Appendix

In this appendix, we present the results of additional analyses that are supplemental to those in the main paper. We divide them into two sections: the first covers diagnostic tests for our RD design assumptions, and the second provides some additional information and analyses.

0.1 Diagnostic Tests

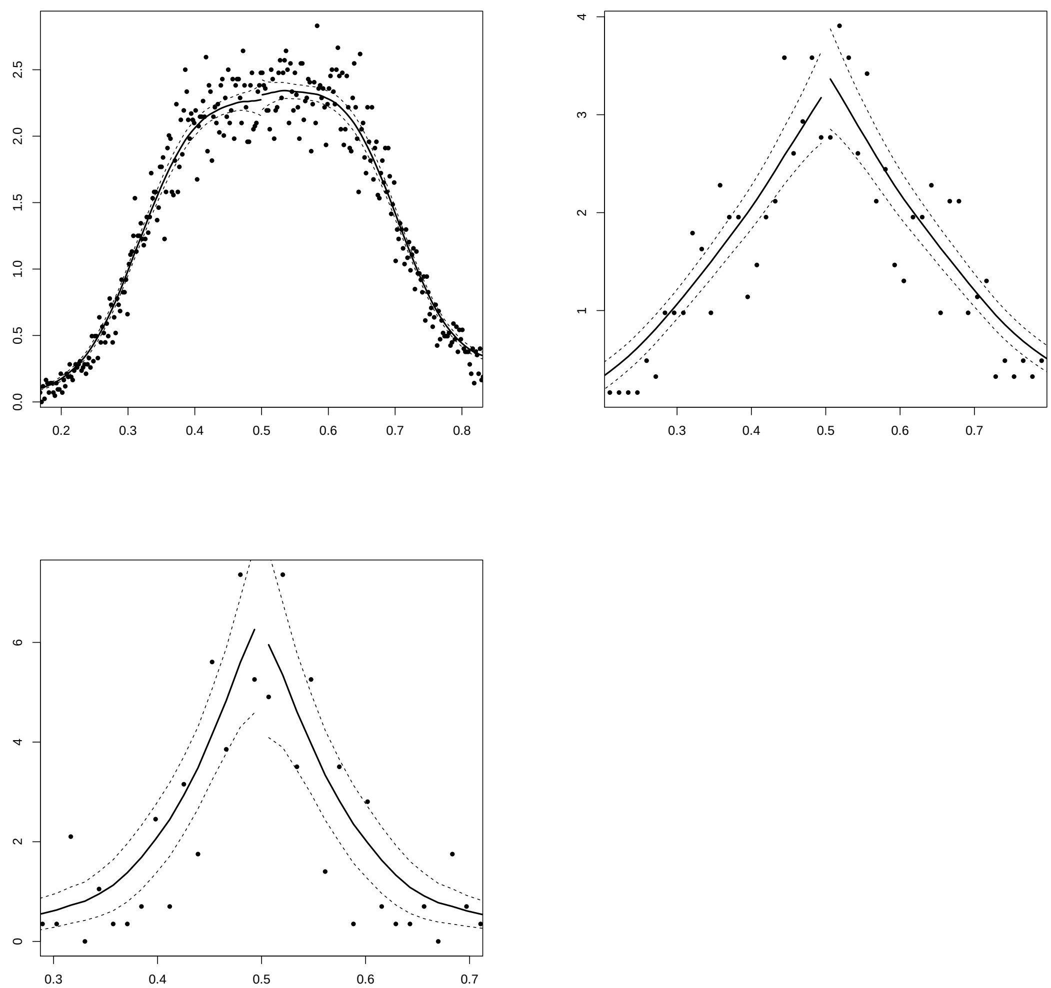
In this section, we present the results of our diagnostic tests. First, we check for evidence of sorting around the cut point, and second, we test whether the RD predicts past outcomes and other pre-treatment covariates, as a placebo test.

One test that can be used to examine the density of the running variable around the cut point checks for a jump in density just above the cut point. This would be suggestive of, but not dispositive proof of, manipulation of the running variable. **?** specifies a formal test for this, which we implement using the `rdd` package in R. It is important to note that without missing data, our data would necessarily pass this test, because our forcing variable, top-two vote share, is by construction equal to 1 minus the other candidate's value. Therefore, even if certain candidates can manipulate the vote share such that they barely win, their opponents will therefore barely lose, resulting in no change in the density around the cut point. However, because there are some missing observations caused by merging the datasets together, the density test can test whether, for example, winners are more likely to report donation data.

Figure 1 visually displays the density before and after the cutoff. The p-value associated with the McCrary test (a Wald test for the null result of no discontinuity in the distribution) is .78 for state legislative elections, .83 for gubernatorial elections, and .72 for US Senatorial elections. This suggests that it is not the case that candidates are systematically more likely to appear in the dataset if they have won, which would threaten the validity of our design.

Although the McCrary test offers evidence that barely winning does not appear to make

Figure 1: Density around the cut point for State Legislative (left), US Senatorial (right) and state gubernatorial elections (bottom)



a candidate more likely to appear in our dataset, it still could still be the case that certain types of candidates can manipulate the running variable such that they barely win and their opponent barely loses. If those candidates elicit donations from systematically different types of donors in a way that affects their participation patterns, then the RD assumptions will be violated. In order to test for this possibility, we perform a series of RD analyses where the outcome is a pre-treatment outcome. We do this for three "placebo outcomes": having donated in the past, being a female (or having a female-sounding name, per DIME), and having donated to an incumbent. Our results consistently pass these tests, with the exception of a few estimates for gubernatorial donors with particularly small bandwidths. Because the number of clusters is very small for that donor pool, the confidence intervals are unusually small, and fail to include 0.

The unit of analyses is the individual donor, clustered at the candidate-race level. For this, as with other RDs, we estimate the effects using a local linear regression with a triangle kernel. We use the `rdd` package in R. We estimate the RD using a range of bandwidths, ranging from 2% to 16%. Figure 2 summarizes our results across all three donor types and all three placebo outcomes.

0.2 Additional analyses

In this section, we present the results of additional analyses. There are two categories. First, we present data plots for each of the RDs in the main paper, and next, we examine the behavior of individuals who donated to multiple different close races in one cycle. We discuss each in turn.

Figure 3 visually displays the data for the 5% bandwidth, with a linear fit plotted on either side of the cut point for reference. These results suggest that the results are not for example driven by a single observation (not that that would necessarily be evidence of a spurious effect). This is especially useful given the magnitude of the sizes we uncover.

Next, we turn to donors who contributed to multiple close races. In order to examine

Figure 2: Placebo tests: estimating the effect of donating and winning on past outcomes

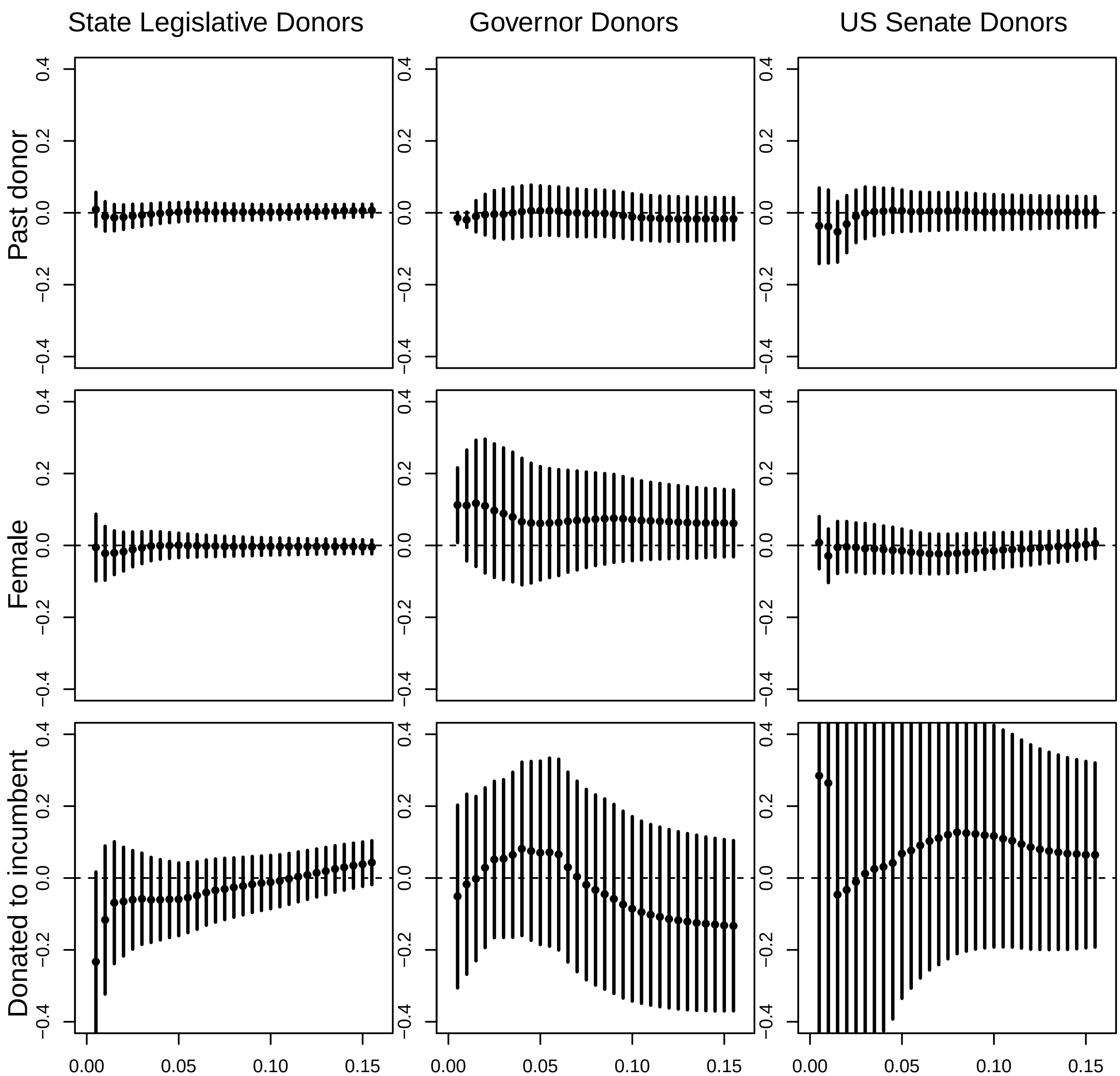


Figure 3: Data plots for main RD analyses

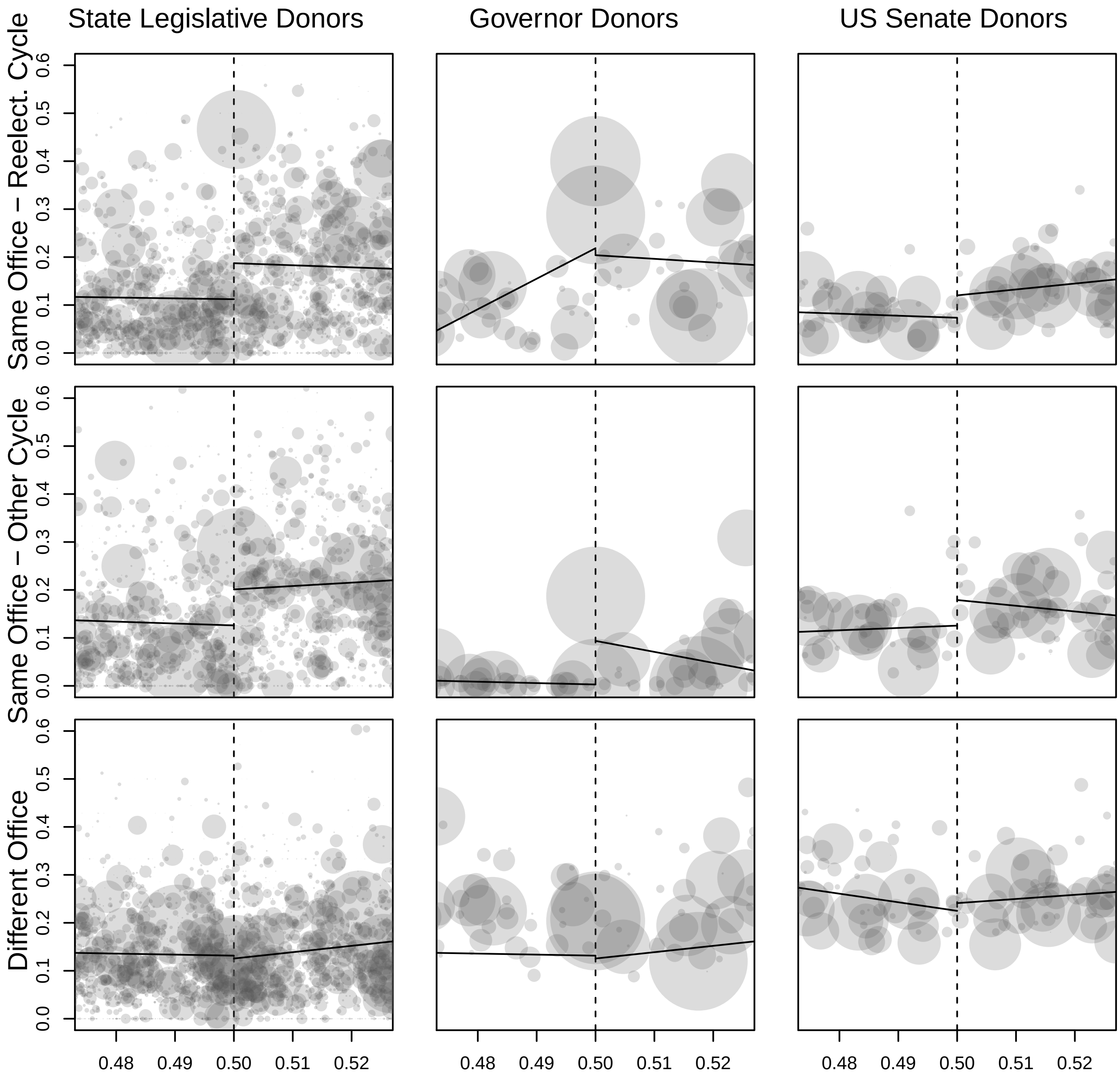


Table 1: Future behavior of donors, by the number of candidates they gave to. Data are restricted to individuals whose candidates were within 2.5% of the cutoff.

	Dependent variable: Any Future Donation		
	Num. of Recipients:		
	1	2	3
1 Win	0.030^{***}	-0.004	0.023
	(0.001)	(0.004)	(0.016)
2 Wins		0.004	0.035^{**}
		(0.004)	(0.016)
3 Wins			0.030^{*}
			(0.017)
Constant	0.437^{***}	0.704^{***}	0.932^{***}
	(0.003)	(0.017)	(0.032)
Cycle F.E.	✓	✓	✓
Observations	1,224,532	34,367	1,180
R^2	0.006	0.009	0.007
Adjusted R^2	0.006	0.009	-0.002
Note:			$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

this, we combine donation data on state legislative, US Senatorial, and gubernatorial races, looking only at races within 2.5% of the cut point. For each donor in our analysis, we regress whether that individual donated in the next 3 cycles (since it doesn't make sense to look at donations to a different office when there are multiple offices) against the total number of wins, as well as fixed effects for election cycle. Table 1 summarizes the results. In order to interpret these as causal effects, we would have to assume that the outcome of races within 2.5% of victory is as-if random, which is a stronger assumption than that of continuity of potential outcomes for our main analysis. Nonetheless, the results are roughly consistent with our previous finding. For both individuals who gave to one and to three candidates, more victories correlates with more participation. Among donors who gave to two candidates, there is no observational relationship.