixed and estimate selection effects as the distance in W-NOMINATE scores between narrowly reelected Republican (or Democratic) representatives and successful challengers from the opposing party who narrowly won the election and replace the former incumbent. Selection effects exceed electoral incentive effects by almost one order of magnitude. The distance between two members of different parties representing otherwise comparable districts is 7.8 to 9.3 times larger than lame ducks' within-incumbent shift by 0.1 to 0.12 points on the W-NOMINATE scale. However, incumbents' responsiveness to electoral incentives remains a powerful determinant of representation. Without electoral competition incentivizing incumbents of both parties to adopt more moderate positions, the distance between them would increase by 0.2 to 0.24 points, i.e., electoral incentives reduce polarization by 21 to 26%. To examine electoral incentives' potential to shape public policy at the aggregate level, I consider legislative outcomes under plausible counterfactual scenarios motivated by ongoing debates on U.S. constitutional design. First, I simulate a scenario in which Congressional lame-duck sessions were abolished by postponing Election Day closer to the date when newly elected members get seated, such that reelection-concerned legislators would still be incentivized to moderate their positions. Second, I consider the case of a modest redistricting reform that would increase political competition for 16% of the incumbents currently representing safe districts, making them electorally vulnerable. Under these plausible counterfactual scenarios, several high-stakes legislative outcomes – on topics ranging from infrastructure, military and veteran expenditures, immigration, agriculture, and health care – would have been flipped. I conclude that electoral incentives are large enough to shape legislation and, hence, public policy.

This paper contributes to a large political economy literature investigating on how electoral incentives constrain incumbent politicians' behavior. Correlational evidence that legislators with a more extreme roll call voting record are more likely to lose re-election suggests that there are rewards from policy moderation ([Ansolabehere et al., 2001](#); [Canes-Wrone et al., 2002](#); [Ansolabehere](#)



and Jones, 2010; Carson et al., 2010; Ansolabehere and Kuriwaki, 2022; see also Hall, 2015). Yet, these results are perfectly consistent with a pure selection mechanism, as the existence of electoral incentives to moderate does not imply that incumbents respond to them. Many empirical studies have compared in-office behavior and policy outcomes implemented by elected and appointed officials (e.g., Besley and Coate, 2003; Lim, 2013; Hessami, 2018), or by officials elected under different rules (e.g., Gagliarducci et al., 2011; Funk and Gathmann, 2013; Bordignon et al., 2016). Since different electoral norms go along with a varied pool of candidates and the selection of different politicians serving under distinct mandates, these studies hardly isolate electoral incentive effects. Building on an extensive body of work that uses term limits to study the last-term behavior of executive politicians (Besley and Case, 1995; List and Sturm, 2006; Alt et al., 2011; Ferraz and Finan, 2011; Lopes da Fonseca, 2020), I not only improve on the identification by cross-person comparisons that confound electoral incentives with selection effects,<sup>5</sup> but also go beyond the study of last-term effects by providing evidence on the mechanism linking lame-duck status with electoral incentives to moderate policy positions. By ruling out first-order competing channels, my results can be interpreted as reduced-form estimates of a model in which lame-duck status primarily affects legislators' voting behavior through the loss of reelection concerns and the removal of electoral incentives to moderate strategically.

Providing the first causal evidence of a significant effect of electoral incentives on legislators' voting, my results lend credibly identified reduced-form support to structural estimates of accountability effects (Iaryczower et al., 2022), but present a striking contrast to observational studies in the congressional setting,<sup>6</sup> and apparently contradicts recent quasi-experimental null findings from the state-legislative context (Fouirnaies and Hall, 2022). Of course, divergent quasi-experimental results do not exclude each other's validity. Yet, the contrast with closely related work estimating plausibly causal null effects of electoral incentives deserves clarification. One possible explanation for the contradicting results is that Fouirnaies and Hall (2022) focus on a lower-salience state-legislative environment where the lack of electoral competition and media coverage typically hampers voters' ability to hold individual legislators to account for extreme

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<sup>5</sup>Alt et al. (2011) address the selection problem exploiting U.S. states that switch from a one-term limit to a two-term limit, comparing termed-out governors in their first term to non-termed-out governors of the same state who also serve their first term in later years. As they note, if the introduction of longer-term limits affects the pool of candidates or reflects a change in voters' overall confidence in government, their estimates of electoral incentives on incumbent performance may be biased. Ferraz and Finan (2011) compare the corruption of Brazilian mayors serving in their second and last term to mayors in their first term who are predicted to win re-election for a second term in the subsequent electoral cycle, and are therefore of similar ability. Depending on the comparability of the types elected across the two cycles, this approach may over- or underestimate the effect of electoral incentives.

<sup>6</sup>The voluminous "congressional shirking" literature that finds no association between retirement decisions and roll-call voting behavior in the U.S. Congress includes but is not limited to Lott (1987), Poole and Romer (1993), Bronars and Lott Jr (1997), Stratmann (2000), Rothenberg and Sanders (2000), see also Lott and Davis (1992) for a review of the literature. An exception is Snyder and Ting (2003) who report that retiring House members take more extreme positions when representing marginal districts, whereby they acknowledge that representatives' retirement decisions could be endogenous. For example, members may retire strategically when expecting electoral defeat, perhaps because of their changing voting record.

roll call voting (Rogers, 2017). By contrast, I focus on highly salient and competitive congressional elections. Another possible explanation of null results for termed-out state legislators could be due to a key limitation shared by most studies exploiting constitutional term limits to estimate incentive effects: Legislators who survived sufficiently many reelection bids to hit the term limit constitute a selected sample of types for whom electoral incentives are least likely a binding constraint such that there is little scope for detecting accountability effects. If voters use elections effectively to sort out incongruent types having preferences misaligned with voters, those who survive repeated reelection bids are either ex-ante congruent types running in competitive districts, or ex-ante unconstrained incumbents representing uncompetitive districts. Finally, term limits diminish opportunities for long-term career advancements and reduce the value of office, which likely attracts more ideological candidates who are naturally less willing to compromise on their convictions in return for rent from office (Hall, 2019; Olson and Rogowski, 2020; Myers, 2023). My findings are complementary, drawn from House incumbents facing highly salient and close re-election bids. That said, this paper contributes an important existence result, showing that the accountability mechanism is operative and effectively constrains incumbent politicians' policy choices, provided an electorally competitive environment.

These findings complement empirical research investigating the impact of wages on politicians' in-office performance, which documents that higher salaries tend to attract higher-quality types but do not incentivize better performance (Gagliarducci and Nannicini, 2013; Mocan and Altindag, 2013; Fisman et al., 2015). Against this backdrop, my results that electoral incentives are effective in constraining legislators' policy choices empirically support the notion that politicians' career concerns combined with electoral competition are the most (and perhaps only) powerful incentive scheme to ensure democratic accountability (Tirole, 1994; Diermeier et al., 2005).

This paper further speaks to a growing empirical literature that decomposes Congressional roll call voting records in legislators' private interests (Levitt, 1996; Washington, 2008), the influence of their core constituency (Mian et al., 2010), peer effects (Harmon et al., 2019), and party control (Canen et al., 2020). Canen et al. (2021) show that party leadership whips rank-and-file members to take more extreme positions, estimating that party control accounts for approximately 65% of the polarization in roll call voting. My findings indicate that, in competitive districts, electoral incentives reduce polarization by 21–26%, while incumbents' loyalty to party leadership remains unaffected by lame-duck status. These results are consistent with the “marginality hypothesis” suggesting that legislators in competitive districts are more responsive to voter preferences (e.g., Ansolabehere et al., 2001; Griffin, 2006) and, therefore, less susceptible to party pressure (Canes-Wrone et al., 2007).

In closely related work, Lee et al. (2004) also leverage a close election RDD to study the relative relevance of electoral incentives and selection effects. Finding that an exogenous increase in electoral strength of House representatives resulting from the incumbency advantage inherited from



a close victory in the preceding election does not alter their voting behavior in the subsequent term, they conclude that voters merely *select* policies by replacing one incumbent with another but cannot *affect* policy by constraining sitting incumbents' policy choices. While supporting a pure selection model of electoral politics, the result is also consistent with closely elected incumbents correctly anticipating their future incumbency advantage and adjusting their voting behavior preemptively. By contrast, this paper exploits unanticipated variation in electoral incentives to show that voters *affect* not only their representatives' voting behavior but also aggregate legislative outcomes.

Finally, this paper relates to an emergent literature advancing on descriptive accounts of strategic candidate positioning. Complementing recent evidence that electoral competition encourages candidates to strategically moderate their campaign communication to appeal to the median voter (Le Pennec, 2023; Di Tella et al., 2023), I show that political competition incentivizes *actual* policy moderation.

The remainder of the paper is structured as follows: Section 2 describes the institutional setting and the data. Section 3 presents and validates the empirical strategy. Section 4 reports the main results. Section 5 sheds light on electoral incentives and alternative mechanisms behind lame ducks' reversion to extreme positions. Section 6 examines the external validity of the main findings. Section 7 quantifies the substantive implications of electoral incentives on representation and legislative outcomes. Section 8 concludes.

## 2 Empirical Setting and Data

### 2.1 Lame-Duck Sessions in the U.S. Congress

A lame-duck session of the U.S. Congress occurs when a chamber of the current Congress reconvenes in its old composition after the election for the next Congress has been held, but before the current Congress concludes its constitutional term and newly elected representatives assume office. These post-election sessions are referred to as lame-duck sessions due to the presence of exiting lame-duck members, having either lost their re-election bid or chosen to retire from office without seeking re-election. Despite lacking an immediate electoral connection to their constituency, lame-duck legislators actively participate in congressional proceedings as full members of Congress, retaining the same voting rights as reelected representatives. Growing awareness of the political agency problems inherent to lame duck sessions, i.e., concerns about departing members' vulnerability to corruption and ability to provide decisive support for unpopular legislation, led to the Twentieth Amendment to U.S. Constitution in 1933. In the era before the Twentieth Amendment, the final regular session of each Congress had always been a lame-duck session lasting from Election Day at the beginning of November in even years until the new Congress would convene on March 4 of the subsequent year.

Although the Twentieth Amendment abolished regular lame-duck sessions and anticipated the inauguration of the new Congress on January 3, it did not preclude Congress from reconvening in its old composition during the period after the November elections and before the seating of new members in the subsequent January. Under the Twentieth Amendment, lame-duck sessions can still occur when at least one chamber provides for an existing session to resume after general elections, or simply continues meeting in intermittent sessions during the period spanning elections.<sup>7</sup>

Lame duck sessions occurred only exceptionally in the post-war period. However, Congress convening in post-electoral lame-duck sessions has become the new norm in recent decades. The U.S. House of Representatives has convened in a lame-duck session after every general election since 1998. While earlier lame-duck sessions tended to focus on a few specific issues (e.g., the ratification of the General Agreement on Tariffs and Trade in 1994, or the Clinton impeachment in 1998), more recent Congresses reconvened after general elections to vote upon a multitude of contentious high-stakes issues, including appropriation bills lifting the debt ceiling (2010, 2014-2020), landmark legislation like the Don't Ask Don't Tell Act (2010), revisions of the National Defence Authorization Act (2010-2012, 2016, 2020), tax reforms (2010-2014), Iran Sanctions (2016), and

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<sup>7</sup>A third, yet rarely used possibility is that the leadership of a chamber invokes contingent authority granted by the chamber to call for a session to resume after elections. Two other possibilities have never occurred since the ratification of the Twentieth Amendment: Congress could enact a law that requires a new session to convene after elections, or the President could convoke Congress to convene in an extraordinary session after elections.

COVID-19 appropriations (2020).<sup>8</sup> This paper focuses on lame-duck sessions of the U.S. House in the most recent 111<sup>th</sup> to 116<sup>th</sup> Congresses (2008-2020) with more than 20 non-unanimous roll call votes, i.e., the only contemporary lame duck sessions exceeding the minimum number of roll calls allowing to scale legislators’ position by the W-NOMINATE procedure. Table 1 provides an overview of the six lame-duck sessions held by the U.S. House between 2010 and 2020.

TABLE 1: LAME-DUCK SESSIONS IN THE U.S. HOUSE OF REPRESENTATIVES FROM THE 111<sup>th</sup> TO THE 116<sup>th</sup> CONGRESS

Lame Duck Session	# Scalable Roll Calls	Example Roll Calls	House Incumbents (Democrats / Republicans)		
			All Members	RD Sample	Lost Election
111 <sup>th</sup> Congress (Nov. 15, 2010 – Dec. 22, 2010)	55	Appropriations for Military Constructions and Veteran Affairs (207 yea – 206 nay); Don’t Ask, Don’t Tell Repeal Act (250 yea – 175 nay)	434 (255/179)	309 (180/129)	54 (52/2)
112 <sup>th</sup> Congress (Nov. 13, 2012 – Jan. 1, 2013)	29	Spending Reduction Act (215 yea – 209 nay); Tax Relief Provisions (257 yea – 167 nay); Asthma Inhalers Relief Act (229 yea – 182 nay)	431 (191/240)	304 (132/172)	20 (6/14)
113 <sup>th</sup> Congress (Nov. 12, 2014 – Dec. 11, 2014)	34	Approval of Keystone XL Pipeline (192 yea – 224 nay); EPA Science Advisory Board Reform Act (229 yea – 191 nay); Act on Energy Needs of the Insular Areas (219 yea – 206 nay)	435 (201/234)	306 (135/171)	13 (11/2)
114 <sup>th</sup> Congress (Nov. 14, 2016 – Dec. 08, 2016)	32	Midnight Rules Relief Act (240 yea – 179 nay); Appropriations for Energy and Water Development (235 yea – 180 nay)	435 (188/247)	324 (139/189)	4(0/4)
115 <sup>th</sup> Congress (Nov. 13, 2018 – Dec. 21, 2018)	37	Manage our Wolves Act (196 yea – 180 nay); Child Protection Improvements Act (217 yea – 185 nay); Alaska Remote Generator Reliability and Protection Act (202 yea – 171 nay)	432 (196/236)	301 (136/165)	23 (0/23)
116 <sup>th</sup> Congress (Nov. 16, 2020 – Dec. 28, 2020)	22	Marijuana Opportunity Reinvestment and Expungement Act (228 yea – 164 nay); Amendment to the U.S.-Mexico Economic Partnership Act (227 yea – 180 nay)	430 (233/195)	288 (186/102)	13 (13/0)

Notes: The Table presents an overview of lame-duck sessions in the U.S. House of Representatives between the 111<sup>th</sup> and 116<sup>th</sup> Congresses (2010-2020), listing the number of roll call votes used for scaling House Members by the W-NOMINATE algorithm, along with examples of bills subject to roll call votes during the lame duck sessions. The Table further lists the number of House Members at the beginning of each lame-duck session by party (Democrats/ Republicans), and the number of House representatives in the RD sample, i.e., incumbents defending their seat in a competitive general election race against a main challenger of opposite political orientation, which excludes members not running for re-election (because retiring, lost nomination in their party’s primary, or running for higher office), members running unopposed or members whose strongest opponent is a minor party candidate of the same political orientation, members who switched party affiliation during the congressional term, and members whose position cannot be scaled by W-NOMINATE. The rightmost column indicates the number of incumbents in the RD sample who lost their re-election bid and returned as lame ducks to the post-electoral House sessions.

## 2.2 Roll Call Data and Legislator Positions

We are interested in whether the loss of reelection concerns leads lame-duck representatives to take less moderate positions on roll-call votes after elections. Obtaining individual roll call voting records of U.S. House Representatives in the 111<sup>th</sup> to 116<sup>th</sup> Congresses (2008-2020) from the vote-view.com database (Lewis et al., 2022), I use the W-NOMINATE procedure (Poole and Rosenthal, 1985) to locate incumbents’ roll call voting positions on the liberal-conservative scale, separately for the post-electoral lame duck session and the pre-electoral regular session in each congressional

<sup>8</sup>For excellent historical overviews of lame-duck sessions in the U.S. Congress, see Jenkins and Nokken (2008a,b); for more details on the legal framework governing the conduct of lame-duck sessions in the post-Twentieth Amendment and legislative actions taken in contemporary lame duck sessions, see Hudiburg (2022).

term. The W-NOMINATE algorithm applies a discrete choice model to locate legislators in the ideological space, placing legislators with similar roll call voting records closer to each other. I use the R implementation of `wnominate` (Poole et al., 2011) to extract the first-dimension W-NOMINATE score running from -1 (liberal) to +1 (conservative).

Following recommendations in Poole and Rosenthal (1985), I exclude uninformative lopsided roll calls on which more than 97.5% of House members agreed from the scaling, and restrict attention to House members casting at least 20 scalable votes, which according to Poole (cited in Nokken, 2013) is the minimum number of votes required to reliably estimate a legislator’s roll call position. Because 70 reelected and 8 lame-duck members in my sample cast less than 20 votes in the post-electoral lame-duck session, the latter restriction reduces my sample of reelection-seeking incumbents to 1832 reelected and 127 lame-duck members. There is no indication of endogenous sample attrition at the cutoff,<sup>9</sup> and even if there was, my difference-in-discontinuities design considers within-incumbent changes in roll-call positions between the pre- and post-election period, yielding internally valid estimates for the vast majority of incumbents who cast more than 20 votes in the lame duck session. In the discussion of mechanisms (see Section 5.2), I provide further evidence that selective abstention is not the driving force behind lame ducks’ more extreme position-taking. Appendix Figure A.1 presents the distribution of W-NOMINATE scores in pre-electoral regular sessions and post-electoral lame-duck sessions.

Given the relatively small sample sizes of Democratic (82) and Republican lame ducks (45) who sought reelection unsuccessfully, most of my analysis pools representatives of both parties, using the Republican W-NOMINATE and the *negative* of the Democratic W-NOMINATE as a measure of *roll call extremism*:

$$\text{Roll Call Extremism}_{i(p)} = \begin{cases} \text{W-NOMINATE}_{i(p)} & \text{if } p = \text{Republican} \\ -\text{W-NOMINATE}_{i(p)} & \text{if } p = \text{Democrat} \end{cases} \quad (1)$$

Deliberately departing from previous work that uses the absolute value of W-NOMINATE as an indicator of roll call voting extremity (e.g., Canes-Wrone et al., 2002; Fourinaies and Hall, 2022), the above definition of *roll call extremism* accommodates representatives crossing the origin, i.e., Democrats with a positive W-NOMINATE score and Republicans whose W-NOMINATE score is negative. *Roll call extremism* preserves the unit of measurement of the W-NOMINATE and is therefore directly interpretable. An increase in *roll call extremism* reflects a Democratic (Republican) incumbent taking a more liberal (conservative) position. It is worth noting that *levels* of

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<sup>9</sup>Regressing a dummy equal to 1 if an incumbent’s post-election roll call position cannot be scaled by W-NOMINATE on the right-hand side of the baseline RD-equation (2) with MSE-optimal bandwidth and a triangular kernel yields a discontinuity estimate of 0.028 (robust p-value accounting for clustering at the incumbent-level = 0.741).

W-NOMINATE scores are not directly comparable across congresses and sessions, as incumbent positions are estimated separately by congress  $\times$  sessions, i.e., on a different set of roll calls and in comparison to different sets of representatives composing the House. Moreover, within a congressional term, *levels of roll call extremism* are not comparable across parties. My difference-in-discontinuities design therefore conditions on a full set of congress  $\times$  party fixed effects and evaluates lame duck incumbents' *relative* repositioning, comparing their change in *roll call extremism* in the post-electoral session with respect to the pre-election period to the change in *roll call extremism* of reelected incumbents of the same party, serving in the same Congress and voting on the same set of roll calls. Congress  $\times$  party fixed effects also control for possibly divergent incentives for incumbents of different parties due to changing majority status,<sup>10</sup> and for possible imbalances in the distribution of lame ducks across parties because of wave elections. As can be seen in Table 1, elections come either as red or blue waves with either Democrats (2010, 2014, 2020) or Republicans (2012, 2016, 2018) losing many seats, such that in any given post-electoral session lame-duck members are concentrated within one of the two parties. In midterm elections (2010, 2014, 2018), the party that does not currently hold the White House incurs particularly large losses.

### 2.3 Election Returns and Auxiliary Data

I combine data on House incumbents' roll call voting with general election results collected by the [MIT Election Data and Science Lab \(2017\)](#). My RD strategy considers House representatives running in a competitive race against a main challenger of opposite political orientation, which excludes incumbents who retire, seek election for higher office or lost nomination in their party's primary, as well as incumbents who run unopposed or whose strongest opponent is a minor party candidate of similar political orientation (e.g., a Democrat whose strongest opponent affiliates with the Green party).<sup>11</sup> This leaves me with a sample of 1959 incumbents seeking re-election, 127 of which lost their re-election bid and returned as lame-duck members to the post-electoral session. Given that U.S. House elections select the winner by plurality rule, my RD design relies on the strongest opponent's vote share margin as the assignment variable that designates reelection-seeking incumbents to lame ducks if and only if their vote share falls behind their strongest opponent's.

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<sup>10</sup>For example, leadership of a party that is about to lose majority status in the subsequent Congress may be tempted to adjust the legislative agenda in the lame-duck session to push through pieces of legislation that would unlikely pass under majority control by the opponent party in the next Congress.

<sup>11</sup>I also exclude 3 incumbents who switched party during the congressional term preceding the general election, as well as House speakers who preside the House meetings but rarely cast a vote in roll calls.

TABLE 2: SUMMARY STATISTICS

PANEL A: MAIN VARIABLES OF INTEREST	Mean	Std. Dev.	Min.	Max.	Obs.
$\Delta$ Roll Call Extremism	-0.162	0.264	-0.760	0.597	1959
Post-Election Roll Call Extremism	0.488	0.268	-0.333	1.000	1959
Pre-Election Roll Call Extremism	0.650	0.197	-0.042	1.000	1959
$\Delta$ W-NOMINATE (Democrats)	0.270	0.228	-0.548	0.760	990
Post-Election W-NOMINATE (Democrats)	-0.391	0.254	-0.999	0.333	990
Pre-Election W-NOMINATE (Democrats)	-0.661	0.209	-1.000	0.042	990
$\Delta$ W-NOMINATE (Republicans)	-0.051	0.252	-0.730	0.597	969
Post-Election W-NOMINATE (Republicans)	0.588	0.244	-0.072	1.000	969
Pre-Election W-NOMINATE (Republicans)	0.640	0.184	-0.025	0.995	969
Strongest Opponent's Vote Share Margin (%)	-0.283	0.193	-0.984	0.311	1959
Lame Duck Incumbent	0.065	0.246	0	1	1959
PANEL B: AUXILIARY VARIABLES					
$\Delta$ Party Loyalty (%)	0.012	0.078	-0.481	0.330	1959
Post-Election Party Loyalty (%)	0.914	0.085	0.341	1.000	1959
Pre-Election Party Loyalty (%)	0.902	0.056	0.508	1.000	1959
$\Delta$ Absenteeism (%)	0.009	0.064	-0.307	0.514	1959
Post-Election Absenteeism (%)	0.039	0.064	0.000	0.553	1959
Pre-Election Absenteeism (%)	0.031	0.034	0.000	0.422	1959
Incumbent's Expected Margin of Victory	0.283	0.151	-0.111	0.900	1959
District's Share of Independents	0.306	0.051	0.156	0.549	1959
Legislative Effectiveness	1.050	1.252	0.000	16.314	1956

Notes: The Table presents summary statistics for the sample of 1959 U.S. House representatives of the 111<sup>th</sup> to 116<sup>th</sup> Congresses, who seek re-election in a competitive race against a main challenger of opposite political orientation. *Roll Call Extremism* measures the liberalism (conservatism) of Democratic (Republican) legislators based on W-NOMINATE scores as defined in equation 1.  $\Delta$  *Roll Call Extremism* is the difference between an incumbent's post-election *roll call extremism* (lame-duck session) and her *roll call extremism* before general elections (regular sessions). Corresponding changes and levels in first-dimension W-NOMINATE scores, estimated by congress  $\times$  session, are reported separately for members of the Democratic and Republican parties. The *Strongest Opponent's Vote Share Margin* is the the difference in vote shares between the main challenger's general election vote share and the incumbent's vote share (in percent). *Lame Duck Incumbent* is the treatment indicator of interest, taking the value 1 if the strongest opponent's vote share margin is positive and the incumbent loses the election, 0 otherwise. *Party Loyalty* is the session-specific share of roll votes the incumbent casts in agreement with her own party's whip. *Absenteeism* measures the session-specific proportion of roll calls in which the incumbent does not cast a vote. *Incumbent's Expected Margin of Victory* is the linear prediction from regressing the incumbent's actual vote share margin on incumbent's lagged vote share interacted with congress  $\times$  party fixed effects (including all lower order terms). *District's Share of Independents* is the share of respondents in pre-election waves of the Cooperative Congressional Election Survey (2006-2020) living within district borders who identify as independents rather than as Republicans or Democrats. The share is calculated over an average number of 963 respondents per district. *Legislative Effectiveness* is an index reflecting the weighted sum bills an incumbent sponsored during the current term relative to the average House member serving in the same term, whereby bills get higher weights the more substantive they are and the further they move in legislative process. Legislative effectiveness scores are normalized to mean 1 in each Congress.

Table 2, Panel A, presents summary statistics for the main outcomes of interest, reelection-seeking incumbents' lame-duck status, and their strongest opponent's vote share margin. Panel B provides descriptives of auxiliary variables used to explore the mechanisms behind the effect of lame-duck status on *roll call extremism*. To assess the roles of party leadership and selective abstention, I consider *party loyalty*, defined as the fraction of votes cast in line with the party whip; and absen-



teeism, measured as the proportion of roll calls an incumbent missed in a given session. Second, to shed light on logrolling motives, I consider differential effects depending on the degree of incumbents' involvement in lawmaking, as proxied by the *legislative effectiveness* score developed and made available by [Volden and Wiseman \(2014, 2023\)](#). The *legislative effectiveness* score is an index reflecting the weighted sum of bills an incumbent sponsored during the current term relative to the average House member serving in the same term, whereby bills get higher weights the more substantive they are and the further they move in the legislative process (e.g., a bill gets higher weight when considered by a committee, or even higher when passed by the House). The index is normalized to have mean 1 in each Congress. Third, to analyze heterogenous effects depending on House members' *ex-ante* likelihood of winning re-election, I rely on incumbents' *expected margin of victory*, estimated as the linear prediction from regressing incumbents' actual vote share margin on their lagged vote share interacted with *congress*  $\times$  *party* fixed effects (including all lower order terms). As I discuss in more detail below, allowing the autocorrelation of vote shares to vary by party and election year accounts for wave elections that in a given year tend to favor either Democrats or Republicans. Depending on the electoral cycle, a Republican incumbent's *expected margin of victory* may therefore significantly differ from a Democrat's who was elected with the same prior vote share. Fourth, I proxy the share of swing voters by the *district's share of independent voters*, which is calculated over all survey respondents in pre-election waves of the Cooperative Congressional Election Surveys (CCES) fielded between 2006 and 2020 that live within the district's borders defined for the year of the incumbent's reelection bid. Specifically, I pool pre-election waves of the CCES from 2006 to 2020, and then use survey respondents' zip codes to match them to the area covered by a district in a given election cycle. This accounts for redistricting and achieves a minimum number of 481 respondents (987 on average) over which a single *district's share of independent voters* is computed. Finally, I supplement the dataset with pre-determined incumbent characteristics and district-level covariates for validity and robustness checks, which I obtain from the CongressData database ([Grossmann et al., 2022](#)).

### 3 Identification Strategy

#### 3.1 Regression Discontinuity Design

The key identification challenge is to separate electoral incentives from selection effects. As mentioned previously, the predominant approach in the extant literature has been to compare within-incumbent changes in policy choices of exiting members to returning members. The focus on within-incumbent changes flexibly controls for pre-existing level differences and thus improves upon cross-person comparisons. However, if voters select depending on incumbents' in-office behavior, and some re-election seeking politicians strategically adjust policy to changing voter preferences while others do not, this approach compares the policy choices between responsive and unresponsive types of politicians. Simple difference-in-difference estimates may therefore confound electoral incentive effects with voter preferences and the selection of different types into lame-duck status.

To solve this issue, I propose an RD-strategy exploiting as good as random assignment of House incumbents to lame-duck status by close elections. Since House elections are decided by plurality rule, we have perfect knowledge of the mechanism that assigns incumbents to lame-duck status. Incumbents become lame ducks if and only if their strongest opponent in the general election receives a higher vote share. Assuming that incumbents have "imprecise control" (Lee and Lemieux, 2010) over toss-up election outcomes, I leverage plausibly exogenous variation in lame-duck status which is unrelated to voter preferences in the district that legislators represent, as well as orthogonal to incumbents' type, including their pre-election in-office behavior and prior experience.

Formally, I implement the RD strategy defining the lame-duck treatment  $T_{ipc}$  as a dummy variable equal to 1 if incumbent  $i$  of party  $p$  in congress  $c$  loses her re-election bid, and the running variable  $X_{ipc}$  as the vote share margin of the incumbent's strongest opponent, normalized such that  $T_{ipc} = 1$  if  $X_{ipc} > 0$  and  $T_{ipc} = 0$  if  $X_{ipc} < 0$ . I then evaluate the causal impact of lame-duck status on incumbents' roll call voting by estimating local linear regressions of the following form:

$$\Delta Y_{ipc} = \theta T_{ipc} + \beta_1 X_{ipc} + \beta_2 X_{ipc} T_{ipc} + \lambda_{pc} + \epsilon_{ipc} \quad (2)$$

where  $\theta$  is the coefficient of interest representing the causal effect of lame-duck status on  $\Delta Y_{ipc}$ , which is the within-incumbent change in *roll call extremism* as defined in equation (1). Using differenced outcomes reduces measurement error and improves the precision of my estimates, and at the same time, translates into difference-in-discontinuities design, which identifies  $\theta$  as a causal parameter under considerably weaker assumptions. Unlike traditional RD strategies, difference-

in-discontinuities allow for predetermined level differences provided that potential confounds do not vary differentially in the neighborhood of the cutoff (Grembi et al., 2016). *Congress*  $\times$  *party* fixed effects denoted by  $\lambda_{pc}$  ensure comparability of W-NOMINATE-based *roll call extremism* by restricting comparisons of barely unseated lame ducks to narrowly reelected co-partisans serving the same congressional term.

For estimation, I follow Calonico et al. (2014) and Calonico et al. (2019), using a non-parametric approach with MSE-optimal bandwidths and reporting p-values based on bias-adjusted confidence intervals. Within MSE-optimal bandwidths, I linearly downweigh observations more distant from the cutoff using a triangular kernel. Given repeated observations of the same representatives in different congresses, I cluster standard errors at the incumbent level.

There are reasons to believe the coefficient  $\theta$  in equation (2) likely identifies a lower bound on the true last-term effect. First, lame ducks can rerun for office, and political reputations built in their last term may still be valuable in future campaigns. To the extent that close election losers aspire for re-election in the future and thus remain accountable to their constituency, my estimates are attenuated toward zero. A second, more subtle point relates to election timing. My design effectively compares lame ducks at the end of their last term to reelected members at the *beginning* of their next term. If legislators are more accountable to voters at the end of the electoral cycle (e.g., because voters and the media are more attentive to incumbents' behavior just prior to elections), electoral ties are loose for returning members whose next election takes place two years down the road. Although this may attenuate my estimates somewhat, I do not expect attenuation to be large.<sup>12</sup>

### 3.2 Checks on the Validity of the Identification Assumption

The main threat to identification is posed by concerns that close election winners could be differentially more able to manipulate election outcomes. This is a priori extremely unlikely, as it would require close election winners either to have precise information to predict election outcomes which is unavailable to close election losers, or to be differentially able to act upon this information, exerting a campaigning effort just high enough to flip a close prospective defeat into a narrow win.<sup>13</sup>

If incumbents could sort themselves just above the re-election threshold, one would expect the sample proportion of close winners to be substantively higher than the proportion of bare elec-

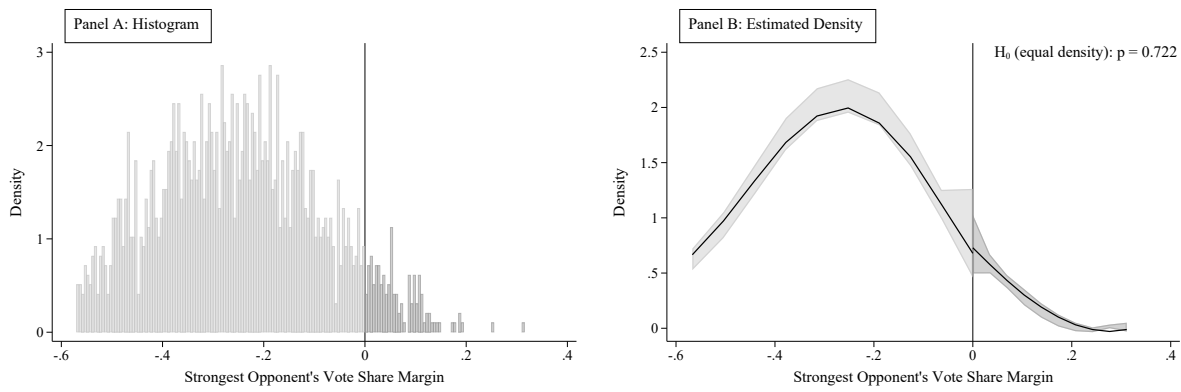
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<sup>12</sup>Studies leveraging random variation in state legislators' term length do not find any evidence of electoral proximity affecting roll call voting positions (Titiunik, 2016; Pomirchy, 2023), although they do find effects of term length on legislative effort (Titiunik, 2016, see also Dal Bó and Rossi, 2011). For evidence on the presence of electoral cycles in judicial sentencing, see, e.g., Huber and Gordon (2004), and Berdejó and Yuchtman (2013).

<sup>13</sup>For excellent discussions of the credibility of close election RD-designs, see Lee (2008), Eggers et al. (2015), De la Cuesta and Imai (2016).

tion losers (McCrary, 2008). Informal evidence against aggregate sorting is provided in Figure 1, Panel A, showing a smooth distribution of observations around the cutoff. Panel B is a graphical representation of a formal density test proposed in Cattaneo et al. (2020). One can see that the estimated densities of close election winners and losers are near each other, with 95% confidence intervals overlapping at the cutoff. Formally, I fail to reject the null hypothesis of equal densities on both sides of the cutoff ( $p = 0.722$ ). This evidence against sorting may also alleviate concerns on endogenous sample attrition due to unobserved W-NOMINATE scores of incumbents who did not cast enough votes to be included in the scaling (see Section 2.2).

FIGURE 1: MANIPULATION TESTS FOR AGGREGATE SORTING

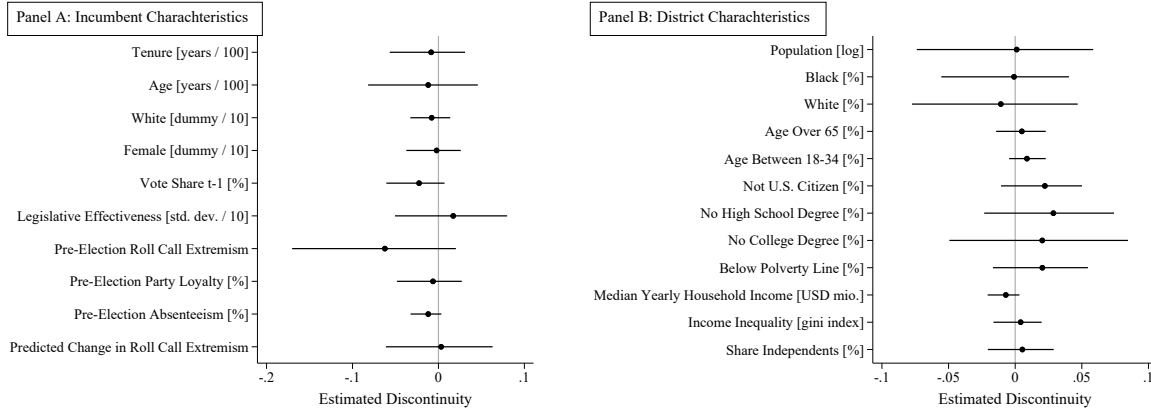


Notes: The Figure in Panel A presents the sample distribution of the *Strongest Opponent's Vote Share Margin* for representatives who win re-election against their runner-up (light grey) and lame-duck incumbents who lost their re-election bid (dark grey). Panel B is a graphical representation of the density test derived in Cattaneo et al. (2020), plotting density estimates (solid lines) using local quadratic approximations and a triangular kernel along with bias-adjusted 95% confidence intervals (shaded areas). The density test fails to reject the null hypothesis of equal density at the cutoff with a robust p-value equal to 0.722.

If some types of incumbents were differentially able to flip close elections, one would expect observable incumbent characteristics to vary discontinuously at the cutoff. I thus implement a series of balancing tests by regressing pre-determined incumbent- and district-level covariates on the righthand side of equation (2). Although continuity of confounders that are time-invariant over a two-year congressional term is not necessary for identification in my *difference-in-discontinuities* design, similar *levels* close to the threshold may grant some confidence in the key assumption of common *trends* around the cutoff. Resulting point estimates along with bias-corrected 95% confidence intervals are presented in Figure 2. Continuity of incumbent characteristics (Panel A) suggests that close election winners and narrow losers are of the same type, while balanced voter preferences and district characteristics provide evidence that incumbents in close races could not predict election outcomes (Panel B). Specifically, the absence of significant discontinuities in incumbents' pre-election *roll call extremism*, *absenteeism*, *party loyalty* (Panel A) suggests that rep-

representatives facing close elections did not strategically alter in-office behavior depending on the election outcome.

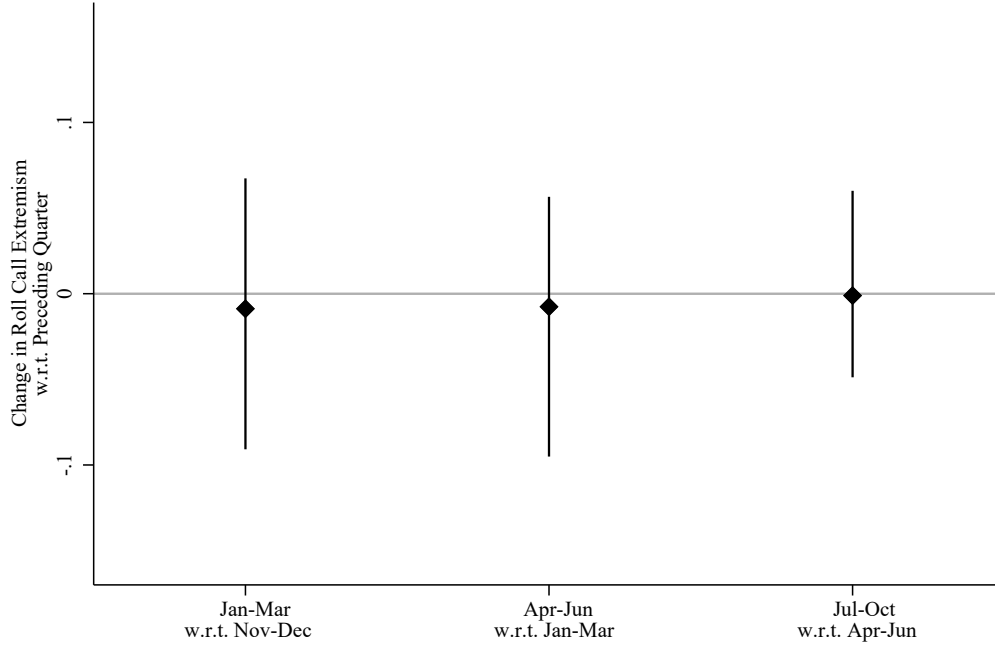
FIGURE 2: BALANCING TESTS ON INCUMBENT TYPE AND DISTRICT CHARACTERISTICS



Notes: The Figure presents results from balancing tests on incumbent (Panel A) and district characteristics (Panel B). Point estimates (dots) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering by House representative are obtained from local linear specifications of equation (2) with MSE-optimal bandwidths and triangular kernels.

Out of 20 balance tests, none reveals a discontinuity significant at conventional confidence levels, except pre-election absenteeism ( $p = 0.098$ ). However, the discontinuity in pre-election absenteeism is small in magnitude, and one false-positive result is expected under multiple testing for balancing of 20 covariates. More worrisome is the imprecisely estimated discontinuity in pre-election *roll call extremism*, which cannot rule out a substantively large imbalance in roll call voting positions prior to elections, with narrowly ousted lame-duck incumbents appearing *less* extreme than barely reelected co-partisans. I address these concerns in three different ways. First, I tackle invalid inference inherent to multiple testing of single coefficients by constructing a joint test, evaluating the discontinuity in the *predicted* change of *roll call extremism*, i.e., the fitted values from a linear regression of the *actual* change of *roll call extremism* on all other incumbent and district characteristics listed in Figure 2. As shown in the bottom row of Panel A, the predicted outcome of interest does not jump at the cutoff, with a point estimate as good as identical to zero. Second, I probe the robustness of my baseline specification to controlling for incumbent and district characteristics including pre-election outcomes. Reassuringly, the inclusion of covariates does not affect my results (see Appendix Table A.1). Third, I remind that identification by difference-in-discontinuities allows for pre-existing level differences, provided that *roll call extremism* of barely reelected and narrowly ousted incumbents follows a parallel trend.

FIGURE 3: TESTING FOR PARALLEL PRE-TRENDS AT THE CUTOFF



Notes: The Figure presents results from tests for pre-trends at the cutoff in the year preceding general elections. Each estimate represents the discontinuity in changes of incumbents' roll call extremism in one quarter with respect to the preceding quarter. Point estimates (diamonds) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering at the incumbent-level are obtained from local linear specifications of equation (2) with MSE-optimal bandwidths and triangular kernels.

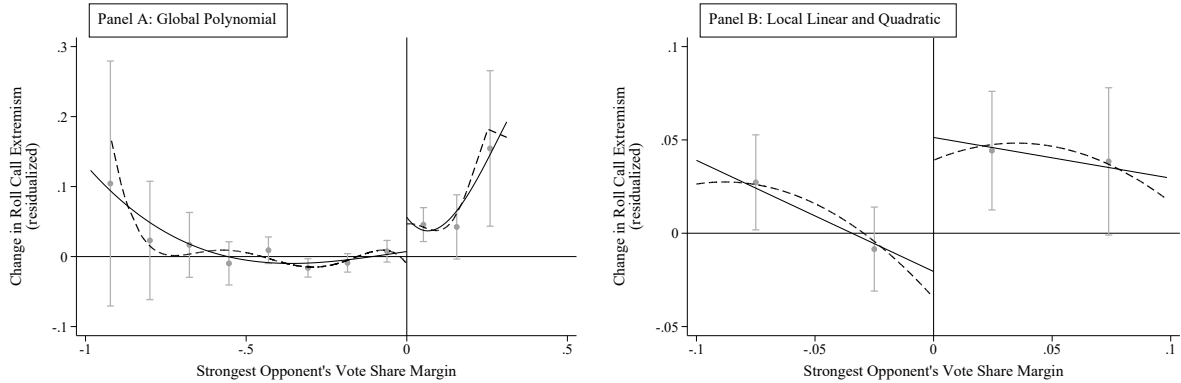
The key identification assumption of parallel trends around the cutoff could be violated if some incumbents had private information on the likely election outcome and differentially adjusted their roll call voting behavior over time upon learning signals of voter preferences or their relative popularity (e.g., through private opinion polling). To check the validity of this assumption, I test for pre-trends in narrow re-election winners' *roll call extremism* relative to bare losers. Specifically, I estimate equation (2) considering discontinuities in quarter-by-quarter changes in *roll call extremism* during the year leading up to general elections. Results presented in Figure 3 show that pre-trends are absent, lending further credibility to identification by a difference-in-discontinuities strategy.



## 4 Main Results

Before turning to formal estimation results, Figure 4 provides prima facie evidence on how lame-duck status affects incumbents' *roll call extremism* relative to reelected co-partisans serving in the same Congress. Panel A considers the whole sample of reelection-seeking incumbents and plots binned averages of representatives' change in *roll call extremism* – demeaned by removing *party*  $\times$  *Congress* fixed effects – against their strongest opponent's vote share margin. One can see that legislators who won their reelection bid do not change their roll call voting behavior relative to their party's average. In contrast, lame-duck incumbents exhibit a significant increase in *roll call extremism* with a clear jump at the threshold value assigning barely losing incumbents to lame-duck status. Restricting attention to incumbent reelection bids decided by a narrow margin of less than 10%, Panel B visually confirms the presence of a sharp discontinuity at the cutoff.

**FIGURE 4:** CHANGES IN INCUMBENT'S ROLL CALL EXTREMISM DEPENDING ON THEIR STRONGEST OPPONENT'S VOTE SHARE MARGIN



*Notes:* The Figure shows local means of within-incumbent changes in residualized *Roll Call Extremism*, net of *party*  $\times$  *Congress* fixed effects, from the regular session to the lame-duck session. Local averages (dots) are calculated within equal-spaced bins of the *Strongest Opponent's Vote Share Margin*, which assigns incumbents to lame-duck status if positive. 95% confidence intervals (spikes) account for clustering at the incumbent level. Panel A uses the whole sample of 1959 reelection-seeking incumbents and plots cubic (dashed lines) as well as quintic (solid lines) fits of the outcome on the assignment variable, which are allowed to differ on each side of the cutoff. Panel B restricts the sample to 297 incumbents whose reelection bid has been decided by a margin of less than 10%, and plots local linear (solid lines) as well as quadratic fits (dashed lines).

Table 3 presents formal discontinuity estimates from local linear regressions as specified in equation (2) with triangular kernel weights. The main outcome of interest is the within-incumbent change in *roll call extremism* from the pre-election regular session to the post-electoral lame-duck session. Column 1 shows results for my preferred specification, which estimates the impact of lame-duck status on a pooled sample of Republican and Democratic incumbents within the MSE-

optimal bandwidth, conditioning on a full set of *party*  $\times$  *congress* fixed effects to ensure comparability of *roll call extremism* across legislators. The discontinuity estimate implies that lame-duck status causes a significant increase in *roll call extremism* by 0.1 points on the W-NOMINATE scale, which ranges from - 1 (the most liberal Democrat) to + 1 (the most conservative Republican in my sample). The effect magnitude thus equals 5% of the ideological distance between the most liberal and the most conservative legislator in polarized America. Evaluated in terms of pre-election W-NOMINATE scores of incumbents in the 116<sup>th</sup> Congress, the implied shift of 0.1 corresponds approximately to one-half the within-party interquartile range or to the average distance between representatives and their own party’s median.<sup>14</sup> Reassurantly, the coefficient is unaffected when controlling for the full set of incumbent and district characteristics listed in Figure 2 including the pre-election level of *roll call extremism* (see Appendix Table A.1, Panel A, Column 2). The result is robust to alternative specifications using different kernel weights and higher-order polynomials of the assignment variable, and to a wide of bandwidths (see Appendix Table A.1 and Appendix Figure A.2). On average, lame-duck legislators who barely lost their reelection bid take significantly more extreme positions compared to their co-partisans serving the same Congress who won reelection by a narrow margin.

As one can see in Table 3, the average increase in *roll call extremism* is driven by both lame-duck Democrats taking more liberal positions reflected in a lower W-NOMINATE score (Column 3) and Republicans voting more conservatively which translates into higher W-NOMINATE scores (Column 5). To facilitate the comparison of effects across samples, Columns 2, 4, and 6 fix the bandwidth to elections decided by a margin of less than 5%. Intriguingly, the impact of lame-duck status is larger for Democrats than for Republicans, which is consistent with structural estimates for U.S. senators in Iaryczower et al. (2022) suggesting that Democrats are more willing to compromise on their policy ideals for a higher probability to retain office. I caveat, however, that the effects of lame-duck status on the subsamples of Democrats and Republicans are too imprecisely estimated to draw strong conclusions on differential effect sizes. The estimated difference in *magnitudes* of lame-duck effects between Democrats and Republicans falls short of statistical significance.<sup>15</sup>

<sup>14</sup>More precisely, the within-party interquartile range is 0.19 for Democrats and 0.18 for Republicans, while the average distance of representatives from their party’s median is 0.11 for both parties.

<sup>15</sup>Formally, I fail to reject the null hypothesis that the *negative* of the coefficient for Republicans in Column 6 equals the coefficient for Democrats in Column 4 ( $p = 0.491$ , two-tailed). The difference between coefficients, however, is statistically significant ( $p < 0.001$ , two-tailed). Analogous tests comparing coefficients in Columns 4 and 6 yield  $p < 0.312$  and  $p < 0.001$ , respectively.

**TABLE 3: THE EFFECTS OF LAME-DUCK STATUS ON CHANGES IN W-NOMINATE AND ROLL CALL EXTREMISM**

	<u>Δ Roll Call Extremism</u>		<u>Democrat Δ W-NOMINATE</u>		<u>Republican Δ W-NOMINATE</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.099*** (0.028) [0.001]	0.120*** (0.030) [0.000]	-0.149*** (0.039) [0.000]	-0.131*** (0.039) [0.000]	0.092** (0.042) [0.065]	0.093** (0.041) [0.413]
Party × Congress FE	Y	Y	-	-	-	-
Congress FE	-	-	Y	Y	Y	Y
Bandwidth	0.064	0.050	0.043	0.050	0.044	0.050
Effective Obs. Left	122	94	54	59	30	35
Effective Obs. Right	75	58	31	35	21	23
Control Mean	0.001	-0.008	0.002	0.005	-0.009	-0.015
Observations	1959	1959	990	990	969	969

*Notes:* The Table presents results from local linear regressions specified in equation 2, reporting the estimated effects of legislators' lame-duck status on changes in their roll call voting behavior during lame duck sessions with respect to the pre-election period of the same congressional term. Outcome variables are the change in *Roll Call Extremism* in the pooled sample which includes reelection seeking House incumbents of both parties (Columns 1 and 2), and the changes in W-NOMINATE scores among Democrats (Columns 3 and 4) or Republicans (Columns 5 and 6). The bandwidths are MSE-optimal in Columns 1, 3, and 5, and restricted to elections decided by a margin of less than 5% in Columns 2, 4, and 6. All regressions use triangular kernel weights, and include *party × congress* fixed effects (Columns 1 and 2), respectively congress fixed effects for split samples (Columns 3 to 6). Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Control Mean reports the average of the residualized outcome, net of *party × congress* fixed effects, within the bandwidth left to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

## 5 Mechanisms

### 5.1 Electoral Incentives, Strategic Moderation, and the Role of Reelection Concerns

This paper’s leading hypothesis is that removing reelection concerns leads lame ducks to adopt more extreme positions. Incumbents vying for reelection against a challenger proposing a platform on the opposite side of the ideological spectrum have electoral incentives to strategically moderate their voting record to commit to a position close to their opponent’s and near to the median voter’s preferred policy (Hotelling, 1929; Downs, 1957). For policy-motivated politicians, this involves compromising on their private ideals in exchange for higher chances of winning elections, which leads to *partial convergence* of policies toward the median (e.g., Wittman, 1977; Calvert, 1985). While electoral incentives to build moderate reputations remain operative for returning members after elections, exiting politicians lose electoral incentives to compromise that had been active before their last term (e.g., Alesina, 1988). The main findings – that lame ducks tend to take more extreme positions, whereas reelected members do not change their roll call voting behavior relative to the party’s average – are consistent with this idea. Next, I provide three pieces of evidence showing that lame ducks’ loss of electoral incentives to moderate is the driving mechanism behind their increasingly extreme voting behavior.

#### A. Effect Heterogeneity by Incumbents’ Electoral Vulnerability

First, I consider heterogeneous effects of lame-duck status on *roll call extremism* depending on incumbents’ electoral strength. Electoral incentives to moderate are naturally more binding for electorally weak incumbents, a prediction that directly follows from formal models in which policy-motivated politicians partially converge toward centrist positions (see Wittman, 1983; Lee et al., 2004). If lame ducks’ post-electoral extremism is caused by loosening electoral ties, one would expect larger effects among electorally more vulnerable incumbents whose electoral constraints were tighter before elections.

To test this prediction, I estimate incumbents’ *ex-ante expected margin of victory* as the predicted value from a linear regression of their actual vote share margin on their vote share in the preceding election interacted with *congress*  $\times$  *party* fixed effects (including all lower order terms). Importantly, allowing the expected vote share margin to vary by party and election captures *ex-ante predictable* changes in incumbents’ electoral strength depending on the electoral cycle. Congressional candidates from the same party as the winning presidential candidate tend to benefit from coattail effects in presidential election years, while midterm elections tend to boost the party that does not currently hold the White House (Erikson, 1988; Alesina and Rosenthal, 1989, 1996; Fair,

1996; Lewis-Beck and Stegmaier, 2000).<sup>16</sup>

To test for heterogenous effects of lame-duck status depending on incumbent vulnerability, I split my sample by terciles of the *expected margin of victory* in the subsample of incumbents facing an ex-post close re-election bid, i.e., by terciles of *expected margin of victory* for incumbents whose re-election is decided by a vote share margin of less than 5%.<sup>17</sup> Specifically, I divide legislators into electorally *vulnerable* (with an *expected margin of victory* below 8%), *contested* and relatively *safe* incumbents (with an expected margin between 8% and 14.3%, respectively above 14.3%).

**TABLE 4: HETEROGENEOUS EFFECTS OF LAME-DUCK STATUS ON ROLL CALL EXTREMISM DEPENDING ON INCUMBENTS' EXPECTED MARGIN OF VICTORY**

	<i>Vulnerable Incumbents</i> (1 <sup>st</sup> Tercile)		<i>Contested Incumbents</i> (2 <sup>nd</sup> Tercile)		<i>Safe Incumbents</i> (3 <sup>rd</sup> Tercile)	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.169***	0.149***	0.105**	0.087**	0.039	0.039
	(0.052)	(0.049)	(0.044)	(0.044)	(0.054)	(0.051)
	[0.001]	[0.000]	[0.015]	[0.029]	[0.850]	[0.238]
Bandwidth	0.038	0.050	0.036	0.050	0.042	0.050
Effective Obs. Left	25	29	19	32	28	33
Effective Obs. Right	20	24	13	18	14	16
Control Mean	0.019	0.015	-0.049	-0.045	0.015	0.006
Observations	128	128	184	184	1647	1647

*Notes:* The Table presents results from local linear regressions specified in equation 2, reporting the estimated effects of lame-duck status on within-incumbent changes in *Roll Call Extremism* depending on incumbents' ex-ante expected margin of victory. The sample is divided in vulnerable (Columns 1 and 2), contested (Columns 3 and 4), and safe incumbents (Columns 5 and 6) by terciles of the distribution of the expected vote share margin within ex-post close elections decided by an actual margin of less than 5%. The subsamples include observations below the 34<sup>th</sup> percentile (vulnerable), between the 34<sup>th</sup> and 67<sup>th</sup> percentiles (contested), and above the 67<sup>th</sup> percentile (safe). The bandwidths are MSE-optimal in Columns 1, 3, and 5, and fixed to ex-post close elections decided by an actual margin of less than 5% in Columns 2, 4, and 6. Other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

Table 4 presents the results. Consistent with lame-duck legislators responding to the relaxation

<sup>16</sup>Since the Civil War, there had been only 3 instances in which the presidential party won House seats in midterm elections, gaining never more than 9 seats compared to an average seat loss of 26 in the post-war period. The 3 midterm elections before my sampling period in which the presidential party won seats were 1934 (Roosevelt, Democratic gain of 9 seats), 1994 (Clinton, Democratic gain of 4 seats), and 2002 (Bush, Republican gain of 8 seats). Presidential party House members (e.g., Democrats in the 111<sup>th</sup> Congress, see Table 1) should therefore expect a lower vote share in midterm elections compared to representatives from the other party elected with the same vote share, whereas incumbents elected in a midterm wave election (e.g., Republicans in the 112<sup>th</sup> Congress) may estimate this electoral advantage to shrink toward the end of the term.

<sup>17</sup>By construction, incumbents' *expected margin of victory* is highly correlated with their *actual* vote share margin, which is the negative of the running variable in my RD design. If I split observations by terciles of the entire sample distribution, there would be insufficient variation to estimate effect heterogeneity close to the cutoff. Appendix Figure A.3 shows the distributions of *expected vote share margin* in the sample of all reelection-seeking incumbents (Panel A), and the subsample of incumbents facing an *ex-post* close election within the bandwidth of a 5% vote share margin (Panel B).

of differentially binding electoral constraints, effects are larger among electorally weaker incumbents. We observe the largest shift to more extreme positions for lame ducks who were ex-ante most *vulnerable* (Columns 1 and 2), substantively smaller effects on legislators in the second tercile of electoral strength (Columns 3 and 4), and only insignificant changes in roll call voting positions of relatively *safe* incumbents losing their reelection bid unexpectedly (Columns 5 and 6). Notably, the pattern is not driven by different MSE-optimal bandwidth choices across samples (uneven columns), with highly similar results under fixed bandwidths to ease comparability (even columns). The difference in coefficients estimated on *vulnerable* and *safe* incumbents is statistically significant at conventional levels. A two-tailed test rejects the equality of coefficients in Columns 1 and 5 ( $p = 0.053$ ), and in Columns 2 and 6 ( $p = 0.099$ ).

### B. Effect Heterogeneity by Districts' Share of Swing Voters

Second, I evaluate effect heterogeneity depending on the proportion of swing voters in the incumbents' districts. Electoral returns to strategic moderation are arguably highest for incumbents seeking reelection in districts with a high concentration of swing voters that can be swayed by "centrist" policy positions. This is a direct implication of probabilistic voting models in which voters differ in their policy preferences and the degree of party identification. In these models, candidates strategically adjust their policy platform to voter preferences, attaching more weight to more numerous groups of voters and to less partisan voters who are more sensitive to policy (Lindbeck and Weibull, 1987, 1993). If reelection-seeking incumbents strategically moderate their policy position to persuade swing voters and lame ducks lose the electoral incentives to do so, one should expect larger effects of lame-duck status on the *roll call extremism* of legislators representing districts with a higher proportion of swing voters.

To evaluate this prediction, I proxy the proportion of swing voters with the district's share of voters identifying as independents rather than Republicans or Democrats. The *district's share of independent voters* is calculated over all survey respondents in pre-election waves of the Cooperative Congressional Election Surveys (CCES) fielded between 2006 and 2020 that live within the district's borders defined for the year of the incumbent's reelection bid. Specifically, I pool pre-election waves of the CCES from 2006 to 2020, and then use survey respondents' zip codes to match them to the area covered by a district in a given election cycle. This accounts for redistricting and achieves a minimum number of 481 respondents (987 on average) over which a single *district's share of independent voters* is computed. Crucially, within the relevant subsample of close elections, the *district's share of independent voters* is uncorrelated with the *incumbent's expected margin of victory* ( $r = 0.05$ ), indicating that my measure for swing voters is indeed conceptually distinct from incumbent's vulnerability.<sup>18</sup>

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<sup>18</sup>These statistics refer to the relevant subsample of close elections decided by a margin of less than 5%. In the



**TABLE 5: HETEROGENEOUS EFFECTS OF LAME-DUCK STATUS ON ROLL CALL EXTREMISM DEPENDING ON DISTRICTS' SHARE OF INDEPENDENT VOTERS**

	<i>Partisan Districts</i> (1 <sup>st</sup> Tercile)		<i>Moderate Districts</i> (2 <sup>nd</sup> Tercile)		<i>Swing Districts</i> (3 <sup>rd</sup> Tercile)	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.029 (0.044) [0.499]	0.015 (0.045) [0.013]	0.098** (0.043) [0.038]	0.104* (0.055) [0.083]	0.151*** (0.048) [0.009]	0.163*** (0.043) [0.005]
Bandwidth	0.047	0.050	0.085	0.050	0.036	0.050
Effective Obs. Left	30	32	53	31	22	31
Effective Obs. Right	20	20	27	19	10	19
Control Mean	-0.006	-0.001	0.001	-0.012	-0.009	-0.013
Observations	865	865	504	504	590	590

*Notes:* The Table presents results from local linear regressions specified in equation 2, reporting the estimated effects of lame-duck status on within-incumbent changes in *Roll Call Extremism* depending on *Districts' Share of Independent Voters* as proxied by the share of respondents in the CCES pre-election surveys (2006-2020) that identify as independents rather than as Republicans or Democrats. The sample is divided in partisan (Columns 1 and 2), moderate (Columns 3 and 4), and swing districts (Columns 5 and 6) by terciles of the distribution of the share of independents within ex-post close elections decided by a margin of less than 5%. The subsamples include observations below the 34<sup>th</sup> percentile (partisan), between the 34<sup>th</sup> and 67<sup>th</sup> percentiles (moderate), and above the 67<sup>th</sup> percentile (swing districts). The bandwidths are MSE-optimal in Columns 1, 3, and 5, and fixed to ex-post close elections decided by a margin of less than 5% in Columns 2, 4, and 6. Other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

To assess effect heterogeneity depending on the proportion of swing voters in the incumbent's district, I again divide the sample by terciles of *district's share of independent voters*, whereby the terciles are defined on the distribution of close elections decided by a vote share margin of less than 5%.<sup>19</sup> I then estimate RD-equation (2) separately on the subsamples of *partisan* districts (share of independents below 29.8%, *moderate* and relatively *swing* districts (with a share of independents between 29.8% and 32.6%, respectively above 32.6%).

In Table 5, one can see that the effect of lame-duck status on *roll call extremism* is increasing with the proportion of independent voters in a district's electorate. In line with the expectation that lame-duck legislators respond to the easing electoral pressure for strategic moderation toward swing voters, the most significant shifts toward extreme positions are observed among incumbents representing *swing districts* (Columns 5 and 6). There are comparatively smaller effects on lame ducks representing moderately partisan districts (Columns 3 and 4), and only negligible changes in roll call voting positions of incumbents representing the most *partisan districts* (Columns 1 and 2). The estimates are precise enough to reject the null of equal coefficients in Columns 1 and 5 ( $p = 0.082$ ),

overall sample, the number of respondents per district is slightly smaller (965 on average, 293 at minimum), and there is a weakly negative correlation between *district's share of independent voters* and *incumbent's expected margin of victory* ( $r = -0.271$ ).

<sup>19</sup>Appendix Figure A.4 shows the distributions of *district's share of independent voters* in the sample of all reelection-seeking incumbents (Panel A), and the subsample of incumbents facing an *ex-post* close election within the bandwidth of a 5% vote share margin (Panel B).

respectively in Columns 2 and 6 ( $p = 0.023$ ).

### C. Effect Heterogeneity by Retirement Age

Third, I examine how the increase in *roll call extremism* among lame-duck legislators varies depending on their decision to run for office in the subsequent election. If the loss of reelection concerns leads lame ducks to adopt more extreme voting positions, one would expect this effect to be more pronounced among those who exit electoral politics. In contrast, defeated incumbents who maintain electoral ambitions should retain electoral incentives to moderate and preserve political capital for their next election bid.

**TABLE 6:** THE EFFECTS OF LAME-DUCK STATUS ON RERUNNING AND ROLL CALL EXTREMISM DEPENDING ON RETIREMENT AGE

	Retirement Age ( $\geq 60$ )		Pre-Retirement Age ( $< 60$ )	
PANEL A: EFFECTS ON RERUNNING (T+1)	(1)	(2)	(3)	(4)
	-0.731*** (0.212) [0.006]	-0.744** (0.229) [0.014]	-0.203 (0.188) [0.523]	-0.181 (0.198) [0.807]
Bandwidth	0.052	0.050	0.058	0.050
Control Mean	0.800	0.839	0.840	0.825
Effective Obs. Left	35	31	75	63
Effective Obs. Right	21	15	46	43
PANEL B: EFFECTS ON ROLL CALL EXTREMISM				
	0.247*** (0.041) [0.000]	0.236*** (0.042) [0.000]	0.034 (0.029) [0.336]	0.074*** (0.034) [0.000]
Bandwidth	0.048	0.050	0.093	0.050
Control Mean	-0.042	-0.042	0.021	0.008
Effective Obs. Left	31	31	113	63
Effective Obs. Right	13	15	56	43
Observations	914	914	1045	1045
Party $\times$ Congress FE	Y	Y	Y	Y

*Notes:* The Table presents results from local linear regressions specified in equation 2. The dependent variables are a dummy equal to one if the incumbent reruns for the House or any other elected office (Panel A), and the within-incumbent change in *Roll Call Extremism* from the regular to lame-duck session (Panel B). The sample is divided in incumbents having reached the minimum age requirement for a full and immediate pension (Columns 1 and 2), and incumbents younger than 60 (Columns 3 and 4). Bandwidths are MSE-optimal in Columns 1 and 3 and fixed to elections decided by a vote share margin of less than 5% in Columns 2 and 4. Other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

Clearly, both *roll call extremism* and the propensity to exit electoral politics are endogenous to

lame-duck status, with narrow election losers being 35 percentage points less likely to rerun for elected office in the subsequent cycle compared to their barely reelected co-partisans with a baseline probability to rerun of 0.83 (see Appendix Table A.3). I therefore use retirement age as a predetermined predictor of incumbents' propensity to exit politics. As shown in Appendix Figure A.5), the propensity to rerun for office remains remarkably stable at around 87% for incumbents aged 40 to 60, yet starts to decline gradually after age 60, which coincides with the minimum age requirement for Congress members to obtain a full and immediate pension upon retirement.<sup>20</sup>

Given the better outside options provided by pension payments, one might suspect defeated legislators of retirement age to exit electoral politics with a higher probability than early- and mid-career election losers. Indeed, as shown in Panel A of Table 6, narrow election losers in retirement age are 73 percentage points less likely to rerun for an elected office with respect to a 0.83 baseline probability among retirement-aged bare winners (Columns 1 and 2). In contrast, legislators below retirement age are much more persistent in seeking reelection. Departing from a similar baseline propensity to rerun, pre-retirement-aged election losers are only 22 percentage points less likely to stand for election in the upcoming cycle (Columns 3 and 4). Reflecting the differential effects of electoral loss on the propensity to rerun, lame-duck status causes retirement-aged legislators to adopt markedly more extreme positions than do closely defeated early- and mid-career politicians. The positional shift by retirement-aged lame ducks corresponds to 0.24 points on the W-NOMINATE scale (Panel B, Columns 1 and 2), which is about thrice the increase in *roll call extremism* among lame ducks below 60 years of age (Panel B, Columns 3 and 4).

Of course, it is possible that retirement-aged representatives differentially react to lame-duck status for reasons other than their higher propensity to exit electoral politics. The evidence that the lower probability of rerunning correlates with a reversion to more extreme positions is thus best viewed as suggestive for lame ducks' loss of reelection concerns driving the result. A causal interpretation would require imposing the exclusion restriction that lame-duck status increases roll-call extremism only through its impact on political ambition, without affecting legislators' voting records through alternative channels.

## 5.2 Alternative Mechanisms

The evidence presented thus far strongly supports the idea that lame ducks' extreme voting record is due to the loss of reelection concerns, which removed electoral incentives for strategic moderation to persuade swing voters. However, lame ducks may not only lose electoral ties to voters but also accountability to party leadership and co-legislators. Their more extreme voting behavior

<sup>20</sup>The exact age requirement depends on the Congress members' pension plan and years of civil service (see Isaacs, 2023). In my sample of close elections decided by a margin of less than 5%, one single incumbent aged below 60 would qualify for full and immediate pension payments.

could also result from selective participation in roll calls on issues where they hold strong preferences, from emotional backlash after electoral loss, or from post-congressional career concerns.

#### A. *Loyalty to Party Leadership*

First, I consider the possibility that lame ducks' deviation to more extreme voting positions is due to differential incentives to toe the party line, as the party leadership loses its grip on exiting members of Congress. In the U.S. Congress, party control is institutionalized in the whip system. Minority and Majority Whips are the second-ranking members of each party's leadership, whose main task is to ensure party discipline in roll call voting, rewarding rank-and-file legislators who toe the party line, and punishing those who deviate with the assignment, respectively withdrawal of, e.g., seats and chairs in powerful committees, floor time, bills on the agenda, federal expenditures targeted to their district, or leadership political action committee campaign funds (see [Smith, 2007](#); [Evans, 2018](#)). These disciplining incentives are operative for returning incumbents but are likely ineffective on members leaving office. Given evidence for the presence of party influence on roll call voting ([Snyder and Groseclose, 2000](#); [McCarty et al., 2001](#)), more recent findings show that party control is a main driver of polarization in legislative voting ([Canen et al., 2020, 2021](#)), suggesting that lame-duck incumbents' lack of party discipline may work in the direction *opposite* to my findings. On the other hand, one might suspect that exiting legislators' post-congressional careers could be particularly reliant on support from party leadership (e.g., if they aim for a job within the party or affiliated organizations).

While correlational evidence on lame-duck members' party loyalty is decisively mixed,<sup>21</sup> I directly test for a causal effect of lame-duck status on *party loyalty*, evaluating the effect of close electoral defeat on the change in incumbents' share of votes cast in line with the own party's whip. Examining incumbents' change in *party loyalty* as the outcome in regression equation (2) yields precisely estimated null results, as shown in Table 7, Columns 1 and 2. This result aligns with the "marginality hypothesis" that legislators representing competitive districts are more responsive to voters (e.g., [Ansolabehere et al., 2001](#); [Griffin, 2006](#)) and, hence, less susceptible to party pressure ([Canes-Wrone et al., 2007](#)).

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<sup>21</sup>[Stratmann \(2000\)](#) finds that retiring legislators in the 98<sup>th</sup> to 103<sup>rd</sup> Congresses (1983-1995) vote more often in party line than returning members, whereas [Figlio \(1995\)](#) reports the opposite: Retiring members of the 94<sup>th</sup> to 97<sup>th</sup> Congresses (1975-1983) voted less frequently with the majority of their party. [Jenkins and Nokken \(2008b\)](#) document that exiting House members in lame-duck session of the pre-Twentieth Amendment era (45<sup>th</sup> to 72<sup>nd</sup> Congress, 1877-1933) more likely deviated from party line than returning members.

TABLE 7: LOYALTY TO PARTY LEADERSHIP, SELECTIVE ABSTENTION, AND ROLL CALL EXTREMISM

	$\Delta$ Party Loyalty (%)		$\Delta$ Absenteeism (%)		$\Delta$ Roll Call Extremism			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.009 (0.027) [0.902]	-0.011 (0.027) [0.961]	0.046** (0.021) [0.071]	0.044** (0.022) [0.036]	0.099*** (0.028) [0.001]	0.120*** (0.030) [0.000]	0.104*** (0.028) [0.000]	0.121*** (0.030) [0.000]
Control for $\Delta$ Absenteeism	N	N	N	N	N	N	Y	Y
Bandwidth	0.046	0.050	0.073	0.050	0.064	0.050	0.062	0.050
Effective Obs. Left	88	94	138	94	122	94	118	94
Effective Obs. Right	53	58	81	58	75	58	74	58
Control Mean	0.002	-0.000	0.019	0.021	0.019	0.011	0.019	0.011
Observations	1959	1959	1959	1959	1959	1959	1959	1959

Notes: The Table presents results from local linear regressions specified in equation 2, reporting the estimated effects of lame-duck status on within-incumbent changes in *Party Loyalty* (Columns 1 and 2) *Absenteeism* (Columns 3 and 4), and *Roll Call Extremism* (Columns 5 to 8) in the post-electoral lame-duck session with respect to the pre-electoral regular sessions of the same congressional term. *Party Loyalty* is the percentage share of votes cast in line with the own party's whip. *Absenteeism* is the percentage share of roll calls the incumbent did not cast a vote. The bandwidths are MSE-optimal in odd-numbered columns, and fixed to close elections decided by a vote share margin of less than 5% in even-numbered columns. All regressions use triangular kernels and include *party*  $\times$  *congress* fixed effects. Columns 7 and 8 additionally control for the change in *Absenteeism*. Other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

### B. Selective Abstention

A second alternative explanation for lame ducks taking more extreme positions is selective abstention. The loss of electoral accountability could induce representatives to exert less effort and attend fewer House floor meetings. Absent re-election concerns motivating incumbents to vote on behalf of their constituency, lame ducks might vote only on issues they personally care about. If preference intensity correlates with preference extremity, a more extreme roll call voting record could emerge as a byproduct of participatory shirking rather than as the consequence of removing electoral incentives to moderate strategically. Indeed, a large correlational literature on congressional shirking documents that lame-duck legislators miss more roll call votes than returning members of Congress (Lott, 1987, 1990; Lott and Bronars, 1996; Herrick et al., 1994; Rothenberg and Sanders, 2000).

Estimates in Table 7, Columns 3 and 4, confirm these findings, providing evidence for a causal relationship between lame duck status and roll call *absenteeism*.<sup>22</sup> Narrowly out-selected lame ducks are 4.5 percentage points less likely to participate in post-electoral roll calls compared to closely reelected colleagues. To determine whether selective abstention drives lame ducks' increase in *roll call extremism*, I perform a mediation analysis. If differential abstention was the main channel

<sup>22</sup>This first causal evidence for participatory shirking in the U.S. Congress complements similar findings in different settings. Fourniaies and Hall (2022) show that the absence of electoral incentives causes termed-out U.S. state legislators to participate in fewer floor votes, while Fiva and Nedregård (2023) provide evidence that Norwegian MPs' absenteeism rates in national parliamentary votes increase after losing renomination in local party conventions. Neither of these studies, however, finds an effect of lame-duck status on legislators' voting position conditional on voting.

through which lame-duck status affects positional changes in legislators’ voting record and thus fully or partially mediated the effect of lame-duck status on *roll call extremism*, one would expect a sharp drop in the coefficient of interest upon controlling for the endogenous change in *absenteeism*. Columns 5 and 6 report the baseline estimate of lame-duck status on *roll call extremism* using the MSE-optimal and a fixed 0.05 bandwidth, respectively, while Columns 7 and 8 re-estimate analogous equations conditioning on the within-incumbent change in *absenteeism* from regular to the post-electoral lame-duck sessions. We observe that the coefficients of interest are as good as identical across specifications, strongly suggesting selective abstention does not account for lame-duck members’ more extreme voting behavior.

### C. Logrolling Motives

A third alternative channel by which lame-duck status could increase *roll call extremism* could be lame-duck legislators’ loss of logrolling motives. Policy-oriented legislators may be primarily interested in achieving policy change by sponsoring bills and forging majority coalitions to get their proposals enacted into law. In a divided government where legislative chambers and the executive are controlled by different parties, successful bills often require bipartisan support. Policy-oriented legislators may therefore engage in vote trading across party lines and compromise on some policy positions in exchange for support of their own proposals. Returning House members thus could exhibit more moderate voting records because they keep committed to (perhaps implicit) vote trading contracts to secure future support for their proposals, whereas exiting lame ducks inevitably quit this dynamic game.<sup>23</sup>

If the removal of logrolling motives, rather than electoral incentives to moderate, drive lame-duck incumbent’s *roll call extremism*, one would expect larger effects among legislatively more engaged lawmakers. To test this hypothesis, I split my sample by the median *legislative effectiveness* of incumbents facing close elections (i.e., the median *legislative effectiveness* of incumbents whose re-election is decided by a vote share margin of less than 5%).<sup>24</sup> The *legislative effectiveness* score (Volden and Wiseman, 2014) measures within-congress differences across legislators in proposing substantively important bills and moving them through the legislative process. As can be seen in Table 8, the lame duck effect on *roll call extremism* is similar across legislatively active incumbents (Columns 1 and 2) and legislatively less engaged representatives (Columns 3 and 4). If anything, legislatively more active members seem to react differentially less to seat loss, per-

<sup>23</sup>Stratmann (1992) provides empirical evidence for logrolling in the U.S Congress, primarily among legislators with intense policy preferences; see also Cohen and Malloy (2014), and Battaglini et al. (2023). Theoretical accounts of logrolling go back to Buchanan and Tullock (1965). See, e.g., Carrubba and Volden (2000) and Casella and Palfrey (2019) for vote trading in dynamic settings, and Casella and Macé (2021) for an extensive overview.

<sup>24</sup>Appendix Figure A.6 shows the distributions of *legislative effectiveness* in the sample of all re-election seeking incumbents (Panel A), and the subsample of incumbents facing an *ex-post* close election within the bandwidth of a 5% vote share margin (Panel B).



haps because more policy-oriented representatives have stronger policy preferences and are less inclined to compromise on ideology to retain office.

**TABLE 8: EFFECT OF LAME DUCK STATUS ON ROLL CALL EXTREMISM DEPENDING ON INCUMBENT’S LEGISLATIVE ACTIVITY**

	High Legislative Activity		Low Legislative Activity	
	(1)	(2)	(3)	(4)
	0.114*** (0.043) [0.005]	0.107*** (0.040) [0.005]	0.156*** (0.036) [0.000]	0.162*** (0.038) [0.000]
Bandwidth	0.031	0.050	0.058	0.050
Effective Obs. Left	28	48	56	45
Effective Obs. Right	19	27	36	31
Control Mean	0.018	0.005	0.022	0.017
Observations	759	759	1197	1197

*Notes:* The Table presents results from local linear regressions specified in equation 2, reporting the estimated effects of lame-duck status on within-incumbent changes in *Roll Call Extremism* depending on incumbents’ legislative activity. The sample is divided in legislatively more active (Columns 1 and 2) and less active incumbents (Columns 3 and 4) by the median of the legislative effectiveness score within the sample of ex-post close elections decided by a margin of less than 5%. The bandwidths are MSE-optimal in Columns 1, and 3, and fixed to ex-post close elections decided by an actual margin of less than 5% in Columns 2, and 4. Other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

#### D. Emotional Backlash and Post-Congressional Career Concerns

Fourth, I consider the possibility that lame ducks’ reversion to more extreme positions, rather than a rational response to the loss of re-election concerns, could reflect an emotional reaction to electoral defeat. The loss of office may trigger an emotional backlash as a consequence of perceived injustice, disappointment, or grief. Aggrieved individuals who feel treated unfairly because they did not get the outcome they *expected* under an incomplete contract may retaliate by taking costly actions against the counterparty (Hart and Moore, 2008, see also Fehr and Schmidt, 1999; Fehr et al., 2011, and for a political economy application Passarelli and Tabellini, 2017). If defeated lame ducks expected to win reelection and perceived the election outcome as “unfair”, they might take more extreme positions as an act of defiance against voters who did not re-elect them. Moreover, if incumbents exhibit loss aversion (Kahneman and Tversky, 1979; Köszegi and Rabin, 2006), emotional cues might also explain the result that only narrow losers react to election outcomes whereas close election winners do not adapt their voting positions differentially with respect to the average co-partisan sitting in the same congress.

Although I cannot test for this mechanism directly, extant empirical work shows that emotional backlash is triggered by *unexpected* loss and is relatively short-lived, typically fading within a few

days (e.g., [Card and Dahl, 2011](#); [Eren and Mocan, 2018](#)). Yet, the earliest roll call vote used to scale legislators' positions in lame-duck sessions occurs 6 days after Election Day, and 90% of the scalable post-electoral roll calls in my sample take place more than two weeks after elections. Moreover, we observe the largest shift to extreme positions among ex-ante vulnerable lame ducks who lost *expectedly*, whereas the effect of lame-duck status on *roll call extremism* is indistinguishable from zero for ex-ante safe incumbents who lost by surprise (Table 4).

Similarly, it is not obvious how lame ducks' post-congressional career concerns by themselves would account for the empirical pattern we see in the data. A uniform incentive to trade extreme roll-call voting positions for future employment does not explain why the effects of lame-duck status on *roll call extremism* are limited to retirement-aged incumbents who are electorally vulnerable and represent districts with many swing voters. Although I cannot completely rule out a second-order influence of emotions or post-congressional career concerns, I conclude that the first-order mechanism driving lame ducks' reversion to extreme positions is the loss of electoral incentives to strategically moderate their positions before elections.

## 6 External Validity

### 6.1 Beyond Lame-Duck Sessions

Lame-duck sessions provide a unique quasi-experimental setup to causally identify the effect of reelection concerns on strategic policy moderation. One naturally wonders whether the conclusions drawn from this particular setting extend to legislator behavior in regular sessions. In post-electoral lame-duck sessions, the House could pursue systematically different legislative agendas, perhaps because voters are less attentive after elections, or because the current majority tries to expedite partisan legislation before the new House gets seated under different majorities.<sup>25</sup>

To check for systematic differences between pre-electoral and post-electoral legislative agendas, Appendix Figure A.7 compares roll call votes in lame-duck and regular sessions across several dimensions: policy areas (Panel A), various proxies for issue importance (Panel B), types of bills voted upon (Panel C), and the majority margin by which votes are decided (Panel D). The distribution of votes across bill types and 20 major policy areas – as classified by the *comparative agendas project* (Wilkerson et al., 2023) – is strikingly similar between lame-duck and regular sessions, with very minor differences in the relative frequencies of votes concerning social welfare, energy bills, and international relations. Votes held before and after general elections are also comparable in terms of importance, with Presidents and interest groups equally likely to take a public stance, and roll calls having the same likelihood of being designated as a key vote by *votesmart*. Roll call votes in lame-duck sessions are somewhat more frequently rated as key votes by the *Congressional Quarterly Almanac*, and House speakers are 3 percentage points more likely to cast a vote during these sessions. Lastly, roll calls in both sessions are decided by roughly similar vote margins, suggesting that individual legislators’ pivotality does not differ significantly between sessions. Overall, this descriptive analysis indicates that, while not identical, legislative agendas and the characteristics of roll-call votes in lame-duck and regular sessions are highly similar.

Next, I examine whether the effects of removing reelection concerns on lame ducks’ voting record are specific to majority constellations and post-electoral changes thereof. Parties’ majority status in the U.S. House changed twice during the period under study. In 2010, Republicans won a landslide victory, gaining 63 seats and taking control of the House. They maintained their majority until the 2018 election when Democrats regained control by flipping 41 seats. Because of these two wave elections, incumbents whose party lost its majority status are prevalent in my sample of lame-duck legislators, representing 59% of the incumbents losing their seat against a challenger of the opponent party. To evaluate the external validity of my findings across different majority

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<sup>25</sup>Since equation (2) controls for a full set of *Congress*  $\times$  *party* fixed effects, varying majority constellations are not a threat to the internal validity of my results. To the extent that voters are less attentive after elections resulting in lower accountability of reelected legislators in lame-duck sessions, my estimates represent a lower bound on electoral incentive effects in regular sessions.

constellations, I split incumbents into two distinct subsamples, first by their party's current majority status (Appendix Table A.6), and second by their prospective majority status in the subsequent congress (Appendix Table A.8). Third, I divide incumbents into three distinct groups depending on whether their party's majority status remained unchanged, transitioned from majority to minority, or from minority to majority (Appendix Table A.8). RD estimates are qualitatively consistent and quantitatively similar across all of these subsamples. While some of the estimates are too imprecise to rule out modest differences between subgroups, substantial effects are present across all of them.

## 6.2 Over Time

To further assess the external validity of my findings, I evaluate whether the effects of removing reelection concerns among lame-duck legislators are specific to different contexts of congressional politics. Since lame-duck sessions have only recently become the new norm, and given the minimum requirement of 20 scalable roll call votes to calculate W-NOMINATE, my sample covers a relatively short period spanning six Congressional elections from 2010 to 2020. Nonetheless, the U.S. political landscape has undergone several significant changes during this time. In the last decade, the U.S. has witnessed the rise and fall of the Tea Party movement during President Obama's first two terms, the emergence of Trumpism and Donald Trump's norm-breaking presidency, and the subsequent progressive shift within the Democratic Party. Appendix Table A.5 presents the effects of lame-duck status on *roll call extremism* across distinct time periods, splitting the sample once into the three congressional terms before and the three terms after the advent of Trumpism in the 2016 election, and then into three distinct groups of elections held either during President Obama's first term (2010/12 elections), his second term (2014/16 elections), or during Trump's presidency (2018/20 elections). RD estimates for all of these periods are substantive, significant, and comparable in size. Despite the major shifts in the political landscape over these years, removing reelection concerns has had remarkably similar effects on lame-duck incumbents' voting records, suggesting electoral incentives for strategic moderation are pervasive within the U.S. institutional context.

## 6.3 Away from the Cutoff

Regression discontinuity estimates are locally identified for incumbents losing their reelection bid by a coin flip. To explore how far these local results extend to legislators who lost the election by wider margins, I conduct an event study analysis comparing lame ducks' and reelected members' *roll call extremism* in the post-electoral lame-duck session and the four quarters preceding the election. Formally, I estimate dynamic difference-in-differences equations of the following form:

$$Y_{iptc} = \sum_{\substack{t=0 \\ t=-4 \\ t \neq -2}} \tau_t \text{Period}_t \times \text{LameDuck}_{ic} + \phi_{ic} + \psi_{pct} + \varepsilon_{iptc} \quad (3)$$

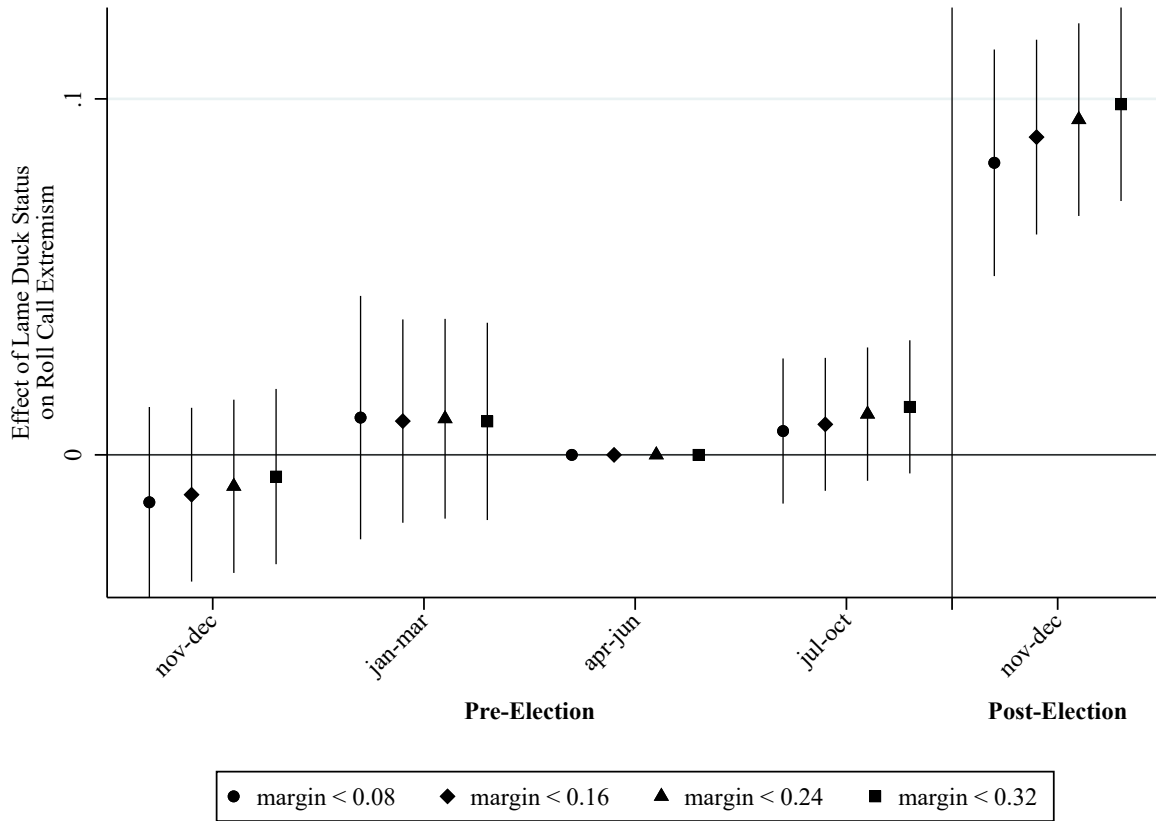
where  $Y_{iptc}$  designs the level of *roll call extremism* for incumbent  $i$  of party  $p$  in quarterly period  $t$  of Congress  $c$ .  $\text{LameDuck}_{ic}$  is a time-invariant dummy equal to one for legislators  $i$  who serve in Congress  $c$  but are not reelected to the next Congress  $c + 1$ .  $\text{Period}_t$  are indicators for the four quarters preceding the election ( $t < 0$ ), respectively the final period coinciding with the lame-duck session ( $t = 0$ ). The second quarter of the election year (April-June) is the omitted period of reference ( $t = -2$ ). Given *incumbent*  $\times$  *congress* ( $\phi_{ic}$ ) and *party*  $\times$  *congress*  $\times$  *period* fixed effects ( $\psi_{pct}$ ), the sequence  $\{\tau_t\}_{t=-4}^0$  designates quarter-specific difference-in-difference estimates off the parties' average shift in voting positions over time. Errors are clustered at the incumbent level.

Figure 5 presents the results of a specification that adjusts for the slightly negative pre-trends following suggestions in Goodman-Bacon (2021).<sup>26</sup> Keeping the control group of reelected members fixed to incumbents winning their reelection bid by a vote margin smaller than 20%, I extend the definition of the treated group to include to lame ducks losing with increasingly wider margins, ranging from 8% to the sample maximum of 32%. As one can see, the effects are highly similar in magnitude compared to the baseline RD estimates, indicating that the findings extend beyond the marginal legislators losing a toss-up election. However, it is worth noting that 77% of the reelection-losing legislators in my sample were defeated by a margin smaller than 10%. Therefore, the external validity of event-study estimates is still limited to incumbents representing competitive districts and may not generalize to safe districts where electoral incentives to moderate are likely muted.

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<sup>26</sup>The coefficient estimates, along with standard errors, are also presented in Appendix Table A.9, Columns 1 to 4. Estimates unadjusted for negative pre-trends (see Appendix Figure A.8 and Appendix Table A.9, Columns 4 to 8) are qualitatively consistent yet somewhat smaller in magnitude. This is what one would expect if incumbents learn about their reelection prospects and anticipate election outcomes, which biases difference-in-difference estimates toward zero.

**FIGURE 5: THE EFFECT OF LAME-DUCK STATUS ON INCUMBENT'S ROLL CALL EXTREMISM: EVENT-STUDY ESTIMATES**



*Notes:* The Figure plots dynamic difference-in-differences estimates of the effect of lame duck status on *Roll Call Extremism* in the four quarters of the year preceding the general election and the lame-duck period after elections, with the second quarter of the election year (April-June) as the omitted category of reference. The sample is restricted to incumbents reelected by a vote share margin of less than 20% (control group), and incumbents who lost reelection by a margin of less than 8% (circles), 16% (diamonds), 24% (triangles), or 32% (squares). Estimates are pre-trend-adjusted (Goodman-Bacon, 2021) and condition a full set of Member  $\times$  Congress fixed effects as well as Congress  $\times$  Party  $\times$  Period fixed effects. 95% confidence intervals (spikes) account for clustering by House representatives.

## 7 Implications

### 7.1 Electoral Incentives, Selection Effects, and Polarization

In representative democracies, there are two fundamentally different roles of elections in determining public policy. On the one hand, voters *select* politicians whose ideology is ex-ante aligned with their own preferences. On the other hand, the threat of losing office imposes reelection concerns that *incentivize* ex-post responsiveness to voter preferences. I estimate that electoral incentives induce policy moderation by 0.1 to 0.12 points on the W-NOMINATE scale (Table 3, Columns 1 and 2). One naturally wonders about the relative importance of electoral incentives and selection effects.

To benchmark electoral incentives against selection effects, I estimate the selection component as the distance of roll call voting positions between narrowly elected Democrats and Republicans in otherwise comparable districts, i.e., keeping voter preferences fixed. Intuitively, I ask by how much a district's W-NOMINATE score changes between two consecutive Congresses, depending on whether an incumbent barely held her seat, or narrowly lost to the challenger of the opponent party. In practice, I re-estimate the baseline RD-equation (2) on a different outcome: instead of focusing on the within-incumbent change in *roll call extremism* from the regular to the lame-duck session of the same Congress, I consider the within-district change in W-NOMINATE scores from one Congress to the next. The discontinuity at the cutoff estimates the selection effect, capturing how much a district's roll call voting record changes on the W-NOMINATE scale when a Democrat incumbent narrowly loses her reelection bid and is replaced by a Republican (or vice versa), compared to an otherwise identical district where the Democrat barely held the seat.



**TABLE 9: SELECTION: THE EFFECT OF ELECTORAL TURNOVER ON ROLL-CALL VOTING POSITIONS**

	Democrat Replaced: $\Delta$ W-NOMINATE		Republican Replaced: $\Delta$ W-NOMINATE		Incumbent Replaced: Shift in Opponent's Direction	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.950*** (0.044) [0.000]	0.948*** (0.044) [0.000]	-0.910*** (0.048) [0.000]	-0.902*** (0.045) [0.000]	0.932*** (0.033) [0.000]	0.929*** (0.033) [0.000]
Election FE	Y	Y	Y	Y	-	-
Party $\times$ Election FE	-	-	-	-	Y	Y
Bandwidth	0.049	0.050	0.041	0.050	0.048	0.050
Effective Obs. Left	58	58	25	34	89	92
Effective Obs. Right	34	35	20	23	55	58
Control Mean	-0.078	-0.078	0.057	0.066	-0.074	-0.074
Observations	984	984	956	956	1940	1940

*Notes:* The Table reports effects of incumbents' seat loss on the shift in a district's roll call voting position in the subsequent Congress compared to the current congressional term, with roll call voting positions estimated by W-NOMINATE on roll call votes in regular sessions (omitting votes in the lame duck sessions). Outcome variables are the change in a district's W-NOMINATE scores from the current to the subsequent Congress if the seat is currently held by a Democratic incumbent (Columns 1 and 2) or a Republican incumbent (Columns 3 and 4). For the pooled sample analysis (Columns 5 and 6), changes in W-NOMINATE are multiplied by -1 if the seat is currently held by a Republican, such that estimated effects of turnover reflect a positional shift toward the current incumbent's opponent party. All results are based on local linear regressions specified in equation 2 using triangular kernel weights, and include *party  $\times$  election* fixed effects (Columns 5 and 6), respectively election fixed effects for split samples (Columns 1 to 4). Bandwidths in uneven columns are MSE-optimal, in even columns fixed to elections decided by a vote share margin smaller than 5%. All other notes as under Table 3. Standard errors clustered by House incumbent in parentheses: \*

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

Table 9 reports estimates of selection effects. Replacing a Democratic incumbent with a Republican challenger causes a conservative shift in a district's roll call voting record by 0.95 points on the W-NOMINATE scale (Columns 1 and 2). Conversely, selecting the Democratic challenger instead of reelecting a Republican incumbent leads to a liberal shift by approximately the same magnitude (Columns 3 and 4). On average, replacing the incumbent with the challenger of the opponent party moves a district's roll-call voting representation 0.93 points in the opponent's direction (Columns 5 and 6), compared to an otherwise identical district that retained its incumbent.<sup>27</sup>

Unsurprisingly, selection effects are large and exceed electoral incentive effects by almost one order of magnitude. The distance between two members of different parties representing otherwise comparable districts is 7.8 to 9.3 times larger than lame ducks' within-incumbent shift by 0.1 to 0.12 points on the W-NOMINATE scale. This does not mean, however, that electoral incentives are irrelevant. Without electoral competition incentivizing incumbents of both parties to adopt more moderate positions, the distance between them would increase by 0.2 to 0.24 points. Thus, my estimates imply that electoral incentives reduce polarization by 21 to 26%.

<sup>27</sup>To address concerns that changes in the W-NOMINATE scores from one Congress to the next are not comparable across members of different parties, Appendix Table A.4 replicates Table 9, additionally controlling for the election winner party's average shift between the two consecutive Congresses. Results are as good as identical.

## 7.2 Constitutional Design and Counterfactual Legislative Outcomes

### A. Aggregate Effects of Electoral Incentives

Electoral incentives induce individual legislators to strategically adjust their voting record to more moderate positions, which has important consequences for political representation at the district level. This naturally begs the question of whether electoral incentives' effects are large enough to affect aggregate legislative outcomes. To answer this question, I propose a simple framework, which uses the causally identified parameters to evaluate roll-call voting outcomes under plausible counterfactual scenarios.

To estimate aggregate counterfactual outcomes, I proceed in three steps. Separately for each voting proposal  $v$ , I first predict each legislator  $i$ 's *actual* probability of voting "yes" from a simple probit regression of a dummy = 1 if the legislator voted "yes" on her *actual* W-NOMINATE score (equation 4). Second, I use the vote-specific coefficients  $\beta_v$  obtained from the previous regression to predict legislators' *counterfactual* probability of voting yes from their counterfactual W-NOMINATE $_i^C$  (equation 5), which depends on the counterfactual scenario considered. Finally, I aggregate the counterfactually flipped individual votes by summing up the difference in probabilities to obtain the additional number of "yes" votes (equation 6), which then is added to the actual number of "yes" votes and subtracted from the actual number of "no" votes.

$$\hat{P}_i = \mathbb{P}(\text{yes}_i \mid \text{W-NOMINATE}_i) = \Phi(\text{W-NOMINATE}_i \beta_v) \quad (4)$$

$$\hat{P}_i^C = \mathbb{P}(\text{yes}_i \mid \text{W-NOMINATE}_i^C) = \Phi(\text{W-NOMINATE}_i^C \beta_v) \quad (5)$$

$$\sum_i (\hat{P}_i^C - \hat{P}_i) = \text{Additional number of yes votes} \quad (6)$$

### B. Counterfactual I: Abolishment of Lame-Duck Sessions

The first counterfactual scenario is motivated by ongoing debates on the democratic legitimacy of lame-duck lawmaking. A frequently raised concern about lame-duck sessions is that electorally unaccountable lame-duck members vote on issues without having to answer to voters. Indeed, my results suggest that lame ducks' lack of electoral incentives to moderate leads them to move toward extreme policy positions, in the direction opposite of what the pivotal voter (who selected the opponent candidate) would have preferred. These concerns are not new, but date back to the era before the adoption of the Twentieth Amendment to the U.S. Constitution in 1933, which de facto abolished regular lame-duck sessions until they became a new routine in the 1990s. Anecdotaly, it was lame-duck Republicans who were defeated in the 1922 elections and voted for

the highly unpopular Ship Subsidy Bill that motivated Senator George Norris to sponsor the bill that ultimately led to the Twentieth Amendment (Goodman and Nokken, 2004). Similar concerns resurfaced in 2010 after the lame-duck session of the 111<sup>th</sup> Congress, which included more than 50 House members who lost reelection and another 50 retiring members, and nevertheless passed several pieces of controversial landmark legislation (Nagle, 2011). Although no legislative proposals have been made to eliminate contemporary Congressional lame-duck sessions, similar proposals have been put forward in some state legislatures.<sup>28</sup>

I simulate a scenario in which lame-duck sessions were abolished by postponing Election Day closer to the date when newly elected members get seated, such that reelection-concerned legislators would still be incentivized to moderate their positions. Specifically, I use my estimated effect of lame-duck status on *roll call extremism* (Table 3, Column 1) and assume that lame-duck legislators in my sample would take more moderate positions by 0.1 points on the W-NOMINATE scale, imputing a 0.1 higher (lower) counterfactual W-NOMINATE score for Democrat (Republican) lame ducks in post-electoral sessions (equation 5).

I find that had lame ducks been electorally incentivized to moderate, 5 high-stakes legislative outcomes would have been flipped. The House of the 111<sup>th</sup> Congress would have rejected 3 roll call votes related to appropriation bills for infrastructure, military, and veteran affairs. Neither would the House have adopted a controversial Senate amendment to the DREAM Act concerning the rights of undocumented young immigrants. Additionally, in the lame-duck session of the 115<sup>th</sup> Congress, the House would not have adopted a resolution related to an appropriation bill for agricultural and food safety purposes (“Farm Bill of 2018”). The resolution provided for the consideration of the final version farm bill – which as such undisputed and had large bipartisan support in both chambers – but tied this provision to another provision that prevented the House for the rest of the Congress from taking action concerning the civil war and the humanitarian crisis in Yemen.

Of course, these results do not imply that the 111<sup>th</sup> Congress would have failed to approve any funding provided by the counterfactually rejected appropriation bills, nor does it mean that the House would have left unconsidered the Farm Bill of 2018. It is, however, unlikely that these measures would have been adopted in their current form. In the 111<sup>th</sup> Congress, the majority of Democrats would have needed to compromise on the spending bills to gain the support of their fiscally more conservative co-partisans, whereas the majority of Republicans in the 115<sup>th</sup> may have had to concede at least a debate on the situation in Yemen. In any case, the analysis suggests that (the loss of) electoral incentives to moderate positions by a large enough number of legislators has the potential to shape the legislative agenda and its output.<sup>29</sup>

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<sup>28</sup>see for example Michigan (<https://gophouse.org/rep-howell-plan-end-legislatures-lame-duck-sessions>) and Illinois ([https://www.ilhousegop.org/house\\_gop\\_seeks\\_end\\_to\\_lame\\_duck\\_sessions](https://www.ilhousegop.org/house_gop_seeks_end_to_lame_duck_sessions)).

<sup>29</sup>Unsurprisingly, the counterfactual only flips legislative outcomes of the lame-duck sessions in the 111<sup>th</sup> and 115<sup>th</sup>

### C. Counterfactual II: Redistricting to Competitive Districts

Lame-duck status after electoral loss is not the only source of lacking incentives to moderate. For legislators running for reelection in safe districts, the threat of losing office against a challenger of the opponent party is absent, as is the pre-election incentive to moderate. Increasing polarization in the U.S. Congress has been associated with an increasing number of safe seats and with redistricting practices undermining electoral competition and accountability (e.g., Grainger, 2010; Carson et al., 2014; Altman and McDonald, 2015). My finding that electoral incentives to moderate are strong for electorally vulnerable incumbents but absent for those representing relatively safe districts echoes these concerns (see Table 4).

Motivated by these results, I examine a counterfactual scenario in which redistricting assigns relatively safe incumbents to more competitive districts such that they become electorally vulnerable and thus incentivized to moderate positions. Specifically, I assume that one-quarter of the relatively *safe* incumbents in my sample – those with an expected margin of victory between 14.3% and 23% – to more competitive districts in which they are electorally *vulnerable* by the definition underlying the analysis in Table 4 (with an expected margin of victory  $< 8\%$ ). This modest redistricting reform would incentivize a total of 417 legislators in my sample (or 16% of the  $6 \times 435$  voting members in the 6 congresses I consider) to take more moderate positions prior to elections by 0.11 points on the W-NOMINATE scale, which corresponds to the difference in incentive effects estimated on *vulnerable* and *safe* incumbents (Table 4, Columns 2 and 6).

Had another 16% of the voting members in the 111<sup>th</sup> to the 116<sup>th</sup> Congresses been electorally vulnerable and incentivized to moderate positions strategically before elections, the outcomes of 77 roll call votes would have been flipped.<sup>30</sup> Among these, there are 6 (out of a total of 151) votes designated as key votes by the Congressional Almanac. For example, the majority of House Democrats of the 111<sup>th</sup> Congress would not have adopted the *American Clean Energy and Security Act* nor would it have passed the final versions of the *Preservation of Access to Care for Medicare Beneficiaries and Pension Relief Act* nor the *Affordable Care Act of 2010* (“Obamacare”). Conversely, the Republican-led House of the 115<sup>th</sup> Congress would not have passed *American Health Care Act of 2017*, which sought to partially repeal the *Affordable Care Act of 2010*. These results indicate that electoral incentives, along with the institutional reforms that create reelection concerns, have the potential to induce policy moderation at the legislative output level.

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Congress in which the number of lame-duck members was largest (see Table 1).

<sup>30</sup>To the extent that more competitive districts would not only create incentives but also select more moderate types of incumbents (see e.g., Jones and Walsh, 2018), my counterfactual analysis underestimates the overall effect of redistricting.

## 8 Conclusion

Elections have a duplicate purpose in representative democracies. On the one hand, voters select politicians who implement policies depending on their own convictions. On the other hand, recurrent elections provide incentives for elected politicians to keep policy aligned with voters' interests. Congressional lame-duck sessions offer a unique opportunity to disentangle electoral incentive effects and selection effects on public policy.

Addressing the problem of non-random selection into the last term, I leverage quasi-random assignment of close election losers to lame-duck status and compare them to co-partisans serving in the same Congressional term who narrowly won their reelection bid. I find that assignment to lame-duck status causes House members to take more extreme positions post elections, with lame-duck Democrats voting more liberally and lame-duck Republicans more conservatively. Remarkably, the result prevails under a variety of political constellations and is unlikely driven by the specificities of lame-duck sessions.

Consistent with lame ducks losing electoral incentives to moderate strategically, these effects are larger for ex-ante more vulnerable incumbents, for representatives who sought reelection in districts with a higher proportion of swing voters, and for legislators of retirement age who are least likely to rerun for office in the future. I find no evidence for several alternative mechanisms behind lame ducks' reversion to extreme voting positions, suggesting that the results can be interpreted as a reduced form estimate of a model in which lame-duck status affects legislators' voting behavior primarily through the loss of electoral incentives to moderate strategically.

I conclude that electoral incentives are an influential determinant of substantive representation and public policy. My analyses suggest that electoral incentives' effects are large enough to shape high-stakes legislative outcomes under plausible counterfactual scenarios. These findings have implications for constitutional design, directly speaking to current debates on abolishing the lame-duck period, and informing debates on redistricting, and on term limits which increase electoral turnover at the expense of accountability. While dominated by selection effects, electoral incentives are a strong moderating force, reducing polarization in competitive districts by 21 to 26%.

This finding not only contrasts with the view of ideologically rigid politicians whose policy positions are solely determined by convictions, but also contradicts the strict interpretation of the Downsian convergence mechanism, which would predict full convergence of office-seeking candidates' policy positions to the median voter's bliss point. However, it aligns with the broader notion that political competition incentivizes politicians to compromise on their private ideals, resulting in *partial convergence* toward the center. Importantly, my results show that the pressure to converge is limited to electorally competitive environments, indicating that the disappearance of competitive districts could be a fundamental driver behind the surge of polarization in Congress.

over the last decades. Understanding how district-level competition between candidates shapes legislators' policy positions, I suspect, would be a promising area for future research.

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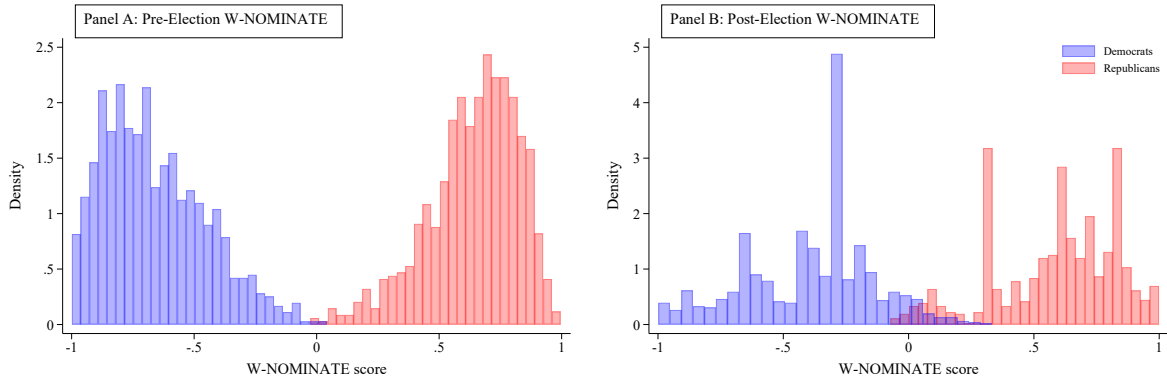


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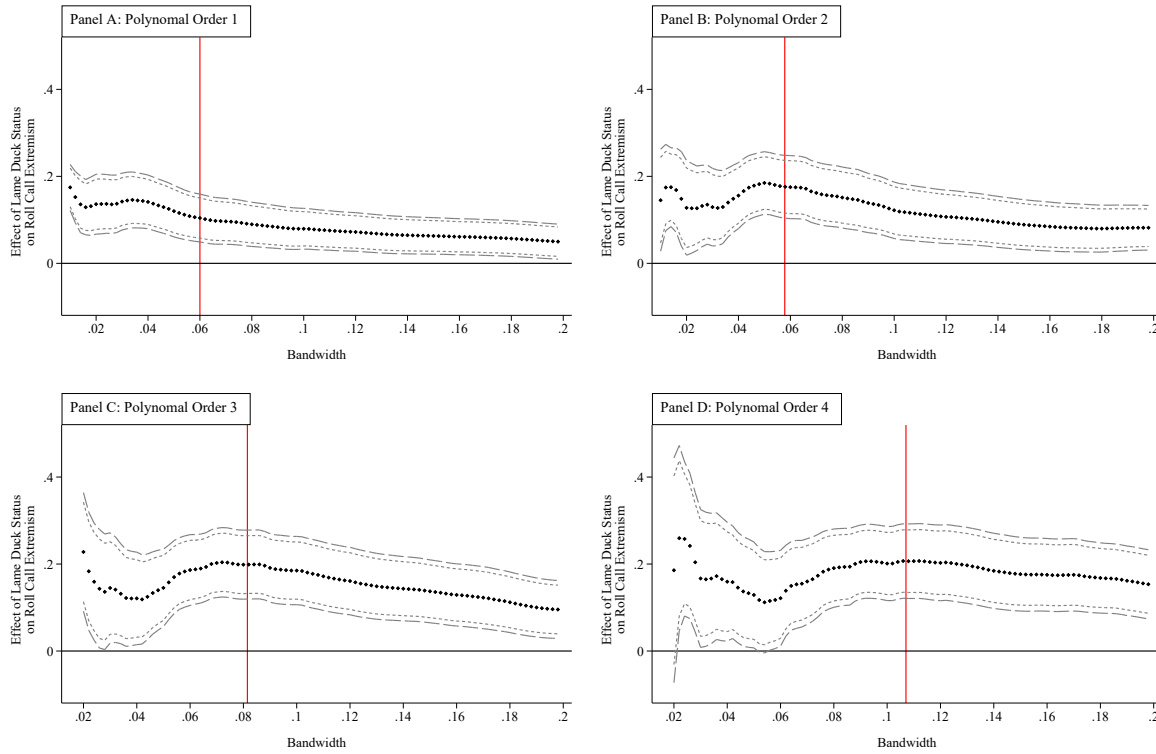
## Appendix

FIGURE A.1: DISTRIBUTION OF W-NOMINATE SCORES IN REGULAR AND LAME-DUCK SESSIONS



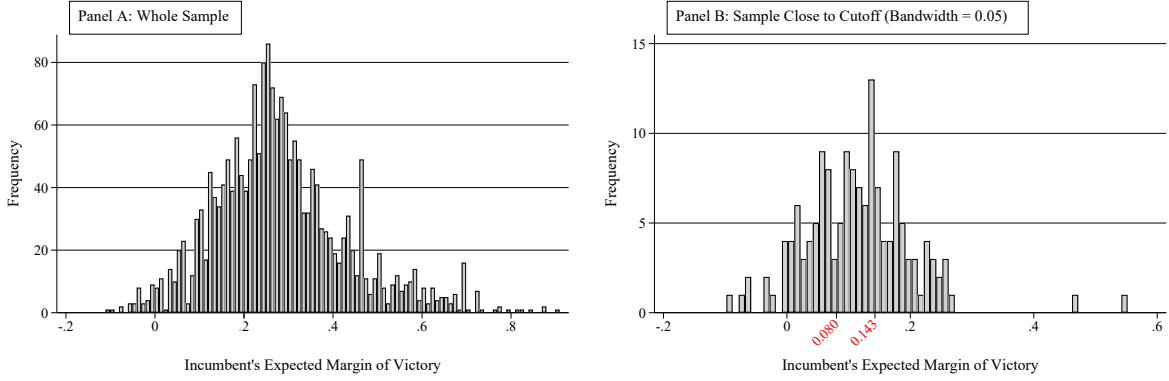
Notes: The Figure presents the sample distributions of roll-call voting positions in regular sessions (Panel A) and post-electoral lame-duck sessions (Panel B). Positions are estimated by extracting the first dimension of W-NOMINATE scores, estimated separately by  $congress \times session$  using the R implementation of the W-NOMINATE algorithm (Poole et al., 2011). First-dimensional W-NOMINATE scores range from -1 (most liberal) to +1 (most conservative). The sample includes 1954 re-election seeking House incumbents in the 111<sup>th</sup> to the 116<sup>th</sup> Congresses whose roll call voting record can be scaled separately before and after general elections 2010-2020.

**FIGURE A.2: EFFECT OF LAME-DUCK STATUS ON CHANGE IN INCUMBENT’S ROLL CALL EXTREMISM: ROBUSTNESS TO DIFFERENT BANDWIDTHS AND POLYNOMIALS**



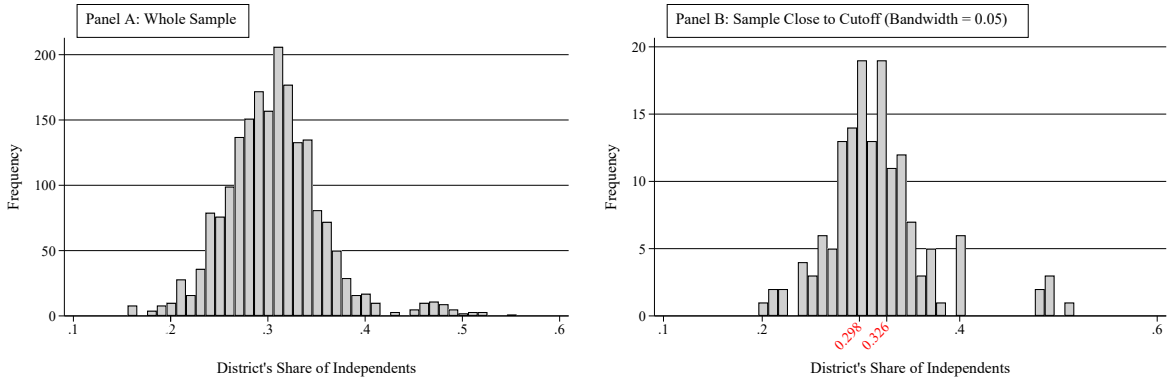
*Notes:* The Figure plots discontinuity estimates (black dots) for the effect of lame duck status the change in House incumbents' *Roll Call Extremism* from the regular sessions before general elections to the lame duck session after elections for different bandwidths and polynomials. Bandwidths range from 0.01 to 0.2 in local linear (Panel A) and local quadratic (Panel B) specifications of equation 2, respectively from 0.02 to 0.2 for cubic and quartic specifications (Panels C and D). All regressions use triangular kernel weights and include *party*  $\times$  *congress* fixed effects. 95% (dashed grey lines) and 90% (dotted grey lines) confidence intervals account for clustering House representatives.

**FIGURE A.3: DISTRIBUTION OF INCUMBENTS' EXPECTED MARGIN OF VICTORY**



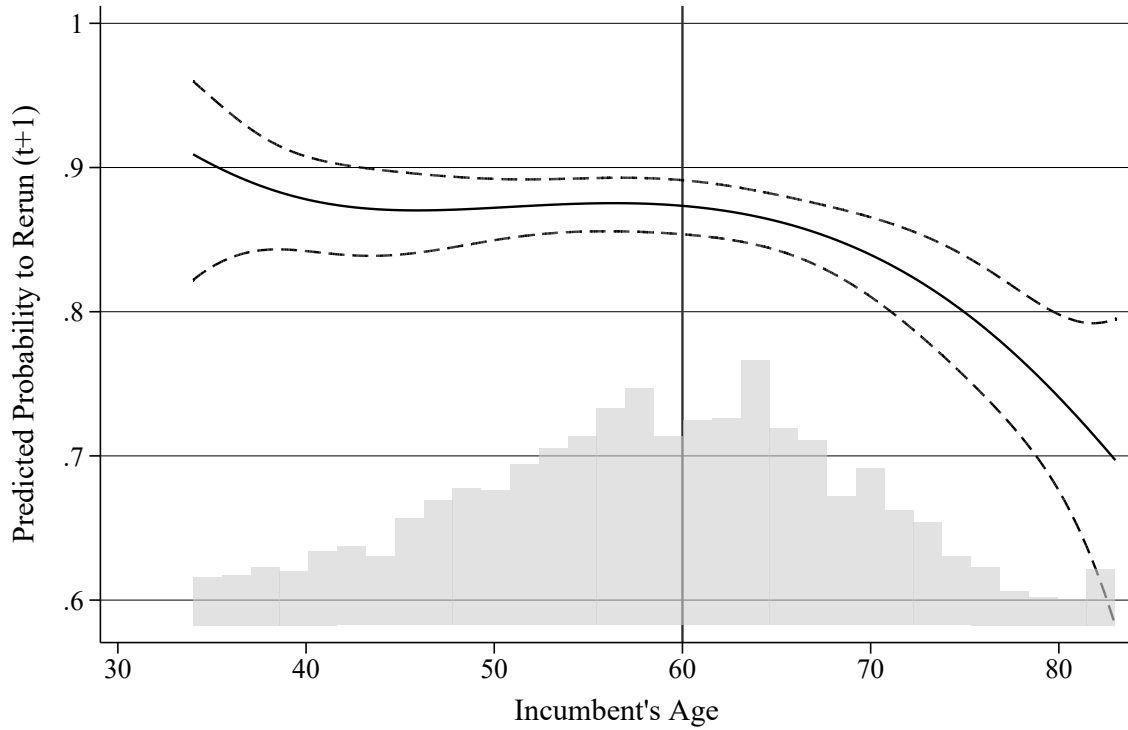
*Notes:* The Figure presents the sample distributions of the *Incumbents' Expected Margin of Victory*. *Incumbents' Expected Margin of Victory* are the fitted values from a linear regression of the incumbent's actual vote share margin relative to their strongest opponent on the incumbent's lagged vote share interacted with *congress*  $\times$  *party* fixed effects. Panel A shows the distribution in the full sample, and Panel B the distribution within a 0.05 bandwidth around the cutoff value of a zero *actual* vote share margin. Values in red indicate the thresholds between the first and second, respectively the second and third terciles underlying the analysis of expected "toss-up", "competitive", and "safe" re-election bids in Table 4.

**FIGURE A.4: DISTRIBUTION OF DISTRICTS' SHARE OF INDEPENDENTS**



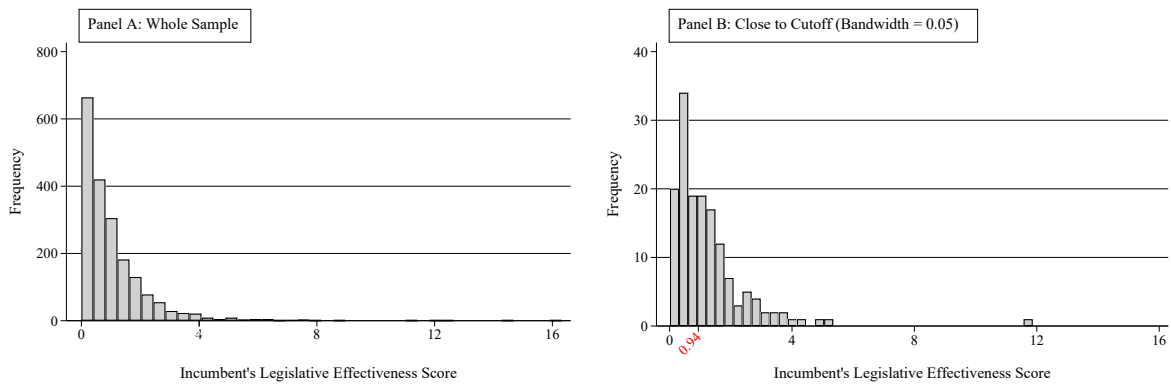
*Notes:* The Figure presents the sample distributions of the *Districts' Share of Independent Voters* as proxied by the share of respondents in the CCES pre-election surveys (2006-2020) that identify as independents rather than as Republicans or Democrats. Panel A shows the distribution in the full sample, and Panel B the distribution within a 0.05 bandwidth around the cutoff value of a zero vote share margin. Values in red indicate the thresholds between the first and second, respectively the second and third terciles underlying the analysis of "partisan", "moderate", and "swing" districts in Table 5.

FIGURE A.5: PREDICTED PROBABILITY TO RERUN BY AGE



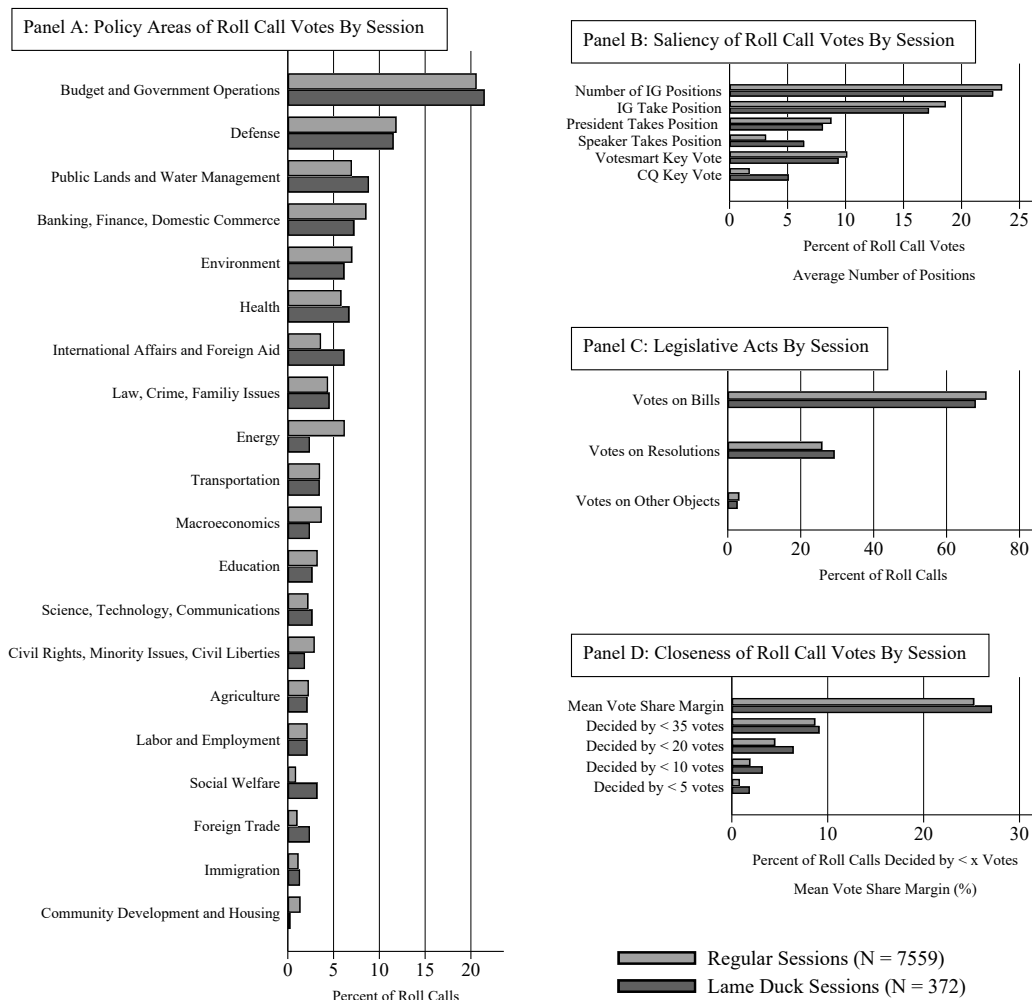
*Notes:* The Figure presents the incumbent's probability to rerun for any federal, state, or local elected office in the subsequent electoral cycle predicted from probit regression on a fourth-order polynomial of incumbents' age, along with 95% confidence intervals accounting for clustering at the level of House representatives. The underlying histogram represents the age distribution of the 1959 House incumbents in the sample, winsorized at the 99<sup>th</sup> (83 years) and 1<sup>st</sup> percentile (34 years). The 62-year threshold depicts the minimum age requirement for a full and immediate pension as a former member of Congress who served at least 5 years.

**FIGURE A.6:** DISTRIBUTION OF INCUMBENTS' LEGISLATIVE EFFECTIVENESS SCORE



*Notes:* The Figure presents the sample distributions of the incumbents' term-specific legislative effectiveness score (Volden and Wiseman, 2014). Panel A shows the distribution in the full sample, and Panel B the distribution within a 0.05 bandwidth around the cutoff. The value in red indicates the median legislative effectiveness score for the sample split underlying the analysis in Table 8

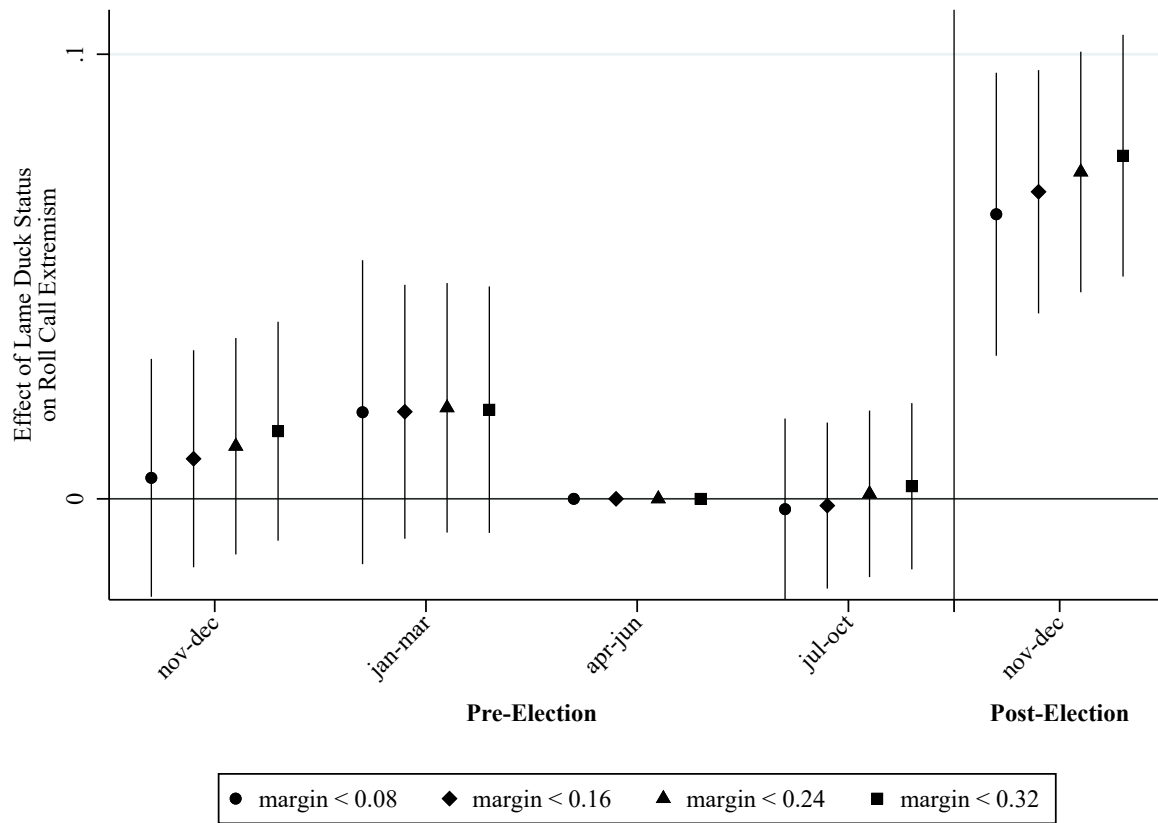
**FIGURE A.7: POLICY ISSUES, SALIENCY, OBJECTS, AND CLOSENESS OF ROLL CALL VOTES IN LAME-DUCK AND REGULAR SESSIONS**



*Notes:* The Figure presents descriptive statistics of roll call votes held during the regular pre-election sessions of (light grey) and post-electoral lame-duck sessions of the 111<sup>th</sup> to 116<sup>th</sup> Congresses (2009-2020). Panel A presents the distribution of roll call votes across policy areas by session, reporting the session-specific percentage share of roll calls assigned to one 20 major topic areas as classified by the *comparative agendas project* (Wilkerson et al., 2023). Panel B shows properties that proxy for roll call votes' saliency or "importance", by session: the average number of interest groups taking a position on a roll call vote (conditional on any interest group taking a position) according to data collected by *maplight.org*, the percent of votes on which at least one interest group takes a position according to *maplight.org*, the percent of votes on which the U.S. President takes a position or on which the House Speaker votes according to *voteview.com* (Lewis et al., 2022), or on which the House speaker votes, and the percent of votes that are classified as a "key vote" by *votesmart.org* or by the *Congressional Quarterly*. Panel C reports the percentage share of roll calls by session and legislative acts, i.e. votes on bills, resolutions, or on any other subject (e.g., elections, nominations, procedural votes). Panel D presents session-specific measures of the closeness of roll call votes: the average vote share margin of the majority in a given roll call, and the share of roll calls decided by a margin of less than 35, 20, 10, or 5 individual votes.



**FIGURE A.8: THE EFFECT OF LAME-DUCK STATUS ON INCUMBENT'S ROLL CALL EXTREMISM: EVENT-STUDY ESTIMATES UNADJUSTED FOR PRE-TRENDS**



*Notes:* The Figure plots dynamic difference-in-differences estimates of the effect of lame duck status on *Roll Call Extremism* in the four quarters of the year preceding the general election and the lame-duck period after elections, with the second quarter of the election year (April-June) as the omitted category of reference. The sample is restricted to incumbents reelected by a vote share margin of less than 20% (control group), and incumbents who lost reelection by a margin of less than 8% (circles), 16% (diamonds), 24% (triangles), or 32% (squares). All estimates are conditional on Member  $\times$  Congress fixed effects and Congress  $\times$  Party  $\times$  Period fixed effects. 95% confidence intervals (spikes) account for clustering by House representatives.

**TABLE A.1:** THE EFFECTS OF LAME-DUCK STATUS ON THE CHANGE IN ROLL CALL EXTREMISM: ROBUSTNESS TO HIGHER-ORDER POLYNOMIALS, ALTERNATIVE KERNEL WEIGHTS, AND COVARIATE ADJUSTMENT

	Polynomial 1		Polynomial 2		Polynomial 3		Polynomial 4	
PANEL A: TRIANGULAR KERNEL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.099*** (0.028) [0.001]	0.105*** (0.020) [0.000]	0.176*** (0.037) [0.000]	0.159*** (0.026) [0.000]	0.199*** (0.040) [0.000]	0.172*** (0.032) [0.000]	0.207*** (0.044) [0.000]	0.175*** (0.036) [0.000]
Bandwidth	0.064	0.064	0.058	0.058	0.081	0.081	0.107	0.107
Effective Observations	197	194	182	179	241	237	325	319
PANEL B: UNIFORM KERNEL								
	0.071*** (0.025) [0.008]	0.081*** (0.023) [0.001]	0.141*** (0.040) [0.001]	0.143*** (0.032) [0.000]	0.226*** (0.048) [0.000]	0.186*** (0.042) [0.000]	0.226*** (0.048) [0.000]	0.196*** (0.043) [0.000]
Bandwidth	0.079	0.079	0.068	0.068	0.069	0.069	0.091	0.091
Effective Observations	236	232	209	205	213	209	267	263
PANEL C: EPANECHNIKOV KERNEL								
	0.090*** (0.027) [0.002]	0.100*** (0.022) [0.000]	0.184*** (0.038) [0.000]	0.166*** (0.029) [0.000]	0.212*** (0.043) [0.000]	0.181*** (0.036) [0.000]	0.208*** (0.046) [0.000]	0.172*** (0.040) [0.000]
Bandwidth	0.066	0.066	0.055	0.055	0.075	0.075	0.103	0.103
Effective Observations	208	204	179	176	224	220	309	303
Observations	1959	1928	1959	1928	1959	1928	1959	1928
Party $\times$ Congress FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-Election Outcome	N	Y	N	Y	N	Y	N	Y
Covariates	N	Y	N	Y	N	Y	N	Y

*Notes:* The Table presents results from local polynomial regressions, probing robustness of the main results reported in Table 3 to including higher polynomial orders of the assignment variable (columns). to alternative kernel weights (panels), and to covariate adjustment (uneven columns). Columns 2, 4, and 6, adjust for all covariates listed in Figure 2, excluding the predicted change in roll call extremism but including the level of the pre-election outcome variable. The bandwidths for covariate-adjusted estimation are fixed at the MSE-optimal bandwidth for the corresponding baseline specifications in Columns 1, 3, and 5. All other notes as under Table 3. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

TABLE A.2: TESTS FOR COVARIATE BALANCE

PANEL A: DISTRICT CHARACTERISTICS							
	Discontinuity	Std. Error	Robust P-Value	Bandwidth	Eff. Obs.	Total Obs.	Sample Mean
District Population [log]	0.001	0.029	0.924	0.070	210	1931	13.494
Share Black [%]	-0.001	0.021	0.771	0.068	206	1931	0.135
Share White [%]	-0.011	0.029	0.808	0.084	242	1931	0.763
Age Over 65 [%]	0.005	0.009	0.518	0.094	276	1931	0.141
Age Between 18 and 34 [%]	0.009	0.007	0.158	0.051	155	1931	0.222
Not U.S. Citizen [%]	0.022	0.014	0.184	0.090	263	1931	0.069
No High School Degree [%]	0.029	0.023	0.261	0.088	262	1959	0.129
No College Degree [%]	0.020	0.033	0.506	0.081	239	1959	0.608
Below Poverty Line [%]	0.020	0.017	0.241	0.068	206	1931	0.144
Median Household Income [1000 USD]	-7.017	5.195	0.177	0.077	231	1931	64.144
Income Inequality [gini index]	0.004	0.008	0.816	0.084	244	1931	0.457
Share Independents [%]	0.005	0.012	0.652	0.090	266	1959	0.306
PANEL B: INCUMBENT CHARACTERISTICS							
Tenure [terms served]	-0.424	1.005	0.686	0.075	224	1959	5.354
Age [years]	-1.187	2.943	0.706	0.084	246	1959	58.591
White [dummy]	-0.079	0.110	0.616	0.094	279	1959	0.810
Female [dummy]	-0.021	0.143	0.878	0.051	158	1959	0.197
Vote Share t-1 [%]	-0.022	0.015	0.178	0.063	197	1959	0.656
Legislative Effectiveness [std.]	0.172	0.320	0.577	0.092	268	1956	0.000
Pre-Election Roll Call Extremism	-0.062	0.042	0.144	0.074	223	1959	0.650
Pre-Election Party Loyalty [%]	-0.006	0.017	0.695	0.078	235	1959	0.902
Pre-Election Absenteeism [%]	-0.012	0.008	0.095	0.059	183	1959	0.031
PANEL C: JOINT TEST							
Predicted Change in Roll Call Extremism	0.003	0.031	0.783	0.075	222	1928	-0.163

Notes: The Table presents balancing checks on district (Panel A) and incumbent characteristics (Panel B). Each row reports the discontinuity estimated by a local-linear regression with MSE-optimal bandwidth and triangular kernels, along with standard errors, robust p-values from bias-adjusted estimates, the optimal bandwidth, the effective and total number of observations as well as the sample mean of the covariate. Panel C performs a joint test by evaluating the discontinuity in the *predicted* change of roll call extremism, i.e., the fitted values from a linear regression of the actual change in roll call extremism on all incumbent and district characteristics listed in Panels A and B.

**TABLE A.3: THE EFFECTS OF LAME-DUCK STATUS ON THE PROBABILITY TO RERUN FOR ELECTED OFFICE**

	(1)	(2)	(3)	(4)
	-0.353** (0.145) [0.056]	-0.362** (0.159) [0.133]	-0.355*** (0.111) [0.008]	-0.343*** (0.116) [0.001]
Party $\times$ Congress FE	Y	Y	Y	Y
Covariates	N	N	Y	Y
Bandwidth	0.059	0.050	0.065	0.050
Effective Obs. Left	111	94	120	92
Effective Obs. Right	71	58	76	57
Control Mean	0.829	0.830	0.836	0.830
Observations	1959	1959	1928	1928

Notes: The Table presents results from local linear regressions specified in equation 2, reporting the effect of lame-duck status on the probability to rerun for elected office. The outcome variable is a dummy equal to 1 if the incumbent in the subsequent electoral cycle reruns for any elected office, including for the House, the Senate, for Governor, or for lower office (e.g., mayor). The bandwidths are MSE-optimal in Columns 1 and 3, and restricted to elections decided by a margin of less than 5% in Columns 2 and 4. All regressions use triangular kernel weights, and include *party  $\times$  congress* fixed effects. Columns 3 and 4 additionally control for the full set of covariates listed in Table A.2, Panels A and B. Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Control Mean reports the average probability to rerun for incumbents within the bandwidth left to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

**TABLE A.4: SELECTION: THE EFFECT OF ELECTORAL TURNOVER ON ROLL-CALL VOTING POSITIONS – ROBUSTNESS**

	Democrat Replaced: $\Delta$ W-NOMINATE		Republican Replaced: $\Delta$ W-NOMINATE		Incumbent Replaced: Shift in Opponent's Direction	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.955*** (0.045) [0.000]	0.953*** (0.045) [0.000]	-0.958*** (0.049) [0.000]	-0.940*** (0.046) [0.000]	0.943*** (0.034) [0.000]	0.939*** (0.033) [0.000]
Election FE	Y	Y	Y	Y	-	-
Party $\times$ Election FE	-	-	-	-	Y	Y
Average Shift of Winner's Party	Y	Y	Y	Y	Y	Y
Bandwidth	0.049	0.050	0.041	0.050	0.048	0.050
Effective Obs. Left	58	58	25	34	89	92
Effective Obs. Right	34	35	20	23	55	58
Control Mean	-0.078	-0.078	0.057	0.066	-0.074	-0.074
Observations	984	984	956	956	1940	1940

Notes: The Table replicates the results in Table 9, controlling for the average positional shift of the election winner's party from the current to the subsequent Congress, calculated as the average change of the outcome variable of the party's members elected to both Congresses. All other notes as under Table 9. Standard errors clustered by House incumbent in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

**TABLE A.5:** THE EFFECTS OF LAME-DUCK STATUS ON CHANGES IN ROLL CALL EXTREMISM ACROSS CONGRESSES

	111 <sup>th</sup> – 113 <sup>th</sup> Congress (2010-2014 elections)		114 <sup>th</sup> – 116 <sup>th</sup> Congress (2016-2020 elections)		111 <sup>th</sup> – 112 <sup>th</sup> Congress (2010-2012 elections)		113 <sup>th</sup> – 114 <sup>th</sup> Congress (2014-2016 elections)		115 <sup>th</sup> – 116 <sup>th</sup> Congress (2018-2020 elections)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	0.142*** (0.036) [0.000]	0.146*** (0.039) [0.001]	0.144** (0.056) [0.001]	0.095** (0.046) [0.001]	0.125*** (0.041) [0.002]	0.133*** (0.044) [0.003]	0.168*** (0.049) [0.002]	0.171*** (0.054) [0.002]	0.146** (0.057) [0.001]	0.093* (0.052) [0.001]
Party × Congress FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bandwidth	0.066	0.050	0.026	0.050	0.071	0.050	0.038	0.050	0.026	0.050
Effective Obs. Left	57	44	18	50	45	34	14	17	13	43
Effective Obs. Right	47	35	17	23	40	27	10	11	16	20
Observations	1006	1006	953	953	687	687	647	647	625	625

*Notes:* The Table presents results from local linear regressions specified in equation 2 for different subsamples of legislators serving in the 111<sup>th</sup> – 113<sup>th</sup> (Columns 1 and 2) and the 114<sup>th</sup> – 116<sup>th</sup> Congresses (Columns 3 and 4), respectively in the 111<sup>th</sup> – 112<sup>th</sup> (Columns 5 and 6), the 113<sup>th</sup> – 114<sup>th</sup> (Columns 7 and 8), and the 115<sup>th</sup> – 116<sup>th</sup> (Columns 9 and 10). Bandwidths are MSE-optimal in uneven columns, and fixed to elections decided by a margin of less than 5% in even columns. All regressions include *party* × *congress* fixed effects and use triangular kernel weights. Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

**TABLE A.6: THE EFFECTS OF LAME-DUCK STATUS ON CHANGES IN ROLL CALL EXTREMISM DEPENDING ON PARTYS' CURRENT MAJORITY STATUS**

	Current Majority		Current Minority	
	(1)	(2)	(3)	(4)
	0.154*** (0.037) [0.000]	0.125*** (0.032) [0.000]	0.125 (0.099) [0.274]	0.114 (0.091) [0.000]
Party $\times$ Congress FE	Y	Y	Y	Y
Bandwidth	0.029	0.050	0.064	0.050
Effective Obs. Left	34	70	28	24
Effective Obs. Right	31	48	10	10
Observations	1167	1167	792	792

*Notes:* The Table presents results from local linear regressions specified in equation 2 for two subsamples of legislators whose party holds the majority (Columns 1 and 2) or the minority of House seats (Columns 3 and 4) in the current Congress. Bandwidths are MSE-optimal in uneven columns, and fixed to elections decided by a margin of less than 5% in even columns. All regressions include *party  $\times$  congress* fixed effects and use triangular kernel weights. Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

**TABLE A.7:** THE EFFECTS OF LAME-DUCK STATUS ON CHANGES IN ROLL CALL EXTREMISM DEPENDING ON PARTYS' PROSPECTIVE MAJORITY STATUS

	Prospective Majority		Prospective Minority	
	(1)	(2)	(3)	(4)
	0.168*** (0.053) [0.001]	0.133** (0.054) [0.002]	0.104*** (0.034) [0.006]	0.110*** (0.037) [0.000]
Party $\times$ Congress FE	Y	Y	Y	Y
Bandwidth	0.029	0.050	0.057	0.050
Effective Obs. Left	15	32	74	62
Effective Obs. Right	13	23	46	35
Observations	1014	1014	945	945

*Notes:* The Table presents results from local linear regressions specified in equation 2 for two subsamples of legislators whose party won the majority (Columns 1 and 2) or the minority of House seats (Columns 3 and 4) in the subsequent Congress. Bandwidths are MSE-optimal in uneven columns, and fixed to elections decided by a margin of less than 5% in even columns. All regressions include *party  $\times$  congress* fixed effects and use triangular kernel weights. Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.

**TABLE A.8: THE EFFECTS OF LAME-DUCK STATUS ON CHANGES IN ROLL CALL EXTREMISM DEPENDING ON THE CHANGE PARTYS' MAJORITY STATUS**

	No Change in Majority Status		From Minority to Majority Status		From Majority to Minority Status	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.101**	0.095**	0.290**	0.079	0.176***	0.159***
	(0.046)	(0.044)	(0.125)	(0.089)	(0.055)	(0.054)
	[0.048]	[0.029]	[0.206]	[0.058]	[0.002]	[0.005]
Party $\times$ Congress FE	Y	Y	Y	Y	Y	Y
Bandwidth	0.043	0.050	0.024	0.050	0.045	0.050
Effective Obs. Left	42	53	7	20	21	21
Effective Obs. Right	30	33	6	10	12	15
Observations	1295	1295	330	330	334	334

*Notes:* The Table presents results from local linear regressions specified in equation 2 for three subsamples of legislators i) whose party won the majority of House seats for the next Congress but currently holds the minority of seats (Columns 3 and 4), ii) whose party currently holds the majority of seats but transitions from majority to minority in the subsequent Congress (Columns 5 and 6), iii) whose party's majority status remains unchanged (Columns 1 and 2). Bandwidths are MSE-optimal in uneven columns, and fixed to elections decided by a margin of less than 5% in even columns. All regressions include *party  $\times$  congress* fixed effects and use triangular kernel weights. Effective Observations are the number of incumbents within the bandwidth left, respectively right to the cutoff. Standard errors clustered by House representative in parentheses: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust p-values based on bias-adjusted estimates in brackets.



TABLE A.9: THE EFFECT OF LAME DUCK STATUS ON ROLL CALL EXTREMISM: EVENT-STUDY ESTIMATES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lame Duck $\times$ Nov-Dec Before Election	-0.0133 (0.0136) [0.606]	-0.0112 (0.0124) [0.814]	-0.0088 (0.0124) [0.835]	-0.0061 (0.0125) [0.831]	0.0047 (0.0136) [0.761]	0.0090 (0.0124) [0.618]	0.0118 (0.0124) [0.544]	0.0152 (0.0125) [0.373]
Lame Duck $\times$ Jan-Mar Before Election	0.0105 (0.0174) [0.742]	0.0095 (0.0145) [0.710]	0.0101 (0.0143) [0.686]	0.0094 (0.0141) [0.703]	0.0195 (0.0174) [0.562]	0.0196 (0.0145) [0.423]	0.0205 (0.0143) [0.425]	0.0200 (0.0141) [0.426]
Lame Duck $\times$ Jul-Oct Before Election	0.0067 (0.0104) [0.406]	0.0086 (0.0095) [0.413]	0.0114 (0.0095) [0.355]	0.0135 (0.0095) [0.201]	-0.0023 (0.0104) [0.820]	-0.0015 (0.0095) [0.834]	0.0011 (0.0095) [0.890]	0.0028 (0.0095) [0.771]
Lame Duck $\times$ Nov-Dec After Election	0.0820*** (0.0162) [0.021]	0.0893*** (0.0139) [0.021]	0.0942*** (0.0138) [0.021]	0.0985*** (0.0138) [0.018]	0.0640*** (0.0162) [0.024]	0.0691*** (0.0139) [0.024]	0.0735*** (0.0138) [0.024]	0.0772*** (0.0138) [0.021]
Joint Significance of Leads ( <i>p-value</i> )	0.415 [0.037]	0.362 [0.678]	0.306 [0.638]	0.290 [0.597]	0.601 [0.710]	0.418 [0.874]	0.396 [0.899]	0.357 [0.887]
Sample Restriction: Lost By Margin $< x\%$	8	16	24	32	8	16	24	32
# Lame Ducks	83	120	125	127	83	120	125	127
# Reelected	549	549	549	549	549	549	549	549
Observations	3145	3330	3355	3365	3145	3330	3355	3365
R-squared	0.913	0.917	0.917	0.917	0.916	0.918	0.918	0.918
Pre-Trend-Adjusted	Y	Y	Y	Y	N	N	N	N
Member $\times$ Congress FE	Y	Y	Y	Y	Y	Y	Y	Y
Congress $\times$ Party $\times$ Period FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The Table reports dynamic difference-in-differences estimates of the effect of Lame Duck status on *Roll Call Extremism* in periods before and after the general election, with the second quarter of the election year (April-June) as omitted category of reference. The sample is restricted to incumbents reelected by a vote share margin of less than 20% (control group), and incumbents who lost reelection by a margin of less than 8% (Columns 1 and 5), 16% (Columns 2 and 6), 24% (Columns 3 and 7), or 32% (Columns 4 and 8). The highest margin in the sample by which an incumbent in lost re-election is 31.12%. Estimates in Columns 1 to 4 are adjusted for pre-trends following Goodman-Bacon (2021). All regressions control for the full set of *member  $\times$  congress* fixed effects and *congress  $\times$  party  $\times$  period* fixed effects. Standard errors clustered by House representative in parentheses: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . P-values associated to wild cluster bootstrapped confidence intervals accounting for clustering at the Congress-level in brackets. Joint Significance of Leads reports the p-value of an F-test that the coefficients measuring pre-election differences in *Roll Call Extremism* are all equal to zero. P-values of analogous Wald tests based on the wild cluster bootstrap at the Congress-level in brackets.