Late Adoption and Collective Action: Social Media Expansion and the Diffusion of Black Lives Matter

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Abstract

This paper explores the impact of social media expansion in its later stages on collective action, focusing on Black Lives Matter (BLM) protests in 2020. Using data from over 100 million tweets and leveraging plausibly exogenous variation in super spreading events, we show that pandemic exposure increased social media adoption in predominantly white, rural, and Republican-leaning counties. "Late adopters" played a crucial role in spreading online and offline BLM protests to new areas, mobilizing more effectively than existing users. Our evidence suggests a shift in preferences among late adopters, beyond merely reducing coordination costs.

Keywords: social media, Twitter, BLM, protest, COVID-19
JEL classification: P16, D7

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1 Introduction

The trade-off between expanding reach and preserving unity stands at the core of many collective action problems (Ostrom, 1990; De Mesquita, 2010; Della Porta and Diani, 2015; Barbera and Jackson, 2019). Social media has emerged as a tool for outreach and coordination of protest, particularly in the early stages of its roll-out (Zhuravskaya et al., 2020; Manacorda and Tesei, 2020; Enikolopov et al., 2020; Qin et al., 2021). As social media platforms attract more users, the impact of late adopters on protest mobilization is ambiguous.

First, late adopters may be selected along dimensions that favor or hinder protest compared to early adopters. Second, late adopters face a different social media environment upon entry. This may curb mobilization if newcomers hold opposing views, are less engaged, or if a larger and more diverse network leads to information pollution. Conversely, late adoption may boost protest mobilization if existing networks facilitate learning about and joining a movement or alter newcomers' preferences more effectively.

In the absence of exogenous variation in late adoption, this question has remained largely unanswered in the literature. In this paper, we exploit the COVID-19 pandemic as a shock to social media adoption in an already saturated market and investigate whether this can explain the unprecedented scope of protests for Black Lives Matter (BLM) in the spring of 2020. Late adopters are defined as those users that have created their Twitter account during the pandemic.[1] We build a novel push-pull instrument for late adoption, exploiting quasi random variation in pandemic exposure (push factor) through so-called super spreading events (SSEs) and combining this with plausibly exogenous variation in the pre-pandemic size of the local Twitter network (pull factor).[2] Throughout, we focus on social media adoption at the extensive margin (i.e. new users) and locations with a low ex-ante propensity to protest (i.e. counties with no prior BLM protest).

The unique context of BLM during the pandemic offers two important features that allow us to investigate this question. First, the pandemic was associated with record growth of new users and online activity on social media in a context where these platforms had been established and well known for over 15 years. In the first months of the pandemic, Twitter reported a surge in daily active users by 24% which "was driven by an increased engagement due to the COVID-19 pandemic" (Twitter, 2020). The pandemic affected social media adoption unevenly – in geographic, demographic and socioeconomic dimensions. We think of these new users as never-takers in the absence of pandemic exposure.

Second, the BLM movement was conceived on Twitter in 2013, quickly becoming one of the most popular hashtags on the platform (McKersie, 2021). At the same time, BLM experienced an unexpected and viral protest trigger in the midst of the pandemic. Videos about the murder of

[1] We use the terms new users and late adopters interchangeably. They are defined as those Twitter accounts that have been created after the outbreak of the pandemic in the US and before the murder of George Floyd between January 21st 2020 and May 24th 2020. We also refer to the social media platform X with its previous name "Twitter" and use the terminology "tweets" and "tweeting" for convenience.

[2] The pull component of the instrument is based on Müller and Schwarz (2023) who use a popular tech and music festival in 2007 which heavily promoted Twitter when it launched. We also show that the instrument developed by Müller and Schwarz (2023) does not deliver a sufficiently strong first stage for the number of new users, supporting the notion that drivers of social media adoption in the early stages of its roll-out do not perform well for late adoption in saturated markets.

George Floyd at the hands of police officer Derek Chauvin on May 25th 2020 were watched over 1.4 billion times on Twitter and the hashtag #BlackLivesMatter surged to 8.8 million mentions per day (PEW, 2020). Consequently, new users that may have joined the platform for other reasons were inevitably exposed to this viral protest trigger.

We approach the empirical analysis in two steps. In the first part of the paper, we focus on the "push" side of the instrument, showing that so-called super spreading events (SSEs) increase social media adoption. Specifically, we count the number of SSEs within a 50 km radius from the county border (but not within the county) until the mid-April 2020, when there was little information about the spread of the virus among policy-makers and the local population alike. We provide various pieces of evidence that support the plausibility of conditional quasi random exposure. SSEs are not correlated with baseline social media penetration, prior BLM protest, Google searches for BLM related topics in the past, differences in mortality rates across race, lockdown stringency and an array of other economic, demographic, educational or social capital related county characteristics.

To assess the impact of pandemic exposure on social media adoption, we use different measures as well as their first principle component: *i*) from a random sample of geo-located tweets containing the most common 100 English words, we identify the creation date of the Twitter profile and count the number of new users at the county level that joined the platform between January and May of 2020 (versus those that were already on the platform in December 2019); *ii*) we do the same with a sample of Twitter users that end up tweeting about BLM after the murder of George Floyd; and *iii*) Google searches for Twitter in the month leading up to the protests, capturing the overall interest in the platform.

In our analysis, SSEs act as a proxy for pandemic exposure. We show that SSEs predict local COVID-19 deaths and cases. However, they may also increase the salience of the pandemic beyond the local death toll. For instance, and in line with Campante et al. (2024), they may trigger fear of infection, prompt individuals to stay at home and substitute real-life interactions with virtual ones or seek information about the pandemic online. Using Google mobile phone mobility data, we show evidence that SSEs increase the time spent at home rather than at work, in transit, for leisure or commerce, suggesting a substantial reduction in offline activities compared to the previous year. Our estimates suggest that a two standard deviation increase in pandemic exposure (15.1 SSEs) moves a county from the 25th percentile to the 50th percentile of social media use or equivalently increases the number of new Twitter accounts by 12%.

Pandemic exposure affects Twitter adoption unevenly: counties with a higher white population share and larger support for Trump in the presidential election of 2016, as well as more rural counties disproportionately adopt Twitter in response to SSEs. Counties with an older population are also more likely to take-up social media, potentially because younger, tech-savvy individuals had already adopted Twitter by 2020. We also show that the effect of SSEs is magnified in settings with a larger baseline Twitter network.

Turning to the direct effect of pandemic exposure on protest, we show that a one standard deviation increase in SSEs increases the likelihood of observing a BLM protest by 2.3 percentage points. SSEs also boost online BLM protest as measured by the number of tweets that mention BLM and the number of users that follow the official BLM Twitter account. We also show that a rise in the salience of racial inequality, lower opportunity costs of protesting, or a scattering rather than a diffusion of protest cannot explain our results. Nevertheless, pandemic exposure may have affected BLM protest beyond its impact on new users. Therefore, in the second part of the paper, we strengthen the link between late adoption and BLM protest.

We introduce the push-pull instrument for pandemic Twitter adoption. We build on an observation from the first part which documents that pandemic-induced social media adoption was particularly effective in settings where the baseline network was large enough to attract new users to the platform. This is in line with the literature on path-dependence in technology adoption and the nascent literature about social media as "collective traps", which posit that the marginal return of joining a platform increases with its size (Arthur, 1989; Bursztyn et al., 2023). We create a push-pull type instrument that combines exogenous variation in baseline Twitter penetration (pull factor), drawing from the instrument by Müller and Schwarz (2023), with pandemic exposure through SSEs (push factor) to predict the number of new Twitter users. Again, we provide an array of exercises – akin to those above – that validate the plausibility of the exclusion restriction. We complement this identification strategy with an event study design that compares BLM protest in response to the murder of George Floyd for counties with different levels of pandemic Twitter adoption.

Our estimates confirm that social media adoption during the pandemic (i.e. new Twitter users) mobilized subsequent offline and online BLM protest: a 1 percent increase in the number of late adopters increases the likelihood of experiencing a BLM protest by 0.24 percentage points and increases BLM-related tweets by 10% relative to the mean.[3] These findings also suggest that online and offline protest are complements in our context, which stands in contrast to the literature on "slacktivism" that highlights the shift from more effective forms of collective action towards more symbolic activism online (Christensen, 2011; Schumann and Klein, 2015). We also find evidence that late adopters contribute more to the diffusion of BLM than existing users, i.e. those users that had entered the platform by December 2019.

There are multiple, mutually non-exclusive explanations for our findings. Larger networks may always reduce coordination costs because ideological sorting and echo chambers prevent information pollution. In addition, established online communities can reduce coordination costs by offering extensive information on BLM narratives and logistics to late adopters. Larger networks also provide a stronger signal of others' preferences, particularly when posts about George Floyd are shared and liked by many people. Additionally, this visibility can persuade new users to update their beliefs about BLM. While it's challenging to isolate these mechanisms, we provide evidence that both persuasion and reduced coordination costs contribute to our findings.

We begin by investigating the possibility that the expansion of social media may have altered preferences. Specifically, we leverage individual-level survey data to investigate the effects of social media adoption on political preferences in the short and medium run. We exploit the fact that older respondents are more likely to take up social media since they are less likely to have already used it at baseline. We interact our push-pull instrument with the age of the survey respondent to predict social media use at the individual-level. We include county fixed effects,

[3] In the context of social media use and protest in Russia, Enikolopov et al. (2020) find that a 10 percent increase in VK users (Russian equivalent to Facebook) increases the likelihood of protest by 4.6 percentage points.

effectively comparing differences in attitudes between older and younger respondents in counties that were more or less exposed to the push-pull shock. Using data from the Cooperative Election Study (CES), we find evidence that social media use increases the likelihood of having attended a protest in the past year, becoming politically active on social media, voting for Biden in the 2020 presidential election and holding more progressive views towards the police and racism. Using data from the PEW American Trends panel, we also find that survey respondents in counties with higher pandemic exposure consume more news about George Floyd on social media and hold more favorable views towards the BLM movement but not other progressive issues.

Next, we verify whether this mobilization effect is limited to collective action in support of BLM. This allows us to assess whether persuasion is universal or whether social media can still reduce coordination costs - even for new users that oppose the majority view on the platform.[4] We find an effect on counter-mobilization in the form of tweets for "All Lives Matter" and "Blue Lives Matter", which are both rallying cries in opposition to BLM. Using data from the ACLED US Crisis Monitor, we show that social media adoption intensifies other protests, including anti-mask and anti-social distancing protest. The effects on protests organized by QAnon or "Proud Boys", as well as other groups that support Trump (including those related to perceived election fraud) are comparatively small and noisily estimated, indicating that new Twitter users did not mobilize protest at the political fringes.

Overall, our results suggest that the expansion of social media in later stages affects the dynamics of collective action in meaningful ways. Late adopters are selected along political, economic and demographic dimensions and they contribute to protest diffusion, potentially even to a greater degree than early adopters. Our evidence is consistent with the notion that late adoption increases the ideological and demographic diversity of social media platforms but that this diversity does not hamper protest mobilization. Instead, new users mobilize protest across the ideological spectrum, potentially because they rapidly segregate into their respective echo chambers and energize a new wave of protesters. In contrast to existing studies on social media adoption and protest, we also find that late adoption alters preferences, suggesting that the dominance of social movements on these platforms can shape attitudes of newcomers more effectively than in setting where the social media landscape has not yet consolidated.

This study contributes to several strands in the literature. To our knowledge, it is the first to establish a causal relationship between pandemic exposure and social media adoption. Existing research centers on supply-side constraints and initial staggered roll-outs (Manacorda and Tesei, 2020; Enikolopov et al., 2020; Müller and Schwarz, 2021; Melnikov, 2021). In contrast, we focus on isolating exogenous variation in late adoption.

Second, we contribute to the literature on the political effects of social media (see Aridor et al. (forthcoming) for a succinct overview of the literature). There is a rich literature examining the effect of technology adoption on protest (Campante et al., 2018; Christensen and Garfias, 2018; Guriev et al., 2021; Enikolopov et al., 2020; Manacorda and Tesei, 2020; Boyer et al., 2020),

[4] The platform we analyze is Twitter, the users of which used to be younger and more likely to be democrats than the general US population, at least up to 2019 (Wojcik and Hughes, 2019). Even thought we cannot provide data at the individual level, we show that new adopters are located in counties with higher share of people over 25 years old and higher Republican vote share. We take this as a strong suggestive evidence that the new users are more likely to also be older and more at the right politically.

xenophobia, polarization, political preferences, social capital, and network formation (Falck et al., 2014; Boxell et al., 2017; Enikolopov et al., 2018, 2020; Guriev et al., 2021; Melnikov, 2021; Fujiwara et al., 2023; Müller and Schwarz, 2023, 2021; Campante et al., 2022; Boken et al., 2023; Enikolopov et al., 2024). Complementing this literature, we focus on newcomers to social media - the users that would have been never-takers in the absence of the pandemic - which can impact collective action in different ways. With the exception of Bursztyn et al. (2023), the literature has not yet investigated the timing of social media adoption and size of the social media network.

More generally, our analysis adds to a large literature that analyzes the determinants of social movements and protests, ranging from macro level drivers, such as local institutions or socio-economic conditions (Lipsky, 1968; Eisinger, 1973; McCarthy and Zald, 1977; Besley and Persson, 2011; Dube and Vargas, 2013; Berman et al., 2017), to micro level drivers, including individual decision making processes (Ellis and Fender, 2011; Guriev and Treisman, 2020; Sangnier and Zylberberg, 2017; Chenoweth et al., 2022; Cantoni et al., 2022) and different aspects of individual and social psychology (Guriev and Treisman, 2020; Sangnier and Zylberberg, 2017; Passarelli and Tabellini, 2017; González and Prem, 2020; Cantoni et al., 2019; Bursztyn et al., 2021), as well as the diffusion of social movements and protest across space and through networks (Becker et al., 2020; Casanueva, 2021; García-Jimeno et al., 2022). More narrowly, we add to the nascent literature on the causes and consequences of the Black Lives Matter movement and prominent police killings (Mazumder, 2018; Dave et al., 2020; García and Ortega, 2022; Chenoweth et al., 2022; Ba et al., 2023; Celislami et al., 2023; Gethin and Pons, 2024).

The remainder of the paper is organized as follows. In section 2, we provide a background on the BLM movement, present motivating evidence and describe our main data sources. In section 3, we detail our empirical strategy and first set of results on pandemic exposure and social media adoption. Then, in section 4, we focus on new Twitter users and BLM protest. We shed light on the role of persuasion versus coordination in section 5. Section 6 concludes.

2 Background and data

2.1 BLM history and motivating evidence

The Black Lives Matter (BLM) movement emerged on social media after the acquittal of George Zimmerman in the deadly shooting of a Black teenager named Trayvon Martin. The movement was founded by three Black activists, Alicia Garza, Patrisse Cullors, and Opal Tometi in July of 2013 with the aim to end systemic racism, abolish white supremacy and state-sanctioned violence (Black Lives Matter, 2020), and more generally, to "fundamentally shape whites' attitudes toward Blacks" (Mazumder, 2019). Over the following months, an ever-increasing but small number of activists coalesced under the hashtag #BlackLivesMatter on Twitter and Facebook. In August of 2014, after a court decision to not indict the responsible police officer in the fatal shooting of Michael Brown in Ferguson, #BLM became one of the most widely used hashtags on Twitter. The hashtag was used 1.7 million times in the three weeks following the court decision, compared to 5000 tweets in all of 2013, confirming its status as a mainstream social media phenomenon (Freelon et al., 2016; Anderson and Hitlin, 2016). In contrast to early adopters, new users encountered a social media environment with a large and established BLM network.

After the murder of George Floyd on May 25th, 2020, the BLM movement experienced an unprecedented expansion both geographically and demographically. Protesters took to the streets when a video of the murder of George Floyd went viral on social media, showing how police officer Derek Chauvin suffocated George Floyd using a choke-hold. The video spurred unrest in Minneapolis but the protests quickly expanded to other parts of the United States, including communities that had never engaged in BLM protests before. The number of BLM protests quadrupled in May and June of 2020, compared to previous peaks in 2016 (see Figure A1). The spike in protest activity and its coverage was not only the largest at the time but also the most extensive in the entire history of United States political protest, according to some observers (NYT, 2020, WP, 2020). The prominence of BLM on Twitter peaked after the murder of George Floyd, when #BlackLivesMatter became the most popular hashtag on Twitter, peaking at 8.8 million mentions per day and videos on Twitter about the murder of George Floyd at the hands of police officer Derek Chauvin were watched over 1.4 billion times in the two weeks after his death (PEW, 2020). Pro-BLM narratives attracted significantly more attention on social media than anti-BLM narratives (Dunivin et al., 2022).

Table A5 reports a few examples of tweets from our sample, illustrating how social media, especially Twitter, spread awareness and mobilized support for the Black Lives Matter movement after George Floyd's murder. The tweets reveal a diverse set of users, including suburban moms, educators, and self-ascribed "allies" from various backgrounds, engaging with the #BlackLivesMatter hashtag to express solidarity, share resources, and organize demonstrations in their communities. Many users emphasized the role of social media in exposing injustices and mobilizing action, with some noting the need to extend the conversation offline to educate older generations and affect change. They also highlight the recognition of white privilege and the commitment of allies to stand with the Black community against systemic racism. Furthermore, the examples illustrate how the movement gained traction in predominantly white suburbs, with reports of large turnouts at local rallies and car parades. These tweets provide a snapshot of how social media amplified the Black Lives Matter movement across different socio-demographic groups.

In Figure A2, we plot all counties that observed a BLM protest after the murder of George Floyd, splitting the sample into counties that had and did not have a BLM-related protest before the pandemic.[5] In line with the narrative in the tweet examples, the BLM movement broadened its base in 2020. The geographic distribution of protesting counties does not follow the typical coastal divides but is spread across all of the US. Most importantly, we find that counties with no prior BLM protest make up half of the counties that protest in response to the murder of George Floyd. Similarly, Figure A3 shows that counties that experience a BLM protest for the first time in 2020 have a substantially higher white population share compared to those that protested before.

[5]We use data from *Elephrame* on BLM events between 2014 and 2020 and describe this data set in more detail in the next section and in Appendix A.

2.2 Main data sources

In this section, we present the main data sources. We give a more detailed description of all variables, including exact definition, geographic unit, time frame and sources of all variables in Tables A1 to A4 of Appendix A.

Black Lives Matter. This data comes from the crowd-sourced platform Elephrame. It provides information on the place and date of each BLM protest and estimated number of participants, as well as a link to a news article covering the protest. We extracted and geo-located all protests from August 2014 to September 2020. These protests are decidedly pro Black Lives Matter. We add information on BLM and other protests from the US Crisis Monitor, a joint project between ACLED and the Bridging Divides Initiative (BDI) at Princeton University, that collects real-time data on different types of political violence and protests in the US from 2020 onward.

Twitter. We collect three types of Twitter data at different points in time (before the pandemic, during the pandemic but before the murder of Floyd and in the three weeks after the murder of Floyd). First, from the Twitter API we collect a random sample of tweets, using the 100 most common words in English. Second, we collect the universe of tweets with BLM related hashtags. This includes the hashtags #BlackLivesMatter, #BlackLifeMatters, #BLM, and, separately, the #AllLivesMatter, and #BlueLivesMatter hashtags (see Appendix Table A5 for some examples). Third, we scrape information on all followers of the official Black Lives Matter Twitter account. With the help of a geo-location algorithm, we can assign about 5 to 20% of Twitter users (depending on the sample) to counties. We show in Appendix Table A2 that the characteristics of counties for which we have geo-located tweets are indistinguishable to characteristics of the full sample. Importantly, we can identify the creation date of the Twitter profile. This allows us to assign "old users" (those that created their profile before January 20th 2020) and "new users" (those that created their profile between January 21st and May 25th 2020) to counties. Overall, this data allows us to measure *i*) online protest for and against BLM *ii*) late Twitter adoption and *iii*) information on baseline Twitter penetration.

Super spreading events (SSE). Information on SSEs are collected by a set of independent investigators and researchers from London School of Hygiene and Tropical Medicine (Leclerc et al., 2020). These are retrieved from scientific journals and news reports on SSEs, which are defined as "clusters" or "outbreaks" of COVID-19 infections with a minimum of 2 infections outside of the home.[6] For the whole period (January to August 2020), we identify a total of 1023 SSEs in the USA. Most commonly, events occur in nursing homes, prisons, factories, and retribution (correction facility) or medical centers. Table A3 provides descriptive statistics about each type of event. We describe the nature of these events in more detail in Section 3 and lay out the limitations of the SSE data set and how we address those in Appendix A.

[6] In our data set only 3 SSE are associated to less than 5 infections.

Additional data sources. We complement these three main sources with an array of additional data sets described in more detail in Appendix A. The set of control variables comes from the American Community Survey as well as the US Census Bureau. In addition, we use data on lockdown stringency, Google searches, mobile phone data on individual level mobility, as well as geo-localized data on George Floyd street art. We also exploit individual-level survey data from the Cooperative Election Study and the American Trends Panel, and election results for the 2012 and 2016 presidential elections from MIT Election Data and Science Lab (2018).

2.3 Descriptive statistics

We report detailed summary statistics in Table A4, distinguishing between counties with no BLM events before the pandemic and those with prior BLM events for comparison. As outlined above, we use information that is available at different points in time: *i*) three weeks after George Floyd's murder, *ii*) the day of the murder, *iii*) before the murder but after the pandemic started in January 2020, *iv*) later outcomes and *v*) baseline county characteristics before the outbreak of the pandemic.

Focusing on the sample of counties with no prior BLM protest, the average likelihood of observing a BLM-related protest at the county level between May 25th and June 14th lies at about 5%. There are on average 0.06 events per county in the three weeks following George Floyd's murder and the average number of participants is approximately 21 with a maximum of 5.5K participants.[7] Conditional on recording a BLM protest, the average number of participants is about 355. In the three weeks following George Floyd's murder we can identify over 180 tweets per county using BLM-related hashtags.

Counties that protest for the first time in 2020 have a lower Black population share (9% vs. 16%), are more rural (11% vs. 40% in large cities and suburbs) and have a higher vote share for Republicans in the 2016 presidential election (66% vs. 45%) compared to traditional protesters. The vast majority of counties where there was no history of protest for a BLM-related cause continue to not protest after the murder of George Floyd (2,636 counties, which is approximately 85% of all counties). However, we observe that among the sample of "no BLM event before" 132 counties start to protest for the first time during the pandemic. We also report summary statistics on the traditional protesters, i.e. counties that have had a prior BLM protest. Among those 339 traditional protesters, 163 counties did not protest after the murder of George Floyd and 176 counties continue to protest. As expected, the average probability of observing a protest in response to the murder of George Floyd is 10 times higher among traditional protesters compared to other counties. Remarkably, however, the first-time protesters make up more than 40 % of all counties that protested during the pandemic.

[7] The average sets the number of participants in places with no BLM protests as zero.

3 Pandemic exposure and social media adoption

3.1 Empirical strategy

3.1.1 Estimating equation

In this section, we show that the pandemic acted as a large enough push factor that led to the adoption of social media in an otherwise saturated social media market. Specifically, we investigate the effect of local pandemic exposure on late adoption at the county level. We leverage plausibly exogenous variation in the occurrence of super spreading events. As we will show, SSEs primarily contribute to the spread of the virus, as evidenced by an increase in the number of COVID-19 related deaths and cases. In addition, and in line with Campante et al. (2024), SSEs may also increase the salience of the pandemic beyond its direct impact on local COVID-19 deaths and cases. For instance, individuals may spend more time at home and substitute real world interactions with virtual ones, or they sign up to learn about the pandemic in real time, or they seek to express themselves or find connection in times of crises.

Our treatment Z_c is measured as the sum of all SSEs that occur within 50 km of the county border but not within the county until 6 weeks before the murder of George Floyd. In all specifications, we focus on the set of counties that never protested for BLM before and estimate the following regression:

$$Social\ Media_c = \beta_1 \boldsymbol{Z_c} + X_c\beta_X + \zeta_s + \epsilon_{cs} \tag{1}$$

$$Z_c = \sum_{w=1}^{t-6} \text{SSE}_{-csw}^{\leq 50km} \tag{2}$$

Our outcome of interest in specification $Social\ Media_c$ is an index of social media penetration that comprises the first principal component of multiple variables[8]: *i*) from a random sample of geo-located tweets containing the most common 100 English words, we identify the creation date of the Twitter profile and count the number of new users at the county level that joined the platform between January and May of 2020 (verus those that were already on the platform in December 2019); *ii*) we do the same with a sample of Twitter users that end up tweeting about BLM after the murder of George Floyd; and *iii*) Google searches for Twitter in the 5 weeks leading up to the protests, capturing the overall interest in the platform.

All specifications include state fixed effects ζ_s which capture unobserved characteristics at the state level that could be related to both the prevalence of SSEs and our outcomes of interest. These include, for instance, testing capacities, lockdown stringency, differences in state laws, or policing strategies, all of which are mandated at the state level. In addition, we include a vector of county level controls $\mathbf{X}_c$ that account for within-state heterogeneity. Specifically, we include variables that are associated with participation in the BLM movement, such as a dummy for urban counties and Black population share and the poverty rate among Blacks,

[8]In Appendix Table A6, we show the correlation between all measures, the eigenvalue of the first principal component and the factors loadings for each variable. Reassuringly, every variable is coded in the expected direction: the PC1 gets bigger as they get bigger.

as well as the use of deadly force by police (i.e. number of Black people who died during an encounter with the police, from 2014 to 2019 and in 2020 up to May 25th). We also control for underlying political and attitudinal factors and socioeconomic drivers of protest, such as the vote share for Republicans in the 2012 and 2016 presidential elections, median household income, unemployment rate, community resilience (an indicator developed by the United States Census Bureau that captures the capacity of county to absorb the health impacts of pandemics), and a proxy for social capital. We cluster standard errors at the state level but show in Appendix B that our results are robust to spatial clustering.

3.1.2 More details on super spreading events

Super-spreaders are individuals who are an order of magnitude more contagious than others. This phenomenon, well-known in epidemiology, is instrumental in infectious disease spread (e.g. Galvani and May (2005)) and of particular importance for COVID-19, where 70–80% of transmissions can be traced back to just 10–20% of cases (Adam et al., 2020; Endo et al., 2020; Miller et al., 2020). SSEs occur when a large number of people are infected at the same event. Our data set includes outbreaks and clusters with "two or more test-confirmed cases of COVID-19 among individuals associated with a specific non-residential setting with illness onset dates within a 14-day period" (see Appendix A for more details). The average SSE is associated with 130 cases. This is a large number since the average number of detected cases at the county level lies at 164 by the end of May 2020. The majority of the approximately 1000 SSEs in our data are recorded in the medical care sector (see Table A3). We exclude specific event locations in a robustness check and control for testing capacities with state fixed effects and the presence of health care facilities with the community resilience index in all specifications.

We leverage SSEs in the early stages of the pandemic, i.e. those that occurred between January and April 13th 2020, for two reasons. First, infections were sufficiently high to introduce a significant number of infected individuals. Second, lock-down measures were not yet stringent enough (in addition to the lack of public awareness) to restrict group gatherings and encourage mask-wearing. We illustrate in Figure A4 that the overwhelming majority of SSEs (solid blue line) occurred between the second week of March and the last week of April. The red dotted line of Figure A4 shows that the increase in the number of new COVID-19 cases coincided with the increase in SSEs.[9] The green dashed line illustrates that SSEs leveled off when state-issued stringency measures became more prevalent and binding (as measured by the stringency index from the Oxford COVID-19 Government Response Tracker). We argue that during this time window, the occurrence of SSEs was mainly driven by the presence of a highly infectious person, rather than heterogeneity in risk preferences or other underlying factors that could drive both SSEs and social media adoption. We only include SSEs until April 13th 2020 - 6 weeks prior to George Floyd's murder, to account for the fact that SSEs further into the pandemic may be more endogenous. As we will show in the robustness checks in Appendix B, expanding or narrowing the time window between 5 to 8 weeks before the murder of George Floyd does not substantially alter our results.

[9] In Figure A5, we illustrate this time period relative to the spread of COVID-19 (measured as the cumulative number of COVID-19 related deaths) and the surge in BLM protests.

In addition, we exploit the geographic dimension of exposure. Specifically, we use the number of SSEs within a 50 km (or approximately 30 mile) radius from the county border while excluding SSEs that occurred within the county.[10] This has two advantages. First, any unobserved factor that predicts our outcome of interest would also have to predict SSEs in nearby counties. Second, by including nearby events we can leverage more variation in pandemic exposure, which does not halt at county borders. Figure 1b illustrates the geographical distribution of SSEs across US counties. We depict the identifying variation, i.e. the number of SSEs in 50 km proximity to the county border up to April 13th. We show in Appendix B1 that our results are robust to changing the geographic range of SSEs and to weighting SSEs by their distance or their probability of occurrence (see Appendix B.1 for more details). In addition, our results are robust to controlling for SSEs within the county and excluding SSEs that occur in prisons.

We verify that SSEs are a relevant measure of pandemic exposure by estimating its effect on local COVID-19 related cases and deaths in columns 1 and 2 of Appendix Table A7. Our estimates suggest that one additional SSE adds one COVID-19 related case per 10,000 inhabitants and one COVID-19 related death per 130,000 inhabitants. However, we take these as lower bound estimates since SSEs could impact the salience of the pandemic above and beyond the local death toll. For instance, using Google mobility data from mobile phones, we show in column 3, that SSEs increase the time spent at home in the month leading up to the murder of George Floyd compared to previous year. Let us caveat here that pandemic exposure affects a myriad of outcomes above and beyond social media adoption that may drive BLM protest. We assess the degree to which other competing pandemic-related drivers can explain the surge in protest at the end of this section. In addition, we dedicate the entirety of section 4 to identify the direct effect of late adopters on BLM protest, combining insights from this section with those in the existing literature.

3.1.3 Plausibility of quasi random exposure

A key empirical challenge in ascertaining the causal effect of SSEs on the use of social media is that both could be driven by unobserved factors. For instance, tight-knit and socially active communities may both increase the spread of the virus and use social media more actively. We start by using an array of county level characteristics to predict our treatment. In panel a of Figure 2, we predict the number of SSEs in close proximity with a number of local characteristics to assess whether there is any systematic correlation between our instrument and the local socio-economic, demographic or social media characteristics. We include the baseline set of controls but do not include state fixed effects in this regression in order to investigate potentially important correlates which are only available at the state level, such as lockdown stringency and the disproportionate death burden on the Black population. Coefficients are standardized for comparability.

First, we assess whether social media penetration before the outbreak of the pandemic predicts SSEs in close proximity. It is possible that SSEs are more likely to be detected and reported in settings with high social media use, which would indicate that SSEs capture how digitally

[10]We illustrate the construction of our instrument in Figure A6 using the example of Arizona.

connected counties are in the first place rather than exposure to the pandemic. We test this by looking at a random sample of geo-located Twitter users in December of 2019 as well as the sample of Twitter users that are early adopters, i.e. those profiles that joined the platform during one of the largest Twitter promotion campaigns in 2007 at the South by South West (SXSW) festival. We see no evidence for a systematic relationship between social media use at baseline and SSEs.

Next, we probe the exogeneity of SSEs with respect to BLM protest. We look at the geographic distance to Minneapolis, the location of George Floyd's murder and the origin of BLM protest in 2020. If SSEs are spatially correlated with distance to Minneapolis, we may capture geographic diffusion patterns rather than pandemic exposure. In addition, we look at various measures for exposure to previous BLM protest and BLM related topics, using a dummy variable for whether a county's neighbor had a BLM protest between 2014 and 2019 or whether a county's neighbor protested for BLM in 2020, as well as Google searches for the term BLM and Black Lives Matter in the weeks leading up to the murder of George Floyd. All coefficients are small in magnitude and insignificant with the exception of the small and marginally significant coefficient for neighboring BLM protest between 2014 and 2019. We also address the concern that SSEs predict the severity of lockdown stringency and therefore the opportunity cost of protesting. While this variable would be captured in the state fixed effects, restrictions on mobility may be relevant across state lines. Moreover, if SSEs disproportionately affect the Black community, then we may capture an unobserved factor that either riles up or incapacitates prospective protesters. Reassuringly, we do not find that lockdown stringency or the death burden on Blacks predicts SSEs.

We continue this exercise with a battery of potentially relevant county characteristics, such as economic inequality and poverty all measured at baseline in 2018 from the American Community Survey (Gini, poverty rate, female poverty rate), education (Bachelor degree, some college education share), demographics (female above 15, population aged 15 to 25 and population above 65), economic variables (average rent and job density) as well as social capital variables (various religious and sports organizations etc.). Reassuringly, we find no systematic pattern between these variables and SSEs. We provide additional robustness checks that probe the plausibility of the exclusion restriction and validity of our instrument in section 3.2.3 below.

3.2 Main results

3.2.1 Social media adoption after the outbreak of the pandemic

We investigate the effect of pandemic exposure on social media adoption in Table 1. We present our index of social media adoption in column 1. It is the first principal component of the outcomes in columns 2 to 6. These include the (log) number of new Twitter accounts based on a random sample of tweets, the Google search intensity for the term Twitter, as well the (log) number of new Twitter accounts that end up mentioning BLM. Throughout, we find that pandemic exposure increases social media adoption. Our estimate in column 1 suggests that a two standard deviation increase in pandemic exposure (15.1 SSEs) moves a county from the 25th percentile to the 50th percentile of social media use.

In columns 2 to 6, we focus on the sub-components of the index. We find that pandemic exposure increased the raw number of new Twitter accounts (column 2) and the log number of new Twitter accounts (column 3). The magnitude is large: the coefficient of column 3 suggests that a one standard deviation increase in pandemic exposure led to a 6% increase in the number of new Twitter accounts. In column 4, we find that pandemic exposure significantly increased Google searches for Twitter. We take this as proxy for the overall interest in platform.

Next, we focus on the new Twitter users that tweet about BLM after the murder of George Floyd. While the random sample of Twitters users sheds light on overall Twitter take-up - irrespective of whether they end up mobilizing for BLM - this measure allows us to zoom in on the universe of users that engage with the BLM hashtag after the protest trigger. Columns 5 and 6 reveal that pandemic exposure increases the number of new users that express views on BLM in the three weeks following the murder of George Floyd.[11]

Using high frequency mobility data from Google, we show that the average time spent at home in the week leading up to George Floyd's murder increases with pandemic exposure.[12] This indicates that individuals in exposed counties substantially reduced the time spent outside of their home, which may indicate a higher propensity to spend time online.

In a last step, we examine whether the *timing* of social media adoption coincides with occurrence of SSEs in neighboring counties. This allows us to verify that social media adoption did not precede pandemic exposure but rather responds to it. We run a naive event-study design with county and day fixed effects, where the treatment is a dummy variable that switches on after the first SSE in a neighboring county within 50 km of the county border. We cluster standard errors at the county level. Figure 3 illustrates our results, presenting point estimates for each day relative to event time and displaying 90% confidence intervals. We find no evidence for an increase in Twitter adoption in the 42 days leading up to the first SSE. Instead, the number of new Twitter users gradually increases afterwards, emphasizing the relevance of SSEs for social media adoption. In addition, this exercise reveals that the timing of pandemic exposure matters, which alleviates concerns about omitted time-invariant factors that drive SSEs and Twitter adoption.

3.2.2 Heterogeneity in social media adoption

We investigate the characteristics of counties that experience an increase in social media adoption in response to pandemic exposure. We consider the full sample of counties since we are interested in the features of late adopters relative to all counties, including those with a BLM protest history. In Table 2, we estimate the effect of SSEs on new Twitter users, splitting the sample into above and below within state median of various demographic and political county characteristics. Demographic characteristics are from the 2018 American Community Survey, while election results are from MIT Election Data and Science Lab (2018).

[11] In Appendix Table A8, we use SSEs to instrument for local COVID-19 deaths instead of estimating the direct effect of SSEs on social media adoption. As mentioned before, these estimates likely underestimate the effect of pandemic exposure since SSEs impact the salience of the pandemic beyond the local death toll. Our 2SLS estimates are consistent with the reduced form estimates and imply that a one standard deviation increase in pandemic exposure (23 COVID-19 related deaths per 100K inhabitants) would move a county from the 30th to the 70th percentile of social media usage.

[12] In Appendix Figure A7, we show the evolution of time spent at home (=residential stay), compared to time spent in transit, grocery shopping, work, retail and parks between March first and May 24th, compared to the same date in the previous year.

In columns 1 and 2, we focus on the share of whites in the population. The coefficient is larger (albeit imprecisely estimated) for counties with a higher share of whites. Turning to the vote share for Republicans in the 2016 presidential election, we find that take-up is higher in counties that favored Trump. Columns 5 and 6 reveal that social media adoption was more pronounced in counties with a lower population density. Lastly, we split the sample into counties with an above and below share of individuals below the age of 25. We find that counties that adopt social media in response to pandemic exposure tend to be older, suggesting that younger and tech-savvy individuals had already adopted social media before the pandemic. We will leverage this finding in section 5, when we look at individual level social media adoption.

Overall, our findings suggest that the pandemic affected social media adoption unevenly along socio-economic, demographic and political lines and that these differ substantially from those counties that traditionally protested for a BLM related cause. While we cannot assess the political leaning of individual users, these results suggest that new users have shifted ideological distance to traditional BLM protester - at least in terms of the characteristics in their county of residence.

In a next step,we focus on differences in baseline Twitter use. Figure 4 illustrates the distribution of Twitter penetration at baseline, i.e. the log number of Twitter users in December of 2019 (top panel) and Twitter adoption during the pandemic (bottom panel) for each of the following sub-groups: counties with no previous BLM protest and no protest after Floyd's murder, counties with no previous BLM protest but with protest after Floyd's murder, and counties with previous BLM protest. Importantly, the counties that protest for the first time after Floyd's murder are those with higher levels of Twitter penetration at baseline and higher take-up of Twitter during the pandemic relative to other counties that never protested before. This suggests that the baseline Twitter network facilitates subsequent Twitter adoption. In Appendix Table A7 column 5, we test this hypothesis more systematically by interacting SSEs with the baseline Twitter network and find that the interaction strongly predicts Twitter take-up. We leverage this observation in the second part of the paper, when we dive deeper into the heterogeneity of Twitter adoption, focusing on SSEs as a push factor and the baseline Twitter network as a pull factor for social media adoption.

3.2.3 Summary of robustness checks

In order to provide additional pieces of evidence for the plausibility of quasi random exposure and to validate the robustness of our instrument and main results, we run a series of empirical exercises in Tables B1 to B6 which we describe briefly here and in more detail in Appendix B. We always present baseline results in column 1 and focus on Panel A, which replicates the specification used in column 1 of Table 1.

We begin by probing the sensitivity of our instrument with respect to geographic distance and time lag in Table B1. Columns 2 to 4, narrow and expand the distance to SSEs from 25 km to 50 km and 100 km. In columns 5 to 7, we probe the robustness with respect to the time window considered, moving closer to and farther away from the murder of George Floyd (5 weeks, 7 weeks and 8 weeks instead of 6 weeks before the murder). The results remain robust, precisely estimated and similar in magnitude.

In a second set of exercises in Appendix Table B2, we exclude SSEs in prisons as they may impact the public perception of exposure to the pandemic differently and may also be related to factors that drive BLM protests. Next, in column 3, we also include the number of SSEs in the own county to account for correlation between neighboring and own SSEs. In column 4, we control for whether a neighboring county had a BLM protest between 2014 and 2019. In column 5, we weight SSEs both by their geographic distance to the county border. Lastly, in column 6 we weight each observation (i.e. each county) by their inverse probability of being treated by the instrument, using LASSO.[13] In doing so, we give more weight to counties that had a low a priori likelihood of having a SSEs in close proximity based on observables. Essentially, this captures the most surprising occurrences of SSEs and therefore further improves on the plausibility of the exclusion restriction.

We probe the robustness of our main results in Appendix Tables B5 and B6. The top panel corresponds to our main reduced form specification 1, using SSEs to predict the social media index. In columns 2 to 9 of Appendix Table B5, we consider the robustness of our main result with respect to changes in the sample composition and the definition of the treatment variable. In columns 2 and 3, we exclude counties and whole states on the coasts and our results hold. We do this for two reasons: first, counties and states next to the ocean will mechanically have fewer neighboring counties with SSEs. Second, we want to verify that our results extend to counties in states with low ex-ante exposure to social media and BLM protest. Column 4 and 5 include, as an additional control, the number of COVID-19 cases (resp. deaths) in the past seven days. In doing so, we account for heterogeneity in the trajectory of the COVID-19 pandemic when the cumulative number of SSEs in neighboring counties over the whole period are similar. Columns 6 to 9 take spatial correlation into account, considering 50 km, 100km, 150km and neighboring counties as distance thresholds, addressing any concerns related to spatially correlated SSEs and social media take-up. Finally, in Table B6, we change the estimation method to a probit and we check that the results hold when controlling with ex-ante probability of protesting.[14] All of these checks yield consistent results.

3.3 Additional results

3.3.1 Super spreading events (SSEs) and BLM protest

In Appendix Table A9, we look at the direct effect of pandemic exposure on different measures for online and offline BLM protest in the three weeks after the murder of George Floyd.[15] The estimate of column 1 suggests that a one standard deviation increase in pandemic exposure (7.6 SSEs) increases the probability of observing a BLM protest by 2.3 percentage points. This is a large effect considering that the overall protest probability lies at 4.8%. In column 2, we look at the intensive margin, i.e. the number of BLM protest in the three weeks following the murder of George Floyd, and find that BLM offline protests also increase in frequency. Focusing on the

[13]We describe this approach in more detail in Appendix B.

[14]See more details in section 4.1.3 and Appendix B.

[15]Our preferred time window for the protest outcomes is three weeks because it allows us to capture a large share of the protest behavior (66 percent of BLM protests following George Floyd's murder can be observed in this three week window) while limiting the potential for confounding factors. We show in Table B4 that shortening or expanding this time frame yields consistent, and for longer time windows even stronger, results.

scope of the protests in column 3, we find a negative but non significant and noisy estimate for the effect of COVID-19 on the total number of BLM protesters.

Next, we investigate the impact of pandemic exposure on online protest, specifically BLM-related content on Twitter. In column 4, we report as an outcome the total number of geo-localized BLM-related tweets in a county in the three weeks following George Floyd's murder. These are based on the universe of tweets that use the hashtags #BlackLivesMatter #BlackLifeMatters, #BLM or #GeorgeFloyd, or their non-hashtag equivalent (e.g. "Black Lives Matter"). We end up with a total of 2.2 million Tweets that we aggregate at the county level. We find a positive but noisily estimated effect for pandemic exposure on the number of BLM tweets. These tweets could however be either in favor or against the movement. In order to proxy online support for the BLM movement more explicitly, we scrape information on all followers of the official BLM account and geo-localize each of those Twitter users. We present the results using this outcome in column 5.[16] We find that places that were more exposed to the pandemic also have a larger number of followers of the official BLM account. A one standard deviation increase in pandemic exposure leads to 10 more Twitter users following the official BLM account per county, which is more than 60% of the mean in our sample.

3.3.2 Alternative explanations

In the previous sections, we have established a causal link between pandemic exposure and social media take-up as well as BLM protest. In Appendix Table A10, we explore additional (non-exclusive) mechanisms for the diffusion of the BLM protests in response to the pandemic, considering *i*) scattering rather than a diffusion of protest; *ii*) increased salience of racial inequality; and *iii*) lower opportunity costs of protesting. We interact SSEs with a variety of county characteristics to assess whether the salience of the pandemic ignited BLM protest differentially across places. We always control for SSEs and the interacting variable.

First, we examine the spatial diffusion of Black Lives Matter (BLM) protests in response to pandemic exposure. In column 1, we interact SSEs with the distance to the origin of the protest in Minneapolis. We do not see evidence that pandemic exposure magnified protest in counties that were closer to the location of George Floyd's murder. It is also possible that the pandemic substituted protest away from some locations to others rather than mobilizing protest in new counties. For instance, the pandemic may have changed the scope and structure of BLM protests (smaller but more numerous). We have already shown that the pandemic did not significantly impact protest participation, indicating that large protests are not split into many small protest across different locations. In columns 2 and 3, we investigate the possibility that counties in close proximity inspire BLM protest nearby. Specifically, we test whether pandemic exposure generates more protest in counties whose neighbors protested for a BLM related cause before the pandemic (column 2) or in the days before the county experienced a protest (column 3). We do not find evidence for the spatial diffusion of protest in response to pandemic exposure. We argue that this is consistent with the social media mechanism because online exposure to the

[16]The scraping was conducted on March 2022 meaning: i) that we are not able to capture users that stopped following BLM; ii) that we are capturing users that may have started following well after the murder of George Floyd and iii) that we are capturing users that started following before the pandemic.

protest trigger is much less dependent on learning over time or through geographic proximity.

Second, we investigate whether the pandemic heightened awareness of racial inequalities, thereby influencing protest activity, independent of social media. We consider whether racial disparities in the adverse consequences of pandemic exposure proxied by a higher death toll among Blacks (relative to their population share) might have increased propensity to protest. Pandemic exposure should ignite more BLM protest where the death burden on Blacks is particularly high.[17] We find a small and marginally significant effect for the interaction term, suggesting that pandemic exposure in states with a higher death burden for the Black population increases the BLM protest probability. In addition, we proxy interest for BLM before the murder of George Floyd with Google searches for BLM-related keywords. Again, pandemic exposure should mobilize protests disproportionately in places that have previously increased their interest in BLM related content. We do not find that an increased interest in racial injustice before the protest trigger increased the probability of observing a BLM protest after the murder of George Floyd.

Third, we test whether the results can be explained by a decrease in the opportunity cost of protesting. It is possible that new people joined the movement because they had lower (social and economic) opportunity costs of protesting during the pandemic. We proxy the decrease of economic opportunity cost using the average unemployment rate in the year before the murder of George Floyd. We proxy the decrease of social opportunity cost with the stringency of social distancing measures at the state level. If opportunity costs play an important role, then pandemic exposure should magnify BLM protest in places with high unemployment and strict lockdown stringency. We do not find evidence that these significantly contribute to the diffusion of BLM.

4 Late adoption and BLM protest

In the previous section, we have provided evidence that pandemic exposure generated a surge in late adopters. We have also shown that pandemic exposure increased BLM protest in places that never protested for a BLM related cause before. We hypothesize that the pandemic shifted a substantial proportion of the population to the digital space which was then exposed to an unexpected and viral protest trigger - the murder of George Floyd. As mentioned before, the pandemic impacted an array of factors that could have influenced BLM protest beyond social media adoption. We have shown some evidence that these factors do not predict BLM protest patterns. In the following section, we further strengthen the link between social media adoption and BLM protest.

[17] COVID-19 deaths by race are only available at the state-level. This is why we interact local pandemic exposure with Black death burden to predict BLM protest.

4.1 Push-Pull Instrument for new Twitter users

4.1.1 Estimating equation

We start by investigating the correlation between BLM protest and new Twitter users, i.e. those geo-located Twitter profiles at the county level that have been created after the outbreak of the pandemic and before the murder of George Floyd. Our specification writes as follows:

$$BLM_c = \beta_1 log(1 + New\,Twitter\,Users)_c + \mathbf{X}_c\beta_{\mathbf{X}} + \gamma_s + \epsilon_c \tag{3}$$

Our outcomes of interest BLM_c include online and offline protest for BLM as well as their first principal component. We look at *i*) the likelihood of recording any BLM protest in the three weeks following the murder of George Floyd, *ii*) the number of protests, *iii*) the total number of participants, *iv*) the number of geo-localized tweets containing BLM related hashtags or keywords and *v*) the number of geo-localized followers of the official BLM Twitter account. Equivalent to our specification in the previous section, we include state fixed effects and the same vector of county level controls $\mathbf{X}_c$ to account for within state heterogeneity.

The concern with a causal interpretation of β_1 is that there may be unobserved county characteristics that drive both late social media adoption and BLM protest. We mitigate concerns about reverse causality by limiting social media adoption to the pre-George Floyd period. We tackle concerns about the endogeneity of pandemic Twitter adoption with an instrumental variable strategy that combines insights from the previous section with those from the existing literature. The first stage writes as follows:

$$log(1 + New\,Twitter\,Users)_c = \ \delta_1\,\mathbf{Z_c} \times \mathbf{N_c} + \delta_2\,N_c + \delta_3\,Z_c + X_c\delta_X + \zeta_s + \varepsilon_c \tag{4}$$

where N_c is the baseline network (pull factor) and Z_c pandemic exposure (push factor). We draw from the literature on path dependence in technology adoption that suggests increasing returns to joining a social media platform when the existing network is large (Arthur, 1989; Bursztyn et al., 2023). We show in Appendix Table A7 that baseline Twitter networks magnify subsequent take-up.

Since the baseline Twitter network is endogenous, we leverage exogenous variation in baseline Twitter networks borrowing from Müller and Schwarz (2023) who leverage a film, interactive media, and music festival and conference called South by South-West (SXSW) held annually in Austin Texas. The 2007 edition heavily promoted Twitter and crucially determined the spread of Twitter in the early stages of its roll-out. N_c is measured as the logarithm of one plus the number of SXSW followers who created their account in March 2007 in the county and neighboring counties and Z_c remains the number of SSEs in the neighboring counties until six weeks leading up to the murder of George Floyd. We also control for Z_c and N_c separately.[18]

[18] The set of baseline controls now includes $Pre\,SXSW\,Users_c$ i.e. the logarithm of one plus the number of SXSW followers in the county and neighboring counties that created their account before March 2007, as a proxy for the general interest in Twitter and the festival.

The exclusion restriction of the instrument requires that the interaction between SSEs and SXSW followers only impacts BLM protest through the number of new Twitter users, given the set of controls and fixed effects in our model. It is possible that the interaction between pandemic exposure and baseline Twitter use affects BLM protest beyond its impact on new Twitter users. For instance, the salience of racial inequality of the pandemic may be larger in places that are closer to SSEs and have a higher social media penetration. Therefore and analogous to the previous section, we predict our treatment with an array of county and state characteristics and report the results in panel B of Figure 2. Reassuringly, we find no systematic correlation between the vast majority of county characteristics and our push-pull instrument. If anything, counties with neighbors that are traditional protesters are less likely to be exposed to the push-pull instrument, alleviating concerns about spatial spillovers.

Appendix C presents more details on the push pull instrument. Table C1 presents the direct effect of pull factor N_c on baseline Twitter use in December of 2019. As anticipated, SXSW users are only strong predictors of baseline Twitter use in counties that had never protested for a BLM related cause before but not among traditional protesters. This is in line with our prior that instruments leveraging early stage social media roll-out do not perform well in saturated social media markets. Next, Appendix Table C2 validates the assumption that baseline Twitter use is a strong predictor for subsequent Twitter use. Panel A shows that baseline users in 2019 predict the number of new users in 2020. More importantly, panel B shows that the combination of push and pull factors $Z_c \times N_c$ is an even stronger predictor of Twitter adoption during the pandemic.

Lastly, Appendix Table C3 uses push and pull factors separately to instrument for new users. We show that SXSW users do not predict late adoption, beyond their direct effect on the baseline network in 2019. We also show that our baseline results using the push-pull instrument remain unchanged when we include controls for COVID-19 related deaths and logged number of users at baseline. The first stage becomes weaker but the the coefficient of interest does not change in magnitude or precision. Section 4.1.3 provides an array of additional exercises that validate the plausibility of the exclusion restriction and robustness of our results.

4.1.2 Results

Table 3 present our second set of main results based on the push-pull instrument. We present both OLS and 2SLS coefficients for new Twitter users, i.e. late adopters. First stage and reduced form coefficients, as well as Kleinbergen-Paap F-statistics are reported at the bottom of the table. Our outcome of interest in column 1 is the first principal component of various proxies for online and offline BLM protest.[19] Specifically, we look at the likelihood, number and size (total participants) of BLM protest offline in columns 2 to 4. Columns 5 and 6 focus on online protest, in the form of tweets that contain BLM-related hashtags and keywords and in the form of more explicit support for BLM proxied by the number of geo-localized followers of the official BLM account. We find a positive and precisely estimated effect of new users on protest. The magnitudes are large: our 2SLS estimate in column 2 suggests that a one percent increase

[19] We detail the construction of the first principal component in Appendix Table A11. We show the correlation between all measures, the eigenvalue of the first principal component and the factors loadings for each variable.

in new Twitter users increases BLM protest likelihood by 0.2 p.p., which is approximately 5% relative to the mean. We also find that the number of new users increases the frequency of BLM but does not significantly impact the size of the protest.

In addition, column 5 and 6 suggest that online protest increases in response to Twitter adoption. We find that a one percent increase in new Twitter users increases the number of BLM related tweets by 19 and adds 1.7 new followers to the official BLM Twitter account. Overall, we find that Twitter adoption increases BLM protest both online and offline. This stands in contrast to a strand of the social science literature that has emphasized "slacktivism" i.e. the substitution away from more effective forms of collective action to more ineffective and symbolic activism online (Christensen, 2011; Schumann and Klein, 2015).

In column 7, we include both old and new users in the regression and instrument for both with SSEs, SXSW and their interaction. The first stage becomes weaker, but both 2SLS and OLS results suggest a similar pattern: older users contribute less to BLM protest mobilization compared to newer users. This may not be surprising since these existing users had not mounted BLM protest previously. However, the surge in late adopters in 2020 led to the diffusion of BLM protest to these new counties. This could be either because new users helped reach a critical mass of prospective protesters. Alternatively, social media adoption may energize protest early on but not later, potentially because new users are confronted with new information that energizes protest right after adoption. We investigate these and other mechanisms in the next section.

4.1.3 Robustness

We repeat and extend the set of robustness checks from the previous section 3.2.3. We focus on Panel B of robustness Table B1 to Table B6, which replicates the specification in column 2 of Table 3. We verify in Tables B1 and B2 that our results are not sensitive to changes in the definition of Z_c, i.e. the push factor to social media adoption. We show in columns 2 to 4 of Table B1 that SSEs in closer proximity generate a weaker first stage. Conversely, expanding or narrowing the time window in which we consider SSEs does not substantially affect the predictive power of the instrument. The main effect of new Twitter users on BLM protest probability remains remarkably similar in terms of magnitude and precision. Similarly, Table B2 confirms that excluding SSEs in prisons, controlling for SSEs in the own county, controlling for previous BLM protests in neighboring counties, and applying various weighting schemes on SSEs does not significantly change the size and significant of our coefficient of interest (albeit producing a slightly weaker first stage). Table B5 considers the robustness of our main result with respect to changes in the sample composition and the definition of the treatment variable. We exclude coastal states and counties, control for the prevalence of COVID-19 deaths and allow for spatial correlation. Again, our results remain robust and precisely estimated.

In Table B6, we continue to validate our main findings, again focusing on Panel B. We run a probit instead of a linear probability model and our results are more precisely estimated. In column 3, we expand on the idea of comparing counties with similar ex-ante BLM protest probabilities and go beyond the socio-demographic and political controls. Using LASSO, we select the subset of relevant county-level variables that determine past BLM events and create a propensity score for protesting, based on the selection of these variables. We include this variable as an additional control and confirm that our results remain robust. In columns 4 to 6, we include fixed effects for various ranges of the pre-pandemic protest probabilities. We split the fixed effects along the thresholds that produce county groups of different sizes: 3 groups (with 1000 counties each), 30 groups (with 100 counties each) and 300 groups (with 10 counties each). This allows us to compare counties within narrow bands of ex-ante protest probabilities. Again, we find that counties with a higher number of new Twitter users increase the probability of observing a BLM protest.

4.2 Event Study around murder of George Floyd

We exploit an event study design that allows us to account for time invariant unobserved heterogeneity at the county level. More specifically, we will exploit the timing of the unexpected protest trigger - the murder of George Floyd on May 25th 2020 to compare differences in BLM protest intensity across counties with varying levels of pandemic Twitter take-up. Our event-study specification writes as follows:

$$\text{BLM}_{ct} = \zeta_c + \delta_{st} + \sum_{\substack{k=T_0 \\ k\neq -1}}^{T_1} \beta_k \log(1 + \text{New Twitter Users})_c \times \mathbb{1}_{t=\text{May 25th}+k} + \epsilon_{it} \quad (5)$$

We include county and state-day fixed effects, which account for any county characteristic that could influence the overall propensity of county to protest for a BLM related cause as well as any state-specific time-varying shocks that could hinder BLM protest, such as lockdown stringency on the day of the protest. Importantly, our time window is restricted to the two weeks before and after the protest trigger, such that unobserved time-varying characteristics at the county level would have to vary within that narrow time frame. It is also worth noting that there were no BLM protest in the United States after the outbreak of the pandemic and before the murder of George Floyd, such that the coefficients for the pre-period $\beta_{k<May25th}$ will mechanically be estimated as precise zeroes. Nonetheless, the coefficients $\beta_{k>May25th}$ will be informative since they tell us whether counties with a higher take-up of Twitter during the pandemic will differentially respond to the protest trigger.

Our findings, presented in Figure 5, are consistent with the cross-county results. The top panel shows beta coefficients for a simple dummy variable that switches on after the murder of George Floyd. The outcome BLM_{ct} is measured as the cumulative number of BLM protest in the county for each day $t = k$. As expected, BLM protest increases immediately after the viral protest trigger on May 25th 2020. In the bottom panel, we interact the indicator function for the post George Floyd period with pandemic Twitter adoption. Counties with a greater influx of new Twitter accounts exhibit a more pronounced response to triggers in terms of their BLM protest activity.

Table 4 column 1 presents the corresponding coefficient. In column 2, we change the definition of the outcome to the probability of observing a BLM protest on each day. Our estimates suggest that a one percent increase in the number of new Twitter users increases the likelihood of observing a BLM protest at any given day after the protest trigger by 0.03 percentage points.

In columns 3 and 4, we distinguish between counties without and with prior BLM protest. While the coefficient for the latter sample is larger, counties without previous BLM protest experience a 40% increase in protest probability relative to the mean (compared to 20% for counties with previous BLM protest) in response to a 10% increase in new users. Next, in column 5, we augment the two way fixed effects specification with our previous instrumental variable approach, where we predict the number of new Twitter users with the push-pull instrument. Again, we find a positive, precisely estimated and larger effect of new Twitter users on BLM protest probability.

In a last step, we run a horse race between new Twitter users and baseline Twitter users. The advantage of the difference in differences estimation is that we can include county and state-day fixed effects, which accounts for a significant portion of the unobserved heterogeneity. In column 6, we add the interaction between the log of one plus the number of geo-localized Twitter users at baseline in December 2019. The effect of baseline Twitter penetration is positive but imprecisely estimated and much lower than the effect of new users, mirroring our previous findings that show that protest diffused to new geographies and places with a low ex-ante probability of protesting.

5 Coordination costs and changes in preferences

Protest mobilization can be driven by two mutually non-exclusive forces: a reduction in coordination costs and through persuasion. Leveraging early adoption, the literature has emphasized the importance of coordination costs (Manacorda and Tesei, 2020; Enikolopov et al., 2020). However, selection into early adoption may favor coordination costs over persuasion if these users are already politicized. In addition, early adopters experience social media as a "blank slate" upon entry – rather than a politically consolidated environment. In our context, late adopters encountered a social media environment with a large and established BLM network. The murder of George Floyd triggered a large wave of imagery, information and anti-racist narratives which may have moved their preferences.

In the following section, we use individual-level survey data on social media use and political preferences to shed light on changes in attitudes in response to social media adoption. In addition, we investigate other protests which did not have a large and established network and are less likely to be supported by the majority of existing users. In particular, we leverage information on counter-mobilization and other protests to investigate whether coordination costs decrease for supporters of causes on the opposite side of the political spectrum.

5.1 Individual-level social media adoption and attitudes towards BLM

5.1.1 Instrumenting individual-level social media adoption

We draw from two main sources to assess the effects of social media adoption on attitudes towards BLM: the Cooperative Election Study (CES) and the PEW American Trends Panel. The CES was conducted in November of 2020 and contains information on respondents' social media consumption, attitudes towards the police and racism, vote in the 2020 election and – importantly – their county of residence. This allows us to connect county level shocks to individual-level take-up. We start by investigating the correlation between social media use and a variety of outcomes Y_i, including whether the respondent has attended a protest, march or demonstration in the past year, whether the respondent takes any form of political action on social media, and attitudes towards the police and about racism (see Appendix Table A12 for a precise survey question).

$$Y_i = \beta_1 \text{Social Media Use}_i + \mathbf{X}_i \beta_{\mathbf{X}} + \gamma_c + \epsilon_i \tag{6}$$

We include county fixed effects γ_c and an array of individual level controls $\mathbf{X}_i$ (age, gender, employment status, number of children, dummies for religious affiliation, dummies for race and citizenship status) and cluster standard errors at the county level. Our treatment is Social Media Use$_i$ measured as a dummy variable that indicates whether the respondent has used social media over the past 24 hours. We take this as a crude indicator for social media use combining the extensive and intensive margin.

In order to leverage exogenous variation in individual-level take up, we follow the same intuition as in the previous section. Twitter adoption depends on the baseline network and local pandemic exposure with the caveat that social media adoption is limited in saturated markets and will expand only where there is scope for adoption. Following our findings in section 3.2.2, we proxy the scope for adoption at the individual level with the age of the respondents. Younger respondents will be less sensitive to the combination of push and pull forces to social media adoption since they are already social media users. About 90% of respondents under the age of 33 – the bottom age quartile – have used social media in the last 24 hours compared to about 60% in the top quartile above the age of 63 (see Appendix Figure A8).

Based on this observation, we instrument social media use at the individual-level with the interaction of the push-pull instrument $Z_c \times N_c$ with the age of the respondent, while controlling for county fixed effects ζ_c and the separate interactions of Z_c and N_c with age, as well as the same set of individual level controls X_i. The first stage writes as follows:

$$\begin{aligned} \text{Social Media Use}_i = \; & \delta_1 \, Z_c \times N_c \times Age_i \\ & + \delta_2 \, N_c \times Age_i + \delta_3 \, Z_c \times Age_i \\ & + \mathbf{X}_i \delta_{\mathbf{X}} + \zeta_c + \varepsilon_i \end{aligned} \tag{7}$$

Our estimation compares differences in political preferences between older and younger respondents in high versus low exposure counties, i.e. counties with a greater shock to social media adoption. The identifying assumption requires that the triple interaction $Z_c \times N_c \times Age_i$ only impacts political preferences through the adoption of social media. In other words, within the same county older respondents will only change their political preferences in response to the push-pull instrument through a higher likelihood of signing up to social media compared to younger respondents. Any age-differential responses to baseline social media penetration or pandemic exposure will be captured in the interactions $N_c \times Age_i$ and $Z_c \times Age_i$ respectively. Any unobserved characteristics at the county level that determine social media use and political preferences more generally will be captured in the fixed effects.

The exclusion restriction is violated if, for instance, baseline social media penetration combined with exposure to SSEs in close proximity induced the older generation to reduce mobility and isolate more than younger respondents, such that their overall media consumption has increased - including social media. We assess this concern by estimating a placebo first stage, looking at the the consumption of different media outlets. We exploit information in the CES on the use of TV, newspapers, and radio over the past 24 hours. We report in Appendix Table A13 that our instrument predicts social media use while it does not predict the consumption of other media outlets.[20]

5.1.2 Results: voting, attitudes on racism and the police

We present the results on social media and on political preferences in Table 5. We present OLS, 2SLS and reduced form results, as well as first stage coefficients, standard errors and Kleinbergen Paap F-statistics at the bottom of the table. Throughout, the first stage F-statistics remain below the conventional threshold. This is to be expected since we only have a very crude measure of social media adoption that does not capture the timing of social media adoption, nor does it capture which social media platform was used. In order to assess the degree of the weak instruments problem and following Olea and Pflueger (2013), we run a weak IV test that gives us effective F-statistics and corresponding thresholds that indicate how much worst case bias is associated with a reduction in strength of the first stage.[21]

In column 1, we exploit information on whether the respondent has participated in any protest, march or demonstration within the last year. Our 2SLS estimates suggest that social media use increases the likelihood of joining a protest. Similarly, column 2 suggests that respondents are also more likely to be politically active on social media. This includes posting or forwarding a story, photo, video, link or comment on social media, as well as following a political event, reading a story or watching a video about politics.

Next, we examine the voting behavior and attitudes of respondents. Specifically, we show in column 3 that respondents (within the same county) are more likely to vote for Biden in the 2020 election when they increase their use of social media in response to the individual level social media adoption shock. In column 4, we build an index that captures leniency towards police officers – a topic that has been prominently discussed within the BLM community, including a sub-strand of the movement that demands to defund the police. The index is constructed as the first principal component of eight questions, ranging from the requirement of police officers to wear body cameras, a reduction in police presence by 10%, a national registry for violent police officers, a ban on the use of choke holds and more (see Appendix Table A12 for exact wording of these questions). Higher values indicate higher leniency towards the police.[22] Both the OLS

[20] P-values: for social media 0.06, TV 0.346, newspaper 0.382, radio 0.388

[21] This is equivalent to the Stock-Yogo weak ID test with corresponding critical values but does not assume i.i.d. Intuitively, the tau value quantifies the maximum relative bias in the IV estimate that you're willing to accept compared to the bias in an OLS estimate. For example, a tau of 5% means you would accept an IV estimate with up to a 5% worst-case bias relative to the bias in the OLS estimate caused by endogeneity. In our case, we would have to be willing to accept a more than a 30% worst-case bias relative to the OLS estimate.

[22] We detail the construction of the first principal component in Appendix Table A14. We show the correlation between all measures, the eigenvalue of the first principal component and the factors loadings for each variable. Every variable is coded in the expected direction: most appear with a positive coefficient, except question 334c for which lower values indicate higher support for the police.

and 2SLS estimates reveal a negative coefficients but 2SLS estimates are more noisy. We take this as suggestive evidence that social media take-up increases the demand for more scrutiny of the police.

In column 5, we repeat this exercise, this time using attitudes towards racism as an outcome. The index is comprised of four question with higher values of the index indicating less awareness of racial inequalities. The set of questions includes views on the advantages of white people in society, that racial problems are not isolated situations, that slavery created conditions that make it hard for Blacks to advance, that the success of other minorities proves that Blacks can do the same.[23] Similar to the previous column, we find consistently negative but noisily estimated effects of social media use on attitudes towards racism, indicating that respondent become more aware of racial issues. It is important to caveat that attitudinal changes associated with social media adoption may either come directly through exposure to new information but also indirectly through the participation in BLM protest and subsequent changes in preferences.

Throughout, we find that older respondents exhibit more progressive views in response to the social media adoption shock. We show in Appendix Figure A9 that this is not true for older respondents on average. In fact, looking at the outcomes considered in Table 5 by age cohort, we find that the older age cohorts are less likely to protest, be politically active on social media, vote for Biden or hold views that are critical of police and that are anti-racist. We take this as suggestive evidence that our results do not capture political selection.

5.1.3 Results: attitudes on the BLM movement

We exploit more short-term, BLM-specific information from the PEW American Trends Panel Survey, which was conducted at the height of BLM protest between June 4th and June 10th of 2020. We describe the underlying data in more detail in Appendix Section A. Since the location of the respondent is anonymized in the survey, we cannot apply any of the identification strategies discussed in the previous sections. The only available information is the severity of exposure to COVID-19 in respondent's county of residence in June 2020. However, the rich set of individual-level controls and placebo checks assuage concerns about omitted variable bias. In all regressions, we control for respondents' race, whether or not they live in a metropolitan area, gender, age, education, income and whether or not they lean towards the Democratic party.

Table 6 shows the results. Columns 1 - 2 show the intensity and form of news consumption in the context of George Floyd's murder. Higher levels of COVID-19 are positively and significantly associated with more news consumption about George Floyd and more social media news consumption about George Floyd. Then, we analyze whether this change in mode of news consumption is accompanied by a change in attitudes. In column 3 and 4, we find that respondents are also more likely to agree with the statement that the BLM protests arise because of structural racism and not as an excuse for criminal behavior.

In column 5, we verify whether pandemic exposure increased the salience of racial inequality more generally and extends to settings unrelated to George Floyd and BLM. We do not find

[23]See again Appendix Table A14. We show the correlation between all measures, the eigenvalue of the first principal component and the factors loadings for each variable. Every variable is coded in the expected direction: questions 440b and 441a, where higher values indicate more awareness of racism, are given negative weights.

evidence that respondents are more likely to report that higher hospitalization rates of Blacks during the pandemic are caused by circumstances beyond their control, rather than personal choices or lifestyle. To rule out that exposure to COVID-19 in the earlier stages of the pandemic is just a proxy for more progressive leaning counties, we use an additional question that deals with an unrelated issue: legal status for undocumented immigrants. Individuals living in counties with higher exposure to COVID-19 are not more likely to prefer more rights for undocumented immigrants, alleviating some of the concern about unobserved heterogeneity.

5.2 Counter-mobilization and other protest

Table 7 explores the role of late adoption on different forms of political expression and protest for different causes. In column 1, we examine the impact of new Twitter users on George Floyd street art and graffiti which we scrape and geo-localize from the *Urban Art Mapping George Floyd and Anti-Racist Street Art.* Graffiti has emerged as prominent vehicle for advocacy within anti-racist movements (Mathieu, 2018; Cappelli et al., 2020). We find that the surge in Twitter use during the pandemic does not correspond with an increase in George Floyd-related street art, potentially because such these are challenging to replicate in places that have only recently engaged with BLM. Additionally, these forms of expressions are used by younger people, which are more likely to become social media users earlier than older people.

Next, in columns 2 to 5, we leverage information about the number of protests for BLM and other causes in 2020 from the ACLED US Crisis Monitor (we describe this data in more detail in Appendix A). We expand the observation period from May 25th 2020 until December of 2020 since BLM protest have crowded out other protests in May and June of 2020. In column 2, we confirm our previous findings that new Twitter users increase the number of BLM protests. In column 3, we look at all other protests excluding BLM and find that social media penetration increases protest universally. Our estimates suggest that a 10% increase in new Twitter users, increases the number of BLM protest by 37% and all other protests by 34%.

Based on the ACLED protest descriptions, we classify protests as pro-Trump if they involve actors such as the "Proud Boys" or QAnon, protest perceived election fraud or explicitly support Trump.[24] The coefficient is close to zero and imprecisely estimated, suggesting that new users do not mobilize for conspiratorial, populist or explicitly pro-Trump causes.

There are many possible explanations for these findings. First, the surge in conspiratorial and stop the steal protests emerged in late 2020, after the presidential election and peaking in early January 2021, such that many of these are not included in the data set. Second, it is possible that new Twitter users are energized to join protests when they enter the platform but that this mobilization effect subsides over time. By late 2020, these users may have joined the ranks of "old users" that - as we have shown before - drive protest to a lesser degree. Third, Twitter may be particularly suited for mobilizing protest for certain causes over others, potentially because of the political make-up of its existing users.

We examine the possibility that mobilization is more short-lived and potentially more centrist

[24]The keywords we include are: Stop the Steal, Proud Boys, Qanon, election fraud, ballot, pro-Trump, Trump support, in support of Trump, WAF (Women for America First) and MAGA. The results are similar when we only include pro-Trump protest and if we exclude counter demonstrations to BLM protest (tables not reported).

in columns 5 to 7. First, we turn to protests that lament social distancing rules and mask mandates. These protests peaked earlier in the pandemic and are less explicitly pro-Trump. We detect an increase in mobilization in similar magnitude to BLM protest. In addition, we examine explicit counter-mobilization to BLM on Twitter. We examine the impact on the use of the most popular hashtags in opposition to BLM #AllLivesMatter and #BlueLivesMatter. These served as a relativization of the BLM rallying cry and emphasized the importance of the safety of whites and police officers. Again, we scrape the universe of tweets that contain these hashtags and assign them to counties based on the location of the tweeters. We find that new Twitter users also mobilize for causes in opposition to BLM online.

6 Conclusion

In this paper, we investigate how late adopters of social media shape collective action, using the Black Lives Matter (BLM) protests during the COVID-19 pandemic as a case study. Developing a novel instrument to predict new Twitter users at the county level based on pandemic exposure and pre-pandemic social media networks, our empirical analysis reveals that pandemic-induced social media adoption was associated with increased participation in BLM protests, online and offline. While social media adoption shapes preferences in favor of BLM, it also spurs counter-mobilization and other protests, emphasizing the potential for social media to facilitate collective action across the political spectrum.

Our work contributes to a growing body of literature on the political effects of social media and the diffusion of social movements, highlighting the role of technology adoption in modern activism. We emphasize that the composition of social media networks can shape collective action in ways that differ from early adoption.

Our findings open several avenues for future research. Firstly, a deeper investigation into the mechanisms by which social media influences political preferences and mobilizes collective action across different political and social movements would enrich our understanding of digital activism. Secondly, examining the long-term effects of pandemic-induced social media adoption on political engagement and social capital could reveal important dynamics about the sustainability of movements initiated or amplified through digital platforms. Lastly, further exploration of the role of algorithmic curation and echo chambers in shaping the political landscape and the potential for counter-mobilization efforts would provide critical insights into the challenges and opportunities presented by social media in the context of political activism.

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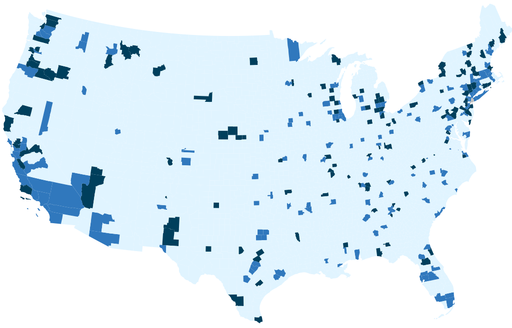
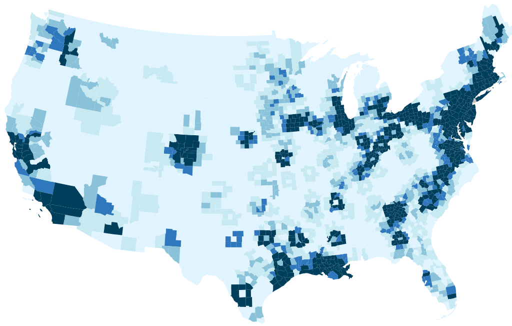
Figures and Tables

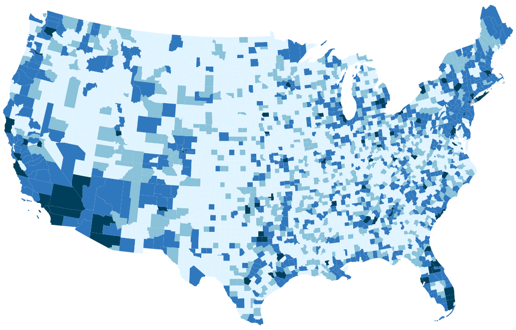
Figure 1: **Geographic distribution of BLM protest, social media and super spreading events (SSEs)**



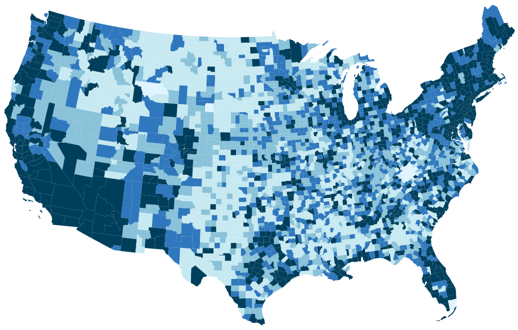
(a) BLM protest after George Floyd



(b) SSEs in neighboring county



(c) Late Twitter adoption
(logged number of new Twitter users)

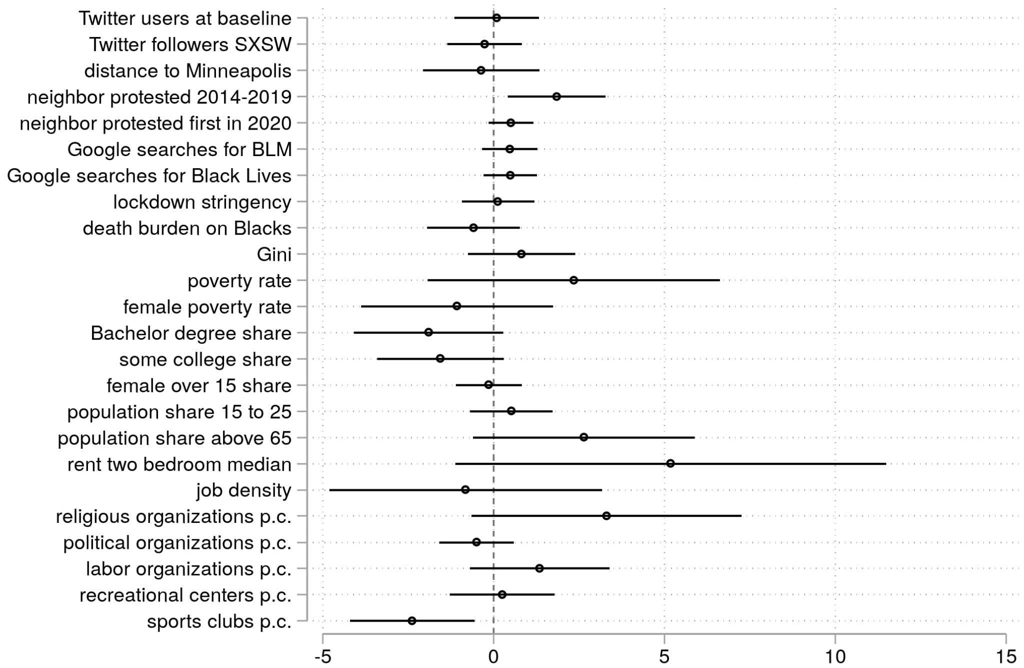


(d) Late social media adoption index
(first principle component)

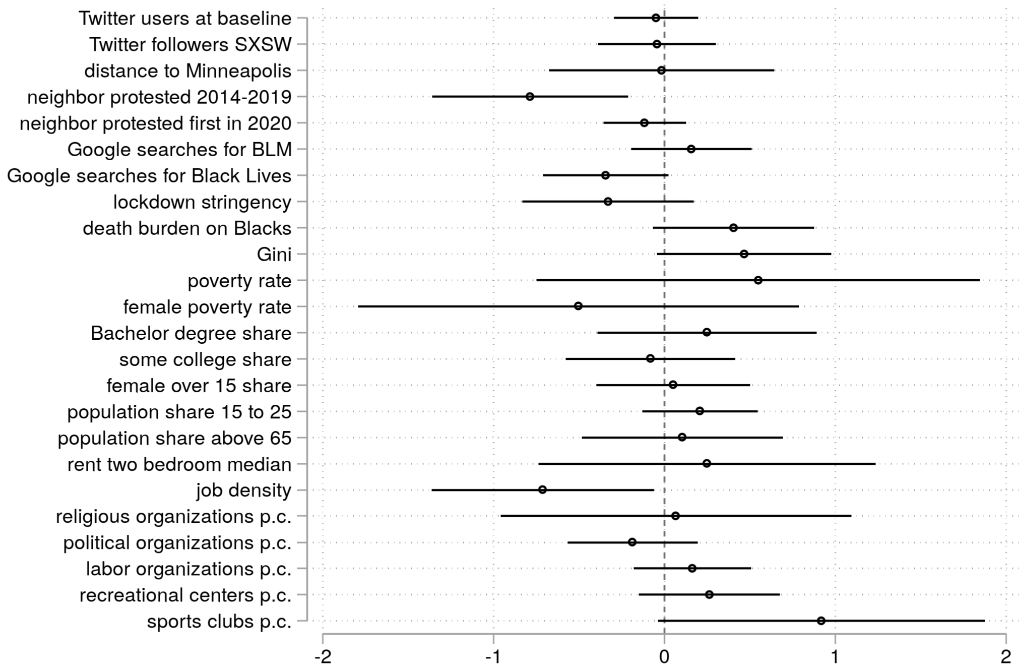
Note: geographic distribution of outcome, treatment and instrument across counties. Sub-figure (a) shows the location of counties with at least one BLM protest as reported by *Elephrame* in the three weeks following George Floyd's murder. Dark blue color indicates counties without BLM protest before 2020 but protest in 2020. Medium blue indicates protest for BLM before and after 2020. Light blue indicates no BLM protest in 2020. Sub-figure (b) shows instrument Z_c, i.e. the cumulative number of SSEs in neighboring counties less than 50km from the county border but not within the county, until 6 weeks before the murder of George Floyd. Sub-figures (c) and (d) show social media outcomes, specifically the log of one plus the number of new Twitter profiles created between January and May 2020 observed in a random sample of tweets collected in the 3 weeks preceding the murder of George Floyd (c), and the index for pandemic social media adoption, that is comprised of the first principal component of the (log) new Twitter users, Google searches for Twitter as well as (log) new Twitter users that end up tweeting about BLM after the murder of George Floyd (d).

Figure 2: **Plausibility of quasi random exposure**

(a) standardized coefficients for dep. var. super spreading events (Z_c)

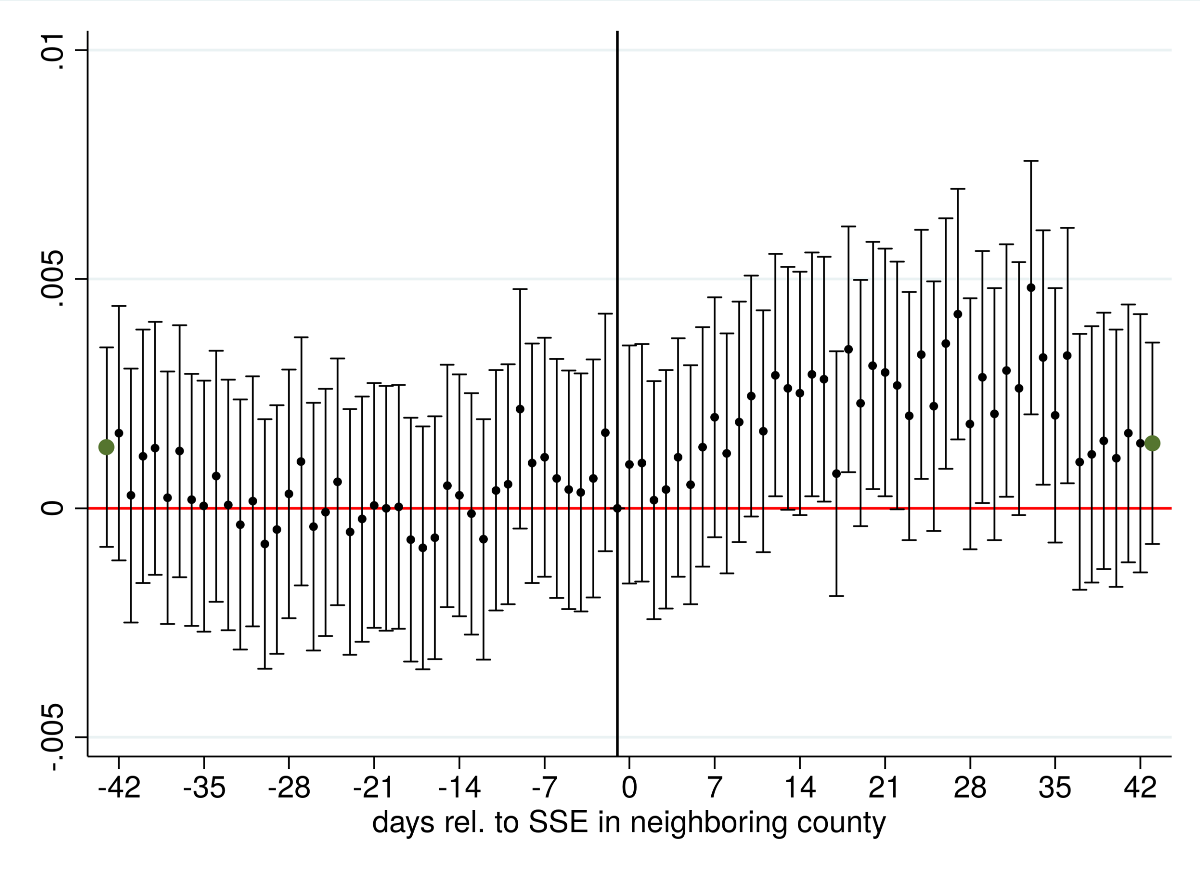


(b) standardized coefficients for dep. var. super spreading event × baseline Twitter network ($Z_c \times N_c$)



Note: Plausibility of the instrument exogeneity. We run the baseline specification 1 but use our instruments as outcomes: in subfigure (a) the dependent variable is the cumulative number of SSEs within 50km radius from the county border up to six weeks before the murder of Floyd and in subfigure (b) the interaction between this and the baseline Twitter network N_c, which is the logarithm of one plus the number of SXSW followers who created their account in March 2007 in the county and neighboring counties. We exclude state fixed effects since the death burden on Blacks and lockdown stringency is only available at the state level, and standardize all variables. Twitter users at baseline are measured as the log of 1 plus geolocalized Twitter users in a sample of tweets from December of 2019. Twitter followers SXSW is the logarithm of one plus the number of SXSW followers in the county and neighboring counties that created their account before March 2007. Google searches are measured in the 3 weeks leading up to Floyd's murder. Population statistics come from the American Community Survey of 2018. Religious and other organizations per capita are measured in 2014. Female poverty rate, poverty rate and job density are re-scaled by a 1:2 ratio for readability.

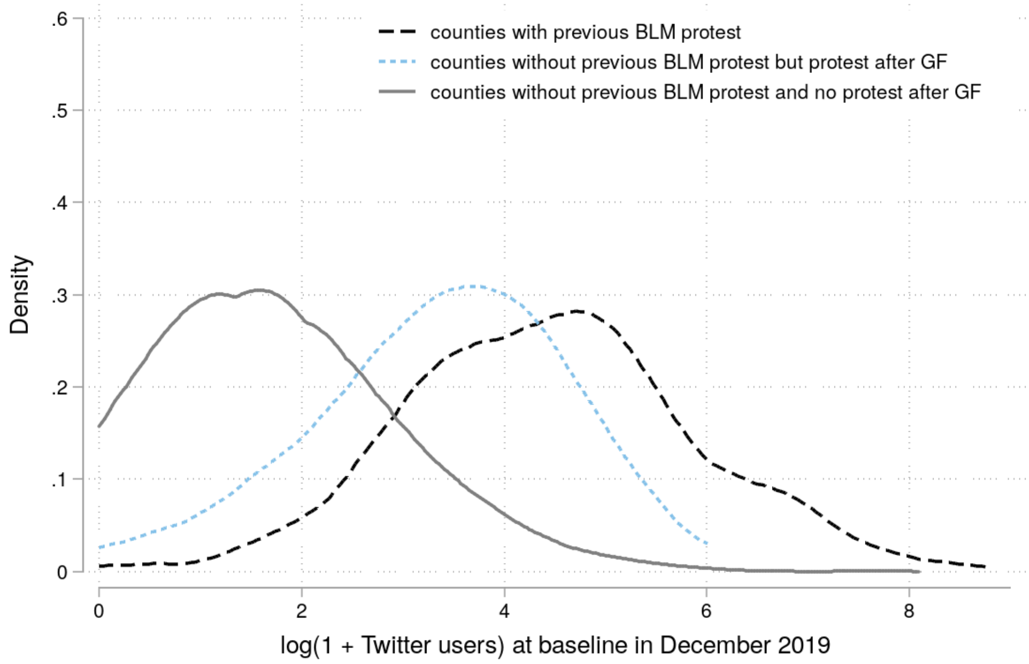
Figure 3: Timing of super spreading events (SSEs) and subsequent Twitter adoption



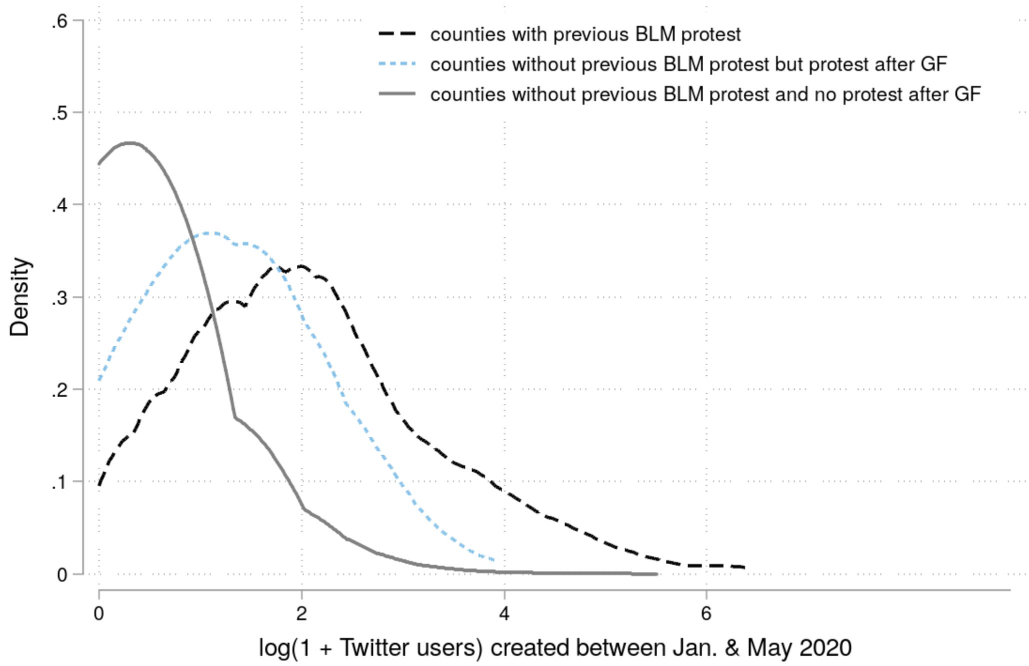
Note: Event study graph according to following specification: New Twitter Users$_{ct}$ = $\sum_{k=T_0}^{-1} \beta_k \times \text{SSE}_{-c,k} + \sum_{k=0}^{T_1} \beta_k \times \text{SSE}_{-c,k} + \zeta_c + \delta_t + \epsilon_{ct}$ with county and time fixed effects. Time is measured in days before and after an SSE in a neighboring county within 50km of the county border.

Figure 4: **Twitter penetration for counties w/ and w/o BLM protest**

(a) Pre-existing Twitter users created before the pandemic



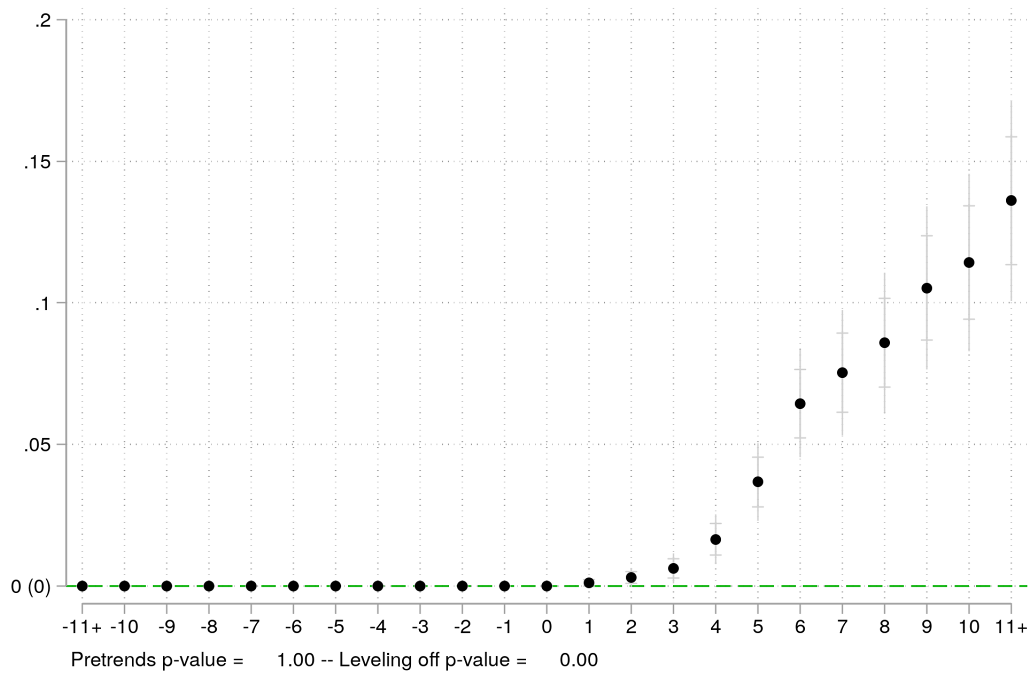
(b) new Twitter users created during the pandemic



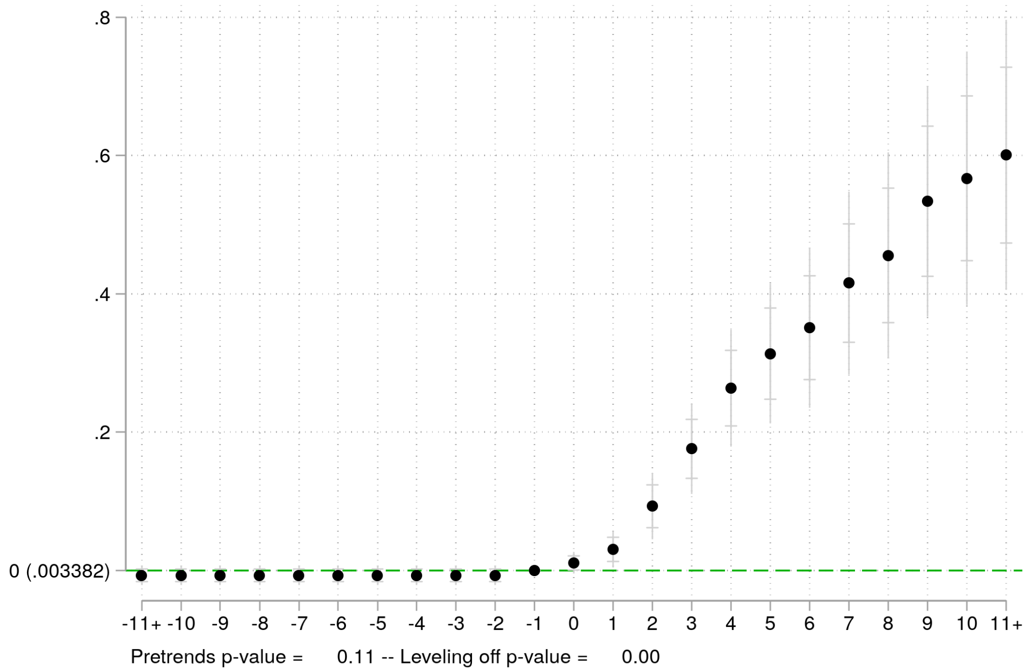
Note: Kernel density plot for Twitter penetration. In (a), baseline Twitter penetration is measured as the log of one plus the number of geo-localized Twitter accounts at the county level in December 2019 from a random sample of tweets. In (b), new Twitter users is measured as the log of one plus de number of geo-localized Twitter accounts created during the pandemic from a random sample of tweets collected in the 3 weeks preceding Floyd's murder. Sample is split in counties that had at least one recorded BLM protest before Floyd's murder (dark blue line with long dashes), those that did not have any BLM protest prior to but had protests after (light blue line with short dashes), and those that had no protests at all (solid grey line).

Figure 5: **Event Study Design: Cumulative BLM protest after George Floyd and in counties with higher pandemic Twitter adoption**

(a) Post George Floyd



(b) Post George Floyd × log(1 + New Twitter Users)



Note: Event study graph using xtevent by Freyaldenhoven, Hansen, Pérez and Shapiro (forthcoming) following specification 5 with county and state-day fixed effects (panel a only uses county fixed effects). Time is measured in days before and after the murder of George Floyd. There are no BLM protests in the days leading up to the murder of GF, hence the precise zero for pre-periods. Event date is the same for all counties and set to May 25th 2020, the day of the murder of George Floyd. In panel a, the policy is a dummy variable that switches on for days t post George Floyd. In panel b, we interact the policy with the log of one plus the number of new Twitter users created after the outbreak of the pandemic but before the murder of George Floyd in a sample collected before Floyd's murder.

Table 1: **Push factor Z_c: Super Spreader Events (SSEs) increase social media adoption during the pandemic**

	1st PC (col.2-6) **Social Media Index** (1)	new Twitter users (2)	log new Twitter users (3)	Google search for Twitter (4)	new BLM Twitter users (5)	log new BLM Twitter users (6)	Time spent at home (7)
$Z_c = \sum_{w=1}^{t-6} \text{SSE}\ {}^{\leq 50km}_{-c,w}$	0.0159**	0.0804*	0.00795*	0.136*	0.197*	0.00746*	0.0251***
	(0.00719)	(0.0414)	(0.00445)	(0.0711)	(0.100)	(0.00441)	(0.00629)
Observations	2,730	2,767	2,767	2,730	2,767	2,767	1,022
R-squared	0.106	0.022	0.143	0.079	0.023	0.190	0.621
Mean of dep. var.	0	1.238	0.445	60.52	2.486	0.630	10.01
County controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Estimation results corresponding to specification 1. Outcome in column 1 is the first principle component of all outcomes in columns 2 to 6. Z_c is measured as the cumulative number of SSEs within 50 km of the county border but not within the county until six weeks before the murder of George Floyd, i.e. until early April 2020. Outcomes in columns 2 and 3 are based on a random sample of geolocalized Tweets, using the most common 100 English words. In column 3, we present the relative Google search intensity for Twitter in the month of April 2020, normalized such that interest is between 0 (no interest) and 100 (most interested county). Outcomes in columns 5 and 6 are based on observing users in the universe of geolocalized Tweets that contain BLM-related hashtags and keywords after the murder of George Floyd. Outcome in column 7 is based on the Google mobility index in the week of 18-24 May 2020, which measures how much time individuals spend at home relative to January 3 - February 6 2020, rather than at work, at places of commerce or in parks and recreational facilities. All specifications include state fixed effects. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 2: **Heterogeneity in social media adoption by county characteristics: Super Spreader Events (SSEs) and new Twitter users**

Dep var.	log new Twitter users							
Sample split:	share whites		vote Republican 2016		population density		share below age 25	
	> median	≤ median	> median	≤ median	> median	≤ median	> median	≤ median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(18)
$Z_c = \sum_{w=1}^{t-6} \text{SSE}\ _{-c,w}^{\leq 50km}$	0.00427	0.00268	0.0106***	0.00399	0.00248	0.00758**	0.00304	0.00948***
	(0.00276)	(0.00265)	(0.00388)	(0.00265)	(0.00218)	(0.00290)	(0.00249)	(0.00351)
Observations	1,540	1,565	1,540	1,565	1,539	1,566	1,540	1,565
R-squared	0.286	0.605	0.302	0.569	0.606	0.184	0.576	0.412
Mean of dependent variable	0.431	0.797	0.386	0.841	0.923	0.313	0.772	0.460
County controls	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y

Note: Estimation results from specification 1, using the full sample of counties and split by above and below median for multiple county level variables from the American Community Survey of 2018 and from 2016 election results: population share of whites, vote share for Republicans in the 2016 presidential election, population density, and age of population. Median is calculated within states. Dependent variable is the log of one plus the number of new Twitter accounts created between January and May 2020, based on a random sample of geo-located Tweets. State fixed effects and baseline set of controls are included in all regressions. Treatment Z_c is measured as the cumulative number of SSEs less than 50km from the county border but not within the county until six weeks before the murder of George Floyd. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: **Push-pull Instrument ($Z_c \times N_c$): new Twitter users increase BLM protest**

2SLS:	1st PC BLM protest (1)	at least one BLM protest (2)	number of BLM protests (3)	total participants (4)	Tweets BLM (5)	followers @BLM (6)	1st PC BLM protest (7)
new Twitter users	2.433***	0.243**	0.323**	48.76	1,892***	171.3***	3.309**
	(0.500)	(0.0959)	(0.152)	(63.19)	(624.0)	(42.74)	(1.288)
old Twitter users							-0.593
							(0.687)
OLS:							
new Twitter users	0.722***	0.0506***	0.0745***	21.98***	713.6**	59.03**	0.489***
	(0.175)	(0.00957)	(0.0161)	(4.964)	(312.8)	(26.37)	(0.153)
old Twitter users							0.214***
							(0.0334)
Observations	2767	2767	2767	2767	2767	2767	2767
Mean dep. var.	0	0.0477	0.0636	21.03	183.3	16.19	0
Kleibergen-Paap F stat	9.587	9.587	9.587	9.587	9.587	9.587	3.584
Reduced form $Z_c \times N_c$	0.0241	0.00439	0.00453	0.00914	7.652	0.835	0.0241
	(0.0121)	(0.00172)	(0.00399)	(1.010)	(5.180)	(0.576)	(0.0121)
First stage $Z_c \times N_c$	0.00617	0.00617	0.00617	0.00617	0.00617	0.00617	0.00298
	(0.00286)	(0.00286)	(0.00286)	(0.00286)	(0.00286)	(0.00286)	(0.00466)
County controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Estimation results from specification 3. New Twitter users are measured as the log of one plus new geo-located accounts at the county level created after the beginning of the pandemic but before George Floyd's murder based on a random sample of Tweets. Old Twitter users are the same but created account before January 20th 2020. Instrument $Z_c \times N_c$ is the push-pull instrument for pandemic Twitter take-up, combining pandemic exposure Z_c with baseline Twitter penetration N_c from Müller and Schwarz (2023). Outcome in column 1 is the first principle component of all outcomes used in columns 2 to 6. Columns 2 to 4 use protest information from *Elephrame*, reporting respectively a dummy variable for any BLM-related protest in the three weeks following the murder of George Floyd, the number of these protests and the total number of participants. Column 5 reports the number of geo-located Tweets that use at least one BLM-related hashtag or keyword in the three weeks following the murder. Column 6 reports the number of geo-located accounts that follow the official BLM account @BlkLivesMatter. Last column instruments both new and old users with the push-pull instrument. All specifications include state fixed effects. They also include Z_c and N_c separately. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. We report Kleibergen-Paap rkWald F statistic, first stage coefficients and standard errors. Standard errors (in parentheses) are clustered at the state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: **Difference in differences: new Twitter Users increase BLM protest**

outcome:	cumul. BLM	daily BLM	daily BLM	daily BLM	daily BLM	daily BLM
sample:	full sample	full sample	no BLM before	has BLM before	full sample	full sample
estimation:	OLS	OLS	OLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Post GF × new Twitter users	0.224***	0.0345***	0.00591***	0.0798***	0.0488***	0.0330***
	(0.0235)	(0.00343)	(0.000917)	(0.00839)	(0.00718)	(0.00613)
Post GF × old Twitter users						0.00111
						(0.00282)
Observations	86996	86996	77560	9268	86996	86996
No. of counties	3108	3108	2771	337	3108	3108
Mean dep. var.	0.0339	0.0339	0.00153	0.0400	0.00570	0.00570
Kleibergen-Paap F stat					84.77	
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State-day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Difference in difference estimation with county and state-day fixed effects for the 13 days before and after the murder of George Floyd (GF) on May 25th 2020. Treatment is the interaction between a dummy for the post GF period and the log of one plus the number of new Twitter accounts created between January 21st and May 4th 2020. Post dummy is included in the set of controls and Twitter use is captured with the county fixed effects. Column 1 is equivalent to event study results presented in Figure 5, using the cumulative number of BLM protest in the days following the murder of GF. Columns 2 to 6 use any BLM protest on day t as an outcome variable. We distinguish between counties without any BLM protest and those with at least one BLM protest between 2014 and 2019 in columns 3 and 4 respectively. In column 5, we predict the new Twitter users with our push-pull instrument $Z_c \times N_c$, combining pandemic exposure in the form of cumulative SSEs 6 weeks before the murder of George Floyd Z_c and the SXSW-associated early Twitter penetration N_c interacted with the post dummy. We also include the respective interaction between Z_c and N_c and the post dummy. Column 6 also includes the interaction between the post dummy and baseline Twitter users, measured as the log of one plus the number of geo-localized Twitter accounts in December 2019 from a random sample of Tweets. F-stats are reported at the bottom of the table. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: **Individual-level evidence on social media use and political preferences: Cooperative Election Study November 2020**

	(1) has protested for any cause	(2) political activity on social media	(3) voted for Biden in 2020	(4) PC1 police	(5) PC1 racism
2SLS:					
Social media use	0.684***	1.568***	0.616**	-0.919	-1.237
	(0.250)	(0.317)	(0.299)	(0.919)	(0.987)
OLS:					
Social media use	0.0453***	0.703***	0.0438***	-0.268***	-0.244***
	(0.00272)	(0.00337)	(0.00528)	(0.0179)	(0.0175)
Reduced form:	0.00247**	0.00366**	0.000915	-0.00504	-0.00218
$Z_c \times N_c \times \ Age_i$	(0.00119)	(0.00182)	(0.00164)	(0.00655)	(0.00694)
Observations	48,420	48,420	48,420	48,349	47,130
Mean dep. var.	0.0784	0.529	0.429	0	0
First stage coef.	0.00330	0.00330	0.00330	0.00332	0.00364
First stage s.e.	(0.00170)	(0.00170)	(0.00170)	(0.00170)	(0.00169)
Kleibergen-Paap F stat	5.758	5.758	5.758	5.853	5.233
Effective F stat	5.425				
Critical value τ= 30%	6.280				
Individual controls	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes

Note: Individual level regression following specification 6 with county fixed effects and individual controls, including age, gender, employment status, number of children, dummies for religious affiliation, dummies for race and citizenship status. Outcomes are taken from the Cooperative Congressional Election Study conducted in November of 2020. Column 1 is dummy variable for whether respondent has attended a protest, march or demonstration in the past year. Column 2 is a dummy variable for whether the respondent has used social media in the past 24 hours to post or forward a story, video or link about politics, or to post a comment about politics, or to watch a video about politics. Column 3 is the first principle component of question that demand the oversight of police including allowing individuals and their families to sue police officers, end the DoD program that sends military surplus to police departments, create a national registry of police who have been investigated, ban the use of choke holds, decrease the number of police officers, require police to wear body cameras, and eliminate minimum sentencing for drug offenses. Column 4 is the first principle component of anti-racist attitudes, including that white people have advantages in society, that racial problems are not isolated situations, that slavery created conditions that make it hard for Blacks to advance, that the success of other minorities does not prove that Blacks can do the same. Treatment is measured as the use of social media in the past 24 hours. The instrument is the previous push-pull instrument $Z_c \times N_c$ interacted with the age of the respondent. We also include the respective interaction between Z_c and N_c and the respondent's age. F-stats are reported at the bottom of the table. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: **Individual-level evidence on pandemic exposure, news consumption and attitudes: PEW June 2020**

	News consumption		**Attitudes towards BLM**		**Other Attitudes**	
	Follow news about GF	Receive news about GF on social media	BLM protest to justify criminal behavior	BLM protest because of structural racism	Higher Black COVID hospitaliz. not their fault	Rights of undocumented migrants
	(1)	(2)	(3)	(4)	(5)	(6)
COVID-19 deaths per capita [category]	0.0445***	0.0252*	-0.0195*	0.0240***	0.00612	-0.00496
	(0.00952)	(0.0152)	(0.0107)	(0.00886)	(0.00629)	(0.00531)
Observations	9,048	9,048	9,048	9,048	9,048	9,048
R-squared	0.066	0.152	0.145	0.104	0.129	0.082
Mean dep. var.	3.336	2.721	3.341	3.600	1.605	1.797
Black	Y	Y	Y	Y	Y	Y
Metropolitan area	Y	Y	Y	Y	Y	Y
Female	Y	Y	Y	Y	Y	Y
Democrat	Y	Y	Y	Y	Y	Y
Age [category]	Y	Y	Y	Y	Y	Y
Education [category]	Y	Y	Y	Y	Y	Y
Income [category]	Y	Y	Y	Y	Y	Y

Note: Individual-level regressions of COVID-19 related deaths at the county level measured as categories low [1], medium [2] and high [3] on various attitudinal outcomes. PEW does not provide exact location of respondent and only provides coarse info on pandemic exposure (i.e. three categories). All columns include controls for various characteristics of the repondent: a dummy for Black, living in a metropolitan area, identifying as female, as Democrat, and for levels of age (18-29, 30-49, 50-64, 65+), education (high school or less, some college, college graduate +) and income (30K or less, 30-75K, 75 or more). Outcomes are measured as dummy variables based on the following questions. column 1: *"How closely have you been following news about the demonstrations around the country to protest the death of George Floyd, a black man who died while in police custody?"*; column 2: *How much, if any, news and information about the demonstrations to protest the death of George Floyd have you been getting on social media (such as Facebook, Twitter, or Instagram)?*; column 3: *How much, if at all, do you think each of the following has contributed to the demonstrations to protest the death of George Floyd? Some people taking advantage of the situation to engage in criminal behavior*; column 4: *How much, if at all, do you think each of the following has contributed to the demonstrations to protest the death of George Floyd? Longstanding concerns about the treatment of black people in the country*; column 5:*Do you think the reasons why black people in our country have been hospitalized with COVID-19 at higher rates than other racial or ethnic groups have more to do with... Circumstances beyond people's control*; column 6: *Which comes closer to your view about how to handle undocumented immigrants who are now living in the U.S.? There should be a way for them to stay in the country legally, if certain requirements are met.* *** p<0.01, ** p<0.05, * p<0.1

Table 7: **New Twitter users and other protest: graffiti, counter mobilization, and COVID-19 protest**

2SLS:	George Floyd Street Art (1)	All 2020 BLM protest (2)	All 2020 other protest (3)	Populist & pro-Trump (4)	Anti mask & distancing (5)	All Lives Matter Tweets (6)	Blue Lives Matter Tweets (7)
new Twitter users	-0.120	4.917***	6.163***	0.0337	3.124***	517.3**	59.32**
	(0.113)	(1.774)	(2.034)	(0.120)	(1.016)	(226.3)	(23.48)
OLS:							
new Twitter users	0.000442	1.056***	1.055***	0.0672***	0.373***	192.0**	24.03**
	(0.00229)	(0.163)	(0.176)	(0.0154)	(0.0747)	(95.10)	(10.06)
Observations	2767	2767	2767	2767	2767	2767	2767
Mean dep. var.	0.00939	1.314	1.811	0.0549	0.365	47.49	6.125
Kleibergen-Paap F stat	9.587	9.587	9.587	9.587	9.587	9.587	9.587
County controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Estimation results from specification 3. New Twitter users are measured as the log of one plus new geo-located accounts at the county level created after the beginning of the pandemic but before George Floyd's murder based on a random sample of Tweets. Instrument $Z_c \times N_c$ is the push-pull instrument for pandemic Twitter take-up, combining pandemic exposure Z_c with baseline Twitter penetration N_c from Müller and Schwarz (2023). Outcome in column 1 is scraped from the Urban Art Mapping George Floyd and Anti-Racist Street Art database. Outcomes in columns 2 to 5 come from the ACLED US Crisis Monitor 2020 and count the number of protests associated with each topic. Outcomes in column 6 and 7 are the number of geo-localized Tweets that contain the hashtags or phrases #AllLivesMatter and #BlueLivesMatter. All specifications include state fixed effects. They also include Z_c and N_c separately. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. We report Kleibergen-Paap rkWald F statistic. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix

Contents

Appendix A: Data Appendix

A.1 Description of Data Sources

A.1.1 Pandemic-related Data

Super spreading events. Our identification strategy relies, in part, on records of SSE in the early stages of the pandemic. In this section, we discuss the limitations of the SSE data set and how we address these in the empirical section. The data set is collected from various sources by researchers from the London School of Hygiene and Tropical Medicine and published as a free access data base for researchers and the media under the *SARS-CoV-2 Superspreading Events from Around the World* Project.

A main challenge in the construction of this data base is that there is no standard definition of a SSE. The database mainly refers to "outbreak" and "clusters" for which they use the UK Government Public Health Definition: "two or more test-confirmed cases of COVID-19 among individuals associated with a specific non-residential setting with illness onset dates within a 14-day period." The outbreak definition is expanded to "identified direct exposure between at least 2 of the test-confirmed cases in that setting (for example under one metre face to face, or spending more than 15 minutes within 2 metres) during the infectious period of one of the cases when there is no sustained local community transmission - absence of an alternative source of infection outside the setting for the initially identified cases". In our data set, the minimum number of cases associated to a SSE is 3 with only 3 SSE having less than 5 associated cases.

The data base draws from one main source: Leclerc et al. (2020) who performed a systematic review of available literature and media reports to find settings reported in peer reviewed articles and media with "outbreak" or "cluster" characteristics. There were various extensions to this data set, using articles of journalists, expanding that data set to second and third generation events by Swinkels (2020), and including the Western Pacific Region for a project of the World Health Organisation (under the project lead of Fatim Lakha, also from the London School of Tropical Medicine and Hygiene). We will primarily draw from Leclerc et al. (2020), as we focus on SSEs in the United States during the early stages of the pandemic. Note that the data we use was downloaded at the beginning of 2021 and may not include updates or corrections made afterwards.

There are various limitations in the measurement of SSEs. First, there exists some uncertainty about the exact date of the SSE. If, for instance, there was a COVID-19 cluster at a worker dormitory, the exact date of the transmission event is difficult to narrow down. In these cases, researchers make an approximation based on the timing of tests and overall case numbers. We address this concern by using the cumulative number of SSEs until a certain cut-off date (first week of April in the baseline version of the instrument), thereby not relying on the specific timing of the SSE. Second, for many SSEs it is not known exactly how many people were infected (either directly at the SSE or by somebody who was infected at the initial SSE). The database always uses the lowest number cited in the articles about the SSE but actual numbers can be much higher. The actual detected number of cases will be related to testing capacity and potentially other unobserved factors at the county level. For this reason, we use the most simple version of the instrument, i.e. counting the number of SSEs rather than using the cases associated with the SSE. Third, the GPS coordinates of SSEs are almost always approximate. For instance, when an SSE occurred somewhere in city A, typically the database uses GPS coordinates for a random location within that city, not for the precise location. In a robustness check, we verifiy that our results are not sensitive to changing the radius around SSEs to account for potential measurement error. Overall, the measurement error in SSEs would only bias our results if it is somehow related to the counties' overall propensity to join Twitter or to protest (and is not captured in the set of controls or state fixed effects). One important exercise addresses this concern: SSEs do not predict baseline social media penetration (Table B3) or past BLM events (Table B4, columnn 2). If SSEs were disproportionately recorded in places with a higher likelihood of Twitter adoption we should see a systematic relationship to previous Twitter adoption, which is not the case. Similarly, if SSEs were disproportionately recorded in places with a higher likelihood of a BLM event occurring, we should see a systematic relationship to previous BLM protest, which, again, is not the case.

COVID-19. Data on COVID-19 related deaths and cases in the USA at the county level comes from the New York Times. This data set provides the cumulative count of cases and deaths every day for each county in the USA, starting from January 21, 2020 when the country's first COVID-19 case was reported. A key limitation of COVID-19 cases data is that it depends on the testing facilities and the availability of test kits in the region. We therefore mainly rely on COVID-19 related deaths as a measure of exposure to the pandemic. We also obtain data on daily COVID-19 hospitalizations and deaths by race and ethnicity at the state-level from the Center for Disease Control and Prevention.

Community resilience. One of the most important COVID-19 related control variables used in our empirical analysis is the ability of counties to cope with the pandemic. This variable comes from the United States Census Bureau's Community Resilience Estimates. These estimates measure the capacity of individuals and households to absorb, endure, and recover from the health, social, and economic impacts of a disaster such as a hurricane or a pandemic. For each county the population living under each of 11 risk factors is estimated and these factors are aggregated into 3 composite risk factors: (i) population with 0 risk factors; (ii) population with 1-2 risk factors, and (iii) population with 3 or more risk factors. These risk factors are based on households' and individuals' socio-economic and health conditions. Risk factors include: Income-to-Poverty Ratio, single or zero caregiver household, unit-level crowding defined as > 0.75 persons per room, communication barriers (defined as either limited English-speaking households or no one in the household over the age of 16 with a high school diploma), no one in the household is employed full-time, disability posing constraint to significant life activity, no health insurance coverage, being aged 65 years or older, households without a vehicle and households without broadband Internet access. For our analysis we look at populations within each county that are classified as living under 3 or more risk factors.

Lockdown stringency. We use data from the Oxford COVID-19 Government Response Tracker (Hale et al., 2020) to measure the restrictiveness of the government's pandemic policy. Use of this data is inspired by recent work which shows that stringent policies lead to lower mortality, mobility and consequently spread of infection during the pandemic (Jinjarak et al., 2020; Askitas et al., 2020). This data provides four key indices (i) an overall government response index, (ii) a containment health index, (iii) an economic support index, and (iv) an original stringency index which captures the strictness of lockdown-style policies. Each of this indices reports values between 1 and 100 and varies across states and weeks.

A.1.2 Social Media and Protest Data

New and old Twitter users. In March of 2021, we collected through the Twitter API a random sample of 3 million English-language tweets posted between May 4 and May 24 2020, by searching for tweets containing the 100 most common words in English at random instants in the sampling period. In addition, through a similar procedure, we collected one million tweets from 765,000 users containing the word "the" during random intervals in a the week of December 1-7 2019. Each observation (each tweet) contains the text and timestamp of the tweet, the name and user identifier of the profile, the creation date of the profile and in some cases the location of the Twitter profile (more on geo-location below). Old users are defined as those Twitter profiles from the 2020 random sample of tweets that have created their profile before December 31st 2019. New users are defined as those that created their profile between January 21st 2020 (after the outbreak of the pandemic) and before May 25th 2020 (before the murder of George Floyd). Pre-existing Twitter users in 2019 are defined as the users seen in the December 2019 sample. In order to replicate the Müller and Schwarz (2023) instrument, in November 2021, we also collected the locations and profile creation dates of all of 639,915 followers of the South by South-West Festival @SXSW Twitter account.

Twitter in the three weeks following George Floyd. We collected tweets using the Twitter Academic Research API. In particular, we collected the universe of tweets that contain the keywords "BLM", "Black Lives Matter", "Black Life Matters" or "George Floyd",[25] including retweets, between May 25 and June 14. For each tweet, we extract the time and text of the tweet, the user, the user's stated location, and account creation date. From this, we construct a measure for online protest which is the (log) number of geo-localized, BLM related tweets in the three weeks following the murder of George Floyd. Based on this data, we also construct the (log) new BLM Twitter users, which are the Twitter accounts that end up tweeting about BLM in the three weeks following the murder of George Floyd but have created their account between January 21st and May 24th of 2020.

[25]These keywords are considered both in when appearing separated with space, or without spaces as a hashtag (e.g. #BlackLivesMatter)

BLM account followers. As an additional outcome, we use the number of all followers of the official BLM account @Blklivesmatter. We collected the followers and their geolocation in February 2022. This gap between the period of analysis and the date of data collection can lead to measurement error because we do not know the starting date of following. Accounts that followed the official BLM account may stop following it and accounts that are computed as followers may start following just a few hours before the collection. Similarly, geolocation of accounts may have changed between the period of study and the date of data collection.

Geo-location of tweets. We follow the literature in assigning the location of a tweet or a user by extracting information on their self-reported location from their Twitter profile (Enikolopov et al., 2020; Takhteyev et al., 2012; Müller and Schwarz, 2023). Not all users report a location and among those who do, not all state a valid location (e.g., "in the heart of Justin Bieber") so we restrict the sample to the users that state a valid location that can be matched to a USA county (in particular, we exclude users whose location only mentions a state). The location is an arbitrary text field which is not meant to be machine-readable. We use the Nominatim geocoding engine (based on the Open Street Map database) to find the coordinates of the most likely match for the location. We then filter out all locations outside the US and all locations that are too vague (i.e. that map the whole country or a whole state). Finally, we map these coordinates to counties using the US Census Bureau cartographic boundary files. Across our different tweet collections, we end up with 23.3 million tweets. This approach has clear limitations as it relies only on self-reported locations and may not be representative of the whole Twitter universe. We report summary stats on the counties for which we were able to assign tweets and compare them to the characteristics of the full set of counties in Table A2. We would be particularly concerned if counties with geolocalizable tweets were substantially different from other counties. Reassuringly, counties without localizable tweets only form a tiny minority: out of the 3107 counties in our universe, only 47 (1.5%) are not attributed any tweet.

Google mobility. We use data on mobility, collected through mobile phones that use Google apps (such as Google Maps). This data collects information on the time a person spent on certain mobility tasks like the time spent in parks, being at home, doing groceries, in the transit stations and finally at their workplace (as identified by Google). This information is then aggregated at the county level to measure the aggregate daily mobility. It is constructed relative to the average mobility on the same days of the week in the January 3 - February 6 2020 period. We use this data for the period between March 1st 2020 and May 24th 2020.

Google searches. We also use the Google Trends data to analyze patterns of search activity before and after the death of George Floyd. Each variable is a normalized index of search activity for a given search term. The indices are specified on a Nielsen's Designated Market Area (DMA) level. A DMA is a region of the United States that consists of counties and ZIP-codes. There are 210 DMA regions covering the US. Search activity is averaged across the period of interest: each observation is a number of the searches of the given term divided by the total searches of the geography and time range, which is then normalized between regions such that the region with the largest measure is set to 100. The important limitation of the Google Trends data is that an index of search activity is an integer from zero to one hundred with an unreported privacy threshold. We use the search term Twitter for the five weeks leading up to the murder of George Floyd. We also use the search term BLM for the three weeks before Floyd's murder.

BLM protest Elephrame. Elephrame is a crowd-sourced platform that collects data on Black Lives Matter and other protests. It provides information on the place and date of each BLM protest and estimated number of participants, as well as a link to a news article covering the protest. The observation period starts with the first BLM demonstration for Eric Garner on 7/19/2014 and consists of any public demonstration or public art installation focused on "communicating the value of a Black individual or Black people as a whole". Each observation is manually collected from sources that include press, protest organizers, participants and observers. We extracted all protest records from August 2014 to September 2020 and geo-coded their location.

BLM and other protest from ACLED US Crisis Monitor. The Armed Conflict Location & Event Data Project (ACLED) dataset compiles instances of political violence and demonstrations across the United States, particularly in response to significant societal and political tensions such as those following the contentious 2020 general election, incidents of police violence, and large-scale protests like those related to the Black Lives Matter movement. The dataset's assembly involves the aggregation and coding of events based on a diverse array of sources, including over 2800 national, regional, and local media outlets, to ensure comprehensive coverage and mitigate biases inherent in any single source. Major variables in the ACLED dataset encompass detailed codings of event types (e.g., protests, violence against civilians, strategic developments), locations (with specificity down to the city level and, in certain cases, sub-city regions in major cities like New York and Los Angeles), dates, and actors involved, including both state forces and non-state actors like militias and protest groups. The dataset distinguishes between various security forces (e.g., police, national guard) and non-state actors, including armed groups and militias, with a nuanced classification system that accounts for the complexities of political violence and protest actions within the U.S. context. ACLED reports 21,582 protest in the United States in 2020. 40% of the protests in the data set are linked to the pandemic.

George Floyd Street Art. We extract information on the location of street art representing or referring to George Floyd from the Urban Art Mapping George Floyd and Anti-Racist Street Art database. The crowd-sourced website run by researchers from the University of St. Thomas documents street art from around the world created in the aftermath of the murder of George Floyd. Their archive is a repository of images made available for research and education. The website contains geo-tagged information and images of George Floyd related street art, which we match to counties. The data does not contain time stamps and has no information on when these images were added. For this reason, we can only interpret the street art as cross-sectional snapshots at time of accessing the website in January of 2022. Overall, we record 2183 images across 70 counties. Most of the images (1467) are recorded in Minneapolis.

A.1.3 Survey Data and other County Controls

The American Trends Panel survey by Pew Research Center. To zoom in and move from county to individual level analysis, we employ the American Trends Panel survey (ATP), conducted by Pew Research Center. The panel is based on a representative sample of U.S. adults who participate via self-administrated online web-survey. Participants with no internet access were provided with tablets and wireless connection to answer the survey, which is crucial for studying the effects of social media. For our analysis we draw from the panel wave 68 that took place from June 4 to June 10, 2020. Participants were questioned on a wide range of topics, including the Black Lives Matter movement, police brutality, ideologies and politicians, race relations, social issues, the coronavirus and president Donald Trump. The survey also contains a group of demographic variables, which are included in our analysis as controls: race, age, gender, education, income, political leanings, level of urbanisation of participants' region. It is important to note that the participants' region of residence is anonymised, therefore the exact data on COVID-19 cases and deaths is not available. However, the panel does include a categorical version of this data: whether the prevalence of COVID-19 in the respondents' region is low, medium or high. We can make only associative conclusions based on this limited information.

The Cooperative Election Study. The 2020 Cooperative Election Study (CES), spearheaded by a collaboration of 60 research teams, offers an expansive dataset from a survey administered to 61,000 participants across the United States. This study, facilitated by YouGov, was designed to capture a wide array of information regarding Americans' voting behaviors, political attitudes, and electoral experiences in the context of the 2020 elections. The CES was conducted in two waves to capture both pre-election and post-election sentiments among U.S. voters. The pre-election wave of the survey took place from September 29 to November 2, 2020, encompassing the crucial final weeks leading up to the election. Following the election, the post-election wave was conducted from November 8 to December 14, 2020, allowing for the collection of respondents' reactions to the election outcomes, their voting experiences, and their perspectives on the electoral process. The survey has information on the location of respondents at the ZIP code level which we then merge to counties in our data set.

Use of deadly force by police. We obtain this from the collaborative platform Fatal Encounters. This data is collected by a multi-disciplinary team at the University of Southern California. The results are published as part of the *National Officer-Involved Homicide Database*. The data is available from

2000 onward and contains the name, gender, race, and age of each victim and the specific address where the death occurred, among other variables.

Additional county-level controls. We include unemployment data available on a monthly basis at the county level from the Local Area Unemployment Statistics of the US Bureau of Labor Statistics and the total population, population by ethnicity, and income statistics such as Black poverty rate and median household income (all in 2018), as well as past Republican vote share (in 2012 and 2016) from the American Community Survey. We use a dummy for urban counties which is constructed from the Office of Management and Budget's February 2013 delineation of metropolitan and micropolitan statistical areas.[26] The measure of social capital that we use aggregates the information on the number of local organizations.[27]

[26] 2013 NCHS Urban-Rural Classification Scheme for Counties, Vintage 2012 postcensal estimates of the resident U.S. population. NCHS Urbanization levels are designed to be convenient for studying the difference in health across urban and rural ares. This classification has 6 categories: large "center" metropolitan area (*inner cities*), large "fringe" metropolitan area (*suburbs*), median metropolitan area, small metropolitan area, micropolitan area and non-core (non-metropolitan counties that are not in a micropolitan area).

[27] This includes: (a) civic organizations; (b) bowling centers; (c) golf clubs; (d) fitness centers; (e) sports organizations; (f) religious organizations; (g) political organizations; (h) labor organizations; (i) business organizations; and (j) professional organizations.

Table A1: Sources of the variables used in the analysis

Name	Exact Definition	Geographic Unit	Time Frame	Source
COVID cases and deaths	Cumulative cases and deaths attributed to COVID-19	County	Daily, January 21 to August 20, 2020	NYTimes, `https://github.com/nytimes/covid-19-data`
BLM protests	Number of BLM protests on each day and estimated number of participants	County	August 2014 to August 20, 2020	Elephrame
Superspreader events	Timing and location of known superspreader events	County	January to August 2020	Swinkels, K. (2020). SARS-CoV-2 Superspreading Events Around the World [Google Sheet]. Retrieved from `www.superspreadingdatabase.com`
New Twitter users	Number of distinct users whose account was created after January 21, 2020 from the county in a random sample of 3 million English language tweets posted between May 4 and May 24, 2020. Geolocation based on the location indicated in the user's profile.	County	May 4 - May 24, 2020	Twitter API
Old Twitter users	Number of distinct users whose account was created before January 21, 2020 from the county in a random sample of 3 million English language tweets posted between May 4 and May 24, 2020. Geolocation based on the location indicated in the user's profile.	County	May 4 - May 24, 2020	Twitter API
Preexisting Twitter users in 2019	Number of distinct users appearing in a random sample of 1 million English language tweets posted between December 1 and December 7 2020. Geolocation based on the location indicated in the user's profile.	County	December 1 - 7, 2019	Twitter API
New BLM Twitter users	Number of distinct users from the county whose account was created between January 21 and May 24, 2020 and using keywords related to Black Lives Matter in the 3 weeks following the murder of George Floyd. Geolocation based on the location indicated in the user's profile.	County	January 21 to May 24, 2020	Twitter API
Google searches for Twitter	Index indicating the importance of searches for "Twitter" among all Google searches, averaged in the 5 weeks before the murder of George Floyd.	Designated Marked Area	April 20 - May 24, 2020	Google Trends API
Time spent at home	Time spent at home, compared to pre-pandemic baseline, in the week before the murder of George Floyd	County	May 18 - May 24, 2020	Google Community Mobility Report
Share of white population		County	2018	American Community Survey
Population density		County	2018	American Community Survey
Share of population below age 25		County	2018	American Community Survey

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Name	Exact Definition	Geographic Unit	Time Frame	Source
Tweets BLM	Number of tweets mentioning keywords related to Black Lives Matter in the 3 weeks following the murder of George Floyd. Geolocation based on the location indicated in the user's profile.	County	May 25 - June 14, 2020	Twitter API
Followers @BLM	Number of users geolocated in the county and following the @Blklivesmatter Twitter account.	County		Twitter API
Cooperative Election Study 2020 survey results	See Table A12 for exact variables	County	2020	CCES
PEW June 2020 survey results	See notes of Table 6	County	June 2020	Pew Research Center American Trends Panel - Wave 68
George Floyd street art	Number of pieces of street art related to the murder of George Floyd. Geolocated according to the map embedded in the web page.	County		Urban Art Mapping: George Floyd and Anti-Racist Street Art database
All 2020 BLM protest	Number of entries in the ACLED US Crisis Monitor data whose associated actor or notes contain "BLM", and occurring after the murder of George Floyd.	County	May 25 - December 31, 2020	ACLED US Crisis Monitor
All 2020 other protest	Number of entries in the ACLED US Crisis Monitor data whose associated actor or notes does not contain "BLM", and occurring after the murder of George Floyd.	County	May 25 - December 31, 2020	ACLED US Crisis Monitor
Populist and pro-Trump protests	Number of entries in the ACLED US Crisis Monitor data occurring after the murder of George Floyd, and whose associated actors or notes contain one of the following: Stop the Steal, Proud Boys, QAnon, election fraud, ballot, pro-Trump, Trump supp, in support of Trump, WAF, or MAGA.	County	May 25 - December 31, 2020	ACLED US Crisis Monitor
Anti-mask protests	Number of entries in the ACLED US Crisis Monitor data occurring after the murder of George Floyd, and whose associated actors or notes contain one of the following: pandemic, mask mandate, social distancing, coronavirus, public health order.	County	May 25 - December 31, 2020	ACLED US Crisis Monitor
All Lives Matter tweets	Tweets containing "All Lives Matter" or #AllLivesMatter, posted in the 3 weeks following the murder of George Floyd. Geolocation based on the location indicated in the poster's profile.	County	May 25 - June 14, 2020	Twitter API
Blue Lives Matter tweets	Tweets containing "Blue Lives Matter" or #BlueLivesMatter, posted in the 3 weeks following the murder of George Floyd. Geolocation based on the location indicated in the poster's profile.	County	May 25 - June 14, 2020	Twitter API
Distance to Minneapolis	Geographical distance, in kilometers, from the center of the county to the center of Minneapolis.	County		Coordinates: Census Bureau

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Name	Exact Definition	Geographic Unit	Time Frame	Source
Google searches for BLM/Black Lives Matter	Index indicating the importance of searches for "BLM"/"Black Lives Mayyer" among all Google searches, averaged in the 3 weeks prior to the murder of George Floyd.	Designated Marked Area	May 25 - June 15, 2020	Google Trends API
Lockdown stringency	Measure of the strength of the government response, on May 24.	State	May 24	OxCGRT government response index
SXSW users, Pre-SXSW users	Followers of the @SXSW Twitter account that joined the network, respectively, in March 2007, and before March 2007. Geolocation based on the location indicated in the user's profile.	County	2007	Twitter API
Police-caused deaths, 2014-2019	Number of Black people who died during an encounter with the police between 2014 and 2019	County	2014-2019	Fatal Encounters
Police-caused deaths, 2020	Number of Black people who died during an encounter with the police, in 2020 before May 25th	County	January 1 - May 24, 2020	Fatal Encounters
Unemployment, 2019-2020	Mean unemployment in the 12 months from May 2019 to April 2020	County	May 2019 - April, 2020	Bureau of Labor Statistics: Local Area Unemployment Statistics
Share of Black population		County	2018	American Community Survey
Black poverty rate		County	2018	American Community Survey
Urbanization level	From 1 = urban to 6 = rural	County	2013	NCHS Urban-Rural Classification Scheme for Counties
Resilience: population with 3+ risk factors	Measure of the capacity of individuals and households to absorb, endure, and recover from the health, social, and economic impacts of a disaster such as a hurricane or a pandemic. For each county the population living under each of 11 risk factors is estimated and these factors are aggregated. This variable measures the share of the population with 3 risk factors or more.	County	2018	Census Bureau Community Resilience Estimates
Median household income		County	2016	Census Bureau Opportunity Atlas
Republican vote in 2012	Share of the vote in the 2012 presidential election that was a vote for Mitt Romney.	County	2012	MIT Election Data and Science Lab (2018)
Republican vote in 2016	Share of the vote in the 2016 presidential election that was a vote for Donald Trump.	County	2016	MIT Election Data and Science Lab (2018)
Social capital	Total number of social organizations (religious, cultural, political, etc) divided by population	County	2014	Rupasingha et al. (2006)

Table A2: Summary statistics - counties with and without geo-localized Tweets

	All counties					**Counties with Tweets**					**Counties without Tweets**				
From 25th of May to 14th of June 2020:	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Presence of BLM events	3107	0.099	0.299	0.000	1.000	3060	0.101	0.301	0.000	1.000	47	0.000	0.000	0.000	0.000
Number of BLM events	3107	0.256	1.381	0.000	36.000	3060	0.259	1.391	0.000	36.000	47	0.000	0.000	0.000	0.000
Participants in BLM events	3107	279.777	5988.694	0.000	323687.500	3060	284.075	6034.424	0.000	323687.500	47	0.000	0.000	0.000	0.000
Participants per event	308	542.793	879.336	0.000	8991.319	308	542.793	879.336	0.000	8991.319	0	.	.	.	.
Tweets mentioning BLM	3107	639.621	4554.716	0.000	171409.000	3060	649.445	4588.878	0.000	171409.000	47	0.000	0.000	0.000	0.000
New users tweeting about BLM	3107	7.669	49.729	0.000	1839.000	3060	7.787	50.100	0.000	1839.000	47	0.000	0.000	0.000	0.000
Tweets mentioning #AllLivesMatter	3107	137.357	845.598	0.000	28943.000	3060	139.467	851.896	0.000	28943.000	47	0.000	0.000	0.000	0.000
Tweets mentioning #BlueLivesMatter	3107	18.086	114.973	0.000	4117.000	3060	18.364	115.830	0.000	4117.000	47	0.000	0.000	0.000	0.000
Neighbor protested first	3107	0.348	0.476	0.000	1.000	3060	0.352	0.478	0.000	1.000	47	0.085	0.282	0.000	1.000
On the 25th of May 2020:															
COVID deaths (total)	3107	24.592	141.298	0.000	3304.000	3060	24.967	142.347	0.000	3304.000	47	0.170	0.433	0.000	2.000
COVID cases (total)	3107	462.140	2441.670	0.000	72010.000	3060	469.127	2459.700	0.000	72010.000	47	7.234	14.774	0.000	66.000
COVID deaths (per 1000)	3107	0.113	0.248	0.000	2.935	3060	0.114	0.250	0.000	2.935	47	0.034	0.094	0.000	0.493
COVID cases (per 1000)	3107	2.794	5.666	0.000	145.513	3060	2.811	5.695	0.000	145.513	47	1.664	3.088	0.000	15.273
Superspreader events, 6+ weeks ago, neighboring	3107	3.081	9.807	0.000	143.000	3060	3.114	9.871	0.000	143.000	47	0.936	3.158	0.000	20.000
Black death burden	3107	1.346	0.963	0.000	4.104	3060	1.354	0.956	0.000	4.104	47	0.801	1.219	0.000	4.104
Lockdown stringency index	3107	68.451	8.513	47.220	89.810	3060	68.546	8.439	47.220	89.810	47	62.242	10.830	49.070	82.410
Before the 25th of May 2020:															
Google searches for Twitter	3057	61.278	11.242	17.000	100.000	3011	61.385	11.242	17.000	100.000	46	54.283	8.884	41.000	74.000
Residential stay	1349	10.641	3.400	3.600	26.286	1348	10.641	3.401	3.600	26.286	1	11.000	.	11.000	11.000
Later outcomes:															
Followers of @BlkLivesMatter	3107	66.784	533.916	0.000	20058.000	3060	67.807	537.938	0.000	20058.000	47	0.149	0.360	0.000	1.000
Followers of @BlkLivesMatter created during the pandemic	3107	1.596	11.634	0.000	453.000	3060	1.620	11.722	0.000	453.000	47	0.021	0.146	0.000	1.000
Street art count	3107	0.726	26.761	0.000	1467.000	3060	0.737	26.966	0.000	1467.000	47	0.000	0.000	0.000	0.000
County characteristics:															
Black police-related deaths (2014-2019)	3107	0.684	3.227	0.000	84.000	3060	0.694	3.251	0.000	84.000	47	0.021	0.146	0.000	1.000
Black police-related deaths (2020)	3107	0.047	0.301	0.000	6.000	3060	0.048	0.303	0.000	6.000	47	0.000	0.000	0.000	0.000
Unemployment rate (year average)	3107	4.691	1.550	0.708	19.650	3060	4.705	1.548	0.708	19.650	47	3.801	1.483	1.642	10.267
Black population share	3107	0.100	0.147	0.000	0.875	3060	0.100	0.147	0.000	0.875	47	0.063	0.148	0.000	0.654
Non-white population share	3107	0.144	0.162	0.000	0.928	3060	0.145	0.161	0.000	0.928	47	0.136	0.203	0.001	0.757
Large cities	3107	0.020	0.141	0.000	1.000	3060	0.021	0.142	0.000	1.000	47	0.000	0.000	0.000	0.000
Suburban areas	3107	0.118	0.323	0.000	1.000	3060	0.120	0.325	0.000	1.000	47	0.043	0.204	0.000	1.000
Smaller towns	3107	0.234	0.423	0.000	1.000	3060	0.236	0.424	0.000	1.000	47	0.106	0.312	0.000	1.000
Rural areas	3107	0.628	0.484	0.000	1.000	3060	0.624	0.484	0.000	1.000	47	0.851	0.360	0.000	1.000
BLM events (2014-2019)	3107	0.631	4.248	0.000	117.000	3060	0.640	4.280	0.000	117.000	47	0.000	0.000	0.000	0.000
Black poverty rate	3107	0.281	0.225	0.000	1.000	3060	0.282	0.223	0.000	1.000	47	0.221	0.343	0.000	1.000
Population share with 3+ risk factors	3107	25.901	5.019	10.684	48.444	3060	25.876	5.020	10.684	48.444	47	27.551	4.719	19.106	40.584
Vote share for republicans (2016)	3107	0.633	0.156	0.041	0.960	3060	0.632	0.156	0.041	0.946	47	0.732	0.139	0.389	0.960
Vote share for republicans (2012)	3107	0.596	0.148	0.060	0.959	3060	0.595	0.148	0.060	0.959	47	0.676	0.151	0.335	0.897
Median household income (2016)	3107	48806.607	13288.617	20170.891	129150.344	3060	48831.619	13323.342	20170.891	129150.344	47	47178.123	10780.648	24306.000	75938.000
Social capital	3107	1.385	0.705	0.000	6.887	3060	1.374	0.682	0.000	6.887	47	2.046	1.487	0.000	6.071
Distance to Minneapolis	3107	1216.773	555.760	11.998	6474.706	3060	1220.736	555.942	11.998	6474.706	47	958.745	482.766	241.317	1898.310
Log(SXSW followers created before March 2017)	3107	0.115	0.258	0.000	1.474	3060	0.116	0.260	0.000	1.474	47	0.029	0.141	0.000	0.693
Log(SXSW followers created during March 2017)	3107	0.193	0.351	0.000	1.658	3060	0.196	0.352	0.000	1.658	47	0.029	0.141	0.000	0.693

Note: Descriptive statistics of all variables by different sub-samples depending on the presence of geolocalized tweets from any of our tweet datasets. Different panels correspond to different moments at which each variable was measured.

Table A3: Summary statistics for super spreading events by their type

Location of SSE event	Total events	Total Events 6 weeks before GF's murder	Total Cases
Community	11	8	504
Development Center	12	12	1612
Event/group gathering	12	12	450
Industry	113	83	17660
Medical	137	130	13684
Nursing Home	269	255	26527
Prison	186	182	44414
Rehabilitation / Medical	258	246	26776
Restaurant/Bar	5	4	1164
Retail	3	0	46
School	2	2	173
Other	15	13	1337

Note: Descriptive statistics on super spreading events (SSE) by the place where they took place. First column describe the location, second column the total number of events of this category, third column the total number of event in each location during our period of consideration (up until 6 week before the murder of George Floyd), and column 4 the total number of COVID-19 cases registered.

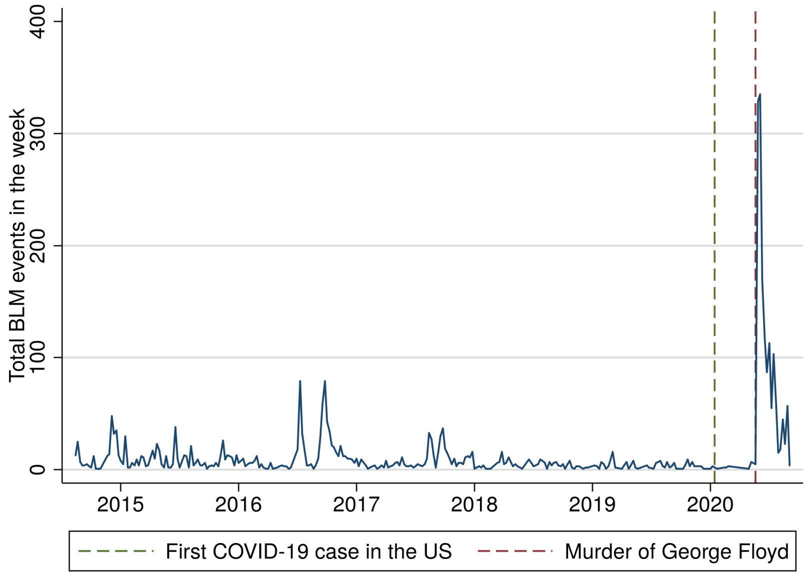
Table A4: Summary statistics - counties with and without prior BLM event

	All counties					No BLM event before					Has BLM event before				
From 25th of May to 14th of June 2020:	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
Presence of BLM events	3106	0.099	0.298	0.000	1.000	2768	0.048	0.213	0.000	1.000	338	0.518	0.500	0.000	1.000
Number of BLM events	3106	0.250	1.348	0.000	36.000	2768	0.064	0.322	0.000	5.000	338	1.778	3.642	0.000	36.000
Participants in BLM events	3106	270.759	5968.521	0.000	323687.500	2768	21.026	172.090	0.000	5500.000	338	2315.911	17979.700	0.000	323687.500
Participants per event	307	539.141	878.429	0.000	8991.319	132	355.115	621.538	0.000	5500.000	175	677.949	1010.498	0.000	8991.319
Tweets mentioning BLM	3106	611.006	4266.999	0.000	171409.000	2768	183.306	1098.817	0.000	48000.000	338	4113.592	12001.456	2.000	171409.000
New users tweeting about BLM	3106	7.461	48.374	0.000	1839.000	2768	2.488	13.971	0.000	624.000	338	48.192	134.500	0.000	1839.000
Tweets mentioning #AllLivesMatter	3106	134.741	833.066	0.000	28943.000	2768	47.488	326.063	0.000	15659.000	338	849.290	2224.119	0.000	28943.000
Tweets mentioning #BlueLivesMatter	3106	17.753	113.478	0.000	4117.000	2768	6.125	36.206	0.000	1647.000	338	112.976	312.535	0.000	4117.000
Neighbor protested first	3106	0.348	0.477	0.000	1.000	2768	0.334	0.472	0.000	1.000	338	0.464	0.499	0.000	1.000
On the 25th of May 2020:															
COVID deaths (total)	3106	24.461	141.132	0.000	3304.000	2768	8.366	46.396	0.000	1025.000	338	156.266	382.483	0.000	3304.000
COVID cases (total)	3106	459.678	2438.202	0.000	72010.000	2768	164.485	663.300	0.000	15169.000	338	2877.112	6677.133	0.000	72010.000
COVID deaths (per 1000)	3106	0.113	0.248	0.000	2.935	2768	0.099	0.230	0.000	2.935	338	0.224	0.345	0.000	2.010
COVID cases (per 1000)	3106	2.791	5.664	0.000	145.513	2768	2.596	5.662	0.000	145.513	338	4.391	5.430	0.000	40.048
Superspreader events, 6+ weeks ago, neighboring	3106	3.070	9.790	0.000	143.000	2768	2.327	7.564	0.000	143.000	338	9.154	19.279	0.000	140.000
Black death burden	3106	1.346	0.963	0.000	4.104	2768	1.344	0.985	0.000	4.104	338	1.363	0.755	0.000	4.104
Lockdown stringency index	3106	68.445	8.508	47.220	89.810	2768	68.210	8.543	47.220	89.810	338	70.367	7.969	47.220	89.810
Before the 25th of May 2020:															
Google searches for Twitter	3056	61.265	11.222	17.000	100.000	2731	60.523	10.941	17.000	100.000	325	67.505	11.623	41.000	100.000
Residential stay	1348	10.633	3.387	3.600	26.286	1023	10.006	3.016	3.600	26.286	325	12.606	3.722	4.429	26.143
Later outcomes:															
Followers of @BlkLivesMatter	3106	63.198	495.174	0.000	20058.000	2768	16.186	94.110	0.000	3304.000	338	448.195	1421.137	0.000	20058.000
Followers of @BlkLivesMatter created during the pandemic	3106	1.540	11.207	0.000	453.000	2768	0.441	2.167	0.000	78.000	338	10.536	32.057	0.000	453.000
Street art count	3106	0.703	26.735	0.000	1467.000	2768	0.009	0.206	0.000	10.000	338	6.382	80.924	0.000	1467.000
County characteristics:															
Black police-related deaths (2014-2019)	3106	0.677	3.207	0.000	84.000	2768	0.207	0.724	0.000	15.000	338	4.527	8.589	0.000	84.000
Black police-related deaths (2020)	3106	0.047	0.301	0.000	6.000	2768	0.014	0.131	0.000	3.000	338	0.314	0.783	0.000	6.000
Unemployment rate (year average)	3106	4.691	1.550	0.708	19.650	2768	4.713	1.575	0.708	17.442	338	4.506	1.323	2.492	19.650
Black population share	3106	0.100	0.147	0.000	0.875	2768	0.093	0.146	0.000	0.875	338	0.157	0.142	0.009	0.727
Non-white population share	3106	0.144	0.162	0.000	0.928	2768	0.134	0.160	0.000	0.928	338	0.231	0.150	0.014	0.801
Large cities	3106	0.020	0.140	0.000	1.000	2768	0.001	0.027	0.000	1.000	338	0.178	0.383	0.000	1.000
Suburban areas	3106	0.118	0.323	0.000	1.000	2768	0.105	0.307	0.000	1.000	338	0.228	0.420	0.000	1.000
Smaller towns	3106	0.234	0.423	0.000	1.000	2768	0.201	0.400	0.000	1.000	338	0.506	0.501	0.000	1.000
Rural areas	3106	0.628	0.483	0.000	1.000	2768	0.694	0.461	0.000	1.000	338	0.089	0.285	0.000	1.000
BLM events (2014-2019)	3106	0.617	4.183	0.000	117.000	2768	0.000	0.000	0.000	0.000	338	5.672	11.510	0.000	117.000
Black poverty rate	3106	0.281	0.225	0.000	1.000	2768	0.283	0.236	0.000	1.000	338	0.263	0.099	0.000	0.600
Population share with 3+ risk factors	3106	25.899	5.019	10.684	48.444	2768	25.957	5.066	10.684	48.444	338	25.428	4.600	11.763	39.456
Vote share for republicans (2016)	3106	0.633	0.156	0.083	0.960	2768	0.656	0.141	0.083	0.960	338	0.446	0.143	0.084	0.818
Vote share for republicans (2012)	3106	0.596	0.148	0.060	0.959	2768	0.614	0.140	0.060	0.959	338	0.456	0.131	0.092	0.823
Median household income (2016)	3106	48795.991	13277.575	20170.891	129150.344	2768	47521.697	12362.349	20170.891	129150.344	338	59231.630	15713.952	28625.617	120936.812
Social capital	3106	1.384	0.705	0.000	6.887	2768	1.426	0.726	0.000	6.887	338	1.037	0.336	0.334	2.744
Distance to Minneapolis	3106	1216.679	555.825	11.998	6474.706	2768	1192.851	538.680	34.438	6474.706	338	1411.818	648.904	11.998	6395.519
Log(SXSW followers created before March 2017)	3106	0.114	0.258	0.000	1.474	2768	0.090	0.228	0.000	1.427	338	0.311	0.378	0.000	1.474
Log(SXSW followers created during March 2017)	3106	0.193	0.350	0.000	1.658	2768	0.157	0.312	0.000	1.658	338	0.489	0.482	0.000	1.619

Note: Descriptive statistics of all variables for different sub-samples depending on the presence or absence of BLM protesting history before the murder of George Floyd. Different panels correspond to different periods where different data is measured.

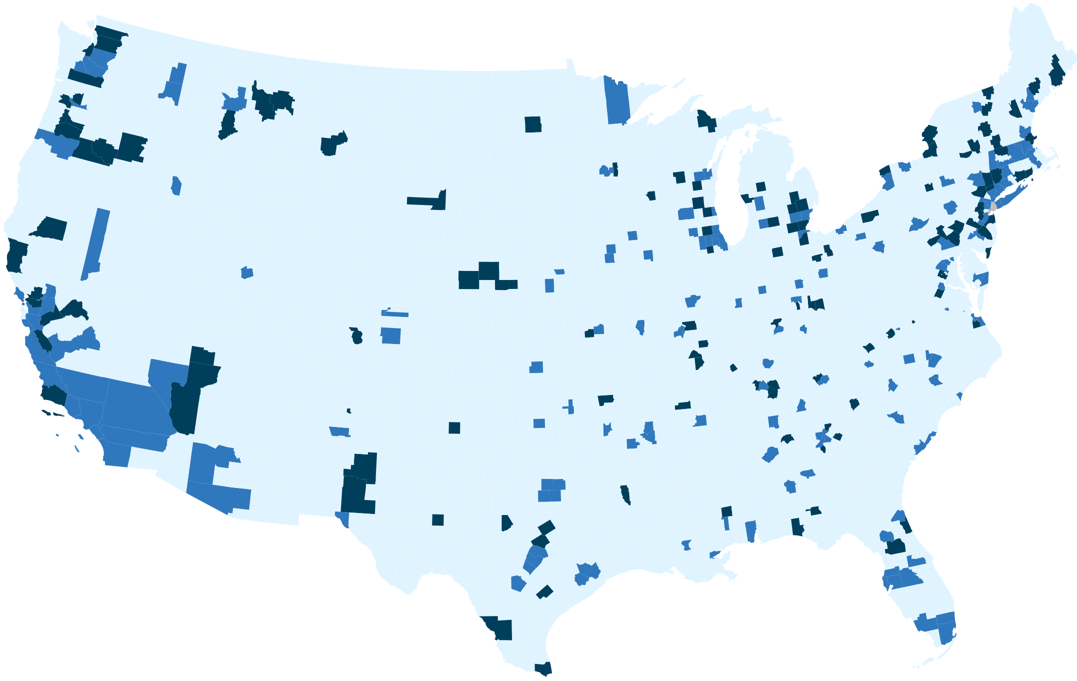
A.2 Additional Tables and Figures

Figure A1: BLM events over time



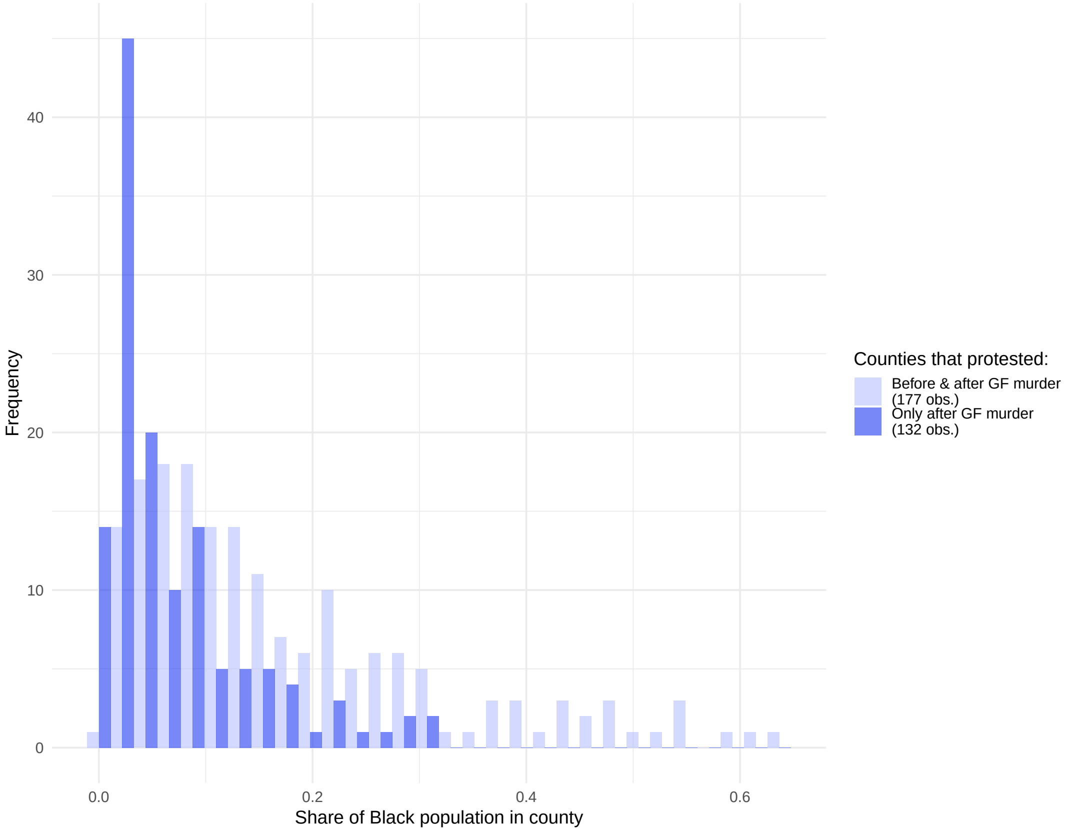
Note: Number of BLM events per week in the US from August 2014 to September 2020. The green vertical line denotes the week of the first confirmed COVID-19 case in the US (January 21, 2020), and the red vertical line denotes the week of the murder of George Floyd (May 25, 2020).

Figure A2: Half of the counties that protest after the murder of George Floyd never protested for BLM before



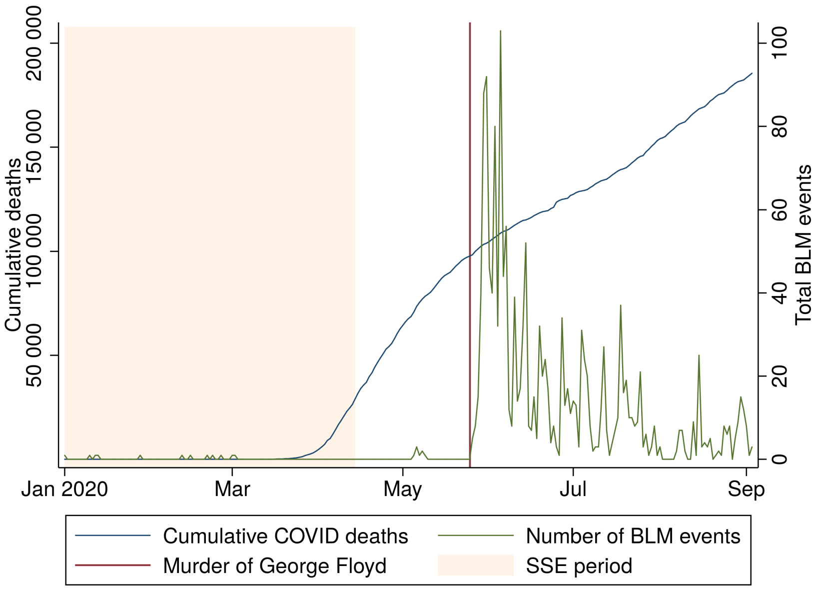
Note: This map shows whether US counties that protested in the three weeks following the murder of George Floyd (May 25 to June 14, 2020) already held a BLM protest before the murder of George Floyd or not. Counties in dark blue protested both before and after the murder of George Floyd. Counties in black are counties whose first BLM protest was after George Floyd's murder. Counties in light blue did not protest after the murder.

Figure A3: Counties that protested for BLM for the first time after the murder of George Floyd have lower Black population share



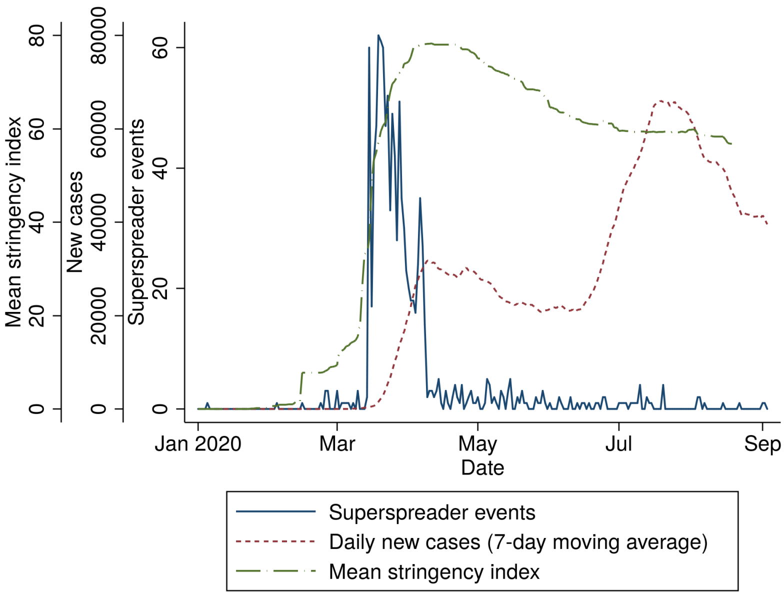
Note: Histogram of the number of counties that protest before and after (in light blue) or just after the murder of George Floyd (in dark blue) depending on the share of Black population in the county.

Figure A5: Timing of SSEs relative to Floyd's murder, protest and COVID-19 deaths



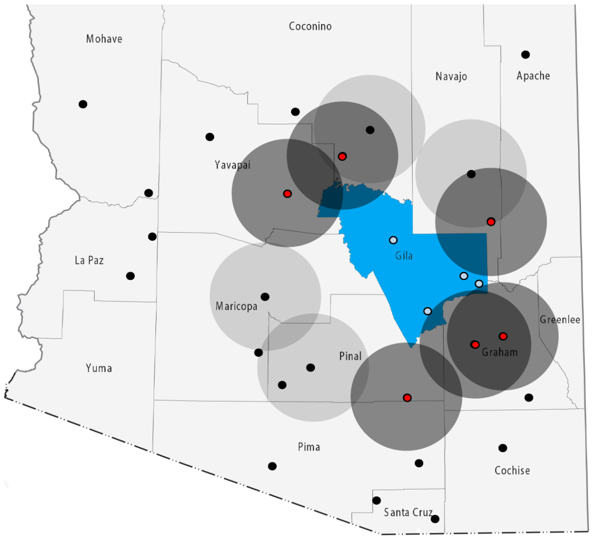
Note: Plot showing the evolution per day of various variables of interest. The blue line shows cumulative COVID-19 deaths and BLM events. The green line shows the evolution of the number of BLM events per day from January to September 2020 as reported by the New York Times. The red vertical line denotes the week of the murder of George Floyd (May 25, 2020), and the orange shaded area is the period we consider for super spreading events.

Figure A4: Super spreading events arise in early stages of the pandemic when lockdown stringency is low but COVID-19 prevalence is high enough



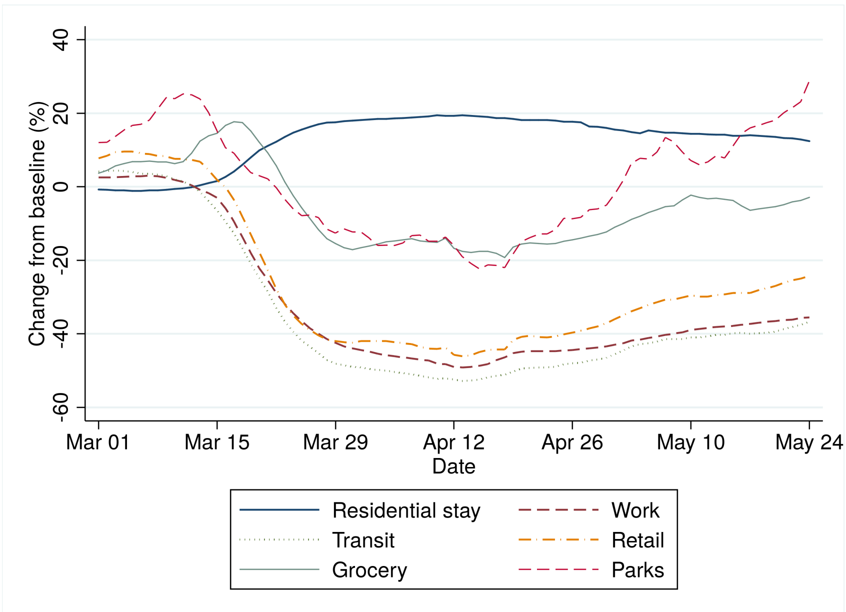
Note: Plot showing the evolution of the number of SSE events, social distancing measures and new COVID-19 cases. Solid (blue) line represents the number of daily total SSEs over time (January 2020 to September 2020). Dashed (green) line shows the daily average stringency index across all US states, as measured by the Oxford COVID-19 response tracker. Dotted (red) line shows the number of daily new COVID-19 cases as recorded by the New York Times

Figure A6: Super spreading Events outside of county border but within 50km radius



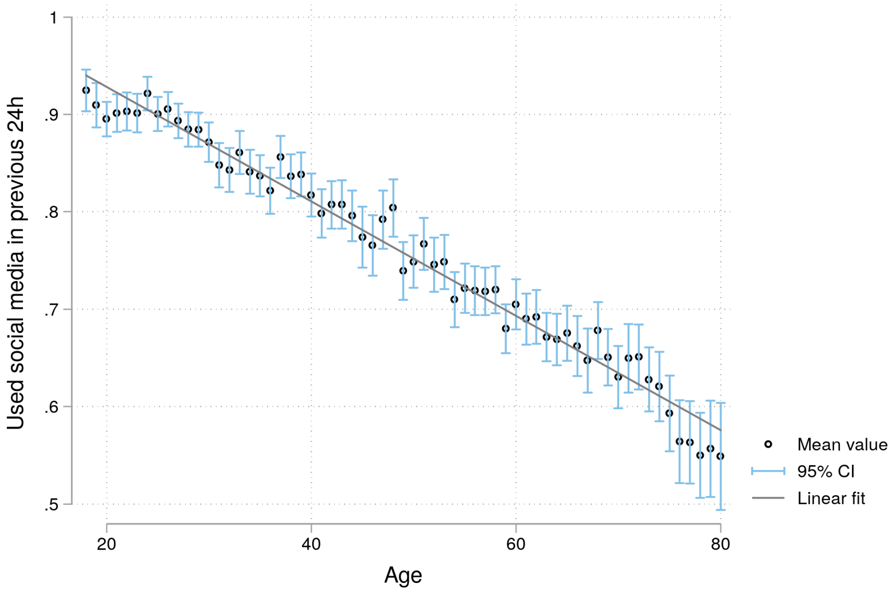
Note: Example of the construction of the instrument we use for COVID-19 deaths per 1000 population: the total number of SSEs in neighbouring counties during a certain period. Red point are the super spreading events assigned to the blue county. Gray shaded areas represent the 50km radius around each super spreading event. Black points represent super spreading events that are not assigned to the blue county because are too far away from the border. White points represents super spreading events that are inside the county and therefore not assigned to the county (to increase exogeneity).

Figure A7: Evolution of mobility index



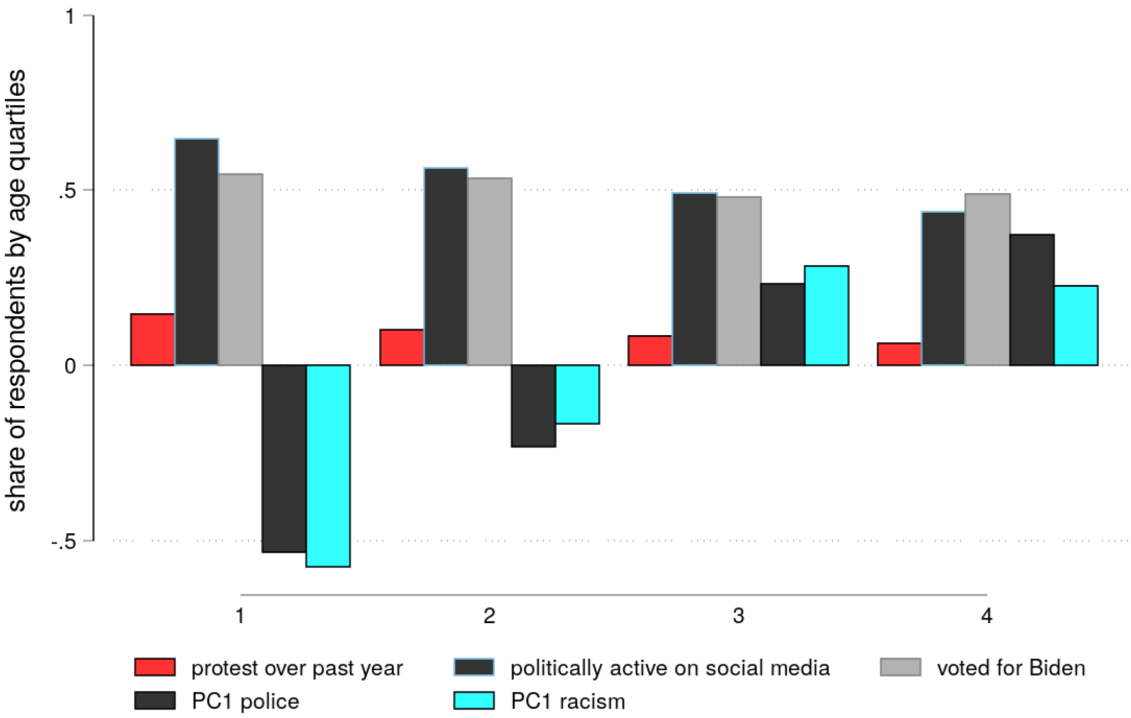
Note: This graph represents the components of the Google Community Mobility index: residential stay, and mobility to different types of places, between March 1st and May 24th, 2020. The index is relative to the average mobility to these places in the same day of the week between January 3 and February 6, 2020. The displayed value is an average of the 7 previous days.

Figure A8: Social media use by age from CES



Note: Graph of the probability of having used social media in the past 24 hours by age, among the respondents of the Cooperative Election Study (CES) conducted in November 2020.

Figure A9: Political preferences by age quartile from CES



Note: Bar graph showing share of respondents by age quartiles for all outcomes used in Table 5, using information from the Cooperative Election Study (CES) conducted in November 2020. The age thresholds are 0-33, 34-49, 50-63 and older than 63.

Table A5: Example Tweets

Date	**Text**
May 29, 2020	While #BlackLivesMatter is raising awareness on Twitter, it shouldn't stop there. While you're inside with your families, talk about racism and discrimination. Especially with older generations who don't use social media and don't see further than the national new's portrayal.
May 30, 2020	This is called UNITY. this is what white america doesn't want. they're afraid of the non racist whites to form patternship and unity with POC bc then they will be out numbered. I stand by my brothers #BlackLivesMatter https://t.co/EPYE9HKkBN
May 31, 2020	Reach out to black friends, peers, and social media connections to LISTEN to them with the understanding that I do not know what their struggles are like as a person that has lived with privilege. #BlackLivesMatter
Jun 2, 2020	If it weren't for Twitter and social media the videos of George Floyd and Ahmed Arbery would have not been seen and murderers would have walked free. Fact. #BlackLivesMatter
Jun 4, 2020	(3/7) We will also be sharing courses made by the Arist community designed to educate allies. The first example: https://bit.ly/anti-racism101 This 20-day text message course will teach you about systemic racism against Black people and how you can practice anti-racist allyship.#BlackLivesMatter
Jun 4, 2020	I made a decision when I came on twitter to keep it strictly for work. I have other social media for expressing personal and political views. However, given the events of the last week, I feel compelled to say something - so here is my bit #BlackLivesMatter #WhitePrivilege
Jun 6, 2020	#IAmASuburbanMom and Black Lives Matter to me! I just went to a rally in a suburb of Atlanta, and there are a lot of us moms who want racial justice and change!
Jun 7, 2020	White privlage means you CAN walk away from #BlackLivesMatter when you get weary and you go back to your regular routine. Our black and coloured allies don't have that privilege to simply walk away. It's their life. Recognizing our white privilege means refusing to walk away. 3/3
Jun 11, 2020	1/ I've been trying to learn more about all the complexities of everything going on lately, and how to be a better ally, better support the #blacklivesmatter movement & simply be an anti-racist. For what it's worth, here's a few things I've found to be especially helpful:
Jun 13, 2020	There was a #BlackLivesMatter car parade in my VERY white, VERY red suburban San Antonio neighborhood today. I was afraid we'd be the only car. There were 50 of us!!!

Note: Chosen examples of tweets after the murder of George Floyd showing an increase awareness and change in attitudes about BLM and racism.

Table A6: Principal component analysis of online presence

	New Twitter users	New Twitter users (log)	Google searches for Twitter	New BLM Twitter users	New BLM Twitter accounts (log)
New Twitter accounts	1				
Log New Twitter account	0.534	1			
	[0.000]				
Google searches for Twitter	0.140	0.271	1		
	[0.000]	[0.000]			
New BLM Twitter accounts	0.987	0.471	0.136	1	
	[0.000]	[0.000]	[0.000]		
New BLM Twitter accounts (log)	0.487	0.872	0.284	0.451	1
	[0.000]	[0.000]	[0.000]	[0.000]	
PC1 coef.	.5255036	.4796956	.137813	.5057774	.4679049

p-values in brackets. PC1 eigenvalue: 2.88 (58% of variance, PC2 eigenvalue: 1.06)

Note: The table reports the correlation among the online activity measures, and the factor loadings of the first principal component.

Table A7: **Push factor: SSEs predict COVID-19 deaths, cases, and new Twitter users**

	COVID-19 cases/1000 (1)	COVID-19 deaths/1000 (2)	Residential stay (3)	Log new Twitter users (4)	Log new Twitter users (5)
Z_c	0.0974**	0.00751***	0.0251***	0.00795*	-0.0132***
	(0.0363)	(0.00144)	(0.00629)	(0.00445)	(0.00417)
$Z_c \times$ Pre-existing Twitter users in 2019					0.00492***
					(0.00103)
Observations	2,767	2,767	1,022	2,767	2,767
R-squared	0.136	0.295	0.781	0.237	0.517
Mean of dep. var.	2.590	0.0990	10.01	0.445	0.445
County controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes

Note: Estimation results corresponding to specification 1. Outcomes in column 1 and 2 are COVID-19 related cases and deaths per 1000 inhabitants before May 25th 2020. Outcome in column 3 is based on the Google mobility index in the week of 18-24 May 2020, which measures how much time individuals spend at home relative to January 3 - February 6 2020, rather than at work, at places of commerce or in parks and recreational facilities. Outcomes in columns 4 and 5 are measured as the log of one plus new Twitter users that have created their account between January and May 2020 based on a random sample of geolocalized English-language tweets collected between May 4 and May 24 2020. Z_c is measured as the cumulative number of SSEs within 50 km of the county border but not within the county until six weeks before the murder of George Floyd, i.e. until mid-April 2020. All specifications include state fixed effects. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table A8: **Instrumenting COVID-19 deaths with Super Spreader Events (SSEs) - Pandemic exposure increases social media adoption**

	1st PC Social Media Index (1)	new Twitter users (2)	log new Twitter users (3)	Google search for Twitter (4)	new BLM Twitter users (5)	log new BLM Twitter users (6)	Time spent at home (7)
2SLS:							
COVID-19 (deaths/1000)	2.143**	10.71*	1.060**	18.28**	26.18*	0.994**	3.885***
	(0.893)	(5.637)	(0.480)	(8.838)	(14.07)	(0.473)	(0.931)
OLS:							
COVID-19 (deaths/1000)	0.0121	-0.208	0.00778	-0.210	-0.374	0.0835	1.551***
	(0.100)	(0.507)	(0.0713)	(1.166)	(1.232)	(0.0782)	(0.353)
Observations	2,730	2,767	2,767	2,730	2,767	2,767	1,022
Mean of dep. var.	0	1.238	0.445	60.52	2.486	0.630	10.01
First stage coef.	0.00744	0.00751	0.00751	0.00744	0.00751	0.00751	0.00646
First stage s.e.	(0.00146)	(0.00144)	(0.00144)	(0.00146)	(0.00144)	(0.00144)	(0.00144)
F first stage	26.03	27.04	27.04	26.03	27.04	27.04	20.16
County controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Estimation results corresponding to specification 1. Outcome in column 1 is the first principle component of all outcomes in columns 2 to 6. For the 2SLS specification, the instrument, Z_c, is measured as the cumulative number of SSEs within 50 km of the county border but not within the county until six weeks before the murder of George Floyd, i.e. until early April 2020. It is used to instrument for cumulative COVID-19 related deaths per 1000 inhabitants until May 25th. . Outcomes in columns 2 and 3 are based on a random sample of geolocalized Tweets, using the most common 100 English words. In column 3, we present the relative Google search intensity for Twitter in the month of April 2020, normalized such that interest is between 0 (no interest) and 100 (most interested county). Outcomes in columns 5 and 6 are based on observing users in the universe of geolocalized Tweets that contain BLM-related hashtags and keywords after the murder of George Floyd. Outcome in column 7 is based on the Google mobility index in the week of 18-24 May 2020, which measures how much time individuals spend at home relative to January 3 - February 6 2020, rather than at work, at places of commerce or in parks and recreational facilities. All specifications include state fixed effects. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. We report Kleibergen-Paap rkWald F statistic, the first stage coefficients and corresponding standard errors. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table A9: **Super spreader events increase BLM protest**

Reduced Form:	At least one BLM protest (1)	Number of BLM protests (2)	Total participants (3)	Tweets BLM (4)	Followers @BLM (5)
$Z_c = \sum_{w=1}^{t-6} \text{SSE}\ {}^{\leq 50km}_{-csw}$	0.00303*	0.00466**	-0.608	14.36*	1.313**
	(0.00163)	(0.00212)	(1.874)	(7.838)	(0.600)
Observations	2,767	2,767	2,767	2,767	2,767
R-squared	0.055	0.063	0.024	0.022	0.032
Mean of dep. var.	0.0477	0.0636	21.03	183.2	16.18
County controls	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y

Note: Estimation results corresponding to specification 1. Outcomes for BLM protest: Columns 1 to 3 use protest information from *Elephrame*, reporting respectively a dummy variable for any BLM-related protest in the three weeks following the murder of George Floyd, the number of these protests and the total number of participants. Column 4 reports the number of geo-located Tweets that use at least one BLM-related hashtag or keyword in the three weeks following the murder. Column 5 reports the number of geo-located accounts that follow the official BLM account @BlkLivesMatter. Last column instruments both new and old users with the push-pull instrument. Z_c is measured as the cumulative number of SSEs within 50 km of the county border but not within the county until six weeks before the murder of George Floyd, i.e. until early April 2020. All specifications include state fixed effects. Control variables include: the share of Black population, urban, median household income, unemployment share, Black poverty rate, 3+ risk factors/community resilience, Republican vote share in 2012 and 2016, social capital (number of different types of civic organizations) and deadly force used by police against Black people. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table A10: **Alternative explanations: scattering, salience, opportunity cost**

	At least one BLM protest in county after murder of George Floyd						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z_c	-0.000250	0.00220	0.00102	-0.00681	-0.00145	0.000556	-0.00634
	(0.00847)	(0.00414)	(0.00288)	(0.00536)	(0.00620)	(0.00333)	(0.0210)
$\times$ distance to Minneapolis	2.14e-06						
	(5.49e-06)						
$\times$ neighbor traditional BLM protester		0.000988					
		(0.00443)					
$\times$ neighbor protested just before			0.00223				
			(0.00325)				
$\times$ death burden on Blacks				0.00757*			
				(0.00391)			
$\times$ Google searches for BLM					-0.000193		
					(0.000117)		
$\times$ unemployment						0.000589	
						(0.000917)	
$\times$ lockdown stringency							0.000129
							(0.000303)
Observations	2,767	2,767	2,767	2,767	841	2,767	2,767
R-squared	0.149	0.148	0.148	0.151	0.233	0.148	0.148
Interacting var. control	Y	Y	Y	Y	Y	Y	Y
County controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Estimation of the effect of Z_c, the number of SSE in neighbouring counties (50km radius), on the presence of BLM protests as well as the interaction of Z_c with different variables – all measured before the protest trigger – while controlling for the variable itself (not reported). The sample consists of counties with no BLM protest before George Floyd's murder. Column 1 shows interaction with distance to Minneapolis. Column 2 shows the interaction term with a dummy equal to one if at least one neighbouring county protested for BLM at any time before the Floyd's murder. Column 3 uses the interaction with a dummy variable that switches on if during the 3 weeks after the murder of George Floyd at least one neighboring county protested before the county of interest (or in the 3 week if the county did not have a protest). The interacting variables in column 4 is the disproportionate death burden on Blacks (death share relative to population share) up to one week prior to George Floyd's murder. Column 5 uses the Google searches for "BLM" 3 weeks prior to George Floyd's murder. Column 6 shows the interaction with the average unemployment rate in the year preceding Floyd's murder. Column 7 uses an index capturing the stringency of social distancing measures on May 24. All specifications include state fixed effects and the same controls as the baseline specification. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Principal component analysis of BLM protest

	at least one BLM protest	number of BLM protests	total participants	Tweets BLM	followers @BLM
at least one BLM protest	1				
number of BLM protests	0.541	1			
	[0.000]				
total participants	0.148	0.631	1		
	[0.000]	[0.000]			
tweets BLM	0.261	0.775	0.707	1	
	[0.000]	[0.000]	[0.000]		
followers @BLM	0.248	0.773	0.716	0.992	1
	[0.000]	[0.000]	[0.000]	[0.000]	
PC1 coef.	.5469969	.5485032	.4408923	.3169014	.3242317

p-values in brackets. PC1 eigenvalue: 2.47 (49% of variance, PC2 eigenvalue: 1.82)

Note: The table reports the correlation among the BLM protest measures, and the factor loadings of the first principal component.

Table A12: Cooperative Election Study 2020: Selected Survey Questions

Category	**ID**	**Question**	**Scale (low/high)**
Police	CC20_334a	Do you support or oppose eliminating mandatory minimum sentences for non-violent drug offenders?	Support/Oppose
	CC20_334b	Do you support or oppose requiring police officers to wear body cameras that record all of their activities while on duty?	Support/Oppose
	CC20_334c	Do you support or oppose increasing the number of police on the street by 10 percent even if it means fewer funds for other public services?	Support/Oppose
	CC20_334d	Do you support or oppose decreasing the number of police on the street by 10 percent and increasing funding for other public services?	Support/Oppose
	CC20_334e	Do you support or oppose banning the use of choke holds by police?	Support/Oppose
	CC20_334f	Do you support or oppose creating a national registry of police who have been investigated for or disciplined for misconduct?	Support/Oppose
	CC20_334g	Do you support or oppose ending the Department of Defense program that sends surplus military weapons and equipment to police departments?	Support/Oppose
	CC20_334h	Do you support or oppose allowing individuals or their families to sue a police officer for damages if the officer is found to have "recklessly disregarded" the individual's rights?	Support/Oppose
Racism	CC20_440a	White people in the U.S. have certain advantages because of the color of their skin.	Strongly agree to Strongly disagree
	CC20_440b	Racial problems in the U.S. are rare isolated situations.	Strongly agree to Strongly disagree
	CC20_441a	Irish, Italians, Jewish, and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.	Strongly agree to Strongly disagree
	CC20_441b	Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	Strongly agree to Strongly disagree
Media Use	CC20_300_1	In the past 24 hours have you [1] Used social media (such as Facebook or Youtube) [2] Watched TV news [3] Read a newspaper in print or online [4] Listened to a radio news program or talk radio [5] None of these	No/Yes
	CC20_300d_3	In the past 24 hours, did you do any of the following on social media (such as Facebook, Youtube or Twitter)? [1] Posted a story, photo, video or link about politics [2] Posted a comment about politics [3] Read a story or watched a video about politics [4] Followed a political event [5] Forwarded a story, photo, video or link about politics to friends [6] None of the above	No/Yes
Protest	CC20_430a_4	During the past year did you attend a political protest march or demonstration?	No/Yes

Note: Framing of the questions used in our analysis from the Cooperative Election Study 2020. The values are given from the value with the lowest numerical coding to the value with highest numerical coding.

Table A13: **Placebo first stage: other media consumption from the Cooperative Election Study November 2020**

	(1) social media	(2) TV	(3) newspaper	(4) radio
$Z_c \times N_c \times \ Age_i$	0.00330*	-0.00151	0.000920	0.00114
	(0.00172)	(0.00158)	(0.00104)	(0.00131)
	[0.060]	[0.346]	[0.382]	[0.388]
Observations	48420	48420	48420	48420
Individual controls	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses, p-values in square brackets. Placebo first stage regression. Individual level regression with county fixed effects and individual controls, including age, gender, employment status, number of children, dummies for religious affiliation, dummies for race and citizenship status. Outcomes are taken from the Cooperative Congressional Election Study conducted in November of 2020. Column 1 is dummy variable for whether respondent has used social media in the past 24 hours. Column 2 to 4 use the same question for TV, newspapers and radio. The treatment variable is the previous push-pull instrument $Z_c \times N_c$ interacted with the age of the respondent. We also include the respective interaction between Z_c and N_c and the respondent's age. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table A14: Principal component analysis of Cooperative Election Study 2020 data

(a) Racism

	CC20_440a	CC20_441b	CC20_440b	CC20_441a
CC20_440a	1			
CC20_441b	0.760	1		
	[0.000]			
CC20_440b	-0.648	-0.608	1	
	[0.000]	[0.000]		
CC20_441a	-0.695	-0.746	0.632	1
	[0.000]	[0.000]	[0.000]	
PC1 coef.	.5107706	.5133582	-.4689424	-.505638

p-values in brackets. PC1 eigenvalue: 3.05 (76% of variance, PC2 eigenvalue: 0.43)

(b) Attitudes towards police

	CC20_334a	CC20_334b	CC20_334c	CC20_334d	CC20_334e	CC20_334f	CC20_334g	CC20_334h
CC20_334a	1							
CC20_334b	0.155	1						
	[0.000]							
CC20_334c	-0.266	-0.0849	1					
	[0.000]	[0.000]						
CC20_334d	0.311	0.134	-0.547	1				
	[0.000]	[0.000]	[0.000]					
CC20_334e	0.254	0.264	-0.262	0.318	1			
	[0.000]	[0.000]	[0.000]	[0.000]				
CC20_334f	0.211	0.328	-0.221	0.279	0.378	1		
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]			
CC20_334g	0.334	0.140	-0.393	0.505	0.348	0.281	1	
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]		
CC20_334h	0.254	0.284	-0.294	0.332	0.363	0.420	0.344	1
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	
PC1 coef.	.307678	.2386616	-.3598705	.4064626	.3637737	.3502633	.3948246	.3778281

p-values in brackets. PC1 eigenvalue: 3.12 (39% of variance, PC2 eigenvalue: 1.19)

Note: Each table reports the correlation among the racism and police attitude measures, and the factor loadings of the first principal component. The detailed wording of the questions is given in Table A12.

Appendix B: Robustness Checks

Our robustness checks focus on two dimensions: *i*) robustness to changes in the definition and construction of our pandemic exposure variable Z_c and *ii*) robustness of our main results to sample composition, spatial correlation and other confounding factors.

B.1 Instrument Robustness and Validity

Changing the radius around SSEs. In the baseline specification, we choose the 50km threshold as a distance of the SSE to the county border, as it is approximately two times the average radius of a county in the US.[28] To make sure that this choice is not driving our results, we change the radius of influence to 25 km, 100 km and 150 km (columns 2, 3 and 4 of Table B1 respectively). In the reduced form results of Panel A, the magnitudes of the effect of Z_c decreases with distance. This is expected, since the Z_c and its variation will be larger for larger radii. The results stay significant for larger radii, but become imprecisely estimated for the 25 km radius (p=0.11). This may be due to more SSE only affecting their own county due to the smaller radius, limiting the effective sample size. In the 2SLS results in Panel B, the effect stays significant for all values (the standard error even decreases with increasing radius), and only decreases slightly in magnitude for the 150 km radius.

Changing the time window of SSEs. Similarly, in our preferred specification, we take into account the SSEs that occurred in a specific time window that we call "window of opportunity" where there were enough cases to observe SSEs and the social distancing measures were not applied strictly or widely enough. Specifically, we count the number of SSEs between the beginning of the COVID-19 outbreak until April 13th 2020 (i.e., six weeks before Floyd's murder). In columns 5 to 7 of Table B1 we expand and narrow this window to make sure our results are not driven by the specific timing of SSEs. In particular, we count SSEs until April 20th, 5 weeks before the murder of Floyd (column 5), on April 6th, 7 weeks before (column 6) and on March 30th, 8 weeks before (column 7). Results are robust to changes in the time window.

Excluding SSEs in prisons. A non-negligible number of SSEs occurred inside prisons. We exclude SSEs in prisons in a robustness check in column 2 of Table B2 for two reasons. First, it is likely that by the nature of prisons, the geographical spread of cases stemming from an SSE in a prison is quite limited and less relevant for the overall population and the protesting population. Second, SSEs in prisons may affect BLM protests (and, to a lesser extent, social media usage) in ways other than through overall social media adoption, for instance, by increasing awareness of the disproportionate incarceration of Black people. While the salience of racial inequality in prisons may be a possible mechanism, with this exercise we investigate whether our results are indeed solely driven by this subsample of SSEs. We exclude SSEs in prisons in column 2 and find that our results are not sensitive to this choice.

Controlling for SSEs in the county. Our pandemic exposure variable measures the effect of having an SSE outside the county within 50 km of the county border and excluding the effect of SSEs that take place within its border. Therefore, in our analysis a county is "not affected" by an SSE if its border is either further than 50 km from the SSE, or the SSE happened within its boundaries. We expect the effect of SSEs to be different between these groups: presumably, counties far away will have no COVID-19 cases from this SSE, while the county where the SSE took place will have a lot of cases and deaths caused by the event. To assuage the concern that correlation of SSEs across counties is driving the variation in SSE exposure, we add as a control the number of SSEs that occurred within the county itself. Estimates are presented in column 3 of Table B2 and show that the results of the baseline specification are robust to the addition of this control.

Controlling by previous BLM protest in neighbors We observed in the balance test (Figure 2) that SSEs may be correlated with BLM protests in 2014-2019 in neighboring counties. To make sure that this does not affect the results, we control for the presence of such protests in column 4 of Table B2. This does not affect the results.

[28] For reference, the average radius of a county is 28 km and the average radius of a state is 220 km.

Weighting SSEs by distance. In our baseline specification, we count any SSE that occurred in a 50 km radius outside the border of a county as an additional SSE affecting the county in an uniform way. However, an SSE 1 km away from the border is likely to have a different level of influence from a SSE 49 km away. To ensure that this simplification is not driving the results, we refine the level of influence by weight the SSEs by a linear function decreasing with distance (column 5 of Table B2), giving less weight to events that are more distant. The results are robust to this distance weighting procedure. The magnitude of the coefficients in the reduced form regressions in Panel A change, since the weighting changes the average of Z_c.

Weighting SSEs by the inverse probability of occurrence. The probability of being near a county that has an SSE is not constant over all counties. For instance, counties neighboring cities have likely a higher probability of being treated by our instrument as their neighbors may be more likely to experience an SSE. This could be a violation of the exclusion restriction because the probability of being treated by our instrument at a certain level is not uniform, and this heterogeneity could be related to certain county characteristics that could in turn be related to the level of social media usage or the probability of protesting. To address this concern, we weight each observation by the inverse probability of being treated. . Using LASSO (a regularized regression procedure that performs variable selection and avoids overfitting, Tibshirani 1996), we select relevant variables predicting (by a logit model) the probability of having a neighbor with an SSE among a set of county characteristics, including a large set of socio-demographic and economic characteristics extracted from the American Community Survey (such as population, population density, race distribution, age groups, poverty rates, among others), indicators for different levels of urbanization, geographical indications (latitude, longitude, and state dummies), as well as the minimum and maximum of these variables for neighboring counties. We use the LASSO selected model to predict the probability of a county having a neighbor with an SSE, then weight the observations by the inverse of this probability. This means that counties with a higher probability of having a neighbor with an SSE that actually had a neighbor with an SSE are weighted less than counties with a lower probability of being treated that are actually treated. Estimates are presented in column 6 of Table B2. Our results stay significant, although the magnitude of the 2SLS effect in panel B becomes lower.

Effect of SSEs on Twitter SSEs may be correlated to pre-existing social media presence. For example, some counties may generally be more sociable both offline and online. In order to test this, we look at the effect of SSEs on past Twitter use in December of 2019 (we describe the construction of this variable in the previous section). In Table B3, we show that SSEs do not predict past Twitter users.

Plausibility of exclusion restriction for BLM. If our push-pull instrument $Z_c \times N_c$ were to pick up any underlying factors correlated with the overall likelihood of protesting for a BLM-related cause, then this would challenge a causal interpretation of our estimates. To probe the plausibility of the exclusion restriction, we estimate the effect of instrumented new Twitter users on the likelihood of observing past BLM protests, using as a sample the set of all counties instead of counties where no protests has been observed before George Floyd's murder. If our instrument was correlated with the county unobservables that also predict the likelihood of observing BLM protests, then we would expect to see a statistically significant relationship between our instrumented new Twitter users and likelihood of observing a BLM protest in the past. In column 2 of Table B4, we show that instrumented new Twitter users do not predict the presence of BLM events between 2014 and 2019. We take this as additional evidence for the plausibility of our identifying assumption.

B.2 Robustness of Main Results

In this section, we focus on our main results and run robustness checks including changing definitions in treatment and outcome, estimation method, spatial correlation and concerns about the overall propensity to use social media or to protest.

Time window of protests. In our baseline specification, we choose the three week window following Floyd's murder since it captures the vast majority of BLM-related protests, while being close enough to the exposure to COVID-19 on May 24th, right before the protest trigger. We show that our main results are robust to reducing this time window to 2 weeks and expanding this time window to 6 and 9 weeks (columns 3 to 5 of Table B4 respectively).

Excluding coastal counties and states. Coastal states and counties might behave differently with regard to our instrument, to social media adoption, and to protest. Coastal regions are generally denser, which increases the chance of having an SSE (Figure 1 shows the density of SSEs). Coastal counties also differ in the construction of the COVID-19 exposure variable. As defined, we positively label those counties that have SSEs in neighboring counties. Coastal counties naturally have fewer land neighbors, which decreases its chances of being treated. Traditionally, these counties have also had higher BLM protest activity, so it is also instructive to obtain results for less active landlocked counties. We show that our results are robust to excluding coastal counties (column 2 of Table B5), as well as coastal states (column 3).

Controlling for COVID-19 prevalence during the protest period. The intensity of COVID-19 around the protest could directly affect the willingness and opportunity to protest (e.g. due to health concerns or local restrictions). In column 4 and 5 of Table B5, we show that out results are not sensitive to adding a control for the number of COVID-19 cases or deaths in the 7 days before May 24th.

Accounting for spatial correlation. Observations are likely to be spatially correlated for several reasons. For instance, there could be spatially-correlated unobserved factors influencing the decision to protest (such as weather conditions or available TV and radio stations). Clustering by state does not entirely remove these errors because correlation across state borders remains (Colella et al., 2019). To overcome this problem, we use Conley standard errors that allow for spatial correlation within a certain distance. Column 6, 7 and 8 of Table B5 show the estimates when allowing spatial correlation between observations in a 50, 100 and 150km radius. Column 9 of Table B5 shows the estimates when allowing spatial correlation with all neighboring counties. Reassuringly, our results remain robust.

Probit estimation. In our baseline specification the effect of social media is additive. It might be the case that the effect would be multiplicative of some characteristics of the counties. Using a Probit model accounts for this possibility. Non-linear models with many covariates (typically when using fixed effects) suffer from the incidental parameter problem resulting in bias of the estimates (Manski et al., 1981; Lancaster, 2000; Wooldridge, 2015). To reduce the extent of this problem we omit the state fixed effects, which significantly reduces the number of covariates. We use an OLS in the first stage, but estimate the second stage with a Probit model. Results are presented in column 2 of Table B6. The Probit model delivers more precisely estimated coefficients (although magnitudes are not directly comparable).

Controlling for propensity to protest. Our main specification already controls for the number of BLM events that took place in the county in previous years. While this gives some indication of the county's propensity to protest, this is essentially an imprecise measure, since counties having a non-zero probability of protesting might simply not have protested before by random chance. Using LASSO regression, we construct a variable measuring propensity to protest in the 3 weeks following a notable death of a Black person in an encounter with the police. We consider a death notable if it received national media coverage. We construct a dataset of 31 notable deaths between 2014 and 2019, and we select our predictors among a set of county characteristics, including a large set of socio-demographic and economic characteristics extracted from the American Community Survey (such as population, population density, race distribution, age groups, poverty rates, among others), indicators for different levels of urbanization, geographical indications (latitude, longitude, and state dummies). We add this propensity to protest variable as a control in our regression. It yields a continuous measure of ex-ante protest probability even for the sub-set of counties with no prior BLM protest. We first use it directly as a control (column 3 of Table B6). Our results remain robust and are more precisely estimated.

In addition, we include fixed effects for different levels of the propensity to protest. We group observations by groups of 1000, 100 and 10 units with similar propensity to protest and add fixed effects for each group. Results are shown in columns 4 to 6 of Table B6. This is essentially a matching-like strategy, where the fixed effects ensure that observations with similar propensity are compared. Results are robust to the inclusion of fixed effects, except in Panel B where results become imprecisely estimated for groups of size 10.

Table B1: Instrument robustness - SSE timing and distance

Panel A: Superspreader events on social media index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reduced form: $Z_c = \sum_{w=1}^{t-6} \text{SSE}_{-c,w}^{\leq 50km}$	0.0159**	0.0304	0.00668***	0.00449***	0.0157**	0.0158**	0.0184**
	(0.00719)	(0.0186)	(0.00168)	(0.00125)	(0.00696)	(0.00706)	(0.00781)
Observations	2,730	2,730	2,730	2,730	2,730	2,730	2,730
R-squared	0.106	0.106	0.105	0.106	0.105	0.105	0.105
Mean dep. var.	0	0	0	0	0	0	0

Panel B: New Twitter users on presence of BLM protests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2SLS: new Twitter users	0.243**	0.241**	0.250***	0.214***	0.239**	0.241**	0.236**
	(0.0959)	(0.114)	(0.0871)	(0.0665)	(0.0946)	(0.0957)	(0.0952)
Observations	2,767	2,767	2,767	2,767	2,767	2,767	2,767
Mean dep. var.	0.0477	0.0477	0.0477	0.0477	0.0477	0.0477	0.0477
First stage coef. $Z_c \times N_c$	0.00617	0.00818	0.00318	0.00186	0.00617	0.00590	0.00715
First stage s.e. $Z_c \times N_c$	(0.00286)	(0.00758)	(0.00170)	(0.00101)	(0.00283)	(0.00279)	(0.00338)
Kleibergen-Paap F stat	9.587	4.974	9.319	16.31	9.515	8.700	10.69
Distance	50 km	25 km	100 km	150 km	50 km	50 km	50 km
Lag	6 weeks	6 weeks	6 weeks	6 weeks	5 weeks	7 weeks	8 weeks
All controls	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y

Note: Results on robustness of the results to variations in the measure Z_c of superspreader events, using different time and distance selection. The sample consists of counties with no BLM protest before George Floyd's murder. Panel A presents results corresponding to the reduced form results in Column 1 of Table 1 (ie. the effect of the measure Z_c of exposure to superspreader events on the Social Media Index) and Panel B presents results corresponding to the 2SLS results in Column 2 of Table 3 (ie. the effect of the logarithm of one plus the number of new Twitter users in the sample, instrumented by $Z_c \times N_c$). Column 1 corresponds to the baseline definition of Z_c. Columns 2 to 4 vary the distance at which SSE are counted from 25 to 150km. Columns 5 to 7 vary the time lag between the murder of Floyd and the last SSE, going back 5, 7 or 8 weeks. All specifications include the whole set of controls and state fixed effects. For 2SLS estimates, we report the Kleibergen-Paap rkWald F statistic, the first stage coefficient and corresponding standard errors. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table B2: Instrument robustness - SSE definition and weighting

Panel A: Superspreader events on social media index

	(1)	(2)	(3)	(4)	(5)	(5)
Reduced form: $Z_c = \sum_{w=1}^{t-6} \text{SSE}_{-c,w}^{\leq 50km}$	0.0159**	0.0142**	0.0140*	0.0154**	0.0360*	0.0160*
	(0.00719)	(0.00625)	(0.00777)	(0.00674)	(0.0199)	(0.00857)
Observations	2,730	2,730	2,730	2,730	2,730	2,727
R-squared	0.106	0.104	0.107	0.108	0.106	0.090
Mean dep. var.	0	0	0	0	0	0

Panel B: New Twitter users on presence of BLM protests

	(1)	(2)	(3)	(4)	(5)	(5)
2SLS: new Twitter users	0.243**	0.244**	0.222**	0.266***	0.244**	0.174**
	(0.0959)	(0.0925)	(0.0908)	(0.0960)	(0.109)	(0.0761)
Observations	2,767	2,767	2,767	2,767	2,767	2,764
Mean dep. var.	0.0477	0.0477	0.0477	0.0477	0.0477	0.0479
First stage coef. $Z_c \times N_c$	0.00617	0.00617	0.00522	0.00638	0.0107	0.00701
First stage s.e. $Z_c \times N_c$	(0.00286)	(0.00270)	(0.00302)	(0.00284)	(0.00718)	(0.00379)
Kleibergen-Paap F stat	9.587	7.900	7.775	9.353	6.775	8.198

Excluding SSEs in prisons		Y				
Control SSE in county			Y			
Neighbor had BLM protest in 2014-2019				Y		
SSE distance weighting					Y	
SSE probability weighting						Y
All controls	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y

Note: Results on robustness of the results to variations in the measure Z_c of superspreader events, using different time and distance selection. The sample consists of counties with no BLM protest before George Floyd's murder. Panel A presents results corresponding to the reduced form results in Column 1 of Table 1 (ie. the effect of the measure Z_c of exposure to superspreader events on the Social Media Index) and Panel B presents results corresponding to the 2SLS results in Column 2 of Table 3 (ie. the effect of the logarithm of one plus the number of new Twitter users in the sample, instrumented by $Z_c \times N_c$). Column 1 corresponds to our baseline specification. Column 2 excludes SSEs that took place in prisons. In column 3, a control is added for the number of SSEs within the county 6 weeks before the murder of George Floyd. Column 4 controls for whether BLM protests took place in a neighboring county between 2014 and 2019. Columns 5 weighs the effect of SSEs by distance with smaller weights given to more distant SSEs. Weights are applied linearly. In column 6, observations are weighted by the inverse probability of observing a SSE affecting the county if a SSE is observed, no SSE if no SSE is observed. All specifications include the whole set of controls and state fixed effects. For 2SLS estimates, we report the Kleibergen-Paap rkWald F statistic, the first stage coefficient and corresponding standard errors. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table B3: Prediction of Twitter presence from COVID-19 SSE instrument

Outcome:	Log(Pre-existing Twitter users in 2019)		
	(1)	(2)	(3)
Subsamble:	All counties	No BLM events before	Has BLM event before
$Z_c = \sum_{w=1}^{t-6} \text{SSE}\ {}^{\leq 50km}_{-csw}$	0.00134	0.00422	-0.00252
	(0.00379)	(0.00517)	(0.00447)
Observations	3,106	2,767	333
Mean of dep. var	2.034	1.738	4.439
All controls	Y	Y	Y
State fixed effects	Y	Y	Y

Note: This tables show a regression of pre-existing Twitter presence (measured by the log of the number of pre-exisiting users in the December 2019 sample) on the pandemic exposure Z_c. Column 1 shows the result for all counties, column 2 for the sub-sample of counties that did not hold a BLM protest before the murder of George Floyd, and column 3 for the sub-samples of counties that did. All specifications include state fixed effects and all standard controls. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table B4: Robustness of main results - outcome definition

	Presence of BLM events				
	3 weeks	Past events	2 weeks	6 weeks	9 weeks
	(1)	(2)	(3)	(4)	(5)
2SLS: new Twitter users	0.243**	0.0328	0.182**	0.265***	0.196**
	(0.0959)	(0.0987)	(0.0855)	(0.0961)	(0.0795)
Observations	2,767	3,106	2,767	2,767	2,767
Mean dep. var.	0.0477	0.108	0.0354	0.0665	0.0795
First stage coef. $Z_c \times N_c$	0.00617	0.000924	0.00617	0.00617	0.00617
First stage s.e. $Z_c \times N_c$	(0.00286)	(0.00261)	(0.00286)	(0.00286)	(0.00286)
Kleibergen-Paap F stat	9.587	5.570	9.587	9.587	9.587
All controls	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y

Note: Robustness of the results to changes in the window in which BLM events are considered. Results correspond to the 2SLS results in Column 2 of Table 3 (ie. the effect of the logarithm of one plus the number of new Twitter users in the sample, instrumented by $Z_c \times N_c$). The sample consists of counties with no BLM protest before George Floyd's murder, except in column 2 where the full sample is considered. Column 1 corresponds to the baseline specification. Column 2 predicts past BLM events (likelihood of observing a BLM event between 2014 and 2019) and uses all counties instead of only the counties with no BLM protest before George Floyd's murder. Columns 3, 4 and 5 present different time windows for BLM protests: 2, 6 and 9 weeks. All specifications include the whole set of controls and state fixed effects. We report the Kleibergen-Paap rkWald F statistic, the first stage coefficient and the mean of dependent variable. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table B5: Robustness of main results - sample composition and spatial correlation

Panel A: Superspreader events on social media index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reduced form: $Z_c = \sum_{w=1}^{t-6} \text{SSE}_{-c,w}^{\leq 50km}$	0.0159**	0.0224***	0.0159***	0.0159**	0.0160**	0.0159**	0.0159***	0.0159***	0.0159**
	(0.00719)	(0.00656)	(0.00520)	(0.00719)	(0.00723)	(0.00695)	(0.00601)	(0.00554)	(0.00700)
Observations	2,730	2,580	1,686	2,730	2,730	2,731	2,731	2,731	2,731
R-squared	0.106	0.097	0.168	0.106	0.107	0.106	0.106	0.106	0.106
Mean dep. var.	0	-0.0419	-0.0864	0	0	0	0	0	0

Panel B: New Twitter users on presence of BLM protests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
2SLS: new Twitter users	0.243**	0.216*	0.221***	0.243**	0.239**	0.243**	0.243***	0.243**	0.243***
	(0.0959)	(0.110)	(0.0645)	(0.0959)	(0.0945)	(0.112)	(0.0823)	(0.0987)	(0.0903)
Observations	2,767	2,616	1,697	2,767	2,767	2,768	2,768	2,768	2,768
Mean dep. var.	0.0477	0.0428	0.0371	0.0477	0.0477	0.0477	0.0477	0.0477	0.0477
First stage coef. $Z_c \times N_c$	0.00617	0.00530	0.0105	0.00617	0.00610	0.00617	0.00617	0.00617	0.00617
First stage s.e. $Z_c \times N_c$	(0.00286)	(0.00362)	(0.00590)	(0.00286)	(0.00288)	(0.00357)	(0.00147)	(0.00264)	(0.00371)
Kleibergen-Paap F stat	9.587	10.85	48.34	9.587	9.636	6.741	84.80	12.43	7.193
Excluding coastal		counties	states						
COVID-19 cases in past 7 days				Y					
COVID-19 deaths in past 7 days					Y				
State clustering	Y	Y	Y	Y	Y				
Spatial clustering						50 km	100 km	150 km	neighbors
All controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: Results on robustness of the main results to changes in the sample composition, controls and clustering. The sample consists of counties with no BLM protest before George Floyd's murder. Panel A presents results corresponding to the reduced form results in Column 1 of Table 1 (ie. the effect of the measure Z_c of exposure to superspreader events on the Social Media Index) and Panel B presents results corresponding to the 2SLS results in Column 2 of Table 3 (ie. the effect of the logarithm of one plus the number of new Twitter users in the sample, instrumented by $Z_c \times N_c$). Column 1 correspond to our baseline specification. Columns 2 and 3 exclude coastal counties and states. Column 4 includes as an additional control the number of new COVID-19 cases in the 7 days leading up to Floyd's murder, and column 5 the number of new COVID-19 deaths. Columns 6 to 9 take spatial correlation into accounts: column 6 assumes that counties less than 50 km away can have correlated errors, resp. 100 km for column 7 and 150 km for column 8. Column 9 allows neighboring counties to have correlated error terms. All specifications include the whole set of controls and state fixed effects. For 2SLS estimates, we report the Kleibergen-Paap rkWald F statistic, the first stage coefficient and corresponding standard errors. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table B6: Robustness of main results - estimation method, protest propensity, and clustering

Panel A: Superspreader events on social media index	(1)		(3)	(4)	(5)	(6)
Reduced form: $Z_c = \sum_{w=1}^{t-6} \text{SSE}\ _{-c,w}^{\leq 50km}$	0.0159**		0.0178*	0.0160**	0.0149**	0.0141**
	(0.00719)		(0.00888)	(0.00719)	(0.00713)	(0.00693)
Observations	2,730		1,537	2,730	2,730	2,730
R-squared	0.106		0.085	0.046	0.032	0.106
Mean dep. var.	0		0	0	0	0

Panel B: New Twitter users on presence of BLM protests	(1)	(2)	(3)	(4)	(5)	(6)
2SLS: new Twitter users	0.243**		0.337**	0.245**	0.236**	0.197
	(0.0959)		(0.135)	(0.0970)	(0.110)	(0.122)
IV Probit: new Twitter users		1.138***				
		(0.276)				
Observations	2,767	2,766	1,560	2,767	2,767	2,767
Mean dep. var.	0.0477	0.0477	0.0744	0.0477	0.0477	0.0477
First stage coef. $Z_c \times N_c$	0.00617	0.00467	0.00295	0.00637	0.00637	0.00284
First stage s.e. $Z_c \times N_c$	(0.00286)	(0.00281)	(0.00305)	(0.00289)	(0.00260)	(0.00357)
Kleibergen-Paap F stat	9.587	11.17	7.434	9.655	9.076	4.433
All controls	Y	Y	Y	Y	Y	Y
Propensity to protest			Y			
Propensity to protest group: size				1000	100	10
State clustering	Y	Y	Y	Y	Y	Y
State fixed effects	Y		Y	Y	Y	Y

Note: Results on robustness of the main results to changes in the controls and the weighting of observations. The sample consists of counties with no BLM protest before George Floyd's murder. Panel A presents results corresponding to the reduced form results in Column 1 of Table 1 (ie. the effect of the measure Z_c of exposure to superspreader events on the Social Media Index) and Panel B presents results corresponding to the 2SLS results in Column 2 of Table 3 (ie. the effect of the logarithm of one plus the number of new Twitter users in the sample, instrumented by $Z_c \times N_c$). Column 1 correspond to our baseline specification. Column 2 (in Panel B only) estimates the second stage with an IV Probit model (with an OLS in the first stage) and omits state fixed-effects. Column 3 adds a control for the propensity to protest derived from our LASSO selection model. Columns 4 to 6 add fixed effects for propensity to protest for groups of size 1000, 100 and 10 respectively. All specifications include the whole set of controls and state fixed effects (except for column 2). For 2SLS estimates, we report the Kleibergen-Paap rkWald F statistic, the first stage coefficient and corresponding standard errors as well as the mean of dependent variable. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Appendix C: Twitter Instrument

C.1 Details on SXSW instrument

This appendix details the construction of the instrument for baseline Twitter penetration used in Section 4. SXSW is a film, interactive media, and music festival and conference held annually in Austin, Texas. During the March 2007 edition Twitter was heavily promoted, leading to a rapid increase in the social network's popularity. Müller and Schwarz (2023) use this event to construct an instrument for Twitter penetration in the US by exploiting the fact that, through network effects, places that had more accounts created by visitors to SXSW continued to have more accounts created later on. It is not possible to directly measure the accounts created by SXSW attendees: instead, Müller and Schwarz (2023) measure the number of followers of the official account of the festival (@SXSW) that joined Twitter during the month of the festival (March 2007). To reproduce this instrument, we collect information of all the followers of the @SXSW account of the South by Southwest festival, the date they joined Twitter, and the location set in their profile. The dataset we end up with is not entirely identical to the one used by Müller and Schwarz (2023): some users created on or before March 2007 might have started or stopped following SXSW later. They might also have changed their location between the time Müller and Schwarz collected their dataset and when we collected ours (2019 versus November 2021). Finally, our geolocation method might be different: we automatically geocode the location given by the user using Nominatim, as described in the Data section. Müller and Schwarz (2023) do not detail their geolocation method. For comparison, we attribute 52% of users to US counties (excluding imprecise locations and locations outside the US). In comparison, Fujiwara et al. (2023) (reusing this instrument) are able to locate 58% of users that joined Twitter between 2006 and 2008 using a similar method.

For each county we compute the number of followers whose account was created in March 2007 and the number of users whose account was created before this date. With our data collection and user localization strategy, we find users that follow @SXSW and joined in March 2007 in 172 counties, only 67 of which did not have BLM events before (Müller and Schwarz (2023) find 155 affected counties). To increase the number of treated counties, and thus the power of our identification, we also consider users in neighboring counties: assuming that Twitter presence diffuses, in part, geographically,[29] these counties should also have a higher number of Twitter users. We find 817 such counties, 618 of which did not have a BLM protest before.

We estimate the log number of observed Twitter users in December 2019 using the number of users that joined during SXSW controlled by the number of SXSW followers that joined before. This variable controls for the interest in SXSW festival and acts as a proxy control for the general interest in Twitter in the county. The specification is as follows:

$$\begin{aligned} Twitter_c =& \xi_0 + \xi_1 SXSW\,Users_{sc} + \xi_2 SXSW\;Pre\,Users_{sc} \\ &+ \mathbf{X}_c \xi_{\mathbf{X}} + \gamma_s + \eta_{cs} \end{aligned} \tag{8}$$

where SXSW Users_{sc} is the logarithm of one plus the number of SXSW followers who created their account in March 2007 in the county and neighboring counties, and Pre SXSW Users_{sc} is the logarithm of one plus the number of SXSW followers in the county and neighboring counties that created their account before March 2007.

C.2 Comparison of SSE and SXSW

In this section, we compare the push-pull instrument with using each sub-component to predict new Twitter users. Recall that the baseline instrument leverages the interaction between SSEs and SXSW followers and controls for both in the first stage. We posit that SSEs drive new users while SXSW primarily predict old users. To show this more systematically, in Table C3, we investigate how the SXSW instruments performs in predicting new Twitter users compared to SSE alone, as well as the interaction between the two. In a first step, in column 1, we estimate equation 3, this time using just the SXSW instrument as a predictor for new users and find that SXSW delivers a sufficiently strong first stage and that new users drive BLM protest. In column 2, we compare this to using SSEs alone to predict new users. The SSE instrument alone delivers a weaker first stage but larger effect size, suggesting

[29]This assumption is also made by Müller and Schwarz at the level of a county. Here we just extend it to neighboring counties.

that counties that adopted Twitter in response to SSEs respond more to the protest trigger. Column 3, replicates our main specification.

Next, in columns 4 to 6, we control for old users (baseline users in 2019) and pandemic exposure (COVID-19 related deaths at the county level just before the murder of Geroge Floyd) in the second stage. This allows us to see whether the various instruments predict new users above and beyond their separate effects on pandemic exposure and the existing network. Column 4 reports the results when using the SXSW instrument alone to predict new users and controlling for baseline users as well as pandemic exposure. The first stage F-statistic drops substantially compared to column 1 and the effect of new users on BLM protest becomes imprecisely estimated. This suggests that SXSW does not predict new users well once we control for baseline use. In column 5, we do the equivalent exercise for the SSE instrument. We find that SSEs predict new users above and beyond its effect on local COVID-19 deaths. In last step, in column 6, we use our preferred instrument but control for baseline users and pandemic exposure also in the second stage. This captures any direct effect of these variable on BLM protest. The first stage is below the conventional threshold, but reassuringly, we find that new users - as predicted by our preferred push-pull instrument - significantly drive BLM protest above and beyond the separate effects from baseline use and pandemic exposure. The effect size is statistically indistinguishable from our baseline specification that does not include these controls in the second stage.

Table C1: Effect of SXSW users on Twitter presence

Outcome:	Log(Preexisting Twitter users in 2019)		
	(1)	(2)	(3)
Subsample:	All counties	No BLM events before	Has BLM event before
Log(SXSW users)	0.394***	0.373***	0.0447
	(0.108)	(0.103)	(0.130)
Log(Pre-SXSW users)	0.361***	0.382***	0.0722
	(0.0764)	(0.0896)	(0.0802)
Observations	3,106	2,767	333
Mean of dep. var	2.034	1.738	4.439
F statistic	13.44	13.02	0.119
All controls	Y	Y	Y
State fixed effects	Y	Y	Y

Note: This table shows the first stage regression for predicting existing Twitter users at the end of 2019 (measured in the December 2019 sample of tweets) in the county using SXSW followers that joined Twitter during the festival in the county and its neighboring counties. Column 1 shows the result for all counties, column 2 for the sub-sample of counties that did not hold a BLM protest before the murder of George Floyd, and column 3 for the sub-samples of counties that did. All specifications include state fixed effects and all standard controls. We report Kleibergen-Paap rkWald F statistic. Standard errors (in parentheses) are clustered at the state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C2: Effect of pre-existing Twitter users and of the push-pull instrument on new users during the pandemic

Outcome:	Log(New users)		
	(1)	(2)	(3)
Subsample:	All counties	No BLM events before	Has BLM event before
Panel A: Preexisting users			
Log(Preexisting users in 2019)	0.413***	0.370***	0.740***
	(0.0173)	(0.0174)	(0.0563)
Observations	3,106	2,767	333
Mean of dep. var	0.615	0.445	2.007
Panel B: Push-pull instrument $Z_c \times N_c$			
Z_c	0.00253	0.000402	0.000132
	(0.00323)	(0.00378)	(0.00484)
N_c	0.222***	0.181***	0.0439
	(0.0659)	(0.0613)	(0.148)
$Z_c \times N_c$	0.00101	0.00617**	0.000140
	(0.00249)	(0.00286)	(0.00327)
Observations	3,106	2,767	333
Mean of dep. var	0.615	0.445	2.007
Kleibergen-Paap F stat	5.856	9.587	0.0523
All controls	Y	Y	Y
State fixed effects	Y	Y	Y

Note: This table shows the effect of pre-existing Twitter users on new Twitter users during the pandemic, and of the push-pull instrument on new Twitter users during the pandemic. Column 1 shows the result for all counties, column 2 for the sub-sample of counties that did not hold a BLM protest before the murder of George Floyd, and column 3 for the sub-samples of counties that did. Panel A present the effect of Twitter users measured prior to the pandemic in December 2019 while panel B shows the effect of the push-pull instrument $Z_c \times N_c$. All specifications include state fixed effects and all standard controls. In panel B, we report Kleibergen-Paap rkWald F statistic for the first-stage prediction of preexisting users. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table C3: Variants of the instrument

	at least one BLM protest					
instrument	SXSW (1)	SSE (2)	SSExSXSW (3)	SXSW (4)	SSE (5)	SSExSXSW (6)
new Twitter users	0.154*	0.381*	0.243**	0.279	0.381*	0.420**
	(0.0861)	(0.204)	(0.0959)	(0.247)	(0.216)	(0.161)
old Twitter users				-0.0679	-0.103	-0.117**
				(0.0851)	(0.0747)	(0.0556)
COVID deaths (per 1000)				0.0457*	0.0515**	0.0526**
				(0.0255)	(0.0254)	(0.0258)
Observations	2767	2767	2767	2767	2767	2767
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.0477	0.0477	0.0477	0.0477	0.0477	0.0477
Kleibergen-Paap F stat	11.32	3.194	9.587	3.867	5.742	5.327

Note: This table shows the first stage regression for predicting existing Twitter users at the end of 2019 (measured in the December 2019 sample of tweets) in the county using SXSW followers that joined Twitter during the festival in the county and its neighboring counties. Column 1 shows the result for all counties, column 2 for the sub-sample of counties that did not hold a BLM protest before the murder of George Floyd, and column 3 for the sub-samples of counties that did. All specifications include state fixed effects and all standard controls. We report Kleibergen-Paap rkWald F statistic. Standard errors (in parentheses) are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1