Authority After the Tempest: Hurricane Michael and the 2018 Elections

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June 13, 2021

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

[1] The category of the hurricane refers to the maximum sustained 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.

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. On the one hand, we find that voters who were subjected to worse weather turned out at lower rates after we control for the polling place consolidation of the county they lived in. We find, however, that the number of polling places a county eliminated had a much larger effect on turnout than the amount of rainfall voters experienced. In fact, at the very edges of the counties covered by the Executive Order we find no weather effect at all—but that the turnout of voters who lived just inside the covered counties was reduced on average by nearly 2 percentage points. 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 counties closed a majority of their polling places, 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 of county-level responses to the Executive Order allows us to precisely test the effects of these choices. 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. As the quote in the Introduction from professor emeritus Robert Montjoy makes evident, 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 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

[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.

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.

Polling Place Consolidation

Even as Floridians had access to widespread early in-person voting in 2018, Hurricane Michael and Executive Order 18-283 allowed for and effected major polling place consolidation in the covered counties. 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

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

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 choices made by local election administrators under the flexibility granted by the Executive Order.

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).

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 the choices made by election administrators udner to the discretion afforded by Executive Order 18-283. 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 leverage this heterogeneity to explore the effect of closed polling places on turnout, and expect the turnout effect of the storm was lower (that is, less negative) in the counties where more polling places were open. 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 and vote mode. Untreated voters who do not serve as matches are excluded from our models. Although it may seem counterintuitive to exclude data from our models, this matching procedure substantially improves the parallel trends assumptions necessary for a rigorous difference-in-differences analysis (Sekhon 2009, 496; 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 in counties that closed more polling places in response to the Executive Order, and where the relative rainfall was higher.

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 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.

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.

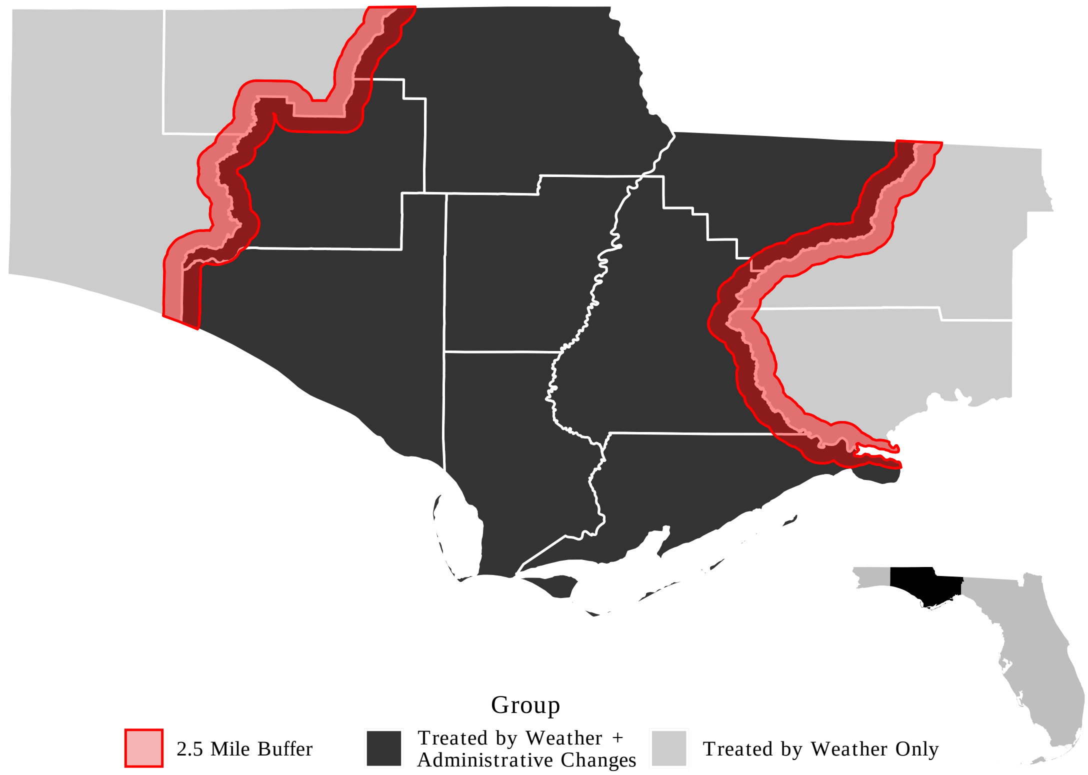


Figure 1: Treated and Control Counties with 2.5 Mile Buffer

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

Table 1: Treatment Status for Selected Voters

	Treatment Received	
Group	Administrative	Weather
Selected Voters in Covered Counties	Yes	Yes
Selected Voters in Uncovered Counties in Panhandle	No	Yes
Selected 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 the number of polling places each county consolidated, 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 matching, difference-in-differences, and a regression discontinuity, three powerful tools for establishing causality. 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. To 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, we begin by calculating how far each voter lived from the closest planned polling place, and how far she lived from the closest polling place that was actually open on election day. Using the registered voter file, we can tell not only *whether* a voter participated, but also *how* they participated. Using a multinomial logistic regression, we test whether the difference between the planned and actual distance-to-polling-place were associated with vote-mode in 2018. This specification allows us to test our final hypothesis:

Hypothesis 4: As the difference between the actual and planned distance to the closest polling place increased for voters, they were more likely to vote absentee and to abstain from voting, 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 nearest neighbor approach. We use a genetic algorithm to determine the weight each characteristic should receive for the matching procedure (Sekhon 2011).[6] 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

[6]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.

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.

Table 2: Balance Table for Statewide Matching

	Means: Unmatched Data		Means: Matched Data		Percent Improvement			
	Treated	Control	Treated	Control	Mean Diff	eQQ Med	eQQ Mean	eQQ Max
% White	76.5%	62.3%	76.5%	76.5%	100.00	100.00	100.00	100.00
% Black	17.1%	13.1%	17.1%	17.1%	99.82	99.82	99.82	99.82
% Latino	2.1%	17.4%	2.1%	2.1%	99.99	99.99	99.99	99.99
% Asian	1.0%	2.0%	1.0%	1.0%	100.00	100.00	100.00	100.00
% Female	52.5%	52.4%	52.5%	52.5%	100.00	100.00	100.00	100.00
% Male	45.8%	44.9%	45.8%	45.8%	99.95	99.95	99.95	99.95
Age	52.2	52.5	52.2	52.3	91.65	67.92	76.95	85.31
% Democrat	39.2%	37.1%	39.2%	39.2%	99.36	99.36	99.36	99.36
% Republican	43.6%	35.0%	43.6%	43.6%	99.51	99.51	99.51	99.51
% with Some College	69.0%	75.1%	69.0%	69.0%	99.62	98.76	97.97	91.59
Median Income	\$50,643	\$62,941	\$50,643	\$50,658	99.88	96.74	94.96	86.81

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

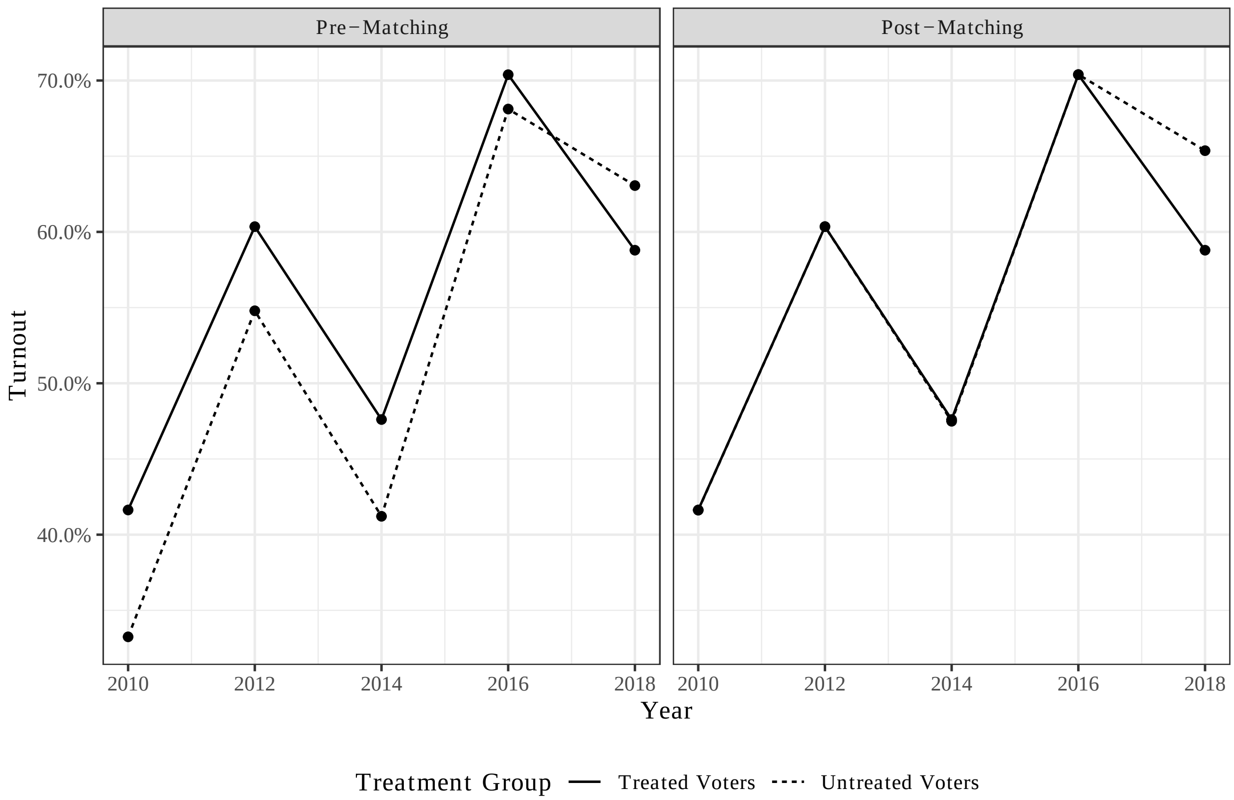


Figure 2: General Election Turnout for Treated and Control Voters, 2010 – 2018

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, *Treated × 2018* estimates the average marginal effect of Hurricane Michael on turnout for treated voters. Model 2 also 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 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, *Treated × 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 in counties where fewer polling places were open (*Treated × 2018 × Share of Expected Polling Places Open*). 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 county polling place 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. In each model, robust standard errors are clustered at the level of the match (Abadie and Spiess 2020).

Table 3: Turnout, 2010 — 2018

	Turnout				
	(1)	(2)	(3)	(4)	(5)
Treated	0.0003***	0.0004***	0.0005***	−0.00002	0.0001
	(0.00002)	(0.00004)	(0.00005)	(0.0002)	(0.0002)
2018	0.104***	0.104***	0.104***	0.189***	0.168***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)
Treated × 2018	−0.066***	−0.066***	−0.066***	−0.067***	−0.019***
	(0.001)	(0.001)	(0.001)	(0.005)	(0.006)
Treated × 2018 × Relative Rainfall in 2018				0.0004	−0.048***
				(0.003)	(0.003)
Treated × 2018 × Share of Expected Polling Places Open in 2018					0.124***
					(0.003)
Includes Other Matched Covariates		X	X		
Includes control for CD competitiveness			X		
Includes rainfall and its interactions				X	X
Includes share of polling places open and its interactions					X
Observations	5,925,990	5,925,990	5,925,990	5,925,990	5,925,990
R^2	0.004	0.167	0.167	0.005	0.008
Adjusted R^2	0.004	0.167	0.167	0.005	0.008

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

The coefficient on *Treated × 2018* in Table 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 6.6 percentage points. Multiplied across the nearly 200,000 registered voters in the treated counties indicates that some 13,000 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. 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 highly moderated by the share of polling places each county had to close. The estimated treatment effect ranges from -9.4 percentage points in Bay County (where 6 of 44 polling places were open, and the rainfall was 184% of normal) to a *positive* treatment of 4.7 percentage points in Franklin County, where 8 polling places were open compared to just 7 planned ones (and rainfall was just 120% of normal). As we demonstrate in the Supplementary Information, county-specific treatment effects corroborate the finding that polling place closures had a *far larger* effect on turnout than relative rainfall—and that there was apparently a positive AME in Franklin County. 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 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. We consider these differences sufficiently small to assume that, on average, paired voters were faced with identical weather effects.

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 present the plotted 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

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	73.4%	62.3%	73.4%	73.4%	100.00	100.00	100.00	100.00
% Black	22.6%	13.1%	22.6%	22.6%	99.91	99.91	99.91	99.91
% Latino	1.0%	17.4%	1.0%	1.0%	99.97	99.97	99.97	99.97
% Asian	0.3%	2.0%	0.3%	0.3%	100.00	100.00	100.00	100.00
% Female	53.1%	52.4%	53.1%	53.1%	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	53.3	52.5	53.3	53.3	97.77	67.15	78.41	80.01
% Democrat	45.6%	37.1%	45.6%	45.6%	99.95	99.95	99.95	99.95
% Republican	40.9%	35.0%	40.9%	40.9%	98.84	98.84	98.84	98.84
% with Some College	62.9%	75.1%	62.9%	62.9%	99.71	98.25	96.90	84.93
Median Income	$45,981	$62,941	$45,981	$45,917	99.63	93.69	89.36	75.56

and their controls.

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

$$\begin{aligned} v_{it} = \beta_0 + \beta_1 WeatherTreatment_i + \beta_2 2018_t + \beta_3 WeatherTreatment_i \times 2018_t + \\ \beta_4 AdministrativeTreatment_i + \beta_5 AdministrativeTreatment_i \times 2018_t + \\ \delta Y_{it} + \delta Z_i + \mathcal{E}_{it}. \end{aligned} \tag{1}$$

Individual i's turnout (v) in year t is a function of the year and their location. In the equation, β_1 *Weather Treatment*$_i$ measures the historical difference between voters treated by the hurricane's weather and the rest of the state. β_2*2018*$_t$ measures the change in turnout in 2018 from the baseline for control voters, while β_3*Weather Treatment*$_i$ $\times$ *2018*$_t$ tests the weather effect for the voters treated by the hurricane's weather in 2018. β_4*Administrative Treatment*$_i$ measures the historical difference between weather-treated voters who lived inside and outside the covered counties, and β_5*Administrative Treatment*$_i$ $\times$ *2018*$_t$ measures 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 matrix δY_i includes

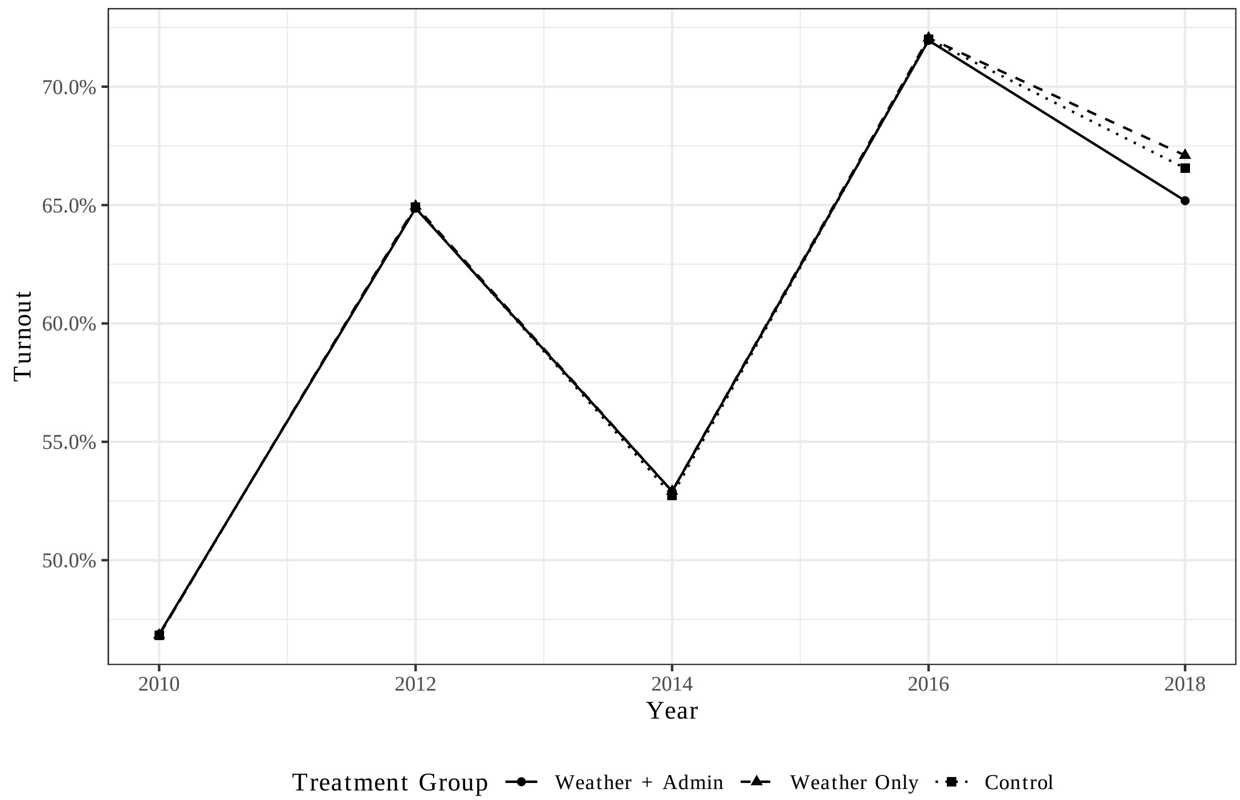


Figure 3: General Election Turnout for Untreated Voters, Voters Treated by Weather, and Voters Treated by Weather and Administrative Changes, 2010–2018

the measures for relative rainfall and polling place closures interacted with group and 2018 dummies. The matrix δZ_i includes the covariates used in the matching procedure, with latitude and longitude excluded.

Table 5 presents the results of these models, again fit using an ordinary least squares specification. Model 1 does not include δZ_i, while the matrix is included in Models 2 and 3. Model 3 also includes estimates for congressional district competitiveness in 2018. Finally, in Model 4, we once again investigate whether the treatment effects were moderated by polling place closures and relative rainfall. While the models include the full matrix δY_i, we display only rain and polling place closures' influence on the administrative treatment effects in the table for the sake of legibility. Robust standard errors are clustered at the level of the original voter receiving both treatments from which the others arise.

Table 5: Turnout, 2010 — 2018

	Turnout			
	(1)	(2)	(3)	(4)
Weather Treatment	0.001**	0.012***	0.013***	−0.00003
	(0.0003)	(0.002)	(0.002)	(0.002)
Administrative Treatment	−0.0004	−0.023***	−0.023***	0.003
	(0.001)	(0.003)	(0.003)	(0.004)
2018	0.074***	0.074***	0.074***	−0.096***
	(0.002)	(0.002)	(0.002)	(0.018)
Weather Treatment × 2018	0.005	0.005	0.005	−0.180***
	(0.004)	(0.004)	(0.004)	(0.033)
Administrative Treatment × 2018	−0.019***	−0.019***	−0.019***	0.065
	(0.006)	(0.006)	(0.006)	(0.046)
Administrative Treatment × 2018 × Relative Rainfall in 2018				−0.096**
				(0.039)
Administrative Treatment × 2018 × Share of Expected Polling Places Open in 2018				0.143***
				(0.040)
Constant	0.591***	−0.275***	−0.283***	0.594***
	(0.004)	(0.029)	(0.030)	(0.034)
Includes Other Matched Covariates		X	X	
Includes control for CD competitiveness			X	
Includes rainfall and its interactions				X
Includes share of polling places open and its interactions				X
Observations	473,220	473,220	473,220	473,220
R^2	0.004	0.160	0.160	0.013
Adjusted R^2	0.004	0.160	0.160	0.013

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

The coefficients on *Weather Treatment × 2018* and *Administrative Treatment × 2018* are of most substantive interest here. The coefficient on *Weather Treatment × 2018* indicates that there was no significant weather effect for voters at the very edges of the covered counties, Hurricane Michael notwithstanding.

There was, however, a negative *administrative* treatment effect for voters just inside the covered counties. *Administrative Treatment × 2018* in models 1–3 indicates that, for voters just inside the covered counties, turnout was depressed relative to their matches just across the county border by 1.9 percentage points.

Model 4 once again demonstrates that these effects were moderated by polling place consolidation and the strength of the storm—with polling place consolidation having a far larger impact. In this set of administratively treated voters, there is a negative relationship between polling place consolidation and relative rainfall. The average voter in the buffer in Bay County (where 6 of 44 polling places were open) saw rainfall 155% of normal; the average voter in Gadsden and Liberty Counties, where the expected number of polling places were open, saw rainfall that was 213% and 229% of normal, respectively. Multiplying out the coefficients from model 4 in Table 5 results in estimated average administrative treatment effects ranging from -6.4 points in Bay County to +0.35 points in Gadsden. Once again, we see that county-level polling place consolidation had a far larger influence on turnout than the storm itself.

It is important to remember that these effects reported in Table 5 are the treatment effect on the treated voters included in these models—that is, those at the outermost edges of the covered counties, and their nearby matches. Nevertheless, the average administrative effect of -1.9 percentage points is substantively quite large. Despite the efforts of Executive Order 18-283, the administrative costs imposed by Hurricane Michael meaningfully depressed turnout in counties where administrators used their discretion under the Executive Order to consolidate polling places. As model 4 indicates, however, the Executive Order may have *increased* turnout where counties were able to keep the bulk of their polling places open.

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 4 demonstrates the share of registered voters that cast a ballot either at the polling place, early in person, or absentee in each general election from the past decade. In each case, the denominator is the number of registered voters in 2018. Figure 4 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.

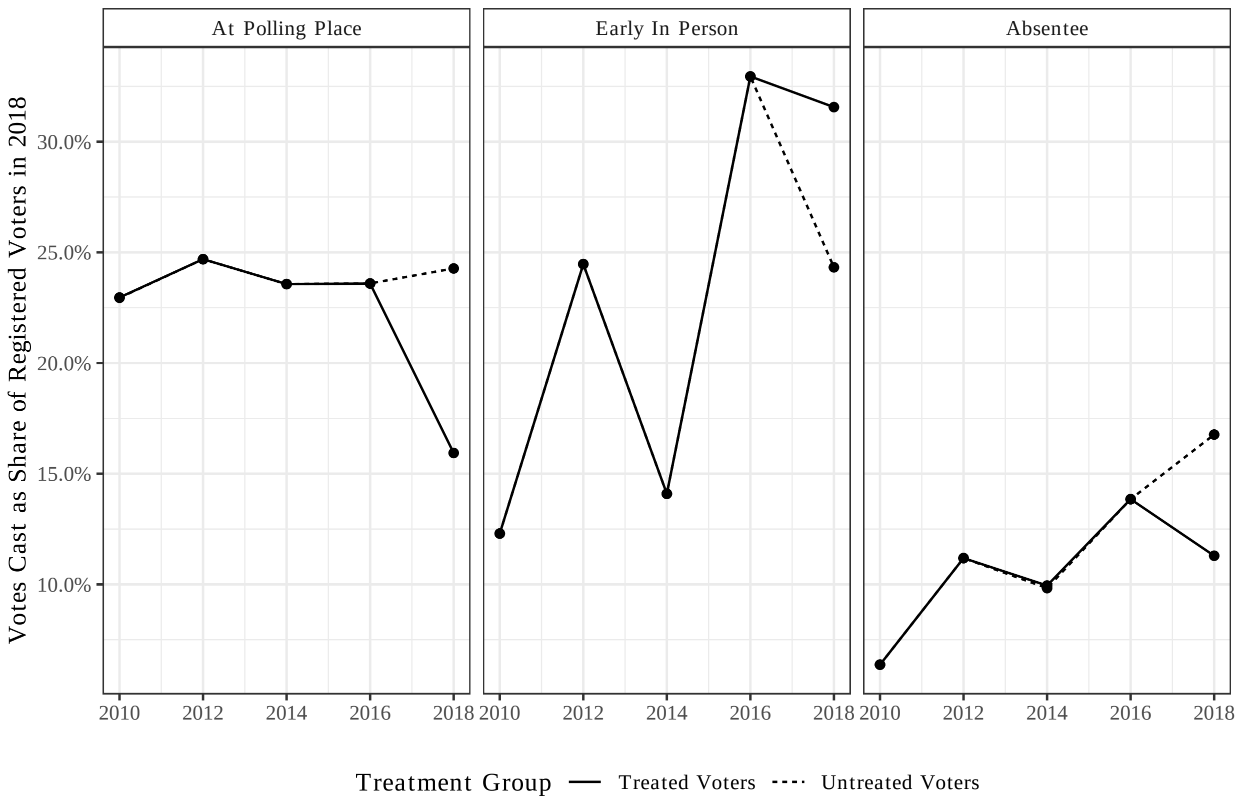


Figure 4: Average Marginal Effect of Hurricane Michael on Vote Mode

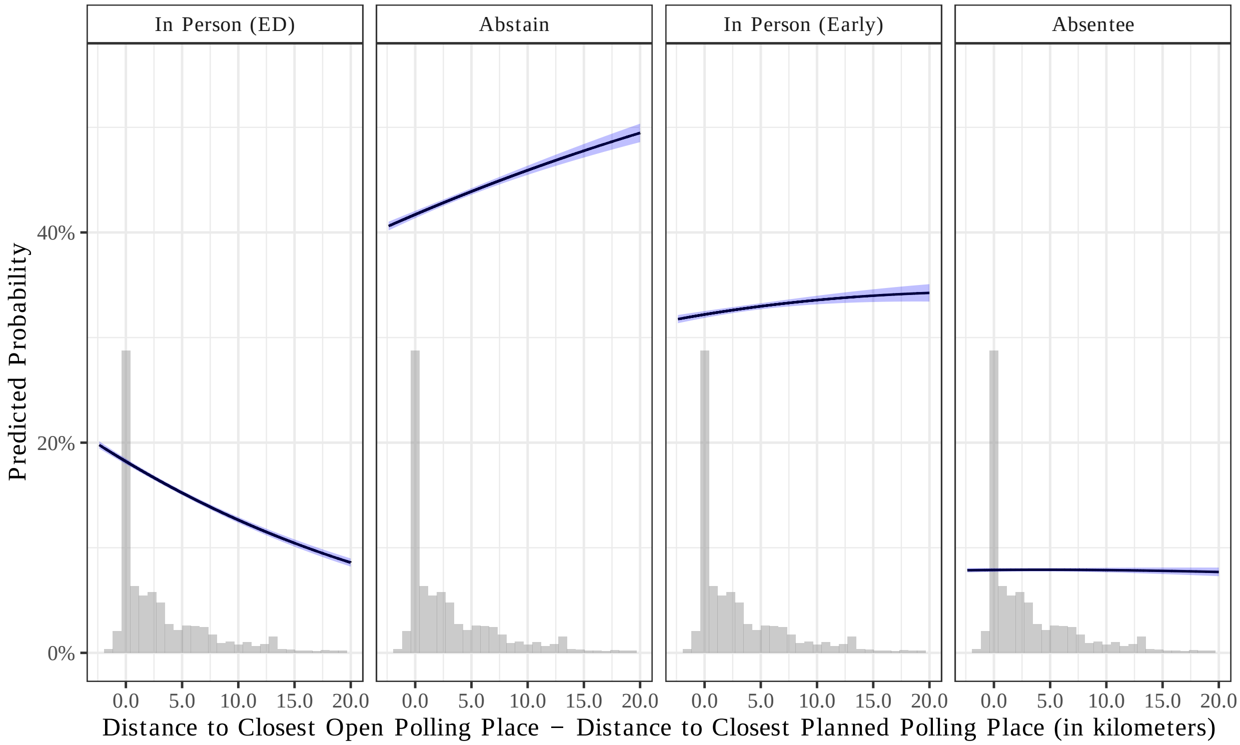
To more directly estimate the effect of Hurricane Michael and the polling place closures allowed under the Executive Order on vote-mode, we measure how far each treated voter lived from the closest planned polling place and the polling place that actually opened on election day. Using a multinomial logistic regression, we test whether increasing the difference between these distances was related to vote-mode or abstention 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 5 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 5 indicates that, as voters suddenly had to travel further to the nearest polling place, they were substantially less likely to vote in person on election day ("In Person (ED)"). The bulk of these voters *did not* shift to absentee voting or early in-person voting; rather, they were much more likely to abstain from casting a ballot at all. Thus, although administrators took steps to make early and mail voting easier, these efforts were not particularly effective at offsetting the costs associated with polling place consolidation.

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 weeks before the 2016 presidential election, killing dozens and causing more than $2.5 billion



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

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

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 election administrators' 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.

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 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 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 for the region as a whole.

This overall administrative effect, however, masks considerable heterogeneity at the county level. Counties that did not close their polling places saw negligible or even positive turnout effects. These results demonstrate the importance of polling place locations, even in the context of permissive convenience voting. Loosening restrictions on where mail ballots could be sent and how they could be returned had little effect on the use of mail ballots in the election in counties with major polling place closures. Without the Executive Order, polling places would still have been moved because some had been destroyed, but the discretion granted to reduce the number of polling places apparently substantially reduced turnout. Thus, the Executive Order likely increased the administrative costs of voting where polling places were closed.

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. 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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Supplementary Information

Contents

Changes in Covered Counties

Table A1: Changes in Covered Counties

	Polling Places			Early Voting Days		
County	Actual	Expected	Share Open	2018	2016	Change
Bay	6	44	13.6%	10	9	1
Calhoun	6	6	100.0%	15	13	2
Franklin	8	7	114.3%	10	8	2
Gadsden	25	25	100.0%	15	13	2
Gulf	2	10	20.0%	10	8	2
Jackson	3	14	21.4%	10	13	-3
Liberty	7	7	100.0%	13	13	0
Washington	4	12	33.3%	8	13	-5

Impact on Registrations

As discussed in the body of this paper, our estimates all test the effect of the hurricane on turnout as a share of registered voters. This probably leads to an underestimation of the treatment effect. As Figure A1 makes clear, the number of registrations in the weeks before the election in the treated counties was substantially lower than we might have expected based on the rest of the state.[1] Because our estimates exclude the individuals who would have registered and voted in the absence of the storm, our estimated treatment effects are likely highly conservative.

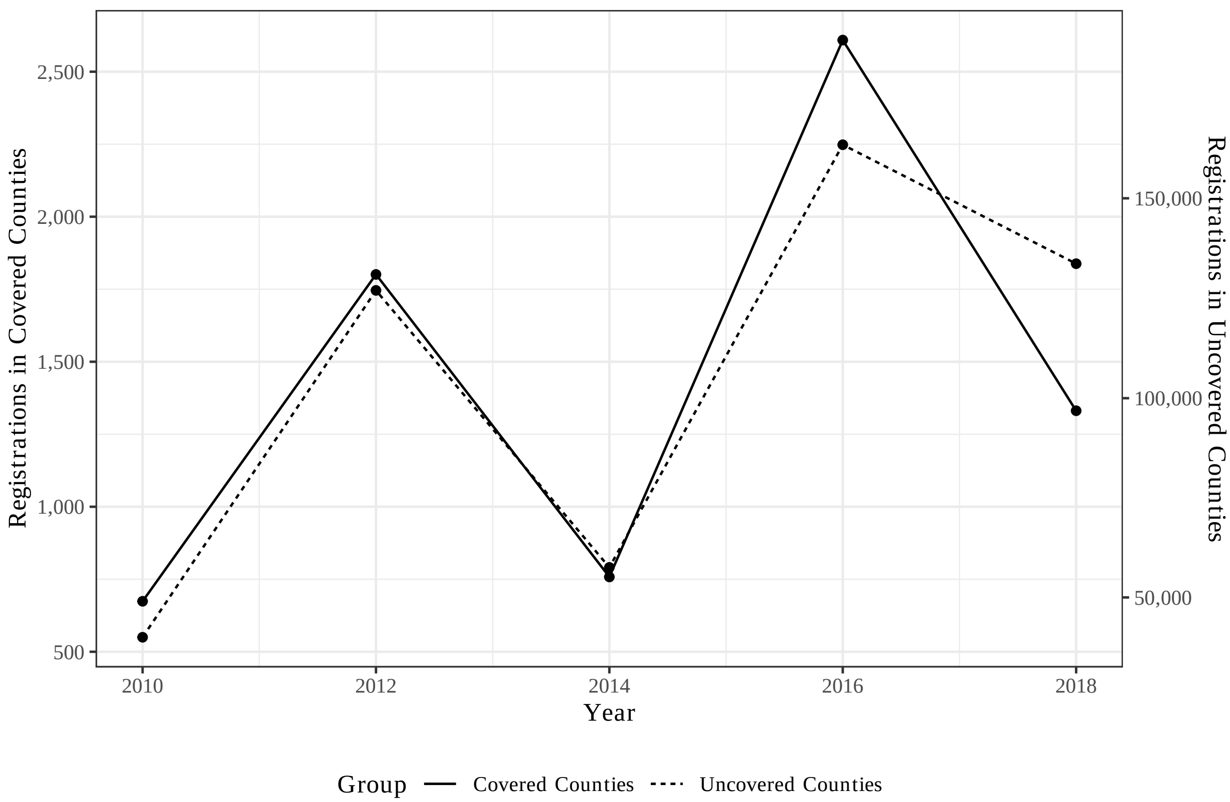

Figure A1: Registrations in Final Weeks Before Election

[1]Because the storm impacted the registration deadline in some of the treated counties in 2018, we plot the total number of registrations in the 5 weeks prior to election day each year.

Alternative Processing Approaches for AME

In the body of the paper, we use nearest-neighbor matching and a genetic weighting process. Here, we demonstrate that our primary results are robust to a variety of different preprocessing approaches.

In model 1 of Table A4 we do not process the data in any way before running a difference-in-differences model. In other words, every treated voter and potential control voter is included once, and all voters receive a weight of 1. This is a formalization of the left-hand panel of Figure 2 in the body of the paper. In model 2, we present this same specification but with county-specific time trends. Model 3 presents the primary model from the body of this paper, but with county linear time trends.

In model 4, we use an approach called entropy balancing (Hainmueller 2012). In this approach, every treated voter is given a weight of 1, while every control voter receives a unique weight based on their sociodemographic characteristics and past turnout history. Balancing is done using the same covariates used for the primary match in the body of the manuscript.

In model 5, we use propensity score matching (Caliendo and Kopeinig 2008). Each voter's propensity score is calculated using the same covariates as in the body of the paper. After estimating each voter's propensity score, we use a nearest-neighbor matching approach. Each treated voter is matched with 5 controls. Matching is done with replacement, and ties are randomly broken.

In model 6, we match treated voters to 5 controls using only individual-level characteristics (race, gender, party affiliation, age, and historical turnout). Control voters must exactly match their treated voters; treated voters who do not exactly match any control voters are dropped. Once again, matching is done with replacement, and ties are randomly broken.

As a reminder, the estimated treatment effect from the body of the paper was -6.6 percentage points. Table A4 makes clear that our results are robust to a variety of preprocessing and weighting approaches. Entropy balancing and propensity score matching return estimated effects within 0.1 percentage points of our primary models, as does the matched model including county-linear time trends. Exact matching and unprocessed difference-in-difference approaches return substantially larger treatment effects.

Table A2: Alternative Processing Approaches

	Unprocessed	Unprocessed	Primary Model	Entropy Balancing	Propensity Score	Exact Match
			voted		to	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.043^{***}	23.928^{***}	9.900^{***}	0.000	0.001^{***}	0.014^{***}
	(0.001)	(1.310)	(1.685)	(0.0001)	(0.0004)	(0.0004)
2018	0.137^{***}	-0.090^{***}	-0.080^{***}	0.105^{***}	0.105^{***}	0.123^{***}
	(0.0001)	(0.0002)	(0.001)	(0.0002)	(0.001)	(0.001)
Treated × 2018	-0.099^{***}	-0.055^{***}	-0.066^{***}	-0.067^{***}	-0.067^{***}	-0.081^{***}
	(0.001)	(0.002)	(0.001)	(0.0003)	(0.001)	(0.001)
Constant	-0.282^{***}	-94.677^{***}	-80.717^{***}	-0.300^{***}	-0.308^{***}	-0.377^{***}
	(0.001)	(0.364)	(1.525)	(0.001)	(0.002)	(0.002)
Includes Matched Covariates	X	X	X	X	X	X
County Fixed Effects		X	X			
County-Linear Time Trends		X	X			
Observations	60,041,820	60,041,820	5,925,990	60,041,820	5,925,990	5,773,440
R^2	0.152	0.188	0.193	0.161	0.161	0.175
Adjusted R^2	0.152	0.188	0.193	0.161	0.161	0.175

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

County-Specific Effects

In the body of this paper, Figure 2 presents the overall pre- and post-treatment trends for treated and control voters. However, lumping each of the treated counties together masks considerable heterogeneity. In Figure A2 we plot the unprocessed and matched turnout trends for treated and control voters, broken out for each of the 8 treated counties. Figure A2 makes clear that the treatment effect varied substantially by county.

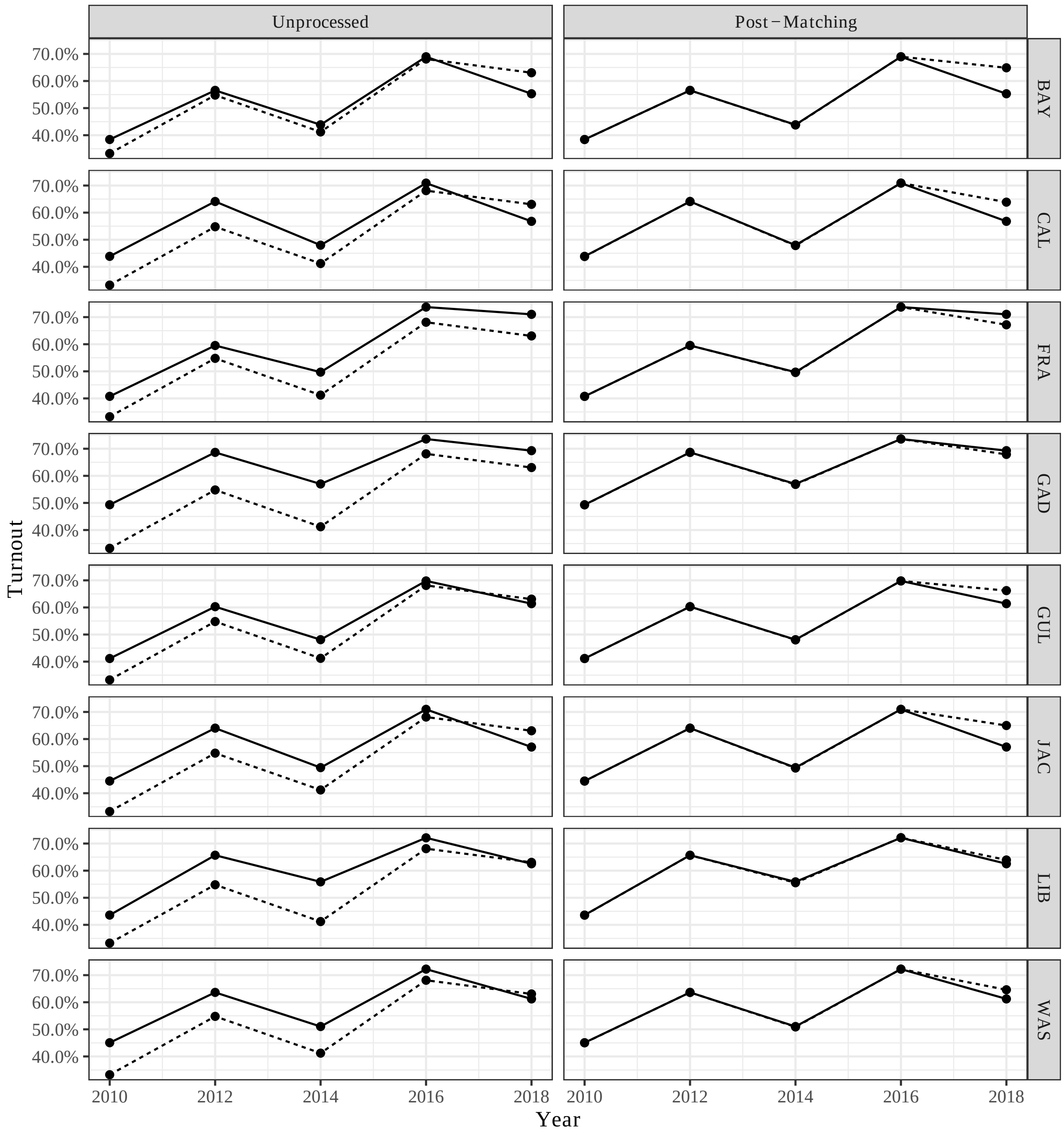

Figure A2: Pre- and Post-Matching County Plots

Table A3 re-estimates model 1 from Table 2 in the body of the paper, but interacts the treatment term with each of the treated counties. This allows us to measure the difference in treatment effect for each county. The reference category in Table A3 is Bay County.

Table A3: Turnout, 2010 — 2018

	Turnout
Treated	0.0002^{***}
	(0.00003)
2018	0.130^{***}
	(0.001)
Treated × 2018	-0.096^{***}
	(0.001)
Treated × 2018 × Calhoun	0.025^{***}
	(0.005)
Treated × 2018 × Franklin	0.134^{***}
	(0.005)
Treated × 2018 × Gadsden	0.109^{***}
	(0.003)
Treated × 2018 × Gulf	0.048^{***}
	(0.005)
Treated × 2018 × Jackson	0.016^{***}
	(0.003)
Treated × 2018 × Liberty	0.081^{***}
	(0.007)
Treated × 2018 × Washington	0.062^{***}
	(0.004)
Includes Fixed Effects for Treated County	X
Includes Fixed Effects for Treated County interacted with 2018	X
Observations	5,925,990
R^2	0.010
Adjusted R^2	0.010

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

As discussed in the body of the paper we argue that the treatment effects are largely moderated by the number of polling places each county kept open, and that these effects were larger than the relative rainfall. In Figures A3 and A4, we plot each county's estimated treatment effect from Table A3 against the relative rainfall experienced by the average voter in each county, and share of polling places that county kept open. The line of best fit is weighted by the number of registered voters in each county. The relationship is clear: while there is virtually no relationship between county rainfall and the estimated treatment effect ($R^2 = 0.03$), the treatment effect was much larger in counties where more polling places were closed ($R^2 = 0.80$).

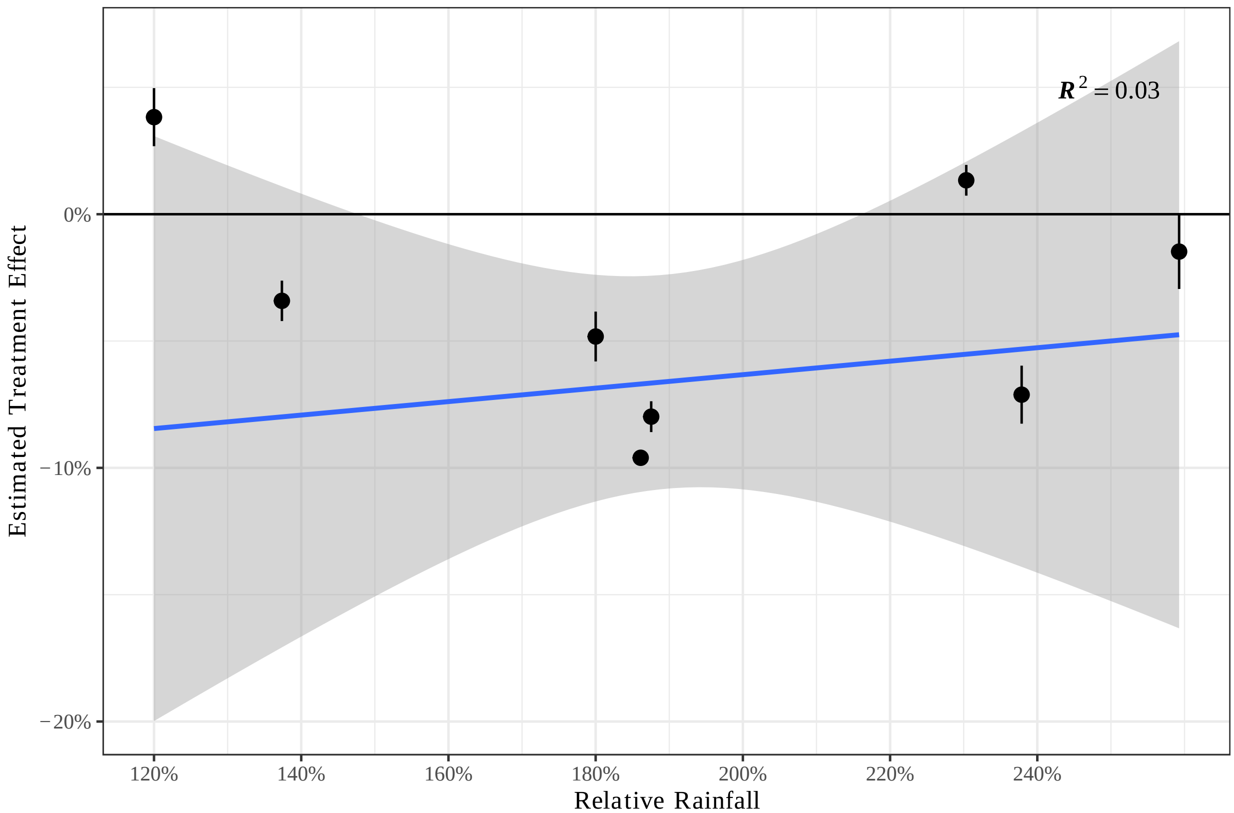

Figure A3: Relationship Between County Treatment Effect and Relative Rainfall

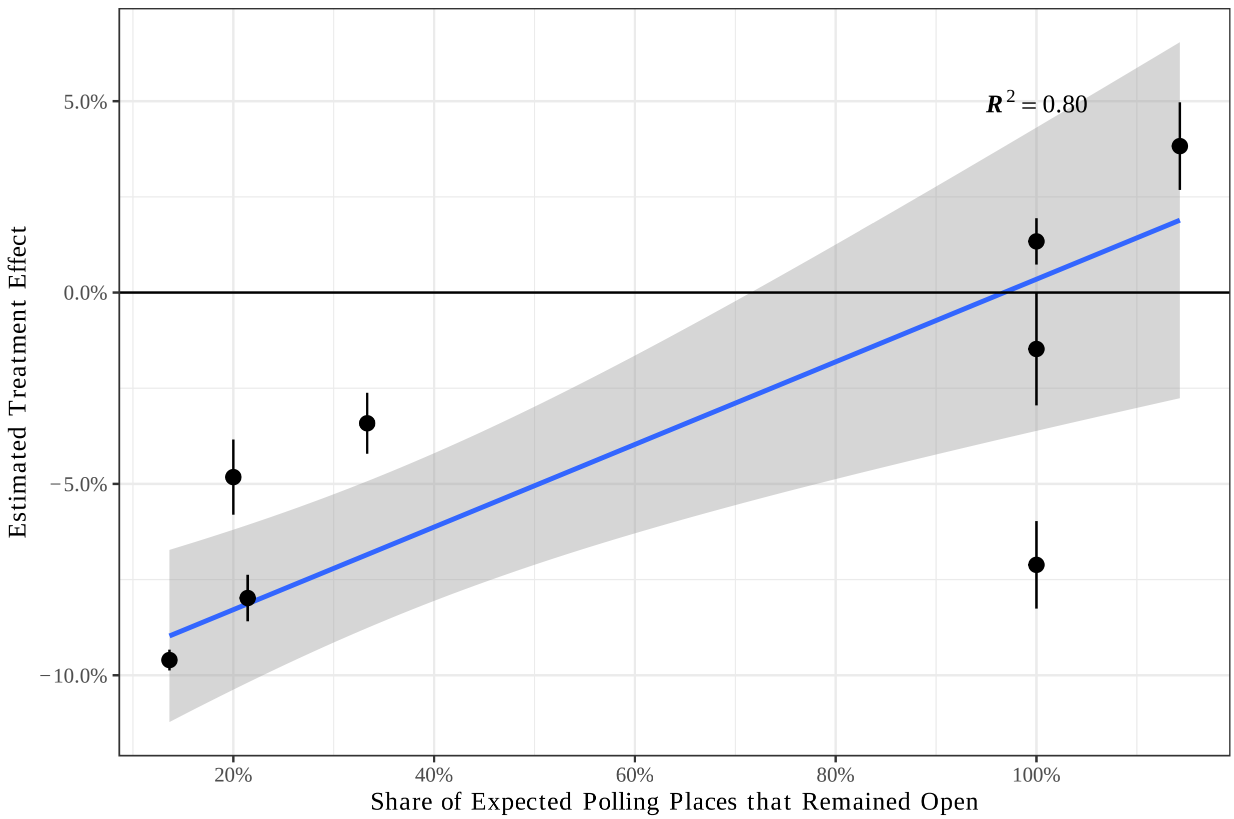

Figure A4: Relationship Between County Treatment Effect and Share of Polling Places Open

Alternative Processing Approaches for Triple-Differences Model

In the body of this manuscript we match pairs of voters on either side of the administrative county borders in the Florida panhandle to identify the administrative effect of the hurricane. Our pool of voters treated by the administrative and weather effects live within 2.5 miles of a county not covered by the Executive Order, while potential controls—that is, voters treated only by the weather—live within 2.5 miles of a covered county. Each voter in each pair is then matched with 5 voters elsewhere in the state.

Here, we show that our primary results hold even when we include *all* voters who live within

2.5 miles of a covered county, and all untreated voters anywhere. In model 1 in Table **??**, we present the results of the unmatched model that does not include a matching procedure. This model includes all voters in the state *except* for voters in counties covered by the Executive Order who do not live within 2.5 miles of an uncovered county, and voters in the adjacent, uncovered counties who do not live within 2.5 miles of a county covered by the Executive Order. Model 2 includes a county-linear time trend, and model 3 re-estimates our matched model from the body of the paper with a county-linear time trend.

Models 1 and 3 both continue to identify a negative administrative treatment effect very similar to that included in the body of the paper. Model 2, however, which includes all voters and a county-linear time trend, estimates a nonsignificant administrative treatment effect. Although each model would ideally estimate comparable treatment effects, the benefits provided by the matching procedure ensure that each voter treated only by the weather is similar in terms of past turnout, geography, and rainfall is far superior to one that includes all voters in the buffer treated only by the weather. We conclude that our model is robust to alternative specifications as presented in models 1 and 3, and that model 2 does not invalidate the primary estimates in the paper.

In Figure A5 we break out the trends for each of the treated counties' turnout, turnout among voters who were treated only by the weather, and voters elsewhere. In the left-hand panel we present the turnout of all voters; in the right-hand panel, we plot the turnout of the selected weather-only and control voters for each county. As a reminder, both Calhoun and Gulf Counties are entirely surrounded by other counties covered by the Executive Order, and no registered voters in Franklin County live within 2.5 miles of Wakulla, the nearest county not covered by the Executive Order. As such, these 3 counties are not included.

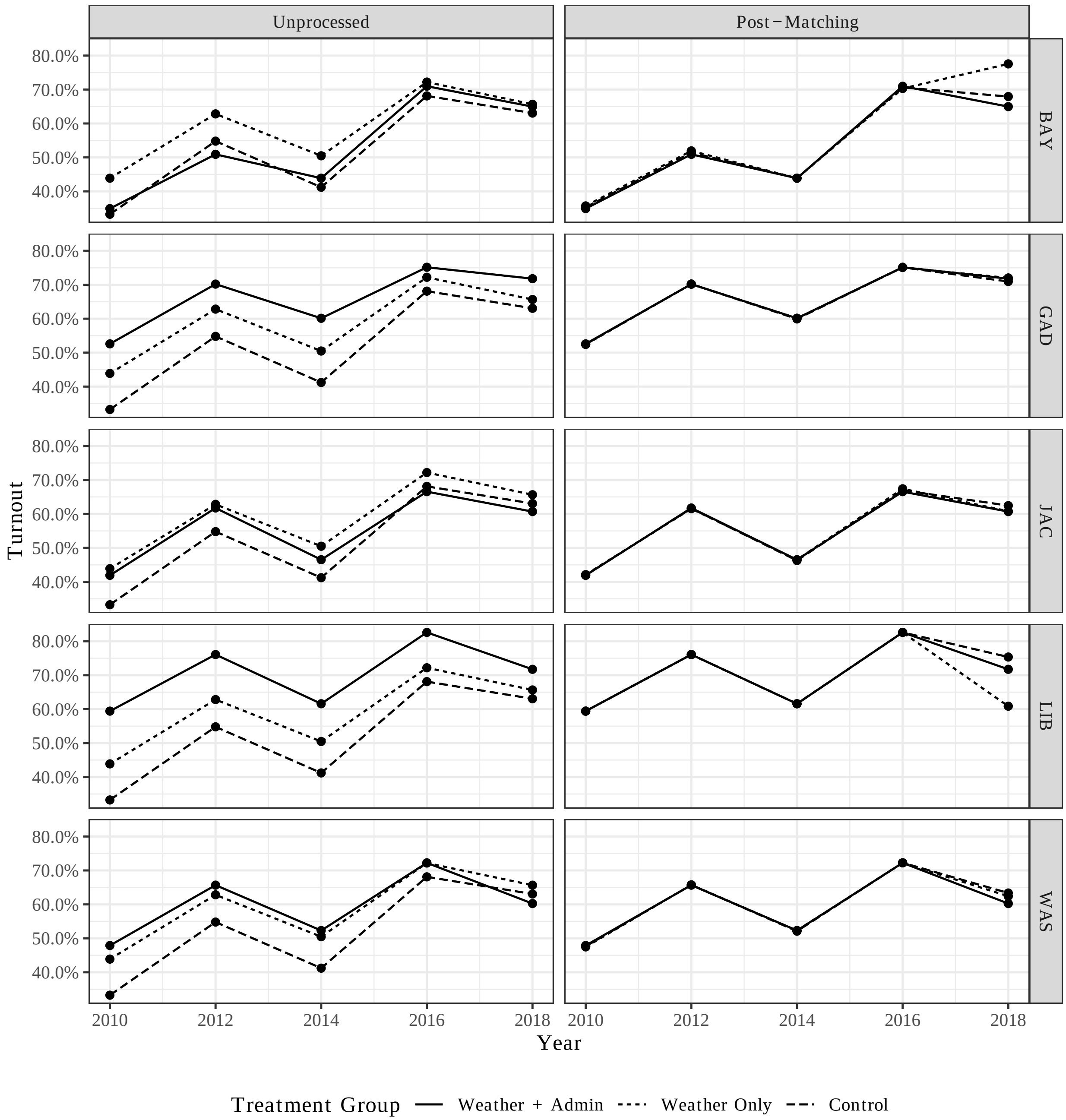

Figure A5: Pre- and Post-Matching County Plots

Figure A5 presents visual corroboration for what we find in the body of the paper—namely, that counties with more closures saw negative administrative treatment effects. The negative administrative treatment effect in Bay County is clearly quite large, while the positive administrative treatment effect is clear for Liberty County. Somewhat surprisingly, the weather-only matched voters for Bay County saw substantially higher turnout in 2018 relative to the controls elsewhere in the state; however, as discussed in the body of this paper, the weather was relatively mild at the edges of Bay County. Meanwhile, weather-treated voters just outside of Liberty County were subjected to the worst weather of the group; their turnout was evidently severely depressed, although the administrative effect in Liberty mitigated much of this drop. In each county, the matching procedure substantially improves the reasonableness of the parallel trends assumption necessary for valid causal inference.

Multinomial Regression Table

In Figure 5 in the body of the paper, we show the marginal effects plot based on a mulinomial logistic regression. Because those coefficients can be difficult to interpret on their own, we have included the regression table here. While the coefficients have been exponentiated in this table, the standard errors have been left unadjusted.

Table A5: Vote Mode in 2018 (Relative to In-Person on Election Day)

	Abstain (1)	Early (2)	Absentee (3)
Change in Distance to Polling Place (km)	1.047***	1.042***	1.037***
	(0.001)	(0.002)	(0.002)
Distance to Closest Planned Polling Place (km)	0.974***	0.940***	1.001
	(0.003)	(0.003)	(0.002)
White	0.955	1.036	0.953
	(0.043)	(0.047)	(0.064)
Black	0.664***	1.051	0.900
	(0.047)	(0.050)	(0.069)
Latino	0.957	0.871*	0.847
	(0.066)	(0.074)	(0.105)
Asian	1.251**	1.182*	1.080
	(0.091)	(0.097)	(0.135)
Male	0.965**	1.015	0.993
	(0.015)	(0.015)	(0.021)
Democrat	0.802***	0.807***	1.147***
	(0.024)	(0.026)	(0.037)
Republican	0.649***	1.242***	1.146***
	(0.023)	(0.024)	(0.036)
Age	1.001**	1.011***	1.025***
	(0.0005)	(0.0005)	(0.001)
Constant	0.429***	0.300***	0.011***
	(0.056)	(0.060)	(0.086)
Includes vote-mode in 2010, 2012, 2014, and 2016		X	
Number of Observations		197,533	
McFadden Pseudo R2		0.271	

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Standard errors in parentheses.

References

Caliendo, Marco, and Sabine Kopeinig. 2008. "Some Practical Guidance for the Implementation of Propensity Score Matching." *Journal of Economic Surveys* 22 (1): 31–72. https://doi.org/10.1111/j.1467-6419.2007.00527.x.

Hainmueller, Jens. 2012. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies." *Political Analysis* 20 (1): 25–46. https://doi.org/10.1093/pan/mpr025.

Table A4: Alternative Processing Approaches

	Unprocessed	Unprocessed Turnout	Primary Model
	(1)	(2)	(3)
Weather Treatment	0.084***	29.647***	−20.487***
	(0.002)	(5.144)	(6.885)
Weather Treatment × 2018	−0.054***	−0.008	0.004
	(0.004)	(0.006)	(0.006)
Administrative Treatment	−0.002	13.638**	37.483***
	(0.003)	(6.194)	(2.928)
Administrative Treatment × 2018	−0.023***	0.001	−0.018***
	(0.007)	(0.010)	(0.006)
2018	0.137***	−0.090***	−0.084***
	(0.0001)	(0.0002)	(0.003)
Constant	−0.283***	−94.677***	−68.443***
	(0.001)	(0.364)	(6.532)
Includes Matched Covariates	X	X	X
County Fixed Effects		X	X
County-Linear Time Trends		X	X
Observations	59,167,655	59,167,655	473,220
R^2	0.152	0.188	0.188
Adjusted R^2	0.152	0.188	0.188

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