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Stay at Home if You Can: COVID-19 Stay-at-Home Guidelines and Local Crime

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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 is disproportionately affecting more vulnerable areas and households.

Keywords: crime, rational choice, COVID-19, lockdown, crime opportunities

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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 the 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), they also generated large 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 relation between the pandemic and crime. Catastrophic events provide a unique opportunity to analyse human behavior in a "natural experiment setting". In fact, the COVID-19 pandemic has been considered the "largest criminological experiment in history" (Stickle and 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 and Hodgkinson, 2020; Felson et al., 2020; Eisner and Nivette, 2020; Halford et al., 2020; Stickle and 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 and Hodgkinson, 2020; Brantingham et al., 2021). For instance, the decrease in crime might be weaker 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 and Kaufman, 2021; Estévez-Soto, 2021).

COVID-19 crime research has mainly focused on developed countries with low levels of crime, mostly from the Northern Hemisphere, and with highly restrictive lockdown measures. Additionally, most of this research uses variation at large geographic areas (i.e., cities, districts, countries), potentially hiding relevant neighborhood heterogeneities that are key to understand 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 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. 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 that had to leave 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, less 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 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 discussing our results, policy implications, and limitations of this study.

2 COVID-19 and crime

2.1 Potential mechanisms

The COVID-19 pandemic and government restrictions that followed can change crime rates through three potential mechanisms: opportunities, strains, and public resource allocation.

The first mechanism is the change of criminal opportunities that takes place during the pandemic according to Routine Activity Theories. 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 and Felson, 1979; Felson and 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 less victims and perpetrators are circulating and thus less 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 availability of potential targets and increase 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 closure of convenience stores, restaurants, and supermarkets reduce informal guardianship of employees, customers or even bystanders (Felson et al., 2020; Payne et al., 2021). Likewise, there might be an increase in intimate partner violence during the pandemic since there are more opportunities due to the higher time that potential victims and motivated offenders are together with weak surveillance from third parties (Halford et al., 2020; Langton et al., 2021; Piquero et al., 2020).

Opportunities also have a central role in the Economic Model of Crime which is consistent with the motivated offender of Routine Activity Theories (Clarke and Felson, 2017). Individuals evaluate 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 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 rejection to follow government measures, as well as drug offenses, which might be affected by increased police street presence during lockdowns (Neanidis and 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). Furthermore, an increase in the number of domestic violence cases would be expected given the higher amount of time spent at home by victims and abusers, increased transactional costs of reporting to police, and the decrease in bargaining options outside the relationship (Abrams, 2021; Hsu and Henke, 2021; Silverio-Murillo et al., 2020).

An additional mechanism 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 attractiveness of committing 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 the 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; iii) losing jobs and removal from social and leisure activities to alleviate stress (Campedelli et al., 2020; Kaukinen, 2020; Kim and Phillips, 2021; Payne and Morgan, 2020; Peterman et al., 2020). The strain mechanisms might lead to increase in both non-violent and violent property street crimes (robberies, thefts, burglaries) and in the domestic sphere (intimate partner violence or children abuse), though more impact would be expected on more expressive and violent crimes were anger plays a stronger role (Campedelli et al., 2020). Additionally, suffering isolation and strains might lead individuals to alcohol consumption, potentially triggering or accentuating criminal or violent behaviors (Kim and Phillips, 2021; Payne and Morgan, 2020).

A third mechanism is the reallocation of police resources during the pandemic. The use police resources to enforce social distancing measures, sanitary policies, and other travel and mobility restrictions has an opportunity cost of less police forces enforcing crime and thus decreasing the chances of being detected and arrested (Kim and Phillips, 2021; Poblete-Cazenave, 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. For example, theft and burglary reports exhibit a significant decrease due to the pandemic (e.g., Abrams, 2021; Andresen and Hodgkinson, 2020; Campedelli et al., 2020; De la Miyar et al., 2021; Hodgkinson and Andresen, 2020; Langton et al., 2021; Mohler et al., 2020; Payne et al., 2021; Poblete-Cazenave, 2020). Some studies, but not all, show a significant drop in residential burglaries, while nonresidential 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 and Hodgkinson, 2020; Campedelli et al., 2020; Langton et al., 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), non-significant effects (e.g., Ashby, 2020; Mohler et al., 2020), or even a spike (Rosenfeld and 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 of 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 several 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 and Rana (2021) in England showed that, despite the significant drop in several property and violent crime categories due to the national lockdown, the impact of local lockdowns showed less effect on crime reduction, on 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 pandemic's impact on crime across very diverse areas of cities (Andresen and Hodgkinson, 2020; Campedelli et al., 2020; Kim and 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 economic conditions of specific areas might also affect legitimate opportunities (Andresen and 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 risk of victimization. A relevant aspect that affects vulnerable households in their capacity to comply with 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). To the best of our knowledge, this key aspect has not been tested in the literature. Our study includes not only Guntin's measure of household members' type of occupation, but also an additional survey were respondents answered if they were actually working from home during the pandemic.

Third, most of the studies have limitations both in terms of measurement and the 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. Several studies do not include direct measures of population mobility but rather evaluate change in crime rates comparing control and treatment periods or areas where mobility is inferred or assumed (Ashby, 2020; Andresen and Hodgkinson, 2020; Neanidis and Rana, 2021; Poblete-Cazenave, 2020). 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). However, 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 time series or interrupted time series models with control groups that involve pre-pandemic periods. Few studies include actual control groups using difference-in-differences 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 as only the former involves violence and together account for 60% of crimes reported to police.

3 The COVID-19 pandemic in Uruguay

3.1 COVID-19 in Uruguay in 2020

The first case of COVID-19 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's 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.[1] 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]

3.2 Individual freedom and social responsibility

In contrast to some countries that imposed strict lockdowns to stop the spread of the virus, Uruguay did not enforce a countrywide lockdown or mandatory house confinement.

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

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. Under President Lacalle Pou's motto "Individual Freedom with Social Responsibility," the government trusted citizens to voluntary adhere to hygiene guidelines and social distancing.

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 other South American countries.[2] 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 the Americas,[3] with a stringency index value well below that of countries like Argentina, Brazil, or Peru. 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 schools started reopening on June 29th. As a result, workplace mobility soon returned to its pre-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 Figures 1 and 2). In fact, the national health care system was under severe strain during 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

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

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

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 a mandatory house confinement was never imposed, the aggregate decrease in urban mobility may hide an 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.

3.3 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 and 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.[4] In addition, Guntin shows that income-poor workers are less likely to have work-from-home jobs than income-rich workers.[5] 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]

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

[4]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 to 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.

[5]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.

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 have been working from home since March 2020.[6] 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. Overall, these results suggest that low income neighborhoods are expected to exhibit lower shares of residents with work-from-home jobs.

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 where associated with a decrease in levels of the two most frequent offenses: theft and robberies.[7] For our main results we will 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 and 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.

[6]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?").

[7]Thefts ($\approx 45\%$) and robberies ($\approx 15\%$) account for about 60% of the crimes reported to police in Montevideo.

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 offenses reported to police in Montevideo from the Ministry of Interior of Uruguay. 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 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 first estimate the following static specification:

$$\ln y_{mt} = \delta \text{Post}_{mt} + \beta_m + \gamma_t + \varepsilon_{mt}, \tag{1}$$

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, Post_{mt} is the post-treatment variable such that Post_{mt} is equal to 1 if $m \in \{3, \ldots, 12\}$ and $t = 2020$, β_m denotes month fixed effects to account for monthly seasonality ($m = 1, \ldots, 12$), and γ_t denotes year fixed effects to account for time trends ($t = 2014, \ldots, 2020$). In this specification, the coefficient δ represents the average percentage change in reports after the stay-at-home measures begin in March 2020. Results for thefts and robberies are reported in the online appendix. We also report results obtained after replacing γ_t for a linear time trend. As expected, point estimates are negative and statistically significant. On average, theft reports dropped about 23% and robbery reports dropped about 30% during 2020 pandemic months (i.e., March to December). When the specification includes a linear time trend instead of year fixed effects the drop is 18% and 12%, respectively.

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

$$\ln y_{mt} = \sum_{\tau=3}^{12} \delta_\tau D_{\tau mt}^{2020} + \beta_m + \gamma_t + \varepsilon_{mt}, \tag{2}$$

where $D_{\tau mt}^{2020}$ are the post-treatment dummy variables for the pandemic months such that $D_{\tau mt}^{2020}$ is equal to 1 if $m = \tau$ and $t = 2020$. Results for thefts and robberies show point estimates that are generally negative and statistically significant when we include year fixed effects. This is not always the case when we replace γ_t for a linear time trend. However, point estimates are always negative for both crimes. In addition, the estimated drop in reports 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 (full tables are reported in the online appendix).

[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 in 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 a 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 thefts (Panel A) and robberies (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 the stay-at-home measures which begin 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]

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 provides 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 alternative measures 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. As a result, we would expect less daily movements 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).

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 test the following version of the canonical difference-in-differences estimator:

$$\begin{aligned}\ln y_{imt} = {} & \delta_1 \text{Pre}_{mt} + \delta_2 \text{Treat}_i \times \text{Pre}_{mt} + \delta_3 \text{Post}_{mt} + \delta_4 \text{Treat}_i \times \text{Post}_{mt} + \ldots \\ & + \alpha_i + \beta_m + \gamma_t + \theta_{it} + \varepsilon_{imt},\end{aligned} \tag{3}$$

where y_{imt} is the number of incidents reported in neighborhood i in month m of year t, Pre_{mt} is a pre-treatment placebo variable equal to 1 in the period March-December 2019, Post_{mt} is the post treatment variable equal to 1 during the treatment period March-December 2020, 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 ($i \in \{1, \ldots, 62\}$), β_m denotes month fixed effects to account for monthly seasonality ($m = 1, \ldots, 12$), γ_t denotes year fixed effects to account for trends in crime common to all neighborhoods ($t = 2014, \ldots, 2020$), and θ_{it} denotes neighborhood-year fixed effects to account for neighborhood-specific trends.

In this specification, the coefficient δ_3 represents the average drop in reports for the untreated neighborhoods (i.e., bottom work-from-home quartile) after the stay-at-home measures begin on March 13th, 2020 and the coefficient δ_4 represents the average difference in reports between high- and low-mobility neighborhoods in Montevideo (i.e., the average treatment effect). If Equation (3) captures the crime trends observed in these groups before March 2020 accurately, then the coefficients δ_1 and δ_2 should not be statistically different from zero.

The corresponding estimates for theft and robbery are reported in Tables 1 and 2, respectively. Both tables report the ordinary-least-square estimates for alternative versions of Equation 3: (i) with and without the pre-treatment dummy variables (i.e., columns (1) and (3) vs. columns (2) and (4) of Tables 1 and 2) and (ii) with and without neighborhood-year fixed effects (i.e., columns (1) and (2) vs. columns (3) and (4) of these tables).[8] The average treatment effect, $\hat{\delta}_4$, is always negative and statistically significant for both crimes when neighborhood-year fixed effects are excluded from the regression. According to these results, neighborhoods from the top work-from-home quartile experienced an average decrease of 13% in theft reports and 42% in robbery reports. In contrast, when we account for neighborhood-specific trends, the average treatment effect remains negative (-27% for theft and -7% for robbery) but statistically significant only in the case of thefts. Results suggest that neighborhood-year fixed effects are not necessary for thefts as both $\hat{\delta}_1$ and $\hat{\delta}_2$ are not statistically different from zero. In contrast, neighborhood-year fixed effects are necessary for robberies.

[INSERT TABLES 1 AND 2 HERE]

Next, we consider an event-study approach in our research design. Following Miller et al. (2019), we consider the following dynamic specification:

$$\begin{aligned} \ln y_{imt} = & \sum_{\tau=3}^{12} \delta_\tau^1 D_{\tau mt}^{2019} + \sum_{\tau=3}^{12} \delta_\tau^2 \text{Treat}_i \times D_{\tau mt}^{2019} + \dots \\ & \sum_{\tau=3}^{12} \delta_\tau^3 D_{\tau mt}^{2020} + \sum_{\tau=3}^{12} \delta_\tau^4 \text{Treat}_i \times D_{\tau mt}^{2020} + \dots \\ & + \alpha_i + \beta_m + \gamma_t + \theta_{it} + \varepsilon_{imt}, \end{aligned} \tag{4}$$

where $D_{\tau mt}^{2019}$ are monthly pre-treatment placebo dummy variables for the period March-December 2019, while $D_{\tau mt}^{2020}$ are the post-treatment dummy variables for the period March-December 2020. Therefore, we are including ten immediate leads and ten

[8]Note that when we remove the pre-treatment dummy variables we are just transforming Equation 3 into the classic two-by-two difference-in-differences model with fixed effects:

$$\ln y_{imt} = \delta_3 \text{Post}_{mt} + \delta_4 \text{Treat}_i \times \text{Post}_{mt} + \alpha_i + \beta_m + \gamma_t + \theta_{it} + \varepsilon_{imt}.$$

immediate lags relative to the January-February 2020 period (recall that stay-at-home restrictions entered into force on March 13th, 2020). Anticipatory effects allow us to provide compelling evidence that post-treatment differences between counterfactual trends and actual trends are likely to be zero (Cunningham, 2021).[9]

Figures 9 and 10 show the main results of this paper for theft and robbery, respectively. Each figure presents three event studies: the top graph plots the ordinary-least-square estimates of the lead and lag effects for the untreated neighborhoods with a low share of work-from-home jobs ($\hat{\delta}^1_\tau$ and $\hat{\delta}^3_\tau$ with $\tau = 3, \ldots, 12$), the middle graph plots the estimates of the lead and lag effects for the treated neighborhoods with a high share of work-from-home jobs ($\hat{\delta}^1_\tau + \hat{\delta}^2_\tau$ and $\hat{\delta}^3_\tau + \hat{\delta}^4_\tau$ with $\tau = 3, \ldots, 12$), while the bottom graph plots the difference between the above point estimates for each period (i.e., the placebo effects $\hat{\delta}^2_\tau$ and the actual treatment effects $\hat{\delta}^4_\tau$). Note that the solid grey line indicates the month on which stay-at-home restrictions were implemented (March 2020), that January and February coefficients are set to zero, and that all event studies were plotted on the same scale.

[INSERT FIGURES 9 AND 10 HERE]

Once again, we find some differences when we evaluate the potential impact of COVID-19 restrictions on violent and non-violent property crimes. According to the bottom panel of Figure 9, most pre-treatment coefficient estimates are not different from zero and, as a result, there is no evidence of differences in the expected number of reported thefts between these two groups (treated and untreated) in 2019. In contrast, after March 2020 we find a persistent treatment effect. The average treatment effect suggests that reported thefts in the treated neighborhoods dropped by an additional 25% to 40% during the pandemic months of 2020. Our estimates show a decline in theft

[9]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.

reports in the untreated neighborhoods that was small and temporary (top panel of Figure 9). In contrast, the decline in theft reports in the treated neighborhoods was significant and persistent (middle panel of Figure 9).

For robberies, we do not find evidence of a treatment effect after March 2020. The bottom panel in Figure 10 suggests 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.

5.3 Robustness

For our main results we compared neighborhoods with the highest share of residents with work-from-home jobs (the top 25%) to neighborhoods with the lowest 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 (all figures reported in the online appendix).

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

We also evaluate the robustness of the results to the threshold used to select treated and untreated neighborhoods. Our main results used a 25% threshold (i.e., top versus bottom quartiles). In addition, we report results using 33% and 50% 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 to 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 thefts 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. In the case of robberies we do not observe important differences with what was reported before.

Finally, we remove Montevideo's central business district (CBD) from our group of treated neighborhoods. Work-from-home statistics can hide the effect of nonresidents that are not commuting to these neighborhoods due to COVID-19 guidelines but are potential offenders or victims. According to Mauttone and Hernández (2017), Municipio B is the municipality that attracts the most trips in Montevideo. We identify the actual limits of Montevideo's CBD as the nine neighborhoods in Municipio B and remove those neighborhoods from the analysis. We do not observe important differences with what was reported before.

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-centred, with volatile temper, and thus, less sensitive or responsive to changes in their environment (Nagin and Paternoster, 1994; Nagin and 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., 2020), particularly if they have recently experienced economic and psychological strains (Kim and 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 and Pease, 1990) from non-violent to violent property crimes were, after some weeks of reduced mobility, offenders are adapting to a new context with less 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 use violence to be successful in the obtention of illicit goods. Although there are less victims in the streets, there are 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 and Eckert, 2018) there is an increase in the expected gain from robberies with available victims in the streets (Cheung and Gunby, 2021).

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, 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 type of areas: 'crime generator areas' were 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 and 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 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 and Hodgkinson, 2020; Campedelli et al., 2020; Kirchmaier and 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), 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 and Felson, 2020). This differential capacity to stay at home and avoid victimization is relevant given that crime in the city disproportionally concentrates in few "hotspots" (Jaitman and 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 and Tucker, 2020; Deaton, 2021; Furceri et al., 2020; Garrote Sanchez et al., 2021; Palomino et al., 2020; Yamamura and 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. First, the pandemic has given credit to situational crime prevention strategies (Eck and 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 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 strains might probably have mid or long-term effects on crime, when strains accumulate, become more frequent and more intense (Eisner and Nivette, 2020; Payne et al., 2021). Additionally, strains mechanisms might be more relevant in the long term when government measures relaxed and thus changes in routines will be less important. Thus, crime prevention will require to complement hardening of criminal opportunities with social programs that focus on helping more vulnerable households to alleviate the strains product of the socio-economic crisis. The need to include social crime prevention programs might be particularly relevant in some regions of the world such as Latin America where the pandemic has taken place in the context of very poor socio-economic conditions, weak criminal justice system and state institutions, and high levels of crime and violence.

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 has been conducted in the United States, Europe, and Australia, results might be different in locations where there is considerable variation of 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 nonresidential 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 figure 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 less police resources available due to infection or reallocation to tasks related to enforcing mobility restriction or sanitary policies. Some studies (e.g., Abrams, 2021) have claimed that underreporting during the pandemic might not be such a significant issue and show how the drop in crimes that are mainly reported by citizens (e.g. burglaries, robberies, thefts, etc.) is similar to those mostly reported by police (e.g. drug crimes). A recent 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). 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.

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 distance from their home increases (see, e.g., Block et al., 2007; Brantingham and 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).

Third, 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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Tables and figures

Table 1: Difference-in-differences designs for thefts

		(1)	(2)	(3)	(4)
Pre	$\hat{\delta}_1$		-0.04		0.00
			(0.05)		(0.04)
Treat × Pre	$\hat{\delta}_2$		-0.04		-0.12**
			(0.06)		(0.06)
Post	$\hat{\delta}_3$	-0.12***	-0.13***	-0.05	-0.06
		(0.04)	(0.04)	(0.04)	(0.04)
Treat × Post	$\hat{\delta}_4$	-0.13**	-0.14**	-0.27***	-0.27***
		(0.05)	(0.05)	(0.06)	(0.06)
N		2688	2688	2688	2688
AIC		-520.7	-522.4	-969.0	-976.0
FE: Neighborhood	α_i	Yes	Yes	Yes	Yes
FE: Month	β_m	Yes	Yes	Yes	Yes
FE: Year	γ_t	Yes	Yes	Yes	Yes
FE: Neighborhood × Year	θ_{it}	No	No	Yes	Yes
Standard Errors		Clustered	Clustered	Clustered	Clustered

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports the OLS estimates for alternative versions of Equation 3: (i) with and without the pre-treatment dummy variables (i.e., columns (1) and (3) vs. columns (2) and (4)), and (ii) with and without neighborhood-year fixed effects (i.e., columns (1) and (2) vs. columns (3) and (4)). Standard errors are clustered at the neighborhood level. All models were estimated using the R package *fixest* by Bergé (2018).

Table 2: Difference-in-differences designs for robberies

		(1)	(2)	(3)	(4)
Pre	$\hat{\delta}_1$		0.08		−0.09
			(0.08)		(0.07)
Treat × Pre	$\hat{\delta}_2$		−0.21***		0.11
			(0.07)		(0.10)
Post	$\hat{\delta}_3$	−0.07	−0.06	−0.25***	−0.25***
		(0.07)	(0.07)	(0.07)	(0.07)
Treat × Post	$\hat{\delta}_4$	−0.42***	−0.45***	−0.07	−0.07
		(0.09)	(0.09)	(0.10)	(0.10)
N		2688	2688	2688	2688
AIC		1760.3	1734.7	1490.8	1492.3
FE: Neighborhood	α_i	Yes	Yes	Yes	Yes
FE: Month	β_m	Yes	Yes	Yes	Yes
FE: Year	γ_t	Yes	Yes	Yes	Yes
FE: Neighborhood × Year	θ_{it}	No	No	Yes	Yes
Standard Errors		Clustered	Clustered	Clustered	Clustered

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports the OLS estimates for alternative versions of Equation 3: (i) with and without the pre-treatment dummy variables (i.e., columns (1) and (3) vs. columns (2) and (4)), and (ii) with and without neighborhood-year fixed effects (i.e., columns (1) and (2) vs. columns (3) and (4)). Standard errors are clustered at the neighborhood level. All models were estimated using the R package *fixest* by Bergé (2018).

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

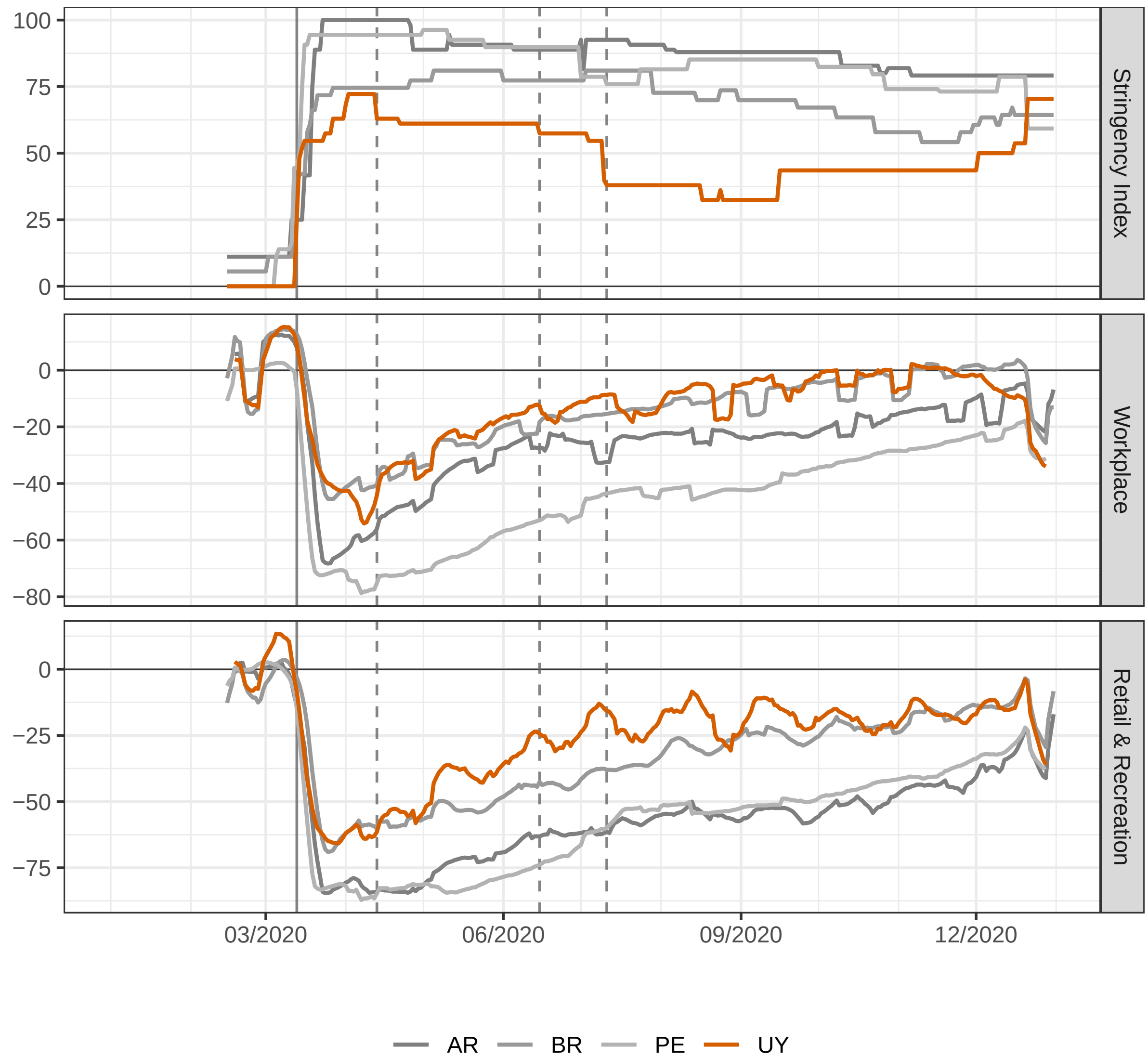

Figure 2: Time series plots of Oxford COVID-19 Government Response Tracker's (OxCGRT) stringency index (Hale et al., 2021) and Google mobility indexes (seven-day averages, Google, 2020a,b) for Argentina (AR), Brazil (BR), Peru (PE), and Uruguay (UY). The solid grey line indicates the date on which stay-at-home guidelines were implemented (March 13th, 2020). The dashed grey lines indicate the dates on which stay-at-home guidelines 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).

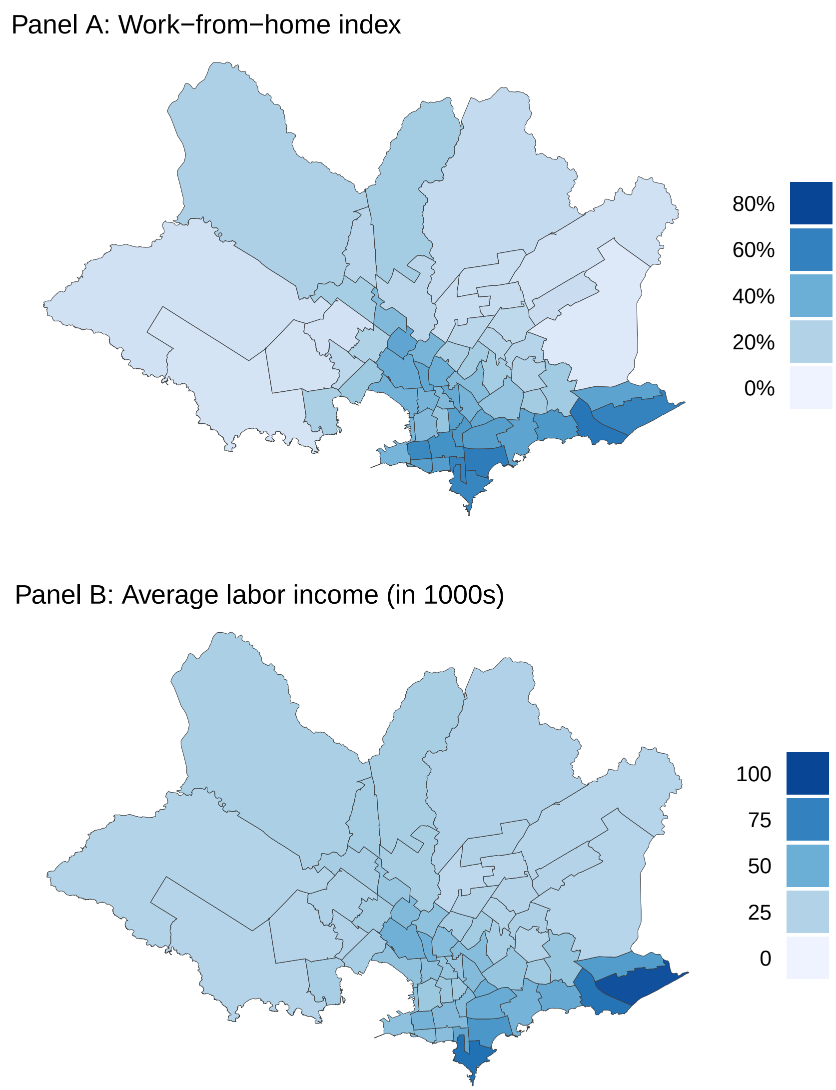

Figure 3: Work-from-home index (Guntin, 2021) and average labor income (in thousands of local currency, monthly, 2019) in Montevideo, by neighborhood.

Figure 4: Time series plots of daily police reports (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).

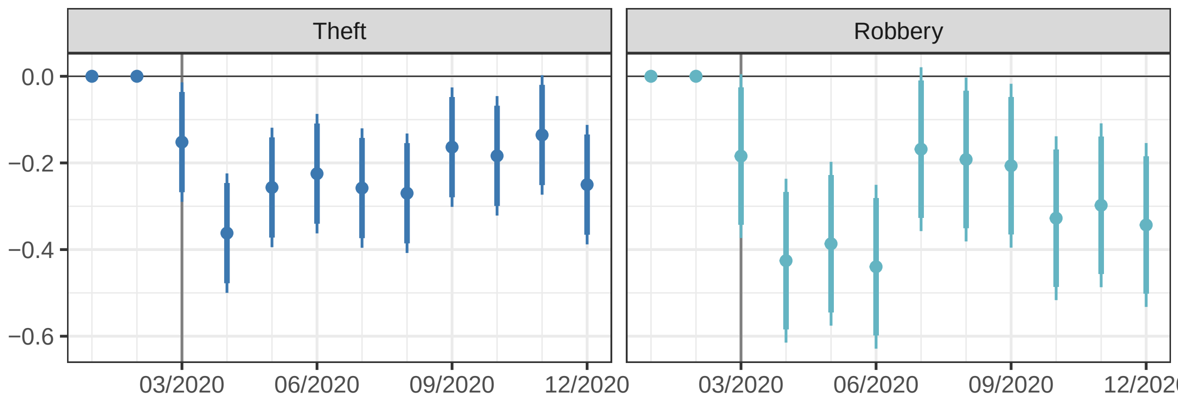

Figure 5: Dynamic event-study designs. Point estimates, $\hat{\delta}_\tau$, are reported with 95% and 90% confidence intervals by month. January and February coefficients are set to zero. The solid grey line indicates the month in which stay-at-home guidelines were issued (March 2020).

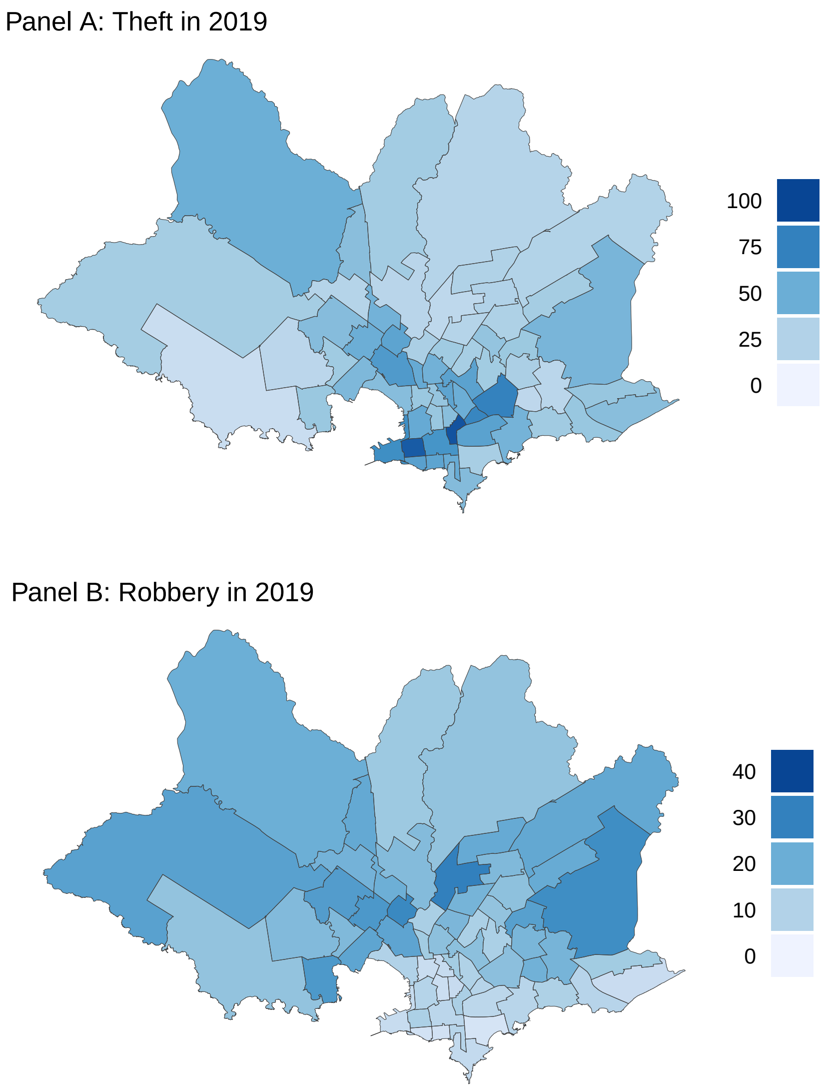

Figure 6: Pre-pandemic distribution of crime reports in Montevideo (police reports per 10,000 habitants, monthly average, 2019).

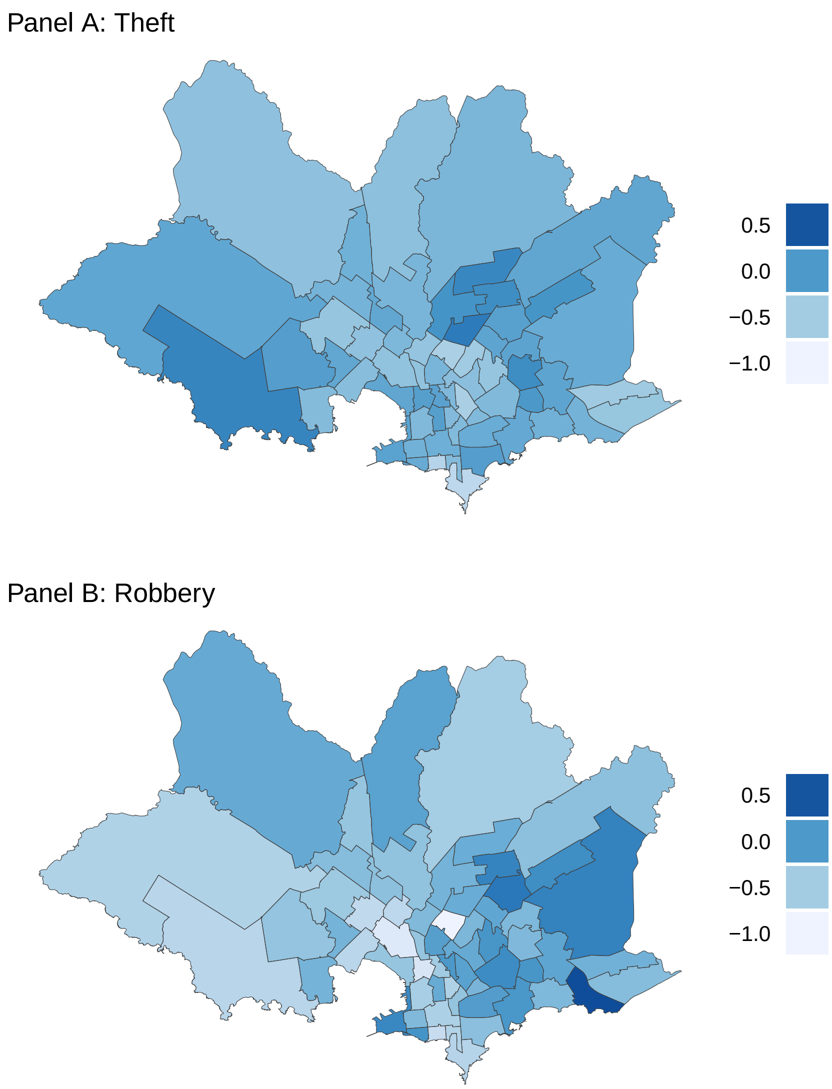

Figure 7: Event-study design results based on Equation 1 (point estimates, $\hat{\delta}$), by neighborhood.

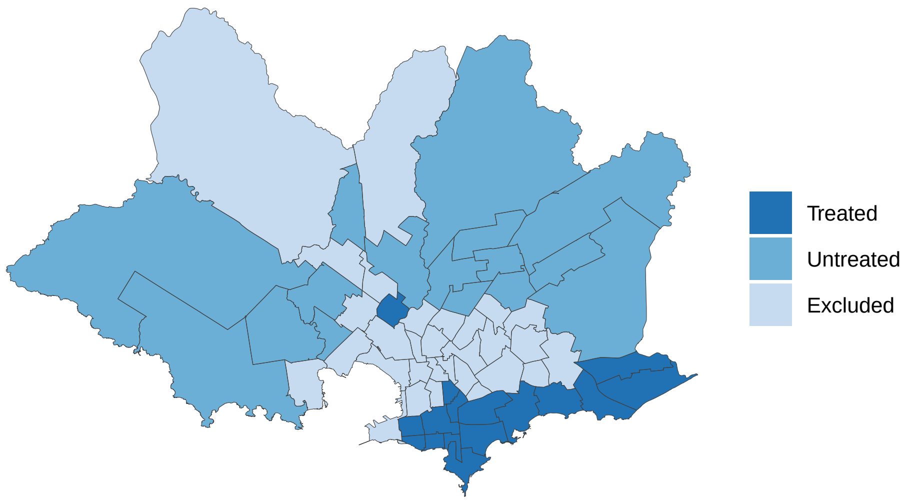

Figure 8: Treated, untreated, and excluded neighborhoods based on the work-from-home (tasks) index of Guntin (2021) and a 25% threshold.

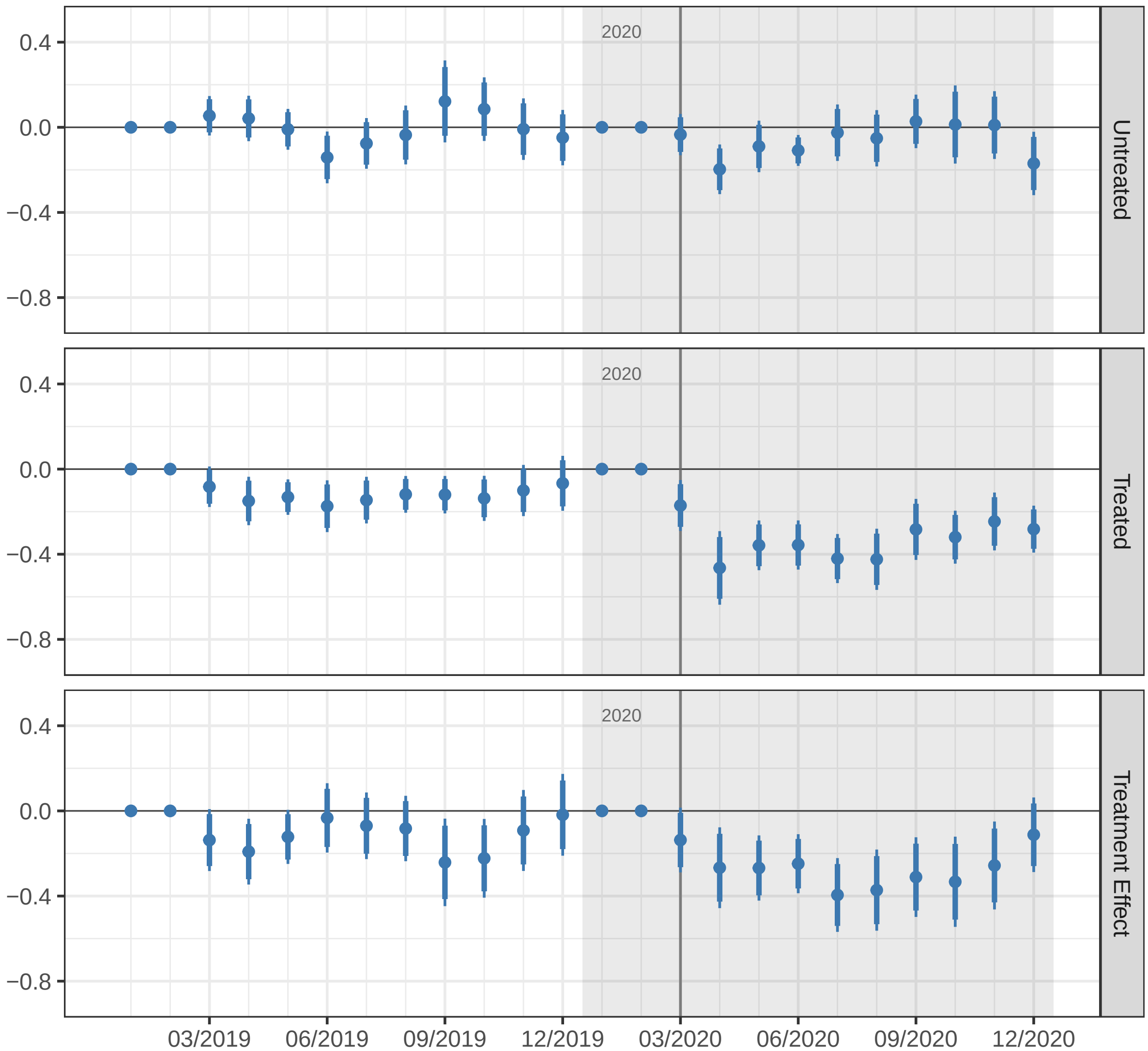

Figure 9: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level.

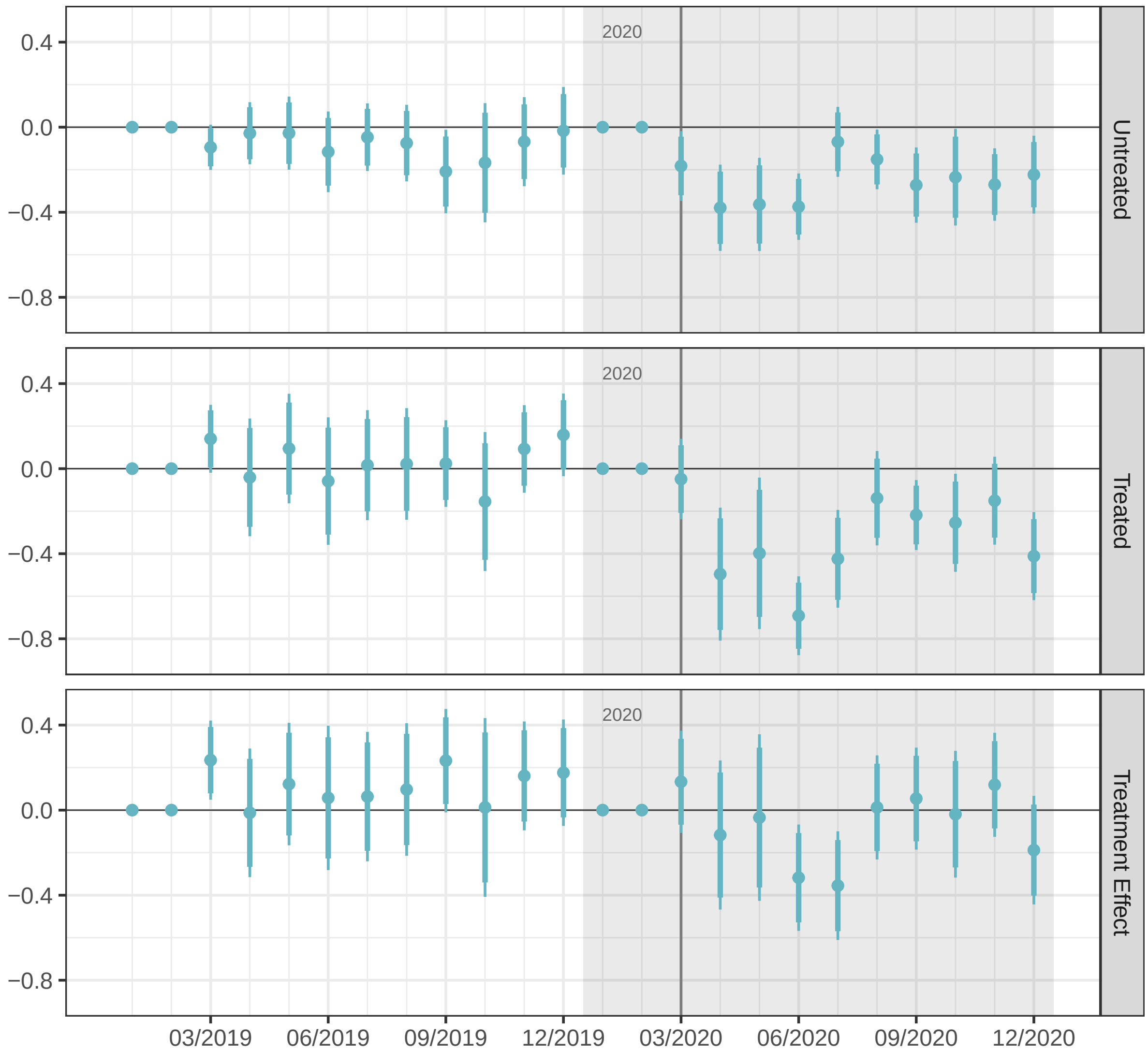

Figure 10: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level.

A Online appendix (not for publication)

A.1 Additional tables

Table A.1: Event-study designs for thefts

	(1)	(2)	(3)	(4)
Post	−0.23***	−0.18***		
	(0.05)	(0.04)		
03/2020			−0.15**	−0.11
			(0.07)	(0.11)
04/2020			−0.36***	−0.32***
			(0.07)	(0.11)
05/2020			−0.26***	−0.21*
			(0.07)	(0.11)
06/2020			−0.22***	−0.18*
			(0.07)	(0.11)
07/2020			−0.26***	−0.21**
			(0.07)	(0.11)
08/2020			−0.27***	−0.23**
			(0.07)	(0.11)
09/2020			−0.16**	−0.12
			(0.07)	(0.11)
10/2020			−0.18**	−0.14
			(0.07)	(0.11)
11/2020			−0.14*	−0.09
			(0.07)	(0.11)
12/2020			−0.25***	−0.21*
			(0.07)	(0.11)
N	84	84	84	84
AIC	−233.3	−148.0	−232.6	−135.2
FE: month	Yes	Yes	Yes	Yes
FE: year	Yes	No	Yes	No
Time Trend	No	Yes	No	Yes
Standard Errors	Standard	Standard	Standard	Standard

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports the coefficients from the event study specification (equations 1 and 2). All models were estimated using the R package *fixest* by Bergé (2018).

Table A.2: Event-study designs for robberies

	(1)	(2)	(3)	(4)
Post	−0.30***	−0.12**		
	(0.06)	(0.06)		
03/2020			−0.18*	−0.01
			(0.10)	(0.16)
04/2020			−0.43***	−0.25
			(0.10)	(0.16)
05/2020			−0.39***	−0.21
			(0.10)	(0.16)
06/2020			−0.44***	−0.27
			(0.10)	(0.16)
07/2020			−0.17*	0.01
			(0.10)	(0.16)
08/2020			−0.19*	−0.02
			(0.10)	(0.16)
09/2020			−0.21**	−0.03
			(0.10)	(0.16)
10/2020			−0.33***	−0.15
			(0.10)	(0.16)
11/2020			−0.30***	−0.12
			(0.10)	(0.16)
12/2020			−0.34***	−0.17
			(0.10)	(0.16)
N	84	84	84	84
AIC	−176.9	−77.3	−179.5	−64.4
FE: month	Yes	Yes	Yes	Yes
FE: year	Yes	No	Yes	No
Time Trend	No	Yes	No	Yes
Standard Errors	Standard	Standard	Standard	Standard

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports the coefficients from the event study specification (equations 1 and 2). All models were estimated using the R package *fixest* by Bergé (2018).

A.2 Robustness: work-from-home (survey)

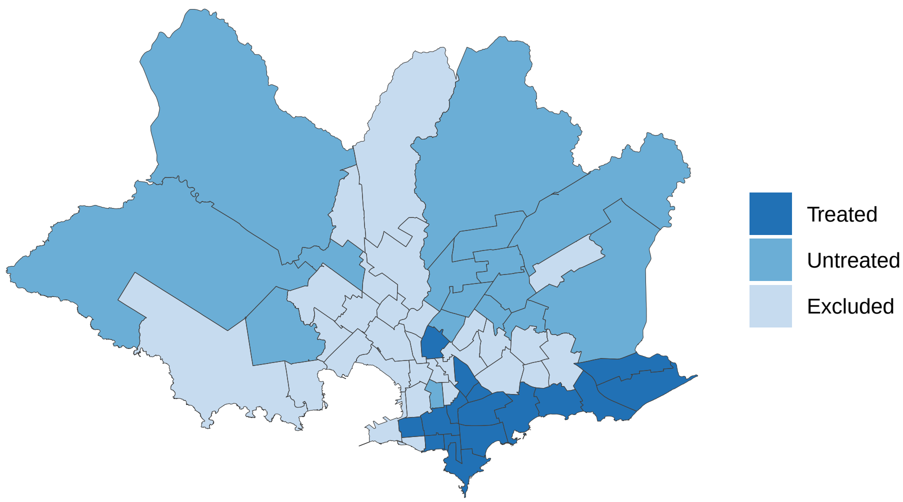

Figure A.1: Treated, untreated, and excluded neighborhoods based on INE's *Encuesta Continua de Hogares* survey questions regarding remote work and a 25% threshold.

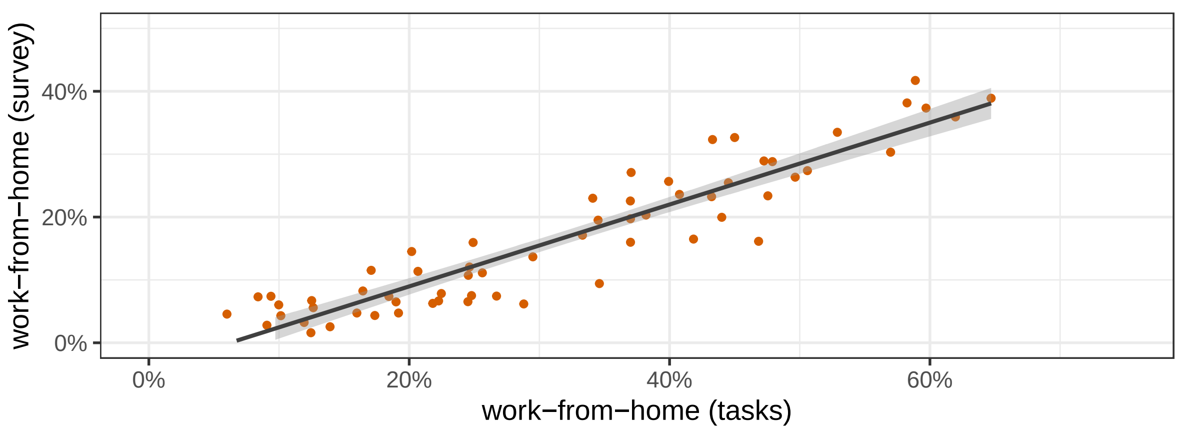

Figure A.2: Scatterplot shows correlation between the work-from-home (tasks) index (Guntin, 2021) and the work-from-home (survey) index. The linear relationship between average decline and work-from-home index (solid dark grey line) and 95% confidence intervals (shaded area).

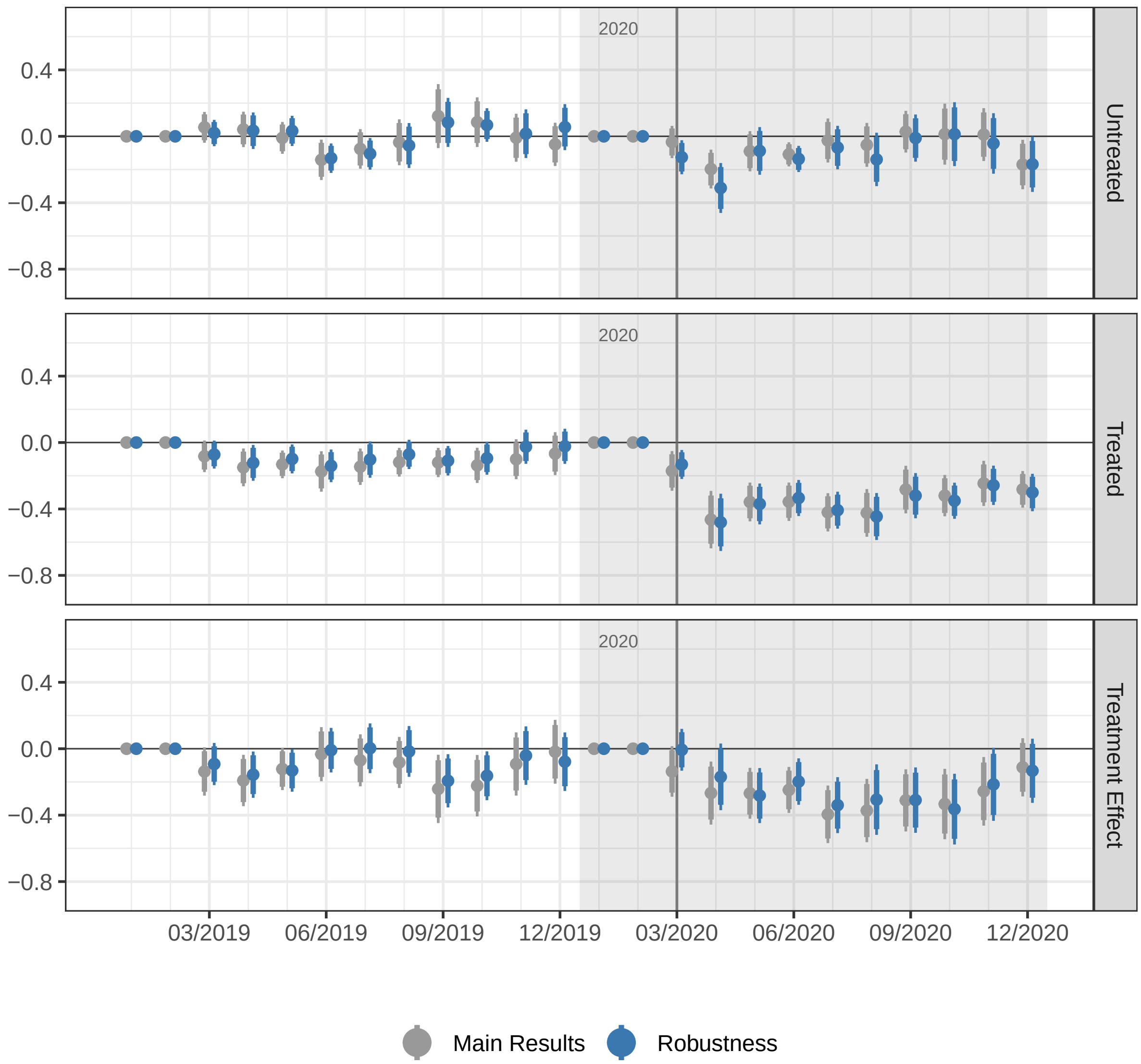

Figure A.3: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using the work-from-home (tasks) index, robustness results obtained using the work-from-home (survey) index.

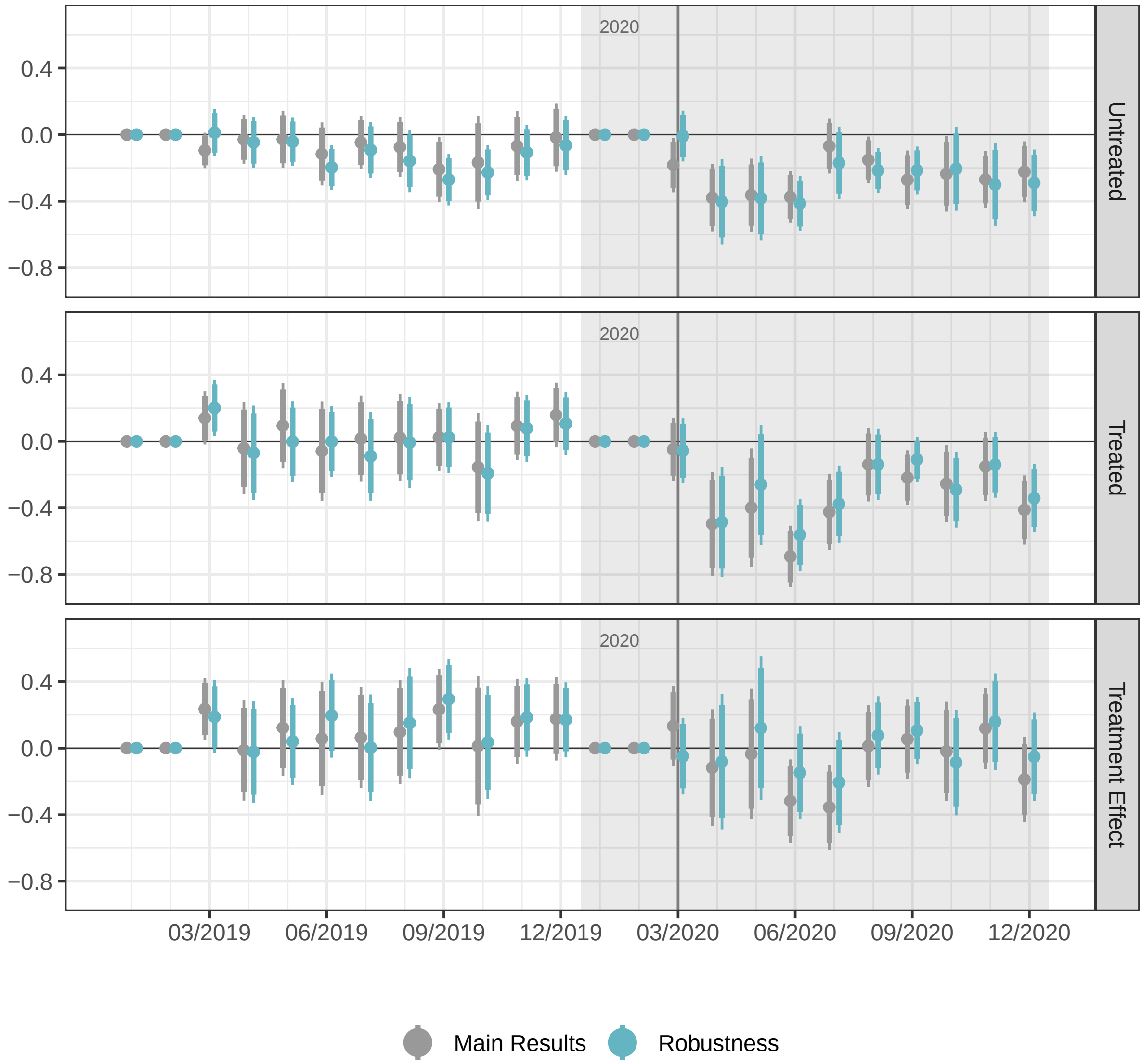

Figure A.4: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using the work-from-home (tasks) index, robustness results obtained using the work-from-home (survey) index.

A.3 Robustness: 33% threshold

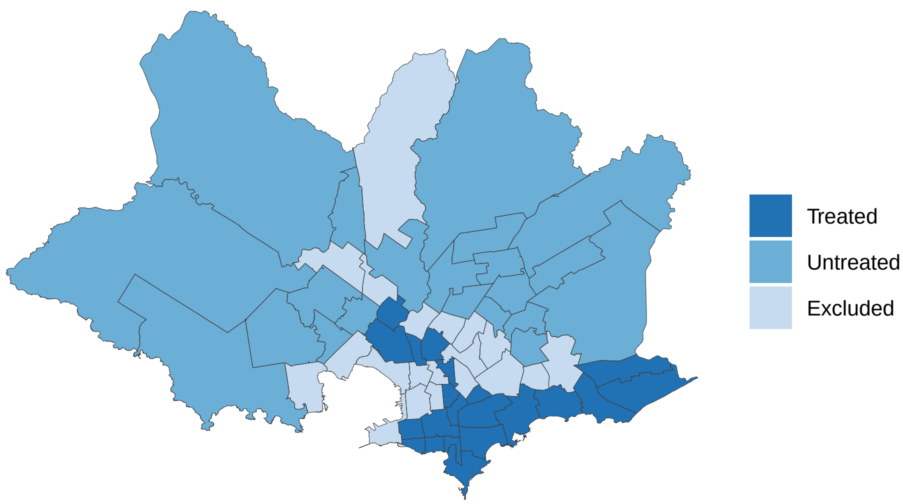

Figure A.5: Treated, untreated, and excluded neighborhoods based on the work-from-home (tasks) index of Guntin (2021) and a 33% threshold.

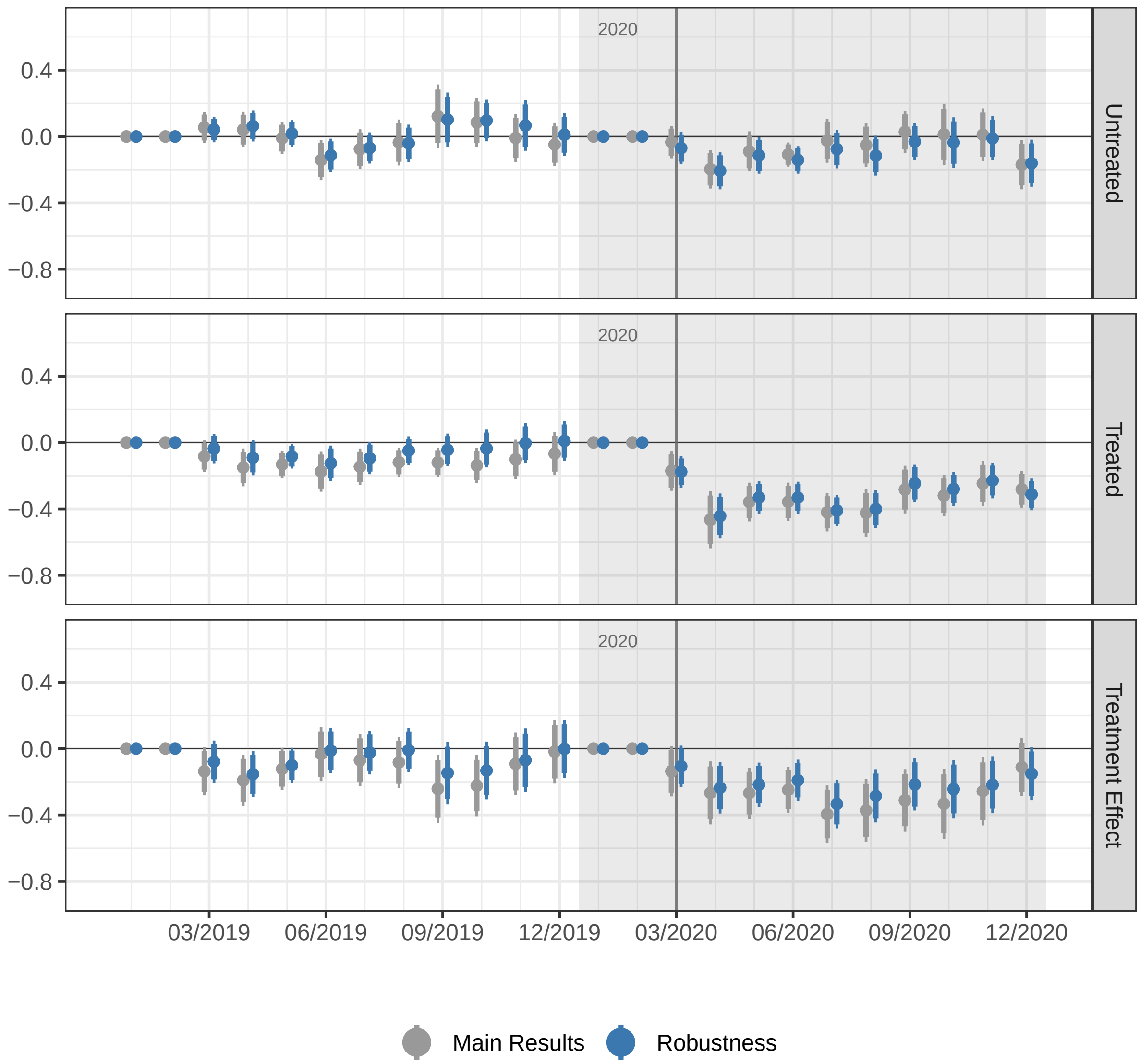

Figure A.6: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using a 25% threshold, robustness results obtained using a 33% threshold.

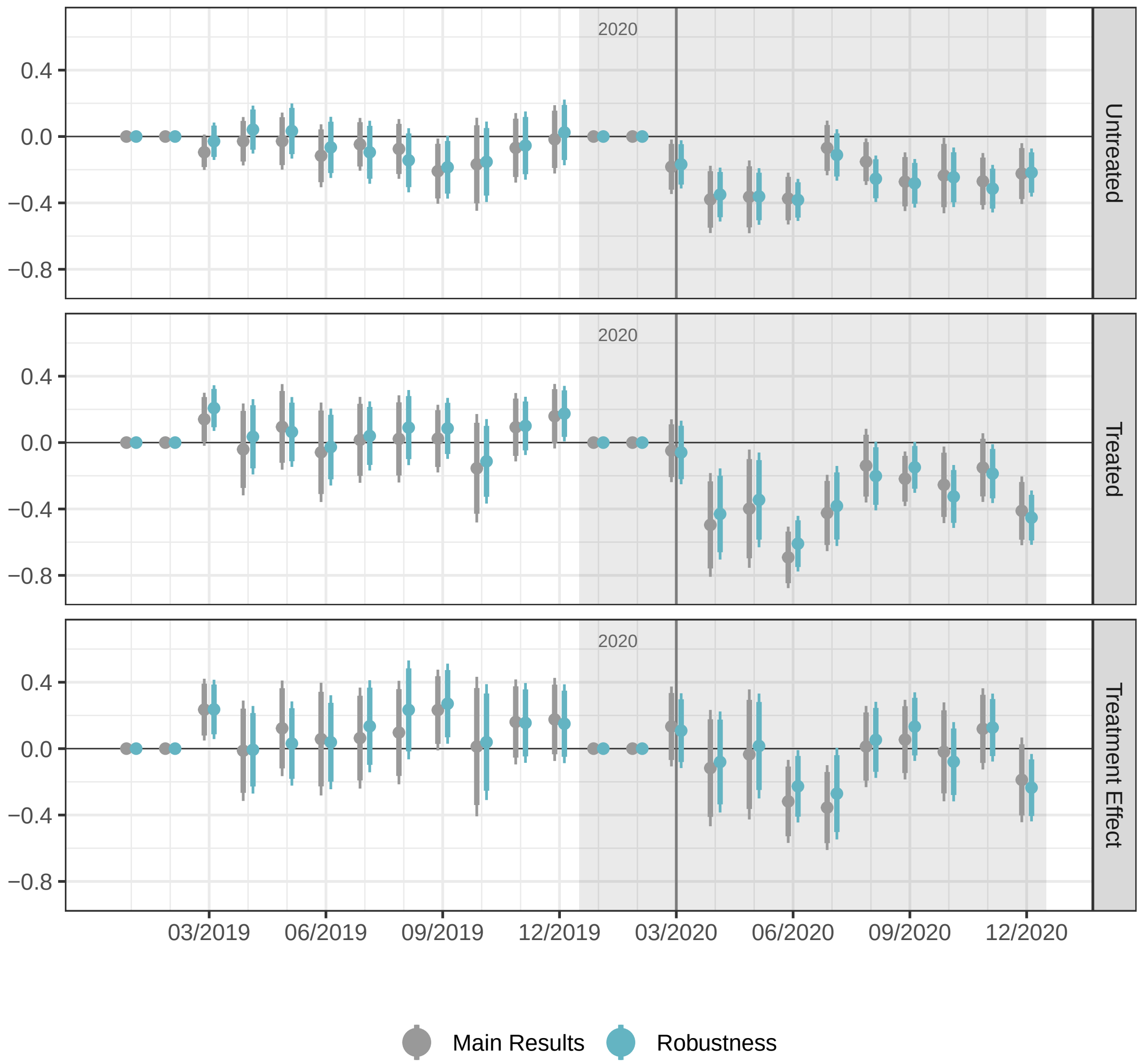

Figure A.7: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using a 25% threshold, robustness results obtained using a 33% threshold.

A.4 Robustness: 50% threshold

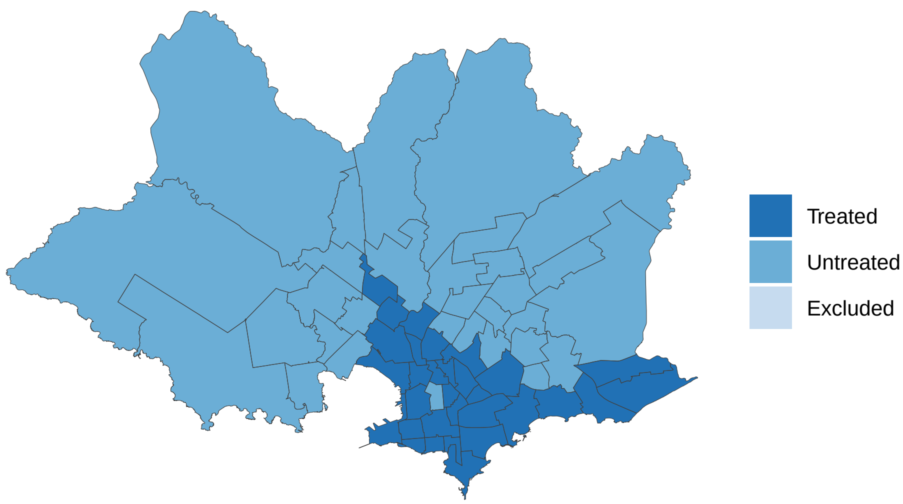

Figure A.8: Treated, untreated, and excluded neighborhoods based on the work-from-home (tasks) index of Guntin (2021) and a 50% threshold.

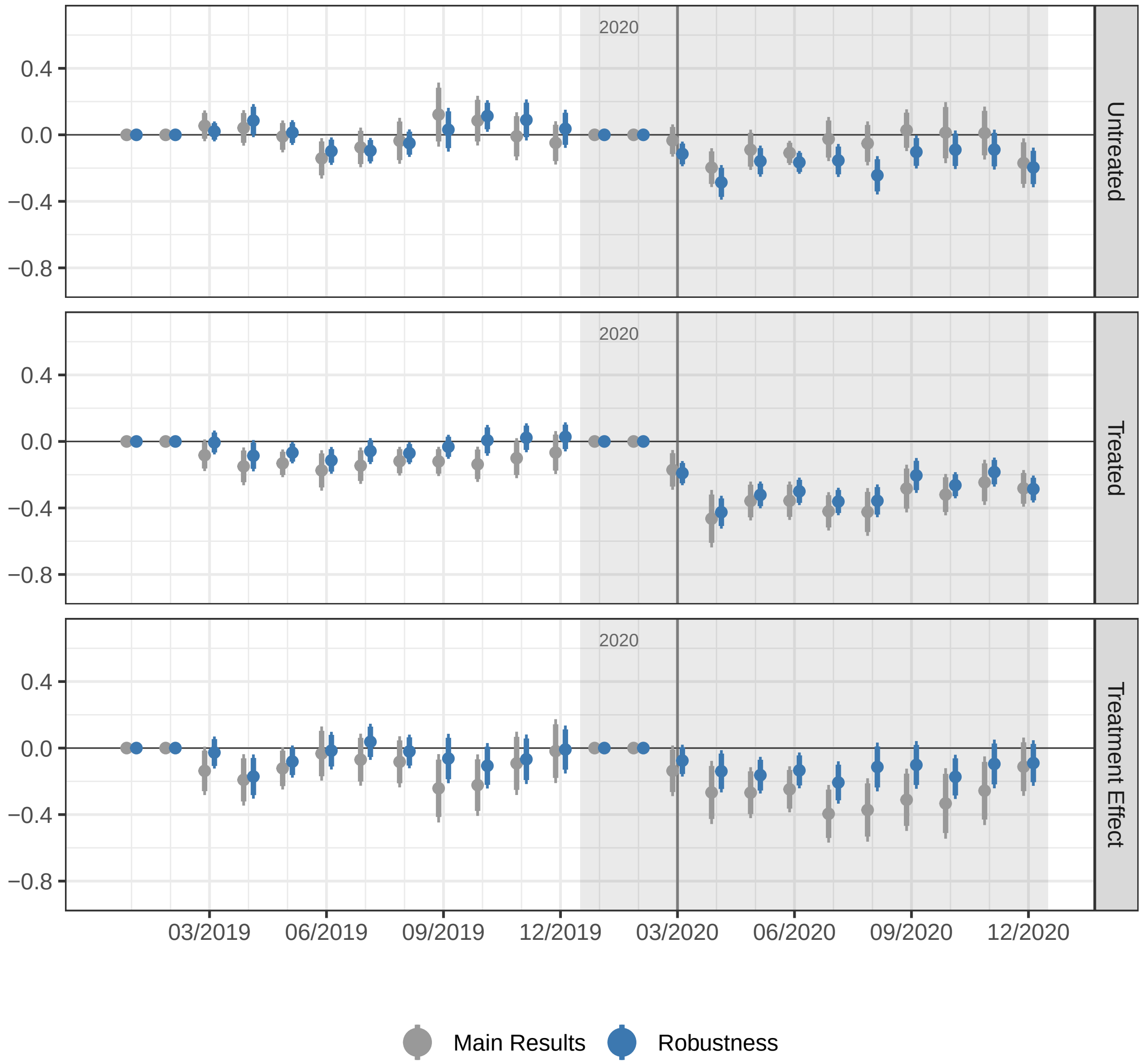

Figure A.9: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}_{\tau}^{1} \ldots \hat{\delta}_{\tau}^{4}$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using a 25% threshold, robustness results obtained using a 50% threshold.

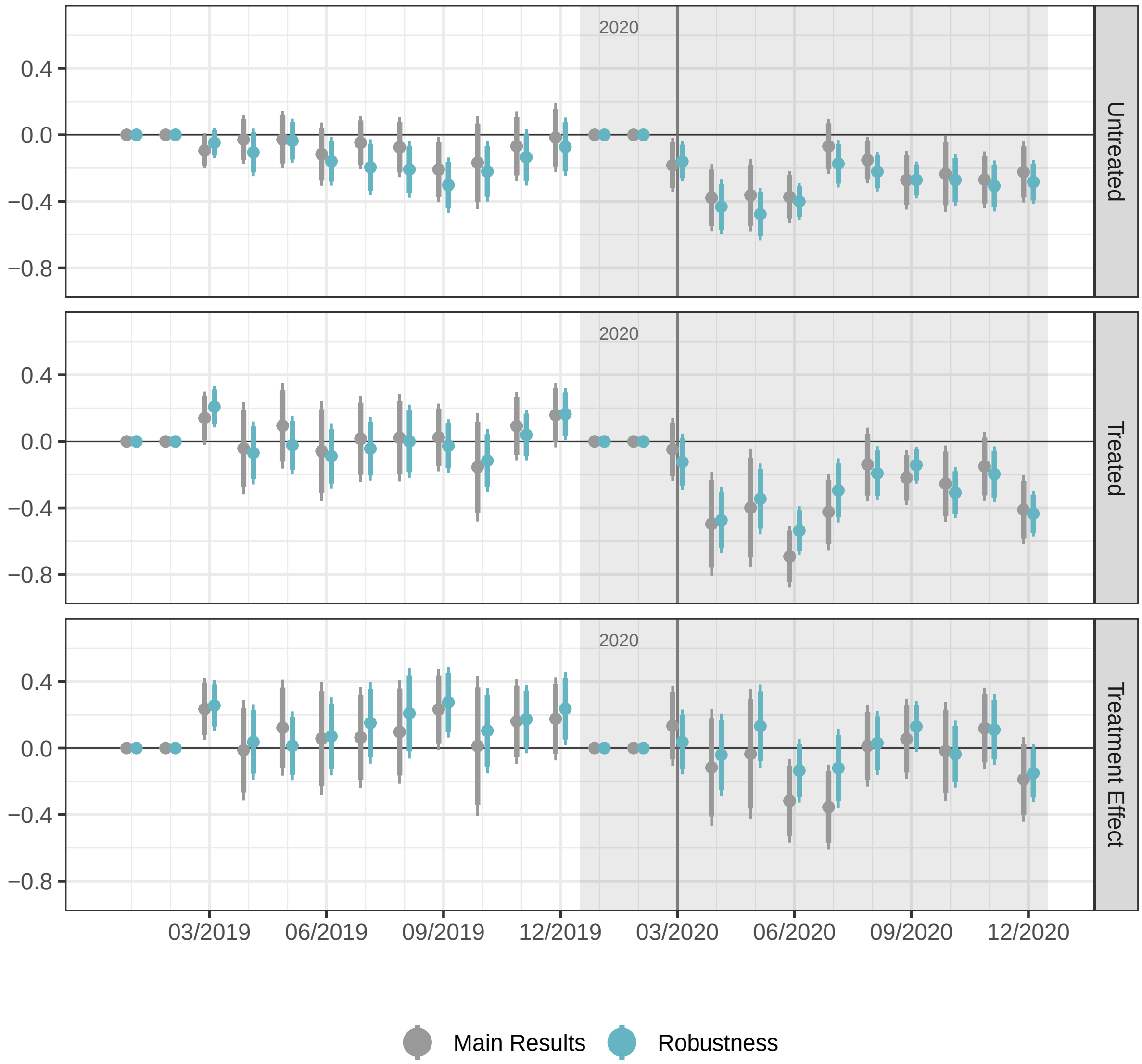

Figure A.10: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \dots \hat{\delta}^4_\tau$ (with $\tau = 3, \dots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using a 25% threshold, robustness results obtained using a 50% threshold.

A.5 Robustness: no 2020 fixed effects

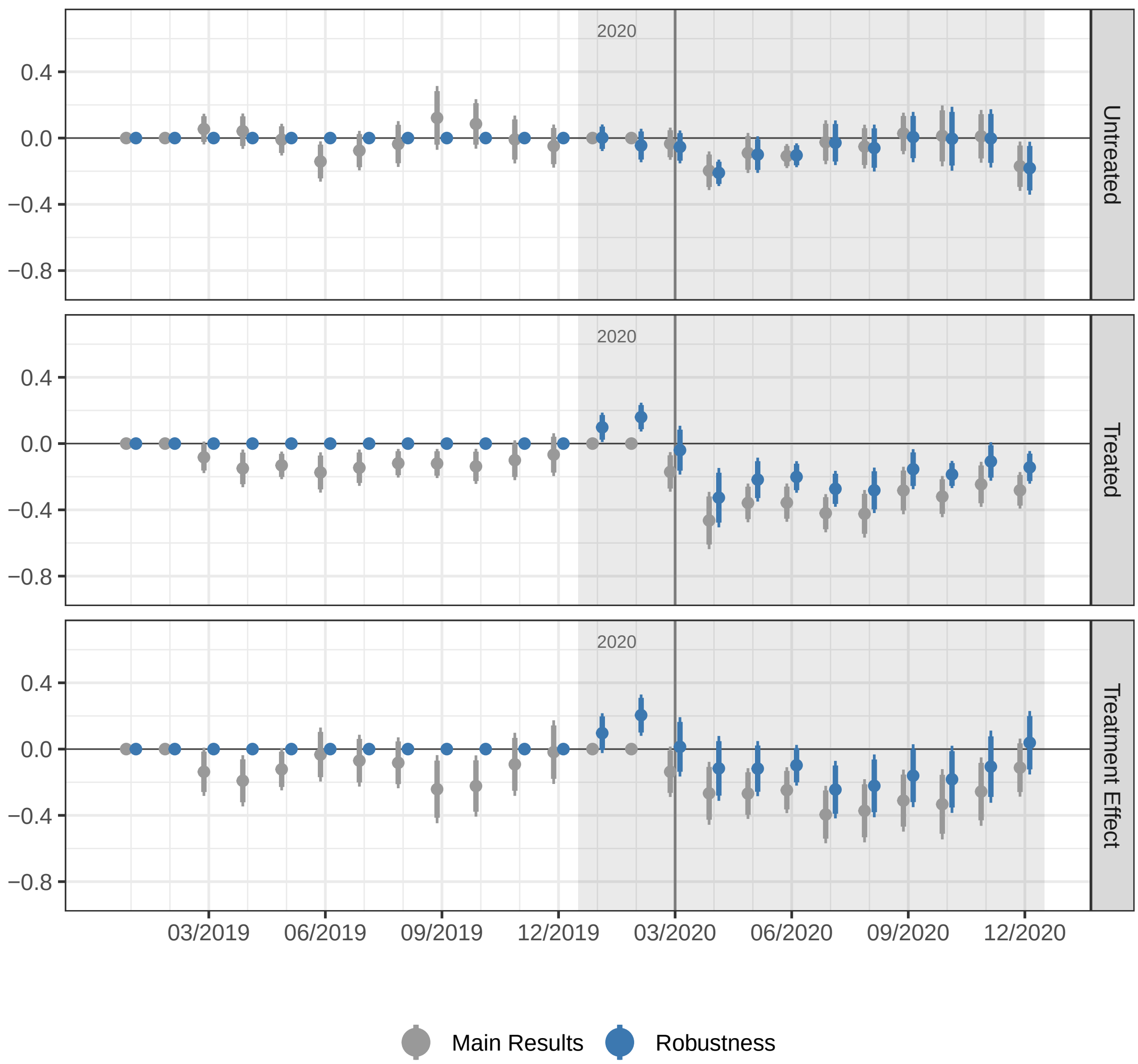

Figure A.11: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \dots \hat{\delta}^4_\tau$ (with $\tau = 3, \dots, 12$), and 95% and 90% confidence intervals by month are reported. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained setting January and February coefficients to zero, robustness results obtained assuming a 2020 year fixed effect equal to the 2019 fixed effect.

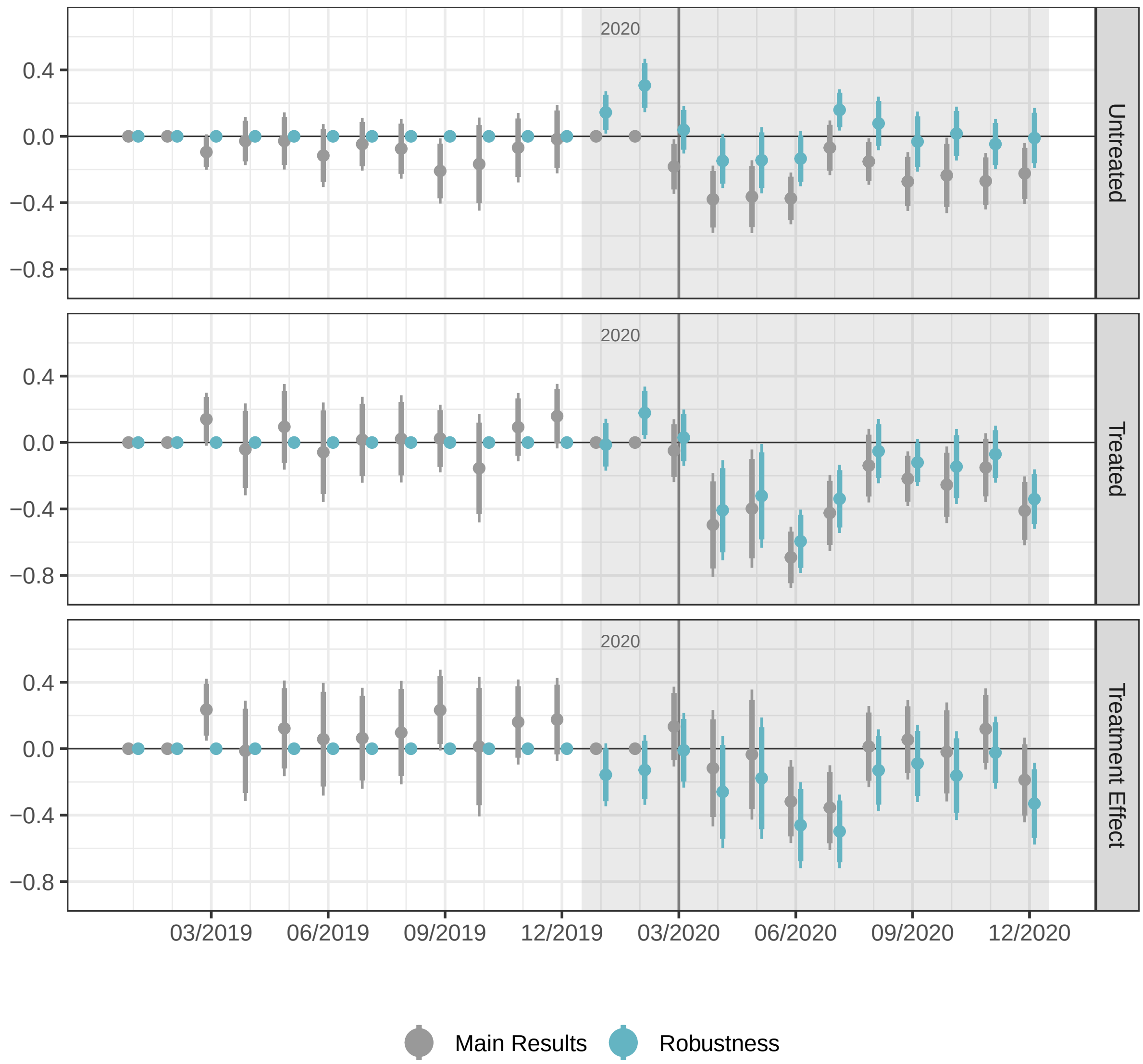

Figure A.12: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained setting January and February coefficients to zero, robustness results obtained assuming a 2020 year fixed effect equal to the 2019 fixed effect.

A.6 Robustness: Central Business District excluded

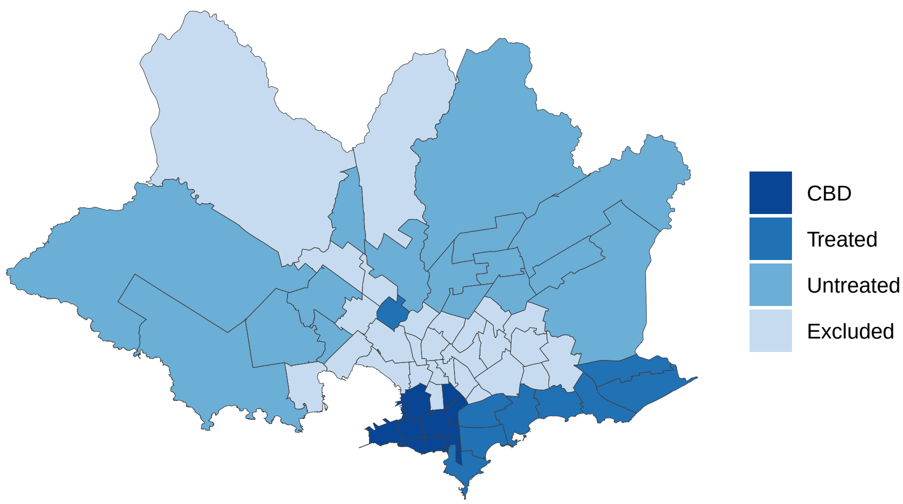

Figure A.13: Treated, untreated, and excluded neighborhoods based on the work-from-home (tasks) index of Guntin (2021) and a 25% threshold. Neighborhoods in Montevideo's central business district (CBD) also excluded.

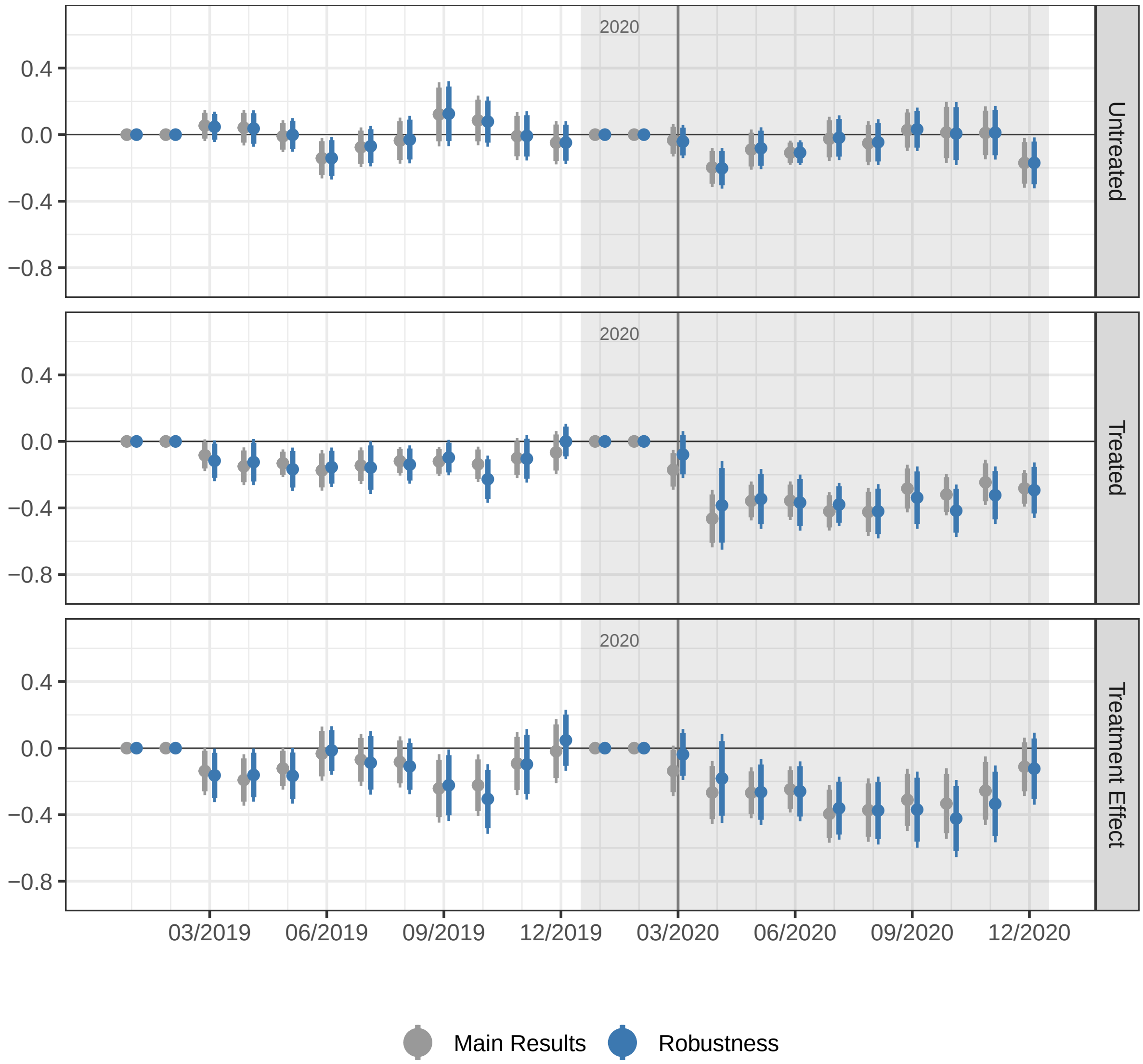

Figure A.14: Dynamic difference-in-differences design for theft (Equation 4). OLS point estimates, $\hat{\delta}_\tau^1 \ldots \hat{\delta}_\tau^4$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using all 62 neighborhoods, robustness results obtained after removing neighborhoods in Montevideo's central business district.

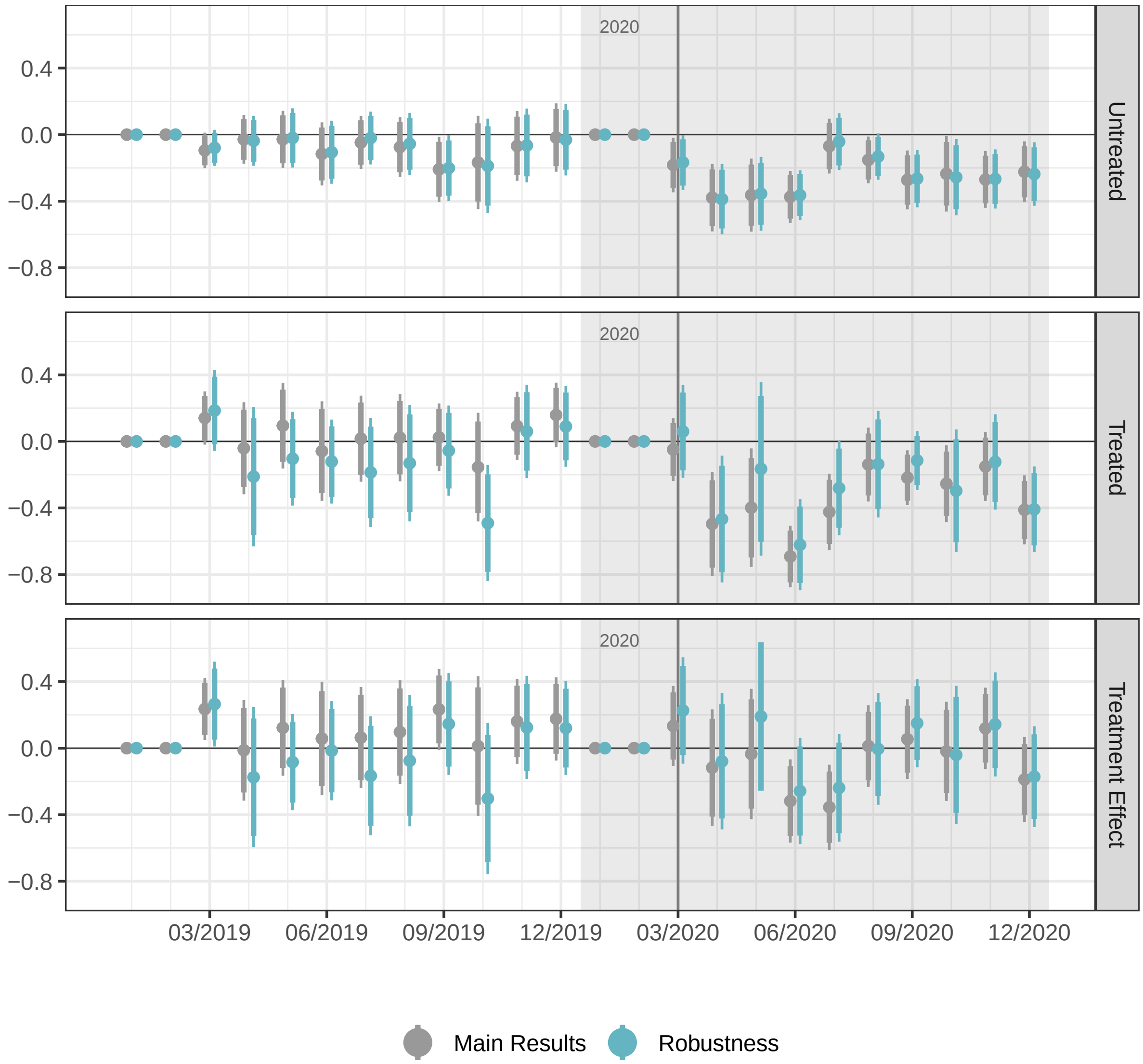

Figure A.15: Dynamic difference-in-differences design for robbery (Equation 4). OLS point estimates, $\hat{\delta}^1_\tau \ldots \hat{\delta}^4_\tau$ (with $\tau = 3, \ldots, 12$), and 95% and 90% confidence intervals by month are reported. January and February coefficients are set to zero. The solid grey line indicates the period on which stay-at-home restrictions were implemented (March 2020). Standard errors are clustered at the neighborhood level. Main results obtained using all 62 neighborhoods, robustness results obtained after removing neighborhoods in Montevideo's central business district.

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