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The heterogeneous effects of COVID-19 lockdowns on crime across the world

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Abstract

There is a vast literature evaluating the empirical association between stay-at-home policies and crime during the COVID-19 pandemic. However, these academic efforts have primarily focused on the effects within specific cities or regions rather than adopting a cross-national comparative approach. Moreover, this body of literature not only generally lacks causal estimates but also has overlooked possible heterogeneities across different levels of stringency in mobility restrictions. This paper exploits the spatial and temporal variation of government responses to the pandemic in 45 cities across five continents to identify the causal impact of strict lockdown policies on the number of offenses reported to local police. We find that cities that implemented strict lockdowns experienced larger declines in some crime types (robbery, burglary, vehicle theft) but not others (assault, theft, homicide). This decline in crime rates attributed to more stringent policy responses represents only a small proportion of the effects documented in the literature.

Keywords COVID-19, Cross-national, Strict lockdown, Crime, Synthetic control

Introduction

The COVID-19 pandemic involved an unprecedented change in social dynamics worldwide. The rapid diffusion of the virus and its health costs led governments to deploy policies to restrict mobility, reduce the diffusion of the virus, and avoid the collapse of health systems. The implementation of these mobility restrictions, coupled with voluntary work-from-home policies implemented by organizations and voluntary social distancing measures implemented by households, resulted in an

unparalleled global reduction in human mobility, marking an extraordinarily quiet period (Lecocq et al., 2020). One of the key questions regarding the effects of this ‘global natural experiment’ has been its impact on crime (Boman & Mowen, 2021).

Mirroring research on crime and catastrophic events such as earthquakes (García Hombrados, 2020), studies at the intersection of COVID-19 and crime have centered on cities as the unit of analysis. These studies have employed a wide range of research designs, including interrupted time series, differences-in-differences, regression discontinuity, structural break analysis, and event studies (Koppel et al., 2023), documenting inconsistent effects across different offenses. For example, research in the US, Canada, the UK, Australia, Mexico,

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India, New Zealand, Sweden, and China consistently documented a significant decrease in the reported incidents of property crimes, such as robbery, theft, or burglary (Abrams, 2021; Andresen & Hodgkinson, 2020; Ashby, 2020; Balmori de la Miyar et al., 2020; Chen et al., 2023; Cheung & Gunby, 2021; Felson et al., 2020; Gerrell et al., 2020; Halford et al., 2020; Koppel et al., 2023; Langton et al., 2021; Payne et al., 2021; Poblete-Cazenave, 2020; Vilalta et al., 2023). On the other hand, other property crimes, such as vehicle theft, showed more varied outcomes. Reductions were reported for Australia, the US, China and UK (Andresen & Hodgkinson, 2020; Chen et al., 2023; Halford et al., 2020; Koppel et al., 2023; Mohler et al., 2020), while other studies documented no effects or even an increase for the US and Canada (Ashby, 2020; Hodgkinson & Andresen, 2020; Meyer et al., 2022). The relationship between the pandemic restrictions and violent crimes is less clear. While some studies focusing on cities in the US, UK, Australia, Sweden, India, Mexico, and Peru show significant reductions in reported assaults or homicides (Abrams, 2021; Calderon-Anyosa & Kaufman, 2021; Gerrell et al., 2020; Halford et al., 2020; Payne et al., 2021; Poblete-Cazenave, 2020; Vilalta et al., 2023), other studies in Australia, the US, and Mexico show no significant effects (Balmori de la Miyar et al., 2020; Campidelli et al., 2020a; Koppel et al., 2023; Lopez & Rosenfeld, 2021; Meyer et al., 2022; Payne et al., 2020).

Since most of this evidence is based on single-city studies or, at best, studies that compare different cities within one country (e.g., Abrams, 2021; Ashby, 2020; Meyer et al., 2022) or a few countries (Cecatto et al., 2021), inconsistency across studies is not surprising given the different characteristics of cities, type of stay-at-home government restrictions, voluntary measures implemented by organizations and households, and period of analysis. The only study with broad international coverage is Nivette et al. (2021a), which included 23 cities across the Americas, Europe, the Middle East, and Asia and documented an average reduction of 37% in reported offenses following stay-at-home government restrictions. Although property crimes (i.e., theft, vehicle theft, burglary, and robbery) exhibited a significant reduction, violent crimes showed a mixed picture with a reduction of assaults, while homicides showed no effects. In addition, Nivette et al. (2021a) documented heterogeneity in crime reduction across locations, with the largest crime drops in cities that applied stricter lockdowns. These results are consistent with a recent systematic review, which indicated that most crimes exhibited a significant reduction following COVID-19 restrictions, except for homicides (Hoeboer et al., 2024).

Despite the accumulated empirical evidence on the relationship between the COVID-19 pandemic and crime

across different types, one of the limitations of this literature is the focus on the generic role of the pandemic and associated measures. Considering the widespread impact of the pandemic, most studies compare current trends with a counterfactual scenario representing the expected crime rate based on pre-pandemic periods (Abrams, 2021; Hodgkinson & Andresen, 2020; Nivette et al., 2021a). Utilizing cities' pre-pandemic trends as control groups implies the comparison of cities affected by the pandemic with those not impacted by the pandemic. This entails assessing the effects of a general 'treatment,' encompassing various and distinct government restrictions and stay-at-home policies associated with the pandemic and considering individual and corporate responses, which may be partially independent of government policies. Exceptionally, some studies have exploited heterogeneities across neighborhoods to identify a potential causal effect on crime at a local level. For example, when initial restrictions were relaxed in Bihar (India), crime rates increased. Still, the rise was less pronounced in areas with more stringent restrictions compared to those with less severe measures, and notably, this uptick did not extend to violent crimes (Poblete-Cazenave, 2020). Instead, evidence from London (UK) shows that easing national lockdown measures diminished the effect of stringent lockdowns across all property and violent crimes (Neanidis & Rana, 2023). Evidence from Oslo (Norway) suggests that when general COVID-19 restrictions were accompanied by the closure of bars and pubs, there were additional significant reductions in theft, violent crimes, vandalism, and fraud (Gerrell et al., 2022). However, the scarcity of integrated international data sets (Boman & Mowen, 2021; Nivette, 2021) means we lack causal estimates of the impact of specific types of government policies on crime at a global level.

Consequently, in this paper, we investigate how different policies implemented by local governments during the COVID-19 pandemic affected crime across cities. The goal is to identify the causal impact of strict lockdown policies on the number of offenses reported to local police. More specifically, our main research question is: *What is the cross-national impact on crime of strict lockdown policies that require citizens not to leave their homes?* To answer this question, we first analyze the spatial and temporal variation of government responses to the pandemic in 45 cities throughout the year 2020. We define strict lockdown conditions as instances where governments require citizens not to leave the house with no or minimal exceptions. Next, we apply a generalized synthetic control approach, which involves building a weighted combination of control groups of all the cities in those periods without strict lockdown. Thus, only cities that implemented 'strict lockdowns' are considered

'treated,' some of them intermittently, some of them permanently during the period of study. This empirical strategy allows us to isolate the effect of strict lockdowns on crime by accounting for the effects of other stay-at-home policies in cities with less stringent restrictions. We conclude by discussing the policy implications of our results for implementing crime prevention policies that may compromise individuals' freedom of movement.

Data and methods

The outcome variable

The outcome variable is the number of crime incidents reported to police each month in each of the 45 cities in our sample between January 2018 and December 2020 for six crime types: assault, theft, burglary, robbery, vehicle theft, and homicide.¹ The cities were selected to maximize geographical coverage. The integration, aggregation, and comparison of crimes across these units were based on the International Classification of Crime for Statistical Purposes (Bisogno et al., 2015). In some cities, the six crime categories were not available or did not fit our classification. For example, vehicle theft was not considered a separate category from theft in some cities (e.g., Zurich), and burglary is not clearly distinguished from another type of property crime in other cases (e.g., Montevideo). In other cities, crime counts were available but not for the relevant period of study (e.g., Tel Aviv). Additionally, some cities lacked enough monthly cases for some crimes, typically homicide (e.g., Ljubljana). As a result, some cities could not be included in some of the analyses (see Table A2 of the Supplementary Materials). Finally, due to the surge in violence following the George Floyd incident on May 25, 2020, our main results are based on data before May 2020 for the 15 US cities in our study. However, we report the results using all US data in section E of the Supplementary Materials.

The date of the time series refers to the date when the offense presumably occurred, as recorded by the police. In cases where this information was not available, the reporting date was used (e.g., Mexico City). Since not all reported crimes were investigated, the number may under-represent the volume of crime reported to the police. Additionally, for most cities, the time series starts on 1 January 2018 or 2019 and ends on 31 December 2020. Time series information and available crime categories for each city are presented in Table A2.

To compare the changes in crime trends following the onset of the COVID-19 pandemic and the implementation of stay-at-home restrictions, we calculated monthly

indexes for each city and crime type. The indexes were constructed such that the 2019 average equals 100 for each time series. Figure 1 shows the indexes for all cities in a single graph, with the average trend for each crime type highlighted in orange. We used monthly time series to minimize suppression since, in some cities, the frequency of crimes per day was almost zero. This occurred most often for homicide.

Event study design

For each crime, we have an outcome matrix with typical element y_{it} with 45 columns corresponding to cities ($N=45$) and 36 rows corresponding to the months from January 2018 to December 2020 ($T=36$). We first estimated the average effect of the COVID-19 pandemic on crime incidents using a two-way fixed effects (static) regression given by:

$$y_{it} = \delta D_{it}^1 + \beta' w_{it} + \alpha_i + \theta_t + \gamma_t + \varepsilon_{it}, \quad (1)$$

where y_{it} is the monthly index of crime incidents for a given type of crime in city i on month t . The (first) treatment variable D_{it}^1 is a dummy variable that takes the value of 1 during the period starting in March 2020 and 0 in the period preceding it. In this case, the treatment variable captures all stay-at-home restrictions implemented by governments, voluntary work-from-home policies implemented by employers, voluntary social distancing implemented by individuals, and any other change in behavior that can be attributed to the COVID-19 pandemic. In this specification, the coefficient δ represents the average percentage change in crime incidents (relative to the 2019 average) after the onset of the COVID-19 pandemic in March 2020.

For certain crimes, patterns have been found to increase periodically. As a result, we used weather covariates and fixed effects to control for seasonal and long-term trends. The vector w_{it} includes weather covariates (monthly average rain and temperature for each city), α_i denotes city fixed effects to control for factors that vary across cities but not across time, θ_t denotes month fixed effects to account for monthly seasonality, and γ_t denotes year fixed effects to account for time trends in the data.

Next, we estimated the average effect of the COVID-19 pandemic on crime incidents for each pandemic month using a (dynamic) regression given by:

$$y_{it} = \sum_{j=3}^{12} \delta_j D_{it}^j + \beta' w_{it} + \alpha_i + \theta_t + \gamma_t + \varepsilon_{it}, \quad (2)$$

where D_{it}^j are treatment dummy variables for each of the pandemic months (March to December) of 2020. In this specification, the coefficient δ_j represents the average

¹ See Table A1 of the Supplementary Materials. See also the supplementary materials of the previous study (Nivette et al., 2021b).

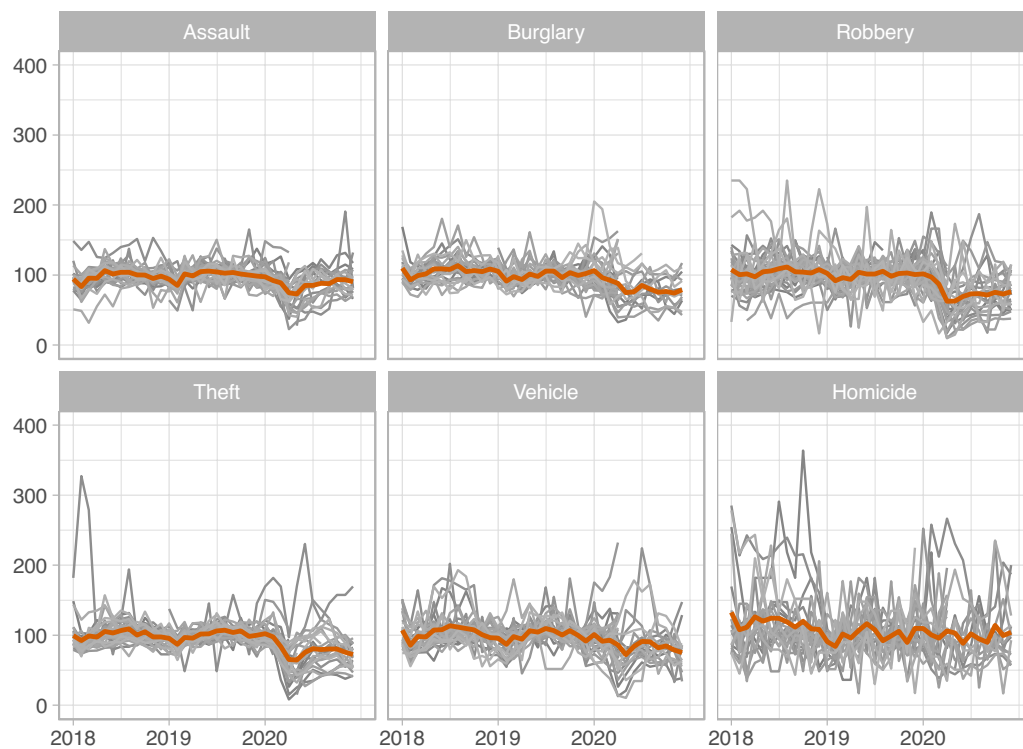


Fig. 1 Monthly crime indexes. The indexes are constructed such that their 2019 average equals 100. The average trend for each crime type is highlighted in orange

percentage change in crime incidents for month j (relative to the 2019 average).

Matrix completion design

Our main goal is to evaluate how the implementation of strict lockdowns impacted crime trends in 2020. Let D_{it}^2 be the (second) treatment variable equal to 1 if city i was in strict lockdown during month t . The treatment variable has the same dimension as the outcome matrix and takes the value of 1 when strict lockdown restrictions (not just recommendations to stay-at-home) were in place in city i , while 0 represents the period before or following the implementation of strict lockdown restrictions in that city. Since strict lockdowns were implemented intermittently, the treatment adoption exhibits an on-off pattern as the date cities enter/exit a strict lockdown varies across cities.

The date lockdown restrictions were implemented across cities is not always clear. As a result, we relied on a sub-index from the Oxford COVID-19 Government Response Tracker (OxCGRT). The sub-index C6 monitors “orders to shelter-in-place and otherwise confine to the home,” takes ordinal values {0, 1, 2, 3}, and is reported daily. The index takes the value 0 if no measures are in place, 1 when “recommend not leaving house,” 2 when “require not leaving house with exceptions,” and 3 when

“require not leaving house with minimal exceptions” (Hale et al., 2020). The sub-index C6 is plotted for each of the 45 cities in the sample in Figure B1 of the Supplementary Materials. To construct our (second) treatment variable, a city is considered under strict lockdown (i.e., the second treatment variable is 1) when the sub-index C6 takes the values 2 or 3 (“require not leaving house”). When the sub-index C6 takes the value 0 or 1, the city is considered untreated (i.e., the second treatment variable is 0). The second treatment variable is plotted in Figure B2 of the Supplementary Materials.²

Under the potential outcomes framework (Rubin, 1974), for each city i and month t , there are a pair of potential outcomes, y_{it}^1 and y_{it}^0 , that correspond to the potential outcomes under treatment (strict lockdown) and control condition (stay-at-home recommendation but no strict lockdown), respectively. To assess the effect of strict lockdowns for each crime type, we need to estimate a counterfactual matrix y_{it}^0 in which elements represent the monthly index of crime incidents for cities that are not in strict lockdown (untreated cities). When a city

² A city was considered treated if at least one day of the month the city was under strict lockdown. While length of strict lockdown varies significantly across the sample, most lockdowns lasted longer than 2 weeks (see Figure B2).

Table 1 The impact of the COVID-19 pandemic on crime

	Assault	Burglary	Robbery	Theft	Vehicle	Homicide
Average effect	– 20.06	– 20.43	– 28.25	– 31.53	– 18.81	– 10.35
Std. error	2.34	4.29	4.90	3.92	3.84	6.43
95% C.I. lower	– 24.81	– 29.20	– 38.14	– 39.45	– 26.63	– 23.62
95% C.I. upper	– 15.31	– 11.66	– 18.37	– 23.61	– 10.98	2.91
p-value	0.00	0.00	0.00	0.00	0.00	0.12
N	1161	906	1295	1277	1017	781
R-squared	0.35	0.34	0.34	0.46	0.28	0.25

OLS estimates of Eq. (1) for each type of crime, including weather controls and city, month, and year fixed effects. Standard errors are clustered at the city level

is in strict lockdown ($D_{it}^2 = 1$), the outcome variable is removed from the matrix of outcomes, and the observation is treated as missing. The objective is to “complete” the matrix of outcomes under the assumption that no lockdown has taken place (Liu et al., 2024). The causal quantity of interest is the average treatment effect on the treated (ATT).

In this paper, we used the *matrix completion* (MC) counterfactual estimator of Athey et al. (2021) and Liu et al. (2024). The MC estimator is given by:

$$y_{it}^0 = \beta' w_{it} + \alpha_i + \gamma_t + L_{it} + \varepsilon_{it}, \quad (3)$$

where y_{it}^0 is the monthly index of crime incidents for a given type of crime for untreated cities, α_i and γ_t denote unit (city) and time fixed effects, L is a matrix to be estimated with typical element L_{it} (see Liu et al., 2024), and w_{it} are weather covariates. Athey et al. (2021) provide an algorithm that uses regularization to estimate the model and impute the missing values in the counterfactual matrix. This estimator generates a counterfactual outcome y_{it}^0 for each treated observation and, as a result, we can estimate the individual treatment effect δ_{it} as the difference between the estimated y_{it}^0 and the observed y_{it} , $\hat{\delta}_{it} = y_{it} - y_{it}^0$. Next, we can compute ATTs of interest as averages of the $\hat{\delta}_{it}$ for a subset of the observations under treatment. For example, we can compute the overall ATT by calculating the average for all treated observations. Alternatively, we can compute the ATT for each of the pandemic months separately or the ATT for each month relative to the beginning of a lockdown, etc. Standard errors and confidence intervals can be obtained using 1000 block bootstraps at the city level and jackknife (leave-one-out) methods, as in Liu et al. (2024).

If the treatment (strict lockdown) impacts crime, the observed frequency of monthly crime rates should be lower in the treated periods than in the counterfactual estimates. Therefore, rather than comparing the pandemic’s impact on crime in each city to what would have occurred without it (utilizing pre-pandemic crime trends

as a baseline), we are examining the effect of strict lockdown measures on crime rates in cities like Mexico or London. To achieve this, we use cities such as Malmö or Stockholm, where no strict lockdown was imposed during the pandemic, as control groups. Thus, our ATT analysis yields an estimation of the reduction in crime attributable to the enforcement of stringent lockdown measures while controlling for the effect of the pandemic in cities that did not enforce such measures yet still experienced decreased mobility due to stay-at-home government policies and behavioral adjustments by organizations and individuals.

Results

The impact of the COVID-19 pandemic on crime

We begin by evaluating the overall impact of the COVID-19 pandemic on crime. Table 1 reports the average percentage change in crime incidents for each crime type after the onset of the COVID-19 pandemic in March 2020 from Eq. (1). Our results show that, on average, across all the cities, there was a substantial drop in crime relative to the 2019 average. This effect is found to be particularly large and statistically significant for theft (31.5% drop in monthly counts), robbery (28.2% drop), burglary (20.4% drop), assault (20.1% drop), and vehicle theft (18.8% drop). In contrast, the effect on homicide was smaller (10.3% drop) and not statistically significant.

Figure 2 plots the average percentage change in crime incidents for each month after March 2020 obtained from Eq. (2). Our results show there was a large drop in crime during the first few months of the pandemic (mainly March, April, and May), followed by a moderate bounce back during the northern hemisphere summer of 2020. The drop in crime settled around 25% for burglary, robbery, and vehicle theft, and close to 30% for theft. The drop was smaller for assault, settling around a 10% drop. Finally, we observe a small and persistent drop in homicide that is not statistically significant. Overall, these estimates are consistent with those documented in the

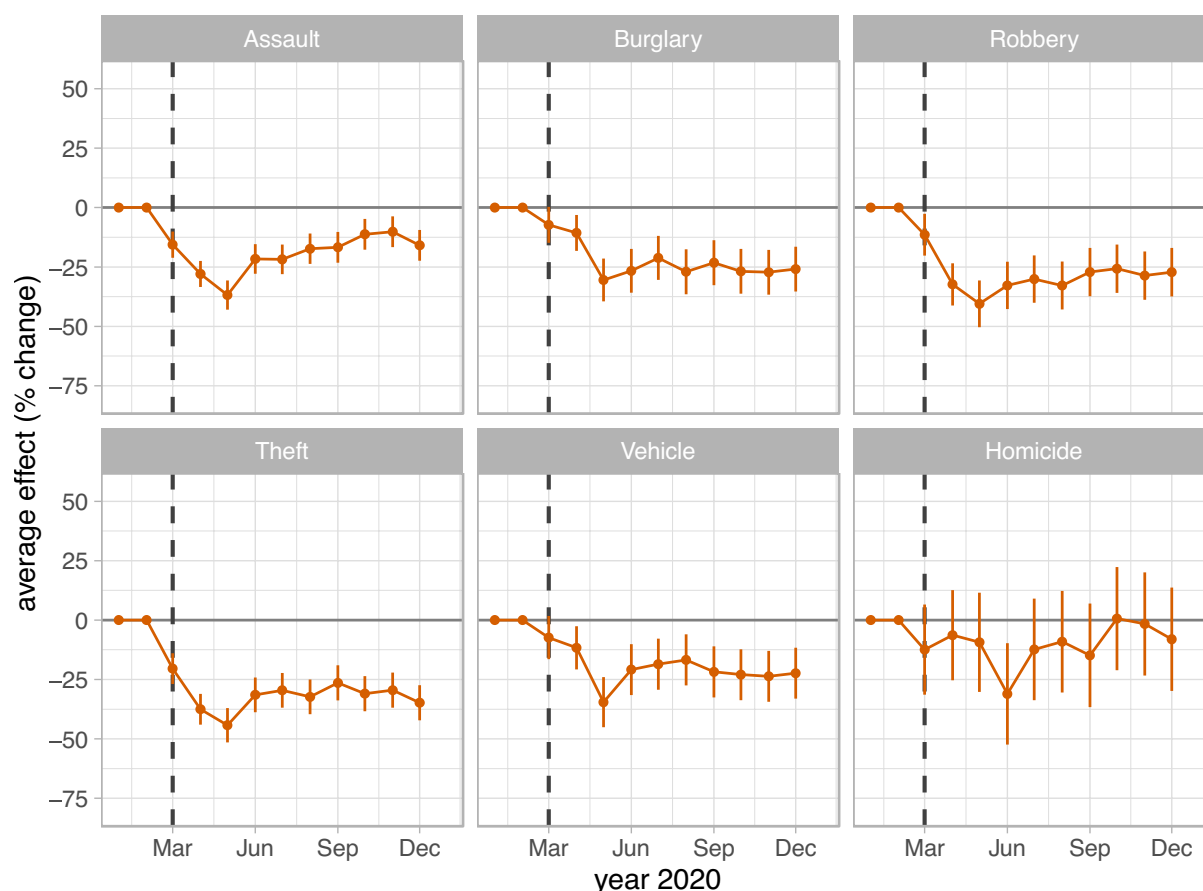


Fig. 2 Average effect of the COVID-19 pandemic on crime by month

literature, although somewhat smaller in comparison to the reductions in crime reported in Nivette et al. (2021a).

The impact of strict lockdown on crime

To evaluate the impact of strict lockdowns on crime, we need to identify the periods in which strict lockdown restrictions, not just stay-at-home recommendations, were in place in each city. In addition, we need to control for changes in behavior that can be attributed to the pandemic (e.g., voluntary work-from-home policies) but not to a strict lockdown mandated by governments. Figure B2 of the Supplementary Materials shows the temporal and spatial variation of *strict lockdown* for all cities and periods in our sample. Before March 2020, all cities were considered untreated. After March 2020, cities that imposed strict lockdowns were considered treated, some intermittently and some permanently. For example, Mendoza and Lima were treated during all the study periods because they imposed strict lockdowns from March until the end of 2020. In contrast, Stockholm and Malmö were control cases for the whole period because no strict lockdown was imposed. London was considered under treatment

during the first three months and the last two months of 2020 but was considered a control case during spring and all summer of the northern hemisphere. Table C1 of the Supplementary Materials shows that being under strict lockdown (i.e., when the second treatment variable is equal to 1) was associated with a substantial reduction in mobility, in addition to the reduction observed in cities without strict lockdown (i.e., the control cities). For example, cities under strict lockdown experienced a drop in retail and recreation mobility that is 89.8% larger than what was observed in the control cities. Similarly, the drop was 58.8% larger for transit mobility and 64.7% larger for workplace mobility.

Table 2 presents the estimated ATT of strict lockdown using the MC counterfactual estimator. Our results show that, on average, across all the cities and all of 2020 months, the impact of strict lockdowns was negative for all crimes relative to the cases without strict lockdowns. This effect was found to be particularly large and statistically significant in the case of robbery (19.1% drop in monthly counts), burglary (14.9% drop), and vehicle theft (11.9% drop). However, the impact of strict

Table 2 Average treatment effect on the treated (ATT) of strict lockdown on crime

	Assault	Burglary	Robbery	Theft	Vehicle	Homicide
ATT	– 3.47	– 14.92	– 19.15	– 5.36	– 11.89	– 12.95
Std. error	4.60	5.70	4.96	4.89	6.48	14.98
95% C.I. lower	– 11.04	– 24.29	– 27.31	– 13.41	– 22.54	– 37.59
95% C.I. upper	4.10	– 5.55	– 11.00	2.69	– 1.24	11.69
p-value	0.45	0.01	0.00	0.27	0.07	0.39
N	38	31	42	41	33	25
T	36	36	36	36	36	36

MC estimates include weather controls and city and time fixed effects

lockdowns on assault (3.5% drop) and theft (5.4% drop) was small and not statistically significant. Finally, the effect of strict lockdowns on homicide was large (12.9% drop) but not statistically significant. Nevertheless, the analysis of homicide included fewer cities due to missing observations and a lack of sufficient volume of monthly rates (e.g., only 25 cities were included in the analysis of homicides).³⁴

Next, we estimate the dynamic ATT of strict lockdown on crime for each month, relative to cities that did not experience a strict lockdown in that month (i.e., the difference between the estimated effect of the COVID-19 pandemic on crime for the treated and untreated each month).⁵ Visual inspection of Fig. 3 reveals that the ATTs of strict lockdown on crime were observed only after May 2020. In addition, there was substantial heterogeneity across the different waves of the pandemic. For example, the monthly ATT of strict lockdown on assault exhibited a U-shape with a significant reduction in the first months of the pandemic (an additional 25% drop relative to cities with less stringent stay-at-home policies). However, the effect gradually ceased to be significant, with no effect after August. Robbery exhibited a similar trend with a

more substantial and persistent initial reduction (a 30% drop) that gradually ceased to be statistically significant after the June/August period, except for a large drop in October. Burglary, in contrast, exhibited a W-shaped pattern with two drops in the period, a significant reduction in the first wave of the pandemic (a 30% drop) and a second large reduction, though smaller, during the second wave (a 20% drop). The effect of strict lockdown on theft and vehicle theft across time was less clear, and although some specific months exhibit statistically significant reductions, most months showed non-significant differences. Likewise, there was no apparent effect of strict lockdown on homicide across time due to the lack of sufficient volumes of cases.

A follow-up question is: *what is the impact of strict lockdown on crime as consecutive months of lockdown accumulate?* Fig. 4 shows the ATT of strict lockdown on crime relative to the start of the strict lockdown. This was obtained by computing the ATT for treated observations that correspond to any first month of strict lockdown, then we compute the ATT for treated observations that correspond to any second consecutive month of strict lockdown, and so on. Our results show mostly non-significant reductions in crime in the first two months of strict lockdown relative to cities with less stringent mobility restrictions. However, as months of lockdown accumulated, there was an increasingly significant reduction in crime rates for burglary, robbery, and assault. In contrast, there was little effect on theft and vehicle theft, and no significant effect on homicide.

Nevertheless, these results should be interpreted with caution because the analysis was based on a reduced sample. While almost all cities in the sample (except for Stockholm and Malmo) experienced at least one month of strict lockdown, the number of cities under strict lockdown for two or more consecutive months was substantially smaller. For example, the impact of strict lockdowns on robbery that involves four consecutive months was estimated using only ten cities. Thus, as we consider the effect of longer lockdowns, we have fewer cities, and

³ Placebo test results are reported in Table D1 of the Supplementary Materials. Treatment is introduced four months before the actual treatment (strict lockdown) creating an in-time placebo period. We then estimate the ATT for the placebo period using the MC estimator. We find no evidence for a lockdown effect on crime for any of the crime types considered (all p-values > 0.05).

⁴ The results using all US data are reported in section E of the Supplementary Materials. Our results for assault, burglary, robbery, theft, and vehicle theft remain mostly unchanged (see Figures E1–E3). In contrast, we now observe a large and statistically significant reduction in homicide. However, this effect was not due to a drop in homicide in the treated but due to a large increase in homicide in the control cities, which included all the US cities. For a discussion of a potential Minneapolis effect due to the Floyd case see Ratcliffe & Taylor (2023).

⁵ Figure B3 of the Supplementary Materials plots the estimated effect of the COVID-19 pandemic on crime for the two groups of cities considered (treated and untreated) for each month. The results show that cities under strict lockdown (the treated) had systematically larger reductions in crime across all types of crime compared to cities with less stringent stay-at-home policies (the untreated).

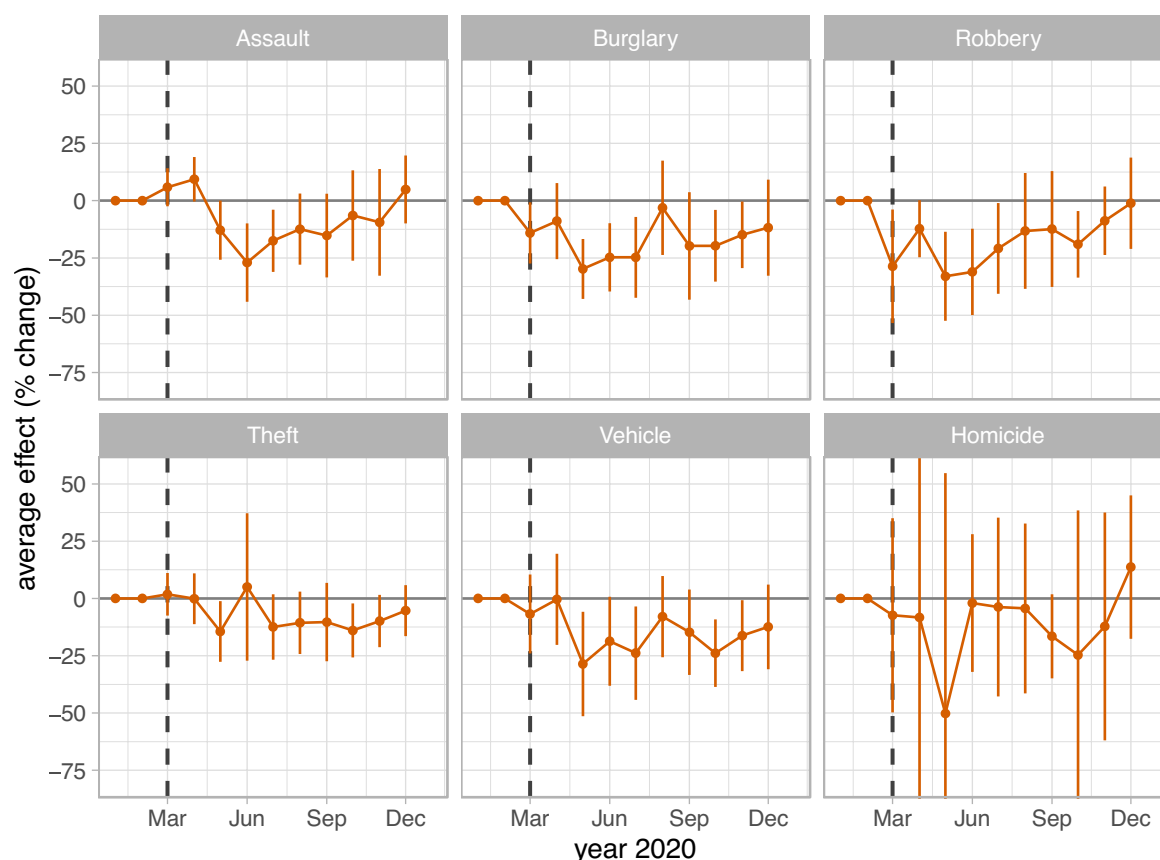


Fig. 3 Average treatment effect on the treated (ATT) of strict lockdown on crime by month. Cities under strict lockdown are considered treated

rejections of the null hypothesis of no effect are more likely to be driven by the idiosyncratic effects of the cities remaining in the sample. Moreover, we only considered the cumulative effect of consecutive periods of treatment. If the treatment (strict lockdown) was interrupted by periods of no lockdown, the new period of lockdown was considered as a new first month of treatment. For example, London had two strict lockdown periods: one lasting three months in the northern hemisphere spring and the second one of 2 months in winter.

Discussion

Our findings show that cities under strict lockdown experienced substantial declines in robberies, burglaries, and vehicle thefts, compared to cities under less stringent stay-at-home orders. However, when comparing cities with strict and non-strict lockdowns, we found no significant effects on assaults, thefts, and homicides. Non-economic violent crimes, such as assaults and homicides, are often situational and linked to spontaneous conflicts in public settings associated with leisure activities (Wilcox & Cullen, 2018). The initial stages of the COVID-19 pandemic involved the closure of the night-time

economy and the cessation of public situational contexts (e.g., pubs, bars, and other outlets) where such violent frictions are more likely to occur (Ejrnæs & Scherg, 2022; Gerrell et al., 2022). Thus, when strict lockdowns were implemented, the opportunities for these types of violent crimes were already significantly reduced. It is also possible that some shift took place from violent frictions in public settings to more private settings. There is evidence of an increase in reports of domestic incidents (Piquero et al., 2021), particularly by current partners and not by former partners (Ivandić et al., 2020). Moreover, a portion of homicides occurs within the context of organized crime activities, which was less impacted by the stringency of health measures (Hoeber et al., 2024).

The results regarding theft reports are puzzling. In fact, this economically motivated street crime exhibits one of the most consistent findings across the COVID-19 literature (Hoeber et al., 2024). One possibility is that theft showed stability during strict lockdown because the reduction of criminal opportunities due to changes in routine activities had already taken place in the first weeks of the pandemic, where people had significantly decreased their interactions in the public sphere (Felson

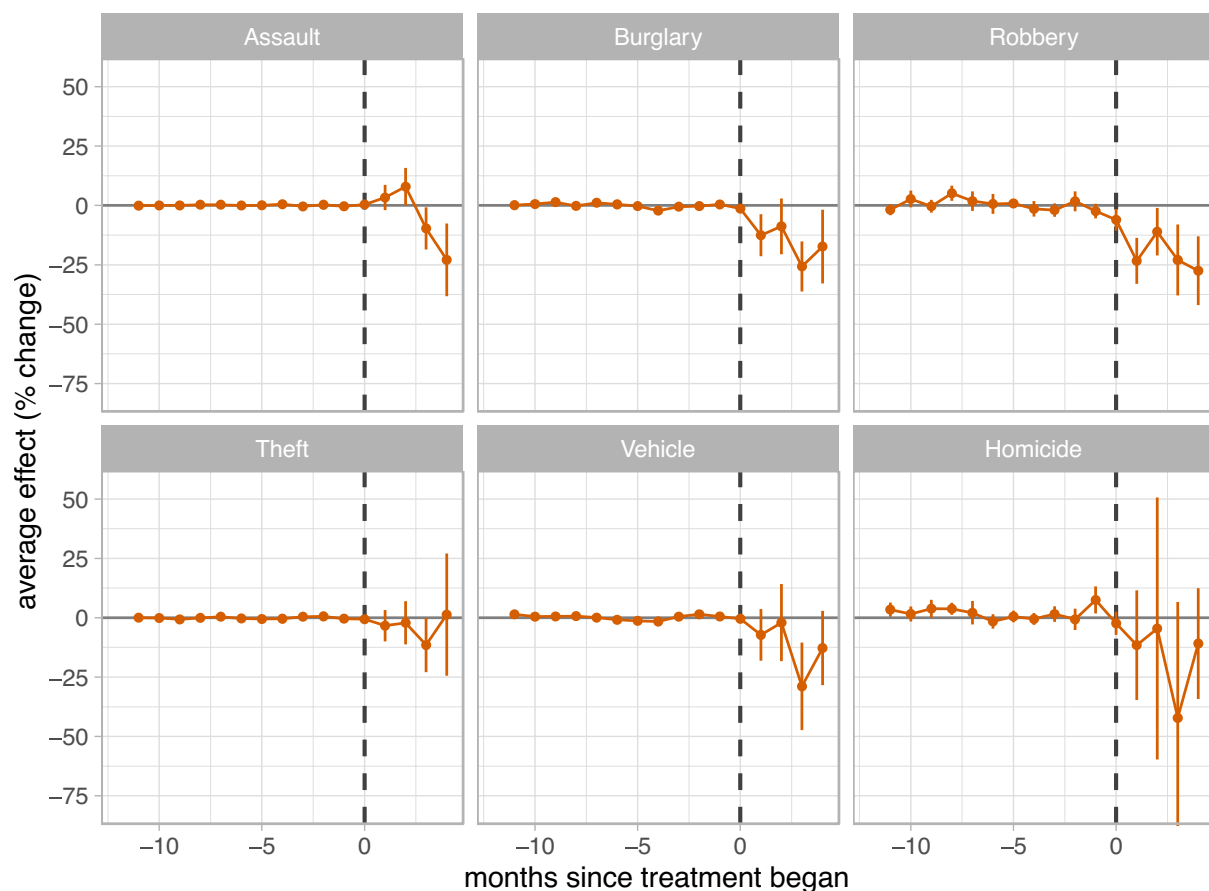


Fig. 4 Average treatment effect on the treated (ATT) of strict lockdown on crime relative to the start of the strict lockdown

et al., 2020). Additionally, this new context, with fewer potential victims due to reduced interpersonal contacts in the streets coupled with an increase of capable guardianship at homes, might have led robbers and burglars to switch to thefts. These findings are consistent with previous evidence on “functional displacement” to other crimes, particularly with strongly motivated offenders, when there is an expectation of reducing risks, and usually toward less serious crimes (Rossmo & Summers, 2021; see also Johnson et al., 2014). Theft is a very generic category, and more fine-grained data would allow us to understand how this displacement might be associated with some specific categories of theft like shoplifting, bicycle theft, or theft of items left outside houses in porches or garages. Yet, more research is needed to understand why strict lockdowns might have had different effects on property crimes with economic motivations and which contextual and specific mechanisms explain these differences.

It is hard to know if changes in crime rates during strict lockdowns are attributable to mechanisms distinct from alterations in criminal opportunities. Although the

literature mentions opportunities and strains as potential explanatory mechanisms (Campedelli et al., 2020a; Stickle & Felson, 2020), research has focused mainly on the role of opportunities, with few exceptions showing how changes in interpersonal violence and violent property crimes during the pandemic can be partially explained by geographical differences associated with poverty, unemployment, and inequality (Andresen & Hodgkinson, 2020; Campedelli et al., 2020b). Strains are more likely to have long-term effects on crime (Eisner & Nivette, 2020), as government measures are relaxed, and routine changes become less relevant (Payne et al., 2021). For example, research conducted in the US suggests that the surge in violent crimes during reopening phases following lockdowns may be attributed to a combination of heightened opportunities and the accumulation of strains (Ridell et al., 2021). Nevertheless, empirical support for the role of strains in the COVID-19 literature is weak, and its relevance as an explanatory mechanism has been challenged, notably when it comes to the prediction of crimes such as domestic violence (Aebi et al., 2021; Hodgkinson et al., 2023). Our analysis does not reveal

significant differences between more violent situational crimes (like assaults) and economically motivated crimes (such as theft), even after several consecutive months of lockdown. However, our findings should be taken with caution, given the limitations of our analysis (e.g., the exclusion of US cities after May 2020 and a limited number of cases with extended strict lockdowns).

Our results show that the additional reduction in crime rates due to strict lockdowns was small and stronger mobility restrictions did not translate into substantially larger drops in crime. In other words, the relationship between mobility and crime does not appear to be linear as further reductions in mobility had marginal effects on crime. This result suggests that crime reductions during the pandemic were not only driven by local sanitary restrictions implemented by governments but also by people's preventive behavior and organizations' policies (e.g., flexible work-from-home conditions) (Barrero et al., 2021). Thus, when a strict lockdown was imposed, both people and organizations had already reacted, altering routine activities and crime opportunities (Stickle & Felson, 2020). In simpler terms, strict lockdowns did not substantially change the number of potential victims on the streets or the occupancy levels in households despite reducing mobility. These had already decreased significantly beyond the initial mobility decline prompted by the initial guidelines, as well as the precautionary measures taken by organizations and individuals. Thus, stricter lockdowns had only a marginal effect, as the new scenario did not significantly increase the difficulties or costs of finding criminal targets (Nagin, 2013).

Our findings carry policy implications. This study suggests that most of the crime reduction took place without the need for a costly and extensive 'massive social incapacitation' of citizens by the government (strict lockdown), forcing them to have a 'house arrest experience' (Baker, 2020). While the estimates in Table 2 show a negative average effect of strict lockdowns for all crimes (relative to cases without strict lockdowns), they indicate a diminishing or null effect when compared to the findings in Table 1. This implies that achieving crime reduction can rely more on citizens' autoregulation and less on sacrificing citizens' freedom of movement. During the COVID-19 pandemic, policymakers explored alternatives that balance public health concerns and preserve individual liberties. Similarly, effective crime reductions can be attained through measures that are less restrictive of citizens' freedom of circulation.

This study is not without limitations. First, many cities in the sample have a very low frequency of homicides. Although our study finds no significant effects on homicides, the low frequency of these incidents presents challenges in terms of statistical inference when determining

how these crimes were affected by strict lockdowns. This is a common limitation in natural disaster studies, which focus on aggregate measures of violent crimes rather than homicides (Doucet & Lee, 2015). Second, our study is limited by the use of police records. This not only presents the challenge of comparing and harmonizing crime categories across different legal frameworks in various countries (Aebi, 2010) but also involves issues related to the reporting, recording, and publishing of data, which vary significantly across crime categories (Ashby et al., 2022; Buil-Gil et al., 2021; Xie & Baumer, 2019). Particularly problematic is that selection biases not only influence how victims report crimes and how police officers record them, but these processes are also heterogeneous across units of analysis (Estienne & Morabito, 2016; Torrente et al., 2017). Additionally, the pandemic might have further exacerbated bias in crime measurement. For example, underreporting might have occurred due to victims and police fearing contagion. At the same time, under-recording could have resulted from reduced police department capacity to register, respond to calls, and patrol (Wallace et al., 2021). Nevertheless, some studies have shown that part of the crime drop is not an artifact of underreporting by providing robustness checks by contrasting trends of different types of crimes before and after the pandemic (Abrams, 2021), or by triangulating police crime records with victimization surveys (Perez-Vincent et al., 2021). Finally, our sample has a limited geographic variance which affects the external validity of our findings. Although the sample almost doubled the number of cities in relation to Nivette et al. (2021a) and included relevant cities from South America and the Caribbean, there is still an overrepresentation of North America and Europe. One challenge is to incorporate more cases from underrepresented regions and have a more representative sample in terms of low- and middle-income non-western societies (Boman & Mowen, 2021; Eisner, 2023) with more variability of crime indicators, correlates of crime, but also in terms of validity of their police crime statistics (Mendlein, 2021; Rogers & Pride-more, 2017).

Conclusions

During the COVID-19 pandemic, governments implemented a variety of stay-at-home policies to reduce mobility and prevent the spread of the virus. Cities under strict lockdowns across North America, South America, Europe, Asia, and Oceania experienced larger declines in property crimes, such as robbery, burglary, and vehicle theft, when compared to cities under less stringent stay-at-home policies. However, more stringent stay-at-home policies did not seem to have a more significant effect than less stringent policies on assault, theft, or homicide.

The reduction in crime rates attributed to these more stringent policies represents only a small proportion of the overall effect of the pandemic on crime. Relevant lessons can be extracted regarding the necessity of implementing stringent measures on citizens' rights and freedom of movement to reduce crime.

Supplementary Information

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Supplementary Material 1.

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Author contributions

N.T. and S.F. conceived the project. N.T. and C.D. initiated data collection and prepared the data. S.F. conducted the analyses. N.T., S.F., and C.D. interpreted the analyses and wrote the manuscript. The remaining authors contributed data or facilitated access to data used in the analyses. All authors reviewed the manuscript and contributed to intellectual content. All authors read and approved the final manuscript.

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Availability of data and materials

Data and codes to conduct analysis in this study are available on the website of one of the authors: www.carlosddiaz.com.

Declarations

Ethics approval and consent to participate

Not applicable.

Competing interests

The authors declare that they have no competing interests.

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