THE EFFECTS OF PUBLIC CAMPAIGN FUNDING: EVIDENCE FROM SEATTLE'S DEMOCRACY VOUCHER PROGRAM

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During each election cycle, the city of Seattle distributes four \$25 vouchers to every registered voter, which may be donated to and redeemed by campaigns for city office. Through a difference-in-differences research design, we study the causal effect of Seattle's program on various outcomes in city council elections in the first two cycles after implementation, with comparison cities drawn from large cities in Washington and California. We find a 53% increase in total contributions and a 350% increase in number of unique donors; these effects are largely explained by large increases in small donations, defined as contributions less than \$200. We find statistically insignificant evidence of decreases in private donations, although our point estimates suggest moderate-to-substantial crowd-out ratios. We further estimate a highly significant 86% increase in number of candidates coupled with substantial decreases in incumbent electoral success. These results provide some of the first causal evidence of the effect of decentralized public campaign finance schemes, while speaking to the impacts of campaign regulation more generally on donation activity, candidate entry, and incumbency advantage.

1. INTRODUCTION

American election campaigns are expensive. During the 2020 federal election cycle, candidates reported raising an average of \$37 million per Senate race and almost \$4 million per House race (Federal Election Commission, 2021). The high expense of campaigns extends to many state and local races; for example, candidates reported raising more than \$400,000 per race on average in 2015 Seattle city council races, from more than 2000 donors on average

The large amount of money required to run campaigns, coupled with a relatively small number of donors supplying the bulk of these donations,¹ fuels a persistent concern that public policy may be tilted in favor of those who have the means to donate (see, e.g. Overton, 2011). In turn, this has led to a number of efforts to provide public funding of political campaigns. Such programs—such as New York City's 8:1 matching program—are often explicitly aimed at empowering small donors. However, to date there is very little evidence of their effectiveness on either donation activity or electoral outcomes.

We study the impact of a unique program implemented in Seattle starting in 2017. During each election cycle, the city distributes four "Democracy Vouchers," worth \$25 each, to every regis-

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*This paper has benefited from extensive discussions with and comments from Katie Baird, Jacob Bastian, Rachel Heath, Jason Kerwin, Mark Long, Bryan Stuart, and numerous seminar participants. Special thanks to Jungyoun Kim for assistance in implementing the synthetic difference-in-differences estimation. All errors remain our own. Authors declare that they have no financial or non-financial interest in the content or outcome of this research.

¹During the 2020 federal election cycle, the 1.4% of Americans who contributed \$200 or more were responsible for approximately 76% of all donations to federal candidates, parties, and outside committees. See: <https://www.opensecrets.org/elections-overview/donor-demographics?cycle=2020&display=A>.

tered voter. These vouchers may be donated to candidates for city office. Over two election cycles since implementation, this has led to distributions of approximately \$1.75 million per cycle, nearly as much as the \$2 million in average total contributions across election cycles prior to implementation. While city agencies have issued reports and limited descriptive analyses have been conducted,² to our knowledge there have been no attempts to assess the causal impacts of this program. Additionally, evidence of the effects of Seattle's program is particularly important, as it has been cited as a model for nationwide expansion by members of Congress and presidential candidates.³

We fill this gap by providing an analysis of the program's effects on city council races during the first two election cycles post-implementation. Our difference-in-differences strategy compares outcomes in Seattle pre- and post-Democracy Vouchers to those in comparison cities drawn from other large cities in Washington and the largest cities in California. Our identification assumption is that, in the absence of Democracy Vouchers, trends in Seattle would be the same as average trends in these other cities. While our difference-in-difference method is imperfect, there exists little evidence in the literature on the effects of public funding and of campaign regulation more generally.

Our main results are as follows. Per 100,000 residents, the Democracy Voucher program led to increased donations of \$31,000 (approximately 53%) per race. We also find an increase of approximately 350% in the number of donors per race. We further show that the program led to approximately 270% more dollars donated by small donors, defined as those who gave up to \$200. The effect on the value of small donations in fact exceeds the total effect, suggesting lower large donations. We also measure the effect of the program on private campaign giving, as measured by non-voucher donations. We find negative but insignificant effects on the total value of private donations along with positive but insignificant effects on the number of small private donors; our results, while insignificant, cannot rule out substantial crowd-out of private giving.

Next, we estimate the program's impact on electoral competitiveness. We find an 86% increase in the number of candidates per race but no effect on winners' final vote percentages. Finally, we show that the program led to a large and significant decrease in the probability that incumbents run for reelection, a large but insignificant decrease in the probability that incumbents are reelected, and a large and significant decrease in incumbents' final vote percentage.

After presenting our main results, we perform a series of robustness checks that speak to both identification and inference. First, while we see some evidence of differential pre-trends, we show

²These studies are descriptive in nature, contrasting the donor pools pre- and post-implementation to population demographics. For example, McCabe and Heerwig (2019) show that in 2017 the number of voters who used their vouchers represented 4.03% of the voting age population, a significant increase from the 1.49% that made a cash donation in 2013. They also note that the donor pool in 2017 overrepresented older and wealthy individuals compared to their share in the population.

³One aspect of the "For the People Act" (H.R. 1) as proposed in 2021 would provide for a pilot program directly modeled after the Seattle program. Additionally, during the 2020 Presidential campaign cycle, candidates Bernie Sanders and Kirsten Gillibrand stated their intent to implement a small donor voucher program for federal elections (Gutman, 2019; Grayer and Nobles, 2019). Gillibrand's proposal announcement directly referenced Seattle's program, stating, "My federal plan is modeled in part on a successful program currently being used in Seattle" (Gutman, 2019).

that extrapolating estimated pre-treatment trend differences cannot explain effects of the magnitudes we estimate (see Kahn-Lang and Lang, 2020; Rambachan and Roth, 2019). We further show that our donation results are qualitatively the same if we estimate them in raw (rather than per capita) terms and that “synthetic difference-in-difference” estimates (see Arkhangelsky et al., 2021) are essentially identical to our main results. We also perform a “placebo” analysis for county-level elections, showing that we fail to estimate spurious “effects” for King County council elections, for which vouchers were not available; indeed, King County elections appear to have been trending toward being less competitive. Finally, we show that inference is qualitatively identical if performed using permutation tests (see Fisher, 1935; Young, 2019) rather than standard asymptotic approximations, which may be invalid due to our “1 vs. N” study design.

Our results speak directly to two broad areas in the literature.⁴ First, our results on donation activity relate to a series of papers that investigate the relationship between public funding schemes and donation/campaign activity. For example, Malbin, Brusoe and Glavin (2012) show that New York City’s increased “multiple match” ratio—from 1:1 to 4:1 and later 6:1—was associated with a higher percentage of both total contributions and total donors being “small” (defined as those who give up to \$250). Using a 2011 court order suspending Arizona’s opponent “matching” provision as a natural experiment, Dowling et al. (2012) find no evidence that privately funded campaigns responded to the threshold that avoided opponent matching funds.

Our results on private donation activity also relate to a set of studies that investigate the relationship between public funding and private giving more broadly, often in the nonprofit sector. Empirical studies of potential crowd-out of private nonprofit donations by government grants has produced mixed results (see Borgonovi, 2006; Heutel, 2014), with some evidence that a mechanism for crowd-out may be reduced fundraising activities (see Andreoni and Payne, 2010). The reduced fundraising effort channel may be relevant for campaigns as well, as survey and interview evidence shows that candidates who accept public funding tend to spend less time fundraising (see Francia and Herrnson, 2003; U.S. Government Accountability Office, 2010; Miller, 2011); however, we hesitate to interpret these results causally due to selection into accepting public funding. The related results for nonprofit giving may be particularly relevant given findings suggesting that political and nonprofit giving may be substitutes (Petrova et al., 2020).

Second, our results speak to the effects of campaign regulation on electoral competitiveness. Avis et al. (2021) use a regression “kink” design to show that limits on spending in Brazilian mayoral elections led to more candidates per race who were on average less wealthy. Taylor and McEleney (2019) provide evidence that Toronto’s public finance regime was associated with more candidates per race. A number of other studies investigate associations between public funding and candidate entry, with mixed results (see Butcher and Milyo, 2020; Donnay and Ramsden, 1995; Lott, 2006; Stratmann and Aparicio-Castillo, 2007). Finally, results studying Maine’s and Arizona’s public funding schemes seem to depend on whether candidates participate in the program (com-

⁴In addition to the sources we cite here, we also note that public campaign funding has been discussed at length in the legal literature (see Hasen, 1996; Overton, 2011, for prominent examples).

pare Malhotra, 2008; Powell, 2012), and endogenous participation makes it difficult to establish causation.

Further, our results speak to the literature on “incumbency advantage,” the general historical tendency of incumbents to win elections at higher rates than challengers.⁵ Taylor and McEleney (2019) find minimal association between public funding and incumbency advantage in Toronto municipal elections. Our results are most similar to those in Avis et al. (2021), who find that campaign spending limits mitigated the incumbency advantage in Brazilian mayoral elections.

Finally, we note that we do not measure impacts on policy outcomes. On this topic, a number of papers have found a robust relationship between corporate donations and policy outcomes,⁶ partially validating concerns that such donations are associated with higher returns to donors (Fisman et al., 2012). In contrast, Masket and Miller (2015) provide evidence that candidates taking advantage of public funding in Maine and Arizona showed similar legislative voting records to those who did not. While we do not measure the program’s effects on legislators’ voting, policy outcomes, or political participation, we highlight these as important issues for future study.

In sum, we provide the first evidence of the effects of Seattle’s novel Democracy Vouchers program on measures of donation activity and electoral competitiveness. Our results are particularly important given national and local proposals to expand public campaign funding, many of which are modeled directly after Seattle’s program. Our results speak to broader issues on the relationship between public campaign funding, on the one hand, and donation activity and political competitiveness, on the other.

2. SEATTLE’S DEMOCRACY VOUCHERS PROGRAM

2.1. *Public Campaign Funding Schemes*

As of 2019, 14 U.S. states had some form of public funding program to directly distribute public money to campaigns for state office.⁷ These most commonly take the form of the “simple public option” or “full funding model” (Klumpp, Mialon and Williams, 2015), whereby candidates who raise a sufficient number of small contributions are eligible for a large lump-sum grant, but then they are prohibited from spending more than the grant amount.

Alternatively, a number of large municipalities have public campaign funding schemes that are “decentralized” in terms of deciding which candidates get public money. One popular scheme is the “multiple match” model, whereby public funds are provided to “match” small-dollar private donations to candidates, usually conditional on reaching some eligibility threshold (Malbin, Brusoe and Glavin, 2012). New York City, one of the earliest to implement this funding scheme, matches

⁵Existing literature of incumbency advantage gives various estimates of the magnitude and mechanism for the effect (see Gelman and King, 1990; Trounstine, 2011; Cox and Katz, 1996).

⁶Studies have shown that corporate activities such as connections to winning parties, politically active board members, or donating to a large number of candidates, are positively correlated with outcomes such as increased government procurement contracts, abnormal positive returns, and increased market capitalization (see Cooper, Gulen and Ovtchinnikov, 2010; Goldman, Rocholl and So, 2013; Ovtchinnikov and Pantaleoni, 2012; Jayachandran, 2006; Luechinger and Moser, 2014).

⁷See: <https://www.ncsl.org/research/elections-and-campaigns/public-financing-of-campaigns-overview.aspx>.

private campaign contributions at an 8:1 ratio (New York City Campaign Finance Board, 2021). These programs are often explicitly aimed at increasing the importance of small donors at the expense of those who would otherwise make large donations (Malbin, Brusoe and Glavin, 2012; Overton, 2011).

Public funding schemes operate within constitutional bounds set by federal and state courts. While courts have struck down funding provisions explicitly aimed at “leveling” the playing field for opponents of those who spend large sums of money,⁸ they have tended to uphold the constitutionality of “multiple match” programs. In 2019, the state Supreme Court affirmed the constitutionality of Seattle’s Democracy Voucher program in *Elster v. City of Seattle* (2019), rejecting arguments that the program constituted forced speech.

2.2. *Seattle’s Program*

Seattle holds municipal elections, including those for mayor and nine city council seats, in odd-numbered years. In 2015, city voters passed I-122, a ballot initiative promoted as “Honest Elections Seattle” (Seattle Ethics & Elections Commission, 2020). Included in this reform package was authorization for the Democracy Vouchers program, funded by a voter-approved \$30 million property tax increase spread over a ten-year period (Seattle Ethics & Elections Commission, 2020).

The program operates as follows. In each municipal election year, the Seattle Ethics and Elections Commission (SEEC) sends out four \$25 vouchers (total of \$100) to each registered voter. Figure 1 gives examples of a voucher and the envelope in which they are mailed. Voters have the option to distribute these vouchers to participating candidates in municipal races, and they can choose to allocate their vouchers to one campaign (for a total of \$100) or to separate campaigns. These vouchers are then redeemed by the candidates’ campaigns, and the SEEC distributes the value of the pledged voucher to the campaign. Unused vouchers expire if they are not redeemed during the election year.

The program was first implemented in the 2017 election. In that year, vouchers were redeemable by candidates in races for two city council seats and for city attorney, but not for mayor. The program continued for seven city council races in 2019. The program expanded to mayoral elections in 2021.

Campaign participation in the Democracy Voucher program is not automatic and comes with restrictions. To participate, campaigns must demonstrate a minimum number of qualified contributions and signatures.⁹ Further, council candidates who enroll in the Democracy Voucher program

⁸Prior to 2011, a few states operated programs that combined the initial lump-sum funding with further “matching” or “leveling” funds for publicly-funded candidates to keep pace with high-spending privately-funded ones (Klumpp, Mialon and Williams, 2015). These types of “leveling” provisions have been generally found to be unconstitutional under the First Amendment (see *Davis v. Federal Election Commission*, 2008; *Arizona Free Enterprise Club v. Bennett*, 2011).

⁹For a districted city council seat, candidates must have 150 qualified contributions and 150 qualified signatures. For an at-large city council seat, eligibility is met with 400 qualified contributions and 400 qualified signatures. A qualified contribution is a donation greater than \$10 from a Seattle resident.

FIGURE 1: EXAMPLE ENVELOPE AND VOUCHER

are subject to lower cash contribution limits¹⁰ as well as lower total expenditure limits.¹¹

Table 1 gives a summary of the program for 2017 and 2019, the elections that we analyze. To date, 38 candidates have participated in the program, including 16 of 18 of candidates who advanced beyond the “Top Two” primary. In 2017, 45,621 vouchers were redeemed for over \$1 million, the majority of which went to city council candidates. In 2019, the total amount of funding grew to almost \$2.4 million with 98,179 voucher redemptions, exclusively by candidates for city council. While the amount distributed was large compared to pre-2017 averages in Seattle, only 2.1% and 5.2% of distributed vouchers were redeemed in 2017 and 2019, respectively.¹²

¹⁰\$350 per donor (which may include \$100 dollars from Democracy Vouchers), as compared to \$550 for non-participants.

¹¹At-large and districted city council candidates who participate are subject to a limits of \$450,000 and \$225,000, respectively, during the primary and general elections. Participating candidates can request for release from these spending limits if they are being dramatically outspent by non-participating candidates.

¹²In 2017, vouchers were mailed to approximately 550,000 voters with a total value of \$55 million. In 2019, vouchers

TABLE 1
DEMOCRACY VOUCHER DISTRIBUTIONS

Year	Accepting Candidates	Amount	Donor-Candidate Pairs
2017	4	\$965,800	13,957
2019	34	\$2,389,325	34,106

Source: Authors' calculations from SEEC data. 2017 featured two council races and one city attorney race for which vouchers were redeemable.

Notes: 2019 featured seven council races for which vouchers were redeemable. An additional \$27,550 and \$65,150 in vouchers were redeemed by candidates who did not file campaign finance reports in 2017 and 2019, respectively.

Seattle's program is unique. Unlike "full funding" schemes, there is no large grant given to fund campaigns. Unlike "multiple match" programs, Seattle's program allows voters, at their individual discretion, to direct money to campaigns without having to put forth any money of their own. While no similarly-structured voucher programs have been implemented, Seattle's novel program has directly influenced proposals for similar programs elsewhere, including bills in Congress¹³ and proposals by presidential candidates.¹⁴ Accordingly, evidence of the effectiveness of Seattle's program is particularly important to inform these proposals for expansion. While the program is still in its infancy, Seattle's experience represents a unique opportunity to learn the effectiveness of voucher-style public funding for campaigns to better inform these proposals.

3. COMPARISON CITIES AND DATA

3.1. *Comparison Cities*

As stated by Avis et al. (2021), "[t]esting the effects of spending limits empirically is challenging because campaign finance reform is usually applied uniformly across elections and jurisdictions, making it difficult to identify an appropriate comparison group." A similar difficulty arises in studying the effects of public *funding* programs, as we do here. Seattle's Democracy Vouchers program was implemented non-randomly and in only one city, and thus we are left with imperfect comparisons.

Our primary empirical strategy is a difference-in-difference design, which necessitates choosing a set of comparison cities to construct an assumed counterfactual for Seattle. We use two sets of comparison cities. Our main estimates compare to all cities simultaneously, while we present results using each set separately in the Appendix.

were mailed to approximately 450,000 voters with a total value of \$45 million (Seattle Ethics & Elections Commission, 2018, 2019). Henderson and Han (2020) study a campaign to increase program awareness as an avenue toward higher participation but only find a significant effect for voucher holders who were prior voters. McCabe and Heerwig (2019) show that the Democracy Voucher program led to a more diverse and more egalitarian donor pool.

¹³For example, Pramila Jayapal, the U.S. Representative for Washington's Seventh District, which includes Seattle, introduced H.R. 1613, "Democracy Dollars Act," in 2019, proposing the creation of a pilot program for federal elections in three states. The bill did not pass, but the same proposal was included as Section 5101 of the "For the People Act of 2021," which passed the U.S. House on March 3, 2021. Source: <https://www.congress.gov/bill/117th-congress/house-bill/1/text>.

¹⁴See Footnote 3 for references to 2020 proposals by Bernie Sanders and Kirsten Gillibrand.

First, we compare Seattle to other large cities in Washington state. These cities, which constitute the next six largest cities in the state by population, are summarized in Appendix Table A.1, Panel A. Each has a city council with a similar structure to Seattle's, with a mix of districted and at-large council seats as well as staggered four-year terms. These other cities operate under the same sets of election rules and the same state laws as Seattle, including all city council elections being held under a "Top Two" primary system. However, these cities are different from Seattle in that they are substantially smaller and have had different population growth patterns over the past decade.

Second, we compare Seattle to a set of the largest cities in California. These cities are summarized in Panel B of Appendix Table A.1. In contrast to the Washington comparison cities, the California cities have populations much closer to Seattle's. However, elections and public campaign disclosure operate under different sets of rules in California than in Washington, with some variation across cities in California. For instance, while most municipal elections in California operate under a "Top Two" primary system, there are some exceptions: for example, Oakland and San Francisco conduct local elections via a single round of ranked-choice voting without primaries.

3.2. *Data Sources*

According to the SEEC, the Democracy Voucher program has two primary goals: to increase the number of voters donating to municipal elections and to increase the number of candidates (Seattle Ethics & Elections Commission, 2019). We gather data to measure these and related outcomes.

First, we gather data on donations to campaigns. For cities in Washington, including Seattle, this data is maintained in a centralized repository by the Washington Public Disclosure Commission (PDC).¹⁵ We were able to collect data for elections held 2003-2020 in all seven cities identified in Appendix Table A.1, Panel A.¹⁶ In California, similar transaction-level data is reported on Forms 460 and 460A, Schedules A (cash) and C (in-kind).¹⁷ Unfortunately, California does not have a centralized source for municipal campaign finance data. Therefore, we assemble California donation outcomes from data that is made publicly available by city clerks' offices, and we are thus constrained to data that they choose to provide publicly. A full description of our data coverage is shown in the final column of Appendix Table A.1, Panel B.

Second, we gather primary and general election results for all city council races held 2001-20. For Washington state, this required gathering public election data from county election officials' websites, supplemented where necessary with data provided by the Washington State Archives. For

¹⁵Data since 2007 available here: <https://www.pdc.wa.gov/browse/open-data/contributions-candidates-and-political-committees>. Pre-2007 data was provided to the authors by PDC staff. The PDC maintains that their archives are a "best-effort" to compile campaign financial information and verifies their data with original reports from campaigns themselves.

¹⁶In Washington, candidates for municipal office are required to fully report contributions and expenditures if the office is in a jurisdiction with at least 5,000 registered voters or the campaign expects, or already has, received at least \$5,000 in contributions. Source: <https://www.pdc.wa.gov/learn/publications/candidates-guide-reporting>.

¹⁷All candidates for any public office in California must file if contributions exceed \$2,000. Source: <https://www.sos.ca.gov/campaign-lobbying/campaign-disclosure-and-requirements/campaign-filing-requirements/where-campaign-statements-must-be-filed-candidates-officeholders-and-campaign-committees-local-level>.

California, we use the California Elections Data Archive (CEDA),¹⁸ which features comprehensive election data for all relevant years in California.

We supplement donation and election data with two additional data sources. To measure the number and value of Democracy Vouchers redemptions since 2017 in Seattle, we use publicly-available data from the Seattle Ethics and Election Commission (SEEC). This allows us to observe all voucher donations. Finally, in order to scale our donation outcomes for population, we gather city population estimates by year from intercensal estimates made publicly available by the U.S. Census Bureau.

3.3. Descriptives of Donation Outcomes

In both states, contribution data is reported by *donation*, as is the Democracy Voucher data from SEEC. Therefore, our first step is to aggregate donations such that each entry coincides to a unique donor-campaign pair in a given election cycle.¹⁹ We then calculate donation aggregates at the race level. We fill in candidates' missing race data—in other words, which seat candidates are contesting—by cross-referencing candidate names in our election data. For candidates and/or races where no candidates filed donation reports, we impute donation data such that no donations were received.

Table 2 provides comparisons of means for Seattle and both sets of comparison cities, both before and after the program's inception. Contribution amounts and number of donors are calculated per 100,000 residents.²⁰ Panel A reveals sharp changes in many of our primary outcomes in Seattle from pre-2017 (Column 1) to afterward (Column 2). Total donations increased approximately 60%, while number of unique donors increased more than 350%. Note that both of these measures were trending upward in each set of comparison cities (Columns 3-4 and 5-6 respectively), but less sharply than in Seattle. Figure 2 shows trends by four-year election cycle, highlighting the sharp uptick in Seattle in 2017-20, especially prominent for number of donors.

Next, we define "small" donations as donations up to \$200.²¹ Table 2, Panel A shows a large increase in both the value of small donations and the number of small donors. Appendix Figure A.1 presents means by four-year election cycle, showing a large increase in Seattle in the final period (2017-20) for both the value and number of small donors.

In Panel A, we also present means of the value and number of private (non-voucher) donations. In order to calculate the value of private donations, we subtract the value of Democracy Voucher redemptions from the total value reported by campaigns. To calculate total number of

¹⁸This data is publicly available. See <https://csus-dspace.calstate.edu/handle/10211.3/210187>.

¹⁹That is, if a single donor gave 10 donations of \$25 each to a single campaign, this would be counted as one donor giving \$250. Similarly, the Democracy Voucher data is reported per \$25 voucher. Therefore, if, as we commonly see in the data, an individual gave all four vouchers to one candidate, we code that as a single transaction of \$100. Doing analysis at the donor-campaign level is a step suggested by Malbin, Brusoe and Glavin (2012).

²⁰Results are qualitatively identical if we instead perform the analyses without rescaling (see Section 6.3 and Appendix Table A.8).

²¹We choose this threshold since it matches the Federal Elections Commission's threshold for itemizing contributions. Source: <https://www.fec.gov/help-candidates-and-committees/filing-reports/individual-contributions>. We also perform our "small donors" analysis using \$100 as the cutoff, with qualitatively similar results (see Appendix Table A.3).

TABLE 2
SUMMARY OF OUTCOME DATA

	(1)	(2)	(3)	(4)	(5)	(6)
	<u>Seattle</u>		<u>Comparison Cities</u>			
			<u>Washington</u>		<u>California</u>	
<u>Panel A: Donation Outcomes</u>	<u>2003-16</u>	<u>2017-20</u>	<u>2003-16</u>	<u>2017-20</u>	<u>2003-16</u>	<u>2017-20</u>
<i>All Donations</i>						
Total Contributions (in \$1000)	59.872 (3.221)	98.536 (2.747)	31.013 (3.587)	48.875 (8.211)	39.554 (5.014)	74.353 (26.678)
Number of Donors	307.529 (23.201)	1109.251 (75.527)	170.921 (14.842)	192.525 (28.455)	83.772 (9.702)	105.844 (18.494)
<i>Small Donations ($\leq$ \$200)</i>						
Total Contributions (in \$1000)	16.677 (1.445)	65.412 (5.755)	9.327 (0.831)	10.014 (1.556)	3.883 (0.554)	5.256 (1.066)
Number of Donors	226.677 (21.779)	1019.419 (80.427)	135.419 (10.966)	131.092 (19.176)	32.295 (4.720)	43.492 (8.637)
<i>Private Donations</i>						
Total Contributions (in \$1000)	59.872 (3.221)	47.205 (4.693)	31.013 (3.587)	48.875 (8.211)	39.554 (5.014)	74.353 (26.678)
Number of Donors	307.529 (23.201)	373.829 (37.255)	170.921 (14.842)	192.525 (28.455)	83.772 (9.702)	105.844 (18.494)
Observations	37	9	156	46	161	62
<u>Panel B: Electoral Outcomes</u>	<u>2001-16</u>	<u>2017-20</u>	<u>2001-16</u>	<u>2017-20</u>	<u>2001-16</u>	<u>2017-20</u>
Number of Candidates	3.756 (0.438)	7.778 (0.123)	2.560 (0.116)	3.109 (0.266)	3.935 (0.213)	4.275 (0.244)
Percent Vote for Winner	0.654 (0.020)	0.577 (0.033)	0.663 (0.014)	0.629 (0.023)	0.657 (0.012)	0.633 (0.019)
Incumbent Run	0.780 (0.082)	0.444 (0.025)	0.583 (0.037)	0.500 (0.053)	0.593 (0.033)	0.688 (0.063)
Incumbent Win	0.659 (0.090)	0.444 (0.025)	0.491 (0.038)	0.500 (0.053)	0.564 (0.035)	0.537 (0.071)
Incumbent Vote Percent	0.483 (0.061)	0.265 (0.039)	0.388 (0.028)	0.336 (0.042)	0.424 (0.027)	0.439 (0.046)
Observations	41	9	175	46	337	80

Source: Authors' calculations from data described in Section 3.2.

Notes: All outcomes are per race. Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Donation outcomes (Panel A) scaled to per-100,000 residents. For Donation Outcomes (Panel A), sample coverage by city and year is listed in final column of Appendix Table A.1. For Election Outcomes (Panel B), sample includes all elections held 2001-20 in all 15 cities.

FIGURE 2: TREND GRAPHS FOR DONATION OUTCOMES **(A) TOTAL CONTRIBUTIONS****(B) NUMBER OF DONORS**

Source: Authors' calculations from data described in Section 3.2.

Notes: All outcomes are per race. Bars show means by comparison group (Seattle, Washington Comparison Cities, California Comparison Cities) across four-year election cycles. Error bars show 90% confidence intervals, calculated from asymptotic normal distribution with standard errors clustered by city-election cycle. Comparison cities identified in Appendix Table A.1. All outcomes scaled to per-100,000 residents. Sample coverage by city and year is listed in final column of Appendix Table A.1.

private donors, we subtract the number of unique Democracy Voucher donors by campaign then aggregate by race.²² For private donations, we see a decrease in total contributions together with an increase in number of donors. In both sets of comparison cities, both value and number of private donations increased. Trends by four-year election cycle across the three groups are shown in Appendix Figure A.2.

3.4. Descriptives of Election Outcomes

Panel B of Table 2 summarizes averages of measures of electoral competitiveness. We define number of candidates by counting the number of names that appear on the primary election ballot.²³ The number of candidates in Seattle more than doubles (from 3.756 to 7.778) from pre- to post-2017, while the number of candidates per race increases by a small amount in both sets of comparison cities. This increase is shown very prominently in Figure 3, Panel A.

Table 2, Panel B also reveals sharp declines in incumbent success in Seattle, measured by the proportion of incumbents who run for reelection (from 0.780 to 0.444), the proportion of incumbents who win (from 0.659 to 0.444),²⁴ and incumbents' final vote share (from 0.483 to 0.265).²⁵ We note

²²We do *not* match names between the Democracy Voucher redemption data (from SEEC) and campaign finance reports (from PDC). Accordingly, our calculations for numbers of private donors for Seattle post-2017 may be biased downward if individuals, for instance, gave both through the Democracy Voucher program and privately.

²³In some cases, no primary election was held. This occurred in some Special elections and in some regular elections in which two or fewer candidates qualified for the ballot. In those cases, we define number of candidates as the number appearing on the general election ballot. We ignore "Write-In" votes for purposes of defining number of candidates.

²⁴Note that we define "Incumbent wins" unconditional on whether an incumbent runs or not, in contrast to Malhotra (2008).

²⁵For the measure of incumbent vote percentage, if the general election ballot did not include an incumbent, this is

a lack of similar decreases in measures of incumbent success in either Washington or California comparison cities.

FIGURE 3: TREND GRAPHS FOR ELECTION OUTCOMES

Source: Authors' calculations from data described in Section 3.2.

Notes: All outcomes are per race. Bars show means by comparison group (Seattle, Washington Comparison Cities, California Comparison Cities) across four-year election cycles. Error bars show 90% confidence intervals, calculated from asymptotic normal distribution with standard errors clustered by city-election cycle. Comparison cities identified in Appendix Table A.1. Sample includes all elections held 2001-20 in all 15 cities.

4. EFFECTS ON DONATION ACTIVITY

In this section, we estimate the effect of the availability of Democracy Vouchers on donation activity. Our main empirical specification is Equation (1), a difference-in-differences specification that includes two-way fixed effects (see, e.g. Conley and Taber, 2011).

$$(1) \quad y_{rst} = \alpha_s + \alpha_t + \beta Post_t Seattle_s + X_{rst}\gamma + \epsilon_{rst}$$

In Equation (1), y_{rst} is an outcome for race r , city s , election cycle t . Each race r corresponds to a unique seat on a city council. Election cycles comprise two years (2001-02, etc.). $Post_t$ is an indicator for the cycle being 2017-18 or later, and $Seattle_s$ is an indicator for city s being Seattle. We include city and election cycle fixed effects in all specifications, denoted by α_s and α_t , respectively, as well as controls for Special and At-Large (city-wide) elections.

In our main analysis, we compare Seattle simultaneously to all comparison cities in both Washington and California. Supplemental results, comparing Seattle to Washington and California cities separately, are provided in the Appendix. All contribution amount results are in thousands of dollars, and all donation outcomes are calculated per 100,000 residents.

coded as the incumbent receiving 0% of the vote.

4.1. All Donations

Our results for total contributions are shown in Column 1 of Table 3. We estimate an effect of approximately \$31,000 per 100,000 residents, an effect that is marginally significant. If we perform the comparisons separately for Washington and California cities, we estimate effects of \$35,000 and \$18,000 respectively, with only the former statistically significant (see Appendix Table A.2, Column 1).

Our estimates for total donors are shown in Column 2. Our primary estimate shows an effect of 833 additional donors per 100,000 residents. Estimates derived from considering Washington and California cities separately are quite similar, and all estimates are highly significant (see Appendix Table A.2, Column 2).

These estimated effects are substantively quite large. An effect of \$31,000 represents an increase of approximately 53% as compared to the average total donations per race in Seattle prior to the program's implementation. An increase of 833 donors per 100,000 residents is a massive increase: it implies an increase of more than 0.8 percentage points in per-capita terms, or approximately 270% as compared to the pre-program average in Seattle.

TABLE 3
EFFECTS ON DONATION OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)
	All		$\leq \$200$		Private Donations	
	Contrib	Donors	Contrib	Donors	Contrib	Donors
Post $\times$ Seattle	31.535* (16.777)	833.532*** (49.148)	51.056*** (3.623)	829.863*** (53.269)	-21.486 (20.757)	72.492 (52.023)
Observations	471	471	471	471	471	471
R-squared	0.384	0.793	0.804	0.802	0.371	0.713
Mean in Seattle 2003-2016	59.872	307.529	16.677	226.677	59.872	307.529

Notes: All outcomes are per race and are summarized in Table 2, Panel A. See Section 3.2 for discussion of data sources. Outcomes scaled to per-100,000 residents. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample coverage by city and year is listed in final column of Appendix Table A.1. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.2. Effects on Small Donations

Next, we analyze the effects on donation activity from small donors. We define "small donors" as those who donate up to \$200 to a campaign in a given election cycle, and we note that results are qualitatively similar if we instead define "small" donations as those up to \$100 in value (see Appendix Table A.3). We estimate regressions of the form in Equation (1) on the total amount of contributions derived from donations of less than \$200 as well as the number of unique donors who gave up to that amount.

Our estimated effect on total contributions derived from small donations is given in Table 3, Col-

umn 3. An effect of approximately \$51,000 is large and highly significant. The estimated effect on the number of small donors, in Column 4, shows a similarly large and highly significant increase. The effects on both contributions from and the number of small donors are highly significant and numerically very similar when comparing Seattle to Washington or California cities separately (see Appendix Table A.2, Columns 3-4).

As to magnitude, these estimated effects are enormous. An increase of \$51,000 is more than 300% of the average funds from small donors in Seattle pre-2017, while 830 additional small donors per 100,000 residents represents an increase of more than 350% over the pre-program average in Seattle.

Additionally, the effect on small donations (\$51,000, Column 1) is larger than the effect on all donations (\$31,000, Column 3), suggesting that the program led to a decrease in value of *large* donations. Further, the effect on the number of donors (Column 2) and the number of small donors (Column 4) are quite similar in magnitude, which implies little effect on the number of donors giving more than \$200. Taken together, the results in Columns 1-4 of Table 3 suggest an increase in the total amount of contributions, comprised of a large increase in small contributions but a smaller decrease in large contributions. The number of small donors increased massively, while the number of large donors was relatively unaffected.

4.3. *Crowd-In vs. Crowd-Out*

Finally, we investigate whether donations due to the Democracy Voucher program “crowd out” private giving activity. We note that two prominent features of the program’s design as described in Section 2 imply opposite effects. First, the requirement for qualifying donations pushes in the direction of “crowd in” since securing qualifying donations provides eligibility to redeem vouchers. Alternatively, the lowered maximum contributions for participants is a potential mechanism for “crowd out” of private donations.

Table 3, Column 5 shows the estimated effect on private donations. Our point estimate of -\$21,000 implies substantial “crowd out,” but it is statistically insignificant. Estimates that compare to Washington and California cities separately are similarly insignificant (Appendix Table A.2, Column 5). While our results are insignificant, we acknowledge that we cannot rule out substantial crowd out; simply comparing our estimated effect of -\$21,000 to the total expenditures of the program of \$52,000 (per race, per 100,000 residents) suggests “crowd out” of approximately 40%. Our estimates on the effect on the number of private donors, shown in Column 6, are similarly imprecise, but we note that the positive point estimates suggest “crowd in” in terms of number of donors.

5. EFFECTS ON ELECTORAL COMPETITIVENESS

Here, we present estimates of the program’s effect on measures of electoral competitiveness. We again estimate difference-in-differences specifications according to Equation (1), where y_{rst} identifies one of the outcomes in Table 2, Panel B, for race r , city s , election cycle t . As with the

donor outcomes, we include city and election cycle fixed effects as well as controls for Special and At-Large elections.

First, we measure the effect on the number of candidates per race, noting that a stated goal of the program was to increase the number of candidates running for office. Our estimate of approximately 3.2 additional candidates per race is large, representing an increase of 86% over the pre-program average in Seattle. Estimates are similar in magnitude and also highly significant when comparing Seattle to Washington or California cities separately (see Appendix Table A.4, Column 1). Column 2 shows a small and insignificant negative effect on the percent of final vote for the winner of elections.

TABLE 4
EFFECTS ON ELECTION OUTCOMES

	(1)	(2)	(3)	(4)	(5)
	Number of Candidates	% Vote for Winner	Incumbent Run	Incumbent Win	Incumbent Vote %
Post × Seattle	3.232*** (0.557)	-0.036 (0.034)	-0.362*** (0.106)	-0.146 (0.109)	-0.178** (0.081)
Observations	688	688	688	688	688
R-squared	0.727	0.948	0.663	0.605	0.619
Mean in Seattle 2001-2016	3.756	0.654	0.780	0.659	0.483

Notes: All outcomes are per race and are summarized in Table 2, Panel A. See Section 3.2 for discussion of data sources. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample includes all elections held 2001-20 in all 15 cities. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special × At-Large included in all specifications. *** p<0.01, ** p<0.05, * p<0.1.

We present evidence of effects on incumbent “entrenchment” in Columns 3-5. Column 3 shows a large and highly significant negative effect on the probability of incumbents running for reelection. The decrease of 36 percentage points is quite large, especially when compared to 78% of incumbents running pre-program in Seattle.²⁶ We see a smaller, insignificant negative effect on the probability that incumbents win reelection (Column 4), together with a negative and significant effect on incumbent vote percentage (Column 5).

For purposes of robustness, we estimate effects on competitiveness separately for Washington and California comparison cities. We find similar negative and significant effects on incumbents’ decision to run and on final vote percent for both sets of comparison cities (see Appendix Table A.4, Columns 3 and 5). The negative effect on incumbents’ probability of winning is significant only when comparing to Washington cities (see Appendix Table A.4, Column 4).²⁷

²⁶There is limited evidence in the academic literature on how the existence of public financing influences incumbents’ decisions to run for reelection. Supporters of I-122 highlighted that campaigning for city council would necessarily become a more “grassroots” effort, forcing incumbents to compete for vouchers in a larger and less familiar donor pool, while at the same time reducing the barrier to entry for candidates that might struggle to connect to large donors (Clark, 2015; Durning, 2021).

²⁷We note the contrast between the non-significant estimates in Columns 2 and 4. The estimate in Column 2 is a

In sum, our estimates of the effect on election outcomes suggest that Seattle’s program was very effective at increasing electoral competitiveness. Elections featured almost twice as many candidates, incumbents were much less likely to run, and incumbents on average received a substantially smaller shares of the final vote. Further, these results hold across different sets of comparison cities. Finally, we find that incumbents were less likely to be reelected, but this result is imprecisely estimated and significant only in one specification.

6. ROBUSTNESS

In this section, we perform a battery of robustness checks that address concerns in two main categories: identification and inference. The former concern stems from possible endogeneity of the Democracy Vouchers program, while the latter arises from the fact that the program was only implemented in a single city.

6.1. *Parallel and Non-Parallel Trends*

The validity of our difference-in-differences identification strategy relies upon a counterfactual “parallel trends” assumption. That is, we assume that Seattle, in the absence of the program, would have trended the same way as (the average of) our comparison cities. While we cannot check this assumption directly, we can provide support for its plausibility and assess our estimates’ sensitivity to its failure.

First, noting that we have data from seven pre-treatment election cycles (2003-16) for donation outcomes and eight (2001-16) for ballot outcomes, we test for differential trends in the pre-treatment period. To do this, we estimate Equation (2) restricted to elections prior to 2017. In this specification, t is year and $Seattle_s$ is an indicator for the race being in Seattle. As above, γ_s is a city fixed effect. “Parallel trends” implies a null hypothesis that $\gamma_2 = 0$.

$$(2) \quad y_{rst} = \gamma_s + \gamma_1 Year_t + \gamma_2 Seattle_s \times Year_t + u_{rst}$$

In Appendix Table A.5, estimates of Equation (2) show that the assumption of parallel trends in the pre-program period tends to be rejected for donation outcomes (Panel A) but generally is not rejected for ballot outcomes (Panel B).

Next, we adopt a sensitivity analysis suggested by Kahn-Lang and Lang (2020) to show that our results are robust, at least qualitatively, to non-parallel trends.²⁸ In effect, they suggest assessing whether the treatment effect “survives” extrapolation of a non-parallel trend into the treatment

“precise zero” in the sense that the point estimate is relatively small (about 5% of the pre-2017 mean) with a standard error of the same magnitude, suggesting confidence intervals that do not include large effects. In contrast, the estimate in Column 4 does not preclude large effects: the point estimate is approximately 24% of the pre-program mean and any confidence interval built around this mean would include very large effects.

²⁸This is one of many new methods that have been suggested to assess sensitivity of results to failure of parallel trends assumptions in a difference-in-differences framework (see, e.g. Kahn-Lang and Lang, 2020; Rambachan and Roth, 2019).

period. In performing this check, we estimate the specification in Equation (3).

$$(3) \quad y_{rst} = \delta_s + \delta_1 Year_t + \delta_2 Seattle_s Year_t + \delta_3 Seattle_s \times Post_t + u_{rst}$$

Equation 3 allows for arbitrary non-parallel trends in the pre-program period. The trend in Seattle ($\delta_1 + \delta_2$) is then extrapolated into the post-treatment period, and deviations from that trend identify the treatment effect, δ_3 . While this specification nests the standard difference-in-differences method—which assumes $\gamma_2 = 0$ —we note that it requires a quite different identification assumption: the assumed counterfactual is that Seattle would trend the same post-treatment as it did pre-treatment, in contrast to traditional difference-in-difference design which assumes Seattle would trend the same as the comparison cities post-treatment.

Estimated treatment effects, presented in Table 5, are qualitatively the same as our main results. Panel A compares quite well to Table 3: we estimate large, significant effects on the value and number of total donations (Columns 1-2) and the value and number of small donations (Columns 3-4). Results on private donations (Column 5-6) are qualitatively similar to our main results, except the effect on number of private donors is now significant.

Similarly, the results in Table 5, Panel B, compare quite well to our main results on election outcomes in Table 4. We estimate large and highly significant effects on number of candidates, together with statistically weaker evidence of decreases in incumbent advantage.

6.2. Analysis via Synthetic Difference-in-Differences

As an additional robustness check, we estimate effects using the synthetic difference-in-differences method proposed by Arkhangelsky et al. (2021). Standard synthetic control methods construct a counterfactual as a weighted average of comparison units—in this setting, cities. The synthetic control estimator generalizes this approach by also calculating weights for time periods, with weights calculated to achieve balance between pre- and post-program time periods (see Abadie, 2021; Arkhangelsky et al., 2021). For this procedure, we use pre-program outcome data as the only predictors, as motivated by the leading cases in Arkhangelsky et al. (2021).

The method requires a balanced panel, in our case consisting of cities and election cycles. We first collapse our data by city-election cycle such that observations represent means by race.²⁹ For donation outcomes, we use the following two “balanced panels”: (1) all Washington cities in cycles 2003-2020; (2) all Washington cities, Los Angeles, Sacramento, San Diego, San Francisco in cycles 2007-2020. For election outcomes, our analysis sample consists of all cities in cycles 2001-20.

Estimates of program effects under this procedure are shown in Table 6.³⁰ Panels A and B give estimates for donation outcomes for our two different balanced panels. Most importantly, these

²⁹E.g., in 2019, Seattle held seven city council races. We collapse these to a single observation where each outcome represents the mean across the seven observations.

³⁰In order to benchmark the SDID results, we also present two additional sets of estimates on these same “balanced panels.” In Appendix Table A.6, we show standard difference-in-difference results on the balanced panels. Appendix Table A.7, we give synthetic control estimates on the balanced panels. A comparison of these results to those in Tables 3-4 and 6 reveals that they are qualitatively identical to both our main results and the SDID results.

TABLE 5
REGRESSIONS WITH NON-PARALLEL TRENDS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Donation Outcomes						
	All		$\leq \$200$		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Year	2.512*** (0.940)	3.609*** (0.974)	0.173*** (0.051)	1.528** (0.695)	2.512*** (0.940)	3.609*** (0.975)
Year $\times$ Seattle	-2.337 (1.657)	-8.252 (5.972)	-0.613* (0.337)	-6.579 (4.880)	-2.393 (1.631)	-9.103 (5.523)
Post $\times$ Seattle	50.790*** (15.287)	888.734*** (77.403)	55.285*** (5.035)	871.395*** (70.705)	-1.537 (16.512)	138.201** (67.431)
Observations	471	471	471	471	471	471
R-squared	0.375	0.792	0.805	0.801	0.362	0.713
Panel B: Election Outcomes						
	Number of Candidates	% Vote for Winner	Incumbent		Vote %	
			Run	Win		
Year	0.010 (0.018)	-0.003** (0.001)	-0.000 (0.003)	-0.002 (0.004)	-0.003 (0.003)	
Year $\times$ Seattle	0.009 (0.078)	0.004 (0.004)	-0.017 (0.012)	-0.007 (0.015)	-0.007 (0.010)	
Post $\times$ Seattle	3.465*** (0.947)	-0.074 (0.045)	-0.215* (0.113)	-0.111 (0.110)	-0.119 (0.109)	
Observations	688	688	688	688	688	
R-squared	0.725	0.947	0.657	0.596	0.610	

Notes: All outcomes are per race and are summarized in Table 2. See Section 3.2 for discussion of data sources. Donation Outcomes (Panel A) scaled to per-100,000 residents. Regression specification in Equation (3). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. For Donation Outcomes (Panel A), sample coverage by city and year is listed in final column of Appendix Table A.1. For Election Outcomes (Panel B), sample includes all elections held 2001-20 in all 15 cities. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

estimates are quite similar to the point estimates in Table 3. Further, comparing point estimates in Panels A and B suggests that they are not very sensitive to choice of which “balanced panel” is used for estimation. Table 6, Panel C shows estimates for election outcomes. Here as well, point estimates of treatment effects are very similar to the corresponding estimates in Table 4.

For inference, we estimate treatment effects “as if” each of our comparison cities (excluding Seattle) were “treated” with Democracy Vouchers in 2017-20, and we present the fraction of these permutations that our point estimate is greater than in absolute value. This is one of the methods suggested by Arkhangelsky et al. (2021) and also similar to a standard approach to inference in synthetic control methods (see Abadie, 2021). For example, in Panel C, our treatment effect of approximately 4.5 additional candidates is greater than the estimated effect under 14 of 14 alternative treatments, where 14 represents the number of cities used for estimation excluding Seattle.

TABLE 6
ESTIMATED EFFECTS UNDER SYNTHETIC DIFFERENCE-IN-DIFFERENCES SPECIFICATION

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Donation Outcomes						
<i>Sample: All Washington Cities 2003-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	35.645	913.797	51.117	885.825	-21.565	87.747
Fraction of Permutations Larger Than	6/6	6/6	6/6	6/6	4/6	3/6
Panel B: Donation Outcomes						
<i>Sample: All Washington Cities and LA, SD, SF, Sacramento 2007-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	21.198	895.269	56.302	898.296	-31.120	69.219
Fraction of Permutations Larger Than	6/10	10/10	10/10	10/10	8/10	7/10
Panel C: Election Outcomes						
<i>Sample: All Washington and California Cities 2001-20</i>						
	Number of	% Vote	Incumbent			
	Candidates	for Winner	Run	Win	Vote %	
Treatment Effect	4.526	-0.008	-0.276	-0.042	-0.141	
Fraction of Permutations Larger Than	14/14	0/14	11/14	2/14	7/14	

Notes: Table presents estimates using method and code from Arkhangelsky et al. (2021). Per-race outcomes, summarized in Table 2, are collapsed to be mean by city-election cycle, in order to create a balanced panel. Comparison cities identified in Appendix Table A.1. See Section 3.2 for discussion of data sources. Donation outcomes (Panels A and B) scaled to per-100,000 residents. “Fraction of Permutations Larger Than” gives the proportion of “placebo” treatments that the estimated treatment effect is larger than in absolute terms.

6.3. Analysis in Levels

During the period of our study, the population of Seattle was growing quite quickly, as shown in Appendix Table A.1. Accordingly, a natural concern is that the trends that we pick up are due

to divergent population patterns. In partial response to this concern, in our main analysis we define all donation outcomes as per-100,000 residents. To show that our results are not dependent upon this choice, we re-estimate our results at the race level without rescaling but controlling for population. These results are given in Appendix Table A.8. All results are qualitatively similar to our main results, and indeed we find significant “crowd out” under this alternative specification (Appendix Table A.8, Column 5). We note that the effect on total contributions is insignificant under this alternative specification (see Appendix Table A.8, Column 1).

6.4. Analysis at the County Level

As a further check, we reproduce our analysis at the *county* level. These results have at least two possible interpretations. First, they may be viewed as a “placebo” or falsification test, since Democracy Vouchers were unavailable in all county-level races, including those in King County, which includes Seattle. Alternatively, these results may be interpreted as estimating “spillovers” whereby the availability of Democracy Vouchers in Seattle led to citizens becoming accustomed to donating to political campaigns, in turn affecting contributions to campaigns for other races.

To perform this exercise, we gather donation and election data for counties. For donations, we collect data from PDC for the five largest counties by population in Washington, dating back to 2003. Unfortunately, since there is no central repository for donation data to campaigns for county offices in California, our donation results use data only from Washington counties. For measuring electoral competitiveness, we use data on all county council/commission elections for these same five counties in Washington along with the twelve largest counties in California by population, with the latter data gathered from the California Elections Data Archive (CEDA).

We estimate “treatment effects” at the county level analogous to our analysis at the city level. For these county council races, we estimate Equation (4), where $y_{r dt}$ is an outcome for race r , county d , election cycle t . $Post_t$ is an indicator for election cycle being 2017-18 or later, while $King_d$ is an indicator for King County. α_d and α_t are county and election cycle fixed effects, respectively. As in our city-level analyses, we estimate all donation results per 100,000 residents.

$$(4) \quad y_{r dt} = \alpha_d + \alpha_t + \beta Post_t King_d + \epsilon_{r dt}$$

Our estimates for donations are shown in Appendix Table A.9, Panel A. We see no significant effects on the value or number of total donations (Columns 1-2) or on the value or number of small donations (Columns 3-4). Further, point estimates are orders of magnitude smaller than our results at the city level shown in Table 3. Appendix Table A.9, Panel B gives estimates for election outcomes. These suggest that incumbents were actually enjoying more success in King County after the implementation of the Democracy Vouchers program in Seattle.

6.5. Placebo Treatments and Permutation Tests

Now, we turn to inference, which is complicated by two factors in this setting. First, our estimation sample contains a relatively small number of cities. Second, throughout the course of our study, we only observe “treated” units in a single city (Seattle). As such, our design is a “1 vs. N” difference-in-differences design, where standard inference based on asymptotic results may not be valid.

The issue of a single treated unit in a difference-in-differences framework is investigated at length in Donald and Lang (2007) and Conley and Taber (2011). We perform a deterministic version of the randomization inference procedure suggested by Conley and Taber (2011), estimating placebo treatment effects using all possible combinations of untreated city-election cycle pairs (see also Fisher, 1935; Young, 2019).

After dropping the two treated pairs (Seattle 2017-18 and Seattle 2019-20), we have 101 unique city-cycle pairs in our donation data and 148 unique pairs in our election data. Since our main results feature two such pairs as “treated” (Seattle 2017-18 and 2019-20), for each placebo repetition (z), we assign two city-election cycle pairs and estimate Equation (5).

$$(5) \quad y_{rst} = \alpha_{s(z)} + \alpha_{t(z)} + \beta_{(z)} Treat_{st(z)} + \epsilon_{rst(z)}$$

For each outcome, this exercise yields $\frac{k(k-1)}{2}$ estimates of $\hat{\beta}_{(z)}$, where k is the number of city-cycle pairs. From this, we approximate the distribution of $\hat{\beta}$ under the null of no effect.

Table 7 compares our treatment effect estimates (from Tables 3-4) to quantiles of $\hat{\beta}_{(z)}$ as calculated from this procedure. Inference is qualitatively similar to what we would conclude from standard asymptotic results. The effect on total contributions is positive and on the margin of significance; effects on total donors, value of small donations, and number of small donors are large and highly significant. In fact, for many of our outcomes—in particular those involving number of donors—estimated treatment effects are larger in magnitude than the *extreme values* of the placebo distributions. Similar to our main results in Section 4, estimates on the value and number of private donations are insignificant. For election outcomes (Panel B), our estimated effect on the number of candidates is highly significant, while the significance of other effects is less clear.

6.6. Population vs. Sampled Data

Finally, we note that our analysis here uses population rather than sampled data. That is, as discussed by Abadie et al. (2020), in our setting, “there is no population such that the sample can be viewed as small relative to that population,” but rather “the estimand is the population value of that parameter.” Accordingly, standard methods that frame inference based on repeated resampling from an infinite population may not apply in this setting. We do not perform a finite-sample correction on our estimates, but note that Abadie et al. (2020) and Xu (2021) provide guidance on two fronts. First, they say that the “correct” standard errors are smaller than the canonical ones that we calculate. Accordingly, our inference may be conservative. Second, they suggest interpret-

TABLE 7
PERMUTATION QUANTILES UNDER NULL OF NO EFFECT

	Estimate	P-value	1%	5%	50%	95%	99%
Panel A: Donation Outcomes							
Contributions	31.535	0.112	-45.662	-33.278	-0.974	32.649	146.186
Donors	833.532	0.000	-101.904	-70.198	-1.925	75.308	116.591
Contributions $\leq$ \$200	51.056	0.000	-5.557	-3.914	-0.052	4.172	6.339
Donors $\leq$ \$200	829.863	0.000	-81.040	-52.250	-0.696	56.859	85.890
Private Contributions	-21.486	0.227	-45.662	-33.278	-0.974	32.649	146.186
Private Donors	72.492	0.100	-101.904	-70.198	-1.925	75.308	116.591
Panel B: Election Outcomes							
Number of Candidates	3.232	0.002	-1.979	-1.447	-0.036	1.469	2.727
Percent Vote for Winner	-0.036	0.616	-0.144	-0.106	-0.006	0.124	0.179
Incumbent Run	-0.362	0.050	-0.486	-0.344	0.023	0.273	0.355
Incumbent Win	-0.146	0.492	-0.493	-0.362	0.011	0.309	0.403
Percent Vote for Incumbent	-0.178	0.235	-0.354	-0.259	0.007	0.241	0.322

Notes: Estimates correspond to point estimates given in Tables 3 and 4 for Panels A and B, respectively. Table presents quantiles of estimated treatment effects with assignment of all unique sets of two city-election cycle pairs, excluding Seattle 2017-18 and Seattle 2019-20. Election cycles cover pairs of years (2001-02,...,2019-20). All outcomes are per race and are summarized in Table 2. Regression specification for each permutation given in Equation (5). Comparison cities identified in Appendix Table A.1. See Section 3.2 for discussion of data sources. Donation outcomes (Panel A) scaled to per-100,000 residents. For Donation Outcomes (Panel A), sample coverage by city and year is listed in final column of Appendix Table A.1. For Election Outcomes (Panel B), sample includes all elections held 2001-20 in all 15 cities. City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. Quantiles calculated from 5,050 permutations in Panel A, 10,878 permutations in Panel B.

ing uncertainty in this setting as arising from uncertainty about unobserved potential outcomes, which leads them to suggest permutation-type tests similar to those performed above.

7. CONCLUSION

In this paper, we present evidence of the effects of Seattle's novel Democracy Vouchers program on city council races during the first two cycles after implementation. We find that the program led to large increases in both total donations and total number of donors, increases that are largely explained by increased donations from small donors. We find suggestive but insignificant evidence of crowd-out of private donations. As to electoral competitiveness, we find a large and highly significant increase in the number of candidates per race coupled with substantively large but statistically weaker evidence of a decrease in measures of "incumbency advantage."

To our knowledge, our results are some of the first empirical evidence assessing the causal impact of large decentralized public funding mechanisms such as voucher or "multiple match" programs. These results are particularly important given proposals at both the national and local level aimed at expanding public funding, many of which include aspects modeled directly after Seattle's program.

Our results leave open a number of important issues for future research. First, while we provide results on donation and election outcomes, we do not measure *policy* outcomes. As reducing the importance of large donors in the policymaking process is often a stated goal of public funding schemes, results on policy outcomes are particularly important in assessing these programs' impacts.

Second, while we show that Seattle's program led to large increases in political participation via donation activity, other forms of political participation may also be impacted. For example, an important question is whether the Democracy Vouchers program or similar, decentralized programs lead to increased voter "turnout" or to increases in other forms of political participation.

Third, Seattle's program is still relatively novel, and its effects may change over time as donors and/or candidates adapt their behavior to the new environment. Will a higher percentage of vouchers be redeemed in the future? Will self-funded candidates choose to "opt out" due to spending limitations, as has occurred in public funding schemes elsewhere? Will patterns of heterogeneity in the program's effects emerge? Will campaigns change their behavior, such as by pivoting fundraising efforts toward newly-empowered voucher-holding small donors?

We leave these questions to future research. For now, our results provide evidence that Seattle's Democracy Voucher program succeeded in broadening the pool of donors and increasing the competitiveness of city council elections. Why this was true, whether these effects are persistent, and whether this program is scalable to other cities are important issues that we leave for future work.

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APPENDIX A: SUPPLEMENTAL TABLES AND FIGURES

TABLE A.1
DETAILS FOR SEATTLE AND COMPARISON CITIES

	Population		City Council Seats		Term	Donation Data
	2010	2020	Districted	At-Large	Limits	
Panel A: Seattle and Comparison Cities in Washinton						
Seattle	608,660	769,714	7	2	2003-19	
Spokane	208,916	222,050	6	1	2 Terms	2003-19
Tacoma	198,397	219,945	5	3	10 Years	2003-19
Vancouver	161,791	186,192	6	0	None	2003-19
Bellevue	122,363	148,073	0	7	None	2003-19
Kent	92,411	130,676	7	0	None	2003-19
Everett	103,019	112,482	7	0	None	2003-19
Panel B: Comparison Cities in California						
Fresno	494,665	530,267	7	0	2 Terms	2014-20
Los Angeles	3,792,621	3,970,219	15	0	3 Terms	2005-20
Long Beach	462,257	454,681	9	0	3 Terms	None
Oakland	390,724	424,891	7	1	None	2010-20
Sacramento	466,488	512,838	8	0	None	2008-20
San Diego	1,307,402	1,422,420	9	0	2 Terms	2008-20
San Francisco	805,235	866,606	11	0	2 Terms	2003-20
San Jose	945,942	1,013,616	10	0	2 Terms	None

Sources: For population, U.S. Census Bureau, Population Division (2020*b*, 2012) (Panel A); U.S. Census Bureau, Population Division (2020*a*, 2012) (Panel B). Council details collected from public city council websites. Notes: All districts are single member, with the exception of Spokane which has three districts that elect two members each. All council members serve four-year terms. San Francisco has a joint city/county government with a Board of Supervisors.

FIGURE A.1: TREND GRAPHS FOR SMALL DONATIONS **(A) TOTAL CONTRIBUTIONS $\leq$ \$200****(B) NUMBER OF DONORS $\leq$ \$200**

Source: Authors' calculations from data described in Section 3.2.

Notes: All outcomes are per race. Bars show means by comparison group (Seattle, Washington Comparison Cities, California Comparison Cities) across four-year election cycles. Error bars show 90% confidence intervals, calculated from asymptotic normal distribution with standard errors clustered by city-election cycle. Comparison cities identified in Appendix Table A.1. All outcomes scaled to per-100,000 residents. Sample coverage by city and year is listed in final column of Appendix Table A.1.

FIGURE A.2: TREND GRAPHS FOR PRIVATE DONATIONS **(A) TOTAL PRIVATE CONTRIBUTIONS****(B) NUMBER OF PRIVATE DONORS**

Source: Authors' calculations from data described in Section 3.2.

Notes: All outcomes are per race. Bars show means by comparison group (Seattle, Washington Comparison Cities, California Comparison Cities) across four-year election cycles. Error bars show 90% confidence intervals, calculated from asymptotic normal distribution with standard errors clustered by city-election cycle. Comparison cities identified in Appendix Table A.1. All outcomes scaled to per-100,000 residents. Sample coverage by city and year is listed in final column of Appendix Table A.1.

TABLE A.2
EFFECTS ON DONATION OUTCOMES
(WASHINGTON AND CALIFORNIA COMPARISONS SEPARATELY)

	(1)	(2)	(3)	(4)	(5)	(6)
	All		$\leq \$200$		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Panel A: Washington Comparison Cities						
Post $\times$ Seattle	35.714*** (9.713)	852.401*** (53.828)	52.907*** (3.566)	854.367*** (54.422)	-17.966 (12.277)	81.375 (56.864)
Observations	248	248	248	248	248	248
R-squared	0.745	0.802	0.818	0.808	0.724	0.714
Mean in Seattle 2003-2016	59.872	307.529	16.677	226.677	59.872	307.529
Panel B: California Comparison Cities						
Post $\times$ Seattle	18.686 (23.866)	811.422*** (50.937)	49.676*** (4.036)	812.480*** (56.862)	-35.091 (27.644)	38.927 (35.701)
Observations	269	269	269	269	269	269
R-squared	0.339	0.832	0.848	0.849	0.323	0.716
Mean in Seattle 2003-2016	59.872	307.529	16.677	226.677	59.872	307.529

Notes: All outcomes are per race and are summarized in Table 2, Panel A. See Section 3.2 for discussion of data sources. Outcomes scaled to per-100,000 residents. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample coverage by city and year is listed in final column of Appendix Table A.1. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.3
SMALL AND LARGE DONATIONS
(DEFINING SMALL AS $\leq \$100$)

	(1)	(2)
	$\leq \$100$	
	Cont	Donors
Post $\times$ Seattle	47.927*** (3.934)	804.716*** (54.378)
Observations	471	471
R-squared	0.832	0.808
Mean in Seattle 2003-2016	10.347	189.219

Notes: All outcomes are per race. See Section 3.2 for discussion of data sources. Outcomes scaled to per-100,000 residents. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample coverage by city and year is listed in final column of Appendix Table A.1. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.4
EFFECTS ON ELECTION OUTCOMES
(WASHINGTON AND CALIFORNIA COMPARISONS SEPARATELY)

	(1)	(2)	(3)	(4)	(5)
	Number of Candidates	% Vote for Winner	Incumbent Run	Incumbent Win	Vote %
Panel A: Washington Comparison Cities					
Post $\times$ Seattle	2.847*** (0.412)	-0.016 (0.043)	-0.322*** (0.118)	-0.234** (0.111)	-0.164* (0.090)
Observations	271	271	271	271	271
R-squared	0.864	0.958	0.681	0.604	0.627
Mean in Seattle 2001-2016	3.756	0.654	0.780	0.659	0.483
Panel B: California Comparison Cities					
Post $\times$ Seattle	3.225*** (0.645)	-0.030 (0.031)	-0.351*** (0.112)	-0.077 (0.125)	-0.155* (0.079)
Observations	467	467	467	467	467
R-squared	0.708	0.947	0.676	0.619	0.638
Mean in Seattle 2001-2016	3.756	0.654	0.780	0.659	0.483

Notes: All outcomes are per race and are summarized in Table 2, Panel A. See Section 3.2 for discussion of data sources. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample includes all elections held 2001-20 in all 15 cities. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special $\times$ At-Large included in all specifications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.5
PARALLEL TRENDS REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Donation Outcomes						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Year	1.745*** (0.487)	4.865*** (1.639)	0.240*** (0.087)	2.903** (1.249)	1.745*** (0.487)	4.865*** (1.639)
Year × Seattle	-2.158 (1.322)	-11.878** (5.451)	-0.812*** (0.300)	-9.552** (4.625)	-2.158 (1.322)	-11.878** (5.451)
Observations	354	354	354	354	354	354
R-squared	0.662	0.719	0.724	0.701	0.662	0.719
P-value	0.107	0.032	0.008	0.042	0.107	0.032
Panel B: Election Outcomes						
	Number of Candidates	% Vote for Winner	Incumbent			
			Run	Win	Vote %	
Year	-0.019 (0.026)	-0.003 (0.002)	-0.003 (0.005)	-0.002 (0.006)	-0.004 (0.004)	
Year × Seattle	0.040 (0.081)	0.004 (0.004)	-0.013 (0.013)	-0.007 (0.016)	-0.005 (0.011)	
Observations	553	553	553	553	553	
R-squared	0.715	0.945	0.659	0.605	0.607	
P-value	0.620	0.279	0.323	0.644	0.642	

Notes: All outcomes are per race and are summarized in Table 2. See Section 3.2 for discussion of data sources. All analyses restricted to pre-2017, prior to implementation of Democracy Vouchers. Donation Outcomes (Panel A) scaled to per-100,000 residents. Regression specification in Equation (2). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. For Donation Outcomes (Panel A), sample coverage by city and year is listed in final column of Appendix Table A.1. For Election Outcomes (Panel B), sample includes all elections held 2001-20 in all 15 cities. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special × At-Large included in all specifications. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.6
ESTIMATED EFFECTS UNDER BALANCED PANEL DIFFERENCE-IN-DIFFERENCES

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Donation Outcomes						
<i>Sample: All Washington Cities 2003-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	24.650	842.309	52.681	860.863	-32.659	16.258
Fraction of Permutations Larger Than	6 / 6	6 / 6	6 / 6	6 / 6	6 / 6	2 / 6
Panel B: Donation Outcomes						
<i>Sample: All Washington Cities and LA, SD, SF, Sacramento 2007-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	24.444	891.783	55.424	895.124	-32.865	65.733
Fraction of Permutations Larger Than	8 / 10	10 / 10	10 / 10	10 / 10	9 / 10	9 / 10
Panel C: Election Outcomes						
<i>Sample: All Washington and California Cities 2001-20</i>						
	Number of	% Vote	Incumbent			
	Candidates	for Winner	Run	Win	Vote %	
Treatment Effect	3.641	-0.029	-0.347	-0.179	-0.191	
Fraction of Permutations Larger Than	14 / 14	6 / 14	13 / 14	7 / 14	12 / 14	

Notes: Table presents estimates using difference-in-differences method with two-way fixed effects with balanced panel of cities and election cycles. Per-race outcomes, summarized in Table 2, are collapsed to be mean by city-election cycle, in order to create a balanced panel. Comparison cities identified in Appendix Table A.1. See Section 3.2 for discussion of data sources. Donation outcomes (Panels A and B) scaled to per-100,000 residents. "Fraction of Permutations Larger Than" gives the proportion of "placebo" treatments that the estimated treatment effect is larger than in absolute terms.

TABLE A.7
ESTIMATED EFFECTS UNDER SYNTHETIC CONTROL METHOD

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Donation Outcomes						
<i>Sample: All Washington Cities 2003-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	39.600	913.133	56.531	907.296	-17.709	87.083
Fraction of Permutations Larger Than	6/6	6/6	6/6	6/6	3/6	5/6
Panel B: Donation Outcomes						
<i>Sample: All Washington Cities and LA, SD, SF, Sacramento 2007-20</i>						
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Treatment Effect	20.283	939.063	58.509	928.928	-37.026	113.012
Fraction of Permutations Larger Than	5/10	10/10	10/10	10/10	8/10	10/10
Panel C: Election Outcomes						
<i>Sample: All Washington and California Cities 2001-20</i>						
	Number of	% Vote	Incumbent			
	Candidates	for Winner	Run	Win	Vote %	
Treatment Effect	4.248	-0.017	-0.193	0.005	-0.078	
Fraction of Permutations Larger Than	14/14	1/14	10/14	2/14	3/14	

Notes: Table presents estimates using synthetic control method, as implemented using “synth” command in Stata. Per-race outcomes, summarized in Table 2, are collapsed to be mean by city-election cycle, in order to create a balanced panel. Comparison cities identified in Appendix Table A.1. See Section 3.2 for discussion of data sources. Donation outcomes (Panels A and B) scaled to per-100,000 residents. “Fraction of Permutations Larger Than” gives the proportion of “placebo” treatments that the estimated treatment effect is larger than in absolute terms.

TABLE A.8
RESULTS IN LEVELS

	(1)	(2)	(3)	(4)	(5)	(6)
	All		≤ \$200		Private Donations	
	Cont	Donors	Cont	Donors	Cont	Donors
Post × Seattle	30.465 (158.178)	6,081.349*** (393.535)	374.086*** (29.993)	6,169.625*** (392.256)	-364.398** (175.118)	414.208 (288.989)
Population (in 100k)	291.519** (117.259)	376.460** (181.474)	16.681 (10.787)	133.071 (91.576)	291.978** (116.454)	383.448** (169.735)
Observations	471	471	471	471	471	471
R-squared	0.355	0.777	0.802	0.832	0.345	0.662
Mean in Seattle 2003-16	369.8	1898	102.6	1397	369.8	1898

Notes: All outcomes are per race and are summarized (per-100,000 residents) in Table 2, Panel A. See Section 3.2 for discussion of data sources. Outcomes in raw terms, not scaled to per-100,000 residents. Regression specification in Equation (1). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison cities identified in Appendix Table A.1. Sample coverage by city and year is listed in final column of Appendix Table A.1. Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). City and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special × At-Large included in all specifications. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.9
COUNTY-LEVEL RESULTS

	(1)	(2)	(3)	(4)	(5)
Panel A: Donation Outcomes					
	All		≤ \$200		
	Contrib	Donors	Contrib	Donors	
King × Post	25.216 (34.680)	35.036 (138.107)	2.706 (8.141)	6.913 (106.580)	
Observations	156	156	156	156	
R-squared	0.686	0.690	0.694	0.672	
Mean in King County 2003-2016	165.936	808.340	43.285	618.936	
Panel B: Election Outcomes					
	Number of Candidates	% Vote for Winner	Incumbent		
			Run	Win	Vote %
King × Post	-0.079 (0.350)	0.003 (0.033)	0.225** (0.090)	0.192* (0.100)	0.181*** (0.068)
Observations	498	498	498	498	498
R-squared	0.705	0.946	0.751	0.703	0.715
Mean in King County 2001-2016	1.936	0.822	0.787	0.723	0.637

Notes: All outcomes are per race. See Section 3.2 for discussion of data sources. Donation Outcomes (Panel A) scaled to per-100,000 residents. Regression specification in Equation (4). Standard errors in parentheses, clustered by city-election cycle. Election cycles cover pairs of years (2001-02,...,2019-20). Comparison counties for Panel A are Clark, Pierce, Snohomish, and Spokane (in Washington). Comparison counties for Panel B are Clark, Pierce, Snohomish, and Spokane (in Washington) and also Alameda, Contra Costa, Fresno, Kern, Los Angeles, Orange, Riverside, Sacramento, San Bernardino, San Diego, San Francisco, and Santa Clara (in California). Post is an indicator for post-implementation of Democracy Vouchers in Seattle (election cycles 2017-18 and 2019-20). County and election cycle fixed effects included in all specifications. Controls for Special, At-Large and Special × At-Large included in all specifications. *** p<0.01, ** p<0.05, * p<0.1.