

# Does Public Financing Motivate Electoral Challengers? *Appendix*

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## A Variable Descriptive Statistics

Table 1 includes descriptive statistics for the variables used in the empirical analysis.

Variable	Min	Max	Mean	SD
Number of Candidates in General Election	1.000	828.000	141.800	131.254
Number of Seats	20.000	400.000	75.580	54.477
Chamber	0.000	1.000	0.492	0.500
Term Limits	0.000	1.000	0.127	0.333
Government Ideology	17.510	73.620	50.310	13.312
Logged Expenditures	41.400	5521.200	610.600	760.918
Ranney Index	0.513	1.000	0.867	0.101
Professionalism (1)	-1.876	8.584	0.023	1.513
Professionalism (2)	-3.268	3.170	0.015	0.733

Table 1: Variable Descriptive Statistics

## B Results with Dynamic Panel Model

Although I control for time-varying confounding in the two-way fixed effects models, the method may not eliminate the effects of unobserved temporal variation within all US state legislatures between 1976-2018. To account for this potential bias, I have assessed the outcome using a dynamic panel model by lagging the number of candidates with a contemporaneous treatment and state and year fixed effects. These results can be found in Table 2.

This dynamic panel model includes a lagged dependent variable to account for the potential of public financing’s electoral impact being delayed until after the policy has been utilized in an election, as well as state and year fixed effects to account for unobserved individual-specific effects. Dynamic panel models are biased in a finite samples (Nickell 1981), but this bias decreases as the number of time periods increases (Beck and Katz 2011). The data utilized in this paper have a large number of time periods, which abates concerns of bias.

The dynamic panel model outcomes are similar to the results in the main text. The treatment estimates continue to be positive and substantively large. These results are consistent with what I report in the main text.

## C Parallel Trends

A key assumption to the two-way fixed effects model is parallel trends, in which states that did and did not receive public funding should experience the same trend in candidates across elections in the absence of treatment (Ding and Li 2019). Because there is a possibility that states began to offer public financing with the hope of increasing the number of candidates running for office, I assess the parallel trends assumption below by graphing the average number of candidates prior to the implementation of public financing

Table 2: Two-Way Fixed Effects Models of Effect of Public Financing on Candidate Totals With Lagged Dependent Variable, 1976-2018

	Number of Candidates in General Elections (Lagged)	
	(1)	(2)
Public Financing	19.191 (13.767)	11.578 (13.548)
Total Number of Seats		-0.909*** (0.035)
Upper Chamber		12.730*** (7.273)
Term Limits		12.924 (11.433)
Government Ideology		0.254** (0.144)
Logged Expenditures		10.098** (5.471)
Folded Ranney Index		-96.386*** (33.840)
Professionalism (1)		3.394 (4.209)
Professionalism (2)		-7.896* (4.298)
Adjusted $R^2$	0.319	0.454
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	10,115	10,115

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

in treated states compared to the average number of candidates prior to treatment in control states. As the first public financing project was passed by Minnesota in 1974, I assess the mean candidate totals for treated and untreated states from 1968-1976.

In the elections without treatment, both treated and untreated states follow a similar trend in the number of candidates. Based on Figure 1, there do not appear to be large, meaningful differences in the trends of candidate counts in treated and untreated states prior to the introduction to public funding. Taken together with the estimates from the lagged dependent variable model, which does not assume parallel trends and has similar results to those of the non-lagged two-way fixed effects models, I interpret this finding as favorable evidence for the parallel trends assumption.

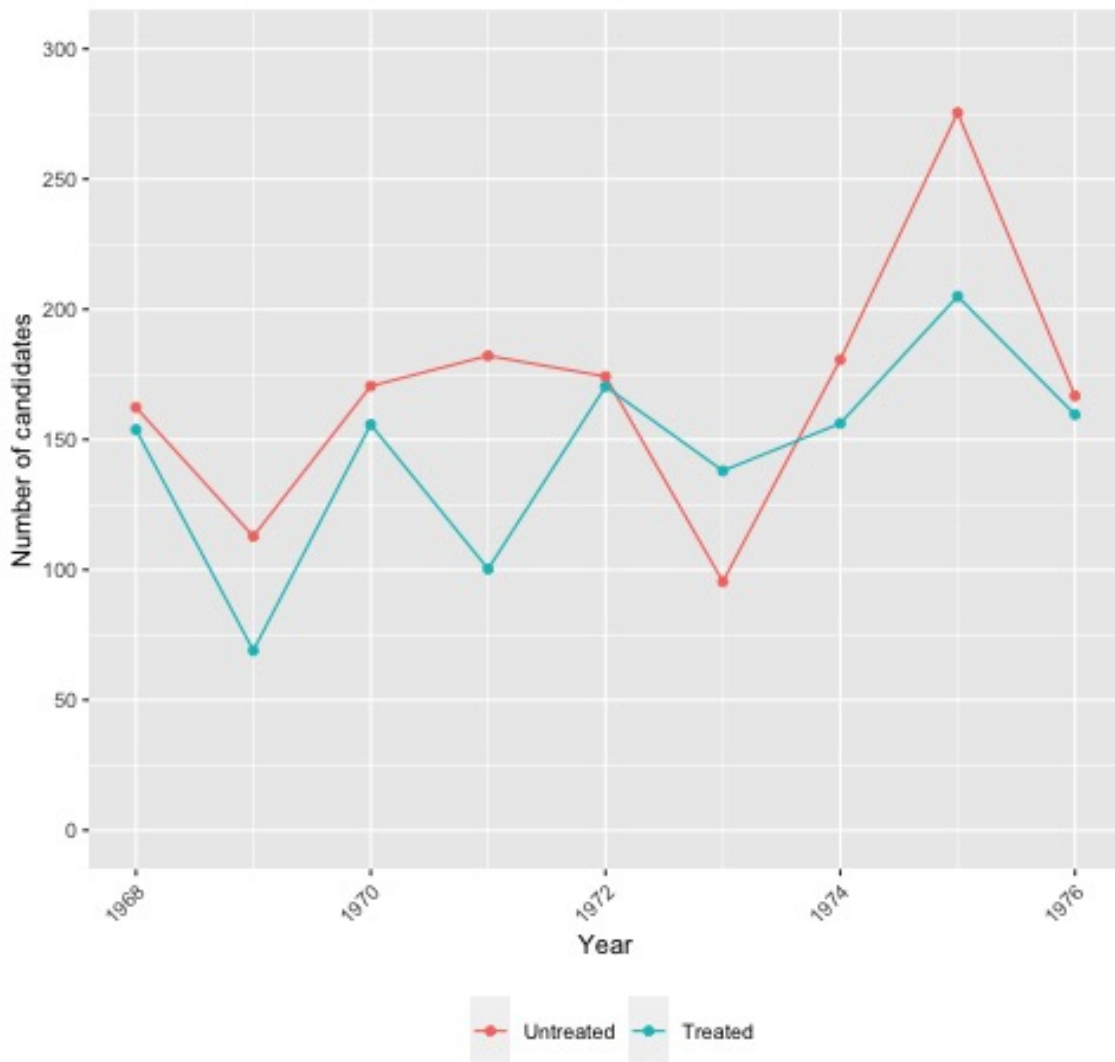


Figure 1: Number of candidates running for state legislative seats, 1968-1976

## D Covariate Balance

I assess whether the weighting model mitigates potential bias by examining the balance of covariates by treatment status. Figure 2 graphs the standardized mean differences between treated and untreated cases with and without weighting. The dashed line indicates a threshold of 0.20, which I define as a sufficiently small difference to consider a covariate balanced. All but one of the variables reach the 0.20 threshold, and the consistently lower values depicted by the blue (weighted) line indicate that the weights do, in fact, improve balance in the data.

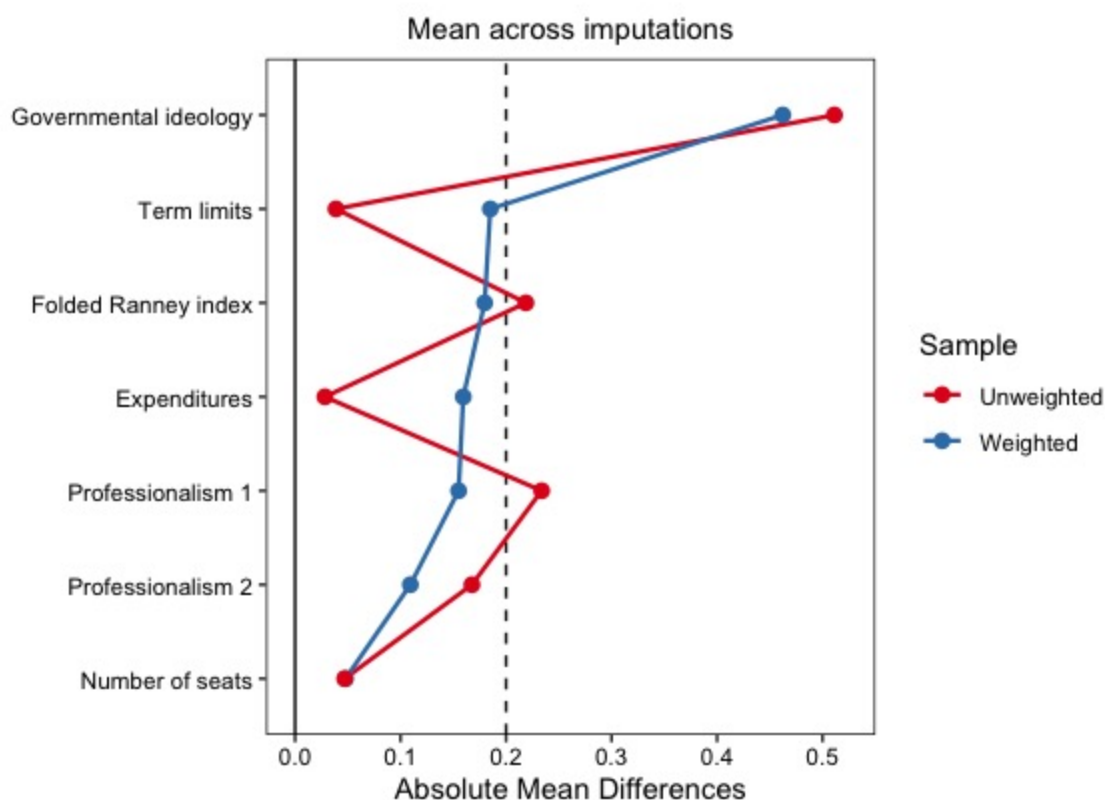


Figure 2: Covariate Balance

## E District Level Analysis

While my research analyzes the impact of public funding on electoral competition at the state-year level, alternate units of analysis may also be beneficial to determine a more granular impact of public financing on electoral competition. Unfortunately, individual-race level data do not exist on such an extensive scale as the data used in my analysis of all 50 states between 1976-2018. Recent work by Kilborn and Vishwanath (2021a) has attempted to remedy the lack of long term data on individual candidates who receive public financing, but their data collection still only spans the years 2000-2016 for a dataset including Arizona and Maine, and 2008-2016 for Connecticut data (Kilborn and Vishwanath 2021b). In this section, I analyze how both the number of candidates

running for office and incumbency are impacted by public funding using Kilborn and Vishwanath (2021a)'s Arizona, Connecticut, and Maine data.<sup>1</sup>

Table 3: Two-Way Fixed Effects Models of Effect of Public Financing on Candidate Totals in AZ, CT, and ME State Legislative Elections, 2000-2016

	Candidates Running for State Legislature			
	(AZ, CT, & ME)	(AZ)	(CT)	(ME)
Public Financing	0.143 (0.137)	0.422*** (0.061)	0.023 (0.0459)	-0.008 (0.017)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	6,017	1,250	1,554	3,213
Adjusted $R^2$	0.253	0.141	0.302	0.111

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

Table 4: Two-Way Fixed Effects Models of Effect of Public Financing on Incumbency in AZ, CT, and ME State Legislative Elections, 2000-2016

	Incumbents Running for State Legislature			
	(AZ, CT, & ME)	(AZ)	(CT)	(ME)
Public Financing	-0.086 (0.060)	-0.245*** (0.061)	0.076*** (0.028)	-0.060*** (0.018)
District fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	6,017	1,250	1,554	3,213
Adjusted $R^2$	0.148	0.232	0.252	0.119

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

The two-way fixed effects results using data from Arizona, Connecticut, and Maine state legislative elections from 2000-2016 indicate that public financing has a substantively small, but generally positive effect on the number of candidates running for office, as well as a generally negative effect on the number of incumbents running for office. Table 3 indicates that that the estimates of all three Clean Elections states together, as well as Connecticut and Maine, are small in magnitude and not statistically significant. Only in Arizona state legislative elections is there a statistically significant and substantively large estimate, where public financing increased electoral competition by about 0.4 candidates in a given district.

<sup>1</sup>Because Kilborn and Vishwanath (2021a) only collected data in election years which included the use of public financing, CT results include elections in years 2008-2016.

Moving to the incumbency results, Table 4 shows that public financing has a negative and statistically significant impact on the incumbency advantage in Arizona and Maine, as well as a slight negative impact on incumbency across all three states together. However, public financing appears to slightly bolster incumbents in Connecticut legislative elections.

Taken together, the conclusion that public financing improves electoral competition by influencing more candidates to run for office and reducing the incumbency advantage at the individual-candidate level should be interpreted with caution. A major issue with this district-level analysis is that there is very little variation among districts in terms of the number of candidates running for office. There is a small number of candidates in any given district race; the vast majority of races have two candidates, while very few have four or five candidates. Because there is so little variation in the dependent variable, it is unlikely to find a large substantive impact of public financing on electoral competition. In addition, the time period includes only nine elections in Arizona and Maine, all of which occur in the years after public financing was implemented, and five elections in Connecticut, which introduced public financing in its 2008 legislative elections.

Additionally, individual-level data are particularly challenging for testing my theory, as I need to aggregate the number of candidates running for office at the district level. The Kilborn and Vishwanath (2021*a*) individual-level data serve as the ideal unit of analysis for research on representation, such as the ideological focus of the authors' paper. When the outcome of interest is a count, as the total number of candidates running for office is, the ideal unit of analysis is at the state level.

While this data collection effort is incredibly impressive, its short time period and small scope are not ideal for the research design of this paper. Particularly, data only exist for the time in which states have utilized Clean Elections. Although my dataset is on the state level rather than the individual level, the longer time period and inclusion of all states that did and did not utilize public financing over time allows for a discussion of the long term effects of public financing in comparison to states that do not publicly finance their elections, as well as within states before and after the advent of public funding.

## F Alternative Measures of Competition

While the number of candidates is the best measure of electoral competition in terms of consistency and availability, there exist alternative measures of competitiveness. I assess two measures of electoral competitiveness from Klarner (2013). The first is win margin, which measures the average percent margin of the total vote by which winning candidates win. I also utilize 4-year safe seat percentage, which measures the percentage of seats that are "safe," as measured by a winning margin of 10% or more. These results illustrate that there is a slight negative impact of public financing on safe seats and margins of victory, with a slight positive impact of public financing on margin of victory in Model 4. However, although the results are generally negative, these findings are both substantively small and statistically insignificant.

While vote share and safe seat percentage present alternative measures of electoral competition, the primary interest of this paper is to measure the emergence of possible options for voters and determine whether public financing lowers the barrier to entry

Table 5: Two-Way Fixed Effects Model of Effect of Public Financing on Electoral Competition in All State Legislatures, 1976-2013

	Safeness of Seats		Margin of Victory	
	(1)	(2)	(3)	(4)
Public Financing	-2.030 (2.712)	-0.752 (2.075)	-0.399 (4.567)	2.330 (3.231)
Total Number of Seats		0.000 (0.001)		-0.002 (0.002)
Upper Chamber		0.010 (0.076)		-0.152 (0.161)
Term Limits		-3.385* (1.870)		-7.969*** (2.406)
Government Ideology		-0.081* (0.037)		-0.039 (0.055)
Logged Expenditures		-3.174** (1.755)		-3.105 (2.441)
Professionalism (1)		1.717*** (0.390)		2.377*** (0.763)
Professionalism (2)		1.250*** (0.400)		1.220* (0.712)
Adjusted $R^2$	0.701	0.718	0.820	0.834
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	8,260	8,260	8,260	8,260

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



for candidates by allowing access to campaigns for those who would otherwise be financially unable to run for office. In addition, vote share and margin of victory are two retrospective measures of competition, as they are measured after an election cycle ends. Alternatively, because candidate emergence is measured during a campaign, it serves as a more prospective measure of competition. Although I do not find that public financing decreases the number of safe seats or results in closer elections in terms of vote margin, my conclusion holds that the representative impacts of public financing increasing the number of candidates running for office may include positive downstream effects like more confidence in democratic elections and greater feelings of satisfaction in government (Miller 2014; Lee, Clark and Vayas 2020).

The number of candidates running speaks to the ability of public financing to increase access to running for office, as opposed to safe seat percentage and vote margin, which indicate increased access to winning office. Based on the findings of this paper, public financing's primary influence on electoral competition is not on retrospective outcomes like margins of victory or safeness of seats, but instead that it decreases the barrier to entry and allows more people to run for office.

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