Shutdown policies and conflict worldwide

Nicolas Berman, Mathieu Couttenier, Nathalie Monnet, Rohit Ticku

Shutdown policies and conflict worldwide

Nicolas Berman [†] Mathieu Couttenier [‡] Nathalie Monnet [§] Rohit Ticku [¶]

September 2021

Abstract

We provide evidence on the link between the policy response to the SARS CoV-2 pandemic and conflicts worldwide. We combine daily information on conflict events and government policy responses to limit the spread of SARS CoV-2 to study how demonstrations and violent events vary following shutdown policies. We use the staggered implementation of restriction policies across countries to identify the dynamic effects in an event study framework. Our results show that imposing a nation-wide shutdown is associated with a reduction in the number of demonstrations, which suggests that public demonstrations are hampered by the rising cost of participation. However, the reduction is short-lived, as the number of demonstrations are back to their pre-restriction levels in two months. In contrast, we observe that the purported increase in mobilization or coordination costs, following the imposition of restrictions, is not followed by a drop of violent events that involve organized armed groups. Instead, we find that the number of events, on average, increases slightly following the implementation of the restriction policies. The rise in violent events is most prominent in poorer countries, with higher levels of polarization, and in authoritarian countries. We discuss the potential channels underlying this heterogeneity.

Keywords: SARS CoV-2, Conflict, Violence, Mobility
JEL classification: D74, E65, J18

[*]This work was supported by French National Research Agency Grants ANR-17-EURE-0020, and by the Excellence Initiative of Aix-Marseille University - A*MIDEX. . Mathieu Couttenier acknowledges financial support from the IDEXLYON, University of Lyon (French National Research Agency, "Programme Investissements d'Avenir" ANR-16-IDEX-0005) and financial support from the French National Research Agency ANR-20-COV4-0005-01. Nathalie Monnet acknowledges financial support from the Swiss National Research Foundation (grant *Economics of Conflict and Violence* P0GEP1-175125 and grant *Land Concentration and Rural Poverty*, 100018/178902). Rohit Ticku acknowledges financial support from the Institute for the Study of Religion, Economics and Society, Chapman University. An Online Appendix to this paper is available here: Online Appendix.

[†]Aix-Marseille Univ., CNRS, EHESS, Centrale Marseille, AMSE, France, and CEPR. Email: nicolas.berman@univ-amu.fr.
[‡]University of Lyon, ENS Lyon, GATE Lyon/St-Etienne and CEPR. Email: mathieu.couttenier@ens-lyon.fr.
[§]University of Geneva. Email: nathalie.monnet@unige.ch.
[¶]Robert Schuman Center for Advanced Studies, European University Institute, Italy. Email: rohit.ticku@eui.eu.

1 Introduction

On March 31, 2020 the U.N. Secretary General, Antonio Guterres, cautioned that the SARS CoV-2 epidemic could lead to "enhanced instability, enhanced unrest, and enhanced conflict." The SARS CoV-2 epidemic can exacerbate conflict by upending social and political protections. The effect could be severe for the vulnerable populations: those caught up in war and persecution, or those living in densely populated areas with dismal state capacity.[1] Critically, countries have responded with varying degree of restrictions to limit the spread of SARS CoV-2. The policy response to the pandemic can itself have a bearing on conflict situations. Anecdotal evidence suggests that mobility restrictions to flatten the epidemic curve, that increase the mobilization and coordination costs, have a direct and negative effect on conflict. For instance, anti-corruption protests in Iraq and Lebanon came to a screeching halt following the announcement of the pandemic,[2] while India's lockdown terminated nationwide protests against the mistreatment of Muslims.[3] The flipside is rising scapegoating of minorities or vulnerable groups, especially when states are preoccupied with combating the pandemic.[4] Moreover, authoritarian regimes could also use the global preoccupation with SARS CoV-2 as an opportunity to crush armed opposition. For example, the Myanmar military stepped up its offensive against ethnic armed rebel groups in Rakhine, Chin, Karen and northern Shan state,[5] while the United Nations appealed for a global ceasefire to countervail the escalation in violence.[6] Some regimes may also use the global epidemic as an opportunity to stifle democratic opposition. In Azerbaijan, members of the opposition parties were locked up for allegedly violating a lockdown.[7]

The policies imposed to contain the pandemic have also hampered economic activity. The World Bank estimates a 4.6% decline in world GDP per capita in 2020, which earmarks the worst recession since the Second World War.[8] The decline in per capita incomes can push millions of people worldwide into extreme poverty. The effect of restrictions on conflict through the income mechanism is a priori ambiguous. On the one hand, the current economic downturn may lead to more conflict, by reducing the individual opportunity cost of violence, protests and rebellions (Becker, 1968; Grossman, 1991; Dal Bó and Dal Bó, 2011), and by hampering the capacity of the state to fight opponents or buy off opposition (Berman et al., 2011, Fearon

[1] Shared responsibility, global solidarity: Responding to the socio-economic impacts of COVID-19 (UNSDG, March 2020, url).
[2] A great and sudden change: the global political violence landscape before and after the COVID-19 pandemic (ACLED, August 4, 2020, url).
[3] Would-be autocrats are using COVID-19 as an excuse to grab more power (The Economist, April 23, 2020, url).
[4] For example, there have been a few incidents of physical violence against Muslims, in addition to hateful messages on social media, since it was discovered that a Muslim religious gathering was the source of many SARS CoV-2 cases (The Economist, April 23, 2020, url). Among Rohingya refugee camps in Bangladesh SARS CoV-2 is being attributed as divine punishment for women's "dishonorable acts" and not observing *Purdah* (veiling) (International Organization of Migration, April 19, 2020, url).
[5] Myanmar military steps up attacks as SARS CoV-2 spreads (AlJazeera, April 16, 2020, url).
[6] While some warring groups have responded to a call for ceasefire, in many of the most fragile situations there has not been any let up in fighting, and in some cases the fighting has intensified (UN.org, April 3, 2020, url).
[7] Would-be autocrats are using COVID-19 as an excuse to grab more power (The Economist, April 23, 2020, url).
[8] COVID-19 to Plunge Global Economy into Worst Recession since World War II, World Bank (June 8, 2020, url).

and Laitin, 2003). On the other hand, states with more limited resources are less attractive "prizes" to be seized, which may lead to a decrease in conflict intensity (Bates et al., 2002; Besley and Persson, 2010).[9]

In this paper, we provide evidence on how enforcing restrictions to limit the spread of SARS CoV-2 affects conflicts globally and how conflict dynamics vary across types of events, and depend intrinsically on country-specific socio-economic or institutional contexts. We take advantage of the joint release of daily information on conflict events by the *Armed Conflict Location and Event* dataset, on the one hand, and on government policy responses by the Oxford COVID-19 Government Response Tracker, on the other hand, to estimate how shutdown policies are correlated to the dynamic of conflict outcomes. We use a difference-in-differences strategy that exploits the cross-country variation in the timing of restriction policies. Studying how restriction policies shape conflict behavior is subject to its own methodological challenges. First, estimates are likely to be biased because the SARS CoV-2 pandemic dynamics, and how people responded to it, can be systematically different between countries that implemented restriction measures and the non-implementing countries (Goodman-Bacon and Marcus, 2020). Second, countries have implemented a host of measures to contain the pandemic spread, which makes comparison in terms of the policy treatment difficult (Goodman-Bacon and Marcus, 2020).

We assuage the first concern by implementing a flexible event study design, where we compare the pre-shutdown conflict trends between the treatment and the control groups. Specifically, we regress conflict outcomes on a set of binary indicators that range from 120 days before and 120 days after the set of restriction policies. In our baseline estimates, we control for the timing of the SARS CoV-2 outbreak to eliminate the possibility that conflict actors may scale down activity in response to the pandemic (Farboodi et al., 2020). We further include country-specific time trends to account for trends in initial country characteristics, such as income levels or population density, that can jointly determine the pandemic and the conflict dynamics. We also estimate the conflict dynamics around specific restriction policies to address whether countries are comparable in terms of the restrictions they implemented. In the sensitivity analysis we compare the evolution of conflict events around eight types of restrictions – which include policies like shelter-in-place, closure of schools, restriction on internal move, etc. – separately.

We uncover a compelling pattern of conflict dynamics around restriction policies depending on the nature of the conflict. We find three striking results for demonstrations, which include spontaneous acts of rioting or protesting. First, we observe a sharp decline in demonstrations following the imposition of restriction policies. The magnitude of the effect is sizable: our estimates imply a decline by 0.8 demonstration events per day during the first month, which is substantial compared to the sample average of 1.5 events. Our

[9] One of the consequences of the current slowdown in economic activity is a collapse of the market for commodities, from agricultural to oil and mineral prices. Such drop of commodity prices may have multiple effects on conflict. Studying such effects is beyond the scope of this paper, in which we focus on the specific impact of COVID-19 related shutdown and lockdown policies.

results are robust to a battery of sensitivity checks, and especially, to the inclusion a full set of interactions between restriction indicators and a measure of country-specific media freedom, which alleviates concern that the decline in demonstrations can be driven by short-term constraints on media reporting. Overall, the evidence is in line with the literature on political demonstrations which suggests that public rallies are hampered by factors that raise the opportunity cost of participation (Madestam et al., 2013; Kurrild-Klitgaard, 2013). Second, the reduction is short-lived, as the number of demonstrations are back to their pre-restriction levels in the second month, and remain there in the subsequent periods. Third, this U-shaped dynamic is similar across regions, when we consider specific restriction measures, and across countries with different socio-economic or institutional contexts. We further explore whether a return to pre-pandemic behavior, due to countries relaxing the mobility or social distancing constraints, can explain the resurgence in demonstrations. Restricting the sample to countries that imposed the restriction policies for at least three months, we observe a similar resurgence in demonstration activity, which suggests that the easing of mobility constraints is not the first-order determinant of the resurgence.

Our results suggest that the short-lived decline in demonstrations can be due to two non-exclusive explanations: the negative economic consequences of the restriction policies could have fueled public resentment or individuals may have overtime discounted the potential health risk from the pandemic. While there is substantial anecdotal evidence for economic deprivation having contributed to rising demonstrations across countries,[10] we use different empirical exercises to assess if the information-based mechanism can be an important determinant. We find that the timing of a country having implemented restriction policies compared to rest of the region, the scale of SARS CoV-2 related fatalities in the neighborhood, or access to information technology has no bearing on demonstration activity across countries. We therefore consider the rise in public resentment to be a more likely explanation for the resurgence in demonstrations. Alternatively, we estimate whether the observed drop in demonstrations can be due to a temporary substitution between online and offline protests, using monthly data on the number of petitions and signatures from *change.org*. We fail to detect any substitution towards online protests in the immediate aftermath of the restrictions. This non-result should however be interpreted with caution because of data limitation in measuring online protests (shorter time-period and absence of daily dimension).

In contrast, we observe that the purported increase in mobilization or coordination costs, following the imposition of restrictions, is not associated with a decline in violent events involving organized armed groups. If anything, the number of events, on average, increases slightly in the aftermath of the policies' implementation. This lack of correlation between restriction policies and violent events however masks a significant heterogeneity. First, we observe negligible link between restriction policies and violent events

[10] A resurgence in public gatherings following SARS CoV-2 restrictions have been reported in various countries such as Israel, Spain, United Kingdom and Hong Kong. See: The pandemic is an era of protests — and protest restrictions, Washington Post (October 2, 2020, url).

in Latin America, while we uncover a positive association in Asia and Africa. We also consider specific restriction policies and find that the surge in violent events is mostly associated with the imposition of shelter-in-place and restrictions to internal movements policies. Second, we assess whether countries where the roots of conflict are present – such as countries with high poverty, ethno-religious strife or non-democratic institutions – are particularly susceptible to rise in violent events following the policy responses to the SARS CoV-2 pandemic. We find a rise in violent events in poorer countries, which could be due either to the lower opportunity cost of fighting (Becker, 1968; Grossman, 1991; Dal Bó and Dal Bó, 2011) or the lower state capacity to fight armed opposition (Berman et al., 2011, Fearon and Laitin, 2003). The escalation in violent events is also more conspicuous in countries with higher ethno-religious polarization or fractionalization, which is in line with evidence that pandemics provide a pretext for the scapegoating of minorities (Voigtländer and Voth, 2012; Jedwab et al., 2019). Finally, we find that authoritarian countries are more amenable to violent conflicts in the aftermath of restrictions. This result is consistent with evidence that authoritarian leaders are incentivized to repress their challengers, especially when the "world is not watching" (Durante and Zhuravskaya, 2018).

Our paper contributes to the literature in several ways. We contribute to research on the consequences of the SARS CoV-2 pandemic. Ongoing research has assessed the macroeconomic implications of the SARS CoV-2 (Atkeson, 2020; McKibbin and Fernando, 2020; Guerrieri et al., 2020), the role of climate, or mass media in spreading SARS CoV-2 (Carleton and Meng, 2020; Bursztyn et al., 2020). Notably, Chinazzi et al. (2020) study the effect of travel restrictions on the spread of the virus. Closest to our study, our exercise complements the analysis in Bloem and Salemi (2021), who investigate the links between SARS CoV-2 and conflict using a number of country-specific case studies. Our approach pools many countries together, and by doing so ignores a number of potentially important sources of heterogeneity; on the other hand, we hope to uncover regularities that can help to understand and anticipate some of the main threats associated with the current restrictions. Next, we contribute to the literature on infectious diseases and civil conflicts, which shows that health shocks from infectious diseases can potentially cause civil conflicts (Cervellati et al., 2017). We highlight that policy response to an epidemic can significantly impact civil conflicts in contexts where the roots of conflicts are already present. We finally contribute to the literature that distinguishes between conflict events based on the actors involved or on the choice of tactics (Leventoğlu and Metternich, 2018; Chenoweth et al., 2011; Chenoweth and Cunningham, 2013; Stephan and Chenoweth, 2008). We complement this literature by highlighting a significant heterogeneity in the drivers of conflict that involve general public versus those which involve organized armed groups.

The next section discusses the theoretical channels and anecdotal evidence to motivate the relationship between SARS CoV-2 restrictions and conflict outcomes. Section 3 presents the data and our baseline research design. Section 4 discusses the results and their robustness, and section 5 concludes.

2 Restriction policies and conflict: channels and anecdotal evidence

2.1 Channels

SARS CoV-2 restrictions policies and types of conflict. The restriction measures may directly affect the "deep determinants" of conflict, i.e., the incentives for particular groups and individuals to engage in disputes that may turn into violent conflict. The most direct impact of such policies is a peace-inducing one: through social distancing and limited mobility, restrictions increase mobilization and coordination costs, which in turn reduces the scope for organized violence. The effectiveness of mobility restrictions in reducing the scope for organized violence is however likely to depend of the nature of conflict that can differ both in the types of actors involved (civilians versus armed groups), in the choice of tactics (peaceful demonstrations versus violent attacks) or in the motivations. A large literature highlights that armed combat or non-violent campaigns can shape conflict outcomes differently (Stephan and Chenoweth, 2008; Chenoweth et al., 2011; Chenoweth and Cunningham, 2013). Leventoğlu and Metternich (2018) present a conceptual framework to explain the difference in types of actors that engage in violent and non-violent dissent. Lower class citizens are more likely to join rebel organizations and use violent tactics against the state, since the opportunity cost of participating in violent combat is lower. Middle class citizens, whose opportunity cost of participating in anti-government activities is higher, are more likely to oppose the state through peaceful protests or riots. The club goods framework in economics provides an alternative approach to differentiate between types of conflict actors, their participation costs, and their tactical choices. Groups that demand a higher degree of self-sacrifice from members,[11] and distinguish themselves from rest of the population,[12] are more adept at screening out "free-riders" (Iannaccone, 1992; Iannaccone and Berman, 2006). Such groups are also more proficient at producing lethal organized violence (Berman, 2003; Iannaccone and Berman, 2006). The self-selection of members with a lower opportunity cost of participation or those with a higher level of commitment implies that armed conflict is less likely to be affected by policy restrictions than peaceful demonstrations. Further, the timing of restriction policies may impact demonstrations and armed conflict differently across different settings, which we discuss next.

Roots of demonstrations. Demonstrations gather more civilians (who are generally unknown to each other), are more spontaneous and short-lived events than armed conflicts. The opportunity cost of participation, the mobilization or coordination costs and the public resentment are the major drivers of demonstrations (Madestam et al., 2013; Kurrild-Klitgaard, 2013). The policy response to the SARS Cov-2 pandemic may impact demonstration incidence through these different drivers. On the one hand, the current economic downturn may lead to more demonstrations by reducing the individual opportunity cost of protest (Becker,

[11] This can be in the form of promoting austere living or martyrdom in combat as a higher principle.
[12] For example, the group may espouse a more extremist ideology than the rest of the society.

1968; Grossman, 1991; Dal Bó and Dal Bó, 2011), and by fueling the public resentment. On the other hand, the imposition of restrictions has dramatically increased the mobilization and coordination costs for the common public, especially by reducing the spontaneous acts of protests.[13] Interestingly, the way in which restriction policies affect demonstrations can vary over time. The imposition of restriction policies immediately increases the mobilization and coordination costs. On the other hand, the negative economic impact of restriction policies, that fuels public resentment, unfolds after sometime. Hence, we expect a U-shaped response of public protests to the imposition of restriction policies: a decrease in the very short-term and a resurgence in the medium-term. An alternative channel that would also generate a U-shape response is based on information availability: in the early stage of the pandemic, individuals may have interpreted restriction policies as a signal of potential health risks, before eventually realizing that such risks were (from an individual perspective) lower than expected. In the empirical section, we perform a number of tests to determine whether the information channel partly explains the results.[14]

Roots of armed conflict. The policy response to the SARS CoV-2 pandemic may impact armed conflict through multiple channels (Rohner, 2020). First, by slowing down economic activity, they lower income and aggravate poverty (e.g. Martin et al., 2020). The effect of restrictions through the income mechanism is a priori ambiguous. On the one hand, as in the case of demonstration, the current economic downturn reduces the individual opportunity cost of violence (Becker, 1968; Grossman, 1991; Dal Bó and Dal Bó, 2011), and by hampering the capacity of the state to fight opponents or to buy off opposition (Berman et al., 2011, Fearon and Laitin, 2003). On the other hand, states with more limited resources are less attractive "prizes" to be seized, which may lead to a decrease in armed conflict intensity (Bates et al., 2002; Besley and Persson, 2010). A second channel is the potential exacerbation of ethnic or religious tensions generated by the policies' implementation. In particular, SARS CoV-2-related restrictions may lead to the scapegoating of minorities or vulnerable groups, especially when state resources are diverted to combat the pandemic. Third, some regimes and political leaders have also used the global epidemic as an opportunity to stifle democratic opposition, restrict freedom of assembly, or increase surveillance of societies (Coyne and Yatsyshina, 2020). This may lead to more acute repression and violence in the short-run, and to a degradation of democratic institutions. Finally, SARS CoV-2 restrictions may trigger shifts in the balance of power, which has an ambiguous effect on armed conflict. In workhorse contest models, a more balanced power implies more armed conflict (Rohner and Thoenig, 2020). Starting from low levels of conflict due to a large potential cost of rebellion (Esteban and Ray, 2008), a small shift in the balance of power due to the restriction policies (such as due a negative shock to the state capacity) can result in a significant escalation of conflict. This

[13]The conjecture is consistent with evidence that political demonstrations are affected by changes in the private cost of participation (Kurrild-Klitgaard, 2013; Madestam et al., 2013).

[14]Another possibility is that "traditional" offline protests decline due to higher mobilization costs, but is replaced by online participation, a form of protesting that has been on the rise recently. We also discuss this potential substitution in the empirical part of the paper.

channel is of particular relevance for highly polarized countries.[15]

Bargaining failures. Another way of thinking about the consequences of pandemic-related restrictions on demonstration and armed conflict is through the lens of bargaining. Low income or ethnic (or religious) grievances affect individuals' and armed groups' incentives to engage in conflict, by changing the expected costs, gains, or fighting technologies. Yet, conflict eventually occurs only if peaceful bargaining is not achieved. In situations where the conditions for disputes and conflict already pre-exist (e.g., poor, socially polarized countries with autocratic institutions), conflict may become more likely during the SARS CoV-2 pandemic because the associated policies have reduced the scope for a peaceful resolution of disputes. Conflict theories generally mention three main sources of bargaining failures (Jackson et al., 2009): asymmetric information, agency problems, and commitment issues. All three could in principle be affected in the SARS CoV-2 context. First, access to war zones may be restricted, and opportunities of conflict mediation may become more limited. Second, the uncertain impact of restrictions makes it more difficult for government to credibly commit to specific transfers aiming at avoiding conflict. Next, as mentioned previously, the costs of repression may decrease at the same time as policies are implemented, as international attention is diverted from these actions, and as leaders can legitimize them on account of implementing strict policies. This may affect the individual cost/benefit perceived by leaders and make conflict resolution more difficult.

Summary and predictions. To summarize, we expect the policy response to the pandemic to have different effects on demonstrations and armed conflict. The rise in participation and coordination costs may be higher in the case of demonstrations, which we expect to decline more in the immediate aftermath of the policy implementation. However, this decline is likely to be attenuated by the rise in public dissent due to subsequent economic deprivation. Hence, we anticipate a U-shaped response of demonstrations to the imposition of restriction policies. The impact of restriction policies on armed conflict is more nuanced and ambiguous, but we expect this type of violence to increase more in contexts where the roots of conflicts are already present: countries with non-democratic institutions, high levels of poverty, or group polarization.

2.2 Anecdotal evidence

SARS CoV-2 restriction ends a demonstration and creates another: For three months prior to the SARS CoV-2 outbreak, hundreds of student activists and working-class women had been protesting the passing of the Citizen Amendment Act (CAA), which was viewed as discriminatory against the Muslim minorities in India.[16] On March 22, the Indian Prime Minister, Narendra Modi, imposed a nationwide lockdown in response to the SARS CoV-2 outbreak. The months long anti-CAA agitation came to an abrupt end

[15]Given the time-span of our empirical exercise, we concentrate here on channels which may plausibly have short-term consequences for conflict risk. As Rohner (2020) points out, the SARS CoV-2 crisis and associated policies also have a number of long term implications, through their effect on education or international cooperation.

[16]How the coronavirus lockdown impacted anticitizenship law protests in India, Peninsula Press (June 18, 2020, url).

following the lockdown announcement. The imposition of a nationwide lockdown however created another crisis as millions of migrant workers were rendered jobless and away from home. Riots broke out in many parts of the country, as stranded migrant workers demanded provision of public transport services to return home.[17]

Rebel group uses SARS CoV-2 restriction to attack state forces: On March 23, the Islamist insurgent group Boko Haram carried out a grievous attack on an army base in Chad, in which 92 soldiers were killed. On March 24, Boko Haram insurgents ambushed a military convoy and killed 47 soldiers in Nigeria.[18] Both the events occurred just days after SARS CoV-2-related restrictions were imposed in the respective countries. The escalation in violence suggested that Boko Haram was using the state's pre-occupation with the SARS CoV-2 as an opportunity to step up attacks and to recruit new members (Berman et al., 2020).

SARS CoV-2 restriction flames religious tension: In early March, an international conference brought thousands of Muslim missionaries to Indian capital Delhi. On March 22, local authorities shut down the conference premises in response to cases of SARS CoV-2 infection among the participants. As missionaries returned home they became a conduit for spreading SARS CoV-2 across different parts of the country.[19] The event and the subsequent media reporting created a widespread resentment against ordinary Muslims. There were incidents of physical violence against Muslims, in addition to hateful messages on the social media.[20]

Political leader uses SARS CoV-2 restrictions to stifle opposition: In Venezuela, President Nicolás Maduro's government used the emergency powers, acquired to contain the SARS CoV-2 crisis, to stifle dissent.[21] The government used different tactics such as raids on private property, arbitrary detentions and media smear campaigns to target political opponents. On April 2, the government announced a Furia Bolivariana campaign, which purported to protect *Chavismo* from its opponents. Over a hundred incidents that targeted the opposition supporters were documented following the announcement. The ensuing climate of fear has diminished political mobilization against the incumbent regime. This was evident during the parliamentary elections held on December 6, which were boycotted by twenty seven opposition parties and experienced a low voter turnout.[22] President Maduro led coalition won an overwhelming majority in the disputed elections and has strengthened its grip on power.

[17] Charged in lockdown riot cases, migrant workers afraid to return to Gujarat, Firstpost (October 8, 2020, url). Similar patterns where observed in Irak. Beginning October 2019, a wave of anti-government protests engulfed the city of Baghdad. The protest movement, popularly known as the "Tishreen Revolution", was directed against rampant corruption and at the government's failure to create employment. The public gatherings came to a halt due to the SARS CoV-2 outbreak and the subsequent lockdown, but resentment against the government's inept handling of Covid crisis and a worsening economic situation eventually resulted in renewed protests. Episodes of violence, and thousands of protesters marching down the streets thus marked the one-year anniversary of the original protest.

[18] Boko Haram kills troops in "deadliest" Chad raid, BBC (March 25, 2020, url).

[19] How Tablighi Jamaat event became India's worst SARS CoV-2 vector, AlJazeera (April 7, 2020, url).

[20] Would-be autocrats are using coronavirus as an excuse to grab more power, The Economist (April 23, 2020, url).

[21] How Maduro Is Using Covid-19 to Silence His Opponents Even Further, Americas Quarterly (July 21, 2020, url).

[22] 'Maduro's Grip on the System is now Total.' Venezuela's Opposition Faces Uncertain Future After Parliamentary Elections, Time (December 7, 2020, url).

The preceding four vignettes, of course, do not constitute an exhaustive catalogue of conflicts related to the policy response to the SARS CoV-2 pandemic. Still, they help us to envision different channels through which SARS CoV-2-related restrictions could impact conflict events worldwide. We explore these linkages more formally in the subsequent empirical analysis.

3 Data and empirical strategy

3.1 Data and stylized facts

Conflict. We use conflict event data from the *Armed Conflict Location and Event* dataset (Raleigh et al., 2010, ACLED, hereafter), which we extracted from the ACLED website on September 12, 2020. The data contain daily information on conflict events with specific details on the nature and the actors on both sides of the conflicts. Events are compiled from various sources, including press accounts from regional and local news, humanitarian agencies, and research publications.[23] Our analysis focuses on 134 countries, for which data on policies is also available, from January 1, 2016 to September 1, 2020 (see Online Appendix Table A1.1 for a full list of countries). We do not consider the last ten days of data (September 2-12, 2020), as it is likely that reporting of events for this period was still incomplete when we retrieved the data.[24]

In our estimations, we consider two broad categories of conflict events as defined by ACLED, irrespective of whether ACLED identifies them as directly related to SARS CoV-2: "violent events" and "demonstrations."[25] "Violent events" include battles, violence against civilians and remote violence, and are carried out by armed groups with a political objective. On the other hand, "demonstrations" are spontaneous actions with a political motive and include protests and riots by the public. The two event categories thus differ in the nature of the actors involved: the former consists of organized armed groups or states, while the latter involves individuals or groups representing the general population. In our baseline estimates, we separately consider the daily number of violent events and demonstrations observed in each country.

Figure 1.(a) displays the weekly number of events since January 1, 2019. The total number of demonstrations declines dramatically from March 2020 onward. In March 2020, it is 16.5% lower than the number of demonstrations at the same period in the previous year; in the first half of April 2020, it is more than 50% lower than in the first half of April 2019. The drop is however temporary, as by July 2020, demonstrations

[23] These data have been widely used in recent conflict literature, see for instance Besley and Reynal-Querol (2014), Michalopoulos and Papaioannou (2016), and Berman et al. (2017).

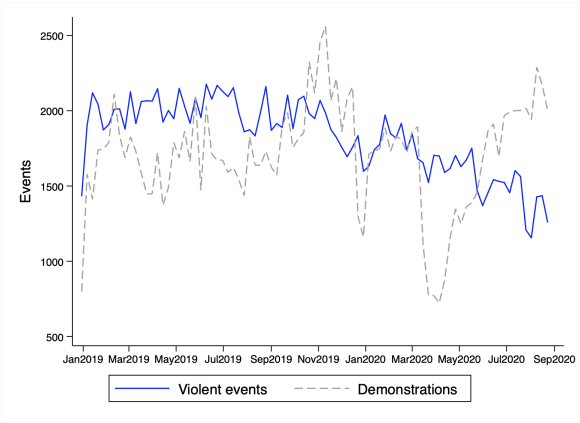
[24] ACLED is updated on a daily basis, but events are added retrospectively if they did not yet appear in the various sources immediately after they occur.

[25] Though they account for a smaller share of the observations (17.8%), ACLED also reports information on "Strategic developments," events which are not directly conflictual. These events typically include "pivotal events within campaigns of political violence," such as peace agreements, non-violent transfer of territory, arrests, etc. We do not account for non-violent events in this paper.

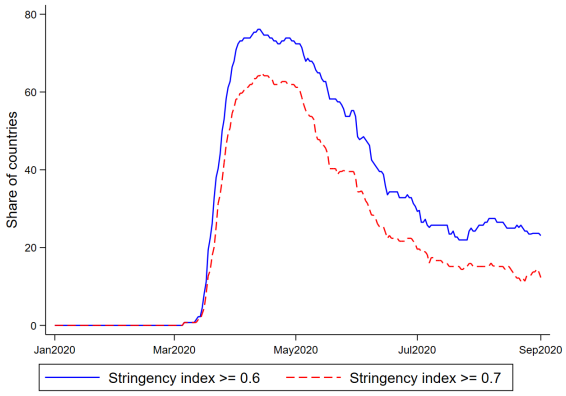
surge back to their average in 2019. The drop in violent events is less striking, yet they also decline by almost 20% in March-April 2020 compared to the previous year.

Figure 1: Conflict events and policy responses to the SARS CoV-2 pandemic

(a) Conflict



(b) Restriction policies



Source: Authors' computation from ACLED and OxGRT data. See main text for details.

SARS CoV-2-related Policies. Information on the various governmental policies in response to the SARS CoV-2 outbreak are collected from the Oxford COVID-19 Government Response Tracker (OxCGRT) (Hale et al., 2020).[26] OxCGRT systematically assembles information on several policy responses governments have implemented, using public sources such as official government press releases and newspaper articles. Data include information on 18 indicators gathered under four categories: containment and closure policies, economic policies, health system policies and miscellaneous policies. In this paper, we focus on SARS CoV-2 related policy responses under the OxCGRT category "containment and closure policies.". This categories include eight specific policies: the closings of school, workplaces and public transport, travel restrictions (internal and international), limitations of public gatherings, and stay-at-home requirements. Measures are ranked on a scale ranging from 0 to 4: 0 means no measure is in place, 1 means that the measure is a recommendation, and 2 to 4 denote mandated measures of different scale or intensity.[27] The OxCGRT data also contain information on whether the measure is a national or a regional one.

We pursue two strategies to measure restriction policies. As our baseline, we compute a global index which includes all policies and define a "shutdown" as the period during which this index exceeds some threshold value of restriction intensity. In addition to it, we consider each policy separately in our estimations. The first strategy uses an aggregate index, which is essentially a replication of the "stringency index" computed in OxCGRT data, with the only difference that we consider only nation-wide measures, and ignore regional

[26] Data were downloaded on September 12, 2020.

[27] For instance, in the case of closure of schools, a value of 2 implies a required measure to close some part of the schooling system (only high schools or only public schools), while a value of 3 implies a complete shutdown of the schools at all levels.

policies. Our stringency index thus takes the following form:

$$\text{stringency index}_{it} = \frac{1}{8}\sum_{p=1}^{8}\left(\frac{p_{it}}{\max p_{it}}\right) \tag{1}$$

such that each policy p_{it} is weighted by its maximum value for each country i at day t. The stringency index ranges from 0 (no policy implemented) to 1 (all policies implemented at their highest restriction level). We define a binary restriction measure, henceforth called *shutdown*, which switches to 1 when the stringency index exceeds a certain threshold. In choosing the threshold we face a trade-off, which is apparent in Figure A1.2 (online appendix). The curves represent the share of countries under lockdown, starting from the lowest cutoff value at the top (0.1), down to the highest values at the bottom (0.9). Given that countries tend to start with a package of measures, very low cutoff values make little sense, as they are very correlated with higher values at the start of the shutdown; at the same time, low threshold values imply that countries remain in the "shutdown" state even when most measures have been lifted. For very high cutoff values (0.9), on the other hand, shutdown are very uncommon and short-lasting. We select 0.6 as our baseline cutoff, because it is approximately the threshold of stringency at which the majority of restriction policies are implemented,[28] and because this threshold level allows us to retain enough sample variation. However, this choice being partly arbitrary, we also perform several sensitivity exercises. First, we show results obtained with alternative thresholds, within the $[0.4; 0.8]$ interval. Second, we estimate the effect of each of the eight restriction policies separately.

According to our baseline definition, 112 countries in the sample enforced a shutdown between March 6 and September 1 (in online appendix Table A1.2 we display the list of starting dates by countries). On the other hand, 22 countries do not impose a shutdown, i.e. in their case the stringency index remains below 0.6 during the entire period. Shutdown lasts for 79 days on average, but there is a large heterogeneity across countries, with a standard deviation of 58 days and a maximum of 170 days for two countries (Paraguay and Honduras). Since implementing restriction policies, countries have not monotonically increased the severity of restrictions over time. Around one-fifth of the countries in the sample have implemented multiple waves of shutdowns (up to three waves).[29] To account for this pattern, we define day 1 to be the day when a shutdown first occurs, and consider days when the stringency index declines below our defined cutoff like an absence of a shutdown. Post-shutdown periods are excluded from our baseline sample.

Figure 1.(b) depicts the global move toward restriction measures in response to the SARS CoV-2 crisis. Until early March, none of the countries in our sample had imposed a shutdown, based on the cutoff of 0.6 for the stringency index. Within a month since then, shutdowns had affected about half of the countries in our

[28] If five out of eight policies are fully implemented, the index equals 0.625.

[29] In the sensitivity analysis, we replicate our baseline results by removing countries with multiple shutdown periods. Results are largely unchanged (Figure A5.15 for demonstrations and Figure A5.16 for violent events).

sample. Online appendix Figure A1.1 displays the same pattern when focusing on each policies separately.

Country characteristics. The impact of restriction policies on conflict may depend on underlying country characteristics, as discussed in Section 2. To avoid any reverse causality issue, we consider pre-sample average of the indicators, over the period 2010-2015, for income (real GDP per capita) and political institutions. Data on GDP per capita comes from World Development Indicators (World Bank Group et al., 2010). Information on country-specific ethnic and religious polarization and fractionalization come from Reynal-Querol (2002) and Montalvo and Reynal-Querol (2005).[30] Fractionalization reflects the probability that two randomly selected individuals from a given country will not share a certain characteristic. Polarization capture the extent to individuals in a country are distributed across different groups. A high polarization index means that there is a large minority in a country.[31] Both variables can capture the relationship between ethnic cleavages and political instability or conflict that could exist across countries (Esteban and Ray, 1994, 1999; Montalvo and Reynal-Querol, 2005). We measure democracy using the combined Polity/Freedom House index as available from the Quality of Government dataset (Teorell et al., 2020). Finally, global institutional quality is proxied by a measure of the rule of law from the Worldwide Governance Indicators (World Bank Group et al., 2010), again retrieved from Quality of Government dataset. This variable captures the confidence in the rules of society by including perceptions on crime, the judiciary system and enforceability.

Since the beginning of SARS CoV-2 pandemic, Reporters Without Borders has been documenting state censorship, disinformation, and negative consequences on the right to reliable news.[32] These effects are argued to be more prominent in countries where media independence and pluralism or respect for freedom of press was rather low before the pandemic.[33] North Korea is a glaring example of how the Reporters Without Borders's evaluation of pre-SARS CoV-2 press freedom across countries correlates with media reporting during the pandemic. The country, which is ranked at the bottom of the 2020 World Press Freedom Index, did not report a single SARS CoV-2 case for alteast a year since the disease first emerged.[34] To account for potential bias in the reporting of conflict that could be due to media restrictions, we include the press freedom index from Reporters Without Borders (Quality of Government dataset).[35] Again, we consider the pre-sample average of press freedom to avoid reverse causality concerns.

In our attempt to explain how restrictions influence demonstrations, we study a potential channel that can be an alternative to the coordination and mobilization costs: an information-based mechanism. In the

[30] Data is available on the authors' personal websites, url.

[31] When comparing the fractionalization index with the polarization index, the latter takes into consideration the relative size of each group as a weight, while the size of each group has no effect on the former.

[32] A real time update of SARS CoV-2 related violations are compiled here.

[33] Reporters Without Borders use anecdotal evidence in the 2020 World Press Freedom Index to highlight a correlation between press freedom violations related to the SARS CoV-2 epidemic and the ranking in the Index, url.

[34] "North Korea Has Adopted Severe Measures to Stave Off the Coronavirus", Carnegie Endowment for International Peace (December 17, 2020), url. The World Press Freedom index measures the countries' press freedom record in the previous year, url.

[35] The press freedom index measures countries on a scale of 0-100, with 100 representing countries with the least press freedom. We inverse the measure, a higher score indicating greater press freedom, to ease interpretation.

early stage of the pandemic, individuals may have interpreted restriction policies as a signal of potential health risks, before eventually realizing that such risks were (from an individual perspective) lower than expected. To study such informational channel, we exploit three distinct information: (i) the timing of policy restrictions in countries within the same region; (ii) SARS CoV-2 related-deaths in surrounding countries a week before the timing of the country-specific lockdown policies, weighted by their bilateral distance; and (iii) country-levels of cell-phone coverage, as provided by the World Telecommunication/ICT Indicators Database (ITU, 2021).[36] We eventually investigate if the rising mobility and coordination costs result in protests to move online, by using data from *change.org* on the monthly number of petitions and signatures over the period 2019-2020.

Table 1: Descriptive Statistics

	Obs.	Mean	S.D.	Min	Max
Violent Events	170861	2.11	7.89	0	175
Demonstrations	170861	1.50	6.01	0	325
Shutdown (SI $\geq$ 0.6) & year=2020	25074	0.40	0.49	0	1
Shutdown (SI $\geq$ 0.7) & year=2020	25046	0.30	0.46	0	1
GDP per capita (in thousand $)	161965	6.65	11.49	0	80
Ethnic polarization	126136	0.54	0.21	0	1
Ethnic fractionalization	126136	0.54	0.27	0	1
Religious polarization	126136	0.62	0.31	0	1
Religious fractionalization	126136	0.37	0.20	0	1
ICRG Quality of Government	131006	0.44	0.14	0	1
Rule of law	163910	-0.54	0.68	-2	1
Press Freedom (inverse scale)	162690	-40.74	18.19	-95	-7

Note: Source: authors' computation from ACLED, QoG, OxCGRT and Reynal-Querol (2002) data. See main text for more details.

Descriptive statistics. Table 1 provides summary statistics for the main variables (Table A1.3 in online appendix gives the full list of variables and their summary statistics). The two measures of conflict events that are reported in our sample period display similar daily averages and standard deviation. Our measures of shutdown, with a cutoff at 0.6 or 0.7 for the stringency index, are also relatively similar. Given that ACLED primarily focuses on developing countries, unsurprisingly, the countries included in our sample exhibit lower GDP per capita (about 6,650 current USD, compared to a world average of 15,000 over the same period), quality of government score (0.44, as compared with a cross-country average of 0.53 at the world level), rule of law indexes (-0.54, while the world level is -0.08) compared to the world average. On the other hand, the level of ethnic heterogeneity is more comparable to the world average (0.54/0.54 in our sample versus 0.51/0.44 at the world level, for polarization and fractionalization respectively). Religious polarization and fractionalization, on the other hand, are both higher in our sample than for the world average (0.62/0.37 in

[36]The measure is defined as 2016-2019 average of the number of mobile-cellular subscriptions per 100 inhabitants in each country.

our sample vs. 0.47/0.28 for the global average).

3.2 Empirical strategy

Baseline specification. We use an event-study design to examine whether the time path of estimated effect on conflict changes discretely in response to the restriction policies. This allows us to demonstrate if there are any pre-existing trends in conflict intensity propensity across countries that implemented the restriction policies, compared to the non-implementing countries. Though the absence of pre-trends does not rule out endogeneity issues – an issue we come back to at the end of the section – they are informative of the slope of unobserved counterfactual trends. The event-study design also enables us to estimate how the association between restriction policies and conflict evolves over time. We regress conflict outcomes on a set of restriction indicators ranging from 120 days before to 120 days after implementation of the restriction policies,controlling for country and year × month fixed-effects, as well as country-specific linear time trends. The latter relax the common trend assumption and capture potential correlated trends in conflicts, SARS CoV-2 outbreak and restriction policies. We further include a set of SARS CoV-2 outbreak dummies denoting the days since the first SARS CoV-2 case has been recorded in the country, aiming to control for any behavioral change among the conflict actors that could be in response to the disease outbreak.[37]

Our specification takes the following form:

$$\text{Conflict}_{it} = \sum_{k=-120}^{120} \beta_k \text{Shutdown}_{i,t-k} + \sum_{k=0}^{\infty} \gamma_k \text{Covid}_{i,t-k} + \mu_{ym} + \eta_i + \eta_i \times T_{ym} + \varepsilon_{it}, \tag{2}$$

where Conflict_{it} is either the number of violent events or the number of demonstrations in country i at day t. $\text{Shutdown}_{i,t-k}$ denote the set of indicator variables capturing each day around the implementation of a shutdown policy at time t. $\text{Covid}_{i,t-k}$ is an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $(t-k)$ days ago. μ_{ym} are year-month fixed-effects, capturing common year-month shocks, in particular the global spread of the SARS CoV-2 that correlates with lockdown policies, and worldwide seasonality in conflict incidence. η_i are country fixed-effects accounting for any time-invariant or slow-moving country characteristics, such as political system, institutions, or culture, that may affect conflict, SARS CoV-2 outbreak and restriction policies. Finally, $\eta_i \times T_{ym}$ are country-specific linear time trends that allow for the unobserved country-specific conflict propensities to trend linearly over time. β_k are the coefficients of interest, which identify the variations in conflict intensity around the implementation of

[37] For instance Farboodi et al. (2020) suggest that forward looking and rational actors would internalize the cost of getting sick. They find evidence that individuals in the United States significantly reduced their social activity even before the state and the local governments imposed the restriction policies.

restriction policies. Finally, note that in these estimations we exclude the post-restriction periods, i.e. the periods during the restrictions are lifted.[38]

Since our dependent variable is the count of conflict events, the baseline model is estimated using a Poisson pseudo maximum likelihood estimator (PPML). The estimation technique is preferable to log-linearized models that are estimated with OLS, as their estimates can be biased under heteroskedasticity (Santos Silva and Tenreyro, 2006), an issue particularly relevant in our case given that our dependent variable, conflict events, may be affected by skewed measurement error (Millimet and Parmeter, 2021). Further, the PPML method provides a natural way to deal with zero values of the dependent variable. We also replicate the baseline analysis with OLS for comparison – the dependent variable in this case is either the count of events in level or the Inverse Hyperbolic Sine transformation of the number of events.[39] Finally, standard errors are clustered at the country level to permit valid inference if errors are auto-correlated within country. To illustrate the impact of restriction policies more concisely and to increase the precision of the estimates, we also consider a corresponding specification with monthly time periods, i.e., where we replace the daily shutdown dummies by a set of month dummies capturing the number of months before and since the start of the restriction (within a four month window around the start of the event).

Threats to identification. The validity of the difference-in-differences (DD) strategy relies on the assumption that countries having implemented shutdown policies are similar to the non-implementing countries. The dynamics of SARS CoV-2 pandemic across countries and how people behaved makes likely that the assumption is violated (Goodman-Bacon and Marcus, 2020). Further, countries have implemented a number of policy measures in response to the pandemic, which makes comparison in terms of the policy treatment difficult (Goodman-Bacon and Marcus, 2020). To address the first concern, we implement a flexible event study design, where we compare the pre-shutdown conflict trends between the treatment and the control groups. In addition, we control for the timing of SARS CoV-2 outbreak to eliminate a likely spurious negative effect of the shutdown measures, since conflict activity could also scale down in response to the disease outbreak. The inclusion of country-specific linear time trends also accounts for the possibility that different trends in initial country characteristics, such as in terms of GDP or population density, could have jointly affected the policy response to the pandemic and conflict outcomes. Finally, we address concern regarding the comparability of shutdown policies in a robustness check where we perform the event study analysis around specific shutdown measures.

Despite the inclusion of various controls for SARS CoV-2 outbreak and country-specific trends, our results remain subject to caution. Specifically, we cannot fully rule out the possibility that the duration of restriction

[38] Given that the post-restrictions period is excluded from the estimation, the β_k coefficients should be interpreted as deviations from the reference period, which is the pre-restriction period up to 120 days before the shutdown starts.

[39] This transformation, frequently used in applied econometric work, approximates the natural logarithm while allowing to keep zero-valued observations in the estimation (MacKinnon and Magee, 1990).

policies is influenced by conflict patterns in the aftermath of the policies implementation. The way in which countries may respond to a surge in conflict is unclear. On the one hand, they may decide to lift restrictions. We would in that case be more likely to observe long-lasting policies in countries where the increase in conflict is limited: our estimates would represent a lower bound of the true effect. But they might as well do the opposite, i.e., reinforce restrictions whenever conflict intensifies. To get an idea of the direction and magnitude of this bias, we estimate equation (2) on a sub-sample of countries for which our shutdown measure equals 1 for at least three consecutive months. As shown later, the results are similar in this case.

Another potential cause of endogeneity is related to non-classical measurement error in the dependent variable. The count of conflict events may have skewed errors (Millimet and Parmeter, 2021), and heteroscedasticity may be present. The use of a PPML estimator alleviates the problems related to log-transforming a variable which has heteroscedastic errors (Santos Silva and Tenreyro, 2006). Non-classical measurement error could also arise if restrictions negatively affect the reporting of conflict events – therefore generating a downward bias in our estimates. Taking seriously this potential threat, we include in our baseline estimations interactions terms between the policies dummies and an indicator of press and media freedom – the assumption being that media reporting is more negatively impacted in countries where freedom of press was low before the pandemic – which is consistent with the anecdotal evidence discussed earlier.

To conclude, though we do our best to account for potential sources of endogeneity biases and to provide evidence that such biases are limited, we cannot claim that our results are causal. The tests we perform are only suggestive, and the implementation of shutdown policies in response to the SARS CoV-2 pandemic is not a natural experiment with an observable counterfactual. Still, we believe that our results are informative about the different dynamics that various types of conflicts may follow across countries in the aftermath of the implementation of such policies.

The impact of country-characteristics. We also allow the dynamics of conflict following restriction policies to differ along the line of country characteristics. First, we split the sample according to country characteristics that, in light of the theories developed in Section 2, could affect how violence responds to such policies – ethnic or religious divisions, income, democracy level in particular. We then include interactions between our policy restriction indexes and dummies denoting high (above median) or low (below median) levels of a given country characteristics. The specification takes the form:

$$\text{Conflict}_{it} = \sum_{m=-4}^{4} \text{Shutdown}_{i,t-m}(\beta_k^H C_i^H + \beta_k^L C_i^L) + \sum_{k=0}^{\infty} \gamma_k \text{Covid}_{i,t-k} + \mu_{ym} C_i^H + \mu_{ym} C_i^L + \eta_i + \eta_i \times T_{ym} + \varepsilon_{it}, \tag{3}$$

We consider the monthly decomposition of the effect of policies in these estimations, mostly because the noise associated with the daily one is magnified when using such a sample split. In equation (3), we control

for time dummies interacted with high/low country characteristics dummies, so that β_k^H and β_k^L can be interpreted as the deviation of a country belonging to the high/low group to its average conflict level, around the implementation of the policies. Finally, note that, when considering country characteristics other than income, we also systematically control for interaction terms between restriction policies and real GDP per capita, as most of our country indicators may correlate with income.

4 Results

4.1 Demonstrations

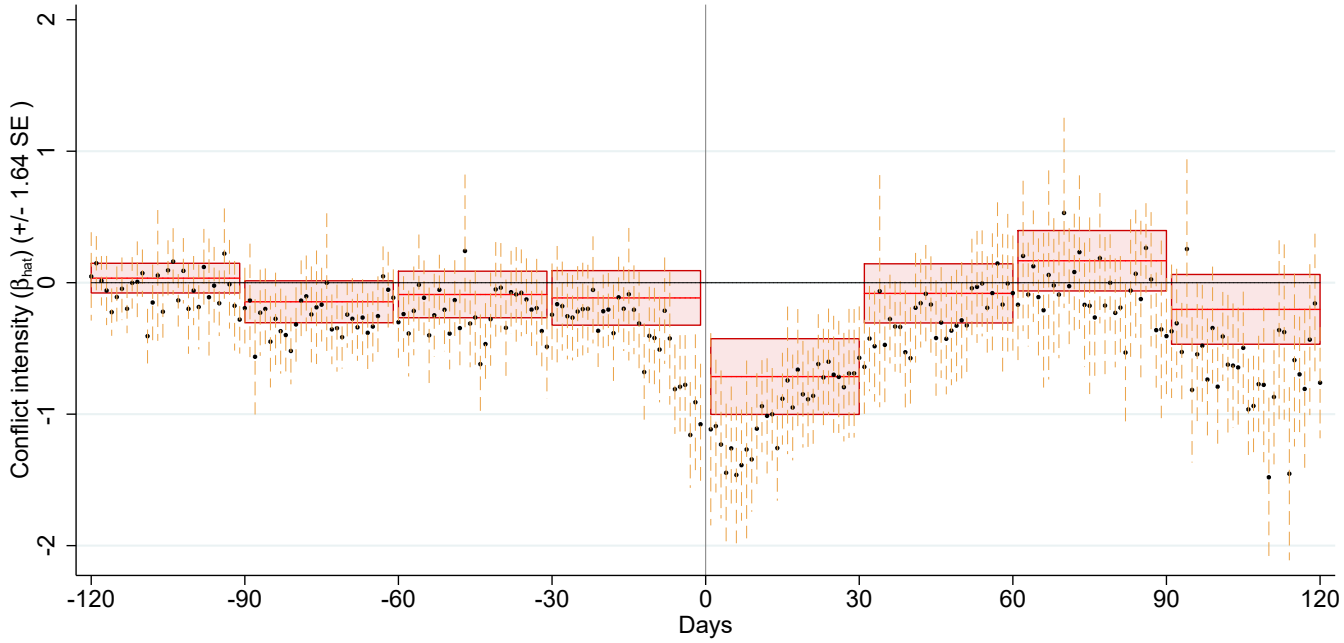
Baseline estimates. Figure 2 displays the main dynamic estimates of the evolution of the number of demonstrations in a country, i.e. both protests and riots, around the implementation of the restrictions. The points denote the daily estimates ($\hat{\beta}_k$, with k $\in [-120, ..., 120]$), with dotted lines representing 90% confidence intervals. The red boxes depict the monthly estimates and their confidence intervals. The number of events start declining around a week before the policies are implemented, and are estimated to drop on average by 0.8 event per day during the first month. The decline in the number of events just prior to Day 0 is likely due to the fact the our measure is defined around the stringency level of restrictions rather than simply when *any* restriction measure was first imposed.[40] The decline in demonstrations is substantial, when we compare to the unconditional mean of 1.5 events per day observed in our sample.

The decline of shutdown policies on demonstrations is short-lived: after a month, the number of demonstrations is back to their pre-crisis level, and remains around that level in the following months. Overall, on average, our results suggest SARS CoV-2-related restrictions are only associated with a temporary drop in demonstrations, and then, after a month, there is a noticeable resurgence of demonstration activity. This U-shaped pattern is reinforced in our sensitivity analysis. First, using different thresholds to define a country-specific shutdown (Figures A3.5 and A3.7) leads to similar results. Second, the results are strikingly similar across regions (online appendix Figures A7.20.(a) for Africa, A7.20.(c) for Asia, A7.20.(e) for Latin America), whether we use an OLS estimator instead of PPML (Figures A4.9 and A4.10), remove countries with multiple waves of shutdown (Figure A5.15) or exclude outliers (Figure A9.25), or if we estimate the effect of each policy separately (Figure A6.17).[41] Similarly, we do not observe any substantial differences across countries with different socio-economic or institutional characteristics: the same short-lived decline in demonstrations is

[40] Alternatively, the pre-shutdown decline could be due to anticipation of the restrictions: however, looking at the measures individually (Figures A6.18.(a) to A6.18.(f) in the online appendix) we find that, with few exceptions, the decline in demonstrations is statistically significant only after measures are implemented. This suggests that the observed pre-shutdown decline found in Figure 2 mostly comes from the fact that some measures are already in place before our shutdown measure switches to 1.

[41] The effect is comparable in size and duration across all policies, except for restrictions to international movements which exhibit a much smaller and statistically insignificant decline.

Figure 2: Timing of Shutdown and Demonstrations



Note: This figure plots the coefficients β_k of equation 2 when conflict represents the daily number of demonstrations in a country. Shutdown$_{i,t-k}$ is an indicator that equals 1 since when the Stringency index is equal or above a cutoff of 0.6 (range between 0-1), in a given country. The points denote the daily estimates, with dotted lines representing 90% confidence intervals. The red boxes depict the monthly estimates and their confidence intervals. All specifications include an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $t-k$ days ago, year-month fixed-effects, country fixed-effects and country-specific linear time trends. Standard errors in parentheses are clustered at the country-level. Table A1.3 in the online appendix contains descriptive statistics about each variable used in the estimation.

found regardless of the country's level of income, ethnic or religious divisions, or institutional quality (online appendix Figures A11.30 to A11.31(a)-(d)).

Several mechanisms could in principle explain these results. First, this temporary drop in demonstrations may be spurious, i.e., it can either be due to hampering of media reporting in the short-run, or by countries actually relaxing the mobility or social distancing constraints over time. Yet, controlling in these estimations for a full set of interaction terms between the policy implementation dummies and a measure of initial country-specific media freedom leaves the results largely unchanged (online appendix Figure A2.3). Similarly, when restricting the sample to countries where the shutdown lasted for at least three months – i.e. countries that maintained the restriction for a longer time period – we find similar results (online appendix Figure A5.13).

Two non-exclusive explanations remain. The more natural explanation is that restrictions increased coordination and mobilization costs, but that this increase was quickly counteracted by the negative consequences of the policies – unemployment, economic deprivation – which pushed demonstrations back to their pre-restrictions level. Alternatively, in the early stage of the pandemic, individuals may have interpreted restriction policies as a signal of potential health risks, before eventually realizing that such risks were (from

an individual perspective) lower than expected. This information-based mechanism would also explain why the drop is only temporary (even in the absence of economic deprivation). In Section A12 of the online appendix we provide suggestive evidence that this information channel is less likely to be driving our results. We first show that the relationship between shutdown policies and demonstrations does not differ depending on whether the country implemented the policy later or relatively early, compared to the region average (Figure A12.36). Similarly, splitting the sample based on whether the country is surrounded by neighbors with relatively high or low disease lethality (SARS CoV-2 related-deaths weighted by bilateral distance) does not affect the short-lived reduction in demonstrations (Figure A12.37). Second, we show that our results do not differ much across countries with different levels of cell-phone coverage (Figure A12.38).[42] More details about these estimations appear in the online appendix. Though these are imperfect tests – the timing of a country's shutdown within its region may be endogenous, for instance – they fail to provide support for the information channel. Hence, we consider as more likely the coordination/mobilization costs channel: the net cost of participating to demonstrations rises in the immediate aftermath of the policies' implementation, but the factors underlying the demonstrations remain, and are potentially magnified by the shutdown policies, leading conflict to move quickly move back to its pre-shutdown level. This U-shape pattern accords well with several of the case studies discussed in Bloem and Salemi (2021). In particular, a short-lived drop in demonstrations is documented by Bloem and Salemi (2021) in the case of India, Chile, South Africa, Uganda, Venezuela, Algeria, and to a smaller extent in the Philippines.[43]

Offline versus online protesting. In the online appendix A13 we make use of data from *change.org* on the monthly number of petitions and signatures over the period 2019-2020, for all countries to investigate whether the observed drop in demonstrations could be partially explained by a substitution between online and offline protesting. Though we do find that the monthly level of petitions and online signatures has increased during the SARS CoV-2 period (Figure A13.39) we do not find evidence that shutdown policies are associated with a significance increase in this form of protesting. Taken at face value, this suggests that there was no substitution between online and offline protesting, at least not within country. This non-result should however be interpreted with caution, because the data spans over a short-period (2019-2020) and because we cannot use the daily dimension of our policy measure.

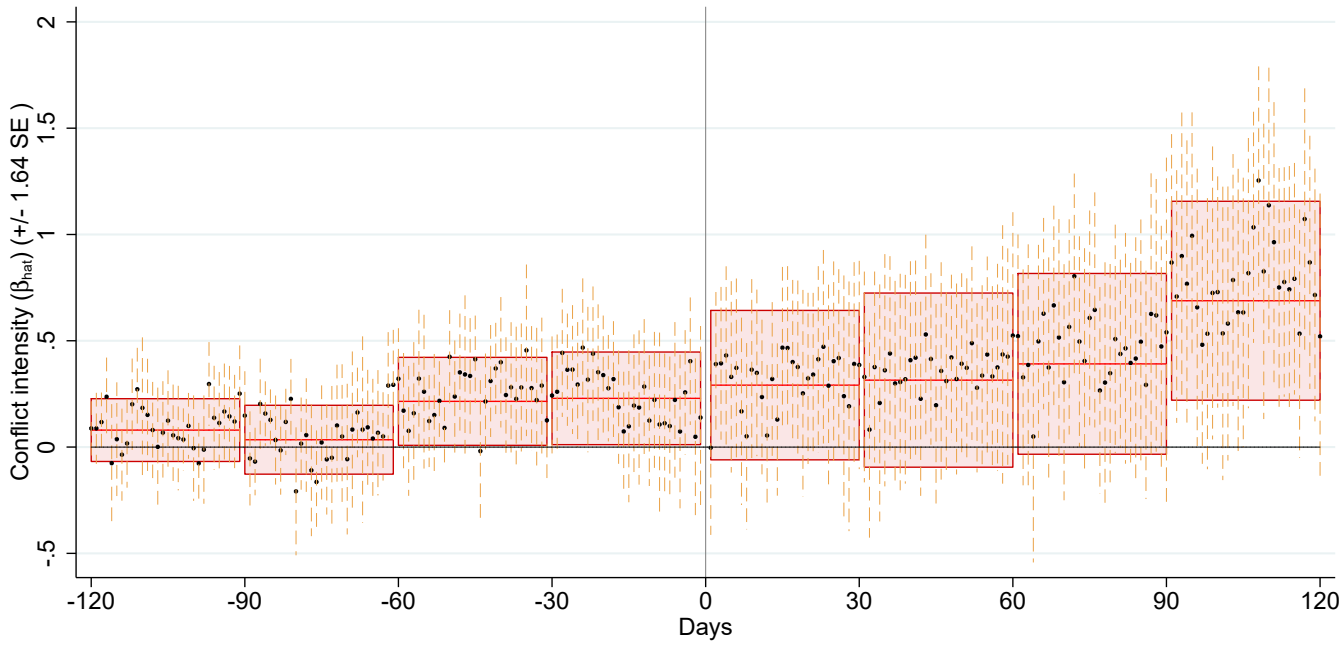
[42] This result is in contrast with recent literature which suggests that information technology is a significant driver of protests and political mobilization (Zhuravskaya et al., 2020; Manacorda and Tesei, 2020).

[43] In the case of Lebanon, demonstrations are not found to decrease by Bloem and Salemi (2021); a factor they mention in this specific case is that the pandemic may have accelerated the country's ongoing currency crisis. Note that the U-shaped pattern documented by Bloem and Salemi is mainly driven by protests. The patterns they observe for riots are more mixed: U-shaped in the case of South Africa and Chile, short-run increase in the case of India, Lebanon and Venezuela.

4.2 Violent Events

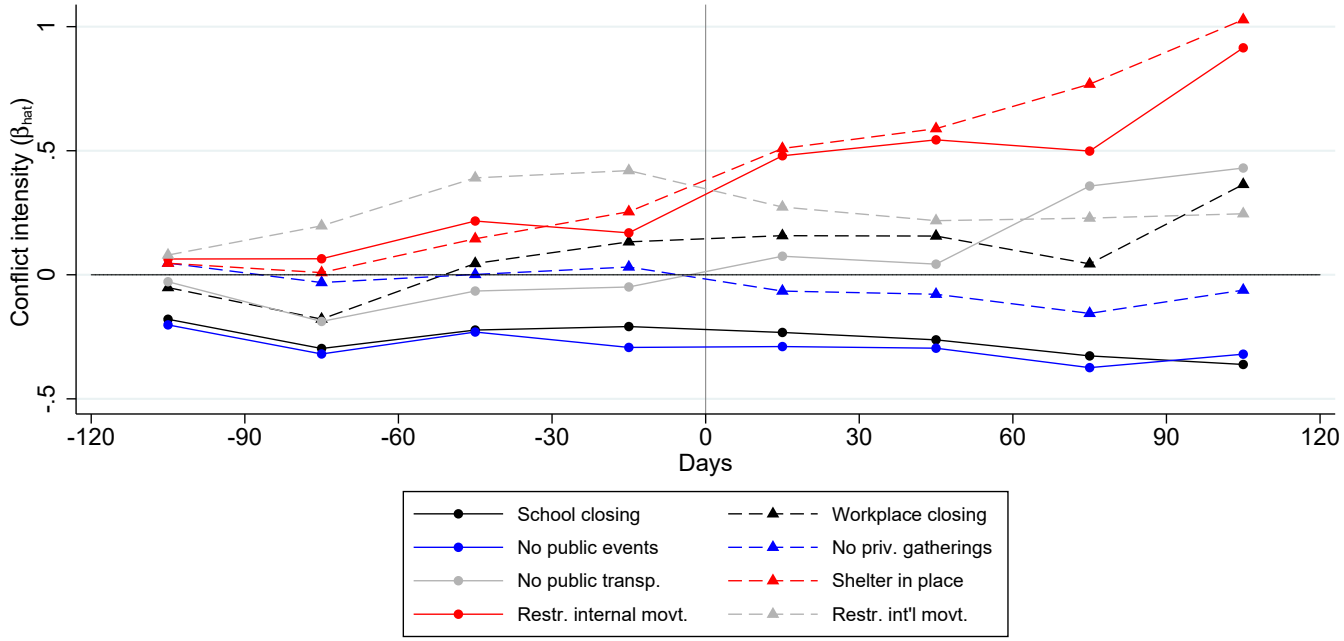
Figure 3 shows how violent events – battles, violence against civilians, remote violence – evolve around the implementation of shutdown policies, in a similar way as in the previous figure. The results differ sharply from those on demonstrations: we do not observe an instantaneous drop in violent events. If anything we find that the number of events, on average, slightly increases following the implementation of the measures: though the effect is imprecisely estimated, estimates for month $t + 4$ are statistically different from the pre-implementation period, and the average post-implementation coefficient is significantly higher than the pre-implementation average. A similar lack of decline is observed when using a linear estimator (Figures A4.11 and A4.12) or when removing countries that imposes multiple shutdown periods (Figure A5.16). On the other hand, using a higher threshold for the stringency at 0.7 leads to a sharper estimated increase in conflict (Figure A3.6). In fact, increases in the intensity of violent events is only found for cutoffs of stringency indexes of 0.5 and above (Figure A3.8), and the sharpest increase is found for cutoffs of 0.6 or above. Similarly, restricting the sample of policy-implementing countries to those in which restrictions lasted at least three months also points to a stronger increase in violent conflict following the implementation of these policies (Figure A5.14).

Figure 3: Timing of Shutdown and Violent Events



Note: This figure plots the coefficients β_k of equation 2 when conflict represents the daily number of violent events in a country. $\text{Shutdown}_{i,t-k}$ is an indicator that equals 1 since when the Stringency index is equal or above a cutoff of 0.6 (range between 0-1), in a given country. The points denote the daily estimates, with dotted lines representing 90% confidence intervals. The red boxes depict the monthly estimates and their confidence intervals. All specifications include an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $t - k$ days ago, year-month fixed-effects, country fixed-effects and country-specific linear time trends. Standard errors in parentheses are clustered at the country-level. Table A1.3 in the online appendix contains descriptive statistics about each variable used in the estimation.

Figure 4: Conflict dynamics across specific restriction policies



Note: This figure plots the coefficients equivalent to β_k from equation 2 – when conflict represents the daily number of violent events in a country – but estimated for each policy measure separately. We first estimate equation 2 using dummies for specific policies instead our shutdown index. The figure shows the average of the obtained coefficients, averaged by month for readability. All specifications include an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $t-k$ days ago, year-month fixed-effects, country fixed-effects and country-specific linear time trends. Standard errors in parentheses are clustered at the country-level. Table A1.3 in the online appendix contains descriptive statistics about each variable used in the estimation.

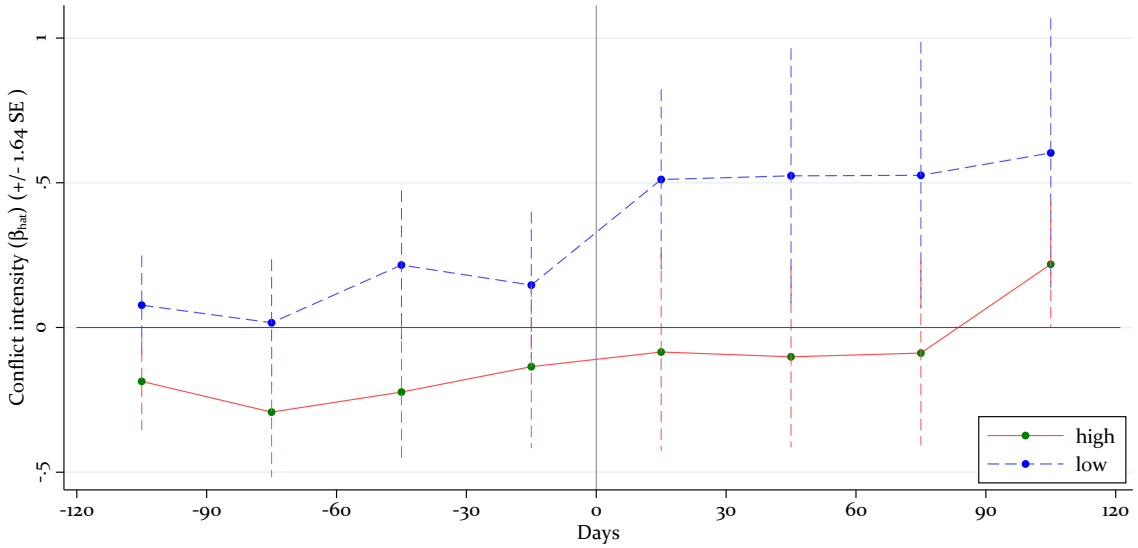
The increase in mobility and coordination costs caused by shutdown policies do not appear to be associated with a drastic reduction in events that involve armed actors. Yet, this average effect masks a significant heterogeneity. In subsequent analysis, we address some of the potential sources of heterogeneity.

Heterogeneity across policies. We first estimate whether our results could be driven by specific restriction policies. The results obtained when considering each policy separately are shown in Figure 4 (online appendix Figure A6.19.(a) to Figure A6.19.(h) show the detailed results with confidence intervals and daily estimates). Shelter-in-place and restrictions to internal movements are the two policies that are associated with the strongest increase in violence. These measures are arguably among the most stringent restrictions in the set of policies that we consider. The other types of restrictions do not appear to be significantly associated with changes in the number of violent events.

Types of events. We next utilize the richness of the ACLED dataset by focusing on the sub-event categories among violent events. We observe a significant increase in battles after the implementation (Figure A10.27), and to a lesser extent on the violence against civilians (Figure A10.29). These results hint towards potential bargaining failures such as reduced opportunities for conflict mediation and a lower cost of repression "when the world is not watching" (Durante and Zhuravskaya, 2018). We however do not detect any changes in

the dynamics of remote violence. These are types of violent events which "aim at creating asymmetrical conflict dynamics by preventing the target from responding" and include various tactics of explosions (bombs, drone strikes, chemical weapons, suicide bombing, etc.) (Raleigh et al., 2010). It is plausible that SARS CoV-2-related restrictions have affected access to conflict-specific capital technologies (such as explosives or advanced weapons). In that sense, restriction policies could also change the nature of violent conflict by affecting fighting technologies.

Figure 5: GDP per capita and violent events



Note: This figure plots the coefficients β_k^H (in red) and β_k^L (in blue) of equation 3 when conflict represents the daily number of violent events in a country. Shutdown$_{i,t-k}$ is an indicator that equals 1 since when the Stringency index is equal or above a cutoff of 0.6 (range between 0-1), in a given country. The specification controls for time dummies interacted with high/low GDP per capita dummies (high C_i^H versus low C_i^L). The points denote the monthly estimates, with dotted lines representing 90% confidence intervals. The specification includes an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $t-k$ days ago, year-month fixed-effects, country fixed-effects and country-specific linear time trends. Standard errors in parentheses are clustered at the country-level. Table A1.3 in the online appendix contains descriptive statistics about each variable used in the estimation.

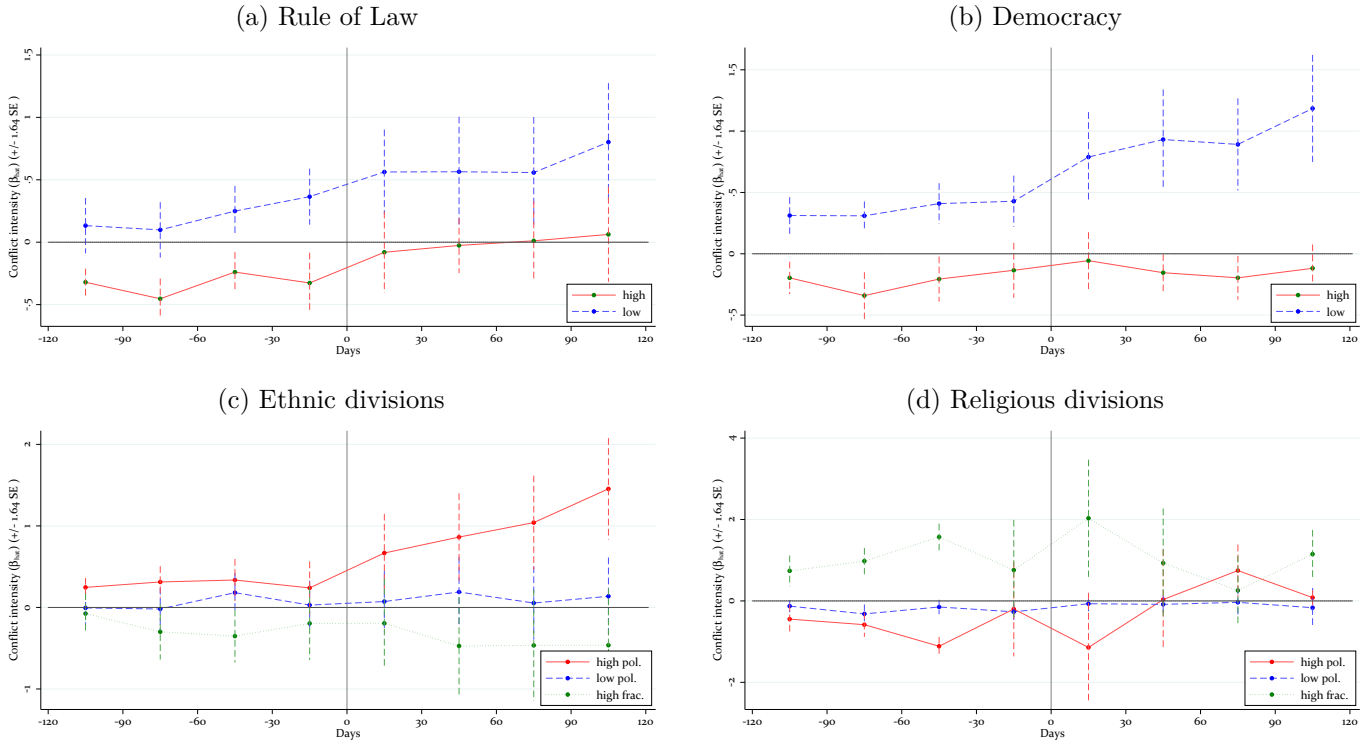
Country characteristics. The dynamics of violent events around the implementation of restriction policies may differ across countries.[44] As preliminary evidence, we allow our estimate to vary by continent. While we observe little changes in conflict in Latin America, violent events are found to intensify in Asia and Africa, the increase being more delayed but also stronger in the latter case (online appendix Figures A7.20.(b) for Africa, A7.20.(d) for Asia, A7.20.(f) for Latin America).

We explore the argument further by allowing heterogeneity across country-specific characteristics, and by

[44] Bloem and Salemi (2021) show that country-specific factors matter. In the case of violent event, they notably discuss the case of Syria, which experienced a dramatic decline in violent conflict around the time of the SARS CoV-2 outbreak, but this drop coincides with a ceasefire agreement covering the Idlib governorate and involving Turkey and Russia. This drop can be seen in our data as well. Whether the ceasefire was due to the pandemic is therefore unclear, but it coincides very much with the start of the policy measures, making it difficult to separate the two effects. Section A8 of the online appendix further discusses the Syrian case and show the results obtained when excluding Syria from our sample. In section A9, we go further and exclude from our sample all outliers countries, i.e., observations that are 2 standard deviations away from the residual mean. The increase in violent events is slightly reinforced in this case.

splitting the sample across different dimensions that are motivated by the theoretical discussion: i) pre-period real GDP per capita (Figure 5); ii) pre-period institutional quality (rule of law and democracy scores) (Figures 6.(a) and (b)); and iii) ethno-religious characteristics, i.e., polarization and fractionalization (Figures 6.(c) and (d)). Polarization and fractionalization measures capture the relationship between ethnic and religious cleavages and political instability or conflict that could exist within countries (Esteban and Ray, 1994, 1999; Montalvo and Reynal-Querol, 2005). Note that we include polarization and fractionalization measures jointly to account for their correlation. We return to this specific point below. Finally, with the exception of the results on income, the estimates are obtained after controlling for the effect of GDP per capita. The visual inspection of our results delivers three interesting features, that are globally consistent with our second prediction: restrictions are more likely to be associated with an exacerbation of violence in countries where the roots of conflict are already present.[45]

Figure 6: Country characteristics: violent events



Note: This figure plots the coefficients β_k^H (in red) and β_k^L (in blue) of equation 3 when conflict represents the daily number of violent events in a country. $\text{Shutdown}_{i,t-k}$ is an indicator that equals 1 since when the Stringency index is equal or above a cutoff of 0.6 (range between 0-1), in a given country. The specification controls for time dummies interacted with high/low country characteristics dummies (high C_i^H versus low C_i^L). The points denote the monthly estimates, with dotted lines representing 90% confidence intervals. All specifications control for interaction between restriction policies and real GDP per capita, and include an indicator variable that equals 1 if as of day t country i had confirmed the first SARS CoV-2 case $t-k$ days ago, year-month fixed-effects, country fixed-effects and country-specific linear time trends. Standard errors in parentheses are clustered at the country-level. Table A1.3 in the online appendix contains descriptive statistics about each variable used in the estimation.

First, the rise in violent events is slightly more pronounced in poorer countries, which could be due to different

[45] Similar results are obtained in online appendix Figures A11.34 when we use the alternative stringency threshold to define the shutdown binary.

operating mechanisms (Figure 5, and robustness Figure A11.33 in the online appendix). By aggravating poverty, shutdown policies may have reduced the opportunity cost of violence (Becker, 1968; Grossman, 1991; Dal Bó and Dal Bó, 2011) or impeded the state capacity to fight against armed opposition (Berman et al., 2011, Fearon and Laitin, 2003). The latter channel is especially unique to the pandemic as states have diverted immense resources to contain the spread of SARS CoV-2. Keeping poverty level constant, violence may also increase because a peaceful resolution of the conflict becomes more difficult, particularly because the negative shock to state revenues may create commitment problems.

Second, we find that the rise in violence is more conspicuous in more authoritarian countries (Figure 6.(b) and robustness Figure A11.34.(b) in the online appendix). This result is in line with anecdotal evidence that authoritarian regimes or leaders have used emergency powers acquired during the pandemic to repress political opposition (Coyne and Yatsyshina, 2020).

Third, Figure 6.(c) show that the increase in violent events is stronger for more ethnically polarized countries.[46] Little change in violence is observed for other categories. The results are more mixed in the case of religious divisions (Figure 6.(d)): though violence displays a slight increase for highly polarized countries, the estimates are not statistically significant. The problem here is that the correlation between religious polarization and fractionalization is extremely high (0.94 in our sample), which may prevent us from disentangling the effect of each variable. Indeed, when estimating the role of fractionalization (online appendix Figure A11.32.(a)) or polarization alone (online appendix Figure A11.32.(b)), we find a similar pattern in both cases: increase in countries with high levels, a stagnation in low level countries. Overall, these various results and robustness exercises (Figures A11.33 and A11.34 in the online appendix) suggest that ethnic and religious cleavages play an important role in shaping the impact of SARS CoV-2-related restriction policies on violent conflict. Indeed, both ethnic and religious tensions may have been amplified by the restriction policies. The escalation in violence could be due to scapegoating of minorities that gets heightened during epidemics (Voigtländer and Voth, 2012; Jedwab et al., 2019).[47]

Overall, we can summarize the results of this last section as follows: (i) restriction policies are not associated with peace – if anything, violent events increased in some countries when restrictions have been enforced; (ii) the most vulnerable countries are those where the antecedents for disputes are already present, i.e. poor, socially divided countries, and those with authoritarian regimes. In these countries, violent events increased

[46] Ideally, we would like to include four categories in these estimations: high polarization, low polarization, high and low fractionalization. However, given the large number of fixed-effects and the presence of country specific trends, collinearity prevents us from estimating the "low fractionalization" category. The opposite trends in conflict activity in ethnically highly "polarized" and "fractionalized" countries is consistent with evidence that measures of polarization and fractionalization tend to run in opposite directions in explaining conflict behavior (Esteban and Ray, 2008). The opposite trend in conflict activity is less pronounced for countries with high religious polarization and fractionalization, which is likely due to the fact that religious polarization and fractionalization measures are highly correlated.

[47] Jedwab et al. (2019) find that Black Death mortality increased Jewish persecution, while Voigtländer and Voth (2012) show that Black Death pogroms created anti-Jewish sentiment that persisted over centuries.

the most, either because the policies exacerbated pre-existing tensions, or because they made peaceful resolutions of ongoing disputes more difficult to reach.

5 Conclusion and Policy Discussion

In this paper, we provide real-time evidence of how enforcing restrictions to limit the spread of SARS CoV-2 correlate with conflicts globally. First, we observe a sharp decline in demonstrations, that involve spontaneous acts of public protests or rioting, following the imposition of restriction policies. But the reduction is short-lived, as the number of demonstrations are back to their pre-restriction levels in the second month, and remain there in the subsequent periods. Second, we observe that the purported increase in mobilization or coordination costs, following the imposition of shutdown policies, is not associated with a decline in violent events that involve organized armed groups. Such type of violence actually intensifies around the policies' implementation in countries which exhibit an initial fertile ground for conflicts.

Though discussing the overall welfare implications of the policy response to contain the SARS CoV-2 pandemic is beyond the scope of the paper, our results are informative of the contexts in which various types of conflict may be exacerbated by such policy response. Our results suggest in particular that armed conflict may intensify in poor countries with weak institutions and pre-existing ethno-religious divisions. As such increase in violence comes on top of the direct effect of the pandemic on economic indicators, these countries may remain trapped in a long-lasting conflict-poverty circle. International cooperation may help mitigating these risks by targeting aid and vaccination support to specific countries. We also find that some policies – shelter-in-place, restrictions to internal movements – are associated with stronger rises in armed conflict.

Our work is however only a first step to try to understand how shutdown policies might impact conflict. Given the preliminary nature of the data, the relative short time span currently available, and the remaining causality issues, more work is surely needed. Many important dimensions of the conflict-covid nexus are not considered in our analysis. For instance, future research could try to further explore cross-country heterogeneity in conflict responses, and consider within-country characteristics, such as urbanization and local income levels. This would probably help to identify the impact of restriction policies in a more causal way. Given the volatility of many commodity markets during the period, how natural resources rich regions react to the spread of the virus is surely an important question to study as well. Finally, more research is needed to understand the dynamics of demonstrations. One could try to assess whether the characteristics of these demonstrations has changed, both in terms of nature (offline and online) and in terms of content (what are the issues at stake – e.g., economic versus institutional grievances).

References

Atkeson, A. (2020). What will be the economic impact of covid-19 in the us? rough estimates of disease scenarios. Technical report, National Bureau of Economic Research.

Bates, R., A. Greif, and S. Singh (2002). Organizing violence. *Journal of Conflict Resolution 46*(5), 599–628.

Becker, G. S. (1968). Crime and punishment: An economic approach. In *The economic dimensions of crime*, pp. 13–68. Palgrave Macmillan, London.

Berman, E. (2003). Hamas, taliban and the jewish underground: an economist's view of radical religious militias.

Berman, E., J. N. Shapiro, and J. H. Felter (2011). Can hearts and minds be bought? the economics of counterinsurgency in iraq. *Journal of Political Economy 119*(4), 766–819.

Berman, N., M. Couttenier, N. Monnet, R. Ticku, et al. (2020). Conflict in times of covid-19. *COVID-19 in Developing Economies*, 147.

Berman, N., M. Couttenier, D. Rohner, and M. Thoenig (2017). This mine is mine! how minerals fuel conflicts in africa. *American Economic Review Volume 107*.

Besley, T. and T. Persson (2010). State capacity, conflict, and development. *Econometrica 78*(1), 1–34.

Besley, T. and M. Reynal-Querol (2014). The legacy of historical conflicts: Evidence from africa. *American Political Science Review 108*(2), 319–336.

Bloem, J. R. and C. Salemi (2021). Covid-19 and conflict. *World Development*, 105294–105294.

Bursztyn, L., A. Rao, C. Roth, and D. Yanagizawa-Drott (2020). Misinformation during a pandemic. *University of Chicago, Becker Friedman Institute for Economics Working Paper* (2020-44).

Carleton, T. and K. C. Meng (2020). Causal empirical estimates suggest covid-19 transmission rates are highly seasonal. *medRxiv*.

Cervellati, M., U. Sunde, and S. Valmori (2017). Pathogens, weather shocks and civil conflicts. *The Economic Journal 127*(607), 2581–2616.

Chenoweth, E. and K. G. Cunningham (2013). Understanding nonviolent resistance: An introduction. *Journal of Peace Research 50*(3), 271–276.

Chenoweth, E., M. J. Stephan, and M. J. Stephan (2011). *Why civil resistance works: The strategic logic of nonviolent conflict*. Columbia University Press.

Chinazzi, M., J. T. Davis, M. Ajelli, C. Gioannini, M. Litvinova, S. Merler, A. P. y Piontti, K. Mu, L. Rossi, K. Sun, et al. (2020). The effect of travel restrictions on the spread of the 2019 novel coronavirus (covid-19) outbreak. *Science 368*(6489), 395–400.

Coyne, C. J. and Y. Yatsyshina (2020). Pandemic police states. *Available at SSRN 3598643*.

Dal Bó, E. and P. Dal Bó (2011). Workers, warriors, and criminals: social conflict in general equilibrium. *Journal of the European Economic Association 9*(4), 646–677.

Durante, R. and E. Zhuravskaya (2018). Attack when the world is not watching? us news and the israeli-palestinian conflict. *Journal of Political Economy 126*(3), 1085–1133.

Esteban, J. and D. Ray (1999). Conflict and distribution. *Journal of Economic Theory 87*(2), 379–415.

Esteban, J. and D. Ray (2008). Polarization, fractionalization and conflict. *Journal of peace Research 45*(2), 163–182.

Esteban, J.-M. and D. Ray (1994). On the measurement of polarization. *Econometrica: Journal of the Econometric Society*, 819–851.

Farboodi, M., G. Jarosch, and R. Shimer (2020). Internal and external effects of social distancing in a pandemic. Technical report, National Bureau of Economic Research.

Fearon, J. and D. Laitin (2003). Ethnicity, insurgency and civil war. *American Political Science Review 97*(1), 75–90.

Goodman-Bacon, A. and J. Marcus (2020). Using difference-in-differences to identify causal effects of covid-19 policies. In *Survey Research Methods*, Volume 14, pp. 153–158.

Grossman, H. I. (1991). A General Equilibrium Model of Insurrections. *The American Economic Review 81*(4), 912–921.

Guerrieri, V., G. Lorenzoni, L. Straub, and I. Werning (2020). Macroeconomic implications of covid-19: Can negative supply shocks cause demand shortages? Technical report, National Bureau of Economic Research.

Hale, T., A. Petherick, T. Phillips, and S. Webster (2020). Variation in government responses to covid-19 version 4.0. blavatnik school of government working paper. april 7, 2020.

Iannaccone, L. R. (1992). Sacrifice and stigma: Reducing free-riding in cults, communes, and other collectives. *Journal of political economy 100*(2), 271–291.

Iannaccone, L. R. and E. Berman (2006). Religious extremism: The good, the bad, and the deadly. *Public choice 128*(1-2), 109–129.

ITU (2021). The world telecommunication/ict indicators database.

Jackson, M. O., M. Morelli, et al. (2009). Strategic militarization, deterrence and wars. *Quarterly Journal of Political Science 4*(4), 279–313.

Jedwab, R., N. D. Johnson, and M. Koyama (2019). Negative shocks and mass persecutions: evidence from the black death. *Journal of Economic Growth 24*(4), 345–395.

Kurrild-Klitgaard, P. (2013). It's the weather, stupid! individual participation in collective may day demonstrations. *Public Choice 155*(3-4), 251–271.

Leventoğlu, B. and N. W. Metternich (2018). Born weak, growing strong: Anti-government protests as a signal of rebel strength in the context of civil wars. *American Journal of Political Science 62*(3), 581–596.

MacKinnon, J. G. and L. Magee (1990). Transforming the dependent variable in regression models. *International Economic Review 31*(2), 315–339.

Madestam, A., D. Shoag, S. Veuger, and D. Yanagizawa-Drott (2013). Do political protests matter? evidence from the tea party movement. *The Quarterly Journal of Economics 128*(4), 1633–1685.

Manacorda, M. and A. Tesei (2020). Liberation technology: Mobile phones and political mobilization in africa. *Econometrica 88*(2), 533–567.

Martin, A., M. Markhvida, S. Hallegatte, and B. Walsh (2020). Socio-economic impacts of covid-19 on household consumption and poverty. *Economics of disasters and climate change 4*(3), 453–479.

McKibbin, W. J. and R. Fernando (2020). The global macroeconomic impacts of covid-19: Seven scenarios. *Covid Economics 10*.

Michalopoulos, S. and E. Papaioannou (2016). The Long-Run Effects of the Scramble for Africa. *American Economic Review 106*(7), 1802–1848.

Millimet, D. and C. Parmeter (2021). Accounting for skewed or one-sided measurement error in the dependent variable. *Political Analysis forthcoming*.

Montalvo, J. G. and M. Reynal-Querol (2005). Ethnic polarization, potential conflict, and civil wars. *American economic review 95*(3), 796–816.

Raleigh, C., A. Linke, H. Hegre, and J. Karlsen (2010). Introducing ACLED: an armed conflict location and event dataset: special data feature. *Journal of peace research 47*(5), 651–660.

Reynal-Querol, M. (2002). Ethnicity, political systems, and civil wars. *Journal of Conflict Resolution 46*(1), 29–54.

Rohner, D. (2020). Covid-19 and conflict: Major risks and policy responses. *Peace Economics, Peace Science and Public Policy 1*(ahead-of-print).

Rohner, D. and M. Thoenig (2020). The elusive peace dividend of development policy: From war traps to macro-complementarities. Technical report, mimeo.

Santos Silva, J. and S. Tenreyro (2006). The log of gravity. *The Review of Economics and statistics 88*(4), 641–658.

Stephan, M. J. and E. Chenoweth (2008). Why civil resistance works: The strategic logic of nonviolent conflict. *International security 33*(1), 7–44.

Teorell, J., S. Dahlberg, S. Holmberg, B. Rothstein, P. Alvarado, and S. Alelsson (2020). The Quality of Government Standard Dataset. *The Quality of Government Institute University of Göteborg. Göteborg*.

Voigtländer, N. and H.-J. Voth (2012). Persecution perpetuated: the medieval origins of anti-semitic violence in nazi germany. *The Quarterly Journal of Economics 127*(3), 1339–1392.

World Bank Group, D. Kaufmann, A. Kraay, and M. Mastruzzi (2010). *Worldwide governance indicators*. World Bank Group.

Zhuravskaya, E., M. Petrova, and R. Enikolopov (2020). Political effects of the internet and social media. *Annual Review of Economics 12*, 415–438.