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 measurement and strategic candidate emergence have prevented researchers from systematically evaluating whether donors 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 over a more-moderate candidate decreases their party's share of general-election contributions by 6-7 percentage points in the median contest and 18-19 percentage points when the ideological contrast between candidates is largest. This financial penalty is larger for corporate PACs than individual donors and is driven symmetrically by donors withdrawing support from extremist nominees and rallying behind their opponents. Applying a complementary panel-based identification strategy, I replicate these core findings and further document that the financial penalty to more-extreme candidates has fallen by nearly 50% since 2000. Overall, these results show how general-election donors' relative ideological preferences act as a marked, yet waning, moderating force in American politics.

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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 extremity and campaign contributions. Observers often worry that donors disproportionately favor more-extreme candidates and, as a result, that campaign finance contributes to the extraordinary polarization of American politics. Yet donors' preferences could also act as a moderating force in American elections, 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, obtaining direct empirical evidence on this question is challenging, because candidates likely strategically select into running based on their fundraising prospects, and traditional measures of candidates' ideological positioning are endogenous to their fundraising outcomes. As a result of these measurement and design problems, or their focus on a limited sample of elections, existing research reaches widely conflicting conclusions—from a fundraising advantage for more-extreme candidates (Ensley, 2009; Oklobdzija, 2017; Stone and Simas, 2010) to a penalty imposed by access-seeking donors (Barber, 2016*b*; Hall, 2015; Meisels, 2024).[1] Resolving this discrepancy is central to understanding the forces shaping the ideological composition of American legislatures, because fundraising plays a critical role is determining who runs for office (Carnes, 2018; Fowler and McClure, 1990), whether candidates persist across election cycles (Bonica, 2017; Thomsen, 2025), and which candidates ultimately prevail (Avis et al., 2022; Erikson and Palfrey, 2000; Fouirnaies, 2021; Gerber, 1998; Green and Krasno, 1988).

To overcome these challenges, this paper pairs a new candidate ideology scaling and massive dataset of primary-election vote returns with a regression discontinuity (RD) design originated by Hall (2015) to evaluate whether general-election donors punish extremist primary nominees in Congress, 1980-2022, and state legislatures, 1996-2022. This new ide-

[1] A third strand of literature identifies no difference between moderate and extremist candidates' fundraising prospects (Claassen, 2007; Grant and Rudolph, 2002; Johnson, 2010; McCarty, Poole, and Rosenthal, 2006).

ology scaling is trained exclusively on primary-election contributions made by individual donors, allowing me to capture candidates' ideological positioning without contamination from general-election fundraising and strategic donors, and correlates highly within party with standard measures of roll-call voting ($r = .74$ for Democrats, $r = .70$ for Republicans). Further, by incorporating state legislative elections alongside congressional races, I expand my sample fifteen-fold and gain leverage to examine heterogeneity across dimensions of press coverage, election salience, and the timing of elections.[2] Taken together, this central design identifies the effect of nominating a relative extremist candidate in the primary election on their party's share of general-election contributions, holding fixed all district-level confounders. I complement this approach with a panel-based identification strategy that extends the analysis to the universe of contested general elections.

Combining the RD design with my primary-specific ideology scaling, I find that the "coin-flip" primary nomination of an extremist over a more-moderate candidate decreases their party's share of general-election contributions by 6-7 percentage points in the median election in my sample. This financial penalty increases to 18-19 percentage points when the contrast between candidates is most pronounced and is largest in highly-consequential open-seat elections. Disaggregating by donor type, I further show that the financial penalty to extremist nominees is twice as large among corporate PACs than individual donors and is driven symmetrically by donors withdrawing support from the extremist's party and rallying behind their opponent.

To assess the robustness of these central results, I replicate my baseline analyses using a panel-based identification strategy from Ansolabehere, Snyder, and Stewart (2001), which compares changes in candidates' general-election fundraising as the midpoint between Democratic and Republican general-election candidates varies. This design uses district-by-regime fixed effects to hold district-level confounders constant and allows me to study the universe

[2] State legislatures are also critical policymaking bodies in their own right, with primary responsibility over policy areas including education, healthcare, and election administration. Further, these institutions are a primary source of future members of Congress and could influence national polarization (Thomsen, 2014).

of contested general elections. 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 close contested primary elections. Leveraging the statistical power that this design provides, I also show that my results are robust to an alternative measure of candidate ideology based entirely on the roll-call voting records of candidates who have served, currently serve, or will serve in legislative office.

Building on these central findings, I adapt this panel-based identification strategy to examine how the financial penalty to extremist primary nominees has evolved over time. Using this design, I document that the financial penalty to extremist nominees has declined by nearly 50% since 2000, both among corporate PACs and individual donors.

Finally, I harness the rich institutional heterogeneity within and between state legislatures to help account for this decline and better understand where the financial penalty to extremist nominees is largest. Pairing a difference-in-differences design from Fouirnaies (2018) with models of electoral selection, I present evidence that corporate PACs have reallocated funds to relative extremists because these candidates are increasingly electorally viable, rather than due to differential changes in the expected tenure or value of access through relative moderates versus extremists, nor have corporate PACs become more partisan in their giving. Additional analyses reveal that the financial penalty to extremist nominees is larger when press coverage is stronger or elections are more salient—suggesting that structural changes in the media environment and the increasing importance of top-of-the-ballot elections also help explain the decline in the financial penalty to extremist nominees.

Collectively, these results temper concerns that donors' relative ideological preferences fuel legislative polarization, at least in general elections. My findings indicate that general-election donors act as a moderating force in American elections in response to the nomination of extremist primary candidates, but one that has faded in recent years.

The remainder of this paper is organized as follows. In the next section, I outline theoretical and empirical perspectives on individual and corporate donors' support for more-extreme candidates. The third section introduces my empirical strategy, including the primary-election data, RD design, and new ideological scaling. Drawing on this design, section four examines the aggregate effect of extremist nominees on campaign contributions. Next, section five disaggregates the overall effect by donor type. In section six, I replicate my main results using an observational panel method that allows me to generalize beyond districts featuring close contested primary elections and evaluate how the penalty has changed over time. Section seven leverages the institutional heterogeneity within and between state legislatures to help understand where the financial penalty to extremists is largest and why it may have declined. Finally, section eight discusses key implications and concludes.

2 Theoretical and Empirical Perspectives on Donor Support for Relative Moderate and Extremist Candidates

Campaign contributions in American elections are the product of a combination of strategic and expressive motivations. To interpret the financial consequences of nominating a relative extremist over a moderate, it is important to consider how these distinct motives might influence donors' support for more-moderate versus more-extreme candidates. In particular, I focus on corporate PACs and individual contributors—the two largest sources of direct campaign funds in American elections. As I outline below, corporate PACs are typically understood as strategic, access-seeking actors, while individual donors are thought to contribute expressively to candidates who align with their ideological preferences. Yet, as I discuss below, these motivations generate competing expectations about whether each group systematically favors relative moderate or extremist candidates. Ultimately, resolving this tension is a key empirical contribution of this paper.

2.1 Corporate PACs

Corporate PACs are widely viewed as strategic, instrumental contributors, but theories of the type of influence they pursue have shifted over time. Early theories of corporate PAC behavior modeled campaign contributions as instruments exchanged in a competitive market for immediate private benefits (e.g., Baron, 1989; Denzau and Munger, 1986; Groseclose, 1996; Grossman, 1994; Grossman and Helpman, 1996, 2001; Lessig, 2011). In these models, donors "bid" to maximize expected policy returns, and candidates adjust their representation to match their donors' preferences. Even beyond important identification concerns, however, empirical tests of these models have produced inconsistent and often null results.[3] In a review of 36 prior studies, Ansolabehere, de Figueiredo, and Snyder (2003) find that three in four estimates linking contributions to favorable roll-call votes are either statistically insignificant or reach opposite conclusions. Moreover, as Tullock (1972) first observed, the sheer scale of potential policy rents vastly exceeds observed contribution levels, raising questions about whether donations directly purchase favorable legislative outcomes at all.[4]

In response to these limitations, more recent work suggests that corporate PACs primarily contribute to gain access to policy markers and influence which issues receive attention, rather than directly affect legislative outcomes (Hall and Wayman, 1990; Snyder, 1992).[5] Consistent with this view, empirical work finds that corporate PACs target powerful legislators and committees (Fouirnaies, 2018; Fouirnaies and Hall, 2018; Grier and Munger, 1991; Powell and Grimmer, 2016; Romer and Snyder, 1994), incumbents (Fouirnaies and Hall, 2014), and members of the majority party (Cox and Magar, 1999). While the ultimate policy value of this access remains unclear (Fowler, Garro, and Spenkuch, 2020; Fouirnaies and Fowler, 2022), experimental evidence suggests that revealed donors are substantially more likely to gain access to members of Congress than those who do not identify themselves as donors

[3] See, for example, Bronars and Lott (1997), Conley and McCabe (2012), Langbein and Lotwis (1990), McCarty and Rothenberg (1996), Moore, Powell, and Reeves (2013), Stratmann (1991, 2002, 2005), Wawro (2001) and Wright (1985, 1989, 1990, 1996, 2004).

[4] See also Milyo, Primo, and Groseclose (2000).

[5] Other early studies of access-seeking contributors include Austen-Smith (1995), Chin, Bond, and Geva (2000), Gopoian, Smith, and Smith (1984), Herndon (1982), and Langbein (1986).

(Kalla and Broockman, 2016).

Although initially developed to model the market for private policy benefits, canonical theories of investor-contributors can also be adapted to characterize access-seeking corporate PACs' allocation problem, as Baron (1989) suggests. Generally, these models imply that corporate PACs contribute to a candidate based on three key considerations: the probability the recipient wins, the recipient's expected tenure in office, and the value of access through that recipient if elected (Baron, 1989; Palda, 1980; Snyder, 1990, 1992, 1993; Welch, 1980). Candidates who are likely to win, remain in office, and/or hold influential positions should thus attract greater contributions from corporate PACs.

In practice, however, these three strategic considerations generate ambiguous predictions about whether corporate PACs favor relative moderate or extremist candidates. On one hand, relative moderates may be more electorally viable (Ansolabehere, Snyder, and Stewart, 2001; Canes-Wrone, Brady, and Cogan, 2002; Hall, 2015; Hall and Snyder, 2015; Handan-Nader, Myers, and Hall, 2025) and offer more stable, long-term access than relative extremists, causing corporate PACs to favor relative moderates. Alternatively, if the value of access through relative extremists is greater—whether because they are more likely to hold leadership positions, be a member of the majority, or simply get more done—corporate PACs may instead favor relative extremists.

2.2 Individual Contributors

Individual contributors, in contrast to corporate PACs, are theorized to support ideologically-proximate candidates, primarily as a form of political expression (Ansolabehere, de Figueiredo, and Snyder, 2003; Austen-Smith, 1987; Cameron and Enelow, 1992; Magee, Brock, and Young, 1989; Morton and Cameron, 1992). Consistent with these predictions, both survey and administrative data show that donors are more likely to contribute to candidates whose roll-call records or stated positions align with their own policy preferences (Barber, 2016*a*,*c*; Barber, Canes-Wrone, and Thrower, 2017; Gimpel, Lee, and Pearson-Merkowitz, 2008; Hill and Huber, 2017).

Yet despite their well-documented motivations, it remains uncertain whether individual donors favor relative moderates or extremists on average. One possibility is that individual donors may allocate general-election contributions in ways that mirror general-election voters' revealed preference for moderation at the ballot box (e.g., Ansolabehere, Snyder, and Stewart, 2001; Canes-Wrone, Brady, and Cogan, 2002; Hall, 2015; Hall and Snyder, 2015). But it is not immediately clear whether these electoral preferences translate into financial support, because donors are highly unrepresentative of the overall electorate. For example, prior research indicates that donors are disproportionately wealthy and well-educated (Verba, Schlozman, and Brady, 1995) and that they hold more ideologically extreme views than the average voter (Bafumi and Herron, 2010; Hill and Huber, 2017; La Raja and Schaffner, 2015). As a result, while general-election voters clearly reward relative moderates, the American donorate may be sufficiently skewed toward ideologues that individual contributors favor relative extremists on balance.

2.3 Prior Research on Relative Moderate and Extremist Candidates' Fundraising Prospects

A small number of important papers have used causal designs to identify whether donors advantage relative moderate or extremist candidates. Using a close primary regression discontinuity design in U.S. House elections, Meisels (2024) finds that more-moderate primary-election nominees raise 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. Including these additional contests increases my sample size fifteen-fold and substantially augments my statistical power. As I describe in Section 7, the rich heterogeneity across state legislatures also allows me to study how press coverage, election timing, and the salience of elections might moderate the financial penalty to extremists, all of which 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 ideology scaling that uses only contributions received during the primary election from individual donors to scale candidates.[6] In subsequent sections, I show that failing to make this adjustment would cause the researcher to over-estimate the treatment effect by approximately 35%.[7] Finally, in addition to a regression discontinuity design, I apply a complementary panel-based identification strategy which allows me to generalize beyond the small set of districts featuring close contested primaries and replicate my analyses using a measure of candidate ideology that is entirely distinct from campaign contributions.

Similarly, Hall (2015) finds tentative evidence in U.S. House races that the narrow primary nomination of an extremist candidate substantially decreases their 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 single largest source of campaign funds—or corporate PACs, nor do they capture how this financial penalty has evolved over time. As I illustrate in Sections 6 and 7, studying the decline of the financial penalty to more-extreme candidates offers new insight into the motivations of corporate PACs and individual donors, in addition to establishing a highly consequential trend in American elections.

Finally, two important studies leverage state-level changes in campaign finance laws in a difference-in-differences framework to identify whether limits on corporate and individual donors (Barber, 2016*b*) or parties (La Raja and Schaffner, 2015) increase legislative polariza-

[6] 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).

[7] 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.

tion. While valuable, these studies face the critical design challenge that states experiencing rising polarization may also be more likely to adopt new campaign finance restrictions, making it difficult to disentangle the effects of the policies from underlying political trends. The regression discontinuity design I adopt holds fixed the underlying political environments, offering stronger identification of causal effects.

3 Empirical Strategy

Despite widespread interest in whether donors advantage relative moderates or extremists, obtaining causal evidence on this question is challenging because candidates likely strategically select into running based on their fundraising prospects, 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 the more-extreme candidate in the primary election on their party's fundraising outcomes in the general election. Given this research design, I then introduce a new purpose-built ideology 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 donors advantage relative moderates or extremists, I harness the "as-if" random variation in close primary elections between relative moderate and extremist candidates. This 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

$$Y_{dpt} = \beta_0 + \beta_1 \textit{Extremist Primary Win}_{dpt} + f(V_{dpt}) + \varepsilon_{dpt}, \tag{1}$$

where $\textit{Extremist Primary Win}_{dpt}$ 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 candidate's primary vote margin). This design facilitates direct counterfactual comparisons of parties' fundraising outcomes between districts that narrowly nominate the relative moderate and extremist primary-election candidate.

For information on candidates' primary-election vote shares in state legislatures, I draw on a massive original dataset of primary-election returns collected in collaboration with Fouirnaies and Hall (2020), Handan-Nader, Myers, and Hall (2025), and Rogers (2023). Partial data on congressional primary elections comes from Ansolabehere et al. (2010) and was extended through 2022 by the author.[8] For information on campaign contributions, I assemble a dataset containing all general-election contributions from the Federal Election Commission (FEC; used for Congress) and National Institute on Money in Politics (NIMSP; used for state legislatures). This dataset includes both itemized and unitemized contributions made after the date of the primary election but before the general election.[9] 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.[10]

The key identifying assumption underlying this design is that districts that narrowly nominate a relative moderate candidate are, in the limit, identical to districts that narrowly

[8]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.

[9]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.

[10]I exclude the Nebraska state legislature from my analysis because legislators in Nebraska are not formally affiliated with either party. Data for a small number of state legislatures is missing for 1996 and 1998. My results are highly similar when omitting these two election cycles in state legislatures.

nominate the more-extreme 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, 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 reestimating Equation 1 where the outcome is the party's share of fundraising in the previous election cycle or its lagged presidential or legislative vote share. 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 Tables A.3 and A.4 in Appendix A.2 are all small in magnitude, 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 districts featuring 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 districts. To evaluate whether these estimates generalize to a broader array of electoral contexts, I replicate my main analyses using an observational panel method that is 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 6.

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

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 introduce endogeneity in two ways. For brevity, I outline these two concerns briefly below and refer the reader to Appendix A.1 for a more detailed discussion.

The first challenge posed by jointly scaling candidates based on the contributions they received both before and after the primary election is that candidates' positions in the associated scaling could be partially a function of their primary-election outcome. For example, some donors may prefer to contribute to candidates who run in the general election or weight ideological proximity differently in higher-salience general elections. This possibility would be problematic because it may cause bare-primary winners and bare-primary losers to appear systematically different, or even for their classification as relative moderates and extremists to be flipped.[11] A second concern is that candidates who experience more fundraising success may appear artificially moderate if donors contribute on the basis of candidates' non-ideological characteristics. For example, access-seeking corporate PACs may funnel contributions to candidates who are most likely to be elected, causing them—and the candidates to which they contribute—to appear artificially moderate (Hall and Snyder, 2015).

To address these concerns, I restrict the data used to infer candidates' ideological positions in two ways. First, due to 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 contributions made on the basis of non-ideological candidate characteristics may cause candidates who are more successful fundraisers to appear artificially moderate, I further restrict the set of contributions that I use to scale candidates to donations made by individual donors, which are thought to contribute largely on the basis of ideological or partisan congruence.[12] 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.[13]

[11]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.

[12]In defense of this assumption, I evaluate the extent to which individual donors contribute randomly with respect to ideology using a simple diagnostic statistic proposed by McCarty, Poole, and Rosenthal (2006) and studied more recently by Bonica (2014). This statistic measures the ideological dispersion of a contributor's recipients, calculated as the contribution-weighted standard deviation of candidates' rank-ordered roll-call scores, scaled from -1 to 1. If donors contribute randomly with respect to ideology, this statistic will equal .577 in large samples, while values less than .577 indicate that contributions were made at least partially on the basis of ideology. Among individuals who made at least 10 distinct contributions, I estimate that 99.4% have contribution-weighted standard deviations below .577 and 90% have contribution-weighted standard deviations below .15. These results indicate that the overwhelming majority of contributors I use to scale candidates are substantially motivated by ideological considerations.

[13]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).

The restrictions I impose are quite meaningful. In Appendix A.1, I show that using post-treatment or non-ideologically motivated contributions to scale candidates would cause the researcher to "flip" 17% of primary-election candidates' designations as moderates and extremists, leading the researcher to over-estimate the treatment effect by roughly 35%. I also present evidence in Appendix A.1 that these restrictions have their intended effect. Specifically, using a candidate-level RD, I show in Figure A.2 that winning a primary election does not affect a candidate's estimated ideology after making these restrictions. And, based on a series of simulations, I show in Figure A.3 that altering a candidate's primary-election fundraising success does not affect their estimated ideological positions.

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.[14,15] This estimation procedure 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, 2025).[16,17] Then donor

[14]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.

[15]Other studies that have adopted this scaling approach include Hall (2015, 2019) and Handan-Nader, Myers, and Hall (2025).

[16]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 28,987 distinct incumbent legislators and was downloaded from `https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/SGOQ7G`.

[17]Recent work shows that standard measures of roll-call voting ideology may systematically misrepresent the ideological positions of legislators who vote against their direct policy interests for expressive reasons (i.e., cast "protest votes") (Duck-Mayr and Montgomery, 2023; Fowler and Lewis, 2024). In Appendix A.9, I show that my results are highly similar when employing roll-call scalings from Fowler and Lewis (2024) that account for non-ideological protest voting in the U.S. House.

j's revealed ideological preference is given by

$$Donor\ Ideology_{-i,j} = \frac{\sum_{w \neq i} Roll\text{-}Call\ Voting_w\ Contribution_{wj}}{\sum_{w \neq i} Contribution_{wj}}, \tag{2}$$

where I leave out candidate i when estimating donor j's ideology to avoid a feedback loop.[18] Subsequently, I estimate each candidate's ideology as

$$Cand\ Ideology_i = \frac{\sum_j Donor\ Ideology_{-i,j}\ Contribution_{ij}}{\sum_j Contribution_{ij}}. \tag{3}$$

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

Using this *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 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 Scaling

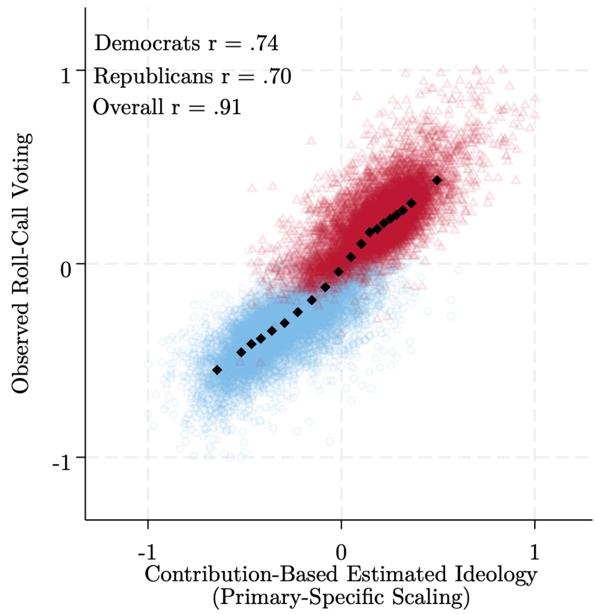
For candidates who ultimately take office, it is possible to validate this primary-specific scaling by comparing it with legislators' observed roll-call voting records. I conduct two empirical exercises to facilitate this comparison.

First, Figure 1 plots the relationship between legislators' *Primary-Specific Scaling* and their roll-call voting scores, as measured by DW-NOMINATE and NP-Scores. As the figure shows, the overall correlation is .91, while the within-party correlation is .74 for Democrats and .70 for Republicans.

Second, I use legislators' *Primary-Specific Scaling* to predict the outcome of nearly 84

[18] 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., $Donor\ Ideology_j = \frac{\sum_i Roll\text{-}Call\ Voting_i\ Contribution_{ij}}{\sum_i Contribution_{ij}}$).

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 in office (i.e., DW-NOMINATE or NP-Scores) for Democrats (circles) and Republicans (triangles). Diamonds represent equal-sample-size averages of the data.



million roll-call votes cast in Congress and state legislatures during my period of study. To do so, I follow Bonica (2014, 2018) and calculate the share of roll-call votes that can be correctly classified using an optimal cutting-point procedure described in Poole (2007).[19] I report these results, and the coverage of my sample of roll-call votes, in detail in Appendix A.12. In short, I find that my *Primary-Specific Scaling* correctly predicts 89.5% of roll-call votes in my sample ($APRE = .716$), outperforming CFscores and an indicator for party, and closely behind DW-NOMINATE and NP-Scores themselves (91.1%; $APRE = .759$).[20]

In sum, despite restricting the size of the training contribution matrix, I am still able to consistently predict candidates' roll-call voting records.

Finally, to ensure that my results are not an artifact of this contribution-based scaling, I

[19] 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.

[20] $\text{APRE}_i = \frac{\sum_{j=1}^{J}\{\text{minority vote}_j - \text{classification errors}_{ij}\}}{\sum_{j=1}^{J}\text{minority votes}_j}$ 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.

replicate my main panel-based results in Appendix A.5 using a measure of candidate ideology that is independent of campaign contributions. This measure draws on the state legislative roll-call voting records of prior, current, or future state legislators who face another candidate with a state legislative roll-call voting record, either in a congressional or state legislative election. The results using this strategy are highly similar to the findings reported in the body of this paper, but are estimated less precisely due to the limited sample size.

4 Effect of Extremist Nominees on General-Election Campaign Contributions

Having detailed my empirical strategy and outlined competing theoretical perspectives on whether donors, 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 patterns, 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 punish extremist primary nominees on average? 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 ideological 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. When the horizontal axis is greater than zero, the extremist candidate wins the primary nomination and represents their party in the general election. When the horizontal axis is instead less than zero, the moderate candidate wins the primary nomination and runs in the general election. As the

Figure 2 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of the more-extreme candidate causes a 6-7 percentage point decline in their party's share of general-election contributions relative to a more-moderate candidate. Black dots represent averages within equal-sample-sized bins of the running variable. Red lines plot fitted values from OLS regressions estimated separately on either side of the discontinuity using the underlying data.

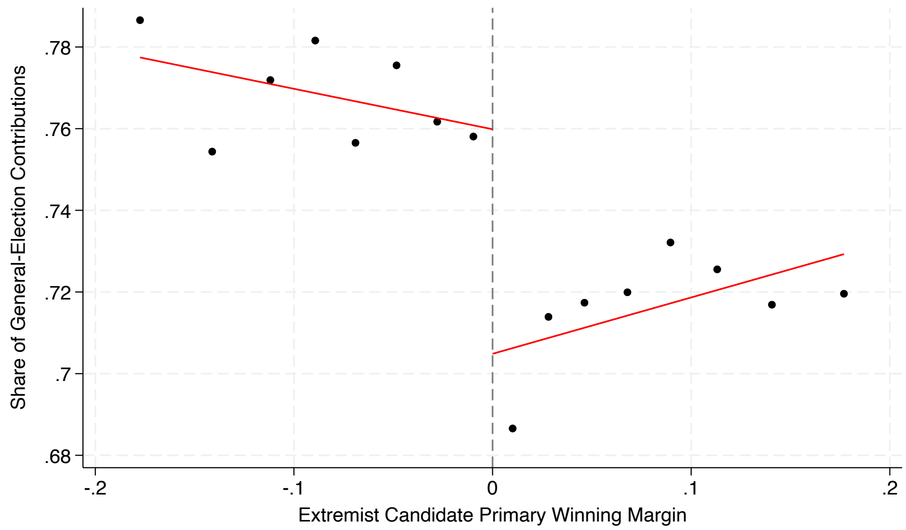


figure depicts, when a district shifts from barely nominating a relative moderate candidate to an extremist, the candidate's party receives approximately 6-7 percentage points less of all general-election contributions.[21]

Table 1 evaluates this relationship more formally. As is standard in RD analyses, I report estimates across a variety of specifications for $f(V_{dpt})$ and varying bandwidths. In the first column, I use a 10% bandwidth and a local-linear specification of the running variable that allows for different slopes on either side of the discontinuity (i.e., a spline). In the second column, I fit a third-order polynomial with a spline. The third column reports the effect estimated by the method from 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.[22] Finally, column four reports the estimate from Imbens and Wager's

[21]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.

[22]I implement this method using the Stata function *rdrobust*.

Table 1 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of the more-extreme candidate causes a 6-7 percentage point decrease in that party's share of general-election contributions.

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.06	-0.07	-0.07	-0.07
	(0.02)	(0.03)	(0.02)	(0.02)
N	2,661	5,223	2,777	5,223
Specification	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-
Bandwidth	.10	-	0.10	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

(2019) optimized RD estimator, which obtains the finite-sample minimax estimator for the discontinuity.[23]

Across specifications, Table 1 reports consistent negative effects of nominating an extremist primary candidate on the party's general-election contribution share. Consider the coefficient reported in column two. Here, I estimate that nominating an extremist causes a 7 percentage point decrease in their party's share of total general-election contributions relative to a moderate.[24] Looking across the table, I find uniform evidence that extremist nominees damage their party's fundraising prospects, with precisely-estimated effects ranging from 6 to 7 percentage points.

The estimates reported in Table 1 aggregate over a variety of different primary-election

[23]This method requires a bound, B, on the second derivative of the conditional response function. Following Imbens and Wager (2019), I estimate the conditional response function using a global quadratic regression and then conservatively multiply the estimated curvature by two. I implement this method using the R package *optrdd*.

[24]For brevity, I focus on this third-order polynomial specification throughout the remainder of the paper. Results are highly similar across all four specifications presented in Table 1.

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). 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.[25] To evaluate this possibility, I examine open-seat races—a set of primary contests where neither the relative moderate nor extremist possesses an incumbency advantage. Open-seat races are also highly consequential contests in and of themselves; fully 77% of state legislators and 64% of members of Congress first enter office through an open-seat election in my sample.

A second trait of primary elections that is relevant for interpreting these overall effects is whether a district is 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 2. In the first column of Table 2, I replicate my baseline estimate from column two of Table 1. 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 is larger in magnitude than in my overall sample (-7 vs. -9 percentage points), and this difference is statistically significant ($t = 2.26$, $p < .025$; SEs clustered by district). Clearly, 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 2 studies only districts that are "safe" for a party. As expected,

[25] 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 2 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share By Primary Type in Congress, 1980-2022, and State Legislatures, 1996-2022. The financial penalty imposed on more-extreme primary nominees 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	-0.07	-0.09	-0.03
	(0.02)	(0.04)	(0.03)
N	5,223	1,845	2,529
Specification	Cubic	Cubic	Cubic
Spline	Yes	Yes	Yes

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression.

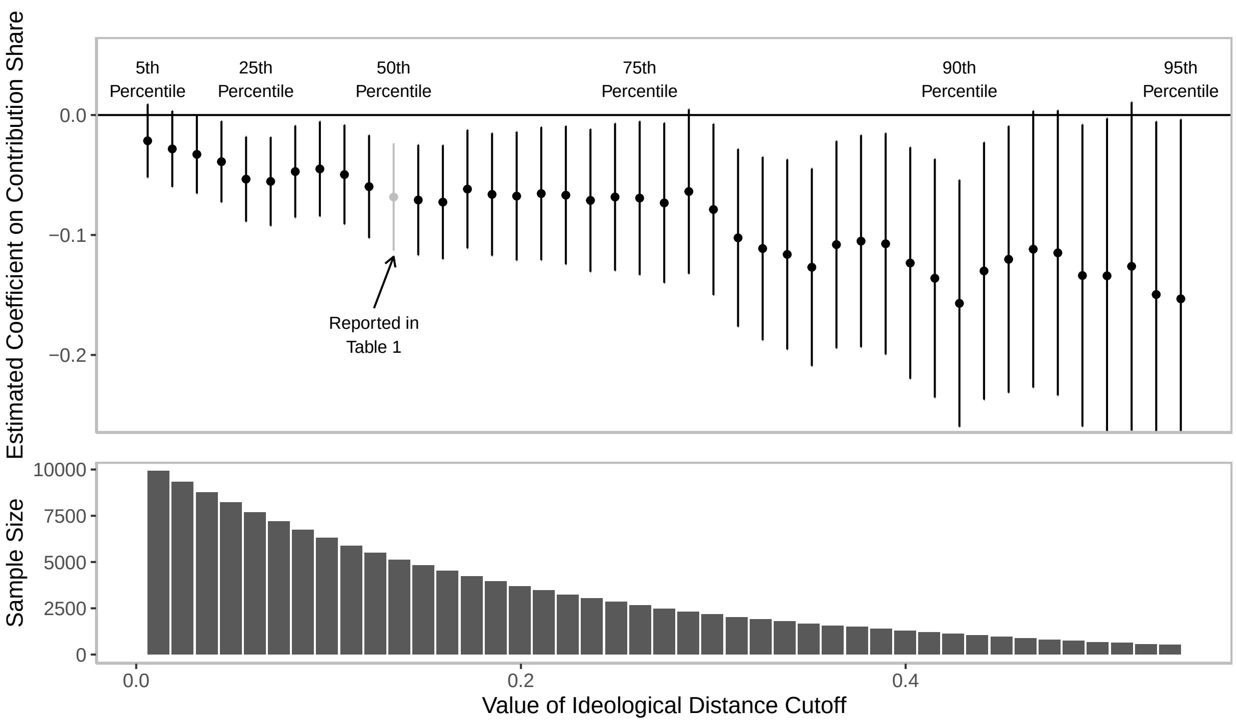
I find that the financial penalty to extremists is smaller in these uncompetitive districts, perhaps due to heightened partisan loyalty or a lack of viable alternatives.

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 Ideological Contrast

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 relative moderate and extremist candidates.

Figure 3 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share Across Possible Ideological 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-election candidates required to identify relative moderates and extremists). Estimates are based on 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 relative moderate and extremist candidates is increased, the effect of nominating the more-extreme candidate on general-election contributions grows.



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 across 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. The horizontal axis of Figure 3 plots the cutoff value, and the 5th, 25th, 50th, 75th, and 95th percentiles of this distribution are reported at the top of the figure. The top panel

Table 3 – Effect of Nominating the More-Extreme Primary-Election Candidate Across Treatment Intensities in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of the more-extreme candidate causes a 18-19 percentage point decline in their party's share of general-election contributions in contests with the largest ideological contrast between primary-election candidates.

	Share of Total General Election Contributions		
	(1)	(2)	(3)
Extremist Primary Win	0.01	0.01	0.02
	(0.02)	(0.01)	(0.02)
Extremist Primary Win · Distance	-0.20	-0.20	-0.20
	(0.05)	(0.03)	(0.04)
N	5,538	10,442	10,442
Specification	Linear	Cubic	Cubic
Spline	Yes	No	Yes
Bandwidth	.10	-	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. Lower-order *Distance* term is omitted from table for brevity.

plots the estimates and 95% confidence intervals across values of the cutoff. The lower panel reports the sample size for each regression. For reference, the estimate reported in column two of Table 1 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 2 percentage points at the 5th percentile of the cutoff distribution to 18 percentage points at the 95th 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 the smallest and largest between-candidate ideological contrasts. The results are reported in Table 3.[26] Summing the first and second rows of 3, I find that financial penalty to extremists

[26] I exclude the CCT and IW specifications from Table 3 because *rdrobust* and *optrdd* are not designed to

is approximately 18-19 percentage points in races where the contrast between candidates is largest.

5 Which Donors Punish Extremist Primary Nominees?

The results presented thus far indicate that, in aggregate, general-election donors punish extremist primary nominees. While these aggregate-level estimates are most consequential for election outcomes, they may obscure heterogeneity that is essential for interpreting the overall penalty and its underlying sources. For example, Section 2 describes how canonical theories of donors' motivations yield ambiguous predictions about whether individual contributors and corporate PACs favor relative moderates or extremists. This section evaluates these competing mechanisms. It also examines whether the financial penalty to extremist nominees is driven by donors withdrawing support from the extremist's party or rallying around their opponent.

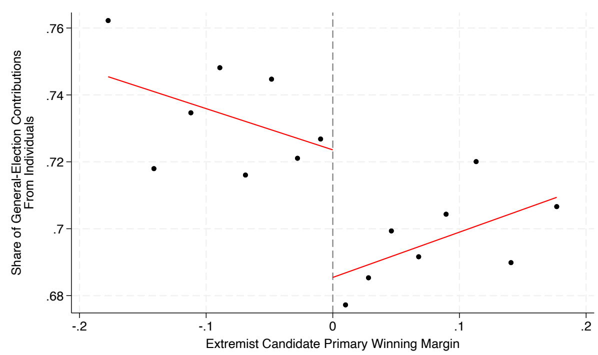
5.1 Individuals and Corporate PACs Punish Extremist Nominees

As discussed in Section 2, existing research disagrees on whether individual donors and corporate PACs advantage relative moderate or extremist candidates. Individual donors, like general-election voters, may prefer relative moderates—or, if the donorate is heavily skewed towards ideologues, their contributions may instead favor relative extremists. Corporate PACs, likewise, may disproportionately value access to relative extremists, or they may prefer moderates who are more electorally viable. Ultimately, whether these sources of campaign finance advantage relative moderates or extremists is an empirical matter to which I now turn.

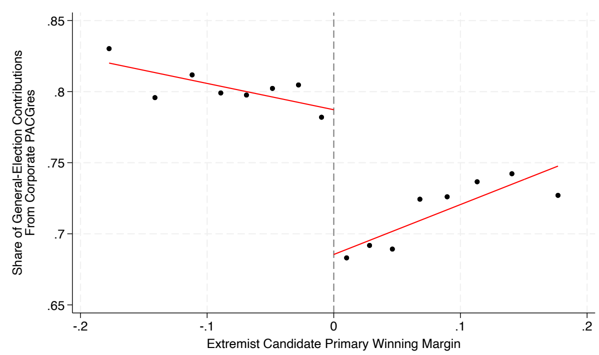
To answer this question, I disaggregate each party's fundraising total into its various sources using donor-level industry classifications from the Center for Responsive Politics

estimate interaction terms.

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



(a) Contributions from Individuals



(b) Contributions from Corporate PACs

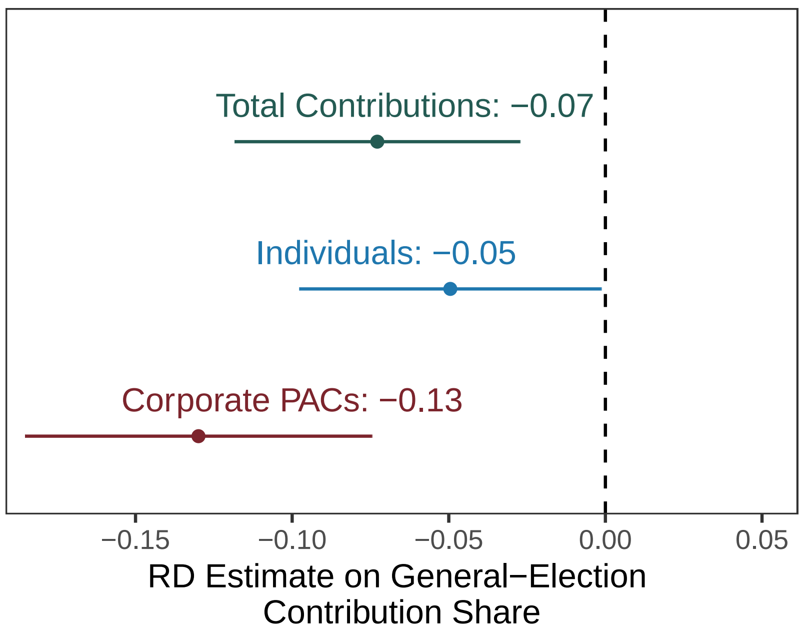
(CRP) and NIMSP.[27] For each donor type, I construct a new outcome variable containing the party's share of general election-contributions originating from that source. These variables measure the extent to which a given contribution source advantages a party.

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 horizontal axis is greater than zero, the extremist candidate wins the primary nomination and represents their party in the general election. When the horizontal axis is instead less than zero, the moderate candidate wins the primary nomination and runs in the general election. In both plots there appears to be a sharp decrease in contribution shares at the discontinuity, with a noticeably larger jump for corporate PACs than individual donors.

Figure 5 presents formal estimates of these discontinuities using a third-order polynomial

[27] 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 the main text. Industry-level classifications are available for all years of the NIMSP data.

Figure 5 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share by Donor Type in Congress, 1980-2022, and State Legislatures, 1996-2022. Corporate PACs impose a larger financial penalty on the more-extreme primary nominee in the general election than individual donors. This figure reports estimates using a cubic specification of the running variable.



specification of the running variable with a spline.[28] Horizontal lines in the plot represent 95% confidence intervals. For reference, the first estimate, labeled "Total Contributions," corresponds to the estimate from column two of Table 1.

The second estimate in Figure 5 focuses on contributions from individual donors. I find that, when a party narrowly nominates a relative extremist, its share of general-election contributions from individuals donors declines by 5 percentage points. The final estimate in Figure 5 aggregates contributions from corporate PACs. Here, I estimate that the financial penalty imposed by corporate PACs on extremist nominees is approximately 13 percentage points. The difference in penalties imposed by individual donors and corporate PACs is highly significant ($t = 5.43$, $p < .001$; SEs clustered by district).

Overall, I find strong evidence that both individual donors and corporate PACs punish

[28] Appendix Table A.5 reports additional estimates using the series of specifications reported in Table 1.

extremist nominees, but that this penalty is larger among corporate PACs than individual donors. Broadly, these results suggest that individual donors' relative ideological preferences in general elections are more closely aligned with those of general-election voters than previously assumed. They also indicate that corporate interests favor the combination of expected tenure, electoral viability, and access that more-moderate candidates offer relative to more-extreme candidates.

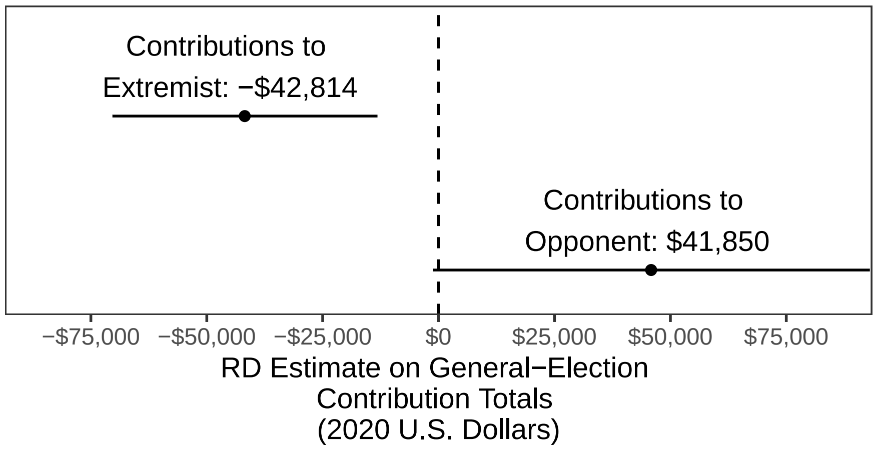
I also investigate heterogeneity in the financial penalty to extremists across corporate industries. Since the results do not change the substantive interpretation of this section, I refer interested readers to Appendix A.4 for complete results. In short, I find that the financial penalty to extremist nominees is remarkably stable across all ten corporate industries defined by the FEC and NIMSP.

5.2 Symmetric Effects Among Extremist-Party and Opposing-Party Donors

Because fundraising relative to an opponent is likely more consequential than absolute dollar totals, the analysis so far has focused on candidates' general-election contribution shares. However, studying contribution shares obscures whether the financial penalty to extremists is driven by donors abandoning the extremist party's nominee, rallying around their opponent, or some combination of the two. To differentiate these pathways, I examine the financial penalty to extremist nominees in aggregate dollars.

Specifically, I reestimate Equation 1 with parties' logged contribution totals as the outcome. Figure 6 presents estimates from this analysis using a third-order polynomial of the running variable. In the first row, I estimate that the "coin-flip" primary nomination of an extremist *reduces* their party's general-election fundraising total by roughly $43,000 relative to a barely-winning moderate's party. This estimate is consistent with a substantial penalty among extremist-party donors. The second row of Figure 6 focuses on contributions to the opposing party. Here, I find that close primary nomination of an extremist *increases* the op-

Figure 6 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Totals in Congress, 1980-2022, and State Legislatures, 1996-2022. The financial penalty to more-extreme primary nominees is driven symmetrically by donors abandoning the extremist party and rallying around their opponent. This figure reports estimates using a cubic specification of the running variable.



posing party's general-election fundraising by $42,000.[29] While the latter effect is estimated imprecisely, the difference between the estimates for extremist- and opposing-party donors is highly significant ($t = 5.39$, $p < .001$; SEs clustered by district).

Taken together, these results indicate that the financial penalty to extremist nominees is driven in roughly equal proportion by withdrawal among extremist-party donors and increased mobilization among their opponents' donors.

6 RD Estimates Generalize to Universe of Contested General Elections

In the previous two sections, I 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 districts featuring close primary elections because

[29]Due to a transcription error, an earlier version of this manuscript reported that the coefficient on "Contributions to Opponent" was positive but substantively small. After resolving this error, the coefficient on "Contributions to Opponent" is similar in magnitude to the coefficient on "Contributions to Extremist."

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-based identification strategy 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.

6.1 The Midpoint Method

Specifically, I replicate my main analyses using the "midpoint" method from Ansolabehere, Snyder, and Stewart (2001).[30] 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 becomes unambiguously more moderate while the Republican becomes more extreme.

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

$$Y_{dt} = \beta_0 + \beta_1 Midpoint_{dt} + \beta_2 Distance_{dt} + \delta_t + \gamma_i + \varepsilon_{dt}, \tag{4}$$

where $Midpoint_{dt} = \frac{Dem\ Ideology_{dt} + Rep\ Ideology_{dt}}{2}$ is the midpoint between the Democratic and Republican candidate's *Primary-Specific Scaling*, $Distance = |Dem\ Ideology_{dt} - Rep\ Ideology_{dt}|$ is the distance between the two parties' candidates, and Y_{dt} is one of the outcomes introduced in Sections 4 and 5. The term δ_t stands in for year fixed effects, and γ_i represents either

[30] Other studies that employ the midpoint design include Bonica and Cox (2018), Hall and Snyder (2015), Hall (2019), and Handan-Nader, Myers, and Hall (2025). 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).

district-regime fixed effects or district presidential vote share.

The magnitude of the coefficient on *Midpoint*, however, is not immediately comparable to the RD estimates reported above.[31] To make these estimates comparable, I apply a simple linear transformation to the *Midpoint* coefficient.[32] 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.[33] I then multiply the *Midpoint* coefficient by this average change, yielding a *Midpoint* estimate that is comparable to my RD estimates. Throughout the paper, I present estimates from the midpoint method in terms of the RD scale.

6.2 RD and Midpoint Method Yield Consistent Results

Figure 7 compares my RD and midpoint method estimates. In this figure, estimates from the midpoint method are plotted with triangles and dashed 95% confidence interval bars, while the baseline RD estimates are reported with squares and solid error bars. I report estimates separately for total contributions (first pair) and contributions from individual donors and corporate PACs (second and third pairs, respectively). As the figure illustrates, the midpoint and RD estimates are highly consistent in magnitude, differing by one percentage point at most. However, because the midpoint regression incorporates all contested general elections, estimates from the midpoint method are substantially more precise than the RD.

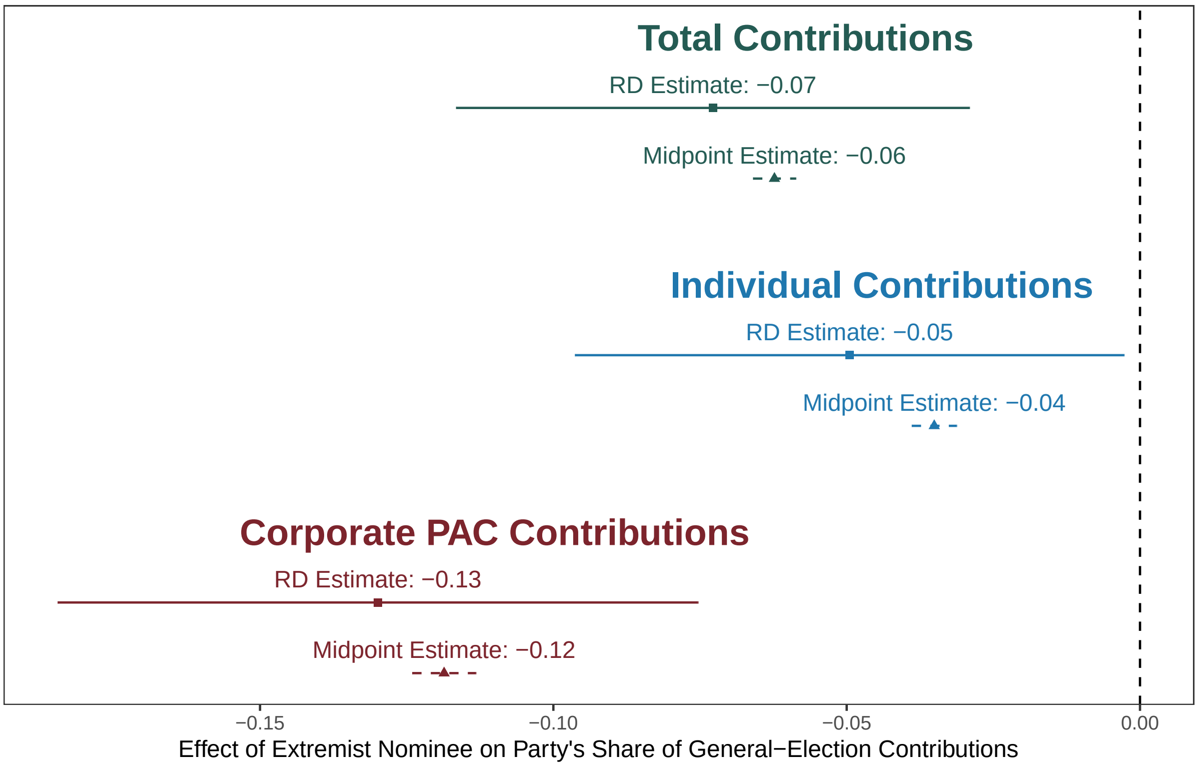
Overall, the consistency between the RD and midpoint estimates indicates that my central results generalize beyond the set of districts featuring close contested primary elections. I now rely on this added statistical power to study over-time variation in the financial penalty to extremists.

[31] Before transformation, β_2 represents the change in the Democratic candidate's general-election contribution share resulting from a shift from the leftmost to rightmost midpoint.

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

[33] Intuitively, this division is necessary because replacing a relative moderate nominee with a relative extremist (or vice versa) only shifts the midpoint between general-election candidates by half the distance between the alternate nominees.

Figure 7 – Comparison of RD and Midpoint Estimates of the Effect of More-Extreme Candidates on their Party's Share of General-Election Fundraising in Congress, 1980-2022, and State Legislatures, 1996-2022. This figure compares RD and midpoint estimates of the financial penalty to more-extreme candidates, after transforming the midpoint estimates to the same scale as the RD. Both methods yield highly similar point estimates.

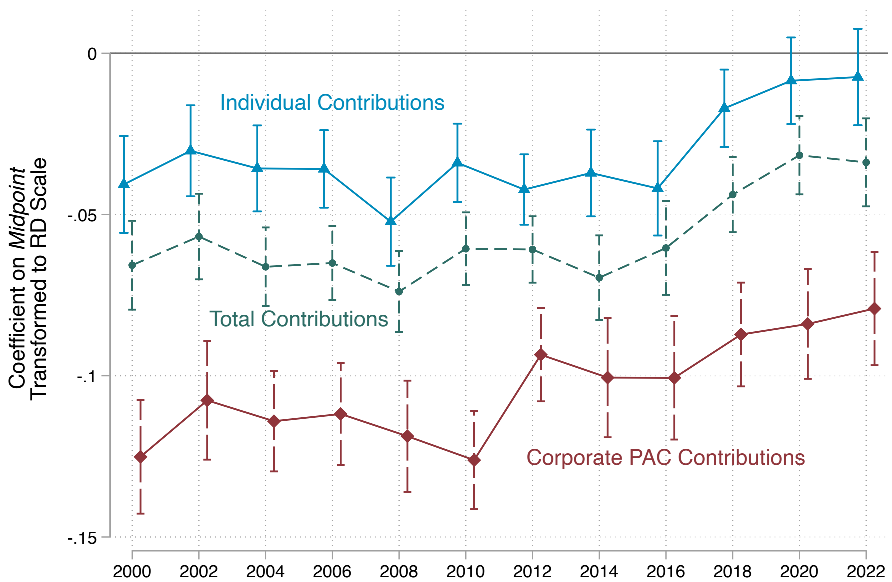


6.3 Extremist Nominees Face a Shrinking Financial Penalty

The political and informational environment in which donors contribute has changed markedly over the past two decades. These shifts raise the possibility that donors' responses to extremist nominees have evolved during the same period. Given sample size restrictions, it is challenging to comprehensively evaluate whether the financial penalty to extremists has changed over time using the RD. The midpoint regression, however, which leverages data across all contested general elections, provides the statistical power necessary to answer this question with confidence.[34]

[34]Nevertheless, in Appendix A.7 I show that my conclusions remain unchanged when using the RD to estimate over-time change in the financial penalty to extremist nominees.

Figure 8 – Financial Penalty to Extremist Nominees Over Time in Congress and State Legislatures, 2000-2022. The financial penalty to more-extreme candidates has declined by half since 2000. Points represent estimates of β_1 from Equation 4, after applying a linear transformation that aligns the scale of the Midpoint and RD estimates. Models are estimated using presidential vote share to hold the district median constant. Bars represent 95% confidence intervals. Green circles represent total contributions, red diamonds represent corporate PAC contributions, and blue triangles represent individual contributions.



Using district presidential vote share to hold partisanship constant, I reestimate Equation 4 separately for every even-year election cycle in my sample since 2000. Again, to make these estimates comparable to the RD, I apply the same linear transformation to these coefficients as described in Section 6.1. Figure 8 plots the results, along with 95% confidence intervals, for total contributions, individual donors, and corporate PACs.

In addition to confirming that corporate PACs are more sensitive to extremist nominees than individual donors, I find that the financial penalty to extremists has declined steadily since 2000. For example, in 2000, I estimate that an extremist nominee could expect their share of total general-election contributions to be 7 percentage points less than a moderate candidate. However, by 2022, this penalty had declined to 3.5 percentage points. Overall, the decline in the financial penalty to extremists appears to be driven roughly equally by individual donors and corporate PACs, raising important questions about the source of this decline.

7 State-Level Heterogeneity in the Financial Penalty to Extremist Nominees

What factors help explain the decline in the financial penalty to extremist nominees? And where is the financial penalty to extremists particularly strong? While a comprehensive mediation analysis is beyond the scope of this paper, in this section I evaluate several of the most plausible and substantively important possibilities, guided in part by the theoretical perspectives introduced in Section 2. By investigating these patterns, we can also learn more about the underlying roots of the financial penalty to extremist nominees. To facilitate this analysis, I focus in this section on state legislatures, where rich heterogeneity within and between states offers valuable leverage to study variation in the financial penalty to extremist nominees.

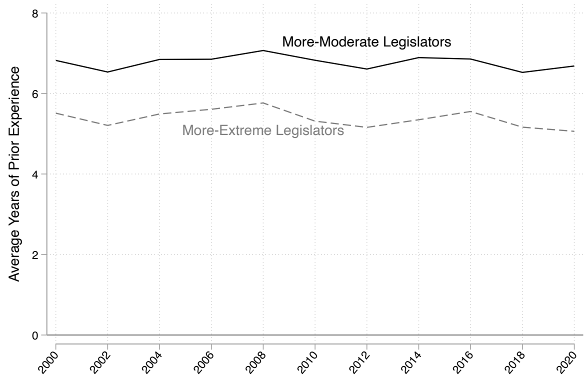
7.1 More-Moderate Candidates' Declining Electoral Success

One of the most striking results from the previous section is the sharp decline in the financial penalty imposed on extremist primary nominees by corporate PACs. This trend among individual donors is consistent with a broader increase in partisanship across the American public, as shown in Appendix A.10, but corporate PACs are thought to prioritize strategic considerations over ideological alignment. Why have access-oriented corporate donors become increasingly likely to support relative extremists?

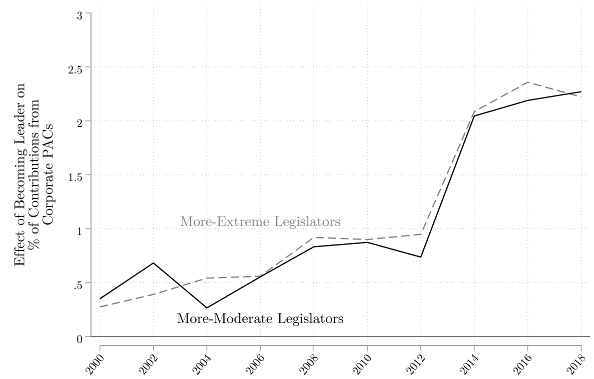
To evaluate a potential source of this decline, I return to the theoretical models of access-seeking contributions outlined in Section 2. These models suggest that access-seeking donors value three considerations when allocating funds: a candidate's probability of winning, the expected value of access through that candidate, and the candidate's anticipated tenure in office. Figure 9 examines how these three factors have evolved over time for more-moderate

Figure 9 – Over-Time Change in the Factors that Motivate Corporate PAC Contributions in State Legislatures, 2000-2022. This figure plots the expected tenure of, value of access to, and win-probability advantage of more-moderate versus more-extreme candidates in state legislatures, 2000-2022. Panel A plots the average years of prior legislative experience for more-moderate and more-extreme candidates. Panel B reports the within-legislator difference-in-differences estimate of the revealed value of attaining leadership status in terms of corporate PAC contributions, as first studied by Fouirnaies (2018). Panel C plots the win-probability penalty to more-extreme general-election candidates, as measured using the midpoint method and transformed to the RD scale.

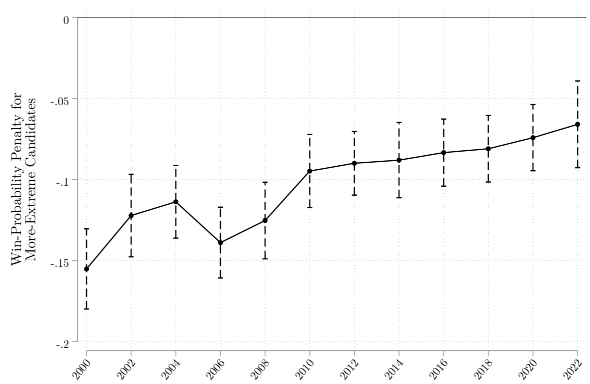
(a) Expected Tenure



(b) Value of Access



(c) More-Extreme Candidates' Probability of Winning



and more-extreme nominees.

First, in Panel A of Figure 9, I plot legislators' expected tenure in office, as proxied by their average years of prior experience.[35,36] For simplicity, I classify legislators in a binary

[35]I study average years of prior experience, rather than average total experience, to eliminate censoring bias caused by the fact that current legislators' complete electoral history is unknown.

[36]Panel A ends in 2020 because the tenure of legislators serving in seats with four year terms is not yet observable in my sample.

fashion as more-moderate or more-extreme based on their roll-call voting record and the median member of their party. The black line in Panel A of Figure 9 plots the expected prior tenure for more-moderate legislators, while the dashed gray line reports the same quantity for more-extreme legislators. While more-moderate legislators are, on average, slightly more experienced than more-extreme legislators, this difference has remained remarkably constant across the period of study. Hence, differential changes in relative moderate versus extremist candidates' expected tenure do not appear to explain corporate PACs' increasing willingness to fund extremist nominees.

Using the same legislator classifications from Panel A, Panel B of Figure 9 studies the revealed value of access to more-moderate and more-extreme legislators. To do so, I focus on state legislative chamber leaders, whose positions make them particularly valuable targets for access-seeking contributors, and implement a difference-in-differences design that compares contributions from corporate PACs to more-moderate and more-extreme legislators before and after attaining a leadership position.[37] This design was first studied by Fouirnaies (2018). Panel B of Figure 9 plots my estimates of the effect of becoming a leader on a legislator's share of total chamber-level contributions from corporate PACs.[38] Clearly, the value of access to legislative leaders has increased over time, matching Fouirnaies (2018). However, this increase is highly similar for more-moderate and more-extreme leaders, suggesting that changes in the value of access to relative moderates and extremists do not explain corporate

[37]Specifically, I estimate

$$\begin{aligned} \textit{Share Industry Contributions}_{ict} = &\sum_{t=2000}^{2018} [\beta_{1,t} \textit{Leader}_{ict} \times \textit{Extreme}_{ict}] + \\ &\beta_2 \textit{Majority Member}_{ict} + \zeta_i + \delta_{ct} + \varepsilon_{ict}, \end{aligned} \tag{5}$$

where $\textit{Share Industry Contributions}_{ict}$ represents legislator i's share of all contributions made by corporate PACs in chamber c and session t, $\textit{Leader}_{ict}$ indicates whether legislator i was a leader in chamber c in session t, and $\textit{Extreme}_{ict}$ indicates whether legislator i was more extreme than the median member of their party in chamber c in cycle t. The term $\textit{Majority Member}_{ict}$ indicates whether legislator i was a member of the majority of chamber c in session t, and ζ_i and δ_{ct} stand in for legislator and chamber-by-session fixed effects, respectively. The coefficients $\beta_{1,t}$ represent the difference-in-differences estimate of the revealed value of attaining leadership status in session t in terms of corporate PAC contributions. Data on legislative leaders is from Fouirnaies (2018).

[38]I exclude 2020 and 2022 from Panel B of Figure 9 because leadership data for these legislative sessions are not yet available from traditional sources.

PACs' increasing willingness to fund extremist primary nominees.

Finally, I study the probability that more-extreme candidates win their election. To do so, I reestimate the midpoint regression from Equation 4 after substituting in an indicator for the Democratic candidate's victory as the outcome.[39] As before, I transform the estimates to the RD scale so that larger values represent a greater advantage for more-extreme candidates. The estimates are plotted in Panel C of Figure 9. As the figure depicts, I estimate that the win-probability penalty imposed on more-extreme candidates by voters has declined by half since 2000. Hence, as more-extreme candidates become increasingly likely to win office, it appears that access-seeking PACs have strategically shifted their contributions towards relative extremists.

An alternative explanation is that, rather than reallocating funds to increasingly-competitive extremists, corporate PACs have become more partisan in their giving. Appendix A.10 shows that this possibility is highly unlikely. Specifically, Appendix Figure A.10 plots the probability that corporate PACs and individual donors contribute to at least one Democrat and one Republican, conditional on making at least five contributions in an election cycle.[40] I find that the probability that corporate PACs contribute to candidates from both parties has remained remarkably constant at 80% during the period of study. The same probability for individual donors, however, has declined from 35% in 2000 to less than 5% in 2022. These results suggest that, while individual donors have become increasingly partisan over the past two decades, corporate donors have not.

To recapitulate, based on the theoretical perspectives introduced in Section 2, the results presented in this section suggest that the declining financial penalty to extremist nominees imposed by corporate PACs is not driven by differential changes in the expected tenure or relative value of access to more-moderate versus more-extreme legislators, nor have corporate PACs become more partisan in their giving. Instead, the results suggest that access-seeking

[39]These estimates are distinct from Handan-Nader, Myers, and Hall (2025), which focuses on state legislative candidates' vote-share advantage. However, my results are highly similar using the Democratic candidate's general-election vote share instead of an indicator for victory.

[40]The results are highly similar across a variety of cutoffs.

contributors have responded to shifting electoral dynamics: as more-extreme candidates have become increasingly viable general-election contenders, corporate PACs have adapted by reallocating funds to these candidates.

7.2 Donor Information, Election Timing, and Election Salience

Finally, to better understand the conditions under which extremist nominees face financial penalties, I examine how the size of the penalty varies across electoral contexts. In particular, I assess whether differences in the information environment, election timing, and election salience are associated with systematic variation in the financial penalty. Identifying these contextual sources of heterogeneity offers insight into the broader forces that shape the financial penalty to extremists and its recent decline.

To maximize statistical power, I use the midpoint regression (Equation 4) as the baseline specification in this section and transform the resulting estimates to the RD scale. With the exception of the estimates for election timing—where limited variation in off-cycle elections renders the RD too noisy to draw strong inferences—all subsequent results replicate using the a heterogeneity-in-discontinuities framework that adapts equation Equation 1. Because these moderating variables are not randomly assigned—both in the midpoint method and RD approach—any causal interpretation in this section requires caution. For reference, column one of Table 4 estimates the baseline financial penalty to extremist nominees in state legislatures.[41]

First, a key pre-condition for a financial penalty to extremists 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. To test this prediction, I draw on Myers's (2025) measure of the congruence between state legislative districts and newspaper markets, which plausibly exogenously shapes the amount of newspaper coverage

[41] The estimates in column one of Table 4 are smaller than the estimates plotted in Figure 7 because the former only includes state legislatures while the latter includes state legislatures and Congress.

Table 4 – Variation in Financial Penalty to More-Extreme Candidates in State Legislatures, 1996-2022. The financial penalty imposed on more-extreme candidates is greater in districts that receive stronger legislative press coverage, stronger in more-professionalized state legislatures, and (maybe) weaker in odd-year elections. *New Congruence* and *Professionalization* are scaled to run from 0 (lowest) to 1 (highest), and *Midterm Year* and *Odd Year* are indicator variables. This table reports estimates from the midpoint method that are transformed to the RD scale.

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Midpoint	-0.05	-0.05	-0.05	-0.04
	(0.00)	(0.00)	(0.00)	(0.00)
Midpoint · News Coverage		-0.03		
		(0.01)		
Midpoint · Professionalism			-0.02	
			(0.01)	
Midpoint · Midterm				0.00
				(0.00)
Midpoint · Odd-Year				0.02
				(0.01)
N	13,043	11,466	13,043	13,043

Note: Robust standard errors clustered by district are reported in parentheses. Lower-order terms are omitted from table for brevity.

that the public receives about their legislative election.[42] I scale this variable, *News Coverage*, to run from 0 (least congruent district) to 1 (most congruent district) in my sample. Column two of Table 4 interacts *News Congruence* with *Midpoint*. Here, I estimate that the financial penalty to extremists is 60% larger in magnitude in the most congruent districts in my sample in comparison to the least congruent districts. A more realistic one standard deviation increase in *News Congruence* (.19) is associated with a 12% increase in the magnitude of the financial penalty to extremists. Broadly, these results suggest that the political information environment plays a meaningful role in the financial penalty to extremists. These findings also raise the possibility that the erosion of local press coverage (Hayes and Lawless, 2018; Martin and McCrain, 2019; Napoli et al., 2017; Peterson, 2021;

[42]Data on *News Coverage* is not available for a small number of districts. My results are highly similar when excluding these districts throughout Table 4, rather than only in column two.

Worden, Matsa, and Shearer, 2022) may help account for the reduced financial penalty to extremists, particularly among individual donors.

Next, in column three I interact *Midpoint* with Squire's (2007; 2017) measure of legislative professionalism, scaled to run from 0 (least professionalized) to 1 (most professionalized) in my sample. To the extent that professionalization makes legislative races more salient to donors, the financial penalty to extremists may be greater in more-professionalized states. Column three presents evidence in line with these predictions. I find that the penalty to extremist nominees is roughly 40% larger in magnitude in the most professionalized legislatures in comparison to the least professionalized legislatures. Since the standard deviation of legislative professionalism is .25, a one standard deviation increase in professionalism is associated with a 10% increase in the magnitude of the financial penalty. These results match Handan-Nader, Myers, and Hall (2025), who report suggestive evidence that the electoral return to moderation is greater in more-professionalized states.

Finally, in column four I test whether the financial penalty to extremists is smaller in midterm and odd-year elections. While election timing captures a bundle of potential treatments—including turnout and coattail effects—it nonetheless represents a substantively important contextual factor with clear electoral implications. I estimate in column four of Table 4 that the financial penalty to extremists is substantially smaller in odd-year elections but no different in midterm-year elections. However, as noted above, the corresponding estimates in the RD are highly imprecise.[43]

Taken together, these results suggest that the financial penalty to extremist nominees is shaped by the broader electoral context in which candidates compete. Donors appear more responsive to ideological differences between candidates when information is readily available, elections are more salient, and (maybe) when elections coincide with national contests.

[43] This is likely because there are few odd-year elections in the RD sample.

8 Discussion and Conclusion

American legislatures are more polarized today than at any point in the past half century, and observers often worry that donors are partially responsible. By disproportionately contributing to more-extreme candidates, these critics contend, donors advantage relative extremists in elections and facilitate the extraordinary polarization of American politics. Yet obtaining systematic evidence on whether donors advantage relative moderate or extremist candidates is challenging, because candidates strategically select into running based on their fundraising prospects, and traditional measures of candidates' ideological positions are endogenous to their fundraising outcomes. This paper overcomes these empirical challenges by pairing a regression discontinuity design in congressional and state legislative primary elections with an original candidate ideology scaling and panel-based models of electoral selection.

Leveraging the RD design, I document that the "coin-flip" primary nomination of an extremist candidate over a more-moderate opponent decreases their party's share of general-election contributions by 6-7 percentage points in the median primary. For the largest ideological contrasts, the financial penalty to extremist primary nominees grows to 18-19 percentage points. Critically, this financial penalty is not limited to districts featuring close primary elections. Studying a complementary panel-based identification strategy, I find that donors punish extremist nominees across the full set of contested congressional and state legislative general elections in my sample, on average.

Overall, these findings indicate that general-election donors act as a moderating force in American politics in response to the nomination of extremist candidates. By penalizing extremist nominees, general-election donors may directly affect these candidates' competitiveness and likelihood of winning office (Avis et al., 2022; Erikson and Palfrey, 2000; Fouirnaies, 2021; Gerber, 1998; Green and Krasno, 1988). In addition to directly influencing candidates' fundraising totals, this financial penalty may also indirectly shape the composition of the pool of legislative office seekers and parties' strategies; extremist candidates who anticipate fundraising challenges may opt to not run at all (Carnes, 2018; Fowler and Mc-

Clure, 1990; Thomsen, 2025), while parties may allocate campaign resources towards more financially-viable moderates (La Raja and Schaffner, 2015).

At the same time, this moderating filter has weakened substantially over the past two decades. In 2000, extremist nominees raised about 7 percentage points less in the general election than more-moderate candidates; by 2022, that gap had narrowed to less than 3.5 percentage points. As the financial penalty to extremist nominees declines, candidates who were once deterred by limited donor support may now find more viable paths to office.

Taken together, this paper reveals a previously under-recognized constraint on more-extreme candidates: the preferences of general-election donors. While the penalty has fallen in recent decades, when voters nominate more-extreme candidates, their party suffers financially in the general election.

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Online Appendix

Do Donors Punish Extremist Primary Nominees?

Evidence from Congress and American State Legislatures

Contents

A.1 Strategic Donating and Post-Treatment Bias

In this section, I outline two forms of endogeneity 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, and I document how my preferred scaling method addresses these concerns.

Post-Treatment Bias

The first concern with jointly scaling a candidate based on the contributions they receive 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, or even for their classification as relative moderates and extremists to be flipped.

Such a scenario would arise if the composition of a candidate's 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 corporate PACs.

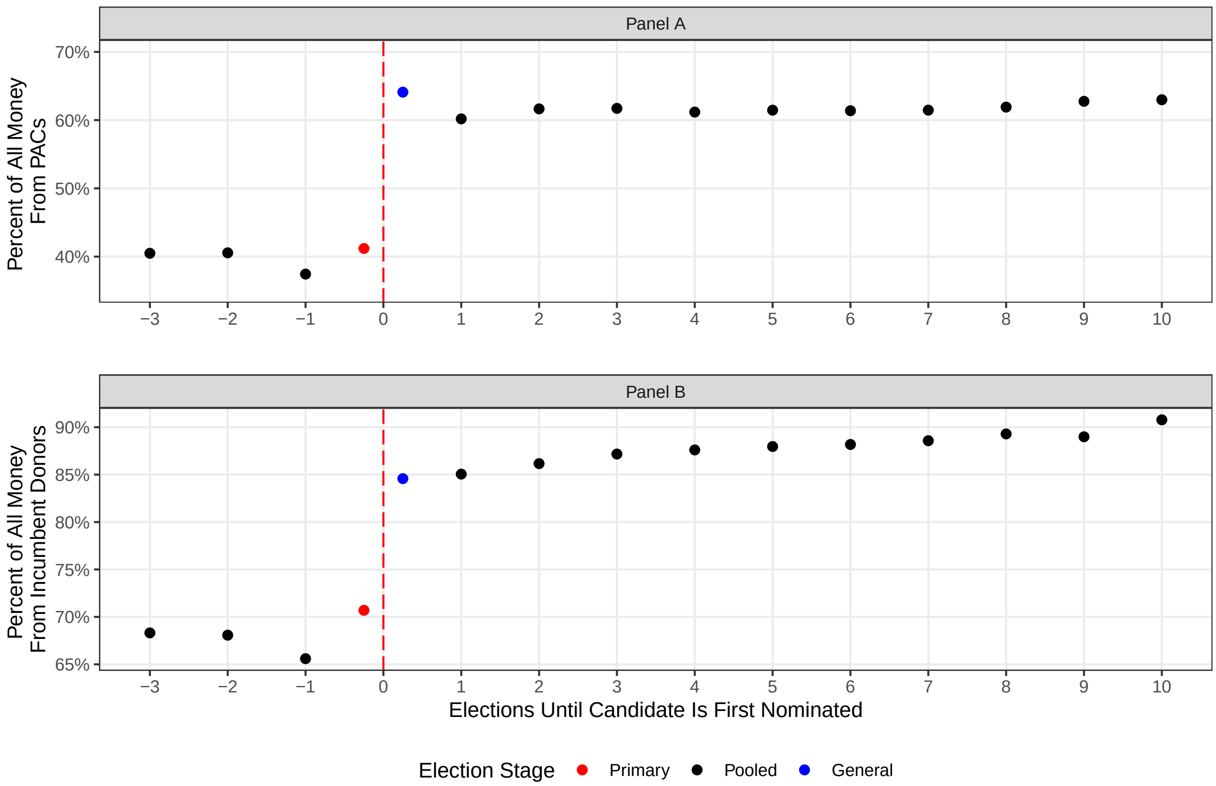
To further illustrate these compositional effects, I introduce the concept of an "incumbent donor." For election cycle t and candidate i, I define an incumbent donor as a donor that has contributed to at least one incumbent by the time of election t that is not candidate i.[1] Incumbent donors are critical contributors because my method relies precisely on donors who contribute to both incumbents and non-incumbents to bridge roll-call voting scores from the former to the latter. I calculate the share of each candidate's donors that are "incumbent donors," weighted by contribution amounts, and again restrict the analysis to candidates who eventually win at least one primary election.[2] Panel B of Figure A.1 plots this share averaged across candidates in a given horizontal axis. Overall, I find that candidates' individual donorates become significantly more connected to other incumbents after they win their first primary.

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. In Figure A.2, I formally

[1] 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 general election.

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

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. The 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.



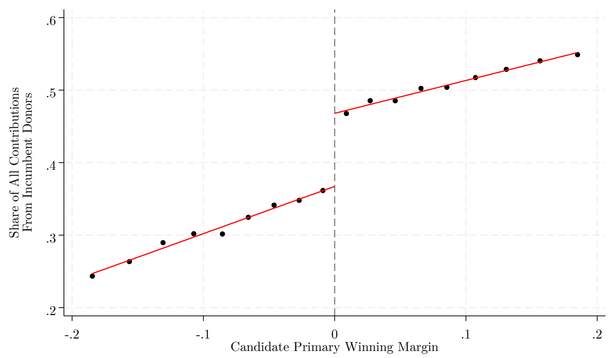
estimate the compositional effects identified in Figure A.1 using an RD.

Specifically, the unit of analysis in Figure A.2 is the candidate, and the running variable is the candidate's primary-election winning margin. This RD identifies the causal effect of winning the primary election on the composition of that candidate's contributions. In the first row of Figure A.2, I plot the RD estimate of the effect of winning a primary on a candidate's share of *all* contributions from incumbent donors or corporate PACs. As is apparent, winning a primary election substantially increases the candidate's share of *all* contributions from incumbent donors and corporate PACs.

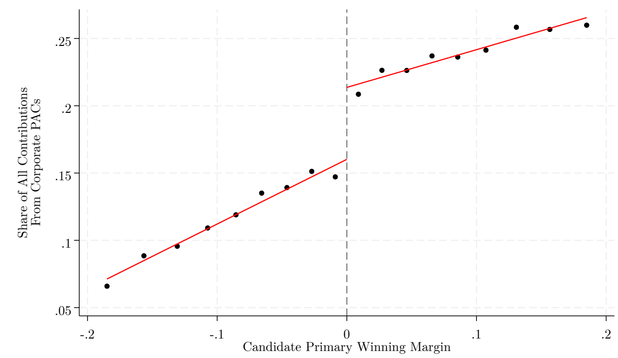
To address concerns about post-treatment bias, in Section 3.2 I describe how I exclude all contributions made during the general election when scaling candidates. In the second row of Figure A.2, I reestimate the candidate-level RD after restricting the outcomes to

Figure A.2 – RD Estimate of Effect of Winning Primary Election on Donor Composition in Congress, 1980-2022, and State Legislatures. Winning the primary election increases a candidate's share of *all* contributions and the share of *all* contributions from incumbent donors (first row), but this effect disappears when the sample is restricted to *primary-election* contributions (second row).

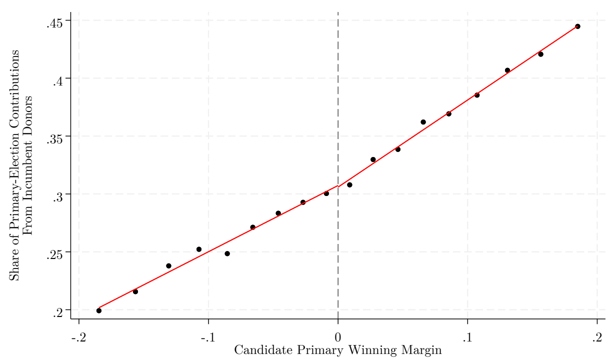
(a) Share of **All** Contributions From Incumbent Donors



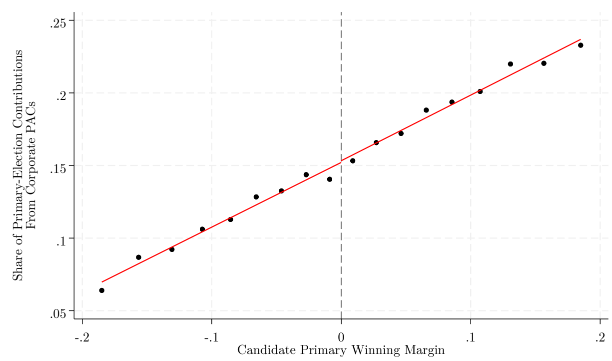
(b) Share of **All** Contributions From Corporate PACs



(c) Share of **Primary-Election** Contributions From Incumbent Donors



(d) Share of **Primary-Election** Contributions From Corporate PACs



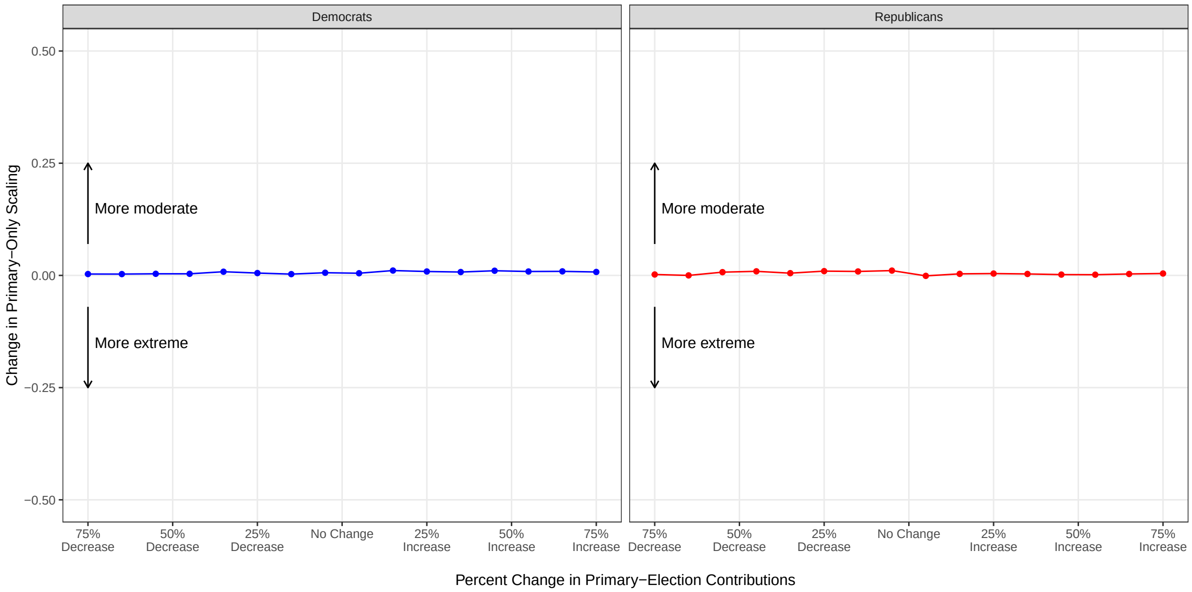
include only primary-election contributions. After making this restriction, the discontinuity disappears entirely, indicating that the data restrictions I introduce address concerns about post-treatment bias.

Scalings Conflate Moderation with Fundraising Success

A second concern is that campaign finance-based scalings may conflate ideological moderation with fundraising success if donors contribute on the basis of candidates' non-ideological characteristics. This concern is particularly acute when jointly scaling candidates based on primary- and general-election fundraising or including contributions from access-seeking PACs. However, since I omit general-election contributions and contributions from access-seeking PACs from my preferred ideological scaling, I focus in this section on concerns related to primary-election fundraising from individual donors.

While it is difficult to test this concern directly, I begin by running a series of simulations

Figure A.3 – Simulated Effect of Altering Candidates' Primary-Election Fundraising Success. This figure plots the results from a series of simulations where candidates' primary-election fundraising success is increased or decreased, holding fixed the underlying ideological space. Altering primary-election fundraising success does not meaningfully affect candidates' estimated *Primary-Only Scaling*.



that evaluate whether altering a candidate's primary-election fundraising success systematically affects their estimated ideology. Specifically, I bootstrap candidate i's primary-election contribution matrix—increasing or decreasing their number of contributions by factors between 5% and 75%—while holding fixed all other candidates' primary-election contributions.[3] Using this modified contribution matrix, I calculate the full set of candidate ideology scalings following the methodology described in Section 3 and extract candidate i's scaling (henceforth, $\textit{Bootstrapped Primary-Only Scaling}_i$). I then calculate the change in candidate i's scaling caused by altering their fundraising success as

$$\textit{Scaling Change}_i = \textit{Primary-Only Scaling}_i - \textit{Bootstrapped Primary-Only Scaling}_i,$$

where $\textit{Primary-Only Scaling}_i$ is candidate i's true *Primary-Only Scaling*.[4]

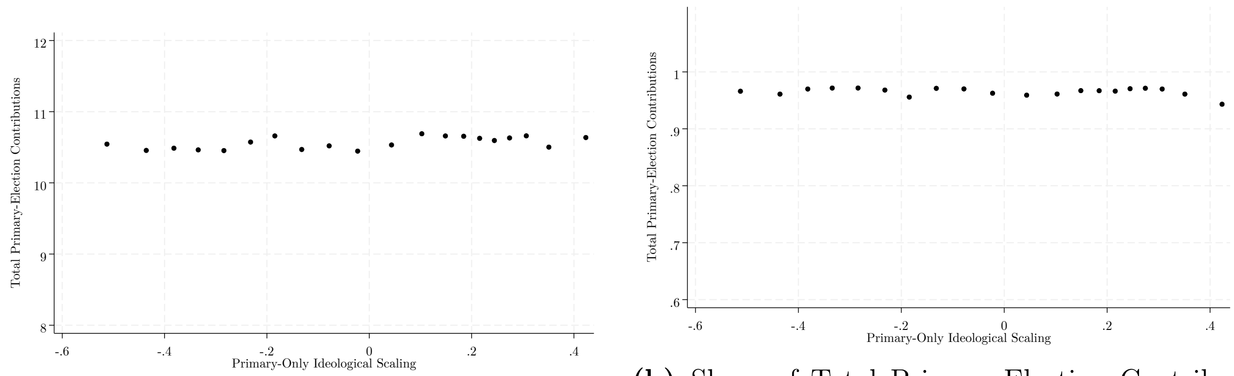
Figure A.3 plots the average value of *Scaling Change* separately for Democratic and Republican candidates. I find that permuting candidates' primary-election contributions does not meaningfully affect their estimated ideological positions. The estimated change in candidates' ideological positions is less than .01 across sample size factors.[5]

[3] I iteratively bootstrap an individual candidate's contributions, rather than the universe of scalable candidates at the same time, in order to hold the underlying ideological space fixed. Because this process is computationally costly, I conduct this analysis for a random sample of 1000 candidates.

[4] For clarity, I set the polarity of the change in scalings such that larger values indicate more-moderate positions.

[5] This difference is equal to less than 2% of the standard deviation of the true *Primary-Only Scaling*.

Figure A.4 – Empirical Relationship Between Primary-Election Fundraising and Candidates' *Primary-Only Scaling.*



(a) Total Primary-Election Contributions

(b) Share of Total Primary-Election Contributions

To further probe whether my measurement of candidate ideology is endogenous to candidates' primary-election fundraising success, Figure A.4 plots binned averages of candidates' total primary-election fundraising and share of primary-election fundraising (vertical axis) across their estimated ideology using my *Primary-Only Scaling* (horizontal axis). As the figure illustrates, there is no evidence that candidates' positions estimated using the *Primary-Only Scaling* are endogenous to their primary-election fundraising success.

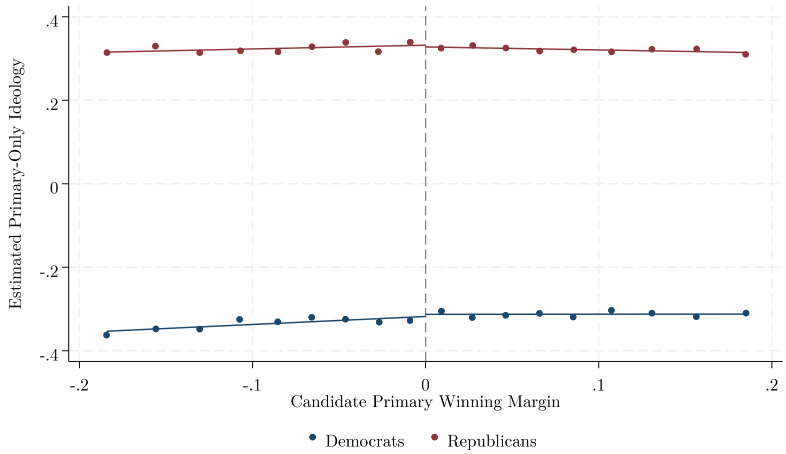
Finally, if the restrictions I impose on the scaling process in Section 3.2 address the concerns documented in this section, there should be no causal effect of winning the primary election on a candidate's estimated ideology. Figure A.5 tests this prediction using a candidate-level RD in primary elections. The running variable in Figure A.5 is a candidate's primary-election winning margin and the outcome is their *Primary-Only Scaling*. I plot estimates separately for Democrats (in blue) and Republicans (in red). As Figure A.5 depicts, I find that there is no effect of winning a primary election on a candidate's estimated ideology scaling.

Evidence that Strategic Donating and Post-Treatment Would Bias Estimates

Having documented forms of strategic donating and post-treatment bias in the contribution matrix and how my preferred scaling method addresses them, I now provide direct evidence of how failing to account for these biases would 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 from all donors to scale candidates (henceforth, the *Unrestricted Scaling*). These *Unrestricted 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 *Unrestricted 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 *Unrestricted Scaling*. As is apparent, using

Figure A.5 – Effect of Winning the Primary Election on Candidates' *Primary-Only Scaling* in Congress, 1980-2022, and State Legislatures, 1996-2022. Winning the primary election does not change candidates' *Primary-Only Scaling*.



general-election contributions to scale candidates significantly affects primary candidates' relative positioning. Using the *Unrestricted Scaling* would cause the researcher to "flip" 17% of primary candidates' moderate and extremist designations, relative to the *Primary-Specific Scaling* ($1859/10861 \approx .17$).

Second, to evaluate whether these "flips" are consequential, Table A.2 replicates Table 1 using *Unrestricted Scalings*. The estimates across Table A.2 are negative and significant, indicating that my substantive conclusions would be unchanged using *Unrestricted Scalings*. However, the estimates using *Unrestricted Scalings* are significantly larger than the estimates when using *Primary-Specific Scalings*. For example, column one indicates that using the *Unrestricted Scalings* would inflate my coefficient estimate by roughly 35% in comparison to *Primary-Specific Scalings* (-8 vs -6 percentage points).

Overall, 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, an ideological scaling that is entirely distinct from campaign contributions.

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

	Unrestricted Scaling Scaling Classification		
Primary-Specific Scaling Classification	Moderate	Extremist	N
Moderate	9002	1859	10861
Extremist	1859	9002	10861
N	10861	10861	21722

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

Table A.2 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share Using *Unrestricted Scaling* in Congress, 1980-2022, and State Legislatures, 1996-2022. RD estimates of the effect of nominating the more-extreme candidate on their party's share of general-election contributions are approximately 35% larger when using *Unrestricted Scalings*.

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.08	-0.09	-0.09	-0.09
	(0.02)	(0.03)	(0.02)	(0.02)
N	3,145	6,519	3,200	6,519
Specification	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-
Bandwidth	.10	-	0.10	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

A.2 RD Balance Tests

The key identifying assumption that underlies my 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 fundraised 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. Further, using the standard McCrary (2008) manipulation test, Figure A.6 shows that I fail to reject the null hypothesis of no jump at the discontinuity (p-value = .595).

Finally, Table A.4 tests for chance imbalances in two additional relevant variables: the party's lagged presidential and legislative vote shares. As the table shows, I find no evidence of an imbalance in these variables that would contribute to the estimates reported in the main paper.

Table A.3 – **Effect of Nominating the More-Extreme Primary-Election Candidate on Lagged General-Election Contribution Share in Congress, 1980–2022, and State Legislatures, 1996–2022.**

	Lagged Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.00	-0.00	0.02	0.00
	(0.03)	(0.03)	(0.04)	(0.04)
N	1,701	3,388	1,409	3,388
Specification	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-
Bandwidth	.10	-	0.08	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

Figure A.6 – **Density of the Running Variable Using McCrary (2008) Test in Congress, 1980–2022, and State Legislatures, 1996–2022.**

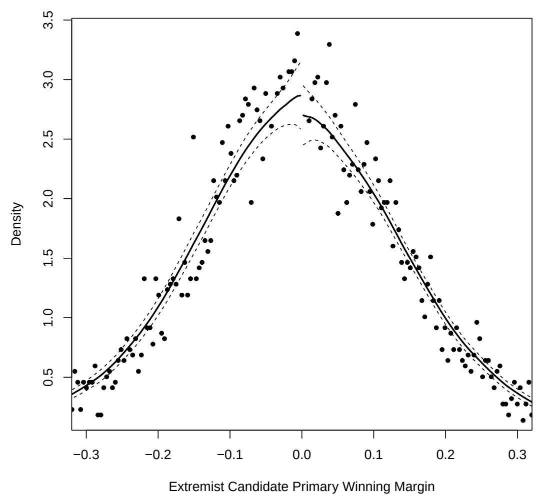


Table A.4 – Effect of Nominating the More-Extreme Primary-Election Candidate on Lagged Party Presidential and Legislative Vote Share in Congress, 1980–2022, and State Legislatures, 1996–2022. Districts that narrowly nominate the more-extreme primary-election candidate do not differ in terms of prior support for their party's presidential candidate (columns 1-4) or legislative candidate (columns 5-8).

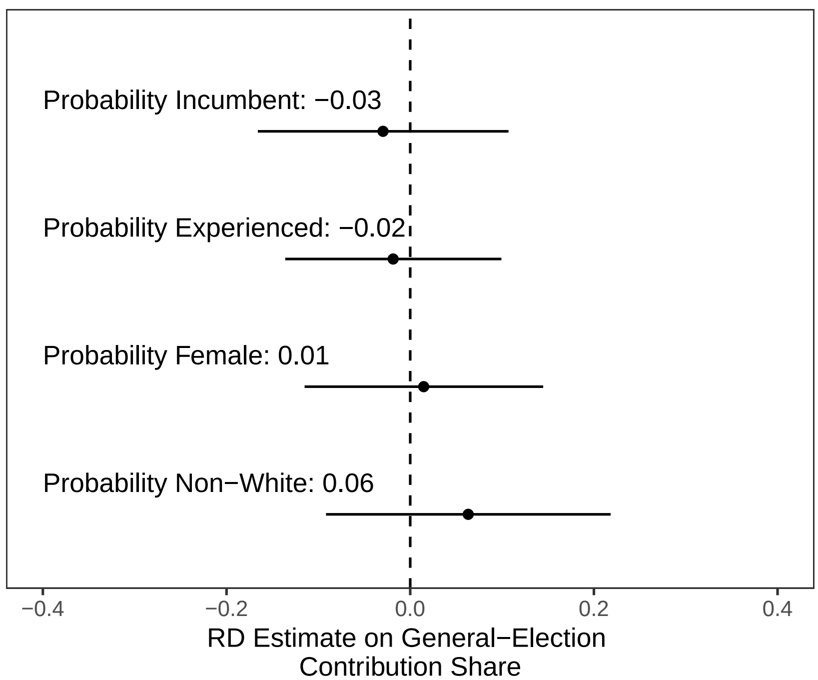
	Party's Lagged Presidential Vote Share				Party's Lagged Legislative Vote Share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Extremist Primary Win	-0.00	0.00	-0.00	-0.00	-0.00	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
N	1,643	3,315	1,742	3,315	1,848	3,675	2,200	3,675
Specification	Linear	Cubic	CCT	IW	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-	Yes	Yes	-	-
Bandwidth	.10	-	0.11	-	.10	-	0.12	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

A.3 Characteristics of Moderate and Extremist Bare-Winners

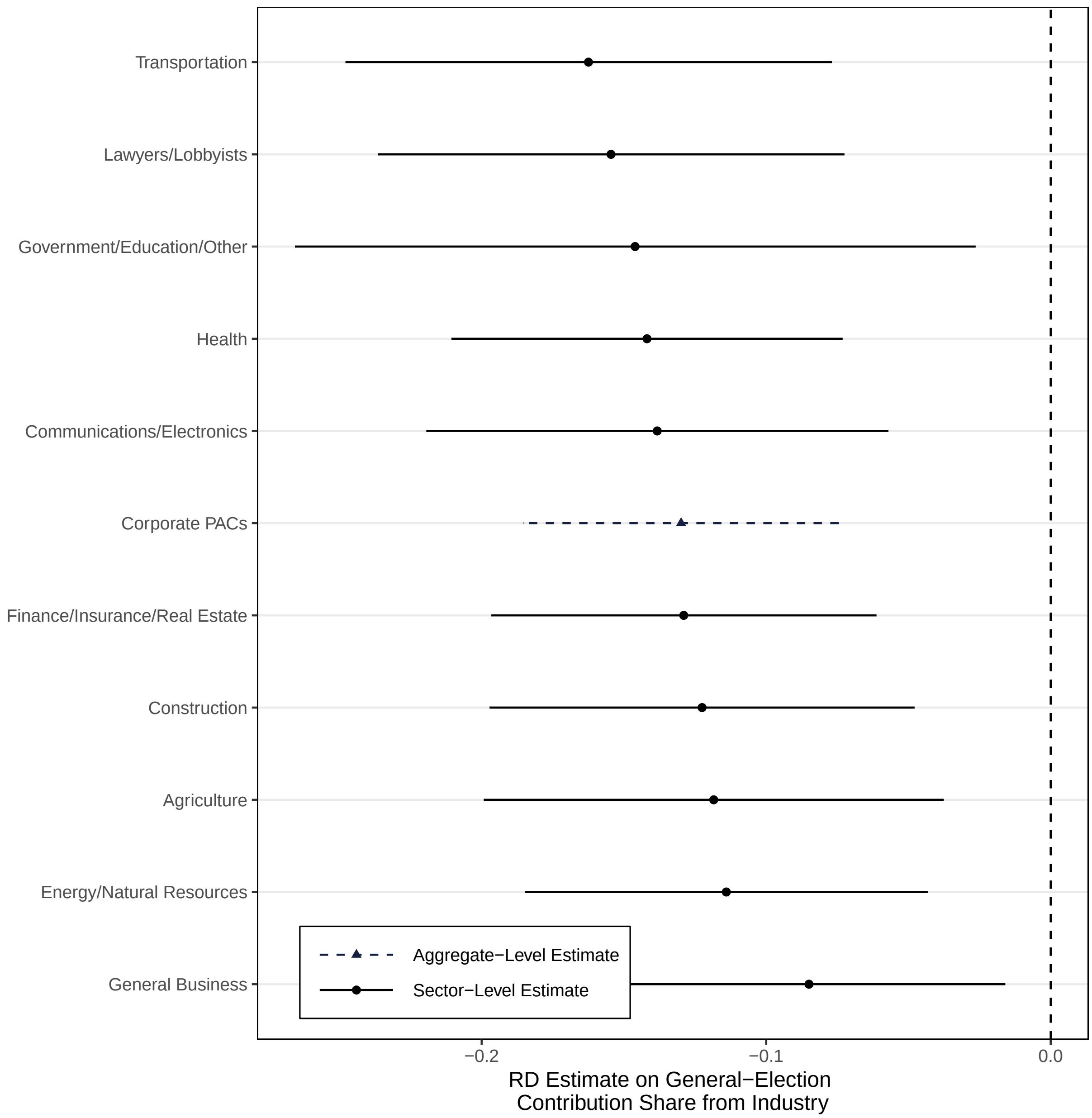
As Marshall (2022) notes, my politician characteristic RD design identifies the aggregate effect of candidate ideology and all other candidate-level characteristics that differ between the two types of barely-winning candidates (i.e., compensating differentials). Studying this bundled treatment is appropriate 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 Figure A.7, 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.7 – Characteristics of More-Moderate and More-Extreme Bare-Winners. This figure plots the difference in the probability that more-extreme and more-moderate bare-winners are an incumbent, have previous office-holder experience, are female, and are non-white. Data on experience, gender, and race are limited to candidates for Congress.



A.4 RD Estimates by Corporate Industry

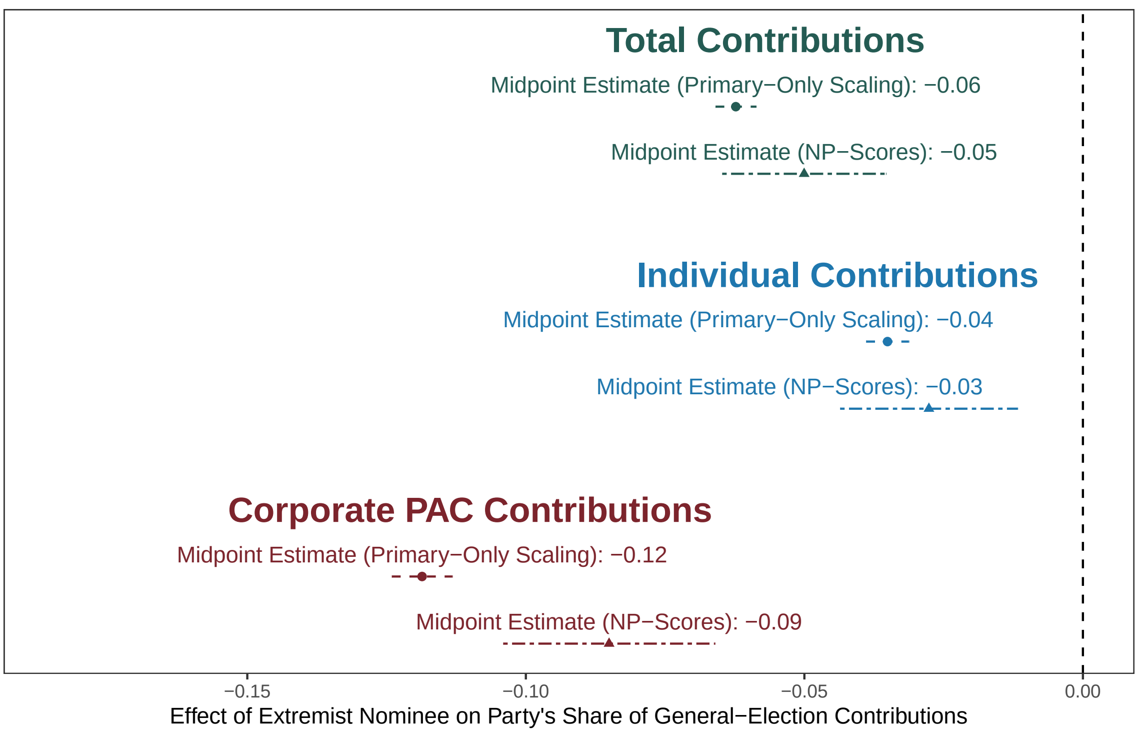
Figure A.8 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share by Corporate Industry in Congress, 2000-2022, and State Legislatures, 1996-2022. The penalty to relative extremist nominees is similarly sized across all 10 corporate industries defined by NIMSP and the FEC. This figure reports estimates using a cubic specification of the running variable.



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

To ensure that my results are not an artifact of the contribution-based scaling, I replicate the panel-based analysis from Section 6 using a measure of candidate ideology that is independent of campaign contributions. This measure draws on the state legislative roll-call voting records of prior, current, or future state legislators who face another candidate with a state legislative roll-call voting record, either in a congressional or state legislative election. The results are plotted in Figure A.9. 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.9 – Comparison of Midpoint Estimates Using Campaign Finance-Based and Roll Call-Based Scalings in Congress, 1980-2022, and State Legislatures, 1996-2022. This figure compares midpoint estimates using *Primary-Specific Scalings* and the NP-Scores of prior, current, or future state legislators who face another candidate with a state legislative roll-call voting record. Estimates are transformed to the RD scale. This figure uses Democratic presidential vote share to hold the district median constant.



A.6 Additional RD Estimates for Individuals and Corporate PACs

Table A.5 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share from Individual Donors and Corporate PACs in Congress, 1980-2022, and State Legislatures, 1996-2022. The close primary nomination of the more-extreme candidate causes a 11-13 percentage point decrease in that party's share of general-election contributions from corporate PACs and 4-5 percentage point decline among individual donors.

	Share of General-Election Contributions From Corporate PACs				Share of General-Election Contributions From Individuals			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Extremist Primary Win	-0.12	-0.13	-0.11	-0.11	-0.04	-0.05	-0.05	-0.05
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
N	2,598	5,106	2,845	5,106	2,588	5,091	3,155	5,091
Specification	Linear	Cubic	CCT	IW	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-	Yes	Yes	-	-
Bandwidth	.10	-	0.11	-	.10	-	0.13	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

A.7 RD Estimates Over Time

Table A.6 – Financial Penalty Imposed on the More-Extreme Primary Nominee Over Time Using the RD. The close primary nomination of the more-extreme candidate causes 6-7 percentage point decline in their party's share of general-election contributions, but this penalty has declined significantly in recent years.

	Share of Total General Election Contributions	Probability Experienced	Probability Non-White	Probability Incumbent
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.10	-0.01	0.04	-0.02
	(0.03)	(0.06)	(0.08)	(0.04)
Extremist Primary Win $\times$ Year $\geq$ 2012	0.06	-0.03	0.01	-0.04
	(0.02)	(0.04)	(0.05)	(0.03)
N	5,223	5,223	5,223	5,223
Specification	Cubic	Cubic	Cubic	Cubic
Spline	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression.

A.8 RD Estimates Using Only In-State Donors to Scale Candidates

Table A.7 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022, Using Only In-State Contributions to Scale Candidates. This table replicates Table 1 after scaling candidates only on the basis of contributions from in-state donors.

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.06	-0.06	-0.06	-0.06
	(0.02)	(0.02)	(0.02)	(0.02)
N	2,608	4,952	3,022	4,952
Specification	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-
Bandwidth	.10	-	0.12	-

Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

A.9 RD Estimates Using Scalings that Adjust for Protest Voting

Table A.8 – Effect of Nominating the More-Extreme Primary-Election Candidate on their Party's General-Election Contribution Share in Congress, 1980-2022, and State Legislatures, 1996-2022, Using Fowler-Lewis Scores to Anchor U.S. House Incumbents' Positions. This table replicates Table 1, except that incumbents' roll-call voting records in the U.S. House are measured using scalings that account for protest voting from Fowler and Lewis (2024).

	Share of Total General Election Contributions			
	(1)	(2)	(3)	(4)
Extremist Primary Win	-0.05	-0.06	-0.06	-0.06
	(0.02)	(0.02)	(0.02)	(0.02)
N	2,662	5,170	2,996	5,170
Specification	Linear	Cubic	CCT	IW
Spline	Yes	Yes	-	-
Bandwidth	.10	-	0.11	-

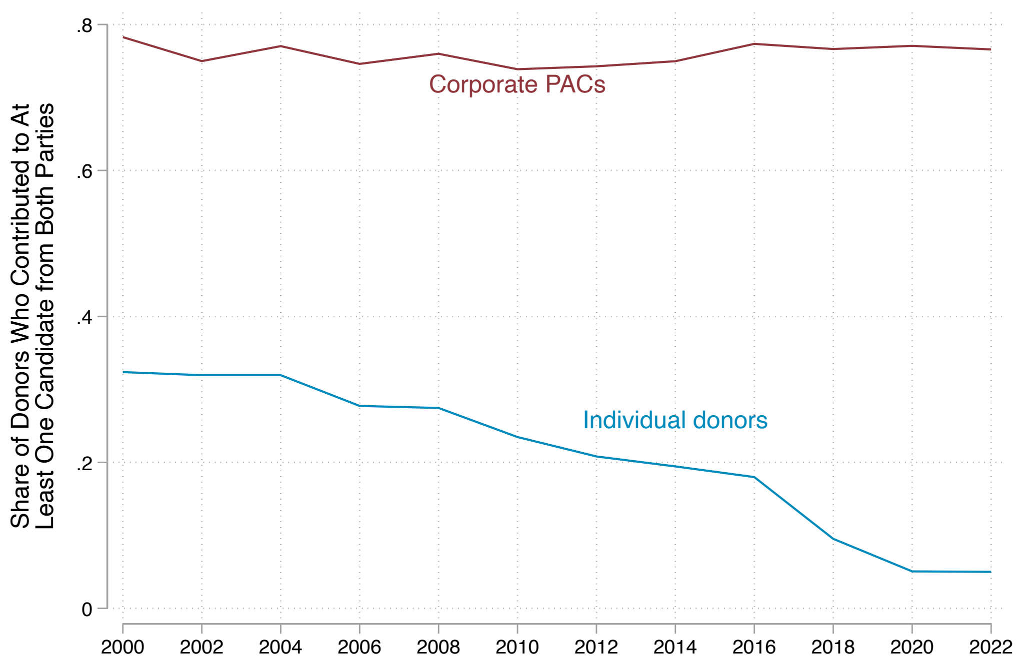
Note: Robust standard errors clustered by district are reported in parentheses. The running variable is the extremist candidate's win margin in the primary election. Spline indicates that the regression function was fit separately on either side of zero. Cubic refers to a third-order polynomial regression. CCT refers to the method from Calonico, Cattaneo, and Titiunik (2014). IW refers to the method from Imbens and Wager (2019).

A.10 Donor Partisanship Over Time

This section evaluates a potential competing explanation for the decline in the financial penalty to extremist nominees among corporate PACs. Instead of reallocating funds to increasingly-competitive extremist nominees, corporate PACs could have become increasingly partisan in their donating.

To probe this possibility, Figure A.10 plots the share of individual and corporate donors who contributed to at least one candidate from both parties, conditional on making at least five contributions in a given election cycle.[6] The red line in the figure shows that the share of corporate PAC that contribute to both parties has remained remarkably constant at 80% during the period of study. These results indicate that corporate PACs have not become more partisan in recent years. Individual contributors, in contrast, have become substantially more partisan. In 2000, roughly 35% of individual contributors donated to candidates of both parties, but that number has declined to 5% in 2022.

Figure A.10 – Share of Donors Who Contributed to At Least One Democratic and One Republican Candidate in Congress, 1980-2022, and State Legislatures, 2000-2022. This figure plots the share of individual and corporate donors who contributed to at least one candidate from both parties, conditional on making at least five contributions in a given election cycle.



[6]The results are highly similar across a variety of cutoffs.

A.11 Attenuation Bias Simulations

Another alternate explanation for the decline in the financial penalty to extremist nominees is attenuation bias. Specifically, a combination of an increase in measurement error and a decrease in within-party heterogeneity could cause the financial penalty to extremists to appear to decline when its true value remains constant.[7] This section evaluates this possibility using a simulation.

Let the true midpoint regression be expressed as

$$Y_{dt} = \beta_0 + \beta_1 Midpoint_{dt} + \varepsilon, \tag{6}$$

where Y_{dt} is the Democratic candidate's share of general-election contributions and $Midpoint_{dt}$ is the true midpoint between Democratic and Republican candidates.[8] Let the variance of $Midpoint_{dt}$ be σ_M^2, and assume that the observed midpoint is given by

$$\widehat{Midpoint}_{dt} = Midpoint_{dt} + \nu_{dt}, \tag{7}$$

where $\nu_{dt} \sim (0, \sigma_\nu^2)$. The observed financial penalty to extremists is estimated as

$$Y_{dt} = \tilde{\beta}_0 + \tilde{\beta}_1 \widehat{Midpoint}_{dt} + u_{dt}. \tag{8}$$

Then the classical error-in-variables model indicates that $\tilde{\beta}_1 = \lambda \beta_1$, where $\lambda = \frac{\sigma_M^2}{\sigma_M^2 + \sigma_\nu^2}$.

Clearly, either an increase in σ_ν^2 (measurement error) or a decrease in σ_M^2 (decreasing party heterogeneity) would increase attenuation bias. Fortunately, it is possible to estimate each of these quantities over time for candidates who are ultimately elected. To measure σ_ν^2, I calculate the variance of the difference between candidates' *Primary-Only Scaling* and their true roll-call voting score (i.e., DW-NOMINATE or NP-Score). To measure σ_M^2, I calculate the variance of legislators' DW-NOMINATE or NP-Score. Using these quantities, I calculate λ_t, the attenuation factor in year t.

Finally, I simulate how much of the decline in the financial penalty to extremists documented in Figure 8 that over-time change in λ_t would explain. Specifically, I fix the baseline value of financial penalty to extremist nominees at its observed value in 2000, and then estimate the counterfactual penalty as the product of the baseline value and λ_t. This quantity is plotted in red in Figure A.11.[9] Because I estimate that the penalty has declined slightly more among corporate PACs than individual donors, I conservatively focus on corporate PACs in Figure A.11. The black line in the figure plots the observed penalty to extremist primary nominees.

As is apparent in Figure A.11, attenuation bias does not explain a meaningful proportion of the decline in the financial penalty to extremists. In this counterfactual scenario, the estimated penalty to extremists remains roughly constant and is far from the observed decline

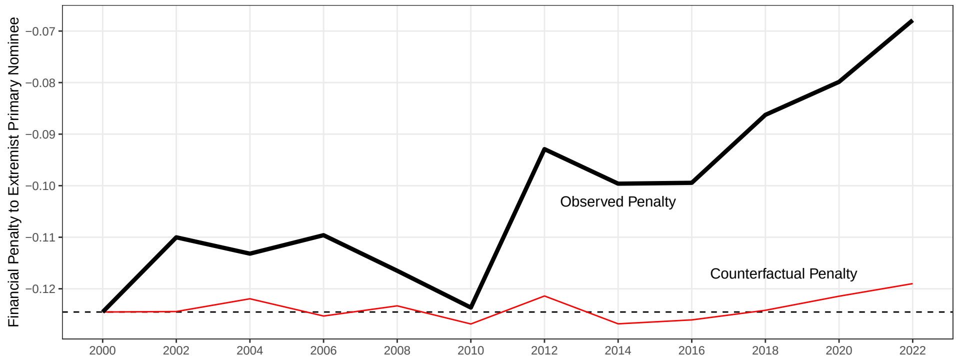
[7]Note, however, that a decrease in within-party ideological heterogeneity absent measurement error would not bias my estimates.

[8]For simplicity, I omit γ_i, the distinct's Democratic presidential vote share. Results are highly similar when including this variable, because measurement error is not correlated with the district's underlying ideological composition.

[9]As in the main text, I transform the midpoint coefficients to match the RD scale.

plotted with the black line. In fact, it'd require a set of λ_t that are nearly ten times the observed λ_t to fully explain away the observed decline in the financial penalty to extremist nominees.

Figure A.11 – Counterfactual Financial Penalty to Extremist Primary Nominees. This figure plots the counterfactual financial penalty to extremist primary nominees where the baseline penalty is fixed at its original value in 2000 and changes are caused by shifts in the signal-to-noise ratio, λ_t.



A.12 Roll-Call Classification Exercise

To further validate my preferred ideological scaling, this section uses candidates' *Primary-Specific Scaling* to predict the outcome of observed roll-call votes in Congress and state legislatures.

Data on roll-call votes in Congress was downloaded from Voteview (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.[10] 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.9 reports the total number roll-call votes in this dataset by level and year.

To evaluate the predictive ability of my ideological scaling and other measures of candidate ideology, I calculate the optimal cutting point between "yea" and "nay" votes following Poole (2007). 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. Leveraging these cutpoints, I impute predicted roll-call votes and compare the result to the true votes cast.

Table A.10 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).[11] 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.

[10] I include the unbalanced panel of states from 2000-2009 in my main analyses to evaluate the predictive capacity of my scalings over an extended time frame. The results in Table A.10 are very similar if I instead focus on the years for which I have a balanced panel.

[11] $\text{APRE}_i = \frac{\sum_{j=1}^{J}\{\text{minority vote}_j - \text{classification errors}_{ij}\}}{\sum_{j=1}^{J}\text{minority votes}_j}$ 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.

Table A.9 – 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	128,433	4,490,115
2002	882,478	235,085	647,393				

Table A.10 – Percent of Congressional and State Legislative Roll-Call Votes Classified Correctly. The *Primary-specific scaling* predicts 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 records (DW-NOMINATE/NP-Scores).

Scaling	Overall	Congress	State Legislatures
DW-NOMINATE/NP-Scores	0.911	0.904	0.910
	(0.759)	(0.764)	(0.751)
Primary-Specific Scaling	0.895	0.895	0.897
	(0.716)	(0.720)	(0.713)
Static CFscore	0.886	0.891	0.882
	(0.696)	(0.734)	(0.658)
Party	0.857	0.845	0.850
	(0.587)	(0.500)	(0.584)

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