Electoral Penalties for Ideological Extremism in U.S. Federal and State Elections[*]

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Abstract

Do voters hold candidates accountable for their ideological positions? Past work on this topic has focused almost exclusively on U.S. House elections. It typically finds that candidates pay an electoral penalty for ideological extremity. In this paper, we extend the study of accountability to a far wider range of offices. We use roll call voting and campaign finance receipts to measure ideological moderation/extremism and a difference-in-difference identification strategy to examine accountability across five different offices: U.S. House, U.S. Senate, governor, state house, and state senate. In every office, ideologically moderate candidates are rewarded at the ballot, and extremists are punished. But we find significant variation in the penalty for extremism across offices. The smallest penalty for extremism is in state legislative elections, where voters barely punish extremists, and the largest penalty is in gubernatorial elections, where candidates receive a 2.5-5% lo in vote share for plausible changes in positions. Our results have implications for literatures on representation, accountability, congress, and state politics.

Keywords: Accountability, representation, state politics

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1 Introduction

Are politicians punished for taking ideologically extreme positions, and rewarded for taking ideologically moderate ones? Electoral sanctioning for policy extremity is one of the foundations of democratic governance. There is a large literature on electoral accountability for ideological extremism in the U.S. House. The bulk of these studies find that ideological moderation has an important influence on candidates' vote margins (e.g., Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Hall 2015), although there is some evidence that the electoral penalty for extremity is declining as elections are growing more nationalized (Wilkins 2013; Bonica and Cox 2018; Tausanovitch and Warshaw 2018). But there are two important limitations of the existing literature. First, few existing studies use a design-based approach to measure the effect the ideological moderation in elections (but see Hall 2015, 2019). In addition, there is almost no literature on accountability for ideological extremity in other offices (but see Rogers 2017).

In this paper, we use a unified research design to examine electoral accountability across a number of federal and state offices in American elections, including the U.S House, U.S. Senate, governor, and state legislatures. The first building block of any study of accountability for extremism is data on the ideological positions of candidates. Unfortunately, there is no perfect measure of candidate positions. Various measures tend to capture somewhat different dimensions of candidates' political orientations (Tausanovitch and Warshaw 2017; Bonica 2018). As a result, we use all available panel measures of candidate positions in our analyses, including both roll call-based measures (Poole and Rosenthal 2007; Shor and McCarty 2011) and campaign finance-based measures (Bonica 2014, 2018; Hall 2015). We examine the effect of candidates' ideological positions on their electoral margins using a difference-

in-difference identification strategy (Angrist and Pischke 2008).

We find that candidates across all offices are held accountable for their ideological positions. Consistent with past work, we find that voters prefer moderate candidates. Moreover, the size of the penalty for extremism is larger than in some past studies that use cross-sectional regression frameworks.[1] But we find significant variation across offices. The smallest penalty for extremism is in state legislative elections, where voters barely punish extremists, and the largest penalty is in gubernatorial elections, where candidates receive a 2.5-5% increase in vote share by moving halfway across the range of ideological positions in their party.

Our results have implications for literatures on representation, accountability, congress, and state politics. Most importantly, they show that ideologically extreme candidates pay a penalty at the ballot box. There are clear electoral incentives for moderation across all offices. The largest incentives for responsiveness are among governors, who pay a large penalty for extremism. In addition, our results show that politicians have electoral incentives to be responsive to their constituents. However, our results suggest that the penalty for extremism is declining in recent elections as voting becomes more nationalized. This decline in accountability for extremism could further fuel polarization in Congress and state governments.

The paper proceeds as follows. First, we review past literature on electoral accountability for ideological extremism. Second, we discuss our data and research design. Third, we discuss our findings on electoral accountability in the U.S. House, U.S. Senate, governors, and state legislatures. Finally, we briefly conclude.

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1. This could be because these studies are biased by confounders. It could also be because many past studies control for variable that are measured post-treatment (e.g., after donors and voters learn about candidates' ideological extremism).

2   Prior Literature

The spatial voting theory implies that citizens should be more likely to vote for individual candidates that share their ideological preferences (Downs 1957). Since the average voter tends to be more moderate than elected officials (Fiorina and Abrams 2008), this implies that more moderate positions should be rewarded at the ballot box. Indeed, a number of prior studies find that ideological moderation has a small, but important, influence on candidates' vote margins.

Table 1 summarizes prior work on electoral accountability for ideological extremity in American elections. It shows that most prior studies have focused on the U.S. House. Moreover, most of these studies have focused only on incumbents. For instance, examining elections between 1956-1996, Canes-Wrone, Brady, and Cogan (2002, 133) find that shifting from the middle of their party to the extremes lowers an incumbent's vote share by "1 to 3 percentage points." In recent years, several studies have used campaign finance data to measure the ideological positions of non-incumbents as well. For instance, Hall and Snyder (2015) uses campaign contribution data to measure the ideological positions of both incumbents and non-incumbents in U.S. House elections from 1980-2010. They show that a "a one standard deviation move to the right" is associated with an increase in the Democratic candidate's vote share of "1.3 to 2 percentage points." Using this same set of campaign finance-based data, Hall (2015) uses a regression discontinuity design (RDD) to examine what happens when extremists win primary elections. He finds that when parties nominate ideological extremists in House elections, 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. In a related paper, Hall and Thompson find that the mechanism for this electoral penalty is probably largely

variation in turnout, whereby extremists mobilize the other party's base (Hall and Thompson 2018).

There is much less prior work on electoral accountability for ideological extremism in other offices. In one of the few studies on state governments, Rogers (2017) uses the common space ideal points for state legislators developed by Shor and McCarty (2011) to examine whether state legislators pay an electoral price for ideological extremity. He finds that incumbents in state legislative elections pay about a one percentage point penalty for ideological extremity.

Methodologically, Table 1 shows that most prior studies on electoral accountability use cross-sectional designs, where they regress electoral margins on extremity and a host of controls (e.g., district ideology, candidate fundraising, incumbency status, etc). This leads to two well-known inferential problems. First, these studies typically lack a causal identification strategy. As a result, they could be confounded by any number of omitted variables. Second, the control variables are typically measured post-treatment. For instance, candidates' fundraising could be influenced by their ideological extremity. This can introduce severe, and unknown, levels of bias in the results of these studies (Montgomery, Nyhan, and Torres 2018). Our paper is part of a smaller set of recent studies that use a quasi-experimental research design to identify the effect of candidates' ideological positions on electoral margins (Hall 2015, 2019).

3   Data and Research Design

In order to evaluate electoral accountability for ideological extremity across offices, we build a panel dataset of constituency-level election returns for U.S. House, U.S. Senate, governor, state house, and state senate elections (see Table 2). For U.S. House

Table 1: Previous Large-Scale Panel Studies on Electoral Accountability for Ideological Extremism

Author	Office	Time Period	Non-incumbents?	Measure of Extremity	Research Design	Findings
Erikson and Wright (2000)	U.S. House		No	Roll Call Ideology	XS with controls	Small penalty
Ansolabehere, Snyder, and Stewart (2001)	U.S. House	1976-1996	No	Roll Call Ideology	XS with controls	Small penalty
Canes-Wrone, Brady, and Cogan (2002)	U.S. House	1956-1996	No	Roll Call Ideology	XS with controls	Small penalty
Carson et al. (2010)	U.S. House	1956-2004	No	Roll Call Partisanship	XS with controls	Small penalty
Wilkins (2013)	U.S. House	1900-2010	No	Roll Call Ideology	XS with controls	Small penalty in recent elections
Montagnes and Rogowski (2015)	U.S. House	1996-2006	Yes	NPAT-based Ideology	XS with controls	No penalty in recent elections
Hall and Snyder (2015)	U.S. House	1980-2010	Yes	Campaign Finance	XS with controls	Small penalty
Hall (2015)	U.S. House	1980-2010	Yes	Campaign Finance	RDD using primaries	Large penalty
Rogers (2017)	State Leg.	2001-2010	No	Roll Call Ideology	XS with controls	Small penalty
Bonica and Cox (2018)	US House/Sen.	1980-2012	Yes	Campaign Finance	XS with controls	Small penalty
Hall (2019)	U.S. House	1980-2012	Yes	Campaign Finance	TSXS (DiD)	Large penalty
Utych (2019)	U.S. House	1980-2012	Yes	Campaign Finance	XS with controls	Small penalty in recent elections

and Senate elections, we have data from 1980-2018. For state government elections, we have data from 1994-2018.[2] The outcome variable is the Democratic candidate's share of the two-party vote for each office.[3]

Table 2: Offices

Office	Temporal Coverage	Source of Election Results
House	1980-2018	MIT Election Data and Science Lab (2017a)
Senate	1980-2018	MIT Election Data and Science Lab (2017b)
Governor	1994-2018	CQ
State Senate	1994-2018	Klarner (2018)
State House	1994-2018	Klarner (2018)

3.1 Treatment Variable: Candidates' Ideological Extremity

The main treatment variables in our analysis are various measures of candidates' ideological positions. In an ideal world, we would have a comparable measure for both incumbents' and challengers' ideological positions in all elections based on candidates' roll call votes and public platforms. In other words, we would have something akin to the Nominate scores developed by Poole and Rosenthal. However, we generally lack such a measure for challengers in legislative elections. We also lack it for both challengers and incumbents in non-legislative elections where candidates do not cast roll call votes (e.g., gubernatorial elections). As a result, scholars have increasingly turned to measures of candidates' ideological positions based on their campaign donations (Bonica 2013, 2014; Hall 2015). The assumption of these studies is that donors make

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2. The more limited time frame here is due to the limited availability of data on the ideological positions of state legislative candidates prior to this.

3. As Hall, Yoder, and Karandikar (2017) points out in the context of their study of accountability for foreclosures in the Great Recession, "it might seem more logical to use incumbent party vote share, rather than Democratic party vote share, as our dependent variable... However, it seems unlikely that counties trend in terms of their general support for incumbents, and far more likely that they might trend in terms of their partisanship. As such, it makes more sense to use the ... specification with Democratic vote share as the dependent variable, so that we can account for these trends directly."

donations to candidates that reflect their ideological positions. Thus, the ideological positions of donors can be used as a proxy for the ideology of candidates.

Table 3: Data on Ideological Extremity

Office	RC-based Ideal Point (Static)	DW-DIME (Static)	CF-Score (Dynamic)	Donor Nominate Score (Dynamic)
House	X (DW-Nominate)	X	X	X
Senate	X (DW-Nominate)	X	X	X
Governor		X	X	X
State Senate	X (Shor-McCarty)	X	X	
State House	X (Shor-McCarty)	X	X	

We use four distinct campaign finance-based proxies for candidates' ideology in our analysis (see Table 3). Each of these measures tends to capture slightly different dimensions of candidates' political orientations (Tausanovitch and Warshaw 2017; Bonica 2018).

First, we use a measure of candidates' ideology based on their roll call voting behavior. For members of Congress, we use DW-Nominate scores, which we obtained from the voteview.com website (Poole and Rosenthal 2007; Lewis et al. 2019). These scores are static overtime for each candidate, and only available for incumbents and winning challengers. For state-legislators, we use the Shor-McCarty scores, which are based on their roll call votes and bridged across states using the NPAT test from Project Vote Smart (Shor and McCarty 2011).

Second, we use the DW-DIME scores developed by Bonica (2018). These scores are based on a machine learning algorithm to infer roll-call scores from campaign contribution data. The DW-DIME scores correspond quite closely to candidates' DW Nominate scores within party. But they are only available for candidates with a large number of donors. Moreover, they are based on the full history of candidates' campaign finance contributions. Thus, the DW-DIME scores use pre-and post-election contributions. They are static overtime for each candidate.

Third, we use the dynamic CF-Scores developed by Bonica (2014). These are based on a correspondence analysis of the patterns in 100 million contribution records from state and federal elections. It provides an indicator of the latent political orientation of candidates based on these donations patterns. This measure has the advantage of being available for almost all candidates using nearly all of the available campaign finance data. Moreover, it corresponds extremely well to candidates' party identification. But it is less strongly correlated within-party with candidates' DW-Nominate scores (Tausanovitch and Warshaw 2017). In addition to the main dimension of ideology, it could be capturing other aspects of candidates' ideological positions or other factors such as candidate quality. In addition, it is based on both pre-and post-election contributions (Hall 2015).

Finally, we use the average estimated positions of candidates' donors in a given cycle. Similarly to Hall and Snyder (2015) and Hall (2015), the donors' positions are estimated as the contribution-weighted average DW-NOMINATE score (Poole and Rosenthal 2007) of incumbents they have donated to, but excluding donations to candidate$_i$ when computing the score for candidate$_i$. The measurement model here is simpler than in Bonica's work, but it has the advantage of being based only on candidates' donors in a particular election cycle. This ensures that it is measured entirely based on donations *prior* to the election. We currently have developed this measure for U.S. House, U.S. Senate, and gubernatorial candidates. In the future, we hope to add it for state legislative candidates.

We standardize each of our measures of candidates' ideological positions so the regression estimates are comparable across offices. This means that the regression estimates show the effect of a one standard deviation move to the right in candidates' ideological positions. However, it is important to keep in mind that a one standard deviation move to the right roughly spans the full range of within-party candidate

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positions. Most constituencies are very unlikely to see this large a change in candidate positions from election-to-election. In other words, we will rarely observe an extremely conservative Republican running in an urban congressional district, or a moderate one running in a rural district. So we also show the effect of a more plausible two-standard deviation shift in candidate conservatism within unit (Mummolo and Peterson 2018). This generally yields much smaller point estimates of the effect of plausible shifts in candidates' ideological positions on their vote shares.

3.2 Research Design

In order to estimate the causal effect of changes in candidates' ideological positions, we estimate a series of difference-in-difference panel models using the following equation:

$$DemVotePct_{it} = CandConservatism_{it} + IncParty_{i,t-1} + \gamma_{ip} + \tau_{rtp}, \qquad (1)$$

A move to the right in candidate conservatism (CandConservatism) should always increase Democratic vote share if citizens are voting spatially. Indeed, a move to the right means that Democratic candidates are taking more moderate positions, while Republican candidates are taking more extreme positions. We include Democratic and Republican candidates as separate rows in our data. We then cluster standard errors by election.

Our difference-in-difference models use constituency-party fixed effects (γ_{ip}), to account for time-invariant confounders in each constituency, such as the partisan or ideological preferences of voters.[4] Our models also use region-year-party fixed effects (τ_{rtp}), to control for time-varying confounders at the region and national levels (Fowler

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4. For legislative districts, we use decade x district fixed effects to account for changes in districts due to redistricting. For statewide elections, we use state fixed effects.

and Hall 2018). The region-year-party fixed effects mean that our analysis is comparing constituencies with more ideologically extreme candidates with constituencies that have less ideologically extreme candidates from the same party in the same region and year. However, we obtain substantively similar results using a variety of other specifications. All models cluster standard errors by constituency (e.g., state or legislative district).

In some respects, this research design is inferior to the regression discontinuity (RD) design used by Hall (2015). Given its transparent and testable identifying assumptions, the RD design is an appealing mode of causal inference, but its emphasis on observations near the RD threshold restricts the effective sample size (Caughey, Warshaw, and Xu 2017). Indeed, there are relatively few nearly tied contested primaries in Senate and gubernatorial elections. Moreover, primary election data for state legislative seats is not readily available. Our difference-in-difference design enables us to examine each office using a unified framework. In addition, it leverages a far larger number of elections than an RDD. Finally, our focus on using fixed effects to account for unobserved confounders enables us to avoid using control variables that could be measured post-treatment (Montgomery, Nyhan, and Torres 2018).

4   Results

We now move on to discuss our main results for the effects of candidates' ideological extremity on vote margins in U.S. House, U.S. Senate, governor, state house, and state senate elections. In each section, we show the results of a series of difference-in-difference models on the effect of candidate moderation/extremity. The measures of candidate positions vary a bit by office (as reflected in the regression tables). However, we have tried to use all available measures of candidates' ideological positions that

are available for each office. Recall that if voters are voting spatially, a shift to the right by candidates should always increase Democratic vote share.

4.1   U.S. House

Prior research strongly suggests that voters punish U.S. House candidate for ideological extremism (e.g., Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Hall 2015). Our results for U.S. House elections from 1980-2018 are broadly consistent with prior findings (Table 4).

Table 4: Penalty for Ideological Extremism in U.S. House Elections

	Dependent variable: Democratic vote share			
	RC Ideal Point (Static) (1)	DW-DIME (Static) (2)	CF-Score (Dynamic) (3)	Donors Nom. Score (Dynamic) (4)
Candidate Conservatism	0.079*** (0.030)	0.092*** (0.022)	0.036*** (0.006)	0.081*** (0.012)
Inc. party (lagged)	0.028*** (0.003)	0.038*** (0.003)	0.041*** (0.003)	0.036*** (0.003)
Effect of Plausible Counterfactual Δ in Conservatism	0.011***	0.016***	0.015***	0.023***
Time Period:	1980-2018	1980-2018	1980-2018	1980-2016
District-Decade-Party FEs	X	X	X	X
Region-Year-Party FEs	X	X	X	X
Observations	6,233	7,650	10,450	8,033
R^2	0.894	0.883	0.887	0.882
Adjusted R^2	0.832	0.809	0.823	0.805

Note: SE's clustered by district-decade and dist.-year.   *p<0.1; **p<0.05; ***p<0.01

First, we examine accountability for incumbents based on their DW-Nominate scores (Poole and Rosenthal 2007). This analysis indicates that a one standard deviation shift in ideological moderation (e.g., moving from the extreme of a candidate's party to the middle of the cross-party ideological distribution) leads to a 8 percentage

point increase in vote share. As we mentioned earlier, however, it is extremely unlikely candidates will shift across the range of their party's ideological positions within a particular constituency. A more realistic two-standard deviation shift within unit would lead candidates to get about a 1% increase in vote share. Next, we examine accountability based on candidates' DW-DIME scores, which are based on campaign donations but are designed to be similar to DW-Nominate scores (Bonica 2018). This analysis indicates that a one standard deviation shift to the center would lead to a 9% increase in vote share. A more plausible within-unit shift would lead to a 1.6% increase in vote share.

Next, we examine the effect of variation in two dynamic measures of candidates' positions. The third column of Table 4 shows that a one-standard deviation shift in dynamic CF-Scores leads to a 3.6% change in vote share. A more plausible within-unit shift would lead to a 1.5% increase in vote share. Finally, the fourth column of Table 4 examines the effect of variation in candidate positions based on candidates' general election donors DW-Nominate scores. This indicates that candidates are punished by 8.1 percentage points for a one standard deviation increase in extremity. A more plausible within-unit shift would lead to a 2.3% change in vote share.

Overall, our analyses using a variety of both roll call and campaign finance-based measures of candidate positions suggest that voters punish U.S. House candidates for ideological extremity and reward them for moderation. However, the size of plausible within-unit shifts in candidate positions is generally small. We find that plausible counterfactual shifts in candidate conservatism within unit lead to a 1.1-2.3% change in vote share. In other words, we find that extremists are penalized by up to 2.3 percentage points in U.S. House elections. Thus, our results indicate that while House candidates are held accountable for their positions, the size of the reward for moderation and punishment for extremism is relatively small.

Our results for the effect of ideology on vote margins in House races are generally in-line with prior work. But there are some fine-grained differences. The effects we uncover are somewhat larger than in some previous cross-sectional studies (e.g., Canes-Wrone, Brady, and Cogan 2002). But, they are smaller than in Hall (2015)'s RDD study of what happens when extremists win primaries, which found an 8-12% penalty for extremists.

4.1.1 Changes in Reward for Ideological Moderation Overtime

In this section, we examine whether the electoral rewards for moderation in U.S. House elections have waned overtime as elections have become more nationalized (Hopkins 2018). Several studies have found suggestive evidence that the electoral reward for moderation is declining overtime (Wilkins 2013; Bonica and Cox 2018; Tausanovitch and Warshaw 2018). However, none of these studies uses an identification strategy that accounts for possible confounders.

Table 5 shows how the reward for moderation in U.S. House elections varies overtime. The first row shows the reward for moderation in the 2010s, and later rows show the interaction between the reward for moderation in the 2010s and decade specific indicators. The table shows that the reward for moderation in the 2010s was generally small and insignificant. It is also smaller than in previous decades. The largest reward for moderation occurred during the 1980s. In general, there appear to be smaller rewards for moderation in subsequent decades, which is consistent with previous work showing that the effect of candidates' ideology started declining in the 1990s (Wilkins 2013; Bonica and Cox 2018).

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Table 5: Overtime Changes in Reward for Ideological Moderation in House Elections

	Dependent variable: Democratic vote share			
	RC Ideal Point (Static) (1)	DW-DIME (Static) (2)	CF-Score (Dynamic) (3)	Donors Nom. Score (Dynamic) (4)
Candidate Conservatism (2010s)	0.026 (0.042)	0.005 (0.044)	0.017*** (0.006)	0.049 (0.033)
Conservatism x 2000s	0.077 (0.062)	0.102** (0.051)	0.024** (0.011)	0.029 (0.037)
Conservatism x 1990s	0.004 (0.070)	0.042 (0.060)	0.015 (0.013)	0.018 (0.037)
Conservatism x 1980s	0.131* (0.075)	0.194*** (0.067)	0.049** (0.021)	0.065 (0.043)
Inc. party (lagged)	0.028*** (0.003)	0.037*** (0.003)	0.040*** (0.003)	0.036*** (0.003)
Time Period:	1980-2018	1980-2018	1980-2018	1980-2016
District-Decade-Party FEs	X	X	X	X
Region-Year-Party FEs	X	X	X	X
Observations	6,233	7,650	10,450	8,033
R^2	0.894	0.883	0.887	0.882
Adjusted R^2	0.833	0.810	0.823	0.805
Note:				*p<0.1; **p<0.05; ***p<0.01

4.1.2 Variation between Midterm and Presidential Years

Next, we examine whether the reward for moderation varies between midterm and presidential years. It is plausible that elections might be more nationalized in presidential years, which would yield a smaller reward for moderation in those elections.

Table 6: Heterogeneity between Midterm and Presidential Years in Reward for Ideological Moderation in U.S. House Elections

	Dependent variable: Democratic vote share			
	RC Ideal Point (Static) (1)	DW-DIME (Static) (2)	CF-Score (Dynamic) (3)	Donors Nom. Score (Dynamic) (4)
Candidate Conservatism	0.073** (0.031)	0.085*** (0.024)	0.030*** (0.006)	0.074*** (0.013)
Conservatism x Midterm	0.013 (0.011)	0.011 (0.011)	0.012* (0.007)	0.014 (0.010)
Inc. party (lagged)	0.028*** (0.003)	0.038*** (0.003)	0.041*** (0.003)	0.036*** (0.003)
Time Period:	1980-2018	1980-2018	1980-2018	1980-2016
District-Decade-Party FEs	X	X	X	X
Region-Year-Party FEs	X	X	X	X
Observations	6,233	7,650	10,450	8,033
R^2	0.894	0.883	0.887	0.882
Adjusted R^2	0.832	0.809	0.823	0.805

Note: *p<0.1; **p<0.05; ***p<0.01

Table 6 above shows the results of our analysis. We find no substantial difference between the reward for moderation in presidential and midterm years. The difference between midterm and presidential years is generally small and not statistically significant.

4.2 U.S. Senate

Next, we examine the effect of candidates' ideological positions in U.S. Senate elections (Table 7). Due to the smaller number of Senate elections, the results here are noisier than for House Elections. Overall though, they show that Senate candidate are penalized for extremity and rewarded for moderation.

Table 7: Penalty for Ideological Extremism in U.S. Senate Elections

	Dependent variable: Democratic vote share			
	RC Ideal Point (Static) (1)	DW-DIME (Static) (2)	CF-Score (Dynamic) (3)	Donors Nom. Score (Dynamic) (4)
Candidate Conservatism	0.020 (0.032)	0.077*** (0.030)	0.110*** (0.035)	0.136*** (0.035)
Inc. party (lagged)	0.040*** (0.013)	0.045*** (0.010)	0.047*** (0.010)	0.042*** (0.011)
Effect of Plausible Counterfactual Δ in Conservatism	0.006	0.026***	0.043***	0.048***
Time Period:	1980-2018	1980-2018	1980-2018	1980-2016
State-Party FEs	X	X	X	X
Region-Year-Party FEs	X	X	X	X
Observations	628	847	940	841
R^2	0.803	0.734	0.733	0.728
Adjusted R^2	0.538	0.501	0.532	0.497

Note: SE's clustered by district-decade and dist.-year.   *$^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$*

First, we examine accountability for incumbents based on their DW-Nominate scores (Poole and Rosenthal 2007). This analysis indicates that incumbents are generally not penalized for extremity in roll call voting. Next, we examine accountability based on candidates' DW-DIME (Bonica 2014). This analysis indicates that a plausible change in candidate positions leads to a 2.6% change in vote share. Third, we examine accountability using candidates' dynamic CF-scores. This analysis indicates a 4.3% reward for shifting two standard deviations toward the center within

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constituencies. Finally, we examine accountability using candidates' cycle-specific general election donors. This analysis indicates that a plausible change in candidate positions leads to a 4.8% change in vote share.

4.3   Governor

Next, we examine the effect of candidates' ideological positions in gubernatorial elections. Due to the small number of gubernatorial elections, the results here quite noisy. But they provide suggestive evidence that the penalty for extremism in gubernatorial elections is larger than in House elections and similar in size to Senate elections.

Table 8: Penalty for Ideological Extremism in U.S. Gubernatorial Elections

	Dependent variable: Democratic vote share		
	DW-DIME (Static)	CF-Score (Dynamic)	Donors Nom. Score (Dynamic)
	(1)	(2)	(3)
Candidate Conservatism	0.105** (0.040)	0.097* (0.051)	0.158*** (0.039)
Inc. party (lagged)	0.025** (0.010)	0.015 (0.010)	0.008 (0.009)
Effect of Plausible Counterfactual Δ in Conservatism	0.024**	0.026**	0.046***
Time Period:	1994-2018	1994-2018	1994-2016
State-Party FEs	X	X	X
Region-Year-Party FEs	X	X	X
Observations	377	419	372
R^2	0.764	0.751	0.778
Adjusted R^2	0.419	0.412	0.480
Note:			*$p<0.1$; **$p<0.05$; ***$p<0.01$

Table 8 shows the results. There is no roll-call based measure of the positioning of gubernatorial candidates. So, first we examine accountability based on candidates' DW-DIME scores. This analysis indicates that plausible counter-factual changes in

positions leads to a 2.4% shift in vote shares. Second, we examine the effect of changes in candidates' dynamic CF-scores. This indicates that plausible shifts in CF-scores lead to a 2.6% shift in vote share. Finally, we examine the effect of variation in candidates' cycle-specific general election donors Nominate scores. This indicates that plausible shifts in positions lead to a 4.6% shift in vote share.

4.4 State Legislature

In this section, we examine accountability in state legislative elections. Prior work suggests that voters know much less about state legislative elections than congressional and gubernatorial elections. As a result, we might expect accountability to be more muted in state legislative elections, particularly in state house elections. Indeed, Rogers (2017) finds only modest effects of accountability in state house elections.

Table 9: Penalty for Ideological Extremism in State Senate Elections

	Dependent variable: Democratic vote share		
	RC Ideal Point (Static)	DW-DIME (Static)	CF-Score (Dynamic)
	(1)	(2)	(3)
Candidate Conservatism	0.018 (0.014)	0.017 (0.012)	0.020*** (0.003)
Inc. party (lagged)	0.016*** (0.003)	0.006** (0.003)	0.012*** (0.002)
Effect of Plausible Counterfactual Δ in Conservatism	0.001***	0.003**	0.013***
Time Period:	1994-2018	1994-2018	1994-2018
District-Decade-Party FEs	X	X	X
Region-Year-Party FEs	X	X	X
Observations	21,215	12,573	30,488
R^2	0.894	0.899	0.890
Adjusted R^2	0.862	0.860	0.859
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 9 shows our results for state senate elections. First, we examine the effect of shifts in roll-call based ideal points (i.e., Shor-McCarty scores). We find that a one standard deviation shift in ideal points leads to a 1.8% change in vote share. However, there is little within-district variation in ideal points, largely due to the fact that these ideal points are fixed overtime. So the effect of plausible counter-factual shifts in within-district ideal points is close to zero. Next, we examine the effect of variation in DW-DIME scores. Here, we find that plausible counterfactual shifts in these scores leads to a .3% shift in vote shares. Lastly, we examine the effect of variation in dynamic CF-scores. Here, we find that a plausible counterfactual shift in these scores leads to a substantially larger 1.3% shift in vote shares.

Table 10: Penalty for Ideological Extremism in State House Elections

	Dependent variable: Democratic vote share		
	RC Ideal Point (Static)	DW-DIME (Static)	CF-Score (Dynamic)
	(1)	(2)	(3)
Candidate Conservatism	0.017*** (0.006)	0.066*** (0.016)	0.106*** (0.005)
Inc. party (lagged)	0.014*** (0.001)	0.007*** (0.001)	0.010*** (0.001)
Effect of Plausible Counterfactual Δ in Conservatism	0.000***	0.012***	0.046***
Time Period: District-Decade-Party FEs Region-Year-Party FEs	1994-2018 X X	1994-2018 X X	1994-2018 X X
Observations	52,810	20,674	54,599
R^2	0.867	0.891	0.875
Adjusted R^2	0.817	0.840	0.816
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 10 shows our results for state house elections. First, we examine the effect of shifts in roll-call based ideal points (i.e., Shor-McCarty scores). We find that a one

standard deviation shift in ideal points leads to a 1.7% change in vote share. But the effect of plausible counter-factual shifts in within-district ideal points is close to zero. Next, we examine the effect of variation in DW-DIME scores. Here, we find that plausible counterfactual shifts in these scores leads to a 1.2% shift in vote shares. Lastly, we examine the effect of variation in dynamic CF-scores. Here, we find that a plausible counterfactual shift in these scores leads to a substantially larger 4.6% shift in vote shares.

4.5   Comparing Effects of Extremism Across Offices

Finally, we compare the penalty for extremism, and rewards for moderation, across offices. For this analysis, we subset our data to only include elections after 1992. This makes the time period of the analysis roughly similar across offices. In addition, we focus on the standardized measure of candidate conservatism since this measure is arguably more comparable across offices than the within-constituency counterfactuals we focused on in the previous sections.

Figure 1 compares the effect of ideological moderation and extremity on candidates' vote shares. While there is variation across measures of candidate conservatism, overall, the graph shows that candidates across all offices are rewarded for moderation and punished for ideological extremity. But the penalty for extremity is generally lowest in state legislative elections and highest in senate and gubernatorial elections.

Figure 2 compares the effect of candidates' conservatism on their probability of winning. Overall, this graph shows candidates across all offices are less likely to win if they take ideologically extreme positions. But there is significant heterogeneity across measures. There appears to be no effect of variation in roll call-based ideal points on candidates' probability of winning. But there are clear effects using the
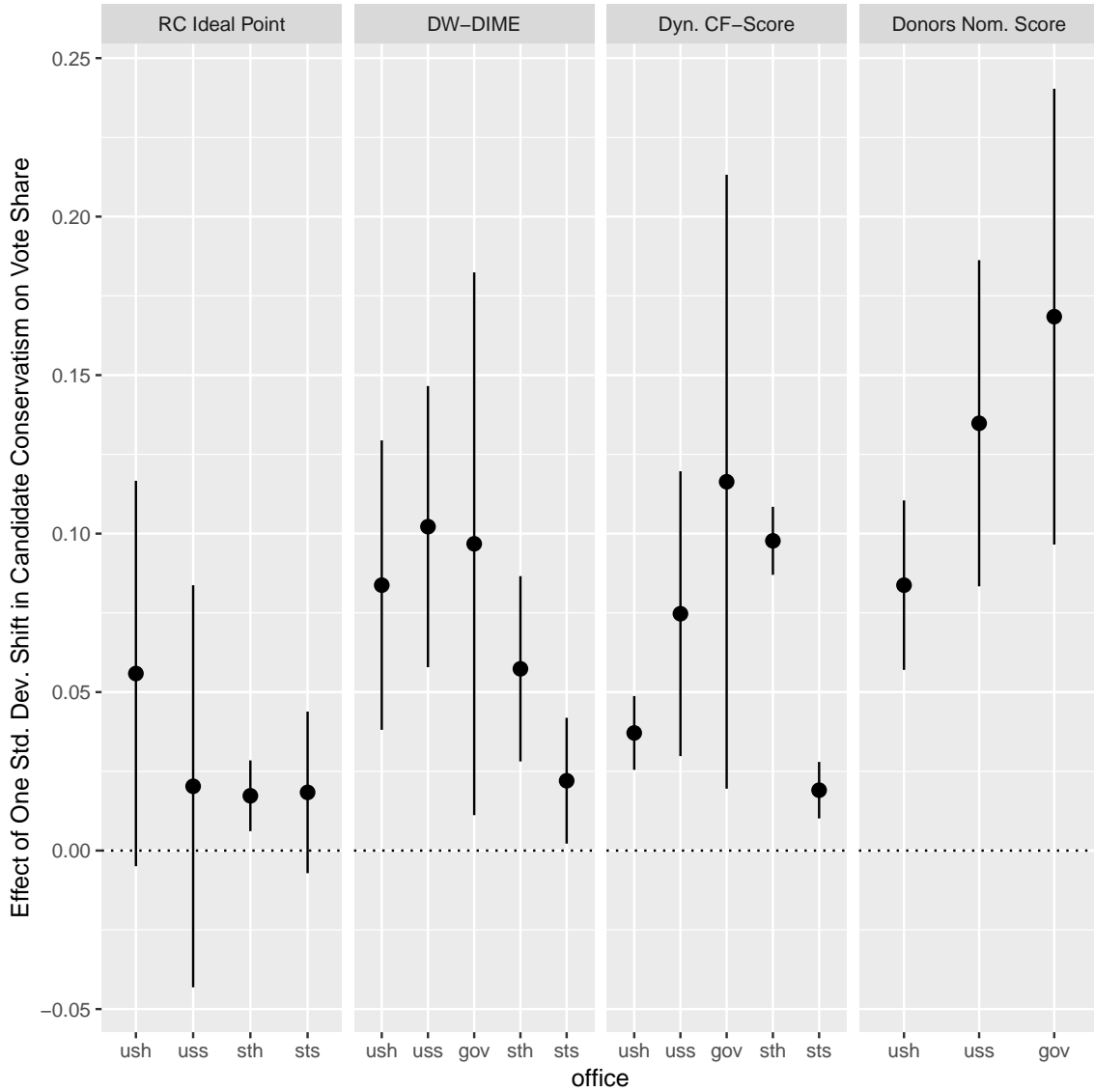
20

Figure 1: Penalty for Ideological Extremism Across Offices (Vote Share)

donation-based measures. Substantively, the reward for moderation again appears to generally be largest in gubernatorial elections, and smallest in state legislative elections. The higher penalty for extremity in gubernatorial elections could be due to the fact that voters are willing to cross party lines in gubernatorial elections to a great extent than in congressional elections. Moreover, voters know far more about gubernatorial candidates' ideological positions than they do about state legislative
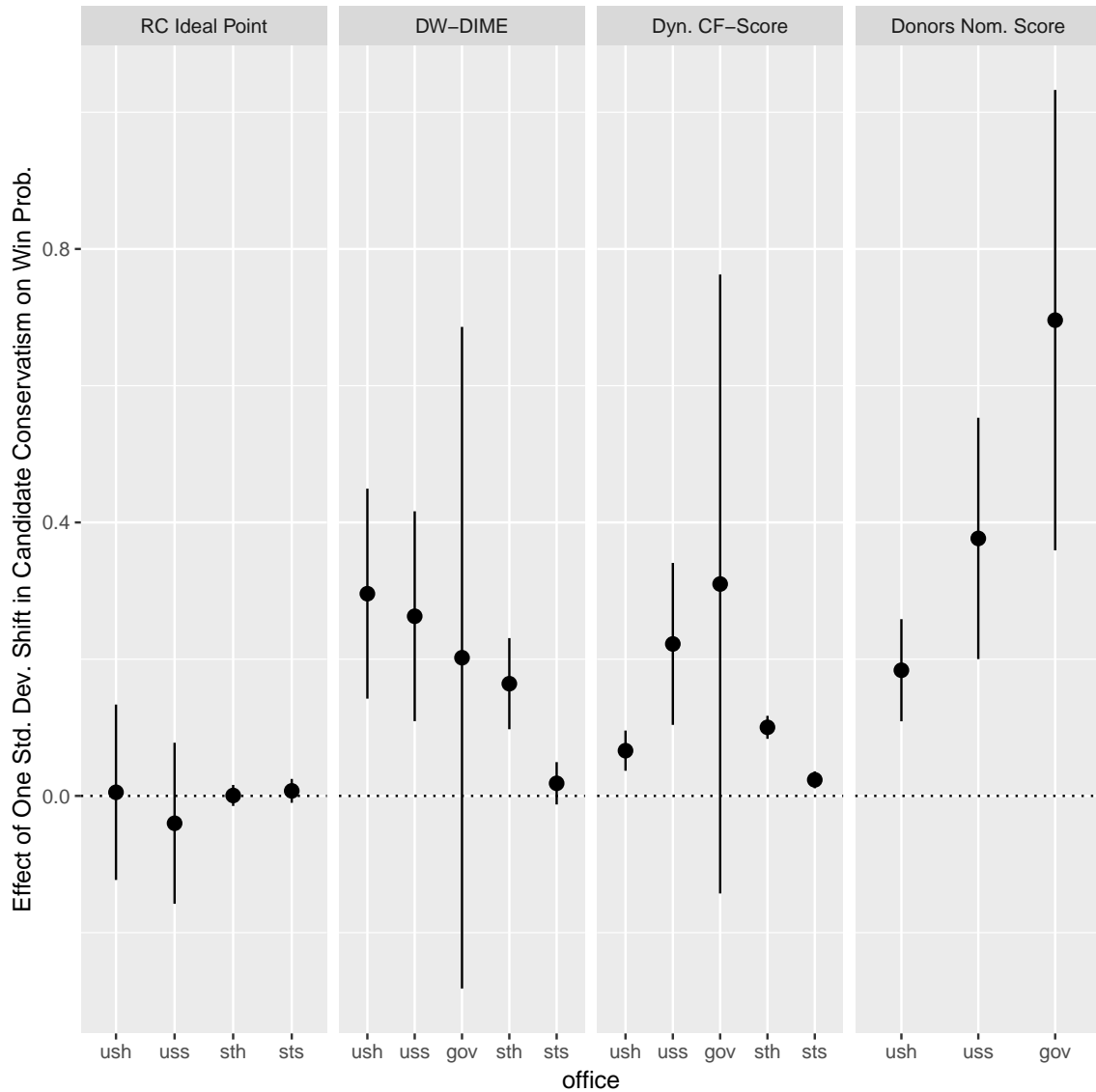
candidates' positions.

Figure 2: Penalty for Ideological Extremism Across Office (Probability of Victory)

5   Conclusion

This paper has examined electoral accountability for ideological extremity across offices in American elections. We have shown that ideologically extreme candidates pay

a penalty at the ballot box across each office. The smallest electoral penalty is in state legislative elections, while the largest electoral penalty for extremism is in gubernatorial elections. This provides a strong incentives for governors to be responsiveness to their constituents, which provides an electoral foundation for the consistent finding in past work that state policy is responsive to constituent preferences (Erikson, Wright, and McIver 1993; Caughey and Warshaw 2018).

Methodologically, our paper shows that the point estimates of accountability vary considerably using different measures of candidate positions. We believe one lesson is that future studies on accountability should employ a variety of measures of candidate positions rather than relying upon a single measure. Future work should also deploy multiple causal identification strategies where feasible. However, it is important for scholars to avoid controlling for post-treatment variables in cross-sectional regression models.

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