Political Behavior
https://doi.org/10.1007/s11109-025-10014-w

ORIGINAL PAPER

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

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Accepted: 4 February 2025

Abstract
Do mass mobilizations drive social change? This paper explores this question by studying how the Black Lives Matter protests following George Floyd's death influenced the 2020 U.S. presidential election. Using rainfall as an instrument for protest participation and complementary difference-in-differences analyses, we show that protest activity significantly increased Democratic vote share in affected coun- ties. Our research makes three key contributions. First, we show causal evidence for the effect of one of the largest protest movements ever recorded on electoral out- comes. Second, we provide evidence of novel temporal dynamics: while protests ini- tially triggered a conservative backlash, they ultimately generated progressive shifts in voting behavior. Third, we identify mechanisms driving these effects, showing that rather than merely mobilizing existing Democratic voters, protests substantively shifted political preferences and beliefs about racial inequality.

Keywords Collective Action · Black Lives Matter · Presidential Elections · Protests

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) move- ment emerged, aiming to confront and overhaul patterns of racial inequality manifested in incarceration and police brutality (Williamson, Trump, and

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Published online: 04 March 2025

Einstein 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 par- ticipated 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). This dual potential is particularly salient for the BLM movement, which garnered widespread attention but also faced criticism and counter-mobilization in some contexts, as we explore in our analysis (Kim and A. Lee 2021). 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 impor- tant issue (AP 2020b).

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 elec- tion 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 in- strumental 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; William J Collins and Margo 2007; Madestam et al. 2013; Wasow 2020; Casanueva 2021; Meier, Schmid, and Stutzer 2019). The general idea behind the instrument is that rainfall shocks discourage prospec- tive 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 counties with protests to the change in those without.

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 show that the effect of protests is larger in counties with relatively small, white, and low-educated pop- ulations.

Next, we examine the evolution of the effect over time. Our results reveal an interest- ing reversal: Initially, BLM protests caused a counter-reaction, increasing support for the Republican party among those living in areas with more protesting activity. This immedi- ate backlash may be due to the media's focus on the more violent aspects of the protests. However, as these violent aspects receded from the public discourse over time, we ob- serve that those living in protest counties increasingly shifted towards the Democratic 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 of whether protests advance or hinder protesters' goals. On the one hand, riots in Los Angeles and Tea Party protests, as well as protests advo- cating civil rights protests, immigration, and the environment, generated support for the protesters' goals (Carey Jr, R. P. Branton, and Martinez-Ebers 2014; Enos, Kaufman, and Sands 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; William J 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. Our findings help resolve these conflicting results by demonstrating how protest movements shape electoral outcomes through both immediate backlash and longer-term attitude changes.

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.

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 in- crease police accountability holds central importance (Williamson, Trump, and Einstein 2018). Politically, BLM protests are often implicitly associated with the Democratic party. Democratic party traditionally champions 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 fac- tors 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 engen- dering 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 (Granovet- ter 1978; Passarelli and Tabellini 2017; Hollyer, Rosendorff, and Vreeland 2015; Little, Tucker, and LaGatta 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 com- plementarity 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 network, offer- ing 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, Trump, and Einstein 2018), and his death coincides with widespread dissatisfaction with the incumbent presi- dent. Once protests reached a critical mass, network effects transformed the initial demon- strations into an unprecedented social movement.

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

Network-based theories propose that networks can amplify information effects (Bursz- tyn et al. 2021). While anonymous protesters can be dismissed as extremists or radicals, environmental cues from one's 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, Green, and Larimer 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 elec- toral 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 cov- erage (Wasow 2020). The nature of protesting activities plays a crucial role in agenda seeding because it determines whether the media frames a movement positively or neg- atively. 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 per- ception of BLM protests among Americans on opposite sides of the political spectrum (SignalAI 2021; Bolsover 2020).

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 rep- resents one of the most comprehensive efforts to track protest events across the U.S., we acknowledge it is a crowd-sourced dataset and may suffer from some degree of mea- surement 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 al- most 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 calcu- late 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 mea-

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

sured 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 aver- age 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 2020a). We obtain county-level racial attitude data from the Coop- erative Election Study (CES; Schaffner, S. Ansolabehere, and Luks 2021a), 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 fol- lows 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 se- lected 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 de- tailed 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 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

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% 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 protests attended by 0.20% of the population. In protest counties, these numbers are 2.7 days and 0.50%. 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%- age points between 2016 and 2020. In protest counties, this increase was 3% 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.

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 endo- geneity 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, William J 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 rain- fall 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, Lefgren, and Moretti 2007; Ranson 2014; Baylis et al. 2018; Frijters, Lalji, and Pakrashi 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 there- fore 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 con- ditions and outcome variables are generally spatially correlated. Such dependencies are often ignored in applied work, but they can severely bias IV estimates (Plu¨mper 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 of any of our results, but it produces implausibly large effect size estimates.

An important modelling decision for these type of models is the choice of spatial weighting matrix **W**, which represents the degree of spatial correlation between observa- tions. 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 the distance between them.[3] We estimate the model using the GMM-IV approach outlined in Drukker et al. 2013a, 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:

$$\mathrm{Y}_i = \beta_0 + \lambda \sum_{\mathrm{j}=1}^{N} \mathrm{W}_{ij}\mathrm{Y}_j + \beta_1 \mathrm{Protests}_i + \alpha X_i + \mathrm{u}_i \tag{1}$$

$$\mathrm{u}_i = \rho \sum_{j=1}^{N} \mathrm{W}_{ij}\mathrm{u}_j + \epsilon_i \tag{2}$$

Y_i is the outcome variable in county $i \; \varepsilon \; \{1, 2, .., N\}$. 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 cor- relate 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. *P-rotests*$_i$ 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 pro-

[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.

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

test 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 compo- sition*, measured by the number of Whites, Blacks, Asians, and Hispanics as a fraction of the population in county i. Race is likely to be an important driver of BLM protests. As additional demographic control variables, we consider a county's education level (frac- tions of people with high school, college and graduate degrees) and median age, as well as economic control variables, such as the median income and unemployment rate. The co- efficients 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 protest counties would have de- veloped along the same path as it did in non-protest counties, had the protests not oc- curred. This is the so-called parallel trends assumption. It is important to note that this methodology does not require that protests are equally likely to occur in Democratic and Republican counties.

We estimate the following model:

$$\mathrm{Y}_{it} = \beta_1 \times \mathrm{Protests}_i \times \mathrm{PostGF}_t + \alpha_i + \gamma_t + \epsilon_{it} \tag{3}$$

where Y_{it} is the Democratic vote share in county i in year t. For each county, we con- sider 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 geo-political 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). We additionally esti-mate a dynamic version of Eq. (3) to test the parallel trends assumption.

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

[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.

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:

$$\mathrm{Y}_{ict} = \sum_{t=-10}^{10} \tau^t \mathrm{Protests}^t + \alpha_c + \gamma_t + \epsilon_{ict} \quad (4)$$

where *Yict* is an indicator variable that takes the value of 1 if individual *i* residing in county *c* identifies as a Democrat in week *t*, with *t* ranging from −10 (10 weeks before George Floyd's death) to 10 (10 weeks after). *Protestst* 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*−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-protest 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 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.

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% point increase in the fraction of the population that goes out to protest raises the Democratic vote share in that county by 0.33% 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 the population of a county. In counties with at least one protest, Table 2 shows that the average attendance corresponds to 0.5% of the population. Our estimates thus translate into a 1.2 to 1.8% 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 that the 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.

Table 2 Main results

	Model 1	Model 2	Model 3
Attendees/Population	0.033*** (0.006)	0.024*** (0.006)	0.027*** (0.004)
Rain prob.	−0.027** (0.012)	−0.025** (0.012)	−0.021** (0.009)
λ	2.060*** (0.482)	2.938*** (0.482)	2.919*** (0.438)
ρ	5.646*** (1.134)	5.112*** (1.134)	5.255*** (0.748)
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 et al. 2013b). *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

The spatial parameters λ and ρ are highly statistically significant, showing the pres- ence of large spatial dependencies. These results demonstrate the importance of account- ing 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 sam- ple window selection, alternative spatial structures, alternative protest measures, alterna- tive weather instruments, ignoring spatial autocorrelation, adding state fixed effects, and weighing counties by population size.[7]

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 areas with relatively small fractions of African Ameri- cans. Although this result might appear counterintuitive, it is important to note that close to 90% of African Americans already support the Democratic party, leaving little room to move opinions in a progressive direction (Gramlich 2020). Moreover, some sources sug- gest 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

[7] The effect sizes are slightly smaller when we add state fixed effects (Table A4), although they remain statistically significant. The reason for the smaller effect sizes is that the analysis restricts identification to within-state variation in protest activity, effectively discarding between-state differences in average protest activity.

Table 3 Heterogeneous treatment effects

	Below median	Above median
Panel A: Fraction African Americans		
Attendees/Population	0.041*** (0.006)	0.019*** (0.005)
Rain prob.	−0.044*** (0.014)	0.0003 (0.013)
λ	4.195*** (0.525)	1.056*** (0.246)
ρ	3.190*** (0.587)	5.258*** (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*** (0.006)	0.013*** (0.004)
Rain prob.	−0.028** (0.013)	−0.034*** (0.011)
λ	4.931*** (0.647)	1.184*** (0.274)
ρ	3.061*** (0.495)	5.778*** (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*** (0.008)	0.015*** (0.003)
Rain prob.	−0.054*** (0.014)	0.005 (0.011)
λ	4.723*** (0.659)	1.030*** (0.215)
ρ	4.757*** (1.206)	7.387*** (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

tend to be stronger (Wellman and Wortley 1990), which makes fellow residents who engage in protests an even more salient and informative event (Lohmann 1994b; Bursztyn et al. 2021).

More general, it is important to consider that our methodology identifies a local av- erage 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 push 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).

Mechanisms

The previous analysis shows that BLM protests caused an increase in 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.

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 mes- saging (S. D. Ansolabehere, Iyengar, and Simon 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 a small but distinct effect on voter turnout (Goldstein and Freedman 2002; Stevens et al. 2008; Barton, Castillo, and Petrie 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, Kaufman, and Sands 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. How- ever, by looking at voter registration, they cannot separate political conversion, where voters who would have registered anyway register with a different party, from pure mo- bilization, where voters who would not otherwise have registered do so because of the riot. Directly related to our paper, (Holbein and Hassell 2023) document an increase in voter registration across the board following the 2020

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*** (0.281)	3.792*** (0.286)	4.069*** (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	−0.533***	−0.609***	−0.652***
	(0.203)	(0.134)	(0.130)
Rain prob.	0.242	−0.257	−0.246
	(0.360)	(0.314)	(0.313)
λ	0.004	−0.087*	−0.092**
	(0.057)	(0.045)	(0.044)
ρ	1.991**	0.159	0.114
	(0.861)	(0.841)	(0.846)
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*** (0.219)	0.552*** (0.142)	0.602***(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

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.

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 dis- crimination may explain why voters have swayed voters towards the Democratic Party, because the Democratic party purports to champion minority rights and advocates poli- cies 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 dis- advantage (Schaffner, S. Ansolabehere, and Luks 2021b). 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 fa- vors*" and "*Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class*".[8]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 of 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 a decrease in the share of those who claim that black people should work their way up without favors (all $p < 0.008$), and an increase in the share of people who think that slavery caused the disadvantaged position of African Americans today (all $p < 0.003$). In other words, BLM protests appear to have achieved their goal of changing people's attitudes about discrimination against African Americans, which, in turn, may have changed people's ballot box decisions.

IV Assumptions

For our instrumental variable method to be valid, rainfall should have a discouraging ef- fect 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.[9]

The second assumption is the exclusion restriction, which holds that rainfall during the protest window should only affect voting outcomes through its effect on BLM protests. This assumption is controversial because precipitation likely affects non-

[8] 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.

[9] 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.

protest variables such as crime and mood that might affect voting behavior (Mellon 2021). To test this assumption, we first examine whether rainfall between 26 May 2020 and 7 June 2020 is 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 Democratic vote shares. This is consistent with our main results, as precipitation reduces protest activity, which in turn reduces votes going to the Democratic 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 Democratic 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.[10] 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 Sect. 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 present at all. Hence, we do not believe that our results are driven by a direct effect of rainfall on media coverage.

[10] Table A8 shows the regression tables.

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 Sect. 3.1. This approach poses three method-ological advantages compared to the previous analysis. First, we now estimate the average treat- ment effect on the treated (ATET) instead of the local average treat-ment 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 sub-stantial, we can study the evolu- tion of the effect of protests over time.

We start with a replication of our IV analysis using a difference-in-differences method- ology. Table 5 gives the regression results for the effect of protests on the 2020 presiden- tial election. Similar to our main results, we find that BLM protests lead to a leftward shift in voting patterns. The table shows that the presence of at least one BLM protest causes a 5.9% point increase in a county's Democratic vote share in the 2020 election as compared to earlier elections in the same county. These results are qualitatively and quantitatively similar to our IV estimates, albeit slightly larger in magnitude. Figure A7 shows the corresponding event study plots. Although there are slight trend deviations during the pre-treatment period, the post-treatment jump is several times larger than the largest pre-treatment violation. Hence, we conclude that the observed effect is robust to different assumptions about diverging pre-trends (Rambachan and Roth 2023).[11]

In our next step, we use survey data from the Gallup Covid Panel to examine the evo- lution 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 a 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 caused a significant *in- crease* in Democratic identification in protest counties.

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

	Democratic vote share
Protests (yes/no)	0.059***
	(0.002)
County fixed effects	Yes
Election fixed effects	Yes
Observations	18,318
Adjusted R-squared	0.096

Notes: The table shows the difference-in-difference estimates for the effect of BLM protests on the Demo- cratic vote share in presidential elections. County-level election data are from 2000 to 2020. *Protests (yes/no)* is an indicator variable that takes the value of 1 if at least one protest took place in a county. All other definitions are as before

[11] We cannot formally estimate the honest difference-in-differences approach of (Rambachan and Roth 2023) because their methodology requires at least one post-treatment period after treatment onset.

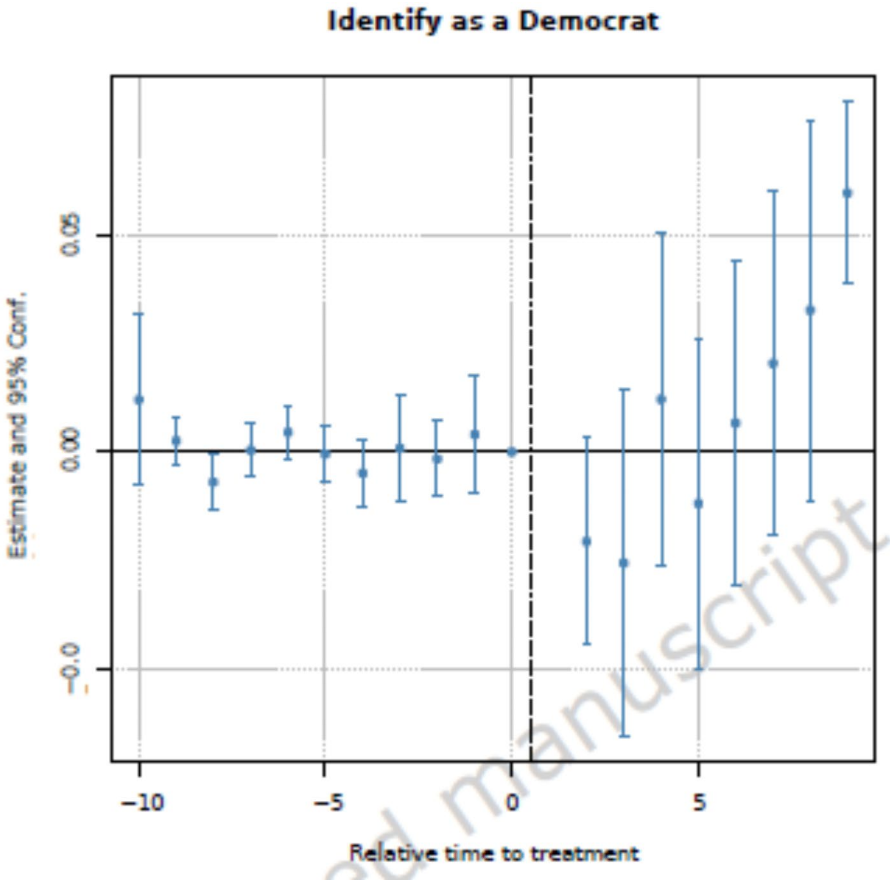

Fig. 1 Evolution of effect of BLM protests on po- litical orientation. The figure shows the dynamic difference-in-difference estimates for the effect of BLM protests on the likelihood that people identify as a Democrat. Relative time to treatment mea- sures the number of weeks to the main BLM protest window. Data are from the Gallup Covid Panel survey

These results highlight the importance of considering the time dimension when eval- uating 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 portrayals of violence or disruption. Over time, however, these initial emotional responses 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 Democratic vote shares in the 2020 election.

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. Us- ing both instrumental variable and difference-in-differences approaches, we document a significant increase in Democratic vote shares as the result of BLM protests. Some of these effects likely only pertain to peaceful protests. At the same time, however, we pro- vide 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 Democratic 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 sig- nals their perceived grievances with racial injustices, as well as their intention to vote Democrat. Through imitation and conversion, protest participation can consequently cre- ate 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 2020b).

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 con- clusions, our findings certainly offer encouragement for marginalized groups to organize and participate in collective action.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s11109-025-10014-w.

Declarations

Competing Interests The authors declare no competing interests.

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