INSPER - INSTITUTE OF EDUCATION AND RESEARCH
DOCTORAL PROGRAM IN BUSINESS ECONOMICS

ANDRÉ LUIZ PEREIRA MANCHA

Essays on criminal gangs, police and illegal markets.

São Paulo - SP
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2023

To my beloved Leonardo.

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"Every day you may make progress.
Every step may be fruitful.
Yet there will tretch out before you an ever-lengthening,
ever-ascending, ever-improving path.
You know you will never get to the end of the journey.
But this, so far from discouraging, only adds to the joy and glory of the climb."
(Winston Churchill)

Resumo

Um entendimento dos mecanismos que impulsionam o comportamento criminoso é crucial para a elaboração de políticas eficazes no combate ao crime e violência. Mais especificamente, facções criminosas podem explorar a ineficiência ou ausência do estado para expandir o controle territorial e conduzir atividades ilegais, como a venda de mercadorias roubadas e tráfico de drogas. Os três ensaios desta tese lançam luz sobre como a regulação de mercado e mudanças no patrulhamento policial afetam o crime e contribuem para formulação políticas de segurança pública.

Em particular, o primeiro artigo avalia o impacto de uma supervisão mais rigorosa de empresas de desmontagem de veículos ("desmanches"). Utilizando dados do estado de São Paulo e a variação na supervisão causada por uma regulamentação estadual, mostro que o roubo de automóveis diminuiu substancialmente mais em áreas com presença de desmanches. Este artigo destaca que os desmanches podem atuar como provedores de liquidez para gangues de roubo de veículos. Após um monitoramento mais rigoroso pelo estado, os criminosos enfrentam mais dificuldades em negociar veículos roubados, o que causa uma redução no roubo de automóveis.

O segundo ensaio investiga os mecanismos por trás de um aumento impressionante em homicídios após reduções abruptas no patrulhamento policial. Exploro a ocorrência de 29 greves da polícia militar em diferentes estados brasileiros entre 2000 e 2020. Além disso, utilizo informações sobre a presença de facções criminosas para investigar se o aumento no crime violento pode ser atribuída a esses grupos durante um período de menor vigilância policial. Meus resultados mostram que o aumento em homicídios é maior em áreas de intensos dominadas por gangues. Exercícios de robustez mostram que as mortes de suspeitos criminosos são o principal impulsionador desse aumento, e não há uma tendência prévia de violência antes da greve policial. Essas descobertas sugerem que a ausência do estado podem desencadear conflitos intensos pelo controle territorial. Apresento um modelo teórico para mostrar como mudanças abruptas no nível de patrulhamento afetam os incentivos das facções para iniciar uma guerra.

Por último, o terceiro ensaio aproveita a implementação gradual de esquadrões de elite da polícia em municípios do Ceará. O governo estadual decidiu expandir o batalhão especial da polícia militar (CPRaio) da capital para os municípios do interior. A ordem de implementação seguiu critérios populacionais, com municípios grandes recebendo esquadrões de elite da polícia antes dos municípios menores. Eu mostro que a implementação do CPRaio levou a uma grande redução em roubos, com resultados menores ou não significativos em outros crimes. Esses resultados, combinados com as descobertas do segundo ensaio, revelam que o aumento da presença policial pode ter um efeito diferente da diminuição na patrulha policial. Enquanto a expansão de policiamento altamente militarizado afetou principalmente roubos, a redução abrupta na vigilância levou a um aumento em homicídios.

Keywords: Crime, Market Regulation, Police, Deterrence, Natural Experiment.
JEL Classification: K20, K42.

Abstract

An understanding of the mechanisms that drive criminal behavior is crucial to designing effective policies to tackle property and violent crimes. More specifically, criminal gangs and drug trade organizations may explore the state's inefficiency or absence to expand territorial control and run illegal businesses such as the sale of stolen goods and drug trafficking. The three essays in this thesis shed light on how market regulation and shifts in police patrolling affect crime outcomes and provide valuable insights into security public policies.

In particular, the first paper evaluates the impact of more strict supervision of legal authorities on vehicle dismantling firms ("junkyards"). Using data from the São Paulo state and the exogenous variation in supervision caused by a state-level regulation, I find that auto theft decreased substantially more in areas with the presence of junkyards. This paper highlights that junkyards may act as liquidity providers to criminals specialized in the robbery of vehicles. Following harsher state monitoring, criminals face more difficulties in negotiating stolen vehicles, which causes a reduction in auto theft.

The second essay investigates the mechanisms behind an astonishing increase in homicides following abrupt reductions in police patrolling in Brazil. I explore the occurrence of 29 military police strikes in different Brazilian states between 2000 and 2020. Moreover, I use the information on drug trade organizations' presence to investigate if the shift in violent crime may be attributed to these groups during a period of lower police surveillance. My results show that the increase in homicides is larger in areas of intense gang conflicts. Robustness exercises show that the deaths of suspected criminals are the main driver of such an increase, and there is no pre-trend of violence before the police strike. These findings suggest the state's absence triggers intense conflicts for territory control. I present a novel theoretical model to show how abrupt shifts in police patrolling caused by police strikes affect criminal gang incentives to start a war.

Last, the third essay leverages the phased roll-out of police elite squads across municipalities in Ceará. The state government decided to expand the military police special battalion (*"CPRaio"*) from the capital to countryside municipalities. The roll-out order followed population criteria, with large municipalities receiving police elite squads prior to small municipalities. I show that the CPRaio roll-out led to a large reduction in violent property crime, with smaller or no significant results on other crimes. From a policy perspective, these results combined with the findings of the second essay reveal that increased police presence may have a different effect from decreased police patrolling. While the expansion of highly militarized policing affected primarily robberies, the abrupt decrease in surveillance led to an escalation of violent deaths.
Keywords: Crime, Market Regulation, Police, Deterrence, Natural Experiment.
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Introduction

Illicit markets and criminal activities pose significant challenges to societies worldwide. This dissertation delves into the intricate dynamics between market regulation, law enforcement strategies, and crime in the context of Brazil. Each chapter explores a distinct facet of this complex relationship, shedding light on the effectiveness of regulatory measures and law enforcement interventions.

The opening chapter investigates the impact of market regulation on the reduction of auto theft in Brazil. Focusing on the increased supervision of junkyards, the study explores the effects of state regulations imposing strict rules on the sale of recovered spare parts from vehicles. Empirical evidence drawn from data on auto theft and other crimes in São Paulo reveals a significant reduction in stolen vehicles following enhanced monitoring of junkyards. The findings not only underscore the role of monitoring capabilities in reducing crime but also highlight the localized impact in areas near junkyards, emphasizing the need for targeted policy measures.

The second chapter shifts the focus to the consequences of reduced police surveillance during police strikes in Brazil. Leveraging these strikes as a natural experiment, the study examines the link between policing and gang conflicts, with a particular emphasis on gang-related homicides. The findings demonstrate a substantial increase in violent deaths in areas disputed by criminal gangs during police strikes. The research contributes to the broader literature on state presence and organized crime, providing empirical evidence on the impact of policing on crime rates in an environment of intense gang confrontation.

The final chapter delves into the effects of a large-scale police reorganization in Ceará, Brazil. Analyzing the roll-out of elite police squads within military police battalions, the study assesses the impact of better salaries, training, and manpower on crime outcomes. The results reveal a significant decrease in violent property crime following the reorganization, emphasizing the nuanced effects of law enforcement strategies on distinct crime categories. Additionally, the cost-benefit analysis suggests that the investment in police reorganization yields substantial reductions in robbery rates, contributing valuable insights to the literature on the causal relationship between policing strategies and crime.

In tandem, these chapters provide a comprehensive exploration of the interplay between market regulation, law enforcement, and crime in Brazil. The findings contribute not only to academic research but also offer practical insights for policymakers grappling with the persistent challenges posed by criminal activities in the country.

We now move to presenting each paper separately. We start with "Dismantling a Market for Stolen Goods: evidence from the Regulation of Junkyards in Brazil"; then, we move to the two papers on police and crime with "When the State Steps Down: police strikes and gang-related deaths in Brazil"; finally, we conclude with "Police Reorganization and Crime: evidence from the military police in Ceará, Brazil".

1 Dismantling a Market for Stolen Goods: evidence from the regulation of junkyards in Brazil.

1.1 Introduction

The relationship between market regulation, firm supervision, and crime has received limited attention in the literature, despite the prevalence of illicit markets. Criminal organizations exploit the blurred lines between legal and illegal products to convert stolen goods into cash. When there are no consequences for buying or selling illegal products, some firms may choose to obtain their supplies from illegal sources to maximize profits. This raises the possibility that increasing barriers to the trade of illegal goods may impact criminal incentives (Becker, 1968).

This paper investigates the effect of market regulation on the reduction of auto theft. Specifically, I focus on the increased supervision of junkyards resulting from state and federal laws that impose strict rules on the sale of recovered spare parts from crashed and impounded vehicles. In this context, legislative changes and enhanced monitoring of junkyards may reduce criminal incentives by raising the costs associated with operating in the illicit market for spare parts.

To assess the causal effect of these policy changes on stolen vehicles, I evaluate the impact of the new legislation using panel data on auto theft and other crimes in São Paulo from 2011 to 2019. I exploit the exogenous variation caused by the regulatory approval for junkyards as a quasi-experiment. The federal government granted each state the autonomy to determine the manner and timing of implementing traceability measures for items sold by junkyards, resulting in significant variability in the extent of monitoring across different states. For instance, the state of São Paulo developed a comprehensive system that mandated the use of QR codes to track spare parts, while other states have yet to fully implement the federal law requirements for dismantling firms.

I present empirical evidence that market regulation and increased supervision of junkyards led to an average reduction of 4.35% in the number of stolen vehicles per year[1]. These results support the hypothesis that monitoring capabilities play a crucial role in reducing crime. Additionally, an analysis of junkyard locations and stolen vehicles in São Paulo suggests that the decline in auto theft is concentrated in neighborhoods near junkyards, highlighting the high incidence of auto theft in these areas. These findings have important implications for policymakers, indicating the need to invest in effective monitoring systems and target areas with high property crime rates.

[1] In the remainder of the paper *robbery* of vehicles refers to cars taken using violence, *theft* of vehicles to cars taken without the use of violence, and *auto theft* or *stolen vehicles* as the sum of robberies and thefts.

To ensure the validity of my results, I conduct a series of robustness checks. Firstly, event-study analyses reveal no pre-existing downward trend in stolen vehicles. Secondly, I demonstrate that the impact of the regulation is larger in neighborhoods with a larger incidence of junkyards, providing further evidence for the relevance of these firms in my identification strategy. Last, I perform falsification tests using other violent and property crimes as dependent variables. The results show a specific reduction in auto theft, ruling out the possibility of a general decrease in crime rates following the new regulation on junkyards. Thus, I provide compelling evidence that other crimes did not significantly change, and there was no displacement of offenses to other forms of robbery, highlighting the effectiveness of the regulation in reducing crime.

Additionally, I conduct robustness tests on the effect of junkyard locations and the dynamics of auto theft. I leverage the effect of the new regulation on junkyards that were forced to close, assessing the relevance of the locations of firms that faced more difficulties in staying in the market. The findings indicate that auto theft decreased by an additional 30% in districts near junkyards that were closed following the regulation, compared to the results considering all dismantling firms. While it cannot be entirely ruled out that these junkyards chose to close or relocate for reasons unrelated to the regulation, this result suggests significant heterogeneity in the decrease in stolen vehicles at the census tract level.

Finally, I test the effect of the institutional change on vehicle insurance prices to assess the effects on local citizens' welfare. Insurance prices reflect the willingness to pay to protect an asset from risks such as robbery and theft. I find that the regulation on junkyards reduced vehicle insurance prices by 7.09% given the 4.35% reduction in auto theft. These results suggest that enhanced supervision on the market for vehicle spare parts increased social welfare both reducing crime and insurance prices. The implied elasticity of 1.63 suggests that insurance prices are very sensitive to auto theft.

My findings contribute to the broader literature on the economic returns to crime and the effectiveness of legislative changes as crime control measures (d'Este, 2020; Draca, Koutmeridis, and Machin, 2019; Chimeli and Soares, 2017; Pereira and Pucci, 2022; Ayres and Levitt, 1998; Gonzalez-Navarro, 2013; Vollaard and Van Ours, 2011). These studies have shown that criminals are highly responsive to market conditions and how firms involved in illicit trade can impact crime rates and the demand for illegal goods. In this paper, I extend this literature by providing compelling evidence of how legislative changes that enhance the supervision of firms can affect crime by increasing the costs associated with converting stolen goods into cash. My research focuses on monitoring a market that can be exploited for the sale of vehicle spare parts and highlights how criminals assess legal markets to obtain liquidity when the monitoring by legal authorities is weak and consumers opt for illicit goods due to their lower prices. My findings provide rigorous empirical evidence supporting public policies aimed at reducing the financial gains of criminal gangs through enhanced market regulation.

While some evidence exists on how legislative changes can lead to an unintended increase in violent crimes in Brazil (Chimeli and Soares, 2017; Pereira and Pucci, 2022), to the best of my knowledge, this paper is the first to document the effects of increasing supervision in legal firms on crime. Similar to these studies, I use a new regulation as a natural experiment to evaluate the impact of legislative changes on crime. However, I differ from them by showing evidence in the opposite direction, how the increase in monitoring capabilities can reduce illicit trade and subsequently affect crime. Furthermore, my paper uses more granular data that allows for the exploration of the interplay between firm locations and crime following enhanced supervision. My findings also contribute to the literature on the association between illegal markets and crime (Adda, McConnell, and Rasul, 2014; Owens, 2014), albeit through the lens of changing the monitoring of legal businesses associated with illegal markets rather than altering the criminal status of consumers.

From a policy perspective, this paper provides valuable insights into the role of legislative changes in reducing crime rates in developing countries, particularly in Latin America, where crime is a pervasive problem (Soares and Naritomi, 2010). Existing literature suggests that factors such as the opportunity cost of illegal activities and the potential returns in illegal markets affect individuals' decisions to engage in criminal behavior (Chalfin and McCrary, 2017; Draca and Machin, 2015). Despite the significant body of research on criminal deterrence (Levitt, 2002; Evans and Owens, 2007; Johnson and Raphael, 2012; Drago, Galbiati, and Vertova, 2009), there has been limited empirical evidence on the role of market regulation in curbing crime. I provide compelling evidence that legislative changes can lead to a substantial and persistent reduction in crime rates. These findings highlight the complementarity of legislative reforms with traditional crime reduction strategies, such as investments in law enforcement and incarceration policies. Furthermore, the case of regulating junkyards illustrates that the costs of implementing monitoring mechanisms were relatively low compared to the long-term benefits of reducing crime, as the firms themselves bore most of the compliance costs, including reporting sales and acquisitions to the state Traffic Authority.

The remainder of the paper is structured as follows. Section 1.2 provides the background of the market for vehicle spare parts and the regulation of junkyards in Brazil. Section 1.3 presents the data used in the paper and the empirical strategy. Section 1.4 presents the results of increasing supervision of junkyards at the municipality level, while Section 1.5 shows the effect of junkyard locations on auto theft after the regulation. Finally, Section 1.6 discusses policy implications and concludes the paper.

1.2 Institutional Setting

In this section, I describe the market for spare parts and the junkyard regulation approved in São Paulo before other Brazilian states.

1.2.1 Junkyards and the market for vehicle spare parts

Junkyards in this setting are suppliers of spare parts. Brazil has more than 46 million registered vehicles, an average of one per five people (Sindipeças, 2021). The industry estimates an average life cycle of ten years for cars (CNN, 2021), which demand spare parts for regular maintenance and to replace items damaged in traffic accidents. Automobile manufacturers also provide spare parts, and the difference between these and junkyards is straightforward; the former sells brand-new items, whereas the latter focus on the recovery of used parts. Moreover, while vehicle assemblers and large auto-service companies mainly buy from manufacturers, end users and small mechanical workshops usually buy spare parts from junkyards due to the lower price. The Brazilian Automotive Recycling Association (ABCAR) estimates that junkyards' annual revenues represent about 10 percent of the market for spare parts, which would be US$ 450 million according to data for 2020.

Anecdotal evidence points to a possible interplay between junkyards and auto theft. Without proper regulation and monitoring of the dismantling activity, it is hard to distinguish if spare parts sold by these firms come from vehicles acquired through public auctions or from the illegal market (stolen cars). Thus, weak supervision over junkyards creates conditions for collusive agreements with criminals specializing in auto theft. In this context, monitoring and strict rules to open and keep a dismantling firm may impose a barrier to acquiring spare parts illegally, thus affecting auto theft.

Furthermore, junkyards that acquire vehicles from the illegal market have lower costs than competitors that only buy cars through legal auctions. Given the risks of keeping stolen products, criminals sell them at prices much lower than similar items in the legal market. Figure A.1 shows the problem junkyards face when acquiring vehicles. Arguably junkyards may choose to operate (1) only in the legal market, (2) only in the illegal market, or (3) combine purchases from the legal and illegal markets[2].

Last, since consumers hardly distinguish between legal and illegal products in this market, the traceability of spare parts is fundamental to deterring criminals from selling illegal goods. The intuition is that criminals have lower incentives to sell stolen products when potential buyers can quickly identify the illegal origin of these items. Assuming that even junkyards established as formal companies can dismantle vehicles acquired illegally, it is essential to provide tools to ensure the authenticity of products to reduce the demand for illegal goods. In this context, creating mechanisms to verify the authenticity of items sold by junkyards may allow consumers to exert complementary supervision of legal authorities, which decreases monitoring costs.

[2] I present in the Appendix 1.7 a model that shows how increased supervision of the spare parts market affect prices and quantities of the illegal items sold by junkyards. In this setting, the increased risk of being punished reduces the supply of illegal goods through changes in the incentives of criminals, junkyards, and the final consumer.

1.2.2 State Law 15.276 ('Junkyard Law')

Auto theft is a prevalent crime in metropolitan areas and large municipalities. There was a huge increase in these events in São Paulo over the 2000s, reaching more than 165,000 stolen vehicles in 2014. The possible interplay between auto theft gangs and junkyards motivated public debates to increase the supervision of dismantling firms. The first policy recommendation in 2012 suggested a complete prohibition of junkyards in the state.[3] In 2013, the State Security Secretary sent a proposal to the Legislative House to banish junkyards from public auctions of crashed and apprehended vehicles.

However, junkyard owners claimed alternatives to tackle the illegal market of stolen cars without closing all dismantling firms. The ABCAR argued to the State Department of Traffic and Vehicles (DETRAN-SP) that junkyards had economic and environmental value since these firms recover items that otherwise would become garbage. Moreover, ABCAR mentioned that these firms fill a gap in the market by providing spare parts for old and/or imported cars. Hence, both sides agreed to an intermediate solution through a more strict market regulation. Thus, the state government and DETRAN-SP proposed law 15.276 ('Junkyard Law') in January 2014, for which junkyards had six months (up to July 2014) to adapt their operation.

The Junkyard Law increased the legal requirements for dismantling firms. To acquire vehicles in public auctions, junkyards might have a permit issued by DETRAN-SP. This permit must be renewed annually and is mandatory to dismantle cars, such as for selling spare parts. Following the new regulation, junkyards must present (1) a registered by-law (a business document), (2) any criminal records of owners and employees, (3) a municipality business license, (4) a technical capacity certificate, (5) a tax-compliance certificate, (6) an environmental certificate, (7) electronic records of all vehicles acquired and spare parts recovered that allow tracking any sale and acquisition, and (8) periodically an updated list of employees (regular and temporary staff).

The regulation also focused on the traceability of items sold by junkyards. After acquiring a vehicle, junkyards must report all spare parts recovered as inventory in the DETRAN-SP system. These firms must also present a technical report signed by a certified employee regarding the dismantled vehicle and provide a complete list of items that will be discarded. Last, the regulation imposed the obligation to provide an identification number to junkyard clients in their receipts. This number is an authenticity code to track the item on the DETRAN-SP website. In October 2015, the state government improved this control by requiring a QR code tag for all spare parts recovered and sold by junkyards. Figure A.2 shows photos highlighting the QR identifier of an engine recovered by a junkyard. Junkyards, mechanical stores, and consumers must pay a fine of up to US$10,000 if spare parts do not show the QR code or if they cannot prove the legal origin of these products facing legal authority inspections.

The institutional change seems to be a relevant driver in the impressive 40 percent drop in

[3] State law project number 4.330: <https://www.camara.leg.br/propostas-legislativas/553717>.

auto theft from 2014 to 2019. One year after the Law, inspections commanded by a task force involving DETRAN-SP, state police, and the State Tax Authority closed about 700 non-complier junkyards[4] in São Paulo (G1, 2015).

1.3 Data and empirical strategy

1.3.1 Data

I use monthly data on auto theft and other crimes from 2011 to 2019 at the municipality and district levels. This panel data comprises 645 municipalities of São Paulo. Detailed crime data is very scarce in Brazil, there are huge differences regarding data reported by each state secretary, and some provide only state-level data for short periods. Therefore, composing a detailed panel data set comprising granular information regarding municipalities from all 27 Brazilian states is a challenging task. By focusing on São Paulo, I overcame this issue since the state provides high-frequency and geolocated crime data before and after approving the Junkyard Law. I also use data regarding violent deaths and other offenses in the same period and information from the national census provided by the Brazilian Bureau of Statistics (IBGE) regarding socioeconomic and demographic characteristics, especially population and gross domestic product per capita at the municipality level.

Last, I use Tax and Labor National Authority data to identify the number of junkyards in each municipality of São Paulo. To identify junkyards, I rely on a specific code called the "Classificação Nacional de Atividade Econômica" (CNAE), which classifies the activity of negotiating used and recovered spare parts. By using this code, I can identify all legally registered junkyards in the Labor Authority dataset (RAIS) and access additional information such as the firms' identification code (CNPJ) and the number of employees registered in each junkyard. To gather more detailed information, I cross-reference each CNPJ identified through the specific CNAE code with the Tax Authority's records to retrieve the addresses of the junkyards. I leverage these addresses to find the precise coordinates of each junkyard, allowing for the geolocation of their positions at the street level. This unique dataset provides the opportunity to analyze the dynamics of auto theft in relation to the distance from junkyards, enabling a more in-depth exploration of the spatial relationship between these establishments and criminal activity.

[4] Table A.1 shows some descriptive statistics collected from the Ministry of Labor's RAIS dataset about junkyards legally registered. Junkyards in São Paulo have, on average, 4 employees and about 40% percent of these firms did not report any labor information in 2017, three years after the new regulation. These pieces of evidence reinforce significant changes in the market for spare parts after the Junkyard Law.

1.3.2 Empirical strategy

1.3.2.1 The Junkyard Law

The distribution of junkyards in each municipality at the time of the institutional change is a unique opportunity to test the effect of regulating junkyards on auto theft. Since the focus of the regulation is to monitor the illicit trade of auto parts in São Paulo, municipalities with a larger incidence of junkyards will arguably be the most affected by the enhanced supervision. Law enforcement inspections started using the system implemented to register cars acquired by junkyards to check the authenticity of spare parts. The monitoring system was further enhanced by mandating the use of QR code tags on all items. As a result, consumers and legal authorities got a very efficient tool to verify the authenticity of auto parts using a smartphone. Consequently, following the legislative change, junkyards faced a higher probability of punishment if they engaged in the trade of illicit spare parts, which likely led to a reduction in the demand for goods from illegal suppliers.

To identify the effect of the Junkyard Law on auto theft, I focus on the exogenous variation caused by the Junkyard Law within São Paulo. I exploit how market regulation affects auto theft by comparing municipalities with and without the presence of junkyards. After the Law, a larger decrease in auto theft in cities with junkyards compared to those without these firms is evidence of the interplay between strict monitoring of the dismantling business and stolen vehicles. However, if there is no difference when comparing municipalities with and without junkyards, confounding factors other than the new regulation may have driven the decrease in auto theft. I use Equation 1.1 to estimate the effect of the Junkyard Law given the presence of dismantling firms. In this setup, the differences-in-differences estimator is:

$$y_{it} = \phi_i + \phi_t + \beta_1 * Law_t + \beta_2 * Law_t \times D_i + X_{it} + \mu_{it} \tag{1.1}$$

Where D_i is a dummy variable equal to one when there is at least one junkyard in the municipality i. Hence, the coefficient β_2 shows the effect of the Junkyard Law across cities with and without junkyards after controlling for location and time fixed effects. To account for the difference in the size of each municipality, I use the rate of auto theft as the dependent variable, i.e., the number of stolen vehicles per 100 thousand inhabitants. I also test other violent and property crimes to investigate if the regulation affected other offenses.

Table 1 presents descriptive statistics for both the treated and control groups prior to the approval of the Junkyard Law. The treated group consists of municipalities that have at least one junkyard, while the control group comprises municipalities without dismantling firms. Municipalities in the treated group exhibit higher levels of violent crimes, including total homicides and deaths resulting from robberies, as well as auto theft and other robberies. On the other hand, the control group shows a greater incidence of bank robberies and cases of bodily injuries. Additionally, Figure 1 depicts the trajectory of auto theft for both the treated and control groups from 2011 to 2019.

The validity of my findings relies on two crucial assumptions. Firstly, auto theft rates in both the treated and control municipalities followed similar trends prior to the implementation of the Junkyard Law in July 2014, and the decrease in stolen vehicles was observed only after the legislation change. Secondly, the decrease in stolen vehicles is exclusively due to the new regulation and increased supervision over dismantling firms, rather than being driven by other confounding factors. It is therefore important that the regulation does not impact other types of crimes, as this could suggest a general decline in overall criminal activity instead of stricter supervision of dismantling firms. Under these assumptions, the coefficient β_2 represents the relative change in auto theft following the Junkyard Law.

Table 1 – Descriptive Statistics at the Baseline - Treated and Control Group

	All Municipalities	Treated Group	Control Group
Homicides	7.12	8.20	6.66
	(0.19)	(0.21)	(0.25)
Deaths in Robberies	0.49	0.58	0.46
	(0.04)	(0.05)	(0.05)
Body Injuries	473.33	448.67	483.80
	(4.59)	(7.95)	(5.59)
Auto-theft	100.61	200.53	58.19
	(2.73)	(7.01)	(1.71)
Robbery (others)	123.95	243.21	73.31
	(3.41)	(8.34)	(2.50)
Theft (others)	774.99	934.79	707.14
	(9.84)	(16.77)	(11.71)
Bank Robbery	0.60	0.24	0.75
	(0.07)	(0.05)	(0.10)
Cargo Theft	4.16	5.01	3.80
	(0.40)	(0.33)	(0.55)
GDP per capita	24,296.23	30,796.68	21.536.16
	(412.43)	(807.07)	(462.06)
n	645	192	453

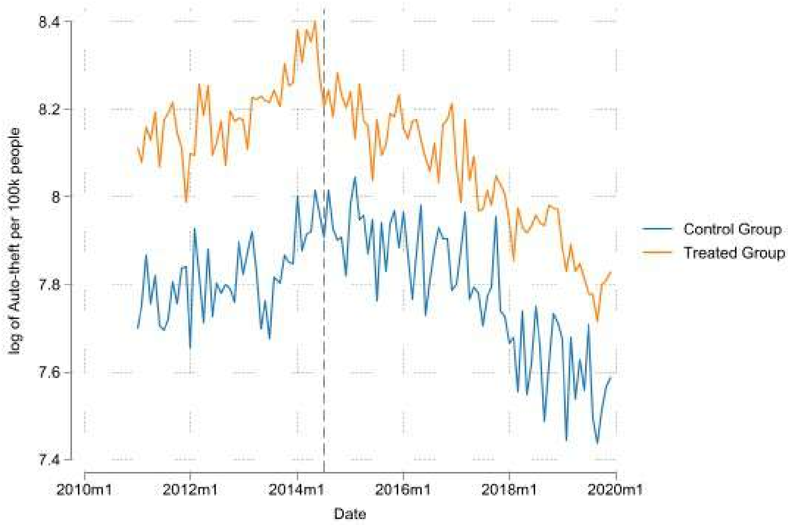
Note: This table shows descriptive statistics prior to the Junkyard Law (2011m1 to 2014m7) for all municipalities of São Paulo and also for the treated and control group. Homicides, Deaths in Robberies, Body Injuries, Auto-theft, Robbery (others), Theft (others), Bank Robbery, and Cargo theft are presented as monthly average rates by municipalities (cases by one hundred thousand inhabitants). The GDP per capita is the average annual Gross Domestic Product per capita in Brazilian Reais. Standard-errors in parenthesis.

1.3.2.2 The effect of junkyard location

Driving a stolen car for long distances increases the probability of being caught by police officers. Therefore, criminals who keep collusive agreements with junkyards arguably prefer to drive short distances to minimize the risk of apprehension. It is hard to believe that criminals would drive a car for hours within a municipality if they had a closer place to hide and sell the vehicle.

Despite the inherent difficulties in assigning which junkyards acquired vehicles illegally, I assume the location of junkyards identified by the Federal Tax Authority as a proxy variable to assess the effect of distances to junkyards on auto theft. The supervision of dismantling firms was significantly weaker before the Junkyard Law. Therefore, it is possible that some of these

Figure 1 – Auto-theft - Treated x Control Group



Notes: This graph shows the evolution of auto-theft in treated and control municipalities from January 2011 to December 2019.

firms operated in both legal and illegal markets. Figure A.3 shows in orange the location of all active junkyards in São Paulo for 2014. I use the Ward-like hierarchical clustering algorithm presented by Chavent et al., 2018 to define 15 clusters of junkyards in São Paulo (Figure A.3). Given the relatively large number of junkyards in these areas, I define these clusters as a baseline to measure the effect of junkyard location on auto theft after implementing the new regulation.

I evaluate the effect of the Law within São Paulo, the largest municipality in the state with the highest number of junkyards in Brazil. As described before, it is reasonable to assume that a criminal would not drive a stolen car for long distances and hours because of the probability of being followed by police officers. Hence, stealing a car in districts closer to one of these 15 clusters would be arguably less risky for criminals who sell stolen vehicles to junkyards. Evaluating the heterogeneous effect of the law by distances to junkyards clusters also allows testing whether there was a displacement of criminal activity, that is, whether gangs moved to districts far from these areas following the strict monitoring of these firms.

The spatial analysis of a new regulation for junkyards presents two significant challenges. First, junkyard locations in São Paulo maybe are not randomly assigned. If junkyards are near streets with a larger incidence of auto theft, this will characterize a selection bias. Second, even if junkyard locations were randomly assigned, it is difficult to define a counterfactual for their absence due to contamination concerns. Places far from junkyards before the regulation can be affected if junkyards' owners decide to reallocate within the city, and the treatment ('Junkyard Law') would contaminate districts of the control group. To overcome these issues, I define a second specification using only junkyards that left the market after the new regulation. As described before, legal authorities closed many dismantling firms following the Junkyard Law.

Some probably moved to other states or businesses, given the increased supervision and larger costs to adapt to the new regulation. Hence, the location of closed junkyards captures the change in auto theft near firms that arguably had more difficulty complying with the Junkyard Law. Second, I perform a falsification test using distances to police stations as the dependent variable in the baseline regression. This approach allows checking whether the deterrence effect of police increased after the Junkyard Law, affecting the number of vehicles robbed in police station neighborhoods. Thus, I can verify whether the decrease in robberies near junkyards was larger than in locations with a strong police presence.

I combine data regarding junkyard locations and registers of auto theft at the street level to evaluate whether the decrease in stolen vehicles is larger near to junkyards. I estimate a differences-in-differences model with time fixed effects to absorb all common shocks in auto theft across districts. I also include district-fixed effects to control for unobservable crime determinants that are invariant at the local level. I obtain the differences-in-differences estimator using the following model:

$$y_{it} = \phi_i + \phi_j + \phi_t + \sum_d \beta_1^d T_i^d Law_t + \mu_{it} \tag{1.2}$$

where the subscripts i, j, and t denote census tract, district and date; T_i^d is 1 if the census tract i lies at a distance d from a junkyard; d defines six categories of distance: up to 0.5 km, 0.5–1.0 km, 1.0–1.5 km, 1.5–2.0 km, 2.0–2.5 km, and 2.5–3.0 km. The mark of 3.0 km is the median distance between census tracts and the closest junkyard cluster. Therefore, areas in São Paulo with a junkyard cluster at 3.0 km are the *'treated group'*, while neighborhoods at longer distances are the *'control group'*. The dependent variable y indicates robbery and theft of vehicles in a census tract i in time t. I control for census tract, month, and year fixed effects. Besides, I allow each district to have different linear trends. These variables capture heterogeneous dynamics led by different district policies, such as changes in policing strategy. The error term μ_{it} is clustered at the district level.

The identification comes from two main assumptions. The first is that census tracts more than 3.0 km from junkyards were not affected by the regulation of dismantling firms (non-contamination of the control group). Second, the reduction in stolen vehicles is driven by the presence of junkyards instead of other confounding factors. Therefore, the distance from other firms or buildings would not cause a decrease in auto theft contemporaneous with the regulatory change. The variable Law_t controls the Junkyard Law's approval effect in all census tracts within São Paulo. Hence, the coefficient β_1 shows the specific effect of the proximity to junkyards on auto theft after the legislation change.

1.4 The effect of the Junkyard Law

1.4.1 Results at the municipality level

Table 2 reports the results from Equation 1.1 using as the dependent variable the number of stolen vehicles per 100 thousand inhabitants in municipalities of São Paulo from 2003-2019. In this approach, 192 municipalities that present at least one junkyard are the *treatment group*, and the remainder of 453 is the *control group*. I show the differences-in-differences point estimates to the Junkyard Law in São Paulo with and without controlling by GDP. My results show a significant decrease in the number of stolen vehicles in cities with junkyards. The drop in auto theft is equivalent to 4.35% fewer stolen vehicles per year in municipalities with at least one junkyard. These findings highlight the interplay between dismantling firms and the increased supervision following the legislation change. Moreover, these results also alleviate concerns about confounding factors non-related to junkyards driving the reduction in auto theft.

To test the parallel trends assumption, I run an event study of Equation 1.1 to assess the dynamic effect of the legislation change. Figure 2 shows pre and post-coefficients of the Junkyard Law. There is no evidence of a previous trend in auto theft, and the effect of the new regulation increases over time. These findings validate the parallel trends assumption to the differences-in-differences model and are supportive evidence of the causal effect of the Junkyard Law reducing auto theft.

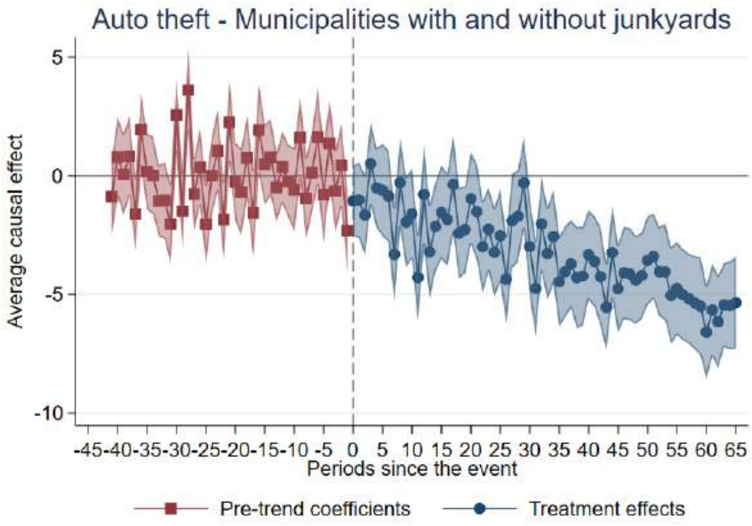
Thus, my results show that the market regulation reduced auto theft through increased supervision of dismantling firms. The event study suggests that it has taken some time for the Junkyard Law to become effective in reducing auto theft, which reflects improvements in the monitoring over time, as the QR code implementation in São Paulo (15 months after approving the Law) and legal authorities' support through inspections of junkyards and punishment of non-complier firms.

Table 2 – Results - Baseline Specification

	Auto-theft (1)	Auto-theft (2)	Robbery (3)	Robbery (4)	Theft (5)	Theft (6)
Junkyard Law × Presence of Junkyards	-2.534***	-2.532***	-1.225***	-1.224***	-1.309***	-1.308***
	(0.143)	(0.142)	(0.0701)	(0.0701)	(0.116)	(0.116)
log(GDP)		0.391		0.169		0.222
		(0.257)		(0.103)		(0.229)
Observations	69,204	69,204	69,204	69,204	69,204	69,204
R-squared	0.628	0.628	0.648	0.648	0.455	0.455
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table shows the results of my baseline specification. The sample comprises 645 municipalities in São Paulo in the period 2003-2019. The treatment in my differences-in-differences design is given by the variable *"Junkyard Law × Presence of Junkyard"* that assumes value one from July 2014 only to municipalities in São Paulo that have at least one junkyard. Columns 1 and 2 show the results of auto-theft per 100 thousand inhabitants as the dependent variable. The dependent variable in columns 3 and 4 is vehicles robbed per 100 thousand inhabitants, and in cols 5 and 6, the number of thefts of cars per 100 thousand inhabitants. Robust standard errors are shown in parentheses.
*p<0.1,**p<0.05,***p<0.01

Figure 2 – Event Study - Baseline Specification



Notes: The effect of the Junkyard Law on auto theft is estimated under the unconditional parallel trends assumption. Red lines give point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the municipality level. Under the null hypothesis of the parallel trends assumption holding in all periods, these should be equal to zero. Blue lines provide point estimates and uniform 95% confidence bands for the treatment effect of the Law allowing clustering at the municipality level. The treatment in my differences-in-differences design is given by the variable *"Junkyard Law* × *Presence of Junkyard"* that assumes value one from July 2014 only to municipalities in São Paulo that have at least one junkyard.

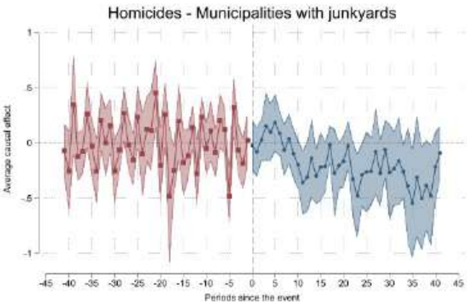
1.4.2 Robustness and Potential Mechanisms

A general decrease for all crimes: It is important to test whether the decrease in auto theft following the approval of the Junkyard Law is a result of a general downward trend in criminality or the specific effect of monitoring dismantling firms. To address this concern, I conducted a falsification test to examine if other crimes also decreased following the legislation change. Figure 3 presents the event study estimates for different types of violent and property crimes, and the results provide no evidence of an overall decrease in other crimes. Furthermore, the majority of other crimes remained unchanged for an extended period after the legislation change, indicating a specific effect of the law on auto theft.

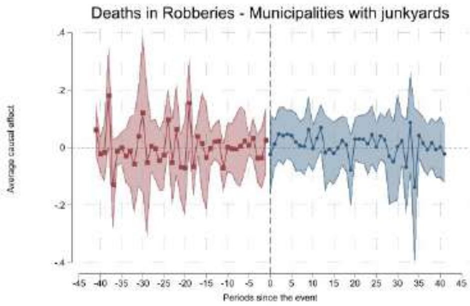
Criminals moving to other crimes: It is possible that the strict supervision of junkyards led some criminals to shift their criminal activities from auto theft to other property crimes in order to obtain liquidity. To investigate this possibility, I used Other Robberies, Other Theft, Bank Robberies, and Cargo Robberies as dependent variables in the Falsification Test. Panels (c) to (f) in Figure 3 demonstrate that there is no evidence of displacement to these crimes. Therefore, there was no increase in other property crimes in municipalities with at least one junkyard following the legislation change.

Displacement: My findings indicate that a general decline in crime rates does not drive a decrease in auto theft. Therefore, the decrease in stolen vehicles cannot be attributed to an increased deterrence effect of the police on crime. However, it is still possible that criminals

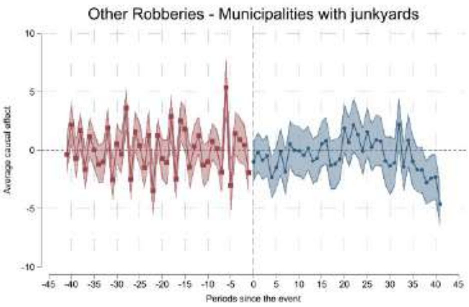
Figure 3 – Event Study - Other Crimes



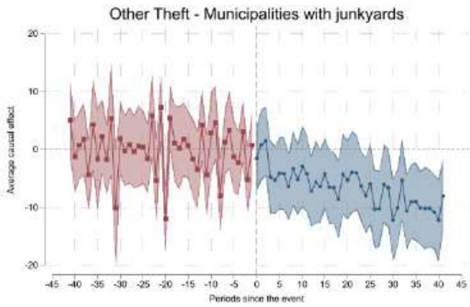
(a) Homicides



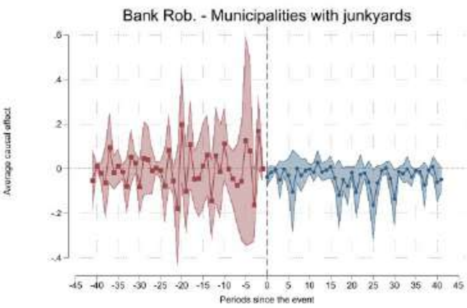
(b) Deaths in Robberies



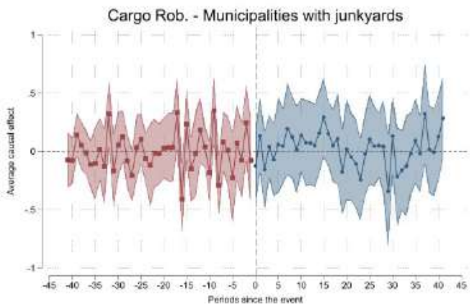
(c) Other Robberies



(d) Other Thefts



(e) Bank Robbery



(f) Cargo Robbery

Notes: The effect of the Junkyard Law on auto theft is estimated under the unconditional parallel trends assumption. Red lines give point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the municipality level. Under the null hypothesis of the parallel trends assumption holding in all periods, these should be equal to zero. Blue lines provide point estimates and uniform 95% confidence bands for the treatment effect of the Law allowing clustering at the municipality level. The treatment in my differences-in-differences design is given by the variable *"Junkyard Law $\times$ Presence of Junkyard"* that assumes value one from July 2014 only to municipalities in São Paulo that have at least one junkyard.

shifted their activities to municipalities without junkyards, where law enforcement efforts on stolen vehicles and the illegal spare parts market may be lower. To address this concern, I compared the trends in auto theft between treated and control groups. Figure 1 demonstrates that auto theft decreased in both treated and control groups following the legislation change, providing evidence that the presence of junkyards in a municipality leads to a larger decrease in auto theft following stricter regulations for dismantling firms. In summary, my findings are not driven by an increase in stolen vehicles in municipalities of the control group, which would support the hypothesis of displacement to other cities.

1.5 The effect of junkyards' location on vehicles robbed

1.5.1 Results at the district level

Table 3 presents the results of Equation 2.5. I show the effect of the law by the distance of junkyards to vehicles stolen through robbery in column 1 and theft in column 2. The number of robbed vehicles decreased more in census tracts between 500 and 1.000 meters to one of the 15 junkyard clusters than in non-affected areas (census tracts located more than 3.0 km from a junkyard cluster). Figure A.4 shows the estimates and uniform 95 percent confidence bands. My findings show that the Junkyard Law in São Paulo changed the dynamic of auto theft closer to junkyards. Census tract and district-specific time fixed effects do not explain the decrease in vehicles robbed in these neighborhoods.

These findings provide further support for the use of the location of dismantling firms as a relevant proxy variable for measuring the impact of the new regulation on auto theft. Additionally, they strengthen the credibility of the argument that criminals specializing in stolen vehicles are unlikely to travel long distances, making regions with a higher concentration of junkyards probable destinations for their activities. Therefore, the district-level approach contributes to a deeper understanding of the increased supervision of junkyards and complements the aggregate results observed at the municipality level.

1.5.2 Robustness

Junkyards closed after the law. The causal effect of market regulation on auto theft comes from imposing higher costs on criminals converting stolen vehicles into cash. For junkyards that before the regulation only operated in the legal market, shifts in monitoring would hardly force a transition to the illegal market. Assuming the same probability of all junkyards acquiring vehicles from criminals may bias my results and underestimate the effect of the regulation on junkyards.

To address this issue, I propose using only the location of junkyards closed after the law in Equation 2.5. The identification assumption is that these firms had more difficulty complying with the new regulation. Additional costs to implement the traceability demanded by legal

Table 3 – Results Robbery and Theft - Distance to Junkyards and Police Stations

	Junkyard Clusters		Distance to Police Stations		Closed Junkyards	
	Robbery	Theft	Robbery	Theft	Robbery	Theft
	(1)	(2)	(3)	(4)	(5)	(6)
up to 0.5km	0.077	0.181	-0.095	0.060	-0.330***	-0.092***
	(0.120)	(0.245)	(0.094)	(0.039)	(0.074)	(0.033)
0.5 to 1.0km	-0.252**	0.108	-0.165*	-0.009	-0.149***	-0.037
	(0.123)	(0.103)	(0.087)	(0.028)	(0.056)	(0.024)
1.0 to 1.5km	-0.181*	0.015	-0.138	-0.024	0.005	-0.022
	(0.103)	(0.127)	(0.098)	(0.026)	(0.046)	(0.018)
1.5 to 2.0km	-0.143	0.129	-0.080	-0.026	-0.095**	-0.037**
	(0.130)	(0.093)	(0.084)	(0.025)	(0.046)	(0.017)
2.0 to 2.5km	-0.130	0.036	-0.118	0.020	-0.105**	0.004
	(0.082)	(0.111)	(0.102)	(0.027)	(0.047)	(0.018)
2.5 to 3.0km	0.114	0.165*	-0.082	-0.016	-0.050	-0.000
	(0.071)	(0.090)	(0.099)	(0.036)	(0.047)	(0.019)
Observations	249,836	61,269	249,836	61,269	249,836	61,269
R-squared	0.177	0.307	0.174	0.332	0.174	0.332
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports estimated coefficients and standard errors clustered at the census tract level in parentheses. The dependent variable in columns 1 and 3 is the log(number of robberies), and columns 2 and 4 are the log(number of theft). Columns 1 and 2 show the results of equation 2.5 using the centroid of junkyards clusters presented in Figure A.3 as reference points to measure the distance d to each census tract of São Paulo. Columns 3 and 4 report a falsification test using police stations as reference points to measure the distance d to each census tract of São Paulo. Columns 5 and 6 present results using junkyards closed after the new regulation as reference points to measure the distance d to each census tract of São Paulo.
*p<0.1,**p<0.05,***p<0.01

authorities and provide detailed information regularly arguably affected the profitability of junkyards. In this context, firms that relied on acquiring stolen vehicles faced more difficulties adapting to the new regulation. Thus, the more dependent a junkyard was on the illegal market, the higher the probability it would close after the legislation change.

Columns 5 and 6 of Table 3 show the results of Equation 2.5 for vehicles stolen through robbery and theft using only junkyards closed after the Junkyard Law. The interpretation of this robustness check comes when comparing baseline estimates in column 1 to the estimates in column 5. The number of vehicles robbed decreased more in census tracts up to 500 meters from junkyards closed after the law. This drop is 31 percent greater than the overall effect estimated considering all junkyards. Figure A.5 compares both estimates. Although I cannot completely rule out that factors other than the dependence on stolen vehicles made these junkyards leave the market, my results suggest significant heterogeneity within junkyards.

Falsification test (distance to police stations). The larger decrease in auto theft near junkyards is compelling evidence of the causal effect of the Junkyard Law through increased monitoring of dismantling firms. However, suppose that police capability increased contemporaneously with the Junkyard Law. In that case, my results may capture the deterrence effect of police instead of the causal effect of junkyards' location. To shed some light on the mechanism driving the reduction in auto theft by census tract, I show the effect of the distance to police stations on vehicles robbed after the legislation change. The proximity of a police station represents a substantial risk for criminals, and an overall increase in police capabilities would likely reduce auto theft rates in areas closer to these buildings. Furthermore, by conducting this falsification test, I can evaluate the influence of the legislation change using a different location (police stations) to verify if the presence of junkyards exerts a stronger influence on auto theft compared to other locations. Columns 3 and 4 in Table 3 show the estimates of Equation 2.5 using distances to police stations as the explanatory variable. I do not find a similar decrease in stolen vehicles closer to police stations after the institutional change.

These results show that proximity to junkyards is more relevant than the distance to police stations in explaining the decrease in auto theft after the Junkyard Law. Last, even if the decrease in auto theft closer to junkyards can be related to the deployment of police officers to these areas, I argue that it would also be a consequence of the new regulation that increased police inspections on junkyards, such as more extensive surveillance and police patrols in the neighborhoods of these firms.

1.6 Discussion and Conclusion

1.6.1 Local Welfare

Extensive research in the literature has shown a negative correlation between crime rates and property values (Gibbons, 2004; Besley and Mueller, 2012). This evidence suggests that individuals are willing to pay a premium to protect their assets and avoid exposure to high-crime environments (Thaler, 1978; Linden and Rockoff, 2008). In light of the substantial and persistent reduction in auto theft observed in this paper, it is reasonable to hypothesize that there was an impact on the risk perception of local citizens, potentially influencing their willingness to pay for vehicle insurance.

To investigate the effect of market regulation on local welfare, I examine the average vehicle insurance prices in municipalities with and without junkyards. The intuition is that the drop in auto theft resulting from the institutional change will likely lead citizens to reassess their need for vehicle insurance, given the reduced risk of car theft. Consequently, I expect to observe a larger drop in insurance prices in municipalities that show larger reductions in auto theft.

I use data on insurance vehicle transactions in São Paulo between 2011 and 2019, provided by the *"Superintendência de Seguros Privados"* (SUSEP) on a semester basis. These records provide detailed information on each insurance contract, including vehicle type and fabrication year. By constructing a panel dataset at the municipality level, I estimate Equation 1.1 with the mean insurance price by car model as the dependent variable. Thus, I assess the impact of the new regulation on insurance prices in municipalities with at least one junkyard specializing in vehicle parts, as these areas experienced more substantial reductions in auto theft.

Figure A.6 shows the event study estimates on insurance prices. There is a large and sustained drop in the average insurance price following the new regulation. This decrease corresponds to a 7.09% reduction in insurance prices in municipalities with at least one junkyard, compared to the mean of the control group. Considering the combined effect of the 4.53% reduction in auto theft and the 7.09% decrease in insurance prices, I estimate an insurance price elasticity of 1.63 with respect to auto theft. These findings suggest that the reduction in auto theft not only contributes to improved local welfare by lowering crime rates but also brings tangible economic benefits to vehicle owners through reduced insurance costs.

1.6.2 Policy Implications

From a policy perspective, my results provide robust evidence regarding the effectiveness of market regulation in reducing criminal outcomes. Specifically, by improving monitoring capabilities at a lower cost, the regulation led to decreased junkyards purchases from illegal sources. As a result, there was a reduction in the demand for stolen goods, which in turn reduced the incentives for auto theft gangs, leading to a decrease in the number of stolen vehicles following the new regulation. This finding highlights the relevance of monitoring markets that are associated with illicit trade to deter criminal activities. Therefore, it is crucial to implement strong market regulation to prevent criminals from exploiting the low monitoring and converting illicit goods into cash.

We learn from the Junkyard Law that changes in legislation exert a complementary role to investments in public security to deter criminals. São Paulo spent US$ 5 billion on Police Forces and US$ 1 billion on Penitentiaries in 2020, which account for 10% of the state budget. Despite the public investment in developing a system to supervise junkyards, the amount spent seems very small compared to the benefits of a significant reduction in auto theft following the new regulation. As a reference, DETRAN-SP had a budget of US$ 150 million in 2020 to perform all tasks related to traffic and vehicle legislation. Furthermore, junkyard owners paid most of the cost of the regulation when adapting their firms to the Junkyard Law requirements.

One of the most relevant findings is the persistence of the drop in stolen vehicles after the legislation change. The effect of the regulation on auto theft is large and increases over time when comparing municipalities with and without junkyards in São Paulo. Moreover, the larger decrease in auto theft seems directly associated with more strict monitoring of junkyards' revenues and acquisitions, especially the system managed by DETRAN-SP to track all spare parts recovered and sold by these firms. This result sheds some light on the crucial role of supervising firms to reach a larger reduction in property crime when approving more strict market regulations. As a guide for future public policies and other states, monitoring mechanisms are essential when approving new legislation to tackle a market for stolen goods.

Last, regarding potential spillovers, there are arguably effects on the formalization of firms and tax revenues. Given the reduced demand for stolen vehicles and increased supervision of dismantling firms, the regulation forced junkyards to report their sales to legal authorities, increasing tax revenues collected from spare parts.

1.6.3 Concluding Remarks

Despite the anecdotal evidence regarding the association of a market for stolen goods and crime, there is little evidence of the interplay between firms and criminals. This paper presents compelling evidence of the reduction in auto theft following increased supervision of dismantling firms. The decrease in auto theft is not related to an overall downward trend in crime, socioeconomic conditions, or intrinsic characteristics of municipalities that have increased the supervision of junkyards. Following the new regulation, I show evidence of a larger decrease in the number of stolen vehicles for districts closer to junkyards. Last, there is no displacement to other robberies following the new regulation.

The traceability of items sold by junkyards seems crucial to leverage the effectiveness of market regulation by increasing the difficulty of converting stolen items into cash. Although Brazil had created a regulation on junkyards at the country level, the autonomy of each state about when and how to implement the new legal requirements made it difficult to enforce the supervision of junkyards homogeneously across states. Criminals may have decided to move to states with lower monitoring to exploit legal authorities' lack of commitment to the new regulation. With these caveats in mind, this paper illustrates how market regulation and supervision of firms affect a potential market for stolen goods and complements public security policies to reduce crime.

Appendix A: Theoretical Model, Figures, and Tables

1.7 Theoretical Model

The conceptual setting

Let the price for spare parts be p_C for criminals selling to junkyards, p_J^I for junkyards selling stolen goods to the final consumer, and p_J^L for junkyards selling legal goods to the final consumer. Furthermore, let the expected punishing for being in the illegal market as μ_C for criminals, μ_J for junkyards, and μ_F for the final consumer.

Criminals. Let's assume that there is a cost of c to steal a car, increasing and convex. Furthermore, suppose that there is a continuum of identical criminals in $[0, 1]$. The problem of the criminal $x \in [0, 1]$ is to decide how many vehicles she steals. She maximizes in q

$$p_C q - [c(q) + \mu_C q],$$

and the optimum interior solution q_x is given by

$$q_x = c'^{-1}(p_C - \mu_C).$$

Integrating both sides in the number of agents we have

$$Q^I := \int_0^1 q_x dx = \int_0^1 c'^{-1}(p_C - \mu_C) dx = c'^{-1}(p_C - \mu_C),$$

where Q^I is the aggregate quantity of illegal spare parts.

Junkyards. Basically, the junkyards only act by buying vehicles (legally or illegally) and selling recovered spare parts. The quantity Q^L of spare parts from public auctioning is inelastically offered to them. Assuming free entry and a competitive market, quantities demanded and supplied by junkyards must be equal, profits are zero, and the problem of a junkyard y is

$$p_J^L q_y^L + p_J^I q_y^I - [p_J^L q_y^L + p_C q_y^I + \mu_J q_y^I] = 0.$$

Note that $p_J^L q_y^L$ cancels. Dividing the role expression by q_y^I we find

$$p_J^I - p_C - \mu_J = 0$$

for all junkyards.

Final Consumers. Finally, the consumer (also a continuum in 0 to 1) has a utility u for the total amount of spare parts acquired, and u is increasing and concave. The consumer z maximizes choosing q^L and q^I,

$$u(q^L + q^I) - [p_J^L q^L + p_J^I q^I + \mu_F q^I].$$

The existence of interior solutions q_z^L and q_z^I may respect

$$u'(q_z^L + q_z^I) = p_J^L = p_J^I + \mu_F.$$

Integrating analogously to the case of criminals, we have

$$u'(Q^L + Q^I) = p_J^L = p_J^I + \mu_F.$$

Solving the system, we find Q^I from

$$u'(Q^L + Q^I) = c'(Q^I) + \mu_C + \mu_J + \mu_F.$$

And the prices are given by

$$p_J^L = c'(Q^I) + \mu_C + \mu_J + \mu_F, \;\; p_J^I = c'(Q^I) + \mu_C + \mu_J, \;\; p_C = c'(Q^I) + \mu_C.$$

The result is quite intuitive, the expected marginal value of punishment is added to the price in each stage. The final step is to calculate the derivatives. To simplify the notation let's call $u''(Q^L + Q^I)$ as u'' and $c''(Q^I)$ as c''. Using the implicit derivative theorem, we find the effect on prices and quantities with respect to the expected punishments:

- illegal market supply

$$\frac{\partial Q^I}{\partial \mu_C} = \frac{\partial Q^I}{\partial \mu_J} = \frac{\partial Q^I}{\partial \mu_F} = \frac{1}{u'' - c''} < 0.$$

- price for criminals selling to junkyards

$$\frac{\partial p_C}{\partial \mu_J} = \frac{\partial p_C}{\partial \mu_F} = \frac{c''}{u'' - c''} < 0, \;\; \frac{\partial p_C}{\partial \mu_C} = \frac{u''}{u'' - c''} > 0.$$

- price for junkyards selling legal spare parts

$$\frac{\partial p_J^I}{\partial \mu_C} = \frac{\partial p_J^I}{\partial \mu_J} = \frac{u''}{u'' - c''} > 0, \;\; \frac{\partial p_J^I}{\partial \mu_F} = \frac{c''}{u'' - c''} < 0.$$

- price for junkyards selling illegal spare parts

$$\frac{\partial p_J^L}{\partial \mu_C} = \frac{\partial p_J^L}{\partial \mu_J} = \frac{\partial p_J^L}{\partial \mu_F} = \frac{u''}{u'' - c''} > 0.$$

Interpreting the Mechanisms

The parameters μ's show how the agents perceive the increased risk of punishment after the regulation of the spare parts market. Ideally, if we have a detailed dataset with the price and quantities of items sold by junkyards before and after the regulation, it would be possible to test these mechanisms. Even if we obtain data about the prices of original items, this proxy variable may not be accurate since manufacturers usually sell these items for a price not directly comparable to recovered spare parts sold in junkyards.

As a theoretical exercise, if we could access the prices of legal and illegal items sold by junkyards, it would be possible to decompose all effects in prices given the increased supervision after the institutional change:

$$\begin{cases} \Delta\mu_F & = \Delta(p_J^L - p_J^I) \\ \Delta\mu_J & = \Delta(p_J^I - p_C) \\ \Delta\mu_C & = \Delta(p_C - c'(Q^I)) \end{cases}$$

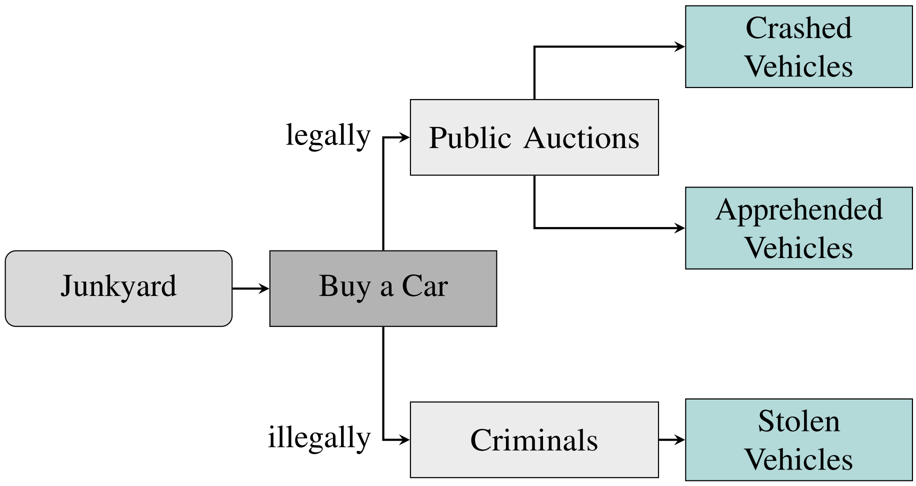
1.8 Figures and Tables

Table A.1 – Descriptive Statistics - Junkyards

	2014	2015	2016	2017
Junkyards (total)	959	907	863	887
Junkyards established before 2014 (percentage)	100%	78.3%	65.6%	59.9%
Employees (mean)	4.5	4.4	4.4	4.4
Nominal Salary (median)	1,224.8	1,388.2	1,293.8	1,433.2

Note: This table shows information regarding junkyards of São Paulo state collected from the Ministry of Labor's RAIS dataset (RAIS - MTE).

Figure A.1 – Junkyards: how do they acquire cars to dismantle?



Notes: Junkyards can use the legal or illegal market to acquire cars to dismantle. Changes in supervision and punishment may make it difficult to access the illegal market, decreasing the demand for stolen vehicles.

Figure A.2 – Field Evidence - Items sold by Junkyards

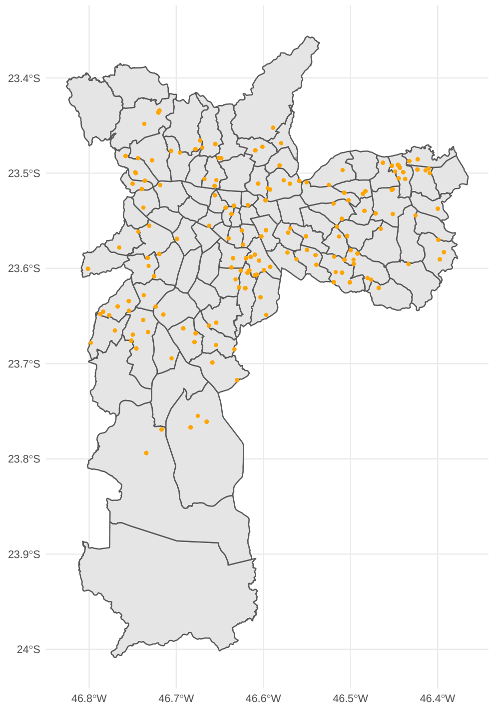
(a)

(b)

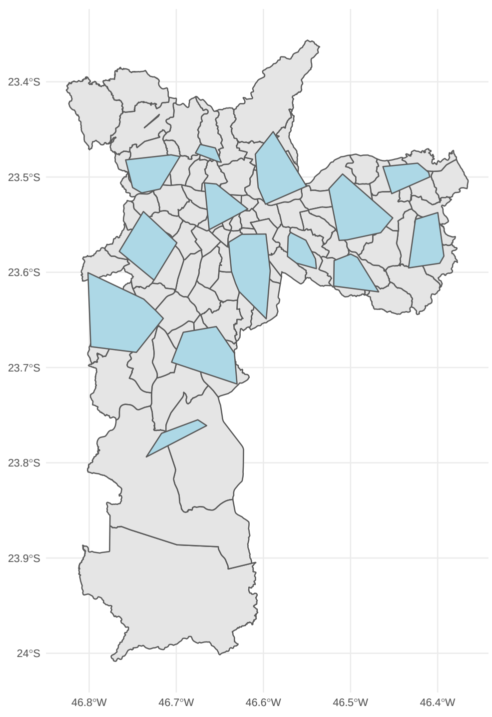
(c)

Notes: Photos taken in Salvador Luchesi's Junkyard in São Paulo. After the law, any vehicle spare part in stock must present an identifier QR Code proving the legal origin of the product. The consumer can verify the authenticity of the QR code in the State Traffic Authority ("DETRAN-SP") by scanning it using a smartphone.

Figure A.3 – Clustering Analysis



Junkyards coordinates.



Junkyards clusters.

Figure A.4 – District Level - Baseline Results

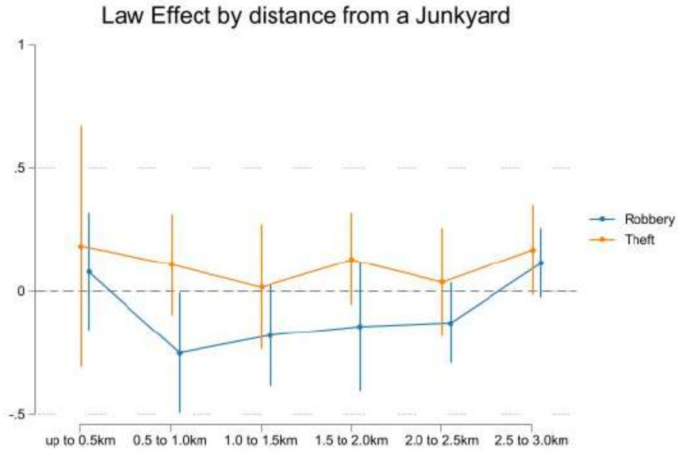
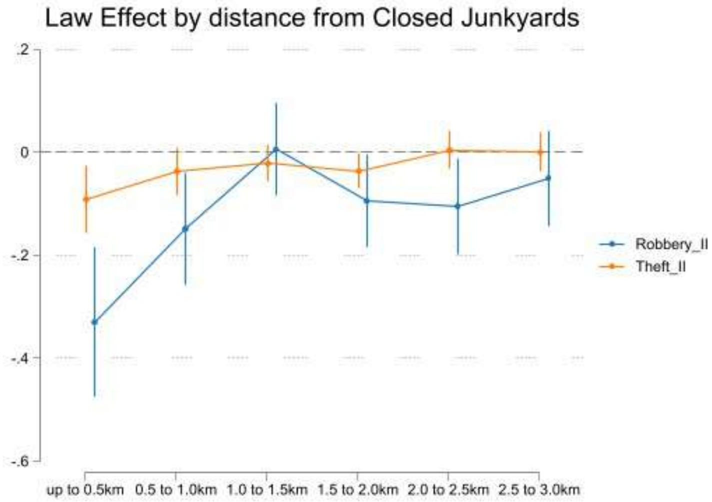
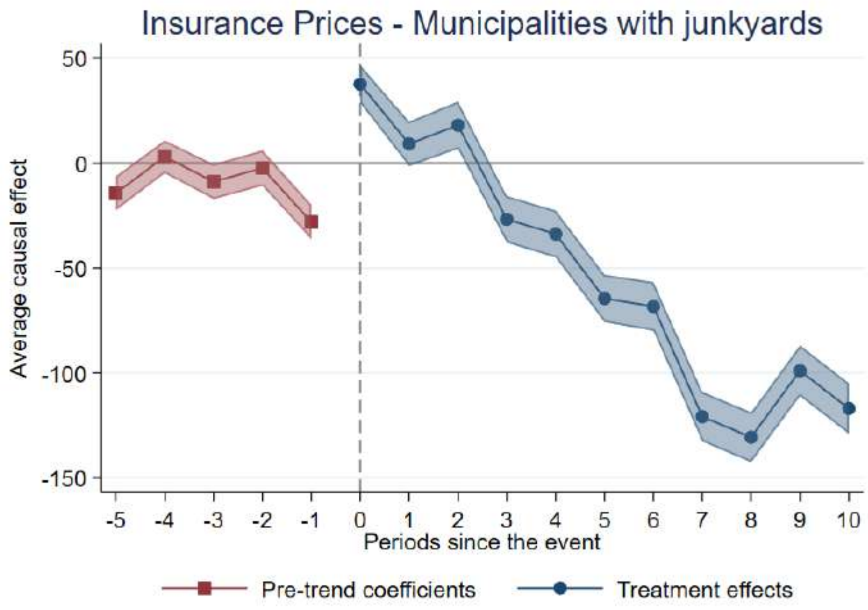


Figure A.5 – District Level - Closed Junkyards



Notes: These graphs plot estimated coefficients to the distance of junkyard clusters (top row) and closed junkyards (bottom row) for equation 2.5. The sample includes 14,479 census tracts in São Paulo. Covariates include census tract, district, year, and month fixed effects. Errors are clustered at the district level. Dots represent point estimates, and the bar represents a 95% confidence interval.

Figure A.6 – Event Study - Insurance Prices



Notes: The effect of the Junkyard Law on vehicle insurance prices is estimated under the unconditional parallel trends assumption. Red lines give point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the municipality level. Under the null hypothesis of the parallel trends assumption holding in all periods, these should be equal to zero. Blue lines provide point estimates and uniform 95% confidence bands for the treatment effect of the Law allowing clustering at the municipality level. The treatment in my differences-in-differences design is given by the variable *"Junkyard Law $\times$ Presence of Junkyard"* that assumes value one from July 2014 only to municipalities in São Paulo that have at least one junkyard.

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2 When the State steps down: Police Strikes and gang-related deaths in Brazil.

2.1 Introduction

Large-scale violence involving organized crime presents a significant challenge to law enforcement agencies, and the success in countering such criminals depends on how state agents and police forces interact with drug trade organizations (Trejo and Ley, 2020; Arias, 2006). However, the effectiveness of state interventions in reducing violence remains unclear, as they may unintentionally exacerbate violent outcomes by stimulating rival gangs' attempts to gain control of territories, especially in the aftermath of a police crackdown (Dell, 2015). Moreover, while previous research has predominantly focused on the effects of increased police presence on crime (Magaloni, Franco-Vivanco, and Melo, 2020), little is known about the consequences of abrupt reductions in policing on criminal organizations. Specifically, it is unknown whether criminal gangs would exploit a decrease in police surveillance to regain control over drug trafficking turfs or to initiate violent attacks against rival groups.

This paper fills this gap in the literature by investigating the link between police surveillance and gang conflicts, focusing on the impact of policing on gang-related homicides in Brazilian states. Using police strikes as a natural experiment, I leverage the exogenous variation in surveillance to identify the causal impact of police presence on violent crime in areas where criminal gangs are competing for territory. Illegal markets are characterized by difficulties in assigning property rights and enforcing agreements (Schelling, 1971; Buchanan, 1973), which frequently result in violence being the primary option for criminals seeking to protect and expand their turf (Magaloni, Franco-Vivanco, and Melo, 2020). In such an environment, the threat of police intervention in gang conflicts may discourage some disputes that would otherwise have occurred.

Given Brazil's position as a top supplier of cocaine to Europe (REUTERS, 2020), controlling drug trafficking routes is critical to the success of any organized criminal group (OCG) (Calderón et al., 2015; Lessing, 2017). Specifically, the North and Northeast regions of Brazil are particularly strategic to OCGs as they serve as logistic hubs for drugs produced in neighboring countries. In this context, the impact of gang conflicts and increased levels of violence in disputed areas is a pressing concern for state authorities. In this paper, I focus on the conditions that police strikes may escalate the violent outcomes of drug trafficking disputes, specifically in cases where OCGs perceive a reduction in police surveillance as an opportunity to attack their rivals aiming for a hegemonic position in contested areas.

I test the link between reduced police surveillance and gang-related homicides with a novel dataset combining daily homicides and police strike events from 2000 to 2020. I focus on homicides following police strikes across states in Brazil to investigate if violent deaths increased more in regions disputed by criminal gangs. Regarding the identification of gang-disputed territories, I leverage media reports and socioeconomic information to define areas where there is arguably a high incidence of gang conflicts. For the state of Ceará, I assess more granular data at the district level that allows for identifying areas with reported gang presence within a municipality and also checking the criminal records of victims.

Overall, Brazilian states affected by Military Police strikes report 45% more homicides in these events, and deaths are mostly caused by firearms used in public spaces. Furthermore, I show in a differences-in-difference approach that gang-disputed territories in Ceará are disproportionately affected with a 69% increase in daily homicides, for which suspected gang members account for 66% of the victims. There is no evidence of increases in other types of death or a previous upward trend in homicides before the strike, which rules out the possibility of a generalized increase in violence during and before the event. Last, I do not observe the same increase in violent deaths in Ceará following a police strike that occurred in the context of a single hegemonic criminal gang, which reinforces the relevance of gang competition to understanding a huge increase in homicides during these events.

To shed light on the mechanisms driving my results, I present a model drawing from previous literature on conflicts (Powell, 2006) that demonstrates which conditions trigger a war when there are shifts in the expected payoff of confrontation. I use my theoretical model to investigate why an OCG decides to attack a rival following police strikes, which in this framework decreases confrontation losses given a reduction in the probability of police intervention. In this setting, if a criminal group chooses to accommodate instead of fighting, the rival can use the opportunity to attack first. Therefore, a larger expected payoff relative to the confrontation costs may stimulate gangs to choose to fight rather than be accommodating.

My findings contribute to the literature on state presence and organized crime (Arias, 2006; Calderón et al., 2015; Dell, 2015; Lessing, 2017; Magaloni, Franco-Vivanco, and Melo, 2020; Blattman et al., 2021) by providing new evidence on the impact of police patrols on crime rates. I leverage a natural experiment to present robust evidence that reductions in police patrols can have a significant effect on crime (Andenaes, 1974; Takala, 1979; White, 1988; Sherman and Eck, 2003; Cardoso and Resende, 2018; Piza and Chillar, 2021). Specifically, I show that this effect may be distinct from the traditional deterrence and incapacitation effects of policing (Becker, 1968; Ehrlich, 1981), as reductions in patrols may have broader impacts on crime through other channels. Therefore, I extend the existing literature by highlighting that reduced police presence may trigger violent gang conflicts in settings where organized crime groups pose a serious threat to the state. Overall, this paper sheds light on the relevance of considering the broader socioeconomic context in which changes in police surveillance occur[1].

[1] Similarly, White, 1988, shows that the social tension between the upper and working class contributed to the

Last, my findings also contribute to a broader literature on the effect of police on crime (Levitt, 1995; Levitt, 2002; McCrary, 2002; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca, Machin, and Witt, 2011), specifically in the context of organized criminal groups. While previous research has used instrumental variables and terrorist attack-related events to examine the impact of increased policing on crime, they have not addressed the challenges posed by criminal organizations. These studies have typically focused on contexts where gang conflicts are uncommon, such as after a terrorist attack where police deployment is concentrated in specific locations. In contrast, my findings provide rigorous empirical evidence about the effect of police presence on crime in an environment of intense gang confrontation and lethality. My research offers new insights into the role of police in deterring property and violent crimes in these complex settings.

The remainder of this paper is organized as follows. Section 2.2 presents an overview of police forces and criminal gangs in Brazil. Section 2.3 introduces the conceptual framework and some comparative statistics exercises. I introduce the data and describe the empirical strategy in Section 2.4. Sections 2.5 and 2.6 present the main results, such as sensitivity and robustness checks. Section 2.7 concludes.

2.2 Criminal Gangs, Police Strikes and Violent Deaths in Brazil

2.2.1 Gangs at War (Brazil, 2016-2020)

The Brazilian drug trade is dominated by two criminal organizations, the "*Primeiro Comando da Capital*" (PCC) and the "*Comando Vermelho*" (CV), which have historically hold a non-compete agreement. However, in mid-2016, PCC started an expansionary plan to control drug trafficking on the Brazil-Paraguay border, disrupting the agreement with CV and inciting a series of gang conflicts throughout the country (ELPAIS, 2016). To keep control of strategic regions, PCC and CV formed alliances with local gangs in the North and Northeast of Brazil, such as the "*Família do Norte*" (FDN) and "*Guardiões do Estado*" (GDE). The case of GDE in Ceará is noteworthy as its rapid growth was supported by PCC[2].

The growth of GDE and increased competition in drug trafficking faced resistance from CV, leading to intense conflicts in peripheral areas of Fortaleza, the state capital. This turf war has had devastating consequences for the citizens of Ceará, as the gangs regulate district traffic hours, forcibly remove people from their homes, and punish civilians suspected of being informants or

increased violence during the Boston police strike in 1919.

[2] Documents intercepted in an investigation conducted by the State Secretary of Penitentiaries (link) show that the constitution of GDE occurred on January 1, 2016, and it estimates that almost 20 thousand prisoners joined the criminal organization (about 70% of the prisoners of Ceará) in a process called *"batismo"* (baptism). Even as an independent group, the gang leadership has settled a temporary agreement to have PCC as an ally and drug supplier.

associated with rival gangs. As a result of these gang conflicts, as both groups have recruited a large number of soldiers inside and outside of prisons, violent deaths have sharply increased in recent years (FOLHA, 2018).

2.2.2 Strikes of Police Forces in Brazil (2000-2020)

The Brazilian public security system is a complex structure comprising various branches of law enforcement, including the Federal Police, State Police, and Municipal Guards. The State Police is responsible for street patrol and criminal investigations, with the Military Police handling surveillance and repression of criminal acts and the Civil Police conducting investigations.

According to the Federal Constitution, police forces, including the Armed Forces and Military Police, are prohibited from going on strike. This prohibition was extended to the Federal and Civil Police by a Supreme Court ruling in 2017. The reasoning is that police forces provide an essential service to society that cannot be interrupted, as it would expose civilians to danger. Despite this legal restriction, there have been numerous police strikes in different Brazilian states since 2000, primarily demanding improved wages, benefits, and working conditions. Strike-participating police officers may face administrative prosecution and military crimes, but most are eventually pardoned as part of agreements to restore police patrols.

Moreover, the occurrence of police strikes in Brazil is set against a backdrop of high levels of criminal activity and a sky-high rate of homicides. In 2020, the country had over 50,000 intentional violent deaths, an average rate of 23.6 homicides per 100,000 inhabitants (FBSP, 2021). Brazilian cities have been identified as some of the most violent places in the world regarding homicides per 100,000 people, with Caucaia/CE, Feira de Santana/BA, and Cabo de Santo Agostinho/PE being particularly concerning. Although the motivations behind most homicides are unclear, there is some evidence pointing to the interplay between gang conflicts and violent deaths (Silva Lopes and Ferreira, 2021).

2.3 Conceptual Framework

This section presents a conceptual framework to explain how a decline in the probability of police intervention increases the expected payoff in gang conflicts, leading criminals to choose violence over accommodation. In periods of reduced police patrolling, gangs may use violence to expand their territories. Despite the high costs of such conflicts, the absence of police intervention creates a strong incentive to conquer contested drug-trafficking territories. Therefore, sudden shifts in surveillance, such as police strikes in Brazil, can have a significant effect on increasing violent deaths by fueling the escalation of criminal gang conflicts. Understanding the dynamics of gang conflicts in the absence of effective law enforcement can provide valuable insights into designing policies to reduce gang-related violence.

2.3.1 The conceptual setting

In areas where multiple criminal organizations coexist, conflicts over drug trafficking control are almost inevitable. The absence of enforceable property rights and contracts prevents gangs from committing to a stable territorial division (Levitt and Venkatesh, 2000). These groups may use violence to attack rivals and expand their turfs, which may lead to police intervention in these contested territories. Specifically, the military police respond to shootings in gang turfs by mobilizing officers to prevent further violence and protect civilians. However, during periods of reduced police capacity, criminals may perceive a lower probability of intervention and exploit the opportunity to attack their rivals without police interference.

The occurrence of police strikes in Brazil leads to a significant reduction in police patrolling, which is highly visible to criminals due to widespread media coverage. Despite their short duration of a few days on average, these strikes can have a profound impact on the incentives of criminal gangs fighting for the control of drug trafficking. Consequently, police strikes may act as a trigger for gang conflicts within territories contested by these groups. This, in turn, is likely to result in a higher incidence of violent deaths in these regions, particularly among individuals associated with criminal activity and drug trafficking.

2.3.2 Theoretical Model

I propose a novel model to analyze the dynamics of criminal gang conflicts. I explore the inability of criminal organizations to commit with a stable territorial division and the potential interference of the military police when a conflict starts. I show how the police affect the gang's decision to start a war, even when there is no change in the balance of power. My model emphasizes the relevance of exogenous shifts in the probability of police intervention that may trigger violent gang conflicts[3].

In the context of drug trafficking, two gangs, "A" and "B" compete over a flow of benefits or "a pie" valued "V" within a given area. The Military Police, represented by a player "C" intervenes when violent gang conflicts occur. In this setting, gangs A and B share the pie into Q_A and Q_B, respectively, where $Q_A + Q_B = V$, excluding the possibility of a Pareto improvement. In each period, gangs can choose to use violence to secure a payoff $D_g(t)$, but this action is costly and depletes the gang's resources, including weapons, soldiers, and supplies. If in time *t* a gang decides to break the original status quo division and use violence to fight for a monopolistic position it obtains:

$$\begin{aligned} D_g(t) &= (p_g * V) + (1 - p_g) * 0 - L_g - (p_C * L_C) \\ &= (p_g * V) - L_g - (p_C * L_C) \end{aligned} \tag{2.1}$$

Where p_g is the probability that a gang *g* = *A, B* achieves a monopoly over the contested territory after spending the resources L_g. For the sake of simplicity, I assume $p_A + p_B = 1$. If *A*

[3] In Appendix 2.9 I show extensions of the model to account for multiple periods and the possibility of bargaining.

chooses to run after the beginning of a war, the gang reduces confrontation damages, but it also loses the future flow of trafficking rents. To simplify this scenario, I assume a zero payoff when gang g is defeated with probability $(1 - p_g)$, no matter if this gang chooses to fight or run away. Last, I consider a probability p_C of police intervention that causes additional losses in criminal conflicts.

Therefore, gang g obtains at least $D_g(t)$ if it decides to fight, which is the minimum payoff in any equilibrium. Under these conditions, a gang decides to attack the rival when the expected payoff of a conflict is larger than the benefits from the current *status quo* division:

$$D_g(t) > Q_g(t) \tag{2.2}$$

Regarding the possible equilibrium in a setting with criminal gangs g = *A, B*, Equation 2.2 indicates four possibilities to a given probability of police intervention p_C:

1. $(D_A(t) \leq Q_A(t); D_B(t) \leq Q_B(t)) \rightarrow (A$ chooses *"Peace"*; B chooses *"Peace"*)
2. $(D_A(t) \leq Q_A(t); D_B(t) > Q_B(t)) \rightarrow (A$ chooses *"Peace"*; B chooses *"War"*)
3. $(D_A(t) > Q_A(t); D_B(t) > Q_B(t)) \rightarrow (A$ chooses *"War"*; B chooses *"War"*)
4. $(D_A(t) > Q_A(t); D_B(t) \leq Q_B(t)) \rightarrow (A$ chooses *"War"*; B chooses *"Peace"*)

The effective deterrence of gang conflicts is largely dependent on the parameters of confrontation costs (L_A, L_B) and the probability of police intervention (p_C) in my model. When these parameters reach a certain threshold, the majority of the time, both gangs A and B will choose to maintain *"Peace"*. To investigate the effect of police strikes on the behavior of criminal gangs, I focus on the conditions that shifts in p_C may stimulate one or both gangs to choose to attack their rivals. By examining this mechanism, I aim to shed light on the interplay between police and gang violence.

2.3.2.1 The Decision to Start a War

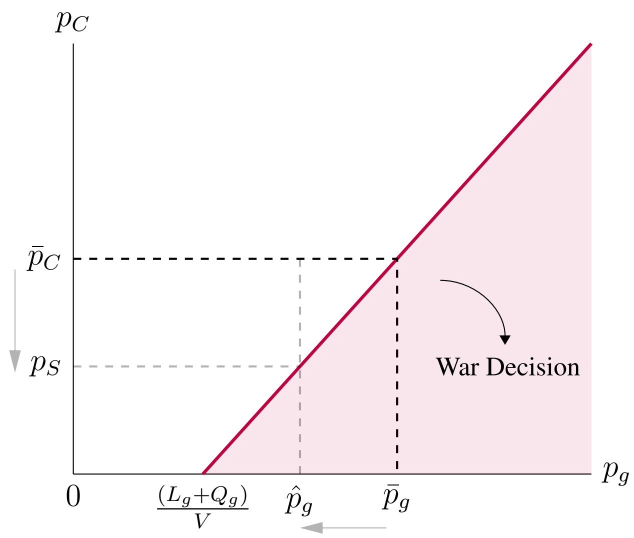
In areas where gangs hold significant power and influence, military police strikes can have a deep impact on the dynamics of criminal activity. By reducing the perceived threat of police intervention, represented by the variable p_C, these strikes decrease potential losses in gang conflicts. Combining Equation 2.1 and 2.2, it is possible to assess how changes in the probability of police intervention p_C trigger violent conflicts:

$$\begin{gathered}(p_g * V) - L_g - p_C * L_C > Q_g \\ p_g * \frac{V}{L_C} - \frac{(L_g + Q_g)}{L_C} > p_C\end{gathered} \tag{2.3}$$

Equation 2.3 indicates the conditions under which a gang will initiate a conflict, given a particular combination of victory probability (p_g), conflict losses (L_g, L_C), and turf control

(Q_g). In light of this equation, Figure 7 offers a visual representation of the impact of police intervention on the decision-making process of gangs A and B. Given the probability of police intervention $\bar{p}_C$, the decision to engage in war is contingent on a victory probability surpassing $\bar{p}_g$.

Figure 7 – A Theoretical Model for Gang Conflicts



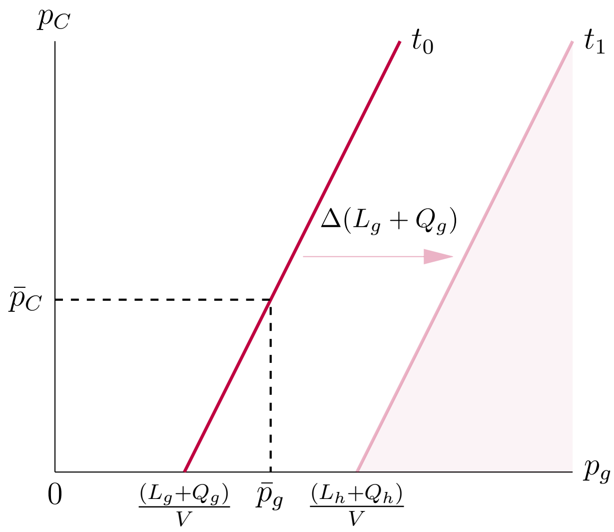
Notes: The dashed area shows when a gang decides to start a war. **(I.)** To some $(\bar{p}_C, L_g, Q_g)$, if a gang g with probability of victory p_g starts a war, any gang with $p_h > p_g$ also decide by the conflict. **(II.)** A decrease in the probability of police intervention from $\bar{p}_C$ to p_S reduces the minimum probability of victory for which a gang g decides to start a war.

Hence, in a scenario where two rival gangs, A and B, share equal territorial control and face identical expected conflict losses, the gang with a larger p_g is predisposed to initiate a conflict with the opponent at a specific level of police intervention. This feature is illustrated by the shaded region in 7, which depicts the decision threshold for a gang to start a war, given the probability of police intervention. At certain combinations of variables $(\bar{p}_C, L_g, Q_g)$, if a criminal gang with $\bar{p}_g$ chooses to initiate a conflict, then any rival gang with $p_g > \bar{p}_g$ would also choose war. These findings suggest that gang violence is affected not only by the degree of police intervention but also by the relative power dynamics and strategic calculations of both gangs.

2.3.3 Police Strikes

How do shifts in the probability of police intervention affect the decision of a criminal gang to start a war? In this paper, I am interested in the response of drug trade organizations to police strikes that arguably decreased conflict losses. I show in Figure 7 that a decrease in the probability of police intervention to p_S results in a corresponding decrease in the minimum probability of victory necessary for a gang to decide to initiate a war. Consequently, given the

Figure 8 – Shifts in Conflict Losses and Territory Control



Notes: The dashed area shows when a gang decides to start a war after a positive shift in conflict losses and territory control. **(P3)** The higher the conflict costs fewer the combinations of p_c and p_g that trigger a conflict.

same parameters, (L_g, Q_g), gangs that would not have initiated a war under the standard level of police intervention $\bar{p}_C$ may now choose to engage in conflict following a police strike.

Equation 2.3 and Figure 7 provide some insights regarding the mechanisms that drive increased gang conflicts following reductions in police patrolling. A police strike amplifies the perceived gains from engaging in a conflict, compelling gangs to attack the rival even in situations where territory is evenly split. Furthermore, if the decrease in the probability of police intervention reaches a lower critical threshold, and criminal organizations suffer comparable losses in conflicts, even groups with a low probability of victory will choose to start a war.

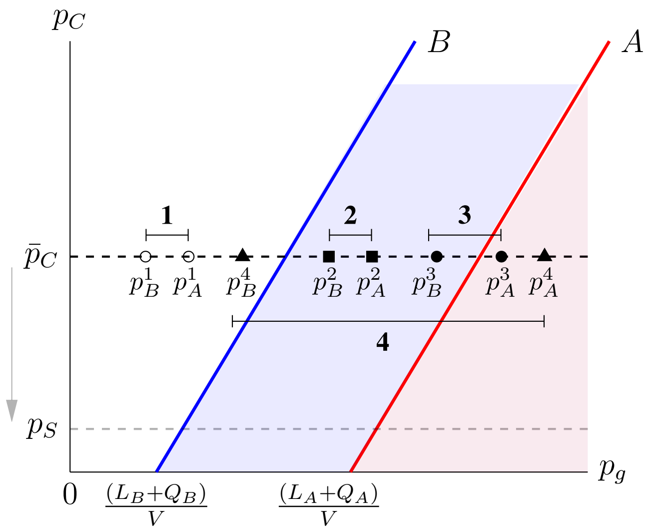
2.3.3.1 Comparative Statics

Now I will show some comparative statics exercises. First, I present the effect of a shift in conflict losses and turf control on the decision of a gang to start a war, and second, the possible equilibrium following police strikes when the distribution of power between gangs is unequal. In both exercises I consider only the case of an interior solution, that is, when $(p_c, p_g, L_g, Q_g, V) > 0$.

2.3.3.2 Shifts in Conflict Losses and Turf Control

Figure 8 illustrates the link between gang violence, police intervention, and conflict losses. Specifically, the graph demonstrates that in t_0 a gang with a probability of victory $\bar{p}_g$ will initiate a conflict if the probability of police intervention falls below $\bar{p}_c$. However, an increase in conflict costs, represented by $\Delta(L_g + Q_g)$, shifts the curve to the right in t_1. As a result, the same

Figure 9 – Distribution of Power and Conflicts



Notes: The blue dashed area shows when gang B decides to start a war and the red shaded area the same to Gang A. **(I.)** Assuming the unequal distribution of power between gangs, i.e., $p_A > p_B$ and $Q_A > Q_B$, and the same conflict losses $L_A = L_B$, the distribution of (p_A, p_B) defines which gang will start a war. **(II.)** To any set of parameters $(pc, pg, Lg, Qg, V) > 0$, a decrease in the probability of police intervention increases the number of combinations that lead to war.

combination of $\bar{p}_g$ and $\bar{p}_c$ no longer triggers a war. Thus, as potential losses in confrontation and/or territorial control increase, conflicts become more difficult following police strikes. This shift can be so significant that even a gang with a high probability of victory will not choose to initiate a war. Consequently, when conflicts become too costly, fewer combinations of p_c and p_g will lead to violence, and reductions in police intervention may not be sufficient to incite a conflict between criminal gangs.

2.3.3.3 Possible Equilibrium following Police Strikes

How an unequal territorial division affects the possible equilibrium between criminal gangs following police strikes? Suppose that gang A is more powerful than gang B, which means a higher probability of victory ($p_a > p_b$) and a larger share of the disputed turf ($Q_a > Q_b$). Assuming equal losses in confrontation, i.e., $L_a = L_b$, the distribution of p_a and p_b defines which gang will start a war following shifts in the probability of police intervention $\bar{p}_C$.

Figure 9 presents the four possible scenarios of confrontation depending on the probability of victory of gangs A and B. The blue-shaded area shows when gang B starts a war, and the red-shaded area is similar to gang A. Assuming a probability of police intervention equal to $\bar{p}_C$, there are some values (p_A^1, p_B^1) for which no shift in police intervention makes both gangs decide to start a war (**scenario 1**). When the probabilities are (p_A^2, p_B^2), Gang B decides to start a war (**scenario 2**) whereas to (p_A^3, p_B^3) both gangs will choose the confrontation (**scenario 3**). Last, when the probabilities of victory are (p_A^4, p_B^4), gang A will decide to start a war (**scenario 4**).

Last, I show in Figure 9 that a decrease in the probability of police intervention from $\bar{p}_C$ to p_S increases the number of combinations that the decision to start a war is binding to both gangs to any set of parameters $(p_c, p_g, L_g, Q_g, V) > 0$. Thus, when the probability of confrontation with police decreases abruptly in police strikes, even gangs with few chances of victory and small turf control will decide to start a war. This is the more significant result of the model that reveals why a police strike potentially triggers violent conflicts between criminal gangs.

With such theoretical results, I now proceed to discuss the data and empirical strategy to assess the effect of reducing police patrolling on violent deaths and specifically on homicides registered in territories with documented criminal gang conflicts.

2.4 Data and Empirical Strategy

2.4.1 Data

I exploit the quasi-experiment of police strikes between 2000 and 2020 to investigate the effect of a sharp reduction in policing on violent crime. I collected daily homicide data from 5,568 municipalities located in 26 states and the federal district of Brazil, and I focus on violent deaths as defined by the International Statistical Classification of Diseases and Related Health Problems (ICD-10): aggression (X85-Y09) and legal intervention (Y35-Y36), sourced from the Ministry of Health Mortality Information System (SIM-DataSUS). For the state of Ceará, I access more detailed records provided by the state security secretary covering the period 2014-2020, with information on the district where the deaths occurred and the identification of the victims. Regarding the timing of police strikes, I used data from the Interunion Department of Statistics and Socioeconomic Studies (DIEESE)[4] based on reports from media, labor unions, and class associations.

Table 2 shows that the average number of homicides during Military Police strikes is higher than the average from 2000 to 2019. On the other hand, deaths during Civil Police strikes are not distinguishable from the average in the sample. Table 3 provides an overview of police strike statistics in Brazil. The duration of Civil Police strikes is notably longer compared to those of the Military Police. This disparity is partially attributed to the Supreme Court's extension of the strike veto to the Civil Police only in 2017. Figure B.14 highlights that most Military Police strikes last less than seven days.

Last, I use the personal identification of victims in the state of Ceará to examine any prior involvement in illicit activities. The state's Judiciary has an online platform that provides access to criminal case records filed in the courts. The system allows searching for records using personal names or case codes. My analysis draws on a database of 27,307 records obtained by consulting victims' names in the Judiciary's system. The database shows relevant information

[4] The report "Balanço das Greves" is available here. For this paper, I request to DIEESE more detailed records of all Civil and Military Police strikes between 2000 and 2020.

Table 2 – Daily Homicides Brazil - (2000-2019)

	Mean	Std. Err.	[95% Conf. Interval]	
Full Sample	1.462	0.0016	1.459	1.465
Military Police Strikes	1.757	0.0720	1.615	1.898
Civil Police Strikes	1.455	0.0088	1.437	1.475

Note: the average number of daily violent deaths across states of Brazil using the International Classification of Disease (ICD) codes X85-Y09, and Y35-Y36. *Source:* SIM - DataSUS.

Table 3 – Police Forces Strikes (2000-2020)

Police Force	**Events**	**Duration (Mean)**	**Duration (Std. Dev.)**
Military	29	8.52	6.04
Civil	194	20.46	29.85

Note: Strikes are measured as the length in days of reduced patrolling reported by newspapers, unions, and employee associations. *Source:* Balanço das Greves - DIEESE.

about each case, including the start and termination dates, as well as relevant tags that indicate the subject matter of each case. The parties involved in each case are identified by their full names, including defendants and plaintiffs.

I developed an indicator to measure engagement in illegal activities using three specifications for suspected criminals. Table 4 shows the tags used to track drug trafficking, violent crimes, and civil prosecution. At least 53% of individuals identified in the data have prior criminal records. When considering only violent and gang-related crimes, this percentage decreases, yet still stands at 39%. I excluded repeated names to mitigate over-identification, even when deaths occurred at different dates or locations. It is important to note that not all individuals involved in illegal activities have records in the Judiciary System. Thus, my results are a lower-bound estimate of the number of suspected criminals killed in Ceará. Table 5 presents a comparative analysis of the incidence of violent deaths across districts in Fortaleza. I compare the full dataset with a reduced sample comprised of only suspected gang members before and after the GDE foundation in 2016. The results highlight significant heterogeneity among the districts, with some experiencing a substantial decrease in homicides post-GDE (e.g., District 5 reported a 17% decrease), while others saw an increase (e.g., District 13 saw a 16% growth). This heterogeneity in violent deaths across districts after 2016 suggests that gang conflicts may disproportionately impact certain regions of Fortaleza.

2.4.2 Empirical Strategy

2.4.2.1 Identifying Gang Turfs in Ceará

Determining the location of gang turfs is a challenging task. Despite the lack of official documentation outlining the headquarters of these criminal organizations, local media in Ceará

Table 4 – Identifying Suspected Criminals

Suspected Criminals (1) n = 14.423	Robbery, Theft, Drug Trafficking, Criminal Gang Member, Illegal Gun Possession, Domestic Violence, Falsification, Fraud, Traffic Transgression and Stolen Goods.
Suspected Criminals (2) n = 12.694	Robbery, Theft, Drug Trafficking, Criminal Gang Member, Illegal Gun Possession, Domestic Violence and Stolen Goods.
Suspected Criminals (3) n = 10.675	Robbery, Drug Trafficking and Criminal Gang Member.

Notes: In the full sample there are 27.307 registers with personal identification. At least 14.423 (53%) of them present a previous criminal record in the State Judiciary System.

Table 5 – Violent Deaths by District - Full Sample and Suspected Criminals

		Homicides (Full Sample)		**Homicides (Susp. Criminals)**	
		Pre	Post	Pre	Post
District 1	Mean	1.673	1.366	0.775	0.611
	Std. Err.	(0.041)	(0.040)	(0.035)	(0.035)
District 2	Mean	1.806	1.505	0.770	0.620
	Std. Err.	(0.045)	(0.032)	(0.035)	(0.024)
District 3	Mean	1.383	1.377	0.718	0.609
	Std. Err.	(0.044)	(0.035)	(0.039)	(0.030)
District 4	Mean	1.669	1.244	0.757	0.562
	Std. Err.	(0.048)	(0.030)	(0.033)	(0.031)
District 5	Mean	1.671	1.391	0.745	0.611
	Std. Err.	(0.046)	(0.038)	(0.036)	(0.029)
District 6	Mean	1.093	1.345	0.500	0.572
	Std. Err.	(0.037)	(0.037)	(0.066)	(0.030)
District 7	Mean	1.414	1.389	0.540	0.570
	Std. Err.	(0.036)	(0.037)	(0.034)	(0.024)
District 8	Mean	1.592	1.486	0.679	0.672
	Std. Err.	(0.042)	(0.034)	(0.035)	(0.028)
District 9	Mean	1.475	1.345	0.566	0.585
	Std. Err.	(0.042)	(0.026)	(0.036)	(0.022)
District 10	Mean	1.517	1.254	0.523	0.501
	Std. Err.	(0.045)	(0.028)	(0.033)	(0.028)
District 11	Mean	1.528	1.666	0.593	0.684
	Std. Err.	(0.041)	(0.033)	(0.035)	(0.022)
District 12	Mean	1.366	1.692	0.546	0.731
	Std. Err.	(0.035)	(0.035)	(0.033)	(0.024)
District 13	Mean	1.338	1.559	0.537	0.655
	Std. Err.	(0.041)	(0.036)	(0.039)	(0.024)

Notes: The average violent deaths registered across districts of Ceará reported by the Security Secretary. Suspected criminals' deaths reported using Specification 3 presented in Table 4. *Source:* SSPDS-CE.

covers the districts where police and community reports indicate the presence of such groups[5]. These areas are typically situated in the peripheries of Fortaleza, characterized by their strategic location near major roadways that provide access to the Port of Ceará, a crucial conduit for the export of illicit drugs to Europe (VEJA, 2021). Additionally, these neighborhoods are often characterized by poverty and higher levels of violence compared to other districts of the metropolitan region of Ceará.

To identify criminal gang areas in Ceará, I combined qualitative information from local news sources with data on socioeconomic indicators, including average income, violent deaths, and the proportion of slum areas within the respective territories. Based on this analysis, I have identified districts where local media reports the presence of criminal gangs and where the aforementioned socio-economic indicators fall below the median values for the urban area of Fortaleza. In my setting, districts 6, 11, 12, and 13 are assumed as potential criminal gang turfs and are depicted in red on a map, as shown in Figure B.1. The remaining areas are considered non-target districts ("*control group*"). Despite the challenges associated with objectively measuring criminal gang presence, I assume that these areas present a larger probability of being disputed by criminal organizations.

To provide additional evidence about the incidence of gang conflicts in these districts, I show in Figure B.7 the results of a t-test comparing the monthly homicides of suspected gang members before and after the entry of GDE and increased gang competition. My results show a significant increase in homicides in districts 6, 11, 12, and 13, while most other districts saw a large and significant decrease. This suggests a possible reallocation of gang-related conflicts and criminals to the territories disputed by GDE and CV. Table 6 shows that districts 11, 12, and 13, show some of the lowest income levels and highest homicide rates in Ceará. By leveraging the heterogeneity between these areas, I will exploit the variation in violent deaths across gang territories and districts of the "*control group*" in the metropolitan region.

2.4.2.2 Polices Strikes, Homicides and Gang Related deaths

To assess the effect of police strikes on violent deaths, I use two identification strategies. The first approach leverages the natural experiment created by 29 Military Police strikes, and 194 Civil Police strikes since 2000 in different states of Brazil. The reduction in policing during these strikes provides a unique opportunity to assess the effect of law enforcement on violent deaths. Furthermore, I examine the heterogeneity in the effect of police strikes across the type of police force (Military and Civil) and the states that have or do not have ongoing gang conflicts. Specifically, I will focus on comparing the increase in violent deaths in Ceará with other states in Brazil. In my setting, some states are unaffected by Military or Civil Police strikes in the period of analysis. Thus, the identification of the effect of police strikes comes from the exogenous

[5] I collected data from different sources, but especially the newspaper *"O Povo"*. One of these articles is available at this link

Table 6 – Socioeconomic Variables by District

District	Population	% Non White	% Men	Avg. Hous. Income	Hom.'000 people (avg. 2017-20)
District 1	173,761.00	47.3%	45.7%	R$ 6,341.36	37.84
District 2	214,388.00	70.0%	49.0%	R$ 1,149.90	86.06
District 3	205,137.00	68.6%	48.6%	R$ 1,454.04	69.10
District 4	164,268.00	60.3%	46.5%	R$ 2,455.80	47.03
District 5	313,642.00	58.0%	46.8%	R$ 2,461.27	35.07
District 6	362,681.00	62.3%	47.4%	R$ 1,863.39	36.33
District 7	265,925.00	62.9%	48.2%	R$ 2,639.19	48.42
District 8	236,970.00	68.2%	48.1%	R$ 1,334.49	44.20
District 9	233,811.00	66.7%	48.5%	R$ 1,449.47	65.44
District 10	176,767.00	50.5%	46.1%	R$ 4,797.70	30.83
District 11	405,347.00	69.5%	48.4%	R$ 1,162.96	92.45
District 12	453,354.00	71.0%	48.8%	R$ 981.37	86.85
District 13	436,962.00	70.1%	49.9%	R$ 1,447.73	68.77

Notes: Data regarding population, race, gender, and income obtained from CENSO-2010. Average income by household reported in 2010 BRL. The average number of homicides by district reported by the Security Secretary. *Source:* IBGE, CENSO-2010 and SSPDS-CE.

variation in homicides caused by police strikes, both across states that are affected and not affected and before and after each strike event.

The panel data structure allows controlling for unobserved time and location-fixed effects, which may be correlated with strike occurrences. The high frequency of the data enables us to precisely determine the start date of each strike, further reinforcing the exogeneity of the variation in policing. The random timing of police strikes minimizes the influence of other confounding factors, such as changes in unemployment or state-level budget cuts, which may not affect crime rates in the very short term. I also include state-fixed effects to control for unobservable crime determinants invariant at the state level. The equation to estimate the police effect on homicides across states in Brazil is:

$$homicides_{it} = \alpha_i + \beta_1 * \text{PM}_{strike_{it}} + \beta_2 * \text{PC}_{strike_{it}} + \phi_t + \mu_{it} \tag{2.4}$$

Where the subscripts i and t respectively denote state and date; $\text{PM}_{strike_{it}}$ and $\text{PC}_{strike_{it}}$ are dummies equal to one during the military and civil police strike days in the state i; ϕ is a set of time-fixed effects that includes the year, month, and weekday dummies; α are state-fixed effects. The dependent variable, homicides, represents the daily number of violent deaths in a specific state. The SIM-DataSUS provides information on various demographic factors, including gender, age, and cause of death, making it possible to assess the heterogeneous effect of police strikes on different specifications of the dependent variable in the Equation 2.4.

A possible inference concern is the potential serial correlation in the dependent variable over time in this framework. The standard solution is to estimate standard errors allowing for within-cluster auto-correlation. However, the validity of robust cluster estimators depends on the number of clustering units, and settings with few clusters generally lead to biased estimators (Cameron and Miller, 2015; Angrist and Pischke, 2008). As my quasi-experimental setting shows few clusters ("states"), I estimate bootstrap standard errors to overcome a potential auto-correlation.

The identification strategy relies on two key assumptions. The first is that the start date of a police strike is unpredictable and thus exogenous to criminals. Therefore, the decision of police officers to go on strike that day is not related to other factors that can affect violent deaths in the short run. In this case, strikes represent a quasi-experiment that breaks the simultaneity between crime and police presence. The second crucial hypothesis is that the increase in violent deaths during strike days is exclusively due to the sudden reduction of police officers in the streets and not other confounding factors.

In my second empirical analysis, I exploit the variation in homicides across districts of Ceará following a police strike. Specifically, I use a differences-in-differences model to estimate the effect of reducing police patrols on violent deaths, focusing on gang turfs. This analysis aims to test if a drastic reduction in police leads to increased violent deaths in these gang-controlled territories. By incorporating data on the victim's criminal background, I shed light on the extent to which the increase in deaths can be attributed to individuals with prior criminal records. A significant increase in gang-related deaths would support my theoretical model's hypothesis that reducing police surveillance allows criminal gangs to engage in violent confrontations with their rivals.

I include a series of time-fixed effects to absorb all common shocks in the evolution of homicides across districts, and I also include location-fixed effects to control unobservable crime determinants invariant at the district level. In this setting, gang turfs are the "*treatment group*", while the remaining districts are the "*control group*". I obtain the difference-in-differences estimator of the effect of a police strike on homicides in gang turfs using the following model:

$$homicides_{jt} = \alpha_j + \beta_1 * \text{PM}_{strike_t} + \beta_2 * (\text{PM}_{strike_t} * Area_j) + \phi_t + \mu_{jt} \tag{2.5}$$

Where the subscripts j and t respectively denote districts and date; $Area_j$ is a dummy equal to one if the district is a gang turf as specified in the previous section; PM_{strike_t} is a dummy equal to one during the military police strike days; ϕ is a set of time-fixed effects that includes the year, month, and weekday dummies; α are district-fixed effects. The dependent variable $homicides$ indicates the number of daily homicides in a given district. Similarly to the state level approach, I evaluate the heterogeneous effect of the police strike using the information on victims' gender and age. I estimate bootstrap cluster-robust standard errors to calculate the confidence intervals and alleviate inference concerns regarding a few clusters (Cameron and Miller, 2015).

In this framework, the identification strategy relies on the hypothesis that gang turfs and districts of the "*control group*" present parallel trends in violent deaths before the police strike. Therefore, the coefficient β_2 represents the increase in homicides caused by a Police Strike in criminal gang turfs. The second hypothesis is that the increase in violent deaths following a police strike is exclusively due to conflicts in criminal gang turfs and not other confounding factors.

2.4.2.3 Event Study

Regarding the possibility that strikes occur precisely in periods of growing violence, I employ an Event Study to assess the dynamic effects of police strikes on crime by analyzing the trends in homicides before, during, and after the event. By including a variable that indicates the number of days before and following the start of a strike, I aim to determine if there is evidence of a pre-trend in homicides. In this approach, I test if the increase in violent deaths results from the strike or a continuation of a previous trend. I use the Event-Study specification presented in Clarke and Tapia-Schythe, 2021:

$$homicides_{it} = \alpha_i + \sum_{j=2}^{J} \beta_j (Lag_j)_{it} + \sum_{k=1}^{K} \gamma_k (Lead_k)_{it} + \phi_t + \mu_{it} \tag{2.6}$$

Where lags and leads to the beginning of a strike are defined as follows:

$$\begin{aligned}
(Lag_J)_{it} &= 1[t \leq Strike_i - J] \\
(Lag_j)_{it} &= 1[t = Strike_i - j] \; for \; j \in (1, ..., J-1) \\
(Lag_k)_{it} &= 1[t = Strike_i + k] \; for \; j \in (1, ..., K-1) \\
(Lag_K)_{it} &= 1[t \geq Strike_i + K]
\end{aligned}$$

2.5 The Effect of Police Strikes on Violent Crime

2.5.1 Police Strikes and Violent Deaths

I begin by showing the results of Equation 2.4, using total homicides and deaths by gender as dependent variables. Table 7 presents the point estimates for the effect of military police (β_1) and civil police (β_2) strikes on homicides. My findings indicate that a decrease in police patrols leads to a significant increase in total homicides, particularly among men. The outcomes are only significant in the case of military police strikes, suggesting that surveillance and regular street patrols play a crucial role in deterring crime. The β_1 coefficient is equivalent to a 45% increase in daily homicides compared to the average.

Regarding the leading cause of death, I present in Table 8 that firearms are the primary factor driving the increase in violent deaths during police strikes. Additionally, I show in Table 9 that individuals between the ages of 15 to 45 years old are most commonly the victims, although these results should be evaluated with caution due to the significant amount of missing information on age in the SIM-DataSUS. Last, Table B.1 highlights that hospitals and public spaces, such as streets and avenues, are the most frequent locations where homicides occur in these events. The results are suggestive evidence of the profile of victims, young men killed by firearms in the streets of states affected by police strikes.

Table 7 – The effect of Police strikes on violent deaths - Baseline and Gender Results

	Dependent Variable	β_1	95% Confidence Interval	
			Lower	Upper
Military Police Strikes	Total Homicides	2.934	0.880	4.988
	Homicides (Men)	2.829	0.825	4.833
	Homicides (Women)	0.104	-0.022	0.230
	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
Civil Police Strikes	Total Homicides	0.364	-0.030	0.758
	Homicides (Men)	0.331	-0.053	0.715
	Homicides (Women)	0.032	-0.002	0.066

Notes: The coefficients β_1 and β_2 show the change in the number of violent deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level.

Table 8 – The effect of Police strikes on violent deaths - Cause of Death

	Dependent Variable	β_1	95% Confidence Interval	
			Lower	Upper
Military Police Strikes	Firearms	2.776	0.884	4.668
	White arms	0.116	-0.032	0.264
	Body Injuries	0.011	-0.028	0.050
	Car Crash	-0.008	-0.012	-0.003
	Legal Intervention	-0.002	-0.050	0.046
	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
Civil Police Strikes	Firearms	0.326	0.038	0.614
	White arms	0.000	-0.039	0.039
	Body Injuries	-0.002	-0.013	0.010
	Car Crash	0.004	-0.004	0.012
	Legal Intervention	0.025	-0.031	0.082

Notes: The coefficients β_1 and β_2 show the change in the number of violent deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level.

Table 9 – The effect of Police strikes on violent deaths - Homicides by Age

	Dependent Variable	β_1	95% Confidence Interval	
			Lower	Upper
Military Police Strikes	Homicides (<15)	0.116	-0.007	0.239
	Homicides (15 - 25)	0.681	0.055	1.307
	Homicides (26 - 45)	0.671	0.225	1.117
	Homicides (>45)	0.179	0.073	0.285
	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
Civil Police Strikes	Homicides (<15)	0.009	-0.012	0.031
	Homicides (15 - 25)	0.141	-0.006	0.288
	Homicides (26 - 45)	0.132	-0.007	0.271
	Homicides (>45)	0.026	-0.014	0.065

Notes: The coefficients β_1 and β_2 show the change in the number of violent deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level.

In summary, a decrease in police presence leads to a significant increase in violent deaths. This effect occurs only in strikes of the military police, which last an average of eight days, and I do not find a similar increase in deaths following strikes of the civil police in Brazil. The magnitude of my results is larger than estimates of a 16% increase in homicides by Cardoso and Resende, 2018, which is probably a consequence of using high-frequency data to reach a more precise identification of the effect of police strikes on homicides. My findings contribute to the literature about the effect of police on violent crime (Evans and Owens, 2007; Levitt, 2002), providing evidence of an adverse effect of decreasing police presence on violent deaths.

2.5.2 Police Strikes in Ceará

I proceed to examine the heterogeneous effects of police strikes on violent deaths across states in Brazil. My results indicate a 45% increase in daily homicides caused by military police strikes in Brazil, considering 29 of these events across different states. However, some states ins Brazil, such as Ceará, are heavily impacted by disputes over the control of drug trafficking. My theoretical model shows that in the context of gang confrontation, police strikes can trigger violent and lethal conflicts. Therefore, to assess the heterogeneous effect of police strikes in different criminal contexts in Brazil, I present the estimates of Equation 2.4 by state. The findings presented in Table 10 reveal that following a military police strike, the increase in homicides in Ceará was 2.5 times greater than my baseline results. This disparity was largely driven by an increase in male fatalities, but there was also a noticeable rise in female deaths.

To account for the different population sizes of Brazilian states, I provide a back-of-envelope calculation in the final column of Table 10. The impact of police strikes on violent deaths in Ceará (CE) is equivalent to 90% of the average daily homicides in the state, representing the highest proportional increase among all states in Brazil that experienced military police strikes between 2000 and 2020. Additionally, I combine these findings with information on the presence of drug trade organizations (DTOs) in each state. I utilize data from the *"Anuário Brasileiro de Segurança Pública"* (GAZETA, 2019), which reports the number of DTOs per state. Figure B.8 displays the estimates from the final column in Table 10 by the number of DTOs, revealing a positive correlation between the presence of criminal gangs and the rise in fatalities following police strikes.

Lastly, as discussed in Subsection 2.2.1, drug trade organizations initiated a series of violent confrontations across different states in Brazil starting in 2016. To gain further insights into the relationship between gang conflicts and the increase in homicides during strikes, I compare the change in fatalities before and after 2016. Figure B.9 illustrates that military police strikes after 2016 exhibit an effect that is 17% larger on homicides compared to events that occurred prior to this year. This evidence further highlights the role of criminal confrontations in the escalation of homicides during police strikes.

My findings demonstrate the considerable variation across states and the relevance of taking into account the social context in which police strikes occur. In regions where criminal gangs are prevalent, a sudden reduction in police presence can incite violent confrontations, enabling rival gangs to engage in conflict without state intervention. For the state of Ceará, the results suggest that a police strike may be a catalyst for the gang conflict mechanism described in the theoretical model. As the probability of police intervention (p_c) decreases dramatically, the expected reward of attacking a rival (D_g) increases significantly, resulting in gangs choosing to engage in conflict rather than leaving themselves vulnerable to attack.

Table 10 – The effect of Police strikes by State

	Total Homicides (β_1)	**Homicides (Men)**	**Homicides (Women)**	**Mean**	**(β_1 / Mean)**
Baseline	2.934***	2.829***	0.104*	6.45	0.45
	(1.027)	(1.002)	(0.063)		
AL	0.001	0.200	-0.198***	4.53	0.00
	(0.303)	(0.280)	(0.026)		
AM	1.704***	1.132**	0.572	3.22	0.53
	(0.524)	(0.491)	(0.034)		
BA	9.288***	8.939***	0.350***	12.67	0.73
	(0.307)	(0.294)	(0.015)		
CE	6.954***	6.577***	0.377***	7.76	0.90
	(0.358)	(0.336)	(0.025)		
ES	3.440***	3.200***	0.239***	4.46	0.77
	(0.626)	(0.589)	(0.037)		
MA	1.139***	1.238***	-0.098***	4.37	0.26
	(0.340)	(0.317)	(0.025)		
PA	3.867***	3.337***	0.531***	7.71	0.50
	(0.348)	(0.330)	(0.025)		
PB	1.202***	1.208***	-0.005	3.32	0.36
	(0.249)	(0.236)	(0.017)		
PE	3.275***	3.198***	0.077	11.22	0.29
	(0.880)	(0.827)	(0.054)		
PI	0.954**	0.979**	-0.024	1.90	0.50
	(0.402)	(0.379)	(0.026)		
PR	-2.333*	-2.491**	0.158**	7.89	-0.30
	(1.171)	(1.107)	(0.066)		
RN	1.901***	1.904***	-0.002	3.30	0.58
	(0.444)	(0.413)	(0.032)		
RO	0.000	0.136	-0.136***	1.99	0.00
	(0.388)	(0.365)	(0.025)		
RR	0.957***	1.073***	-0.116***	1.36	0.70
	(0.315)	(0.297)	(0.023)		
SC	1.424***	1.516***	-0.091	2.49	0.57
	(0.316)	(0.302)	(0.019)		
SE	-0.652	-0.779	0.127*	2.64	-0.25
	(1.315)	(1.245)	(0.0716)		
TO	0.588	0.477	0.112	1.67	0.35
	(1.313)	(1.240)	(0.0747)		

Notes: The coefficient β_1 shows the change in the number of violent deaths during Military Police strikes across states in Brazil. All errors are clustered at the state level.

2.5.3 Robustness

In this section, I present additional evidence regarding the validity of my results by exploiting previous trends and alternative specifications.

Parallel Trends. To address potential threats to my research design, I run an event study using Equation 2.6 to assess the robustness of the findings. The results of this exercise indicate that a military police strike leads to a 65% increase in homicides on days 2 and 3 compared to the daily average (Figure B.2), and there is no previous trend of growing violence before the events[6].

Heterogeneity. To examine the heterogeneity of the effect by the length of a military strike, I aggregated these events into three groups. The findings suggest a larger effect on violent deaths for strikes lasting more than 11 days, with no clear evidence of a previous trend in homicides in all groups (Figure B.10).

Unrelated Criminal Deaths. Last, I run a falsification test using pedestrian, cyclist, and biker traffic deaths as a dependent variable. The results in Table 11 show no significant increase in fatalities not directly related to decreased police patrols or caused by criminal activities. Overall, these results provide evidence of the effect of military police strikes on homicides.

Table 11 – Falsification Test - Deaths in Traffic Accidents

	Dependent Variable	β_1	**95% Confidence Interval**	
			Lower	Upper
Military Police Strikes	Pedestrians	0.099	-0.039	0.238
	Cyclists	0.018	-0.031	0.067
	Bikers	-0.007	-0.111	0.096

	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
Civil Police Strikes	Pedestrians	0.024	-0.031	0.078
	Cyclists	0.001	-0.015	0.016
	Bikers	-0.002	-0.065	0.060

Notes: The coefficients β_1 and β_2 show the change in the number of traffic accident deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level.

Alternative specification. Given that the number of homicides is count data with non-negative integer values, a Poisson regression model is often suggested as a more appropriate method to estimate the causal effect of police on crime. To address the relatively low incidence of homicides compared to other offenses, I run a Poisson regression model with fixed effects and standard errors clustered at the state level as a robustness test. The estimates from this model are the effect of a 1-day strike on the logarithm of the daily number of homicides, expressed as a percentage. The results of this Poisson regression model, presented in Table 12, indicate a 42% increase in daily homicides during Military Police strikes, which is consistent with my baseline results.

[6] I also tested the specification proposed by Sun and Abraham, 2021. The results are qualitatively similar and are displayed in Figure B.13

Table 12 – TWFE x Poisson Estimates

	TWFE	**Poisson**
Military Police Strikes (β_1)	2.934***	0.420***
	(2.73)	(6.14)
Civil Police Strikes (β_2)	0.364*	0.050*
	(1.84)	(2.43)

Notes: The coefficients β_1 and β_2 show the change in the number of violent deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level. t-statistics in parenthesis. *p<0.1,**p<0.05,***p<0.01

2.6 The Effect of Police Strikes on Gang Related Deaths

2.6.1 Police Strikes and Gang-related Homicides in Ceará

I argue - and show formally in Section 2.3 - that an abrupt reduction in the probability of police intervention increases the expected payoff of gang conflicts, thus inciting violent disputes over contested territories. In the previous section, I presented evidence from Ceará, where two gangs compete in drug trafficking, that the rise in homicide rates during police strikes is substantially larger compared to other states in Brazil. Nonetheless, it remains necessary to show that this spike in fatalities occurred specifically in gang territories and not in areas unrelated to drug trafficking.

In this section, I examine the effect of military police strikes on violent deaths across gang territories in Ceará. To do this, I employ a difference-in-differences specification outlined in Equation 2.5 to assess the change in homicide rates in areas with documented criminal activity, as described in Section 2.4.2.1. Furthermore, to shed light on the nature of these deaths, I decompose the total increase in homicides based on victims' prior criminal records and quantify the proportion of suspected criminals killed. According to my theoretical model, I expect a larger increase in deaths in gang turfs compared to other districts following a military police strike. Furthermore, if the analysis reveals a prevalence of victims with prior criminal records, it would provide robust evidence of gang-related homicides.

Table 13 reports the results from the estimation of Equation 2.5 using total homicides and deaths by gender as the dependent variable. I highlight the overall effect of the military police strike in Ceará (β_1) and the differences-in-differences point estimates to criminal gang turfs (β_2). My findings demonstrate that in contexts of gang competition, the escalation of violent deaths during a military police strike is primarily concentrated in contested territories. This implies that the decrease in police patrols triggered violent confrontations in gang territories, and disputes between organized criminal groups are a critical factor in explaining the rise in violence in the metropolitan area of Fortaleza. The β_2 coefficient corresponds to a 69% increase in daily homicides compared to the average.

Consistent with my identification strategy, the results in Table 14 suggest that the deaths of suspected criminals drive the increase in homicides, as demonstrated by using the three specifications presented in Table 4. These results indicate that 66% of violent deaths during the Military Police strike are attributed to suspected criminals. Moreover, I show in Figure B.12 that in police strikes there is a shift in the ratio of suspected criminal deaths in total homicides by districts, supportive evidence that strikes have a large effect on the dynamics of violent deaths involving criminals in Ceará.

These findings have important implications for understanding the mechanisms driving the increase in homicides during police strikes in contexts of drug trafficking competition. They support my conceptual model of gang conflicts, which highlights the role of criminal disputes in driving violent conflict, and demonstrate the importance of accounting for this socioeconomic context when evaluating the impact of police strikes on violent crime.

Table 13 – The effect of Police strikes on gang-related homicides - Total and Gender

	Dependent Variable	β_1	**95% Confidence Interval**	
			Lower	Upper
PM_{Strike}	Total Homicides	0.159	-0.161	0.479
	Homicides (Men)	0.216	-0.092	0.524
	Homicides (Women)	-0.056	-0.117	0.005

	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
$PM_{Strike} * GangTurfs$	Total Homicides	1.023	0.205	1.841
	Homicides (Men)	0.887	0.239	1.535
	Homicides (Women)	0.136	-0.128	0.400

Notes: The coefficient β_1 shows the change in the number of violent deaths during the Military Police strike in Ceará. β_2 reports the effect on violent deaths in gang territories. All errors are clustered at the district level.

Table 14 – The effect of Police strikes on gang-related homicides - Suspected Criminals Deaths

Variables	**Total Homicides**	**Suspected Criminals (1)**	**Suspected Criminals (2)**	**Suspected Criminals (3)**
$PM_{Strike} * GangTurfs$	1.023***	0.678***	0.608***	0.502*
	(0.378)	(0.252)	(0.231)	(0.261)

Notes: $PM_{Strike} * GangTurfs$ reports the effect of police strikes on violent deaths in gang territories. The decomposition of suspected criminals from total homicides follows the three specifications presented in Table 4. All errors are clustered at the district level. *p<0.1,**p<0.05,***p<0.01

2.6.2 Robustness

Gang Conflicts. My findings show that gang competition drives the escalation of violence during police strikes. This result is supported by my conceptual model of gang conflicts, which demonstrates how criminal organizations exploit a reduction in the probability of police intervention to attack their rivals. However, this raises the question regarding a possible increase in violent deaths during police strikes when there is no competition for drug trafficking in a state. My analysis suggests that in such cases, rapid and temporary shifts in the probability of police intervention are unlikely to trigger conflicts since a single gang holds the monopoly of drug trafficking.

To test this hypothesis, I performed a robustness test using a police strike that occurred in 2011 in the state of Ceará, Brazil, when the Comando Vermelho (CV) held a dominant position in drug trafficking. By comparing the number of violent deaths during the 2011 strike with the 2020 strike, when the GDE had emerged as a significant challenger to the CV's control of strategic routes and territories in the state, I examine how different scenarios of gang competition affect homicides. Although detailed victim identification data were not available before 2014, my analysis of data from SIM-DataSUS controlling for location and time-fixed effects demonstrate in Figure B.6 that the increase in violent deaths during the 2020 strike was larger than during the 2011 strike, particularly in the metropolitan area of Ceará[7]. These results suggest that the police strike triggers more violence in contexts of gang competition than in scenarios where a single gang controls drug trafficking.

Parallel Trends. My results provide evidence of a statistically significant increase in deaths during the strike, however, it is important to acknowledge that these findings may be confounded by pre-existing factors. Specifically, a violation of the parallel trend assumption could lead to spurious correlations, potentially undermining the validity of the estimates. To address this concern, I run an Event Study using Equation 2.6. The results presented in Panel (a) of Figure B.5 provide strong support for the argument that gang conflicts drive the increase in deaths, as there was no significant rise in homicides in gang turfs prior to the police strike in 2020.

Furthermore, I observe a larger increase in deaths of suspected criminals (Figure B.5 - Panel (b)) compared to homicides of individuals without criminal records in the state judiciary (Figure B.5 - Panel (c)). This finding offers supportive evidence of a higher incidence of deaths likely related to criminal gangs, particularly considering that my measure of crime involvement represents a conservative estimate, as it is possible that some of the individuals killed during police strikes may be criminals without criminal records in the state judiciary.

Gang Districts. Another potential concern for the identification strategy is the specification of "treated" and "control" districts within the differences-in-differences framework. It is possible that criminal gang activity exists in districts not included in the treated group presented in Figure B.1, and my findings reflect increased violence due to factors unrelated to criminal gang turfs or drug trafficking competition in Fortaleza. To address this concern, I conduct a falsification test by randomizing the "treated group" while keeping the number of selected districts constant. The results of this placebo test are reported in Table 15, revealing no significant effect on homicides. Figure B.4 provides additional insights into the dynamics of homicides in this placebo test.

To further enhance the credibility of this falsification test, I examine the distribution of the coefficient β_2 from Equation 2.5 across all possible combinations of districts as the treatment group. Figure B.11 demonstrates that the majority of combinations yield zero or negative effects

[7] The metropolitan area considers the following cities: Fortaleza, Caucaia, Maracanaú, Aquiraz, Cascavel, Eusébio, Pindoterama, Guaiúba, Pacatuba, Horizonte, Itaitinga e Pacajus

for the military police strike, with only a few showing values closer to the estimates of $\beta_2 = 1.023$ from my baseline specification. This analysis ensures robustness to the identification strategy by reinforcing that the observed impact of the military police strike on homicides is not merely driven by chance or the specific selection of treated districts.

Table 15 – The effect of Police strikes on gang-related homicides - Falsification Test (Gang Turfs)

	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
	Total Homicides	-0.064	-0.454	0.326
$PM_{Strike} * Random_{AIS}$	Homicides (Men)	0.050	-0.337	0.436
	Homicides (Women)	-0.113	-0.272	0.046

	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
	Homicides (<15)	0.042	-0.069	0.153
$PM_{Strike} * Random_{AIS}$	Homicides (15-45)	-0.240	-0.636	0.156
	Homicides (>45)	0.134	-0.082	0.350

Notes: $PM_{Strike} * Random_{AIS}$ reports the effect of police strikes on violent deaths in pseudo-gang territories. All errors are clustered at the district level.

2.6.3 Discussion

Overall, this paper contributes to our understanding of the complex interplay between police strikes, gang competition, and violent crime and has significant implications for policymakers and law enforcement agencies seeking to address these issues. The findings highlight the need for effective measures to combat gang violence and improve public safety.

My results demonstrate that criminal gang-disputed territories are disproportionately affected by the increase in homicides during police strikes. These findings are consistent with my proposed model of gang conflicts, as the sudden decrease in police presence raised the expected gains from violent conflicts, resulting in confrontations between organized criminal groups and leading to an increase in violent deaths in gang-controlled areas.

However, the impact of police strikes on crime extends beyond just homicide rates. With the significant presence of criminals in these areas, the sudden shift in police patrolling could impact other forms of criminal activity as well. The rise in property crime during police strikes is another possibility, as criminal gangs may exploit robberies and thefts to fund their operations and pay soldiers to attack rival groups. Additionally, the incentive for criminals to commit robberies could increase due to the lower probability of arrest in the standard Becker model.

Table 16 presents the estimates of the effect of police strikes on other crimes in criminal gang-controlled areas using Equation 2.5. The results indicate a significant increase in robbery (about 35% compared to the baseline) in criminal gang turfs following the military police strike. While the individuals who committed these robberies cannot be identified, these findings further support other effects of reducing police presence on crime.

Therefore, this paper reveals the stark impact of police strikes on crime in areas affected by organized criminal gangs. The results suggest that not only does the sudden reduction in police presence lead to an increase in homicides, but it also impacts other criminal activities such as robbery. These findings highlight the importance of maintaining police presence in areas affected by criminal gangs to ensure public safety and prevent the rise in criminal activities.

Table 16 – The effect of Police strikes on other crimes

	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
$PM_{Strike} * GangTurfs$	Robbery	174.8	67.4	282.2
	Theft	64.1	-3.02	131.2
	Drugs	3.33	-8.04	14.69
	Guns	2.68	-5.09	10.44
	Rape	2.73	-2.96	8.43

Notes: $PM_{Strike} * GangTurfs$ reports the effect of police strikes on other crimes in gang territories. All errors are clustered at the district level.

2.7 Conclusions

This paper offers important insights regarding the impact of police strikes on violent deaths and highlights the unique interpretation of this natural experiment. Using a novel dataset, I show that police strikes lead to a significant increase in violent deaths, particularly in territories disputed by organized criminal groups. These results suggest that criminal gangs exploit the absence of police presence to attack rivals, highlighting the importance of considering the interplay between criminal gangs and state authorities in evaluating the effect of shifts in policing on crime.

My study sheds light on the role of organized crime groups in understanding the impact of police on crime and provides important policy implications for violence reduction. Specifically, the inability to commit to a *status quo* division of gang turfs during police strikes leads to conflicts, as gangs choose to fight rather than allow rivals to attack first. My findings underscore the importance of considering the specific context of gang competition and territorial disputes when evaluating the effect of policing on crime and suggest that targeted interventions may be necessary to reduce violence in these settings.

Appendix B: Figures, Tables, and Extensions of the Theoretical Model

2.8 Figures and Tables

Table B.1 – The effect of Police strikes on violent deaths - Place of Death

	Dependent Variable	β_1	**95% Confidence Interval**	
			Lower	Upper
Military Police Strikes	Homicides (Public Spaces)	1.596	0.340	2.852
	Homicides (Home)	0.049	-0.142	0.240
	Homicides (Hospitals)	0.990	0.374	1.606
	Homicides (NA)	0.299	-0.007	0.605
	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
Civil Police Strikes	Homicides (Public Spaces)	0.053	-0.124	0.231
	Homicides (Home)	0.031	-0.010	0.072
	Homicides (Hospitals)	0.129	-0.048	0.306
	Homicides (NA)	0.147	0.006	0.288

Notes: The coefficients β_1 and β_2 show the change in the number of violent deaths during Military and Civil Police strikes respectively. All errors are clustered at the state level.

Table B.2 – The effect of Police strikes on gang-related homicides - Total and Age

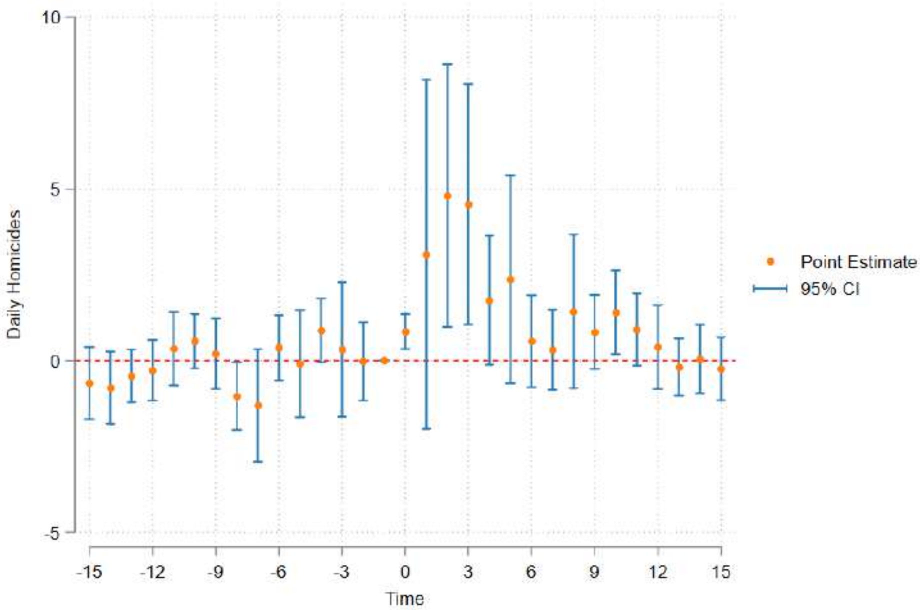
	Dependent Variable	β_2	**95% Confidence Interval**	
			Lower	Upper
$PM_{Strike} * GangTurfs$	Total Homicides	1.023	0.205	1.841
	Homicides (<15)	0.027	-0.113	0.168
	Homicides (15-45)	0.807	0.171	1.443
	Homicides (>45)	0.189	-0.067	0.445

Notes: $PM_{Strike} * GangTurfs$ reports the effect on violent deaths in gang territories. All errors are clustered at the district level.

Figure B.1 – Districts of Fortaleza/CE

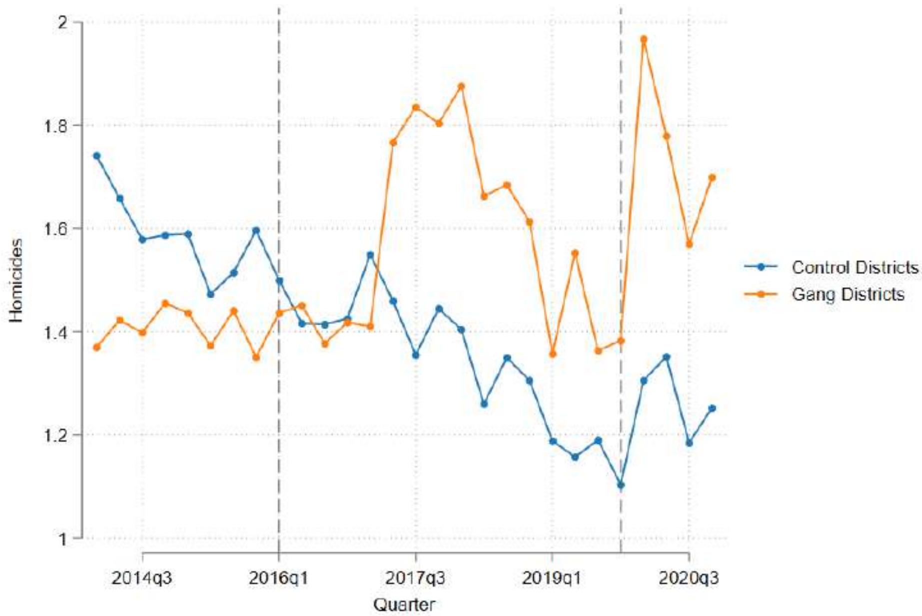
Notes: The map highlight Integrated Areas of Security ("AIS") in the metropolitan region of Fortaleza according to the State Secretary of Ceará.

Figure B.2 – Event Study - Military Police Strikes



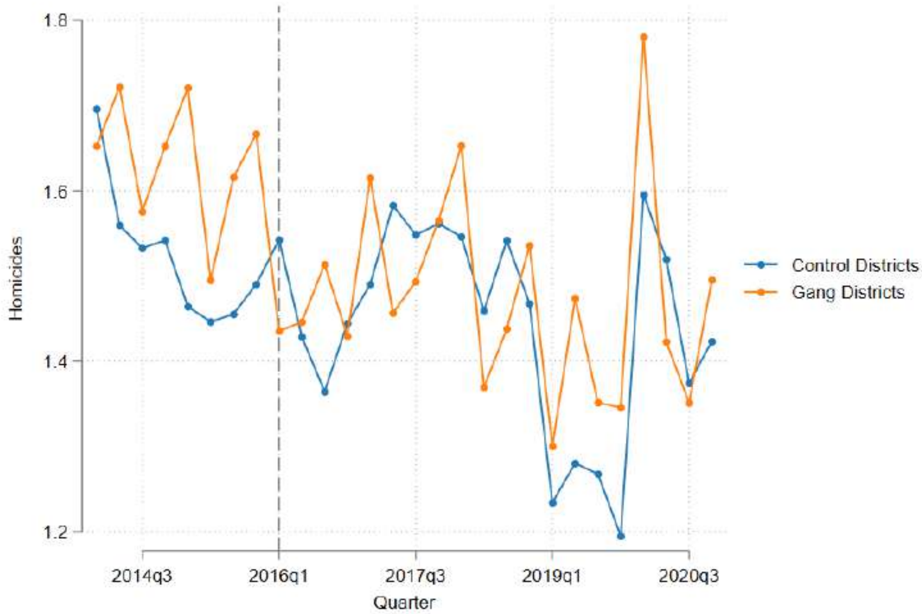
Notes: There is no clear evidence of a previous trend in homicides before the beginning of a Military Police Strike. The increase in violent deaths is significant and it lasts for couple of days.

Figure B.3 – Treated and Control Group - Pre and Post Trend



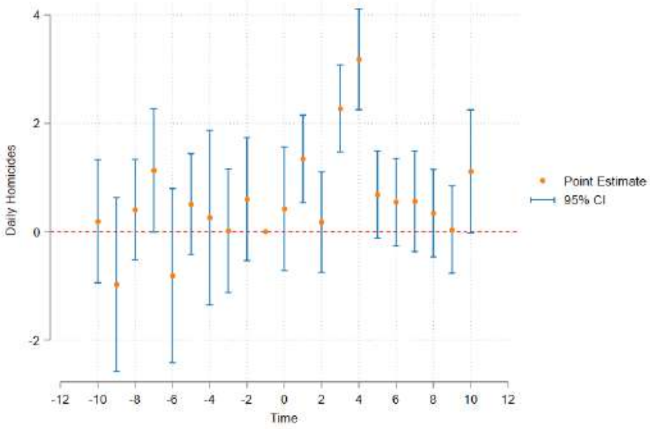
Notes: The first vertical red line indicates the entry of GDE in the first quarter of 2016 and the second one shows the quarter before the Military Police strike.

Figure B.4 – Falsification Test - Pre and Post Trend

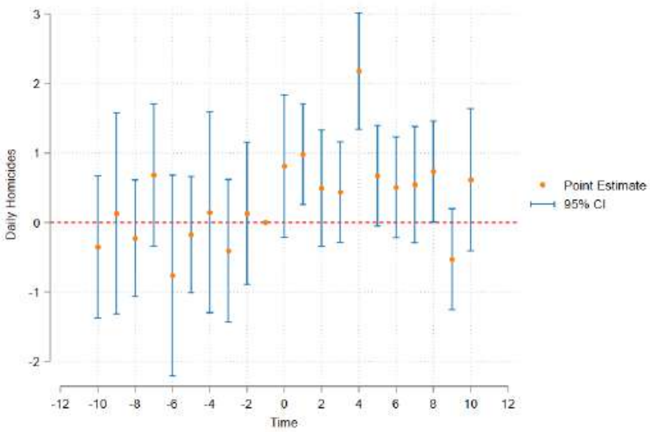


Notes: A random assignment of target and non-target districts do not show significant differences in homicides following the military police strike.

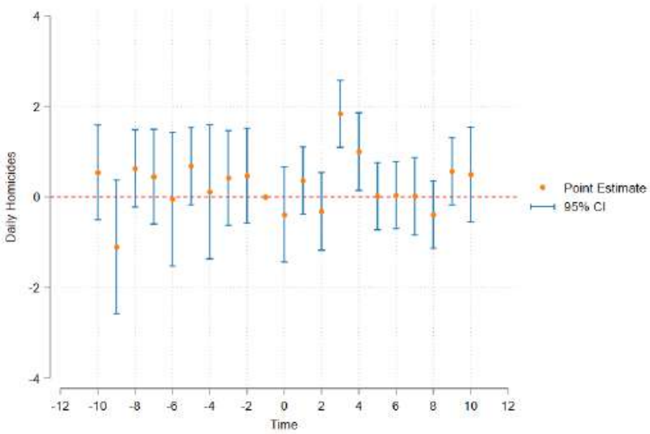
Figure B.5 – Event Study - Police Strikes in Criminal Gang Turfs



(a) Total Homicides



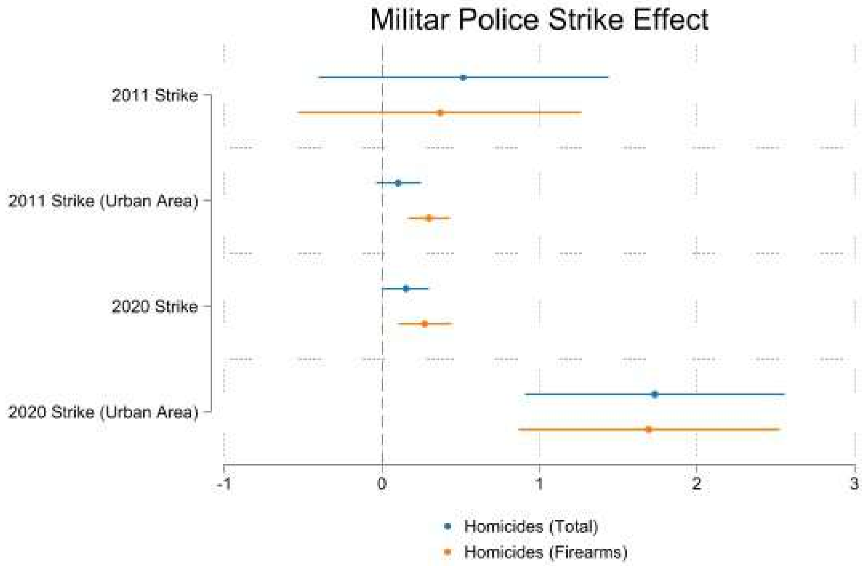
(b) Any Criminal Records



(c) No Criminal Records

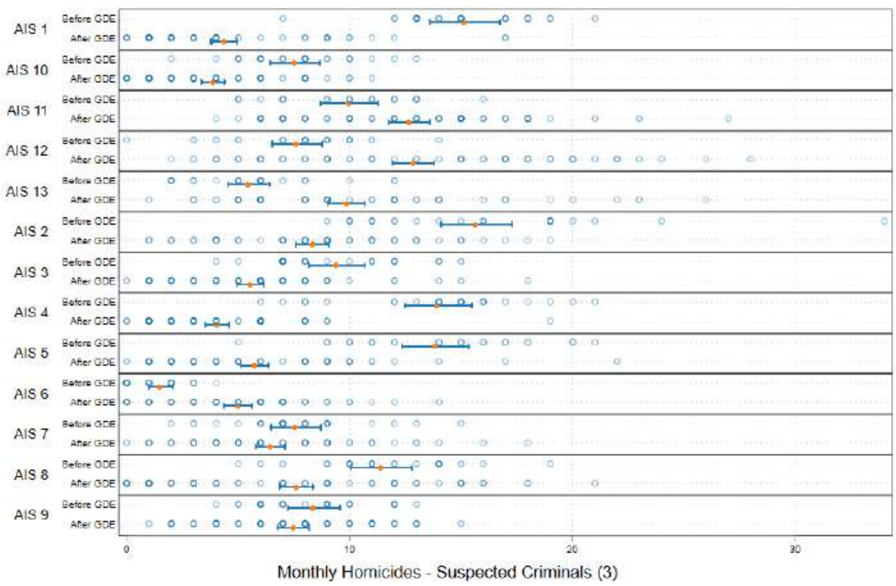
Notes: There is no clear evidence of a previous trend in homicides before the beginning of a Military Police Strike in contested turfs of Ceará. The increase in violent deaths is significant and lasts for a few days.

Figure B.6 – Military Police Strike 2011 x 2020



Notes: The increase in violent deaths in the 2020 Police Strike (when CV faced the competition of GDE by the control of criminal gang turfs in Ceará) is larger than what happened in the 2011 Police Strike (when CV had a hegemonic position).

Figure B.7 – Treated and Control Group specification

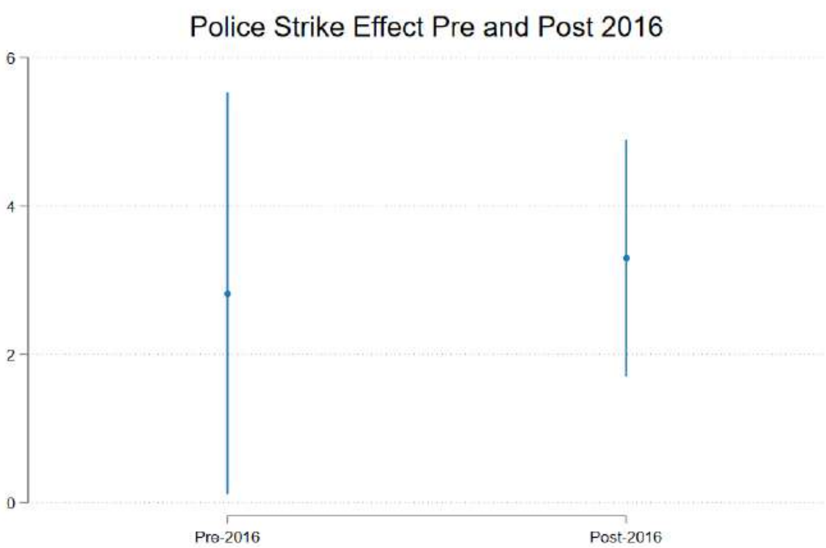


Notes: Using a t-test at a 95% confidence level we see that Districts 6, 11, 12 and 13 present a significant increase in monthly homicides of suspected criminals.

Figure B.8 – Police Strikes - Average Increase in Homicides by Number of DTOs

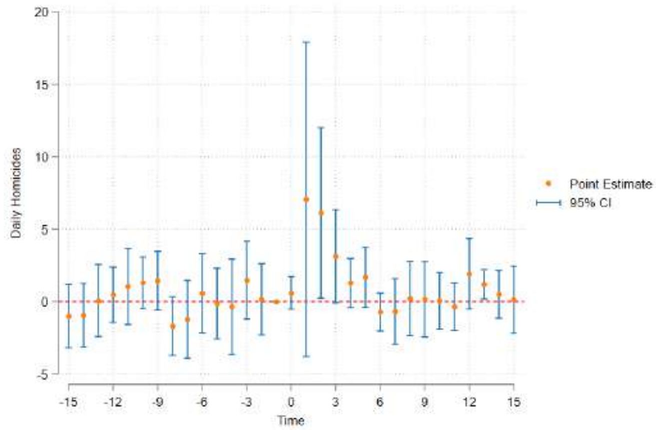
Notes: This figure shows that there is a positive correlation between the number of DTOs and the increase in homicides following police strikes. The case of Ceará is remarkable since the state shows the largest proportional increase in fatalities.

Figure B.9 – Police Forces Strikes - Pre and Post 2016

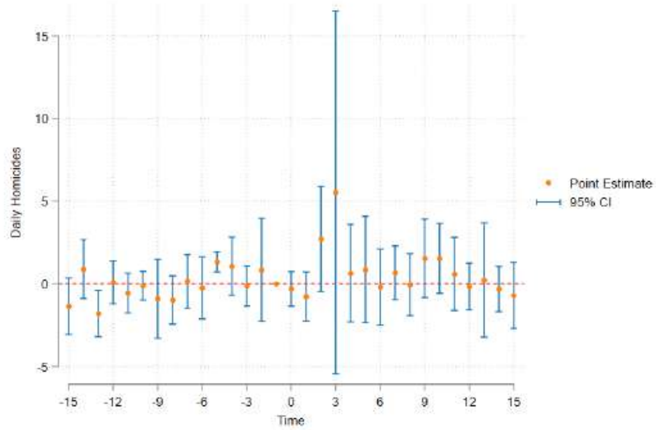


Notes: This figure shows that the effect of strikes that occurred after 2016 on daily homicides is 17% larger than the average effect of strikes prior to 2016.

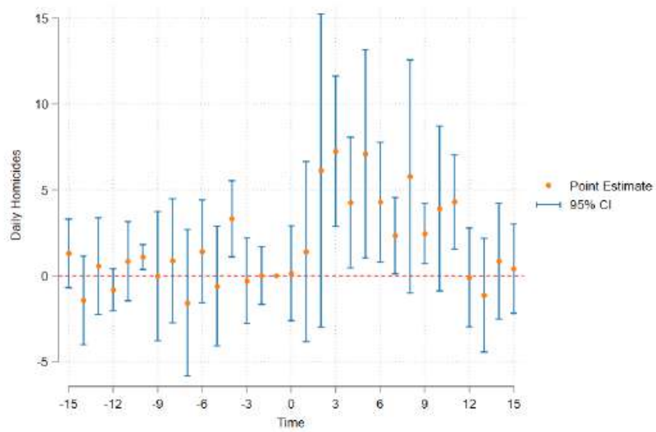
Figure B.10 – Heterogeneous Effect by strike duration



(a) Small Events



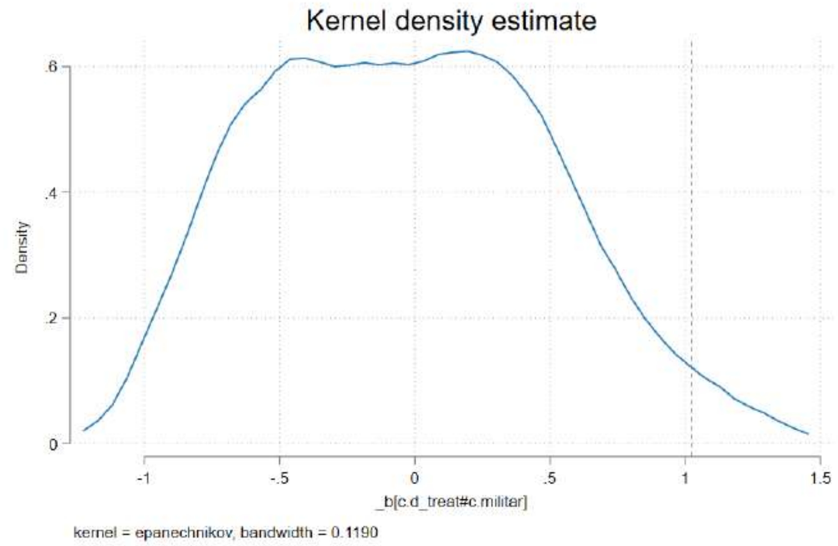
(b) Medium Events



(c) Large Events

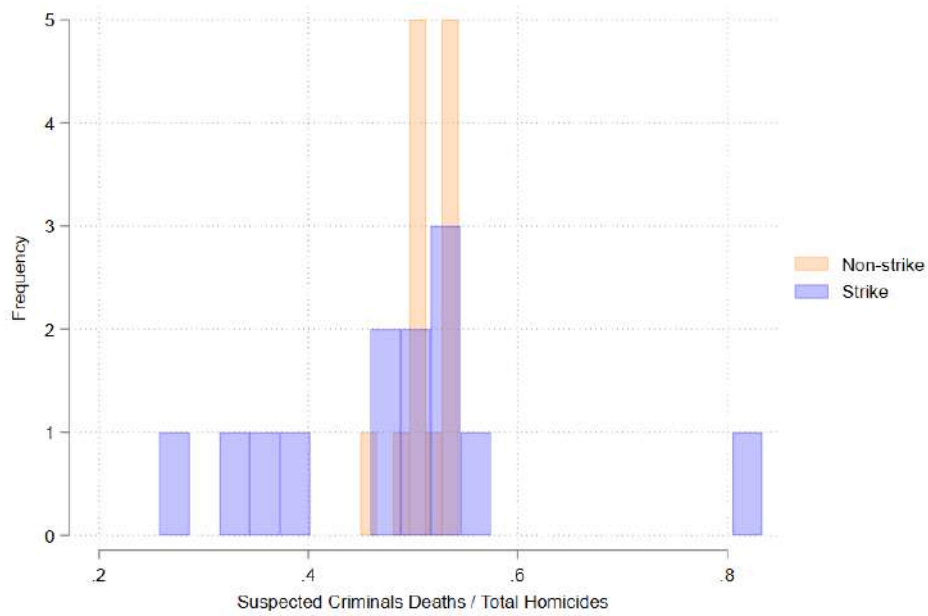
Notes: There is no clear evidence of a previous trend in homicides before a Military Police Strike outbreak. Events longer than 11 days seem to be resilient affecting homicides.

Figure B.11 – Randomization of Gang Districts (Treated Units)



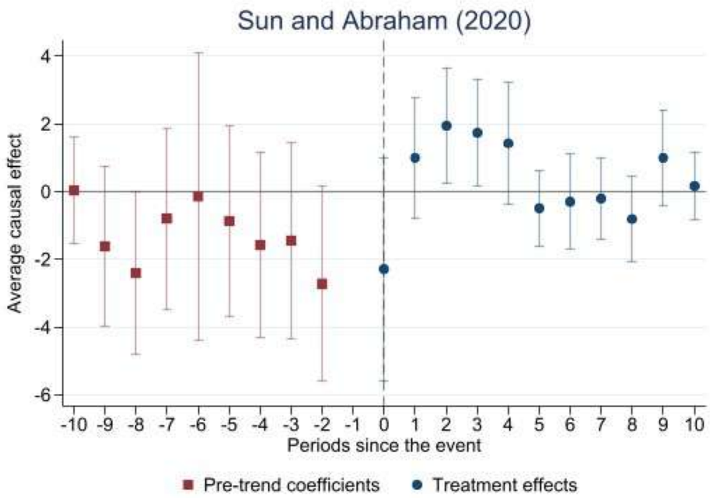
Notes: This figure shows the distribution of the coefficient β_2 of Equation 2.5 across all possible combinations of districts as the treatment group. Most combinations yield zero or negative coefficients and few show a value closer to the estimates of $\beta_2 = 1.023$ of our baseline specification.

Figure B.12 – Proportion of suspected criminal deaths in total homicides by district



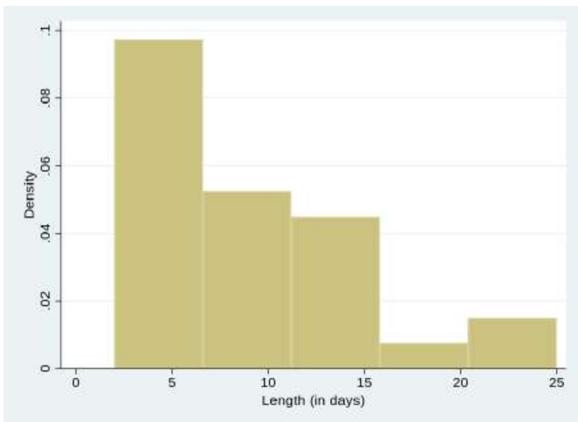
Notes: This figure shows the distribution of the ratio of suspected criminal deaths in total homicides by the districts of Ceará. The distribution in non-strike days (*orange shaded area*) is concentrated on the average while on strike days there is a spread of the distribution around lower and extreme values across districts.

Figure B.13 – Robustness - Event Study State Level



Notes: Testing the event-study specification proposed by Sun and Abraham, 2021 the results are qualitatively similar.

Figure B.14 – Military Police Stikes - Histogram (2000-2020)



Notes: Most of the strikes last less than seven days. I exploit this variation to assess the heterogeneous effects of the event by length.

2.9 Extensions of the Theoretical Model

2.9.1 Including dynamics in the model

In Section 2.3, I present a static version of a two-gang model engaged in territorial disputes over an area denoted as V. If we consider that these gangs are continuously fighting for control of drug trafficking over an extended period, it is reasonable to assume that they take into account not only the current status quo but also future dividends associated with maintaining the *status quo* division or acquiring new territories.

Let's assume that Gangs $g = A$ and $g = B$ have the same discount factor τ, meaning the present value of the flow of drug trafficking rents in dispute can be expressed as the sum of an infinite geometric progression given by $V' = \frac{V}{(1-\tau)}$. Similarly, the present value of the status quo division for each gang g is $Q_g' = \frac{Q_g}{(1-\tau)}$.

As a result, we can modify Equation 2.3 to account for the future value of drug trafficking rents in the decision to initiate a war:

$$\begin{aligned} p_g * \frac{V}{L_C} * \frac{1}{(1-\tau)} - \frac{(Q_g)}{L_C} * \frac{1}{(1-\tau)} - \frac{(L_g)}{L_C} > p_C \\ p_g * \frac{V * \alpha}{L_C} - \frac{(L_g + Q_g * \alpha)}{L_C} > p_C \end{aligned} \tag{2.7}$$

Here, $\alpha = \frac{1}{(1-\tau)} > 1$, assuming a discount factor $0 < \tau < 1$. Introducing dynamics affects the results depicted in Figure 7 in two main ways:

Intercept. After introducing the discount factor, for $p_c = 0$, we have

$$p_g = \frac{(L_g + Q_g * \alpha)}{V * \alpha} = \frac{L_g}{V * \alpha} + \frac{Q_g}{V} \tag{2.8}$$

with a discount factor $\alpha > 1$, the expression above is smaller than the static version of the model. This means that the curve shifts to the left, resulting in a wider range of combinations that could trigger a war following a reduction in police surveillance. The intuition behind this result is straightforward: considering all the future rents of expanding a drug trafficking territory, there is a larger set of combinations leading to war following a police strike.

Slope. Setting $p_g = 0$ in Equation 2.7, we obtain

$$p_c = -\frac{(L_g + Q_g * \alpha)}{L_c} = -\frac{L_g}{L_c} - \frac{Q_g * \alpha}{L_c} \tag{2.9}$$

and therefore the slope of Figure 7 becomes more negative when considering the discount factor. This change means that gangs are now less sensitive to changes in the probability of police intervention. For example, given the same decrease in p_c, a gang with a small probability of victory would not decide to start a war. The intuition is that when considering the future rents of drug trafficking, gangs require a larger push, given that they have more to lose if they do not defeat their rival.

2.9.2 A example of bargaining

This section discusses an example of bargaining within the context of the baseline model. In Scenarios 2 and 4 (as shown in Figure 9), only one gang decides on conflict, while the other prefers peace. In such cases, due to the high costs associated with conflicts, a bargaining range emerges, meaning the gang that chooses not to engage in a confrontation during a police strike would prefer to avoid a costly war.

Let's suppose that in time t, both gangs realize that the military police will initiate a strike. Gang A decides to attack the rival, while gang B prefers to keep the *status quo* and avoids a costly conflict. In a multiple-period game, a good approximation for this scenario is assuming that $D_A(t+1) > D_A(t)$ and $D_B(t+1) = D_B(t)$, i.e., gang A's expected payoff in a conflict increases in the next period, whereas gang B's expected payoff remains unchanged.

In this setting, the minimum territorial division that gang A would accept to avoid war during a police strike would be the difference between the total amount of drug trafficking rents and its increased expected payoff:

$$\frac{V}{(1-\tau)} - D_A(t+1) \tag{2.10}$$

If the minimum territorial division proposed by gang A is less than gang B's expected payoff from confrontation, there is no bargaining range, and gang B would also choose war. Thus, this condition can be expressed as:

$$D_B(t) > \frac{V}{(1-\tau)} - D_A(t+1) \tag{2.11}$$

By rearranging terms and subtracting $D_A(t)$ from both sides of Equation 2.11, we obtain the following condition:

$$D_A(t+1) - D_A(t) > \frac{V}{(1-\tau)} - [D_A(t) + D_B(t)] \tag{2.12}$$

The left side of Equation 2.12 represents the shift in gang A's expected payoff caused by the police strike, while the right side is the bargaining range. In other words, it is the difference between the total amount of drug trafficking rents and what each gang could ensure in the *status quo*. Therefore, when there is a bargaining range, it is possible that gangs decide to accommodate instead of fighting, even in a context where at least one of them could potentially gain by attacking the rival during a police strike.

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3 Police reorganization and crime: evidence from the military police in Ceará, Brazil

3.1 Introduction

While there is a growing trend of militarization in policing worldwide (Flores-Macıas and Zarkin, 2023), the literature has primarily focused on the impact of supplying military equipment to local law enforcement (Bove and Gavrilova, 2017; Harris et al., 2017; Gunderson et al., 2021; Lowande, 2021) or instances of militarization where armed forces assume domestic policing roles (Blair and Weintraub, 2023; Flores-Macıas and Zarkin, 2023). Therefore, there is a lack of understanding regarding the effects of law enforcement strategies, training, and better salaries on police performance and crime. This study aims to address this gap by providing empirical evidence from a large-scale expansion of high-speedy police patrolling originally designed to act in Fortaleza, the capital of Ceará state and the fifth-largest Brazilian city with a population of over 2.7 million, characterized by alarmingly homicide rates and notorious conflicts involving drug trafficking organizations.

Specifically, despite some evidence on the effect of work conditions on the selection and retention of police officers (Wilson et al., 2010; Schuck and Rabe-Hemp, 2018), there is still little research on the role of a substantial law enforcement reorganization on police performance and crime deterrence. This paper focuses on the reorganization of law enforcement in Ceará, Brazil, to assess the effect of better salaries, training, and manpower on crime. The argument supporting changes in law enforcement posits that traditional policing may prove ineffective in deterring criminals from engaging in executions, robberies, and other acts of violence in contexts where drug trade organizations detain significant power. This issue is particularly relevant in Latin American countries, where police forces are often characterized by inadequate training, limited resources, and, in some instances, complicity with organized crime (Flores-Macıas and Zarkin, 2023).

To investigate the effect of police reorganization, I leverage data on the increased militarization of police in Ceará to assess the impact on crime outcomes. The analysis exploits the phased roll-out of an elite squad ("CPRaio") within military police battalions across municipalities in the state. Officers who join these elite squads undergo rigorous selection processes, often volunteering from the regular police force. These selection procedures are highly demanding and designed to assess discipline, critical thinking, emotional stability, and physical fitness (Singh, 2000). These distinctive characteristics may shape public perception and influence comparisons between police officers and members of elite squads.

In 2015, the government initiated an ambitious plan to have these police special teams in 66 out of 184 municipalities. The criteria for determining the priority cities were the population size rather than existing levels of violence. The roll-out occurred in three phases from 2015 to 2022, providing a unique opportunity to assess the impact of police reorganization on crime using two methodologies. First, I exploit the phased roll-out of CPRaio squads on Ceará municipalities within a staggered difference-in-differences framework. Second, I perform a regression discontinuity design using the population criteria defined to guide the CPRaio expansion, which allows comparing the quasi-random assignment of elite squads in municipalities of similar sizes.

I find that the reorganization of law enforcement caused a strong decrease in violent property crime (robberies), and also some evidence of a reduction in homicides and an increase in firearms seized, however, these two last findings are significant only at a ten percent level of confidence in some specifications. Compared to the baseline levels prior to the police reorganization, my findings show a 6.5% reduction in robberies, a decline of 2 violent deaths per 100 thousand inhabitants (a 4% reduction compared to the state average), and an 11.8% increase in the number of guns seized. For offenses such as theft, sexual abuse, and domestic violence, which often occur opportunistically in areas and moments with limited police presence, there were no significant improvements. Lastly, considering the costs associated with the reorganization program, cost-benefit analyses indicate that each robbery reduction costs USD 10,000, similar to the findings of Bove and Gavrilova, 2017.

My findings contribute to a broader literature on the causal effect of increased investments in police forces on crime (Bove and Gavrilova, 2017; Harris et al., 2017; Evans and Owens, 2007; Machin and Marie, 2011; Gunderson et al., 2021; Lowande, 2021). However, I differentiate from these studies by exploring a large-scale reorganization of the military police, focusing on officers that undergo rigorous training to handle high-risk situations, extending beyond mere access to heavy armaments or additional resources for hiring police officers. Furthermore, my paper also relates to studies on the militarization of police forces (Flores-Macıas and Zarkin, 2023; Blair and Weintraub, 2023; De Bruin, 2022; Magaloni and Rodriguez, 2020), albeit with a focus on the regular deployment of a highly specialized police wing rather than armed forces operating in temporary situations.

This paper is organized as follows. Section 3.2 provides insights into the distinction between elite squads and regular policing, as well as the CPRaio phased roll-out in Ceará. Section 3.3 outlines the data and identification strategy employed to assess the effect of police elite squads on crime. Section 3.4 presents the results, and Section 3.5 discusses potential mechanisms and policy implications. Finally, Section 3.6 concludes the paper.

3.2 Institutional Setting

3.2.1 CPRaio and Crime in Ceará

In 2004, the Public Security Secretary and the Military Police Command of Ceará established the first unit of a fast-response Police Battalion, now known as the *"Batalhão de Rondas de Ações Intensivas e Ostensivas"* (CPRaio). The primary objective of CPRaio is to conduct street patrols using heavy armaments and high-speed motorcycles, different from other highly militarized police wings such as BOPE, ROTA, and SWAT which primarily use cars when deployed.

Militarized patrols on motorcycles can potentially be more effective in densely populated urban environments, as they offer access to narrow corridors that may be impassable for larger vehicles (SSPDS-CE, 2023). This feature is particularly valuable in informal settlements and *"favelas"* where the road network is either incomplete or non-existent. Furthermore, motorcycles allow making quick exits, which can enhance their willingness to enter and patrol high-crime neighborhoods or blocks. This capability is crucial for police operations in impoverished neighborhoods and *"favelas"* of Ceará, where accessing certain areas by car poses significant challenges.

Initially, CPRaio operated exclusively in Fortaleza, the state capital. However, in 2015, the Governor decided to expand the Battalion to other municipalities in Ceará. The first phase of expansion encompassed the largest municipalities, including Juazeiro do Norte, Sobral, and Quixadá. The second phase began in 2016 and targeted the remaining municipalities with populations exceeding 50,000. Finally, the third phase, initiated in 2020, focused on cities with populations ranging from 30,000 to 50,000. As of today, CPRaio has 46 units and approximately 2,500 police officers, with 25% of them patrolling the capital. By the end of the CPRaio expansion, the fast-response Police Battalion will be present in 66 out of 184 municipalities in Ceará (SSPDS-CE, 2021).

The increase in fast-response patrolling in Ceará was driven by two main factors. Firstly, the Public Security Secretary reported that CPRaio was responsible for seizing 30% of all firearms in the state in 2020. Additionally, the Battalion made 7,000 arrests of suspects and recovered 2,000 stolen vehicles. Secondly, the growth of CPRaio units in the state was prompted by the outbreak of violent gang conflicts in Ceará. In 2016, the factions "Comando Vermelho" (CV) and "Guardiões do Estado" (GDE) engaged in an intense territorial dispute, leading to a significant increase in the homicide rate in Ceará. Municipalities such as Caucaia and Maracanaú, which were contested by GDE and CV, exhibited homicide rates of approximately 100 deaths per 100,000 people. As most of these conflicts occurred in "favelas" and peripheral districts, motorcycle patrolling became crucial for conducting police raids.

3.3 Data and Empirical Strategy

3.3.1 Data

To investigate the effect of the police reorganization on crime across municipalities in Ceará, I collect data from 184 Ceará municipalities provided by the Ministry of Health, Mortality Information System (SIM-DataSUS), the Security Secretary of Ceará (SSPDS-CE), and the Brazilian National Bureau of Statistics (IBGE). The SIM-DataSUS is the primary source for assessing records of violent deaths[1]. The SSPDS-CE provides detailed data on violent and property crime within Ceará, enabling us to examine the impact of the CPRaio expansion on various types of crimes, including homicides, robbery, theft, sexual abuse, and drug possession. Additionally, the IBGE reports annual measures of socioeconomic variables at the municipality level, such as population size and gross domestic product.

For the information on the phased roll-out of CPRaio squads, this paper relies on the data shared by the Security Secretary (SSPDS-CE). I obtained a detailed calendar from the Secretary, highlighting the start date of each CPRaio Battalion in Ceará. According to the SSPDS-CE, the expansion of CPRaio Battalions followed the population size of each municipality. Therefore, the phased roll-out of CPRaio Battalions was not directly driven by violent and property crime outcomes, and this caveat is crucial for the identification strategy employed in this paper.

Figure C.1 illustrates the distribution of treated cities in each roll-out phase. The CPRaio was initially implemented in the state capital in 2004, and subsequent expansion to countryside municipalities occurred between 2015 and 2017. Following the population criteria, nine CPRaio battalions were established during phase 1, followed by 34 battalions in phase 2 and 24 battalions in phase 3. Table 1 presents the evolution of crime and socioeconomic indicators for each group in 2015 and 2020. Notably, Fortaleza experienced a significant decrease in violent deaths. However, the municipalities that received a CPRaio battalion during phases 1 to 3 still exhibited alarmingly high levels of homicides and robberies, although lower than those in the state capital. It is important to note that these levels remain exceptionally high when compared to other countries, as a matter of comparison the average homicide rate in the OECD stands at 2.6 murders per 100,000 inhabitants.

Table 1 – Descriptive Statistics - Socioeconomics

	Fortaleza (n=1)		Phase 1 (n=9)		Phase 2 (n=34)		Phase 3 (n=24)		Control Group (n=116)	
	2015	2020	2015	2020	2015	2020	2015	2020	2015	2020
Homicide Rate	71.86	49.26	48.71	46.80	40.82	47.51	27.62	30.42	26.20	33.19
Robbery Rate	1,557.39	1,363.74	397.57	312.50	466.91	382.35	87.95	88.70	43.17	52.85
GDP per capita	31,787.50	28,260.15	15,021.26	13,969.87	14,297.52	13,410.76	7,795.82	7,337.87	7,034.17	6,425.15
Formal Jobs Rate	22,079.14	24,410.84	11,875.03	15,309.05	13,424.96	19,037.88	7,933.69	12,502.34	6,536.77	9,427.66

Note: This table shows some descriptive statistics to Fortaleza (state capital), municipalities treated in different phases of CPRaio expansion and the ones that still do not have a CPRaio batallion (control group). Homicides, Robberies, and Formal Jobs are presented as annual rates by one hundred thousand inhabitants. The GDP per capita is the average annual Gross Domestic Product by a person in Brazilian Reais.

[1] Violent deaths are identified using the International Statistical Classification of Diseases and Related Health Problems (ICD-10) for aggression (X85-Y09) and legal intervention (Y35-Y36)

3.3.2 Identification Strategy

A crucial challenge in empirically assessing the effect of policing on crime is the issue of reverse causality, where crime and police deployment are simultaneously determined. In the context of police reorganization, if the municipalities selected to receive the fast-response Police Battalion are the ones with the highest levels of violence, it could lead to biased estimates of the effect of police on crime.

One factor that mitigates concerns about reverse causality is the selection criterion based on population size. While larger municipalities typically exhibit higher crime rates, the phased roll-out of CPRaio enables assessing its effect on violent crime by comparing municipalities of nearly identical sizes but treated at different time periods. For instance, consider the case of Brejo Santo, which was selected for phase 2 with a population of 50,196 inhabitants, while São Gonçalo do Amarante, with 49,306 citizens, only joined the CPRaio expansion in phase 3.

To evaluate the causal effect of police reorganization on crime, I employ two identification strategies, which are discussed below.

3.3.2.1 A Differences in Differences approach

To identify the dynamic treatment effect of the police reorganization caused by the CPRaio phased roll-out, I use the method proposed by Callaway and Sant'Anna, 2021. The authors present a framework applied to differences-in-difference models with staggered adoption. In my setting, there is variation in the treatment timing across municipalities of Ceará. Once units receive the CPRaio batallion, they remain 'treated' for the following periods. In this setup, the average treatment effect is

$$ATT(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)C}\right]}\right)(Y_t - Y_{g-1})\right] \tag{3.1}$$

where G is a binary variable that indicates the implementation time of CPRaio, and C is a binary variable equal to 1 if the municipality does not have the Batallion in any period. The p_g is the generalized propensity score that indicates the probability of receiving the CPRaio at time g, conditional on pre-treatment variables, and Y is the potential outcome variable. In the context of the CPRaio expansion, the population sizes and previous homicide rates are key pre-treatment variables in this approach.

In municipalities without a CPRaio Batallion at any time, observed outcomes are 'untreated' in all periods. In this setting, these 'never treated' cities are fixed comparison groups for all remaining municipalities that, in some period, received a CPRaio squad. If pre-treatment variables do not play a significant role in the identification, the expression 3.1 collapses to

$$ATT(g,t) = \mathbb{E}[Y_t - Y_{g-1}|G_g = 1] - \mathbb{E}[Y_t - Y_{g-1}|C = 1] \tag{3.2}$$

Equation 3.2 shows that the average effect of receiving a CPRaio squad in time g is identified by taking changes in crime outcomes compared to the most recent period before having the batallion and adjusting by the changes in crime experienced by the 'never treated' group. Under the parallel trends assumption, the latter path of outcomes is the counterfactual scenario to municipalities of the 'treatment group' if they had received a CPRaio squad.

The estimation of treatment effects relies on the validity of the parallel-trend and no-anticipation assumptions. To examine these assumptions, the standard approach involves conducting event study plots, which combine Average Treatment Effect on the Treated (ATT) estimates using a set of indicators for observations in periods 1 to k before the treatment, with a horizon $h \geq 0$, and present a series of pre-trend coefficients. Selecting the optimal periods for analysis can be challenging, as F-tests utilizing large periods (k) may result in low power and significant biases (Borusyak, Jaravel, and Spiess, 2022).

As illustrated in Figure C.1, the city of Fortaleza was the first to establish a CPRaio squad, preceding other municipalities in Ceará by eleven years. Including Fortaleza in the differences-in-differences model would require testing the parallel-trend and no-anticipation assumptions over a considerable number of periods. In order to obtain more reliable treatment effect estimates, I exclude Fortaleza in the empirical analysis, focusing solely on the remaining municipalities in Ceará for the staggered differences-in-differences framework.

3.3.2.2 A Regression Discontinuity Design

The police reorganization delivers a 'treatment' assignment mechanism similar to a regression discontinuity design (Thistlethwaite and Campbell, 1960; Hahn, Todd, and Van der Klaauw, 2001) where the running variable is population. In my setting, I can identify the specific day on which a municipality received a CPRaio Batallion for each population threshold P_j (P_1 : $pop > 100.000$, P_2 : $50.000 < pop < 100.000$ and P_3 : $30.000 < pop < 50.000$). In Phase 1 there were only 9 municipalities assigned to the treatment, which increases the chances of noisy estimates given the reduced sample size and lack of statistical power (G. W. Imbens and Lemieux, 2008). Hence, in this exercise, I focus on Phase 2 given the larger amount of assigned to treatment units (32 municipalities) which will enhance the precision of my RDD estimates.

According to the SSPD-CE, the population estimates used to define the roll-out order came from IBGE, however, seven municipalities below the threshold of 50.000 inhabitants received a CPRAio squad in Phase 2 (Figure C.2). Therefore, the CPRaio Phase 2 setup shows a treatment assignment mechanism of a fuzzy regression discontinuity design. Receiving a CPRaio squad depends on the population size in a probabilistic way but not perfectly. Municipalities with more than 50 thousand inhabitants assigned to Phase 2 are the compliers for which it is possible to estimate the intention-to-treat (ITT) effects (G. W. Imbens and Angrist, 1994; Angrist, G. W. Imbens, and Rubin, 1996).

The estimation of the ITT can be performed parametrically as follows:

$$\text{CPRaio}_i = \beta_t T_i + g(P_i) + \gamma_i + \epsilon_i \tag{3.3}$$

$$Y_i = \beta_y \hat{\text{CPRaio}}_i + g(P_i) + \gamma_i + \mu_i \tag{3.4}$$

where T_i is an indicator function that takes the value of one for municipalities i assigned in Phase 2 and zero otherwise, and $g(P_i)$ represents a flexible function in the population size, γ_i are municipality fixed effects, and both error terms ϵ_i and μ_i are clustered at the municipality level. The coefficient β_t in Equation 3.3 identifies the reduced-form (first-stage) effect of being eligible to a CPRaio squad on the actual assignment to Phase 2. The coefficient β_y in Equation 3.4 identifies the reduced-form effect of being eligible to Phase 2 on crime outcomes[2] (Y_i).

3.3.2.3 Validity of the RD Design

In the RDD setup, the parameter β represents the causal effect of the CPRaio roll-out Phase 2 on crime outcomes, given that two assumptions hold: (i) the sample is balanced in pre-determined characteristics between municipalities assigned and not assigned to Phase 2, and (ii) municipalities cannot manipulate population estimates to intentionally fall above the threshold (P_2) and receive a CPRaio Battalion. Figure C.3 displays the t-statistics and standardized coefficients of β using the gross domestic product, municipality budget, and the number of formal jobs as dependent variables in Equation 3.4. The results indicate that these baseline characteristics are well-balanced between the treatment and control groups. Additionally, Figure C.4 illustrates that there is no bunching of population estimates near the Phase 2 threshold (P_2). The McCrary test provides a p-value of 0.16, which fails to reject the null hypothesis of no manipulation (McCrary, 2008).

3.4 Results

3.4.1 DiD estimates

Figures 1 and 2 depict event-study plots that illustrate the impact of police reorganization on the rate of various crimes per 100 thousand inhabitants, including homicides, robberies, theft, drug and gun seized, sexual abuse, and domestic violence. Table 2 presents aggregated estimates of the treatment effect (Callaway and Sant'Anna, 2021), shedding light on the causal impact of the phased roll-out of CPRaio. The findings demonstrate the dynamic effect on crime outcomes following the introduction of a CPRaio battalion in municipalities. Specifically, increased police patrolling resulted in a significant reduction in robberies and homicides, while no significant effects were observed for other crime categories. Examining the dynamic estimates, an immediate decrease in robberies occurred, and this effect persisted over time. For homicides, although the aggregated effect is negative, the estimates are very noisy over time and for some periods indistinguishable from zero.

[2] In the Appendix 3.6 I discuss the measurement of crime outcomes by municipalities assigned and not assigned to CPRaio Phase 2 roll-out. In summary, I show how I normalize the data to account for the fact that municipalities received a CPRaio squad at different moments.

The results suggest that the police reorganization primarily exert a deterrence effect on violent crimes, particularly those involving firearms in public spaces. However, for offenses such as theft, sexual abuse, and domestic violence, which often occur opportunistically in areas and moments with limited police presence, there were no significant improvements.

Table 2 – CPRaio Phased Roll-out - Aggregated Treatment Effect Estimates

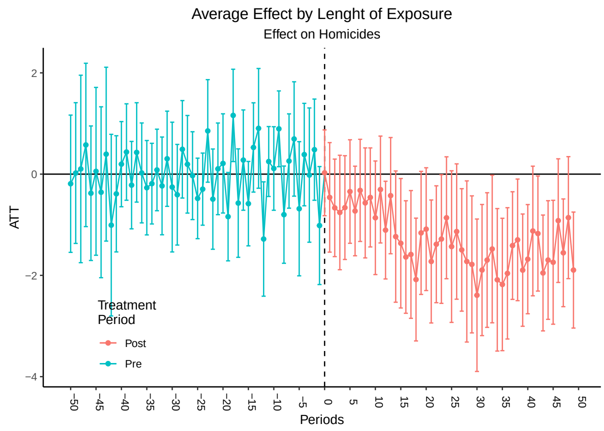
	Partially Agregated				Single Parameters
Homicides	t=12	t=24	t=36	t=48	Total
	-1.106	-0.862	-1.960	-0.859	-2.114
	(0.492)	(0.614)	(0.661)	(0.614)	(0.369)
Robberies	t=12	t=24	t=36	t=48	Total
	-13.762	-15.350	-19.669	-21.337	-20.720
	(4.100)	(3.777)	(4.389)	(5.136)	(3.982)
Theft	t=12	t=24	t=36	t=48	Total
	-1.889	0.472	-5.787	-0.261	-2.180
	(2.142)	(4.357)	(2.330)	(3.108)	(2.038)
Guns Seized	t=12	t=24	t=36	t=48	Total
	0.319	-0.612	-0.537	0.025	-1.260
	(1.056)	(1.154)	(1.279)	(1.427)	(0.776)
Drugs Seized	t=12	t=24	t=36	t=48	Total
	-2.090	-1.062	-2.353	-3.145	2.989
	(1.553)	(3.374)	(2.434)	(2.518)	(2.897)
Sexual Abuse	t=12	t=24	t=36	t=48	Total
	0.049	0.669	0.738	0.454	0.445
	(0.400)	(0.370)	(0.405)	(0.478)	(0.260)
Domestic Violence	t=12	t=24	t=36	t=48	Total
	1.823	-0.381	2.104	0.346	1.454
	(1.308)	(1.432)	(1.772)	(1.513)	(1.327)

Note: The table reports aggregated treatment effect parameters under the unconditional parallel trends assumptions. The columns "Partially Aggregated" report in each row the average treatment effects by the length of exposure to a CPRaio squad; here, t indexes the length of exposure to the treatment in months. The column "Single Parameters" represents a further aggregation of each parameter type (Overall ATT).

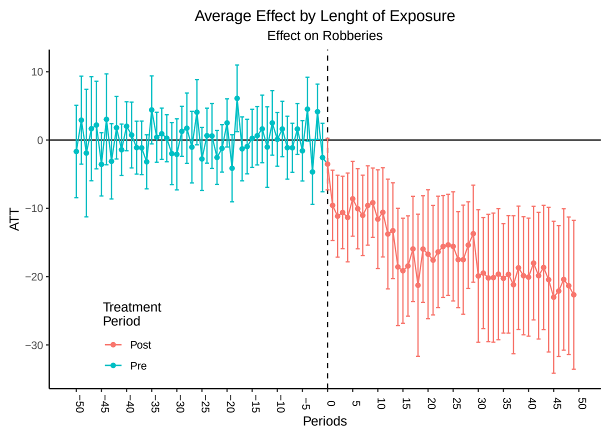
3.4.2 RDD Estimates

Panel A in Table 3 reports the RD estimates on homicides, robberies, theft, guns seized, sexual abuse, and domestic violence per 100 thousand inhabitants using a linear polynomial. Panel B reports the results for the same outcomes using a quadratic polynomial. In the first column, I show that municipalities assigned to the CPRaio roll-out Phase 2 reported a decrease of 5.2 percentage points in homicides per 100 thousand inhabitants. In the fourth column, I also show that CPRaio squads increased guns seized by 11.8 percentage points in these municipalities. Although these results are significant only at a 10% confidence level, they show that CPRaio battalions present both a deterrence and incapacitation effect, reducing homicides and also the number of firearms in possession of criminals. To other crimes, I do not find a significant effect. The results using a quadratic specification are not statistically significant, which is probably a consequence of a small sample for which high-order polynomials can put much weight in observations far from the threshold (Gelman and G. Imbens, 2019).

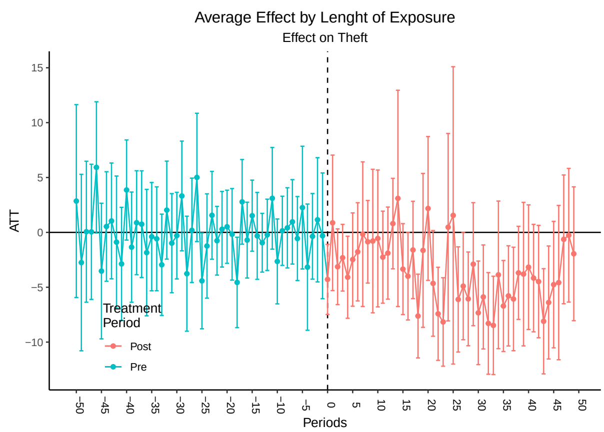
Figure 1 – The Effect of CPRaio - Homicides, Robberies and Theft.



(a) Homicides



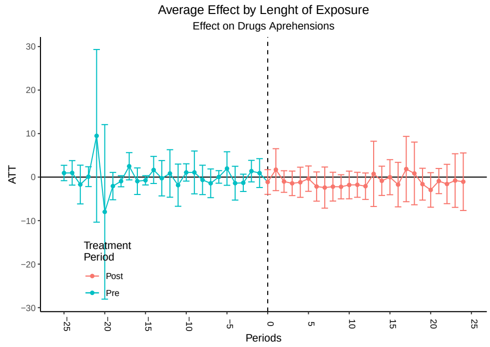
(b) Robberies



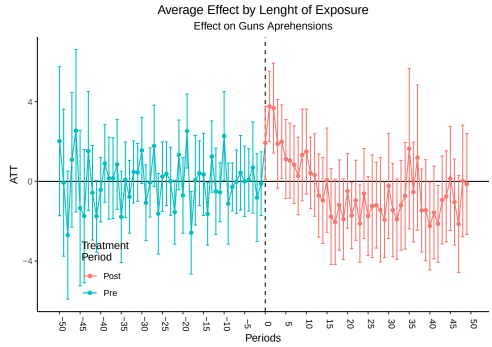
(c) Theft

Notes: The effect of the CPRaio phased roll-out is estimated under the unconditional parallel trends assumption. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the municipality level. Under the null hypothesis of the parallel trends assumption holding in all periods, these should be equal to zero. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect.

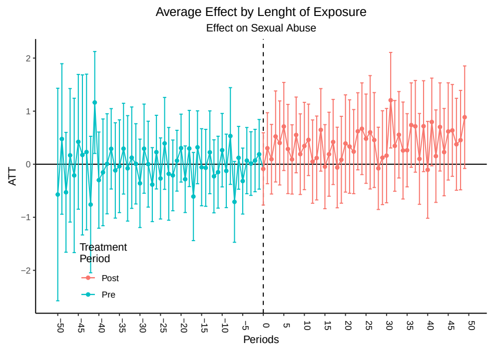
Figure 2 – The Effect of CPRaio - Other Crimes



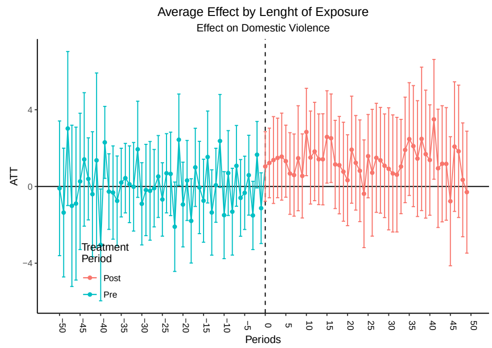
(a) Drugs Seized



(b) Guns Seized



(c) Sexual Abuse



(d) Domestic Violence

Notes: The effect of the CPRaio phased roll-out is estimated under the unconditional parallel trends assumption. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the municipality level. Under the null hypothesis of the parallel trends assumption holding in all periods, these should be equal to zero. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect.

Figure 3 shows graphically the Panel A results described above. Subfigure (a) shows the negative variation in the rate of homicides per 100 thousand inhabitants, and subfigure (d) reports the positive variation in the average number of guns seized per 100 thousand inhabitants. All estimates using optimal bandwidths following Calonico, Cattaneo, and Titiunik, 2014 to minimize the mean squared error of the local polynomial RD point estimator.

3.4.3 Interpreting DiD and RDD estimates

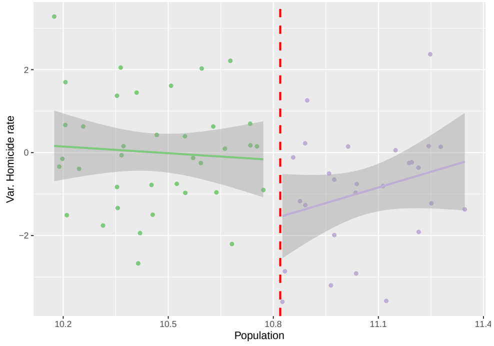
The DiD estimates (Table 2) and the RDD results (Table 3) demonstrate significant heterogeneity when comparing municipalities of varying sizes and levels of violence. The Average Treatment Effect (ATE) estimated through differences in differences captures the overall impact of CPRaio squads in Phases 1 to 3, encompassing municipalities with populations ranging from 30 to 350 thousand inhabitants. The finding of a significant reduction in robberies and homicides in the DiD estimates suggests that CPRaio patrols have a pronounced deterrence effect in larger

Table 3 – CPRaio Phased Roll-out - Fuzzy RDD Estimates

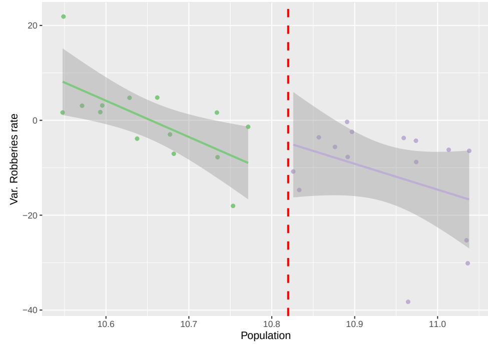
	Δ Homicides per 100k pop.	Δ Robberies per 100k pop.	Δ Theft per 100k pop.	Δ Guns Seized per 100k pop.	Δ Sexual Abuse per 100k pop.	Δ Domestic Violence per 100k pop.
Panel A: Linear Specification						
RD Estimator	-5.231	-21.553	-7.254	11.850	0.679	-1.803
Robust p-value	0.088*	0.627	0.287	0.097*	0.476	0.902
Robust conf. int.	[-14.891 , -0.283]	[-92.158 , 50.076]	[-28.566 , 6.123]	[0.172 , 35.679]	[-1.366 , 3.459]	[-623.595 , 536.370]
CCT-optimal BW	0.648	0.275	0.713	0.735	0.696	0.422
Eff. number of obs.	64	29	73	74	70	44
Panel B: Quadratic Specification						
RD Estimator	5.943	-22.268	-1.917	-43.91	0.335	6.301
Robust p-value	0.821	0.641	0.939	0.761	0.802	0.948
Robust conf. int.	[-15.554 , 20.511]	[-59.661 , 33.310]	[-38.338 , 34.907]	[-196.733 , 286.013]	[-2.987 , 4.061]	[-84.225 , 77.790]
CCT-optimal BW	0.623	0.450	0.489	0.670	0.591	0.664
Eff. number of obs.	62	48	54	68	58	66

Note: The table reports RD estimates of the effect of CPRaio phase 2 on the rate of Homicides, Robberies, Theft, Guns Seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants in Ceará Municipalities around the threshold of 50 thousand inhabitants. Panel A shows the results for a first-degree polynomial estimation. Panel B shows the results for a second-degree polynomial estimation. Optimal bandwidths following Calonico, Cattaneo, and Titiunik, 2014 were chosen to minimize the mean squared error of the local polynomial RD point estimator. Following that same work, I report robust-bias corrected p-values and 90% CIs. Coefficients significantly different from zero at 99%(***), 95%(**) and 90%(*) confidence level.

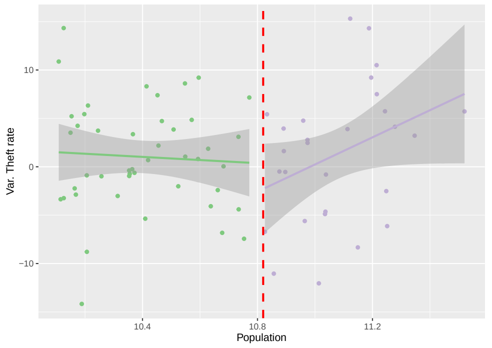
Figure 3 – The Effect of CPRaio - Fuzzy RDD



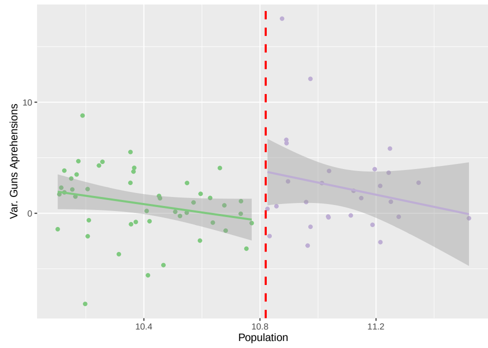
(a) Homicides



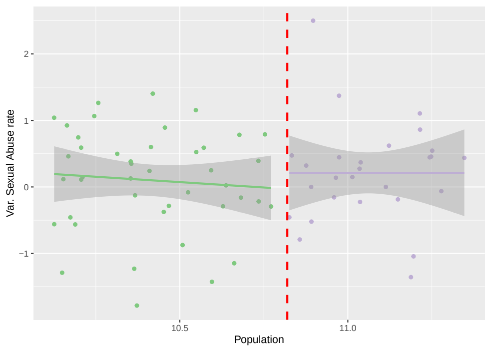
(b) Robberies



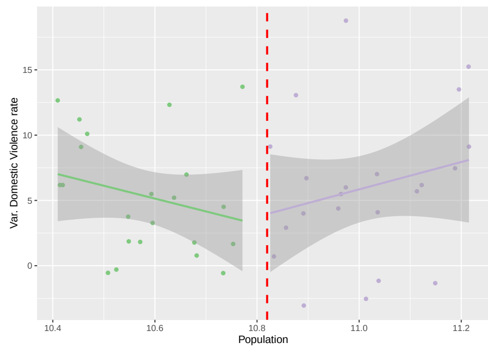
(c) Theft



(d) Guns Seized



(e) Sexual Abuse



(f) Domestic Violence

Notes: The figure shows the effect of CPRaio roll-out phase 2 on the rate of Homicides, Robberies, Theft, Guns Seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará Municipalities. The outcomes are measured in the period pre and post-CPRaio implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated accordingly to Calonico, Cattaneo, and Titiunik, 2015. All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo, and Titiunik, 2014, the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

municipalities (the first treated), thus driving the overall ATE in the entire sample. On the other hand, the intention-to-treat (ITT) estimates obtained from the fuzzy RDD analysis reveal a local average treatment effect (LATE) when comparing similar municipalities. Specifically, the LATE in my RDD estimates indicates that the presence of a CPRaio squad in municipalities with 50 thousand inhabitants has an incapacitation effect, as evidenced by the increase in guns seized. This effect is arguably related to the reduction in homicides found in this exercise, as a higher rate of firearm seizures implies a greater likelihood of incarcerating individuals who may have potentially used these guns in committing murder.

3.4.4 Robustness

DiD with covariates. The differences-in-differences (DID) results presented in Table 2 were estimated using Equation 3.2 under the unconditional parallel trends assumption. To enhance the credibility of my results, I conduct additional tests by examining the effect of CPRaio squads conditional on time-varying covariates. This approach provides an extra degree of robustness and helps mitigate potential bias from unobserved confounders (Roth et al., 2023). Specifically, I estimate Equation 3.1 including linear trends and gross domestic product at the municipality level as covariates[3]. Figure C.5 show these estimates compared to the results obtained from the baseline specification. With the exception of homicides, the conditional and unconditional estimates yield similar results. Notably, the shift from a reduction to a zero effect of CPRaio squads on homicides after controlling for covariates reinforces the event-study findings presented in Figure 1. This figure shows noisy estimates for homicides over an extended period after the treatment, only achieving significance in the overall aggregation. After accounting for covariates, this overall effect also disappears.

Sharp RDD. As mentioned in Section 3.3, due to the presence of imperfect compliance, my regression discontinuity approach takes the form of a Fuzzy RDD. Consequently, I estimate the intent-to-treat (ITT), the effect of eligibility for CPRaio Phase 2. To assess the average treatment effect (ATE) of elite squads on crime, I exclude non-compliant municipalities from the sample to simulate a Sharp RDD. The results presented in Table C.1 indicate that most of the estimated signs remain the same, although the confidence intervals are wider due to the reduced number of observations. Notably, the Sharp RDD estimates demonstrate a statistically significant reduction in homicides, consistent with the findings obtained using the Fuzzy RDD approach. Figure C.6 shows graphically these results, which are very similar to those obtained in the Fuzzy RDD analysis.

[3] Gross domestic product data is released annually at the municipality level, therefore I create a monthly linear trend to each municipality according to the annual baseline levels.

3.5 Potential Mechanisms and Policy Implications

Military Weapons. Previous studies by Bove and Gavrilova, 2017 and Harris et al., 2017 have demonstrated that the deployment of military equipment has a significant impact on reducing street-level crime. They argue that the visibility of specialized tools, gear, vehicles, and clothing creates a perceptual deterrence effect. In the case of CPRaio squads, their use of motorcycles, distinctive uniforms, and heavy weaponry sets them apart from regular police units (SSPDS-CE, 2023). Therefore, the decrease in robberies observed during the phased roll-out of CPRaio squads to all municipalities in Ceará can plausibly be attributed to the perception of a well-equipped special force engaged in local law enforcement. It is surprising, however, that this perceptual deterrence mechanism does not seem to affect other crimes such as theft or sexual abuse, suggesting that not all offenses are equally affected by this perception. Furthermore, the Local Average Treatment Effect (LATE) estimates obtained through the Regression Discontinuity Design (RDD) show a reduction in homicides and an increase in guns seized, indicating the presence of heterogeneous effects of CPRaio squads across municipalities. Hence, the role of police elite squads on local law enforcement seems to have a different impact on cities around the 50 thousand inhabitants threshold compared to the overall effect observed across all municipalities that received a CPRaio battalion.

Selection and Training. The literature, as shown by Singh, 2000, highlights the rigorous scrutiny and training that members of elite squads undergo to ensure their proficiency in firearms, extreme discipline, physical fitness, and emotional stability. In the case of joining a CPRaio squad, regular police officers must apply for a specialized course in motorcycle patrolling that lasts for 278 hours (SSPDS-CE, 2023). The course includes physical training, theoretical classes, and practical instruction in motorcycle riding, police shooting, personal defense, and other relevant disciplines. This emphasis on training likely enhances the effectiveness of the augmented manpower, as CPRaio police officers are carefully selected and prepared to operate as a highly specialized unit within the state police force. Last, since elite squads are usually deployed to high-risk occurrences, CPRaio troopers receive higher salaries and have reduced raids. The financial incentives of joining CPRaio arguably stimulate the enrollment of the best police officers in the training program.

Policy Implications. The phased roll-out of CPRaio squads carries significant policy implications, particularly for the reduction of violent property crimes. The decrease in robberies reported in Table 2 is equivalent to a 6.53% reduction compared to the previous average rate in municipalities that received CPRaio squads. This effect is 3.4 times larger than the estimates shown by Bove and Gavrilova, 2017 and comparable to the findings of Evans and Owens, 2007, who documented a 5% decrease in robberies. Furthermore, the RDD analysis reveals a reduction of 5.2 percentage points in homicides, which translates to an average decline of 2 deaths per 100,000 inhabitants, although statistically significant only at the 90% confidence level.

Benefit-Cost Analysis. According to the 2022 state government financial statements, a total expenditure of BRL 2.8 million (USD 560 thousand) was reported for the expansion of CPRaio squads. With the implementation of 16 elite squads that year, the average cost per CPRaio unit amounts to BRL 175 thousand (USD 35 thousand). Based on my estimation of a 6.53% decrease in violent property crimes, this translates to a reduction of 52 robberies across the 16 municipalities that received a CPRaio battalion. Therefore, the cost per robbery reduced is approximately USD 10 thousand, similar to the findings of Bove and Gavrilova, 2017 who reported a cost of USD 9.6 thousand per robbery reduced.

3.6 Conclusion

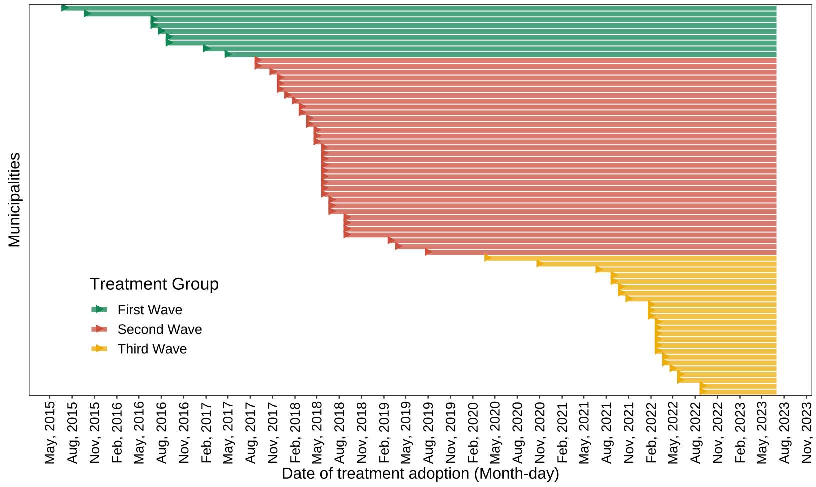
This paper aimed to investigate the effect of law enforcement reorganization on crime, focusing on the case of CPRaio phased roll-out in Ceará. By leveraging a large-scale expansion of CPRaio patrolling, this study sheds light on the implications of improving police salaries, training, and manpower.

The results demonstrate that the reorganization of the military police, characterized by specialized training, access to military-grade equipment and high-speed motorcycles, was associated with a significant reduction in violent property crimes. This effect surpasses previous estimates in the literature regarding the effect of police on crime. Additionally, the findings reveal a decrease in homicides in municipalities around the 50 thousand inhabitants threshold of the CPRaio roll-out Phase 2, although statistically significant only at the 90% confidence level.

The law enforcement reorganization in Ceará provides valuable insights for policymakers and researchers seeking evidence-based strategies to enhance public safety. The significant reduction in violent crimes underscores the relevance of specialized training, proper selection processes, and manpower. These findings contribute to the ongoing discussions on the role of funding the police to promote public safety and serve as a guide for law enforcement policies, particularly in contexts of high violence and the prevalence of criminal gangs.

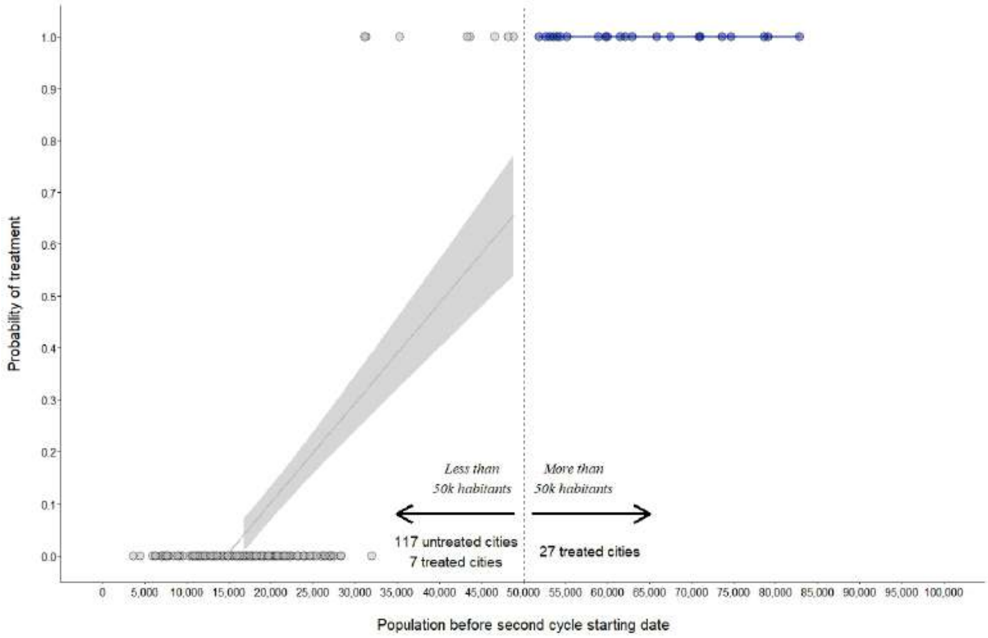
Appendix C: Tables and Figures

Figure C.1 – CPRaio phased roll-out



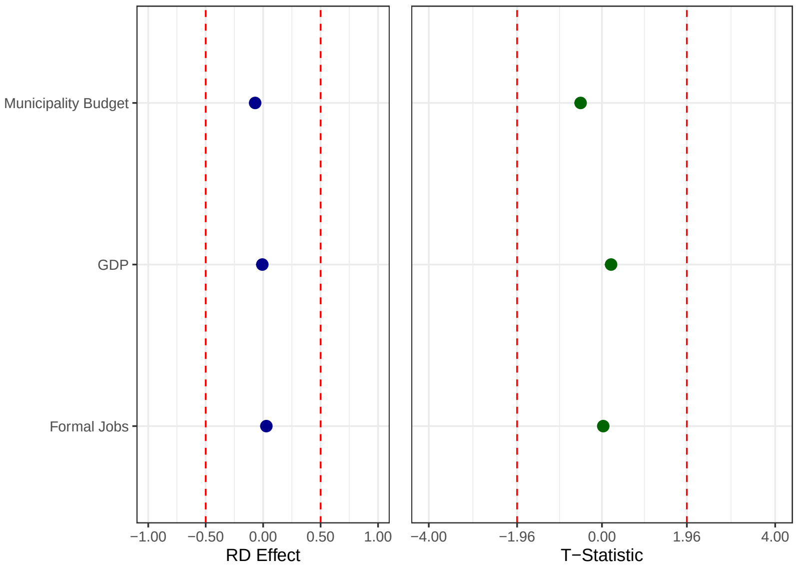
Notes: The CPRaio was initially implemented in the state capital in 2004, and the first expansion to countryside municipalities took place between 2015 and 2017. Nine battalions were established during Phase 1, thirty-four in Phase 2, and twenty-four in Phase 3.

Figure C.2 – CPRaio Phase 2



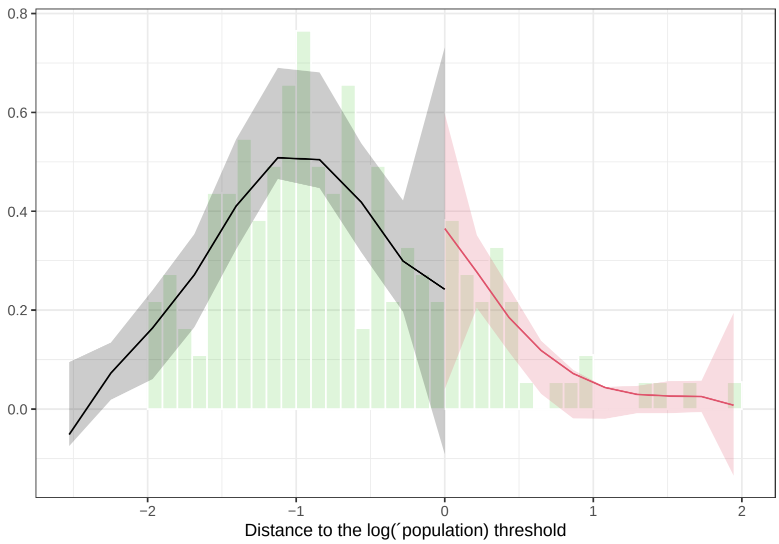
Notes: This figure shows the imperfect compliance in CPRaio roll-out Phase 2. Seven municipalities below the threshold of 50.000 inhabitants received a CPRaio squad.

Figure C.3 – Baseline covariate balance around the CPRaio Phase 2 threshold



Notes: This figure shows the robust-bias corrected t-statistics and standardized coefficients from the baseline covariates' balance RD estimates. For each variable, I run an RD with linear polynomial and uniform kernel specification. Optimal bandwidth following Calonico, Cattaneo, and Titiunik, 2014 were chosen to minimize the mean squared error of the polynomial RD point estimator. In the t-statistics graph, I show the 5% significance level thresholds in red.

Figure C.4 – McCrary Test



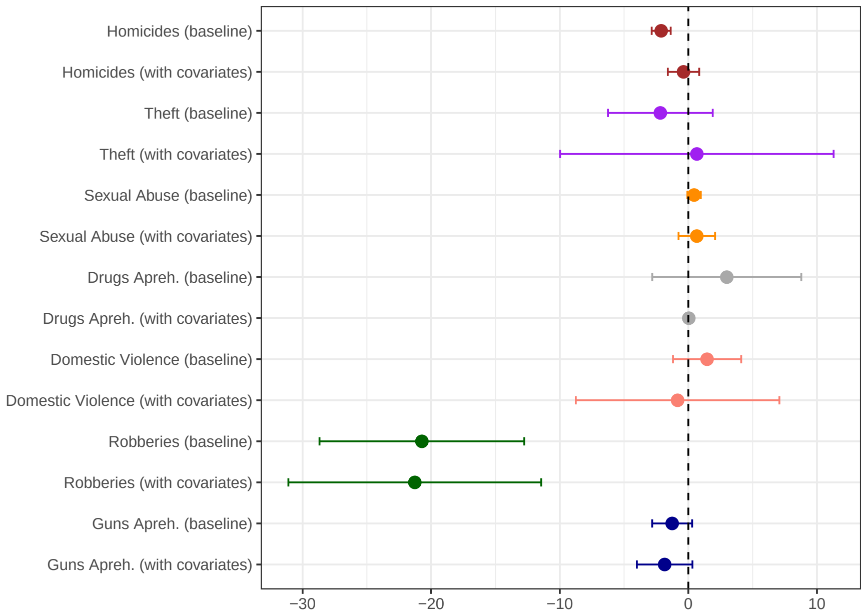
Notes: This figure displays the McCrary density test for the running variable around the CPRaio Phase 2 cutoff. McCrrary test p-value = 0.16 (McCrary, 2008).

Table C.1 – CPRaio Phased Roll-out - Sharp RDD Estimates

	Δ Homicides per 100k pop.	Δ Robberies per 100k pop.	Δ Theft per 100k pop.	Δ Guns Seized per 100k pop.	Δ Sexual Abuse per 100k pop.	Δ Domestic Violence per 100k pop.
Panel A: Linear Specification						
RD Estimator	-2.095	-6.551	-3.889	3.229	0.343	2.535
Robust p-value	0.030**	0.366	0.279	0.245	0.613	0.767
Robust conf. int.	[-3.950 , -0.538]	[-10.627 , 3.087]	[-12.668 , 2.618]	[-1.467 , 8.540]	[-0.763 , 1.669]	[-13.859 , 19.934]
CCT-optimal BW	0.545	0.281	0.525	0.665	0.565	0.349
Eff. number of obs.	48	24	47	58	49	29
Panel B: Quadratic Specification						
RD Estimator	-2.662	-8.248	-1.826	1.188	0.076	5.303
Robust p-value	0.083*	0.198	0.945	0.948	0.936	0.594
Robust conf. int.	[-5.809 , -0.150]	[-18.564 , 2.274]	[-18.665 , 17.151]	[-7.277 , 7.874]	[-2.288 , 2.076]	[-13.384 , 26.200]
CCT-optimal BW	0.545	0.380	0.438	0.590	0.598	0.607
Eff. number of obs.	48	33	41	50	50	50

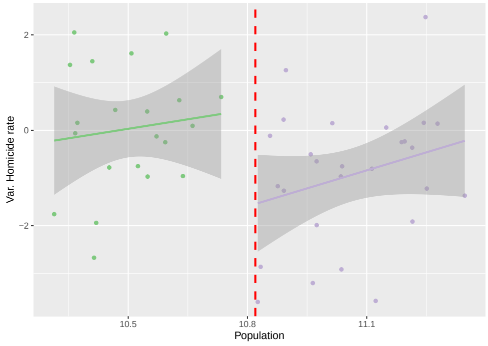
Note: The table reports RD estimates of the effect of CPRaio phase 2 on the rate of Homicides, Robberies, Theft, Guns Seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants in Ceará Municipalities around the threshold of 50 thousand inhabitants. Panel A shows the results for a first-degree polynomial estimation. Panel B shows the results for a second-degree polynomial estimation. Optimal bandwidths following Calonico, Cattaneo, and Titiunik, 2014 were chosen to minimize the mean squared error of the local polynomial RD point estimator. Following that same work, I report robust-bias corrected p-values and 90% CIs. Coefficients significantly different from zero at 99%(***), 95%(**) and 90%(*) confidence level.

Figure C.5 – Conditional and Unconditional Differences in Differences

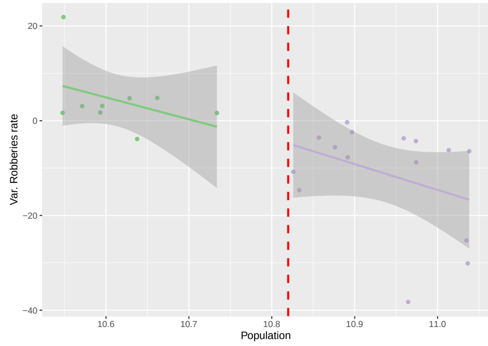


Notes: This figure displays a comparison of the overall Average Treatment Effects (ATE) of CPRaio squads on crime outcomes under the baseline specification (without time-varying covariates) and an alternative specification controlling for linear trends and gross domestic product at the municipality level.

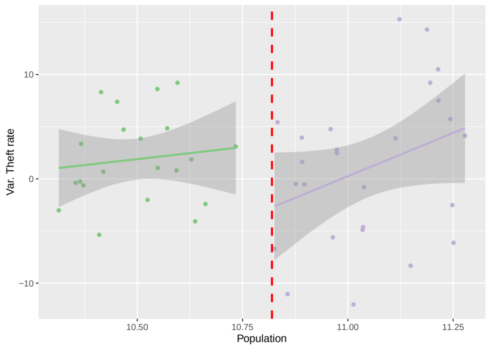
Figure C.6 – The Effect of CPRaio - Sharp RDD



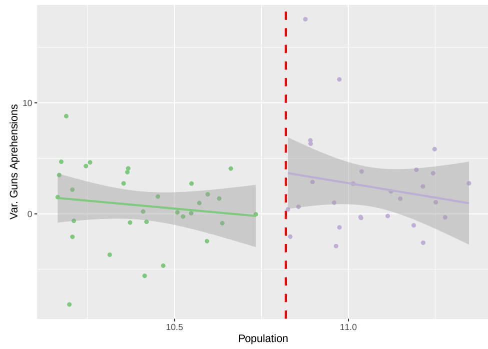
(a) Homicides



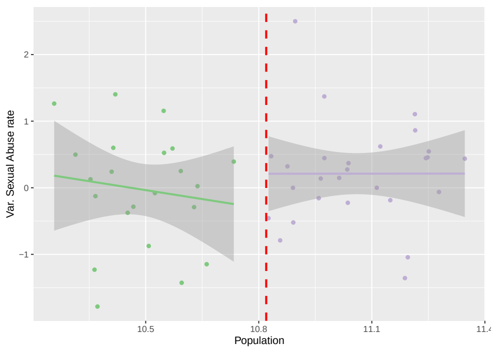
(b) Robberies



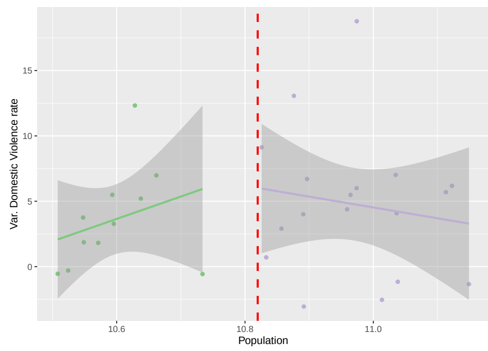
(c) Theft



(d) Guns Seized



(e) Sexual Abuse



(f) Domestic Violence

Notes: The figure shows the effect of CPRaio roll-out phase 2 on the rate of Homicides, Robberies, Theft, Guns Seized, Sexual Abuse, and Domestic Violence per hundred thousand inhabitants graphically in Ceará using a Sharp RDD where I exclude the non-complier municipalities. The outcomes are measured in the period pre and post-CPRaio implementation in municipalities with more than 50 thousand inhabitants from 2014 to 2019. Plots were generated accordingly to Calonico, Cattaneo, and Titiunik, 2015. All estimates use a linear specification and a triangular kernel. Following Calonico, Cattaneo, and Titiunik, 2014, the optimal bandwidths were chosen to minimize the mean squared error of the local polynomial RD point estimator.

Appendix D: Measurement of Crime Outcomes in the RDD

In Figure C.1, I illustrate the phased roll-out of CPRaio starting in 2015, where municipalities received battalions on different dates during each phase. Specifically, in my RDD exercise, Phase 2 began in September 2017 and concluded in August 2019, with thirty-four municipalities receiving a CPRaio squad during that period. In April 2020, Phase 3 started, which lasted until October 2022, assigning twenty-four municipalities to the roll-out.

To ensure comparability between treated and non-treated municipalities, I subset the sample to the period between January 2015 and December 2019, excluding the effect of CPRaio squads assigned in Phase 3. This ensures that I am comparing municipalities that received the treatment with those that did not.

To account for the varying lengths of exposure among municipalities assigned to Phase 2, I calculate the variation in monthly average pre- and post-roll-out completion for all crime outcomes. Thus, the variation in crime outcome for a municipality i assigned to CPRaio Phase 2 is calculated as:

$$\Delta y_i^t = \frac{\left[\sum_{i=d_i}^{\text{Dec,19}} y_i/(\text{Dec,19} - d_i)\right]}{\left[\sum_{\text{Jan,15}}^{d_i-1} y_i/((d_i - 1) - \text{Jan,15})\right]}$$

Here, d_i is a variable indicating the month when municipality i assigned to Phase 2 received the CPRaio squad. In summary, Δy_i^t captures the variation in monthly crime outcomes between the pre-and post-CPRaio period from January 2015 to December 2019.

For the control group, I establish the start date of Phase 2 (September 2017) as a baseline. The crime outcomes for these municipalities are measured as:

$$\Delta y_i^c = \frac{\left[\sum_{i=\text{Sep,17}}^{\text{Dec,19}} y_i/(\text{Dec,19} - \text{Sep,17})\right]}{\left[\sum_{\text{Jan,15}}^{\text{Aug,17}} y_i/(\text{Aug,17} - \text{Jan, 15})\right]}$$

In these equations, y_i represents the crime outcomes at the municipality level, and the date formats are standardized as "Month, Year" (e.g., "Dec,19" and "Jan,15").

3.7 References

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The investigation into the impact of market regulation not only underscores the relevance of monitoring capabilities in crime reduction but also emphasizes the huge concentration of crime in particular neighborhoods. Examining the consequences of reduced police surveillance during st4rike4s, this chapter demonstrated a substantial increase in gang-related homicides in areas disputed by criminal factions. The results underscore the delicate balance between law enforcement presence and gang conflicts, suggesting that abrupt reductions in policing can inadvertently escalate violent outcomes. This contributes crucial insights into the challenges faced by law enforcement agencies dealing with organized crime, particularly in regions where criminal groups vie for territorial control.

Final Remarks

In this dissertation, I investigate the relationship between market regulation, law enforcement interventions, and crime in the context of Brazil. The three chapters have yielded valuable insights into the dynamics of criminal activities, shedding light on effective strategies to curb illicit markets and enhance public safety.

The investigation into the impact of market regulation not only underscores the relevance of monitoring capabilities in crime reduction but also emphasizes the huge concentration of crime in particular neighborhoods. Examining the consequences of reduced police surveillance, we demonstrated a substantial increase in gang-related homicides in areas disputed by criminals. The results underscore the delicate balance between law enforcement presence and gang conflicts. Last, the investigation into the effects of police reorganization revealed a significant decrease in violent property crime following the roll-out of elite police squads. The findings contribute to the literature on the causal relationship between law enforcement strategies and crime outcomes, emphasizing the multifaceted impact of police reorganization on distinct crime categories.

The implications of these findings extend beyond academia, offering practical considerations for policymakers facing the persistent challenges posed by criminal organizations in Brazil. The research highlight the relevance of targeted market regulation, monitoring capabilities, and law enforcement strategies in reducing crime.

In conclusion, this dissertation contributes to the growing body of research on crime dynamics and law enforcement strategies. From a policy perspective, the insights derived from this research offer valuable considerations for crafting effective and targeted interventions.