Did Private Election Administration Funding Advantage Democrats in 2020?*

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Abstract

Private donors contributed more than $350 million to local election officials to support the administration of the 2020 election. Supporters argue these grants were neutral and necessary to maintain normal election operations during the pandemic, while critics worry these grants mostly went to Democratic strongholds and tilted election outcomes. These concerns have led twenty-four states to restrict private election grants. How much did these grants shape the 2020 presidential election? To answer this question, we collect administrative data on private election administration grants and election outcomes. We then use new advances in synthetic control methods to compare presidential election results and turnout in counties that received grants to counties with identical average presidential election results and turnout before 2020. While counties that favor Democrats were much more likely to receive a grant, we find that the grants did not have a noticeable effect on the presidential election. Our estimates of the average effect of receiving a grant on Democratic vote share range from 0.02 percentage points to 0.36 percentage points. Our estimates of the average effect of receiving a grant on turnout range from -0.03 percentage points to 0.13 percentage points. Across specifications, our 95% confidence intervals fail to include effects on Democratic vote share larger than 0.58 percentage points and effects on turnout larger than 0.40 percentage points. We characterize the magnitude of our effects by asking how large they are compared to the margin by which Biden won the 2020 election. We find that the effects are not large enough to have swung the 2020 election from Biden to Trump.

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1 Introduction

Private donors contributed more than $350 million to local election officials to support the administration of the presidential election in 2020.[1] This nearly matches the supplemental funding Congress appropriated to support local election administration in 2020[2] and is a substantial share of the $2 to $3 billion spent in a typical national election (Mohr et al. 2018).[3] The private donors and their supporters argue this money was necessary to ensure all eligible citizens had a chance to vote amid the disruptions caused by the COVID-19 pandemic.[4] Many local election officials echo this view with one anonymous official saying they used the funding to "alleviate choke points and barriers to voting."[5] Favoring these arguments, Michigan voters approved an amendment to their state's constitution in 2022 that protects the right of local governments to receive private funding for election administration.

Critics of these donations argue that private donors overwhelmingly favored Democratic-leaning counties and municipalities and that these grants have the potential to tilt elections in favor of one party by increasing the participation of some citizens more than others.[6] In one complaint filed before the Federal Election Commission, the plaintiff points out that a large share of the funds donated in 2020 went to Democratic-leaning parts of the country and alleges that the donors have a "hidden motive to increase Joe Biden's statewide vote."[7] These concerns led twenty-four states to adopt laws banning or limiting private donations to local election officials.[8] How much did private election administration grants tilt the 2020 election?

[1] See https://www.techandciviclife.org/our-work/election-officials/grants/ and https://pollingaccessgrants.org/

[2] https://www.eac.gov/payments-and-grants/CARES

[3] For a review of recent estimates of the cost of elections, see https://electionlab.mit.edu/sites/default/files/2022-05/TheCostofConductingElections-2022.pdf.

[4] https://www.techandciviclife.org/2020covidsupport/

[5] https://www.techandciviclife.org/election-officials-made-democracy-happen-in-2020/

[6] See, e.g., https://thefga.org/briefs/show-me-the-zuckerbucks-outside-money-infiltrated-missouris-2020-ele

[7] https://eqs.fec.gov/eqsdocsMUR/7854_01.pdf

[8] https://www.ncsl.org/elections-and-campaigns/prohibiting-private-funding-of-elections

We address this question by combining county-level administrative data on turnout, presidential voting, election spending, and election administration with records of which county governments received a private donation from the largest private election administration donor in 2020, the Center for Tech and Civic Life (CTCL). We document that counties that support Democrats were more likely to apply for private election funding in 2020, which is consistent with critics' allegations. Since Democratic-leaning counties were much more likely to apply, a simple comparison of turnout and presidential vote shares in counties that did or didn't receive a grant fails to reveal the grant's impact. We mitigate this bias by comparing grant-funded counties to those without funding but with similar pre-2020 turnout and voting trends using recent advancements in synthetic control methods (Arkhangelsky et al. 2021).

We find that, despite the scale of the CTCL grant program in 2020 and the tendency of the money to go to Democratic-leaning counties, private funding did not noticeably advantage Joe Biden in the 2020 presidential election. We estimate that receiving a grant increased support for Democrats by between 0.02 and 0.36 percentage points and increased turnout by less than 0.13 percentage points. We validate our estimates using alternative machine learning and econometric approaches to estimating the effects, and these approaches produce similar estimates. Since large counties and counties in battleground states have a larger effect on the aggregate election outcome, we also estimate the effects in these counties separately. We find that grants had a similar effect in large counties and counties in battleground states. We also present evidence that the small average effects are not masking large effects for the small number of counties that received relatively large grants.

To characterize the magnitude of the effects, we compare our estimates to the state-by-state margins in the 2020 presidential election. We conduct a simple analysis in which we remove the average effect on turnout and Democratic vote share from every grant-receiving county's vote total. Despite the razor-thin margins in 2020, we find that the turnout and

Democratic vote share effects are not large enough to have swung the election to Donald Trump.

Beyond the ongoing policy debate about private election funding, this paper contributes to a growing social scientific literature on the effects of local election administration. Democratic and Republican officials often disagree over how much to spend on elections and how they should be funded (Hasen 2012; Mohr et al. 2019). This leads to a conventional wisdom that spending more or less will have substantial effects on the outcomes of elections. Yet, while some changes in state and local election administration can affect participation and alter the composition of the electorate, the turnout effects tend to be modest, and the compositional effects are often hard to predict (Cantoni and Pons 2021; Clinton et al. 2020; Gerber, Huber, and Hill 2013; Gronke et al. 2008; Kaplan and Yuan 2020; Tomkins et al. 2023; Thompson et al. 2020; Yoder et al. 2021). Further, despite large differences between the Democratic and Republican positions on how much money to spend on elections, Democratic local officials do not produce more turnout or higher Democratic vote shares than do Republican local officials (Ferrer, Geyn, and Thompson 2023). This paper advances this literature by evaluating a large increase in resources rather than a single policy change. If local election officials are motivated to increase participation and are well informed about what administrative changes will be most effective, providing them resources should have a larger effect on turnout than any individual policy change. Our findings suggest that there may be less room for increasing turnout with local administrative changes than previously expected.

This paper also contributes to the large literature on the role of money in American politics. A vast literature studies the effects of campaign finance and the influence of special interests (see, e.g., Ansolabehere, De Figueiredo, and Snyder Jr 2003; Levitt 1994). Many reforms have sought to limit the influence of money in politics by change who can spend how much money on which races (see, e.g., Fouirnaies and Fowler 2022; Hall 2016; Kilborn and Vishwanath 2022; Yorgason 2021). While many public officials and members of the public are concerned that donations to support local election administration offer a new, previously untapped way to influence election results without giving directly to candidates, our results suggest this may not be a substantial risk.

When considering the implications of our results, it is important to note that our paper reports estimates of the average effect of additional election funds at the current margin. If, for example, election officials had much smaller budgets than in 2020, grants may help to maintain the most basic election functions and thereby have substantial effects on participation and election outcomes. More resources may also help local governments maintain better security measures or make voting more convenient. While we cannot measure this directly, many of the recipients said the grants helped them make their local election more secure.[9]

2 Private Election Administration Grants in 2020

2.1 Center for Tech and Civic Life's 2020 Grant Program

In fall 2020, Mark Zuckerberg and Priscilla Chan donated approximately $350 million to the Center for Tech and Civic Life (CTCL), a Chicago-based nonprofit, to administer a grant program for local election administration. The CTCL invited all local governments responsible for administering elections to apply for funding and gave the funding to every eligible government that applied.[10] The grants were intended to offset election administration expenses incurred from June 2020 until December 2020. The CTCL determined the maximum amount of each grant based on the eligible voting population of each jurisdiction as well as other demographics. According to our calculations, CTCL gave the median grant-receiving county approximately $0.81 per voting-age resident. The typical local government spends

[9]For example, one election official told the CTCL: "We are a small community struggling to find ways to handle unfunded mandates, especially during a pandemic. It means a lot to us to ensure our election process is done in all the right ways." https://www.techandciviclife.org/grant-update-october/

[10]Unless cited to a different source, we rely on the CTCL's website for details about the grants. https://www.techandciviclife.org/our-work/election-officials/grants/

approximately \$8 per eligible citizen on elections (Mohr et al. 2018), making this a roughly 10% increase in the typical recipient's election funding in a normal year.

According to reports submitted to the CTCL, election officials intended to use the grants in a variety of ways to make election day run smoother, offer alternative ways to vote, and reduce COVID transmission.[11] Officials reported using the money to hire poll workers and other temporary staff, purchase mail balloting equipment and supplies, obtain protective equipment such as masks, and purchase other standard election equipment. One election administrator told the CTLC that "this unprecedented voter participation simply would have crippled the administration of our elections with devastating effects if we were left with the limited available municipal funds."[12]

2.2 Grant and Election Data

We draw on grant and election data to study the effect of private election administration grants in the 2020 election. We build our dataset of grant recipients using the CTCL's 2020 tax filing which contains a list of every grant made under this program. We digitized this tax document and extracted all of the recipient names and grant amounts. We also compiled and cleaned county-level presidential election results from 1992 to 2020. These results were collected from secretaries of state and reported in Dave Leip's Atlas of US Presidential Elections.[13]

The official responsible for running elections varies across states and even, occasionally, within states (Kimball and Kropf 2006). In ten mostly New England and Midwestern states, election administration is largely handled by municipal governments.[14] In these states, we cannot distinguish between counties that did and did not receive grants because

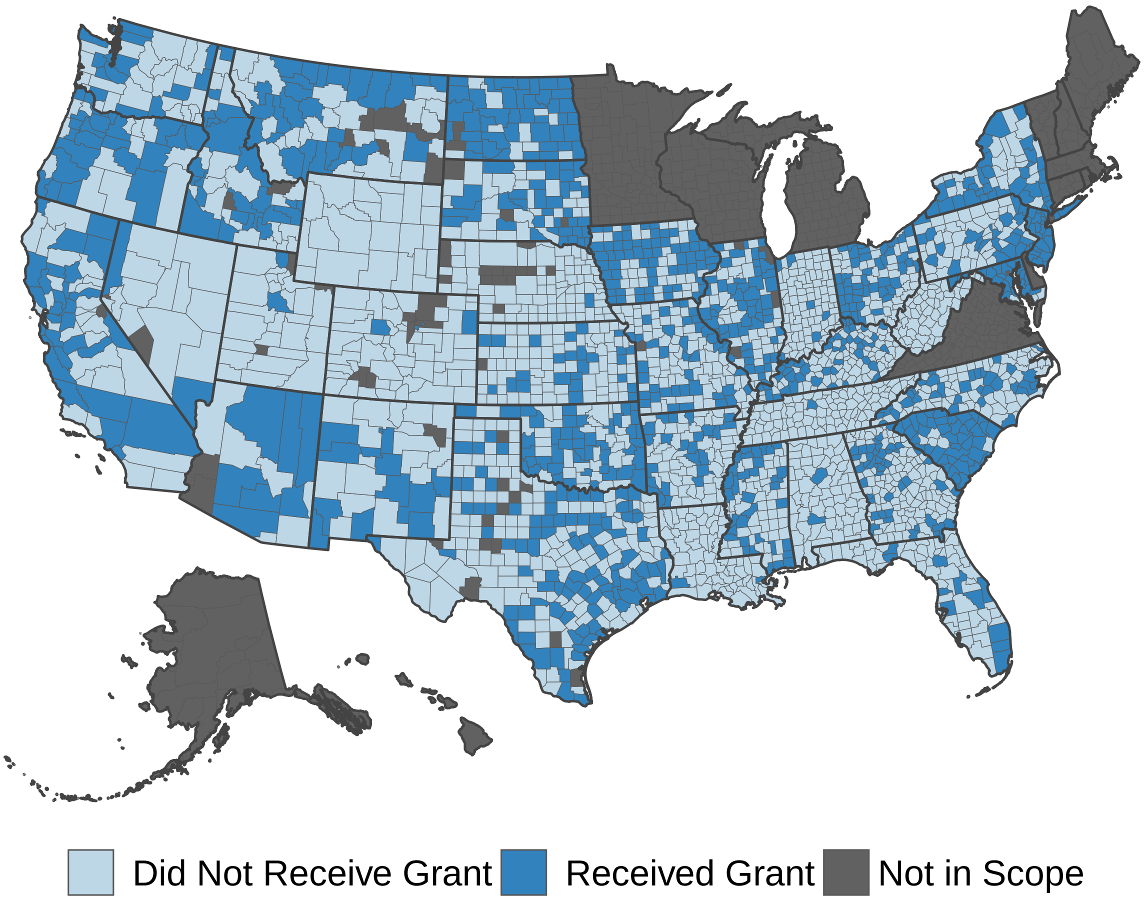
[11] https://www.techandciviclife.org/grant-update-november/

[12] https://www.techandciviclife.org/election-officials-made-democracy-happen-in-2020/

[13] Alaska's secretary of state reports election results at the election district level rather than the borough level, which is the equivalent of counties in Alaska and the level at which CTCL made grants in Alaska. We exclude Alaska from our data.

[14] These states are Connecticut, Maine, Massachusetts, Michigan, Minnesota, New Hampshire, Rhode Island, Virginia, Vermont, and Wisconsin.

Figure 1: **Geographic Distribution of Grants**



there are often many municipalities within a given county. To avoid incorrectly labeling these counties as receiving a grant when only a small portion of the county received a grant, we withhold these states from our analysis. We also withhold five counties with municipal election administrators in states where elections are typically run by county officials.[15] Finally, we exclude 59 counties that either have fewer than 1,000 residents or have changing borders during our analysis period given the challenges associated with estimating turnout when the population estimate in the denominator of turnout will be noisy.

[15]We exclude Cook, St. Clair, Vermilion, and Winnebago counties in Illinois and Jackson County in Missouri.

2.3 Reasoning About the Effects of Election Administration Grants

Should we expect grants like this to affect turnout and election results? One way to reason about this is to consider each of the changes in election administration that the grant money facilitates and evaluate who the change targets, how much it will increase participation in the targeted group, and how different the targeted and non-targeted groups are in terms of expected partisan voting.[16]

Consider a grant-receiving county that spends the money on additional poll workers—more counties mentioned spending grant money on temporary staff or poll workers than on any other spending category.[17] Staff may make the process of registering or requesting an absentee ballot easier, activities that are necessary for well-run elections but are unlikely to have substantial effects on participation. They may also help keep lines shorter. Suppose the additional staff reduce the average wait time by 10 minutes, a quite large effect. Pettigrew (2021) finds that wait times decrease future turnout by approximately 1 percentage point for every hour a person waits. If this effect is linear and applies to people deciding whether to stay or leave based on line length, not just in future years, then reducing wait times by 10 minutes for the average voter increases participation by 0.17 percentage points. Imagine there are two polling places in a county with equal numbers of voters using each in a typical year. The local election official sends the new staff to only one location. Suppose further, as an extreme example, that 75% of voters in the precinct that received the extra staff typically vote for Democrats while only 25% of voters in the other precinct vote for Democrats. In this extreme example, overall turnout increases by 0.085 percentage points and Democratic vote share increases by an even smaller 0.021 percentage points. Similar exercises using a similar approach for other common interventions—like adding polling places (Clinton et al. 2020; Tomkins et al. 2023; Yoder 2018), adding ballot drop boxes, and expanding early voting hours (Gronke et al. 2008; Kaplan and Yuan 2020)—lead to the same conclusions. A

[16] This section draws heavily from Grimmer and Hersh (2023) and private conversation with the authors.
[17] `https://www.techandciviclife.org/grant-update-november/`

large share of counties also reported spending money in ways that should have even smaller effects on participation and partisan balance like purchasing personal protective equipment for poll workers or purchasing new election equipment.

Even after combining many small effects, this exercise leads us to expect that the effect of this grant money on turnout and Democratic vote share is quite small if there is any detectable effect at all.

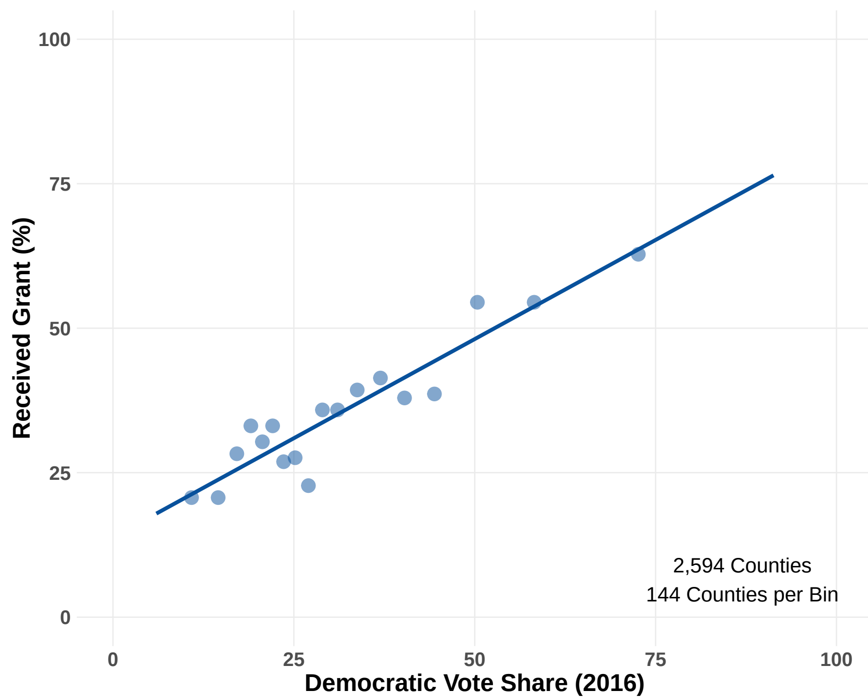
3 Democratic-Supporting Counties Were More Likely to Receive Grants

One of the key concerns among critics of the private election administration grants is that Democratic-leaning counties were more likely to receive them. If Democratic counties are more likely to receive the money, and the money leads to higher turnout, the money will increase the statewide Democratic vote share when the state adds up the county totals.

In Figure 2 we document how much more likely Democratic-leaning counties were to receive money. We use Democratic vote share in the 2016 presidential election as a measure of partisan lean. We find that the CTCL gave grants to about 30% of counties where Donald Trump received approximately 75% of the two-party presidential vote in 2016. Meanwhile, the CTCL gave to 65% of counties where Hilary Clinton received 75% of the two-party vote in 2016.

Why were Democratic-leaning counties more likely to receive grants? As we discuss above, CTCL reports giving every eligible applicant a grant, and this is consistent with everything we have read about this program. This means that any differences we see between grant recipients and non-recipients is a result of which counties applied. Might county officials have applied because they had more resources and staff, because they expected more interest in the election, or because the official tasked with applying was motivated to do

Figure 2: **Democratic-Leaning Counties More Likely to Receive Election Assistance Grants.** Each dot represents the average of 144 counties binned based on two-party Democratic presidential vote share from 2016. The regression line is fit to the underlying county-level data.



so? We find that, while some of these explanations can partly account for why Democratic-leaning counties receive more grants, they do not fully explain this pattern.

In Table A.1 in the online appendix, we document that larger counties were more likely to receive a grant which may in part reflect the capacity to apply. Similarly, we find that counties with more election-related spending prior to 2020 were more likely to receive a grant, providing further evidence that capacity helps explain which counties applied. We also find that counties in states with more Democratic-leaning counties were more likely to receive these grants, while counties in the most competitive states were not substantially more likely to receive a grant. While these other factors partially account for the tendency of Democratic-leaning counties to receive a grant, we generally still find that counties that more strongly favored Clinton in 2016 were more likely to receive a grant even when compared counties with similar populations in the same state. Our regression estimates imply that a 20-percentage-point increase in Clinton vote share in 2016 is associated with a 5-percentage-point increase in the likelihood of receiving a grant when compared to other counties with similar populations in the same state. Given the attention to COVID in summer and fall 2020, Democratic-leaning counties may have had a higher death rate and been more concerned about the effects of COVID on election administration leading them to apply for a grant. We evaluate this in Table A.1, and we do not find much evidence for this explanation. Finally, in Table A.1, we also present evidence, drawing on data from Ferrer, Geyn, and Thompson (2023), that counties led by Democratic local officials may be more likely to receive these grants and that this may partially explain the relationship between grant receipt and support for Democratic candidates.

4 Grants Did Not Substantially Increase Turnout or Democratic Vote Share

In this section, we detail our finding that the private election administration grants did not substantially increase turnout or Democratic vote share, and we explain how we estimate the effects of the grants. This section has five parts: First, we describe our estimation strategy and why it is appropriate for this setting. Second, we present graphical evidence that grants did not substantially increase turnout or Democratic vote share. Third, we report estimates of the effect of the grants on turnout and Democratic vote share. Fourth, we document our independent analysis of grants to municipalities in Wisconsin producing similar results as our main estimates. Fifth, we discuss alternative estimation strategies and document how all of these strategies yield similar results. Sixth, we present evidence that effects are similar in more and less competitive states and more and less populous counties. Finally, we document that the effects are less positive in counties that received larger grants, suggesting that funding is not substantially affecting turnout or the composition of the electorate at the current margin.

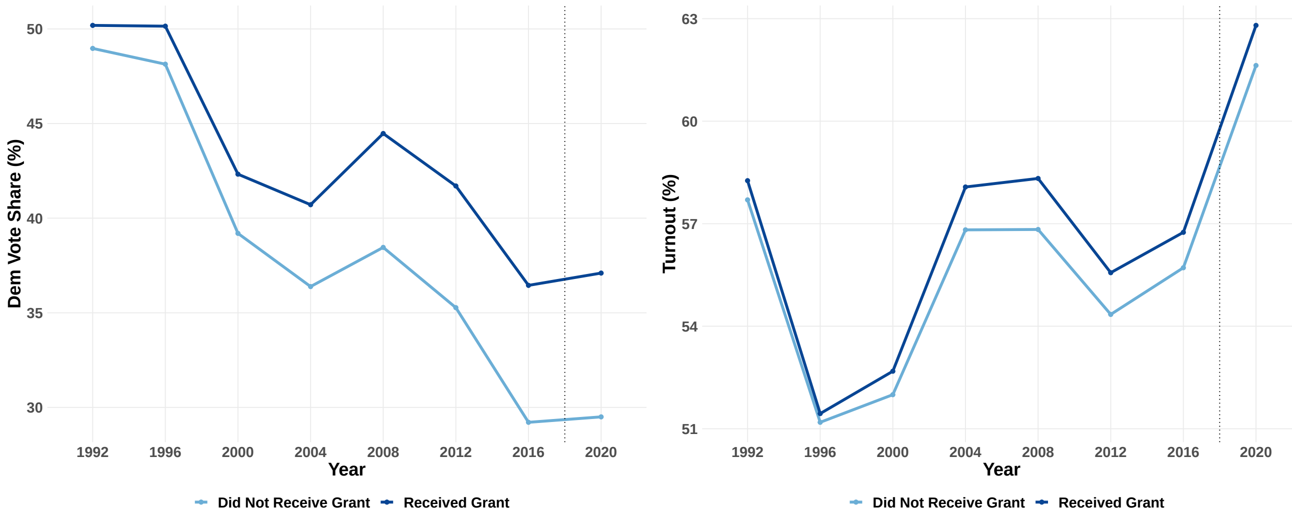
4.1 Estimating the Effect of Grants in the 2020 Election

Our goal in this section is to estimate the average effect CTCL grants had on turnout and Democratic vote share. As we document in Figure 2, grant-receiving counties favored Democrats in 2016, four years before the grants were made. Given the tendency of counties to continue voting for the same party from one election to the next, we would expect grant-receiving counties to favor Democrats in the 2020 election more than counties that did not receive grants even if the grants had no effect on Democratic vote share or turnout. This type of selection can be accounted for using a difference-in-differences strategy, where we compare the difference between 2020 and pre-2020 turnout and vote share for Democrats in treated counties with the analogous difference in untreated counties. This approach would yield valid causal estimates under the assumption that turnout and Democratic vote share would have increased by the same amount in 2020 in treatment and control counties in the absence of the grants. Since we have measures of county-level turnout and Democratic vote share for many elections prior to 2020, we evaluate the plausibility of this assumption in Figure 3. We find that Democratic vote share is decreasing slower in treated counties than in control counties. We also find that turnout is increasing faster in treated counties. These differences in treated and control trajectories suggest that simple difference-in-differences estimates will dramatically overstate the effect of grants on Democratic vote share and turnout.

To address the shortcomings with the standard difference-in-differences design in our setting, we follow Arkhangelsky et al. (2021) in reweighting our difference-in-differences regressions with weights ensure the treatment and control units are on similar trajectories prior to the treatment and make the pre-treatment period as similar to the post-treatment period as possible.[18] This approach has three steps: 1) compute county weights that make the trend in the control units approximately equal to the trend in the treated units, 2)

[18]This approach, which they call synthetic difference-in-differences, builds on the synthetic control method and other related approaches (Athey et al. 2021; Abadie, Diamond, and Hainmueller 2010; Ben-Michael, Feller, and Rothstein 2021; Xu 2017)

Figure 3: **Democratic Vote Share Was Declining Slower and Turnout Was Increasing Faster in Grant-Receiving Counties Long Before 2020.**



compute election weights that make the pre-treatment period as similar as possible to the post-treatment period among control units, and 3) estimate a reweighted difference-in-differences regression weighting by the product of the county and election weights.

Formally, we compute county weights ω_i that minimize the expression

$$\text{argmin}_{\omega_0 \in \mathbb{R}_+, \omega \in \Omega} \sum_{t=1}^{T_{pre}} (\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it})^2 + \zeta^2 T_{pre} ||\omega||_2^2$$

where Y_{it} is the outcome in county i and election t, T_{pre} is the number of pre-treatment periods, N is the number of control and treated units, N_{co} is the number of control units, Ω is the set containing all ω in which all ω_i fall between between 0 and 1 inclusive and ω sums to one, and ζ is a regularization parameter proposed in Arkhangelsky et al. (2021). Since ω_0 is not regularized, ω_0 represents the average pre-treatment difference between the treated and control units and means that the county weights produce a weighted control mean that follows the same trajectory as the treatment mean but may not be at the same level.

We then compute time weights λ_t that minimize an expression nearly identical to the expression for the unit weights:

$$\text{argmin}_{\lambda_0 \in \mathbb{R}_+, \lambda \in \Lambda} \sum_{i=1}^{N_{co}} (\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t Y_{it} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} Y_{it})^2$$

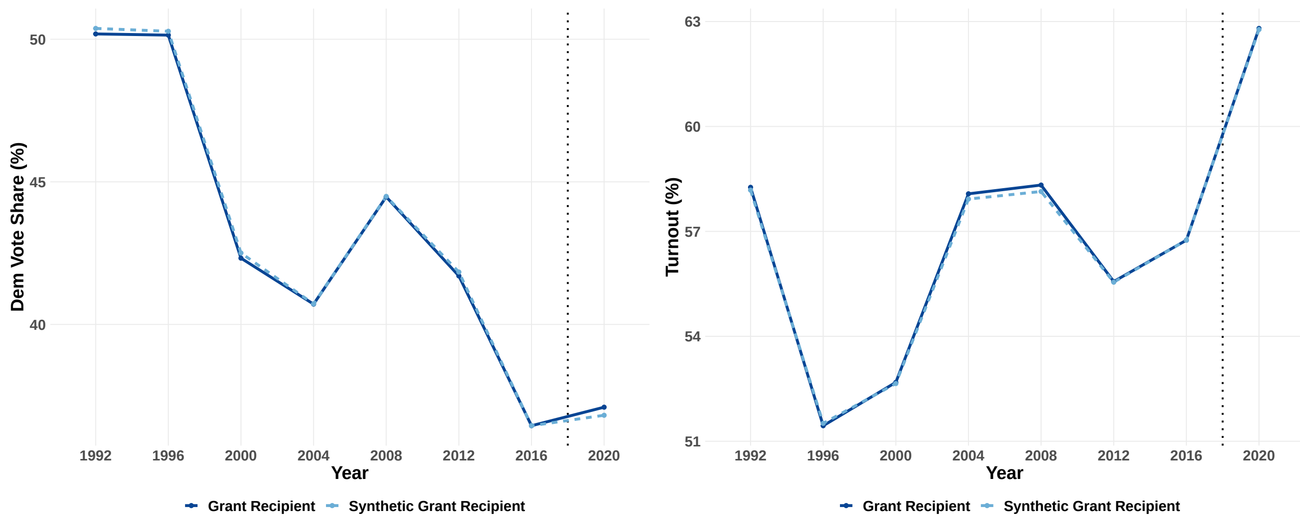
We use the product of these weights as the weights in a weighted difference-in-differences least squares regression

$$\text{argmin}_{\tau,\alpha,\beta} \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t$$

where W_{it} is an indicator for the treatment, α_i is a county fixed effect, β_t is an election fixed effect, and τ is the treatment effect estimate. As with the classical synthetic control method (Abadie, Diamond, and Hainmueller 2010) and generalized synthetic control method (Xu 2017), the weighted regression coefficient $\hat{\tau}$ from the synthetic difference in differences method yields consistent estimates for the average treatment effect on the treated (ATT) under a low-rank approximation for the untreated potential outcome Y^0, which requires that election outcomes in the absence of the grants can be approximated by county intercepts, election year shocks, and low-rank time varying slopes at the county level. This design assumption is strictly weaker than the parallel trends assumption required by difference in difference methods, which we see from Figure 3 is implausible.

To validate the weighted difference-in-differences estimator, we study the 2016 presidential election as a placebo case. We do this by deleting 2020 from our data and pretending that the grants were handed out in 2016. We then rerun the weighted difference-in-difference procedure to produce estimates of the placebo effect. Since the grants were not handed out until 2020, an unbiased estimator will find placebo effects that are close to zero. We present these estimates in Section A.2 in the online appendix. Consistent with the goal of the estimator, we confirm that the weighted difference-in-differences approach fails to estimate a statistically significant placebo effect.

Figure 4: **Trends in Democratic vote share and turnout in treated and synth difference-in-differences counterfactual over time**



(a) Trends in Democratic Vote Share

(b) Trends in Turnout

4.2 Graphical Evidence That Grants Had Minimal Effect on Democratic Vote Share and Turnout

First, we present graphical evidence that CTCL grants did not substantially increase turnout or vote share for Democrats. Figure 4 compares Democratic vote share and turnout for the average grant recipient over time to the counterfactual implied by synthetic difference-in-differences. Across both the Democratic vote share panel on the left and the turnout panel on the right, we see the synthetic difference-in-differences procedure produces a counterfactual that almost perfectly matches the average trajectory for grant recipients. The exceptions to this perfect match are in 1992 where counterfactual Democratic vote share is slightly higher than observed Democratic vote share and 2004 and 2008 where counterfactual turnout is slightly lower than observed turnout. In all three cases, the differences are small with gaps of less than 0.25 percentage points.

Figure 4 also makes clear that any average effect of the grants is so small as to not be visible in the Democratic vote share or turnout plots. If there were a visible effect in either plot, it would appear as a difference between the grant recipient and counterfactual lines

Table 1: **Election Administration Grants Did Not Noticeably Advantage Democrats or Increase Turnout in 2020.**

	Dem Vote Share (%)				Turnout (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grant Recipient in 2020	3.24	0.36	0.10	0.02	0.24	-0.03	0.13	0.03
	(0.34)	(0.11)	(0.12)	(0.11)	(0.21)	(0.15)	(0.14)	(0.14)
Num Grant Recipients	924	924	924	924	924	924	924	924
Num Counties	2,597	2,597	2,597	2,597	2,597	2,597	2,597	2,597
Observations	20,776	20,776	20,776	20,776	20,776	20,776	20,776	20,776
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Weights	No	No	Yes	Yes	No	No	Yes	Yes
Year Weights	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 8 presidential elections from 1992 to 2020.

in 2020. Instead, these lines continue to perfectly overlap in 2020 just like they did prior to 2020, implying that the average grant did not substantially advantage either party or noticeably increase turnout.

4.3 Estimates of Effect of Grants on Democratic Vote Share and Turnout

Next, we present our estimates of the effect of receiving a grant on turnout and Democratic vote share. Table 1 reports our estimates using a variety of estimation approaches. Column 1 is the simple difference-in-differences regression estimate of the effect of grant receipt on Democratic vote share. As we establish in Section 4.1, this is a dramatic overestimate of the effect because, even before 2020, Democrats were increasingly performing better in counties that received a grant than in counties that did not. In column 2, we present estimates from difference-in-differences regressions including time weights but not including county weights. The 2020 election result is much more similar to the 2016 result than any other previous period in our data, so the weight selection procedure places all of the mass on 2016 in the pre-treatment period. This means that column 2 is equivalent to a two-group, two-period difference-in-differences design using only 2016 and 2020. With this estimator, we find that grants increase Democratic vote share by 0.36 percentage points. In column 3, we present our estimate from a difference-in-differences regression with county weights but no time weights. We find that, compared to counties that did not receive a grant but were on a similar average presidential voting trajectory prior to 2020, and after accounting for remaining pre-2020 average differences in Democratic vote share, grant recipients had only a 0.1-percentage-point higher Democratic vote share in 2020. Column 4 presents our difference-in-differences estimate using both time and county weights. Using this specification, we estimate that grants caused an average increase in Democratic vote share of 0.02 percentage points. In summary, once we compare grant recipients to more similar counties, we find that these grants did not substantially increase Democratic vote share in receiving counties.

In columns 5 through 8, we present estimates of the effect of a grant on turnout using the same estimation strategies as in columns 1 through 4. Grant recipients and non-recipients are on more similar turnout trajectories than Democratic vote trajectories prior to 2020. Accordingly, the four estimation strategies produce more similar results. In column 5, we present a likely upwardly biased 0.24-percentage-point difference-in-differences estimate of the effect of the grants on turnout. In columns 6 through 8 we find that, when we use time weights, county weights, or both, our estimates range from -0.03 percentage points to 0.13 percentage points.

Focusing on our preferred specification in columns 4 and 8 where we use both time and county weights in a difference-in-differences regression, we find that the grants did not substantially increase Democratic vote share or turnout. Our estimates are highly precise on both outcomes. The standard error we report in column 4 is 0.11 percent, meaning that we could reject a hypothetical effect of the grants on Democratic vote share that is greater than 0.25 percentage points. Similarly, the standard error we report in column 8 is 0.14 meaning that we could reject a hypothetical effect of 0.30 percentage points.

4.4 Estimated Effects Similar or Less Favorable to Democrats in Wisconsin

In this section, we supplement our main finding with new municipality-level election data from Wisconsin. We find that the grants did not substantially increase Democratic vote share and or turnout.

As we discuss in Section 4.1, our main analyses study places where elections are administered at the county level because most parts of the country administer elections at that level, presidential election data is widely available at the county level, and changing municipal boundaries add potentially systematic noise to the election results data. Wisconsin is one of the states we leave out of this analysis because elections are primarily conducted at the municipal level (Herron 2023). To validate our main findings, we constructed a separate dataset of municipality-level election results and grant receipt in Wisconsin. We provide more details on this dataset in Section A.5 in the online appendix.

Using the same estimation strategies we use in Table 1, we report estimates of the effect of private election administration grants in Wisconsin in Table A.5 in the online appendix. We estimate very similar effects of the grants on Democratic vote share in Wisconsin as in our nationwide county-level analysis. Our preferred synthetic difference-in-differences estimator with county and year weights in column 4 estimates an effect of 0.11 percentage point. Across all three of our synthetic difference-in-differences estimates, our point estimates range from 0.11 to 0.31 percentage points, and we cannot reject the null hypothesis that the grants had no effect on Democratic vote share. The upper bound of the 95% confidence interval on our preferred estimate is an effect of 0.48 percentage points.

In columns 6 through 8 of Table A.5, we report our estimates of the effect of grants on turnout in Wisconsin. Contrary to the expectation that grants increased turnout in Democratic strongholds and thereby advantaged Democrats, we find that municipalities that received grants had a modest but statistically significant drop in turnout of approximately 0.7 percentage points. We interpret this result as evidence against grants improving turnout in

Democratic strongholds rather than strong evidence that the grants caused lower turnout in grant-receiving Wisconsin municipalities.

4.5 Estimated Effects Not Sensitive to Estimation Strategy

We examine the robustness of our main estimates in 1 using five alternative estimation strategies. Across all five approaches, we find similar, substantively small effects of the grants on turnout and Democratic vote share. Our estimates of the effect on Democratic vote share, reported in Table A.3 in the online appendix, range from 0.15 percentage points to 0.40 percentage. Our estimates of the effect on turnout, reported in Table A.4 also in the online appendix, range from -0.02 percentage points to 0.47 percentage points. These estimates come from five different strategies: 1) weighted regressions like synthetic difference-in-differences but without county-specific intercepts so the weights attempt to balance treatment and control outcomes on levels rather than trends, 2) regularized synthetic control (Doudchenko and Imbens 2016), 3) weighting the control units such that pre-2020 control-group outcome means exactly match treatment means while deviating as little as possible from uniform weights (Hainmueller 2012), 4) predicting the outcome and treatment propensity using random forests and using these estimates for augmented inverse propensity weighting (Athey, Tibshirani, and Wager 2019), and 5) predicting the outcome and treatment propensity using ensemble learners—pooling generalized additive models, boosting, regression trees, splines, and elastic nets—and using these predictions for augmented inverse propensity weighting.

4.6 Estimated Effect Similar in Battleground States

One concern is that, while the grants had a small effect on average, they may have had a larger effect in the closest states. We evaluate this claim by estimating the average effect of the grants on Democratic vote share and turnout in two sets of the most competitive states—states decided by less than 5 percentage points and states that the Cook Political Report identified as battleground states prior to election day.

In Table A.6 in the online appendix, we document that, even in the most competitive states, the effect of the grants was small. In column 1, we find that the effect of grants on Democratic vote share in close states was 0.54 percentage points. While this point estimate is larger than the effect we estimate using the full sample, the smaller sample size also means the subgroup estimates are substantially noisier, and we are unable to reject the hypothesis that the grants had no effect. When we extend our analysis to the battleground states according to the Cook Political Report, our estimate is more precise, and the estimated effect on Democratic vote share is nearly identical to our estimates from Table 1 using all counties.

In columns 3 and 4, we find that the effect of the grants on turnout in states decided by less than 5 percentage points and those the Cook Political Report labeled as battlegrounds was approximately the same size as we estimate using the full sample. In both cases, we cannot reject the hypothesis that the grants failed to increase turnout. We can also rule out positive effects on turnout of greater than 0.75 percentage points in the closest states.

4.7 Estimated Effect Similar in Populous Counties

Private election funding would have a larger effect on the aggregate election outcome if it was most effective in counties with more voters. Are the effects of grants larger in populous counties? We evaluate this possibility by splitting our sample into terciles by voting-age population and estimating effects for each subgroup separately. We present our results in Table A.7 in the online appendix. We find that the effects on turnout and Democratic vote share are no larger in more and less populous counties.

4.8 Effects not Larger for Counties Receiving Larger Grants

If additional spending on local election administration increases turnout or Democratic vote share, this would most likely happen because local officials use the money to make it easier for citizens to participate. This implies that the effects of money should increase as they get larger or, at the very least, be unrelated to grant size. While CTCL reports using a formula to decide the maximum amount each county was eligible to receive, the amounts that CTCL distributed to counties ranged from \$0.63 per voting-age resident at the 25th percentile to \$1.38 per voting-age resident at the 75th percentile. Might our small effect estimates mask an effect in counties that receive larger grants?

To answer this question, we split treated counties into three groups based on the amount of grant money going to the county per voting-age resident and produce separate synthetic difference-in-differences estimates of the effect of small, medium, and large grants. We present the results of these analyses in Table A.8 in the online appendix. Contrary to the expectation that the effect may be limited to places receiving the largest grants, we find that small grants increased Democratic vote share and turnout by more than large grants. We estimate that small grants increased Democratic vote share by 0.62 percentage points and turnout by 0.32 percentage points while large grants decreased Democratic vote share by 0.61 percentage points and turnout by 0.27 percentage points. Given that, unlike our other analyses, the synthetic difference-in-differences weights do a poor job of matching pre-2020 treated and control trajectories for these subgroups, we also present estimates using entropy balancing to match pre-2020 outcome means for control counties to the treated county means. Using entropy balancing, we no longer find negative effects for the largest recipients—the effects are almost exactly zero. We also find slightly smaller effects in places that received smaller grants. These effects average out to approximately the average effect estimates we present in sections 4.3 and 4.5.

5 Characterizing the Magnitude of the Effects

How large are the effects of private election administration funding? In this section, we benchmark the magnitude of our effect estimates against the remarkably tight margin of the 2020 presidential election. The 2020 presidential election turned on four states decided by margins of 1.16 percentage points or less: Georgia, Arizona, Wisconsin, and Pennsylvania. The margins in these states were widely understood to be very tight. Are the effects of the private election administration grants as small or smaller than these margins?

One simple way to interpret the effect size is to compare the effect of private election funding on Democratic vote share to the margin in these four close states. Of our three main estimates reported in Table 1, two of them are too small to have changed the outcome in any state, including Georgia, Arizona, Wisconsin, and Pennsylvania. Our third estimate is about as large as the margin in Wisconsin but still smaller than the margin in Pennsylvania. This is not to say that grant funding was sufficient to change the outcome in any of these states—only a subset of counties received the money, so county-level effects that are roughly the same magnitude as the margin in the state are not large enough to have swung the statewide outcome.

How large are the effects on turnout? One way to interpret these effects is to compare them to the effects of other election administration changes. The effect of the grants on turnout was less than half of the effect of an extra day of early voting (Kaplan and Yuan 2020), roughly half of the effect of a mailer encouraging citizens to vote by mail (Hopkins et al. 2021), less than one-tenth of the effect of universal vote by mail (Gerber, Huber, and Hill 2013; Thompson et al. 2020), and less than one-twentieth of the effect of mobile voting (Fowler 2020). While the estimated effects of polling place locations on turnout are typically noised than our estimates of the effects of grants, our estimates of the effects of grants tend to be smaller than the effect of having your polling place moved further away (Clinton et al. 2020; Tomkins et al. 2023; Yoder 2018). Our estimates of the effects of grants on turnout are small compared to all of these administrative changes.

Given the strong tendency of the grants to go to Democratic-leaning counties, the grants could advantage Democrats more than is implied by the Democratic vote share effect alone. On the other hand, many counties did not receive grants, so the effect of the grants on statewide totals is substantially smaller than the effect in the average county. To account for these concerns, we conducted a simple simulation study. In our simulation, we remove the average effect on turnout and Democratic vote share from all of the treated counties and assume untreated counties remain unchanged.[19] In states where we do not have grant data, we impute the probability that a county received a grant based on 2016 Democratic vote share and sample 1,000 random possible treatment assignments in those states. We then handle the counties we randomly assigned to treatment in that simulation as we handle the truly treated counties, removing the average effect of the grants on turnout and vote share.

Based on this simple simulation, we find that the estimates from two of our three of our weighted difference-in-differences estimation strategies imply that the grants were too small to swing the outcome of any statewide election. The estimates from our third weighted difference-in-differences strategy are large enough to have changed the outcome of the election in Georgia and Arizona but inconsistent with changing the outcome of the election in Pennsylvania or Wisconsin. Put together, this suggests that, even compared to the margin in very close elections recent elections, the effects of the grants were quite small.

It is important to note that, while these simulations help us understand the magnitude of the effects, we do not intend them as a reflection of what would have happened in the 2020 election had CTCL not made any grants. Our simulations do not account for the general equilibrium effects of the grants, such as changes in partisan spending, get-out-the-vote operations, or other government spending on election administration. Instead, we view our simulations as consistent with our interpretation that the grants had minimal effects.

[19] We also include municipal-level data from Wisconsin in our simulation analysis.

6 Discussion

The large influx of private funding for election administration in 2020, and the fact that Democratic counties were more likely to receive it, has led many politicians, journalists, and pundits to speculate that the funding advantaged Democrats. Despite these widespread concerns, we present evidence that these grants did not substantially increase turnout or Democratic vote share. Our results answer one of the key questions at the center of the debate over private funding of election administration, suggesting that it is not

Still, our findings leave unanswered two important questions: First, while we find that private funding did not increase turnout, it may have improved the election on other important dimensions. Many of the local officials who received the money said that they would have had trouble reporting their election results on time without the grants. Others said that the money allowed them to hire more staff which may have made the election run more smoothly, made voting more convenient, or improved election security and the accuracy of the count. We cannot observe these effects of the money, but they are important when deciding whether grant programs like these are effective. Second, the large backlash suggests that the grants may have led some citizens to doubt the outcome of the election. If that is the case, it is a potential cost worth considering in future attempts to shore up local election funding.

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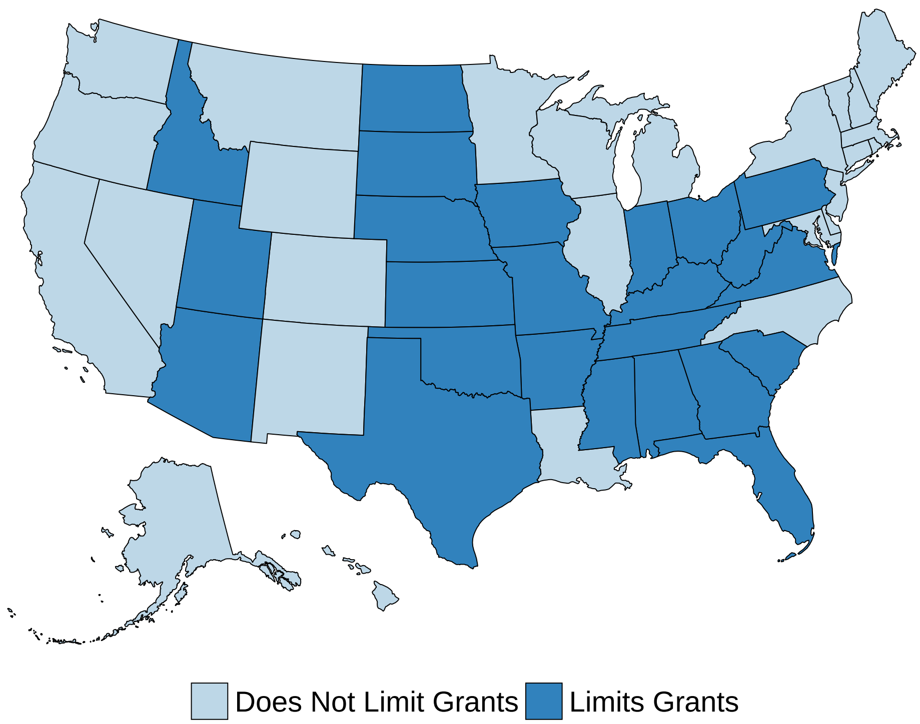
Online Appendix

Intended for online publication only.

Contents

A.1 New Laws Limiting Private Election Administration Grants

Figure A.1: **Twenty-Four States Have Passed Laws Limiting Private Election Administration Grants Since 2020.**



As we discuss in Section 1, twenty-four states have passed laws banning or substantially limiting private donations that support local election administration. Figure A.1 maps these states. Nearly every Southern state has passed a ban or limit on private funding. The only two Southern states that have not passed such a limit, Louisiana and North Carolina, have Democratic governors who vetoed legislative bills that introduced limits.

A.2 Decomposing Grant Selection

Table A.1: **Democratic-Leaning Counties Were More Likely to Receive a Grant in 2020.**

	Received Grant						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag Dem Vote Share	0.69	0.48	0.48	0.28	0.30	0.08	0.04
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.22)	(0.12)
Log(Population)		0.05	0.05	0.07	0.07	0.10	0.07
		(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Battleground			-0.02				
			(0.02)				
Covid Death Rate					-0.02		
					(0.02)		
Spending Per Capita (2016)						0.02	
						(0.01)	
Dem Clerk							0.06
							(0.03)
Constant	0.14	-0.26	-0.26				
	(0.02)	(0.07)	(0.07)				
Observations	2594	2594	2594	2594	2591	305	1104
State FEs	No	No	No	Yes	Yes	Yes	Yes

Robust standard reported in parentheses. Covid death rate is the number of deaths per 1,000 residents prior to September 1, 2020. Spending per capita is the amount the county spent on elections per voting age resident in 2016.

A.3 Validating Weighted Diff-in-Diff Using 2016 as a Placebo Treatment Period

Table A.2: **Weighted Difference-in-Differences Approach Balances Dem Vote Share and Turnout in 2016.**

	Dem Vote Share (%)				Turnout (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grant Recipient in 2016	3.38	0.81	0.48	0.38	0.12	-0.14	-0.20	-0.17
	(0.36)	(0.22)	(0.23)	(0.23)	(0.17)	(0.10)	(0.10)	(0.10)
Num Grant Recipients	924	924	924	924	924	924	924	924
Num Counties	2,594	2,594	2,594	2,594	2,594	2,594	2,594	2,594
Observations	18,158	18,158	18,158	18,158	18,158	18,158	18,158	18,158
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Weights	No	No	Yes	Yes	No	No	Yes	Yes
Year Weights	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 7 presidential elections from 1992 to 2016.

A.4 Alternative Strategies for Estimating the Effect of Grants

Table A.3: **Election Administration Grants Did Not Noticeably Advantage Democrats in 2020, Alternative Estimators.**

	Dem Vote Share (%)				
	(1)	(2)	(3)	(4)	(5)
Grant Recipient in 2020	0.28	0.40	0.23	0.25	0.15
	(0.13)	(0.77)	(0.11)	(0.11)	(0.07)
Num Grant Recipients	924	924	924	924	924
Num Counties	2,597	2,597	2,597	2,597	2,597
Observations	20,776	20,776	2,597	2,597	2,597
Estimator	SDID Without Intercept	Synthetic Control	Entropy Balancing	Causal Forest	Super Learner

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data for columns 1, 2, 6, and 7 is a balanced panel of counties in the 8 presidential elections from 1992 to 2020. Data for columns 3, 4, 5, 8, 9, and 10 is wide with 7 lags of the dependent variable included. Synthetic control is a regularized synthetic control. SDID without intercept is synthetic difference-in-differences without county fixed effects. Entropy balancing is maximum entropy reweighting to balance grant recipients and non-recipients on the average of each lag of the outcome. Causal forest is double machine learning using random forests for both the outcome and propensity models. Super learner is double machine learning using an ensemble learner for both the outcome and propensity models.

Table A.4: **Election Administration Grants Did Not Noticeably Increase Turnout in 2020, Alternative Estimators.**

	Turnout (%)				
	(1)	(2)	(3)	(4)	(5)
Grant Recipient in 2020	0.03	-0.02	0.47	0.00	0.04
	(0.14)	(0.39)	(0.39)	(0.14)	(0.12)
Num Grant Recipients	924	924	924	924	924
Num Counties	2,597	2,597	2,597	2,597	2,597
Observations	20,776	20,776	2,597	2,597	2,597
Estimator	SDID Without Intercept	Synthetic Control	Entropy Balancing	Causal Forest	Super Learner

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data for columns 1, 2, 6, and 7 is a balanced panel of counties in the 8 presidential elections from 1992 to 2020. Data for columns 3, 4, 5, 8, 9, and 10 is wide with 7 lags of the dependent variable included. Synthetic control is a regularized synthetic control. SDID without intercept is synthetic difference-in-differences without county fixed effects. Entropy balancing is maximum entropy reweighting to balance grant recipients and non-recipients on the average of each lag of the outcome. Causal forest is double machine learning using random forests for both the outcome and propensity models. Super learner is double machine learning using an ensemble learner for both the outcome and propensity models.

A.5 Estimating the Effect of Grants in Wisconsin

We supplement our main county-level anlaysis with a municipality-level analysis in Wisconsin. We built our main municipality-level analysis dataset from four sources: Wisconsin's Legislative Technology Services Bureau provided election results from 1990 to 2020 at the 2020 municipal ward level. We aggregate this data to the municipal level and link it to a list of all Wisconsin municipalities from Wisconsin's Department of Administration. We add grant amounts by hand to the list of municipalities with geocodes. Finally, we join this data with estimates of the voting age population in each municipality by mapping 2000 and 2010 Census block population statistics into 2020 Census blocks and aggregating to the municipal level. Given that we do not have visibility into how the Legislative Technology Services Bureau computed ward-level election results, we also replicate their work, collecting the original ward-level election results from 2004 to 2020. Our two election datasets are highly correlated and, in many cases, show the exact same number of votes for each party in the same municipality and year.

As we discuss in 4.4, Table A.5 captures our finding that the grants did not substantially increase Democratic vote share and, if anything, reduced turnout.

Table A.5: **Election Administration Grants Did Not Noticeably Advantage Democrats or Increase Turnout in Wisconsin in 2020.**

	Dem Vote Share (%)				Turnout (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grant Recipient in 2020	1.33	0.31	0.24	0.11	-0.64	-0.71	-0.70	-0.69
	(0.58)	(0.20)	(0.19)	(0.19)	(0.40)	(0.28)	(0.28)	(0.29)
Num Grant Recipients	206	206	206	206	206	206	206	206
Num Counties	1,843	1,843	1,843	1,843	1,843	1,843	1,843	1,843
Observations	12,901	12,901	12,901	12,901	12,901	12,901	12,901	12,901
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Weights	No	No	Yes	Yes	No	No	Yes	Yes
Year Weights	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 8 presidential elections from 1992 to 2020.

A.6 Investigating How Grant Effects Vary Across Counties

A.6.1 Effect of Grants in Battleground States

Table A.6: **Election Administration Grants Did Not Noticeably Advantage Democrats or Increase Turnout in 2020, Battleground States.**

	Dem Vote Share (%)		Turnout (%)	
	Close States	Cook Battlegrounds	Close States	Cook Battlegrounds
Grant Recipient in 2020	0.54	0.04	0.10	-0.46
	(0.29)	(0.21)	(0.32)	(0.22)
Num Grant Recipients	119	326	119	326
Num Counties	421	906	421	906
Observations	3,368	7,248	3,368	7,248
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
County Weights	Yes	Yes	Yes	Yes
Year Weights	Yes	Yes	Yes	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 8 presidential elections from 1992 to 2020. Close states are those where the winner was decided by fewer than 5 percentage points. Cook battleground states are those that the Cook Political Report identified as battlegrounds prior to election day.

A.6.2 Effect of Grants by County Population Tercile

Table A.7: **Election Administration Grants Did Not Noticeably Advantage Democrats or Increase Turnout in 2020, Voting Age Population Tercile.**

	Dem Vote Share (%)			Turnout (%)		
	Small Population	Medium Population	Large Population	Small Population	Medium Population	Large Population
Grant Recipient in 2020	-0.24	0.15	-0.42	-0.71	0.08	-0.14
	(0.17)	(0.13)	(0.21)	(0.24)	(0.23)	(0.23)
Num Grant Recipients	256	253	415	256	253	415
Num Counties	866	865	865	866	865	865
Observations	6,928	6,920	6,920	6,928	6,920	6,920
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County Weights	Yes	Yes	Yes	Yes	Yes	Yes
Year Weights	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 8 presidential elections from 1992 to 2020. Small, medium, and large population counties are defined by terciles of voting age population.

A.6.3 Effect of Grants by Grant Size Tercile

Table A.8: **Election Administration Grants Did Not Noticeably Advantage Democrats or Increase Turnout in 2020, Grant Size Tercile.**

	Dem Vote Share (%)			Turnout (%)		
	Small Grant	Medium Grant	Large Grant	Small Grant	Medium Grant	Large Grant
Grant Recipient in 2020	0.62	0.08	-0.61	0.32	0.07	-0.27
	(0.12)	(0.14)	(0.18)	(0.21)	(0.21)	(0.22)
Num Grant Recipients	305	304	315	305	304	315
Num Counties	1,978	1,977	1,988	1,978	1,977	1,988
Observations	15,824	15,816	15,904	15,824	15,816	15,904
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County Weights	Yes	Yes	Yes	Yes	Yes	Yes
Year Weights	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors estimated with 1,000 county block bootstrap samples reported in parentheses. Data is a balanced panel of counties in the 8 presidential elections from 1992 to 2020. Grant sizes are determined by tercile of grant size per voting age resident among recipients. Small is the smallest tercile, and large is the largest tercile.