Hurricane Michael and Floridian Turnout*

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Abstract

The United States is facing unprecedented challenges to election administration from the novel coronavirus. In response, many election administrators and advocates are calling for expanded vote-by-mail options to reduce in-person voting. To test the efficacy of loosened vote-by-mail rules we look to the experience of Florida in 2018, when Hurricane Michael devastated parts of the panhandle. By leveraging cross-jurisdiction variation in loosened restrictions in a double-matched triple-differences model, we show that loosened restrictions on vote-by-mail alone were not successful at eliminating administrative costs to voting. As administrators around the country 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 advantange of them.

*The authors thanks Many People for their comments on this project. All errors are our responsibility.

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Research Design and Expectations

Based on prior research, we expect that turnout was substantially depressed in the treated counties in 2018. This depressed turnout, however, was likely caused by both individual- and administrative-level mechanisms.

Individual-Level Effects

We know that Hurricane Michael caused substantial destruction; as discussed in the introduction to this paper, residents lost their lives, flooding was widespread, and the hurricane caused billions of dollars of property damage. Would-be voters were now faced with myriad disruptions to their daily lives; it is likely that the direct effects of the weather, therefore, reduced turnout substantially. 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).

Administrative Effects

The hurricane also caused problems for county election administrators, as the reporting around the Governor's executive order makes clear.[1] Some mail voters[2] were residing in locations other than their registered addresses; would-be poll workers were unavailable; and the eight counties covered by the executive order collectively saw just 62 of the anticipated 127 polling places opened. These factors could have increased the costs of voting even for residents who were not directly impacted by the hurricane, and increased the costs of voting even more for individuals who were directly impacted. Absent mitigation, the

[1] Executive Order 18-283 can be found here: https://www.flgov.com/wp-content/uploads/2018/10/SLT-BIZHUB18101809500.pdf. In the event that this link no longer works, it is on file with the authors.

[2] We use the terms mail and absentee voting interchangeably throughout this paper.

administrative effects of Hurricane Michael likely would have decreased turnout above-and-beyond the individual effects of the storm.

Executive Order 18-283 sought to offset the administrative barriers to voting by allowing county election administrators 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, 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 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 rules reduce the impact of closed polling places?

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 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 match 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.

This design allows us to test our first hypothesis:

Hypothesis 1: Turnout in the eight treated counties was lower in 2018 due to the combined effects of the hurricane, county-level responses, and the executive order.

Testing 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. 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. Any difference in turnout observed between groups that live just over a county border from one another, therefore, can be attributed to the administration of the election in their respective county.

To disaggregate the individual and administrative effects of Hurricane Michael, we employ a double-matched triple-differences (or difference-in-difference-in-differences) specification.

We begin by constructing our set of treated voters. These treated voters include all registered voters who live in a treated county and within two miles of a bordering, untreated county. Each treated voter is then each matched to one voter who lives in an untreated county, but within two miles of a treated county. As discussed above, we assume that the only difference between each member of each pair is the administrative context in which they lived because they lived in close geographic proximity to one another. These matches form our set of "primary control voters."

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."

At this point, we have four distinct groups of voters: treated voters; primary control voters; secondary control voters who serve as controls for the treated voters; and secondary control voters who serve as controls for the primary control voters. While the treated, primary control, and full set of secondary control voters are mutually exclusive, secondary control voters are allowed to serve as controls for each of the other two groups. Indeed, because the primary control voters have been selected to resemble the treated voters, overlaps among these groups is highly likely.

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 begins[3] by estimating whether 2018 was associated with depressed turnout for our primary control voters vis-à-vis their (matched, secondary) controls and their own vote history. 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 in turnout, therefore, can be attributed to the individual-level effects of the storm.

The model then estimates a second difference-in-differences model, asking whether 2018 was associated with depressed turnout for treated voters vis-à-vis their (matched, secondary) controls and their own vote history. This model estimates the net individual- and administrative-

[3]In reality, the model estimates each of the following steps simultaneously; for narrative purposes, we describe the effects of the model in a sequential fashion.

level effects faced by treated voters in 2018.

The model finally estimates the difference in the treatment associated with 2018 for the treated and primary control voters. Because the only difference between treated and primary control voters is the administrative district in which they lived, this estimated difference is interpreted as the administrative effect on turnout in the treated counties.

The double-matched triple-differences model allows us to test our second hypothesis:

Hypothesis 2: After controlling for weather, we expect that turnout in the treated counties was slightly lower than in untreated counties. We expect that Executive Order 18-283 was largely, but not entirely, successful at offsetting the depressive administrative effects of Hurricane Michael.

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 estimate the efficacy of mail-voting within the treated counties, we begin by calculating how far each voter lived from her 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 3: 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 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.

Although the treated counties were the 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. 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.

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

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	77.0%	62.0%	77.0%	77.0%	100.00	100.00	100.00	100.00
% Black	17.0%	13.0%	17.0%	17.0%	100.00	100.00	100.00	100.00
% Latino	2.0%	17.0%	2.0%	2.0%	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	53.0%	52.0%	53.0%	53.0%	100.00	100.00	100.00	100.00
% Male	46.0%	45.0%	46.0%	46.0%	100.00	100.00	100.00	100.00
Age	52.23	52.49	52.23	52.23	97.76	94.17	94.71	94.05
% Democrat	39.0%	37.0%	39.0%	39.0%	100.00	100.00	100.00	100.00
% Republican	44.0%	35.0%	44.0%	44.0%	100.00	100.00	100.00	100.00
% with Some College	69.0%	75.0%	69.0%	69.0%	99.79	99.29	98.38	89.71
Median Income	$50,643	$62,941	$50,643	$50,659	99.87	97.55	96.13	84.03

After matching the individual voters, we can construct historical turnout estimates for the treated and control voters. Figure 1 plots the turnout in the past few elections for our treated and control voters. As Figure 1 makes clear, treated voters consistently turned out at higher rates than control voters from 2010 – 2016. In 2018 however — the year when Hurricane Michael wreaked havoc on voters in the treatment group — 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.

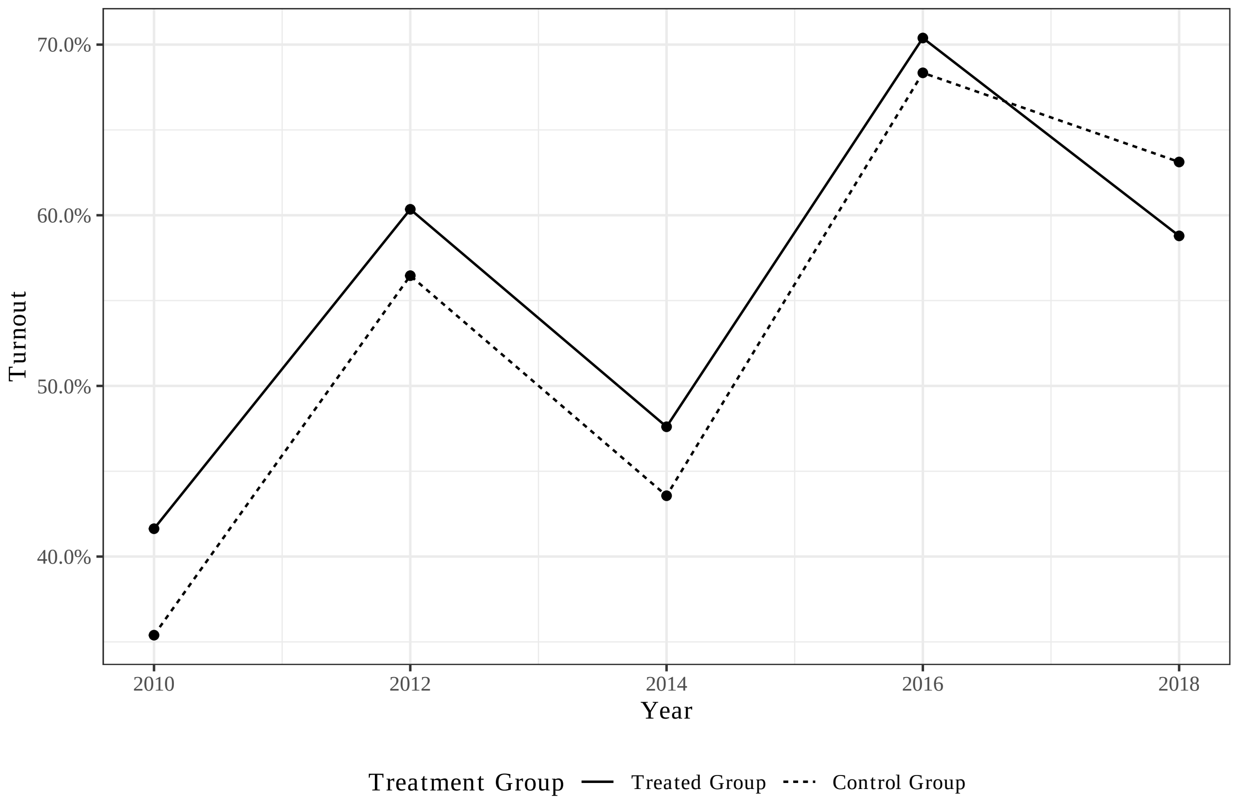

Figure 1: General Election Turnout for Treated and Control Voters, 2010 – 2018

Table 2 formalizes Figure 1 into a differences-in-differences regression specification. We employ an ordinary least squares specification.[5] The dependent variable takes the value 1 if a voter cast a ballot in a given year, and 0 if she did not. Model 1 includes only three variables in addition to the constant. *Treated* measures the gap between treated and control voters in the 2010 – 2016 period. *2018* measures the increase in turnout observed among control voters in 2018, while *Treated* × *2018* measures whether turnout in 2018 departed further or less from the baseline for the treated voters than the control voters. Model 2 includes the same variables, but also includes the characteristics on which the voters were matched. Model 3, finally, also includes measures for congressional district competitiveness. Because

[5]Although the dependent variable here is binary — it takes the value 0 if a voter does not participate, and 1 if she does — the coefficients produced by logistic regressions in the difference-in-differences context are largely uninterpretable. We thus use a linear specification here. When the models are estimated using a logistic specification, the treatment effect is consistent.

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***
	(0.001)	(0.001)	(0.001)
Midterm	−0.229***	−0.229***	−0.229***
	(0.001)	(0.001)	(0.001)
Treated × Midterm	0.022***	0.022***	0.022***
	(0.001)	(0.001)	(0.001)
Treated × 2018	−0.095***	−0.095***	−0.095***
	(0.001)	(0.001)	(0.001)
Constant	0.624***	−0.191***	−0.199***
	(0.0004)	(0.002)	(0.002)
Includes Other Matched Covariates		X	X
Includes control for CD competitiveness			X
Observations	7,945,810	7,945,810	7,945,810
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 -9.5 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. This specification involves matching voters in treated counties to voters in neighboring, untreated counties. This allows us to control for weather effects, while letting the administrative context vary. Treated and these matched, "primary control voters" are then matched to voters elsewhere in the state, allowing us to test the overall effect of the storm on turnout. By comparing the treatment effect for the primary control voters to the treatment voters in a triple-differences framework, we can decompose the individual-level effects from the administrative effects of Hurricane Michael.

We begin by identifying all voters who lived within two miles of a county with a different treatment status. Figure 2 shows the map of counties in the region. The eight treated counties are drawn in a medium gray, and the untreated border counties are drawn in dark gray. All treated and control voters come from the light gray buffer, which extends for two miles on either side of the border between a treated and untreated county. There are not voters in Gulf, Calhoun, or Franklin Counties who live within two miles of an untreated county.

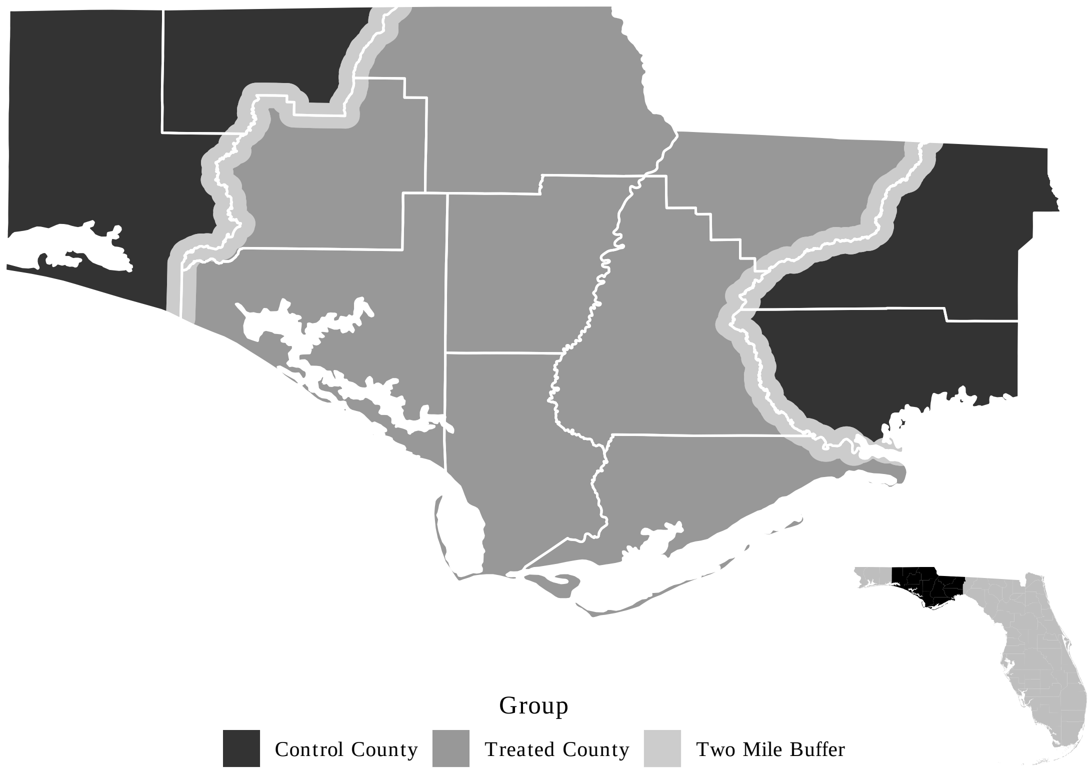

Figure 2: Treated and Control Counties with Two Mile Buffer

Each voter inside the buffer in a treated county is matched with one voter in the buffer in an untreated county, once again using the genetic matching algorithm developed by 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 below 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	74.9%	82.7%	74.9%	74.9%	100.00	100.00	100.00	100.00
% Black	20.7%	12.1%	20.7%	20.4%	96.51	96.63	96.63	96.63
% Latino	1.5%	1.6%	1.5%	1.3%	-153.02	-144.51	-144.51	-144.51
% Asian	0.4%	0.5%	0.4%	0.4%	100.00	100.00	100.00	100.00
% Female	53.4%	52.8%	53.4%	53.4%	98.07	98.13	98.13	98.13
% Male	45.4%	45.7%	45.4%	45.5%	76.18	76.98	76.98	76.98
Age	52.536	51.612	52.536	52.446	90.26	72.06	70.58	60.06
% Democrat	44.2%	38.8%	44.2%	44.2%	100.00	100.00	100.00	100.00
% Republican	41.9%	45.0%	41.9%	41.9%	99.47	99.48	99.48	99.48
% with Some College	63.8%	66.7%	63.8%	65.0%	58.28	43.39	26.07	-17.56
Median Income	$47,598	$49,407	$47,598	$47,242	80.30	-26.88	-17.50	11.06

The match procedure improves the balance between treated and primary control voters substantially for 10 of the 11 characteristics listed in Table 3. The share Latino — the only characteristic to go unimproved — is below 2 percent for both groups and the difference is unlikely to cause any problems. Although latitudes and longitudes are not displayed in the table, the average treated voter lives just 8.5 miles from her primary control voter. This distance satisfies our assumption that treated and primary control voters faced the same weather effects from the hurricane. Figure 3 makes clear that the parallel trends assumption is satisfied for post-matching treated and primary control voters. Turnout patterns in the two groups track very closely. In the pre-treatment period, treated voters turned out at slightly higher rates than their primary controls in midterm years. To the extent this impacts our analysis, it will make any estimated negative treatment effect in 2018 conservative.

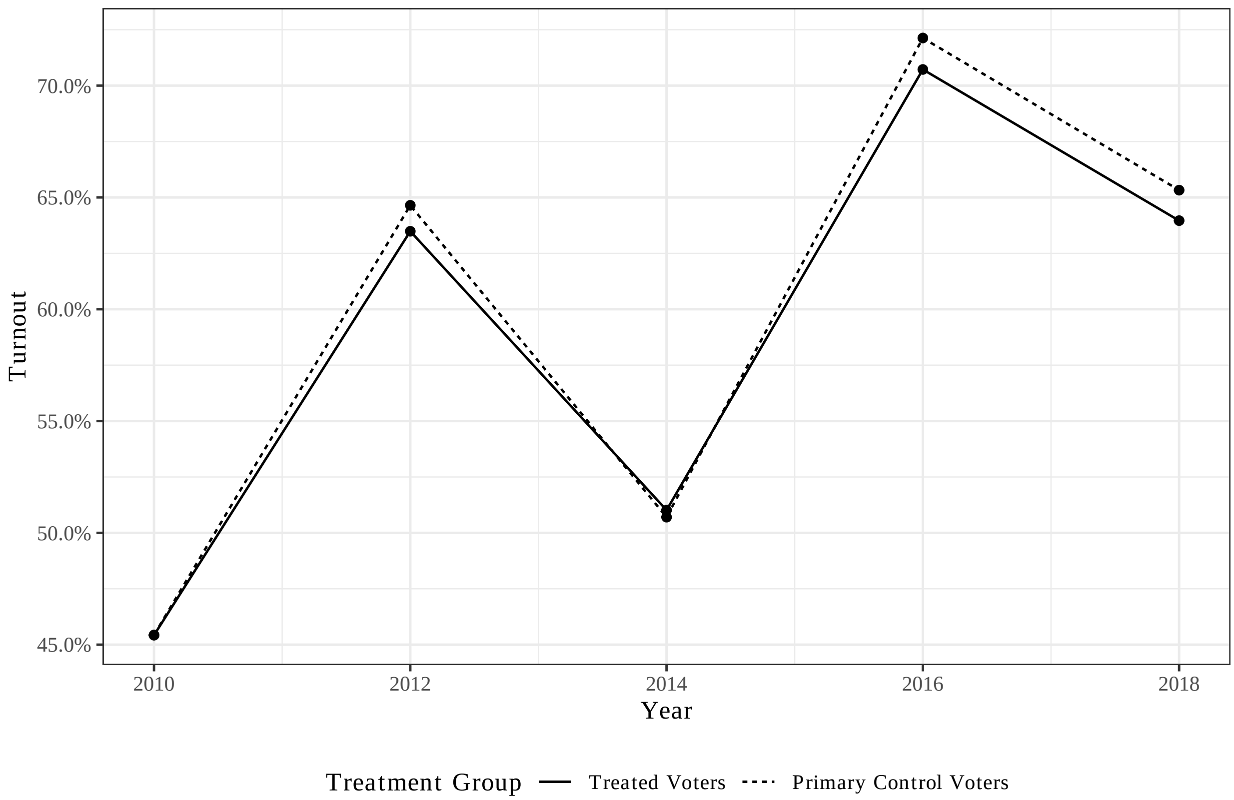

Figure 3: General Election Turnout for Treated and Primary Control Voters, 2010 – 2018

Once our set of treated and primary control voters has been identified, each of these voters is matched with five other voters that lived in neither the treated nor the immediately surrounding counties. 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. Nevertheless, we adopt this shorthand for referring to the treated counties and their neighbors.

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	74.9%	62.3%	74.9%	74.9%	100.00	100.00	100.00	100.00
% Black	19.7%	13.1%	19.7%	19.7%	100.00	100.00	100.00	100.00
% Latino	1.8%	17.4%	1.8%	1.8%	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	53.0%	52.4%	53.0%	53.0%	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	52.403	52.489	52.403	52.398	93.30	90.74	88.85	80.02
% Democrat	43.6%	37.1%	43.6%	43.6%	100.00	100.00	100.00	100.00
% Republican	41.3%	35.0%	41.3%	41.3%	100.00	100.00	100.00	100.00
% with Some College	63.8%	75.1%	63.8%	63.8%	99.94	99.79	98.00	74.54
Median Income	$47,154	$62,941	$47,154	$47,003	99.05	97.51	93.67	73.01

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, $b_1 Pandhandle_i$ measures the historical difference between voters in the panhandle (both treated and matched, untreated individuals) and the rest of the state. $b_2 2018_t$ measures the statewide change in turnout in 2018 from the baseline, while $b_3 Panhandle_i \times 2018_t$ tests whether turnout changed differently in 2018 in the panhandle than it did elsewhere. $b_3 Panhandle_i \times 2018_t$, therefore, is our estimation of the individual-level, or weather related, effect of the hurricane. $b_4 Treated_i$ measures the historical difference between treated and control observations in the buffer, and $b_5 \mathit{Treated}_i \times \mathit{2018}_t$ tests whether the change in turnout in 2018 was different for voters living in the treated counties than for their matched controls in the panhandle. 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 $b_6 \mathit{SecondaryControlGroup1}_i$ term.

We also allow for the possibility that there are different gaps between groups of voters in midterm and presidential years. As Figure 3 indicates, the primary control voters historically had higher levels of turnout than the treated voters in presidential years, but that gap disappears in midterm years. These potential differences are captured in the variables $b_7 \mathit{Midterm}_t$, $b_8 \mathit{Panhandle}_i \times \mathit{Midterm}_t$, and $b_9 \mathit{Treated}_i \times \mathit{Midterm}_t$. 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 includes only the dummies discussed above, while Model 2 includes all the covariates on which the matching procedures were performed. 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	0.072***	0.072***	0.071***
	(0.003)	(0.003)	(0.003)
2018	0.214***	0.214***	0.214***
	(0.003)	(0.003)	(0.003)
Panhandle × 2018	−0.041***	−0.041***	−0.041***
	(0.005)	(0.005)	(0.005)
Treated	−0.013***	−0.013***	−0.013***
	(0.003)	(0.003)	(0.003)
Treated × 2018	−0.015***	−0.015***	−0.015***
	(0.006)	(0.005)	(0.005)
Secondary Control Group 1	0.020***	0.020***	0.018***
	(0.002)	(0.002)	(0.002)
Midterm	−0.224***	−0.224***	−0.224***
	(0.002)	(0.002)	(0.002)
Panhandle × Midterm	0.021***	0.021***	0.021***
	(0.004)	(0.004)	(0.004)
Treated × Midterm	0.014***	0.014***	0.014***
	(0.005)	(0.004)	(0.004)
Constant	0.612***	−0.269***	−0.288***
	(0.002)	(0.008)	(0.008)
Includes Other Matched Covariates		X	X
Includes control for CD competitiveness			X
Observations	446,070	446,070	446,070
R^2	0.045	0.177	0.177
Adjusted R^2	0.045	0.177	0.177

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

The coefficients on *Panhandle* in Table 5 demonstrate that prior to 2018, Panhandle voters generally saw turnout that was 7.1 – 7.2 percentage points above the turnout of the secondary controls. The combination of *Treated* and *Treated* × *Midterm* indicates that treated voters turned out at lower rates than the primary control voters in presidential elections, but at the same rate in midterms. Among the secondary control voters, *2018* indicates that turnout was 21.4 percentage points above what past elections would have predicted, reflective of the nationwide surge in turnout observed in 2018.[6]

The coefficients on *Panhandle* × *2018* and *Treated* × *2018* are of most substantive interest here. *Panhandle* × *2018* indicates that turnout for the primary control voters in 2018 was about 4.1 percentage points below what historical turnout and the behavior of voters elsewhere in the state would have predicted. This decrease can be understood as the direct, individual-level effect of the storm on voters near the border of the treated counties.

Treated × *2018* indicates that, for voters just inside the treated counties, turnout was depressed by an *additional* 1.5 percentage points. This 1.5 percentage point decrease in turnout for voters inside the treated counties above-and-beyond their nearby matches is the administrative effect on turnout. Treated voters, therefore, saw net turnout 5.5 percentage points below what might have been expected in the absense of the hurricane. Though this net effect is smaller than the effect reported in Table 2, this is perhaps unsurprising: these treated voters lived on the outermost edges of the counties covered by the executive order, and therefore likely saw smaller individual-level effects. If the effects of the storm had been as extreme in these areas, the executive order would likely have also covered neighboring counties.

This estimated administrative effect can only be understood as the treatment effect on the treated; it does not necessarily reflect the administrative effect faced by *all* voters in the treated counties. And, in fact, because there are no treated voters from Gulf, Calhoun, or Franklin Counties included in these models, it is likely not fully representative of the expe-

[6]Because we include all registered voters, not only those who were registered for the entire time period, some of this increase is also due to the registration — and turnout — of voters who were ineligible to do so in earlier years.

rience faced by all voters. Nevertheless, this 1.5 percentage point decrease in turnout from administrative effects is large relative to the overall depressive effect of Hurricane Michael estimated in Table 2 (-9.5 percentage points). This indicates that the administrative effects likely played a substantial role in the overall depressed turnout in these counties.

Where Did the Ballots Go?

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: where did these ballots go? 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 (but in neither treated nor neighboring counties). Figure 4 demonstrates the share of registered voters that cast a ballot either at the polling place, early in person, or absentee. In each case, the denominator is the number of registered voters in 2018.

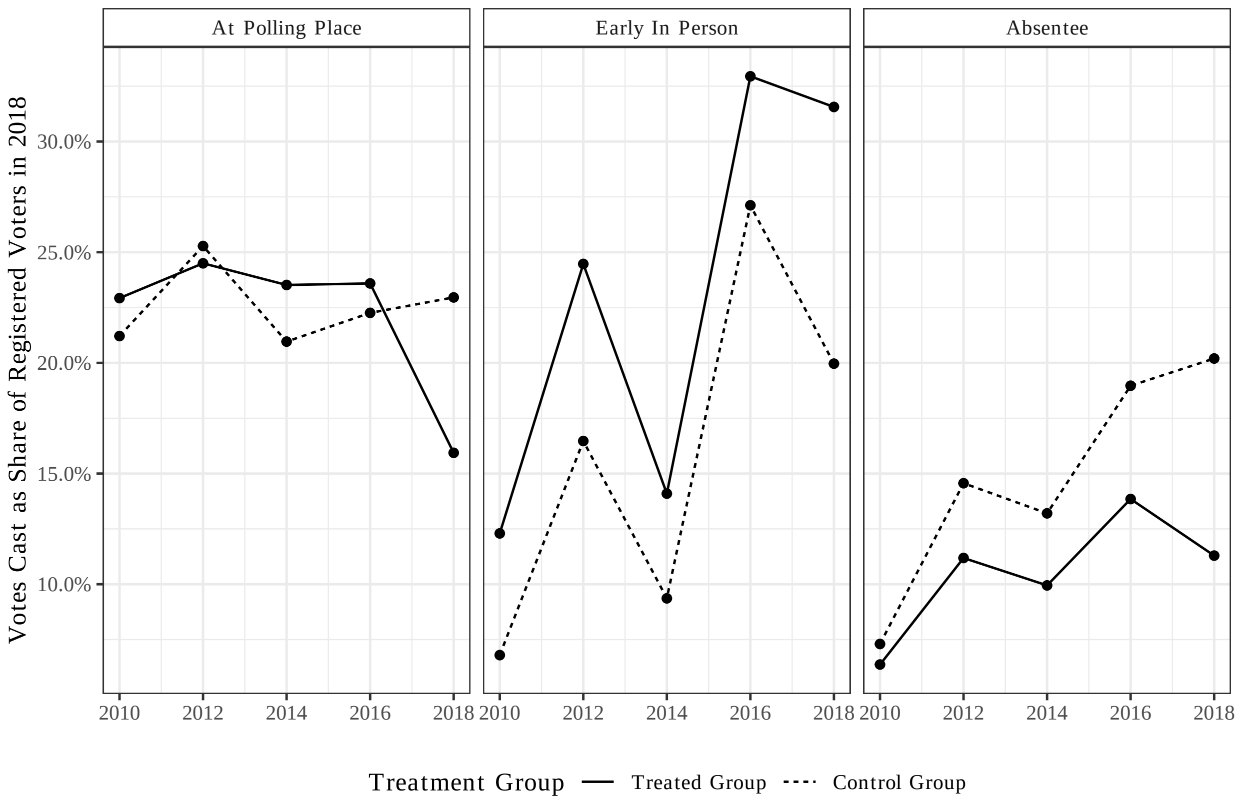

Figure 4: Marginal Effect of Relocated Polling Place on Vote Mode

Figure 4 makes clear that the decline in turnout was a product of lower turnout on election day, and perhaps via absentee voting. It is 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 closing of polling places on vote-mode, we measure the distance between each voter in the treated counties and the closest planned polling place, and the closest polling place actually open on election day. Using a multinomial logistic regression, we test whether increasing the difference between these 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.

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

	Early	Absentee	Abstain
	(1)	(2)	(3)
Change in Distance to Polling Place (km)	0.038^{***}	0.038^{***}	0.046^{***}
	(0.002)	(0.002)	(0.002)
Distance to Closest Planned Polling Place (km)	-0.060^{***}	-0.0002	-0.030^{***}
	(0.003)	(0.002)	(0.003)
White	0.041	-0.043	-0.050
	(0.048)	(0.066)	(0.044)
Black	0.007	-0.121^{*}	-0.419^{***}
	(0.051)	(0.070)	(0.047)
Latino	-0.148^{**}	-0.183^{*}	-0.051
	(0.075)	(0.107)	(0.067)
Asian	0.154	0.064	0.220^{**}
	(0.098)	(0.136)	(0.092)
Male	0.018	-0.002	-0.037^{**}
	(0.015)	(0.022)	(0.015)
Democrat	-0.200^{***}	0.144^{***}	-0.235^{***}
	(0.026)	(0.038)	(0.024)
Republican	0.216^{***}	0.140^{***}	-0.422^{***}
	(0.025)	(0.036)	(0.023)
Age	0.011^{***}	0.025^{***}	0.001^{**}
	(0.001)	(0.001)	(0.0005)
Constant	-1.172^{***}	-4.571^{***}	-0.816^{***}
	(0.061)	(0.088)	(0.057)
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.

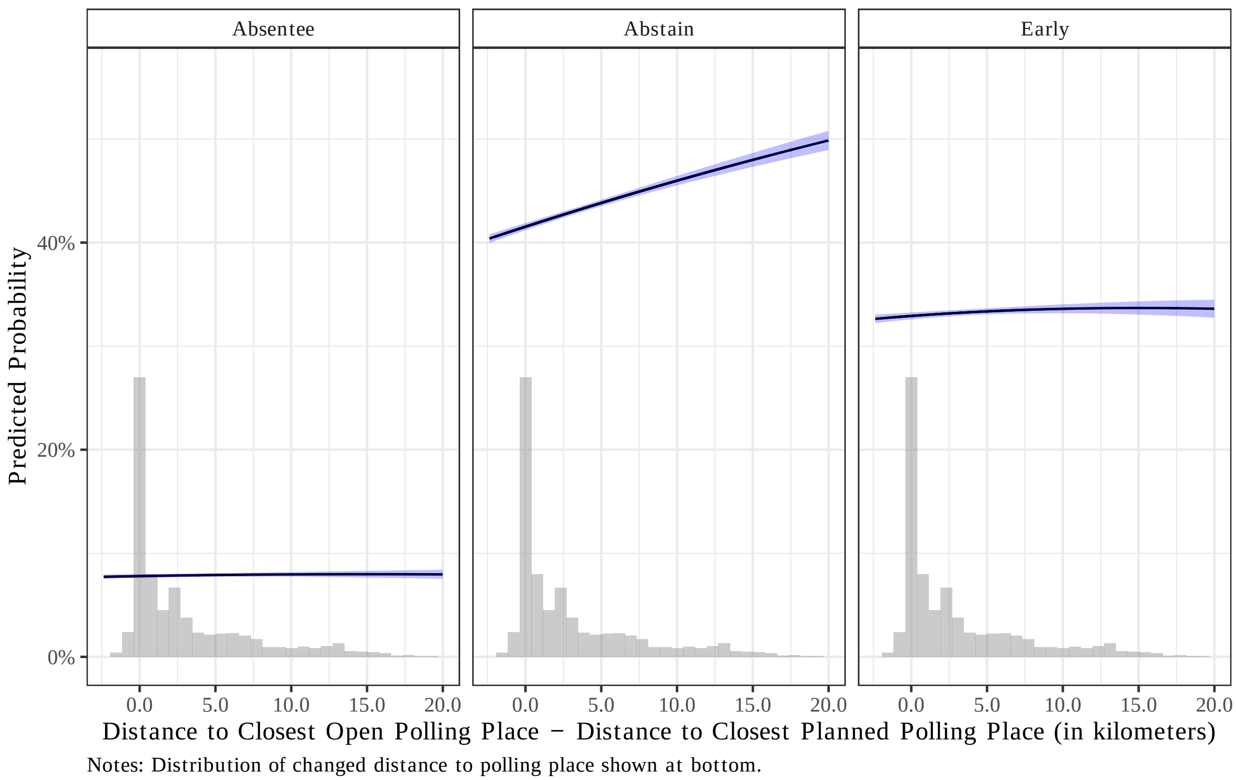

Figure 5: Share of Voters Participating by Vote Method, 2010 – 2018

Figure 5 indicates that, after controlling for other characteristics and historical vote modes, voters who suddenly had to travel further to the nearest polling place were substantially more likely to abstain from voting in 2018. Voters who had to travel an additional 10 kilometers to the nearest polling place were roughly five percentage points more likely to abstain than participate in person on election day. While Table 6 indicates that increased distances also led to higher mail and early voting, Figure 5 shows that the practical effect is actually quite small. Despite eased early and mail voting rules, voters were far more likely to abstain from voting than to take advantage of these options when they were faced with longer drives to the polling place.

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. Superstorm 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 these nature impact turnout — and whether state response is sufficient to recoup turnout — is therefore vitally important, particularly in swing states such as Florida and North Carolina.

The 2020 election will face a different sort of disruption: as the novel coronavirus upends voting across the country, it is becoming clear that many voters will avoid physical polling places, opting instead to vote by mail or to simply abstain. In response to the threat posed by COVID-19 to voting, some states such as New York (Vielkind 2020) have moved to loosen restrictions on mail balloting in their primaries — just as Florida did in the parts of the state hardest-hit by Hurricane Michael before the 2018 general election. Although COVID-19 looks much different than a hurricane, its effects on election administration promise to share many similarities. We need to look no further than Milwaukee, Wisconsin's primary election experience to see the similarities. Although Milwaukee generally has 180 in-person voting sites, these sites were consolidated to just 5 (Herndon 2020). Whether polling places are shuttered due to structural damage or a public health crisis, their closures impose costs on voters.

As this paper demonstrates, the 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 did not even come close to 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. Although the administrative effect on turnout would likely have been larger in the absence of the executive order, the net administrative effects were nonetheless substantial.

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.

If election administrators do not look to past crises to understand how voters will respond, the administrative effects of the novel coronavirus on general election turnout this fall might be large. Future research will no doubt leverage pre-existing administrative regimes to understand the sorts of voting environments least susceptible to disruption from the coronavirus — 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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