The Long Shadow of COVID-19 Lockdowns
on Nonstate Actor Violence[*]

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Abstract

Early studies of the effects of COVID-19 lockdowns on nonstate actor violence presented competing hypotheses and yielded mixed results. Economic arguments primarily claimed that lockdowns would increase violence and implied that their effects would last after lockdowns were lifted. Conversely, logistical arguments claimed that lockdowns would decrease violence and that their effects would endure only as long as lockdowns were in place. Using new, more precise, and comprehensive data and measures, this study directly compares these competing arguments globally. The study finds that both economic and logistical factors affected violence and that violence was lower overall in the short and long term. Logistical factors likely outweighed economic ones due to the inability of nonstate actors to capitalize fully on the negative economic effects of lockdowns when population movements were disrupted. The study also disaggregates the economic effects of lockdowns, finding the strongest support for state capacity-based arguments.

For centuries lockdowns have been used to limit the spread of infectious diseases. Therefore, it is not surprising that lockdowns were the primary tool that governments used to stop the spread of COVID-19 prior to vaccines. Lockdowns constitute an array of measures restricting population movements, such as curfews, quarantines, stay-at-home orders, travel bans, and so forth. Lockdowns not only reduced the spread of the virus, but had far-reaching implications for education, mental health, and social activism, among other issues (Alizadeh et al. 2023). While the effects of lockdowns on these issues are well known, their effects on national security and violence by nonstate actors, such as al-Qaeda, Boko Haram, and the Islamic State of Iraq and Syria (ISIS), are less well understood and more contested.

State-imposed lockdowns have been hypothesized to affect nonstate actor violence through two competing pathways. Economic arguments predominantly claim that lockdowns increase violence by weakening states' counterinsurgency capacities and enhancing public support for antigovernment groups (Mehrl and Thurner 2021; Mercy Corps 2021; Marsai and Tarròsy 2022; Berman et al. 2022). They imply that lockdowns affect violence when they are in place and after they have been lifted. Logistical arguments claim, in contrast, that lockdowns decrease violence because restrictions on population movements make it difficult for nonstate actors to conduct attacks operationally (Brancati, Birnir and Idlbi 2023). They further claim that lockdowns decrease violence only when they are in place.

Early studies of the effects of lockdowns yielded conflicting results. Both Mehrl and Thurner (2021) and Berman et al. (2022) found that lockdowns did not significantly affect nonstate actor violence globally. Gutiérrez-Romero (2022) found, in

contrast, that lockdowns significantly increased violence against civilians in Africa, while Brancati, Birnir and Idlbi (2023) found that they decreased ISIS attacks in the Middle East and North Africa. Lockdowns, the latter hypothesized, likely affected ISIS less than other groups given its large financial reserves, rural base, and focus on non-civilian targets.

Using new, more precise, and comprehensive data and measures, this study builds on these important studies in several ways. It adjudicates between these two hypotheses theoretically, expecting lockdowns to have decreased violence overall in the short term while lockdowns were in place and population movements were disrupted. It directly tests economic and logistical arguments globally in the short and long term and finds that lockdowns decreased violence overall in both. It tests economic arguments more precisely than previous studies by directly measuring the economy instead of using lockdowns as a proxy for it and by distinguishing attacks *on* nonstate actors from attacks *by* nonstate actors. It further expands the sample of cases on which logistical arguments are tested and confirms their short-term effects. It also finds new and unexpected results. Specifically, it finds that the economy had significant effects but that the effects were only short-term, while logistical arguments had unanticipated long-term effects. Unpacking economic arguments, the analysis finds the most support for state capacity-based arguments.

Although COVID-19 is no longer a public health emergency, understanding the impact of lockdowns on nonstate actor violence remains an important question. Given the worldwide impact of the pandemic, it is important to get the historical record right. Another pandemic is also likely in the future and may be even

deadlier than COVID-19 (Biden 2023; Gates 2022; WHO 2023). Even with advances in vaccine development, lockdowns are likely to remain a vital early mitigation tool in future emergencies. Lockdowns are also commonly used outside of public health emergencies to fight insurgencies. Therefore, knowing, if, when, and why lockdowns mitigate nonstate actor violence is useful for designing better counterinsurgency operations.

I. Locked in Debate

Economic and logistical arguments offer competing arguments regarding the short- and long-term effects of state-imposed COVID-19 lockdowns.[1] Most economic arguments claim that the lockdowns were likely to have indirectly increased nonstate actor violence by decreasing the wealth of states (Mehrl and Thurner 2021; Marsai and Tarròsy 2022; Berman et al. 2022). Decreased wealth, according to these arguments, reduces the security resources of states (e.g., arms, equipment, and personnel) and the ability of states to buy off the opposition. They also assert that poverty, resulting from the lockdowns, incentivizes civilians to align themselves with armed groups out of a new-found sympathy for their anti-government rhetoric and a pragmatic need to make ends meet (Mehrl and Thurner 2021; Mercy Corps 2021; Marsai and Tarròsy 2022; Berman et al. 2022).

While most economic arguments claim that COVID-19 lockdowns were likely to have increased violence, not all make this claim. Berman et al. (2022) argue that

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[1]Nonstate actors also imposed lockdowns in territories they controlled but this was not common. They include: The Arakan Army in Myanmar, the Sudan People's Liberation Movement-North, the National Liberation Army in Colombia, and various gangs in Brazil. These lockdowns, as well as subnational lockdowns, are not the subject of this study.

lockdowns may have reduced violence because poorer states are less attractive prizes for nonstate actors. Brancati, Birnir and Idlbi (2023) argue, meanwhile, that the lockdowns may have decreased violence by reducing the wealth of nonstate actors. Gutiérrez-Romero (2022) argues, however, that decreased funds for nonstate actors likely increased violence, as financially-strained armed groups attacked civilians at higher rates to compensate for their financial losses. Regardless of the direction of their effect, economic arguments imply that the impact of lockdowns ought to have been long-term, lasting after lockdowns were lifted and until the economy recovered.

Logistical arguments claim, in contrast, that lockdowns were likely to have decreased violence directly because restrictions on population movements made it more difficult for nonstate actors to conduct attacks operationally (Brancati, Birnir and Idlbi 2023). These restrictions depopulate public areas and expose nonstate actors and their clandestine activities. For this reason, Brancati, Birnir and Idlbi (2023) hypothesized that lockdowns were likely to reduce violence more in densely populated areas, where nonstate actors rely heavily on civilians to screen their activities. Logistical arguments also claim that the effects of lockdowns should have lasted only in the short term, that is, when lockdowns were in place and population movements were disrupted. The authors' analysis confirmed their hypotheses but was limited to ISIS's behavior in Egypt, Iraq, and Syria.

II. Leveraging Lockdowns

Overall, the COVID-19 lockdowns were likely to have reduced nonstate actor violence when they were in place but not necessarily after they were lifted. When the lockdowns were in place and population movements were disrupted, the lockdowns

made it difficult for nonstate actors to leverage any reduction in the capacity of states to fight nonstate actors and any increased grievances against states. Only once the lockdowns were removed and population movements returned to pre-pandemic levels, were nonstate actors in a position to capitalize on any lingering negative economic effects of lockdowns for states.

To elaborate, the lockdowns were likely to have reduced the counterinsurgency capacity of states to some extent because the lockdowns substantially reduced state revenue worldwide (World Bank Group 2022). Revenue declined because the lockdowns reduced economic activity, increased unemployment, and decreased consumer spending. The effects of the lockdowns were not likely to have been immediate since most states had some financial resources in reserve. They were likely, though, to have occurred when the lockdowns were in place as long as the lockdowns were stringent and enduring. The effects were also likely to have endured after the lockdowns were lifted under these conditions because economic recoveries are typically gradual. Moreover, locals, once recruited, generally face significant barriers in leaving armed groups and may not desire to leave them once they have joined.

The lockdowns were not likely to have affected all states equally. The lockdowns had more adverse economic effects on developing countries than developed ones because they exacerbated pre-existing fragilities in the former (World Bank Group 2022). The lockdowns were more likely, therefore, to have affected the counterinsurgency capacities of the former than the latter. Together, the developed states that support the UN Trust Fund for Counter-Terrorism actually increased their pledges to the Trust annually between 2020-2022 (UNCCT 2022, 115). The Trust finances the

UN Office of Counter-Terrorism, which aids member states in building the capacity to combat terrorism and coordinating counterinsurgency operations, among other things. The United States, a major contributor to this fund, also increased its counterterrorism financing globally in this period (US Department of Homeland Security 2022). At the same time, states that were more adversely affected economically by the lockdowns were also less likely to have kept the lockdowns in place.

The lockdowns were also likely to have increased sympathy for nonstate actors where these actors were active. The lockdowns posed economic hardships for individuals and fostered grievances among the general public. These grievances ignited protests worldwide (Wood et al. 2022; Iacoella, Justino and Martorano 2024). They were likely to be especially pronounced in underdeveloped states where governments distributed less money to the public to compensate for the hardships people experienced as a result of the lockdowns. Nonstate actors were also widely reported to have attempted to exploit these grievances to recruit members and to increase support for their organizations more generally (Marone 2020).

However, the lockdowns themselves made it difficult for nonstate actors to capitalize on any reduction in the capacity of states to fight nonstate actors and any increased grievances against governments arising from them when they were in place. When the lockdowns were in place, they prevented nonstate actors from conducting their operations by exposing their activities, as logistical arguments claim (Fitton-Brown 2021, 5). These activities include planting improvised explosive devices, launching attacks, and even planning future attacks. The lockdowns also reportedly made it easier for security forces to identify nonstate actors, locate their smuggling

routes, and seize illegal goods. In India, the lockdowns allowed security forces to corner nonstate actors in their hideouts and confiscate their supplies (Sharma 2020). In Colombia, they helped security forces identify smuggling routes, seize cocaine shipments, and capture high-ranking members of criminal organizations (Guarin 2021, 68). Even where states lacked the funds to monitor and enforce the lockdowns, people widely complied with the lockdowns due to public health concerns, social norms, and so forth (Mills, Symons and Carter 2022; Shi et al. 2025).

The lockdowns also made it challenging for the nonstate actors to capitalize on any anti-government grievances that they provoked among the public to recruit support. Due to the lockdowns, nonstate actors moved online to recruit support (Europol 2021). According to retired US Army General, Stanley A. McChrystal, and other security officials interviewed, this left the nonstate actors slightly more exposed to detection.[2] Not even six months into the pandemic, the US government made its largest-ever seizure of terrorism-related cryptocurrency terrorism (Office of Public Affairs 2020). One of the schemes halted in this seizure involved the selling of fake K95 masks by ISIS operatives.

The lockdowns also made it difficult for nonstate actors to take advantage of any increased support they garnered online to launch attacks. With fewer people on the streets, people also had less intelligence about the location of government forces to share with nonstate actors. They were also less valuable to nonstate actors in terms of hiding information from the government about their whereabouts since nonstate actors were less active in the first place. To take advantage of these opportuni-

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[2]Personal Communication, November 8, 2021.

ties, nonstate actors would have had to wait until the lockdowns were removed and population movement returned to pre-pandemic levels.

III. Empirical Approach

To analyze statistically the effect of COVID-19 lockdowns on nonstate actor violence, I take two separate approaches. First, I use fixed effects (FE) Poisson regression. I use this approach to establish a correlation among lockdowns and violence, explore the short- and long-term effects of lockdowns, and examine the different pathways by which lockdowns may affect nonstate actor violence. I use Poisson models due to the count structure of the outcome data.[3] I use state-fixed effects to control for any unobserved, non-time-varying differences across states. Due to the state-fixed effects, states that did not experience any violent events are dropped from the models. Consequently, these models include at most sixty-two states.[4] This results in a cleaner comparison, with only states where nonstate actors were active compared to each other.[5] Lockdowns were unlikely to have affected nonstate actor violence outside of these states.

Using this approach, I first analyze the effect of lockdowns on nonstate actor violence at the state level in the short and long term. Formally,

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[3]I do not use FE negative binomial models, which are also appropriate for count data, because they do not control for all stable covariates (Allison and Waterman 2002).

[4]Afghanistan, Algeria, Angola, Austria, Bangladesh, Benin, Burkina Faso, Burundi, Cameroon, Central African Republic, Chad, Colombia, Democratic Republic of Congo, Côte d'Ivoire, Djibouti, Ecuador, Egypt, Ethiopia, France, The Gambia, Ghana, India, Indonesia, Iran, Iraq, Israel, Kenya, Lebanon, Libya, Malaysia, Mali, Mauritania, Morocco, Myanmar, Nepal, New Zealand, Niger, Nigeria, Pakistan, Papua New Guinea, Paraguay, Peru, Philippines, Russian Federation, Rwanda, Saudi Arabia, Senegal, Somalia, South Sudan, Sri Lanka, Sudan, Syrian Arab Republic, Tajikistan, Thailand, Togo, Tunisia, Turkey, Uganda, Ukraine, United Kingdom, Venezuela, and Yemen.

[5]In these states, at least one violent event occurred on one-quarter of the days in the analysis period. More than one violent event occurred on nearly two-thirds of these days.

$$y_{it} = \beta_0 + \beta_1 lockdown_{it} + \beta_2 lockdown(midterm)_{it} + \beta_3 lockdown(longterm)_{it} +$$
$$\beta_4 statecontrols_{it} + \alpha_i + \epsilon_{it},$$

where y_{it} is the number of violent events that occur on day t in state i and where α_i and ϵ_{it} represent the state-fixed effects and error term, respectively. The state-level controls include: state violence (total week, lag), deaths (week total, lag), entrance, and exit.

Finding that lockdowns reduced nonstate actor violence overall, I analyze if the reduction in violence was offset in the short and long term by the negative economic effects of lockdowns, and by which ones. To do this, I add variables for GDP (ln, lag) and unemployment (lag) to the above model. The economic indicators are lagged one-quarter so that they are not conflated with the outcome variable and because their effect on violence is unlikely to have been immediate.

$$y_{it} = \beta_0 + \beta_1 lockdown_{it} + \beta_2 lockdown(midterm)_{it} + \beta_3 lockdown(longterm)_{it} +$$
$$\beta_4 GDP_{it-1} + \beta_5 unemployment_{it-1} + \beta_6 statecontrols_{it} + \alpha_i + \epsilon_{it},$$

Prior to estimating this model, I examine the effect of lockdowns on the economic indicators and the effect of the economic indicators on nonstate actor violence (as above but without controlling for lockdowns). In the models where the economy is the outcome measured, lockdowns are lagged one-quarter so that their effect is not conflated with the economic indicators and because their effect is unlikely to have been immediate.[6] If GDP and unemployment offset the logistical effects of

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[6] I analyze these relationships using separate regression models (Baron and Kenny 1986) instead of newer approaches for causal mediation analysis (Imai, Keele and Tingley 2010) due to the sequencing of the effects. The latter approach requires that the predictors and mediators are measured in the same period in all equations. In the first model, I estimated the effect of lockdowns$_{(t-1)}$ on GDP$_{(t)}$

lockdowns, lockdowns should be associated with a decline in the economy (i.e., lower GDP and higher unemployment), the decline in the economy should be associated with an increase in nonstate actor violence, and the decline in violence associated with lockdowns should be larger when the economic indicators are controlled for in the model.

Subsequently, I investigate whether the overall reduction in violence was potentially due to logistical arguments. If lockdowns reduce violence because they make it more difficult for nonstate actors to operate, logistical arguments predict that lockdowns will reduce violence more in high-population-density districts. To test if this is the case, I move from the state level to the district level and examine variation within states across districts based on population density. I examine this variation by interacting population density with lockdowns and evaluating the significance of the interaction effects with Wald Tests. Rejecting the null hypothesis of a Wald Test (i.e., coefficients for the main effects and interaction terms are simultaneously equal to zero) indicates that an interaction effect is statistically significant. I also include district and economic controls in these models.

$$y_{idt} = \beta_0 + \beta_1 lockdown_{it} + \beta_2 lockdown * popdensity_{idt} + \beta_3 popdensity_{idt} + \\ \beta_2 GDP_{it-1} + \beta_2 unemployment_{it-1} + \beta_6 statecontrols_{it} + \beta_6 districtcontrols_{idt} + \alpha_i + \epsilon_{idt},$$

where y_{idt} is the number of violent events that occur on day t in district d in state i and where α_i represents the state-fixed effects and ϵ_{idt} represents the error term. The district-level controls include: population, size (km^2), state-controlled district,

and unemployment$_{(t)}$. In the second, I estimated the effect of GDP$_{(t-1)}$ and unemployment$_{(t-1)}$ on nonstate actor violence$_{(t)}$. And, in the third model, testing if the economy offset the effect of the lockdowns on nonstate actor violence, I estimated the effect of lockdowns$_{(t)}$ on nonstate actor violence$_{(t)}$, controlling for GDP$_{(t-1)}$ and unemployment$_{(t-1)}$.

and contested district.

To test the robustness of the findings from the FE Poisson regression, I replicated the models using two other estimation techniques. These models, like FE Poisson models, only establish a correlation among my covariates. The first is FE Poisson pseudo maximal likelihood (PPML) regression with state-fixed effects and robust standard errors. I used PPML to address concerns that any potential differences between this analysis and Berman et al. (2022) are driven by the technique. PPML is very similar but is more efficient than a standard Poisson model (Correia, Guimaraes and Zylkin 2020). It yields identical conclusions to the FE poisson model in this case. The second is zero-inflated Poisson (ZIP) regression with state-clustered standard errors. I use ZIP models to explore how lockdowns behave in all states in the world, including states that did not experience violence in the analysis period.

Using the second approach, I try to identify the causal effect of lockdowns on nonstate actor violence. The primary method I use for this purpose is instrumental variable (IV) regression. The IV regression is preferable over other techniques that I use because it produces consistent results in the case of unobserved heterogeneity, simultaneity, and dynamic endogeneity when instruments are valid. The instruments, described further below, appear to be valid. They are significantly related to lockdowns and are associated with high F-statistics. The instruments also pass tests of overidentifying restrictions, as well as placebo and balance tests.

The different sources of endogeneity that the IV regression addresses are potential concerns for this analysis. The initial decision of states to impose lockdowns is unlikely to be related to nonstate actor violence. States initially imposed lockdowns

due to public health concerns and the recommendation of the World Health Organization (WHO). Most states adopted lockdowns in close proximity of WHO declaring COVID-19 a pandemic. Within one month of this declaration, no fewer than 187 countries had mandated the closing of schools and/or non-essential businesses (OxCGRT 2023). WHO declared COVID-19 a pandemic to encourage states to adopt mitigation strategies (Ghebreyesus 2020). Its decision was unrelated to nonstate actor violence. At the time, four states accounted for 90% of COVID-19 cases, none of which experienced nonstate violence in the analysis period.

However, it is possible that the intensity of nonstate actor violence within states affected the decision of states to leave the lockdowns in place for longer or to lift them earlier. States were unlikely to have left the lockdowns in place for longer if they were effective in reducing nonstate actor violence. Were this the case, states would likely have only left restrictions in place within certain districts, but not the state as a whole. Trends in the data are consistent with this expectation.[7] It is more likely that states lifted the lockdowns earlier due to their negative economic effects. States with underdeveloped economies were more adversely affected by the lockdowns than states with developed economies and less likely to have maintained the lockdowns as a result. They were also more likely to experience nonstate actor violence.

I complement the IV regression with different methods of causal inference because even though the instruments pass all the relevant tests, there are no direct tests of the instruments' validity to fully eliminate concerns about their quality. I conduct

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[7]See footnote 18.

a subpopulation analysis that covers the early pandemic period (first 30, 60, and 90 days) for which exogeneity claims are strongest.[8] However, this period is relatively short and does not allow us to examine the effect of lockdowns after they have been lifted. I analyze this period using the FE Poisson regression described previously, as well as difference-in-difference (DiD) models and event study models (i.e., a type of DiD model) (Borusyak, Jaravel and Spiess 2024; de Chaisemartin et al. 2024).[9] DiD models, which other researchers used to study the effects of lockdowns on nonstate actor violence, do not make strict exogeneity assumptions. Instead, they identify the effect of lockdowns on violence by comparing differences in changes in violence between states that adopted lockdowns and states that did not adopt them or had not yet adopted them. However, they assume that lockdowns remain in effect for the entire analysis period once they have been adopted. For the early pandemic period, this is the case for most countries.[10] However, it is not true for the entire analysis period and the reason why I do not use them on the full population.

In addition to the subpopulation analysis, I analyze the full population using the

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[8]Exogeneity claims are strongest for this period since only 8 countries did not have lockdowns [50] in the first 30 days: Belarus (none); Burundi (violence); Chad (violence); Japan (none); Nicaragua (none); Taiwan (none); Tajikistan (violence); and Tanzania (none). Three of the eight experienced violence in the analysis period. Chad, Tajikistan, and Tanzania adopted them within 60 days. No additional country adopted them within 90 days. Very few countries lifted lockdown in these periods as well, see footnote 10.

[9]I do not use staggered difference-in-difference models because there are many units (i.e., days, weeks, and months) for which only a few states adopted lockdowns (Callaway and Sant'Anna 2021; Sun and Abraham 2021).

[10]The following countries lifted lockdowns before the end of the indicated period: 30-day period (i.e.. Turkmenistan); 60-day period (i.e., South Korea; Solomon Islands; Turkmenistan; and Zambia); 90-day period (i.e., Benin; Bulgaria; Burkina Faso; Cambodia; Czech Republic; Estonia; Finland; Iran; Jordan; Laos; Luxembourg; Malta; New Zealand; Nicaragua; Niger; Norway; Serbia; Slovak Republic; Slovenia; Solomon Islands; South Korea, Switzerland; Turkmenistan; Timor-Leste; Tunisia; and Zambia).

matrix completion (MC) method. The MC approach estimates the average treatment effect on the treated by imputing counterfactual outcomes for treated observations. Unlike difference-in-difference and event study models, it does not assume that lockdowns remain in place for the entire analysis period. However, it only allows for limited carryover effects (i.e., limited periods over which the effect of lockdowns can persist once they have been removed). While logistical arguments do not anticipate substantial carryover effects, economic arguments do. It also does not perform well when the number of states is small (Liu, Wang and Xu 2024). I discuss in greater detail the (dis)advantages of these techniques and why I do not use alternative methods in the online appendix. I also report in the appendix the full results of all models and tests discussed in the text.

IV. Data and Measures

The data used in the statistical analysis ranges between January 1, 2018 and December 31, 2022 in as many as 168 states.[11] The measures used in the analysis are as follows.

Non-State Actor Violence

To measure violence, I used the *Armed Conflict and Location Event Data (ACLED)* project used by all previous quantitative studies. It geolocates violent events daily in the world based on a range of sources to mitigate potential reporting biases.[12] Were

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[11] By including the pre-pandemic period in the analysis, I can test the effect of the pandemic apart from lockdowns. This, among other things, allows me to compare the effect of lockdowns after they were lifted to when there were no lockdowns in place before the pandemic.

[12] They include government sources, such as police and military forces, journalists, nongovernmental organizations, and civilians, including essential workers generally not subject to lockdowns (e.g., medical personnel).

it the case that the data all came from reports of news events, the number of violent events might have declined during lockdowns due to the inability of journalists to report on events or due to stories of COVID-19 crowding out stories about non-state actor violence.[13] The study includes all "violent events" initiated by nonstate actors (i.e., battles (51.42%), explosions/remote violence (27.70%), violence against civilians (20.84%), and violence against demonstration events (0.03%).[14] While I analyze all violent events jointly, I also analyze attacks against non-civilians and civilians separately. Analyzing them separately allows me to test if any decline in violence associated with lockdowns is due to fewer civilians being in public spaces and, hence, a decrease in potential civilian targets. It also allows me to test economic arguments claiming that lockdowns should be associated with an increase in attacks against civilians by nonstate actors seeking to recuperate their economic losses.

Using these data, I identified the number of violent events that occurred in states overall, as well as the number of violent events that occurred in districts within states, to test logistical arguments. Of the 1.43 million violent events in the analysis period, I determined that 101,472 events were initiated by nonstate actors and that 58,577 events were initiated by state actors. The initiator was unknown for 36,660 events. Previous economic studies did not distinguish between violence initiated *by* and *against* nonstate actors. This is important since lockdowns can have countervailing effects. An increase in violence by one set of actors may be offset by a decline in

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[13] We also do not see evidence of reporting biases in the data. If these biases were driving the results, we would not expect to observe in the analysis, as we do, the decline in the economy (i.e., decrease in GDP and increase in unemployment), which was associated with lockdowns, being significantly associated with an increase in nonstate actor violence.

[14] Nonstate actors are "rebel groups" and state actors are "state forces" in the ACLED dataset. (See codebook for definitions).

another, resulting in an overall null effect, as some studies have observed.

I identified the initiators and targets of attacks based on ACLED's event notes using human-assisted, natural language processing with Python and other tools. Nonstate actors were coded as the initiators if they were the subject (active voice) (e.g., "subject X attacked") or the agent (passive voice) (e.g., "attacked by agent X") of the action. If the verb did not indicate directionality and/or responsibility (e.g., "engaged with", "clashed with" or "exchanged fire"), the initiator was coded as unknown. The approach has high validity.[15]

Lockdowns

To measure lockdowns, I used the *Oxford Covid-19 Government Response Tracker* (2020-2023), which most previous analyses used. The Tracker measures eight individual state-issued lockdown policies.[16] The containment and closure policies are: school closings; workplace closings; public event cancellations; size limits on gatherings; public transportation closings; shelter-in-place orders; internal travel restrictions; and international travel restrictions.[17] The Tracker also provides a stringency index [0-100] based on these measures, which evaluates daily the overall restrictiveness of state-issued lockdown policies.

I analyze both the stringency index and the individual containment policies in

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[15] Eighty-seven percent of events that I coded and Brancati, Birnir and Idlbi (2023) human-coded as ISIS-initiated coincided for the relevant period.

[16] While the Tracker provides information on some subnational lockdowns, I used the information on national or state-issued lockdowns. The choice to impose or maintain these lockdowns is less likely to be affected by nonstate actor violence in particular geographic locales than subnational lockdown measures.

[17] Higher Tracker values are significantly correlated with greater declines in air pollution, indicating that the Tracker captures well reduced-population movements ($p \leq 0.01$).

this study. The primary measure that I use in the analysis is a dichotomous measure of the index (i.e., lockdowns [50]). It is coded 1 if the stringency index was ≥50 on a given day and 0 otherwise. Fifty is the midpoint of the index and the approximate mean of the index in the pandemic period. States with concurrent mandatory school and business closures and international travel restrictions had scores around 50. Dichotomizing the index, allows me to apply certain estimation techniques and test the long- and short-term effects of lockdowns. The results for the full stringency index, alternative cutpoints [40, 60] of the index, and the eight individual policy measures are described below and presented in the appendix. Where appropriate, the lockdown measures are lagged, as indicated below.

I define and measure the short-, mid-, and long-term effects of lockdowns as follows. The short-term effects are the effects when lockdowns are in place. Thus, lockdown [50] (shorterm) – or simply lockdown [50] – is coded 1 when lockdowns [50] were in place and 0 otherwise. The long-term effects are the effects after the last lockdown in a state is removed. Thus, lockdown [50] (longterm) – or simply postlockdown [50]) – is coded 1 when the last lockdown [50] was lifted in a state and 0 otherwise. The mid-term effects are the effects after the first lockdown was lifted and before the last lockdown was imposed (excluding days in which lockdowns were in place). Lockdown [50] (midterm) is coded 1 when a lockdown meeting this criterion was in place and 0 otherwise. This measure is not of particular interest here and is not discussed to simplify the discussion. The reference or omitted category in the statistical analyses is prelockdown [50]. It is coded 1 for the period prior to the first lockdown [50] in a state and 0 otherwise.

Figure 1 depicts the mean number of violent events initiated daily in states by nonstate and state actors given lockdowns (i.e., lockdowns [50]).[18] From this figure, three things are apparent: First, nonstate actors initiated more violent events than state actors. Second, the mean number of violent events initiated by nonstate and state actors was significantly lower when lockdowns were in place than when they were not (t-test, p≤0.01). The state effect is consistent with economic arguments claiming lockdowns undermine the ability of states to fight counterinsurgencies. Third, the means for state and nonstate actors were significantly lower for the post-lockdown than the prelockdown period, suggesting that lockdowns continued to have an effect after they were lifted (t-test, p≤0.01). Similar patterns are evident using other measures of lockdowns.

Lockdown Instruments

For the IV regression, I created two instruments as proxies for lockdowns: pandemic date and border deaths. Pandemic date is the number of days before or after WHO declared COVID-19 a pandemic on March 11, 2020. I expect this instrument to be strongly associated with states initially adopting state-level lockdowns for reasons previously described. I do not expect the date of the pandemic to be independently associated with a decline in violence (violating the monotonicity assumption) because very few nonstate actors pledged to, and even fewer did, put their weapons

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[18]Graphs of the number of nonstate actor violent events per day indicate that there was not a general decline in the number of violent events prior to lockdowns being imposed or a general rise in violence prior to the last lockdowns being lifted. The lack of an obvious trend suggests that the decision of states to impose or lift the lockdowns is unrelated to the level of violence in countries. If the lockdowns were imposed or lifted to reduce the violence, we might have seen an increase in violence right before these events.

Figure 1: Means Comparison (Lockdown, Pre- and Post-Lockdown)

(a) Nonstate Actors

(b) State Actors
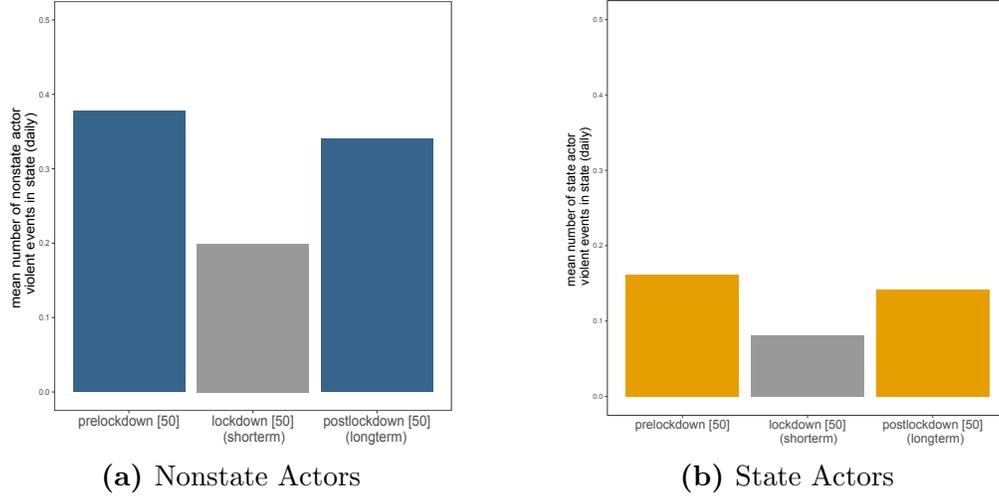
down in response to a call for a global ceasefire by the UN Secretary-General in the previous month. Since pandemic date may be a stronger proxy for lockdowns for different time periods, I repeated the analysis on different subsets of the data.

Border deaths is the total number of deaths in the previous week from COVID-19 in bordering states. It should be strongly associated with lockdowns since states were worried that COVID-19 would spread through travel. It is unlikely to have been significantly related to the intensity of nonstate actor violence in states otherwise. Were it the case that border deaths increased nonstate actor violence in other states, border deaths could have independently caused other states to have imposed state-level lockdowns in response to increased nonstate actor violence. There is no anecdotal evidence to this effect. Moreover, were this the case, states would likely have only imposed lockdowns in the affected districts and not at the state level – the level at which lockdowns are measured in this analysis. I calculated border deaths

using *GeoDataSource* to identify border states and *WHO COVID-19 Dashboard* to determine the death tolls.

Economic Factors

Unlike previous economic studies, which used lockdowns as proxies for the economy, I measured the economy based on real GDP (ln USD millions, 2014, quarterly) and unemployment (%, quarterly) (EMIS 2023). These indicators test economic arguments more precisely because they directly measure the economy per quarter at the state level. The time interval is important because lockdowns were lifted and reimposed in states within the same year. The data are the best available data, but they are only available for about half the countries in the dataset and one-third of those that experienced violence.[19]

The indicators also help unpack the mechanisms by which the economy may affect nonstate actor violence. GDP tests two prominent economic arguments. I can distinguish between them because one suggests GDP should decrease violence while the other suggests it should increase it. GDP tests arguments that lockdowns decrease nonstate actor violence because less wealthy states are less attractive prizes for nonstate actors (Collier and Hoeffler 1998; Berman et al. 2022). It also tests state capacity-based arguments, which claim that lockdowns increase nonstate actor violence by reducing the ability of states to finance counterinsurgency operations (Mehrl and Thurner 2021; Marsai and Tarròsy 2022; Berman et al. 2022). States with higher GDPs can purchase more arms and equipment, afford more security personnel, and buy off the opposition to a greater extent. Higher GDPs have been

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[19]It is also important to recognize that for these data, like any economic data, there may be measurement error, especially in authoritarian states.

associated statistically with reduced violence within states (Fearon and Laitin 2003).

GDP does not fully represent a state's ability to finance counterinsurgency operations, however, because states can receive financial support from abroad, among other reasons. It also does not represent all factors related to a state's capacity to conduct counterinsurgency operations, such as bureaucratic quality (Hendrix 2010). Measures for these aspects of state capacity do not vary within years. Direct measures of military capacity similarly do not vary within years and are not reliable and/or available for authoritarian states and nonstate actors. Accurate, reliable, and precise information about the financial resources of nonstate actors, especially small ones, does not exist either. Therefore, I cannot directly test hypotheses that lockdowns decrease nonstate actor violence by reducing their ability to finance operations (Brancati, Birnir and Idlbi 2023). However, I expect the negative economic effects of lockdowns to have been greater for nonstate actors than states due to the latter's ability to manipulate lockdowns to their advantage.

GDP can also be related to the economic well-being of the populace. Therefore, I measure the effect of lockdowns on individuals more precisely with unemployment. Unemployment allows me to test arguments that the economic harm caused by lockdowns leads individuals to sympathize, support, and join armed groups. Unemployment reduces people's incomes, which has been shown in some studies to affect people's affinity with armed groups in randomized experiments (Gilligan, Mvukiyehe and Samii 2013; Blattman and Annan 2016). Berman et al. (2011) argue that unemployment increases nonstate actor violence by leading armed groups to switch to less precise tactics that increase the unintentional killing of civilians, especially in

densely populated areas. A similar result in the case of lockdowns is unlikely since lockdowns reduce the number of civilians on the streets. I, nonetheless, test this hypothesis directly by examining the effect of unemployment on civilian casualties.

Logistical Factors

To test logistical arguments, I measured the population density of ACLED level-I districts.[20] Logistical arguments predict that lockdowns are likely to be associated with a greater reduction in nonstate actor violence in high-population-density districts because nonstate actors rely more heavily on the public in these districts as cover for their operations. With this measure, I can also test the predictions of Berman et al. (2011) that higher unemployment should be associated with higher civilian casualties in densely populated areas. I compiled population density data (1000 persons/km^2) using *GeoRef.net* and other official sources. I identified all districts not included in ACLED because they had not experienced violence using information from *GeoRef.net* and the *International Organization for Standardization.*

Using these resources, I also measured district population (in millions), district size (km^2), and state control of a district to account for other characteristics of districts. I expected more populated and larger districts to have had more violent events due to the higher number of potential targets in them. I expected state-issued lockdowns to have been less effective in districts where states had limited authority. I created separate indicator variables for full, partial, or no state control over districts if states established and/or enforced the laws, rules, regulations, codes, etc. in all, some, or no localities within a district, respectively. I constructed the state control

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[20]I also examined the distribution of the violence between districts that serve as nonstate actors' bases of operation and those that do not in the appendix. The findings support logistical arguments.

measures using a multitude of sources (e.g., governmental and non-governmental reports, news, and so forth).

Controls

To control for other aspects of the pandemic that may have affected nonstate actor violence, I measured COVID-19 deaths in states (total week, lagged) using the *WHO COVID-19 Dashboard*. Pandemics themselves, Koehnlein and Koren (2022) argue, can undermine state capacity. I also measured foreign military intervention. Exit is coded 1 every day after a foreign state removed its last military forces from another state and 0 otherwise. Entrance is coded 1 every day after a foreign state militarily invaded another state and 0 otherwise. To control for factors that varied across but not within states over time, such as regime type, I used state-fixed effects.

V. Results

To begin, I present the results of the analysis establishing the overall correlation between lockdowns and nonstate actor violence in the short and long term and the models exploring the mechanisms by which lockdowns may affect nonstate actor violence. I then describe the results of the analysis attempting to identify the effect of lockdowns on nonstate actor violence.

Overall Effect

Table 1 presents the results of the models establishing the overall relationship between lockdowns and violence at the state level. These models are estimated using FE Poisson regression with state-fixed effects. The unit of analysis is the state-day. In Model 1, I analyze the effect of lockdowns on nonstate actor violence in the short

and long term. As Figure 2a demonstrates, lockdowns were associated with fewer violent events initiated by nonstate actors in states relative to the prelockdown period in the short (17%) and long (6%) term.

The results are consistent with my argument that the logistical effects of lockdowns were likely to outweigh the negative economic effects of lockdowns on nonstate actor violence in the short term when lockdowns were in place and population movements were limited. I did not explicitly predict, nor did logistical arguments predict, that lockdowns would reduce violence once they were lifted. Once lockdowns were lifted, however, population movements did not immediately return to prepandemic levels, which may explain the effect. They did not due to continued worries regarding infections, vaccine shortages, unemployment, and inertia, among other factors (IMF 2020).

Table 1: Overall Effect

	Model 1	Model 2
dependent variable	NSA	SA
lockdown [50] (shortterm)†	-0.19**	-0.42**
	(0.01)	(0.02)
lockdown [50] (midterm)	0.03**	-0.39**
	(0.01)	(0.02)
lockdown [50] (longterm)†	-0.06**	-0.29**
	(0.01)	(0.02)
state violence (total week, lag)	9.12e-05**	
	(2.17e-06)	
deaths (week total, lag)	-4.88e-05**	-0.0001**
	(6.48e-06)	(0.00001)
entrance	-1.54**	1.01**
	(0.05)	(0.03)
exit	-1.71**	-2.73**
	(0.03)	(0.06)
log likelihood	-74827.99	-41513.69
observations	112,556	81,720
states	62	45
Wald Test (Chi2)†	107.91	36.73
prob>chi2	0.00	0.00

Note: *p≤0.05 and **p≤0.01. †Null hypothesis is that coefficients are equal. NSA=number of violent events initiated by nonstate actors; SA=number of violent events initiated by state actors.

In Model 2, I analyze the effect of lockdowns on state violence in the short and

long term. According to it, lockdowns also reduced attacks against nonstate actors by state actors. Figure 2b illustrates this model. According to it, lockdowns were associated with fewer attacks by state forces against nonstate actors in the short (34%) and long (25%) term relative to the prelockdown period. These results are consistent with economic arguments claiming that lockdowns weaken states' counterinsurgency capacities.

Figure 2: Short- and Long-term Effects of Lockdowns

(a) Nonstate Actors (Model 1)

(b) State Actors (Model 2)
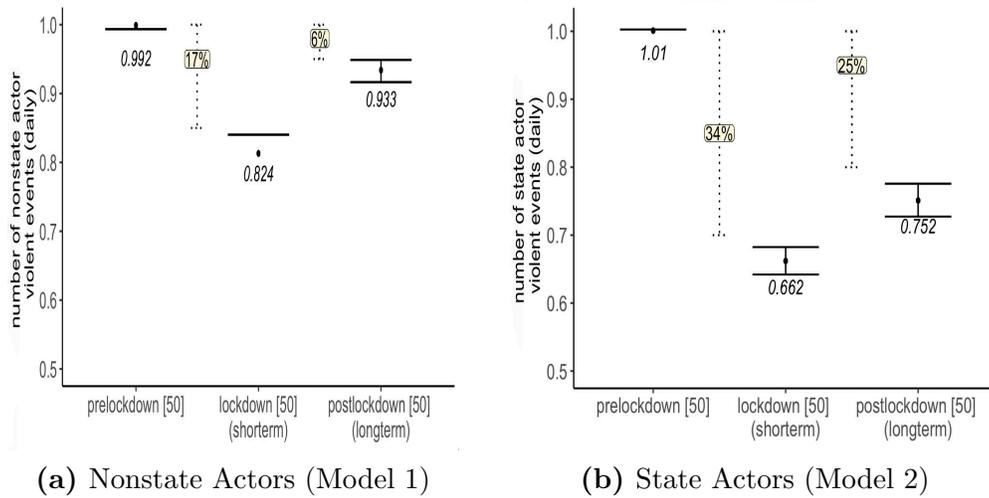
Note: Figure depicts the predicted number of violent events initiated daily by nonstate actors at the state level where the fixed effect is zero and controls are set at their means. Solid bars represent 95% confidence intervals. For each lockdown measure, the value represents the predicted number of violent events when the measure equals one and all other lockdown measures equal zero. The predicted value for prelockdown [50] (omitted category) is calculated by setting all lockdown measures in the model to zero. The predicted value for lockdown [50] (midterm) is not depicted in the figure.

Note: Figure depicts the predicted number of violent events initiated daily by state actors at the state level where the fixed effect is zero and controls are set at their means. For each lockdown measure, the value represents the predicted number of violent events when the measure equals one and all other lockdown measures equal zero. The predicted value for prelockdown [50] (omitted category) is calculated by setting all lockdown measures in the model to zero. The predicted value for lockdown [50] (midterm) is omitted from the figure. Solid bars represent 95% confidence intervals.

To test the robustness of these findings, I examined alternative lockdown measures and time periods, included additional controls in the models, and analyzed the data

using alternative estimation techniques. The short-term effects are robust to most alternative lockdown measures (i.e., stringency index, alternative index cutpoints, and eight policy measures), and to models analyzing different time periods (i.e., 2020-2023), using additional controls (i.e., month and year), and applying different estimation techniques (i.e., PPML and ZIP). The long-term effects are robust to most of these conditions as well.

It is worth noting that the overall decline in violence is not simply a result of a reduction in the number of civilians in public. In other models, I analyze separately attacks against non-civilians and civilians. In the non-civilian models, I find that lockdowns significantly reduced attacks against non-civilians in the short and long term. These effects are also robust across alternative lockdown measures and estimation techniques. In the civilian models, I find mixed results in terms of the direction and significance of the effect of lockdowns depending on the estimation technique. The results, therefore, do not support economic arguments claiming that nonstate actors increased attacks on civilians globally to compensate for their financial losses.

Economic Pathway

Having found that lockdowns reduce nonstate actor violence overall, I analyze if the overall reduction in violence was offset by the negative economic effects of lockdowns on states and by which ones. I first examine the effects of lockdowns on GDP and unemployment. Economic arguments predict lockdowns would decrease GDP and increase unemployment. Figure 3 demonstrates the results of these models. The models are estimated using ordinary least squares regression (OLS) with state-fixed effects. The unit of analysis is the state-day. Lockdowns are lagged by one

Figure 3: Economic Effects of Lockdowns

**(a)** GDP (Model 3)   **(b)** Unemployment (Model 4)
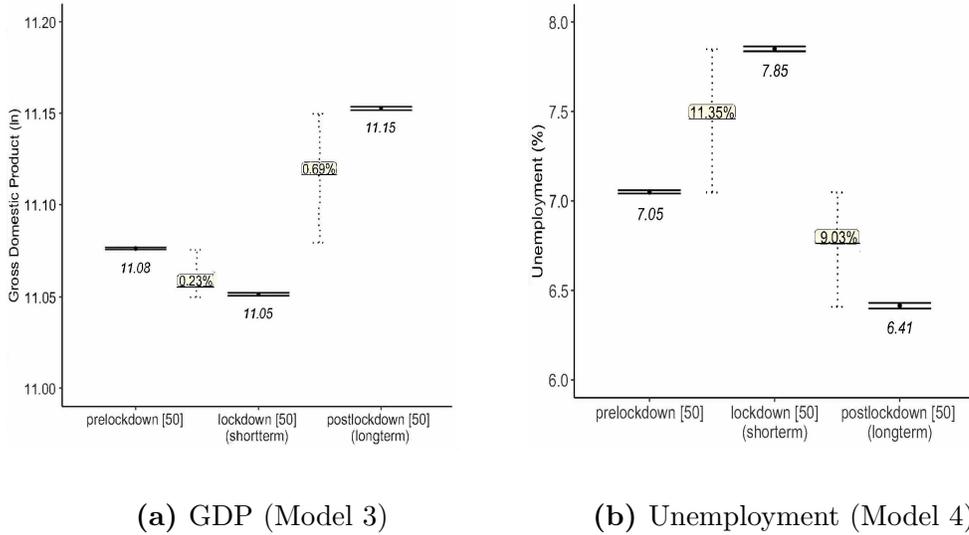
Note: Figures predict either GDP or unemployment (quarterly) at the state level based on models estimated using ordinary least squares regression with state-fixed effects. The models include lagged indicators (one quarter) for the short-, mid- and long-term effects of lockdowns [50] and a control for COVID-19 deaths (total week, lag). Prelockdown [50] is the reference category omitted from the model. The predicted values for the three lockdown indicators are calculated by setting the given indicator equal to one (all other lockdown indicators to zero) and the control for deaths to its mean. The value for prelockdown was calculated by setting the values for all three lockdown measures equal to zero. The results for lockdowns [50] (midterm) are not depicted. Bars represent 95% confidence intervals. N=139,727 (Model 3); N=127,309 (Model 4).

quarter in these models since their economic effects should not be immediate.

The results indicate that lockdowns were significantly associated with lower GDPs (0.23%) and higher unemployment (11.35%) in the short term relative to the pre-lockdown period, as economic arguments predict. They were associated with the converse in the long term. After the last lockdowns were lifted in states, GDP was higher and unemployment was lower than in the prelockdown period. This may be because the expansion in economic activity in the recovery was stronger and faster than initial projections (IMF 2021). The results for both GDP and unemployment are robust to alternative lockdown measures.

Figure 4: Economy's Relationship to Nonstate Actor Violence

(a) GDP

(b) Unemployment
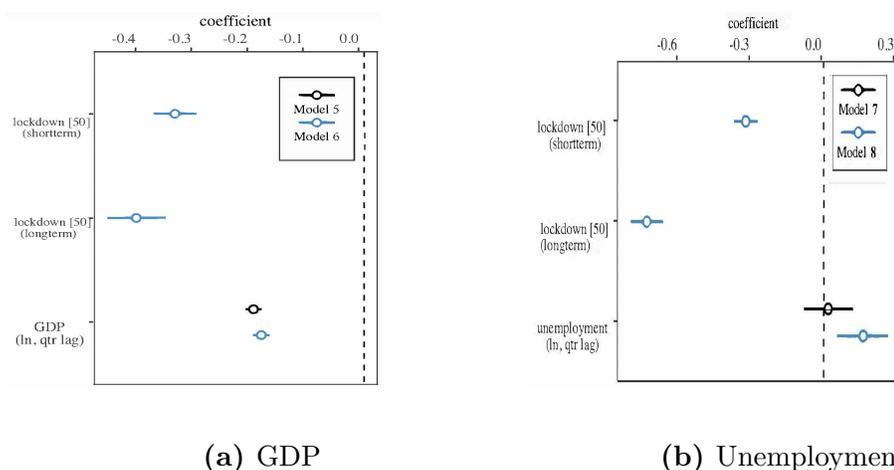
Note: Figures depict the coefficients of models predicting the number of nonstate actor violent events initiated daily at the state level using FE Poisson regression with state-fixed effects. The models include indicators for the short-, mid-, and long-term effects of lockdowns [50], as well as controls for state violence (total week, lag), and COVID-19 deaths (week total, lag). The reference category omitted from the models is prelockdown [50]. The coefficients for lockdowns [50] (midterm) and the controls are not shown. Solid bars represent 95% confidence intervals. GDP is divided by 10 and unemployment is multiplied by 10 for presentation purposes. Bars represent 95% confidence intervals.

Having established that lockdowns negatively affected the economy, I then test if GDP and unemployment increased nonstate actor violence with and without controlling for lockdowns. Figure 4a and Figure 4b present the results for GDP and unemployment, respectively. These models are estimated using FE Poisson regression with state-fixed effects. The unit of analysis is the state-day. There are no more than twenty-four states included in these models due to the limited coverage of the economic data.[21]

As the models in this figure demonstrate, lower GDP and higher unemployment

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[21]The GDP models include: Austria, Colombia, Ecuador, Egypt, France, Ghana, India, Indonesia, Iran, Israel, Kenya, Malaysia, New Zealand, Nigeria, Paraguay, Peru, Philippines, Russian Federation, Saudi Arabia, Sri Lanka, Thailand, Turkey, Ukraine, and the United Kingdom. The unemployment models do not include: France, Ghana, India, Indonesia, Kenya, and Peru, but include Morocco, Tajikistan, Tunisia, and Venezuela.

are associated with an increase in nonstate actor violence. However, only the effect of GDP is significant in both models (i.e., the models with and without measures for lockdowns). GDP is robust across additional model specifications, including additional controls (i.e., year and month) and estimation techniques (i.e., PPML with state-fixed effects and standard errors clustered by state-quarter and ZIP models with standard errors clustered by state-quarter). Unemployment is not.

The robust GDP effect and weak unemployment effect favor state capacity-based arguments, claiming that lockdowns reduce the ability of states to finance counterinsurgency operations by decreasing state wealth. The significant decline in attacks by state forces under lockdowns, as demonstrated by Model 2, further supports state capacity-based arguments. The results do not support economic arguments that decreased state wealth reduces nonstate actor violence by making states a less attractive prize for nonstate actors since lower GDP is associated with increased violence. The results also do not support economic arguments regarding nonstate actors unintentionally killing more civilians by switching tactics due to unemployment. In separate models presented in the appendix, unemployment is not significantly associated with increased attacks on civilians by nonstate actors. We cannot rule out the possibility, however, that the insignificant effect of unemployment results from limitations in the data.

Lockdowns, according to the models in Figure 4, are significantly associated with less nonstate actor violence relative to the prelockdown period in the short and long term. In these particular models, the long-term effect is significantly greater than the short-term effect. This result is likely specific to the smaller subset of states included

in the economic models.[22] The fact that lockdowns weaken the economy and that a weakened economy is associated with an increase in nonstate actor violence, suggests that the baseline analysis presented in the previous section, which covers a larger number of states but does not control for the economy, underestimates the direct effect of lockdowns on nonstate actor violence.

Logistical Pathway

Finally, having found that lockdowns were associated with a reduction in violence overall in the short and long term, I investigate whether this reduction in violence was potentially due to lockdowns making it more difficult for nonstate actors to operate. I do this by moving from the state level to the district level and examining if the intensity of nonstate actor violence is different in low- and high-population density districts. Table 2 presents the results of these models. The models are estimated using FE Poisson regression with state-fixed effects. The unit of analysis is the state-district-day. The models include state- and district-level controls.

In Model 9, I examine the effect of lockdowns and population density on nonstate actor violence. The interaction effect between lockdowns and population density in this model is significant, according to a Wald Test. The results of this model are depicted in Figure 5. As Figure 5 demonstrates, lockdowns are associated with a greater reduction in nonstate actor violence in high-population density districts. The analysis, thus, confirms the findings of previous logistical studies on a much

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[22]This conclusion is based on the fact that if I analyze the effect of lockdowns on nonstate actor violence on the same population of states included in the economic models without controlling for GDP and unemployment, lockdowns have a larger effect in the long term than the short term in these models as well.

Table 2: Lockdown District Interactions

| dependent variable	Model 9 NSA	Model 10 NSA (non-civilians)
lockdown [50]†	-0.19**	-0.25**
	(0.01)	(0.01)
lockdown [50]*	-0.06**	-0.04**
population density†	(0.01)	(0.01)
population density†	-0.05**	-0.06**
	(0.003)	(0.003)
state violence (total week, lag)	0.001**	0.001**
	(8.66e-06)	(8.86e-06)
deaths (week total, lag)	-3.90e-05**	-4.92e-05**
	(6.07e-06)	(7.89e-06)
population	0.004**	0.004**
	(0.0004)	(0.0005)
size (km^2)	-1.75e-06**	-1.89e-06**
	(7.49e-08)	(9.33e-08)
entrance	-1.93**	-2.40**
	(0.05)	(0.06)
exit	-1.49**	-1.59**
	(0.03)	(0.03)
state-controlled district	1.88**	1.89**
	(0.01)	(0.02)
contested district	-0.82**	-0.86**
	(0.01)	(0.01)
log likelihood	-258167.34	-208794.67
observations	2,510,892	2,375,217
states	62	57
Wald Test (Chi2)†	865.44	976.23
prob>chi2	0.00	0.00

Note: *p≤0.05 and **p≤0.01. †Null hypothesis is that coefficients
are equal. NSA=number of nonstate actor violent events.

larger number of cases.[23]

In Model 10, I examine the effect of population density on violence against non-civilians who were not subject to the lockdowns. The interaction effect remains significant in these models. The results indicate that the effect for population density is not due simply to a greater decline in civilians in public places in high-population density districts. The results for the interaction effect between population density and lockdowns are robust to alternative measures of lockdowns (i.e., stringency index and cutpoints), additional controls (i.e., year and month), and alternative estimation techniques (i.e., PPML with state-fixed effects and district-clustered standard errors; ZIP models with district-clustered standard errors).

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[23]The violent events for the three countries in previous logistical studies (i.e., Egypt, Iraq and Syria) constitute less than one-fifth of the violent events in this analysis.
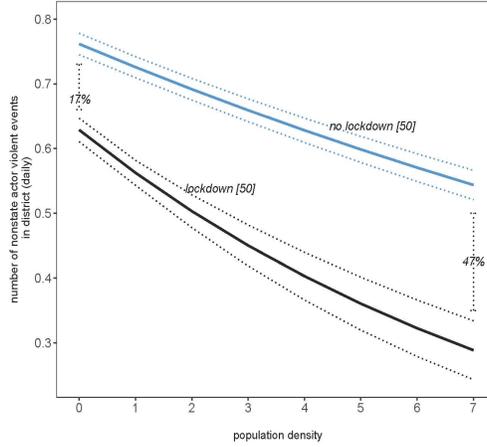
Nonstate Actors (Model 9)

Note: Figure depicts the predicted number of nonstate actor violent events initiated daily at the district level based on a FE Poisson model with state-fixed effects. The model depicts the values for (no) lockdowns for varying levels of population density. In this figure, the fixed effect is zero and controls are set at their means. Dotted lines represent 95% confidence intervals. N=2,510,892.

Identification

Having found that lockdowns are significantly associated with an overall reduction in nonstate actor violence despite their negative effects on the economy, I now use fixed-effects IV regression, as well as other techniques of causal inference, to try to identify the effects of lockdowns on nonstate actor violence at the state level. Table 3 presents the results of the IV regression. The IV models presented in this table include state-fixed effects and state-level controls. Various tests presented in the table indicate that the instruments used in these models are strong instruments. The first-stage regression models indicate that the instruments are significantly related to lockdowns. The F-statistics are high (>100).[24] The Sargan-Hansen tests indicate

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[24]The null hypothesis of the F-statistic is that the instruments are not correlated with the endogenous explanatory variable. Rejecting it, indicates that they are related to it.

that the null hypothesis that the instruments are uncorrelated with the error term cannot be rejected. Placebo tests and balance tests provided in the appendix likewise suggest that the instruments are valid.[25],[26]

Model 11 analyzes the overall effect of lockdowns on non-state actor violence. It is based on the same population of 62 countries as Model 1. According to it, lockdowns significantly reduced nonstate actor violence by about 11% relative to the prelockdown period holding all other variables in the model constant at their means.[27]

In Model 12, I add controls for GDP and unemployment. The number of states included in the analysis is much smaller as a result. The results also indicate that lockdowns reduce nonstate actor violence. The results are the same whether GDP and unemployment are included in the same or in separate models. In the appendix, I repeat this analysis with alternative measures of lockdowns, additional controls, and time periods (i.e., 2020-2023) since the quality of the instruments may vary over

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[25]For the placebo tests, I identified two pretreatment variables/placebos and regressed them on the instruments. According to this test, if the instrument(s) are not statistically related to the placebos, this is indirect evidence that the instrument(s) are valid. My two pretreatment variables/placebos are indicator variables for Asia and Asia/Oceania. They are appropriate pretreatment variables/placebos because these regions were the epicenter of the COVID-19 pandemic with the virus originating in China. The instrument(s) are not statistically related to the pretreatment variables/placebos.

[26]For the balance tests, I examine the absolute standardized difference across the model covariates for the lockdowns and the instruments. According to this test, if an instrument behaves in an as-if random fashion, the sample means of the baseline covariates should not differ significantly across levels of the instrument. Based on a conservative standard of an absolute standardized difference of 0.10 or more, all of the model covariates are balanced for one or more of the instruments. Specifically, all of the covariates are balanced for pandemic distance and all but one of the covariates are balanced for border deaths.

[27]Residual-vs-fitted value plots indicate that the heteroskedasticity in these models is modest. Transforming the outcome variable through either a natural-log transformation or a square-root transformation does not improve the fit.

Table 3: Instrumental Variable Regression

	Model 11	Model 12
Second-stage Results		
dependent variable	NSA	NSA
lockdown [50]	-0.10**	-3.49e-01**
	(0.03)	(0.04)
state violence (total week, lag)	1.45e-03**	8.01e-04**
	(1.41e-05)	(1.34e-05)
deaths (week total, lag)	-9.51e-06	2.74e-5
	(7.71e-06)	(9.90e-06)
entrance	-2.56**	
	(0.07)	
exit	-9.08**	
	(0.09)	
GDP (ln, lag)		-2.49**
		(0.09)
unemployment (lag)		-3.51e-03
		(5.54e-3)
constant	0.84**	29.14**
	(9.11e-03)	(1.09)
observations	112,556	30,311
states	62	18
Sargan-Hansen Statistic	0.53	1.77
First-stage Results		
dependent variable	lockdown [50]	lockdown [50]
pandemic distance	-4.34e-04**	-4.25e-04**
	(3.92e-06)	(8.15e-06)
border deaths	5.6e-05**	4.06e-05**
	(5.39e-07)	(1.03e-06)
constant	0.38**	9.08**
	(2.16e-03)	(0.36)
controls	-	-
F-Test	178.08**	266.27**
Reduced Form Results		
dependent variable	NSA	NSA
border deaths	-1.96e-05**	-1.96e-05**
	(2.94e-06)	(2.94e-06)
pandemic distance	-2.35e-04**	-2.35e-04**
	(2.21e-05)	(2.21e-05)
constant	1.02**	1.02**
	(0.01)	(0.01)

Note: *p≤0.05 and **p≤0.01. Model 11 controls denoted as (-) are: state violence (total week, lag), deaths (week total, lag), entrance, and exit. Model 12 controls denoted as (-) are: state violence (total week, lag), deaths (week total, lag), GDP (ln, lag) and unemployment (lag). Entrance and exit were dropped from this model due to collinearity.

time. In these models, lockdowns consistently, significantly reduce nonstate actor violence.

The results of alternative identification strategies support these results. My subpopulation analysis of the early days of the pandemic indicates that lockdowns (i.e., lockdowns [50]) were associated with a significant reduction in violence relative to the prelockdown period in the first 30, 60, and 90 days of the pandemic despite the small time period. I reach the same conclusions whether I analyze the subpopulation data with FE Poisson regression, difference-in-difference models, or event study models. Lockdowns were also associated with less nonstate actor violence using the MC approach when including state-level and district-level controls.[28]

Conclusion

Understanding the effect of lockdowns on nonstate actor violence remains an important question not only for the historical record, but also for policies surrounding future public health policies and counterinsurgency strategies. The previous analysis advances our understanding of it in several ways. In contrast to prior global studies, the analysis finds that COVID-19 lockdowns were significantly associated with a reduction in nonstate actor violence both in the short and long term. It finds support for two competing sets of hypotheses about the economic and logistical effects of lockdowns on nonstate actor violence, using more precise data in the case of the former and a larger number of countries in the case of the latter. The analysis also provides evidence that the effect of lockdowns on nonstate actor violence is causal,

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[28]There were too few observations to include economic controls in the subpopulation analysis and the MC models.

although causality is very difficult to establish definitively in observational studies.

The analysis further suggests that logistical factors outweigh the negative economic effects of lockdowns on states given the overall reduction in violence in the short and long term. While we cannot test logistical arguments directly, the analysis finds evidence consistent with them, namely that lockdowns are associated with a greater reduction in violence in densely populated areas. In parsing out the different pathways by which the economy may affect nonstate actor violence, the analysis finds the strongest support for economic arguments claiming that lockdowns reduce the capacity of states to fight insurgents by decreasing their wealth.

Interestingly, the reduction in violence endured after the last lockdowns were lifted. It may have lasted after lockdowns were lifted because people did not fully resume their past activities once the lockdowns were lifted due to continued worries about infections, a lack of vaccines, and inertia, among other reasons. The negative effect of lockdowns on the economy, in contrast, did not endure after the lockdowns were lifted. This may be because the expansion in economic activity in the recovery was stronger and faster than initial projections. Thus, unlike many other effects of lockdowns related to the pandemic, in the case of nonstate actor violence, the evidence heavily favors a net positive effect.

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