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UNITED NATIONS  
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WIDER Working Paper 2021/57

# **Do pandemics lead to rebellion? Policy responses to COVID-19, inequality, and protests in the USA**

Francesco Iacoella,<sup>1</sup> Patricia Justino,<sup>2</sup> and Bruno Martorano<sup>3</sup>

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**Abstract:** This paper analyses how inequality across counties in the United States of America has shaped the impact of the COVID-19 pandemic on the incidence of protests. The empirical analysis combines weekly data between January and December 2020 on levels of COVID-19-related policy stringency and protest incidence at the county level with measures of county-level inequality at the start of the pandemic. The results show that more stringent measures to contain the pandemic were instrumental in driving the incidence of protests, but only in counties with high levels of inequality before the start of the pandemic, where grievances may have been initially stronger. Further analysis suggests that the impact of government responses to the COVID-19 pandemic is largely explained by changes in economic conditions (rise in unemployment and changes in economic activities and spending) in counties with the highest levels of inequality. Unequal counties with lower trust in political institutions but higher levels of social trust and civic engagement at the start of the pandemic are also more likely to experience more protests as a response to more stringent policies.

**Key words:** I18, D63, D70

**JEL classification:** COVID-19, lockdown, protests, inequality

**Note:** figures and tables at the end

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## 1 Introduction

Understanding the causes and consequences of protests and organized collective dissent has been a longstanding question in political economy. This literature has identified several determinants of protests, ranging from economic and social exclusion to political ideology, push back against repression, and the expansion of social media (Tarrow 1998; Chenoweth and Stephan 2008; Sangnier and Zylberberg 2017; Steinert-Threlkeld 2017; Larson et al. 2019; Rozenas and Zhukov 2019; Enikolopov et al. 2020; Fergusson and Molina 2020; Manacorda and Tesei 2020; Wasow 2020).

One of the earliest and most prominent theories of civil unrest focuses on the role of economic inequality in motivating deprived groups and individuals to protest (Gurr 1970; Lichbach 1989; Boix 2008). However, empirical analyses of the direct impact of inequality on protests have generated ambiguous results (Nollert 1995; Dubrow et al. 2008; Solt 2015), and we have only limited understanding of what triggers may lead those at the bottom of the income distribution to protest. We know even less about the causal mechanisms that may explain a possible relationship between inequality and protests. This paper seeks to address these questions in the context of the ongoing COVID-19 pandemic.

The global COVID-19 pandemic offers a unique opportunity to study the relationship between inequality and protests. This pandemic is a global disaster that has taken the world by surprise. At the time of writing, almost 120 million people worldwide have been diagnosed with the virus and over 2.5 million have died because of it. First identified in China's Wuhan region in December 2019, the virus Sars-Cov-2 (the cause of COVID-19) rapidly spread to the rest of the world. By April 2020, most countries had adopted stringent measures to reduce population movements and social gatherings to avoid its diffusion. After a brief lull during the summer months, the number of reported cases started to dramatically increase from September 2020 and intensive care units filled up again. As a result, governments across the world introduced a new round of lockdown measures and other public health interventions to reduce the spread of the virus.

As the virus spread, so did social discontent: protests across the world increased by almost 30 per cent between January 2020 and January 2021.<sup>1</sup> Media accounts describe how restrictive measures on population mobility have fuelled public demonstrations and sometimes violent protests in several countries since the start of the pandemic. In Italy, restaurant owners took to the streets in Cremona and taxi drivers lined up in neat rows in Piazza Castello in Turin to protest against lockdown measures during the second wave of the pandemic.<sup>2</sup> In Germany and France, protests against movement restrictions have erupted too, even though generous social policy measures were introduced to cushion living standards against the negative consequences of the crisis.<sup>3</sup> In the United States of America (USA), angry protesters have taken to the streets since mid-April 2020 to voice their anger over lockdown restrictions or racial discrimination and to rally against or in support of Trump.<sup>4</sup>

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<sup>1</sup> <https://acleddata.com/#/dashboard>

<sup>2</sup> See <https://www.bbc.com/news/world-europe-54701042>.

<sup>3</sup> See <https://www.theguardian.com/world/2020/nov/02/latest-coronavirus-lockdowns-spark-protests-across-europe>.

<sup>4</sup> See <https://www.bbc.com/news/av/world-us-canada-52344540>.

While COVID-related protests in the USA and elsewhere in the world seemed initially to be fuelled by political polarization and ideological anger at government-imposed restrictions, there is now a growing understanding that these public demonstrations might reflect ingrained discontent against persistent inequalities, structural divisions, and discrimination—which have been amplified by the different impact of the pandemic across different racial, ethnic, and economic groups. Highest risk areas may have faced a ‘perfect storm’, where public dissent driven by the economic costs associated to the health crisis may have magnified long-standing social and economic grievances. Evidence of this vicious cycle of public health crisis, persistent inequalities, and social discontent is, however, largely anecdotal. The aim of this paper is to empirically disentangle the role of pre-existing inequality in explaining the relationship between policy restrictions to contain the spread of COVID-19 and the incidence of protests in the USA and what mechanisms may explain such a relationship.

While plenty of media accounts have linked the rise of protests in the USA in 2020 to the spread of the COVID-19 pandemic, the causal relationship between these two phenomena is theoretically much less clear. Recent evidence on the social, economic, and psychological effects of the COVID-19 pandemic may explain how government-imposed restrictions may have caused social discontent, which subsequently led to protests. The length of lockdowns and stay-at-home orders may have driven people to resent social isolation and social distance measures (Briscese et al. 2020; Armbruster and Klotzbücher 2020) and to become less willing to accept restrictions to their freedom and liberties (Alsan et al. 2020), particularly as unemployment and economic insecurity rose (IMF 2020; Li and Coppo 2020; Sedik and Xu 2020). When perceptions of unfairness and anger at economic decline and social isolation reach certain thresholds, many react by engaging in protests and demonstrations (Justino and Martorano 2019).

However, a pandemic caused by a virus like COVID-19, which is easily transmissible via close social contact, may increase social discontent via the mechanisms outlined above without affecting the incidence of protests. This is because people try to socially distance from each other to avoid exposure to the virus and avert infecting others. Compliance with government social distance policies and fear of falling ill or of spreading the virus to others will increase the individual costs of participating in protests. The escalation of violence during protests—which became a concern across US states—may have also constrained protest participation (Dave et al. 2020a, 2020b). In addition, policing of curfews and social distance guidelines may have increased the social coordination costs of protests—due to the likelihood of large gatherings being broken up by law enforcement.

Overall, there was an increase in protests in the USA in 2020 in relation to previous years (see Figure B1 in Appendix B), but with large variation across different counties, as shown in Figure 1. Why did protests rise when a pandemic was rampant, and what explains variation in protest incidence during the COVID-19 pandemic across US counties? To answer these questions, we use a new dataset compiled by the US Crisis Monitor, a collaboration between the Armed Conflict Location & Event Data Project (ACLED) (Raleigh et al. 2010) and the Bridging Divides Initiative at Princeton University. The US Crisis Monitor collected real-time data on protests across US counties between January and December 2020.<sup>5</sup> To measure the impact of COVID-related policies, we exploit the Oxford COVID-19 Government Response Tracker (OxCGRT) (Hale et al. 2020), which collects systematic information on policy responses to the pandemic taken by governments in 180 countries. The OxCGRT classifies policies across levels of stringency (from

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<sup>5</sup> The analysis in this paper does not include the riot and attacks to the Capitol on 6 January 2021. Van Dijcke and Wright (2021) document the characteristics of participants in the riot, while Dave et al. (2021) analyse the effect of the January 6 riot on the spread of COVID-19 in the localities of the rioters.

1 to 100) used to restrict the spread of COVID-19. For each US county, we match weekly-aggregated figures on COVID-related protest events with weekly changes in the level of stringency of pandemic-related policies. The detailed level of disaggregation of these datasets across units of analysis and time allows us to compare weekly variation in protest incidence across US counties which experienced different degrees of policy restrictions in response to the expansion of COVID-19. To test the role of inequality, in addition to estimating these average effects, we use data on inequality at county-level compiled by the United States Census Bureau to estimate the non-linear effect of policy stringency on protest incidence across county-level inequality quintiles.

In all estimations, we use county fixed effects and state-week fixed effects to generate within-county estimates of the effect of policy restrictions on protest incidence across county inequality levels between January and December 2020. This is akin to a difference-in-differences strategy that compares protest incidence following the implementation and changes in COVID-related restrictive policies in the US throughout 2020.<sup>6</sup> This strategy reduces potential biases resulting from policy measures and protests being correlated with county-specific trends or changes in unobservable variables (such as, for instance, localized economic shocks). However, it is still possible that policy stringency levels and protests are correlated with each other due to unobservable time-varying county-level characteristics or reverse causality. To address these concerns, we use an instrumental variable (IV) strategy using the number of COVID-19 cases in neighbouring states as a source of exogenous variation. We also exploit levels of policy stringency in neighbouring states as an additional IV. Both ordinary least squares (OLS) and two-stage least squares (2SLS) results show that more stringent policies have fuelled protests across the USA, but only in the most unequal counties. The coefficients indicate that a one standard deviation increase in the stringency index leads to a six-percentage point increase in the probability of a protest taking place in the most unequal counties. When analysing the effect of each component of the stringency index separately, results show that stay-at-home orders, restrictions on domestic travel, and public transport restrictions have the largest impact on protest incidence in the most unequal counties. These results are robust to alternative samples, different standard errors clustering methods and fixed effects, and alternative constructions of the independent and dependent variables.

To investigate the mechanisms through which government policy restrictions may affect the incidence of protests, we test two main hypotheses. According to the existing literature, shocks like the COVID-19 pandemic may magnify economic inequalities and lead to protests due to changes in economic conditions or due to changes in political and social preferences (Enos et al. 2019; Rozenas and Zhukov 2019; Enikolopov et al. 2020). The first mechanism—the deterioration of economic prospects—has been shown by political scientists, sociologists, and economists to be central to social mobilization, protests, and civil unrest (Gurr 1970; Boix 2008; Verwimp et al. 2019; Iacoella et al. 2020; Ponticelli and Voth 2020). There is a growing consensus that the COVID-19 pandemic will lead the global economy into the worst recession since the Great Depression (IMF 2020). Recent evidence shows also that past health crises have contributed to increasing social instability—even though they were smaller in scale than the current COVID-19 pandemic—due to their effects on socio-economic outcomes (González-Torres and Esposito 2020; Li and Coppo 2020; Sedik and Xu 2020). Lockdowns and other restrictive measures implemented to reduce the spread of COVID-19 have adversely affected labour markets and led to substantial job losses, particularly among those employed in low-skilled jobs (Sedik and Xu 2020). These drastic reductions in living standards in a short period of time may stimulate grievances and feelings of injustice, motivating people to protest.

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<sup>6</sup> California was the first state to issue stay-at-home orders on 19 March: <https://www.gov.ca.gov/wp-content/uploads/2020/03/3.19.20-attested-EO-N-33-20-COVID-19-HEALTH-ORDER.pdf>.

Political preferences may have also contributed to explaining people's reactions to government responses to the ongoing COVID-19 pandemic. Notably, it appears that the COVID-19 crisis has led some groups to embrace populist and more extreme political positions, particularly among those who perceive preventive measures, such as wearing face masks or staying at home, as unjustified interventions that threaten human rights and individual freedoms.<sup>7</sup> Social psychology and political science scholars have long highlighted the role of group identity and political preferences in promoting social mobilization (Tilly 1978; Simon and Klandermans 2001; Dalton et al. 2010; Tilly and Tarrow 2015; Verba et al. 1995; Wasow 2020). The recent Black Lives Matter (BLM) movement is an example of how these factors can play an important role in explaining people engagement in protests. Recent evidence also shows that participation in the Capitol insurrection on 6 January 2021 was connected to political preferences in previous elections (van Dijke and Wright 2021). In addition, factors such as social and political trust and levels of civic engagement and social cooperation may play key roles in mobilizing individuals to protest, including during COVID-19 (Tarrow 1998; Justino 2015; Sangnier and Zylberberg 2017; Acemoglu et al. 2018; Justino and Martorano 2018, 2019; Larson et al. 2019; Aghajanian et al. 2020; Manacorda and Tesei 2020; Levi and Stoker 2000).<sup>8</sup>

The mechanism analysis we conduct in the paper lends stronger support to the economic downturn hypothesis. Results show that more restrictive measures to curb population movements and social gatherings during COVID-19 have increased unemployment and reduced economic activities and spending in US counties with initially higher levels of inequality. We find limited direct support for the political preferences hypothesis, although further heterogeneity analysis suggests that policy stringency measures were associated with more protests in the most unequal counties which voted for the Democratic party in the 2016 elections and exhibited, at the start of the pandemic, lower levels of trust in the president, lower satisfaction with democracy, and higher levels of social capital (which may have facilitated protest coordination).

The paper contributes to several literature strands. First, we show strong evidence for a causal effect of the interaction between inequality and policy stringency on protest incidence during the ongoing pandemic due to the adverse economic effects of policies to contain the spread of the virus. A large literature in political science and sociology has long postulated a positive relationship between economic inequality and protests (Gurr 1970; Lichbach 1989; Boix 2008; Solt 2015; Tilly and Tarrow 2015). However, the causal identification of such a relationship and of the mechanisms that may shape it has been more elusive. Our results effectively support an extended version of Gurr's original deprivation theory whereby reactions to government policies (like those related to COVID-19) affect the likelihood of protests taking place in communities where grievances already exist.

Second, we contribute to a new and growing literature analysing the social and economic consequences of pandemics, including the ongoing COVID-19 pandemic (Almond 2006; Voigtländer and Voth 2013; Blair et al. 2017; Atkeson 2020; Barro et al. 2020; Deb et al. 2020; Eichenbaum et al. 2020; Galletta and Giommoni 2020; González-Torres and Esposito 2020). Less is known about their political consequences (Aksoy et al. 2020), although some studies have suggested that the COVID-19 and other pandemics may affect the popularity of incumbent

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<sup>7</sup> See <https://www.nytimes.com/2020/05/03/us/coronavirus-masks-protests.html> and <https://www.bbc.com/news/world-latin-america-52351636>.

<sup>8</sup> Civic capital and social preferences have also been shown to be key determinants of adherence to COVID-19 restrictions (Aghajanian et al. 2021; Barrios et al. 2021; Brück et al. 2020; Egger et al. 2020; Barrios and Hochberg 2020; Allcott et al. 2020; Dasgupta et al. 2020; Durante et al. 2021; Bargain and Aminjonov 2020; Borgonovi and Andrieu 2020; Brodeur et al. 2021).

governments, conditional on their effectiveness in containing the spread to the virus (Flückiger et al. 2019; Bol et al. 2020; Giommoni and Loumeau 2020; Herrera et al. 2020). We contribute to this literature by showing how the COVID-19 pandemic has amplified existing economic inequalities in ways that increased social tensions and protests.

Finally, we add to a body of work on the links between rising inequality and political polarization in the US (Hacker 2006; Bartels 2008; McCarthy et al. 2006) and the consequences of such polarization for sustainability of democracy (Bartels 2008; Hacker and Pierson 2010; Gilens 2012). This literature argues that rising economic inequality in the US since the 1980s has led to increased political polarization, the capture of the political system by those at the top of the income distribution, increased disenfranchisement of the economically excluded, and the erosion of democratic structures. We show that counties with initially high levels of economic (and racial) inequality were in fact more likely to protest during the COVID-19 pandemic against government policies that further deteriorated their economic standing—despite those policies curtailing the spread of the pandemic. These results are of relevance to the USA but also to other countries where political polarization has been growing alongside economic inequality, and we discuss these implications further in the conclusion.

The paper proceeds as follows. Section 2 discusses the evolution of the COVID-19 pandemic in the US, the persistence of inequalities, and the recent rise in protests and presents the datasets we will use in the analysis. Section 3 discusses the main empirical strategy. Section 4 presents the main results and robustness analysis. Section 5 analyses the mechanisms that may explain the main results. Section 6 concludes the paper.

## **2 Context and data**

The paper analyses the relationship between public health policies, inequality, and protests across US counties (third-level administrative units). The final sample includes 3,142 US counties from 50 states and the District of Columbia. For each county, we compiled time-variant information on: (i) weekly-aggregated figures on COVID-related protest events, (ii) weekly changes in COVID-related policies, with a specific focus on movement-restriction policies, and (iii) a measure of income inequality, expressed as a Gini index, for 2019. The period of analysis runs between January and December 2020.

### **2.1 COVID-related protest events**

2020 was a year of turmoil all around the world. In the case of the USA, politically motivated violence and instability characterized the period up to the 2020 general election in November, fuelled by increased political polarization, violent hate crimes, and large-scale social movements. Several protests in the USA in 2020 were motivated by social discontent against police brutality, led by the Black Lives Matter (BLM) movement and sparked by the killing of George Floyd in Minneapolis in May.<sup>9</sup> Demonstrations were also held in support and against President Trump and, from November, against the results of the general election—eventually leading to the insurrection of January 6 and the second impeachment of the former president.

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<sup>9</sup> See <https://web.archive.org/web/20200602235547/https://www.nytimes.com/article/george-floyd-protests-timeline.html>.



American citizens also took to the streets to protest their discontent towards the government's handling of the pandemic. From mid-April 2020, several states witnessed protests against COVID-19 stay-at-home orders.<sup>10</sup> In Michigan, citizens concerned about the economic fallout of movement restrictions marched through the streets of Lansing, the capital, armed with assault rifles, Trump signs, and American flags. In Utah, a state where movement restrictions remained relatively mild throughout 2020, protesters showed their anger against facial-covering and social-distancing policies, deeming them unconstitutional and a violation of their rights. Even less-conservative states like California and Colorado experienced their share of anti-lockdown protests, mostly motivated by fear of the economic and social consequences of business closures and social distancing.

All protest events that occurred in the US between 1 January and 31 December 2020 were recorded by the Armed Conflict Location & Event Data Project (ACLED) and the Bridging Divides Initiative (BDI) at Princeton University under the US Crisis Monitor initiative. The dataset includes dates, actors, locations, fatalities, and types of all political violence and demonstration events in the US.<sup>11</sup> All events are accompanied by a note describing the context of each event. Using these notes, we are able to code protest events based on their motives and actors. We identify four distinct types of protests: COVID-related protests, which represent our main outcome interest, Black Lives Matter-related protests, Trump-related protests, and general election-related protests.<sup>12</sup> Figure 2 illustrates the evolution of these different types of protest in 2020. The incidence of the four types of protests changed substantially across the year. BLM protests, which peaked at over 30,000 per week, are concentrated around the date of the killing of George Floyd in May. Protests in support of and against Trump and protests related to the presidential elections and Joe Biden's victory increase in the weeks leading to and following the general election. Protests related to COVID-19 are more evenly distributed but three peaks can be identified, coinciding with the COVID-19 waves in April, July, and November 2020. We base our main empirical analysis on the variable measuring protests related to COVID-19 but consider all other protest types later in the paper.

## 2.2 COVID-19 and containment policies

In an effort to curb the number of new COVID-19 infections, almost all countries across the world introduced policy measures to restrict the movement of people and social gatherings, both within and outside national borders. These policies have dramatically changed the ways in which billions of citizens conduct their daily lives (Fang et al. 2020). Since 31 December 2019, around 200 governments have implemented external border restrictions and internal movement restrictions to reduce the spread of COVID-19. In over 180 countries, governments have implemented nationwide or localized school closures to reduce the transmission of the virus between children and from children to adults (Azevedo et al. 2020; Cheng et al. 2020; Fang et al. 2020).

The US federal government was reluctant to respond to the COVID-19 pandemic with movement limitations and lockdown-style measures. Hence, state governors implemented their own

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<sup>10</sup> See <https://edition.cnn.com/2020/04/16/us/protests-coronavirus-stay-home-orders/index.html>.

<sup>11</sup> See <https://acleddata.com/special-projects/us-crisis-monitor/>.

<sup>12</sup> To code these categories, we identify whether each event's note contains specific terms and assign the event to the specific category. All events including the words 'virus', 'COVID', 'restriction', and 'pandemic' are coded as COVID-related protests. All events mentioning the word 'Trump' are coded as Trump-related protests. All events mentioning the words 'Biden' or 'election' are coded as election-related protests. BLM protests are codified based on the main actor reported by ACLED for each event.

measures, with large policy variation across states. As shown in Figure 1, coastal states consistently adopted more stringent policies in the period between January and December 2020, reflecting the faster spread of the virus in these areas (also the most populous in the country). In contrast, mid-western and southern states adopted much laxer restrictions and returned to them only in the summer, when holidaymakers and, later, university students acted as super-spreaders.<sup>13</sup>

Policy measures enacted in response to the COVID-19 pandemic have been meticulously recorded by the Blavatnik School of Government at the University of Oxford (Hale et al. 2020). The Oxford COVID-19 Government Response Tracker (OxCGRT) collects systematic information on enacted ‘lockdown’-style measures and scores the stringency of such measures. A total of 19 indicators are currently being monitored across 180 countries. Some of these indicators, particularly those related to movement and gathering restrictions, are aggregated into a Stringency Index ranging between 1 and 100, with 100 representing complete lockdown. We rescaled the Stringency Index into a continuous variable ranging between 0 and 1, with higher values representing more restrictive conditions.

Data show that stringency measures peaked in the USA in April, when the index reached an average of 0.60 (Figure 3). Differences in stringency measures across states are substantial even at this peak stage, with Arkansas, North and South Dakota, Utah, and Iowa showing the lowest levels of restrictions, and Maryland, Maine Kentucky, and Idaho showing the highest levels (index above 0.85). From April onwards, restrictions were relaxed in almost every state (except for New Mexico), although again with large variation across states. Some states, such as Iowa and Oklahoma, experienced a large drop in restrictions in May–June and remained stable afterwards. Most of the country, however, slowly and gradually lowered restrictions until October, when they reached the lowest point in the index (0.35). After October, most states increased restrictions to social movement in response to the surge in cases.<sup>14</sup>

### 2.3 Inequality across US counties

Inequality in the USA is higher than in almost any other developed country (Piketty 2014). Disparities, particularly with respect to economic status and race, started to increase in every state since the late 1970s and were exacerbated after the Great Recession of 2007–08 (Boushey 2020). In 2019, the USA had a Gini coefficient of 0.46, far above the average Gini coefficient in other OECD countries.<sup>15</sup>

In this study, we use information on Gini index calculated using the United States Census Bureau data. The Gini index is calculated at county-level by the Census Bureau using household income data from 2019 American Community Survey. Estimates are available for 3,103 of the 3,142 counties in the USA.<sup>16</sup> The Gini index ranges between 0 (perfect equality) and 1 (perfect inequality). The minimum value observed in our sample is 0.30 and the maximum is 0.71—indicating large variation in income inequality across US counties (Figure 4). The most unequal counties are located

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<sup>13</sup> See <https://www.reuters.com/article/uk-health-coronavirus-usa-idUKKCN24V2G5?edition-redirect=uk>.

<sup>14</sup> Hawaii is an exception: restrictions in the state were tightened from August to curb the spread of the virus caused by increased tourism. South Dakota, in contrast, has been consistently reducing its restriction policies.

<sup>15</sup> See <https://www.pewresearch.org/fact-tank/2020/02/07/6-facts-about-economic-inequality-in-the-u-s/>.

<sup>16</sup> Missing counties are due to inconsistency in identification codes, which did not allow us to match the inequality data and our main dataset for all US counties.

in the District of Columbia and in the South East—in particular, in the states of Louisiana and Mississippi. The most equal counties are located in Utah.

### 3 Empirical strategy

The empirical analysis we conduct in this paper is based on a regression model that expresses protest incidence as a function of the stringency of anti-COVID-19 policies and its interaction with the level of inequality in each US county. Formally, we have:

$$Y_{xit} = \beta_0 + \beta_1 \text{Stringency}_{it} + \beta_2 \text{Stringency}_{it} * \text{Gini}_{xi} + \beta_3 \text{Health controls}_{xit} + \beta_4 \text{Controls}_{xit} + \delta_x + \theta_{it} + \varepsilon_{xit}, \quad (1)$$

where  $Y_{xit}$  is a binary indicator with value one if a protest related to COVID-19 occurred at the county level  $x$ , in state  $i$ , and in week  $t$ . Our main independent variables are the index measuring the stringency of anti-COVID-19 policies in state  $i$  in the week  $t$  and the interaction term,  $\text{Stringency}_{it} * \text{Gini}_{xi}$ , which represents the index measuring the stringency of anti-COVID policies interacted with the Gini index in the same county-state pair. The latter variable intends to capture the implementation of restrictive policies, conditional on the level of inequality in each county (measured by the Gini index). Coefficient  $\beta_2$  thus measures how county-level restrictive policies may translate into variation in protest activity in areas with different levels of inequality. If, as we hypothesize, inequality magnifies the effect of restrictive policies on protests, this coefficient will be positive.  $\beta_1$  captures the effect of policy stringency on protests at average levels of inequality.

Equation (1) implies that the effect of the level of policy stringency on protest incidence varies linearly with the level of inequality. However, it might be possible that the impact of policy stringency varies across different levels of inequality in a non-linear manner, as hypothesized in the introduction to the paper. To capture these non-linearities, we interact levels of policy stringency with quintiles of the Gini index, as follows:

$$Y_{xit} = \beta_0 + \beta_1 \text{Stringency}_{it} + \beta_2 \text{Stringency}_{it} * q1_{xi} + \beta_3 \text{Stringency}_{it} * q2_{xi} + \beta_4 \text{Stringency}_{it} * q3_{xi} + \beta_5 \text{Stringency}_{it} * q4_{xi} + \beta_6 \text{Stringency}_{it} * q5_{xi} + \beta_7 \text{Health controls}_{xit} + \beta_8 \text{Controls}_{xit} + \delta_x + \theta_{it} + \varepsilon_{xit}, \quad (2)$$

where  $q$  refers to quintiles of the Gini index, with  $q1$  indicating counties with the lowest levels of within-county inequality and  $q5$  representing the most unequal counties.

Equations (1) and (2) also include two sets of controls. First, we introduce a vector of health control variables (*Health controls*) to take into consideration a possible direct effect of the intensity of the health crisis on protests (Barrios et al. 2021). These controls include the (log) total number of COVID-19 cases and the (log) total number of COVID-19 deaths in each county/week. Data are obtained from the Opportunity Insights Economic Tracker, a project compiling private-sector sourced data about the COVID-19 pandemic and several economic outcomes since January 2020.<sup>17</sup> In the case of COVID-19 cases and deaths, the tracker relies on

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<sup>17</sup> See <https://tracktherecovery.org>.

the New York Times county-level daily data, which we aggregate for each calendar week. The definition and sources for all variables used in the paper are provided in Table A1 in Appendix A. Other controls are based on a longstanding literature showing a close association between the incidence of protests and several social and economic variables (Almond and Verba 1963; Dahl 1971; Dalton et al. 2010; McCarthy and Zald 1977; Verba and Nie 1972): percentage of non-white population, percentage of people employed in services, percentage of people aged 25 years or older with a university degree, and (log) per capita income. These data were obtained from US Census Bureau data for 2019 and measured at county level. Since the data refer to fairly stable cross-sectional county characteristics that change only very slowly across time, we interact these indicators with a linear time trend (see, for instance, Manacorda and Tesei 2020). Madestam et al. (2013) show, in addition, that political protests can be affected by local weather conditions. We control for weather conditions using the (log) average rainfall for each county-month. Monthly rainfall data for 2020 was obtained from the Climate Hazards Group InfraRed Precipitation with Station (CHIRPS) data archive.<sup>18</sup>

To capture unobserved heterogeneity at the national and the local level, we also include in the regressions above week varying state ( $\theta_{it}$ ) and county ( $\delta_x$ ) fixed effects. The baseline estimation uses weights based on the size of the population at the county level. Standard errors are clustered at the state level to allow for unrestricted correlation in the error term across observations in the same state; other clustering methods are explored later in the paper.

## 4 Empirical results

### 4.1 Main results

Table 1 presents the results from our main model specifications (1) and (2) above. Column 1 shows that our coefficient of interest—the interaction term between stringency and inequality—has a positive and statistically significant impact on the number of protests. Moving to equation (2), column 2 shows that protests respond more to the interaction between restrictive policies and inequality in less equal counties. The interaction terms between the index of stringency of policy responses and all inequality quintiles are always positive but the interaction term is statistically significant only for the most unequal counties (in quantiles 4 and 5 of the Gini coefficient). In columns 4, 5, and 6, we add population weights, fixed effects, and additional controls in sequence. The coefficients of interest remain largely unchanged across all specifications. Our preferred specification in column 6 includes population weights, county fixed effects, state-week fixed effects, and all controls discussed above. The results in this column indicate that a one standard deviation increase in the stringency index leads to an increase of six percentage points (i.e.  $0.221 \times 0.289$ ) in the probability that a protest occurs in the most unequal counties.

Figure 5 visually depicts the effect of policy stringency on protests at different levels of inequality. In Panel A of Figure 5, we constrain the effect of stringency to vary linearly with the level of inequality. Results confirm that the impact of stringency is magnified by the level of inequality: the effect is negative at very low levels of inequality and progressively becomes more strongly positive as inequality rises. Panel B of Figure 5 shows that the impact is not linear, justifying the decision of allowing the effect of stringency to vary across different quintiles of inequality. As discussed,

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<sup>18</sup> CHIRPS (v2.0) is a quasi-global, 5-degree gridded map of terrestrial precipitation estimates stretching from 1981 to 2020.

statistically significant effects are only found at the top of the inequality distribution, while the effects at the bottom of the distribution are close to zero.

The results are robust to changes in the baseline specification, different standard error clustering methods, and using different sets of fixed effects. Results are shown in Table B1 in Appendix B. In column 2, we exclude from the main regression all time-invariant controls, which were included in the model interacted with a linear time trend. In column 3, we re-estimate the baseline regression by using wild cluster bootstrap standard errors, to account for the number of clusters we have. The next two columns use standard errors clustered at the county and state-week levels, respectively. We also re-estimated equation (2) by replacing county with state fixed effects (column 6), and by adding state fixed effects to the main estimation (column 7). Results remain unchanged across all different specifications.

## 4.2 Endogeneity

The estimations above rely on the use of the interaction between the Gini index for county  $x$  before the outbreak of COVID-19 and the policy stringency indicator in state  $i$  implemented after the outbreak of the pandemic as a source of exogenous variation to explain changes in the level of protests for county  $x$  after the outbreak of COVID-19. One concern with this empirical strategy is that we may violate the exclusion restriction. This could happen if, for instance, the time invariant component (the Gini index) is endogenously determined. The structure of the data ensures that the incidence of protests during week  $t$  is unlikely to affect levels of inequality—which were recorded in the same county during the previous year. Controlling for county fixed effects and state-week fixed effects further lessens concerns about omitted variable biases and effectively reduces our identification strategy to a difference-in-differences setting, whereby we compare the impact of policy stringency on protests before and after the introduction of policies to restrain the spread of COVID-19.

Endogeneity may, however, persist if policy stringency levels and protests are correlated for other reasons. For instance, the level of policy stringency might be shaped by previous protests when they acted as super spreaders (see Dave et al. 2020a, 2020b, 2020c, 2021). To further address these concerns, we test if trends in the outcome variable (probability of protests) before the outbreak of the COVID-19 pandemic are parallel across different levels of exposure to the treatment (different levels of policy stringency and different levels of inequality). In this exercise, we follow Christian and Barrett (2017) and plot the observed number of protests over time separately for counties with levels of policy stringency above and below the median. We also run an additional test that considers the observed number of protests over time separately for low-middle unequal counties (those in the first three quintiles) and highly unequal counties (those in the two highest quintiles). To conduct this test, we need information on protest events prior to the beginning of the pandemic. Since the US Crisis Monitor data is only available from 1 January 2020, we test for parallel trends using the alternative Google’s Global Database for Events, Language and Tone (GDELT).<sup>19</sup> Results in Figure 6 show that trends in the probability of protest are parallel in areas with different levels of stringency (Panel A), as well as in those with different levels inequality (Panel B).

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<sup>19</sup> The GDELT Project processes and organizes information on events worldwide from digitalized newspapers and news agencies, as well as from web-based news aggregators. Events are coded following the Conflict and Mediation Event Observations (CAMEO) coding, which provides a list of approximately 15,000 events and 60,000 political actors.

Despite these reassuring tests, our results could still be biased if protests influence the policy measures introduced by governments to contain the spread of COVID-19. To further address these endogeneity concerns, we take inspiration from the recent contribution of Dietrich et al. (2021), who instrument policy stringency in one country using the average number of new COVID-19 cases in the region as a source of exogenous variation that affects the design of national policies. Given the federal organization of the USA, we instrument each state level of policy stringency using the average number of new COVID-19 cases in the neighbouring states. The choice of the instrument is justified by the fact that the evolution of the number of cases of COVID-19 in one state may affect the design of policy against COVID-19 in the neighbouring states. For example, a rapid increase in COVID-19 cases in one state might convince neighbouring states to introduce travel restrictions. At the same time, we assume that the number of new cases of COVID-19 in neighbouring counties affects protests in one state only through state policies. This is a reasonable assumption given that, in the case of the USA, failures to systematically address the COVID-19 pandemic at the federal level meant that each state was responsible for defining its own policies to contain the spread of the virus.

As an alternative instrument, we proxy also state-level stringency measures with the average level of policy stringency in the neighbouring states. It is plausible to assume the existence of spill-over effects in policy design across states sharing the same borders. Due to the absence of federal coordination and the lack of recent pandemic experience, states may have decided to impose certain restriction policies when they observed their neighbouring states doing so. It is likely that specific states introduce stringent measures to contain the rapid increase in COVID-19 cases if similar policies were effective in containing the virus in neighbouring states. Similarly, states might be more willing to relax policy restrictions against COVID-19 if they observe the same actions leading to positive outcomes in neighbouring states. In both cases, it is reasonable to assume that changes in policy against COVID-19 in neighbouring states affect protests in one state only through state policies.

Table 2 reports the IV estimates. Column 2 of Panel B shows that the stringency index is positively correlated with the number of COVID-19 cases in the neighbouring states. Column 3 of Panel B shows that the stringency index is positively correlated with the stringency index computed in the neighbouring states. In both cases, the strong first stage F-statistic attests for the relevance of our instrument, while the Anderson-Rubin Wald tests further confirm the validity of the estimates (Table 2). Panel A shows that the estimates obtained from the second stages of the 2SLS regression replicate the previous OLS results, showing a positive effect of policy stringency on protests in the most unequal counties.<sup>20</sup>

### 4.3 Disaggregating policy responses

In this section, we extend the main results by investigating the impact of specific policy interventions. To do that, we replace the stringency index with each of its components. As discussed before, the stringency index is built using eight of the 19 policy indicators recorded by OxCGRT. These eight indicators record specific information about containment and closure policies enacted in each US state with respect to school closures, workplace closures, public events cancellation, gatherings restrictions, public transportations restrictions, stay-at-home orders, and domestic and international travel restrictions. We test the separate effect of these eight components. Components are coded with values that range between 0 and 3 (or 4 for some

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<sup>20</sup> Conceivably, policies implemented in neighbouring states and their COVID-19 cases might have a lagged effect on the policies implemented in that state. For this reason, we replicate our 2SLS analysis lagging the IV by one week. Results remain comparable with main findings and are available upon request.

indicators).<sup>21</sup> Results in Table 3 show that coefficients representing these measures remain statistically significant when used individually. Stay-at-home orders, restrictions on domestic travel, and public transport restrictions have the largest impact on protest incidence in the most unequal counties.

#### 4.4 Other protest types

An important question is whether the results we have discussed in the paper so far indicate a specific effect of the stringency of policy responses on protests related to COVID-19 or reflect a broader relationship between policy stringency, inequality, and any type of protest that occurred in 2020. As discussed above, the US Crisis Monitor provides weekly updates on protests alongside notes on each recorded protest event. These notes have allowed us to differentiate between protests related to COVID-19, protests related to the Black Lives Matter (BLM) movement, protests in support of and against Trump, and protests related to the presidential election results and Biden's victory.

To isolate the effect of the stringency of policy responses on different types of protest, we have re-estimated the baseline regressions by substituting our main dependent variable with dummies for alternative protest events and for all protests (Table 4). The results show that the interaction between policy stringency and inequality *reduces* the probability of protests related to BLM, Trump, and the presidential election in counties in the top inequality quintiles. These results indicate that the interaction between inequality and policy stringency does not explain the rise in protests in the USA during the COVID-19 pandemic in 2020, except for protests related to COVID-19 policy restrictions.

This is an important result. Several studies have long pointed out that, with few exceptions, the very poor (those at the bottom of the distribution) rarely protest or engage in other forms of collective mobilization or political participation (see review in: Dalton 2017). This is largely because the opportunity cost of engagement in collective mobilization is higher for those at the bottom of the distribution than for those at the top due to the risk of losing income/wages, lack of time, and coordination challenges (La Ferrara 2002; Bardhan 2005; Dalton 2017). The results in Table 4 are in line with these analyses. This stands in contrast with our main result. Taken together, these two results suggest that counties with the highest levels of inequality were more likely to experience protests only when these protests were *directly* motivated by the negative economic effects of the pandemic. We discuss in more detail in the concluding section of the paper the implications of these results.

#### 4.5 Additional robustness tests

This section implements a series of additional robustness tests to assess further the validity of the result above when we consider different samples and use alternative definitions for the independent variable and dependent variable.

*Different samples.* We test here if specific states may drive the main results by running a series of regressions which exclude each US state at a time from the sample. We present the estimated coefficients graphically in Figure B2 in Appendix B. Results remain constant throughout the

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<sup>21</sup> As an example, a school closure indicator with value 0 indicates that no measure has been taken to reduce access to schools. A value of 1 indicates that the government has recommend partial school closings, while a value of 3 (the highest possible) indicates full closing of schools in the county.

sample. Even when the coefficient estimates drop the most (when Indiana and Massachusetts, respectively, are removed from the sample), values remain close to our main estimation coefficient.

*Alternative definitions of inequality.* We replicate the main estimates by using alternative measures of inequality. In column (2) of Table B2 in Appendix B, we replace the 2019 Gini with the Gini index calculated in 2018. The source, as before, is the US Census Bureau’s American Community Survey. In column 3 we replace the Gini coefficient with the income share held by households in the top five per cent of income earners. Data on income shares were compiled from the Geography of Social Capital in America project, collected across US counties by the US Congress Joint Economic Committee. In column 4, we use a measure of racial inequality as reported by the Black-White neighbourhood segregation level. This indicator measures spatial separation between black and white citizens within each county, with higher values indicating higher segregation. Data are compiled by the Geography of Social Capital in America project. Results remain unchanged when we use these alternative definitions of inequality.

*Alternative definitions of protests.* We test the robustness of our baseline estimation by replacing the dummy variable on whether a given county experienced protest events in a specific week with the log-transformation of the actual number of protests that took place in the same county (column 2 of Table B3 in Appendix B). This new variable captures the intensity of protests that occurred in each week across different states, allowing us to recalculate the main results at the extensive margin. The coefficient of interest remains positive and statistically significant for counties in the most unequal income quintiles. The result shows that a one standard deviation increase in the stringency index leads to an increase of 11 percentage points (i.e.,  $0.221 \times 0.520$ ) in the probability that a protest occurs in the most unequal counties.

## 5 Mechanisms and contextual factors

What mechanisms explain why policy stringency measures may have caused an increase in protests in the most unequal US counties in 2020? To answer this question, we first consider the two mechanisms discussed in the introduction: changes in economic conditions and changes in social and political preferences. Additionally, we test if the incidence of protests varies with important contextual factors, namely political preferences, political and social trust, and civic engagement.

### 5.1 Changes in economic conditions

Policy measures to contain the COVID-19 pandemic, especially lockdowns and stay-at-home orders, have caused severe economic damage, including the closure of many small businesses, reduction in consumer expenditure, and unemployment. To measure this mechanism, we use data from different sources. First, we accessed information on the number of small business opened (defined by having had at least one transaction in the previous three days) and on small business transactions and revenue data aggregated from several credit card processors. Data were obtained from the Opportunity Insights Economic Tracker. Second, we obtained information on consumer expenditure from credit and debit card spending, again from the Economic Tracker, which provides aggregated, anonymized, and seasonally adjusted figures across US counties from January 2020. These data are available for 1,685 of the original 3,142 US counties. Finally, we use data on unemployment levels from two distinct sources. First, we make use of weekly information compiled by the Economic Tracker from private workforce management platforms. Data on unemployment from these sources is available for 770 counties and allow us to calculate unemployment levels for low-, middle-, and high-income groups. Second, we accessed data on county-level unemployment from the Bureau of Labor Statistics (BLS). These data are available



for 3,141 counties, although they are collected monthly (not weekly). All indicators from the Economic Tracker are originally calculated as differences from January 2020 baseline values,<sup>22</sup> and data from BLS is modified to take the same form. Using these baseline values, we calculate as our outcome of interest differences in each variable in relation to the previous week/month.

Results for this analysis are reported in Table 5. Although some of the results need to be interpreted with care due to drops in sample size for some variables, we find, as expected, a negative and significant impact of the interaction between inequality and policy stringency on the number of opened small business, total revenue, and consumer expenditure in the most unequal counties. Policy stringency also appears to have reduced employment levels in the most unequal counties, and more so for low-paid employees. The findings are consistent whether the Economic Tracker or BLS data are used.

## 5.2 Changes in political preferences

As discussed in the introduction to the paper, the effects we observe may also be due to changes in political preferences. We proxy political preferences by state-level pre-electoral polls on preferences for the 2020 presidential candidates. Information is compiled daily from different sources by the FiveThirtyEight project from *ABC News* and averaged at week-level by the authors (although data is missing for several states/weeks). Results in Table 6, which must be interpreted with caution given the small sample size, indicate that the interaction between policy stringency and income inequality did not have a significant impact on political preferences before the election. Changes in political preferences post-COVID-19 are, however, very difficult to capture given the ongoing pandemic. To supplement the analysis above, we ask in the next section whether initial political preferences may have shaped our main results.

## 5.3 Social and political context: heterogeneity analysis

In this section, we add to the narrative above by examining heterogenous effects of the estimates across the distribution of initial political preferences at the start of the pandemic at the county level. We do this in several ways in Table 7. First, we split the sample across electoral results for the 2016 presidential elections along counties won by the Republican party and counties won by the Democratic party. Data are compiled by the Geography of Social Capital in America project. Results (columns 1 and 2) show that policy stringency is more likely to result in protests in the more unequal counties that also voted for the Democratic party in the 2016 elections.

Second, political preferences can be expressed in terms of voting preferences but also in how citizens trust their state institutions and democratic processes. Levels of political trust have been repeatedly shown to be negatively associated with social mobilization (Justino 2015; Sangnier and Zylberberg 2017; Justino and Martorano 2018, 2019; Levi and Stoker 2000). To capture these effects, we split the sample into counties showing trust levels in the president above and below the mean, respectively (Table 7, columns 3 and 4). These variables are measures on a scale of 1 to 7 in a Likert-scale.<sup>23</sup> In columns 5 and 6, we include the percentage of individuals (above and below the mean) who are satisfied with democracy. Information on these variables was obtained from

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<sup>22</sup> Baseline values are calculated as the average of small business revenue, small business openness, and employment between 1 January and 31 January 2020. More details on the methodology used to build the variables is available here: [https://raw.githubusercontent.com/OpportunityInsights/EconomicTracker/main/docs/oi\\_tracker\\_data\\_documentation.pdf](https://raw.githubusercontent.com/OpportunityInsights/EconomicTracker/main/docs/oi_tracker_data_documentation.pdf).

<sup>23</sup> Likert scaling is a bipolar scaling method measuring either positive or negative responses to a statement. Lower values indicate disagreement with the statement, while higher values indicate agreement.

the Latin America Public Opinion Project (LAPOP).<sup>24</sup> Data for the US in 2019 are based on information for approximately 1,200 individuals which we averaged at state level (the lowest possible disaggregation level), covering all 50 US states. Results show that protests are more likely to take place in the most unequal counties where trust in the president is below the median and citizens are less satisfied with democracy.

Finally, social factors may also matter in explaining protests since the ability to draw on networks and organizations may facilitate social coordination and help social movements overcome asymmetric information challenges (Tarrow 1998, Acemoglu et al. 2018; Larson et al. 2019; Aghajanian et al. 2020; Manacorda and Tesei 2020). To capture these social characteristics at the start of the pandemic, we have split the sample across values above or below the average for two types of variables (Table 8). The first is the percentage of individuals who trust other people. These data are collected as above from LAPOP. Second, we split the sample into counties showing values above or below the median value for the following civic engagement variables: number of religious organizations; number of civic organizations; number of political organizations; and number of labour organizations. These data are compiled by the Geography of Social Capital in America project and obtained from multiple sources, including the US Census Bureau, County Business Patterns, and the American Community Survey. Results show that the interaction between policy stringency and inequality is a significant determinant of protests in the most unequal counties which also have higher levels of social trust and civic engagement. This finding is in line with previous evidence on the relevance of social trust and cooperation in protest participation and in the coordination of social movements and collective action.

## 6 Conclusion

This paper attempted to identify how inequality across US counties has shaped the impact of the COVID-19 pandemic on the incidence of protests. This empirical analysis made use of weekly data compiled between January and December 2020 on levels of COVID-19-related policy stringency, weekly data on protest incidence, and measures of county-level inequality at the start of the pandemic. The main results show that the implementation of policy restrictions to contain the virus led to increases in the incidence of protests in US counties with high levels of inequality before the start of the pandemic: a one standard deviation increase in the stringency index leads to a six percentage points increase in the probability of a protest taking place in the most unequal US counties. Stay-at-home orders, restrictions on domestic travel, and public transport restrictions have the largest impact on protest incidence in these counties. These results are robust to the use of different samples, how standard errors are clustered, to the use of different fixed effects, and to alternative definitions of the independent and dependent variables.

This result validates a long-standing theory of civil unrest, which emphasises the role of economic inequality in motivating deprived groups and individuals to protest (Gurr 1970). To date, empirical analyses of the direct impact of inequality on protests have, however, generated ambiguous results and provided evidence to only statistical associations. This paper made use of the unique context afforded by the ongoing global COVID-19 pandemic to study the relationship between inequality and protests and the mechanisms shaping this relationship. The main results effectively support an extended version of Ted Gurr's original deprivation theory, whereby reactions to government

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<sup>24</sup> LAPOP is an academic institution devoted to the collection of data on public opinion across the Americas. Despite the name of the project only mentioning Latin America, their main survey, the *AmericasBarometer*, is a rigorous comparative survey on public opinion covering most of North, Central, and South America and the Caribbean.

policies (like those related to COVID-19) affect the likelihood of protests taking place in places where grievances already exist.

Further analysis strongly suggests that the impact of government responses to the COVID-19 pandemic is largely explained by changes in economic conditions (rise in unemployment and changes in economic activities and spending) in counties with the highest levels of inequality. We find limited direct support for a possible political channel, but additional heterogeneity analysis suggests that policy stringency measures were associated with more protests in the most unequal counties which voted for the Democratic party in the 2016 elections and exhibited, at the start of the pandemic, lower levels of institutional trust but higher levels of social trust and civic engagement.

This paper suggests new avenues for future research. First, several studies in development economics and political economy have identified the long-term effects of health and economic shocks. Our results suggest that health crises may have profound immediate short-term effects, with potentially important second-order effects in how societies and politics are organized. These second-order effects remain for now under-researched. Second, the paper strongly suggests that inequality affects different protests in different ways. The existing literature on protests and social mobilization has dealt with protests as largely homogenous phenomena, and analyses of protest motivations rarely investigate the variation in the types of protests. Our results indicate that economic inequality and underlying deprivation theories may only explain the incidence of protests motivated by economic factors. Other protest types may not be affected by perceptions or experiences of deprivation and several other factors may be at play. This may also explain why, to date, empirical tests of the original deprivation theory may have yielded mixed results. Future research needs to take much more explicitly into consideration the variation in factors that motivate protests and social mobilization.

The paper has also important policy implications. For some time, there has been growing concern among scholars and political commentators about the political consequences of rises in inequality in the USA since the 1980s—and in many other parts of the world (Piketty 2014). Studies have shown evidence for links between increases in inequality and unprecedented rises in far-right and populist political parties, the election of several governments with strong nationalistic overtones (including in the USA), and the erosion of democratic institutions (Hacker 2006; Piketty 2014; Rodrik, 2018; Algan et al. 2017; Iacolla et al. 2020). Our results indicate that US counties with initially high levels of economic (and racial) inequality were in fact more likely to protest during the COVID-19 pandemic against government policies that further deteriorated their economic standing. Although common wisdom and the existing literature suggest that those at the bottom of the distribution rarely protest or engage in the political system, our results show that protest incidence was highest among those in the most unequal counties, where many protested despite the risks of falling ill or spreading the virus. Although media accounts associate these protests to politically motivated positions against the use of masks and limits to private liberties, the evidence provided in the paper suggests that these protests were motivated by the drastic economic effects of such policies on living standards. The Biden administration is implementing new social protection measures to address these profound economic effects of the pandemic. However, the political and social consequences of such a drastic economic shock are yet to be completely understood and may take decades to be fully grasped.

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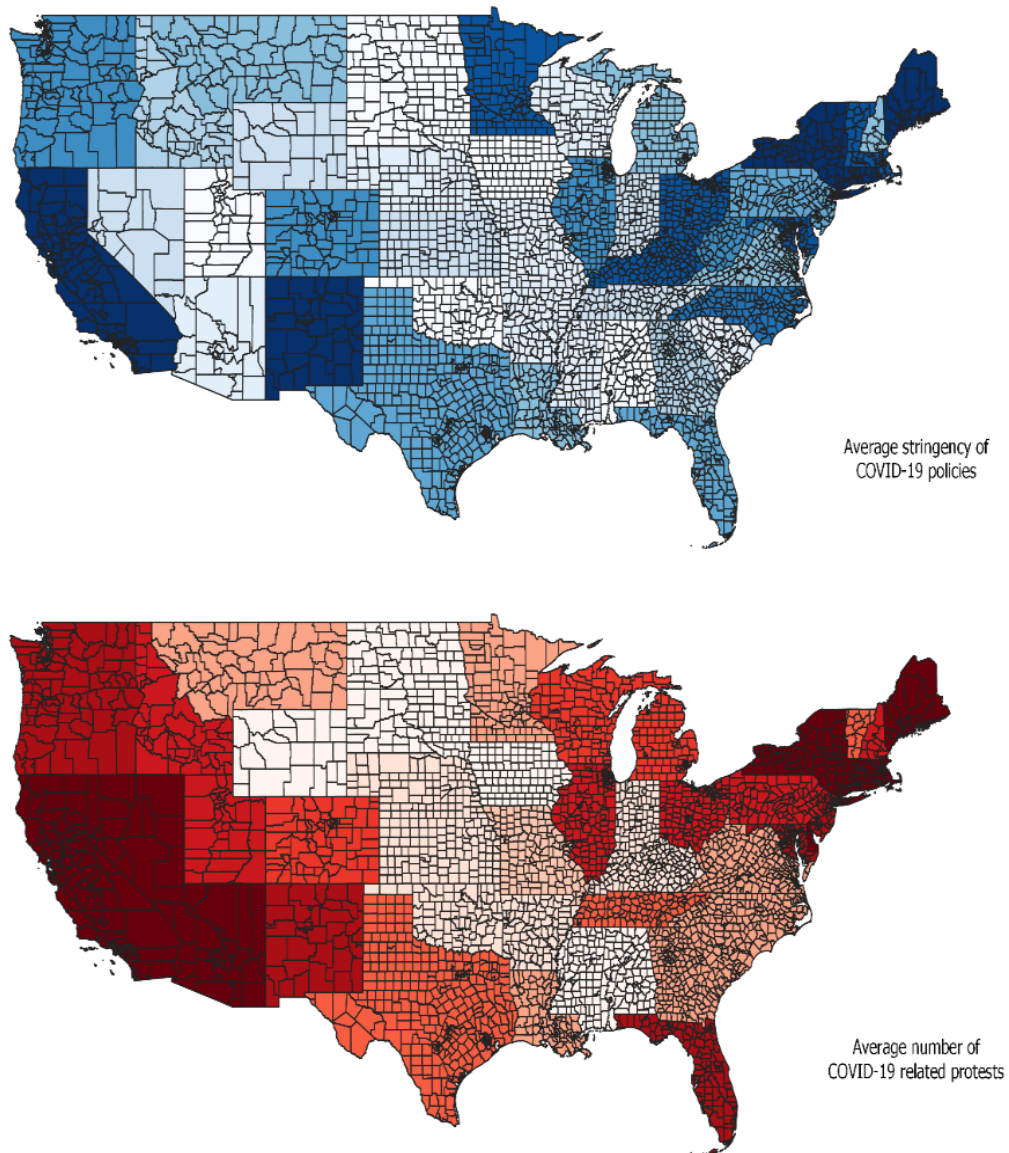
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## Figures and tables

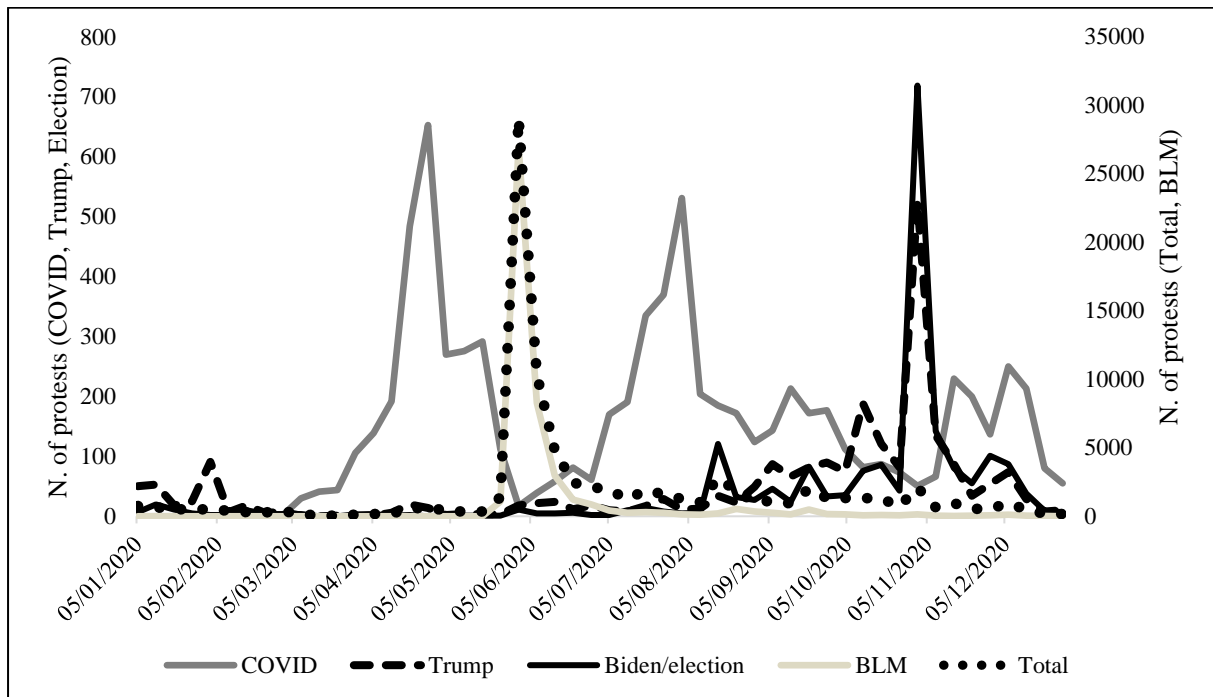
Figure 1: Stringency and protests in the USA



Note: in both maps, darker colour shades represent higher intensity of the event (i.e. darker blue corresponds to stricter stringent measures, and darker red corresponds to more protests).

Source: authors' calculations using the OxCGRT and ACLED Crisis Monitor.

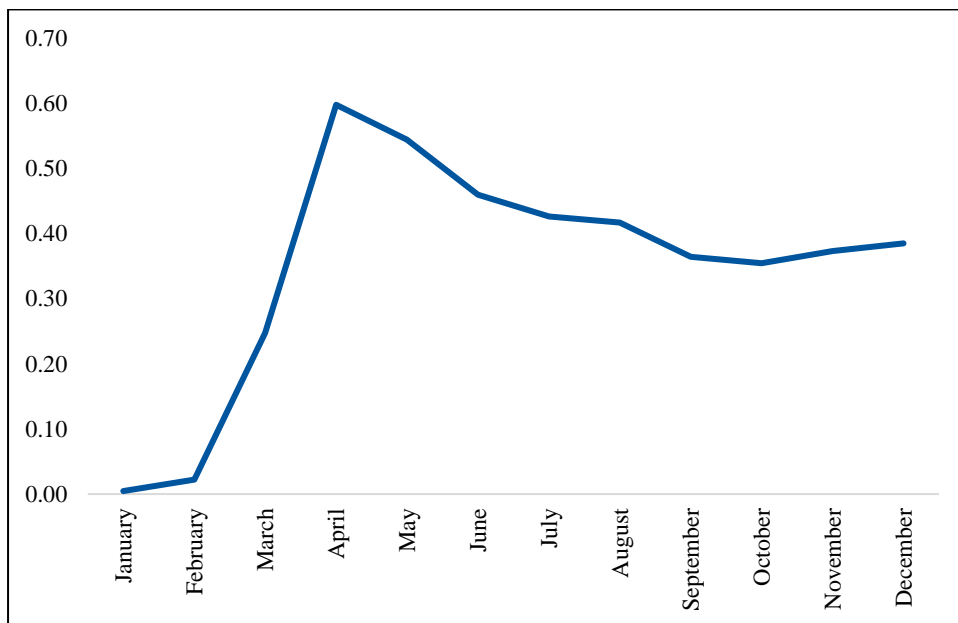
Figure 2: Protest events by type in 2020



Note: left y-axis refers to Trump-, election-, and COVID-related protests, while the right y-axis refers to total protests and protests organized by BLM.

Source: authors' calculations based on the ACLED Crisis Monitor data.

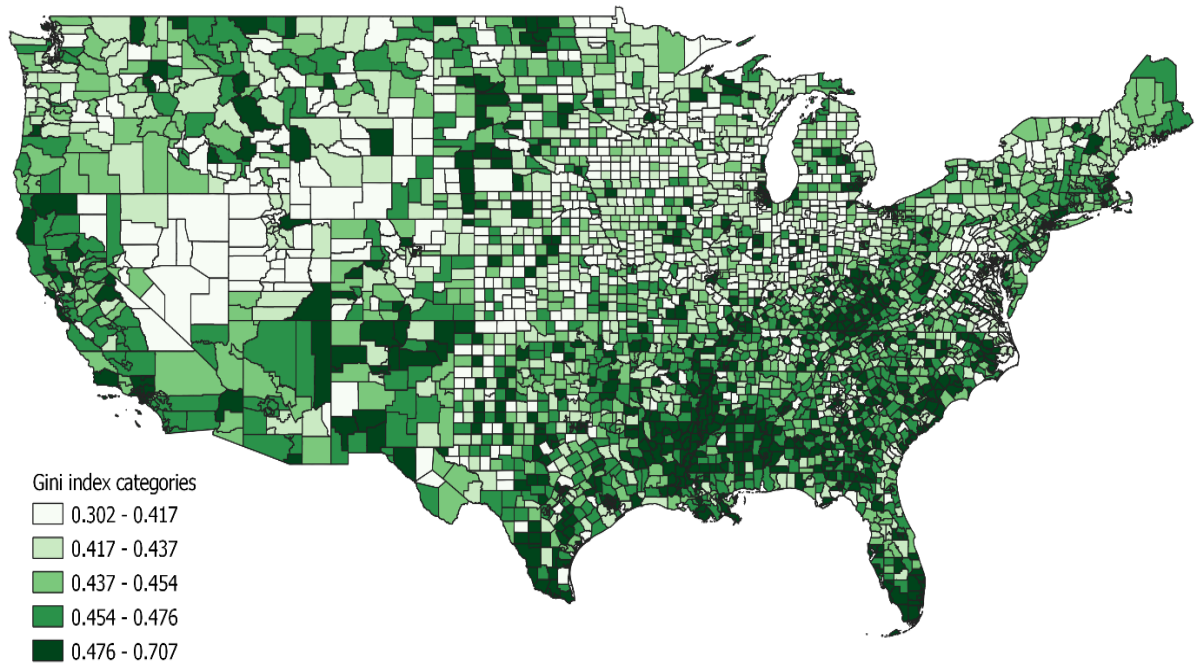
Figure 3: Average stringency index in the USA in 2020



Note: the blue line reports stringency index values as recoded by the authors (ranging between 0 and 1).

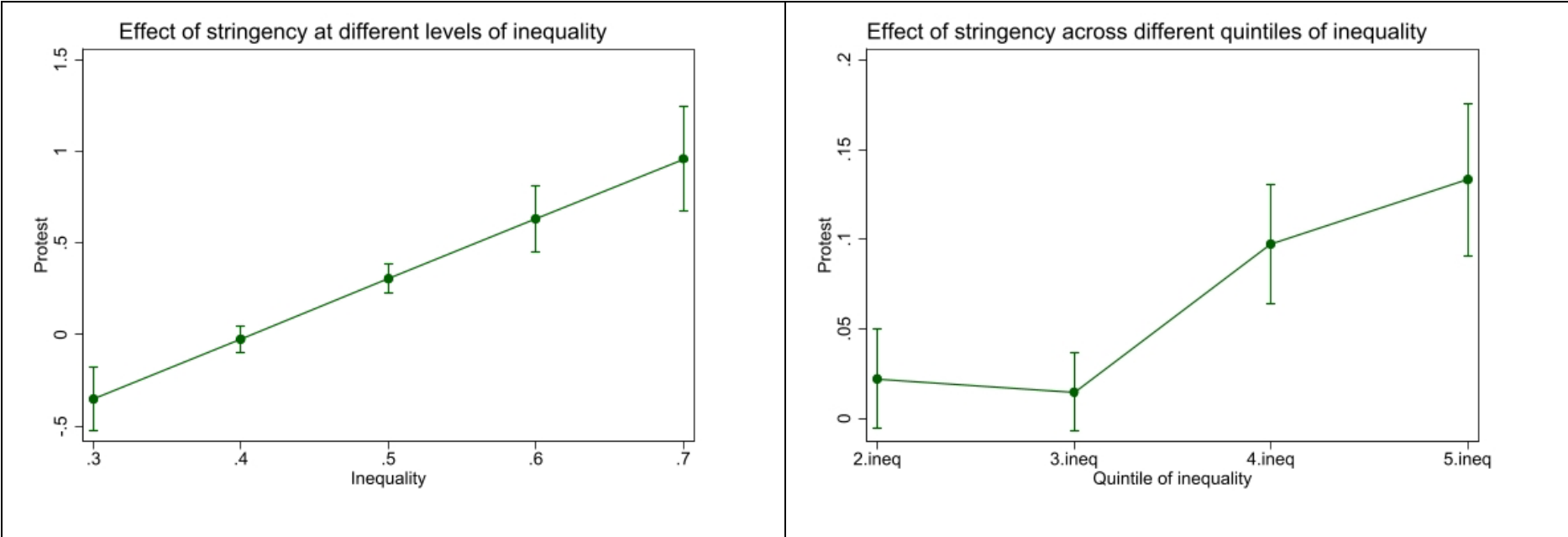
Source: authors' calculations based on the OxCGRT.

Figure 4: Gini Index across US counties, 2019



Source: authors' calculations using US Census Bureau data.

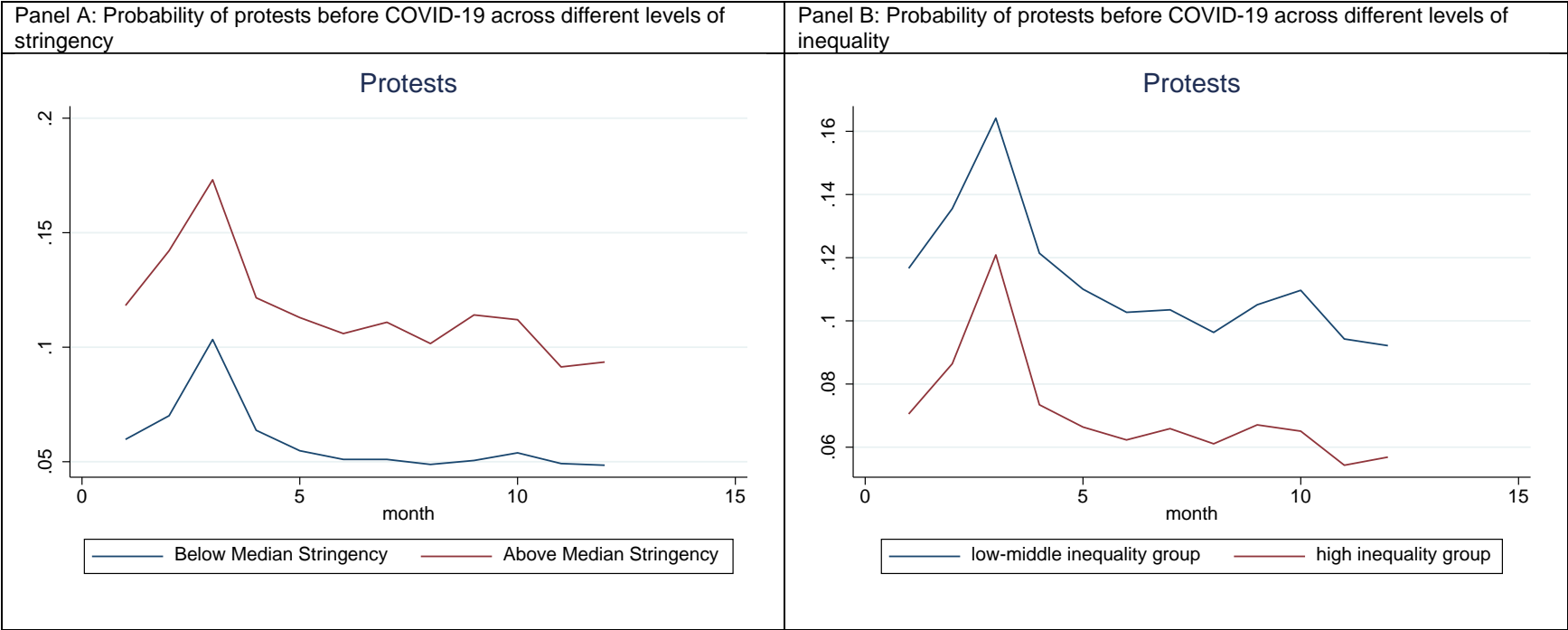
Figure 5: Effect of stringency on protests at different levels of inequality



Note: Panel A shows the effect of stringency at different levels of inequality. Panel B shows the effect of stringency on different quintiles of inequality. Both panels report estimated marginal effects. Vertical lines indicate 95 per cent confidence intervals for each marginal effect.

Source: authors' own calculations.

Figure 6: Probability of protests before COVID-19



Source: authors' calculations based on GDELT data.

Table 1: COVID-19, policy responses, and protests in US counties

	(1)	(2)	(3)	(4)	(5)	(6)
Stringency index	-0.118*** (0.041)	0.018*** (0.005)	0.078*** (0.019)	0.028 (0.025)	0.060* (0.030)	0.014 (0.032)
Stringency index * inequality (continuous)	0.081*** (0.012)					
Stringency index * q2		0.012* (0.007)	0.029 (0.033)	0.060** (0.028)	0.050 (0.030)	0.048 (0.031)
Stringency index * q3		0.030*** (0.010)	0.136** (0.057)	0.074** (0.030)	0.032 (0.024)	0.032 (0.024)
Stringency index * q4		0.081*** (0.027)	0.396*** (0.120)	0.301*** (0.050)	0.217*** (0.037)	0.212*** (0.037)
Stringency index * q5		0.085*** (0.027)	0.549*** (0.107)	0.446*** (0.061)	0.304*** (0.047)	0.289*** (0.047)
Constant	-0.079** (0.036)	-0.002** (0.001)	-0.003 (0.002)	0.037** (0.014)	-0.086*** (0.030)	-0.080** (0.036)
Observations	155,471	157,045	157,045	157,045	157,020	155,471
R-squared	0.404	0.019	0.134	0.394	0.403	0.404
Population weights	Y	N	Y	Y	Y	Y
County fixed effects	Y	N	N	Y	Y	Y
State-week fixed effects	Y	N	N	Y	Y	Y
Health controls	Y	N	N	N	Y	Y
Other controls	Y	N	N	N	N	Y

Note: regressions in columns 2 to 6 are weighted by county population. Regressions in columns 3 to 6 include county fixed effects and state-week fixed effects. Regression in column 5 includes health controls at the county level: (log) total number of COVID-19 cases and (log) total number of COVID-19 deaths. Controls in column 6 include: log total number of COVID-19 cases, log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. 'q2–5' refer to quintiles of Gini index. In all specifications, standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 2: Instrumental variable (IV) regressions

	(1)	(2)	(3)
	Baseline estimation	IV: COVID-19 cases in neighbouring states	IV: policy stringency in neighbouring states
<i>Panel A: second stage</i>			
Stringency index	0.014 (0.032)	0.050 (0.041)	0.018 (0.040)
Stringency index * q2	0.048 (0.031)	0.050 (0.035)	0.043 (0.031)
Stringency index * q3	0.032 (0.024)	-0.008 (0.031)	0.038 (0.023)
Stringency index * q4	0.212*** (0.037)	0.162*** (0.027)	0.190*** (0.036)
Stringency index * q5	0.289*** (0.047)	0.235*** (0.038)	0.279*** (0.049)
<i>Panel B: first stage</i>			
IV		0.087*** (0.004)	0.009*** (0.0003)
Observations	155,471	155,471	155,471
Controls	Y	Y	Y
County FE	Y	Y	Y
State-year FE	Y	Y	Y
Kleibergen-Paap rk LM statistic		30.120	31.100
Kleibergen-Paap Wald rk F statistic		115.591	256.544
Anderson-Rubin Wald test		15.7	11.35

Note: in column 2, the IV is computed using the average value of COVID-19 cases in neighbouring states. In column 3, the IV is computed using the average value of the stringency index in neighbouring states. Controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2-5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 3: Disaggregated COVID-19 policy responses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline estimation	School closures	Workplace closures	Public events cancellation	Gathering restrictions	Public transport restrictions	Stay-at-home	Domestic movement restrictions	International movement restrictions
Stringency index (SI)	0.014 (0.032)	-0.005 (0.006)	-0.008 (0.006)	-0.013* (0.007)	-0.006 (0.004)	-0.041** (0.016)	-0.011 (0.011)	-0.000 (0.011)	0.003 (0.016)
SI * q2	0.048 (0.031)	0.008 (0.005)	0.008 (0.008)	0.008 (0.008)	0.007* (0.004)	0.017 (0.015)	0.020* (0.011)	0.009 (0.010)	0.037* (0.019)
SI * q3	0.032 (0.024)	0.002 (0.005)	0.007 (0.005)	0.004 (0.008)	0.003 (0.003)	0.012 (0.012)	0.017* (0.009)	0.006 (0.010)	0.007 (0.014)
SI * q4	0.212*** (0.037)	0.032*** (0.005)	0.043*** (0.008)	0.041*** (0.008)	0.023*** (0.004)	0.041 (0.026)	0.072*** (0.015)	0.056*** (0.017)	0.046** (0.022)
SI * q5	0.289*** (0.047)	0.050*** (0.007)	0.059*** (0.010)	0.064*** (0.013)	0.033*** (0.006)	0.077*** (0.028)	0.087*** (0.018)	0.078*** (0.018)	0.049** (0.024)
Constant	-0.080** (0.036)	-0.057 (0.040)	-0.033 (0.036)	-0.034 (0.041)	-0.036 (0.043)	-0.018 (0.038)	-0.016 (0.030)	-0.038 (0.034)	-0.053 (0.039)
Observations	155,471	155,383	155,475	155,475	155,475	155,475	155,475	155,475	155,471
R-squared	0.404	0.403	0.403	0.402	0.402	0.400	0.404	0.404	0.401
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
County fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-week fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



Table 4: Different protest types

	(1)	(2)	(2)	(3)	(4)
	Baseline estimation	Total protests	BLM protests	Trump-related protests	Election-related protests
Stringency index	0.0137 (0.0323)	-0.117*** (0.0412)	-0.293*** (0.0393)	0.0182 (0.0210)	-0.0236 (0.0183)
Stringency index * q2	0.0482 (0.0310)	0.000328 (0.0440)	-0.00855 (0.0258)	-0.00867 (0.0189)	-0.00689 (0.0107)
Stringency index * q3	0.0321 (0.0240)	0.0463 (0.0368)	0.0116 (0.0286)	-0.000595 (0.0165)	0.00401 (0.00904)
Stringency index * q4	0.212*** (0.0367)	-0.0288 (0.0591)	0.0146 (0.0294)	-0.0662** (0.0294)	-0.0380** (0.0160)
Stringency index * q5	0.289*** (0.0468)	-0.0942** (0.0458)	-0.0878* (0.0440)	-0.0717** (0.0348)	-0.0478* (0.0247)
Constant	-0.0797** (0.0356)	0.515*** (0.0540)	0.658*** (0.0809)	-0.0446 (0.0316)	-0.0926*** (0.0318)
Observations	155,471	155,471	155,471	155,471	155,471
R-squared	0.404	0.481	0.394	0.316	0.261
Controls	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2-5' refers to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 5: Economic mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Small business open	Small business revenue	Consumer expenditure	Unemployment level	Unemp. low income	Unemp. mid income	Unemp. high income	Unemp. level (BLS)
Stringency index (SI)	0.018*	0.080***	0.005	0.016***	0.038***	0.016***	-0.009	8.117***
	(0.009)	(0.012)	(0.011)	(0.005)	(0.008)	(0.005)	(0.006)	(0.783)
SI * q2	-0.000	-0.000	-0.006	0.003	0.005	0.003	0.007	0.141
	(0.005)	(0.007)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.219)
SI * q3	-0.009*	-0.004	-0.012*	0.005*	0.010*	0.006*	0.008	-0.019
	(0.005)	(0.007)	(0.006)	(0.003)	(0.005)	(0.003)	(0.005)	(0.307)
SI * q4	-0.018***	-0.008	-0.018***	0.013***	0.019***	0.014***	0.013***	0.414
	(0.005)	(0.008)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.473)
SI * q5	-0.033***	-0.025***	-0.032***	0.019***	0.028***	0.021***	0.017***	1.199**
	(0.006)	(0.008)	(0.007)	(0.004)	(0.006)	(0.004)	(0.005)	(0.499)
Constant	-0.010	-0.080***	0.036***	-0.032***	-0.044***	-0.040***	-0.015***	-6.183***
	(0.010)	(0.015)	(0.013)	(0.005)	(0.007)	(0.005)	(0.003)	(1.114)
Observations	93,517	93,517	78,528	34,491	22,026	24,924	14,642	155,462
R-squared	0.366	0.227	0.232	0.523	0.542	0.547	0.504	0.533
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y	Y	Y	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 6: Political mechanisms

	(1)	(2)
	Share of votes for Biden	Share of votes for Trump
Stringency index	-1.069 (1.140)	0.311 (1.190)
Stringency index * q2	0.385 (0.241)	-0.134 (0.236)
Stringency index * q3	0.291 (0.271)	-0.034 (0.330)
Stringency index * q4	0.653 (0.415)	0.049 (0.408)
Stringency index * q5	0.585 (0.362)	-0.062 (0.416)
Constant	39.844*** (1.452)	49.156*** (2.224)
Observations	57,264	57,264
R-squared	0.980	0.977
Controls	Y	Y
County FE	Y	Y
State-year FE	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 7: Initial political preferences

	(1)	(2)	(3)	(4)	(5)	(6)
	Political preferences		Trust in the President		% individuals satisfied with democracy	
	Dem won in 2016	Rep won in 2016	<i>Above median</i>	<i>Below median</i>	<i>Above median</i>	<i>Below median</i>
Stringency index	-0.013 (0.081)	0.024 (0.017)	0.106* (0.059)	0.000 (0.023)	-0.027 (0.053)	-0.018 (0.036)
Stringency index * q2	0.131 (0.081)	0.025 (0.018)	0.024 (0.050)	0.038 (0.032)	0.025 (0.046)	0.054 (0.038)
Stringency index * q3	0.079 (0.065)	0.011 (0.018)	0.005 (0.038)	0.029 (0.028)	0.033 (0.047)	0.022 (0.026)
Stringency index * q4	0.218*** (0.070)	0.195*** (0.048)	0.204*** (0.045)	0.091** (0.034)	0.204*** (0.043)	0.167*** (0.049)
Stringency index * q5	0.336*** (0.082)	0.090** (0.042)	0.232*** (0.044)	0.356*** (0.092)	0.211*** (0.051)	0.349*** (0.072)
Constant	-0.107* (0.064)	-0.013 (0.031)	-0.098 (0.064)	-0.116** (0.055)	0.002 (0.064)	-0.072 (0.044)
Observations	24,299	131,144	76,021	79,450	78,321	77,150
R-squared	0.416	0.274	0.445	0.359	0.478	0.346

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 8: Initial social preferences and civic engagement indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Trust in other people		N. religious organizations		N. civic organizations		N. political organizations		N. labour organizations	
	<i>Above median</i>	<i>Below median</i>	<i>Above median</i>	<i>Below median</i>	<i>Above median</i>	<i>Below median</i>	<i>Above median</i>	<i>Below median</i>	<i>Above median</i>	<i>Below median</i>
Stringency index	-0.003 (0.037)	0.053 (0.063)	0.017 (0.038)	0.006 (0.009)	0.022 (0.042)	0.010** (0.004)	0.041 (0.094)	-0.001 (0.008)	0.008 (0.050)	0.025*** (0.009)
Stringency index * q2	0.066 (0.046)	0.022 (0.037)	0.057 (0.035)	0.013* (0.008)	0.062 (0.040)	-0.001 (0.004)	0.120 (0.087)	0.013 (0.009)	0.101** (0.045)	-0.009 (0.009)
Stringency index * q3	0.045 (0.027)	0.013 (0.045)	0.037 (0.027)	0.020 (0.015)	0.037 (0.030)	0.001 (0.004)	0.026 (0.080)	0.039*** (0.009)	0.056 (0.040)	0.003 (0.011)
Stringency index * q4	0.234*** (0.060)	0.157*** (0.039)	0.223*** (0.039)	0.008 (0.012)	0.232*** (0.039)	0.003 (0.004)	0.321*** (0.075)	0.048*** (0.008)	0.270*** (0.051)	-0.008 (0.010)
Stringency index * q5	0.330*** (0.079)	0.238*** (0.057)	0.302*** (0.050)	0.000 (0.007)	0.315*** (0.052)	0.002 (0.004)	0.376*** (0.088)	0.067*** (0.015)	0.346*** (0.065)	0.020 (0.018)
Constant	0.029 (0.056)	-0.108* (0.059)	-0.087** (0.037)	-0.001 (0.008)	-0.082** (0.039)	-0.009 (0.006)	-0.101* (0.054)	-0.006 (0.018)	-0.092** (0.041)	0.018 (0.012)
Observations	76,150	79,321	75,071	80,399	55,771	99,700	11,699	143,742	33,121	122,349
R-squared	0.384	0.444	0.402	0.224	0.397	0.079	0.409	0.139	0.398	0.115

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years and older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

## Appendix A: Variables and data sources

Table A1 below presents a summary of how our main variables were constructed and links to their data sources.

Table A1: Summary of main variables

Variable	Description	Source
Protest events	Number of protest events by county between 1 January and 31 December 2020. Protest events are categorized based on their motivation: COVID-related, related to Donald Trump or to the presidential elections, or associated to the BLM movement.	<a href="#">ACLED US Crisis Monitor</a>
COVID-19 containment policies	Stringency level of COVID-19 containment policies by each US state as measured by OxCGRT. We also analyse the different components that are used to construct the main index: school closures, workplace closures, public events cancellation, gatherings restrictions, public transportations restrictions, stay-at-home orders, and domestic and international travel restrictions	<a href="#">The Oxford COVID-19 Government Response Tracker</a>
Inequality	Income inequality is measured using the Gini coefficient (ranging 0 to 1) as calculated by United States Census Bureau using household-level income data from the 2018 and 2019 American Community Survey.	<a href="#">US Census Bureau</a>
COVID-19 cases and deaths	Number of total COVID-19 cases and deaths by county at the end of each calendar week. Data are compiled by the Opportunity Insights Economic Tracker using the New York Times and the Atlantic daily tracking data. Data were compared by the authors with official records at national level and were found to match almost perfectly. Slight differences are most probably due to the impossibility of assigning some cases/deaths to the exact location in which they were registered.	<a href="#">Opportunity Insights Economic Tracker</a>
Small business revenue	Small business transactions and revenue data aggregated from several credit card processors obtained by <a href="#">Womply</a> . Data are reported as seasonally adjusted weekly percentage change from January 2020.	<a href="#">Opportunity Insights Economic Tracker</a>
Small business openness	Number of small businesses open, as defined by having had at least one transaction in the previous three days. Data obtained by <a href="#">Womply</a> . Data are reported as seasonally adjusted weekly percentage change from January 2020.	<a href="#">Opportunity Insights Economic Tracker</a>
Consumers' credit and debit card spending	Aggregated and anonymized county-level purchase data from consumer credit and debit cards spending obtained by <a href="#">Affinity Solutions</a> . Data are reported as seasonally adjusted weekly percentage change from January 2020.	<a href="#">Opportunity Insights Economic Tracker</a>
Unemployment (private sources)	Number of active employees based on aggregated information from multiple data providers. This series is based on firm-level payroll data from <a href="#">Paychex</a> and <a href="#">Intuit</a> , worker-level data on employment and earnings from <a href="#">Earnin</a> , and firm-level timesheet data from <a href="#">Kronos</a> . Data are reported as the weekly percentage change from January 2020. Based on this percentage share, the authors calculate unemployment as 1-employment levels. Unemployment levels are presented for all employees and divided by their income bracket, calculated depending on median household income in the area where they live (low income = less than US\$46,000 per year; middle income = between US\$46,000 and US\$78,000 per year; high income = higher than US\$78,000 per year).	<a href="#">Opportunity Insights Economic Tracker</a>
Unemployment (public source)	County-level unemployment reported on a monthly basis by the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) programme. Figures are estimated from the household-level Current Population Survey (CPS).	<a href="#">US Bureau of Labor Statistics</a>

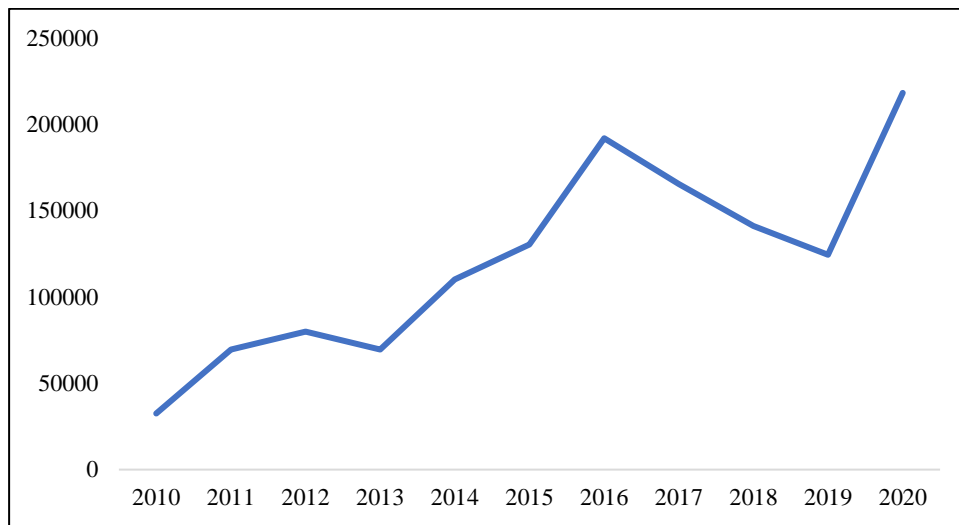
(Continued)

Table A1: Summary of main variables - continued

Unemployment (public source)	County-level unemployment reported on a monthly basis by the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) programme. Figures are estimated from the household-level Current Population Survey (CPS).	US Bureau of Labor Statistics
Income share held by households in the top 5% of income earners	Share of total income held by households in the top 5 per cent of income earners in each county compiled by the Geography of Social Capital in America project and obtained from the American Community Survey, 2012–16, 5-year estimates.	Geography of Social Capital in America project
Religious organizations	Number of religious congregations per 1,000 people by county. Data compiled by the Geography of Social Capital in America project of the US Congress Joint Economic Committee and obtained from the 2010 U.S. Religion Census: Religious Congregations and Membership Study.	Geography of Social Capital in America project
Civic organizations	Number of civic organizations per 1,000 people by county compiled by the Geography of Social Capital in America project of the US Congress Joint Economic Committee and obtained from the 2015 County Business Patterns survey (estimates for people living in each county are obtained from the 2015 American Community Survey).	Geography of Social Capital in America project
Political organizations	Number of political organizations per 1,000 people by county compiled by the Geography of Social Capital in America project of the US Congress Joint Economic Committee and obtained from the 2015 County Business Patterns survey (estimates for people living in each county are obtained from the 2015 American Community Survey).	Geography of Social Capital in America project
Labour organizations	Number of labour organizations per 1,000 people by county compiled by the Geography of Social Capital in America project of the US Congress Joint Economic Committee and obtained from the 2015 County Business Patterns survey (estimates for people living in each county are obtained from the 2015 American Community Survey).	Geography of Social Capital in America project
Black/white segregation	Black–white neighbourhood segregation at county level expressed as a continuous variable ranging between 0 (minimum segregation) and 1 (maximum segregation). Values are compiled by the Geography of Social Capital in America project of the US Congress Joint Economic Committee and obtained from the 2011–15 American Community Survey estimates.	Geography of Social Capital in America project
Presidential candidate preferences	Share of preferences for the Democratic or Republican presidential candidate before the 2021 General Election. Data are extracted from state-level polls conducted by different bodies and compiled by the FiveThirtyEight project from <i>ABC news</i> broadcasting company. We include results from all polls available between 1 January 2020 and the week of the election. Shares are averaged by week in case more than one poll was conducted each week. Data availability depends on polls being conducted in a specific state/week and being compiled by FiveThirtyEight. Information is present for each of the 50 states included in the analysis for at least eight weeks.	FiveThirtyEight

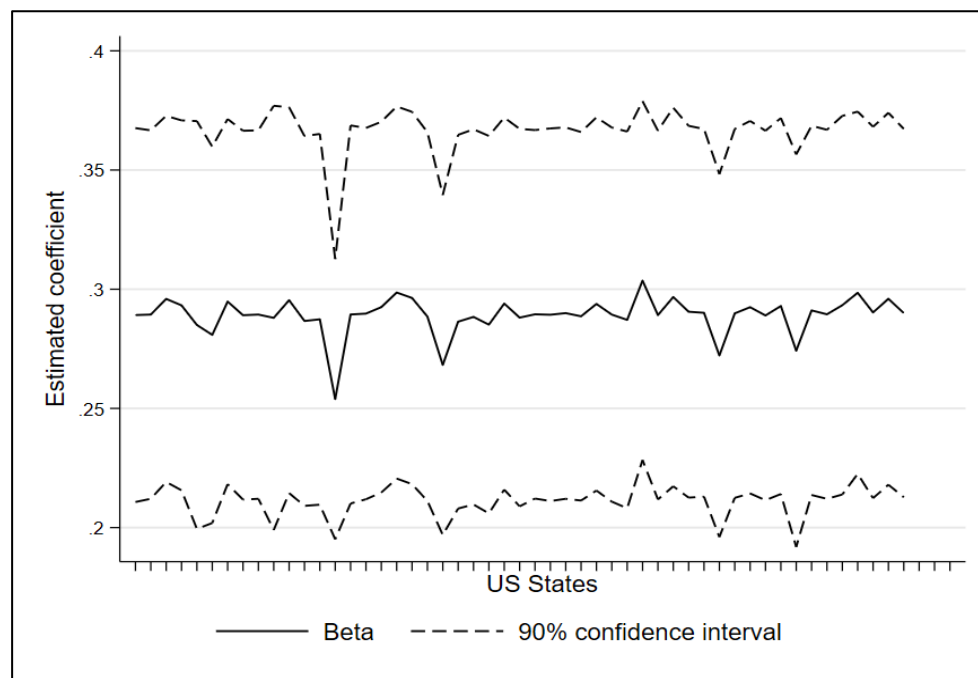
## Appendix B: Additional robustness tests

Figure B1: Protest events through time in the USA



Source: authors' own illustration based on GDELT project data.

Figure B2: Different samples—Jackknife estimation



Note: the figure reports the coefficients of the variable 'Stringency index\*q5', estimated for the baseline specification (Table 1, column 6), dropping one state at the time.



Table B1: COVID-19, policy responses, and protests in US counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline estimation	Baseline estimation excluding time invariant controls	Baseline estimation using wild bootstrap	Baseline estimation clustering at county level	Baseline estimation clustering at state and week level	Baseline estimation—replacing county with state fixed effects	Baseline estimation—adding state fixed effects
Stringency index	0.014 (0.032)	0.054* (0.031)	0.014 (0.032)	0.014 (0.030)	0.014 (0.037)	0.039 (0.028)	0.028 (0.025)
Stringency index * q2	0.048 (0.031)	0.051 (0.031)	0.048 (0.031)	0.048 (0.031)	0.048 (0.030)	0.025 (0.030)	0.060** (0.028)
Stringency index * q3	0.032 (0.024)	0.031 (0.025)	0.032 (0.024)	0.032 (0.028)	0.032 (0.025)	0.061 (0.048)	0.074** (0.030)
Stringency index * q4	0.212*** (0.037)	0.218*** (0.038)	0.212*** (0.037)	0.212*** (0.039)	0.212*** (0.043)	0.269*** (0.048)	0.301*** (0.050)
Stringency index * q5	0.289*** (0.047)	0.298*** (0.046)	0.289*** (0.047)	0.289*** (0.051)	0.289*** (0.049)	0.459*** (0.066)	0.446*** (0.061)
Constant	-0.080** (0.036)	-0.139*** (0.034)	0.081* (0.046)	-0.080** (0.035)	-0.080 (0.048)	0.046*** (0.015)	0.037** (0.014)
Observations	155,471	155,570	155,471	155,471	155,471	157,045	157,045
R-squared	0.404	0.404	0.404	0.404	0.404	0.279	0.394
Population weights	Y	Y	Y	Y	Y	Y	Y
County fixed effects	Y	Y	Y	Y	Y	Y	Y
State-week fixed effects	N	N	N	N	N	Y	Y

Note: regression in column 2 excludes time-invariant controls. Regression from column 3 computes errors using wild bootstrap method. Regression in column 4 clusters standard errors at county level. Regression in column 5 clusters standard errors at state and week level. Regression in column 6 replaces county-level with state-level fixed effects, while regression in column 7 includes both county fixed effects and state-level fixed effects. 'q2–5' refer to quintiles of Gini index. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table B2: Alternative definitions of inequality

	(1)	(2)	(3)	(4)
	Baseline estimation	Gini index (2018)	Income share of top 5%	Black–white segregation
Stringency index	0.014 (0.032)	0.016 (0.032)	0.024 (0.036)	0.092** (0.043)
Stringency index * q2	0.048 (0.031)	0.039 (0.028)	0.036 (0.032)	-0.032 (0.040)
Stringency index * q3	0.032 (0.024)	0.032 (0.028)	0.018 (0.027)	-0.004 (0.045)
Stringency index * q4	0.212*** (0.037)	0.211*** (0.038)	0.204*** (0.032)	0.095* (0.055)
Stringency index * q5	0.289*** (0.047)	0.285*** (0.048)	0.268*** (0.047)	0.174*** (0.050)
Constant	-0.080** (0.036)	-0.078** (0.036)	-0.079** (0.038)	-0.093** (0.040)
Observations	155,471	155,471	155,171	155,471
R-squared	0.404	0.404	0.404	0.403
Controls	Y	Y	Y	Y
County FE	Y	Y	Y	Y
State-year FE	Y	Y	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table B3: Alternative protest definitions

	(1)	(2)
	Baseline estimation	COVID protests (log)
Stringency index	0.0137 (0.0323)	0.108 (0.0644)
Stringency index * q2	0.0482 (0.0310)	0.0371 (0.0417)
Stringency index * q3	0.0321 (0.0240)	-0.0302 (0.0806)
Stringency index * q4	0.212*** (0.0367)	0.183** (0.0794)
Stringency index * q5	0.289*** (0.0468)	0.520*** (0.124)
Constant	-0.0797** (0.0356)	-0.327** (0.125)
Observations	155,471	155,471
R-squared	0.404	0.510
Controls	Y	Y
County FE	Y	Y
State-year FE	Y	Y

Note: controls include the log total number of COVID-19 cases, the log total number of COVID-19 deaths, log average rainfall, plus the interaction between a linear time trend and the following cross-sectional county characteristics: percentage of non-white population, percentage of people employed in services, percentage of people 25 years older with university degree, and log of per capita income. Regressions are weighted by county population. All specifications also include county fixed effects and state-year fixed effects. 'q2–5' refer to quintiles of Gini index. Standard errors are clustered at the state level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .