Do Primary Elections Exacerbate Congressional Polarization?

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Abstract

Many argue that primary elections increase polarization in the U.S. Congress. We test this hypothesis by exploiting the fact that primary election dates and candidate filing dates vary by state. Implementing differences-in-differences designs that account for idiosyncratic differences between each member in each Congress and each bill by party, we test whether members vote differently before or after their state's filing deadline or primary election date. We find no evidence that members of Congress vote in more extreme or partisan ways in order to deter primary challengers, but we do find that they vote in a more ideologically extreme way during their contested primary campaigns. However, the substantive magnitude of this effect is small, explaining approximately one percent of the overall level of congressional polarization. The polarizing effect of primary elections appears to be greater in the Senate than in the House, smaller on party-priority legislation, greater for more moderate members and those from moderate constituencies, and smaller in states utilizing non-partisan primaries.

Members of the U.S. Congress do not closely represent the preferences of their constituents, with Republicans consistently to the right of the median voter in their constituency and Democrats consistently to the left (e.g., Bafumi and Herron 2010). The ideological differences between congressional Democrats and Republicans are substantively large, normatively troubling, and increasing over time (see McCarty 2019). Paradoxically, voters strongly prefer more ideologically moderate candidates, so both parties could win more seats if they were willing to field more moderate candidates (Hall 2015, 2019). Why, therefore, are our elected officials so extreme relative to their constituents, and why is there so much polarization in Congress? One common explanation is that incumbent members of Congress have to worry about "getting primaried." That is, they fear that if they are too moderate, a more extreme candidate will challenge and defeat them in their party's primary election.

In this paper, we test whether and to what extent primaries exacerbate congressional polarization as claimed. More specifically, we test whether, holding constant the composition of the chamber, members of Congress would vote differently if they knew they had less reason to worry about primary competition. We exploit the fact that different states hold their primary elections at different times and have different filing deadlines for primary challengers. Analyzing approximately 8 million roll-call votes taken in Congress from 1995 to 2022, we test whether members vote in more partisan or ideologically extreme ways before the filing deadline for primary challengers (potentially with the goal of deterring challengers from running) or before the primary election date (potentially with the goal of defeating primary challengers). Our differences-in-differences design allows for idiosyncratic differences between each member in each two-year Congress and between each bill by party. Identification comes from comparing members of the same party but from different states voting on the same bill, where one member's filing date or primary election date has already passed and the other's has not.

We find that members become less partisan and more ideologically moderate after their primary date has passed. However, the magnitudes of these effects are substantively small. Our estimates imply that if members always voted as if they had just won their primary election, the average difference in voting behavior between Democrats and Republicans in Congress would be approximately 1 percent smaller. We further find that much of the polarizing effect of primaries arises because members who would have cast a moderate vote are more likely to abstain during their primary campaign. We also find that the polarizing effect of primary elections appears to be greater in the Senate than the House, is smaller for party-priority legislation, and weaker in states that utilize a non-partisan primary system.

Related Literature

A large literature shows that general elections in the U.S. have a moderating effect. All else equal, more moderate candidates are more likely to win general elections (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Canes-Wrone and Kistner 2022; Fowler 2020; Fowler et al. 2023; Hall 2015; Hall and Thompson 2018). Other studies have found little evidence that members of Congress moderate their positions in response to electoral incentives (Fowler and Hall 2016; Fu 2023; Poole 2005; Stone 1980), suggesting that most of the moderating effects of general elections are through selection rather than incentives. If general elections do have a moderating effect, and if voters prefer moderate politicians, why are our elected officials so polarized?

A common response to this question among pundits and commentators is that incumbent politicians have to worry about potential primary challengers. Theoretically, there are good reasons to be concerned about this possibility. Palfrey (1984) shows that office-motivated candidates might diverge significantly from the median voter's preference if they are worried that they will be flanked by a more extreme candidate. In Palfrey's model, two competing candidates strategically select their

platforms, and then a third candidate strategically enters and selects the platform that maximizes her vote share. Primary elections are not explicitly included in the model, but the purportedly polarizing force of primaries that observers have in mind is similar to the polarizing force of the third candidate in the model. This model assumes that voters sincerely support the candidate closest to them. If voters are strategic, primaries will not necessarily have a polarizing effect because forward-looking primary voters may have an incentive to nominate the more moderate candidate who has a greater chance of winning the general election.

Empirically, there is little compelling evidence that primaries polarize candidates. As expected, primary-election voters are more ideologically extreme than general-election voters (Hill 2015), although the differences are modest (Sides et al. 2020). When states switched from party conventions to direct primaries, the ideological extremism of their members of Congress, if anything, decreased (Hirano et al. 2010). Primary elections appear to select higher-quality candidates (Hirano and Snyder 2014, 2019) but not ideologically extreme candidates (Cowburn 2022; Hirano et al. 2010). Furthermore, voters and especially donors appear to behave strategically in primary elections (Hall and Snyder 2015), suggesting that any polarizing effect of primaries might be mitigated by forward-looking people who want to ensure that their preferred party wins the general election.

Boatright (2013) argues that despite popular rhetoric to the contrary, primary challenges have not become more common over time. He also claims that competitive primaries have no effect on roll-call voting, although he tests this hypothesis using DW-NOMINATE scores, which are not well-suited to this question because they do not allow members' ideological scores to vary idiosyncratically over time (see Theriault 2016). Meyer (2021) finds that U.S. Senators with a primary challenger are more likely to vote with the party whip in the months leading up to the primary election. However, this cross-sectional analysis does not examine within-member variation in primary competition, so it could potentially conflate the effects of primary competition with factors that cause or are correlated

with primary competition. Furthermore, this analysis does not account for the fact that primary election dates vary across states, so the bills leading up to a primary election vary by state.

Our study builds upon this literature by testing whether members of Congress change their roll-call voting in response to the primary election calendar. Unlike the analysis of Hirano et al. (2010), the counterfactual question is not about the effect of primaries versus an alternative nominating system. Rather, we estimate the effect of having to worry less about primary competition because the filing deadline or primary election date has passed. In our main specifications, we do not condition on whether a member was challenged in a primary (or lost a primary, which is very rare) because this is potentially endogenous to their roll-call behavior. Instead, we estimate the reduced-form effect of no longer having to worry about a primary challenger (at least until the next Congress). When pundits argue that primaries are exacerbating polarization, they are typically not comparing primaries to alternative nominating institutions. Rather, they are arguing that incumbents are more extreme than they would otherwise be if they didn't have to worry about their (potential) primary challengers. Therefore, our study assesses this empirical claim as directly as possible by testing whether incumbents change their behavior when they no longer have to worry about the primary and can focus on the general election.

Data and Design

We collect data from Voteview.com on congressional roll-call votes taken in the U.S. House of Representatives between 1995 and 2022 (Lewis et al. 2022). We collect data from the Federal Election Commission on the congressional primary filing deadlines and election dates by state between 1996 and 2022.[1] We also utilize information on contested primaries and retirements as inferred from

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[1] In most cases, the filing deadlines and election dates are the same for the House and Senate seats within the same state, but when they differ, we account for this and match an incumbent to the

official election returns and verified using Wikipedia and Ballotpedia. We assemble a data set in which every row is a member by roll-call vote. We exclude those who are not explicitly members of the Democratic or Republican parties. To avoid any potentially conflating effect of members losing reelection, we only include votes that were taken before the general election.

Two states warrant special consideration. Louisiana typically uses a runoff system with no partisan primaries, so we classify the primary date as the date of the general election. Since we also exclude roll-call votes taken after the general election, this effectively means that we do not learn much about the effects of primary election dates in Louisiana. Connecticut has partisan primaries, but they are almost never competitive. Instead, the party nominees are typically selected at the party conventions, which take place before the primary elections. Therefore, we exclude Connecticut from our main analyses, although in Appendix Table A1, we separately test for the effects of the party convention dates in Connecticut. This leaves us with more than 7 million member-votes for our analyses of the U.S. House and more than 800,000 member votes for our analyses of the U.S. Senate.

Congressional primary election dates vary considerably across states. The earliest primary elections are typically in early March of the election year, and the latest primary election dates (ignoring Louisiana) are typically in September, meaning there is a six-month period every two years during which some members of Congress have already had their primary election and others are still competing in their primary campaign. Similarly, the candidate filing deadlines for congressional primary elections also vary considerably across states. The earliest filing deadlines are typically in November or December of the year before the general election, and the latest deadlines are typically in July of the election year, leaving an eight-month period in each election cycle during which some members of Congress know whether they will be challenged in the primary and by whom while others

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relevant date for their state and chamber. In some cases, the filing deadlines differ for incumbents and non-incumbents. In these cases, we utilize the filing deadline for non-incumbents because this is the deadline by which an incumbent will learn whether or not she will face a primary challenger.

do not yet know if they will experience a primary challenge. Figure A1 in the Appendix visually illustrates this variation for the 2020 election cycle.

To estimate the effect of primary elections, we run regressions of the following form:

$$\text{ExtremeVote}_{ipcb} = \beta*\text{AfterPrimary}_{ib} + \gamma_{ipc} + \delta_{pb} + \varepsilon_{ipcb},$$

where ExtremeVote_{ipb} is a binary variable indicating whether legislator i from party p in Congress c cast an ideologically extreme vote on bill b, AfterPrimary_{ib} is a binary variable indicating whether the primary election date for member i has passed by the time bill b came up for a vote, γ_{ipc} represents member-Congress fixed effects,[2] and δ_{pb} represents bill-party fixed effects.

Utilizing the method of Fowler and Hall (2013), we classify each roll-call vote as conservative or liberal,[3] and we then classify an extreme vote as a conservative vote taken by a Republican or a liberal vote taken by a Democrat. As an alternative outcome, we also study whether members take a partisan vote, as measured by whether their vote aligns with the majority of their party.

This design accounts for the fact that some members are more likely than others to cast an extreme vote, and we allow this to vary idiosyncratically by Congress. We also account for the fact that an extreme vote is more likely on some bills than others, and we allow this to vary idiosyncratically by party. This is implicitly a differences-in-differences design where identification comes from members of the same party voting on the same bill at a time when one member's primary has taken place but the other's has not. Specifically, our fixed effects mean that we are effectively conducting

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[2] In the rare cases when members change parties, we treat this as a new member, so when we refer to member-Congress fixed effects, this is a shorthand for member-party-Congress fixed effects.

[3] Specifically, we make an initial guess about the directionality of the bill using levels of support from each party. For example, if Republicans are more likely than Democrats to vote *yea*, then we classify the yea vote as the conservative vote on that bill. Using these codings, we estimate Conservative Vote Probabilities (CVP) for each member in each Congress. We then check whether our estimated CVP scores are positively or negatively correlated with the conservative vote on that bill. If the correlation is negative (which, in practice, is extremely rare), we flip the coding for that bill, and re-estimate CVP scores. We repeat this procedure until the CVP scores are positively correlated with conservative voting on every bill.

separate differences-in-differences designs for each Congress by party, and we are pooling together evidence from all of them. Our parallel trends assumption is that differences in roll-call voting around the time of a member's primary election would, in expectation, be the same as differences in the roll-call voting of other members from the same party on the same bills but whose states do not have a primary election at the same time if not for the effect of primary elections. In the Appendix, we present the results of an event study which lend further credibility to the parallel trends assumption. We find no evidence of differential trends in extreme voting before the primary election date.

To illustrate the strength of the design, we explain why the following phenomena would not bias our estimates. If party leaders strategically time controversial bills to be after primary elections, this would not violate our assumptions because our bill-party fixed effects account for the fact that some bills are more controversial than others, and identification comes from comparing how members from the same party (but from different states with different primary election dates) vote on the same bill.[4] If states with more extreme members tend to have earlier or later primary elections, this too would not be a source of bias because our member-Congress fixed effects account for the fact that some members are more extreme than others.

We can modify this design in several ways to answer different questions and probe mechanisms. We also test whether members change their voting behavior around filing deadlines— the dates by which challengers must announce that they are running in a primary. We further subset our data to explore heterogeneity over time or across different types of members, bills, or electoral institutions.

To be clear, our research design allows us to estimate the effect of variation in the threat of primary competition on the voting behavior of a member of Congress, holding constant the identity

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[4] However, this would potentially influence the local average treatment effect that our design estimates. Specifically, if party leaders avoid especially controversial bills during primary season, we would estimate the effect of primaries on the less controversial bills.

of that member. Therefore, we focus on the incentive effects of primaries rather than any selection effects. Of course, primaries could potentially influence Congressional polarization by changing who is nominated and runs in general elections, and these effects have been explored in other studies (e.g., Cowburn 2022; Hirano et al. 2010). Our design holds constant the identity of members and focuses on the incentive effects of primaries. Although both effects are potentially important, the incentive effects are of greater interest when thinking about why individual members don't moderate more to perform better in general elections.

An additional caveat is that our design does not allow us to estimate the effect of *entirely* removing the threat of primary competition. Even after the primary election date has passed, a member of Congress might still be worried about the possibility of primary competition in their *next* term. Therefore, our estimates may understate the total incentive effects of primary competition. Nevertheless, the passage of the primary election date should coincide with a significant drop in the relevance and salience of primary competition on roll-call voting. So if primary competition does meaningfully exacerbate polarization, we should detect a meaningful effect of the primary election date on roll-call voting.

Theoretical Expectations

Before presenting the results, we briefly discuss our theoretical expectations. In congressional primary and general elections, candidates are faced with two distinct but overlapping electorates (Jacobson 2013). Therefore, if primary elections exacerbate polarization as many scholars and observers claim, we would expect members of Congress to be less likely to cast ideologically extreme or partisan votes after their primary election date has passed. [5] Of course, primary elections need not

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[5] This prediction is unclear for members who lose their primary election. For them, we would predict that they should shift toward their preferred personal ideology, which could either be more or

exacerbate polarization since primary voters and self-identified partisans are not typically more ideologically extreme than incumbent members of Congress (e.g., Bafumi and Herron 2010), and since primary voters may be forward-looking (e.g., Hall and Snyder 2015).

If primary elections exacerbate polarization, we have ambiguous predictions for the effects of filing deadlines. On one hand, members might vote in more extreme or partisan ways before the filing deadline with the goal of deterring primary challengers. On the other hand, if they indeed experience a primary challenge, they might vote in more extreme or partisan ways after the filing deadline. In our subsequent analyses, we subset the data and explore heterogeneity to separately estimate these potentially competing effects.

As mentioned above, we utilize two different measures of roll-call voting—ideologically extreme voting and partisan voting. As expected, these two measures are positively correlated, but because many partisan votes are not ideologically extreme, and vice versa, the correlation is not especially strong.[6] Specifically, the correlation coefficient is approximately .31 in the House and .36 in the Senate. If primary competition changes the way members cast their roll-call votes, would we expect a bigger effect for ideologically extreme voting or partisan voting? The answer surely depends on what kind of voting behavior members believe is likely to motivate potential primary challengers and primary voters.

In the most famous examples of veteran members of Congress "getting primaried," such as Joe Crowley (D-NY14) and Eric Cantor (R-VA7), their opponents were not motivated by the fact that

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less extreme. However, very few members of the House (on average, approximately 4 per election year) lose their primary election, so these cases are not likely to meaningfully influence our estimates. As previously discussed, we do not condition on whether members won or lost their primary election because this could potentially bias our estimates.

[6] For example, consider a bill in which all Democrats and a majority of (but not all) Republicans vote yea. Most likely, the yea vote will be classified as liberal. Therefore, Republicans voting nay will be classified as taking an extreme but anti-partisan vote, while the Republicans voting yea will be classified as taking a partisan but moderate vote.

the incumbents weren't partisan enough. If anything, perhaps they were viewed as too partisan. Instead, their opponents appear to have been motivated by the ideological moderation of the incumbents. Therefore, if these anecdotes reflect a larger phenomenon, we would expect to see greater effects on ideologically extreme voting than on partisan voting.

Results

Table 1 shows the estimated effect of filing deadlines and primary election dates on roll-call voting. The top panel shows effects on casting ideologically extreme votes, and the bottom panel shows effects on casting partisan votes. Columns 1 and 2 show results for the U.S. House, and Columns 3 and 4 show results for the U.S. Senate. Columns 1 and 3 show our baseline specification, which estimates the extent to which members change their roll-call voting once the primary election date in their state has passed. Columns 2 and 4 also estimate the effect of the filing deadline.

In the U.S. House, we estimate that members are 0.23 percentage points less likely to cast an ideologically extreme vote and 0.15 percentage points less likely to cast a partisan vote after the primary election. Both estimates are in the expected direction, and the former estimate is statistically distinguishable from zero (p = .006). Therefore, primary elections appear to have a polarizing effect, but the magnitude of the effect is substantively small.

On average, across our period of study, Republicans in the House are 55 percentage points more likely to cast an ideologically conservative vote than Democrats.[7] Therefore, the 0.23 percentage point effect on ideologically extreme voting is approximately half a percent, or one two-hundredth, of the overall partisan polarization that we observe in Congress. Therefore, if we imagine that no member

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[7] Using the same data analyzed in Column 1 of Table 1, we regress conservative voting on an indicator for party and bill fixed effects. The resulting coefficient on party is .547, indicating that on average, Republicans are 55 percentage points more likely to vote conservatively than Democrats on the same bill.

of Congress were worried about primary competition and all of them voted 0.23 percentage points more moderately, then overall polarization in Congress would be approximately 1 percent smaller than it currently is.

Before the primary election date has passed, the average rates of extreme and partisan voting in our sample are approximately 78.02 and 92.53 percent, respectively. Therefore, we estimate that the rate of ideologically moderate voting shifts from approximately 21.98 to 22.21 percent after the primary election date passes, which is approximately a 1 percent increase. We also estimate that the rate of non-partisan voting increases from 7.47 to 7.62 percent, which is approximately a 2 percent increase.

To provide another benchmark for interpreting the substantive magnitude of our estimates, Fowler (2022) estimates that a member of Congress becomes 0.105 percentage points more likely to cast a conservative vote for every 1-percentage-point increase in the Republican presidential vote share in her district. Therefore, the effect of primary election dates that we estimate is comparable to the effect of a member's district becoming approximately 2 percentage points more in favor of her party in presidential elections.

In the U.S. Senate, we find no evidence that the primary election calendar influences partisan voting, but we do find that members become 0.69 percentage points less likely to cast an ideologically extreme vote after their primary election date has passed, and this estimate is statistically significant (p = .017). The magnitude of this estimated effect is approximately three times as large as that in the U.S. House but still substantively small. Because the Senate has fewer members and because only a third of them are up for reelection in a given Congress, each incumbent who is up for reelection is likely subject to more scrutiny from the public, the media, and potential challengers, which could potentially explain why we detect larger effects in this chamber. However, for the same reasons, our estimates are statistically less precise in the Senate than in the House.

In Columns 2 and 4 of Table 1 we also test for the effects of filing deadlines. We find that members of the House become more likely to cast an ideologically extreme or partisan vote after their filing deadline has passed. The magnitudes of these estimated effects in the House are comparable to (but in the opposite direction of) those of the primary election date. In other words, our estimates suggest that members of the House become slightly more ideologically extreme and partisan during their primary election campaign season, and after the primary election, they revert back to their pre-filing-deadline levels of extremism or partisan voting. Alternatively, members of the Senate do not appear to significantly change their voting behavior after the filing deadline, but they do become less extreme (but not less partisan) after their primary election date.

As previously discussed, the average effects of the filing deadline are theoretically ambiguous. On one hand, a member might have an incentive to cast more extreme votes after the filing deadline once they learn that they will face a primary challenger, but they might also have an incentive to cast more extreme votes before the filing deadline with the hope of deterring challengers. Perhaps the estimates in Table 1 reflect the combination of these two competing mechanisms. However, if we focus only on cases where a member is seeking reelection and did not face a primary challenger, we obtain similar estimates and we still find little evidence that members moderate their roll-call voting after their filing deadline (see Appendix Table A2).

Taken together, these results suggest that members of Congress change their roll-call voting behavior with the goal of defeating primary challengers but not necessarily with the goal of deterring primary challengers. The results also suggest that to the extent that primary elections influence roll-call voting, they increase ideologically extreme voting more so than they increase partisan voting. However, even the former effect is substantively small. The incentive effect of primary elections on ideologically extreme voting can only explain approximately 1 percent of the observed partisan polarization in Congress.

Our main analyses exclude cases in which a member abstained from voting. In Table A3 in the Appendix, we analyze abstention and discuss the extent to which our results are potentially driven by the decisions of members to not cast a vote. We find that members are less likely to cast roll-call votes during their primary campaigns, and we find suggestive evidence that much of the effect of primary election dates is explained by abstention. Specifically, we find that members are not significantly more or less likely to cast extreme votes after their primary election date, but they are more likely to cast moderate votes, suggesting that to the extent that members appear more extreme during their primary campaigns, this is partly because they are more likely to abstain when they would have otherwise cast an ideologically moderate vote.

Heterogeneous Effects

This section explores several different dimensions of potential heterogeneity in our estimates. Because we found consistent effects of the primary election date but not filing deadlines, and because we found consistent effects on ideologically extreme voting but not partisan voting, this section focuses on the effects of the primary election date on ideologically extreme voting.

Figure 1 flexibly explores heterogeneity over time. Specifically, we run the specification from the top panel of Columns 2 and 4 and of Table 1 separately for different time periods, and we plot coefficients along with the 95% confidence intervals. We could show separate estimates for each Congress or two-year election cycle, and we do so in Figure A2 in the Appendix, but these election-cycle-specific estimates are imprecise relative to the effect sizes. Therefore, Figure 1 shows the moving average estimates from three contiguous election cycles. For example, the estimates corresponding with 1998 include the 1996, 1998, and 2000 election cycles.

We see suggestive evidence that these effects have changed nonmonotonically over time. Specifically, the incentive effects of the primary election date appear largest between approximately

2010 and 2014 in the House and between approximately 2006 and 2010 in the Senate. Some of this variability may be due to chance, but interestingly, we do not see evidence that the polarizing effects of primary elections are consistently increasing or decreasing over time.

One of the most prominent instances of a veteran member of Congress losing a primary election was when Eric Cantor, Republican House Minority Leader, lost to the relatively unknown Dave Brat in 2014. Although this was a salient case in the news, it appears to have followed rather than preceded any uptick in the effect of primary election dates on roll-call voting that we observe in Figure 1. Even before Cantor was primaried, members of Congress appear to have moderated their roll-call voting after primary elections, and they did not become more likely to do so after 2014.

Table 2 assesses fourteen additional dimensions of heterogeneity in the effect of the primary election date on ideologically extreme roll-call voting in both the House and Senate. We run our baseline specification separately for different subsets of the data, reporting the estimated coefficients and standard errors in the table. For example, the table shows separate estimates for Democrats and Republicans, members of the majority and the minority, members with and without a primary challenger, and so on. For each of these potential dimensions of heterogeneity, we also test whether the estimated effect of the primary election date differs between these two samples. To do this, we modify our previous specification by also interacting the after-primary indicator with the variable of interest. When we do this, we also interact our bill-party fixed effects and our member-party fixed effects with this variable of interest. The estimated difference and standard error are shown in the "Diff" column.

Unfortunately, the standard errors associated with the estimated differences are typically large relative to even the average effect of the primary election date, so most of our estimated interactive coefficients are statistically insignificant. Below, we summarize these results, and when relevant, we discuss the magnitude of the estimated differences, whether the difference is consistent with

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theoretical expectations, and whether we observe the same difference in both the House and Senate. Since the average effect of the primary election date on extreme roll-call voting has a negative sign, we write that an estimated effect is greater for one subset relative to another if the estimate is more negative.

Row 1 of Table 2 tests whether the effect of the primary election date differs for relatively moderate vs. extreme members. We measure the Conservative Vote Probability (Fowler and Hall 2013) of each member in each Congress, and we classify members as moderate if they are more ideologically moderate than the median member of their party in that Congress. We estimate that the effect of the primary election date is more than twice as large for relatively moderate members in both chambers.

In the second row, we test whether our estimated effect varies according to the partisanship of a member's district. For the House, we compute the average two-party vote share of each district across all presidential elections within the same redistricting cycle. For the Senate, we compute the average two-party vote share of each state across all presidential elections between 1996 and 2020. We classify all districts and states that are above the 25th percentile and below the 75th percentile as purple. All else equal, we would expect bigger effects in districts where members can expect a more competitive general election and a more competitive primary election. Purple districts likely have more competitive general elections but less competitive primary elections, so the differential effect is theoretically ambiguous. We detect a larger effect of the primary election in purple districts and states in both the House and the Senate.

The next two rows of Table 2 test for differences between parties. The estimated effect of the primary election date is bigger for Republicans than for Democrats in both the House and Senate. We do not find a consistent difference between the majority and minority parties. If anything, the estimated effect is bigger for the majority party in the House and for the minority party in the Senate.

The next row tests for differences between more and less experienced members. We classify a member as experienced if they are in their fifth Congress or later, which is approximately the median level of experience in both chambers. Theoretically, we might expect smaller effects for more experienced members whose policy positions are better known by voters and who have less risk of losing reelection. Indeed, we estimate greater effects for less experienced members in both chambers, and in the Senate, the estimated effect is twice as large for less experienced members.

The next row tests whether the effects differ for members with and without a primary challenger. These analyses exclude members who did not seek reelection. Reassuringly for the design and the theory, we do not detect a statistically significant effect of the primary election date for members who did not face a primary challenger, and we detect a greater, statistically significant effect in both chambers for members who did face a primary challenger. In the following row, we separately examine members who did and did not seek reelection. Similarly, in both chambers, we detect no significant effect for retiring members and a larger effect that is statistically significant for non-retiring members.

We further test whether the effect of the primary election date is greater for members who faced a particularly viable challenger. We focus on members who defeated a primary challenger, and utilizing data from Bonica (2016), we classify challengers who raised more than $50,000 before the primary election date as well-funded. This data is only available for the 2008-2016 election cycles, so these analyses are restricted to those Congresses. Surprisingly, we detect larger effects for members without a well-funded challenger, but these differences are likely attributable to chance.

We next test whether, among those members who faced a viable challenger, the effects of primary election dates are greater for those who faced particularly extreme challengers. Utilizing estimates from Bonica (2016) of candidate ideology inferred from their campaign contributions, we classify a challenger as ideologically extreme if they are more extreme than the median primary

challenger from their party and election cycle. We estimate notably larger effects for members with extreme challengers in the Senate but not the House.

We also test for heterogeneity across different kinds of roll-call votes. Following previous studies (e.g., Snyder and Groseclose 2000), we classify a congressional vote as close if more than 35 percent and less than 65 percent of the voting members voted *yea*. In both chambers, we detect larger effects for close votes. We also separately examine bills that are classified by Curry and Lee (2020) as being part of the majority party agenda. In both chambers, we detect greater effects for non-priority legislation, and in the Senate, this estimated difference is statistically significant. However, this is the only one out of 28 heterogeneity tests in the table that is statistically significant, and we would expect a significant result to arise by chance, so this could be a false positive.

We further test for differences between procedural and non-procedural votes. Votes to pass a bill, amend a bill, agree to a conference report, override a veto, confirm a nominee, or issue a verdict are classified as non-procedural, and all other votes are classified as procedural. We would expect more substantive disagreement over non-procedural votes, so we might expect greater effects in these cases, and that's what we find in the House but not the Senate.

The final two rows of Table 2 test whether the effects of primary dates differ across the primary rules utilized in a state. We find no consistent evidence that the effect of the primary election date is greater in states using a closed primary system, meaning that only those registered with the party can vote in a primary. However, in both chambers, we estimate that the polarizing effect of primaries is greater under partisan primaries than with nonpartisan primaries such as the top-two system in California and Washington and the top-four system in Alaska.

We have also examined heterogeneity across different policy domains as classified by Clausen through the 113[8]th Congress in the U.S. House. Although we do not have clear theoretical predictions,

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[8] See voteview.com/articles/issue_codes.

readers may be interested to know the kinds of bills for which we detect the greatest effects. We find that members moderate their voting behavior after their primary election on bills pertaining to government management (0.38 percentage points) and miscellaneous policy (0.53 percentage points), we detect suggestive effects in the domain of foreign and defense policy (0.28 percentage points), and we do not detect substantively meaningful or statistically significant effects for bills pertaining to social welfare, agriculture, and civil liberties.

Discussion and Conclusion

Why are elected officials so polarized when voters are so moderate? This has been one of the most perplexing puzzles in American politics over the past several decades. A common explanation among pundits, practitioners, and some scholars is that elected officials are worried that if they moderate too much, they will lose their primary election to a more ideologically extreme candidate. Even if primary losses are rare in practice, nevertheless, the fear of "getting primaried" may explain much of the polarization we see among elected officials.

This paper offers the most direct test yet of this hypothesis. We exploit the fact that filing deadlines and primary election dates vary by state, and we test whether members change their roll-call voting around these critical dates on the calendar. Our differences-in-differences designs implicitly account for ideological differences between members in each Congress, and they also account for differences by party across each bill. Identification comes from situations in which members from the same party vote on the same bill but the filing deadline or the primary election date has passed for one and not the other.

We find no evidence that members vote in more ideologically extreme or partisan ways with the goal of *deterring* primary challengers. If anything, members become more partisan and ideologically extreme after their filing deadline has passed, and this is still true even for members who did not face

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a primary challenger. We do, however, find that members vote in more ideologically extreme and partisan ways with the goal of *defeating* primary challengers. Members become less partisan and ideologically extreme after their primary election date passes, and this is especially true of members who faced a primary challenger. We detect clearer and more consistent effects on ideologically extreme voting than on partisan voting, so primaries appear to increase ideological extremism more than they increase party cohesion.

Our results suggest that there would likely be less congressional polarization if incumbent members of Congress did not have to worry about primary competition. However, the substantive magnitude of this effect is small. The polarizing effect of the primary campaign explains approximately one percent of the average difference between Democrats and Republicans in the U.S. House. So on the whole, primary elections do not explain a meaningful share of congressional polarization, and they do not resolve the puzzling disconnect between moderate voters and their polarized elected officials.

Lending additional credibility to our results, we find that the effects of primary election dates mostly vary in ways we would theoretically expect. For example, we detect larger effects in cases where a member faces a primary challenger and no effect in cases where a member is retiring or has no primary challenger. We also detect greater effects for less experienced members who have less of an ideological reputation and potentially more reason to worry about primary challengers.

We also find that the polarizing effect of primary elections is greater for close votes, suggesting that these effects could have consequences for substantive legislation and policy debates. We also find that much of the effect of primary election dates is explained by abstention. In particular, members appear to avoid casting moderate votes during their primary campaigns.

We find suggestive evidence that the polarizing effect of primary elections is greater for moderate members and those from purple constituencies, which means primaries likely increase the homogeneity of party members. The irony is that those representing purple constituencies have the

greatest risk of losing their general election, so the polarizing effect of primary elections may cause the incumbent's party to lose support in the cases where it matters most.

Even if our results were stronger than they are, this paper should not be interpreted as a recommendation against primary elections. Even if primary elections do exacerbate polarization, they also provide accountability for elected officials in partisan districts, and they likely have many other important effects that are outside the scope of this study. We do find suggestive evidence that the polarizing effect of primaries is greater for partisan primaries than for non-partisan primaries such as the top-two system. Therefore, although the average effect of primaries on roll-call voting is small, how we nominate candidates likely has consequences for policy, representation, and polarization.

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Table 1. Effects of Primary and Filing Dates on Roll-Call Voting, 1995-2022

|	(1)	(2)	(3)	(4)
	U.S. House		U.S. Senate	
	DV = Extreme Vote			
After Primary	−.0023**	−.0027**	−.0069*	−.0062
	(.0008)	(.0009)	(.0028)	(.0035)
After Filing		.0024**		−.0009
		(.0008)		(.0026)
	DV = Partisan Vote			
After Primary	−.0015	−.0018*	.0008	.0005
	(.0008)	(.0008)	(.0027)	(.0031)
After Filing		.0017*		.0005
		(.0008)		(.0023)
Member-Congress FEs	X	X	X	X
Bill-Party FEs	X	X	X	X
Observations	7,119,428	7,119,428	844,018	844,018

*Standard errors, corrected for two-way clustering by member and bill, in parentheses; ** p<.01, * p<.05. The table presents the results of eight different regressions, all of which include member-Congress and bill-party fixed effects. Columns 1 and 2 show results for the U.S. House, and Columns 3 and 4 show results for the U.S. Senate. The top panel analyzes ideologically extreme voting, while the bottom panel analyzes partisan voting.*

## Table 2. Heterogeneous Effects of Primary Dates on Roll-Call Voting

	U.S. House			U.S. Senate		
	No	Yes	Diff	No	Yes	Diff
Moderate member	−.0011	−.0026*	−.0015	−.0026	−.0092*	−.0066
	(.0007)	(.0012)	(.0014)	(.0032)	(.0042)	(.0053)
Purple constituency	−.0018	−.0030*	−.0012	−.0016	−.0121**	−.0104
	(.0011)	(.0012)	(.0015)	(.0035)	(.0043)	(.0056)
Republican	−.0012	−.0033*	−.0021	−.0020	−.0110**	−.0090
	(.0011)	(.0013)	(.0018)	(.0038)	(.0042)	(.0057)
Majority party	−.0013	−.0031*	−.0018	−.0092*	−.0046	.0046
	(.0012)	(.0013)	(.0019)	(.0045)	(.0031)	(.0053)
Experienced	−.0024*	−.0020	.0005	−.0100*	−.0048	.0052
	(.0011)	(.0012)	(.0015)	(.0039)	(.0038)	(.0054)
Contested in primary	−.0015	−.0039**	−.0024	−.0049	−.0156**	−.0107
	(.0010)	(.0013)	(.0015)	(.0043)	(.0056)	(.0070)
Retiring	−.0023**	−.0021	.0001	−.0102**	−.0005	.0098
	(.0009)	(.0025)	(.0026)	(.0035)	(.0064)	(.0073)
Well-funded challenger	−.0085**	−.0073	.0013	−.0180*	−.0023	.0157
	(.0023)	(.0051)	(.0057)	(.0080)	(.0223)	(.0232)
Extreme challenger	−.0158	−.0007	.0152	.0026	−.0445	−.0471
	(.0083)	(.0057)	(.0096)	(.0250)	(.0419)	(.0501)
Close vote	−.0018	−.0026*	−.0008	−.0037	−.0118**	−.0081
	(.0010)	(.0011)	(.0013)	(.0034)	(.0038)	(.0046)
Priority legislation	−.0023**	−.0019	.0004	−.0117**	.0197	.0314*
	(.0009)	(.0043)	(.0042)	(.0035)	(.0110)	(.0121)
Procedural vote	−.0026*	−.0007	.0019	−.0042	−.0094*	−.0052
	(.0010)	(.0009)	(.0012)	(.0032)	(.0036)	(.0038)
Closed primary	−.0032**	−.0001	.0031	−.0055	−.0138*	−.0083
	(.0009)	(.0021)	(.0023)	(.0032)	(.0056)	(.0064)
Nonpartisan primary	−.0024**	.0017	.0041	−.0068*	.0041	.0108
	(.0009)	(.0056)	(.0056)	(.0029)	(.0130)	(.0126)

Standard errors, corrected for two-way clustering by member and bill, in parentheses; ** p<.01, * p<.05. The Yes *and* No *columns show separate estimates of the effect of the primary election date on ideologically extreme voting for different subsamples. The* Diff *columns show the difference in the estimate between two subsamples of interest.*

Figure 1. Effects of Primary Election Dates over Time

U.S. House
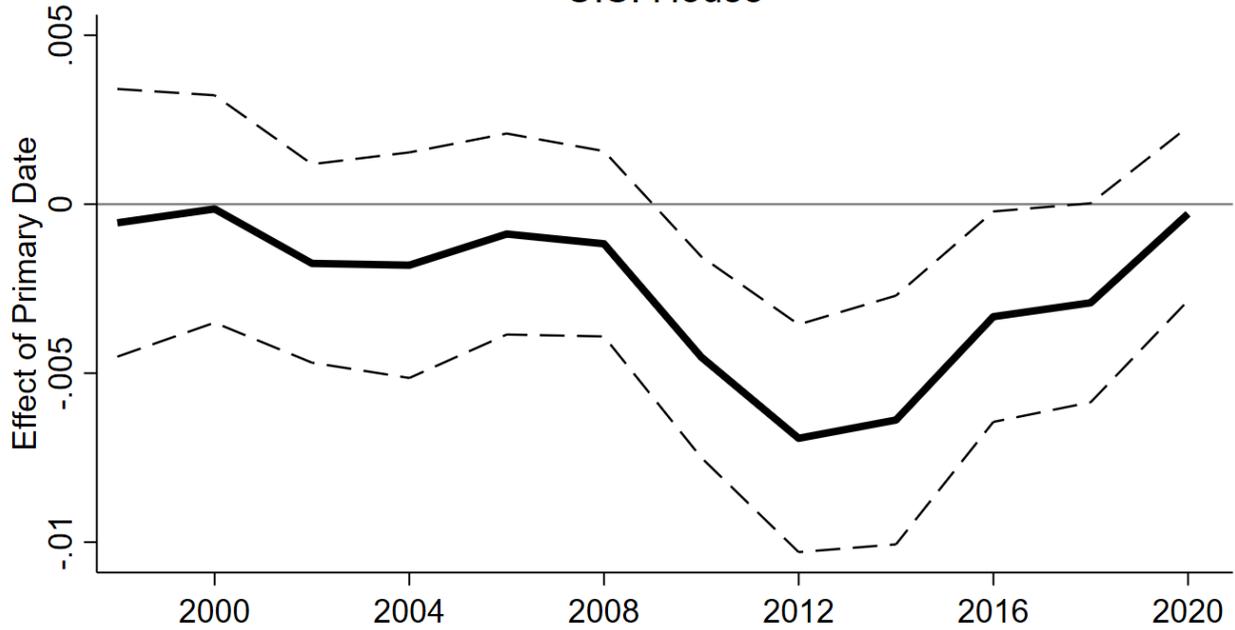
U.S. Senate
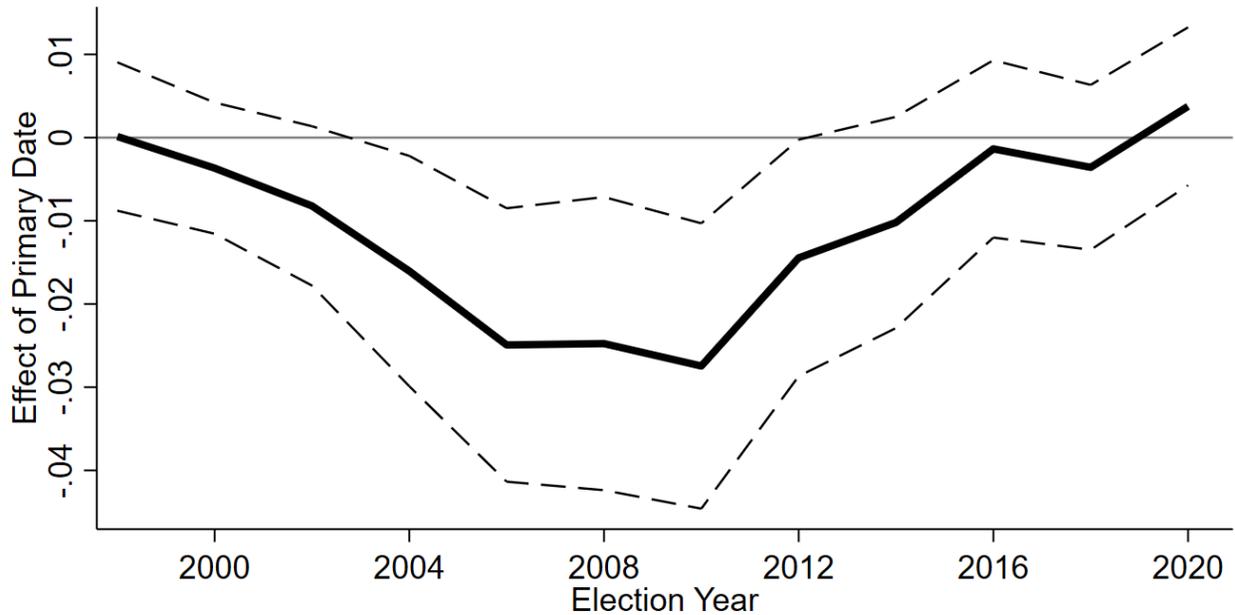
The figure shows how the estimated effects of the primary election date on extreme voting have changed over the period of our analyses. The figure shows the coefficients and 95 percent confidence intervals arising from a replication of the specification from Columns 1 and 3 in Table 1 for different time periods. Each estimate is a moving average that includes the even-year election cycle before and after that year. For example, the estimates corresponding with 1998 show the average effects across the 1996, 1998, and 2000 election cycles.

Appendix

Figure A1 visually illustrates the variation in filing deadlines and primary election dates across states. Specifically, the figure plots the period between the filing deadline and the primary election date for each state during the 2020 election cycle. We see considerable variation in both filing deadlines and primary election dates. Even excluding Louisiana, there is an approximately 6-month period during which some states have already held their primary elections but other states have not. Furthermore, the states in the figure are sorted according to their primary election date, and no obvious patterns stand out. For example, larger states do not systematically have earlier primaries than smaller states. California and Texas have early primaries, New York is in the middle, Florida is later. Similarly, the early or late states do not clearly overrepresent particular regions, richer or poorer states, or Democratic or Republican states.

Figure A2 shows separate estimates of the effect of primary election dates on extreme voting for each two-year election cycle in our analysis. Although the cycle-specific estimates are imprecise, this figure tells the same general story as Figure 1. The negative effect of the primary election date on extremism was greatest in the U.S. House around 2012 and greatest in the U.S. Senate around 2008.

Table A1 estimates the effect of party convention dates in Connecticut in the U.S. House. As discussed in the main text, congressional primaries are rarely contested in Connecticut, and the party nominees are typically selected in party conventions. To assess the effects of these party convention dates, we modify the specifications from Column 2 of Table 1, adding in members from Connecticut. We also code a binary variable indicating whether the party convention date has passed for these members. We only have Connecticut party convention dates from 2000 and onward, so this analysis only includes data from the 106th through the 117th Congresses.

The estimated effects of filing deadlines and primary election dates is virtually unchanged when we include Connecticut in the analysis. More interestingly, we can also estimate the effect of

party convention dates for members from Connecticut. Unfortunately, these estimates are statistically imprecise, but there is suggestive but not statistically significant evidence that convention dates have a similar effect in Connecticut as primary election dates in other states. Column 1 of Table 1 estimates that members from the 49 other states become 0.30 percentage points less likely to cast an ideologically extreme vote after the primary election date, and members from Connecticut become 0.40 percentage points less likely to cast an ideologically extreme vote after their party convention date.

Table A2 further assesses the possibility that members vote in more extreme or partisan ways before their filing deadline in order to deter primary challengers. Specifically, we re-run the specifications from Column 2 and 4 of Table 1 but focus only on members who sought reelection but did not face a primary challenger. If these members had voted in more extreme ways in order to deter a primary challenger, we might expect them to moderate their roll-call voting once the filing deadline passes and they see that they successfully deterred a challenger.

We find no evidence in either chamber that these members changed their roll-call voting after their primary election date. This is what we would expect since they had no primary challenger. More interestingly, we see no evidence in either chamber that they moderated their roll-call voting after the filing deadline passed and they learned for sure that they would not have a primary challenger. If anything, we see some evidence in the House that they became more extreme after the filing deadline passed. One potential explanation is that some members are worried about relatively moderate primary challengers and moderate their roll-call voting in order to deter them, but do not detect a similar pattern in the Senate.

Table A3 assesses the extent to which our results are explained by abstention. We replicate the design from Columns 2 and 4 of Table 1, but we include all member-votes in our analyses, even cases where the member abstained. Table A3 shows the results of five different specifications with different binary outcome variables. The first column examines abstention. We see that members are more likely

to abstain after their filing deadline and less likely to abstain after their primary election date. This is consistent with the possibility of a mechanical effect in which members are less likely to be in Washington during their primary campaign. Of course, whether a member is in Washington or not is endogenous, and members might strategically schedule campaign events at times when they might otherwise have to cast a controversial roll-call vote.

To assess whether members strategically abstain on different kinds of bills and to see whether this can explain our main results, we replicate the same specification from Column 1 but code binary indicators for an ideologically extreme vote, an ideologically moderate vote, a partisan vote, and a non-partisan vote. For example, in Column 2 of Table A3, the dependent variable is a binary variable that takes the value of 1 if the member cast an extreme vote and 0 if they abstained or cast a moderate vote. Interestingly, members are neither more nor less likely to cast an extreme vote after their primary election date, but they are significantly more likely to cast a moderate vote. Therefore, to the extent that members are more likely to cast moderate votes after their primary election date, this appears to be largely explained by the fact that members who would have cast a moderate vote are more likely to abstain while their primary campaign is still ongoing. This could be because those who would have cast moderate votes are more likely to be in competitive primaries, or it could be that they strategically avoid casting votes that could disappoint their partisan base before their primary election date.

Figure A3 shows the results of an event study, assessing the extent to which extreme voting varies with the amount of time until or since the primary election. We compute the number of days between each vote and the primary election, and we generate indicator variables for the number of 30-day periods (which we call *months* for convenience) between the primary election day and the roll-call vote. For example, all votes taken 0-29 days after the primary election are classified as 0 months after the primary, votes taken 30-59 days after the primary are classified as 1 month after, and so on.

We drop all observations that are classified as more than 4 months before or after the primary election to focus on the most informative period for which we have the most available data.

We replicate our baseline regression specification, except that instead of including a single indicator for whether the vote came after a member's primary election date, we have multiple indicators for 4 months before, 3 months before, and so on through 4 months after. Votes classified as 1 month before are the omitted category. Figure A3 shows the estimated coefficients along with the upper and lower bounds of their estimated 95-percent confidence interval.

Unfortunately, these event-study estimates are imprecise, particularly in the Senate. In the U.S. House, we see that the estimated coefficients for the months before the primary election are all close to zero and statistically insignificant, meaning that members do not appear to vote in more or less extreme ways as the primary date draws nearer. Next, we see that the estimated effect of the primary election date emerges soon after the primary election. We also see that the effect in the U.S. House persists in the following months and, if anything, it increases slightly. Therefore, the estimated effect of the primary election is not just a momentary blip; members continue to decrease the extreme voting (relative to the primary election campaign season) throughout the general election campaign season.

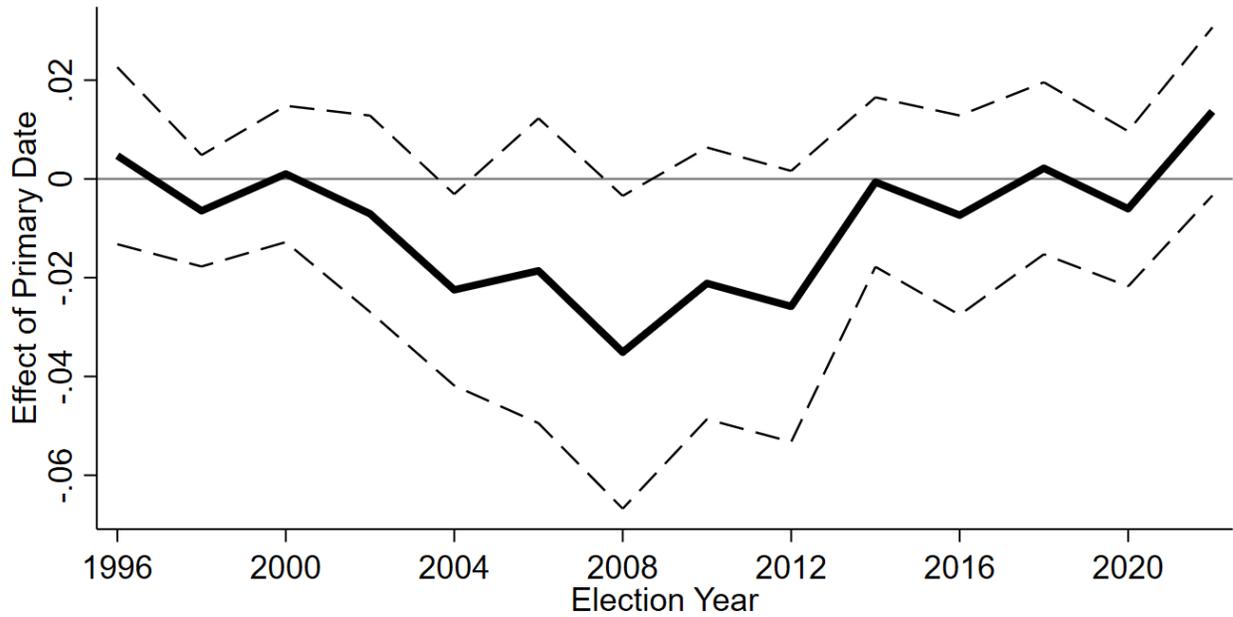**Figure A1. Primary Campaign Season by State in 2020 Election Cycle**
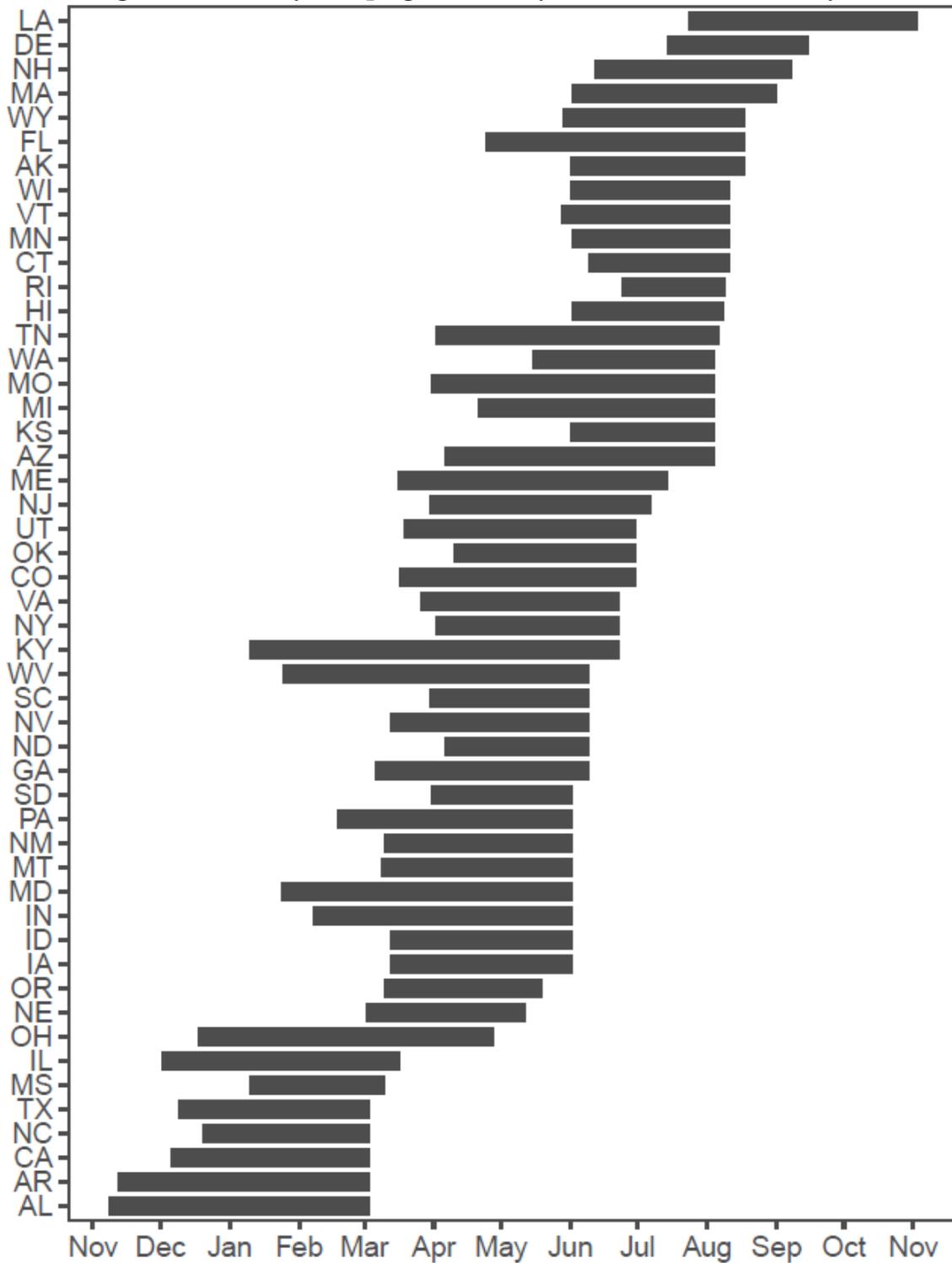
The solid bars show the period ranging from the filing deadline to the primary election date in each state during the 2020 election cycle. States are sorted according to their primary election date.

**Figure A2. Effects of Primary Dates in Each Election Cycle**
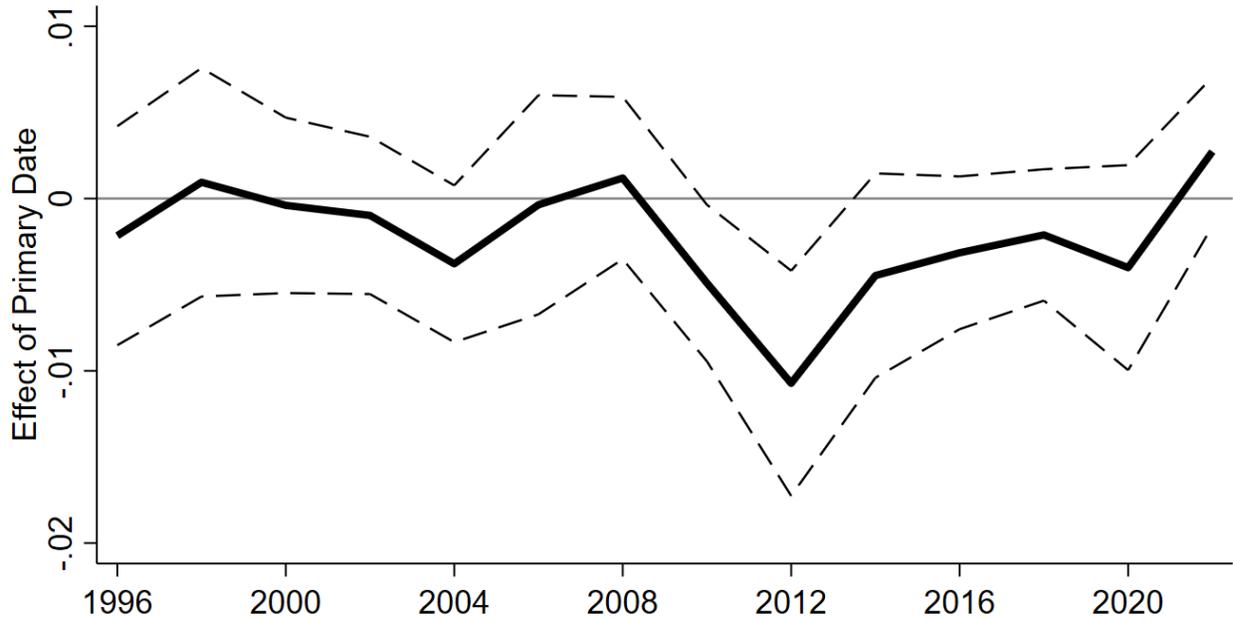
The figure shows how the estimated effects of the primary election date on extreme voting have changed over the period of our analysis. The figure shows the coefficients and 95 percent confidence intervals arising from a replication of the specification from Columns 1 and 3 of Table 1 for each individual election cycle in our analysis.

Figure A3. Event Study

U.S. House
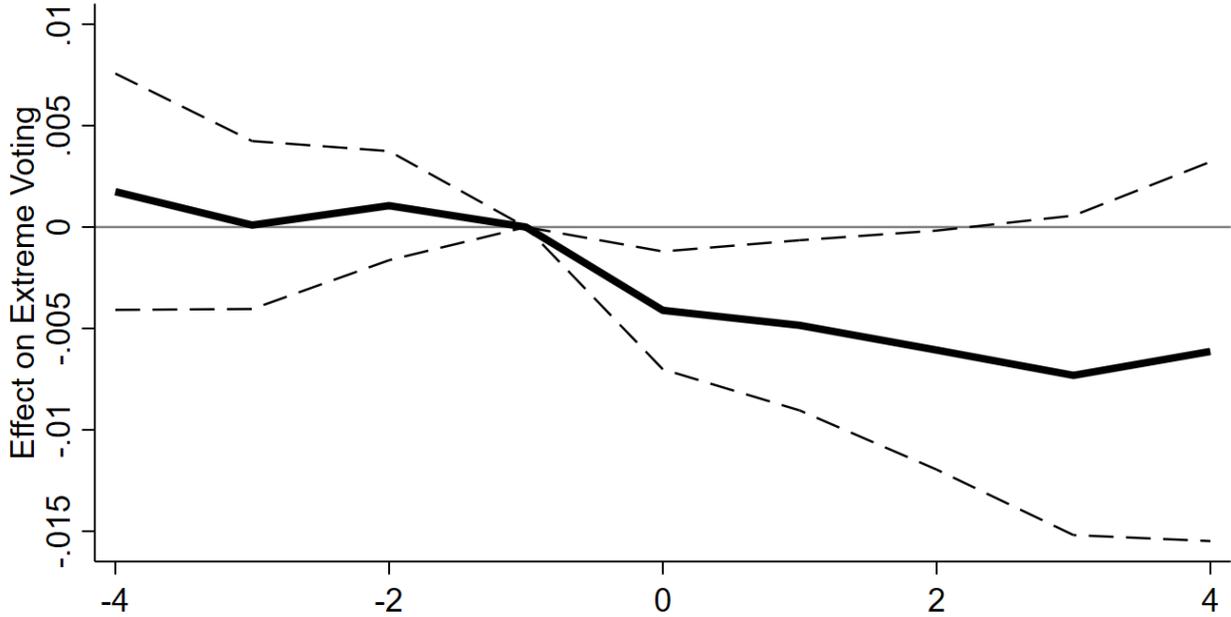
U.S. Senate
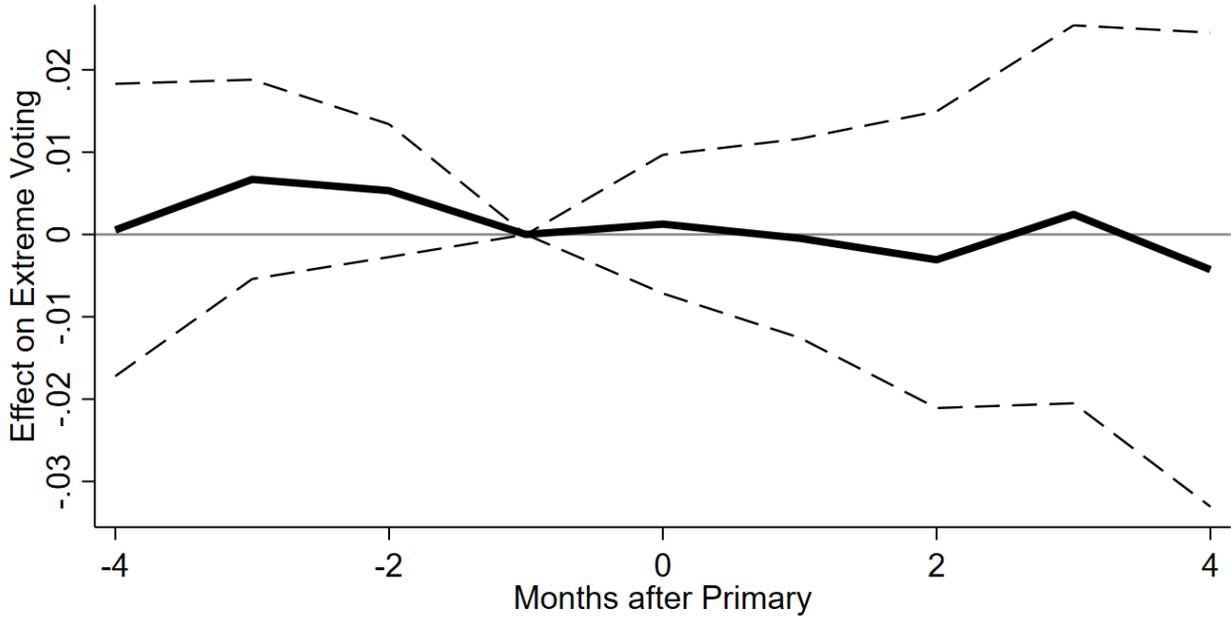
The figure shows the estimated coefficients and upper and lower bounds of the estimated 95% confidence intervals from an event-study regression. Specifically, we utilize the baseline specification but instead of a single indicator for whether a vote took place after the primary election date, we include indicators for the number of months after the primary election date. The baseline category are the votes that took place 1 month (1-30 days) before the primary election.

Table A1. Effect of Connecticut Party Convention Dates in U.S. House, 1999-2022

	DV = Extreme Vote	DV = Partisan Vote
After Primary	−.0030**	−.0017*
	(.0009)	(.0008)
After Filing	.0021*	.0011
	(.0009)	(.0008)
After CT Party Convention	−.0040	−.0012
	(.0057)	(.0037)
Member-Congress FEs	X	X
Bill-Party FEs	X	X
Observations	6,178,526	6,178,526

Standard errors, corrected for two-way clustering by member and bill, in parentheses; ** p<.01, * p<.05. The variables After Primary *and* After Filing *take a value of 0 in Connecticut. The variable* After CT Party Convention *takes a value of 0 in all other states. The estimated effect of the party convention dates in Connecticut is similar to that of the primary date in other states, although the former estimates are not statistically significant.*

Table A2. Effects of Filing Deadline for Non-retiring Members with No Primary Challenger

	U.S. House		U.S. Senate	
	Extreme	Partisan	Extreme	Partisan
After Primary	−.0020	−.0011	−.0004	.0019
	(.0011)	(.0010)	(.0051)	(.0056)
After Filing	.0026**	.0021*	−.0063	−.0018
	(.0010)	(.0009)	(.0038)	(.0041)
Member-Congress FEs	X	X	X	X
Bill-Party FEs	X	X	X	X
Observations	4,307,539	4,307,539	299,677	299,677

*Standard errors, corrected for two-way clustering by member and bill, in parentheses; ** p<.01, * p<.05. The table replicates the analyses in Columns 2 and 4 of Table 1, but it only includes members who sought reelection and did not face a primary challenger.*

Table A3. Results Including Abstainers

	Abstain	Extreme	Moderate	Partisan	Non-partisan
	U.S. House				
After Primary	−.0043*	.0003	.0041**	.0026	.0017*
	(.0017)	(.0016)	(.0010)	(.0017)	(.0008)
After Filing	.0084**	−.0035*	−.0049**	−.0063**	−.0021**
	(.0015)	(.0014)	(.0010)	(.0015)	(.0008)
Member-Congress FEs	X	X	X	X	X
Bill-Party FEs	X	X	X	X	X
Observations	7,417,507	7,417,507	7,417,507	7,417,507	7,417,507
	U.S. Senate				
After Primary	−.0048	−.0021	.0069	.0047	.0001
	(.0057)	(.0060)	(.0036)	(.0062)	(.0030)
After Filing	.0107*	−.0092*	−.0015	−.0089	−.0018
	(.0045)	(.0044)	(.0028)	(.0046)	(.0023)
Member-Congress FEs	X	X	X	X	X
Bill-Party FEs	X	X	X	X	X
Observations	871,585	871,585	871,585	871,585	871,585

*Standard errors, corrected for two-way clustering by member and bill, in parentheses; ** p<.01, * p<.05.*