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Fighting Crime in Lawless Areas: Evidence from Slums in Rio de Janeiro†

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We use Rio de Janeiro's slum pacification program initiated in 2008 to analyze the effect of policies targeting crime in lawless areas. We correct the bias from the unobserved rise in crime reporting via the use of a proxy variable and bounded variation assumptions. We find that the program reduced the murder and robbery rates but strongly increased the assault and threat rates. We explain these results by providing evidence that increased enforcement weakened the security service that gangs provide on their turf, and may incentivize criminals to switch from serious to less serious crimes. (*JEL* H76, K14, K42, O17, O18, R23)

A new era of violence is dawning in which crime kills far more people than wars, armed conflicts, and terrorism combined (UNODC 2019). Many of the countries concerned have fragile economies that do not allow the proper enforcement of the law, producing lawless areas—for example, slums in major cities around the world—and leaving their citizens exposed to criminal violence. A criminal order is then established in these areas, led by gangs and organized crime, that replaces the state. To reclaim these territories, the state has to reestablish its physical presence and regain the trust and loyalty of its inhabitants. However, the best way to restore order in these areas remains unclear. As such, it is important that governments determine how to enforce the law and understand the effects of policies targeting crime in lawless areas.

Our analysis here identifies the effects of policies targeting crime in lawless areas and investigates a number of mechanisms that potentially produce side effects. In particular, we focus on a pacification policy that was initiated at the end of 2008 in Rio de Janeiro, which is home to a large number of slums, known

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as favelas, where law enforcement is weak.[1] This policy consisted of sending a special unit of the military police into groups of favelas to fight and drive away drug gangs and subsequently installing a new police station with a Pacifying Police Unit (Unidade de Polícia Pacificadora—henceforth, UPP) to regain control in the local area. By the end of 2014, 37 UPPs had been established in Rio de Janeiro, covering 830,000 inhabitants. This policy provides an ideal setting for our analysis, as it represents a major shock to law enforcement levels that will likely restore order in areas where the state is absent but may also produce measurable side effects on a number of crime indicators.

We investigate the consequences of pacification on crime using official data on the monthly number of different categories of crime and police activity at the UPP level from January 2007 to June 2016. The empirical approach appeals to the geographical and time variations in the pacification of the favelas, which was progressive over time due to limited capacity and funding. The pacified areas were presumably not chosen randomly, as enforcement usually targets areas with higher crime. We therefore compare before and after crime figures in favelas that were pacified at different dates. By focusing on the subset of favelas that were pacified, our identifying assumption is that the order in which favelas were pacified is exogenous to the unobserved factors explaining the crimes. Measuring the causal impact of a policy aimed at fighting crime using official crime data also raises another important empirical issue. The pacification of favelas may have increased individuals' crime reporting behavior as they perceive a greater likelihood of crime being solved or trust institutions more. We take two approaches to correct the bias resulting from this (unobserved) change in crime reporting following pacification. The first is novel, and uses the number of reported accidents as a proxy for the individual reporting rate. This method point identifies the treatment effect by correcting each reported crime by the increase in the accident reporting rate.[2] Second, we apply bounded variation assumptions, in the spirit of Manski and Pepper (2000), and estimate the causal effect of the policy by assuming a variety of different levels of the rise in the reporting rate.

We estimate that pacification reduced the murder rate by 7 percent (but with a low confidence level), while the assault rate rose by 66 percent. We also find that the number of robberies and people killed by the police fell by 29 percent and 15 percent, respectively, and that threats rose by 82 percent. Some of these numbers are notably affected by the first bias correction, as the crime reporting rate is estimated to have increased by 23 percent. With this adjustment, the theft and rape rates no longer rise after pacification. The second bias correction provides an upper bound of the increase in reporting that would reverse the effect. For instance, the reporting rate of assaults would need to have increased five times more than that of accidents for the treatment effect to change sign and become negative, which appears unlikely. Overall, the policy produced a 47 percent rise in the total number of crimes in the

[1] Rio de Janeiro had a murder rate of over 30 per 100,000 in 2017. By way of contrast, the same figure in Europe is around 1 per 100,000.

[2] Outcomes such as murders are systematically reported and are not affected by this bias. We thus only apply this correction to other outcomes, such as assault, theft, and rape, which are more likely to be affected by this bias.

pacified areas. These findings could explain why the policy was not necessarily well received by favela inhabitants (Jovchelovitch and Priego-Hernandez 2013; Musumeci 2017; Ribeiro and Vilarouca 2018).

We carry out a number of robustness tests. Public authorities may have decided to pacify favelas with particular crime trends first, but the lack of any significant pretreatment changes when estimating dynamic treatment effects supports the assumption of the exogeneity of policy timing. The posttreatment dynamic effects are also in line with the main aggregate effects. However, the lack of significant dynamic effects for murders casts doubt on its aggregate effect, which should therefore be treated more tentatively than the other estimates. In addition, the dynamic treatment estimates show that the assault rate, for example, increases immediately and markedly after pacification, which is unlikely to only reflect greater reporting given that restoring trust takes time. Our use of reported accidents as a proxy for the unobserved reporting rate entails two assumptions. The first is that the policy affects the number of reported accidents only through a change in reporting behavior without affecting the underlying number, an assuption supported by several pieces of evidence. Second, we assume that the reporting rate for each crime indicator is equally affected by the treatment. This assumption is untestable but is relaxed in our second reporting correction method of applying bounded variation assumptions to each crime indicator separately. An additional threat to identification is that gang members who are driven away from pacified favelas might simply move to other favelas that are not yet pacified so that the control group is also affected by the treatment (Miguel and Kremer 2004). We provide evidence against the presence of significant spillover effects *between* favelas that have been pacified.

There are a number of explanations for this rise in some crime indicators. First, the drug gangs that controlled favelas prepacification are known to provide security for their turf (Lessing 2012; Valle Menezes 2014), so the removal of the established criminal order may lead to a crime wave. Using differences in the style of gang governance in Rio de Janeiro, we show that pacification was indeed more harmful in areas that were controlled by gangs that were better known for providing security to the inhabitants. Second, we discuss two mechanisms that may lead criminals to switch from high-level crime (e.g., murders and robberies) to low-level crime (e.g., assaults, threats, and thefts) and provide supporting graphical and institutional evidence. The substantial confiscation of firearms following pacification may change both the behaviors of criminals and the consequences of their crimes. In addition, increased law enforcement greatly reduces the expected punishment for less serious crimes relative to more serious ones, and may thus affect the choices of local criminals.

Last, we identify spillovers *outside* pacified favelas, with a decrease in murders, robberies, and thefts in neighboring areas. This is consistent with drug gangs being mainly active in the buffer zone surrounding the favelas. Exploiting additional data from the Brazilian Health Ministry, we also show that this drop in murders mainly concerns gun deaths among Black and mixed-race people.

This article makes three contributions. Our work first relates to research on the effects of exogenous changes in police presence on crime. For example, Di Tella and Schargrodsky (2004); Klick and Tabarrok (2005); and Draca, Machin, and Witt

(2011) find that more police patrols reduce thefts and street crimes but have no effect on murders, while in Levitt (1997); Evans and Owens (2007); and Chalfin and McCrary (2018), increased policing reduces murders and robberies more than less violent crimes. These contrasting results may reflect different contexts or different mechanisms. However, as noted in Chalfin and McCrary (2017), the crime deterrence literature has paid only little attention to the mechanisms behind the results. We here identify one mechanism that may explain higher crime following expanded law enforcement, the gang governance effect, and discuss other mechanisms that produce substitution effects resulting from weapon confiscations and marginal deterrence.

Second, the bias from the endogenous increase in individual reporting behavior was discussed in Levitt (1998), but no solution has been proposed to date other than the analysis of crimes that are always reported (e.g., murders) or the use of unbiased data, such as victim surveys (Soares 2004; Gibson and Kim 2008; Vollaard and Hamed 2012).[3] We take two approaches to endogenous reporting. We first introduce a simple novel method to correct for unobserved changes in reporting via a proxy variable. The second method relies on assumptions about the size of the increase in reporting. These allow us to estimate the effect of greater enforcement on all categories of crime, and to identify differential effects across them.

Third, our research contributes to the limited number of empirical papers addressing the consequences of fighting drug gangs in lawless areas. Taking an ethnographic approach, Magaloni, Franco-Vivanco, and Melo (2020) characterize five types of criminal gang regimes and their relationship to the dynamics of violence in the favelas of Rio de Janeiro. They provide support for this classification by examining the effect of the UPP policy on homicides and police killings by these different criminal regimes.[4] Ferraz and Ottoni (2013) analyze the effects of the same policy on homicides and police killings up to 2012, finding a significant drop in both.[5] Last, Dell (2015) considers the effect of a Mexican anti-drug-gang policy and finds that it greatly increased the number of murders. Her result reflects the weakening of the major drug cartels, leading to the emergence of numerous new competing drug gangs, as in Biderman et al. (2019). We add to this work in a number of ways. By accounting for the unobserved change in reporting, we can estimate the effects of pacification on all types of crime in addition to murders, leading to a better understanding of crime overall. For example, we globally confirm the analysis of murders and gang governance in Magaloni, Franco-Vivanco, and Melo (2020) and extend this to other types of crime. We also discuss other mechanisms that may produce negative side effects from the targeting of drug

[3] Victim surveys are often not available to researchers. For instance, no victim survey has been carried out in Rio de Janeiro recently as far as we know.

[4] Magaloni, Franco-Vivanco, and Melo (2020) provide a detailed ethnographic analysis of criminal governance, taking a number of qualitative approaches as well as estimating regressions, while our approach is more economic and quantitative. From this perspective, our research and theirs are complements rather than substitutes. Their identification strategy differs from ours and relies on stronger identifying assumptions, as their difference-in-difference approach covers all Rio de Janeiro favelas, so favelas that were never pacified appear in the control group.

[5] They also show results for robberies, thefts, and assaults using data from Disque Denúncia, an anonymous crime reporting tool. However, crimes are still likely to be underreported here, as we discuss in Section IB.

gangs and positive externalities on neighboring areas, and characterize the profile of murders that fell with pacification.

The remainder of this article is organized as follows. Section I provides some background on the pacification policy and describes the data. Section II then introduces the empirical strategy, and Section III presents the main results. Section IV conducts a number of robustness tests, and Section V identifies one mechanism and discusses two others that may lie behind the rise in certain types of crimes. Section VI provides geographical analyses on the effects of pacification on crime. Section VII concludes.

I. Background and Data

A. *The Pacification of Favelas*

The Favelas of Rio de Janeiro.—A favela (or slum) is an informal urban area of low-income households with poor provision of services and infrastructure. Since the 1970s, with a large number of workers migrating from the poorer states of Brazil to Rio de Janeiro, the number of favelas has increased considerably, and almost 23 percent of the population of Rio de Janeiro currently lives in favelas (Cavallieri and Vial 2012). Favelas are desirable areas for drug gangs to set up business, as they combine a weak state presence, an attractive location to sell drugs to richer neighborhoods, and a geography that is well suited for military defense. Drug gangs have progressively gained control over these marginalized communities and enforced their own laws. Fights between drug gangs for the control of business and territory, coupled with the increasing sophistication of the weapons they use, have led to an escalation of violence (Leeds 1998; Penglase 2008).

The Pacification Policy.—In October 2007, Brazil was chosen to host the 2014 FIFA World Cup. The failure of previous policies to reign in urban violence, combined with the concerns expressed by the international community regarding public security in the areas around World Cup events, compelled the state of Rio de Janeiro to introduce a new policy called the UPP. This policy is rooted in the principle that criminal operations depend heavily on the territorial control of favelas and aims to establish state control and a permanent police presence in the favelas. There are 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 help from the military for the larger occupations, invades and occupies this group of favelas. They arrest or kill the gang members who did not leave, and search for hidden drugs and weapons. Finally, once the area had been secured, a police station is set up, and a community-based policing unit, composed entirely of new recruits who have received special training in community policing and human rights, is permanently assigned to the pacified group of favelas.

In some communities, social development programs promoting better access to sanitation, health care, and education (the "Social UPP") were created. However, these social programs were never fully implemented and did not succeed in bringing about social inclusion.[6] Finally, an additional policy was implemented jointly with the UPP policy. Created in 2009, the Sistema Integrado de Metas (SIM) aimed to measuring police unit performance via a set of strategic crime indicators. Civilian and military police officers can receive monetary rewards for their productivity and good practices. This policy does not represent a threat to our identification strategy since it was applied equally to all police units in Rio de Janeiro at the same time.

The Criminal Factions of Rio de Janeiro.—Since the 1980s, a number of gangs have fought to control Rio de Janeiro, and three main drug gangs have dominated the favelas since the 2000s: Comando Vermelho (CV), Amigos dos Amigos (ADA), and Terceiro Comando Puro (TCP).[7] Militias, another type of organization, emerged in Rio de Janeiro in the 1990s with the aim of chasing out drug gangs (Zaluar and Conceição 2007). Mainly composed of former police officers, prison guards, and firefighters, the militias have themselves progressively moved into drugs and other illegal business. Drug gangs are specialized in activities such as drug trafficking, murder, arms trafficking, robberies, extortion, kidnapping, prostitution, and human trafficking. However, they refrain from excessive violence against civilians in the favelas (Arias and Barnes 2017). The main victims of the gangs are criminals themselves, via the ongoing struggle to control areas, and those who are punished for having betrayed them. Confrontations with the police are also often deadly. By 2008, the majority of the city's favelas were controlled by criminal organizations (Barcellos and Zaluar 2014).

B. *Data*

Crime Data.—We use official information from the Instituto de Seguranca Pública (ISP), the institute that is in charge of producing crime data in the state of Rio de Janeiro (Instituto de Seguranca Pública n.d). The Civil Police record the nature, date, and location of each crime committed in the state of Rio de Janeiro. Although very detailed, geolocated crime-level data are not publicly available, the ISP has made public monthly aggregated figures for different categories of crime and police activity at the UPP and district levels.[8] Districts are much larger than UPPs and correspond to territorial areas under the responsibility of a district police station. The data cover the January 2007 to June 2016 period. Over this period, 37 UPPs were established in the city of Rio de Janeiro.[9]

To avoid an excessive number of indicators and some categories of crime that only occur rarely, we aggregate these indicators into broader categories and consider the following groups: police actions (corresponding to the sum of drug seizures, weapons seizures, arrests with a warrant, car recoveries, and arrests in

[6] See https://www.insightcrime.org/news/analysis/rio-pacification-limits-upp-social and https://www.insightcrime.org/news/analysis/what-latam-cities-can-learn-brazil-upp-policing-model.

[7] See https://insightcrime.org/news/analysis/favela-battle-reveals-complexity-of-rio-criminal-landscape.

[8] As UPPs only appear, by definition, in pacified favelas, we do not have this type of crime information in nonpacified favelas.

[9] An additional UPP was installed in the Mangueirinha favela, located in Duque de Caxias, another city in the state of Rio de Janeiro. We do not consider this UPP in this study.

flagrante delicto), police killings (the number of people killed by the police), murder (intentional homicides, assaults resulting in death, and robberies ending in death); assault (assaults with bodily injury not resulting in death and attempted murders); rape; robbery; theft; threats; and extortion.[10] The data also contain information on the number of accidents (both fatal and nonfatal) that corresponds to traffic events such as vehicle collisions, vehicles running over pedestrians, or collisions with fixed objects.

Pacification and Gang Data.—The ISP also provides the dates of BOPE intervention and UPP installation. An area is considered to be pacified when a police station has been officially inaugurated, that is, after the BOPE has regained control of the area. The numbers of police officers assigned to each UPP after the intervention come from http://www.upprj.com/.[11] Cross-checking information from various sources, we identified the criminal group that controlled each favela prior to the intervention.[12]

Socioeconomic Data.—We use the open data portal of the municipality of Rio de Janeiro to compute population and socioeconomic indicators at the UPP level (Prefeitura do Rio de Janeiro n.d.). This portal has made available socioeconomic data from the 2010 census, carried out by the Brazilian Institute of Geography and Statistics, and geospatial vector data at the census tract level.[13] It also provides geospatial vector data for the areas covered by each UPP. One difficulty in matching these geospatial vector data comes from census tracts that lie partially inside and partially outside of a UPP area. We tackle this issue by assigning census data to UPPs weighted by the degree of geographical overlap between them.

Remark.—Another nonofficial source of crime information is the anonymous crime reporting system Disque Denúncia, a portal installed in Rio de Janeiro by an NGO in 1995. This operates via an anonymous phone line and website to encourage individuals to report crimes. However, the concern regarding the greater propensity of residents to report crime following pacification also applies here; as such, these data do not provide additional information on the reporting rate. Individuals may still not report crimes if they believe that these will not be acted on by the justice system.[14] In addition, information campaigns encouraging favela residents to

[10] Another way to classify crime would be to consider the intention of the criminal rather than the result of his behavior. Murder would include intentional homicides and attempted murders, while assault would cover assaults with bodily injury not leading to death as well as those that lead to death. As the pacification policy had little impact on assaults resulting in death and attempted murders, the results from this alternative definition are similar to those from the original definition.

[11] The website address is no longer valid. This information can be found in the Internet Archive: http://web.archive.org/web/20170814095808/http://www.upprj.com/index.php/informacao/informacao-interna.

[12] The main sources are Mapa do Ocupação Territorial Armada no Rio, favelascariocas.blogspot.com, InSight Crime, RioOnWatch, O Globo, and Folha de S.Paulo.

[13] These datasets were retrieved in June 2018 from http://portalgeo-pcrj.opendata.arcgis.com. This link is no longer valid. The datasets are, however, available in the replication material.

[14] As noted on the website of the NGO (https://disquedenuncia.org.br/numeros), the propensity of inhabitants to use Disque Denúncia depends strongly on their confidence in the police.

use Disque Denúncia were carried out at the same time as pacification.[15] The UPP policy is therefore likely to affect the use of this hotline. For example, the Disque Denúncia coordinator noted that, at the beginning of the occupation of the complexo de Alemão, there was not only an increase in the number of connections and complaints but also a change in the profile of whistle-blowers.[16]

C. *Descriptive Statistics*

Table 1 presents descriptive statistics for the different crime variables at the UPP level before and after pacification. These show a fall in the number of serious crimes (murders, police killings, and robberies) but an increase in the number of less serious crimes (assaults, thefts, and threats). Police actions and the number of accidents also rose considerably. Overall, the total number of crimes seems to have risen sharply over the period.

Table 2 lists the descriptive statistics for various socioeconomic characteristics of the households located inside or outside favelas according to whether they live in areas that were pacified.[17] In columns 1 and 2, favela residents are significantly poorer and more deprived in terms of service and infrastructure access than are the inhabitants of other areas of the city. Inside the favelas, households in pacified areas are slightly poorer and have less access to electricity than those in nonpacified areas (columns 3–4). The few areas covered by UPPs that are not officially classified as favelas are also significantly poorer and less literate than the areas outside favelas and outside UPPs (columns 5–6).

Online Appendix Table A.1 shows the timing of the interventions and some other UPP descriptive statistics (number of police officers, population, identity of the gang controlling the territory prepacification, etc.). Starting at the end of 2008, the favela pacification dates are distributed fairly evenly over time.

Last, we compare crime in the UPPs to that in the rest of Rio de Janeiro (outside the UPPs) using information on the number of crimes at the district level. We plot the annual murder and assault rates in Figure 1. There is a general downward trend in extreme violence, as the murder rate decreases in both UPPs and the rest of the city. However, the trend is different for other indicators. The number of assaults increases sharply in the UPPs and is close to that in the rest of the city at the end of the period. Online Appendix Figure A.1 also shows that the number of police actions rose more in UPPs than in the rest of the city, while the number of people killed by the police fell much more than in the rest of Rio de Janeiro.

[15] See https://professorlfg.jusbrasil.com.br/artigos/121915032/o-perfil-das-denuncias-ao-disque-denuncia-antes-e-apos-a-implantacao-das-upps.

[16] See http://g1.globo.com/rio-de-janeiro/noticia/2011/05/disque-denuncia-bate-recorde-mensal-de-denuncias-e-ligacoes.html. As another example, the average number reporting on Disque Denúncia in the favelas of Providencia and Santa Marta prepacification was about ten per month; this figure tripled in the month the UPP arrived (see https://piaui.folha.uol.com.br/materia/policia-camera-acao).

[17] The open data portal of the municipality of Rio de Janeiro also provides geospatial vector data for the areas covered by the favelas. Using the same procedure as described above, we calculate socioeconomic indicators at the favela level.

Table 1—Crime before and after Pacification

	Before pacification (2007–2008)		After pacification (2015–2016)	
	Mean	SD	Mean	SD
Murder	22.8	(17.4)	16.9	(21.4)
Assault	236.5	(140.6)	572.6	(299.3)
Robbery	370.2	(684.6)	166.8	(191.0)
Theft	247.9	(393.4)	262.3	(159.5)
Police action	386.5	(265.3)	775.0	(374.8)
Police killings	28.3	(24.8)	8.5	(10.7)
Threats	137.5	(120.0)	263.2	(171.4)
Rape	8.8	(8.0)	18.5	(15.5)
Extortion	4.0	(4.9)	5.1	(6.9)
Total events	1,589.2	(1,694.4)	2,464.3	(1,006.6)
Accidents	52.3	(60.8)	80.3	(71.3)

Notes: This table lists the annual mean value per 100,000 inhabitants of crime indicators before and after pacification using monthly crime data for favelas covered by the 37 UPPs installed in Rio de Janeiro. Standard deviations appear in parentheses.

Table 2—Average Socioeconomic Characteristics in Rio de Janeiro

			Favela		Nonfavela	
	Favela	Nonfavela	UPP	No UPP	UPP	No UPP
	(1)	(2)	(3)	(4)	(5)	(6)
Income per capita (reals)	390.22	1,371.39	379.54	395.63	625.03	1,406.12
Household size	3.26	2.85	3.31	3.23	3.08	2.84
Homeowner (percent)	76.16	72.37	76.41	76.04	72.66	72.35
Households electricity (percent)	77.36	96.24	73.65	79.2	88.29	96.58
Households water (percent)	96.54	98.96	97.18	96.23	99.14	98.95
Illiterate +15 years old (percent)	6.45	1.92	6.52	6.42	3.71	1.85

Note: These statistics are calculated from the 2010 census.

II. Empirical Strategy

This paper takes advantage of geographical and time variations in the pacification of Rio's favelas. Limited capacity and limited funding meant that the introduction of the policy was staggered over time. We can thus compare pacified favelas to similar nonpacified favelas at almost any point in time. The choice of favelas to be pacified was clearly not random but was determined largely by political considerations related to the organization of the World Cup and the Olympic Games that can be assumed to be independent of the factors determining crime. In particular, the favelas close to tourist areas, close to Olympic Games facilities, and on the route between the international airport and downtown Rio de Janeiro were more pacified than others (see Figure 2).

One approach to estimate the effects of pacification would be to directly compare pacified and nonpacified favelas regardless of favela type in a difference-in-difference estimation. The identifying assumption here is that the choice of favelas to be pacified was exogenous after controlling for the fixed heterogeneity in favela crime level and the specific time trend for each type of crime. As we do not observe crime

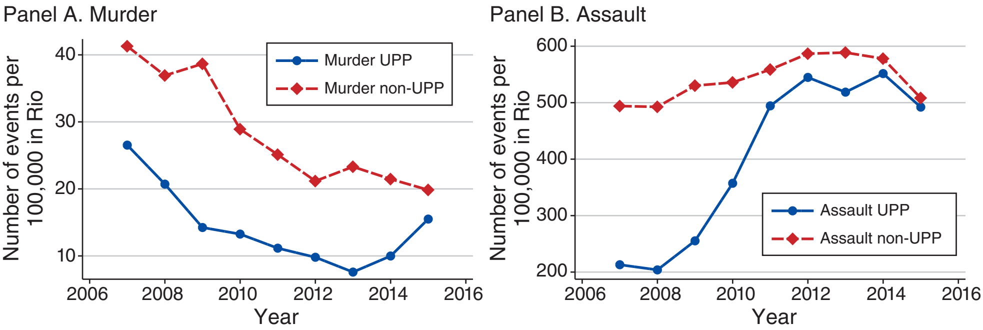

FIGURE 1. CRIME TRENDS IN UPPS AND THE REST OF THE CITY (OUTSIDE UPPS)

Note: Figures plot the annual crime rates per 100,000 inhabitants in Rio de Janeiro in areas that were covered by UPPs at the end of the study period and in areas that were never covered by UPPs (i.e., in the rest of the city).

in favelas that were never pacified, we take a different approach and restrict our analysis to favelas that were pacified over the 2007–2016 period. We thus compare pacified favelas to those that will be pacified in the future, which are likely to be similar.[18] The identifying assumption in this approach is that the timing of pacification is exogenous to the unobserved factors explaining crimes; in other words, the order in which favelas are pacified is independent of crime, conditional on fixed effects and common time trends in the group of favelas that were treated at the end of the policy. This identifying assumption is weaker and may be considered to be more convincing.

Online Appendix Figure B.1 supports this assumption by showing that the geographical location of pacified favelas over time in the set of treated favelas does not follow any clear pattern. We plot the characteristics of the UPPs as a function of their pacification date in online Appendix Figures B.2–B.3. These reveal no obvious order in favela pacification dates by their characteristics, except perhaps by their size (the large groups of adjacent favelas were pacified later). Finally, we show in Section IVA that the timing of favela pacification is uncorrelated with pretreatment crime trends. This set of evidence provides support for our identifying assumption.

We analyze the crime rate (i.e., crime relative to the population) to account for different UPP population size. We model $Crime^C_{i,t}$, the crime rate of type C (murders, robberies, etc.) in UPP i in month t, as an exponential:

$$(1) \quad Crime^C_{i,t} = \exp\big(\alpha\, Intervention_{i,t} + \beta Pacified_{i,t} + \mathbf{X}'_{i,t}\theta + \nu_i + \gamma_t + \epsilon_{i,t}\big),$$

where $Pacified_{i,t}$ is a dummy variable for the UPP i area being pacified in month t. In this regression, β is the coefficient of interest and captures the average effect of being pacified on the crime rate. $Intervention_{i,t}$ indicates whether BOPE, the police special task force, is pacifying the area i in month t. It is important to account for this first stage of the policy, as this may influence crime before the favela is actually

[18] At the beginning of the period, the control group contains all the favelas that will be pacified. As the policy rolls out, treated favelas leave the control group for the treatment group.

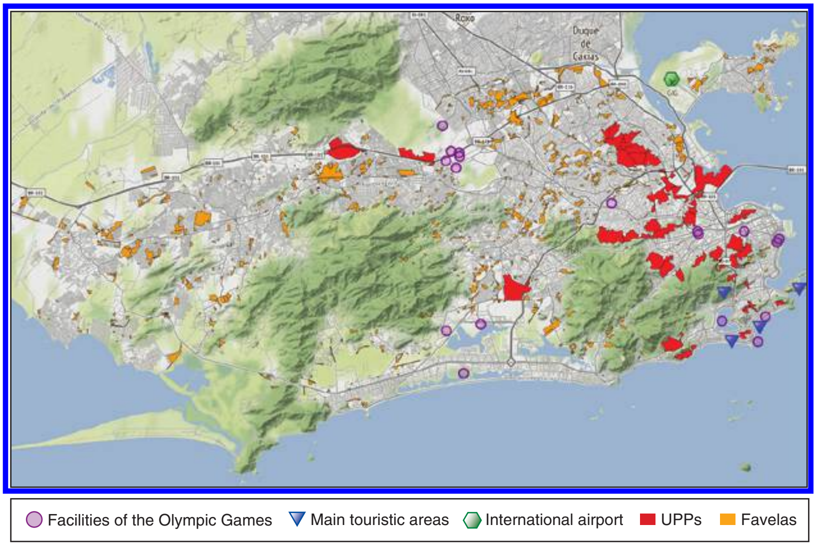

Figure 2. Location of UPPs, Favelas, Olympic Games Facilities, Main Tourist Areas, and the International Airport

pacified. $\mathbf{X}_{i,t}$ includes UPP linear time trends to account for particular developments in crime rates and population changes in each UPP. The UPP fixed effect ν_i captures a fixed unobserved heterogeneity in crime, such as one specific area being more violent than another. The time fixed effect γ_t picks up the evolution of crime C that is common to all areas pacified over the period. Last, $\epsilon_{i,t}$ is the error term, which is assumed to be independent of the other explanatory variables. To account for the potential autocorrelation of the error terms within UPPs, the standard errors are clustered at the UPP level.

We analyze official crime data that were reported to the police. These data have the advantage of being systematically collected over time, contain information about almost all categories of crime, and are similar to the other official crime data that are typically analyzed by researchers. However, official data do also have one drawback: crime might be underreported.

As we do not observe actual crime levels but only reported ones, we add structure to the empirical model and consider the following relationship:

$$Crime_{i,t}^{C,R} = Crime_{i,t}^{C} \times RR_{i,t}^{C}, \tag{2}$$

where $Crime_{i,t}^{C,R}$ is the reported level of crime of type C in UPP i in period t and $RR_{i,t}^{C}$ its analogous reporting rate. Using the above and equation (1), the specification we wish to estimate is

$$\begin{aligned}\ln\left(Crime_{i,t}^{C,R}\right) = {} & \alpha\, Intervention_{i,t} + \beta Pacified_{i,t} + \mathbf{X}_{i,t}'\theta + \nu_i \\ & + \gamma_t + \ln\left(RR_{i,t}^{C}\right) + \epsilon_{i,t}.\end{aligned} \tag{3}$$

Assuming that $\epsilon_{i,t}$ is independent of the other explanatory variables, we will estimate the causal effect of pacification on crime. However, the reporting rate is unobserved and is likely to be correlated with the treatment. Favela residents may well become more likely to report crime following pacification as they may trust institutions more, be less afraid of officially reporting a crime, or have realized the moral requirement to report crime.

There is thus an endogeneity problem, and the estimated treatment effect will be positively biased. If the estimated coefficient is negative, then we will underestimate (in absolute value) the real effect but can at least be sure that it is negative. Interpretation becomes more difficult when the estimated coefficient is positive. The real effect could actually be negative, but the higher reporting rate may suffice to more than offset it. We propose two adjustments to tackle this issue. The first is novel and relies on a proxy variable and the addition of a simple structure to the empirical model. The second consists in making bounded variation calculations, with the novelty residing in the adaptation of this method to reporting issues.

A Novel Correction for the Unobserved Reporting Rate.—The idea behind the first correction is to find a proxy variable—i.e., a variable that is affected by the change in reporting behavior but not by the treatment. Table 1 shows that the reported accident rate rose over the analysis period, and we suggest that this rise came about for reporting reasons rather than being directly affected by pacification. We discuss and formally test this assumption in Section IVB. We focus below on the econometric aspect of the solution, presuming that our assumptions hold.

Formally, we assume that $Accident_{i,t} = \exp\left(\mathbf{X}'_{i,t}\lambda + d_i + d_t + u_{i,t}\right)$ and that $Accident^R_{i,t} = Accident_{i,t} \times RR^A_{i,t}$. As such, $\ln\left(Accident^R_{i,t}\right) = \mathbf{X}'_{i,t}\lambda + d_i + d_t + \ln\left(RR^A_{i,t}\right) + u_{i,t}$. We also assume that $E\left[Intervention_{i,t}u_{i,t}\right] = 0$ and $E\left[Pacified_{i,t}u_{i,t}\right] = 0$, that is, the treatment and $u_{i,t}$ are independent. In other words, $Accident^R_{i,t}$ is correlated with the treatment only via $RR^A_{i,t}$, the reporting rate. The policy otherwise has no direct effect on the underlying number of accidents.

We then postulate that the reporting rate $RR^j_{i,t}$ for a given event j (crimes or accidents) can be multiplicatively decomposed into three components: $\ln\left(RR^j_{i,t}\right) = \ln\left(RR^j_i\right) + \ln\left(RR^j_t\right) + \ln\left(RR_{i,t}\right)$. The first is specific to one event category and one UPP and is constant over time, the second captures the common time trend for all UPPs of the reporting rate of one event category, and the third is time varying and specific to a given UPP but covers all types of crime equally. In other words, only the UPP-specific time-varying part of the reporting rate, $RR_{i,t}$, is common to all categories of crime and accidents and is affected by the policy in the same proportion. This assumption allows the reporting rate to vary over time, across events, and across UPPs.[19]

The term $\ln\left(RR^j_i\right)$ is constant within a UPP and is absorbed by the UPP fixed effect; equally, $\ln\left(RR^j_t\right)$ is absorbed by the common time period indicator.

[19] For example, the Maria da Penha Law that was introduced in Brazil in 2006 to reduce male domestic violence may have increased the reporting rate of rapes without affecting that of other types of crime. In another context, Gibson and Kim (2008) and Vollaard and Hamed (2012) provide empirical evidence of different underreporting intensities by type of crime.

For simplicity of presentation, we have not changed the notation of the fixed effects, although these do now include these reporting rate components. We obtain the two following specifications:

$$\ln\left(Crime_{i,t}^{C,R}\right) = \alpha\, Intervention_{i,t} + \beta\, Pacified_{i,t} + \mathbf{X}_{i,t}'\theta + \nu_i + \gamma_t + \ln\left(RR_{i,t}\right) + \epsilon_{i,t} \text{ and} \tag{4}$$

$$\ln\left(Accident_{i,t}^{R}\right) = \mathbf{X}_{i,t}'\lambda + d_i + d_t + \ln\left(RR_{i,t}\right) + u_{i,t}. \tag{5}$$

Combining equations (4) and (5) yields

$$\ln\left(Crime_{i,t}^{C,R}\right) - \ln\left(Accident_{i,t}^{R}\right) = \alpha\, Intervention_{i,t} + \beta\, Pacified_{i,t} + \mathbf{X}_{i,t}'(\theta - \lambda) + (\nu_i - d_i) + (\gamma_t - d_t) + \left(\epsilon_{i,t} - u_{i,t}\right). \tag{6}$$

From our previous assumption, $Intervention_{i,t}$ and $Pacified_{i,t}$ are not correlated with the new residual $\epsilon_{i,t} - u_{i,t}$ so that α and β are identified. This solution is easy to implement and amounts to deflating the treatment effect for each underreported crime by the increase in the accident reporting rate. To do so, we simply subtract the log of the accident rate from the log of the crime rate.[20]

A Bounded Variation Approach.—The first correction method assumes that the UPP-specific time-varying component of the reporting rate is the same for all underreported crimes. This assumption may be considered to be strong. The reporting rate of thefts could have been less affected by pacification than that of accidents,—or the reporting rate of assaults more affected. Our second correction method relaxes this assumption using treatment bounds in a partial-identification approach, following Manski and Pepper (2000).

Formally, we first estimate the rise in the accident reporting rate as a benchmark. We then assume that the reporting rate of each crime increases after pacification by a ratio Δ relative to that for the reporting of accidents. Online Appendix D provides additional details on this approach. We obtain bounds on the effects by varying the value of Δ. For instance, when $\Delta = 1$, the reporting rate of crime C is assumed to rise in line with that of accidents; when $\Delta = 2$, it increases twice as much as that of accidents; and when $\Delta = 0$, the reporting rate of this crime is unaffected by pacification.

[20] Online Appendix C presents an alternative solution for the bias that is similar in spirit to this correction. This alternative relaxes the assumption that the proxy variable is not directly affected by the policy and allows pacification to have a direct effect on accidents, but constrains this effect to be identical for fatal and nonfatal accidents. This approach yields similar conclusions.

III. Main Results

This section first presents the estimated effects of pacification on crime from OLS regressions without any correction for reporting, and then those that apply our two solutions.[21] We also consider a number of socioeconomic characteristics that may influence the effect of pacification.

Results without Correction for Reporting.—Panel A of Table 3 reports the estimated coefficients without correcting for the endogenous rise in reporting. These cover the nine crime indicators presented in the data section as well as the total number of crimes. The murder rate fell by about 7 percent after pacification (but only at the 10 percent significance level), while the robbery rate fell by 13 percent. On the contrary, the assault, rape, theft, and threat rates rose by 104 percent, 14 percent, 28 percent, and 124 percent, respectively.[22] Police forces have also changed the way in which they work following pacification. The intensity of police actions rose by 112 percent, while the number of people killed by the police dropped by 15 percent. We also find that pacification began to produce effects as soon as the BOPE intervention phase started for most crime categories.

Results with Accidents as a Proxy for the Reporting Rate.—The results from the estimation of equation (6) using the number of accidents as a proxy for the unobserved reporting rate appear in panel B of Table 3. The estimated treatment effect when unobserved reporting is corrected (panel A versus panel B in Table 3) reveals an increase in the accident reporting rate following pacification (see Section IVB). The correction reduces (resp., increases) in absolute value the estimated effect for crimes that were positively (negatively) affected by pacification. The signs of some effects remain unchanged. The increase in the assault rate is now 66 percent, which is smaller than before but still strongly significant, and the reduction in the robbery rate is now -29 percent and more significant. Some other results are substantially affected by this correction. While the theft and rape coefficients are still positive, they are no longer significant, and the effect on extortion is now significantly negative. The overall number of crimes rose significantly by 47 percent inside pacified areas when we correct for the reporting rate. All of our estimates are similar if we do not include UPP linear time trends, except that the negative effect on robbery is no longer significant without correction for the reporting bias and the negative effect on rape is significant with correction for the reporting bias (see online Appendix F).

Results Using Bounded Variation Assumptions.—Using the result that the accident reporting rate increased by 23 percent following pacification (see below in Section IVB), we estimate equation (6) assuming that the reporting rate of crime C

[21] The crime data at detailed geographic levels generally contain many zeros. For the remainder of our analysis, we will add a constant $c = 0.5$ to all crime observations in order to use the log-transformation in OLS regressions. We justify the choice of this constant in online Appendix E and show that the results are robust to the use of other positive constants.

[22] An estimated coefficient for crime C equal to $\hat{\beta}$ means that reported crime C varies by $100 \times \left[\exp\left(\hat{\beta}\right) - 1\right]\%$ with pacification.

Table 3—Baseline Results with and without Correction

	Murder	Assault	Robbery	Theft	Extortion
Panel A. Without correction for reporting bias					
Intervention	−0.0216	0.548	−0.00844	0.315	0.0362
	(0.0487)	(0.123)	(0.110)	(0.0885)	(0.0262)
Pacified	−0.0688	0.715	−0.138	0.245	0.0157
	(0.0340)	(0.0943)	(0.0653)	(0.0671)	(0.0170)
	Police action	Police killings	Threats	Rape	Total events
Intervention	0.812	−0.0899	0.716	0.135	0.552
	(0.156)	(0.0413)	(0.124)	(0.0523)	(0.0950)
Pacified	0.751	−0.165	0.806	0.129	0.591
	(0.129)	(0.0424)	(0.0925)	(0.0340)	(0.0662)
	Murder	Assault	Robbery	Theft	Extortion
Panel B. With correction for reporting bias					
Intervention	N.R.	0.539	−0.0174	0.306	0.0272
		(0.130)	(0.142)	(0.101)	(0.0660)
Pacified	N.R.	0.509	−0.344	0.0387	−0.190
		(0.104)	(0.0844)	(0.0740)	(0.0589)
	Police action	Police killings	Threats	Rape	Total events
Intervention	N.R.	N.R.	0.707	0.126	0.543
			(0.124)	(0.0715)	(0.108)
Pacified	N.R.	N.R.	0.601	−0.0767	0.385
			(0.114)	(0.0589)	(0.0825)
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	4,218	4,218	4,218	4,218	4,218

Notes: This table presents the treatment effects from the estimation of equation (1) in panel A (no correction) and equation (6) in panel B (with correction via the proxy variable) for the crime indicators at the head of each column. This correction is not applied for murders, police actions, and police killings, which are assumed to always be reported. All regressions include UPP fixed effects, month fixed effects, and linear time trends specific to each UPP. The observation unit is the UPP × month, and the sample includes observations for all UPP located in Rio de Janeiro between January 2007 and June 2016. N.R. indicates that the reporting-bias correction is not relevant for that crime. Standard errors clustered by UPP appear in parentheses.

increases by a ratio of Δ relative to this rise. We consider Δ values at 0.25 intervals in the $[0,5]$ range. This wide range is chosen so that we can identify the relative increases in the reporting rate that would lead to a reversal of the estimated effects.

The results for the crimes that may be underreported are plotted in Figure 3. The estimated effects turn out to be sensitive to the assumed value of Δ, which is to be expected given the wide range that we analyze. The treatment effect is overall very likely to be positive for assaults and threats, even with variations in the reporting rate that are very different than those of accidents. For instance, the reporting rate of assaults would have had to increase by five times more than that of accidents for the estimated treatment effect to turn negative, which seems unlikely. Similarly, the likely treatment effect is negative for robberies. On the contrary, the treatment effects for rape and theft seem more uncertain. With a rise in their reporting rates

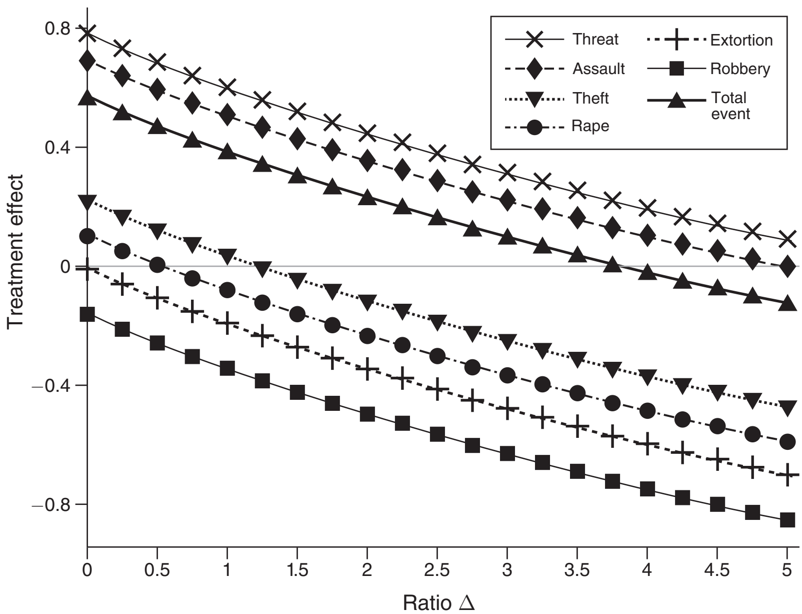

Figure 3. Bounded Variation Assumptions

Notes: Δ represents the ratio of the change in the reporting rate for crime C relative to that for accidents. The points of the figure refer to the treatment effects from the estimation of equation (6) where Δ is relative to the 23 percent rise in the reporting of accidents. The ratio Δ is in 0.25 intervals in the $[0,5]$ range. The curved lines correspond to the different crimes that are potentially underreported. All regressions in this figure include UPP fixed effects, month fixed effects, and linear time trends specific to each UPP. The observation unit is the UPP $\times$ month, and the sample includes observations for all UPPs located in Rio de Janeiro between January 2007 and June 2016.

close to that of accidents (i.e., with Δ between 0.5 and 1.5), their treatment effects can be either positive or negative.

Heterogeneity.—We now consider potential heterogeneity in the effect of pacification according to the favelas' socioeconomic characteristics. We are not interested in the direct causal effect of these characteristics on crime, which would be challenging to establish. We instead analyze their interaction effect with the treatment, controlling for unobserved fixed heterogeneity that could produce higher or lower values for these socioeconomic variables. In online Appendix G, we show that homeownership, literacy, and income levels seem to improve the efficiency of the policy. More educated individuals may better understand that it is in their interest to react positively to pacification, and homeowners may watch over their neighborhood more closely following pacification, which could prevent some crimes, especially murders and robberies, from occurring. In addition, favelas whose inhabitants have better job prospects may have lower crime rates.

IV. Robustness

This section presents a number of robustness tests to assess the validity of our results.

A. *Dynamic Effects*

One threat to the identifying assumptions behind the causal effect of the policy is that pacification took place earlier in favelas with higher homicide rates or particular trends for a given crime. To account for these concerns, we have included UPP fixed effects and linear trends specific to each UPP. Another concern could be that the authorities decided to first pacify the favelas with rising (or falling) crime rates in a nonlinear way. In this case, the usual diagnostic is to check whether the timing of the policy is correlated with the (nonlinear) trends in crime rates *before* the policy actually took place. The presence of pre-event trends could indicate reverse causality and invalidate the exogeneity assumption for pacification timing. Conversely, the absence of any pre-trends is evidence in favor of the exogeneity assumption regarding policy timing.

We test this assumption via an event study specification, which allows us to nonparametrically estimate the presence of pre-trends associated with the policy while controlling for other factors. The pre-trend estimation can be interpreted as a placebo regression, where current crime indicators are regressed on future pacification. This empirical approach exploits the staggered timing of pacification to estimate the dynamic effects of pacification over time. The fact that some favelas were already pacified while others were pacified later allows us to separately identify the UPP fixed effects, calendar-time fixed effects, and time fixed effects relative to the date of pacification (relative time). As the event studies will mainly be used to test for pre-trends, there is no need to correct for the reporting rate, as the latter only changes once the policy is introduced. We aggregate observations at the quarterly level to smooth out monthly variations and estimate the following equation:

$$\ln\left(Crime_{i,t}^{C,R}\right) = \nu_i + \gamma_t + \sum_{k=-12}^{-2} \pi_k \mathbf{1}\left\{t - T_i = k\right\} + \sum_{k=0}^{12} \tau_k \mathbf{1}\left\{t - T_i = k\right\} + \epsilon_{i,t}. \tag{7}$$

The dynamic effects of the policy over time are captured by the coefficients on the event-quarter dummies, $\mathbf{1}\{t - T_i = k\}$, which are equal to 1 when the quarter t is k quarters away from T_i, the date of the BOPE intervention in UPP i, with $k = -12, \ldots, 0, \ldots, 12$. We bin up the event-quarter dummies beyond some range when they are not well balanced: observations more than 11 quarters before or over 11 quarters after the BOPE intervention are captured by the $\mathbf{1}\{t - T_i \leq -12\}$ and $\mathbf{1}\{t - T_i \geq 12\}$ dummies, respectively. The event-quarter dummy $k = -1$ is omitted from the specification, so the other coefficients should be interpreted with respect to that period. The coefficients π_k capture the changes in crime rates of the future treated areas *before* the BOPE intervention: these allow us to evaluate the pre-event trends. In the absence of a trend, they should all be zero. The coefficient τ_k measures the evolution of crime rates k periods *after* the areas were pacified, which illustrates the dynamic effect of the policy. Last, ν_i and γ_t are UPP and quarter fixed effects, respectively. In an additional test, we restrict the event-quarter dummy $k = -12$ to be zero, as recommended in Borusyak and Jaravel (2018), since the dynamic treatment effects are only identified up to a linear trend. This is roughly equivalent to rotating the graph of coefficients around the first lag to set the linear pre-trend to zero.

The results are depicted in Figures 4 and 5; these include two panels, A and B, according to whether the event-quarter dummy $k = -12$ is restricted to be zero or not. There are no pre-event trends so that among the pacified favelas, the date of pacification would be independent of any significant pre-trends in crime after controlling for time-invariant characteristics and a common time trend. Tests for the joint significance of the pre-trends produce insignificant test statistics except for police actions and robberies in panel A and extortion, murders, and police actions in panel B, where we can reject the null hypothesis at the 5 percent level. However, the visual inspection of the graphs shows that this pre-trend is only slight compared to the effect that we estimate post-pacification.

The event studies show that the increase in some of the crime indicators is unlikely to result only from greater crime reporting. For instance, the assault rate rises immediately and sharply after the BOPE intervention, which is difficult to reconcile with the slow process of restoring trust in the police that would be necessary for inhabitants to increase their crime reporting.

The dynamic effects confirm most of the results in Table 3. However, the result for murders is mixed, as it reveals no effect, which casts doubt on the estimated fall after pacification in Table 3. The results for robbery are also questionable, but it should be remembered that these are not corrected for the change in reporting. Consistent with the bounded variation results, the dynamic treatment effects for robberies become more negative when we assume an increase in reporting. For instance, when we increase the reporting rate by 23 percent after pacification, the fall in robberies appears to be significant and robust (see online Appendix Figure H.1).

We also investigated whether heterogeneous treatment effects and the issue of negative weights—identified, for instance, by Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfœuille (2020)—might affect our results. This is a relevant question given the heterogeneity in the number of police officers per resident deployed in the UPPs. We implement the dynamic treatment effect estimator proposed by de Chaisemartin and D'Haultfœuille (2020) for staggered-adoption designs. The results in online Appendix H.3 are very similar to those above. Last, we estimate the event studies via negative binomial regressions. These are not sensitive to the presence of zeros in the dependent variable and can be used to test the robustness of the findings to the constant that is added to all crime observations in the log regressions estimated via OLS. The results in online Appendix Figure H.4 confirm the main results.

B. *Accidents as a Proxy for the Reporting Rate*

Motives for Reporting Crimes and Accidents.—The mechanism underlying the reporting of an event is a priori the same for crimes and accidents. On the one hand, the perpetrator of the offense runs a legal risk: as for any crime, the perpetrator of an accident, even if involuntary, is subject to criminal proceedings if there is an infraction. On the other hand, the victim seeks to declare the offense to

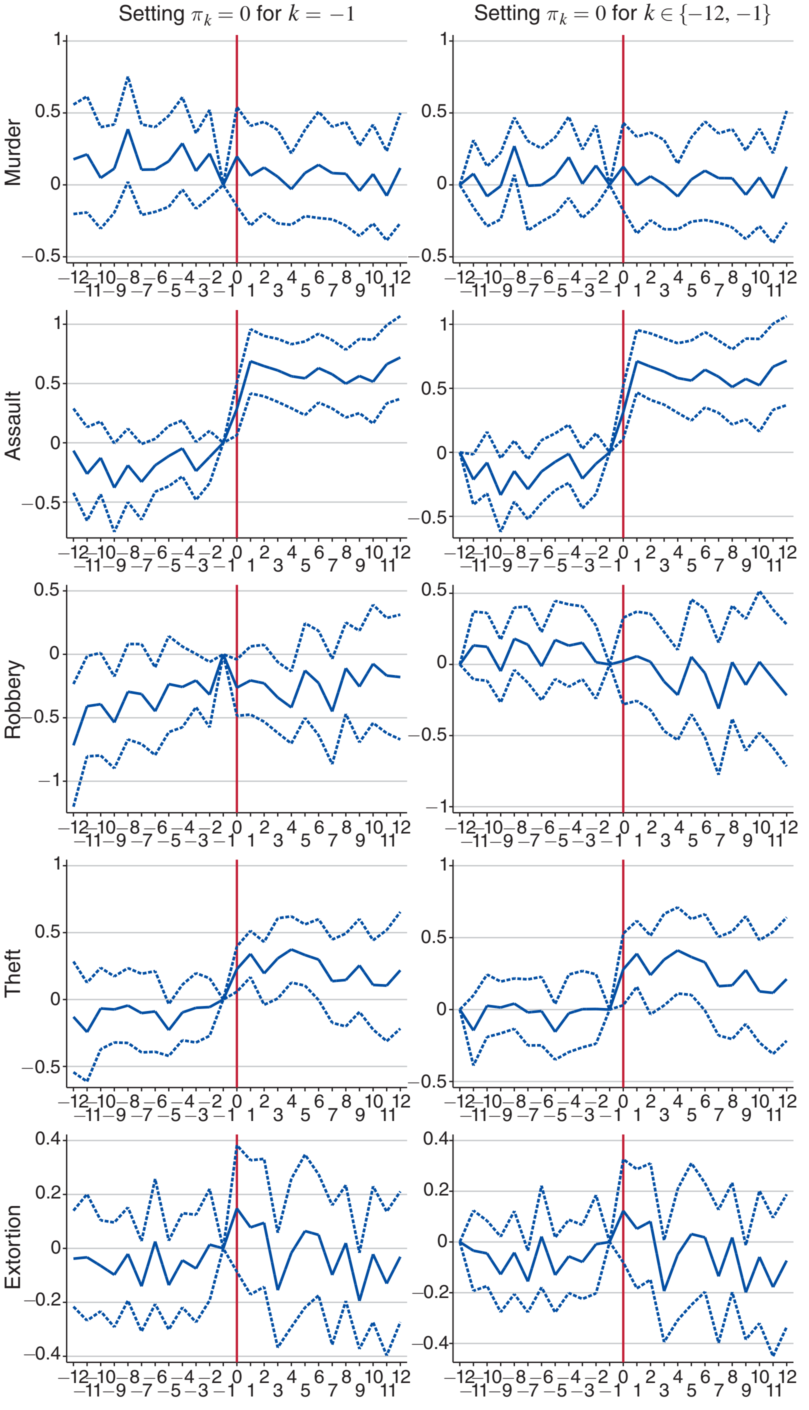

FIGURE 4. EVENT STUDIES FOR VARIOUS CRIME INDICATORS

Notes: These figures plot the quarterly crime rate, for different crime indicators, as a function of the time since the BOPE intervention. The solid lines correspond to the values of $\pi_k\,(k < 0)$ and $\tau_k\,(k \geq 0)$, as a function of *k*, obtained from the estimation of equation (7) on the sample of all UPPs for which $k \in [-12, 12]$. Standard errors are clustered at the UPP level, and dashed lines represent the 95 percent confidence intervals.

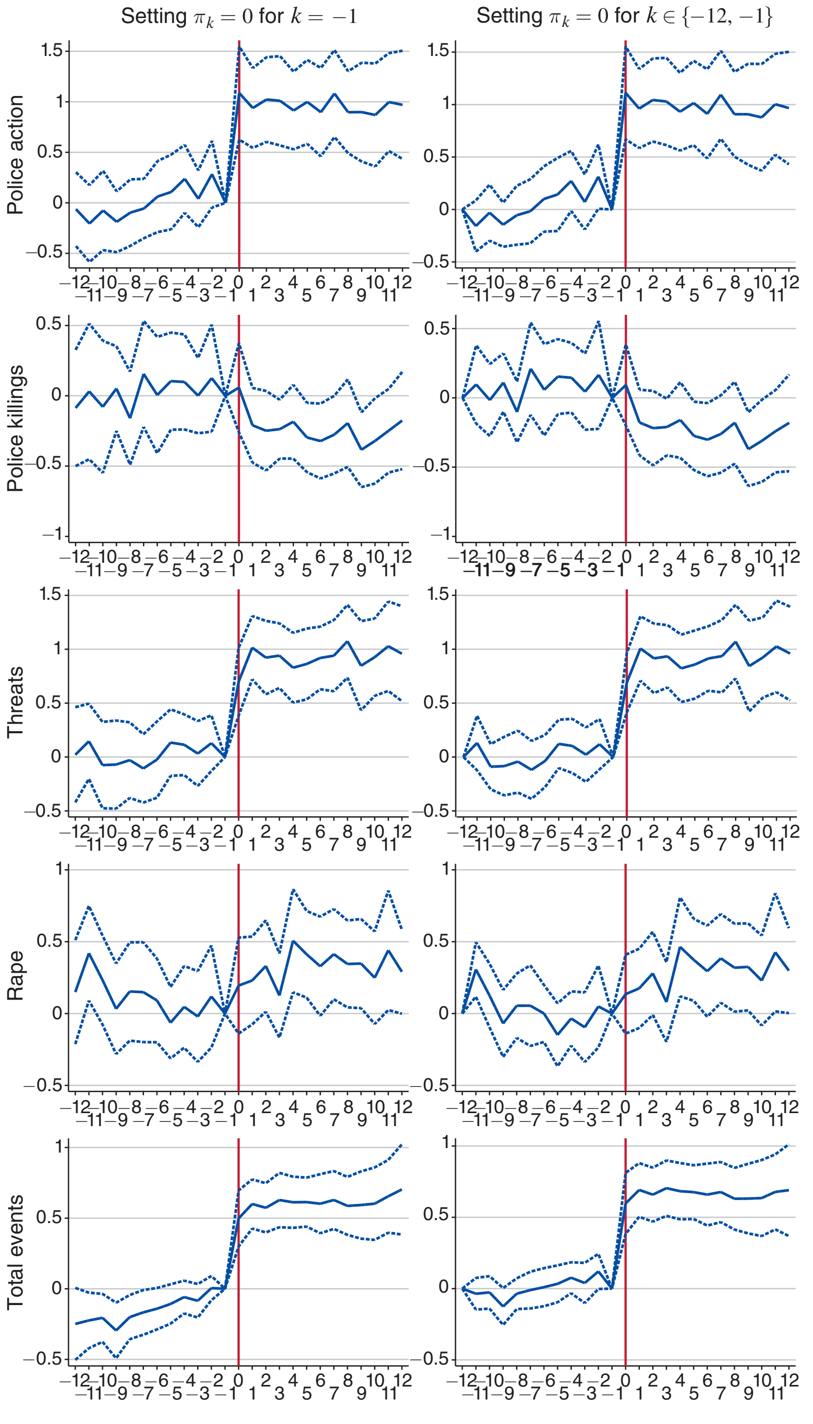

Figure 5. Event Studies for Various Crime Indicators

Notes: These figures plot the quarterly accident rate as a function of the time since the BOPE intervention. The accident rate includes both fatal and nonfatal accidents. The solid lines correspond to the values of π_k $(k < 0)$ and τ_k $(k \geq 0)$, as a function of k, obtained from the estimation of equation (7) on the sample of all UPPs for which $k \in [-12, +12]$. Standard errors are clustered at the UPP level, and the dashed lines represent the 95 percent confidence intervals.

obtain compensation, be it monetary or judicial. Favelas are poor neighborhoods, and many of their residents are not insured against theft, robbery, or street accidents.[23] As such, in the case of an accident, for example, there is no compensation for material damage, and hospital expenses will be limited to the amount covered by mandatory insurance (which is only small). Inhabitants may therefore declare these events (i.e., crimes and accidents) not for insurance reasons but for a sense of justice to see that perpetrator is sentenced and to be financially compensated following a court decision. In addition, crimes and accidents may both be underreported for fear of retaliation. This explanation is obvious for crimes and is also straightforward for accidents. As the perpetrator of an accident is likely to be uninsured and, thus, unwilling to pay the penalty if convicted, he or she will likely have a conflictual relationship with the victim.

Pacification Has No Effect on the Underlying Level of Accidents.—Using reported accidents as a proxy for the unobserved crime reporting rate requires that the policy only affect the number of reported accidents via the change in reporting. We test this assumption by analyzing the effect of pacification on accidents. We first consider all accidents: column 1 of Table 4 shows that these rose by about 23 percent. We then test whether this rise is higher in UPPs located in the south of Rio de Janeiro and close to the beach; these UPPs are in more central areas of Rio de Janeiro and are likely to attract more people (neighbors, tourists, etc.) and have more street activity, and thus accidents, following pacification.[24] Column 2 shows that there is no significant difference between the number of reported accidents in these UPPs and the others. The underlying level of accidents did not then increase more in areas where it may have been expected to do so.

To further rule out that pacification directly increased the underlying level of accidents, we separately estimate the effect of pacification on fatal and nonfatal accidents. As was the case for murders, fatal accidents are systematically reported to the police and are thus not susceptible to reporting effects. On the contrary, as with many other categories of crime, nonfatal accidents are presumably not always reported, as explained above. We find that pacification had no effect on reported fatal accidents (columns 3 and 4 of Table 4), while the reported rate of nonfatal accidents rose significantly (columns 5 and 6). There is no reason why any potential effect of pacification on street activity should affect nonfatal accidents but not fatal accidents. Fatal and nonfatal accidents do not reflect individual choice and are mostly random.

[23] Insurance coverage is low in Brazil. About 150 and 120 million Brazilians do not have health or life/personal accident insurance, respectively. In addition, about 38 million vehicles (70 percent) in Brazil are uninsured: about 90 percent of new vehicles are insured, but only 10 percent to 15 percent of those over ten years old. The price is the main reason for low insurance coverage: see *Jornal do Brasil* (https://www.jb.com.br/index.php?id=/acervo/materia.php&cd_matia=907566&dinamico=1&preview=1) and *Estadão* (https://jornaldocarro.estadao.com.br/carros/mais-de-30-milhoes-de-veiculos-rodam-sem-seguro-no-brasil/). Those living in favelas are presumably even less insured than those in the rest of the country. The economic development literature generally states that insurance coverage is low in developing countries due to informal insurance and the lack of a significant formal insurance market. For instance, Cole et al. (2013) identify liquidity constraints, lack of trust, and limited salience (as insurance companies do not reach out to favela residents). All of these factors are likely at play for Brazilian favelas.

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

Table 4—Treatment Effect on Accidents

	All accidents		Fatal accidents		Nonfatal accidents	
	(1)	(2)	(3)	(4)	(5)	(6)
Intervention	0.00899	0.0201	−0.00206	−0.00169	0.0177	0.0290
	(0.0506)	(0.0565)	(0.0310)	(0.0346)	(0.0490)	(0.0545)
Pacified	0.206	0.204	0.0113	0.0129	0.202	0.200
	(0.0565)	(0.0723)	(0.0311)	(0.0376)	(0.0564)	(0.0724)
Intervention × *Central Zones*		−0.147		−0.000000803		−0.153
		(0.125)		(0.0346)		(0.124)
Pacified × *Central Zones*		0.00787		−0.00706		0.0132
		(0.101)		(0.0308)		(0.100)
UPP fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
UPP linear time trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,218	4,218	4,218	4,218	4,218	4,218

Notes: This table presents the estimated treatment effect for all accidents (columns 1 and 2), fatal accidents (columns 3 and 4), and nonfatal accidents (columns 5 and 6) as the outcome variable. All of the regressions include UPP fixed effects, month fixed effects, and linear time trends specific to each UPP. The observation unit is the UPP × month, and the sample includes observations for all UPPs located in Rio de Janeiro between January 2007 and June 2016. Central denotes UPPs located in more central areas of Rio de Janeiro that are likely to attract more people and generate more street activity, and thus accidents, following pacification. Standard errors clustered by UPP appear in parentheses.

Last, we check for pre-trends for all accidents by estimating equation (7): the estimated coefficients are depicted in Figure 6. The left-hand panel A reveals a significant pre-trend. However, with individual fixed effects and calendar-time effects, the linear element of time-to-treatment effects is not identified, and only nonlinear pre-trends are an issue for the identifying assumption. When we additionally impose that the coefficient $\pi_k = 0$ for $k = -12$, which amounts to restricting the linear trend to be zero, the right-hand panel B confirms that there is no nonlinear pre-trend (the joint significance of the pre-trends is rejected at the 5 percent level).

The above evidence is convergent in showing that pacification had no direct effect on accidents, so increased reported accidents is a good indicator of reporting, which we estimate to have risen by 23 percent in pacified areas. It is difficult to gauge whether this is a large increase. In other circumstances, reporting could have risen more as residents have been wary regarding pacification. Although the police encourage residents to report crime, there was a long tradition of distrust of the police prepacification, particularly due to police violence in favelas (Human Rights Watch 2009; Amnesty International 2015).

Moreover, many favela residents suspected that the policy would only be temporary and would probably be discontinued after the Olympic Games (Musumeci 2017; Ribeiro and Vilarouca 2018).[25] Despite the end of the intimidation of armed gangs, some residents may thus consider it risky to switch sides and cooperate with the police.

[25] This fear of only temporary pacification turned out to be justified. In 2018, the minister of public security announced that nearly half of the UPPs will be either deactivated or integrated into other units (see https://www1.folha.uol.com.br/cotidiano/2018/04/upps-serao-extintas-para-reforcar-policiamento-de-outras-regioes-do-rio.shtml).

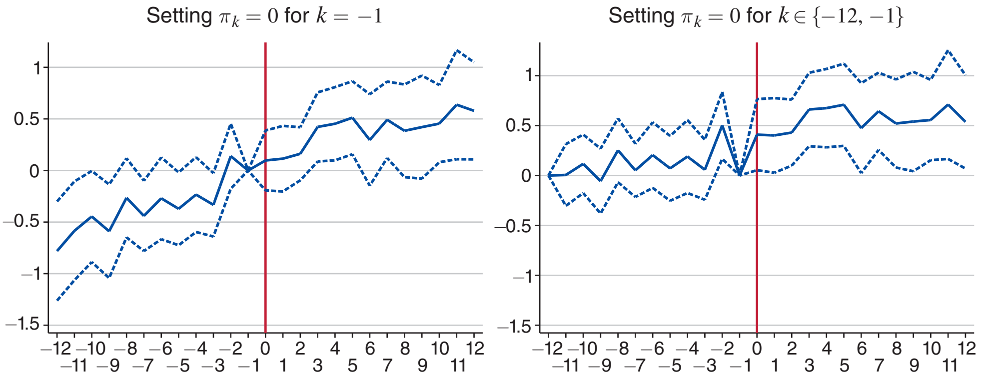

Figure 6. Event Studies for Accidents

Notes: These figures plot the quarterly crime rate for different crime indicators as a function of the time since the BOPE intervention. The solid lines correspond to the values of π_k $(k < 0)$ and τ_k $(k \geq 0)$, as a function of k, obtained from the estimation of equation (7) on the sample of all UPPs for which $k \in [-12, +12]$. Standard errors are clustered at the UPP level, and the dashed lines represent the 95 percent confidence intervals.

C. *Spillovers between Favelas*

Gang members who are driven away from pacified favelas may simply move to not-yet-pacified favelas controlled by the same gang, or they could engage in a turf war against rival gangs. In this case, the control group is also affected by the treatment, and our conclusions may be misleading (Miguel and Kremer 2004). For instance, the conclusion of an apparent fall in murders may result not from a lower figure in pacified favelas but rather a rise in favelas that were not yet pacified.

We investigate this mechanism by making use of the pacification of the Cidade de Deus favela, which started in November 2008. Controlled by CV, Cidade de Deus was the first large favela, with 40,000 residents, to be pacified in Rio de Janeiro. This represented a significant shock that could potentially spill over to not-yet-pacified favelas. We identify this spillover by comparing the crime rates in favelas controlled by either CV or rival criminal gangs where pacification had not yet started, before and after the date at which the BOPE entered Cidade de Deus.[26] We carry out this test by estimating the following equation:

$$(8) \qquad \ln\left(Crime_{i,t}^{C,R}\right) = \nu_i + \gamma_t + \rho\, CV_i \times CidadeDeDeus\_Pacification_t + \mathbf{X}_{i,t}'\theta + \epsilon_{i,t},$$

where $CidadeDeDeus\_Pacification_t$ indicates that the pacification of Cidade de Deus had started (i.e., the BOPE had entered) and CV_i indicates whether the UPP i was controlled by CV before its pacification. We focus on the potential short-term effects of this shock one year after it occurred to avoid picking up the effects of

[26] This test cannot identify any spillover that equally affected unpacified favelas controlled by CV and those controlled by rival criminal gangs. However, this case seems fairly unlikely.

TABLE 5—SPILLOVER EFFECTS BETWEEN FAVELAS FOLLOWING CIDADE DE DEUS PACIFICATION

	Murder	Assault	Robbery	Theft	Extortion
CV × CidadeDeDeus_Pacification	−0.135	0.220	−0.296	−0.404	0.0210
	(0.197)	(0.136)	(0.253)	(0.116)	(0.0435)
	Police action	Police killings	Threats	Rape	Total events
CV × CidadeDeDeus_Pacification	−0.197	−0.00188	−0.00894	0.0940	−0.195
	(0.280)	(0.142)	(0.230)	(0.135)	(0.0985)
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	1,116	1,116	1,116	1,116	1,116

Notes: This table presents the estimated ρ parameter in equation (8) for the different crime indicators at the head of each column. All of the regressions include UPP fixed effects, month fixed effects, and linear time trends specific to each UPP. The sample includes all UPPs located in Rio de Janeiro that the BOPE had not started to pacify by the end of 2009. We drop observations after 2009. Standard errors clustered by UPP in parentheses.

future pacifications, and drop observations after 2009. We keep only the favelas that the BOPE had not yet started to pacify by the end of 2009, leaving us with information on 31 UPPs. As these favelas had not yet been pacified, there is no reason for the reporting rate to vary with the treatment variable.

Table 5 presents the estimated coefficients from equation (8). There are no clear spillovers between favelas from the pacification of Cidade de Deus, except for theft, which fell.

We further consider the effect of the pacification of the headquarter of CV, which started in November 2010. We again find no significant spillovers effects on the not-yet-pacified favelas (see online Appendix Table I.1). Favela pacification could also lead gang members to move back to the gang's headquarters. We therefore estimate equation (6) without the UPPs that contain gang headquarters.[27] Our main results are very robust to this omission (see panel A of online Appendix Table I.2). Last, gang members in pacified favelas may try to extend their territory by moving into areas with unclear status, such as contested favelas. If these spillovers are substantial, our main results should be affected by the removal of these favelas: this turns out not to be the case (see panel B of online Appendix Table I.2). These tests confirm the absence of any spillover effects between favelas that may affect our findings.

It is important to note that we find no significant spillover effects within the set of favelas that are pacified at the end of the study period. Our estimated treatment effects are therefore not biased by spillovers, which is what matters for the conclusions to be correct. This implies not that gang members did not move within favelas but rather that there was no meaningful subsequent effect on crime. This equally does not imply that they did not move to favelas that were never pacified. For example, Magaloni, Franco-Vivanco, and Melo (2020) analyze data covering all Rio de Janeiro favelas, including some surrounding the Rio municipality, and uncover evidence of negative spillovers, while our analysis is restricted to pacified favelas.

[27] The UPPs containing headquarters are Fazendinha, Nova Brasilia, Adeus Baiana, Alemao, Chatuba, Fe Sereno, Parque Proletario, and Vila Cruzeiro for CV and Rocinha for ADA.

Some observers have indeed suggested that pacification pushed drug traffickers to favelas located outside of the city.[28]

D. *Additional Robustness Checks*

We have carried out a number of additional robustness checks. First, the date at which a UPP is considered to be pacified may be thought to be endogenous and result from governmental choice. However, this date primarily reflects the duration of the BOPE intervention, which varies substantially due to the heterogeneity of gang resistance. We tackle this potential endogeneity via a robustness test where we set the treatment variable at the beginning of the BOPE intervention instead of the pacification date; this does not change our results (see online Appendix J). Second, our analysis contains a limited number of clusters (37 UPPs), which can be a problem for statistical inference. We thus apply the wild cluster bootstrap procedure proposed in Cameron, Gelbach, and Miller (2008) and run a randomization test, following Fisher (1935), which is considered to perform well with a small number of clusters. The results from these tests confirm those using clustered standard errors (see online Appendix K). Third, official crime data may be manipulated by the police to artificially embellish the effectiveness of pacification; online Appendix L provides evidence against any obvious manipulation. Fourth, we adopt alternative empirical approaches to test the robustness of the main specification (see online Appendix M). To avoid issues regarding the log of zero, we estimate equation (6) both without the logarithmic transformation and via pseudo-Poisson maximum likelihood (see Bellégo, Benatia, and Pape 2022). The findings remain essentially unchanged. The use of a first-difference estimator also produces similar point estimates and provides evidence in favor of the strong exogeneity assumption associated with our empirical strategy. Fifth and last, we show that the results continue to hold if we remove Batan and Cidade de Deus, the first favelas to be pacified, which lie in a different area of the city (see online Appendix N).

V. Mechanisms

One clear goal of pacification was to break the paradigm of areas controlled by gangs with weapons of war.[29] By eliminating some part of the gang's organization, confiscating weapons in circulation, and reinstating a police presence, the policy should have produced a less criminogenic environment and a general fall in crime. Our results instead reveal that the effect of pacification on crime is not unequivocal. Crimes strongly associated with gang activities, such as murder and extortion, do seem to have declined, but other types of crimes, such as assaults and threats, have increased. In what follows, we investigate one mechanism and discuss two others that may explain this pattern.

[28] See https://www.insightcrime.org/news/analysis/rio-de-janeiro-stray-bullet-problem-resurges and https://www.insightcrime.org/news/analysis/rio-pacification-pushed-crime-to-city-limits.

[29] This was clearly stated by the state secretary of public security, José Mariano Beltrame, during interviews. See, for instance, http://www.theguardian.com/world/2010/apr/12/rio-de-janeiro-police-occupy-slums or https://www.nytimes.com/2010/10/11/world/americas/11brazil.html.

A. *The Gang Governance Effect*

Drug gangs often seek community cooperation to guarantee the safety and profitability of their illicit business. In particular, reducing other crimes helps not to scare customers and prevents the police from entering the gang's territory, therefore allowing gangs to carry out their activities as they wish. Many studies have documented how gang authority is exercised over residents in practice (e.g., Dowdney 2003; Arias 2006a, b; Penglase 2014). On the one hand, drug gangs run an expedient, harsh, and arbitrary justice system that punishes all those designated as guilty of an offense. The punishments can range from warnings to beatings to execution, and are often public in order to serve as examples that reinforce their authority. On the other hand, drug gangs also play an important role in resolving neighborhood disputes and provide a form of welfare benefits through the free and illegal provision of facilities (water, electricity, internet, etc.) and financial support to those in need. At the same time, favela residents rarely perceive the state as being a protective force (Wolff 2015). They express a lack of trust in the police and a strong sense of discrimination that is based on their place of residence and skin color (Perlman 2010). The excessive use of violence by the police, including extrajudicial executions (Human Rights Watch 2009), has considerably undermined the legitimacy of the police and may have increased the power of drug traffickers (Soares 2000; Arias and Rodrigues 2006).[30]

Due to their strong local social involvement, drug gangs were probably better than the police at maintaining order and deterring certain types of crimes, albeit with varying degrees of success. By chasing away gang members, pacification removed some of the crimes that were previously committed by gangs (the composition of which depended on the portfolio of gang activity). However, the diminished gang presence may lead to an increase in crime, as some inhabitants are now no longer discouraged from criminal activity.

The pacified favelas were either under the control of CV or ADA or were contested by different criminal groups (ADA, CV, TCP, or militias). Expert reports and academic research have shown that ADA and CV had different philosophies when imposing themselves on their territory. ADA, which is known to wield significant social power in the communities that it controls, attempts to gain support from residents and less often resorts to violence.[31] CV also provides a form of governance but seems to care less about security and residents' well-being (Magaloni et al. 2018) and is well known for extorting residents.[32] For instance, CV is the only criminal gang in Rio de Janeiro that allows the sale of crack cocaine on its turf,[33] even though this has negative social impact and encourages both violence and property crime (De Mello 2015; Baumer et al. 1998). In the same vein, Rocinha (Rio's

[30] Hundreds of individuals, mostly young, Black, and favela residents, are killed by the police every year in Rio de Janeiro (Human Rights Watch 2009; Amnesty International 2015). This is partly due to the law giving Brazilian police exceptional leeway in invoking self-defense in the case of murder charges. As such, the police often enjoy impunity.

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

[32] https://www.justice.gov/sites/default/files/eoir/legacy/2014/09/29/red_command.pdf.

[33] See Bullock (2019) and https://www.insightcrime.org/news/analysis/south-america-drug-slums-jurisdiction-organized-crime.

biggest favela), which was under the control of ADA before pacification, used to be relatively peaceful and to function harmoniously (Rekow 2016; Glenny 2015). Following the arrest of the historical boss of Rocinha and then the gradual weakening of UPP activity, CV progressively took over control of Rocinha and has imposed a harsh system of extortion on economic activity.[34]

From this description of the different gangs' philosophies, we conjecture that gang governance was more effective in deterring residents' crime in favelas controlled by ADA than in those controlled by CV before pacification. This conjecture is supported by the descriptive statistics in Table 6. Prepacification, there was less violence in favelas controlled by ADA (column 1) than in those controlled by CV (column 3). This difference is particularly salient for assaults, robberies, and thefts.

We test this gang-governance effect by comparing the effects of pacification in areas controlled by CV and ADA, expecting less favorable outcomes in ADA-controlled areas. The governance system in favelas is likely to have less effect when gangs fight each other for control over territory (Wolff 2014), leading to violent crimes such as murders (see Table 6). To focus on the governance style of each gang, we only consider areas that were controlled by a single gang before pacification and drop the four UPPs that were contested prepacification. We then estimate a separate pacification effect of the two main types of gangs as follows:

$$\begin{aligned}(9)\qquad \ln\left(Crime_{i,t}^{C,R}\right) \;=\;& \alpha_1 Intervention_{i,t} + \alpha_2 Intervention_{i,t} \times ADA_i \\ &+ \beta_1 Pacified_{i,t} + \beta_2 Pacified_{i,t} \times ADA_i \\ &+ \mathbf{X}'_{i,t}(\theta - \lambda) + (\nu_i - d_i) + (\gamma_t - d_t) + (\epsilon_{i,t} - u_{i,t}).\end{aligned}$$

The coefficient β_1 shows the effect of pacification on CV territories, and β_2 any differential in ADA areas. CV governance probably also dissuades residents' crimes. We therefore can only identify the difference in governance between ADA and CV and may underestimate the true effect of gang governance.

Table 7 shows that the murder rate fell in CV areas but not in ADA areas, and assaults rose much more in the latter. Similarly, theft rose in ADA areas but not in CV areas. This suggests that ADA control deterred residents' crime, especially low-level crime (i.e., theft and assault), more than CV control did. The number of people killed by the police fell significantly more in the favelas controlled by CV, while the number of police interventions rose much more in ADA territories, in line with ADA's less confrontational approach to the police. To check the robustness of the results, we also estimate our main equation (6) separately for ADA and CV areas, which has the advantage of not constraining the other coefficients to be equal across these areas. This produces very similar results (see online Appendix O). Overall, these results provide evidence that gang governance plays an important role in dissuading favela residents from committing crimes.

[34] https://noticias.uol.com.br/cotidiano/ultimas-noticias/2017/09/26/com-extorsao-a-mototaxistas-quadrilha-de-rogerio-157-fatura-r-100-mil-por-mes.htm, https://theintercept.com/2017/09/25/rocinha-favela-rio-de-janeiro-violence, and https://www.lemonde.fr/m-actu/article/2017/11/24/rocinha-ou-l-impossible-redemption-des-favelas_5219552_4497186.html.

Table 6—Crime by Criminal Faction before Pacification

	Before pacification (2007–2008)					
	ADA		CV		Contested	
	Mean (1)	SD (2)	Mean (3)	SD (6)	Mean (5)	SD (6)
Murder	21.6	(18.1)	21.2	(16.7)	35.2	(15.8)
Assault	153.1	(87.8)	245.0	(149.9)	280.8	(70.1)
Robbery	170.0	(252.7)	433.2	(767.6)	179.3	(132.6)
Theft	124.7	(78.6)	286.9	(443.9)	128.5	(18.2)
Police action	203.2	(111.5)	419.5	(277.1)	384.1	(221.0)
Police killings	17.6	(9.0)	31.9	(27.1)	16.6	(9.3)
Threats	81.1	(32.4)	148.9	(134.6)	128.6	(15.5)
Rape	7.4	(4.2)	9.3	(8.7)	6.8	(5.0)
Extortion	3.1	(2.2)	4.1	(5.4)	4.2	(2.7)
Total events	863.8	(489.1)	1,759.7	(1,898.4)	1,302.5	(301.6)
Accidents	40.1	(37.4)	54.4	(65.9)	53.8	(43.7)

Notes: This table presents the annual mean value per 100,000 inhabitants for crimes over the 2007–2008 period (i.e., prepacification) according to the identity of the gang controlling the favelas before pacification, using monthly crime data on favelas covered by the 37 UPPs installed in Rio de Janeiro.

B. *Other Mechanisms*

Our main results indicate a shift from high- to low-level crime. For crimes against the person, the assault and the threat rates rise with pacification while the murder rate falls; for crimes against property, thefts increase (but not significantly) while there are fewer robberies. There are a number of channels through which criminal behavior may shift to less serious crimes, and here we discuss the confiscation of firearms and the marginal deterrence effect.

Firearm Confiscation.—One clear objective of the policy was to remove weapons from favelas. For instance, the BOPE intervention secured favelas by systematically searching for weapons caches. With fewer firearms in circulation, criminals will be less well armed and thus more likely to commit less risky crimes. In addition, criminal activity is less likely to result in a murder. Information on monthly firearm seizures by the police in each UPP suggests that the goal of weapon reduction in the pacified favelas was met, as the number of weapons seized by the police fell by over two-thirds between 2007 and 2014 (see online Appendix Figure P.1). In addition, dynamic treatment effects confirm that weapon confiscations increased by about 50 percent during the BOPE intervention and then dropped by about 50 percent, both relative to confiscation prepacification, indicating a lower quantity of firearms in circulation after pacification (see online Appendix Figure P.2). Firearm confiscations may thus play a role in explaining our results.

Marginal Deterrence.—Other mechanisms may drive criminals toward less serious crimes. In particular, criminals' choices partly reflect the legal punishment for different types of crime. As law enforcement rises with pacification, so does the probability of arrest after committing a crime. This in turn increases the relative cost of a high-level crime compared to a low-level one, as the former are more

Table 7—Results According Gang Control Prepacification

	Murder	Assault	Robbery	Theft	Extortion
Pacified	−0.0807	0.399	−0.337	−0.0530	−0.233
	(0.0464)	(0.122)	(0.0996)	(0.0833)	(0.0612)
Pacified × *ADA*	0.119	0.472	−0.0716	0.505	−0.0546
	(0.0687)	(0.209)	(0.254)	(0.242)	(0.160)
Bias correction	No	Yes	Yes	Yes	Yes
	Police action	Police killings	Threats	Rape	Total events
Pacified	0.609	−0.199	0.454	−0.117	0.300
	(0.144)	(0.0492)	(0.121)	(0.0699)	(0.104)
Pacified × *ADA*	1.049	0.165	0.458	0.0391	0.285
	(0.225)	(0.0569)	(0.425)	(0.111)	(0.212)
Bias correction	No	No	Yes	Yes	Yes
Intervention	Yes	Yes	Yes	Yes	Yes
Intervention × ADA	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	3,762	3,762	3,762	3,762	3,762

Notes: These are the regression results differentiating the treatment effect by the gangs that controlled the favelas before pacification. The favelas controlled by CV are the reference group. Contested favelas are excluded from the regressions. Standard errors clustered by UPP appear in parentheses.

severely punished by the law, an effect known as marginal deterrence (Stigler 1970; Mookherjee and Png 1994). This change in relative cost encourages criminals to switch from high- to low-level crimes. This mechanism is all the more likely in our context, as drug gangs closely monitor and severely punish petty crimes (Dowdney 2003; Arias 2006a). By contrast, the police undoubtedly sought to exert some control over petty crime postpacification, but the priority of their murder-oriented mission and the limited resources at their disposal have not enabled them to curtail low-level crimes (Arias and Barnes 2017).

C. *Who Commits Crimes?*

The nature of criminal activity changed after pacification, which raises the question of who commits crimes after pacification. Although we do not have conclusive evidence here, information from field studies combined with the mechanism we propose suggests that crime postpacification may be committed by both former and new criminals, with both responsible for increasing less serious crimes. Aggregate data cannot tell us how much of the rise in certain crimes reflects new offenders and how much reflects substitution to less serious crime by existing criminals. Weakened gang governance encourages new criminals but says nothing about which crimes they will commit. Conversely, firearm confiscation affects both old and new criminals and will lead both to commit less serious crimes. Similarly, marginal deterrence potentially applies to both old and new criminals and pushes both toward lower-level crime. In practice, Grillo (2013) and De Souza (2019), among others, note that not all gang members left after pacification and that new forms of criminality have emerged.

VI. Geographical Analysis

This section presents ancillary analyses exploiting the geographic aspect of the data. We first show that there are spillover effects *outside* the pacified favelas. We then consider how pacification affected the profile of murder victims and the weapons used to kill them in Rio de Janeiro.

A. *Spillovers outside Pacified Favelas*

Pacification may affect crime rates in neighborhoods close to favelas. Drug gangs are indeed often very active in the buffer zone outside the favelas, and these neighboring areas can sometimes be more dangerous than the favelas themselves (Barcellos and Zaluar 2014). In addition, some gang activities are not in the immediate vicinity of the favelas they control. Last, the elimination of gang governance may lead the favela inhabitants to commit more crimes inside the favelas rather than outside.

We test for spillovers in the rest of Rio de Janeiro using information from the ISP on the number of crimes at the district level from January 2007 to June 2016. Our dataset contains 38 districts.[35] These data can be used to determine the number of crimes in Rio de Janeiro outside of UPPs. To this end, we subtract the number of crimes in UPPs located within a district from the total number of crimes in that district. When a UPP is located in more than one district, the number of crimes is broken down across districts according to the UPP population share in each district. This procedure can produce some minor inconsistencies, such as a small negative value for some crime observations (which are replaced by zero) and noninteger crime numbers.

We construct the treatment variable using geospatial vector data at the census tract level.[36] We calculate the distance between the centroids of all the census tracts in Rio de Janeiro state and determine whether they lie inside an area covered by UPPs and to which district they belong (when they are in Rio de Janeiro). We then define Ω_d as the set of census tracts that are inside district d but not in the area covered by the UPPs and $\mu_{c,d}$ as the weight of census tract c in district d (i.e., the population of census tract c over the population of district d that lives outside the UPPs). N_c^k is the size of the population at the distance of k meters from census tract c, and $n_{c,t}^k$ is the size of the population that is pacified at date t, with $k \in$

[35] The ISP provides geospatial vector data for the areas covered by the districts. There were 42 districts in Rio de Janeiro in 2016. However, we merge districts 1 and 4, which border each other, because they are substantially smaller than the others. In addition, three police stations were opened during the period and combined with preexisting police stations that were previously responsible for the jurisdiction of the new stations. The mapping between the districts and UPPs of Rio de Janeiro appears in online Appendix Figure Q.1.

[36] Geospatial vector data and population at the census tract level are provided by the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatística 2010). There are 28,318 census tracts in the state of Rio de Janeiro, of which 10,504 are in Rio de Janeiro.

$\{0\text{–}3{,}000; 3{,}000\text{–}6{,}000; 6{,}000\text{–}9{,}000\}$.[37] The treatment variable $DistrictPacif_{d,t}^{k} = \left(\sum_{c\in\Omega_d}\mu_{c,d}n_{c,t}^{k}\right)/\left(\sum_{c\in\Omega_d}\mu_{c,d}N_{c}^{k}\right)$ is the average value of the percentage of the pacified population living at the range distance of k meters from district d at date t. This average value is calculated over the set of census tracts (weighted by their population) that are inside district d but outside of the area covered by the UPPs. We estimate the following equation:

$$\ln\left(Crime_{d,t}^{C,R,O}\right) = \nu_d + \tau_t + \sum_k \theta_k DistrictPacif_{d,t}^{k} + \epsilon_{d,t}, \tag{10}$$

where $Crime_{d,t}^{C,R,O}$ corresponds to the number of crimes that took place in district d but outside of the UPPs during month t. The spillover effect is identified by time series variations in the percentage of the population that is pacified and by their geographical dispersion relative to the districts. We do not correct for the unobserved reporting rate, as there is little reason for the reporting rate of crimes committed outside UPPs to change with the pacification policy.

Table 8 presents the estimation results from equation (10). The murder rate fell in the areas of districts that are located close to pacified favelas (between 0 and 3,000 meters), in line with many territorial conflicts taking place in areas surrounding favelas (Barcellos and Zaluar 2014). Similarly, there are fewer robberies and thefts outside UPPs in areas located close to pacified favelas. There are no significant spillovers in areas located more than 3,000 meters from the UPPs.

B. *Victim Profiles and Murder Weapons*

To obtain insights into the profile of potential victims and causes of death following pacification, we collected monthly data on the number of people who died following attacks in Rio de Janeiro from the Brazilian Health Ministry database Datasus (Ministério da Saúde n.d.). These data provide details over the January 2007 to June 2016 period on victim characteristics, such as gender and race, and the type of weapon used in the attack, which we use to study the effect of pacification on the types of murder.

The data do not indicate where the attack took place. However, when the death occurs in a hospital, we know the hospital's name and, therefore, its location.[38] We hereafter assume that the seriously injured are transferred from the site of the attack to the nearest (large) hospital, which allows us to consider the distance between the hospitals and the pacified favelas. We focus on large hospitals, as victims of violent assault are only rarely transferred to small hospitals given that these do not have intensive care units. This leads us to keep 17 hospitals in the database.[39] These

[37] The results are robust to other similar range distances. The lack of precision in the data prevents us from estimating the spillover effects at a finer geographical level. The geographical perimeter of a district is very large compared to a UPP, and there are not enough districts and UPPs to compensate for this coarseness. Defining finer treatment variables increases the correlation between them and produces multicollinearity issues. The chosen precision of the treatment variables reflects the trade-off between geographical precision and the strength of the correlation between them.

[38] About 37 percent of murder victims die in hospital.

[39] We define large hospitals as those with more than 45 total deaths over the 9.5-year study period.

Table 8—Spillover Effects outside Favelas

	Murder	Assault	Rape	Robbery	Theft
DistrictPacif					
0–3,000 meters	−1.402	0.149	−0.626	−0.907	−0.609
	(0.544)	(0.278)	(0.425)	(0.496)	(0.255)
3,000–6,000 meters	−0.0925	−0.257	0.623	0.693	−0.283
	(0.491)	(0.454)	(0.626)	(0.614)	(0.448)
6,000–9,000 meters	0.110	−0.201	−0.835	−0.0423	−0.265
	(0.729)	(0.515)	(0.628)	(0.664)	(0.586)
District fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
District linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	4,332	4,332	4,332	4,332	4,332

Notes: This table lists the estimated parameter θ_k, with $k \in \{0\text{–}3{,}000; 3{,}000\text{–}6{,}000; 6{,}000\text{–}9{,}000\}$, in equation (10) for the five different crimes at the head of each column. Crimes refer to those that took place in district d but not in the UPPs of district d during month t. All of the regressions include district fixed effects, month fixed effects, and linear time trends specific to each district. The observations cover all districts in Rio de Janeiro between January 2007 and June 2016. Standard errors clustered by UPP appear in parentheses.

hospitals account for 90 percent of the people who died in hospitals following an attack, and they experience an average of 30 deaths per year from violence (the analogous figure for small hospitals is only 0.5 deaths per year).

For each hospital, we calculate the percentage of the population that is pacified at a distance of k meters, with $k \in \{0\text{–}3{,}000; 3{,}000\text{–}6{,}000; 6{,}000\text{–}9{,}000\}$.[40] We define N_h^k as the total population living at a distance of k meters from hospital h and $n_{h,t}^k$ as the population that is pacified at date t within the same distance. The treatment variable is $HospitalPacif_{h,t}^k = n_{h,t}^k / N_h^k$, and we estimate the following Poisson regression:

$$death_{h,t} = \exp\left(\nu_d + \tau_t + \sum_k \theta_k HospitalPacif_{h,t}^k\right), \tag{11}$$

where $death_{h,t}$ corresponds to the number of deaths following assaults that occurred in hospital h during month t. We estimate this regression using the number of deaths of different types of individuals: everyone, men, women, White, and Black and mixed race. The pacification effect is identified by the time series variation in the percentage of the population living in pacified favelas and their geographical dispersion relative to the hospitals.

The results are presented in Table 9 for all types of deaths, deaths by firearms, and deaths other than firearm (e.g., with a knife). The number of these deaths in hospitals fell nonsignificantly for those that are close to pacified favelas (between 0 and 3,000 meters). Even if the effects are not significant, this decrease mostly concerned men (column 2) and is especially noticeable for deaths by firearms (panel B) but not for those killed in other ways (panel C). The drop in the number

[40] As in Section VIA, we use geospatial vector data and population at the census tract level provided by the Brazilian Institute of Geography and Statistics. We use centroids to calculate the distance between the census tracts and the hospitals.

Table 9—The Effect of Pacification on Individuals Who Died in Hospital Following an Assault

HospitalPacif	All (1)	Men (2)	Women (3)	White (4)	Black & mixed (5)
Panel A. All types of killings					
0–3,000 meters	−1.605	−1.735	−0.0556	−0.580	−2.175
	(1.120)	(1.184)	(0.943)	(1.191)	(1.232)
3,000–6,000 meters	−0.522	−0.503	0.0498	−0.261	−0.613
	(1.202)	(1.189)	(2.092)	(1.701)	(1.151)
6,000–9,000 meters	1.455	1.111	5.204	0.862	1.782
	(3.110)	(3.205)	(3.222)	(2.346)	(3.689)
Panel B. Killed by firearms					
0–3,000 meters	−1.942	−2.068	−0.342	−0.566	−2.624
	(1.377)	(1.416)	(1.576)	(1.576)	(1.548)
3,000–6,000 meters	−0.567	−0.413	−2.290	−0.757	−0.488
	(1.556)	(1.605)	(2.324)	(2.398)	(1.510)
6,000–9,000 meters	1.398	1.208	3.625	1.373	1.234
	(3.652)	(3.692)	(3.827)	(3.305)	(4.283)
Panel C. Killed by other methods (no firearm)					
0–3,000 meters	−0.349	−0.411	0.402	−0.696	−0.247
	(1.172)	(1.154)	(1.879)	(1.321)	(1.300)
3,000–6,000 meters	−0.263	−0.697	3.487	1.198	−0.852
	(1.367)	(1.719)	(3.953)	(1.311)	(1.787)
6,000–9,000 meters	1.163	0.0694	6.789	−1.494	2.751
	(2.924)	(3.096)	(6.031)	(3.629)	(3.445)
Hospital fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Hospital linear time trends	Yes	Yes	Yes	Yes	Yes
Observations	1,938	1,938	1,938	1,938	1,938

Notes: This table presents the estimated θ_k parameter, with $k \in \{0\text{–}3{,}000; 3{,}000\text{–}6{,}000; 6{,}000\text{–}9{,}000\}$, in equation (11) for three different types of death occurring in a hospital following an attack (all murders, murders with a firearm, and murders without a firearm) and for five different types of individuals (everyone, men, women, White, and Black and mixed-race). All of the regressions include hospital fixed effects, month fixed effects, and linear time trends specific to each hospital. Standard errors clustered by hospital appear in parentheses.

of deaths is mainly observed and is significant at the 10 percent level among Black and mixed-race people (column 5), who make up the majority of the population in the poor neighborhoods of Rio de Janeiro and its favelas.

This analysis suggests that the decrease in murders induced by pacification mainly concerns people assaulted with firearms and attacked in favelas or in poor neighborhoods near them. The confiscation of firearms could explain the decrease in the number of people killed with guns. Admittedly, these results could also be explained by lower gang activity in and around the favelas as gangs have been driven out. In practice, these results are probably explained by a combination of these two effects, but we are not able to disentangle them.

VII. Conclusion

The research described here has highlighted the adverse consequences of the war on crime in areas with little state presence: while the number of the most serious crimes fell, the number of other crimes rose strongly. We obtain these results using two simple solutions for endogenous changes in underreporting, which is an important step in the measurement of the consequences of crime policy. The first relies on a proxy variable, and the second is a complementary approach via bounded variation assumptions. These methods can be applied in numerous other situations where there are reporting issues. We then investigate the mechanisms that lie behind our results. Drug gangs rule over the territory they control, so driving them out can trigger crime waves. In addition, less serious crimes may be substituted for serious crimes, presumably because weapon confiscations lead to fewer serious crimes and the higher probability of arrest.

We have demonstrated the complexity of implementing crime policies in lawless areas without producing unintended consequences. These findings are therefore relevant for the design of these policies. Law enforcement by the police may be desirable to restore peace, and their efforts to seize firearms are valuable, as this does seem to reduce murders and weaken the ability of gangs to rule locally. However, it is important to focus not only on the most serious crimes. After reclaiming gang-controlled territory, the police may have difficulty containing the various channels that lead to higher crime and restoring order. In particular, less serious crimes should not be neglected, as they are numerous and the local population is the main victim. Governments should therefore pursue the objective of tackling minor crimes and thus gain the cooperation of the population, which is a condition for the success of such policies. Our results also suggest that it would be necessary to go beyond the mere restoration of the law by the police. To represent a valid alternative, the official judicial system needs to be made more efficient in order for it to compete with that offered by gangs.

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