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Mind the (Participation) Gap: Vouchers, Voting, and Visibility

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Abstract

This study exploits the introduction of a new type of public financing of elections—campaign finance vouchers—to estimate the effects of neighborhood-level political cross-pressure on citizens' decisions to participate in low-cost political activities which vary in their publicness: voting (private) and vouchering (public). Does proximity to ideologically divergent neighbors affect one's use of publicly disclosed campaign finance vouchers? We find that cross-pressured individuals are slightly *more* likely to use a campaign finance voucher than similarly situated individuals who are ideologically typical for their precinct. We also find evidence that precinct-level cross-pressure does not drive voucher users to shade their voucher donations toward candidates who are ideologically closer the precinct mean. While our study is limited to a relatively liberal city (Seattle), our results replicated across two election cycles in that city, and our methods can easily be extended to future elections. Finally, our findings raise questions about the empirical assumptions that have shaped the development of campaign finance jurisprudence since 1976.

Keywords

Campaign finance, disclosure, vouchers, political cross-pressure, matching

Introduction

Most campaign finance regimes in the United States require public disclosure of private contributions to candidates in excess of a modest threshold ($200 in federal elections, less in many state and local elections). Critics of mandatory disclosure argue that it chills individuals from exercising their First Amendment right to speak and associate through the contribution of money to candidates, and that this chilling effect is particularly likely to occur among political minorities. Disclosure is said to muffle the voices of people who would otherwise contribute to the marketplace of ideas. In recognition of this risk, the U.S. Supreme Court requires disclosure exemptions for donors to minor parties and to independent candidates when there is a demonstrable risk of harassment. *Buckley v. Valeo*, 424 U.S. 1, 64–74 (1976).

Whether disclosure chills political participation is an empirical question (Wood, 2018). The relationship between disclosure and participation is difficult to untangle because no extant disclosure regime was randomly assigned or otherwise rolled out in a fashion that would allow researchers to isolate plausibly comparable "treatment" and "control" groups. Nor can researchers make headway on the question using a discontinuity design because donations below disclosure thresholds are not observed. In this study, we attempt to shed some new light on this old and difficult question by leveraging an innovative local campaign financing program. Although small-dollar public funding programs have been advocated for decades (see, e.g., Ackerman & Ayres, 2002; de Figueiredo & Garrett, 2005; Foley, 1994; Hasen, 1996; Overton, 2004), the first publicly funded system of campaign finance vouchers was adopted only recently, by the city of Seattle, Washington. Each registered voter receives four $25 vouchers, which may be assigned to candidates running for local office. The vouchers are accompanied by information about disclosure of the voucher contributions (Appendix A). Candidates who wish to redeem vouchers agree to various restrictions on private fundraising and demonstrate their viability by raising small private donations. Research concurrent with ours has shown that Seattle's voucher program expanded the campaign finance donor pool (McCabe & Heerwig, 2018), though not yet to the extent that

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advocates have promised.[1] Because all voucher contributions are publicly disclosed, Seattle's program allows us to analyze the effects of political cross-pressure on the decision to assign campaign finance vouchers.

Important to our research design, Seattle's voucher program equalizes the personal financial costs of voting and contributing money (up to the voucher amount). Both votes and vouchers can be assigned by mail or by waiting in line, and without payment of a fee. Yet, the privacy costs of voting and vouchering are very different. Seattle does not record, let alone release, information about which candidate received the vote of which voter, whereas vouchering is public in exactly this way. Because voting and vouchering both provide support to the candidate on the receiving end, and because both forms of support can be provided at negligible cost to the voter, we would expect eligible individuals to make the same participate-or-abstain decision with respect to both political resources—*if* voting and vouchering were equally public, if the choice sets were identical, and if voters were equally knowledgeable about both programs. See Table 1. However, the latter two conditions were not satisfied in the 2017 Seattle election. The voucher program was brand new (so citizens probably knew less about it than they knew about voting), and candidates in the highest profile race on the ballot (mayor) were not eligible to receive vouchers. Moreover, spending limits and limited appropriations for the voucher program may have discouraged candidates from mobilizing voucher contributions with the same gusto that they mobilize votes.[2] Due to some combination of these factors, we observe a large baseline participation gap. The citywide rate of participation by vouchering was approximately 4.5% in 2017 (up from 1 to 2% in the fully private system), and the voter turnout rate was 63%.

Our goal in this paper is to assess whether variations in the participation gap may be explained by neighborhood-level cross-pressure. Our strategy is to test whether voucher use varies systematically between individuals who are ideologically similar to their neighbors and individuals who are local political outliers, relative to baseline rates of turnout. Put differently, we estimate the treatment effect of *neighborhood ideological cross pressure* on registered voters' private (votes) and public (vouchers) political participation decisions.

Table 1. Comparison of Different Modes of Political Participation.

	Costly	Free
Public	Private campaign Donations	Vouchers
Private		Voting

Note. Campaign finance vouchers allow us to compare political participation in two free activities, one of which is public (voucher donation) and one of which is private (voting).

We use two matching strategies to estimate the effect of neighborhood cross-pressure on voting and vouchering. In the first analysis, registered voters in the treatment group (distant from their neighborhood's ideological mean) and registered voters in the control group (similar to their neighborhood's ideological mean) are matched on ideology and on a measure of their propensity to participate in politics. For the second analysis, which was not pre-registered, we do not match on ideology, in effect treating ideologically moderate Seattleites as counterfactuals for extremists (conditional on their propensity to participate in politics). We added the second design, which rests on much stronger assumptions, after discovering that there is little variance in the mean ideology of neighborhoods in Seattle. This means that the treatment dose in our pre-registered design is very small—the registered voters classified as distant from their neighborhood mean are in reality fairly close to it, just not as close as those who are classified as typical.

Results from both designs are similar and do not support the hypothesis that neighborhood-level cross-pressure causes local political outliers to participate less in public (vouchering) relative to private (voting) political activity. The observed effects of neighborhood-level dissonance on voting and vouchering are both very small and, if anything, local political outliers are *less* likely to vote but *more* likely to voucher than otherwise similar registered voters who are not local outliers. This cuts against the conventional wisdom about the supposed chilling effect of disclosure. We replicated our design on data from the 2019 election and find similar results for vouchering among politically cross-pressured voters, when compared to population matches who were not cross-pressured.

We also estimate the effect of neighborhood cross-pressure on donors' choice of voucher recipients. Among pairs of voters matched on ideology and propensity to participate, we find that those who are neighborhood cross-pressured are less likely to give to candidates whose ideology is similar to the donor's own ideology. Oddly, though, the vouchers assigned by these cross-pressured voters are *not* more likely to go to candidates who are close to the ideological mean of their neighborhood, relative to the vouchers assigned by the matched-pair voter in the control group. Thus, while cross-pressure appears to have some effect on the choice of whom to contribute, it is not the social-conformity (homophily) effect we hypothesize.

Our study is limited to a relatively liberal city (Seattle), and it remains to be seen whether our findings will generalize to other jurisdictions. But because we find no evidence of chilling or neighborhood-conformity effects, our results suggest that courts should be cautious about imposing an anonymity requirement on voucher programs.[3]

Cross-Pressure, Ideology, and Campaign Finance Participation

Some people are embedded in ideologically congenial social networks. Other people are political minorities within their networks; they experience "cross-pressure" between their personal political beliefs and those of their friends, neighbors, co-workers, and the like. At least since Campbell et al. (1960)'s seminal study, *The American Voter*, social scientists have been trying to understand how cross-pressure from one's social environment affects political participation. Mutz (2002) conducted two in-depth surveys with individuals who reported discussing politics with others. Respondents who were exposed to different viewpoints were much less likely to report having voted than respondents who had discussed politics with like-minded colleagues. Cross-pressured respondents also reported lower rates of public political participation (e.g., working for a campaign or attending a rally). McClurg (2006), used data from a 1984 survey of residents of 16 neighborhoods in South Bend, Indiana, and found that local political minorities are less likely to work on a campaign, display a bumper sticker or sign, donate money, attend political meetings, or vote.[4]

Mutz (2002) notes that these effects of cross-pressure could be due to self-doubts induced by exposure to competing ideas, to a fear of being shamed by one's peer group, or to a combination of these factors. If the mechanism is induced self-doubt, the effect should manifest equally with respect to anonymous (e.g., voting) and publicly visible (e.g., displaying campaign signs, making disclosed campaign contributions) forms of political participation. By contrast, if shaming or outright harassment is to blame, only visible forms of political behavior are likely to be affected.[5]

Critics of campaign finance disclosure requirements often raise the specter of shaming and harassment. In *Buckley v. Valeo*, 424 U.S. 1 (1976), the Supreme Court assumed that disclosure would demobilize participation among supporters of independents and minor-party candidates, because of cross-pressure from supporters of the two main political parties who dominate the social environment. *Id.* at 70. The Court's assumed mechanism of chilling is the risk of harassment (Torres-Spelliscy, 2012). On the other hand, it is possible that disclosure could actually encourage participation by some citizens—those who enjoy credibly signaling their support for particular candidates (Gilbert, 2013).

Three recent studies present mixed results about the relationship between campaign contributions, political cross-pressure, and social pressure. Using a survey experiment, La Raja (2014) finds that reminding respondents about disclosure depresses (hypothetical) campaign contributions among persons who report having different political views than their family, co-workers, and neighborhood.[6] Oklobdzija (2019) compared the ideological distribution of donors to a "dark money" organization, which took the conservative side on a ballot initiative, to the ideological distribution of donors to transparent groups on the same side. He found that donors to the non-disclosing group—donors who had a reasonable expectation of anonymity—were more liberal on average (per Bonica's 2013 ideology scores) than donors to the disclosing groups. This could be evidence of disclosure chilling or "distorting" public contributions.

On the other hand, Wood and Spencer (2016) find little evidence that disclosure rules chill political participation among ideological outliers. Analyzing historical campaign contribution data in state elections, they leverage variation in the strength of disclosure policies between states and across time, finding that disclosure has a small negative effect on the willingness to contribute, but that the negative effect is statistically indistinguishable between mainstream donors and ideological outliers within zip codes.[7]

Following existing theories and prior studies, our first hypothesis is that under a voucher regime with full disclosure, citizens who are political minorities in their neighborhoods will be less likely to use their vouchers than they would be if they lived in a neighborhood where they are politically typical. By contrast, we expect to find no effect of neighborhood cross-pressure on the private form of low-cost political support, that is, voting. Our second hypothesis is that the effect of neighborhood cross-pressure on voucher use will be larger among conservatives than among liberals. Conservative-elite opposition to vouchers and disclosure may legitimate opting out among the conservative masses, as a kind of protest against the voucher program or the disclosure requirement (Druckman et al., 2013; McGraw et al., 1995). Moreover, in a generally liberal city such as Seattle, conservative political outliers may face greater opprobrium for their choices than far-left outliers.

Third, we test the hypothesis that, conditional on using one's voucher, neighborhood cross-pressure will induce insincere voucher assignments. Rather than giving to candidates who are close to her own ideology, the cross-pressured voter will give to candidates near the mean ideology of her neighborhood. This builds on recent work finding that donors who moved districts changed their donation patterns, donating a larger share of their contributions to Democrats once they moved to a more Democratic district (Kettler & Lyons, 2019).

As the next section explains, only candidates for city offices were eligible to receive vouchers in the 2017 election. To the extent that municipal politics are nonideological, this limitation would presumably render neighborhood cross-pressure irrelevant. But as Warshaw (2019) concludes, "recent work has overturned the longstanding consensus that local politics was essentially nonideological." (see also Boudreau et al. (2015a, 2015b); Tausanovitch and Warshaw (2014, 2013)) Scholars who have undertaken to map local and national issue spaces find the same main left-right dimension of conflict (Cann, 2018; Tausanovitch & Warshaw, 2014). Moreover, the policies enacted by local governments are responsive to mass opinion, particularly in the domain of social policy (Warshaw, 2019). Many of the hot-button social issues of our day—policing, criminal justice, single-family zoning, school segregation, tolerance for public disorder, even guns and immigration—are the stuff of city politics.

As in many other big cities, some candidates run for office in Seattle as far-left vanguardists, while others style themselves as centrist problem solvers. For example, in the election we studied, city-council candidate Teresa Mosqueda ran as a "Progressive Labor Democrat," promoting workers' rights, racial equity, affordable-housing development, and higher taxes on big business.[8] Similarly, candidate Jon Grant urged high affordability mandates on housing development, a new corporate tax, safe-injection sites for drug users, and restorative justice programs in lieu of traditional policing and incarceration.[9] Candidate Hisam Goueli, by contrast, said that the city should reduce regulatory barriers to housing development. He argued that the affordable-housing mandates would backfire, "causing market-rate prices to soar, displacing even more middle-class families"[10] While we would not expect neighborhood cross-pressure to affect vouchering behavior in Seattle council races as much as it might in, say, a race for President or Congress, we certainly expect it to have some effect. We also suspect that the early adopters of vouchers (i.e., the residents who used their vouchers in the 2017 and 2019 elections) are probably relatively well informed about city politics and the political opinions of their neighbors. It is among this segment of the electorate where the effects of neighborhood cross-pressure are most likely to manifest.

Data and Research Design

The Seattle Voucher Program and the 2017 Election

Our study site is Seattle, Washington, home of Honest Elections Seattle, which is the first and, to date, only campaign finance voucher program in the United States. Seattle launched its voucher program in 2017. All Seattle residents who were registered to vote as of November 2016 were mailed four \$25 campaign finance vouchers on January 2, 2017.[11] For each voucher assigned, Honest Elections Seattle discloses the name of the contributor, the name of the assignee (candidate), the date of assignment, and voucher ID number. In 2017, only candidates running for city council or city attorney were eligible to receive voucher assignments. To redeem vouchers, candidates had to make a showing of viability and accept private contribution limits and spending limits. In all, five candidates vying for a city council seat qualified to collect/spend vouchers in the August 1, 2017 primary election, and four of those five (two for each seat) proceeded to the general election where they remained eligible. The incumbent city attorney qualified for the program; his opponent did not.

The choice set on the ballot was very similar across precincts.[12] However, the choice set for voting was substantially different than the choice set for vouchering. The top-of-the-ticket race was for mayor, and mayoral candidates weren't eligible to receive voucher assignments. Because the ballot choices were very similar across precincts in 2017, we assume there was no differential mobilization of ideologically similar people in different precincts. (This assumption is necessary to our design, as we treat the observed outcome of a voter in one precinct as the counterfactual outcome for a voter in another precinct)

Data and Notation

With respect to the subjects of our study, let i represent each individual who was registered to vote on the date of the first voucher mailing, with $i \in \mathrm{I}$, or the set of all registered voters on January 2, 2017. Let c represent individual candidates who are eligible to receive vouchers, with $c \in \mathrm{C}$, or the set of all eligible candidates. P_i represents the turnout propensity for each registered voter, as estimated by the private voter file data firm Catalist LLC, with $P_i \in [0, 100]$. Let L_i represent each registered voter's liberalism as estimated by Catalist,[13] with $L_i \in [0, 100]$.

With respect to geography, let h represent each neighborhood in Seattle, which we define as coterminous with voting precincts, and $h \in \mathcal{H}$ or all Seattle neighborhoods. $\overline{L_h}$ represents the arithmetic mean of L_i for the subset of I residing in neighborhood h. Let D_i represent the absolute ideological distance from registered voter i to the mean of her neighborhood, such that $D_i = |L_i - \overline{L_h}|$. Further, we define $T_i \in \{0, 1, 2\}$ as an indicator for whether individual i is ideologically atypical for her neighborhood ($T_i = 2$), ideologically typical for her neighborhood mean ($T_i = 0$), or somewhere in between ($T_i = 1$).[26] We code registered voters with an ideological distance D_i in the top quartile as "ideologically atypical" ($T_i = 2$) and voters with an ideological distance in the bottom quartile of the measure as "ideologically typical" ($T_i = 0$). We test our first hypothesis, about the effect of local political minority status on participation in the voucher regime, by estimating the effect of switching T_i from 0 to 2.

We define the "participation gap" as the difference between the choice to use one's vouchers (V) and the choice to use one's ballot (B). Let $Y_i^{\mathrm{V}}(T_i = t)$ represent an indicator denoting the choice (potential outcome) of i to use her voucher when $T_i = t$, and $Y_i^{\mathrm{B}}(T_i = t)$ represent an indicator denoting the choice (potential outcome) of i to cast a ballot when $T_i = t$. Formally, the participation gap Y_i^{G} is

$$Y_i^{\mathrm{G}}(T_i = t) = \left[Y_i^{\mathrm{B}}(T_i = t) - Y_i^{\mathrm{V}}(T_i = t)\right] \tag{1}$$

The participation gap increases as ballot use increases and as voucher use declines. The percent of registered voters in Seattle who turned out in the 2017 general election was 63.6% while only 4.5% of registered voters used their vouchers, for a baseline, population-level participation gap of 59.1%. (Table 2)

Identifying Assumptions and Matching Strategies

For each version of Y_i (B for ballot, V for voucher, G for gap) we only observe Y_{2i} (Y_i for the treated units) if $T_i = 2$

Table 2. Summary Statistics.

	Term	Source	Mean	Min	Max	σ	N. Obs
Liberalism	L_i	Catalist	57.96	6.1	90.5	11.06	472 681
Absolute ideological distance	$\lvert L_i - \overline{L_h}\rvert$	Authors	8.19	0	50.06	6.73	472 681
Real ideological distance	$L_i - \overline{L_h}$	Authors	0	−50.06	37.10	10.60	472 681
Turnout probability	P_i	Catalist	73.67	0.36	99.79	26.82	472 681
Voucher used	Y_i^V	SEEC	0.045	0	1	0.19	472 681
Voted Nov 17	Y_i^B	King County	0.53	0	1	0.50	472 681
Age		Catalist	45.61	18	114	17.26	472 681
Female		Catalist	0.51	0	1	0.50	472 681
Black		Catalist	0.05	0	1	0.22	472 681
Asian		Catalist	0.09	0	1	0.29	472 681
Hispanic		Catalist	0.03	0	1	0.18	472 681
White		Catalist	0.81	0	1	0.40	472 681

Notes. The table presents summary statistics of the campaign finance voucher program during the 2017 municipal elections in Seattle, WA. The data are derived from the Seattle Ethics and Elections Commission (SEEC), the King County voter file, and Catalist LLC.

but not if $T_i = 0$. Similarly, we only observe Y_{0i} (Y_i for the control units) if $T_i = 0$ but not if $T_i = 2$. Therefore, we must assume that, conditional on covariates, we can treat the observed voucher-participation decisions of residents who are ideologically typical for their neighborhoods as a proxy for the unobserved decisions that residents who are ideologically atypical for their neighborhood *would* make if they moved to a neighborhood where they were typical. Similarly, we must be able to treat the observed voucher-participation decisions of citizens who are ideologically atypical for their neighborhoods as a proxy for the unobserved decisions that citizens who are ideologically typical for their neighborhood *would* make if they moved to a neighborhood where they were atypical

$$\mathrm{E}[Y_{0i}|T_i = 2, L_i, P_i] = \mathrm{E}[Y_{0i}|T_i = 0, L_i, P_i] \tag{2}$$

$$\mathrm{E}[Y_{2i}|T_i = 2, L_i, P] = \mathrm{E}[Y_{2i}|T_i = 2, L_i, P_i] \tag{3}$$

We condition on ideology (L_i), propensity to vote (P_i), race (white/nonwhite), gender, and age. Ideology may well be correlated with voucher participation, owing to a liberal skew in the set of eligible donees (see Appendix D), or because citizens' enthusiasm for the voucher program may vary with ideology. The propensity to vote may be correlated with voucher use to the extent that Catalist's estimate reflects a latent orientation to participate in politics more generally. Our reported estimates correspond to a local average treatment effect, $\tau = \mathrm{E}[Y_{2i} - Y_{0i}]$, among the subset of voters who are matched on ideology, propensity to vote, and demographic characteristics.

One might worry that in matching on propensity to vote (or even ideology), we are matching on a post-treatment variable. It is true that we do not observe voters' ideology or propensity to vote prior to their moving to the neighborhood where they lived during the election for which we have data. If most citizens become less likely to vote when living in a politically uncongenial neighborhood, and if Catalist's propriety model for P_i gives weight to eligible voters' recent decisions to vote or abstain, then by matching on P_i we may be selecting control units whose latent orientation to participate in politics, independent of neighborhood, is weaker on average than that of the treatment units. This could bias toward zero our estimate of the effect of neighborhood on political participation (voting or vouchering). However, as we show with QQ plots in Appendix B1, the distribution of P_i is the same among registered voters who are neighborhood political outliers and registered voters who are not. This suggests that the Catalist propensity-to-vote measure is picking up latent characteristics of the citizen, rather than effects of the neighborhood on the citizen's behavior.[14]

We sort registered voters into 20 liberalism quantiles and 20 vote propensity quantiles and then match registered voters within each quantile for whom $T = 0$ with registered voters in that band for whom $T = 2$.[15] We require exact matching on liberalism quantiles and vote propensity quantiles and nearest neighbor matching on demographics (age, gender, race). The matching process reduces our data from 472,682 observations to 77,372. Figure 1 presents the absolute mean differences for the unadjusted and adjusted sample analyzed in our matching analysis. Matching improves balance for all variables, except for liberalism in the high-dose match, since we do not match on liberalism for that dataset.[16]

Figure 2(a) shows the common support on ideology for registered voters who are ideologically typical ($T = 0$) versus atypical ($T = 2$) of their precinct. Figure 2(b) shows the common support for treatment and control units on turnout propensity. These figures point to a difficulty with our preregistered design: there is very little common support on ideology in the atypical (treatment) and typical (control) groups. The precinct means are clustered in a small moderate-liberal zone; the interquartile range of precinct means is 55.37–60.26 (see Appendix D1). In the area of common support, voters range from centrist to moderately liberal. While voters whose views are very dissonant with their neighbors may be chilled from contributing vouchers without anonymity guarantees, it strikes us as doubtful that centrist voters would be materially less likely to use their vouchers if they live in a center-left rather than a center-center precinct, or that center-left voters would be less likely to use their vouchers if they live in a center-center rather than a center-left precinct. Thus, the voters that we hypothesize are most likely to be chilled—those nearer to the ideological extremes—are never observed in neighborhoods where they are typical.

Given the limited area of common support, our pre-registered design is akin to a low-dose experiment. For comparison, the standard deviation of ideology among registered voters in Seattle is about 11, and the mean (and median) ideological distance of a treatment-group voter from her precinct mean is around 13.5. Only about half of the treatment-group voters have an ideology more than one standard deviation away from the average ideology of their precinct. Because of this limitation, which we did not foresee when we submitted our pre-analysis plan for this project, we also present results of a "higher dose" design where we drop ideology and match only on turnout propensity, age, gender, and race. This design allows us to make use of observations from voters who are very distant from their precinct mean, but it rests on the very strong assumption that, conditional on P_i and demographics, these extreme voters have the same potential outcomes (voting and vouchering) in this election as the centrist and center-left voters who are near their precinct means. Put differently, the "high-dose" design assumes no differential mobilization by ideology.

Findings and Analysis

Neighborhood Effects on the Decision to Use Vouchers

Our first hypothesis is that exposure to (residence in) an ideologically incongruent neighborhood will make registered voters less likely to use their vouchers than if they lived in a neighborhood where they were typical. We estimate the effects of cross-pressure on political participation using regression analysis on the matched data.[17] We present the results in Table 3 . In both the low-dose and the high-dose designs, neighborhood has only negligible effects on voting and vouchering; less than one percentage point. While several of the confidence intervals cross zero, the estimates are precisely measured as near-zero effects. Contrary to our hypothesis, the participation gap (voting minus vouchering) is slightly smaller in the treatment group than in the control group, In other words, local political outliers are slightly *more* likely than non-outliers to use their vouchers, though the lower end confidence interval of one of the four estimates is -0.001. In sum, there is no evidence that the publicness of vouchering chills participation by local political outliers, notwithstanding the fact that the participation gap is huge in absolute terms (the baseline rate of vouchering in the control groups is 0.05–0.06, whereas the corresponding rate of voting is between 0.6 and 0.7.)

Although we only pre-registered models analyzing the 2017 election, we have since collected data from the 2019 election that allows us both to replicate our findings with new data and to conduct a longitudinal analysis comparing within-voter changes in behavior. Since these analyses were not pre-registered, they should be considered exploratory. More voters used vouchers in 2019 (6.6%) than in 2017 (4.5%), and cross-pressured voters in 2019 were, on average, 1.5 percentage points more likely to use vouchers and two percentage points more likely to vote than those not cross-pressured. (Again, bear in mind that the baseline rate of voting is an order of magnitude larger than the baseline rate of vouchering.) Between 2017 and 2019, about 25% of voters moved between precincts, allowing us to conduct difference-in-difference analyses comparing (1) those who moved into areas where they became cross-pressured in 2019 to those

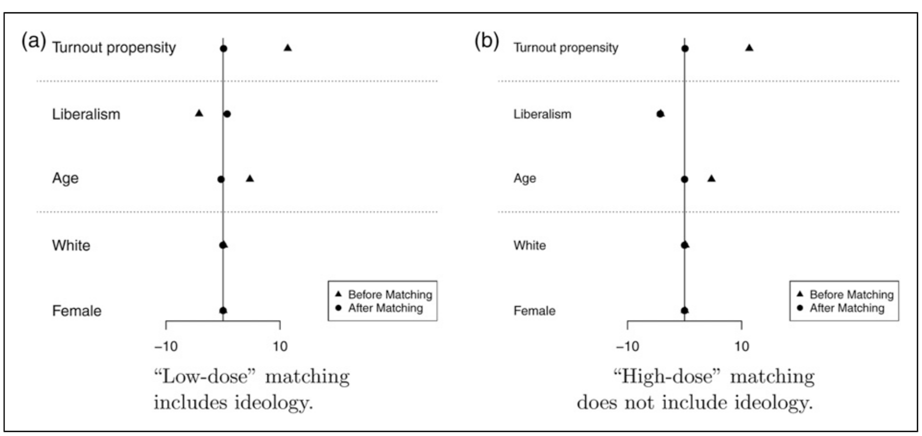

Figure 1. Balance on covariates in low and high-dose designs. *Notes*: Balance improvements for low-dose (left panel) and high-dose (right panel) matches. Mean (Treatment)—Mean (Control) for Turnout Propensity, Liberalism, Age, Race, and Gender, before (triangles) and after (circles) matching.

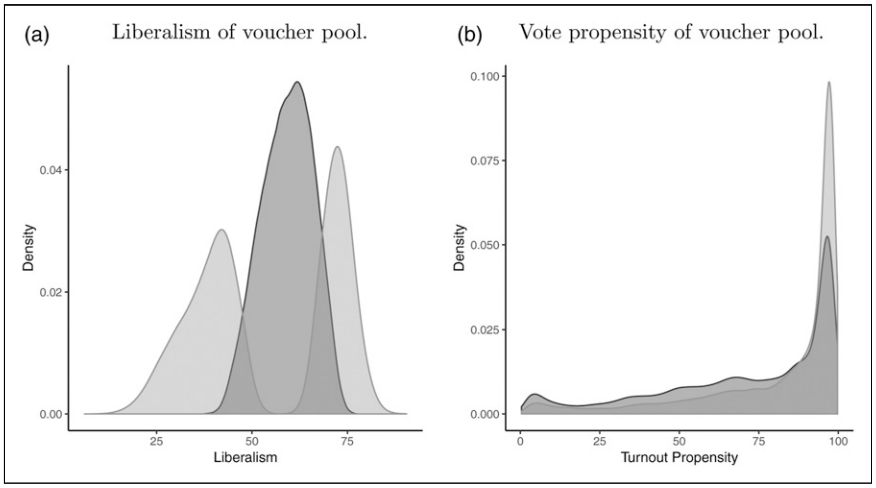

Figure 2. Common support across liberalism and vote propensity. *Notes*: Distribution of (a) liberalism scores and (b) vote propensity across voters who are ideologically typical (dark gray) or atypical (light gray) of their precinct. Areas with overlap show the common support region. In the low-dose sample (*N* = 77,366), we include both ideology and vote propensity as matching variables, in addition to age, gender, and race. In the high-dose sample (*N* = 472,682), we do not include ideology.

who were not cross-pressured in either year and (2) those who moved out of areas where they were cross-pressured in 2017 to those who remained cross-pressured in both years. Those who moved from a precinct where they were not cross-pressured in 2017 to one in which they were cross-pressured in 2019 were about 6.8 percentage points more likely to use vouchers and 12 percentage points more likely to vote in 2019 when compared to voters who moved between 2017 and 2019 but were not cross-pressured in either year (though the estimates are not statistically significant at conventional levels). Those who moved from a precinct where they were cross-pressured in 2017 to one in which they were not in 2019 were about as likely both to use vouchers and vote as those who moved but remained cross-pressured in both years. Results are reported in Appendix H.

Heterogeneous Neighborhood Effects by Ideology

In the previous section we evaluated voucher use among all local political minorities. Here we investigate our hypothesis that the "chilling effect" of neighborhood ideological distance on vouchering (relative to voting) will be greater among conservatives. There are at least two mechanisms by which conservatives might be chilled by a voucher program: priming, that is, exposure to elite-conservative rhetoric, or social opprobrium from Seattle liberals. To test for heterogeneous effects by ideology, we re-run our models with an indicator for whether the voter is to the right of her precinct mean, and an interaction term between this indicator and the "atypical" dummy.[18] The results are presented in Table 4.

We find that conservatives who are ideologically atypical relative to their precincts are around 2.6 percentage points more likely to use their vouchers than conservatives who are locally typical, as shown in Model 3 (typical = 0.079–0.070, or around 1 percentage point; atypical = 0.079–0.070–0.009 + 0.036, or around 3.6 percentage points). Yet when it comes to voting, locally atypical conservatives are around 5.8 percentage points less likely to vote than their locally typical counterparts. In other words, among those who are relatively conservative compared to their neighbors, living in an ideologically dissonant precinct seems to discourage the private form of political activity (voting) but, if anything, encourages the more publicly visible political activity (vouchering). The pattern is reversed among liberals as illustrated in Figure 3. All of the voucher-qualified candidates in Seattle in 2017 were centrist to liberal, with Liberalism scores above 50. It is possible that we might observe neighborhood-based chilling of conservatives' voucher assignments if more conservative candidates qualify to redeem vouchers (see Appendix E1).[19]

Sincerity of Voucher Assignment

Our final hypothesis is that local political minorities are more likely to bend to social cross-pressure and assign their vouchers to socially approved candidates rather than to candidates whom the voter personally prefers. We assume, to a first approximation, that candidates are personally preferred insofar as they are ideologically similar to the voter, and that candidates are socially approved when they are ideologically similar to the mean ideology of voters in the precinct.

Testing our hypothesis about the sincerity of voucher assignments requires an additional identifying assumption—namely, that every observed voucher user is an "always contributor," meaning they would use their

Table 3. Linear Probability Models of Voting and Vouchering by Political Cross-Pressure.

	Voting		Voucher		Participation Gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Low-dose match						
Control mean	0.627	0.627	0.053	0.053	0.574	0.574
(std. dev)	(0.484)	(0.484)	(0.226)	(0.226)	(0.496)	(0.496)
(Intercept)	0.627	−0.699	0.054	−0.247	0.574	−0.451
	[0.622,0.632]	[-0.72,-0.677]	[0.052,0.056]	[-0.259,-0.236]	[0.569,0.578]	[-0.476,-0.427]
Atypical	−0.008	−0.007	0.004	0.002	−0.012	−0.010
(T=2)	[-0.015,-0.001]	[-0.013,-0.002]	[0.001,0.007]	[-0.001,0.005]	[-0.019,-0.005]	[-0.016,-0.003]
Num. obs	77,372	77,372	77,372	77,372	77,372	77,372
High-dose match						
Control mean	0.707	0.707	0.060	0.060	0.648	0.648
(std. dev)	(0.455)	(0.455)	(0.237)	(0.237)	(0.485)	(0.485)
(Intercept)	0.632	−0.783	0.051	−0.169	0.581	−0.614
	[0.629,0.635]	[-0.796,-0.77]	[0.05,0.052]	[-0.176,-0.162]	[0.578,0.584]	[-0.628,-0.6]
Atypical	−0.002	0.009	0.000	0.007	−0.001	0.002
(T = 2)	[-0.006,0.002]	[0.006,0.013]	[-0.002,0.001]	[0.005,0.009]	[-0.006,0.003]	[-0.001,0.006]
Num. obs	205,680	205,680	205,680	205,680	205,680	205,680
With controls		✓		✓		✓

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Number of observations reported is the number of unique individuals in the dataset. Logit estimates are qualitatively similar. Coefficients represent the local average treatment effect on voting and voucher use for individuals who are typical (intercept term) or atypical for their precincts. The top panel reports estimates on units who were matched by ideology, turnout propensity, age, gender, and race. Because of poor common support, the range of ideology among atypical voters is restricted in these "low-dose" models. The bottom panel reports estimates on units who were matched on turnout propensity, age, gender, and race but not ideology, thus including more ideologically extreme Seattleites. Because we pre-registered the low-dose models, we consider the high-dose models to be exploratory.

Table 4. Linear Probability Models of Voting and Vouchering Among Liberals and Conservatives.

	Voting		Voucher		Participation Gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Low-dose match						
(Typical liberal)						
Control mean	0.627	0.627	0.079	0.079	0.547	0.547
(std. dev)	(0.484)	(0.484)	(0.270)	(0.270)	(0.499)	(0.499)
(Intercept)	0.627	−0.610	0.079	−0.376	0.547	−0.234
	[0.621,0.632]	[-0.696,-0.52]	[0.076,0.083]	[-0.419,-0.335]	[0.542,0.553]	[-0.329,-0.142]
Atypical liberal	0.020	0.020	−0.009	−0.023	0.029	0.043
(T = 2)	[0.012,0.028]	[0.012,0.028]	[-0.014,-0.005]	[-0.028,-0.018]	[0.021,0.037]	[0.035,0.051]
Conservative	0.002	0.008	−0.070	0.009	0.072	−0.001
	[-0.008,0.012]	[-0.016,0.033]	[-0.074,-0.067]	[-0.003,0.021]	[0.062,0.082]	[-0.027,0.026]
Atypical ×	−0.078	−0.073	0.036	0.066	−0.115	−0.139
Conservative	[-0.092,-0.064]	[-0.086,-0.059]	[0.031,0.042]	[0.059,0.072]	[-0.128,-0.1]	[-0.153,-0.125]
Num. obs	77,372	77,372	77,372	77,372	77,372	77,372
High-dose match						
Control mean	0.714	0.714	0.063	0.063	0.650	0.650
(std. dev)	(0.452)	(0.452)	(0.243)	(0.243)	(0.485)	(0.485)
(Intercept)	0.64	−0.746	0.054	−0.185	0.586	−0.562
	[0.636,0.643]	[-0.773,-0.719]	[0.052,0.056]	[-0.197,-0.173]	[0.582,0.59]	[-0.59,-0.534]
Atypical liberal	0.024	0.011	0.024	0.002	0.000	0.009
(T = 2)	[0.019,0.029]	[0.004,0.018]	[0.021,0.027]	[-0.002,0.005]	[-0.006,0.005]	[0.001,0.016]
Conservative	−0.017	−0.014	−0.006	−0.001	−0.011	−0.013
	[-0.023,-0.011]	[-0.019,-0.009]	[-0.009,-0.003]	[-0.003,0.002]	[-0.017,-0.005]	[-0.019,-0.007]
Atypical ×	−0.047	−0.005	−0.046	0.011	−0.001	−0.016
Conservative	[-0.055,-0.039]	[-0.019,0.009]	[-0.05,-0.042]	[0.005,0.018]	[-0.009,0.008]	[-0.031,-0.001]
Num. obs	205,680	205,680	205,680	205,680	205,680	205,680
With controls		✓		✓		✓

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Number of observations is the number of observations in the matched dataset. Estimates predict the local average effect of treatment on voting, voucher use, and the participation gap. The intercept term refers to individuals who are typical for their precinct and liberal. Heterogeneous treatment effects are shown among atypical people who are conservative (*Atypical × Right of Precinct*) and liberal (*Atypical*). The top panel includes individuals who were matched by ideology, turnout propensity, age, gender, and race. Because of poor common support, the range of ideology among atypical voters is restricted in these "low-dose" models. The bottom panel includes individuals who were matched on turnout propensity, age, gender, and race, but not ideology. Because we pre-registered the low-dose models, we consider the high-dose models to be exploratory.

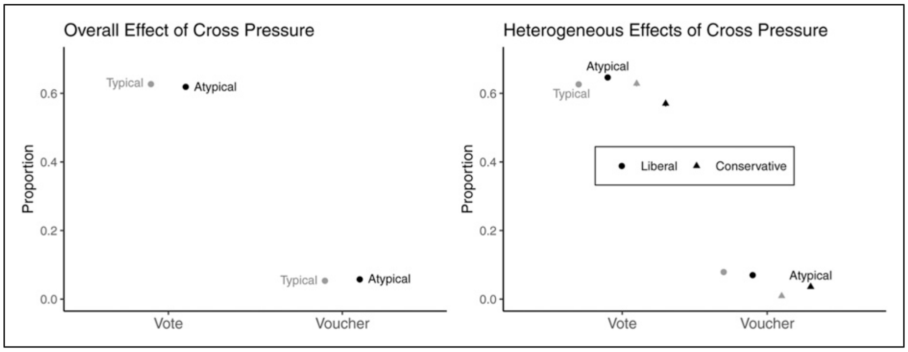

Figure 3. Predicted rate of voting and vouchering, conditional on cross-pressure and ideology. *Notes*: This figure displays the levels that result from combining coefficients in Models 1 and 3 from Tables H1 and 3 (the low-dose, no-controls specifications for voting and vouchering). Levels are generated by summing the coefficients on 1000 bootstrapped estimates as appropriate for each group. Confidence intervals on the levels are plotted but are very small and therefore undetectable. The vote (left) and voucher (right) activity of ideologically typical-of-their-precinct (light gray) and atypical (black) residents is plotted on the left panel, and on the right panel, the levels are split by conservatives (triangles) and liberals (circles).

voucher whether or not they are local political minorities. The estimate must be defined in terms of always-contributors because the relevant counterfactual outcome is undefined for registered voters who would contribute only if they are close to the precinct mean, or only if they are far from the precinct mean. We acknowledge that this assumption is in competition with our first hypothesis, which predicts that individuals may be chilled by neighborhood effects. To the extent that political cross-pressures actually affect local outliers, we are agnostic a priori whether local outliers will respond by using their vouchers insincerely or by dropping out of the voucher market altogether (or some combination). In light of our finding above that neighborhood composition does not chill voucher participation, the assumption that observed vouchers come from always-contributors has some support.

We test the sincerity hypothesis in two steps, first with a dependent variable capturing the dollar-weighted proximity of voucher assignees to the contributor, Y_i^N, and then with a dependent variable capturing the dollar-weighted proximity of voucher assignees to the mean ideology of registered voters in the treatment unit's precinct, Y_i^H. Formally

$$Y_i^N = \sum_{c=1}^{C} p_{ic} \times |L_i - L_c| \tag{4}$$

where p_{ic} represents the proportion of voter i's total voucher contribution assigned to candidate c, L_i represents donor i's ideology, L_c represents candidate c's ideology. Y_i^N increases as the allocation of vouchers departs from sincere vouchering. And

$$Y_i^H = \sum_{c=1}^{C} p_{ic} \times |L_c - \overline{L_h}| \tag{5}$$

where $\overline{L_h}$ is the average precinct ideology of the treatment donor, and the other components of the metric are defined as above. Y_i^H, heterophily, increases as the allocation of vouchers departs from the mean ideology of the treatment unit's precinct.

Note that the possible values for Y_i^N vary from one registered voter to the next, depending on the donor's ideology. Most of the candidates are moderate to liberal,[20] so conservative donors are likely to have larger values for Y_i^N than liberals. Because of this, we perform the sincerity tests only using voters matched on ideology (our "low-dose" design). See balance table in Appendix F.

The sincerity results with the first dependent variable, Y_i^N, suggest that locally ideologically atypical donors may be more likely to assign their vouchers insincerely, that is, to candidates further from their personal ideology, relative to ideologically similar voters who are typical of their precinct. The overall effect is modest and is only distinguishable from zero when we use controls. See Table 5, models 1–2. The average distance between a locally typical donor's ideology and the candidate to which she assigns vouchers is 10.52 points on the 100-point liberalism scale (std. dev = 7.32).

Among conservatives, ideologically typical donors gave to someone who, on average, is around 11 points (10.1 + 0.94) away from them on the ideology scale, and ideologically atypical conservatives gave to someone who is, on average, around 28.7 points away from them (10.1–2.36 + 0.94 + 19.98). These results reflect the fact that conservative voters are on average more distant than liberals from the available donees.[21] The effects are sensitive to specification, and effects among atypical conservatives are muted when we introduce controls.

Our finding that locally atypical donors may assign their vouchers to non-proximate candidates raises the question of whether these donors are signaling homophily with their neighbors. As models 5–6 in Table 5 show, there is no

Table 5. Sincerity Analysis of Voucher Use.

	$DV = Y_i^N$ (Insincere Giving)				$DV = Y_i^H$ (heterophily)			
	Neighborhood Effects		Heterogeneous by Ideology		Neighborhood Effects		Heterogeneous by Ideology	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control mean	10.52	10.52	10.52	10.52	10.52	10.52	10.52	10.52
(std. dev.)	(7.32)	(7.32)	(7.32)	(7.32)	(6.95)	(6.95)	(6.95)	(6.95)
(Intercept)	10.51	44.53	10.1	38.86	10.53	−5.6	11.14	−5.77
	[9.74,11.34]	[39.26,49.65]	[9.1,11.19]	[30.56,47.24]	[9.8,11.35]	[-11.96,0.56]	[10.13,12.25]	[-14.49,2.77]
Atypical	0.18	5.78	−2.36	4.01	5.36	2.87	5.95	2.5
	[-0.78,1.07]	[4.87,6.66]	[-3.47,-1.31]	[1.99,5.88]	[4.46,6.19]	[1.89,3.87]	[4.73,7.11]	[0.62,4.28]
Conservative			0.94	−0.48			−1.40	−0.57
			[-0.84,2.55]	[-2.09,1.07]			[-3.02,0.12]	[-2.35,1.15]
Atypical ×			19.98	0.00			−7.19	−0.07
Conservative			[17.69,22.39]	[-0.05,0.05]			[-9.19,-5.15]	[-0.13,0.00]
With controls		✓		✓		✓		✓
Num. obs	1619	1619	1619	1619	1619	1619	1619	1619

Notes. Ordinary least squares (OLS) models, resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Number of observations is the number of observations in the matched dataset of voucher donors. Estimates predict the local average treatment effect on treated (always-contributors who are atypical for their precinct). The dependent variable in Models 1–4 aims to capture whether donors behave insincerely and is the absolute distance between the ideology of voucher donors and candidate recipients, weighted by the proportion of vouchers assigned to the candidate. The dependent variable in Models 5–8 aims to capture heterophily and is the absolute distance between the mean ideology of the atypical donor's precinct in the matched pair, and the ideology of each donee-candidate, weighted by the proportion of vouchers assigned to each donee by each donor. The sample are units matched on turnout propensity, age, female, race, and ideology (our "low-dose" match). We include an interaction term on conservatives to test whether there are heterogeneous treatment effects by ideology.

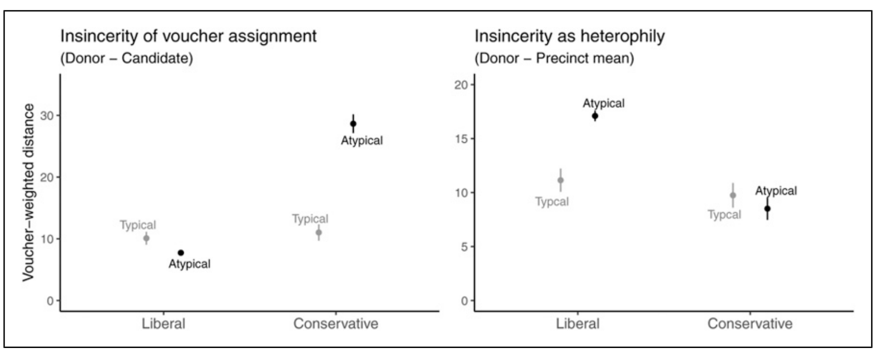

Figure 4. Predicted levels of vouchering by ideology. *Notes*: This figure displays the levels that result from the estimates in Models 1, 3, 5, and 7 in Table 5, the no-controls specifications. Levels are generated from 1000 bootstrapped estimates, with coefficients summed as appropriate for each group. Confidence intervals on the levels are plotted but are very small and therefore undetectable for some levels. The sincerity of liberal (left) and conservative (right) voucher donors who are locally typical (light gray) and atypical (black) residents is plotted on the left panel. The right panel shows heterophily of the same groups.

evidence for the hypothesis that neighborhood pressure induces donors to give toward the treatment precinct's mean. In other words, locally atypical donors are *not* more likely than locally typical donors to give their vouchers to candidates whose ideology is close to the mean ideology of the locally atypical donor's precinct. For control-condition (ideologically typical) donors, the average dollar-weighted distance between the voter's donees and the mean of the corresponding treatment unit's precinct is around 10.52.[22] If treatment (locally atypical) donors felt pressure to conform to the ideology of other voters in their neighborhood, one would expect the coefficient on Atypical to be negative. That is, among pairs of ideologically similar contributors, the donor who is atypical for her precinct would be more likely to shade her giving toward the mean *of that precinct* than the donor who lives among ideological compatriots. Instead, the treatment appears to cause a precisely measured *increase* in dollar-weighted ideological distance between the donor's voucher recipients and the mean of the target (treatment) precinct. As columns 7 and 8 indicate, the overall increase is driven by atypically liberal donors giving to recipients to the donor's ideological left. On average, locally atypical conservative donors giving insincerely may indeed be signaling homophily with their neighbors, with the average atypical conservative voucher donor giving to a candidate whose ideology lies closer to the mean ideology of her precinct than the donor herself.[23]

To put this finding in terms of national politics, imagine a centrist Democratic voter in a conservative precinct, where the mean ideology is somewhere right of center—where a John Kasich-type candidate would do well. Our hypothetical centrist Democrat wants to put up a yard sign. Our sincerity finding implies that she will not put up the sign for her preferred Democratic candidate (e.g., Democratic moderate Amy Klobuchar). Instead, she'll put up a sign for someone like Elizabeth Warren, whose politics are further to the donor's left. Figure 4 illustrates.

Discussion and Conclusion

We hypothesized that socially cross-pressured citizens are more likely to participate in low-cost political activities when they can do so privately (voting) than when the activity is publicly disclosed (voucher assignment). Using data from Seattle's new campaign voucher program we find, contrary to our hypothesis, that ideological cross-pressure from one's neighborhood has a negligible impact on voucher participation overall. There is some evidence, however, of a heterogeneous effect by ideology, with conservatives becoming less likely to vote but more likely to voucher in neighborhoods where they are ideologically atypical, and the opposite occurring among liberals. We also observe a small effect of neighborhood cross-pressure on the degree to which voters assign vouchers to ideologically proximate candidates. Yet rather than shading their voucher assignments in the direction of the neighborhood (precinct) mean, cross-pressured voucher donors actually give to candidates who are slightly *farther* from the mean of their precinct.

These findings, in a local, nonpartisan election, are notable, although one should bear in mind several important caveats. First, ideology is liberal or moderate for most of the voters in our sample, all of the competitive candidates, and all of the precinct means. Our study thus sheds no light on whether conservatives living in liberal neighborhoods would be deterred from giving to conservative candidates, or whether liberals living in a conservative neighborhood would be deterred from giving to liberal candidates.

Second, there may be significant measurement error in the Catalist ideology scores on which we rely, particularly when used as a proxy for ideology in the Seattle issue space. Although preferences in city politics and national politics are correlated (Cann, 2018; Tausanovitch & Warshaw, 2014), it's possible that the Catalist score of one or more of the handful of candidates in our study does not accurately reflect their position on the local ideological spectrum. This possibility makes us particularly cautious about the sincerity of voucher assignment results.

Third, we only examine geographically defined cross-pressure based on where people live. Individuals may face cross-pressure from various other social networks including their families, workplaces, churches, and online communities (Brader et al., 2014). The literature we summarize above shows mixed results for political mobilization and demobilization from other kinds of cross-pressure, especially for state and national elections. Other sources of cross-pressure may affect voucher participation in ways we could not observe.

Fourth, the overall rate of voucher participation in our study was low, though within the program's expected participation rate (Heerwig & McCabe, 2020). We recognize that because Seattle's voucher program was brand new in 2017, it is likely that only the most civically enthusiastic residents had learned about or understood it. Such civic stalwarts may be particularly resistant to ideological pressure from their neighborhoods (or elsewhere).

Finally, in the 2017 election in particular, voting is an imperfect counterfactual for vouchering. On the one hand, both actions could be completed at no cost, and by mail. On the other hand, candidates in the top-of-the-ticket race for mayor were not eligible to receive vouchers in 2017. Because of this, and because many residents of Seattle may have been unfamiliar with the voucher program itself, our participation gap analysis is best taken as a proof-of-concept exercise.

All that said, this study sheds some light on the U.S. Supreme Court's conjecture (shared by many political activists on the conservative side of the spectrum) that mandated disclosure of political activity deters participation among political minorities, and perhaps especially among conservative minorities. We find that precinct-level cross-pressure has no effect on one's willingness to participate in politics in an important, public way: by assigning a campaign finance voucher. If anything, political cross-pressure has a slightly encouraging effect on this public mode of participation. Our findings also suggest that local conservatives in particular are not demobilized from public participation by geographic cross-pressure. These findings are important, because when campaign finance is publicly funded through equally sized vouchers, and the amount of individual vouchers are small, the standard anticorruption rationale for disclosure is quite weak. Our findings, showing no demobilization and no overall shading of voucher assignments toward the mean ideology of the precinct, suggest that campaign finance disclosure requirements may not have the costs normally recited by critics of mandated disclosure.

Our study is only a beginning. Voucher programs, long appreciated by academics and activists, may be catching on. The For the People Act of 2021 (H.R. 1) envisioned a nationwide program of publicly financed campaigns, with voucher programs playing a key role. The research design we employ in this paper can be replicated in any jurisdiction where scholars can gain access to the voter file and information on voucher uptake. The effects that we find in Seattle—a liberal city without great neighborhood political diversity—may or may not replicate in other jurisdictions, or for state and national elections. Nevertheless, future voucher programs are most likely to get their start in liberal cities and states, and careful studies of the Seattle program, made possible by its robust disclosure requirements, should be very useful to reformers across the nation.

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Appendix A

Assignment of Democracy Vouchers is Public Information

This FAQ saying vouchers are public information was attached to 2017 vouchers.

This information sheet was included in the envelope with the 2019 vouchers.

Appendix B

B QQ plots of Catalist's estimates of vote propensity by neighborhood distance

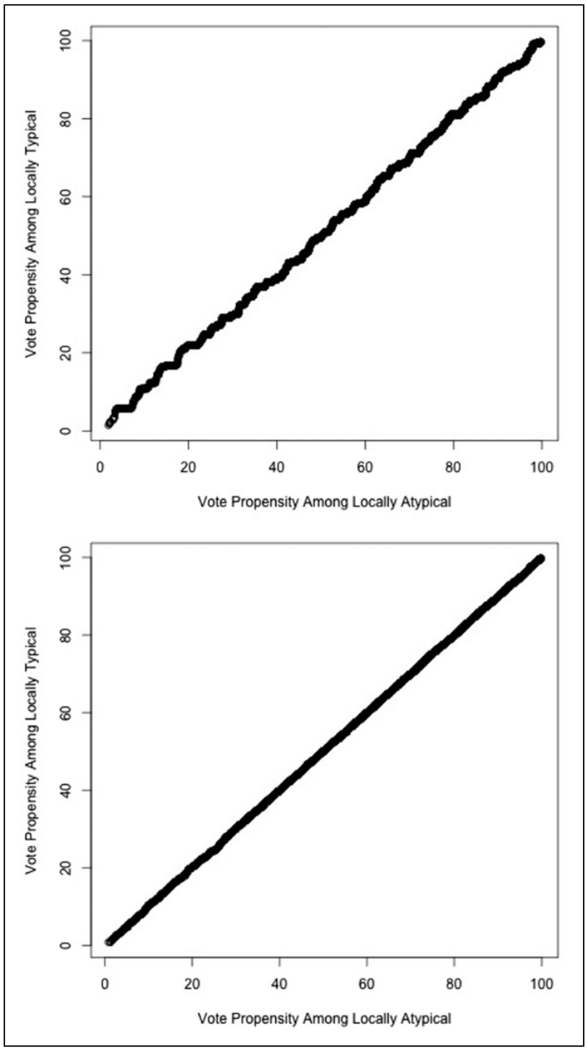

Figure B1. Catalist's estimated vote propensity for registered voters in our small-dose (top) and large-dose (bottom) matches. If voters are less likely to participate when they are atypical and if Catalist's measure adjusts vote propensity estimates based on that characteristic, then our results would be biased toward a null result. The plots reveal that Catalist's measure of vote propensity is no different for atypical and typical voters, reducing concerns about post-treatment bias in our measure.

Appendix C

C Precinct representation after match

Appendix D

D Ideology by precinct versus voter

Appendix E

E Ballot-qualified candidates

Appendix F

F Balance on matched pairs used to analyze sincerity of voucher use

Appendix G

G Robustness checks, using top and bottom third of D_i as cutoffs

Appendix H

H 2019 Replication

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

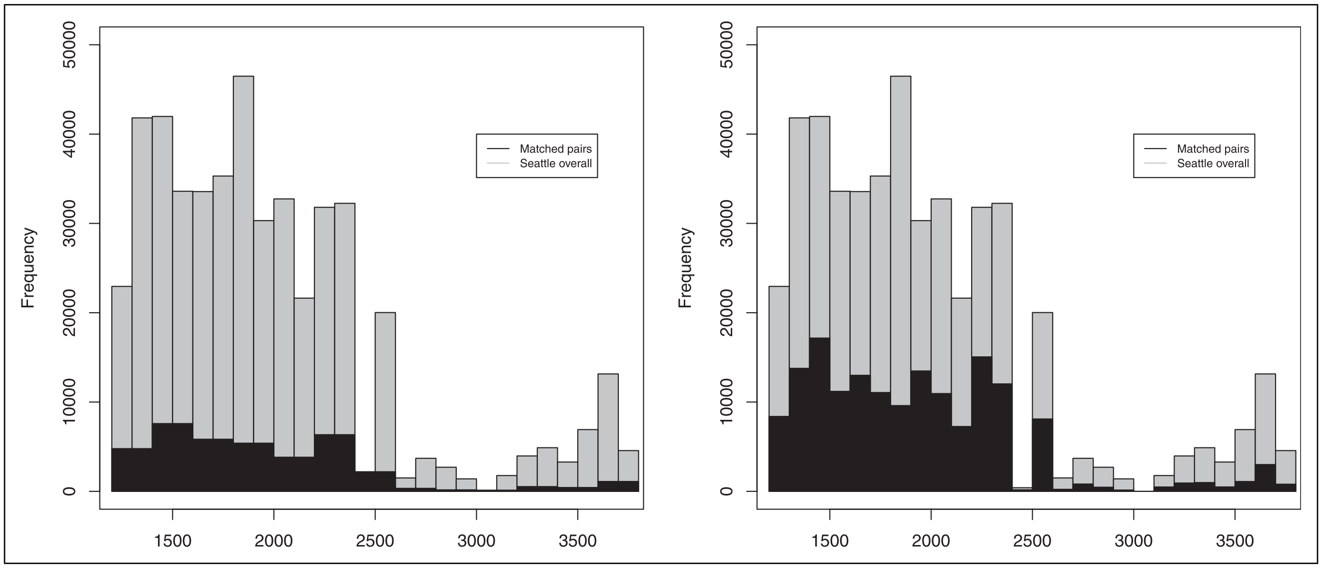

Figure C1 Distribution of precinct data before and after matching in small-dose (left) and high-dose (right) matches. Gray bins show the distribution of registered voters across precincts in Seattle. Black bins show the distribution of registered voters in the matched dataset.

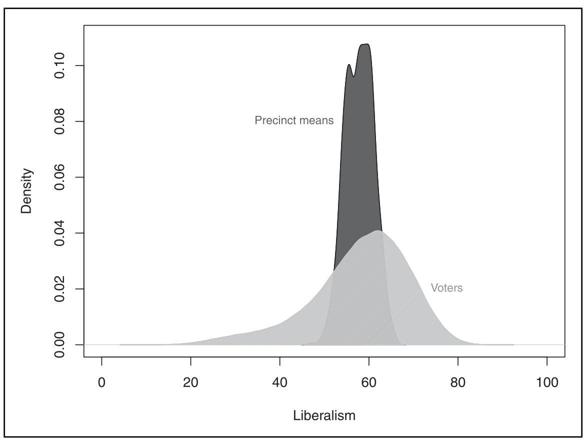

Figure D1. Distribution of ideologies in Seattle. Black density is the distribution of mean ideologies across the 961 precincts in Seattle. Gray density is the distribution of all voter ideologies in Seattle.

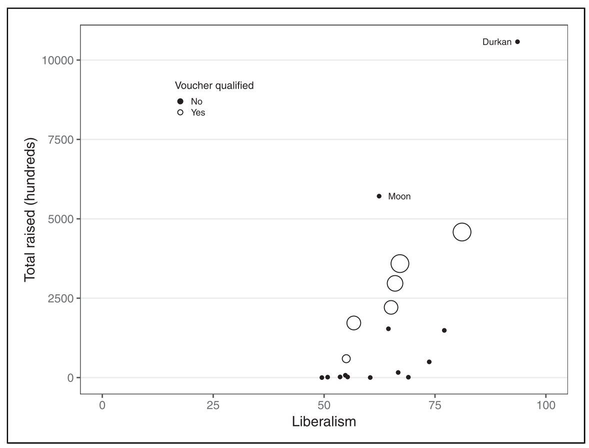

Figure E1. Total funds raised by ideology of ballot-qualified candidates. Candidates for city council who qualified for vouchers are represented by open circles. The size of each circle corresponds to the number of vouchers received. The point labeled "Moon" represents mayoral candidate Cary Moon, who raised $571k but lost. Jenny Durkan raised more than $1 million and won the mayoral race. Mayoral candidates could not participate in the voucher program in 2017. The ideological distribution of voucher-qualified candidates is statistically equivalent to non-qualified candidates.

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Notes

1. McCabe and Heerwig (2018) compare the pool of cash donors in Seattle to the pool of voucher users in 2017. They report that the pool of voucher donors is much larger (4x) than cash donors, and that voucher donors are more likely to come from poorer neighborhoods than cash donors. The composition is otherwise similar: majority female (55%) who are predominately older (>55 yrs old), whiter (88%), more liberal (95%), relative to the population of eligible voters.
2. Voluntary spending limits are a crucial part of public campaign financing programs. The "Honest Elections Seattle" voucher program is currently funded up to $3 million per year (https://

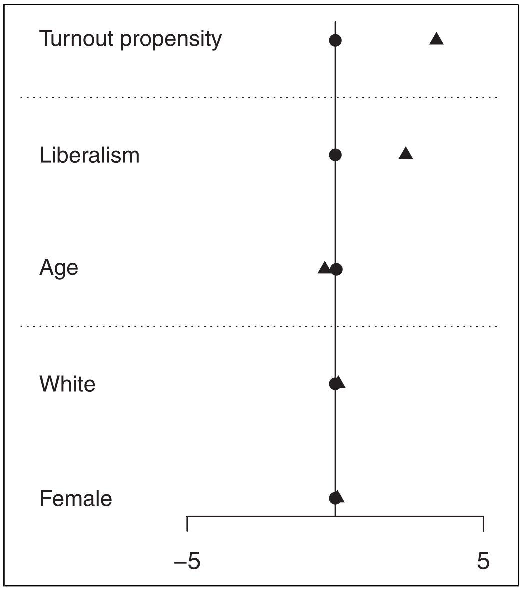

Figure 9. Before (triangles) and after (circles) differences between the mean value in the treatment group and the mean value in the control group (T–C), where match includes turnout propensity, liberalism, age, White, and Female. (Note: for the sincerity analysis, we used only the pre-registered "low-dose" match, for reasons we explain in the main text.)

www.seattle.gov/democracyvoucher/about-the-program). Simulations conducted when the voucher program was under design estimated a participation rate about 5%, roughly what we observe.

3. There are two primary government interests at play in the disclosure jurisprudence: information and corruption. The Supreme Court has long asserted that disclosure of campaign finance information provides helpful information to voters about the policies candidates will support once in office (Levinson, 2015). This government interest should still be furthered in a voucher program given the success that ideal points derived from publicly disclosed campaign finance information have had in predicting floor votes (Bonica, 2018; Elmendorf & Wood, 2018). Nevertheless, the informational interest has long played second fiddle to the anticorruption interest (Shaw, 2016), and is therefore considered more vulnerable (Jiang, 2018). The state interest in preventing corruption or the appearance of corruption seems weaker with respect to voucher programs than with respect to private campaign contributions, since there is no risk that a voucher program without disclosure would make any candidate unduly beholden to any private interest. This is so because the voucher model gives each registered voter the same number (value) of vouchers to contribute. As we learned in expert interviews, the drafters of the Seattle program required

Table G1. Linear Probability Models of Voting and Vouchering by Political Cross-Pressure.

	Voting		Voucher		Participation Gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Low-dose match						
Control mean	0.595	0.595	0.041	0.041	0.554	0.554
(std. dev)	(0.491)	(0.491)	(0.198)	(0.198)	(0.5)	(0.5)
(Intercept)	0.595	−0.779	0.041	−0.183	0.554	−0.597
	[0.622,0.632]	[-0.72,-0.677]	[0.052,0.056]	[-0.259,-0.236]	[0.569,0.578]	[-0.476,-0.427]
Atypical	−0.007	−0.012	0.012	0.011	−0.02	−0.023
(T = 2)	[-0.012,-0.002]	[-0.016,-0.008]	[0.01,0.014]	[0.009,0.013]	[-0.025,-0.014]	[-0.027,-0.018]
Num. obs	146,532	146,532	146,532	146,532	146,532	146,532
High-dose match						
Control mean	0.615	0.615	0.05	0.05	0.566	0.566
(std. dev)	(0.486)	(0.486)	(0.217)	(0.217)	(0.502)	(0.502)
(Intercept)	0.615	−0.774	0.05	−0.17	0.566	−0.603
	[0.613,0.618]	[-0.785,-0.763]	[0.049,0.051]	[-0.177,-0.164]	[0.563,0.568]	[-0.615,-0.591]
Atypical	−0.001	0.006	0.001	0.005	−0.002	0
(T = 2)	[-0.005,0.002]	[0.003,0.009]	[-0.001,0.003]	[0.004,0.007]	[-0.006,0.001]	[-0.003,0.004]
Num. obs	277,068	277,068	277,068	277,068	277,068	277,068
With controls	✓		✓		✓	

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Here, the treatment and control groups are constructed using the top and bottom third of the data in absolute distance from precinct mean. Logit estimates are qualitatively similar. Coefficients represent the local average treatment effect on voting and voucher use for individuals who are typical (intercept term) or atypical for their precincts. The top panel reports estimates on units who were matched by ideology, turnout propensity, age, gender, and race. Because of poor common support, the range of ideology among atypical voters is restricted in these "low-dose" models. The bottom panel reports estimates on units who were matched on turnout propensity, age, gender, and race but not ideology, thus including more ideologically extreme Seattleites. Because we pre-registered the low-dose models, we consider the high-dose models to be exploratory. Number of observations is the number of observations in the matched dataset.

Table G2. Linear Probability Models of Voting and Vouchering Among Liberals and Conservatives.

	Voting		Voucher		Participation Gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Low-dose match (Typical liberal)						
Control mean	0.622	0.622	0.054	0.054	0.568	0.568
(std. dev)	(0.485)	(0.485)	(0.226)	(0.226)	(0.498)	(0.498)
(Intercept)	0.622	−0.746	0.054	−0.224	0.568	−0.522
	[0.618,0.627]	[-0.796,-0.697]	[0.052,0.056]	[-0.25,-0.2]	[0.564,0.573]	[-0.576,-0.465]
Atypical liberal	0.006	−0.002	0.012	0.007	−0.006	−0.009
(T = 2)	[-0.001,0.012]	[-0.008,0.003]	[0.009,0.015]	[0.004,0.01]	[-0.013,0]	[-0.015,-0.003]
Conservative	−0.071	0.001	−0.035	0.008	−0.036	−0.007
	[-0.079,-0.064]	[-0.014,0.016]	[-0.038,-0.032]	[0.001,0.016]	[-0.044,-0.029]	[-0.024,0.008]
Atypical ×	−0.036	−0.025	0.001	0.01	−0.036	−0.035
Conservative	[-0.046,-0.025]	[-0.034,-0.016]	[-0.003,0.005]	[0.006,0.014]	[-0.047,-0.026]	[-0.044,-0.025]
Num. obs	146,532	146,532	146,532	146,532	146,532	146,532
High-dose match						
Control mean	0.626	0.626	0.054	0.054	0.572	0.572
(std. dev)	(0.484)	(0.484)	(0.225)	(0.225)	(0.502)	(0.502)
(Intercept)	0.626	−0.737	0.054	−0.193	0.572	−0.544
	[0.623,0.63]	[-0.758,-0.715]	[0.052,0.055]	[-0.203,-0.182]	[0.569,0.576]	[-0.568,-0.522]
Atypical liberal	0.019	0.005	0.020	−0.001	0.000	0.006
(T = 2)	[0.014,0.024]	[0,0.01]	[0.017,0.022]	[-0.004,0.002]	[-0.006,0.005]	[0,0.011]
Conservative	−0.024	−0.017	−0.009	−0.001	−0.015	−0.016
	[-0.029,-0.019]	[-0.021,-0.012]	[-0.011,-0.006]	[-0.003,0.002]	[-0.021,-0.010]	[-0.021,-0.011]
Atypical ×	−0.040	0.001	−0.038	0.015	−0.002	−0.014
Conservative	[-0.047,-0.033]	[-0.01,0.011]	[-0.041,-0.035]	[0.01,0.02]	[-0.01,0.005]	[-0.025,-0.003]
Num. obs	277,068	277,068	277,068	277,068	277,068	277,068
With controls		✓	✓			✓

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Here, the treatment and control groups are constructed using the top and bottom third of the data in absolute distance from precinct mean. Estimates predict the local average effect of treatment on voting, voucher use, and the participation gap. The intercept term refers to individuals who are typical for their precinct and liberal. Heterogeneous treatment effects are shown among atypical people who are conservative (*Atypical × Right of Precinct*) and liberal (*Atypical*). The top panel includes individuals who were matched by ideology, turnout propensity, age, gender, and race. Because of poor common support, the range of ideology among atypical voters is restricted in these "low-dose" models. The bottom panel includes individuals who were matched on turnout propensity, age, gender, and race but not ideology. Because we pre-registered the low-dose models, we consider the high-dose models to be exploratory. Number of observations is the number of observations in the matched dataset.

Table G3. Linear Probability Models of Vouchering by Ideology of Donor and Candidate.

	$DV = Y_i^N$ (Insincere Giving)				$DV = Y_i^H$ (heterophily)			
	Neighborhood Effects		Heterogeneous by Ideology		Neighborhood Effects		Heterogeneous by Ideology	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control mean	8.30	8.30	8.30	8.30	10.07	10.07	10.07	10.07
(std. dev.)	(6.22)	(6.22)	(6.22)	(6.22)	(5.78)	(5.78)	(5.78)	(5.78
(Intercept)	8.29	45.33	7.27	32.91	10.06	−3.89	10.10	−9.51
	[7.82,8.73]	[40.86,49.98]	[6.84,7.76]	[26.31,39.48]	[9.62,10.5]	[-9.62,2.07]	[9.64,10.56]	[-17.34,-2.06]
Atypical	2.06	6.38	0.31	4.34	4.74	3.77	5.72	3.32
	[1.41,2.73]	[5.73,7.05]	[-0.21,0.78]	[3.08,5.61]	[4.14,5.32]	[3.05,4.48]	[5.08,6.33]	[2.09,4.61]
Conservative			4.30	2.37			−0.14	2.28
			[2.95,5.63]	[1.07,3.68]			[-1.30,1.04]	[0.92,3.64]
Atypical ×			15.15	−0.01			−6.97	−0.06
Conservative			[13.12,17.08]	[-0.05,0.03]			[-8.60,-5.37]	[-0.11,0.00]
With controls		✓		✓	✓			✓
Num. obs	2318	2318	2318	2318	2318	2318	2318	2318

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Number of observations is the number of observations in the matched dataset. Estimates predict the local average treatment effect on treated (always-contributors who are atypical for their precinct). The dependent variable in Models 1–4 aims to capture whether donors behave insincerely and is the absolute distance between the ideology of voucher donors and candidate recipients, weighted by the proportion of vouchers assigned to the candidate. The dependent variable in Models 5–8 aims to capture heterophily and is the absolute distance between the mean ideology of the atypical donor's precinct in the matched pair, and the ideology of each donee-candidate, weighted by the proportion of vouchers assigned to each donee by each donor. The sample are units matched on turnout propensity, age, female, race, and ideology (our "low-dose" match). We include an interaction term on conservatives to test whether there are heterogeneous treatment effects by ideology.

Table H1. Linear Probability Models of Voting and Vouchering by Political Cross-Pressure (2019).

	Voting		Voucher		Participation Gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Low-dose match						
Control mean	0.664	0.664	0.100	0.100	0.565	0.565
(std. dev)	(0.472)	(0.472)	(0.300)	(0.300)	(0.505)	(0.505)
(Intercept)	0.664	−0.404	0.100	−0.247	0.565	−/0.152
	[0.659, 669]	[-0.431, −0.376]	[0.092, 0.098]	[-0.270, −0.235]	[0.560, 0.571]	[-0.183, −0.121]
Atypical	0.033	0.023	0.019	0.014	0.014	0.009
(T=2)	[0.026, 0.041]	[0.016, 0.029]	[0.017,0.026]	[0.009, 0.019]	[0.006, 0.022]	[0.002, 0.017]
Num. obs	64,018	64,018	64,018	64,018	64,018	64,018
With controls		✓		✓		✓

Notes. Linear probability models estimated by ordinary least squares (OLS), resampled (1000 times) to generate 95% confidence intervals (in brackets) around the mean estimate from resampling. Number of observations reported is the number of unique individuals in the dataset. Coefficients represent the local average treatment effect on voting and voucher use for individuals who are typical (intercept term) or atypical for their precincts. Models report estimates on units who were matched by ideology, turnout propensity, age, gender, and race. Because of poor common support, the range of ideology among atypical voters is restricted in these models.

Table H2 Difference-in-Difference Models of Voting and Vouchering by Political Cross-Pressure

	Dependent variable		
	Voting (1)	Voucher (2)	Participation Gap (3)
Treatment Onset			
Atypical × 2019	0.115	0.068	0.047
	(0.101)	(0.086)	(0.063)
95% CI	[-0.083, 0.313]	[-0.101, 0.237]	[-0.076, 0.170]
Observations	17,514	17,514	17,514
Treatment removal			
Typical × 2019	0.010	0.004	0.006
	(0.013)	(0.008)	(0.016)
95% CI	[-0.015, 0.035]	[-0.012, 0.020]	[-0.025, 0.037]
Observations	12,130	12,130	12,130

Notes. Difference-in-difference models estimated by ordinary least squares (OLS) using two-way fixed effects for individuals and time periods (2017 and 2019). Number of observations reported is twice the number of unique individuals in the dataset as each individual is observed in 2 years. Models only use data on individuals who moved precincts between 2017 and 2019. Top panel reports the difference-in-difference estimate among individuals who were treated in neither year and those that were only treated in 2019. Bottom panel reports the difference-in-difference estimate among individuals who were treated in both years and those who were only treated in 2017. Standard errors in all models are clustered by individual.

disclosure to prevent, deter, and uncover any voucher-related fraud.

4. But note that Leighley (1990) found that respondents who reported feeling pressure from a friend to vote for a candidate of the opposite party were *more* likely to turn out to vote than those who did not.
5. We lack the data to track other potential mechanisms, such as persuasion within networks.
6. Studies relying on self-reported political distance may exaggerate the effects of cross-pressure on disclosable contributions, as individuals who are socially anxious may overestimate their ideological distance from their network and also be reluctant to contribute when reminded of disclosure (independent of distance).
7. In addition to these studies on political cross-pressure and the disclosure of campaign contributions, several studies have examined the effects of publicizing campaign contribution tax credits; an alternative to campaign vouchers where donors are reimbursed for a portion of their contribution. Tax credit programs are administered in some states like Ohio and Minnesota and in some localities like New York City. On balance, the research suggests that providing voters with information about tax credits for political contributions increases usage (Boatright & Malbin, 2005; Boatright et al., 2006; Ramsden & Donnay, 2001, though see Schwam-Baird et al., 2016).
8. Ms. Mosqueda's policy positions and candidate statements are available at: https://www.teamteresa.org/about/ and https://ballotpedia.org/Teresa_Mosqueda.
9. A summary of Mr. Grant's campaign themes is available at: https://ballotpedia.org/Jon_Grant.
10. A summary of Mr. Goueli's campaign themes is available at: https://ballotpedia.org/Hisam_Goueli.
11. Residents who became newly registered to vote on or before October 1, 2017 also received four $25 vouchers. Residents of voting age who were citizens or lawful permanent residents but not registered to vote were eligible to apply for vouchers, but did not receive them automatically.
12. The city council and city attorney races were at-large. Also on the ballot were candidates for mayor, King County executive, several positions on the school board and Port of Seattle, and a sales tax measure. http://www2.seattle.gov/ethics/elpub/el_home.asp. Except for the school-board primary, these elections were at-large, presenting voters with the same choices in all precincts. An incumbent running unopposed for the state legislature was also on the ballot in some, but not all, precincts.
13. Catalist's ideology model aggregates a series of linear regressions based on more than 2.7 million unique survey responses to almost 200 individual survey questions ranging from gun control and same-sex marriage, to feminism and Right to Work legislation. Catalist then imputes an ideology score for every individual in their sample based on shared observables with survey respondents. Ansolabehere et al. (2008) have shown that scaled ideology measures are more stable as the number of survey items increases, though we note that Catalist does not disclose its full list of survey questions and all of its predictive models are proprietary. Rhodes and Schaffner (2017) have independently validated Catalist's ideology measure, reporting 0.91 and 0.92 correlations between the Catalist-generated ideology score and ideology measures generated from legislative roll call votes and general population survey questions, respectively.
14. Moreover, even if voters in the treatment group are more civically resolute than voters in the control group, we would still expect the neighborhood-ideology treatment to affect vouchering (public) more than voting (private) behavior, in keeping with our participation gap hypothesis.
15. We use the Matching package in R (Sekhon, 2011). Our original plan to separate registered voters into 100 quantiles was based on the (wrong) assumption that there would be sufficient overlap between treated and control units in each quantile to reliably match (redacted cite to pre-analysis plan).

16. We have very little precinct loss as a result of matching, as can be seen in Appendix C1.
17. Because matching generates good balance between our treatment and control units, we greatly reduce our model dependence (Ho et al., 2007). Our choice to use regression is driven in part by the size of our data. We encountered computational limitations such that, even on a designated high-powered computing cluster, we had to match the full dataset piecemeal in order to extract the matched units. It is therefore much more feasible to present regression results on pre-processed data than attempt to repeat matching on the reduced dataset. The tradeoff is that we do not present Abadie-Imbens standard errors. Instead, we measure uncertainty by bootstrapping our confidence intervals, which accounts for clustering among control units that are matched multiple times.
18. In addition to precinct-normalization, we have also analyzed the political behavior of people who are conservative relative to the mean ideology of Seattle and the mean national ideology in the matched data. The results are substantively and statistically similar.
19. It is possible that conservative candidates may abstain from participating in a voucher program for ideological reasons. It is equally plausible that, despite opposing the public financing of elections, conservative candidates will accept vouchers to avoid defeat. We note a similar dynamic with respect to political action committees: while Democrats often decry taking PAC money for ideological reasons, many nonetheless accept PAC money for fear of unilaterally disarming.
20. Among the seventeen candidates running for city council and city attorney, five received almost 95.5% of all vouchers. These candidates's ideologies range from moderate to very liberal with Liberalism scores of 55, 56.6, 66, 67.1, and 81, respectively. Individually, the candidate with the fewest vouchers collected 7391 while the candidate with the most vouchers collected 15,961. The remaining 12 candidates received the remaining 4.5% of the vouchers, with most collecting fewer than 100 vouchers and none collecting more than 1000. Note that 11 of these 12 candidates did not ultimately qualify for the voucher program so they could not redeem the vouchers they collected for cash. The 12th candidate qualified for the program and ran unopposed.
21. Only 192 voucher donors are conservative and atypical, meaning that multiple control units are matched on the same treatment units in the low-dose model.
22. The distribution of the two dependent variables in Table 5 is different, but their means happen to be the same to two decimal points.
23. A reviewer helpfully raised the question of whether these donations might be strategic—that is, driven by electability concerns—rather than the result of neighborhood cross-pressure. We can't rule out that possibility, but it's not obvious why conservatives who live in neighborhoods where they are ideologically atypical would be more strategic in their vouchering than conservatives who live in neighborhoods where they are typical. Perhaps neighborhood-level exposure to ideological difference makes the reality that one's most-preferred candidate may not be electable more vivid, though we would imagine that most conservatives participating in local politics in a liberal city like Seattle would be well aware of this possibility. In any event, "strategic vouchering" is a topic worthy of investigation in future research.
26. We conducted a sensitivity check by redefining D_i using terciles rather than quartiles. See Appendix G.

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