Essays on Urban Violence and Health

by

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Abstract

Chapters 2 and 3 analyze a place-based policy that reduced the effects of lowering lethal violence at the neighborhood level for several years in some of the most violent neighborhoods on earth. Chapter 2 discusses how this reduction affects short-run learning gains, employment, and incarceration for treated individuals in their early adulthood. The policy increases human capital for students in the short run. Fewer disruptions in the school routine, less student absenteeism, and a safer environment *within* school drive these results. Moreover, younger individuals have a substantially lower likelihood of being incarcerated later.

Chapter 3 evaluates the spatial spillover induced by the policy. I find that the program decreased homicides and police killings in treated areas and did not cause crime displacement to other places in Rio de Janeiro. There is suggestive evidence of crime migration to areas in Rio's metropolitan region and the state's countryside.

In Chapter 4, I investigate how localized heat stress affects vulnerable populations *within* the city of Rio de Janeiro. It is known that temperature shocks increase mortality, and the link is primarily via human physiology. However, most of this evidence comes from cross-city and epidemiological studies in developed countries. This chapter examines the heat-mortality relationship at a fine-grained level within Rio de Janeiro. We rely on novel satellite imagery sources on temperature and administrative health records at the individual level to build a neighborhood-by-month panel over 14 years. Heat stress increases all-cause mortality in individuals aged 60 years or older but does not affect other age groups. In particular, we find that hot days in a typical month in Rio account for 2% of cardiovascular deaths in the population 60+. Access to preventive health care can attenuate the marginal effect of temperature on these deaths. We conclude that temperature shocks are localized within cities, implying that remedial policies should also be localized.

Lay Summary

This thesis discusses development issues raised in urban contexts. In Chapter 2, I analyze the effects of a public policy that decreased violence in poor neighborhoods in the city of Rio de Janeiro on learning gains in the short-run, and on formal employment and incarceration in the medium run. I evaluate the effects of this policy on crime displacement in Chapter 3. Chapter 4 shows that localized heat stress *within* a city increases mortality, mainly for causes of death that reflect underlying chronic conditions, on the elderly. We also show that expanding primary health care coverage can fully mitigate these adverse effects of heat stress.

Preface

The research in Chapters 2 and 3 are original, unpublished and independent work by the author of this thesis, Vinicius Peçanha. Chapter 4 is an original, unpublished, and joint work with Rudi Rocha (Getulio Vargas Foundation) and Dimitri Szerman (Amazon). The other co-authors contributed to the identification of the research question. Vinicius expanded the idea and mostly contributed to the design of the research project, review of the literature, cleaning and analysis of the data, and evaluation of the results. This chapter was written collaboratively.

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Acknowledgements

There is a song (Cobra Rasteira, from the Brazilian group Metá Metá) with two beautiful verses that describe my experience during the Ph.D.: *"Nem todo trajeto é reto // Nem o mar é regular"*. They mean that not every path is straight; not even the sea is linear. During this trajectory over the years in graduate school, I faced several challenges that, while difficult, enriched my unique experience in life. And, most important, I was not alone.

First, I would like to thank my wife, Amanda, for her patience and love. She shared the anxiety, frustrations, and joy throughout this long journey. This Ph.D. is yours as well. I love you. My family also deserves my gratitude. I've been physically distant these years, but I do care. I missed many important moments, from deaths to births, for being abroad. I wish I had been there at any of these events, and I am sorry that I couldn't.

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Vancouver brought me many friends who helped me with academic issues and the distance to Brazil. Fernando and Ieda, thanks for listening to my complaints and showing me that I could have fun in Vancouver. Brayan, even with the distance, our conversations inspired me and helped me understand more about the limitations and the cynism of science – but, also its power. Pablo, Juan, Davide, Anand, Sev, and other members of my cohort: your intelligence and research capacity pushed me further; you also taught me that I could have some fun.

I want to thank my friends from Rio's City Hall. Duda, Clara, and Rodrigo, from whom I learned about the technical part of policymaking and the politics of the process. Working with you made this research more credible to me. Ygor and Adriano, you showed me how careful listening is a fundamental part of a policy. Paulo and Fernando: working with policy is also about managing crises. And good communication is an essential part of this process. I hope we can implement a public policy together again.

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– *À Amanda e à nossa ohana. À minha família pelo amor incondicional.*

Chapter 1

Introduction

The Thesis discusses development economics issues and policies to deal with some of them in an urban scenario. These are relevant questions because most citizens will live in a city by 2030, and the majority will be in urban areas by 2050 (UN, 2018). Notably, cities face several specific challenges, such as urban violence, the impacts of climate change, and how we deal with inequality and spatial segregation (Glaeser and Cutler, 2021). These problems deserve our attention as development economists and demand responses tailored to cities that incorporate specific features of the urban context.

In my Thesis, I analyze policies that deal with some of these concerns. Chapters 2 and 3 examine a policy to reduce urban violence in some city areas. More specifically, in the second chapter, I investigate the short impacts on learning gains and the medium-run effects on incarceration and formal employment of a policy that reduced violence in poor neighborhoods in the city of Rio. In the third chapter, I analyze the spatial consequences in terms of crime in the city and metro area of Rio, and I discuss the crime dynamics after this policy. And finally, in the last chapter, I estimate the impacts of temperature shocks within the city and how localized public policies can mitigate these effects.

The second chapter evaluates a large place-based policy that sharply reduced violence for up to four years in poor neighborhoods (*favelas*) in the city of Rio. There are challenges to isolating the effects of urban violence decrease at the favela level on learning gains and outcomes in early adulthood. Notably, urban violence is ingrained in social contexts that are difficult to rapidly change (UN Habitat, 2007). To overcome this limitation, I evaluate a policy, the Pacification Police Units Program (UPP), that altered policing strategy in treated favelas. Instead of intermittent police raids, the approach focused on the permanent occupation of some of these neighborhoods by community-oriented policing. The program was designed to reduce the territorial control of drug traffickers and to create a safe environment for the mega-events that would happen in Rio, such as the 2014 World Cup and the summer 2016 Olympic games. The program started in 2008, and as of 2014, it had treated 28 out of 53 large favelas in the city, where more than 1.5 million citizens lived.

I exploit the roll-out of this policy to show that it reduced total homicides by a fourth and police killings by almost 40% in treated areas. Moreover, this reduction lasts for six semesters after the start of the program. Students in schools within treated areas have more significant standardized test scores than those in similarly untreated large favelas. Due to the program, their test scores rise by 0.09 standard deviation for the Math exam and 0.07 for Reading. Fewer disruptions in the school routine, less student absenteeism, and a safer environment *within* school drive these results. Observable changes in students' composition, teachers' quality, infrastructure improvements, or increases in parental income do not conduct these outcomes. Moreover, individuals more exposed to the program while growing up have a substantially lower likelihood of being incarcerated in their early adulthood. Each additional year in Primary school after the beginning of the UPP in a treated place reduces the possibility of being imprisoned by 20%. Although there are data limitations, the evidence on the medium-run mechanism is consistent with these effects being caused by changes in cognitive function associated with a less violent childhood and a change in drug-related career opportunities for young men in favelas.

In the third chapter, I discuss potential spatial spillovers caused by this place-based policy. Crime displacement is a primary concern for place-based policies. For example, negative spatial spillovers to areas in the control group violate the SUTVA assumption for causal inference and bias the results. I use data at the police station level (coarser than favela units) to evaluate the likelihood of crime displacement to untreated police stations. I show that the violence indicators in untreated areas in the city of Rio also present a downward trend after the treatment. Given the size of the UPP policy, it is unlikely that a confounder drives these trends. Thus, the results I found in the second chapter are not an econometric artifact caused by crime displacement to untreated areas. However, I also find suggestive evidence of crime migration to regions outside the city of Rio, such as the metropolitan area and the countryside of the state of Rio.

Chapter 4 revisits the heat-mortality relationship by studying the role of localized temperature shocks *within* the city of Rio. A growing stream of causal evidence has shown that changes in environmental factors affect human health. The heat-mortality relationship has attracted particular attention, as the potential risks of climate warming and average temperature changes are expected to be widespread across the globe. Detrimental effects of exposure to heat waves and extremely high temperatures, considered one of the most damaging events, have been well documented in different contexts in developed and developing countries. This has been made possible, to a great extent, by the utilization of plausibly exogenous variation in weather indicators at the national and sub-national levels. A central empirical question, however, is how localized these effects are. This is especially relevant should damage be heterogeneous within regions. While much of the existing evidence comes from estimates of average treatment effects at the regional level, little is still known about the extent to which the impacts and the distribution of damages are localized in general and how effective localized policy responses can be in particular.

We construct novel data from high-frequency satellite imagery on *land surface temperature* (LST) and health records to assemble a neighborhood-by-month panel over 14 years. With these data, we explore within-neighborhood variation in temperature to identify the effects of heat stress on health outcomes. In particular, we investigate the effects of temperature shocks on the mortality rates due to cardiovascular diseases of individuals aged 60 years and older. We find that days above 40 degrees Celsius in a typical month in Rio account for 2% of deaths due to cardiovascular conditions in the population 60+. However, we show that access to preventive health care can mitigate these adverse effects. We contribute to the literature with novel evidence on spatial heterogeneity in heat stress damages, which has implications for the optimal design of policies to attenuate the harmful consequences of temperature shocks.

Chapter 2

The human capital effects of a neighborhood-level reduction in violence: Evidence from Rio's favelas

2.1 Introduction

Urban violence is one of the main concerns for citizens in Low- and Middle-Income Countries and imposes significant welfare and economic losses due to violence (Jaitman et al., 2015). Cerqueira et al. (2019) estimate that Brazil, for example, has economic losses as large as 6% of its GDP. Moreover, shootings and chronic exposure to violence directly impact school outcomes (Monteiro and Rocha (2017); Ang (2021), Koppensteiner and Menezes (2021)) and have long-term consequences in the labor market, health, and prison outcomes (Sviatschi (2022); Chetty et al. (2016); Damm and Dustmann (2014)). Nonetheless, little is known about the human capital effects of reducing urban violence at the neighborhood level and its consequences in the medium run. Most of the papers in the literature focus either on exposure to acute episodes of violence (Rossin-Slater et al. (2020); Bharadwaj et al. (2021); Cabral et al. (2021)), interventions at the individual level to cope with violence (Blattman et al., 2017) or on programs that move individuals out of a violent neighborhood to a safer environment (Chyn, 2018).

In this paper, I estimate the impact of a place-based policy that decreased lethal violence for up to four years in some of the most violent neighborhoods (also known as *favelas*) on earth on short- and medium-run outcomes. Initially, I focus on the human capital effects of this neighborhood-level reduction in violence. I analyze students' performance in standardized test scores and show how the policy induces learning gains. Then, I explore the effect of the policy on altering individuals' trajectories in the medium run by either increasing their chances of formal employment or reducing their criminal involvement[1]. Finally, I discuss if human capital improvement due to a less violent environment translates into medium-run outcomes.

There are several challenges to credibly estimating urban violence reduction at neighborhood levels on human capital effects and their impacts later in life. First, urban violence has deep-root causes that are hard to alter in the short run (UN Habitat, 2007). Thus, successful public policies that decreased urban violence at the local level had either a longer time horizon by focusing on social investments or institutional reforms (Hoelscher and Nussio, 2016). Moreover, as in many other cities in Latin America, criminal groups control parts of the territory and display criminal governance in these neighborhoods, imposing difficulties on the government to implement public policies in there (Uribe et al., 2022). Second, there is an empirical challenge. Data limitations restrict researchers' capacity to address certain questions. Specifically, it is only possible to analyze the medium-run effects of a policy with identified administrative data.

To overcome the first challenge, I analyzed a large public policy that tackled urban violence by changing the logic of policing strategy. Before the program, the Police relied on a 'logic of war' (Prado and Trebilcock, 2018) against drug traffickers focused on intermittent police raids, which often caused shootings in the favelas (Lessing, 2017), disrupting citizens' lives and increasing the risk of these individuals getting caught in the crossfire. With the beginning of this new policy, the strategy shifted to installing permanent police stations in the favelas and operating in the logic of policing closer to citizens' needs. The Pacification Police Units Program (UPP) was established in some poor neighborhoods to end armed control of the territory by drug gangs and introduce community-oriented police in the favelas. The program was designed to curb criminal power in Rio's favelas and to prepare the city for the mega-events (2014 World Cup and 2016 Summer Olympics) happening a few years ahead (Willadino et al., 2018). The UPP program rapidly expanded over time. From the end of 2008 until 2014, 38 UPP units were installed in 28 out of 53 large favelas in Rio, reaching almost 1.5 million citizens 'treated'.

Regarding the empirical challenge, I use several data pieces in this work, such as violence outcomes geocoded at the treated favela level, national standardized test scores from *Prova Brasil*, School Censuses, and administrative datasets linked by name and date of birth. In particular, I employ the school records data for the universe of students enrolled in municipal public Elementary and Middle schools from 2000 to 2014, the Brazilian Employer-Employee

[1]These two medium-run outcomes can be understood as the extreme points of a spectrum of job quality (Musumeci, 2016; Willadino et al., 2018). While drug-related jobs are risky and do not require many years of education, formal labor market positions are stable and correlate with (more) human capital-intensive jobs.

matched dataset (RAIS) that displays information about all formal labor market connections in the state of Rio de Janeiro, and the universe of individuals incarcerated in 2018 for the state of Rio de Janeiro. The administrative datasets allow me to link students in schools in large favelas before the beginning of the program to the labor market and incarceration outcomes later in their lives.

I rely on the staggered introduction of the UPP program, which is unanticipated by the agents and generates a temporal and spatial variation of exposure to the treatment to identify the causal effects. I perform three empirical exercises that shed light on the policy's short- and medium-run effects. First, I use the geocoded violence data for ever-treated favelas to test in a difference-in-differences framework if violence decreased in treated areas relative to not-yet-treated places. Second, I use individual test scores for students in schools located in large favelas to estimate the effects of the policy on learning and human capital. I complement this data with a rich set of information from Students', Teachers', and Principals' socioeconomic surveys to explore the channels for these effects. Finally, I employ the administrative school records data to find the individuals who were enrolled in schools in treated or untreated favelas before the beginning of the UPP program. I link these students with the other administrative datasets – formal labor market and incarceration data – that encompass the universe of individuals with a formal job and incarcerated in 2018. I analyze how the policy changes the likelihood of being present in these medium-run data and how the effects vary depending on the age of individuals when treatment started in their school. I adopt a cohort-place fixed effects strategy (Hoynes et al., 2016) that estimates the Intend-to-Treat effect of being exposed to the treatment at a certain age.

I show that UPP decreases total homicides in treated favelas by almost 25%. Moreover, due to the change in policing strategy in treated favelas, police killings reduce by 38%. The policy induces a level-shift decrease in total homicides until six semesters after the beginning of the treatment. Thus, the results show that UPP significantly reduced exposure to lethal violence in treated favelas. Crime displacement is a primary concern for place-based policies. These negative spatial spillovers violate the SUTVA assumption for causal inference and bias the results. In the Second Chapter of the Thesis, I provide a more detailed discussion of crime displacement by using data at the police station level (coarser than favela units). With this data, I test the likelihood of crime displacement to untreated police stations[2]. I show in the next chapter that the violence indicators in untreated areas also present a downward trend after the treatment. Given the relevance and magnitude of the UPP policy, it is unlikely that a confounder drives these results. Thus, relevant to this paper, there is no evidence of crime displacement to untreated favelas.

[2]I define a police station as untreated if there are large untreated favelas within its catchment area. I will provide a more profound discussion in the next chapter.

For the short-run results, I test whether treated students in Primary school (5th and 9th grades) perform better on national standardized exams than individuals in schools located in untreated favelas. Test scores increase by 0.09 standard deviation for the Math exam and 0.07 for Reading. The results last for (at least) three exam waves after the treatment. These findings are not driven by observable changes in students' composition, teachers' quality, infrastructure improvements, or increases in parental income. Moreover, I do not find the effects of the UPP treatment on other educational outcomes at the school level, such as dropout or age-grade distortion rates. I observe, however, that the significant decrease in violence in treated places translates to less violence *within* school, fewer disruptions in the school routine, and less student absenteeism. These results are consistent with the effects driven by reducing the violence burden and creating a more stable learning routine at school.

In the medium-run, the estimates indicate that the younger the citizen was when treatment started in their school, the lower the probability of being incarcerated in early adulthood. These results hold up to individuals aged 14 years old or less when UPP treatment began. In a similar empirical exercise, I calculate the number of predicted years of exposure to treatment while in Primary school (grades 1 to 9), and I find that each additional year in this educational stage reduces the likelihood of being incarcerated by 20% of the sample mean.

Can human capital impacts captured in the short-run test scores explain medium-run incarceration effects? Short-run test score gains and medium-run reductions in incarceration are both concentrated among boys, suggesting human capital gains may play some role in later life outcomes. Alternatively, short- and medium-run effects could be driven by a third persistent mediating variable unobserved by the econometrician that arises from a reduction in exposure to violence or to criminal gang members. Some plausible examples include improvements in cognitive or psychosocial function that are concentrated among boys or reductions in remunerative employment opportunities in the drug trade (for students in primary school but also in early adulthood).

Although it is difficult to disentangle these mechanisms with the available data, there is evidence that the effects are not driven by a strictly human capital mechanism. First, there are no medium-run effects on participation in the formal labor market. Second, learning effects dissipate over time. I observe two cohorts in both grade 5 and grade 9 (in a repeated cross-section). Large test score gains for exposed cohorts in grade 5 do not persist to grade 9, where there are no learning improvements for these early exposure cohorts.

Closer to this paper, Prem et al. (2021) study the impacts of FARC's conflict termination on educational outcomes in Colombia. They find that the decrease of violence exposure in places with previous FARC presence reduces dropout and modestly improves test scores. I expand their research by discussing the medium-run impacts of violence reduction and by focusing on a context plagued by urban violence. Furthermore, Ang (2021) provides evidence that police killings have detrimental short and medium-run consequences for Hispanic and black individuals. I show that *reducing* lethal violence, especially police violence, increases test scores, but these effects are suggestively higher for white citizens. The results in this paper suggest that only decreasing police killings and lethal violence at the neighborhood level may not close the race gap in test scores.

This work contributes to the growing literature on the impacts of exposure to violence on several outcomes. Monteiro and Rocha (2017), Duque et al. (2019), Koppensteiner and Menezes (2021), and Sviatschi (2022) discuss how acute violent episodes impact schools, health, and labor market results. I expand this literature by analyzing a relatively persistent level shift in violence exposure and not only single episodes. Ferraz et al. (2015), Magaloni et al. (2020), Lautharte (2021), and Ribeiro (2020) evaluate the impact of UPP on violence and criminal governance, infant outcomes, and school routine[3], respectively. I add to this literature by analyzing the effects of the policy on learning outcomes and discussing the policy's medium-run effects.

I also add to the literature on neighborhood effects and policies that alleviate the consequences of growing up in a poor neighborhood (Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018). I contribute to this stream of literature by showing the results of a policy that changed a critical feature of these neighborhoods by reducing homicides and police killings while arguably holding other unobservables such as social networks and interactions, and informal institutions constant. I report the effects on individuals who lived in the same neighborhood as before but, after the policy, in a less violent environment. The results suggest that it is possible to change the opportunity of citizens without moving them to places further away.

Besides this introduction, the paper contains six more sections. In section 2, I analyze the institutional context of Rio's criminal market and the importance of the UPPs in this market. In section 3, I discuss the data and the empirical strategy of the paper. I show the results in section 4 and debate how the short- and medium-run results connect in section 5. Finally, in section 6, I conclude the analysis and discuss future work.

[3]Ribeiro (2020) estimates the effects of the UPP on the number of days a school was closed due to shootings in its surroundings. In this paper, I improve his identification strategy by employing a Difference-in-Differences empirical strategy and by incorporating plausible treatment heterogeneity effects of the UPP policy.

2.2 Context and Policy Intervention

2.2.1 Violence and Drug Gangs in Rio's Favelas

Rio de Janeiro is one of the largest cities in the world, displays endemic violence levels, and has a complex criminal environment in which most of the city's poor neighborhoods are ruled by drug factions (Magaloni et al., 2020). In 2007, a year before the beginning of the Pacification Police Units, the homicide rate in Rio was almost 53 homicides per 100,000 citizens. On top of that, Rio has a violent Police force: in that year, the Police itself killed 902 citizens, with a police killings rate of 14.6 deaths per 100,000 citizens.

These numbers are higher than in other contexts with criminal presence, such as Colombia (37 deaths per 100,000 individuals)[4] and Mexico (8 per 100,000)[5], and are similar to places that display criminals gangs with substantial territorial control such as El Salvador (57 per 100,000)[6]. Barcellos and Zaluar (2014) suggest that criminal groups' turf wars over territorial control and the *modus operandi* of the Police in Rio explain this high homicide rate.

The presence of criminal organizations that control parts of the territory and impose their rule in these areas is widespread in Low- and Middle-Income Countries. For example, Uribe et al. (2022) estimate that more than 70 million citizens in Latin America live under the governance of criminal groups. These organizations negatively impact the socioeconomic development of the neighborhoods where they operate (Melnikov et al., 2020). In Rio de Janeiro and in parts of the state of Rio de Janeiro drug gangs started to operate in areas of the territory in the 1980s. These criminal groups locate in *favelas* (slums) in the hills of the city of Rio de Janeiro and poor neighborhoods in the metropolitan region of the city. Couto and Hirata (2022a) estimate that more than 1.5 million citizens (more than a fifth of the city's population) were under criminal governance in the city of Rio before the beginning of the UPP program.

The drug traffickers control these communities' social and economic activities, impose their laws and judge conflicting cases (Dowdney, 2003). Due to the adverse geography of these places, police incursions are at a disadvantage in clashes with criminals, fortifying the command of the drug gangs over the slums (Serrano-Berthet et al., 2012).

There are three significant drug gangs - Red Command (CV), Friends of Friends (ADA), and Pure Third Command (TCP) – that started acting as prison gangs and, then became criminal enterprises displaying a high degree of criminal governance in the favelas (Hirata and

[4] https://data.worldbank.org/indicator/VC.IHR.PSRC.P5?locations=CO. Accessed in July 2022.

[5] https://smallwarsjournal.com/jrnl/art/homicide-mexico-2007-march-2018-continuing-epidemic-militarized-hyper-violence. Accessed in July 2022.

[6] https://data.worldbank.org/indicator/VC.IHR.PSRC.P5?locations=SV. Accessed in July 2022.

Grillo, 2017b; Blattman et al., 2022). The other major groups in Rio's criminal market are the militias. Initially, the militia groups were formed by off-duty state officers, such as police officers and firefighters, to supposedly protect neighborhoods from drug traffickers. Once they control the place, they sell private protection and other utilities in these poor neighborhoods (Cano and Duarte, 2012). Different from the drug factions, their actions are similar to a mafia as described in Gambetta (1993), involving racketeering, rent-seeking activities, and infiltration into the political system. Recently, there is anecdotal evidence that the militia formed a partnership with drug traffickers and started selling drugs[7].

Within the favela, each leader promotes a vertical, hierarchical structure in the drug trade organization. Also, gang leaders improved upon former institutions in these neighborhoods and developed relatively stable institutions in local governance, acting as local political agents (Arias, 2006, 2009). Unlike terrorists or insurgent groups, the gangs' leaders' objective function is mainly an economic one (Lessing, 2008). Violence is instrumentally used to maintain order (guarantee contracts or enforce their 'laws') or expand their business areas and not to increase *de jure* political power or to destabilize social order by terror acts.

The *locus* of the economic power of these groups are the favelas and poor neighborhoods, which shows how territoriality plays a role in the distribution of power of these factions. The competition among the gangs and the militia, the profitability of these disadvantaged places, and the logic of territorial control engendered an arms race and turf wars over the favelas (Souza e Silva et al., 2008; Wolff, 2017).

The criminal workers that are employed in drug factions are young (more than half of them are below 18 years old), non-white (more than 70%), boys (more than 90%), who were born and live in the favelas they work (more than 70%) and more than half of them were raised by their mothers alone (Willadino et al., 2018). In this survey, Willadino et al. (2018) shows that these workers usually start in the drug business at ages below 15 years old (more than 2\3), don't attend school (78%), dropped out of school before high school, and earn between 1 to 3 times the minimum wage. They mainly enter the drug business for economic reasons, either to help their family or to "earn a lot of money", and most of them, more than 70%, were caught by the Police at least once.

Before the UPP policy, the actions of Police in the status quo were based on a 'logic of war' (Prado and Trebilcock, 2018) with a strategy of intermittent police raids in the favelas, without a clear rationale for its actions (Lessing, 2017). Several reports stated that police corruption was widespread in the Police (Misse, 2010; Soares, 2006). Before the program,

[7] https://odia.ig.com.br/rio-de-janeiro/2018/04/5529467-milicianos-e-traficantes-se-aliam-para-a-venda-de-drogas-e-roubo-de-cargas.html#foto=1 and https://g1.globo.com/rj/rio-de-janeiro/noticia/milicia-controla-o-trafico-de-drogas-e-transporte-publico-em-regioes-da-zona-oeste-do-rio-segundo-investigacao-do-mp.ghtml. Accessed in June 2022.

there were no police stations within the favelas and little law enforcement from the Police in these neighborhoods.

2.2.2 UPP Program

The Pacification Police Units program (UPPs) was launched in December 2008 and had as its primary goals to end the armed control of the territory by drug gangs and to introduce a community-oriented police[8]. The program was not focused on eradicating drug trafficking *per se* but on restoring State control through the monopoly of force in some selected favelas.

The program's original design was intended to improve a suite of public services in favelas. This social arm of the program was called UPP *Social*. This arm would be responsible for mapping communities' demands and proposing policies to address these concerns. Importantly, UPP *Social* would have the political support to implement the social interventions in treated favelas. However, due to political reasons[9], UPP *Social* never worked as initially planned[10] and very few social policies were implemented in treated areas (Magaloni et al., 2018; Dias, 2017; Couto et al., 2016).

Then, the UPP policy ultimately only included three key components: (1) the hiring of new police officers without a history of bribery or connection to the gang (Beltrame and Iaquinto, 2014), (2) the territorial overrun of the space, and (3) the establishment of community policing stations inside favelas with an ongoing presence of police in the streets[11]. This increase in law enforcement in treated favelas caused a rise in the cost of doing drug business in these localities (Felbab-Brown, 2011).

Given the focus on restoring territorial control from drug traffickers in favelas, the UPP intervention has an inherent place-based component. Moreover, decreasing the criminal governance in these places potentially impacts several dimensions of people's lives, and therefore, one might consider UPP as a bundled treatment. For example, the UPP program can increase labor mobility and parental income or improve public and private investments in these areas. Although it may be challenging to disentangle all the treatment prospects, I will address

[8]Policymakers also call the policing strategy as proximate policing (Magaloni et al., 2020). The concept relates to introducing police agents in that favela understand citizens' needs and respect their rights.

[9]In meeting a former coordinator of UPP *Social*, she mentioned that UPP created a substantial political surplus and that several political actors were fighting over this. If policymakers concentrated the social policies in one technical area, it would be harder for other political actors to claim part of the surplus. Then, political actors did not give political support to implement these social policies. More in https://www.wola.org/analysis/what-can-be-learned-from-brazils-pacification-police-model/. Accessed in July 2022.

[10]https://rioonwatch.org/?p=17660. Accessed in June 2022.

[11]Cano et al. (2012) suggest the program was a paradigm shift in Rio's policing strategies against drug criminals. Rather than performing episodic armed incursions in the favelas seeking drugs, guns, and criminals, the Pacification Police Unit program focused on establishing a permanent base of operations in the community and breaking the territorial domination of the drug gangs.

some of these possibilities later in the empirical exercises. Notably, the UPP program did not induce significant emigration of individuals since most of the citizens living in treated favelas were born or lived for more than ten years there (Musumeci, 2016). Thus, the policy arguably did not disrupt previous social interactions among neighbors in the favela.

The Public Security Secretary used two loose criteria to establish a UPP in a slum: (i) the favela had to be a poor community, and (ii) dominated by ostensibly armed criminal groups. Several researchers argue that the program was created as Rio's strategy for the mega events that happen in the city, especially the World Cup 2014 and the Summer Olympics 2016 (Frischtak and Mandel, 2012; Burgos et al., 2011; Magaloni et al., 2018; Silva, 2017; Felbab-Brown, 2011). They argue that the goal of UPP policy was to protect World Cup and Olympics areas, and, therefore, not systematically correlated to crime dynamics in Rio's favelas[12].

Moreover, the project targeted large favelas composed of a cluster of small favelas[13]. These "clusters of favelas" were explicitly defined by the Rio de Janeiro District Attorney (MPRJ) in 2019 based on official information from Rio's City Hall[14]. They have a sizeable population, have been plagued by the presence of drug traffickers since the 1980s, and, importantly, they were the spatial planning unit in which Rio's City Hall historically developed and implemented public policies, which engendered a shared sense of belonging to the neighborhood (Matiolli, 2016). Thus, these large favelas are defined as places that create a unique urban cluster and that share historical bonds among themselves[15].

The program gradually expanded over time. Between 2008 and 2014, 37 Pacification Police Units were installed in the city of Rio de Janeiro, and 28 of 53 large favelas were treated. Figure A.1 displays the spatial and temporal evolution of the favelas treated by the UPP program. In terms of expansion over time, three large favelas were pacified in 2008, three in 2009, thirteen in 2010, seven in 2011, five in 2012, five in 2013, and one in 2014.

In figure A.3, I show that the treated units overlap with these large favelas. I will use the remaining ones as controls[16]. Table A.3 presents the descriptive statistics for the favelas in treated and control areas. Treated and control favelas are similar in almost all dimensions. There are two main differences: i) distance to Olympic venues, as I discussed above[17] and

[12]See also https://www.economist.com/the-americas/2013/09/14/from-hero-to-villain-in-rio. Accessed in June 2021.

[13]Throughout the paper, I will refer to these neighborhoods simply as favelas or large favelas, although colloquially in Rio, people would refer to them as "clusters of favelas".

[14]http://apps.mprj.mp.br/sistema/inloco/. Accessed in July 2022.

[15]https://wikifavelas.com.br/index.php/Sistema_de_Assentamentos_de_Baixa_Renda_(SABREN)#Grandes_Favelas

[16]This definition of treated and control units is similar to the one used in Ribeiro (2020).

[17]Most the Olympic venues are pre-existing structures/sites, so we would not expect construction to be a confounding variable.

ii) the number of residents who earn more than ten times the minimum wage. Although the share is small (less than 2%), treated favelas have almost four times more citizens earning more than ten times the minimum wage than untreated favelas. This happens because some of the treated favelas are located in the wealthiest zone of the city. This area's average income in favelas is higher than in other city zones.

The policy was popular with residents in the early years. In two separate extensive surveys, Magaloni et al. (2018) and Ribeiro and Vilarouca (2018) show how individuals in different localities have divergent responses about their general beliefs about the UPP program. Ribeiro and Vilarouca (2018) show that citizens in early-treated favelas (2008 to 2010) are more likely to approve the program, while individuals in late-treated places have more concerns. These papers suggest that recruiting enough new police officers, funding, and institutional support from the police department became increasingly difficult during the scale-up of the policy, weakening the program's efficacy in later years. Therefore, I take this treatment heterogeneity into account in the empirical strategy and use a DD estimator that explicitly allows for treatment effect heterogeneity over the staggered introduction.

2.2.3 Municipal School System in Rio

Public schooling provision is constitutionally divided in Brazil in the following way: (i) Municipalities (cities) provide preschool, elementary (grades 1 to 5), and middle school (grades 6 to 9) education and Youth and Adult Education; (ii) States supply high school education. Rio de Janeiro has the largest municipal school system in Brazil (Bartholo and Costa, 2016) with around 1500 municipal public schools spread all over its territory, almost 40,000 teachers, and attending more than 600,000 individuals from pre-school to Youth and Adult Education[18].

The system has an open enrollment policy in which parents can choose any school for their children[19] (Bartholo, 2014). Thus, there are no school districts for Municipal schools in Rio. Although this rule can create more heterogeneity among students, da Costa and de Souza Almeida (2019) suggests most students attend a school close to where they live. Indeed, most of the students in the data walk to school and live up to a 15-minute walk distance of their school[20].

Most students in Rio are enrolled in municipal public schools. Lemgruber et al. (2022) show

[18] More information in https://educacao.prefeitura.rio/educacao-em-numeros/. Accessed in June 2022.

[19] Parents or individuals responsible for the students can use an online option or go directly to a school. In both cases, they are shown the schools with vacancies, and they can choose their preferred school. More information in https://carioca.rio/servicos/matricula-nas-escolas-e-creches-municipais/. Accessed in June 2022.

[20] In the main sample, more than 75% of students live up to a 15-minute walk distance, and 84% go to school on foot

that almost 70% of total individuals in primary school years are enrolled in public schools. They also argue that there is a strong stratification between private and public schools: individuals with higher socioeconomic status tend to attend private schools.

There are 409 out of 1540 municipal schools within a 100-meter buffer of large favelas. The typical favela has, on average, 7.7 units, and the typical school in a favela has more than 600 enrollments per year. These suggest that schools are present in large favelas and have a considerable number of students enrolled.

There is evidence that episodes of violence disrupt the school routine. Monteiro and Rocha (2017) and Lemgruber et al. (2022) analyze how drug battles and police incursions in the favelas increase the number of days a school closes for external reasons and raise student absenteeism. Moreover, they show that the status quo policing and the presence of drug criminals in these favelas also affect students' learning. Thus, it is expected that the UPP program could positively affect school outcomes by decreasing exposure to criminal governance and episodes of violence.

2.3 Data and Empirical Strategy

2.3.1 Violence

The impact of the UPP program on violence levels in treated places can be seen as the 'first stage' of the estimation. Had the program not reduced violence exposure, I couldn't hypothesize that the policy's short- or medium-run impacts would be through violence reduction. I utilize monthly official crime rates from the Institute of Public Security (ISP-RJ) from 2007 to 2016 to test if the UPP policy reduces exposure to lethal violence. ISP-RJ defines total homicides as the sum of police killings[21] and homicides committed by individuals, which I label as other homicides in the following tables and graphs.

The Institute of Public Security (ISP-RJ) does not provide geocoded information for all homicides. In fact, I only have data for a limited but spatially more precise dataset of favela-level homicides and police killings encompassing only treated favelas. Thus, I could not use homicides in untreated large favelas in this exercise. I provide a detailed control-group-based analysis of violence using police station-level data in the dissertation's second chapter[22]. I also discuss concerns about potential spatial spillovers of the treatment to control units that may bias the estimates in this chapter.

[21]Police killings refer to homicides committed by police agents while on duty.

[22]Police stations' catchment areas, which are the maximum spatial disaggregation with consistent official statistics, are larger than favelas. So, I define a police station as treated if there is at least one treated favela in its domain, and a control police station has a large untreated favela in its boundaries.

I show the summary statics for the main violence indicators in table A.1. Before the beginning of the UPP program (2007 and 2008), the mean semester homicide rate for ever-treated favelas was 25.4 per 100,000 individuals[23]. In this period, police agents were responsible for most of the homicides committed in these large favelas. The bulk of these deaths happened in police operations within the favelas, which caused armed conflicts between the police and criminals (Monteiro et al., 2020). After the treatment, all violence indicators display a considerable reduction.

The empirical specification is a difference-in-differences setup in which I control for semester and favela fixed effects. The identification assumption is that treated, and not-yet-treated units would follow the same trend over time if the program had not happened. To identify the UPP treatment effects on homicides, I exploit the staggered introduction nature of the policy and the fact that the timing of treatment was arguably exogenous once controlled by unobserved fixed favelas' characteristics. First, there was no official disclosure of information about potentially treated areas. Second, the occupation date was released to the public only a week before the beginning of the treatment. I estimate the following equation:

$$Y_{it} = \lambda_i + \delta_t + \beta D_{it} + \epsilon_{it} \tag{2.1}$$

where $D_{i\tau}$ is a dummy that turns one for periods after the beginning of the policy in favela i, λ_i and δ_t control for favela and semester fixed effects, respectively. The parameter of interest is β which captures the impact of treatment on violence rates. The dependent variables are total homicides, police killings, and other homicides.

To reduce concerns related to the skewness of the dependent variables, I collapse the data at the semester level and use the inverse hyperbolic sine transformation of the dependent variable. I also show that the results are robust to Negative Binomial and Poisson specifications.

As I discussed, there is enough qualitative evidence to be concerned about heterogeneous treatment effects *a priori*. Thus, the preferred estimation incorporates this feature of the policy. I evaluate the impact of the program using the estimator proposed by Borusyak et al.

[23]The yearly mean homicide rate for ever-treated favelas was 50.8 homicides per 100,000 citizens between 2007 and 2008, which it is eight times bigger than US homicides rate in 2008 (Cooper et al., 2011)

(2022)[24].

I estimate a dynamic difference-in-differences specification to discuss how the effects of the policy evolve:

$$Y_{it} = \lambda_i + \delta_t + \sum_{\tau=-7}^{-2} \gamma_\tau D_{i\tau} + \sum_{\tau=0}^{7} \beta_\tau D_{i\tau} + \epsilon_{it} \tag{2.2}$$

where, $D_{i\tau}$ is dummy that turns one if $t = T_i^* + \tau$ and T_i^* is the semester that UPP started in favela i. I collapse the data at the semester level to increase each estimate's precision. The parameters of interest, in this case, are the lags, $\{\beta_\tau\}_{\tau\geq 0}$, and the leads, $\{\gamma_\tau\}_{\tau<0}$. The lags display the impact of the policy in τ periods of the program's implementation. I binned periods after 7 semesters and before 7 semesters of treatment to 7 and -7, respectively. If the program could reduce violence in treated places, I would expect the lag coefficients to be negative. The leads show the program's effects τ periods *before* it started. Assuming no anticipation effects, after controlling for place and time fixed effects, I shouldn't expect any difference in pre-trends. Then, I expect that lead coefficients are not statistically different from zero.

For this dynamic specification, there is not enough power to estimate treatment effects in the estimator proposed by Borusyak et al. (2022)[25]. Therefore, I show the OLS estimates for the dynamic regression.

There are two main concerns for the identification of the treatment effect. First, the violation of SUTVA (Rubin, 1986) due to spatial spillovers to not-yet treated units, which could occur if, for example, drug traffickers migrate from treated areas to not-yet-treated neighborhoods and violence levels increase in their new location. In this case of crime displacement, the estimates would be overestimated. Or, if criminals in untreated favelas adapt to avoid treatment in their areas, which would be a crime deterrence effect of the police. However, I consider that crime deterrence in this setting is a minor concern because the estimates would be a lower bound to the actual effect. I discuss these effects in more detail in the Second Chapter.

[24] Borusyak et al. (2022) DD imputation estimator rely on the potential outcomes model $\mathbb{E}[Y_{it}(0)] = \lambda_i + \delta_t$, where $\mathbb{E}[Y_{it}(0)]$ is the potential outcome for unit i in period t if unit i were not treated. Then, they estimate unit and time fixed effects only on non-treated observations. The next step is to extrapolate the estimates to treated observations by imputing $Y_{it}(0) = \hat{\lambda}_i + \hat{\delta}_t$. The authors calculate the treatment effect for unit i and period t as $\tau_{it} = Y_{it} - Y_{it}(0)$. Finally, they aggregate τ_{it} using weights related to the estimand of interest to provide the estimate of the causal impact: $\tau = \sum_{i,t} \omega_{it}\tau_{it}$. Under some common assumptions, they show that their estimator is more efficient than other competing DD estimators. Moreover, they provide conditions to test if pre-trend estimates differ from zero, while other estimators rely on placebos to shed some light on pre-trends.

[25] Technically, the minimum effective number of observations is below the minimum recommended by the authors. They argue that estimates with insufficient observations may be "unreliable and their SE may be downward biased" (Borusyak et al., 2022).

Second, another concern may be raised if treatment is correlated with a contemporaneous violence shock. That is, if treatment time is associated with increased violence before treatment caused by a temporary shock. Given the focus on pacifying areas close to Olympic venues, I shouldn't expect this to be a concern *a priori*. If the introduction of a UPP in a place correlates with an increase in violence, the pre-treatment estimates in an 'event study' estimation strategy would be significant, a hypothesis that I test and reject.

2.3.2 School outcomes

I use individual grades from national standardized exams on Reading (Portuguese Language) and Mathematics from *Prova Brasil* that happen every two years for all students in the 5th and 9th grades studying in public schools with more than 20 students enrolled in that school-grade. The National Institute of Educational Research from the Ministry of Education (INEP-MEC) designed the exams. Their main goal is to evaluate nationwide the educational performance of schools and design actions to improve learning[26]. *Prova Brasil* also contains socioeconomic surveys filled by students, teachers, and principals. These surveys provide demographic information about these groups and describe the learning environment at school. In particular, I use the variables in the surveys as covariates in the main regressions and I also test the impact of Pacification on changes in the school environment, teachers' expectations, and student composition after the treatment.

The primary sample contains Prova Brasil exams from 2007 to 2015. There are two reasons for this time range. First, data is not consistent at the individual level for the 2005 wave of the exam. There are fewer observations at the student level than in other exam wave[27]. Second, as Ferraz et al. (2015) and Willadino et al. (2018) argue, the UPP program decreased violence successfully until after the 2014 World Cup and the 2016 Summer Olympics. However, violence levels returned to pre-treatment in treated areas after 2016. By including exam years after 2015, the treatment effects could be contaminated by the increase in violence; therefore, I do not include more recent years in the main analysis.

I utilize geocoded schools' locations from Institute Pereira Passos (IPP), Rio's City Hall Research Institute, to restrict the sample to schools within 100 meters of distance to a treated or an untreated complex of favelas. I select this threshold of 100 meters because the paper focuses on exploring the local effects of Pacification on school outcomes. Then, I want to restrict schools located within treated or control favelas. I allow a 100-meter buffer to deal

[26]The scores are used to build the Index of Basic Education Development (IDEB), which is an input for Federal transfers to States and Cities in Brazil.

[27]For example, 41,783 observations for all students in Brazil who took the exam in grade 5 in 2005, while there are 2,310,302 observations in 2007. Moreover, no variable allows me to identify the school attended by the student in 2005. Then, I chose not to incorporate this year into the primary sample.

with any geocoding issue that may arise in the spatial organization of the data[28]. Moreover, I keep only schools that appear in all waves of the Prova Brasil exam. With these criteria, there are 60 schools and 23,291 students in treated areas and 78 schools and 38,760 students in control areas.

I aggregate School Census information, annually updated by the National Institute of Educational Research from the Ministry of Education (INEP-MEC) to the *Prova Brasil* data. They contain information about the universe of schools in Brazil. The variables encompass school characteristics such as the number of enrollments, employees, infrastructure, and student and teacher characteristics. Moreover, the School Census provides information about other educational indicators at the school level, such as pass rate, grade retention, age-grade distortion, and dropout rates. I employ this data to test if educational attainment changes in treated schools relative to schools in the control group. *A priori*, the UPP program may impact these indicators either positively or negatively. On the one hand, if students at the margin of dropping out stay at school longer because of the program, pass rate, grade retention, and even age-grade distortion may be negatively affected. On the other hand, if students of lower (higher) quality move into treated schools, I could observe the program's negative (positive) effect on these educational indicators.

I define a school as treated in a specific exam wave if the school is located in a favela pacified at least three months before the exam[29]. The exams usually happen in November. So, a school is treated if the favela was pacified by June of the exam year. Since the exam happens in odd years, if a UPP treats a school in an even year, the treated year for that school is the year after the occupation. For example, if UPP occupied a favela in 2010, I consider the first treated exam wave to be 2011.

The primary outcomes of interest are the test scores for Math and Reading. To interpret the estimates below, I standardized the test scores by the topic of the exam (Math or Reading), the year of the exam, and the student's grade (5th or 9th grade), using only treated or control schools.

I exploit the staggered introduction of the program to estimate the impacts of the Pacification Police Units on test scores. The identifying assumption is that educational outcomes in treated places would have followed the same common trend as never-treated and not-yet-treated units if these treated units had not been treated. The empirical specification is:

$$Y_{isjt} = \lambda_s + \delta_t + \beta D_{isjt} + \phi' X_{isjt} + \epsilon_{isjt} \tag{2.3}$$

[28]I also use a 250m-buffer as a robustness exercise.
[29]I test alternative definitions for treatment in the robustness section.

where i denotes the student and t the year (wave) when the student takes the standardized test; λ_s and δ_t are the school and wave of exam fixed effect, respectively. D_{isjt} is a dummy that turns one for schools in treated favelas j and waves of the standardized test after the year of the beginning of the UPP's occupation in favela j; X_{isjt} are students' and schools' characteristics and ϵ_{isjt} is the error term. In this specification, β is the parameter of interest, and I test the hypothesis that $\beta \neq 0$. The standard error ϵ_{isjt} is robust to correlations within the same favela, j; therefore, they are clustered at the favela level.

Some of the educational outcomes are available only at the school level. For these cases, I run the regressions at the school level, controlling for school and time fixed effects, weighting by the number of students who take the exam or are enrolled at the school (in the School Censuses data, for example), and clustering the standard errors at the favela level.

I also estimate a Dynamic Difference-in-Differences specification:

$$Y_{isjt} = \lambda_s + \delta_t + \sum_{\tau=-4}^{-2} \gamma_\tau D_{isj\tau} + \sum_{\tau=0}^{3} \beta_\tau D_{isj\tau} + \phi' X_{isjt} + \epsilon_{isjt} \tag{2.4}$$

where, $D_{isj\tau}$ is dummy that turns one if $t = T^*_{isj} + \tau$ and T^*_{isj} is the first wave of the exam after the UPP started in favela j. The omitted category is the exam wave before the beginning of treatment.

The UPP program achieved better results in early treated from 2008 to 2010 Magaloni et al. (2018); Ribeiro and Vilarouca (2018). Thus, there are important concerns of treatment heterogeneity and possible biases that I can cause to TWFE estimation in this setting. Thus, I calculate the program's impact using the Borusyak et al. (2022) imputation estimator. This school sample size is large enough that I can estimate the preferred estimator in the dynamic specification. I also show the results are robust to using other estimators that deal with treatment heterogeneity issues such as De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

There are two key weaknesses of this data. The data does not have unique individual identifiers, so I cannot match students over exam waves. Thus, the sample is a repeated cross-section of students[30]. Moreover, the data is not identified, and I cannot link it to other administrative datasets.

[30] I accommodate the repeated cross-section nature of the data in the estimation strategy proposed by Borusyak et al. (2022), by analyzing the potential outcomes model: $\mathbb{E}[Y_{i(s)t}(0)] = \lambda_{(s)i} + \delta_t$, where s stands for the school attended by individual i in period t. The main difference from panel data estimation is that the fixed effect is at the school level, $\alpha_{(s)i}$.

2.3.3 Medium-run outcomes

Enrollment records I analyze identified administrative enrollment records from Rio's Municipal Secretary of Education. The data encompass the universe of students enrolled in municipal public schools (from pre-school to 9th grade) from 1997 to 2014. I observe information about the students' and parents' names and dates of birth, which allows me to create a unique identifier and link this data with other administrative data. There is also information about students' socioeconomic characteristics such as age, gender, race, parental education, if the students or the responsible receive cash transfers, the parents' occupation, if the student attended pre-school, and if the students live with their parents. There were approximately 2.2 million students that passed through the Municipal School System between 1997 and 2014.

Moreover, I observe the history of students' moves in the Municipal Schooling System. It is an unbalanced panel data at student and academic year levels in which any change in students' status is recorded in the data. For example, suppose a student changes classrooms, goes to another municipal or private school, drops out, or enrolls in the same school for a new academic year. In that case, the data records the movement as a new observation. From this data, I construct the information that contains unique entries at the student x academic year x school levels and, thus, show the school attended by the student in each academic year. I discuss how I construct the data in Appendix A.2. For now, I exploit the cross-sectional variable of students enrolled in 2008 to define students in treated or control schools[31].

RAIS – Brazilian Matched Employer-Employee The RAIS data captures the universe of formal labor market relationships in a year. This data is organized by the Brazilian Ministry of Labor from mandatory forms filled by firms that operate in the formal labor market (Dix-Carneiro, 2014). I have access to RAIS for the State of Rio de Janeiro in 2018. There are more than 4.7 million identified workers in the data. The data also have additional information about occupation, wage, working hours, and job length. For now, I am only interested in the extensive margin of formal labor market relationships, i.e., the presence in the formal labor market; I use only the name and date of birth of individuals with formal jobs.

Incarceration I use confidential administrative data from Rio de Janeiro's District Attorney with the universe of individuals incarcerated between 2018 and 2020 in the State of Rio de Janeiro. There are almost 200k citizens that have passed through Rio's Prison System during these years. The data contain identified information for inmates' names and dates of birth.

[31] I will expand the analysis to incorporate the time-series dimension of the panel data in later research. Then, I would be able to analyze how the UPP program affects the changes in schools, grade failure, and dropouts in treated students relative to students in the control schools.

Also, other variables are related to the felony, such as the crime committed or prison length. However, there are many missing observations, and they are not consistently filled in. Then, as the formal labor market data, I consider only the extensive margin of being incarcerated.

Sample

For the linkage among the different administrative datasets, I use students' names and dates of birth and apply the algorithm described in Appendix A.1. In a nutshell, I block the individuals using the first letter of their names and year of birth, calculate the Jaro-Winkler distance for students' names and conservatively define that two names are a match if the Jaro-Winkler distance is above 0.95 and the dates of birth are the same.

This linkage strategy aims to reduce the likelihood of a false positive while accepting a higher probability false negative. That is, I define as a match only individuals who have highly similar names[32] and were born on the same date. So, I expect the number of matches I find to be smaller than the actual number of matches. The primary assumption for estimating treatment effects is that the true positive rate stays relatively constant over time and across treated and untreated places. If this is the case, the linkage procedure would only introduce measurement error in the dependent variable, reducing the power of the statistical tests I perform.

The medium-run empirical strategy aims to estimate the effects of studying in a treated place on outcomes later in adulthood compared to students who attended a school in an untreated favela. So, I use the linked administrative data to test if students who attended a treated school have a higher likelihood of being in the formal labor market or are less likely to be incarcerated later in their lives compared to students who were enrolled in control schools.

I keep the same schools as the short-run sample to achieve this objective. To avoid concerns related to movement across schools caused by decreasing violence levels in treated favelas, I employ an intent-to-treat empirical strategy similar to Hoynes et al. (2016). I use pre-enrollment information; thus, I define a student as treated if she was enrolled in a treated school before the beginning of the program in 2008. Likewise, I represent the set of individuals in the control group as students in a control school in 2008.

I analyze the cohorts born between 1992 and 2000. I chose this range to capture individuals with ages compatible with being in primary school at the beginning of treatment and old enough to be in the formal labor market or incarcerated in 2018. This implies that at the beginning of treatment at the end of 2008, the student has at least eight years old and she is

[32] Names in Brazil have, on average, two or more family names, which reduces the chances of observing prevalent small names such as 'John Smith'. In the administrative enrollment records, for example, 76% of unique names have three surnames; 33% have four surnames.

at or above 3rd grade. Thus, disaggregated cohort level analysis is bounded from below at age 8 and grade 3.

With these restrictions, 61,635 students appeared in treated or untreated schools after the beginning of the UPP program: 23,982 attended one of the 60 treated schools, and 37,653 attended one of the 78 untreated schools. With the linkage algorithm, I find that 17,965 (29.15%)[33] individuals are present in the formal labor market, and 1,825 (2.96%)are incarcerated.

Empirical specification

I exploit the variation induced by the staggered introduction of the program and by year of birth to estimate the impact of the exposure to the policy on the probability of being in the formal labor market or incarcerated in early adulthood. The identifying assumption is that the timing of individual exposure is plausibly exogenous after controlling cohort and school fixed effects.

There are some reasons to *a priori* expect that the program may positively impact younger individuals more. There is an extensive literature of place-based effects (Sviatschi, 2022; Chetty et al., 2016; Hoynes et al., 2016) that show that younger citizens are more affected by these policies and, also, Willadino et al. (2018) show that most of the teenagers involved with drug trafficking entered the business with 13 and 15 years old. So, policies that could change opportunities in life may have higher impacts later. I employ two main empirical specifications to shed light on the timing of exposure to treatment.

First, I analyze how treatment impacts vary by the age that the individual was when treatment commenced in her school. I employ a cohort-place fixed effects strategy (Bailey et al., 2021; Hoynes et al., 2016; Duflo, 2001), in which the individual is treated at age τ if she was τ years old when treatment started. The empirical specification for this exercise is:

$$\begin{aligned} Y_{ibsj} = \; & \delta_s + \lambda_b + \sum_{\tau=8}^{15} \delta_\tau \mathbb{1}\{s \text{ is treated}\}\mathbb{1}\{age = \tau\} + \dots \\ & + \sum_{\tau=17}^{19+} \beta_\tau \mathbb{1}\{s \text{ is treated}\}\mathbb{1}\{age = \tau\} + \Gamma^{'} X_{ibsj} + \epsilon_{ibsj} \end{aligned} \tag{2.5}$$

where, i indexes the individual, b cohort (year of birth), s is the school individual attended in 2008, and j the favela where the school is located; *age* refers to the individual's age when

[33]Reassuringly, in a survey with more than 2,000 residents at favelas treated by the UPP program, Musumeci (2016) finds that 26.6% of respondents were in the formal labor market in 2016, a statistic that is similar to the one I find.

treatment started in that favela, δ_s is school fixed effect and λ_b is a cohort fixed effect. The standard errors are clustered at the favela level. The coefficients of interest are $\{\delta_\tau, \beta_\tau\}_\tau$ that shows the effects of being exposed to treatment at age τ relative to the omitted category of 16 years old.

The parameters of interest ($\{\delta_\tau, \beta_\tau\}_\tau$) are identified from variation within schools (or favelas) across birth cohorts. Figure A.4 shows the variation in treatment age when treatment started in the individual's favela. In these figures, the timeline (horizontal dimension) displays the year of birth, and the vertical dimension shows the year of treatment. So, for example, if a person were born in 1998 and lives in a favela treated in 2010, this person would be in the fourth vertical line and under 1998 in the horizontal dimension. The red numbers refer to the age individual was when treatment began. The never-treated units allow for the estimation of unrestricted cohort fixed effects.

The second estimation follows the empirical strategy discussed in Hoynes et al. (2016)[34]. Similar to their paper, the UPP program, as a place-based policy, 'does not turn off'. All individuals who live in a treated favela are 'eligible' to receive the treatment. Thus, comparisons to estimate treatment effects should be made 'from above', i.e., relative to other (possibly) treated categories.

Given that individuals enter the drug trafficking business at ages 13 to 15 on average and that most of them drop out of school in Middle school (Willadino et al., 2018), I estimate the predicted number of years a student is exposed to treatment while in primary school relative to be treated after this period. The testable conjecture is that individuals more exposed to treatment before the period in which they make critical decisions in their lives will be more impacted by the UPP program.

To calculate the predicted years of exposure to treatment while in primary school, I employ the institutional fact that students enter primary school in grade 1 at age 6 and that primary school lengths for nine years. So, the predicted grade a student is when treatment arrives is defined by: Predicted grade = Treatment year − (Year of birth + 6) + 1, and (Calendar) years of exposure to primary = 9 − Predicted grade. For example, an individual born in 1998 enters primary school in 2004 at age 6. Suppose that she studies in a place that was treated in 2010. Then, she would be predicted to be in grade 7 when treatment started, and, therefore, she would be exposed to treatment for two years while in primary school. I graphically show this example in Figure A.5.

[34]Hoynes et al. (2016) study the effects of being exposed to Food Stamps during childhood on later outcomes in life.

The empirical specification is:

$$Y_{ibsj} = \delta_s + \lambda_b + \beta EUPP_{bsj} + \Gamma^{'} X_{ibsj} + \epsilon_{ibsj} \tag{2.6}$$

where, i, s, j and b index for individual, school, and favela and, the cohort of birth, δ_s is a school FE, λ_b is a cohort of birth FE, $EUPP_{sjb}$ is the number of (calendar) years an individual was treated while in primary school, X_{isjb} captures individual controls and standard errors are clustered at favela level.

The parameter of interest is β, which captures the marginal impact of an additional treatment while in primary school. The parameter is identified from the variation of age and grade from different cohorts in the same school when the UPP policy commenced in that locality.

In these estimations, the exposure to UPP treatment while in primary school or the age the individual was when treatment started already captures the intensity of treatment. The estimates pick any treatment heterogeneity caused by early treated units. Thus, I run Two Way Fixed Effect regressions for the medium-run specifications.

2.4 Results

2.4.1 Violence

Table 2.1 presents the reduced-form estimation for the UPP treatment. The main independent variable is a dummy that turns one for semesters after the beginning of the treatment in a favela. The results show that UPP significantly reduced exposure to violence in treated favelas. The program cut Total homicides, police killings, and other homicides rates by more than 20%. Significantly, the police killings rate in treated favelas reduces by 38% ($p < 0.05$).

Figure 2.1 shows the dynamic impacts of the Pacification program on violence reduction in treated places. The policy induces a level-shift decrease in total homicides until six semesters after the beginning of the treatment. Figure A.2 displays the effects of police killings and other homicides committed by citizens over time. The pre-trend coefficients suggest no differential pre-treatment trends between treated and not-yet-treated favelas for all the crime indicators. This can be seen as evidence that the UPP did not target favelas hit by contemporaneous shocks in its criminal market.

I discuss the robustness of the results in table A.2. First, I use a negative binomial specification to deal with possible concerns about the right skewness of the dependent variable. The incidence ratio results suggest that treatment reduces total homicide levels by 40% ($p < 0.01$) and police killings by more than 60% ($p < 0.01$). The results do not indicate a statistically significant reduction in other homicides.

The results indicate that the reduction in total homicides was primarily driven by the decrease in police killings in treated favelas, which is consistent with the change in the police *modus operandi* induced by the UPP policy (Lessing, 2017). The police replaced the former strategy of intermittent police raids inside the favelas that increased the likelihood of a crossfire with permanent community-oriented policing that dislodged drug traffickers from the favelas. The new policing strategy developed by the UPP decreased the clashes between the police and criminals and reduced the number of deaths caused by the police.

In the second chapter of the thesis, I provide a more detailed discussion about the robustness of these results using never-treated units. I use coarser data at the police station level to construct violence indicators for police stations with a large untreated favelas in their catchment areas. With this data, I can address the spatial spillover concerns that may affect the identification of the effects. I show in the next chapter that the violence indicators in untreated areas also present a downward trend after the treatment. Given the relevance and magnitude of the UPP policy, it is unlikely that a confounder drives these results. Thus, relevant to this paper, there is no evidence of crime displacement to untreated favelas.

2.4.2 School outcomes

Table 2.2 presents the main results for the impact of UPP treatment on Math and Reading test scores. The Pacification program in the preferred specification[35] causes an increase of 0.09 standard deviation for the Math exam and 0.07 standard deviation for the Reading. The point estimates are robust to the introduction of students and school covariates. Notably, the point estimates for level shift violence reduction in Rio's favelas are almost two times larger in absolute value than the impact of episodes of violence on school outcomes. Monteiro and Rocha (2017) estimate that drug battles in Rio reduce standardized test scores by 0.05sd for 5th graders in the Math exam. At the same time, I find an increase of 0.1sd in Math test scores for Elementary students when homicides and police killings consistently decrease in the favelas.

I show how these effects differ by subsamples in panel A of table 2.10: boys perform better

[35]The preferred specification is column (1), in which I do not control for any covariates. There are two main reasons: (i) treated and untreated favelas and schools in treated or untreated places are already similar in almost all of the covariates before the beginning of the program, as shown in tables A.3 and A.4, and (ii) *a priori*, the composition of students or teachers and, investments at school level can change due to the UPP program. For example, better students may move to treated schools after the pacification, or schools might receive more investment following the UPP entry. In this plausible scenario, composition variables would be *mediators* of the treatment on the effects, and the estimates would suffer from overcontrol bias (Cinelli et al., 2021). Given that UPP is a bundled treatment, I prefer to estimate its total effect on school outcomes, focusing on column (1), and then estimate the impacts of the policy on mediators in separate regressions.

than girls in both exams, and there is suggestive evidence that white boys respond more positively to the UPP treatment than non-white boys for both Math and Reading tests. Since boys are more exposed to drug-related violence in the favelas (Barcellos and Zaluar, 2014), the differential gender effects of the policy are consistent with a more intense reduction in exposure to lethal violence and the presence of drug traffickers (Zaluar, 2011) in treated areas.

To explore the suggestive differences between white and non-white boys, I test in table A.5 how these subgroups vary in several socioeconomic characteristics: white boys perform better on math and reading exams on average, they live more with both parents, display a lower likelihood of having failed a grade or having dropped out before, and work less outside the home than non-white students. Besides, there is evidence that white students are wealthier (measured by income proxies). Monteiro and Rocha (2017) indicate that high-performing students are more affected by episodes of violence in their neighborhoods. I show that my results are consistent with the idea that the same type of students – high(er)-performing individuals – benefit more from the decrease in violence caused by the UPP program.

Figure 2.4 exhibits how the consequences of UPP treatment change by the schooling grade (5th or 9th grade). The effects for the 5th grade appear in the first wave after the beginning of the treatment and persist until cohorts that take the exam up to three waves after the treatment. In contrast, the impacts for students in the 9th grade are contemporaneous (Math) and last for one cohort after the treatment. Although I cannot track individuals over time, students who take the exam in the 5th grade should retake the exam in the 9th grade four years (two waves) after the first test. For example, students who took the 5th-grade exam in a school treated in 2009 would retake the exam in 9th grade four years later. By analyzing the dynamic effects for 9th grade, I observed that treated places don't present differential results for exams that happen two or more waves after the treatment. Students who show a positive treatment effect in the 5th grade seem to have a null impact four years later, in the 9th grade. I consider this empirical fact as suggestive evidence that the effect of UPP on school outcomes is dissipating over time.

One possible reason for these dissipating effects over time is that the constant interaction with the police may impose more psychological costs for teenagers in Middle School. Ang (2021) suggests that even low-risk contact with the police can be detrimental to adolescents. Since the UPP police introduced permanent settlements in treated favelas and adopted a stop-and-frisk strategy that targeted teenagers more (Willadino et al., 2018), these negative effects of police behavior may offset the positive consequences of a less violent environment.

Despite the positive effect on test scores, I do not find impacts of the UPP treatment on other educational outcomes at the school level, such as dropout rates or age-grade distortion. Table

2.6 exhibits these results. These findings suggest that better performance at standardized test scores does not translate into changes in the extensive margin decision of staying or not at school, on average. Monteiro and Rocha (2017) find that drug battles nearby schools do not impact the dropout rate in these schools. Taken together, these results suggest that dropout is relatively inelastic to exposure to violence in Rio's favelas. A possible explanation for this inelastic behavior is that primary schooling (grades 1 to 9) is mandatory in Brazil (Bartholo and Costa, 2016) and school attendance is a criterion for social benefits such as conditional cash transfers (Neri and Camillo Osorio, 2019), which increases to legal and financial costs of dropping out of primary school.

Channels

I explore plausible channels that may explain the increase in standardized test scores after the UPP program below:

School Routine Table 2.8 describes school problems reported by Principals. School routine is less interrupted after the UPP program, in line with Ribeiro (2020) that shows that UPP reduces the number of days in which a school was closed due to gunfights in its surroundings compared to schools located in other untreated favelas. Besides, principals report that students' absenteeism decreases after the treatment. Thus, students are less disrupted in their school routine and, therefore, more present in schools after the slums are pacified.

Expectations and violence within the school I exploit the fact that there are questions about future students' expectations and exposure to violence in the Teachers Survey from *Prova Brasil*. Teachers' beliefs about future high school graduation for children in both elementary and middle school increased by more than 16%. However, the UPP program did not change the expectation of college attendance. That is, teachers believe that the program can help students graduate more in high school, but the effects of the policy on school would not alter the likelihood of college enrollment. Exposure to violence within the school also decreases. Teachers report that aggression from teenagers in middle school reduces by more than 17% and aggression among teenagers by 30%. These results suggest that teachers had a positive expectation about the effect of the UPP policy on learning and the treatment reduced teenagers' violent behavior within school.

Students' composition Another possibility is that different types of students move to treat areas after the beginning of the treatment. The composition of students can shed light on why school outcomes improved in treated places. Conceptually, the composition of students may improve in areas with UPP due to the enrollment of better students who were studying outside the favela, or it may deteriorate because good students previously studying in favelas could exploit educational opportunities outside the favela.

I investigate these hypotheses in table 2.4. Indeed, there is an increase in students enrolled in treated favelas. However, there is no change in observable variables that correlate with students' performance. Thus, there is no evidence of changes in student composition in treated favelas caused by the UPP policy.

Teachers' composition and Infrastructure Given that the UPP policy may have induced other investments in treated favelas and that these areas are less violent, school and teacher quality could have increased in treated places. I show in table 2.3 that these results are not driven by changes in school infrastructure or the quality of teachers[36].

Household income I show that the school results are robust to the inclusion of proxies to household income in table 2.5. Students in the treated sample could have higher household income due to the treatment because the UPP policy may have increased labor opportunities for their parents. Nevertheless, the estimates suggest that the effects of the UPP program on test scores are not driven primarily by changes in household income.

Robustness

I show in table A.6 that students in early-treated favelas (2009-2011) perform better in *Prova Brasil* for both Math and Reading exams. Due to the plausibility of treatment heterogeneity, I perform a robustness exercise with other recent difference-in-difference estimators that address the concerns this sort of heterogeneity raises. Figure 2.2 exhibits the estimation of the leads and lags coefficients for the periods before and after treatment, respectively, for these estimators. I also include the standard TWFE estimator for comparison reasons. The point estimates stay at the same level, around 0.15 standard deviation for Math and 0.1 for Reading, after the first treated wave of the exam. Then, the results are robust to the choice of different recently designed estimators that deal with issues that may arise with treatment heterogeneity.

Since *Prova Brasil*, the national standardized exam, happens every two years, some schools may be treated in the same wave of the exam, but one school receives the treatment before the other. I provide evidence that the UPP program impacts more schools treated for a more extended period. I show in Table A.7 that the results are robust and, indeed, greater than an alternative definition of the treatment: schools are treated in a specific wave if the UPP started in their favelas at least six months before the exam day.

Table A.8 provides robustness estimation for a different buffer around favelas. A school is treated if it is placed up to 250 meters from treated favelas. Analogously, I define that a school is in the control group if it locates up to 250 meters from an untreated favela. Since the UPP

[36] This result is in line with Ribeiro (2013) which shows that UPP did not have an impact on the flow of teachers from or to treated schools.

policy targets favelas ruled by drug traffickers and the criminal governance of these actors are restricted to the favelas they operate, the UPP effects should decrease with distance. That is what happens when I increase the buffer. The UPP program still positively impacts treated schools, but the point estimate is almost 0.02sd lower than the main specification.

2.4.3 Medium-run outcomes

Figure 2.3 presents the point estimates for each age the individual was when treatment started in her school/favela. There is a concave relationship between the incarceration outcomes: the younger the individual is, the lower the likelihood he ends up in prison in his early adulthood. These effects start to be statistically zero at a 90% confidence interval when the individual is aged 14 or more. Concerning the presence in the formal labor market in 2018, the UPP program did not alter the probability of having a formal job later in treated individuals regardless the how old the individual was when treatment commenced.

In table 2.7, I provide a similar piece of evidence but with a reduced-form estimation. I analyze the predicted years of exposure to treatment while in primary school[37]. I find that an additional year of exposure to treatment in primary school reduces the probability of being incarcerated by 0.006 percentage points (p-value < 0.01), which represents a 20% decrease relative to the sample mean (0.0296). As expected from the results above, there are no impacts of the UPP program on the probability of being in the formal labor market.

The UPP program, thus, could reduce incarceration for treated individuals who stay under treatment in primary school for five years or more. To glimpse the effects, I perform a back-of-the-envelope calculation to estimate the amount saved by the program in the costs of having a person incarcerated. There were 9,842 individuals in treated areas in 2008 eligible for this intensity of treatment. On average, without the treatment, 295 of these individuals ($9,842$*0.0296) would be in jail later in life. Relying on estimates of the yearly cost of incarceration of 4320 US dollars [38], in a back-of-the-envelope calculation, the UPP program could save more than 1.2 million US dollars a year. These estimates are a lower bound since they do not contain the social cost of crime or defensive investments.

Although the human capital gains related to the UPP program could not increase participation in the formal labor market, the results indicate that the treatment strongly reduced the incarceration of individuals treated at a younger age. In terms of the formal labor market results, there are some alternatives. First, if the UPP treatment deferentially increases the probability of college enrollment, treated individuals could appear later in the formal labor

[37]This analysis is similar because years of exposure in primary school strongly correlate with the age when treatment started.

[38]https://www.cnnbrasil.com.br/nacional/custo-medio-de-pessoa-presa-no-brasil-e-de-r-18-mil-por-mes-aponta-cnj/. Accessed in November 2022.

market. Although there is suggestive evidence that the learning gains dissipate over time, I intend to investigate this possibility in future research. Second, the UPP program may have increased cognitive abilities in the short run but not other abilities necessary to the formal labor market. This could also explain the null results for the formal labor market outcomes. For the incarceration outcomes, younger treated individuals are less exposed to violence and the presence of drug traffickers (Willadino et al., 2018), suggesting that the effects can be driven both by a reduced burden of violence and by fewer opportunities in the criminal market.

2.5 Interpretation

I have shown that the UPP policy caused (at least) short-term learning gains and a considerable reduction in the likelihood of being incarcerated later in life but no effects on the probability of having a job in the formal sector. Given that the UPP program focused on specific neighborhoods (place-based policy), many elements of these places' environments can explain the results above. Children and teenagers in treated areas (the focus of this paper) grow up in different socioeconomic contexts and social networks than individuals in control favelas, which could justify these outcomes.

I present evidence that some of these environmental channels did not occur. Concerning the test scores results, there is no evidence that neither peer cohorts and student composition within schools nor school infrastructure and teachers' quality are changing (tables 2.3, 2.4 and 2.5). Moreover, parental income does not explain these results. I do find, however, a large decrease of violence in treated places (figure 2.1) which translates to teachers observing less violence *within* the school and to principals reporting fewer disruptions in school routine and students absenteeism (tables 2.8 and 2.9). Taken together, these findings are consistent with effects being driven by a reduction in student cognitive burden from violence and also consistent with a reduction in learning disruptions at school. Given the existing data, it is difficult to differentiate between these explanations.

What can cause the medium-run results, and how these results possibly link to the learning gains in the short run? I partly address this question by performing heterogeneous analysis for sample subgroups. I show these comparisons in table 2.10. The subgroup analysis suggests no simple linear relationship exists between test score gains and later outcomes in life. If test scores proxy improvements in cognitive skills, boys should be expected to perform better in the formal labor market later in life, which does not seem to be the case. In fact, some evidence suggests that any human capital effects may dissipate over time (figure 2.4).

However, I observe that boys are differently more impacted by the policy. They display a bigger improvement in learning in the short run and a sizeable reduction in the likelihood of incarceration later in their lives (results for white and non-white boys in Panel B of table 2.10). Taken together, the evidence on medium-run mechanisms is most consistent with these effects being driven by a change in drug-related career opportunities for young men in favelas. The findings would also be consistent with incarceration effects being driven by changes in cognitive function associated with a less violent childhood. However, data constraints limit my ability to differentiate between these outcomes.

2.6 Conclusion and Policy Takeaways

In this paper, I analyze the short- and medium-run consequences of a place-based public policy that reduced criminal governance and exposure to violence in treated places. I exploit the fact that UPP did not induce criminal migration to control neighborhoods to estimate its impact on school outcomes, formal labor market presence, and incarceration probability. I find that the policy caused an increase of more than 0.08 sd in standardized test scores, a significant decrease in the likelihood of being incarcerated, and no impact on presence in the formal labor market. I provide evidence that this place-based policy may be a plausible instrument to improve life prospects at the neighborhood level.

The results of this paper show that the UPP program is an alternative to the status quo policing strategy of intermittent police raids that display high human and financial costs (Lessing, 2017). In these raids, police agents perform occasional tactical operations in the favelas to apprehend drugs or arrest drug traffickers. When these operations happen, it is frequent that citizens will be caught in the crossfire, and several public services will be disrupted. UPP policy changed this logic of police intervention, reduced homicides, and introduced a permanent community policing strategy in these favelas. Although it can be costly to implement this strategy, the results suggest it pays off.

There are some caveats and directions to expand this research. First, I cannot directly link students' grades in standardized test scores to medium-run outcomes. Thus, I do not observe an individual trajectory in life that passes through education until early adulthood. Instead, I exploit treatment heterogeneity to shed light on different groups that benefit from the policy. In future research, I intend to incorporate information about high-school attendance and grades, college admissions, and teenagers in contact with the Juvenile Justice system. This information would allow me to provide a complete view of schooling outcomes and explore other mechanisms related to criminal involvement.

Second, in the medium-run results, I define a treated person as she attends a treated school. Although most of the students indeed live within a 15-minute walking distance of schools, I could capture in this definition students who live outside of a treated favela as treated individuals. Since criminal governance is more prevalent within favelas' boundaries and

UPP is a place-based policy, I expect that exposure to treatment decreases with distance to a treated area. So, there may be a downward bias in the medium-run estimates. Future research needs to disentangle if these results are driven by students who live or attend a school in the treated areas but live outside the treated areas. After geocoding all the addresses in the administrative enrollment data, I will be able to shed light on this issue.

Third, an exciting extension is understanding how the drug trafficking criminal workforce changed after the UPP and then discussing how this change may alter peer effects within the classroom. After organizing the Juvenile Justice System data, I will link these individuals with administrative enrollment data, and I will be able to test some of the mechanisms I discuss in this paper.

Finally, the UPP program creates a general equilibrium shock in Rio's metropolitan region that changes this criminal market. Although I do not observe crime displacement to untreated favelas in the city of Rio de Janeiro, other areas in the metropolitan region might have suffered from crime migration. Therefore, future research must incorporate these externalities and costs while evaluating UPP's impact.

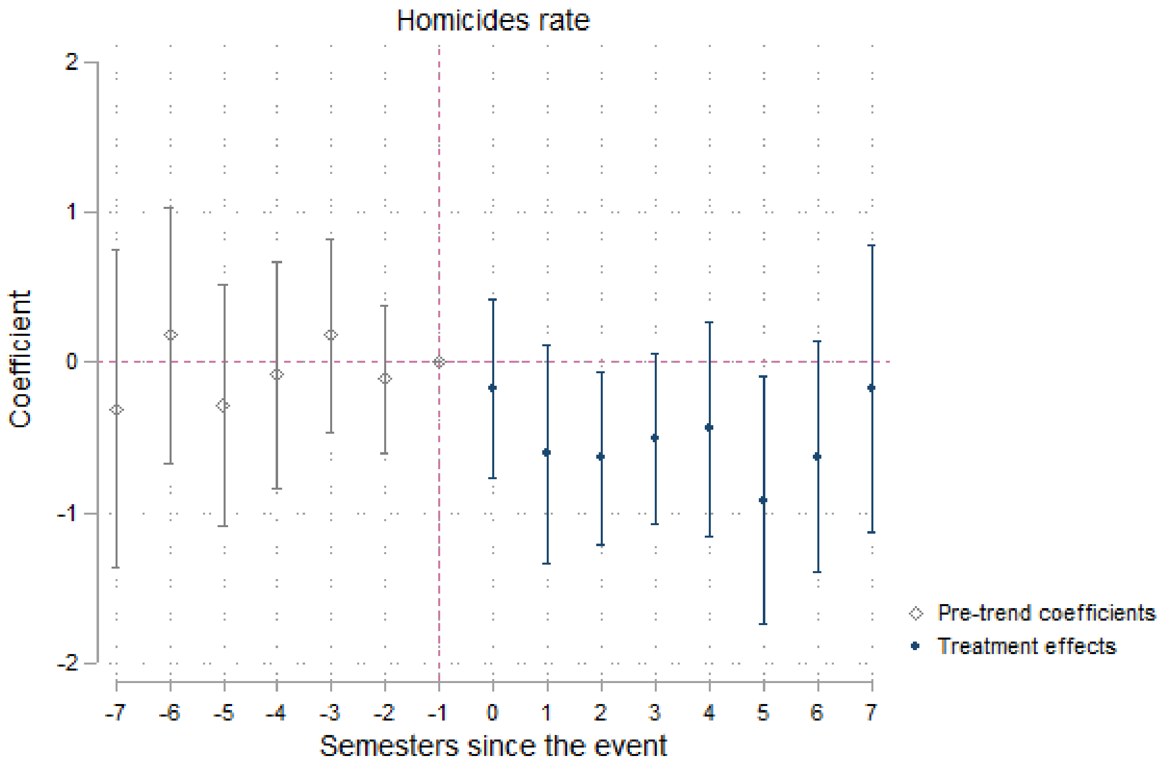
Figures and Tables

Table 2.1: Effects of the UPP treatment on violence rates

	Total homicides	Police killings	Other homicides
Panel A: Borusyak et al. (2022)			
Treat	-0.64	-0.68	-0.37
	(0.17)***	(0.32)**	(0.17)**
Obs.	518	518	518
Panel B: TWFE			
	-0.60	-0.75	-0.26
	(0.20)***	(0.28)**	(0.15)*
Obs.	740	740	740
Semester FE	Yes	Yes	Yes
Favela FE	Yes	Yes	Yes
Mean before treat.	2.84	1.80	1.99

Notes: Table shows the results for regression equation (1) using Borusyak et al. (2022) imputation and TWFE estimators. The dependent variables are inverse hyperbolic sine of semesters' rates per 100,000 citizens. Both regressions control for Semester and UPP (favela) fixed effects, have standard errors clustered at the favela level, and use population as analytical weights. Total homicides is defined by the sum of police killings and other homicides. Borusyak et al. (2022) estimator uses fewer observations because it drops observations in which all units are treated. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 2.1: Dynamic effects of UPP treatment on total homicides rates



(a) Total homicides

Notes: Figure shows the estimates for equation (2) using the TWFE estimator. I do not use Borusyak et al. (2022) estimator in these figures because the minimum effective number of observations is below the minimum recommended by the authors and the estimates may be unreliable. The dependent variable is the inverse hyperbolic sine of semesters' rates per 100,000 citizens. I control for Semester and Favela fixed effects, cluster standard errors clustered at the favela level, and use population as analytical weights. Confidence intervals are at 95%.

Table 2.2: Effects of the UPP treatment on standardized test scores

	(1)	(2)	(3)	(4)
Panel A: Math				
Treat	0.095	0.099	0.098	0.085
	(0.042)**	(0.038)***	(0.041)**	(0.037)**
Panel B: Language				
Treat	0.065	0.070	0.067	0.058
	(0.039)*	(0.036)**	(0.038)*	(0.035)*
Obs.	54,879	54,879	54,879	54,879
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	No	Students	Schools	All

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. Standard errors are clustered at favela level and dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.3: Effects of the UPP treatment on school infrastructure and teachers' composition

	Infra-structure				Teachers' Composition					
	# Employees	# Classrooms	# Computers	Infra index	# Teachers	Fem.	Non-white	College degree	Graduate degree	Age
Treat	1.106	0.308	-0.094	-0.029	1.140	-0.001	-0.029	0.032	0.014	0.025
	(2.001)	(0.215)	(1.650)	(0.103)	(0.749)	(0.015)	(0.032)	(0.023)	(0.014)	(0.394)
Obs.	690	690	690	690	690	690	665	690	689	690
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	50.01	13.94	15.40	2.109	29.10	0.803	0.404	0.696	0.287	41.98

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from School Census. The sample is restricted to years 2007, 2009, 20011, 2013 and 2015 in order to be relatable to the main regressions. Columns that refer to Infra-structure show the number of employees and classrooms at school in a year and a Infra Index that is the sum of Computer Lab, Science Lab, Sports Court and Library. Columns "# Teachers" to "Age" are at school level and are weighted by the number of teachers at the school. Columns reflect the number of teachers at school in a year, the share of female, non-white, with a college degree, if any graduate degree (specialization, master or PhD), and their mean age, respectively. These variables reflect schools' and teachers' characteristics that may influence the time a student spends at school and the quality of learning a student receives. Standard errors are clusters at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.4: Effects of the UPP treatment on student composition

	(1) # Enrollment	(2) Fem.	(3) Non-white	(4) Lives parents	(5) Mother's lit.	(6) Mother above Primary Educ.	(7) Ever Failed	(8) Ever Truant	(9) Inc. index
Treat	13.399	-0.015	0.010	0.000	-0.002	-0.013	0.023	0.011	-0.049
	(3.634)***	(0.011)	(0.006)*	(0.010)	(0.007)	(0.013)	(0.017)	(0.009)	(0.038)
Obs.	690	54,879	54,879	54,257	54,879	34,182	54,879	54,879	50,538
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	83.43	0.499	0.758	0.489	0.944	0.587	0.291	0.0848	0.003

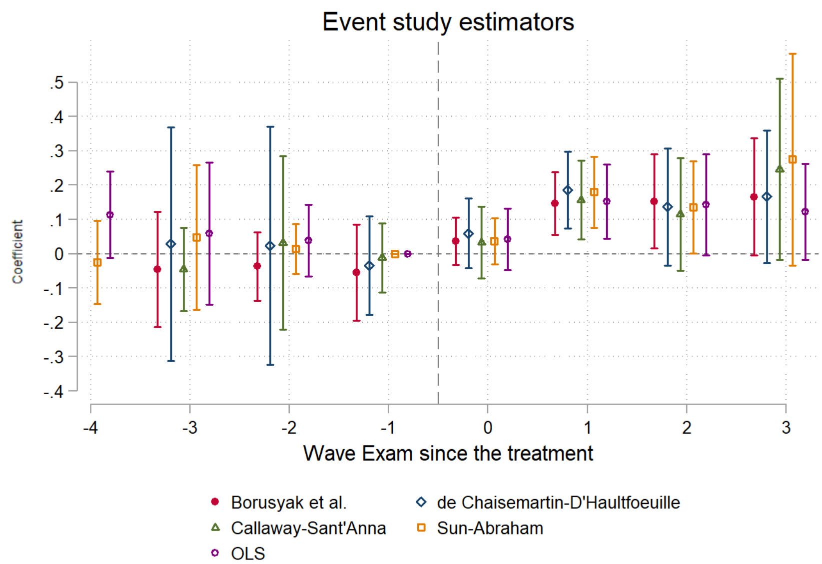
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from a survey answered by students who take the national exam *Prova Brasil*. Students' characteristics were regressed on treatment variables. The dependent variables are: female, non-white, if the student lives with both parents, if the mother is literate, if the mother has education above Primary education, if the student has ever failed a grade before, or if the student has ever been truant from a grade before. I construct an income index for column (9) based on the variables discussed above and the variable if the student doesn't work outside the home. I apply a principal component analysis by each wave (year) of exam and predict its results. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.5: Robustness for the effects of UPP treatment on standardized test scores: proxies for students' household income

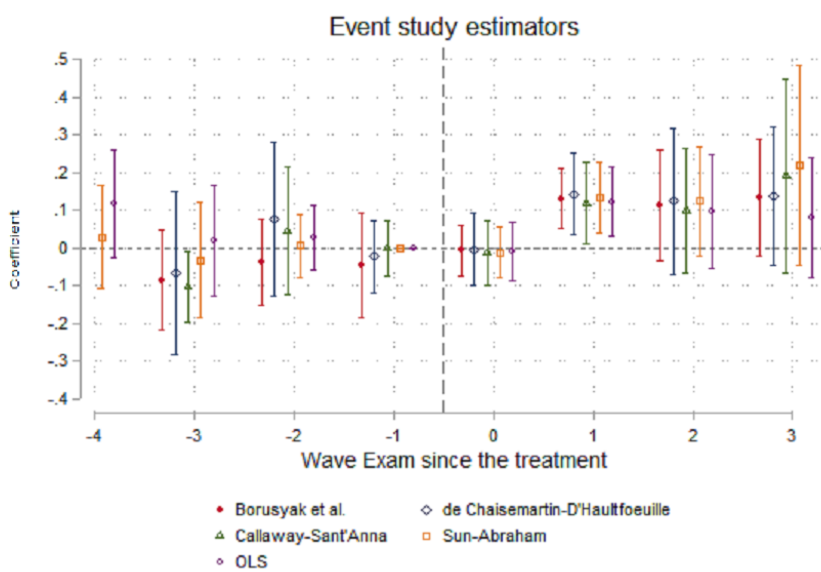
	(1)	(2)	(3)	(4)
Panel A: Math				
Treat	0.095	0.085	0.107	0.105
	(0.042)**	(0.037)**	(0.038)***	(0.039)***
Panel B: Reading				
Treat	0.065	0.058	0.077	0.075
	(0.039)*	(0.035)*	(0.034)**	(0.036)**
Obs.	54,879	54,879	50,538	50,538
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Students controls	No	Yes	Yes	Yes
Schools controls	No	Yes	Yes	Yes
Income controls	No	No	Yes	Yes - Index

Notes: Table shows the results for DD imputation (Borusyak et al., 2022) regressions with different proxies for income. First, I control for several variables that might indicate higher income such as, if the student studied in a private school at some point in his or her life, the number of bathrooms, bedrooms, televisions at home, if there is a freezer, a laundry machining, car or computer at home and if a maid works in his or her house. Second, I construct an income index based on the variables discussed above and the variable if the student doesn't work outside home. I apply a principal component analysis by each wave (year) of exam and predict its results. The other students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. In the first and second columns, I present the results for the main specification: initially controlling only for year and school fixed effects and, then, for all covariates in the main specification; in the third column results, I control for all students and school controls, and I include variables that are the proxies for income; in the forth column, I control for all students' and schools' covariates and, I replace the variables that are proxies for income to the income index I discuss above. Standard errors are clustered at favela level and dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 2.2: Robustness for the effects of UPP treatment on standardized test scores: difference-in-differences estimators



(a) Dynamic effects - Math



(b) Dynamic effects - Reading

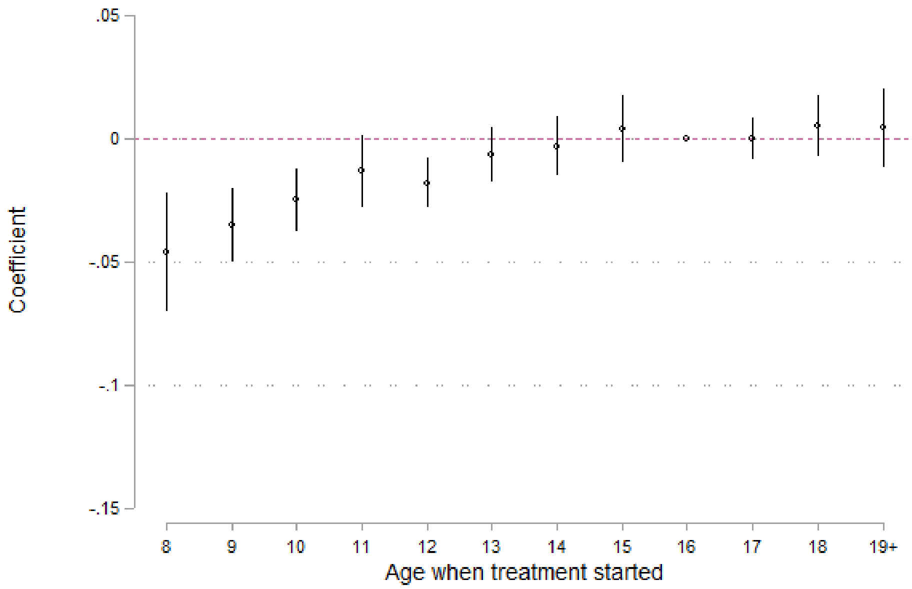
Notes: Figure shows the impact of UPPs on school outcomes by using different DD estimators, controlling for wave of exam and school fixed effects. *A priori*, the composition of students or teachers and, investments at school level can change due to the UPP program. For example, better students may move to treated schools after the pacification or schools might receive more investment following the UPP entry. In this plausible scenario, composition variables would be *mediators* of the treatment on the effects and the estimates would suffer from overcontrol bias (Cinelli et al., 2021). Given that UPP is a bundled treatment, I estimate its total effect on school outcomes in the regressions above. Confidence intervals are at 95% level.

Table 2.6: Effects of the UPP treatment on educational indicators

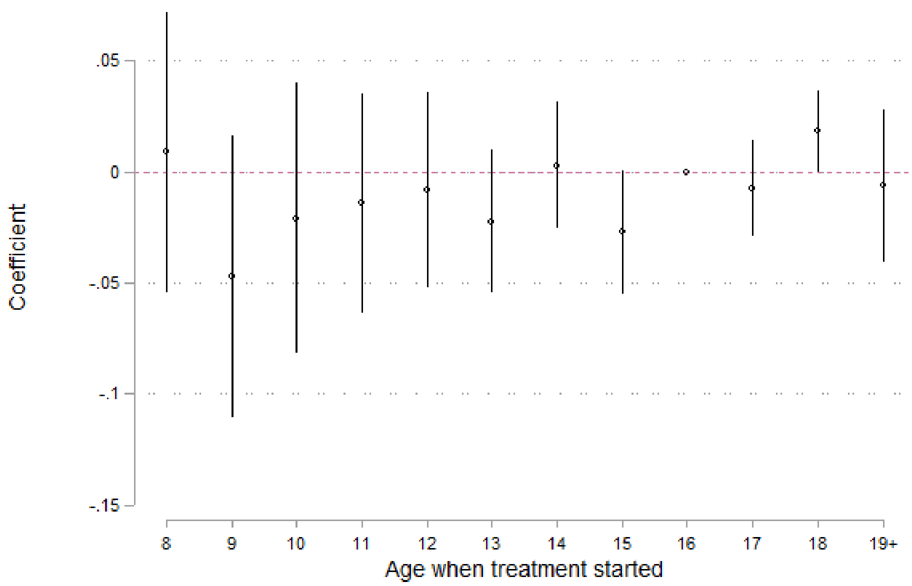
	(1) Pass rate	(2) Fail rate	(3) Dropout rate	(4) Age-grade dist. rate
Treat	0.23	-0.80	0.56	-0.10
	(0.61)	(0.69)	(0.47)	(1.18)
Observations	690	690	690	690
Year FE	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes
Mean Dep. Var (%)	87.93	9.85	2.23	25.06

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from annual School Census data. Columns (1) to (4) are weighted by the number of students enrolled at the school. Dependent variables are the approval (pass rate), failed, drop out and age-grade distortion rates for elementary and middle school. Age-grade distortion is defined by the number of students who are more than two years behind the grade she should be. Standard errors are clusters at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 2.3: Medium-run effects of the UPP treatment by age when treatment started in the school



(a) Incarceration



(b) Formal Labor market

Notes: The figure displays the estimates from equation (4). It shows that how the effects of UPP program vary depending on the age the individual was when treatment started in her school. The dots represent the point estimates and the lines the 95% confidence interval.

Table 2.7: Medium-run results for UPP treatment: years of exposure to treatment while in primary school

	Incarceration	RAIS
	(1)	(2)
EUPP	-0.006	-0.002
	(0.002)***	(0.005)
Observations	61,635	61,635
Year FE	Yes	Yes
School	Yes	Yes
Controls	Yes	Yes
Mean Dep. Var	0.0296	0.291

Notes: Table shows the results for the empirical specification shown in equation 5. The dependent variable is a dummy that turns one if the student appears in the formal labor market (RAIS) or it is incarcerated in 2018. Exposure to UPP is measured by the predicted number of years a student stays in primary school during the UPP program. Controls include gender, race, mother education, father registered, father deceased, lives with mother, enrolled in Social Service. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.8: Effects of UPP treatment on school environment

	Did the school suffer from disruption of scholar activities this year?	*Did the school suffer from lack financial resources this year?*	*Did the school suffer from teachers' absenteeism this year?*	*Did the school suffer from students' absenteeism this year?*	*Did the school suffer from teachers' turnover this year?*	*Does the school regularly offer sports classes in extracurricular activities?*	*Does the school regularly offer arts classes in extracurricular activities?*
Treat	-0.43	-0.00	0.04	-0.10	0.05	0.03	0.04
	(0.09)***	(0.05)	(0.05)	(0.05)**	(0.05)	(0.06)	(0.05)
Obs.	666	664	667	668	667	670	670
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	0.348	0.295	0.148	0.445	0.172	0.870	0.842

Notes: Table shows the results of DD imputation (Borusyak et al., 2022) regressions for outcomes related to principals' perceptions about school problems. The answers come from the Principals' Surveys from Prova Brasil and indicate if problem happened in that academic year. Standard errors are clustered at the favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.9: Effects of UPP treatment on expectations and violence *within* school

	Do you believe that more than half of your students will graduate from high school?	*Do you believe that more than half of your students will attend college?*	*Did an event of verbal or physical aggression happen against teachers or employees this last year?*	*Did an event of verbal or physical aggression happen against students this last year?*
Panel A: Elementary School				
Treat	0.155	0.052	0.049	0.071
	(0.053)***	(0.057)	(0.045)	(0.046)
Obs.	1,589	1,584	1,598	1,575
Mean Dep. Var	0.735	0.268	0.638	0.680
Panel B: Middle School				
Treat	0.121	-0.089	-0.144	-0.241
	(0.071)*	(0.056)	(0.066)**	(0.063)***
Obs.	748	749	778	766
Mean Dep. Var	0.731	0.119	0.842	0.805
Year FE	*Yes*	*Yes*	*Yes*	
School FE	*Yes*	*Yes*	*Yes*	

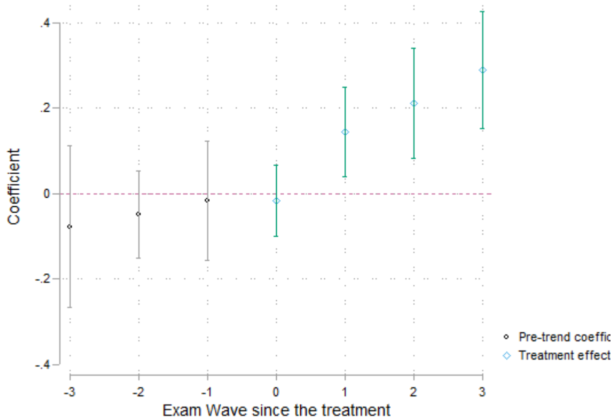
Notes: Table shows the results of DD imputation (Borusyak et al., 2022) regressions for outcomes related to teachers' expectations of students and exposure to violence within school. Answers come from the Teachers' Survey from Prova Brasil. Standard errors are clustered at the favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.10: Heterogeneity of the effects of UPP treatment on short- and medium-run outcomes

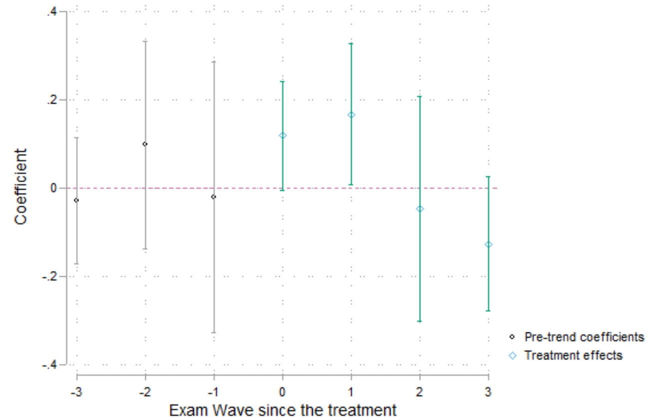
	Girls	Boys	White boys	Non-white boys
Panel A: Test Scores				
Math				
Treat	0.06	0.13	0.20	0.10
	(0.04)	(0.04)***	(0.06)***	(0.05)**
Mean Dep. Var	0.00	0.00	0.00	0.00
Reading				
Treat	0.06	0.08	0.14	0.06
	(0.04)	(0.04)*	(0.05)***	(0.05)
Mean Dep. Var	0.00	0.00	0.00	0.00
Obs.	27,360	27,519	6,670	20,840
Panel B: Incarceration				
EUPP	-0.001	-0.010	-0.007	-0.012
	(0.001)	(0.003)***	(0.004)*	(0.003)***
Obs.	29,938	31,697	9,859	21,838
Mean Dep. Var	0.002	0.056	0.040	0.062
Panel C: Formal Labor Market				
EUPP	-0.003	-0.002	-0.001	-0.003
	(0.006)	(0.006)	(0.010)	(0.007)
Obs.	29,938	31,697	9,859	21,838
Mean Dep. Var	0.279	0.303	0.315	0.297
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*
Controls	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. I re-standardized the dependent variables for each subgroup for this table. Thus, the dependent variable is the standardized test score by topic of the exam, year of the exam, students' grade, gender and race (when applied). Students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. Standard errors are clustered at favela level and dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

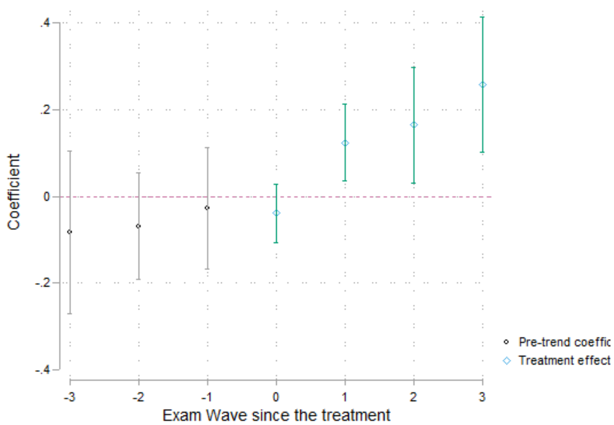
Figure 2.4: Heterogeneity of UPP treatment effects on schooling by grades - Dynamic effects



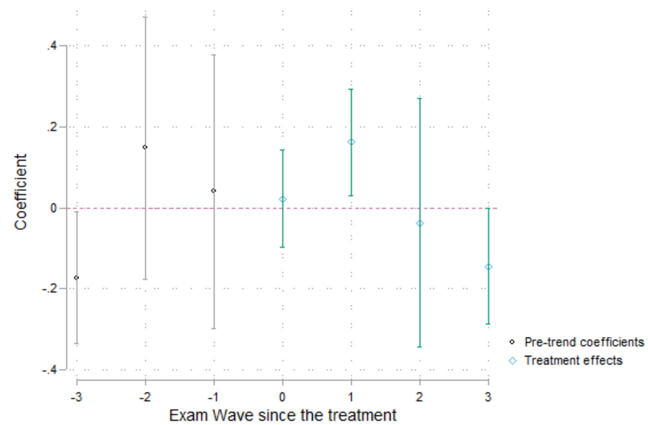
(a) Math - 5th grade



(b) Math - 9th grade



(c) Reading - 5th grade



(d) Reading - 9th grade

Chapter 3

The Impacts of UPP on Violence

3.1 Introduction

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

In many urban areas, criminal groups rule non-contiguous territories within the city or even in more than one city. This spatial feature facilitates the mobility of criminals to non-adjacent regions, reducing the cost of coordinated actions in different localities, and imposes several challenges for the government to design policies that reduce the territorial control of these groups. For example, place-based policies focused on restoring the presence of the state in these areas ruled by criminal groups may induce spatial spillovers to untreated areas. The spatial spillovers created by this type of policy could be either positive or negative. On the

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

one hand, drug gangs could optimally choose a decrease in criminal activities in non-treated places to reduce the probability of receiving the treatment in other parts of their territory. In this case, I would observe a crime deterrence effect of the policy (Becker, 1968; Chalfin and McCrary, 2017). On the other hand, criminals could migrate to other areas ruled by their gangs and continue their criminal activities. Given the criminal context and the possible responses to the treatment, it is not apparent *ex-ante* which areas are prone to receive positive or negative spatial externalities.

The spatial distribution of criminal criminals also creates econometric challenges when evaluating policies focused on territorial control. This spatial criminal structure can be characterized by a network of allies and enemies in space that is largely unknown to researchers due to the lack of data. Thus, any critical shock in parts of this network can easily propagate to units further away, creating local responses in areas that are hard to predict *ex-ante*. Thus, it is difficult to define 'pure' control units that are not affected by the policy.

This paper focuses on the spatial consequences of a large place-based policy that focused on reclaiming territorial control back to the state of areas under the previous domination of criminal groups in an urban context. In the first chapter of the Thesis, I use geocoded violence data for ever-treated favelas, and I show that the UPP policy reduces total homicides and police killings in treated favelas. However, there is no official geocoded violence information for large untreated favelas. Therefore, I could not address spatial spillover concerns in these never-treated units. In this chapter, I overcome this lack of information by using more aggregated data at the police station level. This data covers police stations with large untreated favelas within its boundaries in the city of Rio and also provides information for police stations in the metropolitan area and the countryside of Rio de Janeiro State, which allows me to address spatial spillover concerns to these other untreated units as well.

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

To estimate the impacts of the UPP on violence using data at the police station level, I define a police station treated if there is at least one large favela treated within its boundaries. Analogously, I define a police station within the city of Rio as control if there is a large untreated favela in its domain. I opt for this treatment definition because it is comparable to the first chapter (treatment and control defined the presence of UPP in a large favela), and I can provide suggestive evidence of spatial spillover to the large untreated favelas, although at a coarser geographical definition. Moreover, for empirical exercises using police stations outside the city (metropolitan area or the countryside), I define all these as untreated. I separate them into different geographic regions defined by their criminal characteristics.

I perform some empirical exercises to shed light on crime displacement to areas in the city, in the metropolitan area, and the countryside of Rio de Janeiro State. I find that the policy reduced violence in treated places and induced a response in control units (untreated police stations with large favelas in its boundaries in the city of Rio) that *lowered* crime in these areas. However, there is suggestive evidence that the UPP partially displaced crime to other regions outside the city (metropolitan area and countryside).

There are few papers examining public policies intended to reduce the control of parts of the urban territory by criminal gangs that could move to other non-contiguous places in the city or metropolitan area. Close to this paper, (Magaloni et al., 2020) and Ferraz et al. (2015) evaluate the Pacifying Police Unit Program in Rio, but without explicitly addressing the migration problem in a unified econometric framework. Other papers provide evidence that spatial spillover may be either positive or negative. Blattman et al. (2021) find negative spatial spillovers to nearby areas caused by a place-based intervention targeted in hot spots in Bogota and Dell (2015) evaluates the causal impacts of large-scale interventions to combat trafficking sponsored by Mexican conservative party PAN on drug-related violence. Her results suggest negative spillover effects to places where multiple drug routes coincide. Draca et al. (2011) and Di Tella and Schargrodsky (2004) discover no evidence of spatial spillover due to spatially targeted interventions.

Verbitsky-Savitz and Raudenbush (2012) estimate the impact of a community-policing program established in Chicago using a 'buffer' treatment group given by neighboring units to treatment localities. There is a growing literature that addresses this type of estimation more formally. Clarke (2017) and Butts (2021) propose frameworks to estimate difference-in-differences in the presence of spatial spillovers. Clarke (2017) generalizes the concept of a neighborhood by defining a flexible distance metric and assuming that the treatment effect decays as distance to treatment increases, and Butts (2021) shows that defining a flexible weighting matrix removes biases and allows to estimate the total effects of the treatment. Importantly, though, this matrix has to be known by the econometrician, and some of the weights have to be zero. Given Rio's criminal context, in which the network of alliances and enemies is unknown to the researcher, I leave the implementation of these new difference-in-differences estimators with spillovers for future research. For this paper, I focus on providing suggestive evidence of the effect of the policy in untreated areas and other cities in the metropolitan area.

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

3.2 Context

In the first chapter of the Thesis, I describe the importance of territorial control for the criminal organizations in Rio and the UPP program in detail. In this chapter, I focus on two essential aspects of this context: i) drug factions rule in non-contiguous areas of the territory, which generates an unobservable (to the researcher) network of alliances among favelas under the same drug faction flag; ii) arguably, the UPP program creates general equilibrium effects that impact the criminal market in Rio's metropolitan and countryside regions. I also discuss that the program induces distinctive incentives for different criminal actors and drug factions in this market.

3.2.1 Spatial Distribution of Drug Factions

Initially, I discuss the geographical boundaries of Rio's criminal market. Historically, the decision area for criminal actors was Rio's metropolitan area (Misse, 2007). The rise of drug gangs in the 1980s based on local criminal governance at the favela level and on the projection of power within the prison system strengthened the metropolitan region as the primary geographical criminal market in Rio for two reasons: first, criminal actors need to maintain their criminal governance in the favelas they rule. Then, if other drug gangs threaten their positions, they can rely on criminal support from other favelas under the same drug faction rule. Since the drug faction is present in different areas of the metropolitan region, they could ask for help from any other favela in this area, potentially reinforcing the non-contiguous network of alliances.

The second reason is that, given that these factions act within the prison system and that there is a high probability that drug traffickers go to jail at some point in their lives (Lessing, 2017), criminal actors can create and fortify ties with criminal actors from other favelas while in prison. Besides, drug traffickers in Rio's metropolitan region, if caught, go to the same set of prisons, which facilitates the exchange of information among criminal actors in this geographical boundary. Thus, the prison system also creates incentives for the presence of a non-contiguous criminal network and reinforces the metropolitan area as the locus of this illicit market.

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

Figures 3.7 and 3.8 display how the criminal groups are spread over the city and the state of Rio and also how the spatial distribution of drug factions changes over time. I rely on different sources to create the evolution of the spatial distribution of criminal groups over time. I geocoded maps from other researchers' sources only for the city of Rio de Janeiro. I use more recent maps created by a group of journalists and criminologists that shed light on the spatial distribution in Rio's metropolitan region and the countryside of the State. Note that the criminal groups are spread over the metro region and even in areas in the countryside of the state of Rio de Janeiro. Since 2006, militias have grown in Rio's West Zone and part of the metropolitan region known as Baixada.

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

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

In terms of drug factions exposed to the policy, most of the Pacification Police Units were installed in areas from the Red Command, which caused an unanticipated economic shock to this faction (Misse, 2011; Zaluar, 2012; Muggah, 2017). Until 2009, Red Command was strongest in South Zone and the North Zone of Rio and most of the Pacification Police Units locate in these areas (Rodrigues, 2013)[2]. The militias received only one UPP in their territory.

Using ethnographic and survey data, Zaluar (2012) estimated that in 2005 Red Command (CV) was the strongest drug faction, controlling half of the slums while the other gangs controlled 20 percent. Militias were present in around 10 percent of the favelas. After the beginning of the UPP policy at the end of 2008, the spatial distribution of these factions changed. In the same study, (Zaluar, 2012) shows that militia groups increased and became the strongest territorial gang, controlling 45% of the slums. CV lost territorial power and had 30% of the shantytowns. The other factions kept roughly the same percentage of favelas.

3.2.2 Police structure in Rio

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

There are 40 police battalions in the State of Rio de Janeiro, of which 18 are only in the city of Rio. Meanwhile, there are 130 police stations in the State and 39 in the city of Rio de Janeiro. In terms of workforce, there are around 45,000 Military Police officers[3] and 9,000 thousand Civil agents[4].

The commander of each police battalion has some leverage to allocate military police agents in the territory, which can create spatial correlations within the boundaries of a division.

[2] Figure 3.7 (a) shows the spatial distribution of the drug factions in 2006, before the beginning of the program.

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

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

Therefore, I opt to cluster the standard errors in the empirical specifications below at the police battalion level.

3.3 Data

To overcome the lack of geocoded episodes of violence for the whole city of Rio (or, at least, aggregated at the favela level)[5], I use police station data, a coarser geographic unit than large favelas, to address spatial spillover concerns to large untreated favelas. That is, since the city of Rio does not provide geocoded violence information for all of the large favelas, I use the minimum geographical unit with consistent official data throughout the years – which is at the police station level.

I rely on official crime data from the Institute of Public Security (ISP-RJ), the statistical and data intelligence office of the Secretary of Public Security (SSP-RJ). ISP-RJ consolidates crime incident information and makes the data monthly available. In this chapter, I utilize monthly data from 2004 to 2016 at the police station level.

Police station boundaries may change over time due to the re-optimization of policing strategy. Some police stations are created after the beginning of our database. To avoid missing observations in an unbalanced panel of police stations over time, I aggregated the statistics at the police stations before the changes. I do the same procedure for any other police station information that may change over time: I consider the information related to the police station before the change[6]. In Appendix B.1, I show how I coded these changes.

I utilize the official shapefiles with the precise boundaries for police stations and UPPs from the Institute of Public Security (ISP-RJ) and the shapefile for large favelas from MPRJ In Loco[7], a data aggregator project from Rio de Janeiro's Public Attorney Office. Figure 3.3 (a) displays the distribution of large favelas in Rio on top of the police station's boundaries. I define that a police station is treated if at least one large favela is treated within its limits. If there is more than one large treated favela, I consider the treatment time as the first period in which a favela was treated. A control police station has at least one large untreated favela in its domain and no treated favelas. Figure 3.3 (b) exhibits the police stations treated and in the control for the city of Rio de Janeiro. I opt for this definition for treated and control police stations in the city of Rio to shed light on possible spatial spillovers that might have occurred in untreated large favelas. Although police station data is coarser than the favela level, it is the maximum spatial disaggregation which I can get comparable violence data for

[5]In the First Chapter, I used geocoded data aggregated at ever-treated large favelas. Thus, I observe data at the favela level but only for ever-treated favelas.

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

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

treated and control places because geocoded violence data is not available for all of these large favelas.

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

Figure 3.4 presents the evolution of crime variables over time, and table 3.1 shows the summary statistics for violence outcomes two periods before and one after treatment for police stations in the city of Rio. Control police stations are more violent in the baseline than treated areas. The semester average total homicide rate per 100,000 citizens for the years before the beginning of the policy (2007 and 2008) is slightly smaller ($p < 0.1$) for treated places (24.1) than for control police stations (30.4). The semester police killings rate is statistically the same for treated and control police stations: 7.7 and 6.8 ($p = 0.54$), respectively. Although the violence levels may differ, there are no clear differential trends in periods before the treatment, which is vital for my empirical strategy.

Figure 3.8 shows the spatial distribution of drug factions in areas of the metropolitan region and the countryside. I separate the police stations into groups based on the socioeconomic and criminal characteristics of each area. The region of *Baixada* is composed of poor municipalities with the historical presence of death squads and paramilitary groups (Alves, 2003; Cano and Duarte, 2012); *Niteroi* is a rich neighborhood in the metropolitan region with the presence of the drug gangs in some parts of its territory; *Sao Goncalo* a poor municipality in which drug factions also operate in some areas, and the *Countryside* defined by the rest of police stations in Rio de Janeiro State – these areas are more heterogeneous in terms of socioeconomic characteristics and there is anecdotal evidence that criminal groups migrate to these cities after the UPP policy.

Figure 3.6 shows the time series for violence indicators for these groups. I added to this figure, the evolution of treated and control areas in the city of Rio for comparison reasons. All the regions display a similar downward trend before the beginning of the UPP policy in 2009. After the policy, the trends differ: while violence indicators keep decreasing for treated and control police stations in the city of Rio, there are inflection points in the trends for other regions outside the city.

3.4 Empirical Strategy

There are two main differences between the sample I use in this chapter to the sample in the First Chapter: i) there are never-treated units defined by police stations that have at least one untreated large favela and no treated units within their boundaries, which I refer to as the control group from now on and ii) there are monthly data since 2004, which I use to increase the sample size. I construct semester violence rates from monthly data, and I apply an inverse hyperbolic sine transformation in the dependent variable to reduce concerns raised by the positive skewness of the data.

I follow the same empirical strategy as the First Chapter but with dependent and treatment variables defined at the police station level. I exploit the staggered introduction of the policy to estimate the impact of UPP treatment on violence measures. The identification relies on parallel trends and no anticipation assumptions. Hadn't the treatment occurred, the trajectory of treated areas would follow a similar path to control units.

The estimation equation is:

$$Y_{it} = \lambda_i + \delta_t + \beta D_{it} + \epsilon_{it} \tag{3.1}$$

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

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

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

$$Y_{it} = \lambda_i + \delta_t + \sum_{\tau=-7}^{-2} \gamma_\tau D_{i\tau} + \sum_{\tau=0}^{7} \beta_\tau D_{i\tau} + \epsilon_{it} \tag{3.2}$$

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

The dependent variables are the inverse hyperbolic sine transformation of semester rates of violence indicators. I use three main violence measures: total homicides, police killings, and other homicides. Total homicides are defined by the sum of police killings and other homicides. Police killings refer to homicides committed by on-duty police officers. Usually, these events happen during police raids within the favelas (Monteiro et al., 2020) when the police perform military operations with the goals of arresting drug criminals and apprehending drugs. Given that criminal organizations control the territory, police raids often cause a shootout between police officers and drug traffickers (Hirata et al., 2022). The last violence indicator is other homicides. The variable measures homicides caused by interpersonal violence.

3.5 Results and Discussion

Figure 3.2 exhibits the evolution of homicides and police killings in the city of Rio de Janeiro. There has been a downward trend for both crime indicators since 2009. Considering that the city of Rio has treated and control units, these figures suggest that if there is crime displacement to control areas in the city, the reduction in violence in treated police stations would have to be larger than the displacement effect.

I split the time series for treated and control areas in Rio in figure 3.4. These graphs show that both units display a similar pattern after 2009. The two time series are strongly linearly correlated. The Pearson correlation index is 0.94 for homicides and 0.86 for police killings. These figures suggest that, at least on average, there is no evidence for possible crime displacement to control areas.

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

This suggestive evidence is relevant for the estimation in the First Chapter. If crime displacement happens to units in the control group, the findings for school and medium-run results would be overestimated. Given the findings in this chapter, the estimates in the First Chapter would be a lower bound for the effects of UPP on school and medium-run outcomes.

Despite these downward trends in both treated and control groups, the results for the empirical specification in equation (1) in table 3.2 show that the UPP program did have a differential reduction in total homicides and police killings compared to control areas. The estimates indicate a decrease of around 7% of homicides and 25% of police killings. The results are robust to specifications that deal with the skewness of the dependent variables caused by the count data. Figure 3.5 presents the dynamic effects of the policy. Police killings and homicides remain lower than in control areas up to 6 semesters after the start of the UPP treatment. Importantly, there is no evidence of pre-trends.

Table 3.2 and figure 3.5 show that reductions in police killings drive decreased violence in treated police stations. This result is consistent with the change of police strategy in treated places. Instead of transitory police raids that often caused shootings in the favelas, the UPP program focused on permanent settlements within treated areas, which increased law enforcement and the cost of doing drug business there, reducing violent episodes.

These outcomes add to the results in the First Chapter of the Thesis. Even at the police station level, a coarser geographical area, the UPP program reduces lethal violence. The impacts on total homicides are smaller but expected since the UPP policy targeted violence related to drug trafficking inside the favelas. If anything, the police station level estimation also incorporates any spatial spillovers to treated areas outside the favelas in a treated police station. So, the effects on total homicides suggest that the UPP reduced this type of violence *accounting for* local spillovers. Notably, the estimates for police killings point in the same direction as in the First Chapter. Considering that most police killings happen within the favelas (Monteiro et al., 2020) and the UPP program focused on changing the logic of policing inside the favelas, these outcomes are reassuring. Finally, the consequences of the treatment on other homicides are somewhat expected. The dynamics of other homicides outside the favelas may be different than within the favelas. Although the UPP program could have impacted this type of violence, it was not the focus of the policy.

Thus, the UPP program led to a response of criminal actors in treated favelas. They could stay in these favelas and adapt to a new equilibrium with police officers continuously operating there, migrate to other favelas, or leave the drug business. Indeed, some criminal actors adapted to the new policy and became more discreet in their routine operations (Serrano-Berthet et al., 2012), which is consistent with the results above, and some migrated to other favelas (Menezes, 2018).

There is anecdotal evidence that drug traffickers migrated to places I do not consider in the First Chapter, such as the Metropolitan Region and the countryside of the state of Rio (Willadino et al., 2018). I do not intend to estimate the general equilibrium effects of the policy in this paper, and I leave it for future research. However, I provide suggestive evidence that crime displacement to other areas in the state of Rio de Janeiro is possible.

First, I analyze the temporal evolution of crime indicators to these areas in figure 3.6. The figure suggests that both treated and control police stations in the city of Rio followed similar trends as the other areas before the program started and until 2012. After this period, the trends start to diverge as violence levels increase in other areas. Second, I show in table 3.4 that DD estimates using these different samples as control groups are not stable.

Conceptually, under the parallel trends and SUTVA assumptions, any group that captures contemporaneous shocks could be a suitable control group for treated units. In these cases, the control group imputes the temporal trends after the treatment and provides a counterfactual to the treated group. Assuming that the criminal market was in equilibrium before the beginning of the UPP program, common shocks to this market are expected to affect the players similarly. Thus, under the assumptions discussed, the coefficients should be stable regardless of the control group used. That is not the case. The findings suggest that all other units in the state of Rio receive idiosyncratic shocks around the same period or, more likely, criminals in these areas respond to the UPP program in the city of Rio and migrate to these areas (Miagusko, 2016).

Taken together, these findings suggest that the policy reduced violence in treated places, induced a response in control units that lowered crime in these areas, and partially displaced crime to other regions in the state of Rio.

3.6 Conclusion and Further Steps

Several cities worldwide face urban violence problems that could be magnified by the presence of organized crime that dominates some areas in the urban scenario. This paper addresses the spatial consequences of a public policy - the Pacification Police Units program (UPP) -

designed to reduce the territorial control of drug gangs. These criminal organizations operate in non-contiguous parts of the metropolitan area of the city of Rio de Janeiro, which impose a challenge to estimate the impact of a place-based intervention such as the UPP.

I find that the UPP program decreases crime outcomes in treated areas and reduces crime in untreated police stations in the city of Rio that have at least one large favela within their boundaries. This last finding is consistent with a crime deterrence interpretation. Therefore, there is no evidence that using these units as control groups overestimates the conclusions of the First Chapter of the Thesis. There is suggestive evidence of negative spillovers to untreated regions in the metropolitan area and the countryside of the state of Rio, though.

In general, the empirical exercises in this paper suggest the importance of careful consideration of spatial spillovers while evaluating or designing this type of place-based policy. Considering untreated places that received negative spillovers as a control group might bias the results and distort the cost-benefit analysis of the program. I intend to address how the UPP program changed the equilibrium of this criminal market in future research.

First, the evolution of drug factions and militias over time creates the possibility of estimating the spatial game played by these criminal groups using a revealed preference argument for the value of a favela (Seim, 2006; Adda et al., 2014). With the distribution of drug factions and militias over time, I could define places of conflict and correlate these areas with the economic, demographic, and geographical characteristics of these places. Ideally, this could shed light on the strategies played by the agents in the spatial game. Moreover, I can use another source of homicide data from DATASUS, compiled at the neighborhood level (finer than what I used in this paper), to capture the territorial conflicts of these factions more precisely.

Second, I intend to discuss alternative policies to minimize the probability of migration or even analyze the optimal selection of places to receive the program, considering the migration responses of criminals. In this case, I could build on Fu and Wolpin (2018), who studied the optimal allocation of police forces across cities in the US, extending criminals' choice set to incorporate a locational choice as a response to the treatment.

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

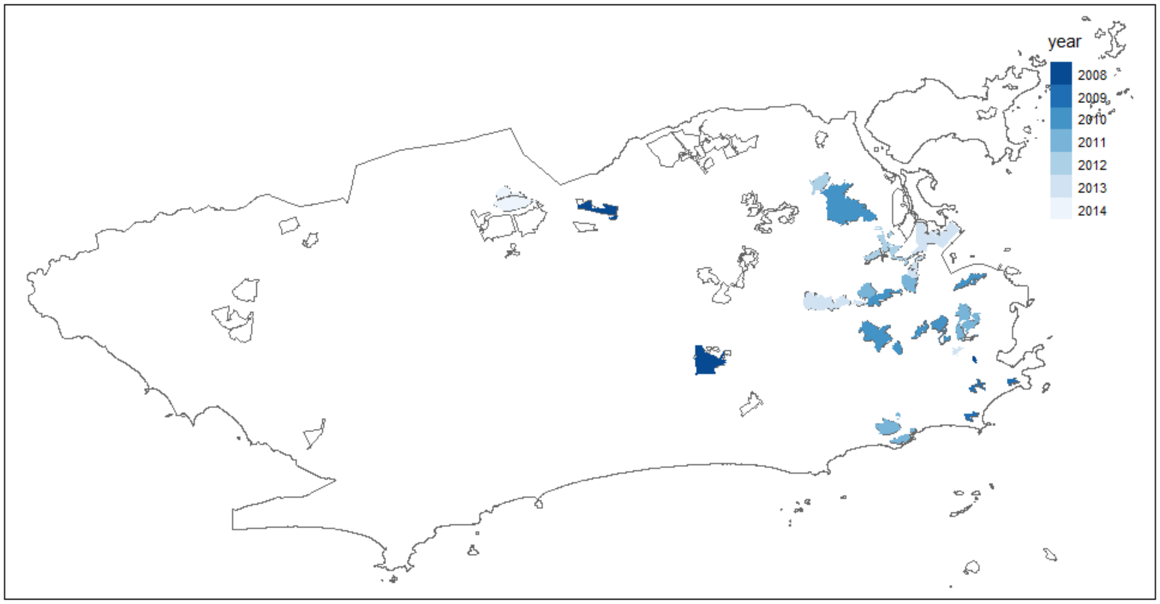


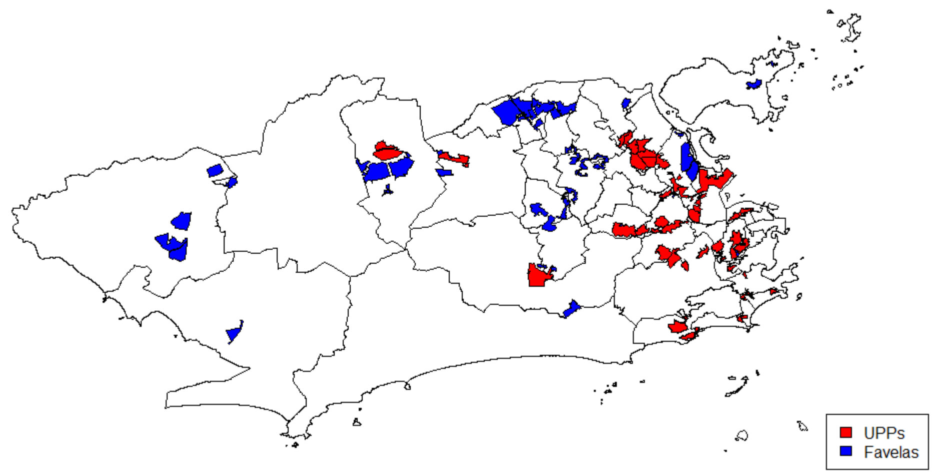
Figure 3.1: Large favelas by treatment year

Figure 3.2: Temporal evolution for violence indicators for the city of Rio de Janeiro

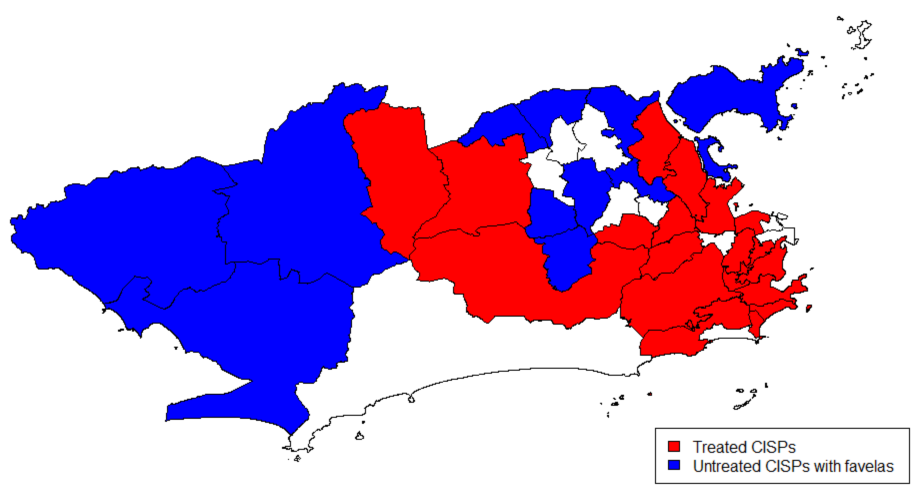
(a) Total homicides rate

(b) Police killings rate

Figure 3.3: Treated and Control police stations in Rio de Janeiro



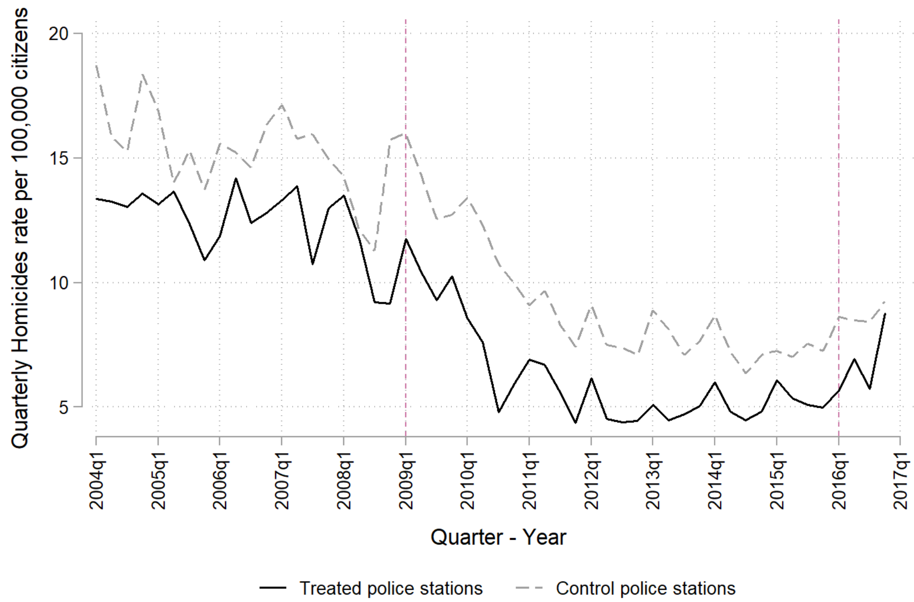
(a) Large favelas and Police Stations boundaries



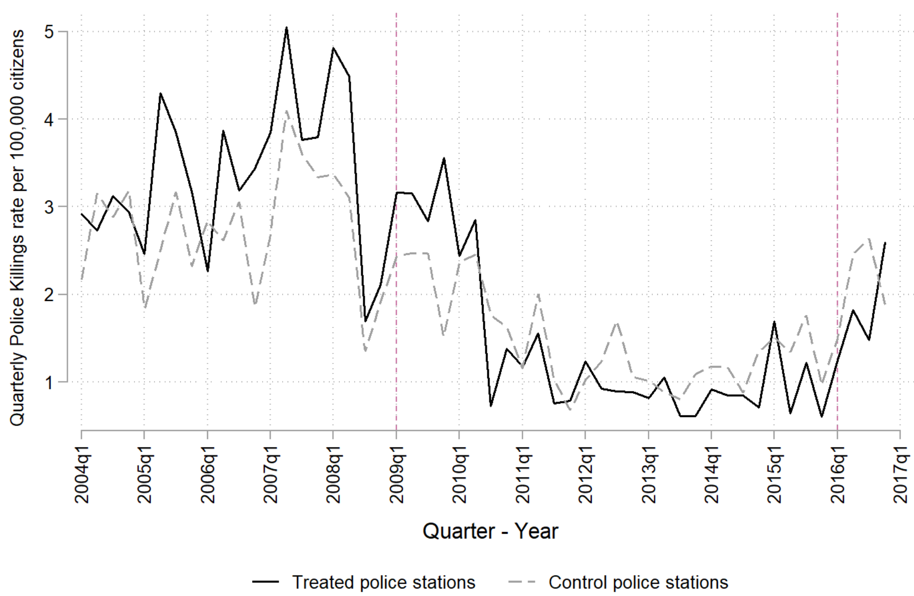
(b) Treated and Control Police Stations

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

Figure 3.4: Temporal evolution for violence indicators for treated and control stations in the city of Rio de Janeiro

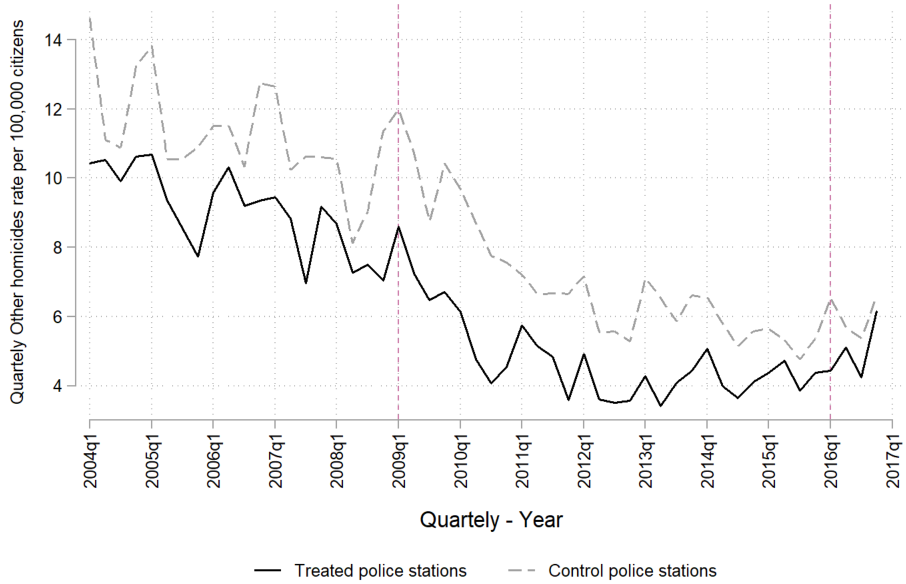


(a) Total homicides rates



(b) Police killings rate

Figure 3.4: Temporal evolution for violence indicators for treated and control stations in the city of Rio de Janeiro (cont.)



(c) Other homicides rate

Table 3.1: Summary statistics violence indicators per period of time

Period	Treat	Control	T-test
Panel A: Total homicides rate			
2004-2006	27.1	32.3	0.12
2007-2008	24.1	30.4	0.08
2009-2016	12.6	19.4	0.00
Panel B: Police killings rate			
2004-2006	6.7	6.1	0.56
2007-2008	7.7	6.8	0.54
2009-2016	3.0	3.7	0.12
Panel C: Other homicides rate			
2004-2006	20.4	26.1	0.03
2007-2008	16.4	23.6	0.01
2009-2016	9.6	15.7	0.00

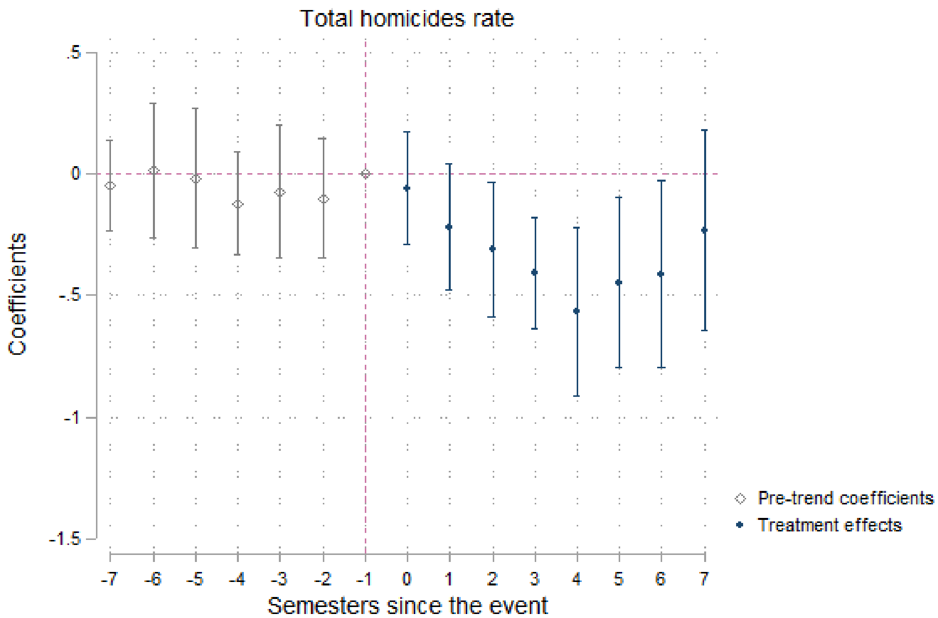
Notes: Table shows the summary statistics for homicide indicators for treated and untreated police stations in different periods of time. Total homicides are defined as the sum of police killings and other homicides. I divide the number of homicides in a semester by the population in each police station to construct the semester rates per 100,000 individuals. Column (1) displays the mean of these semester rates for treated places, column (2) the mean for untreated police stations, and column (3) shows a T-test for the differences of the means.

Table 3.2: Effects of the UPP on lethal violence

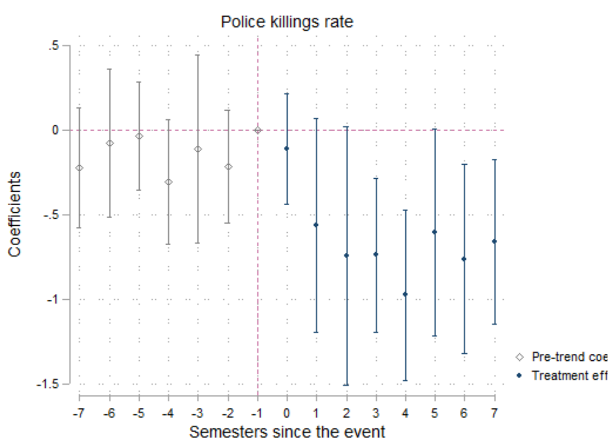
	Total homicides	Police Killings	Other homicides
Panel A: Borusyak et al. (2022)			
Treat	-0.23	-0.49	-0.13
	(0.13)*	(0.11)***	(0.13)
Panel B: TWFE			
Treat	-0.25	-0.47	-0.16
	(0.11)**	(0.12)***	(0.11)
Obs.	754	754	754
Semester FE	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes
Mean before treat.	3.57	1.91	3.29

Notes: Table shows the results for regression equation (1) using Borusyak et al. (2022) imputation and TWFE estimators. The dependent variables are inverse hyperbolic sine of semesters' rates per 100,000 citizens. Both regressions control for Semester and Police station fixed effects, have standard errors clustered at the police battalion level, and use population as analytical weights. * significant at 10%; ** significant at 5%; *** significant at 1%.

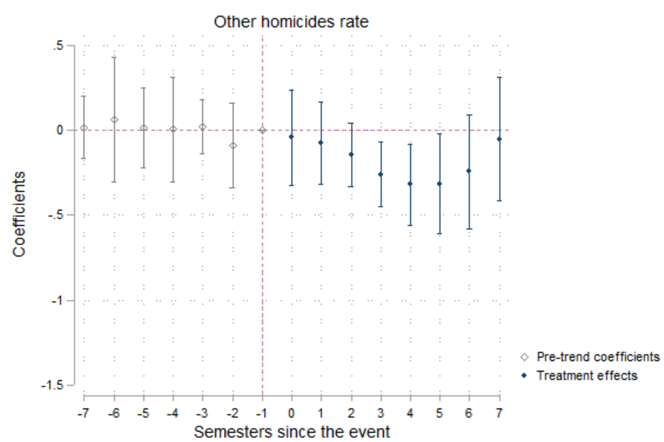
Figure 3.5: Dynamic effects of UPP on violence indicators



(a) Total homicides rates



(b) Police killings rate



(c) Other homicides rate

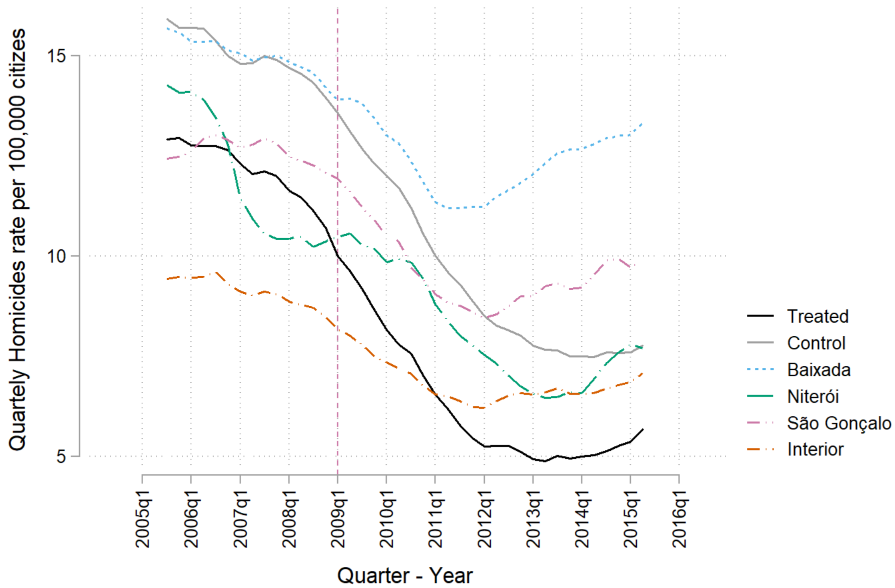
Notes: Figure shows the estimates for equation (2) using the TWFE estimator. I do not use Borusyak et al. (2022) estimator in these figures because the minimum effective number of observations is below the minimum recommended by the authors and the estimates may be unreliable. Total homicides indicator is the sum of police killings and other homicides. The dependent variables are inverse hyperbolic sine of semesters' rates per 100,000 citizens. Both regressions control for Semester and Police station fixed effects, have standard errors clustered at the police battalion level, and use population as analytical weights. Confidence intervals are at 95%.

Table 3.3: Robustness for the effects of UPP on violence indicators

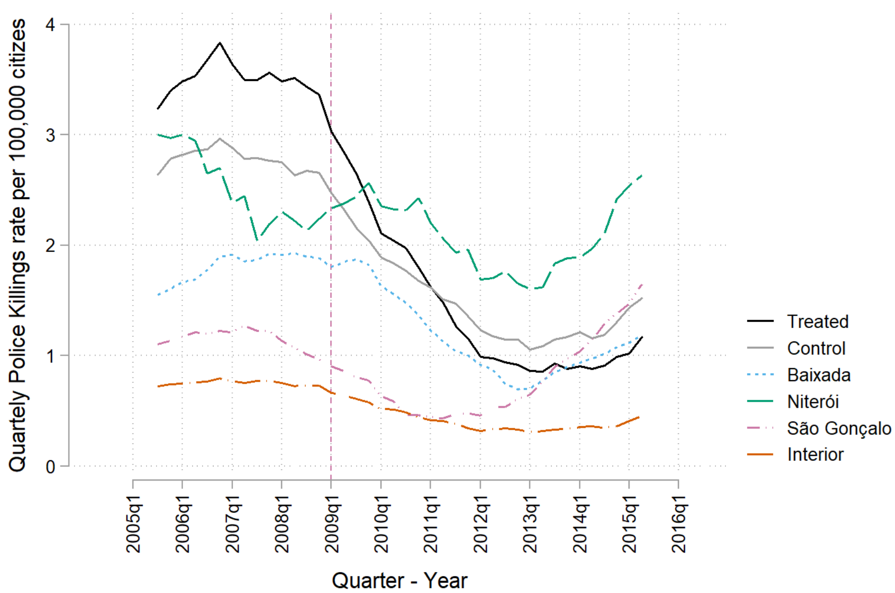
	Total Homicides	Police Killings	Other Homicides
Panel A: Negative Binomial			
Treat	-0.28	-0.44	-0.20
	(0.09)***	(0.14)***	(0.08)**
Incidence Ratio	0.75	0.65	0.82
Panel B: Poisson			
Treat	-0.19	-0.47	-0.11
	(0.08)**	(0.15)***	(0.08)
Incidence Ratio	0.83	0.63	0.89
Obs.	754	754	754
Semester FE	Yes	Yes	Yes
Police Station FE	Yes	Yes	Yes
Mean before treat.	43.11	9.31	33.80

Notes: Table shows the results for regression equation (1) using Negative binomial and Poisson estimators. The dependent variables are counts of the events. Both regressions control for semester fixed effect, police station fixed effect, and police station population (coefficient constrained to one); and, have standard errors clustered at the Police battalion level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 3.6: Temporal evolution of crime indicators for police stations in the state of Rio de Janeiro

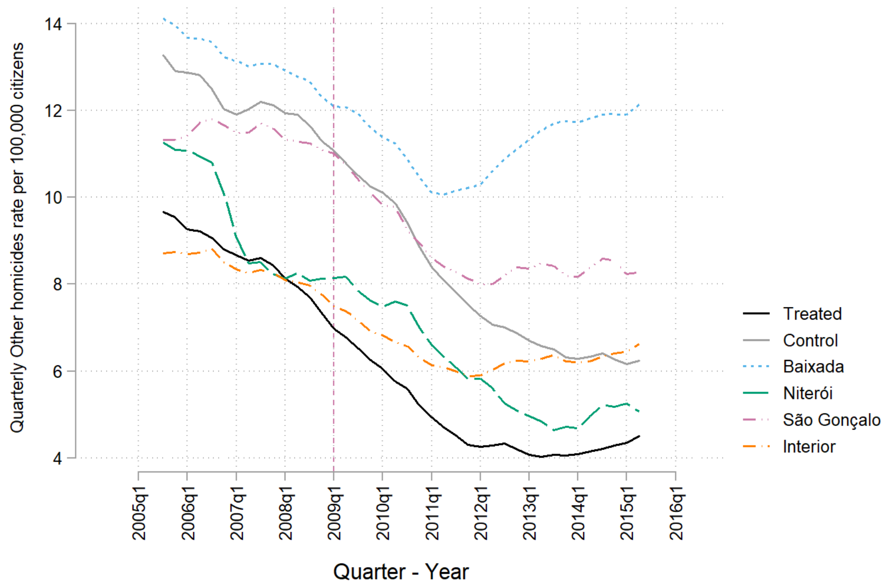


(a) Homicides rates



(b) Police killings rate

Figure 3.6: Temporal evolution of crime indicators for police stations in the state of Rio de Janeiro (cont.)



(c) Other homicides rates

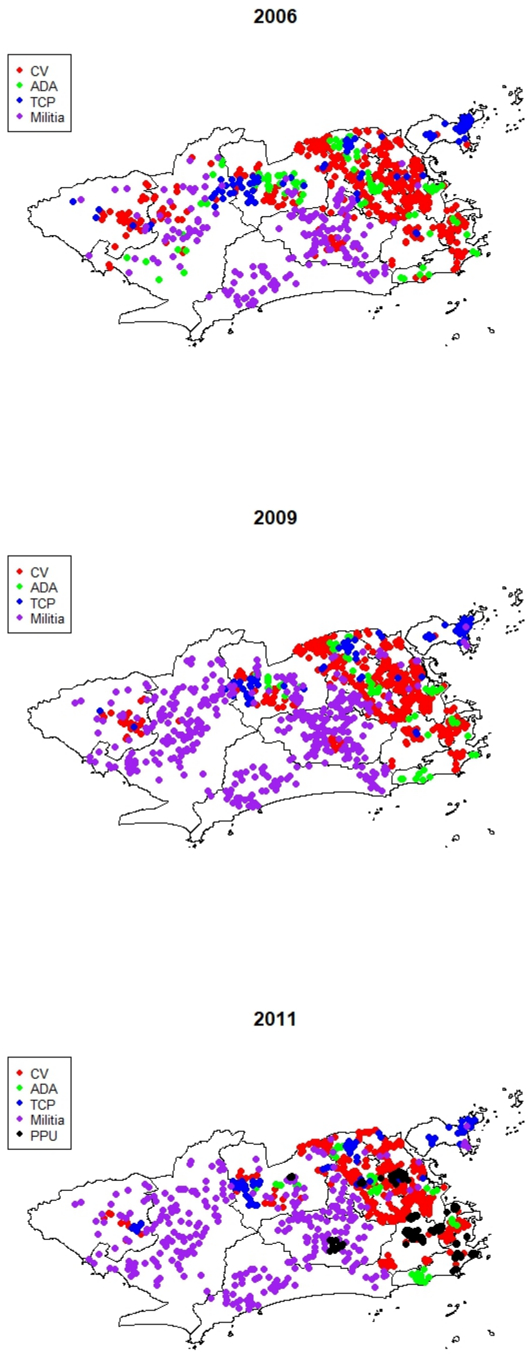
Notes: The figures show the temporal evolution of homicides and police killings in the State of Rio de Janeiro. I consider six quarters before and six quarters after the current period to calculate the moving average. The police stations in the city of Rio that I use in the main estimation are the "Treated"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

Table 3.4: Effects of UPP on crime indicators – different control groups

	Main	Baixada	Niteroi	Sao Goncalo	Countryside
Panel A: Homicides					
Treat	-0.23	-0.58	-0.30	-0.47	-0.47
	(0.13)*	(0.06)***	(0.05)***	(0.05)***	(0.06)***
Mean before treat	3.57	3.74	3.53	3.60	2.79
Panel B: Police Killings					
Treat	-0.49	-0.72	-0.82	-1.02	-0.82
	(0.11)***	(0.14)***	(0.12)***	(0.12)***	(0.12)***
Mean before treat	1.91	1.48	2.06	1.83	0.56
Panel B: Other homicides					
Treat	-0.13	-0.46	-0.04	-0.29	-0.33
	(0.13)	(0.06)***	(0.06)	(0.05)***	(0.06)***
Mean before treat	3.29	3.56	3.18	3.33	2.70
Semester FE	Yes	Yes	Yes	Yes	Yes
Favela FE	Yes	Yes	Yes	Yes	Yes
Obs.	754	935	598	598	2,364

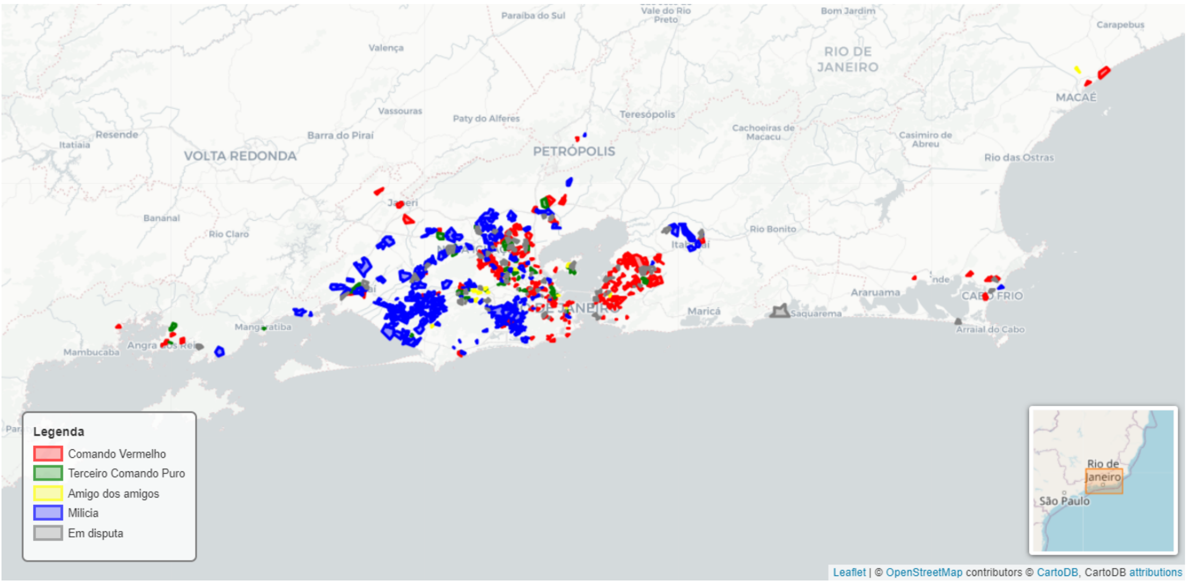
Notes: Table shows the results for regression equation (1) for Borusyak et al. (2022) imputation estimator. The dependent variables are semesters' rates per 100,000 citizens. Both regressions control for Semester and Police stations fixed effects, have standard errors clustered at the police battalion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

Figure 3.7: Distribution of criminal groups in Rio de Janeiro

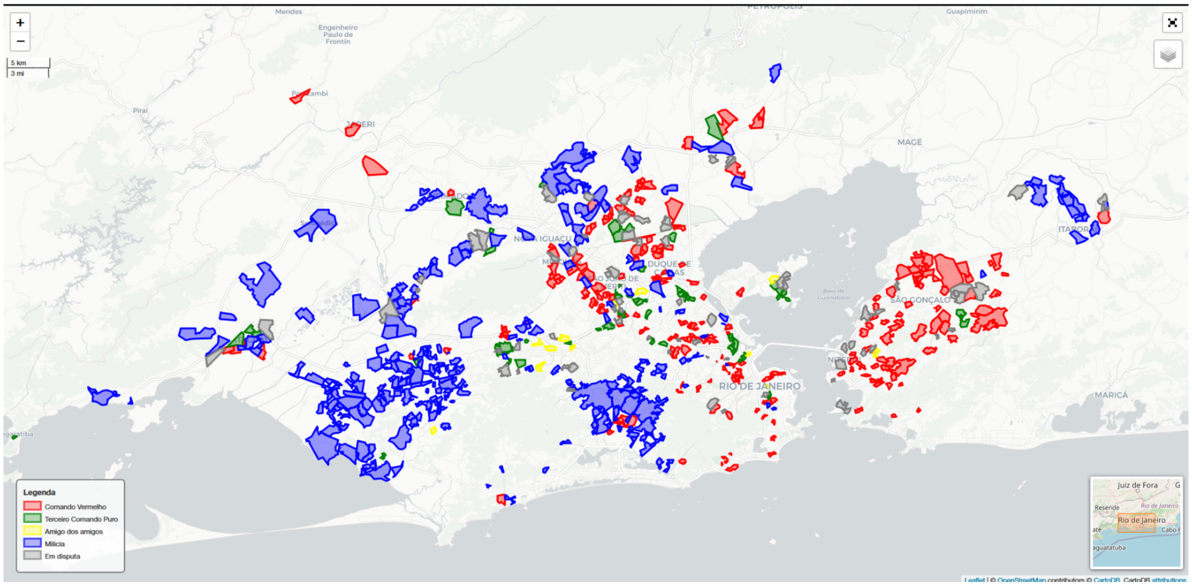


Source: Zaluar (2012) and Zaluar and Barcellos (2014). Geocoded by the author.

Figure 3.8: Distribution of drug gangs and militia in Rio de Janeiro - 2019



(a) State of Rio de Janeiro



(b) Metropolitan Region

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

Chapter 4

Heat and Health: A Tale of a Tropical City

4.1 Introduction

A growing stream of causal evidence has shown that changes in environmental factors affect human health. The heat-mortality relationship has attracted particular attention, as the potential risks of climate warming and average temperature changes are expected to be widespread across the globe. Detrimental effects of exposure to heat waves and extremely high temperatures, considered one of the most damaging events, have been well documented in different contexts in developed and developing countries. This has been made possible, to a great extent, by the utilization of plausibly exogenous variation in weather indicators at the national and sub-national levels. A central empirical question, however, is how localized these effects are. This is especially relevant should damage be heterogeneous within regions. While much of the existing evidence comes from estimates of average treatment effects at the regional level, little is still known about the extent to which the impacts and the distribution of damages are localized in general and how effective localized policy responses can be in particular.

This paper examines the heat-mortality relationship at a fine-grained level within Rio de Janeiro, one of the world's largest and most heterogeneous cities. We rely on novel sources of satellite imagery on temperature and administrative health records at the individual level to build a neighborhood-by-month panel over 14 years. These data allow us to use only intra-city, within-neighborhood variation in daily temperature to identify effects on health outcomes. By exploring the residual variation in temperature measures and outcomes, usually absorbed in previous fixed-effect analyses at the national and sub-national levels, we can contribute with novel evidence on the sources of heterogeneity in damages and for the optimal design of policy response.

More specifically, we study the effects of extreme temperatures on the mortality rates due to cardiovascular conditions of individuals aged 60 years and older. Hot days exacerbate the capacity of the body to regulate its temperature, triggering a physiological process that increases the cardiac and pulmonary responses, which can lead to a higher risk of cardiovascular, respiratory, and cerebrovascular diseases (Kephart et al., 2022). Moreover, vulnerable populations, such as the elderly, have diminished capacity to regulate body core temperature under heat stress and, thus, can be more affected by extreme temperatures (Achebak et al., 2018).

We use data products derived from satellite imagery that measure *land surface temperature* (LST), which is constructed using radiation emitted by the land surface observed by satellites. LST is highly correlated with air temperature, which weather stations measure, and captures thermal energy concentration and human comfort. While weather stations' spatial coverage is typically low and scattered within cities, LST data provide per pixel daily temperature at a nominal pixel spatial resolution of 1km^2. Given its latitude, the city of Rio de Janeiro has 1,524 pixels of 889m^2, which are weighed by the census tract population and used to construct temperatures for each of its 144 neighborhoods, in high-frequency, throughout the analysis. To compute mortality variables, we rely on a rich administrative microdata set, which contains the universe of all deaths in Rio and the exact neighborhood of residence, socioeconomic markers, date, and cause of death. These data allow us to compute mortality rates at the neighborhood-by-month level and assess heterogeneity. Our empirical strategy draws upon a fixed-effects model, typically used to identify causal impacts of temperature on health outcomes in different contexts (Deschênes and Moretti, 2009; Deschênes and Greenstone, 2011; Barreca et al., 2016).

Rio de Janeiro provides a unique empirical setting. First, the city is home to more than 6.3 million people scattered across diverse neighborhoods regarding geographical elements and socioeconomic characteristics. Rio spreads over 1,225km^2 in the planet's tropical zone, which experiences intense insolation all year round. Forested massifs reaching heights superior to 1,000 meters affect patterns of winds and temperature, as they shape the penetration of the Atlantic sea breeze into the hinterland and provide shade, contributing to the formation of microclimates within the city. Neighborhoods located behind the massifs receive the air originating from the ocean in a warmer way than those in the windward position. This phenomenon results in a significant variation in temperature among neighborhoods. There is substantial socioeconomic heterogeneity as well. HDI ranges from 0.970 in the area of Gávea to 0.732 in the contiguous Rocinha to 0.700 in the more distant Complexo do Alemão, where income per capita is only a tenth in comparison to the first. The significant variation in socioeconomic conditions, across and within neighborhoods, overlapped with variation in temperature across microclimates, enabling us to explore heterogeneity in the heat-mortality relationship within the city.

Second, Rio also provides unique variations in access to primary health care and emergency care. We can exploit the restructuring of the city's primary healthcare system, mainly driven by the staggered implementation of Family Health Clinics (FHC). Starting in 2008, this program aimed to expand primary and preventive health care provision in their respective catchment areas. Population coverage started from 3.7% in 2008, reaching more than 50% in 2015, portraying significant variation over time both within and across neighborhoods. Beginning in 2007, in parallel, both the city and the state of Rio de Janeiro improved the physical network of ER facilities with the implementation of more than 20 non-hospital emergency care units to provide emergency care of low and medium complexity, together with the restructuring of the previous physical network of hospital ERs (Bhalotra et al., 2020). This movement generally expanded access points to emergency care, thus reducing the average distance of patients to ER facilities. We rely on precise geocoding of FHC catchment areas and ER facilities and exploit idiosyncratic variation in the differential access to public health care services across time and neighborhoods to examine the extent to which and how the design of local health systems can effectively mitigate damages.

We find that there is enough intra-city variation in exposure for the temperature-mortality relationship to manifest. Indeed, one extra hot day causes mortality rates to increase by 0.53 ($p < 0.01$). In the typical neighborhood-month, these effects account for 2% of deaths due to cardiovascular diseases in the population of 60 years and older. These results are robust to different measures of heat stress. However, there is a possibility to mitigate these effects: access to preventive health care can attenuate the marginal effect of temperature on cardiovascular deaths. One standard deviation increase in coverage by Family Health Clinics reduces the effects of one extra hot day by 27%. Moreover, if Rio had full primary health care coverage, these negative effects of heat stress would be completely mitigated.

This paper contributes novel evidence to the active literature on the health effects of environmental changes and their distributional damages, particularly the growing stream of research on heat-related mortality. The heat effects on mortality have now been well documented in different contexts, and many studies explicitly consider heterogeneity in the distribution of damages and mitigation response (Cohen and Dechezleprêtre, 2022; Garg et al., 2019; Burgess et al., 2017).

A critical aspect in assessing the current estimates, however, is that studies often focus on national or subnational fixed-effects models and rely on temperature indicators measured from a small number of weather stations near cities or in city centers as a proxy for heat stress (as reviewed in Deschenes (2014)). This approach potentially converts much of the existing heterogeneity within locations into average treatment effects across sites. It invariably leads to measurement error since the heat stress experienced by the population is not adequately recorded. This is particularly problematic if there is sorting in population density, vulnerability, and exposure to the most adverse weather conditions. In our setting, exposure to heat stress is neighborhood-specific. At the same time, time fixed-effects control for common city-specific trends, such as fluctuations in the overall healthiness of the population and economic activity. Although not possible, adding location-by-time fixed-effects in previous subnational fixed-effects models would be analogous. Neighborhood or neighborhood-by-month fixed-effects further absorb differences by SES and location that are month-specific. In that sense, by relying on the residual variation in temperature and health outcomes, conditional on a set of within-city fixed-effects, our results reveal that relevant heat-related damages manifest themselves and are unevenly distributed at the local level.

We also explore heterogeneity in damages and examine the extent to which policy responses, again at the very local level, can act as mitigation factors. A few but solid recent studies have documented that access to primary health services can act as a protective factor against temperature fluctuations (Cohen and Dechezleprêtre, 2022). We advance the existing evidence in meaningful ways. We show that heat-related damages can be mitigated with community-level actions, in the short-run, and at the very local level, with the adequate redesign and expansion of health services within cities. We find that access to preventive rather than curative healthcare is mainly instrumental for mitigation efforts. We conjecture that access to preventive care leverages the management of chronic conditions and the adherence to treatment and medications that are typically prescript for continuous use.

Finally, there are several relevant aspects in assessing an urban environment exclusively. While the need for adaptation is likely to be widespread worldwide, urban areas will be on the frontlines of climate warming. Cities are expected to be home to nearly 6.7 billion people, or 70% of the world's population, by 2050 (UN, 2018). With 40% of the world's population living in tropical zones today, we should expect a remarkably swift expansion of tropical cities in the near future. Therefore, this paper's results are especially informative for the optimal design of localized mitigation policies in an increasingly urbanized world. This is particularly salient since the existing evidence on whether the heat-mortality relationship is stronger in rural vs. urban areas has been somewhat mixed – e.g., ranging from a predominantly rural effect (Burgess et al., 2017) to insignificantly different (Cohen and Dechezleprêtre, 2022). This stands out in developing countries, where weather shocks affect agricultural productivity, food intake, and income per capita. In this paper, we place the urban environment under the spotlight. We can revisit the heat-mortality relationship by exploring within-city variation, coupled with a novel approach that made it possible to measure heat stress at a fine-grained level and net the influence of relevant mechanisms typically active in other developing contexts (e.g., agricultural production). Our reduce-form estimates indicate non-trivial and robust adverse effects of heat stress on health outcomes, particularly among the most vulnerable.

The remaining of this paper is organized as follows. Section 3.3 describes the background and the data. Section 4.3 presents and discusses the empirical strategy. In Section 2.4 we present the main results on the heat-mortality relationship in the city of Rio and the main robustness checks. In Section 4.5 we test whether and how access to health services can mitigate damages. Section 4.6 concludes.

4.2 Background and Data

4.2.1 Rio de Janeiro: Geography, Climate and Inequality

The city of Rio de Janeiro spreads over 1,225 km^2 on the tropical zone of the planet, under intense insolation all year round. Rio experiences higher temperatures between December to March, during the summer, and relatively warm winters. According to official measures from weather stations, February is the warmest month, with average daily temperatures around 28C and average maximums higher than 34C. July is the coldest month, but daily temperatures still average above 22C. Despite the small number of weather stations, official records also document significant temperature variations within the city. The average difference between daily maximums and minimums as measured at 1 pm is superior to 5C.[1] Rio is well-known for its natural landscape, with dramatic geographical features scattered across the city being determinant to temperature variation, together with proximity to the ocean as well as general and secondary atmospheric circulation (Neiva et al., 2017). Forested massifs reaching heights superior to 1,000 meters affect patterns of winds and temperature, as they provide shade to some areas and shape the penetration of the Atlantic sea breeze into the hinterland, contributing to the formation of microclimates. In particular, sites behind the massifs receive winds warmer than those in the windward position (Neiva et al., 2017; Rio de Janeiro, 2016; Serra and Ratisbonna, 1941). Neighborhoods in the North the West Zones are usually the warmest, in contrast to areas in the South Zone, where the Atlantic sea breeze cools the air.

The left map of Figure 4.1 shows the main geographical elements of Rio as well as its division into neighborhoods, which are ordered in Administrative Regions (North, West, South, and

[1] All temperature records cited were computed based on official data from INMET over our analysis period.

Central Zones).[2] The city is home to more than 6.3 million people and significant socioeconomic heterogeneity across and within neighborhoods. Poverty rates are nearly twofold in neighborhoods in the North and West Zones in comparison to those in the South Zone, while the Gini index ranges from 0.53 in the North Zone to 0.62 in the South Zone – where, in spite of lower poverty rates, the share of inhabitants living in slums reaches 17%. HDI ranges from 0.970 in the neighborhood of Gávea to 0.732 in the contiguous Rocinha, both in the South Zone, to 0.700 in Complexo do Alemão, where income per capita is only a tenth in comparison to the first (IETS, 2015; IBGE, 2010).[3] In that sense, while socioeconomic disadvantage largely overlaps with exposure to higher temperatures across neighborhoods, there remains much inequality within-neighborhood, generally under more homogeneous temperature conditions.

4.2.2 Temperature

Our empirical approach draws upon a monthly panel of data at the neighborhood level, covering the period from January 2003 to December 2016, on indicators of temperature and heat stress, health outcomes, and access to health services[4]. A key challenge in computing temperature variables in developing countries is the availability of data (Auffhammer et al., 2013), where weather stations' spatial and temporal coverage is typically low and short. In the city of Rio, two parallel official meteorological systems comprise a total of only eight ground weather stations, and most of them have been recording temperature only from the early 2000s onwards[5]. Many papers overcome this problem by using gridded weather data products (e.g., Matsuura and Willmott, 2018), which interpolates weather station data across space and time to form a balanced panel of observations on a fixed spatial grid. Outside of the United States, the grid resolution of these data products is typically high and superior to many kilometers (Hooker et al., 2018). For our study region, this would leave us with a concise cross-sectional variation.

To overcome this challenge, we take an alternative approach and use data products derived from satellite imagery that collect *land surface temperature* (LST), as opposed to *air tem-*

[2]Rio's municipal government officially defines neighborhoods. The current number of neighborhoods in Rio is 162, but this has changed over time with new neighborhoods being created. To maintain spatial comparability in our health data, we aggregate the 162 neighborhoods into the original 144 units.

[3]All figures cited refer to IBGE (2010). The year 2010 is the mid-point in our analysis period and refers to the latest Population Census available.

[4]LST data become available from July 2002 onwards. We select January 2003 onwards into our sample to have 14 full calendar years. We end in December 2016 as this was the last month available at the time of analysis.

[5]Two systems provide meteorological information for the city of Rio de Janeiro. First, the Alerta Rio System with seven ground stations retrieving data on several weather variables (https://www.rio.rj.gov.br/web/georio/alerta-rio). Second, the National Institute of Meteorology (INMET) has one ground station in Rio.

perature (Tair), which is what ground weather stations measure. LST is constructed from spectrum bands that measure radiation emitted by the land surface observed by satellites (Wan, 1999). We use Version 6 of the MYD11A1 data product derived from the MODIS (Moderate Resolution Imaging Spectroradiometer) instrument aboard NASA's Aqua satellite (Wan et al., 2015), which has provided a high-quality global LST product (Lian et al., 2017).[6] This data product provides daily per-pixel land surface temperature at a nominal pixel spatial resolution of 1km. Images have been captured at approximately 1 pm local time every day since July 2002.

The use of thermal remote sensing has been an emergent trend in earth sciences, and environmental epidemiology and applications of satellite-derived LST are now widely reported in the literature to characterize urban heat islands (Azevedo et al., 2016; Hu and Brunsell, 2013; De Ridder et al., 2012; Dousset et al., 2011; Rajasekar and Weng, 2009), to study areas of higher relative temperature within cities (Neiva et al., 2017; White-Newsome et al., 2013; Johnson and Wilson, 2009), and to estimate the near-surface temperature in the absence of meteorological stations (Chen et al., 2015; Good, 2015; Kilibarda et al., 2014).[7] Conceptually, LST is the physical temperature of the top few micrometers underlying the Earth's surface. At the same time, Tair is the thermodynamic temperature of the air at the height of approximately 2 meters above the surface (Lian et al., 2017). Despite these fundamental differences, a growing stream of studies has shown that LST and Tair are strongly related (Good et al., 2017). Both worldwide and local analyses have validated satellite-derived LST by ground-truthing, which typically compares exposure assessments in a specific site covered by both LST images and ground stations. The findings indicate cross-section and time-series correlations between LST and average Tair are often intense, particularly at night. During the day, LST is generally more related to maximum Tair. Yet, this relationship depends on surface type, insolation, and elevation – LST becomes relatively higher than maximum Tair in more sparsely vegetated and bare areas and under stronger insolation (Good et al., 2017; Lian et al., 2017). This is the case of the urban regions in the tropics, where LST often surpasses Tair records. Overall, the difference between LST and Tair increases with Tair (Mildrexler et al., 2011). Consistent with that, LST has been used as a reliable marker of heat islands and weather anomalies. For instance, Good et al. (2017) analyze the August 2003 European heat wave and show that LST maps closely resemble the equivalent Tair over time and space.

In the MODIS data product we use, the city of Rio de Janeiro comprises 1,524 pixels of

[6] The main alternative to MODIS is the LANDSAT satellite, which produces images once every 16 days. Since we need daily data, MODIS is better suited for our purposes.

[7] In the economics literature on the effects of temperature, this approach seems to be rare. We are only aware of the work of Aragón et al. (2018), who study the impact of temperature on small farmers' input decisions in Peru.

889m^2. We use these pixels to construct population-weighted daily temperature and heat stress measures for each of the city's 144 neighborhoods. We use geo-referenced information from Rio's 10,504 census tracts to assign each census tract to a pixel.[8] To make this assignment, we take the census tract's centroid and assign it to the pixel with this point. This procedure creates a daily panel of census tracts. Next, we use geo-referenced information on neighborhoods to assign each census tract to a neighborhood. Finally, we aggregate LST at the neighborhood level by taking the average over the neighborhood's census tracts. Since census tracts are designed to have roughly equal population size (IBGE, 2010), this procedure yields a population-weighted average temperature for each neighborhood-day.

Using satellite data to measure heat stress has advantages and disadvantages. Satellite-derived LST has global coverage and high spatial resolution. Because of that, we can systematically count the daily temperature for the whole city, at a very fine-grained level, for more extended temporal coverage than weather stations allow. On the other hand, the major disadvantage comes from the fact that MODIS cannot provide meaningful data when there is thick cloud coverage. In other words, we have missing temperature data whenever there is cloud coverage. To deal with the issue of missing data, we take the following approach. First, we use data from the available weather stations in Rio. For each neighborhood, we regress land surface temperature (from the satellite data) on air temperature (from the weather station data). Next, we use the fitted values from this regression to fill in any missing data. This procedure is discussed in detail in Appendix C.1. It is similar to that used in other studies in the economics literature that deal with missing observation in weather station data (e.g., Auffhammer and Kellogg, 2011; Schlenker and Roberts, 2009).

We then construct temperature measures at a monthly frequency to compute indicators for heat stress at the month-neighborhood level. As for our first indicator, we count the number of days in a neighborhood-month in which temperatures exceed a given threshold. As further discussed below, we consider 40C as the benchmark, which roughly corresponds to the 90th percentile of the daily LST. As typical in the economics literature, we construct alternative thresholds and bins to explore non-linearities in the heat-mortality relationship. We also use the number of degree-days, the sum of degrees that exceed a given threshold T, e.g., $T = 40C$. Formally the number of degree-days in neighborhood i and month m is

$$\text{Degree-Days} > T_{im} = \sum_{d=1}^{D} \max\{temperature_{idm} - T_{im}, 0\}, \tag{4.1}$$

[8] Census tracts are defined by the Brazilian Institute of Geography and Statistics (IBGE) and typically contain around 100 households. Residential areas in Rio are fairly dense; therefore, census tracts are small relative to 1km x 1km pixel size. Therefore, one census tract is typically entirely contained in one pixel.

where D is the number of days in month m. Finally, we create measures of heat waves. To define a heat wave lasting k days ($k = \{3, 5, 7\}$), we count the number of consecutive days with temperature greater than T in the neighborhood-month.

The evidence indicates that LST strongly correlates with Tair, as mentioned above. We follow the literature and verify this relationship in our data by ground-truthing. Figure 4.2 shows how LST and Tair are related in Rio de Janeiro by using air temperature measurements from one of the ground stations in the city and comparing it to the satellite data we use for the exact location and the same time[9]. The two plots make it clear that the two measures are strongly correlated. However, LST is typically higher than the air temperature, as expected during the day in urban areas under high insolation. A simple time-series OLS regression of LST on Tair at the month-by-year level yields $\widehat{LST} = 1.565 + 1.146Tair$. This indicates that LST records in our data are expected to average approximately 14.6% higher than Tair, plus 1.5C in absolute terms (e.g., 30C in Tair is roughly equivalent to 36C in LST).

Table 4.1, Panel A, shows descriptive statistics of our main temperature variables. The sample size corresponds to 23,016 month-by-neighborhood observations [14 years $\times$ 12 months $\times$ 137 neighborhoods]. The average neighborhood-month LST is 32.6C (SD 4.92C), with about 13 days per month recording LST between 25C-35C, 5.6 days with LST between 35C-40C, and 3.85 days with LST higher than 40C. Figure 4.3 displays the exact distribution of the average number of days per month across 10 LST bins. We also document substantial variation in LST across neighborhoods, as shown on the right map of Figure 4.1. For a given point in time, the average difference between the maximum and minimum LST across neighborhoods is approximately 13C, reaching about 17C in December-February. Consistent with official measures of Tair and complementary literature (e.g., Neiva et al., 2017; Rio de Janeiro, 2016), we find that average LST in neighborhoods in the North and West Zones are usually higher, in contrast to records in areas in the South Zone.

Figure C.1 presents both temporal and cross-sectional variation of the temperature shocks. These figures show the variation of the variable Bin 40+, which calculates the number of days in a month that land surface temperature is above 40 degrees Celsius, in the time and cross-sectional dimensions. Subfigure (a) displays the average fraction of days above 40C in a neighborhood relative to the historical average of days above 40C in that neighborhood. We calculate this measure for each neighborhood-month and, then, average in the cross-section[10].

[9] For this comparison, we choose the ground station of Alerta Rio System, located in the neighborhood of São Cristóvão as it provides hourly temperature, over a longer span of time among the existing stations.

[10] Let B^{40}_{iym} be the number of days above 40C for neighborhood i in year y and month m; $\overline{B}^{40}_{im}$ the historical average for the number of days above 40C in that neighborhood-month. For each period y, m, define the fraction of number of days above 40C relative do the historical average as: $F^{40}_{iym} = B^{40}_{iym}/\overline{B}^{40}_{im}$. Subfigure (a) shows the average F^{40}_{iym} across neighborhoods for each period of time: $\frac{1}{N}\sum_{i=1}^{N} F^{40}_{iym}$.

A value above one suggests that in that period, the average number of days above 40C is higher than the historical average. This figure intends to show the time-series variation in the data. Subfigure (b) presents the cross-sectional variation of the data. It displays the percentage of neighborhoods with a number of days above 40C in that time higher than 1.5 standard deviation of the historical average for that neighborhood. We observe that heat stress varies over time but also in the cross-section. Even when a heat wave arrives, not all neighborhoods are equally exposed to extreme temperatures once controlled by the historical average temperature in the neighborhood-month.

I present a case study for three neighborhoods located in distinct zones of the city in Figure C.2. Interestingly, there is variation in heat stress for these places in the extensive and intensive margin. For the extensive margin, not all neighborhoods display higher temperatures than average in the same periods. That is, there are months in which only one neighborhood 'is exposed to heat stress. In the intensive margin, even in periods in which the three places display higher temperatures than average, there is variation in the shock intensity. These pieces together suggest that microclimates matter in determining heat stress at the local level within the city of Rio (Neiva et al., 2017)[11].

4.2.3 Health Outcomes

Excessive heat and abnormally high temperatures trigger physiological responses that may lead to death. Basu (2009) shows that high temperature increases the risk for cardiovascular, respiratory, and cerebrovascular diseases. Some specific cardiovascular diseases, such as ischemic heart disease, congestive heart failure, and myocardial infarction, also display a high risk of occurring in elevated temperatures. The author highlights that some subgroups, such as Black racial/ethnic groups, women, those with lower socioeconomic status, and several age groups, particularly the elderly and infants, are more affected by elevated temperatures. Achebak et al. (2018) reason that the elderly have diminished physiological capacity to regulate body core temperature under heat stress conditions.

We rely on data from the Brazilian National System of Mortality Records (Datasus/SIM) to compute mortality rates. SIM gathers information on every death officially registered in Brazil. We accessed the data via Datasus/Tabnet Rio, which provides, specifically for the city of Rio, the counting of deaths by descendent's neighborhood of residence, age, race, gender, and education, as well as by diagnostic identified according to the International Classification of Diseases, 10th Revision (ICD-10).[12] Based on literature review and follow-

[11] For a non-technical explanation of microclimates in Rio: https://www.washingtonpost.com/news/capital-weather-gang/wp/2016/08/05/the-summer-olympics-begin-in-rio-despite-the-fact-that-its-technically-winter/. Accessed in November 2022.

[12] Accessed in July 2019: http://tabnet.rio.rj.gov.br/.

ing closer Eisenman et al. (2016) and the Environmental Protection Agency,[13] we selected ICD-10 codes associated with cardiovascular conditions at increased heat-related risk. More specifically, this includes diseases classified under I00-99 (diseases of the circulatory system), syncope and collapse (R55) and sudden deaths (R96), transient cerebral ischaemic attacks and related syndromes (G45), vascular syndromes of the brain in cerebrovascular diseases (G46), and hemiplegia (G81-83).

Based on these codes, we compute mortality rates for each neighborhood and month by counting the deaths of individuals aged 60 or older and dividing by population (per 100,000). Data on the population aged 60 or older are obtained from IBGE (2010), which provides the counting of residents by neighborhood in 2010. As previously mentioned, the year 2010 is the mid-point in our period of analysis and refers to the latest Population Census available.[14] Monthly mortality rates at the neighborhood level are then merged with LST variables to form our panel of data at the neighborhood-by-month level. Table 4.1, Panel B, shows descriptive statistics of mortality rates by causes of death related to cardiovascular conditions at increased heat-related risk. We observed a monthly neighborhood average of 109 deaths per 100,000 individuals aged 60 or older during the analysis period. Among these causes, we observe that mortality is more pervasively related to urgent conditions, such as heart attacks (mean 56.5) and strokes (31.4), though it is also directly related to chronic diseases, such as hypertension (15.7).

4.2.4 Auxiliary Data and Controls

We make use of other pieces of data that are auxiliary to our analysis. First, we identify and geocode healthcare facilities in Rio (this includes hospitals, primary care, and emergency care units) and compute the catchment areas of Family Health Clinics and average distances from census tracts to ER services. We provide further background and details on health policies and the computation of access to healthcare services in Section 4.5. Second, we add a series of controls at the neighborhood-month level to our analysis. This includes mapping Pacifying Police Units, which are part of a law-enforcement program aimed at restoring territorial control of some areas in the city that were previously under the power of criminals and drug gangs. Data on the location and timing of the introduction of these units were obtained from the Institute of Public Security (ISP). We intersected them with the shapefile of neighborhoods to determine the number of units in each neighborhood-year-month. We

[13] https://www.epa.gov/climate-indicators/heat-related-illnesses. Accessed in June 2019.

[14] More specifically, the denominator of mortality rates is computed for the year 2010, and thus it is fixed over time. We perform tests to check whether the results are sensitive to this limitation by adding to our specifications a combination of neighborhood-by-year fixed-effects and specific linear trends on the population, as well as by using the outcome variable, the logarithmic of the counting of deaths. The results remain qualitatively robust.

also collected indicators for access to transportation. Data on Bus Rapid Transit (BRT) and the number of subway stations in each neighborhood were obtained from Pereira Passos Institute. We followed the same procedure of intersecting the shapefile of neighborhoods to compute the number of stations in each neighborhood-year-month.

4.3 Empirical Model

In this paper, we examine the relationship between heat stress and mortality rates due to cardiovascular conditions of individuals aged 60 years and older. Our empirical strategy follows closely fixed-effects models that have been typically used to identify causal impacts of temperature on health outcomes in different contexts (Deschenes, 2014; Deryugina and Hsiang, 2014; Barreca et al., 2016). The equation below, which adapts this class of models to a neighborhood-by-month setting, provides our conceptual setup:

$$h_{iym} = \alpha_y + \delta_{im} + \sum_{j=1}^{J} \lambda_j B_{iym}^{j} + \beta_i t + \gamma' Z_{iym} + \epsilon_{iym} \tag{4.2}$$

Where h_{iym} is the health outcome of neighborhood i, in year y and month m. The term α_y refers to year-fixed effects, while δ_{im} refers to the neighborhood by calendar month fixed effects. Our variables of interest are defined by B_{iym}^{j}, which indicates the number of days with LST within the temperature bin j for neighborhood i in year y and month m. As typical in the economics literature, we test for different definitions of bins and markers of heat stress. In more saturated specifications we also add $\beta_i t$, which is a neighborhood-specific linear trend, where t is the combination of year-month, and control variables Z_{iym}, which include the share of population covered by family health clinics (FHC), a dummy for the presence of an Emergency Care unit, and the number of Pacifying Police Units, Bus Rapid Transportation stations, and subway stations in each neighborhood-month. Control variables thus isolate the potentially confounding influence of relevant public policies on healthcare, transport, and security.[15] The term ϵ_{iym} refers to idiosyncratic error. We estimate standard errors clustered at the neighborhood level, to allow for serial correlation within neighborhoods, and weight regressions by neighborhood population size to smooth noisy variation in mortality in small cells.

In our setting, exposure to heat stress is neighborhood-specific, while year fixed-effects control for common municipality trends, such as fluctuations in the overall healthiness of the population and economic activity. Neighborhood-by-month fixed-effects further control for

[15] There were important expansions in transport and security policies in the late 2000s and early 2010s because of major international events hosted by Rio, such as the Olympics (2016) and the World Cup (2014). The main policy interventions are considered in our set of control variables.

differences by SES, local infrastructure, and other location features that are month-specific and fixed at the neighborhood level. Year fixed-effects also absorb temperature fluctuations that are common to all neighborhoods, while neighborhood-by-month fixed-effects isolate month-specific temperature averages that vary across neighborhoods. This is analogous to control for typical seasonality across microclimates. Conditional on fixed-effects, the residual variation in temperature is plausibly idiosyncratic and arguably unexpected for that neighborhood and month. Identification, therefore, relies on exogenous temperature deviations from neighborhood-month historical averages, conditional upon yearly averages, neighborhood-specific linear trends, and covariates.

Importantly to our empirical approach, the residual variation in LST used for identification varies significantly in the cross-section and in the time series. We examine these patterns in Figure C.1. First, we compute the monthly share of neighborhoods under heat stress, i.e., with the number of days with LST higher than 40C greater than their respective historic average plus 1.5 standard deviation. Second, we compute the monthly average log-deviation of the number of days with LST above 40C from the respective neighborhood historical average. The left-hand plot of Figure C.1 shows that this latter indicator varies substantially over time. The right-hand plot shows that the incidence of heat stress is not homogenous throughout the city at a point in time. We observe months under more pervasive heat stress, hitting almost 80% of the neighborhoods, and periods with less pervasive or with no neighborhood experiencing harsh temperature conditions. In other words, at a point in time, some areas may be suffering harsh temperature conditions, while others may not be.

Conceptually, the introduction of time (i.e., month-year) fixed-effects would underestimate the marginal effect of temperature on mortality because it would discount the effect of heat waves that affect all the neighborhoods in relatively the same way. For example, if Feb/2010 was too hot, the estimates with time fixed-effects would only capture differences among bairros, discarding the average effect of the heat wave itself. The estimates, in this case, will not capture the whole effect of the heat wave and, therefore, they will be smaller. As robustness, we also run the empirical specification with time fixed-effect, but the results show be analyzed with this caveat in mind.

The use of monthly rather than daily data helps us to smooth the computation of noisy daily mortality events at the neighborhood level as well as to overcome the confounding influence of harvesting and other displacement effects (as discussed in Deschênes and Greenstone (2011)).

While it is difficult to raise plausible concerns regarding reverse causality and omitted factors in our empirical setting, a potential caveat pervading our analysis relates to measurement error. The measurement errors in the LST variable could be non-classical due to the imputation method and, thus, the estimates of the parameter of interest could be inconsistent. However, our imputation method based on the nature of missing data in the LST data reduces these concerns.

First, the missing data for LST is not missing completely at random (MCAR). The main reason for missing values in LST data is the presence of cloudy-sky conditions (Mildrexler et al., 2011; Shiff et al., 2021), which implies that regressing mortality on LST directly (complete cases) would introduce selection on clear-sky days. However, LST's data generation process creates missing values at random (MAR), i.e., the occurrence of a missing value is strongly correlated with other observed covariates that predict cloud coverage. Rubin (1976) shows that missing at random happens when, after controlling for several covariates, the missing value is randomly distributed. Therefore, if we project LST on a rich set of weather variables measured from ground-based weather stations, the residual shouldn't be correlated with any other variable.

We use a regression framework to impute the missing values in our dataset. The idea is to explore the time-series variation of several weather variables from the two ground-based weather stations with (almost) complete observations and create an econometric model to predict the missing values in a neighborhood i, year y, and month m. The weather variables in these data are temperature, wind speed, wind direction, precipitation, relative humidity, atmospheric pressure, and evaporation. If LST data is missing at random, after controlling for these several variables that predict cloud coverage, the missing values should not be systematically correlated with other variables. Furthermore, we perform this exercise for each neighborhood individually to account for microclimates in Rio. That is, we allow for neighborhood-specific relationships among weather variables and LST:for each neighborhood, we regress the (complete cases) LST in that neighborhood on this rich set of weather covariates.

4.4 Results

Table 4.2 shows the effects of five different measures of temperature shocks on mortality due to cardiovascular causes for individuals aged 60 years and older. Row 1 shows the effect of one extra "hot" day in a month – i.e., one extra day with temperatures above 40C. The specification in column 1 controls for neighborhood-specific month fixed effects and year dummies. The remaining columns add controls to expurgate spurious variation that might have survived the rich set of fixed-effects included in column 1. We start by adding neighborhood-specific year fixed-effects in column 2, which aims to capture any shock that affects the neighborhood in a year. The point estimates across columns 2 and 3 remain virtually unchanged.

We choose the specification in column 2 as our preferred, as it has a stricter set of controls. In that specification, one extra hot day causes mortality rates to increase by 0.53 ($p < 0.01$). To interpret the magnitude of these effects, note that the typical neighborhood-month has four hot days and that the average mortality rate is 109. Therefore, on the typical neighborhood-month, hot days account for 2% of the average cardiovascular-related elderly fatalities. For the city of Rio de Janeiro and its almost 1 million inhabitants aged 60 years old or more, this represents around 20 additional monthly deaths.

The remaining rows show comparable results using different measures of extreme temperature shocks. For example, the second row exhibits the effects of degree-days above 40C. The estimates reveal that an extra 1C over 40C in a month leads to a 0.11 increase in mortality rates in our preferred specification. In the typical neighborhood-month, there are 13C degree-days above 40C, which translates into 1.3 percent of the average mortality rate. For the heat-wave measures, the mean effect varies from 0.85 to 0.4 percent of the mortality rates. These rows capture the effects of having a determined amount of continuous days with temperatures above 40C. For instance, row '4.Number of 5-day Heat Waves' considers the effects of a heat-wave that lasts more than five continuous days with daily temperatures above 40C. As expected, the point estimates increase with the duration of the heat wave.

We add time fixed-effects in column (3). These fixed-effects absorb most of the variation of a temperature shock that contemporaneously hit the neighborhoods, such as a heat-wave for example. As we expected, the point estimates are smaller. The results are still significant for some measures of shocks but not for all. For our main measure in row 1, we find that even controlling for time fixed-effects, one additional hot day causes the mortality rate to raise by 0.28 ($p < 0.05$) implying more than 10 extra monthly cardio-vascular deaths in a typical neighborhood-month.

Additional Robustness Checks We show that the estimates of column 2 hold for alternative definitions of the empirical specification. Figure C.3 displays these results. We control for different bins, alter the temperature threshold and change specific controls for rainfalls. The point estimates are fairly robust to any of these modifications.

U-shape relationship between temperature exposure and mortality Next, we estimate a flexible functional form for the impacts of temperature on health. We use three-degree bins as our main dependent variables. Each bin displays the number of days in a month the daily temperature is within the bin range. This non-parametric specification allows us to test the non-linearities in the relationship between temperature and mortality. Moreover, we perform two additional exercises that i) shed light on the temporal displacement of temperature shocks on deaths, an effect that is known as "harvesting" that suggests the possibility of delayed effects of temperature exposure, and, ii) provide a placebo exercise in which we test if previous temperature shocks correlate with current deaths. Figures 4.4 and 4.5 exhibit these results. Similar to other papers in the literature, we find a U-shaped response of temperature exposure on deaths (Hsiang et al., 2017).

Heterogeneity Table 4.3 shows how the effects of heat stress vary with the socioeconomic measures at the neighborhood level. We interact the measure of temperature shock with variables retrieved from Census 2010 that shed light on socioeconomic differences among neighborhoods. There is suggestive evidence that neighborhoods with a better socioeconomic environment are less affected by temperature shocks. Table 4.4 presents the effects of heat stress on cardiovascular mortality for subgroups *within* each neighborhood. We calculate the number of deaths by education and race for individuals living in the same area[16]. The results indicate that an extra hot day in a neighborhood causes different effects on the individuals who live there: lower-educated citizens are disproportionately more affected by heat stress. The effects, compared to the sample mean of each group, are almost twice as large as higher-educated individuals who live in the same neighborhood. Taken together, there is evidence that vulnerable populations are more susceptible to the effects of heat stress and that socioeconomic conditions can act as protective factors.

Specific causes of death In table C.1, we breakdown the causes of death from Cardiovascular diseases. As expected, heat stress affects mortality rates related to cardiovascular conditions suggested in the literature (Basu, 2009). In particular, temperature shocks have a higher impact (relative to the mean) on strokes. Each extra hot day causes an increase of 1% in mortality rates due to strokes.

Other causes of death and age groups Table C.3 displays the consequences of heat stress on mortality for other cases of death and age groups. Temperature shocks do not impact other age groups younger than 60 years old (Panels A, B, and C). For individuals aged 60 or more, heat stress impacts all-cause mortality. One extra hot day increases the all-cause mortality rate by 1.55 ($p < 0.01$), which represents 0.46% of the sample mean. To give a glimpse of these results, Barreca et al. (2016) find that one additional hot day affects all-cause and all-age mortality rates by 0.34%[17].

We also observe that exposure to high temperatures increases mortality due to respiratory diseases. Our results for cardiovascular and respiratory diseases are consistent with the fact that exposure to heat stress may exacerbate underlying chronic conditions (de Oliveira et al., 2020; Vaidyanathan et al., 2020). Moreover, this outcome is in line with the findings

[16] Due to data restrictions, we do not observe the population for these cells, such as middle school dropouts aged 60+ in each neighborhood, to construct rates per 100,000 individuals and to weight these regressions. We opt to show these regressions but using an inverse hyperbolic sine transformation in the number of deaths for each subgroup.

[17] To make the comparison similar, we use the most recent sample in Barreca et al. (2016), thus we consider the point estimate in column 3 of Table 3 in their paper.

from Barreca et al. (2016) where air conditioning can mitigate heat-related deaths due to cardiovascular and respiratory diseases. In this paper, we focus on heat-related deaths due to cardiovascular diseases. We will address the channels through which exposure to high temperatures affects respiratory deaths in future research.

Alternative imputation methods In table C.2 we show that the main results are robust to different imputation methods. Interestingly, the point estimate for the regression with complete cases is almost twice as large as the benchmark estimation. This is expected given that LST complete cases selected information on clear-sky conditions, which tend to be warmer.

4.5 Access to Health Services

Table 4.5 estimates differential effects of hot days on health outcomes by access to health services. We start by interacting the number of hot days in a month with the fraction of the population in a neighborhood-month that is covered by a preventive health care program, the *Clinicas de Saude da Familia* (CSF), or family doctors (see section 3.3). The results presented in Columns 1 and 3 indicate that larger coverage of the CSF is associated with smaller hot day effects on mortality and hospitalization, respectively. The interaction term estimates are around -0.66 ($p < 0.05$) for mortality rate. To interpret the magnitude of the interaction term, note that the standard-deviation of CSF coverage is 0.21. Therefore, a one standard-deviation increase in the CSF coverage reduces the effect on mortality rate by 27 percent. Next, columns 2 and 3 interact the number of hot days in a month with the average distance to ER in the neighborhood. The magnitude of point estimates of the interaction terms are very small are not significant at any conventional level.

Table 4.5 gives evidence that preventive health care can significantly mitigate the hot-day effect on mortality due to cardiovascular diseases. The cost of preventive health care is typically much lower than that of emergency room visits, and the evidence presented here shows that it can also be much more cost-effective. Distance to emergency rooms may be ineffective in protecting from temperature shocks because, as Kovats and Hajat (2008) suggest in an extensive literature review, individuals who pass away due to extreme temperatures either die suddenly or fail to reach medical attention.

4.6 Conclusion

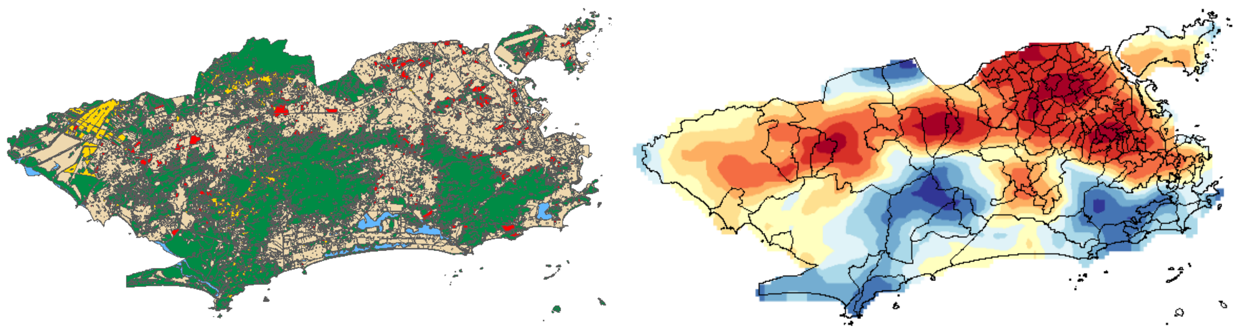
Several papers have shown that temperature shocks and heat stress, in particular, cause an increase in mortality, primarily by cardiovascular diseases. However, we did not know that high local variation in temperature was substantial. We use high-frequency satellite data to construct intra-city temperature measures of temperature exposure and estimate the effects of heat stress on mortality by exploiting neighborhood-specific variation.

We present evidence that temperature shocks are associated with higher mortality rates due to cardiovascular diseases in the elderly, accounting for almost 2% of these deaths in a typical neighborhood-month. Moreover, there is suggestive evidence that more vulnerable neighborhoods and populations are more affected by heat stress. However, preventive health policies may mitigate almost all of these effects: if Rio had universal primary health care coverage, these adverse effects would be completely mitigated.

This last set of results adds to a growing literature discussing the positive effects of expanding primary health care in Brazil and in the city of Rio de Janeiro (Hone et al., 2020; Mrejen et al., 2021; Castro et al., 2019; Hone et al., 2017). Since individuals with chronic diseases are more affected by heat stress (Basu, 2009) and primary healthcare coverage improves medical adherence to treatment of these chronic conditions, we show that primary healthcare access indeed mitigates the harmful effects of heat stress.

However, primary healthcare coverage may not be cost-effective for dealing with local temperature shocks. Given that heat stress at the neighborhood level matters, policymakers may design policies targeted to alleviate the negative effects of temperature shocks. An exciting avenue for future research is understanding which local policies are cost-effective to reduce the burden of heat-mortality deaths. Examples of other policies that could be analyzed are the creation of green areas, parks, cooling centers, subsidies to buy air conditioning machines or electricity bills, or urban interventions that improve the climate resilience of neighborhoods.

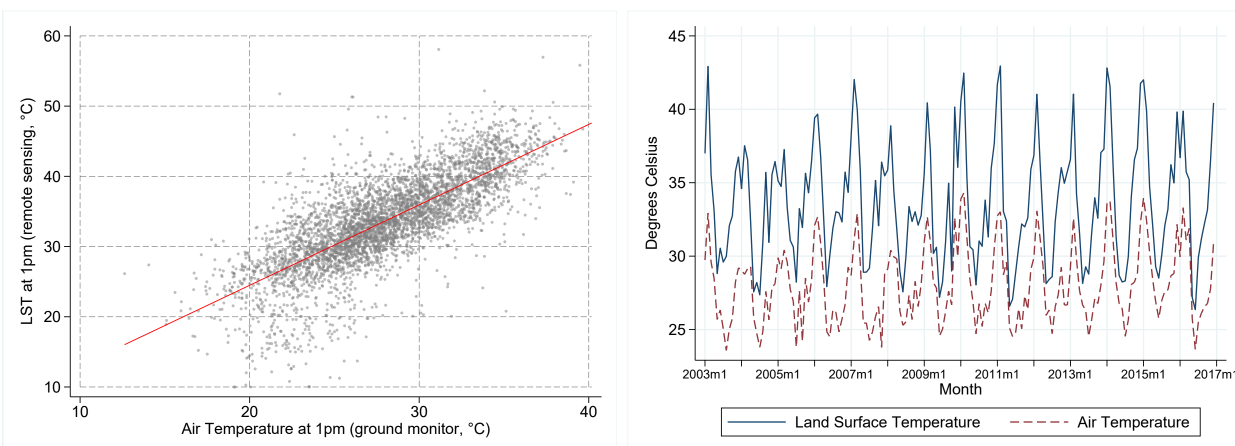
Figure 4.1: Rio de Janeiro: Land Use and Heat Map



(a) Land Use **(b)** Average LST

Data on land use are publicly available from Institute Pereira Passos data lake (data.rio). We construct average LST at the pixel level over the entire period of analysis.

Figure 4.2: LST and Air temperature in Rio de Janeiro



(a) Daily Measures **(b)** Monthly Averages

These figures use temperature data from two sources. Land surface temperature is measured by the MODIS sensor aboard of NASA's Aqua satellite, which overpasses Rio de Janeiro approximately around 1pm local time daily. We construct temperature measures by neighborhood according to the procedure described in the text. We select the São Cristóvão, for comparison with the air temperature data. Air temperature is measured by São Cristóvão's ground station, which provides hourly temperature readings. We select the 1pm reading for comparison with the land surface temperature data. Figure 4.2a shows the daily measures of LST and air temperature at 1pm. The red line represents the linear fit $\widehat{LST_t} = 1.565 + 1.156 AirTemperature_t$ ($R^2 = 0.58$). Figure 4.2b aggregates these data by taking monthly averages. In the Appendix, we provide further analysis of correlations between different temperature measures.

Table 4.1: Summary Statistics of Neighborhood-Month Panel

	Mean	sd
Panel A - Temperature Measures		
Number Days > 40 C	3.85	6.04
Degree Days > 40 C	12.9	25.0
Number of 3-day Heat Waves	0.38	0.83
Number of 5-day Heat Waves	0.15	0.47
Number of 7-day Heat Waves	0.076	0.30
Missing daily Temperature	4.24	6.52
Panel B - Health Outcomes		
Cardiovascular deaths per 100,000 individuals aged 60+	109.5	71.4
Panel C - Controls		
Share population covered CSF	0.093	0.21
Avg. Distance to ER (km)	2.51	2.17
number bus stations in the neighborhood	0.18	0.98
number subway stations in the neighborhood	0.021	0.18
number upps in the neighborhood	0.24	0.70

Notes: Data is a monthly panel of neighborhoods.

Figure 4.3: Neighborhood-Day Monthly Temperature Distribution

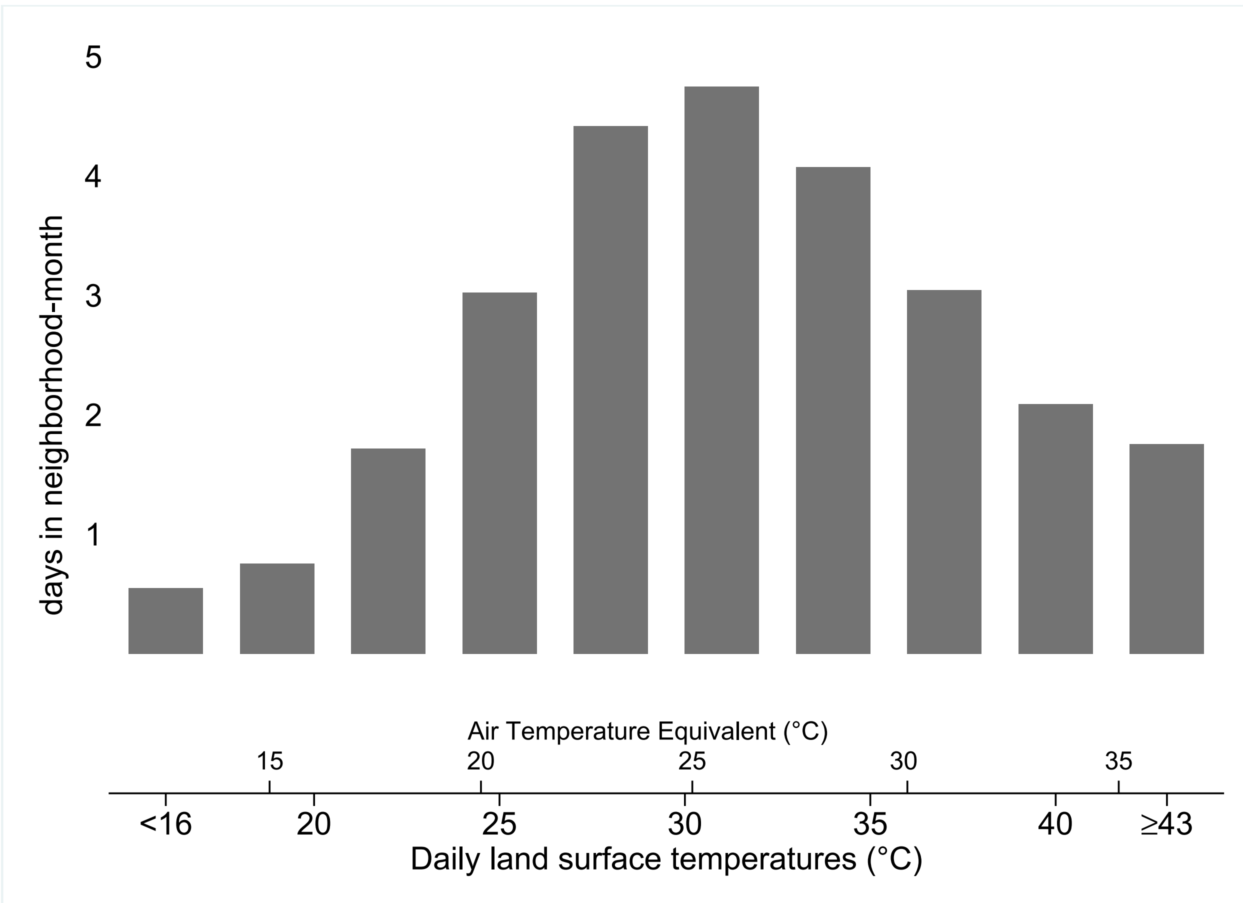
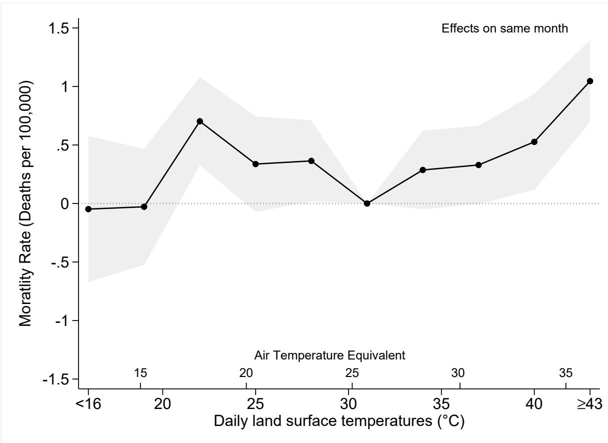


Table 4.2: Effects of Hot Days on Mortality due to Cardiovascular Causes

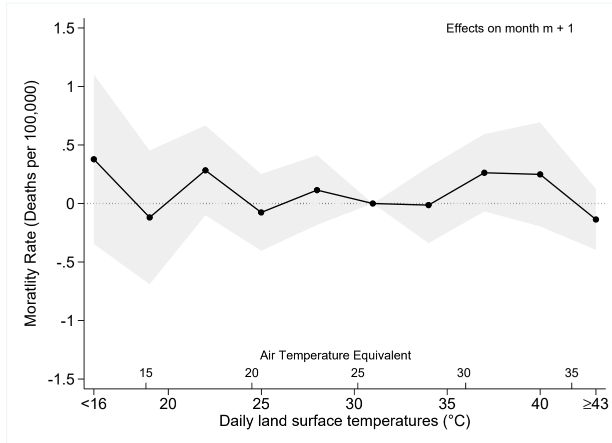
Shock	Cardiovascular Deaths per 100,000 individuals aged 60+		
	(1)	(2)	(3)
1. Number Days > 40 C	0.534	0.526	0.278
	(0.112)***	(0.112)***	(0.138)**
2. Degree Days > 40 C	0.101	0.111	0.039
	(0.023)***	(0.022)***	(0.027)
3. Number of 3-day Heat Waves	2.006	1.945	0.913
	(0.668)***	(0.667)***	(0.717)
4. Number of 5-day Heat Waves	4.968	5.163	2.486
	(0.888)***	(0.918)***	(1.021)**
5. Number of 7-day Heat Waves	5.001	5.730	1.800
	(1.165)***	(1.274)***	(1.327)
Bairro x Month	✓	✓	✓
Bairro x Year		✓	✓
Month x Year			✓
Controls	✓	✓	✓
Observations	23,016	23,016	23,016
Mean dep. var.	109.5	109.5	109.5

Notes: Data is a monthly panel of neighborhoods. Each row shows the effect of an alternative measure of a temperature shock. In row 1, the shock variable is the number of days with LST greater than 40C for a neighborhood i in year t and month m. In row 2, the shock variable is the sum of daily temperatures exceeding 40C in the month. In rows 3 through 5, the shock is the number of events with more than 3, 5, or 7 consecutive days of temperatures exceeding 40 C.
Cardiovascular are diseases coded as I00 to I99, R55, R96, G45-46 and G81-83 in ICD10.
Standard errors clustered at neighborhood level in brackets. All regressions are weighted by the population aged 60+ in each neighborhood. All regressions control for the number of daily LST missing observations in the neighborhood-month.
Linear trend refers to a specific neighborhood linear trend. Controls include the share of population covered by family health clinics (CSF), a dummy for the presence of an emergency care units (UPA), and the number of Pacifying Police Units, bus rapid transportation stations, and subway stations in each neighborhood-month.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 4.4: The Effect of Daily Temperatures on Mortality due to Cardiovascular Causes in Population 60+



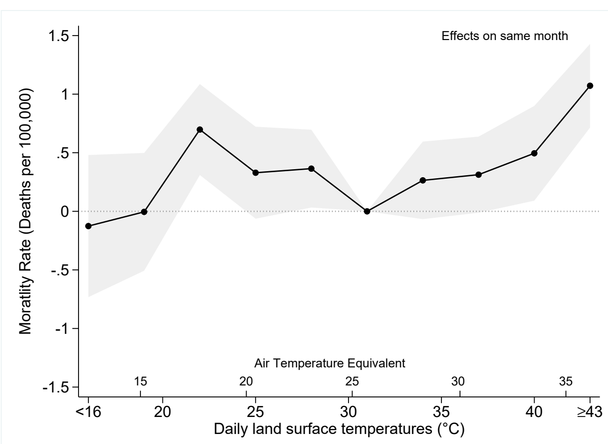
(a)



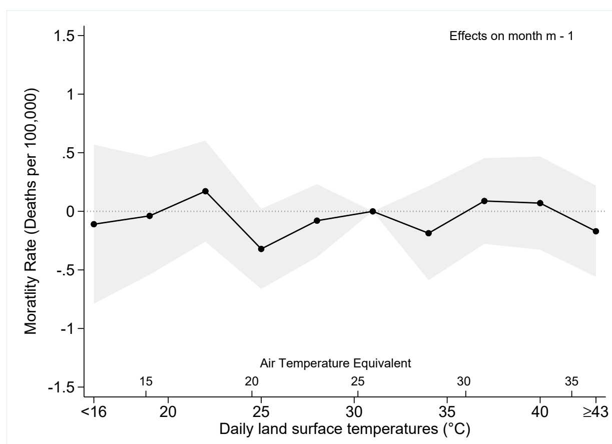
(b)

This figure plots the coefficients from the specification controlling for daily temperatures, binned into three-degree bins, in the previous month. This specification allows for displacement (or "harvesting") and delayed effects of temperature exposure.

Figure 4.5: The Effect of contemporaneous and future (placebo) temperature exposure on Mortality



(a)



(b)

This figure plots the coefficients from the specification including for daily temperatures, binned into three-degree bins, in the next month. The coefficients associated with forward-lagged temperature should have no effect on current temperature. This works as a placebo test.

Table 4.3: Differential Effects of Temperature on Mortality due to Cardiovascular Causes by Socioeconomic Measures

Shock	Mortality Rate (per 100,000 individuals aged 60+)			
	(1)	(2)	(3)	(4)
Number Days $> 40C$	0.811	1.205	0.657	3.025
	(0.202)***	(0.352)***	(0.137)***	(1.250)**
Greater 40 x inc. per capita	−0.146			
	(0.094)			
Greater 40 x % pop. greater 1 min. wage		−0.012		
		(0.006)*		
Greater 40 x % pop. greater 5 min. wage			−0.013	
			(0.010)	
Greater 40 x SES index				−4.121
				(2.066)**
Year Dummies	✓	✓	✓	✓
Bairro x Month Dummies	✓	✓	✓	✓
Bairro x Linear Trend	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	23,016	23,016	23,016	23,016
Mean dep. var.	109.5	109.5	109.5	109.5

Notes: Data is a monthly panel of neighborhods. Each column is a regression with a different socioeconomic variable interacted. In column (1) we interact the temperature shock with the income per capita of the neighborhood, in column (2) with the percentage of people who have income per capita greater than one monthly minimum wage, column (3) with the percentage of people who have monthly income per capita greater than 5 monthly minimum wages, and in column (4) with an index of socioeconomic development of the neighborhood created by Rio's City Hall from 2010 Census Data – higher value means a better socioeconomic environment.
Cardiovascular are diseases coded as I00 to I99, R55, R96, G45-46 and G81-83 in ICD10.
Standard errors clustered at neighborhood level in brackets. All regressions are weighted by the population aged 60+ in each neighborhood. All equations controll for the number of daily LST missing observations in the neighborhood-month.
Linear trend refers to a specific neighborhood linear trend. Controls include the share of population covered byfamily health clinics (CSF), a dummy for the presence of an emergency care units (UPA), and the number of Pacifying Police Units, bus rapid transportation stations, and subway stations in each neighborhood-month.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4.4: Effects of Hot Days on Mortality due to Cardiovascular Causes by Education and Race

Shock	IHS number deaths				
	Baseline	Middle School graduated	Middle School dropout	White	Non-White
Number Days $> 40C$	0.005	0.002	0.006	0.004	0.004
	(0.001)***	(0.001)*	(0.001)***	(0.001)***	(0.001)***
Year Dummies	✓	✓	✓	✓	✓
Bairro x Month Dummies	✓	✓	✓	✓	✓
Bairro x Linear Trend	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	23,016	23,016	23,016	23,016	23,016
Mean dep. var.	2.2	1.0	1.6	1.7	1.2

Notes: Data is a monthly panel of neighborhoods. Each column shows a different dependent variable. The dependent variables are the inverse hyperbolic sine transformation of the number of deaths by education or race in a *neighborhood*, *year*, and *month*. We don't have the population for these cells, such as middle school dropouts aged 60+ in each neighborhood, to construct rates per 100,000 individuals and to weight these regressions.
Cardiovascular are diseases coded as I00 to I99, R55, R96, G45-46 and G81-83 in ICD10.
Standard errors clustered at the neighborhood level in brackets. Hyperbolic inverse sine transformation was used on the dependent variables. All equations control for the number of daily LST missing observations in the neighborhood-month.
Linear trend refers to a specific neighborhood linear trend. Controls include the share of population covered by family health clinics (CSF), the average distance of each neighborhood to the closest emergency room (ER), and the number of Pacifying Police Units, bus rapid transportation stations, and subway stations in each neighborhood-month.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4.5: Mitigation Policies to Temperature shocks on Mortality due to Cardiovascular Causes

Shock	Mortality Rate (per 100,000 individuals aged 60+)		
	(1)	(2)	(3)
Number Days $> 40C$	0.481	0.319	0.470
	(0.134)***	(0.144)**	(0.177)***
Number Days $> 40C\times$ Preventive Care	-0.667		-0.660
	(0.320)**		(0.337)*
Number Days $> 40C\times$ Distance to ER		0.048	0.006
		(0.057)	(0.060)
Bairro x Month	✓	✓	✓
Bairro x Year	✓	✓	✓
Controls	✓	✓	✓
Observations	23,016	23,016	23,016
Mean dep. var.	109.5	109.5	109.5

Notes: Data is a monthly panel of neighborhoods.
Cardiovascular are diseases coded as I00 to I99, R55, R96, G45-46 and G81-83 in ICD10.
Standard errors clustered at neighborhood level in brackets. All regressions are weighted by the population aged 60+ in each neighborhood. All equations control for the number of daily LST missing observations in the neighborhood-month.
Linear trend refers to a specific neighborhood linear trend. Controls include the share of the population covered by family health clinics (CSF), a dummy for the presence of emergency care units (UPA), and the number of Pacifying Police Units, bus rapid transportation stations, and subway stations in each neighborhood-month.
* significant at 10%; ** significant at 5%; *** significant at 1%.

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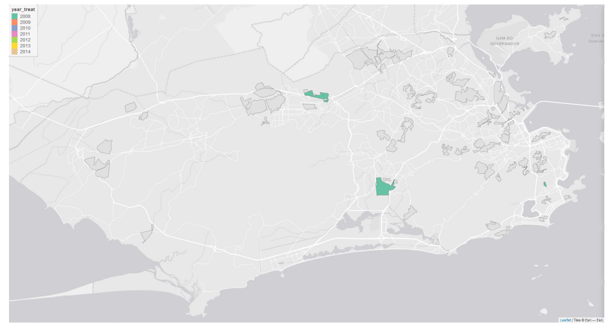
Appendix A

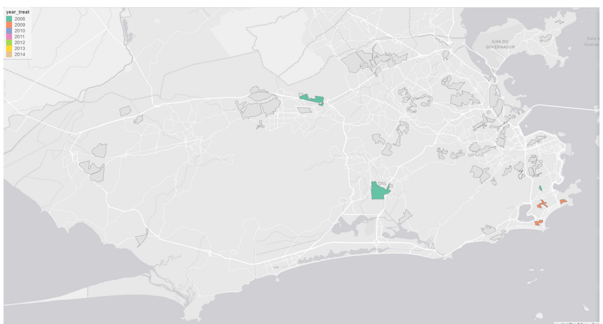
Appendix to Chapter 2

Figure A.1: Spatial and Temporal Evolution of UPPs in the city of Rio de Janeiro

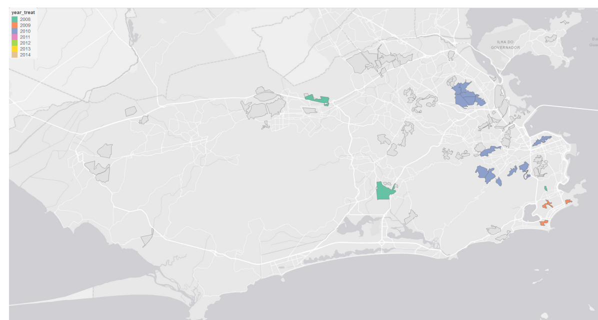
(a) Before Treatment



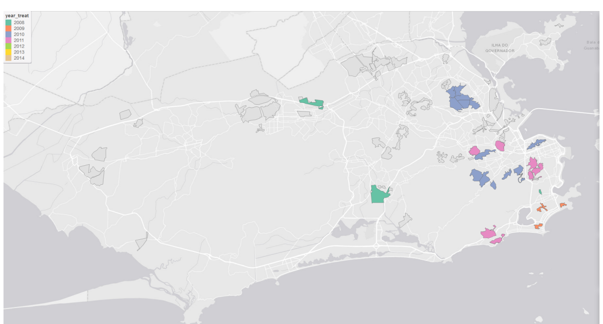
(b) 2008



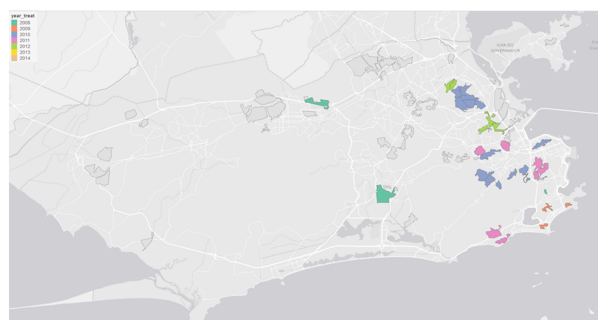
(c) 2009



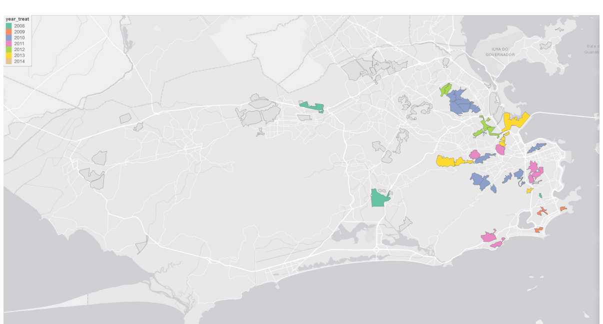
(d) 2010



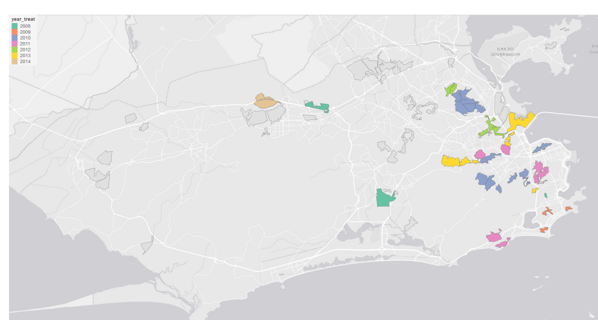
(e) 2011



(f) 2012



(g) 2013



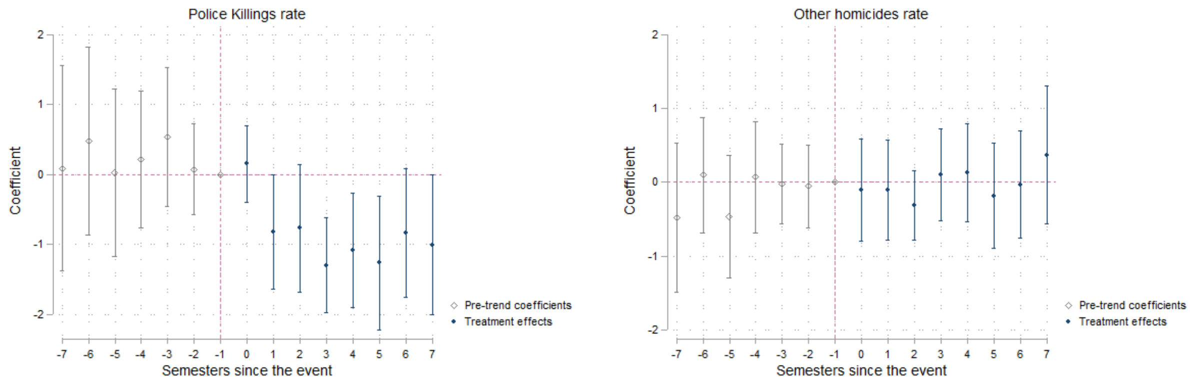
(h) 2014

Table A.1: Summary statistics violence indicators - semester rates per 100,000 individuals

Period	Mean	sd	min	p25	p50	p75	max
Panel A: 2007-2008							
Total homicides	25.4	23.2	0.0	8.8	19.0	35.6	139.2
Police killings	14.0	17.9	0.0	0.0	8.3	20.4	114.4
Other homicides	11.3	13.0	0.0	0.0	7.5	17.7	56.8
Panel B: 2009-2016							
Total homicides	10.6	16.2	0.0	0.0	5.7	14.5	127.3
Police killings	4.2	10.2	0.0	0.0	0.0	5.0	106.1
Other homicides	6.4	10.6	0.0	0.0	1.4	9.9	127.3

Notes: Table shows the summary statistics for violence indicators for two periods: before the consolidation of the UPP policy (2007-2008) and after the beginning of the program (2009-2016). The values are semester rates per 100,000 citizens. ISP-RJ defines total homicides as the sum of police killings and homicides committed by individuals, which I label as other homicides in the following tables and graphs.

Figure A.2: Dynamic effects of UPP treatment on police killings and other homicides rates



(a) Police killings **(b)** Other homicides

Notes: Figure shows the estimates for equation (2) using the TWFE estimator. I do not use Borusyak et al. (2022) estimator in these figures because the minimum effective number of observations is below the minimum recommended by the authors and the estimates may be unreliable. The dependent variables are inverse hyperbolic sine of semesters' rates per 100,000 citizens. Both regressions control for Semester and Favela fixed effects, have standard errors clustered at the favela level, and use population as analytical weights. Confidence intervals are at 95%.

Table A.2: Robustness for the effects of UPP treatment on violence rates

	Total homicides	Police killings	Other homicides
Panel A: Negative Binomial			
Treat	-0.51	-0.98	-0.18
	(0.17)***	(0.31)***	(0.16)
Incidence Ratio	0.60	0.37	0.83
Panel B: Poisson			
Treat	-0.61	-1.23	-0.24
	(0.19)***	(0.31)***	(0.17)
Incidence Ratio	0.54	0.29	0.79
Obs.	740	740	740
Semester FE	Yes	Yes	Yes
Favela FE	Yes	Yes	Yes
Mean before treat.	3.40	1.66	1.74

Notes: Table shows the results for regression equation (1) using Negative binomial and Poisson estimators. The dependent variables are counts of the events. Both regressions control for semester fixed effect, favela fixed effect, and favela population (coefficient constrained to one); and, have standard errors clustered at the favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A.3: Treated and Control clusters of favelas

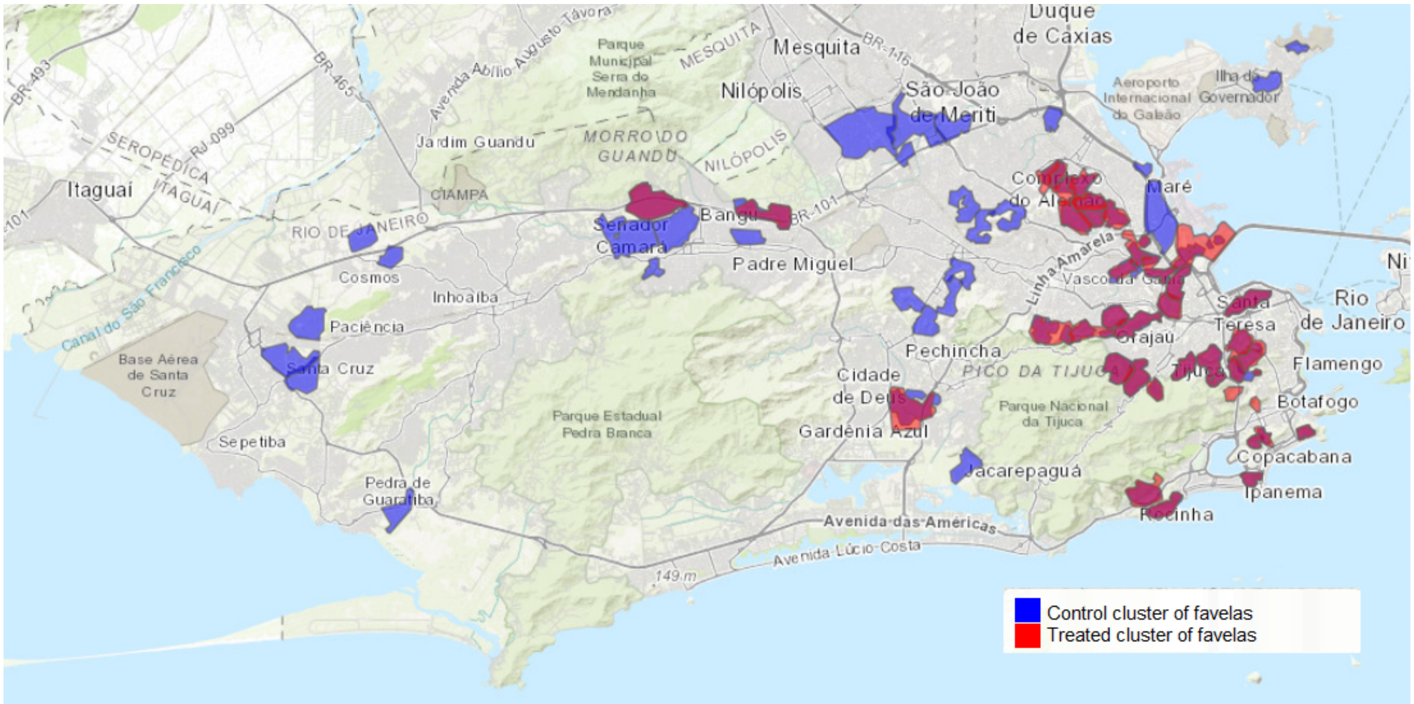


Table A.3: Socioeconomic characteristics form Census 2010 of Treated and Untreated Complexes of favelas

Variables	Mean - Treat	Mean - Control	T-test	Kolmogorov-Smirnov
Socio-devolopment index	0.53	0.52	0.19	0.47
% Inc. < min. wage	2.09	1.73	0.05	0.15
% Inc. < 2 min. wage	78.74	80.28	0.38	0.15
% Inc. > 10 min. wage	1.89	0.43	0.02	0.06
Water service	95.30	97.29	0.39	0.62
Sewage service	92.99	86.71	0.05	0.06
Garbage service	97.56	98.55	0.15	0.54
Avg. bathrooms hh	0.38	0.36	0.06	0.38
Illiteracy rate 10y-14y	2.96	2.98	0.94	1.00
Literacy rate above 5y	94.17	94.27	0.87	0.22
% Non-white	65.29	66.95	0.28	0.08
Avg. residents	22,415	25,854	0.60	0.95
# households	6,889	8,177	0.54	0.96
Avg. residents per hh	3.25	3.24	0.85	0.95
Min. distance to Olympic (km)	3.75	8.26	0.01	0.03

Notes: Table displays summary statistics for socioeconomic variables at the favela level. I retrieve the data from census tracts from Census 2010 and I aggregate at favela level by taking the census tracts in which its centroids are within a favela. For distance to Olympic venues, I geocoded the Olympic venues displayed in Towle (2013) and calculate the minimum distance of a complex of favela to a Olympic venue.

Table A.4: Summary statistics for Treated and Control schools from School Census 2007

Variables	Mean - Treat	Mean - Control	T-test	Kolmogorov-Smirnov
Computer Lab.	0.58	0.38	0.02	0.14
Science Lab.	0.12	0.14	0.68	1.00
Sports Court	0.55	0.68	0.12	0.62
Kitchen	1.00	0.99	0.38	1.00
Library	0.68	0.81	0.09	0.67
Recreation Area	0.43	0.27	0.04	0.32
Washroom outside the building	0.10	0.13	0.61	1.00
# Classrooms available	14.12	14.50	0.70	0.52
# Classrooms used	13.83	14.41	0.56	0.49
# Computers	7.08	6.33	0.40	0.31
Internet	0.97	0.94	0.43	1.00
# Employees	48.43	52.15	0.30	0.18
Teachers' office	0.87	0.85	0.74	1.00
Director's office	0.88	0.87	0.84	1.00
# Enrollment	722.78	843.88	0.06	0.11
# Enrollment Elementary school	442.52	477.37	0.48	0.49
# Enrollment Middle school	161.10	239.31	0.16	0.18
# classes	23.97	26.32	0.22	0.78
# students	716.85	842.83	0.05	0.11
Avg. class size	29.69	31.86	0.00	0.01

Notes: Table displays summary statistics for variables related to school infrastructure and composition of students from 2007 School Census. Variables in which names don't start with "#" or "Avg." show the percentage of schools in a treated or control areas that have the characteristic defined by the variable. The remaining variables are nominal values that show the average number of that characteristic in a treated or control area.

Table A.5: Differences between whites and non-white boys for socioeconomic characteristics in *Prova Brasil*

Variables	Mean: White	Mean: Non-white	T-test
Avg. Math score 2007	207.35	200.75	0.00
Avg. Reading score 2007	188.64	180.55	0.00
Avg. Math score after 2007	232.58	228.19	0.00
Avg. Reading score after 2007	210.44	205.38	0.00
Lives mother	0.90	0.86	0.00
Lives parents	0.56	0.48	0.00
Only public school	0.79	0.79	0.44
Failed	0.29	0.34	0.00
Dropped out	0.08	0.12	0.00
Mother lit.	0.95	0.93	0.00
Father lit.	0.91	0.89	0.00
Mother reads often	0.86	0.85	0.10
Father reads often	0.75	0.74	0.00
Mother above primary	0.62	0.60	0.63
Mother above high school	0.40	0.39	0.50
Works outside home	0.15	0.18	0.00
# Bathrooms	1.30	1.23	0.00
# Rooms	2.00	1.91	0.00
TV	0.97	0.97	0.45
Car	0.36	0.33	0.00
Computer	0.72	0.68	0.00

Notes: Table displays summary statistics for socioeconomic variables for white and non-white boys. I use students' socioeconomic survey to construct these variables. Variables in which names don't start with "#" or "Avg." show the percentage of students in treated or control areas that have the characteristic defined by the variable. The remaining variables are nominal values that show the average number of that characteristic in a treated or control area for each subgroup. The last column ('T-test') shows the p-value of a T-test for the mean difference between the subgroups.

Table A.6: Heterogeneity of Treatment Effects of the UPP on standardized test scores - Early vs. Late treated

	All		2009 − 2011		2013 − 2015	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Math						
Treat	0.095	0.085	0.14	0.14	0.01	0.03
	(0.042)**	(0.037)**	(0.06)**	(0.05)***	(0.04)	(0.04)
Panel B: Language						
Treat	0.065	0.058	0.13	0.12	-0.03	-0.01
	(0.039)*	(0.035)*	(0.05)**	(0.05)**	(0.03)	(0.03)
Obs.	54,879	54,879	44,402	44,402	44,700	44,700
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	All	No	All	No	All

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside the home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. Standard errors are clustered at favela level and the dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.7: Robustness of the effects of UPP treatment on standardized test scores: alternative definitions of treatment

	(1)	(2)	(3)	(4)
Panel A: Math				
Treat until 6 months before the exam	0.097	0.101	0.100	0.103
	(0.042)**	(0.037)***	(0.040)**	(0.036)***
Panel B: Language				
Treat until 6 months before the exam	0.065	0.071	0.067	0.072
	(0.038)*	(0.034)**	(0.037)*	(0.033)**
Obs.	54,879	54,879	54,879	54,879
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*
Controls	*No*	*Students*	*Schools*	*All*

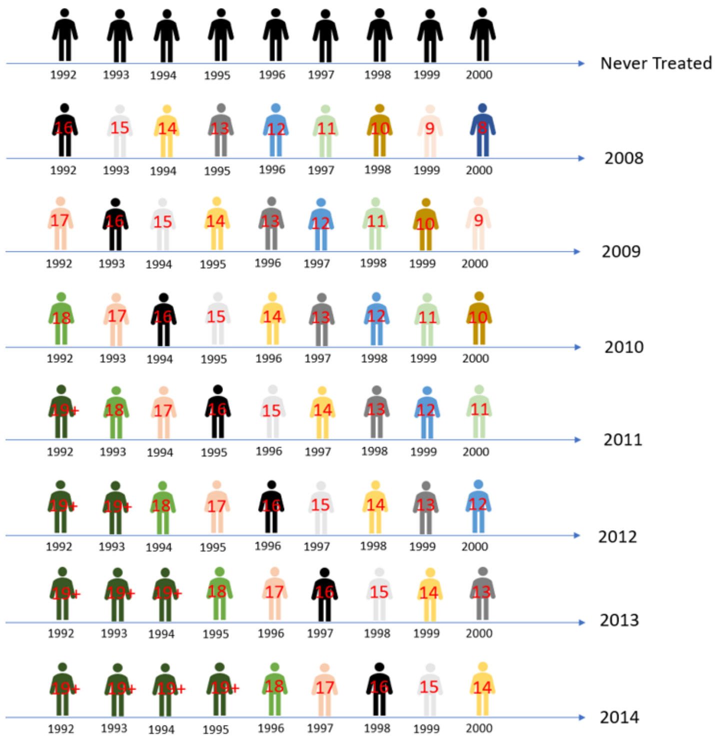
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. In this table, I consider that a school is treated in a exam wave if the favela where the school is located was treated at least 6 months before the exam. Students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. Standard errors are clustered at favela level and dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8: Robustness of the effects of UPP treatment on standardized test scores: alternative definitions of treatment

	(1)	(2)	(3)	(4)
Panel A: Math				
Treat - buffer 250m	0.067	0.069	0.067	0.069
	(0.037)*	(0.035)*	(0.038)*	(0.036)*
Panel B: Language				
Treat - buffer 250m	0.048	0.049	0.050	0.051
	(0.033)	(0.032)	(0.033)	(0.031)
Obs.	75,389	75,389	75,389	75,389
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	No	Students	Schools	All

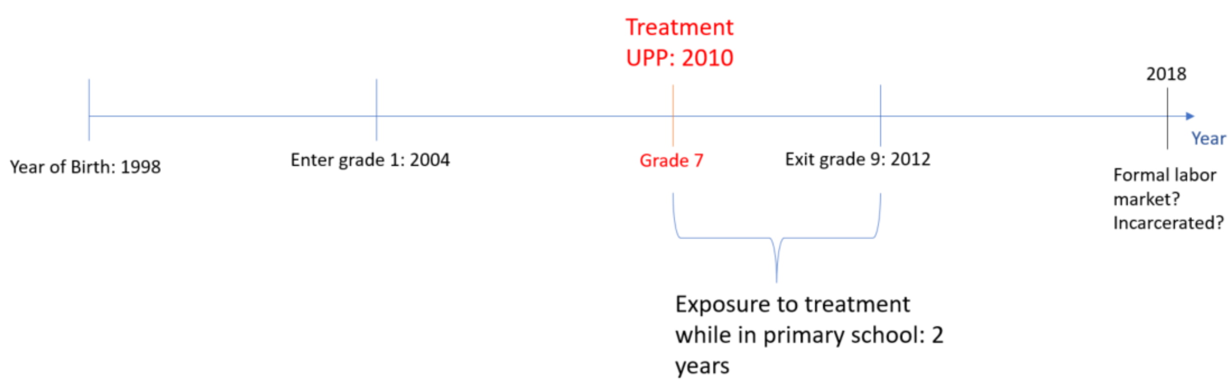
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. In this table, I consider schools that are up to 250m of distance to a treat or control favela. Students' controls include students' characteristics such as gender, race, mother's education, if lives with the mother, if the student has failed a grade or dropped out of school before and if works outside home. Schools' controls are the number of enrollments, the number of employees, the number of computers and an infrastructure index composed by the presence of a computer lab, science lab, library and sports court. Standard errors are clustered at favela level and dependent variable is standardized for each year and grade. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A.4: Age of the individual when treatment starts: variation by cohorts of birth and year of treatment in the school.



Notes: The figure displays information about treated and control cohorts and places used in the medium-run empirical strategy. The vertical variation, from "Never Treated" to "2014", represents the year when a place was treated; the horizontal variation, from "1992" to "2000" shows the year when a person was born. By combining these two pieces of information, I can define how old an agent was when treated started in the favela she lives. For example, an individual born in 1997 who lives in a favela that was treated in 2010 is 13 years old at the beginning of the treatment.

Figure A.5: Example for years of exposure to treatment in primary school



Notes: This example refers to an individual who is born in 1998 and enters primary school in 2004, at age 6. Suppose that she studies in a place that was treated in 2010. Then, she would be predicted to be in grade 7 when treatment started, and, therefore, she would be exposed to treatment for 2 years while in primary school.

A.1 Linkage

A.1.1 Conceptual Framework

- There are two sets A and B, in which each element of these sets is defined by covariates that characterize the element.
- Without loss of generality, I want to match elements of set A with elements in set B.
- Ideally, for each element of $a \in A$, I would search elements in a neighborhood of a in the whole set B. However, this is computationally intensive because I would need to calculate the distance of a to all elements of B.
- For each element of A, we define a subset of B to look for matches.

$$\forall a \in A, \mu(a) := \{b \in B; V_X(a,b) < \delta\}$$

- where, V_X is a distance function based on some covariates X and δ is a criteria/threshold defined by the researcher.
- This criteria doesn't have to be very strict.
- Now, I calculate string and other distances for each element $a \in A$ and $b \in \mu(a)$. That is:

$$\text{For each}\, a \in A, \text{ calculate}\, D(a,b)\ \forall b \in \mu(a)$$

- Define a criteria (threshold) ϵ and matching function M such that:

$$M(a,b) = \begin{cases} 1 & \text{if } D(a,b) < \epsilon \\ 0 & \text{if } D(a,b) \geq \epsilon \end{cases}$$

  Then, define:

$$M(a,B) = \sum_{b \in \mu(a)} M(a,b)$$

- Trade-off: ϵ and false-positive. If the criteria is loose, there is a higher probability of declaring a false match.
- If $M(a,B) = 1$, consider a match.
- If $M(a,B) > 1$, choose a stricter criteria, i.e., $\epsilon' < \epsilon$ until we find an unique element in

B related to a.

- If $M(a, B) = 0$, loosen the criteria, i.e., $\epsilon'' > \epsilon$, until we find a element in B that might be a possible match to a. In this case, the likelihood of being a true match is lower.

In the linkage application, I restrict the searches for individuals born in the same year and that have the same first letter of the first name. This would be analogous to selecting the δ in the discussion above. In the linkage algorithm, it is analogous to *block* the search to these two variables. Then, I calculate the Jaro-Winkler distance to elements that have the same year and the same first letter of the first name. I define very conservative criteria for a match: the observations must have a Jaro-Winkler distance above 0.95 and have the same date of birth. To run the linkage algorithm I use the package "RecordLinkage" in software R.

A.2 Panel student x year

First, I maintain only movements related to grades between the 1st grade and the 9th grade. This choice drops entries associated with Youth and Adult Education (EJA)[1] and with pre-school movements[2] The main reason for this choice is that these students attend separate classrooms with different curricula and have different time schedules than children and teenagers, and, therefore, don't give information about the composition of peers attending a school in a year that can influence the grades in standardized test scores.

Second, I calculate the number of schools that appear for a student in a year. If a student attends only one school in the year, I allocate that school to the student in that academic year. If the student has entries associated with more than one school in a year, I either use the school related to the student's enrollment in that year or, if there is no entry defining an enrollment, I keep the school with the minimum date of inclusion in the data. If there are still more than one school for a student x year, I keep the observation associated with a transfer to that school. If, after all these steps, a student appears in more than one school in a year, I randomly pick which school she attended in that year.

Then, I merge this data with the students' socioeconomic characteristics and I collapse at school x year level, creating a panel that shows the average socioeconomic composition of the schools in a year.

[1] Youth and Adult Education captures students who never attend school before or have more than 15 years old and have not completed Middle school yet.

[2] Although it is extremely important to understand if there are differences for Youth and Adult Education or pre-school attendances in treated and control areas caused by the Pacification, these questions are not the focus of the paper and I will leave the discussion for future research.

Appendix B

Appendix to Chapter 3

B.1 Corrections in police station information

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

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

I document these adjustments below.

Changes in police stations' information:

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

- Data from police station 130 is added to police station 123.

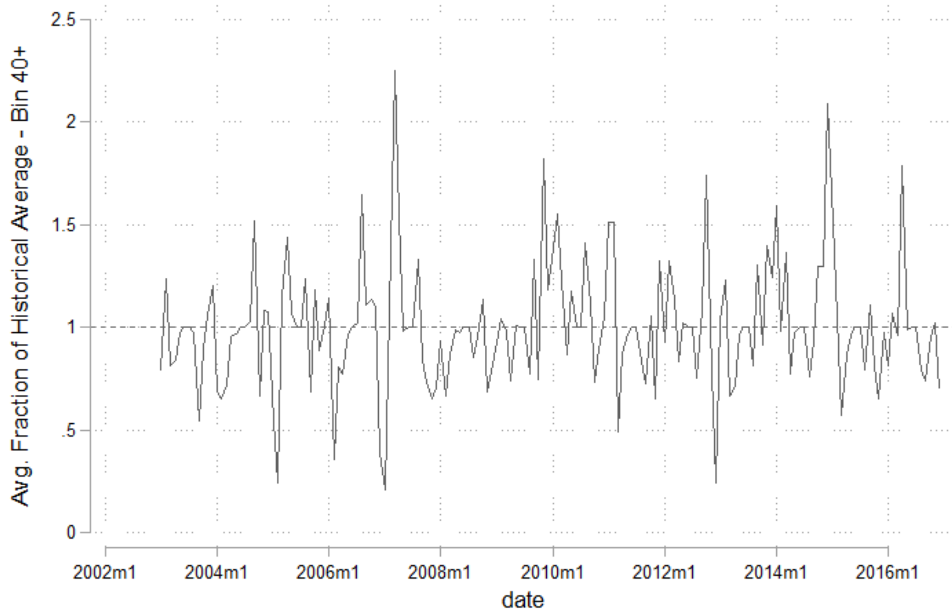
Changes in police battalions' information:

- Police station 5 belongs to police battalion 13;
- Police station 6 belongs to police battalion 1;
- Police station 7 belongs to police battalion 1;
- Police station 18 belongs to police battalion 6;
- Police station 27 belongs to police battalion 9;
- Police station 29 belongs to police battalion 9;
- Police station 31 belongs to police battalion 14;
- Police station 39 belongs to police battalion 9;
- Police station 43 belongs to police battalion 39;
- Police station 100 belongs to police battalion 28;
- Police station 101 belongs to police battalion 10;
- Police station 168 belongs to police battalion 10;
- Police station 35 belongs to police battalion 39;
- Police station 54 belongs to police battalion 40;
- Police station 111 belongs to police battalion 11;
- Police station 112 belongs to police battalion 11;

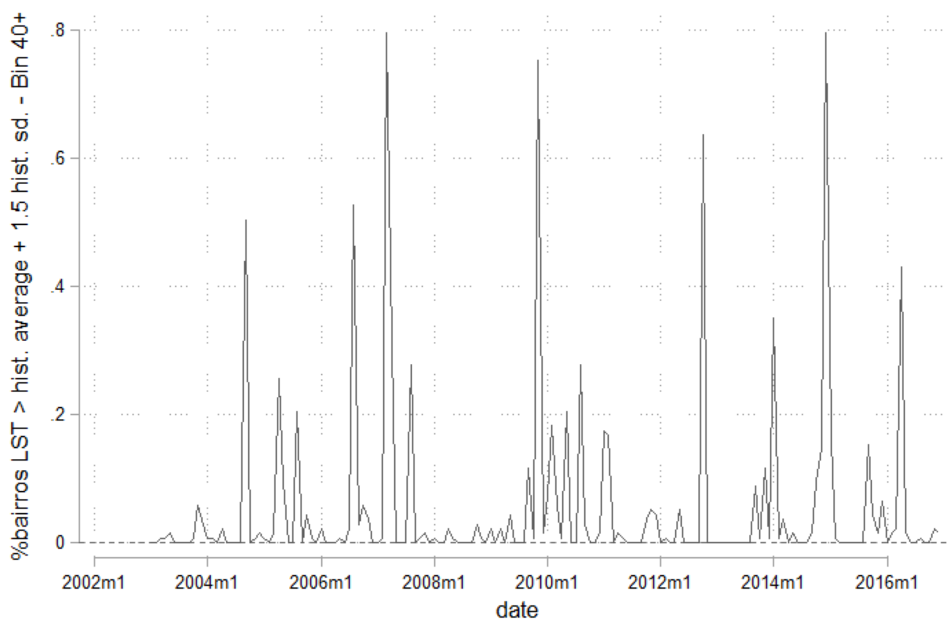
Appendix C

Appendix to Chapter 4

Figure C.1: Time-Series and Cross-Sectional Residual Variation in Heat Stress



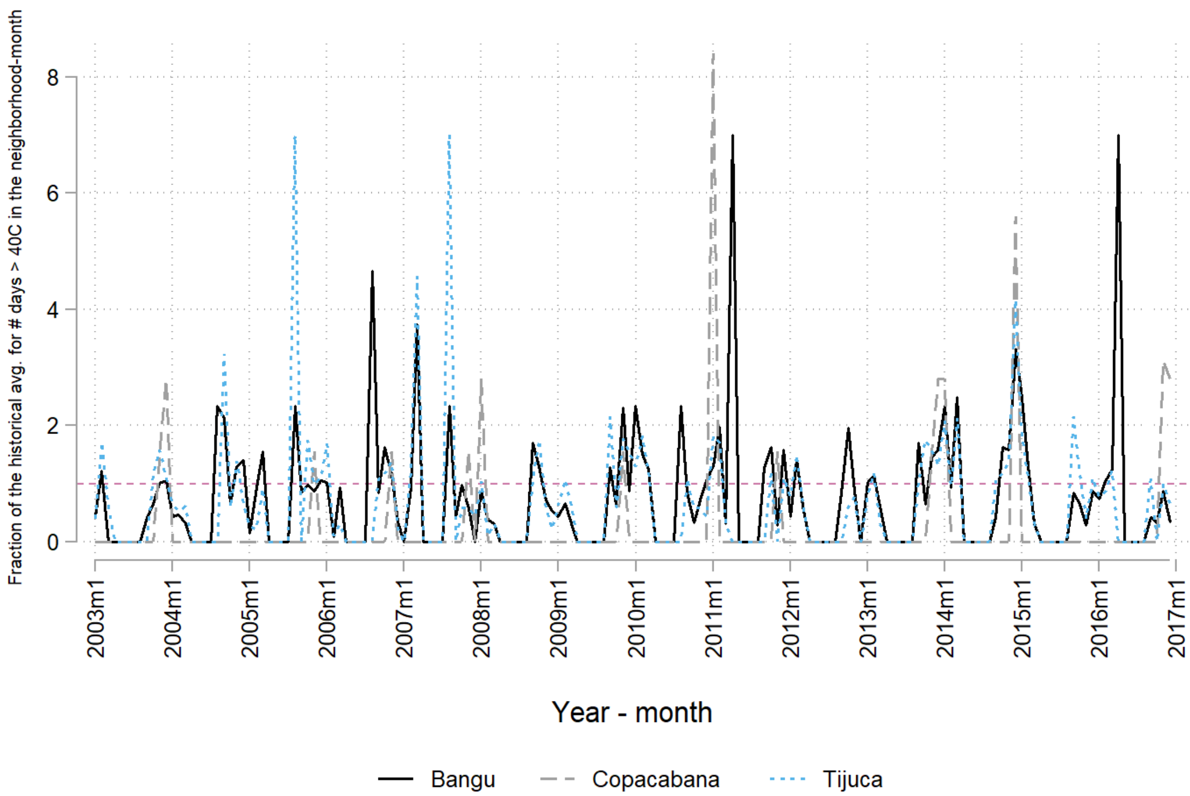
(a) Time-Series Variation



(b) Cross-Sectional Variation

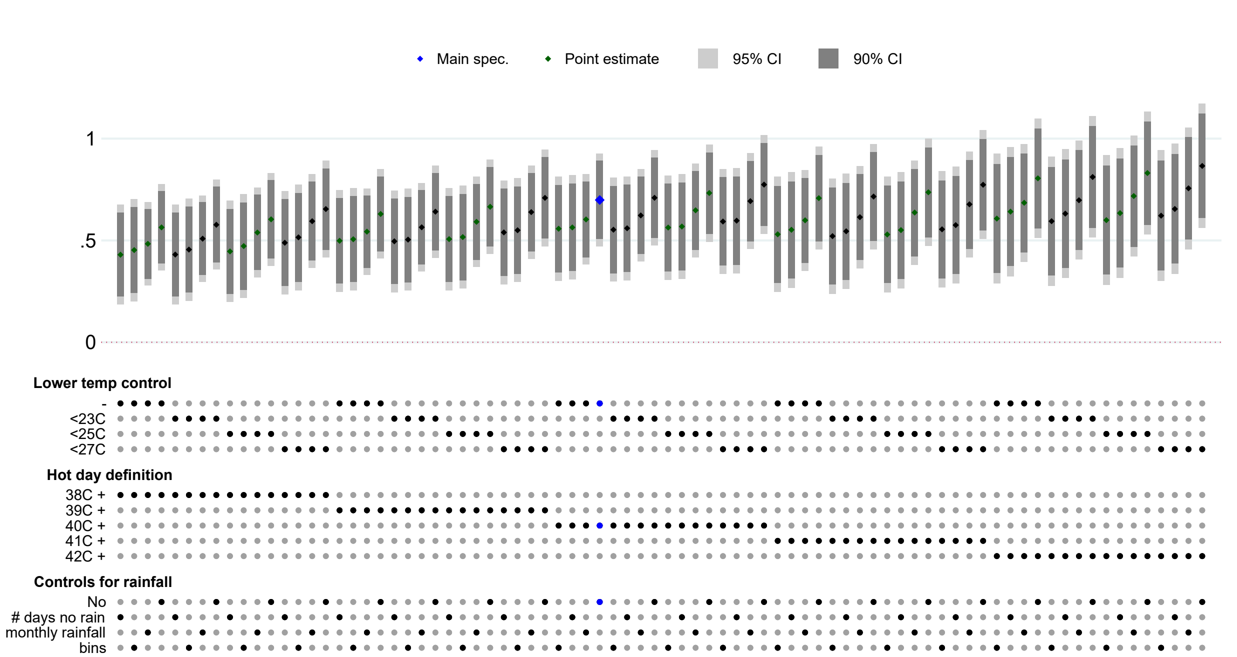
These figures show the variation of the variable Bin 40+, that calculates the number of days in a month that land surface temperature is above 40 degrees Celsius, in the time and cross-sectional dimensions. Figure (a) displays the average fraction of days above 40C in a neighborhood relative to the historical average of days above 40C in that neighborhood. A value above one suggests that in that period of time the average number of days above 40C is higher than the historical average. This figure intends to show the time-series variation in the data. Figure (b) presents the cross-sectional variation of the data. It displays the percentage of neighborhoods that have the number of days above 40C in that period time higher than 1.5 standard deviation of the historical average for that neighborhood.

Figure C.2: Time-series variation for the number of days above 40C as a fraction of the historical neighborhood-month average for selected neighborhoods



This figure shows the temporal evolution of the number of days above 40C in a neighborhood-month as a fraction of its historical average for that neighborhood-month. We present the time series for three neighborhoods in three different regions of the city and that differ in socioeconomic environment and exposure to heat stress. The neighborhood of Bangu in the West Zone of the city has the lowest socioeconomic environment and it is the warmest of the three, while Copacabana in the South Zone is the richer and coolest. Tijuca located in the North Zone stays in the middle for both variables. The goal of this figure is to show there is variation in the *timing* and magnitude of heat stress among the neighborhoods in the city.

Figure C.3: Robustness to different specifications



We show alternative definitions of the empirical specification displayed in Column 3 of Table 2. We control for different bins, alter the temperature threshold and change specific controls for rainfalls. The point estimates are fairly robust to any of these modifications.

Table C.1: Specific Causes - Age 60+

	(1) Heart	(2) Strokes	(3) Hypertensive	(4) Other Cardio
Number days $> 40C$	0.14	0.29	0.10	0.00
	(0.07)**	(0.06)***	(0.04)***	(0.02)
Observations	23,016	23,016	23,016	23,016
Bairro x Year	✓	✓	✓	✓
Bairro x Month	✓	✓	✓	✓
Controls	Full	Full	Full	Full
Mean Dep. Var.	56.50	31.41	15.70	5.875

Notes: The ICD codes for each specific cause are: heart (I20-I52, R55, R96), strokes (I60-I67, I69, G45-G46, G81-G83), hypertensive (I10-I15), other cardio (remaining of the CID letter I). The estimates are for bin 40+, which calculates the number of days with LST temperature greater than 40 celsius for a neighborhood i in year t and month m. Standard errors are clustered at neighborhood level and the regression equation is weighted by the population with 60 or more years old within each neighborhood. Full controls for specific neighborhood linear trend, family health clinics (CSF), emergency care units (UPA), the presence of Pacifying Police Units, bus rapid transportation units and subway stations in each neighborhood × month. All equations are controlled by the number of LST temperature missing observations in the neighborhood-month. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C.2: Effects of LST temperature on Cardiovascular outcomes (per 100,000 Individuals aged 60+) - Alternative Imputations

	(1) Benchmark	(2) Alt - Imput.	(3) Only Temperature	(4) Complete Cases	(5) Missing below 40
Number days ¿ $40C$	0.526	0.593	0.684	1.021	0.963
	(0.112)***	(0.110)***	(0.112)***	(0.186)***	(0.141)***
Observations	23,016	23,016	23,016	23,016	23,016
Bairro x Year	✓	✓	✓	✓	✓
Bairro x Month	✓	✓	✓	✓	✓
Controls	Full	Full	Full	Full	Full

Notes: Cardiovascular are diseases coded as I00 to I99 in ICD10. The estimates are for bin 40+, which calculates the number of days with the complete cases LST temperature greater than 40C for a neighborhood i in year t and month m. Alt-imput. uses ground-based weather observations from 11am to 3pm only, instead of 6am to 5pm as in the Benchmark. Only Temperature uses only temperature variable, Complete cases consider only raw LST data, without the imputation mechanism described in Appendix A, and Missing below 40 considers all missing observations as LST below 40. Standard errors are clustered at the neighborhood level and the regression equation is weighted by the population with 60 or more years old within each neighborhood. Linear trend refers to a specific neighborhood linear trend, Health services, and infra. controls for family health clinics (CSF) and emergency care units (UPA) and socioeconomics controls for the presence of Pacifying Police Units, bus rapid transportation units, and subway stations in each neighborhood × month. All equations are controlled by the number of LST temperature missing observations in the neighborhood-month ; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table C.3: Effects of Hot Days on Mortality by Age and Cause of Death

	All Causes	Cardio-vascular	Respi-ratory	Infec-tious	Neoplasm	Violent/ Accidental
Panel A - Infants (¡ 1y)						
Number Days > 40 C	-0.14	0.04	-0.03	0.13	-0.01	-0.02
	(0.03)	(0.04)	(0.10)	(0.09)	(0.02)	(0.08)
Mean	161.5	1.75	11.52	0.39	1.21	5.05
Panel B - 1y to 19y						
Number Days > 40 C	0.01	-0.00	-0.00	-0.01	0.00	0.00
	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)
Mean	6.13	0.18	0.28	0.43	0.08	3.34
Panel C - 20y to 59y						
Number Days > 40 C	0.04	0.00	-0.01	0.01	0.00	-0.01
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Mean	41.26	7.32	1.99	8.08	1.98	7.16
Panel D - 60y+						
Number Days > 40 C	1.55	0.53	0.27	0.03	0.04	0.02
	(0.34)***	(0.11)***	(0.07)***	(0.15)	(0.09)	(0.06)
Mean	330.7	109.5	47.25	15.20	56.26	12.84
Observations	23,016	23,016	23,016	23,016	23,016	23,016
Bairro x Month FE	✓	✓	✓	✓	✓	✓
Bairro x Year	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Data is a monthly panel of neighborhoods. Each column is a separate regression for the dependent variable expressed in the column. Each panel shows the estimates of heat stress on several mortality outcomes for that specific subsample. All regressions are weighted by that subgroup population in the neighborhood.
Cardiovascular are diseases coded as I00 to I99, R55, R96, G45-46, and G81-83; Respiratory deaths are coded as letter J; Infections are letters A and B; Neoplams are letter C, and Violent and Accidental deaths are letters V, W, X, Y in ICD10.
Standard errors clustered at neighborhood level in brackets.. All regressions control for the number of daily LST missing observations in the neighborhood-month.
Linear trend refers to a specific neighborhood linear trend. Controls include the share of population covered by family health clinics (CSF), a dummy for the presence of emergency care units (UPA), and the number of Pacifying Police Units, bus rapid transportation stations, and subway stations in each neighborhood-month.
* significant at 10%; ** significant at 5%; *** significant at 1%.

C.1 Dealing with Missing Values

There are several methods used to deal with missing values in satellite data. Kang et al. (2018), Malamiri et al. (2018), Gerber et al. (2018), Zhang et al. (2018), Kou et al. (2016), Williamson et al. (2014) and Mildrexler et al. (2011) discuss different methodologies to handle the missing observations.

We use a regression framework to impute the missing values in our dataset. The idea is to explore the time-series variation of several weather variables from the two ground-based weather stations with (almost) complete observations and create an econometric model to predict the missing values in a neighborhood i, year y and month m. The variables in these data are temperature, wind speed, wind direction, precipitation, relative humidity, atmospheric pressure, evaporation. The assumption above translates as: after controlling for these several variables, the missing values should not be systematically correlated with other variables.

Our goal is to find the set of variables and the econometric model that minimizes the average difference between the predicted values and the satellite realizations. To do that, we regress the LST vector for each neighborhood i on the set of chosen variable from the complementary datasets and use the predicted values to calculate the root mean squared errors with respect to the raw LST in that neighborhood[1].

The minimization problem is described as:

$$\begin{aligned}
&\underset{\{g_i, X_i\}_i^N}{\text{minimize}} \quad \frac{1}{N}\sum_i \text{RMSE}(\hat{g}_i(X_i)), Y_i)) \\
&\text{subject to} \\
&X_i = X, g_i = g \;\forall i \quad \text{and} \quad X \in \mathcal{X} \\
&g \quad \text{is a linear function}
\end{aligned}$$

where, Y_i is the LST time series of place i, g is the econometric model and X is the set of variables in the model. The constraints say that we are choosing the same econometric model and the same set of variables in the model for all neighborhoods. The objective function is the average of the root mean square error between the predicted model and the LST time series.

In order to solve this problem, we implement the following algorithm:

[1] Since there are missing values in the vector for the raw LST, this exercise minimizes the average root mean squared errors for the days with complete cases. An useful analogy is that the algorithm below is trained in the sample with complete observations and then creates out-of-sample predictions for the missing values.

1. Take g and X given.

2. We run N regressions, one for each neighborhood of Y_i on $g(X)$.

3. Calculate the empirical distribution of the predicted values $\hat{g}_i(X)$ and compare with the empirical distribution of LST data for the same neighborhood.

4. Calculate the RMSE.

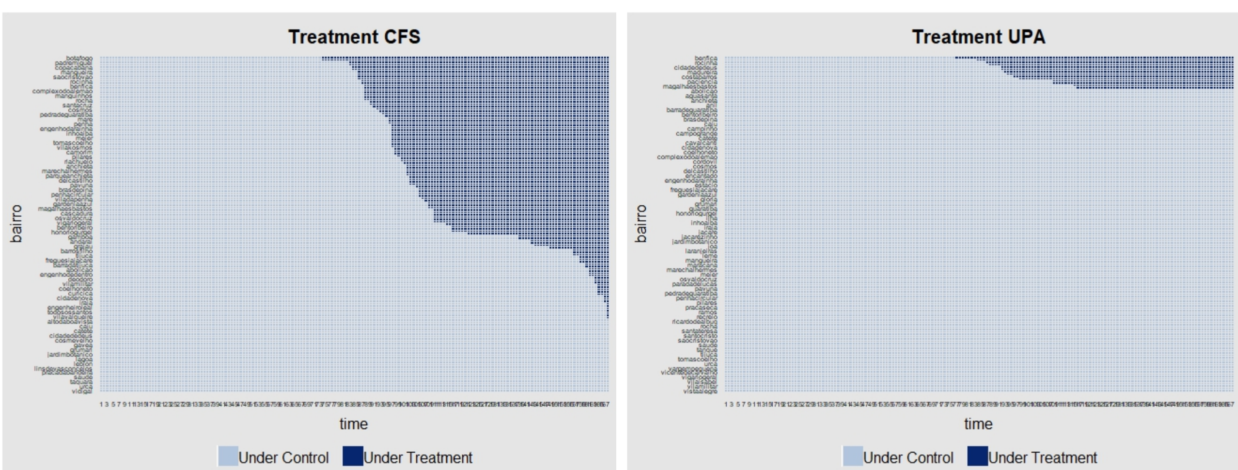
5. Go back to the first step and try different g and X until we minimize the objective function.

The chosen model uses the following set of variables: (i) from Alerta Rio System - hourly observations of air temperature, humidity, a dummy for precipitation and wind speed from 6am to 5pm and (ii) from the National Institute of Meteorology (INMET) - daily observations of maximum air temperature, minimum air temperature, average air temperature, a precipitation dummy, relative humidity and Piche evaporation.

We use two alternative models for robustness checks: one with the same set of variables but restricting observations from the Alerta Rio System to be between 11am and 3pm; the other uses only temperature variables.

C.2 Timing and expansion of health infra-structure

Figure C.4: Expansion of health infrastructure over time and across neighborhoods in Rio



(a) **(b)**

This figure shows the variation over time and across space that we explore to construct the measures of distance to ER and percentage of individuals covered by primary health care.

C.3 Neighborhoods' aggregation

In order to make our datasets consistent, we aggregated the 162 neighborhoods into 144 *bairros*. To do so, we consider recently emancipated neighborhoods as still belonging to their neighborhood of origin. We do the following corrections:

- Gericino is considered as Bangu.
- Vila Kennedy as Bangu.
- Vasco da Gama as Sao Cristovao.
- Parque Columbia as Pavuna.
- Lapa as Centro.
- Freguesia, Ribeira, Zumbia, Cacuia, Pitangueiras, Cocota, Bancarios, Jardim Guanabara, Jardim Carioca, Taua, Monero, Portuguesa and Galeao as Ilha.

The main regressions for individuals aged 60 years old or more consider only neighborhoods with population for this age group greater or equal than 500 people. Seven *bairros* are excluded: Camorim, Campo dos Afonsos, Cidade Universitaria, Grumari, Joa, Paqueta and Saude. We chose this criteria because since these neighborhoods have very low population, their mortality and hosp. were inflated.