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Isolating the incapacitative effect of social distancing on crime: Evidence from Ecuador's Covid-19 lockdown*

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Abstract

Identifying the impact of incapacitation measures on crime, such as imprisonment or curfews, is challenging since any such intervention simultaneously dissuades from engaging in illegal behaviour. We exploit Covid-19 confinement measures as a quasi-experiment to isolate incapacitative from deterrent effects of mobility restrictions in a developing country, Ecuador. Difference-in-differences and event-study estimates show a significant reduction in violent and property crime, relative to comparable months in pandemic-free years. While the fall in violent crime is driven by rape cases, we observe no cross-crime substitution for property crime. Heterogeneity effect analysis indicates that the composite decline in violent crime is entirely attributed to incapacitation. In contrast, the drop in property crime is attenuated in provinces where the economic activity mainly relies on essential sectors and blue-collar occupations, leaving incapacitation to explain 40 to 50% of the composite decrease.

JEL classifications: I18, I19, K42

Keywords: Crime, Incapacitation, Deterrence
Non-Pharmaceutical Interventions, Covid-19
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1 Introduction

Incapacitation measures, such as imprisonment or curfews, are prevalent tools used to reduce crime. They mechanically impede convicted felons committing crime by limiting their mobility, and deter potential offenders through a threat of punishment. Establishing whether these interventions decrease crime has motivated a large body of research that paid great attention to ensure estimating their causal effect on crime.[1] Moreover, assessing the relative importance of incapacitation versus deterrence in preventing crime is complex. Observing a plausibly random source of variation enabling clear identification of these two mechanisms is, in practice, scarce. Because the main punishment instrument of incapacitative measures is based on constraining mobility, any interventions raising expected penalties simultaneously increases deterrence and incapacitation. It follows that most empirical studies on the impact of incapacitative measures might be estimating the composite effect of incapacitation and deterrence (Kessler and Levitt, 1999). Still, this distinction is essential for policy-makers: reductions in crime resulting from deterrence are less costly than enforcing incapacitation.[2]

Non-pharmaceutical interventions (NPIs), such as those implemented to contain Severe Acute Respiratory Syndrome Coronavirus 2 (SARS-CoV-2, hereafter Covid-19) transmissions, offer a quasi-experimental setting to isolate the incapacitative effect of extreme confinement measures on crime. By using difference-in-differences (DID) and event-study models, the objective of this paper is twofold. First, we aim to provide causal estimates on the crime impact of social distancing measures on violent and property crime. Second, we assess the fraction of the estimated effect that might be attributed to incapacitation. As the Covid-19 pandemic severely curbed economic activity, it is reasonable to expect deterrence-related confounders to either amplify or attenuate the change in criminal activity associated with mobility restrictions. Accounting for these influencing forces allows us to provide lower-bound estimates of the incapacitative effect of social distancing policies in the context of a public health crisis.

We conduct this study using data from a developing country, Ecuador, where the fear of contagion prompted the use of social distancing measures from March to August 2020. Given the unexpected,

[1] For instance, several studies aim at estimating the effect of incarceration on crime, the difficulty being that, when crime rises, prison population size mechanically increases. See Levitt (1996) and Johnson and Raphael (2012) for examples of causal identification of imprisonment on crime, exploiting exogenous variation in overcrowding litigation status, and past shocks affecting crime rates, respectively.

[2] That criminal activity is deterred by harsh sanctions over incapacitation implies that sentencing should rely on strong punishments. This would decrease crime at lower costs than resource-intensive imprisonment, curfew enforcement or monitoring of offenders (Becker, 1968).

sudden and sharp nature of Ecuador's Covid-19 lockdown, we exploit within-month variation in confinement measures across pandemic-free years in which no restrictive measure was in force, relative to those months in which the country experienced severe restrictions to mobility, social gatherings and economic activity. Relative to pre-pandemic years, criminal outcomes during the preceding six months, from September 2019 to February 2020, exhibited parallel trends across provinces for the time frame in which no social distancing measure was imposed, and a sharp decline afterwards. That the confinement was enforced nationwide allows not only controlling for time-invariant confounders, but also including province-specific time trends to isolate any time-varying unobservables that might bias the coefficient of interest downwards, overestimating the negative impact of social distance measures on crime.

Baseline results suggest a significant reduction of almost 3 violent and 22 property crimes per 100,000 inhabitants, relative to comparable months in previous years. These effects are sizeable in magnitude, and relatively persistent even once confinement measures were softened. We do not observe any substitution patterns between criminal activities. We then conduct a heterogeneity effects analysis by interacting the DID indicator variable with predetermined economic conditions by province, to isolate incapacitative from deterrent effect. Results suggest the composite decline in violent crime is mostly attributed to incapacitation. It is almost entirely explained by a decline in rapes; homicides and femicides do not show any significant change. On the other hand, the drop in property crime is attenuated in provinces heavily depending on essential sectors and blue-collar occupations. Roughly two-fifths of the composite drop is attributed to incapacitation alone, with robbery and theft displaying the greatest crime reductions.

Identifying the net effect of NPI-related mobility restrictions is not trivial. First, stay-at-home orders limit the mobility of active offenders This is a clear incapacitative effect, akin to the mechanical effect of imprisonment, curfews (Kline, 2011; Carr and Doleac, 2018), or time spent at school during the day (Jacob and Lefgren, 2003; Luallen, 2006; Machin et al., 2011).

Second, NPIs may deter illegal activity by raising costs to commit crime. Policing is, in relative terms, more efficient. Breaching mandated confinement is harshly punished. Pedestrian flows decrease, limiting the pool of potential victims, now confined at home (de la Miyar et al., 2020; Poblete-Cazenave, 2020). The context of these measures – a novel virus, and its intense media coverage – might also affect risk aversion and risk taking behaviours (Neilson and Winter, 1997; Åkerlund et al., 2016). However, by limiting non-essential legal earning activities, social distancing measures might increase expected returns to crime in areas where economic activity was severely interrupted by confinement measures, and render some illegal activities more attractive than others (Gould et al., 2002; Machin and Meghir, 2004; Bell et al., 2018).[3]

Third, NPIs limit the efficiency – or the mere existence – of programmes supporting the socio-economic integration of past offenders and those on the verge of committing crime. The closure of public spaces, social distancing of social workers and resource reallocation, might induce an upsurge in crime (New York Times, 2020b). The net effect of NPIs on crime is thus ambiguous; that their incapacitative and dissuasive effects are stronger than payoffs associated with illegal activities will result in a decrease in criminality.

Our paper offers two main contributions to the literature. First, to the best of our knowledge, this is the first study providing lower-bound estimates of the incapacitative effect of pandemic-induced lockdown on crime, by exploiting a nationwide natural experiment in an emerging economy. Since any incapacitation measures simultaneously dissuades from participating in illegal activities, the fact that neither civilians nor the Ecuadorian government could anticipate the spread of Covid-19 before reaching its first-wave peak of infections, arguably provides an exogenous source of variation that supports the internal validity of our empirical strategy. Moreover, we use administrative data that are nationally representative. This ensures findings to be informative for developing countries facing similar public health challenges, as well as budgetary restrictions that hinder offering social safety nets intended to alleviate households and businesses' economic losses, and increasing the opportunity cost of crime. Our results thus complement previous research that estimates the causal effect of incapacitation on criminal outcomes and recidivism in more developed settings, exploiting exogenous changes in sentencing (Kessler and Levitt, 1999; Owens, 2009; Helland and Tabarrok, 2007), collective pardons (Drago et al., 2009; Buonanno and Raphael, 2013; Barbarino and Mastrobuoni, 2014), youth curfews (Kline, 2011; Carr and Doleac, 2018) and educational interventions (Jacob and Lefgren, 2003; Luallen, 2006; Machin et al., 2011).

Second, our work adds to existing evidence on the side-effects of NPI. While health, labour or domestic violence implications might have been expected, the effects of stay-at-home orders on crime are ambiguous since active offenders and potential victims are mechanically removed from the streets, and legal earning activities are simultaneously limited, possibly modifying incentives to commit crime. We offer robust, precise evidence on the importance of incapacitation in explaining the estimated lockdown-induced crime reduction, even if short-lived. By providing nationally representative estimates, we also contribute to recent works on Covid-19 NPIs and criminal outcomes in big cities.[4]

[3] Since offices and businesses are closed, and potential victims, confined at home, opportunities to engage in non-residential burglaries increase, while returns in residential burglaries, shoplifting, pickpocketing, or robberies decrease.

[4] See evidence from Bihar, India (Poblete-Cazenave, 2020), Medellín, Colombia (Blattman et al., 2020), Mexico City, Mexico

The rest of this paper is structured as follows. Section 2 briefly presents the context of this study; section 3 describes the data used. Section 4 discusses the empirical strategy, and section 5, estimation results. Section 6 concludes.

2 Covid-19 non-pharmaceutical interventions in Ecuador

Ecuador hit the headlines in April 2020 with images of bodies piling up in the streets of Guayaquil, its first Coronavirus hotspot (British Broadcasting Corporation, 2020). Despite major undercounts, Ecuador is thought to have suffered one of the worst Covid-19 outbreaks (New York Times, 2020a). The first positive Covid-19 case was confirmed in Ecuador on February 29, 2020; the first death, case 0, on March 13. On March 12, the government enforced a series of health emergency measures to be implemented on March 16. These were some of the earliest taken across Latin America, before Ecuador reached its peak of first-wave contagion. Measures limited mobility across the country with, initially, a nationwide curfew from 9PM to 5AM. They also restricted vehicle circulation, prohibited social gatherings, and closed schools and international borders. Those who failed to comply were sanctioned. Measures became progressively stricter, with a 7PM-5AM curfew announced on March 21, followed by a 2PM-5AM curfew, on March 25.[5]

On April 12, a 'traffic light' system (*semaforización*, in Spanish) was implemented to regulate mobility and economic activity based on province transmission risk, and adapt restrictions as part of Ecuador's deconfinement strategy. From April to June, the whole country virtually remained red – all restrictions were maintained. In early July, 9 out of 221 cantons were labelled green, with no restriction, and 185 yellow, with partial restrictions; in early August, 11 and 196, respectively. While stay-at-home orders remained the norm, non-essential sectors, such as catering or lodging, could progressively reinitiate activities. Lockdowns became more localised. In September 2020, Ecuador's Constitutional Court ruled out further extending the state of emergency, putting an end to any mobility restrictions.

(de la Miyar et al., 2020), or 25 major US cities (Abrams, 2020).

[5] The severity of Ecuador's measures is seen in various mobility indicators, such as the Inter-American Development Bank 'Coronavirus Impact Dashboard' based on Waze for Cities programme data (`https://www.iadb.org/en/topics-effectiveness-improving-lives/coronavirus-impact-dashboard`), or the United Nations Development Programme in Latin America and the Caribbean and Grandata's heat maps of people's movements (`https://www.latinamerica.undp.org/content/rblac/en/home/coronavirus/data-covid-region.html`).

3 Data

3.1 Data sources

First, we use publicly available criminal offences recorded per month and province by the police since January 2014, as provided by the State Attorney Office (*Ministerio de Gobierno, Fiscalía General del Estado*). We calculated crime rates per 100,000 inhabitants using the most recent population census (2010). Offences are categorised as violent crimes – homicides, femicides and rapes – or property crimes – robbery, simple theft, motor vehicle theft, residential and non-residential burglaries. We process this information to construct a balanced panel of crime rates from September 2014 to August 2020.

Second, we collect information on predetermined economic attributes by province using the 2010 Census. We focus on rates of poverty, economic activity in essential and vulnerable industries (or sectors), and labour force by occupation. Sectors are defined as essential (vulnerable) if less (more) likely to be affected by health emergency measures.[6] Agriculture, energy, mining, public services (utilities), finance, public sector, defense and health are listed essential sectors; construction, commerce, transport, lodging, catering and tourism, real state, entertainment, and non-declared (likely informal), as sectors vulnerable to social distancing measures. In addition, we define predetermined labour force shares by occupation-based skill levels. To keep categories manageable and self-explanatory, we follow the International Standard Classification of Occupations (ISCO-88), and classify occupations out of four skill levels: (1) low-skilled blue-collar occupations correspond to skill level 1 occupations (plant and machine operators and assemblers and elementary occupations); (2) high-skilled blue-collar to skill level 2 (skilled agricultural and fishery workers and craft and related trades workers); (3) low-skilled white-collar to skill level 3 (clerks and service workers and shop and market sales workers); and (4) high-skilled white-collar occupations to skill level 4 occupations (legislators, senior officials and managers, professionals and technicians and associate professionals).

Last, we might observe displacement of criminal activity to areas with lighter mobility restrictions. Because measures and police presence might vary across the country with its gradual deconfinement, crime rates might increase in areas with softened control compared to areas where restrictions were not lifted. We examine this possibility by resorting to weekly reports on *semaforizaón* provided by the National Emergency

[6] We follow Morales et al. (2020) who study the labour market implications of Covid-19 mobility restrictions by economic sectors in Colombia.

Committee (*Comité de Operaciones de Emergencia Nacional*, COE). We calculate the share of red (strongest restrictions), yellow (partial) and green cantons (lowest) by province in the first week of each month, from April to August, to assess whether Ecuador's deconfinement strategy affects the relationship between social distancing measures and crime.

Merging all sources of information and variables described above leads to a final dataset consisting in a balanced panel of 23 provinces, containing 1,656 observations over the September 2014-August 2020 period.[7] The unit of observation is a province-month-year combination, with a year defined as a 12-month window from September previous calendar year to August next year.

3.2 Descriptive analysis

For purposes of visualisation, Figure 1 presents average monthly crime rates from September 2014 to August 2020. We define control years from September 2014 to August 2019, and treated years from September 2019 to August 2020. Months are rearranged, with September to February defined as pre-lockdown months, and March to August as post-lockdown months. March, the month when measures were announced and implemented, is the intervention cutoff. It is set to 0, and displayed by the vertical dashed line. On average, Figure 1 shows a clear decrease in property crime, and a less consistent decrease in violent crime over time since 2014. During the treatment year, from September 2019 to August 2020, we observe a marked but apparently temporary fall in violent and property crime from March 2020 onward, with the strongest reduction in April 2020.

Summary statistics in Table A1 presents similar patterns. Table A1 displays crime rates for control (September 2014-August 2019) and treated years (September 2019-August 2020), for pre-(September-February) and post-lockdown months (March-August). There is virtually no statistically significant difference in means between control and treated years in pre-lockdown months. However, there are about 1 and 17 less violent and property crimes per 100,000 inhabitants, respectively, in March-August 2020 compared to similar months in previous years.

While the clear decrease in crime from March 2020 onwards is highly suggestive of a causal effect, these are, of course, correlations. They might not be interpreted as causal evidence since confounding factors are likely at play. Descriptive statistics suggest, though, a common trend in crime rates between treated and

[7] Galápagos Islands and non-delimited areas are excluded since crime data are not available for all months in these provinces over our period of study.

control years in pre-lockdown months. They support the adoption of both DID and monthly event-study strategies, as we can isolate time-invariant confounding factors. Concerns about province characteristics changing during the pandemic will be addressed by including province-specific, time-varying attributes. In the next sections, we show that estimates hold to such controls, and provide robust evidence in support of the common trend assumption, suggesting that we identify the effect of social distancing, net of other elements affecting crime over time.

4 Empirical strategy

If time-varying unobserved characteristics by province drive crime occurrence and reporting, then naïve, Ordinary Least Squares (OLS) estimates will be biased. This is key since unobserved province and seasonal features might be correlated with unobservables influencing crime occurrence and reporting, as well as authorities' responses to crime. For examples, police and army forces might be strategically deployed in areas that had the highest criminality levels before the Covid-19 pandemic. Moreover, that households are now confined at home, it might become more difficult, almost impossible, to report instances of certain crimes, such as domestic violence. Given that incapacitation and deterrence are positively correlated, and that deterrence alone has a reducing effect on crime, OLS coefficients will typically overestimate the effect of social distancing measures. The econometric methodology presented in this section accounts for these concerns since (i) the Covid-19 lockdown was unexpected and exogenously implemented nationwide, rendering DID and event-study strategies suitable to use; and (ii) any confounders affecting the estimates of interest through unobserved changes in deterrence are controlled for by the inclusion of province-specific fixed effects and other time trends, as we explain below.

Identifying the lockdown impact on crime comes from Ecuador's unexpected implementation of mobility restrictions, before reaching its first-wave peak of contagion. We first consider the following specification:

$$\begin{aligned} C_{pmy} = {} & \beta(Treated_y * After_m) \\ & + (\mu_p * m) + (\lambda_p * y) + (\psi_y * m) + \delta_p + \gamma_m + \theta_y + \epsilon_{pmy} \end{aligned} \tag{1}$$

where C_{pmy} denotes crime outcomes per 100,000 inhabitants of province p in month m and year

y. $Treated_y$ is a binary variable taking value one if the observation belongs to the 'treatment window' (September 2019 to August 2020), and zero otherwise. $After_m$ is a binary variable that takes value one if an observation belongs to those months when Covid-19 mobility restrictions are in force (March to August), and zero in the remaining months of the year-window (i.e. September to February). The parameter β captures the average difference in criminal outcomes between 2020 and pandemic-free years, in those months when the lockdown was implemented. This specification includes fixed effects by province (δ_p), month (γ_m), and year (θ_y), to account for time-invariant shocks on crime. We also include province-specific trends by year and month to account for regional time-varying confounders, and year-specific monthly trends to capture differentials in criminal outcomes per month across the same calendar year. Otherwise stated, robust standard errors are clustered at the province-year level.

The above specification identifies the average effect across all months in which the lockdown was in place. In order to understand the evolution of such an intervention across time, we resort to a (monthly) event-study model:

$$\begin{aligned} C_{pmy} = & \sum_{j=-6}^{-2} \beta_{0j}(Treated_y * month_j) + \sum_{j=0}^{5} \beta_{1j}(Treated_y * month_j) \\ & + (\mu_p * m) + (\lambda_p * y) + \delta_p + \gamma_m + \theta_y + \epsilon_{pmy} \end{aligned} \tag{2}$$

where we explicitly estimate the before-lockdown parallel trends between treated and control groups, and the differential trends attributed to the social distancing measures up to six months after the lockdown started. Coefficients β_{-6},β_{-5},...,β_{-2} trace out monthly changes in the number of crime cases between pandemic-free years and the year the lockdown was implemented up to January 2020, leaving February 2020 as omitted category. Conversely, all remaining coefficients account for the change in crime outcomes between March to August 2020, relative to the same months in previous pandemic-free years. While this specification allows focusing on the dynamic component of the social distancing intervention, we sacrifice the inclusion of year-specific monthly trends in order to avoid perfect collinearity.

Assuming the incapacitation component of the lockdown is unambiguously negative and constant across all provinces, one way to unravel heterogeneous effects driven by unobserved changes in deterrence is to interact the effect of interest with predetermined economic conditions linked with the opportunity costs of crime. To achieve this goal, we adopt an alternative version of specification (1):

$$C_{pmy} = \alpha_1(Treated_y * After_m) + \alpha_2(Treated_y * After_m * W_p)$$
$$+ (\mu_p * m) + (\lambda_p * y) + (\psi_y * m) + \delta_p + \gamma_m + \theta_y + \epsilon_{pmy} \quad (3)$$

In this model, we aim to identify α_1 and α_2. The latter parameter controls for the deviation from the average effect captured by α_1, induced by variation at the cross-section of provinces in a predetermined attribute W_p. In contrast with specification (2), we include all specific-trends by province and years aforementioned. Hence, we can calculate the average effect for all values within the support of W_p, and assess to what extent these differential effects might be attributed to both deterrence and incapacitation, or incapacitation alone.

5 Results

5.1 Baseline

Table 1 presents baseline estimates on the composite effect of Ecuador's Covid-19 lockdown on violent and property crime levels.[8] As indicated by equation (1), we include month, year and province fixed-effects to control for time-invariant unobservables in column (1); monthly and yearly province-specific trends to account for time-varying confounders per province across years and months of each year in column (2); and year-specific monthly trends, to capture differentials in criminal outcomes for distinct months within the same year in column (3). In columns (1)-(3), standard errors are clustered at the province-year level to account for potential serial correlation; in column (4), standard errors are clustered at the province level, and corrected for the small number of clusters via wild bootstrapping (Cameron et al., 2008).

Across all specifications, social distancing measures significantly decrease crime rates. Our preferred model, in column (3), indicates a decrease in 2.72 in violent crime and 21.51 in property crime per 100,000 inhabitants, relative to comparable months in previous years. Even after allowing for intra-province serial correlation, results suggest that property crimes dropped between 12 and 24 cases per 100,000 inhabitants (column (4)).

We estimate the same model for different subtypes of crime (Table A3, Panel A). Results show that

[8] Crime rates might spike in off-curfew hours, when individuals are allowed to leave home, reflecting the effect of stay-at-home orders on the timing of crime, rather than the level of crime within a day. Because we examine the monthly incidence of crime as opposed to the timing of crime, our estimates should be interpreted as the effect of stay-at-home measures on crime rate levels.

the drop in violent crimes is almost completely explained by a decrease in rape cases, with homicides and femicides not affected by the confinement. In contrast, all subcategories of property crimes are significantly affected, with robbery and residential burglaries experiencing the largest declines – about 10 and 4 cases, respectively.

In Figure 2, we present event-study model estimates of specification (2) that accounts for the dynamic effect of social distancing. Non-vertical dashed lines display 99% confidence level intervals. The vertical dashed line shows the intervention cut-off, March 2020, that is set to 0. It is evident that pre-lockdown coefficients for violent (Panel (a)) and property crime (Panel (b)) are not statistically different from zero, supporting the parallel trend assumption required in this setting. April 2020 seems to be the period with the largest declines in both types of crime, and, while there is a slight reversion to the mean, reductions in crime seem to persist for at least six months after the lockdown started. We present the same figures for all crime subcategories in Figure A1. With the notable exception of homicide and femicide, where we observe no effect, all crime subtypes exhibit the same pattern as aggregate indicators.

5.2 Heterogeneity

This section presents the range of effects mediated by province predetermined economic characteristics according to specification (3). As the confinement was implemented nationwide at the same moment in time, variation in its crime-reducing effect might be attributed to differences in pre-lockdown economic conditions. Once the intervention kicks in, predetermined conditions will play a role in either amplifying or attenuating the average effect resulting from baseline specification (1). We claim that, conditional on including fixed-effects and province-specific trends, specification (3) recovers lower-bound estimates of the incapacitative effect of the lockdown, leaving the remaining variation of the effect explained by changes in deterrence-related determinants of crime.

Table 2 presents results of this exercise. We focus on the interaction of the difference-in-differences effect with poverty rates, shares of the labour force in economic sectors deemed vulnerable and/or essential, and in occupation-specific skill levels.[9] As we are also interested to see how gradual deconfinement measures influence crime rates, we include the interaction with the proportion of cantons that transitioned from a 'red traffic light' to a yellow or green light, implying softer to no restriction on mobility,

[9] Table A2 gives summary statistics of predetermined economic attributes at the cross-section of province. On average, in 2010, 42% of the labour force work in essential sectors, and 31% in white-collar occupations. While there is significant variation in occupation rates across provinces, income distribution is highly concentrated, with 68% of the population living in poverty.

respectively.[10]

Table 2, Panel A indicates that the reduction in violent crime is explained by incapacitation alone as none of the province predetermined economic conditions is statistically significant. Panel B suggests a different narrative. With the notable exception of poverty rate, certain economic conditions serve amplify the crime-reducing effect of the confinement, such as the proportion of active population employed in vulnerable sectors and in white-collar positions. Attenuating effects can be attributed to changes in rates of essential activity and blue-collar high-skilled occupation. Regarding the de-escalation of social distancing, moving from full to mild restrictions implies losing more than half the average decline in both violent and property crime. Moving from a setting with full restrictions to none does not seem relevant.[11]

To recover lower-bound effects of incapacitation, we calculate 99% confidence intervals using the limits of the support from the distribution of each predetermined characteristic that serves as either an amplifying or attenuating factor of baseline effects. Then, we focus on the lower limit of such interval. As long as the confidence interval does not include zero, we can claim that the lower limit is a good approximation of the lower-bound of the crime-reducing effect of the lockdown, given that province deterrence-related conditions are the weakest possible. Figure 3 displays results from this analysis for aggregate violent and property crimes, as well as for all crime subcategories. As an example, consider the attenuating role of a larger labour force share in essential sectors in reducing property crime (Panel (a)). Comparing provinces with the lowest participation share (24%) and with the highest share (61%) indicates that the decline in property crimes ranges from 38 (the upper limit of the confidence interval for the province with the minimum occupation rate) to roughly 8 cases per 100,000 inhabitants (the lower limit of the confidence interval for the province with the maximum occupation rate). In contrast, confidence intervals associated with violent crimes overlap, and are very close to zero. This implies that lower-bound effects are very small, with little variation left to be explained by deterrence.

As suggested by this example, Panels (b)-(f), Figure 3, confirm lower-bound incapacitative effects ranging from -0.4 to -0.8 violent crime cases per 100,000 inhabitants. On the other hand, the decrease in property crimes is attenuated by deterrence, specifically when the economic activity mainly relies on essential sectors and blue-collar occupations. Figure 3 points to a lower-bound effect of incapacitation varying from -8 to -11 property crime cases per 100,000 inhabitants, driven by a drop in robberies spanning

[10] See Figure A2 to visualise the evolution of province shares of canton labelled red, yellow and green from June to August 2020.
[11] Statistical insignificance might however be due to a lack of variation since, as of August 2020, very few cantons had deconfined.

from 3.6 to 5.6 cases. Given that the baseline average effect is of about 21 crimes, we can assert that the incapacitative effect of the lockdown is at least two-fifths of the composite effect, with the remaining fraction attributed to deterrence-related economic conditions.

5.3 Robustness

Parallel trend assumption

As indicated by descriptive analysis, crime rates seem to have fallen in post-lockdown months relative to prior months in the treated year, September 2019-August 2020. Ensuring this is due to Ecuador's policy intervention requires establishing that pre-lockdown monthly trends were not different in treated and control years. Namely, as long as crime trends in pre-lockdown months are similar across years, we can assume crime outcomes to have evolved similarly in post-lockdown months, given the absence of any confinement measures, as an appropriate counterfactual. We report event-study estimates in Table A4, in order to perform individual and joint significance tests on pre-lockdown coefficients. With some small significant effects reported for November 2019, for both violent and property crime, the parallel trend assumption appears to hold for all periods. This is strongly supported when we conduct F-tests on the entire set of pre-lockdown coefficient to show that, in all specifications, estimates are statistically indistinguishable from zero at traditional confidence levels.

Do predetermined province characteristics determine future crime rates?

A potential concern in our empirical design is that unobserved province-specific time varying shocks affecting crime are correlated with changes in incapacitation and deterrence before and after the confinement was implemented. While we include a series of province-specific trends to account for this concern, we extend this analysis by including a set of interaction between pre-lockdown (2010) economic indicators used in the heterogeneity analysis with the post-lockdown indicator. This is to check whether these variables explain the observed drop in crime rates in the treatment year. Results from this exercise are displayed in Table A5. We cannot reject the null hypothesis from a joint test of all interaction terms to be statistically insignificant. This indicates that predetermined economic conditions by province may be a source of heterogeneity through the lockdown implementation, but do not determine crime rates in the post-lockdown period alone.

Randomized inference placebo test

To test the control-treatment classification our empirical strategy relied on, we perform a falsification test using the subsample of control years. From 1,500 observations, we select a random sample of 12 months, and falsely define them as treated observations. We then estimate equation (1) to recover the main effect of interest, and replicate this process 1,500 times. As the word placebo suggests, we should expect significant results in no more than 1% replications if we use a 99% confidence level. Otherwise, results would cast doubt on our treatment-control classification. Figure A3 displays the probability density function of the estimated t-statistic of each of these replications for our coefficients of interest, where the vertical red line denotes the mean of a t-student distribution (i.e. 0). The estimated t-distribution follows a normal distribution – it is symmetric and bell-shaped. Table A6 presents statistics of all parameters recovered from this falsification test. Less than 1% of the replications turn out to be significant. In addition, all mean coefficients are virtually zero, with standard errors at least 174 and 268 times higher than reported effects for violent and property crime, respectively. Overall, these results support the treatment-control classification we use.

6 Conclusion

We exploit Ecuador's Covid-19 social distancing measures as a natural experiment to identify the average effect of a lockdown on crime, and isolate its incapacitative effect. Difference-in-differences results indicate a decrease in almost 3 violent crimes per 100,000 inhabitants, mostly attributed to incapacitation. The reduction in about 22 property crimes is attenuated by deterrence, with incapacitation explaining about 40 to 50% of the composite decrease. Estimated effects are sizeable in magnitude, and rather persistent, even once confinement measures are softened. They are in line with Barbarino and Mastrobuoni (2014), the closest article to our work, who identify lower-bound estimates of a 'pure' incapacitation effect, with a prison year saving about 22 crimes. The existence of a causal crime-reducing effect of stay-at-home orders might bear implications for efforts aimed at reducing crime in the longer term, in particular given the possibility of future, lasting confinement episodes, and consequent unemployment. At the very least, our results suggest that, in the midst of a public health crisis, incapacitation measures restricting the mobility of likely criminals and victims might be less effective when social safety nets, possibly weakening incentives to engage in criminal activities, are not available.

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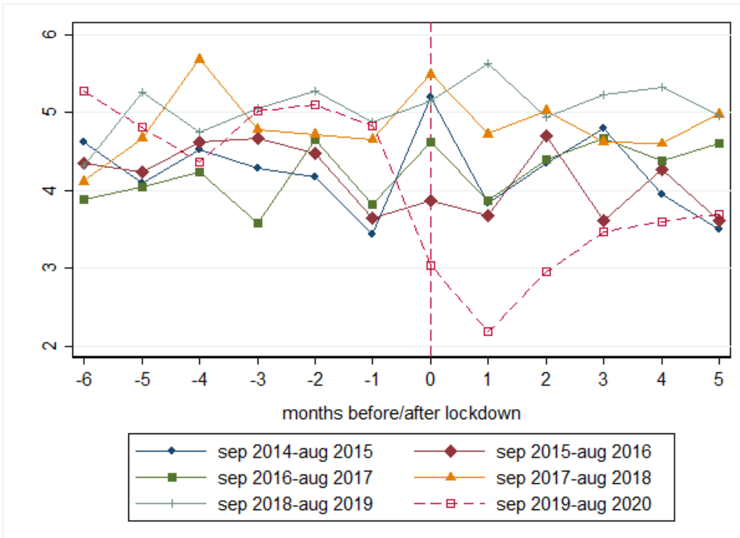
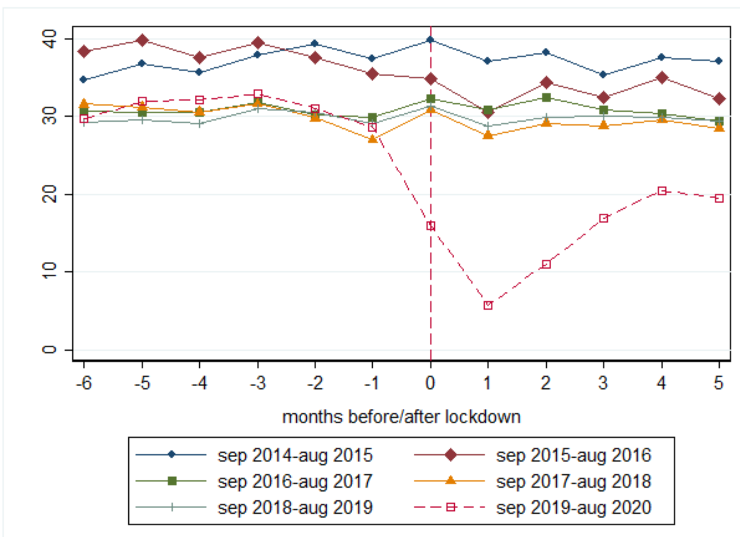
Figure 1: Trends in crime rates, September 2014-August 2020

(a) Violent crime



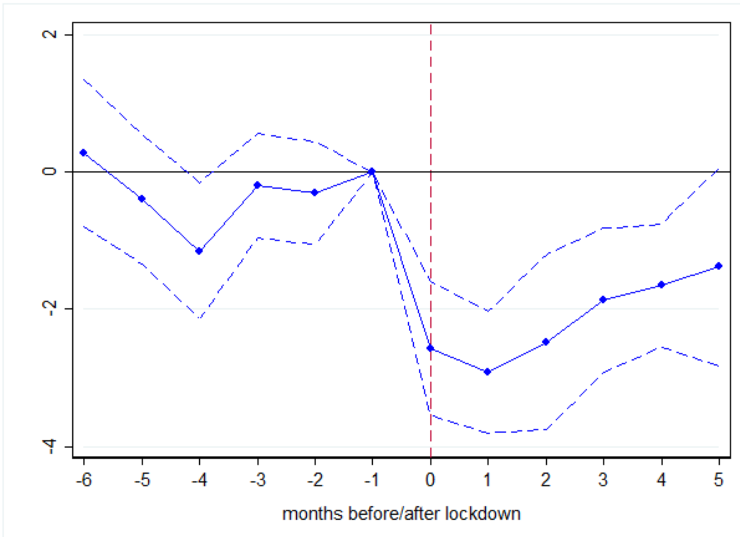
(b) Property crime



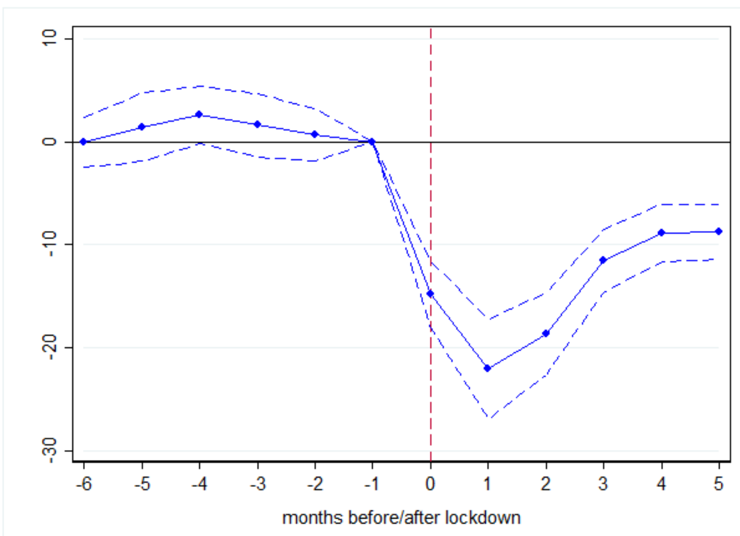
Notes: Panel (a) displays average monthly violent crimes per 100,000 inhabitants in control (September 2014-August 2019) and treated years (September 2019-August 2020), six months before (September-February) and five months after (April-August) the month measures were announced and implemented. March, the intervention cutoff, is set to 0; it is represented by the vertical dashed red line. Panel (b) similarly shows average monthly property crimes per 100,000 inhabitants.

Figure 2: Effect of Covid-19 lockdowns on crime: Event study estimates

(a) Violent crime

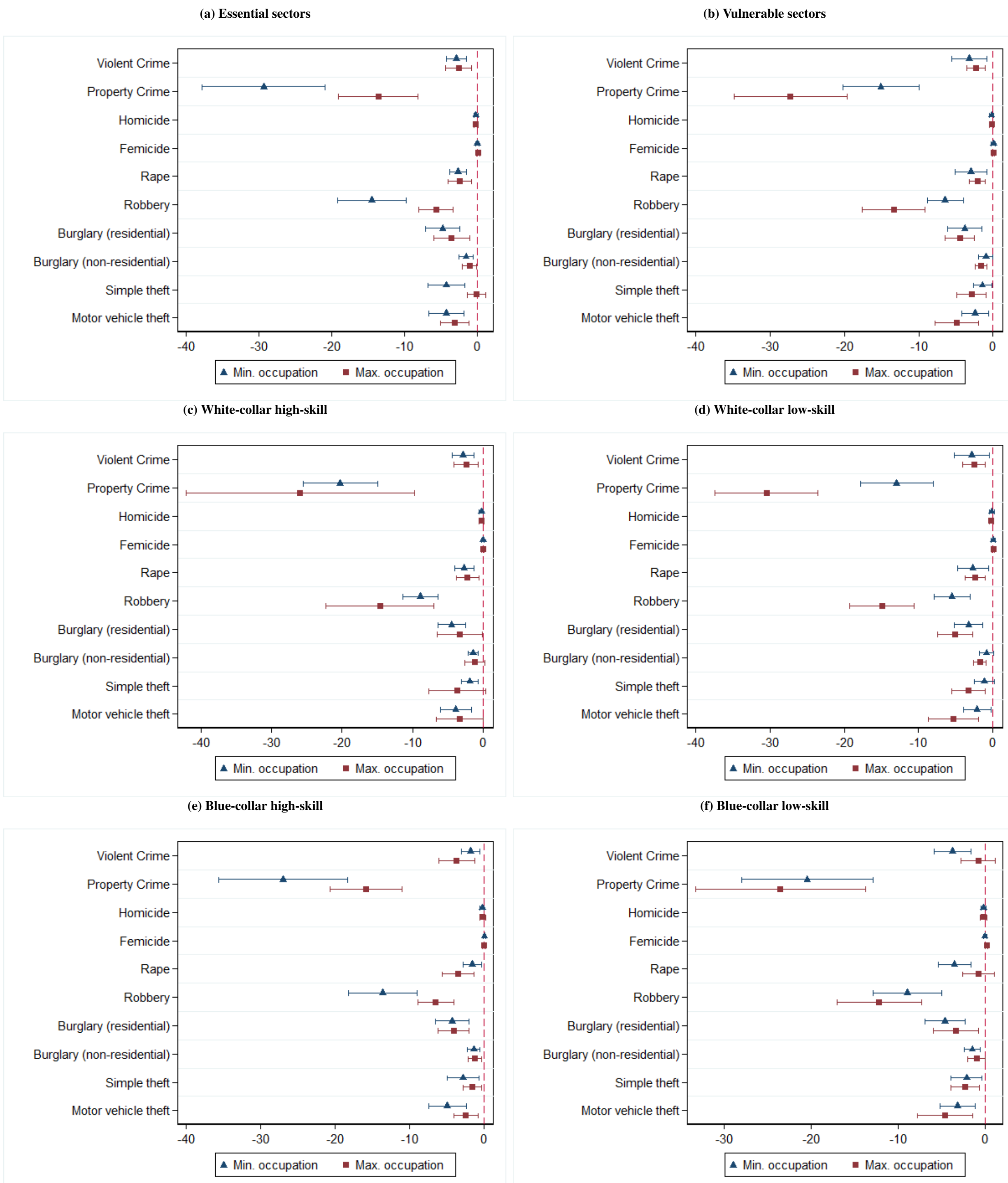


(b) Property crime



Notes: Panel (a) presents event-study estimates for violent crimes per 100,000 inhabitants before and after the implementation of the Covid-19 lockdown, according to specification (2). The omitted category is February 2020. The intervention cutoff is March 2020; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals. Panel (b) similarly shows estimates for property crimes per 100,000 inhabitants.

Figure 3: Effect of Covid-19 lockdowns on crime: Heterogeneity effect analysis



Notes: Figures report 99% confidence level intervals of the difference-in-differences effect of the Covid-19 lockdown on criminal outcomes per 100,000 inhabitants, according to specification (3). Panels (a) and (b) present estimates for provinces with the maximum (red square) and minimum (blue triangle) share of active population working in essential and vulnerable economic sectors, respectively. Panels (c)-(f) similarly display estimates for provinces with the maximum and minimum share of active population working in white-collar high-skilled, white-collar low-skilled, blue-collar high-skilled and blue-collar low-skilled occupations, respectively.

Table 1: Effect of Covid-19 lockdown on crime

	(1)	(2)	(3)	(4)
Panel A: Violent crime				
Treated x After	-1.8480***	-1.8480***	-2.7239***	-2.7239***
	(0.2976)	(0.3121)	(0.5031)	$[-3.825, -0.905]$
R-squared	0.6131	0.6163	0.6198	0.6166
Panel B: Property crime				
Treated x After	-15.1563***	-15.1563***	-21.5147***	-21.5147***
	(1.6713)	(1.5317)	(2.3656)	$[-24.13, -12.13]$
R-squared	0.8611	0.8655	0.8702	0.8658
Observations	1,656	1,656	1,656	1,656
Month FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Province specific trends	No	Yes	Yes	Yes
Year specific trends	No	No	Yes	Yes
Wild Bootstrap (province)	No	No	No	Yes

Notes: Difference-in-differences estimates of the effect of Covid-19 lockdown on violent and property crimes per 100,000 inhabitants as specified by equation (1). Robust standard errors clustered at the province-year level are reported in parentheses in columns (1)-(3). 99% confidence intervals from a wild-bootstrapping correction with 1,500 replications, clustered at the province level, are reported in brackets in column (4). *** p<0.01, ** p<0.05, * p<0.1.

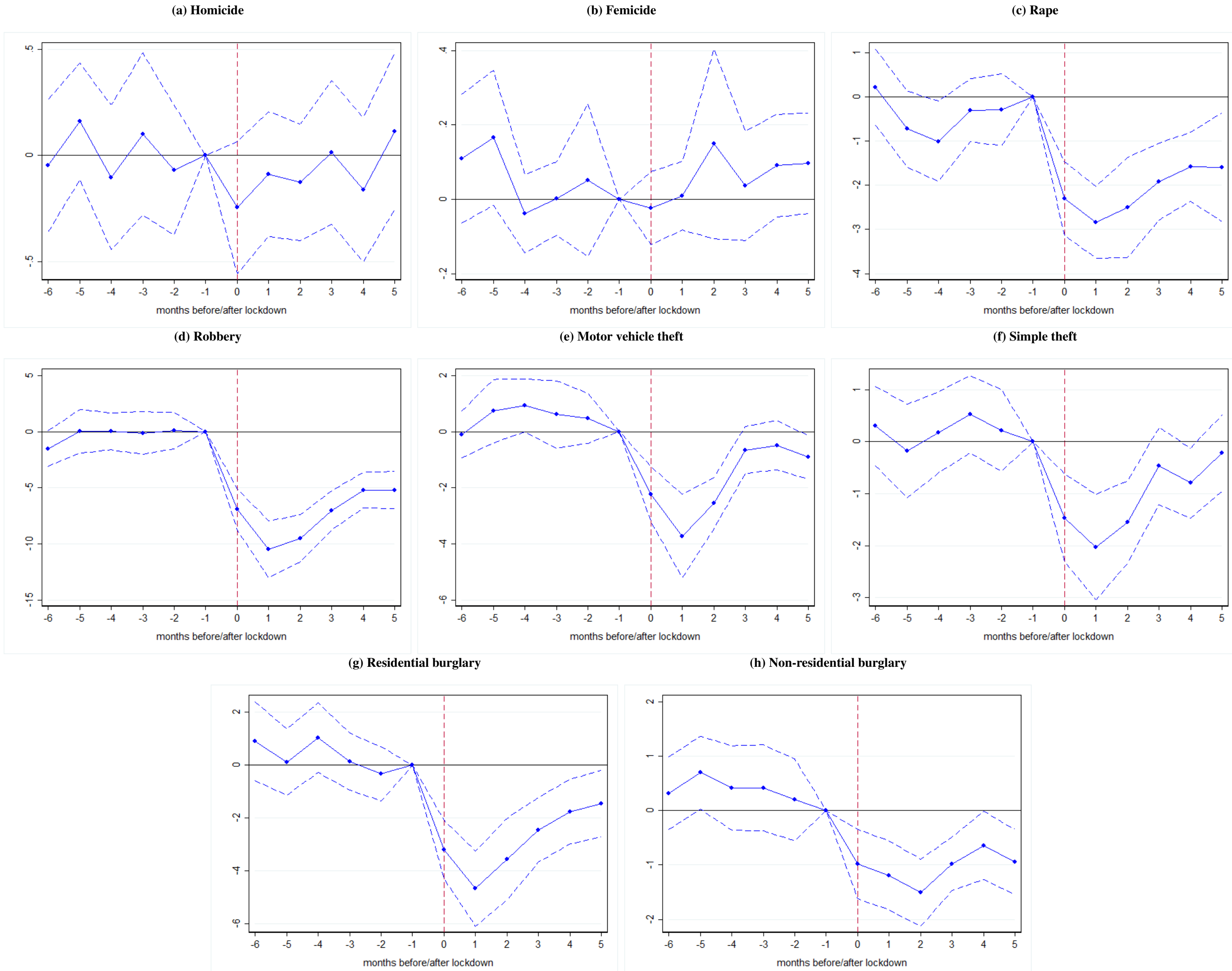
Table 2: Heterogeneous effect of Covid-19 lockdown on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Violent crime								
Treated x After	-2.2382**	-2.2289*	-3.0663***	-4.0538***	-2.4672***	-1.7959	-2.5288***	-2.6794
	(0.9068)	(1.1662)	(0.8037)	(0.9273)	(0.6638)	(1.1041)	(0.7368)	(2.3793)
... x Poverty	-0.7125							
	(1.2852)							
... x Vulnerable		-0.1141						
		(0.2135)						
... x Essential			0.0808					
			(0.1862)					
... x Blue-collar low-skill				0.5128*				
				(0.2619)				
... x Blue-collar high-skill					-0.0836			
					(0.2127)			
... x White-collar low-skill						-0.4567		
						(0.4313)		
... x White-collar high-skill							-0.1760	
							(0.4093)	
Treated x Yellow								1.4242***
								(0.4345)
Treated x Green								-0.3448
								(2.2991)
R-squared	0.6199	0.6199	0.6199	0.6206	0.6199	0.6202	0.6199	0.6218
Panel B: Property crime								
Treated x After	-31.1894**	-1.2366	-39.5275***	-24.5413***	-35.2626***	4.3581	-7.6048*	-17.5732**
	(11.9947)	(5.1688)	(5.7035)	(6.3086)	(5.9780)	(3.9302)	(4.4658)	(7.2562)
... x Poverty	14.1921							
	(16.0054)							
... x Vulnerable		-4.6717***						
		(1.3216)						
... x Essential			4.2509***					
			(1.1001)					
... x Blue-collar low-skill				1.1670				
				(2.0834)				
... x Blue-collar high-skill					4.4776***			
					(1.5172)			
... x White-collar low-skill						-12.7326***		
						(2.0806)		
... x White-collar high-skill							-12.5493***	
							(3.8880)	
Treated x Yellow								8.8418***
								(1.8583)
Treated x Green								1.4836
								(7.9487)
R-squared	0.8710	0.8751	0.8756	0.8703	0.8742	0.8780	0.8740	0.8726
Observations	1,656	1,656	1,656	1,656	1,656	1,656	1,656	1,656

Notes: Estimates from specification (3). All regressions include month, year and province fixed effects, as well as province-specific linear trends per year and month, and year-specific monthly linear trends. Robust standard errors clustered at the province-year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

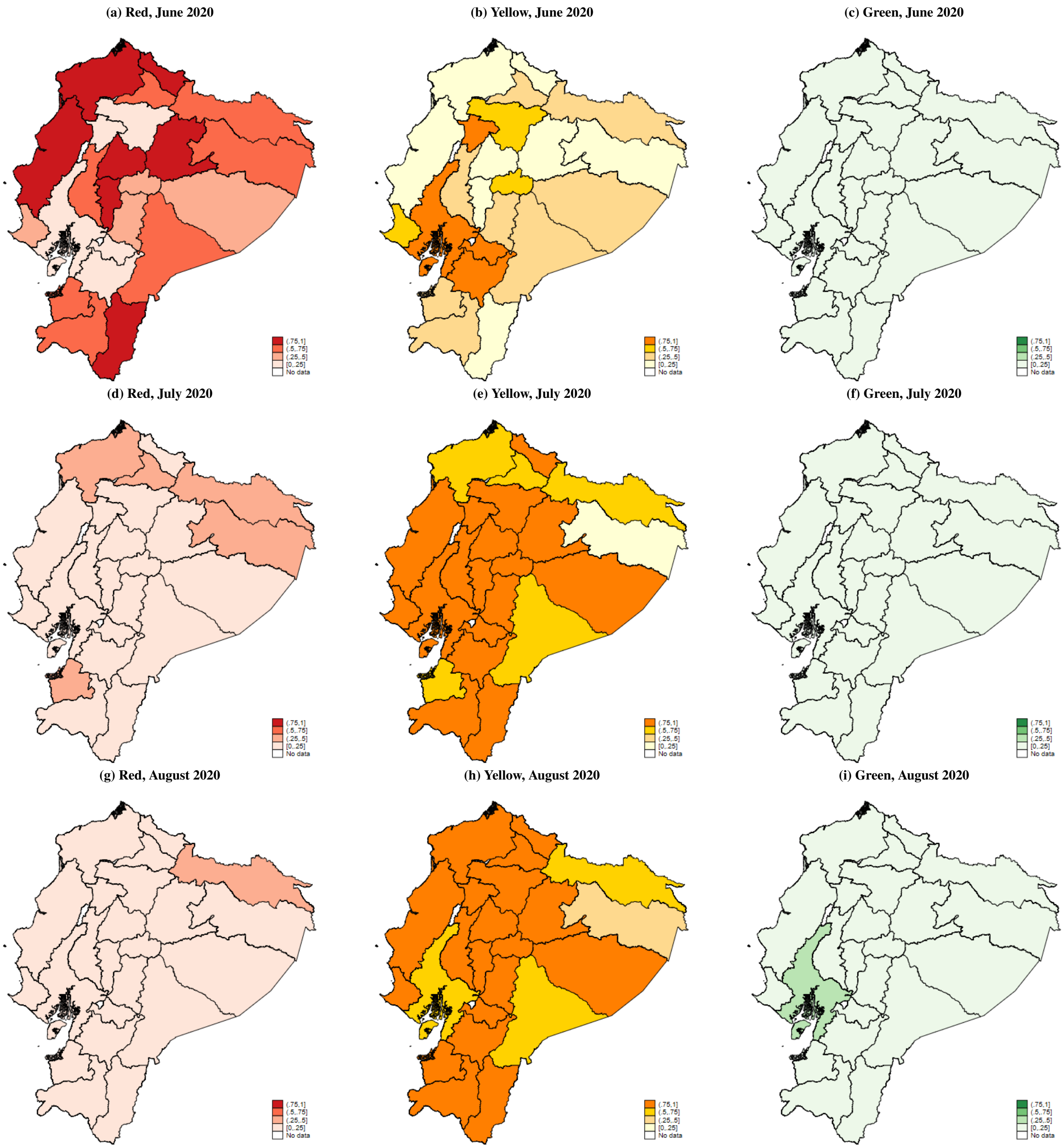
Appendices (for online publication)

Figure A1: Effect of Covid-19 lockdowns on crime: Event study estimates



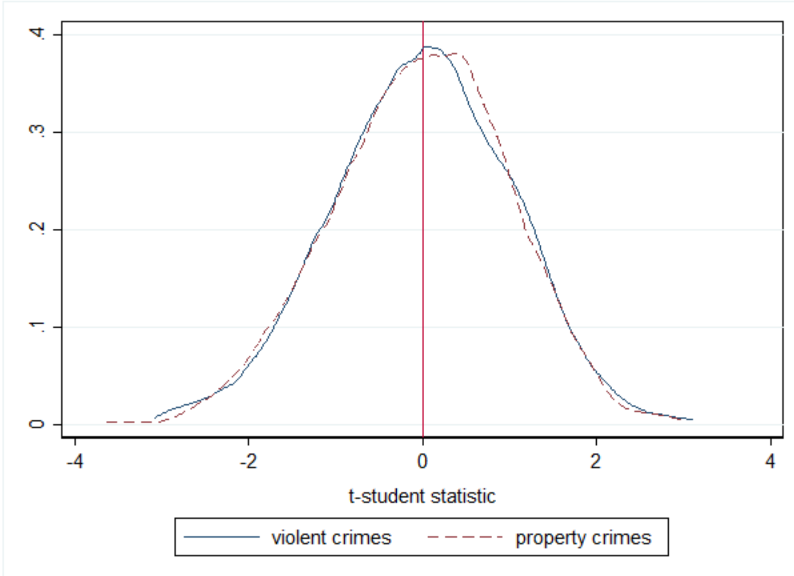
Notes: Figures present event-study estimates for specified crime outcomes per 100,000 inhabitants before and after the implementation of the Covid-19 lockdown, according to specification (2). The omitted category is February 2020. The intervention cutoff is March 2020; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals.

Figure A2: ***Semaforizacón*** **deconfinement strategy, proportion of cantons per province, June-August 2020**



Notes: Proportion of cantons per province according to Ecuador's *semaforizacón* colour labelling, in June (panels (a)-(c)), July (panels (d)-(f)) and August 2020 (panels (g)-(i)).

Figure A3: Robustness checks: Distribution of placebo treatments



Notes: Probability density function of the estimated t-statistic of each of replications of the falsification test. We estimate equation (1) limiting the sample to control years, and randomly allocating treatment to 12 consecutive months. We replicate this process 1,500 times. The solid vertical red line denotes the mean of a t-student distribution (i.e. 0). Specifications include province, month and year fixed-effects, province-specific month and year trends and year-specific month trends. The outcomes variables are violent (solid blue line) and property crimes (dashed red line) per 100,000 inhabitants. The coefficient of interest is the interaction of an indicator of (false) treatment status with an indicator identifying post-lockdown months.

Table A1: Summary statistics: Crime rates per 100,000 inhabitants

	Pre-lockdown months					Post-lockdown months				
	Treated		Control		Difference	Treated		Control		Difference
Variables	Mean	sd	Mean	sd	(1)-(3)	Mean	sd	Mean	sd	(6)-(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Violent crime	4.90	3.23	4.45	2.65	0.448	3.15	2.30	4.55	2.97	-1.400***
					(0.642)					(0.437)
Homicide	0.62	0.62	0.56	0.60	0.053	0.51	0.52	0.54	0.57	-0.036
					(0.099)					(0.080)
Femicide	0.09	0.28	0.10	0.28	-0.009	0.09	0.29	0.08	0.27	0.002
					(0.030)					(0.030)
Rape	4.20	3.03	3.79	2.54	0.405	2.56	2.18	3.93	2.77	-1.366***
					(0.601)					(0.423)
Property crime	31.06	15.21	33.12	15.82	-2.062	14.93	10.25	32.15	16.11	-17.219***
					(3.374)					(2.227)
Robbery	12.35	7.39	11.38	7.43	0.969	5.16	4.53	11.33	7.64	-6.178***
					(1.603)					(1.024)
Motor vehicle theft	5.58	5.41	4.78	4.43	0.805	3.34	3.88	4.73	4.62	-1.396*
					(1.158)					(0.829)
Simple theft	3.15	3.05	4.35	4.13	-1.201*	1.43	1.85	3.90	3.62	-2.465***
					(0.707)					(0.443)
Residential burglary	7.09	3.44	9.43	4.44	-2.341***	3.53	2.61	9.03	4.51	-5.503***
					(0.711)					(0.512)
Non-residential burglary	2.89	1.71	3.19	1.90	-0.293	1.48	1.09	3.15	1.95	-1.676***
					(0.312)					(0.223)
Observations	138		690		828	138		690		828

Notes: Variables are expressed as rates per 100,000 inhabitants, except femicides, expressed per 100,000 female inhabitants. Violent crime includes homicide, femicide and rape. Property crime includes robbery, motor vehicle theft, simple theft, residential and non-residential burglary. Standard deviations are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Summary statistics: Province attributes (2010 population census)

Variables	N (1)	mean (2)	sd (3)	min (4)	max (5)
Total population	23	6.27	8.51	0.84	36.45
Male population	23	3.11	4.22	0.42	18.16
Female population	23	3.16	4.30	0.42	18.30
Active population	23	2.64	3.72	0.33	15.12
Poverty	23	0.68	0.13	0.33	0.87
Share of active population working in:					
Essential sectors	23	0.42	0.11	0.24	0.61
Vulnerable sectors	23	0.43	0.09	0.27	0.58
Blue-collar low-skilled	23	0.26	0.06	0.17	0.42
Blue-collar high-skilled	23	0.31	0.09	0.16	0.46
White-collar low-skilled	23	0.20	0.04	0.12	0.29
White-collar high-skilled	23	0.11	0.03	0.08	0.22
Share of canton by colour label in:					
Red, May 2020	23	1	0	1	1
Yellow, May 2020	23	0	0	0	0
Green, May 2020	23	0	0	0	0
Red, June 2020	23	0.57	0.31	0	1
Yellow, June 2020	23	0.42	0.32	0	1
Green, June 2020	23	0.01	0.05	0	0.25
Red, July 2020	23	0.13	0.17	0	0.50
Yellow, July 2020	23	0.83	0.19	0.25	1
Green, July 2020	23	0.03	0.07	0	0.25
Red, August 2020	23	0.07	0.12	0	0.43
Yellow, August 2020	23	0.90	0.15	0.50	1
Green, August 2020	23	0.04	0.08	0	0.28

Notes: Total, male, female and active population statistics are expressed per 100,000. Essential sectors include agriculture, public utilities, public administration, finance, mining, information and communication, and professional activities. Vulnerable sectors include construction, commerce, lodging and food services, transportation, artistic activities, manufacturing, and real estate. Blue-collar low-skilled occupations are skill level 1 occupations (plant and machine operators and assemblers, and elementary occupations). Blue-collar high-skilled occupations are skill level 2 occupations (skilled agriculture and fishery workers, and craft and related trade workers). White-collar low-skilled occupations are skill level 3 occupations (clerks, service workers, and shop and market sales workers). White-collar high-skilled occupations are skill level 4 occupations (legislators, senior officials, managers, and professionals).

Table A3: Heterogeneous effect of Covid-19 lockdown on crime by type of crime

	Violent crime			Property crime				
	Homicide	Femicide	Rape	Motor vehicle theft	Simple theft	Robbery	Residential burglary	Non-residential burglary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline								
Treated x After	-0.2148**	0.0215	-2.5306***	-3.7087***	-2.2095***	-10.1078***	-4.1675***	-1.3211***
	(0.1007)	(0.0572)	(0.4617)	(0.7262)	(0.5394)	(1.2576)	(0.7256)	(0.2867)
R-squared	0.3168	0.0619	0.6403	0.8075	0.7905	0.8528	0.6632	0.5506
Panel B: Poverty								
Treated x After	0.0652	-0.0024	-2.3010***	-1.1601	-9.8204***	-17.2382***	-2.0995	-0.8712
	(0.2387)	(0.1116)	(0.7850)	(3.3999)	(2.3047)	(4.9527)	(2.3775)	(1.0766)
... x Poverty	-0.4107	0.0351	-0.3368	-3.7386	11.1645***	10.4598*	-3.0336	-0.6600
	(0.2938)	(0.1801)	(1.1554)	(4.8945)	(3.0438)	(6.1723)	(3.1279)	(1.4137)
R-squared	0.3248	0.0619	0.6403	0.8082	0.8001	0.8549	0.6637	0.5507
Panel C: Vulnerable sectors								
Treated x After	-0.3155*	0.1315	-2.0449**	-1.9948	3.2843**	1.0801	-2.8323	-0.7738
	(0.1854)	(0.1846)	(1.0102)	(1.2188)	(1.3904)	(2.4254)	(1.9379)	(0.8047)
... x Vulnerable	0.0232	-0.0253	-0.1119	-0.3949	-1.2657***	-2.5775***	-0.3076	-0.1261
	(0.0397)	(0.0374)	(0.1767)	(0.2797)	(0.3801)	(0.6748)	(0.3910)	(0.1761)
R-squared	0.3244	0.0624	0.6404	0.8079	0.7972	0.8598	0.6635	0.5509
Panel D: Essential sectors								
Treated x After	-0.1569	-0.0841	-2.8252***	-4.9967***	-6.9142***	-20.1629***	-5.5836***	-1.8701***
	(0.2004)	(0.1335)	(0.6618)	(1.3921)	(1.7618)	(3.0843)	(1.4974)	(0.6838)
... x Essential	-0.0137	0.0249	0.0695	0.3040	1.1103***	2.3729***	0.3342	0.1296
	(0.0366)	(0.0321)	(0.1552)	(0.2525)	(0.3358)	(0.5596)	(0.3225)	(0.1458)
R-squared	0.3243	0.0625	0.6403	0.8078	0.7972	0.8606	0.6636	0.5510
Panel E: Blue-collar low-skill								
Treated x After	-0.3448**	-0.1414*	-3.5676***	-4.6381*	-4.5782***	-6.6477**	-6.3796***	-2.2977***
	(0.1603)	(0.0835)	(0.8961)	(2.4253)	(1.7057)	(3.0654)	(1.5181)	(0.5121)
... x Blue-collar low-skill	0.0501	0.0628***	0.3999	0.3583	0.9133*	-1.3342	0.8530*	0.3766**
	(0.0506)	(0.0207)	(0.2644)	(0.8477)	(0.5067)	(1.0529)	(0.4891)	(0.1626)
R-squared	0.3245	0.0631	0.6408	0.8076	0.7919	0.8536	0.6640	0.5515
Panel F: Blue-collar high-skill								
Treated x After	-0.2297	0.0220	-2.2594***	-4.7652***	-3.9956**	-19.7695***	-5.0807***	-1.6516***
	(0.1935)	(0.0933)	(0.6207)	(1.4143)	(1.8230)	(2.7529)	(1.3336)	(0.5696)
... x Blue-collar high-skill	0.0049	-0.0002	-0.0883	0.3441	0.5817	3.1467***	0.2974	0.1077
	(0.0506)	(0.0287)	(0.1959)	(0.3902)	(0.4822)	(0.6592)	(0.3418)	(0.1530)
R-squared	0.3243	0.0619	0.6403	0.8078	0.7917	0.8621	0.6634	0.5508
Panel G: White-collar low-skill								
Treated x After	-0.2577	0.1452	-1.6834*	-1.9560*	3.9211**	3.0372	-0.7437	0.0995
	(0.2025)	(0.1877)	(0.9530)	(0.9946)	(1.7363)	(2.0164)	(1.4642)	(0.7024)
... x White-collar low-skill	0.0211	-0.0609	-0.4169	-0.8625*	-3.0170***	-6.4690***	-1.6849**	-0.6991**
	(0.0867)	(0.0820)	(0.3525)	(0.5127)	(0.9444)	(1.1911)	(0.6889)	(0.3109)
R-squared	0.3243	0.0625	0.6406	0.8079	0.7985	0.8621	0.6649	0.5523
Panel H: White-collar high-skill								
Treated x After	-0.3017**	0.0754	-2.3025***	-3.4714**	3.0516**	-3.2408*	-3.2577***	-0.6865
	(0.1511)	(0.0898)	(0.6904)	(1.4570)	(1.1729)	(1.8130)	(1.1222)	(0.4890)
... x White-collar high-skill	0.0784	-0.0487	-0.2057	-0.2141	-4.7465***	-6.1953***	-0.8208	-0.5725
	(0.0989)	(0.0504)	(0.3615)	(1.0942)	(1.0770)	(1.7844)	(0.8326)	(0.3516)
R-squared	0.3244	0.0621	0.6403	0.8075	0.8004	0.8571	0.6634	0.5512
Panel I: Semaforización								
Treated x After	0.1617	-0.3884	-2.4527	3.9137	-1.0636	-17.7121***	-1.7999	-0.9114
	(0.8882)	(0.2568)	(1.7412)	(2.5871)	(1.5505)	(4.1412)	(3.4902)	(1.5441)
... x Yellow	0.1823	0.1441**	1.0978***	2.3628***	0.5173	2.6980***	2.4276***	0.8361***
	(0.1355)	(0.0675)	(0.3657)	(0.6731)	(0.3962)	(0.8638)	(0.7763)	(0.2844)
... x Green	0.3225	-0.4447*	-0.2227	6.8922***	0.9919	-8.2558*	1.6784	0.1769
	(0.8816)	(0.2561)	(1.6460)	(2.6167)	(1.6688)	(4.2679)	(3.4894)	(1.4844)
R-squared	0.3251	0.0645	0.6416	0.8100	0.7906	0.8540	0.6655	0.5521
Observations	1,656	1,656	1,656	1,656	1,656	1,656	1,656	1,656

Notes: Estimates from specification (3). All regressions include month, year and province fixed effects, as well as province-specific linear trends per year and month, and year-specific monthly linear trends. Robust standard errors clustered at the province-year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Robustness checks: Parallel trend assumption tests

	Violent crime		Property crime	
	(1)	(2)	(3)	(4)
Panel A: Pre-Covid-19 specific trends				
Treated x September 2019	0.2760	0.2760	-0.0788	-0.0788
	(0.5357)	(0.5289)	(1.2171)	(1.2724)
Treated x October 2019	-0.3997	-0.3997	1.4025	1.4025
	(0.4716)	(0.4794)	(1.6599)	(1.7427)
Treated x November 2019	-1.1495**	-1.1495**	2.5905*	2.5905*
	(0.4982)	(0.5114)	(1.3989)	(1.4090)
Treated x December 2019	-0.2020	-0.2020	1.5799	1.5799
	(0.3807)	(0.3840)	(1.5330)	(1.5141)
Treated x January 2020	-0.3057	-0.3057	0.6571	0.6571
	(0.3751)	(0.3844)	(1.2692)	(1.2767)
Panel B: Post-Covid-19 specific trends				
Treated x March 2020	-2.5743***	-2.5743***	-14.8003***	-14.8003***
	(0.4872)	(0.4982)	(1.6152)	(1.6185)
Treated x April 2020	-2.9144***	-2.9144***	-22.1157***	-22.1157***
	(0.4426)	(0.4514)	(2.4187)	(2.4068)
Treated x May 2020	-2.4783***	-2.4783***	-18.6682***	-18.6682***
	(0.6372)	(0.6527)	(1.9774)	(1.9419)
Treated x June 2020	-1.8699***	-1.8699***	-11.5586***	-11.5586***
	(0.5256)	(0.5337)	(1.5328)	(1.4590)
Treated x July 2020	-1.6529***	-1.6529***	-8.8972***	-8.8972***
	(0.4471)	(0.4588)	(1.3929)	(1.2983)
Treated x August 2020	-1.3790*	-1.3790*	-8.7463***	-8.7463***
	(0.7236)	(0.7261)	(1.3493)	(1.2199)
R-squared	0.6173	0.6484	0.8678	0.8920
Observations	1,656	1,656	1,656	1,656
Province-specific monthly trends	No	Yes	No	Yes
Province-specific yearly trends	No	Yes	No	Yes
Pre-Covid-19 trends F-statistics	1.328	1.275	1.443	1.411
p-value	0.256	0.278	0.213	0.224

Notes: Monthly event-study estimates, as specified by equation (2). February 2020 is the omitted category. Robust standard errors in parentheses, clustered at the province-year level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: Robustness checks: Including province-specific time-varying indicators

	Violent crime		Property crime	
	(1)	(2)	(3)	(4)
Treated x After	-2.7239***	-2.7239***	-21.5147***	-21.5147***
	(0.5031)	(0.5274)	(2.3656)	(2.1735)
Poverty x After		0.6947**		-1.0155
		(0.3029)		(1.1144)
Essential x After		2.9310		0.0734
		(2.1854)		(6.0353)
Vulnerable x After		3.5248		1.3264
		(2.2836)		(6.2858)
Blue-collar low-skill x After		-0.6476		-0.1957
		(1.1436)		(2.7425)
Blue-collar high-skill x After		-0.4209		-1.0947
		(1.0040)		(2.8478)
White-collar low-skill x After		-0.6256		-6.5031*
		(1.1432)		(3.6784)
White-collar high-skill x After		2.2881*		-4.2808
		(1.2402)		(5.2148)
R-squared	0.6198	0.6220	0.8702	0.8709
Observations	1,656	1,656	1,656	1,656
Pre-Covid-19 x after F-statistics		1.760		1.091
p-value		0.100		0.372

Notes: Robust standard errors in parentheses, clustered at the province-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Robustness checks: Placebo test

	Violent crime (1)	Property crime (2)
Fake treated x After	0.001	0.002
	(0.174)	(0.537)
Reject H0 at 1%	0.015	0.009
Reject H0 at 5%	0.054	0.047
Reject H0 at 10%	0.105	0.103
Observations	1,500	1,500

Notes: Robust standard errors in parentheses, clustered at the province-year level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.