Authority After the Tempest: Hurricane Michael and the 2018 Elections

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Abstract

Hurricane Michael made landfall in the Florida panhandle 27 days before the 2018 elections. In the aftermath, the governor of Florida issued Executive Order 18-283 granting election officials in 8 impacted counties the autonomy to loosen a variety of voting laws related to early in-person voting, voting by mail ballots, and the number and location of polling places to ensure the orderly conduct of the election. To test the efficacy of the order we deploy a novel research design to separate the effects of the hurricane on turnout from the administrative effects of actions taken by election officials. By leveraging cross-jurisdiction variation in a double-matched, triple-differences model, we show that the executive order was not successful at eliminating declining turnout. As administrators loosen mail-voting restrictions in advance of this fall, they must couple these eased restrictions with strong public education campaigns about how voters can take advantage of them.

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Introduction

As the 2018 elections approached, an unanticipated – but not unprecedented – shape appeared on the Florida horizon: the Category 5 Hurricane Michael.[1] The hurricane made landfall on October 10, 27 days before the election, and would ultimately cause 16 deaths and $25 billion dollars in damage.[2] Would-be voters in the election were now faced with myriad disruptions to their daily lives; the direct effects of the weather, therefore, likely reduced turnout substantially as the recovery from the hurricane progressed. As professor emeritus Robert Montjoy told NPR in the aftermath of the storm, "Whether casting a ballot becomes a higher priority than cleaning out the basement, visiting someone in the hospital, or all the other demands…You certainly expect a lower turnout for those reasons" (Parks 2018).

The storm also affected the administration of the election itself, as polling places were destroyed and potential mail voters found themselves temporarily residing at addresses other than those at which they were registered. The governor of Florida issued Executive Order 18-283[3] as a means to counteract the widespread effects of the hurricane on October 18. Executive Order 18-283 sought to offset the administrative barriers to voting by allowing election administrators in 8 counties in Florida affected by the hurricane to flexibly respond to the damage wrought by the storm. Specifically, Executive Order 18-283 allowed administrators to add early voting locations; begin early voting 15 days before the general election (4 days after the Executive Order was issued), and continue until the day of the election; to accept vote-by-mail requests to addresses other than a voter's registered address; to send vote-by-mail ballots by forwardable mail; to deliver vote-by-mail ballots to electors or electors' immediate family members on election day without an affidavit; to relocate or consolidate polling places; and required poll watchers to be registered by the second Friday

[1]The category of the hurricane refers to the maximum sustained windspeed, according to the Saffir-Simpson hurricane wind scale. A Category 5 hurricane sustains winds greater than 157 miles per hour.

[2]See https://www.nhc.noaa.gov/data/tcr/AL142018_Michael.pdf.

[3]See https://www.flgov.com/wp-content/uploads/2018/10/SLT-BIZHUB18101809500.pdf.

before the general election.

This paper sets out to answer a number of questions: what was the total depressive effect of the hurricane? Did Executive Order 18-283 effectively offset the depressive administrative effects? More specifically, did easing mail-balloting and early voting rules reduce the impact of closed polling places? We propose a novel research design to investigate these interrelated questions – what we are calling a double-matched, triple-difference model – and then demonstrate that the hurricane significantly reduced turnout and that responses to the hurricane by local election officials were unable to overcome the devastation of the hurricane. We conclude with some thoughts about how the instance of Hurricane Michael can inform the conduct of elections under other natural disasters like the COVID-19 pandemic (James and Alihodzic 2020).

Literature Review

This study lies at the intersection of three components of the broader turnout literature: the effects of inclement and severe weather, the capacity for convenience voting reforms to increase participation in elections, and the ability of local election officials to increase turnout by placing polls where voters are able to access them. Our general observation is that while the effects of weather are often negative with regard to participation in elections, the leverage for voting reforms and local officials to counterbalance those depressive effects are limited.

Weather Effects

Variations in weather on election day are generally thought be exogenous to elections (Hansford and Gomez 2010, 269), but also have a clear effect on turnout. Rallings, Thrasher, and Borisyuk (2003) observes "[v]ariable weather patterns are also likely to affect turnout since these too would be regarded as a variable cost in the act of voting" (78). This question has

been frequently examined in the United States.

While the studies produce divergent point estimates, the consensus is that turnout is lower in the presence of rain on election day (Cooperman 2017; Fujiwara, Meng, and Vogl 2016; Hansford and Gomez 2010; Fraga and Hersh 2010; Gomez, Hansford, and Krause 2007; Shachar and Nalebuff 1999; Knack 1994; Merrifield 1993). The effect of rain on turnout, however, is strongest among voters with less of a sense that voting is a civic duty and altogether absent among voters with a strong sense of civic duty (Knack 1994). Fraga and Hersh (2010) find the decrease in turnout is only found in noncompetitive counties; a competitive race is sufficient to induce voters to cast a ballot in the rain. Gatrell and Bierly (2002) find the effect of rain is most pronounced in general elections (where more peripheral voters are brought into the electorate) than primary elections (where the electorate tends to be more partisan). In a comparative context, a study in Sweden (Persson, Sundell, and Öhrvall 2014) found no significant turnout effects of rain on election day, in part due to Sweden's permissive early voting regime. Evidence from Japan, where Typhoon Lan (the equivalent of a Category 4 hurricane) would make landfall the day after the 2017 parliamentary elections, shows voters behaved dynamically as the typhoon approached: voters were more likely to vote early, or earlier on election day, as rainfall increased at the prefecture level (Kitamura and Matsubayashi 2020).

Rain on election day may not be relevant to the considerably more severe damage that follows after a hurricane. Previous natural disasters, such as Hurricane Sandy (2012) in Connecticut, New Jersey and New York and Hurricane Katrina (2005) in New Orleans, may give a better set of boundary conditions on our expectations of how severe, as opposed to inclement, weather may alter electoral behavior. Studies of these events found lower turnout within effected geographic areas (Lasala-Blanco, Shapiro, and Rivera-Burgos 2017; Stein 2015; Debbage et al. 2014; Sinclair, Hall, and Alvarez 2011). Stein (2015) observes, however, that the interactive effect of residing in a county covered by the disaster and the provision of early in-person voting *increased* turnout. Sinclair, Hall, and Alvarez (2011) also

find non-linear effects, where people who experienced considerable flooding were *more* likely to vote in the subsequent election. One factor to bear in mind, however, is the timing of these storms relative to an election. Hurricane Sandy (a Category 3 hurricane) made landfall eight days before the 2012 elections; Katrina (Category 5) made landfall more than a year before the 2006 elections. Hurricane Michael (Category 5) made landfall 27 days before the 2018 elections. We expect, therefore, that the effects of Michael with regard to turnout may be closer in magnitude to the effects observed in the aftermath of Sandy rather than Katrina, despite Michael and Katrina being of comparable windspeed upon landfall.

Early voting and turnout

Florida, like Sweden, has a permissive convenience voting regime. Registered voters can present themselves at an early in-person polling place to cast a ballot in advance of election day. Registered voters can also request a mail ballot on an unrestricted basis and return that ballot before the election. Can these reforms increase turnout? This question is not yet resolved in the literature; we are left unsatisfyingly answering the question about turnout effects of convenience voting reforms with both " 'no' and 'yes' " (Bergman 2015).

There is evidence for a variety of effects when looking at turnout effects of convenience voting reforms. Early in-person voting increased turnout in the 1994 elections among Tennessee counties (Ricardson and Neeley 1996). A study from Ohio found an additional day of early in-person voting increased turnout (Kaplan and Yuan 2020). That being said, early voting as a broad reform (i.e. combining mail voting) *decreased* turnout in the 2004 and 2008 elections (Burden et al. 2014). The literature on turnout effects of elections conducted entirely via the mail, however, is similarly mixed. Studies of presidential elections find absentee voting increases turnout (Leighley and Nagler 2014), though that effect is not found in studies that include midterm elections (Fitzgerald 2005). The picture is no clearer when we look at elections conducted entirely by mail. That reform increases turnout in

Washington (Henrickson and Johnson 2019; Gerber, Huber, and Hill 2013), decreases turnout in California (Elul, Freeder, and Grumbach 2017; Bergman and Yates 2011; Kousser and Mullin 2007), and has no significant effect in Oregon (Gronke and Miller 2012). A recent, national study finds a small boost to turnout following from the adoption of Oregon-style voting by mail (Barber and Holbein 2020).

Polling Place Consolidation

One element of election administration that local authorities can control is the location of polling places. The executive order issued after Hurricane Michael empowered local election officials in the eight affected counties to move or consolidate polling places at will. According to public records we obtained, these counties collectively saw just 62 of the anticipated 127 polling places opened. Relocating or reducing the number of polling places in turn reduces turnout by imposing new search and transportation costs on voters (Brady and McNulty 2011). A moved polling place reduces turnout in a variety of electoral contexts (Cantoni 2020), including local elections (McNulty, Dowling, and Ariotti 2009; Haspel and Knotts 2005) as well as national contests (Kropf and Kimball 2012). 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; Dyck and Gimpel 2005).

The effect of distance to the polling place on voting is nonlinear (Dyck and Gimpel 2005, 541–42; Gimpel and Schuknecht 2003, 481–84). 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).

Grounding our analyses of the effects of Hurricane Michael gives us some expectations as to how the hurricane will alter voting behavior. We expect the direct, weather-related effects of

the hurricane to reduce turnout. The administrative effects will push in operate directions. On the one hand, consolidated polling places likely imposed costs on voters, reducing turnout above-and-beyond on the individual effects. On the other hand, the loosened restrictions on mail voting and relief valve offered by early voting may recover *some but not all* of these displaced voters. This is, of course, not to claim that the local officials in the path of the hurricane sought to reduce turnout. Rather, the work of administering an election — even under the best of circumstances — is difficult. The extraordinary impact of a Category 5 hurricane is perhaps simply too much for election administrators to incorporate into their efforts to conduct a secure and inclusive election.

Research Design and Expectations

We expect that Hurricane Michael depressed turnout in the 2018 midterm election via two causal mechanisms: individual-level effects, and administrative effects.

Estimating the Net Effects of the Hurricane

We begin by testing the net effect of each of these treatments on individual-level turnout. Our central identification strategy involves the use of difference-in-differences models. We use voter-file data from L2 Political to estimate individual-level turnout and to control for individual-level characteristics. L2 uses models to predict individual race / ethnicity and voters' sex but these characteristics are available in self-reported form in the raw voter-file available from the state; as such, we pull sex and race / ethnicity from the publicly available voter file. The L2 data is based on the February 8, 2019, version of the raw voter file, the same file from which we pull race / ethnicity and sex.

By comparing historical and 2018 turnout for voters in the counties hit by the storm to historical and 2018 turnout of voters elsewhere in the state, we can estimate the effect

of the storm on turnout. To ensure a high-quality difference-in-differences specification, we do not include all untreated voters in our control group; rather, we genetically match (Sekhon 2011) each treated voter with five untreated voters along a battery of individual- and neighborhood-level characteristics. Untreated voters who do not serve as matches are excluded from our models. Although it may seem counterintuitive to exclude data from our models, this matching procedure substantially improves the parallel trends assumptions necessary for a rigorous difference-in-differences analysis (Sekhon 2009, 496).

This design allows us to test our first hypothesis:

Hypothesis 1: Turnout among voters in the eight treated counties was depressed in the 2018 election relative to voters in untreated counties. This represents the net effect of both the individual and administrative level treatments.

Decomposing Individual and Administrative Effects

To estimate the administrative effect on turnout, we must control for the individual-level effects of the storm. To do so, we leverage the somewhat arbitrary borders of counties in the Florida Panhandle, an approach similar to that adopted in a different context by Walker, Herron, and Smith (2019). There is no reason to believe that the effects of a hurricane would change dramatically along county borders. We assume, therefore, that voters who lived nearby one another, but on either side of a county border, faced the same weather issues during the 2018 election. Within a narrow buffer around the county border, we can conceive of a voter's county as effectively randomly assigned. Any observed turnout differential, therefore, is attributable to the county in which they happen to live. Our treated voters lived in counties where polling places could be closed or moved shortly before the election, and also where some restrictions were eased, which could have altered turnout.

Of course, self-selection around a geographic boundary is entirely possible; as such, conceiving of the administrative boundary as a quasi-random assignment is perhaps too strong of

an assumption. Treated and control voters, despite living very near to one another, might differ in meaningful ways. To address this potential problem, we adopt the technique developed by Keele, Titiunik, and Zubizarreta (2015) by also matching voters on either side of the boundary. This allows us to directly ensure that the treated and control voters are nearly identical in terms of observable characteristics. We further allow for the possibility that treated and control voters have different baseline turnout propensities by incorporating historical turnout data.

By comparing the turnout of these treated and control voters, we can identify the administrative effect of the hurricane on turnout for the treated voters living within the buffer around the border. By further comparing the turnout of these control voters to (matched) voters elsewhere in the state, we can also estimate the individual effects of the storm, again just for the voters who live near the administrative boundary. We call this a double-matched triple-differences (or difference-in-difference-in-differences) specification. We lay out the specific steps below.

We begin by constructing our set of treated voters. These treated voters include all registered voters who live in a treated county and within 2.5 miles of a bordering, untreated county (See Figure 1). Each treated voter is then matched to one voter who lives in an untreated county, but within 2.5 miles of a treated county. These matches are considered primary control voters.

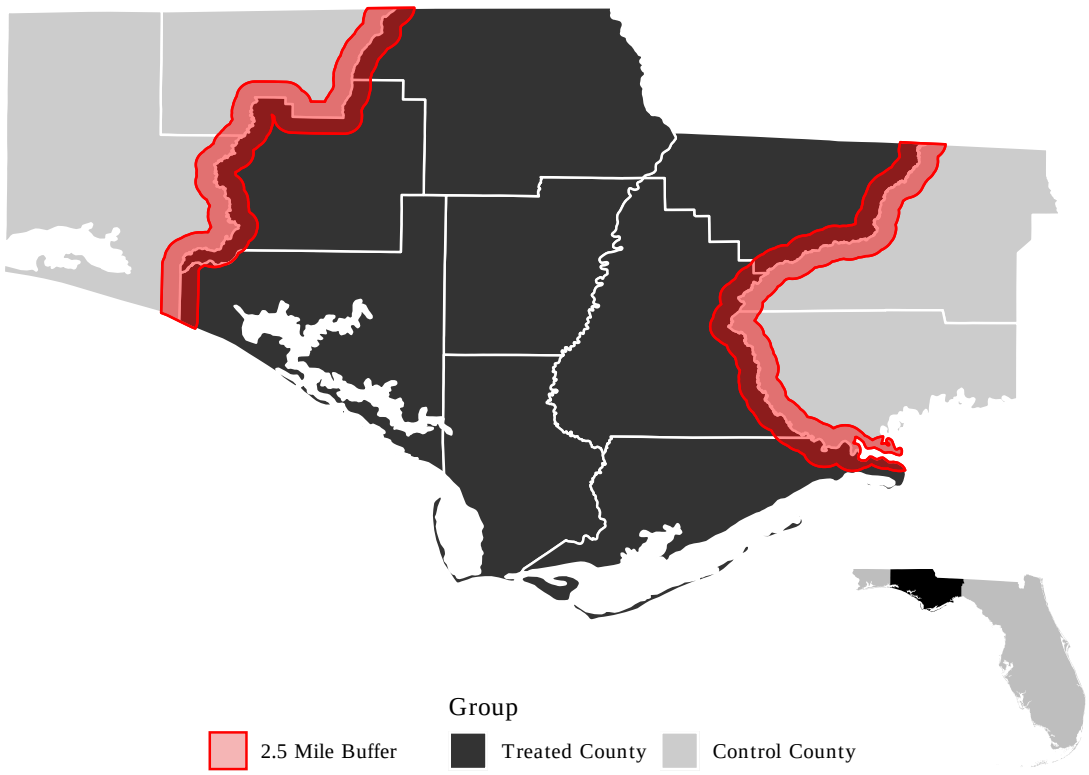

Figure 1: Treated and Control Counties with 2.5 Mile Buffer

Each treated and primary control voter is subsequently matched to five voters elsewhere in the state — that is to say, voters who are neither in the treated counties nor in the counties directly surrounding the treated counties. This exercise is the second match, and the matches are our "secondary control voters." These voters were subject to neither individual-level nor administrative-level treatments.

At this point, we have three distinct groups of voters:

- Treated voters. These voters were subject to individual- and administrative-level effects from Hurricane Michael
- Primary control voters. These voters were subject to individual, but not administrative, effects from Hurricane Michael.

- Secondary control voters. These voters were subject to neither individual nor administrative effects.

Having constructed our pool of voters, we run a triple-differences model. This triple-differences model is, in effect, two simultaneous difference-in-differences models. The model estimates whether 2018 was associated with depressed turnout for our primary control voters vis-à-vis their controls. Because these primary control voters lived in counties not covered by the executive order, we assume that they faced no administrative effects from the storm. Any observed difference between these groups is therefore the individual-level effect of the storm for primary control voters — and, by extension, the treated voters.

The model also estimates turnout differences between treated and primary control voters. Because we assume these closely-located voters faced identical individual-level effects, any difference between treated and primary control voters is the administrative effect on turnout of living in a treated county.

The double-matched triple-differences model allows us to test our second and third hypotheses:

Hypothesis 2: We expect that the hurricane had negative individual-level effects for voters who lived just outside of treated counties.

Hypothesis 3: We expect that the administrative effects of Hurricane Michael were negative, notwithstanding Executive Order 18-283.

Vote Mode

After estimating the double-matched triple-differences model, we turn to vote-mode within the treated counties. We submitted public records requests to each of the eight counties covered by the executive order requesting the planned and actual location of each polling place. Two counties — Calhoun and Liberty — were able to use all of their expected polling

places. Others were either forced to relocate or consolidate polling places. Most notably, Bay County went from an expected 44 polling places to just six.

To test whether the executive order shifted vote mode from in-person to mail voting in the treated counties, we begin by calculating how far each voter lived from the closest planned polling place, and how far she lived from the closest polling place that was actually open on election day. Using the registered voter file, we can tell not only *whether* a voter participated, but also *how* they participated. Using a multinomial logistic regression, we test whether the difference between the planned and actual distance-to-polling-place were associated with vote-mode in 2018. This specification allows us to test our final hypothesis:

Hypothesis 4: As the difference between the actual and planned distance to the closest polling place increased for voters, they were more likely to vote absentee and to abstain from voting, relative to past behavior, all else being held equal.

Results

Overall Turnout Effects

We begin by matching each registered voter in the eight treated counties to five untreated voters elsewhere in the state using a genetic matching algorithm (Sekhon 2011).[4] The individual-level characteristics come directly from the L2 and the registered voter file. The two neighborhood-level characteristics included — median income and share of the population with some collegiate education — are estimated at the block group level, and come from the ACS 5-year estimates ending with 2018. Ties are not broken, which means that some treated voters are assigned more than five control voters; the weights used in the regressions below are adjusted accordingly.

[4]Due to computing constraints, the matching weights were constructed using a one percent random sample stratified by treatment status.

Although the treated counties were at the center of the storm, nearby counties might have also been negatively impacted by the storm. Therefore, voters who live in the counties that border the treated counties are excluded as potential controls. These include Walton, Holmes, Wakulla, and Leon Counties.

Table 1 demonstrates the results of this matching procedure. As Table 1 makes clear, voters in the affected counties were considerably more likely to be white and identify as Republicans, and live in lower-income neighborhoods, than voters in the rest of the state. The post-match control group, however, looks substantially similar to the treated voters.

Table 1: Balance Table for Statewide Matching

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
%White	76.5%	62.3%	76.5%	76.5%	100.00	100.00	100.00	100.00
% Black	17.1%	13.1%	17.1%	17.1%	100.00	100.00	100.00	100.00
% Latino	2.1%	17.4%	2.1%	2.1%	100.00	100.00	100.00	100.00
% Asian	1.0%	2.0%	1.0%	1.0%	100.00	100.00	100.00	100.00
% Female	52.5%	52.4%	52.5%	52.5%	100.00	100.00	100.00	100.00
% Male	45.8%	44.9%	45.8%	45.8%	100.00	100.00	100.00	100.00
Age	52.2	52.5	52.2	52.2	98.54	96.68	97.36	96.17
% Democrat	39.2%	37.1%	39.2%	39.2%	100.00	100.00	100.00	100.00
% Republican	43.6%	35.0%	43.6%	43.6%	100.00	100.00	100.00	100.00
% with Some College	69.0%	75.1%	69.0%	69.0%	99.77	99.00	98.05	88.66
Median Income	$50,643	$62,941	$50,643	$50,654	99.91	98.11	96.89	86.56

Figure 2 plots the turnout in the past few elections for our treated and control voters. As Figure 2 makes clear, treated voters consistently turned out at higher rates than control voters from 2010 – 2016. In 2018, however, this relationship was inverted as turnout among treated voters plummeted from its 2016 level. Although turnout among all voters was higher in 2018 than in 2014, turnout rose by substantially less for the treated voters.

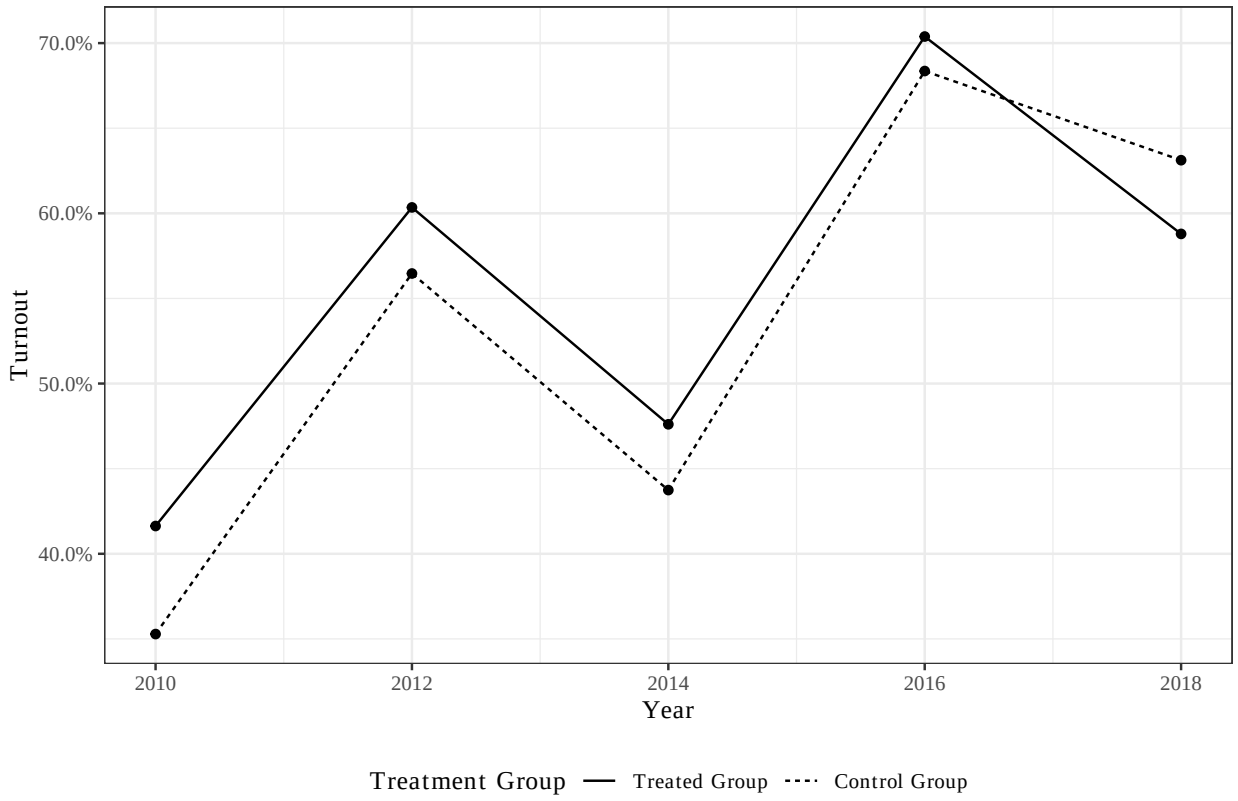

Figure 2: General Election Turnout for Treated and Control Voters, 2010 – 2018

Table 2 formalizes Figure 2 into a differences-in-differences regression. We employ an ordinary least squares specification. The dependent variable takes the value 1 if a voter cast a ballot in a given year, and 0 if she did not. In each model, *Treated × 2018* estimates the casual (net) effect of Hurricane Michael on turnout for treated voters. Model 2 also includes the characteristics on which the voters were matched. Model 3, finally, adds a measure for congressional district competitiveness. Because this variable is "downstream" of treatment — that is to say, the effect of the hurricane could have impacted the competitiveness of certain races — it is not included in the first two models. It should be noted that each of the treated voters lived in uncontested congressional districts. Robust standard errors are clustered at the level of the match (Abadie and Spiess 2019).

Table 2: Turnout, 2010 — 2018

	Turnout		
	(1)	(2)	(3)
Treated	0.030***	0.030***	0.030***
	(0.001)	(0.001)	(0.001)
2018	0.236***	0.236***	0.236***
	(0.0005)	(0.0005)	(0.0005)
Midterm	−0.229***	−0.229***	−0.229***
	(0.0003)	(0.0003)	(0.0003)
Treated × Midterm	0.022***	0.022***	0.022***
	(0.001)	(0.001)	(0.001)
Treated × 2018	−0.094***	−0.094***	−0.094***
	(0.001)	(0.001)	(0.001)
Constant	0.624***	−0.192***	−0.202***
	(0.001)	(0.004)	(0.004)
Includes Other Matched Covariates		X	X
Includes control for CD competitiveness			X
Observations	7,893,265	7,893,265	7,893,265
R^2	0.044	0.191	0.191
Adjusted R^2	0.044	0.191	0.191

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

The coefficient on *Treated × 2018* in Table 2 indicates that Hurricane Michael had a substantial depressive effect in 2018 among the treated voters. Each model estimates that the overall effect — including individual and administrative effects — was -9.4 percentage points. Multiplied across the nearly 200 thousand registered voters in the treated counties indicates that some 19 thousand ballots went uncast due to the hurricane, a major effect in a year when a statewide senate race was decided by 10,033 votes.

Identifying Administrative Effects

As discussed above, our primary strategy for isolating the administrative effects of the hurricane on turnout involves leveraging random assignment around county borders in the Florida panhandle in a double-matched triple-differences specification. Each voter inside the buffer in a treated county is matched with one voter in the buffer in an untreated county, once again using a genetic matching algorithm (Sekhon 2011). These matches serve as our primary control voters. Ties are not broken, which means that some treated voters are assigned multiple primary control voters; the weights used in the regressions are adjusted accordingly.

In some cases, voters on either side of the border are in different congressional districts. This would pose a problem if these races were contested thanks to the potentially mobilizing effects of house races, but the entire buffer falls in uncontested congressional districts. This means that treated and untreated voters are not facing differential mobilization from congressional races. As before, we match on individual- and neighborhood-level characteristics. Importantly, we match treated and untreated voters using their latitude and longitude to ensure that matches live in close proximity to one another. Table 3 presents the results of this matching exercise.

Table 3: Balance Table for Border Buffer Matching

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
%White	71.2%	74.9%	71.2%	71.2%	100.00	100.00	100.00	100.00
% Black	24.8%	18.2%	24.8%	24.7%	98.65	98.66	98.66	98.66
% Latino	1.1%	2.3%	1.1%	1.1%	100.00	100.00	100.00	100.00
% Asian	0.3%	0.8%	0.3%	0.3%	100.00	100.00	100.00	100.00
% Female	53.2%	53.0%	53.2%	53.2%	100.00	100.00	100.00	100.00
% Male	45.4%	45.1%	45.4%	45.4%	100.00	100.00	100.00	100.00
Age	53.1	50.2	53.1	53.1	98.58	87.44	86.74	81.79
% Democrat	47.2%	44.5%	47.2%	47.2%	97.59	97.60	97.60	97.60
% Republican	39.1%	37.7%	39.1%	39.1%	100.00	100.00	100.00	100.00
% with Some College	62.7%	70.0%	62.7%	63.0%	95.63	63.99	62.43	59.15
Median Income	$45,243	$51,335	$45,243	$46,004	87.50	-12.08	43.03	46.56

The match procedure improves the balance between treated and primary control voters sub-

stantially for each of the characteristics listed in Table 3. Although latitudes and longitudes are not displayed in the table, the average treated voter lives 9.5 miles from her primary control voter. Considering that Hurricane Michael was approximately 350 miles wide (Thomas and Sangalang 2018) we consider this average distance sufficiently small to assume that, on average, treated and control voters were faced with identical individual-level effects.

Once our set of treated and primary control voters[5] has been identified, each of these voters is matched with five other voters that lived in neither the treated nor the immediately surrounding counties. This matching procedure follows the same steps detailed in the Overall Turnout Effects section of this paper. Table 4 presents the results of the secondary match. We improve along all characteristics.

Table 4: Balance Table for Secondary Match

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
%White	71.7%	62.3%	71.7%	71.7%	100.00	100.00	100.00	100.00
% Black	23.3%	13.1%	23.3%	23.3%	100.00	100.00	100.00	100.00
% Latino	1.4%	17.4%	1.4%	1.4%	100.00	100.00	100.00	100.00
% Asian	0.5%	2.0%	0.5%	0.5%	100.00	100.00	100.00	100.00
% Female	52.7%	52.4%	52.7%	52.7%	100.00	100.00	100.00	100.00
% Male	45.6%	44.9%	45.6%	45.6%	100.00	100.00	100.00	100.00
Age	52.9	52.5	52.9	52.9	98.12	82.32	87.10	87.22
% Democrat	46.4%	37.1%	46.4%	46.4%	100.00	100.00	100.00	100.00
% Republican	38.7%	35.0%	38.7%	38.7%	100.00	100.00	100.00	100.00
% with Some College	62.9%	75.1%	62.9%	62.9%	99.98	99.30	97.16	82.78
Median Income	$45,913	$62,941	$45,913	$45,928	99.91	99.03	96.22	80.63

In Figure 3 we present the plotted turnout trends from the treatment, primary control, and secondary control groups returned by the matching exercise. Figure 3 makes clear that the turnout gap in these groups was relatively constant prior to 2018 (after accounting for differences in midterm and presidential elections), but that the relative turnout of the treatment group — whose turnout in midterm elections was historically higher than the

[5]For ease of notation, the combined set of treated and primary control voters will henceforth be referred to as "Panhandle voters," while "treated" voters will distinguish Panhandle voters in treated counties from Panhandle voters in other counties. The use of "Panhandle" is a slight misnomer: it excludes Escambia, Santa Rosa, and Okaloosa Counties which are certainly part of the Florida Panhandle, as well as Jefferson County and others to its east which are sometimes considered part of the panhandle.

other groups — noticeably dipped in 2018.

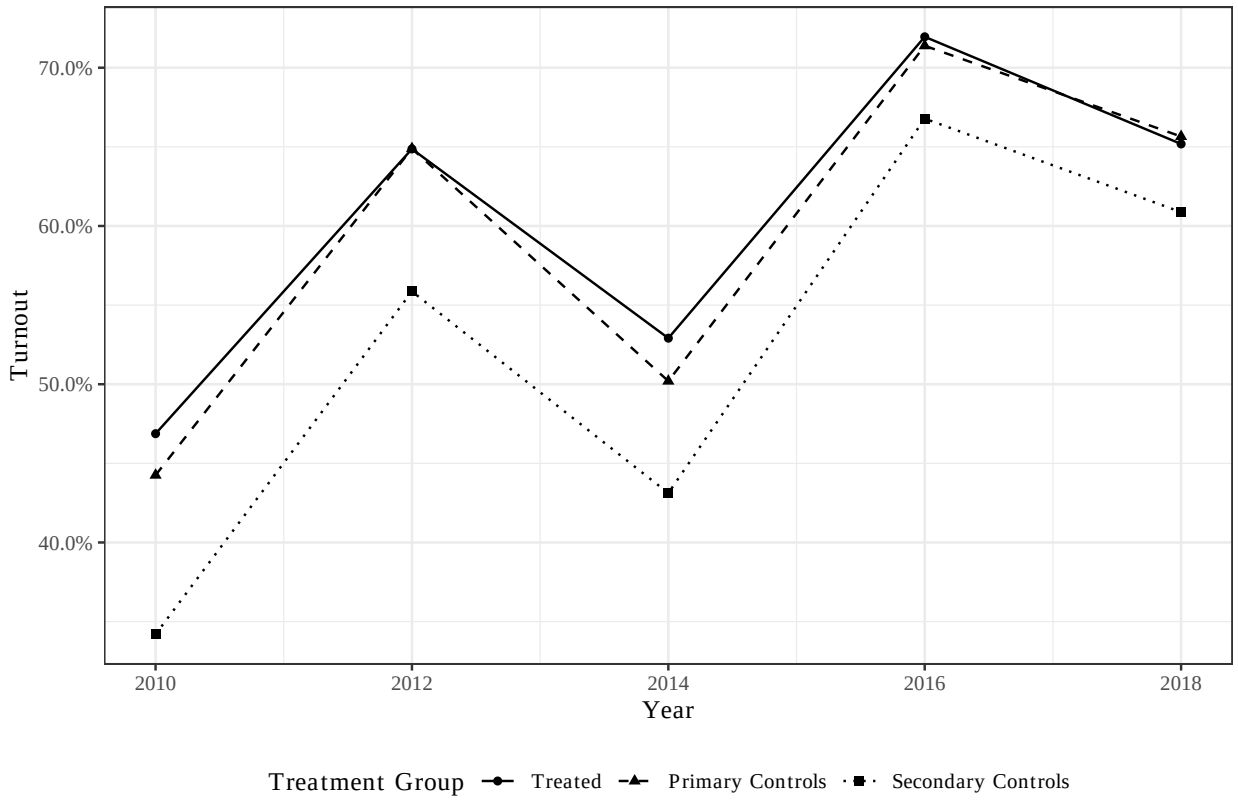

Figure 3: General Election Turnout for Treated, Primary Control, and Secondary Control Voters, 2010 – 2018

Disentangling the administrative and individual effects of the storm requires the estimation of the triple-differences model. This model is estimated by Equation (1).

$$
\begin{aligned}
v_{it} = \beta_0 + \beta_1 Panhandle_i + \beta_2 2018_t + \beta_3 Panhandle_i \times 2018_t + \\
\beta_4 Treated_i + \beta_5 Treated_i \times 2018_t + \beta_6 SecondaryControlGroup1_i + \\
\beta_7 Midterm_t + \beta_8 Panhandle_i \times Midterm_t + \beta_9 Treated_i \times Midterm_t + \\
\delta Z_i + \mathcal{E}_{it}.
\end{aligned} \tag{1}
$$

Individual i's turnout (v) in year t is a function of the year and their location. In the equation, $\beta_1 Panhandle_i$ measures the historical difference between voters in the panhandle and the rest of the state. $\beta_2 2018_t$ measures the statewide change in turnout in 2018 from the baseline, while $\beta_3 Panhandle_i \times 2018_t$ tests whether turnout changed differently in 2018 in the panhandle than it did elsewhere. $\beta_3 Panhandle_i \times 2018_t$, therefore, is our estimation of the individual-level, or weather related, effect of the hurricane. $\beta_4 Treated_i$ measures the historical difference between treated and primary control voters, and $\beta_5 Treated_i \times 2018_t$ tests whether the causal effect of the storm was different for treated voters than for their primary controls. This, then, is the estimated administrative effect of living in a county covered by the executive order. We also test whether the secondary control voters for the treatment group had higher or lower turnout than the other set of secondary control voters using $\beta_6 SecondaryControlGroup1_i$ term.

Figure 3 indicates that there are different gaps between groups of voters in midterm and presidential years. These baseline differences are captured in the variables $\beta_7 Midterm_t$, $\beta_8 Panhandle_i \times Midterm_t$, and $\beta_9 Treated_i \times Midterm_t$. Finally, the matrix δZ_i contains the individual- and neighborhood-level characteristics on which the match was performed, included in some of the models.

Table 5 presents the results of this model, again fit using an ordinary least squares specification. Model 1 does not include δZ_i, while the matrix is included in Models 2 and 3. Model 3 also includes estimates for congressional district competitiveness in 2018. Robust standard errors are clustered at the level of the original treated voter from which the primary and secondary controls arise.

Table 5: Turnout, 2010 — 2018

	Turnout		
	(1)	(2)	(3)
Panhandle	0.081***	0.082***	0.081***
	(0.005)	(0.005)	(0.005)
2018	0.222***	0.222***	0.222***
	(0.002)	(0.002)	(0.002)
Panhandle × 2018	−0.038***	−0.038***	−0.038***
	(0.005)	(0.005)	(0.005)
Treated	0.003	0.003	0.003
	(0.006)	(0.006)	(0.006)
Treated × 2018	−0.031***	−0.031***	−0.031***
	(0.007)	(0.007)	(0.007)
Secondary Control Group 1	0.026***	0.027***	0.026***
	(0.002)	(0.002)	(0.002)
Midterm	−0.227***	−0.227***	−0.227***
	(0.001)	(0.001)	(0.001)
Panhandle × Midterm	0.018***	0.018***	0.018***
	(0.004)	(0.004)	(0.004)
Treated × Midterm	0.024***	0.024***	0.024***
	(0.005)	(0.005)	(0.005)
Constant	0.600***	−0.303***	−0.307***
	(0.003)	(0.016)	(0.017)
Includes Other Matched Covariates		X	X
Includes control for CD competitiveness			X
Observations	600,955	600,955	600,955
R^2	0.048	0.178	0.178
Adjusted R^2	0.048	0.178	0.178

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

The coefficients on *Panhandle × 2018* and *Treated × 2018* are of most substantive interest here. The coefficient on *Panhandle × 2018* indicates that turnout for the primary control voters in 2018 was -3.8 percentage points below that of the secondary controls. Put differently, the individual-level effect of the storm for voters in the buffer zone was -3.8 points.

Treated × 2018 indicates that, for voters just inside the treated counties, turnout was depressed by an *additional* 3.1 percentage points. This 3.1 percentage point decrease in turnout for voters inside the treated counties is the administrative effect on turnout. Taken as a whole, the net effect of the hurricane on voters just inside the treatment counties was therefore -6.9 percentage points.

Importantly, the decomposed administrative- and individual- effects estimated in Table 5 are the average treatment effect on the treated voters (ATT). These models include only treated voters at the very edges of the hardest-hit counties. It is therefore unsurprising that the net effect estimated by the triple-differences models are smaller than the net effects estimated in Table 2, where all voters in the 8 treated counties are included. Nevertheless, the administrative effect of -3.1 percentage points is substantively quite large. Despite the efforts of Executive Order 18-283, the administrative costs imposed by Hurricane Michael meaningfully depressed turnout.

Shifting Vote Modes

Having established that turnout was substantially depressed in the treated counties and that a non-trivial amount of the depression arose from administrative costs, we turn to a new question: did the storm shift *how* people cast their ballots? We know that Executive Order 18-283 loosened restrictions on early and mail balloting; we therefore expect that, relative to the rest of the state, a higher share of ballots in the treated counties cast their ballots in one of these ways.

We return to the matches produced earlier in this paper, where every voter in the treated counties was matched with five voters elsewhere in the state. Figure 4 demonstrates the share of registered voters that cast a ballot either at the polling place, early in person, or absentee in each general election from the past decade. In each case, the denominator is the number of registered voters in 2018.

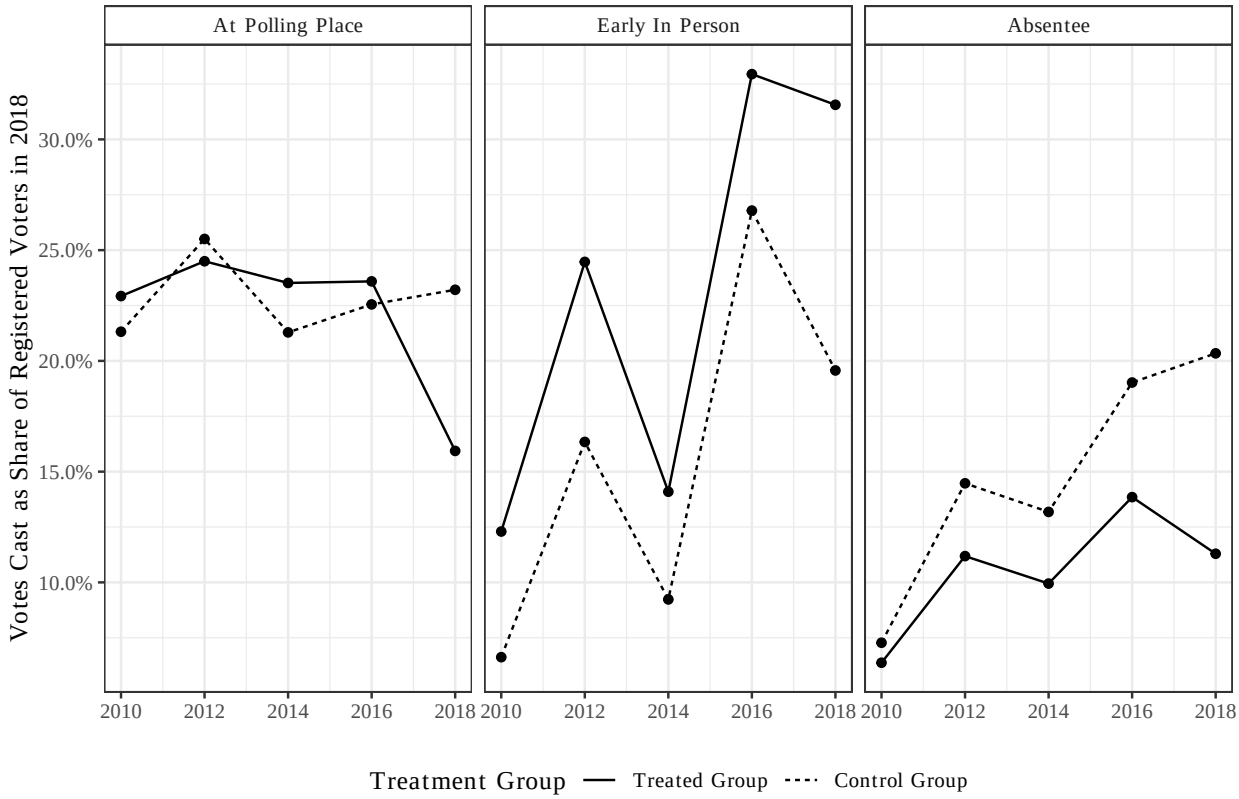

Figure 4: Marginal Effect of Relocated Polling Place on Vote Mode

Figure 4 makes clear that the decline in turnout was a product of lower turnout on election day, and perhaps via absentee voting. It seems possible, however, that early voting was actually higher in the treated counties due to Hurricane Michael. These plots, however, are somewhat noisy. As such, we refrain from estimating difference-in-differences models, as the parallel trends assumption seems to be violated by the historical data. Nevertheless, Figure 4 provides some evidence of how Hurricane Michael shifted vote methods.

To more directly estimate the effect of Hurricane Michael and the executive order on vote-mode, we measure how far each treated voter lived from the closest planned polling place and the polling place that actually opened on election day. Using a multinomial logistic regression, we test whether increasing the difference between this distance is related to vote-mode or abstention in 2018. Table 6 presents the result of this specification, where each option is measured relative to in-person election day voting. In addition to the difference between expected and actual distance to the closest polling place (*Change in Distance to Polling Place (km)*), we include other covariates. *Distance to Closest Planned Polling Place (km)* measures how far a voter lived from her closest planned polling place, in case voters in more remote parts of the counties generally voted differently in 2018 than other voters. We include other covariates for individual characteristics such as race, age, and partisan affiliation. We also include dummies indicating how (or whether) each voter participated in the 2012 – 2016 general elections. The logistic coefficients are transformed into relative risk ratios (standard errors are left untransformed).

Table 6: Vote Mode in 2018 (Relative to In-Person on Election Day)

	Abstain	Early	Absentee
	(1)	(2)	(3)
Change in Distance to Polling Place (km)	1.047***	1.038***	1.038***
	(0.002)	(0.002)	(0.002)
Distance to Closest Planned Polling Place (km)	0.970***	0.942***	1.000
	(0.003)	(0.003)	(0.002)
White	0.951	1.041	0.958
	(0.044)	(0.048)	(0.066)
Black	0.658***	1.007	0.886*
	(0.047)	(0.051)	(0.070)
Latino	0.950	0.862**	0.833*
	(0.067)	(0.075)	(0.107)
Asian	1.246**	1.167	1.066
	(0.092)	(0.098)	(0.136)
Male	0.964**	1.019	0.998
	(0.015)	(0.015)	(0.022)
Democrat	0.790***	0.819***	1.155***
	(0.024)	(0.026)	(0.038)
Republican	0.656***	1.241***	1.150***
	(0.023)	(0.025)	(0.036)
Age	1.001**	1.011***	1.026***
	(0.0005)	(0.001)	(0.001)
Constant	0.442***	0.310***	0.010***
	(0.057)	(0.061)	(0.088)
Includes vote-mode in 2010, 2012, 2014, and 2016		X	
Number of Observations		191,211	
McFadden Pseudo R2		0.269	

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 6 indicates that for each additional kilometer a voter had to travel above-and-beyond the distance to her planned polling place, she was more likely to vote early, to vote by mail, and to abstain altogether than vote in-person on election day. Although these are each statistically significant at the 99 percent level, an examination of the marginal effects plots indicates that their relative importance differed substantially. Figure 5 presents the marginal effect of the change in distance to the nearest polling place on vote method while keeping all other covariates in Table 6 at their means.

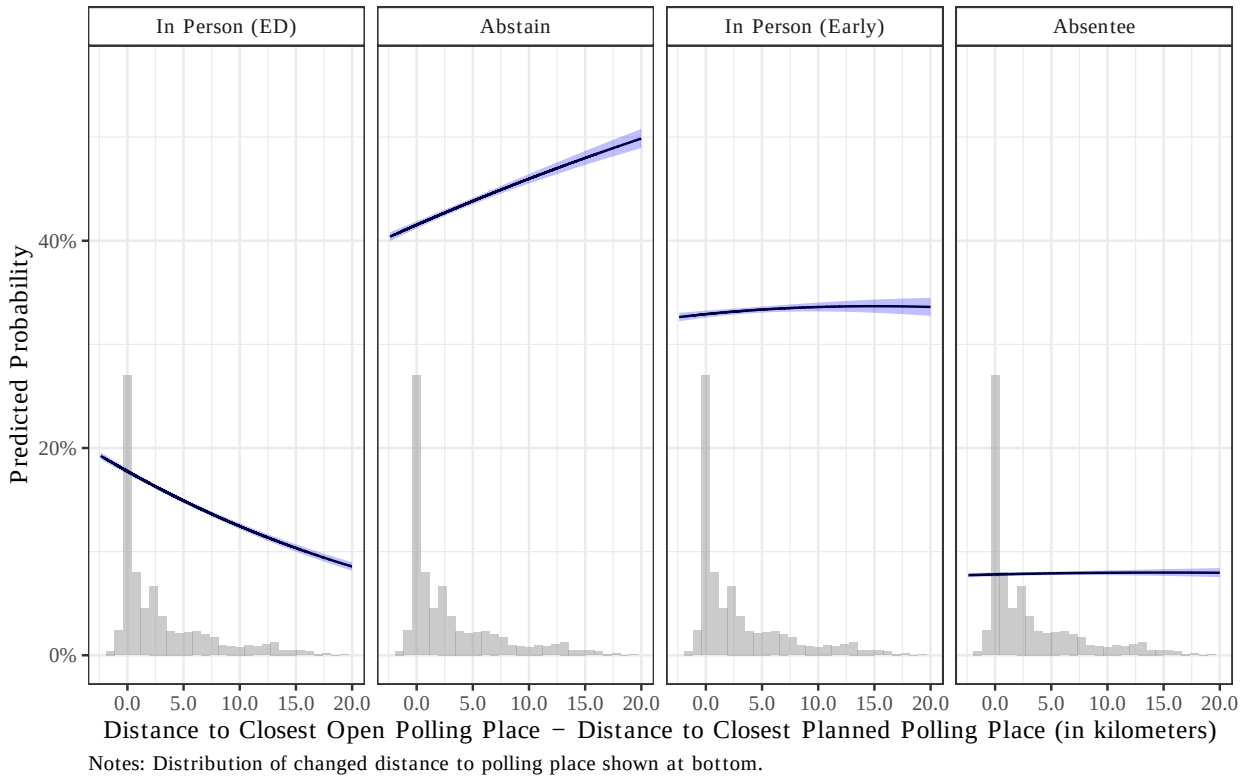

Figure 5: Marginal Effect of Changed Distance to Polling Place on 2018 Vote Mode

Figure 5 indicates that, as voters suddenly had to travel further to the nearest polling place, they were substantially less likely to vote in person on election day ("In Person (ED)"). The bulk of these voters *did not* shift to absentee voting or early in-person voting; rather, they were much more likely to abstain from casting a ballot at all. Thus, although administrators

took steps to make early and mail voting easier, these efforts were not particularly effective.

Discussion and Conclusion

Election Day in the United States consistently falls near the end of hurricane season. Hurricane Michael made landfall on October 10, 2018, less than a month before the highest-turnout midterm election in a century. Hurricane Sandy struck New York and New Jersey just days before the midterm elections in 2012, wreaking immense havoc. Hurricane Matthew struck the Southeastern United States weeks before the 2016 presidential election, killing dozens and causing more than $2.5 billion in damages. Mann and Emanuel (2006) and others have linked Atlantic hurricanes to climate change, indicating that these disruptions to election day activity are likely to increase in coming years. Understanding how storms of this nature impact turnout — and whether states' responses are sufficient to recoup turnout — is therefore vitally important, particularly in swing states such as Florida and North Carolina that are subject to severe coastal natural disasters.

As this paper demonstrates, Florida's response to Hurricane Michael was not particularly effective: although Governor Scott increased access to early and mail voting in eight counties, mail balloting use in these areas actually *dropped* relative to the rest of the state (see Figure 4). Despite the executive order, turnout dropped substantially for voters who suddenly were faced with long distances to the closest polling places. These voters did not move to vote-by-mail options in appreciable numbers.

This is disheartening. Not only did the executive order fail to combat the negative individual-level effects of the hurricane on turnout, it was also insufficient at mitigating the negative administrative effects of closed polling places. Clearly, loosening restrictions on where mail ballots could be sent and how they could be returned was not enough. Without the executive order, polling places would still have been moved because some had been destroyed, but the loosened restrictions on other modes would not have been accessible. Thus, the executive

order likely reduced the administrative costs of voting. Nevertheless, these administrative effects remained quite large and were responsible for nearly half the depressive effect of the storm for voters living at the outer edges of the covered counties.

The data at hand cannot explain why the executive order was ineffective at neutralizing the administrative effects of the hurricane. The timing of the executive order, however, might shed some light. Although the hurricane made landfall on October 10, the executive order was not signed until more than a week later, on October 18 — fewer than three weeks before the November 6 general election. This left little time for an effective public education campaign, perhaps limiting the number of voters who learned and took advantage of the changed rules. We found very few news articles detailing the changes and making the information easily available to voters (but see *WJHG - Panama City* 2018; Vasquez 2018; McDonald 2018; Fineout 2018), and what information did get published often listed only relocated polling places with no information about loosened mail voting restrictions (see, for instance, *Gadsden Times* 2018). It is possible, of course, that local televised news communicated the changes to viewers; however, based on our search of published information, that information would have been difficult to find for voters who missed the televised news. We found no evidence that the Florida Times-Union (the largest paper in Northern Florida) or the Tampa Bay Times (the largest paper in the state) published any articles detailing the changes brought about by the executive order.

Future research will no doubt leverage pre-existing administrative regimes to understand the sorts of voting environments least susceptible to disruption, like those following from the coronavirus in the context of the 2020 elections — but such research will necessarily be backward looking. The experience of Hurricane Michael, on the other hand, gives us important insight about how an emergency that closes polling places will structure turnout. Our research on Executive Order 18-283 makes clear that loosened restrictions on mail voting alone cannot combat the negative turnout effects of shuttered polling places.

The novel coronavirus will perhaps lower turnout even if election administrators respond perfectly. Voting might be low on a list of priorities for individuals who are caring for ailing loved ones, grieving, or dealing with economic crises. Nevertheless, COVID-19 will also pose administrative hurdles to voting: consolidated or relocated polling places, reliance on a vote-by-mail system unfamiliar to many voters, or longer wait times as the number of voters allowed into a polling place at once might all reduce turnout. As administrators consider easing vote-by-mail restrictions, they must look to the case of Florida in 2018. More must be done than simply change the rules; otherwise, the administrative effects of COVID-19 will magnify the individual effects of this public health crisis on voter turnout.

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