Crime Dynamics and Spatial Spillovers after a place-based policy: Evidence from Rio de Janeiro

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Abstract

Urban violence inflicts significant welfare losses in several countries worldwide. Urbanization and other characteristics of cities create incentives for the formation of gangs that control parts of the territory, increasing their resilience to State interventions. Reclaiming these areas is a fundamental step to disrupting these gangs. This paper studies the spatial spillovers effects of a place-based policy designed to regain territorial control in some areas controlled by drug traffickers in the city of Rio de Janeiro. I develop a simple reduced-form empirical framework that uses the stability of difference-in-differences estimations when using alternative control units to shed light on potential SUTVA violations and spatial spillovers and to create bounds for the treatment effects. I find evidence that the program decreased homicides and police killings in treated areas and that it did not induce crime displacement to other places in the city of Rio de Janeiro. There is suggestive evidence of crime migration to areas in Rio's metropolitan region and in the countryside of the state.

Keywords: spatial spillovers, SUTVA, crime displacement, Pacification Police Unit, urban violence

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1 Introduction

Urban violence is a primary concern for several countries in Latin America (Jaitman et al., 2017), imposing substantial welfare costs to its citizens (Cerqueira and Soares, 2016). Moreover, with the rapid growth of cities, most citizens will live in a city by 2030 (UN, 2018). The urbanization process and specific urban characteristics can potentially aggravate the urban violence scenario in the following decades (Glaeser and Sacerdote, 1999; UN Habitat, 2016). This violent urban scenario creates incentives for the formation of youth and street gangs (Venkatesh et al., 2008). These gangs evolve with the lack of public goods provided by the State and become 'political actors' controlling parts of the territory in a city (Blattman et al., 2022). When the drug business became these groups' primary revenue source, maintaining the territorial domain was even more critical to protect drug profits. The domination of territories by armed groups increases the cost of public goods provision and law enforcement in these areas by the State, and it makes these gangs more resilient (Lessing, 2017)[1].

In many urban areas, criminal groups exercise territorial control over non-contiguous areas within or across multiple cities, forming a spatial criminal structure characterized by a complex network of allies and enemies (Bruhn et al., 2021; Owens et al., 2020). This spatial characteristic facilitates the mobility of criminals across non-adjacent regions, reducing the cost of coordinated actions and presenting challenges for the government in designing effective policies to curtail the territorial control of these criminal groups. The lack of comprehensive data on this network poses challenges for researchers, limiting our understanding of the dynamics at play.

This paper analyses the spatial consequences of a large place-based policy that focused on reclaiming territorial control back to the State of areas under the previous territorial domination of criminal groups in an urban context. Implementing place-based policies targeting specific groups or areas can induce mobility among criminals, disrupting the pre-existing equilibrium and creating spatial externalities that may manifest as positive or negative influences. This intricate relationship raises concerns about violating the Stable Unit Treatment Value Assumption (SUTVA) and defining a suitable ex-ante control group plausibly immune to potential spillovers. For example, after this policy, the treated criminal actors may face choices that include relocating to allied favelas, engaging in territorial conflicts with rival factions, or exiting crime. Simultaneously, non-treated criminal actors may adapt their behavior in response to the policy. They might strategically reduce criminal activities to mitigate the risk of being targeted by similar policies or exploit new

[1]In the case of Rio de Janeiro, these groups display criminal governance characteristics in more than 16% of the territory, with almost 2 million citizens living under criminal rule (Couto and Hirata, 2022).

criminal opportunities due to the reshaping of territorial control.

In this context, the intervention's impact extends beyond the treated areas, prompting general equilibrium responses that can impact the criminal market. Ideally, this type of problem requires a structural approach, which is not feasible due to data restrictions on criminals' choices and drug gangs' territorial control. However, can we generate meaningful insights about spatial spillovers from reduced form estimation for the effects of a place-based policy in a context where criminal groups control non-contiguous areas? Failure to consider these spillovers may lead to a skewed assessment of policy effectiveness, either by overestimating the impact of policies or underestimating the total effects when not accounting for positive externalities.

I propose a simple method that can shed light on this question. I employ contextual knowledge about criminal markets to define relevant sub-markets, and then, I analyze the stability of the coefficients when these different sub-markets act as control groups. The central assumption is that all these sub-markets satisfy the parallel trends assumption. Consequently, without spatial spillovers, the treatment effect will exhibit relative stability across different specifications when utilizing these subgroups as control units. If estimates vary across specifications, two potential scenarios are considered: i) local shocks hit the sub-market after the policy, or ii) the policy may contaminate the sub-market. Notably, I argue that, given the integrated nature of this criminal market, local shocks should not be the primary driver behind differing estimates. I can use this intuition to discuss spatial spillovers in a difference-in-differences framework and create bounds for the treatment effects.

The Pacification Police Units program (UPP) was launched in 2008 and focused on reducing the territorial power of criminal organizations and increasing the presence of State in selected *favelas*. The strategy of this policy is to regain the territory and then establish permanent community-oriented policing in the favela. The program was gradually expanded to 38 police stations installed in large favelas – covering 28 out of 53 of the large favelas in the city of Rio and reaching almost a fifth of the city of Rio's population – with the human force of 9,000 newly trained and hired police agents. The rise in police officers permanently allocated in these localities implies an increase in law enforcement and the cost of doing drug business locally, which can induce responses by the treated criminal groups: they could adapt to the new equilibrium and reduce the display of violence; they could move to other areas that belong to the same criminal group; or, they could leave the criminal career path.

I used monthly official criminal records data at the police station level from the Public Security Institute from 2004 to 2016. I consider the state of Rio de Janeiro to be an integrated criminal market, and I define several sub-markets based on socioeconomic and criminal characteristics. First, I construct the treatment group by considering a police station as treated by the UPP policy if at least one large favela is treated within its boundaries. Analogously, I assign a police station within the city of Rio as the *untreated police stations* if there is a large untreated favela in its domain[2]. The other sub-markets are in the metropolitan area or the state's countryside. The region of *Baixada* is composed of poor municipalities with the historical presence of death squads and paramilitary groups, known as militias (Alves, 2003; Cano and Duarte, 2012); *Niteroi* is an affluent neighborhood in the metropolitan region with the presence of the drug gangs in some parts of its territory; *Sao Goncalo* a poor municipality in which drug factions also operate in some areas, and the *Countryside* defined by the rest of police stations in Rio de Janeiro State – these areas are more heterogeneous in terms of socioeconomic characteristics. There is anecdotal evidence that criminal groups migrated to these cities after the UPP policy.

I perform some empirical exercises to shed light on crime displacement in the city, metropolitan area, and countryside of Rio de Janeiro State. The idea is to estimate the empirical model by changing the sub-market used as the control units and use this information to bound the treatment effects of the UPP policy. The policy reduced violence in treated places and induced a response in untreated police stations with large favelas in its boundaries in the city of Rio that *lowered* crime in these areas. However, suggestive evidence indicates that the UPP partially displaced crime to other regions outside the city (metropolitan area and countryside).

Few papers examine public policies intended to reduce the control of parts of the urban territory by criminal gangs that could move to other non-contiguous places in the city or metropolitan area. Close to this paper, Bellégo and Drouard (2024), Magaloni et al. (2020) and Ferraz et al. (2015) evaluate the Pacifying Police Unit Program in Rio, but without explicitly addressing the migration problem in a unified econometric framework and considering the general equilibrium effects of the policy in the whole criminal market. Other papers provide evidence that spatial spillover may be either positive or negative. Blattman et al. (2021) find small negative spatial spillovers to nearby areas caused by a place-based intervention targeted in hot spots in Bogota and Dell (2015) evaluates the causal impacts of large-scale interventions to combat trafficking sponsored by Mexican conservative party PAN on drug-related violence. Her results suggest adverse

[2] Since the policy targeted only large favelas in Rio, this control group is arguably a good starting point in an empirical exercise to estimate treatment effects. Moreover, I can show that this group is similar to the treated group in socioeconomic and criminal characteristics before the policy. The only significant difference is the distance to Olympic venues.

spillover effects to places where multiple drug routes coincide. Draca et al. (2011) and Di Tella and Schargrodsky (2004) discover no evidence of spatial spillover due to spatially targeted interventions. Verbitsky-Savitz and Raudenbush (2012) estimate the impact of a community-policing program established in Chicago using a 'buffer' treatment group given by neighboring units to treatment localities.

There is a growing literature that addresses this type of estimation more formally. Clarke (2017) and Butts (2021) propose frameworks to estimate difference-in-differences in the presence of spatial spillovers. Clarke (2017) generalizes the concept of a neighborhood by defining a flexible distance metric and assuming that the treatment effect decays as distance to treatment increases, and Butts (2021) shows that defining a flexible weighting matrix removes biases and allows to estimate the total effects of the treatment. Importantly, though, this matrix has to be known by the econometrician, and some of the weights have to be zero. Given Rio's criminal context, in which the network of alliances and enemies is unknown to the researcher, I leave the implementation of these new difference-in-differences estimators with spillovers for future research.

A recent paper from Alves et al. (2024) follows an interesting approach to dealing with SUTVA violations in a difference-in-differences framework by imposing minimal structural restrictions on the observed data. The authors use economic reasoning related to elasticities of housing supply and demand to identify plausibly contaminated groups defined by inflows and outflows of residents after a housing tax break policy. Then, they can back out the treatment effect, controlling for potential spillovers. The central intuition for this paper is similar to theirs. Given the difficulty in predicting inflows and outflows of criminals after a place-based policy, I opt for using contextual and economic reasoning in a less formal way than their paper. However, the goal is the same: identify sub-markets that are differently exposed by the policy and use this information to shed light on spatial spillovers.

In the next section, I discuss the institutional context of non-state armed groups in Rio de Janeiro. Section 3 describes the data. In section 4, I examine the empirical strategy. Section 5 investigates the results and robustness exercises, and Section 6 considers further steps and concludes the paper.

2 Context

2.1 Spatial Distribution of Drug Factions

Initially, I discuss the geographical boundaries of Rio's criminal market. Historically, the decision area for criminal actors was Rio's metropolitan area (Misse, 2007). The rise of drug gangs in the 1980s based on local criminal governance at the favela level and on the projection of power within the prison system strengthened the metropolitan region as the primary criminal market in Rio. Recently, there is evidence[3] that the criminal's organizations integrated this market with the state's countryside (Monteiro, 2018; Misse, 2014).

There are two main reasons to consider the state of Rio as a market: first, criminal actors need to maintain their criminal governance in the favelas they rule. Then, if other drug gangs threaten their positions, they can rely on criminal support from other favelas under the same drug faction rule. Since the drug faction is present in different areas of the state, they could ask for help from any other favela in this area, potentially reinforcing the non-contiguous network of alliances. The second reason is that given that these factions act within the prison system and that there is a high probability that drug traffickers go to jail at some point in their lives (Lessing, 2017), criminal actors can create and fortify ties with criminal actors from other favelas while in prison. Besides, drug traffickers, if caught, go to the same set of prisons, which facilitates the exchange of information among criminal actors in this geographical boundary. Thus, the prison system also creates incentives for the presence of a non-contiguous criminal network and reinforces the metropolitan area as the locus of this illicit market.

Furthermore, drug factions do not display a strictly vertical structure at the leadership level (Hirata and Grillo, 2017). Drug traffickers from a favela have the agency to ally with other favelas ruled by the same drug faction. The factions exhibit a *rhizomatic* network structure, i.e., a horizontal network of mutual protection set by sufficiently independent players in the top distribution of power where alliances can be made in any direction (Duarte, 2019). The leaders display vast degrees of autonomy and might form coalitions to protect the territories or invade other places with other leaders. These alliances help criminal actors protect their environment from invasions and plan criminal operations together. While I can assume that the network is common knowledge to criminal actors, I do not have a good proxy for the set of alliances and enemies.

[3] For suggestive evidence, access https://www1.folha.uol.com.br/cotidiano/2018/01/1950465-desemprego-faccoes-e-poder-do-trafico-impulsionam-mortes-no-litoral-do-rj.shtml. Accessed in July 2022.

Figures 4 and 5 display how the criminal groups are spread over the city and the state of Rio and also how the spatial distribution of drug factions changes over time. I rely on different sources to create the evolution of the spatial distribution of criminal groups over time. I geocoded maps from other researchers' sources only for the city of Rio de Janeiro. I use more recent maps created by a group of journalists and criminologists that shed light on the spatial distribution in Rio's metropolitan region and the countryside of the State. Note that the criminal groups are spread over the metro region and even in areas in the countryside of the state of Rio de Janeiro. Since 2006, militias have grown in Rio's West Zone and part of the metropolitan region known as Baixada.

The spatial distribution of drug factions and criminals' networks can create correlated movements in the criminal market in non-contiguous areas. For example, suppose that drug traffickers from Red Command who rule a community in the West Zone of the city decide to invade a place ruled by another drug faction, say a militia group. In the invasion, they ask for soldiers from an allied favela in the North Zone of the city. To weaken the invasion, the militia group could counterattack the alliance by striking the favela in the North Zone. In this case, violent outcomes from these two areas would be positively correlated through the network of alliances and enemies. That is, any shock in a favela in Rio can potentially reverberate to other areas and create a correlation in criminal actors' strategies in this criminal market. Since the UPP was an ambitious public policy that targeted several favelas in Rio de Janeiro and increased the cost of doing criminal business in these areas, the program can be seen as an aggregate shock to this criminal market.

Therefore, the network structure among criminal actors may raise concerns about crime displacement to untreated favelas in Rio de Janeiro. Criminals from a treated favela could move to an untreated favela in response to the UPP program, which could increase crime in the non-treated area. This process could lead to negative spatial spillovers (crime displacement), which bias the results and overestimate the impacts of the police on violence reduction.

In terms of drug factions exposed to the policy, most of the Pacification Police Units were installed in areas from the Red Command, which caused an unanticipated economic shock to this faction (Misse, 2011; Zaluar, 2012; Muggah, 2017). Until 2009, Red Command was strongest in the South Zone and the North Zone of Rio, and most of the Pacification Police Units located in these areas (Rodrigues, 2013)[4]. The militias received only one UPP in their territory.

Using ethnographic and survey data, Zaluar (2012) estimated that in 2005, Red Command

[4]Figure 4 (a) shows the spatial distribution of the drug factions in 2006, before the beginning of the program.

(CV) was the most potent drug faction, controlling half of the slums while the other gangs controlled 20 percent. Militias were present in around 10 percent of the favelas. After the beginning of the UPP policy at the end of 2008, the spatial distribution of these factions changed. In the same study, (Zaluar, 2012) shows that militia groups increased and became the strongest territorial gangs, controlling 45% of the slums. CV lost territorial power and had 30% of the shantytowns. The other factions kept roughly the same percentage of favelas.

2.2 Police structure in Rio

The Police system in Brazil is composed of two Police institutions controlled by the states. The Civil Police is responsible for investigative duties, and the Military Police for patrolling and favela incursions when necessary. In an attempt to rationalize and coordinate these two Police forces, the Secretary of Public Security (SSP-RJ) created the Integrated Areas of Public Security (AISP) that define the area of operation of a military police battalion and the districts of civil police stations contained in the site of each battalion. Then, the police battalions are coarser than police stations, i.e., a police battalion may have several police stations within its boundaries.

There are 40 police battalions in the State of Rio de Janeiro, of which 18 are only in the city of Rio. Meanwhile, there are 130 police stations in the State and 39 in the city of Rio de Janeiro. In terms of workforce, there are around 45,000 Military Police officers[5] and 9,000 thousand Civil agents[6].

The commander of each police battalion has some leverage to allocate military police agents in the territory, which can create spatial correlations within the boundaries of a division. Therefore, I opt to cluster the standard errors in the empirical specifications below at the police battalion level.

3 Data

To overcome the lack of geocoded episodes of violence for the state of Rio (or, at least, aggregated at the favela level), I use police station data, a coarser geographic unit than large favelas, to address spatial spillover concerns to large untreated favelas and other sub-markets. I rely on official crime data from the Institute of Public Security (ISP-RJ), the statistical and data intelligence office of the Secretary of Public Security (SSP-RJ). ISP-RJ

[5] http://olerj.camara.leg.br/retratos-da-intervencao/a-policia-militar-no-rio-de-janeiro. Accessed in August 2022.

[6] http://www.policiacivilrj.net.br/policia_civil_em_numeros.php. Accessed in August 2022.

consolidates crime incident information and makes the data monthly available. In this chapter, I utilize monthly data from 2004 to 2016 at the police station level. I focus on homicides for two reasons: i) reduction of homicides was an explicit goal of the policy (Cano et al., 2012) and ii) there is a lower probability of behavioral changes in reporting of this type of crime after the policy (Bellégo and Drouard, 2024).

Police station boundaries may change over time due to the re-optimization of policing strategy. Some police stations are created after the beginning of our database. To avoid missing observations in an unbalanced panel of police stations over time, I aggregated the statistics at the police stations before the changes. I do the same procedure for any other police station information that may change over time: I consider the information related to the police station before the change[7]. In Appendix A, I show how I coded these changes.

I utilize the official shapefiles with the precise boundaries for police stations and UPPs from the Institute of Public Security (ISP-RJ) and the shapefile for large favelas from MPRJ In Loco[8], a data aggregator project from Rio de Janeiro's Public Attorney Office. Figure 6 (a) displays the distribution of large favelas in Rio on top of the police station's boundaries. I define that a police station is treated if at least one large favela is treated within its limits. If there is more than one large treated favela, I consider the treatment time as the first period in which a favela was treated. A control police station within the city of Rio has at least one large untreated favela in its domain and no treated favelas. Figure 6 (b) exhibits the police stations treated and in the control for the city of Rio de Janeiro. I opt for this definition for treated and control police stations in the city of Rio to shed light on possible spatial spillovers that might have occurred in untreated large favelas. Although police station data is coarser than the favela level, it is the maximum spatial disaggregation that I can get comparable violence data for treated and control places because geocoded violence data is not available for all of these large favelas.

I separate the other police stations outside the city of Rio de Janeiro into groups based on each area's socioeconomic and criminal characteristics. Figure 7 exhibits the other geographical areas in the metropolitan area or the state's rural regions. The *Baixada* region comprises economically disadvantaged municipalities historically marked by the presence of militias. *Niteroi* is a rich neighborhood within the metropolitan region where drug gangs operate in the favelas. *Sao Goncalo* is characterized by poor cities where drug gangs also control some favelas in the region. The *Countryside* category encompasses the re-

[7]For example, some of the police stations change the Police Battalion they are related. In that case, I consider the previous Police Battalion.

[8]`http://apps.mprj.mp.br/sistema/inloco/`. Accessed in June 2022.

maining police stations in Rio de Janeiro State. These areas exhibit more socioeconomic heterogeneity.

In total, there are 37 UPPs units in the city of Rio located in 28 treated large favelas and 25 large favelas not treated that I use to define the control group. At the police station level, these translate to 18 treated and 11 control police stations. Using information provided by ISP-RJ, the population in treated police stations is around 2.8 million citizens, while control police stations have more than 2.3 million individuals. The other sub-markets outside the city of Rio de Janeiro have not been treated[9].

Figure 1 presents the evolution of crime variables over time for different sub-markets, and table 1 shows the summary statistics for violence outcomes two periods before and one after treatment for police stations in the city of Rio, metropolitan region and countryside. All the regions displayed a similar downward trend before the beginning of the UPP policy in 2009. After the policy, the trends differ: while violence indicators keep decreasing for treated and untreated police stations in the city of Rio, there are inflection points in the trends for other regions outside the city. Untreated police stations in the city are more violent in the pre-intervention period (2007-2008) but display the same police killings rate as treated areas. The semester average total homicide rate per 100,000 citizens for the years before the beginning of the policy (2007 and 2008) is slightly smaller ($p < 0.1$) for treated places (24.1) than for control police stations (30.4). The semester police killings rate is statistically the same for treated and control police stations: 7.7 and 6.8 ($p = 0.55$), respectively. In terms of total homicide rate, treated areas show similar levels to other areas in the metropolitan area for the baseline (2004-2008). Regarding police killings rate, treated regions display higher rates than other regions considered. Although the violence levels may differ, there are no clear differential trends in periods before the treatment, which is vital for my empirical strategy.

4 Empirical Strategy

I exploit the staggered introduction of the policy to estimate the impact of UPP treatment on violence measures. The identification relies on parallel trends and no anticipation assumptions. Hadn't the treatment occurred, the trajectory of treated areas would follow a similar path to control units.

[9] The only exception is one UPP in the Baixada region, installed in 2014. Miagusko (2016) argue that the UPP entered this favela after an event caused by drug criminals who had moved to the area after the beginning of the policy in the city of Rio. Thus, I do not consider this region to be directly treated. I prefer to analyze the area as a sub-market to discuss the spatial spillovers.

The estimation equation is:

$$Y_{it} = \lambda_i + \delta_t + \beta D_{it} + \epsilon_{it} \quad (1)$$

where, i denotes the police station and t semester; λ_i and δ_t are the police station and time fixed effects, respectively. D_{it} is a dummy that turns one for semesters after the semester of the beginning of the UPP's occupation in a favela; ϵ_{it} is the error term. In this specification, β is the parameter of interest, and I test the hypothesis that $\beta \neq 0$. I expect the UPP program to reduce violence, so β would be negative. The standard error ϵ_{it} can be correlated to other observations within the same police battalion, i; therefore, they are clustered at the police battalion level.

The UPP policy caused differential responses in treated areas. Residents in early-treated favelas are more prone to approve the program than citizens in late-treated areas (Ribeiro and Vilarouca, 2018; Willadino et al., 2018). Moreover, there is suggestive evidence that the program did better in the beginning (Magaloni et al., 2018). Thus, it is likely that the treatment had heterogeneous effects among treated units. To avoid concerns related to applying a Difference-in-Differences estimation in this context, I employ the estimator proposed by Borusyak et al. (2022) as a robustness exercise.

I also estimate a dynamic difference-in-differences specification:

$$Y_{it} = \alpha + \lambda_i + \delta_t + \sum_{\tau=-6}^{-2} \gamma_\tau D_{i\tau} + \sum_{\tau=0}^{6} \beta_\tau D_{i\tau} + \epsilon_{it} \quad (2)$$

where $D_{it\tau}$ is a dummy that turns one if the temporal distance to treatment is τ. Positive values refer to periods after the beginning of the policy, and negative values to periods before. To increase precision for each bin, I collapse the observations of six consecutive months into semesters. Thus, the estimates reflect the effects of semesters before and after the beginning of treatment. I binned all periods before and after 7 semesters to -7 and 7, respectively, and I include these periods in the regression to minimize threats to the identification. The coefficients of interest, in this case, are $\{\gamma_\tau\}_{\tau<0}$, which represent the effects of treatment in periods before the policy started, and $\{\beta_\tau\}_{\tau>0}$, the treatment effects. I expect that estimates for $\tau < 0$ are not statistically different from zero and that coefficients for $\tau > 0$ display a negative sign.

To reduce concerns raised by the positive skewness of the data, I construct semester violence rates from monthly data. Thus, the dependent variables are the semester crime rates of violence indicators. I use three main violence measures: total homicides, police killings, and other homicides. Total homicides are defined by the sum of police killings and other homicides. Police killings refer to homicides committed by on-duty police officers. Usually, these events happen during police raids within the favelas (Monteiro et al., 2020) when the police perform military operations with the goals of arresting drug criminals and apprehending drugs. Given that criminal organizations control the territory, police raids often cause a shootout between police officers and drug traffickers (Hirata et al., 2022). The last violence indicator is other homicides. The variable measures homicides caused by interpersonal violence. Following Bellégo and Drouard (2024), I also present the estimates for the log of crime rates plus one in robustness exercises.

5 Results and Discussion

The UPP program potentially led to a response of criminal actors in treated favelas: they could stay in these favelas and adapt to a new equilibrium with police officers continuously operating there, migrate to other favelas, or leave the drug business. Indeed, some criminal actors adapted to the new policy and became more discreet in their routine operations (Serrano-Berthet et al., 2012), and some migrated to other favelas (Menezes, 2018). There is anecdotal evidence that drug traffickers migrated to places such as the Metropolitan Region and the countryside of the state of Rio (Willadino et al., 2018). I empirically test these hypotheses.

First, I analyze the temporal evolution of crime indicators to these areas in figure 1. The figure suggests that treated and untreated police stations in the city of Rio, the metropolitan area, and the countryside followed similar trends as the other areas before the program started. This pattern holds until 2012. After this period, the trends diverge as violence levels increase in other areas. Second, I perform the difference-in-differences estimation by alternating the geographical areas used as control units.

Conceptually, under the parallel trends and SUTVA assumptions, any group that captures contemporaneous shocks could be a suitable control group for treated units. In these cases, the control group imputes the temporal trends after the treatment and provides a counterfactual to the treated group. Assuming that the criminal market was in equilibrium before the beginning of the UPP program, common shocks to this market are expected to affect the players similarly. Thus, under the assumptions discussed, the coefficients should be stable regardless of the control group used.

I show in table 2 that DD estimates using these different samples as control groups are not stable. The coefficients change from large and highly significant to small and statistically insignificant depending on the control unit used. For example, the effects of UPP on total homicides are statistically equal to zero when untreated police stations in the city of Rio are the control group. However, when I use the other geographical regions as control units, the effects indicate a sizeable reduction in total homicides, ranging from an 11 percent reduction to a 29 percent decrease compared to the mean total homicide rate for treated units before treatment.

Since the parallel trends assumption is important for the argument, I provide further evidence that it holds for different control units used in the estimation. Figure 2. To facilitate the comparison, I put all the estimates in the same place in Figure 8. Table 3 displays the F-test results for the hypothesis test that the lead coefficients are jointly equal to zero.

I perform some robustness checks in Tables 4 to 6. First, I show that the results are robust to heterogeneous treatment effects (Borusyak et al., 2022). Then, I change the way that the dependent variable is constructed. I analyze the log of the crime levels plus one, and I use the same definition of the dependent variable as in Bellégo and Drouard (2024). The results point in the same direction as before. In particular, the magnitude of the effects is similar to the main estimation.

The findings suggest that all other units in the state of Rio receive idiosyncratic shocks around the same period or, more likely, criminals in these areas respond to the UPP program in the city of Rio and migrate to these areas (Miagusko, 2016; Menezes, 2018; Magaloni et al., 2020). Given that the distribution of territorial control spreads over the state of Rio and that drug criminals operate in this unified market, it is unlikely that idiosyncratic shocks explain the results.

The results for untreated police stations in the city of Rio are interesting. They suggest that relative to untreated police stations in the city of Rio, the UPP program did not reduce total homicides. First, note that I consider data at the police station level, a coarser geographical area than large favelas, which were the target of the policy. If anything, the police station level estimation also incorporates any spatial spillovers to treated areas outside the favelas in a treated police station. So, the effects on total homicides suggest that the UPP had no effect on this type of violence *accounting for* local spillovers in the police stations' catchment areas. Notably, the estimates for police killings point in the show a significant decrease. Considering that most police killings happen within the favelas (Monteiro et al., 2020) and the UPP program focused on changing the logic of policing inside the favelas, this outcome is reassuring[10]. Finally, the consequences of the treatment of other homicides are somewhat expected. The dynamics of other homicides outside the

[10]This result is consistent with the change of police strategy in treated places. Instead of transitory police raids that often caused shootings in the favelas, the UPP program focused on permanent settlements within treated areas, which increased law enforcement and the cost of doing drug business there, reducing violent episodes.

favelas may be different than within the favelas. Although the UPP program could have impacted this type of violence, it was not the explicit goal of the policy.

To understand more about these results, I analyze the crime dynamics in the city of Rio de Janeiro. Figure 9 exhibits the evolution of homicides and police killings. There has been a downward trend for both crime indicators since 2009. Considering that the city of Rio has treated and untreated units, these figures suggest that if there is crime displacement to control areas in the city, the reduction in violence in treated police stations would have to be larger than the displacement effect. Then, I split the time series for treated and untreated areas in Rio in figure 10. These graphs show that both units display a similar pattern after 2009. The two time series are strongly linearly correlated. The Pearson correlation index is 0.94 for homicides and 0.86 for police killings. These figures suggest that, at least on average, there is no evidence for possible crime displacement to control areas.

The reduction of violence in untreated areas in Rio suggests possible alternatives. It could indicate a confounder of the UPP program drives the results in both treated and control areas. For example, an income shock at the city level. I am not aware of any policy or event that simultaneously happened at the city level to be a confounder of the UPP. Alternatively, Menezes (2018) and Cano et al. (2012) argue that the UPP was a critical moment for Rio's criminal market, which induced criminal agents to respond. Serrano-Berthet et al. (2012) suggest that criminals in untreated favelas in the city of Rio changed their behavior in response to the policy. Due to the spatial proximity to other treated places, drug traffickers in favelas in the city of Rio became more discreet to reduce the probability of receiving treatment in their favelas and to adapt to a new equilibrium in which their favelas will possibly be treated. These mechanisms indicate that crime diffusion is a possible result of the UPP policy.

Then, given the plausibility of crime diffusion to untreated areas in the city of Rio, the treatment effects could be viewed as bounded by the minimum and maximum values, representing the spectrum of potential impacts. The minimum treatment effect denotes a scenario where the policy creates positive spatial spillovers, possibly due to criminal actors successfully adapting to the new equilibrium or strategically relocating their economic activities to avoid intervention. In this context, the policy would decrease violence in control units, and the DD estimator would underestimate the effect of the policy. Conversely, the maximum treatment effect signifies a situation where the policy induces crime displacement, resulting in a significant alteration of crime indicators in control areas. In this case, the DD estimator would overestimate the impact of the policy. Thus, treatment effects should be somewhere in between the minimum value and the maximum value. Alternatively, in the presence of crime displacement, one can use the same reasoning to rule out treatment effects greater than the maximum value. Thus, the results indicate that the UPP program was able to reduce police killings by at least 25% and total homicides by at most 29%.

Moreover, the evidence of crime diffusion also helps to estimate the magnitude of spatial spillovers to other control units. Intuitively, an upper bound for the spillovers would be the difference between the coefficients retrieved from the estimation using the control group related to positive spillovers and using the control group associated with negative spillovers. That is, the group associated with crime diffusion would be a counterfactual for the units that showed crime displacement. With this exercise, I can rule out spatial spillovers bigger than this value. For example, if I use the countryside as the control group, the effect of UPP on police killings is a reduction of 48% of the mean of treated units before the treatment. However, if I consider untreated police stations in the city of Rio, this effect is a 25 percent reduction. The difference between these values should provide insights into the magnitude of spatial spillovers. In a back-of-the-envelope calculation for Rio's context, I can bound the spatial spillover effects to be inferior to 26% of the mean before treatment for total homicides and less than 23% for police killings.

Taken together, these findings suggest that the policy reduced violence in treated places, induced a response in untreated units in the city that lowered crime in these areas, and partially displaced crime to other regions in the state of Rio. I do not intend to structurally estimate the general equilibrium effects of the policy in this paper, and I leave it for future research. However, I provide suggestive evidence that crime displacement to other areas in the state of Rio de Janeiro is possible and how one can deal with spatial spillover in a reduced form specification.

6 Conclusion

Several cities worldwide face urban violence problems that could be magnified by the presence of organized crime that dominates some areas in the urban scenario. This paper addresses the spatial consequences of a public policy - the Pacification Police Units program (UPP) - designed to reduce the territorial control of drug gangs. These criminal organizations operate in non-contiguous parts of the metropolitan area of Rio de Janeiro, which imposes a challenge in estimating the impact of a place-based intervention such as the UPP.

I find that the UPP program decreases crime outcomes in treated areas and reduces crime in untreated police stations in the city of Rio that have at least one large favela within their boundaries. This last finding is consistent with a crime deterrence interpretation.

Therefore, no evidence using these units as control groups overestimates the effects of the police. However, there is suggestive evidence of negative spillovers to untreated regions in the metropolitan area and the countryside of the state of Rio, which could overestimate the treatment effects. In general, the empirical exercises in this paper suggest the importance of careful consideration of spatial spillovers while evaluating or designing this type of place-based policy. Considering untreated places that received negative spillovers as a control group might bias the results and distort the cost-benefit analysis of the program.

Future research can address how the UPP program changed the equilibrium of this criminal market in future research. First, the evolution of drug factions and militias over time allows estimating the spatial game played by these criminal groups using a revealed preference argument for the value of a favela (Seim, 2006; Adda et al., 2014). With the distribution of drug factions and militias over time, one could define places of conflict and correlate these areas with the economic, demographic, and geographical characteristics of these places. This could shed light on the strategies played by the agents in the spatial game. Moreover, the researcher can use another source of homicide data from DATASUS, compiled at the neighborhood level (finer than what I used in this paper), to capture the territorial conflicts of these factions more precisely. Another fruitful possibility is to consider discussing how policymakers can design alternative policies to minimize the probability of migration or analyze the optimal selection of places to receive the program, considering the migration responses of criminals. In this case, one could build on Fu and Wolpin (2018), who studied the optimal allocation of police forces across cities in the US, extending criminals' choice set to incorporate a locational choice as a response to the treatment.

Understanding crime dynamics in a city is critical to perform counterfactual treatments. Inferring the drug gangs' preferences and objective function and the game played with the other criminal groups and the police are necessary to anticipate their responses caused by State intervention. This paper provides several stylized facts to comprehend the game these actors play and then design more cost-effective public policies.

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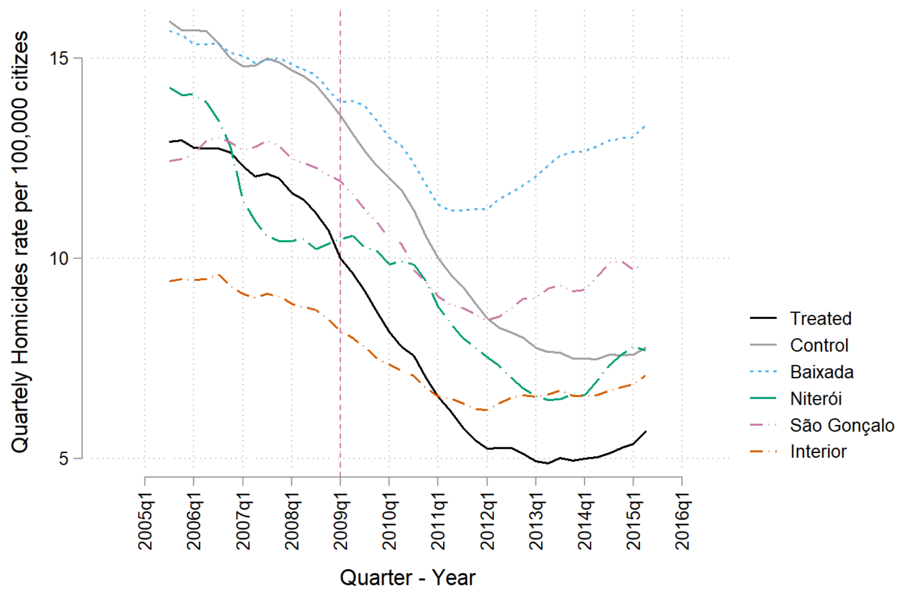
7 Figures and Tables

Table 1: Summary statistics violence indicators per period of time

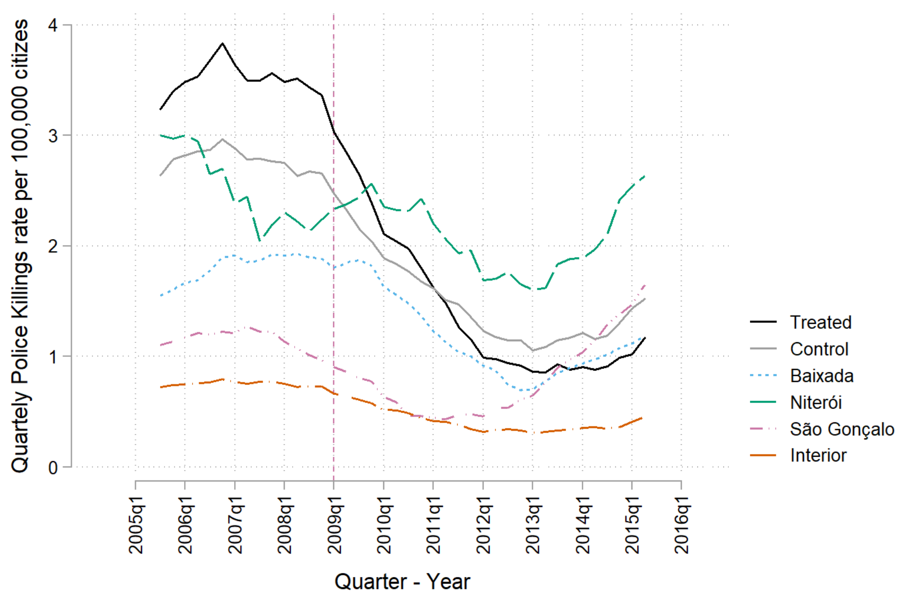
Period	Treated	All regions	Untreated city	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Total homicides rate							
2004-2006	27.15	23.03	32.26	29.64	29.93	25.40	19.38
		(0.20)	(0.13)	(0.30)	(0.52)	(0.68)	(0.04)
2007-2008	24.15	22.36	30.39	27.75	20.36	26.44	19.68
		(0.72)	(0.08)	(0.14)	(0.36)	(0.58)	(0.46)
2009-2015	12.60	16.06	19.38	24.38	17.30	20.48	13.12
		(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.73)
Panel B: Police killings rate							
2004-2006	6.70	2.18	6.15	2.58	5.44	2.08	1.27
		(0.00)	(0.57)	(0.00)	(0.34)	(0.00)	(0.00)
2007-2008	7.72	2.61	6.83	3.54	3.52	2.64	1.68
		(0.00)	(0.55)	(0.00)	(0.01)	(0.00)	(0.00)
2009-2015	3.02	1.38	3.69	1.93	4.41	2.02	0.65
		(0.00)	(0.12)	(0.00)	(0.01)	(0.04)	(0.00)
Panel C: Other homicides rate							
2004-2006	20.44	20.85	26.11	27.06	24.49	23.32	18.11
		(0.89)	(0.03)	(0.00)	(0.25)	(0.39)	(0.50)
2007-2008	16.42	19.76	23.56	24.22	16.85	23.80	18.01
		(0.42)	(0.01)	(0.00)	(0.89)	(0.02)	(0.75)
2009-2015	9.57	14.68	15.68	22.45	12.88	18.46	12.48
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.05)

Notes: Table shows the summary statistics for homicide indicators for treated and untreated police stations in different periods of time. Total homicides are defined as the sum of police killings and other homicides. I divide the number of homicides in a semester by the population in each police station to construct the semester rates per 100,000 individuals. Column (1) displays the mean of these semester rates for treated places, column (2) the mean for untreated police stations, and column (3) shows a T-test for the differences of the means. Values in parenthesis are the p-values from T-test for the differences of the means between treated police stations and police stations that belong to the column's geographical region.

Figure 1: Temporal evolution of crime indicators for police stations in the state of Rio de Janeiro

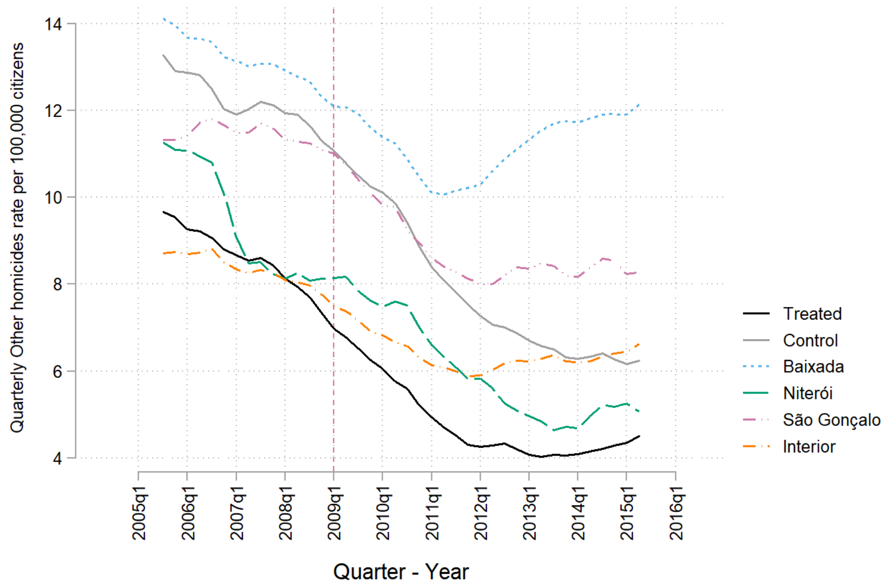


(a) Homicides rates



(b) Police killings rate

Figure 1: Temporal evolution of crime indicators for police stations in the state of Rio de Janeiro (cont.)



(c) Other homicides rates

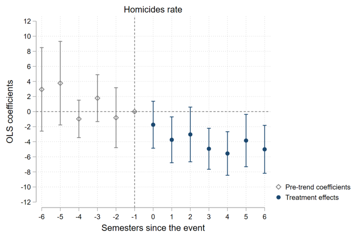
Notes: The figures show the temporal evolution of homicides and police killings in the State of Rio de Janeiro. I consider six quarters before and six quarters after the current period to calculate the moving average. The police stations in the city of Rio that I use in the main estimation are the "Treated" and "Control" lines. The rest of the lines represent areas in the Metropolitan Region ("Baixada", "Niterói", "São Gonçalo") and in the countryside of the state.

Table 2: Effects of UPP on crime indicators for different control groups

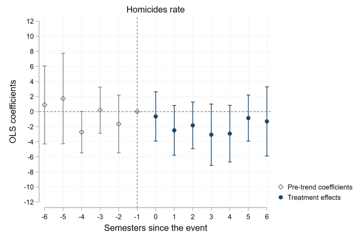
	All	Untreated Rio	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Total homicides						
Treat	-5.86	-0.39	-6.81	-2.69	-5.15	-6.93
	(1.86)***	(1.77)	(2.35)**	(1.45)*	(1.94)**	(1.92)***
Mean before treat	24.20	24.20	24.20	24.20	24.20	24.20
$\|\|Treat/Mean\|\|$	0.24	0.02	0.28	0.11	0.21	0.29
Panel B: Police Killings						
Treat	-3.08	-1.68	-2.71	-2.08	-3.06	-3.23
	(0.74)***	(0.66)**	(0.84)***	(0.95)**	(1.17)**	(0.82)***
Mean before treat	6.77	6.77	6.77	6.77	6.77	6.77
$\|\|Treat/Mean\|\|$	0.45	0.25	0.40	0.31	0.45	0.48
Panel C: Other homicides						
Treat	-2.78	1.29	-4.10	-0.60	-2.09	-3.70
	(1.49)*	(1.73)	(1.83)**	(0.98)	(0.99)*	(1.45)**
Mean before treat	17.42	17.42	17.42	17.42	17.42	17.42
$\|\|Treat/Mean\|\|$	0.16	0.07	0.19	0.04	0.11	0.21
Semester FE	Yes	Yes	Yes	Yes	Yes	Yes
UPP	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,377	754	935	598	598	2,364

Notes: Table shows the results for regression equation (1) for TWFE estimator. The dependent variables are the semesters' rates per 100,000 citizens. Both regressions control for Semester and Police stations fixed effects, have standard errors clustered at the police battalion level and use population as analytical weights. Each column represents a separate regression that uses different samples as control units. 'Mean before treat' refers to the average semester crime rate for treated units before the beginning of treatment. * significant at 10%; ** significant at 5%; *** significant at 1%.

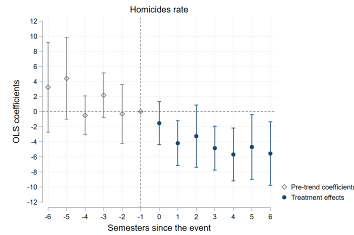
Figure 2: Dynamic effects for different control units – Total homicides



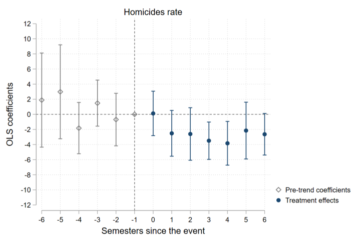
(a) All regions



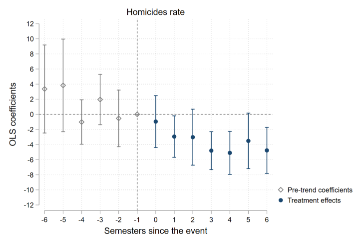
(b) Untreated city Rio



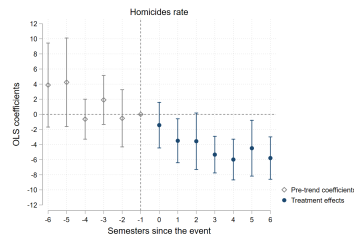
(c) Baixada



(d) Niteroi

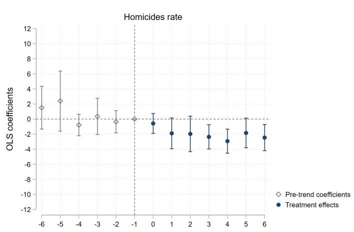


(e) Sao Goncalo

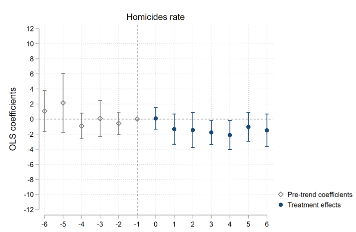


(f) Countryside

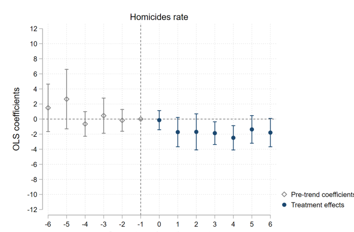
Figure 2: Dynamic effects for different control units – Police killings (cont.)



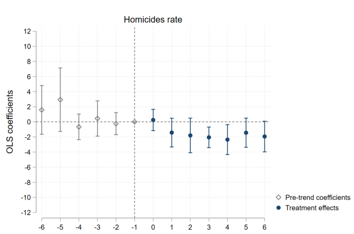
(g) All regions



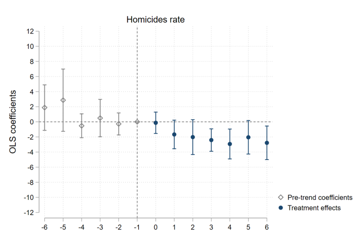
(h) Untreated city Rio



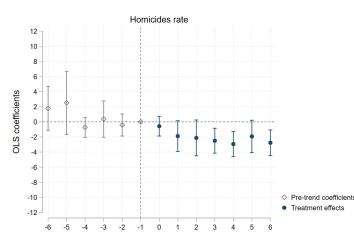
(i) Baixada



(j) Niteroi

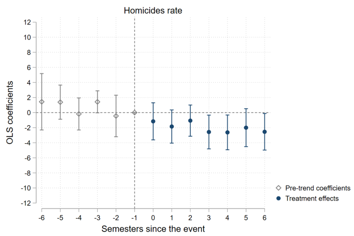


(k) Sao Goncalo

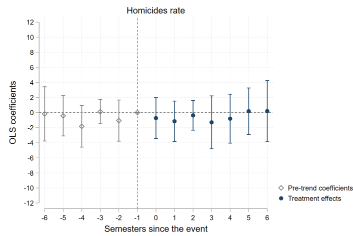


(l) Countryside

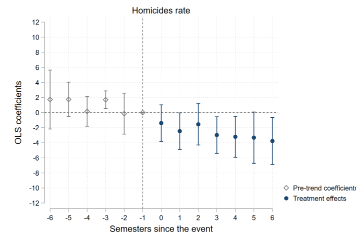
Figure 2: Dynamic effects for different control units – Other homicides (cont.)



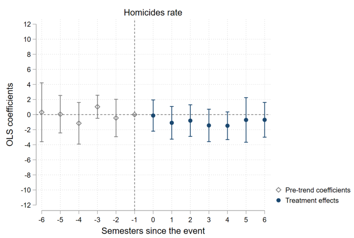
(m) All regions



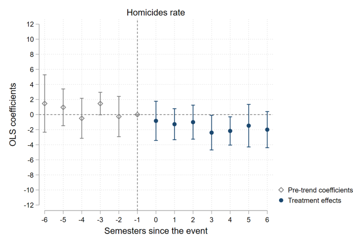
(n) Untreated city Rio



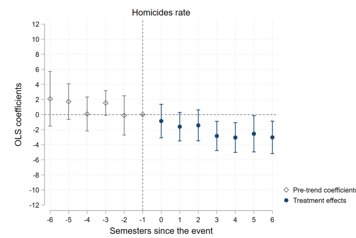
(o) Baixada



(p) Niteroi



(q) Sao Goncalo



(r) Countryside

Notes: These figures report the coefficients for the TWFE estimation of equation (2) for different dependent variables and geographical units used as control group. Each graph uses the indicated region as the control group. Regressions are weighted by the population in each police station and standard errors are clustered at the police battalion level. The vertical bars show the 95 percent confidence interval. The period immediately before the beginning of the policy is normalized to zero.

Table 3: F-test for violence indicators by different control units

Variable	All regions	Untreated city	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Total homicides rate						
p-value	0.15	0.10	0.22	0.15	0.12	0.14
F-statistic	1.72	2.25	1.59	2.02	2.17	1.83
Panel B: Police killings rate						
p-value	0.31	0.27	0.40	0.05*	0.21	0.23
F-statistic	1.23	1.44	1.10	3.04	1.67	1.47
Panel C: Other homicides rate						
p-value	0.29	0.37	0.15	0.44	0.22	0.29
F-statistic	1.28	1.18	1.88	1.04	1.65	1.30
Degrees of freedom	5	5	5	5	5	5
Residual df	39	15	17	12	13	28

Notes: Table shows the F-test for lead variables from equation (2) for different geographical regions. I report the p-value for the hypothesis test that the lead coefficients are jointly equal to zero.

8 Additional Figures and Tables

Figure 3: Large favelas by treatment year

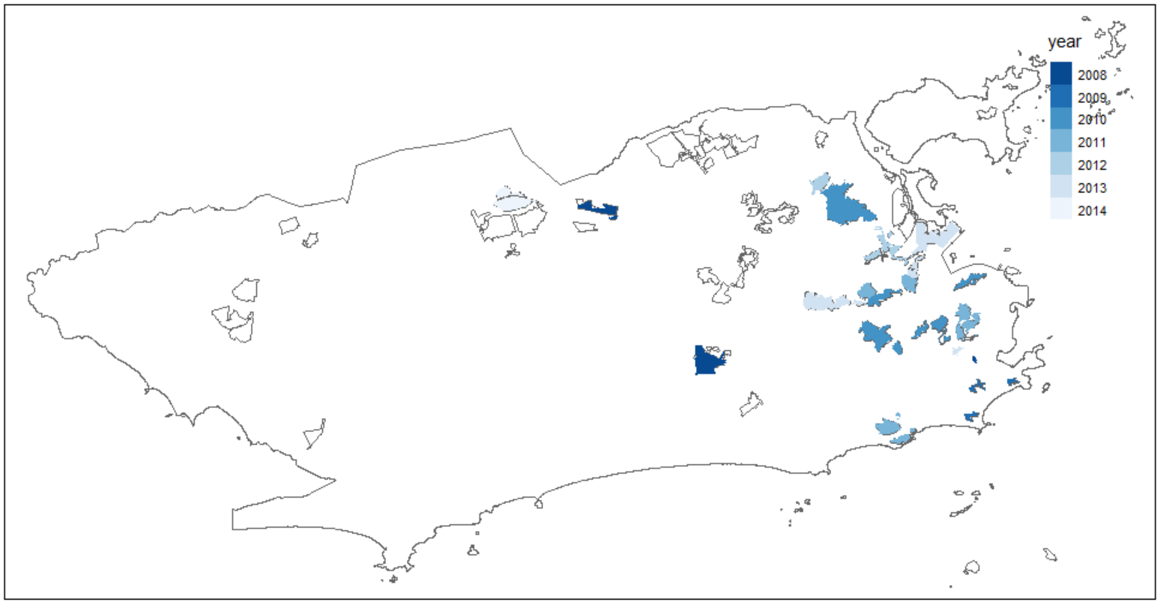
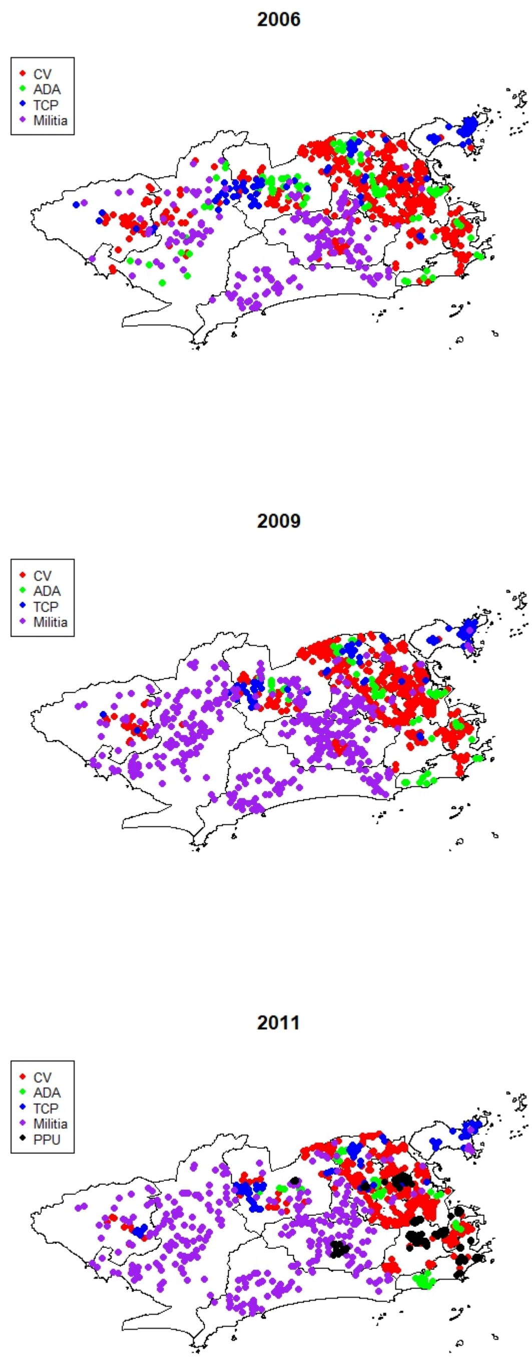
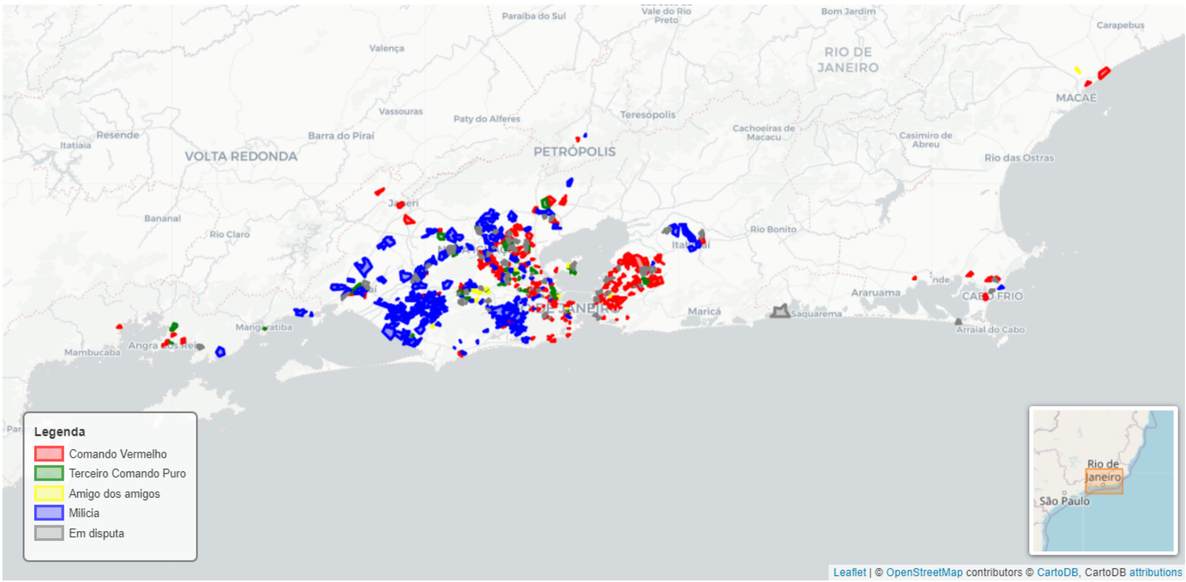


Figure 4: Distribution of criminal groups in Rio de Janeiro

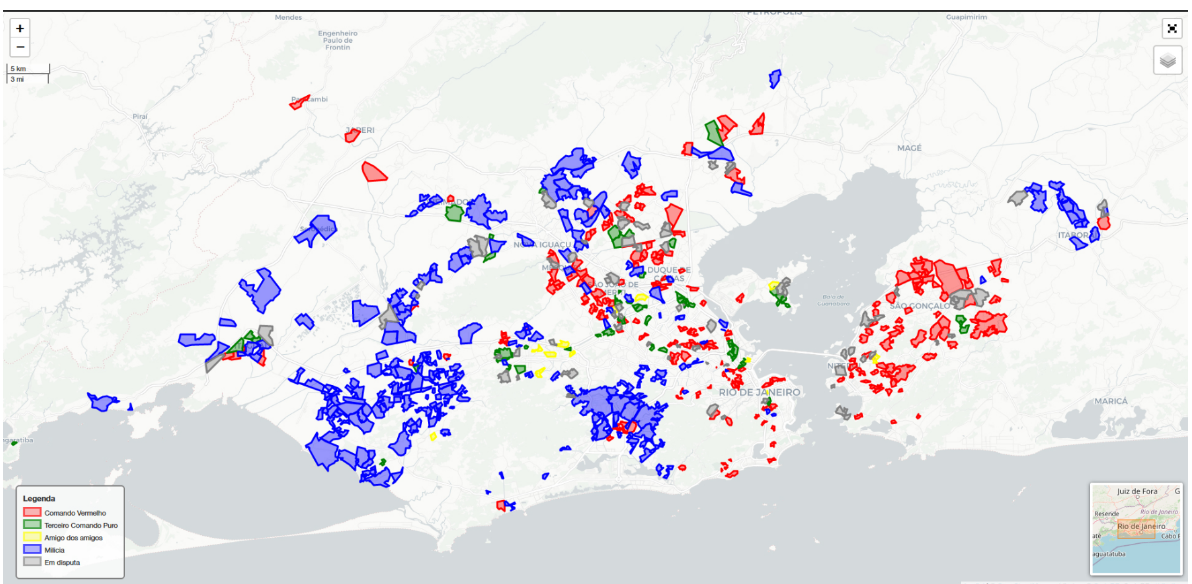


Source: Zaluar (2012) and Zaluar and Barcellos (2014). Geocoded by the author.

Figure 5: Distribution of drug gangs and militia in Rio de Janeiro - 2019



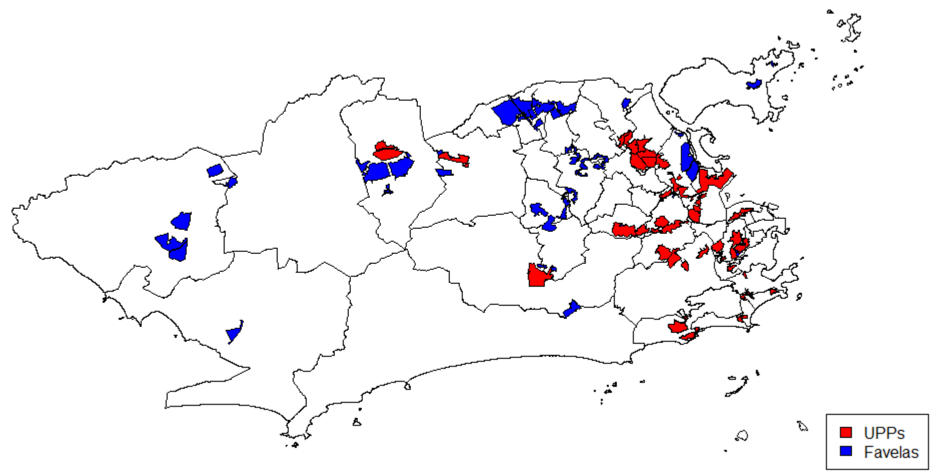
(a) State of Rio de Janeiro



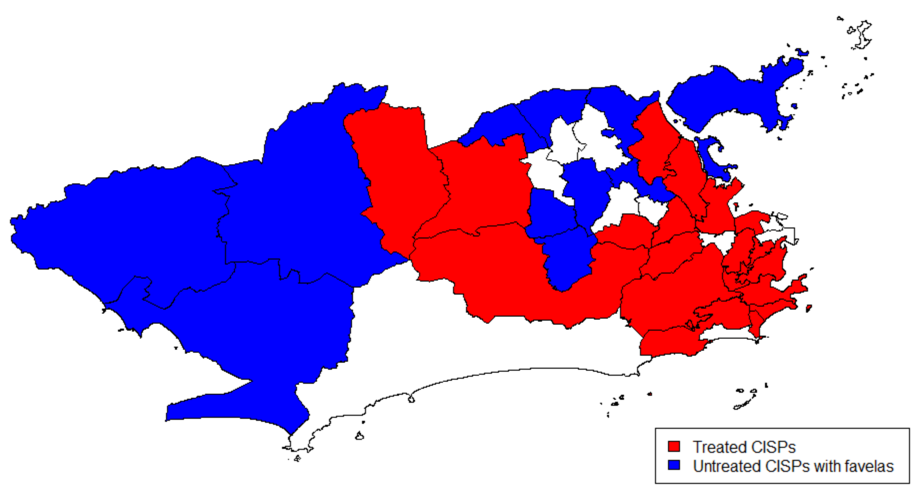
(b) Metropolitan Region

Source: Fogo Cruzado, NEV-USP, GENI-UFF, Pista News. Data comes from Disque-Denuncia. `https://erickgn.github.io/mapafc/` and `https://nev.prp.usp.br/mapa-dos-grupos-armados-do-rio-de-janeiro/` for more information. Accessed in June, 2022.

Figure 6: Treated and Untreated police stations in the city Rio de Janeiro



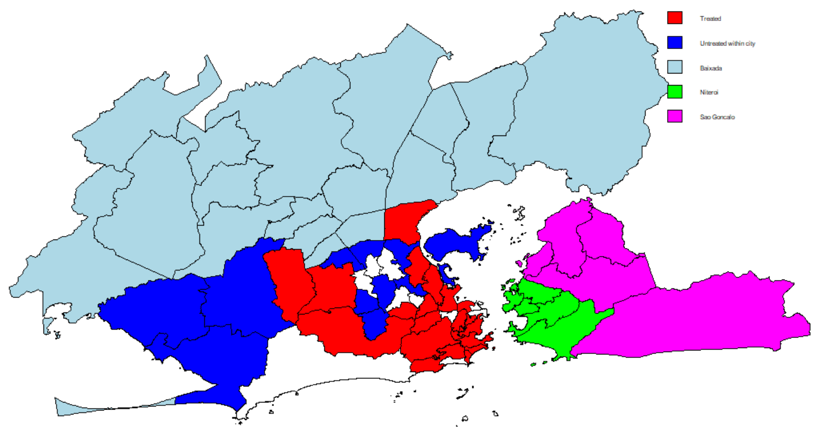
(a) Large favelas and Police Stations boundaries



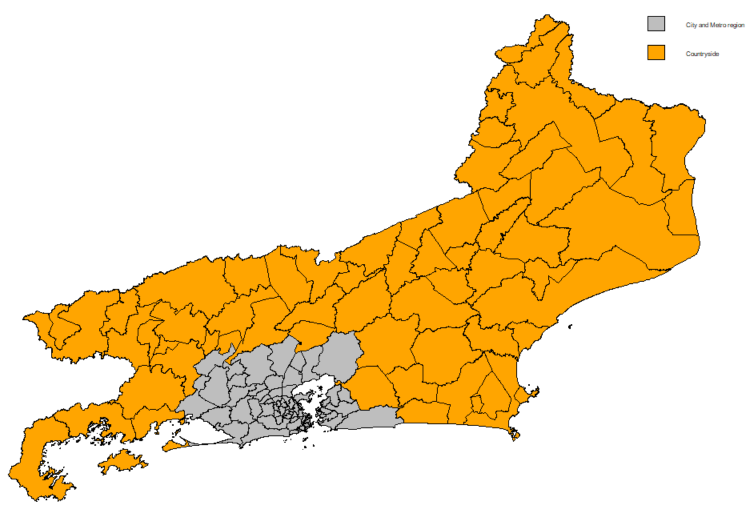
(b) Treated and Control Police Stations

Notes: The figure shows how I defined the police stations treated and in the control group. A police station is treated if there is at least one large treated favela within its domain. Similarly, a police station belongs to the control group if there is no treated favela, but there is at least one large untreated favela in its catchment area.

Figure 7: Treated and Untreated police stations in the state of Rio de Janeiro



(a) Police stations boundaries and geographical regions in the Metropolitan Area



(b) Countryside

Notes: The figure shows how I defined the geographical regions used as control units. Red defines treated police stations, blue untreated police stations in the city of Rio, light blue Baixada, green Niteroi, magenta Sao Goncalo and orange Countryside. Gray defines the Metropolitan area.

Table 4: Robustness for effects of UPP on crime indicators: Borusyak et al. (2022) estimator

	All	Untreated Rio	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Homicides						
Treat	-5.37	0.72	-6.50	-1.92	-5.11	-6.53
	(1.51)***	(1.98)	(2.02)***	(1.08)*	(1.16)***	(1.48)***
Mean before treat	24.20	24.20	24.20	24.20	24.20	24.20
$\lVert Treat\,/\,Mean \rVert$	0.22	0.03	0.27	0.08	0.22	0.27
Panel B: Police Killings						
Treat	-2.92	-1.50	-2.58	-2.56	-3.48	-3.12
	(0.37)***	(0.70)**	(0.49)***	(0.43)***	(0.52)***	(0.37)***
Mean before treat	6.77	6.77	6.77	6.77	6.77	6.77
$\lVert Treat\,/\,Mean \rVert$	0.43	0.22	0.38	0.38	0.51	0.46
Panel C: Other homicides						
Treat	-2.45	2.22	-3.92	0.64	-1.63	-3.42
	(1.35)*	(1.79)	(1.71)**	(0.91)	(0.88)*	(1.32)***
Mean before treat	17.42	17.42	17.42	17.42	17.42	17.42
$\lVert Treat\,/\,Mean \rVert$	0.14	0.12	0.18	0.04	0.09	0.20
Semester FE	Yes	Yes	Yes	Yes	Yes	Yes
UPP	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,377	754	935	598	598	2,364

Notes: Table shows the results for regression equation (1) for Borusyak et al. (2022) imputation estimator. The dependent variables are the inverse hyperbolical sine transformation of semesters' rates per 100,000 citizens. Both regressions control for Semester and Police stations fixed effects, have standard errors clustered at the police battalion level and use population as analytical weights. Each column represents a separate regression that uses different samples as control units. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Robustness for effects of UPP on crime indicators: log in the level of dependent variable

	All	Untreated Rio	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Homicides						
Treat	-0.54	-0.31	-0.59	-0.30	-0.46	-0.55
	(0.08)***	(0.09)***	(0.10)***	(0.07)***	(0.09)***	(0.09)***
Mean before treat	3.18	3.18	3.18	3.18	3.18	3.18
$\|\|Treat/Mean\|\|$	0.17	0.10	0.18	0.09	0.14	0.17
Panel B: Police Killings						
Treat	-0.71	-0.37	-0.56	-0.62	-0.68	-0.76
	(0.12)***	(0.12)***	(0.17)***	(0.20)**	(0.22)***	(0.13)***
Mean before treat	1.96	1.96	1.96	1.96	1.96	1.96
$\|\|Treat/Mean\|\|$	0.36	0.19	0.29	0.32	0.35	0.39
Panel C: Other homicides						
Treat	-0.42	-0.22	-0.50	-0.13	-0.34	-0.44
	(0.07)***	(0.09)**	(0.09)***	(0.08)	(0.08)***	(0.07)***
Mean before treat	2.85	2.85	2.85	2.85	2.85	2.85
$\|\|Treat/Mean\|\|$	0.15	0.08	0.18	0.05	0.12	0.15
Semester FE	Yes	Yes	Yes	Yes	Yes	Yes
UPP	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,377	754	935	598	598	2,364

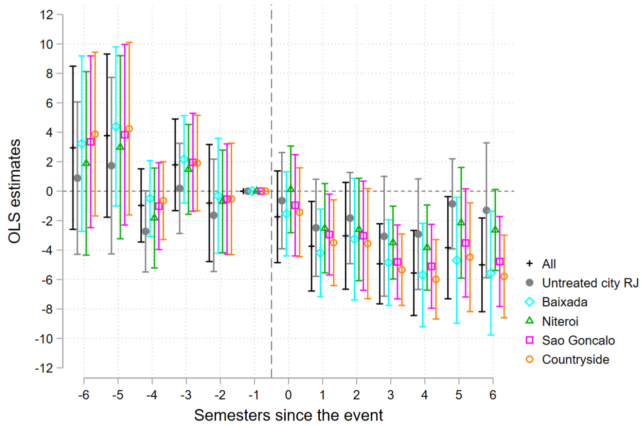
Notes: Table shows the results for regression equation (1) for TWFE estimator. The dependent variables are the log transformation of crime levels plus one. Both regressions control for Semester and Police stations fixed effects, have standard errors clustered at the police battalion level and use population as analytical weights. Each column represents a separate regression that uses different samples as control units. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Robustness for effects of UPP on crime indicators: dependent variable as in Bellégo and Drouard (2024)

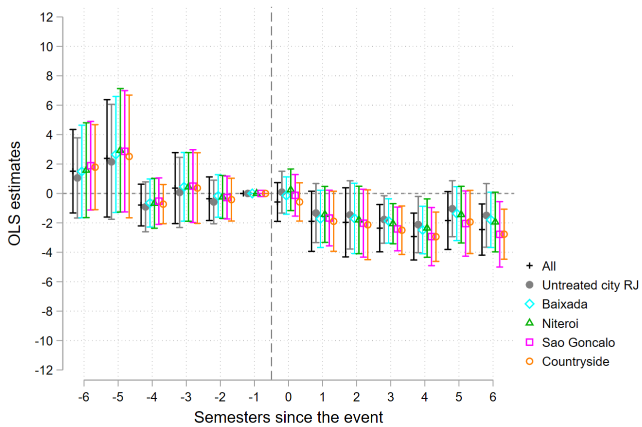
	All	Untreated Rio	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Homicides						
Treat	-0.49	-0.31	-0.58	-0.31	-0.43	-0.49
	(0.08)***	(0.09)***	(0.09)***	(0.06)***	(0.08)***	(0.09)***
Mean before treat	2.93	2.93	2.93	2.93	2.93	2.93
$\|\|Treat/Mean\|\|$	0.17	0.11	0.19	0.11	0.14	0.21
Panel B: Police Killings						
Treat	-0.69	-0.42	-0.57	-0.62	-0.65	-0.72
	(0.10)***	(0.09)***	(0.14)***	(0.17)***	(0.19)***	(0.11)***
Mean before treat	1.72	1.72	1.72	1.72	1.72	1.72
$\|\|Treat/Mean\|\|$	0.40	0.24	0.33	0.36	0.38	0.42
Panel C: Other homicides						
Treat	-0.37	-0.22	-0.49	-0.14	-0.31	-0.38
	(0.08)***	(0.09)**	(0.08)***	(0.08)*	(0.07)***	(0.08)***
Mean before treat	2.60	2.60	2.60	2.60	2.60	2.60
$\|\|Treat/Mean\|\|$	0.14	0.08	0.17	0.05	0.11	0.17
Semester FE	Yes	Yes	Yes	Yes	Yes	Yes
UPP	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,377	754	935	598	598	2,364

Notes: Table shows the results for regression equation (1) for TWFE estimator. The dependent variables are the log transformation of semesters' rates per 100,000 citizens plus one. Both regressions control for Semester and Police stations fixed effects and have standard errors clustered at the police battalion level. Each column represents a separate regression that uses different samples as control units. * significant at 10%; ** significant at 5%; *** significant at 1%.

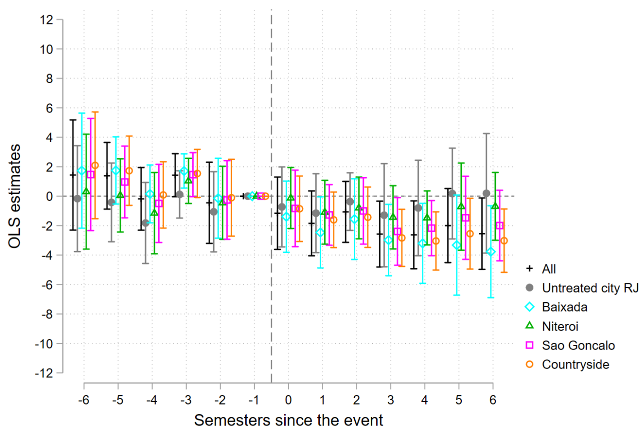
Figure 8: Dynamic effects of UPP on violence indicators for different control units



(a) Total homicides



(b) Police killings



(c) Other homicides

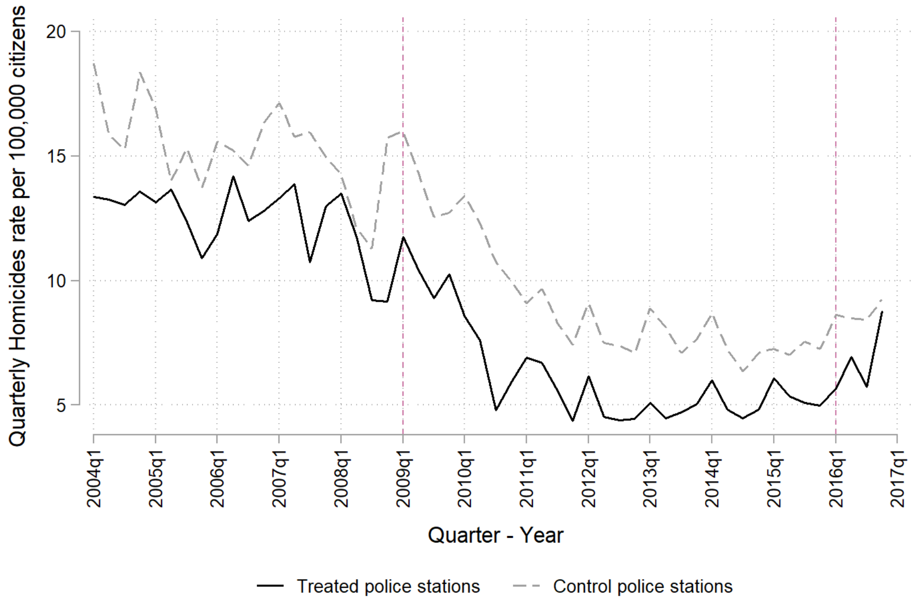
Notes: This figure shows the coefficients of Figure 2 combined together.

Figure 9: Temporal evolution for violence indicators for the city of Rio de Janeiro

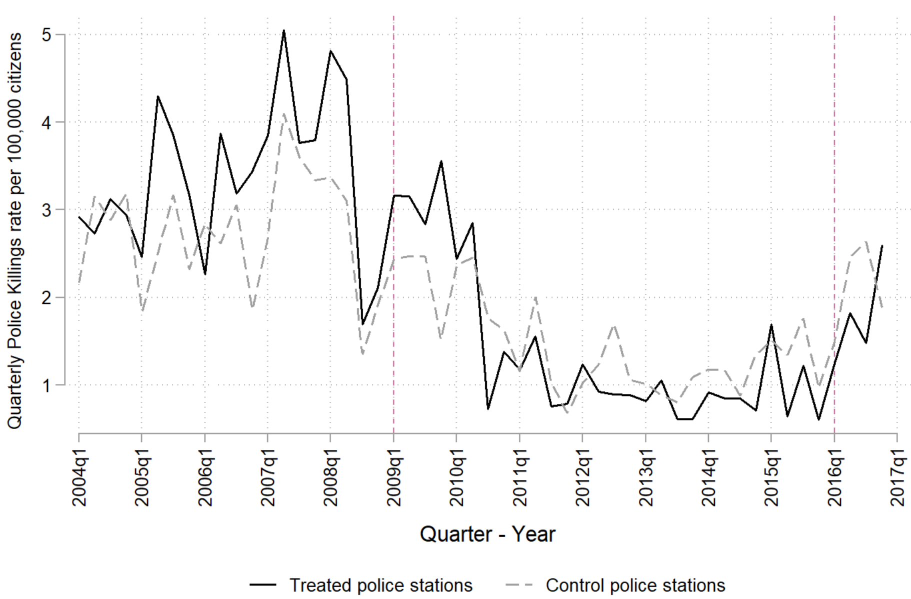
(a) Total homicides rate

(b) Police killings rate

Figure 10: Temporal evolution for violence indicators for treated and control stations in the city of Rio de Janeiro

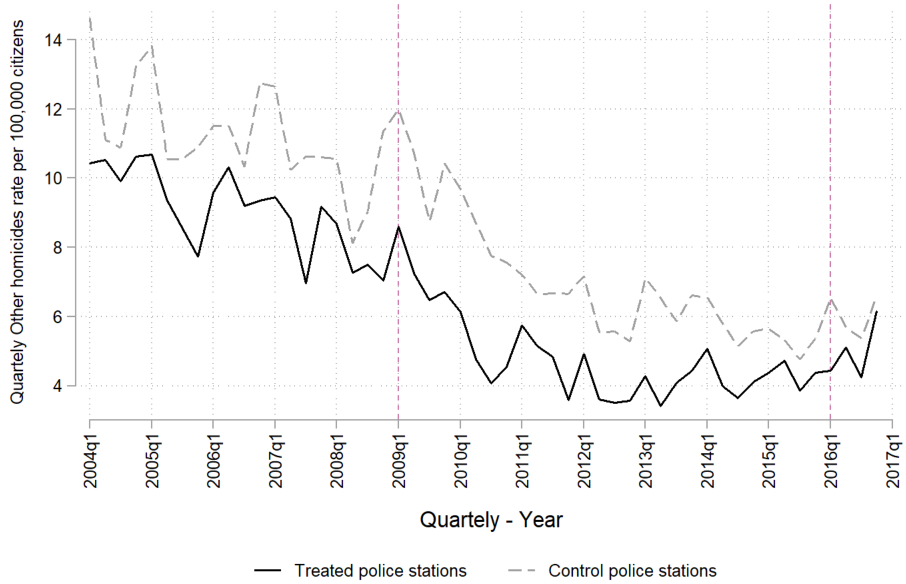


(a) Total homicides rates



(b) Police killings rate

Figure 10: Temporal evolution for violence indicators for treated and control stations in the city of Rio de Janeiro (cont.)



(c) Other homicides rate

A Corrections in police station information

I use official information data for the changes in police station information retrieved from the Institute of Public Security (ISP-RJ). I consider the first information related to a police station. For example, if a new police station is created, I define that its violence data is added to the former police station to which it was related. Then, I consider only police stations that appear over the entire period, from 2004 to 2016.

Moreover, the police stations may change the police battalion to which it belongs. This information is important because police battalions allocate police officers within their boundaries and I cluster the standard error at the police battalion level.

I document these adjustments below.

Changes in police stations' information:

- Data from police station 11 is added to police station 15;
- Data from police station 45 is added to police station 22;
- Data from police station 67 is added to police station 65;
- Data from police station 70 is added to police station 71;
- Data from police station 132 is added to police station 126;
- Data from police station 148 is added to police station 143;
- Data from police station 42 is added to police station 16;
- Data from police station 130 is added to police station 123.

Changes in police battalions' information:

- Police station 5 belongs to police battalion 13;
- Police station 6 belongs to police battalion 1;
- Police station 7 belongs to police battalion 1;
- Police station 18 belongs to police battalion 6;
- Police station 27 belongs to police battalion 9;
- Police station 29 belongs to police battalion 9;
- Police station 31 belongs to police battalion 14;

- Police station 39 belongs to police battalion 9;
- Police station 43 belongs to police battalion 39;
- Police station 100 belongs to police battalion 28;
- Police station 101 belongs to police battalion 10;
- Police station 168 belongs to police battalion 10;
- Police station 35 belongs to police battalion 39;
- Police station 54 belongs to police battalion 40;
- Police station 111 belongs to police battalion 11;
- Police station 112 belongs to police battalion 11;