Do Donors Punish Extremist Primary Nominees?

Evidence from Congress and American State Legislatures

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Abstract

Fundraising is a critical element of legislative elections, yet problems of endogeneity and measurement have prevented researchers from systematically evaluating whether campaign contributions advantage relative moderate or extremist candidates. This paper combines an original candidate ideology scaling with a regression discontinuity design in primary elections in Congress, 1980-2022, and state legislatures, 1996-2022, to evaluate whether donors advantage more-moderate or more-extreme candidates. I find that the "coin-flip" primary nomination of an extremist candidate over a moderate decreases their party's share of general-election contributions by 5-6 percentage points in the median contested primary and 24-28 percentage points when the ideological contrast between candidates is largest. This financial penalty is twice as large for corporate PACs and out-of-state donors as it is for individual and in-state donors. Applying a complementary panel-based identification strategy, I replicate my core findings and further find that the financial penalty to extremists has declined by nearly 50% since 1996. Overall, these results show how general-election donors act as an important, yet waning, moderating force in American elections.

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1 Introduction

As polarization reaches historic levels across American legislative landscapes, researchers and pundits have placed renewed attention on the relationship between candidate ideology and campaign finance outcomes. Observers worry that campaign contributions, particularly from individual donors, disproportionately advantage extremist candidates and contribute to the extraordinary polarization of American politics. Yet campaign contributions could also act as a moderating force in American elections, with donors primarily supporting moderate candidates just as voters prefer moderates at the general-election ballot box. Do donors advantage relative moderate or extremist candidates?

Despite its far-reaching electoral import, recent evidence on this question is mixed. One strand of literature identifies a substantial fundraising advantage for ideologically extreme candidates (Ensley, 2009; Fowler and Lewis, 2024; Oklobdzija, 2017), another reports no differences between moderate and extremist candidates' fundraising prospects (Claassen, 2007; Grant and Rudolph, 2002), while a third set of findings suggests that extremist candidates receive fewer contributions from political action committees (PACs) than their moderate counterparts (Hall, 2015; Meisels, 2024). However, as I detail below, these discrepancies may reflect the peculiarities of a limited sample of elections, or they may be symptomatic of concerns that traditional campaign finance-based measures of candidate ideology are endogenous to candidates' fundraising outcomes. Since fundraising plays a critical role in determining who runs for office (Carnes, 2018), whether candidates persist across election cycles (Bonica, 2017; Thomsen, 2024), and which candidates ultimately prevail (Fouirnaies, 2021), understanding systematically whether campaign contributions advantage extremist candidates has important implications for downstream representation and contemporary polarization.

To evaluate whether donors advantage relative moderate or extremist candidates, I examine fundraising in general elections for the U.S. House and U.S. Senate, 1980-2022, and all

forty-nine partisan U.S. state legislatures, 1996-2022.[1] Since traditional measures of candidates' ideological positioning are derived from the same fundraising outcomes I aim to study, I develop a new ideology scaling using only primary-election contributions from individuals and ideological interest groups, addressing concerns about endogeneity and strategic donating. This measure correlates highly within-party with standard measures of roll-call voting ideology ($r = .75$ for Democrats, $r = .72$ for Republicans) and correctly predicts 89.5% of roll-call votes in my sample. Simply comparing moderate and extremist candidates' general-election fundraising outcomes, however, would yield misleading conclusions, because districts where moderates select into running likely differ dramatically from districts where extremists seek office. Following previous work, I overcome this issue using a regression discontinuity (RD) design in primary elections, estimating the difference in general-election fundraising between districts that barely nominate a relative moderate or extremist in the primary election (Hall, 2015; Meisels, 2024). This design eliminates the empirical difficulty of placing voters and candidates on the same ideological scale (Broockman, 2016), allows for candidates' relative ideological positions to be chosen strategically, and short-circuits concerns about endogeneity, because the outcome (general-election contributions) is distinct from the data used to measure candidates' ideology (primary-election contributions)

Combining the regression discontinuity design and primary-specific ideology scalings, I find that, in the median primary election, the "coin flip" primary nomination of an extremist candidate decreases their party's share of total general-election contributions by 5-6 percentage points relative to a moderate. This financial penalty to extremists increases to 24-28 percentage points when the contrast between moderates and extremists is most pronounced, and is largest in highly-consequential open-seat elections.

Breaking these results down by donor type, I show that, while both individuals and corporate PACs punish extremist nominees, corporate PACs punish extremists at nearly twice the rate of individual donors. Similarly, out-of-state donors are more sensitive to

[1]By studying state legislatures and the U.S. Senate, in addition to the U.S. House, I gain access to nearly 15 times more data than is available using U.S. House elections alone.

extremist nominees than in-state donors, and extremist-party donors, rather than donors to extremists' opponents, appear to drive the overall penalty. Collectively, these results suggest that corporate PACs play an outsized role in driving the electoral advantage to moderates, while countering concerns about the polarizing influence of out-of-state donors (Gimpel, Lee, and Pearson-Merkowitz, 2008).

Next, I take advantage of the rich institutional variation across state legislatures to study heterogeneity in the financial penalty to extremists. By studying the institutional settings where the financial penalty to extremists is larger and smaller, we can learn more about the mechanisms underlying these effects and their electoral implications. Previous work suggests at least four sources of institutional variation that affect the financial penalty to extremism: the level of news coverage, which shapes donors' ability to respond to candidates' ideological positioning; the degree of professionalism within the legislature, which makes races more salient; within-state polarization, which reflects partisan—rather than ideological—loyalty; and the presence of up-ballot contests, reflecting nationalization. I find that the financial penalty to extremists is higher in more-professionalized legislatures, lower in more-polarized states, no different in the absence of national-level contests, and may be significantly larger when news coverage of state legislatures is stronger. Taken together, these results suggest that donors' willingness and ability to punish extremist nominees is augmented by media coverage and higher-salience elections and dampened by strong partisan loyalties.

To probe the robustness of these results, I replicate my baseline analyses using the panel-based identification strategy of Ansolabehere, Snyder, and Stewart (2001), which compares changes in candidates' general-election fundraising as the midpoint between Democratic and Republican general-election candidates varies. My estimates using this method are very similar in magnitude to the RD, yet substantially more precise, suggesting that the financial penalty to extremists extends beyond the set of districts that feature contested primary elections. Further, taking advantage of the statistical power that this panel method provides, I find that the financial penalty to extremists has declined by nearly 50% since

1996, matching the steady decay of the electoral penalty to extremists during the same period (Bonica and Cox, 2018; Canes-Wrone and Kistner, 2022; Handan-Nader, Myers, and Hall, 2024).

Finally, I show that these results are not an artifact of my campaign finance-based scaling method. Using the roll-call records of state legislators who later ran for Congress to identify relative moderates and extremists, I obtain highly similar point estimates to those reported in the main paper.

Collectively, these results temper concerns that campaign finance patterns exacerbate legislative polarization, at least in general elections. My findings indicate that general-election donors act as an important moderating force in American elections in response to the nomination of extremist primary candidates. At the same time, the financial advantage to moderate candidates appears to have attenuated significantly over the past three decades, matching the secular decline of the electoral advantage to moderation.

The remainder of this paper is organized as follows. In the next section, I outline theoretical perspectives on whether campaign contributions advantage extremists, with a particular focus on individual and corporate PAC donors. The third section introduces my empirical strategy, including the regression discontinuity design and a new ideological scaling that addresses concerns about endogeneity and strategic donating. Leveraging this design, section four examines the aggregate effect of extremist nominees on campaign contributions. Next, section five disaggregates the overall effect by donor type. Section six examines institutional heterogeneity in the penalty to extremists. In section seven, I replicate my main results using an observational panel method that holds district partisanship fixed over time and allows me to generalize beyond contested primary elections. Finally, section eight discusses my findings and concludes.

2 Competing Theoretical Perspectives on Donor Support for Moderate and Extremist Candidates

As the amount of money spent on American elections increases exponentially, the degree to which campaign contributions favor moderate or extremist candidates has become a key question in political science. Existing research, however, offers competing predictions for whether campaign contributions in aggregate should advantage relative moderate or extremist candidates. These studies typically focus on contributions from individual donors and corporate PACs, two of the largest sources of campaign funds.

From one perspective, individual donors may broadly allocate general-election contributions according to the same preference for moderates that voters display at the general-election ballot box. A growing literature, for example, finds that moderate candidates outperform extreme candidates in general elections (Ansolabehere, Snyder, and Stewart, 2001; Handan-Nader, Myers, and Hall, 2024; Hall, 2015), and survey evidence suggests that a significant portion of the electorate prefer moderate candidates over extremists (Fowler et al., 2023).

But it is not immediately clear whether these revealed electoral preferences affect candidates' fundraising prospects because the set of voters who contribute to political campaigns are highly unrepresentative of the overall electorate. In addition to wealthier and more educated (Verba, Schlozman, and Brady, 1995), previous work suggests that the typical individual donor to a political campaign holds significantly more ideologically extreme views than the average voter (Bafumi and Herron, 2010; Bonica, 2013; Francia et al., 2003; Schlozman, Verba, and Brady, 2012). Hence, despite the electorate's well-documented preference for moderate general-election candidates, it is possible that the American donorate is so skewed towards ideologues that campaign contributions advantage extremist candidates.

Previous work also yields conflicting predictions about whether corporate PACs favor moderate or extremist candidates. On one hand, a long-running literature suggests that

corporate PACs distribute campaign contributions at least partially on the basis of ideological proximity to candidates (Bonica, 2013, 2014; Poole and Romer, 1985; Poole, Romer, and Rosenthal, 1987; McCarty and Rothenberg, 1996; McCarty and Poole, 1998; McCarty, Poole, and Rosenthal, 2006). For the vast majority of corporate PACs, Bonica (2013) concludes that these ideological preferences are best characterized as centrist, rather than non-ideological. As a result, corporate PACs that donate on the basis of ideological proximity may tend to generate financial advantages for moderate candidates.

On the other hand, an equally-large literature suggests that PACs primarily value strategic considerations, many of which are orthogonal to ideological congruence (Hall and Wayman, 1990; Snyder, 1990). For example, corporate PACs tend to funnel contributions to incumbents (Fouirnaies and Hall, 2014), legislative agenda setters (Fouirnaies, 2018), legislators with procedural power (Berry and Fowler, 2016; Fouirnaies and Hall, 2018), relevant committee members (Esterling, 2007; Powell and Grimmer, 2016), and legislators holding safe seats (Bonica, 2013). Since these strategic considerations are generally independent of candidates' ideological positioning, these results imply that corporate PAC contributions should not favor either relatively moderate or extreme candidates.

As these conflicting perspectives demonstrate, it is not *a priori* clear whether campaign contributions should advantage moderate or extremist legislative candidates. Although voters typically favor moderates at the general-election ballot box, the individual donorate may be so unrepresentative of the electorate that contributions from individuals favor extremists. And corporate PACs may prioritize ideological congruence and donate to moderates, or they may primarily value a form of access to policy makers that is independent of ideology. Ultimately, whether these donor-level motivations advantage relative moderates or extremists, or cancel each other out in aggregate, is an empirical matter that I address below.

A small number of important papers have previously studied this question. Using a close primary regression discontinuity design in U.S. House elections, Meisels (2024) finds that moderate primary-election nominees receive more contributions from corporate PACs than

extremist nominees, while individual donors contribute similar amounts to moderates and extremists. My analysis complements, yet substantially improves upon, this research in scope, data, and design. First, while Meisels focuses on U.S. House elections, I study fundraising in all forty-nine partisan state legislatures and the U.S. Senate, in addition to the U.S. House. By including these additional contests, I gain access to nearly 15 times more data, allowing me to conduct a number of valuable analyses that would be impossible using only data on U.S. House elections. Second, where Meisels primarily differentiates moderate and extremist candidates using CFscores—which have low within-party correlations with roll-call voting and include post-treatment contributions, I introduce and validate a new ideological scaling that uses only contributions received during the primary election from individual donors to scale candidates.[2] In subsequent sections, I show that failing to make this adjustment would cause the researcher to over-estimate the treatment effect by approximately 60%. Further, because a candidate's fundraising relative to their opponent is likely more impactful than raw contribution totals, I focus on candidates' shares of general-election fundraising. Meisels (2024), in contrast, exclusively studies raw fundraising totals. Finally, in addition to a regression discontinuity design, I apply a complementary panel-based identification strategy, which provides the statistical power to systematically evaluate over-time changes and allows me to generalize beyond the small set of close contested primaries included in the RD sample.

Similarly, Hall (2015) finds tentative evidence in U.S. House races that the narrow primary nomination of an extremist candidate causes a large decrease in that party's share of general-election contributions from all types of PACs. While these results are foundational, they do not speak to the donating behavior of individuals—the largest single source of campaign funds and a source that is widely considered to be ideologically motivated—or corporate PACs, nor do they capture heterogeneity in this effect. By studying state legislative races, in addition to congressional contests, I am able to explore this valuable heterogeneity and

[2]In this context, "post-treatment" contributions refer to contributions that candidates receive after the primary-election outcome is observed (i.e., general-election contributions). Appendix G of Meisels (2024) employs a scaling technique that is similar to the method I introduce below. My approach differs from Meisels (2024) in that I exclude contributions from corporate PACs when scaling candidates due to their tendency to combine ideological and strategic motivations (Bonica, 2014, 2018).

obtain more-precise estimates.

In this section, I have outlined theoretical arguments for why campaign contributions might advantage relative moderate or extremist candidates. I have also introduced previous studies that address this question and briefly described the need for additional research on the topic. In the next section, I outline my research design and introduce a new ideological scaling that addresses the concerns raised above.

3 Empirical Strategy

Despite widespread interest in whether campaign finance patterns advantage relative moderates or extremists, obtaining causal evidence on this question is challenging because candidates' ideological positions are chosen strategically and campaign contributions are also commonly used to estimate candidates' ideology. This section addresses these two empirical challenges in turn. I begin by describing a research design that, drawing on Hall (2015) and Meisels (2024), allows me to estimate the causal effect of nominating an extremist candidate in the primary on their party's general-election fundraising outcomes relative to a moderate candidate. Given this research design, I then introduce a new purpose-built ideological scaling that addresses concerns about strategic donating and post-treatment bias while briefly documenting the breadth and importance of these concerns.

3.1 Regression Discontinuity Design in Primary Elections

To evaluate whether campaign finance patterns advantage relative moderates or extremists, I harness the "as-if" random variation in close primary elections between moderate and extremist candidates. This regression discontinuity (RD) design was first introduced by Hall (2015) to study U.S. House candidates' general-election vote shares. More recently, Meisels (2024) extends this design to fundraising in the U.S. House. In this subsection, I introduce the RD, and in the next subsection I describe my procedure for identifying relative moderate

and extremist primary candidates.

For the main results, I estimate OLS regressions of the form

$$Y_{dpt} = \beta_0 + \beta_1 ExtremistPrimaryWin_{dpt} + f(V_{dpt}) + \varepsilon_{dpt}, \quad (1)$$

where $ExtremistPrimaryWin_{dpt}$ is an indicator for the extremist candidate winning party p's primary election in district d and year t, and Y_{dpt} is the party's share of a general-election financial outcome. The term $f(V_{dpt})$ is a flexible function of the running variable (i.e. the extremist candidates' primary vote share). This design facilitates counterfactual comparisons of fundraising outcomes between districts that narrowly nominate relative moderate and extremist primary candidates.

For information on candidates' primary-election vote shares, I rely on primary-election returns compiled by Ansolabehere et al. (2010) and Handan-Nader, Myers, and Hall (2024).[3] For data on campaign contributions, I assemble a dataset containing all general-election contributions from the Federal Elections Commission (FEC, for Congress) and National Institute on Money in Politics (NIMSP, for state legislatures). This dataset includes both itemized and unitemized contributions made after the date of the primary election but before the general election.[4] Collectively, these datasets cover the years 1980-2022 for the U.S. Senate and U.S. House and 1996-2022 for all forty-nine partisan U.S. state legislatures.[5]

The key identifying assumption of this design is that districts that narrowly nominate a relative moderate candidate are, in the limit, identical to districts that narrowly nominate an extremist candidate (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In other words, there must be no district-level sorting at the discontinuity. As Eggers et al. (2015) note,

[3]For the RD, I focus on all primary elections featuring at least two candidates and calculate primary-election vote shares using the top two candidates' vote totals. In the rare case of a primary runoff, I use vote totals from the primary runoff election.

[4]Data on unitemized general-election contributions to congressional candidates was calculated using candidates' monthly, quarterly, pre-general, and post-general Form 3 filings made with the FEC. Unitemized contributions for state-level candidates are available directly from the NIMSP.

[5]I exclude the Nebraska state legislature from my analysis because legislators in Nebraska are not formally affiliated with either party.

this assumption is highly plausible because it is extremely unlikely that primary-election candidates would be able to manipulate vote totals in close elections, or even have the ability to identify their location relative to the discontinuity absent vote modification. Nevertheless, in Appendix A.2 I test for any chance imbalances in my sample by estimating Equation 1 where the outcome is the party's fundraising totals in the previous election cycle. If the "no sorting" assumption holds, these estimates should be null, indicating that, in districts where the more-moderate candidate barely wins, the party did no better in the prior election than in districts where the more-extreme candidate was nominated. The coefficients in Appendix Table A.3 are all exceedingly small, indicating that there is no evidence of such bias.

Under this identification assumption, the RD estimates the effect of narrowly nominating an extremist on their party's general-election fundraising outcomes relative to a moderate. While observers may be most interested in close primary elections because these contests are precisely the settings where the estimated effects are likely to be most meaningful, the results are inherently "local" to a small subset of elections. To evaluate whether these effects generalize to a broader array of electoral contexts, I replicate my main analyses using an observational panel method intended to hold district attributes constant. In addition to identifying a more general estimand than the RD, the panel method is more powerful, reducing the standard errors, and allows me to evaluate variation in the effects over time. These analyses are reported in Section 7.

Finally, as Marshall (2022) notes, my RD design identifies the aggregate effect of candidate ideology and all other candidate-level characteristics that differ between the two types of barely-winning candidates (i.e., compensating differentials). As Hall (2015) notes in the context of the electoral penalty to extremists, studying this bundled treatment is appropriate for evaluating the consequences of primary voters' electoral selection, where all differences between candidate types matter. To understand the underlying mechanisms, however, it is important to examine whether moderate and extremist candidates differ on observable non-ideological characteristics. In Appendix A.3, I test whether barely-winning moderate and

extremist candidates systematically differ in terms of incumbency status, prior office-holding experience, gender, and race. I find no significant differences on these characteristics.

Having described my empirical design, I proceed to outline how I identify relative moderate and extremist candidates. I begin by briefly discussing empirical challenges with existing ideology scalings before introducing a new scaling that addresses these concerns.

3.2 Measuring Ideological Positioning Using Primary-Election Contributions From Individual Donors and Ideological PACs

As the previous section suggests, consistently measuring the ideological positions of both successful and unsuccessful candidates is challenging, particularly when the outcome of interest is also campaign contributions. Traditionally, scholars have used campaign contributions to infer candidates' relative ideological positioning (Bonica, 2014, 2018), but, in the present study, this approach is liable to bias candidates' estimated ideological positions because campaign contributions (i.e., the outcome) are partially determined by primary-election outcomes (i.e., the treatment). Specifically, using primary- and general-election contributions to scale candidates, and then studying general-election financial outcomes, may generate endogeneity in two ways. For brevity, I describe these two concerns briefly below and refer the reader to Appendix A.1 for a more-thorough discussion.

The first concern with jointly scaling a candidate based on the contributions they received both before and after the primary election is that the candidate's position in the associated scaling could be partially a function of their primary-election outcome. Such a scenario would arise if the composition of a candidate's donorate changes after they secure their party's primary nomination. For example, access-seeking PACs may strategically funnel contributions to candidates that are most likely to be elected, regardless of party, causing them—and the candidates to which they contribute—to appear artificially moderate (Hall and Snyder, 2015). In line with this prediction, Panel A of Appendix Figure A.1 shows that winning a primary election causes a 26 percentage point increase in the share of contributions

that a candidate receives from corporate PACs. Further, Panel B of Appendix Figure A.1 shows that winning a primary election also causes a 16 percentage point increase in the share of a candidate's complete donor network that contributed to incumbents (including that candidate herself). Taken together, these results suggest that advancing to a general election causes a large shift in the composition and motivation of a candidates' donors. This possibility is problematic because it may cause bare-primary winners and bare-primary losers to appear systematically different, and even for their classification as relative moderates and extremists to be flipped.[6]

A second set of concerns relates only to bare-primary winners. If primary-election candidates are classified as moderates and extremists based in part on money raised by primary-election winners during the general election, we risk conflating moderation with general-candidates who raise more general-election contributions. This follows because the marginal dollar raised by primary-election winners during the general election may come from donors across an increasingly-wider ideological spectrum, causing that candidate to appear artificially moderate. While it is difficult to test this prediction directly, Appendix Figure A.2 shows that candidates who raise the most contributions have donor bases that also contribute to members of the opposing party nearly four times more often than candidates who raise the least contributions. This possibility is problematic for my analysis because general-election candidates' classifications as moderates and extremists may endogenously reflect fundraising totals, rather than ideology.

To address these concerns, I restrict the data used to infer candidates' ideological positions in two ways. First, because of concerns about post-treatment bias and the fact that primary-election winners will receive additional contributions in the general election that primary losers will not, I restrict the set of training contributions to those received in primary elections. This restriction matches the training procedures of Hall and Snyder (2015). And second, because access-seeking PACs may cause primary winners to appear systematically

[6]In addition to altering the dichotomous classification of candidates as extremists and moderates, this scaling bias would also affect the treatment "intensity," or the degree of ideological contrast between candidates, which I also study below.

more moderate than primary losers, I exclude all PAC contributions from the contribution matrix. This restriction matches the training procedures of Bonica (2014, 2018). In sum, I impute candidates' ideological positions using only contributions made by individual donors during the primary election cycle.[7] This prediction set is entirely distinct from the set of general-election contributions that serve as my outcome. While this approach is statistically inefficient, these restrictions are necessary to short-circuit concerns about strategic donating and post-treatment bias. As I discuss below, these restrictions are quite meaningful. In Appendix A.1.2, I show that using post-treatment contributions to scale candidates would cause the researcher to over-estimate treatment effects by approximately 60%.

With the prediction set in hand, I follow Bonica (2018) and Hall and Snyder (2015) and impute candidates' ideology as the contribution-weighted average roll-call voting score of the incumbents to which a candidate's donors also contributed.[8,9] This estimation process proceeds in two stages and is conducted separately for members of Congress and state legislators. First, I estimate the ideology of all donors as the average contribution-weighted ideology of the incumbents to which a donor contributes. More formally, let $Contribution_{ij}$ be the donation amount from donor j to candidate i and $Roll\text{-}Call\ Voting_i$ be incumbent i's roll-call voting scaling given by DW-NOMINATE for members of Congress (Lewis et al., 2024) or their NP-Score for state legislators (Shor and McCarty, 2011).[10] Then donor j's

[7] I also require that donors donate to at least two distinct incumbents and candidates receive contributions from at least two scaled donors to be included in my analysis, matching Bonica (2014).

[8] Bonica (2018) estimates a variety of supervised ideology scalings. This paper focuses on Boncia's "Supervised CFscores" which use tenfold cross-validation to estimate donors' ideology based on their contribution-weighted donations to incumbent legislators. Candidates' positions are then imputed as the donation-weighted ideology of their donors.

[9] Other papers that have adopted this scaling approach include Hall (2015), Hall and Thompson (2018), and Handan-Nader, Myers, and Hall (2024).

[10] Both DW-NOMINATE and NP-Scores are static over a legislator's career and are comparable across legislative sessions and between chambers. Data on DW-NOMINATE scalings includes 2,267 legislators and was downloaded from `https://voteview.com/data`. The most recent release of NP-Scores includes 27,629 distinct incumbent legislators and was downloaded from `https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/NWSYOS`.

revealed ideological preference is given by

$$Donor\ Ideology_{-i,j} = \frac{\sum_{w \neq i} Roll\text{-}Call\ Voting_w\ Contribution_{wj}}{\sum_{w \neq i} Contribution_{wj}},$$

where I leave out candidate i when estimating donor j's ideology to avoid a feedback loop.[11] Subsequently, I estimate each candidate's ideology as

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

For the remainder of this paper, I refer to this scaling as a candidate's *Primary-Specific Scaling*.

Having outlined my scaling method, I provide two concrete examples of how incorrectly using both primary- and general-election contributions to scale candidates would affect my results. For brevity, the full results are reported in Appendix A.1.2. In short, using the same scaling technique as outlined above, I create a *Post-Treatment Scaling* that uses primary- and general-election contributions to scale candidates. Comparing my preferred *Primary-Specific Scaling* and the *Post-Treatment Scaling*, I first show that using the *Post-Treatment Scaling* would cause the researcher to "flip" 12% of primary candidates' moderate and extremist designations, relative to the *Primary-Specific Scaling*. Second, to evaluate whether these "flips" are consequential, I reestimate my main results using the *Post-Treatment Scaling*. I find that using post-treatment contributions to scale candidates would cause the researcher to over-estimate treatment effects by approximately 60%. In short, the restrictions I impose on the data used to scale candidates are highly consequential.

Finally, using the *Primary-Specific Scaling*, I tentatively identify a primary election as occurring between a relative moderate and extremist when the ideological distance between the two candidates with the top two primary-election vote shares is at or above the median

[11] This method yields a separate donor scaling for every candidate-donor pair. All subsequent results are very similar when including candidate i in donor j's ideology (i.e., $Donor\ Ideology_j = \frac{\sum_i NPScore_i\ Contribution_{ij}}{\sum_i Contribution_{ij}}$).

of the distribution of ideological distances across my sample. In subsequent sections, I show that my results grow as this treatment intensity threshold increases.

3.3 Validating Primary-Specific Ideology Scalings

For candidates that win the general election, it is possible to validate this primary-specific scaling by comparing it with that candidate's roll-call voting in office. I conduct two empirical exercises to facilitate this comparison. First, Figure 1 plots the relationship between the *Primary-Specific Scaling* and legislators' observed roll-call voting as measured by DW-NOMINATE scalings and NP-Scores. As the figure shows, the overall correlation is .90, while the within-party correlation is .75 for Democrats and .72 for Republicans.

Figure 1 – Correlation Between Primary-Specific Scaling and Roll-Call Voting. This figure plots the correlation between general-election winners' contribution-based estimated ideology (i.e., *Primary-Specific Scaling*) and their roll-call voting once in office (i.e., NP-Scores) for Democrats (circle) and Republicans (triangle). Diamonds represent equal-group-size averages.

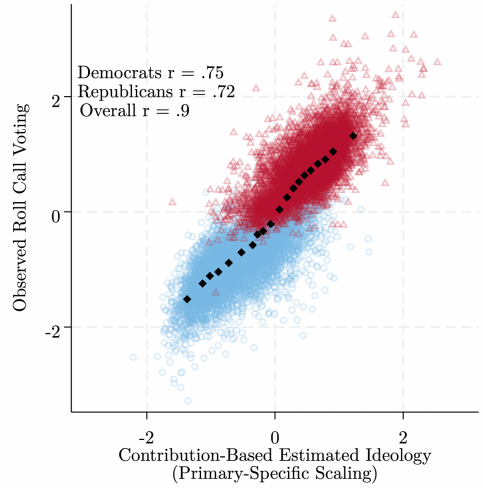


Table 1 – Percent of Congressional and State Legislative Roll-Call Votes Classified Correctly. Primary-specific scalings predict roll-call votes better than CFscores or a naive indicator for party, and nearly as well as scalings derived directly from incumbents' roll-call voting (DW-NOMINATE/NP-Scores).

Scaling	Overall	Congress	State Legislatures
DW-NOMINATE/NP-Scores	0.911	0.904	0.914
	(0.759)	(0.764)	(0.755)
Primary-Specific Scaling	0.895	0.895	0.900
	(0.716)	(0.720)	(0.713)
Static CFscore	0.886	0.891	0.882
	(0.696)	(0.734)	(0.662)
Party	0.857	0.500	0.857
	(0.587)	(0.845)	(0.586)

Note: Aggregate proportional reduction in error (APRE) reported in parentheses. Table is ordered by overall classification rate.

Second, I use the *Primary-Specific Scaling* to predict the outcome of actual roll-call votes. To do so, I follow Bonica (2014, 2018) and calculate the share of state legislative roll-call

votes that can be correctly classified using an optimal cutting-point procedure described in Poole (2007).[12] For this exercise, I construct a panel containing 72 million roll-call votes cast in all 99 state legislative chambers for the years 2000-2024 and 12 million roll-call votes in Congress for the years 1980-2024. Appendix A.9 describes the exact coverage of this dataset and the specifications of this classification exercise.

Table 1 reports the classification rates and aggregate proportional reduction in error (APRE) for the primary-specific scaling and, for comparison, Static CFscores, an indicator for party, and scalings derived directly from incumbents' roll-call voting in office (DW-NOMINATE for members of Congress and NP-Scores for state legislators).[13] DW-NOMINATE and NP-Scores are estimated using roll-call votes themselves and represent a theoretical upper-bound on classification rate, while static CFscores are estimated using the full contribution matrix (i.e., primary- and general-election contributions). I find that the *Primary-Specific Scaling* predicts 89.5% of state legislative roll-call votes correctly ($APRE = .716$), outperforming CFscores and an indicator for party, and closely behind DW-NOMINATE and NP-Scores themselves (91.1%; $APRE = .759$). In sum, despite restricting the size of the training contribution matrix, I am still able to consistently recover candidates' ideological positioning.

Finally, to ensure that my results are not an artifact of this contribution-based scaling, I replicate my main panel-based results in Appendix A.5 using a separate measure of ideology that draws on the state legislative roll-call voting records of members of Congress who previously served in state legislatures.[14] The results using this strategy are highly similar to the findings reported in the body of this paper.

[12]Specifically, for every roll-call in our dataset, I find the maximally-classifying point in one-dimensional space that predicts "Yea" votes on one side and "Nay" votes on the other. I then report the percentage of all votes cast that are correctly predicted.

[13]$\text{APRE}_i = \frac{\sum_{j=1}^{J}\{\text{minority vote}_j - \text{classification errors}_{ij}\}}{\sum_{j=1}^{J}\text{minority votes}_j}$ for scaling i and roll call j. This quantity measures the extent to which a given scaling improves upon the naive prediction that every legislator always votes with the majority.

[14]This approach does not use any campaign contributions to scale candidates. I focus on the panel-based identification strategy, rather than the RD, because few congressional primaries feature two top-finishing candidates with previous state legislative roll-call voting records.

4 Effect of Extremist Nominees on Campaign Contributions

Having detailed my empirical strategy and outlined competing theoretical perspectives on whether campaign contributions, on average, advantage extremist candidates, I begin by presenting results that focus on candidates' aggregate fundraising outcomes. Then, to better understand the sources underlying these trends, I disaggregate these financial outcomes by donor type and institutional settings in subsequent sections.

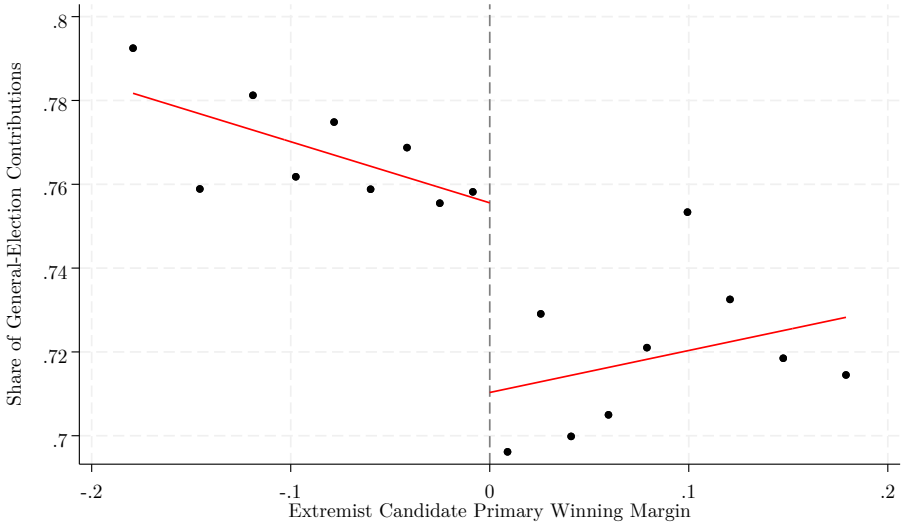
4.1 General Election Donors Punish Extremist Primary Nominees

Do general-election donors, on average, punish extremist primary nominees? Figure 2 plots the data across the discontinuity to answer this question. In this figure, I tentatively identify a race as occurring between a moderate and extremist when the distance between the two candidates is at or above the median of the distribution of ideological distances in my sample. The running variable on the horizontal axis of Figure 2 is the extremist candidate's primary election winning margin, and the outcome on the vertical axis is their party's share of all contributions made during the general election. On the horizontal axis, when the winning margin is greater than 0, the extremist candidate wins the primary nomination and represents the party in the general election. When the winning margin is instead less than 0, the moderate candidate wins the primary nomination and runs in the general election. As the figure depicts, when a district shifts from barely nominating a moderate primary candidate to an extremist, the candidate's party receives approximately 5 percentage points less of the combined contributions in the general election.[15]

Table 2 evaluates this relationship more formally. As is standard in RD analyses, I report estimates across a variety of specifications for $f(V_{dpt})$ and bandwidths. In the first column, I

[15]Note that the outcome is above 50% on both sides of the discontinuity because contested primaries are more common in districts that favor a party both electorally and financially, and some general election races are uncontested.

Figure 2 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a 5-6 percentage point decline in their party's share of total general-election contributions relative to a moderate. Black dots represent averages within equal-sample-sized bins of the running variable. Red lines report OLS regressions estimated separately on either side of the discontinuity using the underlying data.



use a 10% bandwidth and a local-linear specification of the running variable that allows for different slopes on either side of the discontinuity. In columns two through four, I fit a third-order polynomial, a third-order spline, and a fifth-order polynomial of the running variable. Finally, column five reports the effect estimated by the method of Calonico, Cattaneo, and Titiunik (2014), which uses kernel regression with a triangular kernel and a bandwidth that minimizes the mean-squared error of the estimator.

Across specifications, Table 2 reports consistent negative effects of nominating an extremist primary candidate on the party's general-election contribution share. Consider the coefficient reported in column one. Here, I estimate that nominating an extremist causes a 5 percentage point decrease in their party's share of total general-election contributions relative to a moderate.[16] Looking across the table, I find uniform evidence that extremist nominees damage their party's fundraising prospects, with estimates ranging from 5 to 6

[16]For brevity, I focus on this local-linear specification throughout the remainder of the paper.

Table 2 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a 5-6 percentage point decrease in that party's share of general-election contributions.

	Share of Total General Election Contributions				
	(1)	(2)	(3)	(4)	(5)
Extremist Primary Win	-0.05	-0.05	-0.06	-0.05	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	2,807	5,461	5,461	5,461	3,614
Polynomial	1	3	3	5	CCT
Spline	Yes	No	Yes	No	Yes
Bandwidth	.10	-	-	-	0.14

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Polynomial reports the largest exponent of the running variable included in the regression. CCT refers to the method of Calonico, Cattaneo, and Titiunik (2014).

percentage points. While these estimates are all statistically significant at traditional levels, the results lack a degree of precision. In the next subsection, I show that the results become meaningfully more precise as I estimate the treatment effect across different levels of treatment intensity.

The estimates reported in Table 2 aggregate over a variety of different primary-election contexts. To better understand these effects, I disaggregate my overall results by two key features of primary elections.

First, a vast literature finds that incumbents enjoy a substantial electoral and financial advantage over their opponents (e.g., Fouirnaies and Hall, 2014; Levitt and Wolfram, 1997). If incumbency status is correlated with ideological moderation, my results might be explained by the absence of a financial incumbency advantage following an extremist's nomination.[17] To evaluate this possibility, I examine open-seat races—a set of primary contests where neither the moderate nor the extremist possess an incumbency advantage. Open-seat races

[17]In the terminology of Marshall (2022), incumbency would be a "compensating differential." Rather than invalidating the RDD, this differential would be part of the treatment assigned by the close primary election. Nevertheless, evaluating this possibility is important for substantively interpreting my results.

Table 3 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share By Primary Type in Congress, 1980-2022, and State Legislatures, 1996-2022. The financial penalty to extremists is largest in open-seat elections and smaller in districts that are safe for the party.

	Overall Estimate	Open Seat Elections	Districts Safe for Party
	(1)	(2)	(3)
Extremist Primary Win	-0.05	-0.07	-0.03
	(0.02)	(0.03)	(0.03)
N	2,807	1,109	1,275
Polynomial	1	1	1
Spline	Yes	Yes	Yes
Bandwidth	.10	.10	.10

Note: Robust standard errors are are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election.

are also highly consequential contests in and of themselves; in my sample, fully 77% of state legislators and 64% of members of Congress first enter office through an open-seat election.

A second trait of primary elections that is relevant for interpreting these overall effects is whether a district is a safe for the party holding the primary. In districts that are strongly aligned for the primary-holding party, the general-election outcome is relatively predictable, and donors may need not worry about the viability of an extremist nominee. Hence, the financial penalty to extremists may be smaller in these safe primary elections. I test this prediction by identifying districts as "safe" if a party's share of the two-party presidential election vote averaged over a redistricting cycle is greater than 60%. In my sample, almost exactly 50% of districts are classified as "safe."

The findings from this analysis are reported in Table 3. In the first column of Table 3, I replicate my baseline estimate from column two of Table 2. Column two then reports my estimate of the effect of nominating an extremist on general-election contributions in open-seat races. The effect in open-seat races appears to be marginally larger in magnitude than in my overall sample (-5 vs. -7 percentage points), although this difference is not statistically significant. At the very least, this result is inconsistent with the hypothesis that the observed aggregate effect is due to the removal of a financial incumbency advantage. Finally, the third

estimate in Table 3 uses only districts that are "safe" for a party. Matching expectations, I find that the financial penalty to extremism is smaller in these uncompetitive districts, perhaps due to heightened partisan loyalty or a lack of viable alternatives.

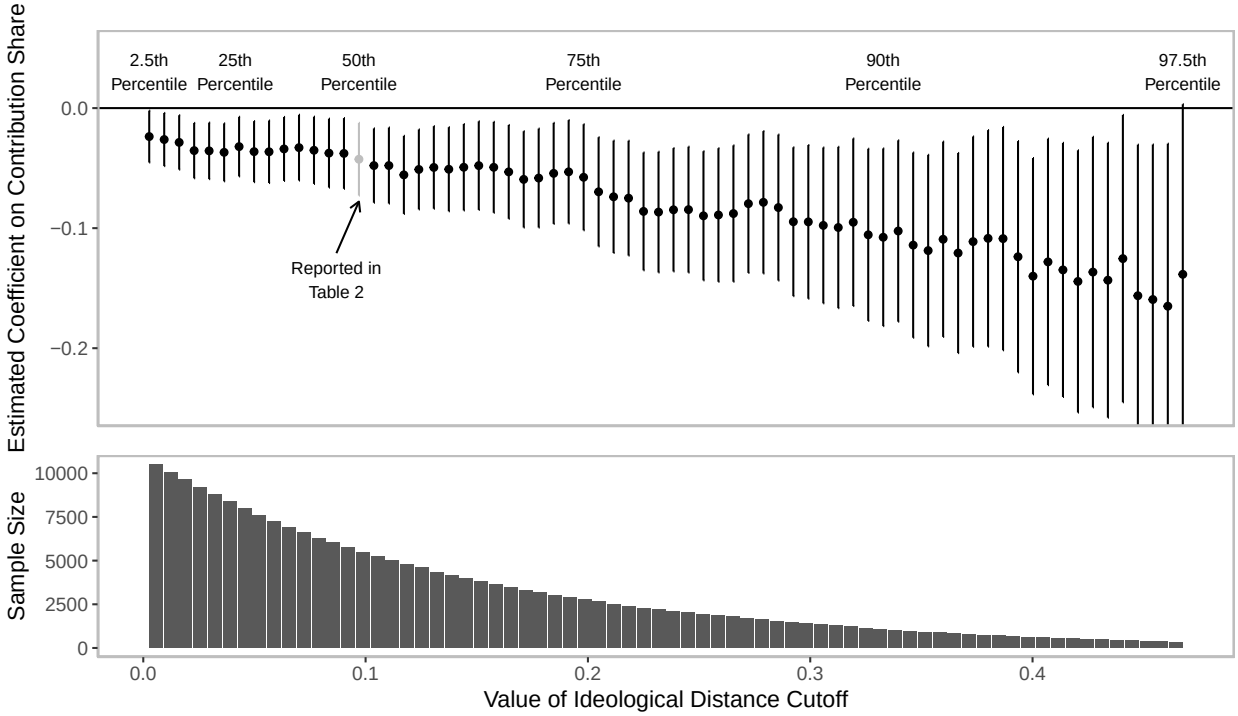
Overall, this section has shown that donors, on average, punish parties that nominate extremist primary candidates. The effect is estimated to be larger in open-seat elections, where the electoral stakes are particularly high, and smaller in districts with uncompetitive general elections.

4.2 Financial Penalty Increases With Candidate Extremism

In the results presented so far, I have identified a primary election as occurring between a relative moderate and extremist when the ideological distance between the two candidates is at or above the median of the distribution of ideological distances across my sample. Since candidates' ideological positions are estimated with a degree of error, this cutoff is designed to ensure that I analyze only true contrasts between candidates' platforms. This cutoff also ensures that the difference between candidates' ideological positioning is meaningful and that voters are able to distinguish moderate and extremist candidates.

There is nothing particularly unique about the median of this distribution, however, and we can learn more about the financial penalty to extremists by studying the variation in candidate contrasts. As the value of the ideological distance cutoff increases, the treatment intensity grows, so an important robustness check is to evaluate whether the identified treatment effect grows in parallel with the ideological cutoff. Figure 3 tests this theoretical prediction by estimating Equation 1 across values of the ideological distance cutoff. Because the sample becomes arbitrarily small as the cutoff increases, I employ a cubic specification of the running variable for this exercise, although the results are highly similar across the specifications reported in Table 2. The horizontal axis of Figure 3 plots the cutoff value, and the 2.5th, 25th, 50th, 75th, and 97.5th percentiles of this distribution are reported at the top of the figure. The top panel plots the estimates and 95% confidence intervals across values

Figure 3 – Effect of Extremist Nominee on General-Election Contributions Across Possible Cutoffs in Congress, 1980-2022, and State Legislatures, 1996-2022. The top panel plots estimates from Equation 1 across different values of the ideological distance cutoff (i.e., the distance between the top two primary candidates required to identify relative moderates and extremists). Estimates use a cubic specification of the running variable fit on all data. Vertical lines represent 95% confidence intervals. The bottom panel reports the sample size for each regression. As the contrast between extremist and moderate candidates is increased, the effect of nominating an extremist candidate on general-election contributions relative to a moderate grows.



of the cutoff. The lower panel reports the sample size for each regression. For reference, the estimate reported in column two of Table 2 is plotted in grey with an accompanying arrow.

I find that the effect of nominating an extremist on general-election receipts grows substantially as the contrast between moderate and extremist candidates increases. These estimates increase from 3 percentage points at the 10th percentile of the cutoff distribution to 18 percentage at the 97.5th percentile of the cutoff distribution.

To more-formally explore this variation, I rescale the ideological *Distance* variable to run from 0 to 1, and interact it with *Extremist Primary Win*. Hence, the interaction term reports the estimated change in the causal effect of nominating a primary extremist between

Table 4 – Effect of Nominating an Extremist Primary Candidate Across Treatment Intensities in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a 24-45 percentage point decline in their party's share of general-election contributions in contests with the largest contrast between primary-election candidates.

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	0.01	0.01	0.02	0.02
	(0.02)	(0.01)	(0.02)	(0.01)
Extremist Primary Win · Distance	-0.29	-0.26	-0.26	-0.26
	(0.07)	(0.04)	(0.05)	(0.04)
Distance	0.17	0.21	0.21	0.21
	(0.05)	(0.03)	(0.05)	(0.03)
N	5,788	10,905	10,905	10,905
Polynomial	1	3	3	5
Spline	Yes	No	Yes	No
Bandwidth	.10	-	-	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Polynomial reports the largest exponent of the running variable included in the regression.

the smallest and largest between-candidate ideological contrasts.[18] The results are reported in Table 4.[19] Looking at the second row, we find that financial penalty to extremists ranges from 24 to 28 percentage points in races where the contrast between candidates is largest.[20] Clearly, the financial penalty to extremist primary nominees in these races is substantial and meaningful.

[18]The coefficient on the lower-order term *Extremist Primary Win* estimates the financial penalty to extremists when the contrast between primary candidates is the smallest in my sample. Since this difference is nearly zero, it is not surprising that I find no effects on this term.

[19]I exclude the CCT specification from Table 4 because it is not possible to estimate an interaction term using *rdrobust*.

[20]These quantities are calculated by summing the coefficients on *Extremist Primary Win* and *Extremist Primary Win · Distance*.

5 Which Donors Punish Extremist Primary Nominees?

The results presented thus far indicate that, in aggregate, donors punish extremist primary nominees in general elections. While these overall estimates are undoubtably important, they may obscure heterogeneity that is essential for interpreting the overall effect. As discussed in Section 2, existing research yields conflicting predictions for whether individual and corporate PAC donations—two of the largest contribution sources—advantage relative moderates or extremists. I begin by testing these competing predictions about individual and corporate PAC donors. I then turn to two other key sources of campaign funds that have received extensive scholarly attention—out-of-state donors and extremist- vs. out-party donors.

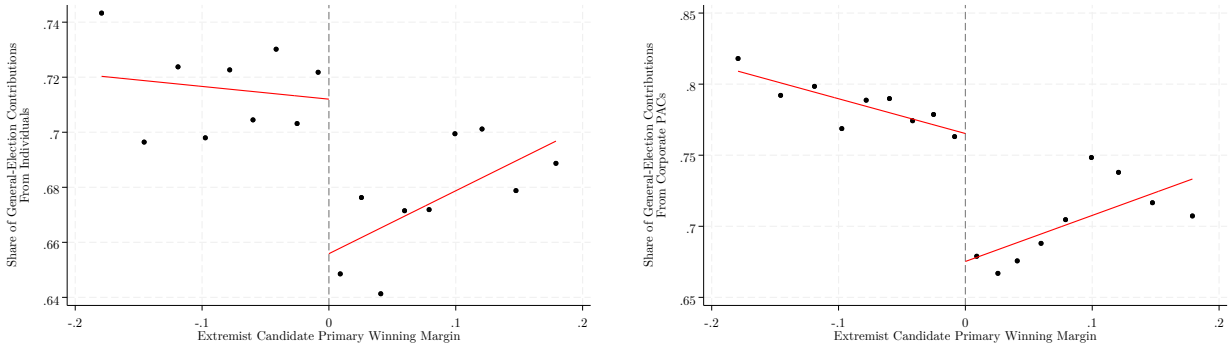
5.1 Individuals and Corporate PACs Punish Extremist Nominees

As discussed in the Section 2, existing research disagrees on whether individual and corporate PAC contributions should favor relative moderates or extremists. Much as candidates benefit from ideological moderation at the ballot box (Hall, 2015), donations from individuals may advantage moderate candidates, or the individual donorate may be so skewed towards extremist donors (Hill and Huber, 2017) that contributions from individuals favor extremists. And corporate PACs might prioritize ideological congruence and donate to moderates (Bonica, 2013), or they might primarily value a form of access to policy makers that is independent of ideology (Hall and Wayman, 1990). Ultimately, whether these sources of campaign funds advantage relative moderates or extremists is an empirical matter to which I now turn.

To answer this question, I disaggregate each candidate's fundraising totals into its various sources using donor-level industry classifications from the Center for Responsive Politics (CRP) and NIMSP.[21] For each donor type, I construct a new outcome variable containing the

[21] CRP industry-level classification are not available for elections before 2000. Hence, for the industry-level estimates reported in the Appendix, I restrict my sample of congressional primaries to the years 2000-2022. I am, however, able to measure total contributions from corporate PACs using classifications from the FEC, so I include all congressional primaries in Figure 4 in the main text. Industry-level classifications are available for all years of the NIMSP data.

Figure 4 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share from Individual and Corporate PAC Donors in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a significant decrease in their party's share of total general-election contributions from both individual and corporate PAC donors relative to a moderate. Black dots represent averages within equal-sample-sized bins of the running variable. Red lines report OLS regressions estimated separately on either side of the discontinuity using the underlying data.



(a) Contributions from Individuals **(b)** Contributions from Corporate PACs

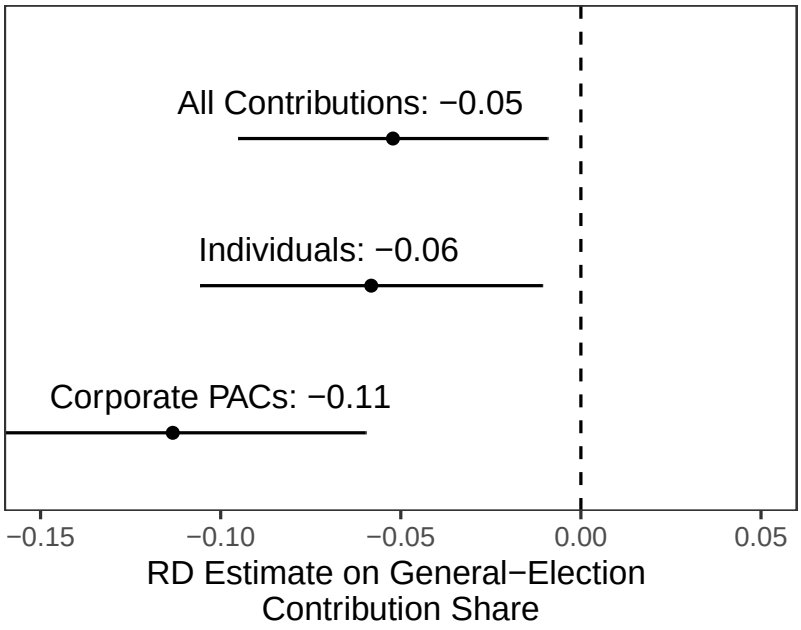
party's share of general election-contributions originating from that source. I also construct an aggregate measure of contributions from corporate PACs and individual donors. These variables measure the extent to which a given contribution source advantages a party.

Using these source-specific contribution shares as the outcome, Figure 4 plots the discontinuity in the data separately for general-election contributions from individual donors (panel A) and corporate PACs (panel B). As a reminder, when the winning margin is greater than 0, the extremist candidate wins the primary nomination and represents the party in the general election. When the winning margin is instead less than 0, the moderate candidate wins the primary nomination and runs in the general election. There appears to be a sharp decrease in contribution shares at the discontinuity in both plots, with a noticeably larger jump for corporate PACs than individual donors.

Figure 5 presents formal estimates of these discontinuities using a local-linear specification of the running variable.[22] Horizontal lines in the plot represent 95% confidence intervals. For reference, the first estimate, labeled "Overall Contributions," corresponds to the estimate

[22]Appendix Table A.4 reports additional estimates using the series of specifications reported in Table 2.

Figure 5 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share by Donor Type in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes larger financial penalty among corporate PACs than individual donors. This figure reports estimates using a local-linear specification of the running variable and a 10% bandwidth.

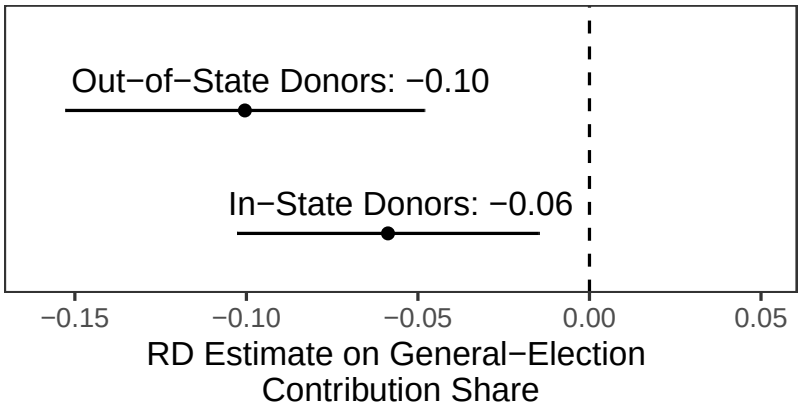


from column two of Table 2.

The second estimate in Figure 5 uses only contributions from individual donors. Here, I find that, when a party narrowly nominates an extremist, its share of general-election contributions from individuals declines by 6 percentage points. The final estimate in Figure 5 aggregates contributions from corporate PACs. My estimate for these donors is a 11 percentage point penalty. As Figure 4 above suggests, I find that contributions from corporate PACs are more sensitive to extremist nominees than contributions from individual donors. The difference in effect sizes between these two contribution sources is highly significant ($t = 2.43$, $p = .015$; SEs clustered by district).

Finally, I investigate heterogeneity within the sets of individual and corporate PAC donors. Since the results do not change the substantive interpretation of this section, I refer interested readers to Appendix A.4 for the complete results. In short, I find that the financial penalty to ideological extremism is remarkably stable across all 11 corporate

Figure 6 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share from In-State and Out-of-State Donors in Congress, 1980-2022, and State Legislatures, 1996-2022. The financial penalty to extremists is larger among out-of-state donors than in-state donors. This figure reports estimates using a local-linear specification of the running variable and a 10% bandwidth.



industries defined by the FEC and NIMSP.

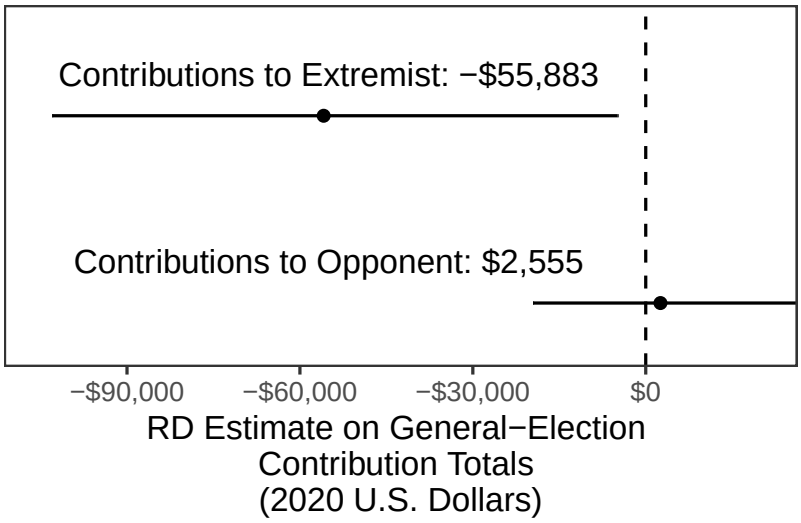
Overall, I find strong evidence that both individuals and corporate donors punish extremist nominees, and this effect is larger for corporate PACs than individuals.

5.2 Greater Penalty Among Out-of-State and Extremist-Party Donors

In addition to theories of individual and corporate giving, political science research has advanced numerous predictions for whether contributions from other groups of donors exacerbate polarization. In this section, I evaluate predictions about two additional highly consequential groups: in-state versus out-of-state donors and extremist-party versus out-party donors.

As digital contribution platforms have surged over the past two decades, the amount of contributions in American elections from out-of-state donors has increased exponentially. In 2022, nearly 80% of contributions to congressional candidates originated from out-of-state donors, up from 25% in 2000. Recent work suggests that the rise of out-of-state donors causes legislators to vote more in line with their party's national donor base (Baker, 2016; Canes-Wrone and Miller, 2022). Out-of-state donors are also thought to be more partisan

Figure 7 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Totals in Congress, 1980-2022, and State Legislatures, 1996-2022. The financial penalty to extremists is driven by donors contributing less to extremist nominees, rather than donors contributing more to extremists' opponents. This figure reports estimates using a local-linear specification of the running variable and a 10% bandwidth.



than their in-state counterparts (Gimpel, Lee, and Pearson-Merkowitz, 2008). Below, I test whether in-state and out-of-state donors differ, on average, in their support for extremist primary nominees.

Second, while I focus on candidates' contribution shares, because fundraising relative to an opponent is likely more consequential than absolute dollar amounts, studying fundraising totals allows me to differentiate between the donating behavior of extremist-party and opposing-party donors. Previous research using the RD suggests that extremist primary nominees suffer at the ballot box because they decrease their party's share of general-election turnout (Hall and Thompson, 2018). Campaign contributions may function in a similar manner, or, since donors can contribute to candidates outside their district, the financial penalty to extremists may be driven by extremist-party donors.

The results from the analysis of in- and out-of-state donors are plotted in Figure 6. Each row in Figure 6 plots an estimate from Equation 1 for a separate group of contributions, and horizontal lines represent 95% confidence intervals. Looking at the two estimates, I find that

out-of-state donors are two times more sensitive to the nomination of an extremist primary candidate than in-state donors. This difference is highly significant ($t = 2.37$, $p = .018$; SEs clustered by district). The results suggest that out-of-state donations are less-polarizing than previously thought.

Second, results from the analysis of extremist-party and opposing-party donations are plotted in Figure 7. Here, I estimate Equation 1 using logged contribution totals as the outcome separately for donations to the extremist candidate (top row) and donations to their opponent (bottom row). Estimates are converted to levels for interpretability. The first estimate in Figure 7 indicates that the "coin flip" primary nomination of an extremist causes their party to raise roughly $56,000 less than a barely-winning moderate's party, consistent with a strong penalty among extremist-party donors. The second estimate, in contrast, indicates that, following the "coin flip" nomination of an extremist, the extremist's opponent raises approximately the same amount relative to a barely-winning moderate's opponent. The difference in estimates between rows one and two of Figure 7 is highly significant ($t = 4.23$, $p < .001$; SEs clustered by district). In short, the financial penalty to extremists appears to be driven by donors abandoning extremist nominees, rather than a surge among their opponents' donors. Future work should seek to understand these striking results in more detail.

6 State-Level Variation in Aggregate Penalty to Extremists

In addition to providing statistical power for aggregate-level analyses, the rich institutional variety of state legislatures allows for valuable analyses of state-level heterogeneity, many of which would be infeasible for congressional elections. By identifying where the penalty to extremists is larger and smaller, we can learn more about the overall financial penalty to extremist nominees. Since I lack "as-if random" variation in these moderating variables,

however, any causal interpretation in this section requires caution.

In this section, I examine heterogeneity in the aggregate-level fundraising results presented in Section 4. Table 5 examines heterogeneity in the aggregate financial penalty to extremism by the level of district news coverage, legislative professionalism, state-level polarization, and election timing. Across all columns, I use a cubic specification of the running variable estimated on the full dataset and interact *Extremist Primary Win* with a key moderator variable.[23] For reference, the first column of Table 5 replicates column two of Table 2.

A key pre-condition for a financial penalty to extremism is that donors have information about candidates' relative ideological positioning. In the absence of such information, donors may be unable to react to candidates' ideological positioning. While it is challenging to test this prediction directly, I can begin to evaluate this mechanism by leveraging inter-district variation in the legislative news environment. To do so, I draw on Myers's (2024) measure of the "congruence" between state legislative districts and newspaper markets. Based on work by Snyder and Stromberg (2010), Myers (2024) finds that the haphazard overlap of these two geographies exogenously shapes the amount of newspaper coverage that voters receive about their state legislator. I scale *News Coverage* to run from 0 (least congruent district) to 1 (most congruent district) in my sample. Column two of Table 5 presents the results. I find that the financial penalty to extremists may be in excess of two times larger in the most congruent districts in comparison to districts with the least congruence, although the results are estimated noisily. These results tentatively suggest that news coverage plays an important role in driving the financial penalty to extremists.

Next, in column three I interact *Extremist Primary Win* with Squire's (2017) measure of legislative professionalization, scaled to run from 0 (least professionalized) to 1 (most professionalized) in my sample. To the extent that it makes legislative races more salient to donors, legislative professionalization may be an important moderator of the penalty to

[23]Data on the moderator variables is not available for a small number of district-years and are omitted from Table 5.

Table 5 – Variation in RD Estimate of Financial Penalty to Extremist Nominees in State Legislatures, 1996-2022. The financial penalty to extremists is larger in more-professionalized state legislatures, smaller in more-polarized states, no smaller in midterm and odd-year elections, and may be significantly larger in districts that receive stronger news coverage. *New Congruence*, *Professionalization*, and *Polarization* are scaled to run from 0 (lowest) to 1 (highest), and *Midterm Year* and *Odd Year* are indicator variables. This table reports estimates using a local-linear specification of the running variable and a 10% bandwidth.

	Share of Total General Election Contributions				
	(1)	(2)	(3)	(4)	(5)
Extremist Primary Win	-0.05	-0.03	-0.04	-0.10	-0.05
	(0.02)	(0.05)	(0.03)	(0.04)	(0.03)
Extremist Primary Win · News Congruence		-0.08			
		(0.07)			
Extremist Primary Win · Professionalism			-0.09		
			(0.04)		
Extremist Primary Win · Polarization				0.09	
				(0.05)	
Extremist Primary Win · Midterm Year					-0.01
					(0.02)
Extremist Primary Win · Odd Year					-0.00
					(0.08)
N	2,190	1,751	1,779	1,947	2,190
Polynomial	1	1	1	1	1
Spline	Yes	Yes	Yes	Yes	Yes
Bandwidth	.10	.10	.10	.10	.10

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Lower-order terms are omitted from the table for brevity. See Appendix Table A.5 for complete results.

extremism. In more professionalized legislatures, for example, the stakes of election outcomes may be elevated, and donors might respond by more-carefully donating to candidates. Column three of reports evidence in line with these predictions. I find that the penalty to extremist primary nominees is two times larger in the most professionalized legislatures in comparison to the least professionalized legislatures. These results match Handan-Nader, Myers, and Hall (2024) and Rogers (2017), who find that the electoral return to ideological moderation is higher in states with more-professionalized legislatures.

The degree of ideological polarization in a state may also influence the penalty to extremists. Donors in highly polarized environments may exhibit stronger partisan—as opposed to ideological—loyalty, leading them to continue to support their party's general-election candidates regardless of their ideological positioning. To evaluate this mechanism, I calculate state-level *Polarization* as the difference between the median Republican and Democrat's NP-Score within each state, averaged across a rolling two-election window. I scale *Polarization* to run from 0 (least polarized) to 1 (most polarized) in my sample. Column four of Table 5 shows that the financial penalty to extremism is nearly erased in the most-polarized states in comparison to the least-polarized states.

Finally, in column five I test whether the financial penalty to extremists is smaller in midterm elections and odd-year elections. By studying the penalty to extremists in elections without a presidential contest (midterm elections) and races without any regularly-scheduled federal race (odd-year elections), I am able to test whether the nationalization of elections has limited the penalty to extremism (Hopkins, 2018; Rogers, 2023). For both midterm and odd-year elections, I estimate relatively precise null interactions with *Extremist Primary Win*. These null results suggest that the nationalization of elections has not limited the penalty to extremism.

In sum, by leveraging the rich institutional heterogeneity across state legislatures, I find that the financial penalty to extremists is larger in more-professionalized states, smaller in more-polarized states, of equal size in midterm or odd-year and on-cycle elections, and may be stronger when legislative news coverage is stronger.

7 Replicating the RD Using Panel Identification Strategy

In the previous sections, I have leveraged the "as-if" random variation in primary-election outcomes to evaluate whether general-election donors punish extremist primary nominees. While observers may be most interested in close primary elections because these contests are precisely the settings where the estimated effects are likely to be most meaningful, the

results are inherently "local" to a small subset of elections. To evaluate whether these effects are generalizable to a broader array of electoral contexts, I replicate my main analyses using an observational panel method intended to hold district attributes constant. In addition to identifying a more general estimand than the RD, the panel method is more powerful, reducing the standard errors, and allows me to evaluate variation in the effects over time.

7.1 The Midpoint Method

I replicate my main results using the "midpoint" method of Ansolabehere, Snyder, and Stewart (2001).[24] This method uses either district fixed effects or district presidential vote share to control for partisanship, and compares changes in the midpoint between Democratic and Republican general-election candidates. In the spatial model, when the midpoint between candidates moves to the right while the distance between the candidates remains the same, the Democratic candidate is unambiguously better off. In adopting this model, I implicitly assume that donors can be arranged on a unidimensional line in the same way voters are traditionally arranged.

For district d in election t, I implement the midpoint method by estimating OLS regressions of the form

$$Y_{dt} = \beta_0 + \beta_1 Midpoint_{dt} + \beta_2 Distance_{dt} + \delta_t + \gamma_i + \varepsilon_{dt}, \tag{2}$$

where $Midpoint_{dt} = \frac{DemIdeology_{dt} + RepIdeology_{dt}}{2}$ is the midpoint between the Democratic and Republican candidates' *Primary-Specific Scaling*, $Distance = |DemIdeology_{dt} - RepIdeology_{dt}|$ is the distance between the two parties' candidates, and Y_{dt} is one of the outcomes introduced in Section 4 and Section 5. The term δ_t stand in for year fixed effects, and γ_i represents either district-regime fixed effects or district presidential vote share.

[24] Other studies that leverage the "midpoint" design include Hall and Snyder (2015), Hall (2019) and Handan-Nader, Myers, and Hall (2024). I prefer the "midpoint" method over the "candidate extremism" method of Canes-Wrone, Brady, and Cogan (2002), because this approach does not require assuming that Democrats and Republicans are to the left and right of the median voter, respectively, or that zero is the reference point from which I compute ideological distances (Hall, 2019).

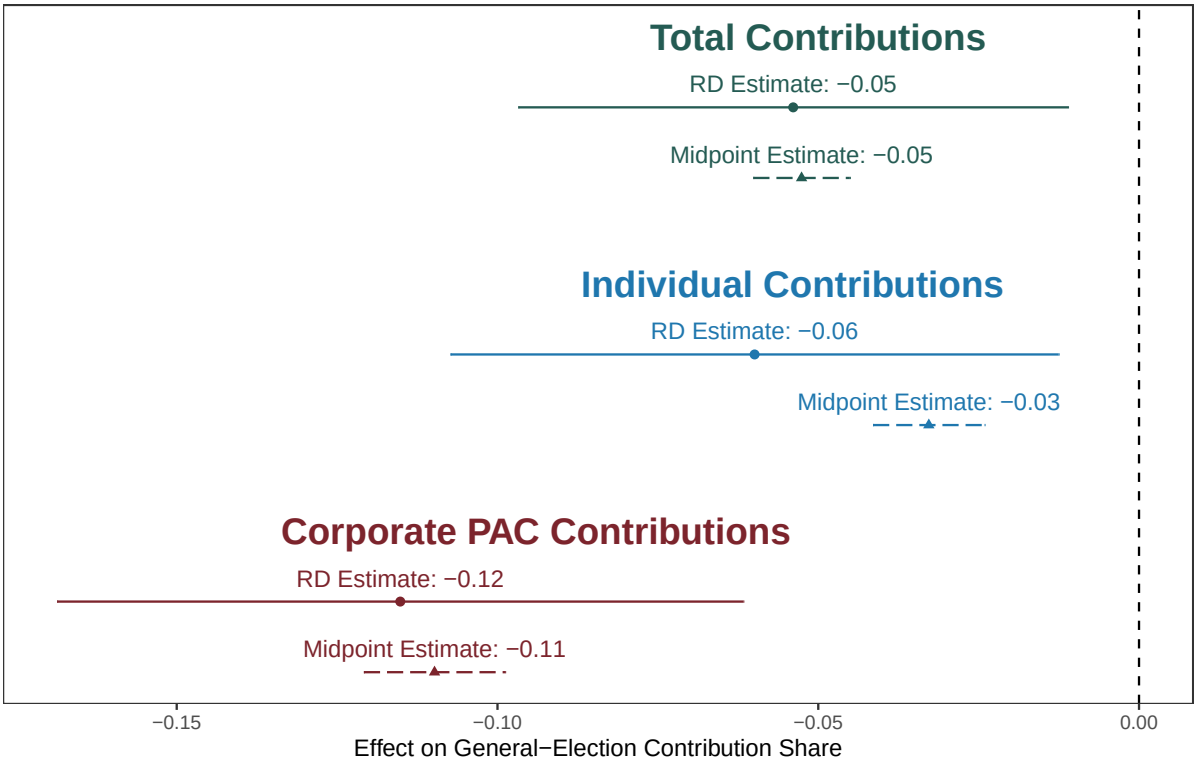
The magnitude of the coefficient on *Midpoint* is not immediately comparable to the RD estimates reported above. To make these estimates comparable, I apply a simple linear transformation of the *Midpoint* coefficient.[25] First, I estimate the average change in candidates' *Primary-Specific Scaling* at the discontinuity and divide this quantity by two; this is the average change in the midpoint between candidates at the discontinuity. I then multiply the *Midpoint* coefficient by this average change, yielding a *Midpoint* estimate that is comparable to my RD estimates. In the main text, I focus on the results using district fixed effects to hold the median constant. In Appendix Table A.6, I show that my results are very similar using presidential vote share.

7.2 RD and Panel Methods Yield Consistent Results

Figure 8 presents the results. The midpoint method estimates are reported with triangles and dashed error bars, while the baseline RD estimates are reported with circles and solid error bars. Estimates are reported separately for total contributions and contributions from individual and corporate PAC donors. Looking at the figure, it is clear that the midpoint estimates are highly consistent in magnitude with the RD estimates. However, because the midpoint method incorporates all contested general elections, these estimates are substantially more precise than the RD estimates. Since the midpoint method rely on a broader set of contested elections than the RD estimates, we can be reasonably confident that the RD estimates are generalizable beyond the discontinuity. I now rely on this added statistical power to study over-time variation in the penalty to extremists.

[25]Note that, as a linear transformation, this process does not affect the relative power or confidence intervals of my estimates.

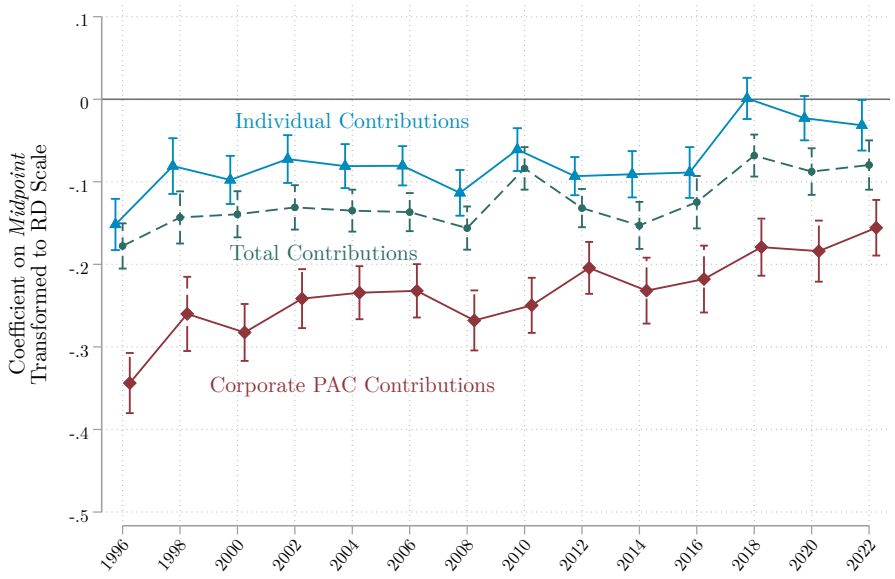
Figure 8 – Comparison of RD and Midpoint Estimates of Effect of Extremist Candidate on General-Election Fundraising in Congress, 1980-2022, and State Legislatures, 1996-2022. This figure compares RD estimates using a local-linear specification of the running variable and a 10% bandwidth with midpoint estimates, after adjusting the midpoint estimates to be on the same scale as the RD estimates. Both methods yield highly similar results.



7.3 Financial Advantage to Moderation Has Declined by 50% Since 1996

Recent work on Congress and state legislatures suggests that the electoral returns to ideological moderation have decreased over the past two decades (Bonica and Cox, 2018; Canes-Wrone and Kistner, 2022; Handan-Nader, Myers, and Hall, 2024). Has the financial return to moderation declined in parallel? Given sample size restrictions, it is impossible to answer this question using the RD. The midpoint method, however, which leverages data in all contested general elections, provides the statistical power necessary to begin to answer this question.

Figure 9 – General-Election Financial Advantage to Moderates Over Time in Congress and State Legislatures, 1996-2022. The financial penalty to extremists has declined by at least half since 1996. Points represent estimates of *Midpoint* from Equation 2 after applying a linear transformation that aligns the scale of *Midpoint* estimates with RD estimates. Models are estimated using presidential vote share to hold the district median constant. Bars represent 95% confidence intervals. Green diamonds represent total contributions, red diamonds represent PAC contributions, and blue triangles represent individual contributions.



Using district presidential vote share to hold partisanship constant, I estimate Equation 2 separately for every on-cycle election year in my sample since 1996.[26] Figure 9 plots the results, along with 95% confidence intervals, for all contributions, individual donors, and corporate PACs. In addition to confirming that PAC contributions are more sensitive to extremist candidates than individual contributors, I find that the financial penalty to extremists has steadily declined since 1996. In 1996, I estimate that an extremist candidate could expect their share of total general-election contributions to be 17 percentage points smaller than a comparable moderate candidate. In 2022, this penalty has declined to 8 percentage points. This decline appears to be driven roughly equally by individual donors and corporate PACs. Investigating why the financial penalty to ideological extremism has

[26] I focus on the years 1996-2022 for this analysis because I have full coverage of both congressional and state legislative primary elections beginning in 1996.

declined will be an important avenue for future research.

8 Discussion and Conclusion

With ever-increasing amounts of money pouring into American elections, researchers and pundits alike worry that campaign finance exacerbates legislative polarization. It is challenging, however, to obtain direct evidence on this question because candidates' ideological positions are chosen strategically and campaign contributions are also commonly used to estimate candidates' ideology. This paper addresses these empirical concerns by pairing a regression discontinuity design in congressional and state legislative primary elections with a new candidate ideology scaling that is trained exclusively on contributions made by individual donors and ideological PACs during primary elections. By short-circuiting concerns about strategic donating and post-treatment bias, this empirical strategy identifies the causal effect of nominating an extremist primary candidate on their party's general-election fundraising outcomes.

Using this design, I find that, in the median contested primary, the "coin flip" primary nomination of an extremist candidate decreases their party's share of general-election contributions by 5-6 percentage points relative to a moderate. For the largest ideological contrasts between candidates, this effect grows to 24-28 percentage points. This penalty is twice as large for corporate PACs and out-of-state donors as it is for individual and in-state donors, and it appears to be largely driven by donors abandoning the extremist candidate, rather than mobilizing their opponent's donors. Studying a complementary panel-based identification strategy, I further show that the financial penalty to extremists extends beyond the set of contested primaries, but has declined by nearly 50% since 1996.

To better understand these effects, I take advantage of the rich institutional heterogeneity across state legislatures. I find that the financial penalty to extremists is larger in more-professionalized states, smaller in more-polarized states, of equal size in midterm elections and on-cycle elections, and may be stronger when legislative news coverage is stronger. These

results suggest that the financial penalty to extremists is larger in more-salient races and is not affected by the nationalization of American elections.

As the amount of money spent on American elections increases exponentially, researchers and pundits have expressed growing concerns that these contributions exacerbate legislative polarization. My analysis indicates that, when extremists win primaries, their party suffers financially in the general election. Taken together, these results show how general-election donors act as an important, yet waning, moderating force in American elections.

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Online Appendix

Do Donors Punish Extremist Primary Nominees?
Evidence from American State and National Legislatures

Contents

A.1 Strategic Donating and Post-Treatment Bias

A.1.1 Documenting Forms of Strategic Donating and Post-Treatment Bias in Contribution Matrix

In this section, I document forms of strategic donating and post-treatment bias that complicate RD-based analyses of general-election fundraising totals when candidates are scaled on the basis of both their primary- and general-election contributions.

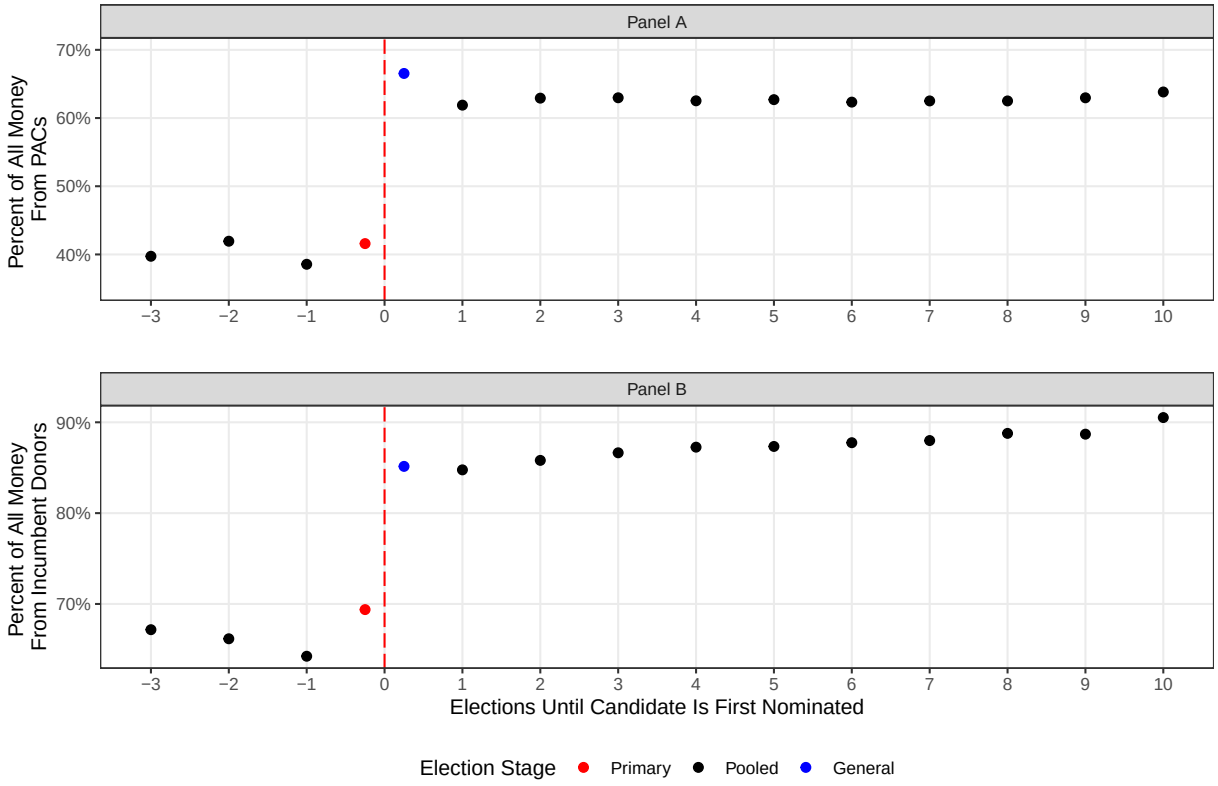
Post-Treatment Scalings as a Function of Primary-Election Outcomes

The first concern with jointly scaling a candidate based on the contributions they received both before and after the primary election is that the candidate's position in the scaling could be partially a function of their primary-election outcome. This possibility is problematic because it may cause bare-primary winners and bare-primary losers to appear systematically different, and even for their classification as relative moderates and extremists to be flipped.

Such a scenario would arise if the composition of a candidates' donorate changes after they secure their party's primary nomination. Using the FEC and NIMSP contribution data described in Section 3.1, Figure A.1 illustrates two such compositional changes. The horizontal axis of this figure reports the number of election cycles until a given candidate wins their first primary nomination, with primary and general elections separated for the election cycle containing a candidate's first primary victory and pooled for all remaining election cycles. To ensure that I am capturing within-candidate changes in donor composition (rather than between-candidate differences), I restrict this analysis to candidates who win a primary election at some point in their career.

For each election cycle, the vertical axis of Panel A plots the share of a candidate's contributions that are from corporate PACs. The results are averaged across all candidates within each horizontal axis bin. The results indicate that winning a primary election causes a substantial increase in the share of contributions a candidate receives from PACs. This

Figure A.1 – Effect of Winning Primary Election and Subsequent Legislative Experience on Donor Composition in Congress, 1980-2022, and State Legislatures, 1996-2022. This figure plots the share of a candidate's *contributions* that come from corporate PACs (vertical axis, Panel A) and incumbent donors (vertical axis, Panel B) averaged across all candidates with equal experience (horizontal axis). For election cycle t and candidate i, an incumbent donor is a donor that contributed to at least one incumbent by the time of election t that is not candidate i. Sample is restricted to candidates who win at least one primary election. Winning a primary election causes a large jump in contributions from corporate PACs, and subsequent legislative experience attracts better-connected donors.



effect is approximately 25 percentage points.[1]

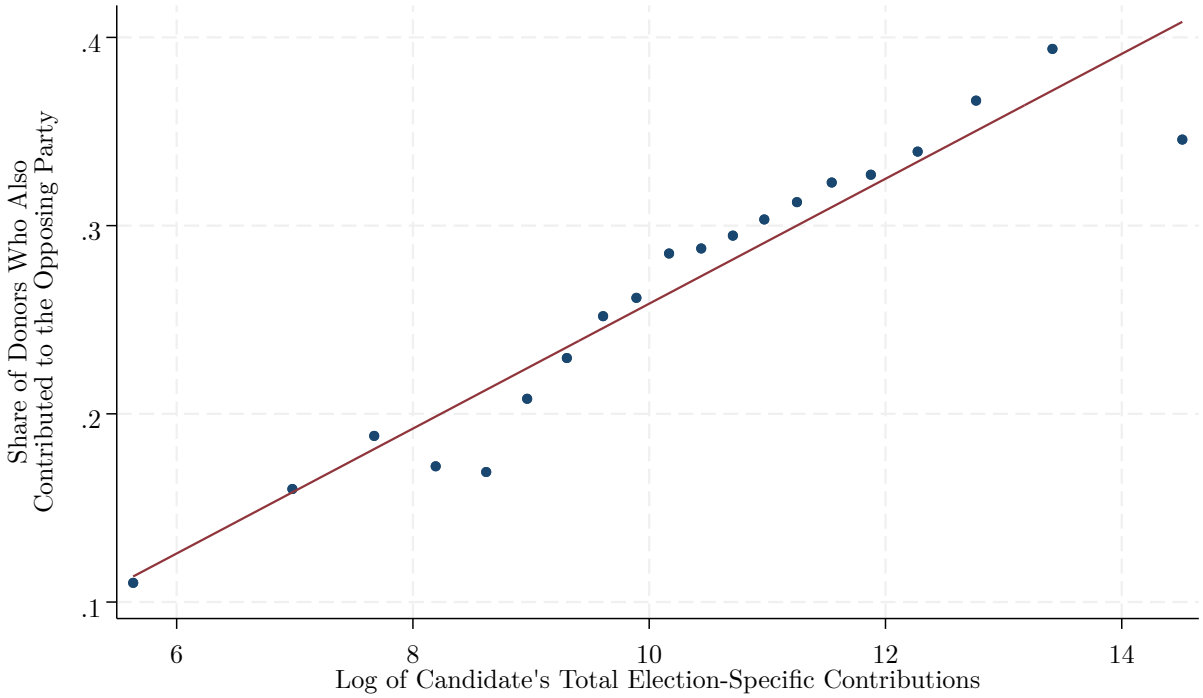
To further illustrate these compositional effects, I introduce the concept of an "incumbent donor." For election cycle t and candidate i, I define an incumbent donor as a donor that has contributed to at least one incumbent by the time of election t that is not candidate i.[2] I calculate the share of each candidate's donors that are "incumbent donors," weighted

[1]Figure A.1 is very similar when the outcome is measured in terms of the share of donors as opposed to donations.

[2]The restriction on t ensures that future donations do not affect prior donor classifications. The restriction on i prevents the incumbent donor share from mechanically becoming one after a candidate wins their first

by contribution amounts, and again restrict the analysis to candidates who eventually win at least one primary election.[3] Panel B of Figure A.1 plots this share averaged across candidates in a given horizontal axis. Overall, I find that candidates' individual donorate becomes significantly more connected to other incumbents after they win their first primary. This effect is approximately 15 percentage points. Taken together, the results presented in panels A and B of Figure A.1 provide strong evidence that winning a primary election may alter candidates' relative ideological scaling if they are scaled in part based on their general-election receipts.

Figure A.2 – Share of Donors Who Also Contributed to the Opposing Party Across Fundraising Totals in Congress, 1980-2022, and State Legislatures, 1996-2022. Candidates who raise the most contributions have donor bases that also contribute to members of the opposing party nearly four times more often than candidates who raise the fewest contributions (10% vs 40%).



general election.

[3]The results, however, are highly similar without donation weights.

Post-Treatment Scalings Conflate Moderation with General-Election Fundraising Success

A second set of concerns relates only to bare-primary winners. If primary-election candidates are classified as moderates and extremists based in part on money raised by primary-election winners during the general election, we risk conflating moderation with general-election candidates who raise more general-election contributions. This follows because the marginal dollar raised by primary-election winners during the general election may come from donors across an increasingly-wider ideological spectrum, causing that candidate to appear artificially moderate.

While it is difficult to test this prediction directly, Figure A.2 shows that candidates who raise the most contributions have donors bases that also contribute to members of the opposing party more often. Specifically, Figure A.2 plots the log total of a candidate's fundraising totals in a given election (horizontal axis) against the share of their contributors that also donated to a candidate of the opposing party (vertical axis). The results are averaged across equal-sample-size bins. I find that candidates who raise the most contributions have donor bases that also contribute to members of the opposing party nearly four times more often than candidates who raise the least contributions (10% vs 40%), suggesting that general-election candidates who raise additional contributions may be artificially classified as moderates if scaled using general-election receipts.

A.1.2 Evidence that Strategic Donating and Post-Treatment Bias Affect Estimates

Having documented forms of strategic donating in the state legislative contribution matrix and outlined how these results may theoretically generate post-treatment bias, I now provide direct evidence of how these biases affect my results. To to so, I create a second version of the scalings introduced in Section 3.2 that use primary- and general-election contributions to

Table A.1 – Top Two Primary Candidates' Moderate/Extremist Classifications Using *Primary-Specific Scalings* and *Post-Treatment Scalings*, Congress, 1980-2022, and State Legislatures, 1996-2022. Table reports candidates' classifications as relative moderates or extremists using *Primary-Specific Scalings* (rows) and *Post-Treatment Scalings* (columns).

	Post-Treatment Scaling Classification		
Primary-Specific Scaling Classification	Moderate	Extremist	N
Moderate	9887	1364	11251
Extremist	1364	9887	11251
N	11251	11251	22502

Note: Sample is restricted to contested primary elections where top two candidates have both a *Primary-Specific Scaling* and *Post-Treatment Scaling*. Unit of analysis is the individual candidate.

scale candidates (henceforth, the *Post-Treatment Scaling*).[4] These *Post-Treatment Scalings* correlate with NP-Scores at very similar rates to the *Primary-Specific Scalings* ($r = .92$ overall, .71 for Democrats, and .72 for Republicans).

First, I compare primary candidates' designation as relative moderates or extremists using the *Primary-Specific Scaling* and *Post-Treatment Scaling*. Table A.1 reports the results. The rows in Table A.1 report candidates' classifications using the *Primary-Specific Scaling*, while columns report candidates' classifications using *Post-Treatment Scaling*. As is apparent, using general-election contributions to scale candidates significantly affects primary candidates' relative positioning. Using the *Post-Treatment Scaling* would cause the researcher to "flip" 12% of primary candidates' moderate and extremist designations, relative to the *Primary-Specific Scaling* ($1364/11251 \approx .12$).

Second, to evaluate whether these "flips" are consequential, Table A.2 replicates Table 2 using *Post-Treatment Scalings*. The estimates across Table A.2 are negative and significant, indicating that my substantive conclusions would be unchanged using *Post-Treatment Scalings*. However, the estimates using *Post-Treatment Scalings* are significantly larger than the estimates when using *Primary-Specifiec Scalings*. For example, take my preferred specifica-

[4]As in the *Primary-Specific Scaling*, I omit corporate PACs from this scaling.

Table A.2 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share, Using *Post-Treatment Scaling* in Congress, 1980-2022, and State Legislatures, 1996-2022. RD estimates of the effect of nominating an extremist candidate on their party's share of general-election contributions are approximately 40% larger when using *Post-Treatment Scalings* (i.e., scalings that include both primary- and general-election contributions). This table replicates Table 2 (which uses *Primary-Specific Scalings*).

	Share of Total General Election Contributions				
	(1)	(2)	(3)	(4)	(5)
Extremist Primary Win	-0.08	-0.08	-0.08	-0.08	-0.08
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
N	2,744	5,449	5,449	5,449	2,666
Polynomial	1	3	3	5	CCT
Spline	Yes	No	Yes	No	Yes
Bandwidth	.10	-	-	-	0.10

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Polynomial reports the largest exponent of the running variable included in the regression. CCT refers to the method of Calonico, Cattaneo, and Titiunik (2014).

tion in column two; using the *Post-Treatment Scalings* would inflate my coefficient estimate by 60% in comparison to *Primary-Specific Scalings* (-8 vs -5 percentage points). This pattern holds across all specifications in the table.

Tables A.1 and A.2 suggest that the scaling correction I employ meaningfully addresses concerns about strategic donating and post-treatment bias on my estimates. In Appendix A.5, I show that my estimates using the *Primary-Specific Scaling* are very similar to estimates obtained using NP-Scores, a measure of ideology for congressional candidates that is entirely distinct from campaign contributions.

A.2 RD Balance Tests

The key identifying assumption behind the regression discontinuity design is that districts that narrowly nominate a relative moderate candidate are, in the limit, identical to districts that narrowly nominate an extremist candidate (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In other words, there must be no district-level sorting at the discontinuity. In Table A.3, I test for any chance imbalances in my sample by estimating Equation 1 where the outcome is the party's fundraising totals in the previous election cycle. If the "no sorting" assumption holds, these estimates should be null, indicating that, in districts where the more-moderate candidate barely wins, the party did no better in the prior election than in districts where the more-extreme candidate was nominated. The coefficients in Table A.3 are all exceedingly small, indicating that there is no evidence of bias.

Table A.3 – Effect of Nominating an Extremist Primary Candidate on Lagged General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a 4 percentage point decline in their party's share of total general-election contributions relative to a moderate.

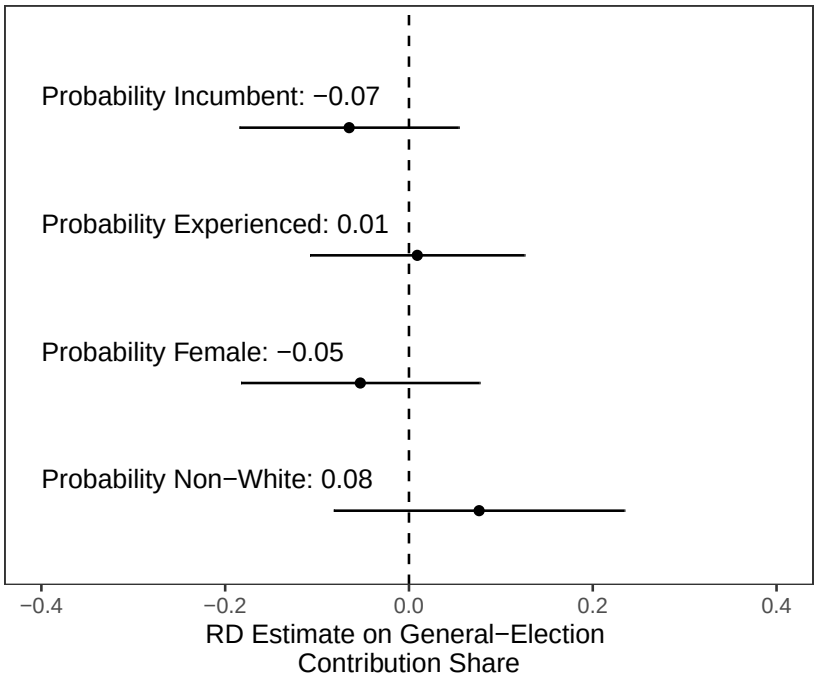
	Lagged Share of Total General Election Contributions				
	(1)	(2)	(3)	(4)	(5)
Extremist Primary Win	-0.00	-0.00	-0.01	-0.00	-0.01
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)
N	1,839	3,657	3,657	3,657	1,822
Polynomial	1	3	3	5	CCT
Spline	Yes	No	Yes	No	Yes
Bandwidth	.10	-	-	-	0.10

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Polynomial reports the largest exponent of the running variable included in the regression. CCT refers to the method of Calonico, Cattaneo, and Titiunik (2014).

A.3 Characteristics of Moderate and Extremist Bare-Winners

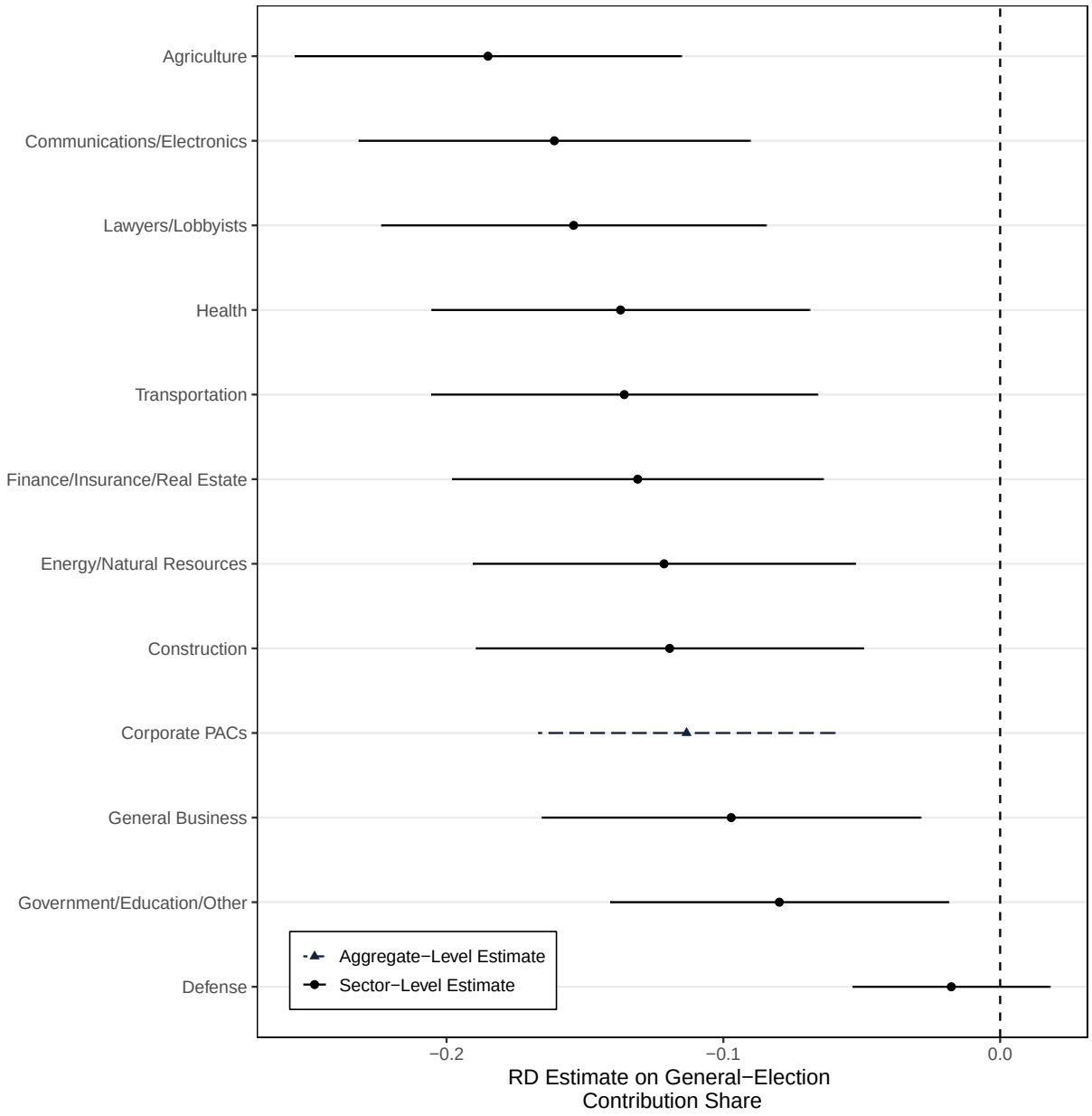
As Marshall (2022) notes, my RD design identifies the aggregate effect of candidate ideology and all other candidate-level characteristics that differ between the two types of barely-winning candidates (i.e., compensating differentials). Studying this bundled treatment is appropriate for evaluating the consequences of primary voters' electoral selection, where all differences between candidate types matter (Hall, 2015). To understand the underlying mechanisms, however, it is important to examine whether moderate and extremist candidates differ on observable non-ideological characteristics. In Table A.3, I test whether barely-winning moderate and extremist candidates systematically differ in terms of incumbency status, prior office-holding experience, gender, and race. I find no significant differences on these characteristics.

Figure A.3 – Characteristics of Moderate and Extremist Bare-Winners in Congress, 1980-2022, and State Legislatures, 1996-2022. This figure plots the difference in probabilities between extremist and moderate bare-winners of being an incumbent, having previous office-holder experience, being female, and being non-white. Estimates are calculated using a local-linear specification of the running variable and a 10% bandwidth. Data on experience, gender, and race are limited to members of Congress.



A.4 RD Estimates by Corporate Industry

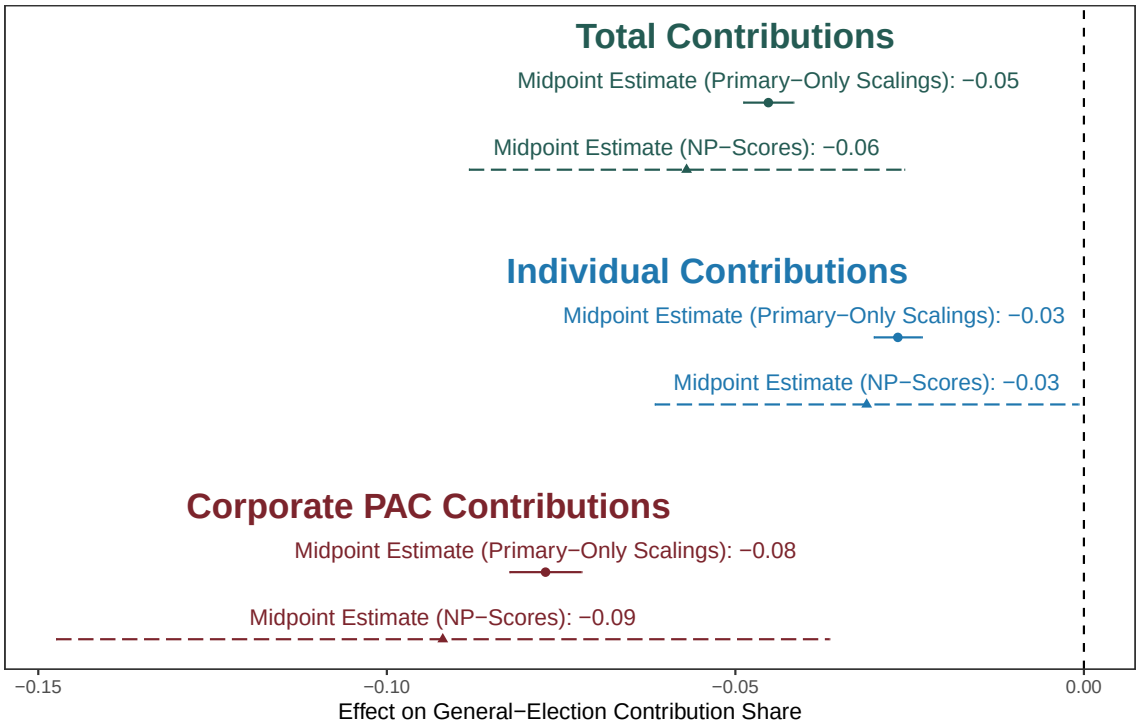
Figure A.4 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share by Corporate Industry in Congress, 2000-2022, and State Legislatures, 1996-2022. The penalty to extremists is similarly sized across all 11 corporate industries defined by NIMSP and the FEC. This figure reports estimates using a local-linear specification of the running variable and a 10% bandwidth.



A.5 Replicating Results Using State Legislators' Roll-Call Voting Records

To ensure that my results are not an artifact of the contribution-based scaling, I replicate the midpoint analysis from Section 7 using a separate measure of ideology that draws on the state legislative roll-call voting records of members of Congress who previously served in state legislatures (NP-Scores).[5] The results are plotted in Figure A.5. As the figure illustrates, my estimates are highly similar using this alternative scaling, although the coefficients are estimated imprecisely due to the small sample size.

Figure A.5 – Comparison of Midpoint Estimates Using Campaign Finance-Based and Roll Call-Based Scalings in Congress, 1980-2022. This figure compares midpoint estimates using *Primary-Specific Scalings* and NP-Scores for members of Congress who served in a state legislature. Estimates are adjusted to be on the same scale as the RD estimates. This figure uses Democratic presidential vote share to hold the district median constant.



[5] I focus on the panel-based identification strategy, rather than the RD, because few congressional primaries feature two top-finishing candidates with previous state legislative roll-call voting records. Because the sample size is small, I apply the midpoint specification that controls for the district median using presidential vote share.

A.6 Additional RD Estimates for Individuals and Corporate PACs

Table A.4 – Effect of Nominating an Extremist Primary Candidate on General-Election Contribution Share from Individual and Corporate PAC Donors in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of an extremist candidate causes a 6-11 percentage point decrease in that party's share of general-election contributions from individual and corporate PAC donors.

	Share of General Election Contributions From Individuals					Share of General Election Contributions From Corporate PACs				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Extremist Primary Win	-0.06	-0.06	-0.07	-0.06	-0.06	-0.11	-0.08	-0.07	-0.09	-0.10
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
N	2,807	5,461	5,461	5,461	2,736	2,808	5,462	5,461	5,462	3,229
Polynomial	1	3	3	5	CCT	1	3	3	5	CCT
Bandwidth	.10	-	-	-	0.10	.10	-	-	-	0.10

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Polynomial reports the largest exponent of the running variable included in the regression. CCT refers to the method of Calonico, Cattaneo, and Titiunik (2014).

A.7 Additional State- and District-Level Heterogeneity

Table A.5 – Variation in RD Estimate of Financial Penalty to Extremist Nominees in State Legislatures, 1996-2022. The financial penalty to extremists is larger in more-professionalized state legislatures, smaller in more-polarized states, no smaller in midterm and odd-year elections, and may be significantly larger in districts that receive stronger news coverage. *New Congruence*, *Professionalization*, and *Polarization* are scaled to run from 0 (lowest) to 1 (highest), and *Midterm Year* and *Odd Year* are indicator variables. This table reports unabbreviated results from Table 5.

	Share of Total General Election Contributions				
	(1)	(2)	(3)	(4)	(5)
Extremist Primary Win	-0.05	-0.03	-0.04	-0.10	-0.05
	(0.02)	(0.05)	(0.03)	(0.04)	(0.03)
News Congruence		0.03			
		(0.04)			
Extremist Primary Win · News Congruence		-0.08			
		(0.07)			
Professionalism			-0.06		
			(0.03)		
Extremist Primary Win · Professionalism			-0.09		
			(0.04)		
Polarization				-0.02	
				(0.03)	
Extremist Primary Win · Polarization				0.09	
				(0.05)	
Midterm Year					-0.01
					(0.01)
Extremist Primary Win · Midterm Year					-0.01
					(0.02)
Odd Year					0.07
					(0.06)
Extremist Primary Win · Odd Year					-0.00
					(0.08)
N	2,190	1,751	1,779	1,947	2,190
Polynomial	1	1	1	1	1
Spline	Yes	Yes	Yes	Yes	Yes
Bandwidth	.10	.10	.10	.10	.10

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist primary candidate's win margin in the primary election. Lower-order terms are omitted from the table for brevity. See Appendix Table A.5 for complete results.

A.8 Additional Midpoint Method Results

Table A.6 – Financial Advantage of More-Moderate Candidates in Contested General Elections, Midpoint Method in Congress, 1980-2022, and State Legislatures, 1996-2022. Holding fixed the *Distance* between candidates, a rightward shift in the *Midpoint* between candidates is associated with a greater share of general-election contributions for the Democrat. Effects are larger for corporate PACs than individual donors, matching RD results.

	Dem. Share of Total General Election Contributions		Dem. Share of General Election Contributions from Individuals		Dem. Share of General Election Contributions from Corporate PACs	
	(1)	(2)	(3)	(4)	(5)	(6)
Midpoint	0.24	0.20	0.15	0.11	0.49	0.34
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)
Distance	-0.02	-0.01	0.00	0.02	-0.01	0.03
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
N	16,253	18,913	15,357	18,085	15,106	17,702
Year FEs	Y	Y	Y	Y	Y	Y
District-by-Regime FEs	Y	N	Y	N	Y	N
Control for Pres. Vote Share	N	Y	N	Y	N	Y

Note: Robust standard errors are clusted by district-regime in parentheses.

A.9 Roll-Call Classification Exercise

This section provides additional detail on the roll-call votes used in Table 1.

Data on roll-call votes in Congress was downloaded from Vote View (Lewis et al., 2024). This dataset includes the universe of roll-call votes cast for the years 1980-2023 and data on roll-call voting in 2024 through September 1st. In total, this includes 12 million roll-call votes.

State legislative roll-call data was assembled from two sources. First, data for the near-universe of roll-call votes cast in all 99 state legislative chambers between January 1st, 2010 and September 1st, 2024 was collected from www.Legiscan.com. This dataset consists of 60.8 million individual votes. I supplement this dataset with 11.2 million roll-call votes for the years 2000-2009 from Fouirnaies and Hall (2022) for a varying panel of 21 states.[6] All together, this roll-call dataset encompasses 72 million distinct votes. Following Bonica (2014, 2018) and Poole (2007), I remove lopsided roll calls with margins greater than 97.5% and omit abstentions and missed votes. Table A.7 reports the total number roll-call votes in this dataset by level and year.

Finally, for each roll call and scaling, I calculate the optimal cutting point between "yea" and "nay" votes following Poole (2007). Leveraging these cutpoints, I impute predicted roll-call votes and compare the result to the true votes cast. Results are reported in Table 1.

[6]I include the unbalanced panel of states from 2000-2009 in our main analyses to evaluate the predictive capacity of my scalings over an extended time frame. The results in Table 1 are very similar I instead focus on the years for which I have a balanced panel.

Table A.7 – Number of Congressional and State Legislative Roll-Call Votes Included in Roll-Call Prediction Sample.

Year	Overall	Congress	State Legislatures	Year	Overall	Congress	State Legislatures
1980	315,742	315,742	–	2003	1,808,744	339,465	1,469,279
1981	202,296	202,296	–	2004	1,162,502	257,096	905,406
1982	245,565	245,565	–	2005	1,749,748	326,389	1,423,359
1983	252,648	252,648	–	2006	1,155,209	261,662	893,547
1984	205,754	205,754	–	2007	1,851,129	554,794	1,296,335
1985	228,355	228,355	–	2008	1,227,608	319,183	908,425
1986	230,797	230,797	–	2009	2,302,626	467,924	1,834,702
1987	253,249	253,249	–	2010	2,527,895	315,142	2,212,753
1988	232,348	232,348	–	2011	5,142,218	431,903	4,710,315
1989	190,199	190,199	–	2012	4,207,630	306,161	3,901,469
1990	253,321	253,321	–	2013	5,209,044	308,007	4,901,037
1991	213,039	213,039	–	2014	4,005,615	279,056	3,726,559
1992	231,964	231,964	–	2015	5,786,226	337,515	5,448,711
1993	298,676	298,676	–	2016	4,342,870	284,653	4,058,217
1994	249,419	249,419	–	2017	6,252,840	338,575	5,914,265
1995	437,149	437,149	–	2018	4,863,361	241,009	4,622,352
1996	227,096	227,096	–	2019	6,510,474	346,421	6,164,053
1997	304,035	304,035	–	2020	3,756,663	137,408	3,619,255
1998	262,188	262,188	–	2021	6,470,780	246,070	6,224,710
1999	301,777	301,777	–	2022	5,025,915	277,911	4,748,004
2000	815,548	290,518	525,030	2023	6,843,060	289,276	6,553,784
2001	1,593,291	257,550	1,335,741	2024	4,513,115	23,000	4,490,115
2002	882,478	235,085	647,393				