Stay at home if you can:
COVID-19 stay-at-home guidelines and local crime[*]

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Abstract

Government responses to the COVID-19 pandemic had an unprecedented impact on mobility patterns with implications for public safety and crime dynamics in countries across the planet. This paper explores the effect of stay-at-home guidelines on thefts and robberies at the neighborhood level in a Latin American city. We exploit neighborhood heterogeneity in the ability of working adults to comply with stay-at-home recommendations and use difference-in-differences and event-study designs to identify the causal effect of COVID-19 mobility restrictions on the monthly number of thefts and robberies reported to police across neighborhoods in Montevideo (Uruguay) in 2020. Our results show that neighborhoods with a higher share of residents with work-from-home jobs experienced a larger reduction in reported thefts in relation to neighborhoods with a lower share of residents with work-from-home jobs. In contrast, both groups of neighborhoods experienced a similar reduction in the number of reported robberies. These findings cast light on opportunity structures for crime but also on how crime during the pandemic has disproportionately affected more vulnerable areas and households.

Keywords: crime, COVID-19, lockdown, crime opportunities, work from home

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1 Introduction

The COVID-19 pandemic is a historical event that significantly affected the lives of millions across the world. As of mid-2021, there have been more than 170 million confirmed cases, and the death toll has surpassed 3.5 million (World Health Organization, 2021). In order to slow down the spread and community transmission of SARS-CoV-2 and avoid the collapse of healthcare systems, governments have imposed different types of mobility restrictions. These restrictions have included a wide range of measures: instruction to stay at home, closure of non-essential business, educational institutions, and places of entertainment, ban on social gatherings, domestic and international travel restrictions, and (sometimes) fines for those that breach these rules. The coronavirus pandemic and the government responses that followed not only had an unprecedented impact on population mobility patterns (Nouvellet et al., 2021), but they also generated substantial economic and social costs in terms of health, unemployment, poverty, mental well-being, and children education (Debata et al., 2020; Kaffenberger, 2021; Nicola et al., 2020; Pieh et al., 2021; Xiong et al., 2020).

Given its impact on mobility patterns, routine activities, and social interactions, there has been increasing interest in understanding the relationship between the pandemic and crime. Catastrophic events provide a unique opportunity to analyze human behavior in a "natural experiment setting." In fact, the COVID-19 pandemic has been considered the "largest criminological experiment in history" (Stickle & Felson, 2020), where it is plausible to assume the exogenous variation of key causal mechanisms associated with crime in the structure of opportunities and psychological strains given the speed and intensity of changes in crime rates when the pandemic started (Andresen & Hodgkinson, 2020; Felson et al., 2020; Eisner & Nivette, 2020; Halford et al., 2020; Piquero, 2021; Stickle & Felson, 2020).

In the context of a worldwide decline in crime (Nivette et al., 2021b), there has been a growing number of studies evaluating the impact of the COVID-19 pandemic on different types of property and violent criminal offenses. However, the relation between the pandemic, changes in social life and opportunities, and crime might be very different depending on the specific government responses, the socio-economic, institutional, and social characteristics of countries (Andresen & Hodgkinson, 2020; Brantingham et al., 2021). For instance, the decrease in crime might be smaller in countries like Sweden, where less restrictive measures have been implemented (Gerell et al., 2020); likewise, the impact of the pandemic on crime might be more accentuated in developing countries affected by poverty, unemployment, and pre-existent levels of street and organized crime (Calderon-Anyosa & Kaufman, 2021; Estévez-Soto, 2021).

COVID-19 crime research has mainly focused on developed countries with low levels of crime, mostly in the Northern Hemisphere, and with highly restrictive lockdown measures. Additionally, most of this research uses variation in large geographic areas (i.e., cities, districts, countries), potentially hiding relevant neighborhood heterogeneities that are key to understanding the local impact of the pandemic on crime. A further problem is the limited inclusion of population mobility measures. Most of these studies do not include mobility measures and infer them by comparing different periods and restricted areas, while those few studies that include urban mobility are limited to short-term periods and country or city level analysis. Additionally, this research focuses on how pandemic restrictions affect crime through altering criminal opportunities and, at best, how it indirectly affects legitimate opportunities. However, little is known about how the diversity of economic opportunities and constraints might differentially affect the capacity of households to comply with stay-at-home restrictions and avoid the risk of being victimized on the streets.

Our paper contributes to the existing literature by exploring the heterogeneous effect of government stay-at-home guidelines on thefts and robberies at the neighborhood level in Montevideo (Uruguay), a Latin American city that has recently experienced a spike in crime and violence. [1] We include a long period of eight years of pre-pandemic observations and a long pandemic period of nine months. In addition, we exploit the heterogeneity in working adults' ability to work from home and comply with stay-at-home recommendations across neighborhoods in Montevideo using two different measures: (1) an index based on the occupation of household members, and (2) an index based on survey responses about households' actual work at home during the pandemic.

We use difference-in-differences and event study designs to identify the causal effect of COVID-19 mobility restrictions on the number of thefts and robberies in Montevideo. We identify neighborhoods with the highest share of residents with work-from-home jobs as the treated geographical units (or alternatively, those who report working from home during the pandemic) and neighborhoods with the lowest proportion of residents with work-from-home jobs (or those who report leaving their homes in order to work) as the control group. Our key assumption is that the higher the share of residents with work-from-home jobs in a neighborhood, the lower the urban mobility of potential victims in that area and, thus, lower chances of experiencing victimization. We report an average treatment effect of -25% to -40% for the number of reported thefts during the pandemic months of 2020 (i.e., a larger decrease in treated neighborhoods). In contrast, both treated and untreated neighborhoods experienced a

[1] Thefts (≈ 45%) and robberies (≈ 15%) account for about 60% of the crimes reported to police in Montevideo.

comparable decrease in the number of robberies reported to the police during the pandemic months of 2020.

The rest of the paper is organized as follows. Section 2.1 presents the theoretical framework, and Section 2.2 reviews previous empirical research. Sections 3 and 4 introduce the COVID-19 pandemic in Uruguay and present some preliminary evidence regarding stay-at-home restrictions and street crime in Montevideo. Section 5 presents the quasi-experimental design, the identification strategy, and the main results of the paper. Finally, in Section 6, we conclude with a discussion of our results, policy implications, and limitations of this study.

2 COVID-19 and crime

2.1 Opportunities, strains, and the pandemic

One of the most relevant mechanisms through which the COVID-19 pandemic and subsequent government restrictions can change crime is through a change in criminal opportunities. According to routine activity theory, crime is unevenly distributed across space and time (Weisburd, 2015) and is the product of criminal opportunities which take place when three conditions converge in space and time: a motivated offender, adequate criminal targets (valuable objects or potential victims), and absence of capable informal guardianship against crime (Cohen & Felson, 1979; Felson & Eckert, 2018). The pandemic generated a significant disruption of routine activities and thus the convergence of the three conditions. However, not all crimes are affected in the same way.

The decrease in mobility observed during the pandemic implies that fewer potential victims and perpetrators are circulating and thus fewer opportunities for thefts, street robberies, assaults, and violent interactions between citizens (Ashby, 2020; Buchanan et al., 2020). There are also reduced opportunities for shoplifting given that several stores are closed or have time restrictions which reduces the availability of potential targets and increases guardianship of owners or employees (Campedelli et al., 2020; Payne et al., 2021). The lack of mobility increases the presence of informal guardianship in houses, making more visible prowling behaviors and thus decreasing the opportunities for residential burglary, vehicle thefts, and thefts from vehicles (Buchanan et al., 2020; Campedelli et al., 2020; Halford et al., 2020; Payne et al., 2021). However, commercial burglaries and even vandalism might increase, given that the closure of convenience stores, restaurants, and supermarkets reduces informal guardianship of employees, customers, or even bystanders (Felson et al., 2020; Payne et al., 2021).

Opportunities also have a central role in the Economic Model of Crime which is consistent with the motivated offender of Routine Activity Theory (Clarke & Felson, 2017). Individuals evaluate the expected costs and benefits of legal and illegal activities and choose to commit crimes when their payoffs exceed those of legal activities (Becker, 1968). COVID-19 restrictions change the costs and benefits of illegal activities, particularly how easy it is to commit crimes and how likely it is to be detected and punished (Abrams, 2021). Given the limited availability of commercial stores, vehicles, and the absence of passers-by, crimes such as shoplifting, theft of/from vehicles, assaults, rapes and robberies will drop given the increase in expected costs (Abrams, 2021). However, some antisocial and public offenses might increase as part of the population's rejection to follow government measures, as well as drug offenses, which might be affected by increased police street presence during lockdowns (Neanidis & Rana, 2021). Homicide offenders might not be deterred by stay-at-home orders, but also many homicides might be associated with drug/gang conflicts (Abrams, 2021; De la Miyar et al., 2021). An additional mechanism associated with legitimate opportunities is the impact of the pandemic on unemployment. Higher levels of unemployment will make it less likely to obtain income through legitimate means, increasing the incentives to commit property crimes, depending on how the government responds with unemployment insurance, aid to business, etc. (Abrams, 2021).

A different mechanism associated with motivations is the role of stress and negative emotions based on strain theories. Adverse situations generate strains, and criminal responses are one way of coping and alleviating frustration and anger (Agnew, 2005). The pandemic has disrupted social life and relations in key areas of public life (e.g., work, school, social activities in the community), increasing isolation and chances that individuals will involve in crime in order to cope with three stressful situations: (i) economic problems; (ii) conflictive interactions and abuse due to extended stay-at-home periods, social isolation, and limitations of potential victims to escape to the public sphere; and (iii) losing jobs and removal from social and leisure activities to alleviate stress (Campedelli et al., 2021; Kaukinen, 2020; Kim & Phillips, 2021; Payne & Morgan, 2020; Peterman et al., 2020). The strain mechanisms might lead to an increase in both non-violent and violent property street crimes (i.e., robberies, thefts) and in the domestic sphere (intimate partner violence or child abuse), though more impact would be expected on more expressive and violent crimes where anger plays a stronger role (Campedelli et al., 2021). Additionally, suffering isolation and strains might lead individuals to alcohol consumption, potentially triggering or accentuating criminal or violent behaviors (Kim & Phillips, 2021; Payne & Morgan, 2020).

2.2 Empirical evidence

Several studies across the world report a drop in police recorded crime during the pandemic. However, research has shown that this decline is heterogeneous both across crimes and across geographical areas.

2.2.1 The impact of COVID-19 government restrictions across criminal offenses

Results for property crimes show a consistent pattern across studies. Theft and burglary reports exhibit a significant decrease due to the pandemic (e.g., Abrams, 2021; Andresen & Hodgkinson, 2020; Campedelli et al., 2021; De la Miyar et al., 2021; Hodgkinson & Andresen, 2020; Langton et al., 2021; Mohler et al., 2020; Payne et al., 2021; Poblete-Cazenave, 2020). Some studies show a significant drop in residential burglaries, while non-residential and commercial burglaries tend to exhibit either an increase or no effect (e.g., Abrams, 2021; Ashby, 2020; Felson et al., 2020; Payne et al., 2021). The impact of government restrictions during the pandemic on violent crimes is less clear. Violent crimes that involve property, such as robberies, show a drop in several studies (e.g., Abrams, 2021; Andresen & Hodgkinson, 2020; Campedelli et al., 2021; Langton et al., 2021; Lopez & Rosenfeld, 2021; Mohler et al., 2020; Payne et al., 2021; Poblete-Cazenave, 2020; Estévez-Soto, 2021). In contrast, the impact on assaults and lethal violence shows a mixed picture, with some studies showing a drop (e.g., Abrams, 2021; Gerell et al., 2020; Poblete-Cazenave, 2020; Lopez & Rosenfeld, 2021), non-significant effects (e.g., Ashby, 2020; Mohler et al., 2020; Perez-Vincent et al., 2021), or even a spike (Rosenfeld & Lopez, 2020).

Conflicting results in magnitude and direction of effects might be due to differences in the characteristics of cities, type of government restrictions, period of analysis, data and, causal identification strategy. A recent cross-cultural comparative study from 23 different countries in the Americas, Europe, Middle East, and Asia shows an overall significant drop in thefts (46%), motor vehicle thefts (39%), burglaries (28%), robberies (47%), assaults (35%), and homicides (14%), but the impact was found to be heterogeneous across cities and strongly dependent on the severity of government restrictions (Nivette et al., 2021b).

2.2.2 The impact of COVID-19 government restrictions across communities

Although initial COVID-19 studies focused on large units of analysis (e.g., provinces or states, cities), recent research has focused on how the effect of the pandemic and government restrictions on crime is associated with the differential distribution of opportunities across different geographical areas of cities. For example, Felson et al. (2020) show how the increase in burglaries in Detroit (United States) during the pandemic took place mostly in areas of the city that were not exclusively residential, while no increase was observed in areas predominantly residential. Campedelli et al. (2020) evaluated the containment policies across communities in Chicago (United States) and showed that not only did significant reductions in burglaries, assaults, drug-related offenses, and robberies take place only in a few specific clusters of communities, but they were also significantly associated with some socio-economic and ecological characteristics of communities (i.e., previous levels of crime, perceived neighborhood safety, vacant housing, income diversity, poverty, population and proportion of old/young groups, self-perceived health status, and perception of neighborhood safety among local residents).

Other studies focus on the difference in criminal opportunities by analyzing the variation in lockdown stringency across districts. For example, a study in Bihar (India) by Poblete-Cazenave (2020) shows that although the state lockdown generated a significant reduction in property and violent crimes, there was a significant increase in the number of property crimes (e.g., burglaries or thefts) as the initial restrictions were relaxed in some districts. Similarly, a study conducted by Neanidis & Rana (2021) in England showed that, despite the significant drop in property and violent crime categories due to the national lockdown, the impact of local lockdowns showed less effect on crime reduction in fewer crime categories, but also heterogeneous effects depending on the type of restrictions: there was a decrease in theft and increase in antisocial behavior and possession of weapons in areas were local authorities implemented strict lockdown measures in relation to those where more lenient or null measures were implemented.

2.3 Contribution of our study

This study contributes to the COVID-19 and crime literature in three ways. First, although COVID-19 research has assessed variation across regions, cities, or districts, understanding the dynamics of crime spatial concentration requires more disaggregated data to better understand the heterogeneity of the pandemic's impact on crime across very diverse areas of cities (Andresen & Hodgkinson, 2020; Campedelli et al., 2020; Kim & Phillips, 2021). This paper uses monthly crime data to examine the heterogeneous impact of COVID-19 (and the associated mobility restrictions) across the 62 neighborhoods of the city of Montevideo, Uruguay.

Second, most research has focused on how the pandemic and government restrictions impact crime through the alteration of criminal opportunities and, in some cases, on how the economic conditions of specific areas might also affect legitimate opportunities (Andresen & Hodgkinson, 2020; Campedelli et al., 2020; Payne et al., 2021). However, less attention has been given to understanding how inequality of economic opportunities and employment conditions might affect citizens' possibilities of staying at home and avoiding the risk of victimization. A relevant aspect that affects vulnerable households in their capacity to comply with the government's stay-at-home recommendations is whether they are employed in jobs that cannot be performed at home, as close physical contact with others is required (Guntin, 2021). Our main results use a measure of household members' ability to work from home based on the tasks required by their occupations. To the best of our knowledge, this aspect has not been tested in the literature.

Third, most of the studies have limitations both in terms of measurement and identification strategy. One advantage of using the aforementioned measures is that they provide a more direct estimation of population mobility which plays a key role in explaining the impact that government restrictions during the pandemic had on crime. While some studies have included direct measures of mobility using Google mobility reports (Abrams, 2021; Halford et al., 2020; Langton et al., 2021; Mohler et al., 2020), the temporal and spatial availability of data does not allow the analysis for periods longer than one year (Estévez-Soto, 2021) or meso-level neighborhood dynamics.

In terms of the causal identification strategy, the great majority of COVID-19 studies are based on interrupted time series models with control groups that involve pre-pandemic periods. Few studies include actual control groups using difference-indifferences designs or regression discontinuity designs but (generally) do not include actual measures of mobility (e.g., Poblete-Cazenave, 2020). Instead, our study exploits neighborhood heterogeneity in levels of compliance associated with working adults' ability to work from home and their survey reports about actual mobility in order to identify the causal effect of COVID-19 mobility restrictions on robberies and thefts. We focus on these two crimes for two reasons: (i) While both theft and robbery involve the unlawful taking or attempted taking of personal property, robbery also involves the use of force or the threat of force (i.e., we have a violent and a non-violent property crime); (ii) Together theft and robbery account for 60% of crimes reported to police in Montevideo. Other potentially interesting categories either have very low reports at the neighborhood level (e.g., homicides), or police-report statistics are less reliable under stay-at-home guidelines (e.g., domestic violence).[2]

[2] Bullinger et al. (2021) document a significant underreporting of domestic violence crimes during lockdown months for the United States due to a pandemic-related change in how victims, witnesses, and law enforcement respond to these episodes.

3 The COVID-19 pandemic in Uruguay

3.1 COVID-19 in Uruguay in 2020

The first case of COVID-19 in Uruguay was reported in Montevideo, the capital and largest city of Uruguay, on March 13th, 2020. The Uruguayan government promptly declared a national state of sanitary emergency: all public events and potential centers of social gathering (e.g., bars, churches, shopping malls) were immediately shut down, as well as private and public schools, while flights were suspended and country borders were closed (completely with Argentina but partially with Brazil due to a dry land border).

Thanks to a fast response from both the health authorities and its people, Uruguay was able to limit the spread of the virus and was initially seen as a global model for how to respond to this worldwide pandemic (Taylor, 2021). As depicted in Figure 1, the government was successful at controlling the spread of the coronavirus SARS-CoV-2 for most of 2020.[3] Daily confirmed new COVID-19 cases were below 100 until November, and the 3.5-million-people country only recorded a total of about 19,000 cases and less than 200 deaths from this disease during that year.

[INSERT FIGURE 1 HERE]

In contrast to other countries that imposed strict lockdowns to stop the spread of the virus, Uruguay did not enforce a countrywide lockdown or mandatory house confinement. Instead, Uruguay's national government appealed to the responsibility of its citizens. Its population was urged to reduce mobility, stay at home, and work from home whenever possible. Figure 2 shows the Oxford COVID-19 Government Response Tracker's (OxCGRT) stringency index (Hale et al., 2021) and two Google mobility indices (Google, 2020a,b) for Uruguay and five other countries for comparison purposes (i.e., Argentina, Brazil, Peru, Spain, and the United States).[4] Uruguay's 2020 outstanding performance was not the result of stringent measures. In fact, its COVID-19 policies were among the less strict in all of the Americas,[5] with a stringency index value well below that of countries like Argentina, Brazil, Peru, or the US (New York State). Just a few weeks after the first case was reported, the national government started to progressively ease several of the pandemic-related restrictions. On April 13th, construction resumed its activity, while some schools started re-opening on June 15th. As a result, workplace mobility soon returned to its pre-

[3] COVID-19 data was obtained from the R package *COVID19* by Guidotti & Ardia (2020).

[4] Mobility data was obtained from the R package *COVID19* by Guidotti & Ardia (2020).

[5] See Roser & Ortiz-Ospina (2021) for a comparative evolution of OxCGRT's stringency index for the Americas and the rest of the world.

COVID levels (not seasonally adjusted). In contrast, urban mobility for retail and recreation places (i.e., restaurants, cafes, shopping centers, parks, museums, libraries, and movie theaters) also increased but without returning to its early-2020 levels (e.g., cafes and restaurants were forced to reduce operating hours while movie theaters remained closed for the rest of 2020). Towards the end of 2020, the national government was forced to reimpose several mobility restrictions as COVID-19 cases spiked (see Figure 1). In fact, the national health care system was under severe strain in 2021 as the country faced a world-record COVID-19 infection rate (Hale et al., 2021).

[INSERT FIGURE 2 HERE]

As a result of these mild mobility restrictions, combined with the initial success at controlling the spread of the virus, people quickly returned to some of their normal activities and, in a matter of weeks, mobility was close to its pre-pandemic level. However, since households were asked to reduce mobility according to their own possibilities and mandatory house confinement was never imposed, the aggregate decrease in urban mobility may hide a heterogeneous response as working from home was not an option for everyone. In this paper, we exploit this heterogeneity to identify different mobility patterns within relatively small geographical areas in the city of Montevideo.

3.2 Working from home in Montevideo

In March 2020, residents of Montevideo were asked to reduce mobility and, if possible, work from home. However, since many jobs cannot be performed from home, some workers may be unable or unwilling to follow stay-at-home recommendations. Following the methodology of Dingel & Neiman (2020) and Mongey et al. (2021), Guntin (2021) shows that based on the tasks required by their occupations, almost 8 out of 10 Uruguayans are likely unable to perform their work duties from home.[6] For example, occupation sectors (2-digit ISCO-08) such as information and communication technology, general and keyboard clerks, and administrative and commercial managers are mostly related to tasks that can be performed at home and do not require close contact with others (e.g., software developers, office clerks, or commercial managers). In contrast, occupation sectors such as skilled forestry, fishery and haunting, stationary plant and machine operators, or personal service workers mostly relate to tasks that require

[6] In order to identify which occupations can be performed at home, Guntin (2021) employs data on 8-digit O*NET-SOC occupations' tasks from the O*NET Program, a project based on surveys of a large pool of workers and firms in the United States and developed under the sponsorship of the U.S. Department of Labor. Regarding the characteristics of households and workers in Montevideo, the author takes advantage of the 2019 *Encuesta Continua de Hogares* conducted by Uruguay's National Institute of Statistics on a 40,000-representative sample of the Uruguayan population.

both presence and close contact (e.g., agricultural producers, fishers, manufacturing workers, waiters, or hairdressers). In addition, Guntin (2021) shows that income-poor workers are less likely to have work-from-home jobs than income-rich workers.[7] For example, Figure 3 shows a positive correlation between the ability to work from home and labor income across Montevideo's 62 neighborhoods.

[INSERT FIGURE 3 HERE]

According to Guntin's estimates for Montevideo, 50% of workers living in neighborhoods within the top quartile of income exhibit work-from-home capabilities, while only 13% of those residing in neighborhoods within the lowest quartile are able to work remotely. These results are in line with estimates reported in the 2020 *Encuesta Continua de Hogares* (Continuous Household Survey) conducted by Uruguay's National Institute of Statistics (INE, for its acronym in Spanish). In 2020, due to the COVID-19 pandemic, INE asked respondents if they had been working from home since March 2020.[8] While 49% of workers with homes in neighborhoods within the top quartile of income reported a shift to work-from-home in April (i.e., the month with the lowest workplace mobility; see Figure 2), only 10% of the workers living in neighborhoods in the lowest quartile worked remotely in the first full month of the pandemic.

Since this heterogeneity in work-from-home possibilities translates into different levels of compliance with COVID-19 stay-at-home recommendations across neighborhoods in Montevideo, in this paper, we exploit these local differences in behavior to study the extent to which stay-at-home guidelines in Uruguay's largest city were associated with a decrease in levels of the two most frequent offenses: theft and robberies. For our main results, we rely on Guntin's estimates for two reasons. First, this approach allows our paper to interact with a novel but growing literature on work-from-home ability measures (Brynjolfsson et al., 2020; Dingel & Neiman, 2020; Gottlieb et al., 2020; Mongey et al., 2021). Second, since these measures can be easily computed, our empirical strategy could be replicated for other countries to test the impact of government restrictions during the COVID-19 pandemic on crime and other local outcomes. Nevertheless, we also employ INE's remote work question to show the robustness of our empirical findings.

[7] Guntin (2021) calculates the mean for the normalized O*NET task-level score. Scores for each task-occupation range from 1 to 5 (the higher the score, the easier to work remotely). Occupations with a score of 4 or higher are considered suitable for work from home.

[8] INE's *Encuesta Continua de Hogares* asked two questions regarding remote work: if the respondent usually works from home and if the respondent worked from home the week before. We use the latter since this is the one consistent with how employment and unemployment statistics are computed (i.e., "did you work for at least one hour last week?").

4 Data and preliminary evidence

4.1 City-level effects

Empirical evidence suggests that stay-at-home policies were associated with a large drop in urban crime across the world during 2020 (Nivette et al., 2021b). Montevideo was not the exception. We obtained geospatial data on the thefts and robberies reported to police in Montevideo from the Ministry of Interior of Uruguay.[9] Figure 4 shows the time series of daily police reports of thefts and robberies in Montevideo for the sample period 01/01/2014 to 12/31/2020. There is a clear sudden drop in the number of offenses that coincides with the beginning of the national state of sanitary emergency (March 13th). The number of police reports increased after the government relaxed several stay-at-home recommendations and workplace mobility recovered its pre-COVID levels. However, neither thefts nor robberies reached late-2019 levels.

[INSERT FIGURE 4 HERE]

We first use event-study designs to start exploring the impact of COVID-19 guidelines on the most frequent police reports in Montevideo. To measure the average percentage change in reports, we estimate the following static specification:

$$\log y_{mt} = \delta \times D_{mt}^{2020} + \mu_m + \gamma_t + \varepsilon_{mt}, \tag{1}$$

where y_{mt} is the total number of incidents reported in Montevideo for a given type of crime (thefts or robberies) in month m of year t, D_{mt}^{2020} is a post-treatment dummy variable equal to 1 from March to December 2020, μ_m denotes month fixed effects to account for monthly seasonality ($m = 1, \dots, 12$), and γ_t denotes year fixed effects to account for time-specific shocks ($t = 2014, \dots, 2020$). In this specification, the coefficient δ represents the average percentage change in reports after the stay-at-home measures began in March 2020. Results for theft and robbery are reported in Tables A1 and A2 of the appendix. As expected, point estimates are negative and statistically significant. On average, theft reports dropped about 23%, and robbery reports dropped about 30% during the 2020 pandemic months (i.e., March to December). When the specification includes a linear time trend instead of year fixed effects, the drops are 18% and 12% for theft and robbery, respectively.

[9] Theft is defined as stealing when there is no violence included, whereas robbery is the use of violence or threat of using violence in order to steal. These two crimes account for about 60% of the total number of offenses reported to police in Montevideo. In 2019, the daily average number of thefts and robberies reported to police was 175 (standard deviation: 23.4) and 67 (12.5), respectively, while in 2020, these figures were 160 (26.6) for theft and 64 (15.4) for robbery. Burglary is not explicitly defined by the Uruguayan Penal Code.

Next, we measure the percentage change in reports for each month using the following dynamic specification:

$$\log y_{mt} = \sum_{\tau=3}^{12} \delta_\tau \times D_{\tau mt}^{2020} + \mu_m + \gamma_t + \varepsilon_{mt}, \qquad (2)$$

where $D_{\tau mt}^{2020}$ are post-treatment dummy variables for each month from March to December 2020. Results for theft and robbery show point estimates that are generally negative and statistically significant when we include year fixed effects (Tables A1 and A2 of the appendix). This is not always the case when we replace γ_t for a linear time trend. In addition, we observe a drop in reports that was larger in the first few months of the pandemic when the Uruguayan government tightened COVID-19 restrictions. The resulting temporal patterns are shown in Figure 5.

[INSERT FIGURE 5 HERE]

Overall, our point estimates depict a pattern consistent with the evolution of the pandemic in Uruguay. However, we can identify some differences between the results for thefts and robberies. In the case of thefts, the maximum effect takes place in April, the same month the OxCGRT's stringency index peaked (Figure 2). As stay-at-home guidelines ease and urban mobility increases, the coefficients get smaller in absolute value. As a result, the size of the effect appears to be decreasing over time (this trend changes in December 2020 when mobility dropped as a consequence of Uruguay's first wave of COVID-19). In the case of robberies, reports dropped by about 40% in the first three months (April, May, and June 2020). As workplace mobility returned to normal levels during the third quarter of 2020, point estimates substantially decreased in absolute value. The size of the effect appears to increase again in the last quarter of the year, even before the beginning of the first COVID-19 wave. As mentioned before, neither thefts nor robberies returned to their pre-pandemic levels.

4.2 Neighborhood-level effects

Next, we focus on Montevideo's 62 neighborhoods. Figure 6 shows the monthly average of police reports per 10,000 habitants by neighborhood in 2019 (i.e., before the arrival of COVID-19) for theft (Panel A) and robbery (Panel B). The pre-pandemic distribution of police reports across neighborhoods suggests that thefts are more common in the inner core of the city (neighborhoods with higher density and higher income), while robberies are more common in northern parts of Montevideo (neighborhoods with lower density and lower income). Southeast neighborhoods appear to be considerably safer than those in the center and north of Montevideo, particularly for violent property crimes. South and southeast neighborhoods also exhibit a relatively larger share of residents with work-from-home jobs (Figure 3).

[INSERT FIGURE 6 HERE]

We replicate the estimates of Equation 1 for each of the 62 neighborhoods in Montevideo. The average percentage change in reports after stay-at-home measures began in March 2020 ($\hat{\delta}$ coefficients) for theft and robbery are reported in the maps shown in Figure 7. The results show point estimates that are mostly negative, with substantial heterogeneity across Montevideo's 62 neighborhoods.

[INSERT FIGURE 7 HERE]

The results in this section document the impact of COVID-19 guidelines on property crimes reported to police in Montevideo during the pandemic months of 2020. Next, we exploit neighborhood heterogeneity in the ability of working adults to comply with stay-at-home recommendations to isolate the role of urban mobility as a determinant of the criminal patterns observed during the COVID-19 pandemic in Montevideo.

5 Quasi-experimental design

5.1 Identification strategy

Next, we ask to what extent these differences in the effects observed across neighborhoods respond to different urban mobility patterns. In other words, is higher mobility at the neighborhood level during the COVID-19 pandemic associated with a less than average decrease in street crime for that neighborhood? Since neither Google nor Apple provide mobility data at the neighborhood level (e.g., Google provides daily data for the Montevideo Metropolitan Area but not for its within-city communities), we need an alternative measure of mobility to answer this question. Our paper uses Guntin's (2021) analysis to distinguish Montevideo's neighborhoods with a high ability to comply with stay-at-home guidelines from those neighborhoods where residents are less able to work from home and reduce mobility.

The key assumption underlying our identification strategy is that a higher share of residents with work-from-home jobs in a neighborhood predicts a lower level of urban mobility in that geographical area. While we do not have neighborhood data on urban mobility, Google mobility indexes suggest a strong correlation between workplace mobility and retail and recreation mobility (see, e.g., Figure 2). In addition, empirical evidence suggests that working-from-home contributes to a reduction in non-work activities. For example, Rafiq et al. (2022) combine US county-level data from the Maryland Transportation Institute and Google Mobility Reports and show that working from home is associated with both fewer workplace visits and fewer non-workplace visits that might have occurred as a part of work tours. In other words, we are assuming that new non-work routines did not offset the mobility reduction caused by the pandemic´s impact on work-related habits. As a result, we would expect lower daily mobility in space and time for potential crime targets in neighborhoods with a relatively higher share of work-from-home jobs, hence a relatively larger fall in crime (i.e., a decrease in crime that takes place in public areas and homes, such as thefts and robberies). [10]

For our main results, we identify neighborhoods with the highest share of residents with work-from-home jobs (the top quartile) as the treated geographical units, while neighborhoods with the lowest proportion of residents with work-from-home jobs (the bottom quartile) as our control group. Figure 8 shows the classification of Montevideo's 62 neighborhoods into treated, untreated, and excluded neighborhoods.

[INSERT FIGURE 8 HERE]

5.2 Difference-in-differences designs

In this section, we use two difference-in-differences designs to identify the potential causal effect of COVID-19 mobility restrictions on the number of thefts and robberies reported to police in Montevideo. First, we estimate the following dynamic difference-in-differences design:

$$\log y_{imt} = \delta \times D_{mt}^{2020} + \sum_{s \neq 2019} \beta_s \times D_{mt}^{s} \times \mathrm{Treat}_i + \alpha_i + \mu_m + \gamma_t + \varepsilon_{imt}, \qquad (3)$$

where y_{imt} is the number of incidents reported in neighborhood *i* in month *m* of year *t*, D_{mt}^{s} is a dummy variable equal to 1 from March to December of year s, and Treat_i is 1 if neighborhood *i* is considered treated (i.e., top work-from-home quartile). In this equation, α_i denotes neighborhood fixed effects to account for differences between the included neighborhoods ($i \in \{1, \dots, 62\}$), μ_m denotes month fixed effects to account for monthly seasonality ($m = 1, \dots, 12$), γ_t denotes year fixed effects to account for trends in crime common to all neighborhoods ($t = 2014, \dots, 2020$). In addition, we report results allowing for neighborhood-year fixed effects to account for neighborhood-specific trends.

In this equation, the coefficient δ represents the average drop in reports for the untreated neighborhoods (i.e., bottom work-from-home quartile) after the stay-at-home measures began on March 2020. The event-study coefficients, β_s, represent the average difference in crime reports between high- and low-mobility neighborhoods in Montevideo in year s, and, as a result, β_{2020} is the average treatment

[10] Note that if the COVID-19 pandemic caused larger adverse effects on labor-market outcomes to those with lower capacity to work from home, then our estimates would be biased against finding any treatment effects.

effect. If Equation 3 captures the crime trends observed in these groups before March 2020 accurately, then the coefficients β_s for $s < 2020$ should not be statistically different from zero.

Figure 9 shows the event-study coefficient estimates for theft (top) and robbery (bottom) under specifications 1 (left) and 2 (right). In all cases, we report ordinary-least-squares estimates with standard errors clustered at the neighborhood level (Tables A3 and A4 of the appendix). Under specification 1, the average treatment effect, $\hat{\beta}_{2020}$, is negative and statistically significant for both crimes. Neighborhoods from the top work-from-home quartile experienced an average decrease of 13% in theft reports and 32% in robbery reports, in addition to the decrease observed in neighborhoods from the bottom quartile. Under this specification, we find no evidence of significant differences in trends in the pre-treatment period for theft. For robbery, however, we find evidence of significant differences in trends under specification 1 since we reject the hypothesis that $\beta_s = 0$ in 2014, 2016, and 2017 (i.e., there is evidence of a "pre-trend"). Under specification 2, the average treatment effect remains negative (-27% for theft and -7% for robbery) but statistically significant only in the case of theft. Once we account for neighborhood-year fixed effects (i.e., under specification 2), we find no evidence of significant differences in trends in the pre-treatment period for both crimes.

[INSERT FIGURE 9 HERE]

We evaluate the sensitivity of our treatment effect estimates to violations of the parallel trends assumption following Rambachan & Roth (2022). We test the sensitivity in the post-treatment period relative to observed violations in the pre-treatment period. In Figure 10, "Original" represents our OLS estimate of the average treatment effect in the post-treatment period (i.e., March to December 2020). "C-LF" are robust confidence sets for the treatment effect in 2020 for different values of $\bar{M}$. For example, when $\bar{M} = 1$ we are bounding the worst-case post-treatment difference in trends to be no larger than the maximum violation in the pre-treatment period (for further details, see p. 12 in Rambachan & Roth, 2022). According to these results, under specification 1 our post-treatment estimates are only robust to violations of the parallel trends assumption of about 50% of the maximal pre-treatment violation. Under specification 2, our significant post-treatment results for theft are robust to violations of parallel trends at least as large as the maximal pre-treatment violation ($\bar{M} = 1$). In sum, we find evidence of a robust treatment effect for theft but not for robbery.

[INSERT FIGURE 10 HERE]

Next, we consider a dynamic difference-in-differences design with monthly treatment variables. Following Miller et al. (2019), we estimate:

$$\log y_{imt} = \sum_{\tau=3}^{12} \delta_{\tau}^{2019} \times D_{\tau mt}^{2019} + \sum_{\tau=3}^{12} \delta_{\tau}^{2020} \times D_{\tau mt}^{2020} + \sum_{\tau=3}^{12} \beta_{\tau}^{2019} \times D_{\tau mt}^{2019} \times \mathrm{Treat}_i + \sum_{\tau=3}^{12} \beta_{\tau}^{2020} \times D_{\tau mt}^{2020} \times \mathrm{Treat}_i + \alpha_i + \mu_m + \gamma_t + \varepsilon_{imt}, \quad (4)$$

where $D_{\tau mt}^{2019}$ are monthly pre-treatment placebo dummy variables for March to December 2019, and $D_{\tau mt}^{2020}$ are monthly post-treatment dummy variables for March to December 2020. Therefore, we include ten immediate leads and ten immediate lags relative to the January-February 2020 period (recall that stay-at-home restrictions entered into force on March 13th, 2020). An absence of statistically significant leads suggests no differences in pre-treatment trends and provides evidence in support of the parallel trends assumption (Cunningham, 2021).[11] Given our parallel trends sensitivity results (Figure 10), we report results allowing for neighborhood-year fixed effects (i.e., under specification 2).

Figure 11 shows the event-study placebo coefficients $\hat{\beta}_{\tau}^{2019}$ and the monthly treatment effects $\hat{\beta}_{\tau}^{2020}$ for theft (top) and robbery (bottom). Note that the solid grey line indicates the month on which stay-at-home restrictions were implemented (i.e., March 2020), that January and February coefficients are set to zero, and that all event studies were plotted on the same scale. In the case of theft, most pre-treatment coefficient estimates are not significantly different from zero, and, as a result, there is no evidence of differences in pre-treatment trends between the treated and untreated neighborhoods in 2019. In contrast, after March 2020 we find a persistent treatment effect. The estimated coefficients suggest that theft reports in the treated neighborhoods dropped by an additional 25% to 40% during the pandemic months of 2020. For robberies, in contrast, we do not find evidence of a substantial treatment effect after March 2020. The bottom panel of Figure 11 shows no difference in most of the pre- and post-treatment coefficient estimates. Except for the months of June and July 2020, both treated and untreated neighborhoods experienced a similar decline in reported robberies during the pandemic months of 2020.

[INSERT FIGURE 11 HERE]

Figure 12 shows the lead and lag estimated effects for theft in the untreated neighborhoods with a low share of work-from-home jobs ($\hat{\delta}_{\tau}^{2019}$ and $\hat{\delta}_{\tau}^{2020}$ with $\tau = 3, \dots, 12$) and the treated neighborhoods with a high share of work-from-home jobs ($\hat{\delta}_{\tau}^{2019} + \hat{\beta}_{\tau}^{2019}$ and $\hat{\delta}_{\tau}^{2020} + \hat{\beta}_{\tau}^{2020}$ with $\tau = 3, \dots, 12$). Our estimates show a decline in theft reports in the untreated neighborhoods that was small and short-lived

[11] See Cunningham (2021) for a complete and updated discussion on the difference-in-differences design and how to provide evidence for parallel trends through event studies.

(top panel of Figure 12). In contrast, the decline in theft reports in the treated neighborhoods was significant and persistent (bottom panel of Figure 12). Figure 13 shows the same estimated effects for robbery. Except for the months of June and July 2020, both treated and untreated neighborhoods experienced a similar decline in reported robberies during the pandemic months of 2020.

[INSERT FIGURES 12 AND 13 HERE]

5.3 Robustness

For our main results, we compared neighborhoods with the largest share of residents with work-from-home jobs (the top 25%) to neighborhoods with the smallest share of residents with work-from-home jobs (the bottom 25%). To rank Montevideo's 62 neighborhoods, we relied on estimates of the ability of households and workers to work-from-home based on the tasks required by their occupations following Guntin (2021). In this section, we report results from several robustness checks (findings are illustrated in Figures A1 to A12 of the appendix).

First, we repeat the analysis using an alternative measure of work-from-home. Instead of the index based on tasks of Guntin (2021) (we call this work-from-home *tasks*), here we employ INE's remote work question (we call this work-from-home *survey*). Our results show a strong correlation between the two indexes (Figure A1), few changes to the relative ranking of Montevideo's 62 neighborhoods (Figure A2) and, as a result, our empirical findings are robust to the choice of index (Figure A3).

We also evaluate the robustness of the results to the threshold used to select treated and untreated neighborhoods. Our main results used an arbitrary threshold of 25% (i.e., top versus bottom quartiles). In addition, we report results using 33% (Figures A4 and A5) and 50% (Figures A6 and A7) thresholds. Overall, our results are robust to the choice of threshold. Nevertheless, as more neighborhoods are added to the treated and control groups, the differences between groups become less apparent.

Next, we evaluate the robustness of the results to the time periods dropped to avoid perfect multicollinearity. In our main specification we dropped the January and February dummy variables (i.e., the two months immediately before the arrival of COVID-19 in Uruguay). As a result, the 2020 (year) fixed effect is estimated based on those two time periods. In addition, we report results obtained assuming a 2020 (year) fixed effect equal to the 2019 fixed effect (i.e., we drop all the 2019 dummy variables but none of the 2020 variables). In the case of theft, we find similar results for the untreated neighborhoods, a persistent decline in reports in the treated neighborhoods that is somewhat smaller than what was reported before, and a persistent treatment effect that is not always statistically significant (Figure A8).

In the case of robbery, we do not observe important differences with what was reported before (Figure A8).

Next, we repeat our main estimations but only employing police reports of criminal incidents that took place during the usual rush hours: 6:00 am to 9:59 am (i.e., people going to work from home) and 4:00 pm to 7:59 pm (i.e., people going home from work). If the assumption underlying our identification strategy is true, then our results should be robust to the exclusion of crimes that did not take place during the usual commuting hours. At other times of the day, it can be argued that working-from-home habits could lead to higher *local* urban mobility in residential neighborhoods (e.g., going for lunch at the local restaurant). Again, we do not observe significant differences with our main estimation results (Figure A9).

We also exclude police reports associated with offenses that did not take place in a public space. Since burglary is not explicitly defined by the Uruguayan Penal Code, police officers usually indicate when the crime took place at a residence or inside a building. We repeat our estimations but excluding police reports that exhibit the usual elements of a burglary. Our results remain qualitatively unchanged (Figure A10).

Finally, it seems reasonable to assume that most concomitant COVID-19 policies impacted residents from both groups equally during the period of analysis (except for their ability to stay home, of course). However, it is also true that urban mobility at the neighborhood level is not just affected by local residents. To address this point, we remove the central business district (CBD) from our group of treated neighborhoods as the work-from-home approach could be hiding the effect of nonresidents that are not commuting to these neighborhoods due to COVID-19 guidelines—that, for example, result in business closures—but are potential offenders or victims. According to Mauttone & Hernández (2017), Municipio B is the municipality that attracts most of the trips in the city. For the sake of precision, we use these neighborhoods to identify the actual limits of Montevideo's CBD (Figure A11). We do not observe important differences with respect to what was reported before (Figure A12).

Overall, the difference-in-differences results reported in Section 5.2 appear to be robust to the different modeling choices considered in this section.

6 Discussion, policy implications, and limitations

In this paper, we examine the effects of government stay-at-home guidelines on robberies and thefts in a Latin American city (Montevideo, Uruguay). Unlike many other studies, this paper includes a long pandemic evaluation sample of nine months (i.e., all of 2020). In addition, we consider a smaller unit of analysis (neighborhoods) to better evaluate the heterogeneous impact of the government restrictions during the pandemic across the city by comparing neighborhoods ranked in terms of their households' ability to work from home and to comply with government recommendations. Our results show that while there was a significant impact on the reduction of non-violent property crimes (thefts) in neighborhoods with a higher capacity to comply with government stay-at-home guidelines and less reported mobility, there were no significant differences in terms of violent property crimes (robberies).

These differences in the impact of the pandemic and government restrictions on violent and non-violent property crimes may be associated with the fact that violent crimes are being committed by antisocial individuals who are less likely to be deterred by a change in incentives or government restrictions (Payne et al., 2021). Individuals with antisocial traits or low self-control are more present-oriented, self-centered, with volatile temper, and thus, less sensitive or responsive to changes in their environment (Nagin & Paternoster, 1994; Nagin & Pogarsky, 2001). Changes in the structure of criminal opportunities generated during the pandemic may be an insufficient deterrent for these type of strongly motivated offenders (Campedelli et al., 2021), particularly if they have recently experienced economic and psychological strains (Kim & Phillips, 2021). For example, there is some preliminary evidence showing that during the pandemic, individuals with "antisocial potential" such as low self-control, low acceptance of moral and legal norms, with previous involvement in crime and with criminal peers, are less likely to comply with social distancing or government restrictions (Nivette et al., 2021a; O'Connell et al., 2021).

Our results may also be showing a "functional crime displacement" (Barr & Pease, 1990) from non-violent to violent property crimes where, after some weeks of reduced mobility, offenders are adapting to a new context with fewer victims in the street and more protected residences (Gerell et al., 2020). Offenders that used to take advantage of empty houses and surroundings to commit burglaries in pre-pandemic times might have to increasingly face owners in their houses or neighbors in their surroundings and may need to use violence to be successful in the obtention of illicit goods. Although there are fewer victims on the streets, there is also less informal guardianship (e.g., bystanders) or formal guardianship (e.g., police) and, thus, in line with rational choice theory (Becker, 1968) and routine activity theory (Felson & Eckert, 2018), there is an increase in the expected gain from robberies with available victims in the streets (Cheung & Gunby, 2022).

Our results are also in line with other studies that show how larger drops in crime during the pandemic appear to take place in specific city clusters with high pre-pandemic levels of crime (Campedelli et al., 2020). More research is needed to understand how the link between the pandemic, government restrictions, and crime is affected by neighborhood characteristics which involve not only pre-existent crime rates but also key ecological and socio-economic aspects such as poverty and income diversity, the proportion of young males, unemployment, vacant housing, or social disorganization (Campedelli et al., 2020). Particularly, future studies should incorporate more explicitly the environmental perspective to evaluate if the heterogeneous impact of the pandemic and government restrictions across neighborhoods can be explained by the presence of two types of areas: 'crime generator areas' where people with no criminal motivations gather, and crimes take place due to the concentration of potential victims and offenders (e.g., parks); or 'crime attractor areas' which are characterized by existent opportunities for specific crimes which attract strongly motivated offenders (e.g., drug market areas, red district prostitution areas) (Brantingham & Brantingham, 1995). While some authors have predicted that crime drop will take place in both types of areas (see Campedelli et al., 2020), it is also possible that the drop in crime attractor areas might be less significant due to the aforementioned inelasticity of motivated offenders.

Finally, our results also show how the pandemic's impact on crime is disproportionately affecting more vulnerable areas and households. COVID-19 research has mainly emphasized how the pandemic and government restrictions affect crime through the alteration of criminal opportunities (Felson et al., 2020). Additionally, some studies show how these criminal opportunities may interact with heterogeneity in economic and job opportunities across different regions during the pandemic (e.g., Andresen & Hodgkinson, 2020; Campedelli et al., 2020; Kirchmaier & Villa-Llera, 2020; Payne et al., 2021). However, one thing is to show how economic pressures generate psychological stress and motivation to get involved in crime in line with strain theory (Agnew, 2005), and another is to evaluate how inequality of economic opportunities might differentially affect the capacity of households to comply with stay-at-home restrictions and avoid the risk of being victimized in public areas. While the former explains the presence of motivated offenders, the latter explains the presence of victims as easy targets when there is less formal and informal guardianship due to the pandemic (Stickle & Felson, 2020). This differential capacity to stay at home and avoid victimization is relevant given that crime in the city disproportionally concentrates in a few "hotspots" (Jaitman & Ajzenman, 2016; Weisburd, 2015) and "harmspots" (Weinborn et al., 2017) in the case of more harmful or severe crimes.

In this paper, we show that this cost of crime disproportionally concentrates in more vulnerable and disadvantaged areas and adds to multiple negative impacts suffered by income-poor households less likely to have work-from-home jobs. While it has been documented that social distancing and lockdown measures have widened inequality around the world, the focus of this literature has been on the pandemic's effect on labor, health, and education outcomes (Adams-Prassl et al., 2020; Almeida et al., 2021; Alstadsæter et al., 2020; Atolia et al., 2021; Bonacini et al., 2021; Chiou & Tucker,2020; Deaton, 2021; Furceri et al., 2020; Garrote Sanchez et al., 2021; Palomino et al., 2020; Yamamura & Tsustsui, 2021). According to our results, the ability to work from home has benefits related to crime victimization that, to the best of our knowledge, have been overlooked so far.

This study has some implications for crime prevention, a topic that has not been thoroughly discussed in the COVID-19 and crime literature (Piquero, 2021). First, the pandemic has given credit to situational crime prevention strategies (Eck & Clarke, 2019), showing that opportunities matter and crime can significantly be reduced, particularly when there is little chance of having encounters between motivated offenders and unprotected victims. At the same time, our results indicate that violent property crimes are less sensitive to the drastic reduction of opportunities, and thus, prevention efforts might require a different approach when it comes to more motivated offenders that are willing to commit a crime and use violence when the illegal opportunities have shrunk. Likewise, while reductions of opportunities seem to have an immediate effect on property crimes, and particularly on non-violent ones, the effects of psychological stressors and strains might probably have mid or long-term effects on crime, when they accumulate, become more frequent, and more intense (Eisner & Nivette, 2020; Payne et al., 2021). Additionally, these psychological mechanisms might be more relevant in the long term when government measures are relaxed, and changes in routines will be less important. Thus, crime prevention will require complementing the hardening of criminal opportunities with social programs that focus on helping more vulnerable households to alleviate the strains product of the socio-economic crisis and improving police-community relations (Lopez & Rosenfeld, 2021). The need to evaluate the lessons of the criminal justice system responses during the COVID-19 pandemic (Piquero, 2021) and to evaluate the inclusion of specific social crime prevention programs is particularly relevant in regions such as Latin America where the pandemic took place in the context of very poor socio-economic conditions, a weak criminal justice system and state institutions, and high levels of crime and violence (Bergman, 2018; Marmolejo et al., 2020).

This study has some limitations that future research should address. First, there is a scope limitation. This study only involved the analysis of two crime categories (thefts and robberies) in a single city. Thus, results may not generalize to other cities or countries that have been affected by the pandemic and government restrictions in 2020 even in the same region. Replicating our analysis to other cities in

Latin America and in other regions of the world, particularly applying Guntin's (2021) measure, will improve our understanding of the heterogeneity of COVID-19 effect on different crimes across small geographical areas of cities. Since most of the empirical research on COVID-19 and crime has been conducted in the United States, Europe, and Australia, results might be different in locations where there is considerable variation in pre-pandemic crime levels, type and strength of government restrictions, and population's adherence to stay-at-home orders. Furthermore, future research will benefit not only by replicating our results with other aggregate crimes but particularly by including subcategories of crime (e.g., residential and non-residential burglary) which would allow a more detailed analysis.

Second, there are measurement limitations. Our analysis is based on crimes reported to the police, and there is a well-known problem in criminology of dark figures or gap between actual crimes and those that are reported or known by the police. More importantly, reporting of crimes might have been affected during the pandemic due to less willingness of victims to report due to fear of getting infected, or fewer police resources available due to infection or reallocation to tasks related to enforcing mobility restrictions or sanitary policies. Yet, some studies have claimed that underreporting during the pandemic might not be such a significant issue and provide evidence that victimization surveys also show a significant drop in some of these property crimes during the pandemic period (Vilalta et al., 2022; see also Perez-Vincent et al., 2021). More importantly, some additional robustness checks assessing the proportion of crimes reported by police and citizens or changes in trends of specific types of crimes before and during the pandemic suggest that is unlikely that reductions in property crimes are mostly based on reporting problems (e.g., Abrams, 2021). A meta-analysis presented evidence of a peak in domestic violence during the pandemic combining studies that used different measures such as police recorded data, police emergency calls, health emergency room admissions, etc. (Piquero et al., 2021). Despite domestic violence being one of the crimes where higher underreporting is expected, this study showed that official data recorded a significant increase despite its measurement bias. Future studies on COVID-19 should follow this lead in order to improve the robustness of results and triangulate crime reported to police, with other sources from health institutions, and particularly with self-report victimization data in order to assess if changes in crime reporting are the result of alternative mechanisms such as underreporting (e.g., Perez-Vincent et al., 2020).

An additional measurement limitation is that since we lack mobility data at the neighborhood level, our two measures tap only on the population that lives in the neighborhood. The pool of potential victims and offenders can involve not only residents but also nonresidents that might circulate in the area due to leisure, work, or even in the case of offenders with the purpose of committing crimes. Yet, there are reasons to believe that these limitations might be less relevant than expected. Criminological research has shown that offenders find it easier and less costly to commit crimes closer to their homes, and thus their offending drops as the distance from their homes increases (see, e.g., Block et al., 2007; Brantingham & Brantingham, 2010). Furthermore, research has shown that this "decay distance" applies also to victims of robbery (see, e.g., Luo et al., 2021; Pizarro et al., 2007).

Finally, although our study advances in the analysis of underlying heterogeneity of pandemic effects across areas of the city, more micro-level research needs to be conducted. We expect that neighborhoods in Montevideo hide significant levels of heterogeneity, and future research would benefit from incorporating smaller geographical units of analysis. A more fine-grained analysis can help us to better understand the interaction between ecological characteristics of areas, changes in different types of criminal opportunities, strategic decisions of offenders, potential displacements of crime between those areas, and how authorities might implement policies in these extraordinary conditions.

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Figures

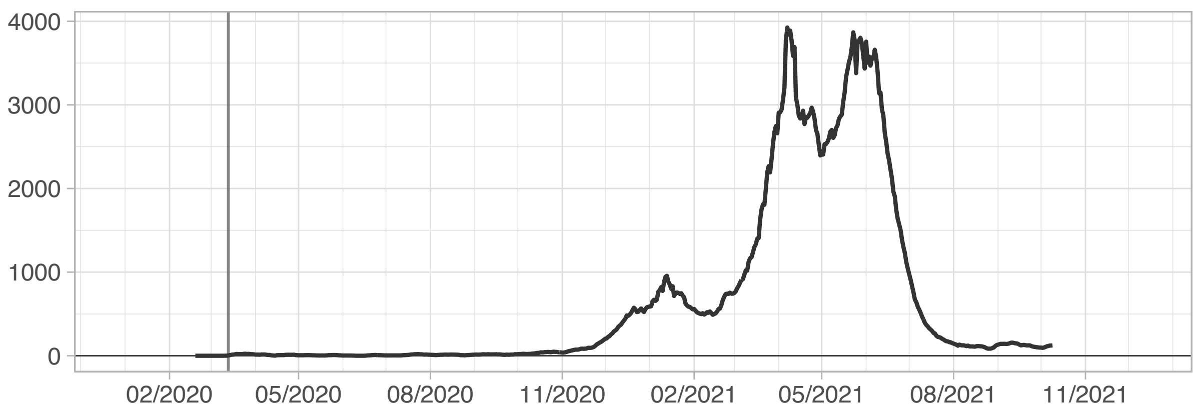


Figure 1: Time series plot of daily confirmed new COVID-19 cases in Uruguay (2020-21). The solid grey line indicates the date on which stay-at-home orders were implemented (March 13th, 2020).

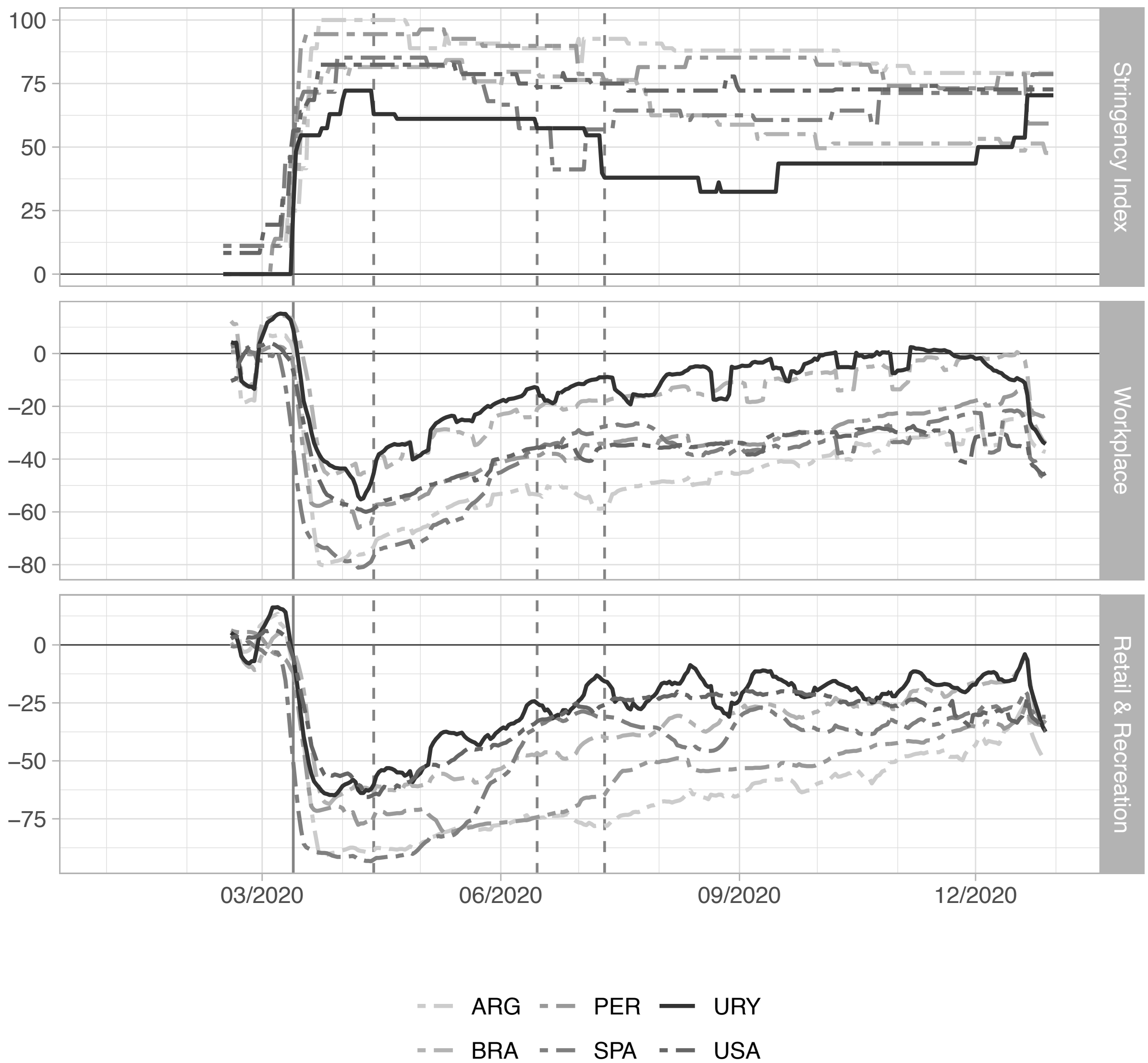


Figure 2: Time series plots of the stringency index (Hale et al., 2021) and Google mobility indexes (seven-day averages, Google, 2020a,b) for Argentina (ARG), Brazil (BRA), Peru (PER), Spain (SPA), the United States (USA; New York State), and Uruguay (URY). The solid grey line indicates the date on which stay-at-home orders were implemented (March 13th, 2020). The dashed grey lines indicate the dates on which stay-at-home orders were eased: April 13th (construction resumes activity), June 15th (first schools reopen), and July 11th (all private and public schools resume activities after winter break).

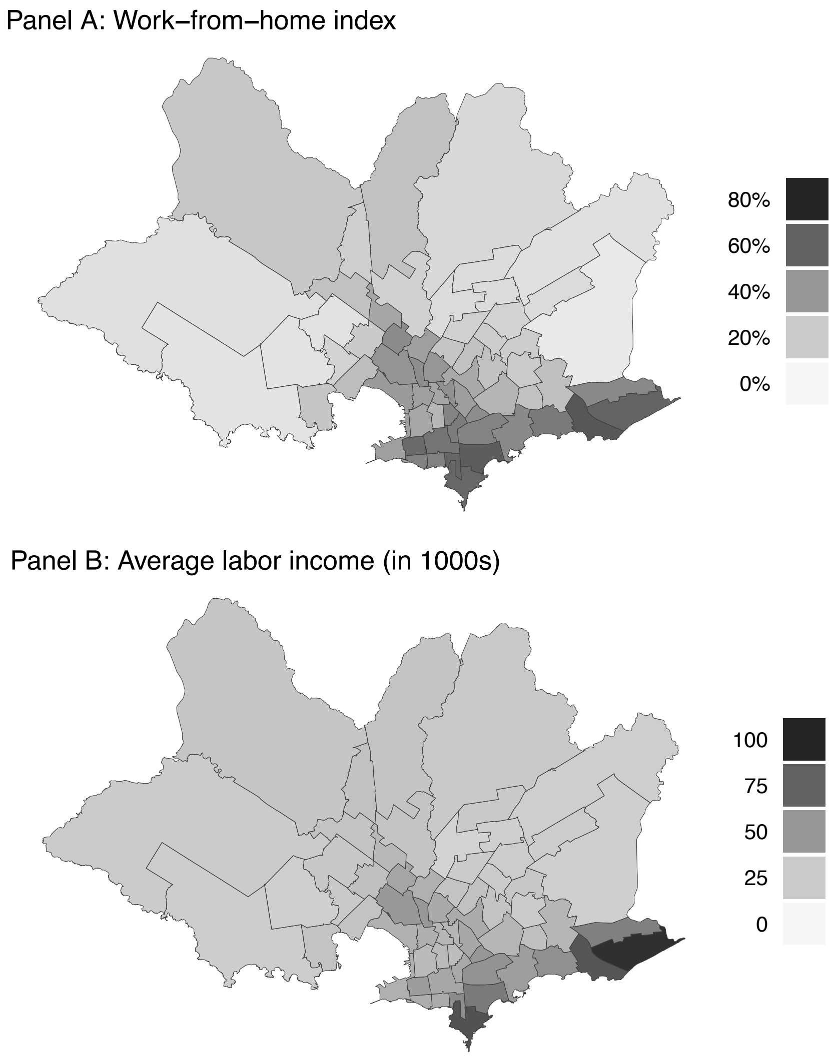


Figure 3: Work-from-home index (Guntin, 2021) and average labor income (in thousands of local currency, monthly, 2019) in Montevideo, by neighborhood.

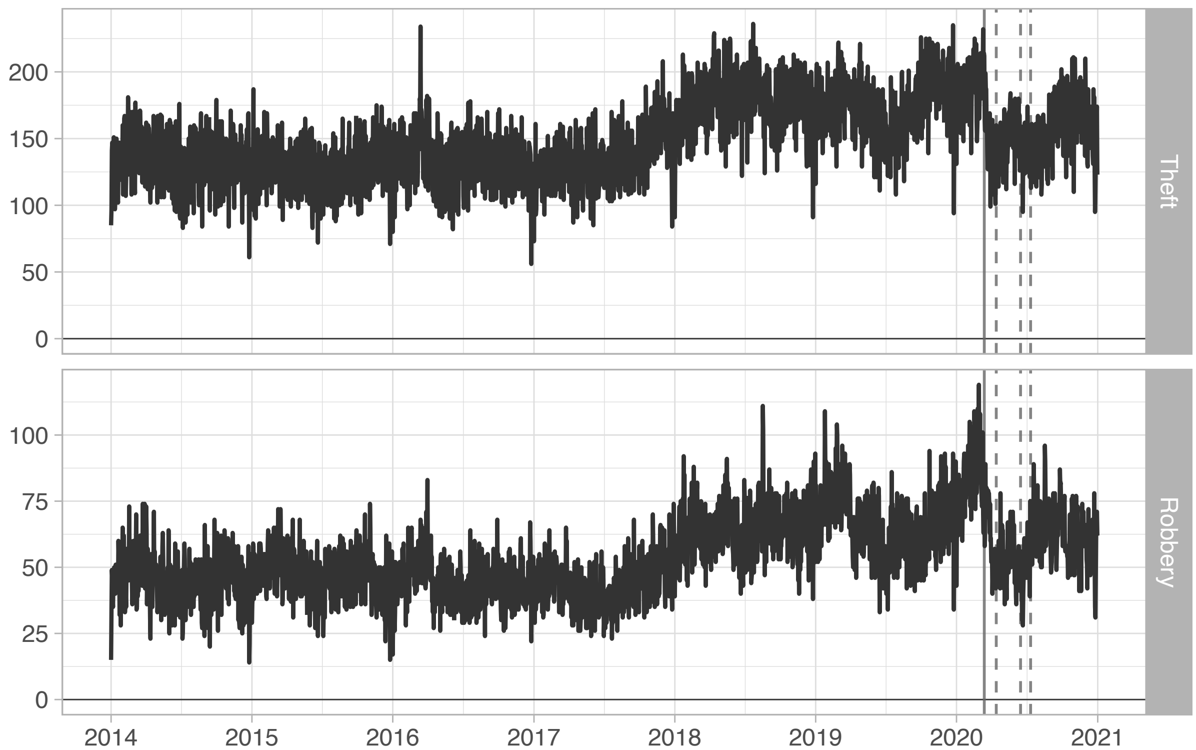


Figure 4: Time series plots of daily crime counts for theft and robbery in Montevideo. The solid grey line indicates the date on which stay-at-home orders were implemented (March 13th, 2020). The dashed grey lines indicate the dates on which stay-at-home orders were eased: April 13th (construction resumes activity), June 15th (first schools reopen), and July 11th (all private and public school resume activities after winter break).

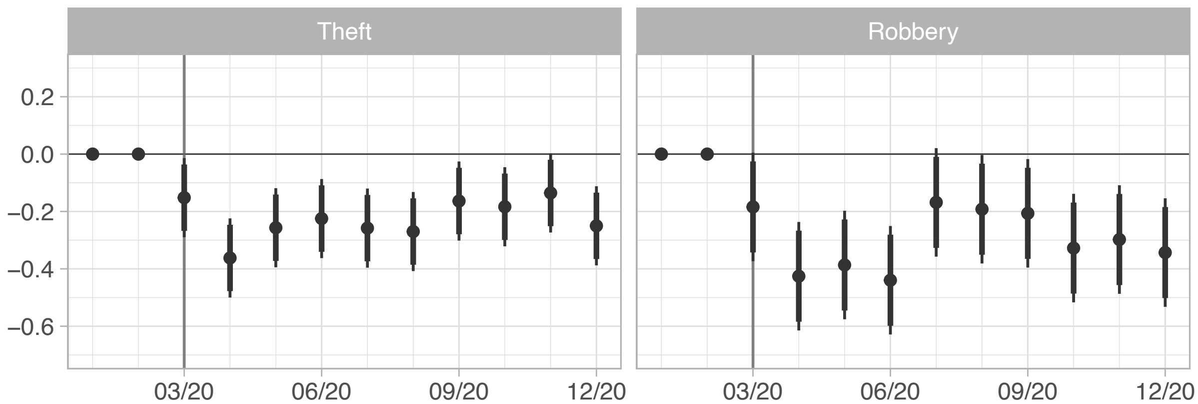
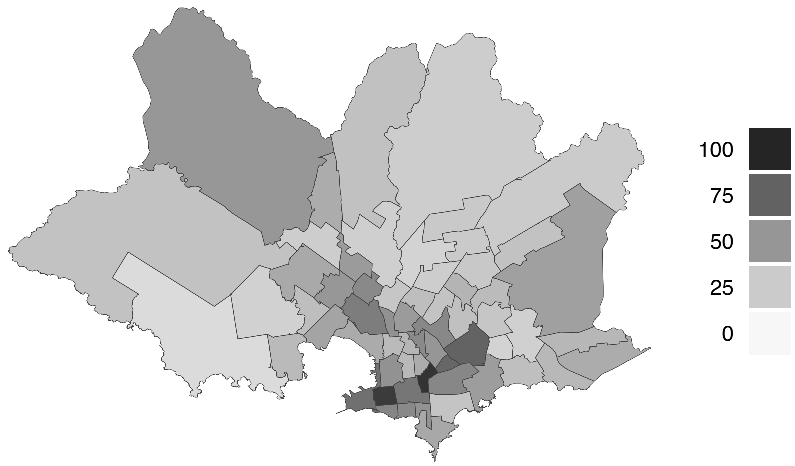


Figure 5: Estimated effects of stay-at-home orders from event-study designs for theft and robbery. Point estimates ($\hat{\delta}_\tau$), 95%, and 90% confidence intervals by month. January and February coefficients are set to zero. The solid grey line indicates the date on which stay-at-home restrictions were implemented (March 2020).

Panel A: Theft in 2019



Panel B: Robbery in 2019

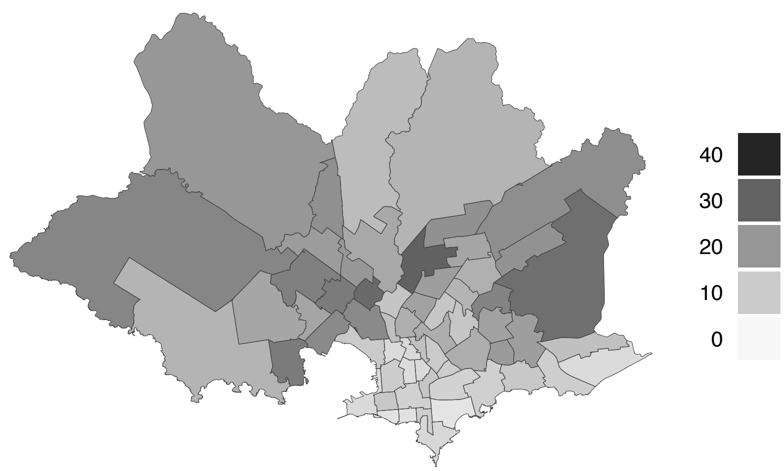


Figure 6: Pre-pandemic distribution of crime reports in Montevideo (police reports per 10,000 habitants, monthly average, 2019).

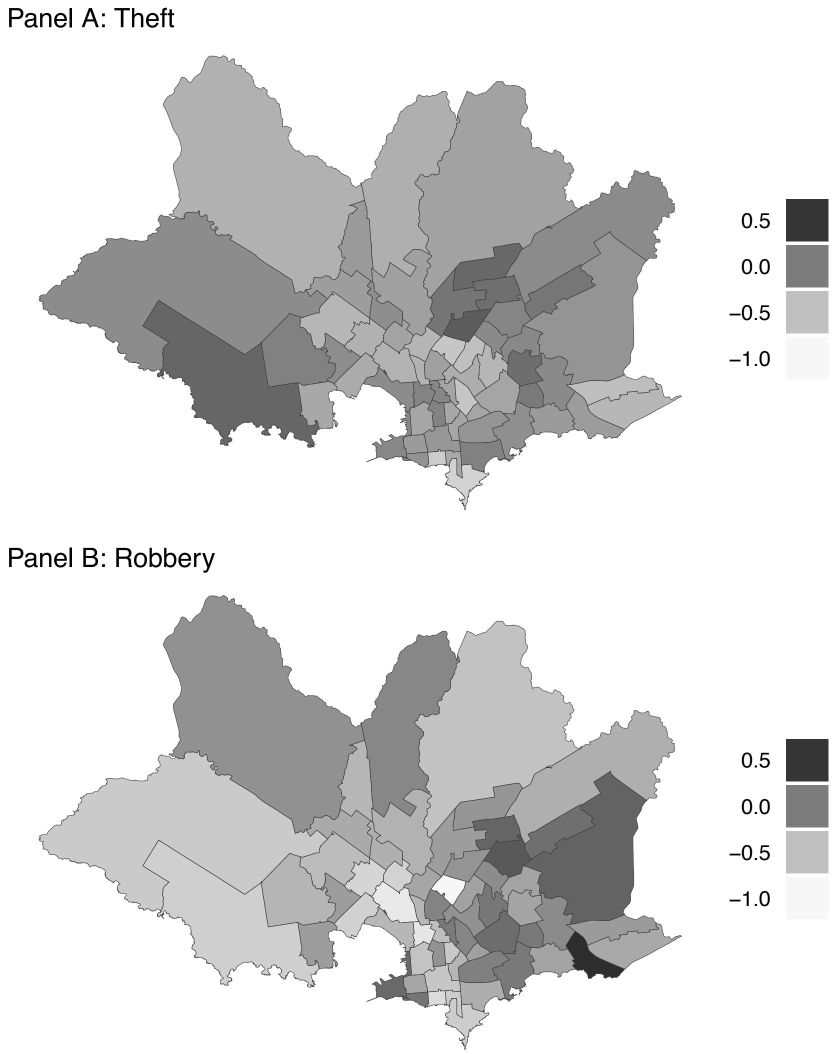


Figure 7: Event-study design results based on Equation 1 (point estimates, $\hat{\delta}$), by neighborhood.

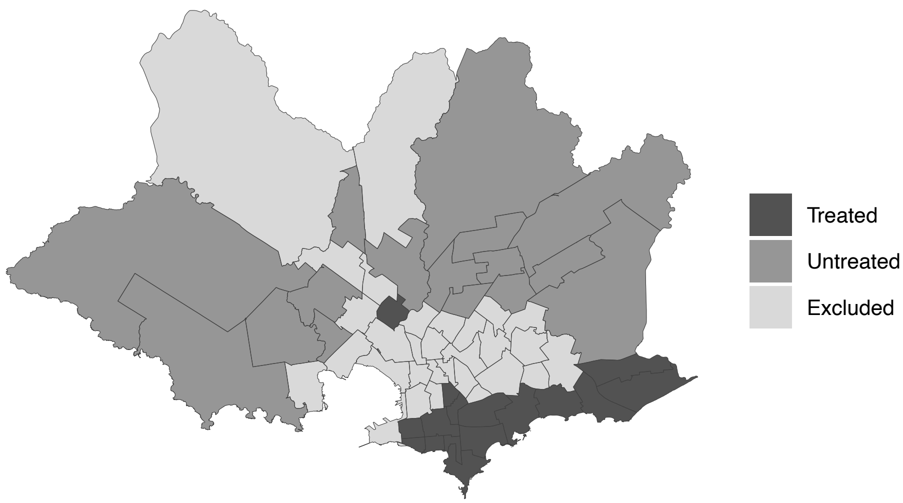


Figure 8: Treated, untreated, and excluded neighborhoods based on the work-from-home (tasks) index of Guntin (2021) and a 25% threshold.

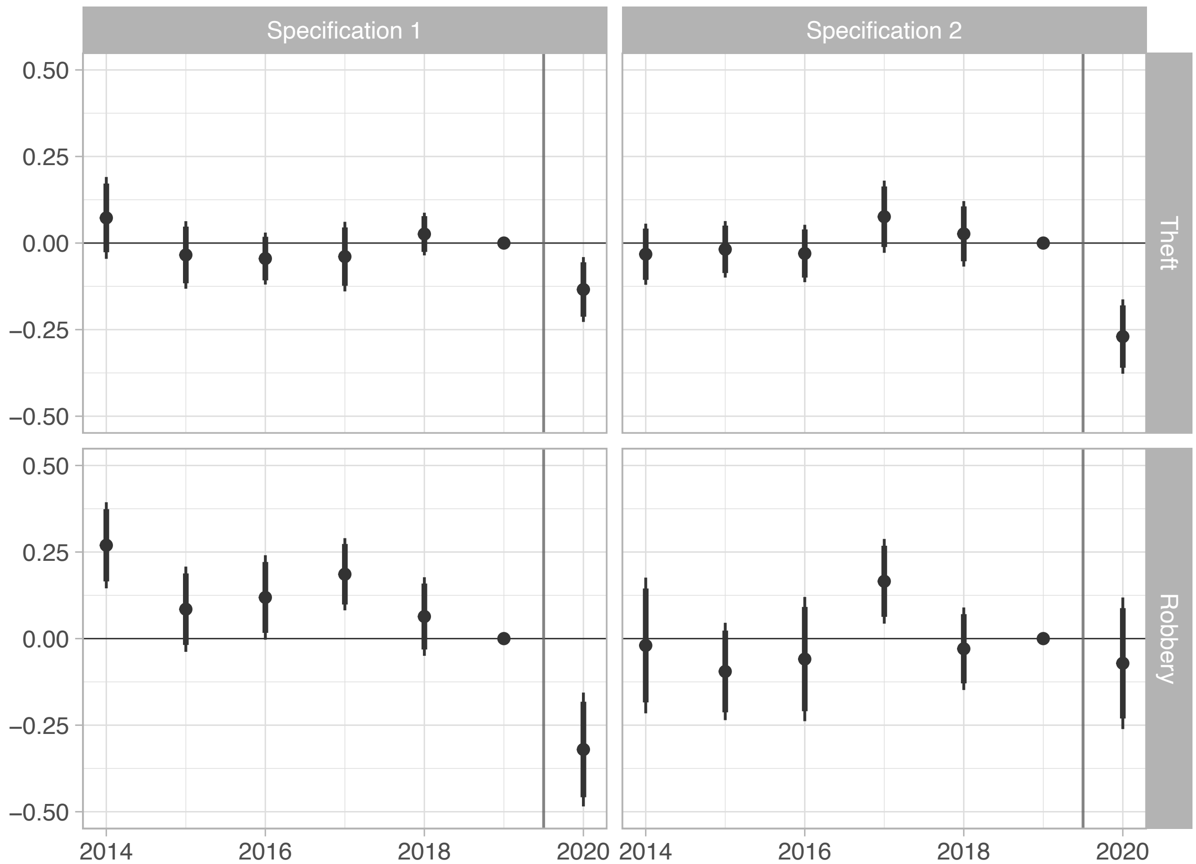


Figure 9: Dynamic difference-in-differences event-study design for theft and robbery (Equation 3). OLS point estimates, $\widehat{\beta}_s$ (with $s = 2014, \ldots, 2020$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the date on which stay-at-home restrictions were implemented (i.e., March 2020). Standard errors are clustered at the neighborhood level. Specification 1 without neighborhood-year fixed effects. Specification 2 with neighborhood-year fixed effects.

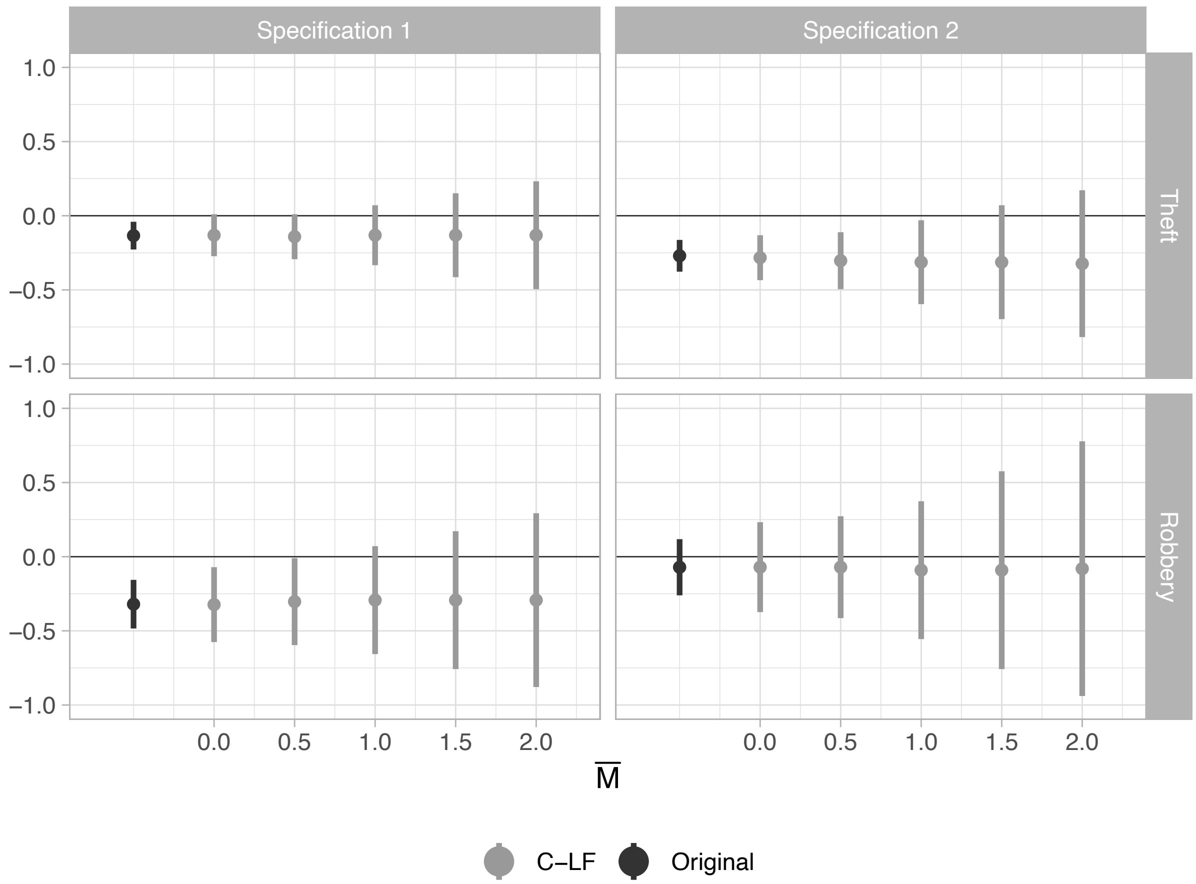


Figure 10: Robust confidence sets for the treatment effect in 2020 (Rambachan & Roth, 2022). Specification 1 without neighborhood-year fixed effects. Specification 2 with neighborhood-year fixed effects.

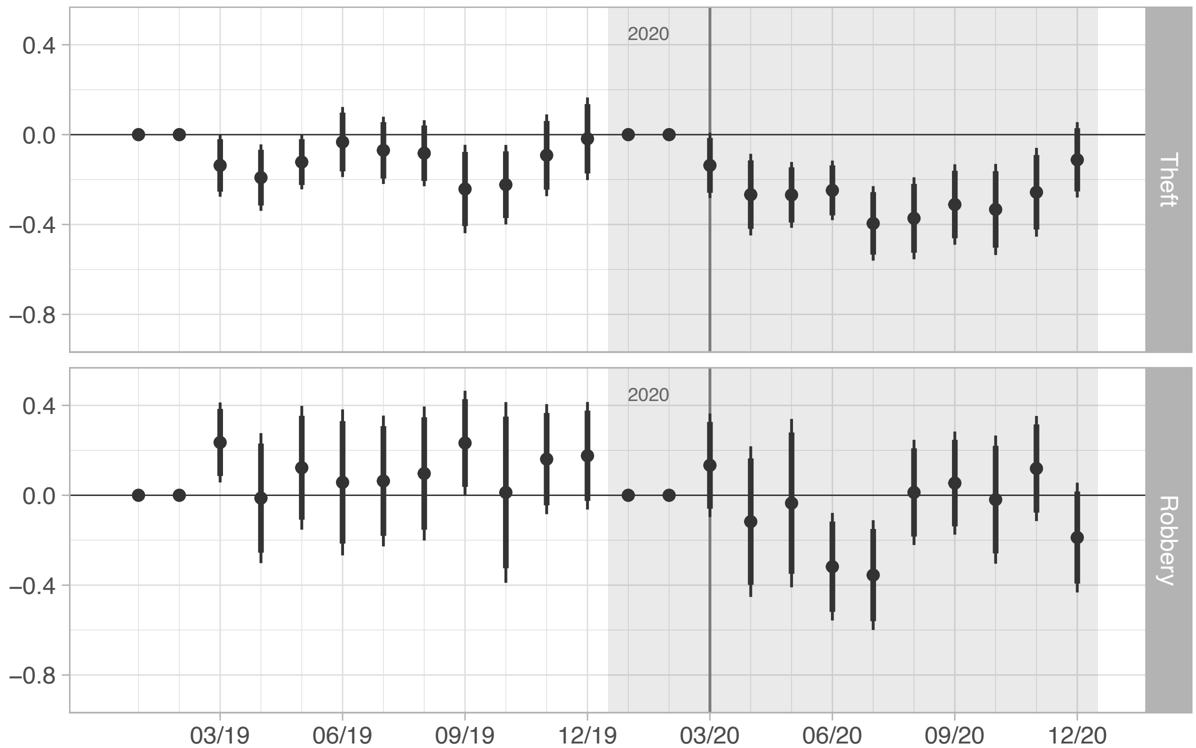


Figure 11: Dynamic difference-in-differences design for theft and robbery (Equation 4). OLS point estimates, $\hat{\beta}_{\tau}^{2019}$ and $\hat{\beta}_{\tau}^{2020}$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the date on which stay-at-home restrictions were implemented (i.e., March 2020). Standard errors are clustered at the neighborhood level.

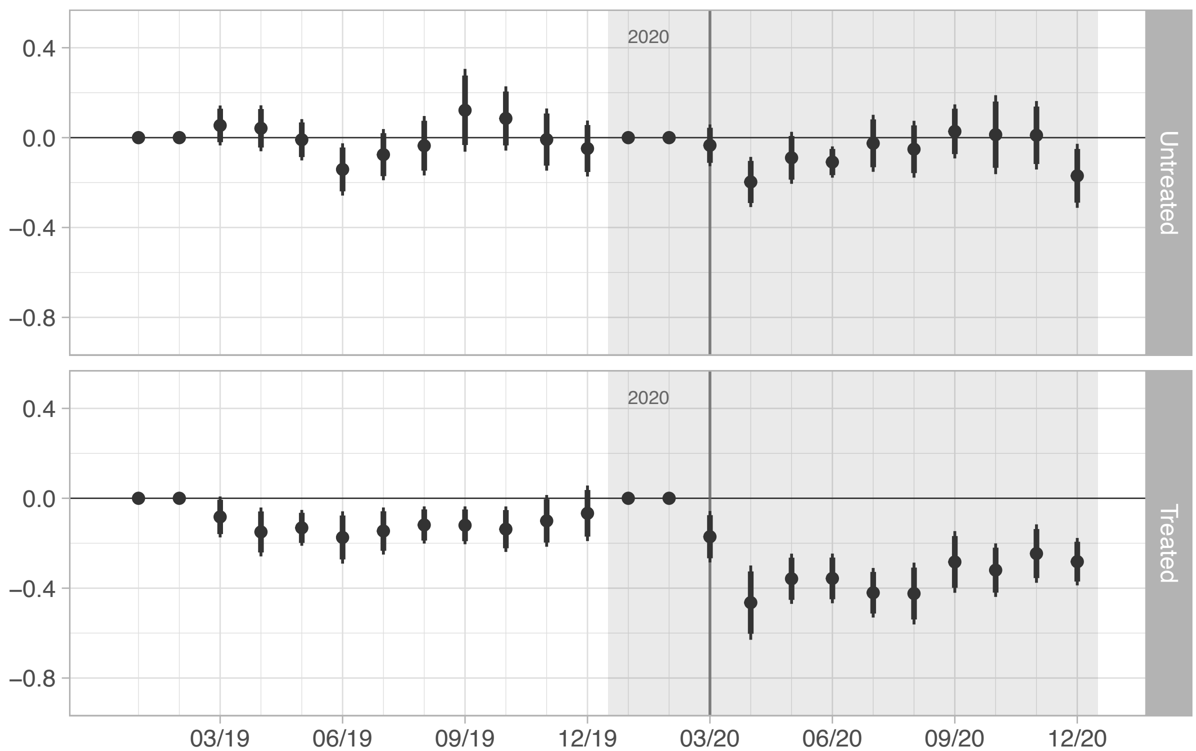


Figure 12: Event-study designs for theft (Equation 4). OLS point estimates, $\hat{\delta}_\tau^{2019}$ and $\hat{\delta}_\tau^{2020}$ (top panel; with $\tau = 3, \dots, 12$), and $\hat{\delta}_\tau^{2019} + \hat{\beta}_\tau^{2019}$ and $\hat{\delta}_\tau^{2020} + \hat{\beta}_\tau^{2020}$ (bottom panel; with $\tau = 3, \dots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the date on which stay-at-home restrictions were implemented (i.e., March 2020). Standard errors are clustered at the neighborhood level.

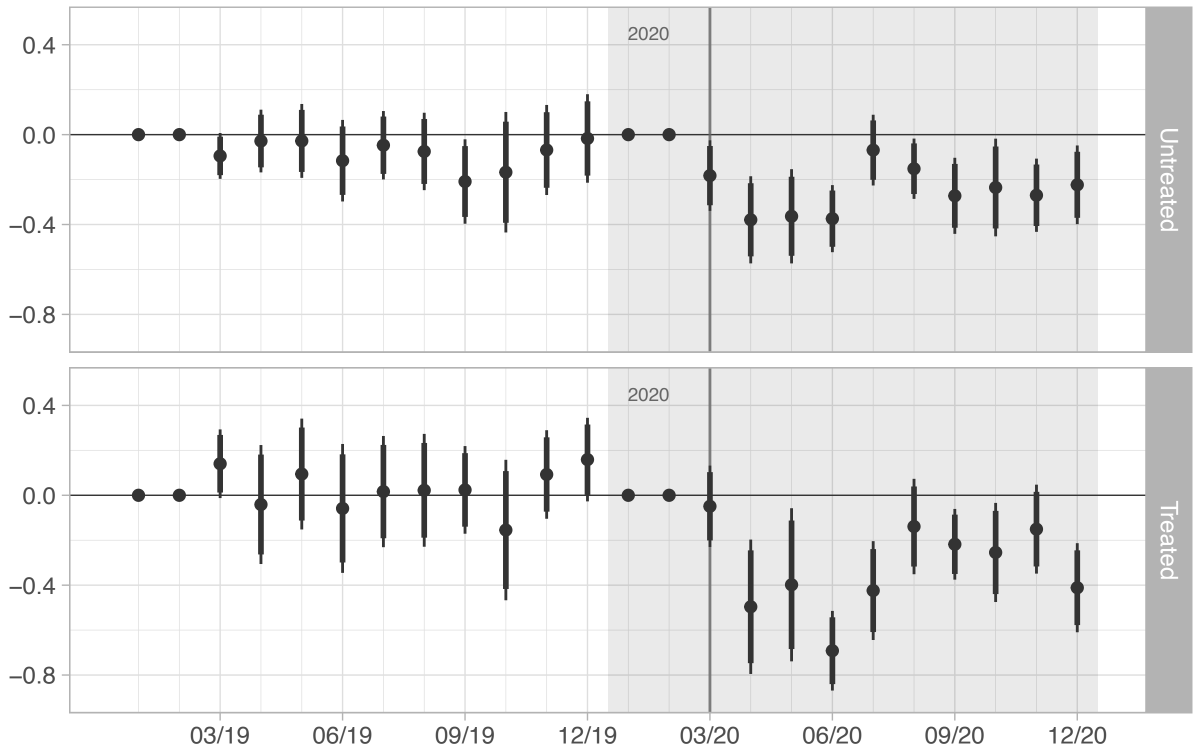


Figure 13: Event-study designs for robbery (Equation 4). OLS point estimates, $\hat{\delta}_{\tau}^{2019}$ and $\hat{\delta}_{\tau}^{2020}$ (top panel; with $\tau = 3, \ldots, 12$), and $\hat{\delta}_{\tau}^{2019} + \hat{\beta}_{\tau}^{2019}$ and $\hat{\delta}_{\tau}^{2020} + \hat{\beta}_{\tau}^{2020}$ (bottom panel; with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the date on which stay-at-home restrictions were implemented (i.e., March 2020). Standard errors are clustered at the neighborhood level.