Electoral Incentives, Voter Turnout and Legislator Behavior: Evidence From Close Elections

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Preface

In modern political economy, public policy outcomes are conceived as the result of strategic interactions among interested agents. These interactions are structured by constraints and incentives imposed by political institutions. Elections are the distinctive and most fundamental political institution of republican government. In representative democracies, elections are not only the characteristic method of preference aggregation, but also the primary mechanism by which citizens control government and hold their representatives to account. While a comprehensive analysis of the incentive structure embodied by democratic elections is clearly beyond the scope of this thesis, this dissertation offers insight into some key aspects of democracy, focusing on electoral incentives provided by close elections.

Close elections stand out due to their potential to influence political outcomes significantly. As expectedly tight races heighten the stakes, close elections alter the incentive structure for both voters and elected officials in a way that shapes public policy. For each individual voter, expectedly close elections may raise the perceived probability of casting the pivotal vote, and therefore increase their propensity to turn out. If expected election closeness asymmetrically affects the turnout of different groups of voters, close elections may alter the composition of the electorate, shaping election outcomes and ultimately public policy. For elected representatives, closely contested elections elevate the risk of losing office, intensifying incentives to implement policy in line with voter preferences, or to strategically adjust policy to positions taken by challenger candidates. This dissertation elaborates on, and tests these hypotheses using novel identification strategies and rigorous econometric methods for causal inference.

The first chapter of the thesis – coauthored with Leonardo Bursztyn, Davide Cantoni, Patricia Funk, and Noam Yuchtman – tests a key prediction of classical political economy models: Close elections incentivize voters to turn out more. We provide evidence of a causal effect of anticipated election closeness on voter turnout, exploiting the precise day-level timing of the release of Swiss national poll results for high-stakes federal referenda, and a novel dataset on daily mail-in voting for the canton of Geneva. Using an event study design, we find that the release of a closer poll causes voter turnout to sharply rise immediately after poll release, with no differential pre-release turnout levels or trends. We provide evidence that polls affect turnout by providing information shaping beliefs about closeness. The effects of close polls are largest where newspapers report on them most; and, the introduction of polls had significantly larger effects in politically unrepresentative municipalities, where locally available signals of closeness are less correlated with national closeness. We then provide evidence that the effect of close polls is heterogeneous, with an asymmetric effect leading to a higher vote share for the underdog. The effect sizes we estimate are large enough to flip high-stakes election outcomes under plausible counterfactual scenarios.

The second chapter considers whether electoral incentives constrain incumbent politicians' policy choices. To answer this longstanding question, I use 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.

The third chapter investigates whether and how officeholders adjust policy strategically for electoral purposes by studying how U.S. House incumbents alter their roll call voting record prior to elections depending on their challenger's platform. Estimating non-incumbent candidates' policy positions using pre-primary transaction-level campaign finance data, I classify as extremist the more liberal (conservative) of the top-two candidates in Democratic (Republican) challenger primaries. Leveraging a regression discontinuity design, I exploit the quasi-random assignment of incumbents to moderate or extremist challengers by close primary elections of the incumbent's opponent party. I find that incumbents alter their roll-call voting record in the direction of their opponent's position, committing to a more moderate policy when running against an extremist challenger and differentiating their position from more moderate opponents. Consistent with strategic responsiveness to electoral incentives, policy adjustment to challengers is confined to re-election seeking incumbents and to incumbents defending a seat in a competitive district. I provide suggestive evidence that incumbents' reaction to challengers is conditioned by the presence of third candidates, and reflects a trade-off between persuading swing voters at the center and mobilizing core supporters. Importantly, incumbents' adjustment is not driven by a valence advantage of moderate over extremist challengers but by incumbents' reaction to opponents' policy positions, suggesting strategic complementarity of policy platforms.

Chapter I
Identifying the Effect of Election Closeness on Voter Turnout: Evidence from Swiss Referenda

With Leonardo Bursztyn, Davide Cantoni, Patricia Funk, and Noam Yuchtman

1 Introduction

Voter turnout is among the political behaviors of greatest interest to social scientists, shaping election outcomes and thus public policy. Yet, there is a surprising lack of clear, causal evidence for one of the most widely-studied drivers of turnout: a voter's response to anticipated election closeness, which is at the heart of voting models dating back to Downs (1957a), and the subject of more than 100 empirical studies (summarized in Cancela and Geys, 2016).[1] On the one hand, observational studies generally find significant, positive correlations between election closeness and voter turnout, but causal inference is undermined by concerns that underlying issue type or the behavior of the political "supply side" (e.g., political advertising) may drive the results.[2] On the other hand, recent field experiments providing far more credible tests (e.g., Enos and Fowler, 2014, and Gerber et al., 2020) find no effect of anticipated election closeness on voter turnout.

In this paper we provide evidence of a significant, causal effect of anticipated election closeness on voter turnout. Specifically, we exploit the precise day-level timing of the release of Swiss national poll results for 52 high-stakes federal referenda, and a novel dataset on *daily* mail-in voting for the canton of Geneva.[3] Using an event study design — thus holding fixed the issue type — we find that the release of a closer poll causes voter turnout to sharply rise immediately after poll release. A one-standard deviation closer poll increases voter turnout by a statistically significant 0.4 percentage points in each of three days immediately following the poll's release. Cumulative turnout remains higher through the election day, indicating that close polls do not just temporally

[1] Such a causal effect might arise for a variety of theoretical reasons, from (perhaps imperfect) instrumental calculations of costs and benefits (Myatt, 2015), to interactions of election closeness with social preferences (e.g., DellaVigna et al., 2016) or with the intrinsic utility from voting (e.g., Riker and Ordeshook, 1968, Brennan and Buchanan, 1984, Schuessler, 2000, Feddersen and Sandroni, 2006, and Ali and Lin, 2013).

[2] See, for example, Barzel and Silberberg (1973), Cox and Munger (1989), Matsusaka (1993), Shachar and Nalebuff (1999), and Kirchgässner and Schulz (2005).

[3] The vast majority — 90% — of votes cast in Geneva for the referenda studied are mail-in ballots. Note that we use the term "referenda" throughout to refer to federal referenda and initiatives. We discuss the institutional details of our setting in Section 2.

shift votes. We find that turnout rates are no different in levels or trends in the days prior to the release of close polls, suggesting that the information contained in the polls was not anticipated.

Importantly, we can exclude that these results are caused by a differential response of the "supply side", i.e. political advertisements. First, the absence of pre-trends suggests that the supply side was not differentially active prior to the release of close polls. Nor does an endogenous supply side response to the close polls, in the days following their release, account for our findings: we observe significant effects of close polls on votes counted the day immediately after a close poll was released — before the supply side could have affected turnout. Moreover, we can directly test for a supply side response, counting political ads in newspapers (the primary form of political advertising in Switzerland, as TV ads are prohibited). We find that, consistent with close polls meaningfully affecting political beliefs and behavior, there is some evidence of a supply side response: ads significantly increase following a close poll. But, this response appears only three days after the release of a closer poll (potentially affecting votes counted four days after poll release), well after voter turnout already significantly increased.

We then use data from across Switzerland to study whether close polls differentially increase turnout when they receive more coverage in local media. Using a canton$\times$vote panel, we study the effect of *within-election* variation in the coverage of the national poll by newspapers read by the citizens of a canton. Importantly, newspapers were the primary source of political information among Swiss voters throughout the period we study.[4] Controlling for canton and vote fixed effects — and thus purging our estimates of the effects of a fixed (national-level) "issue type" driving turnout — we find that greater cantonal newspaper coverage of close polls significantly increases voter turnout. A one standard deviation increase in the newspaper coverage of a poll that is one standard deviation closer than the mean increases turnout by around 0.5 percentage points. To address concerns about endogenous local coverage of polls, we exploit a canton's arguably "incidental" exposure to poll reporting. We define "incidental" reporting on polls in a canton as poll coverage in newspapers that are read in the canton, but whose largest market is *elsewhere*. If newspaper editors target their news coverage (specifically poll coverage) toward their largest cantonal audience, then readers exposed to this reporting in *other* cantons will read it for reasons other than their own canton's election-specific interest. We find that greater exposure to this "incidental" reporting on close polls is associated with greater turnout as well.

To test for a role of beliefs in driving the relationship between close polls and turnout, we propose that in the absence of polls, voters gauge an upcoming election's closeness by "locally sampling" among individuals in their municipality. This will yield more accurate beliefs if the municipality's closeness is typically correlated with closeness at the national level (i.e., if the municipality is "representative"). In unrepresentative municipalities, it is difficult for individuals

[4]The nationally-representative "VOX survey," conducted following each vote, asks Swiss citizens a broad range of political questions. One of these directly asks, "Through which media did you orient yourself and learn about the pros and cons of the last vote?" In each survey, newspapers were the most frequent selection, with around 80% of respondents indicating the importance of newspapers as a source of political information. See Appendix Figure A.1.

to condition their turnout decision on national-level vote closeness, since their locally available signal is less informative about the national aggregate. When polls are introduced, information on national-level closeness becomes widely available, allowing individuals in both representative and unrepresentative municipalities to condition their turnout on national-level closeness. Exploiting the introduction of polls in Switzerland in 1998, we find evidence consistent with our predictions: prior to 1998, municipalities representative of Switzerland exhibit some association between closeness and turnout, while unrepresentative municipalities do not. Following the introduction of polls, the closeness-turnout gradient increases differentially in unrepresentative municipalities, and becomes positive and highly significant. Moreover, in the post-poll period, the closeness-turnout gradient is nearly identical in the two sets of municipalities.

We close the paper by examining whether close polls can affect election outcomes by changing the *composition* of the electorate. Specifically, we consider the possibility that turnout responses to close polls are asymmetric, with supporters of the trailing side in the poll responding differently compared to supporters of the leading side.[5] To do so, we proxy for a municipality's support for the trailing side using the local vote share for parties that endorse the trailing side in the poll. We find that closer polls differentially increase the turnout rate in municipalities with more predicted support for the trailing side. In addition, the *ex post* vote share for the trailing side is significantly greater in these municipalities following a closer poll. This suggests that the higher turnout is driven by supporters of the trailing side.

To evaluate the magnitude of this asymmetric effect of close polls, we simulate election outcomes under counterfactual polling scenarios. Our counterfactuals are motivated by real-world variation in polling outcomes (resulting from sampling and methodological differences) or by restrictions to the publication of polls (enforced in some countries). First, we consider a case in which polls are counterfactually less close, set at average closeness. Under this scenario, supporters of the trailing side would turn out differentially less, potentially overturning referendum results in which the trailing side in the polls ultimately won a close vote. We then consider a case in which polls are counterfactually one standard deviation closer than the actual poll. Under this scenario, supporters of the trailing side would turn out differentially more, again potentially flipping election outcomes. Under these assumptions, several high-stakes referenda conducted in Switzerland over the last years — on topics ranging from immigration to pension reforms and corporate taxation — would have had different outcomes.

Our findings contribute most directly to a large empirical literature testing whether closer polls affect voter turnout. Up to now, the literature on the effects of polls on voter turnout and elections has been mixed. A large literature shows observational associations between election closeness and turnout.[6] However, causal inference in these studies is undermined by concerns that

[5]We use "trailing" and "leading" to describe polls' forecasted outcomes, in contrast to "losing" and "winning", which we use to describe the ultimate election outcomes.

[6]For example, Barzel and Silberberg (1973), Cox and Munger (1989), Matsusaka (1993), Shachar and Nalebuff (1999), and Kirchgässner and Schulz (2005); the literature is summarized in Cancela and Geys (2016).

underlying issue type or the behavior of the political "supply side" (e.g., political advertising) may drive the results. Lab experiments provide evidence that suggests a causal effect of poll closeness on turnout, but their external validity remains to be verified.[7] Recent field experimental work (Enos and Fowler, 2014, and Gerber et al., 2020) randomly assigns voters information about the closeness of an upcoming election and finds that such information does *not* have a causal effect on real world voter turnout.

We contribute to this literature the first credibly causal evidence of a significant effect of close polls on voter turnout in the field, providing a rigorous confirmation of the observational analyses and supporting the external validity of the lab experiments. Our findings present a striking contrast with existing field experimental evidence, which deserves attention. One possible explanation for divergent findings is that our treatment differs from the field experiments: we rely on naturally-occurring exposure to poll information that arrives to entire populations, while field experiments isolate the effects of information arriving from an experimenter at the individual level. Another possible explanation is simply different settings and possibly heterogeneous effects of close polls. Finally, the null results found in field experiments may be due to a limitation they share: the inability of the experimenter to control information voters acquire outside of the experiment. Because there typically exists plentiful information about closeness available to *both* treatment and control subjects in the weeks before an election, treatment and control subjects' beliefs about election closeness may not have differed at all at the moment of the turnout decision. Hence, null results from these field experiments may be due to insufficient variation in beliefs about closeness between treatment and control subjects, not because information about election closeness is unimportant for the turnout decision. Our evidence suggests that this information, provided by polls, indeed can shape turnout.

Our finding of a causal effect of polls contributes to a growing empirical literature identifying determinants of voter turnout, for example, expressive motives (Pons and Tricaud, 2018), personality traits (Ortoleva and Snowberg, 2015), habits (Fujiwara et al., 2016), social considerations (Gerber et al., 2008, Funk, 2010, and DellaVigna et al., 2016), political movements (Madestam et al., 2013), the existence of exit poll results (Morton et al., 2015), and compulsory voting laws (León, 2017 and Hoffman et al., 2017).[8]

Our findings also contribute to an emerging literature on possible asymmetric effects of polls on turnout among supporters of the trailing and leading sides. Theory is ambiguous regarding which side (if any) will turn out more in response to polls (Simon, 1954; Levine and Palfrey, 2007): on the one hand, the trailing side may be motivated and the leading side may be overconfident, producing differentially high turnout among the trailing side. On the other hand, a discouragement effect among the trailing side and a desire to participate on the winning side (i.e., a "band-

[7]For example, Levine and Palfrey (2007), Duffy and Tavits (2008), and Agranov et al. (2018).

[8]Existing work has also structurally estimated rational choice models of voter turnout, emphasizing instrumental motives (e.g., Coate and Conlin, 2004 and Coate et al., 2008), finding mixed results.

wagon effect") may generate greater turnout among supporters of the leading side. Empirical evidence is scarce. Survey-based evidence provides mixed results: Westwood et al. (2020) argues that the projected high probability of a Clinton victory in the 2016 US presidential election made her supporters overconfident and less likely to turn out; Connors et al. (2020) finds instead that supporters of the leading side in polls tend to turn out more. In a lab experiment, Agranov et al. (2018) find evidence of a bandwagon effect, with more turnout the greater the predicted lead of one's preferred side; similarly, implementing a field experiment in South Africa, Orkin (2020) finds evidence in favor of the bandwagon effect. We provide evidence from high-stakes votes that, in some cases, close polls will differentially stimulate the turnout of the losing side.

Finally, our analysis contributes to a growing body of work studying the impact of the media on voter turnout and preferences (Strömberg, 2004, Gentzkow, 2006, DellaVigna and Kaplan, 2007, Enikolopov et al., 2011, Gentzkow et al., 2011, Spenkuch and Toniatti, 2018a, Durante et al., 2019). While much existing work is focused on the effects of partisan, or persuasive, media content on voters' choices, we instead study the media's provision of mere information about vote closeness. This sort of coverage has become increasingly salient during campaigns; data-driven election forecast sites such as Nate Silver's *fivethirtyeight.com* are increasingly popular. Understanding the impact of this sort of content on voters is thus important; our results suggest that it causally shapes voter turnout and potentially can affect election outcomes.

In what follows, in Section 2, we discuss the institutional context of Swiss referenda and in Section 3, we describe our data. In Section 4, we discuss the challenge of identifying a causal effect of anticipated election closeness and present our empirical results from the Geneva event study. In Section 5, we present our analysis of newspaper reporting on polls using canton-level data. In Section 6 we present our analysis of municipality-level data. In Section 7, we present evidence on the underdog effect and conduct our counterfactual analyses of Swiss referenda outcomes. Finally, in Section 8, we offer concluding thoughts.

2 Institutional Context

Switzerland is a federal republic consisting of 26 cantons and 2,202 municipalities (as of 2020). Along with a distinct federal structure, Switzerland has a long tradition of direct democracy.[9]. Since 1891, Swiss citizens have had the right to call for a popular initiative, with which they can revise the federal constitution, if 100,000 signatures are collected in support of the proposed initiative within 18 months. A popular initiative is accepted if the majority of Swiss citizens vote in favor, and the majority of the cantons do so as well. In response to an initiative, the Federal Council and the Federal Assembly may propose a direct counter-proposal; usually, this is a more

[9]Swiss direct democracy has frequently served as a laboratory to study democratic political institutions: e.g., ballot design (Hessami, 2016; Hessami and Resnjanskij, 2019), polling (Funk, 2016), or the voting environment (Funk, 2010; Hainmueller and Hangartner, 2019)

"moderate" proposal.[10] In nearly every case in our data, popular and cantonal majorities go hand in hand. Between 1998 and 2019, there were two votes (out of 193) in which a narrow majority of voters rejected (49.2 % and 49.9 % of voters voting yes) but the cantons approved, and one vote in which a narrow majority of voters approved (with 54.3 % of voters voting yes) while the majority of cantons did not.

In addition to the popular initiative (and the counter-proposal), the Swiss constitution grants two types of referenda rights. First, a referendum can be called on all laws issued by the federal government if supported by 50,000 signatures or eight Swiss cantons. This sort of referendum is then accepted or rejected by a simple majority of the votes cast. Higher-stakes policy choices — any changes to the constitution and some international treaties — are subject to a mandatory referendum requiring a majority of voters and cantons to be passed. For all votes (initiatives and referenda), there is no minimum voter turnout required for the ballot to be binding.

Swiss citizens vote on federal ballots two to four times per year, with each "election day" including votes on multiple proposals. Vote topics vary broadly, from social issues, to military policy, to infrastructure, to participation in international organizations, such as the European Union.[11] Between 1981 and 2019, Swiss citizens voted on 331 federal ballots, and these ballots were held on 115 election days. Given the high stakes involved, it is unsurprising that referenda are politically contentious. Political parties regularly take positions and issue voting recommendations. In the 331 votes between 1981 and 2019, the moderate right-wing party (FDP) provided a recommendation on how to vote in all but one vote; the centrist party (CVP) and the populist right-wing party (SVP) provided recommendations in all but three votes; and the major left-wing party (SP) provided a recommendation in all but 17 votes. The left and the right typically provided voters with contrasting recommendations: there was disagreement among major parties in 271 out of 331 of the votes held between 1981 and 2019.

The voting process in Switzerland is quite convenient. No registration to vote is necessary, and every eligible voter (i.e., Swiss citizen of at least 18 years of age) receives the voting documents by regular mail at home. The voter then casts the ballot either at the polling booth on the election day (always a Sunday) or through early voting.[12] Swiss voters are also provided with detailed information on the substance of the issues on which they will vote. The voting documents sent to eligible voters' homes include the precise questions, arguments for and against each proposition, and often outside opinions from interest groups. In addition, most federal votes are extensively debated in the media (TV, radio and dozens of local newspapers). Political advertising exists only

[10] In the case of a counter-proposal, voters are currently able to approve both the initiative and the counter-proposal (before 1987, voters could only approve the initiative or the counter-proposal, but not both). Voters indicate which they prefer to determine which is to be implemented if both initiative and counter-proposal were approved.

[11] Note that some referenda require a double majority of both voters and cantons to be passed. In nearly every case in our data, popular and cantonal majorities go hand in hand. There is no minimum voter turnout required for the ballot to be binding.

[12] In our sample of Genevan voters, virtually all voters make use of early voting: 90.0% of those turning out use postal voting and 4.3% use voting by internet; only 5.7% cast their vote at the polling booth on Sunday morning.

in newspapers, with political TV and radio ads prohibited under federal law.

In 1998, the Swiss public television station decided to sponsor the first widely-disseminated national voting forecasts in Switzerland. The polls, conducted by the research institute "*gfs.bern*," were eventually disseminated more broadly, through other media as well. Two rounds of polls are typically conducted: results of the first poll are published around 5 weeks before the voting Sunday — before any voting can take place — and results of the last poll are typically released 11 days before the voting date, the Wednesday in the week prior to the election date. Because our event study analysis of Geneva voter turnout relies on the *exact* date of the release of the final poll, we note here that of the 52 votes examined in our analysis of Geneva voter turnout, 2 polls were released 16 days before the voting date, 1 poll 13 days before, 2 polls 12 days before, 44 polls 11 days before, and 3 polls 10 days before.

The release of this national-level poll (and its closeness) before each vote provides the key source of variation we will exploit in our event-study analysis of Geneva voters' turnout. In our analysis of the effects of close polls depending on municipality representativeness, we will compare voter turnout before and after 1998, when polling began. And, in our analysis of the role of newspaper dissemination of poll results, we will examine newspaper articles reporting on these national polls.

3 Data and Summary Statistics

3.1 Data Description

Voter Turnout and Vote Outcomes Data on daily voter turnout in the canton of Geneva are obtained from the office of statistics of the canton. To the best of our knowledge, Geneva — the 6th largest canton, with a population of around 500,000 — is the only canton keeping detailed administrative records on the *timing* of voter turnout.[13] Beginning from approximately 2–3 weeks before election Sunday, the cantonal Service of Popular Votes and Elections registers the number of incoming ballots from early voters at a daily level. Incoming postal ballots (around 90% of the votes cast in our sample) are registered on the *very same day* of any working day (which includes election Sunday and the preceding Saturday). The relatively small number of ballots cast online (around 4%) are recorded automatically every day (including weekends and public holidays) by the e-voting system.[14] There are 52 election days in Geneva for which turnout is observed both before and after the release of pre-election poll results. We thus construct a panel of daily turnout

[13]Turnout is *not* observed for each individual vote (i.e. ballot issue) that is decided on the same election day, as the ballots are placed together in a sealed envelope. The sealed envelope is then mailed, in a larger envelope, together with the signed voter identification card. Voters nearly always cast their ballots on all issues that are decided upon in one election.

[14]We therefore aggregate votes on eligible "voting days," i.e. days when postal ballots are registered, to which we add any incoming ballots by internet recorded on weekends or public holidays immediately preceding the voting day. Our results are robust to excluding online ballots.

for the voting days preceding these 52 election days. We consider cumulative turnout rate as of each day; the log of the daily count of ballots received; the daily turnout rate as a fraction of the eligible voting population in the canton, and the daily "net" turnout rate, calculated as the turnout rate among the eligible voters who have not yet voted in a particular election.

We additionally consider data on voter turnout and referenda outcomes for all of Switzerland. These data are available in disaggregate form for all levels (municipal, cantonal and federal) since 1981 and are provided by the Swiss federal office of statistics.[15] In our analysis, we use data on: eligible voters, votes cast, the number of votes in support of the initiative, and the number of votes against the initiative.[16]

While our main analysis focuses on an *ex-ante* measure of vote closeness (derived from the polls), we also calculate an *ex post* vote closeness measure (based on actual voting outcomes), which is the share of the votes cast for the losing side in a vote. In our municipality×vote-level analysis, we use *ex post* closeness to construct a measure of a municipality's "political unrepresentativeness" prior to the release of any polls: the opposite of the correlation between each municipality's vote share closeness and the national closeness between 1981 and 1998. We also use the measure to calculate a municipality's homogeneity: how much a municipality's voting outcomes differed from 50-50, on average, prior to the release of any polls. We use party recommendations issued prior to the release of pre-election polls and municipality-level party vote shares in the preceeding legislative elections to estimate the *ex ante* support for the trailing side in the poll, which we proxy with the municipality-level vote share for parties that endorse that side in the vote.[17]

Importance of a Vote Each election day typically features several votes (ballot issues). In our analysis, we focus on the issue that voters consider most important, as it plausibly drives the turnout decision. To determine the most important vote on a given election day, we use data from post-electoral surveys: the "VOX surveys" before September 2016, and the "VOTO Surveys" after. We specifically rely on survey respondents' views of the personal importance of each voting issue (or referendum) on a given election day. The question reads: *"Let's talk about the importance this issue had for you personally. Please tell me ... how important the vote about [issue title] has been for you personally. Tell me a number between 0 and 10. 0 means not important at all, 10 very important."*

[15] The municipality-level voting data of the federal office of statistics includes 2,202 municipalities that existed at the end of our sample period, where historical municipalities that merged are aggregated to the set of municipalities in existence at the beginning of 2020. For 19 municipalities, the federal office of statistics reports incomplete data because these municipalities were subject to complex mergers not allowing the aggregation of data by adding up historical electoral returns from formerly independent municipalities. For another 7 municipalities, no data are reported because they share a common ballot box with neighboring municipalities to which electoral returns are aggregated. This leaves us with 2,176 municipalities in our data.

[16] Turnout is calculated at the level of the individual vote. In practice, turnout is very similar for all votes held on a given election day: a regression of turnout on election day fixed effects generates residuals with a standard deviation of 0.128 percentage points.

[17] Data on party recommendations is available from Année Politique Suisse (see `https://swissvotes.ch/page/dataset`) and data on national elections can be obtained from the Swiss federal office of statistics (see `https://www.pxweb.bfs.admin.ch/pxweb/de/`).

We identify the vote with the highest average personal importance score as the one whose poll closeness may affect turnout for that election day.

This survey-based measure of a vote's importance is direct, and it covers all votes we study in our analysis of voter turnout in Geneva. However, it provides incomplete coverage of votes in our analysis of canton×vote level turnout. In our analysis of municipality×vote-level turnout, we wish to study voting in the era prior to the release of polls — going back to 1981, before survey data on the importance of each voting issue were collected. We thus supplement the VOX and VOTO survey data with a count of the number of articles mentioning each vote (issue) in Switzerland's preeminent German newspaper, the *NZZ*, in the three months preceding each election day In the absence of survey data, the issue with the most *NZZ* articles is identified as the most important vote on a given election day. In our canton×vote-level analysis, we are able to include one more election day by shifting to a slightly different survey question from the VOX survey, which asks about the importance of the vote to the nation, rather than about its personal importance. Our results are nearly identical using the personal importance measure, but we prefer to maximize the sample coverage.

TABLE I.1: EXAMPLES OF ELECTION DAYS AND MOST IMPORTANT VOTES

Date	Vote Title	*NZZ* Mentions	Vote Importance (Survey)
1994-09-25	**Federal Penal Code and Military Penal Code (Racial Discrimination)**	**39**	**6.12**
1994-09-25	Federal Decision Abolishing Subsidies for Domestic Breadstuff from Tariff Revenues	16	3.48
2001-03-04	**Initiative "Yes to Europe!"**	**68**	**6.61**
2001-03-04	Initiative "for Lower Prices of Pharmaceuticals"	53	5.79
2001-03-04	Initiative "for Road Safety with 30 km/h in Built-Up Areas"	36	5.53
2009-11-29	**Initiative "against the Construction of Minarets"**	**112**	**6.91**
2009-11-29	Initiative "for a Ban on Exports of War Material"	47	6.28
2009-11-29	Federal Decision on Special Funding for Air Traffic	27	3.85
2019-05-19	**Federal Act on Tax Reform and Funding for Old Age Insurance**	**77**	**7.42**
2019-05-19	Federal Decision Adopting the EU Directive on Gun Control	13	6.49

Notes: *NZZ* Mentions measures the number of times a vote was mentioned in the *NZZ* newspaper in the three months preceding election day. Vote Importance measures the average personal importance attached by VOX/VOTO survey respondents to a vote, on a 0-10 scale (10 indicating maximum importance).

In our analysis of voter turnout in Geneva, the vote we identify as most important determines the poll whose closeness is used as the explanatory variable of interest in our empirical model. In our analysis of municipal and canton-level turnout across Switzerland, the most important vote determines both the turnout rate on a given election day (i.e., the dependent variable of interest) and the poll whose closeness is used as an explanatory variable. In practice, voter turnout varies very little across issues on an election day and our results are robust to using the average closeness across all issues.[18] Table I.1 lists a few examples of election days, with the respective issues (votes) on the ballot and their importance scores. Appendix Table A.1 lists all the election days and the most important vote on each day.

[18] See Appendix Tables A.4 and A.5 for these results.

Pre-Election Poll Results Since 1998, the Swiss Public TV and Radio Corporation (SRG) has sponsored surveys eliciting the voting intentions of Swiss citizens before all federal votes. We collected poll results, as well as the precise timing of their release from the website of the SRG. The poll results are reported as the shares of eligible voters (among those who report an intention to vote), who: (i) are definitely in favor of the proposal; (ii) are somewhat in favor of the proposal; (iii) are somewhat against the proposal; (iv) are definitely opposed to the proposal; (v) do not know; or, (vi) prefer not to answer.[19] Our main variable of interest is the predicted closeness of the final poll prior to a vote. To calculate (ex-ante) poll closeness we first construct the "share yes": the total "yes" support (groups (i) and (ii), who are definitely or somewhat in favor) divided by the total number of respondents indicating support for "yes" or "no" (groups (i), (ii), (iii), and (iv)). We then analogously construct the "share no," and code poll closeness as the share supporting the trailing side in the poll. Closeness thus varies between 0 and 50. In the analysis that spans time periods with and without national polls, we use our *ex post* vote closeness measure, defined as the vote share of the losing side in an election.

Data on Newspaper Coverage of Polls The Swiss Agency of Media Research (*WEMF*) has regularly conducted surveys on newspaper readership since the year 2000, with random samples of cantonal inhabitants interviewed and asked which newspapers they read. The Agency generously shared their data on canton-level newspaper readership with us, allowing us to construct a list of newspapers read by at least 10% of a canton's inhabitants in a given year. Overall, there are 50 newspapers on this list, many of which are read in several cantons (see Appendix Table A.2, for a list of the newspapers). To measure canton-level coverage of pre-election polls, we count the articles mentioning a pre-election poll in each of the newspapers read by at least 10% of a canton's inhabitants, between 2000 and 2014.[20] This absolute count of "poll mentions" is our baseline measure of newspaper poll coverage.[21]

In our empirical analysis below, we will address concerns regarding the endogenous local newspaper coverage of close polls by exploiting a canton's voters' (arguably) "incidental" exposure to polls. We propose that newspaper editors may target their news coverage toward their largest cantonal audience; readers exposed to reporting in *other* cantons will read it for reasons other than their own canton's election-specific interest. We thus can decompose *total* coverage of polls in a canton into two components: first, *endogenous* coverage, which is arguably targeted to-

[19] Note that the poll does not project whether the referendum is likely to receive support from a majority of cantons (which technically is required to pass many of the referenda we study). As noted above, however, the popular vote has nearly always been the binding factor determining the passage of referendum; thus, information on the closeness of this component of the vote alone will be highly informative to voters.

[20] To gauge newspaper coverage of polls, we used three different strategies in this search: online databases, "Factiva" (`https://global.factiva.com`) and "Swissdox" (`https://swissdox.ch/`); newspapers' own online archives; and, manual search in the Swiss National Library in Bern.

[21] We also present analyses using readership-weighted newspaper poll coverage in Appendix Table A.6. Our results are nearly unchanged using this alternative measure.

ward that canton, because it represents a newspaper's largest cantonal audience; second, *incidental* coverage, to which a canton is exposed despite a newspaper's largest audience being in a different canton. We use the newspaper readership data to define incidental poll exposure in two ways: first, coverage by a source with a majority readership in a different canton; second, and more conservatively, coverage by a source with at least 85% readership in other cantons. Appendix Figure A.2, shows how endogenous and incidental coverage vary by canton.

The Political "Supply Side": Political Advertising in Newspapers For our analysis of voter turnout in the canton of Geneva, we hand-collected all political advertisements related to the 52 referenda studied between 2001 and 2019 for the two most widely-read Genevan newspapers: *Le Temps* and *Tribune de Genève*. We aggregate these data to counts of political ads relating to each of the 52 votes at the *daily* level.

For our analysis of voter turnout across Switzerland, we measure political advertising using data from Kriesi (2009) and the *Année Politique Suisse* on political ads in six major Swiss newspapers: *NZZ*, *Blick*, *Tages-Anzeiger*, *Le Matin*, *Journal de Genève*, and *Tribune de Genève*.[22] To measure campaigning intensity at the vote level, we calculate the sum of ads placed in these six major newspapers relating to each vote.

For our canton$\times$vote-level analysis, we collected advertising data from a much broader set of newspapers: all of the newspapers read by at least 10% of any canton's inhabitants. We sum up to the canton$\times$vote level our counts of political ads relating to each vote for each newspaper read in each canton.

3.2 Summary Statistics

We present summary statistics for the datasets used in our empirical analysis in Table I.2. First, we consider our primary dataset of interest: vote$\times$day-level data for the canton of Geneva (Panel A). We observe voting, on days both before and after polls are released (around 15 days per vote) for 52 "most important" votes held on election days since 2001 (766 vote$\times$day observations in total). Around 3% of eligible voters vote on an average day; cumulative turnout is around 28% on the average day (which of course will be higher by the day of the election itself). The average *ex ante* poll closeness in our sample is 38.40 (that is a 62-38 margin for the winning side). Finally, on the average day in our sample, we count 1.6 newspaper ads related to the upcoming vote in the two major Genevan newspapers.[23]

[22] Hanspeter Kriesi generously shared data for votes from 1981 to 2014, which we supplemented with the Année Politique Suisse for more recent votes (see `https://anneepolitique.swiss/pages/campaign_research`, last accessed February 16, 2022).

[23] We are missing ads data for 52 voting days — the election Sunday for each of our votes.

Second, we turn to the vote-level dataset at the federal level (Panel B). It is composed of the "most important" issue for each of the 115 election days for which we have voter turnout data between 1981 and 2019. On average, over 40% of eligible voters turn out; the average margin is around 65%–35%; voters rate the importance of the issue to themselves as a 6 out of 10 in importance; and, the average vote saw around 100 ads placed in the major Swiss newspapers.[24]

Third, we construct a canton×vote-level dataset, including voting data for 26 cantons and 37 "most important" votes held between 1998 and 2014 (Panel C). This panel is limited to votes for which we have a count of newspaper articles mentioning polls and political ads relating to votes in the 50 newspapers read by at least 10% of a canton's population. One can see that this slightly smaller sample, relative to the Geneva dataset that also examines the poll era, does not look very different in terms of poll closeness: on average, this is around 38 (i.e., a 38% share for the losing side) in both samples. We count around 4 newspaper articles mentioning polls for the average vote, with 2.5–3 articles mentioning polls published in newspapers read in a canton, but having a larger market elsewhere (our measure of "incidental" exposure to information). We count, on average, around 70 political advertisements on the most important vote in the newspapers read in a canton. Finally, we note that the personal importance of this set of votes looks very similar to the full sample.[25]

Fourth, we construct a municipality×vote-level dataset, including voting data for 2,176 municipalities and 115 "most important" votes held between 1981 and 2019 (Panel D).[26] In addition to summary statistics that match the vote-level data at the federal level (subject to differences due to the construction of the municipal-level data), one can see that 60% of the votes in our municipality×vote-level analysis were held after polls were introduced; the average municipality has an unrepresentativeness value of -0.59 (meaning that the average correlation between national and municipality closeness is around 0.60), but this ranges from close to -1 (a nearly perfect correlation between municipality and national closeness) to around 0 (implying no correlation between the municipality closeness and national closeness).

[24]Data on the importance of the vote are missing for 32 votes because surveys did not include this question before 1993. Missing data for political ads are inherited from the Kriesi (2009) dataset.

[25]Importance data are missing for five observations because the survey did not receive responses from small cantons for these votes.

[26]We are missing 26 out of 2,202 municipalities that existed at the end of our sample period. For 19 municipalities, the federal office of statistics reports incomplete data because these municipalities were subject to complex mergers not allowing the aggregation of data by adding up historical electoral returns from formerly independent municipalities. For another 7 municipalities, no data are reported because they share a common ballot box with neighboring municipalities to which electoral returns are aggregated.

TABLE I.2: SUMMARY STATISTICS

PANEL A: VOTE × DAY-LEVEL DATA (GENEVA)	Mean	Std. Dev.	Min.	Max.	Obs.
Net Turnout (%)	4.61	1.99	0.02	16.75	766
Turnout / All Voters (%)	3.32	1.33	0.02	12.76	766
Log(Turnout)	8.76	0.55	3.91	10.02	766
Cumulative Turnout (%)	28.52	14.50	0.02	62.90	766
Ex Ante Closeness	38.40	7.65	18.89	48.96	766
Advertisements	1.61	2.47	0	19	714
PANEL B: VOTE-LEVEL DATA					
Turnout (%)	43.78	8.33	27.60	78.78	115
Ex Post Closeness	35.30	9.75	8.03	49.91	115
Importance	6.13	0.87	3.22	7.79	83
Advertisements	107.56	145.18	0	1146	112
PANEL C: CANTON × VOTE-LEVEL DATA					
Turnout (%)	47.27	8.91	21.67	72.61	962
Ex Ante Closeness	37.88	7.47	18.89	48.91	962
Poll Mentions	4.28	3.42	0	24	962
Incidental Poll Mentions	2.89	3.04	0	20	962
Incidental Poll Mentions (< 15% Market Share)	2.40	2.83	0	15	962
Importance	6.12	1.13	0.50	10.00	957
Advertisements	73.93	68.18	0	403	962
PANEL D: MUNICIPALITY × VOTE-LEVEL DATA					
Turnout (%)	44.00	13.02	3.20	100.00	250240
Poll Era	0.60	0.49	0	1	250240
Unrepresentativeness	-0.59	0.19	-0.93	0.03	250240
Trailing Side's Estimated Support	38.23	23.02	0.00	100.00	124032
Vote Share for Trailing Side	42.83	18.25	0.00	100.00	124032
Electorate Size (in 1000)	1.98	7.08	0.03	233.14	250240

Notes: In each dataset, vote-specific variables refer to the most important vote per election day, as indicated by self-reported importance in VOX/VOTO surveys, or, for years prior to the existence of survey measures, by the number of vote mentions in the *NZZ* in the three months preceding the vote. *Vote × Day-level Data:* Net Turnout measures turnout as the daily number of votes cast, in percent of eligible voters not having cast their vote on earlier days. Turnout / All Voters is the daily number of votes cast, in percent of eligible voters. Log(Turnout) is the natural logarithm of the daily number of votes cast. Cumulative Turnout is the daily running total of votes cast, in percent of eligible voters. *Ex Ante* Closeness measures the trailing side's vote share at the federal level in percent, as predicted by the pre-election poll, and varies from 0 to 50 (50 indicating maximum closeness). Advertisements is the daily count of political ads placed in the two major Genevan newspapers *(Tribune de Genève , Le Temps)*. *Vote-level Data:* Turnout is the number of votes cast, in percent of eligible voters at the federal level. *Ex Post* Closeness is the vote share of the losing side at the federal level. Importance measures the average personal importance attached by VOX/VOTO survey respondents to a vote, on a 0-10 scale (10 indicating maximum importance). Advertisements is the number of political ads placed in the six major Swiss newspapers *(NZZ, Tages-Anzeiger, Blick, Tribune de Genève , Le Temps, Le Matin)* in the four weeks preceding election day. *Canton × Vote-level Data:* Turnout measures cantonal turnout as the number of votes cast, in percent of eligible voters. *Ex Ante* Closeness defined and measured as in vote × day-level data. Poll Mentions is the number of times the pre-election poll for a vote is mentioned in cantonal newspapers read by at least 10% of a canton's inhabitants. Incidental Poll Mentions are poll mentions in cantonal newspapers whose largest market is in another canton. Incidental Poll Mentions (<15 % Market Share) are poll mentions in cantonal newspapers, excluding newspapers for which the canton is either the largest market or makes for more than 15% of the newspaper's readership. Importance measures the average personal importance attached by a canton's VOX survey respondents to a vote. Advertisements is the number of political ads placed in cantonal newspapers in the month preceding election day. *Municipality × Vote-level Data:* Turnout is the number of votes cast, in percent of eligible voters at the municipal level. Poll Era is a dummy variable equal to 1 for the 69 votes held after the introduction of pre-election polls. Unrepresentativeness is a municipality's historical tendency to produce voting results unrepresentative of national-level closeness, measured as the negative of the correlation coefficient between municipality-level and national-level *ex post* closeness of voting results before pre-election polls were introduced. Trailing Side's Estimated Support is a proxy for a municipality's ex-ante predisposition to vote for the side which is trailing in the pre-election poll, measured as the percentage share of votes in the preceeding national election for parties having recommended to vote for the side trailing in the pre-election poll. Vote Share for Trailing Side is a municipality's vote share for the side which was trailing in the pre-election poll. Electorate Size is the average number of eligible voters (in thousands) in a municipality across votes held before pre-election polls were introduced.

4 Polls and Turnout: Event Study Evidence

4.1 The Identification Challenge

Abundant evidence exists of a *correlation* between election closeness and voter turnout. This correlation can arise from three sources: first, voters may turn out more when they anticipate a close election — this is the *causal effect* of closeness, working through voter beliefs, that is of interest to us. Second, unobserved *issue type* may drive both closeness and turnout: for example, more important referenda issues (or election races) may be more contentious (and hence closer) and also motivate voter turnout. Third, the actions of the political *supply side*, that is, political actors and organizations with a stake in the referendum (election) outcome, may be correlated with both voter turnout and closeness: for example, high levels of political advertising on two sides of an issue would tend to drive up turnout and closeness.

One can see in Figure I.1 that in our setting, the closeness of Swiss referendum results is indeed strongly, positively associated with voter turnout (Panel A). But, the importance of an issue (measured in voter surveys) and political advertising are also strongly, positively associated with voter turnout (Panels B and C). And, the importance of an issue and political advertising are strongly, positively associated with referendum closeness (Panels D and E) and with each other (Panel F).

4.2 Empirical Framework

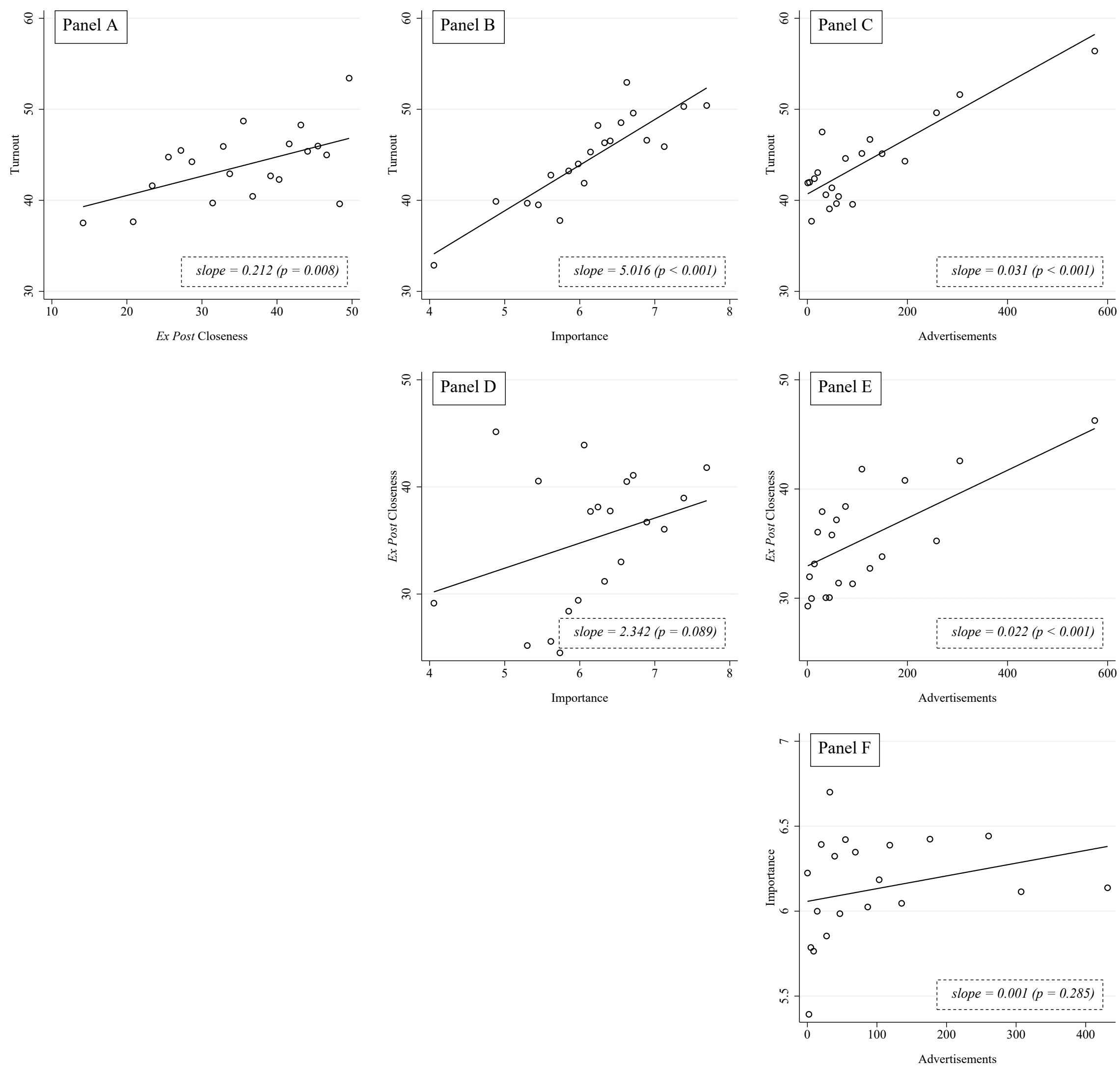
The ideal experiment would randomly shock voters' beliefs about election closeness, while holding fixed the issue type and the political supply side. We identify a setting that approximates this experiment: the sharp arrival of information about election closeness in newly-released polls. Using unique data from the canton of Geneva that allow us to observe voter turnout day-by-day, around the day when polls are released, we can test whether the release of *closer* polls differentially increases voter turnout (accounting for issue fixed effects). Furthermore, we can evaluate whether the natural experiment we study is a good one, by testing for differential turnout levels and trends *prior* to the release of closer polls — such an effect might arise if closer polls were anticipated; if issue types that were associated with closer polls were also associated with different turnout trends, or if the political supply side were differentially active prior to poll release on issues that (eventually) have closer polls.

To be precise, we estimate the following model:

$$turnout_{vd} = \sum_{d} \beta_d \; closeness_v + \alpha_v + \gamma_d + \epsilon_{vd}. \tag{1}$$

This is a simple event study, examining voter turnout by vote×day, where "day" is the number

FIGURE I.1: TURNOUT, CLOSENESS, ISSUE TYPE, AND THE POLITICAL SUPPLY SIDE



Notes: The matrix of binned scatter plots shows pairwise correlations of turnout, *ex post* closeness, vote importance and political advertisements at the vote (i.e., election) level. Turnout is the number of votes cast, as a percentage of eligible voters at the national level. *Ex Post* Closeness is the vote share of the losing side in percent. Importance is the self-reported personal importance attached by respondents of the VOX/VOTO surveys to a vote, on a scale from 0 to 10. Advertisements is the count of political ads in the six major Swiss newspapers *(NZZ, Tages-Anzeiger, Blick, Tribune de Genève, Le Temps, Le Matin)* during the four weeks preceding Election Day. Lines represent the bivariate linear fit with reported slope parameters estimated by simple OLS using heteroskedasticity-robust standard errors. Reported p-values refer to a test that the slope parameter is equal to zero.

of days prior to, or following release of a poll. The coefficients of interest are the sequence of β_d, which are estimated as coefficients on the interaction of poll closeness ($closeness_v$) with a full set of day-to-poll indicators.[27] These tell us how turnout varies in the days before or after a *closer* poll is

[27] We consider only linear effects of closer polls in this analysis, and those below, due to a lack of power to identify

released — accounting for vote (v) and day-to-poll (d) fixed effects. Our proposed mechanism of a causal effect of closer polls through changed voter beliefs about closeness suggests that β_d will be very close to 0 for $d < 0$ and significant and positive for some $d > 0$.

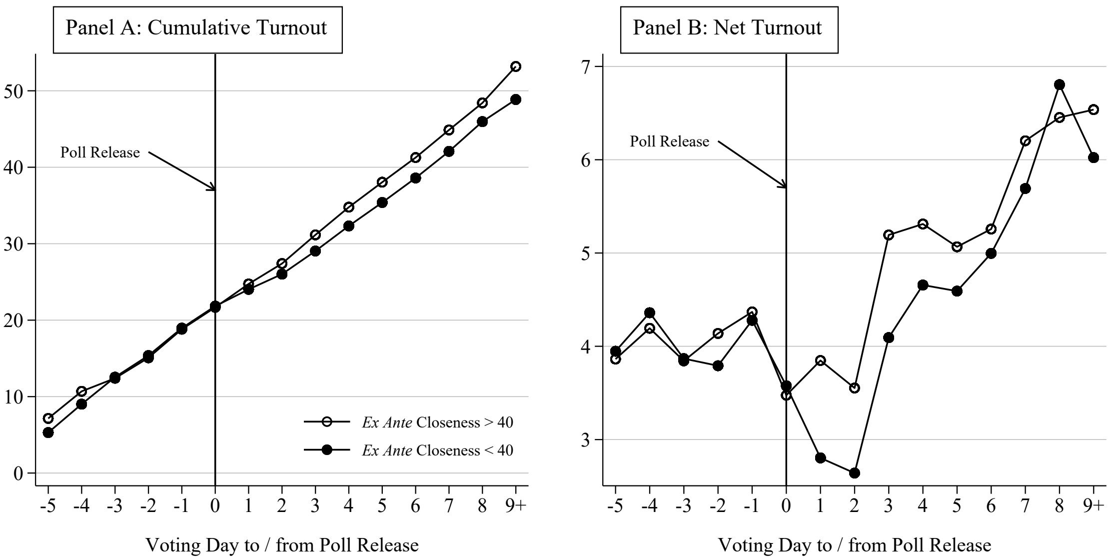
In addition to examining voter turnout, we can also directly study the political supply side by estimating the event study model in equation (1), but predicting political ads by vote$\times$day. If close polls causally shape turnout, one might expect the political supply side to respond to them as well — albeit likely with some lag given the need to develop ads and place them. Crucially, we predict a response of voter turnout *prior* to any political supply side response.

4.3 Evidence from the Canton of Geneva

Our analysis of voter turnout in Geneva examines whether, in the days following the release of closer polls, voters turn out more. In Figure I.2, we present *prima facie* evidence that close polls increase turnout, showing (raw) cumulative voter turnout (Panel A) and net voter turnout rates (Panel B) day by day around the time when polls are released, and splitting polls into above- or below-median closeness (above or below a 40% vote share for the losing side). One can see that voter turnout follows a *very* similar pattern day by day up to poll release for votes that would eventually have closer or less close polls. But, once polls are released, voter turnout diverges sharply, particularly in the three days immediately following poll release.

non-linearities in the relationship between poll closeness and turnout.

FIGURE I.2: UNCONDITIONAL TURNOUT BEFORE AND AFTER POLL RELEASE

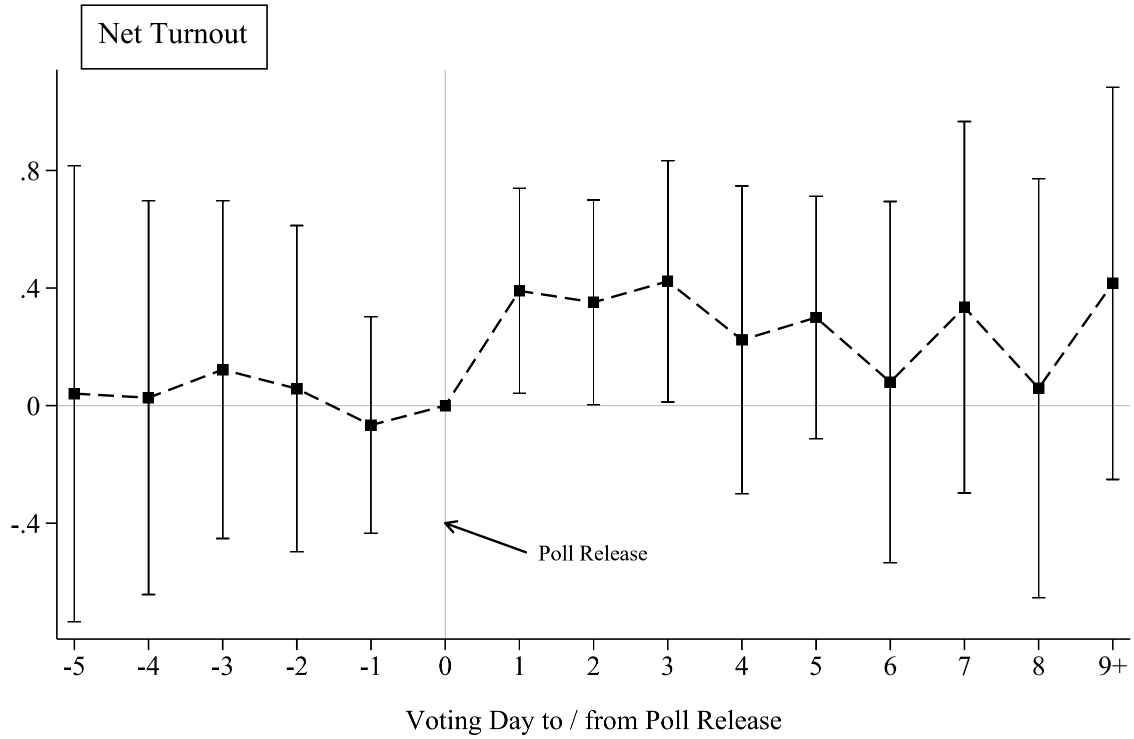


Notes: Panel A plots daily cumulative turnout, i.e., the percentage share of Genevan voters who turned out at or before a given voting day, separately for votes with *ex ante* poll closeness above or below the sample median of 40 (i.e., a losing side vote share above or below 40). Panel B shows an analogous plot for net turnout, i.e., the daily inflow of ballots divided by the stock of remaining voters (who did not turn out before a given voting day). The sample is an unbalanced panel of 52 votes observed from 5 voting days before to 9 voting days after poll release.

We next more formally test for the effect of closer polls. We estimate the event study equation (1) presented above, examining the effects of closer polls on net voter turnout rates day by day.[28] One can see in Figure I.3 coefficient estimates (and 95% confidence intervals) on the interaction of (standardized) poll closeness with each day-to-poll indicator (with the day of poll release the omitted reference day). We present these coefficients along with standard errors clustered at the vote level in Table I.3, column 1. In addition, we present p-values from the wild cluster bootstrap in brackets.

[28]An important limitation of a high-frequency event study analysis such as the one we conduct is that it identifies the immediate effects of close polls, rather than the total effect of close polls on turnout for a given vote.

FIGURE I.3: THE EFFECT OF CLOSER POLLS ON NET VOTER TURNOUT: EVENT-STUDY BASELINE



Notes: The event study graph plots day-specific effects of a one standard deviation increase in *ex ante* (poll) closeness on net turnout, i.e., the daily inflow of ballots in the canton of Geneva divided by the number of remaining Genevan voters who did not turn out before a given voting day, conditional on vote and voting day fixed effects. Squares represent coefficients and spikes depict 95% confidence intervals from OLS estimates (reported in Table I.3, Column 1). The sample is an unbalanced panel of 52 votes observed from 5 voting days before to 9 voting days after poll release, where the day prior to poll release is the omitted category of reference.

Prior to the day when polls are released, we see no difference in turnout rates depending on the closeness of the to-be-released poll. In contrast, on the first three days after a closer poll is released, voter turnout rates are *significantly higher* — by around 0.4 percentage points.[29] The 1.2 percentage point increase in turnout over three days arising from a one standard deviation (7.65 percentage point) increase in poll closeness actually matches the existing observational literature

[29]It is worth clarifying how polls released on day t can produce an increase in votes *counted* on day $t+1$. This can arise through several mechanisms: first, when poll results are released on the morning of day t, voters may respond by mailing a ballot in time for the vote to be counted on day $t+1$. Indeed, since 2003, Geneva prepays the ballot envelope for "A Mail," which arrives the next day when put into a mailbox or brought to the post office before closing hours in the evening (confirmed in personal communication with the cantonal Service of Popular Votes and Elections, April 27, 2021). Remember also that incoming postal ballots are registered on the very same day. Second, even when polls are released on the evening of day t, voters are able to hand-deliver their ballots to the electoral office on day $t+1$ or to vote online on day t or day $t+1$.

quite well. For example, Cox and Munger (1989) and De Paola and Scoppa (2014) estimate that a 10 percentage point increase in closeness raises voter turnout by 1–2 percentage points; our estimates fall in this range. In Table I.3, column 2, we also estimate models including fixed effects for each day-to-election (not perfectly collinear with day-to-poll because the poll release day is not always the same number of voting days prior to the election). These fixed effects have no impact on our results.[30] These effects on turnout are not merely vote shifting across time, as coefficient estimates remain above 0 up through election day (consistent with the higher cumulative turnout for votes with closer polls seen in Figure I.2). We can reject that the cumulative effect of all post-poll coefficients is less than or equal to zero ($p < 0.1$).

We present several robustness exercises in Figure I.4. First, in Panel A, we pool net voter turnout into two-day bins, which increases the precision of the estimated time-varying effect of closer polls, and confirms our baseline results. In Panel B, we adjust the denominator of the voter turnout rate, using the fixed eligible number of voters, rather than accounting for the individuals who already voted on prior days; our results are qualitatively unchanged (though given the large turnout effect on the first day after poll release, subsequent estimated effects are smaller). In Panel C, we present estimates from a balanced panel, limiting the window to 2 days prior to poll release through election day, as some votes do not have voting data for earlier days. One can see that our results are not sensitive to this choice of sample window. Finally, in Panel D, we examine the log of the daily turnout level as the outcome, and again our results are unaffected. In Table I.3, columns 3, and 4, we present coefficient estimates, clustered standard errors, and p-values from the wild cluster bootstrap, from specifications examining log turnout as the outcome and one can see that results are very similar.

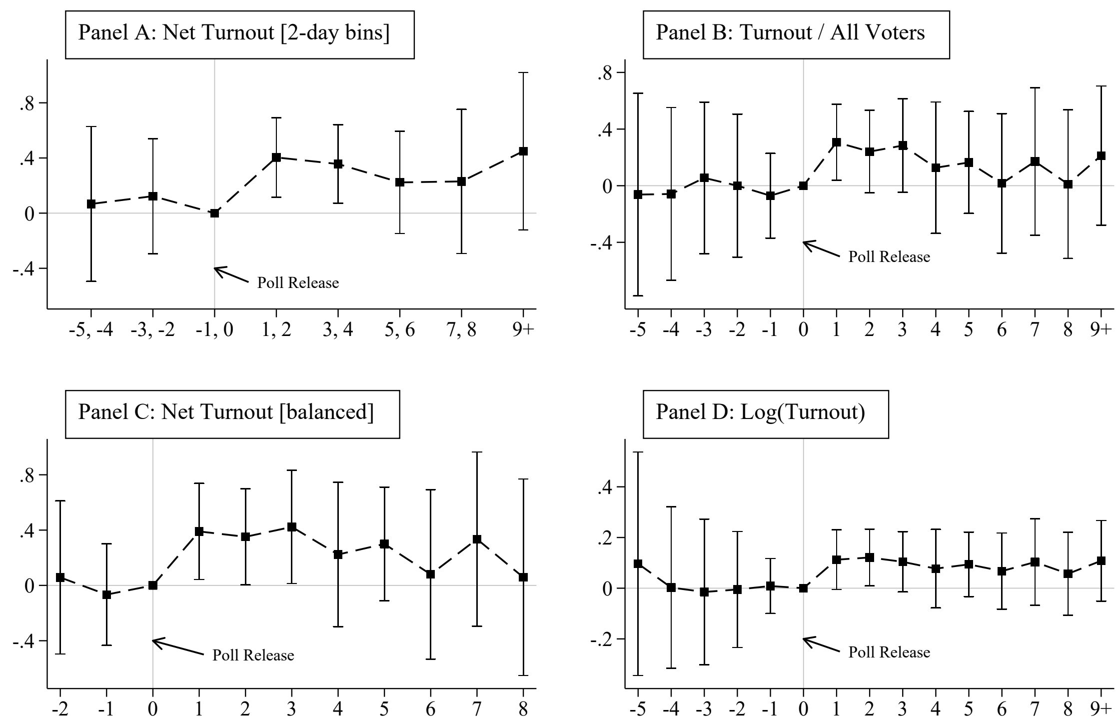
[30]Note that day-to-election fixed effects also account for differences in turnout by days of the week, which are perfectly collinear with day-to-election fixed effects (because election day is always a Sunday).

TABLE I.3: DAILY TURNOUT BEFORE AND AFTER POLL RELEASE DEPENDING ON POLL CLOSENESS: SINGLE DAYS

	Net Turnout (%)		Log(Turnout)	
	(1)	(2)	(3)	(4)
5 days before poll × *Ex Ante* Closeness (std.)	0.0408	-0.0181	0.0964	0.0757
	(0.3864)	(0.3884)	(0.2195)	(0.2230)
	[0.920]	*[0.965]*	*[0.656]*	*[0.703]*
4 days before poll × *Ex Ante* Closeness (std.)	0.0268	-0.0721	0.0027	-0.0329
	(0.3336)	(0.3288)	(0.1587)	(0.1527)
	[0.943]	*[0.850]*	*[0.991]*	*[0.901]*
3 days before poll × *Ex Ante* Closeness (std.)	0.1224	0.0240	-0.0149	-0.0393
	(0.2862)	(0.2911)	(0.1432)	(0.1459)
	[0.719]	*[0.947]*	*[0.973]*	*[0.924]*
2 days before poll × *Ex Ante* Closeness (std.)	0.0576	0.0854	-0.0051	0.0056
	(0.2765)	(0.2850)	(0.1140)	(0.1181)
	[0.868]	*[0.812]*	*[0.987]*	*[0.986]*
1 day before poll × *Ex Ante* Closeness (std.)	-0.0664	-0.0591	0.0087	0.0140
	(0.1835)	(0.1892)	(0.0540)	(0.0557)
	[0.717]	*[0.754]*	*[0.870]*	*[0.796]*
1 day after poll × *Ex Ante* Closeness (std.)	0.3905**	0.3709**	0.1125*	0.1094*
	(0.1737)	(0.1746)	(0.0586)	(0.0600)
	[0.045]	*[0.055]*	*[0.079]*	*[0.088]*
2 days after poll × *Ex Ante* Closeness (std.)	0.3515**	0.3271*	0.1211**	0.1155**
	(0.1734)	(0.1764)	(0.0558)	(0.0569)
	[0.061]	*[0.080]*	*[0.045]*	*[0.055]*
3 days after poll × *Ex Ante* Closeness (std.)	0.4230**	0.4414**	0.1042*	0.1114*
	(0.2045)	(0.2098)	(0.0592)	(0.0620)
	[0.046]	*[0.044]*	*[0.088]*	*[0.083]*
4 days after poll × *Ex Ante* Closeness (std.)	0.2235	0.1975	0.0773	0.0765
	(0.2609)	(0.2702)	(0.0771)	(0.0811)
	[0.413]	*[0.482]*	*[0.329]*	*[0.355]*
5 days after poll × *Ex Ante* Closeness (std.)	0.2998	0.3205	0.0941	0.1019
	(0.2053)	(0.2104)	(0.0634)	(0.0658)
	[0.158]	*[0.142]*	*[0.147]*	*[0.131]*
6 days after poll × *Ex Ante* Closeness (std.)	0.0801	0.0790	0.0675	0.0692
	(0.3061)	(0.3094)	(0.0749)	(0.0775)
	[0.799]	*[0.803]*	*[0.378]*	*[0.378]*
7 days after poll × *Ex Ante* Closeness (std.)	0.3347	0.3398	0.1032	0.1080
	(0.3147)	(0.3237)	(0.0851)	(0.0885)
	[0.298]	*[0.301]*	*[0.233]*	*[0.227]*
8 days after poll × *Ex Ante* Closeness (std.)	0.0591	0.0597	0.0570	0.0607
	(0.3548)	(0.3558)	(0.0815)	(0.0838)
	[0.868]	*[0.866]*	*[0.486]*	*[0.470]*
9+ days after poll × *Ex Ante* Closeness (std.)	0.4162	0.3809	0.1081	0.1065
	(0.3324)	(0.3300)	(0.0793)	(0.0814)
	[0.231]	*[0.266]*	*[0.189]*	*[0.204]*
Test of Cumulative Effects After Poll Release *(p-value)*	*0.092*	*0.103*	*0.074*	*0.079*
	[0.102]	*[0.110]*	*[0.079]*	*[0.083]*
Test of Joint Significance of Leads *(p-value)*	*0.973*	*0.962*	*0.992*	*0.957*
	[0.986]	*[0.980]*	*[1.000]*	*[0.995]*
Outcome Mean	4.611	4.611	8.764	8.764
R-squared	0.498	0.518	0.234	0.257
Observations	766	766	766	766
Vote Fixed Effects	Y	Y	Y	Y
Voting Day from/to Poll Fixed Effects	Y	Y	Y	Y
Day to Vote Fixed Effects	N	Y	N	Y

Notes: The table presents OLS estimates with two measures of daily turnout in Geneva as dependent variables: Net Turnout (columns 1 and 2) defined as the number of votes cast, in percent of eligible voters net of those voters who cast their vote on earlier days; and Log(Turnout) (columns 3 and 4) defined as the natural logarithm of the number of votes cast. *Ex Ante* Closeness is the trailing side's vote share predicted by the pre-election poll whose release date is the omitted day of reference. Test of Cumulative Effects After Poll Release reports the p-value of a one-sided F-test that the sum of the coefficients on days after poll × *Ex Ante* Closeness is less than or equal to zero. Test of Joint Significance of Leads reports the p-value of an F-test that the coefficients on before poll × *Ex Ante* Closeness are all equal to zero. P-values of analogous Wald tests based on the wild cluster bootstrap in brackets. The sample is an unbalanced panel of 52 votes held between 2001 and 2019 observed from 5 voting days before to the last voting day after poll release. Standard errors in parentheses, clustered at the vote level: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

FIGURE I.4: THE EFFECT OF CLOSER POLLS ON NET VOTER TURNOUT: EVENT-STUDY ROBUSTNESS



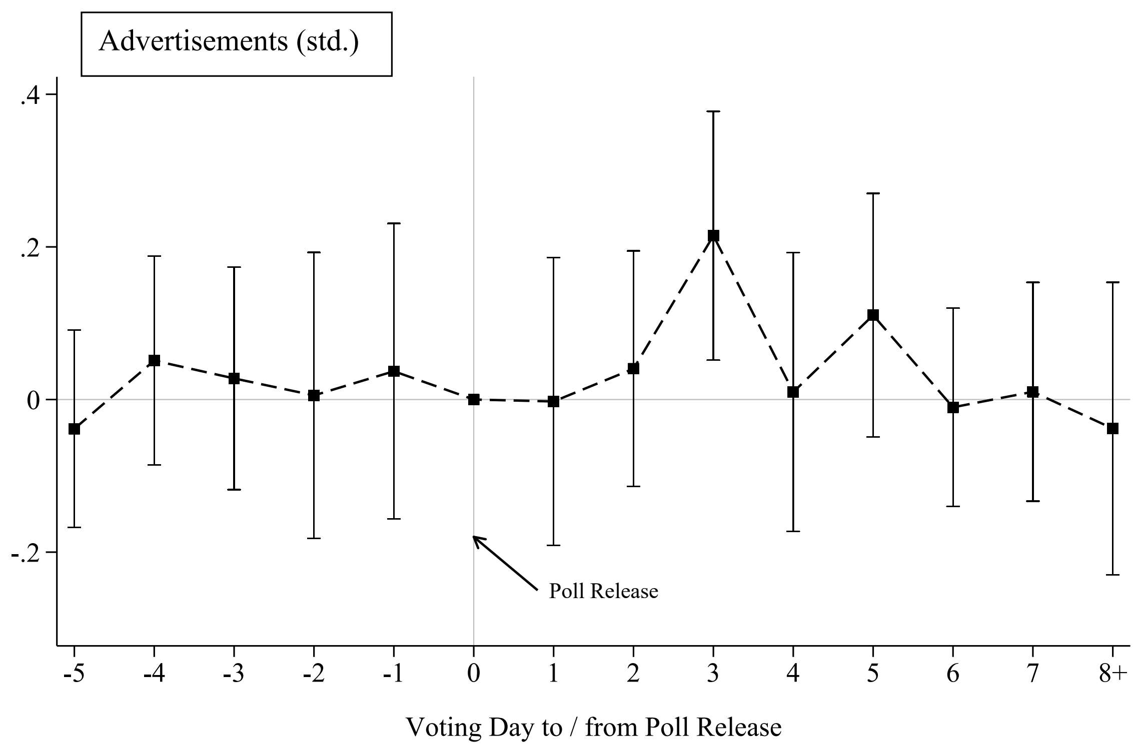
Notes: The figure shows variants of the event study graph presented in Figure I.3. Panel A plots coefficients and 95% confidence intervals for bins of two voting days, reported in Appendix Table A.3, Column 1, using the day of poll release as well as the day just before poll release as the omitted category of reference. Panel B uses the daily inflow of ballots divided by all eligible Genevan voters as an alternative measure of turnout. Panel C drops all voting days in which not every vote has ballots counted, and instead uses a balanced panel of 52 votes observed from 2 voting days before to 8 voting days after poll release. Panel D uses the natural logarithm of the daily number of incoming ballots in the canton of Geneva as an alternative measure of turnout, and plots OLS estimates reported in Table I.3, Column 3.

The results presented in Figures I.3 and I.4, as well as Table I.3 (and in Appendix Table A.3) provide evidence of a causal effect of anticipated closeness on voter turnout. Higher turnout just after the release of close polls is not driven by issue type: time-invariant issue type that might be associated with voter turnout is accounted for by the vote fixed effects; day-varying effects of issue type on voter turnout are unlikely in light of the insignificant differences in voter turnout rates observed for all of the days prior to the release of closer polls.

Nor can the political supply side account for the response of voter turnout to the release of closer polls. The absence of pre-trends suggests that the supply side was not differentially active prior to the release of close polls; poll results do not seem to have been anticipated. However, the release of polls may affect the supply side directly (if this information about closeness was not available to campaigns before) or indirectly (e.g., because anticipated greater voter turnout in closer votes increases the returns to persuasion through ads).

We thus estimate our event study model (equation (1)), but now examining the effects of closer polls on political advertisements day by day, both before and after poll release. The results are reported in Figure I.5. As in Figure I.3, we find no difference in political behavior (in this case advertisements) depending on the closeness of the to-be-released poll prior to poll release. After the release, we continue to see no effect of closer polls on ads until three days after the poll, when we observe a significant increase in ads in response to a closer poll for one day. This suggests that there is *some* supply side response to closer polls, but that it appears with a lag. It also suggests that endogenous changes in the behavior of the political supply side cannot account for all of the voter turnout effect that we observe in response to closer polls: political ads printed three days after the release of a poll would generally affect votes counted four or more days after poll release. Yet we find the largest effects of close polls on votes counted in the first three days after poll release.

FIGURE I.5: THE EFFECT OF CLOSER POLLS ON POLITICAL ADVERTISEMENTS: EVENT-STUDY



Notes: The event study graph replicates Figure I.3 with a standardized measure of political campaigning activity as the outcome. It plots day-specific effects of a one standard deviation increase in *ex ante* (poll) closeness on the standardized number of political advertisements in Geneva's two major newspapers *(Tribune de Genève, Le Temps)*, conditional on vote fixed effects and voting day fixed effects. The sample is an unbalanced panel of 52 votes observed from 5 voting days before to 8 voting days after poll release, where the day of poll release is the omitted category of reference. The last voting day of each vote is dropped because there are no Sunday editions of Geneva's major newspapers.

5 Poll Coverage and Turnout

Our results thus suggest that polls causally affect voters' turnout by providing them with information about upcoming election closeness. We next test a key auxiliary prediction: that greater coverage of close polls will differentially increase turnout. Quite simply, in locations where individuals read newspapers that report more on poll results, the impact of poll closeness should be magnified.[31] Using our canton×vote panel data, we test whether there exists a differential positive relationship between *ex ante* poll closeness and turnout in cantons with greater reporting on polls in local newspapers, controlling for vote fixed effects — and thus a national-level "issue type" — as well as canton fixed effects. We estimate the following model:

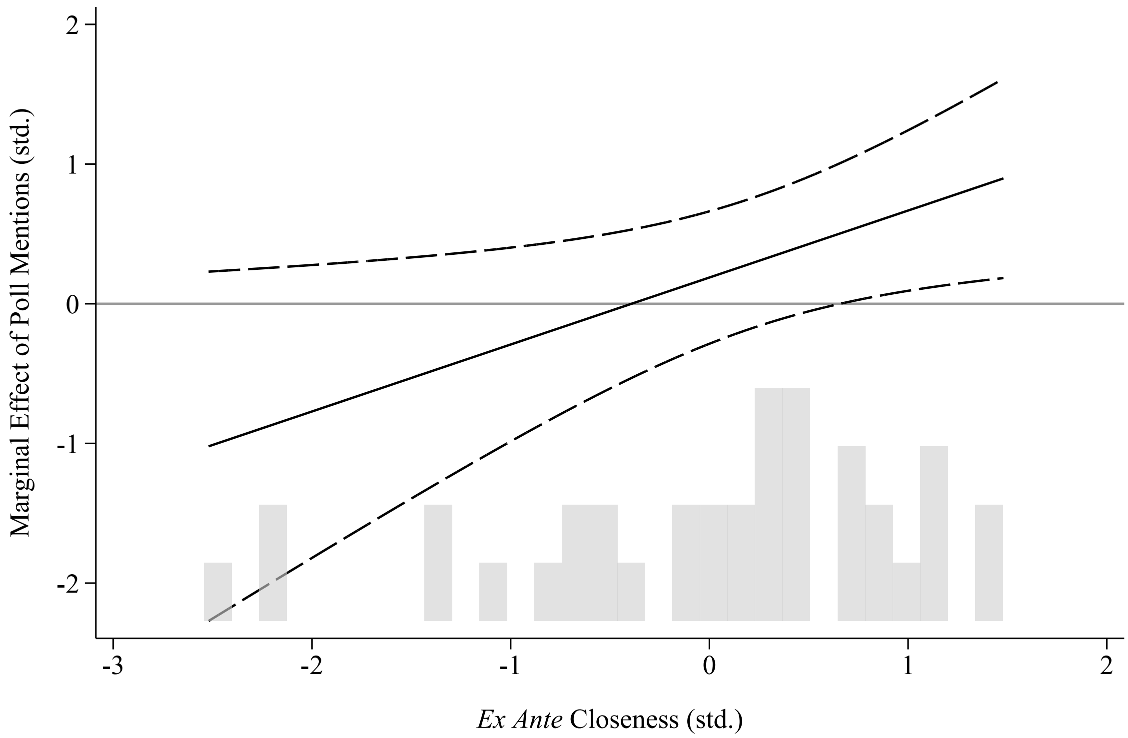
$$turnout_{cv} = \phi_c + \mu_v + \psi_1\, closeness_v \times coverage_{cv} + \psi_2\, coverage_{cv} + u_{cv}, \tag{2}$$

where $turnout_{cv}$ is the turnout rate (in percent) in canton c for vote v, ϕ_c are a set of canton fixed effects, and μ_v are a set of vote fixed effects. The interaction $closeness_v \times coverage_{cv}$ is the explanatory variable of interest, with the coefficient ψ_1 telling us whether close polls have a differential impact on turnout specifically when they are covered more by a canton's newspapers.

In Table I.4, Panel A, column 1, one can see that indeed, voter turnout is significantly greater when *ex ante* closer polls are reported on more often. We plot the estimated effect of one standard deviation greater poll coverage across levels of (standardized) poll closeness in Figure I.6. One can see that a poll that is one standard deviation closer than average (where there is substantial support in the data) increases voter turnout by a statistically significant 0.5 percentage points when newspaper coverage is one standard deviation greater. At average closeness, more coverage has little effect, and when newspapers report more on polls that are *not* close, turnout is predicted to be substantially smaller, as one would expect.

[31] Note that voters receive information about poll closeness from other sources of media as well — in particular, from national Swiss TV and radio. This "common information," available across Swiss cantons would tend to bias our estimates toward 0. On the other hand, exposure to polls on TV or radio that is positively correlated with newspaper coverage across space would tend to produce an overestimate.

FIGURE I.6: MARGINAL EFFECTS OF NEWSPAPER POLL MENTIONS DEPENDING ON POLL CLOSENESS



Notes: The solid line plots the total effect of a one standard deviation increase in poll mentions in cantonal newspapers on cantonal turnout depending on standardized *ex ante* (poll) closeness. Dashed lines represent 95% confidence intervals. The plot is based on OLS estimates reported in Table I.4, Panel A, Column 1. The histogram shows the distribution of (standardized) *ex ante* (poll) closeness across votes.

Of course, it is possible that greater coverage of close polls in locally-read newspapers is correlated with a canton×vote-specific unobservable that might shape turnout. We consider several possibilities. First, it is possible that locally-targeted political campaigning is associated with both local newspaper coverage of close polls and turnout. To evaluate this concern, we estimate equation (2), but predicting the number of political ads in a canton's newspapers for a given vote. As can be seen in Table I.4, Panel A, column 3, while greater newspaper coverage of polls in general is associated with the number of ads, the *differential* coverage of *closer* polls is not associated with political ads. The political supply side thus does not seem to account for our findings.

Another possibility is that newspapers providing more coverage of closer polls may also provide other coverage that increases salience and motivates turnout — for example, increasing persuasive content, in addition to reporting on close polls. One would expect that increased issue salience and/or increased exposure to persuasive content would tend to enhance voters' perceptions of an issue's importance. We thus estimate equation (2), but predicting cantonal voters' *ex post* assessment of an issue's importance. As can be seen in Table I.4, Panel A, column 5, we find no evidence of greater perceived importance of an issue when a canton's newspapers cover close polls more. Thus, alternative newspaper content does not seem to drive our results.[32]

Finally, differences across Switzerland's linguistic-cultural communities represent another possible source of variation in both newspaper poll coverage and voter turnout. For example, perhaps newspapers read by German-speaking Swiss are more likely to report on close polls and German-speaking Swiss are also more likely to turn out to vote in close elections, but greater coverage may not cause the higher turnout. To account for differences in turnout across linguistic-cultural communities depending on a vote's closeness or on a vote's coverage, we control for interactions between an indicator that a canton is German-speaking with pre-election poll closeness as well as with cantonal poll coverage. Including these controls does not affect any of our results (Table I.4, Panel A, columns 2, 4, and 6).

As an alternative approach to addressing concerns regarding the endogenous local newspaper coverage of close polls, we exploit a canton's voters' arguably "incidental" exposure to polls. As explained in section 3.1 above, we decompose *total* coverage of polls in a canton into *endogenous* coverage, by newspapers whose largest audience lies in that canton, and *incidental* coverage, by newspapers that are read in that canton but whose largest audience lies elsewhere. Incidental coverage of polls in our data is only very weakly correlated with endogenous coverage (the correlation is -0.153).[33] We thus examine the impact of incidental coverage of pre-election polls at the canton$\times$vote level, plausibly a "cleaner" source of variation in exposure to information regarding the closeness of an upcoming election.[34]

[32]While we prefer not to control for political ads and vote importance, as they are endogenous with respect to our explanatory variable of interest, we note here that their inclusion in the model presented in Table I.4, column 1, does not affect our results.

[33]We present each canton's endogenous and incidental exposure to newspaper coverage of polls in Appendix Figure A.2.

[34]Of course, it is possible that a newspaper's readers will have correlated political preferences across cantons, which shape turnout; these analyses are thus best viewed as suggestive.

TABLE I.4: NEWSPAPER COVERAGE, CLOSENESS AND CANTONAL VOTER TURNOUT

	Turnout (%)		Advertisements (std.)		Importance (std.)	
PANEL A: POLL MENTIONS IN CANTONAL NEWSPAPERS	(1)	(2)	(3)	(4)	(5)	(6)
Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.4795**	0.5426**	0.0408	0.0389	0.0419	0.0376
	(0.2170)	(0.2010)	(0.0378)	(0.0377)	(0.0506)	(0.0509)
	[0.062]	*[0.024]*	*[0.346]*	*[0.409]*	*[0.432]*	*[0.485]*
Poll Mentions (std.)	0.1877	1.2549**	0.2266***	0.1393**	0.0416	0.0427
	(0.2419)	(0.6185)	(0.0526)	(0.0658)	(0.0597)	(0.0953)
	[0.443]	*[0.036]*	*[0.000]*	*[0.078]*	*[0.504]*	*[0.665]*
R-squared	0.820	0.822	0.876	0.877	0.329	0.329
PANEL B: INCIDENTAL POLL MENTIONS						
Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.3741*	0.4516***	0.0287	0.0519	0.0479	0.0328
	(0.1928)	(0.1635)	(0.0455)	(0.0412)	(0.0359)	(0.0481)
	[0.082]	*[0.017]*	*[0.603]*	*[0.342]*	*[0.207]*	*[0.513]*
Poll Mentions (std.)	-0.0818	1.3947*	0.2459***	0.2977***	0.0364	0.0132
	(0.2722)	(0.7391)	(0.0547)	(0.0857)	(0.0594)	(0.1122)
	[0.766]	*[0.028]*	*[0.001]*	*[0.010]*	*[0.563]*	*[0.909]*
R-squared	0.820	0.821	0.878	0.878	0.329	0.329
PANEL C: INCIDENTAL POLL MENTIONS *(<15% Market Share)*						
Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.3835**	0.5380***	0.0137	0.0335	0.0511	0.0338
	(0.1753)	(0.1752)	(0.0465)	(0.0441)	(0.0335)	(0.0398)
	[0.033]	*[0.002]*	*[0.832]*	*[0.689]*	*[0.171]*	*[0.429]*
Poll Mentions (std.)	0.0234	2.0387**	0.2361***	0.2975***	0.0133	-0.0353
	(0.2673)	(0.8815)	(0.0554)	(0.0766)	(0.0572)	(0.1110)
	[0.932]	*[0.024]*	*[0.001]*	*[0.010]*	*[0.823]*	*[0.755]*
R-squared	0.820	0.822	0.876	0.876	0.329	0.329
Outcome Mean	47.273	47.273	73.927	73.927	6.115	6.115
Outcome Std. Dev.	8.910	8.910	68.180	68.180	1.132	1.132
Observations	962	962	962	962	957	957
German × Poll Mentions (std.)	N	Y	N	Y	N	Y
German × *Ex Ante* Closeness (std.)	N	Y	N	Y	N	Y

Notes: Each panel presents results from six OLS regressions using three dependent variables: cantonal turnout (Columns 1 and 2), the standardized number of newspaper advertisements in cantonal newspapers (Columns 3 and 4), and standardized importance, as rated by a canton's average VOX survey responses (Columns 5 and 6). In Panel A, Poll Mentions (std.) refer to the (standardized) absolute count of poll mentions in cantonal newspapers, i.e., newspapers read by at least 10% of a canton's inhabitants. In Panel B, only Incidental Poll Mentions are considered, i.e., mentions in cantonal newspapers whose main market lies in another canton. Panel C further restricts Incidental Poll Mentions to mentions in newspapers whose cantonal readership accounts for less than 15% of the newspaper's total readership. *Ex Ante* Closeness is the losing side's vote share at the federal level, as predicted by the pre-election poll. All specifications include canton and vote fixed effects. Columns 2, 4, and 6 additionally control for a dummy equal to one for German-speaking cantons, interacted with both *Ex Ante* Closeness (std.) and Poll Mentions (std.). The sample is a panel of 26 cantons, observed in 37 votes held between 2000 and 2014. Standard errors clustered at the vote level in parentheses: $^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

In Table I.4, Panel B, we present estimates from specifications analogous to those in Panel A, but now exploiting within-vote variation in exposure to *incidental* coverage of pre-election polls. One can see that greater coverage of closer polls continues to predict higher turnout (column 1) and that this effect is robust to including controls for interactions between an indicator that a canton is German-speaking with pre-election poll closeness as well as with cantonal poll coverage (column 2). The coefficient on the interaction between poll closeness and incidental exposure is about 25% smaller than the coefficient in Panel A, but this does not necessarily imply that the coefficient in Panel A was biased: our measure of incidental poll coverage necessarily excludes coverage of polls in widely-read newspapers, which would plausibly have large effects on turnout. One continues to see no relationship between greater coverage of closer polls and political advertisements or voters' perceptions of issues' importance (columns 3–6). In Panel C, we repeat the same exercises, but now implementing a more demanding measure of "incidental" newspaper coverage of polls, requiring that a canton represent less than 15% of a newspaper's readership. Our findings are much the same as in Panels A and B: greater coverage of closer polls is associated with significantly higher voter turnout; this does not seem to be driven by different cultural/linguistic groups; and it does not seem to be driven by political ads or changes in voters' perceptions of vote importance.[35]

6 Identifying the Role of Beliefs

Our findings thus far suggest a causal effect of information about election closeness on voter turnout. Such information may affect turnout either through changed beliefs about closeness, or through changes in issue salience, which might also drive turnout. Our finding above that newspaper coverage of close polls has no effect on voter perceptions of issue importance is suggestive of a primary role for beliefs, but we next test for a role of beliefs more directly.

To do so, we exploit the introduction of polls in Switzerland in 1998. In the absence of information from national, pre-election polls, it is plausible that voters will gauge an upcoming election's closeness by "locally sampling" among their friends and neighbors. This strategy will yield beliefs that reflect the actual national-level closeness only if the local sample is politically representative of the country as a whole. In such cases, it may be possible to condition the turnout decision on an informative local signal even in the absence of national polls. In contrast, in politically unrepre-

[35] As a final exercise with the canton×vote data, we use incidental exposure to poll coverage as an instrument for total exposure (and the interaction between poll closeness and incidental newspaper coverage of polls as an instrument for the interaction between poll closeness and total newspaper coverage of polls). In Appendix Table A.7, one can see: (i) strong first-stage estimates; (ii) the coefficient on incidental articles on polls in the first stage predicting total articles on polls is not greater than 1, suggesting that an additional incidental article is not associated with more endogenous articles (the p-value from a one-sided test is < 0.001); and (iii) the IV estimate (using the empirical specification from Table I.4, column 1) is somewhat larger than the OLS. This suggests that endogenous coverage of close polls may be greater when turnout is lower for other reasons: for example, newspaper editors may wish to stimulate turnout when they believe turnout will be lower than they think it ought to be.

sentative municipalities, it will not be easy for individuals to condition their turnout decision on national-level vote closeness.[36] Once polls are introduced, however, voters in *both* politically representative and politically unrepresentative municipalities will be able to condition their turnout on an accurate signal of election closeness.

If the formation of beliefs about closeness are the primary mechanism through which polls shape turnout, one would expect to see several patterns in the data. We predict: (*i*) in the era before polls, the closeness-turnout relationship should be positive in more politically representative municipalities, but there should be a weak or no relationship in politically unrepresentative municipalities. In other words, there should be a significant difference in the closeness-turnout gradient between politically representative and politically unrepresentative municipalities in the era without polls. (*ii*) The introduction of polls should have a significantly larger effect on the closeness-turnout relationship in politically unrepresentative municipalities (the poll has a larger effect on voters' information sets there). (*iii*) There should be convergence toward the same closeness-turnout relationship in the era with polls: i.e., a smaller difference in the closeness-turnout gradient in the era with polls.[37]

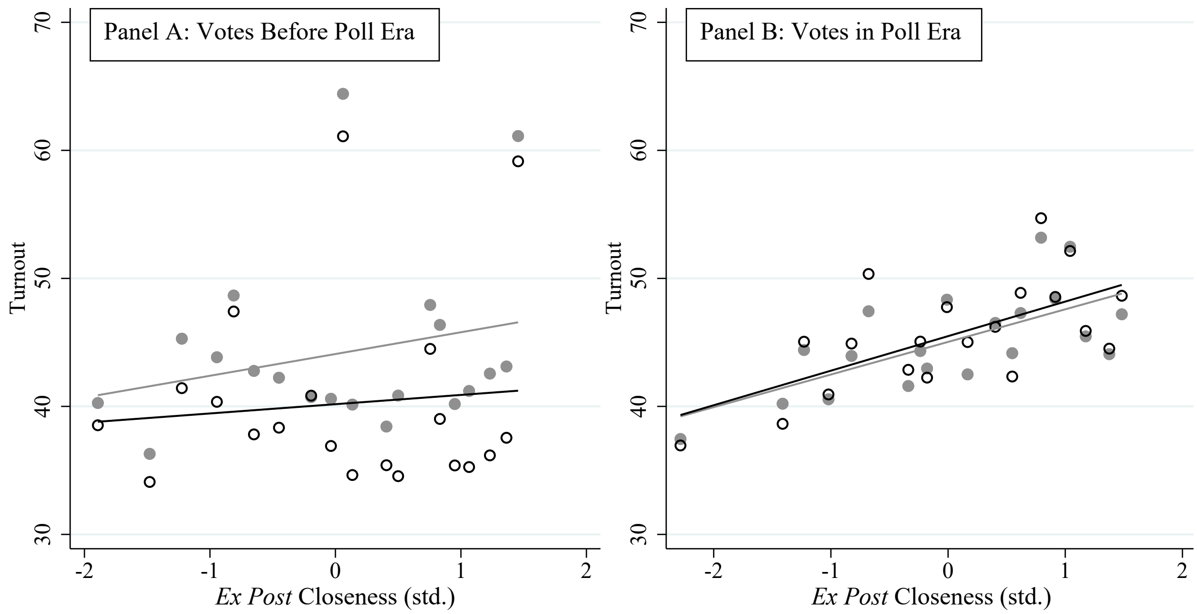
We test these predictions using a municipality$\times$vote panel, pooling data from the era with and without polls (and thus using an *ex post* measure of election closeness). As a reminder, we define "unrepresentativeness" as the opposite of the correlation between each municipality's vote share closeness and the national closeness between 1981 and 1998 (prior to the release of any polls).[38] Before estimating regression models, we begin by providing the raw correlation between election closeness and municipality voter turnout, splitting the sample of municipalities above and below the median level of political unrepresentativeness in our sample, and examining separately the set of votes held before polls were conducted and the set of votes with polls.

[36] Voters in politically unrepresentative municipalities may turn out more in response to local signals of closeness, but because these signals are less correlated with national-level closeness, they will not turn out systematically more for (nationally) closer elections. It is also possible that because their local signals are less informative, they choose not to act on them.

[37] If all voters who wish to condition their turnout on national closeness have access to polls, then there will be complete convergence in turnout rates across municipalities. However, if poll information is imperfectly transmitted, voters in representative municipalities may still have access to better signals of national closeness, implying only partial convergence.

[38] See Appendix Figure A.3, Panel A for the distribution of unrepresentativeness.

FIGURE I.7: THE EFFECT OF CLOSENESS ON TURNOUT BY MUNICIPALITY UNREPRESENTATIVENESS, BEFORE AND AFTER THE INTRODUCTION OF POLLS



		Slopes Before Poll Era	Slopes in Poll Era
— ○	Unrepresentative Municipalities	0.724 (p = 0.646)	2.699 (p = 0.005)
— ●	Representative Municipalities	1.700 (p = 0.244)	2.547 (p = 0.005)
	Differences within Era:	*-0.976 (p = 0.010)*	*0.152 (p = 0.693)*
	Difference in Differences:		*1.128 (p = 0.037)*

Notes: Panel A shows binned scatter plots correlating municipality-level voter turnout and national-level *ex post* closeness, splitting the sample of municipalities above and below median political unrepresentativeness, for 46 votes in the era before pre-election polls were introduced. Panel B replicates Panel A for 69 votes in the era with pre-election polls. Unrepresentativeness is a municipality's historical tendency to produce voting results unrepresentative of national-level closeness, measured as the negative of the correlation coefficient between municipality-level and national-level *ex post* closeness of voting results in the era before pre-election polls were introduced. Estimates of slope parameters as well as p-values associated with tests that (differences in) slopes equal zero are obtained from an OLS regression using all 115 votes, with standard errors clustered at vote level.

In Figure I.7, one can see patterns matching our predictions: in the absence of polls, among relatively unrepresentative municipalities, there is practically no relationship between election closeness and turnout. Among more representative municipalities, there is a stronger positive gradient — the difference in slopes between the representative and unrepresentative municipalities is statistically significant ($p = 0.01$). In contrast, in the era when polls are conducted, there is no difference between unrepresentative and representative municipalities in their relationship between election closeness and voter turnout ($p = 0.693$). In both sets of municipalities the slope is positive and significant ($p < 0.01$) and the difference in differences (comparing the eras with and without polls) is statistically significant as well ($p = 0.037$). Finally, the magnitudes are substantial: a one-standard deviation closer election is associated with around 2.5 percentage points higher turnout when polls are released. In the absence of polls, a one-standard deviation closer election was associated with around 1.7 percentage points higher turnout in municipalities that were representative of Switzerland, and only 0.7 percentage points in municipalities that were unrepresentative.

We next estimate the following model:

$$\begin{aligned} turnout_{mv} \;\; = \;\; & \alpha_m + \gamma_v + \delta_1 \; closeness_v \times unrepresentative_m \times PollEra_v \\ & + \delta_2 \; closeness_v \times unrepresentative_m \\ & + \delta_3 \; unrepresentative_m \times PollEra_v + \varepsilon_{mv}, \end{aligned} \tag{3}$$

examining the relationship between election closeness and municipality voter turnout depending on municipality unrepresentativeness and on the existence of polls, accounting for vote and municipality fixed effects. It is useful to match our hypotheses to regression coefficients. Prediction (*i*) suggests a significant and negative coefficient δ_2. Prediction (*ii*) implies a positive and significant coefficient δ_1. Prediction (*iii*) suggests that the sum of the coefficients $\delta_1 + \delta_2$ will be insignificantly different from zero.

In Table I.5, column 1, we provide regression estimates of equation (3). We find estimates that confirm our predictions: (*i*) there exists a significant difference between representative and unrepresentative municipalities in the relationship between election closeness and turnout in the era without polls (the coefficient on $closeness_v \times unrepresentative_m$ is negative and statistically significant). (*ii*) The effect of the release of polls on the relationship between election closeness and turnout is greater in unrepresentative municipalities (the coefficient on $closeness_v \times unrepresentative_m \times PollEra_v$ is positive and statistically significant). (*iii*) With polls available, there no longer is a significant difference between representative and unrepresentative municipalities in their relationship between election closeness and turnout: We cannot reject that the sum of the coefficients on $closeness_v \times unrepresentative_m$ and $closeness_v \times unrepresentative_m \times PollEra_v$ equals 0, $p = 0.798$.

One might wonder whether unrepresentative municipalities are simply smaller than representative ones, with municipality size driving the patterns observed (unrepresentativeness is negatively correlated with electorate size, $r = -0.15$). To account for the effects of municipality size, we control for the triple interaction among closeness, municipality electorate size, and a Poll Era dummy ($closeness_v \times electorate_m \times PollEra_v$) as well as all of the lower-order terms. One can see in Table I.5, column 2, that accounting for differences in the size of municipalities does not affect our results.[39]

[39]One plausible source of unrepresentativeness is political homogeneity: a very homogeneous municipality will likely *never* have locally close elections, as voters will always skew strongly toward one side. This implies that there

TABLE I.5: HETEROGENEOUS EFFECTS OF ELECTION CLOSENESS AND POLLS DEPENDING ON MUNICIPALITY UNREPRESENTATIVENESS AND NATIONAL LEVEL TURNOUT AS PROXY FOR SALIENCE

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Closeness (std.) × Unrepresentativeness (std.)	-0.5676***	-0.5696***			-0.6292***	-0.6294***
	(0.2132)	(0.2156)			(0.2083)	(0.2109)
Ex Post Closeness (std.) × Unrepresentativeness (std.) × Poll Era	0.6211**	0.6179**			0.6106**	0.6135**
	(0.2983)	(0.3003)			(0.3037)	(0.3063)
National Turnout (std.) × Unrepresentativeness (std.)			0.2673*	0.2564*	0.3503**	0.3394**
			(0.1396)	(0.1412)	(0.1529)	(0.1544)
National Turnout (std.) × Unrepresentativeness (std.) × Poll Era			-0.0321	-0.0472	-0.1075	-0.1237
			(0.2262)	(0.2277)	(0.2434)	(0.2453)
Unrepresentativeness (std.) × Poll Era	1.9756***	2.0288***	1.8838***	1.9440***	1.8515***	1.9118***
	(0.2613)	(0.2629)	(0.2762)	(0.2786)	(0.2665)	(0.2692)
Test for Convergence of Closeness Gradients *(p-value)*	*0.798*	*0.818*			*0.933*	*0.943*
Test for Convergence of Salience Gradients *(p-value)*			*0.189*	*0.244*	*0.202*	*0.260*
Outcome Mean	44.001	44.001	44.001	44.001	44.001	44.001
R-squared	0.697	0.698	0.697	0.697	0.698	0.698
Observations	250240	250240	250240	250240	250240	250240
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Vote Fixed Effects	Y	Y	Y	Y	Y	Y
Electorate Size	N	Y	N	Y	N	Y

Notes: Each column presents results from an OLS regression with municipality-level voter turnout as the dependent variable. Unrepresentativeness is a municipality's historical tendency to produce voting results unrepresentative of national-level closeness, measured by the negative of the correlation coefficient between muncipality-level and national-level *ex post* closeness of voting results in the era before pre-election polls. Poll Era is a dummy equal to 1 for 69 votes held after the introduction of polls in 1998. National Turnout is the Swiss national level turnout rate for the vote. Test for Convergence of Closeness Gradients reports the p-value of an F-test that the sum of the coefficients on *Ex Post* Closeness (std.) × Unrepresentativeness (std.) and *Ex Post* Closeness (std.) × Unrepresentativeness (std.) × Poll Era (std.) equals 0. Test for Convergence of Salience Gradients reports the p-value of an F-test that the sum of the coefficients on National Turnout (std.) × Unrepresentativeness (std.) and National Turnout (std.) × Unrepresentativeness (std.) × Poll Era (std.) equals 0. Columns 2, 4 and 6 control for a triple interaction of Poll Era and the standardized average municipality electorate size with either *Ex Post* Closeness (Column 2) or National Turnout (Column 4) or both (Column 6), as well as lower order terms. The sample is a balanced panel of 2176 municipalities observed in 115 votes held from 1981 to 2019. Standard errors clustered at the vote level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

As noted above, a key question about our findings is whether salience, rather than beliefs about closeness, can explain our findings. In the context of this analysis, it is possible that nationally salient issues are also issues with close polls. If so, and if unrepresentative municipalities increasingly turn out to vote on nationally salient issues over time, then this could produce the patterns observed above. In fact, the release of polls may play a role in stimulating unrepresentative municipalities' greater turnout on nationally salient issues.

To address this possibility, we consider national-level voter turnout as a proxy of national-level salience. We conduct two tests: first, we investigate whether national salience has heterogeneous effects depending on municipality-level unrepresentativeness, and whether this effect changes over time as polls are introduced. We estimate the model in equation 3, but replace $closeness_v$ with

will be little or no correlation between local closeness and national closeness. Indeed, we find a strong correlation between municipality political homogeneity and unrepresentativeness ($r = 0.60$). In Appendix Figure A.4 and Table A.8, we show that the patterns of heterogeneous turnout observed with respect to municipality unrepresentativeness also appear with respect to homogeneity, as we would expect.

(eventual) national level voter turnout for vote v. We present these results in Table I.5, columns 3 and 4: one can see that unrepresentative municipalities turn out more for issues that have higher (national-level) salience, but that this greater turnout does not change following the introduction of pre-election polls.

In a second exercise, we conduct a "horserace" between the time-varying effect of close polls and the time-varying effect of national issue salience. We estimate the model in equation 3, but add the triple interaction of national-level salience, municipality unrepresentativeness, and a poll-era indicator (along with lower-order terms), as controls. As can be seen in Table I.5, columns 5 and 6, the inclusion of these controls has virtually no effect on the coefficients of interest. This analysis further supports our interpretation of polls as providing information shaping beliefs about closeness, rather than increasing the salience of votes.

7 Polls' Effects on the Electorate and Electoral Outcomes

7.1 Changes in the Electorate

Our analysis thus far suggests a causal effect of close polls on voter turnout. One might wonder whether this effect can shape election outcomes. For this to be the case, close polls must not only change the level of turnout, but also the composition of the electorate, beyond a uniform turnout effect. We build on an established literature on possible asymmetric effects of polls on turnout among supporters of the trailing and leading sides (e.g., Simon, 1954; Levine and Palfrey, 2007; Rogers and Moore, 2015; Agranov et al., 2018). Theory is ambiguous regarding which side (if any) will turn out more in response to polls: on the one hand, the trailing side may be motivated and the leading side may be overconfident, producing differentially high turnout among the trailing side. On the other hand, a discouragement effect among the trailing side and a desire to participate on the winning side (i.e., a "bandwagon effect") may generate greater turnout among supporters of the leading side. We test for such an asymmetric response at the municipality-level, the most disaggregated level at which we observe voter turnout.

Because we do not observe municipality-level preferences regarding a referendum prior to the vote itself, we estimate support for the trailing side in the poll using the municipality's vote share in the preceding legislative election for parties endorsing the trailing side in the upcoming vote.[40] We then estimate the following model on a balanced panel of 2,176 municipalities observed in all 57 votes with a pre-election poll (1998–2019):

[40] We use vote shares in the *preceding* legislative election to minimize concerns about simultaneity. We present the distribution of trailing side support across municipality×vote observations in Appendix Figure A.5. Because the parties running in the legislative elections differ across cantons and legislative elections, and because not all parties make recommendations on all votes, all of our analysis will consider estimated support for the trailing side conditional on canton×vote fixed effects.

$$y_{mv} = \lambda_{c(m)v} + \gamma_m + \alpha\, support_{mv} + \beta\, support_{mv} \times closeness_v + \epsilon_{mv}, \tag{4}$$

where y_{mv} is the municipality-level turnout; $support_{mv}$ is the estimated share supporting the trailing side, varying (from 0 to 100%); closeness is defined as the standardized support for the trailing side in the pre-election poll; canton×vote fixed effects and municipality fixed effects are captured by $\lambda_{c(m)v}$ and γ_m, respectively.

TABLE I.6: ASYMMETRIC EFFECTS OF *Ex Ante* CLOSENESS ON TURNOUT AND VOTE SHARES

	(1) Turnout	(2) Vote Share
Ex Ante Closeness (std.) × Trailing Side's Estimated Support	0.0125*	0.0634***
	(0.0073)	(0.0213)
Trailing Side's Estimated Support	0.0120	0.3884***
	(0.0082)	(0.0255)
Outcome Mean	46.853	42.826
R-squared	0.859	0.876
Observations	124032	124032

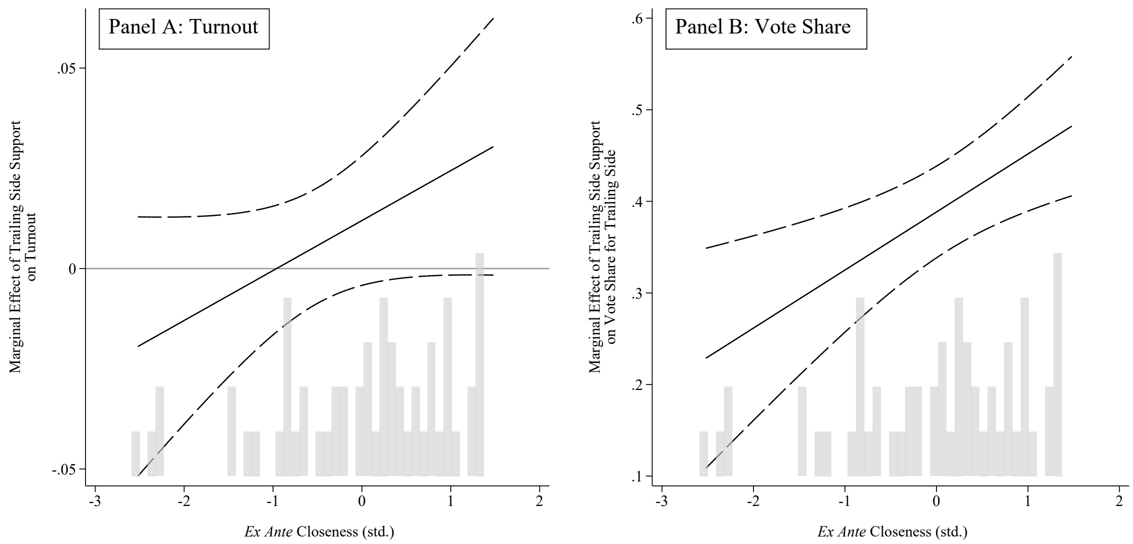
Notes: The table presents estimates from OLS regressions with municipality level voter turnout (column 1) and vote share for the trailing side (column 2) as dependent variables. The trailing side's Vote Share is defined as a municipality's share of votes cast in line with the trailing side in the pre-election poll, i.e., with the minority of poll respondents. Trailing Side's Estimated Support is a municipality's predetermined pre-disposition to vote for the side trailing in the pre-election poll, measured as a municipality's vote share, in percent of votes cast, in the preceding national election for parties whose voting recommendations are in line with the minority of poll respondents. *Ex Ante* Closeness is the trailing side's vote share in the pre-election poll. All specifications include municipality and canton × vote fixed effects. The sample is a balanced panel of 2176 municipalities observed in all 57 votes with a pre-election poll held from 1998 to 2019. Standard errors clustered at the vote level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table I.6, column 1, one can see that for votes with average levels of poll closeness, there is a small, positive, and marginally significant relationship between municipality support for the trailing side and municipality voter turnout. As polls get closer, municipalities with greater estimated support for the trailing side exhibit *differentially* higher turnout — beyond the higher turnout associated with close polls, which is captured by the canton×vote fixed effects. A 20 percentage point (roughly one standard deviation) increase in support for the trailing side in a municipality is predicted to increase turnout by around 0.5 percentage points, when a poll is one standard deviation closer than the mean (a poll of around 55–45).[41] The heterogeneous effect of support for the losing side on turnout as a function of poll closeness is shown graphically in Figure I.8, Panel A. One sees that the closer an election is predicted to be, the larger the gradient between support for the

[41] This can be calculated as the coefficient of 0.012 on trailing side support multiplied by 20, plus the coefficient of 0.0125 on the interaction term multiplied by 20 and by 1 (with the latter reflecting the closer poll).

trailing side and turnout.[42]

FIGURE I.8: MARGINAL EFFECTS OF TRAILING SIDE SUPPORT ON TURNOUT AND VOTE SHARE FOR THE TRAILING SIDE DEPENDING ON POLL CLOSENESS



Notes: The solid line plots the total effect of a unit increase in estimated *ex ante* support for the trailing side on turnout (Panel A) and on the vote share for the side trailing in the poll (Panel B), depending on standardized *ex ante* closeness. Dashed lines represent 95% confidence intervals based on standard errors clustered at vote level. The plot is based on OLS estimates reported in Table I.6. The histograms show the distribution of (standardized) poll closeness across votes.

It remains to be shown whether the greater turnout we observe following close polls in municipalities with greater estimated support for the trailing side is actually driven by supporters of the trailing side within those municipalities. To do so, we examine vote outcomes at the municipality level, testing whether the municipalities with greater estimated trailing side support exhibit a higher *ex post* vote share for the trailing side following the release of closer polls.[43] Thus, we estimate equation 4, but now examining municipality vote share for the trailing side as the outcome.

We present the results in Table I.6, column 2, and one can see that following closer polls, vote shares in favor of the trailing side differentially increase in municipalities with more estimated support for the trailing side. A 20 percentage point (roughly one standard deviation) increase in support for the trailing side in a municipality is predicted to increase the trailing side vote share by around 9 percentage points, when a poll is one standard deviation closer than the mean (a poll of

[42]In Appendix Table A.9 and Figure A.6 one can see that excluding extreme values of estimated support for the trailing side (0% and 100%) does not affect our results.

[43]We note that even a null finding would still imply national-level changes in the composition of the electorate as a result of close polls. If municipalities supporting the trailing side show higher turnout (but with local vote shares held fixed), more supporters of the trailing side turn would turn out nationally.

around 55–45).[44] Thus, not only is there higher turnout in response to close polls in municipalities with greater support for the trailing side, but this response is stronger among supporters of the trailing side within these municipalities. The heterogeneous effect of support for the trailing side on vote shares as a function of poll closeness is shown graphically in Figure I.8, Panel B. One sees that the closer an election is predicted to be, the larger the gradient between support for the trailing side and ultimate trailing side vote share.

7.2 Counterfactual Electoral Outcomes

To evaluate the magnitude of these asymmetric effects of close polls, we simulate election outcomes under counterfactual polling scenarios. Our counterfactuals are motivated by real-world variation in polling outcomes (resulting from sampling and methodological differences) and by restrictions to the publication of polls (enforced in some countries).

We first consider counterfactual outcomes under a scenario in which polls sent a signal of average closeness (61–39; around 1.5 standard deviations below maximal closeness of 50–50), rather than their actual closeness.[45] Under this scenario, supporters of the trailing side in votes with close polls would turn out differentially less, potentially overturning referendum results in which the trailing side in the polls managed to win the referendum.[46]

Specifically, to estimate counterfactual municipality turnout rates and vote shares, we subtract from a municipality's actual turnout rate and actual vote share for the trailing side the differential, asymmetric effects of close polls generated by the municipality's support for the trailing side (as estimated from the interaction terms presented in Table I.6, columns 1 and 3). We estimate counterfactual votes for each side in a vote, by municipality, for the 57 votes that had polls between 1998 and 2019, then aggregate these to the national level. We find that had there been no close polls to stimulate trailing side supporter turnout, three high-stakes referenda would have had different outcomes: two controversial initiatives against immigration (one violating the terms of Switzerland's relationship with the EU), as well as one on pension reform (see Table I.7, Panel A).

[44] This can be calculated as the coefficient of 0.3884 on trailing side support multiplied by 20, plus the coefficient of 0.0634 on the interaction term multiplied by 20 and by 1 (with the latter reflecting the closer poll).

[45] This is equivalent to a scenario without the release of national polls if, in the absence of polls, voters turn out as if knowing that they are in support of the trailing or leading side in a vote (e.g., because this sort of coarse information is available from parties' recommendations and newspaper coverage), and as if they believe that the vote is of average closeness.

[46] This situation is equivalent to the 2016 US election in which Donald Trump trailed in the polls, but won the election.

TABLE I.7: COUNTERFACTUAL TURNOUT AND VOTE RESULTS

	Poll	Votes	
	Yes (%)	Actual Yes (%)	Counterfactual Yes (%)
PANEL A: AVERAGE POLL CLOSENESS			
Initiative "against Abuse of Asylum" (November 24, 2002)	53.75	49.91	54.45
Initiative "against Mass Immigration" (February 9, 2014)	46.24	50.33	48.63
Federal Act on Old Age Pension Reform (September 24, 2017)	53.68	47.31	50.24
PANEL B: INCREASE IN POLL CLOSENESS			
Federal Act on the Army and Military Administration (June 10, 2001)	59.49	50.99	48.74
Federal Act on Corporate Tax Reform (February 24, 2008)	59.74	50.53	48.28
Initiative "for the Expulsion of Criminal Foreigners (November 28, 2010)	55.67	52.91	49.97
Initiative "Limiting the Construction of Second Homes" (March 11, 2012)	58.43	50.63	47.16

Notes: Table lists the actual Yes vote share, the Yes vote share predicted by the pre-election poll, and the counterfactual Yes vote share, for all votes with election outcomes flipped by the counterfactual scenarios. Each panel corresponds to one counterfactual secnario. Panel A assumes pre-election polls were set at average poll closeness and calculates the counterfactual Swiss-level Yes vote share for 57 votes from municipality-level turnout and vote shares obatained from substracting the asymmetric effects of poll closeness implied by estimates in Table I.6, Columns 1 and 2. Panel B replicates Panel A, but assuming counterfactual pre-election polls were one standard deviation (i.e., 7.62 percentage points) closer than actual polls, with counterfactual poll closeness bounded above by 50.

In a second scenario, we consider the possibility that poll results were one standard deviation closer than the actual poll (censored at maximal closeness of 50–50). Under this scenario, supporters of the trailing side in votes with now closer polls would turn out differentially more, potentially overturning referendum results in which the trailing side in the polls lost a close vote. We model counterfactual turnout by applying the estimated asymmetric effects of closer polls that we estimate in Table I.6, columns 1 and 2. We find that four additional referenda — on issues ranging from the armed forces, to immigration, to corporate taxation — would have had different outcomes had polls been just one standard deviation closer (see Table I.7, Panel B).

8 Conclusion

We find that the release of polls causes changes in voter turnout. This effect is unlikely to be driven by purely rational pivotality considerations given the large size of the Swiss electorate. Rather, it is likely due to behavioral factors: social considerations (e.g., arising from anticipated social interactions following the referendum); or, misperceptions regarding pivotality (especially among supporters of the trailing side in the polls). Understanding how political competition interacts with other behavioral and social factors is an important area for future work.

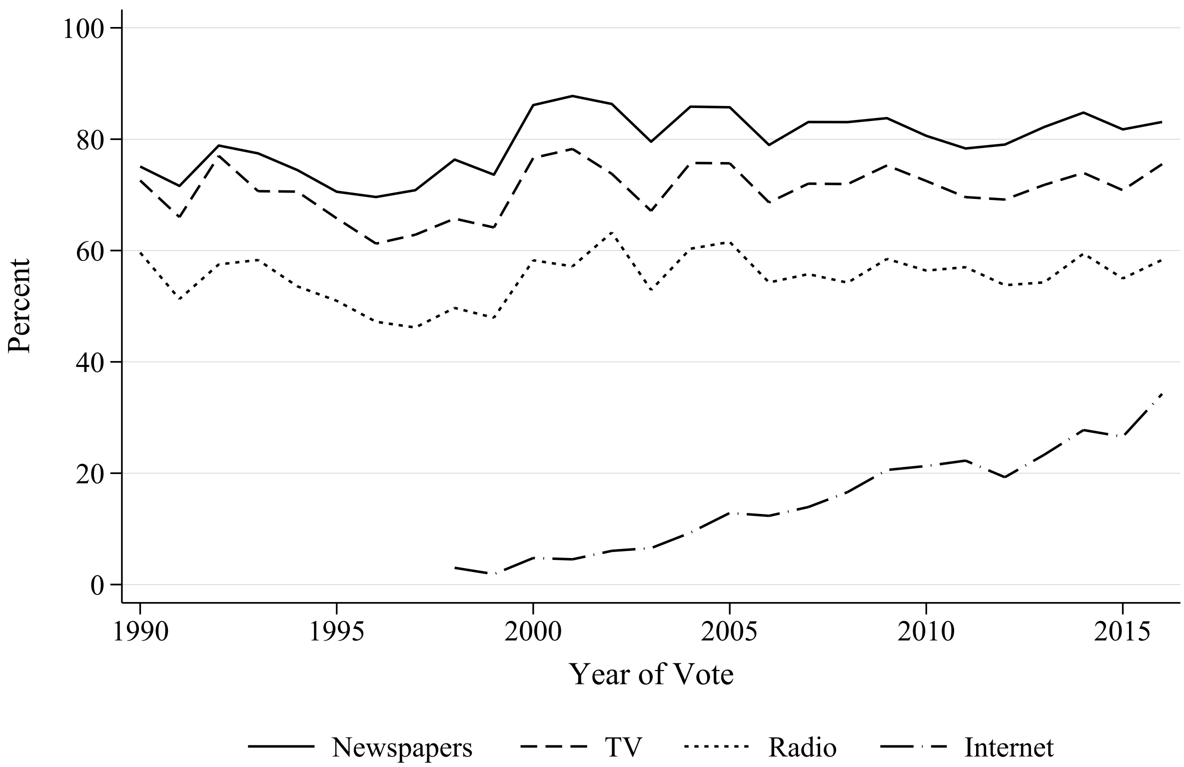
How general is the causal effect of election closeness on turnout? While the context we study has distinctive features — we study voter turnout for referenda, rather than traditional elections, in a country with a long democratic tradition — it likely generalizes to a range of important settings. First, referenda play a role around the world in deciding high-stakes issues: from Brexit, to the decision to end the Pinochet regime, to many important policy issues in the state of California (like Switzerland, with many referenda voted on each year). More generally, Swiss referenda often produce high-stakes political competitions between left- and right-wing parties. In this sense, they are single-issue analogues of the majoritarian political competition that exists elsewhere.

Our estimates of an asymmetric underdog effect suggest magnitudes large enough to shape election outcomes. In addition to the academic interest in this asymmetry, our results have important policy implications. The regulation of polls' conduct and their dissemination can have important consequences for election outcomes. There is a remarkable degree of variation across countries in such regulation: for example, in Australia and in the United States there is none; in Italy, polls are prohibited within 15 days of a vote; and, in Switzerland, no information on polls can be released in the 10 days before the vote. The impact of these regulations on a range of policy outcomes might be far greater than many policymakers realize.

Our results also indicate an important shift in the nature of political communication. While much research on political behavior has focused on the effects of *persuasive* information in social media, in newspapers, on television, or in advertisements, our findings indicate that information about an election's *competitiveness* can shape political behavior as well. In a context of increased political polarization (e.g., Boxell et al., 2022, and Draca and Schwarz, 2021), persuasion aimed at changing the ideological preferences of voters may be less effective, making the turnout margin that we study — changing the ideological composition of the *voting* electorate — potentially more important than in the past.

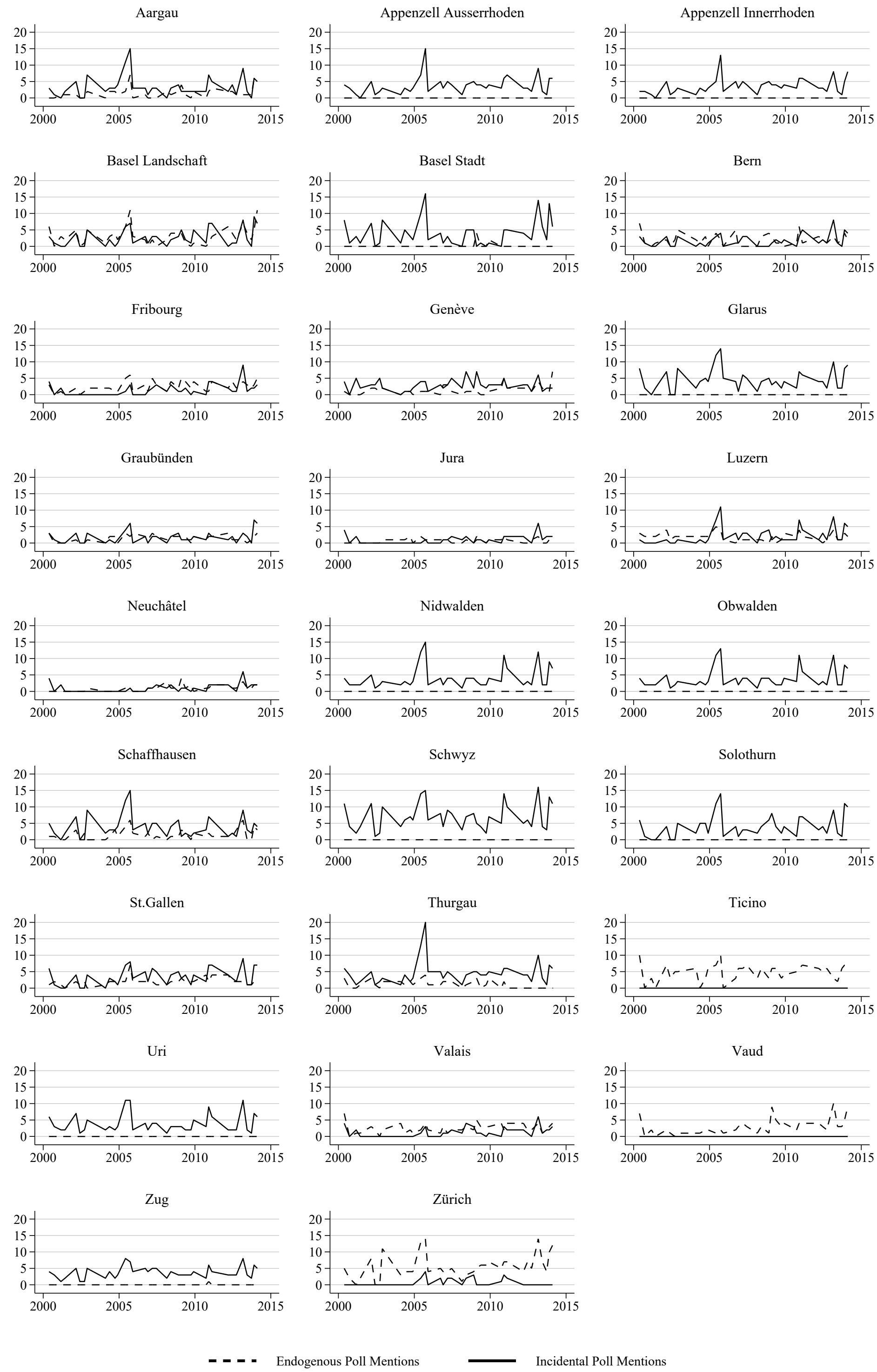
Appendix A: Additional Figures and Tables on Closeness and Voter Turnout

FIGURE A.1: MEDIA USAGE FOR POLITICAL OPINION FORMATION



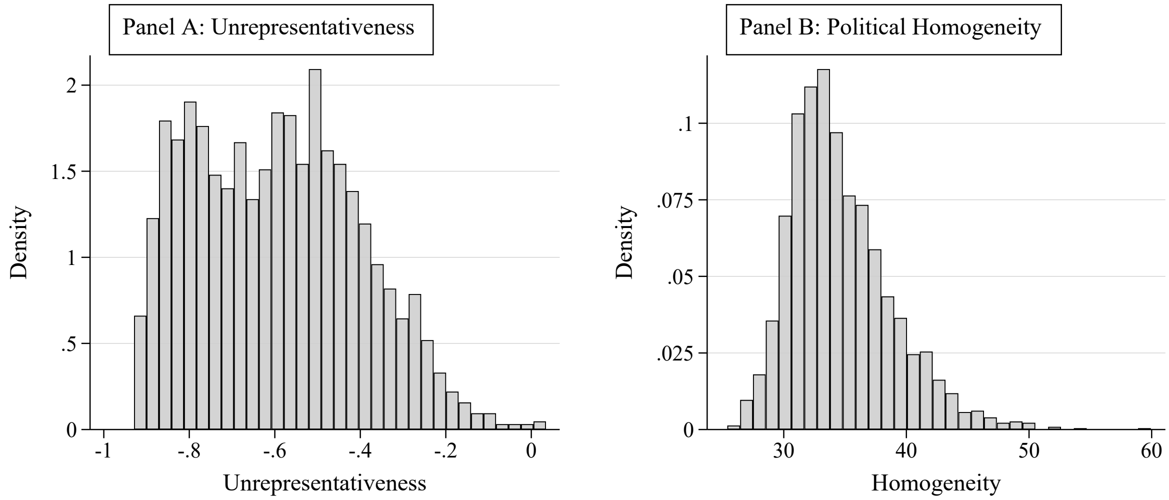
Notes: Responses from the VOX survey to the following prompt: "Through which media did you orient yourself and learn about the pros and cons of the last vote? Please indicate all possibilities that were accurate for the last vote." The graph shows the share of survey respondents who indicated the use of newspapers, TV, radio, or the Internet.

FIGURE A.2: ENDOGENEOUS AND INCIDENTAL POLL MENTIONS IN CANTONS OVER TIME



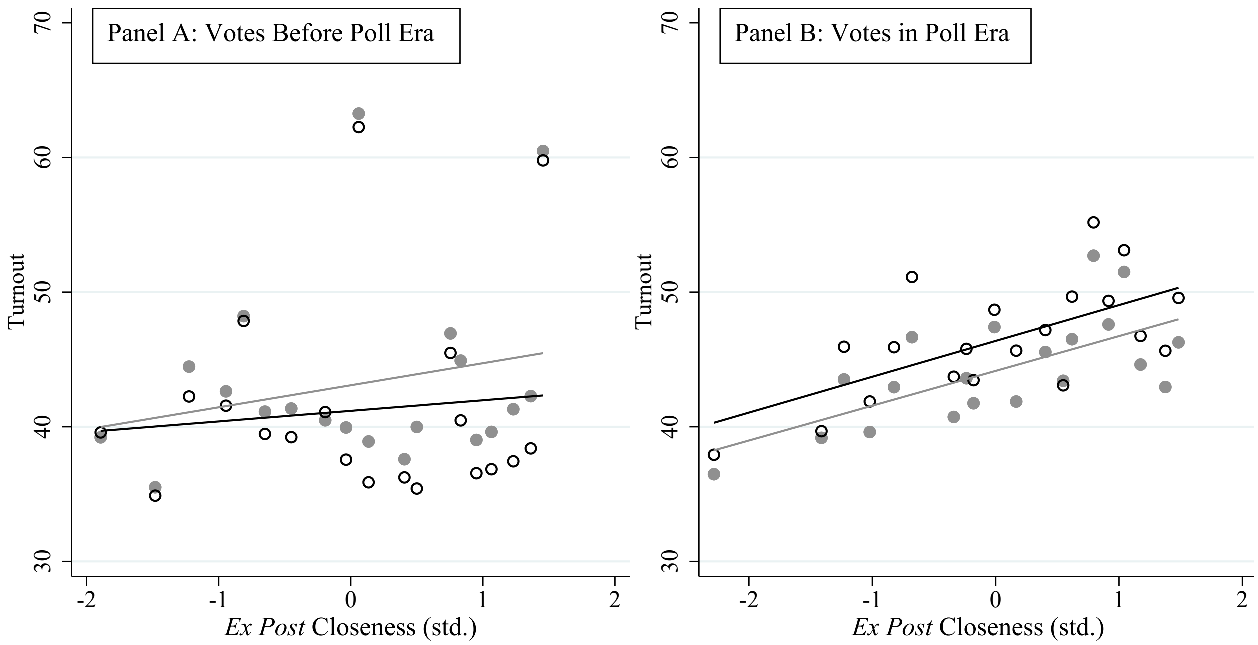
Notes: Each panel plots the number of endogenous and incidental poll mentions over time, for one canton. Endogenous poll mentions are poll mentions in newspapers read by at least 10% of a canton's inhabitants and for which the canton is the largest market. Incidental poll mentions are poll mentions in newspapers read by at least 10% of a canton's inhabitants, but whose largest market is in a different canton.

FIGURE A.3: DISTRIBUTIONS OF MUNICIPALITY UNREPRESENTATIVENSS AND HOMOGENEITY



Notes: The figure shows the distribution of 2176 Swiss municipalities according to unrepresentativeness (panel A) and political homogeneity (panel B). Unrepresentativeness is a municipality's historical tendency to produce voting results unrepresentative of national-level closeness, measured as the negative of the correlation coefficient between municipality-level and national-level *ex post* closeness of voting results in the era before pre-election polls were introduced. Political homogeneity is a municipality's historical tendency to produce outcomes distant from 50-50, as measured by the average municipal-level margin of majority across all votes held in the era before pre-election polls were introduced.

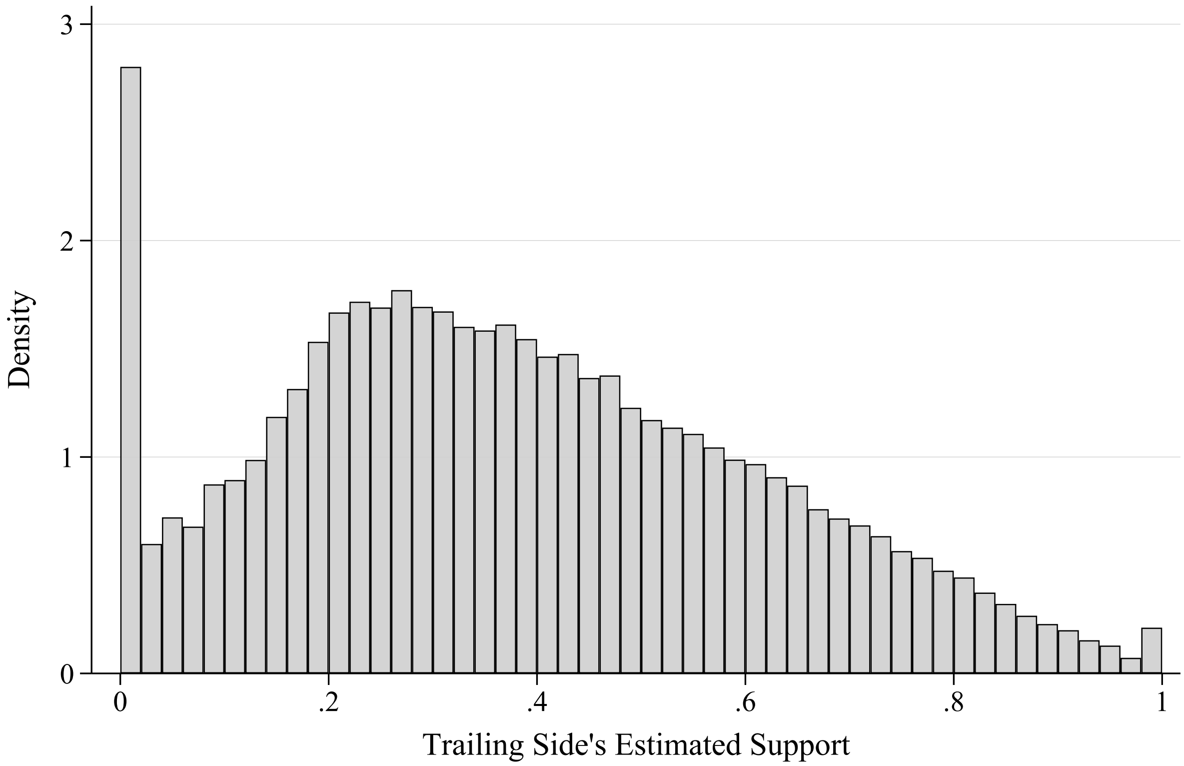
FIGURE A.4: THE EFFECT OF CLOSENESS ON TURNOUT BY MUNICIPALITY HOMOGENEITY, BEFORE AND AFTER THE INTRODUCTION OF POLLS



	Slopes Before Poll Era	Slopes in Poll Era
Homogeneous Municipalities	0.786 (p = 0.612)	2.661 (p = 0.005)
Heterogeneous Municipalities	1.638 (p = 0.267)	2.585 (p = 0.005)
Differences within Era:	*-0.852 (p = 0.005)*	*0.077 (p = 0.800)*
Difference in Differences:		*0.928 (p = 0.030)*

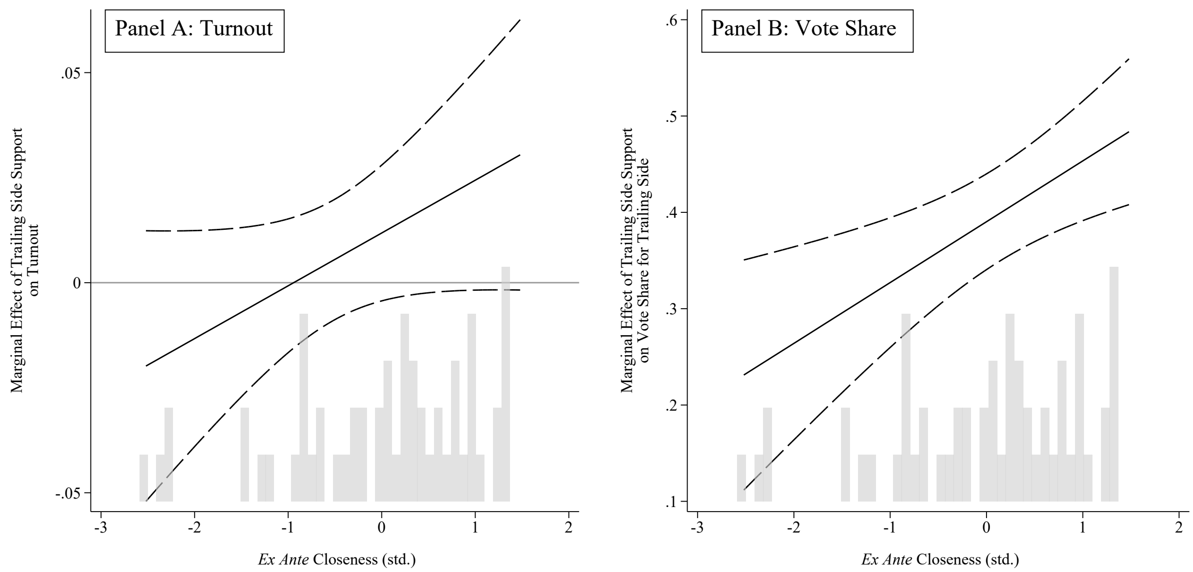
Notes: Panel A shows binned scatter plots correlating municipality-level voter turnout and national-level *ex post* closeness, splitting the sample of municipalities above and below median political homogeneity, for 46 votes in the era before pre-election polls were introduced. Panel B replicates Panel A for 69 votes in the era with pre-election polls. Political homogeneity is a municipality's historical tendency to produce outcomes distant from 50-50, as measured by the average municipal-level margin of majority across all votes held in the era before pre-election polls. Estimates of slope parameters as well as p-values associated with tests that (differences in) slopes equal zero are obtained from an OLS regression using all 115 votes, with standard errors clustered at vote level.

FIGURE A.5: DISTRIBUTION OF TRAILING SIDE'S ESTIMATED SUPPORT



Notes: The figure shows the distribution of Trailing Side's Estimated Support accross 2176 municipalities and 57 votes. Trailing Side's Estimated Support is a municipality's predetermined pre-disposition to vote for the side trailing in the pre-election poll, measured as a municipality's vote share, in percent of votes cast, in the preceding national election for parties whose voting recommendations are in line with the minority of poll respondents.

FIGURE A.6: MARGINAL EFFECTS OF TRAILING SIDE SUPPORT ON TURNOUT AND VOTE SHARE FOR THE TRAILING SIDE DEPENDING ON POLL CLOSENESS: TRIMMED SAMPLE



Notes: The solid line plots the total effect of a unit increase in *ex ante* support for the trailing side on turnout (Panel A) and on the vote share for the side trailing in the poll (Panel B), depending on standardized *ex ante* closeness. Dashed lines represent 95% confidence intervals based on standard errors clustered at vote level. The plot is based on OLS estimates using the trimmed sample that excludes all observations with 0% or 100% support for the trailing side, reported in Table A.9. The histograms show the distribution of (standardized) poll closeness across votes.

TABLE A.1: LIST OF MOST IMPORTANT VOTES BY ELECTION DAY

Date	Title	Turnout (%)	Yes (%)
1981-06-14	Initiative for "Equal Rights of Men and Women"	33.95	60.27
1981-11-29	Federal Decision on the Financial Order Improving the Federal Budget	30.35	68.95
1982-06-06	Federal Penal Code (Violent Crime)	35.19	63.71
1982-11-28	Initiative for "Preventing Abusive Pricing"	32.93	57.94
1983-02-27	Federal Decision on the Revision of Fuel Tariffs	32.42	52.69
1983-12-04	Federal Decision on the Regulation of Citzenship in the Constitution	35.84	60.81
1984-02-26	Initiative "for Civil Service Based on Factual Evidence"	52.77	36.17
1984-05-20	Initiative "against Bank Secrecy and the Power of Banks"	42.52	26.96
1984-09-23	Initiative "for a Safe, Economical and Eco-Friendly Energy"	41.62	45.77
1984-12-02	Initiative "for an Effective Protection of Motherhood"	37.66	15.78
1985-03-10	Initiative "for Extending Paid Holidays"	34.60	34.78
1985-06-09	Initiative "for the Right to Life"	35.72	30.96
1985-09-22	Federal Decision on Risk Guarantees for Innovations in SMEs	40.87	43.11
1985-12-01	Initiative "for Abolishing Vivisection"	37.97	29.47
1986-03-16	Federal Decision on the Accession to the United Nations	50.71	24.33
1986-09-28	Initiative "for Secured Vocational Education and Re-training"	34.82	18.38
1986-12-07	Initiative "for an Fair Levy on Heavy Traffic"	34.74	33.87
1987-04-05	Initiative "for Referenda against Military Expenses"	42.42	40.56
1987-12-06	Federal Decision on "Railway 2000"	47.70	56.99
1988-06-12	Initiative "for Reducing the Retirement Age"	42.02	35.12
1988-12-04	Initiative "against Land Speculation"	52.83	30.78
1989-06-04	Initiative "for Natural Farming - against Animal Factories"	35.96	48.95

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Date	Vote Title	Turnout (%)	Yes (%)
1989-11-26	Initiative “for Switzerland Without an Army and a Comprehensive Peace Policy”	69.19	35.59
1990-04-01	Initiative “against Concrete - for Limiting Road Construction”	41.13	28.51
1990-09-23	Initiative “against Constructing New Nuclear Power Plants”	40.44	54.52
1991-03-03	Initiative “for Promoting Public Transport”	31.24	37.14
1991-06-02	Federal Decision on Federal Budget Reform	33.27	45.65
1992-02-16	Initiative “for the Drastic and Stepwise Limitation of Animal Experiments”	44.50	43.63
1992-05-17	Initiative “against Abuses of Reproduction Technology and Genetic Engeneering”	39.18	73.83
1992-09-27	Federal Decision on the New Railway Link through the Alps (NRLA)	45.91	63.61
1992-12-06	Federal Act on the Accession to the European Economic Area	78.78	49.66
1993-03-07	Initiative “for Abolishing Animal Experiments”	51.26	27.77
1993-06-06	Initiative “for Switzerland without New Fighter Jets”	55.61	42.81
1993-09-26	Federal Decision on Temporary Measures against Cost Increases in Health Care	39.80	80.55
1993-11-28	Initiative “for Reducing Alcohol Problems”	45.51	25.26
1994-02-20	Initiative “for Protecting the Alpine Region against Transit Traffic”	40.86	51.91
1994-06-12	Federal Decision on the Facilitated Naturalization for Young Foreign Nationals	46.78	52.84
1994-09-25	Federal Penal Code and Military Penal Code (Racial Discrimination)	45.93	54.65
1994-12-04	Federal Act on Coercive Measures under the Law on Foreigners	44.06	72.91
1995-03-12	Federal Decision on Curbing Expenditures	37.88	83.38
1995-06-25	Federal Act on Old Age Insurance	40.45	60.71
1996-03-10	Federal Decision Abolishing Cantonal Responsibility for the Equipment of Soldiers	31.04	43.70
1996-06-09	Initiative “Farmers and Consumers - for a natural Agriculture” (counter-proposal)	31.44	77.59
1996-12-01	Federal Act on Labor	46.76	32.97
1997-06-08	Initiative “for a Ban on Exports of War Material”	35.50	22.50
1997-09-28	Federal Decision on the Financing of the Unemployment Insurance	40.65	49.18
1998-06-07	Initiative “for Protecting Life and Environment from Genetic Engeneering”	41.35	33.29

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Date	Vote Title	Turnout (%)	Yes (%)
1998-09-27	Federal Act on Power-Dependent Levies on Heavy Traffic	51.85	57.20
1998-11-29	Initiative “for a Reasonable Drug Policy”	38.39	26.01
1999-02-07	Federal Decision on a Constitutional Article on Transplant Medicine	38.01	87.77
1999-04-18	Federal Decision on a New Constitution	35.93	59.16
1999-06-13	Federal Decision on Maternity Insurance	45.98	38.99
2000-03-12	Initiative “for Halving Motorised Traffic and Conserving Habitats”	42.41	21.33
2000-05-21	Federal Decision on Bilateral Treaties between Switzerland and the EU	48.35	67.19
2000-09-24	Initiative “for Regulating Immigration”	45.31	36.20
2000-11-26	Initiative “for Lower Hospital Costs”	41.69	17.89
2001-03-04	Initiative “Yes to Europe!”	55.84	23.15
2001-06-10	Federal Act on the Army and Military Administration (Armament)	42.55	50.99
2001-12-02	Initiative “for a Credible Security Policy and Switzerland without an Army”	37.96	21.90
2002-03-03	Initiative “for Accession to the UN”	58.48	54.61
2002-06-02	Federal Penal Code (Abortion)	41.85	72.15
2002-09-22	Initiative “Gold Reserves for the Old Age Insurance”	45.21	47.56
2002-11-24	Initiative “against Abuse of Asylum”	47.97	49.91
2003-02-09	Federal Act on Adjusting Cantonal Contributions to Hospitals	28.74	77.36
2003-05-18	Initiative “Energy without Nuclear Power - For a Stepwise Phaseout”	49.77	33.71
2004-02-08	Initiative “Liefelong Custody for Untreatable, Extremely Dangerous Offenders”	45.54	56.19
2004-05-16	Federal Act on Tax Refrom and Revision Stamp Duties	50.85	34.12
2004-09-26	Federal Act on Compensation for Loss of Earings (Motherhood)	53.82	55.45
2004-11-28	Federal Act on Stem Cell Research	37.04	66.39
2005-06-05	Federal Decision on the Association to the EU Schengen-Dublin Agreements	56.64	54.63
2005-09-25	Federal Decision Extending Free Movement of Persons to New EU Member States	54.29	55.98
2005-11-27	Initiative “Initiative for GMO-Free Agriculture”	42.25	55.67

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Date	Vote Title	Turnout (%)	Yes (%)
2006-05-21	Federal Decision on Revising Constituional Provisions for Education	27.80	85.58
2006-09-24	Federal Act on Asylum	48.92	67.76
2006-11-26	Federal Act on Family Allowances	45.01	67.98
2007-03-11	Initiative “for a Unified Social Health Insurance”	45.94	28.76
2007-06-17	Federal Act on Disability Insurance	36.20	59.09
2008-02-24	Federal Act on Corporate Tax Reform	38.63	50.53
2008-06-01	Initiative “for Democratic Naturalizations”	45.18	36.25
2008-11-30	Initiative “for a Flexible Retirement Age”	47.67	41.38
2009-02-08	Federal Decision Extending Free Movement of Persons to New EU Members	51.44	59.61
2009-05-17	Initiative “Yes to Complementary Medicine” (counter-proposal)	38.80	67.03
2009-09-27	Federal Decision on Funding the Disability Insurance by Raising the VAT	41.01	54.56
2009-11-29	Initiative “against the Construction of Minarets”	53.76	57.50
2010-03-07	Federal Act on the Occupational Pension Scheme	45.75	27.27
2010-09-26	Federal Act on the Unemployment Insurance	35.84	53.42
2010-11-28	Initiative “for the Expulsion of Criminal Foreign Nationals”	52.93	52.91
2011-02-13	Initiative “for Protection against Armed Violence”	49.12	43.70
2012-03-11	Initiative “Limiting the Construction of Second Homes”	45.18	50.63
2012-06-17	Federal Act on Health Insurance (Managed Care)	38.65	23.95
2012-09-23	Federal Decision on a Constitutional Article Promoting Music Lessons for the Young	42.42	72.69
2012-11-25	Federal Act on Epizootic Diseases	27.60	68.28
2013-03-03	Initiative “against Rip-Off Salaries”	46.74	67.96
2013-06-09	Federal Act on Asylum	39.43	78.45
2013-09-22	Initiative “Repealing Compulsory Military Service”	46.89	26.79
2013-11-24	Federal Act on Tolls for the Use of National Roads	53.61	39.54
2014-02-09	Initiative “against Mass Immigration”	56.57	50.33

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Date	Vote Title	Turnout (%)	Yes (%)
2014-05-18	Initiative “for Protecting Fair Wages (Minimum Wage Initative)”	56.36	23.73
2014-09-28	Initiative “for a Public Health Insurance”	47.18	38.16
2014-11-30	Initiative “Stop Overpopulation - for Securing Natural Life Resources”	49.98	25.90
2015-03-08	Initiative “for an Energy Tax Instaed of the Value Added Tax”	42.06	8.03
2015-06-14	Initiative “for Bequest Taxes on the Wealthy for Funding the Old Age Insurance”	43.71	28.96
2016-02-28	Initiative “for Enforcing the Expulsion of Criminal Foreign Nationals”	63.73	41.15
2016-06-05	Federal Act on Asylum	46.79	66.78
2016-09-25	Initiative “for a Stronger Old Age Insurance”	43.13	40.60
2016-11-27	Initiative “for a Structured Nuclear Phaseout”	45.38	45.80
2017-02-12	Federal Decision on Facilitaed Naturalization of Third Generation Foreign Nationals	46.84	60.41
2017-05-21	Federal Act on Energy	42.89	58.22
2017-09-24	Federal Act on the Old Age Insurance Reform 2020	47.39	47.31
2018-03-04	Initiative “for Abolishing Radio and Television Fees”	54.84	28.44
2018-06-10	Initiative “for Crisis-Proof Money: Money Creation Only by the Central Bank”	34.55	24.28
2018-09-23	Initiative “for Healthy, Environmentally Friendly and Fair Food”	37.52	38.70
2018-11-25	Federal Act on Social Insurance	48.38	64.72
2019-02-10	Initiative “against Urban Sprawling - for a Sustainable Settlement Development”	37.92	36.34
2019-05-19	Federal Act on Tax Reform and Funding for Old Age Insurance	43.74	66.38

TABLE A.2: LIST OF NEWSPAPERS CONSULTED FOR POLL COVERAGE AND POLITICAL ADS

Newspaper	Language	# of cantons for which it has been used
Aargauer Zeitung	German	1
Badener Woche	German	1
Basellandschaftl. Ztg.	German	1
Basler Zeitung	German	2
Berner Zeitung	German	2
Blick	German	20
Blick am Abend	German	15
Bund	German	1
Büwo	German	1
Caffè della domenica (Il)	Italian	1
Corriere del Ticino	Italian	1
Côte (La)	French	1
Engadiner Post	German	1
(L')Express (aggregated with L'Impartial)	French	1
Freiburger Nachrichten	German	1
Giornale del Popolo	Italian	1
Gruyère (La)	French	1
Liberté (La)	French	1
Matin (Le)	French	6
Matin Dimanche (Le)	French	6
Matin Bleu (Le)	French	6
Mattino della Domenica (Il)	Italian	1
Neue Luzerner Zeitung GES (sometimes aggregated with: Neue Nidwaldner Zeitung; Neue Obwaldner Zeitung; Neue Schwyzer Zeitung; Neue Urner Zeitung; Neue Zuger Zeitung)	German	6
Nouvelliste (Le)	French	1
NZZ	German	3
NZZ am Sonntag	German	14
Ostschweiz am Sonntag	German	4
Quotidien Jurassien (Le)	French	1
Regione Ticino (La)	Italian	1
Rheinzeitung	German	2

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Newspaper	Language	# of cantons for which it has been used
Schaffhauser Nachrichten	German	1
Sonntag (Schweiz am Sonntag from 2013)	German	5
Sonntags Blick	German	21
Sonntags Zeitung	German	19
St. Galler Tagblatt (sometimes aggregated with: Appenzeller Zeitung)	German	5
Südostschweiz GES (Die)	German	4
Südostschweiz am Sonntag	German	1
Tages-Anzeiger	German	7
Temps (Le)	French	2
Thurgauer Zeitung	German	1
Tribune de Genève	French	1
Walliser Bote	German	1
Wiler Zeitung	German	1
Zentralschweiz am Sonntag	German	5
Zuger Woche	German	1
Zürichsee Zeitung	German	1
20 Minuten	German	19
20 Minutes	French	6
20 Minuti	Italian	1
24 Heures	French	1

TABLE A.3: DAILY TURNOUT BEFORE AND AFTER POLL RELEASE DEPENDING ON POLL CLOSENESS: BINS OF TWO DAYS

	Net Turnout (%)		Log(Turnout)	
	(1)	(2)	(3)	(4)
4-5 days before poll × *Ex Ante* Closeness (std.)	0.0669	-0.0155	0.0445	0.0138
	(0.2799)	(0.2757)	(0.1613)	(0.1580)
	[0.828]	*[0.960]*	*[0.816]*	*[0.936]*
2-3 days before poll × *Ex Ante* Closeness (std.)	0.1231	0.0851	-0.0143	-0.0232
	(0.2079)	(0.2105)	(0.1120)	(0.1145)
	[0.655]	*[0.798]*	*[0.994]*	*[0.991]*
1-2 days after poll × *Ex Ante* Closeness (std.)	0.4042***	0.3786***	0.1124***	0.1055**
	(0.1436)	(0.1413)	(0.0411)	(0.0402)
	[0.015]	*[0.018]*	*[0.023]*	*[0.024]*
3-4 days after poll × *Ex Ante* Closeness (std.)	0.3565**	0.3490**	0.0864*	0.0870*
	(0.1417)	(0.1454)	(0.0432)	(0.0458)
	[0.024]	*[0.032]*	*[0.069]*	*[0.083]*
5-6 days after poll × *Ex Ante* Closeness (std.)	0.2231	0.2293	0.0764	0.0786
	(0.1848)	(0.1853)	(0.0466)	(0.0482)
	[0.256]	*[0.242]*	*[0.132]*	*[0.129]*
7-8 days after poll × *Ex Ante* Closeness (std.)	0.2301	0.2294	0.0757	0.0774
	(0.2602)	(0.2634)	(0.0606)	(0.0630)
	[0.400]	*[0.401]*	*[0.235]*	*[0.239]*
9+ days after poll × *Ex Ante* Closeness (std.)	0.4494	0.4105	0.1037*	0.0997
	(0.2843)	(0.2780)	(0.0598)	(0.0609)
	[0.148]	*[0.173]*	*[0.112]*	*[0.128]*
Test of Cumulative Effects After Poll Release *(p-value)*	*0.030*	*0.035*	*0.023*	*0.029*
	[0.048]	*[0.053]*	*[0.035]*	*[0.040]*
Test of Joint Significance of Leads *(p-value)*	*0.812*	*0.812*	*0.848*	*0.912*
	[0.863]	*[0.871]*	*[0.964]*	*[0.990]*
Outcome Mean	4.611	4.611	8.764	8.764
R-squared	0.496	0.517	0.232	0.255
Observations	766	766	766	766
Vote Fixed Effects	Y	Y	Y	Y
Voting Day from/to Poll Fixed Effects	Y	Y	Y	Y
Day to Vote Fixed Effects	N	Y	N	Y

Notes: The table presents OLS estimates with two measures of daily turnout in Geneva as dependent variables: Net Turnout (columns 1 and 2) defined as the number of votes cast, in percent of eligible voters net of those voters who cast their vote on earlier days; and Log(Turnout) (columns 3 and 4) defined as the natural logarithm of the number of votes cast. *Ex Ante* Closeness is the trailing side's vote share predicted by the pre-election poll whose release date (and the preceding day) are the omitted days of reference. Test of Cumulative Effects After Poll Release reports the p-value of a one-sided F-test that the sum of the coefficients on days after poll × *Ex Ante* Closeness is less or equal to zero. Test of Joint Significance of Leads reports the p-value of an F-test that the coefficients on before poll × *Ex Ante* Closeness are all equal to zero. P-values of analogous Wald tests based on the wild cluster bootstrap in brackets. The sample is an unbalanced panel of 52 votes held between 2001 and 2019 observed from 5 voting days before to the last voting day after poll release. Standard errors in parentheses, clustered at the vote level: $^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

TABLE A.4: DAILY TURNOUT BEFORE AND AFTER POLL RELEASE DEPENDING ON AVERAGE POLL CLOSENESS: SINGLE DAYS

	Net Turnout (%)		Log(Turnout)	
	(1)	(2)	(3)	(4)
5 days before poll × Average Closeness (std.)	0.1078	0.0738	0.0406	0.0280
	(0.3745)	(0.3850)	(0.1888)	(0.1978)
	[0.777]	*[0.850]*	*[0.835]*	*[0.887]*
4 days before poll × Average Closeness (std.)	0.0660	0.0138	-0.0337	-0.0482
	(0.3584)	(0.3725)	(0.1638)	(0.1693)
	[0.857]	*[0.971]*	*[0.847]*	*[0.789]*
3 days before poll × Average Closeness (std.)	0.2349	0.1758	0.0548	0.0450
	(0.2818)	(0.2853)	(0.1484)	(0.1520)
	[0.422]	*[0.559]*	*[0.693]*	*[0.736]*
2 days before poll × Average Closeness (std.)	0.2553	0.3246	0.0766	0.1012
	(0.2723)	(0.2843)	(0.1265)	(0.1318)
	[0.363]	*[0.265]*	*[0.534]*	*[0.477]*
1 day before poll × Average Closeness (std.)	-0.0561	-0.0400	0.0452	0.0555
	(0.2327)	(0.2419)	(0.0978)	(0.1022)
	[0.812]	*[0.870]*	*[0.720]*	*[0.665]*
1 day after poll × Average Closeness (std.)	0.5833**	0.5689**	0.1924*	0.1920*
	(0.2655)	(0.2702)	(0.1074)	(0.1122)
	[0.030]	*[0.039]*	*[0.058]*	*[0.069]*
2 days after poll × Average Closeness (std.)	0.5225**	0.4984**	0.1851*	0.1815*
	(0.2435)	(0.2455)	(0.0973)	(0.1009)
	[0.051]	*[0.060]*	*[0.057]*	*[0.066]*
3 days after poll × Average Closeness (std.)	0.5375**	0.5857**	0.1658*	0.1823*
	(0.2598)	(0.2697)	(0.0972)	(0.1018)
	[0.049]	*[0.040]*	*[0.086]*	*[0.070]*
4 days after poll × Average Closeness (std.)	0.2945	0.2672	0.1213	0.1237
	(0.3000)	(0.3091)	(0.1089)	(0.1143)
	[0.341]	*[0.396]*	*[0.284]*	*[0.296]*
5 days after poll × Average Closeness (std.)	0.2247	0.2485	0.1097	0.1200
	(0.2396)	(0.2498)	(0.0967)	(0.1016)
	[0.364]	*[0.335]*	*[0.280]*	*[0.255]*
6 days after poll × Average Closeness (std.)	0.1616	0.1809	0.1103	0.1167
	(0.3060)	(0.3111)	(0.1047)	(0.1093)
	[0.606]	*[0.565]*	*[0.313]*	*[0.303]*
7 days after poll × Average Closeness (std.)	0.3594	0.3595	0.1432	0.1497
	(0.3115)	(0.3241)	(0.1102)	(0.1156)
	[0.262]	*[0.276]*	*[0.203]*	*[0.203]*
8 days after poll × Average Closeness (std.)	0.1014	0.1116	0.1024	0.1105
	(0.3490)	(0.3518)	(0.1115)	(0.1158)
	[0.780]	*[0.758]*	*[0.378]*	*[0.358]*
9+ days after poll × Average Closeness (std.)	0.3325	0.2971	0.1361	0.1358
	(0.3516)	(0.3479)	(0.1086)	(0.1132)
	[0.358]	*[0.406]*	*[0.223]*	*[0.242]*
Test of Cumulative Effects After Poll Release *(p-value)*	*0.095*	*0.101*	*0.084*	*0.086*
	[0.101]	*[0.106]*	*[0.085]*	*[0.086]*
Test of Joint Significance of Leads *(p-value)*	*0.793*	*0.610*	*0.693*	*0.399*
	[0.841]	*[0.695]*	*[0.788]*	*[0.444]*
Outcome Mean	4.611	4.611	8.764	8.764
R-squared	0.500	0.520	0.239	0.263
Observations	766	766	766	766
Vote Fixed Effects	Y	Y	Y	Y
Voting Day from/to Poll Fixed Effects	Y	Y	Y	Y
Day to Vote Fixed Effects	N	Y	N	Y

Notes: The table presents OLS estimates with two measures of daily turnout in Geneva as dependent variables: Net Turnout (columns 1 and 2) defined as the number of votes cast, in percent of eligible voters net of those voters who cast their vote on earlier days; and Log(Turnout) (columns 3 and 4) defined as the natural logarithm of the number of votes cast. Average Closeness is the mean of the trailing side's vote shares across all pre-election polls for votes of the same election day. The polls' release date is the omitted day of reference. Test of Cumulative Effects After Poll Release reports the p-value of a one-sided F-test that the sum of the coefficients on days after poll × Average Closeness is less or equal to zero. Test of Joint Significance of Leads reports the p-value of an F-test that the coefficients on before poll × Average Closeness are all equal to zero. P-values of analogous Wald tests based on the wild cluster bootstrap in brackets. The sample is an unbalanced panel of 52 votes held between 2001 and 2019 observed from 5 voting days before to the last voting day after poll release. Standard errors in parentheses, clustered at the vote level: $^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

TABLE A.5: DAILY TURNOUT BEFORE AND AFTER POLL RELEASE DEPENDING ON AVERAGE POLL CLOSENESS: BINS OF TWO DAYS

	Net Turnout (%)		Log(Turnout)	
	(1)	(2)	(3)	(4)
4-5 days before poll × Average Closeness (std.)	0.1147	0.0642	-0.0196	-0.0381
	(0.2726)	(0.2788)	(0.1420)	(0.1474)
	[0.685]	*[0.825]*	*[0.906]*	*[0.816]*
2-3 days before poll × Average Closeness (std.)	0.2733	0.2714	0.0432	0.0458
	(0.2049)	(0.2068)	(0.1069)	(0.1098)
	[0.189]	*[0.196]*	*[0.778]*	*[0.777]*
1-2 days after poll × Average Closeness (std.)	0.5810***	0.5537***	0.1661**	0.1590**
	(0.1955)	(0.1936)	(0.0624)	(0.0636)
	[0.002]	*[0.003]*	*[0.004]*	*[0.006]*
3-4 days after poll × Average Closeness (std.)	0.4441**	0.4462**	0.1210**	0.1252**
	(0.1693)	(0.1710)	(0.0563)	(0.0591)
	[0.016]	*[0.018]*	*[0.038]*	*[0.041]*
5-6 days after poll × Average Closeness (std.)	0.2212	0.2350	0.0874	0.0907
	(0.1864)	(0.1886)	(0.0573)	(0.0597)
	[0.251]	*[0.223]*	*[0.133]*	*[0.131]*
7-8 days after poll × Average Closeness (std.)	0.2584	0.2559	0.1002	0.1024
	(0.2354)	(0.2396)	(0.0680)	(0.0711)
	[0.294]	*[0.300]*	*[0.148]*	*[0.156]*
9+ days after poll × Average Closeness (std.)	0.3606	0.3177	0.1135	0.1083
	(0.2862)	(0.2741)	(0.0688)	(0.0707)
	[0.219]	*[0.261]*	*[0.105]*	*[0.130]*
Test of Cumulative Effects After Poll Release *(p-value)*	*0.028*	*0.032*	*0.024*	*0.030*
	[0.027]	*[0.031]*	*[0.022]*	*[0.027]*
Test of Joint Significance of Leads *(p-value)*	*0.371*	*0.342*	*0.736*	*0.660*
	[0.403]	*[0.373]*	*[0.801]*	*[0.732]*
Outcome Mean	4.611	4.611	8.764	8.764
R-squared	0.499	0.519	0.238	0.261
Observations	766	766	766	766
Vote Fixed Effects	Y	Y	Y	Y
Voting Day from/to Poll Fixed Effects	Y	Y	Y	Y
Day to Vote Fixed Effects	N	Y	N	Y

Notes: The table presents OLS estimates with two measures of daily turnout in Geneva as dependent variables: Net Turnout (columns 1 and 2) defined as the number of votes cast, in percent of eligible voters net of those voters who cast their vote on earlier days; and Log(Turnout) (columns 3 and 4) defined as the natural logarithm of the number of votes cast. Average Closeness is the mean of the trailing side's vote shares across all pre-election polls for votes of the same election day, whose release date (and the preceding day) are the omitted days of reference. Test of Cumulative Effects After Poll Release reports the p-value of a one-sided F-test that the sum of the coefficients on days after poll × Average Closeness is less or equal to zero. Test of Joint Significance of Leads reports the p-value of an F-test that the coefficients on before poll × Average Closeness are all equal to zero. P-values of analogous Wald tests based on the wild cluster bootstrap in brackets. The sample is an unbalanced panel of 52 votes held between 2001 and 2019 observed from 5 voting days before to the last voting day after poll release. Standard errors in parentheses, clustered at the vote level: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

TABLE A.6: NEWSPAPER COVERAGE, CLOSENESS AND CANTONAL VOTER TURNOUT: READERSHIP-WEIGHTED POLL MENTIONS

	Turnout (%)		Advertisements (std.)		Importance (std.)	
PANEL A: POLL MENTIONS IN CANTONAL NEWSPAPERS	(1)	(2)	(3)	(4)	(5)	(6)
Readership-weighted Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.5567**	0.5744***	0.0217	0.0214	0.0020	-0.0047
	(0.2104)	(0.2089)	(0.0285)	(0.0262)	(0.0462)	(0.0465)
	[0.018]	*[0.010]*	*[0.462]*	*[0.437]*	*[0.967]*	*[0.925]*
Readership-weighted Poll Mentions (std.)	0.1510	0.8465*	0.1340***	0.0954**	0.0608	0.0234
	(0.1688)	(0.4544)	(0.0408)	(0.0432)	(0.0557)	(0.0839)
	[0.369]	*[0.087]*	*[0.000]*	*[0.038]*	*[0.295]*	*[0.788]*
R-squared	0.821	0.823	0.866	0.866	0.329	0.330
PANEL B: INCIDENTAL POLL MENTIONS						
Readership-weighted Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.2993	0.3881**	0.0295	0.0401*	0.0260	0.0086
	(0.1860)	(0.1685)	(0.0294)	(0.0236)	(0.0335)	(0.0493)
	[0.126]	*[0.034]*	*[0.344]*	*[0.129]*	*[0.446]*	*[0.865]*
Readership-weighted Poll Mentions (std.)	-0.2368	1.3535*	0.1904***	0.2078**	0.1145	0.0876
	(0.3455)	(0.7630)	(0.0624)	(0.0943)	(0.0685)	(0.1462)
	[0.510]	*[0.082]*	*[0.003]*	*[0.060]*	*[0.096]*	*[0.581]*
R-squared	0.820	0.822	0.868	0.868	0.331	0.331
PANEL C: INCIDENTAL POLL MENTIONS *(<15% Market Share)*						
Readership-weighted Poll Mentions (std.) × *Ex Ante* Closeness (std.)	0.3530**	0.4571***	0.0228	0.0298	0.0262	0.0087
	(0.1610)	(0.1436)	(0.0247)	(0.0190)	(0.0336)	(0.0462)
	[0.031]	*[0.002]*	*[0.398]*	*[0.169]*	*[0.441]*	*[0.852]*
Readership-weighted Poll Mentions (std.)	-0.1772	1.8231**	0.2021***	0.2177**	0.0729	0.0780
	(0.3239)	(0.7542)	(0.0653)	(0.0819)	(0.0673)	(0.1352)
	[0.617]	*[0.025]*	*[0.003]*	*[0.032]*	*[0.294]*	*[0.600]*
R-squared	0.820	0.823	0.868	0.868	0.329	0.330
Outcome Mean	47.273	47.273	73.927	73.927	6.115	6.115
Outcome Std. Dev.	8.910	8.910	68.180	68.180	1.132	1.132
Observations	962	962	962	962	957	957
German × Poll Mentions (std.)	N	Y	N	Y	N	Y
German × *Ex Ante* Closeness (std.)	N	Y	N	Y	N	Y

Notes: This table replicates Table I.4 with Readership-weighted Poll Mentions, i.e., cantonal newspapers' poll mentions multiplied with the the cantonal share of peolple reading the newspaper in which the poll is mentioned. Each panel presents results from six OLS regressions using three dependent variables: cantonal turnout (Columns 1 and 2), the standardized number of newspaper advertisements in cantonal newspapers (Columns 3 and 4), and standardized importance, as rated by a canton's average VOX survey responses (Columns 5 and 6). In Panel A, Readership-weighted Poll Mentions (std.) refer to the standardized readership-weighted count of poll mentions in cantonal newspapers, i.e., newspapers read by at least 10% of a canton's inhabitants. In Panel B, only Incidental Poll Mentions are considered, i.e., mentions in cantonal newspapers whose main market lies in another canton. Panel C further restricts Incidental Poll Mentions to mentions in newspapers whose cantonal readership accounts for less than 15% of the newspaper's total readership. *Ex Ante* Closeness is the losing side's vote share at the federal level, as predicted by the pre-election poll. All specifications include canton and vote fixed effects. Columns 2, 4, and 6 additionally control for a dummy equal to one for German-speaking cantons, interacted with both *Ex Ante* Closeness (std.) and Readership-weighted Poll Mentions (std.). The sample is a panel of 26 cantons, observed in 37 votes held between 2000 and 2014. Standard errors clustered at the vote level in parentheses: $^{*}p < 0.1$,** $p < 0.05$,*** $p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

TABLE A.7: NEWSPAPER COVERAGE, CLOSENESS AND CANTONAL VOTER TURNOUT: IV ESTIMATES

	First Stage		*Second Stage*
	(1)	(2)	(3)
	Poll Mentions (std.)	Poll Mentions (std.) $\times$ *Ex Ante* Closeness (std.)	Turnout
Incidental Poll Mentions (std.) $\times$ *Ex Ante* Closeness (std.)	0.0231	0.3880***	
	(0.0392)	(0.0433)	
	[0.610]	*[0.000]*	
Incidental Poll Mentions (std.)	0.7351***	0.1020*	
	(0.0397)	(0.0578)	
	[0.000]	*[0.139]*	
Poll Mentions (std.) $\times$ *Ex Ante* Closeness (std.)			0.9789**
			(0.4790)
			[0.072]
Poll Mentions (std.)			-0.2472
			(0.3788)
			[0.530]
R-squared	0.866	0.699	0.819
Observations	962	962	962
Test of Joint Significance of Excluded Instruments *(p-value)*	< 0.0001	< 0.0001	
	[< 0.0001]	[< 0.0001]	
Test β (Incidental Poll Mentions) ≥ 1 *(p-value)*	< 0.0001		
	[< 0.0001]		

Notes: The table presents two-stage least squares estimates using Incidental Poll Mentions as an instrument for Poll Mentions in cantonal newspapers. Columns 1 and 2 show the two first-stage estimates. Column 3 shows estimates of the second stage. Test of Joint Significance of Excluded Instruments reports the p-value of an F-test that all excluded instruments are equal to zero. Test β (Incidental Poll Mentions) reports the p-value of a one-tailed F-test that the effect of Incidental Poll Mentions on endogeneous Poll Mentions is greater than or equal to one. P-values of analogous Wald tests based on the wild cluster bootstrap in brackets. All specifications include canton and vote fixed effects. The sample is a balanced panel of 26 cantons observed in 37 votes held between 2000 and 2014. Standard errors in parentheses, clustered at the vote level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. P-values obtained from the wild cluster bootstrap in brackets.

TABLE A.8: HETEROGENEOUS EFFECTS OF ELECTION CLOSENESS AND POLLS DEPENDING ON MUNICIPALITY POLITICAL HOMOGENEITY AND NATIONAL LEVEL TURNOUT AS PROXY FOR SALIENCE

	(1)	(2)	(3)	(4)	(5)	(6)
Ex Post Closeness (std.) × Homogeneity (std.)	-0.5674***	-0.5659***			-0.6128***	-0.6097***
	(0.1900)	(0.1904)			(0.1885)	(0.1891)
Ex Post Closeness (std.) × Homogeneity (std.) × Poll Era	0.5874**	0.5822**			0.6123**	0.6111**
	(0.2631)	(0.2630)			(0.2785)	(0.2786)
National Turnout (std.) × Homogeneity (std.)			0.1772	0.1687	0.2580**	0.2491*
			(0.1219)	(0.1222)	(0.1270)	(0.1273)
National Turnout (std.) × Homogeneity (std.) × Poll Era			-0.1084	-0.1177	-0.1890	-0.1986
			(0.2118)	(0.2119)	(0.2370)	(0.2374)
Homogeneity (std.) × Poll Era	2.3689***	2.3936***	2.3255***	2.3551***	2.2953***	2.3252***
	(0.2319)	(0.2319)	(0.2476)	(0.2478)	(0.2390)	(0.2395)
Test for Convergence of Closeness Gradients *(p-value)*	*0.913*	*0.928*			*0.998*	*0.995*
Test for Convergence of Salience Gradients *(p-value)*			*0.692*	*0.769*	*0.731*	*0.802*
Outcome Mean	44.001	44.001	44.001	44.001	44.001	44.001
R-squared	0.700	0.700	0.699	0.700	0.700	0.700
Observations	250240	250240	250240	250240	250240	250240
Municipality Fixed Effects	Y	Y	Y	Y	Y	Y
Vote Fixed Effects	Y	Y	Y	Y	Y	Y
Electorate Size	N	Y	N	Y	N	Y

Notes: Each column presents results from an OLS regression with municipality-level voter turnout as the dependent variable. Political Homogeneity is a municipalitiy's historical tendency to produce voting results distant from 50-50, as measured by the average municipal-level margin of majority across all votes held in the era before pre-election polls. Poll Era is a dummy equal to 1 for 69 votes held after the introduction of polls in 1998. National Turnout is the Swiss national level turnout rate for the vote. Test for Convergence of Closeness Gradients reports the p-value of an F-test that the sum of the coefficients on *Ex Post* Closeness (std.) × Homogeneity (std.) and *Ex Post* Closeness (std.) × Homogeneity (std.) × Poll Era (std.) equals 0. Test for Convergence of Salience Gradients reports the p-value of an F-test that the sum of the coefficients on National Turnout (std.) × Homogeneity (std.) and National Turnout (std.) × Homogeneity (std.) × Poll Era (std.) equals 0. Columns 2, 4 and 6 control for a triple interaction of Poll Era and the standardized average municipality electorate size with either *Ex Post* Closeness (Column 2) or National Turnout (Column 4) or both (Column 6), as well as lower order terms. The sample is a balanced panel of 2176 municipalities observed in 115 votes held from 1981 to 2019. Standard errors clustered at the vote level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.9: ASYMMETRIC EFFECTS OF *Ex Ante* CLOSENESS ON TURNOUT AND VOTE SHARES: TRIMMED SAMPLE

	(1) Turnout	(2) Vote Share
Ex Ante Closeness (std.) × Trailing Side's Estimated Support	0.0126*	0.0631***
	(0.0074)	(0.0212)
Trailing Side's Estimated Support	0.0118	0.3903***
	(0.0082)	(0.0254)
Outcome Mean	46.913	43.132
R-squared	0.859	0.876
Observations	119394	119394

Notes: The table presents estimates from OLS regressions with municipality level voter turnout (column 1) and vote share for the trailing side (column 2) as dependent variables. The trailing side's Vote Share is defined as a municipality's share of votes cast in line with the trailing side in the pre-election poll, i.e., with the minority of poll respondents. Trailing Side's Estimated Support is a municipality's predetermined pre-disposition to vote for the side trailing in the pre-election poll, measured as a municipality's vote share, in percent of votes cast, in the preceding national election for parties whose voting recommendations are in line with the minority of poll respondents. *Ex Ante* Closeness is the trailing side's vote share in the pre-election poll. All specifications include municipality and canton × vote fixed effects. The sample is an unbalanced panel of 2176 municipalities observed in 57 votes with a pre-election poll held from 1998 to 2019, excluding all observations with 0% or 100% support for the trailing side. Standard errors clustered at the vote level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter II
Out of Office, Out of Step? Re-election Concerns and Ideological Shirking in Lame-Duck Sessions of the U.S. House of Representatives

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 ab-

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

sence 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, 1957b). 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 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

[2]Consistent with the Downsian notion of rational ignorance (Downs, 1957b), 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).

[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

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 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,

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.

[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.

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 to co-partisans in the same term (by conditioning on a full set of $party \times congress$ fixed effects) ensures the comparability of W-NOMINATE scores between lame ducks and bare election winners, and rules out differences in roll-call voting resulting from majority status and agenda control. The key identification assumption underlying the *difference*-in-discontinuity design is that bare election winners and narrow losers follow parallel trends (Grembi et al., 2016). In addition to providing evidence that bare election winners and narrow losers are similar in pre-determined characteristics, I validate this assumption by showing the absence of pre-trends at the cutoff in the year leading up to 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 116^{th} 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 incumbents 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

[8]In my sample covering the 111^{th} to 116^{th} 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%.

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 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 general 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 111^{th} to 116^{th} 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 II.1 provides an overview of the six lame-duck sessions held by the U.S. House between 2010 and 2020.

[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 II.1: Lame-Duck Sessions in the U.S. House of Representatives from the 111^{th} to the 116^{th} Congress

Lame Duck Session	# Scalable Roll Calls	Example Roll Calls	House Incumbents (Democrats / Republicans) All Members	RD Sample	Lost Election
111^{th} 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)
112^{th} 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)
113^{th} 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)
114^{th} 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)
115^{th} 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)
116^{th} 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 111^{th} and 116^{th} Congresses (2010-2020), listing the number of roll call votes used for scaling House Members by the W-NOMINATE algorithm, along with examples of bills subject to roll call votes during the lame duck sessions. The Table further lists the number of House Members at the beginning of each lame-duck session by party (Democrats/ Republicans), and the number of House representatives in the RD sample, i.e., incumbents defending their seat in a competitive general election race against a main challenger of opposite political orientation, which excludes members not running for re-election (because retiring, lost nomination in their party's primary, or running for higher office), members running unopposed or members whose strongest opponent is a minor party candidate of the same political orientation, members who switched party affiliation during the congressional term, and members whose position cannot be scaled by W-NOMINATE. The rightmost column indicates the number of incumbents in the RD sample who lost their re-election bid and returned as lame ducks to the post-electoral House sessions.

2.2 Roll Call Data and Legislator Positions

We are interested in whether the loss of 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 111^{th} to 116^{th} 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 B.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*:

$$\text{Roll Call Extremism}_{i(p)} = \begin{cases} \text{W-NOMINATE}_{i(p)} & \text{if } p = \text{Republican} \\ -\text{ W-NOMINATE}_{i(p)} & \text{if } p = \text{Democtrat} \end{cases} \tag{5}$$

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 $\times$ sessions, i.e., on a different set of roll calls and in comparison to different sets of representatives composing the House. Moreover, within a congressional term, *levels* of *roll call extremism* are not comparable across parties. My difference-in-discontinuities design therefore conditions on a full set of *congress* $\times$ *party* fixed effects and evaluates lame duck incumbents' *relative* repositioning, comparing their change in *roll call extremism* in the post-electoral session with respect to the pre-election period to the change in *roll call extremism* of re-elected incumbents of the same party, serving in the same Congress and voting on the same set of roll calls. *Congress* $\times$ *party* fixed effects also control for possibly divergent incentives for incumbents of different parties due to changing majority status,[12] and for possible

[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 (6) 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 II.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.

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.

Table II.2: Summary Statistics

Panel A: Main Variables of Interest	Mean	Std. Dev.	Min.	Max.	Obs.
Δ Roll Call Extremism	-0.170	0.273	-0.760	0.597	1954
Post-Election Roll Call Extremism	0.489	0.268	-0.333	1.000	1954
Pre-Election Roll Call Extremism	0.659	0.205	-0.092	1.000	1954
Δ W-NOMINATE (Democrats)	0.291	0.242	-0.547	0.760	984
Post-Election W-NOMINATE (Democrats)	-0.391	0.254	-0.999	0.333	984
Pre-Election W-NOMINATE (Democrats)	-0.682	0.219	-1.000	0.042	984
Δ W-NOMINATE (Republicans)	-0.048	0.247	-0.755	0.597	970
Post-Election W-NOMINATE (Republicans)	0.588	0.244	-0.073	1.000	970
Pre-Election W-NOMINATE (Republicans)	0.636	0.187	-0.092	0.995	970
Strongest Opponent's Vote Share Margin (%)	-0.282	0.193	-0.984	0.311	1954
Lame Duck Incumbent	0.066	0.247	0	1	1954
Panel B: Auxiliary Variables					
Δ Party Loyalty (%)	0.012	0.078	-0.481	0.330	1954
Post-Election Party Loyalty (%)	0.914	0.085	0.341	1.000	1954
Pre-Election Party Loyalty (%)	0.902	0.056	0.508	1.000	1954
Δ Absenteeism (%)	0.009	0.064	-0.307	0.514	1954
Post-Election Absenteeism (%)	0.039	0.064	0.000	0.553	1954
Pre-Election Absenteeism (%)	0.031	0.034	0.000	0.422	1954
Legislative Effectiveness	1.050	1.250	0.000	16.314	1951
Incumbent's Expected Margin of Victory	0.282	0.151	-0.115	0.899	1954

Notes: The Table presents summary statistics for the sample of 1954 U.S. House representatives of the 111^{th} to 116^{th} 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 5. 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 II.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 abstention, 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* $\times$ *party* fixed effects (including all lower order terms). As I discuss in more detail below, allowing the autocorrelation of vote shares to vary by party and election year accounts for wave elections that in a given year tend to favor either Democrats or Republicans. Depending on the electoral cycle, a Republican incumbent's *expected margin of victory* may therefore significantly differ from a Democrat's who was elected with the same prior vote share. 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:

$$\Delta Y_{ipc} = \theta T_{ipc} + \beta_1 X_{ipc} + \beta_2 X_{ipc} T_{ipc} + \lambda_{pc} + \epsilon_{ipc} \tag{6}$$

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 (5). 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* $\times$ *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 (6) 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]

[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

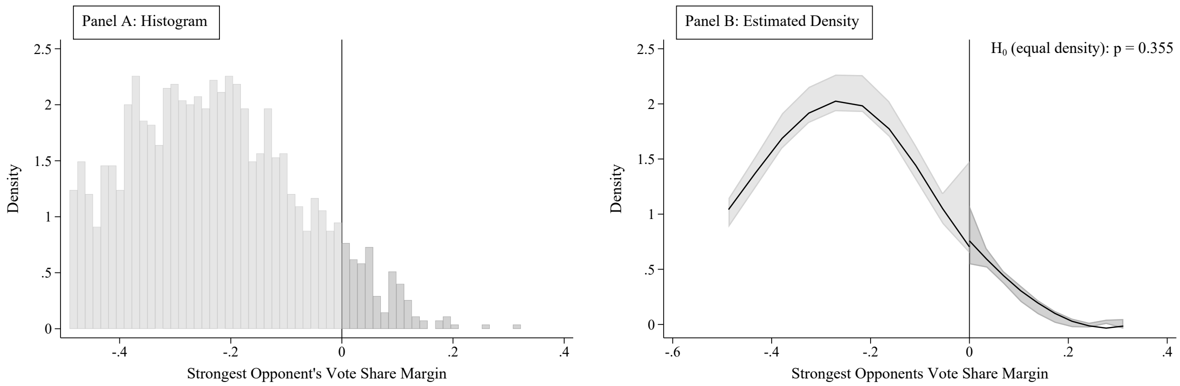
The main threat to identification is posed by concerns that close election winners could be differentially more able to manipulate election outcomes. This is a priori extremely unlikely, as it would require close election winners either to have precise information to predict election outcomes which is 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 II.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).

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).

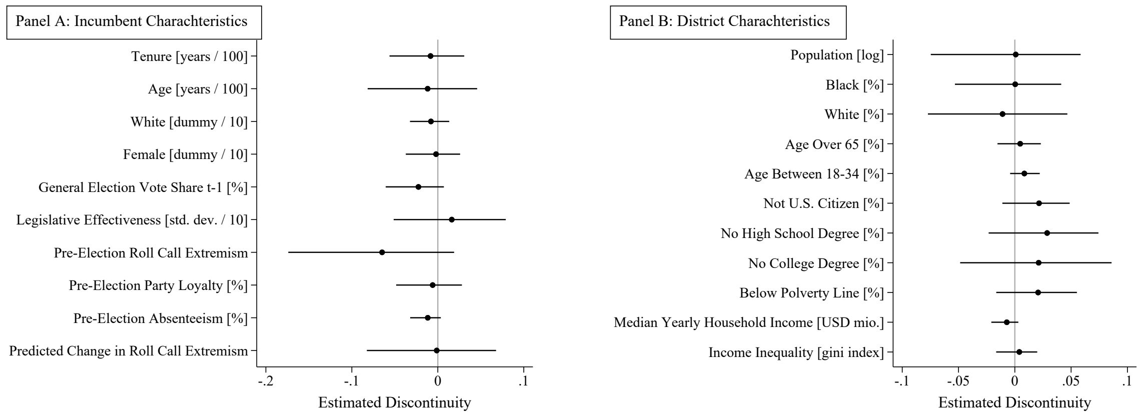
Figure II.1: Manipulation Tests for Aggregate Sorting



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 (6). 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 II.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.

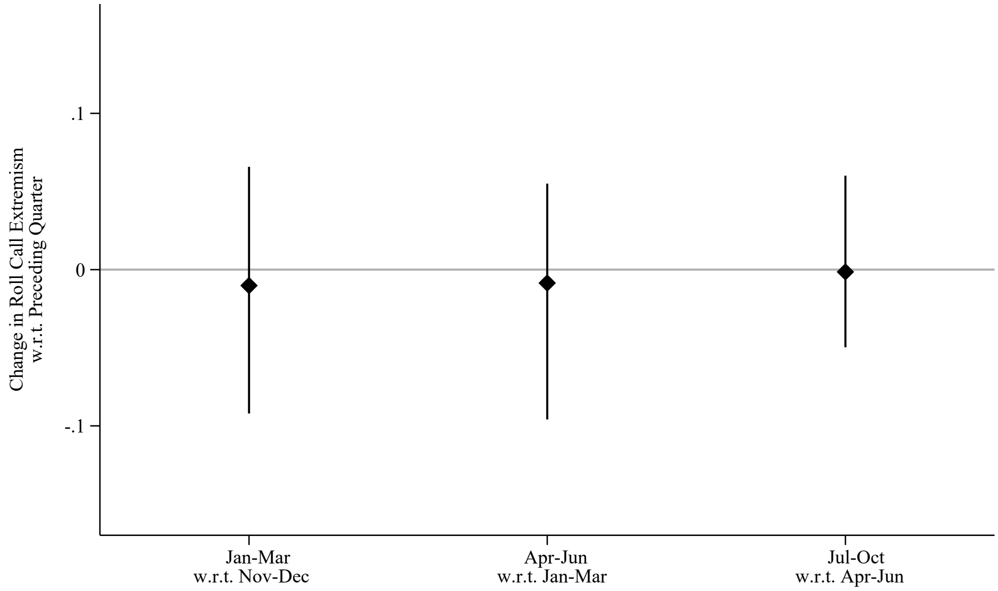
FIGURE II.2: BALANCING TESTS ON INCUMBENT TYPE AND DISTRICT CHARACTERISTICS



Notes: The Figure presents results from balancing tests on incumbent (Panel A) and district characteristics (Panel B). Point estimates (dots) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering by House representative are obtained from local linear specifications of equation (6) 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 II.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 B.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.

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



Notes: The Figure presents results from tests for pre-trends at the cutoff in the year preceding general elections. Each estimate represents the discontinuity in changes of incumbents' roll call extremism in one quarter with respect to the preceding quarter. Point estimates (diamonds) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering at the incumbent-level are obtained from local linear specifications of equation (6) with MSE-optimal bandwidths and triangular kernels.

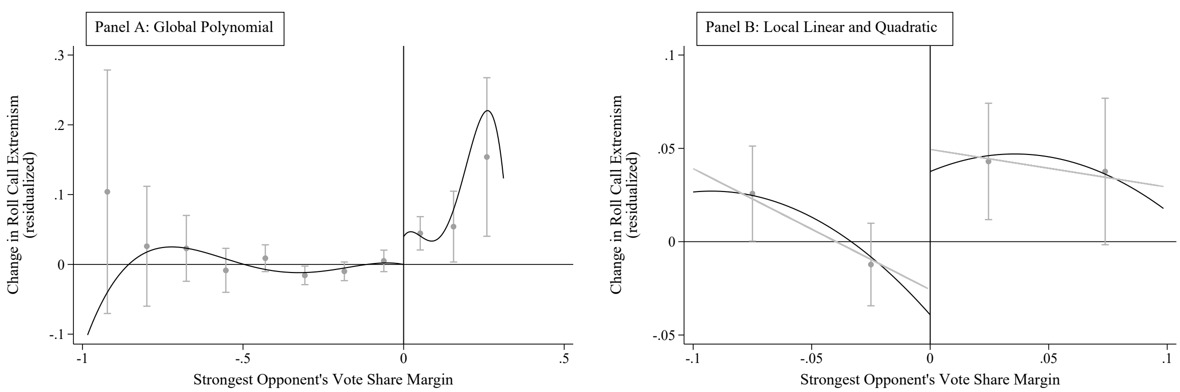
The key identification assumption of parallel trends around the cutoff could be violated if some incumbents had private information on the likely election outcome and differentially adjusted their roll call voting behavior over time upon learning signals of voter preferences or their relative popularity (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 (6) considering discontinuities in quarter-by-quarter changes in *roll call extremism* during the year leading up to general elections. Results presented in Figure II.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 II.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 II.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 II.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 (6) 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 II.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 B.1) and a wide range of alternative bandwidths choices (see Appendix Figure B.2).

TABLE II.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)
	0.097***	0.096***	-0.153***	-0.108***	0.092**	0.046***
	(0.027)	(0.020)	(0.040)	(0.027)	(0.042)	(0.014)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.067]	[0.060]
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	0.074	0.074	0.042	0.042	0.044	0.044
Effective Obs. Left	141	138	52	52	31	30
Effective Obs. Right	81	80	31	30	21	21
Control Mean	0.017	0.017	-0.024	-0.024	-0.003	-0.003
Observations	1954	1923	984	972	970	951

Notes: The Table presents results from local linear regressions specified in equation 6, 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 pooled sample including re-election seeking House incumbents of either party (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 II.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 II.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 116^{th} 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.

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

[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 116^{th} 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).

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, 1957b). 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 copartisan 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 emotional 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 elec-

[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.

torally 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* $\times$ *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 111^{th} Congress, see Table II.1) should therefore expect a lower vote share in midterm elections compared to representatives from the other party elected with the same vote share, whereas incumbents elected in a midterm wave election (e.g., Republicans in the 112^{th} 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 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 (6) on these subsamples, expecting larger

[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 B.3 shows the distributions of *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).

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

	Toss-up (1^{st} Tercile)		Competitive (2^{nd} Tercile)		Safe (3^{rd} Tercile)	
	(1)	(2)	(3)	(4)	(5)	(6)
	0.163***	0.143***	0.101**	0.088**	0.037	0.039
	(0.055)	(0.051)	(0.043)	(0.045)	(0.053)	(0.051)
	[0.002]	[0.001]	[0.014]	[0.023]	[0.870]	[0.245]
Bandwidth	0.037	0.050	0.040	0.050	0.042	0.050
Effective Obs. Left	22	26	26	35	28	33
Effective Obs. Right	21	25	14	17	14	16
Control Mean	0.032	0.033	-0.024	-0.026	0.032	0.022
Observations	127	127	176	176	1651	1651

Notes: The Table presents results from local linear regressions specified in equation 6, 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 33^{rd} percentile (toss-up), between the 33^{rd} and 67^{th} percentiles (competitive), and above the 67^{th} 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. All regressions use triangular kernels and include *party* $\times$ *congress* fixed effects. 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 II.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 empirical 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 II.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)
	0.122***	0.111***	0.152***	0.162***
	(0.045)	(0.041)	(0.037)	(0.038)
	[0.005]	[0.003]	[0.000]	[0.000]
Bandwidth	0.033	0.050	0.055	0.050
Effective Obs. Left	31	48	54	45
Effective Obs. Right	19	28	33	30
Control Mean	0.023	0.006	0.014	0.014
Observations	767	767	1184	1184

Notes: The Table presents results from local linear regressions specified in equation 6, 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. All regressions use triangular kernels and include *party* × *congress* fixed effects. 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 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

[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.

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

[23] Appendix Figure B.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 98^{th} to 103^{rd} Congresses (1983-1995) vote more often in party line than returning members, whereas Figlio (1995) reports the opposite: Retiring members of the 94^{th} to 97^{th} Congresses (1975-1983) voted less frequently with the majority of their party. Jenkins and Nokken (2008b) document that exiting House members in lame-duck session of the pre-Twentieth Amendment era (45^{th} to 72^{nd} Congress, 1877-1933) more likely deviated from party line than returning members.

(Canes-Wrone et al., 2007).

TABLE II.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**	0.097***	0.122***	0.101***	0.123***
	(0.027)	(0.027)	(0.021)	(0.022)	(0.027)	(0.031)	(0.027)	(0.031)
	[0.903]	[0.961]	[0.070]	[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	0.074	0.050	0.070	0.050
Effective Obs. Left	88	94	138	94	141	94	134	94
Effective Obs. Right	53	58	81	58	81	58	79	58
Control Mean	0.011	0.009	0.008	0.011	0.017	0.009	0.021	0.009
Observations	1954	1954	1954	1954	1954	1954	1954	1954

Notes: The Table presents results from local linear regressions specified in equation 6, 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*. 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 II.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 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

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

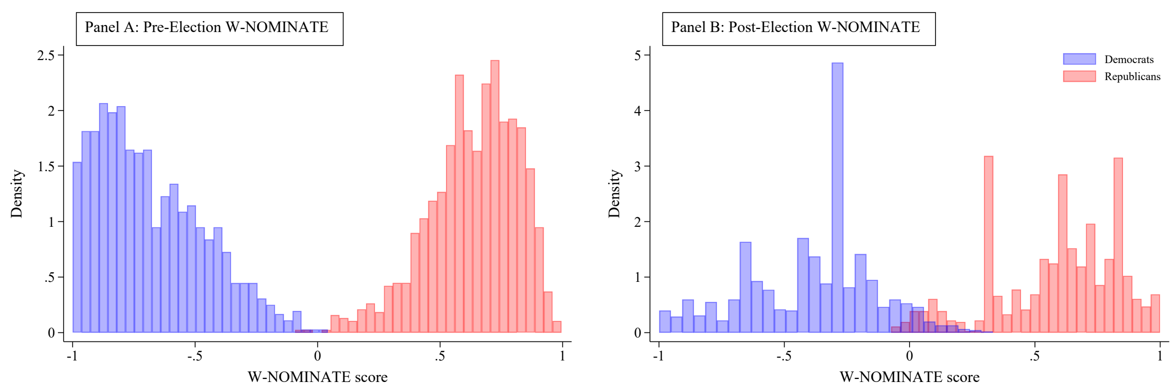
Elections have a duplicate purpose in representative democracies. On the one hand, recurrent elections allow voters to replace badly performing politicians with better types. On the other, the threat of being unseated is thought to constrain incumbents' policy choices to align with voters' interests. 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 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 20^{th} Amendment to the U.S. Constitution, which ended the era of regularly occurring lame-duck sessions in 1933.

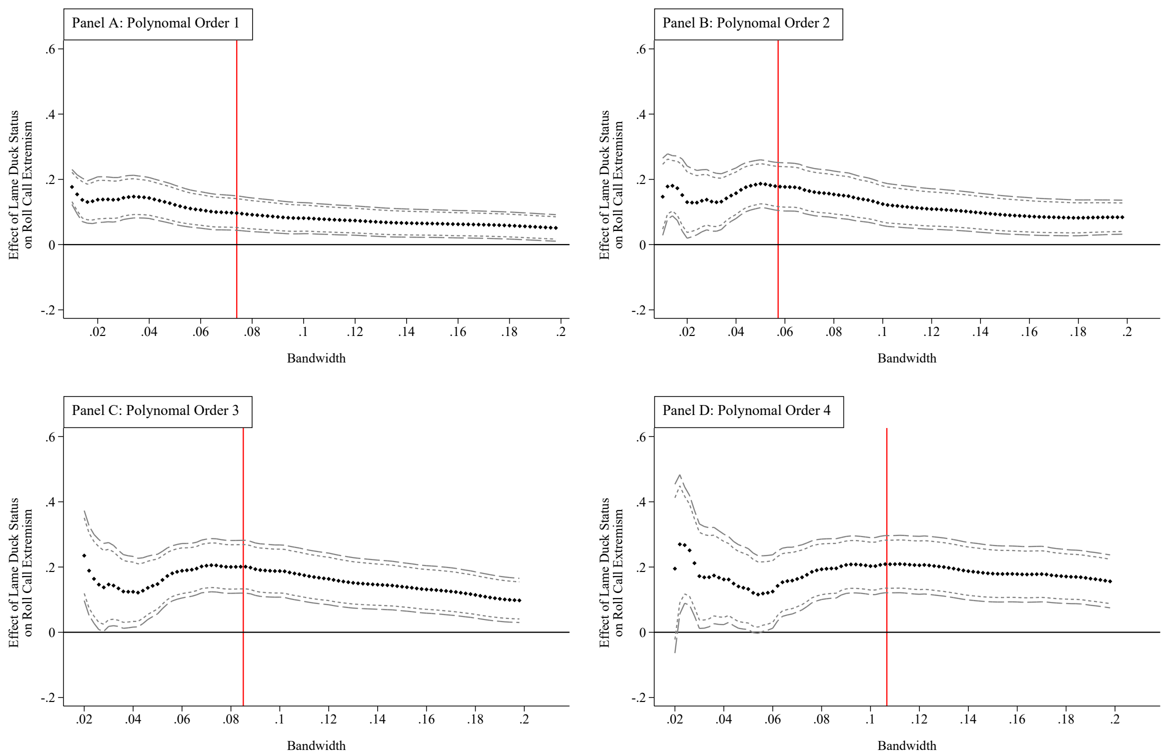
Appendix B: Additional Figures and Tables on Lame Ducks and Ideological Shriking

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



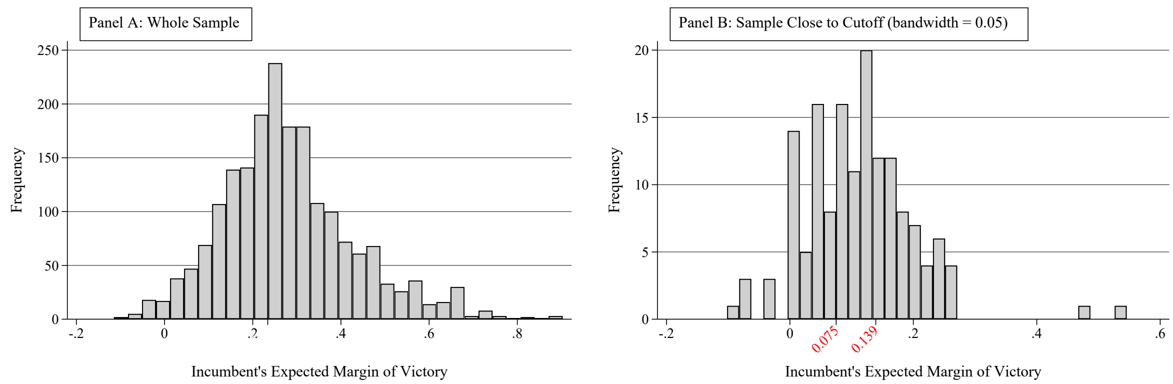
Notes: The Figure presents the sample distributions of roll-call voting positions in regular sessions (Panel A) and post-electoral lame-duck sessions (Panel B). Positions are estimated by extracting the first dimension of W-NOMINATE scores, estimated separately by *congress* $\times$ *session* using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

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



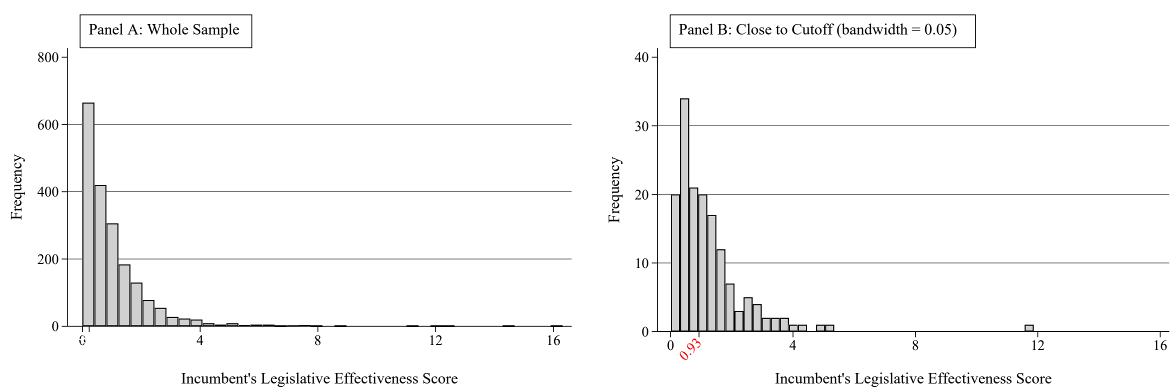
Notes: The Figure plots discontinuity estimates (black dots) for the effect of lame duck status the change in House incumbents' *Roll Call Extremism* from the regular sessions before general elections to the lame duck session after elections for different bandwidths and polynomials. Bandwidths range from 0.01 to 0.2 in local linear (Panel A) and local quadratic (Panel B) specifications of equation 6, respectively from 0.02 to 0.2 for cubic and quartic specifications (Panels C and D). All regressions use triangular kernel weights and include *party* $\times$ *congress* fixed effects. 95% (dashed grey lines) and 90% (dotted grey lines) confidence intervals account for clustering House representatives.

FIGURE B.3: DISTRIBUTION OF INCUMBENTS' EXPECTED MARGIN OF VICTORY



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

FIGURE B.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 II.5

TABLE B.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)
	0.097^{***}	0.096^{***}	0.178^{***}	0.154^{***}	0.201^{***}	0.169^{***}	0.209^{***}	0.173^{***}
	(0.027)	(0.020)	(0.037)	(0.026)	(0.041)	(0.032)	(0.045)	(0.036)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	0.074	0.074	0.057	0.057	0.085	0.085	0.107	0.107
Effective Observations	222	218	181	178	250	246	320	314
PANEL B: UNIFORM KERNEL								
	0.091^{***}	0.089^{***}	0.179^{***}	0.158^{***}	0.228^{***}	0.187^{***}	0.227^{***}	0.197^{***}
	(0.030)	(0.026)	(0.040)	(0.031)	(0.049)	(0.042)	(0.049)	(0.043)
	[0.004]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	0.057	0.057	0.061	0.061	0.070	0.070	0.089	0.089
Effective Observations	181	178	191	188	213	209	266	262
PANEL C: EPANECHNIKOV KERNEL								
	0.090^{***}	0.092^{***}	0.188^{***}	0.164^{***}	0.206^{***}	0.174^{***}	0.207^{***}	0.168^{***}
	(0.027)	(0.022)	(0.039)	(0.029)	(0.043)	(0.035)	(0.047)	(0.040)
	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	0.074	0.074	0.054	0.054	0.080	0.080	0.101	0.101
Effective Observations	222	218	173	170	239	235	301	295
Observations	1954	1923	1954	1923	1954	1923	1954	1923
Party $\times$ Congress FE	Y	Y	Y	Y	Y	Y	Y	Y
Pre-Election Outcome	N	Y	N	Y	N	Y	N	Y
Covariates	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents results from local polynomial regressions, probing robustness of the main results reported in Table II.3 to including higher polynomial orders of the assignment variable (columns) and alternative kernel weights (panels). All other notes as under Table II.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.

Chapter III
Strategic Policy Responsiveness to Opponent Platforms: Evidence from U.S. House Incumbents Running Against Moderate or Extremist Challengers

One of the principal reasons why opposition and political competition are essential to democratic politics is that they provide the mechanism through which democratic leaders are held to account.

— Ian Shapiro (2012), The Moral Foundations of Politics

1 Introduction

Electoral competition between candidates representing diverse political orientations is at the core of representative democracy, and its defining criterion for many economic and political theorists (e.g., Schumpeter, 1942; Downs, 1957b; Dahl, 1971; Sartori, 1976). While most of the literature agrees on the central role of electoral competition in determining public policy in representative democracies, there are two fundamentally opposing views on the mechanism by which elections shape public policy and the role played by non-incumbent candidates. According to the perspective embodied in citizen-candidate models, the role of elections is limited to *political selection*: electoral turnover alters the composition of government and consequently implemented policy. On the other hand, the Downsian paradigm, in which candidates interdependently choose their policy platform, emphasizes officeholders' *electoral incentives* to strategically adjust enacted policies in response to the opponent's platform, creating the possibility for challengers to influence policy outcomes without winning the election.

In citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997), the scope for policy adjustment is limited because candidates cannot credibly commit to a policy platform distinct from their ideal. A central prediction of the citizen-candidate framework thus is *incumbent policy persistence*: politicians cannot strategically adjust their position, and once in office they implement their preferred policy throughout their term in office. Incumbent policy persistence remains an equilibrium outcome in dynamic citizen-candidate settings where voters observe incumbents' past actions in office and incumbents can build reputations by adopting policies more moderate than their ideal. Despite this important difference with respect to static formulations of the citizen-candidate model, they share its main prediction: once in office, incumbents maintain their policy position as voters attribute any deviation to extremist types which then are elected out of office (e.g., Duggan, 2000; Bernhardt et al., 2011; Van Weelden, 2013). In this class of models, the challenger is simply a passive replacement for the incumbent, whose only decision, if any, is on whether to enter electoral competition or not. As incumbents are irresponsive to electoral incentives, challengers cannot affect public policy unless they succeed the incumbent so as to implement their own preferred policy.

In sharp contrast to this view stands the Downsian tradition, where candidates can credibly commit to policy platforms, and equilibrium policy is determined by candidates strategically adjusting their position to voter preferences and to each other. The canonical Downs-Hotelling two-party model with full voter turnout (Hotelling, 1929; Downs, 1957b) predicts vote-maximizing candidates to position themselves close to each other, and close to the median voter. From a decision-theoretic viewpoint, an incumbent increases her chances for re-election by moving closer to her opponent. From a game-theoretic perspective, both candidates converge to the median voter's ideal and implement an identical policy. In reality, full convergence of policy platforms may not obtain if repositioning is costly, for instance, because candidates constrained by past positions taken in office or in prior elections and flip-flopping to the center undermines their credibility in the eyes of the electorate (Bernhardt and Ingerman, 1985; Enelow and Munger, 1993), or because candidates with character incur disutility when offering a platform that betrays their ideal (Kartik and McAfee, 2007). A more realistic interpretation of the Downsian convergence mechanism for the empirically relevant case where candidates inherit locations on opposite sides of the median, is that electoral competition among strategic candidates exerts pressure to adopt moderate policies. In particular, when one candidate approaches the median from one side, it intensifies the pressure on the opponent candidate to shift their position toward the median as well. On the other hand, when one candidate moves away from the median toward a polar position, the other candidate may parlay the resulting gain in electoral strength to a position closer to her ideal by moving in the opposite direction. Policy platforms thus emerge as *strategic substitutes*. Non-incumbent challengers can expect to affect public policy as incumbents are going to commit to a platform adjusted in the direction opposite of their rival's platform shift.

While the prediction that candidates strategically adapt their platform to each other's is unequivocal in models with credible commitment, the prediction of strategic substitutability is not. For example, policy-motivated candidates, who care not only about their own platform as in Kartik and McAfee (2007) but about the expected policy outcome including the policy the opponent would implement conditional on winning (Wittman, 1983; Calvert, 1985), may be more willing to compromise against a more extreme opponent. With policy motivation, strategic complementarity of policy platforms arises because risk-averse candidates are willing to trade some of the policy utility they would obtain conditional on winning against votes by moving to the center in order to prevent victory of the extremist opponent who would implement policy far from the ideal of the compromising candidate. Strategic complementarity can also arise with candidates who face a trade-off between persuading swing voters at the center and mobilizing their core supporters at the extremes. While moderate platforms appeal to swing voters, moving too close to the opponent demobilizes core supporters who may refuse to turn out and abstain from voting due to indifference or alienation (Adams and Merrill, 2003; Bierbrauer et al., 2022), vote for third-party candidates (Palfrey, 1984; Weber, 1992; Callander and Wilson, 2007), or deny active and financial contributions to the incumbent's campaign (Aldrich, 1983). This trade-off creates an incentive to differentiate the platform when the opponent's is too similar, and to take moderate positions if the opponent abandons swing voters on the middle ground. Finally, strategic complementarity can occur due to chase-and-evade incentives when one candidate has a non-policy "valence" advantage like quality or competence. The valence-advantaged candidate then has an interest in mimicking the weaker candidate's platform to deemphasize differences on the policy dimension and intensify the salience of the valence dimension on which he is advantaged, whereas the weak candidate would therefore seek to evade the strong by hiding in the extremes of the policy space (e.g.,Aragonès and Palfrey, 2002; Aragonès and Xefteris, 2012).

Theoretical accounts yield markedly different predictions on whether and how candidates adjust their policy platform to each other and on the ability of non-incumbent candidates to influence public policy. This paper answers these questions empirically, shedding light on the crucial and hitherto understudied role of opposition candidates in ensuring responsive representation. Understanding whether or not incumbents strategically adjust policy to challengers is important beyond assessing the empirical relevance of the theoretical perspectives embodied in two large classes of formal models. The central notion of the Downsian paradigm that electoral competition constrains incumbents' policy is a key tenet of democratic accountability. In practice, the question of whether incumbents respond to electoral incentives has significant implications for constitutional design, particularly regarding institutions aimed at enhancing representation via selection at the expense of accountability (e.g., proportional elections, term limits). Similarly, the *direction* of policy adjustment is not only of interest to spatial theories of voting but more broadly speaks to a frequently raised concern that extremism on one side of the political spectrum breeds extremism on the other (e.g., Iyengar et al., 2012; Stone, 2020). If there is a feedback loop from policy divergence to partisanship, elite and voter polarization can be self-reinforcing and self-perpetuating over time (e.g., Callander and Carbajal, 2022; Diermeier and Li, 2023).

This paper provides evidence that, prior to general elections, U.S. House incumbents commit to new policy platforms by strategically adjusting their roll-call voting behavior in the direction to their opponent's position. Identifying the effect of opponents' on incumbent positions is challenging because candidate positions are jointly determined by preferences of the electorate and, if platform choice is strategic, interdependent. In addition, such analysis requires information on policy positions of non-incumbent candidates whose political orientation cannot be inferred from roll-call voting (Poole and Rosenthal, 1997) or House floor speeches (Gentzkow et al., 2019). To overcome this challenge, I follow an approach pioneered by Hall (2015) and use *pre-primary* transaction-level campaign finance data (1980-2018) to estimate the position of primary candidates on a liberal-conservative scale. Based on this estimate, I classify as extremist the more liberal (conservative) of the top-two candidates in Democratic (Republican) primaries, and the other primary candidate as moderate. Focusing on incumbents whose opponent party holds a competitive primary election with at least two candidates running for nomination, I use a regression discontinuity design to exploit as good as random assignment of incumbents to extremist or moderate challengers generated by close primaries. I thus compare the post-primary roll-call voting behavior of incumbents facing an extremist opponent in the general election to otherwise identical incumbents' post-primary voting record who defend their seat against a moderate challenger.

Crucially, I only consider post-primary roll calls held *prior* to general elections to isolate incumbents' differential response to extremist and moderate challengers from endogenous sample selection. Since incumbents facing an extremist challenger are more likely to win their re-election bid and post-electoral roll call voting is only observable for election winners, it is not an option to use post-electoral roll calls to gauge incumbent's response. On the other hand, pre-election DW-NOMINATE scores (Poole and Rosenthal, 1997) that locate incumbent's roll-call voting on a liberal-conservative scale (from -1 indicating very liberal to +1 indicating very conservative) are fixed over a representative's term in office, and therefore not specific to the post-primary period but largely based on pre-primary roll-calls. To overcome this difficulty, I estimate each incumbent's position — specific to the period between the opponent party's primary and the general election — as the agreement-rate weighted average of DW-NOMINATE scores of all other House members serving in the same Congress, whereby higher (lower) values of this estimate indicate increasing *roll call extremism* for Republicans (Democrats). As an alternative and more directly interpretable outcome, I use incumbents' *party loyalty* in voting on divisive issues, i.e., roll calls on which the majority of Republicans disagrees with the majority of Democrats. Defying the party line and taking sides with the opponent party is, arguably, a strong signal of moderation.

Results for both outcomes tell a qualitatively consistent story. I find that incumbents facing an extremist challenger alter their roll-call voting record to commit to a more moderate position compared to incumbents running against a moderate challenger. Specifically, an extremist challenger causes a decrease in the incumbent's DW-NOMINATE-based estimate of *roll call extremism* by a 0.25 standard deviation. A simple back-of-the-envelope calculation that maps my estimate of *roll call extremism* back to actual DW-NOMINATE scores in the 117^{th} Congress (2021-2023) indicates that the implied shift approximately corresponds to half the within-party inter-quartile range of DW-NOMINATE scores in the U.S House of Representatives, or – alternatively – to the average distance between representatives and their own party's median. Concurrently, compared to incumbents running against a moderate challenger, incumbents facing an extremist are 6–8%

more likely to deviate from party line and to vote in line with the majority of the opponent party. Consistent with strategic complementarity of policy platforms, graphical evidence indicates that the differential impact of extremist (compared to moderate) challengers results from a dual effect: i) incumbents taking more moderate positions when facing an extremist, and ii) incumbents differentiating against moderate challengers by taking more extreme positions.

In the second part of the paper, I provide evidence that incumbents' adjustment to challengers is indeed part of an electoral strategy. Incumbents adjust their policy position in response to challengers only if they run for re-election, while retiring incumbents do not react to the nomination outcome of the opponent party's primary. This excludes non-strategic adaption by benevolent (and boundedly rational) incumbents who might misperceive the outcome of the opponent party's toss-up primary as signaling a shift in voter preferences. Consistent with incumbents responding to electoral incentives, I find that policy adjustment to opponents is confined to incumbents defending marginal seats, i.e., to incumbents who are electorally vulnerable and to districts where electoral returns to adjustment are substantial due to a significant portion of swing voters that could be swayed by policy shifts.

The last set of results investigates the possible mechanisms behind strategic complementarity of policy platforms. The observed pattern that incumbents take more moderate positions against extremists while differentiating their position from moderate challengers' suggests that politicians do consider not only the votes they could win by moderating but also the voters they could lose when offering a platform too close to the opponent's. Consistent with incumbents facing a trade-off between persuading swing voters at the center and mobilizing their core supporters, I find that the incumbent's reaction is stronger when the more moderate of the two potential challengers offers a platform close to the incumbent's. That is, the magnitude of the incumbent's response to a shift in their challenger's position is largest precisely in the case when incentives to differentiate from the moderate challenger are strongest, and the electoral returns to moderation against extremists are highest because the incumbent can attract middle-ground voters that have been abandoned by the opponent party nominating an extremist. Preliminary evidence suggests that the entry of third candidates conditions incumbents' differential response to moderate and extremist challengers. I also consider, but ultimately dismiss, policy motivation and valence-induced chase-and-evade incentives as alternative mechanisms. Importantly, I exclude that results are driven by a valence differential between moderate and extremist challengers, providing evidence that the moderates' valence advantage over extremists, if anything, biases estimates in the opposite direction of my main findings, which thus represent lower bounds on incumbents' reaction *to their opponent's platform*.

The findings of this paper directly speak to a longstanding empirical literature in economics and political science on candidate positioning. A large body of observational studies has investigated candidate convergence and policy responsiveness to voter preferences in the United States, generally reporting substantial divergence of candidate positions that are decreasing in district competitiveness, and small positive correlations between candidate positions and voter preferences (e.g., Ansolabehere et al., 2001; Burden, 2004). While consistent with the Downsian view of candidates adjusting positions strategically, this pattern is also consistent with strategic entry of citizen-candidates depending on the distribution of voter preferences.[1] Focusing on within-party changes between elections, Adams and Somer-Topcu (2009) show that positional shifts in party manifestos positively correlate with past shifts in rival party's manifestos. However, with imperfect controls for public opinion, it is difficult to disentangle strategic adjustment to rival parties from shifts in voter preferences or changes in the economic environment. Le Pennec (2023) demonstrates that candidates in French two-round elections adjust their campaign messages in the second round, although not by taking more moderate policy positions but by advertising non-policy issues that deemphasize the policy dimension. Consistent with policy moderation, Burden (2001) shows that House representatives in the 102^{nd} Congress (1990-1992) exhibited a more moderate roll call voting record after primary elections than before, which may reflect agenda setting by party leadership rather than individual candidates strategically adjusting to their opponent's position.

Causal evidence on strategic position-taking is limited to party manifestos and campaign communication. In a cross-country study, Abou-Chadi and Krause (2020) demonstrate that past victories of right-wing parties lead mainstream parties to accommodate anti-immigrant positions in their party manifestos for subsequent elections. Most closely related to this paper is recent work by Di Tella et al. (2023) who use text analysis of French candidate manifestos and U.S. candidate webpages to locate candidates' campaign discourse along three dimensions: ideology, complexity, and topics discussed. Employing a regression discontinuity strategy similar to the one used in this paper, they test whether candidates strategically adjust their campaign communication to the position of the competitor who won the primary (or first-round election in France) by a narrow margin. While they find evidence for convergence in ideology and topics among French candidates, the convergence of U.S. candidates' campaign platforms seems limited to overall text similarity and complexity, with a small and statistically insignificant reduction in the ideological distance between candidates. As Di Tella et al. (2023) note, the finding of non-convergence of policy positions in the U.S. could be attributable either to the absence of positional adjustment in a highly polarized context, or due to the inability of their design, which focuses on the *distance* between candidate platforms, to detect positional adjustment in the same direction. My finding that policy platforms are strategic complements supports the latter interpretation. When candidates adjust their position in the same direction as their competitor, there is little scope for a change in the distance between their platforms. More generally, — using a different measure of opponent candidates' ideological position and incumbents' actual policy choices as an alternative outcome — this paper confirms the conclusion in Di Tella et al.'s (2023) that candidates strategically adjust

[1]The same applies to Catalinac (2018) who uses Japanese campaign manifestos to show that candidates in single-member districts tend to diverge less than in multi-member districts.

their policy position to the opponent's, albeit with an important qualifier that they do not necessarily aim at getting closer to each other. Focusing on incumbents' voting records, the conclusions of this paper are not susceptible to objections that text similarity of campaign communication may be a natural consequence of candidates engaging with each other's positions rather than reflecting positional changes in response to the opponent's platform. However, the focus on incumbents' actual policy choices is not of mere methodological convenience but adds a substantively novel result. It is not obvious that advertised platforms translate into actual policy. After all, the view that campaign promises are uninformative cheap talk is a key tenet of citizen candidate models.

Providing the first credibly causal effect of challenger positions on House incumbents' voting record, this paper further contributes to a growing empirical literature on the determinants of legislator behavior. Existing work has identified the influence of legislators' own ideology (Levitt, 1996), their party leadership (Canen et al., 2020), their core constituency (Mian et al., 2010), and even their daughters (Washington, 2008) and seat-neighbours (Harmon et al., 2019) on legislators' voting record. With opponent party challengers, this paper adds another key player in the political game and hitherto disregarded driver of incumbent behavior. The most closely related work on incumbent behavior is Lee et al. (2004) who also study the empirical relevance of the Downsian paradigm in the U.S. House of Representatives. Using a regression discontinuity design, they exploit that a close victory of a Democratic candidate in the previous election generates electoral strength for the Democratic incumbent in the next election due to the incumbency advantage. They interpret the finding that incumbents do not change their position after the next election (in which they obtained a higher vote share due to the incumbency advantage) as evidence that electoral competition does not constrain incumbents' policy choices consistent with politicians' inability to commit to new policy platforms.[2] This paper's finding that U.S. House incumbents respond to electoral incentives sharply contrasts with this conclusion. Incumbents do commit to new policy platforms by strategically adjusting their voting behavior prior to elections, consistent with the Downsian paradigm and contrary to predictions of citizen-candidate models. To Mian et al.'s (2010) finding that U.S. House incumbents are responsive to their core constituency, this paper amends evidence that primary elections make incumbents accountable to core supporters of the *opponent* party, whose nomination decision determines the challenger and thereby affects incumbent's policy.

My findings also connect to an emergent literature studying the effects of candidate entry in two-round elections. Most closely related is Hall (2015) who demonstrates that extremists nominated in a toss-up primary get punished by general-election voters. Extremists get fewer votes and are less likely to win general elections for the U.S. House than moderate nominees, an effect largely attributable to extremists activating turnout of the opponent party more than the turnout of their base (Hall and Thompson, 2018). Complementing these analyses of the political demand side, this

[2] Another interpretation of this finding is that forward-looking incumbents with rational expectations strategically adjust their position after the first election because they anticipate their electoral advantage in the next election.

paper examines supply-side responses to extremist nominees. While Hall (2015) shows that extremists tilt the district's post-election roll-call voting record in the direction of the opponent party — i.e., becoming more liberal when Republicans nominate an extremist and more conservative when Democrats nominate an extremist — which is due to a selection effect because extremists are less likely to win, I discover an accountability effect going in the opposite direction. Sitting incumbents moderate their pre-election roll call voting record differentially more when running against an extremist. This moderating effect provides one possible answer to the puzzling question of why primary elections do not increase (Hirano et al., 2010) and even *reduce* (Cintolesi, 2022) polarization of House members' voting record despite strong incentives to pander to their party's primary electorate. Primary voters' ability to pull the opponent party candidate's policy toward their ideal by nominating an extremist also hints at the existence of instrumental benefits to voting for outsider candidates with little chance of winning, suggesting that costs associated with expressive voting may be smaller than previously thought (Pons and Tricaud, 2018).

More generally, this paper relates to research studying campaigning and electoral strategies including the selection of candidates (Dal Bó et al., 2017), their decision to drop out (Lee, 2008; Anagol and Fujiwara, 2016), and to collude with candidates of similar orientations (Granzier et al., 2023). A large body of empirical work has documented persuasive and mobilizing effects of candidates communicating their positions by door-to-door canvassing (Gerber and Green, 2000; Pons, 2018), direct mailings and phone calls (Kendall et al., 2015), television advertisement (Spenkuch and Toniatti, 2018b), or social media (Petrova et al., 2021). This paper provides evidence that, for incumbent candidates, actual policymaking is part of an electoral strategy. While ample evidence that legislators' voting record is consequential for re-election strongly suggests that strategic incumbents *should* consider electoral ramifications of voting decisions (Canes-Wrone et al., 2002; Ansolabehere and Jones, 2010; Carson et al., 2010; Ansolabehere and Kuriwaki, 2022), I show that legislators *do* respond to electoral incentives and adjust their voting record accordingly. This paper thus also speaks to concerns that incumbent politicians strategically manipulate policy to retain office (e.g., Rogoff and Sibert, 1988; Levitt and Snyder, 1997). Given field-experimental evidence that voters are more likely to vote for candidates whose promises align with observed decisions (Cruz et al., 2023), incumbents' ability to credibly commit to new policy positions tailored to opponents by altering their voting record may constitute a source of the incumbency advantage that has yet been overlooked in the literature.

The remainder of the paper is structured as follows: Section 2 describes the setting, the data, and details the procedures for estimating candidate and incumbent positions. Section 3 presents the identification strategy and discusses its validity. Section 4 reports the main results and assesses the robustness thereof. Section 5 conducts heterogeneity analyses and discusses mechanisms behind the main findings. Section 6 concludes.

2 Empirical Setting and Data

2.1 U.S. House Elections

The House of Representatives is the lower chamber of the United States Congress. Its 435 members are elected in single-member districts by plurality or majority rule.[3] General elections are conducted biennially at the beginning of November during even-numbered years in a two-party system dominated by the liberal Democratic party and the conservative Republican party. With limited prospects for electoral success of third-party candidates, the outcome of general elections is ultimately decided by the competition between a Democratic and a Republican candidate.

Each of the two predominant parties selects its nominee in partisan primary elections held 2-9 months in advance of the general election. The laws governing the timing and conduct of primary elections vary from state to state (see, e.g., Boatright, 2014). With a few exceptions,[4] all states require each party to hold separate primaries where voters choose the party's nominee by plurality or majority rule.[5] While in some states only registered party members are allowed to participate in the selection of their party's nominee (closed primaries), in others also unaffiliated voters (semi-closed primaries) or all voters (open primaries) are allowed to choose in which of the two parties' nomination process they participate, provided that they choose the same party for all elected offices at a given primary election day. In practice, however, the primary electorate tends to be composed of each party's core supporters regardless of formal regulations on the openness of the nomination process to non-partisan voters (see e.g., Hill, 2015; Sides et al., 2020).

2.2 Primary Elections Data

Data on primary elections are obtained from the *primary timing project* (Boatright et al., 2019). The dataset covers House primary election results for the period from 1978 to 2018 and provides context information on the date of the primary, district characteristics, and incumbency status of the contested seat. Focusing on competitive primaries with at least two candidates running for nomination, I use the vote shares of the top-two primary candidates to construct the assignment

[3] In most states, the winning candidate in general elections is determined by simple plurality. Exceptions are Georgia and Mississippi where a runoff is held if none of the candidates receives a majority in the first round. Maine and Alaska switched from simple plurality to ranked-choice voting in 2018 and 2020, respectively. My sample does not include any case with ranked choice voting as adopted only at the end of my sampling period (1982-2018) and primary elections for the two seats in Maine were not competitive in 2018, i.e., a single candidate ran for each party's nomination.

[4] Exceptions include non-partisan primaries in the states of Washington, Alaska, and California (from 2012 onward) where all candidates of both parties run in the same primary and the top candidates advance to the general election, regardless of their party affiliation. In these so-called "jungle primaries" it can happen that general elections candidates share the same party affiliation. I therefore exclude these observations from the sample. I also exclude Louisiana which, strictly speaking, does not hold primary elections and instead uses a top-two runoff system where all candidates regardless of party affiliation appear on the general election ballot with a runoff being held in case none of the candidates obtains a majority.

[5] In states with partisan primaries decided by majority rule, a runoff among the top-two candidates is triggered if no candidate reaches the majority of votes.

variable for the RD analysis. Precise information on primary election dates allows to estimate primary candidates' ideological positions relying exclusively on pre-primary campaign receipts avoiding endogeneity of campaign donations to primary election results, and to calculate outcome variables on incumbents' roll-call voting behavior separately before and after the opponent party's primary election.[6] Socio-demographic characteristics including lagged presidential vote shares are used for balancing tests and heterogeneity analyses depending on district competitiveness. To identify seats occupied by an incumbent of the opponent party who runs for re-election, I rely on Boatright et al.'s (2019) distinction between challenger primaries and open seat primaries. While my main analysis focuses on challenger primaries, i.e. primaries nominating a challenger to the opponent party's incumbent who reruns to defend her seat, I use "open seat primaries" to construct an auxiliary sample of primaries nominating a candidate for a seat currently occupied by an incumbent of the opponent party who does *not* seek re-election.

I supplement the data with hand-collected information on gender, race, and prior office experience of the top-two candidates in challenger primaries from the 1996 election cycle onward.[7] Finally, I add an estimate of the top-two candidates' policy positions on the liberal-conservative dimension for both challenger and open seat primaries, as described below.

2.3 Estimating Primary Candidates' Policy Position from Campaign Contribution Data

Empirical research on the U.S. Congress conventionally uses DW-NOMINATE scores (Poole and Rosenthal, 1997) as an estimate of elected representatives' policy position on a liberal-conservative scale ranging from -1 (very liberal) to 1 (very conservative) on which incumbent legislators are placed according to their observed roll-call voting behavior. Corresponding estimates of the policy positions of non-incumbent candidates without a precedent roll-call voting record are harder to come by and must be inferred indirectly from candidate surveys (e.g., Ansolabehere et al., 2001; Burden, 2004), campaign communication (e.g., Catalinac, 2018; Le Pennec, 2023), or campaign contribution patterns (e.g., Bonica, 2014, 2018; Hall and Snyder, 2015). Yet, survey-based measures provide little to no coverage of primary losers. Applying text analysis to scale the content of candidates' webpages and other documents of campaign communication, as in Di Tella et al. (2023), is best suited to study how candidates shape each others' political discourse prior to election, but less appropriate for the purpose of this paper. This paper focuses on how challenger positions differentially affect incumbents' commitment to new policy positions by altering their voting behavior in terms of a DW-NOMINATE-based measure of roll-call extremism, which makes es-

[6] In the case of primary runoffs, I use the vote shares and the date of the runoff election.

[7] I restrict attention to more recent years because my data collection strategy for non-incumbent candidates relies on internet sources, combining the systematic research on searchable databases of election campaigns and politicians (`ourcampaigns.com`, `politicalgraveyard.com`, `ballotedia.org`, `votesmart.org`, `bioguide.congress.gov`) with internet-wide Google searches that often lead to newspaper articles available online or links to candidate webpages which could be accessed via the Wayback Machine (`web.archive.org`)

timates of challenger positions that are directly related to DW-NOMINATE preferable.[8] Bonica (2018) uses supervised machine learning to predict DW-NOMINATE scores for non-incumbent candidates from campaign contributions. In simplified terms, these estimates reside on the intuition that more liberal candidates would receive more funding from donors who usually donate to incumbents with a relatively liberal voting record, while candidates whose funding originates from donors tending to contribute to more conservative legislators would also be more conservative. However, these readily available estimates of candidate positions rely also on post-primary, hence potentially endogenous, campaign contributions. The use of post-primary contributions could lead to biased estimates due to misclassification of moderate and extremist primary candidates if, for example, strategic donors (e.g., access-seeking interest groups) favor primary winners such that extremist nominees appear more moderate.

I therefore follow the approach of Hall and Snyder (2015), and use exclusively pre-primary campaign contributions to bridge roll-call based DW-NOMINATE scalings from U.S. House incumbents to non-incumbent primary candidates via common donors. As other donation-based scalings, Hall-Snyder scores rely on the assumption that donors prefer donating to candidates with policy positions close to their own. While the underlying intuition is similar to computationally intensive donation-based scalings using machine learning techniques (e.g., Bonica, 2014, 2018), Hall-Snyder scores are straightforward to compute in two simple steps. First, incumbents' DW-NOMINATE scores are mapped to their donors:

$$DonorScore_{j,-k} = \frac{\sum_{i \neq k} Contribution_{ij} Nominate_i}{\sum_{i \neq k} Contribution_{ij}}$$

where donor j's score is the contribution-weighted average DW-NOMINATE of all incumbents i donor j contributed to. To avoid feedback loops, I leave out contributions to the non-incumbent candidate k whose score we intend to estimate.[9] In a second step, donor scores are mapped to non-incumbent candidates:

$$CandidateScore_k = \frac{\sum_k Contribution_{jk} DonorScore_{j,-k}}{\sum_k Contribution_{jk}} \tag{7}$$

where candidate k's score is the contribution-weighted average donor score of all donors j that contributed to k.

[8] Another concern with applying Di Tella et al.'s (2023) strategy to the present setting is that my outcome of interest is observed only for incumbent candidates. This would reduce sample size considerably compared to Di Tella et al. (2023) who, in spite of observing outcomes also for non-incumbent candidates, are already underpowered to detect statistically significant effects of extremist opponents on other candidates' political discourse adjustment.

[9] Given the small number of primary candidates with prior office experience in Congress, this restriction is redundant for most non-incumbent primary candidates. However, the condition becomes relevant for incumbents whose Hall-Snyder score I calculate for validation purposes and heterogeneity analyses.

I calculate Hall-Snyder scores using transaction-level campaign finance data for House election cycles between 1982 and 2018 from the Federal Election Commission as compiled and processed by Bonica (2021). I impose a few restrictions on the estimation procedure. First, I exclude several types of transactions that are not indicative for donor and candidate positions, including loans, refunds, transfer payments, and contributions against a candidate. Second, for each candidate k, I do not consider any transactions made after the candidates' primary election date in both steps of calculating k's candidate score. Third, I attenuate measurement error by excluding donors who contribute to fewer than 5 distinct candidates, and candidates that receive from fewer than 5 distinct donors. The threshold of 5 distinct donors and candidates reflects the trade-off between minimizing measurement error and maximizing sample size.[10] After retaining only competitive primary elections for which Hall-Snyder scores can be calculated for both top-two candidates under the aforementioned restrictions, I am able to match a total of 709 competitive primaries to seats currently held by incumbents from the opponent party, of which 490 rerun to defend their seat and 219 end their term without seeking re-election.[11]

Being weighted averages of DW-NOMINATE scores, contribution-based Hall-Snyder scores are bounded between -1 and 1 with increasing values indicating more conservative candidates. It is then straightforward to infer the more extreme of the top-two primary candidates. For Republican primaries, I define the relative extremist as the more conservative candidate with a Hall-Snyder score closer to 1 and the candidate with the lower Hall-Snyder score as moderate, whereas for Democratic primaries I classify the more liberal candidate whose Hall-Snyder score is closer to -1 as extremist and the more conservative candidate with a higher Hall-Snyder score as moderate. Formally, for every primary candidate i whose strongest primary competitor is j,

$$Extremist_{i(p)} = \begin{cases} 1 & \text{if } p = \text{Republican and } CandScore_i > CandScore_j \\ & \text{or } p = \text{Democrat and } CandScore_i < CandScore_j \\ 0 & \text{otherwise.} \end{cases}$$

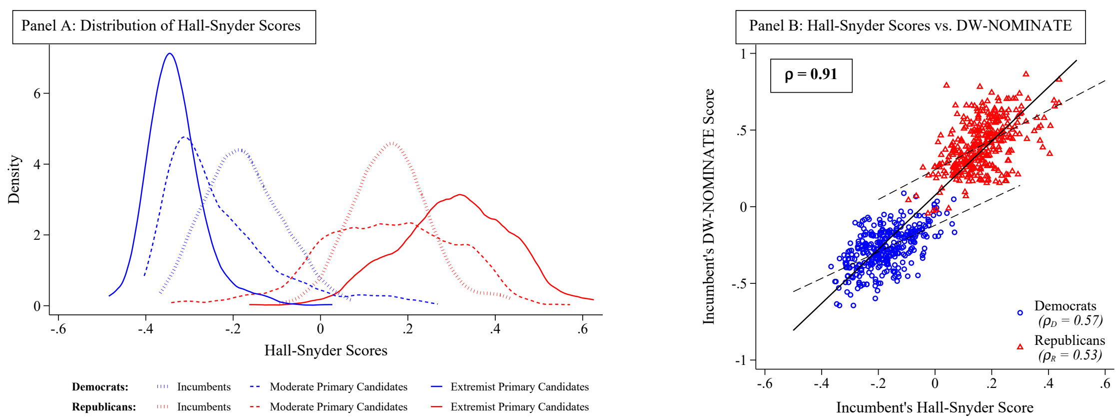
Figure III.1, Panel A presents density estimates for the sample distribution of Hall-Snyder scores by party, separately for moderate and extremist primary candidates, as well as for incumbents. As one would expect, estimated policy positions for members of the two parties generally

[10] Higher thresholds are likely to reduce measurement error by including only candidates whose position is estimated based on larger amounts of information. Lower thresholds yield a larger and less selected set of candidates with less well-funded fringe candidates more likely to be included. In contrast to Hall (2015) who considers any type of competitive primary and uses a threshold of 10, I focus on challenger primaries only, which unavoidably reduces the sample. I therefore prefer a lower threshold, which yields a sample large enough to provide statistical power for meaningful heterogeneity analysis to explore mechanisms behind the main finding. In Appendix Tables C.1 and C.2, I show that the main results, albeit somewhat less precisely estimated, remain virtually identical when using Hall-Snyder scores based on a minimum threshold of 10 or 15 distinct candidates and donors. I also address concerns that measurement error in Hall-Snyder scores drives the results by showing robustness to excluding observations where the difference in primary candidates estimated position is small (see Appendix Figures C.1 and C.2).

[11] I exclude 6 incumbents who lost their own party's primary, 6 incumbents who won their primary but dropped out before the general election mostly due to death, health issues or other exogenous shocks (e.g., scandals), and another incumbent who switched party during the congressional term.

fall on opposing sides of the center with Democrats concentrated on the liberal and Republicans on the conservative side of the political spectrum, while there is little overlap between parties. By construction, moderate primary candidates are closer to the center compared to co-partisan extremists. The within-party difference in means of Hall-Snyder scores between moderate and extremist challengers amounts to 0.15 for Republicans and 0.1 for Democrats. The average distance within primary is 0.12, which roughly corresponds to a 0.5 standard deviation shift in positions on the DW-NOMINATE scale in the 117th Congress,[12] suggesting that extremist and moderate candidates run for nomination under meaningfully differentiated platforms. Based on Hall-Snyder scores, incumbent positions are estimated to be more centrist relative to non-incumbent candidates of the same party, which is in line with related evidence that incumbents tend to adopt more moderate policy positions than challengers (see e.g., Ansolabehere et al., 2001), but also consistent with incumbents just appearing more moderate because they receive more funding from non-ideological access-seeking interest groups.

FIGURE III.1: ESTIMATED CANDIDATE POSITIONS: HALL-SNYDER SCORES FOR INCUMBENT AND NON-INCUMBENT CANDIDATES



Notes: The Figure presents Hall-Snyder scores, as defined by equation 7. Panel A plots kernel densities of Hall-Snyder scores of Democrats (in blue) and Republicans (in red), separately for incumbents (dotted lines), moderate (dashed lines) and extremist (solid lines) primary candidates. The sample includes the top-two candidates of 709 competitive challenger primaries and 705 incumbents of the opponent party.[13] Panel B plots the incumbents' Hall-Snyder score against their DW-NOMINATE score and reports the raw correlation coefficent for the whole sample of 705 incumbents (ρ), and within-party correlation coefficients for Democrats (ρ_D) and Republicans (ρ_R). Lines represent the linear bivariate regression fit by party (dashed lines) or over the whole sample (solid lines).

[12]This relation results from the following back-of-the-envelope calculation: The average distance in Hall-Snyder scores within primary is 0.115, which translates to 0.581 standard deviation in Hall-Snyder scores among incumbents in my sample. For the incumbents in my sample, a 0.581 standard deviation in DW-NOMINATE scores translates into 0.222 points on the DW-NOMINATE scale, which corresponds to a 0.48 standard deviation in the DW-NOMINATE scores of House representatives in the 117th Congress.

[13]I miss Hall-Snyder scores for 4 incumbents due to missing campaign finance data in Bonica (2021).

While primary candidates' Hall-Snyder scores are central to my identification strategy, incumbents' Hall-Snyder scores are useful to validate donation-based estimates by comparing them with observed DW-NOMINATE scores based on the actual voting behavior of elected representatives. Figure III.1, Panel B plots Hall-Snyder scores against DW-NOMINATE scores for all incumbents in my sample. The graph reveals a strongly positive relationship between DW-NOMINATE and Hall-Snyder scores with a correlation coefficient $\rho = 0.91$, suggesting that Hall-Snyder scores have high accuracy in predicting the policy positions candidates would take in House roll calls if they were in office. Also, within-party correlations ($\rho_D = 0.57$ for Democrats, $\rho_R = 0.52$ for Republicans) in my sample are highly similar to those in Hall and Snyder (2015). Clearly, Hall-Snyder scores estimate candidate positions with some error. Yet, the relatively strong correlations prevent systematic misclassification of extremists as moderates and vice-versa. Thus, while measurement error is highly unlikely to be systematic, remaining misclassifications induce classical measurement error leading to attenuation bias. I will therefore interpret my RD estimates as lower bounds on the true effect. This conclusion is supported by extensive robustness checks excluding observations where primary candidates' positions are estimated to be less distant from each other, and hence more susceptible to misclassification (see Appendix Figures C.1 and C.2).

2.4 Estimating House Incumbents' Policy Position from Roll Call Data

While DW-NOMINATE scores are a natural starting point to scale roll call voting positions on the liberal-conservative dimension, it is well known that DW-NOMINATE scores are based on a static model, and, hence, time-invariant over a legislator's term. Yet, to address the question of whether and how incumbents alter their policy position in response to their challengers, I require an estimate specific to the period between primary and general elections. For this purpose, I construct a simple measure of incumbents' roll call extremism specific to the post-primary period, which is based on DW-NOMINATE and its underlying idea that legislators with a similar voting record should be located close to each other on the liberal-conservative scale.[14] Using individual roll-call voting records of U.S. House representatives between 1982 and 2018 from the voteview.com database (Lewis et al., 2022), I proceed in two steps. First, I calculate each incumbent i's *indirect DW-NOMINATE* as the agreement-rate weighted average of other incumbents $j \neq i$' s DW-NOMINATE:

$$\text{Indirect DW-NOMINATE}_i = \frac{\sum_{j \neq i} \alpha_{ij} \text{DW-NOMINATE}_j}{\sum_{j \neq i} \alpha_{ij}} \tag{8}$$

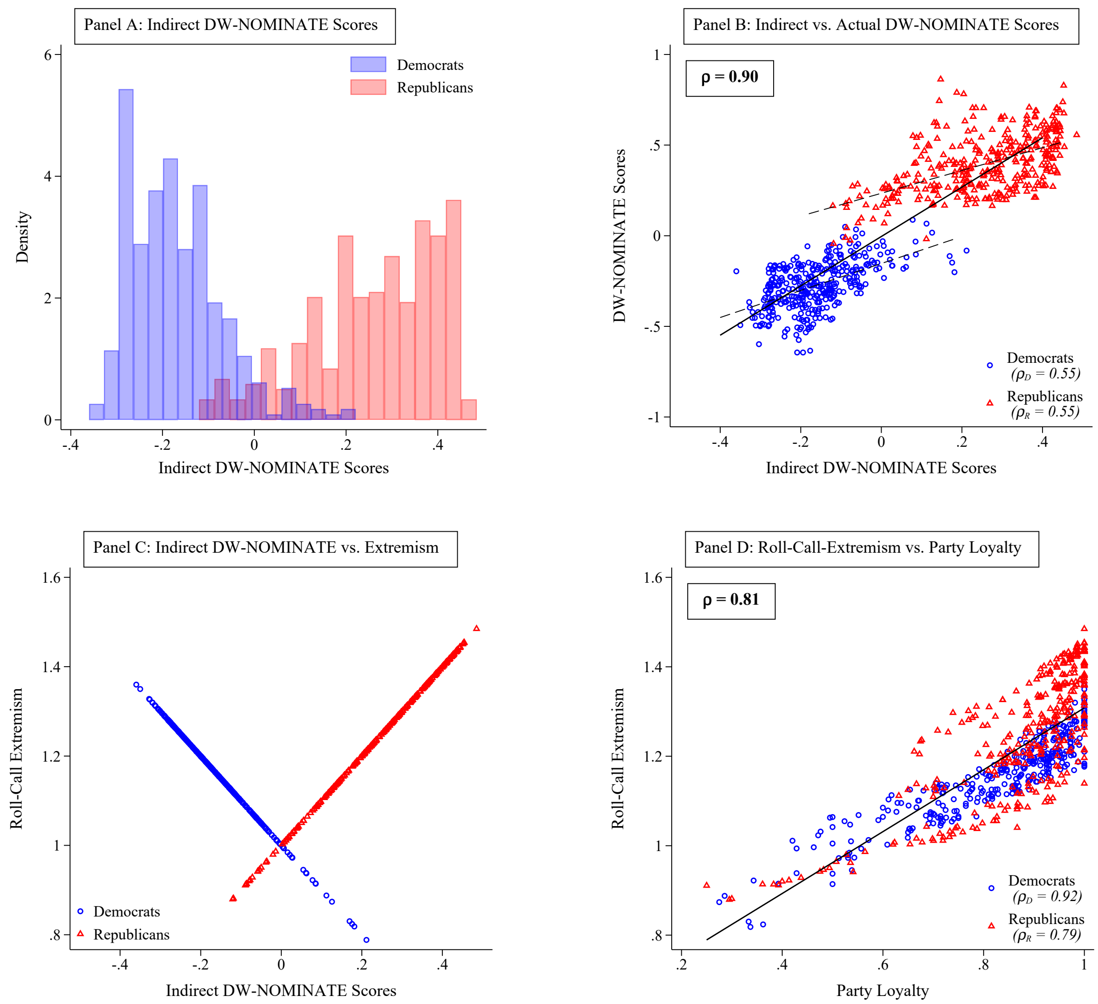
[14] One could be tempted to using incumbents' Hall-Snyder scores as an estimate of their policy position. Yet, changes in the incumbents' Hall-Snyder score in response to the nomination of an extremist challenger likely reflect a recomposition of their donor pool rather than an adjustment of their policy position. For example, even an incumbent who does not adjust her policy position will likely receive a greater amount of campaign contributions from moderate donors when her opposing party nominates an extremist challenger.

where α_{ij} is the agreement rate between Representative i and j, i.e., the share of roll calls for which both i and j vote for the same side.[15] For each incumbent in my sample, I calculate the *indirect DW-NOMINATE* separately for the periods before and after the opponent party's primary, excluding uninformative lopsided votes on which more than 90% of all House members agree. The pre-primary period includes the incumbent's roll calls in the current congressional term held prior to the opponent party's primary election, whereas the post-primary estimate includes all roll call votes held after the opponent party's primary election and no later than 120 days prior to the general election.[16]

[15]Formally $\alpha_{ij} = \frac{1}{n}\sum_{r=1}^{n} I(v_{ir} = v_{jr})$, where I is a dummy variable = 1 if $v_{ir} = v_{jr}$, and v_{kr} is a dummy variable taking the value 1 if representative $k \in \{i, j\}$ votes "yea", and 0 if k votes "nay" in roll call r.

[16]I focus on the last 4 months leading up to the general election when the election campaign enters its crucial phase, i.e., when voters and the media are likely attentive to incumbents' voting behavior and their announcements of policy platforms. The threshold of 120 days also represents a compromise between the accuracy of estimates for individual incumbents and the comparability of estimates between incumbents. Using longer timeframes allows the inclusion of more post-primary roll calls for incumbents whose opponent's primary election takes place early in the election year. This improves the accuracy of their estimated indirect DW-NOMINATE score while hampering the comparability with incumbents whose post-primary votes are concentrated later in the electoral cycle. On the other hand, shorter timeframes enhance comparability among incumbents at the cost of imprecise estimates based on very few votes for all incumbents. Reassuringly, the choice of the 120-day threshold is inconsequential for my results, with point estimates highly similar for a range of alternative thresholds ranging from 273 days (including all post-primary votes) to 45 days (imposing the same timeframe for all incumbents) prior to general elections (see Appendix Tables C.3 and C.4).

FIGURE III.2: ESTIMATED INCUMBENT POSITIONS: INDIRECT DW-NOMINATE, ROLL-CALL EXTREMISM, AND PARTY LOYALTY



Notes: The Figure presents estimates of incumbents' post-primary roll-call voting position for Democrats (blue) and Republicans (red). Panel A depicts the sample distribution of *indirect DW-NOMINATE* scores as defined by equation 8. Panel B plots *indirect DW-NOMINATE* scores against actual DW-NOMINATE scores reporting correlation coefficients (ρ) with bivariate linear regression fits over all incumbents (solid line) and by party (dashed lines). Panel C relates *indirect DW-NOMINATE* scores to *roll call extremism* defined in equation 9. Panel D shows the correlation between *roll call extremism* and *party loyalty*, measured as the percent of divisive roll-call votes cast in party line. The sample consists of 709 incumbents whose opponent party conducts a competitive primary.

Figure III.2, Panel A shows the sample distribution of the indirect DW-NOMINATE by party. Unsurprisingly, the indirect DW-NOMINATE typically places Democratic legislators on the liberal side of the political spectrum, while Republican representatives are situated on the conservative side with some overlap between parties at the center. As shown in Figure III.2, Panel B, the indirect DW-NOMINATE strongly correlates with legislators' actual DW-NOMINATE ($\rho = 0.9$), which further corroborates the validity of the indirect DW-NOMINATE as an estimate of legislators' roll-call voting positions.

To measure incumbents' roll-call extremism, I follow the same logic as in the estimation of non-incumbent candidate positions. Using the fact that DW-NOMINATE scores are bounded between -1 (very liberal) and 1 (very conservative), I define *roll call extremism* as the distance of the incumbents' *indirect DW-NOMINATE* from the opponent party's theoretical extreme:[17]

$$\text{Roll-Call Extremism}_{i(p)} = \begin{cases} |\text{Indirect DW-NOMINATE}_{i(p)} - (-1)| & \text{if } p = \text{Republican} \\ |\text{Indirect DW-NOMINATE}_{i(p)} - 1| & \text{if } p = \text{Democtrat} \end{cases} \quad (9)$$

Roll call extremism has a clear spatial interpretation. As shown in Figure III.2, Panel C, a decrease in *roll call extremism* means that a Democratic (Republican) incumbent takes a more conservative (liberal) position, implying a move in the direction of the opponent party. While a qualitative interpretation of directional changes is immediate, the quantitative assessment of its magnitude is less straightforward. To ease interpretation, I standardize *roll call extremism* to have mean 0 and standard deviation 1. When reporting results below, I offer a more direct assessment of magnitudes based on a back-of-the-envelope calculation that relates standard deviations in *roll call extremism* to DW-NOMINATE scores of House representatives in the 117^{th} Congress. I additionally present the full set of results using an alternative, directly interpretable measure of roll call extremism: party loyalty in divisive roll calls on which the majority of Democrats disagrees with the majority of Republicans. Deviating from party line in divisive votes is arguably a strong and costly signal of platform moderation, as it requires the incumbent not only to take a stance against her own party but also in support of the other party. Indeed, both measures are highly correlated ($\rho = 0.81$) as shown in Figure III.2, Panel D where *party loyalty*, defined as the percentage share of divisive votes cast in party line, is plotted against *roll call extremism*. Finally, to address concerns that levels of (standardized) *roll call extremism* and *party loyalty* may not be comparable across parties and congressional terms, my preferred specifications use differenced outcomes, thus focusing on within-incumbent changes in outcomes from the pre- to the post-primary period.

Additional information on incumbent characteristics beyond their voting record (gender, terms served, birth year) is also obtained from Lewis et al. (2022). I supplement that data with an indicator equal to 1 if an incumbent is "white", i.e., does not identify as a Black, Hispanic or Asian Pacific American according to the Office of the Historian of the U.S. House of Representatives,[18] and with data on House incumbents' local roots in the district they currently represent (Hunt, 2022).

[17] I deliberately depart from previous work that uses the absolute value of NOMINATE-based scalings to measure legislator extremity (e.g., Canes-Wrone et al., 2002; Fouirnaies and Hall, 2022) because taking the distance from the opposite extreme allows accommodating Representatives that cross the origin, i.e., Democrats with positive and Republicans with negative *indirect DW-NOMINATE* scores. I acknowledge that this introduces a small level difference hampering the comparability of *roll call extremism* across parties. However, in my preferred specifications, I use within-incumbent changes in *roll call extremism* effectively accounting for and eliminating level differences.

[18] see `https://history.house.gov/People/Search/`, accessed February 12, 2022.

3 Identification Strategy

3.1 Regression Discontinuity Design

Do incumbents commit to different policy positions by altering their roll-call voting behavior piror to elections in response to their opponent's platform? Answering this question empirically requires an identification strategy dealing with a twofold identification challenge. First, policy platforms of candidates appealing to the same electorate are jointly determined by unobserved voter preferences. Second, when strategic candidates choose a platform, they take into account their opponent's position, which implies interdependence of candidate positions inducing simultaneity bias. At a minimum, identification thus requires an exogenous shift in the opponent's position, which is i) orthogonal to voter preferences, and ii) independent from the incumbent candidate's current position.

Following Hall (2015), I use a sharp regression discontinuity design to leverage exogenous variation in challenger extremism generated by competitive toss-up primaries of the incumbent's opponent party. For competitive primaries where at least two candidates run for nomination, we have precise knowledge of the assignment mechanism that determines whether the incumbent runs against a relatively extreme or moderate challenger, i.e., whether a Democratic (Republican) incumbent runs against a more or less conservative Republican (liberal Democrat). The incumbent gets assigned to an extremist challenger if and only if the more extreme candidate gets a plurality of the vote in the opponent party's primary election. Assuming that agents have at best "imprecise control" (Lee and Lemieux, 2010) over the nomination outcome in close primary elections, I recover a local average treatment effect comparing post-primary roll-call voting behavior of otherwise identical incumbents who only differ in whether the more extreme of the two potential challengers won nomination by a narrow margin. Importantly, local randomization occurs at the district level, which – given single-member districts – coincides with the incumbent level. Thus, the design directly addresses the twofold identification challenge of i) simultaneity due to strategic candidates choosing their positions interdependently, and ii) omitted variable bias stemming from unobserved voter preferences that affect both candidates' policy stances.

Formally, I implement the design defining the treatment variable $T_{i(d)}$ as a dummy equal to 1 if the incumbent i's opponent party nominates the more extreme of the top-two candidates as the challenger for the incumbent's seat in district d, the assignment variable $X_{i(d)}$ as the extremist's top-two candidate vote share margin, normalized such that $T_{i(d)} = 1$ if $X_{i(d)} > 0$ and $T_{i(d)} = 0$ if $X_{i(d)} < 0$. I then evaluate the impact an extremist challenger has on the incumbent's roll-call voting position by estimating equations of the following form:

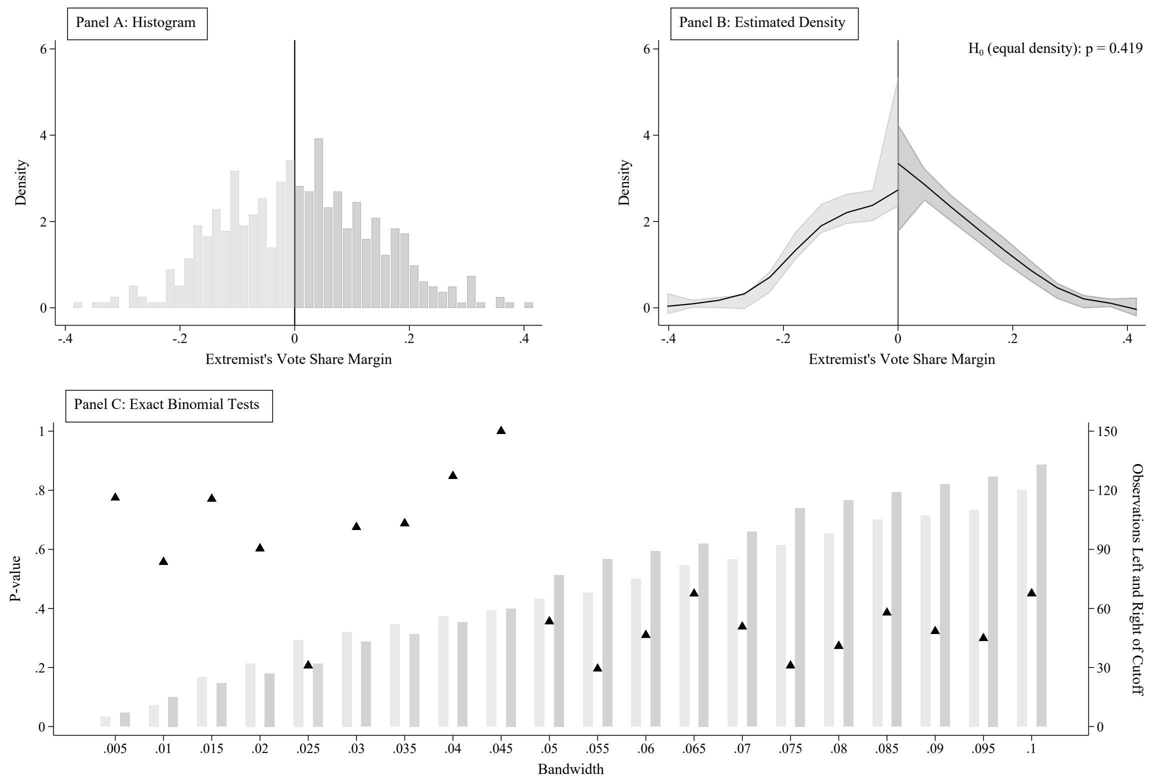
$$\Delta Y_{i(d)} = \alpha + \theta T_{i(d)} + \beta_1 X_{i(d)} + \beta_2 X_{i(d)} T_{i(d)} + [\beta_3 X_{i(d)}^2 + \beta_4 X_{i(d)}^2 T_{i(d)} + \boldsymbol{Z}_{i(d)}] + \epsilon_{i(d)} \qquad (10)$$

where θ is the coefficient of interest representing the causal effect of an extremist challenger relative to a moderate challenger. The main outcome of interest $Y_{i(d)}$ is the incumbent's (standardized) *roll call extremism* or *party loyalty* as defined in Section 2.4. My preferred specifications use differences in post- and pre-primary outcomes in the spirit of reducing measurement error to obtain more precise estimates of θ. At the same time, this estimation strategy effectively converts to a difference-in-discontinuities design. Unlike the traditional regression discontinuity, the difference-in-discontinuities design allows for level differences at the cutoff. Thus, θ is identified as a causal parameter under the much weaker assumption that potential confounds do not vary differentially over time in the neighborhood of the cutoff (Grembi et al., 2016). Following the advice of Gelman and Imbens (2019), I fit local linear splines of the assignment variable on each side of the cutoff, but also probe robustness to second-order polynomials ($X^2_{i(d)}$) and to the inclusion of covariates ($\boldsymbol{Z}_{i(d)}$). 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. In all specifications, I linearly downweight observations far from the cutoff by using a triangular kernel. Given repeated observations of the same incumbent over different election cycles, I cluster standard errors by House incumbent.

3.2 Checks on the Validity of the Identification Assumption

The coefficient θ in equation 10 identifies the causal effect of an extremist challenger on the incumbent's roll-call voting record under the assumption that agents have imprecise control over close primary election outcomes. This assumption would be violated if primary candidates or party elites were able to manipulate primary election results such that extremists and moderates systematically sort on different sides of the cutoff, or if incumbents correctly anticipated the outcome of close primaries and adjusted their position pre-emptively to favor the nomination of an electorally weaker opponent. Such manipulation is a priori extremely unlikely, as it would require precise information on the expected primary outcome and a concentrated effort just high enough to turn a narrow defeat into a narrow victory. To further check the plausibility of my identification assumption, I test two of its implications.

FIGURE III.3: MANIPULATION TESTS FOR AGGREGATE SORTING

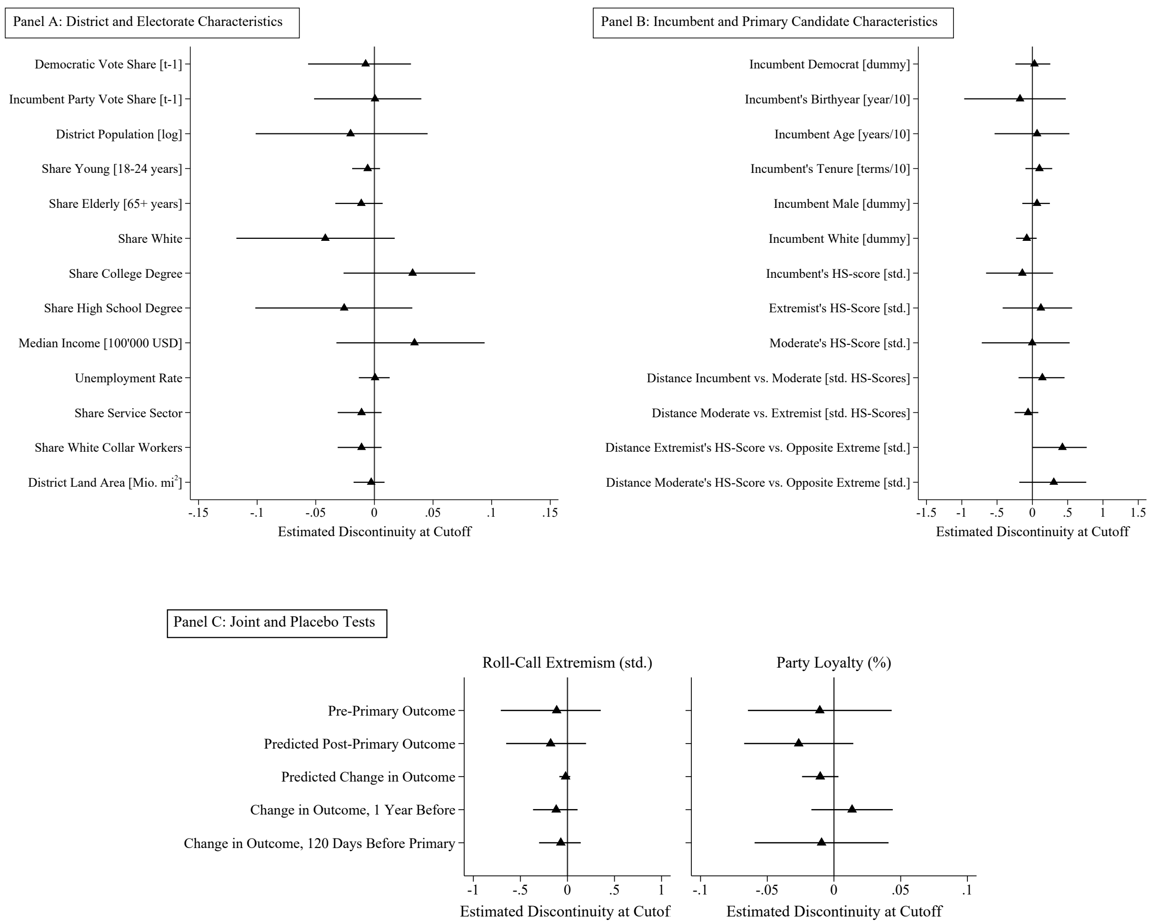


Notes: The Figure presents the sample distribution of the extremist's vote share margin for moderate (light grey) and extremist primary wins (dark grey) in Panel A. Panel B is a graphical representation of the density test in Cattaneo et al. (2020), plotting density estimates (solid lines) using local quadratic approximations and a triangular kernel along with bias-adjusted 95% confidence intervals (shaded areas). Panel C visualizes 20 finite sample exact binomial tests for different bandwidths (x-axis) around the cutoff, with the number of observations within the bandwidth that lie below (light grey bars) and above (dark grey bars) the cutoff, and p-values (black triangles) for exact binomial tests of the null hypothesis that the probability of an extremist primary victory is equal to 0.5. The sample is restricted to 490 competitive primary elections in districts represented by an opponent party incumbent who seeks re-election.

First, if extremists were differentially able to win close primary elections, one would expect "bunching" around the cutoff leading to a discontinuity in the distribution of the assignment variable. Figure III.3, Panel A provides *prima facie* evidence against aggregate sorting, showing that the number of observations just below and just above the cutoff is very similar. Next, I verify that there is no discontinuity in the distribution at the cutoff by implementing formal test proposed in Cattaneo et al. (2020), which is a variant of the McCrary (2008) test with higher statistical power and robust bias-adjusted inference. I fail to reject the hypothesis of equal density of extremist and moderate primary victories at the cutoff ($p = 0.419$). Figure III.3, Panel B provides a graphical representation of the density test, showing that estimated densities at the cutoff are near to each other, with 95% confidence intervals overlapping. Finally, I acknowledge that the size of my sample is small and consider the possibility of being underpowered to reject the null of continuous density at the cutoff. Following suggestions in Cattaneo et al. (2015) and Cattaneo et al. (2017), I therefore compute a series of finite sample exact binomial tests to check whether the number of close extremist primary wins and defeats within a pre-specified bandwidth is different from the number one would expect under a random sample of Bernoulli trials with probability 0.5 of landing on either side of the cutoff. Figure III.3, Panel C plots the number of observations on each side of the cutoff and reports p-values (represented by triangles) for bandwidths ranging from 0.5% to 10% of the extremist's vote share margin. None of the 20 tests rejects the hypothesis that the frequency of extremist wins and defeats was generated by a Bernoulli experiment with probability 0.5, providing evidence against aggregate sorting and lending further empirical support to the conclusion that manipulation of the primary election results is highly unlikely to invalidate my identification assumption.

A second testable implication of my identification assumption is that observable confounders should be continuous at the cutoff. I thus conduct a series of balance tests by regressing predetermined district and candidate characteristics on the righthand side of equation 10. Figure III.4, Panel A presents the results for district-level covariates, plotting the point estimates along with robust 95% confidence intervals. Importantly, there is no discontinuity in voter preferences, as proxied by the vote share for the Democratic candidate in the preceding presidential election and by the presidential vote share for the incumbent's party. Both point estimates are small and statistically indistinguishable from zero. Similarly, there is no significant difference in 11 other socio-demographic and economic characteristics between districts at the cutoff. As one can see in Panel B, also the distributions of incumbent characteristics (party affiliation, birth cohort, age, tenure, gender, and race), candidate positions estimated with pre-primary Hall-Snyder scores are smooth around the cutoff, as are the pre-primary outcome variables (top-row in Panel C). One single exception is the distance of the extremist's Hall-Snyder score to the opposite extreme, which is borderline significant at a 5%-level. Given the large number of covariates (13 district characteristics, 13 candidate characteristics, 2 pre-primary outcomes, for a total of 28), this aligns with the expected number of false positives in statistical testing.

FIGURE III.4: BALANCE TESTS AND PLACEBO CHECKS



Notes: The Figure presents results from balance checks on district- and electorte characteristics (Panel A), incumbent characteristics and estimated pre-primary candidate positions (Panel B), and placebo outcomes (Panel C). Point estimates (triangles) along with bias-adjusted robust 95% confidence intervals (spikes) accounting for clustering at the incumbent level from local-linear specifications of equation 10 with MSE-optimal bandwidth and triangular kernels. The sample is restricted to 490 races with re-election seeking incumbents whose opponent party holds a competitive primary.

More concerning is that some of the coefficients in Panel A and Panel B are imprecisely estimated with confidence intervals that cannot exclude substantively large discontinuities. To address this concern, I construct a joint test by regressing the post-primary outcome variables on all covariates listed in Panels A and B (omitting the distances between Hall-Snyder scores to avoid collinearity) and use the *predicted* outcomes as the dependent variable in equation 10. I follow an analogous procedure for the within-incumbent changes in post-primary outcomes with respect to the pre-primary period. As shown in Panel C, there is no significant discontinuity in predicted post-primary outcomes. In particular, the predicted change in outcomes at the cutoff is small and precisely estimated. Since my preferred specification of equation 10 uses changes in outcomes in the spirit of a difference-in-discontinuity design which allows for level differences provided that observations near the cutoff follow a common trend, I finally assess the validity of this alternative identification assumption conducting two placebo tests. As a first falsification check, I consider the change in incumbents' roll call voting in the same calendar period but in the off-election year of the same congressional term that precedes the election year. As a second falsification exercise, I anticipate the post-primary period to the 120 days before the actual primary election (instead of 120 days before the general election) and thus examine changes in incumbents' voting records in the period just before the primary election with respect to the remainder of the term preceding this period. Reassuringly, I do not find a discontinuity for any of these two placebo outcomes (bottom rows in Panel C).

Overall, the extensive set of validity checks supports the credibility of my identification strategy. There is no sorting around the cutoff and covariates are balanced. In particular, the continuity of voter preferences and incumbents' pre-primary roll call voting positions suggests that my design effectively deals with the twofold identification challenge that candidate positions are interdependent and codetermined by voter preferences. Moreover, the absence of divergent pre-trends at the cutoff lends support to the difference-in-discontinuity strategy, which removes any imbalance in time-invariant confounds. Evidence that my results do not depend on the inclusion of covariates will further affirm this conclusion.

3.3 Compensating Differentials and Challenger Valence

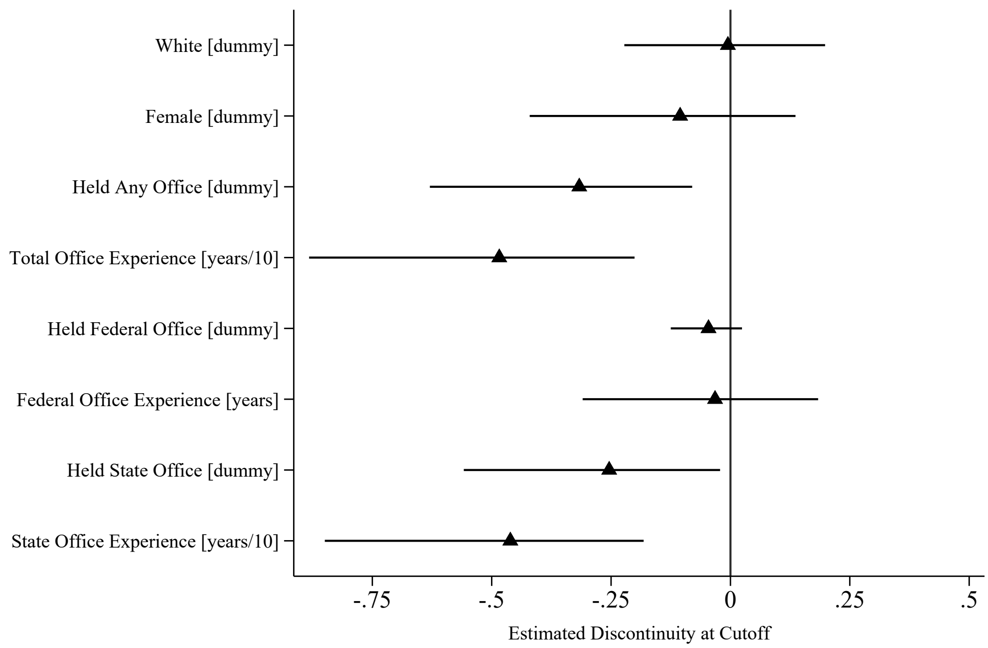
Under the stated assumption that agents have imprecise control over primary results, my design identifies the causal effect of an extremist challenger on incumbents' roll call voting record, evaluated against a counterfactual moderate challenger. However, it is worth noting that without further assumptions, my design does *not* isolate the effect of the extremist's *policy platform* compared to the moderate's. Extremists and moderates likely differ on non-policy characteristics that are valued by voters, commonly referred to as candidate "valence" (Stokes, 1963), which can include race or gender by which voters discriminate, or competence, campaigning skill, or any other source of popularity that confer an exogenous advantage to a candidate.

Drawing on formal spatial models of electoral competition that incorporate a valence dimension, one might presume lower-valence candidates to adopt more extreme platforms because they have an interest in making the policy dimension more salient with respect to the valence-dimension on which they are weak (e.g., Aragonès and Palfrey, 2002; Aragonès and Xefteris, 2012).[19] Even if valence and platform extremism are *unconditionally* uncorrelated, they supposedly are correlated in close elections due to compensating differentials (Gagliarducci and Paserman, 2012; Marshall, 2022). If primary voters tend to favor the extremist's platform because it is closer to the partisan primary electorate's ideal, then the moderate needs a compensating advan-

[19] See also Hummel, 2013, and Grofman et al., 2019 for applications specific to primaries.

tage on the non-policy dimension to end up in a close primary election. Given the largely partisan composition of the primary electorate in the U.S. (see e.g., Hill, 2015; Sides et al., 2020) and evidence indicating that moderate candidates are more likely to lose in primary elections (Brady et al., 2007; Hall and Snyder, 2015), one might thus suspect that narrowly nominated moderate challengers have a valence advantage over extremists who barely won nomination in an otherwise comparable district.

FIGURE III.5: NON-POLICY CHARACTERISTICS OF EXTREMIST COMPARED TO MODERATE BARE PRIMARY WINNERS



Notes: The Figure presents results from estimating local-linear specifications of equation 10 using MSE-optimal bandwidths and triangular kernels with the primary winner's race, gender and prior office experience as dependent variables. *White* is a dummy equal to 1 if the nominee is not Black, Asian or Hispanic. *Female* is a dummy equal to 1 if the challenger is a woman. *Held Any Office* is a dummy equal to 1 if the challenger has ever held federal or state-level elected office prior to the primary election. *Total Office Exprience* measures the duration of prior office experience in decades. *Held Federal Office*, *Federal Office Exprience*, *Held State Office*, and *State Office Exprience* are analogous measures restricting attention either to federal office or state office, respectively. Triangles depict point estimates and spikes represent bias-adjusted robust 95% confidence intervals accounting for clustering at the incumbent level. For limited data availability on prior office experience in earlier years, the sample is restricted to 391 competitive primaries with re-election seeking opponent party incumbents from the 1996 election cycle onward.

To characterize the compound nature of the treatment, I estimate equation 10 with the primary winner's race, gender and prior office experience as dependent variables, whereby I follow common practice in the literature and consider prior office experience as a proxy for valence.[20] As shown in Figure III.5, extremist close primary winners do not differ from barely nominated moderates by race or gender. However, they differ in valence proxied by prior office experience, as expected. Closely nominated moderate challengers are 31.6 percentage points more likely to have held any federal or state-level elected office before nomination than extremist bare primary winners. At the extensive margin, moderates' accumulated office experience exceeds that of extremists by approximately 5 years. Interestingly, this difference in prior officeholding is driven by moderates' higher state-legislative experience, with federal office experience being similar across extremists and moderate challengers at the cutoff.

At a baseline, my RD design thus identifies the causal effect of an extremist challenger, which should not be interpreted as the effect of extremist policy platforms alone, but as the compound effect of extremists' policy position and lower valence. This subtle distinction is of little relevance for answering the main question of this paper whether or not incumbents strategically adjust policy differentially to more extreme opponents. Yet, understanding whether incumbents react to their challenger's policy or their valence is important to understand the mechanism behind strategic adjustment. When discussing mechanisms in section 5.2, I provide evidence that the valence differential between extremists and moderates is not the driver of the adjustment we see in the main results but tends to work in the opposite direction, suggesting that the causal effect of an extremist *challenger* likely represents an *underestimate* of the effect of extremist *platforms*.

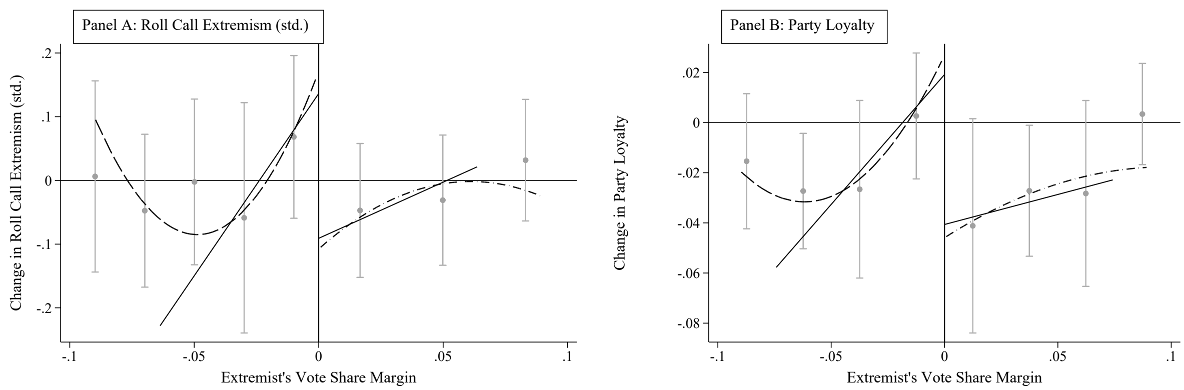
4 Main Results

I now turn to the presentation of the main results on how incumbents change their post-primary policy position in response to extremist and moderate challengers with respect to the pre-primary period. Figure Figure III.4 presents *prima facie* evidence that the changes in (standardized) *roll call extremism* (Panel A) and *party loyalty* (Panel B) jump at the cutoff. Incumbents quasi-randomly assigned to an extremist tend to adopt a more moderate position and are less likely to vote in party line compared to incumbents facing a barely nominated moderate. Intriguingly, this differential effect is driven by adjustments on both sides of the cutoff, i.e., by incumbents taking more moderate positions when facing an extremist, as well as incumbents differentiating themselves from moderate challengers by taking more extreme positions. Consistent with *strategic* adjustment, one

[20] If voters select on valence, prior officeholders are not only of higher "innate" quality, but they can also acquire competence through legislative experience (Padró i Miquel and Snyder Jr., 2006). Many if not most sources of valence advantages identified in the literature have been linked to officeholding (see Groseclose, 2001). Beyond the incumbency status itself (Lee, 2008), examples include name recognition and media presence (Prior, 2006), campaign funding (Fouirnaies and Hall, 2014), popularity due to constituency services and pork barrel spending (Levitt and Snyder, 1997), credibility and reputations for integrity and competence on account of a verifiable track record (Bernhardt and Ingerman, 1985; McCurley and Mondak, 1995), and the deterrence of high-quality competitors (Levitt and Wolfram, 1997). Concordantly, Kawai and Sunada (2022) structurally estimate the valence of candidates for the U.S. House and find that incumbents score substantively higher on the valence dimension.

can see that positional changes are largest for incumbents closest to the cutoff, i.e., precisely for those incumbents who ex ante were most uncertain which one of the two potential challengers they would ultimately face and could not adapt their position in anticipation of the opponent party's primary election outcome.

FIGURE III.6: INCUMBENTS' ADJUSTMENT TO EXTREMIST AND MODERATE CHALLENGERS



Notes: The Figure plots local means of the change in incumbents' standardized *roll call extremism* (Panel A) and the change in *party loyalty* (Panel B) between the pre-primary and the post-primary period. Local averages (dots) are calculated within IMSE-optimal equal-spaced bins (Calonico et al., 2015). 95% confidence intervals (spikes) account for clustering at the incumbent level. Local-linear (solid lines) and local-quadratic (dashed lines) fits on each side of the cutoff are calculated within the respective MSE-optimal bandwidth. The sample is restricted to 253 re-election seeking incumbents whose challenger won nomination by a margin less than 10% of the top-two primary candidate vote share.

Table III.1 complements the graphical evidence with formal estimates of an extremist challenger's effect on the incumbents' (standardized) *roll call extremism*, with results from local linear specifications of equation 10 presented in Panel A and local quadratic specifications in Panel B. Estimated effects on pre-primary *roll call extremism* are small and statistically insignificant (Column 1) and turn into more precisely estimated zeros upon the inclusion of covariates (Column 2). The impact of an extremist challenger on incumbents' post-primary *roll call extremism* is negative and large, albeit imprecisely estimated (Columns 3 and 4). In the spirit of reducing measurement error in the outcome variable, I next consider the within-incumbent change *roll call extremism* from the pre-primary to the post-primary period. The implied decrease in *roll call extremism* by 0.23 (local linear) or 0.31 standard deviations (local quadratic specification) in response to an extremist challenger is of the same order of magnitude as the corresponding effects on post-primary levels of *roll call extremism*, but is now precisely estimated and significant at the 5% level (Column 5). Importantly, the magnitude of coefficients is unaffected when controlling for covariates (Column 6) and for the pre-primary base level of *roll call extremism* (Column 7) although this leads to smaller standard errors and significance at the 1% level.[21]

Table III.1: The Effect of an Extremist Challenger on Incumbents' (Standardized) Roll Call Extremism

	Before Primary		After Primary		Change (After - Before Primary)		
Panel A: Local Linear	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	-0.114	-0.029	-0.280	-0.162	-0.227**	-0.228***	-0.226***
	(0.239)	(0.136)	(0.273)	(0.170)	(0.106)	(0.088)	(0.088)
	[0.585]	[0.922]	[0.288]	[0.293]	[0.021]	[0.006]	[0.006]
MSE-optimal Bandwidth	0.121	0.120	0.099	0.087	0.064	0.059	0.060
Effective Observations	302	294	246	218	169	162	162
Control Mean	-0.119	-0.121	-0.129	-0.135	-0.015	-0.014	-0.014
Panel B: Local Quadratic							
	-0.075	0.021	-0.315	-0.203	-0.306**	-0.331***	-0.330***
	(0.353)	(0.237)	(0.314)	(0.202)	(0.125)	(0.111)	(0.111)
	[0.941]	[0.864]	[0.319]	[0.317]	[0.018]	[0.004]	[0.004]
MSE-Optimal Bandwidth	0.117	0.090	0.157	0.143	0.092	0.079	0.080
Effective Observations	297	223	370	337	234	206	208
Control Mean	-0.122	-0.134	-0.085	-0.095	-0.015	-0.018	-0.017
Observations	490	479	490	479	490	479	479
Covariates	N	Y	N	Y	N	Y	Y
Outcome Before Primary	-	-	N	N	N	N	Y

Notes: The Table reports estimated effects of extremist challengers on incumbents' *roll call extremism* from local polynomial estimation of equation 10, fitting separate polynomials of order 1 (Panel A) or order 2 (Panel B) on each side of the cutoff. The outcome variable is the incumbent's standardized *roll call extremism* prior to the opponent party's primary (Columns 1 and 2), standardized post-primary *roll call extremism* in the 120 days before the general election (Columns 3 and 4), difference in standardized *roll call extremism* between the post- and pre-primary period (Columns 5–7). Columns 2, 4, 6, and 7 adjust for all covariates listed in Figure III.4, Panels A and B, exluding the distance between candidates' Hall-Snyder scores; Column 7 additionaly controls for the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. Effective Observations is the number of observations within the MSE-optimal bandwidth. Control Mean reports the outcome mean within the MSE-optimal bandwidth below the cutoff. The sample is restricted to re-election seeking incumbents. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

An extremist thus causes the incumbent to adopt a 0.23 to 0.31 standard deviations more moderate position compared to an incumbent facing a moderate challenger. How large are these effects? To give an interpretation of the magnitudes, I propose a simple back-of-the-envelope calculation that maps a 0.25 standard deviation change in my DW-NOMINATE-based measure of *roll call extremism* to actual DW-NOMINATE scores in the 117^{th} Congress (2021-2023). In my sample of re-election seeking incumbents, a 0.25 standard deviation in the actual DW-NOMINATE score corresponds to 0.096 points on the DW-NOMINATE scale that ranges from -1 (very liberal) to + 1 (very conservative). In the House of Representatives of the 117^{th} Congress, 0.096 points on the

[21] Included covariates are all district and electorate characteristics listed in Figure III.4, Panel A, as well as all incumbent and candidate characteristics listed in Figure III.4, Panel B, except for the distance in Hall-Snyder scores between candidates, which would be collinear with the included Hall-Snyder scores of the incumbent and primary candidates. Note that the inclusion of covariates reduces the sample size by 11 observations. From Boatright et al. (2019) I inherit missing information on previous presidential election vote shares, and for 8 districts I am not able to supplement this information because of redistricting. For 4 incumbents, Hall-Snyder scores cannot be computed because of missing campaign finance data in Bonica (2021).

DW-NOMINATE scale correspond to 5% of the distance between the most liberal Democrat and the most conservative Republican, or 10% of the distance between the Democratic the Republican party leaders, to roughly one-half of the within-party interquartile ranges, or to approximately the average distance between representatives and their own party's median.[22]

TABLE III.2: THE EFFECT OF AN EXTREMIST CHALLENGER ON INCUMBENTS' PARTY LOYALTY

	Before Primary		After Primary		Change (After - Before Primary)		
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	-0.011	0.002	-0.061	-0.046	-0.060**	-0.053***	-0.054***
	(0.027)	(0.024)	(0.042)	(0.034)	(0.027)	(0.021)	(0.021)
	[0.719]	[0.899]	[0.133]	[0.157]	[0.023]	[0.009]	[0.007]
MSE-Optimal Bandwidth	0.097	0.088	0.085	0.084	0.074	0.071	0.071
Effective Observations	242	219	224	218	202	184	184
Control Mean	0.864	0.866	0.845	0.845	-0.023	-0.024	-0.024
PANEL B: LOCAL QUADRATIC							
	-0.001	0.017	-0.079*	-0.053	-0.071**	-0.072**	-0.074**
	(0.035)	(0.035)	(0.048)	(0.040)	(0.031)	(0.028)	(0.028)
	[0.973]	[0.603]	[0.096]	[0.180]	[0.029]	[0.015]	[0.011]
MSERD-Optimal Bandwidth	0.118	0.095	0.141	0.144	0.111	0.093	0.093
Effective Observations	297	231	340	338	279	229	230
Control Mean	0.869	0.865	0.855	0.857	-0.020	-0.021	-0.021
Observations	490	479	490	479	490	479	479
Covariates	N	Y	N	Y	N	Y	Y
Outcome Before Primary	-	-	N	N	N	N	Y

Notes: The Table reports estimated effects of extremist challengers on incumbents' *party loyalty*, in percent of divisive roll calls cast in party line, from local polynomial estimation of equation 10, fitting separate polynomials of order 1 (Panel A) or order 2 (Panel B) on each side of the cutoff. The outcome variable is the incumbent's *party loyalty* prior to the opponent party's primary (Columns 1 and 2), post-primary *party loyalty* in the 120 days before the general election (columns 3 and 4), difference in *party loyalty* between the post- and pre-primary period (Columns 5–7). Columns 2, 4, 6, and 7 adjust for all covariates listed in Figure III.4, Panels A and B, exluding the distance between candidates' Hall-Snyder scores; Column 7 additionaly controls for the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. Effective Observations is the number of observations within the MSE-optimal bandwidth. Control Mean reports the outcome mean within the MSE-optimal bandwidth below the cutoff. The sample is restricted to re-election seeking incumbents. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Next, I examine extremist challengers' effect on incumbents' *party loyalty*. Results presented in Table III.2 reveal a pattern highly similar to the previous results on *roll call extremism*. There is zero difference in incumbents' party-line voting before the opponent party's primary election (Columns 1 and 2), reassuring that incumbents did not adopt differential positions before learning the nomination of an extremist challenger. After the primary, incumbents facing an extremist are 5 – 7 percentage points less likely to vote in party line than incumbents whose opponent party narrowly nominated a moderate (Columns 3 and 4). When considering within-incumbent

[22]More exactly, 0.096 points on the DW-NOMINATE scale correspond to 45.5% of the Republican interquartile range and 61.9% of Democratic interquartile range, and to 89.6% of the average distance between representatives and their own party's median.

differences between the post- and pre-primary period, these effects are significant at least at the 5% level, whereby the size of the estimated impact remains the same as for post-primary levels of *party loyalty* and robust to controlling for covariates and pre-primary outcome levels (Columns 5 – 7). Evaluated at the control mean of 85% at the left of the cutoff, the jump by 5 – 7 percentage points implies a decrease in party loyalty by around 6 – 8%, which seems small but is due to an overall high party loyalty in the U.S. House. In my sample, however, this corresponds to one-third of a standard deviation, or, in the 117^{th} Congress, to a shift from the 15^{th} to the 85^{th} percentile.

Evidence for both *roll call extremism* and *party loyalty* tells a qualitatively consistent story. Incumbents react to an extremist challenger by moderating their roll-call voting record differentially more than they would if facing a more moderate challenger. These results are robust to an extensive set of robustness checks, including alternative RD-specifications with higher-order polynomials (Appendix Table C.5), different kernels (Appendix Tables C.6 and C.7), and a wide range of bandwidths (Appendix Figures C.3 and C.4). Concerning potential issues pertinent to this paper, I show robustness to alternative thresholds of minimum number of donations for the inclusion of donors and candidates in the estimation of primary candidates' Hall-Snyder score (Appendix Tables C.1 and C.2), and different time windows preceding the general election in the calculation of outcome variables (Appendix Tables C.3 and C.4). Finally, I address concerns of measurement error in Hall-Snyder scores that might lead to misclassification of moderate and extremist primary candidates by re-estimating equation 10 on ever smaller subsamples that successively exclude the 5 percentiles with the smallest distance between primary candidates Hall-Snyder scores, i.e., those observations where misclassification is most likely to occur. Results are remarkably stable and, if anything, effects tend to grow in magnitude with higher distance between primary candidates' estimated positions, consistent with classical measurement error in the treatment variable leading to attenuation bias (Appendix Figures C.1 and C.2)

Overall, there is highly robust evidence that an extremist challenger causes incumbents to adopt positions more moderate than they would against more moderate challengers. This pattern of adjustment is inconsistent with both, the prediction of incumbent policy persistence in citizen-candidate models, and with strategic behavior prescribed by the canonical Downsian convergence mechanism where more moderate opponents exert pressure on the opposite candidate to moderate as well. I now turn to heterogeneity analyses, in which I first provide evidence that incumbents' reaction is indeed part of an electoral strategy, and second, explore potential mechanisms behind this pattern suggesting strategic complementarity of policy platforms.

5 Heterogeneity Analysis and Discussion of Mechanisms

5.1 The Role of Electoral Incentives and Strategic Behavior

5.1.1 Political Ambition and the Role of Re-election Concerns

The leading hypothesis of this paper is that incumbents strategically commit to an adjusted platform in response to their opponent's position. The finding that incumbents differentially adapt their positions after learning whether they are going to face a moderate or extreme challenger is consistent with strategic adjustment to the opponent.

An alternative interpretation of this result, however, could be that the opponent party's nomination outcome carries information about voter preferences and that the shift in the incumbent's policy does not reflect an electoral strategy tailored to her challenger but is the consequence of a benevolent incumbent acting upon her updated beliefs about voter preferences. Although it is, in principle, conceivable that legislators wish to be good representatives who faithfully advance their constituency's interests, it is fairly unlikely that rational incumbents update their beliefs about voter preferences depending on the nomination outcome of close opponent party primaries. Not only are partisan primary voters highly unrepresentative of the district's general electorate, but by design, my RD estimates are based on primary elections where the extremist and the moderate candidate were equally popular. If incumbents are rational, they should recognize that nomination outcomes toss-up primaries are uninformative about voter preferences in their district.

Yet, boundedly rational incumbents may misperceive nomination outcomes as shifts in voter preferences. Another possibility is that extremist and moderate challengers bring up different issues that grow popular during the general election campaign, and that a benevolent incumbent takes up these issues for non-strategic motives. To test for these alternative mechanisms, I estimate equation 10 on an auxiliary sample of incumbents who do not run for re-election, in the spirit of a placebo check.

TABLE III.3: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON INCUMBENTS' ELECTORAL AMBITION

	Incumbent Seeking Re-election				Incumbent not Seeking Re-election			
PANEL A: EFFECT ON Δ ROLL-CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.227**	-0.228**	-0.303**	-0.302**	-0.010	0.021	-0.041	-0.019
	(0.105)	(0.105)	(0.124)	(0.124)	(0.169)	(0.170)	(0.258)	(0.264)
	[0.021]	[0.021]	[0.019]	[0.019]	[0.828]	[0.953]	[0.856]	[0.921]
MSE-Optimal Bandwidth	0.064	0.064	0.092	0.093	0.098	0.095	0.119	0.114
Effective Observations	169	169	234	235	112	107	129	123
Control Mean	-0.032	-0.032	-0.040	-0.040	0.128	0.121	0.119	0.112
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.060**	-0.062**	-0.071**	-0.073**	0.012	0.013	0.021	0.023
	(0.027)	(0.026)	(0.031)	(0.031)	(0.030)	(0.029)	(0.043)	(0.042)
	[0.023]	[0.018]	[0.029]	[0.025]	[0.543]	[0.491]	[0.725]	[0.680]
MSE-Optimal Bandwidth	0.074	0.071	0.111	0.105	0.125	0.133	0.117	0.118
Effective Observations	202	185	279	266	131	138	127	128
Control Mean	-0.013	-0.012	-0.013	-0.014	0.011	0.011	0.010	0.010
Observations	490	490	490	490	219	219	219	219
Polynomial	1	1	2	2	1	1	2	2
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for incumbents who seek re-election (Columns 1 – 4) and incumbents who do not seek re-election (Columns 5 – 8). Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Table III.3 reports the estimated discontinuities for retiring incumbents in Columns 5 – 8, and for comparison represents the main results on the sample of re-election seeking incumbents in Columns 1 – 4.[23] The estimated effects of an extremist challenger on changes in *roll call extremism* and *party loyalty* for retiring incumbents are statistically indistinguishable from zero. Although too imprecisely estimated to rule out small effects, coefficients for retiring incumbents are several orders of magnitude smaller than those for re-election seeking incumbents, or have the opposite sign in case of *party loyalty*.

I thus reject the hypothesis that updated beliefs about voter preferences or issue uptake by benevolent incumbents explain the results. Rather, the effects of extremist challengers being confined to re-election seeking incumbents indicates that incumbents' differential reaction to extremist challengers is part of an electoral strategy.

5.1.2 Seat Marginality and the Role of Electoral Competition

Having shown that incumbents' response to challengers is conditioned by the presence of electoral incentives, I now evaluate how the degree of electoral competitiveness shapes incumbents'

[23]Note that the estimates in Panel A, Columns 1 and 3 do not exactly match the corresponding estimates in Table III.1 because here *roll call extremism* is standardized over the whole sample including retiring incumbents to ensure comparability of coefficients across the two subsamples.

response. Electoral incentives are more binding for incumbents whose chances of winning re-election are ex-ante smaller. Thus, if incumbents strategically adjust their position aiming for an electoral advantage, one would expect stronger reactions from electorally vulnerable incumbents.

To test this hypothesis, I proxy the incumbent's ex-ante electoral strength by the district's two-party vote share for the incumbent party's candidate in the preceding presidential election. I then define a district as "marginal" when the incumbent's electoral strength is below the sample median (presidential two-party vote share between 17% and 53.5%), and as "safe" when the incumbent's electoral strength exceeds the sample median (between 53.5% and 87.5%). It is worth noting that electorally vulnerable incumbents defending a marginal seat still stand fairly high chances of winning re-election. Their expected two-party vote share in the general election, as predicted from their lagged presidential two-party vote share by a linear regression, ranges from 42% to 55% compared to 55%-69% in safe districts.

Results obtained from re-estimating equation 10 on the two subsamples of incumbents holding marginal and safe districts are presented in Table III.4. In Columns 1 – 4, one can see that indeed incumbents strongly react to a more extreme challenger when they defend a marginal seat. Estimated coefficients are highly significant and of almost double the magnitude compared to the corresponding baseline results on the pooled sample in section 4. This is true for both the change in *roll call extremism* (Panel A) and the change in *party loyalty* (Panel B) with respect to the pre-primary period, and holds across specifications with and without covariate adjustment for the pre-primary outcome and different polynomial orders. For incumbents holding a safe seat, on the other hand, the nomination of an extremist rather than a moderate challenger has no discernible impact on roll-call voting behavior. All coefficients in Columns 5 – 8 are economically and statistically indistinguishable from zero.

TABLE III.4: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON DISTRICT COMPETITIVENESS

	Marginal Districts				Safe Districts			
PANEL A: EFFECT ON Δ ROLL CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.372***	-0.387***	-0.537***	-0.548***	-0.004	-0.006	0.009	0.009
	(0.153)	(0.153)	(0.190)	(0.189)	(0.115)	(0.114)	(0.130)	(0.136)
	[0.008]	[0.006]	[0.006]	[0.005]	[0.857]	[0.804]	[0.838]	[0.858]
MSE-Optimal Bandwidth	0.068	0.066	0.087	0.086	0.081	0.086	0.091	0.097
Effective Observations	101	100	121	121	95	99	104	108
Control Mean	0.010	0.019	0.015	0.015	-0.036	-0.028	-0.031	-0.030
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.101***	-0.103***	-0.137***	-0.137***	-0.019	-0.010	-0.009	-0.012
	(0.039)	(0.039)	(0.048)	(0.049)	(0.029)	(0.026)	(0.036)	(0.029)
	[0.006]	[0.006]	[0.006]	[0.007]	[0.575]	[0.750]	[0.932]	[0.631]
MSE-Optimal Bandwidth	0.072	0.069	0.090	0.088	0.097	0.084	0.123	0.141
Effective Observations	107	101	124	122	106	98	141	157
Control Mean	-0.017	-0.017	-0.019	-0.019	-0.008	-0.008	-0.004	0.001
Polynomial	1	1	2	2	1	1	2	2
Observations	241	241	241	241	241	241	241	241
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for marginal districts (Columns 1 – 4) and safe districts (Columns 5 – 8). A district is defined as marginal if the vote share of the incumbent's party in the prior presidential election is above the sample median of 53.5%. Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to re-election seeking incumbents. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

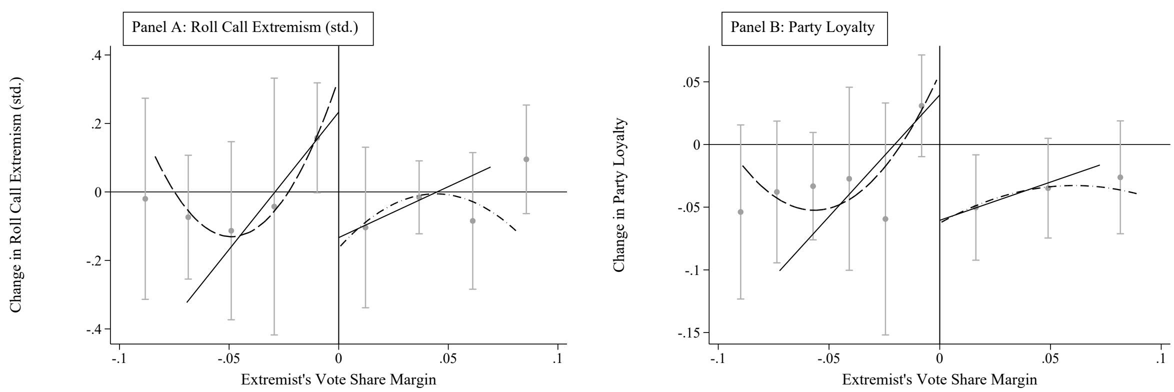
Incumbents not only react differentially more upon learning their challenger's position when they are electorally vulnerable, but adjustment is confined to competitive districts.[24] Incumbents holding safe seats do not take advantage of the opportunity to adjust their platform to their opponent, suggesting that policy adjustment is subject to constraints. Unconditional vote-maximizing behavior thus inadequately describes the pattern observed in the data. Rather than blindly following the challenger, incumbents seem to adjust strategically, carefully trading off electoral returns from platform adjustment with its cost.

One interpretation is that incumbents opportunistically commit to a new position only when electoral returns to platform adjustment are high enough. Incumbents in marginal districts are not only electorally disadvantaged compared to incumbents defending a safe seat, but they also compete in a less partisan environment. By definition, marginal districts are districts that elected House incumbents from one party but greatly supported the opponent party in the last presidential elections. Marginal districts thus have a larger proportion of swing voters that can be swayed by policy, implying higher returns to platform adjustment, and fiercer competition on the policy dimension.

[24]In Appendix Tables C.8 and C.9, I split the sample into quartiles of incumbent electoral strength and find qualitatively similar results: Effects of an extremist challenger are strongest when incumbents are weakest, i.e., largest in the lowest quartile, smaller in the second quartile, and absent in the upper two quartiles.

In line with this interpretation, I find that the close nomination of an extremist as opposed to a more moderate challenger affects general election vote shares only in marginal districts, increasing the incumbent's vote share margin by 8 percentage points from a baseline of around 5 percentage points, whereas the nomination outcome of close challenger primaries has no discernible impact on general elections in safe districts (see Appendix Table C.10).[25] Furthermore, there is indication that competitiveness on the policy dimension is indeed fiercer in marginal than in safe districts. The distance between positions (as estimated by Hall-Snyder scores) of the incumbent and the more moderate of the two potential challengers is by a statistically significant 0.3 standard deviation ($p = 0.001$) smaller in marginal compared to safe districts, suggesting that in marginal districts they more likely appeal to similar groups of voters at the center of the political spectrum.

FIGURE III.7: INCUMBENTS' ADJUSTMENT TO EXTREMIST AND MODERATE CHALLENGERS IN MARGINAL DISTRICTS



Notes: The Figure plots local means of the change in incumbents' standardized *roll call extremism* (Panel A) and the change in *party loyalty* (Panel B) between the pre-primary and the post-primary period. Local averages (dots) are calculated within IMSE-optimal equal-spaced bins (Calonico et al., 2015). 95% confidence intervals (spikes) account for clustering at the incumbent level. Local-linear (solid lines) and local-quadratic (dashed lines) fits on each side of the cutoff are calculated within the respective MSE-optimal bandwidth. The sample is restricted to 134 re-election seeking incumbents in marginal districts whose challenger won nomination by a margin less than 10% of the top-two primary candidate vote share.

Yet, higher electoral returns due to the presence of swing voters provide only a partial explanation of the pattern we see in Figure III.7. If the rival party abandons the center by nominating an extremist, the incumbent clearly has an incentive to moderate to win over swing voters. This accounts for incumbents moderating *differentially* more when running against extremists, but it does not explain why at least part of this differential effect comes from incumbents taking more extreme

[25]This confirms that Hall's (2015) general result also holds for my sample of challenger primaries: The nomination of extremists hurts their party's prospects to win elections. Interestingly, however, Hall finds that the extremist's effect on general election vote shares is negligible if the district is safe for the extremist's *own* party, while I find no effect when the district is safe for the incumbent, i.e., the extremist's *opponent* party.

positions vis-à-vis a moderate challenger. I now turn to the exploration of possible mechanisms behind this pattern.

5.2 Mechanisms Behind Strategic Complementarity

5.2.1 Platform Proximity and the Role of Core Supporters and Third-party Candidates

Why do incumbents moderate when running against an extremist challenger, but differentiate their platform from moderate challengers' position? One possible answer is that incumbents face a trade-off between persuading swing voters and mobilizing their core supporters. By moving toward her opponent, the incumbent appeals to swing voters and increases the number of voters that prefer her policy over the platform offered by her rival, but at the same time she demobilizes her core supporters. If she offers a platform too close to her opponent's, the incumbent's core supporters may refuse to turn out and abstain from voting due to indifference or alienation (Adams and Merrill, 2003; Bierbrauer et al., 2022), vote for third-party candidates (Palfrey, 1984; Weber, 1992; Callander and Wilson, 2007), or deny active and financial contributions to the incumbent's campaign (Aldrich, 1983).

The balance in this trade-off is skewed the most toward a mobilization strategy when the incumbent competes against a moderate challenger with a proximate platform. A moderate challenger with a platform similar to the incumbent's severely limits the scope of moderation on the persuasive margin,[26] and demobilizes the incumbent's core supporters more than a moderate challenger with a distant platform. On the flip side, an extremist challenger is more likely to shift the balance toward a persuasion strategy when he won nomination against a moderate with a platform close to the incumbent's, in which case the incumbent's core supporters get mobilized and the swing voters abandoned by the opponent party can be targeted by the incumbent.[27]

Thus, a testable implication of this mechanism is that incumbents should adjust more when the more moderate the two potential challengers offers a platform similar to the incumbent's, i.e., when there is more need to differentiate vis-à-vis the moderate challenger and there are higher returns to moderation against an extremist challenger. To test this hypothesis, I rely on Hall-Snyder scores to estimate the distance between policy positions of the incumbent and of the more moderate of the two potential challengers. I then split the sample by the median distance in cases where the incumbent and the moderate have "proximate" platforms (below median distance) and "distant" platforms (above median distance). Next, I estimate equation 10 on these two subsamples,

[26] In particular if voters have preferences not only over policy but also prefer to vote for the party they identify with, moderate voters that identify with the incumbent's opponent party likely vote for the moderate opponent even if the incumbent's platform is somewhat closer to their ideal than her opponent's.

[27] Downs (1957b, pp.117-118) made a similar argument noting that "[t]he possibility that parties will be kept from converging ideologically in a two-party system depends upon the refusal of extremist voters to support either party if both become alike—not identical, but merely similar. [. . .] At exactly what point this leakage checks the convergence of A and B depends upon how many extremists each loses by moving towards the center compared with how many moderates it gains thereby."

whereby I restrict attention to incumbents defending marginal seats.

TABLE III.5: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON DEPENDING ON INCUMBENTS' PROXIMITY TO THE MODERATE POTENTIAL CHALLENGER

	Proximate Platforms				Distant Platforms			
PANEL A: EFFECT ON Δ ROLL CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.539**	-0.531***	-0.670**	-0.661**	-0.189	-0.174	-0.268	-0.248
	(0.222)	(0.195)	(0.295)	(0.282)	(0.237)	(0.233)	(0.301)	(0.296)
	[0.013]	[0.006]	[0.032]	[0.029]	[0.334]	[0.376]	[0.406]	[0.443]
MSE-Optimal Bandwidth	0.051	0.051	0.076	0.076	0.082	0.082	0.100	0.100
Effective Observations	48	48	65	64	50	50	57	56
Control Mean	0.072	0.072	0.053	0.047	-0.024	-0.024	-0.062	-0.062
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.122***	-0.126***	-0.167**	-0.166**	-0.045	-0.055	-0.091	-0.080
	(0.044)	(0.045)	(0.076)	(0.075)	(0.051)	(0.062)	(0.082)	(0.080)
	[0.005]	[0.006]	[0.047]	[0.048]	[0.426]	[0.309]	[0.245]	[0.277]
MSE-Optimal Bandwidth	0.058	0.056	0.070	0.070	0.135	0.093	0.103	0.107
Effective Observations	51	48	58	58	73	54	57	59
Control Mean	-0.004	-0.002	-0.012	-0.012	-0.022	-0.025	-0.032	-0.031
Polynomial	1	1	2	2	1	1	2	2
Observations	135	135	135	135	106	106	106	106
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for the cases when the moderate potential challenger has a position close (Columns 1 – 4) or distant to the incumbent's (Columns 5 – 8). Platforms of the incumbent and moderate potential challenger are defined a proximate if the distance between the incumbent's and the moderate's Hall-Snyder score is below the sample median. Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to incumbents seeking re-election in marginal districts. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Results are presented in Table III.5. One can see that the nomination of an extremist challenger causes a decrease in the incumbent's *roll call extremism* by approximately 0.5 standard deviations (Panel A, Column 1) when platforms are proximate, an effect more than twice as large compared to the case where the incumbent and the moderate offer distant platforms (Panel A, Column 5). This result is robust to controlling for the pre-primary level of *roll call extremism* (Panel A, Columns 2 and 6), to including second-order polynomials of the assignment variable (Panel A, Columns 3 and 7), or both (Panel A, Columns 4 and 8). The pattern is highly similar for *party loyalty* as an alternative measure of incumbent extremism (Panel B).

Although the difference in effects between proximate and distant platforms falls short of statistical significance in this small sample, the difference in magnitudes is highly suggestive and consistent with incumbents striking a balance between platform differentiation to secure their base and platform moderation to persuade swing voters. Importantly, I do not find any differential effect depending on platform proximity in safe districts, where incumbents' strong partisan advantage likely dissolves the trade-off between targeting core supporters and swing voters (see

Appendix Table C.11).

Whether this trade-off arises because of endogenous participation, the threat of losing voters to third-party candidates, or a combination thereof is hard to discern. Consistent with third candidates playing a role in shaping incumbents' differential response to extremist and moderate main-party challengers, I find that incumbents adjust their position only when third candidates are present in the general election. Focusing on marginal districts, Table III.6 presents results from estimating equation 10 separately on a subsample restricted to general election races in with only the two Republican and Democratic candidates compete against each other, and on the complementary subsample including the majority of observations where at least one-third candidate gets a non-zero vote share. One can see that the effect of an extremist challenger on incumbents' *roll call extremism* (Panel A) and *party loyalty* (Panel B) are large, negative and statistically significant if third candidates are present (Columns 1 – 4) but close to zero for two-candidate elections (Columns 5 – 8).

TABLE III.6: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON THE PRESENCE OF THIRD CANDIDATES

	Other Candidates Present				Two Candidates Only			
PANEL A: EFFECT ON Δ ROLL-CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.522***	-0.573***	-0.681***	-0.703***	0.072	0.162	-0.198	-0.023
	(0.184)	(0.188)	(0.223)	(0.220)	(0.189)	(0.202)	(0.382)	(0.371)
	[0.003]	[0.002]	[0.003]	[0.002]	[0.862]	[0.634]	[0.602]	[0.897]
MSE-Optimal Bandwidth	0.067	0.061	0.091	0.089	0.069	0.071	0.072	0.073
Effective Observations	76	68	96	93	25	26	27	27
Control Mean	0.00	0.04	0.01	0.01	0.03	0.03	0.01	0.01
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.109**	-0.114***	-0.148**	-0.151***	-0.036	-0.019	-0.108	-0.069
	(0.045)	(0.046)	(0.056)	(0.056)	(0.050)	(0.056)	(0.091)	(0.092)
	[0.011]	[0.009]	[0.010]	[0.009]	[0.495]	[0.710]	[0.267]	[0.451]
MSE-Optimal Bandwidth	0.092	0.084	0.100	0.098	0.060	0.058	0.074	0.075
Effective Observations	96	92	102	102	24	24	27	27
Control Mean	-0.03	-0.02	-0.03	-0.03	-0.00	-0.00	-0.01	-0.01
Polynomial Order	1	1	2	2	1	1	2	2
Observations	183	183	183	183	58	58	58	58
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for the cases where third candidates are present (Columns 1 – 4) or only the incumbent and her main party opponent compete in the general election (Columns 5 – 8). Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to incumbents seeking re-election in marginal districts. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Consistent with the notion that the Downsian convergence mechanism breaks down with the entry of third candidates, the presence of third candidates conditions the incumbents' differential response to main party challengers. I caveat that the above evidence is correlational and should be interpreted with caution, as the entry decision of third-party candidates is likely endogenous to main-party candidates' position. In fact, the close nomination of an extremist challenger in marginal districts increases the probability that at least one other candidate is present by over 30 percentage points (see Appendix Table C.10). Evidence that extremists cause third-candidate entry and that the incumbents' response seems to be conditioned by the presence of third candidates, strongly suggests that third candidates play a crucial role in mediating incumbents' differential response to extremist and moderate challengers. However, without more detailed data on the political orientation and dropout decisions of third candidates who filed for the general election, the precise channel by which they mediate incumbents' policy adjustment to challenger positions remains obscure.[28] Against this backdrop, exploring the mediating effect of third candidates on incumbents' response to main-party challengers, I suspect, would be a promising avenue for further research.

5.2.2 Local Roots and the Role of Policy Motivation

Another interpretation of the result that incumbents' reaction to challengers is confined to marginal districts could be that policy-motivated candidates adjust "as necessary", i.e., that they compromise with their own ideal only if electoral pressure to do so is high enough. Indeed, the observation that they take more moderate positions when facing an extremist challenger is consistent with the idea that policy-motivated incumbents moderate in order to prevent the extremist from winning.

However, this mechanism is hard to reconcile with the result that incumbents adjust their platform differentially more when the opponent's platform is proximate. Remember that policy-motivated incumbents do not (only) care about winning the election per se, but also about the policy that is going to be implemented by the elected candidate, whereby they recognize that candidates with more moderate platforms have a higher probability of winning (e.g., Wittman, 1983; Calvert, 1985; Alesina, 1988). For policy-motivated incumbents, a more extreme opponent has therefore two ambiguous effects. On the one hand, an extremist opponent increases the incumbent's chances of winning given her current position, thus relaxing her re-election constraint and giving her leeway to adopt policies closer to her ideal without decreasing her chances of victory. On the other hand, the victory of a more extreme opponent would inflict greater disutility on the incumbent, which may induce her to adopt a more moderate position in order to prevent the extremist from winning the election. Thus, policy motivation to account for the finding that incumbents take more moderate positions vis-à-vis extremist challengers, would require the sec-

[28] One possible, admittedly speculative, interpretation consistent with these results is that incumbents facing a moderate challenger recognize that there is little scope for winning votes on the persuasive margin by taking a more moderate position, such that they appeal to the more extreme part of their core constituency by taking a more extreme position to deter entry of a fringe candidate on their flank or to reduce the fringe candidate's vote share conditional on entry. Another, mutually not exclusive explanation could be that the nomination of an extremist causes the entry of a moderate third candidate that leads the incumbent to compete for votes at the center.

ond effect to dominate. This is the case when incumbents are risk-averse, i.e., when they prefer a moderate policy with certainty to a gamble for a policy close to their ideal with the risk of losing to a platform far from their ideal. But if policy-motivated incumbents exhibit risk aversion, they are by definition characterized by concave utility functions defined over the policy space. Hence, anything else equal, one would expect incumbents to adjust their platform differentially more in reaction to shifts in ex-ante distant opponent platforms. Yet, results presented in Table III.5 demonstrate the opposite, suggesting that policy-motivated incumbent's distaste for opponent extremists is not the mechanism behind their platform adjustment.

Another way to assess the empirical relevance of this mechanism resides in the observation that it requires incumbents to care about who is going represent "their" district. So, if incumbents' aversion to being represented by an opponent extremist drives positional adjustment, one would expect the reaction to extremist challengers to be more pronounced for incumbents who are locally rooted in their district. To test this hypothesis, I classify incumbents as locally rooted when they are born in the district they currently represent and attended high school there.[29]

TABLE III.7: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON DEPENDING ON INCUMBENTS' LOCAL ROOTS

	Incumbent Locally Rooted				Incumbent Not Locally Rooted			
PANEL A: EFFECT ON Δ ROLL CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.059	-0.106	0.030	-0.003	-0.503***	-0.480***	-0.952***	-0.936***
	(0.200)	(0.159)	(0.277)	(0.219)	(0.208)	(0.203)	(0.251)	(0.252)
	[0.839]	[0.627]	[0.811]	[0.872]	[0.006]	[0.008]	[0.000]	[0.000]
MSE-Optimal Bandwidth	0.079	0.077	0.091	0.097	0.075	0.075	0.083	0.083
Effective Observations	45	45	52	54	67	67	71	71
Control Mean	0.032	0.032	0.057	0.057	-0.005	-0.005	0.012	0.012
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.031	-0.038	0.001	-0.024	-0.107**	-0.112**	-0.219***	-0.223***
	(0.042)	(0.039)	(0.061)	(0.050)	(0.056)	(0.056)	(0.073)	(0.072)
	[0.605]	[0.523]	[0.870]	[0.805]	[0.039]	[0.030]	[0.002]	[0.002]
MSE-Optimal Bandwidth	0.075	0.078	0.081	0.115	0.097	0.093	0.090	0.090
Effective Observations	43	45	46	67	77	74	73	73
Control Mean	-0.010	-0.008	-0.008	-0.006	-0.033	-0.028	-0.028	-0.028
Polynomial	1	1	2	2	1	1	2	2
Observations	99	99	99	99	141	141	141	141
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for the locally rooted incumbents (Columns 1 – 4) and incumbents without local roots (Columns 5 – 8). Incumbents are defined as locally rooted if they either were born or attended high school in the district they currently represent. Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to incumbents seeking re-election in marginal districts. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

[29]Classifying incumbents depending on the city of residence would be problematic for two reasons. First, often the official residence of Members of Congress is Washington D.C., which does not exclude that they have local roots in the district they represent. Second, they can settle in the district they aim to represent for strategic purposes even if they do not have any local ties.

Table III.7 presents results from re-estimating equation 10 on the sub-samples of incumbents with and without local roots in marginal districts.[30] It is apparent that the nomination of an extremist challenger results in minimal to no positional adjustment among locally rooted incumbents (Columns 1 - 4), while incumbents whose hometown lies outside the district strongly react to their challengers' position (Columns 5 - 8). Although the size of point estimates for incumbents without local ties notably depends on the polynomial order of the assignment variable, the pattern is qualitatively robust across all specifications. Locally rooted incumbents who should suffer the most from an opponent extremist representing "their" district react differentially less to an extremist challenger compared to incumbents without local ties. This is once again the opposite of what one would expect if the mechanism behind platform adjustment was policy-motivated incumbents' aversion to opponent extremists.

Given the combined evidence that incumbents' reaction is weaker to changes in ex-ante more distant platforms and stronger when they have weaker personal ties to the district they represent, it is hard to rationalize their strategic platform adjustment by policy motivation alone. To be sure, while this evidence rules out that incumbents' distaste for extreme opponents accounts for the observed pattern that incumbents take more moderate positions in response to extremists, it does not preclude that incumbents have a policy objective. If incumbents have "character" (Kartik and McAfee, 2007), they may not care intrinsically about the challenger's platform but experience disutility (e.g., a psychic cost) from implementing policies that contrast with their ideal. However, this alone cannot drive the pattern we observe in the data. Incumbents who do not care intrinsically about the challenger's platform but simply trade off their own ideal with a more moderate position that secures re-election should take differentially more extreme positions vis-à-vis extremist challengers because extremists relax their re-election constraint.

5.2.3 Prior Office Experience and the Role of Valence

Another possibility is that incumbents adjust their platform differentially to extremist challengers not because they offer more extreme platforms compared to moderates, but because extremists and moderates differ in electorally relevant characteristics other than policy. Such non-policy characteristics that are valued by voters and thus grant an advantage to the candidate are commonly referred to as "valence" (Stokes, 1963). The valence dimension includes observable partisan, racial and gender attributes by which voters may discriminate, but also encompasses unobservable characteristics like competence or campaigning skill, which makes it difficult to measure valence directly. Yet, many if not most sources of valence advantages identified in the literature have been linked to incumbency, examples including name recognition and media presence (Prior, 2006), campaign funding (Fouirnaies and Hall, 2014), popularity due to constituency services and

[30] I focus on marginal districts to which incumbents' strategic adjustment is confined. Pooling observations from marginal and safe districts produces qualitatively consistent results with locally rooted incumbents tending to exhibit relatively stronger reactions compared to incumbents without local roots (see Appendix Table C.12).

pork barrel spending (Levitt and Snyder, 1997), credibility and reputations for integrity and competence on account of a verifiable track record (Bernhardt and Ingerman, 1985; McCurley and Mondak, 1995). Moreover, prior officeholders are not only of higher "innate" quality if voters select on valence, but they can also acquire competence through legislative experience (Padró i Miquel and Snyder Jr., 2006). Indeed, Kawai and Sunada, 2022 who estimate the valence of U.S. House candidates structurally show that incumbents score substantively higher on the valence dimension, consistent with political selection by valence. Since measuring the valence of primary candidates directly is impractical, I follow the standard approach in the empirical literature and interpret prior office experience as a proxy for valence.

Remember that barely nominated extremist challengers have significantly less office experience compared to barely nominated moderates, consistent with theoretical expectations that moderates need higher valence as a compensating differential to end up in close elections with extremists whose platform more likely aligns with the preferences of the primary electorate composed of staunch partisans (see Section 3.3, Figure III.5). Thus, if the opponent's valence has an independent impact on the incumbent's policy adjustment, my RD-estimates reflect the compound effect of a challenger with a more extreme platform and lower valence. What is still a valid estimate of the causal effect of an extremist challenger, understood as a bundle of extreme platforms and lower quality, may represent a biased estimate of the effect of challengers' policy platforms.

However, it is hard to sign the bias a priori considering that formal theory yields predictions in both directions. On the one hand, an increase in the incumbent's valence advantage, which increases her vote share given her policy position, makes moderation less necessary. Policymakers who find it costly to deviate from their own ideal (or their core supporters') can thus afford more extreme positions when running against a lower-valence challenger (Groseclose, 2001). Given that the overall effect of extremist challengers on incumbent extremism is negative and since extremists are of lower valence, this mechanism would induce bias in the opposite direction. Therefore, the RD-estimate would be an underestimate of the true effect of challenger platforms alone.

On the other hand, formal analysis has uncovered another mechanism by which a higher valence advantage can induce incumbents to moderate more. Specifically, introducing a valence dimension to the canonical Downsian model with vote-maximizing candidates who are uncertain about voter preferences leads to chase-and-evade incentives that are increasing in the size of a candidate's valence advantage. Since the valence-advantaged candidate benefits from raising the salience of the valence dimension relative to the policy dimension, the advantaged candidate has an incentive to mimic the disadvantaged candidate, while the disadvantaged candidate is encouraged to differentiate himself from the advantaged candidate by taking a more extreme position (Aragonès and Palfrey, 2002; Hummel, 2010; Aragonès and Xefteris, 2012). This mechanism is thus consistent with valence-advantaged incumbents moderating differentially more against extremists not because of their platform but because of extremists' lower valence compared to moderates. Thus, if valence-induced chase-and-evade incentives are at work and increasing in the size of the valence advantage, the RD-estimate would be a downward biased estimate of the extremist platforms' true effect, with the true effect being closer to zero or even positive.

While I cannot fully disentangle challenger platforms from challenger valence, it is possible to gauge the sign of the bias induced by valence using potential challengers' prior office experience as a proxy for valence. To assess the sign of the bias, I divide the sample of marginal districts in two subsets: the first including only observations where *none* of the two potential challengers has any prior office experience at the federal or state level, the second including only observations where the moderate primary candidate has prior office experience and the extremist has not.[31] Estimating equation 10 on the on the subsample with experienced moderates and inexperienced extremists yields an RD-estimate of the compound effect of i) extremists' more extreme platform, ii) *and* extremists' lower valence to the extent that valence is captured by office experience. In contrast, estimating equation 10 on the subsample with both primary candidates inexperienced, the RD-estimate primarily reflects the impact of different challenger platforms, plus a residual valence differential uncorrelated with prior office experience. Provided that prior office experience accounts for substantial part of unobservable valence, the comparison of the RD-estimates obtained from these subsets allows to gauge the sign of the bias valence induces on the estimated effect of challenger platforms. If an increasing valence advantage leads incumbents to adopt more extreme positions, one would expect larger negative discontinuities in the subsample with equally inexperienced primary candidates, consistent with valence inducing an upward bias. Conversely, larger negative discontinuities in the subsample of experienced moderate primary candidates would be indicative for the presence of chase-and-evade incentives and a downward bias.

[31] I drop one single case in which the extremist is experienced and the moderate is not. Note that the sample is restricted to electoral cycles from 1996 onward because of data availability, see Section 2.2.

Table III.8: The Effects of an Extremist Challenger on Roll Call Extremism and Party Loyalty Depending on Potential Challengers' Prior Office Experience

	All Primary Candidates Inexperienced				Moderate Candidate Experienced			
Panel A: Effect on Δ Roll Call Extremism (std.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.327**	-0.415***	-0.553**	-0.710***	-0.098	-0.009	0.038	0.129
	(0.174)	(0.175)	(0.216)	(0.206)	(0.238)	(0.255)	(0.276)	(0.284)
	[0.039]	[0.007]	[0.011]	[0.001]	[0.887]	[0.809]	[0.730]	[0.507]
MSE-Optimal Bandwidth	0.068	0.067	0.082	0.081	0.064	0.064	0.080	0.081
Effective Observations	40	40	48	47	33	33	40	40
Control Mean	-0.024	-0.024	-0.017	-0.017	-0.014	-0.014	-0.020	-0.020
Panel B: Effect on Δ Party Loyalty								
	-0.063*	-0.056*	-0.108**	-0.107**	-0.053	-0.048	-0.055	-0.047
	(0.036)	(0.035)	(0.041)	(0.041)	(0.057)	(0.060)	(0.067)	(0.070)
	[0.053]	[0.071]	[0.014]	[0.014]	[0.414]	[0.496]	[0.477]	[0.582]
MSE-Optimal Bandwidth	0.100	0.108	0.091	0.092	0.067	0.067	0.081	0.083
Effective Observations	58	65	51	51	34	34	40	41
Control Mean	-0.013	-0.013	-0.015	-0.015	-0.012	-0.012	-0.011	-0.014
Polynomial	1	1	2	2	1	1	2	2
Observations	112	112	112	112	70	70	70	70
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on the changes in the incumbent's standardized *roll call extremism* (Panel A) and *party loyalty* (Panel B) in the post-primary period with respect to the pre-primary period, separately for incumbents whose potential challengers are both inexperienced (Columns 1 – 4) and incumbents whose moderate potential challenger has held elected office at the state or federal level while the extremist has not (Columns 5 – 8). Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for the level of the pre-primary outcome. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to incumbents seeking re-election in marginal districts from the 1996 election cycle onward. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Results reported in Table III.8 provide evidence in favor of the former interpretation. An inexperienced extremist challenger significantly decreases incumbent's *roll call extremism* by over 0.3 standard deviations compared to an unexperienced moderate challenger (Panel A, Columns 1 - 4), with effect sizes comparable to coefficients estimated on the whole sample of marginal districts (Table III.4, Panel A, Columns 1 - 4), suggesting that incumbents' *reaction to policy platforms* and not their response to valence differentials drives the result. In contrast, evaluated against a counterfactual moderate challenger with prior office experience, an inexperienced extremist has no discernible impact on the incumbent's *roll call extremism* (Table III.8, Panel A, Columns 5 - 8), consistent with incumbents exploiting an increasing valence advantage to adopt more extreme positions, which leads to an upward biased estimate of the true effect of challenger platforms. As for *party loyalty*, the pattern is qualitatively similar with effects tending to be larger among unexperienced potential challengers (Panel B, Columns 1 - 4) compared to the subsample where moderates have prior office experience (Panel B, Columns 5 - 8), although the contrast is less clear-cut.

Overall, results of this heterogeneity analysis suggest that incumbents adjust their position to challengers in reaction to the challengers' *platform*, supporting the interpretation of policy platforms as *strategic complements*. Previously presented effects of extremist challengers on incumbent positions that do not account for the valence differential between moderates and extremists are, if anything, upward biased estimates of the effect of challengers' policy platforms. Hence, they represent an underestimate of challenger platforms true effect on incumbents' position, with the true differential adjustment to extremists' *platform* likely being larger in magnitude and more negative because the valence differential tends to work in the opposite direction.

Finally, the evidence is inconsistent with chase-and-evade incentives as a mechanism explaining strategic complementarity of policy platforms. In principle, it could be that valence-advantaged incumbents moderate differentially more in response to extremist platforms because they aim to mimic the platform of the valence-disadvantaged opponent, underscoring their advantage on the valence dimension. However, the predictions underlying this mechanism are inconsistent with at least three pieces of evidence. Formal models predicting chase-and-evade incentives suggest these incentives are stronger when the valence advantage increases. Yet, incumbents do not react in safe districts where they are advantaged the most, with a partisan advantage on top of the incumbency advantage. Second, as shown in Table III.8, an increase in the incumbent's valence advantage by the nomination of an inexperienced extremist, rather than an experienced moderate challenger, does not lead to a stronger reaction of the incumbent; if anything, it leads to a weaker reaction compared to the case where both potential challengers are inexperienced. Third, though mimicking behavior is consistent with incumbents moderating *differentially* more against extremists, it is inconsistent with graphical evidence suggesting that part of the differential effect is due to incumbents differentiating their position from moderate challengers (see Figure III.7). At least for incumbents studied in this paper, chase-and-evade incentives are thus of little empirical relevance. Neither do incumbents react to changes in their valence advantage as would be expected if chase-and-evade incentives were present, nor do the magnitude and the direction of adjustment correspond to levels of incumbents' valence advantage as predicted by formal theory.

6 Concluding Remarks

Providing the first credibly causal evidence that incumbent politicians strategically alter implemented policy in response to opponent candidates' platforms, this paper sheds new light on the mechanisms by which electoral competition shapes public policy. Periodic elections are more than a pure selection mechanism that alters public policy by replacing incumbent candidates with challengers of a different political orientation. Representatives are not ideologically rigid citizen-candidates who, undeterred by electoral pressure, steadfastly adhere to their own convictions. Rather pragmatically, they take into account the electoral consequences of their decisions, and commit to new policy positions depending on their challenger's platform. The role of challengers is therefore not limited to replacing incumbent policy that is unpopular. Non-incumbent challengers affect public policy of elected officeholders. Provided that challengers' support in the electorate is large enough to constitute a credible threat to the incumbent's re-election bid, they can pull incumbents' policy toward the ideal of the opposition by taking more extreme positions.

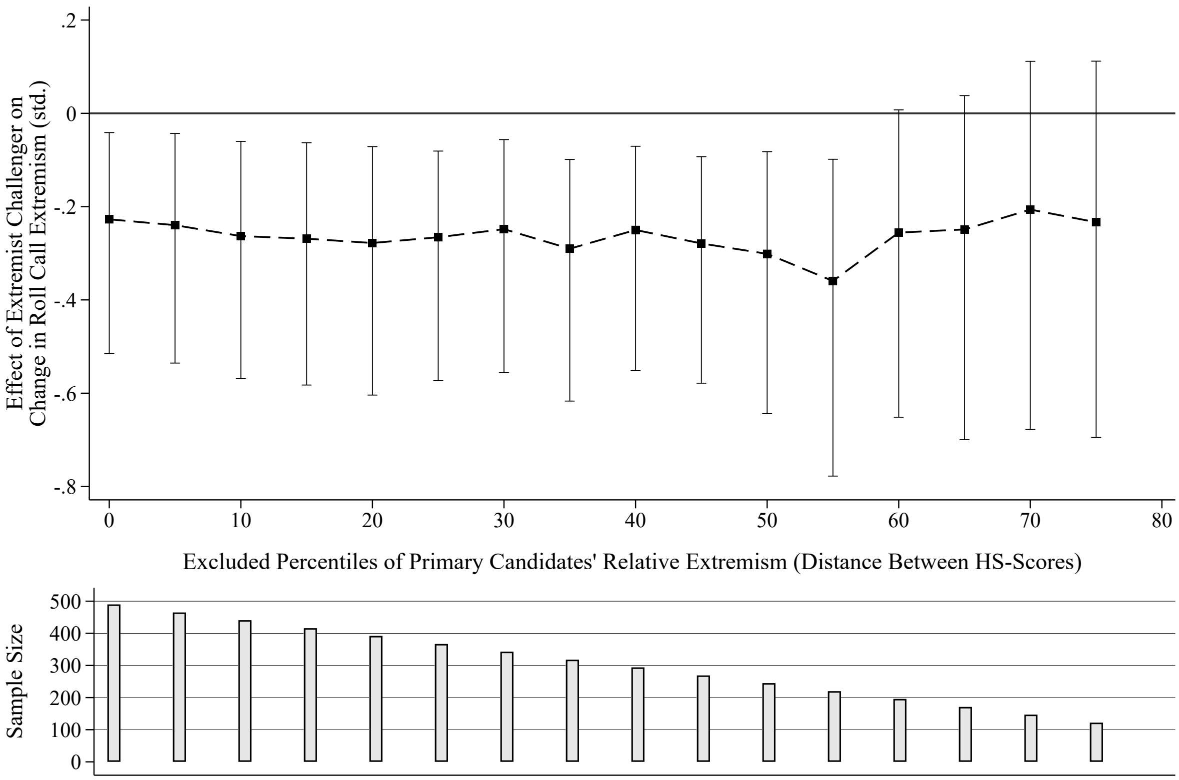
This paper's finding that incumbents adjust their position, but only if they seek re-election in a competitive district supports the central notion of the Downsian paradigm that electoral competition constrains incumbent's policy, which is a key tenet of accountability in representative democracies. Providing evidence that incumbents are responsive to electoral incentives, and compromise to more moderate policies when competing against extremist challengers, this paper's results also offer lessons for constitutional design. In the U.S. context, efforts to enhance competition by depoliticizing the redistricting process may improve government responsiveness and reduce polarization in legislatures, whereas term limits that sacrifice accountability of incumbent legislators for higher electoral turnover may have the opposite effect.

While the finding that incumbents *do adjust* their position strategically to challengers is consistent with the Downsian paradigm, the *direction* of adjustment is inconsistent with strategic behavior underlying the convergence mechanism prescribed by the canonical Downsian two-party model with full voter turnout. Instead of converging toward the center, which would require incumbents to take more moderate positions in response to moderate challengers, incumbents take more moderate positions vis-à-vis extremist challengers. I provide evidence that this pattern of adjustment is driven by incumbents' reaction to challengers' policy positions and not by the valence advantage of moderate challengers over extremists, suggesting that policy platforms are strategic complements in electoral competition.

The exploration of mechanisms behind strategic complementarity of policy platforms indicates that candidates grapple with a trade-off between attracting swing voters in the center and mobilizing core supporters. In the strategic decision to adjust policy to challenger platforms, incumbents not only focus on voters they could persuade, but also consider votes they could lose with more moderate policy. Preliminary evidence suggests that incumbents' reaction to main party challengers is conditioned by the entry of third candidates. Uncovering how third candidates mediate strategic position-taking between dominant candidates, I suspect, would be a promising avenue for further research, as would the question of whether re-elected incumbents keep committed to adjusted positions after elections.

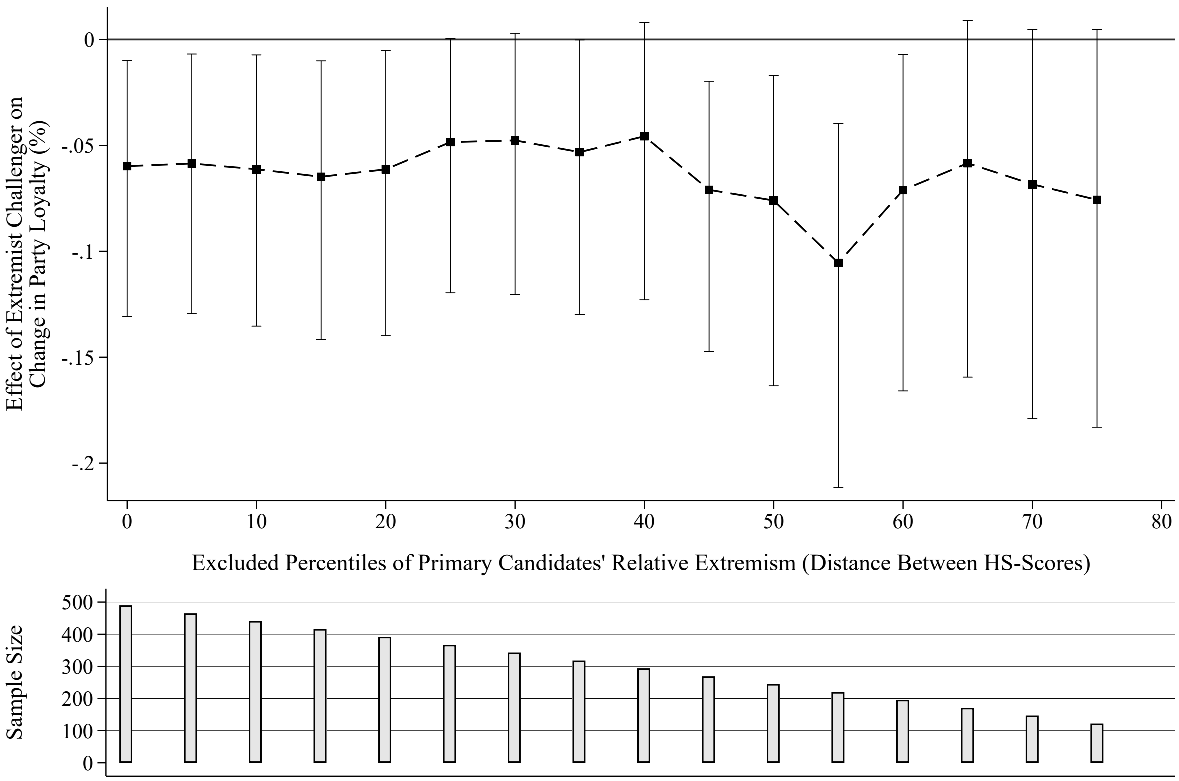
Appendix C: Additional Figures and Tables on Strategic Policy Responsiveness

FIGURE C.1: EFFECT OF EXTREMIST CHALLENGER ON INCUMBENT'S ROLL CALL EXTREMISM DEPENDING ON RELATIVE EXTREMISM OF PRIMARY CANDIDATES



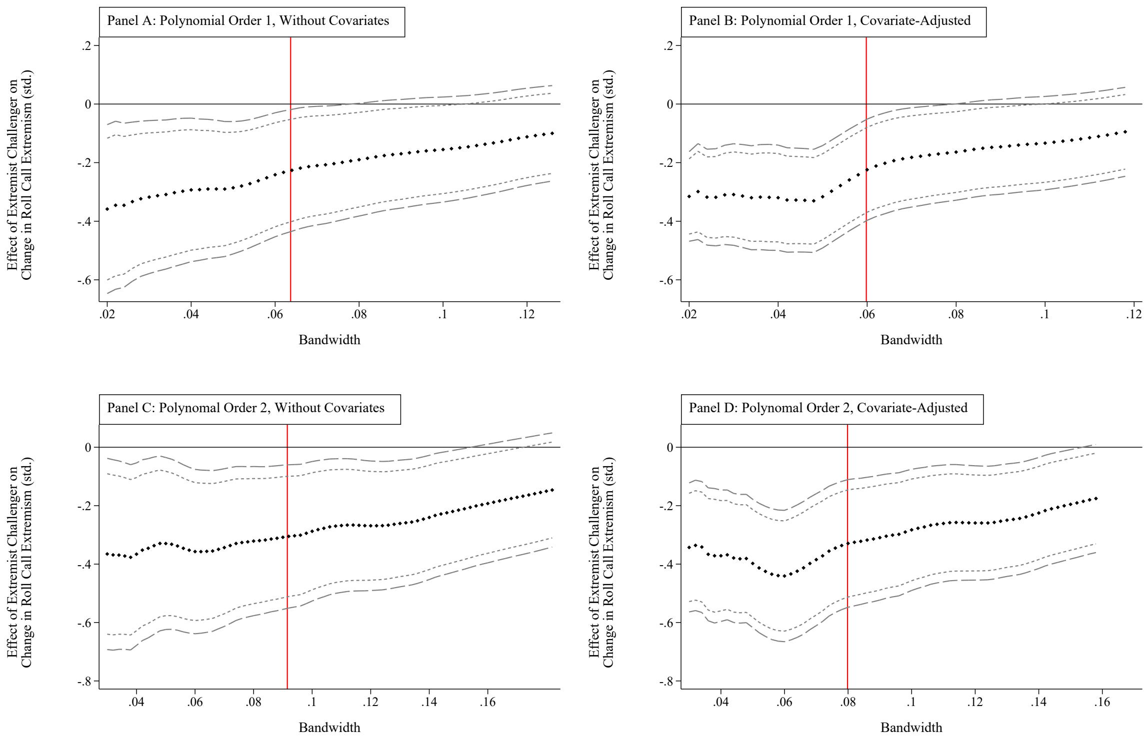
Notes: The Figure plots discontinuity estimates (triangles) of the effect of an extremist challenger on the incumbent's change in standardized *roll call extremism* between the pre-primary to the post-primary period along with bias-adjusted 95% confidence intervals (spikes) accounting for clustering at the incumbent-level. Estimates are obtained from local linear specifications of equation 10 with MSE-optimal bandwidths and triangular kernels. Estimates are based subsamples that successively exclude percentiles with the smallest distance between primary candidates' Hall-Snyder scores as indicated on the x-axis. The bottom panel indicates the size of the subsamples underlying each estimate.

FIGURE C.2: EFFECT OF EXTREMIST CHALLENGER IN INCUMBENT'S PARTY LOYALTY DEPENDING ON RELATIVE EXTREMISM OF PRIMARY CANDIDATES



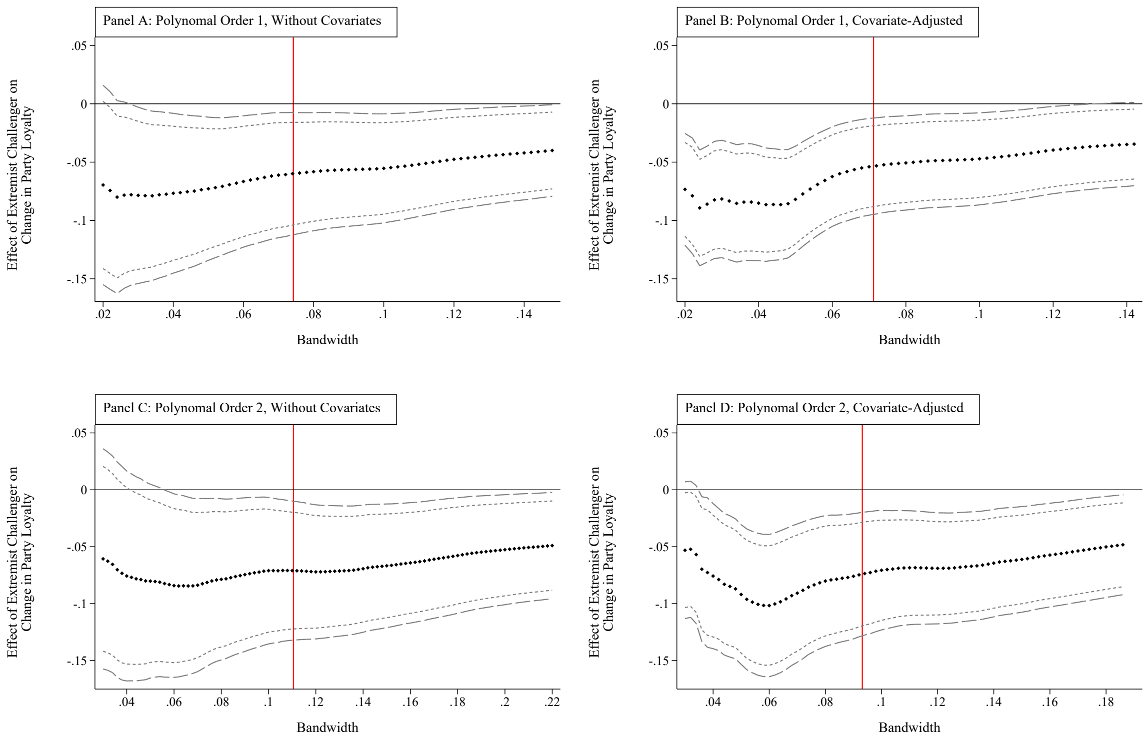
Notes: The Figure plots discontinuity estimates (triangles) of the effect of an extremist challenger on the incumbent's change in *party loyalty* between the pre-primary to the post-primary period along with bias-adjusted 95% confidence intervals (spikes) accounting for clustering at the incumbent-level. Estimates are obtained from local linear specifications of equation 10 with MSE-optimal bandwidths and triangular kernels. Estimates are based subsamples that successively exclude percentiles with the smallest distance between primary candidates' Hall-Snyder scores as indicated on the x-axis. The bottom panel indicates the size of the subsamples underlying each estimate.

FIGURE C.3: EFFECT OF EXTREMIST CHALLENGER ON INCUMBENT'S ROLL-CALL EXTREMISM DEPENDING ON DIFFERENT BANDWIDTHS



Notes: The Figure plots discontinuity estimates (black dots) of the effect of an extremist challenger on the incumbent's change in standardized *roll call extremism* between the pre-primary to the post-primary period for different bandwidths ranging from 0.02 to twice the optimal bandwidth for local linear specifications (Panels A and B) and from 0.03 to twice the optimal badwidth local quadratic specifications (Panels C and D) of equation 10. The MSE-optimal bandwidth is indicated with a red line. 95% (dashed grey lines) and 90% (dotted grey lines) confidence intervals accounting for clustering at the incumbent-level. Esimates in Panels C and D are adjusted for covariates listed in Figure III.4, Panels A and B, exluding the distance between candidates' Hall-Snyder scores.

FIGURE C.4: EFFECT OF EXTREMIST CHALLENGER ON INCUMBENT'S PARTY LOYALTY DEPENDING ON DIFFERENT BANDWIDTHS



Notes: The Figure plots discontinuity estimates (black dots) of the effect of an extremist challenger on the incumbent's change in *party loyalty* between the pre-primary to the post-primary period for different bandwidths ranging from 0.02 to twice the optimal bandwidth for local linear specifications (Panels A and B) and from 0.03 to twice the optimal badwidth local quadratic specifications (Panels C and D) of equation 10. The MSE-optimal bandwidth is indicated with a red line. 95% (dashed grey lines) and 90% (dotted grey lines) confidence intervals accounting for clustering at the incumbent-level. Esimates in Panels C and D are adjusted for covariates listed in Figure III.4, Panels A and B, exluding the distance between candidates' Hall-Snyder scores.

TABLE C.1: THE EFFECT OF AN EXTREMIST CHALLENGER ON THE CHANGE IN INCUMBENTS' (STANDARDIZED) ROLL CALL EXTREMISM AND PARTY LOYALTY: ROBUSTNESS TO HALL-SNYDER SCORES BASED ON MINIMUM THRESHOLD OF 10 TRANSACTIONS

	Δ Roll Call Extremism (std.)			Δ Party Loyalty (%)		
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)
	-0.190*	-0.178**	-0.178**	-0.053*	-0.057***	-0.058***
	(0.124)	(0.084)	(0.083)	(0.027)	(0.021)	(0.021)
	[0.089]	[0.038]	[0.036]	[0.052]	[0.006]	[0.007]
MSE-Optimal Bandwidth	0.070	0.062	0.062	0.088	0.070	0.069
Effective Observations	122	110	110	142	121	119
Control Mean	0.002	0.022	0.022	-0.012	-0.010	-0.010
PANEL B: LOCAL QUADRATIC						
	-0.254	-0.263**	-0.265**	-0.063*	-0.066**	-0.066**
	(0.150)	(0.109)	(0.110)	(0.033)	(0.027)	(0.027)
	[0.111]	[0.038]	[0.038]	[0.060]	[0.020]	[0.034]
MSE-Optimal Bandwidth	0.096	0.086	0.087	0.121	0.101	0.096
Effective Observations	146	140	140	179	153	144
Control Mean	0.009	0.011	0.011	-0.010	-0.009	-0.013
Observations	278	274	274	278	274	274
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Tables III.1 and III.2 to an alternative calculation of Hall-Snyder scores that excludes donors who donate to less than 10 distinct candidates and candidates who receive contributions from less than 15 distinct donors. The outcome variables are the changes in the incumbent's standardized *roll call extremism* (Columns 1 – 3) and *party loyalty* (Columns 4 – 6) in the post-primary period with respect to the pre-primary period. All other notes as under Tables III.1 and III.2. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.2: THE EFFECT OF AN EXTREMIST CHALLENGER ON THE CHANGE IN INCUMBENTS' (STANDARDIZED) ROLL CALL EXTREMISM AND PARTY LOYALTY: ROBUSTNESS TO HALL-SNYDER SCORES BASED ON MINIMUM THRESHOLD OF 15 TRANSACTIONS

	Δ Roll Call Extremism (std.)			Δ Party Loyalty (%)		
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)
	-0.246	-0.246**	-0.242**	-0.082**	-0.069***	-0.072**
	(0.161)	(0.098)	(0.100)	(0.034)	(0.025)	(0.025)
	[0.120]	[0.013]	[0.019]	[0.011]	[0.008]	[0.010]
MSE-Optimal Bandwidth	0.070	0.057	0.057	0.070	0.057	0.057
Effective Observations	82	71	71	82	71	71
Control Mean	-0.000	0.026	0.026	-0.016	-0.008	-0.008
PANEL B: LOCAL QUADRATIC						
	-0.287	-0.345**	-0.356**	-0.099**	-0.096***	-0.096**
	(0.196)	(0.115)	(0.120)	(0.042)	(0.032)	(0.033)
	[0.196]	[0.011]	[0.011]	[0.024]	[0.008]	[0.013]
MSE-Optimal Bandwidth	0.094	0.069	0.061	0.091	0.074	0.064
Effective Observations	92	79	72	92	83	74
Control Mean	0.005	-0.000	0.023	-0.018	-0.016	-0.010
Observations	170	166	166	170	166	166
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Tables III.1 and III.2 to an alternative calculation of Hall-Snyder scores that excludes donors who donate to less than 15 distinct candidates and candidates who receive contributions from less than 15 distinct donors. The outcome variables are the changes in the incumbent's standardized *roll call extremism* (Columns 1 – 3) and *party loyalty* (Columns 4 – 6) in the post-primary period with respect to the pre-primary period. All other notes as under Tables III.1 and III.2. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.3: THE EFFECT OF AN EXTREMIST CHALLENGER ON THE CHANGE IN INCUMBENTS' ROLL CALL EXTREMISM: ROBUSTNESS TO DIFFERENT TIME WINDOWS PRIOR TO GENERAL ELECTIONS

	Local Linear			Local Quadratic		
PANEL A: $<$ 45 DAYS BEFORE GENERAL ELECTION	(1)	(2)	(3)	(4)	(5)	(6)
	-0.221	-0.209	-0.206	-0.196	-0.221	-0.214
	(0.143)	(0.136)	(0.135)	(0.186)	(0.170)	(0.192)
	[0.182]	[0.200]	[0.177]	[0.381]	[0.239]	[0.351]
MSE-Optimal Bandwidth	0.086	0.074	0.074	0.119	0.116	0.094
Effective Observations	196	172	173	260	249	199
Control Mean	0.046	0.040	0.042	0.067	0.070	0.044
PANEL B: $<$ 60 DAYS BEFORE GENERAL ELECTION						
	-0.252**	-0.248**	-0.248**	-0.301**	-0.340**	-0.337**
	(0.106)	(0.104)	(0.103)	(0.132)	(0.137)	(0.135)
	[0.014]	[0.012]	[0.011]	[0.025]	[0.023]	[0.021]
MSE-Optimal Bandwidth	0.079	0.071	0.071	0.103	0.091	0.093
Effective Observations	210	183	183	260	227	229
Control Mean	-0.025	-0.026	-0.026	-0.007	-0.016	-0.016
PANEL C: $<$ 90 DAYS BEFORE GENERAL ELECTION						
	-0.241**	-0.263***	-0.262***	-0.298**	-0.371***	-0.371***
	(0.106)	(0.101)	(0.100)	(0.129)	(0.128)	(0.127)
	[0.015]	[0.006]	[0.006]	[0.030]	[0.006]	[0.006]
MSE-Optimal Bandwidth	0.071	0.065	0.065	0.099	0.085	0.086
Effective Observations	185	172	173	247	218	218
Control Mean	-0.021	-0.024	-0.023	-0.013	-0.013	-0.013
PANEL D: $<$ 120 DAYS BEFORE GENERAL ELECTION						
	-0.227**	-0.228***	-0.226***	-0.306**	-0.331***	-0.330***
	(0.106)	(0.088)	(0.088)	(0.125)	(0.111)	(0.111)
	[0.021]	[0.006]	[0.006]	[0.018]	[0.004]	[0.004]
MSE-Optimal Bandwidth	0.064	0.059	0.060	0.092	0.079	0.080
Effective Observations	169	162	162	234	206	208
Control Mean	-0.015	-0.014	-0.014	-0.015	-0.018	-0.017
PANEL E: $<$ 150 DAYS BEFORE GENERAL ELECTION						
	-0.280***	-0.298***	-0.295***	-0.323**	-0.330***	-0.328***
	(0.111)	(0.090)	(0.090)	(0.125)	(0.112)	(0.112)
	[0.008]	[0.001]	[0.001]	[0.015]	[0.005]	[0.005]
MSE-Optimal Bandwidth	0.054	0.052	0.052	0.090	0.078	0.078
Effective Observations	152	145	146	230	206	206
Control Mean	-0.035	-0.035	-0.032	-0.020	-0.025	-0.025
PANEL F: $<$ 180 DAYS BEFORE GENERAL ELECTION						
	-0.313***	-0.319***	-0.313***	-0.361***	-0.365***	-0.362***
	(0.116)	(0.095)	(0.095)	(0.131)	(0.118)	(0.117)
	[0.005]	[0.001]	[0.001]	[0.008]	[0.003]	[0.003]
MSE-Optimal Bandwidth	0.055	0.053	0.053	0.090	0.078	0.078
Effective Observations	152	147	148	230	206	206
Control Mean	-0.035	-0.029	-0.033	-0.017	-0.021	-0.021
PANEL G: ALL DAYS BEFORE GENERAL ELECTION						
	-0.268**	-0.267***	-0.263***	-0.319**	-0.313***	-0.312**
	(0.117)	(0.096)	(0.096)	(0.133)	(0.119)	(0.119)
	[0.016]	[0.003]	[0.004]	[0.022]	[0.010]	[0.010]
MSE-Optimal Bandwidth	0.055	0.053	0.054	0.091	0.079	0.079
Effective Observations	154	149	150	232	206	206
Control Mean	-0.029	-0.031	-0.032	-0.016	-0.018	-0.018
Observations	490	479	479	490	479	479
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Table III.1 to estimates of incumbents' post-primary *roll call extremism* based on post-primary roll calls held in different time windows preceding the general election. Instead of focusing on post-primary roll calls held within the last 120 days prior to general elections, each panel considers alternative thresholds ranging from the last 45 days (which restricts attention to a uniform time window for all incumbents by considering only roll calls held after the latest of all primary elections in my sample) to 273 (i.e., which for every incumbent considers all post-primary roll calls, although they are be held in different time periods prior to general elections depending on the date of the opponent party's primary). Columns 1 – 3 report estimates from local linear, Columns 4 – 6 from local qaudratic specifications of of equation 10. The outcome variable is the change in the incumbent's standardized *roll call extremism* in the post-primary period with respect to the pre-primary period. All other notes as under Table III.1. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

Table C.4: The Effect of an Extremist Challenger on the Change in Incumbents' Party Loyalty: Robustness to Different Time Windows Prior to General Elections

	Local Linear			Local Quadratic		
Panel A: < 45 Days Before General Election	(1)	(2)	(3)	(4)	(5)	(6)
	-0.074*	-0.065*	-0.064*	-0.083	-0.081**	-0.080*
	(0.038)	(0.033)	(0.033)	(0.046)	(0.039)	(0.039)
	[0.089]	[0.085]	[0.090]	[0.126]	[0.040]	[0.052]
MSE-Optimal Bandwidth	0.083	0.072	0.071	0.131	0.129	0.124
Effective Observations	191	165	157	275	268	259
Control Mean	-0.006	-0.008	-0.006	-0.005	-0.005	-0.005
Panel B: < 60 Days Before General Election						
	-0.058*	-0.057**	-0.058**	-0.065*	-0.065**	-0.068**
	(0.028)	(0.025)	(0.024)	(0.032)	(0.030)	(0.029)
	[0.051]	[0.019]	[0.015]	[0.053]	[0.042]	[0.030]
MSE-Optimal Bandwidth	0.087	0.078	0.077	0.144	0.115	0.115
Effective Observations	224	206	204	347	283	286
Control Mean	-0.011	-0.010	-0.010	-0.008	-0.010	-0.009
Panel C: < 90 Days Before General Election						
	-0.058**	-0.055**	-0.056**	-0.068*	-0.064*	-0.068**
	(0.029)	(0.025)	(0.024)	(0.033)	(0.031)	(0.030)
	[0.046]	[0.024]	[0.017]	[0.052]	[0.055]	[0.039]
MSE-Optimal Bandwidth	0.085	0.077	0.076	0.134	0.109	0.108
Effective Observations	224	203	200	323	271	267
Control Mean	-0.011	-0.011	-0.012	-0.008	-0.011	-0.011
Panel D: < 120 Days Before General Election						
	-0.060**	-0.053***	-0.054***	-0.071**	-0.072**	-0.074**
	(0.027)	(0.021)	(0.021)	(0.031)	(0.028)	(0.028)
	[0.023]	[0.009]	[0.007]	[0.029]	[0.015]	[0.011]
MSE-Optimal Bandwidth	0.074	0.071	0.071	0.111	0.093	0.093
Effective Observations	202	184	184	279	229	230
Control Mean	-0.023	-0.024	-0.024	-0.020	-0.021	-0.021
Panel E: < 150 Days Before General Election						
	-0.059**	-0.054***	-0.055***	-0.071**	-0.073***	-0.074***
	(0.026)	(0.020)	(0.020)	(0.030)	(0.026)	(0.026)
	[0.021]	[0.006]	[0.004]	[0.024]	[0.009]	[0.007]
MSE-Optimal Bandwidth	0.069	0.063	0.063	0.107	0.090	0.090
Effective Observations	183	167	166	272	224	224
Control Mean	-0.030	-0.026	-0.026	-0.027	-0.028	-0.028
Panel F: < 180 Days Before General Election						
	-0.055**	-0.050**	-0.050**	-0.067**	-0.070**	-0.071**
	(0.026)	(0.021)	(0.021)	(0.030)	(0.026)	(0.026)
	[0.031]	[0.012]	[0.010]	[0.033]	[0.014]	[0.012]
MSE-Optimal Bandwidth	0.070	0.065	0.065	0.108	0.090	0.091
Effective Observations	185	171	172	273	225	228
Control Mean	-0.030	-0.030	-0.031	-0.029	-0.029	-0.029
Panel G: All Days Before General Election						
	-0.049*	-0.043**	-0.043**	-0.060*	-0.063**	-0.064**
	(0.026)	(0.021)	(0.021)	(0.030)	(0.027)	(0.027)
	[0.052]	[0.030]	[0.027]	[0.053]	[0.024]	[0.022]
MSE-Optimal Bandwidth	0.070	0.064	0.064	0.106	0.090	0.091
Effective Observations	185	169	169	270	225	227
Control Mean	-0.028	-0.026	-0.026	-0.026	-0.026	-0.027
Observations	490	479	479	490	479	479
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Table III.2 to estimates of incumbents' post-primary *party loyalty* based on post-primary roll calls held in different time windows preceding the general election. Instead of focusing on post-primary roll calls held within the last 120 days prior to general elections, each panel considers alternative thresholds ranging from the last 45 days (which restricts attention to a uniform time window for all incumbents by considering only roll calls held after the latest of all primary elections in my sample) to 273 (i.e., which for every incumbent considers all post-primary roll calls, although they are be held in different time periods prior to general elections depending on the date of the opponent party's primary). Columns 1 – 3 report estimates from local linear, Columns 4 – 6 from local qaudratic specifications of of equation 10. The outcome variable is the change in the incumbent's *party loyalty* in the post-primary period with respect to the pre-primary period. All other notes as under Table III.2. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.5: THE EFFECTS OF AN EXTREMIST CHALLENGER ON THE CHANGE IN INCUMBENT'S (STANDARDIZED) ROLL CALL EXTREMISM AND PARTY LOYALTY: ROBUSTNESS TO HIGHER-ORDER POLYNOMIALS

	Δ Roll Call Extremism (std.)			Δ Party Loyalty (%)		
PANEL A: CUBIC POLYNOMIAL	(1)	(2)	(3)	(4)	(5)	(6)
	-0.321**	-0.365***	-0.364***	-0.079**	-0.076**	-0.080**
	(0.136)	(0.124)	(0.125)	(0.034)	(0.032)	(0.032)
	[0.029]	[0.007]	[0.007]	[0.028]	[0.030]	[0.022]
MSE-Optimal Bandwidth	0.129	0.113	0.113	0.160	0.131	0.131
Effective Observations	317	277	279	372	312	312
Control Mean	0.031	0.022	0.024	-0.009	-0.011	-0.011
PANEL B: QUARTIC POLYNOMIAL						
	-0.333**	-0.378***	-0.371***	-0.077*	-0.080**	-0.082**
	(0.149)	(0.130)	(0.131)	(0.042)	(0.034)	(0.036)
	[0.043]	[0.005]	[0.008]	[0.093]	[0.031]	[0.037]
MSE-Optimal Bandwidth	0.153	0.168	0.162	0.161	0.182	0.165
Effective Observations	365	375	368	375	399	371
Control Mean	0.026	0.028	0.027	-0.009	-0.011	-0.009
Observations	490	479	479	490	479	479
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Tables III.1 and III.2 to higher polynomial orders 3 (Panel A) and 4 (Panel B) of the assignment variable. The outcome variables are the changes in the incumbent's standardized *roll call extremism* (Columns 1 – 3) and *party loyalty* (Columns 4 – 6) in the post-primary period with respect to the pre-primary period. All other notes as under Tables III.1 and III.2. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.6: THE EFFECT OF AN EXTREMIST CHALLENGER ON THE CHANGE IN INCUMBENTS' (STANDARDIZED) ROLL CALL EXTREMISM: ROBUSTNESS TO ALTERNATIVE KERNEL WEIGHTS

	Epanechnikov Kernel			Uniform Kernel		
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)
	-0.216**	-0.222**	-0.219**	-0.174*	-0.148*	-0.151*
	(0.107)	(0.093)	(0.093)	(0.099)	(0.096)	(0.097)
	[0.029]	[0.010]	[0.011]	[0.064]	[0.071]	[0.069]
MSE-Optimal Bandwidth	0.062	0.058	0.059	0.068	0.057	0.057
Effective Observations	168	158	159	183	156	156
Control Mean	0.005	0.005	0.011	-0.010	0.002	0.002
PANEL B: LOCAL QUADRATIC						
	-0.298**	-0.312**	-0.310**	-0.305**	-0.279**	-0.232*
	(0.127)	(0.116)	(0.117)	(0.129)	(0.124)	(0.122)
	[0.023]	[0.011]	[0.012]	[0.013]	[0.036]	[0.071]
MSE-Optimal Bandwidth	0.090	0.080	0.080	0.078	0.071	0.072
Effective Observations	231	208	209	209	183	192
Control Mean	0.002	-0.003	-0.005	-0.005	-0.010	-0.018
Observations	490	479	479	490	479	479
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Table III.1 to weights alternative to the triangular kernel, reporting estimates based on the Epanechnikov kernel (Columns 1 – 3) and the rectangular kernel (Columns 4 – 5). The outcome variable is the change in the incumbent's standardized *roll call extremism* in the post-primary period with respect to the pre-primary period. All other notes as under Table III.1. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.7: THE EFFECT OF AN EXTREMIST CHALLENGER ON CHANGE INCUMBENTS' PARTY LOYALTY: ROBUSTNESS TO ALTERNATIVE KERNEL WEIGHTS

	Epanechnikov Kernel			Uniform Kernel		
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)
	-0.058**	-0.049**	-0.051**	-0.050**	-0.041**	-0.048**
	(0.027)	(0.022)	(0.022)	(0.028)	(0.024)	(0.023)
	[0.028]	[0.018]	[0.013]	[0.047]	[0.048]	[0.023]
MSE-Optimal Bandwidth	0.070	0.069	0.069	0.065	0.057	0.058
Effective Observations	185	181	181	175	156	157
Control Mean	-0.012	-0.012	-0.012	-0.010	-0.009	-0.009
PANEL B: LOCAL QUADRATIC						
	-0.069**	-0.066**	-0.070**	-0.065**	-0.067**	-0.066**
	(0.031)	(0.028)	(0.028)	(0.032)	(0.029)	(0.030)
	[0.029]	[0.025]	[0.017]	[0.049]	[0.022]	[0.027]
MSE-Optimal Bandwidth	0.109	0.095	0.095	0.094	0.082	0.078
Effective Observations	276	231	231	236	213	206
Control Mean	-0.013	-0.014	-0.014	-0.014	-0.012	-0.011
Observations	490	479	479	490	479	479
Covariates	N	Y	Y	N	Y	Y
Outcome Before Primary	N	N	Y	N	N	Y

Notes: The Table probes robustness of results reported in Table III.2 to weights alternative to the triangular kernel, reporting estimates based on the Epanechnikov kernel (Columns 1 – 3) and the rectangular kernel (Columns 4 – 5). The outcome variable is the change in the incumbent's *party loyalty* in the post-primary period with respect to the pre-primary period. All other notes as under Table III.2. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.8: THE EFFECT OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM DEPENDING ON DISTRICT COMPETITIVENESS BY QUARTILES OF INCUMBENT ELECTORAL STRENGTH

	Marginal District (1^{st} Quartile)		Marginal District (2^{nd} Quartile)		Safe District (3^{rd} Quartile)		Safe District (4^{th} Quartile)	
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.564**	-0.564**	-0.179	-0.171	0.011	0.004	-0.060	-0.054
	(0.260)	(0.272)	(0.210)	(0.212)	(0.178)	(0.167)	(0.184)	(0.189)
	[0.024]	[0.041]	[0.271]	[0.293]	[0.932]	[0.998]	[0.660]	[0.738]
MSERD-Optimal Bandwidth	0.073	0.067	0.070	0.071	0.081	0.106	0.078	0.074
Effective Observations	59	52	47	47	52	67	42	38
Control Mean	-0.013	-0.005	-0.010	-0.010	-0.002	0.005	-0.006	-0.013
PANEL B: LOCAL QUADRATIC								
	-0.675*	-0.665*	-0.323	-0.295	-0.031	0.058	-0.041	-0.048
	(0.329)	(0.324)	(0.259)	(0.253)	(0.198)	(0.214)	(0.258)	(0.258)
	[0.062]	[0.061]	[0.165]	[0.185]	[0.792]	[0.756]	[0.983]	[0.974]
MSE-Optimal Bandwidth	0.094	0.098	0.096	0.099	0.096	0.093	0.099	0.097
Effective Observations	66	67	61	66	57	57	48	48
Control Mean	0.002	-0.006	0.002	-0.001	-0.005	0.002	-0.006	-0.006
Observations	120	120	120	120	120	120	120	120
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table replicates results Table III.4, Panel A with the sample split in quartiles of incumbent electoral strength as measured the vote share of the incumbent's party in the prior preidntial election. The outcome is the change in the incumbent's standardized *roll call extremism*. All other notes as under Table III.4. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.9: THE EFFECT OF AN EXTREMIST CHALLENGER ON PARTY LOYALTY DEPENDING ON DISTRICT COMPETITIVENESS BY QUARTILES OF INCUMBENT ELECTORAL STRENGTH

	Marginal Districts (1^{st} Quartile)		Marginal Districts (2^{nd} Quartile)		Safe Districts (3^{rd} Quartile)		Safe Districts (4^{th} Quartile)	
PANEL A: LOCAL LINEAR	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.168**	-0.177**	-0.034	-0.032	-0.020	-0.014	-0.031	-0.006
	(0.068)	(0.073)	(0.039)	(0.043)	(0.046)	(0.043)	(0.044)	(0.037)
	[0.012]	[0.020]	[0.271]	[0.340]	[0.747]	[0.798]	[0.548]	[0.892]
MSE-Optimal Bandwidth	0.069	0.056	0.075	0.073	0.089	0.081	0.106	0.074
Effective Observations	53	44	50	50	54	52	54	38
Control Mean	-0.012	-0.008	-0.013	-0.014	-0.013	-0.011	-0.014	-0.013
PANEL B: LOCAL QUADRATIC								
	-0.201**	-0.202**	-0.049	-0.049	-0.006	-0.019	-0.034	-0.018
	(0.085)	(0.085)	(0.046)	(0.053)	(0.076)	(0.068)	(0.049)	(0.040)
	[0.029]	[0.027]	[0.247]	[0.301]	[0.920]	[0.758]	[0.514]	[0.677]
MSE-Optimal Bandwidth	0.091	0.090	0.103	0.104	0.072	0.068	0.165	0.134
Effective Observations	66	66	67	68	48	46	87	71
Control Mean	-0.013	-0.014	-0.013	-0.013	-0.014	-0.012	-0.009	-0.011
Observations	120	120	120	120	120	120	120	120
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table replicates results Table III.4, Panel B with the sample split in quartiles of incumbent electoral strength as measured the vote share of the incumbent's party in the prior preidntial election. The outcome is the change in the incumbent's *party loyalty*. All other notes as under Table III.4. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.10: EFFECT OF EXTREMIST CHALLENGER ON GENERAL ELECTION OUTCOMES DEPENDING ON DISTRICT COMPETITIVENESS

	Marginal Districts				Safe Districts			
PANEL A: EFFECT ON INCUMBENT'S VOTE SHARE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.040*	0.042*	0.040	0.040	0.007	-0.014	0.000	-0.013
	(0.022)	(0.018)	(0.032)	(0.026)	(0.028)	(0.020)	(0.034)	(0.028)
	[0.086]	[0.074]	[0.317]	[0.170]	[0.995]	[0.697]	[0.974]	[0.833]
MSE-Optimal Bandwidth	0.111	0.058	0.107	0.083	0.078	0.059	0.122	0.087
Effective Observations	150	88	144	120	93	70	138	97
Control Mean	0.52	0.51	0.51	0.51	0.56	0.56	0.56	0.55
PANEL B: EFFECT ON CHALLENGER'S VOTE SHARE								
	-0.043*	-0.044**	-0.044	-0.044	-0.021	-0.006	-0.010	0.010
	(0.022)	(0.017)	(0.033)	(0.024)	(0.030)	(0.024)	(0.035)	(0.032)
	[0.066]	[0.044]	[0.281]	[0.132]	[0.709]	[0.925]	[0.971]	[0.778]
MSE-Optimal Bandwidth	0.108	0.067	0.102	0.098	0.078	0.061	0.128	0.096
Effective Observations	144	100	136	134	92	72	147	103
Control Mean	0.47	0.47	0.47	0.47	0.43	0.43	0.42	0.43
PANEL C: EFFECT ON INCUMBENT'S MARGIN								
	0.083*	0.081*	0.085	0.070	0.029	-0.007	0.004	-0.022
	(0.044)	(0.035)	(0.064)	(0.048)	(0.057)	(0.043)	(0.065)	(0.058)
	[0.072]	[0.074]	[0.285]	[0.264]	[0.839]	[0.825]	[0.866]	[0.815]
MSE-Optimal Bandwidth	0.109	0.062	0.105	0.094	0.077	0.061	0.131	0.091
Effective Observations	147	94	141	127	92	72	149	101
Control Mean	0.05	0.04	0.04	0.04	0.12	0.12	0.14	0.12
PANEL D: EFFECT ON PRESENCE OF THIRD CANDIDATES								
	0.326*	0.351***	0.389*	0.497***	-0.048	-0.036	-0.018	-0.034
	(0.167)	(0.138)	(0.211)	(0.176)	(0.226)	(0.180)	(0.266)	(0.259)
	[0.057]	[0.010]	[0.094]	[0.006]	[0.915]	[0.903]	[0.903]	[0.863]
MSE-Optimal Bandwidth	0.083	0.063	0.110	0.103	0.074	0.077	0.118	0.097
Effective Observations	118	94	149	139	88	91	135	105
Control Mean	0.73	0.69	0.74	0.73	0.75	0.75	0.75	0.78
PANEL E: EFFECT ON THIRD CANDIDATES' VOTE SHARE								
	0.003	0.012	0.004	0.017	0.017	0.023*	0.007	0.000
	(0.007)	(0.008)	(0.011)	(0.012)	(0.013)	(0.011)	(0.014)	(0.015)
	[0.634]	[0.174]	[0.783]	[0.195]	[0.267]	[0.080]	[0.804]	[0.639]
MSE-Optimal Bandwidth	0.127	0.056	0.121	0.086	0.114	0.075	0.104	0.078
Effective Observations	164	85	160	121	131	89	118	92
Control Mean	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Polynomial Order	1	1	2	2	1	1	2	2
Observations	241	241	241	241	241	238	241	238
Covariates	N	Y	N	Y	N	Y	N	Y

Notes: The Table presents estimated effects of extremist challengers on general election outcomes in marginal districts (Columns 1 – 4) and safe districts (Columns 5 – 8). A district is defined as marginal if the vote share of the incumbent's party in the prior presidential election is above the sample median of 53.5%. Outcome variables are the incumbent's vote share (Panel A), the vote share of the main opponent party candidate (Panel B), the incumbent's vote share margin with respect to the main party opponent (Panel C), a dummy equal to 1 if at least one candidate other than Rpublican or Democratic gets a non-zero vote share (Panel D), and the total vote share of all candidates other than Republicans or Democrats (Panel E). Local linear specifications of equation 10 are reported in Columns 1 – 2 and 5 – 6, local quadratic specifications in Columns 3 – 4 and 7 – 8. Even-numbered columns control for all covariates listed in Figure III.4, Panels A and B, exluding the distance between candidates' Hall-Snyder scores. All regressions use MSE-optimal bandwidths and a triangular kernel. The sample is restricted to re-election seeking incumbents. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.11: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON DEPENDING ON INCUMBENTS' PROXIMITY TO THE MODERATE POTENTIAL CHALLENGER IN SAFE DISTRICTS

	Proximate Platforms				Distant Platforms			
PANEL A: Δ EFFECT ON ROLL CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0.049	0.067	0.119	0.133	-0.052	-0.036	0.042	0.035
	(0.163)	(0.188)	(0.240)	(0.245)	(0.245)	(0.239)	(0.358)	(0.347)
	[0.701]	[0.629]	[0.601]	[0.519]	[0.941]	[0.916]	[0.724]	[0.758]
MSE-Optimal Bandwidth	0.127	0.089	0.112	0.107	0.063	0.067	0.081	0.083
Effective Observations	67	51	61	61	35	36	44	44
Control Mean	0.004	-0.003	-0.003	-0.003	-0.021	-0.032	-0.044	-0.044
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.015	-0.000	0.015	0.020	-0.014	-0.014	-0.001	0.008
	(0.036)	(0.035)	(0.047)	(0.046)	(0.062)	(0.058)	(0.074)	(0.089)
	[0.812]	[0.789]	[0.608]	[0.577]	[0.876]	[0.949]	[0.870]	[0.777]
MSE-Optimal Bandwidth	0.099	0.088	0.101	0.098	0.083	0.058	0.117	0.083
Effective Observations	56	51	58	54	44	34	70	44
Control Mean	-0.005	-0.005	-0.004	-0.007	-0.008	-0.004	-0.001	-0.008
Polynomial	1	1	2	2	1	1	2	2
Observations	104	104	104	104	134	134	134	134
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table replicates results for marginal districts presented in Table III.5 for incumbents seeking re-election in safe districts. All other notes as under Table III.5. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

TABLE C.12: THE EFFECTS OF AN EXTREMIST CHALLENGER ON ROLL CALL EXTREMISM AND PARTY LOYALTY DEPENDING ON DEPENDING ON INCUMBENTS' LOCAL ROOTS: ALL DISTRICTS

	Incumbent Locally Rooted				Incumbent Not Locally Rooted			
PANEL A: EFFECT ON Δ ROLL CALL EXTREMISM (STD.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-0.039	0.078	-0.027	-0.033	-0.274**	-0.397***	-0.364**	-0.555***
	(0.147)	(0.107)	(0.210)	(0.146)	(0.137)	(0.118)	(0.159)	(0.155)
	[0.859]	[0.467]	[0.929]	[0.754]	[0.026]	[0.001]	[0.021]	[0.001]
MSE-Optimal Bandwidth	0.075	0.058	0.082	0.105	0.067	0.060	0.099	0.076
Effective Observations	71	54	76	100	116	104	157	128
Control Mean	0.009	0.006	0.036	0.055	-0.010	0.007	-0.034	-0.024
PANEL B: EFFECT ON Δ PARTY LOYALTY								
	-0.072	-0.036	-0.073	-0.053	-0.048	-0.045*	-0.069	-0.088**
	(0.039)	(0.028)	(0.044)	(0.044)	(0.036)	(0.028)	(0.045)	(0.040)
	[0.121]	[0.333]	[0.122]	[0.337]	[0.134]	[0.073]	[0.106]	[0.034]
MSE-Optimal Bandwidth	0.085	0.072	0.134	0.091	0.074	0.085	0.106	0.080
Effective Observations	78	66	123	83	131	139	168	134
Control Mean	-0.004	-0.009	-0.005	-0.005	-0.016	-0.018	-0.018	-0.014
Polynomial	1	1	2	2	1	1	2	2
Observations	179	178	179	178	310	300	310	300
Covariates	N	Y	N	Y	N	Y	N	Y
Outcome Before Primary	N	Y	N	Y	N	Y	N	Y

Notes: The Table replicates results for marginal districts presented in Table III.7 for the whole sample of re-election seeking incumbents. All other notes as under Table III.7. Standard errors clustered by House incumbent in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust p-values based on bias-adjusted estimates in brackets.

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