Série des Documents de Travail

n° 2019-08

Does It Pay to Fight Crime? Evidence From the Pacification of Slums in Rio de Janeiro*

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Does It Pay to Fight Crime? Evidence From the Pacification of Slums in Rio de Janeiro*

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May 21, 2019

Abstract

This paper studies the effects of policies fighting drug gangs. We exploit the pacification program of slums in Rio de Janeiro, whose progressive rollout across several districts allows the identification of its causal effects on several crime indicators measured from official crime data. By combining a proxy variable and by adding simple structure to the empirical model, we correct the endogeneity bias resulting from the unobserved crime reporting change associated with the policy. We find that the program decreases murder rate by 7 percent, but increases assault rate by 51 percent, resulting in a rise in the total number of crimes. Our results are explained both by marginal and absolute crime deterrence effects, and the fact that drug gangs secure the territories under their control.

Keywords: Crime, Criminal Governance, Reporting Rate, Pacification Policy, Drug Gangs, Brazil

JEL: D74, K42

*We thank David Benatia, Pierre Boyer, Bruno Crépon, Xavier D'Haultfoeuille, Joan Monras, Salvatore Piccolo and seminar participants at CREST for insightful discussions and comments. Any remaining errors are ours.

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1 Introduction

With almost 64,000 killings in 2017, Brazil is the world leader in homicides, and its murder rate is also one of the highest, with over 30 homicides per 100,000 inhabitants.[1] Organized crime is one of the driving factors explaining these figures. Indeed, according to the UNODC, the most dangerous countries in the world are countries of drug production or drug transit, and the role of Brazil in the international cocaine trade is growing. In particular, in Rio de Janeiro, police struggles to control the poorest districts of the city known as favelas (slums). The enforcement of law is very weak in these shantytowns. As these territories have been abandoned by the state, they are very violent and have been used for decades by drug gangs to hide from police and trade drugs. As a result, security is one of the major concerns among Brazilians and has been a leading theme of the last Brazilian presidential election. Therefore, it is a primary issue for governments to figure out how to fight organized crime and to understand the effects of policies fighting drug cartels.

This study investigates the effects of a unique policy implemented in Rio de Janeiro to pacify its favelas on several crime indicators. A pacification policy was initiated at the end of 2008 by the State of Rio de Janeiro to fight and chase out drug gangs from its favelas. It consisted in sending special police unit of the Military Police in a groups of favelas to crackdown on drug gangs and, just afterward, to install a new police station with a Pacifying Police Unit (UPP) to recover control over these territories. The implementation of the policy was progressive over time, due to limited capacity and funding. As of end of 2014, 37 UPPs were established within the city of Rio de Janeiro and encompassed 34% of the inhabitants living in favelas. In order to carry out our analysis, we exploit official data containing the monthly number of different categories of crime at the UPP-level from January 2007 to June 2016.

Measuring the causal impact of a policy fighting crime using official crime data rises at least two empirical issues. First, the pacified territories were presumably not chosen randomly. Increase enforcement usually targets areas with higher level of crime, generating positive correlation between crime and the device fighting it. As a matter of fact, the different favelas pacified over the period of interest might not be directly comparable to the favelas that were still not pacified when the policy was dropped, as they are mostly located in the eastern part of the city. Second, official crime data are often contaminated by measurement errors like the under-reporting of crime by victims. A problem arises when this measurement error becomes correlated with the treatment being evaluated. In Rio de Janeiro, the pacification of favelas is likely to have increased the reporting behavior of individuals, because they may have perceived that the likelihood of a crime being solved is higher, or they may fear less of being identified as someone who denounce a crime. Then, the estimated treatment effect could be positively biased. If the estimated effect is negative, then we just underestimate (in absolute value)

[1] For the sake of comparison, the murder rate in Europe is about 1 person per 100,000 inhabitants.

the true effect but we know that it is negative. This becomes more precarious when the estimated coefficient is positive. The true effect could be negative, but the increase in the reporting rate could be high enough to counterbalance it.

The empirical approach builds on the geographical and the time variations of the pacification of the favelas. The data contain information about the 37 areas that were pacified over the period 2007-2016. By working only on favelas that were pacified once, our identifying assumption involves that the timing of the pacification is exogenous to the unobserved factors explaining the crimes. Stated differently, we assume that the order in which favelas were pacified was random after controlling for fixed heterogeneity and a common time trend. This identifying assumption is much weaker than the one resulting from the direct comparison of pacified favelas to favelas that were never pacified, which would assume that pacified favelas were chosen randomly. Yet, it could be possible that the public authorities decided to pacify first the favelas with some specific trends in crime. In such a case, the usual diagnostic is to check whether the timing of the policy is correlated with (non-linear) trends in crime rates *before* the policy took place. The progressive roll-out of the policy provides variations across favelas and across times that permits to estimate the dynamic effects of the pacification. We estimate non-parametrically the presence of such pre-trends associated with the policy by using an event-study specification. The absence of any significant pre-treatment changes provides evidence supporting the identifying assumption.

We propose a new method to correct the bias resulting from the unobserved crime reporting rate change associated with the policy by using a proxy variable and by adding some structure to the empirical model. Namely, the number of reported accidents is affected by the policy only through the change in the reporting behavior induced by the policy, and therefore can be used as a proxy variable for the unobserved reporting rate of individuals. The key assumption underlying this method is that the time-varying part of the reporting rate of each crime indicator is affected in the same proportion by the treatment. Under this assumption, the treatment effect is point identified. To soften this assumption, we also apply bounded variation assumptions, in the spirit of Manski and Pepper [2000], to obtain the upper bound of the increase in the reporting rate that would lead to a reversal of the effect. The bias occurring from the endogenous reporting behavior of individuals has been discussed in Levitt [1998a] but, so far, no solution has been proposed besides the use of non-biased data like victim surveys (Soares [2004], Gibson and Kim [2008], Vollaard and Hamed [2012]).[2] However, such data are often not available to the researcher. We can assume that outcomes like murders are systematically reported and are not concerned by this bias. Therefore, we mainly apply our fix to other outcomes

[2] Chalfin and McCrary [2018] proposes a solution to the measurement errors of the number of police officers used in the literature on the effect of police on crime. However, they do not solve the more classic issue of the endogenous measurement errors of the reported crimes.

like assaults, thefts, or rapes that are likely to be affected by this positive bias.

The estimates indicate that, following the pacification policy, murder rate decreases by 7%, but assault rate increases by 51%. We also find that the number of robberies and of people killed by the police have dropped by 34% and 16%, respectively. Besides, threats have increased by 60%. Some effects are strongly affected by the use of the bias correction. With the fix, the rape rate and the theft rate are not significantly affected by the pacification, but we find significant increases in these variables when we do not fix the unobserved increase in the crime reporting rate. It is possible that the reporting rate of some crime indicators has not evolved similarly to the one of accidents. We show that some results are very robust to variation in their relative reporting rate evolutions by applying bounded variation assumptions. For instance, the reporting rate of assaults should have increased by 50% more than the one of accidents for the treatment effect to be no longer significant, and by 100% to be significantly negative at the 10% level. In summary, the policy has diminished the number of serious crimes (e.g., murders) but has increased the number of less serious crimes more (e.g., assaults), resulting in a rise in the total number of crimes inside the pacified areas by 38%. Our findings could explain why the policy was not necessarily well perceived by the inhabitants of the favelas (Jovchelovitch and Priego-Hernandez [2013], Magaloni et al. [2018b]).

These results may seem surprising, but they suggest that drug gangs provide a form of local order, which is expressed through social support and rigid social control where deviant behaviors of inhabitants are severely punished (Lessing [2012], Valle Menezes [2014]). Specifically, the underlying mechanism would be as follows. In the absence of the pacification policy, the drug gangs provide a governance system that prevents inhabitants of the favelas to commit crime. When the policy is implemented, some gang members are incapacitated and commit less murders. Then, increased enforcement leads to an absolute deterrence effect that prevent some inhabitants from committing felonies. However, the decrease of the drug gangs' governance effect releases some inhabitants who were before contained by the gangs from committing crime and may now engage in criminal activity. Finally, as the probability of being apprehended by the police increases, high-level crimes become relatively more costly, and inhabitants may also switch from high-level crimes (e.g., murders) to low-level crimes (e.g., assaults) due to a marginal deterrence effect. To identify the conditions that generate our empirical findings, we propose a theoretical model that accounts both for the absolute and marginal crime deterrence effects, and the fact that drug gangs secure the territories under their control.

To test the explanation of gang governance, we exploit a difference in the intensity of local order provided by the different gangs of Rio de Janeiro. We find that the pacification policy is more harmful in territories that were controlled by gangs which provides high level of local order, which is consistent with the fact that the gang's governance effect plays an important role. Furthermore, we conduct other robustness checks. The police could manipulate the official figures to artificially embellish the policy effectiveness, but we provide evidence consistent with the absence of obvious manipulations. Another concern would be that gang members that are chased out of some favelas may simply move to other unpacified favelas (controlled by the same gang or not). In that case, the control group would be also affected by the treatment (Miguel and Kremer [2004]). Using the pacification of two big favelas of Rio de Janeiro (*Cidade de Deus* and *Complexo Do Alemão*), we find no evidence of spillover effects *between* favelas, relieving us from this potential issue. However, we show that gang members from pacified favelas might have moved to the periphery of Rio de Janeiro. Finally, our last sets of results extend the analysis in two ways. First, we find evidence of positive spillovers on neighborhoods close to but *outside* favelas, i.e., a decrease of murders and robberies in areas at proximity of pacified favelas (less than 3 km away). Indeed, bloody territorial conflicts between gangs usually occurs at the border but outside the favelas. Moreover, gang members refrain from stealing in the favelas under their control and prefer to steal outside. Second, we analyze the heterogeneous effects of the pacification policy according to socio-economic characteristics of the favelas. Although this final analysis does not provide causal effects, the regressions control for many factors and document interesting results. In particular, homeownership and literacy seem to improve the efficiency of the policy.

The deployment of a massive number of heavily armed police officers over a long period of time in lawless areas is unprecedented, and its effects are mostly unaddressed in the crime literature. One article is closely related to ours. Magaloni et al. [2018a] delve deeply in the social context surrounding the pacification policy. In particular, they investigate the effect of the UPPs on lethal violence in Rio de Janeiro and find a significant decrease in the number of killings by the police, but no effect on the number of homicides. Similarly, Ferraz and Ottoni [2013] study the effects of the pacification policy on violent crimes (murders and police killings) up to 2012, and highlight a significant decrease in violent crime. These articles recognize that the effects of the policy on other crime indicators (assaults, rapes, thefts, or robberies) are affected by the unobserved change in the reporting behavior generated by the policy. Therefore, the authors do not draw a conclusion on crimes other than lethal violence.[3]

Our research complements empirical papers underlying the negative fallout resulting from policies combating crime. In particular, Dell [2015] examines the effect of a Mexican policy explicitly fighting drug gangs. She actually find that the crackdown against drug gangs had increased the number of homicides. Her result stems from the weakening of the major drug cartels, which fosters the emergence of numerous new competing drug gangs, a result that was theoretically shown in Mansour et al. [2006]. Gonzalez-Navarro [2013] identify negative geographical spillovers in the case of a device fighting car theft in Mexico, while Yang [2008] emphasizes a one-to-one displacement to alternative methods to

[3] Magaloni et al. [2018a] provide results on commercial burglary, arguing that this crime indicator is more likely to be well reported to the police. However, the reporting bias issue is still likely to remain, as inhabitants of the favelas, including those owning small stores, do not always own insurance policies.

commit duty-avoidance crime following the increase enforcement of customs in the Philippines.[4]

Previous studies using important increases in security presence focus on policies implemented in lawful states that were not especially designed against drug gangs (see Di Tella and Schargrodsky [2004], Klick and Tabarrok [2005] or Draca et al. [2011]). They all find that an increase in police presence in the streets results in a decrease in thefts, street crime, and violence. Other studies examining the effect of police on crime in the context of developed countries with strong institutions include Sherman and Weisburd [1995], Levitt [1997], Corman and Mocan [2000], Corman and Mocan [2005], Evans and Owens [2007], Machin and Marie [2011], Vollaard and Hamed [2012], and Chalfin and McCrary [2018]. They all converge to show that an increase in police force leads to a decrease in property crimes (theft, robbery, burglary), and some of them sometimes highlight a negative effect on violent crimes (murders, assaults). Finally, our paper is also linked to studies showing a negative effect of incarceration on crime such as Levitt [1996], Levitt [1998b], Levitt [1998c], Corman and Mocan [2000], and Corman and Mocan [2005].

The rest of this article is organized as follows. Section 2 provides some backgrounds on the pacification policy. Section 3 develops a theoretical model. Section 4 describes the data. Section 5 introduces the empirical strategy and the method correcting the reporting bias. Section 6 presents the main results and the robustness checks. Section 7 offers two extensions of the analysis. Finally, Section 8 concludes.

2 The Pacification of Favelas

The favelas of Rio de Janeiro. A favela (or slum) is an informal urban area with low service and infrastructure provision where low-income households live. Over decades of state neglect, a parallel power has evolved inside the slums of Rio de Janeiro, and communities have developed an array of social organizations, rules, and unorthodox solutions to access to basic services. Since the 1970's, with a large number of workers migrating from poorer states of Brazil to Rio de Janeiro, the number of favelas has considerably increased and, nowaday, nearly 20% of the population of Rio de Janeiro lives in favelas

In the mid-1980s, with the spread of cocaine use, drug trafficking became a highly profitable activity. As the main smuggling route moved south from the Caribbean to Latin America, Rio de Janeiro became a major transit hub for cocaine. Favelas characterized by weak state presence and well suited geographic conditions for military defense appeared as extremely desirable territories for drug gangs to set up business. Drug gangs progressively gained control over these marginalized communities

[4] Note that other studies find no (negative) spillovers of crime deterrence actions in the context of developed countries with strong institutions (see Ayres and Levitt [1998], or Grogger [2002]).

and enforced their own law to protect the favelas from infiltration by the police. The divisions between armed drug gangs fighting for business and territory control, coupled with the increasing sophistication of weapons used by gang members, naturally led to a dramatic escalation of violence: the homicide rate in Rio de Janeiro went from 30 per 100,000 in 1980 to 80 per 100,000 in 1989.

To combat this increase in urban violence, the military police, relying on specialized battalions trained in urban warfare, has engaged in frequent raids in the favelas. Since then, favelas' inhabitants are routinely caught in crossfire between the police and drug gangs. From 1994 to 2008, police killed more than 10,000 civilians, all supposedly armed opponents (Lessing [2012]). However, this strategy based on periodic raids has failed to recover these lost territories. In 2008 the majority of the city's favelas were controlled by criminal groups.

The pacification policy. In October 2007, Brazil was chosen to host the 2014 FIFA World Cup. The incapability of the previous policies to restrain urban violence combined with the concerns expressed by the international community about the public security in the surrounding of the World Cup have compelled the government to modify its strategy to fight drug cartels. With the support of the state governor, José Mariano Beltrame (the newly appointed state secretary for public security of Rio's state) proposed a new policy called the Unidade de Polícia Pacificadora (UPP). This policy is rooted in the principle that criminal operations heavily depend on territorial control of favelas. It aims at establishing state control and permanent police presence within the favelas.

The pacification policy involves three key steps. First, the state government announces in advance (without providing an exact date) a group of adjacent favelas to be pacified in order to warn criminals to leave the area and thereby reduce bloodshed. Then, the special police operations battalion (known as the BOPE), with the help from the military for the larger occupations, invades and occupies this group of favelas. They arrest or kill the gang members that did not escape, and search for hidden drugs and weapons. Finally, once it has been secured, a police station is set up and a community-based policing unit, composed entirely of new recruits who have received a special training in community policing and human rights, is permanently assigned to that pacified group of favelas. In some communities, social development programs calling for expanding access to decent sanitation, health care, and education (the so called "Social UPP") have been created. However, this social component was never fully implemented and did not succeed to bridge the gap of social inclusion.[5]

The UPP policy did not completely succeed in ending territorial control by criminal groups. One

[5]For instance, Ignacio Cano (Director of the Rio de Janeiro state university's Laboratory of Violence Analysis) presents the social part as a superficial embellishment that has not changed the life quality in the favelas (August 2014, https://www.insightcrime.org/news/analysis/rio-pacification-limits-upp-social/), while for Robert Muggah (research director of the Rio de Janeiro-based Igarape Institute) the social component of the UPP program was never really kicked in (August, 2017, https://www.insightcrime.org/news/analysis/what-latam-cities-can-learn-brazil-upp-policing-model/).

of the consequences was that, even if it has been undermined, drug trafficking has continued following the UPP installation. For instance, the state secretary for public security recognized that "The basic mission was to disarm the drug dealers and bring peace to the residents [...] I can't guarantee there is no drug dealing going on [...]" (Lessing [2012]). In 2015, Brazil entered recession and was gripped by a political crisis. A year later, the state of Rio de Janeiro was virtually left bankrupt and appealed for federal assistance. This triggered a gradual weakening of the UPP action, and criminal groups are now reclaiming some of the territories they have lost in recent years. In 2018, after months of escalating violence, the Federal government deployed soldiers on the ground and the military took control of public security in the state of Rio de Janeiro.

The criminal factions of Rio de Janeiro. Since the 1980's, three main drug gangs have fought each other for control of favelas: Comando Vermelho (Red Command), Terceiro Comando (Third Command) and Amigos dos Amigos (Friends of Friends). The Comando Vermelho (CV) was created in the 1970s as a self-protection group for prisoners. Originally formed as a left-wing militia organization, the group has then been involved in criminal activities. Starting out with low-level crimes like bank robberies and marijuana trading, it has diversified its activities into the more lucrative cocaine market when the cocaine trade began to boom in the 1980s. The Terceiro Comando (TC) was founded in the mid-1980s as the result of a split with the CV. Throughout most of the 1990s, the CV and the TC, were the dominant criminal actors in Rio de Janeiro. The Amigos dos Amigos (ADA) emerged in the end-1990s, as the result of a schism within CV. ADA expanded quickly in the early 2000s and by 2004 took over the CV's control of Rio's largest favela, Rocinha. Lastly, a more recent criminal group, the Terceiro Comando Puro (TCP), originated from a split with the Terceiro Comando in the 2000s.

In reaction to the prevalence of organized crime, another form of organization, called militias, have emerged in Rio de Janeiro in the 1990's with the aim of chasing out the drug gangs. Composed of active or retired police officers, prison guards, or firefighters, militias funded their activities through extortion rackets, charging protection fees, and trading goods and services like local transports, gas, electricity, or illegal television and Internet connections. Although initially opposed to drug trafficking, militias have progressively moved into drugs and other illegal business.

Criminal factions are all specialized in activities such as drug trafficking, murders, arms trafficking, robberies, extortion, kidnapping, prostitution, or human trafficking. However, they refrain from resorting to too much violence against innocent civilians living inside the favelas. The main victims of the gangs are criminals themselves, coming from their ongoing struggle to control territories and to punish those who betrayed them. Confrontations with the police are also often deadly.

Drug gangs impose themselves on their territory with different philosophies. CV is more known to exercise control over its territory with violence and to care less about security, while ADA favors accommodation with the police and corruption over confrontation. ADA also wields significant social power in the communities it controls, working to gain support from residents by providing services, distributing gifts, organizing cultural events, and providing protection and security to them.[6]

3 A model of criminal faction governance and community cooperation

Drug gangs often seek community cooperation to guarantee the safety and profitability of their illicit business. In particular, to not scare customers and to avoid the police from entering their territory, the absence of other felonies is required to properly operate their activity. A combination of intimidation and service provision strategies are used to obtain the community cooperation (Magaloni et al. [2018a]). By severely punishing those who do not respect the criminal factions' rules, intimidation acts as a substitute of the official judicial system. Intimidation provides a form of local order and discourages inhabitants from committing crime. Service provision strategies can take the form of punctual assistance, provision of welfare services, protection or collaboration rewards offered to the inhabitants. In this setting, we focus on this second form of governance by assuming that the criminal faction provides social support to discourage the inhabitants to commit felonies.

We consider a territory controlled by a criminal faction with a unit mass of homogeneous risk neutral inhabitants. The inhabitants allocate their time between two wealth generating activities and spend a time t_k in activity a_k with $k \in \{1, 2\}$. Activity a_1 does not penalize the gang and can be considered as a legal activity or as a direct cooperation with the criminal faction.[7] The rate of return of activity a_1 is r. Activity a_2 consists of committing felonies. The total number of crimes committed by inhabitants (denoted by N) increases and is concave with the time devoted to activity a_2, $N^{'}_{t_2} > 0$ and $N^{''}_{t_2} < 0$. When they commit a crime, inhabitants choose between a low-level and a high-level crime. We denote by p the degree of enforcement (which is the probability of capture by the police) and by F_i the penalty (from a justice court) for a crime of type $i \in \{l, h\}$, where h and l stand for high-level and low-level, respectively. A high-level crime being more sanctioned by the official law, $F_h > F_l$. Let v be the gross benefit gained from a crime and x be the relative preference of the inhabitants for each type of crime. For each crime committed, a value of x is randomly drawn from an uniformly distribution over $[0, 1]$. Given x, the expected benefit of a crime of type i is $v - |x_i - x| - pF_i$ where $x_l = 0$ and $x_h = 1$. We implicitly assume that the gross benefit v does not depend on the type of crime.[8]

[6] https://www.insightcrime.org/brazil-organized-crime-news/amigos-dos-amigos/

[7] An example of cooperation can be informing drug traffickers of whom enters or leaves the favela.

[8] For instance, consider the criminal act of stealing a car: v will be the value of the car, crime types l and h will correspond to theft and robbery, respectively. High-level crime are not necessarily more profitable than low-level crime, e.g., a car robbery does not generate more money than a car theft.

The criminal faction invests α to socially support the inhabitants, which positively affects the rate of return of activity a_1. This suggests that the inhabitants are rewarded for behaving according to the criminal faction's rules. The profit of the criminal faction is $\pi_g = R(p, N) - \alpha$, with R the revenue of its drug-trafficking activity. We assume that the degree of enforcement - through an incapacitation effect - negatively affect the criminal faction's revenue ($R_p^{'} < 0$). Finally, the number of crimes negatively affects the criminal faction's revenue ($R_N^{'} < 0$). We consider the following timing: i. the gang chooses its level of social support α to the inhabitants; ii. inhabitants select their time allocation between activity a_1 and a_2 (which determines the total number of crimes committed); iii. for each crime committed, inhabitants select either a low-level or high-level crime.

3.1 Equilibrium

We solve the game by backward induction.

Stage iii. Given that they undertake a crime, inhabitants chooses a crime of type i if $v - x - pF_i \geq v - (1-x) - pF_j$ for $i \neq j \in \{l, h\}$.[9] Let q_i be the probability of committing a crime i. In equilibrium, $q_l = \frac{p\Delta_F + 1}{2}$ and $q_h = 1 - q_l$ with $\Delta_F = F_h - F_l$. The expected utility of committing a crime is

$$U(p) = \int_0^{q_l} v - x - pF_l dx + \int_{q_l}^1 v - (1-x) - pF_h dx$$

The expected utility of committing a crime decreases with p since criminal activity becomes globally more costly. This absolute deterrence effect is very standard and is examined for instance in the seminal paper of Becker [1968]. However, as p increases, the inhabitants substitute low-level to high-level crimes since high-level crimes become relatively more costly. This mechanism closely matches the marginal deterrence effect that is discussed in Mookherjee and Png [1994].

Stage ii. The inhabitants choose their time allocation between t_1 and t_2 under the constraint that $t_1 + t_2 \leq \bar{t}$. The time allocated to the criminal activity is determined by

$$\max_{t_2} \quad (\bar{t} - t_2) \times r(\alpha) + N(t_2) \times U(p)$$

Let t_2^+ be the equilibrium amount of time that the inhabitants allocate to the criminal activity given α and p. The first order condition is such that[10]

$$N_{t_2}^{'}\left(t_2^+\right) = \frac{r(\alpha)}{U(p)}.$$

[9] We assume internal solutions exist. This is satisfied if $p\Delta_F$ is not too large.

[10] The second order condition is satisfied since $N_{t_2}^{''} < 0$.

Since r increases in α, the time allocated to the criminal activity decreases with α and the inhabitants commit less crimes when the criminal faction increases its level of social support. Therefore, by increasing the social support, the gang raises the security level in the territory under its control.

Stage i. The gang chooses its level of social support α in order to maximize π_g. Let α^* be the equilibrium investment level of the criminal faction given p. We assume that the second-order condition maximization with respect to α holds. The first-order condition is such that

$$R_N^{'}\left(p, N\left(t_2^+\left(\alpha^*\right)\right)\right) \times N_{t_2}^{'}\left(t_2^+\left(\alpha^*\right)\right) \times \frac{\partial t_2^+\left(\alpha^*\right)}{\partial \alpha} = 1.$$

It is not straightforward whether the level of investment increases or decreases with the degree of enforcement. The level of investment is more likely to decrease with p if $\frac{\partial^2 R}{\partial p \partial N} > 0$ and $\frac{\partial^2 R}{\partial N^2} < 0$.[11] For the rest of the model, we assume that the parameters are such that when the degree of enforcement increases, the gang invests less in the territory. Namely, it is less profitable for drug gangs to provide social support when the police is more present on the territory.

3.2 Degree of enforcement and variation in the number of both types of crime

We study how an increase in law enforcement affects the criminal behavior of the inhabitants and the number of both types of crime. The increased enforcement consists in an increase in the probability of crime detection by the police. Let n_i be the number of crimes of type i: $n_i\left(\alpha^*, p\right) = N\left(t_2^+\left(\alpha^*, p\right)\right) \times q_i\left(p\right)$ with $i \in \{l, h\}$. The effect of p on the number of crimes of type i can be decomposed as follows

$$\frac{dn_i\left(\alpha^*, p\right)}{dp} = \underbrace{\frac{\partial t_2^+\left(\alpha^*, p\right)}{\partial p} \times N_{t_2}'\left(t_2^+\left(\alpha^*, p\right)\right) \times q_i\left(p\right)}_{ADE} +$$

$$\underbrace{\frac{\partial t_2^+\left(\alpha^*, p\right)}{\partial \alpha} \times \frac{d\alpha^*}{dp} \times N_{t_2}'\left(t_2^+\left(\alpha^*, p\right)\right) \times q_i\left(p\right)}_{GGE} + \underbrace{\frac{dq_i\left(p\right)}{dp} \times N\left(t_2^+\left(\alpha^*, p\right)\right)}_{MDE_i} \quad (1)$$

with $i \in \{l, h\}$. Expression (1) is decomposed in three terms: an absolute deterrence effect (ADE), a gang's governance effect (GGE) and a marginal deterrence effect (MDE_i). The two first terms represent the effect of p on the time allocated by the inhabitants to the criminal activity and, therefore,

[11] $\frac{\partial^2 R}{\partial p \partial N} > 0$ means that following a marginal increase in the number of fellonies, the gang's revenue decreases more if the degree of enforcement is low.

on the total number of committed crimes. An increase in p negatively affects the expected utility of an illegal action which discourages the inhabitants to engage in criminal activity ($ADE < 0$). However, it also affects negatively the gang's investment incentives. As the gang's governance effect disappears, the relative rate of return of criminal activity increases ($GGE > 0$). The total number of crimes N increases with the degree of enforcement only if the gang's governance effect is stronger than the absolute deterrence effect (in absolute value). The term MDE_i represents (for a given number of crimes) how the inhabitants substitute low-level to high-level crimes when p increases. The term MDE_i is positive if $i = l$ and negative otherwise. We can state the following proposition.

Proposition 1. *When the degree of enforcement increases*

- *both types of crime decrease if* $ADE + GGE < -MDE_l$
- *low-level crime increases and high-level crime decreases if* $-MDE_l \leq ADE + GGE \leq -MDE_h$
- *both types of crime increase if* $ADE + GGE > -MDE_h$

Proof 1. *This stems from expression (1) and the fact that* $MDE_l > 0$ *and* $MDE_h < 0$.

Proposition 1 shows that when the absolute deterrence effect is sufficiently strong compared to the gang's governance effect, an increase in the degree of enforcement encourages the inhabitants to reduce substantially their time spent on criminal activity. Even if the inhabitants substitute low-level to high-level crimes (due to the marginal deterrence effect), both types of crime decrease since they allocate considerably much less time to the criminal activity. On the other hand, when the absolute deterrence effect is sufficiently weak compared to the gang's governance effect, the reverse holds and both types of crime increase. Finally, when the absolute deterrence and the gang's governance effects almost cancel each other out, the variation in the time allocated by the inhabitants to the criminal activity is of a small amplitude. In this case, the number of low-level crimes increases and the number of high-level crimes decreases.

3.3 Governance efficiency and Pacification.

Hereafter, we study how the pacification policy outcome is affected when the gang becomes more efficient to incentivize the inhabitants to commit less felonies. The profit of the criminal faction is now written as $\pi_g = R(p, N) - \frac{\alpha}{e}$, with e the degree of governance efficiency of the gang.[12] We refer to *Pacification* as a situation where the degree of enforcement is high enough such that the gang does not

[12] Alternatively, we could have assumed that the marginal effect of the social support on the rate of return of activity a_1 is positively affected by the degree of governance efficiency of the gang. Both types of crime would decrease with e and the conclusion will be unchanged. However, it will be less straightforward to define whether the investment level increases or decreases with e.

invest anymore in the territory. Prior to pacification, the gang's investment increases with the degree of governance efficiency, decreasing both types of crime. We can state the following Proposition.

Proposition 2. *The pacification policy is less likely to be beneficial if the degree of governance efficiency is high.*

Proof 2. *Let $\hat{p}$ be the degree of enforcement before pacification and $\bar{p}$ the degree of enforcement after pacification. The number of crimes of type i is $n_i\left(\alpha^*\left(\hat{p},e\right),\hat{p}\right)$ before pacification and $n_i\left(0,\bar{p}\right)$ after pacification. Since $\alpha^*\left(\hat{p},e\right)$ increases in e and n_i decreases in α^*, $n_i\left(\alpha^*\left(\hat{p},e\right),\hat{p}\right)$ decreases in e. Let $\Delta_i^P\left(e\right)=n_i\left(0,\bar{p}\right)-n_i\left(\alpha^*\left(\hat{p},e\right),\hat{p}\right)$ be the effect of pacification on the number of crimes of type i. The pacification is harmful if $\Delta_i^P\left(e\right)>0$. Since $\Delta_i^P\left(e\right)$ increases in e, $\Delta_i^P\left(e\right)$ is more likely to be positive if e increases.*

Assume that a territory can be either under the control of a low-efficiency or a high-efficiency gang. Proposition 2 implies that if crime i increases following the pacification in a territory controlled by a low-efficiency gang, it will increase more in a territory controlled by a high-efficiency gang. On the other hand, if crime i decreases following the pacification in a territory controlled by a low-efficiency gang, it will either less decrease or increase in a territory controlled by a high-efficiency gang.

This model abstracts from intimidation, the other form of governance to incentivize inhabitants to behave according to the criminal faction's rules. Instead, it focuses on the social support, a form of governance provided by gangs to discourage the inhabitants to commit felonies. In practice, intimidation and service provision are often difficult to disentangle, as two sides of the same coin. So modeling one side is somehow equivalent to modeling the other one. Specifically, a model focusing on intimidation would produce similar results if we assumed that low level crime are relatively more punished by the gang than by official justice, which seems plausible.

The model does not account for the crimes committed by the gang. Including these crimes would not change the conclusions of the model. Increased enforcement will undermine the strength and the size of the gang, and thus its criminal activity through an incapacitation effect. It will also encourage the gang members that have not been chased out, arrested or killed to reduce their criminal activity and to substitute low-level crimes to high-level crimes. Overall, the number of high-level crimes committed by the gang will decrease, while low-level crimes could either increase or decrease. The number of low-level crimes committed by the gang will increase only if the marginal deterrence effect is sufficiently strong compared to the absolute deterrence effect and the incapacitation effect. These results are along the same lines than the ones highlighted in our model.

Finally, we do not consider the possibility for the territory to be contested by several gangs. In such a case, the rate of return of activity a_1 will depend on the total level of investment realized by the different gangs. As the number of gangs competing for the territory will increase, the private motive to invest in financial supports and the rate of return of activity a_1 will decrease, which reflects the classic investment problem in a public good. Therefore, the pacification policy is more likely to be beneficial if the territory is contested by different criminal groups rather than monopolized by an unique gang.

4 Data and Descriptive Statistics

We use official information from the Instituto de Seguranca Pública (ISP), which is the institute in charge of recording criminality data in the state of Rio de Janeiro. The data cover the period between January 2007 and June 2016. During this period, 37 UPPs were established in favelas. Note that an additional UPP was also installed in the favela named Mangueirinha, which is located in Duque de Caxias another city of the state of Rio de Janeiro. We do not consider this UPP in this study.

ISP provides various monthly indicators of several crimes at the UPP level. To avoid presenting too many indicators and to deal with some categories of crime that rarely occur, we aggregate some indicators into broader categories so that we study the ten following categories of crime: Police Action (it corresponds to the sum of drug seizures, weapon seizures, arrests with a warrant, car recoveries, and arrests in flagrante delicto), Police Kill (number of people who got killed by the police), Murder (intentional homicides, assaults resulting in death, and robberies ending in death), Assault (assaults with body injury not resulting in death, and attempted murders), Rape, Robbery, Theft, Extortion (all forms of extortion including those with momentary kidnapping), Threat, and Total Event (the total number of events registered, which roughly encompasses all the previously defined indicators). Note that the data also contain information about the number of accidents (fatal and non-fatal ones).[13]

[13]Accidents correspond to traffic events such as vehicle collision, car running over a pedestrian, collision with a fixed point, etc. Besides, another way to classify crime would be to consider the intention of the criminal rather than the result of his actions. Murder would include intentional homicides and attempted murders, while Assault would contain assaults with body injury not resulting in death as well as assaults resulting in death. Since the pacification policy has a very weak impact on assaults resulting in death and attempted murders, the results obtained with this alternative definition are similar to those obtained with the former definition used in the paper.

Table 1: Annual average value (for 100 000 inhabitants) of main crime indicators before and after pacification

	Before pacification 2007-2008	After pacification 2015-2016
Police Action	386.5	775.0
Police Kill	28.3	8.5
Murder	22.8	16.9
Assault	236.5	572.6
Rape	8.8	18.5
Robbery	370.2	166.8
Theft	247.9	262.3
Threat	137.5	263.2
Extortion	4.0	5.1
Accident	52.3	80.3
Total Event	1589.2	2464.3

Table 1 presents the mean values of the different crime variables before and after the pacification. It suggests that the policy is correlated with a decrease in the number of serious events (murders, police killings, robberies) but with an increase in the number of less serious crimes (assaults, thefts, threats). Police actions and the number of accidents have also considerably grown. Overall, the total number of crimes appears to have surged over the study period. These descriptive statistics are simple correlations and require further investigations to establish a causal link between the pacification policy and crime indicators.

We use the Internet portal data.rio, which is an open data access of the municipality of Rio de Janeiro, to specifically determine population and socioeconomic indicators at the UPP level. This portal provides socioeconomic data from the census 2010 realized by the Brazilian Institute of Geography and Statistics, and location shape files at the census tract level, which is roughly equivalent to a city block. This portal also provides location shape files of the areas covered by each UPP. The main difficulty to match these shape files comes from the fact that a census tract can lie partially inside and partially outside an area covered by UPPs. To overcome this issue, we refined our data by creating a grid of 143,990 points, each of them belonging to one census tract. On average, there are 14 points in a census tract. We then inferred population and socioeconomic indicators for each of these points. For instance, if a census tract with P points has N inhabitants and $X\%$ of homeowner, we consider that each of these P points has a population of $\frac{N}{P}$ inhabitants and $X\%$ of homeowner. We then matched these 143,990 points with the UPP shape files and determined population and socioeconomic indicators at the UPP level. Finally, the portal data.rio also provides location shape files of the areas covered by the favelas. Following the same procedure, we defined population and socioeconomic indicators at the favela level.

The data provided by the portal data.rio only allow us to estimate the population of the UPPs in 2010. We use two other sources of information to estimate the evolution of the population. First, IPP [2012] provides an estimation of the population evolution between 2000 and 2010 covered by the 28 first UPPs pacified. Second, data.rio provides the population at the neighborhood level in 2000 and 2010. There are 158 neighborhoods in Rio de Janeiro. Typically, a neighborhood is bigger than an UPP. We matched the grids of points of the nine remaining UPPs (with no information about the population evolution) with the neighborhood shape files, and used it to infer the population of these UPPs. By linearly interpolating the population from 2000 and 2010, we estimate a monthly evolution rate that we extend to 2016 for each UPP. Our results are not sensitive to the population evolution and remain unchanged if we consider a constant population over time.

Table 2 reports the timing of the intervention that are provided by the Instituto de Seguranca Pública, and some other descriptive statistics about the UPPs. Started at the end of 2008, the pacification dates of the favelas are distributed relatively evenly over time. This table also highlights the heterogeneity in the number of new police officers deployed and the size of the population under their juridiction.

Table 2: Descriptive statistics of UPPs

UPP	Date of Bope Intervention	Date of UPP installation	Number of Police Officers	Population
SantaMarta	19/11/08	19/12/08	123	4139
Batan	12/07/08	18/02/09	107	22176
Cdd	11/11/08	16/02/09	343	44515
ChapeuMangueiraEBabilonia	11/05/09	10/06/09	107	3914
PavaoPavaozinho	30/11/09	23/12/09	189	14062
Tabajaras	26/12/09	14/01/10	144	8719
Providencia	22/03/10	26/04/10	209	14765
Borel	28/04/10	07/06/10	287	15707
Andarai	11/06/10	28/07/10	219	14318
Formiga	28/04/10	01/07/10	111	5036
Salgueiro	30/07/10	17/09/10	140	4131
Turano	10/08/10	30/10/10	173	14072
Macacos	14/10/10	30/11/10	221	23341
SaoJoaoQuietoMatriz	06/01/11	31/01/11	208	9748
CoroaFalletFogueteiro	06/01/11	25/02/11	193	14222
EscondidinhoEPrazeres	06/01/11	25/02/11	182	9335
Mangueira	19/06/11	03/11/11	332	17157
SaoCarlos	06/01/11	17/05/11	244	22462
Vidigal	13/12/11	18/01/12	246	12452
Fazendinha	28/11/10	18/04/12	314	22454
NovaBrasilia	28/11/10	18/04/12	340	33803
AdeusBaiana	28/11/10	11/05/12	245	10606
Alemao	28/11/10	30/05/12	320	16071
Chatuba	27/06/12	27/06/12	230	11940
FeSereno	27/06/12	27/06/12	170	5672
ParqueProletario	28/11/10	28/08/12	220	17239
VilaCruzeiro	28/11/10	28/08/12	300	19344
Rocinha	13/12/11	20/09/12	700	71143
Jacarezinho	14/10/12	16/01/13	543	41903
Manguinhos	14/10/12	16/01/13	588	24541
AraraMandela	13/10/12	06/09/13	273	18225
BarreiraVascoTuiuti	03/03/13	12/04/13	150	17040
Caju	03/03/13	12/04/13	350	19411
CerroCora	29/04/13	03/06/13	232	3073
CamaristaMeier	06/10/13	02/12/13	230	15290
Lins	06/10/13	02/12/13	250	14196
VilaKennedy	13/03/14	23/05/14	250	40606

Numbers of police officers which were assigned to each UPP after the intervention come from http://www.upprj.com/. The dates of Bope intervention and UPP installation are from the Instituto de Seguranca Pública.

Table 3 shows descriptive statistics on various socioeconomic characteristics of households located inside or outside favelas, and according to whether they live in areas that were pacified or not.

Table 3: Mean of socioeconomic characteristics across census tracts for different types of location

	Favela	Non-Favela	Favela		Non-Favela	
			UPP	No UPP	UPP	No UPP
	(1)	(2)	(3)	(4)	(5)	(6)
Income per capita	390.22	1371.39	379.54	395.63	625.03	1406.12
Size of household	3.26	2.85	3.31	3.23	3.08	2.84
Homeowner (%)	76.16	72.37	76.41	76.04	72.66	72.35
Households electricity (%)	77.36	96.24	73.65	79.2	88.29	96.58
Households water (%)	96.54	98.96	97.18	96.23	99.14	98.95
Illiterate +15 years old (%)	6.45	1.92	6.52	6.42	3.71	1.85

Columns (1)-(2) in Table 3 show that the residents of favelas are significantly poorer and more destitute in terms of service and infrastructure access than inhabitants of other areas of the city. Inside the favelas, households located in regions that were pacified are slightly poorer and have a lower access to electricity than those that live in areas which were not pacified (columns (3)-(4)).

Table 4: Correlation between socioeconomic characteristics across UPPs

	Homeowners (%)	Literacy (%)	Young (%)	Water (%)	Electricity (%)	Garbage (%)	Income (level)
Homeowners	1.0000						
Literacy	-0.3328	1.0000					
Young	0.0185	-0.1072	1.0000				
Water	-0.2443	0.0457	0.0358	1.0000			
Electricity	-0.1471	0.3428	0.1815	-0.1306	1.0000		
Garbage	-0.1411	0.2737	0.1807	0.2024	0.2294	1.0000	
Income	-0.4528	0.0340	-0.1184	0.0898	0.0326	0.2264	1.0000

Table 4 highlights the absence of important correlation between the observable socioeconomic variables that characterize the UPPs. In particular, the percentage of homeowners is negatively correlated with the average income per household, which might be explained by public housing and homeownership programs that were implemented in the past by the state.

Finally, we plot the characteristics of the UPPs as a function of the date of pacification in Figures 6-7 of Appendix A. Overall, these graphs support the absence of an obvious order in the pacification date of favelas according to their characteristics.

5 Empirical strategy

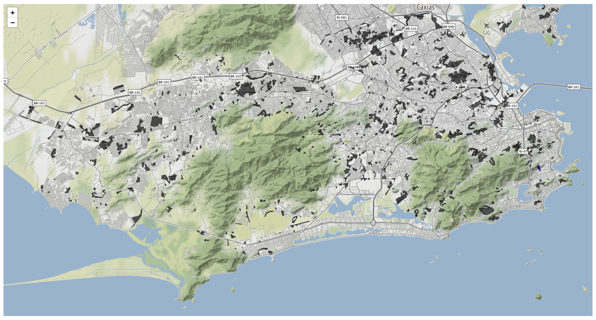
This paper builds on geographical and time variations of the pacification of Rio's favelas. Limited capacity and limited funding imply that the policy adoption was staggered over time. Our identification relies on the progressive roll-out of this policy in the different favelas of Rio de Janeiro. In this setting, it is almost possible to compare pacified favelas to non-pacified favelas at any point of time.

A naive approach would consist in comparing directly the pacified favelas to the non-pacified favelas regarless of the type of the favela, in a difference-in-differences setting, to estimate the effects of the pacification. In that case, the identifying assumption would be that the choice of the favelas to be pacified was random after controlling for the fixed heterogeneity in the crime level of favelas and for the common evolution of a given crime. However, when the policy was stopped, the majority of the favelas were still not pacified. When looking at the geographical distribution of the pacified and non-pacified favelas in Rio in 2016, it clearly appears that most of the favelas located in the western part of the city were not pacified. Therefore, it is possible that the favelas pacified over the period 2007-2016 are not directly comparable to the favelas that were still not pacified when the expansion of policy was stopped.

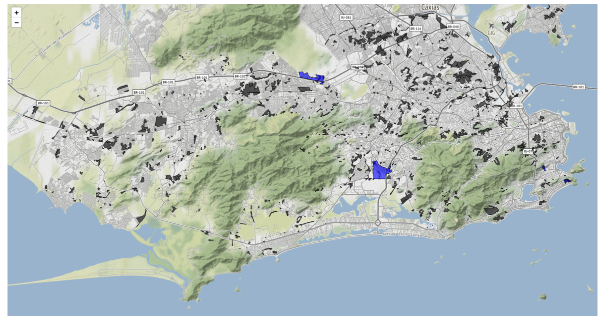
As we do not observe crime in favelas that were never pacified, we adopt another approach and we restrict our analysis solely to favelas that were pacified over the period 2007-2016. With this approach, the identifying assumption implies that the timing of the pacification is exogenous to the unobserved factors explaining the crimes. Stated differently, we assume that the order in which favelas were pacified was random after controlling for fixed effects and common time trends, in the set of favelas that were treated at the end of the policy. Thus, the identifying assumption is much weaker and, therefore, much more convincing. To support this assumption, we first show in Figure 1 that the geographical localization of pacified favelas over time, in the set of treated favelas, is apparently random and does not follow a specific pattern. We also show latter in the paper that the timing of the pacification is uncorrelated with specific pre-treatment crime dynamics and observable socio-demographic characteristics of favelas. This set of evidence supports that the pacification order of these favelas was probably not correlated with unobserved determinants of crime and, therefore, the estimated treatment effects are likely to be causal.

We use the crime rate (i.e., the crime level in proportion to the population) as the outcome variable to account for the heterogeneity of favelas in term of their population size.[14] We observe

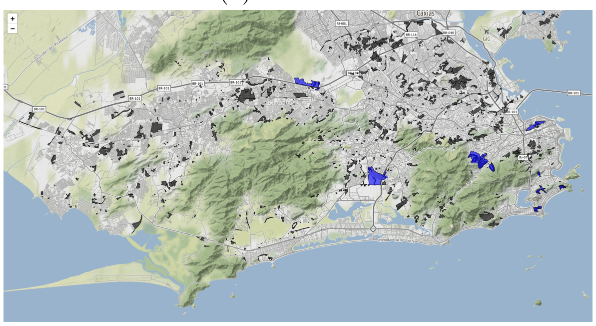
[14] Some favelas are highly populated while some others are much less populated. Then, it is more likely that the number of crimes will be higher in more populated favelas. Using the crime rate as the dependent variable



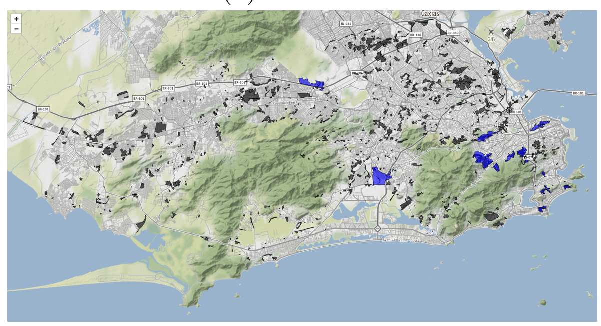
(a) June 2008



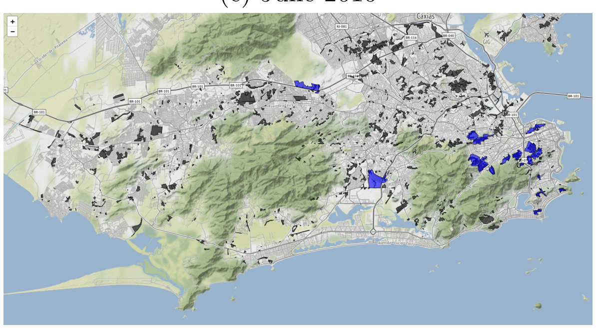
(b) June 2009



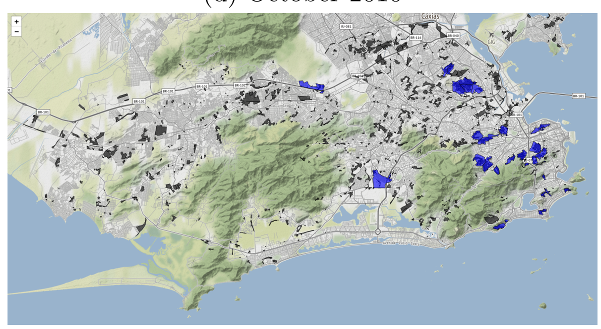
(c) June 2010



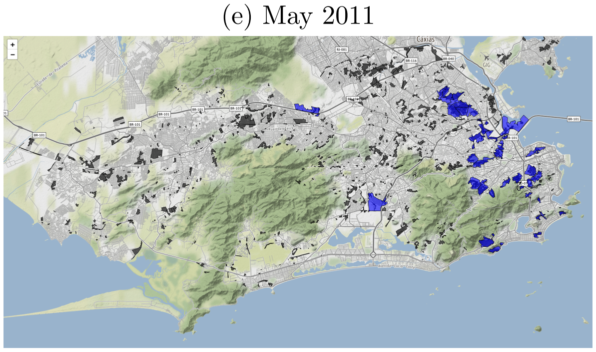
(d) October 2010



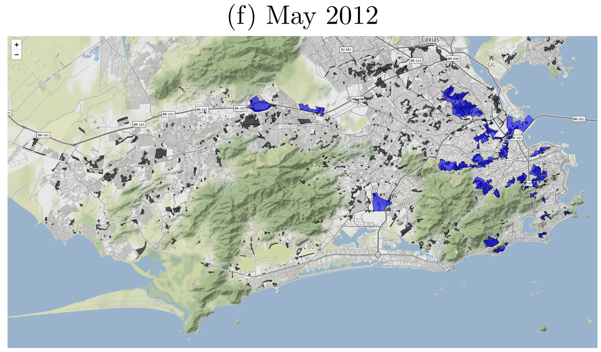
(e) May 2011



(f) May 2012



(g) Jan 2013



(h) December 2013

Figure 1: Geographic distribution of UPPs over time (favelas are in dark grey, and UPPs in blue)

crime indicators at the UPP level, which corresponds to a group of adjacent favelas that are under the administrative authority of a given UPP. We suppose that $\frac{crime^C_{i,t}}{population_{i,t}}$, the rate of crime of category C (e.g., murders, robberies, etc.) in UPP i during month t, follows an exponential model. To simplify notations, we will denote the rate of crime as $Crime^C_{i,t}$, with a capital C, in the rest of the paper. Therefore, the model we would like to estimate is the following:

$$Crime^C_{i,t} = \exp\left(\beta Pacified_{i,t} + X'_{i,t}\theta + \alpha_i + \epsilon_{i,t}\right) \tag{2}$$

where $Pacified_{i,t}$ indicates whether the territory of UPP i is pacified in month t. In this regression, β is the coefficient of interest. It captures the average effect of the pacification on the crime rate. $X_{i,t}$ collects all observed exogenous variables and contains at least a set of common time period indicators and the variable $Intervention_{i,t}$, which indicates that the police special task force (BOPE) is pacifying the territory i during month t. Controlling for the effect of the intervention is important as it might influence the occurrence of crime *before* a favela is actually pacified, which avoids to underestimate the effect of the pacification. The time fixed-effect non-parametrically captures the evolution of crime C that is common to all areas pacified over the period. α_i denotes an UPP fixed-effect that captures the fixed unobserved heterogeneity, like the fact that a specific area is more violent than others, with respect to the population. Finally, $\epsilon_{i,t}$ represents the error term, assumed as independent from other explanatory variables. The exponential model implies that the effect of the pacification over the crime rate is multiplicative. This assumption is more plausible than a pure additive (linear) model, because the favelas are heterogenous in the intensity of crimes. Therefore, it seems unlikely that the pacification results in a common increase (decrease) that is simply added (substracted) in absolute value to the crime rate of each area.

The true amount of crime is generally unobserved. Observed crime data can usually be classified in two groups. First, official crime data arise from individuals (say, the victims of crimes) reporting a crime by going to a police station, where policemen then record it. Second, some surveys allow individuals to declare crime, more discreetly and anonymously. In this paper, we exploit official crime data that were reported to the police. Such data have the advantage of being systematically collected over time, they contain information about almost all categories of crime, and they are similar to other official crime data that are usually available and used by researchers. Yet, official data also have some drawbacks. People may be afraid of officially complaining about a crime and might not want to be seen entering a police station. Also, they must trust the institutions so that they believe a crime will be solved and that the justice can protect them.[15]

directly accounts for this heterogeneity.

[15]In Rio de Janeiro, an ONG operates an anonymous reporting crime system called *Disque Denúncia*. It is implemented through the use of an anonymous phone line or website. Data from *Disque Denúncia* should be

As we do not observe realized crime levels but only reported crime levels, we had some structure to the empirical model and we postulate the following relation:

$$Crime_{i,t}^{C,R} = Crime_{i,t}^{C} \times RR_{i,t}^{C} \tag{3}$$

where $Crime_{i,t}^{C,R}$ stands for the reported level of crime of category C in UPP i during period t, and $RR_{i,t}^{C}$ represents the reporting rate of the crime of category C in UPP i during period t. Using this relation and equation (2), it is immediate to obtain the specification that we would like to estimate:

$$\ln\left(Crime_{i,t}^{C,R}\right) = \beta Pacified_{i,t} + X_{i,t}'\theta + \alpha_i + \ln\left(RR_{i,t}^{C}\right) + \epsilon_{i,t}$$

We further postulate that the reporting rate $RR_{i,t}^{C}$ can be multiplicatively decomposed into three components: the first one is specific to one category of crime, to one UPP, and is constant over time; the second one captures the common time trend across UPPs of the reporting rate of one category of crime; the third one is specific to a given UPP and varies over time but is common between crimes. That is, we have $\ln(RR_{i,t}^{C}) = \ln(RR_{i}^{C}) + \ln(RR_{t}^{C}) + \ln(RR_{i,t})$. The term $\ln(RR_{i}^{C})$ is constant within an UPP, so it is simply absorbed by the inclusion of an UPP fixed-effect, and $\ln(RR_{t}^{C})$ is absorbed by the common time period indicator.[16] Therefore, we get the following specification:

$$\ln\left(Crime_{i,t}^{C,R}\right) = \beta Pacified_{i,t} + X_{i,t}'\theta + \alpha_i + \ln\left(RR_{i,t}\right) + \epsilon_{i,t} \tag{4}$$

Assuming that $\epsilon_{i,t}$ is independent from other independent variables, we should obtain the causal effect of the pacification on the crime rate. However, the reporting rate is unobserved. This is not an issue as long as this unobserved variable is not correlated with other explanatory variables. The problem is that the propensity to report a crime is likely to be correlated with the treatment. Indeed, people are more likely to file a complaint to police when a favela is pacified because they are less afraid of reporting a crime, or because they trust more the institutions. Then, there is an endogeneity issue, and the estimated β is biased if we do not account for it.[17] We propose a solution that relies on the use of a proxy variable, and the addition of simple structure to the empirical model.

less affected by the behaviors leading to the under-reporting of crime, because the time cost to report a crime is much lower and because it is anonymous. However, the concerns regarding the lack of trust are still likely to apply, so that these data are still likely to be affected by the under-reporting concern.

[16] For ease of notation, we do not change the notation of θ and α_i coefficients, but they are different since they now include some components of the reporting rate.

[17] Formally, we do not observe $RR_{i,t}$, and equation (4) writes $\ln\left(Crime_{i,t}^{C,R}\right) = \beta Pacified_{i,t} + X_{i,t}'\theta + \alpha_i + \xi_{i,t}$, where $\xi_{i,t} = \ln(RR_{i,t}) + \epsilon_{i,t}$. Because $\ln(RR_{i,t})$ is likely to be positively correlated with $Pacified_{i,t}$, $Pacified_{i,t}$ is potentially endogenous and we might over-estimate β. Note that the UPP specific time-varying part of the reporting rate, $RR_{i,t}$, is uncorrelated to all explanatory variables but the treatment one, as if it was equal to zero before the policy and it took positive values after the policy.

A solution to handle the bias. The idea is to find a variable that is affected by the change in the reporting behavior but not by the treatment. We have shown in Section 4 that the reported accident rate has increased over the study period. The number of reported accidents probably reacts to the change in the propensity to report events following the pacification, but it should not be directly affected by the pacification itself. Formally, we assume that $Accident_{i,t} = \exp(X'_{i,t}\lambda + d_i + u_{i,t})$ and that $Accident^R_{i,t} = Accident_{i,t} \times RR^A_{i,t}$, with $RR^A_{i,t} = RR^A_i \times RR^A_t \times RR_{i,t}$, where $RR^A_{i,t}$ represents the reporting rate of accidents. It implies that the part of the reporting rate that varies over time (i.e., $RR_{i,t}$) is identical for all categories of events (including all categories of crime and accidents). We obtain the following equation:

$$\ln\left(Accident^R_{i,t}\right) = X'_{i,t}\lambda + d_i + \ln\left(RR_{i,t}\right) + u_{i,t} \tag{5}$$

We also further assume that $E[Pacified_{i,t}u_{i,t}] = 0$, i.e., the treatment and $u_{i,t}$ are independent. In other words, $Accident^R_{i,t}$ is correlated to the treatment (the pacification) only through $RR_{i,t}$, the reporting rate. The policy has otherwise not direct effect on the number of accidents.

By inverting equation (5), we obtain an expression of $\ln(RR_{i,t})$ as a function of $\ln\left(Accident^R_{i,t}\right)$, and by substituting this expression into equation (4), we get:

$$\ln\left(Crime^{C,R}_{i,t}\right) - \ln\left(Accident^R_{i,t}\right) = \beta Pacified_{i,t} + X'_{i,t}(\theta - \lambda) + (\alpha_i - d_i) + (\epsilon_{i,t} - u_{i,t}) \tag{6}$$

Because we assumed that $E[Pacified_{i,t}u_{i,t}] = 0$, $Pacified_{i,t}$ is not correlated with the new residual $\epsilon_{i,t} - u_{i,t}$, and β is identified. This solution is easy to implement. All it takes is to subtract $\ln(Accident^R_{i,t})$ from the log of the crime rate. We present in Appendix B an alternative solution to handle the bias that is similar, in the spirit, to this one. This alternative solution relaxes the assumption that the proxy variable is not directly affected by the policy. Instead, it takes advantage of the existence of twin proxy variables that are identically affected by the policy but where only one of them is a function of the unobserved reporting rate.

Identifying the rise in the reporting rate. By adding a little more structure to the model, we can identify the amplitude of the change in the unobserved reporting rate induced by the policy. We first need to remind from standard econometric textbooks what is the value of the bias when a relevant set of variables are omitted from a linear regression. In a general framework, suppose that the correct specification of a regression model is $Y = X_1\beta_1 + X_2\beta_2 + \epsilon$, but we estimate $Y = X_1\beta_1^{biased} + \epsilon^*$ where $\epsilon^* = X_2\beta_2 + \epsilon$. Then, the value of the bias is $P_{1,2}\beta_2$, where $P_{1,2} = (X'_1X_1)^{-1}X'_1X_2$ is the matrix of coefficients estimated from the regressions of the excluded variables, X_2, on the included variables,

X_1.[18] It is immediate to obtain the unbiased value of β_1: $\beta_1 = E[\hat{\beta}_1^{biased}] - P_{1,2}\beta_2$.

Now we can come back to our case, and add more structure in the relation between the reporting rate and the treatment by specifying the following equation.

$$\ln\left(RR_{i,t}\right) = \delta Pacified_{i,t} + X'_{i,t}\omega + a_i + e_{i,t} \tag{7}$$

By substituting again the expression of $\ln(RR_{i,t})$ that we get from equation (5) into equation (7), we obtain the following equation:

$$\ln\left(Accident^{R}_{i,t}\right) = \delta Pacified_{i,t} + X'_{i,t}(\lambda + \omega) + (a_i + d_i) + (e_{i,t} + u_{i,t}) \tag{8}$$

By construction, the coefficient of $\ln(RR_{i,t})$ from equation (4) is equal to one. It corresponds to the case where $\beta_2 = 1$ in the general framework presented above with just one omitted variable. Now we need to notice that the term $P_{1,2}$ corresponds to the coefficient δ in equation (8). Consequently, the increase in the reporting rate associated to the pacification is directly identified by the parameter δ. We present in Appendix C an extension that provides an alternative solution to recover the unbiased value of β, the treatment effect.

To sum up, we need two assumptions to correct the endogeneity bias generated by the change in the reporting rate induced by the policy. First, we assume the existence of a proxy variable, $Accident^{R}_{i,t}$, through equation (5). It involves that the part of the reporting rate that is time-varying is the same across all categories of event (crimes and accidents). Remark that we do not presume that the reporting rate is the same for all favelas and for all categories of event, which would be unrealistic. Instead, we postulate a much weaker assumption that the UPP specific reporting rate of all categories of event is affected in the same proportion by the policy, but allowing a constant part of the reporting rate to differ across events and UPPs, and allowing a time-varying part of the reporting rate, common across UPPs, to differ across time and categories of event C. This specification controls for much heterogeneity in the reporting rate. Second, we assume that $Accident^{R}_{i,t}$ is correlated to the treatment (the pacification) only through $RR_{i,t}$, the reporting rate. Accident reporting responds to the propensity to declare event, but not the pacification of a favela, as the policy has no reason to directly affect the number of accidents. The treatment effect, β, is point identified thanks to these two assumptions.

Although our method relies on the weakest possible assumptions, we can test the sensitivity of the results to the relaxing of the first assumption, that the UPP specific time-varying part of the reporting rate are identically affected by the policy. For instance, we can consider that the reporting rate of thefts could have been less positively affected by the pacification policy than the one of accidents,

[18] We see that if X_1 and X_2 are not correlated, there is no bias.

because these events are usually more reported to the police. Conversely, the reporting rate of assaults could have been more positively affected by the treatment, because they are usually less reported to the police.[19] So far, we have assumed that $RR^{(c)}_{i,t} = RR^{(a)}_{i,t}$, $\forall c$, where $RR^{(j)}$ is the time-varying part of the reporting rate of event j. Instead, we can relax this assumption and adopt a bounded variation assumption approach in the spirit of Manski and Pepper [2000]. Formally, it is possible that $RR^{(c)}_{i,t} - RR^{(a)}_{i,t} < 0$ (probably for thefts), or that $RR^{(c)}_{i,t} - RR^{(a)}_{i,t} > 0$ (probably for rapes and assaults). More specifically, we note $RR^{(c)}_{i,t} = \rho_{i,t} RR^{(a)}_{i,t}$. When $\rho_{i,t} > 1$, there is an over-reaction to the policy of the reporting rate of crime c compared to the one of accidents, and when $\rho_{i,t} < 1$, there is an under-reaction. By varying the value $\rho_{i,t}$, we can obtain bounds of the effects.[20]

Finally, the occurrence of events like murders is, fortunately, relatively rare. Crime data available at a detailed geographical level generally contain a lot of zeros so that the use of a logarithm function is problematic. Despite this difficulty, we employ a log-linear specification in log for several reasons. First, the empirical model naturally writes in log, as it was shown above. It allows to deal naturally with the reported nature of crime data and to estimate the increase in the reporting rate that is implied by the policy.[21] Second, the use of a log specification to study crime is standard in the literature so our estimation results are directly comparable to those of other studies (see for instance Levitt [1998b], Ayres and Levitt [1998], or Draca et al. [2011]). Third, we believe that the effects are more likely to be multiplicative than additive. Therefore, a specification in log seems to be more appropriate than other standard ones (i.e., OLS in level or poisson regressions), providing that we carefully handle the problem of zeros. To deal with this issue, we add a small constant to all crime data points, as log(0) is undefined. The choice of the constant value is key to minimize the bias that it will mechanically introduce. In general, adding the smallest possible value is not the best solution, as it can change the distribution of the data, depending on the value of the observations. Noting that log transformation squeezes high values and expand low values, the objective is to add a constant that tries to preserve the initial order of magnitude in the data and that approximately maps zero to zero. A rule of thumb is to add a constant that is close to the lowest strictly positive observation. For instance, with crime data, the lowest value above zero is one, then it is advised to add 0.5 or 1 to all the data points before applying the log function. McCune and Grace [2002] provides a procedure that rationalizes this rule.

[19] Gibson and Kim [2008] provides empirical evidence of the intensity of under-reporting by category of crime. They show that robberies and assaults are the less reported crimes in developing countries. Similarly, in the case of Great Britain, Vollaard and Hamed [2012] document that assaults, burglaries, and rapes are the most under-reported crimes.

[20] $\rho_{i,t}$ depends on UPP i and time t because it captures the reaction to the treatment of the reporting rate of crime c compared to the one of accidents. Indeed, the value of the treatment variable, $Pacified_{i,t}$ depends on i and t.

[21] For instance, we would not be able to estimate the value of the increase in the reporting rate with a poisson regression model as the omitted variable bias is more difficult to compute. Besides, the pseudo-maximum likelihood estimator of the Poisson model does not always converge to a solution.

Additional tests in the empirical section reveal that the best constant appears to be 0.5.

6 Results

6.1 Preliminary results

The choice of the constant added to all data points. We begin by testing the sensitivity of the results to the choice of the constant that is added to all data points by varying this parameter near 0.5, with $c = \{0.25, 0.5, 1\}$. Results are presented in Table 17 of Appendix D, respectively in panels A, B, and C. They show that the sign of the effect of the pacification is not affected by the choice of the constant, while the magnitude of the effect differs across the different constants. Furthermore, we look for the constant parameter that provides the closest results to what we get when estimating the model with OLS in level (i.e., without the log transformation). We first divide the coefficient estimated from the OLS in level by the mean value of crime rate over the period. We then compare this effect to the coefficients obtained from an OLS regression in log when adding a constant c, with $c = \{0.005, 0.01, 0.05, 0.1, 0.25, 0.5, 1\}$. We conduct this test for the assault indicator without correcting for the unobserved reporting rate. Results are presented in Table 18 of Appendix D. The effect obtained from the OLS specification without log (column 1) represents about 0.70% of the mean value of assault rate, which is closest to the effect obtained with a log regression when adding a constant c equal to 0.5 (column 3). Therefore, for the rest of the paper, we will always add a constant $c = 0.5$ to all data points.

A change in the reporting rate. People could have increased their propensity to report a crime following the pacification. This change in the reporting behavior of people could strongly bias our results. This positive bias might lead to over-estimate the effect of pacification on positively affected crime, and to under-estimate (in absolute term) the effect on negatively affected crime. Therefore, this issue is particularly problematic for crimes such as assaults, rapes, thefts, threats, and extortion, which increase after the pacification. These effects could be driven by the increase in the reporting of individuals.

Although new police stations are opened inside the favelas, it is not possible to report a crime there. Therefore, the time needed to complain about an event stays the same, and the reduced distance between inhabitants and the closest police station could not explain an increase in crime reporting. A plausible explanation leading to an increase in the reporting behavior could be that, following the pacification policy, people may be less afraid of going to the police station, they might trust more the criminal justice system, or they may have realized the moral need to report the crimes. To test this explanation, we investigate the effect of the pacification on the number of accidents, which is an event that is reported to the police but that should not be influenced by the pacification, because the two phenomena are not directly related. We observes two categories of accident in the data, fatal accidents and non-fatal accidents. As for murders, we assume that fatal accidents are systematically reported to the police. Therefore, fatal accident should not react to the pacification. Rather, we assume that non-fatal accidents are not systematically reported, as many other categories of crime, and they should increase in the same proportion as the reporting rate, as shown in Section 5.

Table 5 presents the effect of the treatment on fatal accidents in columns (1) to (4), and on non-fatal accidents in columns (5) to (8). We cannot reject the null hypothesis that the treatment effect is different from zero for fatal accidents (columns (1)-(2)). By contrast, the rate of non-fatal accidents significantly increases by about 20% following the pacification (columns (5)-(6)). There is no reason why the true number of accidents should increase with the pacification, unless the pacification energizes the activity of people in the streets, which could actually lead to more accidents. However, we have just shown that the pacification policy did not increase the rate of fatal accidents, and the potential energizing effect of the policy has no reason to apply only on non-fatal accidents but not on fatal accidents. To further rule out this explanation, we test whether the effect of the pacification on accidents is stronger in favelas located in the south of Rio de Janeiro and close to the beach.[22] These favelas are located in more central areas of Rio de Janeiro and are likely to attract more people (neighbors, tourists, etc.) and to generate more activity in the streets, and thus accidents, following the pacification. As shown in columns (3), (4), (7), and (8), there is no significant difference in the number of reported accidents in these favelas compared to the other ones. Therefore, the increase in the accident rate is presumably mainly driven by an increase in the reporting behavior of individuals due to the pacification.

Overall, the last two sets of evidence converge to show that the reporting rate of individuals has increased by about 20% with the pacification of favelas. Empirical evidences support that this increase is presumably explained by the fear or the lack of trust to report a crime. Consequently, the correlation between the treatment and the unobserved reporting rate generates a serious endogeneity issue that calls for a fix.

6.2 Main results

Results with a correction of the reporting bias. Panel A of Table 6 reports the estimated coefficients obtained without fixing the bias generated by the unobserved reporting rate for the ten crime indicators that we have presented in Section 4. We find that the murder rate and robbery rate

[22]These central favelas are Chapeu Mangueira E Babilonia, Pavao Pavaozinho, Providencia, Rocinha, Santa Marta, Tabajaras, and Vidigal.

Table 5: An increase in the number of accidents revealing an increase in the reporting rate

	Fatal Accident				Non-Fatal Accident			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pacified	0.0195	0.0113	0.0163	0.0129	0.239***	0.202***	0.242***	0.200***
	(0.0289)	(0.0311)	(0.0304)	(0.0376)	(0.0528)	(0.0564)	(0.0611)	(0.0724)
Pacified × Central			0.0157	-0.00706			-0.0117	0.0132
			(0.0131)	(0.0308)			(0.141)	(0.100)
Intervention × Central			0.00459	-0.0000008			-0.109	-0.153
			(0.0236)	(0.0346)			(0.0818)	(0.124)
Intervention	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4218	4218	4218	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

have decreased, respectively by about 5% and 10%, following the pacification. On the other hand, the assault rate, rape rate, theft rate, and threat rate have increased, respectively by 74%, 12%, 24%, and 79%. Lastly, the intensity of police actions has risen by 78% while the number of people killed by the police has dropped by 15%.

Using the number of accidents as a proxy variable for the unobserved reporting rate, we estimate equation (6) and present the results in Panel B of Table 6. As expected, the correction reduces (resp., increases) in absolute value the estimated effect for the crimes that were positively (resp., negatively) affected by the pacification. The sign of some effects is left unchanged. The increase in the assault rate is now equal to 49%, which is weaker than before but still strongly positive and significant, and the decrease of the robbery rate is now significant and about -35%. Some results are substantially affected by this correction. While positively linked with the policy, the rape rate is now negatively influenced by the pacification. The effect on thefts is no longer significant and the effect on extortion is now significantly negative.

Table 7 adds a linear time trend that is specific to each UPP to account for differences in the (linear) dynamic of a given crime between UPPs. For instance, the assault rate could be increasing in one UPP and decreasing in another one, independently from the treatment, because of different evolutions of the population for instance. The inclusion of UPP-specific time trends provides results comparable with those from Table 6. The main difference is that the treatment effect on the rate rape is negative but non significant when we correct for the unobserved reporting rate.

At this stage, it is useful to discuss which categories of crime event are likely to be under-reported. Obviously, police actions and police killings are not concerned by the reporting bias. Homicides are always registered by the police as long as a body is discovered by the police, so its reporting rate should not change with the implementation of the policy. In theory, thefts and robberies should be reasonably

Table 6: Comparison of results with and without the fix for unobserved reporting rate (without time trends)

	Panel A. Without correction of the reporting bias				
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.0516*	0.738***	0.118***	-0.106	0.249***
	(0.0255)	(0.0937)	(0.0350)	(0.0779)	(0.0735)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.780***	-0.146***	0.793***	0.00993	0.632***
	(0.128)	(0.0337)	(0.0927)	(0.0168)	(0.0705)
	Panel B. With correction of the reporting bias				
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.296***	0.494***	-0.126**	-0.350***	0.00555
	(0.0571)	(0.0986)	(0.0616)	(0.0940)	(0.0741)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.536***	-0.390***	0.549***	-0.234***	0.388***
	(0.139)	(0.0628)	(0.108)	(0.0557)	(0.0827)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	No	No	No	No	No
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Comparison of results with and without the fix for unobserved reporting rate (with time trends)

	Panel A. Without correction of the reporting bias				
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.0688*	0.715***	0.129***	-0.138**	0.245***
	(0.0340)	(0.0943)	(0.0340)	(0.0653)	(0.0671)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.751***	-0.165***	0.806***	0.0157	0.591***
	(0.129)	(0.0424)	(0.0925)	(0.0170)	(0.0662)
	Panel B. With correction of the reporting bias				
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.275***	0.509***	-0.0767	-0.344***	0.0387
	(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.546***	-0.371***	0.601***	-0.190***	0.385***
	(0.139)	(0.0732)	(0.114)	(0.0589)	(0.0825)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

well recorded by the police for insurance purposes. However, favelas are poor neighborhoods, and many of their inhabitants are not insured against thefts or robberies, so that the reporting rate of thefts or robberies should be less than one and could change with the policy.[23] Other categories of crime like assaults, rapes, extortion, or threats, are probably the most under-reported ones.

The results are in line with the theoretical model. Before the pacification policy, the drug gangs provide an incentive that prevents inhabitants of the favelas to commit crime. When the policy is implemented, some gang members are incapacitated and perpetrate less murders. The absolute deterrence effect coming from the presence of the police deter some other criminals from committing crime in general, while the marginal deterrence effect induces some others to switch from serious crimes (e.g., murders) to less serious crimes (e.g., violent assaults), resulting in a relative increase in minor crimes. This one-to-one substitution between similar categories of crime cannot generate an increase in the total number of crimes. Therefore, the decrease of the drug gangs' governance effect may explain why we observe an overall increase in the total number of crimes.

A possible concern for the use of official crime data is that they can be manipulated by the police. When we do not correct for the reporting bias, our estimations highlight negative effects on outcomes like murders or robberies, but it also produces important positive effects on assaults, rapes, thefts and threats. Overall, we find an important increase in the total number of events associated with the policy. If these official data were manipulated by the police, it would be unlikely that we observe such a strong increase in any category of crime. Besides, murders are probably one of the main objective of the policy as it is the more salient category of crime. We find a negative effect on murders that is consistent with the results of Magaloni et al. [2018a], who also find such an effect using anonymously declared crime data from *Disque Denúncia*.[24] Furthermore, we decompose the pacification effect between the twelve months of the year. This decomposition allows us to check whether the number of reported crimes reacts more to the pacification at the end of the year (November-December), or at the end of each trimester, when the police could falsify the numbers to virtually reach a crime level goal, because objectives are usually fixed at a quarterly or a yearly level (see Posner [2010] for a similar argument). Results are presented in Tables 19 and 20 of Appendix E. They do not exhibit a clear decrease in any high-level crime or a clear increase in the action of the police at the end of the year. Overall, evidence are not consistent with policemen cooking the books, so it seems implausible that these data were strongly manipulated by the police.

As a robustness check, we implement the extension of the second step of the correction, presented in Appendix C, that provides an alternative solution to recover the unbiased value of β. We simultane-

[23] Besides, it is noteworthy that making a complaint about a robbery could be more frightening than for a theft as the victims could have seen the face of their abuser.

[24] Actually, our estimated effect on the murder rate is slightly inferior in absolute value to what is obtained in Magaloni et al. [2018a].

ously estimate equation (8) along with equation (15), and we present in Tables 21 and 22 of Appendix F the coefficients $\hat{\beta}_{true} = \hat{\beta}_{biased} - \hat{\delta}$. Estimated coefficients are very similar to the ones contained in Tables 6 and 7. They confirm that the additional assumption made for the second step of the correction is very weak, which builds trust into our estimate of a 20% increase in the reporting rate of individuals.

Finally, we relax the assumption that $RR^{(c)}_{i,t} = RR^{(a)}_{i,t}$ $\forall c$, and we adopt a bounded variation assumption approach, as discussed in Section 5. We assume that $RR^{(c)}_{i,t} = \rho_{i,t} RR^{(a)}_{i,t}$. In practice, we parametrize $\rho_{i,t}$ as follow: $\rho_{i,t} = 1 + \kappa \times Pacified_{-i,t}$, and we vary the κ parameter every 0.1 in the interval $[-0.5; 0.5]$. When $\kappa = 0$, we have $RR^{(c)}_{i,t} = RR^{(a)}_{i,t}$ and we point identify the coefficient β as before. When $\kappa = 0.2$, the reporting rate of crime c is approximately supposed to increase by more than 40%, which is more than double the increase in the reporting rate of accidents (since the increase of the accident reporting rate is 20%). When $\kappa = -0.2$, the reporting rate of crime c is approximately supposed not to increase.[25] Finally, when $\kappa < -0.2$, we assume that the reporting rate of crime c decreases with the policy, which seems to be unrealistic. Therefore, we will not consider the cases where $\kappa < -0.2$.

Results obtained from these assumptions are presented in Table 8 for murders, assaults, rapes, robberies and thefts, and Table 9 for police actions, police killings, threats, extortion, and the total number of events.[26] The lessons learned from these analyzes are insightful. They show that the treatment effect is very likely to be positive for assaults, threats, and the total number of events, even when assuming variations in the reported rate quite different from those of accidents. Similarly, the treatment effect is likely to the be negative for robberies.

Actually, without priors on the increase of the reporting rate of rapes, the treatment effect on rapes appears as uncertain. A 10% increase in its reporting in addition to that of accidents results in a 17% decrease in the rape rate, but a 20% decrease in its reporting rate less than that of accidents results in a 15% increase in the rape rate. Similarly, without any prior, the treatment effect on thefts and, to a lesser extent, on extortion also seem quite uncertain. Conversely, with a prior, it is possible to think that the reporting rate of assaults has increased more than the one of accidents, but it would take to increase by 40% (approximately twice the increase of the one of accidents) for the treatment effect to be no longer significant, and to increase by 100% (more that three times the increase of the reporting rate of accidents) for the treatment effect to become significantly negative. Likewise, if the reporting rate of rapes increases more than that of accidents, it is likely that the number of rapes has

[25] To be exact, it would take $\kappa = -0.17$ for the reporting rate of crime c to not change ($1.2 \times 0.83 \approx 1$), and it would take $\kappa = 0.166$ for the reporting rate of crime c to increase twice more than the one of accidents ($1.2 \times 1.166 \approx 2$).

[26] We report results from bounded variation assumptions for murders, police actions, and police killings for the sake of comprehensiveness, although these events are most probably unaffected by the reporting bias.

decreased following the implementation of the favelas pacification policy. Lastly, the reporting rate of thefts might have increased less than the one of accidents. In this case, the effect on the theft rate would be likely to have increased.

Table 8: Results obtained from bounded variation assumptions: Murder, Assault, Rape, Robbery, and Theft

	κ	Murder	Assault	Rape	Robbery	Theft
Pacified	-50%	0.417***	1.201***	0.615***	0.348***	0.731***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0739)
Pacified	-40%	0.235***	1.019***	0.433***	0.166*	0.549***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	-30%	0.0817	0.865***	0.280***	0.0123	0.395***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	-20%	-0.0516	0.732***	0.146**	-0.121	0.262***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	-10%	-0.169**	0.614***	0.0286	-0.239***	0.144*
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	Equal	-0.275***	0.509***	-0.0767	-0.344***	0.0387
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	10%	-0.370***	0.413***	-0.172***	-0.439***	-0.0566
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	20%	-0.457***	0.326***	-0.259***	-0.526***	-0.144*
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	30%	-0.537***	0.246**	-0.339***	-0.607***	-0.224***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	40%	-0.611***	0.172	-0.413***	-0.681***	-0.298***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Pacified	50%	-0.681***	0.103	-0.483***	-0.750***	-0.367***
		(0.0655)	(0.104)	(0.0589)	(0.0844)	(0.0740)
Intervention		Yes	Yes	Yes	Yes	Yes
UPPs fixed effects		Yes	Yes	Yes	Yes	Yes
Time fixed effects		Yes	Yes	Yes	Yes	Yes
UPPs time trends		Yes	Yes	Yes	Yes	Yes
Observations		4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dynamic effects. The identifying assumption of the causal effect of the policy is that the order of pacification is uncorrelated with unobserved determinants of crime. However, it could be the case that the pacification policy occurred first in favelas with higher homicide rates or with specific linear trends of a given crime. To account for these possible concerns, we have included UPP fixed effect and linear trends specific to each UPP in our baseline specifications to account for linear growth in crime. Another concern could be that the public authorities decided to pacify first the favelas with growing (or decreasing) crime rates. In such a case, the usual diagnostic is to check whether the timing of the policy is correlated with (non-linear) trends in crime rates *before* the policy actually took place. If the pacification influences the evolution of crime rates before its actual implementation, then the presence

Table 9: Results obtained from bounded variation assumptions: Police Action, Police Kill, Extortion, Threat, Total Event

	κ	Police Action	Police Kill	Extortion	Threat	Total Event
Pacified	-50%	1.237***	0.321***	0.502***	1.292***	1.077***
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0824)
Pacified	-40%	1.056***	0.139*	0.320***	1.111***	0.895***
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0824)
Pacified	-30%	0.902***	-0.0144	0.166***	0.957***	0.742***
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0824)
Pacified	-20%	0.769***	-0.148*	0.0329	0.824***	0.608***
		(0.139)	(0.0732)	(0.0589)	(0.114)	(0.0825)
Pacified	-10%	0.651***	-0.265***	-0.0848	0.706***	0.491***
		(0.139)	(0.0732)	(0.0589)	(0.114)	(0.0825)
Pacified	Equal	0.546***	-0.371***	-0.190***	0.601***	0.385***
		(0.139)	(0.0732)	(0.0589)	(0.114)	(0.0825)
Pacified	10%	0.450***	-0.466***	-0.285***	0.505***	0.290***
		(0.139)	(0.0732)	(0.0589)	(0.114)	(0.0825)
Pacified	20%	0.363**	-0.553***	-0.373***	0.418***	0.203**
		(0.139)	(0.0732)	(0.0589)	(0.114)	(0.0825)
Pacified	30%	0.283**	-0.633***	-0.453***	0.338***	0.123
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0825)
Pacified	40%	0.209	-0.708***	-0.527***	0.264**	0.0485
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0824)
Pacified	50%	0.140	-0.777***	-0.596***	0.195*	-0.0206
		(0.139)	(0.0732)	(0.0590)	(0.114)	(0.0824)
Intervention		Yes	Yes	Yes	Yes	Yes
UPPs fixed effects		Yes	Yes	Yes	Yes	Yes
Time fixed effects		Yes	Yes	Yes	Yes	Yes
UPPs time trends		Yes	Yes	Yes	Yes	Yes
Observations		4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of these pre-event trends would invalidate the exogeneity of the pacification timing. Conversely, the absence of pre-trends would support the exogeneity assumption that the timing of the policy was random.

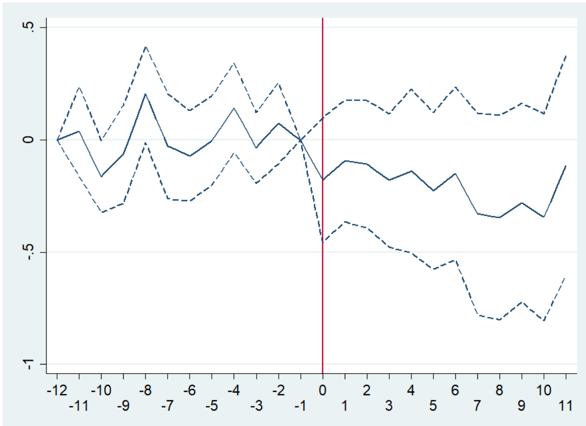
To test this assumption, we adopt an event-study specification, which allows to estimate non-parametrically the presence of such pre-trends associated with the policy while controlling for other factors. The empirical strategy exploits the staggered nature of the pacification policy. The progressive roll-out of the policy provides variations across UPPs and across times that allows to estimate the dynamic effects of the pacification over time in a flexible specification. The fact that some favelas are already pacified while some others are pacified later allows us to separately identify UPP fixed-effects, period fixed-effects (calendar time), and time fixed-effects relative to the date of treatment (relative time). Specifically, we aggregate the observations at the quarterly level to smooth the monthly variations, and we estimate the following specification:

$$\ln\left(Crime_{i,t}^{C,R}\right) = \alpha_i + X_{i,t}'\theta + \sum_{k=-11}^{-2}\pi_k 1[t - T_i = k] + \sum_{k=0}^{11}\tau_k 1[t - T_i = k] + \epsilon_{i,t} \tag{9}$$

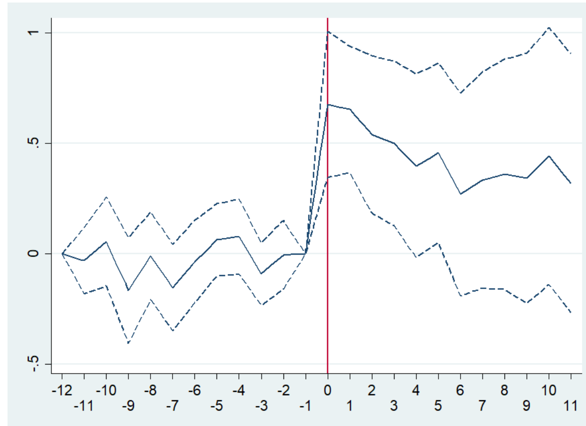
We estimate the effects of the policy over time using the coefficients on the event-quarter dummies, $1[t - T_i = k]$, which are equal to 1 when the number of quarters relative to the date of the pacification is equal to k (i.e., when the quarter t is k quarters away from T_i), the calendar date when an UPP i was pacified, with $k = -12, ..., 0, ..., 11$. As it is not instantaneous to be pacified, the intervention period is removed from the estimation to avoid mixing the pre-event effects with some intervention periods that are varying across UPPs. The event-quarter dummies $k = -12$ and $k = -1$ are omitted from the specification for an identification purpose (Borusyak and Jaravel [2017]). The coefficients π_k capture the changes in crime rates of future treated areas *before* the intervention of the BOPE. They allow to check for the absence of pre-event trends. In that case, they should be all equal to zero. The coefficient τ_k depict the evolution in crime rates *after* the areas were pacified, which depicts the dynamic effect of the policy. We only keep the observations where $k \in [-12, +11]$, which is up to three years before or after the pacification date, so the specification is estimated on a almost-balanced set of UPPs.

Estimation results are presented in Figures 2 and 3. They all demonstrate the absence of pre-event trends, confirming the idea that, among the set of favelas that were pacified at the end of the study period, the pacification was a random process, after controlling for time-invariant characteristics and a common time trend.[27]

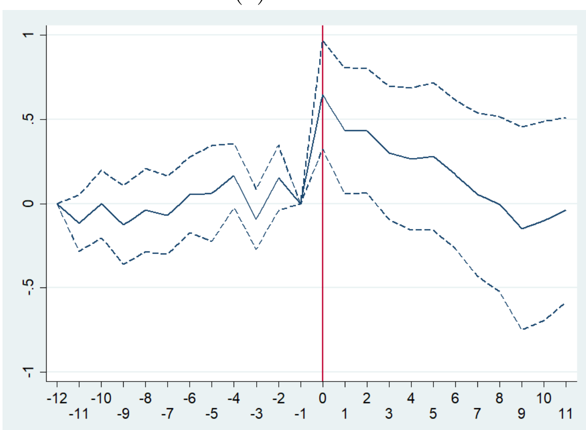
[27]Tests of joint significance of the pre-trends confirm the absence of any significant pre-trends, except for police actions and robberies, where we can reject the null hypothesis at the 10% level, but a visual analysis of the graphs shows that this does not seem to call into question the empirical results.



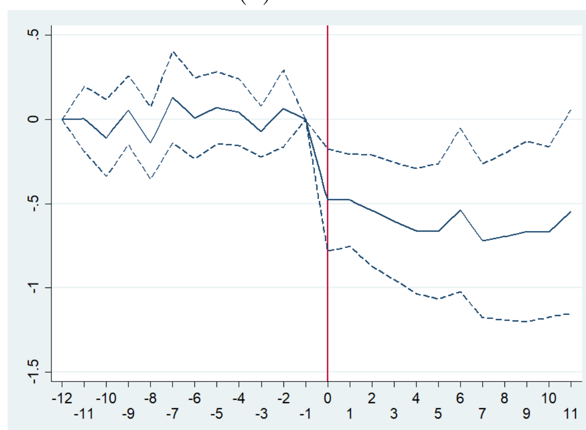
(a) Murder



(b) Assault

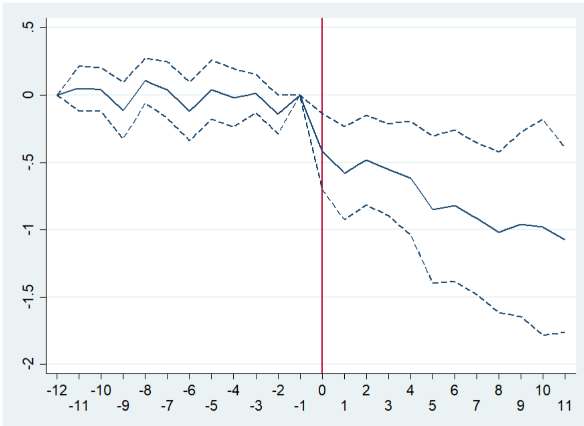


(c) Police Action

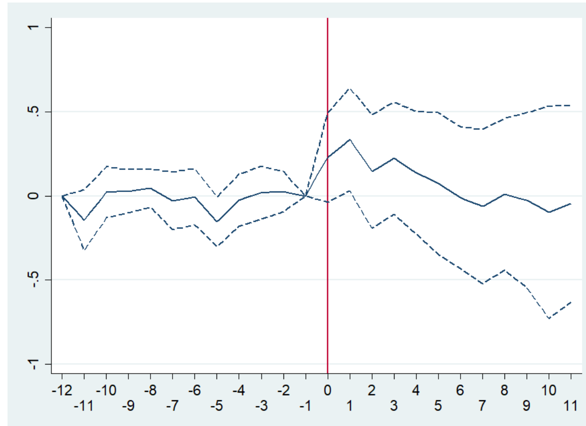


(d) Police Kill

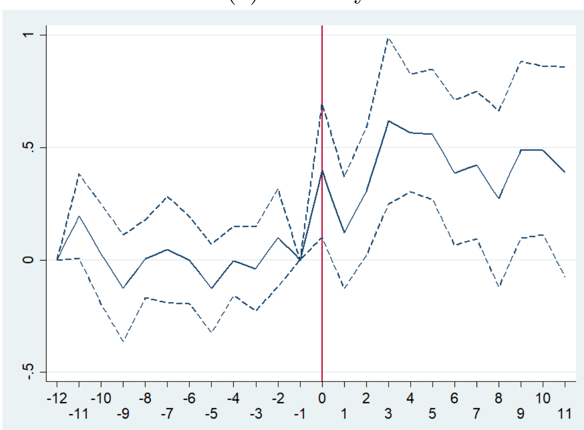
Figure 2: Event studies for Murder, Assault, Police Action, and Police Kill



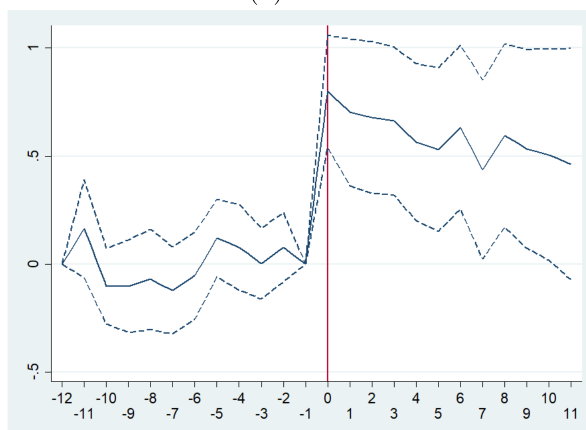
(a) Robbery



(b) Theft



(c) Rape



(d) Threat

Figure 3: Event studies for Robbery, Theft, Rape, and Threat

Spillovers between favelas. Gang members that are chased out of some pacified favelas may simply move to other unpacified favelas controlled by the same gang. Alternatively, gang members expelled from one favela could also engage themselve in a turf war against rivals. Then, the control group would also be affected by the treatment and the conclusions from such an analysis could be misleading (Miguel and Kremer [2004]). For instance, the estimated decrease in the number of homicides could stem from an increase of extreme violence in unpacified favelas, because of the displacement of gang members from the pacified areas to unpacified ones, leading to an apparent but inaccurate estimated decrease of homicide rates in pacified favelas.

To investigate such a mechanism, we make use of the pacification of the *Cidade de Deus*(CDD) favela, whose pacification process started in November 2008. Controlled by CV, CDD is the first big favelas, with 40,000 residents, of Rio de Janeiro to be pacified. Therefore, it represents an important shock that can potentially lead to spillover effects in unpacified favelas. To identify this spillover effect, we compare the crime rates in unpacified favelas controlled either by CV or rival criminal factions, before and after the pacification date of CDD. This test cannot identify a spillover effect that would have similarly affected unpacified favelas controlled by CV and those controlled by rival criminal factions. However, this configuration seems pretty unlikely to occur. To implement this test, we estimate the following equation:

$$\ln\left(Crime^{C,R}_{i,t}\right) = \alpha_i + \kappa CV_i \times CDD\_BOPE_t + X'_{i,t}\theta + \epsilon_{i,t} \tag{10}$$

where CV_i indicates whether the UPP i was controlled by CV before the pacification, and CDD_BOPE_t indicates whether the BOPE has entered CDD or not. We focus on the potential short term effects that could originate from this shock, one year after it occurred, so we drop observations after 2009. We only keep favelas that were still not pacified at the end of 2009, which leaves us with 32 UPPs that are used in this analysis. The favelas used in this analysis are yet to be pacified, therefore there is no reason for the reporting rate to vary with the treatment variable.

Table 10: Between favelas spillover effects following the pacification of CDD

Panel A. Without linear time trends					
	Murder	Assault	Rape	Robbery	Theft
CV × CDD_BOPE	0.0951	-0.0929	0.0280	-0.0102	-0.0889
	(0.104)	(0.0761)	(0.0618)	(0.107)	(0.108)
	Police Action	Police Kill	Extortion	Threat	Total Event
CV × CDD_BOPE	0.0462	-0.134	0.0421*	-0.101	-0.0152
	(0.151)	(0.120)	(0.0243)	(0.0937)	(0.0663)
UPP linear time trends	No	No	No	No	No
Panel B. With linear time trends					
	Murder	Assault	Rape	Robbery	Theft
CV × CDD_BOPE	-0.120	0.224	0.0977	-0.317	-0.452***
	(0.196)	(0.134)	(0.133)	(0.251)	(0.124)
	Police Action	Police Kill	Extortion	Threat	Total Event
CV × CDD_BOPE	-0.182	-0.00642	0.0125	-0.00921	-0.208**
	(0.278)	(0.139)	(0.0432)	(0.227)	(0.0973)
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	1152	1152	1152	1152	1152

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10 presents the estimated coefficient from equation (10). No clear spillover effect between favelas is associated with the pacification of *Cidade de Deus*, except for theft when we control for linear time trends. As an additional robustness check, we also test the effect of the pacification of the complexo do Alemao (the headquarter of CV), which started in November 2010, and which could also generate potential spillovers on other unpacified favelas. We only keep favelas that were still not pacified in 2011 and we drop observations after 2011, which leaves us with 19 UPPs that are used in this analysis. Results are presented in Table 23 of Appendix G. Again, we do not find any strong effects supporting the presence of spillovers affecting the control group.

The pacification of a small favela is likely to induce its gang members to move back to the headquarter of the gang. To test whether this could affect our findings, we estimate Equation 6 without the UPPs that contain gang headquarters.[28] Table 24 of Appendix G demonstrates that the results are very robust to the omission of gang headquarters, which reinforces the absence of spillover effects between favelas contaminating our analysis.

It is important to note that we do not find significant spillover effects within the set of favelas that are pacified at the end of the study period. This does not mean that gang members did not move inside these favelas, but just that there was no meaningful effect following their pacification. All that matters for this study is that these spillovers do not bias the estimated treatment effects for the conclusions to be correct, which seems to be the case here.

Robust inference. To realize correct inference with clustered standard errors, it is necessary to have an important number of clusters. In practice, having 30 to 40 clusters is usually considered as the minimum number of clusters for the asymptotic property of the Wald statistic to be valid. In our application, there are 37 clusters (UPPs), which is at the limit of being enough, so that we might underestimate the standard errors. Therefore, we implement two different procedures to correct this issue. First, we implement a wild cluster bootstrap procedure proposed in Cameron et al. [2008], which is supposed to perform well with a very limited number of clusters. The p-values obtained with this procedure are presented in Table 25 of Appendix H, and are in line with the main results displayed in Table 7.

Second, we run a randomization test, in the spirit of Fisher [1935], which does not rely on asymptotic properties. In a standard randomization test, the attribution of the treatment is randomized between the treated and the non-treated groups. In our case, all the groups (UPPs) received the treatment, so we cannot randomize on this dimension. To adapt the test, we randomized the treatment date of each UPP. The dates of BOPE's intervention are between July 2008 and March 2014 and the pacification dates are between December 2008 and May 2014. Since the duration of intervention is between 0 and 20 months, we attribute to each UPP a (uniform) random pacification date between July 2008 and May 2014. We do not randomize the intervention durations. A randomized intervention date is calculated so that it is equal to the randomized treatment date minus the intervention duration. Thus, we implicitly assume that the intervention duration is specific to each UPP, it stems from the UPP's

[28] These UPPs are: Fazendinha, Nova Brasilia, Adeus Baiana, Alemao, Chatuba, Fe Sereno, Parque Proletario, Vila Cruzeiro, and Rocinha. In an additional test, we also remove Mangueira, Jacarezinha, and Manguinhos from the estimation.

characteristics and does not depend on the date of pacification. To test we assumption, we model the intervention duration as a linear function of some UPP's characteristics and the rank of pacification. Table 26 of Appendix H shows that the intervention duration is determined by UPP's characteristics but not by the timing of pacification, which supports our procedure. We test the null hypothesis that the average treatment effect is zero. If the null hypothesis is true, the increase (or decrease) in crime in each UPP will be the same regardless of when the treatment is received. Therefore, the observed test statistic (i.e., the real estimated treatment effect) should not differ so much from all the randomized test statistics. Then, it is easy to compute the p-value as the proportion of test statistics that are superior (in absolute value) to the observed test statistic. Table 27 of Appendix H presents the p-values obtained from 1000 permutations of the treatment date and confirms the robustness of the results.

Other robustness checks. The estimated effects presented in Table 6 and 7 are obtained using a fixed effects (within) estimator that relies on the strong exogeneity condition. Therefore, we also estimate the pacification effect using a first difference estimator, which is famous for being less efficient, but that needs a weaker condition than the strong exogeneity condition. First difference estimators are presented in Table 28 of Appendix I, which also reports fixed effects estimators for ease of comparison. Although standard errors are much higher with the first difference estimators, point estimates are consistent between first difference and fixed effect estimators, which gives credit to the strong exogeneity assumption.

Finally, as a last robustness check, we also estimate the treatment effect of the pacification in the spirit of equation (6) using two other empirical approaches. We estimate this equation without the logarithmic transformation, and we estimate the treatment effect with a Poisson regression model. We show in Appendix J how to obtain specifications equivalent to equation (6) with OLS in level and with Poisson regression. Estimated results are stored in Appendix J in Tables 29 and 30 for OLS in level, and in Table 31 and 32 for Poisson regressions. The findings remain essentially the sames, which supports the validity of our empirical analysis.

6.3 Gang analysis: testing the mechanisms driving our results

Before the pacification policy was initiated, almost no favelas were free of criminal control. In short, the favelas that have been pacified were either under the control of CV, or under the control of ADA, or were contested by different criminal groups (ADA, CV, TCP or militias). Using and cross-checking information against several online websites and research reports, we were able to gather the identity of the criminal faction controlling each favela prior the intervention.[29]

[29] The main sources are Mapa do Ocupação Territorial Armada no Rio, favelascariocas.blogspot.com, InSight Crime, RioOnWatch, O Globo, Folha de S.Paulo. We consider Batan as a contested one. Prior its pacification

Using the description of the different gangs' philosophy, we conjecture that the governance system was more efficient in favelas controlled by ADA, and less efficient in contested favelas or in favelas operated by CV. In other words, the gang's governance effect was stronger in favelas controlled by ADA than in the other ones. This conjecture is supported by descriptive statistics from Table 11, which confirm that before the pacification the degree of violence was lower in favelas controlled by ADA compared to other ones. This is particularly salient for assaults.

Table 11: Annual average value (for 100 000 inhabitants) of crime rate variables before pacification and by criminal faction

	Before pacification (2007-2008)		
	ADA	CV	Contested
Police Action	203.2	419.5	384.1
Police Kill	17.6	31.9	16.6
Murder	21.6	21.2	35.2
Assault	153.1	245.0	280.8
Rape	7.4	9.3	6.8
Robbery	170.0	433.2	179.3
Theft	124.7	286.9	128.5
Threat	81.1	148.9	128.6
Extortion	3.1	4.1	4.2
Total Event	863.8	1759.7	1302.5

So far, we have mainly found that the policy has diminished the number of serious crimes (murders, police killings, robberies) but increased the number of less serious crimes (assaults, threats) more. This overall increase in the number of crimes is consistent with the gang's governance effect that is presented in our theoretical model. We perform a more advanced test of the plausibility of this effect by using the difference in the intensity of social governance between drug gangs. If the gang's governance effect plays no important role, we should see no serious difference in the effects of the pacification between territories controlled by CV or by ADA. Otherwise, the pacification policy should have delivered less favorable outcomes in ADA-controlled areas than in other ones. Consequently, we decompose the pacification across the three main types of criminal factions and we estimate the following specification:

(which start in July 2008), Batan was under the control of a militia that had expelled a drug faction in September 2007.

$$\ln\left(Crime_{i,t}^{C,R}\right) = \beta_1 Pacified_{i,t} + \beta_2 Pacified_{i,t} \times Contested_i + \beta_3 Pacified_{i,t} \times CV_i + X'_{i,t}(\theta - \lambda) + (\alpha_i - d_i) + (\epsilon_{i,t} - u_{i,t}) \tag{11}$$

The coefficient β_1 stands for the effects of the pacification on ADA's territories, while β_3 represents the differential effect of the policy between ADA's and CV's territories, such that the sum of the coefficients $\beta_1 + \beta_3$ captures the effects on CV's territories. Similarly, the sum of the coefficients $\beta_1 + \beta_2$ draws the effects on contested territories.

Table 12: Heterogeneous results according to the gangs controlling favelas before the pacification

	Murder	Assault	Rape	Robbery	Theft
Pacified	0.0383	0.895***	-0.0735	-0.432*	0.436**
	(0.0545)	(0.160)	(0.0888)	(0.222)	(0.211)
Pacified × Contested	-0.146	-0.206	0.213	0.182	-0.186
	(0.0875)	(0.345)	(0.148)	(0.323)	(0.262)
Pacified × CV	-0.116*	-0.478**	-0.0373	0.0886	-0.494**
	(0.0680)	(0.206)	(0.111)	(0.251)	(0.234)
Intervention × Contested	-0.380**	-0.696	-0.507***	-0.490	-0.858*
	(0.185)	(0.490)	(0.141)	(0.664)	(0.447)
Intervention × CV	-0.189	-0.760*	-0.155	-0.197	-0.576
	(0.175)	(0.391)	(0.134)	(0.553)	(0.381)
Bias correction	No	Yes	Yes	Yes	Yes
	Police Action	Police Kill	Extortion	Threat	Total Event
Pacified	1.655***	-0.0217	-0.278*	0.937**	0.596***
	(0.186)	(0.0413)	(0.147)	(0.406)	(0.173)
Pacified × Contested	-0.863*	-0.120	0.361*	0.110	0.0522
	(0.442)	(0.149)	(0.204)	(0.465)	(0.208)
Pacified × CV	-1.052***	-0.170***	0.0564	-0.466	-0.287
	(0.225)	(0.0584)	(0.160)	(0.427)	(0.209)
Intervention × Contested	-1.658***	-0.164	0.0661	-0.494	-0.699*
	(0.461)	(0.168)	(0.319)	(0.476)	(0.375)
Intervention × CV	-1.228***	-0.165	0.0557	-0.723	-0.456
	(0.369)	(0.138)	(0.122)	(0.453)	(0.373)
Bias correction	No	No	Yes	Yes	Yes
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table12 shows that the murder rate has more decreased in CV's territories than in ADA's territories, and the rate of assaults has increased much less in CV's territories. Similarly, the theft rate has increased in ADA's territories but not in CV's ones. The effect on contested territories are mostly similar in magnitude to those of CV's territories, although they are generally not significant. Overall, it supports the idea that the gang's governance effect plays an important role in explaining our results.

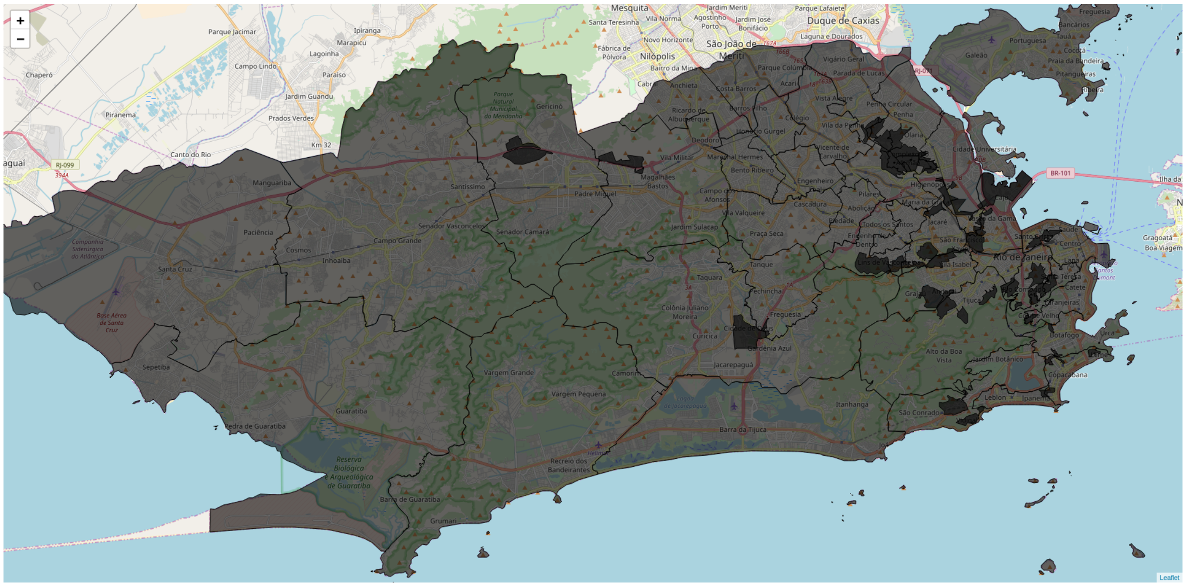


Figure 4: Mapping between districts (in grey) and UPPs (in black)

7 Extensions

7.1 Spillovers outside pacified favelas

Spillovers in Rio de Janeiro but outside favelas. The pacification policy could also have changed the crime rates in neighborhoods located at proximity of favelas. Indeed, drug gangs are often very active in the buffer zone located outside the favelas, so that the areas located just on the other side of the favelas can sometimes be more dangerous than the favelas themselves (Barcellos and Zaluar [2014]). Besides, drug gangs can carry out illegal activities, such as robberies, in surrounding neighborhoods that are not in the immediate vicinity of the favelas they control. To test the presence of spillovers occurring outside favelas within Rio de Janeiro, we use another set of data providing similar information about the number of crimes but at the district level. A district is typically much bigger than an UPP (Rio de Janeiro is divided in 35 districts). Figure 4 draws the districts of Rio de Janeiro along with the UPPs.

To detect the presence of such spillover effects, the first step consists in building the number of crimes occurring outside UPPs. To this end, we subtract the number of crimes that happened in UPPs located within a district to the total crime number of that district. When an UPP is located in several districts, its number of crimes is broken down across districts according to the share of the UPP population living in each district. This procedure can lead to some minor inconsistencies like a small negative value for some crime observations - that are replaced by zero - or non integer crime numbers.

To build the treatment variable, we use the grid of points presented in Section 4. For each point, we compute the percentage of the pacified population living in favelas or territories covered by the UPPs at the range distance of k meters, with $k \in \{0-3000; 3000-6000; 6000-9000\}$.[30] Then, for each district, we compute the average percentage value over the set of points (weighted by the population of each point) that are inside a district but outside the area covered by the UPPs. Let Ω_d be the set of points that are inside district d but not in the area covered by the UPPs, and $\mu_{p,d}$ the weight of point p in district d (i.e., the population of point p over the population of district d that lives outside UPPs). We define N_p^k as the population at the range distance of k meters from point p that lives in favelas or in areas covered by the UPPs, and $n_{p,t}^k$ as the size of this population that is pacified at date t. The treatment variable is

$$PercPacif_{d,t}^k = \frac{\sum_{p \in \Omega_d} \mu_{p,d} n_{p,t}^k}{\sum_{p \in \Omega_d} \mu_{p,d} N_p^k}$$

The spillover effect is identified by time-series variations of the percentage of the population living in favelas and that is pacified, and by their geographical dispersions relative to the districts. In other words, we compare over time districts having a given percentage of residents living in pacified territories in a given distance interval to districts having a similar percentage of residents living in favelas yet to be pacified in the same distance interval. Specifically, we estimate the following specification:

$$\ln\left(Crime_{d,t}^{C,R,NU}\right) = \alpha_d + \tau_t + \sum_k \theta_k PercPacif_{d,t}^k + \epsilon_{d,t} \tag{12}$$

where the notation NU means not in UPPs, e.g., $Crime_{d,t}^{C,R,NU}$ corresponds to the number of crimes that occurred in district d but not in the UPPs of district d during month t. The constructed measure, $Crime_{d,t}^{C,R,NU}$, is imperfect because we only know the number of crimes in favelas that were pacified once, but not in favelas that were never pacified. Therefore, $Crime_{d,t}^{C,R,NU}$ also contains the crimes that took place in favelas that were never pacified. If we accept that there is no spillover between all favelas (including the favelas that were never pacified), then $PercPacif_{d,t}^k$ is not correlated with the number of crimes occuring in favelas that were never pacified, and the coefficients of equation (12) are identified. We do not correct for the unobserved reporting rate because there are no reasons for the reporting rate of crimes committed outside favelas to change with the pacification policy.

Table 13 presents the results from the equation (12). The murder rate decreases in areas of districts that are located close to pacified favelas (between 0 and 3,000 meters), which is in line with the fact

[30] The lack of precision in the data prevents us from estimating spillover effects at a finer geographical level. It is possible to create a set of treatment variables at a finer geographical level, but the geographical perimeter of a district is very large compared to an UPP, and there are not enough districts and UPPs to compensate this coarseness. Defining finer treatment variables increases the correlation between them and generates a multicolinearity issue. Hence, the degree of precision of the treatment variables has been chosen as a tradeoff between geographical precision and absence of strong correlation between them.

that many territorial conflicts take place in areas just surrounding the favelas (Barcellos and Zaluar [2014]). Similarly, rapes, robberies, and thefts occurring outside UPPs decrease in areas located close to pacified favelas. The positive spillovers that we find contrast with Dell [2015] and Gonzalez-Navarro [2013], who find that increased enforcement of the law generates negative geographic externalities.

Table 13: Spillover effects outside the favelas (main crime indicators)

	Murder	Assault	Rape	Robbery	Theft
Panel A. Without linear time trends					
PercPopPacif[0;3]	-0.341***	-0.0442	-0.228***	-0.453***	-0.218***
	(0.123)	(0.0661)	(0.0813)	(0.133)	(0.0769)
PercPopPacif[3;6]	0.0632	-0.0609	0.0577	0.0892	0.107
	(0.119)	(0.117)	(0.124)	(0.178)	(0.120)
PercPopPacif[6;9]	0.170	-0.137	-0.0103	-0.170	-0.213
	(0.209)	(0.186)	(0.227)	(0.243)	(0.209)
DP linear time trends	No	No	No	No	No
Panel B. With linear time trends					
	Murder	Assault	Rape	Robbery	Theft
PercPopPacif[0;3]	-0.440***	0.0366	-0.226*	-0.254**	-0.180**
	(0.142)	(0.0658)	(0.114)	(0.113)	(0.0737)
PercPopPacif[3;6]	-0.0552	-0.186	0.0927	0.109	-0.0454
	(0.189)	(0.160)	(0.204)	(0.213)	(0.167)
PercPopPacif[6;9]	-0.0330	0.0218	-0.167	0.0916	-0.122
	(0.216)	(0.133)	(0.225)	(0.146)	(0.150)
DP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
DP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4104	4104	4104	4104	4104

Clustered standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Spillovers in the western part Rio de Janeiro and in the close periphery of Rio de Janeiro. Following the pacification of favelas in the eastern part of Rio de Janeiro, gang members of pacified favelas could have moved in the western part of Rio de Janeiro, or on the outskirts of Rio de Janeiro, whose favelas have never been pacified (except Mangueirinha, that is located just outside Rio de Janeiro). We test the presence of these spillovers using a set of data providing similar information about the number of crimes at the district (DP) level for the State of Rio de Janeiro, knowing that the State of Rio contains 127 districts in total. We gather the districts of the State of Rio de Janeiro in four parts, as depicted in Figure 5. The eastern part of Rio de Janeiro includes the districts containing UPPs, the western part of Rio de Janeiro contains no UPP, the periphery consists in the relatively dense urban area located around the city of Rio de Janeiro, and the countryside of the State of Rio de Janeiro encompasses the city and its periphery. The countryside is unattractive to drug gangs, they prefer to be located in economic poles. So it is unlikely that they have moved there as a result of the pacification of the Rio de Janeiro's favelas.

To identify the spillover effects from gang displacements, we compare the crime rates in districts localized in the countryside of the State (64 districts in the control group) with crime rates in districts localized in the western part or in the periphery of Rio de Janeiro (respectively 13 and 28 districts in these two treatment groups), according to the percentage of the favelas population of Rio de Janeiro that is pacified. Implicitly, we assume that the number of gang members of a favela is proportional to

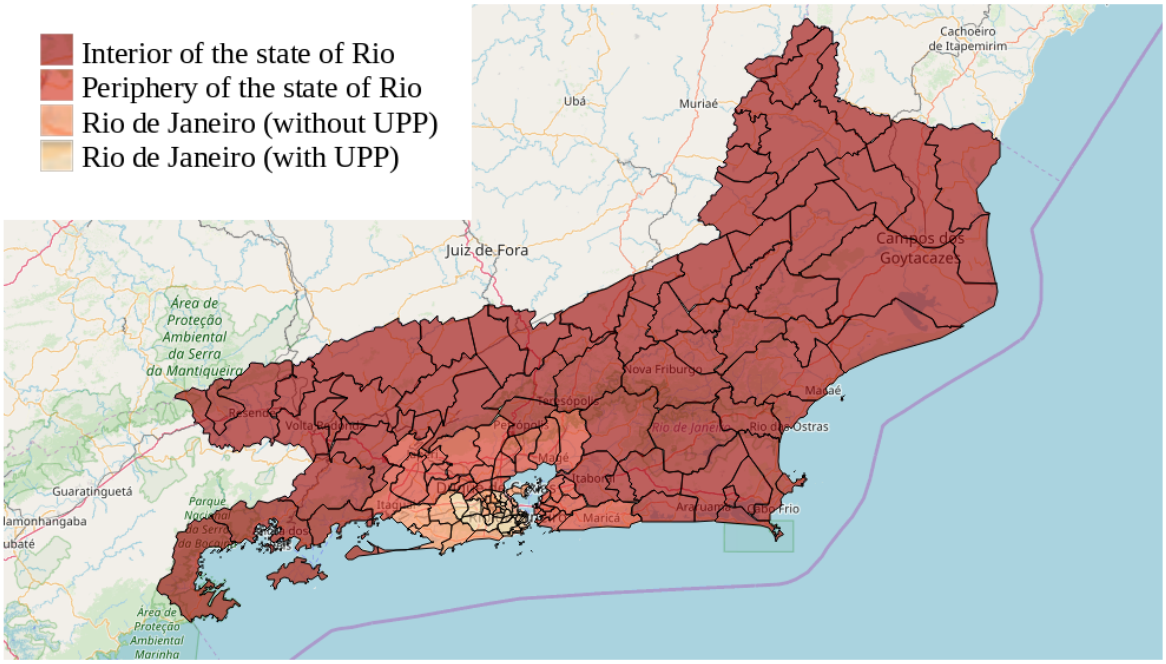


Figure 5: The four groups of districts in the State of Rio de Janeiro

its population. The higher the pacified population, the greater the number of gang members seeking to move away. Because the eastern districts of Rio de Janeiro are directly affected by the policy, they cannot be included in the control group, and we simply exclude them from the estimation (23 districts in the omitted group). Specifically, we estimate the following equation:

$$\ln\left(Crime^{C}_{i,t}\right) = \pi_1 WestRio_i \times \%PopPacif_t + \pi_2 Periphery_i \times \%PopPacif_t + \theta_i + \tau_t + \epsilon_{i,t}$$

where $WestRio_i$ and $Periphery_i$ respectively indicate whether the district i is located in western part or in the periphery of Rio de Janeiro, and $\%PopPacif_t$ represents the percentage of the favelas population of Rio de Janeiro that is living in a pacified area. Table 14 shows that crime indicators representative of gang activity, like police actions, police kills, or robberies, are increasing with the the pacified population of East Rio de Janeiro in the non-pacified districts near East Rio de Janeiro, relative to the countryside of the Rio de Janeiro's State. Besides, the number of thefts is decreasing in these districts.

There is no reason for the reporting rate to specifically change in these "treated" districts, as they are not pacified. They might be affected by the general awareness increase that one must now report crimes, but this is captured with the time dummies. Therefore, we do not implement our correction bias method. In any case, implementing this correction would not change the estimated coefficients as the accident rate in these districts is not affected by this spillover effect.

Table 14: Spillover effects in the West of Rio and in the periphery of Rio, compared to the countryside of Rio's State

	Police Action	Police Kill	Murder	Assault	Robbery	Theft
% pop. pacif. × Periphery	1.712***	0.344	0.359	-0.0197	0.767***	-0.593***
	(0.513)	(0.391)	(0.362)	(0.225)	(0.282)	(0.224)
% pop. pacif. × West Rio	1.273*	1.193***	-0.0242	0.337	0.354	-0.256
	(0.643)	(0.239)	(0.581)	(0.465)	(0.651)	(0.555)
DP fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
DP linear time trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11856	11856	11856	11856	11856	11856

All DPs containing at least one UPP are excluded from the analysis

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7.2 Heterogeneous effects

Lastly, we study several socio-economic characteristics that are likely to influence the causal effect of the pacification policy (the percentage of homeowners, of literacy, and of young people aged between 14-30, the population density, the altitude range, and the number deployed police officers). The variables obtained from the census are observed in 2010, at the beginning of the implementation of the policy (just three favelas were pacified before 2010). It prevents the pacification policy from generating important endogenous variations of these variables. In this analysis, we are not interested in the direct causal effect of these characteristics on crime, which would be quite challenging to identify. Instead, we analyze their interaction effect with the treatment, controlling for unobserved fixed heterogeneity that could generate low or high values for these variables.

Some interaction terms between these variables and the treatment may be endogenous if they were considered separately (e.g., investigating the interaction effect with homeownership without controlling for the interactions with other variables, like the income, could generate an omitted variable bias). For this reason, it is important to simultaneously estimate several interaction effects.[31] In summary, we do not claim to present the causal effects of these heterogeneous effects, but to document interesting correlations that are well controlled for.

Table 15 present the interaction effects estimated for the main crime indicators. While Panel A does not include linear time trends, it exhibits several interesting results. The number of deployed police officers could determine in part the policy efficacy. The pacification policy seems to have been

[31] Descriptive statistics show that all these variables are not strongly correlated. Therefore, they should be no multicolinearity issue.

less efficient in steep favelas (with high difference in altitudes), as it is known that steep sites and mountain areas are difficult to secure (Miguel et al. [2004]). The number of murders has decreased less in favelas where young people make up a large part of the population, as they may be less sensitive to the pacification policy. Finally, in contrast with Glaeser and Sacerdote [1999], pacification seems to have reduced crime more in favelas where population density is relatively high. It is possible that at a very local level, a high population density deters criminals from committing serious crimes (murders, robberies), as it is likely that someone witnesses the crime and then reports it to the police.

However, when we include linear time trends (Panel B), several of these effects disappear. Certainly, the most interesting robust results are that homeownership, literacy, and average income apparently improve the efficiency of the policy. The negative effect of education on crime occurrence has been demonstrated in Lochner and Moretti [2004]. We provide new evidence supporting this mechanism at the most basic level of education: the murder rate decreases more in favelas where inhabitants are more literate.[32] Individuals that are more educated may understand better that it is in their interest to react positively to the pacification policy. Besides, we also find that the murder rate declines more in favelas with more homeowners. It has been shown that homeowners develop more link with their neighbors (DiPasquale and Glaeser [1999]), but very few studies demonstrate convincing evidence about the effect of homeownership on crime (anecdotical evidence is found in Glaeser and Sacerdote [1999]). Homeowners may watch their neighborhood more closely following the pacification, which could prevent some crimes to take place. Finally, the pacification policy has reduced murders more in favelas where income per capita income is higher. The relation between crime and income is ambiguous, as shown in Ehrlich [1973], because higher income can increase the opportunity cost of committing a crime, but it can also rise the wealth to be stolen, the direction of the global effect depending mainly on the degree of risk aversion. Our empirical findings are more in line with the former explanation, confirming the idea that poverty is intrinsically linked to crime.

8 Conclusion

This study investigates the effects of the pacification policy implemented in Rio de Janeiro on several crime indicators. This policy aims at establishing state control and permanent police presence within the favelas by chasing out drug gangs. The progressive roll-out of the policy provides variations across favelas and across times that allows us to estimate the effects of the pacification over the period 2007-2016. A central issue in measuring the effects of the policy is that the propensity to report a crime is likely to increase with the treatment. We propose a new method to correct the bias resulting from

[32] Besides, Monteiro and Rocha [2017] show that the gunfights between drug gangs in Rio decrease student's scores at school.

Table 15: Heterogeneous effects according to several characteristics of UPPs (main crime indicators)

	Murder	Assault	Rape	Robbery	Theft
Panel A. Without linear time trends					
	Murder	Assault	Rape	Robbery	Theft
Pacified	5.361***	7.539	0.647	10.13*	9.120*
	(1.313)	(4.658)	(3.525)	(5.419)	(4.655)
Pacified × Homeowner	-1.498***	-0.969	0.353	-6.893***	-5.272***
	(0.311)	(1.886)	(1.088)	(1.824)	(1.852)
Pacified × Literacy	-4.871***	-6.530	-0.354	-3.785	-4.334
	(1.298)	(4.782)	(3.519)	(4.877)	(4.243)
Pacified × Income	-0.000308***	-0.00000867	0.000318	-0.00188***	-0.000707
	(0.000111)	(0.000516)	(0.000348)	(0.000667)	(0.000669)
Pacified × Youth	2.086***	-1.310	-1.950	1.928	0.536
	(0.537)	(2.529)	(1.604)	(2.229)	(2.334)
Pacified × Altitude range	0.000190	0.00165**	-0.0000186	-0.000897	0.00000915
	(0.000323)	(0.000734)	(0.000433)	(0.00110)	(0.000753)
Pacified × Police officers	-0.000154	-0.000939	-0.00124**	-0.00219**	-0.00144*
	(0.000176)	(0.000868)	(0.000521)	(0.000811)	(0.000815)
Pacified × Pop. density	-0.000151**	0.000413	-0.00000931	-0.000586**	-0.000481
	(0.0000673)	(0.000395)	(0.000211)	(0.000230)	(0.000291)
UPP linear time trends	No	No	No	No	No
Panel B. With linear time trends					
	Murder	Assault	Rape	Robbery	Theft
Pacified	3.722***	6.583	4.457	10.80***	5.746
	(1.352)	(5.056)	(2.891)	(3.946)	(4.009)
Pacified × Homeowner	-1.071***	-2.485	-0.695	-3.047**	-1.746
	(0.285)	(2.217)	(1.470)	(1.300)	(2.060)
Pacified × Literacy	-3.361**	-4.195	-4.390	-8.931**	-4.811
	(1.280)	(5.120)	(3.348)	(3.857)	(3.871)
Pacified × Income	-0.000247**	-0.000595	0.00000425	-0.000554	-0.0000194
	(0.000110)	(0.000898)	(0.000516)	(0.000590)	(0.000906)
Pacified × Youth	1.375**	-0.445	1.140	2.398	1.385
	(0.605)	(3.133)	(2.527)	(2.131)	(3.332)
Pacified × Altitude range	0.000553	0.00107	0.0000221	-0.000678	-0.000187
	(0.000382)	(0.00106)	(0.000649)	(0.00116)	(0.00103)
Pacified × Police officers	-0.000295	0.000140	-0.00000692	-0.000693	0.000283
	(0.000269)	(0.00131)	(0.000840)	(0.000740)	(0.00101)
Pacified × Pop. density	-0.000104	0.000159	-0.000357	-0.000865***	-0.000435
	(0.0000903)	(0.000422)	(0.000341)	(0.000251)	(0.000409)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the unobserved crime reporting change associated with the policy by combining a proxy variable and by adding some structure to the empirical model. The empirical evidence suggests that the policy has diminished the number of serious crimes (murders, police killings, and robberies) but increased the number of less serious crimes (assaults, threats). This increase is large enough so that the policy results in a rise in the total number of crimes inside favelas. These effects are predicted by a theoretical model driven by a combination of both absolute and marginal crime deterrence effects, and the fact that drug gangs secure the territories under their control.

The results highlight a new adverse consequence from wars on drug and on crime in areas with low State presence. Drug gangs protect the territory under their control, and chasing them out can thus unleash a serious criminal wave. They demonstrate the complexity of pursuing a policy fighting crime that does not backfire. The findings are susceptible to help governments to design policies fighting crime in the so-called "no-go areas", where the State presence is very low, and that also exists in many developed countries.

Murders are more likely to affect gang members than normal civilians, who are more exposed to assaults. Moreover, robberies may mainly concern the civilian population, leaving the welfare implications unclear. Evaluating the effect of this policy on welfare is challenging, and would require valuing the cost of life relative to the cost of an assault, which we prefer not to do. The period studied may be a transitional state leading to a new equilibrium to which individuals must become accustomed. Homeownership, literacy, and income per capita seem to improve the efficiency of the pacification policy. This implies that a winning strategy fighting crime cannot avoid focusing on education, social programs, and job creation in violent and poor areas, to prevent young individuals from becoming criminals.

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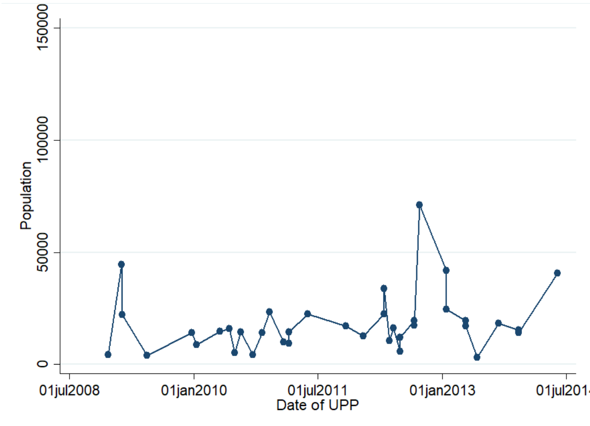
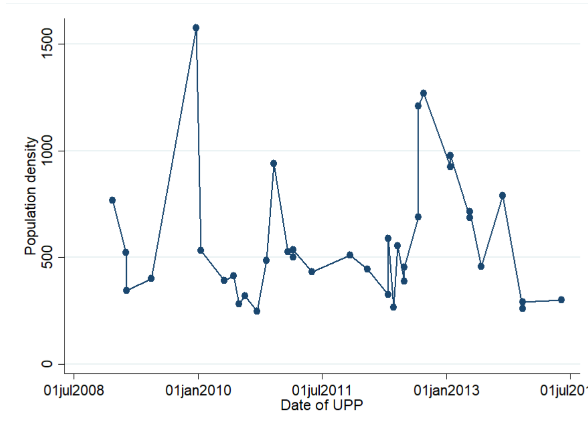
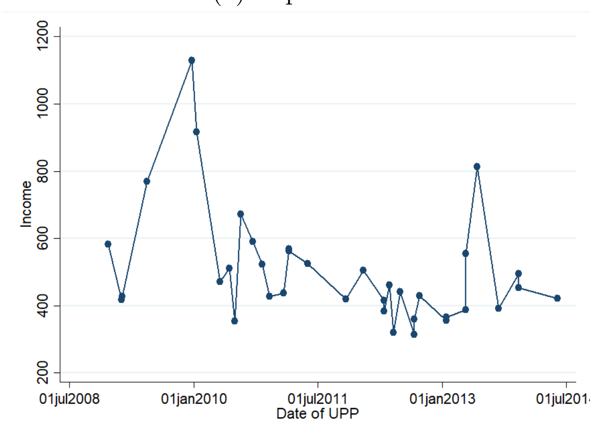
A Graphical evidence of the absence of clear pattern in the timing of pacification



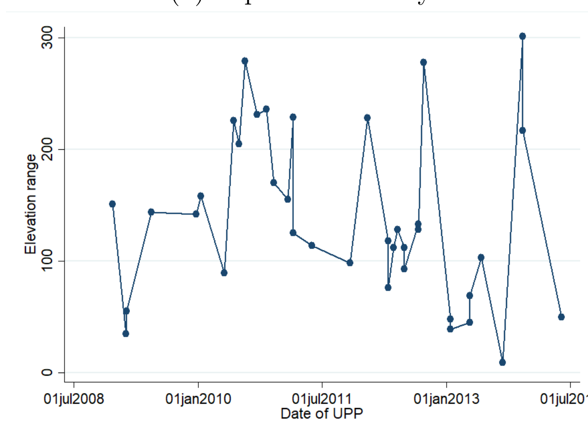
(a) Population



(b) Population density

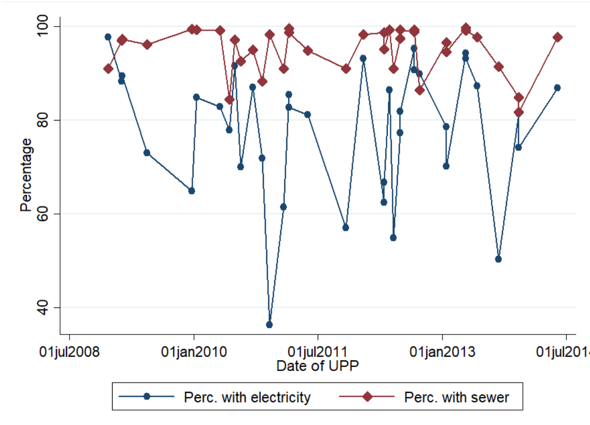


(c) Income per capita

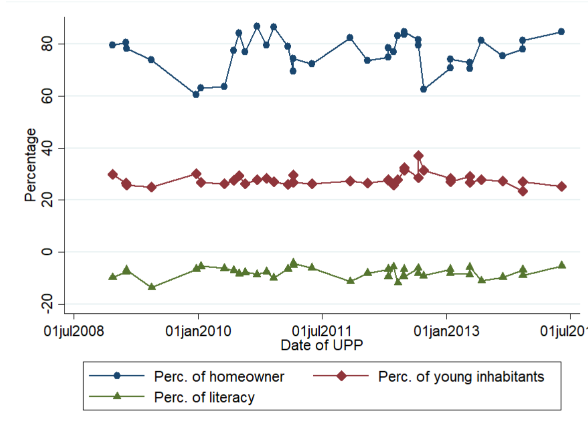


(d) Elevation range

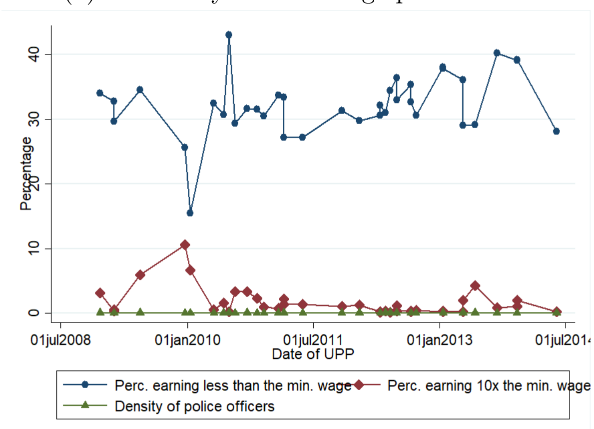
Figure 6: Characteristics of the UPPs as a fonction of the date of pacification



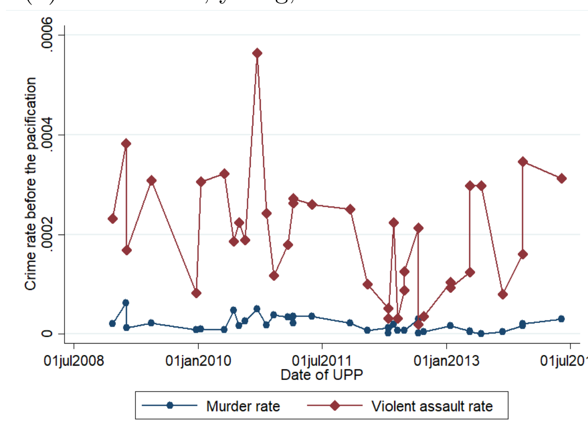
(a) Electricity and sewerage penetration



(b) Homeowner, young, and literate inhabitants



(c) Density of police officers and income inequality



(d) Murders and assaults before the pacification

Figure 7: Characteristics of the UPPs as a fonction of the date of pacification

B An alternative solution to handle the bias resulting from the unobserved reporting rate

The solution presented in Section 5 relies on two assumptions, one of them being that the policy has not direct effect on the proxy variable (the number of accidents). We present herein an alternative solution that relaxes this assumption by introducing a new variable that is very close to the proxy variable, but that is not affected by the reporting rate (i.e., its reporting rate is 100%). Indeed, the number of accidents could increase as a result of the pacification policy if this policy led to an increase in traffic on the streets of favelas.

The number of accidents can be decomposed in the number of fatal accidents and non-fatal accidents. We assume that the number of fatal accidents are always perfectly reported to the police, which seems quite realistic. Moreover, we allow the pacification policy to have a direct effect on the number of accidents, but we constrain this effect to be the same on fatal accidents and on non-fatal accidents. Indeed, accidents are random events, and the occurrence of fatal accidents compared to non-fatal ones is purely incidental. Thus, if the policy has an impact on accidents, it should have the same effect on fatal and non-fatal ones.

Formally,

$$\ln\left(Accident^{F}_{i,t}\right) = d_i + X'_{i,t}\phi + \kappa Pacified_{i,t} + u_{i,t}$$
$$\ln\left(Accident^{NF}_{i,t}\right) = c_i + X'_{i,t}\lambda + \kappa Pacified_{i,t} + \ln(RR_{i,t}) + e_{i,t}$$

Where $Accident^{F}_{i,t}$ and $Accident^{NF}_{i,t}$ are respectively the rate of fata accidents and non-fatal accidents in UPP i during month t. It is direct to obtain

$$\ln\left(Accident^{NF}_{i,t}\right) - \ln\left(Accident^{F}_{i,t}\right) = (c_i - d_i) + X'_{i,t}(\lambda - \phi) + \ln(RR_{i,t}) + (e_{i,t} - u_{i,t}) \tag{13}$$

As before, we have

$$\ln\left(Crime^{C,R}_{i,t}\right) = \alpha_i + X'_{i,t}\theta + \beta Pacified_{i,t} + \ln(RR_{i,t}) + \epsilon_{i,t} \tag{14}$$

By substituting $\ln(RR_{i,t})$ from equation (13) into equation (14), we get the following equation

$$\ln\left(Crime^{C,R}_{i,t}\right) - \frac{\ln\left(Accident^{NF}_{i,t}\right)}{\ln\left(Accident^{F}_{i,t}\right)} = (\alpha_i - (c_i - d_i)) + X'_{i,t}\left(\theta - (\lambda - \phi)\right) + \beta Pacified_{i,t} + (\epsilon_{i,t} - (e_{i,t} - u_{i,t}))$$

This equation can be directly estimated by OLS to obtain an unbiased β coefficient. Table 16 presents the results obtained with this solution. They are very similar to those obtained from the other solution and presented in Tables 6 and 7. This set of findings confirms that the assumption $E[Pacified_{i,t}u_{i,t}] = 0$ in equation (5) is not a strong one.

Table 16: Alternative solution to handle the bias resulting from the unobserved reporting rate

Panel A. Without linear time trends					
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.271***	0.518***	-0.102	-0.326***	0.0299
	(0.0736)	(0.100)	(0.0717)	(0.0975)	(0.0770)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.561***	-0.365***	0.574***	-0.210***	0.413***
	(0.136)	(0.0741)	(0.112)	(0.0712)	(0.0864)
Panel B. With linear time trends					
	Murder	Violent Assault	Rape	Robbery	Theft
Pacified	-0.260***	0.523***	-0.0620	-0.329***	0.0535
	(0.0816)	(0.101)	(0.0681)	(0.0948)	(0.0790)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.560***	-0.356***	0.615***	-0.175**	0.400***
	(0.136)	(0.0835)	(0.115)	(0.0733)	(0.0871)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This alternative solution to handle the bias is simply a variation of what is presented in the body of the paper. We show in Section 6.1 that the pacification policy probably did not have any significant effect on the number of fatal accidents. Therefore, it is not surprising that this alternative solution provides very similar results to those presented in Tables 6 and 7. This alternative solution could be useful for other papers facing a problem where the proxy variable is probably affected by the treatment but where a twin of the proxy variable exists and is unconcerned by the reporting bias.

C Another solution to recover the unbiased value of the treatment effect

In this Appendix, we show that the solution provides an alternative, yet very similar, way to recover the unbiased value of β. After having estimated equation (8), which writes:

$$\ln\left(Accident^{R}_{i,t}\right) = \delta Pacified_{i,t} + X'_{i,t}(\lambda+\omega) + (c_i + d_i) + (e_{i,t} + u_{i,t})$$

Another equation has to be estimated to obtain the biased coefficient of β (it corresponds to $E[\hat{\beta}_1^{biased}]$ in the general framework with an omitted relevant variable):

$$\ln\left(Crime^{C}_{i,t}\right) = \beta_{biased} Pacified_{i,t} + X'_{i,t}\theta' + \alpha_i^{'} + \epsilon_{i,t}^{'} \tag{15}$$

Finally, we can now recover the unbiased value of β, the parameter of interest, by using the following formula $\hat{\beta}_{true} = \hat{\beta}_{biased} - \hat{\delta}$. To test the significance of β_{true}, we need to estimate its variance, which require to know $cov(\hat{\beta}_{biased}, \hat{\delta})$. A simple solution is to simultaneously estimate equation (8) along with equation (15) in a seemingly unrelated regressions (SUR) model, and then to test the difference between $\hat{\beta}_{biased}$ and $\hat{\delta}$. Standard errors are clustered by UPP across both equations to account for the correlation between equation (8) and (15).

Formally, the estimated system writes as following:

$$\begin{pmatrix} \ln\left(Accident^{R}\right) \\ \ln\left(Crime^{C}\right) \end{pmatrix} = \begin{pmatrix} X & 0 \\ 0 & X \end{pmatrix} \begin{pmatrix} b_1 \\ b_2 \end{pmatrix} + \begin{pmatrix} e+u \\ \epsilon' \end{pmatrix}$$

where $\ln\left(Accident^{R}\right)$ and $\ln\left(Crime^{Cr}ight\right)$ are vectors containing their individual observations, X is a matrix containing each vector-variables of equation (8) (or, identically, of equation (15), since the explanatory variables are the same between the two equations), and b_1 (resp., b_2) is the vector of parameters of equation (8) (resp., equation (15)).

This extension necessitates more structure in the process generating the data, as it needs to specify equation (7), which is not necessary otherwise. Both methods provide comparable estimates for β, then this additional structure is presumably realistic.

D Robustness checks on the value of the constant added to all data points in the log-regressions

Table 17: Naive estimates without bias correction testing different constants

	Panel A. Adding a constant $c = 1$ to all data points				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0487*	0.621***	0.0827***	-0.0984*	0.196***
	(0.0257)	(0.0828)	(0.0233)	(0.0534)	(0.0558)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.631***	-0.112***	0.654***	0.0102	0.569***
	(0.110)	(0.0298)	(0.0788)	(0.0109)	(0.0620)
	Panel B. Adding a constant $c = 0.5$ to all data points				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0688*	0.715***	0.129***	-0.138**	0.245***
	(0.0340)	(0.0943)	(0.0340)	(0.0653)	(0.0671)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.751***	-0.165***	0.806***	0.0157	0.591***
	(0.129)	(0.0424)	(0.0925)	(0.0170)	(0.0662)
	Panel C. Adding a constant $c = 0.25$ to all data points				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0919**	0.793***	0.187***	-0.182**	0.291***
	(0.0433)	(0.105)	(0.0467)	(0.0786)	(0.0794)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.866***	-0.228***	0.955***	0.0225	0.605***
	(0.148)	(0.0568)	(0.107)	(0.0246)	(0.0695)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Testing different constants in log-regressions with OLS in level for assaults

	Outcome = Assault rate (mean value = 0.0003923)							
	Panel A: Without linear time trends							
	Regression in level	Regressions in log						
		$c = 1$	$c = 0.5$	$c = 0.25$	$c = 0.1$	$c = 0.05$	$c = 0.01$	$c = 0.005$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pacified	0.000281***	0.637***	0.738***	0.824***	0.922***	0.989***	1.135***	1.196***
	(0.0000429)	(0.0827)	(0.0937)	(0.104)	(0.117)	(0.128)	(0.154)	(0.167)
Intervention	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	No	No	No	No	No	No	No	No
	Panel B: With linear time trends							
	Regression in level	Regressions in log						
		$c = 1$	$c = 0.5$	$c = 0.25$	$c = 0.1$	$c = 0.05$	$c = 0.01$	$c = 0.005$
Pacified	0.000299***	0.621***	0.715***	0.793***	0.881***	0.940***	1.067***	1.120***
	(0.0000440)	(0.0828)	(0.0943)	(0.105)	(0.120)	(0.131)	(0.160)	(0.173)
Intervention	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Checking whether official crime data are manipulated at the end of the year by the police

Table 19: Decomposition of the pacification effect between the twelve months for the main crime indicators (1/2)

	Murder	Assault	Rape	Robbery	Theft
Pacif × Jan	-0.245*	0.735***	0.0481	-0.345*	0.0245
	(0.141)	(0.188)	(0.168)	(0.173)	(0.144)
Pacif × Feb	-0.477***	0.541***	-0.161	-0.429***	0.0616
	(0.120)	(0.168)	(0.126)	(0.152)	(0.162)
Pacif × Mar	-0.352**	0.622***	0.00488	-0.399**	0.221
	(0.148)	(0.204)	(0.155)	(0.189)	(0.174)
Pacif × Apr	-0.288*	0.644***	0.134	-0.310*	0.0493
	(0.149)	(0.196)	(0.142)	(0.182)	(0.161)
Pacif × May	-0.0598	0.500**	0.0469	-0.148	0.137
	(0.203)	(0.213)	(0.198)	(0.181)	(0.172)
Pacif × Jun	-0.408***	0.443**	-0.138	-0.397*	-0.173
	(0.134)	(0.183)	(0.144)	(0.197)	(0.158)
Pacif × Jul	-0.209	0.490**	-0.201	-0.407**	-0.0711
	(0.155)	(0.182)	(0.171)	(0.160)	(0.178)
Pacif × Aug	-0.295*	0.395**	-0.0820	-0.339*	0.0488
	(0.170)	(0.178)	(0.153)	(0.179)	(0.182)
Pacif × Sep	-0.0877	0.319**	-0.0416	-0.330**	0.185
	(0.129)	(0.153)	(0.135)	(0.162)	(0.144)
Pacif × Oct	-0.0968	0.540**	0.0204	-0.124	0.0302
	(0.117)	(0.220)	(0.145)	(0.134)	(0.173)
Pacif × Nov	-0.363**	0.346*	-0.356*	-0.413**	-0.196
	(0.135)	(0.176)	(0.182)	(0.184)	(0.156)
Pacif × Dec	-0.432***	0.570***	-0.187	-0.491***	0.152
	(0.138)	(0.162)	(0.173)	(0.167)	(0.143)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Table 20: Decomposition of the pacification effect between the twelve months for the main crime indicators (2/2)

	Police action	Police kill	Threat	Extortion	Tot event
Pacif × Jan	0.911***	-0.171	0.575**	0.121	0.496***
	(0.273)	(0.152)	(0.226)	(0.150)	(0.144)
Pacif × Feb	0.687***	-0.378***	0.559***	-0.245*	0.373**
	(0.205)	(0.118)	(0.202)	(0.123)	(0.143)
Pacif × Mar	0.642***	-0.377**	0.858***	-0.0788	0.529***
	(0.229)	(0.164)	(0.248)	(0.145)	(0.170)
Pacif × Apr	0.684***	-0.472***	0.571**	-0.266*	0.470***
	(0.211)	(0.147)	(0.215)	(0.133)	(0.150)
Pacif × May	0.636***	-0.291	0.734***	-0.164	0.442***
	(0.194)	(0.204)	(0.196)	(0.199)	(0.153)
Pacif × Jun	0.413	-0.421***	0.443**	-0.183	0.273**
	(0.252)	(0.143)	(0.190)	(0.122)	(0.134)
Pacif × Jul	0.636***	-0.368**	0.571***	-0.223	0.394***
	(0.173)	(0.140)	(0.188)	(0.160)	(0.139)
Pacif × Aug	0.498**	-0.433**	0.742***	-0.214	0.366**
	(0.217)	(0.165)	(0.173)	(0.185)	(0.166)
Pacif × Sep	0.376**	-0.347***	0.788***	-0.258**	0.367***
	(0.166)	(0.112)	(0.185)	(0.103)	(0.121)
Pacif × Oct	0.741***	-0.198	0.652***	-0.145	0.518***
	(0.194)	(0.119)	(0.181)	(0.141)	(0.145)
Pacif × Nov	-0.0858	-0.576***	0.385**	-0.339**	0.103
	(0.228)	(0.149)	(0.181)	(0.145)	(0.146)
Pacif × Dec	0.429**	-0.409***	0.281	-0.270**	0.295**
	(0.211)	(0.128)	(0.178)	(0.116)	(0.134)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Results from the other solution that recover the unbiased value of the treatment effect

Table 21: Estimated $\beta_{true} = \beta_{biased} - \delta$ from a SURE system for different crime indicators (without linear time trends)

	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.2955654***	0.4941271***	-0.1262692**	-0.3500715***	.0055518
	.0573808	.0990722	.0618367	.0943814	.0744184
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.5362353***	-0.3896493***	0.5493802***	-0.2339826***	0.3884491***
	0.1400546	0.0631175	0.1081512	0.0559665	0.0830649
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	No	No	No	No	No
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: Estimated $\beta_{true} = \beta_{biased} - \delta$ from a SURE system for different crime indicators (with linear time trends)

	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.2746591***	0.5087292***	-0.0767055	-0.3440692***	0.0387489
	0.0658048	0.1043738	0.059129	0.0847442	0.0743105
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.5455796***	-0.370791***	0.6005517***	-0.1901139***	0.3852772***
	0.1398801	0.0735082	0.1144423	0.0592188	0.0828316
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G Additional robustness check to test the potential spillover effects between favelas

Table 23: Between favelas spillover effects following the pacification of Alemao

Panel A. Without linear time trends					
	Murder	Assault	Rape	Robbery	Theft
CV × Alemao_BOPE	0.0297	-0.307	-0.118	-0.158	-0.269*
	(0.0565)	(0.227)	(0.0679)	(0.146)	(0.142)
	Police Action	Police Kill	Extortion	Threat	Total Event
CV × Alemao_BOPE	-0.314	0.0780	-0.559*	0.0924	0.0132
	(0.233)	(0.176)	(0.321)	(0.0799)	(0.0229)
UPP linear time trends	No	No	No	No	No
Panel B. With linear time trends					
	Murder	Assault	Rape	Robbery	Theft
CV × Alemao_BOPE	-0.0252	-0.354	-0.143	0.0378	-0.0339
	(0.104)	(0.306)	(0.0879)	(0.186)	(0.152)
	Police Action	Police Kill	Extortion	Threat	Total Event
CV × Alemao_BOPE	-0.221	0.0991	-0.443	0.0373	0.00481
	(0.260)	(0.206)	(0.325)	(0.0718)	(0.0288)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	1140	1140	1140	1140	1140

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 24: Estimations of the main effects without the UPPs containing the gang headquarters

	Panel A. Without linear time trends				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0809***	0.490***	-0.113*	-0.407***	0.00388
	(0.0273)	(0.0998)	(0.0647)	(0.106)	(0.0844)
Bias Correction	No	Yes	Yes	Yes	Yes
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.758***	-0.186***	0.486***	-0.234***	0.343***
	(0.132)	(0.0393)	(0.116)	(0.0642)	(0.0844)
Bias Correction	No	No	Yes	Yes	Yes
	Panel B. With linear time trends				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0923**	0.506***	-0.0805	-0.379***	0.0497
	(0.0375)	(0.103)	(0.0636)	(0.0935)	(0.0828)
Bias Correction	No	Yes	Yes	Yes	Yes
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.767***	-0.197***	0.541***	-0.207***	0.371***
	(0.132)	(0.0477)	(0.118)	(0.0662)	(0.0847)
Bias Correction	No	No	Yes	Yes	Yes
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	3192	3192	3192	3192	3192

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

H Robustness checks on clustered standard errors

Table 25: A wild cluster bootstrap procedure for inference of the main effects

	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0688**	0.509***	-0.0767	-0.344***	0.0387
P-value	(0.0350)	(0.0010)	(0.2080)	(0.0000)	(0.6210)
Bias correction	No	Yes	Yes	Yes	Yes
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.751***	-0.165***	0.601***	-0.190***	0.385***
P-value	(0.0000)	(0.0000)	(0.0000)	(0.0040)	(0.0000)
Bias correction	No	No	Yes	Yes	Yes
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	No	No	No	No	No
Observations	4218	4218	4218	4218	4218

P-values in parentheses. They are obtained from 1000 replications of the wild cluster bootstrap procedure.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26: Determinants of the UPP's intervention duration

	Intervention duration		
Rank of pacification	-0.00482		-0.0707
	(0.114)		(0.136)
Average income per capita		-0.0132***	-0.0138***
		(0.00448)	(0.00498)
Number of favelas		-1.055**	-1.024**
		(0.389)	(0.400)
Population		0.000551**	0.000541**
		(0.000241)	(0.000248)
Population^2		-6.84e-09**	-6.70e-09**
		(3.16e-09)	(3.27e-09)
Constant	5.167***	10.51**	11.66**
	(1.770)	(4.170)	(5.149)
Observations	37	37	37
R-squared	0.0000151	0.369	0.371

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 27: A randomization test of the pacification date for inference of the main effects

	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.069*	0.509***	-0.077	-0.344***	0.039
	(0.057)	(0.001)	(0.238)	(0.001)	(0.606)
Bias correction	No	Yes	Yes	Yes	Yes
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.751***	-0.165***	0.601***	-0.190***	0.385***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
Bias correction	No	No	Yes	Yes	Yes
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

P-values in parentheses. They are obtained from 1000 permutations of the treatment date.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I First difference estimator

Table 28: Comparison between first difference and fixed effect estimators

	Panel A. First Difference Estimators				
	Murder	Assault	Rape	Robbery	Theft
Δ Pacified	-0.00727	0.694**	-0.0331	-0.371	-0.263
	(0.0913)	(0.280)	(0.220)	(0.245)	(0.256)
Bias correction	no	yes	yes	yes	yes
Δ Intervention	yes	yes	yes	yes	yes
	Police Action	Police Kill	Threat	Extortion	Total Event
	0.474*	-0.171	0.599**	-0.158	0.274
	(0.262)	(0.106)	(0.243)	(0.207)	(0.201)
Bias correction	no	no	yes	yes	yes
Δ Intervention	yes	yes	yes	yes	yes
	Panel B. Fixed Effect Estimators				
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0516*	0.494***	-0.126**	-0.350***	0.00555
	(0.0255)	(0.0986)	(0.0616)	(0.0940)	(0.0741)
Bias correction	no	yes	yes	yes	yes
Intervention	yes	yes	yes	yes	yes
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.780***	-0.146***	0.549***	-0.234***	0.388***
	(0.128)	(0.0337)	(0.108)	(0.0557)	(0.0827)
Bias correction	no	no	yes	yes	yes
Intervention	yes	yes	yes	yes	yes
Observations	4181	4181	4181	4181	4181

Time fixed effects are included in all specifications. Clustered standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

J Alternative specifications

J.1 OLS without log-transformation

First, we assume that $Crime^{C}_{i,t} = Crime^{C,R}_{i,t} + NR^{C}_{i,t}$, where $NR^{C}_{i,t}$ is the non-reported share of events of category C. Then, the OLS specification without a logarithmic transformation that we would like to estimate writes:

$$Crime^{C,R}_{i,t} = \beta Pacified_{i,t} + X'_{i,t}\theta + \alpha_i - NR^{C}_{i,t} + \epsilon_{i,t}$$

As before, we do not observe $RR^{C}_{i,t}$. Therefore, we use a proxy variable, $Accident^{R}_{i,t}$ to recover its variations. Now, we assume that the non-reported share of event C can be additively separated into three components: $NR^{C}_{i,t} = NR^{C}_{i} + NR^{C}_{t} + NR_{i,t}$, where NR^{C}_{i} and NR^{C}_{t} are simply absorbed by the inclusion of UPP and time fixed-effects. Again, the main assumption is that $NR_{i,t}$, the time-varying part of $NR^{C}_{i,t}$, is the same for categories of events. Then, we assume the following relation:

$$Accident^{R}_{i,t} = X'_{i,t}\lambda + d_i - NR_{i,t} + u_{i,t}$$

Then, this expression can be plugged into the first equation, to obtain the following one:

$$Crime^{C,R}_{i,t} - Accident^{R}_{i,t} = \beta Pacified_{i,t} + X'_{i,t}(\theta - \lambda) + (\alpha_i - d_i) + (\epsilon_{i,t} - u_{i,t}) \tag{16}$$

Estimating that equation provides an unbiased value of β. It corresponds to the solution that is presented in the main body of the paper to account for the endogeneity of the unobserved reporting rate.

It would also be possible to identify the value of the increase in the unobserved reporting rate. It involves to presume that the negative relationship between the time-varying part of the non-reported share of events and the treatment can be written as follows:

$$NR_{i,t} = -\delta Pacified_{i,t} + X'_{i,t}\omega + c_i + e_{i,t} \quad \text{with } \delta \geq 0$$

By substituting this expression of $NR_{i,t}$ into the Accident equation, it is direct to obtain:

$$Accident^{R}_{i,t} = \delta Pacified_{i,t} + X'_{i,t}(\lambda - \omega) + (d_i - c_i) + (u_{i,t} - e_{i,t})$$

And we could estimate simultaneously the biased value of β as well as the value of δ in a SURE system to recover the unbiased value of β, as before

Here, we only present the results from the main OLS specifications with and without the fix for the reporting bias in Table 29 (without time trends) and Table 30 (with time trends). Results are very similar to what we obtain with log-regressions.

Table 29: Comparison of results obtained from OLS in level with and without the fix for unobserved reporting rate (without time trends)

Panel A. Without correction of unobserved reporting rate					
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.00000672***	0.000281***	0.0000140***	-0.0000364	0.0000648**
	(0.00000239)	(0.0000429)	(0.00000390)	(0.0000437)	(0.0000262)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.000230***	-0.0000154***	0.000210***	0.00000337	0.000946***
	(0.0000617)	(0.00000284)	(0.0000264)	(0.00000317)	(0.000105)
Panel B. With correction of unobserved reporting rate					
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0000380***	0.000250***	-0.0000172*	-0.0000677	0.0000335
	(0.00000925)	(0.0000429)	(0.00000916)	(0.0000476)	(0.0000227)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.000199***	-0.0000467***	0.000179***	-0.0000279***	0.000915***
	(0.0000634)	(0.0000102)	(0.0000307)	(0.00000988)	(0.000103)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	No	No	No	No	No
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

J.2 Poisson regressions

It is also possible to estimate the treatment effect of the pacification policy with Poisson regressions. First, let's assume that the true number of crime takes the form:

$$crime_{i,t} = \exp(\beta Pacified_{i,t} + X'_{i,t}\gamma + \alpha_i) \times population_{i,t}$$

However, we are only able to observe the reported number of crime, such that:

$$\begin{aligned} crime^R_{i,t} &= crime_{i,t} \times RR^C_{i,t} \\ &= \exp(\beta Pacified_{i,t} + X'_{i,t}\gamma + \alpha_i) \times population_{i,t} \times RR^C_{i,t} \\ &= \exp(\beta Pacified_{i,t} + X'_{i,t}\gamma' + \alpha'_i) \times population_{i,t} \times RR_{i,t} \end{aligned}$$

Table 30: Comparison of results obtained from OLS in level with and without the fix for unobserved reporting rate (with time trends)

Panel A. Without correction of unobserved reporting rate					
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.00000707***	0.000299***	0.0000151***	-0.0000380	0.0000733***
	(0.00000238)	(0.0000440)	(0.00000400)	(0.0000356)	(0.0000251)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.000239***	-0.0000166***	0.000229***	0.00000397	0.000999***
	(0.0000646)	(0.00000328)	(0.0000292)	(0.00000330)	(0.000104)
Panel B. With correction of unobserved reporting rate					
	Murder	Assault	Rape	Robbery	Theft
Pacified	-0.0000365***	0.000269***	-0.0000143	-0.0000674	0.0000439*
	(0.00000958)	(0.0000432)	(0.00000868)	(0.0000406)	(0.0000223)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	0.000210***	-0.0000460***	0.000199***	-0.0000255**	0.000970***
	(0.0000667)	(0.0000106)	(0.0000324)	(0.00000941)	(0.000102)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As before, we do not observe the reporting rate so that we use the number of accident as a proxy variable. Assuming that the time-varying part of the reporting rate is the same for all categories of events, we have:

$$\begin{aligned} accident^R_{i,t} &= \exp(X'_{i,t}\omega + d_i) \times population_{i,t} \times RR^A_{i,t} \\ &= \exp(X'_{i,t}\omega' + d'_i) \times population_{i,t} \times RR_{i,t} \end{aligned}$$

Then, by substituting the expression of $RR_{i,t}$ into the main equation, we obtain:

$$crime^R_{i,t} = \exp(\beta Pacified_{i,t} + X'_{i,t}(\gamma' - \omega') + (\alpha'_i - d'_i) \times accident^R_{i,t}$$

which corresponds to the solution proposed in this paper to correct the endogeneity of the unobserved reporting rate. Identifying the value of the increase in the reporting rate is much more difficult to obtain as it would need to compute the value of the bias coming from the omission of a relevant explanatory variable in a Poisson regression model, which is beyond the scope of this paper. The number of accidents is introduced in the specification as an exposure variable. In general, an exposure variable A appears inside a log function in the log-likelihood of the Poisson regression model, and written program of statistical software usually maximize the log-likelihood. Here, the number of accidents contains zeros so that the log-likelihood is undefined and statistical software like Stata cannot estimate this specification.[33] Therefore, we have simply added a constant equal to 0.5 to all accident observations so that the log-likelihood is defined. This procedure may introduce a bias in the value of the estimated β.

We present the results from Poisson specification with and without the correction for the reporting bias in Tables 31 (without time trends) and 32 (with time trends). Estimated coefficients are exponentiated, so their interpretation is straightforward: they are rate ratio corresponding to a one unit increase in the treatment variable. For instance, a coefficient equal to 1.3 (0.7) implies that the expected value of a given crime increases (decreases) by 30% following the treatment. Again, results are very similar to what we obtain with log-regressions, which confirms the robustness of the analysis. Nevertheless, the magnitude of some effects are substantially higher in absolute value for some crime indicators.

In a robustness check, we have deleted the observations with zero accidents and the results obtained in this case are very similar to those obtained when we add a constant equal to 0.5 to all accident observations. We can drop the observations with zero accidents without biasing the estimate of the β coefficient because the occurrence of an accident can be assumed random and independent from the realization of any crime.

[33] A solution would be to manually program the likelihood function and to maximize it, because the likelihood function of the Poisson regression does involve any log function. However, it is often numerically more difficult to maximize a likelihood function rather than a log-likelihood function.

Table 31: Comparison of results obtained from Poisson regressions with and without the fix for the unobserved reporting rate (with time trends)

Panel A. Without correction of the reporting bias					
	Murder	Assault	Rape	Robbery	Theft
Pacified	0.591***	2.183***	1.527**	0.696***	1.275***
	(0.0840)	(0.253)	(0.275)	(0.0919)	(0.109)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	1.968***	0.134***	2.233***	1.265	1.765***
	(0.318)	(0.0410)	(0.240)	(0.263)	(0.145)
Panel B. With correction of the reporting bias					
	Murder	Assault	Rape	Robbery	Theft
Pacified	0.464***	1.570***	1.095	0.563***	0.946
	(0.0680)	(0.172)	(0.217)	(0.0650)	(0.0716)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	1.496***	0.106***	1.658***	0.919	1.326***
	(0.206)	(0.0345)	(0.199)	(0.198)	(0.105)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	No	No	No	No	No
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses. Exponentiated coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 32: Comparison of results obtained from Poisson regressions with and without the fix for the unobserved reporting rate (with time trends)

Panel A. Without correction of the reporting bias					
	Murder	Assault	Rape	Robbery	Theft
Pacified	0.605***	2.119***	1.567**	0.804***	1.263***
	(0.0868)	(0.205)	(0.298)	(0.0440)	(0.0721)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	1.867***	0.134***	2.256***	1.287	1.662***
	(0.277)	(0.0425)	(0.218)	(0.275)	(0.0958)
Panel B. With correction of the reporting bias					
	Murder	Assault	Rape	Robbery	Theft
Pacified	0.475***	1.698***	1.250	0.679***	1.013
	(0.0721)	(0.197)	(0.253)	(0.0387)	(0.0788)
	Police Action	Police Kill	Threat	Extortion	Total Event
Pacified	1.540***	0.101***	1.848***	1.008	1.356***
	(0.206)	(0.0344)	(0.246)	(0.237)	(0.109)
Intervention	Yes	Yes	Yes	Yes	Yes
UPP fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes
Observations	4218	4218	4218	4218	4218

Clustered standard errors in parentheses. Exponentiated coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$