

What Happens When Extremists Win Primaries?

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This article studies the interplay of U.S. primary and general elections. I examine how the nomination of an extremist changes general-election outcomes and legislative behavior in the U.S. House, 1980–2010, using a regression discontinuity design in primary elections. When an extremist—as measured by primary-election campaign receipt patterns—wins a “coin-flip” election over a more moderate candidate, the party’s general-election vote share decreases on average by approximately 9–13 percentage points, and the probability that the party wins the seat decreases by 35–54 percentage points. This electoral penalty is so large that nominating the more extreme primary candidate causes the district’s subsequent roll-call representation to reverse, on average, becoming more liberal when an extreme Republican is nominated and more conservative when an extreme Democrat is nominated. Overall, the findings show how general-election voters act as a moderating filter in response to primary nominations.

...getting a general-election candidate who can win is the only thing we care about.¹

—Rob Collins, National Republican Senatorial Committee

The road to hell is paved with electable candidates.²

—Joseph Ashby, conservative blogger

INTRODUCTION

With the rise of the Tea Party and the phenomenon of moderate incumbents “getting primaried,” political scientists and journalists alike have placed added scrutiny on the role of primary elections in our system of representation.³ As the first stage of candidate selection, primaries play an important role in choosing the people who will

go on to represent voters in Congress. Primary voters exhibit a marked preference for more ideologically extreme candidates (Brady, Han, and Pope 2007; Hall and Snyder 2013), but general-election voters appear to prefer moderates (Ansolabehere, Snyder, and Stewart 2001; Burden 2004; Canes-Wrone, Brady, and Cogan 2002; Erikson and Wright 2000). As a stylized fact, primary voters who prefer extreme candidates are thus thought to face a tradeoff between voting for a candidate closer to their views, but less likely to win office, and a candidate farther from their views but perhaps more “electable.” In this article, I study this tradeoff and its consequences for elections and representation in Congress.

How much does the party’s electoral outlook suffer in a district where its primary voters nominate a more extreme candidate, relative to the counterfactual in which the same district nominates a more moderate candidate? If a district nominates a more extreme candidate, how much does the district’s roll-call voting in the next Congress change relative to this same counterfactual—taking into account both the manner in which the extremist would vote and the probability that the extremist wins office? To answer these questions, I combine a scaling technique for estimating candidate positions based on campaign contributions with a regression discontinuity design in U.S. House primary elections, 1980–2010. This strategy allows me to obtain direct counterfactual comparisons between districts with an extreme or more moderate nominee without using assumptions to place districts and candidates on a single ideological scale and without asserting the exogeneity of differences in candidate positions.

I find that the “as-if” random nomination of the extremist candidate causes a substantial decrease in the party’s vote share and probability of victory in the general election. These decreases are large enough to offset the more extreme roll-call voting that extremist candidates offer to primary voters, on average. They are also large enough to offset any other roll-call effects nominating extremists might have, e.g., inducing incumbents to strategically adopt positions like those of extremists. Indeed, the nomination of the more extreme candidate to the general election produces a *reversal* in observed roll-call voting for the district in the

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¹ http://firstread.nbcnews.com/_news/2013/11/05/21321519-in-shift-gop-vows-to-fight-for-more-electable-candidates-in-senate-primaries?lite

² http://www.americanthinker.com/2011/09/the_road_to_hell_is_paved_with_electable_candidates.html

³ For example, a growing literature studies the link (or nonlink) between primary-election type (open vs. closed, etc.) and polarization (Gerber and Morton 1998; Hirano et al. 2010; McGehee et al. 2014; Rogowski 2013). For an overview of “getting primaried” see Boatright (2013).

next Congress, on average; that is to say, when a more extreme Democrat is nominated, the district's roll-call voting in the next Congress becomes more conservative, and vice versa when a more extreme Republican is nominated. In districts that are safe for the party, this reversal disappears. Because even extreme nominees are more likely to win office anyway in safe districts, the effect on downstream roll-call behavior washes out. The tradeoff voters face is therefore *variable*: in competitive districts they ought to support more moderate primary candidates if they care about winning the office, but in safer districts they have more slack to support extremists.⁴

Overall, however, the nomination of more extreme candidates causes severe damage to the party's electoral prospects. The nomination of an extremist today makes the party much more likely to lose the general election today, and because of the incumbency advantage, the opposing party is much more likely to win the election again two years later, and four years later, and on. As I show, the nomination of the extremist continues to cause an equally large electoral penalty as far as eight years down the line—the farthest increases in voter information because of redistricting. The decisions that primary voters make in the current election cycle echo many years later.

The article is organized as follows. In the next section, I discuss the theoretical perspectives and expectations motivating the research. Following that, in the third section I provide an overview of the data and empirical strategy used to analyze primary and general elections in the U.S. House, 1980–2010. In the fourth section, I analyze general-election outcomes. In the fifth section, I examine the effect as I sharpen the contrast between extreme and more moderate primary candidates. In the sixth section, I analyze the overall effects on legislative behavior and explore the varying tradeoff that primary voters face between “electability” and ideology. In the seventh section, I study the long-term effects of extreme nominees, showing how the electoral penalty (and its accompanying roll-call effect) persists even four terms—eight years—later. In the eighth section, I briefly consider possible causal mechanisms. Finally, I conclude by discussing the implications of the findings.

THEORETICAL PERSPECTIVES

Primaries are a key part of the American electoral system. By selecting especially partisan candidates, primaries may contribute to the large and growing po-

⁴ There may be many other reasons why voters choose to nominate extremists or moderates. For example, if they prefer extremists and they only care about electing a moderate if their representative is going to be pivotal in the legislature (e.g., Krehbiel 1998), then it could be rational for primary voters to nominate extremists almost all of the time since only one legislator, the median, will be pivotal. While interesting, understanding the precise motivations of primary voters is beyond the scope of the present study.

larization of U.S. legislatures.⁵ This growth has been especially marked in recent elections, including in those studied in this article, namely, U.S. House elections, 1980–2010. In primaries like these, as the epigraph hinted, parties and their voters must weigh issues of party brand and ideological “purity” against “electability”—the likelihood of a potential nominee winning the general election.⁶ To understand the possible consequences of primary-election results, we must first understand the electoral costs or gains of nominating more or less ideologically extreme candidates. Only then can we assess the potential tradeoffs at play during the primary election.

While primary candidates may be more or less extreme, we have reasons to believe that the candidates that enter primaries are already polarized. Within a primary, some candidates will be farther to the right or left than others, but even the right-most Democrat is likely to be quite left of the left-most Republican in the opposite primary (see for example Bafumi and Herron 2010). As a result I will refer throughout the article to “relative moderates” and “extremists”—candidates in a primary who are moderate or extreme *relative to their primary opponents*, but who likely lie all to one side of the district's median voter. Classifying primary candidates in this way clarifies our expectations about the consequences of nominating more or less extreme candidates. Even if general-election voters reward nominees who are moderate relative to their primary opponents, polarization may still be high if the pool of candidates as a whole is quite polarized.⁷ Thus in studying the effects of nominating extremists we will learn about one important potential mechanism through which primaries can affect polarization—through the selection of candidates to stand in the general election—but we must be aware that primaries can still affect polarization in other ways, e.g., through affecting the pool of candidates that enter primary elections in the first place.

Considering the overall consequences of nominating extremists is important for the reasons above, but so, too, is studying the way these consequences vary across districts. We might suspect, for example, that any reaction of general-election voters will be muted in especially partisan districts. In these “safe” districts, general-election voters may be likely to elect their party's nominee regardless of her ideological position relative to her primary opponents. This is an important source of variation because many U.S. House districts are safe, by typical standards. In the 2008 presidential

⁵ For information on the size and growth of polarization, see for example McCarty, Poole, and Rosenthal (2006) as well as updated analyses at <http://www.voteview.com>.

⁶ A formal theoretical literature also studies issues related to this tradeoff (e.g., Aranson and Ordeshook 1972; Coleman 1971; Owen and Grofman 2006).

⁷ What is more, the institution of primary elections could play a role in fostering polarization, even if moderates enjoy a large general-election advantage, if they encourage incumbents to adopt strategic positions to satisfy their partisan bases in anticipation of future primary elections. However, Hirano et al. (2010) investigates the introduction of primaries in seven U.S. states and finds no evidence that they increase polarization.

election (the last such election occurring within the period of study), for example, almost exactly half of all Congressional districts had a Democratic presidential vote share above 0.6 or below 0.4. Primaries are likely to play a different role in these districts than in the other half of the districts which are more competitive. In addition to the overall on-average effect of extremist nominations—our single most important quantity of interest—we should therefore also investigate variation in the effect across safe and competitive districts in order to gain further understanding of where and when extremists are punished more or less in the general election.

The effect might vary, also, with the presence of an incumbent candidate. Incumbents possess a large electoral advantage (e.g., Erikson 1971; Gelman and King 1990) which may give them leeway to take different positions. If incumbents are more likely to be relatively moderate primary candidates, then any penalty to candidates labeled more “extreme” might actually be driven by the removal of the incumbent when the extremist wins nomination and not by any other characteristic of the more extreme candidate. This is one reason to investigate the effect in open-seat races, where neither primary has an incumbent running. A second reason is that most incumbents first enter office through open-seat races. Given the well-known persistence of incumbents once in office, open-seat primaries are especially important for selecting candidates.

Short-term electoral outcomes are not the only consequence of nominating extremists in primary elections, either. Even if an extremist performs poorly in the general election, her nomination might be valuable to her supporters in other ways. The podium that the general-election campaign offers might allow an extremist to trumpet her views and incite support for future election cycles, and the views she espouses might make their way into the legislature even if she, herself, does not. This could occur directly through increased demand for her supported policies or indirectly through the threat her candidacy signals to the incumbent anticipating future elections (see for example Sulkin 2005). For these reasons it is important to study not just the immediate electoral consequences of nominating more or less extreme candidates, but also the effects of nominating relatively extreme or moderate candidates on downstream roll-call voting.

In similar ways, extremist nominees might also affect downstream election results. On one side, advocates and others often point to the “galvanizing” effect of extremist nominations. Even if the extreme nominee loses in the general, the logic goes, she can succeed in energizing the party’s base, which can enhance the party’s electoral fortunes the next time around. After losing her Senate election, Tea-Party candidate Christine O’Donnell, for example, declared that “Our voices were heard and we’re not going to be quiet now.... This is just the beginning.”⁸

⁸ <http://www.csmonitor.com/USA/Politics/The-Vote/2010/1102/Lesson-from-Christine-O-Donnell-defeat-where-tea-party-overreached>

On the other side is the cold, hard logic of the incumbency advantage. Incumbents in the U.S. House enjoy enormously high reelection rates (e.g., Jacobson 2012), and the “as-if” random assignment of incumbency status conveys a roughly 40 percentage-point increase in the probability the party retains the seat in the next election cycle (Lee 2008). Once a party controls a seat, it is unlikely to give it up for a long time. Fowler and Hall (2013), for example, show that the election of a Democrat over a Republican in a coin-flip election today makes the district more likely to still be represented by a Democrat 16 years later. The decision to nominate the extremist in the current primary election thus might boost the opposing party’s downstream electoral outcomes if it makes that party more likely to gain incumbency status today.

Finally, we can learn more about the candidate selection process, and can speak to formal models of these processes, by studying the mechanisms underlying the effects of extremist nominations. The electoral effects of nominating extremists might result from their ideological positioning, as implied by spatial models of electoral processes (Downs 1957), or they might result from other differences between the two types of nominees. Probabilistic voting models often predict, for example, that candidates will strategically take more extreme positions when they are disadvantaged on a separate dimension of valence such as quality (e.g., Ansolabehere and Snyder 2000; Aragonés and Palfrey 2002; Groseclose 2001). Models of these forms also suggest comparative statics in which increases in the information about candidate positions lead to larger disadvantages for extreme positions, suggesting another dimension of variation in the response to extremist nominees.

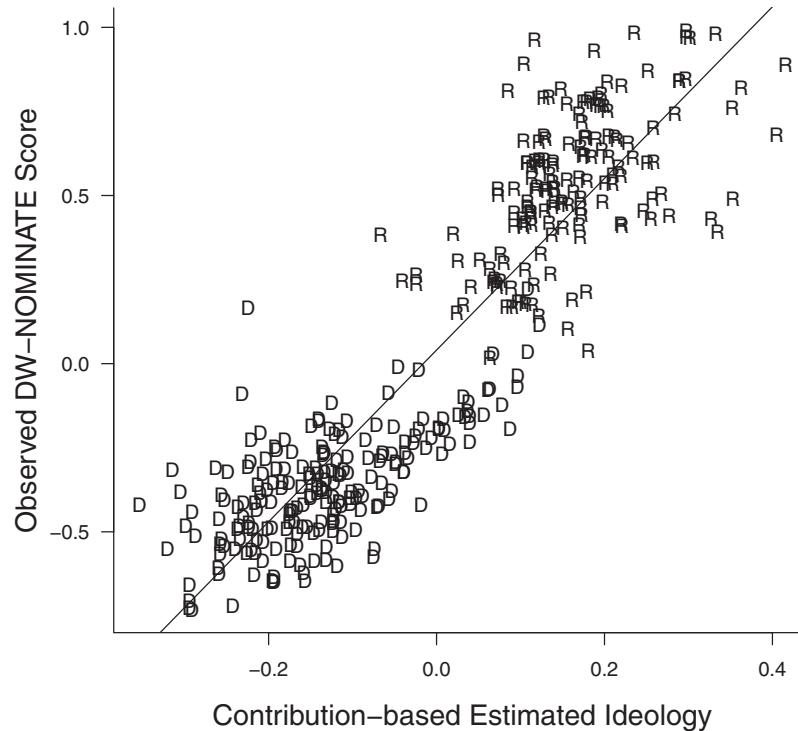
In this section, I have explained why considering the effects of extremist nominations is important and I have situated the study in the context of legislative polarization. I have also described how I operationalize “extremist” primary candidates—those who are ideologically extreme relative to their primary opponents—and offered some important theoretical sources of variation in the consequences of nominating these relatively extreme candidates. I have also laid out theoretical ideas for testable mechanisms underlying the response to extremist nominees. With these in mind, I now proceed to describe the details of the empirical approach and its results.

EMPIRICAL APPROACH

Primary-Election Campaign Contributions Predict Candidate Ideology

For information on classifying candidates as extremists, I rely on the estimated ideological positions of U.S. House primary candidates from Hall and Snyder (2013), which covers the years 1980–2010.⁹ Technical details on the method are available in

⁹ I use the scalings that employ a cutoff of 10 on the number of unique contributions a donor must make in order to enter the scaling procedure.

FIGURE 1. Estimated Ideology of Primary Candidates and Observed Roll-Call Behavior

Notes: For primary candidates who go on to win the general election, the contribution-based estimate of their ideology accords well with how they vote on roll calls. Points labeled “D” represent Democratic incumbents; those labeled “R” are for Republican incumbents.

Appendix B. Candidates are scaled on the basis of their primary-election campaign receipts,¹⁰ imputing each candidate’s ideological position from the contribution-weighted average estimated positions of her donors.¹¹ The donors’ positions are estimated as the contribution-weighted average DW-NOMINATE score (Poole and Rosenthal 1985) of incumbents they have donated to, but excluding donations to candidate *i* when computing the score for each candidate *i*. The technique is similar to those employed in, for example, Bonica (2013) and McCarty, Poole, and Rosenthal (2006). I choose this technique over other options only because it produces scalings that are specific to the primary election. Using general-election contributions—though statistically efficient for many other purposes—would introduce post-treatment bias in the present setting since the “treatment,” the nomination of the extremist, occurs prior to the start of the general-election campaign.

To validate this scaling technique in the sample used for analysis, Figure 1 compares it to observed DW-NOMINATE scores for primary candidates who go on to win the general election. The donor-based scaling of

candidates correlates with observed DW-NOMINATE scores at 0.90.¹² This is consistent with a fuller battery of validation tests presented in Hall and Snyder (2013). As a result, there is good reason to believe that the estimated primary candidate positions are reflective of their actual ideological positioning. Importantly, any random error in these estimated positions biases the subsequent analysis against finding differences in outcomes for more and less extreme candidates.¹³

In primary races with two major candidates, the race is tentatively identified as being between an extremist and a relatively moderate candidate if the difference

¹² The within-party correlations are 0.66 for Democrats and 0.56 for Republicans. These within-party correlations are very similar to those in Bonica (2013), which reports within-party correlations with DW-NOMINATE of 0.66 for Democrats and 0.64 for Republicans, analyzing only sitting incumbents. The within correlation for Republicans is slightly lower in the current sample, probably because the comparison includes some challengers who go on to be incumbents later. Bonica (2013) reports a within-party correlation of 0.49 for these Republican candidates.

¹³ Nonrandom error from strategic donating, on the other hand, would be a serious problem for resulting estimates. Fortunately there are several reasons to suspect this is not an issue. First, scalings from contributions are robust to choices over the types of donors included in the sample (Bonica 2014), and the scalings are highly predictive of actual roll-call voting. In the Appendix, I re-estimate the main results using a completely separate measure of candidate ideology that takes advantage of primary races between state legislators who have roll-call records from their time in office. Point estimates are highly similar using this alternate strategy.

¹⁰ Hall and Snyder (2013) removes all contributions classified for the general election, according to the FEC’s disclosure requirements, and also removes contributions received after the date of the primary election.

¹¹ In practice, weighting these donations by dollar size makes little difference in the scalings.

TABLE 1. Number of Primary Elections In Sample, By Type: U.S. House, 1980–2010

Incumbent Presence	Safe For Party	Competitive	Safe For Other Party	Total
No incumbents (open seat)	100	162	25	287
Open primary, incumbent in other party	15	61	3	79
Incumbent in primary, other primary open	55	61	3	119
Incumbents in both primaries	5	12	2	19
Total	175	296	33	504

Note: The majority of races occur in open seats either in competitive districts or districts that are safe for the party.

between their estimated ideological positions is at or above the median in the distribution of ideological distances between the top two candidates in all contested primary elections. This median distance translates to roughly one-third the distance on the DW-NOMINATE scale between the medians of the two parties in the 112th Congress, and it is approximately two to three times as large as the average distance between representatives and their own party's median.¹⁴ These are therefore races between candidates who offer meaningfully different platforms within the umbrella of their party.

Using a strong cutoff like this has two potential advantages. First, it may reduce the number of incorrect moderate/extreme labels caused by measurement error in the donor scores. Second, it ensures that we are focusing on strong comparisons in which the two primary candidates are starkly different.¹⁵ After presenting main results, I address the use of this cutoff, showing how the results are robust to alternate definitions and, moreover, that changes in the estimates as the cutoff is changed accord with theoretical predictions—namely, that the effect grows in magnitude as the cutoff becomes more extreme (as the contrast between the two candidates sharpens).¹⁶

Dataset Covers U.S. House Elections, 1980–2010

Data on U.S. House primary and general elections are compiled from primary sources by Ansolabehere et al.

¹⁴ These calculations are performed as follows. First, I take the median primary-election distance between candidates, which is roughly 0.109. To convert this number to the DW-NOMINATE scale, I plug it in as X in a regression predicting DW-NOMINATE based on the contribution-based scaling. I then compare this converted number (0.32) to the observed difference in party medians in the 112th Congress (1.072), and the observed mean differences between each representative and his or her party's median (0.14 for the Republicans and 0.1 for the Democrats).

¹⁵ Focusing on this subset of contested primaries makes the estimated results “local” to these kinds of races. It is possible that these races differ from other contested primaries. For example, greater ideological distance between the candidates may depress turnout in the primary (Rogowski 2014). However, these are still precisely the races we want to focus on to understand the differing consequences of choosing between a truly extreme candidate and a more moderate candidate.

¹⁶ In addition, an interactive specification which avoids the use of the cutoff produces substantively similar findings. See Appendix A, Table A.6.

(2010). I focus on elections in the years 1980–2010 to match the data on candidate positions. I keep all primary elections in which at least two candidates have donor scores.¹⁷ Among these elections, I analyze the two candidates with the top two vote totals, and I calculate each candidate's share of the top two vote totals.¹⁸ Full summary statistics are available in Appendix A in Table A.1.

What Kind of Elections Enter the Sample?

The analysis uses a special subset of all primary elections—namely, contested primaries with two “viable” candidates who raise enough money to allow for reliable ideological scaling. Investigating the characteristics of this sample is important for understanding and interpreting the results.

Table 1 breaks down the primary elections that enter the sample according to the presence of incumbents and the safety of the district for the party holding the primary. Districts are defined as “safe” if the party's share of the presidential two-party normal vote, calculated as the average presidential vote share for the redistricting period, is above 60%.¹⁹ As the first row shows, the majority of observations (287 of the 504 total) occur in primaries for races with *no* incumbents present, i.e., no incumbents in either party's primary. Of these 287 races, roughly 35% occur in districts that are safe for the party holding the primary, with the lion's share of the remainder occurring in “competitive” districts, those where the party's presidential normal vote is below 60% (or, equivalently, those where the Democratic presidential normal vote is between 40% and 60%).

Looking across the columns, we also see that the majority of primary races in the sample occur either in districts safe for the party (175 total) or in competitive districts (296). It is unusual for a primary election that occurs in a district safe for the other party to enter the sample. This is probably because these primaries are unlikely to yield viable candidates, and thus are unlikely to be competitive or to feature sufficient

¹⁷ There are 504 such elections in the dataset.

¹⁸ For the few cases of runoff primaries, I include the candidates and vote shares of the two-candidate runoff election.

¹⁹ I use the presidential vote share to avoid concerns of post-treatment bias. In practice, however, results are highly similar using the district's vote share for Congress to calculate the normal vote.

contribution behavior to scale candidates. In the Appendix, I compare these characteristics to those of the larger universe of all primary elections. The districts that enter the sample are no different in terms of their partisanship, but they are disproportionately open-seat races.

Estimation Strategy: Regression Discontinuity Design in Primary Elections

I estimate equations of the form

$$Y_{ipt} = \beta_0 + \beta_1 \text{Extremist Primary Win}_{ipt} + f(V_{ipt}) + \epsilon_{ipt}, \quad (1)$$

where *Extremist Primary Win_{it}* is an indicator variable for the extremist winning party *p*'s primary in district *i* at time *t*. Thus β_1 is the quantity of interest, the RDD estimator for causal effects from the “as-if” random assignment of an extremist in the general election.²⁰ The variable Y_{ipt} stands in for three main outcome variables: party vote share, party victory, and the DW-NOMINATE score of the winning general-election candidate in the ensuing Congress. The term $f(V_{ipt})$ represents a flexible function of the running variable, the extremist candidate's vote-share winning margin, i.e., the extremist candidate's share of the top two candidates' vote less 0.5, which determines treatment status. I present estimates using a variety of specifications for f as well as at different bandwidths, following the usual RDD practices (Imbens and Lemieux 2008). Typically, f either contains a high-order polynomial of the running variable or a local linear specification estimated separately on each side of the discontinuity.²¹

The key identifying assumption of the RDD is that potential outcomes are smooth across the discontinuity, i.e., that districts where the relatively moderate primary candidate barely wins (or, equivalently, those where the extremist candidate barely loses) are in the limit comparable to those in which the relative moderate barely loses (or, equivalently, those where the extremist candidate barely wins). Assumptions of “no sorting” like this have been challenged in the context of general-election U.S. House races (Caughey and Sekhon 2011;

Grimmer et al. 2012; Snyder 2005), but across numerous other electoral contexts are found to be highly plausible (Eggers et al. N.d.).

For primary elections, this assumption is extremely plausible; to sort, candidates would need to have precise information about the expected outcome of primary elections, and would need to exert extra effort only after finding out that the election was going to be extremely close.²² Given the difficulty even general-election campaigns have in predicting votes (Enos and Hersh N.d.), and the typical “running scared” mentality that leads candidates to pull out all the stops (King 1997), this seems unlikely. In addition, I validate the assumption in the Appendix by presenting balance tests using the same samples and specifications as the main results in the article. No evidence of sorting is found.²³

In addition to testing for sorting, it is also important to show that the RDD estimate is not sensitive to choices over the size of the bandwidth and the functional form of the running variable's specification. In the Appendix, I replicate the main analysis on general-election outcomes at a large variety of bandwidths and specifications and show that the resulting estimates consistently produce the same conclusion.

One final aspect of the estimation strategy deserves mention. Like any comparison across candidate types, the RDD I employ measures the total effect of assigning the extremist vs. the relatively moderate candidate to the general election. This includes the component of the overall effect that comes from the change in ideology, but also includes any other factors that differ between the two types of candidates.²⁴ To understand the consequences of primary voters' decision to nominate one kind of candidate or the other, though, we want to include *all* of the differences between the two types of candidates, not only their ideological differences. For the present analysis, then, this feature of the estimation strategy is suitable. However, it is also valuable to understand *why* the observed effects occur. To make progress in this direction, I examine heterogeneity in the effect across contexts as well as characteristics of the more moderate and more extreme bare-winners after presenting the main results.

²² Alternatively, barely winning extremists might have some ability to alter vote totals after the election in a way that barely losing extremists cannot. This seems even less likely.

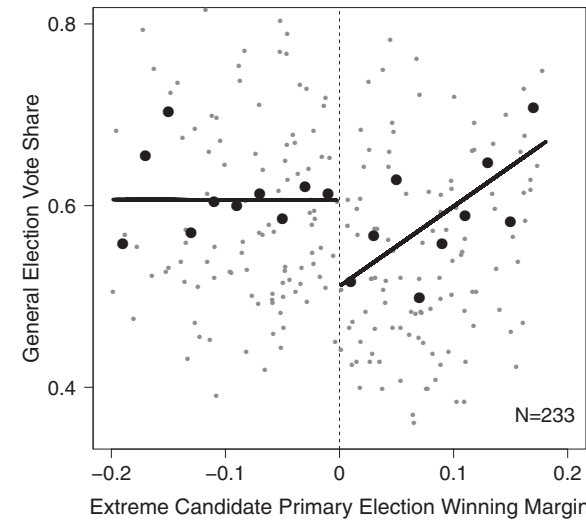
²³ Of 24 balance tests run, only 1 (4%) reject the null of no difference. This single rejection is for one specification of lagged electoral victory, but does not persist across specifications for this variable. See Appendix A for more details.

²⁴ The estimate thus includes any dynamics occurring after nomination, including any changes in the nominee's platform or that of the opposing party's candidate. Because of the “as-if” randomization, however, any such dynamics are consequences of the treatment, and thus mechanisms, rather than sources of bias for the estimates. It is not necessary to assume anything about whether candidates change positions from the primary to the general in order for the RDD estimates to be unbiased, but considering such factors is important for explaining the results. Given the well-known failure of the median voter theorem in House campaigns (e.g., Ansolabehere, Snyder, and Stewart 2001), it seems likely that the results are driven more by voters responding to fixed characteristics of the candidates than to differential changes in general-election positioning by extremist vs. moderate nominees.

²⁰ The use of the RDD to obtain exogenous variation in candidate types rather than party (Democrat vs. Republican) is similar in spirit to several previous studies. Broockman (2009) uses such a design to study the effects of copartisan incumbency in Congress on presidential elections returns. Anastasopoulos (2014), Brolo and Troiano (2012), Broockman (2014), and Ferreira and Gyourko (2014) examine the effects of the “as-if” random assignment of female vs. male candidates to office.

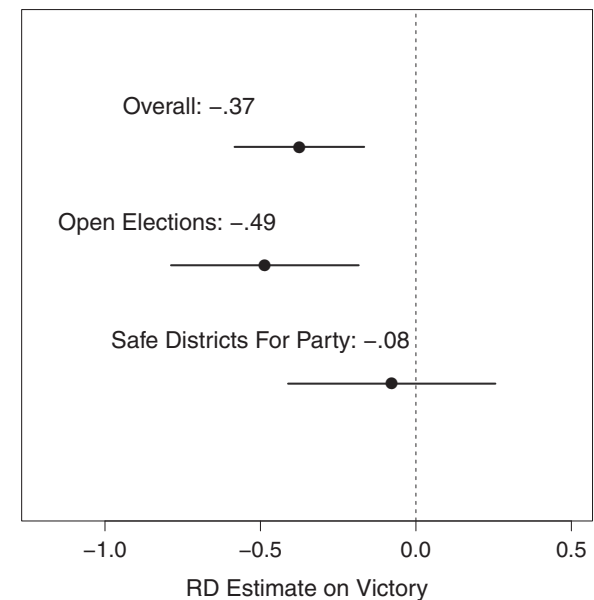
²¹ In the current setting, the treatment indicator pools over a variety of intensities of treatment because some extremists are farther from their moderate opponents (or vice versa), while other races feature relatively close-by candidates. To ensure this does not affect estimates, I also run models controlling for $dist_{it} \equiv |Moderate\ Donor\ Score_{it} - Extremist\ Donor\ Score_{it}|$ and interacting this with the treatment indicator. See Table A.6 in Appendix A.

FIGURE 2. General-Election Vote Share After Close Primary Elections Between Moderates and Extremists: U.S. House, 1980–2010



Notes: The close election of the more extreme primary candidate causes a decrease in general-election vote share for the party. Large black points are averages in 0.02 point bins of the relatively extreme candidate’s winning margin; small gray points are raw data. Lines are OLS fits from raw data estimated separately on each side of the threshold. Average general-election vote shares are above 0.5 on both sides of the discontinuity because contested primaries are more likely to occur in districts where the normal vote is tilted towards the party.

FIGURE 3. Effects of Nominating the Extremist Candidate on General Election Victory Across Primary Types



Notes: The penalty to extremists is largest in primaries for open-seat general election races, and close to zero in primaries for districts that are safe for the party. Estimates are calculated according to Equation 1, using the full data and a third-order polynomial of the running variable. Horizontal lines are 95% confidence intervals from robust standard errors.

RESULTS: EXTREMIST NOMINEES PERFORM WORSE IN THE GENERAL ELECTION

Figure 2 plots the discontinuity in the data. As can be seen, when the extremist goes from barely losing the primary to barely winning it (horizontal axis), the party’s general-election vote share decreases noticeably. Among “coin-flip” primary elections between a relative moderate and an extremist, the nomination of the extremist appears to cause a large decrease in the party’s general-election vote share.

Table 2 presents the estimates from Equation 1 using general-election vote share and victory as the outcome variables, with three specifications for each. In the first and fourth columns, I use a 5% bandwidth and a local-linear specification of the forcing variable, estimated separately on each side of the discontinuity. In the second and fifth columns, I use all the data and include a cubic specification of the running variable. In the third and sixth columns, I employ the “optimal bandwidth” procedure from Imbens and Kalyanaraman (2012).²⁵

²⁵ Because sample sizes are relatively small, especially at small bandwidths, I report the maximum of robust and conventional standard errors for the local linear and cubic specifications (Angrist and Pischke

As the table shows, the “as-if” random assignment of the extremist to the general causes approximately a 8–12 percentage-point decrease in the party’s share of the general-election vote, and a 35–53 percentage-point decrease in its probability of victory. These are large effects.

These estimates average over the different types of primaries, including those for open-seat races as well as incumbent-held seats and districts that are safe, competitive, or unsafe for the party holding the primary. As discussed in the “Theoretical Perspectives” section, investigating the variance in the effect across these district types is important for interpreting it. In Figure 3, I plot RDD estimates of the effect of nominating the extremist on the probability the party wins the general election, estimated separately for three relevant sets of elections. For comparison, the first estimate, labeled “Overall,” corresponds to the overall estimate from the fifth column in Table 2.

The second estimate uses only open elections, i.e., elections with no incumbent present in either primary. For these elections the effect is approximately 12 percentage points larger in magnitude than the overall

(2009). I report the standard errors from Imbens and Kalyanaraman (2012), as reported by the procedure, for the optimal bandwidth specifications.

TABLE 2. RDD Estimates of the Effect of Nominating an Extreme Candidate on General Election Vote Share, U.S. House 1980–2010

	Vote Share General Election	Vote Share General Election	Vote Share General Election	Victory General Election	Victory General Election	Victory General Election
Extremist win	– 0.12 (0.06)	– 0.10 (0.03)	– 0.08 [0.04]	– 0.53 (0.22)	– 0.37 (0.11)	– 0.35 [0.17]
<i>N</i>	83	252	135	83	252	148
RDD bandwidth	5	–	8.51	5	–	9.68
Specification	Local linear	Cubic	IK	Local linear	Cubic	IK

Notes: Maximum of robust and conventional standard errors in parentheses. Columns 3 and 6 use optimal bandwidth technique from Imbens and Kalyanaraman, implemented using `rddob` in Stata. Standard errors from this procedure in brackets.

estimate.²⁶ Open-seat races appear to be doing most of the work in driving the observed penalty to extremists, either directly because these races feature no incumbents or because of other factors that differ across districts that tend to have open-seat races and those that do not. Whatever the reason, this large effect shows that incumbents, or the loss of the incumbency advantage when relatively moderate incumbents lose to more extreme candidates, cannot explain the findings.²⁷

Finally, the third estimate uses only primaries that occur in districts that are safe for the party holding the primary, i.e., districts where the presidential normal vote is at or above 60% for the party. Here we find almost no penalty to nominating the extremist, perhaps because the partisan voters in the district support whomever the party nominates.²⁸ As mentioned in the theoretical section, a large number of districts are safe for one party or the other in U.S. House elections. The tradeoff between “electable” and “ideologically pure” candidates is quite different in these districts, as this heterogeneity in the effect shows.

This section has established the electoral penalty that the party faces when it is “as-if” randomly assigned an extremist candidate for the general election. This penalty is large, probability of victory and it is especially driven by the reactions of general-election voters in open-seat races. The penalty to extremists largely dissipates in districts that are safe for the party.

²⁶ The size of the effect is unlikely to be driven by the increased competitiveness of districts that are likely to have open-seat races. In fact, there is almost no correlation between the presence of an open-seat race and the competitiveness of the district. As a result, the estimated effect in open-seat races is equally large when a control for district safety is included.

²⁷ One possible mechanism related to incumbents, for example, would be if moderates in competitive primaries were more likely to be incumbents than were extremists. Then the observed overall effect could be driven by extremist nominations also being those where the incumbency advantage is not present. As the open elections result shows, this is unlikely to be the case. The penalty is especially large even when neither the moderate nor the extremist is an incumbent. In a related vein, Table A.3 presents estimates in incumbent-contested primaries and also in open primaries taking place in districts where the other party has an incumbent-contested primary.

²⁸ The null hypothesis that the effect is the same in safe districts as in all districts is strongly rejected ($t = 2.74$, $p = 0.007$; robust SEs). The result is equally large when a control for open-seat races is included in the regression.

INCREASING EFFECT SIZE ACROSS CANDIDATE EXTREMISM

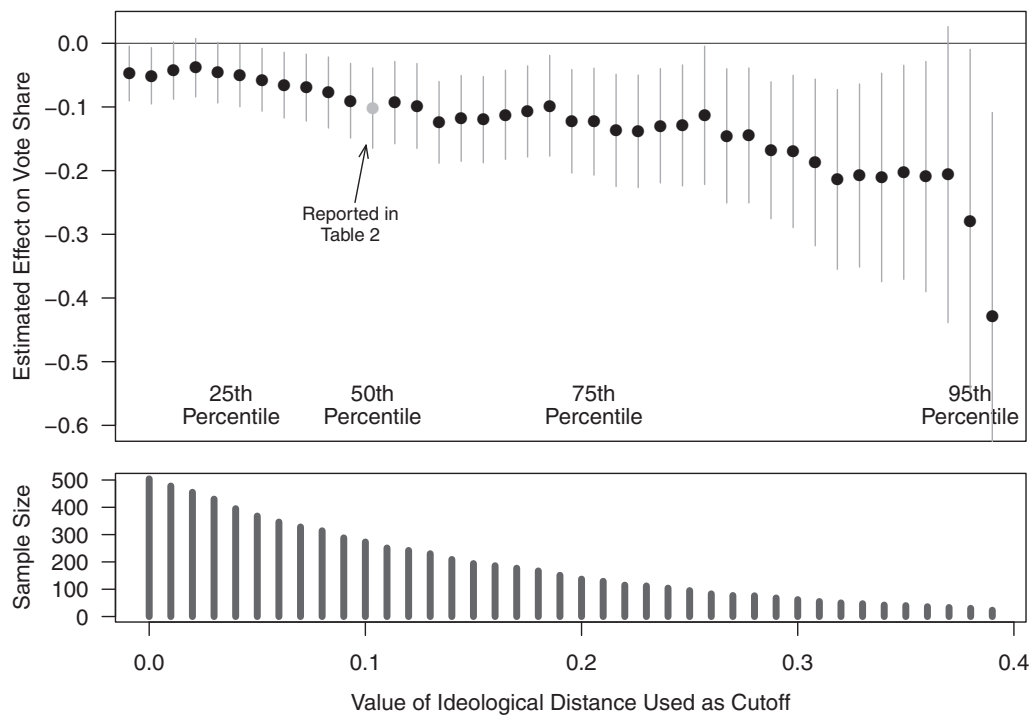
The results presented thus far average over treatments of varying intensity because the ideological distance between the relatively moderate and extreme candidate in contested primaries is not constant across races. To ensure that I only include true contrasts between an extremist and a more moderate candidate, the analysis above employed the median ideological distance between primary candidates as the cutoff for inclusion in the sample. By examining the effect across possible sizes of this cutoff, we can test the theoretical expectation that the electoral penalty should increase as the distance gets larger, and we can verify the robustness of the findings.

There are two methodological reasons to prefer larger cutoffs. First, too small of a cutoff might increase the probability of misclassifications due to measurement error in the donor scores. Measurement error in these scores means that, for candidates closer together in ideology, there is a greater probability that by random chance the more extreme candidate appears to be more moderate than the relatively moderate candidate. This type of error would lead to measurement error in the treatment variable—by identifying a winning candidate as an extremist and thus incorrectly calling the district “treated,” or, likewise, by calling her more moderate and incorrectly calling the district a “control” district—and thus would lead to attenuation bias in the estimates. At larger cutoffs this concern may be mitigated because two very far apart candidates are less likely to be misclassified in this manner.

Second, smaller cutoffs also bring into the sample more elections in which the contrast between the “moderate” and the “extremist” is less than sharp. Given the low information of voters, there is surely a point at which the distance between the candidates ceases to matter, and so the distinction between “treated” districts and “control” districts among these primaries becomes moot. Again, at larger cutoffs this concern dissipates.

Despite these advantages, there is also a cost to this cutoff approach. Although the treatment is “as-if” randomly assigned in the RDD, the *intensity* or “dose” of the treatment is not. Cutting the sample based on

FIGURE 4. Estimate of Effect on General-Election Vote Share Across Possible Cutoffs



Notes: Top panel plots the estimate from Equation 1 across different possible values of the ideological distance cutoff, i.e., the minimum estimated ideological distance between two primary candidates required to define one as “moderate” and the other as “extreme.” As we sharpen the contrast between the two primary candidates, the effect on general-election vote share continues to grow in magnitude. Bottom panel indicates sample size for each estimate. Vertical lines in top panel indicate 95% confidence intervals from robust standard errors. RDD estimates use cubic specification of running variable including whole sample.

intensity can provide a sense of how the effect varies with intensity, but it may also indicate underlying differences in the kinds of districts that have greater or smaller ideological distances between primary candidates. To address this problem, I have also estimated effects including all races in the sample, without employing a cutoff on ideological distance, and including a control for the estimated ideological distance between the candidates as well as an interaction of this distance with the treatment variable. The results, available in [Table A.6](#), are consistent with those presented in the article.

[Figures 4 and 5](#) present RDD estimates for the effect of nominating the extreme candidate over the more moderate candidate on the party’s general-election vote share and probability of winning across possible cutoffs. The horizontal axis represents the cutoff value used, and the 25th, 50th, 75th, and 95th percentiles of the distribution of ideological distances among closely contested primary elections are indicated towards the bottom of the plots. Vertical lines represent 95% confidence intervals from robust standard errors. The estimates reported in the previous sections are highlighted and indicated in the plot with arrows.

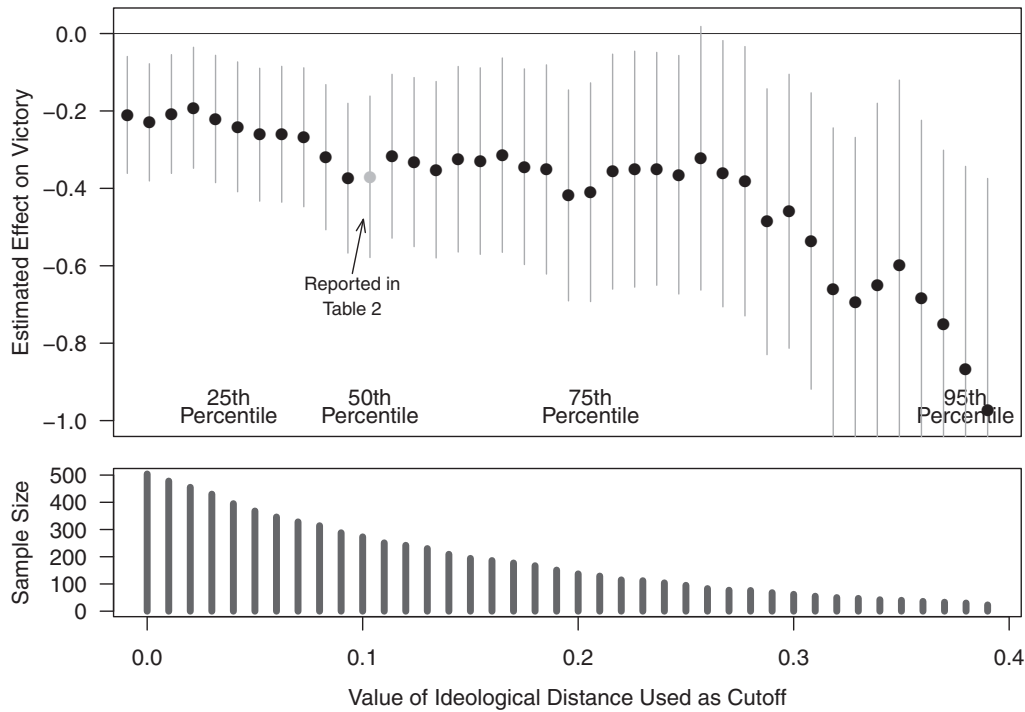
As the figures show, we find similar, negative estimates even when we include all comparisons between

extremists and relative moderates (when the cutoff is zero in the figures). As expected, the estimates grow in magnitude as the cutoff increases. In both cases, the estimated effect more than doubles if the 95th percentile is used as the cutoff—i.e., only using the most extreme cases—relative to the 50th percentile cutoff used in the previous section.²⁹

In contested primary elections, the nomination of an extreme candidate causes a large decrease in the party’s electoral prospects in the general election. This effect is present in many settings and especially large in cases in which the extreme candidate is especially ideologically extreme. Having established this on-average electoral penalty, we can examine its consequences for behavior inside the legislature.

²⁹ The graph does not reflect tests of the null hypothesis that the effect is the same at different values of the cutoff. [Table A.6](#) presents these tests, comparing the effect at the minimum cutoff to that at the maximum. While observed differences are always substantively large, we cannot reject the null that the effect is the same at the minimum and maximum for the vote share specifications and for one of the victory specifications. However, we can reject the null that there is no overall effect in all three victory specifications and in one of the vote share specifications.

FIGURE 5. Estimate of Effect on General-Election Victory Across Possible Cutoffs



Notes: Top panel plots the estimate from Equation 1 across different possible values of the ideological distance cutoff, i.e., the minimum estimated ideological distance between two primary candidates required to define one as “moderate” and the other as “extreme.” As we sharpen the contrast between the two primary candidates, the effect on general-election victory continues to grow in magnitude. Bottom panel indicates sample size for each estimate. Vertical lines in top panel indicate 95% confidence intervals from robust standard errors. RDD estimates use cubic specification of running variable including whole sample.

EFFECTS ON ROLL-CALL VOTING IN CONGRESS

In this section, I link the nomination of an extremist candidate to roll-call voting in the subsequent Congress. As the “Theoretical Perspectives” section discussed, extremist nominations could affect roll-call voting directly, by affecting which candidate and which party attain office, and indirectly, by altering the incentives of those in office.

Two opposing forces compose this first, “direct” effect. On the one hand, the nomination of an extreme candidate pulls subsequent roll-call voting away from the middle and towards the party’s ideological core—to the left when Democratic extremists are nominated and to the right when Republican extremists are—because the extremist is likely to vote more extremely if elected. On the other hand, as the previous section showed, the nomination of the extremist benefits the opponent party in the general, making roll-call voting more likely to shift in the other party’s direction—to the right when Democratic extremists are nominated, and to the left when Republican extremists are.

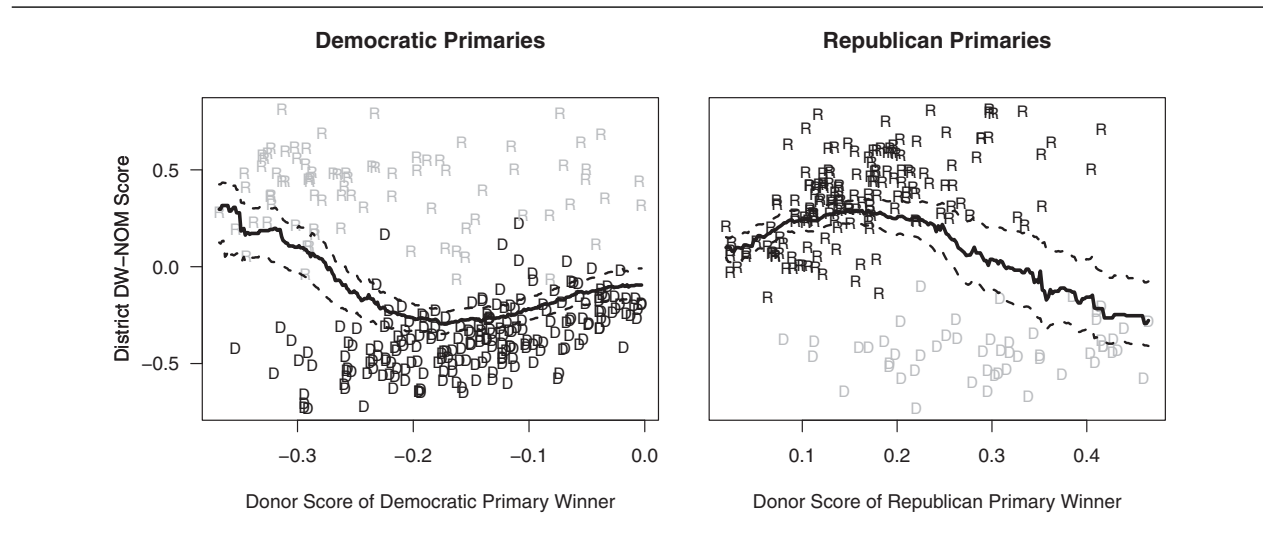
Figure 6 suggests this dynamic in Democratic and Republican primaries, respectively. The horizontal axis represents the estimated donor score of the winning primary-election candidate, and the vertical axis rep-

resents the DW-NOMINATE score of the winning general-election candidate in the ensuing Congress. Points are labeled “R” if the Republican candidate won the general election, and “D” if the Democrat did. Consider the left panel, representing Democratic primaries. In the elections with a relatively moderate Democratic primary winner—observations to the right of the plot—subsequent roll-call voting tends to be liberal (negative on the DW-NOMINATE scale). As we move to the left, i.e., as the winning Democratic primary candidate becomes more extreme, average roll-call voting first becomes more liberal, because the primary winner delivers more liberal roll-call voting. However, as the donor score becomes more extreme, Republicans begin to gain a more significant electoral advantage, and roll-call voting starts to become increasingly conservative.

The same pattern is present in the Republican primaries. Moderate primary winners—those to the left of the plot—tend to win and deliver moderate roll-call voting. As we move to the right, there is an increase in roll-call conservatism, but as we move farther right, this effect is washed out by the increasing electoral victories of Democrats in the general election.

As previewed in the “Theoretical Perspectives” section, the tradeoff between extremist and more moderate nominations may vary across districts. To explore

FIGURE 6. Nominee Ideology and Roll-Call Voting



Notes: Plots the estimated ideology of candidates who win contested primaries against the roll-call score the district receives from its representative in the subsequent Congress. Points are labeled “D” when the Democratic candidate wins the general election and “R” if the Republican wins. Black lines are from kernel smoother, with bootstrapped 95% confidence intervals in dashed gray lines. Extreme primary candidates (those to the left in the Democratic panel, and to the right in the Republican panel) win office so rarely that the district’s roll-call representation swings towards the other party.

TABLE 3. RDD Estimates of the Effect of Nominating an Extreme Candidate on District’s Roll-Call Representation, U.S. House 1980–2010

	Republican Districts			Democratic Districts		
	District DW-NOM	District DW-NOM	District DW-NOM	District DW-NOM	District DW-NOM	District DW-NOM
Extremist win	-0.69 (0.28)	-0.31 (0.16)	-0.56 (0.29)	0.25 (0.28)	0.11 (0.12)	0.12 (0.11)
Extremist win × safe for party	0.73 (0.34)	0.44 (0.18)	0.39 (0.29)	-0.09 (0.28)	-0.19 (0.11)	-0.17 (0.12)
Safe For Party	-0.01 (0.21)	-0.07 (0.14)	-0.01 (0.21)	-0.21 (0.15)	-0.22 (0.08)	-0.22 (0.08)
N	35	99	46	48	153	144
RDD bandwidth	5	—	6.49	5	—	21.73
Specification	Local linear	Cubic	IK local linear	Local linear	Cubic	IK local linear

Note: Maximum of robust and conventional standard errors in parentheses. Third and sixth columns select optimal bandwidth from Imbens-Kalyanaraman and apply with local-linear OLS.

this, as well as to add empirical rigor to the descriptive data presented above, I re-estimate the RDD with the district’s subsequent DW-NOMINATE score as the outcome variable. Overall, I find that the nomination of the extremist causes a reversal in roll-call voting, producing more conservative roll-call voting when Democrats nominate a more extreme candidate and more liberal roll-call voting when Republicans nominate a more extreme candidate (see Figure 8 in the next section). Here, I interact the treatment variable

with an indicator for whether the district is safe for the party holding the primary.

The first three columns of Table 3 investigate the effect in Republican districts. The coefficient on “Extremist Win” reflects the effect of nominating the extremist in districts that are not safe for the party (when “Safe For Party” is zero). Here we see that nominating the extremist produces a leftward shift in roll-call voting. Though the sample sizes make the estimate somewhat unstable, it is consistently negative and

substantively meaningful. In all three cases, moreover, we see that this roll-call reversal disappears in safe districts, where the electoral penalty to nominating the extremist is small.

Consider, for example, the second column, which includes all of the data. In districts that are not safe for the party, nominating the extremist is estimated to cause a -0.31 point decrease in the DW-NOMINATE score of the representative voting on behalf of the district in the next Congress. In safe districts, on the other hand, this effect is estimated to be *positive* 0.13 ($-0.31 + 0.44 = 0.13$), and we can reject the null hypothesis that the effect is the same across these two cases. The same pattern of evidence, though noisier, occurs in Democratic districts, as shown in the latter three columns of the table. In competitive districts, nominating the extremist appears to cause more conservative roll-call voting, but the effect moves close to zero in districts that are safe for the party.

The previous section demonstrated that there is a pronounced electoral penalty to nominating an extreme candidate over a more moderate candidate in contested primaries. It is difficult to evaluate the implications of this penalty for primary voting behavior without considering the overall effects nominating an extremist produces. This effect is in part a composite of the electoral penalty and the differential roll-call behavior of the extremist vs. the general-election candidate of the other party.

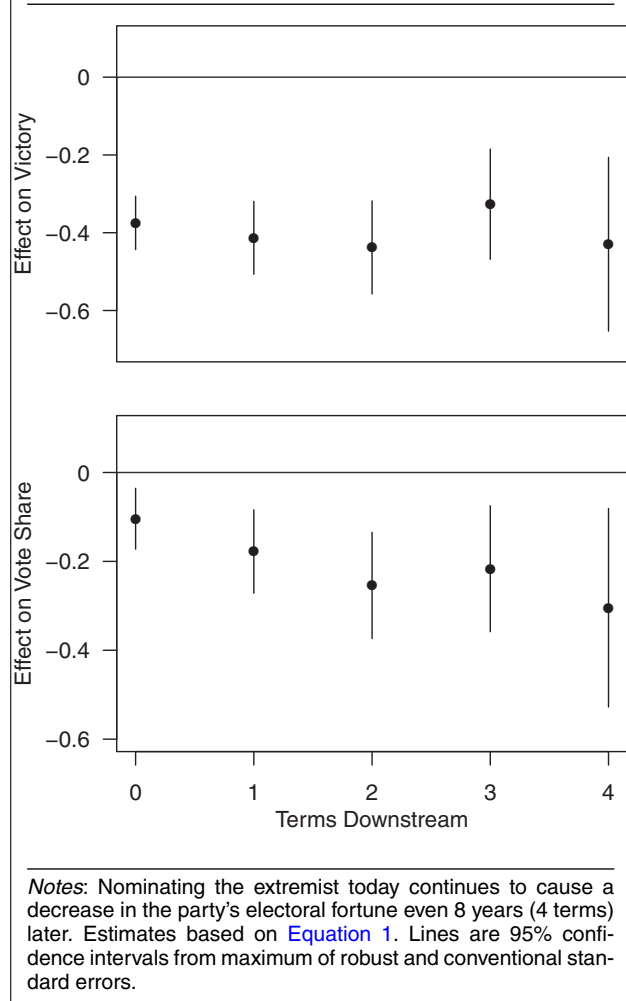
As it turns out, the implications vary depending on district conditions. In competitive districts, the nomination of an extremist may be a “mistake” from the point of view of primary voters who care about average roll-call representation in the legislature. In these districts, the likelihood that the other party will defeat the extreme nominee is too large, and the desired roll-call voting of the extremist nominee too unlikely to manifest itself in the legislature. But in safe districts, this may no longer be the case. In safe districts, the extremist wins enough of the time to make the observed roll-call effect close to zero.

It is more difficult to evaluate the possible “indirect” effects nominating the extremist has on roll-call voting, but the results place some bounds on them. That roll-call voting shifts in the *opposite* direction in competitive districts tells us that these indirect effects cannot be large relative to the change in roll-call voting caused by switching the party of the representative. It is still possible that the nomination of the extremist could cause incumbents in the district in the future to adopt more extreme positions, but any such effect must be small relative to the direct effects from nominating the extremist that flow through the changes in the probability the party wins the seat.

EXTREMIST NOMINEES AFFECT FUTURE ELECTIONS

On average, the nomination of an extremist has a pronounced negative effect on the short-term electoral fortunes of the party. What are the longer-term effects

FIGURE 7. Long-Term Effect of Nominating Extremist on Party's General-Election Victory and Vote Share



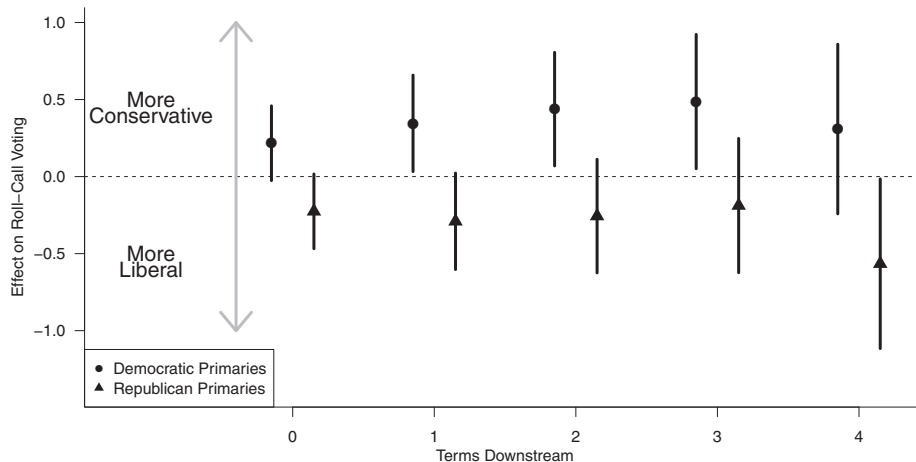
of this nomination? The “Theoretical Perspectives” section laid out arguments for either a “galvanizing” effect, in which nominating the extremist today might help the party tomorrow even if it loses today, or for a sustained penalty brought on by the likelihood of ceding incumbency status to the opposite party.

Consistent with this latter view, Figure 7 presents estimates from Equation 1 using the party's victory and vote share in subsequent electoral cycles as the dependent variable.³⁰ As the figure shows, the nomination of an extremist today continues to cause an equally large decrease in the party's expected probability of victory and vote share even four terms, or eight years, later—the farthest downstream that redistricting allows us to examine.

Figure 8 plots the same for the district's downstream DW-NOMINATE score, separately for Democratic and Republican primaries, pooling over safe and competitive districts. The overall reversal explored in the

³⁰ I use the third-order polynomial of the running variable with the full bandwidth; results are robust to other specifications.

FIGURE 8. Long-Term Effect of Nominating Extremist on District’s Roll-Call Representation



Notes: Nominating the extremist today continues to cause an ideological reversal in the district’s roll-call voting even eight years (four terms) later. The effect is present in both Republican primaries (where observed roll-call voting becomes more liberal) and in Democratic primaries (where observed roll-call voting becomes more conservative). Estimates based on Equation 1. Lines are 95% confidence intervals from maximum of robust and conventional standard errors.

previous section persists even four terms (eight years) later. In Democratic districts, roll-call voting becomes more conservative (increases) on average, and in Republican districts it becomes more liberal. Like in the previous section, this suggests that any indirect effects of extremist nominations on roll-call voting are small. Though extremists might be able to pull future roll-call voting towards their preferred positions without serving in office, any such effect is swamped in the long run (as well as the short run) by the direct effects of nominating the extremist and giving the office to the other party.

The decisions that primary voters make affect the current election. As I have shown, their choice influences the party’s vote share and probability of victory, as well as the style of representation they receive in Congress. But the decisions primary voters make today echo for at least the next decade, too.

DISCUSSION OF POSSIBLE MECHANISMS

How do we account for these effects? In this section I consider a variety of explanations and mechanisms, focusing both on the “bundle” of characteristics that extremists and moderates offer and on other features of the electoral environment. These explanations are not mutually exclusive, nor do they comprise the whole universe of possible explanations, but they do encompass some of the most salient possibilities.

Extremists Do Not Differ On Observable Valences

Perhaps the most obvious possible mechanism other than ideological positioning is that extremist nominees may possess some kind of valence disadvantage relative

to more moderate candidates, as discussed previously.³¹ Although measuring “valence” directly is impossible, we can assess whether bare-winner extremists appear to differ from bare-winner moderates in their previous office-holder experience, a measure often used as a proxy for unobserved quality and known to provide a valence advantage (Jacobson 1989, 2012).³²

The first point estimate in Figure 9 reports the RDD estimate like in Equation 1, but where the outcome is an indicator for whether the winning primary nominee has previous office-holder experience. This tests the hypothesis that part of the “treatment” of nominating the extremist candidate is receiving a nominee with more or less quality, in a particular sense, than the “control” of nominating the moderate candidate. Little difference is found. Using the full data and a third-order polynomial, extremists are estimated to be 9.9 percentage points *more* likely to have previous office-holder experience than more moderate candidates, and we cannot reject the null of no difference. Candidate quality, at least in the form of previous experience, does not appear to be driving the results.³³

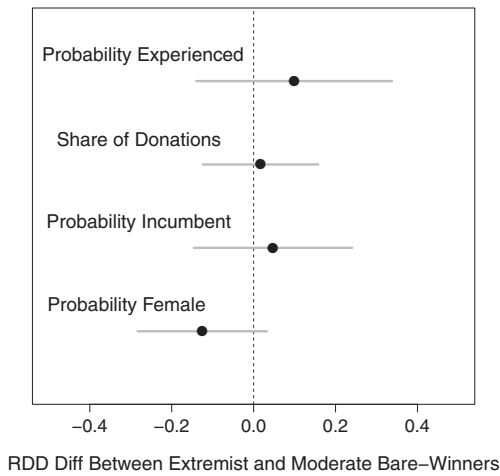
The second point estimate in Figure 9 shows, in the same vein, that extremist nominees do not receive

³¹ Keep in mind that, while the fact that the extremists and more moderate candidates are involved in “coin-flip” elections in the RDD, this does *not* mean that their quality is balanced. Anything that differs between the candidates, including quality, is part of the treatment. That said, the fact that their vote shares are so close suggests their quality is likely to be somewhat similar—otherwise, other factors would have to explain a large amount of vote share in order to counteract the quality differential and make the election so close.

³² Data on previous office-holder experience is from Jacobson (2012) as augmented by Hirano and Snyder (2014).

³³ Figure A.7 in Appendix A shows that this conclusion is robust across bandwidths.

FIGURE 9. Examining the Characteristics of Moderate and Extremist Bare-Winners



Notes: Plots the estimated difference between extremist bare-winners and moderate bare-winners in the probability of having previous office-holder experience, the share of primary donations, the probability of holding incumbency status, and the probability of being female. Moderate and extremist bare-winners do not appear to differ markedly in terms of previous office-holder experience, donations, or incumbency status, but extremist bare-winners are roughly 13 percentage points less likely than moderate bare-winners to be female. Estimates are calculated according to Equation 1, using the full data and a third-order polynomial of the running variable. Horizontal lines are 95% confidence intervals from robust standard errors.

fewer primary contributions than do more moderate nominees—another indication that the two types of nominees do not differ in their underlying quality.³⁴ The third point estimate tests the hypothesis that extremist bare-winners might be less likely to be incumbents, another form of valence advantage. Paralleling the candidate quality and contribution estimates, we see in the figure that extremist bare-winners are, if anything, *more* likely to be incumbents, but the effect is small (4.7 percentage points) and we cannot reject the null of no difference.

Possible Demographic Differences of Extremists and Moderates

The final point estimate shows that extremist bare-winners appear roughly 13 percentage points less likely to be female than moderate bare-winners.³⁵ While interesting, this difference is unlikely to drive the effect.

³⁴ The plotted estimate uses the share of primary contributions that flow to the winning nominee. The result is the same in levels. In such a specification, extremists at the discontinuity are estimated to receive roughly \$20,000 more than moderates, and we cannot reject the null of no difference between the two.

³⁵ I apply the statistical technique from Ferreira and Gyourko (2014) to estimate candidate gender. I calculate the probability that a candidate is female given her first name by using the U.S. census data on the distribution of first names and gender. I code a candidate as female if the probability the first name is a female name is above 0.99,

TABLE 4. Testing Mechanisms, U.S. House 1980–2010

	Share \$ From PACs	General Election Victory
Extremist win	-0.30 (0.11)	-0.13 (0.20)
Congruence × extremist win		-0.39 (0.28)
Congruence		-0.09 (0.16)
<i>N</i>	75	114
RDD bandwidth Specification	$h^{opt} \approx 7.26$ Local linear	$h^{opt} \approx 10.47$ Local linear

Notes: The first column shows that extremist nominees receive fewer contributions from PACs in the general election. In the second column, we see that the penalty to extremist candidates is larger in districts with greater media congruence and thus more information about the nominee. Robust standard errors in parentheses. Optimal bandwidth from Imbens-Kalyanaraman implemented using `rddob` in Stata, estimated using local linear OLS.

Evidence on the gender bias of U.S. House elections is mixed, but what evidence there is suggests that any bias would hurt female candidates rather than help them.³⁶ This would benefit the extremist bare-winners since they are less likely to be women. That being said, this difference may prove worthy of further investigation in future research. It is likely, based on this test, that extremist bare-winners differ from moderate bare-winners across other salient demographic dimensions as well.

Interest Groups Prefer Moderates

Another possible mechanism—separate from those that depend on particular characteristics of the two types of candidates—is that strategic interest groups and other political actors might support the campaigns of more moderate candidates more than those of extremists in the general election. We know, for example, that interest groups are more moderate than voters on average (Bonica 2013). In the first column of Table 4, I re-estimate the RDD with the share of nonindividual contributions the nominee receives from PACs in the general election as the outcome variable. The nomination of the extremist appears to cause a large decrease—approximately a 30 percentage-point decrease—in the share of contributions coming from PACs.³⁷ The results suggest that PACs may play a role

and I code the candidate as male if the probability the first name is male is above 0.99.

³⁶ For a review of this literature see Anzia and Berry (2011).

³⁷ This effect is also visible in dollars as opposed to shares. Using the full data and the third-order polynomial of the running variable, the nomination of the extremist is estimated to cause a -0.21 point change in log contributions from PACs, i.e., approximately a 19% decrease ($\exp(-.21) - 1 \approx 0.19$). However, this estimate is much noisier than the share estimate ($t = -0.93$). Either way, this should only be viewed as a tentative mechanism check. It may be problem-

in encouraging the penalty to extremists. In addition, we might suspect that the PAC effect is also a proxy for the behavior of other political elites in the district who may likewise withdraw support from extremist nominees.

The Penalty to Extremists Grows With Voter Information

Finally, general-election voters may react directly to the ideological positions of candidates, preferring more moderate positions to more extreme ones (Ansolabehere, Snyder, and Stewart 2001; Burden 2004; Canes-Wrone, Brady, and Cogan 2002; Erikson and Wright 2000; Tomz and Van Houweling 2008). This is a likely part of the explanation. Though it is hard to test this mechanism perfectly, we can examine variation in voters' information about the candidates. Plausibly, voters have less opportunity to react to candidates' positions when they have little information about the candidates. To test this, I use data on the degree of media coverage the district receives about its own representatives. Specifically, Snyder and Stromberg (2010) show that the *congruence* between a district and newspaper markets that provide local political news plays an important role in informing voters about their candidates and elected politicians. Snyder and Stromberg (2010) show that voters in districts where this congruence is higher—districts where more newspaper information about local politics is available—are more able to identify, describe, and rate their representatives.

I rescale the congruence measure from Snyder and Stromberg (2010) to run from 0, in the least congruent district in my sample, to 1 in the most congruent, and I interact it with the treatment. The second column of Table 4 presents the results. The penalty to extremists is smaller when information is more scarce and much larger in more congruent districts. Though we cannot reject the null that the effect is the same in the least and most congruent districts, the substantive difference in the effect is extremely large. The penalty to nominating the extremist is estimated to be three times as large in the most congruent district as in the least congruent district.

Identifying the precise channels through which the nomination of the extremist candidate has such pronounced effects is beyond the scope of this study, but investigations into the mechanisms proposed in this section—as well as others—may prove fruitful for future research.³⁸ The evidence presented in this section suggests that information about candidates as well as the beliefs and reactions of interest groups help drive the penalty to extremists while, perhaps surprisingly,

observable valence differences between extremists and more moderate candidates do not. Because extremists and more moderate candidates in the sample do not differ on observable valences, candidate positions are likely to play a major role in the penalty extremists face in the general election.

CONCLUSION

What happens when extremists win primaries? Recent election cycles, in which an unusual number of especially extreme candidates seem to have won nomination, have left many asking this question. It is difficult to answer, however, because the places that nominate extremists differ systematically from the places that nominate more moderate candidates. The present study addresses this problem by employing a regression discontinuity design in primary elections, which ensures that the places with a moderate or extreme nominee are otherwise comparable in expectation. This allows for the direct estimation of causal effects from the nomination of extremists without the need to model the district's ideology or to place candidates and districts on a joint ideological scale.

Using this technique, I show that general-election voters punish the nomination of extreme candidates from contested primaries, on average. In fact, the average electoral penalty to nominating an extremist is so large that it causes an observable ideological shift in the district's subsequent roll-call voting in the opposite direction, towards the opponent party's ideology. Primary voters thus face a clear tradeoff, on average, between supporting a more "electable" candidate or voting based on ideology and risking the loss of the seat to the other party.

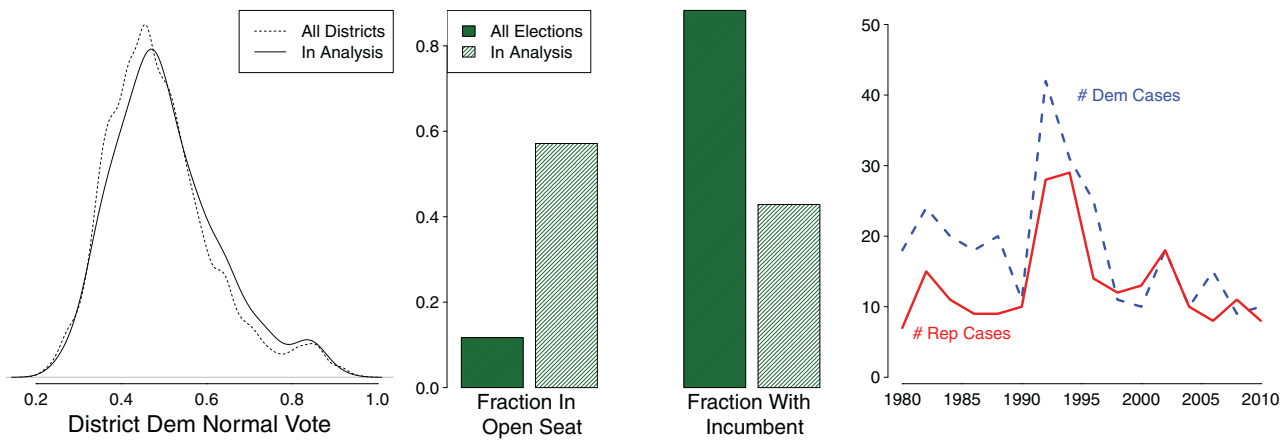
This risk is all the greater because the effects of nominating the extremist extend far into the future. Nominating the extremist today, and risking the loss of the seat today, makes the party much less likely to win the seat in all the subsequent elections within the redistricting period, and likely for much longer. The long-term electoral penalty and roll-call reversal caused by nominating the extremist dispel the notion that extremists can achieve their ideological goals even without winning the seat. In both the short and long run, the nomination of the extremist by one party produces a marked electoral and ideological shift towards the ideology of the opposite party.

Recent political discourse laments the tide of polarization sweeping U.S. legislatures, and a common claim is that primary voters are in part responsible for it. In studying how our electoral system selects candidates for office, and in attempting to explain the sources of the high and growing levels of polarization in our legislatures, we must build on the basic fact that general-election voters in the U.S. House are a tremendous force for moderation in filtering the set of potential candidates for office. When extremists win primaries, they find a supremely challenging electorate standing between them and legislative office.

atic to use contributions to define the treatment (through the scaling procedure) and as the outcome variable, although they are at least separated because the scaling uses only primary contributions and the outcome variable here is only general-election contributions.

³⁸ In this vein, Table A.4 examines heterogeneity in the effect across parties.

FIGURE A.1. Characteristics of the Sample: U.S. House Primary Elections, 1980–2010



Notes: The left panel shows that primary elections entering the sample are similar in terms of district presidential normal vote to all primary elections, 1980–2010. The middle panel shows that those in the sample are much more likely to be open elections. Finally, the right panel shows that there are roughly equal numbers of Democratic and Republican primaries in the sample.

TABLE A.1. Summary Statistics: Comparing Moderate and Extreme Primary-Winners, U.S. House 1980–2010

	Moderate in General	Extremist in General
Vote share	0.60	0.57
Victory	0.80	0.67
Share from party	0.19	0.22
Total from party	\$56,819	\$62,952
Share from PACs	0.73	0.72
Total from PACs	\$92,645	\$83,587
# Observations	259	245

Notes: Moderates appear to outperform extremists in the general, correlationally. Unit of observation is a primary election. Includes all primary elections in which at least two candidates have estimated donor scalings.

APPENDIX A: ADDITIONAL STATISTICAL RESULTS

Summary Statistics

Table A.1 presents summary statistics for the outcome variables of interest, separated out by the “treatment” and “control” cases, i.e., by whether the moderate or extreme candidate wins the primary. We see that moderate nominees tend to have higher general-election vote shares, receive less money from the party, and more money from PACs.³⁹ This correlational evidence suggests that the nomination of an extremist over a moderate harms the party, electorally, but it

³⁹ The reader may notice that both types of candidates have average general-election vote shares above 0.5. This is because contested primaries between two “viable” candidates (those who receive enough donations to be scaled) are more likely to occur in districts where the normal vote is tilted towards the party.

is far from sufficient for making such a conclusion. Districts that nominate extremists are likely to differ markedly from those that nominate moderates. In particular, districts that tend to nominate extremists are likely to have more partisan ideological views, and thus likely to support the nominated extremist in the general election more so than would a moderate, competitive district. The summary statistics are therefore likely to understate the electoral penalty to nominating the extremist. The regression discontinuity design that I employ below confirms this prediction. In fact, the penalty to nominating an extremist is much higher than these statistics suggest.

External Validity of the Sample

To investigate the external validity of the findings, we can compare the sample to the larger set of all primary elections. The left panel of Figure A.1 plots the distribution of the Democratic district presidential normal vote for all districts and for those entering the analysis. We see that the distributions overlap closely; districts that enter the sample do not appear to differ ideologically from the overall population of primary races.

In the middle panel, on the other hand, we see that the primaries entering the sample are much more likely to occur in open-seat races. This makes sense; incumbents have a significant advantage in both primary and general elections (Ansolabehere et al. 2007). As a result, a large number of incumbent-held seats have uncontested or uncompetitive primary elections in both parties and thus do not enter the sample. Finally, the right panel shows that observations in the sample are roughly equally likely to be either Democratic or Republican primaries, and occur consistently over time, with the early 1990s being the most common era represented in the sample.⁴⁰

⁴⁰ 1992 is the most common year in the sample, with 70 observations. This is because of the unusual number of incumbent retirements that

TABLE A.2. RDD Estimates of the Effect of Nominating an Extreme Candidate on General Election Vote Share in Open-Seat Races, U.S. House 1980–2010

	Vote Share General Election	Vote Share General Election	Vote Share General Election	Victory General Election	Victory General Election	Victory General Election
Extremist win	−0.16 (0.09)	−0.11 (0.04)	−0.09 [0.04]	−0.73 (0.31)	−0.49 (0.15)	−0.43 [0.19]
<i>N</i>	43	125	92	43	125	78
RDD bandwidth	5	—	11.71	5	—	9.97
Specification	Local linear	Cubic	IK	Local linear	Cubic	IK

Notes: Maximum of robust and conventional standard errors in parentheses. Columns 3 and 6 use optimal bandwidth technique from Imbens and Kalyanaraman, implemented using `rddob` in Stata. Standard errors from this procedure in brackets.

TABLE A.3. RDD Estimates of the Effect of Nominating an Extreme Candidate on General Election Vote Share, U.S. House 1980–2010

	No Incumbents		Incumbent Side Race		Challenger Side Race	
	Victory	Vote Share	Victory	Vote Share	Victory	Vote Share
Extremist win	−0.49 (0.15)	−0.11 (0.04)	−0.07 (0.18)	−0.10 (0.07)	−0.61 (0.23)	−0.09 (0.03)
<i>N</i>	125	125	82	82	39	39
RDD bandwidth	—	—	—	—	—	—
Specification	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic

Note: Robust standard errors in parentheses.

Open Elections

Figure 3 in the body of the article shows that the effect is large and negative in open-seat races, i.e., those where no incumbent is running in either party’s primary. This is important because it shows that the findings are not driven, on average, by extremist nominees kicking out incumbents. Put another way, the open-seat result shows that the loss of the incumbency advantage is not a mechanism that explains the findings.

To show this result in detail, Table A.2 replicates the main analysis, including the specification reported in Figure 3 (fifth column).

Incumbent Presence

Table A.3 presents more tentative results on the effect in races with incumbents. For simplicity, these estimates are presented always using the full data and a cubic polynomial, but findings are similar for other specifications. The first two columns reproduce the full-data specification for the open-seat races from Table A.2 for comparison’s sake.

In the second two columns, the effect is estimated only in primaries where an incumbent is present. This is a relatively small number of observations, and many of these races occur

in districts safe for the party (third row of Table 1). Because we know the effect is much smaller in safe districts (see Figure 3), it is not surprising that we see the effect attenuate somewhat in these incumbent side races.

The final two columns estimate the effect only for challenger side races in incumbent-held districts, i.e., the primary for the party that does not currently hold the seat. Here the effect is much larger—likely because these primaries tend to occur in more competitive districts (second row of Table 1).

Difference in Effect Across Parties

Table A.4 tests for whether the electoral effects differ across the two parties. To do so, I interact the treatment indicator with an indicator for the primary being a Democratic primary. As the table shows, the effect appears to be *smaller* (less negative) for Democrats. That is to say, Republican extremist nominees appear to receive a larger electoral penalty in the general election than do Democrats. This does not appear to be driven by differences in how extreme the “extremists” of each party are relative to the their moderate opponents; the average distance between the candidates in the contested primaries is no different across the two parties (difference in means = .009, $t = 0.82$).

Validating the RDD Assumption

Like a randomized experiment, the quasi-random variation from the RDD produces balance on pretreatment covariates,

occurred in this year, likely due to a combination of factors including the “check bouncing” scandal, redistricting, and the fact that it was the final year incumbents could keep their campaign funds if they retired (Carroll 1994; Groseclose and Krehbiel 1994).

TABLE A.4. RDD Estimates of the Effect of Nominating an Extreme Candidate on General Election Vote Share Across Parties, U.S. House 1980–2010

	Vote Share General Election	Vote Share General Election	Vote Share General Election	Victory General Election	Victory General Election	Victory General Election
Extremist win	– 0.18 (0.07)	– 0.13 (0.04)	– 0.08 (0.05)	– 0.67 (0.23)	– 0.51 (0.12)	– 0.39 (0.16)
Extremist win × Dem	0.11 (0.06)	0.07 (0.03)	0.06 (0.05)	0.29 (0.18)	0.31 (0.11)	0.26 (0.14)
Dem	– 0.05 (0.04)	0.02 (0.02)	0.01 (0.03)	– 0.08 (0.13)	– 0.04 (0.07)	0.01 (0.09)
<i>N</i>	83	252	135	83	252	148
RDD bandwidth	5	–	8.51	5	–	9.68
Specification	Local linear	Cubic	IK	Local linear	Cubic	IK

Notes: Robust standard errors in parentheses. Columns 3 and 6 use optimal bandwidth technique from Imbens and Kalyanaraman, implemented using `rdob` in Stata.

in expectation, if the identifying assumption of the RDD is met. We can validate this assumption by checking for balance on observed pretreatment covariates, as in Caughey and Sekhon (2011) and Eggers et al. (N.d.). To do so, I rerun Equation 1, but with a variety of pretreatment covariates as the outcome variable. These variables are as follows: absolute distance from 50% of the presidential normal vote in the district (the Democratic presidential normal vote for Democratic primaries, and the Republican presidential normal vote for Republican primaries), averaged over the period 1980–2010, to measure the partisanship of the district; the extreme candidate's share of primary donations; the extreme candidate's share of primary donations from PACs; the absolute value of the district's previous incumbent's DW-NOMINATE score, to measure the ideology of the district; the absolute value of the district's previous incumbent's W-NOMINATE score; and the party's lagged vote share and electoral victory.

There is some concern with using DW-NOMINATE because it is potentially post-treatment. The dynamic nature of DW-NOMINATE means that votes a legislator casts later in her career can affect her DW-NOMINATE scaling for earlier years. Thus the district's observed DW-NOMINATE pretreatment may actually include post-treatment roll-call voting. To make sure this is not an issue, I replicate this balance test also using W-NOMINATE scores, which are fixed by congress and therefore do not have this issue. To do so, I collected raw roll-call votes from www.voteview.com and scaled them, by congress, using the `wnominate` package in R.⁴¹ To adjust for the fact that the resulting W-NOMINATE scores are not comparable across congresses, which will add noise to the balance test (and thus lower the power of the balance test), I include year fixed effects in this final test.⁴²

Table A.5 presents the estimates. As can be seen, there is strong evidence for balance. Estimated differences are small

⁴¹ I set `dims` to 2, and I use an arbitrarily chosen Republican to set the polarity of the first dimension. I use an arbitrarily selected legislator (of either party) to set the polarity of the second dimension, which I do not use for any statistical purpose.

⁴² However, the balance tests turn out to be highly similar without these year fixed effects.

in magnitude, and only 1 out of 24 (4%) of the tests reject the null—roughly the 5% that would be predicted by chance under the null hypothesis (if the tests were independent).

The second rejection is for lagged vote share at the IK bandwidth. As can be seen from the three columns for that test, the results are highly unstable across bandwidths. This is because of the small sample sizes for this test. Because a large number of races occur right after redistricting (see for example the right panel of Figure 2), there are a large number of missing values for lagged electoral outcomes.

To adjust directly for this multiple testing problem, the final column in the table presents adjusted *p* values for the tests using the cubic specification and the full bandwidth (for brevity's sake I do not replicate these adjustments for all tests). The adjustment is carried out using a modified version of the “Free Step-Down Resampling” algorithm presented in Anderson (2008).⁴³ This technique adjusts for the covariance among the tests being run and provides exact probabilities rather than bounds. It is therefore less conservative than the Bonferroni test, which is desirable in the present context since we do not want to bias towards not uncovering imbalances if they exist. Not surprisingly, no evidence for imbalance is found using this technique since no test that was not rejected before will be rejected after adjustment.

In addition to these formal tests, Figure A.2 plots the raw data (along with binned averages) for each balance test, excluding only lagged victory because it is a binary variable. Consistent with the table, it is difficult to discern any evidence of sorting.

RDD Estimates Across Bandwidths and Specifications

The choice of RDD bandwidth and specification is somewhat arbitrary, so it is important to demonstrate that conclusions are not driven by these choices.

Figures A.3 and A.4 plot the RDD estimate— β_1 from Equation 1—across bandwidths for four specifications: the local linear, estimated separately on each side of the

⁴³ Details and code are available from the author upon request.

TABLE A.5. RDD Balance Tests

	Bandwidth Size			Adjusted <i>p</i> value
	5%	IK	–	
Presidential normal vote, Absolute distance from 50%	– 0.002 (0.049)	– 0.009 [0.013]	– 0.045 (0.027)	0.10
Extremist share of primary donations	– 0.013 (0.154)	0.033 [0.100]	– 0.007 (0.071)	
Extremist share of PAC Primary donations	– 0.131 (0.157)	– 0.043 [0.099]	– 0.092 (0.073)	0.21
Extremist total primary donations, \$100,000s	– 0.181 (0.420)	0.037 [0.411]	0.138 (0.189)	
Previous inc DW-NOM score, Absolute value	– 0.010 (0.130)	0.038 [0.052]	– 0.008 (0.071)	0.91
Previous inc W-NOM score, Absolute value (year adjusted)	0.120 (0.212)	– 0.053 [0.043]	– 0.167 (0.096)	
Lag vote share	0.074 (0.141)	– 0.091 [0.033]	0.050 (0.068)	0.46
Lag victory	0.011 (0.392)	0.005 [0.106]	– 0.046 (0.188)	

Notes: Maximum of robust and conventional standard errors in parentheses; standard errors from Imbens-Kalyanaraman in brackets. Column 1 reports results for local linear OLS estimated separately on each side of the discontinuity. Column 2 reports results using the Imbens-Kalyanaraman optimal bandwidth, implemented using `rddob` in Stata. Column 3 reports results using the full data with a cubic polynomial of the running variable. Column 4 reports *p* values for the cubic polynomial tests adjusted for multiple testing using Free Step-Down Resampling.

discontinuity, and quadratic, cubic, and quartic specifications of the running variable. The plots start from a bandwidth of 3 to ensure a reasonable number of data points enter the estimates. Below 3, the regressions rely on very few observations and, though estimates typically become even more negative, they vary greatly. Since no estimates are presented in the article at bandwidths this small, I omit them from the figures.

As the figures show, the conclusions drawn in the article do not vary based on the choice of bandwidth or specification. No matter what specification or bandwidth is used, a large negative effect is found on both vote share and victory, as presented in the article.

To fit as many specifications as possible, these graphs do not include confidence intervals. To give a sense of the noise across bandwidths, Figures A.5 and A.6 plot just the local linear estimate along with 95% confidence intervals. The vast majority of bandwidths support a “statistically significant” finding for the effect both in terms of vote share and victory. There is a small region near 10% where both effects’ confidence intervals cross 0, but it is a small region and is wider than would normally be trusted for a local linear specification (see for example Eggers et al. N.d.).

Finally, Figure A.7 plots the same local linear graph for the estimate on candidate quality reported in Figure 9. Again, the vast majority of bandwidths support the conclusion in the article that bare-winner extremists are no more or less likely to hold previous office-holder experience. There is a small region at very small bandwidths where the effect is quite negative—however, the standard errors are very large and the sample sizes are extremely small in this region.

Treatment Intensity

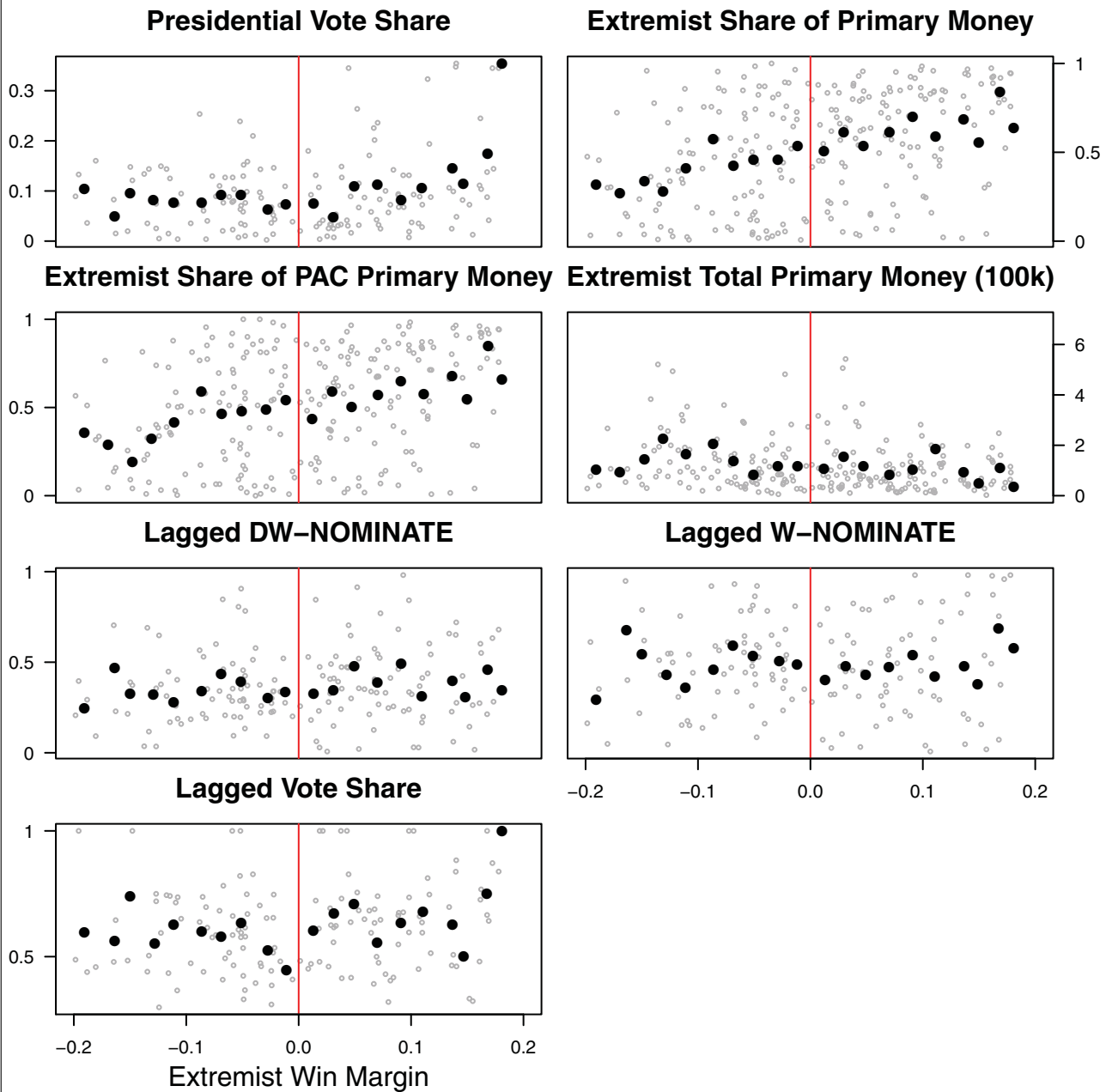
A main complication to the empirical analysis presented in the article is that the binary indicator for “treatment” stands in for a variety of treatment intensities or doses. In some districts that are “treated” with an extremist, the extremist may be relatively moderate, while in other cases she may be quite extreme. To explore this variation in the article, I employ an arbitrarily selected cutoff (the median distance), and I explore how the effect changes as I move this cutoff. To ensure that this cutoff does not drive the results, Table A.6 reports the estimates using no cutoff and instead controls for the distance between the two candidates as well as the interaction of this distance and the treatment.

To make the estimates interpretable, I first re-scale the ideological distance variable to run from 0, in the race where the distance between the moderate and the extremist is the smallest, to 1, in the race where this distance is largest. The coefficient on “Extremist Win” in this table thus reflects the estimated causal effect of nominating the “extremist” in a race where the distance between the moderate and the extremist is at the minimum level observed in the whole sample. The coefficient on the interaction reflects the estimated effect in a race where the distance between them is at the maximum level observed in the sample.

The results are consistent with those in the body of the article.

Looking at the first row, we see that the effects appear to be negative (although insignificant) even at the minimal distance. These point estimates are somewhat hard to trust, though, because we know that there is some degree of noise in

FIGURE A.2. Graphical Balance Tests



Note: Raw data are plotted in gray, with averages in two percentage-point bins of the running variable in black.

the underlying scalings of candidates. For candidates already close together, ideologically, we are therefore likely to misclassify some as “extremists” who are actually “moderates” and vice versa. This measurement error in the explanatory variable biases point estimates toward zero.

For vote share, the estimated effect for a race with the maximal distance ranges from -0.09 ($0.01 + -0.10 = -0.09$ in the IK specification) to -0.18 (local linear specification). For electoral victory, these effects range from -0.53 (cubic specification) to -0.82 (local linear).

The table also reports the p values testing the hypothesis that $\text{Extremist Win} + \text{Extremist Win} \cdot \text{Distance} = 0$. We can strongly reject the null that the effect is zero for the electoral victory estimates, and we can likewise reject for the vote share estimates with the local linear specification. We cannot reject the null for the cubic and IK specifications here, but the point estimates continue to be substantively large—on the same order as those presented in the article, though the comparison is somewhat difficult due to the differing estimation strategies.

FIGURE A.3. RDD Estimate for General-Election Vote Share Across Bandwidths from 3 to 50

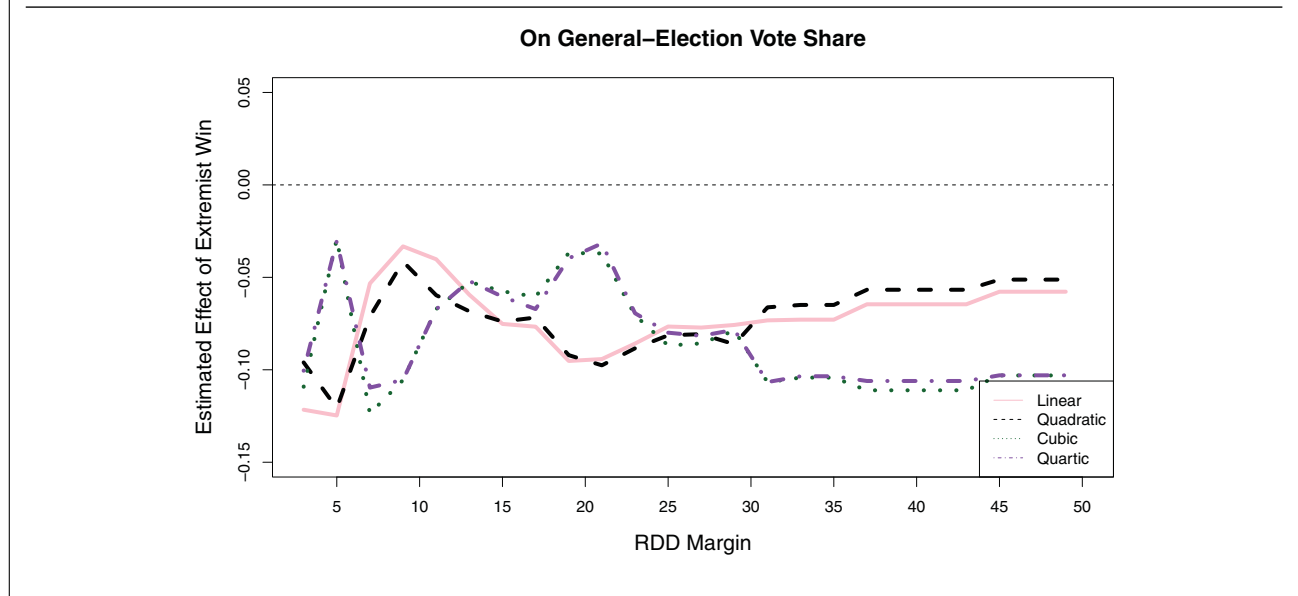
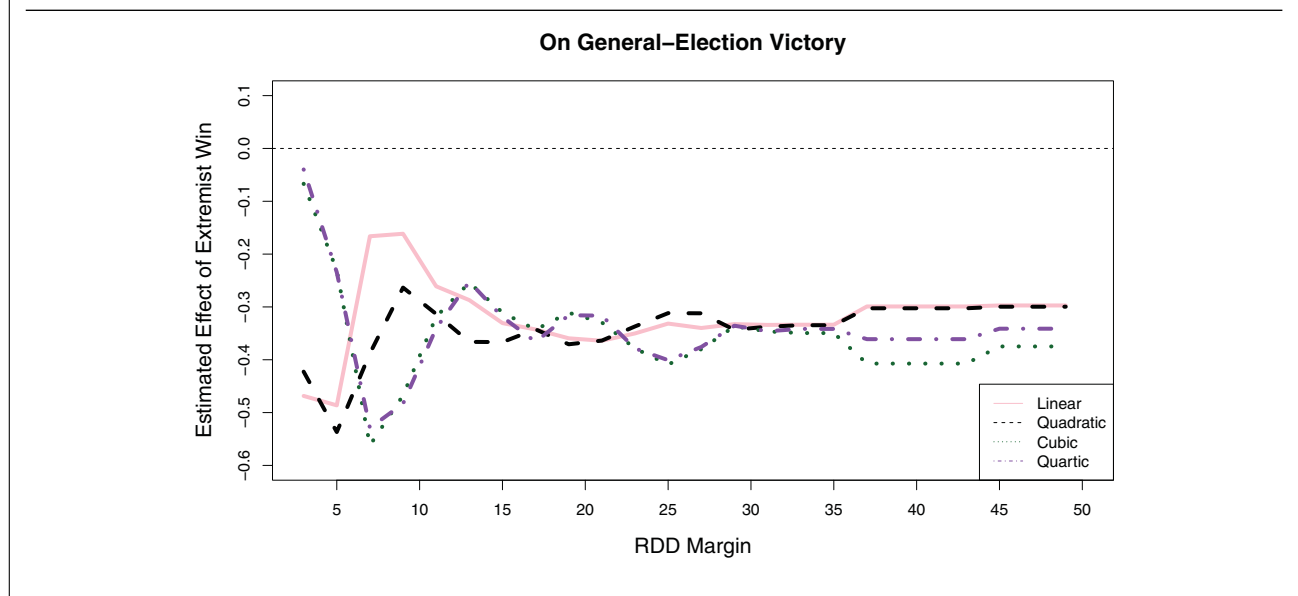


FIGURE A.4. RDD Estimate for General-Election Victory Across Bandwidths from 3 to 50



Replicating Results With Alternate Scalings

To ensure that the results are not driven by any issues with the contribution-based scaling, I replicate the main regressions on vote share and electoral victory using a separate measure of candidate ideology that does not rely on donations in any way. To do so I take advantage of competitive primary races where the top two candidates are both state legislators who have compiled an ideological record from the roll-call votes they cast in state legislatures.

Specifically, I focus on primary races where the top two candidates are state legislators who possess NP Scores (Shor

and McCarty 2011). As before, I calculate the absolute distance between the top two candidates in terms of their NP score, and I estimate the RD only using races where this distance is at or above the median distance across all races.

Table A.7 shows the estimates for the party's vote share and probability of victory. For comparison, the first row reproduces the findings from the article. The second row shows the corresponding estimates identifying moderates and extremists using NP scores instead of contributions. Because there are relatively few such races, the resulting estimates are noisier, but the point estimates are remarkably close to those in the article that rely on the contribution-based estimates.

FIGURE A.5. Local Linear RDD Estimate for General-Election Vote Share Across Bandwidths from 3 to 50

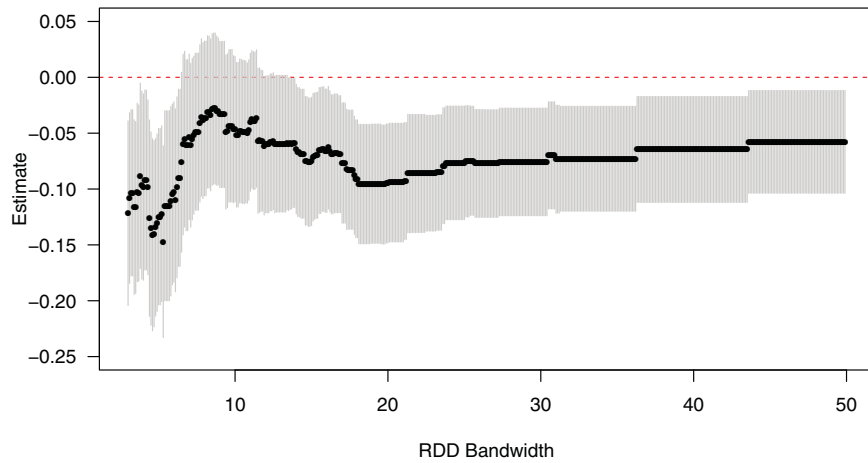


FIGURE A.6. Local Linear RDD Estimate for General-Election Victory Across Bandwidths from 3 to 50

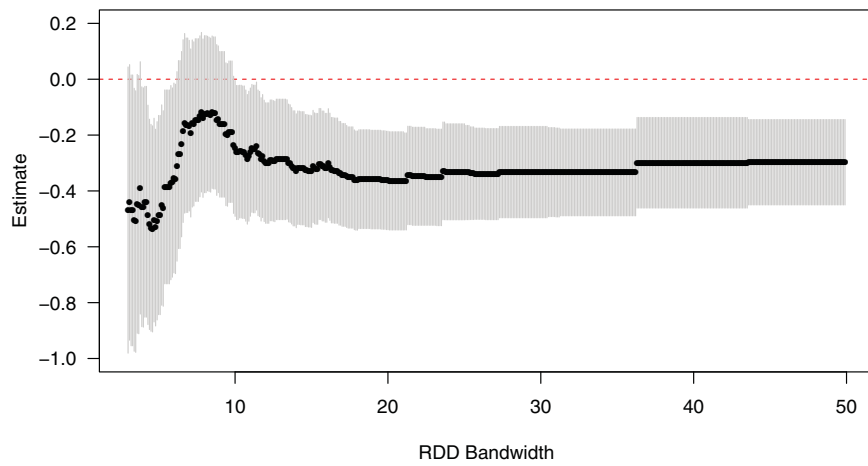


FIGURE A.7. Local Linear RDD Estimate for Nominee Previous Office-Holder Experience Across Bandwidths from 3 to 50

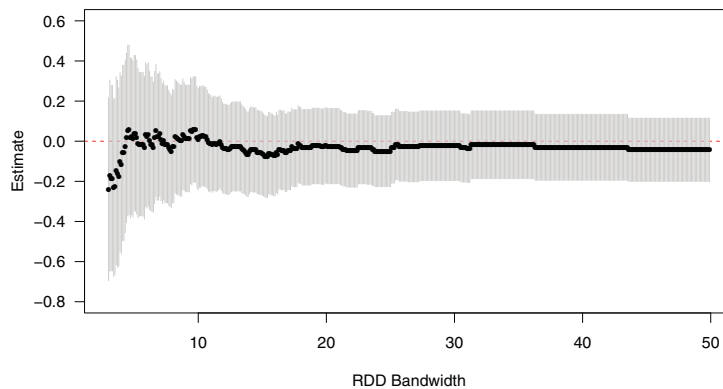


TABLE A.6. RDD Estimates of the Effect of Nominating an Extreme Candidate on General Election Vote Share, U.S. House 1980–2010

	Vote Share	Vote Share	Vote Share	Victory	Victory	Victory
Extremist win	−0.03 (0.04)	−0.04 (0.03)	0.01 (0.03)	−0.23 (0.16)	−0.09 (0.09)	−0.26 (0.15)
Extremist win × distance	−0.15 (0.09)	−0.05 (0.06)	−0.10 (0.08)	−0.59 (0.30)	−0.44 (0.17)	−0.37 (0.27)
Distance	0.16 (0.08)	0.05 (0.04)	0.08 (0.06)	0.64 (0.18)	0.23 (0.12)	0.45 (0.17)
<i>p</i> value for combined effect	0.02	0.08	0.18	0.00	0.00	0.01
<i>N</i>	173	504	261	173	504	208
RDD bandwidth	5	–	7.96	5	–	6.00
Specification	Local linear	Cubic	IK	Local linear	Cubic	IK

Notes: Robust standard errors in parentheses. Columns 3 and 6 use optimal bandwidth technique from Imbens and Kalyanaraman, estimated using local linear OLS. P-Values from test that Extremist Win + Extremist Win × Distance = 0.

TABLE A.7. Replicating Results With State Legislator NP Scores

	Vote Share	Victory
Main scaling	−0.10 (0.03)	−0.37 (0.11)
NP score	−0.13 (0.09)	−0.30 (0.24)

Note: Estimates using third-order polynomial of running variable; robust standard errors in parentheses.

APPENDIX B: SCALING CANDIDATES BASED ON DONATIONS

Below I reproduce the technical details from Hall and Snyder (2013). Readers should consult that article for further details and validation exercises.

Let $Contribution_{ij}$ be the contribution (in dollars) that candidate i receives from donor j . We use *all* donors—both individuals and interest groups—in the procedure.⁴⁴ Consider all incumbents. Let $Nominate_i$ be incumbent i 's ideological position, as estimated from i 's roll call voting record using DW-Nominate. Let

$$Donor\ Ideology_j = \frac{\sum_i Nominate_i Contribution_{ij}}{\sum_i Contribution_{ij}} \quad (2)$$

be the average contribution-weighted ideology of the incumbents to which donor j contributes. This gives an estimate of the “revealed ideological preference” of donor j . A possible problem with this definition is that we allow donor contributions to an incumbent candidate to affect that candidate's own scaling (because that candidate is included in the estimate of the donor's ideology). To avoid this feedback loop, we produce a separate donor scaling for each candidate i and donor j , where we *leave out* candidate i in the estimation

⁴⁴ We have verified, however, that results are substantively the same using only interest group donations.

of donor j 's revealed ideological preference. We define this more nuanced measure as

$$Donor\ Ideology_{-i,j} = \frac{\sum_{w \neq i} Nominate_w Contribution_{wj}}{\sum_{w \neq i} Contribution_{wj}} \quad (3)$$

Finally, for each candidate k , let

$$Cand\ Ideology_k = \frac{\sum_j Donor\ Ideology_{-k,j} Contribution_{kj}}{\sum_j Contribution_{kj}} \quad (4)$$

be the contribution-weighted average *Donor Ideology* of all donors that contribute to k . This serves as our estimate of the ideological position of candidate k . To make the measure more reliable, we exclude donors who make fewer than 20 donations to distinct candidates in our data, and we also exclude candidates who receive fewer than 20 donations from distinct donors.⁴⁵ Later we present results to show that our analysis is not dependent on the choice of threshold. Also in contrast to other scaling methods, we only use donations made during the primary-election cycle when scaling candidates, in order to avoid concerns that strategic interest groups target electable candidates in the general election (and thus make those candidates appear more moderate in the scaling).

We create two versions of these scalings. In the first, we use general-election and primary-election donations to *all* candidates to scale donors (and then use only primary donations from these groups to scale candidates). There is some concern that this method confounds “moderation” with electoral desirability, if some donors are strategic in whom they contribute to. For example, interest groups might donate to incumbents because incumbents are likely to win reelection. This donation behavior would make these interest groups appear moderate (because they would be donating to people from both parties), and in turn make the incumbents they donate to appear moderate, thus creating an artificial link between candidate moderation and electoral success. Even though we never use incumbents in our main analyses, these same interest groups might donate to other candidates in open-seat races, contaminating their estimated ideologies.

⁴⁵ We first exclude donors, and then drop candidates based on the number of donations they receive from the remaining donors.

We therefore construct a second version in which we do not use any donations to incumbents when scaling donors, and in which we do not use any donations to candidates after they become incumbents when scaling candidates.⁴⁶ Throughout the article, we present all of our results using both versions, to stress that they lead to the same conclusions.

To recap, here are the general steps taken to produce our scalings. For each candidate i , we do the following:

1. Estimate donor ideology for candidate i according to Equation 3 based either on *Nominate* scores of incumbent recipients or based on party affiliation of all recipients, using either (a) primary and general-election donations to all candidates excluding candidate i or, (b) primary and general-election donations to nonincumbent candidates, excluding candidate i .
2. Impute ideology for candidate i according to Equation 4, based either on the imputed *Nominate* or party affiliation scores of donors, using either (a) primary-election donations to all candidates or, (b) using only primary-election donations to nonincumbent candidates.⁴⁷

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⁴⁶ We have also used the scaling method where we scale donors using all contributions but only scale candidates using contributions they receive before they become incumbents (if they become incumbents). This method correlates at 0.995 with the method in which we never use incumbent donations for either donors or candidates. All analytical results are identical as a result, so we will not report them in the article.

⁴⁷ "Nonincumbent" candidates are all candidates not in office when they run for election. It includes candidates who go on to become incumbents. For this latter set of candidates, we do not use any contributions they receive after they gain office.

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