OUT OF OFFICE, OUT OF STEP? ELECTORAL INCENTIVES AND STRATEGIC MODERATION IN THE U.S. HOUSE OF REPRESENTATIVES[†]

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Abstract

Do elections constrain incumbent politicians' policy choices? To answer this longstanding question, this paper proposes a novel identification strategy to separate electoral incentives from selection effects. Taking advantage of the unique setup of lame-duck sessions in the U.S. Congress, where lame-duck incumbents who lost re-election vote on the same issues as their re-elected colleagues, I use a close election regression discontinuity design to exploit quasi-random assignment of re-election 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 re-elected 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 ex-ante more vulnerable legislators. I also consider, but ultimately dismiss, several other mechanisms including emotional backlash, logrolling motives, party control, and selective abstention.

Keywords: Elections, Electoral Incentives, Accountability, Legislator Behavior, Polarization
JEL Classification Codes: D72, P0, J45

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.

— 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.

— Barack Obama (2014), Speech in Austin, Texas

1   Introduction

As the founders of the U.S. Constitution noted, there are two 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 of officeholders maintain ex-post *accountability* to voters. The notion that elections discipline officeholders is a core principle of representative democracy.

Unsurprisingly, re-election constraints feature prominently in the theoretical political economy literature on political agency problems starting with Barro (1973) and Ferejohn (1986).[1] 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 legislators to represent the interest of voters, which by a Downsian logic would lead to the implementation of more moderate policies to please the median voter (Downs, 1957). However, the effectiveness of electoral incentives in keeping incumbents' policy choices aligned with voter preferences has been questioned on accounts of i) voters being (rationally) inattentive and hence uninformed about politicians' policy decisions (e.g., Miller and Stokes, 1963),[2] ii) voters' inability

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

[2] 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).

to pre-commit to an effective punishment mechanism when facing a trade-off between selection and control (e.g., Banks and Sundaram, 1993; Fearon, 1999),[3] iii) or on doubts about politicians' ability and willingness to credibly commit to policy platforms other than their own ideological ideal (Alesina, 1988; Osborne and Slivinski, 1996; Besley and Coate, 1997).[4] Given theoretical ambiguity, whether re-election concerns constrain politicians' policy choices is an empirical question that goes to the core of constitutional design. While structural estimates of political agency models find large effects of re-election concerns (Sieg and Yoon, 2017; Aruoba et al., 2019), in particular on roll call voting moderation of U.S. senators facing close re-election bids (Iaryczower et al., 2022), we lack credibly identified quasi-experimental evidence that supports these results.

The key identification challenge is to separate electoral incentives from the selection mechanism. Ample 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. Indeed, a large literature on ideological shirking in Congress typically finds that legislators' roll call voting does not change after they announce retirement.[5] These non-findings are consistent with other accounts of legislators' ideological rigidity (Lee et al., 2004; Poole, 2007), but also with electoral selection over time such that only types who closely match their voters' preferences survive in office until retirement age, or with downward biased estimates due to anticipation effects as incumbents likely decide on retirement before announcing it. More generally, the challenge in establishing a causal nexus between electoral incentives and last-term behavior stems from the difficulty that politicians serving their last term may systematically differ from those vying for re-election. While self-selection is most evident when (perhaps strategic) retirement is under perfect control of the politician, selection also hampers identification of incentive effects in studies exploiting constitutional term limits as an "exogenous" removal of

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[3]If politicians are of the same type and there is no scope for selection, voters can condition their voting rule on incumbent performance to control politicians' incentives. However, if politicians differ in type, voters face a trade-off between setting incentives and selection, i.e., between rewarding incumbent performance with re-election and replacing the incumbent with a higher-quality challenger. Without a credible commitment to retain a well-performing incumbent, voters may renege and replace the incumbent anyway, which in turn weakens the incumbent's incentives not to shirk.

[4]As shown in Alesina (1988), there is no scope for strategic position taking in a one-shot game without an exogenous commitment device; in a repeated game, strategic moderation of policy positions can be obtained as an equilibrium outcome only if politicians have long enough horizons and only weakly discount the stream of future rents from office.

[5]The voluminous observational literature documenting no association between retirement decisions and ideological shirking in the U.S. Congress includes but is not limited to Lott (1987), Poole and Romer (1993), Bronars and Lott Jr (1997), Stratmann (2000), see also Lott and Davis (1992) for a review of the literature. An exception is Rothenberg and Sanders (2000) who find a negligibly small effect of retirement on the change in absolute change in W-NOMINATE scores, which has no directional interpretation and vanishes upon the inclusion of congress fixed effects to adjust for the non-comparability of W-NOMINATE scores across congresses (Carson et al., 2004). Another 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 facing likely defeat, perhaps because of their changing voting record.

re-election concerns (Besley and Case, 1995; List and Sturm, 2006; Alt et al., 2011; Ferraz and Finan, 2011; Lopes da Fonseca, 2020). If voters use elections as a selection mechanism, termed-out politicians who survived enough elections to hit the binding term limit differ from those who are eligible for re-election not only by the absence of electoral incentives and higher office experience but also along other dimensions that affect behavior in office.[6] Improving on identification by cross-incumbent comparisons in the previous term-limits literature, Fouirnaies and Hall (2022) consider within-incumbent changes in the performance of U.S. state legislators hitting binding term limits. Employing a difference-in-differences strategy to keep individual politicians' type fixed, they compare the last-term behavior of termed-out legislators to their own behavior before hitting the term limit, relative to counterfactual changes among legislators serving in the same chamber who remain eligible for re-election. While showing that termed-out legislators exert less legislative effort (in terms of floor attendance, bill sponsorship, committee service), Fouirnaies and Hall (2022) identify a precisely estimated null effect of electoral incentives on state legislators' roll call voting position on the liberal-conservative scale.[7]

This paper takes another approach. Specifically, I consider contemporary lame-duck sessions in the U.S. House of Representatives. Congressional lame-duck sessions occur in the two months between the general elections in November and the January inauguration when newly elected members take office. During this transition period, Congress adjourns in its old composition, including lame-duck officials who retire from office or lost their re-election bid. As lame ducks retain all their powers during this period, this unique institutional setup allows observing both re-elected incumbents and lame ducks, who are freed from re-election 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 re-election 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 re-election bid to barely re-elected 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

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[6]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) tackle the selection issue in a different way, comparing 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.

[7]Aidt and Shvets (2012) use a similar design to estimate last-term effects on pork barrel spending rather than ideological shirking.

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to co-partisans in the same term (by conditioning on a full set of *party* × *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 general elections.

Using this design, I document substantial effects of lame-duck status on legislators' roll call voting records. Consistent with electoral incentives constraining incumbents to compromise toward moderate policy, I find that 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 co-partisans re-elected to the next Congress. Estimated causal effects of lame-duck status on incumbents' roll call voting imply a shift toward more extreme positions by 0.1 units of the W-NOMINATE score, which ranges from -1 (very liberal) to +1 (very conservative). The implied shift thus corresponds to 5% of the distance between the most conservative Republican and the most liberal Democratic House member, or to approximately the average distance House members to their own party's median in the 116th Congress (2019-2021). A simple back-of-the-envelope calculation to gauge the relative importance of incentive and selection effects suggests that estimated incentive effects account for 17% of the distance between a barely ousted incumbent and her newly elected challenger from the other party.

The proposed mechanism behind these results is that electoral incentives induce policy moderation, and the removal of re-election concerns causes lame-duck incumbents to shirk ideologically. Policy-motivated incumbents facing competitive elections have strong incentives to compromise with their own convictions in exchange for votes, whereas the removal of re-election concerns causes lame ducks to revert to their own ideals (Wittman, 1983; Calvert, 1985; Alesina, 1988). Consistent with this channel, I find larger effects of lame-duck status on roll call extremism for electorally more vulnerable incumbents whose *predicted* margin of victory is small, i.e., precisely for those legislators with the *ex-ante* strongest incentives to moderate.

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, I find that 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 in exchange for bipartisan support

of their 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 the removal of 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 genuine ideological shirking rather than a side effect of participatory shirking.

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), and present a striking contrast to extant quasi-experimental null findings from the state-legislative context (Fouirnaies and Hall, 2022). Of course, divergent results do not exclude each other's validity. Yet, there are a few differences that deserve mention. One possible explanation for divergent results is that I examine high-stakes congressional elections, whereas 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 roll call voting (Rogers, 2017). 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 accountability effects: The existence of term limits may lead to endogenous sample selection. If only ex-ante congruent types survive enough re-election bids to hit the binding term limit, there is little scope for detecting accountability effects. In particular, if voters are rational and anticipate that termed-out politicians are unaccountable, it is unlikely that they re-elect potential shirkers into a last term. 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 rents from office (Hall, 2019; Olson and Rogowski, 2020; Myers, 2023). Admittedly, my results are also based on a (although not endogenously) selected sample of House incumbents facing a competitive re-election bid. By design, my estimates recover a local average treatment effect comparing losers to winners of close elections. As most House incum-

bents who lose their re-election bid do so in a close race,[8] these estimates are informative for the lame-duck effect in the U.S. House, whereas accountability effects in uncompetitive settings are beyond the scope of my analysis. That said, this paper contributes to the literature an important existence result, showing that the accountability mechanism is operative and effectively constrains incumbent politicians' policy choices in an electorally competitive environment.

Second, this paper contributes to a large empirical literature that has identified legislators' private interests (Levitt, 1996; Washington, 2008), their core constituency (Mian et al., 2010), peers (Harmon et al., 2019), and party leadership (Canen et al., 2020) as key drivers of legislative voting. My finding that electoral incentives constrain legislators' voting decisions most directly speaks to the debate on how voter preferences shape public policy. In a seminal paper, Lee et al. (2004) find that an increase in electoral strength of U.S. House representatives due to the incumbency advantage inherited from a close victory in the election to the preceding term does not change their voting behavior in the subsequent term, concluding that voters merely *select* policies by replacing one incumbent with another but cannot *affect* policy by constraining sitting incumbents' policy choices. While consistent with a pure selection model of electoral politics, their finding is also consistent with closely elected incumbents correctly anticipating their incumbency advantage in subsequent elections and adjusting their voting behavior pre-emptively. In contrast, Jones and Walsh (2018) find that a plausibly exogenous increase in the Democratic vote share generated by redistricting not only leads to higher re-election probabilities for Democratic incumbents, but also to a more liberal voting record of both Democratic and Republican incumbents upon re-election to the next congressional term. However, since voting records after redistricting are observed only for re-elected incumbents, this result is also consistent with selection, or with an alternative interpretation that politicians faithfully represent their voters' preferences. Because lame-duck sessions allow me to observe both re-elected and exiting incumbents in the same congressional term, and since my close election RD-design ensures comparability of constituencies represented by narrowly defeated and barely re-elected incumbents, I overcome these difficulties and isolate electoral incentives from selection and anticipation effects. My results suggest that voters do *affect* public policy with political competition inducing re-election concerned incumbents to compromise. In line with recent evidence that electoral competition leads candidates to strategically moderate campaign communication to please the median voter (Le Pennec, 2023; Di Tella et al., 2023), I show that electoral incentives result in *actual* policy moderation by incumbents facing competitive re-election bids.

Third, this paper speaks to an extensive body of work comparing 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

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[8] In my sample covering the 111th to 116th Congresses, 77% of re-election losing House incumbents are defeated by a margin of less than 10%, which in an ideal-typical two-candidate race corresponds to vote shares of 45% – 55%.

pool of candidates and the selection of different politicians serving under distinct mandates, these studies hardly isolate electoral incentive effects.

Finally, this paper complements 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.

The remainder of the paper is structured as follows: Section 2 describes the institution setting and the data. Section 3 presents the identification strategy and discusses its validity. Section 4 reports the main results (4.1), provides evidence for electoral incentives as the main driver behind lame ducks' positional adjustment (4.2), and rules out alternative mechanisms (4.3). Section 5 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 re-elected 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 gen-

eral elections, or simply continues meeting in intermittent sessions during the period spanning elections.[9]

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. Specifically, 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 COVID-19 appropriations (2020).[10] This paper focuses on lame-duck sessions of the U.S. House in the most recent 111th to 116th 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.

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[9]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.

[10]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).

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

Lame Duck Session	# Scalable Roll Calls	Example Roll Calls	House Incumbents (Democrats / Republicans)		
			All Members	RD Sample	Lost Election
111th 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)	362 (231/131)	55 (53/2)
112th 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)	323 (136/187)	20 (6/14)
113th 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)	319 (146/173)	13 (11/2)
114th 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)	327 (138/189)	4 (0/4)
115th 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)	324 (136/188)	23 (0/23)
116th 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)	299 (197/102)	13 (13/0)

Notes: The Table presents an overview of lame-duck sessions in the U.S. House of Representatives between the 111th and 116th 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 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 re-election 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 111th to 116th Congresses (2008-2020) from the voteview.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 term. The W-NOMINATE algorithm works by applying a discrete choice model to locate legislators in the ideological space, with legislators having similar roll call voting records being placed close 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 default options and recommendations in Poole and Rosenthal (1985), I exclude uninformative lopsided roll calls on which more than 97.5% of House members agreed and restrict attention to House members casting at least 20 votes, which according to Keith Poole (cited in Nokken, 2013) is the minimum number of votes required to reliably estimate a legislator's roll call position. Because 71 re-elected and 9 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 re-election seeking incumbents to 1826 re-elected and 128 lame-duck members. There is no indication of endogenous sample attrition at the cutoff,[11] 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 4.3), 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 (83) and Republican lame ducks (45) that unsuccessfully sought re-election, 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*:

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

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; Fouirnaies 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 W-NOMINATE scores are not directly comparable across congresses and sessions, as incumbent positions are estimated separately by congress × 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 × 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 re-elected incumbents of the same party, serving in the same Congress and voting on the same set of roll calls. *Congress × party* fixed effects also control for possibly divergent incentives for incumbents of different parties due to changing majority status,[12] and for possible

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[11]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 0.026 (robust p-value accounting for clustering at the incumbent-level = 0.7).

[12]For example, leadership of a party that is about to lose majority status in the subsequent Congress may be tempted

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 re-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).[13] This leaves me with a sample of 1954 incumbents seeking re-election, 128 of which lose their re-election bid and return 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 re-election seeking incumbents to lame ducks if and only if their vote share falls behind their strongest opponent's.

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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.

[13]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.

PANEL A: MAIN VARIABLES OF INTEREST	Mean	Std. Dev.	Min.	Max.	Obs.
Δ 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
Δ 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
Δ 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					
Δ 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
Δ 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
Legislative Effectiveness	1.050	1.252	0.000	16.314	1956
Incumbent's Expected Margin of Victory	0.283	0.151	-0.111	0.900	1959

Notes: The Table presents summary statistics for the sample of 1959 U.S. House representatives of the 111th to 116th Congresses, who seek re-election in a competitive race against a main challenger of opposite political orientation. *Roll Call Extremism* measures the liberalism (conservativsm) of Democratic (Republican) legislators based on W-NOMINATE scores as defined in equation 1. Δ *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 × 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. *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. *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 × party* fixed effects (including all lower order terms).

Table 2, Panel A, presents summary statistics for the main outcomes of interest, re-election 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 absention, I consider *party loyalty*, defined as the fraction of votes cast in line with the party whip; and absenteeism, 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 × 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. 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 irresponsive 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:

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

where θ is the coefficient of interest representing the causal effect of lame-duck status on ΔY_{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 θ 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 × party* fixed effects denoted by λ_{pc} ensure comparability of W-NOMINATE-based *roll call extremism* by restricting comparisons of barely unseated lame ducks to narrowly re-elected 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 θ 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 re-elected 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.[14]

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[14]Studies leveraging random variation in state legislators' term length do not find any evidence of electoral proximity

## 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.[15]

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 election 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 ($p = 0.355$). 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).

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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). On the other hand, observational studies suggest that U.S. senators moderate their roll call voting behavior when elections approach (e.g., Wright and Berkman, 1986; Lindstädt and Vander Wielen, 2011). For evidence on the presence of electoral cycles in judicial sentencing, see, e.g., Huber and Gordon (2004), and Berdejó and Yuchtman (2013).

[15]For excellent discussions of the credibility of close election RD-designs, see Lee (2008), Eggers et al. (2015), De la Cuesta and Imai (2016).
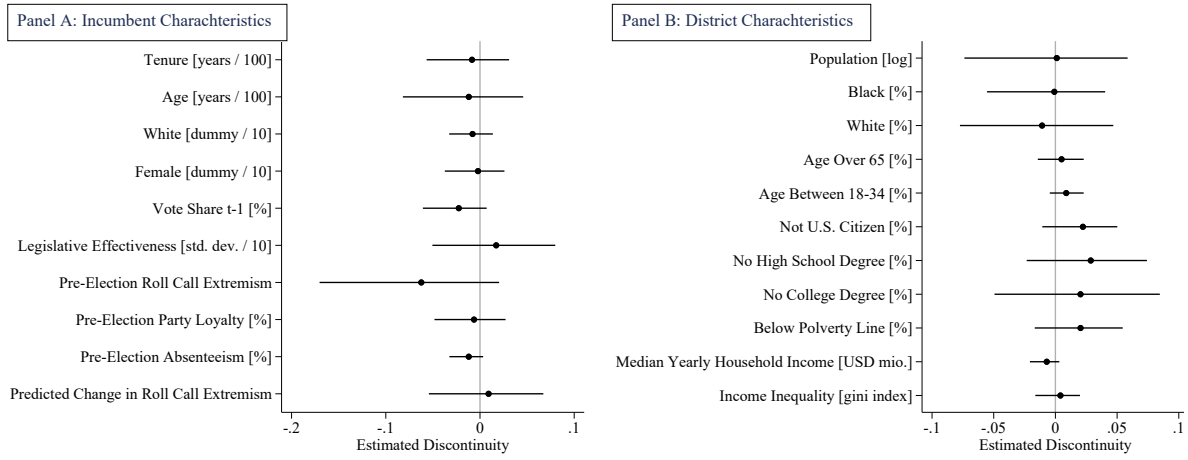
FIGURE 1: MANIPULATION TESTS FOR AGGREGATE SORTING
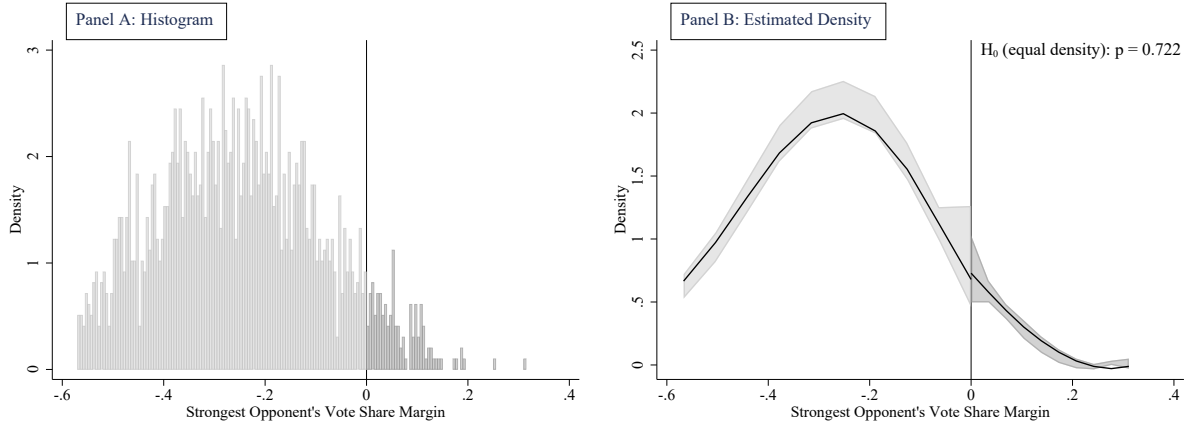
Notes: The Figure in Panel A presents the sample distribution of the *Strongest Opponent's Vote Share Margin* for representatives who win re-election against their runner-up (light grey) and lame-duck incumbents who lost their re-election bid (dark grey). Panel B is a graphical representation of the 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 0.355.

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 representatives facing close elections did not strategically alter in-office behavior depending on the election outcome.
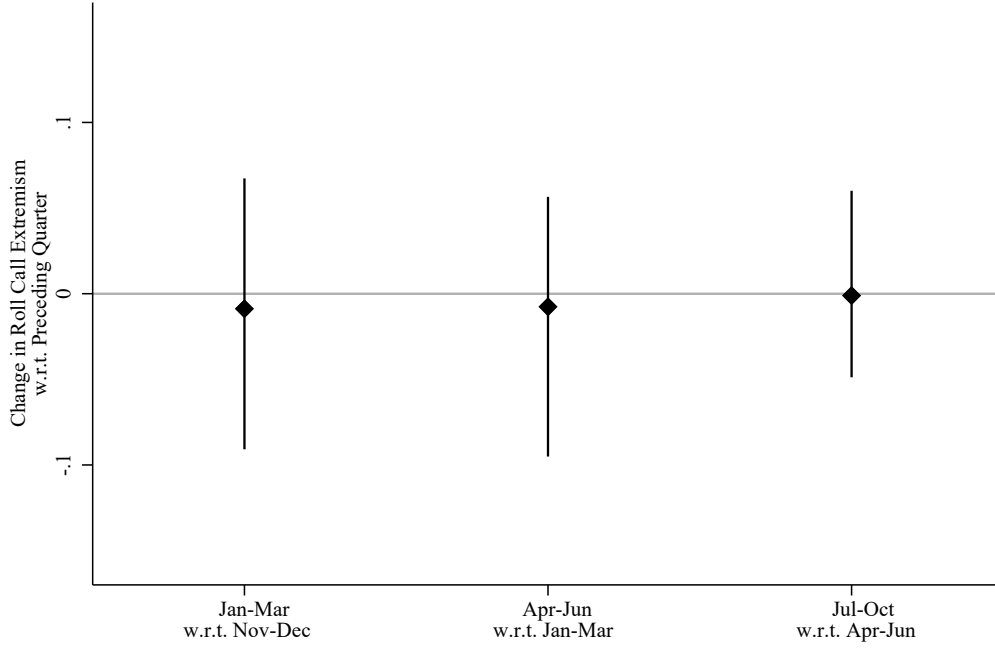
Notes: The Figure presents results from balancing tests on incumbent (Panel A) and district characteristics (Panel B). Point estimates (dots) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering by House representative are obtained from local linear specifications 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 re-elected 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 re-elected and narrowly ousted incumbents follows a parallel trend.

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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   Results

4.1   Lame-Duck Status and Roll Call Extremism

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

FIGURE 4: CHANGES IN INCUMBENT'S ROLL CALL EXTREMISM DEPENDING ON THEIR STRONGEST OPPONENT'S VOTE SHARE MARGIN
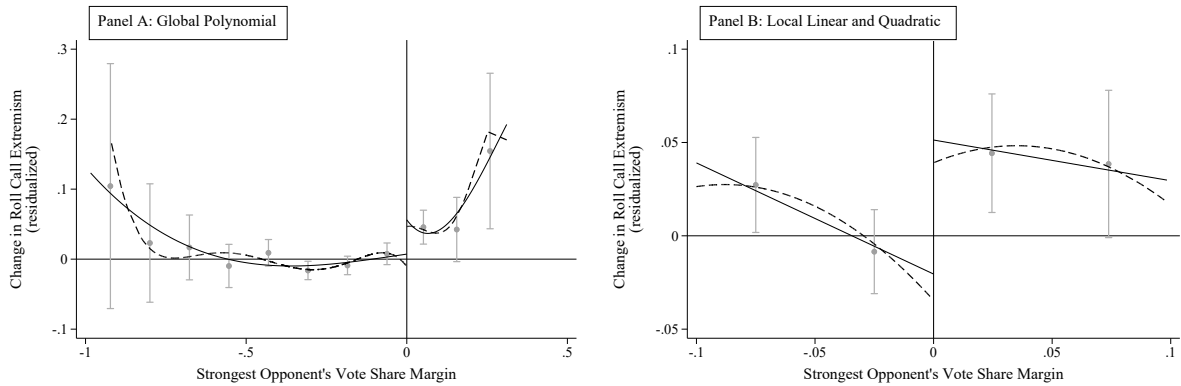
Notes: The Figure shows local means of within-incumbent changes in residualized *Roll Call Extremism*, net of *party × congress* fixed effects, from the regular session to the lame-duck session. Local averages (dots) are calculated within equal-spaced bins of the *Strongest Opponent's Vote Share Margin*, 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 1954 re-election seeking incumbents and plots the quartic fits (solid lines) of the outcome variable on the assignment variable, separately on each side of the cutoff. Panel B restricts the sample to 301 incumbents whose re-election bid has been decided by a margin of less than 10%, and plots local linear (grey lines) as well as quadratic fits (black lines).

Formal estimates from local-linear regressions using triangular kernels and MSE-optimal bandwidth are shown in Table 3. The main outcome of interest is the change in incumbents' *roll call extremism* from the pre-election regular session to the post-electoral lame-duck session. Column 1 presents results using my preferred specification of equation (2) estimating the impact of lame-duck status on the pooled sample of all re-election seeking incumbents, whereby conditioning on

party × congress fixed effects ensures comparability of *roll call extremism* across incumbents. On average, lame-duck legislators who barely lost their re-election bid take more extreme positions compared to co-partisans serving the same Congress who won re-election by a narrow margin. Column 2 additionally controls for the full set of incumbent and district characteristics listed in Figure 2 – with the exception of the predicted change in roll call voting extremism but including the base level of *roll call extremism* prior to elections. Reassuringly, the inclusion of covariates does not affect the coefficient of interest. These results are robust to specifications using different kernels and higher-order polynomials (see Appendix Table A.1) and a wide range of alternative bandwidths choices (see Appendix Figure A.2).

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

	Δ Roll Call Extremism		Democrat Δ W-NOMINATE		Republican Δ W-NOMINATE	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.099***	0.097***	-0.149***	-0.107***	0.092**	0.043***
	(0.028)	(0.020)	(0.039)	(0.027)	(0.042)	(0.014)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.065]	[0.083]
Party × Congress FE	Y	Y	-	-	-	-
Congress FE	-	-	Y	Y	Y	Y
Pre-Election Outcome	N	Y	N	Y	N	Y
Covariates	N	Y	N	Y	N	Y
Bandwidth	0.064	0.064	0.043	0.043	0.044	0.044
Effective Obs. Left	122	120	54	54	30	29
Effective Obs. Right	75	74	31	30	21	21
Control Mean	0.001	0.001	0.002	0.002	-0.009	-0.009
Observations	1959	1928	990	978	969	950

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 re-election 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). Columns 2, 4, and 6, adjust for all covariates listed in Figure 2, exluding 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 regressions use triangular kernel weights, and include *party × congress* fixed effects. 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.

Next, I explore heterogeneous effects across parties, examining the effect of lame-duck status on within-incumbent changes in W-NOMINATE scores conditional on congress fixed effects. Consistent with the main results on *roll call extremism*, we observe that lame-duck Democrats take more liberal positions (Table 3, Column 3), whereas Republican lame ducks vote more conservatively relative to their re-elected co-partisans (Column 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 estimated difference in *magnitudes* of lame-duck effects between Democrats and Republicans falls short of statistical significance.[16] Moreover, point estimates of these split-sample analyses vary somewhat upon the inclusion of covariates (Columns 4 and 6), which is unsurprising given the small number of treated observations in each subsample. Controlling for 20 covariates in addition to congress fixed effects in a sample barely exceeding 50 effective observations may be overly restrictive. I therefore view the split-sample analysis as a particularly demanding robustness check, but caution from overinterpreting differential effect sizes between Republican and Democratic subsamples.

For a quantitative interpretation of effect sizes, I rely on the baseline specification using the pooled sample, which estimates lame-duck status to cause an increase in *roll call extremism* by 0.1 units. Recall that *roll call extremism* is measured in units of the W-NOMINATE score, ranging from -1 (the most liberal Democrat) to +1 (the most conservative Republican in my sample). The increase in *roll call extremism* caused by a close defeat thus equals 5% of the ideological distance between the most liberal and the most conservative legislator in polarized America. In terms of pre-election W-NOMINATE scores of House representatives in the 116th Congress, this is equivalent to the average distance between representatives and their own party's median.[17]

As a benchmark to compare the effect of electoral incentives to selection effects, close election RD-estimates in Lee et al. (2004) imply a distance of 0.37 in DW-NOMINATE scores between roll call voting positions of narrowly elected Republicans and Democrats in otherwise comparable congressional districts. Although not directly comparable to W-NOMINATE, DW-NOMINATE scores also range from -1 to +1 and are as good as perfectly correlated with W-NOMINATE scores in my sample ($\rho = 0.97$). A simple back-of-the-envelope calculation thus suggests that, in competitive districts, the presence of electoral incentives (respectively the removal thereof) causes a shift in incumbents' roll call voting position of about 17% the change that would be induced by the replacement of the incumbent by a challenger of the opponent party.[18] I next provide evidence that the mechanism behind lame ducks' more extreme repositioning is indeed the removal of electoral incentives, ruling out several competing channels.

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[16]Formally, I fail to reject the null hypothesis that the *negative* of the coefficient for Republicans in Column 5 equals the coefficient for Democrats in Column 3 ($p = 0.282$, two-tailed). The difference between coefficients, however, is statistically significant ($p < 0.001$, two-tailed).

[17]More precisely, in my sample of re-election seeking incumbents in 116th Congress, the average distance of pre-election W-NOMINATE scores to their party's median is 0.097 for Democrats and 0.119 for Republicans.

[18]This back-of-the-envelope calculation is executed as follows: To convert W-NOMINATE in DW-NOMINATE scores, I regress the DW-NOMINATE in my sample of re-election seeking incumbents on their pre-election W-NOMINATE, obtaining a coefficient of 0.62. Multiplying 0.62 with my estimated lame-duck effect of 0.1 yields 0.062, which is 17.3% of the 0.37 DW-NOMINATE score selection effect estimated by Lee et al. (2004).

4.2 Electoral Incentvies, Strategic Moderation, or Emotional Backlash

The leading hypothesis of this paper is that the removal of re-election concerns causes lame ducks to adopt more extreme policy positions after elections. Incumbents vying for re-election 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 candidates, this involves compromising on their preferred policy in exchange for higher chances of winning elections (Wittman, 1977, 1983; Calvert, 1985). While electoral incentives to build moderate reputations remain operative for returning members after elections, lame ducks exiting this dynamic game lose incentives to compromise that had been active prior to their last term (Alesina, 1988). Absent re-election concerns motivating strategic moderation, lame-duck incumbents therefore revert to their own ideal and vote sincerely in accordance with their own preferences, whereas the persistence of electoral incentives keeps returning members tied to voter preferences. Given that incentives to moderate are more binding for electorally vulnerable incumbents facing a competitive re-election bid, these theoretical predictions align with the observed pattern that close election losers take more extreme positions while narrow winners keep committed to a more moderate voting record.

However, 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 re-election 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.[19] Moreover, if incumbents exhibit loss aversion (Kahneman and Tversky, 1979; Kőszegi 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 (see also Card and Dahl, 2011; Eren and Mocan, 2018).

Thus, the pattern observed in the baseline results could be explained by both strategic moderation and emotional cues. Although both mechanisms entail observationally equivalent predictions on the main effects of lame-duck status on incumbents' voting behavior, they have sharply contrasting implications for effect heterogeneity depending on *ex-ante expected* election results.

The key implication of the literature on emotional cues is that reactions to unexpected emo-

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[19]The perhaps most notorious example in recent history that could motivate this channel is Donald Trump's behavior in the aftermath of the 2020 presidential election, whose claims of alleged election fraud have been accompanied by increasingly radical positions that culminated in an attempted overthrow of the United States government.

tional shocks are stronger than to expected emotional cues, thus predicting larger effects of lame duck-status on incumbents who were facing an ex-ante safe re-election bid but then experienced *unexpected* defeat. On the other hand, the political economy literature on electoral competition between policy-motivated candidates implies stronger electoral incentives to compromise for electorally weak candidates (Wittman, 1983; Calvert, 1985; Alesina, 1988). If re-election seeking incumbents respond to electoral incentives, they would moderate differentially more when their electoral prospects are uncertain. Hence, lame ducks' post-electoral reversal to extreme positions upon losing their re-election concerns would be larger when defeat was ex-ante *expected* to be more likely.

To disentangle these competing channels, I thus estimate incumbents' *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 × party* fixed effects (including all lower order terms). 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). 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.[20] Presidential party House members (e.g., Democrats in the 111th 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 112th Congress) may estimate this electoral advantage to shrink toward the end of the term. Incumbents of different parties with the same prior vote share, thus have ex-ante differential incentives to take moderate positions in a given congressional term. Specifically, the lower incumbents' *expected margin of victory*, the more vulnerable they are *ex ante*, and the more expected their electoral defeat.

To test for heterogenous effects of lame-duck status depending on incumbents' expected vote share margin, 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%).[21] Specifically, I

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[20]The 3 midterm elections before my sampling period in which the presidential party won seats were 1934 (Roosevelt, Democrat gain of 9 seats), 1994 (Clinton, Democratic gain of 4 seats), and 2002 (Bush, Republican gain of 8 seats). The fourth instance, occurring after my sampling period was 2022 (Biden, Democratic gain of 9 seats).

[21]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 the sample by terciles of the distribution of incumbents' *expected margin of victory* in the whole sample of re-election seeking representatives, there would be too few observations pertaining to lower terciles (i.e., incumbents up for expectedly safe re-election bids) close to the cutoff where (heterogenous) effects of lame-duck status are estimated. Appendix Figure A.3 shows the distributions of

divide the sample into incumbents facing ex-ante "toss-up" races (with an *expected margin of victory* below 7.5%), "competitive" and "safe" re-election bids (with a margin between 7.5% and 14%, respectively above 14%). I then re-estimate equation (2) on these subsamples, expecting larger effects of lame-duck status on representatives facing more competitive elections if the removal of electoral incentives is the driving mechanism, but larger effects on ex-ante "safe" incumbents if their reaction to electoral defeat is mainly driven by an emotional channel.

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

	Toss-up (1st Tercile)		Competitive (2nd Tercile)		Safe (3rd 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	0.038	0.038	-0.029	-0.029	0.033	0.023
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 toss-up (Columns 1 and 2), competitive (Columns 3 and 4), and safe elections (Columns 5 and 6) by terciles of the distribution of the expected vote share margin within ex-post close elections decided by an acutal margin of less than 5%. The subsamples include observations below the 33rd percentile (toss-up), between the 33rd and 67th percentiles (competitive), and above the 67th percentile (safe). The bandwidths are MSE-optimal in Columns 1, 3, and 5, and fixed to ex-post close elections decided by an acutal 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. We observe differentially larger effects of lame-duck status on representatives' *roll call extremism* in ex-ante more competitive elections. Legislators having lost expectedly close "toss-up" races (Columns 1 and 2) exhibit a greater shift to more extreme positions than do lame ducks defeated in ex-ante "competitive" elections (Columns 3 and 4), whereas incumbents unexpectedly failing a "safe" re-election bid (Columns 5 and 6) do not change their roll call voting position at all. The difference in coefficients between "toss-up" and "safe" re-election bids is statistically significant ($p = 0.0792$, two-tailed) based on MSE-optimal bandwidths (Columns 1 and 5). The difference in corresponding estimates using fixed 0.05 bandwidths falls just short of statistical significance at conventional levels ($p = 0.126$, two-tailed), whereby the pattern is qualitatively and quantitatively highly similar (Columns 2 and 6). Overall, the empir-

expected vote share margin 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).

ical evidence is inconsistent with an emotional channel, yet supports the proposed mechanism that ex-ante vulnerable incumbents moderate strategically and lame ducks having lost re-election concerns revert to more extreme positions closer to their ideal. I next rule out several other mechanisms.

4.3 Ruling Out Alternative Mechanisms

An alternative potential mechanism behind lame duck incumbents' increase in *roll call extremism*, is the removal of logrolling motives. Rather than seeking re-election, policy-oriented legislators might be interested in achieving policy change by sponsoring bills and forging coalitions in support of these bills to ensure that their proposals get attention in the legislative process and eventually get passed into law. Forging majority coalitions, in particular for bills requiring bipartisan backing, may involve vote trading, and compromise on some policy positions in exchange for future support of one's own proposals. While returning House members keep committed to (perhaps implicit) promises to secure the future success of their own proposals, exiting lame ducks inevitably quit this dynamic game and could renege on (implicit) vote trading contracts.[22]

TABLE 5: 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***	0.107***	0.156***	0.162***
	(0.043)	(0.040)	(0.036)	(0.038)
	[0.005]	[0.005]	[0.000]	[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 effectivenss 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 acutal 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.

Thus, not the loss of re-election incentives, but the removal of accountability to opponent party

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[22]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.

co-legislators may drive lame-duck incumbents' *roll call extremism*. Although the inherent unobservability of vote trading prevents me from testing this mechanism directly, a testable implication is that legislatively active lawmakers should react differentially more to seat loss compared to incumbents less engaged in the legislative process. To test this hypothesis, I split my sample by 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%).[23] 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 5, the lame duck effect on *roll call extremism* is highly similar across legislatively active incumbents (Columns 1 and 2) and legislatively less engaged representatives (Columns 3 and 4). If anything, legislatively active members seem to react differentially less to seat loss, perhaps because more policy-oriented representatives have stronger policy preferences and are less inclined to compromise on ideology to retain office.

Party leadership losing its grip on exiting members is another candidate mechanism behind lame-duck incumbents deviating to more extreme positions compared to returning co-partisans. 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 in the party organization, in the executive branch, or vie for another elected office).

While correlational evidence on lame-duck members' party loyalty is decisively mixed,[24] 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.

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[23]Appendix Figure A.4 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).

[24]Stratmann (2000) finds that retiring legislators in the 98th to 103rd Congresses (1983-1995) vote more often in party line than returning members, whereas Figlio (1995) reports the opposite: Retiring members of the 94th to 97th 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 (45th to 72nd Congress, 1877-1933) more likely deviated from party line than returning members.

Examining incumbents' change in *party loyalty* as the outcome in regression equation (2) yields precisely estimated null results, as shown in Table 6, 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).

TABLE 6: LOYALTY TO PARTY LEADERSHIP, SELECTIVE ABSTENTION, AND ROLL CALL EXTREMISM

	Δ Party Loyalty (%)		Δ Absenteeism (%)		Δ Roll Call Extremism			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.009	-0.011	0.046**	0.044**	0.099***	0.120***	0.104***	0.121***
	(0.027)	(0.027)	(0.021)	(0.022)	(0.028)	(0.030)	(0.028)	(0.030)
	[0.902]	[0.961]	[0.071]	[0.036]	[0.001]	[0.000]	[0.000]	[0.000]
Control for Δ 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 × congress* fixed effects. Columns 7 and 8 additionally contol 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.

Yet another possible 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 (Lott, 1987, 1990; Lott and Bronars, 1996; Herrick et al., 1994; Rothenberg and Sanders, 2000).

Estimates in Table 6, Columns 3 and 4, confirm these findings, providing evidence for a causal relationship between lame duck status and roll call *absenteeism*.[25] Narrowly out-selected lame

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[25]This first causal evidence for participatory shirking in the U.S. Congress complements similar findings in different settings. Fouirnaies 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

ducks are 4.5 percentage points less likely to participate in post-electoral roll calls compared to closely re-elected 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 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.

5   Conclusion

Elections have a duplicate purpose in representative democracies. On the one hand, recurrent elections allow voters to replace badly performing politicians with better types. On the other, the threat of being unseated is thought to constrain incumbents' policy choices to align with voters' interests. For constitutional design, it is important to understand the channel by which elections shape public policy. If politicians were ideologically rigid and did not respond to electoral incentives, this would make a case for institutions that increase electoral turnover (e.g., term limits) or improve democratic representation (e.g., proportional elections) at the expense of accountability. Whether electoral incentives are effective in constraining incumbents' policy choices has been a longstanding question in economics and political science.

Answering this question empirically is challenging because separating electoral incentives from selection effects is challenging. In this paper, I propose a novel identification strategy that takes advantage of lame-duck sessions in the U.S. House of Representatives where re-election constrained members vote on the same issues as unconstrained lame ducks. Using a regression discontinuity design to exploit quasi-random assignment of re-election seeking incumbents to lame-duck status, I improve on existing designs that fail to isolate incentive effects from the selection of different types into the last term. In contrast to extant empirical evidence in the legislative context, I find a significant causal effect of lame-duck status on legislators' voting. In line with theoretical expectations that electoral incentives induce policy moderation, I find that unconstrained lame-duck incumbents revert to more extreme positions, with Democratic lame ducks voting more liberally and Republican lame ducks voting more conservatively. Consistent with electoral incentives driving these results, the effect of lame-duck status on roll call extremism is more pronounced among more electorally vulnerable legislators. Unlike previous studies, the congressional setting

studies, however, finds an effect of lame-duck status on legislators' voting position conditional on voting.

enables me to rule out several competing mechanisms, including emotional backlash, logrolling motives, party control, and selective abstention. This paper thus contributes a crucial existence result, providing the first credibly identified evidence that electoral incentives effectively constrain incumbents' policy choices.

Yet, a lot of work remains to be done. To what extent these results drawn from high-stakes federal elections carry over to less competitive, low-information environments or to term-limited settings where politicians have ex-ante shorter horizons remains an open question. How electoral incentives, respectively the removal thereof, interact with voter information and politicians' horizons would be, I suspect, important topics for further research. That said, my results have direct implications for ongoing debates over the abolishment of congressional lame-duck sessions, echoing concerns of electoral accountability that had been raised 100 years ago and eventually led to the 20th Amendment to the U.S. Constitution, which ended the era of regularly occurring lame-duck sessions in 1933.

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Appendix

**FIGURE A.1:** DISTRIBUTION OF W-NOMINATE SCORES IN REGULAR AND LAME-DUCK SESSIONS
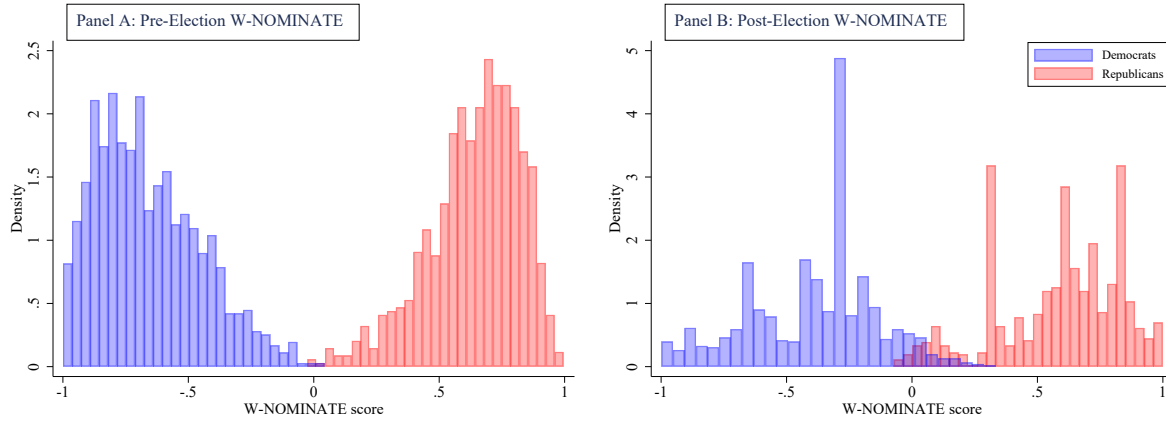
Notes: The Figure presents the sample distributions of roll-call voting positions in regular sessions (Panel A) and post-electoral lame-duck sessions (Panel B). Positions are estimated by extracting the first dimension of W-NOMINATE scores, estimated separately by *congress × session* using the R implementation 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 111th to the 116th 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
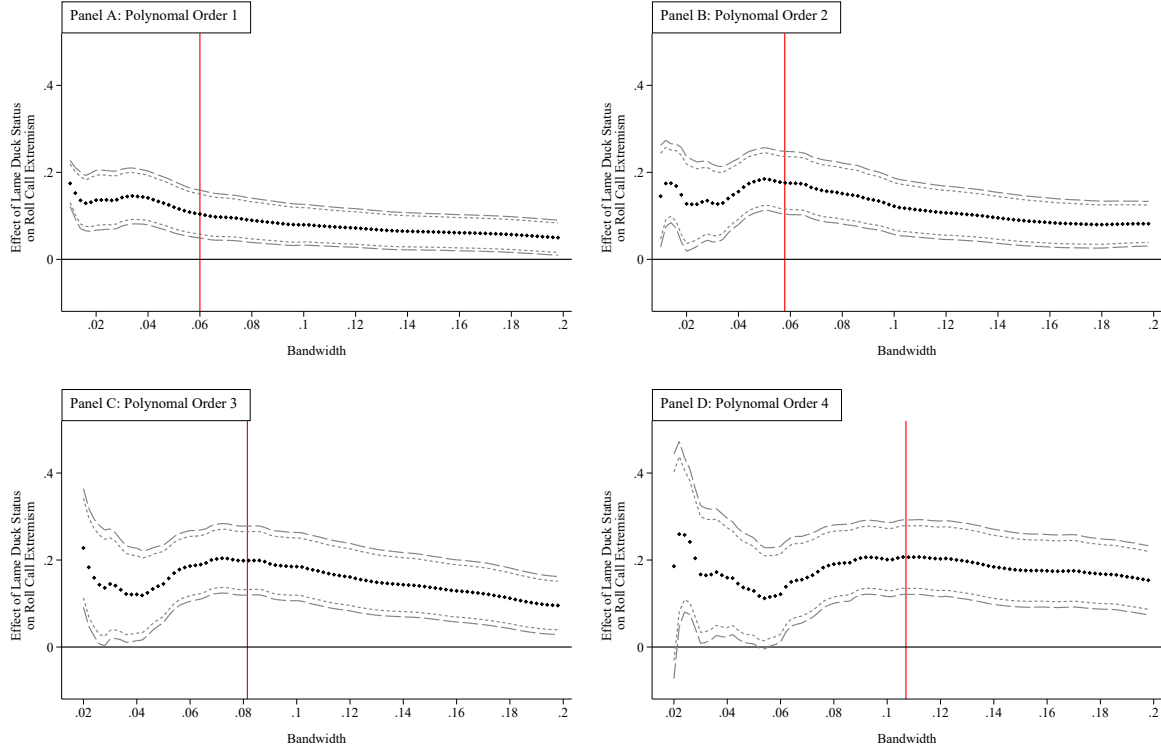

Notes: The Figure plots discontinuity estimates (black dots) for the effect of lame duck status the change in House incumbents' *Roll Call Extremism* from the regular sessions before general elections to the lame duck session after elections for different bandwidths and polynomials. Bandwidths range 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 × 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
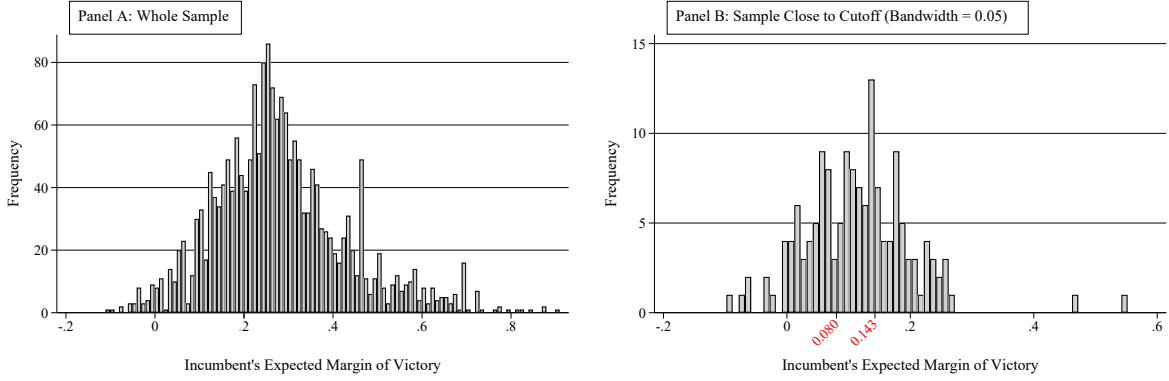
Notes: The Figure presents the sample distributions of the *Incumbents' Expected Margin of Victory*. *Incumbents' Expected Margin of Victory* are the fitted values from a linear regression of the incumbent's actual vote share margin relative to their strongest opponent on the incumbent's lagged vote share interacted with *congress × 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 INCUMBENTS' LEGISLATIVE EFFECTIVENESS SCORE
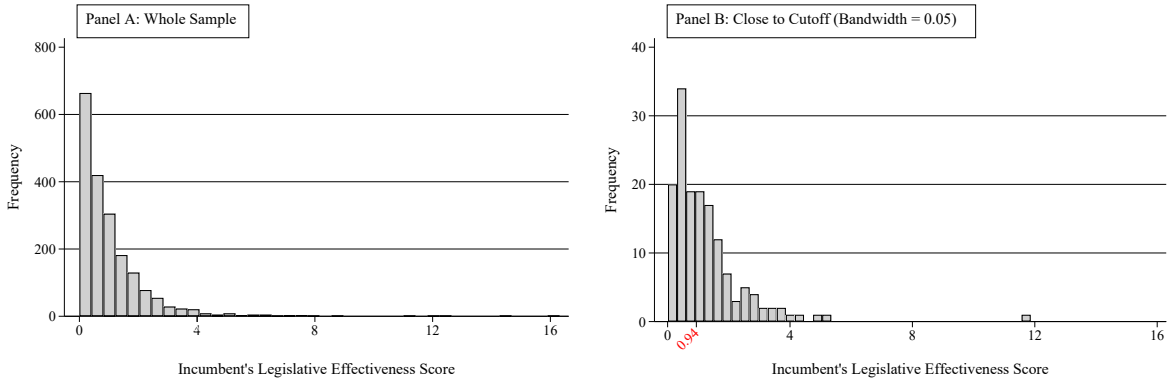
Notes: The Figure presents the sample distributions of the incumbents' term-specific legislative effectiveness score (Volden and Wiseman, 2014). Panel A shows the distribution in the full sample, and Panel B the distribution within a 0.05 bandwidth around the cutoff. The value in red indicates the median legislative effectiveness score for the sample split underlying the analysis in Table 5

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

	Polynomial 1		Polynomial 2		Polynomial 3		Polynomial 4	
PANEL A: TRIANGULAR KERNEL	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.099***	0.097***	0.176***	0.153***	0.199***	0.167***	0.207***	0.172***
	(0.028)	(0.020)	(0.037)	(0.026)	(0.040)	(0.031)	(0.044)	(0.036)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[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***	0.074***	0.141***	0.137***	0.226***	0.186***	0.226***	0.196***
	(0.025)	(0.023)	(0.040)	(0.032)	(0.048)	(0.042)	(0.048)	(0.043)
	[0.008]	[0.004]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[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***	0.091***	0.184***	0.161***	0.212***	0.177***	0.208***	0.171***
	(0.027)	(0.022)	(0.038)	(0.029)	(0.043)	(0.035)	(0.046)	(0.039)
	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[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 × 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) and alternative kernel weights (panels). 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.