Voting in a Pandemic: COVID-19 and Primary Turnout in Milwaukee, Wisconsin

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We report the first study of the effect of the novel coronavirus SARS-CoV-2 (COVID-19) on polling place consolidation and voting behavior. We draw upon individual-level observations from Milwaukee matched to similar observations in the surrounding municipalities to assess whether fewer polling places in the April 2020 presidential primary election decreased turnout in the city. We find polling place consolidation reduced overall turnout by about 8.7 points and reduced turnout among the Black population in the city by about 10 points. We conclude, based on these data, that polling place consolidation even accompanied by widespread absentee voting in the face of an emergency may result in disenfranchisement, particularly among Black voters.

The Wisconsin presidential primary election provides a valuable means to assess how the novel coronavirus SARS-CoV-2 (COVID-19) has altered voting behavior in a natural experiment. The weeks leading up to the presidential primary election on April 7 were tumultuous. Democratic Governor Tony Evers declared a state of emergency on March 12 when there were 8 confirmed COVID-19 cases.[1] On March 17, Evers issued a ban on all gatherings of more than 10 people[2] and on March 27 called for every voter in the state to be sent an absentee ballot (Wise 2020). The Republican-controlled legislature refused this proposal. The weekend before the election, Evers called an emergency session of the legislature hoping to postpone the date of the election. This effort, too, was rebuffed. As a last resort, Evers issued an executive order on April 6 to delay the primary election until the ninth of June[3] which was overturned by the state supreme court.[4] The day before the primary, the U.S. Supreme Court ruled absentee ballots would be invalid if the ballot was not hand-delivered by April 7 or postmarked by election day and received by April 13.[5]

These maneuvers occurred against the backdrop of overstretched electoral resources following from the increasing severity of the COVID-19 pandemic. The *Milwaukee Journal Sentinel* observed the state was short some 7 thousand poll workers on March 31 (Marley and Beck 2020), a shortage which led to polling place consolidation around the state. The reduction in polling places was acute in Milwaukee where just 5 polling places remained open, compared with 182 in November of 2016.[6] Even as polling places were consolidated, a surge in absentee voting occurred: statewide, more than 964 thousand ballots were cast by mail in the April primary, compared with just 171 thousand in the 2016 presidential primary.[7] Nonetheless, there is evidence for "leaked" absentee ballots that were excluded from the set of counted ballots (Stewart 2010): Only 84.8% of mail ballots delivered to voters were ultimately counted. Past research indicates that, under normal circumstances, polling place consolidation leads to lower turnout (e.g., Brady and Mcnulty 2011; McNulty, Dowling, and Ariotti 2009). The circumstances in Milwaukee, in contrast to these earlier studies, are novel in that consolidation here was a consequence of a natural disaster rather than of an administrative decision to reallocate polling places.

This study asks two questions. First, we investigate whether polling place consolidation measurably decreased overall turnout in the context of a primary election with widespread access to vote-by-mail options. Just 16.1% of registered voters in the City of Milwaukee voted in the April primary, while the overall turnout rate in the rest of Milwaukee County and surrounding Waukesha, Washington, and Ozaukee Counties was 42.2%. Second, we aim to measure whether COVID-19, which was more widespread in the City of Milwaukee, depressed turnout through other mechanisms. The opportunity cost literature indicates that household shocks like family emergencies can make it less likely that voters invest the time and resources needed to learn where their polling place is and who is on the ballot; the concentration of COVID-19 in the City of Milwaukee may have increased these opportunity costs and depressed turnout.

Prior Literature

Election administration in the United States is defined by two values: decentralized authority and oversight by partisan, elected officials (Gerken 2007). Election administration in Wisconsin is an extreme version of the first value. While county officials tend to be the front-line administrators for elections, in Wisconsin this task is a duty of officials in each of the 1,851 municipalities (Huefner et al. 2007, pp. 111–136). The population disparity between

Milwaukee on the one hand and rural parts of the state is vast and raises questions of equal administrative capacity (Kimball and Baybeck 2013) when it comes to questions like the placement and staffing of polling places in an election.

These administrative decisions can directly shape participation. Disrupting one's routine with regard to voting – whether by relocating or reducing the number of polling places – reduces turnout by imposing new search and transportation costs on voters (Brady and Mcnulty 2011). A moved polling place reduced the likelihood of voting by about 5.5 points in a 2001 local election (Haspel and Knotts 2005). Consolidation between 2000 and 2008 reduced county-level turnout by about nine-tenths of a point (Kropf and Kimball 2012, p. 68). Increasing the distance to polls in California in 2003 reduced the likelihood of voting in person by between 2 and 4 points, while consolidating polling places in a New York State local election reduced turnout by an average of 7 points (McNulty, Dowling, and Ariotti 2009). A recent study of 9 municipalities in Massachusetts and Minnesota found increasing the distance to the polls by about 0.25 miles reduced turnout by between 2 and 5 points, and that this effect was more pronounced among "high-minority, low-income, and low-car-availability areas" in the context of a non-presidential election (Cantoni 2020, p. 88). While absentee voting is more likely as the distance to the polls increases, this effect is not large enough — at least in past elections — to offset the decrease from consolidation itself (Brady and Mcnulty 2011).

The effect of distance to the polling place on voting is nonlinear (Dyck and Gimpel 2005, pp. 541–542; Gimpel and Schuknecht 2003, pp. 481–484). Dyck and Gimpel (2005) deploy observations ranging from 0.1 to 65 miles from the polling place. They report being one standard deviation from the polls (about 1.75 miles) reduced the likelihood of voting at the polls by 2.3 points, but made absentee voting more likely by 0.9 points. A study of three counties in Maryland in the 2000 election found moving 1 mile *closer* to the polls made voting *more* likely by 0.45 points, while observing generally that "[t]urnout is highest when distances to the polling place are very short, and when they are excessively long, but lower in the middling ranges of distance" (Gimpel and Schuknecht 2003, p. 481).

Vote centers are an alternative convenience voting reform that could be well-suited to counteract the depressive turnout effects of polling place consolidation. Vote centers are distinguished from polling places in two ways: They are open to all voters in an area (like a county or group of precincts) and are centralized (Stein and Vonnahme 2008, pp. 490–491). Vote centers increase turnout among infrequent voters and in low-turnout elections (Stein and Vonnahme 2008, 2012; but see Cortina and Rottinghaus 2019). That being said, this reform was not adopted in the April primary in Wisconsin. Election officials instead maintained the precinct- based assignment for voters instead of opening each polling place to any registered voter in the jurisdiction (Mickle 2020).

The literature discussed above, however, examines the effect of polling place consolidation under more normal circumstances. It is unclear how well this work speaks to elections held during a pandemic. Indeed, with more than 97% of polling places in Milwaukee City closed, the primary contest may be better understood as an example of conducting elections entirely by mail, as is the case in some western states. That reform increased turnout in Washington (Henrickson and Johnson 2019; Gerber, Huber, and Hill 2013), decreased turnout in California (Elul, Freeder, and Grumbach 2017; Bergman and Yates 2011; Kousser and Mullin 2007), and had no significant effect in Oregon (Gronke and Miller 2012). That the same reform has disparate effects where it has been adopted is one reason scholars are left unsatisfyingly answering the question about the turnout effects of convenience voting reforms with both "'no' and 'yes'" (Bergman 2015). Of course, these shifts to vote-by-mail were planned out policy changes accompanied by voter education programs. It seems likely that a last-minute decision to conduct the election this way would be less successful at mitigating any depressive effects.

This literature provides the framing for Hypothesis A: By both increasing the distance voters had to travel to arrive at their polling places and requiring that they learn the location of the new polling place, we expect that polling place consolidation lowered turnout in the City of Milwaukee.

The City of Milwaukee was also home to a worse COVID-19 outbreak leading up to the election. In Milwaukee County there had been roughly 14 positive tests for COVID-19 per 10,000 residents as of the date of the primary election, compared with 7.5 positive tests per 10,000 residents in Ozaukee County, and 4.4 and 4.2 in Washington and Waukesha Counties, respectively.[8] Opportunity cost literature indicates that this probably further decreased the turnout of residents of the city. As Rosenstone (1982) notes, competing demands on voters' time such as "family illness [or the] death of a close friend or relative" (p. 42) can reduce turnout. Other research has found that the death of a spouse (Hobbs, Christakis, and Fowler 2014) and negative health events (Pacheco and Fletcher 2015) decrease participation. The concentration of COVID-19 in the City of Milwaukee may have increased these "opportunity costs" and depressed turnout in addition to any effect associated with the closed polling places.

We form hypothesis B based on the opportunity cost literature: Greater exposure to COVID-19 in the City of Milwaukee reduced turnout above-and-beyond the negative effects due to polling place consolidation.

With both treatment effects pushing in the same direction, it is unclear whether COVID- 19 depressed turnout more via polling place closures or via other mechanisms.

Data and Research Design

We use individual-level voter registration and turnout records from L2 Political to estimate all our models. In addition to providing the information available in the registered voter file, L2 provides estimates for voters' partisan affiliation (voters do not register with parties in Wisconsin), race, household income, and education. Milwaukee is the most segregated large American city with a substantial Black population (Frey 2018). Because L2's racial estimates rely (in part) on neighborhood demographics, this segregation increases the precision of these estimates. Put differently, if the population of a census tract is 98 percentage White, estimates about any individual's race will be far more accurate than a tract in where the population is 50 percentage White and 50 percentage Black. L2 also geocodes voters to their home addresses. Although L2 data includes vote mode in some states' voter files, our copy of the L2 Wisconsin file merely records whether — not how — an individual votes. This is an important limitation: We can test only whether someone shifted from being a voter to being a non-voter, but cannot test who shifted from in-person to mail voting. Nevertheless, these data allow us to test for any net depressive effects on turnout.

Compared to the City of Milwaukee, the rest of the state did not see such drastic consolidation of polling places. Outside of Milwaukee, the state had 10.2 percent fewer polling places open in April 2020 than November 2016 (see Figure 1). The cuts in Milwaukee led to many more registered voters per polling place: Although Washington, Ozaukee, and Waukesha Counties each had 1 polling place for every 7 thousand or fewer voters, there was only 1 polling place for every 103 thousand registered voters in the City of Milwaukee. As discussed above, residents of Milwaukee were also likely subjected to a *second* treatment due to the severity of COVID-19. Simply comparing the turnout of Milwaukee to the suburbs therefore cannot reveal the depressive effect of polling place consolidation alone, but rather the net effect of higher exposure to the pandemic *combined with* poll site closures.

FIGURE 1 ABOUT HERE

Unfortunately, low-level data on the prevalence of COVID-19 on or before election day is unavailable. Shortly after the election the state began publishing counts at the census tract level, but these figures are not available for the period in which they were most relevant for this study — that is, before voters headed to the polls.

To isolate the effect of polling place consolidation from COVID-19, we leverage electoral jurisdiction boundaries as an assignment to treatment mechanism (Kaplan and Yuan 2020; Cantoni 2020). Our primary design is akin to a regression discontinuity in space that exploits the municipal boundary line to compare turnout for voters on either side of the "cutpoint" (Keele, Titiunik, and Zubizarreta 2015). Because of how close these voters lived to one another, it is likely they went about their daily lives in the same local milieu. Social geography has an effect on local politics and participation in elections (Enos 2017; Tam Cho, Gimpel, and Dyck 2006; Gimpel, Dyck, and Shaw 2004), and research from New Orleans indicates that COVID-19 is clustered at the neighborhood level (van Holm, Wyczalkowski, and Dantzler 2020). We therefore assume that, although they lived in different municipalities, the people proximate to each other but on either side of the municipal boundary were similarly exposed to COVID-19. Put differently, we *directly* control for a host of covariates from the voter file, and we *indirectly* control for COVID-19 by selecting pairs of treated and control voters who live in very close proximity to one another.

The regression discontinuity framework, however, assumes that individuals cannot “select” around the cutpoint; that within a narrow window, individuals on either side of the cutpoint are identical. This is probably too strong of an assumption: Voters likely select around the administrative boundary, a problem exacerbated by Milwaukee’s extreme segregation. Keele, Titiunik, and Zubizarreta (2015) suggests a solution to this problem: “When there appears to be strong self-selection around the border of interest, one alternative is to combine designs and to assume that, after conditioning on covariates, treatment assignment is as-if randomized for those who live near the city limit” (p. 228). They combine regression discontinuity and matching methodologies, exploiting the Milwaukee City municipal boundary to investigate whether ballot initiatives increase turnout.

We adopt this approach by genetically matching (Sekhon 2009) each registered voter in Milwaukee City to two voters who live outside the city but in Milwaukee, Waukesha, Washington, or Ozaukee County, which each share a border with Milwaukee City.[9]Although these counties include some urban areas, we refer to the controls as suburban voters for convenience. To be sure, the vast majority of our eventual control voters live very close to the Milwaukee border — and are thus in fact suburbanites in the traditional sense.

Treated and control voters are matched exactly on turnout in the 2016 and 2018 primary elections, and on their partisan affiliation. Voters are also matched on their gender, their household income, whether they have a college education, and their race or ethnicity.

Finally, we include voters' latitude and longitude to ensure physical proximity to one another.

Although this differs from a regression discontinuity in which there is a band around a cutpoint, the logic is the same. As the maximum allowed distance between treated and control voters approaches zero, we narrow the band around the cutpoint represented by the municipal border. For instance, when the maximum distance allowed between a treated voter and her match is 0.5 miles, each voter will live (on average) within 0.25 miles of the border. It is important to note that this is more conservative than matching treated and control voters within a buffer around the border — not only must pairs both live inside a buffer, they must also live near one another within that buffer.

By beginning with a strict geographic restriction, we isolate the causal effect of polling place consolidation on turnout. To estimate the net effect of polling place consolidation *and* COVID-19, we then expand the maximum distance allowed between treated and control voters. These geographic buffers range between 150 feet and 10 miles. While we cannot directly observe the effect of COVID-19, we can observe whether the overall treatment effect grows larger as we introduce more distance between the pairs. Because we have controlled for other relevant covariates, the only additional difference between treated and control voters will be their COVID-19 exposure.

Our results are likely to be somewhat conservative. Some municipalities outside of Milwaukee City reduced their number of polling places (see Figure 1). This means some of our control voters received a very weak treatment, therefore collapsing the difference between the treated and control voters and pushing our estimated treatment effect toward zero. We do not expect that different races in the suburbs and City of Milwaukee differently structured turnout. The April primary ballot included Democratic and Republican presidential primaries, a race for a seat on the state supreme court, seats on the state court of appeals and the state courts, and a statewide referendum. While Milwaukee County and the surrounding counties are in different Appeals Court districts, both judicial districts had races on the ballot, though the race in Milwaukee County was uncontested. At the circuit court level, only Ozaukee County did not have a judicial race in the election. There is, in short, little cause for concern of unique campaign effects biasing our results. The only contextual differences between Milwaukee City and its suburbs are therefore the polling place consolidation and disparate prevalence of COVID-19.

Results

We begin by presenting the results of the matching model, where each treated voter is matched with two control voters.[10] Table 1 demonstrates that the matching procedure was largely successful: We achieve substantial improvement along all characteristics. Milwaukee City is far less White than the suburbs; has far lower incomes and education levels; and saw much lower turnout in recent primary elections. We do not include latitudes and longitudes in the balance table but the average distance between a treated voter and her controls is 2.2 miles. Matching is done with replacement, and ties are broken randomly.

TABLE 1 ABOUT HERE

Table 2 presents the results of ordinary least squares regressions testing the treatment effect. In Table 2 we require treated and control voters to live within 0.5 miles of one another.[11] For this reason, the number of observations in Table 2 is relatively low: Most Milwaukee voters do not live within 0.5 miles of the municipal border and a suburban control, and are thus excluded. In fact, just 13% of registered voters in Milwaukee City (and their matches) are included in this specification. The dependent variable takes the value 1 if a voter cast a ballot in the April primary, and 0 if she did not. We also test whether the treatment effect was different for Black voters than for other voters which Cantoni (2020) indicates is possible. Models 1 and 3 include just the treatment variable (and, in Model 3, the interaction term) while Models 2 and 4 add in the variables on which the matching was performed (but without latitude and longitude).

Some important characteristics are unavailable at the individual-level and are not included in the matching procedure. We expect, however, that these will vary with the characteristics that are included. Car-ownership**,** for one example of an omitted variable**,** is a significant factor in turnout effects of polling place consolidation (Cantoni 2020) and provides a helpful post-hoc test of this assumption Although car ownership is not included in the model, the average treated voter in Table 2 lives in a census tract where 90.61% of households own cars; their controls live in neighborhoods where that figure is 90.64%. We thus have good reason to believe that the matching procedure reduces differences between treated and control voters even for characteristics not directly included.

Although COVID-19 data is not available prior to the election, the Department of Health Services began releasing this data at the census tract level shortly after the election. We use cumulative positivity rates from April 21 — two weeks after the election— to proxy potential COVID-19 rates as of the election as a robustness check in Model 5. Insofar as these are correlated with COVID-19 prevalence on election day, they may be probative to the direct effect of COVID-19 on turnout.[12] However, because the COVID-19 data is not available as of the primary election, Model 5 is not intended to provide definitive proof of the relationship between virus prevalence and turnout. In each model, robust standard errors are clustered at the level of the match (Abadie and Spiess 2019).

TABLE 2 ABOUT HERE

Models 1 and 2 indicate that turnout was depressed by roughly 8.7 percentage points in the April primary in Milwaukee City relative to suburban voters. Models 3 and 4 indicate that this decrease was especially pronounced among Black voters, whose turnout was about 10 percentage points below that of their suburban matches. Model 5 shows no significant turnout effect where a higher share of cumulative COVID-19 tests was positive two weeks after the election. The large treatment effect supports Hypothesis A.

We are also interested in whether the size of the treatment effect grows as we include pairs who live further away from one another. Figure 2 re-estimates of Model 3 from Table 2 using different maximum distances between pairs. As the maximum distance between treated and control voters grows, the number of observations also grows to include all registered voters in Milwaukee City and their matches.

FIGURE 2 ABOUT HERE

The depressive effect for Black and non-Black voters grows as we allow paired voters to live further apart. It is important to note that this is not due to different underlying propensities to vote: The matching procedure requires that the participation (or lack thereof) of treated voters in the 2016 and 2018 primaries is exactly mirrored by their controls. The treatment effect between the half-mile and most lenient models for non-Black voters grows by roughly 1.6 percentage points, and it grows for Black voters by 5.7 points. Given the extreme racial disparities of COVID-19 in Wisconsin (Hayda 2020) it is unsurprising that these "direct effects" are so much greater for Black voters. This provides evidence to support Hypothesis B.

Discussion

On the one hand, polling place closures have long been understood to reduce turnout among voters. On the other hand, when jurisdictions have switched to primarily vote by mail systems, turnout has hardly changed. In the face of COVID-19, it was unclear how closed polling places would affect turnout. The enormous surge in absentee ballots indicated that the negative turnout effects might not have been large, but reporting of extensive lines for in-person voting on election day in Milwaukee (Viebeck et al. 2020) led us to expect that there were measurable negative turnout effects.

This note makes clear that polling place closures reduced turnout in the April primary in Milwaukee in the context of COVID-19, despite unprecedented demand for absentee ballots. The 8.7 percentage point decrease we observe is quite large; this effect amounts to about a third of the 26.8%turnout among control voters. The case of Milwaukee also sheds some light on the direct effect of COVID-19 on turnout. We know that COVID-19 was more widespread in Milwaukee City at the time of the election. Expanding the distance between treated and control voters led to larger treatment effects. Because the only thing varying in these specifications was space — and, therefore, COVID-19 exposure — this provides some evidence that COVID-19 directly reduced turnout (even the inclusion of the prevalence of the virus two weeks following the election was not related with turnout). However, despite our best efforts to reduce salient differences between treated and control voters, it is possible that Figure 2 indicates that the depressive effect of polling place consolidation on turnout is larger for central-city dwellers than those at the edges. We therefore consider these results as the first, indirect estimates of the effect of COVID-19 on turnout, which future work should measure directly.

These data have two boundary conditions it is important to bear in mind. First, the onset of the pandemic and the timing of the April primary did not allow for a robust public messaging campaign about mail voting options and it may be the case that the August and November elections, held after the initial phase of the pandemic, saw smaller effects due to less severe polling place consolidation and better voter education. The City of Milwaukee may well have learned from their April experience: In the August partisan primary, there were 168 polling places open in the city.[13] Second, it may be the case that the larger depressive effect for Black rather than for non-Black voters that we observe is a product of the relatively high segregation rate in Milwaukee compared to other American cities. Why polling place consolidation disproportionately depressed turnout among Black voters is unclear and should be the focus of future research based in other localities. This finding, nonetheless, raises concerns about racial representation when jurisdictions are forced by a natural emergency to consolidate polling places.

This note answers one question related to the effect of COVID-19: Given the pandemic, how do polling place closures affect turnout? Future research must consider the overall turnout and representational impacts of COVID-19 on this year's contests. It is worth noting that recently published research found that the April primary was not linked to any surge in COVID- 19 cases in Wisconsin (Leung et al. 2020), which should allay concerns that polling places can only be kept open at the expense of public health. The primary elections in Milwaukee, Wisconsin, make one thing clear: Even as many voters transition to vote-by-mail in the face of a pandemic, polling place consolidation can still have disenfranchising effects — particularly for Black voters.

Notes

[1] See https://www.dhs.wisconsin.gov/covid-19/cases.htm.

[2] See https://evers.wi.gov/Documents/COVID19/UPDATEDOrder10People.pdf.

[3] See https://bit.ly/3fJTqZT.

[4] See https://wapo.st/2Cg79sK.

[5] See https://www.supremecourt.gov/opinions/19pdf/19a1016_o759.pdf.

[6] See https://elections.wi.gov/elections-voting/2016/fall and https://elections.wi.gov/node/6524.

[7] See https://elections.wi.gov/sites/elections.wi.gov/files/2020-05/April%202020%20Absentee%20Voting%20Report.pdf.

[8] See https://www.dhs.wisconsin.gov/covid-19/county.htm.

[9] There are two instances where we cleaned the data for the purposes of our matching methodology. First, there are 143 observations in the L2 data that are problematic. According to latitude and longitude coordinates these observations are within the boundary of Milwaukee City but are coded as outside the city. We omit these observations. Second, Rather than require voters to obtain and mail absentee request forms, the villages of Whitefish Bay and Bayside automatically sent mail ballot applications to all registered voters, potentially driving up their turnout relative to Milwaukee City (Gilbert 2020). We therefore exclude voters in these villages as potential controls.

[10] Due to computing constraints, we use a 1% sample of voters (stratified by treatment status) to generate weights used in the actual matching model, though the whole pool is eventually used for the matching procedure itself.

[11] A treated voter might live within the cutoff distance from one of her controls but not the other. The regression weights are updated for each regression to reflect this possibility.

[12] Positive test rates are calculated as positive counts divided by the sum of positive and negative counts. The Department of Health Services replaces counts of less than 5 COVID-19 cases with "-999;" we re-code these as the mean value "2." See: https://data.dhsgis.wi.gov/datasets/covid-19-historical-data-table.

[13] See https://elections.wi.gov/node/6527.

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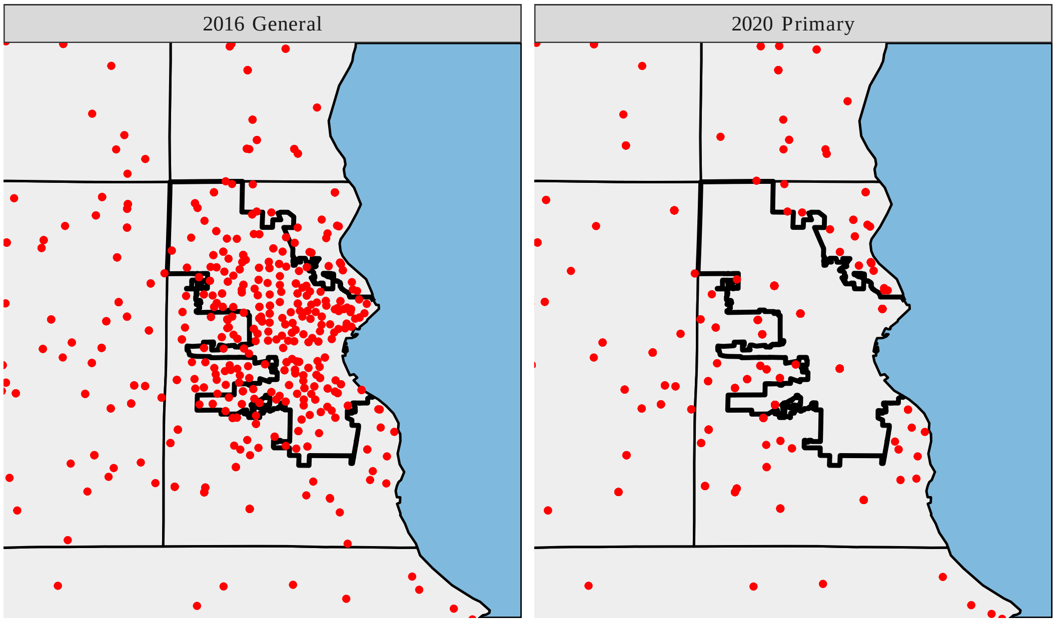
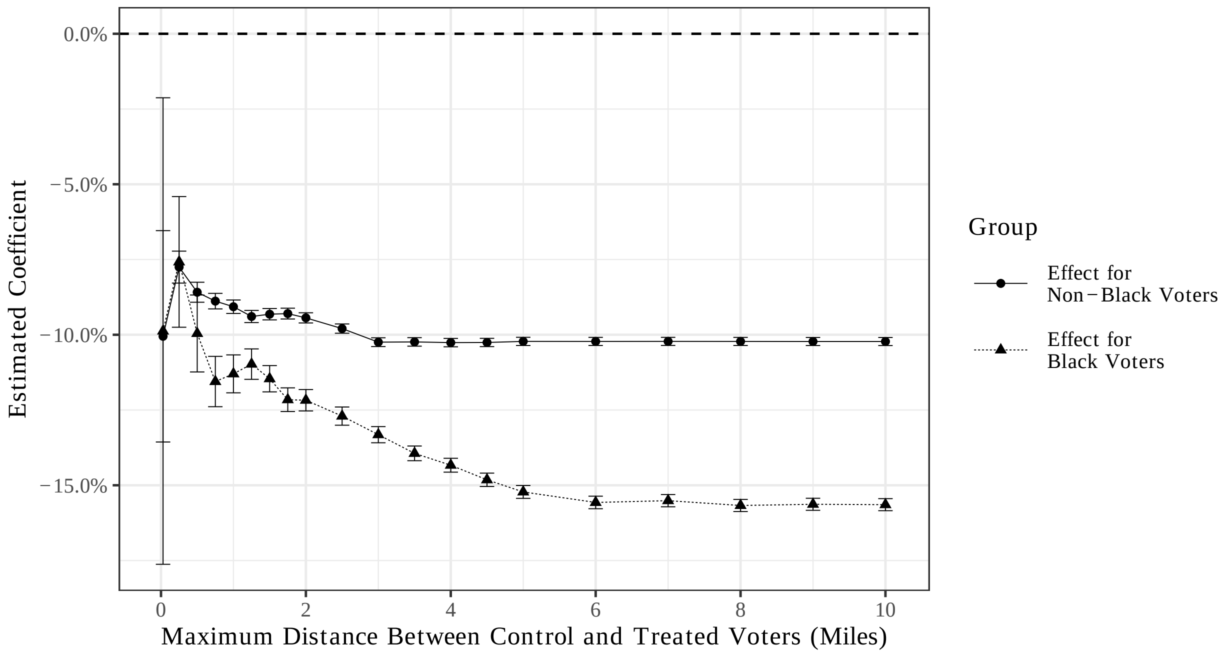


Source: Wisconsin Elections Commission.
Notes: Thin lines show county borders. Thick lines show Milwaukee City border.



Notes: 95% confidence bars shown.

Figure 1: Estimated Depressive Effect of Living in Milwaukee, 2020 Primary

Table 1: Balance Table

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
% Voted in 2016 Primary	26.8%	51.6%	26.8%	26.8%	100.00	100.00	100.00	100.00
% Voted in 2018 Primary	15.2%	27.9%	15.2%	15.2%	100.00	100.00	100.00	100.00
% Male	42.6%	45.5%	42.6%	42.6%	100.00	100.00	100.00	100.00
% Democrats	65.5%	20.7%	65.5%	65.5%	99.99	99.99	99.99	99.99
% Republican	8.6%	58.4%	8.6%	8.6%	99.99	99.99	99.99	99.99
Income	$59,317	$99,255	$59,317	$59,334	99.96	99.96	99.91	99.83
% with Collegiate Education	12.7%	33.3%	12.7%	12.7%	100.00	100.00	100.00	100.00
% White	46.3%	76.0%	46.3%	46.3%	100.00	100.00	100.00	100.00
% Black	30.8%	0.9%	30.8%	30.8%	100.00	100.00	100.00	100.00
% Latino	8.8%	3.4%	8.8%	8.8%	100.00	100.00	100.00	100.00
% Asian	1.9%	1.7%	1.9%	1.9%	100.00	100.00	100.00	100.00

Table 2: Turnout in 2020 Primary

	Turnout				
	(1)	(2)	(3)	(4)	(5)
Lives in Milwaukee	-0.087^{***}	-0.087^{***}	-0.086^{***}	-0.086^{***}	-0.086^{***}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Black		-0.036^{***}	-0.032^{***}	-0.029^{***}	-0.032^{***}
		(0.004)	(0.006)	(0.006)	(0.006)
Black × Lives in Milwaukee			-0.014^{**}	-0.013^{**}	-0.014^{**}
			(0.007)	(0.007)	(0.007)
Positive Test Rate					-0.004
					(0.014)
Constant	0.268^{***}	0.051^{***}	0.270^{***}	0.050^{***}	0.271^{***}
	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Includes Other Matched Covariates		X		X	
Includes County Fixed Effects	X	X	X	X	X
Observations	181,181	181,181	181,181	181,181	181,181
R^2	0.011	0.357	0.012	0.357	0.012
Adjusted R^2	0.011	0.357	0.012	0.357	0.012

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Robust standard errors (clustered by at level of match) in parentheses.
Models 2 and 4 also include the covariates listed in Table 1.