Warm Glow from Voting vs. Direct Costs: Evidence from the 2020 Election, Black Lives Matter Protests, and Mail-in Balloting

Warm Glow from Voting vs. Direct Costs: Evidence from the 2020 Election, Black Lives Matter Protests, and Mail-in Balloting

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Abstract

The 2020 General Election was idiosyncratic in many ways: COVID-19 was spreading across the nation, racial justice protests were a top issue, and mail-in balloting access was considerably expanded. Theory suggests that agents vote when direct benefits of voting exceed costs. Theoretically, Black Lives Matter protests increased social benefits of voting while mail-in balloting reduced costs, leading to higher turnout. This paper analyzes the effect of protests and mail-in balloting on voting outcomes (turnout and margin) using two quasi-experimental research designs, finding precisely estimated effects for both events. Using June rainfall as an instrumental variable, I estimate the effect of the 2020 protests on the presidential election outcome at the county level. Results indicate that the median protest increased predicted Republican margin by 0.019 percentage points and predicted turnout by 0.113 percentage points, robust to many specifications and sets of controls. Using a difference-in-difference approach to estimate the effect of moving to universal mail-in balloting yields much larger effect sizes: Republican margin increased by 0.45 percentage points and turnout by 2.52 percentage points, driven primarily by increased voting among older populations. Black Lives Matter protests appear to have limited effect—though they galvanized opposition voters more than supporters—while mail-in balloting greatly increased turnout. Neither of these features are likely to have affected the outcome of the 2020 election; though they both contributed to the increased turnout, they only explain 9% (upper bound 17%) of the overall increase.

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1 Introduction

Why do people vote? Theoretical models suggest that voting occurs when the direct benefits of voting (intrinsic and social benefits) exceed the costs (Gerber et al., 2008). In 2020, the local intensity of Black Lives Matter protests plausibly increased the social benefits of voting by making voting behavior more salient and public. For example, after a large protest, agents may feel more pressured to vote by those around them as friends and family inquire about voting intentions and behavior. The effect is likely not specific to supporters/opposers of the protest cause, as both sides are galvanized by such events. On the other hand, mail-in balloting reduces voting costs as voters do not have to travel to polls, stand in line, or—specifically in 2020—fear COVID-19 infection while voting. This paper analyzes the effect of these two unique features of the 2020 general election on two main outcomes, margin and turnout, using quasi-experimental research designs.

Prior papers have investigated the effects of protests on a variety of outcomes. Collins & Margo (2007) study protests in the USA using an IV approach to investigate the effect of 1960s protests on housing prices in subsequent decades. They find that cities with high severity riots had lower property values than cities with low severity riots. Electorally, Madestam et al. (2013) find that the Tea Party protests in 2009 led to large multiplier effects in the 2010 midterm elections, estimating that each marginal protester brings "an additional 12 votes to the Republican camp."

Protests in 2020 gave rise to competing hypotheses: would the Black Lives Matter protests help Democratic candidates through increased voter turnout of young and minority people or help Republican candidates by virtue of the so-called "suburban strategy" which posits that violent protests increase support for "law and order" candidates (Republicans)? I estimate the causal effect of 2020 protests on voter turnout and margin using an IV approach in order to assess the efficacy of protesting on changing voting behavior. I find precisely estimated, though small, results. Compared to a county with no protests, the median protest increases Republican margin by 0.019 percentage points and turnout by 0.113 percentage points, robust to many specifications and controls, including mail-in balloting, COVID-19 cases, and prior election outcomes. These effect sizes are much smaller than those that Madestam et al. find.

The estimated effect appears to be primarily driven by Republican backlash in Democratic areas and vice versa. However, Democratic backlash is smaller in magnitude and statistically insignificant, resulting in the net effect benefitting Republicans. In particular, Democratic areas that had large protests experienced higher Republican margins (0.126 percentage points for the median protest). Surprisingly, there is limited treatment effect heterogeneity: a county's racial makeup plays no role in determining the effect of protests on elections. However, racial animus (as measured by the presence of racial epithets in Google search queries) appears to reverse the direction of the overall trend. Protests in areas that have high racial animus increased Democratic margins (0.265 percentage points for the median protest). In both of these cases, it appears that protests motivate backlash against the local status quo and motivate voters to vote but do not change minds. Overall, the results indicate that protests had little to no marginal local impact for two potential reasons. First, protests became a national issue, changing the entire political landscape, thereby limiting local effects. Second, protests appear to influence motivation, though do not change minds, a result of a polarized national environment. As a result, margin largely remains unchanged.

In the second part of the paper, I estimate the causal effect of reducing the cost of voting by expanding access to mail-in balloting. While 63 counties allowed universal mail-in balloting for the first time, 118 others did not change their policies from 2016, creating a natural experiment. I exploit this natural variation using a difference-in-difference approach to estimate the causal effect of mail-in balloting. I find precisely estimated and large effect sizes of expanding access to mail-in balloting. Republican margin increased by 0.45 percentage points and turnout increased by 2.52 percentage points, much larger effect sizes than the protest estimates. These effects are driven by much higher voting rates by older individuals in counties that had universal mail-in balloting. Given that older individuals are more at risk of fatality as a result of COVID-19, one interpretation of these results is that older individuals who did not have access to mail-in balloting simply did not vote for fear of contracting the virus. This also helps explain why mail-in balloting aided Republicans in this cycle, though conventional political speculation would predict that expanded voting access helps Democrats.

2 Literature Review

There are three main areas of literature relevant to this paper's topic: 1) Voter Motivation, 2) Protest Effects, and 3) Black Lives Matter polling/research.

2.1 Voter Motivation

Theory does not provide a clear answer for voter motivation. Some theoretical work has suggested that there are several components to voter motivation. Passarelli and Tabellini (2013) conclude that political unrest is caused by emotional aggrievement (perception of unfairness) which in turn leads to differential voting patterns and policy changes. Theoretical voting models predicated on rational voting agents do not predict the high turnout observed in elections across the word. In these models, agents only vote when the expected utility from policy differences between candidates exceeds the cost of voting, a condition rarely met when there are large numbers of voters. As Hegel remarked in *Elements of the Philosophy of Right*, "the casting of a single vote is of no significance where there is a multitude of electors." Why, then, do individuals vote?

Empirical literature has found social norms to be an influential factor in voter turnout. For example, Gerber et. al (2008) use a field experiment to show that voting is driven by social pressure. The authors find that households that received mailings promising to publicize their turnout to neighbors participated at much higher rates than others. Publicizing voting behavior to neighbors increases voter turnout by 8 percentage points relative to the control of 29.7 percentage points. The authors conclude that using social norms and pressure to induce positive civic behavior works. DellaVigna et al. (2016) extend this approach to determine whether voting is a signaling mechanism to improve social image. The theoretical model underpinning this field experiment is one of pride in telling others that you voted and shame in admitting that you did not. The authors estimate that voters assign an average of \$18 of value on "voting to tell others" and non-voters assign \$13 when asked by a surveyor about their voting habits. These papers show that voter motivation is related more to social pressures than to policy-driven utility. Extending to 2020, social pressure might be the driving force behind both participation in BLM protests and its resultant effect on voting patterns. As a potential mechanism, racial justice protests may affect feelings about fulfilling civic duty (perhaps heterogeneously based on race itself), thereby increasing turnout. This paper estimates the social influences of voter turnout by investigating locally specific social pressure.

Gerber et al. also develop a theoretical model for voter turnout. They predict that individuals vote only when the direct benefits of voting exceed the costs. Since an individual vote is inconsequential to the overall outcome, direct benefits only include intrinsic benefits (fulfillment of civic duty) and extrinsic benefits (telling others I voted). The cost of voting in America is different by location but includes having the proper ID, registering in advance, driving to the polls, and standing in line. Though most voters do not face long wait times at the polls, 30% of voters stood in line for 30 minutes or more while 15% waited more than one hour[1]. In 2020, the cost of voting also includes assessing the risk of COVID-19 infection at the polls, which may drive turnout down as well.

As a result, expansion of mail-in balloting significantly reduces voting costs as individuals no longer have to travel, stand in line, or fear infection. To wit, this paper finds that universal mail-in balloting increased turnout by 2.52 percentage points relative to the baseline of counties that did not expand access at all. Republican margin increased by 0.45 percentage

[1] Quealy Parlapiano. *Election Day Voting in 2020 Took Longer in America's Poorest Neighborhoods.* New York Times. January 4, 2021

points, as older voters (65+)—who tend to vote Republican—voted more in states with universal mail-in balloting, perhaps fearing COVID-19 infection the most of any age bracket.

2.2 Protest Effects

Empirical papers have used an IV methodology to assess the effect of protests on various outcomes—both electoral and economic. Widely used in development economics, rainfall has been used as an instrument to study the effect of dams, conflict, and income on outcomes of interest, as it provides a source of randomness allowing researchers to identify causal effects.[2] However, the exclusion restriction has recently been challenged as it may not be exogeneous for the studied outcomes (Sarsons, 2015). The use of rainfall in this paper's context is much more likely to be exogenous: it is unlikely that rain in June 2020 would affect voting through channels other than the protests. One possible story against the exogeneity claim would be that rainfall in June limited COVID-19 transmission as individuals stayed inside, thereby leading to better health and economic outcomes in November, leading to differential voting patterns in the election. This explanation is unlikely, and results in Table 19 suggest that it is not empirically supported either.

Collins & Margo (2007) pioneered the rainfall IV approach. The authors investigate the effect of 1960s protests on housing prices in subsequent decades (1960-1980). They find that cities which experienced high severity riots (based on arrests, size, deaths) had lower property values than cities with low severity riots, *and* that the effect was most pronounced on black owned property. MLK was assassinated on April 4th, and 100 riots subsequently erupted. The authors note that the Dade County sheriff referred to the rainfall as "beautiful," indicating some domain-specific knowledge that rainfall decreases protest severity/turnout. They find that April 1968 rainfall has a negative effect on protest severity and the 2SLS

[2]The first instrumental variable ever in Economics was also rainfall. Phillip Wright (1928) developed instrumental variables to estimate the elasticities of supply and demand for flaxseed, the source of linseed oil used to make paint. Since rainfall plausibly shifts the supply curve for linseed without shifting the demand for linseed, it was used by Wright to estimate the slope of the demand curve. Other instruments were used to estimate the slope of the supply curve. Wright used these elasticities to perform a welfare analysis of tariffs.

model results in uniformly negative coefficients on the riot severity variable, providing a robustness check on their results.

Madestam et al. (2013) extend this approach to the tea-party protests in 2010 and use rainfall on April 15, 2009 (Tax Day, the day of the protests) as a source of exogenous variation on protest size. The outcome variables of interest are vote share and policy making. They find that rainfall on placebo dates from 1980-2008 did not produce an effect as large as the estimated one for April 15, 2009, providing suggestive evidence that rainfall is a weak, but effective instrument. They find that on average, rainfall decreases rally size by 51%. Counties with no rainfall on April 15 had a 7% higher Republican vote share. Additionally, the authors find that each marginal protester brings "an additional 12 votes to the Republican camp" indicating that protests have significant spillover effects.

The model developed by Gerber et al. provides a theoretical explanation for how protests may affect electoral outcomes. Protests may make voting more salient by raising awareness about particular policy issues and increasing civic engagement writ large. If individuals are more likely to ask about an agent's voting behavior because of the perceived importance of a particular election, that may lead to more social pressure on the agent to vote. For further discussion, see Section 3 (Models).

Extending to 2020, protesters may feel acute social pressure to "join the movement" and post about BLM on various social media outlets, but this may or may not translate to voting behavior. To this end, I investigate the effect of this year's protests on vote share and voter turnout by using similar methodology as Madestam et al. The two discussed papers show differing effects of protests on outcome variables based on who protested—property values tended downward following Democratically-aligned protests but Republican vote share tended upward when Republicans protested. The results presented here directionally agree with those findings.

2.3 Black Lives Matter

The Black Lives Matter movement arose in 2014, with the fatal shooting of Michael Brown by police officer Darren Wilson in Ferguson, Missouri. With no formal organization and a decentralized network of leadership, between 2014 and 2020, Black Lives Matter was largely a peripheral movement that rose to national attention primarily when black men and (to a lesser extent) women were shot or brutalized by law enforcement. Though not explicitly affiliated with either major party, most supporters of BLM identify with the Democratic party, and the opposition identifies as Republican. Since 2014, dozens more individuals have been victimized by police brutality. The following are victims whose deaths were covered extensively in the national media: Eric Garner (2014), Laquan McDonald (2014), Philando Castile (2016), and Alton Sterling (2016). The movement significantly accelerated in 2020 with the killing of George Floyd on May 26, along with the deaths of Breonna Taylor, Ahmaud Arbery, and others across the country. These deaths sparked international outrage and protests, even as the coronavirus pandemic spread across the world.

Some surveys have been done on the effect of the 2020 racial justice protests on election outcomes. According to data from Associated Press VoteCast—a large voter survey conducted for the AP by the National Opinion Research Center at the University of Chicago—nine out of ten voters said that protests about police violence were a factor in their voting. Three-fourths of voters called it a major factor. One-fifth of all voters said it was the *most* important factor, even with the coronavirus pandemic raging across the United States. The survey sample size was 140,000 voters.

As hypothesized, the protests were a polarizing issue. Though a vast majority of voters found the issue important in casting their ballot, they disagreed on candidate preference, with 53% voting for Biden and 46% voting for Trump, almost mirroring the national popular vote outcome. In other words, protests were an important factor in voting choice for 75% of the electorate, but did not meaningfully move the needle for either candidate (perhaps a result of asymmetric polarization). Turnout effects are a bit more clear, according to the survey. In Jefferson County, which encompasses Louisville, KY where Breonna Taylor was killed, turnout rose by more than 10% in 2020 relative to 2016, but did not eclipse 2012 or 2008 levels.

This paper sheds light on the question of voter motivation and social pressure. If there is higher turnout among protesting groups in areas that experienced large protests, that may provide suggestive evidence for the social theory of voting. On the other hand, higher turnout among opposing groups may signal a more policy-driven voting motivation, as opposition voters may be fearful of Democratic police defunding. Given that we find higher turnout in areas that had larger protests, but increased vote share for Republicans, the latter theory appears to be supported empirically. These results provide valuable political insight regarding whether the 2020 protests have increased support for the opposition party, a manifestation of asymmetric polarization. Finally, this research helps shed light on the efficacy of protesting in 2020. In effect, do Black Lives Matter protests matter?

The rest of the paper is structured as follows. First, I present some theoretical models. Then, I discuss the motivation behind the IV methodology in Section 4 and summarize the data in Section 5. Section 6 presents empirical results for protests and Section 7 presents results for mail-in balloting. Sections 8 and 9 discuss and conclude the paper and offer topics for future research.

3 Models

Two theoretical models underpin the results in this paper, a turnout model and a vote-choice model. Though the parameters are not estimated, the models provide intuition and hypotheses for the results.

3.1 Voter Turnout Model

I use Gerber et al.'s voter turnout model as a starting point for identifying the parameters that may change as a result of political protests and expanded access to mail-in balloting. The key model parameters in determining whether a rational agent votes are the probability that their vote will be pivotal (p), the difference in utility from candidate attributes/policy stances (B), the direct benefit of voting (D), and the cost of voting (C). Accordingly, for an agent to actually vote, the following must hold:

$$pB + D > C \tag{1}$$

For any election with a reasonable number of voters, p becomes sufficiently small to ignore the first term. I am left with $D > C$, without a functional form on D. Gerber et al. break this utility into two parts: the intrinsic and extrinsic benefits of voting. The intrinsic utility comes from the feeling of fulfilling one's civic duty. The extrinsic utility is primarily a function of avoiding shame when others ask whether or not you voted. I impose a linear functional form on the unknown utility function of D, composed of D_I (intrinsic utility) and D_E (extrinsic utility):

$$U(D) = \beta_1 D_I + \beta_2 D_E \tag{2}$$

The parameter of interest is D_E, given the assumption that protests do not increase fulfillment via civic duty directly. Rather, protests increase the salience of the importance of voting, thereby increasing potential social interactions that will bring up voting. I assume D_E is proportional to the probability that voting action is perceived by others.

$$D_E = \frac{\pi_r(\alpha + \beta_3 D_I)}{\beta_2} \tag{3}$$

The D_I term captures a potential interaction between intrinsic and extrinsic motivation.

π_r is the perceived probability that voting behavior will be observed, and α is a measure of how important extrinsic consequences are to the agent. Additionally, I normalize by the coefficient on D_E in Equation 2. Rewriting the original equation:

$$pB + \beta_1 D_I + \pi_r \alpha + \pi_r \beta_3 D_I > C \tag{4}$$

The protests are potentially increasing π_r, thereby making turnout more likely. Mail-in balloting is simply reducing C, theoretically tilting the cost-benefit analysis toward voting. In areas that have seen particularly large protests, this salience metric may have differentially increased relative to other locations. Concretely, if location i saw large protests, some engaged agents may have a heightened sense of awareness of political issues. As a result, they may be more likely to ask their friends (unengaged agents) whether they have registered to vote, made a plan to vote, and voted. Expecting this social interrogation regarding their voting behavior and not wanting to be shamed[3], the unengaged agents may be socially compelled to vote. Note that this mechanism may run both ways, as both sides of the political spectrum may be galvanized by protests.

The key identification assumption is that π_r decays with distance from protest. That is, it has the highest effect size close to the protest location. In defense of this assumption, protesters likely live close to the protest location and are the most engaged agents. Their friends and family are likely to live close to them. In opposition to this assumption, it is quite possible that the issue has become too nationalized to still have localized impacts (π_r is constant across the US). The truth is most likely somewhere in between. As a result, regression estimates likely underestimate the direct effect of protesting because media coverage has attenuated any locally specific effects.

Expansion of mail-in balloting must reduce voting costs, as agents are still able to vote in-person if wanted. By increasing the action set, at the very least costs remain the same.

[3] DellaVigna et al. (2016)

3.2 Vote Share Model

Protesters hoped that protests would lead to a gain in vote share for Democratic candidates. I consider the propensity of voting for a Republican as a function of several variables. These equations are intended to illustrate a concept, not explain voting choice entirely.

$$R_i(m, s, \vec{x})^4 \tag{5}$$

In Equation 5, m is the media lean of individual i's media consumption normalized to be between $-n$ and n ($-n$ hyper-left, n hyper-right), s is a measure of protest salience/size, and $\vec{x}$ is a vector of controls. Any potential functional form includes many interaction terms. I consider the functional form presented in Equation 6.

$$R_i(m, s, \vec{x}) = \gamma m + \delta s + \underbrace{\zeta m \cdot s}_{\text{media-protest interaction}} + \vec{B}^T \vec{x} \tag{6}$$

A priori, one would assume that δ is negative (sympathetic observers reduce their propensity to vote for Republican candidates). However, the media that individual i consumes will filter ideas about the protest through a particular lens, affecting the individual's propensity to support/oppose the movement.

The possibility of asymmetric polarization is now more apparent. The interaction term may increase individual i's propensity to vote Republican if she watches right-leaning media ("need law and order to counter violent protests") while decreasing individual j's propensity if she watches left-wing media. The same effects are true for social media, with feeds distilling the protests in a left or right-wing fashion, influencing behavior.

[4] R_i is not a probability, but can be converted to one via logistic expansion $\left(P_R = \frac{\exp(R_i)}{\exp(R_i)+1}\right)$

4 Methodology

4.1 OLS

To investigate the effect of protests on the two outcomes, I employ a traditional OLS regression and an IV regression. The baseline OLS regression will have the following form:

$$\underbrace{(\text{vote}_i^{20} - \text{vote}_i^{16})}_{\text{voting outcome changes}} = \beta \cdot \text{size}_i + \lambda \cdot \text{vote}_i^{12} + \gamma_1 \vec{X_i^{20}} + \gamma_2(\vec{X_i^{20}} - \vec{X_i^{16}}) + \epsilon_i \tag{7}$$

Where vote_i^y represents the voting outcome (either turnout or margin for the Republican Party) in location i (county-level) in year y, size_i is the number of protest participants per 100 capita in the month of June in location i, and $\vec{X}_i^y$ is a vector of demographic controls for location i in year y (race, age, education, unemployment, etc. and a column of ones). Following from prior literature's approaches to historical election outcome controls, I control for both the absolute demographics in the year of interest and the change from a prior election year (DellaVigna and Kaplan, 2007).

Equation 7 is an OLS difference-in-difference approach, where the estimand (β) is the location-specific temporal change. In order to adequately control for national political environment changes, county-level voting outcomes will be normalized to be the difference from the national outcome. If a county has 60% turnout and the US as a whole has 55% turnout, that county will have a 5 percentage point turnout differential.

Several more traditional OLS regressions will be run with interaction terms on the size_i variable. Interacting the size variable with 2016 election outcomes and demographics (black population as a % of total) quantifies the differential impact of protesting. For example, a county that voted for Trump by 15 points in 2016 may be much less likely to be moved by a BLM protest whereas a county where 60% of the population is black may be mobilized by the protests.

The mail-in balloting regressions are the same, though the protest variable is substituted for a dummy treatment variable which is equal to 1 if access became universal in 2020.

4.2 IV & Motivation

There are a few issues with the traditional OLS regression. Most importantly, the β coefficient from OLS is likely to be biased upward because of the unobserved determinants of the change in voting patterns. For example, a location that had high protest size may be more "politically active" and will also have high voter turnout. Political activity drives both demonstrations and turnout. To get closer to protest size exogeneity, I use June 2020 rainfall as an instrument. Rainfall is plausibly exogenous to a location after controlling for historical rainfall, and it affects protest size directly (by somewhat inhibiting protesters from mobilizing). To check for instrument relevance, I run the first stage regression in Equation 8 (results are reported in Section 6.1).

$$\text{size}_i = \pi \cdot \text{rainfall}_i^{\text{june}} + \gamma \vec{X}_i + \epsilon_i \tag{8}$$

To see the importance of controlling for historical rainfall, consider Seattle, WA and Phoenix, AZ. Without conditioning on historical rainfall, the variation in protest size that June rainfall generates would be correlated with differences across places like Seattle, WA (high probability of rain) and Phoenix, AR (low probability of rain). Possibly, turnout would be high in Arizona in the absence of protests, since voters may have been concerned with border policy or attacks against their late senator John McCain. Because it is less likely to rain in Phoenix than Seattle, the instrument would be (negatively) correlated with the omitted determinants of voter turnout, violating the exclusion restriction. Therefore, the June rainfall instrument is exogenous conditional on historical rainfall, but not unconditionally.

If the estimated $\hat{\pi}$ in Equation 8 is statistically significant and negative, that provides suggestive evidence that the identified mechanisms are supported by the data. The 2SLS regression will look similar to the OLS approach, except with the addition of the June rainfall variable and controls for historical rainfall:

$$(\text{vote}_i^{20} - \text{vote}_i^{16}) = \beta \cdot \hat{\text{size}}_i + \lambda \cdot \text{vote}_i^{12} + \gamma_1 \vec{X_i^{20}} + \gamma_2(\vec{X_i^{20}} - \vec{X_i^{16}}) + \eta \vec{R_i} + \epsilon_i \tag{9}$$

Where $\vec{R_i}$ is the vector of historic average precipitation by month for location i from 1980 to 2010. Note that $\hat{\text{size}}_i$ is being instrumented for from the first stage. I add interaction terms to this regression to investigate differential effects of protesting. I provide placebo (falsification) regressions ($\text{vote}_i^{16} - \text{vote}_i^{12}$ and $\text{vote}_i^{12} - \text{vote}_i^{08}$) as added robustness checks, presented in Section 6.6.

4.3 Difference-in-Difference (Mail-in)

For the effect of mail-in balloting, I run a simple difference-in-difference regression with a dummy variable of whether a state moved to universal mail-in balloting from 2016 to 2020. The control group contains states that remained at Level 1. The regression format is as follows:

$$(\text{vote}_i^{20} - \text{vote}_i^{16}) = \chi \cdot \text{treated}_i + \lambda \cdot \text{vote}_i^{12} + \gamma_1 \vec{X_i^{20}} + \gamma_2(\vec{X_i^{20}} - \vec{X_i^{16}}) + \alpha + \epsilon_i \tag{10}$$

$\hat{\chi}$ is the estimate that represents the causal effect of moving to universal mail-in balloting, controlling for all other characteristics, as above. Regressions that estimate the causal effect of mail-in balloting do not require an instrumental variable, as the difference-in-difference approach effectively controls for this (Section 7).

The key identification assumption for a difference-in-difference approach is that in the absence of the change in mail-in balloting policy, the change in the outcome would have been the same for the two groups. This parallel trends assumption is assessed in Section 7.1 (Figure 4) and visually appears to hold.

5 Data

Because this project attempts to combine weather, protests, and elections, there are three main data sources, as well as controls. All summary statistics are at the city-level, though most regressions will be run aggregated at the county-level due to election data availability.

5.1 Weather Data

Both historical and 2020 June precipitation data come from the National Oceanic and Atmospheric Administration (NOAA). Meteorological data are collected by weather stations across the USA, synthesized, and reported out to the public with less than 24 hours of lag time. The NOAA also compiles 30-year historical data for most of its constituent weather stations. Though the data are by weather station, they can be easily be converted to city-level. When more than one weather station corresponds to a city, an average of reported precipitation is used (Houston, for example, is represented by 12 weather stations in the historical averages). The most recent historical period is 1980-2010, and as such I use that period as a control. A summary table of historical rainfall by month as well as June 2020 rainfall is presented in Table 1 (unweighted by population).

June & July have historically been the months with the highest median precipitation and highest variation in precipitation. The IV methodology should provide reasonable results, since there is some meaningful variation in precipitation in summer months. Though June 2020 appears to be a dry month relative to historical averages, the first stage regression yields favorable results, as rainfall decreases turnout by approximately 0.7 units (see Section 6.1 for full results).

5.2 Protest Data

Protest size data was compiled by the Crowd Counting Consortium (CCC), which emerged out of a collaborative effort to provide an accurate estimate of the number of people who

Table 1: Monthly Rainfall Summary (Source: NOAA)

Month	Min	Median	Mean	Max	Std Dev.
Jan	0.49	6.85	7.14	38.80	4.30
Feb	0.77	6.48	6.76	26.67	3.81
Mar	0.60	7.93	7.85	34.11	3.71
Apr	0.122	8.16	7.33	29.32	3.36
May	0.05	9.28	8.24	20.88	4.09
June	0	9.97	8.56	25.64	4.84
July	0	9.35	8.09	27.46	4.89
Aug	0.01	8.57	7.71	25.76	4.81
Sep	0.03	8.55	7.63	35.01	4.48
Oct	0.58	7.92	7.39	49.01	3.83
Nov	0.37	7.95	7.76	43.01	4.33
Dec	0.62	7.38	7.63	36.86	4.50
June 2020	0	2.43	2.83	14.12	2.34

participated in the Women's March on Washington in January 2017. The CCC collects publicly available data on political crowds reported in the United States, including marches, protests, strikes, demonstrations, and riots. The CCC has identified BLM related protests in over 2200 cities across the US, covering all 50 states. Protest size is calculated by leveraging both local reporting and satellite imagery when available. 980 cities make it into the final sample, as there are data availability issues and protests under 10 people are cut from the data.

Since the protest data is aggregated at the month level, it is likely that the protest size variable double counts enthusiastic protesters who protested more than once. In other words, protest per capita is better understood as protester-days per capita (the same protester on two different days counts as two protesters). This may be more of a feature than a bug because locations that have enthusiastic protesters are given more weight. Table 2 summarizes the protest size per 100 capita data (unweighted).

Protesters per 100 capita is a useful transformation for interpretation, as it can be understood as percent of the population that protested (a value of 2 indicates 2% of the city's

Table 2: Crowd Size Summary

Min	25th Percentile	Median	Mean	75th Percentile	Max	Std. Dev
0.0068	0.56	1.35	3.69	3.37	135.74	8.74

population protested). This variable has fairly good properties, but is above 100 for two cities (Montpelier, VT and Wilton, CT). This is likely because these towns are small (populations are less than 10,000), very liberal, and likely had individuals from neighboring cities and towns come to protest. These observations are removed from the data to reduce the influence of outliers. The cities included in the sample cover more than 98 million people, approximately one-third of the US population. They also include 4.1 million protesters.

5.3 Election Data

The project requires both historical and 2020 election data, including total votes and vote share for each party at the city level. 2020 election data were purchased from David Leip's Election Atlas, a well known political science resource. Aggregating precinct-level data (available only for 2016) to city-level data is a near impossible task, with tens of thousands of precincts and unavailable accurate precinct-to-city mappings. As a result, regressions will be aggregated to the county-level.

Given the identification assumption—city-wide protests influence voting behavior within and just outside of the city—the county-level approach results in lower power given that most counties may spread beyond the immediate vicinity of a large city. This is mitigated by the fact that most counties only have one large population center, where protests occur. The city-level sample includes 980 cities, and the county conversion reduces the sample size to 761 (only 200 or so counties encompass two or more of the cities in the original sample).

To estimate the causal effect of mail-in balloting, data from the National Conference of State Legislatures and FiveThirtyEight are used to divide states into four levels of access:

1. Receiving a mail-in ballot requires an excuse and the pandemic is not a valid excuse

(5)[5].

2. Receiving a mail-in ballot requires an excuse, the pandemic is a valid excuse, but nothing is mailed automatically (17).

3. Receiving a mail-in ballot requires an excuse, the pandemic is a valid excuse, and applications are sent to all eligible voters (16)[6].

4. All eligible voters are sent a ballot in the mail (11).

The main difference-in-difference estimate compares counties that moved from Level 1 to Level 4 from 2016 to 2020 with counties that remained at Level 1. Five states had universal mail-in balloting prior to 2020 and they are excluded from this regression. Other regressions presented in Section 7 estimate the effect of each level of access as well.

5.4 Controls

Demographic variables (population, education, median household income, etc.) have been collected from the Census Bureau and Harvard's Social Explorer (which leverages the Census Bureau and ACS survey results). For all target election target years, 5-year ACS Survey estimates have been used. For example, 2012-2016 ACS Survey estimates are used for 2016 demographic controls. I also use unemployment figures from Harvard's Social Explorer, a potentially useful control given the economic turmoil of 2020.

Extensions presented in Sections 6.4 (Racial Animus) and 6.5 (COVID-19) employ other data sources, which are described before results.

[5] The numbers in parentheses indicate how many states are at the given level

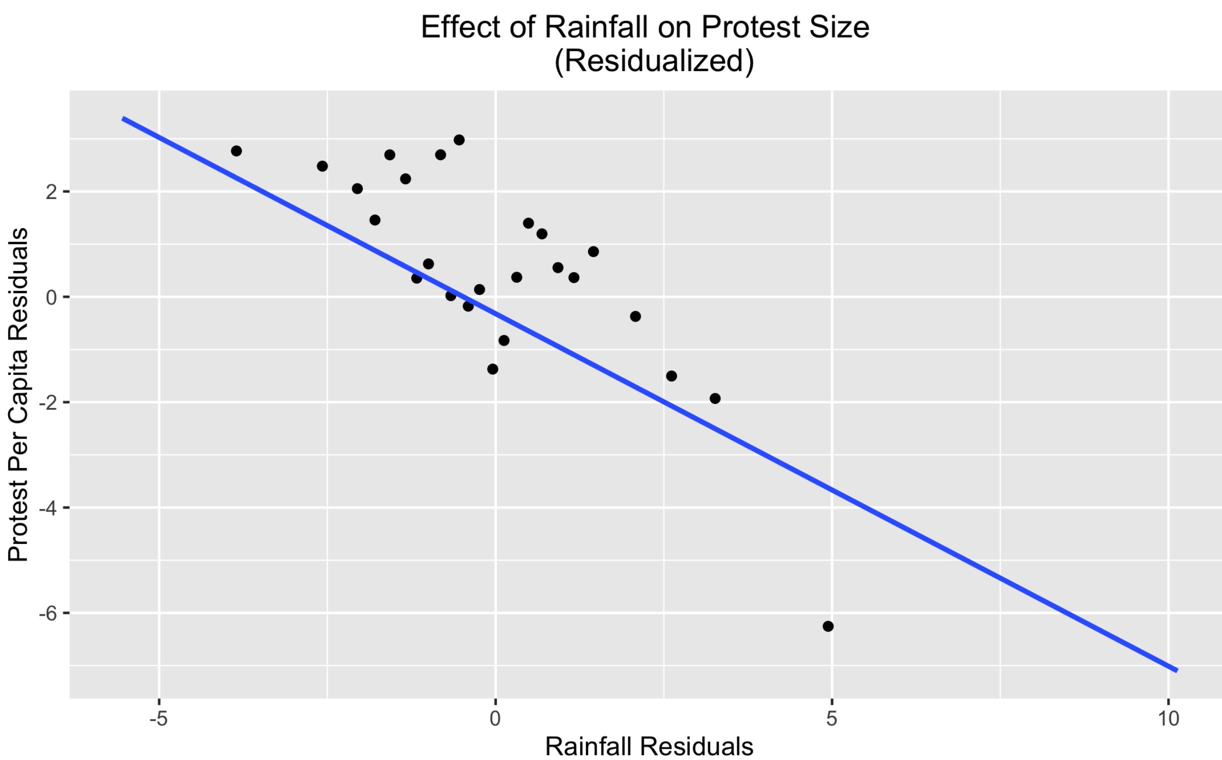
[6] Florida and New Mexico are coded as 2.5 because there are differences among counties

6 Results & Discussion

6.1 First Stage Regression

Figure 1 shows the binned residual-on-residual scatter plot of June 2020 precipitation and protest size at the county-level. This enables a 2D-visualization of the key variables from a higher-dimensional regression, an application of the Frisch-Waugh-Lovell theorem (Frisch and Waugh (1933) and Lovell (1963)). The figure displays a clear negative, strong, linear trendline providing suggestive evidence for the validity of this instrument. Regression analysis (Tables 3 & 4) confirms the visual results.

Figure 1: Binned residual scatterplot from county-level Model 8



I use several specifications to test the strength of the June 2020 rainfall instrument, with appropriate log transformations for right-skew variables (% Black, Median HHI). The issue with weak instruments is that 1) 2SLS estimates will be biased toward OLS and 2) standard errors are not reliable because the usual asymptotic theory does not provide a good approximation of the sampling distribution of the 2SLS estimate. To assess the strength of the rainfall instrument, I do two things, as suggested by Andrews, Stock, and Sun (2019). First, I report F statistics to gauge the strength of the instrument (see bottom-most row in Table 3). Second, I report Anderson and Rubin (1949) confidence intervals which are robust to weak instruments for 2SLS regressions (Tables 7 & 8). Table 3 summarizes the city-level results. All presented results use a weighted regression based on protest size. Reported standard errors use the HC1 estimator.

All specifications yield a negative coefficient on the rainfall instrument, consistent with our model assumptions. A coefficient of -0.8 (Model 8) indicates that a one-inch increase in rainfall in June is associated with a 0.8 percentage point decrease in protest size (per capita). Given that the population weighted mean of protest size per capita and June 2020 rainfall is only 4.2 and 2.9 respectively, a coefficient of -0.8 represents a fairly large relative effect size (a 35% increase in monthly rainfall is associated with a 20% relative decrease in protest size). Unsurprisingly, this is smaller than the effect found in Madestam et al. (51%) as these rainfall totals are aggregated at the month-level instead of the day-level.

All models show that June Rainfall is statistically significant at the $p = 0.05$ level. Without historical rain controls, the F-statistic increases to about $\approx$ 18, but decreases to 8.18 and 10.5 in Models 7 (full controls) and 8 (backward variable selection approach) respectively. A first-stage F-statistic of 10 indicates that the bias of two-stage least squares is at most 30% of the bias of ordinary least squares using the heteroskedasticity robust critical values introduced by Montiel Olea & Pflueger (2013). Fairly high F-statistics and negative point estimates are suggestive evidence that rainfall is a strong, valid instrument. However, since I only have election data at the county-level, I present county-level regression estimates as well. Aggregating by county also enables several additional controls, like unemployment (Section 6.2).

As in Table 3, Table 4 also shows all specifications with a negative coefficient on the rainfall instrument. Without historical rain controls, the F-statistic increases to about $\approx$ 14,

Table 3: City-Level First Stage Regression Results

	Dependent variable:							
	Protest Per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
June 2020 Rain	−3.522***	−3.328***	−2.793***	−2.944***	−2.746***	−0.825***	−0.772***	−0.799***
	(1.215)	(1.140)	(0.686)	(0.740)	(0.642)	(0.304)	(0.270)	(0.247)
Historical June Control	1.379**	1.679***	1.616***	1.571***	1.717***	−0.468	−0.182	
	(0.593)	(0.570)	(0.394)	(0.396)	(0.354)	(0.392)	(0.367)	
Black Pop	8.027			4.446	−6.310	−6.802	−13.196***	−12.632***
	(10.610)			(6.971)	(5.179)	(5.132)	(3.250)	(3.245)
Median HHI		0.0002***		−0.00002	−0.0001	0.00002	−0.00001	
		(0.0001)		(0.0001)	(0.00005)	(0.00003)	(0.00002)	
No High School			0.797***	0.736**	0.346	0.863***	0.599***	0.600***
			(0.298)	(0.332)	(0.315)	(0.136)	(0.115)	(0.110)
High School			−0.335***	−0.337***	−0.199**	−0.304***	−0.206***	−0.204***
			(0.067)	(0.076)	(0.086)	(0.038)	(0.028)	(0.030)
Bachelor+			0.561***	0.582***	0.406**	0.404***	0.279***	0.272***
			(0.116)	(0.140)	(0.165)	(0.064)	(0.044)	(0.043)
Partisan Lean					−0.213***		−0.153***	−0.153***
					(0.064)		(0.025)	(0.028)
Observations	931	924	923	923	923	923	923	923
R^2	0.312	0.354	0.544	0.547	0.628	0.817	0.851	0.851
Adjusted R^2	0.310	0.352	0.542	0.544	0.625	0.814	0.848	0.849
F-Statistic	8.41	8.53	16.56	15.84	18.32	7.40	8.18	10.50
Note:	*p<0.1; **p<0.05; ***p<0.01							

City-level relationship of June 2020 Rainfall on Protest Size. Model 8 created via a backward variable selection approach on Model 7. Standard errors reported using the HC1 estimator.

Table 4: County-Level First Stage Regression Results

	Dependent variable:							
	Protest Per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
June 2020 Rain	−3.423***	−1.929***	−1.817***	−1.456***	−0.664**	−0.711***	−0.718***	−0.697***
	(1.068)	(0.663)	(0.451)	(0.388)	(0.263)	(0.269)	(0.268)	(0.216)
Historical June Control	1.237***	1.474***	0.823***	0.918***	−0.342	−0.221	−0.138	
	(0.474)	(0.425)	(0.292)	(0.241)	(0.303)	(0.299)	(0.299)	
Black Pop			−0.113	−2.505***	−0.979	−2.108***	−2.062***	−2.067***
			(0.539)	(0.627)	(0.741)	(0.505)	(0.519)	(0.633)
Median HHI	−13.228***		−18.573***	−15.914***	−11.987***	−11.325***	−10.686***	−10.695***
	(3.700)		(3.155)	(2.395)	(2.215)	(1.872)	(1.619)	(1.657)
No High School		1.675***	1.134***	0.577**	1.196***	0.901***	0.956***	0.968***
		(0.288)	(0.279)	(0.286)	(0.155)	(0.213)	(0.205)	(0.219)
High School		−0.416***	−0.334***	−0.158	−0.380***	−0.258***	−0.254***	−0.262***
		(0.106)	(0.129)	(0.098)	(0.070)	(0.073)	(0.075)	(0.085)
Bachelor+		0.374***	0.743***	0.414***	0.531***	0.373***	0.336***	0.340***
		(0.137)	(0.155)	(0.121)	(0.107)	(0.106)	(0.103)	(0.109)
Dem Vote Share 2016				27.844***		−8.064	−6.093	
				(5.314)		(18.295)	(18.876)	
Repub Vote Share 20216						−23.923	−22.789	−16.357***
						(16.197)	(16.616)	(4.841)
March-May Unemp Change							−0.182	−0.198
							(0.148)	(0.153)
Observations	749	749	749	749	760	749	749	749
R^2	0.544	0.718	0.825	0.865	0.905	0.915	0.916	0.916
Adjusted R^2	0.542	0.716	0.823	0.863	0.903	0.913	0.914	0.914
F-Statistic	10.30	8.47	16.24	13.99	6.40	6.97	7.13	10.43
Note:	*p<0.1; **p<0.05; ***p<0.01							

County-level relationship of June 2020 Rainfall on Protest Size. Model 8 created via a backward variable selection approach on Model 7. Standard errors reported using the HC1 estimator.

Table 5: Interaction Instrument Joint F-statistics

IV Model	1	2	3	4	5	6	7	8
Joint F-Statistic	10.43	5.05	7.84	9.79	5.28	7.76	5.78	10.28
June Rainfall	✓	✓	✓	✓	✓	✓	✓	✓
% Black Interaction		✓			✓		✓	✓
% Unemployed Interaction			✓		✓	✓	✓	✓
Median HHI Interaction				✓		✓	✓	✓
All (LASSO)								✓

Joint F-statistics of linear models predicting protest size using rainfall and interacted term (for instrument selection). LASSO used on full set of interactions.

but decreases to 7.13 and 10.43 in Models 7 (full controls) and 8 (backward variable selection approach) respectively. Relative to the city-level estimates, this is a weaker instrument, as one would suspect. Aggregating by county reduces some of the power of the rainfall instrument. Still, an F-statistic of 7 corresponds to the 2SLS model having 35% of the bias of OLS at most (Montiel Olea & Pflueger, 2013). As in the city-level specifications, the rainfall effect size is fairly large relative to mean protest per capita.

To increase the strength of the instrument specification, I leverage interaction variables. Interacting an instrument with controls produces new, valid instruments (see the Appendix for a short proof). I provide some new specifications with their joint F-statistics. These interaction instruments strengthen the exogeneity condition in the 2SLS regression. The baseline for all interaction specifications is Model 8 from Table 4 (Model 7 but with some historical months removed).

Model 8 in Table 5 includes all possible interaction instruments which are then fed through a regularizing LASSO regression which removes several interactions. Even still, the F-statistic does not exceed Model 1's F-statistic. Balancing prediction quality and a parsimonious specification, I proceed with Model 4, which has a fairly high joint F-statistic and adds a second instrument, strengthening the overall prediction (the joint F-statistic is lower because there are two instruments now, but the overall prediction power is higher).

6.2 Unemployment Effects

A priori, one may suspect that unemployment has some explanatory power for 2020 protests, as the economic downturn may have spurred increased uproar. I summarize some effects of unemployment on protest size in Table 6 (county-level data) based on the full control set of Model 8 from Table 4. Monthly unemployment figures are absolute (first four specifications).

Table 6: Unemployment Effects

	Dependent variable:					
	Protest Per Capita					
	(1)	(2)	(3)	(4)	(5)	(6)
June 2020 Rainfall	-0.540^{**}	-0.587^{**}	-0.651^{**}	-0.720^{***}	-0.718^{***}	-0.742^{***}
	(0.227)	(0.248)	(0.257)	(0.264)	(0.268)	(0.269)
March Unemp	-0.863^{*}					
	(0.444)					
April Unemp		-0.443^{***}				
		(0.144)				
May Unemp			-0.376^{**}			
			(0.150)			
June Unemp				-0.370^{**}		
				(0.171)		
March-May Change					-0.182	
					(0.148)	
March-June Change						-0.140
						(0.173)
Observations	749	749	749	749	749	749
R^2	0.919	0.922	0.919	0.918	0.916	0.915
Adjusted R^2	0.917	0.920	0.917	0.916	0.914	0.913
Note:					*p<0.1; **p<0.05; ***p<0.01	

Effect of Unemployment Rates on Protest Size. Specifications 5 and 6 use a change in unemployment to control for baseline. Standard errors reported using the HC1 estimator.

It appears that higher unemployment led to smaller protests, a surprising result. However, once we control for baseline unemployment rates and instead estimate the effect of changes in unemployment rate since the onset of COVID-19 lockdowns (e.g. March to May), we find no significant effect. Given these results, it seems safe to conclude that protests appear to be unaffected by economic strife and were borne more out of racial grievances. The point estimates for rainfall remain significant and of the same magnitude as in Table 4.

6.3 2020 Election Outcomes

This section explores the effect of protesting on two 2020 election outcomes using the Equation 7 regression format: county-level normalized Republican vote share margin (NRM) and normalized turnout (NT). Equations 10 and 11 presents the explicit form of NRM and NT.

$$\underbrace{\mathrm{NRM}_i^{20}}_{\text{Normalized Margin}} = \underbrace{\mathrm{RVS}_i^{20}}_{\text{Republican Vote Share}} - \mathrm{DVS}_i^{20} - \underbrace{(\mathrm{RVS}^{20} - \mathrm{DVS}^{20})}_{\text{Popular Vote Margin}} - \mathrm{NRM}_i^{16} \tag{11}$$

$$\mathrm{NT}_i^{20} = \mathrm{Turnout}_i^{20} - \mathrm{Turnout}_i^{16} - (\mathrm{Turnout}^{20} - \mathrm{Turnout}^{16}) \tag{12}$$

RVS_i^y and DVS_i^y are the Republican and Democratic vote shares in county i in year y. Without the subscript, they represent the national vote shares. This construction attempts to mitigate the effect of third-party vote share and any overall changes in the national political environment. By differencing out national changes, it is easier to approximate county-specific changes in turnout / margin. Normalized turnout is a similar construction, which differences out the national turnout rates.

As mentioned in the Methodology Section, both the absolute controls and the difference from the prior election year's controls are used in all regressions. The following categories of controls are used and added sequentially to specifications in Tables 7 and 8:

1. Economic: Unemployment, Median HHI, Gini Index, Poverty Rates, Population Density
2. Race: Black, Asian, Hispanic
3. Education: No high school, high school, college degree or more
4. Age: Binned Age brackets (18-34, 35-64, 65+)

5. Election Outcomes: Prior election outcomes, turnout rates

Expansion of mail-in balloting may have been a crucial factor in turnout and potentially vote share as well. Since June rainfall is likely orthogonal to mail-in balloting decisions/changes, the IV regression estimates presented in this section—in effect—control for such changes. Still, there are natural controls in the data and Section 7 explores mail-in balloting as an independent explanatory variable in the 2020 election.

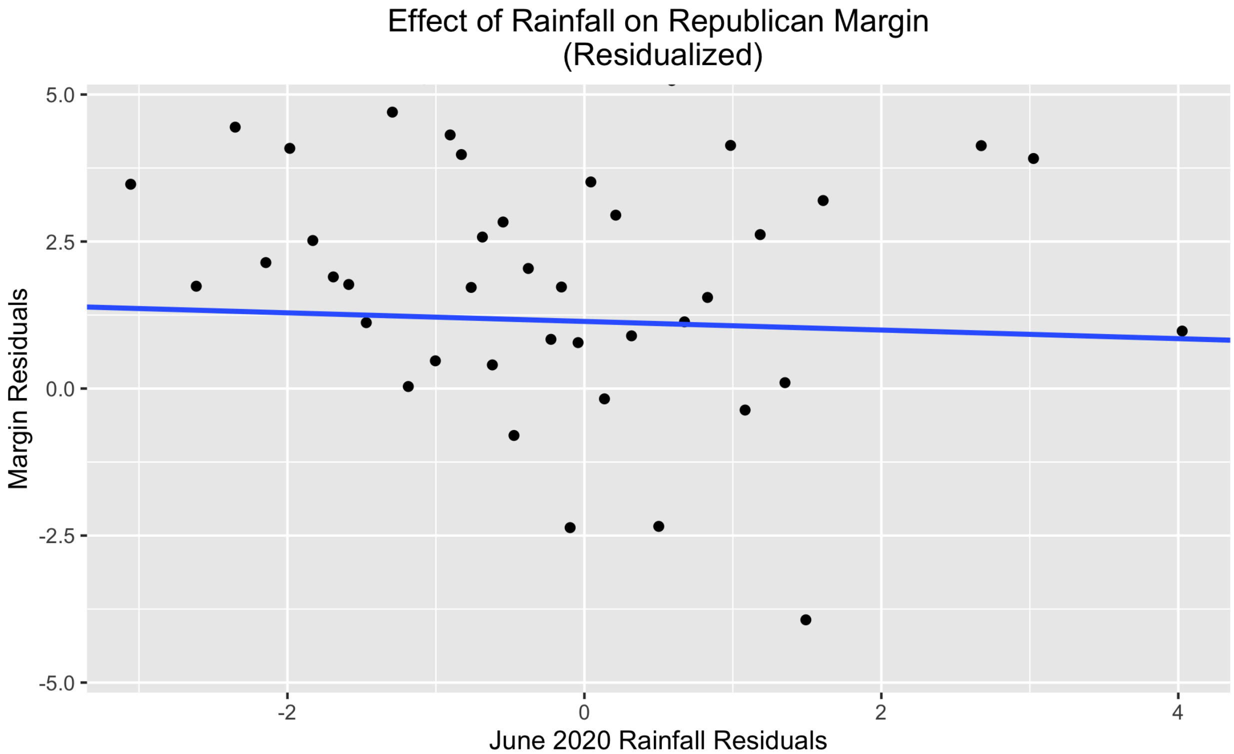
To show the effect of the instrument on the 2SLS estimate, Figures 2a and 2b display the relationship between rainfall and the margin/turnout respectively (these are the reduced form scatterplots; placebo reduced forms for 2012 and 2016 are presented in Section 6.6 and the Appendix, respectively). Increased rainfall leads to lower Republican margin and lower turnout. The scaling of the x and y axes are the same in both panels. The impact of rainfall is larger for turnout than for Republican margin.

Theoretically, increased rainfall lowers protest size (protesters become more unwilling to go outside). This leads to lower social pressure on voters (π_r from Section 3.1, Equation 3) because protest salience is lower. Ultimately, this leads to lower turnout. I also find that Republicans tend to benefit from protests, discussed in further detail below. The 2SLS estimates (Tables 7 & 8) correspond to the ratio between the slopes in Figure 2 and the slope in Figure 1 (first-stage, Section 6.1).

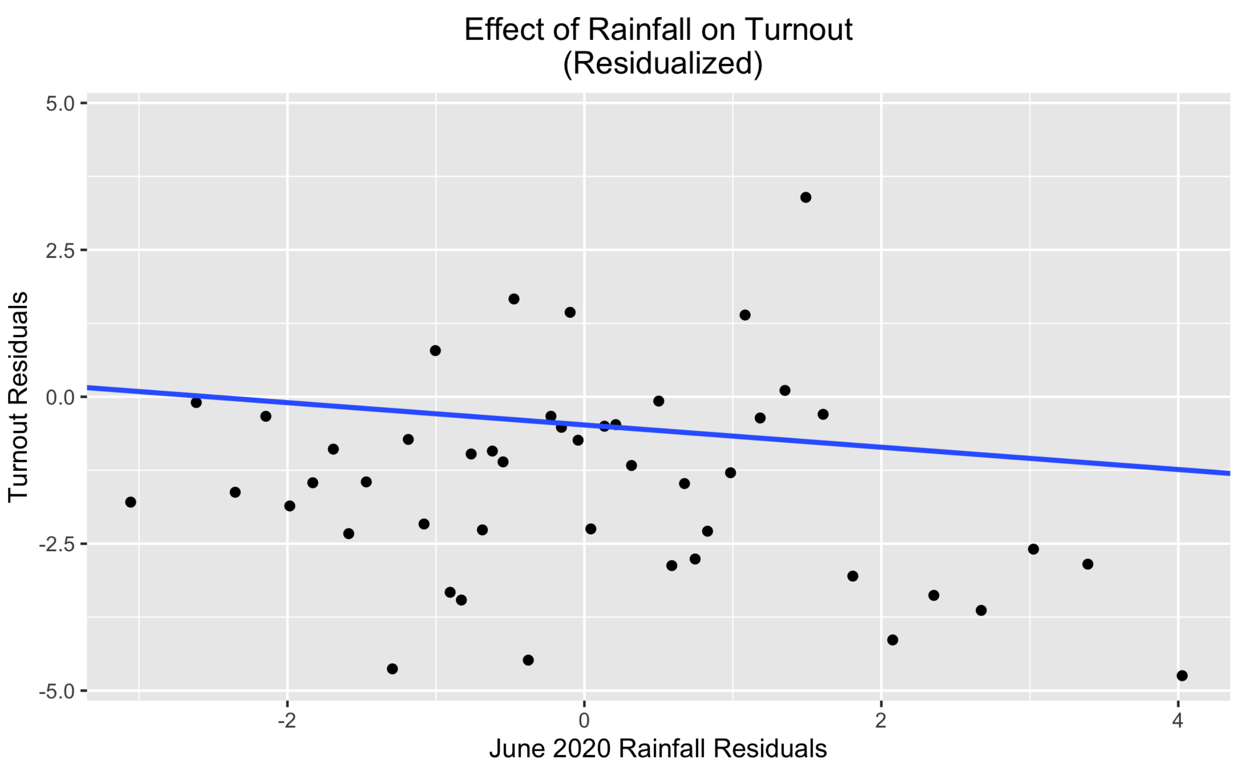
Tables 7 & 8 present the baseline 2020 regression results for margin and turnout respectively, weighted by 2019 county-level population. Weighting by protest size produces similar results, with slightly lower overall effect sizes (see Appendix Tables 29 & 30). Specifications sequentially include the above list of controls; all specifications include prior election outcomes.

The effect of protests was quite small on margin, and a bit larger on turnout. For a 1-unit increase in per capita protest size, Republican margin increased by approximately 0.03 percentage points and turnout increased by 0.17 percentage points. The margin effect size appears to be robust and significant for various specifications, while the turnout effect

Figure 2: Residualized Outcome Scatterplots vs. Rainfall



(a) Binned Margin Scatterplot



(b) Binned Turnout Scatterplot

is only precisely estimated in specification 4.

Table 7: Instrumental Variable Estimates of the Impact on 2020 Margin

	Dependent variable:			
	Republican Margin			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.041***	0.025**	0.023*	0.033**
	(0.011)	(0.012)	(0.013)	(0.013)
2016 Margin	−0.977***	−0.971***	−0.972***	−0.980***
	(0.005)	(0.008)	(0.008)	(0.006)
Partisan Lean	1.937***	1.925***	1.926***	1.941***
	(0.011)	(0.015)	(0.015)	(0.012)
Election Controls	✓	✓	✓	✓
Economic Controls	✓	✓	✓	✓
Race Controls		✓	✓	✓
Education Controls			✓	✓
Age Controls				✓
Observations	749	749	749	749
R^2	0.994	0.995	0.995	0.996
Adjusted R^2	0.994	0.995	0.995	0.996
Anderson-Rubin CI	(0.009, 0.073)	(0.011, 0.039)	(.005,0.042)	(0.001, 0.061)
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 Presidential Republican Margin (IV approach). Specifications use increasing set of controls. Standard errors reported using the HC1 estimator.

Importantly, these regressions are estimating location-specific effects, differencing out overall national changes. Total 2020 election turnout was historically high at 62%, 7.5 points higher than 2016 turnout.[7] Counties that saw large BLM protests had slightly higher turnout, though voters opposed to the protests were more highly mobilized. That is, turnout changed such that overall margin for Republicans increased.

To get a relative sense of the effect sizes, note that the median protest size is 0.63, thereby resulting in a 0.019 percentage point increase in predicted Republican margin and a 0.11 percentage point increase in predicted turnout over a county with no protests. DellaVigna

[7] This is a comparison of the voting-age population (VAP) turnout rate, not voting-eligible population (VEP) turnout rate (66%). The VEP difference between 2016 and 2020 was similar (6 percentage points)

Table 8: Instrumental Variable Estimates of the Impact on 2020 Turnout

	Dependent variable:			
	Turnout			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.027	0.011	−0.005	0.172**
	(0.068)	(0.066)	(0.062)	(0.075)
2016 Turnout	−0.198***	−0.123***	−0.180***	−0.250***
	(0.043)	(0.043)	(0.047)	(0.049)
Partisan Lean	−0.360*	−0.525**	−0.420**	−0.509***
	(0.205)	(0.210)	(0.212)	(0.189)
Election Controls	✓	✓	✓	✓
Economic Controls	✓	✓	✓	✓
Race Controls		✓	✓	✓
Education Controls			✓	✓
Age Controls				✓
Observations	749	749	749	749
R^2	0.505	0.560	0.587	0.649
Adjusted R^2	0.495	0.547	0.573	0.634
Anderson-Rubin CI	(-0.045,0.093)	(-0.059,0.075)	(-0.070,0.067)	(0.011,0.352)
Note:			*p<0.1; **p<0.05; ***p<0.01	

Effect of protest size on 2020 General Election turnout (IV approach). Specifications use increasing set of controls. Standard errors reported using the HC1 estimator.

et al. (2007) estimate that the entry of Fox News into a market increases Republican margin by 0.4 to 0.7 percentage points and turnout by 1.78 percentage points. Madestam et al. (2013) find that tea party protesting led to large multiplier effects: a 1 unit increase in per capita protest size increased Republican margin by 18 percentage points, though note that the average protest was much smaller. Comparing the effect sizes, 2020 BLM protests appear to be far weaker than Fox News or Tea Party protests. Potential hypotheses for the lower effect sizes are discussed in Section 8.

To more precisely understand the effect of protests, several regressions with interactions on the protest per capita variable are presented, as there may be a differential impact of protests based on county-level factors. For example, counties with high black populations may have seen an outsize increase in turnout rates due to BLM protests. This hypothesis has some domain-specific a priori evidential support: national media has extensively covered the effect of black turnout in the Georgia general and runoff elections, which helped the Democratic Party win the Presidential election and gain control of the Senate, respectively.

I run five interaction regressions for both outcomes, with the full set of controls and instrumental variables from specification 4 in Tables 7 and 8. With two endogenous variables (e.g. protest per capita and black · protest per capita), I need two instrumental variables. I therefore use both rainfall and black · rainfall as instruments. The interaction variables are % black population, population density, median HHI, % of population with a bachelor's degree or higher, and 2016 margin. Estimates for the protest per capita variable and the interaction term are presented. Whether or not a variable like % black population is significant is irrelevant to the question, as it does not relate to the effect of protesting on electoral outcomes. Thus, those estimates are not presented. Tables 9 and 10 present the results on margin and turnout, respectively.

In Table 9, only one margin interaction regression has significant results. There seems to be differential impacts of protesting based on the median HHI of a county (reported on natural log scale). For the average county (log median HHI of 10.8, median HHI of $50,000),

Table 9: Interaction Estimates of the Impact on 2020 Margin

	Dependent variable:				
	Margin				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	0.054	0.073	-1.010^{***}	0.007	0.030
	(0.042)	(0.044)	(0.299)	(0.061)	(0.028)
Black Interaction	-0.009				
	(0.017)				
Pop. Density Interaction		-0.004			
		(0.004)			
Median HHI Interaction			0.095^{***}		
			(0.027)		
Bachelor+ Interaction				0.0004	
				(0.001)	
2016 Margin Interaction					-0.00001
					(0.0003)
Observations	749	749	749	749	749
R^2	0.996	0.996	0.996	0.996	0.996
Adjusted R^2	0.996	0.996	0.996	0.996	0.996
Note:					*p<0.1; **p<0.05; ***p<0.01

Effect of protest size & various interactions on 2020 Presidential Republican margin (IV approach). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

an additional unit of per capita protest increases Republican margin by 0.02 percentage points. For a county with median HHI of $134,000 (log median HHI of 11.8), the effect increases to 0.12 percentage points. On the other hand, protests in poorer counties benefit Democrats. For a county with median HHI of $18,000 (log median HHI of 9.8), the effect is -0.07 percentage points. These are fairly low overall effect sizes, though they may have tipped the scales in close elections (Georgia, for example).

On the turnout side (Table 10), only the 2016 turnout interaction regression produces significant results. In particular, the effect of a 1-unit increase in protest size for the median county (59.96% turnout) is a 0.224 percentage point increase in turnout. This indicates that protests were more effective in "politically active" counties that have had high turnout in the past.

Table 10: Interaction Estimates of the Impact on 2020 Turnout

	Dependent variable:				
	Turnout				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	−0.149	0.088	2.113	−0.118	2.323**
	(0.220)	(0.255)	(2.857)	(0.176)	(1.029)
Black Interaction	0.114				
	(0.071)				
Pop. Density Interaction		0.007			
		(0.026)			
Median HHI Interaction			−0.180		
			(0.264)		
Bachelor+ Interaction				0.005	
				(0.003)	
2016 Turnout Interaction					−0.035**
					(0.017)
Observations	749	749	749	749	749
R^2	0.635	0.633	0.634	0.634	0.681
Adjusted R^2	0.617	0.615	0.616	0.617	0.665
Note:	*p<0.1; **p<0.05; ***p<0.01				

Effect of protest size & various interactions on 2020 General Election turnout (IV approach). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

One potential explanation for low effect sizes and increased margin for the opposition party is that protests are only effective past a certain threshold. Tables 11 & 12 present results for counties with above average protest per capita values (greater than median), with specifications sequentially adding controls, as in Tables 7 & 8. This reduces the sample size from $n = 749$ to $n = 373$, but still produces significant results. Overall, the effect sizes for both margin and turnout increased, though were directionally the same. In counties with above average protest size, Republicans benefited by 0.05 percentage points and turnout increased by 0.275 percentage points per additional protester per capita. Increased effect sizes indicate that there are some locally specific effects of protesting, though the changes are relatively small, likely due to issue nationalization.

Table 11: Margin Results for Above Average Protests

	Dependent variable:			
	Republican Margin			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.033^{**}	0.012	0.012	0.052^{**}
	(0.013)	(0.018)	(0.018)	(0.020)
2016 Margin	-0.984^{***}	-0.987^{***}	-0.986^{***}	-0.985^{***}
	(0.006)	(0.005)	(0.005)	(0.006)
2020 Lean	1.955^{***}	1.954^{***}	1.952^{***}	1.944^{***}
	(0.011)	(0.010)	(0.011)	(0.013)
Observations	373	373	373	373
R^2	0.995	0.995	0.996	0.997
Adjusted R^2	0.994	0.995	0.995	0.996
Note:			*p<0.1; **p<0.05; ***p<0.01	

Effect of protest size on 2020 Presidential Republican margin (IV approach) for protest sizes above the median (0.63). Specifications use increasing set of controls, as in Table 7. Standard errors reported using the HC1 estimator.

As another segmentation strategy, I split the sample into Democratic and Republican counties, based on if they are more or less Democratic-voting than the national popular vote in 2016. Tables 13 through 16 present results for both segments and outcomes.

Table 12: Turnout Results for Above Average Protests

	Dependent variable:			
	Turnout			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.158**	0.112	0.087	0.275***
	(0.071)	(0.072)	(0.074)	(0.085)
2016 Turnout	−0.270***	−0.224***	−0.250***	−0.314***
	(0.061)	(0.083)	(0.088)	(0.089)
Partisan Lean	−0.744***	−0.827***	−0.763***	−0.765***
	(0.201)	(0.238)	(0.248)	(0.232)
Observations	373	373	373	373
R^2	0.701	0.734	0.752	0.802
Adjusted R^2	0.688	0.718	0.734	0.782
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 General Election turnout (IV approach) for protest sizes above the median (0.63). Specifications use increasing set of controls, as in Table 8. Standard errors reported using the HC1 estimator.

Table 13: Margin Results for Republican Leaning Counties

	Dependent variable:			
	Republican Margin			
	(1)	(2)	(3)	(4)
Protest Per Capita	−0.071*	−0.073**	−0.064*	−0.028
	(0.037)	(0.035)	(0.033)	(0.026)
2016 Margin	−0.974***	−0.972***	−0.977***	−0.986***
	(0.009)	(0.010)	(0.008)	(0.005)
2020 Lean	1.925***	1.924***	1.941***	1.958***
	(0.017)	(0.019)	(0.016)	(0.010)
Observations	525	525	525	525
R^2	0.994	0.994	0.995	0.997
Adjusted R^2	0.994	0.994	0.995	0.997
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 Presidential Republican margin (IV approach) for counties that were more Republican than the nation overall in 2016. Specifications use increasing set of controls, as in Table 7. Standard errors reported using the HC1 estimator.

Table 14: Turnout Results for Republican Leaning Counties

	Dependent variable:			
	Turnout			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.254	0.312	0.279	0.349
	(0.307)	(0.287)	(0.277)	(0.237)
2016 Turnout	-0.152^{**}	-0.151^{**}	-0.221^{***}	-0.348^{***}
	(0.064)	(0.075)	(0.082)	(0.079)
Partisan Lean	-0.823^{***}	-0.813^{***}	-0.637^{***}	-0.725^{***}
	(0.218)	(0.223)	(0.247)	(0.220)
Observations	525	525	525	525
R^2	0.544	0.580	0.619	0.697
Adjusted R^2	0.530	0.562	0.599	0.676
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 General Election turnout (IV approach) for counties that were more Republican than the nation overall in 2016. Specifications use increasing set of controls, as in Table 8. Standard errors reported using the HC1 estimator.

Table 15: Margin Results for Democratic Leaning Counties

	Dependent variable:			
	Republican Margin			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.056^{***}	0.045^{***}	0.042^{**}	0.036^{*}
	(0.015)	(0.016)	(0.017)	(0.020)
2016 Margin	-0.968^{***}	-0.963^{***}	-0.965^{***}	-0.980^{***}
	(0.006)	(0.009)	(0.009)	(0.013)
Partisan Lean	1.931^{***}	1.918^{***}	1.916^{***}	1.943^{***}
	(0.012)	(0.019)	(0.018)	(0.024)
Observations	224	224	224	224
R^2	0.995	0.995	0.995	0.996
Adjusted R^2	0.994	0.995	0.995	0.996
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 Presidential Republican margin (IV approach) for counties that were more Democratic than the nation overall in 2016. Specifications use increasing set of controls, as in Table 7. Standard errors reported using the HC1 estimator.

Table 16: Turnout Results for Democratic Leaning Counties

	Dependent variable:			
	Turnout			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.123^{*}	0.125^{*}	0.120^{*}	0.197^{***}
	(0.068)	(0.067)	(0.068)	(0.068)
2016 Turnout	-0.134^{***}	-0.086^{**}	-0.111^{**}	-0.150^{***}
	(0.031)	(0.039)	(0.048)	(0.042)
Partisan Lean	0.113^{*}	-0.008	0.013	0.002
	(0.060)	(0.080)	(0.085)	(0.078)
Observations	224	224	224	224
R^2	0.680	0.704	0.710	0.768
Adjusted R^2	0.657	0.673	0.674	0.728
Note:				*p<0.1; **p<0.05; ***p<0.01

Effect of protest size on 2020 General Election turnout (IV approach) for counties that were more Democratic than the nation overall in 2016. Specifications use increasing set of controls, as in Table 8. Standard errors reported using the HC1 estimator.

Quite surprisingly, protests led to higher Republican margins in Democratic counties, and higher Democratic margins in Republican counties, though the latter effect is not statistically significant ($p = 0.05$) for all specifications. The turnout effects appear to be similar across all counties, although the Republican county specifications are not statistically significant. This indicates that the overall effect was primarily driven by a backlash against BLM protests in Democratic areas, which slightly increased Republican margins in those counties. Republican areas—even if they had protests—did not see much of a change in electoral outcomes.

To check if protests had heterogeneous impacts based on the ability of voters to influence outcomes, I subset to battleground states as well. In Section 3.1, it was shown that when there are many voters, voting is not influenced by probability of casting the deciding vote, since this probability is vanishingly small in the limit. Intuitively, though, it is still possible that voters in "swing" or "battleground" states have a belief that their vote is crucial to the outcome of the election. As a result, they may be more motivated to vote based on factors such as protests. This hypothesis would predict that the effect sizes for protests should be higher in battleground states.

Battleground states are defined as states in which the FiveThirtyEight's pre-election forecast does not predict a favorite of greater than 70%. More concretely, Trump was given a 62% chance of winning Texas, making it count as a battleground. Wisconsin, where Biden had a 94% chance of winning, does not meet the criterion for a battleground state. FiveThirtyEight's forecasts use demographics, polls, and prior voting patterns and are widely considered among the best. Only seven states count as battlegrounds: Arizona, Florida, Georgia, Iowa, North Carolina, Ohio, and Texas. Results for margin and turnout are presented in Table 17.

Table 17: Results for Battleground States

	Dependent variable:	
	Margin	Turnout
	(1)	(2)
Protest Per Capita	0.026	−0.114
	(0.023)	(0.186)
2016 Margin	−0.978***	0.222***
	(0.004)	(0.041)
Partisan Lean	1.951***	−0.389***
	(0.009)	(0.089)
Observations	178	178
R^2	1.000	0.862
Adjusted R^2	1.000	0.830
Note:	*p<0.1; **p<0.05; ***p<0.01	

Effect of protest size on both outcomes (IV approach) for counties in battleground states, as defined by FiveThirtyEight pre-election forecasts. Specifications use full set of controls. Standard errors reported using the HC1 estimator.

The estimates for both outcomes are statistically insignificant. The non-result here underscores the correctness of the voter turnout model. People are not differentially motivated to vote based on their ability to sway an election because the probability of doing so is extremely small.

Taken together, the results in this section indicate that protests had mixed effects. Overall, they increased turnout, but benefited Republicans slightly more. Democrats benefited more in poorer counties while Republicans benefited in richer, Democratic-leaning counties. Turnout differentially increased in politically active counties, though it is unclear whether the higher turnout benefited a particular party. Surprisingly, county-level racial composition and urban/rural divides did not produce heterogeneous effects.

6.4 Racial Animus & IAT

Another possible explanatory variable for differential effect sizes is conscious or subconscious racial animus within a county or state. Seth Stephens-Davidowitz has used Google search data to identify areas that have high levels of racial animus, based primarily on the presence of racial epithets in search queries. Stephens-Davidowitz (2013) finds a 4 percentage point electoral cost to Barack Obama for being black across the country, with higher costs in areas with higher racial animus, even after controlling for prior elections. This variable may have a significant impact on how BLM protests are viewed by individuals within a particular area, as a conscious measure of racial animus.

The Race Implicit Association Test (IAT) has been discussed at length in psychological and, more recently, economic literature. It attempts to measure subconscious racial preferences. Positive scores are indicative of preferences for Europeans over Africans (white vs. black). Scores range from -2 to 2. I use county-level means of the Race IATs as a subconscious measure of animus. Table 18 reports the results of a regression with interactions of protest on both IAT scores and search query data.

Though the search query data is from 2004-2007, I find precisely estimated and consequential effect sizes for protests and racial animus. The racial animus and the interaction terms have negative point estimates. That is, for a median county (protest size of 0.63 and racial animus of 59.5), a one unit increase in protest size results in a -0.42 percentage

Table 18: Results on 2020 Election Controlling for Racial Animus

	Dependent variable:	
	Republican Margin	Turnout
	(1)	(2)
Protest Per Capita	0.178**	0.666
	(0.076)	(0.457)
Race IAT	−1.030	−3.355
	(0.729)	(4.280)
Racial Animus	−0.008***	0.013
	(0.003)	(0.017)
Protest-IAT Interaction	−0.038	−1.218
	(0.287)	(1.217)
Protest-Animus Interaction	−0.002	−0.002
	(0.001)	(0.006)
Observations	727	727
R^2	0.996	0.676
Adjusted R^2	0.996	0.659
Note:	*p<0.1; **p<0.05; ***p<0.01	

Effect of protest size, racial animus (measured using county-level IAT and DMA-level google search data for racial epithets), and interactions on both outcomes (IV approach). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

point change in margin (helping Democrats). This is quite surprising, but is similar to the counter-intuitive effects seen when splitting among Democratic and Republican counties. There appears to be Republican backlash against protests in lower animus areas and vice versa. Turnout is largely unaffected. These results suggest that the effect of protests was motivating individuals to vote for their party, but not changing minds.

Peculiarly, though there are significant effects of racial animus, IAT scores—a more frequently employed metric—do not produce significant heterogeneity. This may be because IAT scores reflect a more subconscious racial preference. That is, county-level subconscious racial biases may be too similar across the nation and may not manifest themselves as filters through which protests influence behavior. On the other hand, racial epithet use in search queries is a much more overt manifestation of racism, and thus influences behavior. It is notable that margin estimates are directionally similar for both animus and IAT scores (turnout estimates are insignificant across the board).

6.5 COVID-19 Effects

Perhaps there is a COVID-19-related reason for the low effect size of protests. Though the media gave much airtime to the protests, the main issue on voters' minds was the governmental response to the pandemic. This limited the potential of protests to change voting behavior. This hypothesis would indicate that historically high turnout was due more to COVID-19 than to protests. To check this hypothesis, we can estimate the effect of per capita confirmed COVID-19 case data right before the election (10/31). These data are freely available from the Johns Hopkins COVID-19 Database. Table 19 reports results for a regression with full controls (based on specification 4 from Tables 7 & 8).

This hypothesis appears to be incorrect. Margin and turnout are both unaffected by COVID-19 per capita case rates. Protest estimates are largely unchanged and remain precisely estimated in this specification.

Table 19: COVID-19 Control Results

	Dependent variable:	
	Republican Margin	Turnout
	(1)	(2)
Protest Per Capita	0.030**	0.171**
	(0.014)	(0.077)
COVID-19	−1.025	3.740
	(3.838)	(23.765)
Observations	749	749
R^2	0.996	0.658
Adjusted R^2	0.996	0.642
Note:		*p<0.1; **p<0.05; ***p<0.01

Effect of protest size and COVID-19 case data on both outcomes (IV approach). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

6.6 Falsification Tests

To validate the research design, I conduct falsification (placebo) tests on 2016 and 2012 election outcomes. A priori, I expect to find little or no impact of protest size, given that protests occurred in 2020. Tables 20 & 21 summarize the 2016 regressions. Figure 5 in the Appendix shows binned scatterplots for both outcomes, with the same scale of Figure 2.

As expected, none of the estimates are statistically significant, save for the first margin regression. By adding more controls, the point estimate shrinks, indicating that it is largely picking up unobserved variables (likely something akin to political engagement).

Similar tables (22 & 23) for the 2012 election are also presented as an additional check (there may be concerns that 2016 was a unique election). Figure 3 shows binned scatter plots for the 2012 outcomes. Comparing Figure 3 to Figure 2, it is clear that there is far more spread and a weaker trend-line when using 2012 outcomes, as expected. The results are the same: only the first margin regression shows a significant result. These robustness checks provide further evidence that the 2020 regression approximates the true effect of protesting on election outcomes and is not a manifestation of omitted variable bias.

Table 20: 2016 Placebo Margin Results

	Dependent variable:				
	Republican Margin				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	0.468***	0.089	0.139*	0.135	0.150
	(0.133)	(0.110)	(0.082)	(0.093)	(0.092)
2012 Margin	0.163	0.377***	0.303**	0.212**	0.217***
	(0.172)	(0.143)	(0.123)	(0.084)	(0.084)
Partisan Lean	−0.477	−0.942***	−0.922***	−0.678***	−0.690***
	(0.334)	(0.282)	(0.241)	(0.155)	(0.156)
Observations	749	749	749	749	749
R^2	0.449	0.646	0.767	0.805	0.808
Adjusted R^2	0.440	0.637	0.759	0.797	0.800

Note: *p<0.1; **p<0.05; ***p<0.01

Placebo regression estimating effect of 2020 protest size on 2016 Presidential Republican margin (IV approach). Specifications use increasing set of controls (as in Table 7). Standard errors reported using the HC1 estimator.

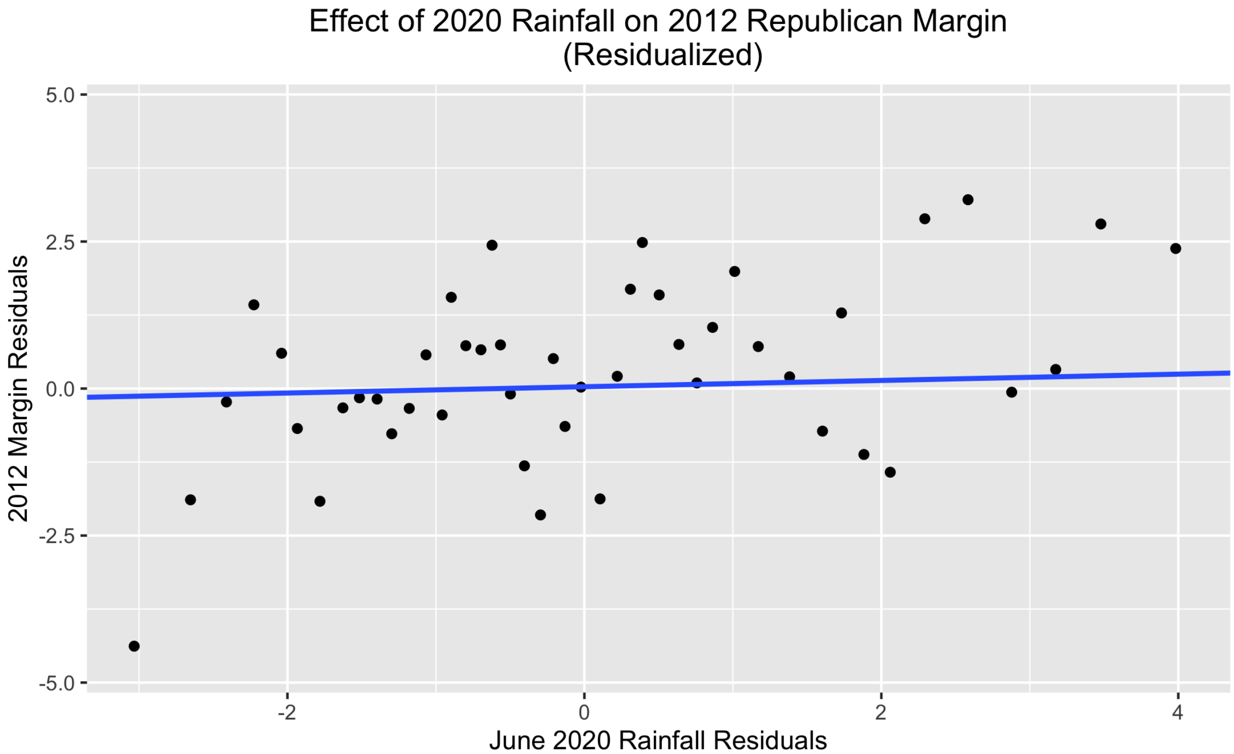
Table 21: 2016 Placebo Turnout Results

	Dependent variable:				
	Turnout				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	0.080*	0.046	0.043	0.018	0.024
	(0.047)	(0.041)	(0.042)	(0.041)	(0.041)
2012 Turnout	−0.148***	−0.139***	−0.152***	−0.182***	−0.187***
	(0.024)	(0.026)	(0.027)	(0.031)	(0.031)
Partisan Lean	0.007	−0.021	−0.027	0.012	0.004
	(0.065)	(0.065)	(0.065)	(0.061)	(0.061)
Observations	749	749	749	749	749
R^2	0.275	0.423	0.435	0.524	0.531
Adjusted R^2	0.261	0.407	0.416	0.504	0.510

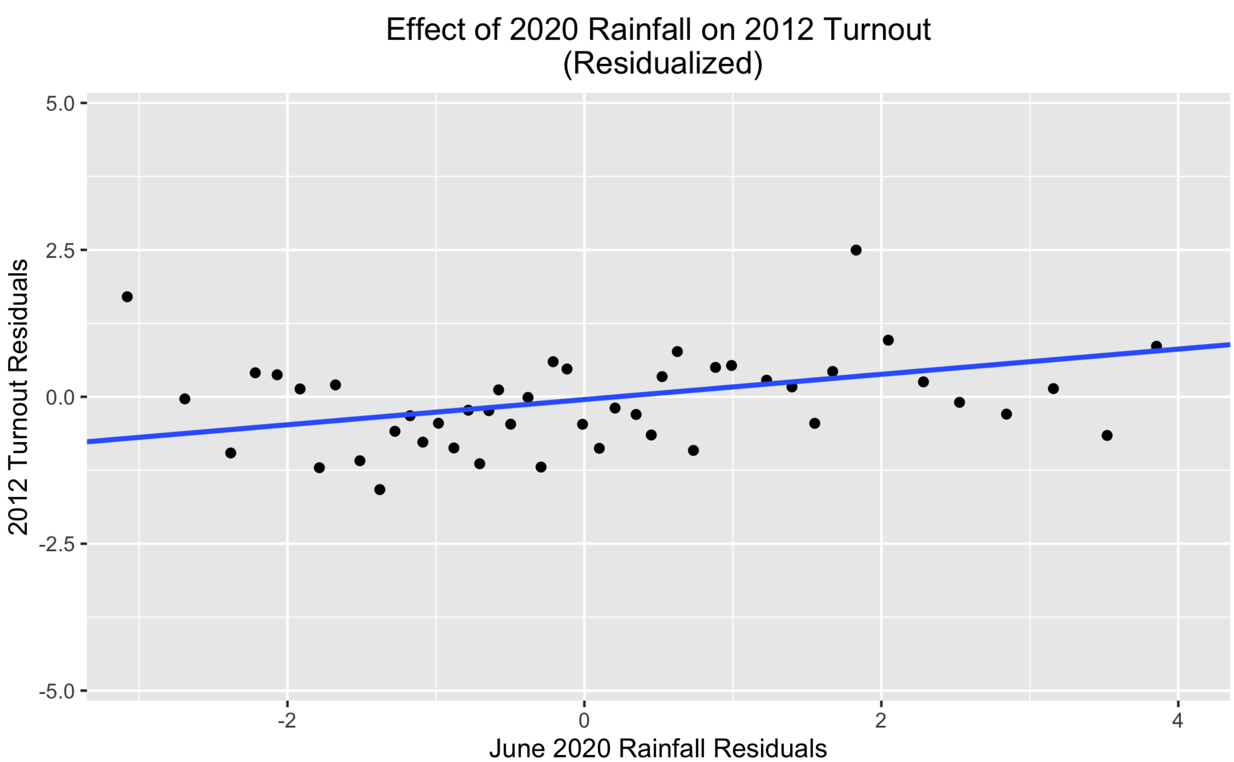
Note: *p<0.1; **p<0.05; ***p<0.01

Placebo regression estimating effect of 2020 protest size on 2016 General Election turnout (IV approach). Specifications use increasing set of controls (as in Table 8). Standard errors reported using the HC1 estimator.

Figure 3: 2012 Residualized Outcome Scatterplots vs. 2020 Rainfall



(a) Binned 2012 Margin Scatterplot



(b) Binned 2012 Turnout Scatterplot

Table 22: 2012 Placebo Margin Results

	Dependent variable:				
	Republican Margin				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	0.432^{***}	0.040	0.128	0.011	0.011
	(0.129)	(0.134)	(0.112)	(0.127)	(0.127)
2008 Margin	-1.860^{*}	-3.831^{***}	-3.760^{***}	-3.085^{***}	-3.079^{***}
	(0.973)	(1.109)	(0.941)	(0.632)	(0.650)
Partisan Lean	3.569^{*}	7.431^{***}	7.160^{***}	5.910^{***}	5.899^{***}
	(1.930)	(2.195)	(1.857)	(1.262)	(1.300)
Observations	749	749	749	749	749
R^2	0.467	0.637	0.745	0.796	0.796
Adjusted R^2	0.458	0.627	0.737	0.788	0.787

Note: *p<0.1; **p<0.05; ***p<0.01

Placebo regression estimating effect of 2020 protest size on 2012 Presidential Republican margin (IV approach). Specifications use increasing set of controls (as in Table 7). Standard errors reported using the HC1 estimator.

Table 23: 2012 Placebo Turnout Results

	Dependent variable:				
	Turnout				
	(1)	(2)	(3)	(4)	(5)
Protest Per Capita	−0.020	−0.015	−0.039	0.019	0.017
	(0.071)	(0.062)	(0.062)	(0.067)	(0.066)
2008 Turnout	0.081^{***}	0.026	0.009	−0.001	−0.011
	(0.025)	(0.026)	(0.027)	(0.024)	(0.024)
Partisan Lean (2008)	−0.408	-1.904^{***}	-1.859^{***}	-2.137^{***}	-2.500^{***}
	(0.700)	(0.688)	(0.667)	(0.584)	(0.568)
Observations	749	749	749	749	749
R^2	0.088	0.205	0.226	0.256	0.277
Adjusted R^2	0.071	0.184	0.201	0.225	0.245

Note: *p<0.1; **p<0.05; ***p<0.01

Placebo regression estimating effect of 2020 protest size on 2012 General Election turnout (IV approach). Specifications use increasing set of controls (as in Table 8). Standard errors reported using the HC1 estimator.

7 Mail-In Balloting

7.1 Difference-in-Difference Estimates of Mail-in Balloting

Though protests appear to have a limited effect on both margin and turnout, the 2020 elections had another potentially significant structural change. Due to the pandemic, several states expanded access to mail-in balloting, theoretically reducing voting costs and leading to increased turnout. It is possible that mail-in balloting expansion led to impacts on margin as well. The four levels described in Section 5.3 are used to assess the effect of mail-in balloting. As a baseline, note that historically universal mail-in states had the highest county-level average turnout in 2020 (77.5%) while new universal states had turnout of 71.4%, Level 1 had 59.8%, Level 2 65.6%, and Level 3 69.97%. At a first glance, this is suggestive evidence that mail-in balloting increases turnout at every level, though of course there may be several confounding factors at play, which motivates the difference-in-difference analysis below.

In prior election cycles, only five states sent mail ballots to all eligible voters; in 2020, this expanded to 11 states. This provides an interesting natural experiment, allowing us to compare states that moved from Level 1 to Level 4 between 2016 and 2020 and those that remained at Level 1 ($n = 68$ and 113 counties respectively). Table 24 presents a difference-in-difference estimate for the effect of universal mail-in balloting. The independent variable is a treatment dummy for whether a county moved to Level 4 in 2020 from Level 1 in 2016 (denoted "Treated" in regression tables). The regression strategy is described in greater detail in Section 5.3 (Methodology). The full set of controls from Section 6.3 is used.

The effect of expanding mail-in balloting is quite large and significant. Republicans *benefited* from universal mail-in balloting by 0.45 percentage points and turnout increased by 2.52 percentage points. The effect sizes are also significant if we cluster at the state level, even though there are only eleven states in the sample (see last row of Table 24).

These effect sizes far exceed those of protesting. Surprisingly, ease of mail-in balloting benefits Republicans, not Democrats, contrary to political assumptions. This may be a

Table 24: Difference-in-Difference Estimate of Universal Mail-in Balloting

	Dependent variable:	
	Republican Margin	Turnout
	(1)	(2)
Treated	0.450***	2.517**
	(0.173)	(1.252)
2016 Margin	−0.976***	0.165***
	(0.007)	(0.056)
2016 Turnout	0.015*	−0.144**
	(0.008)	(0.072)
Observations	181	181
R^2	0.999	0.783
Adjusted R^2	0.998	0.738
State-Level Clustered SE	0.190**	1.482*
Note:	*p<0.1; **p<0.05; ***p<0.01	

Estimate of moving to universal mail-in balloting between 2016 and 2020 on both outcomes (DID approach). Both specifications use full set of controls. Standard errors reported using the HC1 estimator and state-level clustered SEs reported in last row.

result of older voters using mail-in ballots because of the risk of in-person voting in the 2020 election cycle. We can assess this hypothesis by interacting the treatment variable with the percentage of the population above the age of 65. Results are presented in Table 25. Given the similarity of the results of HC1 and clustered standard errors, but the limited clusters ($n = 11$), Table 25 reports HC1 standard errors only.

While the margin treated estimate increases, the turnout estimate shrinks and becomes insignificant. The interaction in the turnout estimate is significant and large, indicating that the effect of mail-in balloting appears to have been concentrated among older populations. In this sample, the average proportion of the population that is above 65 is 15.3%. The effect of treatment, then, on turnout rate is $-3.457 + 15.3 \cdot 0.44 = 3.275$. For the average county, moving to universal mail-in balloting resulted in a 3.275 percentage point increase in turnout, primarily driven by older voters. In general, treated counties saw 0.44 percentage point higher turnout per unit increase in the population over 65. This helps explain why

Table 25: Universal Mail-in Balloting Interacted with Age

	Dependent variable:	
	Republican Margin	Turnout
	(1)	(2)
Treated	0.840**	−3.457
	(0.363)	(3.126)
% Pop over 65	−0.019	−0.450***
	(0.020)	(0.167)
Over 65-Treatment Interaction	−0.029	0.440**
	(0.024)	(0.217)
Observations	181	181
R^2	0.999	0.793
Adjusted R^2	0.998	0.748
Note:	*p<0.1; **p<0.05; ***p<0.01	

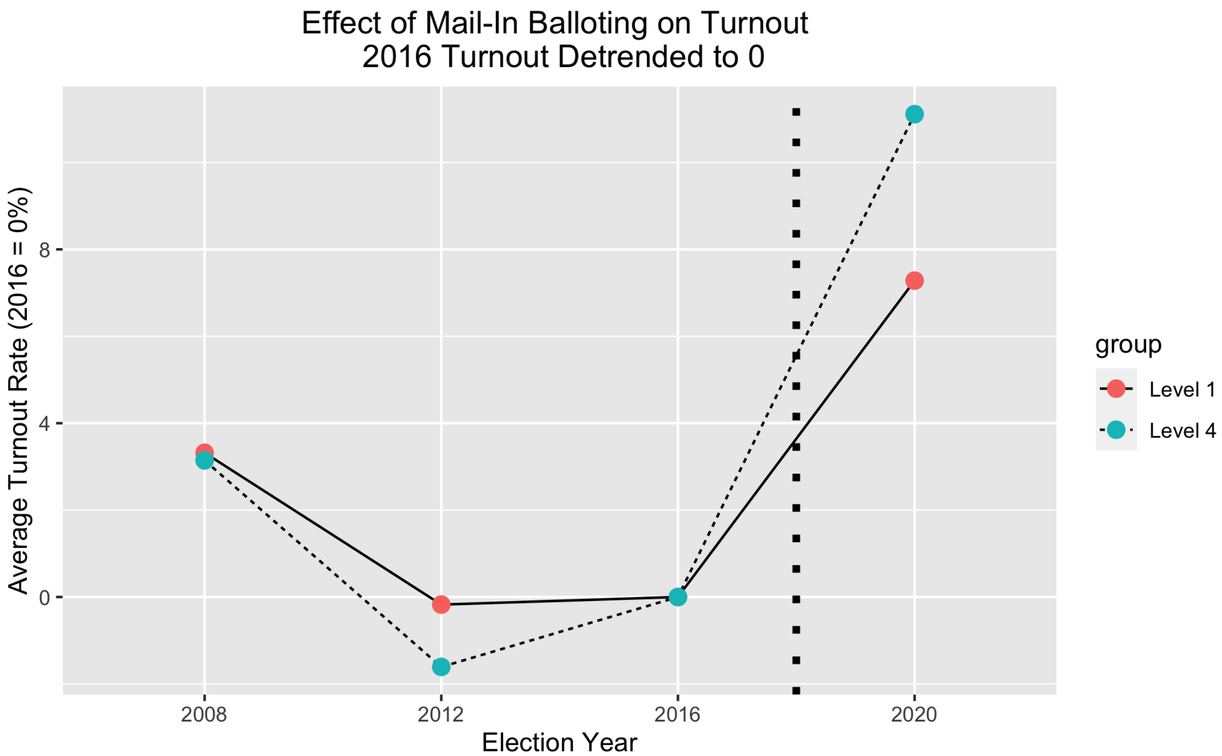
Estimates of moving to universal mail-in balloting, 65+ proportion of population, and interaction on both outcomes (DID approach). Both specifications use full set of controls. Standard errors reported using the HC1 estimator.

mail-in balloting expansion helped Republicans in this cycle, though the margin interaction regression results are a bit uncertain.

To assess the validity of the DID design, Figure 4 visually validates the parallel trends assumption. Data for both groups was normalized to be 0 in the immediately prior time period (2016). Prior to 2020, average county-level turnout rates appear to be moving together, but treated counties saw a larger jump in turnout rates in 2020 compared to the control group. Figure 6 (see Appendix) assesses the parallel trends assumption using all counties (Levels 1 through 4). The assumption appears to hold for all groups.

Motivated by the parallel trends holding for all groups, DID estimates for Levels 2 and 3 relative to baseline of Level 1 is presented in Table 31 (see Appendix). Directionally, most of the point estimates are similar to the ones for Level 4, though it is notable that the Level 3 turnout estimate is negative (and insignificant). Only one specification yields a significant estimate (Margin, Level 3). It appears that mail-in balloting is most effective

Figure 4: Parallel Trends of Level 1 and Level 4



when universalized, which reduces friction and increases ease of access. Note that the main difference between Levels 2 & 3 and Level 4 is that Level 3 requires voters to *apply* for a mail-in ballot and Level 2 requires voters to apply for the application itself. Automatically mailing ballots thus sees the most significant increase in voting. This result is supported by simple applications of nudge theory, a well-documented phenomenon in the literature. It also relates to the cost of voting. Automatically mailing ballots reduces costs to zero, while moving to other levels still preserves some barriers to voting. Applying for an application for a mail-in ballot (Level 2) may be viewed by voters as just as difficult as going to the polls.

7.2 Mail-in Balloting Effect on Protests

Two further specifications are presented in this section to assess the effect of mail-in balloting on protest effects. First, I assess whether controlling for mail-in ballot status changes the results of the prior section (Table 26). Second, Tables 27 & 28 present results by subsetting the data by mail-in level. I include the full set of controls from specification 4 in Tables 7 & 8.

Table 26: Mail-in Control Results

	Dependent variable:	
	Republican Margin	Turnout
	(1)	(2)
Protest Per Capita	0.030**	0.166**
	(0.012)	(0.072)
Mail-in Level	0.266***	0.914***
	(0.042)	(0.314)
Observations	749	749
R^2	0.997	0.669
Adjusted R^2	0.997	0.654
Note:	*p<0.1; **p<0.05; ***p<0.01	

Estimates of protest size and mail-in level on both outcomes (IV approach). Both specifications use full set of controls. Standard errors reported using the HC1 estimator.

From Table 26, we see that controlling for mail-in does little to change the effect sizes from the prior section, though mail-in levels themselves are highly significant predictors of both margin and turnout. Additionally, the effect sizes match with those in Table 24. In particular, moving from Level 1 to Level 4 increases Republican margin by ≈ 0.8 percentage points and turnout by 2.7 percentage points, ceteris paribus (cf. 0.45 and 2.5 percentage points in Table 24). These results provide a robustness check on the difference-in-difference estimates from before.

Tables 27 & 28 present results based on mail-in category. Republican margin increases the most in high-difficulty voting areas (Level 1), though turnout appears to be unaffected across all categories. Party differences in fear of COVID-19 may help explain this difference. In states with primarily in-person voting, Republicans may have been motivated to turn out by protests, but Democrats stayed home fearing infection.

In sum, these results indicate that mail-in balloting had a far greater effect on the 2020

Table 27: Mail-in Subset Margin Results

	Dependent variable:			
	Republican Margin by Mail-in Level			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.092***	0.034**	−0.016	0.047*
	(0.018)	(0.016)	(0.014)	(0.024)
2016 Margin	−0.987***	−0.989***	−0.958***	−1.007***
	(0.007)	(0.005)	(0.008)	(0.019)
Partisan Lean	1.968***	1.974***	1.912***	1.995***
	(0.016)	(0.010)	(0.017)	(0.040)
Mail-in Level	Level 1	Level 2	Level 3	Level 4
Observations	113	264	243	129
R^2	1.000	0.999	0.998	0.996
Adjusted R^2	0.999	0.999	0.998	0.995
Note:				*p<0.1; **p<0.05; ***p<0.01

Estimate of protest size on 2020 Presidential Republican margin (IV approach). Specifications subset to mail-in level (Level 1 to Level 4). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

Table 28: Mail-in Subset Turnout Results

	Dependent variable:			
	Turnout by Mail-in Level			
	(1)	(2)	(3)	(4)
Protest Per Capita	−0.020	0.081	0.044	−0.144
	(0.159)	(0.145)	(0.088)	(0.111)
2016 Turnout	−0.238	−0.431***	−0.079	−0.128*
	(0.098)	(0.147)	(0.073)	(0.070)
Partisan Lean	−0.597***	−0.621**	−0.325***	0.154
	(0.198)	(0.262)	(0.119)	(0.175)
Observations	113	264	243	129
R^2	0.878	0.806	0.702	0.790
Adjusted R^2	0.829	0.780	0.654	0.720
Note:				*p<0.1; **p<0.05; ***p<0.01

Estimate of protest size on 2020 General Election turnout (IV approach). Specifications subset to mail-in level (Level 1 to Level 4). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

election outcomes than did protests, though both changes helped Republicans and increased turnout overall.

8 Discussion

The results in Section 6 yield small effect sizes, with Republicans reaping the benefits of larger protests. Interestingly, the effect appears to come from higher Republican margins in Democratic leaning areas. Few interaction regressions yield significant results, underscoring the consistency of the trend. With Madestam et al.'s results in mind in which Tea Party protests significantly impacted 2010 midterm elections, the small effect sizes here are quite surprising. BLM protests barely tipped the scales, and when they did, the opposition party benefited. There are several potential explanations for these results.

Given the nature of the pandemic, it is possible that COVID-19 was the top issue in voters' minds, and as a result protests had a small effect size. However, results presented in Table 19 rebuke this argument empirically.

The nationalization of coverage is another hypothesis for low effect sizes. The presented results attempt to pin down the *local* effect of protests, but this is difficult in a nationalized political environment. Said another way, there is no marginal local impact of protesting when social media and news outlets are able to broadcast events in one corner of the country to all other locations instantly. This is exacerbated by the domination of national news outlets relative to local news stations and papers. If this is true, the effect sizes may be significant underestimates, as protests changed the national political environment itself, but had little local impact on top of that.

Third, and related to the above, there may be a polarization explanation. With rising social media and news consumption, voters have already made up their minds. Rather than swaying behavior, protests are simply reinforcing beliefs and increasing motivation to turnout. Some 2020 polls suggested that the group of undecided voters was significantly smaller than in 2016. This hypothesis is supported by higher estimates for turnout relative to margin in most specifications. It may also explain backlash against protests in Democratic areas that increased Republican margin. That is, individuals were motivated to vote for their party when they saw protests, but their vote was not changed by the goals of protests.

Finally, the data sets used may not have enough power. By aggregating at the county-level, there is too little power to detect the true effect size. In general, urban areas saw protests, but voting data is tabulated at the county-level. Perhaps protests are only locally effective at a very small radius. It seems likely that city-level vote counts would show slightly higher effect sizes, though it is unclear how much of a factor this would be given that most large cities contain the majority of the population of their home county.

Most likely, there is a combination of factors at play here. Given a priori knowledge of outcomes in Republican strongholds (such as Georgia) and the overall national vote margin, the presented results appear to be contradictory to our experience. However, Democrats did indeed lose several seats in the House, and several state-level margins were thin.

With respect to voting costs, expansion of mail-in balloting had significant impacts on election outcomes. The effect is concentrated among older voters, who voted far more in counties with universal mail-in balloting. Where mail-in balloting was expanded, it was more influential in the 2020 elections than protests were. Still, only six states (68 counties) actually expanded access to Level 4.

To analyze the relative effect of protests and mail-in balloting on the overall election, I present a decomposition of the effects on turnout. Overall, voting-age population turnout increased 7.5 percentage points nationally in 2020 relative to 2016. The population-weighted mean protest size was 1.96 with an estimated $\hat{\beta}$ of 0.17. Thus, approximately 4.4% of the change in turnout can be attributed to protests. If I instead use the 95% CI upper bound of $\hat{\beta}$ (0.32), 8.4% of the change can be explained by protests.

With respect to mail-in balloting, a shift to universal mail-in balloting only affected 12.8% of the population. The estimated DID effect was 2.52 percentage points. Estimates of moving to Level 2 or 3 were insignificant. Historically universal states already had mail-in balloting. These areas are ignored for the policy calculation. Multiplying together and dividing by 7.5 (the national turnout change), approximately 4.3% of the change can be explained by the expansion in mail-in balloting. The 95% CI upper bound for the DID estimate is 4.97%, which would imply that 8.5% of the overall change can be explained by mail-in balloting.

Overall, this paper's results only explain 8.7% (upper bound of 16.9%) of the overall national change in turnout. 91% (83% lower bound) remains unexplained, though could be related to national effects of protests—which are unable to be estimated—or specific factors with this year's elections.

9 Conclusion

In conclusion, why do people vote? This paper shows that voting motivation is more sensitive to voting costs than to social motivation, though both have positive effects on turnout.

Protests appear to motivate both sides to turnout at slightly higher rates, though Republicans benefit from the higher turnout. These effects are primarily driven by electoral backlash in high protest-size areas, leading to slightly higher Republican margins in mostly Democratic areas.

Future research should aim to identify city-specific effects by aggregating voting data at the precinct level. However, aiming to pin down a local effect for a national phenomenon may be a fruitless exercise.

On the other hand, mail-in balloting access clearly matters. Though it is unclear whether expanded access to mail-in balloting will remain in future election cycles, it had a remarkable effect on both turnout and margin in 2020. Given that Republicans appear to have benefited from expanded access, perhaps they should embrace the change as opposed to fighting it based on a potential increase in voter fraud.

Finally, I conclude by noting that—regardless of electoral outcomes—2020 protests have had a significant social and cultural impact which is bound to reverberate through politics. More people are talking about race than before, an achievement in and of itself. Though the short-term effects appear to be limited, a longer-term realignment of race discussions in the country could lead to longstanding political changes over the next several years. Perhaps by 2024, BLM protests will have had the last word.

10 Appendix

To prove that interacted instruments are also valid instruments, consider the following regression:

$$Y_i = \beta_0 + \beta_1 \cdot X_i + \beta_2 W_i + \epsilon_i$$

where X is endogenous and W is a control. Suppose an instrument Z satisfies the exogeneity condition for a valid instrument, given the control W. That is, $E[\epsilon|W, Z] = E[\epsilon|W]$. Then, we can extend to interactions between W and Z:

$$E[u|W, Z \cdot W] = E[u|W] \qquad \text{and} \qquad E[u|W, Z, Z \cdot W] = E[u|W]$$

where both equalities hold, as the interaction term is simply cZ because conditioning on W, $W \cdot Z$ is a scalar.

Tables 29 & 30 show the effects of protesting on margin/turnout, weighting based on protest size instead of county population. They are counterparts to Tables 7 and 8 from Section 6.3. Notice that the effect sizes are almost identical for margin and a bit smaller for turnout, though still precisely estimated.

Figure 5 presents the 2016 margin and turnout scatter plots, which were omitted from Section 6.6 (Falsification Tests). These are the reduced form graphs, showing weak linear relationships between residualized rainfall and residualized outcomes, validating the research design.

Figure 6 presents visual evidence supporting the parallel trends assumption for all levels of mail in balloting prior to 2020. All levels saw a jump in turnout rates, though the states with universal mail-in balloting appear to have seen the most growth, regardless of whether or not this was a new program. 2016 Turnout is normalized to 0.

Motivated by the validity of the parallel trends assumption for all levels, I present DID estimates for Level 2 and 3 in Table 31. The results are discussed in Section 7.1

Table 29: Protest Size Weighted Instrumental Variables Estimates of the Impact on 2020 Margin

	Dependent variable:			
	Republican Margin			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.032***	0.031**	0.029**	0.032**
	(0.011)	(0.014)	(0.013)	(0.014)
2016 Margin	−0.987***	−0.989***	−0.989***	−0.990***
	(0.006)	(0.005)	(0.005)	(0.005)
Partisan Lean	1.956***	1.959***	1.960***	1.962***
	(0.012)	(0.010)	(0.010)	(0.010)
Observations	749	749	749	749
R^2	0.997	0.998	0.998	0.998
Adjusted R^2	0.997	0.997	0.998	0.998
Note:	*p<0.1; **p<0.05; ***p<0.01			

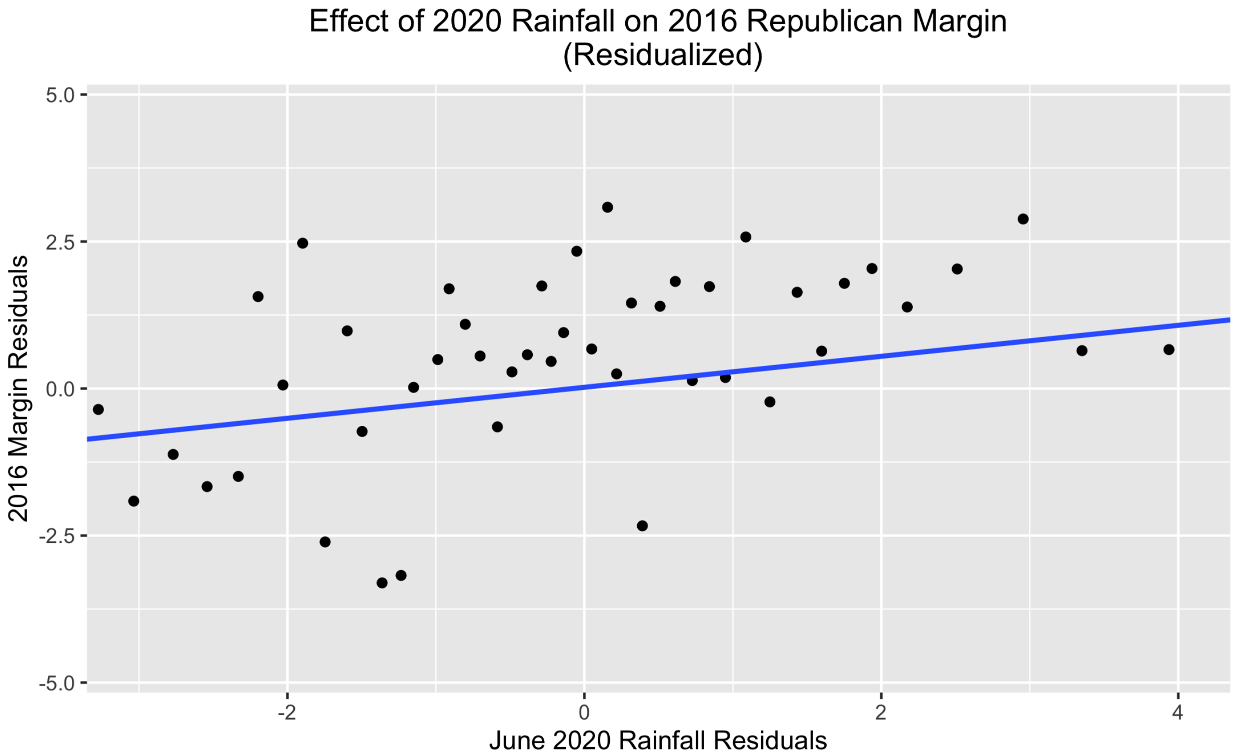
Estimate of protest size on 2020 Presidential Republican margin (IV approach), weighting by protest size. Specifications use increasing set of controls. Standard errors reported using the HC1 estimator.

Table 30: Protest Size Weighted Instrumental Variables Estimates of the Impact on 2020 Turnout

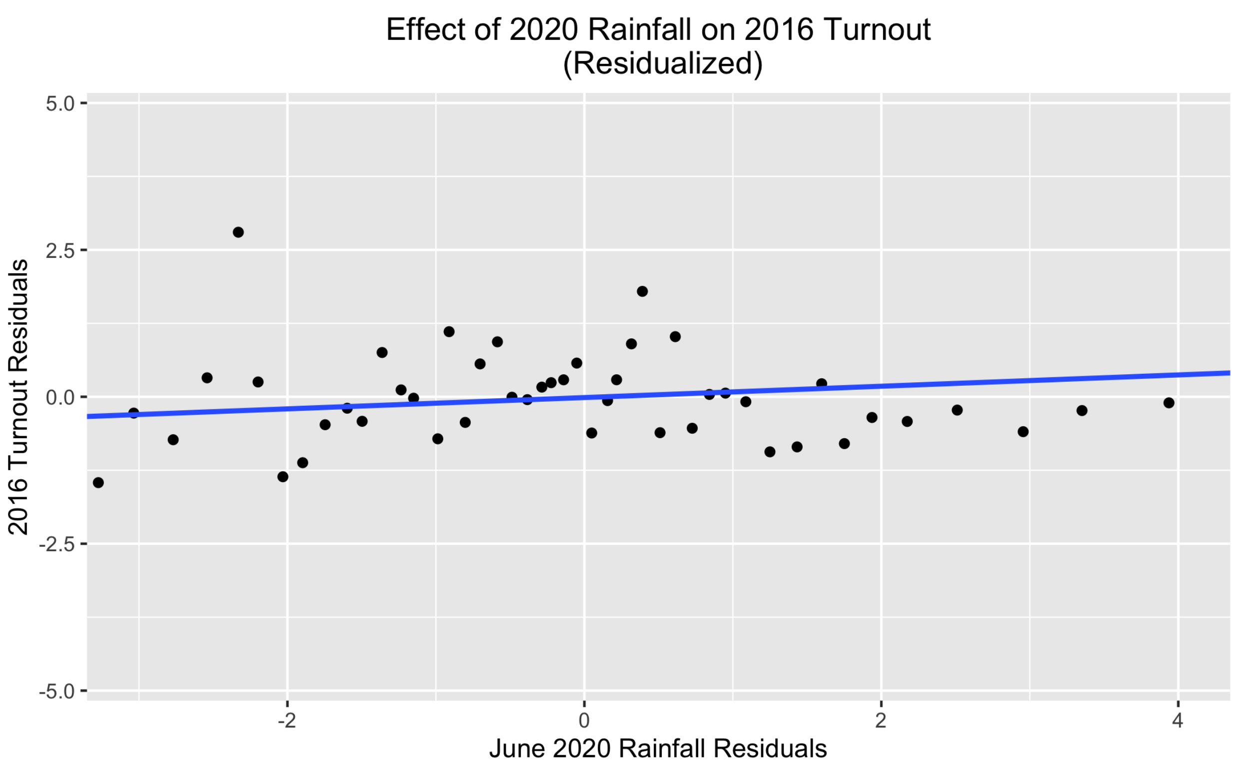
	Dependent variable:			
	Turnout			
	(1)	(2)	(3)	(4)
Protest Per Capita	0.142**	0.075	0.035	0.112*
	(0.061)	(0.052)	(0.053)	(0.063)
2016 Turnout	−0.205***	−0.150***	−0.166***	−0.231***
	(0.036)	(0.043)	(0.047)	(0.050)
Partisan Lean	−0.858***	−0.962***	−0.916***	−0.891***
	(0.117)	(0.109)	(0.113)	(0.114)
Observations	749	749	749	749
R^2	0.856	0.878	0.885	0.897
Adjusted R^2	0.854	0.875	0.881	0.893
Note:	*p<0.1; **p<0.05; ***p<0.01			

Estimate of protest size on 2020 General Election turnout (IV approach), weighting by protest size. Specifications use increasing set of controls. Standard errors reported using the HC1 estimator.

Figure 5: 2016 Residualized Outcome Scatterplots vs. 2020 Protests



(a) Binned 2016 Margin Scatterplot



(b) Binned 2016 Turnout Scatterplot

Figure 6: Parallel Trends of All Levels

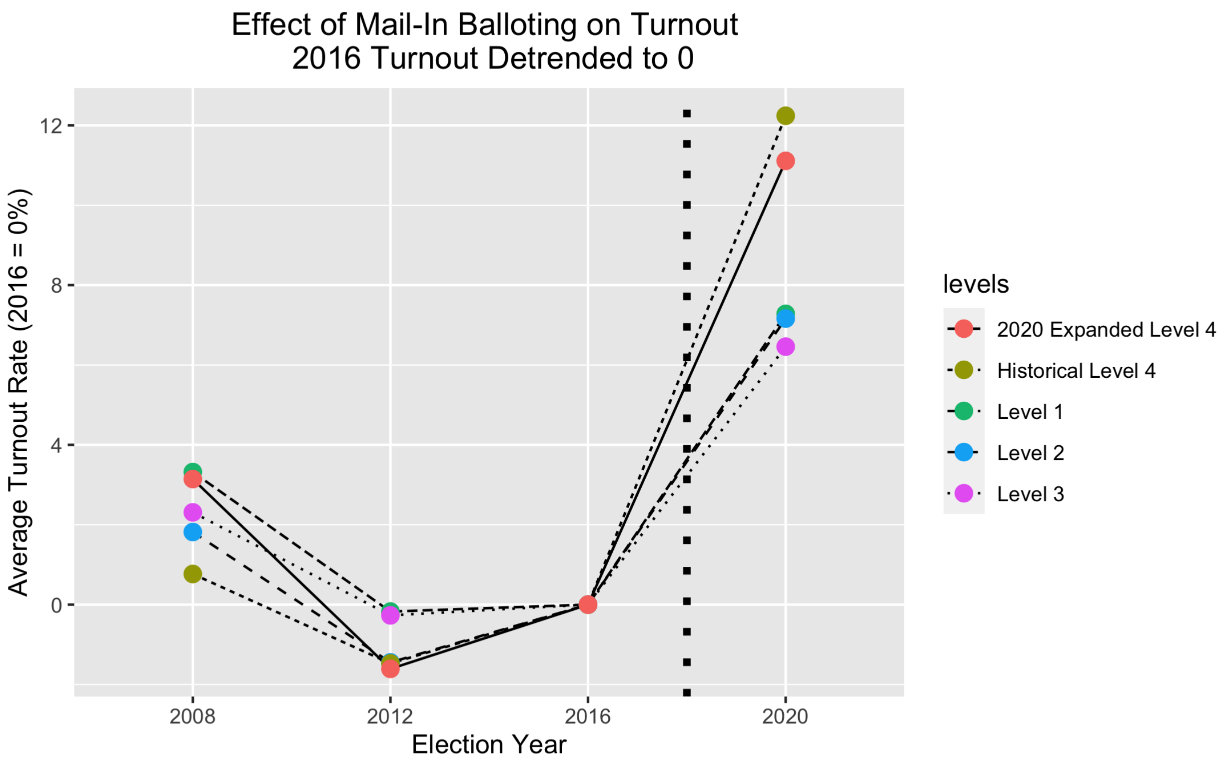


Table 31: Difference-in-Difference Estimate of Levels 2 and 3 of Mail-in Balloting

	Dependent variable:			
	Margin (Level 2)	Turnout (Level 2)	Margin (Level 3)	Turnout (Level 3)
	(1)	(2)	(3)	(4)
Treated	0.061	0.176	0.348^{***}	-0.710
	(0.070)	(0.711)	(0.083)	(0.705)
2016 Margin	-0.989^{***}	0.349^{***}	-0.965^{***}	0.304^{***}
	(0.005)	(0.094)	(0.006)	(0.057)
2016 Turnout	0.005	-0.360^{***}	-0.022^{***}	-0.137^{***}
	(0.004)	(0.088)	(0.007)	(0.046)
Observations	377	377	318	318
R^2	0.998	0.786	0.997	0.754
Adjusted R^2	0.998	0.767	0.997	0.727

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Estimate of moving to Level 2 or 3 of mail-in balloting access between 2016 and 2020 on both outcomes (DID approach). All specifications use full set of controls. Standard errors reported using the HC1 estimator.

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