Weather to Protest: The Effect of Black Lives Matter Protests on the 2020 Presidential Election

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Abstract

Do mass mobilizations bring about social change? This paper explores this question by studying the impact of the Black Lives Matter protests that erupted after George Floyd's death on the 2020 presidential election. We show, through an IV and a Diff-in-Diff approach, that variation in protesting activity caused increased support for the Democratic party in counties with heightened protest activity. Our analysis examines the effects of these protests not only on voting but also on public opinion. By distinguishing between the short-term backlash and the long-term effect on racial attitudes and voting behavior, we provide causal evidence of the protests' overall effect, as well as insights into the timeline and mechanisms through which this influence materialized. We show that the observed effects cannot be fully attributed to changes in turnout, and that protests also engender shifts in people's attitudes about racial disparities.

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1 Introduction

In the United States, African Americans experience disproportionately many interactions with the police, the criminal justice system and the carceral state (Crabtree and Yadon, 2022). It is against this institutional backdrop that the Black Lives Matter (BLM) movement emerged, aiming to confront and overhaul patterns of racial inequality manifested in incarceration and police brutality (Williamson et al., 2018). The BLM movement surged to greater national prominence in May 2020. Following the death of George Perry Floyd Jr. at the hands of police officers on 25 May 2020, a series of BLM protests erupted in Minneapolis and quickly spread nationwide (Reny and Newman, 2021; Morris and Shoub, 2023). In the ensuing weeks, an estimated 15 to 26 million people participated in what has been deemed the largest series of protests in US history. These demonstrations demanded, among other things, reforms of the criminal justice system and called for an end to police brutality against African Americans (Dave et al., 2020; Eichstaedt et al., 2021).

Previous protest research shows that collective action movements can induce social change by pushing event-relevant issues to the top of the public agenda (Birkland, 1998; Wasow, 2020) as well as through liberal shifts in public opinion on racial issues (Lee, 2002). Yet, the violent nature of some protests may induce a backlash, leading some voters towards more conservative politicians that advocate for law and order (Wasow, 2020). Regardless of the direction of the effect, BLM protests undoubtedly brought the issue of racial injustice to the forefront during the 2020 presidential election (Reny and Newman, 2021; Boehmke et al., 2023). Approximately three-quarters of voters stated that the protests significantly influenced their voting decisions, with one-fifth even considering them to be the single most important issue (AP, 2020*b*).

The current paper explores the effect of Black Lives Matter protests on the 2020 U.S. presidential election. Specifically, we investigate how differences in protest intensity across counties influenced voter behavior, focusing on both party preferences and election turnout. Our analysis examines both the immediate and longer-run impact of these protests on public opinion and political preferences. By distinguishing between short-term and long-term effects, we provide evidence of both the protests' overall effect on voting, and the timeline and mechanisms through which this influence materialized.

To estimate the causal effect of protests, we use two different methodologies: an instrumental variable approach and a difference-in-difference approach. Our instrumental variable approach follows a growing literature that uses weather shocks as an exogenous source of variation in protests (Collins et al., 2004; Collins and Margo, 2007; Madestam et al., 2013; Wasow, 2020; Casanueva, 2021; Meier et al., 2019). The general idea behind the instrument is that rainfall shocks discourage prospective protesters from taking to the streets while being unrelated to other election-relevant factors. We supplement this analysis with a difference-in-differences methodology that compares the change in election outcomes in protest counties to the change in election outcomes in non-protest counties.

Our results demonstrate that BLM protests caused a marked shift in local support for the Democratic party. An analysis of mechanisms shows that this effect cannot be fully attributed to increased voter mobilization, and that protests also shifted people's attitudes about racial disparities. This result suggests that BLM protests caused a progressive shift among Independent or Republican-leaning voters. Heterogeneity analyses suggest that the effect of protests is larger in counties with relatively small, white, and low-educated populations.

When we examine the evolution of the effect over time, our results reveal an interesting reversal. Initially, BLM protests caused a counter-reaction, increasing support for the Republican party among those living in areas with more protesting activity. This immediate backlash might be attributed to the media visibility of the more violent aspects of the protests. However, as these violent aspects receded from the public discourse over time, we observe that those living in protest counties increasingly shift towards the Democrat party. This evolution highlights the importance of taking into account the time dimension of electoral events when evaluating the effects of protests.

The main contribution of our paper is to evaluate the political impact of one of the largest collective action movements ever observed. Prior research provides somewhat mixed answers to the question whether protests advance or hinder protesters' goals. On the one hand, riots in Los Angeles and Tea Party protests, as well as protests advocating civil rights protests, immigration, and the environment, generated support for the protesters' goals (Carey Jr et al., 2014; Enos et al., 2019; Madestam et al., 2013; Branton et al., 2015; Mazumder, 2018; Hungerman and Moorthy, 2023). On the other hand, violent race riots in the 1960s negatively affected property values and labor market outcomes for African Americans, and raised support for law-and-order politicians (Collins et al., 2004; Collins and Margo, 2007; Wasow, 2020). Similar results were found in Egypt by El-Mallakh (2020), where anti-government protests elevated support for the incumbent regime. This ongoing debate is indicative of the complexity and methodological challenges related to the phenomena in question. Our findings indicate that large and consequential protest movements can substantially influence key election results, thus suggesting the ability of underrepresented communities to improve outcomes through collective action movements.

This article proceeds in four sections. The following section provides the motivation and background for this study by outlining existing theories and evidence for protest mobilization and its link with electoral outcomes. The article then presents the empirical strategy, followed by the main findings and a set of robustness checks. The last section provides concluding remarks on our findings.

2 Background

The phrase "Black Lives Matter" first emerged in 2013 as a twitter hashtag that called attention to the acquittal of George Zimmerman, a mixed-race, White/Hispanic man who shot an unarmed black teenager. Since then, BLM has evolved into a comprehensive protest movement aiming to address persistent racial disparities in economic, social and political outcomes. Although the decentralized nature of BLM makes it challenging to pinpoint the movement's exact goals, the desire to reform police departments and to increase police accountability holds central importance (Williamson et al., 2018). Politically, BLM protests are often implicitly associated with the Democratic party. Democrats traditionally champion minority causes, and during their 2020 convention, they openly embraced the (non-violent) imagery and themes of the BLM movement (Linskey, 2020). Furthermore, many participants in the George Floyd protests expressed distinct anti-Trump sentiments. The association between the BLM movement and the Democratic party forms the basis of our rationale for examining the effect of racial injustice protests on the outcome of the 2020 presidential election.

The unprecedented scale of the 2020 BLM protests raises the question of why individuals participate in protests in the first place. To comprehend protest participation, it is important to consider the associated costs and benefits. The costs are contingent on factors such as the location and the timing of the protest, as well as individuals' commuting times and job flexibility. Central to our paper is the premise that precipitation increases the discomfort of protesting thereby making it relatively costlier.

The primary benefit of protesting is traditionally assumed to be their effect on engendering social change (Tullock, 1971). More nuanced work suggests that participation in public action yields additional psychological rewards (Granovetter, 1978; Passarelli and Tabellini, 2017). One such reward is that individuals who are exasperated about perceived injustices may find it rewarding to "fight the good fight", regardless of the outcome. Other psychological benefits of protesting depend on the expected size of the protest (Granovetter, 1978; Passarelli and Tabellini, 2017; Hollyer et al., 2015; Little et al., 2015). For instance, publicly expressing anger or dismay becomes more appealing when many others share those emotions. Likewise, because larger crowds are more likely to incite societal change, individuals may perceive participation in large protests as more meaningful. Collectively, these factors give rise to a strategic complementarity in protest participation, making collective action a contagious phenomenon whereby protests beget further protests (Shadmehr and Bernhardt, 2011; Casper and Tyson, 2014; Steinert-Threlkeld, 2017).

The advent of social media has further enhanced the value of participating in protests. Platforms such as Twitter and Facebook facilitate the exchange of information, making it easier for large groups to coordinate collective actions, thereby lowering the expected participation costs (Steinert-Threlkeld, 2017; Jost et al., 2018). Moreover, social networks increase the visibility of individuals' involvement in protests within their own network, offering the opportunity to signal their opinions as well as their virtue.

Given the aforementioned factors, the eruption of large-scale BLM protests is not entirely surprising. George Floyd's passing highlighted and intensified perceptions of systemic racial injustice towards African Americans (Williamson et al., 2018), and his death coincides with widespread dissatisfaction with the incumbent president. Once protests reached a critical mass, network effects transformed the initial demonstrations into an unprecedented social movement.

To understand how large-scale collective action movements such as BLM can effect societal change, prior research emphasizes the importance of information channels, network effects, and agenda seeding. Information-based theories suggests that political activism reveals privately-held dissatisfaction to the general public (Lohmann, 1994*b*). Specifically, protest activity serves as an informative signal about the consequences of previous policies. Hence, protests raise awareness of social problems and change the perceived importance of these issues among the population, which may subsequently shape individuals' voting decisions.

Network-based theories propose that networks can amplify information effects (Bursztyn et al., 2021). While anonymous protesters can be dismissed as extremists or radicals, environmental cues from one's own social context are more difficult to ignore (Schmitt-Beck and Mackenrodt, 2010). Furthermore, people often consider their network when deciding whether to vote and whom to vote for (Quattrone and Tversky, 1988; Gerber et al., 2008; Cantoni et al., 2019). Insofar as protest participation signals an intention to vote for a particular party or politician, engaging in a protest can thus influence the electoral choices of non-participating connections. Due to the contagious nature of protest participation, network effects can create virtuous (or vicious) cycles of increasing protest numbers and increasing support for their purported cause.

Agenda seeding theory posits that protesters introduce new issues into the public's consciousness by organizing and attending events that enhance the valence of media coverage (Wasow, 2020). The nature of protesting activities plays a crucial role in agenda seeding, because it determines whether the media frame a movement positively or negatively. While peaceful protests generally generate sympathy for minority concerns and protester demands, more forceful actions may provoke the opposite reaction. Because the 2020 BLM protests were characterized by stark differences in media portrayals between liberal and conservative outlets, agenda-seeding helps explain the highly polarized perception of BLM protests among Americans on opposite sides of the political spectrum (SignalAI, 2021; Bolsover, 2020).

3 Data and Empirical Strategy

We compile our data set from multiple independent sources. Information on Black Lives Matter protests comes from the Crowd Counting Consortium (CCC), an organization that assembles publicly available dissent and collective action statistics. While the CCC represents one of the most comprehensive efforts to track protest events across the U.S., we acknowledge it is a crowdsourced dataset and may suffer from some degree of measurement error. There is however some work on evaluating the quality of the CCC in comparison to other social movement datasets like ACLED, showing that both are almost identical when measuring the number of events per day with some overestimation of participants in CCC (Dorff et al., 2023). Moreover, as will become clear, we use an instrumental variable approach, which is a commonly used method to reduce measurement error.

We use data from the National Oceanic and Atmospheric Administration (NOAA) to calculate county-level daily precipitation levels during the main protest window. NOAA reports daily precipitation levels for each weather station in the United States. For each county, we select the weather station that is closest to the center of the county. We calculate the total amount of rainfall during the protest window by taking the sum of the daily precipitation levels between 26 May and 7 June 2020.[1] We additionally obtain the daily likelihood of rain during this window, which is measured by NOAA as the probability of at least 0.01 inch of precipitation at the weather station on a given day of the year. We calculate the average precipitation likelihood for the protest window by taking the average of the daily rainfall probabilities over this period. We use this variable to control for general climatic conditions that may correlate with voting-relevant characteristics such as the average age, income and ethnic composition of a county. In other words, we only consider rainfall *shocks*, as defined by rainfall conditional on the general probability of rain during that period.

County-by-county voting data come from the MIT Election Data and Science Lab for the 2012-2016 elections and from the Associated Press for the 2020 election (Data and Lab, 2017; AP, 2020*a*). We obtain county-level racial attitude data from the Cooperative Election Study (CES; Schaffner et al., 2021*a*), and gather other county-level characteristics from the US Census. Covid-19 statistics are collected from The New York Times Coronavirus Database (The New York Times, 2021). The first section of the Online Appendix gives a detailed description of the data set. We focus our analysis on the two-week period from 26 May to 7 June 2020, which directly follows George Floyd's death on May 25th. Subsection "Sample Window Selection" in the Appendix examines the robustness of our results to different sample window choices.

We use additional data on self-reported partisanship from the Gallup– COVID-19 Survey (Gallup, 2020). This a nationally representative web survey of U.S. adults that ran daily between March and August 2020 (N=85,106). Members were randomly selected using random-digit-dial phone interviews that cover landline and cellphones and address-based sampling methods. The first section of the Online Appendix gives a detailed description of the data set. We use this data set to probe how the impact of protests evolves in the months following the protest window.

Table 1 provides summary statistics. The data cover 3,053 of all 3,139 US counties. *Protest county* is an indicator variable that takes the value of 1 if at least one protest occurred between 26 May and 7 June 2020. In our sample, 40 percent of counties are protest counties. *Days of protests* is the number of days with at least one protest during that window, and *Attendees/Population* is the total number of attendees at BLM protests as a fraction of the county's population. On average, counties experienced 1.1 days of

[1] Ideally, we would only consider rainfall during the day, as protests almost exclusively take place during daytime. However, NOAA only reports daily precipitation levels.

Table 1: Summary Statistics

	Mean	Min	Max
Protest county	0.40	0	1
Days of protests	1.05	0	13
Days of protests, protest counties	2.65	1	13
Attendees/Population (%)	0.20	0	10.10
Attendees/Population (%), protest counties	0.50	0	10.10
Δ Democratic vote share	0.02	−0.27	0.19
Δ Democratic vote share, protest counties	0.03	−0.22	0.19
Rainfall	0.39	0	4.83
Rainfall, protest counties	0.40	0	4.83

Notes: The table displays county-level summary statistics. *Protest county* is an indicator variable that takes the value of 1 if at least one protest occurred between 26 May and 7 June 2020. *Days of protests* is the number of days with at least one protest during that window. *Days of protests, protest counties* is the number of protest days in counties with at least one protest. *Attendees/Population* is the total number of attendees at BLM protests as a fraction of the county's population. *Attendees/Population, protest counties* is the corresponding fraction in counties with at least one protest. Δ *Democratic vote share* is the change in the fraction of votes going to the Democratic party between the 2016 and 2020 presidential elections. Δ *Democratic vote share protest counties* is the change in the Democratic vote share in counties with at least one protest. *Rainfall* is the total amount of rain (in millimeters) during the protest window. *Rainfall, protest counties* is the total amount of rainfall in counties with at least one protest.

protests attended by 0.20 percent of the population. In protests counties, these numbers are 2.7 days and 0.50 percent. Δ *Democratic vote share* is the change in the fraction of votes going to the Democratic party between the 2016 and 2020 presidential elections. The average fraction of votes going to the Democratic candidate increased by 2 percentage points between 2016 and 2020. In protest counties, this increase was 3 percentage points on average. *Rainfall* is the total amount of rain (in centimeters) during the protest window. The average total precipitation was 0.39 centimeters across all counties, and 0.40 centimeters in counties with at least one protest.

3.1 Methodology

The goal of our analysis is to estimate the causal effect of BLM protests on the 2020 presidential election. The main empirical problem is that unobserved political sentiments likely influence both protesting activity and voting behavior. To circumvent this endogeneity problem, we use two methodologies: an instrumental variable approach and a difference-in-difference approach.

Our instrumental variable (IV) approach exploits the fact that people are less likely to protest when it rains. If rainfall during this period did not otherwise affect the presidential election outcome, we can use the resulting variation in protesting activity to estimate the causal effect on the 2020 presidential election. The plausibly exogenous nature of rainfall makes it a widely used instrumental variable across the social sciences. Paving the way for our study, Collins and Margo (2007) and Madestam et al. (2013) were among the first to apply this method to protesting activity.

The use of rainfall as an instrumental variable for protesting activity is not without controversy. A recent study by Mellon (2021) highlights several scenarios in which rainfall may violate the exclusion restriction. The exclusion restriction is the assumption that rainfall during the protest window only affects voting outcomes through its effect on BLM protests. Previous research has shown that rainfall can affect factors such as violent crime and mood, which in turn may influence voting behavior, thus compromising the validity of the instrument (Jacob et al., 2007; Ranson, 2014; Baylis et al., 2018; Frijters et al., 2020). However, Mellon (2021) suggests that these issues can often be mitigated by using daily rainfall shocks (i.e., rainfall conditional on general weather patterns) rather than overall rainfall levels. Additionally, concerns about exclusion restriction violations may be lessened when the outcome variable of interest is measured significantly later than the protests themselves, as is the case in our study, because the influence of any confounding factors affected by rainfall is likely to dissipate over time. Nevertheless, we concede that our IV analysis might be imperfect. We therefore supplement the IV analysis with a difference-in-differences methodology (explained below) to mitigate some of the concerns and add methodological robustness to our results.

A second complication of using rainfall as an instrument is that both weather conditions and outcome variables are generally spatially correlated. Such dependencies are often ignored in applied work, but they can severely bias IV estimates (Plümper and Neumayer, 2010).[2] To take into account both spatial autocorrelation and the endogenous nature of protesting activity, we estimate a spatial two-stage least squares model that explicitly models the spatial dependencies between counties and instruments for protest activity using rainfall. Ignoring spatial autocorrelation does not change the direction for any of our results, but it produces implausibly large effect size estimates.

An important modeling decision for this type of models is the choice of spatial weighting matrix $\mathbf{W}$, which represents the degree of spatial correlation between observations. The most commonly used spatial weighting matrices are based on border overlap and geographic distance (Beck et al., 2006). Because rainfall does not stop at county borders, we opt for the latter and assume that the spatial autocorrelation between counties is inversely proportional to distance between them.[3] We estimate the model using the GMM-IV approach outlined in Drukker, Egger and Prucha (2013), which allows us to obtain consistent estimates of the causal effect of BLM protests on the presidential election in the presence of spatial autocorrelation.[4]

We estimate the following model:

[2] See Appendix A2 for a more detailed explanation of the problem and the solution.

[3] We exclude counties in Hawaii and Alaska to estimate the model.

[4] Subsection Alternative Spatial Structure examines the sensitivity of our results to different specifications of the spatial structure.

$$Y_i = \beta_0 + \lambda \sum_{j=1}^{N} W_{ij} Y_j + \beta_1 \widehat{Protests}_i + \alpha X_i + u_i \tag{1}$$

$$u_i = \rho \sum_{j=1}^{N} W_{ij} u_j + \varepsilon_i \tag{2}$$

Y_i is the outcome variable in county $i \in \{1, 2, \ldots, N\}$. Our main outcome of interest is the change in the Democratic vote share between 2016 and 2020. Using the change rather than the level eliminates unobserved time-invariant characteristics that may correlate with voting behavior. Additional outcome variables we consider are the change in turnout rate between 2016 and 2020, and attitudes towards discrimination and racial injustice. $\widehat{Protests}_i$ is the number of protesters between 26 May and 7 June 2020 as a fraction of the county's population.[5] We instrument for this variable using rainfall shocks during this period. W_{ij} specifies the spatial relationship between counties i and j such that $W_{ij} Y_j$ measures the relationship between vote shares in surrounding counties and county i. X_i contains a set of main protest controls that are included in every regression (both first and second stage) consisting of several variables. First, *average rain probability* is the average likelihood of precipitation during the protest window as calculated by NOAA. This variable controls for the general climatic conditions in a county. Second, *population size* is the number of people living in county i in 2019. This variable accounts for the fact that more protests happen in more populous areas. Third, *Covid cases and deaths* are the number of Covid-related cases and deaths in a county. This variable helps account for the fact that rainfall may have influenced the spread of Covid-19. Fourth, *Density* is the population density of county i, which controls for the fact that it might be easier to organize protests in more densely populated counties. Last, we control for *racial composition*, measured by the number of Whites, Blacks, Asians, and Hispanics as a fraction of the population in county i. Race is likely to be as an important driver of BLM protests. As additional demographic control variables, we consider a county's education level (fractions of people with high school, college and graduate degrees) and median age, and as additional economic control variables, we consider the median income and unemployment rate. The coefficients λ and ρ indicate the strength of spatial autocorrelation in the outcome variable and the error term respectively.

Our second methodology to estimate the causal effect of BLM protests on the 2020 election is a difference-in-differences approach that compares the change in vote shares in protest counties with the change in vote shares in counties without protests. The main identifying assumption is that the political sentiment in protests counties would have developed along the same path as it did in non-protest counties, had the protests not occurred. This is the so-called parallel trends assumption. It is important to note that this

[5] In robustness checks we consider 'number of protest days' as an alternative measure of protesting activity.

methodology does not require that protests are equally likely to occur in Democrat and Republican counties.

We estimate the following model:

$$Y_{it} = \beta_1 \times Protests_i \times PostGF_t + \alpha_i + \gamma_t + \varepsilon_{it} \tag{3}$$

where Y_{it} is the Democrat vote share in county i in year t. For each county, we consider three election years: 2012, 2016, and 2020. $Protests_i$ is the protest activity in county i between 26 May 26 and 7 June. We consider three measures: (i) a binary variable that takes the value of 1 if a protest occurred, (ii) a continuous variable for protest attendees as a fraction of the population, and (iii) the number of protest days during the protest window. $PostGF_t$ is a binary variable that takes the value of 1 for elections that take place after George Floyd's death (i.e., the 2020 election). α_i are county fixed effects that control for all time-invariant county characteristics such as culture, geography, general political orientation, total covid deaths/cases, etc. γ_t are year fixed effects that control for all common shocks such as major national and geopolitical events. β_1 gives the difference-in-differences estimate for the effect of BLM protests on the 2020 presidential election.[6] To estimate the difference-in-differences models, we apply the estimator developed by Gardner (2022)

In addition, we aim to distinguish between the immediate effect of BLM protests in the weeks following George Floyd's death, and the longer term impact that evolves in the ensuing months. To do so, we use individual-level data from the Gallup Covid Panel to estimate a dynamic difference-in-differences model following the same DID logic as before. We consider the period from 13 March 2020 (the start of the panel) until 31 July 2020 (Gallup Covid Panel changed their sampling strategy in August). We estimate the effect of protests for each week before and after George Floyd's death in protest counties, using individuals living in non-protest counties as a control group using the following model:

$$Y_{ict} = \sum_{t=-10}^{10} \tau^t Protests_c^t + \alpha_c + \gamma_t + \varepsilon_{ict} \tag{4}$$

where Y_{ict} is an indicator variable that takes the value of 1 if individual i residing in county c identifies as Democrat in week t, with t ranging from -10 (10 weeks before George Floyd's death) to 10 (10 weeks after). $Protests_{ct}^t$ are a set of indicator variables that take the value of 1 if county c is t weeks away from having at least one protest during the main protest window. For example, $Protests_{ct}^{-1}$ takes the value of 1 if person i filled the survey 1 week before George Floyd's death in protest county c. For all non-protests counties, these variables always take the value of 0. All other definitions are the same as before. The coefficients τ^{-10} to τ^{-1} measure the effect of protests before they actually

[6]Because we use county and year fixed effects, the DID estimator does not include separate dummies for $Protests_i$ and $PostGF_t$. These are absorbed by the fixed effects.

Table 2: Main results

	Model 1	Model 2	Model 3
Attendees/Population	0.035^{***}	0.024^{***}	0.025^{***}
	(0.007)	(0.007)	(0.004)
Rain prob.	-0.028^{**}	-0.025^{**}	-0.020^{**}
	(0.012)	(0.012)	(0.009)
λ	1.858^{***}	2.587^{***}	2.485^{***}
	(0.479)	(0.479)	(0.428)
ρ	5.679^{***}	5.178^{***}	5.443^{***}
	(1.184)	(1.184)	(0.745)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053

Notes: The table shows the effect of BLM protests between 26 May and 7 June 2020 on the change in the Democratic vote share between the 2016 and 2020 presidential elections. All effects are estimated using a GMM-IV estimator (Drukker, Egger and Prucha, 2013). *Attendees/Population* is the total number of people who attended the protests as a fraction of the population. This variable is measured in percentages to ease interpretation. *Rain prob.* is the average probability of rainfall. All estimations control for population size, density, racial composition, and cumulative Covid-19 case and death counts on the day prior to the election. λ and ρ indicate the strength of spatial autocorrelation in the outcome variable and the error term respectively. *Demographic controls* contain a county's racial composition and the median age, and *Economic controls* contain the unemployment rate and the median income. Standard errors are in parentheses.

occur, which will be used to examine the validity of the parallel trend assumption. τ^1 to τ^{10} give the causal effects of protests for weeks 1 to 10 after the protest window.

4 Main Results

Table 2 presents the main results of the instrumental variable analysis. Using only rainfall-induced protest variation, we find a positive effect of BLM protests on the change in the Democratic vote share between 2016 and 2020. Model 1 shows that a 0.1 percentage point increase in the fraction of the population that goes out to protest raises the Democratic vote share in that county by 0.35 percentage points. This effect is economically and statistically significant ($p < 0.001$). Models 2 and 3 show that the effect remains highly significant when we control for demographic and economic control variables (both $p < 0.001$).

To interpret the magnitude of the estimated effect, we can scale our estimates by the attendance of BLM protests relative to a county's population. In counties with at least one protest, Table 1 shows that the average attendance corresponds to 0.5 percent of the population. Our estimates thus translate into 1.2 to 1.8 percentage points boost of the Democratic vote share as a result of BLM protests in protest counties. The estimated magnitude is in line with related work. In the context of the 1960s civil rights protests, Wasow (2020) finds nonviolent protests caused a 1.6-2.5% increase in the Democratic vote share. Similarly, Madestam et al. (2013) show 2009 Tea Party protests in 2009 caused a 1.04 % increase in the share of the population voting for the Republican Party. It should be noted, however, that previous research did not account for spatial dependencies, and might thus overestimate the effect of protesting activity.

The spatial parameters λ and ρ are highly statistically significant, showing the presence of large spatial dependencies. These results demonstrate the importance of accounting for spatial autocorrelation. A large set of robustness checks, presented in more detail in Appendix A3 in the Online Appendix, shows that our results are robust to the sample window selection, alternative spatial structures, alternative protest measures, alternative weather instruments, ignoring spatial autocorrelation, adding state fixed effects, and weighing counties by population size.

Table 3 shows variation in the estimated effect of protests on voting behavior across counties based on racial composition, population size, and education levels. We find that the effect of protests is stronger in area with relatively small fractions of African Americans. Although this result might appear counterintuitive, it is important to note that close to 90% of African Americans already support the Democrat party, leaving little room to move opinions in a progressive direction (Gramlich, 2020). Moreover, some sources suggest that the majority of BLM protesters were White (Fisher, 2020). Our results further indicate that protests engender larger effects in smaller counties and counties with lower education levels. Similar to counties with few African Americans, counties with small and low-educated populations tend to vote Republican.

To interpret the finding that protests had larger effects in smaller counties, one must consider that smaller communities may be sensitive to local activism and events in ways that large, dense urban areas are not. Social ties in relatively small towns tend to be stronger (Wellman and Wortley, 1990), which makes fellow residents who engage in protests an even more salient and informative event (Lohmann, 1994*a*; Bursztyn et al., 2021).

More general, it is important to consider that our methodology identifies a local average treatment effect, meaning that we only identify the effect of protests in areas in which rainfall influences protest activity. This likely precludes extremely conservative places where people do not join BLM protests independent of the weather conditions. Moreover, even in places where rainfall does affect protests, it remains a possibility that BLM protests pushes those who would anyway vote Republican further to the right. Hence, we cannot dismiss the possibility that BLM protests also induce a backlash against the movement among more conservative voters. Nevertheless, our results are in line with more recent evidence showing that the protests prompted higher voting registration among Whites and in smaller states (Holbein and Hassell, 2023).

Table 3: Heterogeneous treatment effects

	Below median	Above median
Panel A: Fraction African Americans		
Attendees/Population	0.041^{***}	0.019^{***}
	(0.006)	(0.005)
Rain prob.	-0.044^{***}	0.0003
	(0.014)	(0.013)
λ	4.195^{***}	1.056^{***}
	(0.525)	(0.246)
ρ	3.190^{***}	5.258^{***}
	(0.587)	(1.038)
Main protest controls	Yes	Yes
Demographic controls	Yes	Yes
Economic controls	Yes	Yes
Observations	1,539	1,514
Panel B: Population size		
Attendees/Population	0.031^{***}	0.013^{***}
	(0.006)	(0.004)
Rain prob.	-0.028^{**}	-0.034^{***}
	(0.013)	(0.011)
λ	4.931^{***}	1.184^{***}
	(0.647)	(0.274)
ρ	3.061^{***}	5.778^{***}
	(0.495)	(0.942)
Main protest controls	Yes	Yes
Demographic controls	Yes	Yes
Economic controls	Yes	Yes
Observations	1,519	1,534
Panel C: Education		
Attendees/Population	0.036^{***}	0.015^{***}
	(0.008)	(0.003)
Rain prob.	-0.054^{***}	0.005
	(0.014)	(0.011)
λ	4.723^{***}	1.030^{***}
	(0.659)	(0.215)
ρ	4.757^{***}	7.387^{***}
	(1.206)	(1.708)
Main protest controls	Yes	Yes
Demographic controls	Yes	Yes
Economic controls	Yes	Yes
Observations	1,551	1,502

Notes: The table shows a heterogeneity analysis of our main results. Panel A considers counties in which the fraction of African Americans is below or above the median level. Panel B considers counties below and above the median population size, and Panel C considers counties above and below the median education levels, as measured by the fraction of individuals with a graduate degree. All other definitions are as in Table 2.

4.1 Mechanisms

The previous analysis shows that BLM protests caused an increase of the Democratic vote share in the 2020 presidential election. To shed light on possible mechanisms, the current section examines the effect of BLM protests on turnout rates and racial attitudes.

4.1.1 Turnout

In general, election outcomes are jointly determined by the number of people who come out to vote and the respective parties they vote for. Hence, the increase in the Democratic vote share suggests either an increase in the mobilization of Democratic-leaning voters, or a shift of Republican or Independent-oriented individuals towards the Democratic party. A potential mechanism through which BLM protests affected turnout is campaign messaging (Ansolabehere et al., 1999). For example, Donald Trump used Twitter to disseminate negative campaign messages related to BLM protests, trying to tie negative connotations to the Democratic party (Lonsdale, 2021). Previous studies show that such negative campaign messaging can have small but distinct effect on voter turnout (Goldstein and Freedman, 2002; Stevens et al., 2008; Barton et al., 2016; Gross and Johnson, 2016).

To explore the relative importance of these mechanisms, the current section examines the effect of protests on the overall turnout rate. The turnout rate is defined as the total number of votes in a county divided by the number of eligible voters. Analogous to our main analysis, we consider the change in turnout rates between the 2016 and 2020 presidential elections to remove time-invariant unobserved factors. We again employ a spatial two-stage least squares method to account for spatial autocorrelation and use rainfall as an instrument for protesting activity.

Table 4, Panel A presents the results. We find no significant effect on turnout in the first two specifications (both $p > 0.453$). Only after including economic controls do we find a significant estimate ($p = 0.042$). Hence, even though the 2020 election was characterized by historically high turnout rates, these rates are at most partly explained by local protesting activity. This finding suggests that while turnout may have played a role, BLM protests likely also elevated support for the Democratic party through a progressive shift among undecided voters.

To interpret the absence of a turnout effect, it is important to note that previous work also provides mixed results on whether protests affect turnout. Enos et al. (2019) studies the 1992 Los Angeles Riot and suggested that these events had a distinct effect in mobilising African American and white voters to register to vote. However, by looking at voter registration, they cannot separate political conversion, where voters who would have registered anyway register with a different party, from pure mobilization, where voters who would not otherwise have registered do so because of the riot. Directly related to our paper, Holbein and Hassell (2023) document a, increase in voter registration across the board following the 2020 BLM protests, which somewhat contrasts our results. One potential reason for this discrepancy is that we consider actual voting choices in the 2020 presidential election, whereas they consider voter registration immediately after the protest window.

Table 4: Ancillary analyses

	Model 1	Model 2	Model 3
Panel A: Turnout			
Attendees/Population	0.007	−0.007	0.017**
	(0.010)	(0.009)	(0.009)
Rain prob.	−0.020	−0.011	−0.010
	(0.017)	(0.015)	(0.015)
λ	−0.816***	−0.681***	−0.388*
	(0.207)	(0.211)	(0.207)
ρ	4.075***	3.792***	4.069***
	(0.281)	(0.286)	(0.312)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053
Panel B: Blacks should not receive special favors			
Attendees/Population	−0.620***	−0.626***	−0.646***
	(0.206)	(0.138)	(0.130)
Rain prob.	0.334	−0.235	−0.238
	(0.368)	(0.316)	(0.313)
λ	−0.008	−0.086*	−0.087**
	(0.056)	(0.045)	(0.044)
ρ	2.134***	0.122	0.107
	(0.714)	(0.845)	(0.849)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	2,556	2,556	2,556
Panel C: Slavery caused current disparities			
Attendees/Population	0.583***	0.552***	0.602***
	(0.219)	(0.142)	(0.133)
Rain prob.	−0.339	0.219	0.170
	(0.388)	(0.330)	(0.328)
λ	0.026	0.104*	0.117**
	(0.074)	(0.058)	(0.057)
ρ	2.415***	0.192	0.170
	(0.798)	(0.850)	(0.838)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	2,556	2,556	2,556

Notes: The table shows the effect of BLM protests between 26 May and 7 June 2020 on the change in the turnout rate between 2016 and 2020 (Panel A), whether Blacks should not receive special favors (Panel B), and whether slavery caused today's disparities (Panel C). All other definitions are as in Table 2.

4.1.2 Racial Attitudes

To investigate *why* the BLM movement may have changed voting preferences, we now turn to the effect of protests on racial attitudes. Stronger perceptions of widespread discrimination may explain why voters have swayed voters towards the Democrats, because the Democratic party purports to champion minority rights and advocates policies such as affirmative action. We use data from the Cooperative Election Study (CES) to estimate the effect of BLM protests on perceptions of discrimination and racial disadvantage (Schaffner et al., 2021*b*). In particular, we consider the following two statements: "*Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors*" and "*Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class*".[7] We aggregate evaluations of these questions at the county level, and employ the same spatial two-stage least squares methodology we used before. It should be noted that the CES data is not necessarily representative for the US population at the county-level. Although this does not affect the internal validity of our estimates, we acknowledge that the external validity may be lowered.

Table 4, Panels B and C show the results. We find that BLM protests caused an increase in the share of people who think that slavery caused the disadvantaged position of African Americans today (all $p < 0.003$), and a decrease in the share of those who claim that black people should work their way up without favors (all $p < 0.008$). In other words, BLM protests appear to have achieved its goal of changing people's attitudes about discrimination of African Americans, which, in turn, may have changed people's ballot box decisions.

4.2 IV assumptions

For our instrumental variable method to be valid, rainfall should have a discouraging effect on protesting activity. This is the so-called relevance condition. To test the validity of this assumption, we report the first-stage results in Table A6 in the Online Appendix. The first-stage regression shows the estimated effect of rainfall on protest activity. The results indicate that rainfall has a strong negative effect on protest participation. Therefore, we conclude that the instrument passes the relevance test.[8]

The second assumption is the exclusion restriction, which holds that rainfall during the protest window should only affects voting outcomes through its effect on BLM protests. This assumption is controversial, because precipitation likely affects non-protest variables such as crime and mood that might affect voting behavior (Mellon, 2021). To test the assumption, we first examine whether rainfall between 26 May 2020 and 7 June 2020 is

[7]The CES asks some additional race-related questions, but these are only asked to minority respondents and are therefore not directly relevant for our analysis.

[8]While the Spivreg package in Stata/R does not report F-statistics, the first-stage F-stat in our specification without spatial autocorrelation is 16.47.

correlated with election results in 2016 and 2012 to see whether rainfall can be considered conditionally independent. To be consistent with our main analysis, we use the change in the Democratic vote share compared to the prior election as the outcome variable. Second, we examine the effect of rainfall right before or right after the main protest window on the 2020 election to see whether rainfall affects voting through other channels such as crime or mood.

The results in Table A7 in the Online Appendix, Panels B and C show no significant association between rainfall in 2020 and earlier elections. This suggests that rainfall is independent of general trends in political sentiments. Panel A gives the reduced form estimates for the effect of rainfall on the 2020 election, showing a negative association between rainfall and Democrat vote shares. This is consistent with our main results, as precipitation reduces protest activity, which in turn reduces votes going to the Democrat party.

For our second test, we consider nine additional 13-day windows, six periods before the protests, and three periods after. For each of these periods, we estimate the effect of rainfall on our main outcome variable, namely the change in the Democrat vote share between the 2020 and 2016 elections. For all nine periods, we calculate each county's total amount of rainfall, as well as the average likelihood of rain in that area during that period. Similar to our main analyses, we use the latter as a control variable that helps us isolate rainfall shocks, rather than rainfall in general.

Figure A6 in the Online Appendix shows the results.[9] We find evidence of potential exclusion restriction violations, as there appears to be a significant association between rainfall outside the protest window and voting behavior. Although part of this association may be caused by serial correlation in weather patterns (most of the significant estimates are right before and right after the main protest period), we cannot dismiss the concern that rainfall affects voting through other channels than BLM protests. We therefore consider an alternative methodological approach in Section 5.

On a last note, one may be worried that rainfall directly affects media coverage of protests, rather than indirectly through its effect on protests. For example, extreme rainfall might crowd out other news items (including local protests), such that people in rainfall counties are less exposed to the BLM movement independent of the fact that fewer people go out to protest. It is important to note, however, that only very few places experienced extreme rainfall events, and that BLM protests were among the most salient news events of the year. Moreover, even among arguably less salient protests such as the Tea Party movement, the total effect (direct and indirect) of rainfall on media coverage was small (Madestam et al., 2013). Hence, the direct effect was plausibly even smaller, if existent at all. Hence, we do not believe that our results are driven by a direct effect of rainfall on media coverage.

[9]Table A8 shows the regression tables.

5 Short-Term vs. Long-Term Effects

To supplement our IV results, the current section presents the results of a difference-in-differences analysis, explained in Section 3.1. This approach poses three methodological advantages compared to the previous analysis. First, we now estimate the average treatment effect on the treated (ATET) instead of the local average treatment effect (LATE), which means that we consider *all* counties rather than only those in which rainfall affects protest activity. Second, we circumvent potential violations of the exclusion restriction associated with rainfall. Third, and potentially most substantial, we can study the evolution of the effect of protests over time.

We start with a replication of our IV analysis using a difference-in-differences methodology. Table 5 gives the regression results for the effect of protests on the 2020 presidential election. Similar to our main results, we find that BLM protests lead to a leftward shift in voting patterns. Model 1 shows that the presence of at least one BLM protest causes a 3.5 percentage point increase in a county's Democrat vote share in the 2020 election as compared to the 2016 and 2012 elections in that same county. Model 2 shows that a 1% increase in the fraction of the population that attends BLM protests causes a 1.9 percentage point increase in the Democrat vote share. Model 3 suggests that each additional day of protests raises the Democrat vote share by 0.9 percentage points. These results are qualitatively and quantitatively similar to our IV estimates.

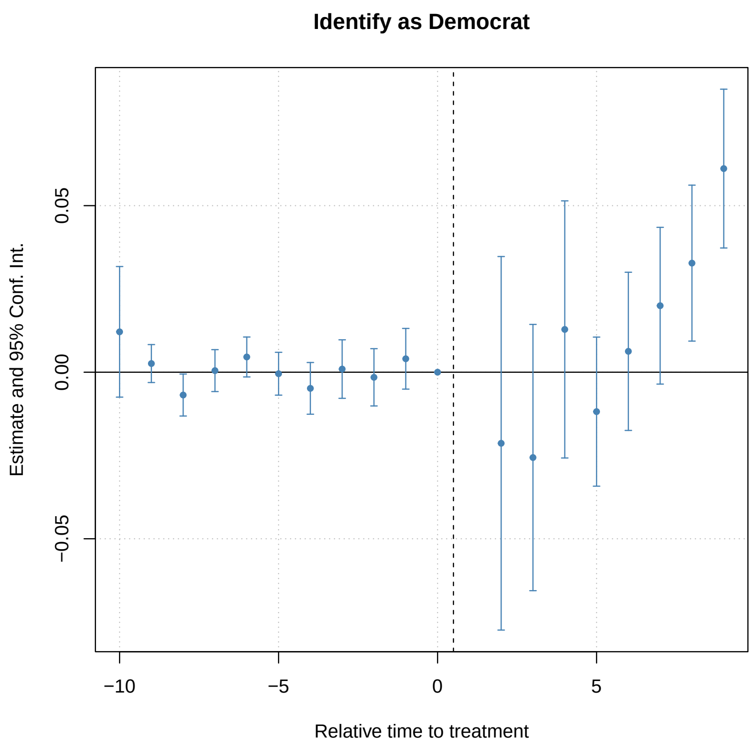
Table 5: Effect of BLM protests on voting, difference-in-differences

	Model 1	Model 2	Model 3
Protests (yes/no)	0.035***		
	(0.001)		
Attendees/Population		0.019***	
		(0.003)	
Days of protests			0.009***
			(0.0004)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	9,159	9,159	9,159
Adjusted R^2	0.953	0.950	0.953

Notes: The table shows the difference-in-difference estimates for the effect of BLM protests on the Democratic vote share in presidential elections. County-level election data are from 2012, 2016, and 2020. *Protests (yes/no)* is an indicator variable that takes the value of 1 if at least one protest took place in a county. *Attendees/Population* is the total number of people who attended the protests as a fraction of the population. *Days of protests* is the total number of days in which protests took place during the main protest window. All other definitions are as before.

In our next step, we use survey data from the Gallup Covid Panel to examine the

Figure 1: Evolution of effect of BLM protests on political orientation



Notes: The figure shows the dynamic difference-in-difference estimates for the effect of BLM protests on the likelihood that people identify as Democrat. *Relative time to treatment* measures the the number of weeks to the main BLM protest window. Data are from the Gallup Covid Panel survey.

evolution of the treatment effect over time. Figure 1 shows the results. Following the main wave of BLM protests, find an immediate *decrease* in the likelihood that respondents identify as Democrat as the result of local BLM protests. Although this reduction is statistically insignificant, there is suggestive evidence that BLM protests caused an initial backlash against the movement. Over time, this backlash disappears and even reverses. Indeed, eight weeks after the protests took place, BLM protests cause a significant *increase* in Democrat identification in protest counties.

These results highlight the importance of considering the time dimension when evaluating the effects of protests, as the immediate effects might be different from the longer run impacts. In the immediate aftermath of the protests, public reactions might have been driven by emotional responses to the protests' intensity, including media portrayals of violence or disruption. Over time, however, these initial emotional response appear to subside and be replaced by longer-term reflection on the issues raised by the protests, such as racial injustice and police brutality. As such, despite the initial backlash, BLM protests caused an increase in Democrat vote shares in the 2020 election.

6 Conclusion

We examine the effect of the Black Lives Matter (BLM) protests, which erupted after the death of George Floyd in May 2020, on the presidential election later that year. Using both instrumental variable and difference-in-differences approaches, we document a significant increase in Democrat vote shares as the result of BLM protests. Some of these effects likely only pertain to peaceful protests. At the same time, however, we provide suggestive evidence that the totality of protests also caused an initial backlash, with increased support for the Republican party in the immediate aftermath of the protests. Ancillary analyses indicate that turnout alone does not fully account for the observed increase in Democrat vote shares, suggesting a progressive shift among Independent or Republican-leaning voters. To support this claim, we present evidence that protests altered attitudes towards affirmative action and the role of slavery in explaining current racial disparities. In addition, heterogeneity analyses suggest that the effect of protests is relatively large in counties with smaller, whiter, and lower-educated populations.

Our analysis documents the effect of protests at the local rather than the national level. Hence, we posit that networks form a crucial transmission mechanism. Networks create local spillover effects because an individual's decision to engage in a BLM protest signals their perceived grievances with racial injustices, as well as their intention to vote Democrat. Through imitation and conversion, protest participation can consequently create a ripple effect whereby one protester potentially influences multiple non-participants (Steinert-Threlkeld, 2017). Alternative channels such as media coverage presumably play a more important role at the national level and are thus intuitively less appealing to explain between-county variation, although we cannot dismiss that local news coverage plays a mediating role as well. Irrespective of the exact transmission mechanism, it is noteworthy that the emotional impact of BLM protests in May and June remained highly salient until the 2020 presidential election (AP, 2020*b*).

Our findings contribute to the ongoing debate on whether demonstrations help or harm the protest's objectives. Prior research, notably Wasow (2020), highlights the central role of violence in shaping public perception: activism that eschews violence tends to align public opinion with the protesters' demands, whereas disruptive protests often lead to a backlash. Our results suggest that the timing of the subsequent election may be an alternative explanation for the mixed results in the protest literature. Specifically, our findings suggest that the temporal proximity of a protest to electoral events influences the direction of the results. When elections occur shortly after a major protest, voters might be more inclined to support law-and-order candidates as a means to re-establish stability. Conversely, if elections are more distant from the protest events, the public's response may be more aligned with the protesters' objectives. As such, depending on the timing, protests could either harm or help the cause.

In conclusion, our paper demonstrates that large-scale collective action can have a significant impact on important societal outcomes. While future research will have to assess whether the BLM movement also achieved its primary goal of promoting equal treatment in the criminal justice system, our findings reveal a clear and sizeable impact on the 2020 presidential election, as well as on racial attitudes. It also remains an open question whether our results generalize to other protest movements, countries, and time periods. Yet, while it is important to exercise some caution in drawing overly general conclusions, our findings certainly offer encouragement for marginalized groups to organize and participate in collective action.

Competing Interests The authors declare no competing interests.

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A Online Appendix

The Online Appendix provides additional details about the data and analyses described in the main text of the article, as well as follow-up analyses and robustness checks. Section S1 provides a detailed overview of data. Section S2 presents additional robustness checks and the results of the first-stage analysis.

A1 Data

We compile our data set from multiple independent sources. Information on Black Lives Matter protests comes from the Crowd Counting Consortium (CCC), an organization that assembles publicly available dissent and collective action statistics.[10] We focus our analysis on the two-week period from 26 May to 7 June 2020, which directly follows George Floyd's death on May 25th.[11] The initial data set contains 4,870 racial justice protests during this period. After excluding online protests and protests that could not be matched to a county, our sample contains 4,667 protests across 1,222 counties. For each county, we aggregate total number of protest attendees, which serve as our main measure of protest activity. The CCC reports both lower and upper bounds for the number of protesters, and to be conservative in our estimates, we use the lower number for our main analyses.[12]

We obtain precipitation data from the National Oceanic and Atmospheric Administration (NOAA). NOAA collects daily weather statistics from approximately 15,000 weather stations across the United States. For each county, we obtain daily precipitation levels between 26 May and 7 June 2020 at the weather station closest to the center of the county. We additionally obtain the daily likelihood of rain during this window, which is the probability of 0.01" inch of precipitation or more at the weather station on a given day. We then calculate the average precipitation likelihood for the protest window by taking the average of the daily rainfall probabilities over this period. We use this variable to control for general climatic conditions that may correlate with voting-relevant characteristics such as the average age, income and ethnic composition of a county.

County-by-county voting data for presidential elections come from the MIT Election Data and Science Lab for the 2012-2016 elections and from the Associated Press for the 2020 election (Data and Lab, 2017; AP, 2020*a*). Our main variable of interest is the change in the Democratic vote share between 2016 and 2020. Using the change rather than the level eliminates unobserved time-invariant characteristics that may correlate with voting behavior.

[10] CCC data was validated for the 2017 Women's March using cellphone tracking data and social media activity (Sobolev et al., 2020). The authors conclude that the CCC provides accurate estimates of protest sizes.

[11] Subsection Alternative Window examines the robustness of our results to different sample window choices.

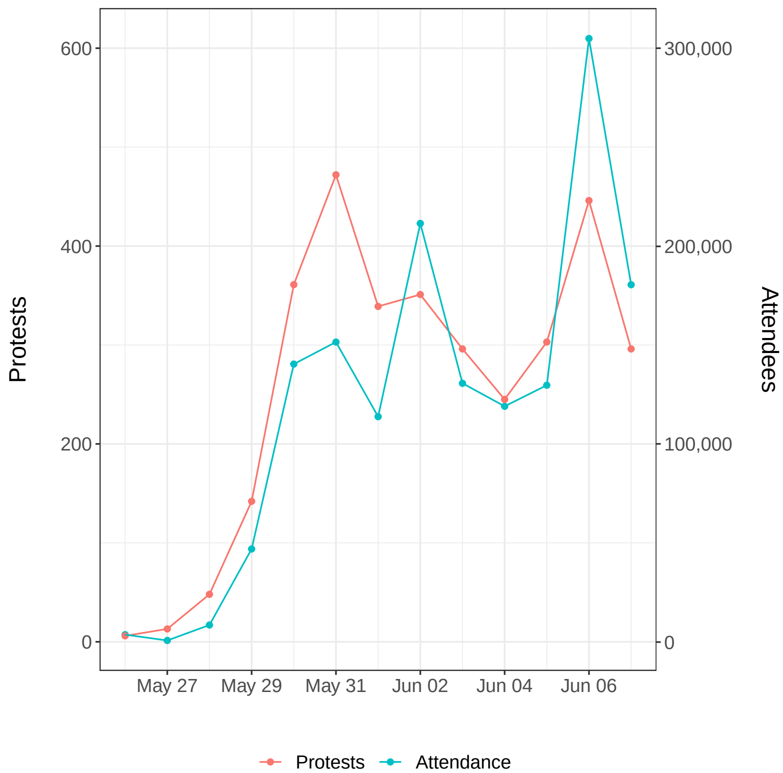
[12] The results do not materially change when we consider the upper bound instead.

We obtain county-level racial attitude data from the Cooperative Election Study (CES; Schaffner et al., 2021*a*). The CES is a representative survey 60,000 American adults that is administered by YouGov. The survey consists of a pre-election part, administered in September, and a post-election part, administered in November. We consider two questions from the post-selection survey that ask respondents to evaluate the following statements: "*Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors*" and "*Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class*". Both are rated on a five-point scale, ranging from strongly agree to strongly disagree. We recode these answers such that the lowest score of 1 corresponds to "strongly disagree" and the highest score of 5 to "strongly agree".

We gather information on county-level demographic and economic variables from the US Census. For each county, we obtain information on the total population, the number of eligible voters, the fractions of African Americans, Asians, Hispanics and Whites, and the fractions of people who did not finish high school, finished only high school, finished vocational school, have some college experience, finished college, and finished a postgraduate degree. We further obtained the population density, median age, the median income, and the unemployment rate, all measured in 2019. We obtain county-level Covid-19 statistics from The New York Times Coronavirus (Covid-19) Data (The New York Times, 2021). For each county, we consider the cumulative case load and death count on the day prior to the presidential election.

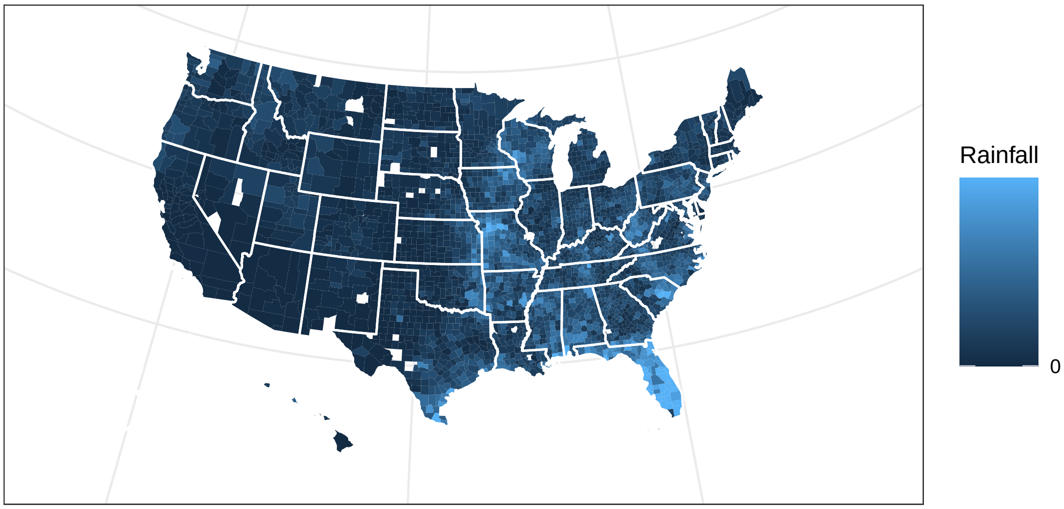
Figure A1 shows the development of BLM protests over the course of our sample period. After George Floyd's death on 25 May 2020, BLM protests were initially confined to Minneapolis and a few other cities. Yet, in a matter of days, the movement had spread nationwide, reaching its first peak in the weekend of May 31 and remaining high thereafter. Figures A2 to A4 display the geographical dispersion of rainfall, protesting activity and voting across the United States. The figures show that protests mostly erupted in Democratic-leaning counties, which corroborates our intuition that BLM protests likely reflected latent political preferences. Figure A2 demonstrates the spatial correlation of rainfall patterns, which highlights the importance of controlling for spatial dependencies in our estimators.

Figure A1: Protests activity from May 26 to June 7



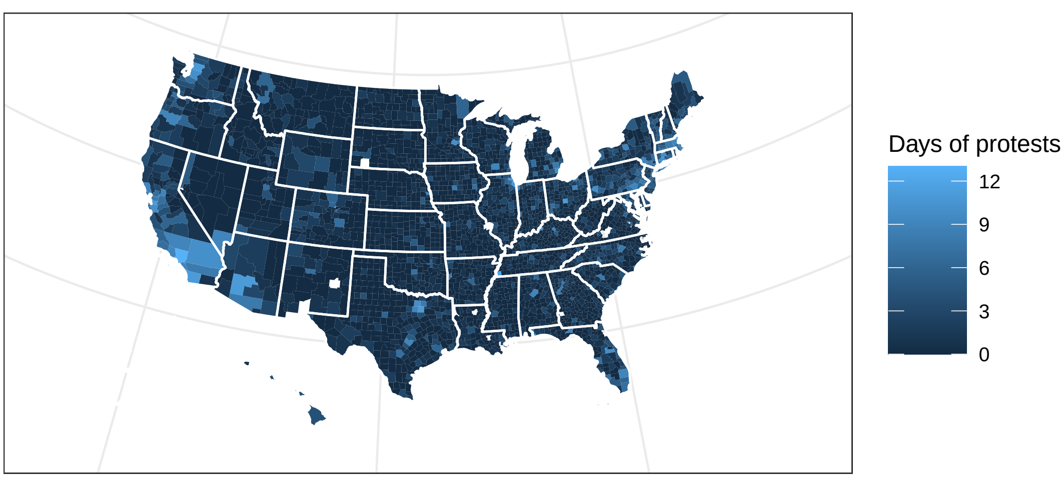
Notes: The figure displays the number of protest counties (red, left axis) and the number of protests attendees per day for all counties combines (blue, right axis).

Figure A2: US Map of rainfall per county



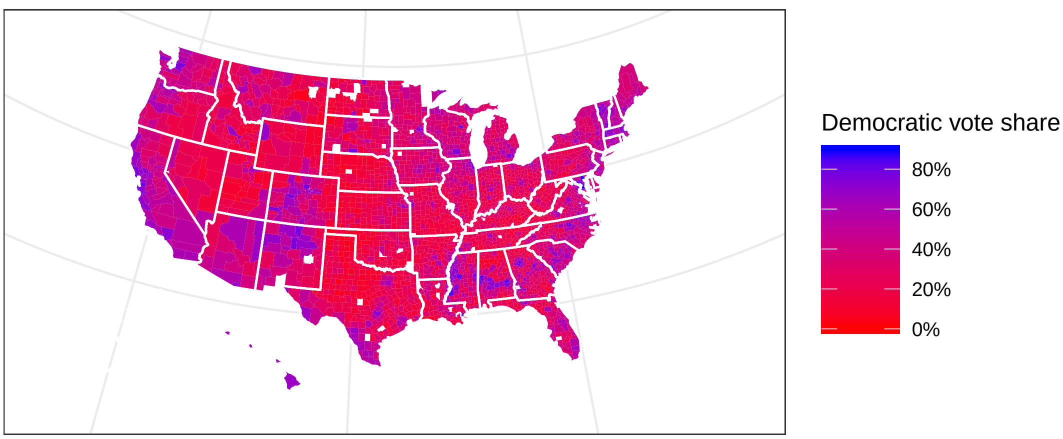
Notes: The figure displays the distribution of the logarithm of rainfall between 26 May and 7 June 2020 across the United States. Rainfall is top-winsorized at the 99% level

Figure A3: US Map of days of protests per county



Notes: The figure displays the distribution of the number of protest days between 26 May and 7 June 2020 across the United States.

Figure A4: US Map of the 2020 presidential election outcome per county



Notes: The figure displays the distribution of the Democratic vote share in the 2020 presidential election across the United States.

A1.1 Gallup Covid Panel

During the COVID-19 pandemic, Gallup conducted a targeted survey starting on March 13, 2020, which drew daily random samples from the Gallup Panel, a probability-based and nationally representative panel of U.S. adults. From March 13 to April 26, 2020, approximately 1,200 daily completes were collected, and from April 27 to August 2020, the daily completes were reduced to approximately 500. Importantly for the purpose of our study, the survey encompasses questions about respondents' political orientation, as well as socioeconomic information such as employment status, income, and other demographic variables. Our main outcome variable is alignment with the Democrats. We use a question in which respondents were asked, "In politics, as of today, with which political party do you most closely affiliate?" The possible answers included "Democrat", "Republican", "Independent", and "Other Party". We code responses to this questions as a binary variable (Democrat support) that takes the value of 1 if a respondents aligns with the Democrat party and 0 otherwise.

Table Table A1 presents summary statistics of key demographic variables from the survey.

Table A1: Summary Statistics Gallup Covid Panel

	Mean	Min	Max
Democrat	0.43	0	1
Protest county	0.87	0	1
Democrat, protest county	0.44	0	1

Notes: The table gives summary statistics for the Gallup Covid Panel data ($N = 85,106$). *Democrat* is an indicator variable that takes the value of 1 if a respondent identifies with the Democrat party. Protest county is an indicator variable that takes the value of 1 if a respondent lives in a protest county. *Democrat, protest county* gives the fraction of Democrat identifiers in protest counties.

A2 Spatially Adjusted Instrumental Variables

A complication of using rainfall as an instrument is that both weather conditions and outcome variables are generally spatially correlated. Such dependencies can severely bias IV estimates (Plümper and Neumayer, 2010). To see why, consider the effect of any endogenous variable $\mathbf{x}$ on outcome $\mathbf{y}$. Now imagine a hypothetical county C1 that experiences spillover effects from bordering counties C2, C3 and C4, whereby the relative importance of each surrounding county in determining C1's outcome is captured by the spatial weighting matrix $\mathbf{W}$. The unexplained error term of county C1 thus consists of a non-spatially dependent part $\mathbf{u}$ that correlates with $\mathbf{x}$, and a spatially dependent part $\rho\mathbf{W}\mathbf{y}$ that depends on the outcomes of the surrounding counties and the strength of spillover effects ρ. The standard two-stage least-squares approach tackles the endogeneity issue by instrumenting for $\mathbf{x}$ with an exogenous variable $\mathbf{z}$, but ignores the spatial interdependence.

Doing so results in a violation of the exclusion restriction, because the instrument $\mathbf{z}$ correlates with the outcome disturbances through $\mathbf{Wy}$, *even if the instrument itself is randomly distributed* (Betz et al., 2020). To understand why, note that the instrument $\mathbf{z}$ affects the outcome value of C1 (through $\mathbf{x}$), which spills over into the surrounding counties, and subsequently spills back into C1 as a second-order spatial lag. If, moreover, the instrument has a similar spatial structure as the outcome variable, the bias will be substantially higher and generally leads to highly inflated effect size estimates.

A3 Robustness Checks

This subsection includes a number of robustness checks for our main results, addressing variation in the sample window frame, alternative spatial correlation structures, alternative protest measures, alternative weather instruments, and ignoring spatial autocorrelation.

A3.0.1 Sample Window Selection

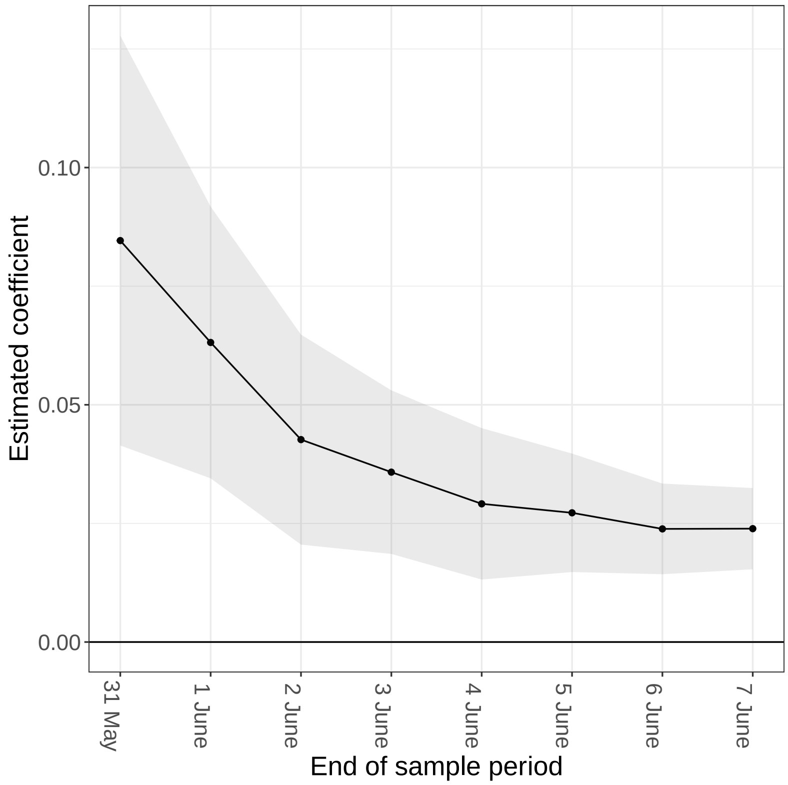
Our second set of robustness checks examines the sensitivity of our results to our sample period selection. Thus far, we have focused all our analysis on the period from 26 May to 7 June 2020. Because this window choice is admittedly somewhat arbitrary, we consider a set of alternative sample periods that start on May 26 and end anywhere between May 31 and June 6. We adjust all variables to appertain to the restricted sample period, and we report the results of the most complete model that includes both demographic and economic controls.

Figure A5 displays the estimated effects for different protest windows. Unsurprisingly, the effect declines in the length of the sample period. Protests were arguably most intense and attention-grabbing—and thus most consequential—in the first few days after George Floyd's death. More importantly, our main conclusion that BLM protests led to an increase in the democratic vote share holds for a wide range of alternative sample periods. If anything, Figure A5 suggests that the effect of protests on the Democratic vote share is even stronger in more restrictive time frames.

A3.0.2 Alternative Spatial Structure

In our third robustness check, we examine the sensitivity of our results to different choices of the spatial weighting matrix $\mathbf{W}$. Because most spatial structures are relatively similar, however, incorrectly choosing one option over another need not strongly affect our results. Hence, even a misspecified $\mathbf{W}$ provides a significant improvement compared to ignoring spatial dependencies altogether (LeSage and Pace, 2014). In our main specification, we calculate distance between counties using haversine distance, which calculates a spherical distance between the spatial units from coordinates. Here, we consider five alternatives: Euclidean distance, haversine distance based on radians, and Minkowski distances of

Figure A5: Effect of BLM protests on the 2020 presidential election for different sample periods



Notes: The figure displays the GMM-IV estimates for the effect of protest attendees as a fraction of the population on the change in the Democratic vote share between 2016 and 2020 for different sample periods endings. Each sample starts on May 26. All models include demographic and economic controls.

order 1, 2 and 3 (see Drukker, Peng, Prucha and Raciborski, 2013, for implementation details).

The results in Table A2 show that our estimates are robust to different choices of the spatial weighing matrix. The effect of protest attendance on the Democratic vote share is statistically significant for each of the five models, and the coefficients range from 0.024 to 0.027. Hence, our conclusions do not appear to depend on the exact specification of **W** .

A3.0.3 Protest Days as a Measure of Protesting Activity

In our fourth robustness check, we consider an alternative measure of protesting activity, namely the number of protesting days. We define 'days of protests' as a county's number

Table A2: Alternative spatial structures

	Model 1	Model 2	Model 3	Model 4	Model 5
Attendees/Population	0.026^{***}	0.025^{***}	0.024^{***}	0.026^{***}	0.027^{***}
	(0.004)	(0.007)	(0.004)	(0.004)	(0.005)
Rain prob.	-0.020^{**}	-0.015^{*}	-0.018^{**}	-0.020^{**}	-0.020^{**}
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
λ	2.418^{***}	0.025	2.415^{***}	2.418^{***}	2.422^{***}
	(0.400)	(0.198)	(0.392)	(0.400)	(0.401)
ρ	5.844^{***}	-0.602	5.453^{***}	5.844^{***}	5.992^{***}
	(1.031)	(1.145)	(0.836)	(1.031)	(1.129)
Main protest controls	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes	Yes	Yes
W	Euclidean	Rhaversine	Minkowski (1)	Minkowski (2)	Minkowski (3)
Observations	3,053	3,053	3,053	3,053	3,053

Notes: The table shows the results for different choices of the spatial weighting matrix **W**. All models are estimated a GMM-IV estimator (Drukker, Egger and Prucha, 2013). All models include demographic and economic control variables. All other definitions are as in Table A6.

of days during our protest window with at least one BLM protest.

Panel A of Table A3 presents the results. Using rainfall-induced protest variation, we find a positive effect of BLM protests on the Democratic vote share in the 2020 presidential election. The results are similar to our main analysis. Model 1 shows that an additional day of protests raises the Democratic vote share in that county by an average of 1.2 percentage points ($p < 0.001$). Models 2 and 3 show that the effect remains highly significant when we control for demographic and economic control variables (both $p < 0.001$). Hence, our conclusions do not seem to hinge on our specific measure for protesting activity.

Table A3: Additional robustness checks

	Model 1	Model 2	Model 3
Panel A: Days of protests			
Days of protests	0.012***	0.005***	0.005***
	(0.002)	(0.001)	(0.001)
Rain prob.	−0.030***	−0.022***	−0.019**
	(0.010)	(0.008)	(0.008)
λ	0.911**	2.031***	1.899***
	(0.441)	(0.314)	(0.314)
ρ	5.573***	6.068***	6.299***
	(0.855)	(0.811)	(0.865)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053
Panel B: Two rainfall instruments			
Attendees/Population	0.037***	0.023***	0.025***
	(0.007)	(0.004)	(0.004)
Rain prob.	−0.030**	−0.025***	−0.020**
	(0.012)	(0.009)	(0.009)
λ	1.982***	2.600***	2.506***
	(0.486)	(0.424)	(0.405)
ρ	5.637***	5.187***	5.429***
	(1.185)	(0.742)	(0.847)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053
Panel C: Temperature instruments			
Attendees/Population	0.043***	0.023***	0.020***
	(0.006)	(0.005)	(0.005)
Rain prob.	−0.041**	−0.023*	−0.021*
	(0.016)	(0.012)	(0.011)
λ	4.362***	3.384***	3.667***
	(0.926)	(0.739)	(0.686)
ρ	3.469***	5.839***	5.954***
	(0.652)	(1.543)	(1.457)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	1,444	1,444	1,444
Panel D: Ignoring spatial autocorrelation			
Attendees/Population	0.153***	0.124***	0.063**
	(0.035)	(0.032)	(0.028)
Rain prob.	−0.062*	−0.110***	−0.032
	(0.033)	(0.034)	(0.034)
First-stage F-stat	16.47	10.04	11.22
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053

Notes: The table shows robustness checks for our main analysis. Panel A considers the number of protest days as an alternative measure of protesting activity. Panel B uses both the total amount of rainfall and the number of rainy days as instruments for protesting activity. Both models are estimated using a GMM-IV estimator (Drukker, Egger and Prucha, 2013). Panel C shows the estimation results when we ignore spatial autocorrelation and use standard two-stage least squares estimation. All other definitions are as in Table A6.

A3.0.4 Alternative Instruments

In our fifth robustness check, we address the issue that our instrument—the amount of rainfall—ignores potentially important variation in the distribution of rainfall over time.

For a given amount of rainfall, one day of heavy rain showers may discourage protesters more (or less) than two weeks of continuous drizzle. To account for both sources of rainfall variation, the current section extends the main model with an additional instrument in the form of the number of rainy days, defined as days with at least 0.1 millimeter of precipitation.[13]

Table A3, Panel B shows the results. In line with our main findings, *Attendees/Population* exerts a positive influence on the share of votes going to the Democratic candidate. In fact, adding the number of rainy days as an additional instrument leaves the coefficients virtually unchanged. To avoid redundancy, we therefore favor our main specification in which the total amount of rainfall serves as the sole instrument for protesting activity.

Another possibility is to consider weather variables such as temperature as instruments for protesting activity. Here, we consider average and maximum temperature during the protest window as additional instruments.[14] The results in Table A3 are highly similar to our main results.[15] Because it is unclear whether temperature satisfies the monotonicity requirement for instrumental variable analyses, however, we believe that our main rainfall instrument is more trustworthy.

A3.0.5 Ignoring Spatial Autocorrelation

Table A2 shows that the exact choice of the spatial autocorrelation structure does not materially change our results. Hence, one may be inclined to think that we can ignore spatial autocorrelation altogether. The current section shows the standard two-stage least square estimates without controlling for spatial dependencies. The results in Table A3, Panel D show that the standard two-stage least squares model inflate the effect of protests on vote shares three to five times. This result highlights the importance of controlling for spatial spillovers, even if the exact specification of the spatial structure is less consequential.

A3.1 State Fixed Effects

Table A4 shows that the results remain qualitatively unaltered when we include state fixed effects that control for unobserved heterogeneity at the state level.

[13] Conducting an analysis with only *days of rain* leads to the same conclusions.

[14] Not all weather stations measure temperature. Hence, the sample used in the current analysis is smaller.

[15] Table A9 shows the first-stage results. Adding temperature appears to increase the strength of the instrument.

Table A4: Effect of BLM protests on voting, state fixed effects

	Model 1	Model 2	Model 3
Attendees/Population	0.005^{*}	0.004^{**}	0.005^{**}
	(0.003)	(0.002)	(0.002)
Rain prob.	0.042^{***}	0.019^{**}	0.020^{**}
	(0.010)	(0.008)	(0.008)
λ	5.439^{***}	4.476^{***}	3.938^{***}
	(0.557)	(0.459)	(0.488)
ρ	6.400^{***}	6.445^{***}	8.129^{***}
	(1.591)	(1.728)	(2.646)
State fixed effects	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053

Notes: The table shows the results when we control for state-fixed effects. All definitions are as in Table A6.

A3.2 Weighted Regressions

Table A5 shows that when we weigh counties by population size, the results remain similar.

Table A5: The Effect of BLM Protests on the 2020 Presidential Election, weighted, no spatial adjustment

	Model 1	Model 2	Model 3
Attendees/Population	0.072^{***}	0.085^{***}	0.066^{***}
	(0.010)	(0.018)	(0.011)
Rain prob.	-0.112^{***}	-0.094^{***}	-0.076^{***}
	(0.012)	(0.014)	(0.010)
Observations	3053	3053	3053
First-stage F-stat	71.3	27.84	44.33
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes

Notes: The table shows the results when we weight counties by population size. The results are obtained with a standard 2SLS estimator without spatial adjustment. All other definitions are as in Table A6.

A4 Additional Tables

Table A6: Results first stage

	Model 1	Model 2	Model 3
Total rain	-0.092^{***}	-0.078^{**}	-0.072^{**}
	(0.031)	(0.031)	(0.030)
Rain prob.	0.821^{***}	0.561^{***}	0.354^{*}
	(0.200)	(0.197)	(0.195)
λ	0.103	-0.866	0.328
	(0.647)	(0.736)	(0.674)
ρ	2.524^{***}	3.303^{***}	2.785^{***}
	(0.291)	(0.460)	(0.385)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053

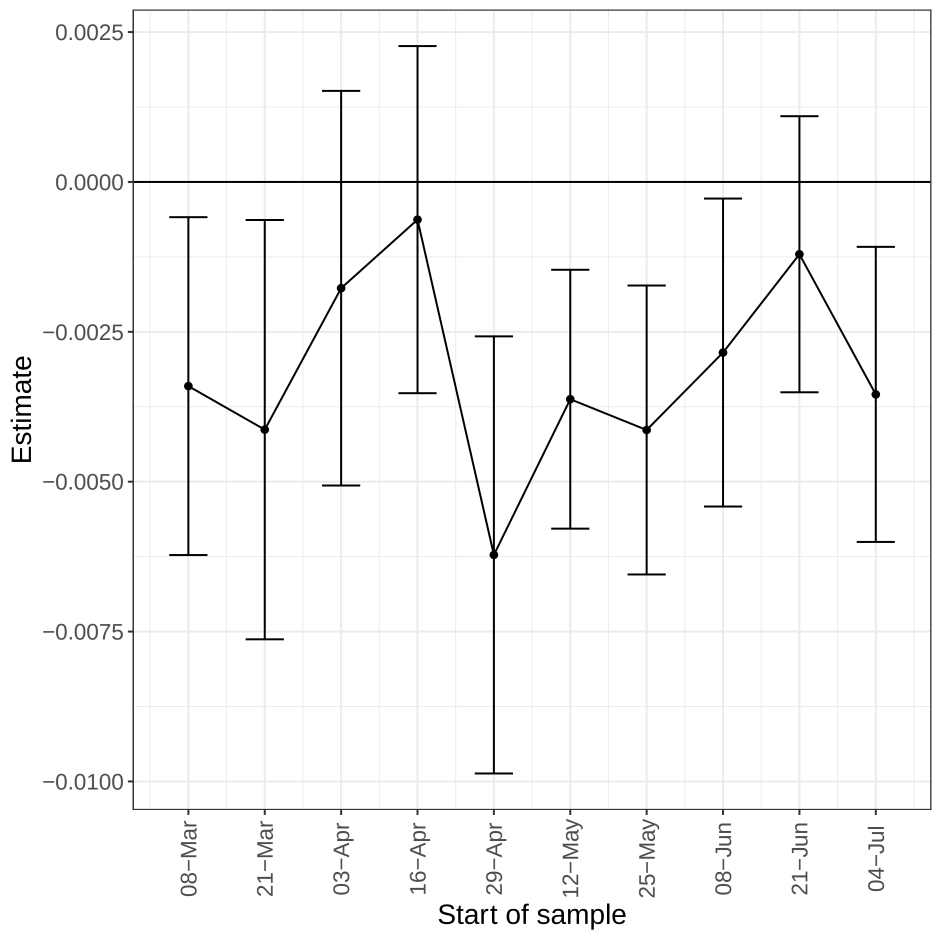
Notes: The table shows the effect of rainfall on the number of BLM protesters as a fraction of the population between 26 May and 7 June 2020. All effects are estimated using a spatial-autoregressive model (Drukker, Prucha and Raciborski, 2013). *Total rain* is the amount of rainfall in centimeters during this period. *Rain prob.* is the average probability of rainfall. *Population* is the county's population size in 100,000s. All estimations control for population size, population density, racial composition, and cumulative Covid-19 case and death counts on the day prior to the election. λ and ρ indicate the strength of spatial autocorrelation in the outcome variable and the error term respectively. *Demographic controls* contain a county's racial composition and the median age, and *Economic controls* contain the unemployment rate and the median income. Standard errors are in parentheses.

Table A7: Reduced form estimates for current and previous elections

	Model 1	Model 2	Model 3
Panel A: Change in Democratic vote share 2016-2020			
Total rain	-0.008^{***}	-0.005^{***}	-0.005^{***}
	(0.002)	(0.001)	(0.001)
Rain prob.	0.010	-0.008	-0.007
	(0.009)	(0.007)	(0.008)
λ	0.283	1.270^{***}	1.334^{***}
	(0.413)	(0.336)	(0.330)
ρ	5.646^{***}	5.478^{***}	5.734^{***}
	(0.859)	(0.547)	(0.577)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053
Panel B: Change in Democratic vote share 2012-2016			
Total rain	-0.003	-0.0005	-0.0003
	(0.002)	(0.002)	(0.002)
Rain prob.	-0.021^{*}	-0.060^{***}	-0.055^{***}
	(0.012)	(0.010)	(0.010)
λ	0.239^{**}	0.134	0.195^{**}
	(0.108)	(0.087)	(0.086)
ρ	4.583^{***}	4.350^{***}	4.363^{***}
	(0.293)	(0.217)	(0.218)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053
Panel C: Change in Democratic vote share 2008-2012			
Total rain	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Rain prob.	0.016^{**}	0.018^{***}	0.018^{***}
	(0.006)	(0.007)	(0.007)
λ	0.856^{***}	0.844^{***}	0.880^{***}
	(0.078)	(0.091)	(0.091)
ρ	9.019^{***}	8.698^{***}	8.747^{***}
	(0.473)	(0.444)	(0.455)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	3,053	3,053	3,053

Notes: The table shows reduced form estimates for the effect of rainfall between 26 May and 7 June 2020 on election outcomes. Panel A considers on the change in the Democratic vote share between the 2016 and 2020 presidential elections. Panel B considers the change in the Democratic vote share between the 2012 and 2016 elections, and Panel C considers the change in the Democratic vote share between the 2008 and 2012 elections. All effects are estimated using a spatial-autoregressive model (Drukker, Prucha and Raciborski, 2013). All other definitions are as in Table 2.

Figure A6: Placebo regressions of rainfall on voting behavior



The figure shows the reduced-form estimates for the effect of rainfall during seven two-week periods on the Democratic vote share in the 2020 presidential election. The horizontal axis shows the start date of each sample. Bars indicate estimated effects and error bars give 95% confidence intervals.

Table A8: Placebo regressions

	Protest period	Placebo1	Placebo2	Placebo3	Placebo4	Placebo5	Placebo6	Placebo7	Placebo8	Placebo9
Total rain	-0.004^{***}	-0.003^{**}	-0.004^{**}	-0.002	-0.001	-0.006^{***}	-0.004^{***}	-0.003^{**}	-0.001	-0.004^{***}
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Rain prob.	-0.008	0.040^{***}	0.039^{***}	0.026^{***}	0.017^{**}	0.009	-0.002	-0.010	-0.003	0.007
	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
λ	1.606^{***}	1.515^{***}	1.636^{***}	1.521^{***}	1.522^{***}	1.740^{***}	1.916^{***}	1.693^{***}	1.877^{***}	1.761^{***}
	(0.334)	(0.295)	(0.313)	(0.327)	(0.334)	(0.354)	(0.331)	(0.325)	(0.295)	(0.281)
ρ	5.722^{***}	6.693^{***}	6.306^{***}	6.227^{***}	6.149^{***}	5.607^{***}	5.955^{***}	5.661^{***}	6.144^{***}	6.457^{***}
	(0.596)	(0.829)	(0.929)	(0.820)	(0.750)	(0.590)	(0.645)	(0.645)	(0.657)	(0.763)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,053	3,053	3,053	3,053	3,053	3,053	3,053	3,053	3,053	3,053

Notes: The table shows reduced-form estimates for the effect of rainfall *before* George Floyd's death on 25 May 2020 on the change in the Democratic vote share between the 2016 and 2020 presidential elections. Period 1 is from 8 March to 20 March, Period to from 21 March to 2 April, Period 3 from 3 April to 15 April, Period 4 from 16 April to 28 April, Period 5 from 29 April to 11 May, Period 6 from 12 May to 24 May, Period 7 from 8 June to 20 June, Period 8 from 21 June to 3 July, and Period 9 from 4 July to 16 July. All effects are estimated using a spatial-autoregressive model (Drukker, Prucha and Raciborski, 2013). All other definitions are as in Table A6.

Table A9: Results first-stage temperature as additional instrument

	Attendees/Population		
	Model 1	Model 2	Model 3
Total rain	-0.118^{**}	-0.069	-0.063
	(0.056)	(0.052)	(0.052)
Max temp	0.003^{**}	0.003^{**}	0.003^{***}
	(0.001)	(0.001)	(0.001)
Av. temp	0.003^{**}	0.003^{**}	0.003^{***}
	(0.001)	(0.001)	(0.001)
Rain prob.	0.642^{*}	0.126	-0.042
	(0.329)	(0.306)	(0.308)
λ	0.161	2.210^{**}	2.852^{***}
	(1.393)	(1.093)	(1.060)
ρ	3.069^{***}	1.265^{***}	1.126^{**}
	(1.147)	(0.367)	(0.464)
Main protest controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Economic controls	No	No	Yes
Observations	1,444	1,444	1,444

Notes: The table shows the first-stage results when we use rainfall, maximum temperature, and average temperature as instruments for protesting activity. All definitions are as in Table A6.