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

Kevin Morris* Peter Miller†

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Abstract

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*Researcher, Brennan Center for Justice at NYU School of Law, 120 Broadway Ste 1750, New York, NY 10271 (kevin.morris@nyu.edu)

†Researcher, Brennan Center for Justice at NYU School of Law, 120 Broadway Ste 1750, New York, NY 10271 (peter.miller@nyu.edu)

Introduction

Intro

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Prior Literature

Disrupting one's routine with regard to voting – whether by relocating polling places or reducing the total number of polls – reduces turnout. Moving or reducing the number of polling places imposes new search and transportation costs on voters (Brady and Mcnulty 2011). A study of the 2001 election in Atlanta, Georgia reports a moved polling place reduces the likelihood of voting by about 5.5 points (Haspel and Knotts 2005). Comparing turnout between the 2000 and 2008 presidential elections revealed polling place consolidation at the county level reduced turnout by about nine-tenths of a point, even after controlling for the increased turnout in 2008 and changes in voting technology (Kropf and Kimball 2012, 68). In their study of the 2003 gubernatorial election in California, Brady and McNulty (2011) report that increasing the distance to polls reduces the likelihood of voting at the polls by between 2 and 4 points, as the distance to the polls increases from 0 to, at most, 1 mile. 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. Consolidating polling places in a New York State school district 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 one-quarter mile 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). In their study of Clark County, Nevada in the 2002 general election, 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' race, household income, and education. L2 also geocodes voters to their home addresses.

Although Milwaukee, Wisconsin reduced the total number of polling places from 182 in the general election of 2016 to just 5, the rest of the state did not see such drastic consolidation: outside of Milwaukee, the state had 10.2% fewer polling places open in April, 2020, than November of 2016. However, residents of Milwaukee were also likely subjected to a *second* treatment: COVID-19 was apparently a worse crisis in Milwaukee City. In Milwaukee County there had been roughly 2.5 deaths per 10,000 residents attributed to COVID-19 as of May 14, the first day for which the New York Times made county-level data available,[1] compared with 0.9 deaths per 10,000 residents in Racine County, and 1.1, 0.6, and 0.3 in Ozaukee, Waukesha, and Washington Counties, respectively. 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 the the pandemic and the consolidation of polling places.

To isolate the effect of polling place consolidation from the worse effects of COVID-19, we leverage electoral jurisdiction boundaries as a assignment to treatment mechanism (Kaplan and Yuan 2020; Cantoni 2020). Our primary design is therefore a regression discontinuity in space that exploits the municipal boundary line, comparing turnout for voters on either side of the "cutpoint" boundary (Keele, Titiunik, and Zubizarreta 2015). Here, we assume that voters who live in close proximity to one another but on either side of the administrative boundary were exposed to similar experiences with COVID-19; there is little reason to believe that COVID-19 operated differently on either side of an administrative boundary within a sufficiently narrow band. Though these voters lived in different municipalities they likely

[1]Data can be found here: https://github.com/nytimes/covid-19-data.

shopped at the same grocery stores and ate at the same restaurants.

The traditional regression discontinuity framework, however, relies on the assumption that individuals cannot "select" around the cutpoint; in other words, that within a narrow window individuals on either side of the cutpoint are identical. That is perhaps too strong of an assumption. 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 in the context of a regression discontinuity in space: "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). This is the approach we take: we genetically match (Sekhon 2009, 499, 2011) each registered voter in Milwaukee City (henceforth referred to as "treated" voters) to two voters who live outside the city but in Milwaukee, Racine, Waukesha, Washington, or Ozaukee County.[2] 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. Just as narrowing the band allows us to home in on the effect of polling place closures, by expanding the maximum allowed distance we can estimate the net effect of polling place consolidation *plus* the worse effects of COVID-19.

This set up allows us to test two hypotheses:

[2]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.

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.[3]

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

Our matching procedure is highly successful: we achieve 100 percent improvement in the mean difference between treated and control voters along 9 of our 13 covariates. Latitude improves by 96.3%, longitude by 98.7%, and household income and share male improve by more than 99.95%. The average pair of treated and control voters live 2.5 miles apart.

Table 1 presents the results of ordinary least squares regressions testing the treatment effect. In Table 1 we require treated and control voters to live within 0.5 miles of one another.[4] 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; research indicates that minority voters are both less likely to use vote-by-mail options and more likely to have their ballots rejecting, implying that the closed polling places might have decreased their turnout by even more than others'. Models 1 and 3 include just the treatment variable (and, in Model 3, the interaction terms) while Models 2 and 4 add in the variables on which the matching was performed (but without latitude and longitude). Robust standard errors are clustered at the level of the match (Abadie and Spiess 2019).

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

[4]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.

Table 1: Turnout in 2020 Primary

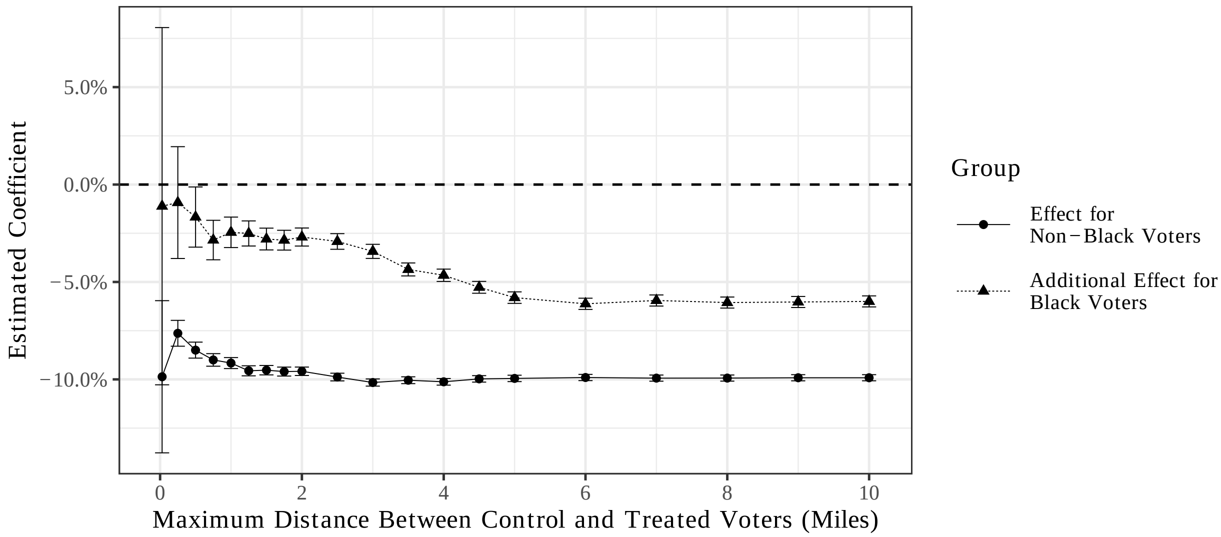
	Turnout			
	(1)	(2)	(3)	(4)
Lives in MKE	-0.086^{***}	-0.087^{***}	-0.085^{***}	-0.085^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Black		-0.038^{***}	-0.025^{***}	-0.030^{***}
		(0.004)	(0.006)	(0.006)
Black × Lives in MKE			-0.017^{**}	-0.016^{**}
			(0.008)	(0.008)
Constant	0.261^{***}	0.057^{***}	0.263^{***}	0.056^{***}
	(0.001)	(0.004)	(0.001)	(0.001)
Includes Other Matched Covariates		X		X
Includes County Fixed Effects	X	X	X	X
Observations	165,910	165,910	165,910	165,910
R^2	0.011	0.348	0.012	0.348
Adjusted R^2	0.011	0.348	0.012	0.348

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Robust standard errors (clustered by at level of match) in parentheses.

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. Because of the tight geographic restriction imposed here, we argue that this represents the causal effect of polling place consolidation. This is a large treatment effect, and supports our 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 also in 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 1 using different maximum distances between treated and control voters.[5]

[5]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 our most conservative pool, we have just 82 treated Black voters,



Notes: 95% confidence bars shown.
"Effect for Non−Black Voters" refers to the variable "Lives in MKE,"
while "Additional Effect for Black Voters" refers to "Black × Lives in MKE."
The overall effect for Black voters is therefore the sum of both estimates.

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

As we allow the maximum distance between treated and control voters to grow, the overall treatment effect and interaction effect grow in magnitude; turnout in Milwaukee was apparently depressed by mechanisms above-and-beyond those explained by polling place consolidation and the voter demographics for which we controlled in the matching procedure. As discussed above, Milwaukee was hard-hit by the COVID-19 crisis; this analysis demonstrates that COVID-19 likely directly depressed turnout in Milwaukee City. The difference in overall treatment effect between the most half-mile and most lenient models is roughly 3.2 percentage points (the interaction effect grows by 4.4 percentage points). Thus COVID-19 likely reduced turnout relative to the suburbs (through mechanisms other than polling place consolidation) by more than 3 percentage points for non-Black voters, and as much as 7.6 percentage points for Black voters. This provides evidence to support Hypothesis B.

while Black voters make up just 5% of the treated voters in the second-most-conservative model. 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 percent confidence level. The coefficient on Black × Lives in MKE in this model is -2.6 points.

Discussion

As discussed in the opening pages of this manuscript, polling place closures have long been understood to reduce turnout among voters. This research, of course, did not examine the effect of closures in the context of a global pandemic. It is possible that, given the public discussion about voting in a pandemic, voters might have shifted seamlessly to voting by mail. The Wisconsin Elections Commission ("April 7, 2020 Absentee Voting Report" 2020), for instance, reported that the number of mail-in ballots jumped from 170,614 in the 2016 presidential primary to 964,433 in the 2020 primary. It was unclear whether polling place reductions would still depress turnout in the context of COVID-19.

This manuscript makes clear that polling place closures reduced turnout in the 2020 primary in Milwaukee even in the context of COVID-19. While the effect was perhaps smaller than it would have been absent robust messaging about mail voting, the 8.6 percentage point decrease in a primary contest is still quite large. Troublingly, the depressive effect was apparently larger for Black than for non-Black voters, raising concerns about racial representation in the November 2020 elections as jurisdictions shift to vote-by-mail options. Whether this is because racial minorities are perhaps less likely to request mail ballots this year (Morris 2020), because their mail ballots are rejected at higher rates (Shino, Suttmann-Lea, and Smith 2020), or some combination of these factors is unclear.

This manuscript answers one small question related to the effect of COVID-19 on this year's elections: given the reality of the pandemic, how do polling place closures affect turnout? Turnout in 2020 for our matched controls was virtually identical to 2016 turnout, implying that COVID-19 had little impact outside of polling place closures, though this effect demands to be studied directly, as does whether the overall effect varies by race. The primary elections in Milwaukee, Wisconsin, do make one thing clear: 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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