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

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August 21, 2020

Abstract

We report the first study of the effect of the novel coronavirus SARS-CoV-2 (COVID-19) on voting behavior, as effected through polling place consolidation. We draw upon individual-level observations from Milwaukee matched to similar observations in the surrounding counties to assess whether fewer polling places in the primary election decreased turnout in the city. We find polling place consolidation reduced overall turnout by about 8.5 points and reduced turnout among the black population in the city by about 10.2 points. This effect becomes more pronounced as the distance between treated and control observations on either side of the municipal boundary increases, suggestive that COVID-19 itself reduced turnout separate from polling place consolidation. We conclude on the basis of these data that conversion to widespread absentee voting in the general election will result in disenfranchisement, which may be particularly marked among racial minorities.

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The Wisconsin 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 Wisconsin 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] On March 27, Evers called for every voter in the state to be sent an absentee ballot (Wise 2020). The Republican-controlled legislature refused this initiative. 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 June 9[3] which was overturned by the state supreme court.[4] The U.S. Supreme Court also 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. The *Milwaukee Journal Sentinel* observed the state was short some 7 thousand poll workers on March 31 (Marley and Beck 2020). The reduction in polling places was acute in Milwaukee. Five polling places remained open, compared with 182 in November of 2016.[6] Even as polling places were consolidated, a surge in absentee voting occurred. While 831 thousand ballots were cast by mail in the 2016 *general election*, more than 1.09 million mail ballots were returned in the primary this year.[7] Nonetheless, there is evidence for "leaked" absentee ballots (Stewart 2010): only 84.8% of mail ballots delivered to voters were ultimately returned in time to be counted. Overall, the consolidation of polling places reduced turnout in the election, and this effect was larger among the black population in the city.

[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/index.php/node/4414 and https://elections.wi.gov/node/6847.

Prior Literature

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, 68). Increasing the distance to polls in California in 2003 reduced the likelihood of voting in person by between 2 and 4 points. Absentee voting is more likely as the distance to the polls increases, but this effect is not large enough to offset the decrease from consolidation itself (Brady and Mcnulty 2011). 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 nine municipalities in Massachusetts and Minnesota found increasing the distance to the polls by about 0.25 miles reduces turnout by between 2 and 5 points, and that this effect is more pronounced among "high-minority, low-income, and low-car-availability areas" in the context of a non-presidential election (Cantoni 2020, 88).

The effect of distance to the polling place on voting is nonlinear (Dyck and Gimpel 2005, 541–42; Gimpel and Schuknecht 2003, 481–84). Dyck and Gimpel (2005) deploy observations ranging from .1 to 65 miles from the polling place. They report being one standard deviation from the polls (about 1.75 miles) reduces the likelihood of voting at the polls by 2.3 points, but makes absentee voting more likely by 0.9 points. A study of three counties in Maryland in the 2000 election finds moving 1 mile *closer* to the polls makes voting *more* likely by 0.45 points, while observing generally "[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, 481).

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. L2 also geocodes voters to their home addresses. The data indicates whether someone voted, but not vote mode. That we find negative treatment effects indicates that increased absentee voting did not *entirely* offset the effect of polling place consolidation.

The rest of the state did not see such drastic consolidation of polling places. Outside of Milwaukee, the state had 10.2% fewer polling places open in April 2020 than November 2016. Residents of Milwaukee were also likely subjected to a *second* treatment due to the severity of COVID-19. 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, 4.2, and 3.4 in Washington, Waukesha, and Racine Counties, respectively.[8] 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 and poll site closures.

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 a regression discontinuity in space that exploits the municipal boundary line to compare turnout for voters on either side of the "cutpoint" boundary (Keele, Titiunik, and Zubizarreta 2015). Research from New Orleans indicates that COVID-19 is clustered at the neighborhood level (van Holm, Wyczalkowski, and Dantzler 2020), and we therefore assume that voters who live in close proximity to one another but on either side of the boundary were exposed to similar experiences with COVID-19.

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

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. Voters very near one another but on opposite sides of the border might differ in meaningful ways. Keele, Titiunik, and Zubizarreta (2015) offers one way of dealing with 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" (page 228). We adopt this approach by genetically matching (Sekhon 2009, 2011) each registered voter in Milwaukee City to two voters who live outside the city but in Milwaukee, Racine, Waukesha, Washington, or Ozaukee County.[9] Although these counties include some urban areas such as the City of Racine, 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. After the matching procedure has been completed, we test how the estimated treatment effect changes as we vary the maximum distance allowed between treated and control voters.

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 are in fact reducing 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 within a buffer, they must also live near one another *within* that buffer. Just as narrowing the band allows us to home in on the effect of polling place closures, by expanding the maximum

[9]Each of these counties shares a border with Milwaukee County. 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 / ethnicity. Voters are also matched on their latitude and longitude to ensure physical proximity to one another.

allowed distance we can estimate the net effect of polling place consolidation plus Milwaukee City's increased exposure to COVID-19.

This set-up allows us to test two hypotheses:

Hypothesis A: When the maximum distance allowed between treated and control voters approaches zero, voters in Milwaukee will have turned out at a lower rate than their controls just over the municipal border. This effect will be considered the effect of consolidated polling places.[10]

Hypothesis B: As we allow the maximum allowed distance to increase, the negative treatment effect will grow larger. We expect that the worse effects of COVID-19 depressed turnout above-and-beyond the effects of consolidated polling places in Milwaukee City.

Results

We begin by presenting the results of the matching model, where each treated voter is matched with two control voters.[11] 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 2.5 miles. Matching is done with replacement, and ties are broken randomly.

[10]Insofar as some of the suburban municipalities closed some polling places, any treatment effect will be biased towards zero, thus making our estimates conservative.

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

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	27.0%	50.0%	27.0%	27.0%	100.00	100.00	100.00	100.00
% Voted in 2018 Primary	15.0%	27.0%	15.0%	15.0%	100.00	100.00	100.00	100.00
% Male	43.0%	45.0%	43.0%	43.0%	99.97	99.97	99.97	99.97
% Democrats	66.0%	25.0%	66.0%	66.0%	99.99	99.99	99.99	99.99
% Republican	9.0%	54.0%	9.0%	9.0%	100.00	100.00	100.00	100.00
Income	\$59,317	\$96,615	\$59,317	\$59,330	99.96	99.96	99.92	99.83
% with Collegiate Education	13.0%	32.0%	13.0%	13.0%	100.00	100.00	100.00	100.00
% White	46.0%	75.0%	46.0%	46.0%	100.00	100.00	100.00	100.00
% Black	31.0%	2.0%	31.0%	31.0%	100.00	100.00	100.00	100.00
% Latino	9.0%	4.0%	9.0%	9.0%	100.00	100.00	100.00	100.00
% Asian	2.0%	2.0%	2.0%	2.0%	100.00	100.00	100.00	100.00

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.[12] 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). To directly control for the effects of the pandemic Model 5 also includes the positive COVID-19 test rate in each voter's census tract as of two weeks after the election.[13] Robust standard errors are clustered by at the level of the match (Abadie and Spiess 2019).

[12] 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.

[13] Positive test rates are calculated as positive counts divided by the sum of positive and negative counts. Counts are as of April 21 because the data does not become available until after the election. The Department of Health Services replaces counts of less than 5 with "-999;" we re-code these at the mean "2." See: https://data.dhsgis.wi.gov/datasets/covid-19-historical-data-table.

Table 2: Turnout in 2020 Primary

	Turnout				
	(1)	(2)	(3)	(4)	(5)
Lives in Milwaukee	-0.086^{***}	-0.087^{***}	-0.085^{***}	-0.085^{***}	-0.085^{***}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Black		-0.038^{***}	-0.025^{***}	-0.030^{***}	-0.024^{***}
		(0.004)	(0.006)	(0.006)	(0.006)
Black × Lives in Milwaukee			-0.017^{**}	-0.016^{**}	-0.015^{**}
			(0.007)	(0.007)	(0.007)
Positive Test Rate					-0.027^{*}
					(0.014)
Constant	0.261^{***}	0.057^{***}	0.263^{***}	0.056^{***}	0.266^{***}
	(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	165,910	165,910	165,910	165,910	165,798
R^2	0.011	0.348	0.012	0.348	0.012
Adjusted R^2	0.011	0.348	0.012	0.348	0.012

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Robust standard errors (clustered by at level of match) in parentheses.
Models 2, 4, and 5 also include primary turnout in 2016 and 2018; partisan affiliation; gender; household income; collegiate education; and race / ethnicity.

Models 1 and 2 indicate that turnout was depressed by roughly 8.6 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, who saw turnout nearly 10.2 percentage points below that of their suburban matches. Model 5 indicates that turnout was perhaps lower where a higher share of COVID-19 tests were positive ($p < 0.1$). Although controlling for positive test rates slightly decreases the interaction effect, it leaves the estimate for non-Black voters unchanged. That the treatment effect is largely impervious to this control for COVID-19 prevalence indicates that the geographic restriction effectively accounts for exposure to the pandemic. The large treatment effect supports Hypothesis A.

We are also interested in whether the findings hold when we relax the geographic assumption by further restricting the maximum allowed distance and whether the size of the treatment effect grows as we include pairs who live further away from one another. Figure 1 re-estimates of Model 3 from Table 2 using different maximum distances between pairs.[14]

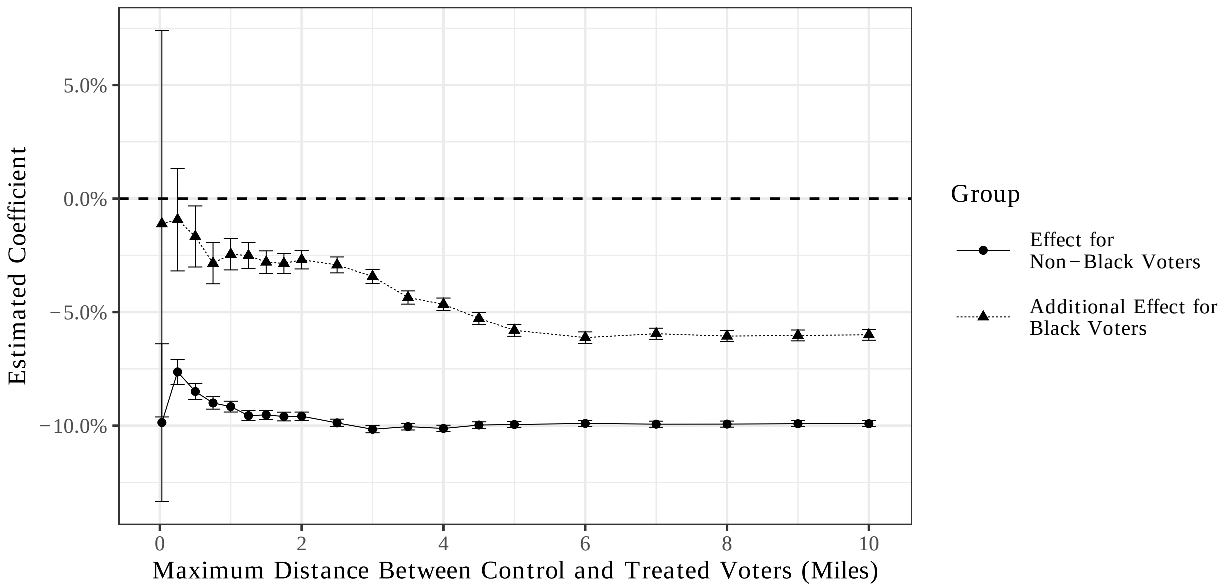

Figure 1: Estimated Depressive Effect of Living in Milwaukee, 2020 Primary

As we allow the maximum distance between treated and control voters to grow, so too does the apparent exposure to COVID-19. When treated and control voters live within a half-mile of one another, treated voters live in tracts where the positive test rate was 2.2 percentage points higher than their controls. However, when all matched pairs are retained (regardless of their distance from one another) treated voters' tracts had positive test rates 6.3 points higher than their controls.

As exposure to COVID-19 increases for treated voters relative to control voters, the overall depressive effect and interaction effect grow in magnitude. The difference in overall treatment effect between the half-mile and most lenient models is roughly 1.4 percentage points (the

[14]The interaction effect becomes non-significant at the narrowest bands, though it remains negative. This is probably due more to the fact that very few treated Black voters lived near the municipal border and had matches just on the other side. In an alternative model, where we match all Milwaukee voters within 0.125 of the boundary to suburban voters within the same distance of the boundary, we maintain excellent covariate matches and the interaction effect is significant at the 99% confidence level. The coefficient on Black × Lives in Milwaukee in this model is -2.6 points.

interaction effect grows by 4.3 percentage points). Thus COVID-19 likely reduced turnout relative to the suburbs (through mechanisms other than polling place consolidation) by 1.4 percentage points for non-Black voters, and as much as 5.7 percentage points for Black voters. This provides evidence to support Hypothesis B.

Discussion

Polling place closures have long been understood to reduce turnout among voters. This note makes clear that polling place closures reduced turnout in the 2020 primary in Milwaukee in the context of COVID-19 and unprecedented demand for absentee ballots. While the effect was perhaps smaller than it would have been absent robust messaging about mail voting, the 8.6 percentage point decrease is quite large (turnout among control voters was 26.1%). We also find this demobilizing effect better described as logarithmically approaching a limit rather than the nonlinear effect found in prior studies. Troublingly, the depressive effect was larger for Black than for non-Black voters, raising concerns about racial representation in the November 2020 elections as jurisdictions shift to greater access to mail ballots. Why polling place consolidation disproportionately depressed turnout among Black voters is unclear and should be the focus of future research.

This note answers just 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. The primary elections in Milwaukee, Wisconsin, make one thing clear, however: voters will not seamlessly transition to vote by mail, and polling place closures this fall will come at the expense of turnout — particularly the turnout of Black Americans.

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