

How The Public Funding Of Elections Increases Candidate Polarization*

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Abstract

I show that the public funding of elections produces a large decrease in the financial and electoral advantage of incumbents. Despite these effects on electoral competition, I demonstrate that public funding produces *more* polarization and candidate divergence—not less. Finally, I establish that this effect is at least in part due to the fact that public funding disproportionately affects the contribution behavior of access-oriented interest groups, groups who, I show, systematically support moderate incumbents. Access-oriented interest groups therefore help generate the incumbency advantage and mitigate polarization by supporting moderate legislators.

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In his 1907 State of the Union address, President Theodore Roosevelt told the nation that “there is a very radical measure which would, I believe, work a substantial improvement in our system of conducting a campaign.”¹ He was referring to the public funding of political campaigns, which he believed would remove the need for candidates to seek out significant campaign funds from private sources. Over 100 years later, campaign finance reform remains a salient topic in American politics, and among possible reforms, the public funding of elections is an increasingly popular choice. In addition to the federal government’s funding of presidential elections, fourteen U.S. states now offer some kind of public funding to candidates for state offices,² and similar programs have been proposed for the U.S. House and Senate.³

Theoretical support for public funding programs stems from the notion that candidates will be more competitive if they have more equal access to campaign funds, and that they will spend less time listening to well-financed interest groups and more time listening to voters—and thus will be more responsive to constituent needs in the legislature—if they have access to campaign subsidies. For example, the Arizona Advocacy Network declares that public funding in its home state “has allowed candidates who want to represent the voters run for office and win.”⁴ The Center for Governmental Studies writes in its 2003 report that “public funding...enhances voter choice and produces more democratic institutions of government” (8). Common Cause echoes these thoughts on their website: “It’s time to get our elected officials out of the fundraising game and insist they do the job we elected them to do: tackle important issues and listen to their constituents.”⁵ Claims that increased electoral competition and candidate entry move incumbents closer to the median voter—a formalized version of “listening to constituents”—rest on a firm foundation in political science, too (Downs 1957).⁶ In this paper, I use two design-based techniques for causal inference to confirm that public funding in state legislatures has leveled the financial and electoral playing fields, as advocates had predicted it would. But rather than make legislators more responsive

¹<http://www.theodore-roosevelt.com/images/research/speeches/sotu7.pdf>

²<http://www.ncsl.org/legislatures-elections/elections/public-financing-of-campaigns-overview.aspx>

³http://www.followthemoney.org/press/Reports/Elections_and_Public_Financing.pdf

⁴<http://www.azadvocacy.org/issues-sp-2005009908/clean-elections>

⁵<http://www.commoncause.org/site/pp.asp?c=dkLNK1MQIwG&b=4773857>

⁶ Ansolabehere, Snyder, and Stewart (2001) presents evidence that “competition exerts some pressure on candidates to fit with their constituents” (abstract). Griffin (2006) likewise finds that “elected officials who represent more competitive districts are indeed more responsive to their constituents’ preferences” (abstract). Many other papers come to similar conclusions (e.g., Burden 2004; Erikson and Wright 2000; MacRae 1952).

to constituents, public funding has caused a significant increase in legislative polarization and candidate divergence.⁷

If public funding is successful in leveling the financial and electoral playing fields, why does the new electoral landscape—in which individuals and not interest groups dominate campaign financing—lead to so much polarization? The answer is that individuals, as it turns out, prefer to support more extreme candidates than do access-oriented interest groups in state legislative campaigns. Public funding limits the influence of access-oriented interest groups and makes candidates more dependent on individual donations, leaving moderate incumbents disproportionately worse off financially. The increase in electoral competition thus coincides with the decreasing electoral influence of the most financially-abled moderate donors and a reduction in the financial and electoral fortunes of more moderate incumbents. The evidence therefore suggests that access-oriented interest groups play a key role in generating the incumbency advantage and act as a buffer against polarization. The mechanism I uncover is thus consistent with a new and growing literature on the polarizing effects of individuals vs. groups in American politics at both the federal and state level.⁸

To establish these results, I employ data on state legislative elections, campaign contributions, and legislator roll-call ideology, and I examine the differences between states and years with and without public funding in place. Specifically, I employ a difference-in-differences regression discontinuity design that measures how the causal effects of incumbency on campaign receipts, on election results, and on roll-call behavior change over time in states that implement public funding relative to those that do not.

The paper is organized as follows. In the next section, I provide a brief background on public funding programs in state legislatures. In the subsequent section, I describe the datasets used in the analysis and preview the findings. The following three sections present the estimated effects of public funding on, in order, the financial advantage of incumbents, their electoral advantage,

⁷In studying the public funding of elections, the paper builds on an existing empirical literature which focuses on its effects on electoral competition. In Appendix B, I review the existing literature on competition, summarizing the data, methods, and conclusions of each paper. Perhaps because of the different states and time periods studied, these papers are divided on whether public funding increases competition (Donnay and Ramsden 1995; Malhotra 2008; Mayer, Werner, and Williams 2006; Miller 2011; Stratmann 2009; Werner and Mayer 2007) or does not (Government Accountability Office 2010; Gross, Goidel, and Shields 2002; Jones and Borris 1985; Mayer and Wood 1995; Powell 2012; Primo, Milyo, and Groseclose 2006).

⁸Bonica (2013) documents the more moderate preferences of group donors at the federal level. Barber (2013) presents evidence that contribution limits increase polarization when they prevent groups from donations but decrease it when they prevent individuals from donating.

and their roll-call behavior in the legislature. After considering explanations for the way in which public funding causes candidates to diverge, I conclude.

1 Background

In recent years, Arizona, Connecticut, and Maine have implemented new “clean election laws” which offer public funding to legislative candidates in exchange for foregoing all other contributions.⁹ Important for the analyses below, Arizona’s and Maine’s programs were passed by initiative, and Connecticut’s by the legislature in the aftermath of a scandal over campaign contributions involving the governor. As such, the strategic considerations of legislators themselves are unlikely to have played a role in the timing of the programs. Although the analysis never makes the unrealistic assumption that these reforms were “randomly assigned,” it employs a “parallel trends” assumption (discussed later in the paper) that is especially plausible given this history.

In passing these reforms, these three states have followed Minnesota and Wisconsin, which implemented similar, albeit smaller, public funding programs for legislative candidates in the 1970s.¹⁰ These are the five states whose public funding programs I study in this paper.¹¹ For simplicity, the statistical analyses compute an average effect over these five states; this strategy provides the single most informative point estimate for understanding the typical effects of a typical public funding program in the U.S. states. Where possible, I test for whether the effects appear stronger for the more recent clean election states. Consistent with recent literature, clean election laws appear to have a larger impact than partial public funding programs (e.g., Miller 2014).

⁹In *Arizona Free Enterprise Club v. Bennett*, the Supreme Court struck down one aspect of Arizona’s clean election law—namely, a “trigger” in which candidates could receive additional funding when their opponent received more donations. The main components of the law, including the availability of public funds for candidates, remain in place. See for example <http://www.brennancenter.org/legal-work/arizona-free-enterprise-club-v-bennett>. Two of the states, Arizona and Maine, implemented term limits at the same time as public funding. All results are robust to the exclusion of these two states (see Appendix A).

¹⁰In the aftermath of *Arizona Free Enterprise Club v. Bennett*, the Wisconsin legislature has voted to remove its public funding program (see for example <http://www.wisconsinwatch.org/2011/06/30/campaign-financing-dead-in-wisconsin/>). However, this decision occurs subsequent to the end of the timeframe employed in the analyses below.

¹¹Hawaii also passed a public funding program for its state legislature in 1978. However, because the program was not indexed to inflation, and because it was already small in the first place, it has never provided a meaningful amount of funding to candidates. For more information, see for example: <http://www.commoncause.org/site/pp.asp?c=dkLNK1MQIwG&b=8629869>. As such, I do not consider it to be a publicly-funded state. Regardless, its inclusion as treated, control, or neither does not change estimated effects in a noticeable manner.

The programs disburse public money to candidates who volunteer after meeting certain qualifications. These qualifications differ by state¹² but typically require that the candidate first obtain a relatively small number of contributions from donors to demonstrate her viability. Once candidates select into public funding, they no longer can receive other donations.

The decision to “opt-in” to public financing is a strategic one; as a result, I always analyze outcomes based on the state-level presence of a public funding program, avoiding bias from selecting on candidates who choose to participate. However, participation rates are high. In 2008, for example, the Arizona and Maine lower houses averaged a 95.4% participation rate, while their state senates averaged a 94.3% participation rate; in 2008, the Connecticut state senate had a 94.4% participation rate (Gleason et al. N.d.). This is important because it suggests the potential for public funding to have significant effects on the financial connections of incumbents.

With the vast majority of candidates opting into these programs, few incumbents can reap the benefits of their increased fundraising ability, and few candidates are permitted to receive significant interest group donations. The Connecticut State Elections Enforcement Commission, for example, reports that in 2008 and again in 2010, 97% of all contributions to all candidates for the legislature came from individuals, while less than 50% came from individuals in 2006 before the program took effect. The lack of interest group financial influence after public funding is likely due to the overwhelming draw of public funding for almost all candidates. The relative ease of qualifying for public funding and the programs’ typically generous disbursements make opting out of the program costly and undesirable. Fundraising can be an unpleasant task, and public funding provides the opportunity to avoid it. Francia and Herrnson (2003) and Miller (2014), for example, show how public funding leads candidates to devote far less time to fundraising.¹³

2 Data

I employ three main datasets that provide information at U.S. state legislatures.

¹²For a full account of these differences see, e.g., <http://www.ncsl.org/legislatures-elections/elections/public-financing-of-campaigns-overview.aspx>

¹³Although difficult to test empirically, we can also speculate that opting out of public funding in state legislative races could itself be an unpopular move with voters, leaving the candidate open to accusations of corruption and ties to “special interests.”

State legislative election returns are from ICPSR dataset 34297 (Klarner et al. 2013). The dataset contains individual election returns for state legislative candidates from 1967-2010. I remove state-chambers with multi-member districts, as well as Nebraska’s non-partisan unicameral legislature. I calculate vote shares as shares of the two-party vote, and I remove all elections in which a third-party candidate received the most, or second most, votes. I omit observations in which the outcome is observed subsequent to a redistricting cycle.¹⁴

Raw records of state legislative campaign donations comes from The National Institute on Money in State Politics (<http://www.followthemoney.org>). This financial dataset provides all recorded donations made to candidates for state offices from 1990-2010 at the level of the donor. The dataset uses state disclosure requirements to identify donors as individuals or groups, allowing for separate analyses of the two, and it provides sector codings that separate non-ideological interest groups—those thought to be “access-oriented” in their donation behavior (e.g., Snyder 1992)—from single-issue groups and party committees. Ideological groups are those organized for a single issue. I include in this category any groups that the dataset identifies explicitly as single issue (e.g., PACs on abortion, guns rights, and other such causes). I define access-oriented groups, following previous literature (e.g., Snyder 1992), as all groups that are (a) not single-issue groups, (b) not coded as a labor group, because such groups are known to be extremely partisan in their donation behavior (e.g., Snyder 1992), and (c) not affiliated with either party, e.g., a state party committee which disburses party money to selected partisan candidates. The remaining groups, which I call “access-oriented,” are primarily corporations and other business entities. Though these groups might well have ideological positions (e.g., Bonica 2013), their primary goal is to gain access to those in office and, as such, they exhibit starkly different donation behavior than ideological groups (Fournaies and Hall N.d.; Snyder 1992).

Roll-call based estimates of the ideological positions of state legislators, called NP Scores, are from Shor and McCarty (2011) and cover the years 1993-2011.¹⁵ NP Scores are normalized so that liberal legislators have more negative scores and conservatives more positive scores; for the

¹⁴Information on off-cycle redistrictings for 1990–present was generously provided by Carl Klarner. For elections before 1990—only used in the analysis on vote share—I assume redistricting occurs in years ending in ‘2’. For observations with off-cycle redistricting, this coding scheme will simply add noise and not bias towards finding an effect.

¹⁵The dataset is available at <http://americanlegislatures.com/data/>.

Table 1 – Summary Statistics. Compares publicly-funded and non-publicly-funded state legislative elections (upper and lower chambers). Incumbents’ vote share and share of contributions are lower when public funding is in place. Donors contribute less money, overall, when public funding is in place, and access-oriented interest groups in particular donate much less. Yet, incumbents exhibit more extreme roll-call behavior when public funding is in place.

	Full Sample		Publicly-Funded	Not Publicly-Funded
	Years	Mean [N]	Mean [N]	Mean [N]
Incumbent Vote Percentage	1968–2010	76.62% [76,738]	69.21% [6018]	77.25% [70,720]
Incumbent Money Percentage	1990–2010	80.03% [27,203]	66.19% [1947]	81.10% [25,256]
Total Individual Donations	1990–2010	\$43,882 [27,203]	\$23,989 [1947]	\$45,556 [25,256]
Total Non-Ideological Group Donations	1990–2010	\$63,663 [27,203]	\$15,271 [1947]	\$67,736 [25,256]
Absolute NP Score (Extremism)	1993–2011	0.71 [33,727]	0.86 [2167]	0.70 [31,560]

Note: Differences in year ranges are due to data availability. Total donations are in inflation-adjusted 1990 dollars. NP Scores measure legislator ideology and are from Shor and McCarty (2011). The absolute value of NP Scores roughly indicates more extreme ideology.

purposes of a rough summary, the absolute value of the scores indicates legislator extremism, with larger scores pointing to more extreme voting records.

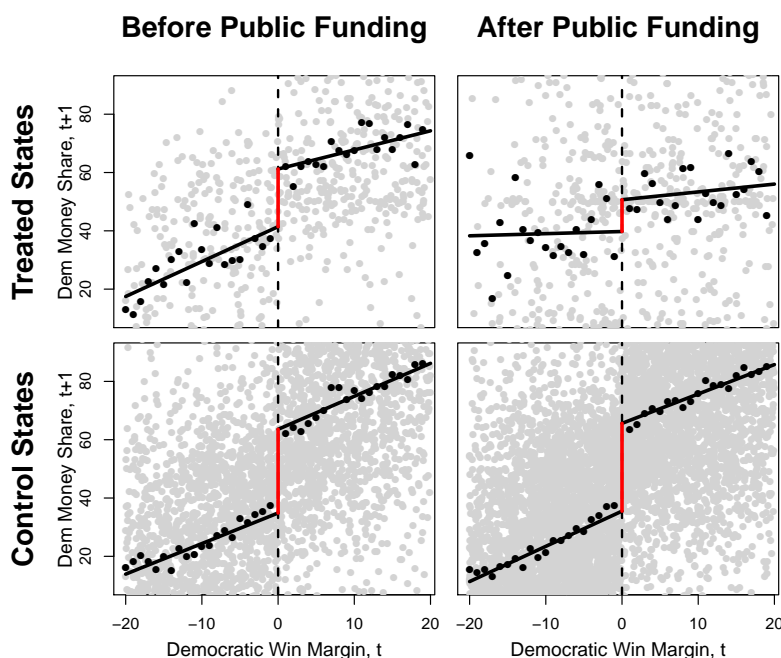
All three datasets are unbalanced, either because election cycles are not the same in all states or because of the differing availability of donation records. Appendix C provides exact information about the year ranges covered for each state in the sample.

Table 1 offers an overview of the datasets used in the analyses, including the years covered by each variable and the sample sizes with and without public funding in place. Although far cruder than the actual analysis, the simple differences between the two settings foreshadow the findings. Incumbents in elections with public-funding programs in place have a lower average vote share (first row), lower average share of all campaign contributions (second row), and are more ideologically extreme (fifth row) than other incumbents. And while both individual donations and group donations are lower with public funding in place, access-oriented interest group donation patterns differ the most across the two contexts (third and fourth rows). Access-oriented interest groups donate an average of almost \$68,000 per candidate when public funding is not in place, but only \$15,271 with public funding in place, a 77% difference. Individual donations, in contrast, are about 50% lower.

These raw differences are not themselves causal estimates. They compare across states and over time in ways that are likely to induce bias. The remainder of the paper applies econometric methods for causal inference to show that the broad picture outlined in Table 1 is, nevertheless, telling.

3 Financial Effects of Public Funding

Figure 1 – Change in the financial incumbency advantage across treated and control states: U.S. state legislatures, 1990–2010. In all cases, incumbents possess a large financial advantage, as shown by the jump in the Democratic money share for the next election cycle when the Democratic win margin in the current election cycle switches from negative to positive. There is a pronounced decrease in this jump after treated states implement public funding; no such decrease is seen in control states.



Note: Dark points are averages in 1 percentage-point bins of the Democratic vote share winning margin. Black lines are from OLS. Includes both state upper and lower houses.

The most immediate goal of campaign finance reform is to change the distribution of campaign funds, and in particular to remove the advantage that some candidates—primarily incumbents—hold over others. Any arguments over the effects of campaign finance reform on incumbent behavior depend upon this first link. I begin, therefore, by investigating how public funding reforms affect the financial advantage of incumbents in state legislatures.

Figure 1, in a format that will occur repeatedly in the paper, presents a graphical approximation of the difference-in-differences regression discontinuity design estimator. The regression discontinuity design (RDD) (Imbens and Lemieux 2008; Lee 2008) isolates the causal effect of party incumbency on, in this case, campaign contributions by examining the “jump” in contributions a party receives when districts go from just barely electing a member of the opposite party (when the win margin in the graphs is negative) to just barely electing a member of the party of interest. In the graphs I use the Democratic party, but this choice is arbitrary because of symmetry and does not change the results. The logic of the estimator is that we can remove the selection bias problem—namely, that incumbents are a special subgroup of all candidates—by focusing on elections that are so close that the outcome is quasi-random. Among these elections, the assignment of one candidate or the other to represent the district is as-if random, so we can analyze these races similar to the way we would analyze a randomized experiment.¹⁶

The two columns of the top row show the change in the financial incumbency advantage (the “jump”) for the treated states—those that implement public funding programs. The bottom row shows the same change in the control states—those that never implement a public funding program. The difference-in-differences estimator is similar to the difference between the change in jumps in the top row and the same change in the bottom row of the figure.¹⁷

In all four cases, there is a large jump in the share of contributions flowing to the Democrat in the next election cycle when the Democrat switches from losing to winning the current election (i.e., when the Democratic vote share winning margin becomes positive). But comparing across cases in the top row, which are the states that switch from not having public funding to having it, we can see that public funding leads to a marked decrease in the advantage. This is not the result of any nation-wide decrease in the financial incumbency advantage; over the same time period, states that do not implement public funding see no meaningful change in the size of the “jump” (the bottom row).

¹⁶Technically this is a simplification. Rather than treat close races as an experiment, we must introduce a small amount of modeling to extrapolate to the “true” experiment, which occurs when the win margin is exactly zero. The formal analysis below adds these necessary details.

¹⁷Because not all the treated states implement public funding at the same time, the difference-in-differences uses a different control set for each one (matched properly based on the timing of the treatment). The graph, on the other hand, simply defines the control group before treatment as all observations before the year 2000, and the control group after treatment as all observations including and after the year 2000. As a result it is similar, but not identical, to the actual estimation strategy. Though crude, it gives a clear insight into the findings.

To implement the difference-in-differences regression discontinuity design more formally, I use OLS to estimate equations of the form

$$\begin{aligned} Dem\ Money\ Share_{id,t+1} = & \beta_1 Dem\ Win_{idt} + \beta_2 Public_{it} + \beta_3 Dem\ Win_{idt} \cdot Public_{it} \\ & + f(Dem\ Win\ Margin_{idt}) + \gamma_i + \delta_t + \epsilon_{id,t+1} \end{aligned} \quad (1)$$

where $Dem\ Money\ Share_{id,t+1}$ measures the Democratic share of all campaign contributions in state i in district d in the subsequent election, $t + 1$. The variable $Dem\ Win_{idt}$ is the RDD incumbency indicator; it takes on the value 1 if the Democratic candidate wins the current in election in time t , and 0 if the Republican wins. The variable $Public_{it}$ takes the value 1 if a public funding program is in place in state i at time t , and 0 otherwise. The function $f(Dem\ Win\ Margin_{idt})$ represents some specification of the “forcing variable,” the Democratic vote share winning margin in the current election in time t . This function serves to extrapolate to the discontinuity. Following best practices, I present results using a variety of specifications and bandwidths to demonstrate the robustness of the result. For this and all subsequent analyses, Appendix A presents further estimates at a variety of bandwidths and specifications that show the stability of the results. Finally, γ_i and δ_t represent state and year fixed effects, respectively.

The quantity of interest is β_3 , the coefficient on the interaction of $Public$ and $Dem\ Win$, which measures how much public funding changes the incumbency advantage—in this case, the financial incumbency advantage. To see this, first consider β_1 . This parameter captures the RDD estimate—the difference between districts that barely elect Democrats and those that barely elect Republicans—when public funding is not in place and $Public$ thus equals 0. When $Public$ equals 1—when public funding is in place—both barely Democrat and barely Republican districts see an average change in the level of $Dem\ Money\ Share$ measured by β_2 . The discontinuity or “gap” between districts that barely elect Democrats and those that barely elect Republicans changes by β_3 .¹⁸

¹⁸Thus, to be clear, β_3 is the quantity of interest and we do not need to add back in β_2 when calculating the change in the incumbency advantage caused by public funding. Because both Democratic barely winners and barely losers receive the same β_2 effect from public funding, β_2 cancels out in considering the change in the advantage after public funding.

This setup allows for unobserved differences between states as well as unobserved common time trends. It provides a valid causal estimate if treated states would have *changed* over time like the control states if they had not passed campaign finance reform—a much weaker assumption than, for example, supposing that treated states would have had the same outcomes as control states if they had not passed campaign finance reform. To relax this assumption, I also consider alternate specifications in which states have varying time trends. All results are robust to these alternatives (see Appendix A). In addition, Appendix A presents results that replace the state fixed effects with dummies for each state-chamber, ensuring that results are not driven by pooling across the two chambers within each state. Estimates remain nearly unchanged.¹⁹

Three features of the RDD estimator are worth discussing, briefly. First, the RDD estimates a *local* effect, i.e., an effect specific to districts with close elections. This focus is natural in the present study since public funding’s effects are most important in districts where both parties have a chance of winning the seat. Second, the RDD estimates a “party” advantage since the winner at time t need not run again at time $t + 1$. This is a non-issue for the roll-call analysis since it focuses on behavior before the election at time $t + 1$ (thus avoiding any issues about the decision to run again), but it does slightly change the interpretation of the analyses on donations and vote share. Finally, several papers have questioned the use of RDDs with U.S. House data, due to evidence of imbalances between barely-winners and barely-losers (Grimmer et al. 2012; Caughey and Sekhon 2011). This is not an issue in this paper, which uses state legislative data, because these imbalances are not found in these elections (Eggers et al. 2013). To be sure that these three factors do not pose a problem, Appendix A also presents results using the incumbency advantage estimator from Gelman and King (1990). Substantive findings are unchanged.

Table 3 presents estimated results at three different bandwidths and specifications of the forcing variable. In all cases, β_3 is estimated to be large and negative. In the middle column, for example, I use a 5 percentage-point bandwidth around the discontinuity and a local linear specification of the forcing variable, estimated separately on each side of the discontinuity. Here, Democratic (or Republican) incumbency is estimated to cause a more than 22 point increase in the percentage of donations flowing to the Democratic (or Republican) party when public funding is not in place. After public funding is put in place, party incumbency causes only a 4.7 percentage point increase

¹⁹I have also tested for whether effects differ across lower and upper houses. No evidence is found.

(22.21 - 17.51 = 4.7)—a decrease in the advantage of more than 17 percentage points. By providing funds to challengers and simultaneously preventing incumbents from raising unlimited funds, public funding almost completely removes the financial advantage of incumbents.

Table 2 – Public funding and the financial incumbency advantage: U.S. state legislatures, 1990–2010. RDD regressions estimating the change in the financial incumbency advantage when public funding is put in place. Across specifications and bandwidths, public funding causes a marked decrease in the advantage.

	Dem Money Share	Dem Money Share	Dem Money Share
Dem Win	24.41 (2.66)	22.21 (1.98)	25.62 (2.23)
Public Funding · Dem Win	-20.37 (5.24)	-17.51 (4.10)	-21.98 (10.46)
Public Funding	17.11 (3.96)	13.93 (3.24)	11.91 (7.08)
N	2421	4020	27203
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

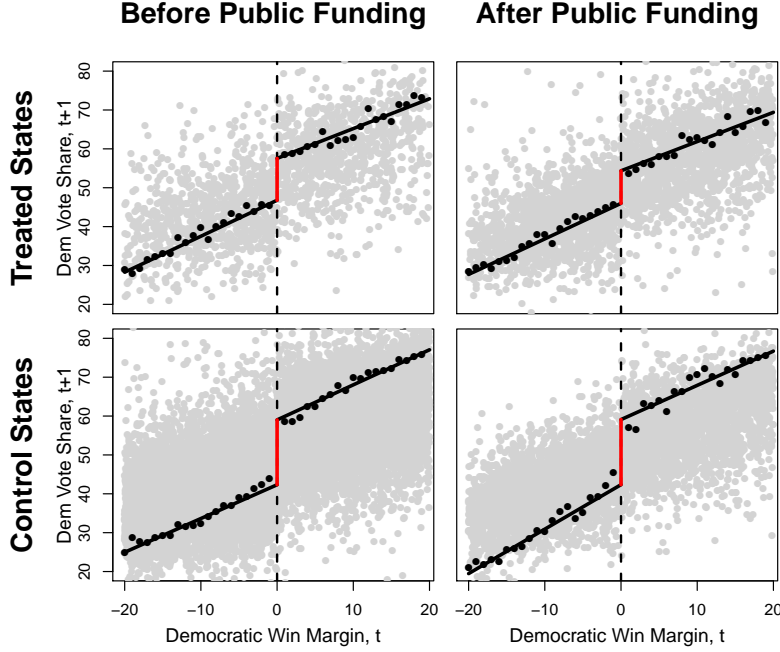
State-clustered robust standard errors in parentheses. All columns include a function of the running variable (Democratic vote share winning margin) using the given specification (either Local Linear or Cubic). Analyses include both state lower and upper houses.

This section has shown how public funding accomplishes its most immediate goal: it profoundly reshapes the financial landscape of elections. Incumbents (and their parties) no longer receive enormous funds just in return for holding office. The following sections consider the electoral consequences of this change in the financial landscape.

4 Electoral Effects of Public Funding

Paralleling the analysis on campaign contributions, Figure 2 shows the electoral advantage of incumbents before and after public funding in treated and control states, respectively. Looking across the top row, we see that public funding decreases the incumbency advantage; when Democrats go from barely losing to barely winning in the current electoral cycle, the jump in their vote share in the subsequent election is no longer as large after public funding. In the bottom row, we see that,

Figure 2 – Change in the electoral incumbency advantage across treated and control states: U.S. state legislatures, 1968–2010. In all cases, incumbents possess a large electoral advantage, as shown by the jump in the Democratic vote share for the next election cycle when the Democratic win margin in the current election cycle switches from negative to positive. There is a pronounced decrease in this jump after treated states implement public funding; no such decrease is seen in control states.



Note: Dark points are averages in 1 percentage-point bins of the Democratic vote share winning margin. Black lines are from OLS. Includes both state upper and lower houses.

over the same time period, there is no change in the incumbency advantage in the control states, which do not implement public funding.

Like before, we can also investigate this effect more formally by using OLS to estimate equations of the form

$$\begin{aligned}
 Dem\ Vote\ Share_{id,t+1} = & \beta_1 Dem\ Win_{idt} + \beta_2 Public_{it} + \beta_3 Dem\ Win_{idt} \cdot Public_{it} \\
 & + f(Dem\ Win\ Margin_{idt}) + \gamma_i + \delta_t + \epsilon_{id,t+1} \quad (2)
 \end{aligned}$$

where all variables are defined as before and the outcome variable is now $Dem\ Vote\ Share_{id,t+1}$, the Democratic share of the two-party vote in state i in district d in the subsequent electoral cycle. As before, the quantity of interest is β_3 , the coefficient on the interaction of $Public$ and $Dem\ Win$.

As Table 3 shows, this coefficient is consistently negative. Public funding decreases the electoral incumbency advantage by as much as 8.19 percentage points. For example, again using the middle column, the party incumbency advantage is estimated to be 10.45 percentage points when public funding is not in place, but only 4.18 percentage points after public funding ($10.45 - 6.27 = 4.18$).²⁰ Thus, public funding causes the electoral incumbency advantage to decrease by more than half. These estimates are consistent with those in Werner and Mayer (2012), who study state lower houses and find that public funding is associated with a roughly 50% decrease in the electoral incumbency advantage.

To convey the most information, Table 3 uses the entire election dataset, running back to 1968. However, in Appendix A I show that the results are substantively similar—if anything, even stronger—if I restrict to only the period 1990–2010, to make the time period comparable to the other analyses. Because the effect becomes stronger, it suggests that clean election states exhibit larger effects than partially funded states, since restricting the time period in this manner removes significant treatment variance in Minnesota and Wisconsin (who implemented their programs in the 1970s).

That public funding affects electoral outcomes to such an extent may seem surprising given the literature on campaign spending, which by and large finds that it has modest effects on vote share (e.g., Abramowitz 1988; Erikson and Palfrey 2000; Gerber 1998, 2004; Green and Krasno 1988; Jacobson 1978, 1990; Levitt 1994). However, the marginal effect of challenger spending is likely to be greater than that of incumbent spending, because incumbents already possess much of the brand value that spending can purchase (Jacobson 1978). By funding more challengers and reducing incumbent war chests, public funding can therefore deliver large electoral effects. It funnels money to candidates for whom the marginal value is high, while at the same time taking money away from candidates for whom its marginal value is low.

²⁰As discussed previously, the coefficient on *Public* reflects a change in the level of funding for both barely winning and barely losing Democratic districts after public funding, and thus differences out in the calculation of the incumbency advantage with public funding in place and in the calculation of how much this effect has changed.

Table 3 – Public funding and the electoral incumbency advantage: U.S. state legislatures, 1968–2010. RDD regressions estimating the change in the electoral incumbency advantage when public funding is put in place. Across specifications and bandwidths, public funding causes a marked decrease in the advantage.

	Dem Vote Share	Dem Vote Share	Dem Vote Share
Dem Win	10.52 (0.87)	10.45 (0.89)	12.08 (1.16)
Public Funding · Dem Win	-5.05 (1.46)	-6.27 (1.69)	-8.19 (2.75)
Public Funding	2.98 (0.95)	2.70 (1.12)	4.50 (1.81)
N	6419	10538	72492
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

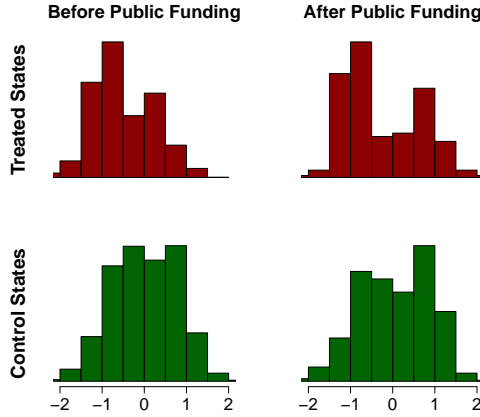
State-clustered robust standard errors in parentheses. All columns include a function of the running variable (Democratic vote share winning margin) using the given specification (either Local Linear or Cubic). Analyses include both state lower and upper houses.

5 Candidate Ideology

Public funding brings profound changes to the electoral landscape, eliminating the financial advantage of incumbents and reducing their electoral advantage to less than half its typical size. Advocates relied on these “first-stage effects” to, in turn, produce candidate convergence through competition. Yet early evidence shows that public funding and the resulting competition produces *more* divergence, not less. In a preliminary study of Arizona and Maine, Masket and Miller (2012) present correlational evidence that incumbents who enter after clean election laws are in effect are more extreme, on average, than those who entered the legislature before the laws were in effect. The results in this section are consistent with this view, using data on a wider set of states—both those that implement funding and those that do not—and using a design-based approach to estimate the causal effects of the programs themselves.

Figure 3 plots the distribution of estimated legislator ideology, in the form of NP Scores, before and after public funding in the treated states (top row) and control states (bottom row). NP Scores are estimated using traditional random-utility based maximum likelihood scaling methods in Shor and McCarty (2011), based on state legislative roll-call data. The scores range, in the

Figure 3 – Changes in roll-call polarization across treated and control states: U.S. state legislatures, 1993–2010. Treated states (those that implement public funding) exhibit a large increase in polarization as measured by roll-call ideology, while control states exhibit no such change.



Note: Histograms measure the distributions of NP Scores (Shor and McCarty 2011).

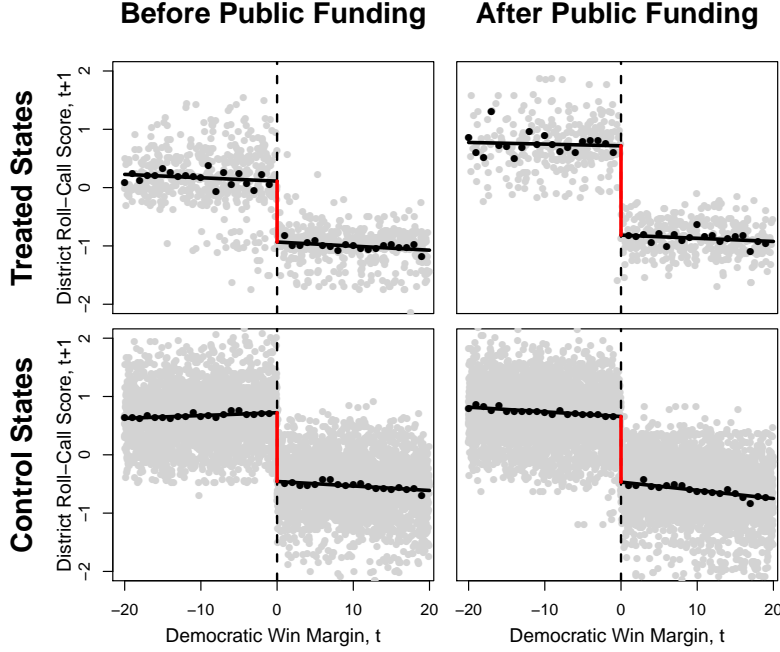
present context, from a minimum of -3.39 to a maximum 5.25, with 99% of legislators’ scores between 2 and -2. Smaller numbers indicate more liberal voting and larger numbers indicating more conservative voting. The method uses common survey answers and shared roll-call votes to “bridge” across legislatures and over time, making the estimates comparable across states and time, even though the set of bills that are voted on differ.

As the top row shows, public funding causes the distribution to become more polarized, with few legislators left in the middle and two “humps” representing the Democrats (to the left) and Republicans (to the right). This increase in polarization is not mirrored in the control states, where the change over the same time period appears much less striking. Public funding thus appears to increase overall polarization in the legislature. As I show next, this increase in overall polarization is closely linked to polarization between candidates *within* a district. It is not the case that overall polarization is increasing while, district by district, legislators are actually moving closer to their median constituents.²¹

Figure 4 uses the same RDD approach from the previous sections, but with legislator roll-call behavior as the outcome. As a district switches from barely electing a Republican to barely electing

²¹This is consistent with evidence for the U.S. House presented in McCarty, Poole, and Rosenthal (2009), who find that “congressional polarization is primarily a function of the differences in how Democrats and Republicans represent the same districts rather than a function of which districts each party represents” (abstract).

Figure 4 – Changes in the ideological gap across treated and control states: U.S. state legislatures, 1993–2010. In all cases, there is a large gap in the roll-call behavior of districts that receive either a Democrat or a Republican from a close electoral result, as shown by the downward jump in the NP Score when the Democratic win margin in the current election cycle switches from negative to positive. There is a pronounced growth in the size of this downward jump after treated states implement public funding; no such change is seen in control states.



Note: Dark points are averages in 1 percentage-point bins of the Democratic vote share winning margin. Black lines are from OLS. Includes both state upper and lower houses.

a Democrat, the “jump” now reflects the differential way in which the two partisans represent the same (hypothetical) district. In this sense, the estimated effect measures candidate divergence rather than overall polarization—what McCarty, Poole, and Rosenthal (2009) call “intradistrict divergence.” Looking across the top row, we see that this jump grows after public funding in the treated states. There is no change in this gap in the control states over the same time period.

Like before, we can also investigate this effect more formally by using OLS to estimate equations of the form

$$\begin{aligned}
 \text{District Ideology}_{id,t+1} = & \beta_1 \text{Dem Win}_{idt} + \beta_2 \text{Public}_{it} + \beta_3 \text{Dem Win}_{idt} \cdot \text{Public}_{it} \\
 & + f(\text{Dem Win Margin}_{idt}) + \gamma_i + \delta_t + \epsilon_{id,t+1} \quad (3)
 \end{aligned}$$

where all variables are defined as before and the outcome variable is now $District\ Ideology_{id,t+1}$, the estimated NP Score for the legislator in state i in district d in the subsequent legislative period. As before, the quantity of interest is β_3 , the coefficient on the interaction of *Public* and *Dem Win*.

As shown in Table 4, public funding produces a large increase in the ideological gap between representatives of the “same” hypothetical district. Without public funding, the random assignment of a Democratic legislator instead of a Republican produces a 1.16 point decrease in the district’s NP Score, i.e., significantly more liberal voting.

Table 4 – Public funding and polarization: U.S. state legislatures, 1993–2010. RDD regressions estimating the change in the party representation gap when public funding is put in place. Across specifications and bandwidths, public funding causes a marked increase in the gap between Democrats and Republicans.

	NP Score	NP Score	NP Score
Dem Win	-1.15 (0.07)	-1.16 (0.07)	-1.16 (0.07)
Public Funding · Dem Win	-0.34 (0.10)	-0.34 (0.10)	-0.35 (0.11)
Public Funding	0.28 (0.06)	0.29 (0.05)	0.29 (0.07)
N	2497	4153	32292
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses. All columns include a function of the running variable (Democratic vote share winning margin) using the given specification (either Local Linear or Cubic). Analyses include both state lower and upper houses.

In the RDD framework, the choice of a Republican or a Democratic legislator is random, meaning that the set of districts that receive a Democrat are directly comparable to those with a Republican—including in their ideological preferences. Under fully convergent representation, there should thus be *no difference* in the way the two sets of districts are represented. Since the districts have the same preferences, their legislators should cast the same roll-call votes—and thus have the same NP Scores—regardless of their party. Instead, we find a large gap, consistent with previous literature (Ansolabehere, Snyder, and Stewart 2001; Bafumi and Herron 2010; Lee, Moretti, and Butler 2004; McCarty, Poole, and Rosenthal 2009). Though these legislators represent the same constituents, they vote in radically different directions depending on their partisan affiliations.

After public funding, this gap becomes even larger. Indeed, after public funding this effect becomes -1.51 ($-1.16 + -0.35$), a 30% increase in the party representation gap. There is widespread legislator divergence in American politics today, but public funding has significantly increased this gap in the state legislatures where it operates. In the next subsection, I explore one mechanism that underlies this effect.

5.1 Publicly Funded Divergence

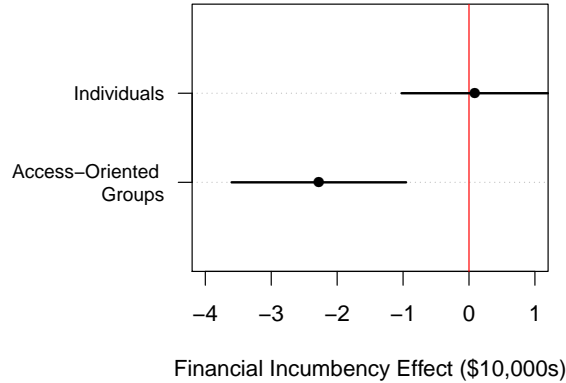
Why does public funding lead to such marked candidate divergence? Although there are many possible reasons, a major explanation lies in the way public funding affects access-oriented, non-ideological interest group influence over elections.

Access-oriented interest groups are disproportionately active in campaign finance, and especially in targeting incumbents. As Table 1 suggested, it is these groups who must reshape their contribution behavior most after public funding is put in place. To show this more formally, I estimate the effect of public funding on the financial incumbency advantage (equation 1), but using total access-oriented interest group donations and total individual donations, respectively, as the dependent variables. Figure 5 plots the estimated effect for each, i.e., the coefficient on the interaction of *Public* and *Dem Win* from equation 1 using each outcome variable. The change in the size of the financial incumbency advantage from public funding is entirely driven by access-oriented interest groups, who are forced to reduce the amount of money they allocate based on incumbency by an average of over \$20,000 per race. Individuals do not change the manner in which they reward incumbents financially at all based on public funding.

This differential effect is important because access-oriented groups differ in their ideological preferences from other donors. In federal elections, access-oriented interest groups tend to be moderate in their donation patterns (e.g., Bonica 2013). Almost all such groups, when scaled based on their contribution behavior, appear directly in between the ideological estimates of the two parties' legislators (Bonica 2013). The same strategic interest group preference for moderate candidates exists at the state level.

Figure 6 shows the differing contribution behavior of access-oriented interest groups and individuals across levels of candidate ideology, estimating the relationship using a simple kernel smoother. In the left panel, access-oriented groups display a clear preference for moderate candidates. To

Figure 5 – Changes in the financial incumbency advantage caused by public funding, by donor type. Plots estimates for β_3 from equation 1, the effect of public funding on the financial advantage that incumbents receive from individual donors and access-oriented interest groups, respectively. Public funding causes access-oriented interest groups to direct far less money to incumbents, while individuals make no such change.

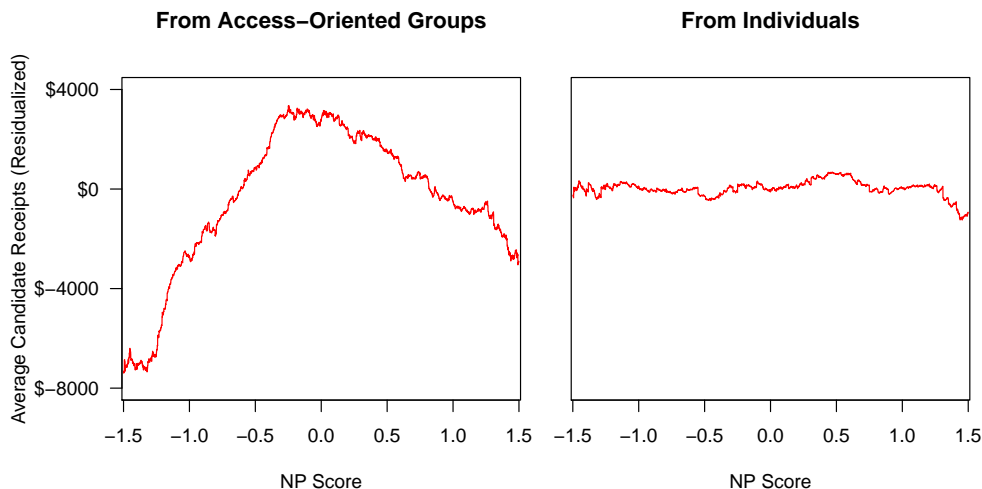


Note: Estimates are from OLS of analogous form to Equation 1, but with the specified outcomes variables. Black lines indicate 95% confidence intervals constructed from state-clustered standard errors.

reduce noise, contributions are de-measured by state and party. “Moderate” candidates—those with an NP score around zero—receive on average \$12,000 more than an extreme liberal candidate (with NP score around -1.5) and about \$7,000 more than an extreme conservative candidate (NP score approximately 1.5). Given that the average total contributions to a candidate from access-oriented groups is about \$64,000 (see Table 1), these are large differences. The right panel presents the same information for individual donors. No pattern is seen. Individual donors do not prefer moderates the way access-oriented groups do.

The composition of candidates’ campaign receipts—their “portfolios”—reveals the moderate nature of these access-oriented interest groups, as well as the way in which public funding removes their influence. To examine these portfolios, I use the method from Hall and Snyder (2013) to first scale donors in terms of their contribution behavior, which simply calculates the average proportion of all donations from each donor that that donor gives to Republicans. The method is similar in spirit to other contribution-based scaling methods (e.g., Bonica 2013), and I only choose it over other methods here so that I can obtain year-by-year candidate estimates that do not include public funds. Donor scores range from 1, indicating a group that only ever gives to Republican candidates, to -1, indicating a group that only ever gives to Democratic candidates. Using these

Figure 6 – Donation patterns in state legislatures. Access-oriented interest groups exhibit a strong preference for moderate candidates; individual donors exhibit no such preference.

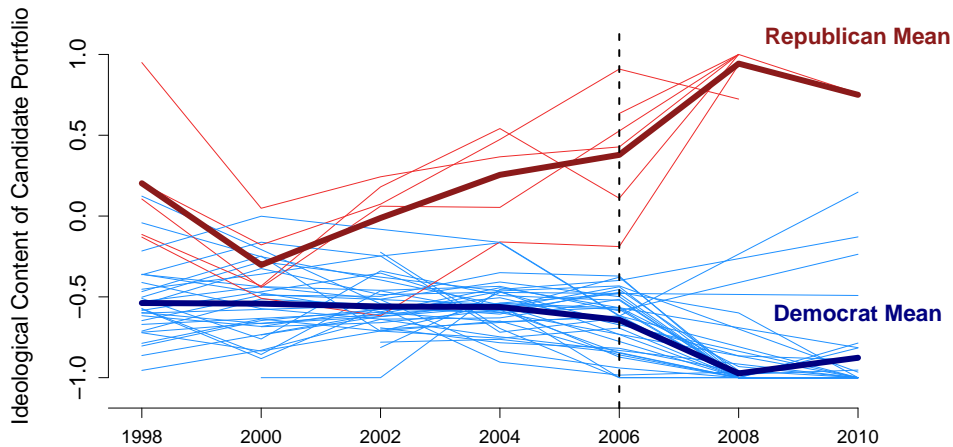


Note: The left panel presents a kernel smoother relating candidate roll-call ideology (NP score) to residualized total contributions from access-oriented groups (de-meaned by state and party). The right panel presents the same for residualized total contributions from individuals. Because of sparsity, the graphs exclude the 4.8% of observations with NP scores less than -1.5 or greater than 1.5.

donor scores, candidate ideology is then imputed as the mean donor score of all the donors the candidate received donations from (weighted by the donation amount). To keep in only reliable information, the method drops donors who give to fewer than 20 candidates over their lifetime, and drops candidates who receive donations from fewer than 20 donors over their whole career (Bonica 2013). After public funding, this means that many candidates are dropped—one reason why the main analysis on polarization uses NP scores rather than donor-based scores. But we can still see how the remaining candidates’ portfolios change.

Figure 7 presents the change over time in candidates’ portfolios in Connecticut. I focus on Connecticut because its higher thresholds for qualifying for public funding mean that more candidates receive enough contributions to have donor-based scalings. Each line in the plot represents an individual candidate’s portfolio-based ideology, i.e., the weighted average ideology of her donors over time. The graph thus only includes “straddlers,” i.e., incumbents who were in office before public funding and remained in office for at least some time afterwards. The dark bold lines are the party averages.

Figure 7 – Ideological Donor Portfolios of Connecticut State Legislative Candidates Over Time. Plots the average estimated ideology of donors to candidates. Each line represents a candidate’s portfolio over time. Dark, bold lines are the party averages. A value of 1 indicates that the candidate’s entire portfolio came from donors that only gave to Republicans; a value of -1 indicates that the candidate’s entire portfolio came from donors that only gave to Democrats. The vertical dotted line indicates the last election before public funding.



As the figure shows, candidate portfolios are almost perfectly polarized after public funding is put in place. By 2008, almost every candidate receives either a -1 score or a +1 score, meaning almost every Democratic candidate received all of her donations from donors that *only* gave to Democrats and almost every Republican candidate received all of her donations from donors that *only* gave to Republicans. This makes sense in light of the fact, mentioned earlier, that 97% of all contributions in Connecticut after public funding come from individuals. Unlike interest groups, individuals are unlikely to spread their contributions across the parties.

The difference between this pattern of contributions and that observed in legislatures without public funding is stark. Candidates in Connecticut before public funding is put in place, when groups are able to donate at will, are clustered more towards the middle of the ideological scale, as the left side of the plot shows. The purely partisan giving after the reform takes effect thus indicates, again, the lack of strategic interest group financial influence after public funding. This lack makes it harder for moderate incumbents to win, and provides an incentive for all candidates to adopt more extreme views.²²

²²For example, Democratic candidates no longer have financial incentives to please moderate, liberal business interests, and Republican candidates have new incentives to cater to the ideological wing of the party, composed mainly of individuals.

There could be other explanations for the increase in candidate divergence as well. Regardless, by preventing donors from contributing to most candidates, public funding has a major effect on the ability of strategic, access-oriented interest groups to grant a financial advantage to those in office. On its face, this effect might seem welcome to those who believe—like the advocates who pushed for public funding—that interest group money makes incumbents less responsive to constituents. The Arizona Advocacy Network, for example, says on their website that “Before Arizona voters passed Clean Elections in 1998, Big Money controlled who ran for public office and who was elected.”²³ Maine Citizens for Clean Elections declares online that voters support public funding to “break the ties between wealthy special interests and our elected leaders.”²⁴ But, perhaps surprisingly, many interest groups—those that are strategic rather than ideological—are actually *moderate* in their contribution patterns. They support incumbents who, relative to other options, offer policies likely to be closer to constituent preferences. By taking away the ability of these groups to contribute in this way, public funding disproportionately affects more moderate incumbents.²⁵

Why do these groups prefer moderates? Exploring the preferences of these groups is beyond the scope of this study, but one explanation appears especially promising. It is well understood that access-oriented interest groups prefer incumbents expected to be in office longer (Snyder 1992). These incumbents are not only more likely to attain positions of greater power in the legislature, but are more likely to be sympathetic to interest groups who have supported them over the course of many years. We also know that moderate candidates tend to do better electorally (Ansolabehere, Snyder, and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Hall and Snyder 2013). Access-oriented interest groups are thus likely to support moderates, regardless of any of their own ideological preferences, because of the longer expected time horizon these candidates offer. Whatever the source of access-oriented interest groups’ preference for moderate incumbents, the result has unexpected consequences for public funding.

²³<http://www.azadvocacy.org/issues/clean-elections>

²⁴<https://www.maine cleanelections.org/history>

²⁵Although being moderate relative to other legislators is not necessarily equivalent to being “responsive” to one’s constituents (the constituents could, after all, have extreme preferences relative to other districts), we have good reasons to believe that removing moderate incumbents means removing more responsive incumbents. Most polarization—as seen above—is the result of how the two parties represent the same constituents (Bafumi and Herron 2010; McCarty, Poole, and Rosenthal 2009), with liberal candidates offering positions to the left of the district median and conservative candidates offering positions to the right. Because candidates are positioned in this way, moderation means moving to the middle and thus being closer to the district median—hence, more “responsive.”

6 Conclusion

The first-order contribution of this paper is to evaluate the consequences of an oft-discussed policy intervention. The public funding of elections is a double-edged sword. It accomplishes the first-stage goal of advocates in leveling the financial playing field in state legislatures, and because money matters for electoral outcomes, it brings new electoral threats to previously safe incumbents. However, public funding has accomplished these goals at the cost of alienating moderate candidates. The result is greater candidate divergence in elections and more polarization in the legislature.

This does not necessarily mean that public funding *only* makes incumbents less “responsive” to constituents. It is entirely possible, for example, that severing the financial links between legislators and interest groups reduces unobserved political “corruption” at the same time as it increases polarization in the legislature. Incumbents after public funding may offer fewer favors to interest groups, although this is difficult if not impossible to measure. Either way, roll-call polarization is a salient feature of the political climate, and public funding’s effects on it are notable.

Recent events have raised doubts about the effect public funding will have on future state legislative elections. The Supreme Court has struck down the “triggers” that allowed states to provide extra funds to candidates when their opponents raise and spend more money.²⁶ However, states are still able to provide money for candidates, leaving intact the most important component of the programs, and thus “public financing...remains a powerful tool in the regulatory environment” (Miller 2014: 11). If the lack of triggers and any other erosion of the programs restores some advantage to incumbents, though, the results of this paper suggest it could also mean a reduction in legislative polarization if it opens the door for access-oriented interest groups to begin donating again.

In evaluating a significant policy reform, this paper has also presented evidence relevant to bigger theoretical questions. Why do incumbents possess such a large electoral advantage in U.S. elections? The dramatic reduction in the incumbency advantage after public funding suggests that one part of the electoral advantage is the result of the tremendous financial resources that incumbents in less-regulated electoral environments possess. Moreover, because the reduction in incumbents’ financial advantage comes almost entirely from access-oriented interest groups, the evidence strongly suggests

²⁶<http://www.brennancenter.org/legal-work/arizona-free-enterprise-club-v-bennett>

that these groups play a key role in generating the incumbency advantage, consistent with recent scholarship on the “financial incumbency advantage” (Fournaies and Hall N.d.).

Beyond supporting incumbents and protecting them electorally, I have also documented that access-oriented groups systematically support moderate legislators. Strategic interest groups desire access, but they particularly desire access to moderates. When these groups can no longer contribute financially, incumbents—but especially moderate incumbents—lose out.

Finally, what links exist between electoral competition and candidate divergence? Though electoral competition surely induces candidate convergence in the right setting, the evidence in this paper establishes quite clearly that competition is not a sufficient condition for convergence. It can produce widely varying candidate behavior depending on context. In the case of public funding, the competition produced is among extreme candidates, at least in part because of the unanticipated stifling effects the reforms have had on moderate, access-oriented interest groups, and the result is more polarization.

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Appendix A: Additional Statistical Results

Relaxing the Parallel Trends Assumption

The analysis in the paper relies on the identifying assumption that the outcome variables in the states that implement public funding would have changed in the same way, over time, as the same variables in states that did not implement public funding. This is a relatively weak assumption, as it allows states to vary in any unobserved ways that are fixed over time and allows for any unobserved time trends common across states. Nevertheless, it can be violated if states that implement public funding are already trending in a dissimilar way to how the control states are trending. To test for this possibility, Tables 5, 6, and 7 present the results with the addition of state-specific linear time trends. These trends allow states to differ from each other in a linear fashion in their trend behavior over time. As the tables show, we continue to find equally strong evidence for public funding's effects on the financial incumbency advantage, the electoral incumbency advantage, and polarization. This lends a great deal of plausibility to the parallel trends assumption.

Table 5 – Public Funding and the Financial Incumbency Advantage: Adding State-Specific Trends

	Dem Money Share	Dem Money Share	Dem Money Share
Dem Win	24.29 (2.77)	22.33 (2.04)	25.77 (2.21)
Public Funding · Dem Win	-20.25 (5.37)	-17.62 (4.30)	-22.18 (10.42)
Public Funding	14.13 (8.11)	17.07 (3.96)	13.62 (6.49)
N	2421	4020	27203
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
State Specific Time Trends	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 6 – Public Funding and the Electoral Incumbency Advantage: Adding State-Specific Trends

	Dem Vote Share	Dem Vote Share	Dem Vote Share
Dem Win	10.68 (0.89)	10.62 (0.91)	12.16 (1.17)
Public Funding · Dem Win	-4.92 (1.46)	-6.10 (1.74)	-7.93 (2.80)
Public Funding	1.12 (1.69)	0.81 (1.84)	3.02 (1.78)
N	6419	10538	72492
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
State Specific Time Trends	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 7 – Public Funding and Polarization: Adding State-Specific Trends

	NP Score	NP Score	NP Score
Dem Win	-1.14 (0.07)	-1.15 (0.07)	-1.16 (0.07)
Public Funding · Dem Win	-0.35 (0.09)	-0.35 (0.10)	-0.34 (0.11)
Public Funding	0.01 (0.11)	0.06 (0.05)	0.14 (0.07)
N	2497	4153	32292
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
State Specific Time Trends	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Term Limits

Arizona and Maine both implemented term limits at the same time as they implemented public funding programs. However, it is hard to believe that term limits could have nearly the same effects on incumbent finances as does a reform that interacts so directly with campaign finance. In addition, the results are robust to the removal of both Arizona and Maine, as shown in Tables 8, 9, and 10.

Table 8 – Public Funding and the Financial Incumbency Advantage, AZ and ME Removed

	Dem Money Share	Dem Money Share	Dem Money Share
Dem Win	23.08 (2.57)	22.73 (2.05)	25.52 (2.28)
Public Funding · Dem Win	-11.43 (5.18)	-13.10 (6.34)	-9.07 (7.38)
Public Funding	28.52 (3.30)	22.77 (3.73)	2.89 (5.26)
N	2215	3677	25831
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 9 – Public Funding and the Incumbency Advantage, AZ and ME Removed

	Dem Vote Share	Dem Vote Share	Dem Vote Share
Dem Win	10.45 (0.92)	10.44 (0.95)	12.16 (1.21)
Public Funding · Dem Win	-5.79 (1.52)	-7.22 (1.67)	-7.79 (3.13)
Public Funding	3.34 (1.41)	4.24 (1.23)	4.89 (2.32)
N	5997	9847	69212
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 10 – Public Funding and Polarization, AZ and ME Removed

	NP Score	NP Score	NP Score
Dem Win	-1.14 (0.07)	-1.14 (0.07)	-1.15 (0.07)
Public Funding · Dem Win	-0.30 (0.16)	-0.32 (0.16)	-0.36 (0.13)
Public Funding	0.21 (0.10)	0.39 (0.12)	0.28 (0.09)
N	2330	3879	31032
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Comparison to Gelman-King Results

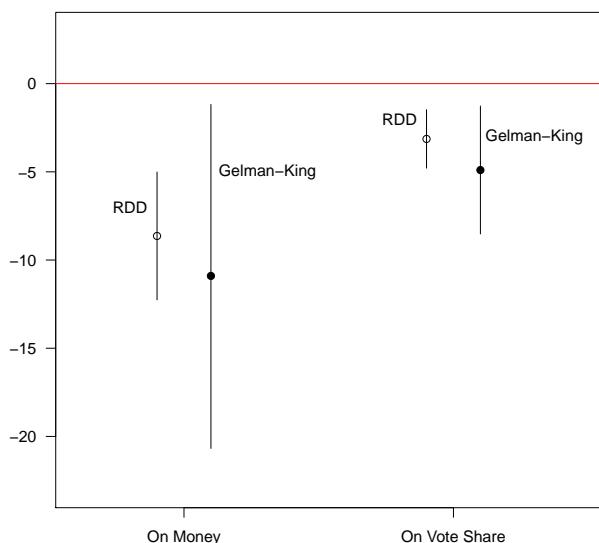
To ensure that the findings are not dependent on the choice of the incumbency advantage estimator, and to address concerns that the RDD captures the “party” incumbency advantage, I replicate the financial and electoral analyses using the estimator from Gelman and King (1990). Specifically, I estimate models of the form

$$Y_{i,t+1} = \beta_1 Inc_{it} + \beta_2 Public_{it} + \beta_3 Public_{it} \cdot Inc_{it} + \beta_4 Y_{it} + \beta_5 Party_{it} + \gamma_i + \delta_t + \epsilon_{i,t+1} \quad (4)$$

where Y is an outcome (either the share of contributions or of votes at time $t + 1$ as in the paper). The variable Inc_{it} takes the value 1 if the Democrat is the incumbent at time $t + 1$ (i.e., the Democrat won and is herself running for reelection), 0 if neither candidate is an incumbent, and -1 if the Republican is the incumbent at $t + 1$. The variable $Public_{it}$ is the usual dummy indicating a public funding program. The variable Y_{ist} represents lagged outcome variable for district i in state s at time t or the Democratic share of all contributions, and $Party_{it}$ takes the value 1 when the Democrats hold the seat after the election at time t and -1 if the Republicans do. Finally, γ_i and δ_t represent state and year fixed effects, respectively. As in the paper, the quantity of interest is β_3 , measuring the change in the incumbency advantage caused by public funding.²⁷

Figure 8 compares the estimated change in each advantage caused by public funding, for the RDD used in the paper and the Gelman-King estimator, respectively.

Figure 8 – Replicating the Analysis with the Gelman-King Estimator. Compares estimates for the change in the financial and electoral advantages caused by public funding.



As the figure shows, results are substantively identical using this alternate estimation strategy. There are two conclusions from this exercise. First, the results are not sensitive to the choice of estimator, and second, the effect appears to be largely, if not exclusively, the result of changes in the “personal” incumbency advantage.

²⁷Again, the main effect for public funding, measured by β_2 , does not enter the calculation of this change since it affects both incumbent-treated and open-seat “control” observations.

Robustness of RDD

Estimates in the paper are presented with three different bandwidths: elections within a 5% winning margin, 10%, and the whole sample (50%). To verify that these somewhat arbitrary (although customary) selections are not driving the findings, the figures below present estimates for the three analyses—on the financial incumbency advantage, the electoral incumbency advantage, and on ideological positioning in the legislature—using all bandwidths from 3% to 50% and using four possible specifications of the running variable. These specifications are: local linear, estimated separately on each side of the discontinuity, or a quadratic, cubic, or quartic specification. As the plots show, the substantive finding is unchanged across bandwidths, and specification choices are inconsequential. Although each graph presents four lines, one for each specification used, the four almost always overlap perfectly.

Figure 9 – Financial Incumbency Advantage: RDD Estimate Across Bandwidths and Specifications. Plots estimates of β_3 from equation 1 across RDD margins and bandwidths. Regardless of bandwidth or specification, public funding is estimated to cause a large decrease in the financial incumbency advantage. Note that the four lines overlap almost perfectly.

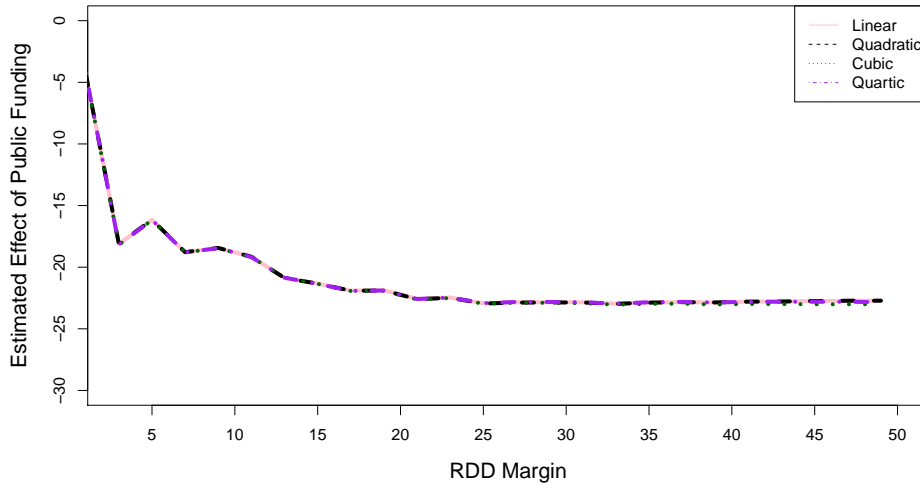


Figure 10 – Electoral Incumbency Advantage: RDD Estimate Across Bandwidths and Specifications. Plots estimates of β_3 from equation 2 across RDD margins and bandwidths. Regardless of bandwidth or specification, public funding is estimated to cause a large decrease in the electoral incumbency advantage. Note that the four lines overlap almost perfectly.

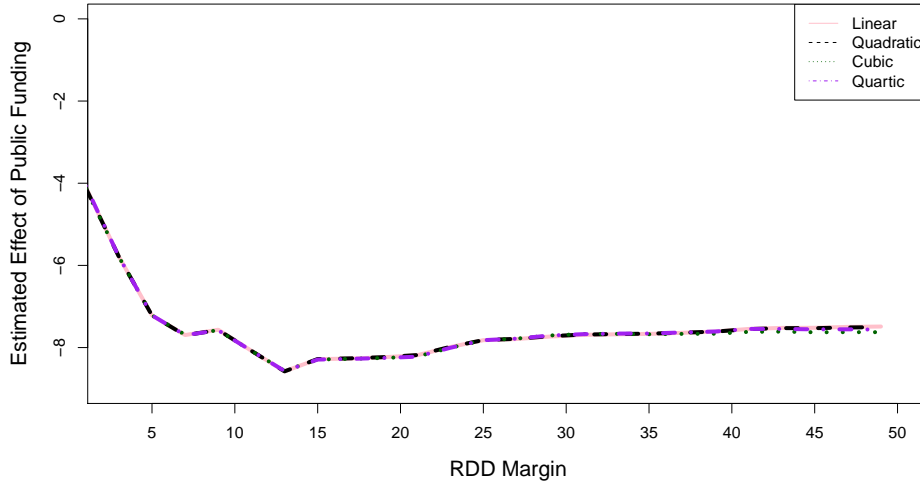
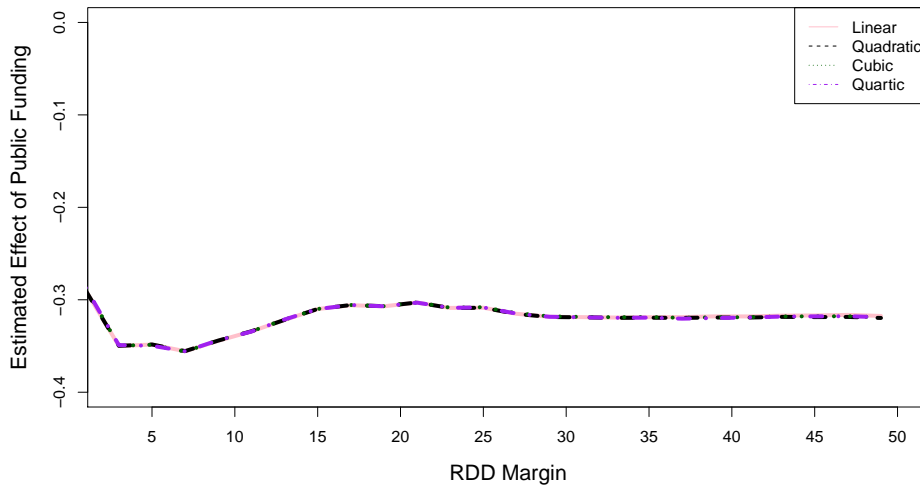


Figure 11 – Ideological Gap: RDD Estimate Across Bandwidths and Specifications. Plots estimates of β_3 from equation 3 across RDD margins and bandwidths. Regardless of bandwidth or specification, public funding is estimated to cause a large increase in polarization, i.e., in the ideological gap between a Democrat and a Republican representing the same hypothetical district. Note that the four lines overlap almost perfectly.



Standard Errors

The main result in the paper is that public funding causes a large increase in polarization. Table 4 reports these econometric results. Although there are a large number of elections after public funding observed in treated states, the treatment is fixed at the state-level and errors are expected to be correlated within states. In the paper, I report robust standard errors clustered by state to address this problem. However, it is well known that clustered standard errors can exhibit downward bias when there are few treated units, especially when significant temporal autocorrelation is present (Bertrand, Duflo, and Mullainathan 2004).

Table 11

	State-clustered SEs	Block Bootstrap- t
Effect on Ideology	[-.56, -.13]	[-.66, -.06]

To address this potential issue, I re-estimate the standard errors using a block bootstrap- t method. Simulations in Cameron, Gelbach, and Miller (2008) show that this method is best for this application. The method relies on sampling states as blocks with replacement—thus preserving the correlation structure within states—and then calculating bootstrapped replications of the pivotal statistic (in this case, the t -statistic from the regression coefficient β_3 in equation 3). Table 11 compares the estimated 95% confidence intervals using this method vs. that reported in the paper, for column 3 in Table 4. As can be seen in the table, the finding remains “statistically significant,” i.e., 0 is not contained in the 95% confidence interval, using this alternative method of inference.²⁸

Time Frame for Electoral Analysis

The election data starts well before the other two datasets, extending back to 1968. In the body of the paper, I report estimates using the entire dataset. To verify that the results do not depend on the earlier years but are consistent when using the more modern period used in the analyses on contributions and polarization, Table 12 replicates the analysis from Table 3 using only data measuring the outcome variable (the Democratic vote share at $t + 1$) in the range 1993–2010. As can be seen, the results are consistent with those in the paper (and if anything, stronger).

State-Chamber Fixed Effects

To establish that the results are not driven by pooling across state upper and lower houses, Tables 13, 14, and 15 replicate the main analyses (on contributions, vote share, and roll-call voting). As can be seen, blocking the analysis on state and chamber produces no appreciable change in the estimates.

²⁸Note that the confidence interval from the bootstrap does not need to be symmetric around the point estimate, because the small-sample distribution of the test statistic is directly estimated.

Table 12 – Public Funding and the Incumbency Advantage, 1993–2010

	Dem Vote Share	Dem Vote Share	Dem Vote Share
Dem Win	11.53 (1.39)	11.85 (1.18)	14.19 (1.29)
Public Funding · Dem Win	-5.83 (1.36)	-7.78 (1.83)	-11.31 (3.17)
Public Funding	2.12 (0.79)	1.38 (1.05)	4.61 (1.89)
N	2516	4128	31228
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 13 – Public Funding and the Financial Incumbency Advantage, State-Chamber Fixed Effects

	Dem Money Share	Dem Money Share	Dem Money Share
Dem Win	24.08 (2.61)	22.16 (2.03)	25.55 (2.23)
Public Funding · Dem Win	-20.15 (5.42)	-17.31 (4.05)	-21.84 (10.45)
Public Funding	16.56 (4.09)	13.53 (3.42)	11.87 (7.06)
N	2421	4020	27203
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State-Chamber Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 14 – Public Funding and the Incumbency Advantage, State-Chamber Fixed Effects

	Dem Vote Share	Dem Vote Share	Dem Vote Share
Dem Win	10.44 (0.87)	10.40 (0.89)	12.08 (1.16)
Public Funding · Dem Win	-5.06 (1.49)	-6.23 (1.73)	-8.24 (2.77)
Public Funding	2.92 (0.96)	2.63 (1.11)	4.53 (1.80)
N	6419	10538	72492
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State-Chamber Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Table 15 – Public Funding and Polarization, State-Chamber Fixed Effects

	NP Score	NP Score	NP Score
Dem Win	-1.15 (0.07)	-1.16 (0.07)	-1.16 (0.07)
Public Funding · Dem Win	-0.35 (0.09)	-0.34 (0.09)	-0.34 (0.11)
Public Funding	0.29 (0.05)	0.31 (0.04)	0.29 (0.07)
N	2497	4153	32292
RDD Bandwidth	3	5	50
Specification	Local Linear	Local Linear	Cubic
Election Fixed Effects	Yes	Yes	Yes
State-Chamber Fixed Effects	Yes	Yes	Yes

State-clustered robust standard errors in parentheses.

Appendix B

Table 16 presents a review of previous papers that study the effect of public funding interventions on electoral competition. Of the 12 papers, five find an increase in electoral competition, five find no effect, and two report mixed findings.²⁹ To my knowledge, no existing study analyzes public funding’s effect on legislator behavior inside the legislature.

Table 16 – Previous studies of the effect of state public campaign financing programs on electoral competition.

Paper	States	Chambers	Years	Method	Finding
Donnay and Ramsden (1995)	MN	Upper, Lower	1966-1990	OLS	Increase
GAO (2010)	AZ, ME	Upper, Lower	2000-2008	Observational	No Effect
Gross, Goidel, and Shield (2002)	All	Gov	1978-1997	Pooled OLS	No Effect
Jones and Borris (1985)	MN	Upper, Lower	1976, 1980	Observational	Mixed
Malhotra (2008)	AZ, ME	Upper, Lower	1992-2002	OLS by state	Increase
Mayer, Werner, and Williams (2006)	AZ, HI, ME, MN, WI	Lower	1990-2004	Observational	Mixed
Mayer and Wood (1995)	WI	Upper, Lower	1964-1990	Observational	No Effect
Miller (2011)	AZ, ME	Upper, Lower	1990-2006	Observational	Increase
Powell (2012)	ME	Upper, Lower	1994-2008	OLS	No Effect
Primo, Milyo, and Groseclose (2006)	All	Gov	1978-2004	Panel OLS	No Effect
Stratmann (2009)	ME, MN (w. 40 control states)	Lower	1980-2006	Panel OLS	Increase
Werner and Mayer (2007)	AZ, ME	Upper, Lower	1990-2006	Observational	Increase

Note: When year ranges differ by state within a study, the largest possible range is reported. Studies that report statistical tests not in the form of a regression (e.g., a difference in means test) are called “Observational.” Studies with “Mixed” findings are those that report either different results for different outcome variables, or different results by state or time period.

Appendix C: Data Coverage

Tables 17, 18, and 19 show the date ranges, sample sizes, and percentage of observations with public funding in place by state.

In Table 17, varying start dates come from the varying years for which Follow the Money starts different states’ datasets.

Note also that in Table 19 many states are listed as starting in 1992 even though the polarization data does not start until 1993. This is because the dataset is merged with election data, and candidates elected in 1992 cast roll-call votes in 1993.

²⁹Mayer, Werner, and Williams (2006) report generally positive findings, but I classify their findings as mixed based on their conclusion: “We are left with something of a mixed picture” (263).

Table 17 – States and years used for analyses of campaign contributions.

State	Year Start	Year End	# Obs	% Years Publicly Funded
AK	1990	2010	357	0
AL	1998	2010	409	0
AR	2000	2010	163	0
AZ	1996	2010	218	74
CA	1998	2010	672	0
CO	1996	2010	610	0
CT	1996	2010	1151	26
DE	2000	2010	306	0
FL	1998	2010	485	0
GA	1992	2010	1936	0
HI	1998	2010	363	0
IA	1998	2010	810	0
ID	1992	2010	324	0
IL	1996	2010	1183	0
IN	1994	2010	1028	0
KS	1996	2010	1116	0
KY	1994	2010	1035	0
MD	1998	2010	165	0
ME	1996	2010	1370	74
MI	1996	2010	1020	0
MN	1996	2010	325	100
MO	1996	2010	1335	0
MS	1999	2007	482	0
MT	1990	2010	1323	0
NC	1996	2010	1169	0
ND	1998	2010	160	0
NH	1996	2010	180	0
NJ	1997	2007	152	0
NM	1992	2010	865	0
NV	1990	2010	390	0
NY	1998	2010	995	0
OH	1996	2010	898	0
OK	2000	2010	400	0
OR	1990	2010	753	0
PA	1998	2010	1505	0
RI	1994	2010	980	0
SC	1996	2010	1035	0
SD	2000	2010	196	0
TN	1996	2010	857	0
TX	1998	2010	978	0
UT	1990	2010	717	0
VA	1999	2009	583	0
WA	1990	2010	266	0
WI	1998	2010	732	100
WY	1992	2010	673	0

Table 18 – States and years used for analyses of election results.

State	Year Start	Year End	# Obs	% Years Publicly Funded
AK	1968	2010	742	0
AL	1970	2010	1581	0
AR	1968	2010	790	0
AZ	1968	2010	654	28
CA	1968	2010	2191	0
CO	1968	2010	1825	0
CT	1968	2010	4166	9
DE	1968	2010	1178	0
FL	1968	2010	1430	0
GA	1968	2010	4470	0
HI	1968	2010	846	97
IA	1968	2010	2830	0
ID	1968	2010	710	0
IL	1968	2010	2549	0
IN	1968	2010	2121	0
KS	1968	2010	3179	0
KY	1969	2010	2485	0
MA	1968	2010	4441	0
MD	1970	2010	489	0
ME	1968	2010	3832	29
MI	1968	2010	2839	0
MN	1974	2010	3287	96
MO	1968	2010	3971	0
MS	1971	2007	1675	0
MT	1968	2010	2460	0
NC	1970	2010	1742	0
ND	1968	2010	537	0
NH	1968	2010	503	0
NJ	1971	2009	463	0
NM	1968	2010	1981	0
NV	1968	2010	960	0
NY	1968	2010	4645	0
OH	1968	2010	2548	0
OK	1968	2010	1806	0
OR	1968	2010	1558	0
PA	1968	2010	5017	0
RI	1968	2010	3113	0
SC	1968	2010	2745	0
SD	1968	2010	683	0
TN	1968	2010	2528	0
TX	1968	2010	3478	0
UT	1968	2010	1982	0
VA	1969	2009	2056	0
WA	1968	2010	553	0
WI	1968	2010	2547	77
WY	1968	2010	942	0

Table 19 – States and years used for analyses of polarization.

State	Year Start	Year End	# Obs	% Years Publicly Funded
AK	1992	2010	283	0
AL	1994	2010	533	0
AR	1992	2010	748	0
AZ	1992	2010	189	49
CA	1992	2010	766	0
CO	1992	2010	649	0
CT	1992	2010	1410	16
DE	1992	2010	413	0
FL	1992	2010	988	0
GA	1992	2010	1450	0
HI	1992	2010	436	0
IA	1992	2010	768	0
ID	1992	2010	241	0
IL	1992	2010	1051	0
IN	1992	2010	1008	0
KS	1992	2010	1105	0
KY	1992	2010	987	0
MD	1994	2010	192	0
ME	1992	2010	1071	60
MI	1992	2010	842	0
MN	1992	2010	297	100
MO	1992	2010	1298	0
MS	1992	2007	673	0
MT	1992	2010	697	0
NC	1992	2010	795	0
ND	1992	2010	184	0
NH	1992	2010	175	0
NJ	1993	2007	171	0
NM	1992	2010	650	0
NV	1992	2010	284	0
NY	1992	2010	1541	0
OH	1992	2010	748	0
OK	1992	2010	1017	0
OR	1992	2010	469	0
PA	1992	2010	1799	0
RI	1992	2010	827	0
SC	1992	2010	1164	0
SD	1992	2010	278	0
TN	1992	2010	954	0
TX	1992	2010	1320	0
UT	1992	2010	699	0
VA	1993	2009	829	0
WA	1992	2010	210	0
WI	1992	2010	906	100
WY	1992	2010	612	0

Years measured as election years (e.g., 1992 denotes a candidate who wins election in 1992 and casts roll-call votes starting in 1993).