Authority After the Tempest: Hurricane Michael and the 2018 Elections*

Short Title: 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 issued Executive Order 18-283, allowing election officials in 8 impacted counties to loosen a variety of voting laws and consolidate polling places but providing no emergency funding to maintain the planned number of polling places. We test the efficacy of the order using a novel research design that separates the weather effects of the hurricane on turnout from the administrative effects of how the election was run. We find little evidence that the hurricane itself (as proxied by historically-relative rainfall) reduced turnout, but that the Executive Order likely had large, negative turnout effects thanks to widespread polling place consolidation. Natural disasters need not spell turnout disasters if state and local election officials can avoid reducing the number of polling places.

Keywords. voter turnout; election administration; climate change; Hurricane Michael; voting behavior; Florida

Supplementary material for this article is available in the appendix in the online edition.

Replication files are available in the *JOP* Dataverse (https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the *JOP* replication analyst.

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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 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 before the general election. The Executive Order covered Bay, Calhoun, Franklin, Gadsden,

[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.

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, the state did not provide 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 outlays. The state's Executive Order took a different approach by allowing for these polling places to be closed, but attempting to offset these inconveniences by loosening restrictions on mail and early voting.

This paper sets out to answer a number of questions: what was the total depressive effect of the hurricane on turnout? 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 approach 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 received neither an administrative nor weather treatment from Hurricane Michael—we decompose the dual effects of the hurricane on turnout.

Our results paint a complex picture. While we do not find evidence that the amount of rainfall from the hurricane drove turnout declines, we do find that polling place closures and increased travel distances meaningfully depressed turnout; each additional mile a voter had to travel was associated with a decrease in turnout of between 0.6 points (for the region as a whole) and 1.1 points (for voters at the edges of the covered counties). We show that turnout declines were concentrated among voters who would otherwise have voted by mail or in person on election day; conversely, early in-person voting was actually higher in 2018 as a result of the hurricane. 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; Gori et al. 2022). 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 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 inconvenient 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 the authors

[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.

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 turnout effects of two hurricanes making landfall proximate to elections: Typhoon Lan in the 2017 House of Representatives elections in Japan and Superstorm Sandy[5] in the Northeastern US in 2012. Lan 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, the observed behavioral responses indicate that the availability of early voting allowed some voters to participate who might not have as the weather got worse.

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 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. Whatever questions remain about the impact of early in-person voting on turnout in normal times, it may 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 destroyed polling places across the region, and the Executive Order allowed ad-

[5]Lan was the equivalent of a Category 4 hurricane, featuring wind speeds of between 130 and 156 miles per hour. Sandy was a Category 3 hurricane with wind speeds between 111 and 129 miles per hour.

ministrators 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.

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 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 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 (2022) 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 2022, 609). 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.

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 increased early and absentee 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). The devastation in areas like Bay County following from the hurricane made it impossible to deploy the planned number of polling places based on resources allocated to the county for the purposes of administering the election.

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 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 ("SI"), 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 in the covered counties. 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. The L2 data is based on the February 8, 2019, version of the raw voter file; we pull self-reported race / ethnicity and sex from the same raw file.

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) estimates daily rainfall at some 13,000 geographical points around the United States. We use the `rnoaa` (Chamberlain 2021) package in `R` 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 point in the country. Voters' individual exposure to rainfall is calculated as the average of the three closest weather points, inversely weighted by distance.[6]

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; the Executive Order itself was issued just 4 days before early voting began), 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 SI we include a table detailing the number of

[6] It is important to note that using rainfall as a proxy for hurricane strength fails to account for other devastating phenomena associated with Hurricane Michael, such as storm surges or infrastructure damage. Unfortunately, precise data on these phenomena are unavailable at a fine-grained level; the literature instead looks to emergency declarations (e.g. Stein 2015) or rainfall (Kitamura and Matsubayashi 2021). We thus use relative rainfall in conjunction with the designation as "covered" by the Executive Order in our analyses, though we are aware that these do not fully capture geographical heterogeneity in devastation of the storm.

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 that of voters elsewhere in the state, we can estimate the AME of the storm on turnout in these counties. 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, vote mode, and registration date. 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. In the SI we show that our results do not change if we restrict the pool to treated voters registered prior to the 2010 election and their controls. Untreated voters who do not serve as matches are excluded from our models. Although it may seem counterintuitive to exclude data, this matching procedure substantially improves the plausibility of the parallel trends assumptions (Sekhon 2009, 496). As we show in the SI, our estimated AME is robust to a variety of different pre-processing and modeling choices. This design allows us to test our first hypotheses:

Hypothesis 1: Turnout among voters in the eight treated counties was depressed in the 2018 election relative to voters in untreated counties.

Hypothesis 1a: The negative AME will be larger where voters saw more relative rainfall.

Hypothesis 1b: The negative AME will be larger where voters had increased travel distances imposed due to polling place closures.

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 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 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. To strengthen the plausibility that these two sets of voters were identically treated by the weather, we match on each voter's relative rainfall. Geographic proximity is ensured by also matching on latitude and longitude.

By comparing the 2018 turnout of these voters, we 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 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 an 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.

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.

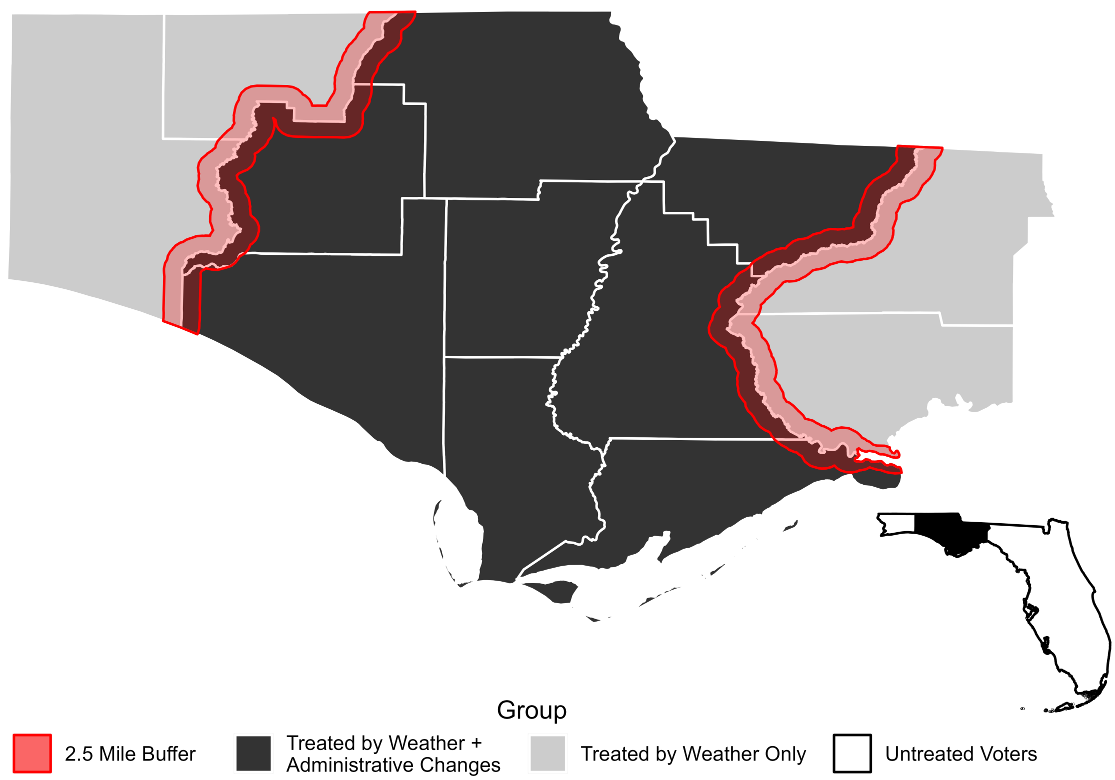

Figure 1: Treated and Control Counties with 2.5 Mile Buffer

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

The double-matched triple-differences model allows us to test hypotheses two, three, and four:

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

Hypothesis 3: We expect a positive administrative treatment effect for voters who received both weather and administrative treatments *when they did not have to travel further to the nearest polling place.*

Hypothesis 4: In contrast, we expect negative administrative treatment effects for voters who had to travel further to the nearest polling place (Morris and Miller 2022). In other words, we expect that increased travel distances overwhelmed any potential positive effects arising from loosened mail and early voting rules.

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 SI, our estimatation of the administrative treatment effect is robust to specifications including 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 administratively 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 a multinomial logistic regression, we test whether the difference between the planned and actual distance-to-polling-place was associated with vote-mode in 2018. This specification allows us to test our final hypothesis:

Hypothesis 5: 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 refer to this set of treated voters as "Both Treatments Voters" because they received both the weather and administrative treatments. 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 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.

We exclude voters who live in the bordering Walton, Holmes, Wakulla, and Leon Counties from the group of potential controls. According to public records requests we filed, these counties did not reduce polling places or increase 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

[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.

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	
	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%	2.2%
% 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%	39.2%
% Republican	43.6%	35.0%	43.6%	43.6%
% with Some College	52.7%	61.1%	52.7%	52.8%
Median Income	$50,643	$62,941	$50,643	$50,714
Registration Date	2002-03-13	2004-10-17	2002-03-13	2002-07-29

Figure 2 plots the turnout in the 2010–2018 elections for our treated and control voters. The left-hand panel shows the turnout of the both-treatments and potential control voters registered in 2018. In the right-hand panel, we plot the turnout of both-treatments voters and their selected 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 combined weather and administrative 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

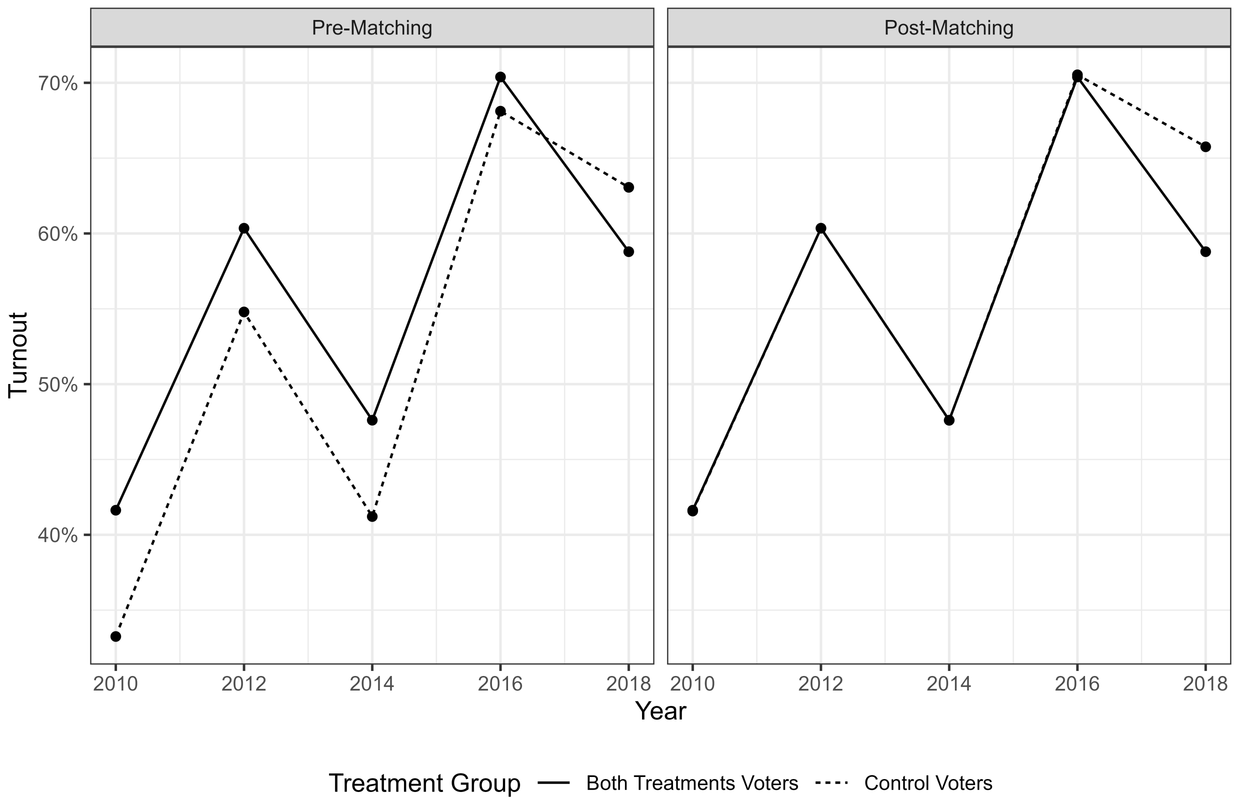

Figure 2: General Election Turnout for Voters Covered by Executive Order and Their Controls, 2010 – 2018

if a voter cast a ballot in a given year, and 0 if she did not. In each model, the coefficient on *Both Treatments* × *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 voters treated by the weather and the Executive Order. 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 races—it is not included in the first two models.

In model 4, we test whether the treatment effect was different where relative rainfall was higher with the inclusion of *Both Treatments* × *2018* × *Relative Rainfall*. Finally, in model

5, we ask whether the treatment effect was different for voters who had to travel further than expected to cast an in-person ballot (*Both Treatments* × *2018* × *Change in Distance to Closest Polling Place*). Model 5 includes controls for rainfall 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 the 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 using a nonnested multiway clustering approach (Cameron, Gelbach, and Miller 2011).

The coefficient on *Both Treatments* × *2018* in Table 3 indicates that Hurricane Michael had a substantial depressive effect in 2018 among the voters receiving both treatments. Models 1 – 3 indicate that the hurricane reduced turnout in the treated counties by roughly 6.9 percentage points. Multiplied across the nearly 200,000 registered voters in the treated counties indicates that some 13,800 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 mediated 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.

Figure 3 makes these relationships even clearer. The figure calculates the treatment effect for each of the 8 covered counties (these estimates can be found in the SI). These estimates are plotted against the relative rainfall experienced by the average voter in each county (in the left-hand panel), and share of polling places that county kept open (in the right-hand panel). 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 ($R^2 = 0.05$), the treatment effect was much larger in counties where more polling places were closed ($R^2 = 0.84$).

Table 3 and Figure 3 provide support for Hypotheses 1 and 1b, but not for 1a: clearly, the AME of the hurricane and administrative treatments was large and negative in the 8 counties covered by the executive order. Similarly, these effects were highly mediated by the share of polling places that were closed and how much further voters had to travel to the nearest polling place. However, we fail to uncover evidence that the strength of the storm as proxied by rainfall depressed turnout.

Table 3: Turnout, 2010 — 2018

	Model 1	Model 2	Model 3	Model 4	Model 5
Both Treatments × 2018	-0.069***	-0.069***	-0.069***	-0.096	-0.059
	(0.010)	(0.010)	(0.010)	(0.080)	(0.072)
Both Treatments × 2018 × Relative Rainfall				0.014	0.002
				(0.044)	(0.040)
Both Treatments × 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	0.283	0.283	0.053	0.054
R2 Adj.	0.051	0.283	0.283	0.053	0.054

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

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

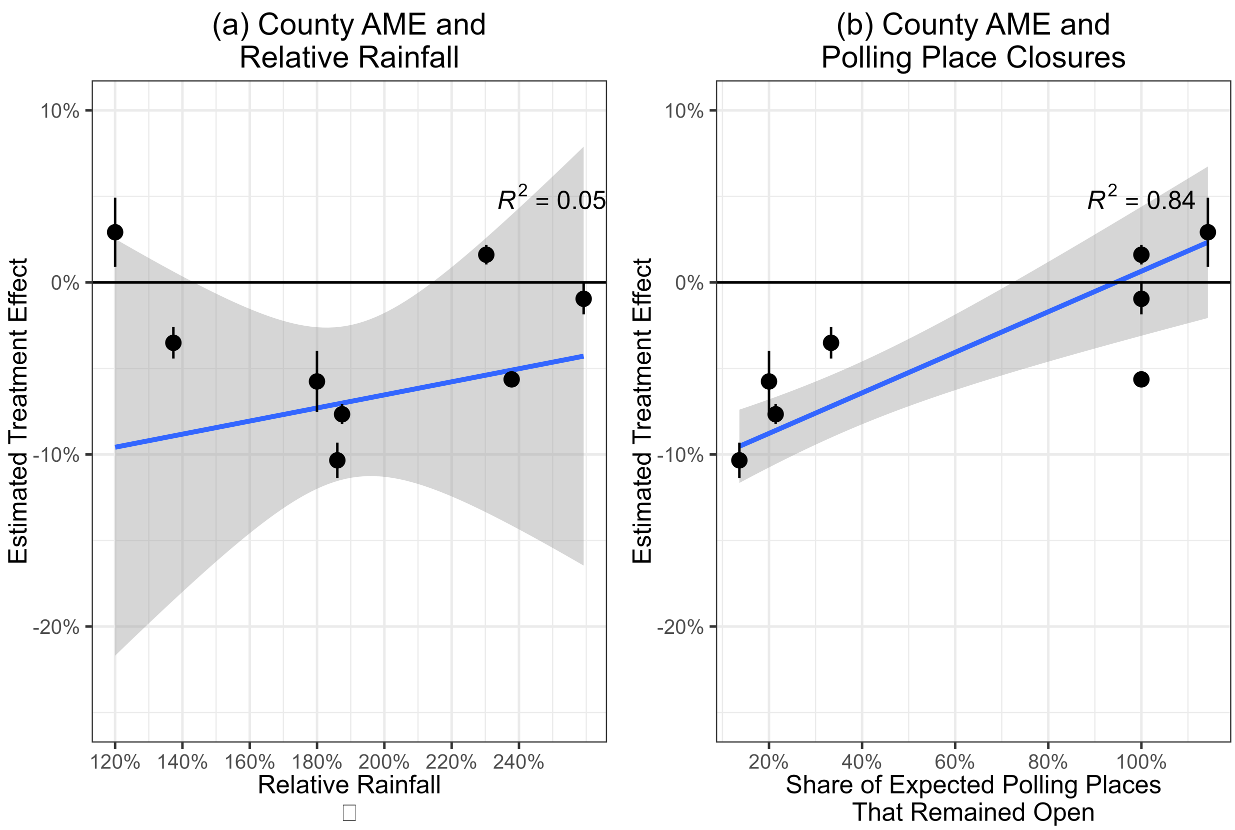

Figure 3: Relationship Between County AMEs and Rainfall, Polling Place Closures

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 model. 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. 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, but the entire buffer falls in uncontested districts. This means that "weather-only" and "weather and administratively" treated 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 turnout, 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 3.5 miles from one another. Importantly, the relative rainfall faced by the two groups is virtually identical: while rainfall during the period was 163% of normal for the voters outside the covered counties, it was 165% 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 4 we plot the turnout trends from the three sets of voters returned by the matching exercise. Figure 4 makes clear that the turnout gap between between these three groups is eliminated in the base period and gives us visually apparent evidence that turnout was lower in the counties treated by both the weather and the Executive Order, providing preliminary evidence of a negative administrative treatment effect. We econometrically test the robustness of this relationship in Table 5.

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*$_c$ $\times$ *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*$_c$ $\times$ *2018*$_t$, meanwhile, takes the value 0 for all observations except in 2018 for voters in the counties covered by the Executive Order.

Table 4: Balance Table for Secondary Match

	Means: Unmatched Data		Means: Matched Data	
	Treated	Control	Treated	Control
% White	75.8%	62.3%	75.8%	76.0%
% Black	20.2%	13.1%	20.2%	20.2%
% Latino	1.0%	17.4%	1.0%	1.0%
% Asian	0.3%	2.0%	0.3%	0.3%
% Female	52.8%	52.4%	52.8%	52.8%
% Male	46.0%	44.9%	46.0%	46.0%
Age	53.7	52.5	53.7	53.7
% Democrat	42.6%	37.1%	42.6%	42.6%
% Republican	42.9%	35.0%	42.9%	42.9%
% with Some College	47.9%	61.1%	47.9%	47.9%
Median Income	$48,631	$62,941	$48,631	$48,513
Registration Date	2001-05-19	2004-10-17	2001-05-19	2001-09-18

$$v_{ict} = \beta_0 + \beta_1 WeatherTreatment_c \times 2018_t + \beta_2 AdministrativeTreatment_c \times 2018_t + \delta County_c + \delta Year_t + \delta Z_{ict} + \mathcal{E}_{ict}. \tag{1}$$

Individual 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 $\delta County_c$ and $\delta Year_t$ contain county and year fixed effects, respectively. The

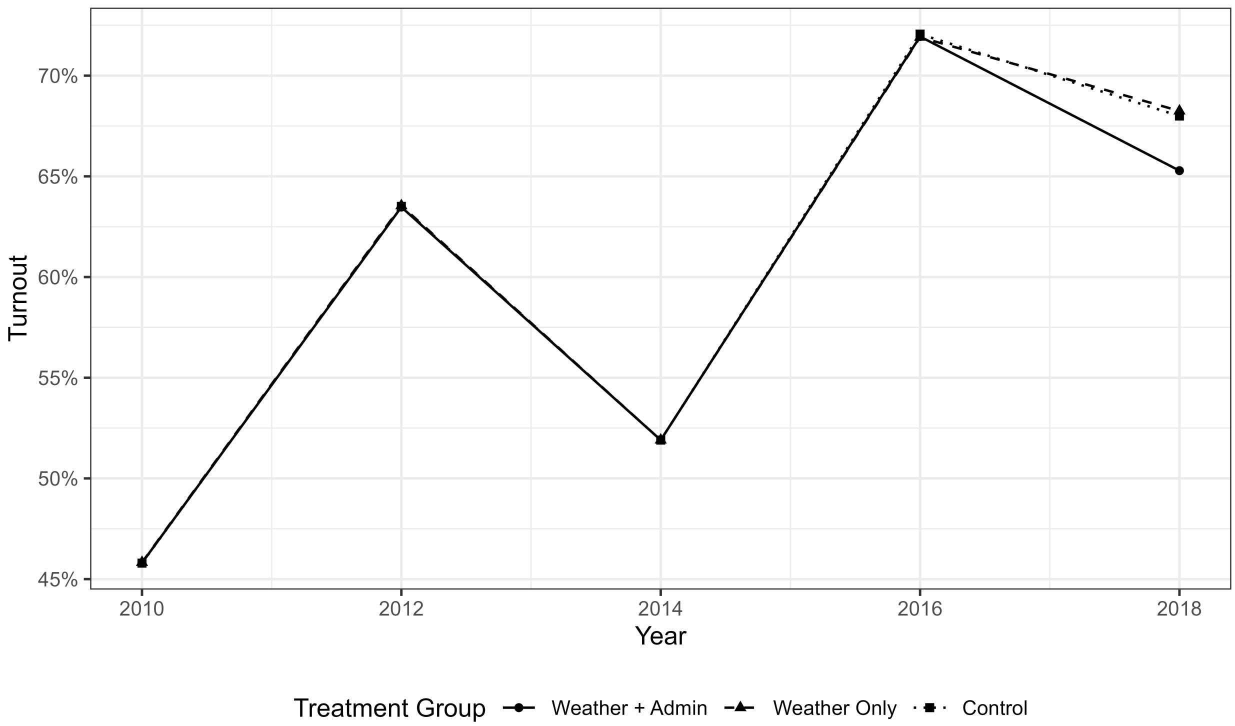

Figure 4: General Election Turnout for Untreated Voters, Voters Treated by Weather, and Voters Treated by Weather and Administrative Changes, 2010–2018

matrix δZ_{ict} includes the measures for relative rainfall and polling place closures interacted with year, county, and treatment dummies.

Table 5 presents the results of these models, again fit using an ordinary least squares specification. In models 6–8, Bay County is the reference category. Models 5 and 8 include *Weather Treatment × 2018* to test whether weather-treated voters turned out at lower rates, and *Weather Treatment × 2018 × Relative Rainfall* to test whether any effect was mediated by rainfall (Hypothesis 2). These models make clear that rainfall was not associated with lower turnout for these voters. This is in agreement with the results presented above, where rainfall for *all* voters in the 8 covered counties was not associated with turnout.

The inclusion of *Administrative Treatment × 2018* and *Administrative Treatment × 2018 × Change in Distance to Closest Polling Place* in Models 4, 5, 7, and 8 allows us to test Hypotheses 3 and 4, which predicted a positive administrative treatment effect for voters who had to travel no further to the nearest polling place (H3), but an effect that was negatively mediated by the change in distance to the nearest polling place (H4). When voters did not have to travel further, the value of *Administrative Treatment* $\times$ *2018* $\times$ *Change in Distance to Closest Polling Place* is equal to zero, meaning that the coefficient on *Administrative Treatment* $\times$ *2018* captures the effect of the other aspects of the executive order such as loosened mail balloting rules. With the exception of Washington and perhaps Liberty Counties, we find no support for Hypothesis 3; even when voters did not have to travel further to the closest polling place, there was not generally a positive administrative treatment effect.

The evidence here for Hypothesis 4, on the other hand, is mixed: while models 4 and 5 do not indicate that the administrative treatment effect was mediated by increased travel distances at the 95% confidence level, models 7 and 8 do. Models 7 and 8 allow for base administrative treatment effects to vary by county. This variation captures both observable (such as the number of additional days of early voting) and unobservable (such as communication of changed mail voting rules) differences in the implementation of the Executive Order in these counties, probably rendering them more appropriate. Regardless of the standard error and ensuing significance level, the coefficient consistently falls between -1.0 and -1.2pp for each additional mile of travel.

Model 6 also generally supports the conclusion that closed polling places led to large negative administrative treatment effects. In Bay County, where the majority of polling places were closed, the estimated administrative effect was -12.7 percentage points. Liberty County sits at the other end of the spectrum: no polling places were closed, and the administrative treatment effect was an estimated *positive* 10.9 percentage points.

We consider these results to be supportive of Hypothesis 4, especially when considered in light of the mediating effect of increased travel distances measured in the previous section estimating the AME in the eight covered counties. Taken as a whole, these results provide further corroboration for the conclusion that what mattered for turnout in the panhandle in

2018 was polling place consolidation at the county level, *not* rainfall or the Executive Order as a single, monolithic treatment with a consistent effect across the covered counties.

Shifting Vote Modes

Having established that turnout was substantially depressed in the treated counties and the depression arose largely 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. 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), but inconsistent with our Hypothesis 5.

Table 5: Turnout, 2010 — 2018

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Administrative Treatment × 2018	-0.028		-0.029	0.006	0.035	-0.127***	-0.044	-0.170
	(0.027)		(0.046)	(0.009)	(0.078)	(0.004)	(0.024)	(0.221)
Weather Treatment × 2018		-0.012	0.003	-0.016	-0.109	0.060*	-0.004	-0.097
		(0.025)	(0.041)	(0.009)	(0.067)	(0.027)	(0.036)	(0.194)
Gadsden Administrative Treatment × 2018						0.126***	0.043	-0.010
						(0.004)	(0.024)	(0.090)
Jackson Administrative Treatment × 2018						0.128***	0.045	0.053
						(0.004)	(0.024)	(0.029)
Liberty Administrative Treatment × 2018						0.236***	0.153***	0.087
						(0.022)	(0.033)	(0.106)
Washington Administrative Treatment × 2018						0.108***	0.079***	0.099**
						(0.006)	(0.004)	(0.034)
Administrative Treatment × 2018 × Change in Distance to Closest Polling Place				-0.012	-0.012		-0.010*	-0.011**
				(0.008)	(0.008)		(0.004)	(0.004)
Weather Treatment × 2018 × Relative Rainfall					0.053			0.062
					(0.043)			(0.125)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Changed Distance to Polling Place and Interactions				✓	✓		✓	✓
Rainfall and Interactions					✓			✓
Cluster Level:	IGC	IGC	IGC	IGC	IGC	IGC	IGC	IGC
Num.Obs.	524340	524340	524340	524340	524340	524340	524340	524340
R2	0.054	0.054	0.054	0.063	0.069	0.075	0.079	0.082
R2 Adj.	0.053	0.053	0.053	0.063	0.068	0.074	0.078	0.081

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

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

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.

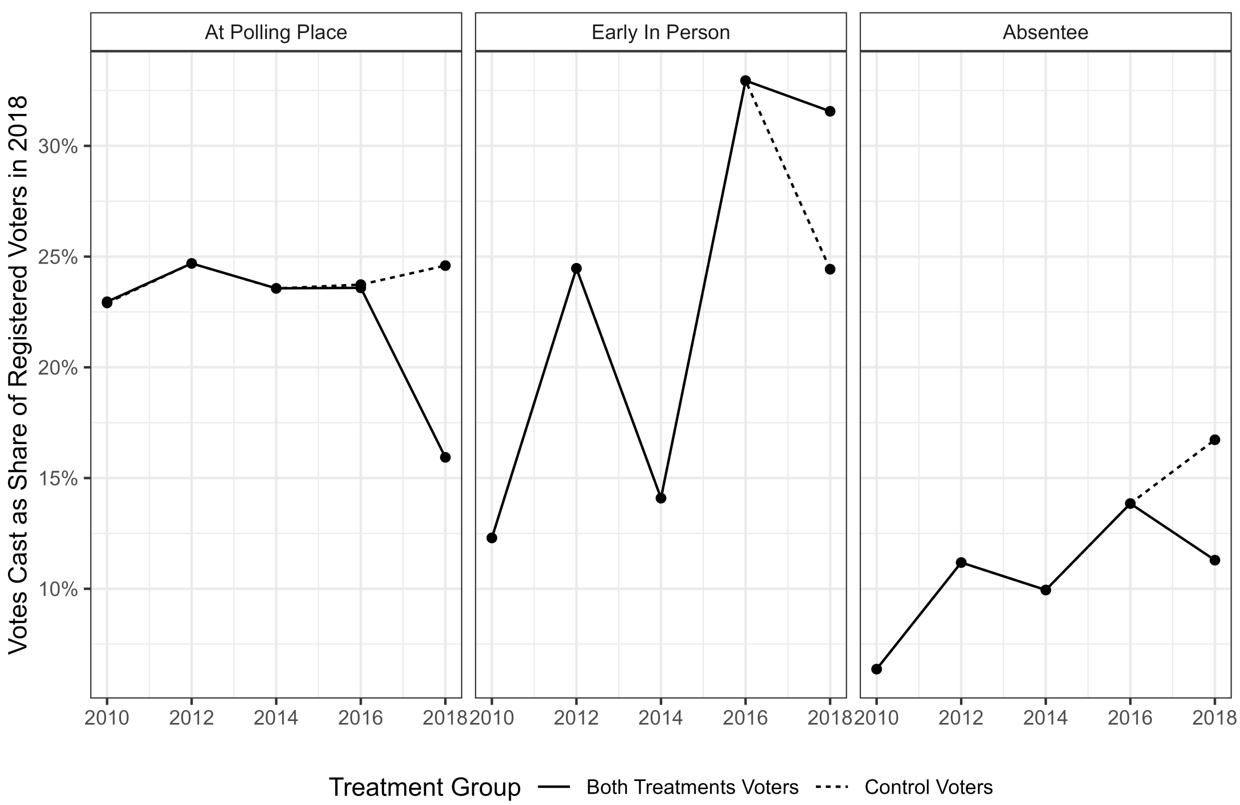

Figure 5: Average Marginal Effect of Hurricane Michael on Vote Mode

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 SI). 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.

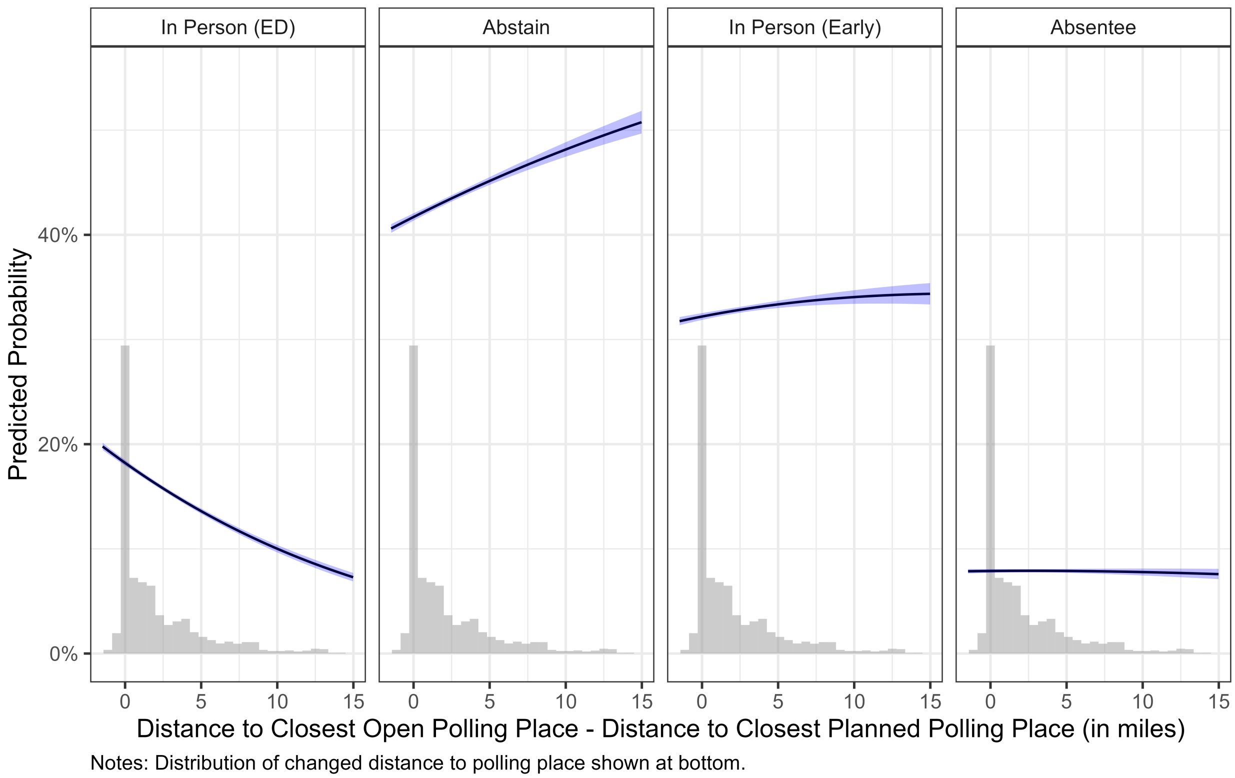

Figure 6: Marginal Effect of Changed Distance to Polling Place on 2018 Vote Mode

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. Thus, we find mixed support for our Hypothesis 5: increased distance to the polls did increase abstention but had no significant effect on voting by an absentee ballot.

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[8] struck the Southeastern United States weeks before the 2016 presidential election, killing dozens in the United States and causing about $10 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 funding to erect emergency polling places in tents or military trucks, as administrators did in the aftermath of 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 largely ineffective: although Executive Order 18-283 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 place. These voters did not move to vote-by-mail options in appreciable numbers. Decomposing the weather

[8] https://www.nhc.noaa.gov/data/tcr/AL142016_Matthew.pdf

and administrative treatments for voters at the edges of the treated counties indicates that observed turnout declines were not driven in any material way by the direct effects of the storm, at least as proxied by rainfall.

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 media, 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 face of natural disasters. Nevertheless, this article joins a growing body of research articulating the central importance of keeping polling places open.

Acknowledgments

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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 covered 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.

[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.

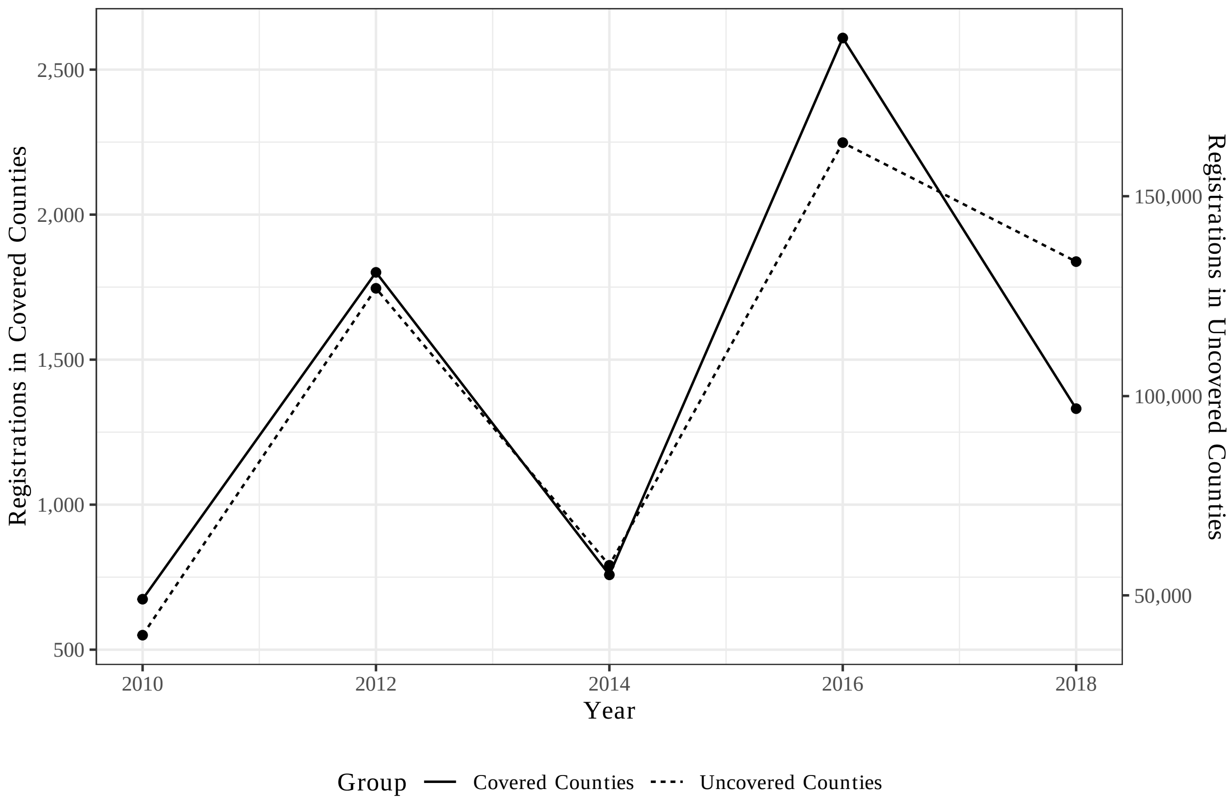

Figure A1: Registrations in Final Weeks Before Election

AME Event Study Plots

In Figure A2 we display the event study plot for the overall treatment effect, as well as the treatment effect for each county individually.

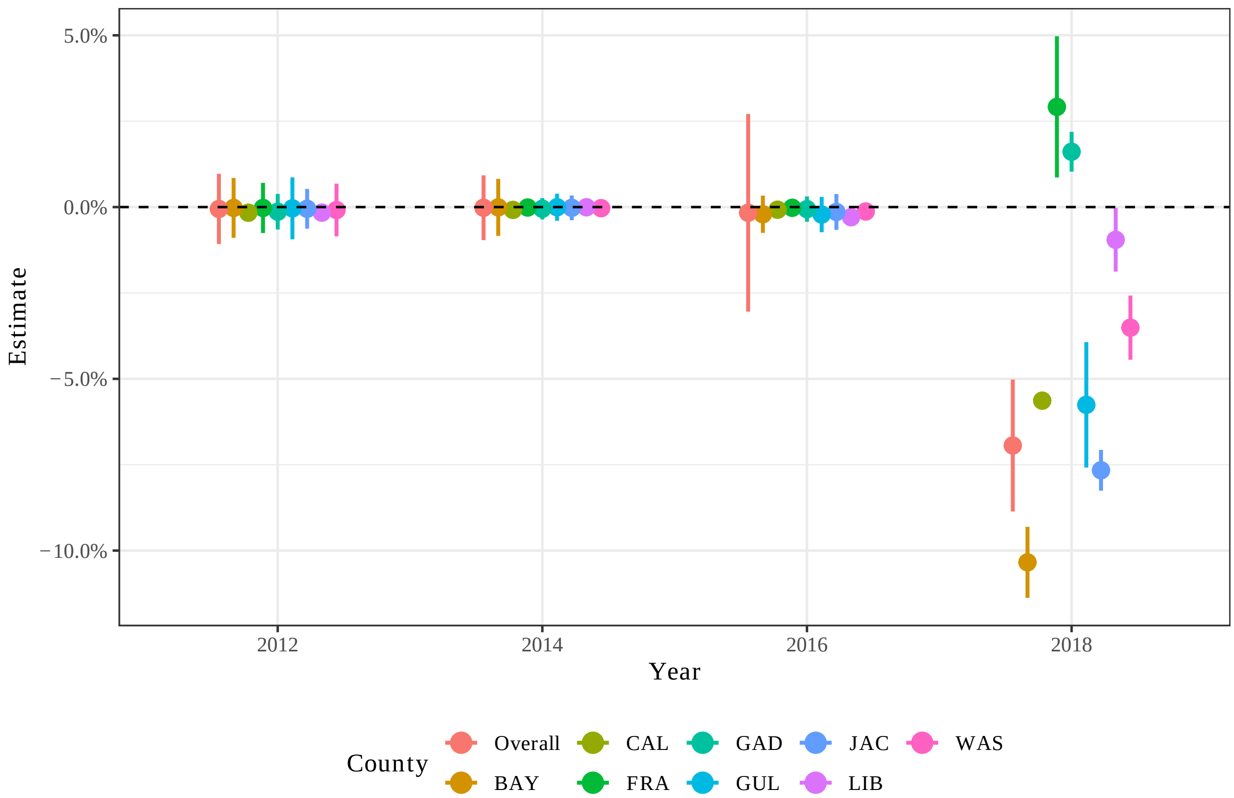

Figure A2: Event Study Plot, Both Treatments Voters

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 pre-processing approaches.

In model 1 of Table A2 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 linear 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 -6.8 percentage points. Table A2 makes clear that our results are robust to a variety of preprocessing and weighting approaches. While entropy balancing, propensity score matching, and unmatched models with county linear time trends return more conservative estimates, the unmatched and exact match models without county linear time trends estimate a larger effect. In no case is the estimated effect smaller than -5.5 points or statistically nonsignificant.

Table A2: Alternative Processing Approaches

	Unprocessed	Unprocessed	Primary Model	Entropy Balancing	Propensity Score	Exact Match
Both Treatments × 2018	-0.099***	-0.055***	-0.068***	-0.063***	-0.063***	-0.081***
	(0.009)	(0.014)	(0.014)	(0.009)	(0.009)	(0.009)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓
Matched Covariates	✓	✓	✓	✓	✓	✓
County Linear Time Trends		✓	✓			
Cluster Level:	IC	IC	IGC	IC	IGC	IGC
Num.Obs.	60041805	60041805	5925990	60041805	5925990	5773440
R2	0.274	0.274	0.283	0.268	0.269	0.286
R2 Adj.	0.274	0.274	0.283	0.268	0.269	0.285

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

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

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 A3 we plot the unprocessed and matched turnout trends for treated and control voters, broken out for each of the 8 treated counties. Figure A3 makes clear that the treatment effect varied substantially by county.

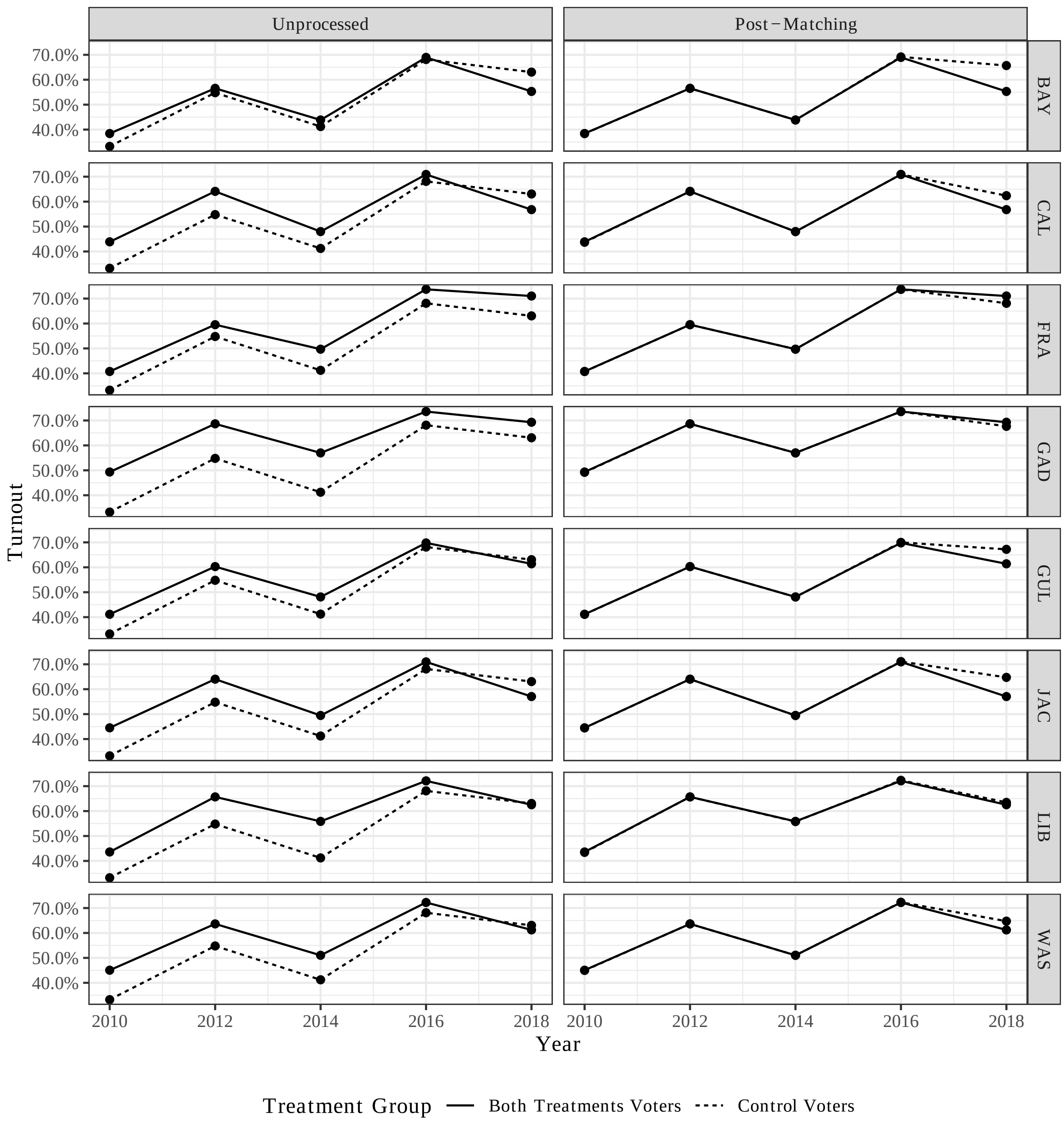

Figure A3: Pre- and Post-Matching County Plots

Table A3 presents the county-specific treatment effects for the 8 treated counties plotted in Figure 3 in the body of the manuscript. The reference category in Table A3 is Bay County.

Table A3: Turnout, 2010 — 2018

	Model 1
Both Treatments × 2018	-0.103***
	(0.005)
Both Treatments × 2018 × Calhoun	0.047***
	(0.004)
Both Treatments × 2018 × Franklin	0.133***
	(0.009)
Both Treatments × 2018 × Gadsden	0.120***
	(0.005)
Both Treatments × 2018 × Gulf	0.046***
	(0.007)
Both Treatments × 2018 × Jackson	0.027***
	(0.004)
Both Treatments × 2018 × Liberty	0.094***
	(0.006)
Both Treatments × 2018 × Washington	0.068***
	(0.007)
Year Fixed Effects	✓
County Fixed Effects	✓
Treated County interacted with County and Year FEs	✓
Cluster Level:	IGC
Num.Obs.	5925990
R2	0.057
R2 Adj.	0.057

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

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

Administrative Treatment Effect Event Study Plots

In Figure A4 we display the event study plot for the administrative treatment effect derived from the triple-differences model, as well as the treatment effect for each county individually. Although the estimates are not perfectly null in the base periods, they corroborate our overall story. There was a clear administrative treatment effect in Bay, Washington, and Liberty Counties, notwithstanding some movement in the pre-treatment periods.

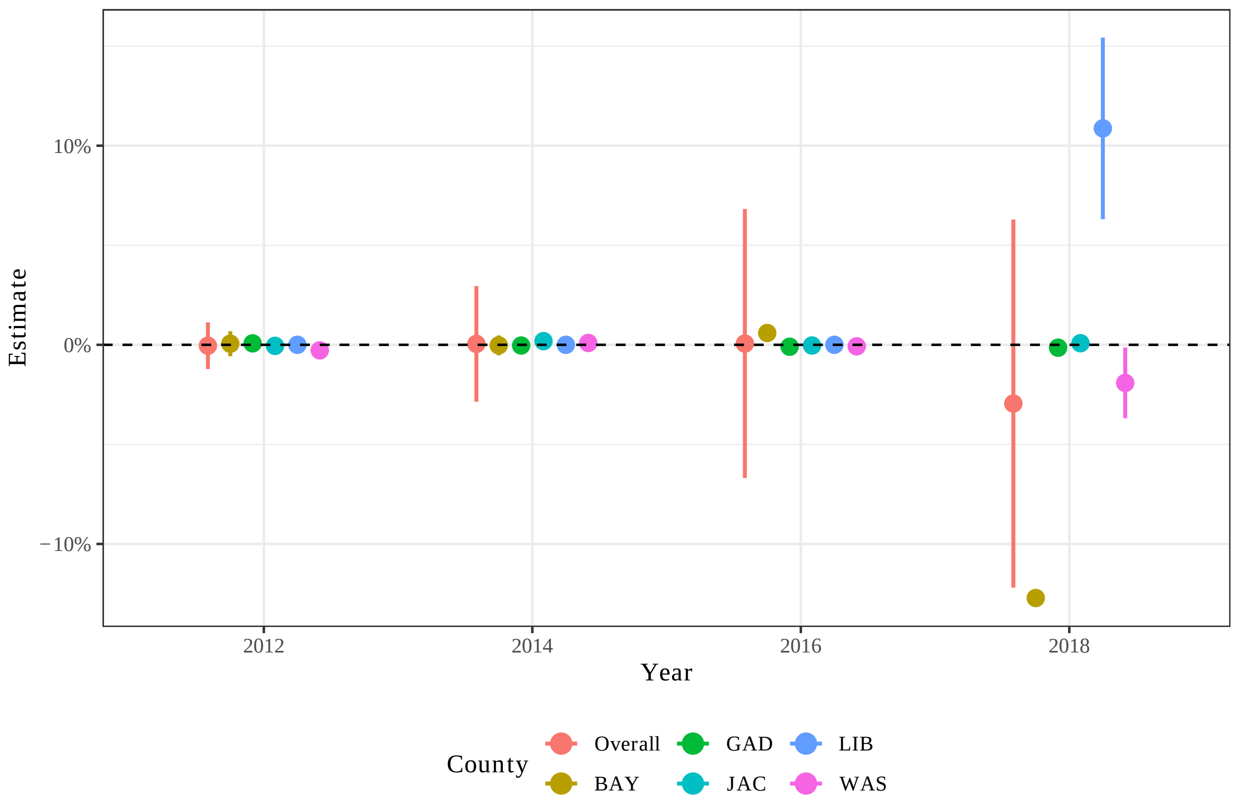

Figure A4: Event Study Plot, Administrative Treatment, Voters in Buffer

Alternative Modelling 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

Table A4: Turnout, 2010 — 2018

	Model 1
Administrative Treatment × 2018	-0.099***
	(0.011)
Weather Treatment × 2018	0.000
	(0.008)
Year Fixed Effects	✓
County Fixed Effects	✓
Matched Covariates	✓
Num.Obs.	61396925
R2	0.275
R2 Adj.	0.275

Robust standard errors clustered at county and individual level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

a county not covered by the Executive Order, while 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 begin by presenting the results of a regression model in which every registered voter in the state is included. As discussed in the text, voters in counties adjacent to the counties covered by the Executive Order are excluded from the AME section due to the possibility that they received weak weather treatments (they are also excluded from the AME robustness checks). In our primary triple-differences approach we only include Panhandle voters that live within 2.5 miles of the border between covered and uncovered counties. Thus, Table A4 is the only specification that includes all voters in the state of Florida. By including matched covariates, Model 1 here is very similar to Model 1 of Table A2, except that voters in the counties abutting those covered by the Executive Order are included. Consistent with the other results in this paper, we find that voters in the covered counties had lower turnout in 2018, but that those voters in the adjacent counties—where the possibility for a weak treatment existed—did not turn out at lower rates.

Next, 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 models 1–4 in Table A5, we present unmatched models. These models include 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 1 includes neither a county linear time trend nor the covariates used in matching; model 2 adds a county linear time trend to model 1. Models 3 and 4 mirror models 1 and 2, but both include the matching covariates. Models 5–8 mirror models 1–4, but in each case use the matched sets of voters as described in the body of the text. We consistently observe that the administrative treatment effect is highly influenced by the additional distance treated voters had to travel to the closest polling place due to consolidation. In each model in Table A5, the reference county is Bay.

Table A5: Turnout, 2010 — 2018

	Unprocessed	Unprocessed	Unprocessed	Unprocessed	Matched	Matched	Matched	Matched
Administrative Treatment × 2018	-0.014	-0.131	-0.014	-0.131	-0.170	-0.193	-0.170	-0.193
	(0.149)	(0.144)	(0.149)	(0.144)	(0.221)	(0.218)	(0.221)	(0.218)
Weather Treatment × 2018	-0.075	-0.006	-0.075	-0.006	-0.097	-0.077	-0.097	-0.077
	(0.068)	(0.057)	(0.068)	(0.057)	(0.194)	(0.189)	(0.194)	(0.189)
Gadsden Administrative Treatment × 2018	-0.161**	-0.066	-0.161**	-0.066	-0.010	-0.011	-0.010	-0.011
	(0.054)	(0.054)	(0.054)	(0.054)	(0.090)	(0.086)	(0.090)	(0.086)
Jackson Administrative Treatment × 2018	-0.104***	0.003	-0.104***	0.003	0.053	0.056	0.053	0.056
	(0.014)	(0.014)	(0.014)	(0.014)	(0.029)	(0.030)	(0.029)	(0.030)
Liberty Administrative Treatment × 2018	-0.230**	-0.135	-0.230**	-0.135	0.087	0.085	0.087	0.085
	(0.068)	(0.068)	(0.068)	(0.068)	(0.106)	(0.100)	(0.106)	(0.100)
Washington Administrative Treatment × 2018	-0.133***	-0.028	-0.133***	-0.028	0.099**	0.104**	0.099**	0.104**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.034)	(0.031)	(0.034)	(0.031)
Administrative Treatment × 2018 × Change in Distance to Closest Polling Place	-0.002**	-0.002**	-0.002**	-0.002**	-0.011**	-0.010*	-0.011**	-0.010*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)
Weather Treatment × 2018 × Relative Rainfall	0.013	-0.005	0.013	-0.005	0.062	0.052	0.062	0.052
	(0.033)	(0.028)	(0.033)	(0.028)	(0.125)	(0.120)	(0.125)	(0.120)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Rainfall and Interactions	✓	✓	✓	✓	✓	✓	✓	✓
Changed Distance to Polling Place and Interactions	✓	✓	✓	✓	✓	✓	✓	✓
Matched Covariates			✓	✓			✓	✓
County Linear Time Trends		✓		✓		✓		✓
Cluster Level:	IC	IC	IC	IC	IGC	IGC	IGC	IGC
Num.Obs.	59177125	59177125	59177125	59177125	524340	524340	524340	524340
R2	0.075	0.076	0.274	0.275	0.082	0.083	0.282	0.282
R2 Adj.	0.075	0.076	0.274	0.275	0.081	0.082	0.281	0.282

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

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

In Figure A5 we break out the trends for each of the administratively 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 weather and administratively treated voters and their matched controls. 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.

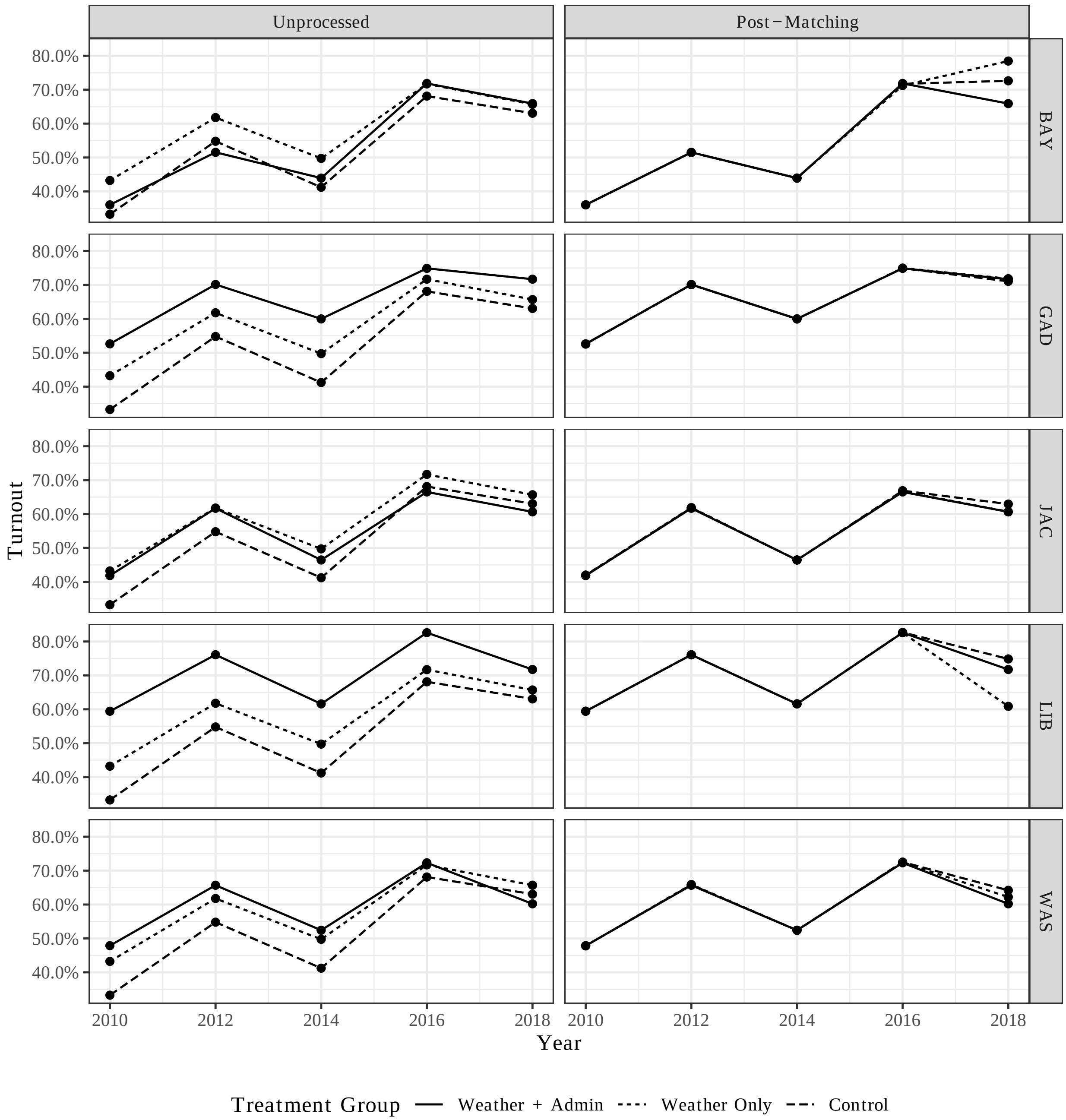

Figure A5: Pre- and Post-Matching County Plots

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. 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 County (where no polling places were closed despite this bad weather) 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.

Limiting the Panel to Voters Registered Prior to 2010

In the body of the manuscript, we include all voters registered as of the 2018 election, including them in the base period regardless of whether they were registered or not. Here, we show that our results do not change when we limit the pool to individuals who were registered prior to the 2010 midterm elections, and thus were registered for the entire study period.

Table A6 presents the results for this restricted pool for the AME of the hurricane in covered counties. Table A7 presents the results using this pool for the triple-differences models. The point estimate for the AME differs by 0.1 points from the primary model, and the effect of each additional mile on turnout is virtually identical in both models. Somewhat surprisingly, we retain a negative administrative treatment effect for Bay County after controlling for changed distance to polling places. This may point to heterogeneous treatment effects by age that this study does not explore (the treated population retained here is about 4.5 years older than the full population of treated voters registered as of the 2018 election). In Table A7, the reference category is Bay County.

Table A6: Turnout, 2010 — 2018

	Model 1	Model 2	Model 3	Model 4	Model 5
Both Treatments × 2018	-0.069***	-0.069***	-0.069***	-0.103	-0.060
	(0.017)	(0.017)	(0.017)	(0.061)	(0.047)
Both Treatments × 2018 × Relative Rainfall				0.018	0.004
				(0.036)	(0.028)
Both Treatments × 2018 × Change in Distance to Closest Polling Place					-0.007**
					(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.	4091160	4091160	4091160	4091160	4091160
R2	0.043	0.161	0.161	0.044	0.045
R2 Adj.	0.043	0.161	0.161	0.044	0.045

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

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

Table A7: Turnout, 2010 — 2018

	Model 1	Model 2	Model 3	Model 4
Administrative Treatment × 2018	-0.038	-0.149***	-0.064**	-0.345
	(0.022)	(0.003)	(0.021)	(0.208)
Weather Treatment × 2018	0.007	0.055***	-0.011	0.032
	(0.017)	(0.009)	(0.024)	(0.190)
Gadsden Administrative Treatment × 2018		0.127***	0.041	-0.078
		(0.003)	(0.021)	(0.096)
Jackson Administrative Treatment × 2018		0.165***	0.079***	0.098**
		(0.003)	(0.021)	(0.029)
Liberty Administrative Treatment × 2018		0.258***	0.172***	0.024
		(0.028)	(0.035)	(0.110)
Washington Administrative Treatment × 2018		0.100***	0.071***	0.113***
		(0.006)	(0.003)	(0.032)
Administrative Treatment × 2018 × Change in Distance to Closest Polling Place			-0.010**	-0.012**
			(0.003)	(0.004)
Weather Treatment × 2018 × Relative Rainfall				-0.029
				(0.127)
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓
Rainfall and Interactions				✓
Changed Distance to Polling Place and Interactions			✓	✓
Cluster Level:	IGC	IGC	IGC	IGC
Num.Obs.	376320	376320	376320	376320
R2	0.039	0.053	0.057	0.060
R2 Adj.	0.039	0.052	0.056	0.059

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

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

Multinomial Regression Table

In Figure 6 in the body of the paper, we show the marginal effects plot based on a mulinomial logistic regression. We include the regression table here. While the coefficients have been exponentiated in this table, the standard errors have been left unadjusted.

Table A8: Vote Mode in 2018 (Relative to In-Person on Election Day)

	Abstain	Early	Absentee
Change in Distance to Polling Place (miles)	1.077***	1.068***	1.060***
	(0.002)	(0.002)	(0.003)
Distance to Closest Planned Polling Place (miles)	0.959***	0.905***	1.002
	(0.004)	(0.005)	(0.004)
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	1.081
	(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***
	(0.000)	(0.000)	(0.001)
Intercept	0.429***	0.300***	0.011***
	(0.056)	(0.060)	(0.086)
Vote-mode in 2010, 2012, 2014, and 2016		✓	
Number of Observations		197533	
McFadden Pseudo R2		0.271	

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

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.