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 and consolidate polling places. We test the efficacy of the order using a novel research design to separate the weather effects of the hurricane on turnout from the administrative effects of actions taken by election officials. We show that the Executive Order was successful at eliminating much of the turnout decline following from the hurricane when counties maintained polling places in their planned, pre-election configuration, but voters in counties with many closed polling places were much more likely to abstain than shift to early or mail voting. We argue that natural disasters need not spell turnout disasters if administrators are able to avoid reducing the number of polling places available to voters.

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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. On October 18, the governor of Florida issued Executive Order 18-283[3] as a means to counteract the widespread effects of the hurricane. Executive Order 18-283 sought to offset the administrative barriers to voting by allowing election administrators in 8 Florida counties 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 wind speed, according to the Saffir-Simpson hurricane wind scale. A Category 5 hurricane sustains winds greater than 157 miles per hour, as measured as the peak 1-minute wind at a height of 33 feet. See https://www.nhc.noaa.gov/pdf/sshws.pdf.

[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. The Executive Order covered Bay, Calhoun, Franklin, Gadsden, Gulf, Jackson, Liberty, and Washington Counties.

Although the Executive Order allowed for greater flexibility in the administration of the 2018 election, it was equally notable for what it did not do: namely, provide any emergency funding for the election. According to public records requests we filed with the 8 covered counties, they did not receive any emergency election funding in the aftermath of the storm. In places like Bay County, where the damage was so severe that it threatened to inhibit polling place siting as late as the 2022 midterms (McCreless 2021), erecting emergency polling sites would likely have required substantial financial cost. The state's Executive Order took a different approach by allowing for these polling places to be closed, but attempting to offset these costs by loosening restrictions on mail voting.

This paper sets out to answer a number of questions: what was the total depressive effect of the hurricane on turnout in the election? Did Executive Order 18-283 effectively offset the effects of the weather? More specifically, did easing mail-balloting and early voting rules reduce the impact of closed or moved polling places? We propose a novel research design to investigate these interrelated questions—what we are calling a double-matched, triple-difference model. We use a geographical regression discontinuity that takes advantage of the fact that voters on either side of the outermost borders of the counties covered by the Executive Order were treated to identical *weather* effects from the hurricane, but that only some of them were further treated by the administrative changes allowed by the Executive Order. We strengthen the plausibility of this design by using a matching design to select voters subject only to the weather treatment that look very similar to those who received both treatments. By further matching each of these pairs of voters to registered voters elsewhere in the state—voters who were not impacted by Hurricane Michael—we decompose the weather and administrative effects of the hurricane on turnout.

Our results paint a complex picture. We find that increased travel distances due to polling place consolidation had a much larger effect on turnout than the amount of rainfall voters experienced. The heterogeneity in county-level polling place consolidation makes clear that this was a function of polling place consolidation. Moreover, we show that voters who suddenly had to travel much further than planned to a polling place did not seamlessly shift to loosened mail voting options, but were instead substantially more likely to abstain from voting altogether. In short, counties that avoided polling place closures saw negligible turnout effects, but where voters were faced with much longer distances to their polling place, loosened restrictions did little to offset those costs.

As hurricanes grow increasingly frequent and intense due to climate change, understanding how to manage elections to ensure that they remain equitable and accessible will only become more important. While this is abundantly clear in the United States, where federal elections are held in early November, it is equally true for democracies around the globe. Typhoon Lan, for instance, disrupted Japanese elections in 2017 as we discuss below. While conducting an election under such circumstances is never easy, our results indicate that major turnout losses can perhaps be avoided if polling places remain open.

Literature Review

The institutional and weather conditions of Hurricane Michael make it ripe for studying the interactive effects of severe weather, polling place siting, and administrative regimes. Indeed, the heterogeneity in polling place closures as a result of the storm allows us to precisely test the impact of these closures. Understanding these relationships will be of key importance in the coming years as climate change leads to increasingly strong storms (Mann and Emanuel 2006). This is doubly true in the American context, where federal elections are held at the end of hurricane season. Although little work has explored how these effects interact, we here consider how Florida's permissive early voting regime, the Executive Order's allowance of polling place consolidation, and severe weather might have collectively structured turnout in 2018. Our general conclusion from the extant literature is that early voting could have likely served as a "relief valve" on the pressures introduced by the inclement weather, but that polling place consolidation likely had major, negative turnout effects.

Early Voting and Inclement Weather

It is well established that inclement weather on election day reduces turnout in both the American (Cooperman 2017; Hansford and Gomez 2010) and international context (Rallings, Thrasher, and Borisyuk 2003), especially in noncompetitive and general elections (Gatrell and Bierly 2002; Fraga and Hersh 2010). A recent study based on Irish parliamentary elections indicates that this is especially true in densely populated areas (Garcia-Rodriguez and Redmond 2020). Severe weather reduces turnout by increasing the opportunity cost of voting: driving to a polling place or, worse, waiting outside in line to vote is obviously much more costly in severe weather events. A natural disaster can increase burdens on households even if it strikes before election day, perhaps leaving them less likely to learn about the candidates, locate their polling place, and cast a ballot.

Although Floridians in the panhandle faced a Category 5 hurricane in 2018, the hurricane arrived against the backdrop of Florida's permissive early voting infrastructure. Since 2008, about 25% of Floridians, on average, have cast their ballots early in-person, prior to election day.[4] It seems plausible that this availability could have sufficiently reduced the cost of voting to offset some of the negative effects associated with the storm. While research on the impact of early in-person voting on turnout in non-emergency times has returned mixed results (see, for instance, Ricardson and Neeley 1996; Larocca and Klemanski 2011; Burden et al. 2014; Kaplan and Yuan 2020), a growing body of literature suggests that the availability of early in-person voting might be important in the context of severe weather. One study in Sweden, for instance, found no significant turnout effects of rain on election day, which they attribute

[4]This estimate is based on our analysis of Voter Registration Supplements to the Current Population Survey over six general elections between 2008 and 2018.

to Sweden's permissive early voting regime (Persson, Sundell, and Öhrvall 2014, 337); voters were able to avoid an incoming storm by casting a ballot in advance.

Most relevant to our study of Hurricane Michael are the effects of Superstorm Sandy on turnout in the Northeastern US in 2012 and Typhoon Lan[5] in the 2017 House of Representatives election in Japan. The typhoon made landfall the day after election day, though it appears voters behaved dynamically as the typhoon approached: voters were more likely to vote early, or earlier on the day of the election, as rainfall increased in prefectures in the path of the typhoon (Kitamura and Matsubayashi 2021). Of course, we cannot know which individuals who voted early would have braved the storm and voted even in the absence of such an option, and which would have opted to stay home. Nevertheless, it is not unreasonable to assume that the availability of early voting allowed some voters to participate who would not have in worse weather.

The experience of Superstorm Sandy in the Northeastern United States in 2012, a storm whose political impacts have been studied by a number of scholars (Lasala-Blanco, Shapiro, and Rivera-Burgos 2017; Velez and Martin 2013), provides more evidence of the importance of early voting in the face of severe weather. Stein (2015, 69) argues that turnout in counties impacted by Superstorm Sandy decreased by 2.8% between 2008 and 2012—a full 2% more than the rest of the country. He finds, however, that counties that provided for early in-person voting actually saw *higher* turnout in 2012 than other comparable counties. It seems that, whatever questions remain about the impact of early in-person voting on turnout in normal times, that such an option may provide a way to recoup some of the lost turnout caused by a natural disaster.

[5] Lan was the equivalent of a Category 4 hurricane, featuring wind speeds of between 130 and 156 miles per hour.

Polling Place Consolidation

Even as Floridians had access to widespread early in-person voting in 2018, Hurricane Michael destroyed polling places across the region, and the Executive Order allowed administrators to consolidate voting locations rather than open emergency sites. In fact, just 61 of the planned 125 polling places were open across the 8 counties covered by the Executive Order. Understanding the impact of these consolidations in light of the hurricane is important for situating the anticipated effect of the storm on turnout—and, in particular, the effect of the state's decision to allow counties to consolidate polling places rather than provide emergency funding for election administration.

Voting rights advocates recently argued that polling place closures should be avoided in an emergency, even when vote-by-mail restrictions are loosened. While Hurricane Michael preceded the coronavirus pandemic, the arguments made in 2020 against widespread closures apply equally to closures from a hurricane. As Macías and Pérez (2020) at the Brennan Center for Justice argued, "[m]any Americans do not have access to reliable mail delivery, and many do not have conventional mailing addresses for ballot delivery. Eliminating polling sites would completely disenfranchise these voters." The Center for American Progress made a similar argument, writing that "[w]hile vote by mail is an option that works for many Americans, it is not a viable option for everyone. Specifically, eliminating all in-person voting options would disproportionately harm African American voters, voters with disabilities, American Indian and Alaska Native voters, and those who rely on same-day voter registration" (Root et al. 2020). In other words, voting rights advocates argue not only that polling place closures in an emergency reduce turnout, but that the turnout reductions do not fall evenly across the electorate.

The scholarly literature bears this out. Although Stein (2015) argues that counties impacted by Superstorm Sandy that consolidated polling places saw *higher* turnout than those that were affected but did not consolidate their polling places, this result is something of an outlier. The extant literature is consistent in its conclusion that polling place consolidation reduces turnout. Relocating or reducing the number of polling places reduces turnout by imposing new search and transportation costs on voters (Brady and McNulty 2011). A moved polling place 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).

Although there has been little work on the effect of polling place consolidation on turnout in the face of a storm, recent work indicates that last-minute polling place consolidation reduced turnout during the Covid-19 pandemic in 2020. During the April 2020 primary election in Milwaukee, Wisconsin, the municipality went from 182 to just 5 polling places. Morris and Miller (2021) shows that this consolidation had major, negative turnout effects, even though Wisconsin has a robust absentee voting regime. They conclude: "Even as many voters transition to vote-by-mail in the face of a pandemic, polling place consolidation can still have disenfranchising effects" (Morris and Miller 2021, 13). While polling place closures and movements seem to impose costs on voters and reduce turnout even under the best of circumstances, it seems possible that these costs are much higher when coupled with the other demands on voters' time imposed by emergency situations—even when other alternatives such as absentee voting are readily available.

Grounding our analyses of the effects of Hurricane Michael gives us some expectations as to how the hurricane altered voting behavior. We expect the direct, weather-related effects of the hurricane reduced turnout. The administrative effects—that is, the turnout effects arising from decisions made by election administrators under the latitude granted by the Executive Order—will push in opposite directions. On the one hand, consolidated polling places likely imposed costs on voters, reducing turnout above-and-beyond the direct effects of weather. On the other hand, the 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 a complex, interconnected process involving multiple actors (Hale, Montjoy, and Brown 2015; Brown, Hale, and King 2019). Without emergency funding provided by the state, it is not clear that officials in places like Bay County could have kept the planned number of polling places open.

Research Design and Expectations

We expect that Hurricane Michael depressed turnout in the 2018 midterm election via two causal mechanisms: weather effects and administrative effects. By weather effects, we mean the direct costs imposed on voters, such as destroyed or damaged property and temporary relocation. Administrative effects refer to the turnout effects of how the election was run such as closed polling places and increased access to mail voting. Throughout our analyses, we examine the effects of the hurricane on voters registered as of the 2018 election. Put differently, we do not test the turnout of *eligible citizens*. Conditioning turnout on registration status raises important questions when the treatment might influence registration (see Nyhan, Skovron, and Titiunik 2017). That is likely the case here: as we demonstrate in the Supplementary Information, it seems probable that Hurricane Michael reduced registrations in the days before the registration deadline. Our models cannot capture these turnout effects; as such, our estimated negative treatment effects should be considered conservative, as we are not measuring the turnout of individuals whose registration—and subsequent participation—was impeded by the storm.

Estimating the Overall Effects of the Hurricane

We begin by testing the average marginal effect (AME) of Hurricane Michael on turnout. The AME is the net effect of both the weather and the administrative effects 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 and the latitude and longitude of each voter's residential address. 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.

In addition to the individual-level characteristics from the voter file, we also proxy each voter's exposure to Hurricane Michael using rainfall data. The National Oceanic and Atmospheric Administration (NOAA) makes daily rainfall data available for some 13,000 stations around the United States. We use the `rnoaa` (Chamberlain 2021) package to measure the amount of rain that fell between October 10 and November 6 in 2018 (relative to the average rainfall in that period from 2000 to 2017) at each weather station in the country. Voters' individual exposure to rainfall is calculated as the average of the three closest weather stations, inversely weighted by distance.

Finally, we incorporate information garnered from public records requests sent to each of the 8 treated counties. Although the counties did not, by-and-large, take advantage of the opportunity to add early voting days granted by the Executive Order (no county increased the number of days by more than 2), some counties did reduce the number of polling places. Three counties (Calhoun, Gadsden, and Liberty) closed no polling places, while a fourth (Franklin) actually added an additional polling place. The other four covered counties cut the number of polling places by at least two-thirds. We calculate how far each voter lived from the closest *planned* polling place in her county, and how far she lived from the closest polling place that was *actually open* on election day. We leverage this heterogeneity to explore the effect of an increased distance to the nearest polling place, and expect the turnout effect of the storm was larger (that is, more negative) for treated voters who suddenly had to travel much further to the nearest in-county polling place. In the Supplementary Information we include a table detailing the number of polling places and days of early voting in each covered county.

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 AME 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, including past turnout, registration date, and vote mode.[6] 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 plausibility of the parallel trends assumptions necessary for a rigorous difference-in-differences analysis (Sekhon 2009, 496; Imai, Kim, and Wang 2020). As we show in the Supplementary Information, our estimated AME is robust to a variety of different pre-processing and modeling choices.

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. We expect that the negative AME will be larger for voters who had to travel further than expected to their nearest polling place, and where the relative rainfall was higher.

[6] Voters registered as of the 2018 election are included in each year, even if they were not yet registered, and are marked as nonparticipants in any election in which they did not vote. The "registration date" available in the registered voter file appears to more aptly capture the last time a registrant updated her information.

Decomposing Weather and Administrative Effects

To estimate the administrative effect on turnout, we must control for the weather effects encountered by each voter. 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). This is often referred to as a geographical regression discontinuity (Keele and Titiunik 2015). There is no reason to believe that the direct, weather 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. Put differently, these voters were identically "treated" by the weather effects of the hurricane. 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 *not* to the weather, but the administrative effects of the county in which they happen to live. While all these voters were "treated" by the hurricane, only those in the covered counties also received the administrative treatment arising from the Executive Order.

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 according to their historical turnout and vote mode. To strengthen the plausibility that these two sets of voters were identically treated by the weather, we also match on each voter's relative rainfall.

By comparing the 2018 turnout of these voters, we can identify the administrative effect of the Executive Order on turnout for the administratively treated voters living within the buffer around the border. By further comparing the turnout of these voters to (matched) voters elsewhere in the state, we can also estimate the weather effects of the storm. 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 voters who received an administrative treatment. These voters include all registered voters who live in a county covered by the Executive Order and within 2.5 miles of a bordering, uncovered county (See Figure 1). Each treated voter is then matched to one voter who lives in an uncovered county, but within 2.5 miles of a covered county. All of these voters were treated by the weather, but only those in the covered counties were also treated by the administrative changes. Although Calhoun, Franklin, and Gulf Counties were covered by the Executive Order, no voters in these counties live within 2.5 miles of an uncovered county; as such, no voters from these counties are included in these models.

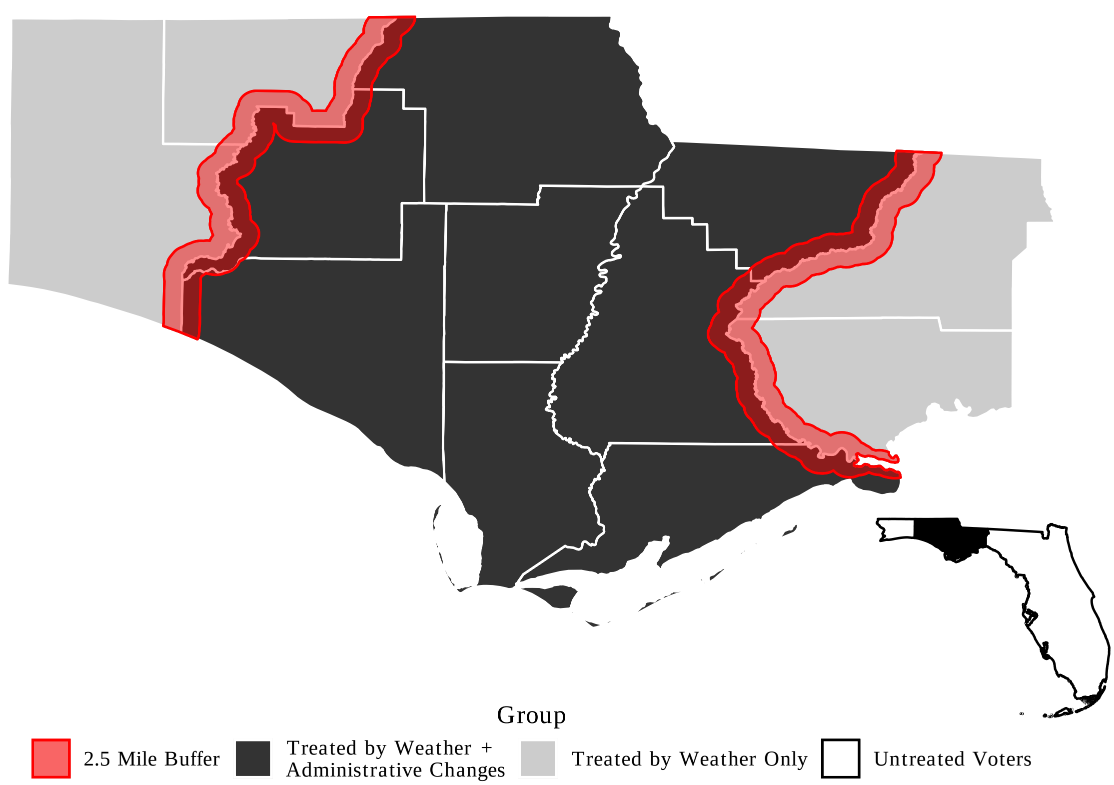


Figure 1: Treated and Control Counties with 2.5 Mile Buffer

Each of these voters is subsequently matched to five voters elsewhere in the state—that is to say, voters who received neither a weather treatment *nor* an administrative one. This exercise is the second match, and the matches are our control voters.

Table 1 summarizes the treatment status of our three groups of voters.

Table 1: Treatment Status for Selected Voters

	Treatment Received	
Group	Administrative	Weather
Voters in Covered Counties	Yes	Yes
Voters in Uncovered Counties in Panhandle	No	Yes
Voters Elsewhere	No	No

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 voters treated only by the weather vis-à-vis the controls who received no treatment. Because these treated 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 weather effect for all voters treated by the weather, regardless of whether they received an additional, administrative treatment.

The model also estimates turnout differences between voters treated by the weather and administrative effects, and those treated only by the weather. Because we assume these closely-located voters faced identical weather effects, any difference between them is the administrative effect on turnout of their county's response to the Executive Order.

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

Hypothesis 2: We expect that the hurricane had negative weather effects for voters who lived just outside of covered counties.

Hypothesis 3: We expect that the administrative effect will be largely driven by increased travel distance to the nearest polling place for administratively treated voters, other things equal. Where many polling places were closed we anticipate a large, negative administrative effect (Morris and Miller 2021). In contrast, where most polling places remained open, we expect small negative or small positive administrative effects.

In short, our empirical strategy incorporates three powerful tools for establishing causality: matching, difference-in-differences, and a regression discontinuity. As we demonstrate in the Supplementary Information, our estimated administrative treatment is robust to specifications including a county-linear time trends, and without any matching at all.

Vote Mode

After estimating the double-matched triple-differences model, we turn to vote-mode within the treated counties. Specifically, we test whether polling place closures allowed under the Executive Order shifted vote mode from in-person to either early or mail voting in the treated counties. 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, all else being held equal.

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 nearest neighbor approach. We use a genetic algorithm to determine the weight each characteristic should receive for the matching procedure (Sekhon 2011).[7] The individual-level characteristics come directly from 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 randomly broken, and matching is done with replacement.

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. According to public records requests we filed, these counties did not reduce polling places or early voting days because of the hurricane. While they received no administrative treatment, we exclude them because of their potential weak weather treatment.

Table 2 demonstrates the results of this matching procedure. As Table 2 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. Though the matching process included historical vote mode, these are not included in Table 2 but Figure 2 shows that the procedure was effective at reducing historical differences between the treated and potential control voters.

Figure 2 plots the turnout in the past few elections for our treated and control voters. The

[7] Due to computing constraints, the matching weights were constructed using a one percent random sample stratified by treatment status. The weights derived from the genetic algorithm are then used to perform the nearest-neighbor match for all treated voters.

Table 2: Balance Table for Statewide Matching

	Means: Unmatched Data		Means: Matched Data	
	Treated	Control	Treated	Control
% White	76.5%	62.3%	76.5%	76.5%
% Black	17.1%	13.1%	17.1%	17.1%
% Latino	2.1%	17.4%	2.1%	2.1%
% Asian	1.0%	2.0%	1.0%	1.0%
% Female	52.5%	52.4%	52.5%	52.5%
% Male	45.8%	44.9%	45.8%	45.8%
Age	52.2	52.5	52.2	52.2
% Democrat	39.2%	37.1%	39.2%	38.5%
% Republican	43.6%	35.0%	43.6%	41.7%
% with Some College	69.0%	75.1%	69.0%	69.0%
Median Income	$50,643	$62,941	$50,643	$50,727
Registration Date	2002-03-13	2004-10-17	2002-03-13	2002-04-03

left-hand panel shows the turnout of all voters registered in 2018. In the right-hand panel, we plot the turnout of treated voters and only their controls. As Figure 2 makes clear, turnout in the treated counties was consistently higher than the rest of the state—until 2018, when the hurricane hit. In the right-hand panel, we see that there was a substantial, negative treatment effect in 2018.

Table 3 formalizes the right-hand panel of 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, *Full Treatment × 2018*—a dummy that takes the value 1 in 2018 for treated voters and is 0 in all other years and for all other voters—estimates the average marginal effect of Hurricane Michael on turnout for treated voters. Each model also includes county and year fixed effects. Model 2 includes the characteristics on which the voters were matched. Model 3 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

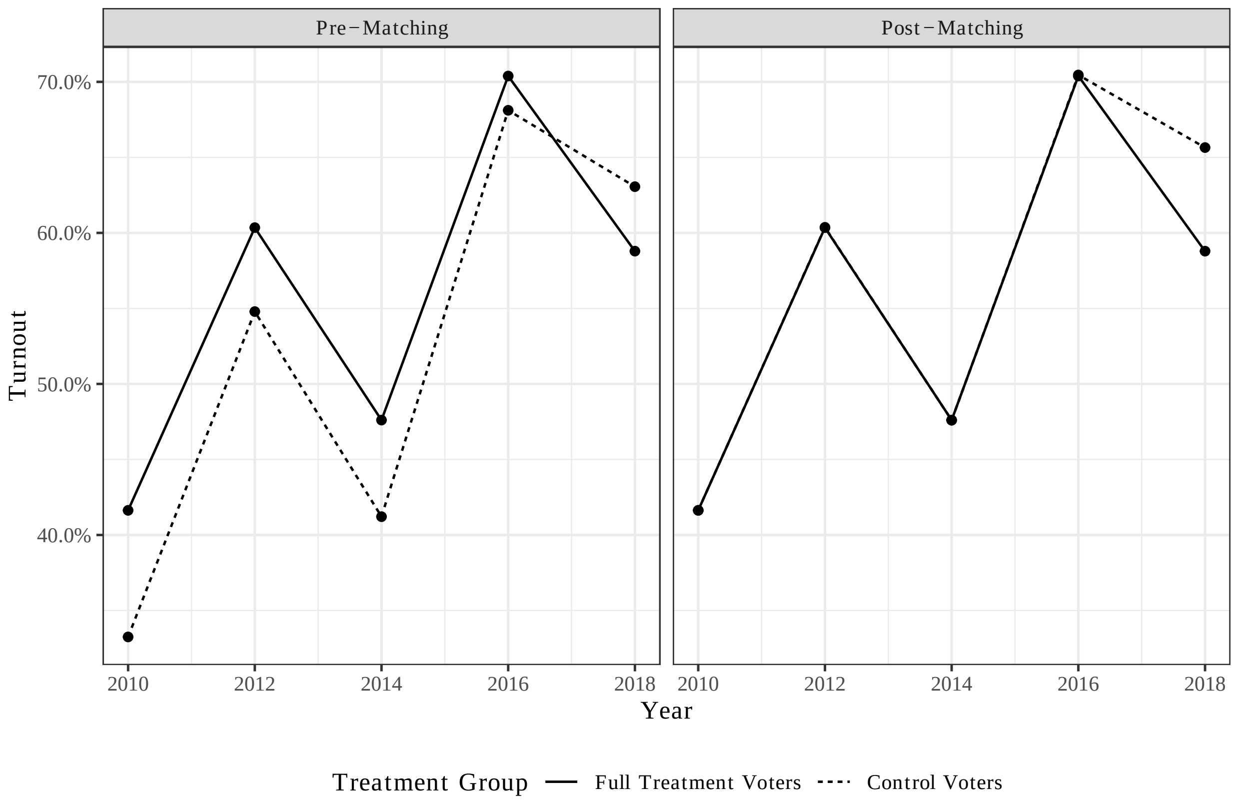


Figure 2: General Election Turnout for Voters Covered by Executive Order and Their Controls, 2010 – 2018

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.

In model 4, we allow for the possibility that the treatment effect was different where the hurricane had greater intensity. In this model, *Full Treatment × 2018 × Relative Rainfall* allows the treatment effect to vary based on our proxy for hurricane strength. Finally, in model 5, we ask whether the treatment effect was different for voters who had to travel further to cast an in-person ballot (*Full Treatment × 2018 × Change in Distance to Closest Polling Place*). Model 5 includes controls for hurricane strength to tease apart the effect of polling place closures from hurricane strength. In models 4 and 5, control voters are assigned the rain and changed distance values of their treated voter. While the regressions include the full set of uninteracted and interaction terms, we display only these variables' impact on the treatment estimate in table. The clustered nature of the data is somewhat complex: observations are clustered by individual, by matched group, and by county, and these groups are not nested. We thus report robust standard errors clustered at each of these levels using the nonnested multiway clustering approach developed by Cameron, Gelbach, and Miller (2011).

Table 3: Turnout, 2010 — 2018

	Model 1	Model 2	Model 3	Model 4	Model 5
Full Treatment × 2018	-0.068***	-0.068***	-0.068***	-0.096	-0.060
	(0.009)	(0.009)	(0.009)	(0.080)	(0.072)
Full Treatment × 2018 × Relative Rainfall				0.014	0.003
				(0.044)	(0.040)
Full Treatment × 2018 × Change in Distance to Closest Polling Place					-0.006**
					(0.002)
Year Fixed Effects	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓
Matched Covariates		✓	✓		
CD Competitiveness			✓		
Rainfall and Interactions				✓	✓
Changed Distance to Polling Place and Interactions					✓
Cluster Level:	IGC	IGC	IGC	IGC	IGC
Num.Obs.	5925990	5925990	5925990	5925990	5925990
R2	0.051	0.280	0.280	0.053	0.054
R2 Adj.	0.051	0.279	0.280	0.053	0.054

Cluster notation is as follows: I(ndividual); (Matched)G(roup); C(ounty)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The coefficient on *Full Treatment* $\times$ *2018* in Table 3 indicates that Hurricane Michael had a substantial depressive effect in 2018 among the treated voters. Models 1 – 3 indicate that the hurricane reduced turnout in the treated counties by roughly 6.8 percentage points. Multiplied across the nearly 200,000 registered voters in the treated counties indicates that some 13,600 ballots went uncast due to the hurricane, a major effect in a year when a statewide senate race was decided by 10,033 votes.

Model 4 indicates that the turnout effect was not moderated by the strength of the hurricane as proxied by rainfall. It should be noted, however, that there is not a tremendous amount of variation in relative rainfall among treated voters: the interquartile range for rainfall relative to the historical average stretches from 174% to 200%. Model 5 makes clear that the treatment effect was much larger for voters who had to travel further to the closest polling place: every additional mile a voter had to travel above-and-beyond the planned distance led to a turnout decline of 0.6 points. Once we control for how polling place consolidation impacted travel distances, the overall treatment effect is no longer statistically significant, indicating that much of the treatment effect can be attributed to the consolidation. In short, Table 3 indicates that the negative turnout effects of a Category 5 hurricane that strikes weeks before an election can be mitigated by avoiding polling place consolidation.

Identifying Administrative Effects

As discussed above, our primary strategy for isolating the administrative effects of the hurricane on turnout involves leveraging as-if random assignment around county borders in the Florida panhandle in a double-matched triple-differences specification. Each voter inside the buffer in a covered county is matched with one voter in the buffer in an uncovered county, once again using a genetic matching algorithm (Sekhon 2011). Ties are broken randomly, and matching is done with replacement.

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 U.S. 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. In constructing our full set of voters treated by weather effects, equalizing individual-level exposure to Hurricane Michael is of paramount importance. As such, in this first match, we include only historical vote mode, voters' relative rainfall, and latitude and longitude. This ensures that the voters treated by weather and administrative effects and those treated only by the weather will have similar past turnout trends and live near one another.

After matching, these pairs of voters live an average of about 3.6 miles from one another. Importantly, the relative rainfall faced by the two groups is virtually identical: while rainfall during the period was 164% of normal for the voters outside the covered counties, it was 167% of normal for the voters inside the covered counties. It is worth noting that the causal identification of the administrative effect does not require that rainfall perfectly proxies the weather effects of the hurricane, but rather that these pairs were subjected to comparable individual-level effects from the storm. We consider this assumption satisfied by the close residential proximity of these pairs and their nearly identical relative rainfall.

Once our full set of voters exposed to weather effects has been identified, each of these voters is matched with five other voters that lived in neither the covered 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.

In Figure 3 we plot the turnout trends from the three sets of voters returned by the matching exercise. Figure 3 makes clear that the turnout gap between voters treated by weather and administrative effects, and those treated only by the weather, is eliminated in the base period, as is the turnout gap between the full set of voters treated by the weather and their controls.

Table 4: Balance Table for Secondary Match

	Means: Unmatched Data		Means: Matched Data	
	Treated	Control	Treated	Control
% White	73.4%	62.3%	73.4%	73.4%
% Black	22.6%	13.1%	22.6%	22.6%
% Latino	1.0%	17.4%	1.0%	1.0%
% Asian	0.3%	2.0%	0.3%	0.3%
% Female	53.1%	52.4%	53.1%	53.1%
% Male	45.6%	44.9%	45.6%	45.6%
Age	53.3	52.5	53.3	53.2
% Democrat	45.6%	37.1%	45.6%	44.4%
% Republican	40.9%	35.0%	40.9%	39.1%
% with Some College	62.9%	75.1%	62.9%	62.9%
Median Income	$45,981	$62,941	$45,981	$45,841
Registration Date	2000-08-16	2004-10-17	2000-08-16	2000-09-08

Disentangling the administrative and weather effects of the storm requires the estimation of the triple-differences model. This model is estimated by Equation (1). In the model, *Weather Treatment*$_i$ $\times$ *2018*$_t$ is a time-variant dummy that is 1 in 2018 for voters in the panhandle, and 0 for all other voters and in all other periods. *Administrative Treatment*$_i$ $\times$ *2018*$_t$, meanwhile, take the value 0 for all observations except in 2018 for voters in the counties covered by the Executive Order.

$$
\begin{gathered}
v_{it} = \beta_0 + \beta_1 WeatherTreatment_i \times 2018_t + \\
\beta_2 AdministrativeTreatment_i \times 2018_t + \\
\delta County_i + \delta Year_t + \\
\delta Z_{it} + \mathcal{E}_{it}.
\end{gathered} \tag{1}
$$

The estimation strategy, then, takes the form of a two-way fixed effects model. Individual

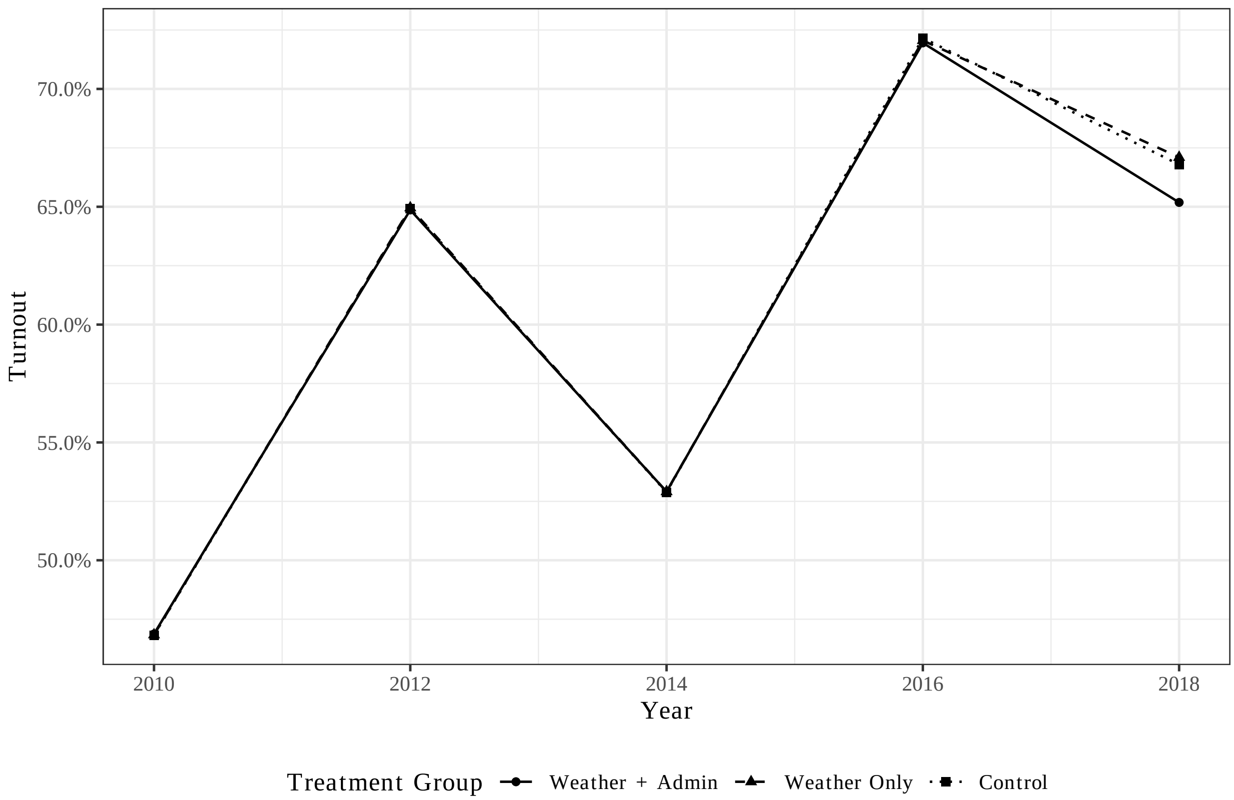
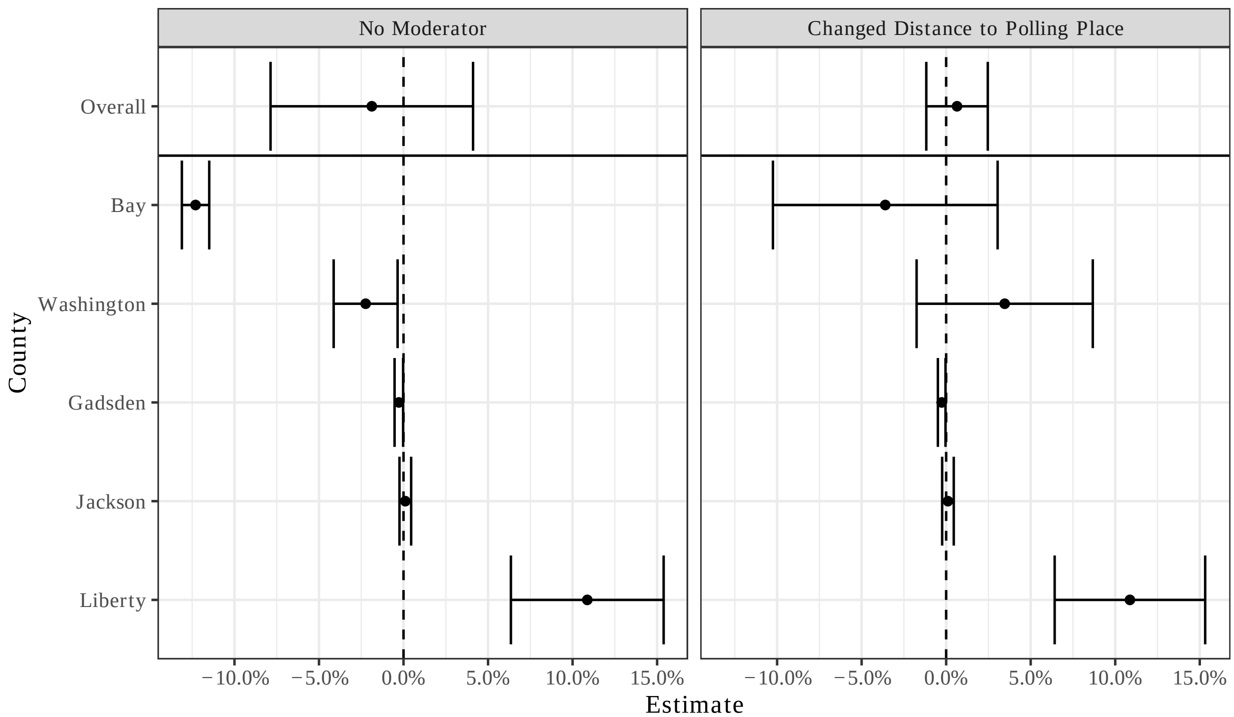


Figure 3: General Election Turnout for Untreated Voters, Voters Treated by Weather, and Voters Treated by Weather and Administrative Changes, 2010–2018

i's turnout (v) in year t is a function of the year and their location. In the equation, β_1 tests the weather effect for the voters treated by the hurricane's weather in 2018, and β_2 captures the estimated administrative effect of living in a county covered by the Executive Order, above-and-beyond the effect associated with the weather treatment. The matrices $\delta County_i$ and $\delta Year_t$ contain county and year fixed effects, respectively. The matrix δZ_i includes the measures for relative rainfall and polling place closures interacted with year, county, and treatment dummies.

In Figure 4 we present the results of these models, again fit using an ordinary least squares specification. The full table can be found in the Supplementary Information. In the left-hand panel, we present estimates of the administrative treatment effect without controlling for the treated voters' changed distance to their nearest polling place. The right-hand panel, meanwhile, shows the administrative effect after we control for this key variable. We show the overall estimated treatment effect for the administratively-treated counties as a whole at the top of each panel, followed by the estimated treatment effect for each individual county. The bottom panels are the result of single models, in which each county's estimated treatment effect is shown relative to a null hypothesis of zero treatment effect, rather than a null hypothesis of zero difference from the reference treated county. Robust standard errors are clustered at the individual, matched group, and county level.



Notes: Robust standard errors clustered by county on left and match on right.
Models include county and year fixed effects, and a dummy indicating weather−treated voters in 2018,
each interacted with a dummy indicating the administratively treated county. In the right panel,
changed distance to polling place is interacted with the other covariates to produce consistent estimates.

Figure 4: Estimated Adminsitrative Treatment Effects

Figure 4 makes a number of things clear. First, neither model estimates a statistically significant treatment effect for the treated counties *as a whole*, although 4 of the 5 individual counties' treatment effect is significantly different than 0 before controlling for polling place closures. This provides further corroboration for the notion that what mattered in the Panhandle in 2018 was how many polling places were consolidated in the face of the storm, *not* the Executive Order as a single, monolithic treatment with a consistent effect across the covered counties.

Secondly, as we move from the left-hand to right-hand panels, we see that the bulk of the administrative treatment is explained by the polling place consolidation. For Bay County, for instance, the point estimate is halved and the estimated effect is statistically nonsignificant once we account for the new distance some voters had to travel to the closest polling place. The administrative treatment effect actually becomes positive (though statistically nonsignificant) for Washington County once we control for the effect of these closed polling places, and the estimated effects in Gadsden and Jackson Counties remain minuscule (and, for Jackson, is statistically nonsignificant). The large effect in Liberty County likely reflects both the county's ability to keep polling places in this area open, and the relatively poor weather in the buffer.

Although Liberty County voters in the buffer were subjected to worse weather than any of the other buffer voters (rainfall for Liberty County voters was 229% of normal, compared with 131%, 140%, 155%, and 213% for the buffer voters in Washington, Jackson, Bay, and Gadsden Counties, respectively), the county kept all its polling places open. The presence of adverse weather may have created more space for the other administrative changes allowed under the Executive Order to "recoup" lost turnout due to the storm; indeed, as we show in the Supplementary Information, the weather treatment for the matched voters just outside of Liberty County was in fact severely depressed relative to voters elsewhere in the state.

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? Fujiwara and colleagues (2016) find rain disrupts the habit forming nature of voting, but do not consider convenience voting.

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 5 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 5 makes clear that the decline in turnout was a product of lower turnout on election day and via absentee voting, while it seems that early voting was higher in the treated counties due to Hurricane Michael, a finding similar to that of Stein (2015).

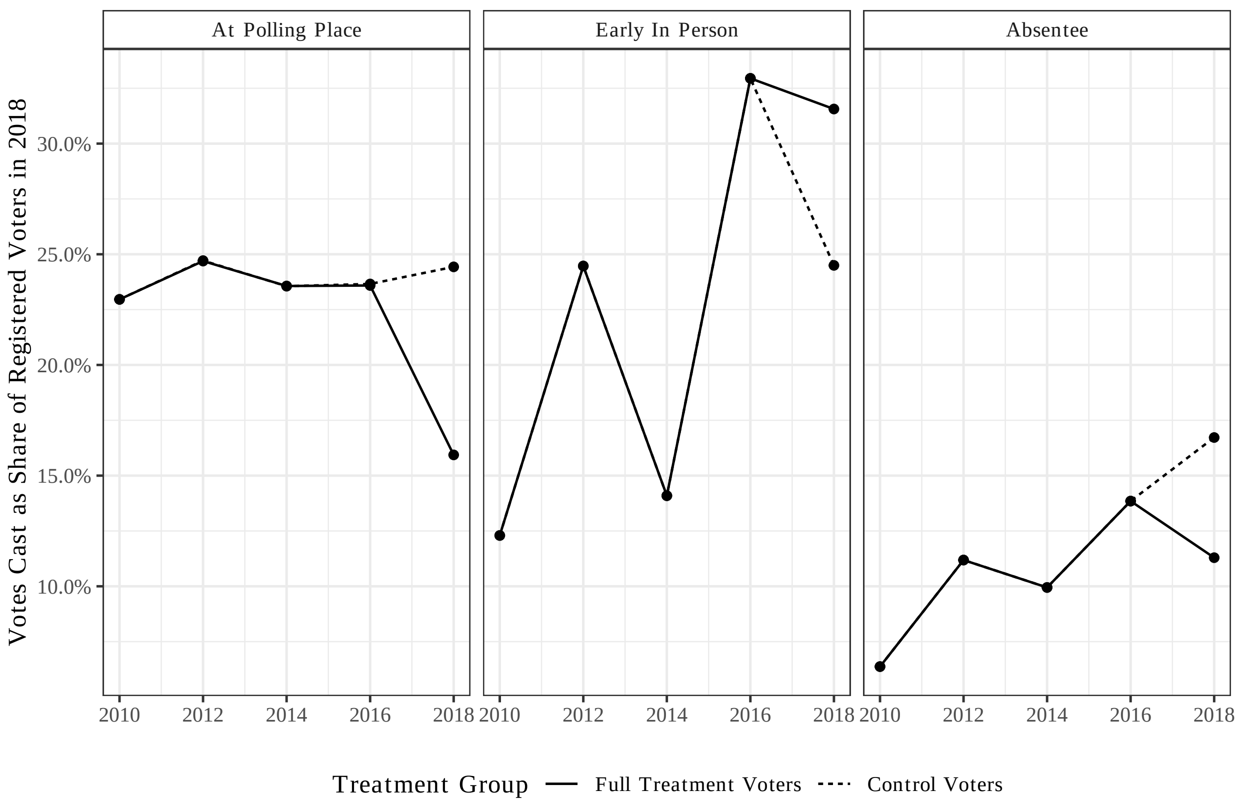


Figure 5: Average Marginal Effect of Hurricane Michael on Vote Mode

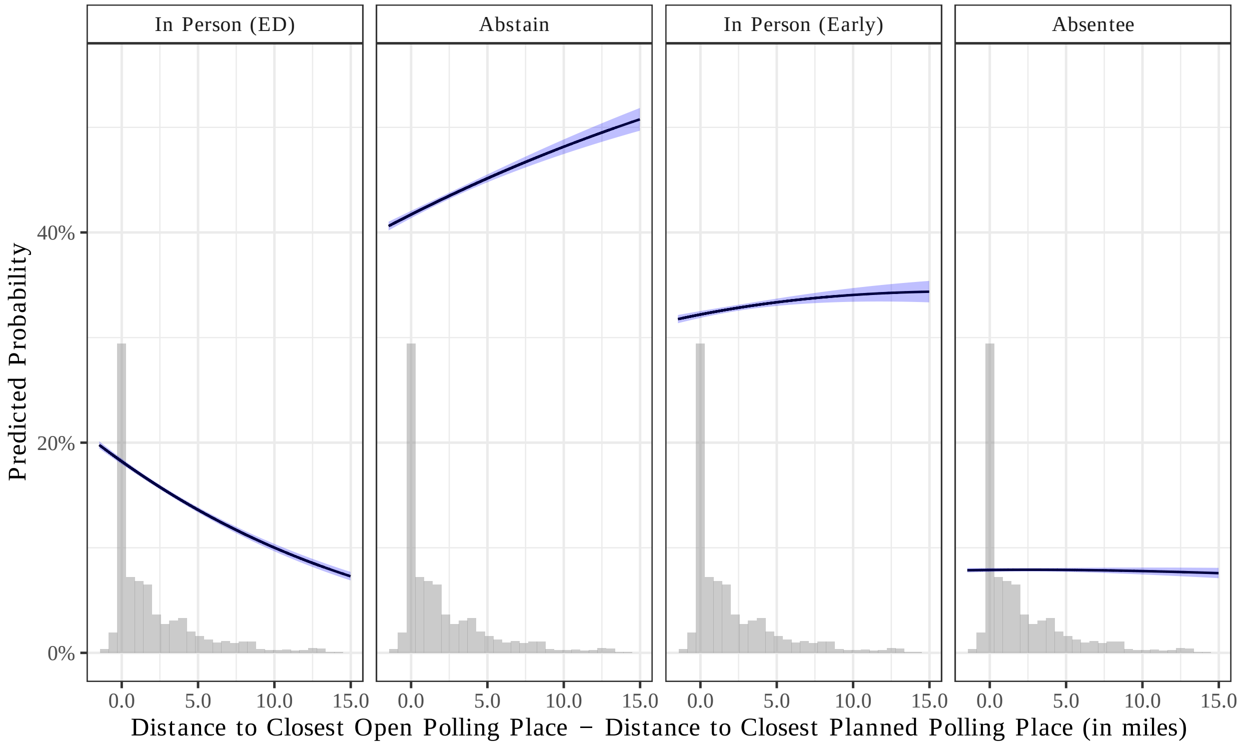
We use a multinomial logistic regression to directly test whether an increase in distance to the nearest polling place was related to vote-mode in 2018. In addition to the difference between expected and actual distance to the closest polling place, we include other covariates. We measure how far a voter 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 control for individual characteristics such as race, age, and partisan affiliation. We also include dummies indicating how (or whether) each voter participated in the 2010–2016 general elections. While we include all the voters in each of the covered counties, this set-up will primarily test effects in the counties that saw the most consolidation; voters in counties where few polling places were closed will see little-to-no difference between the planned and actual distance to a polling place.

Because the coefficients from the mulinomial logistic regression are difficult to interpret on their own, we include here the marginal effects plots from this model (the full regression table can be found in the Supplementary Information). Figure 6 presents the marginal effect of the change in distance to the nearest polling place on vote method while keeping all other covariates in the model at their means.

Figure 6 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 the state took steps to make early and mail voting easier, these efforts were overwhelmed by the widespread polling place closures.

Discussion and Conclusion

Election Day in the United States consistently falls near the end of hurricane season. 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



Notes: Distribution of changed distance to polling place shown at bottom.

Figure 6: Marginal Effect of Changed Distance to Polling Place on 2018 Vote Mode

weeks before the 2016 presidential election, killing dozens and causing more than $2.5 billion in damages. And in October of 2018—less than a month before the highest-turnout midterm election in a century—Hurricane Michael made landfall. 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 state and local responses are sufficient to avoid depressed turnout—is therefore vitally important, particularly in swing states such as Florida and North Carolina that are subject to severe coastal natural disasters.

The State of Florida took a gamble on the 2018 election. With polling places destroyed, something needed to be done. On the one hand, the state could have sent emergency funding to erect emergency polling places in tents or military trucks, as administrators did in the aftermath of Superstorm Sandy (Cooper 2012), or implement the sorts of drive-through options seen around the country in 2020 (Glickhouse 2020; McCullough 2020). Instead, the state allowed for major polling place consolidation and loosened mail voting laws, perhaps in an attempt to protect the franchise for voters who suddenly lived much further from their polling places.

As this paper demonstrates, Florida's response to Hurricane Michael was only somewhat effective: although Governor Scott allowed for 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 5). 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 cannot be attributed solely to the weather: even after decomposing the weather and administrative effects of the storm, we find there were substantial negative administrative effects where polling places were closed. In Liberty County, however—where polling places remained open and voters faced severe weather—we find a positive administrative effect, indicating that the Executive Order was effective when not joined with consolidation.

The data at hand cannot explain why the polling place closures resulted in such extensive turnout reductions, and why the loosened provisions granted under the Executive Order did not recoup these losses. 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.

Natural disasters cause immense disruptions in the lives of Americans, and these effects will only grow in the coming decades. Loss of life and loss of property are devastating enough—they should not be accompanied by the loss of the franchise as well. As this study demonstrates, election administrators can avoid inadvertently curtailing access to the ballot box by maintaining in-person voting options and easing other restrictions. Of course, maintaining planned levels of polling places requires extensive resources—resources that the State of Florida did not provide in the panhandle in 2018. Managing elections is a difficult job under even the best of circumstances; this is surely even more true in the fact of natural disasters. Nevertheless, this article joins a growing body of research articulating the central importance of keeping polling places open.

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