Master Thesis

Economics and Business: Policy Economics

Intertemporal and Heterogeneous Effects of Lockdown Mandates on Violent Crime in Chile

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Abstract

This thesis studies dynamic impacts on violent crime of lockdown imposition and removal during the COVID-19 pandemic across Chile's 346 municipalities. I provide estimates of dynamic impacts of lockdown entry and exit on five variables of violent crime, and explicitly explore the mechanisms behind this relationship. Estimates show an expected negative sign when lockdown is imposed and positive sign when the lockdown is lifted. The reduction of violent crime due to lockdown entry was predominantly larger during the first wave. The changes in violent crime trends during the COVID-19 lockdowns seem to be aligned with changes in mobility due to the restriction on social interactions and the increase of criminals' search costs of potential victims. Lockdown imposition is associated with a decrease in employment during the first COVID-19 wave. However, an extended analysis reveals that unemployment may not be a mechanism through which lockdown impacted violent crime in Chile during the COVID-19 pandemic.

Keywords: COVID-19, lockdown mandates, dynamic effects, violent crime, mobility, unemployment.

Contents

1 Introduction

COVID-19 pandemic had a great impact on the healthcare system, economy, and different aspects of society. Measures such as physical distancing and stay-at-home orders were effective in reducing the spread of virus, but also had significant effects on the patterns of crime across the globe. In Latin America, the most violent region of the world (OECD, 2021), almost all the crime rates declined during the imposition of lockdowns, increasing after the lift of these measures, following world trends. Nevertheless, the majority of the studies that shape the understanding of criminal activity during the COVID-19 period are conducted in the developed world.

This investigation intends to broaden the understanding of how COVID-19 lockdown imposition and removal can impact on criminal activity in developing countries, particularly on violent crime. To address this question, I analyze the first and second wave of COVID-19 in Chile, where municipalities entered and exited lockdown several times during the pandemic and differed in the timing of the lockdown imposition and removal. Events of imposing and lifting lockdown provide a great number of natural experiments between March 2020 and July 2021.

Studies related to the effect on crime rates during the coronavirus pandemic are widely documented. Overall, the empirical evidence shows that most types of crimes experienced a temporal decline during COVID-19 lockdowns linked to mobility-restricting policies (Hoeboer et al., 2024; Nivette et al., 2021; Abrams, 2021; Lopez and Rosenfeld, 2021; Monteiro et al., 2021). There are other investigations that also take into consideration the relaxation of the restrictions, finding an expected increase in crime variables (Andresen and Hodgkinson, 2020; Buil-Gil et al., 2021). Yet, analyses and studies of the dynamic effects of lockdown imposing and removal on crime are still scarce.

The contribution of this paper is to provide estimates of dynamic impacts of lockdown imposition and removal on five variables of violent crime, and to explore the mechanisms behind this relationship. To achieve this, I exploit the exogenous variation of the multiple natural experiments provided by both entry to and exit from lockdown during the first and second COVID-19 wave in Chile. Particularly, I study dynamic impacts, taking into consideration intertemporal and heterogeneous treatment effects using the de Chaisemartin and D'Haultfœuille (2023) estimator. The identifying assumption is that the timing of lockdown mandates is conditionally random. I check this by estimating placebo effects, which can be used to inspect the no-anticipation and parallel trends assumptions by testing the null hypothesis that all placebos are jointly equal to zero.

I combine data from several sources to examine the effect of lockdown entry and exit on crime and test my hypotheses. For data on crime the source are the Official Statistics of Crimes of Major Social Connotation (DMCS), Domestic Violence (VIF), Incivilities and other facts reported by the Police of Chile to the Ministry of the Interior and Public Security. I consider five crime groups: the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. I gather administrative data on the exact dates at which municipalities enter and exit lockdown, indicators of COVID-19 infection rates, daily data on mobility from cell phone towers, and monthly data on formal sector employment outcomes.

When the period of analysis is the whole pandemic period, my findings show a significant decrease in police cases following lockdown entry for the five variables of violent crime. Regarding the analysis for each COVID-19 wave, estimates for the first wave present expected negative signs for the effects on crime of lockdown entry and positive signs for lockdown exit. However, the magnitudes of these effects suggest that the negative effect on crime of the lockdown imposition is higher than the rise on criminal activity once the lockdown is lifted. On the other hand, for the second wave the results also exhibit the expected signs, but the magnitude and significance of the coefficients illustrate that the reduction of violent crime due to lockdown entry was predominantly higher during the first wave. Moreover, in the second wave the impact of lockdown removal is relatively higher than its effect during the first wave for DMCS and robbery, but it does not apply for the other categories of crime.

I explicitly analyze potential mechanisms behind the variation in violent crime after lockdowns are imposed and lifted. To do this, I estimate dynamic impacts of lockdown entry and exit on mobility and employment outcomes. The results are consistent with mobility as a mechanism through which lockdown mandates impacted violent crime in

Chile. Additionally, the estimates show that lockdown imposition is linked with a reduction in employment during the first COVID-19 wave. However, overall the results are not consistent with unemployment being a channel through which lockdown affected violent crime in Chile during the COVID-19 pandemic.

To systematically address my research question, this article is organized as follows. Section 2 presents the Literature Review, which describes the key features of relevant previous studies. Section 3 shows the institutional setting and data, while Section 4 presents the empirical strategy. The results of the dynamic effect of lockdown mandates on crime are detailed in section 5. Section 6 analyzes the main mechanisms behind the change in criminal activity. Section 7 shows robustness checks and section 8 describes the limitations of this study. Finally, section 9 concludes.

2 Literature Review

2.1 COVID-19 and Crime

Studies related to the effect on crime rates during the coronavirus pandemic are widely documented. Several investigations on domestic violence found a significant increase in crime rates (Piquero et al., 2021). Nevertheless, empirical evidence largely shows that most types of crimes experienced a temporal reduction during COVID-19 lockdowns, regardless of the differences in methodologies and data (Nivette et al., 2021; Hoeboer et al., 2024). Most of these studies are conducted in Western countries and are focused on the short-term effect of lockdowns on crime, finding a significant decrease in crime rates linked to mobility-restricting policies.(Abrams, 2021; Ashby, 2020; Langton et al., 2021; Lopez and Rosenfeld, 2021). Other investigations also include the analysis of the relaxation of the restrictions, finding an expected increase in crime variables (Andresen and Hodgkinson, 2020; Buil-Gil et al., 2021; Díaz-Faes et al., 2023; Meyer et al., 2022; Neanidis and Rana, 2023).

Given the novelty of the topic, analyses and studies of the dynamic effects of lockdown imposing and removal on crime are still scarce. There are 2 studies that adopt this approach with an economic perspective. Poblete-Cazenave (2024) investigates the dynamic impact of a temporary policy restricting social encounters on criminal activity in India using criminal case-level and arrest data. The study uses a regression discontinuity design in time to document the immediate decline in crime of over 35% due to the lockdown. Moreover, considering that lockdowns' impact on crime outcomes can differ throughout time, this paper adopts an event study approach to capture differential effects along these margins.

On the other hand, Bhalotra et al. (2024) identify dynamic impacts of the imposition and lifting of lockdown on three measures domestic violence across Chile's municipalities, using high-frequency administrative data. The study uses the de Chaisemartin and D'Haultfœuille (2020) estimator (DID_M) and finds an increase in domestic violence helpline calls and shelter occupancy after lockdown is imposed in Chile, identifying male job loss as a mechanism driving domestic violence.

In empirical evidence regarding Latin American countries, Perez-Vincent et al. (2021) document a decrease of 59% in arrests and 52% in property crimes in Buenos Aires, Argentina using survey and monthly crime data. Alvarado et al. (2021) find a 40% drop in homicides within the first month of lockdown as well as a decrease in thefts using a difference-in-differences approach and administrative data from Colombia. Estevez-Soto (2021) reports a nearly 40% decline in aggregate crime which is associated with the change in mobility in Mexico. Also for Mexico, using municipality-level national crime data, Balmori de la Miyar et al. (2021) find that most of crimes rise back to pre-pandemic levels when the lockdown ends, following a U-shaped trend.

2.2 Lockdown Mandates, Mobility and Crime

Studies about the impact of the recent COVID-19 epidemic on the mobility of people are broadly documented (Anke et al., 2021; Badr et al., 2020; Borkowski et al., 2021; de Palma et al., 2022; Li et al., 2022; and Santana et al., 2023). Furthermore, a great number of papers that investigate the relationship between COVID-19 and crime highlight the mobility of people as the main mechanism. Most of these studies are focused on the short-term effect of lockdowns mandates on crime finding a significant reduction in crime rates linked to mobility-restricting policies (Abrams, 2021; Ashby, 2020; Felson et al., 2020; Langton et al., 2021; Lopez and Rosenfeld, 2021).

Following Poblete-Cazenave (2024), I use the empirical framework provided by Balkin and McDonald (1981) and Cohen and Felson (1979) to analyze the mechanism through which lockdown mandates impact crime. To do this, it is important to understand that the objective of lockdowns is to limit social interaction by keeping the population at home.

Balkin and McDonald (1981) derive a theoretical model of crime causation in the context of a market framework using traditional microeconomic theory. In this model, the behavior of offenders as well as the behavior of potential are endogenously modelled. Victims decide the exposure time defined as the amount of time they spend out in public and criminals choose the amount of time they spend searching for potential victims. In this model, the crime rate, the probability of finding a victim per unit of offenders' search time, and the probability of being a victim per unit of exposure time are simultaneously determined.

In the context presented in this paper, lockdowns imposition limits individuals' exposure time and restrict their daily activities by forcing them to stay home. Together with the exogenous decrease in exposure time to crime for most people, the model predicts a reduction in the offenders' probability of finding victims. Holding other conditions unchanged, this effect is caused by an increase in the criminal's amount of time spent searching for potential victims and the decrease in the the risk of victimization (McDonald and Balkin, 2020).

The model of Cohen and Felson (1979) presents a routine activity approach for analyzing crime rate trends. This model defines that the convergence in time and space of motivated criminals, suitable targets, and the absence of capable guardians is useful for explaining crime rate trends. The lack of any of these factors is sufficient to prevent the materialization of a successful direct-contact crime. The lockdown mandates during the COVID-19 pandemic might have impacted the behavior of offenders, potential victims, and police in different ways. Thus, the net impact of lockdowns on crime is not clear.

Regarding the effect on offenders, the Cohen and Felson (1979) model indicates that they might decide to stay out of the streets to comply with the lockdown as an optimal response to the lower supply of potential victims. The restriction on social interactions might decrease crime by increasing offenders' search costs due to fewer people on the streets and for shorter lengths of time. Nevertheless, if criminal activity is their main or only source of income, potential offenders might be forced to go on the streets despite the fear of contracting the virus.

On the other hand, countries required police officers to enforce and monitor lockdowns compliance. Despite being a developing country, lockdown mandates were strict in Chile and police officers conducted spot inspections, penalizing thousands of citizens. As the Cohen and Felson (1979) model predicts, police deployment could discourage violent crime by affecting opportunities for offenders due to the presence of a capable guardian.

2.3 COVID-19, Unemployment and Crime

Wide is the research regarding the effects of COVID-19 on unemployment trends (Blustein et al., 2020; Gangopadhyaya and Garrett, 2020; Gezici and Ozay, 2020; Petrosky-Nadeau and Valletta, 2020; Smith et al., 2021). Likewise, there is vast empirical research that analyzes the relationship between unemployment and crime (Cantor and Land, 1985; Edmark, 2005; Fougère et al., 2009; Jawadi et al., 2021; Lee and Holoviak, 2006; Levitt, 2001; and Prescott and Pyle, 2019)

To understand how unemployment acts as a channel through which COVID-19 and the related lockdown measures impact violent crime, I use the theoretical model of time allocation presented by Raphael and Winter-Ebmer (Raphael and Winter-Ebmer, 2001). This model conceptualizes criminal activity as a form of employment that requires time and generates income. The rational criminal makes decisions comparing returns to time use in legal and illegal activities. Ceteris paribus, the decrease in income and potential earnings linked with involuntary unemployment increases the relative returns to illegal activity.

Under the context investigated in this paper, COVID-19 lockdown mandates might affect labor market conditions, leading to a rise in unemployment. As the literature highlights, worse economic conditions are a risk factor of crime. Therefore, if the returns from legal activities are reduced due to higher unemployment or limited job opportunities, individuals might be more likely to engage in criminal activity. For this reason, unemployment may be a mechanism through which lockdown mandates can affect violent crime.

2.4 Recent Literature about the Estimation of Causal Effects for Natural Experiments

Researchers generally rely on natural experiments to investigate causal-inference questions for which randomized experiments are impractical. During the last decades, many social scientists have used the two-way fixed effects (TWFE) regression as a default empirical approach for estimating causal effects from panel data using natural experiments.

Usually, the natural experiments that are used in social sciences are policy changes which, unlike randomized experiments, are decided by policy makers. It implies two things. First, policy changes sometimes present turns and twists, i.e., a legislative change can be applied and then reverted again or can vary in the intensity of its application across administrative units. Second, policy makers may not randomly decide where to implement a legislative change, making treated and control regions not comparable. Literature developed recently has shown that in more complex designs, TWFE estimators are not equivalent to DID estimators. Therefore, the TWFE estimator does not estimate the average treatment effect on the treated and it may assign negative weighs to the treatment effects of some events (de Chaisemartin and D'Haultfœuille, 2023).

A survey conducted by de Chaisemartin and D'Haultfoeuille (2023) showed that 26 of the 100 papers with the most Google Scholar citations published by the American Economic Review from 2015 to 2019 have estimated TWFE regressions to obtain the effect of a treatment on an outcome. Nevertheless, only two of those 26 papers have a typical design with a binary treatment and homogeneity in the timing of the treatment.

One of these more complex designs where TWFE estimators are not equivalent to DID estimators is the binary and staggered design, which only difference with respect to the classic DID design is the variation in treatment timing. Several heterogeneity-robust DID estimators in binary and staggered designs have been proposed in the recent years by Borusyak et al. (2024), Callaway and Sant'Anna (2021), Gardner (2022), Liu et al. (2024), and Sun and Abraham (2021). Analogously, in this type of design, the DID_M estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) is identical to the estimator of ATT using the not-yet-treated as controls in Callaway and Sant'Anna (2021).

Additionally, a treatment in heterogeneous adoption designs (HAD) refers to when groups are not treated in period one, some or all groups are treated in the second period, and the treatment intensity or dose can be different across the treatment group's units (de Chaisemartin and D'Haultfœuille, 2020). Estimators for this type of treatment have been developed recently by Callaway et al. (2024), de Chaisemartin and D'Haultfoeuille (2018), de Chaisemartin and D'Haultfoeuille (2020), de Chaisemartin et al. (2024a), de Chaisemartin and D'Haultfoeuille (2024b), and Graham and Powell (2012).

Finally, Chaisemartin and D'Haultfoeuille (2023) propose heterogeneity-robust DID estimators that can be used in designs with a nonbinary and/or non-staggered treatment. These estimators can also be applied in designs with a binary and staggered treatment. Moreover, these estimators are heterogeneity-robust when lagged treatments can affect the outcome, similarly as in Borusyak et al. (2024), Callaway and Sant'Anna (2021), and Sun and Abraham (2021). Thus, unlike the de Chaisemartin and D'Haultfœuille (2020) estimator (DID_M), the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ) obtains unbiased estimates of the impact of the contemporaneous treatment on the outcome when lagged treatments can affect the outcome and the treatment may turn on and off.

2.5 Contribution to the Literature

This thesis contributes to this literature in three main ways. First, the study focuses on Chile, a developing country located in Latin America, the most violent region in the world. This is particularly relevant taking into consideration that there is few empirical evidence of the impact of policy on criminal activity in countries from this region. The results of this investigation broaden the understanding of how lockdown mandates impact on crime in Latin America, providing insights that can be applied to strengthen the public safety policy.

Second, this paper uses state-of-the-art econometric techniques regarding the estimation of dynamic effects of lockdown mandates on criminal activity. Unlike most studies that investigate the short-term impact on crime, this thesis analyzes a longer time span and explicitly takes into account both the entry to and exit from lockdown as treatments. In this regard, this study adds to the literature by providing estimates using an approach based on intertemporal and heterogeneous treatments.

Third, I explicitly investigate the potential mechanisms behind the changes in violent crime after lockdowns are imposed and removed. The changes in violent crime trends during the COVID-19 lockdowns seem to be aligned with changes in mobility due to the restriction on social interactions and the increase of criminals' search costs of potential victims. Moreover, lockdown entry is associated with a decline in employment during the first COVID-19 wave. Nevertheless, an extended analysis reveals that unemployment may not be a mechanism through which lockdown impacted violent crime in Chile during the COVID-19 pandemic.

3 Institutional Setting and Data

3.1 Lockdown mandates in Chile

Unlike most of other heavily affected countries, Chile implemented lockdowns at a localized level, shutting down municipalities rather than larger regions or the whole country. I study the first and second COVID-19 waves in Chile. The first wave was between March and September 2020, while the second wave lasted from January to July 2021. During this period 304 of Chile's 346 municipalities were under lockdown, differing in the timing of the lockdown imposition and removal across municipalities over time. In the first wave, 70 municipalities entered to lockdown. The second wave was more severe in terms of infection and mortality rates, leading the government to impose lockdowns in 245 municipalities.

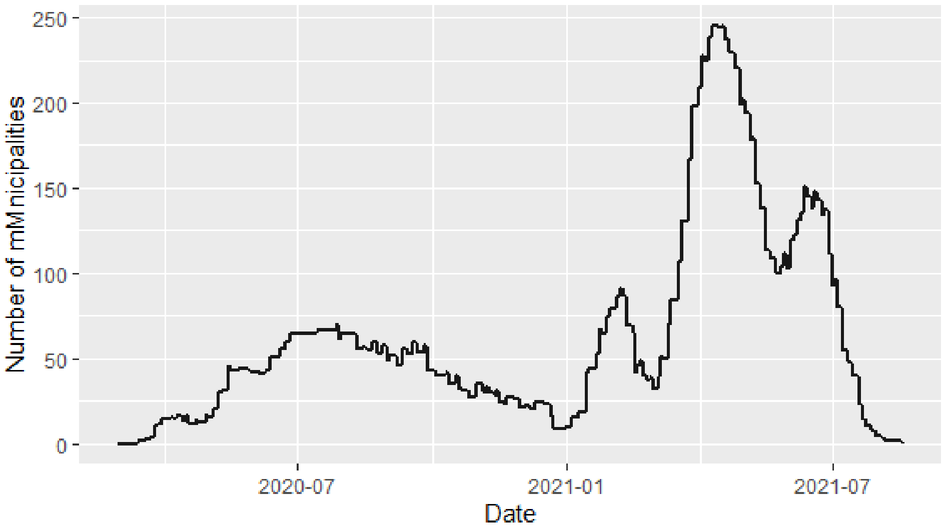
Lockdown mandates were strict and citizens were allowed to go out just twice a week for 3 hours each time, previously having requested a permit. Only essential workers including health, laboratory, veterinary, garbage collection, and public workers were exempt from the mandate. Nightly curfews were in place regardless of the lockdown status. Police officers conducted spot inspections, penalizing thousands of citizens. Breaches of COVID-19 lockdown rules could be penalized with fines up to 12.5 million Chilean pesos (€14,000) and prison sentences up to 5 years. Lockdown mandates were introduced by the Ministry of Health based on their evaluation of the number of current active cases and healthcare system capacity, however there was no declared metric. As a consequence, announcement of the lockdown mandate at the municipality level was unexpected, which is crucial to meet the identifying assumption of the empirical strategy developed and applied in this paper. The lockdown took effect within 1-3 days after the announcement. Municipalities were in lockdown between 6 and 172 days in the first wave and between 3 and 171 days in the second wave.

3.2 Data

I combined data from several sources to examine the effect of lockdown entry and exit on crime and test my hypotheses. Data on the exact dates of lockdown imposition and removal for each municipality from March 14$^{\text{th}}$ 2020 to May 3$^{\text{rd}}$ 2021 was manually gathered by Bhalotra et al. (2024). I continued the process until August 18$^{\text{th}}$ 2021, when the last lockdown was lifted. Figure 1 documents the number of municipalities currently under lockdown between March 2020 and August 2021. I obtained administrative data on COVID-19 infection rates from the Ministry of Science, Technology, Knowledge and Innovation.

For data on crime the source are the Official Statistics of Crimes of Major Social Connotation (DMCS), Domestic Violence (VIF), Incivilities and other facts reported by the Police of Chile to the Ministry of the Interior and Public Security. These statistics account for all criminal acts (police cases) recorded by the police during the period and are composed of formal complaints made by citizens to a police unit after the occurrence of the crime, plus crimes that the police become aware of when making an arrest in flagrante delicto, i.e. while the crime is occurring. I consider five crime groups: the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides. The variables are presented in cases per 100,000 inhabitants at the municipality-monthly

Figure 1: Temporal Dynamics of Lockdown Imposition



Note: Figure 1 shows the number of municipalities currently in lockdown at a given moment of time between March 2020 and August 2021.

level.

Municipality-daily data on mobility is obtained from the mobile phone dataset made available by Pappalardo et al. (2023), covering the period March to September 2020. I also model the data at the municipality-monthly level by using that dataset. I analyze three mobility indices: The Index of Internal Mobility, quantifying the amount of mobility within each municipality of the country; the Index of External Mobility which quantifies the mobility between municipalities; and the Index of Mobility, which takes into consideration both mobility within and between municipalities. All the three indices range in $[0, \infty)$, where a value of 0 indicates no mobility at all. The three indices are normalized with respect to the number of users that reside in the municipality. For example, an Index of Mobility equal to 5 means that the number of movements within, to, or from that municipality is around 5 times higher than the number of users that reside in that municipality.

I use the number of employed individuals in the formal private sector at the municipality-monthly level as a measure to analyze the unemployment mechanism. I obtained this variable from the 20% random sample of affiliated workers to the Unemployment Insurance, data managed by the Pensions Superintendence of Chile. It is important to mention that the Unemployment Insurance's target group are dependent workers from the formal private sector older than 18 years old and regulated by the Labor Code. Therefore, this sample is representative of the total affiliated workers to the Unemployment Insurance, but it cannot be extrapolated to the total population.

4 Empirical Strategy

This section explains how I look to identify dynamic impacts of lockdown entry and exit on five categories of violent crimes in Chile. I analyze the first and second wave of COVID-19 in Chile, where municipalities entered and exited lockdown several times during the pandemic and differed in the timing of the lockdowns imposition and removal. Events of imposing and lifting lockdown provide a great number of natural experiments between March 2020 and July 2021. I identify dynamic impacts, taking into consideration intertemporal and heterogeneous treatment effects using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ).

Denote crime $C_{m,t}$ as the outcome variable and lockdown status $L_{m,t}$. When the treatment is lockdown entry, $L_{m,t}$ takes the value 1 if lockdown is in place in municipality m and time t. When the treatment is lockdown exit, $L_{m,t}$ takes the value 1 if lockdown is not in place in municipality m and time t, given that municipality m has entered lockdown before. For every m, let F_m denote the first period at which municipality's treatment changes, and

$$T_m = \max_{m':L_{m',1}=L_{m,1}} F_{m'} - 1$$

denote the last period where there is still a municipality with the same period-one treatment as m. Also, for all (m, t), let

$$N_t^m = \#\{m' : L_{m',1} = L_{m,1}, F_{m'} > t\}$$

be the number of municipalities m' with the same period-one treatment as m, and that have kept the same treatment from period 1 to t. Now, for every every municipality m such as $F_m \leq T_m$, and every $\ell \in \{1, ..., T_m - F_m + 1\}$, $N^m_{F_m-1+\ell} > 0$. In simple words, for every municipality m such as the first period at which its treatment changes is less than or equal to the last period where there is still a municipality with the same period-one treatment, and for any period where the lockdown can be in place, the number of municipalities m' with the same period-one treatment, and that have kept the same treatment from period 1 to $F_m - 1 + \ell$ is higher than zero. To estimate the actual-versus-status-quo (AVSQ) effect of m at $F_m - 1 + \ell$, I use

$$DID_{m,\ell} = C_{m,F_m-1+\ell} - C_{m,F_m-1} - \frac{1}{N^m_{F_m-1+\ell}} \sum_{m':L_{m',1}=L_{m,1},F_{m'}>F_{F_{m-1+\ell}}} (C_{m',F_m-1+\ell} - C_{m',F_m-1})$$

a DID estimator comparing the $F_m - 1$-to-$F_m - 1 + \ell$ crime evolution of m to that of municipalities with the same baseline treatment, and that have kept that treatment from period 1 to $F_m - 1 + \ell$. For all m such that $F_m \leq T$, let

$$S_m = 1\{L_{m,F_m} > L_{m,1}\} - 1\{L_{m,F_m} < L_{m,1}\}$$

be equal to 1 for municipalities whose lockdown status switches from 0 to 1, and -1 for municipalities whose lockdown status switches from 1 to 0 at F_m. For the whole pandemic period, and the first and second COVID-19 wave, I use

$$DID_\ell = \frac{1}{N_\ell} \sum_{m:F_m-1+\ell \leq T_m} N_{m,t} S_m DID_{m,\ell}$$

to estimate the weighted average effect, among municipalities, of the whole treatment path, ℓ months after the beginning of the first treatment (lockdown entry or exit). The estimates are weighted by municipality population, $N_{s,t}$. I conduct inference using clustered standard errors at the municipality level. First, I estimate dynamic effects of the lockdown imposition for the whole pandemic period, i.e., from March 2020 to July 2021. To do this, I consistently include 16 event-study effects and compute 15 placebo estimators. The number of event-study effects reflects the number of periods during which the impact of the treatment is analyzed. The placebo estimators compare the outcome evolution of switchers and of their controls, before switchers' treatment changes for the first time. Then, I estimate the effects for each COVID-19 wave separately, also including lockdown removal as treatment in addition to lockdown entry. For the first wave, I include 4 event-study effects, since the number of municipalities under lockdown declined after 4 months. In the case of the second wave, as it was more intense in terms of duration, I consider 5 event-study effects[1].

[1]The estimators proposed by de Chaisemartin and D'Haultfoeuille (2023) are computed by the

When the treatment is lockdown removal, estimates at $t = 0$ compare municipalities which exited lockdown between the current and previous period to those that remained under lockdown. Predictors of lockdown timing such as mayor's political party are captured by municipality fixed effects. Effect of school closure are absorbed by time fixed effects, since schools closed in the whole country on March 16 2020. By definition, the DID_ℓ estimator purges municipality and time fixed effects. I present the results without including controls and then I discuss results controlling for COVID-19 infection rates.

The identifying assumption of this empirical strategy is that the timing of lockdown mandates is conditionally random upon time and municipality fixed effects. As defined before, placebo estimators compare the outcome evolution of switchers and of their control groups, before switchers' treatment changes for the first time. de Chaisemartin and D'Haultfœuille (2023) show that placebos can be used to inspect the no-anticipation and parallel trends assumptions by testing the null hypothesis that all placebos are jointly equal to zero. Except for homicides, the rejection rate of that joint test is 1% or less for the crime outcomes. These results might suggest the violation of the parallel trends and no-anticipation assumptions of the underlying event-study estimators.

Unlike the commonly-used two-way-fixed-effects approach, the DID_ℓ estimator is robust to heterogeneous treatment effects across groups and over time. The reason I use the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ) instead of de Chaisemartin and D'Haultfœuille (2020) estimator (DID_M), as in Bhalotra et al. (2024), is that when lagged treatments can affect the outcome and the treatment may turn on and off, the DID_M estimator may not obtain unbiased estimates of the impact of the contemporaneous treatment on the outcome.

`did_multiplegt_dyn` Stata (see de Chaisemartin, Ciccia, D'Haultfoeuille, Knau, Malézieux and Sow, 2024b) and R (see de Chaisemartin, Ciccia, D'Haultfoeuille, Knau, Malézieux and Sow, 2024a) commands.

5 Dynamic Effects of Lockdown Mandates on Crime

5.1 Whole Pandemic Period

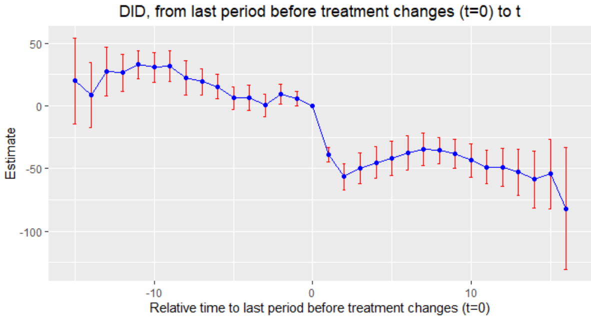
Figure 2 presents the dynamic effects estimates of the lockdown imposition for the five crime outcomes during the whole pandemic period. I include 16 event-study effects and compute 15 placebos. Estimates show a significant decrease in police cases following lockdown entry for the five variables of violent crime. The estimated weighted average effects 16 months after the beginning of the first lockdown reveal a decrease of 103.11, 20.98, 39.40, 3.19, and 0.49 cases per 100,000 inhabitants, respectively for DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides.

Figure 2 also shows the placebo estimators comparing the evolution of violent crime of switchers and of their controls, before switchers' lockdown was imposed for the first time. The joint tests of the null that all placebos are equal to zero compute p-values of 1% or less for DMCS, robbery, robbery with violence and intimidation, and vehicle theft. These results might suggest the violation of parallel trends and no-anticipation assumptions of the underlying event-study estimators, indicating that the estimators for these types of crime may be biased. Meanwhile, the p-value of the F-test for homicides is larger than 10%, suggesting the compliance of parallel trends and no-anticipation assumptions for this type of violent crime.

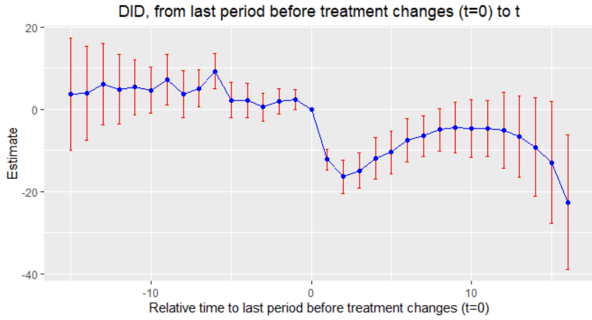
Given that there are periods during which there is no switcher or no control group, the dynamic effects estimates of the lockdown exit cannot be computed for the whole pandemic period. The interruption of this treatment and the differences between the first and second wave regarding infection and mortality rates make it plausible to analyze each wave separately.

Figure 2: Dynamic Impacts of Lockdown Imposition on Violent Crime

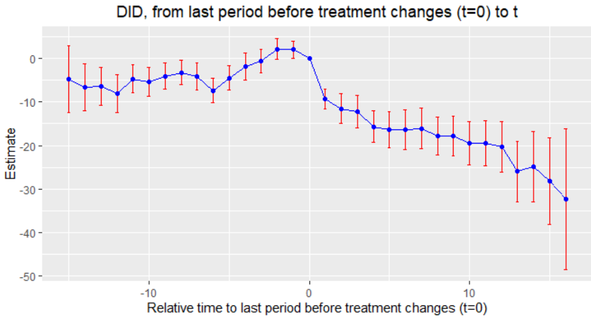
(a) DMCS



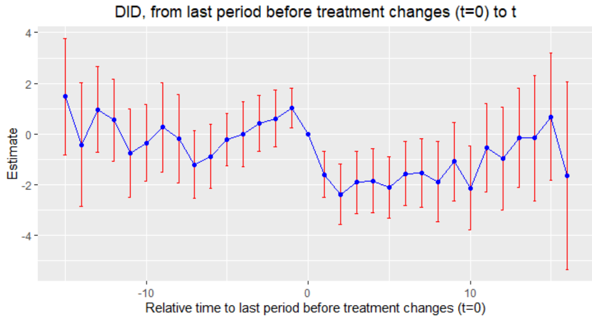
(b) Robbery



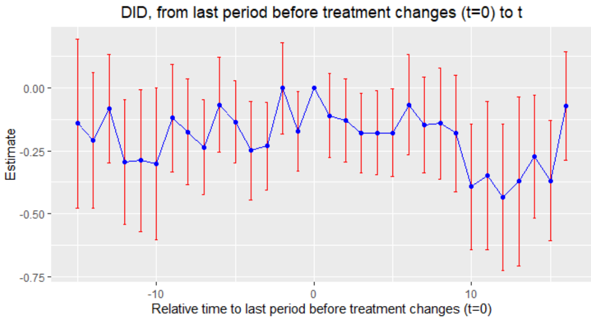
(c) Robbery with Violence and Intimidation



(d) Vehicle Theft



(e) Homicides



Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 16 event-study effects and compute 15 placebo estimators. Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. The estimation sample contains all municipalities at risk of lockdown entry. Estimations do not include controls. Specifications conditional on control for COVID-19 infection rates are discussed in Section 6. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

5.2 First and Second Wave

5.2.1 First COVID-19 wave

Panel A of Table 1 presents the estimates of the weighted average effect of the whole treatment path for the first COVID-19 wave. The first row shows a negative effect in criminal activity following lockdown imposition for the five outcome variables. In terms of magnitude, the estimated weighted average effects 4 months after the first lockdown entry reveal a respective decrease of 73.67, 20.39, 21.42, 4.03, and 0.25 cases per 100,000 inhabitants for DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides. All the coefficients are statistically significant at 5% level.

The second row of Panel A exhibits an expected increase in criminal activity when the treatment is lockdown removal. The dynamic effects 4 months after the first lockdown exit show a raise of 46.88, 10.36, 11.60, 3.19, and 0.15 cases per 100,000 inhabitants, respectively for DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides. Only the coefficient for homicides is not statistically significant. Although Panel A presents expected signs for the coefficients of lockdown entry and exit, the magnitudes of these effects suggest that the negative effect on crime of the lockdown imposition is higher than the rise in criminal activity after the lockdown is lifted.

5.2.2 Second COVID-19 wave

Panel B shows the estimates of the weighted average effect for the second COVID-19 wave. Similarly to the coefficients for the first wave, the estimates in Panel B present the expected negative signs when lockdown is imposed (except for vehicle theft) and positive signs when the lockdown is lifted. The first row of Panel B exhibits a decline of the magnitude for all the coefficients compared to the effect of lockdown entry in the first wave. Moreover, only the coefficients for DMCS and robbery with violence and intimidation are statistically significant. For the second wave, the estimated weighted average effects 5 months after the first lockdown entry show a decrease of 25.48 cases per 100,000 inhabitants for DMCS and a reduction of 4.20 cases for robbery with violence and intimidation. These results illustrate that the reduction of violent crime due to lockdown imposition was predominantly larger during the first wave.

Table 1: Impacts of Lockdown Entry and Exit on Crime for each COVID-19 wave

	(1) DMCS	(2) Robbery	(3) Robbery VI	(4) Vehicle theft	(5) Homicides
Panel A: First Wave					
Lockdown entry	-73.67***	-20.39***	-21.42***	-4.03***	-0.25*
	(6.03)	(2.45)	(2.29)	(0.84)	(0.10)
Lockdown exit	46.88***	10.36**	11.60***	3.19**	0.15
	(14.09)	(4.00)	(3.16)	(1.05)	(0.14)
Panel B: Second Wave					
Lockdown entry	-25.48**	-2.37	-4.20*	1.78	-0.21
	(9.47)	(6.12)	(2.01)	(1.19)	(0.61)
Lockdown exit	58.67***	12.72*	2.05	0.78	0.10
	(11.41)	(5.41)	(2.51)	(1.05)	(0.60)
Controls	No	No	No	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_{ℓ}). Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. For lockdown entry, the sample contains all municipalities at risk of lockdown imposition. For lockdown exit, the sample considers all municipalities that have entered lockdown and hence are at risk of exit. For the first wave, I include 4 event-study effects. In the case of the second wave, I consider 5 event-study effects. Estimations do not include controls. Specifications conditional on control for COVID-19 infection rates are discussed in Section 6. I conduct inference using clustered standard errors at the municipality level.

The second row of Panel B also shows an expected increment in crime when lifting lockdown. For the second wave, the dynamic effects 5 months after the first lockdown exit present an increase of 58.67, 12.72, 2.05, 0.78, and 0.10 cases per 100,000 inhabitants, respectively for DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides. However, only the coefficients for DMCS and robbery are statistically significant. For these two type of crimes the positive effect of lockdown exit is significantly higher than the negative effect of lockdown entry throughout the second COVID-19 wave. Furthermore, the impact of lockdown removal in the second wave is relatively higher than this effect during the first wave for DMCS and robbery, but it does not apply for the other categories of crime.

6 Mechanisms

In this section, I explicitly investigate potential mechanisms behind the changes in violent crime after lockdowns are imposed and removed. To do this, I estimate dynamic impacts of lockdown entry and exit on mobility and employment outcomes.

6.1 Mobility

Figure 3 presents the dynamic effects estimates of the lockdown imposition and removal for the three mobility indices during the first COVID-19 wave at the municipality-daily level. Panel A shows the results considering lockdown imposition as treatment, while Panel B analogously does it for lockdown removal. For these estimates at the daily level, I consistently include 25 event-study effects[2]. Panel A reveals that the estimated dynamic effects show a significant decrease in the number of movements following lockdown entry for the three mobility indices. The estimated weighted average effects 25 days after the beginning of the first lockdown episode present a reduction of 1.39, 0.80, and 0.59, respectively in the magnitude for the Index of Mobility, Index of Internal Mobility, and Index of External Mobility. On the other hand, the average cumulative effects per treatment unit of lockdown lifting are positive and statistically significant for the three measures of mobility, with respective magnitudes of 0.44, 0.10, 0.34, for the Index of Mobility, Index of Internal Mobility, and Index of External Mobility.

Figure 3 also shows the placebo estimators comparing the evolution of the three mobility indices of the treatment groups (switchers) and of their controls (non-switchers), one week before the first lockdown was imposed (Panel A) and removed (Panel B). The joint tests of the null that all placebos are equal to zero compute p-values of around 0% for the Index of Mobility, Index of Internal Mobility, and Index of External Mobility. These results might suggest the non-compliance of both the no-anticipation and parallel trends

[2]The reason I include only 25 event-study effects and no more is due to limitations of the R and STATA command combined with processing limitations

assumptions of the underlying event-study estimators for the three mobility variables and for the two treatments.

Figure A1 presents the dynamic effects estimates of the lockdown imposition and removal for the three mobility indices during the first COVID-19 wave at the municipality-monthly level. Panel A of Figure A1 presents the estimates for lockdown entry, while Panel B considers lockdown exit as treatment. I consistently include 4 event-study effects, following the results provided in Section 5. The estimated weighted average effects 4 months after the beginning of the first lockdown imposition reveal a decrease of 18.71, 12.68, and 6.02, respectively for the Index of Mobility, Index of Internal Mobility, and Index of External Mobility. Meanwhile, the dynamic effects 4 months after the first lockdown exit presented in Panel B of Figure A1 shows a raise of 31.38, 28.55, and 2.83 respectively for Index of Mobility, Index of Internal Mobility, and Index of External Mobility.

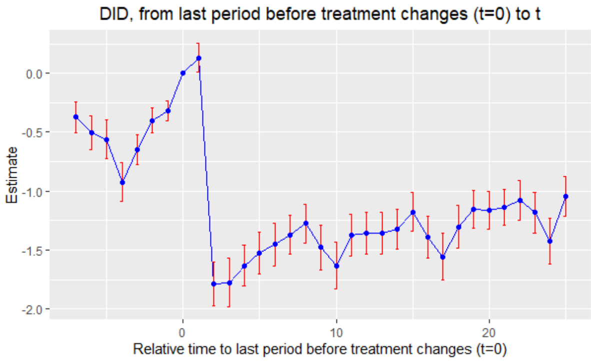
The estimated dynamics effects of both lockdown entry and exit on mobility outcomes detailed in this section line up with the main results presented in Section 5. Therefore, these results are consistent with mobility as a mechanism through which lockdown impacted violent crime in Chile.

Figure 3: Dynamic Impacts of Lockdown Imposition and Removal on Mobility

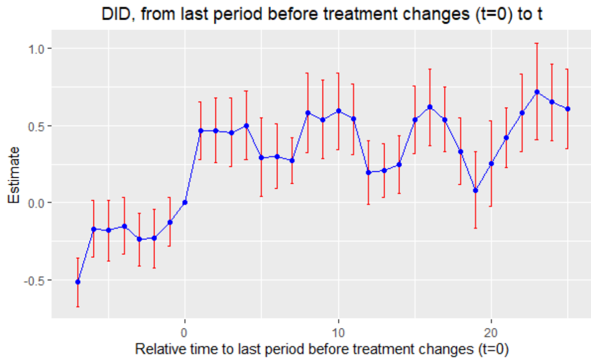
Panel A: Lockdown Entry

Panel B: Lockdown Exit

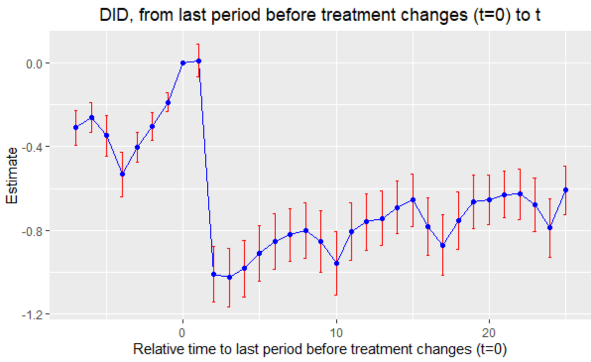
(a) Index of Mobility



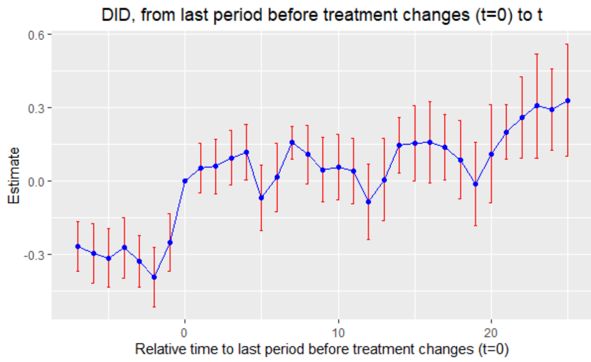
(b) Index of Mobility



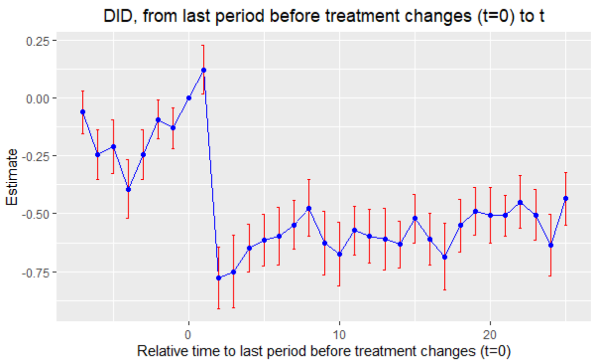
(c) Index of Internal Mobility



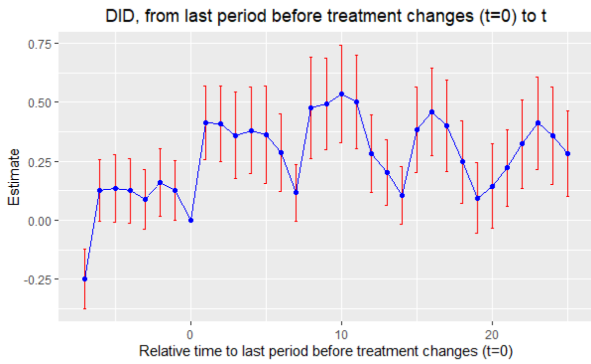
(d) Index of Internal Mobility



(e) Index of External Mobility



(f) Index of External Mobility

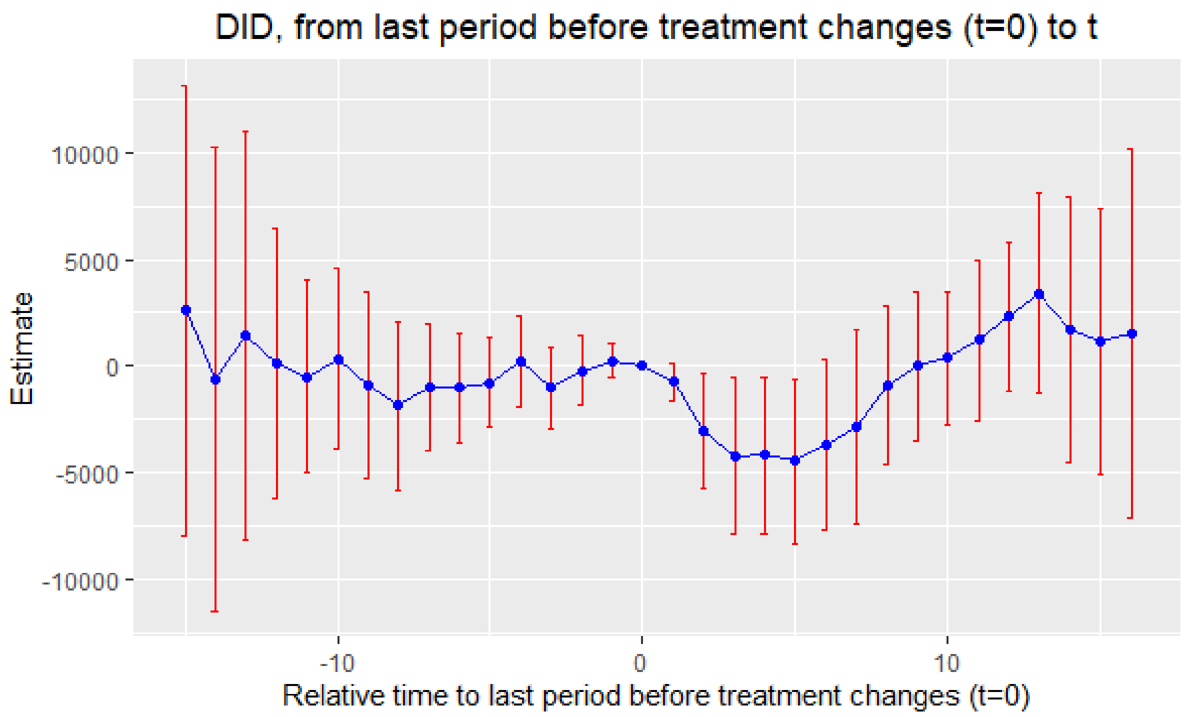


Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 25 event-study effects. I compute 7 placebo estimators when the treatment is lockdown entry. Outcome variables are Index of Mobility, Index of Internal Mobility and Index of External Mobility at the municipality-daily level. For Panel A, the treatment is lockdown entry and the estimation sample contains all municipalities at risk of lockdown entry. For Panel B, the treatment is lockdown exit and the sample considers all municipalities that have entered lockdown and hence are at risk of exit. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

6.2 Unemployment

Figure 4 presents the estimated dynamic effects of the lockdown imposition for the number of employed individuals in the formal private sector during the whole pandemic period. I include 16 event-study effects and 15 placebos. The estimated weighted average effects 16 months after the imposition of the first lockdown entry reveal a decrease of -4,753.91 employed individuals. Results show a few statistically significant event-study effects estimates between t=2 and t=5. Nevertheless, it is important to highlight that the average cumulative (total) effect 16 months after the first lockdown entry is not statistically significant.

Figure 4: Dynamic Impacts of Lockdown Entry on the Number of Employed Individuals in the Formal Private Sector - Whole Pandemic Period



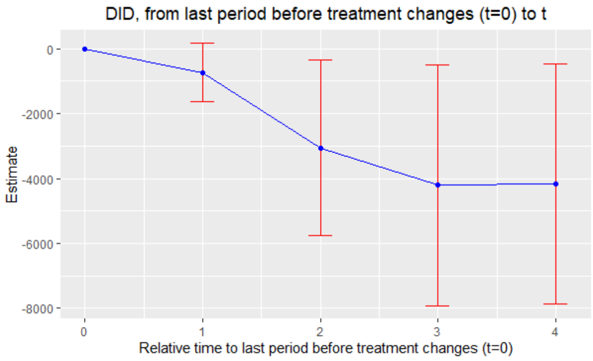
Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 16 event-study effects and compute 15 placebo estimators. Outcome variable is number of employed individuals in the formal private sector at the municipality-monthly level. The estimation sample contains all municipalities at risk of lockdown entry. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure 5: Dynamic Impacts of Lockdown Entry and Exit on the Number of Employed Individuals in the Formal Private Sector - First and Second COVID-19 Wave

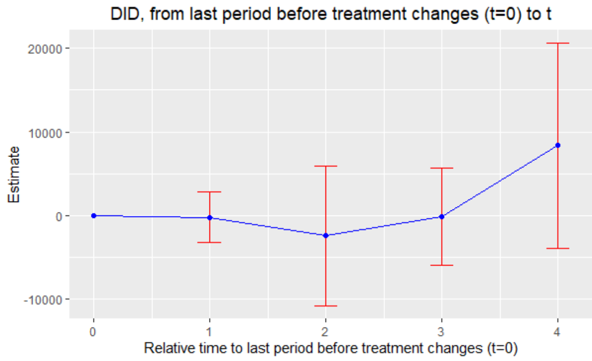
Panel A: Lockdown Entry

Panel B: Lockdown Exit

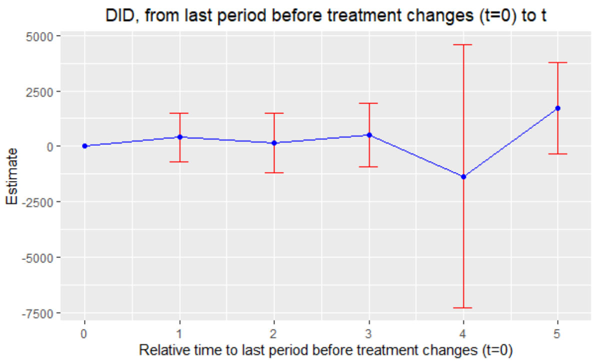
(a) First COVID-19 Wave



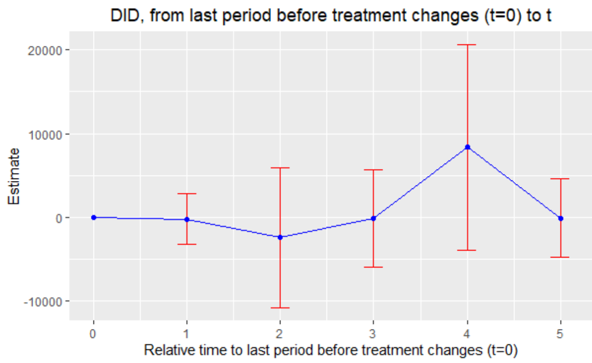
(b) First COVID-19 Wave



(c) Second COVID-19 Wave



(d) Second COVID-19 Wave



Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). Outcome variable is number of employed individuals in the formal private sector at the municipality-monthly level. For Panel A, the treatment is lockdown entry and the estimation sample contains all municipalities at risk of lockdown entry. For Panel B, the treatment is lockdown exit and the sample considers all municipalities that have entered lockdown and hence are at risk of exit. For the first wave, I include 4 event-study effects. In the case of the second wave, I consider 5 event-study effects. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure 4 also shows the placebo estimators comparing the evolution of the employment outcome for switchers and of their controls, before switchers' lockdown was imposed for the first time. The joint test of the null hypothesis that all placebos are equal to zero calculates a p-value larger than 10%. This means that I cannot reject the null hypothesis that all placebos are jointly equal to zero. This result might suggest the compliance of the parallel trends and no-anticipation assumptions of the underlying event-study estimators.

Analogously, Figure A2 in the Appendix presents the estimated dynamic effects of the lockdown imposition for the number of employed individuals in the formal private sector divided by the working age population during the whole pandemic period. The results are similar to the estimates presented in Figure 4: The estimated weighted average effects are not statistically significant and the test of joint nullity of the placebos computes a p-value larger than 30%.

Figure 5 shows the dynamic effects estimates of the lockdown entry and exit for the number of employed individuals in the formal private sector during the first and second COVID-19 wave. Panel A presents the results taking into consideration lockdown imposition as treatment, while Panel B analogously does it for lockdown exit. I consistently include 4 event-study effects for estimates of the first COVID-19 wave (Graphs (a) and (b)) and 5 event-study effects for the dynamic effects of the second wave (Graphs (c) and (d)). Graph (a) shows a negative and significant average cumulative effect per treatment unit 4 months after the beginning of the first lockdown of 5,242.09 employed individuals. In other words, this result reflects the decrease in employment after the lockdown imposition during the first COVID-19 wave predicted by the literature. On the other hand, the estimated dynamic effects on the number of employed individuals presented in the other 3 graphs reveal estimates that are not statistically significant.

Figure A3 in the Appendix presents the dynamic effects estimates of lockdown entry and exit for the number of employed individuals in the formal private sector over the working age population during the first and second COVID-19 wave. None of the estimated weighted average effects are statistically significant.

Apart from the negative and significant average cumulative effect presented in Graph (a) of Figure 5, all the estimated dynamic effects of lockdown mandates on the employment outcomes are not statistically significant. Therefore, these results are not consistent with unemployment being a mechanism through which lockdown impacted violent crime in Chile during the COVID-19 pandemic.

7 Robustness Checks

To analyze the sensitivity of my estimates, I conduct three robustness checks. First, for a further validation of the no-anticipation and parallel trends assumptions, I perform the estimation of the effects of lockdown entry for the whole pandemic period, but taking the year 2018 as the reference year to compute the placebo estimators instead of 2019. In October 2019, social uprising, know as the 'Estallido Social', began in Chile and civil protests took place throughout Chile. To avoid the possible relationship between social unrest and crime rates, I conduct the estimations and obtain the placebos using the year 2018. Analogously to Figure 2, Figure A4 shows the dynamic effects estimates of the lockdown entry for the five crime outcomes during the whole pandemic period taking the year 2018 as a reference year. The joint tests of the null that all placebos are equal to zero compute p-values of less than 0.1% for DMCS, robbery with violence and intimidation, vehicle theft, and homicides. These results suggest the compliance of parallel trends and no-anticipation. Meanwhile, the p-value of the F-test for robbery is around 8%.

Second, I perform analogous estimations as in Figure 2 and Table 1, but now controlling for COVID-19 infection rates Figure A5 presents the dynamic effects estimates of the lockdown imposition for the crime outcomes during the whole pandemic period including COVID-19 infection rates as control variable. I consistently include 16 event-study effects and compute 8 placebo estimators. Except for vehicle theft, all the coefficients present a relatively smaller magnitude than the estimates without controls.

Table A1 shows the dynamic effects estimates of the lockdown entry and exit on the five crime outcomes for each COVID-19 wave separately and controlling for COVID-19 infection rates. All the coefficients maintain the sign and statistical significance. For the first wave, including infection rates as control leads to relatively smaller dynamic effects of lockdown entry and exit, except for robbery with violence and intimidation and vehicle theft, which effects increase when the treatment is lockdown exit. For the second wave, effects of lockdown imposition are relatively smaller when including the control variable, except for robbery with violence and intimidation. When the treatment is lockdown exit, the magnitude of the effect on DMCS is slightly larger and of the effect on robbery is slightly smaller than the specification without controls.

Third, I perform the analysis restricting the sample to only the Metropolitan Region of Santiago in order to check if municipalities of this region drive the results. This region concentrates around the 40% of the country's population and roughly the 40% of total violent criminal cases. I only analyze the first wave, since all the municipalities of this region entered to lockdown at the same time during the second wave, therefore, there are no switchers or control group. Table A2 present estimates for municipalities of the Metropolitan Region with and without controlling for COVID-19 infection rates. All the coefficients are statistically significant, except for homicides when the treatment is lockdown entry. For the effect of lockdown imposition, the magnitude of the effect on DMCS and robbery is around 50% smaller than the estimations with the national sample and almost the same for robbery with violence and intimidation and vehicle theft. When the treatment is lockdown exit, the magnitudes for DMCS and robbery with violence and intimidation are almost the same than for the national sample. Magnitudes for robbery, vehicle theft, and homicides are significantly higher than the analogous effects with the main sample.

8 Limitations

The data used in this study presents some limitations. As most papers, this study uses monthly data crime, which might not reveal within-month dynamics. For example, there are a few municipalities that entered and exited lockdown (or exited and entered) during the same month. The effect of these multiple shifts within the same month is not captured by the database. Moreover, governments and local authorities might have implemented different policies that could affect criminal activity during the same time span. Access to high-frequency data, e.g., at the criminal report level, would allow me to isolate an immediate effect of lockdown mandates on crime as in Poblete-Cazenave (2024).

Data of the Official Statistics of Crimes of Major Social Connotation (DMCS) consider criminal offenses recorded by the police. These are composed of formal complaints made by citizens to a police unit after the occurrence of the crime, plus crimes that the police becomes aware of when making an arrest while the crime act is occurring. Nevertheless, it is important to consider that the database does not contain criminal acts that occurred but were not denounced or reported by the police. Thus, the total number of police cases is likely to be underestimated. This matter is more relevant when taking into consideration that a great number of cases might have not been reported or might have been reported time after the offense was committed in the context of COVID-19. Data that uses the date of the crime incident, rather than the date when the crime was recorded by the police may be useful in order to capture the impact of the lockdown mandate on the incidence of crime.

The analysis of the mechanisms also presents limitations. Regarding the mobility mechanism, the dataset made available by Pappalardo et al. (2023) covers until September 2020, therefore it is not possible to extend the analysis to the second COVID-19 wave. On the other hand, several precautions and concerns must be taken when analyzing and interpreting employment data. First, as mentioned in Section 3.2, the Unemployment Insurance's target group are dependent workers from the formal private sector older than 18 years old and regulated by the Labor Code. It means that this sample excludes workers with an apprenticeship contract; workers younger than 18 years old; domestic workers; pensioners; independent workers; and public sector personnel. For this reason, this sample is representative of the total affiliated workers to the Unemployment Insurance, but it can not be extrapolated to the total population. Second, due to limitations of the data, the municipality that is taking into consideration to perform the analysis is the employer's municipality and not the employee's one. The assumption in this case is that the employee works in the same municipality that he or she resides. This is a strong assumption considering that in Chile a great part of the labor force travel to other municipalities to get to their jobs. Third, in this database the number of workers is expressed as a range and not as an exact number, therefore I use the mark class as value to conduct the analysis. For this reason, the monthly variation of the employment might not be precisely captured.

9 Conclusions

This master thesis studies the impact of lockdown imposition and removal on violent crime during the first and second wave of COVID-19 in Chile. I exploit the fact that municipalities entered and exited lockdown several times during the pandemic and differed in the timing of imposing and lifting lockdown. Using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ), I identify dynamic impacts, taking into consideration intertemporal and heterogeneous treatment effects.

Estimates for the whole pandemic period show a significant decrease in police cases following lockdown entry for the five variables of violent crime. Placebo estimators do not suggest the compliance of parallel trends and no-anticipation assumptions of the underlying event-study estimators, except for homicides. During each COVID-19 wave, estimates show an expected negative sign when lockdown is imposed and positive sign when the lockdown is lifted. For the first wave, the results indicate that the negative effect on crime of the lockdown imposition is higher than the rise in criminal activity after the lockdown is lifted. Moreover, the results illustrate that the reduction of violent crime due to lockdown entry was predominantly larger during the first wave. In the second wave, the positive effect of lockdown exit is significantly higher than the negative effect of lockdown entry for DMCS and robbery. This positive effect is relatively higher than the effect during the first wave, but it does not apply for the other categories of crime.

The estimated dynamics effects of both lockdown entry and exit on mobility outcomes line up with the main results presented in Section 5. Therefore, these results are consistent with mobility as a mechanism through which lockdown impacted violent crime in Chile. Nevertheless, dynamic effects estimates of lockdown mandates on the employment outcomes are not statistically significant. These results are not aligned with unemployment being a mechanism through which lockdown impacted violent crime in Chile during the COVID-19 pandemic.

The results in this study deepen the understanding about how crime was affected during the COVID-19 pandemic and the drivers behind this impact. Nevertheless, they do not capture medium- or long-term effects after the end of the last lockdown exit. Crime, and especially violent crime, was reconfigured after COVID-19 in Latin America, leading to a climate of insecurity and new challenges in public safety. Future research should focus on further addressing the root causes of crime, which is crucial to strengthen the public safety policy and ensure citizen security.

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Appendix

A Tables and Figures

Table A1: Impacts of Lockdown Entry and Exit on Crime Controlling for Infection Rates

	(1)	(2)	(3)	(4)	(5)
	DMCS	Robbery	Robbery VI	Vehicle theft	Homicides
Panel A: First Wave					
Lockdown entry	-58.46***	-13.99***	-20.47***	-2.99***	-0.22*
	(6.08)	(2.87)	(2.22)	(0.84)	(0.10)
Lockdown exit	44.69**	9.42*	12.03***	3.39**	0.17
	(13.72)	(3.90)	(3.19)	(1.07)	(0.14)
Panel B: Second Wave					
Lockdown entry	-20.53*	-0.15	-4.93*	1.26	-0.06
	(9.48)	(6.17)	(2.00)	(1.19)	(0.61)
Lockdown exit	60.28***	12.21*	3.47	1.22	-0.03
	(12.24)	(5.51)	(2.59)	(1.08)	(0.60)
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. For lockdown entry, the sample contains all municipalities at risk of lockdown imposition. For lockdown exit, the sample considers all municipalities that have entered lockdown and hence are at risk of exit. For the first wave, I include 4 event-study effects. In the case of the second wave, I consider 5 event-study effects. Estimations control for COVID-19 infection rates. I conduct inference using clustered standard errors at the municipality level.

Table A2: Impacts of Lockdown Entry and Exit on Crime for Metropolitan Region

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lockdown entry	-39.85***	-31.97***	-9.85**	-11.15*	-21.74***	-19.97***	-3.89*	-4.92**	-0.02	-0.18
	(7.79)	(8.29)	(3.82)	(4.36)	(3.28)	(3.25)	(1.56)	(1.69)	(0.14)	(0.16)
Lockdown exit	45.89**	47.26***	14.87***	15.50***	11.30**	10.92**	6.86***	6.75***	1.18***	1.21***
	(14.39)	(14.30)	(4.47)	(4.62)	(3.61)	(3.68)	(0.94)	(0.94)	(0.14)	(0.14)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

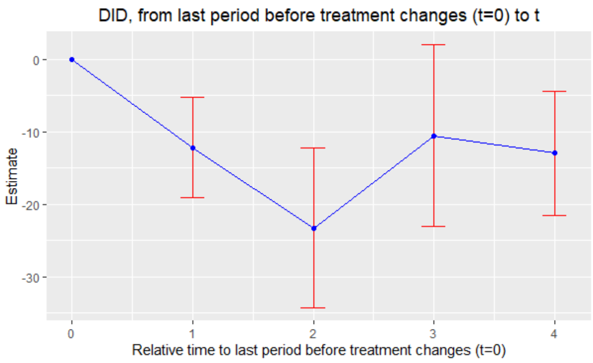
Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS (columns (1)-(2)), robbery (columns (3)-(4)), robbery with violence and intimidation (columns (5)-(6)), vehicle theft (columns (7)-(8)), and homicides (columns (9)-(10)) at the municipality-monthly level. For lockdown entry, the sample contains all municipalities at risk of lockdown imposition. For lockdown exit, the sample considers all municipalities that have entered lockdown and hence are at risk of exit. I include 4 event-study effects. Estimations include and do not include controls. I conduct inference using clustered standard errors at the municipality level.

Figure A1: Dynamic Impacts of Lockdown Imposition and Removal on Mobility at the Monthly Level

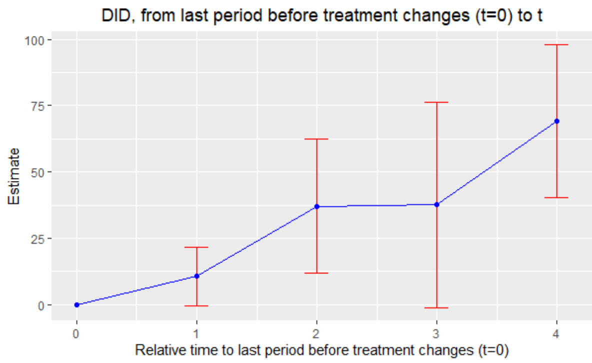
Panel A: Lockdown Entry

Panel B: Lockdown Exit

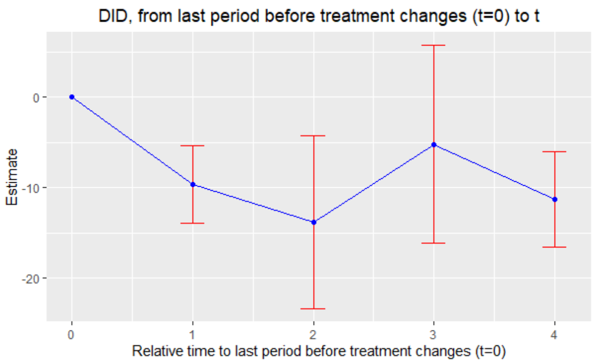
(a) Index of Mobility



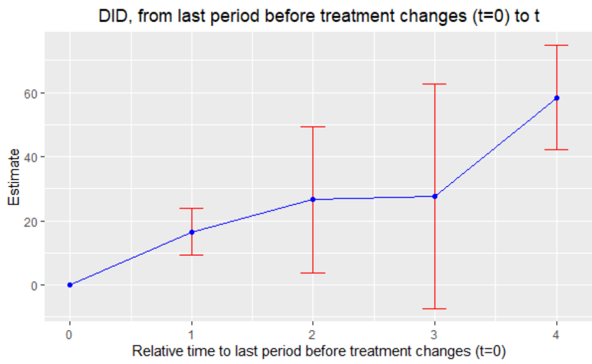
(b) Index of Mobility



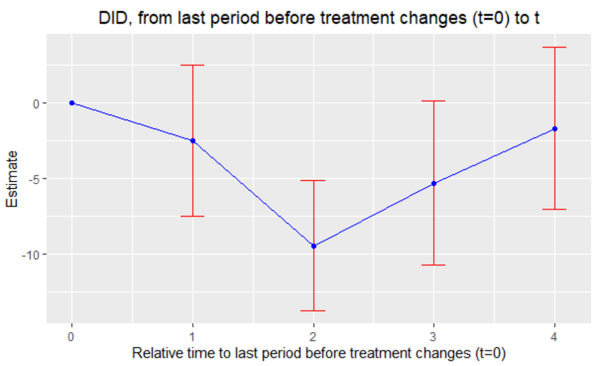
(c) Index of Internal Mobility



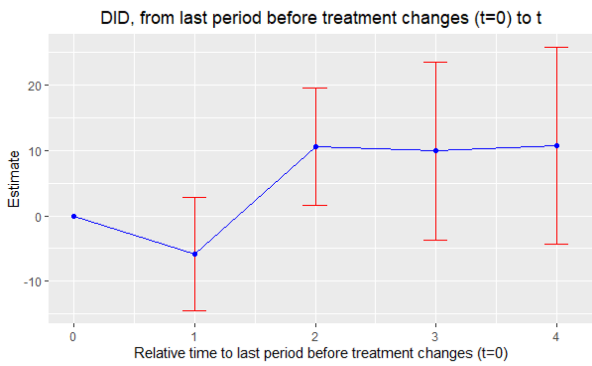
(d) Index of Internal Mobility



(e) Index of External Mobility

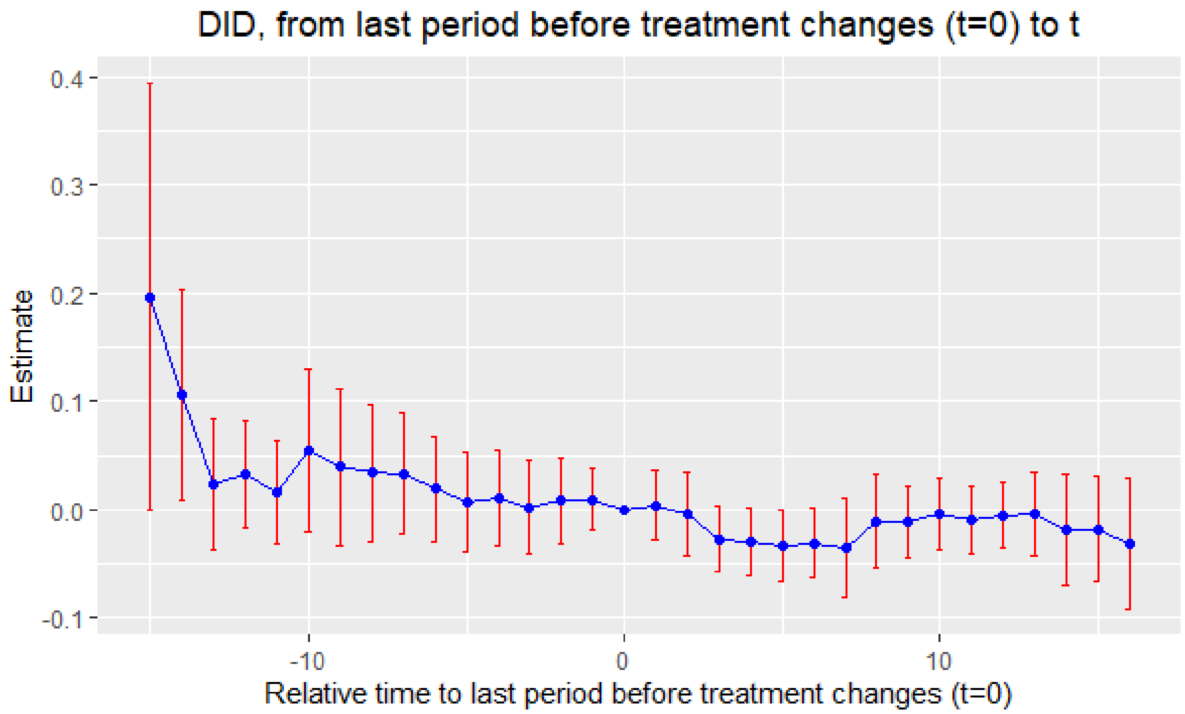


(f) Index of External Mobility



Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 4 event-study effects. Outcome variables are Index of Mobility, Index of Internal Mobility and Index of External Mobility at the municipality-monthly level. For Panel A, the treatment is lockdown entry and the estimation sample contains all municipalities at risk of lockdown entry. For Panel B, the treatment is lockdown exit and the sample considers all municipalities that have entered lockdown and hence are at risk of exit. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure A2: Dynamic Impacts of Lockdown Entry on the Number of Employed Individuals in the Formal Private Sector Over the Working Age Population - Whole Pandemic Period



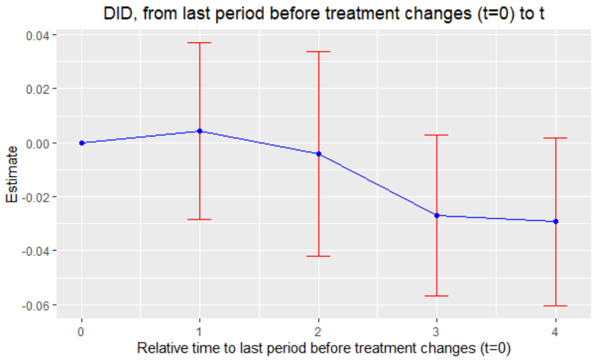
Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 16 event-study effects and compute 15 placebo estimators. Outcome variable is number of employed individuals in the formal private sector divided by the working age population at the municipality-monthly level. The estimation sample contains all municipalities at risk of lockdown entry. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure A3: Dynamic Impacts of Lockdown Entry and Exit on the Number of Employed Individuals in the Formal Private Sector Over the Working Age Population - First and Second COVID-19 Wave

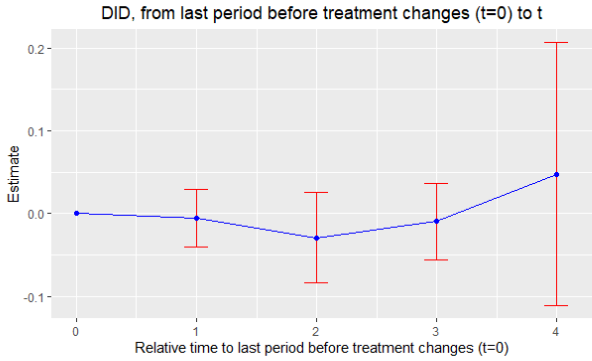
Panel A: Lockdown Entry

Panel B: Lockdown Exit

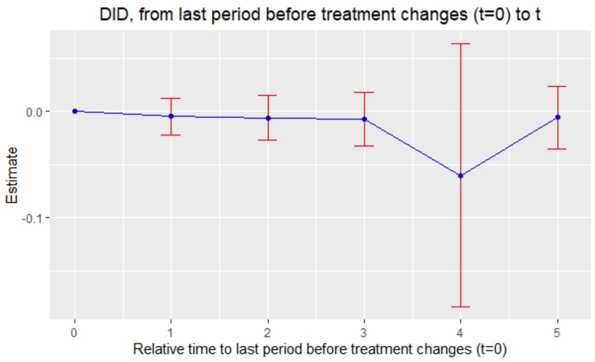
(a) First COVID-19 Wave



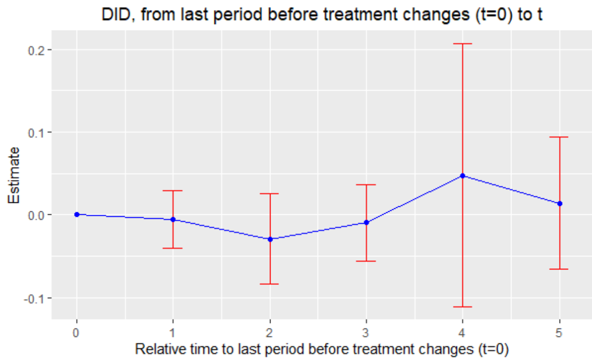
(b) First COVID-19 Wave



(c) Second COVID-19 Wave



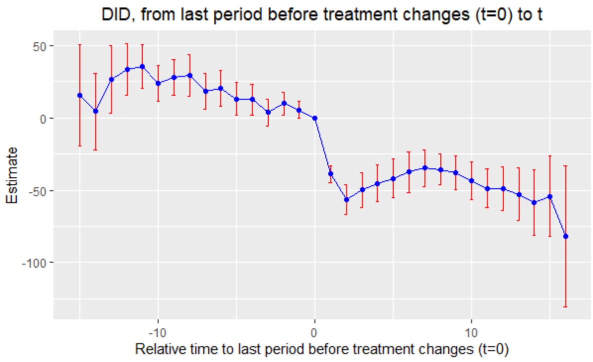
(d) Second COVID-19 Wave



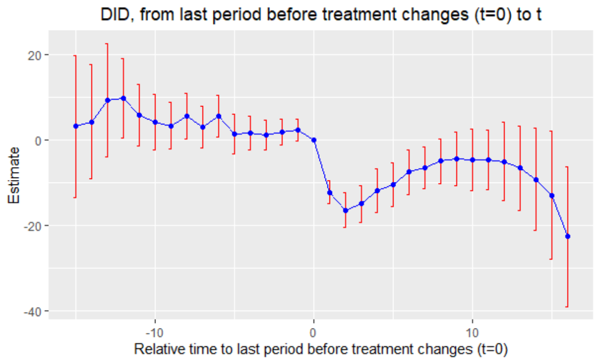
Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). Outcome variable is number of employed individuals in the formal private sector divided by the working age population at the municipality-monthly level. For Panel A, the treatment is lockdown entry and the estimation sample contains all municipalities at risk of lockdown entry. For Panel B, the treatment is lockdown exit and the sample considers all municipalities that have entered lockdown and hence are at risk of exit. For the first wave, I include 4 event-study effects. In the case of the second wave, I consider 5 event-study effects. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure A4: Impact of Lockdown Imposition on Violent Crime - 2018 Comparison Year

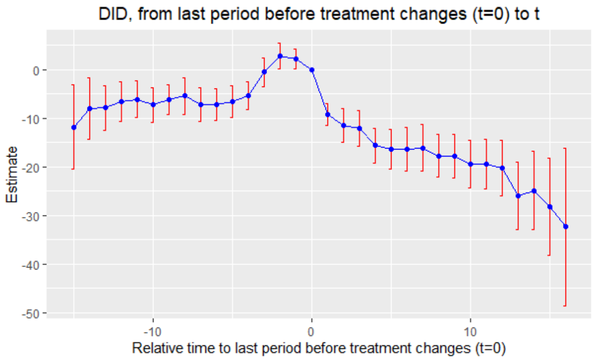
(a) DMCS



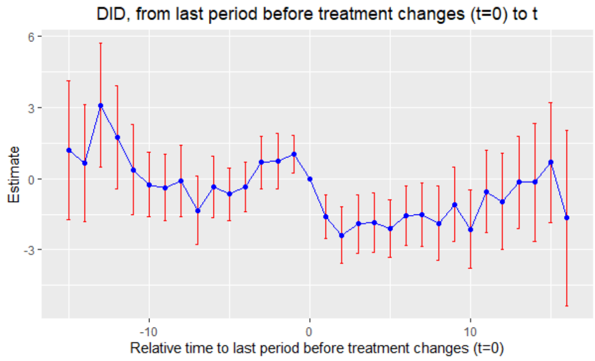
(b) Robbery



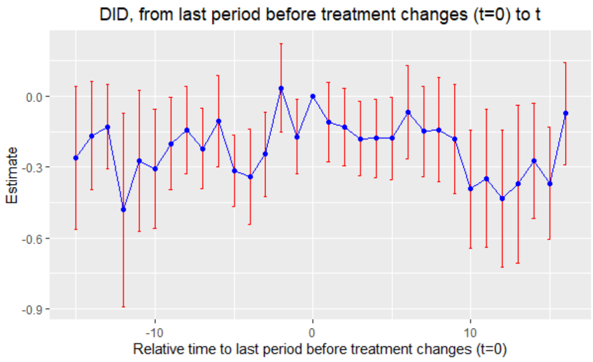
(c) Robbery with Violence and Intimidation



(d) Vehicle Theft



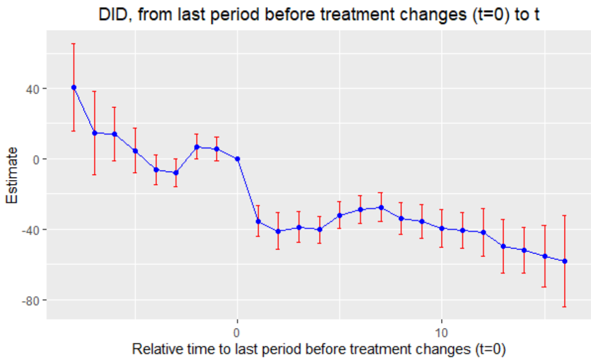
(e) Homicides



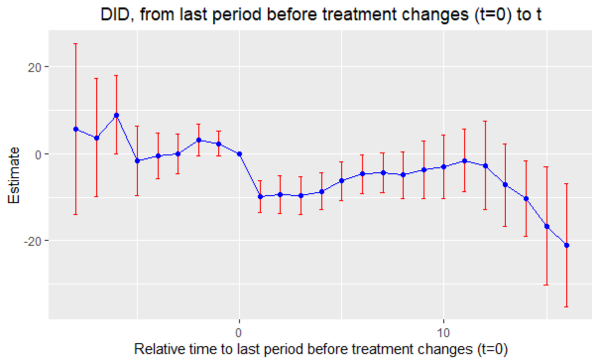
Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_ℓ). I consistently include 16 event-study effects and compute 15 placebo estimators. Comparison year is 2018. Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. The estimation sample contains all municipalities at risk of lockdown entry. Estimations do not include controls. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.

Figure A5: Impact of Lockdown Entry on Violent Crime Controlling for Infection Rates

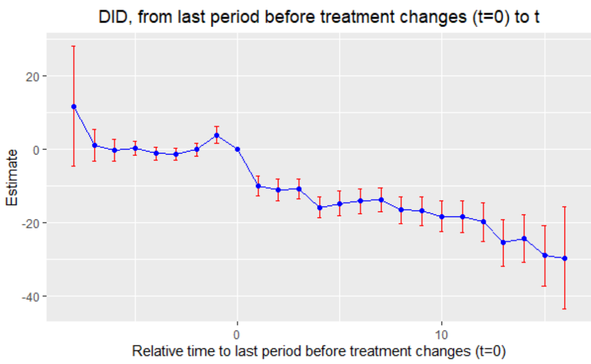
(a) DMCS



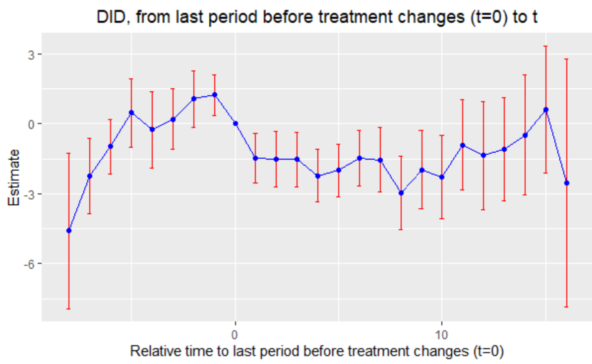
(b) Robbery



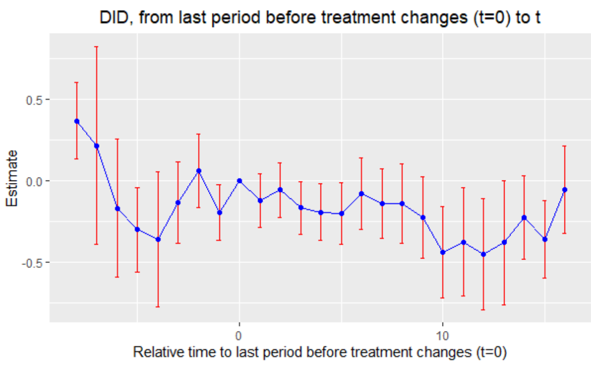
(c) Robbery with Violence and Intimidation



(d) Vehicle Theft



(e) Homicides



Notes: Dynamic effects estimates are obtained using the de Chaisemartin and D'Haultfœuille (2023) estimator (DID_{ℓ}). I consistently include 16 event-study effects and compute 8 placebo estimators. Outcome variables are police cases per 100,000 inhabitants of the aggregated DMCS, robbery, robbery with violence and intimidation, vehicle theft, and homicides at the municipality-monthly level. The estimation sample contains all municipalities at risk of lockdown entry. Estimations control for COVID-19 infection rates. I conduct inference using clustered standard errors at the municipality level. Confidence intervals are displayed at a 95% level.