Moving *the* Opportunity? Evidence from Pacifying Slums in Rio de Janeiro

Vinicius Peçanha*

UBC

Abstract

This paper studies the short- and medium-run impacts of a policy that decreased exposure to violence of children and teenagers in one of the largest and most violent cities in the world. I exploit the staggered introduction of a place-based policy that dislodged drug traffickers out of the slums they ruled and introduced community-oriented policing. The Pacification Police Units Program (UPP) changed an important feature of these neighborhoods by reducing homicides and police killings by more than 50% in treated areas, while arguably holding other unobservables such as social networks and interactions, and informal institutions constant. First, in the short-run, children attending schools in treated favelas perform 0.1sd better in Math and 0.07sd better in Reading in national 5th and 9th grades standardized exams. I find no evidence that this finding is driven by compositional changes for students or teachers and I present suggestive evidence of a reduction in exposure to violence *at school* and an increase in teachers' aspirations for students. Second, using individual links using administrative data in the medium-run, when children from UPP phase-in communities are in their early 20s, I find that boys who were less than 13 years old when treatment commenced in their favelas of residence are 5% more likely to be in the formal labor market in 2018 and 46% less likely to be incarcerated. I find no evidence that human capital accumulation can explain impacts on adult outcomes and conclude instead that these impacts are more likely driven by a decrease in residential exposure to drug traffickers in childhood.

Key words: UPP, place-based policy, violence exposure, school outcomes, labor market, incarceration

JEL Codes: O10, I25, D74

*University of British Columbia – Department of Economics. Email: viniciusrpecanha@alumni.ubc.ca

1 Introduction

Urban violence is one of the main concerns for citizens in Low- and Middle-Income Countries and imposes major 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 (2021a), Koppensteiner and Menezes (2021a)) and have long-term consequences in labor market, health and prison outcomes (Sviatschi (2022); Chetty et al. (2016); Damm and Dustmann (2014)). Nonetheless, little is known about the short and medium-run gains of reducing urban violence, and, more importantly, the effects of growing up in a less violent environment. 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 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 violence exposure for up to eight years in some of the most violent neighborhoods on earth on short- and medium-run outcomes. The Pacifying Police Units Program (UPP) was introduced in some favelas with the goals of ending armed control of the territory by drug gangs and introducing community-oriented police. The program was designed to curb criminal control 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. The policy was gradually expanded over time and, since the beginning of the program in 2008 and the introduction of the last unit in 2014, 38 UPPs were installed, reaching 264 communities, and almost 1.5 million citizens. I am not aware of any other place-based policy that targeted violent communities at this scale elsewhere. I exploit this unique context to make two main contributions. First, the policy changed an important 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. This allows me to more credibly identify the impact of growing up in a less violent environment *per se*. Second, by discussing the impacts of UPP on short-term schooling results, and medium-run labor market and incarceration outcomes, I can shed light on the channels that link the decrease of violence while growing up to later outcomes in life.

A major concern about place-based policy is the violation of the SUTVA (Rubin, 1986) assumption. If the treatment generates spatial spillovers to untreated areas, the estimator is biased. I overcome this issue by considering untreated neighborhoods that appear not to be negatively affected by the treatment[1] and I perform several robustness exercises to show that the results hold even after dealing with possible criminal migration stories.

I use several data pieces in this work such as violence outcomes (homicides, police killings, street thefts, and auto thefts) at the UPP 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 data about school records for the universe of students enrolled in municipal public Elementary and Middle schools from 2000 to 2014, the Brazilian Employer-Employee matched dataset (RAIS) that displays information about all formal labor market connections in the state of Rio de Janeiro, the universe of individuals in jail in 2018 for the state of Rio de Janeiro and the universe of individuals enrolled to receive social services from Federal government (*Cadunico*).

To settle the findings, first, I apply a difference-in-differences strategy that exploits the staggered introduction of the UPP program and uses not-yet-treated UPPs as the comparison groups for violence outcomes. Then, I rely on the fact the UPPs targeted large favelas in the city of Rio de Janeiro, known as 'Complexes of Favelas'. I show that some of these complexes were treated and others were not, and, importantly, UPP treatment is not systematically correlated with neither violence dynamics nor socioeconomic characteristics. If anything, treatment is correlated with distance to Olympic venues. For the short-run effects on schooling, I employ a difference-in-differences research design that accounts for potential treatment heterogeneity (Borusyak et al. (2022)) in which schools in untreated complexes of favelas are used as the never-treated units. I perform several robustness exercises to deal with the concern commonly related to place-based policies of crime displacement to other neighborhoods. Regarding the medium-run effects, I use a cohort-place fixed effects strategy (Bailey et al. (2021); Hoynes et al. (2016); Duflo (2001)), in which the individual is considered treated if she was below a certain age threshold when Pacification started in the place she lives.

There are three main sets of results: (i) UPP reduces exposure to violence and police killings by more than 50% in treated places; (ii) schools located in UPP areas perform better in national standardized exams – 0.1 standard deviation for Math and 0.07 sd for Reading, and (iii) male students with less than 13 years old who arguably live in treated areas have a higher probability of joining the formal labor market (5%) and a lower likelihood to be in prison (46%) later in their lives.

Concerning possible channels for short-run results, there is no strong evidence for changes in neither students' nor teachers' composition or infrastructure investments in treated

[1] I show these results in the first chapter of the thesis.

schools after the treatment but there is a significant increase in enrollments. I do not observe changes in approval, drop out or age-grade distortion rates caused by the UPP program. There is suggestive evidence that UPP decreased exposure to violence *within* schools by around 25% and that the program increased teachers' beliefs about future high school graduation for treated students. In terms of mechanisms that connect short-run to medium-run results, there are two possible stories: i) better results in standardized test scores could be due to an increase in cognitive abilities; ii) the decrease in drug traffickers' presence in treated communities could have a direct effect on the demand for criminal labor and an indirect effect on children and teenagers' exposure to criminal peers[2]. Both of these stories are consistent with short-run impacts on school outcomes and a better prospect of formal labor market participation and prison probability. There is suggestive evidence that the second alternative is more likely to fit the results: school results seem transitory to students who possibly take the standardized exam in both 5th and 9th grade, characteristics correlated with stronger schooling effects don't explain labor market impacts and the decrease in the likelihood to be in jail appears to affect boys regardless other characteristics they have.

This work contributes 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)). Closer to this paper, Prem et al. (2021) study the effects 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. I also contribute 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 (2021b), Ang (2021b) and Sviatschi (2022) discuss how the presence of violent episodes impact schools, health and labor market results. Finally, Ferraz et al. (2015), Magaloni et al. (2020), Lautharte (2021) and Ribeiro (2020) evaluate that impact of UPP on violence, infant outcomes, and educational results. I add to this literature by improving the identification strategy and the understanding of UPP impacts on other outcomes.

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 provide a brief discussion about how UPP impacts individuals that live in treated areas. In sections 4 and 5, I show the empirical framework and I debate the results. Finally, in section 6, I conclude the analysis and discuss future work.

[2] Individuals could spend less time 'hanging out' with drug traffickers, even if they not directly work with them, or the students could be less affected by criminal peers within school (Willadino et al., 2018).

2 Institutional Context

In this section, I discuss the criminal environment in the city of Rio de Janeiro and its changes over time, and how policymakers designed and implemented the UPP program. In Appendix C, I briefly describe the Municipal School System.

2.1 Criminal actors

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 poor neighborhoods of the city are ruled by drug factions (Magaloni et al., 2020).

In the geopolitics of drugs, Perlman (2010) argues that Rio de Janeiro is a distribution hub for exporting drugs, especially cocaine and marijuana, to Europe (via North Africa) and the US (via Miami and New York). Souza e Silva et al. (2008) posit that the cocaine drug trade was consolidated in Rio de Janeiro during the 1980s when Italian and Latin American drug dealers negotiated with Rio's drug traffickers. This process increased the profitability of marginalized territories where incipient drug trade was conducted, creating incentives for their military protection (Soares, 2005), which is in line with the theory of gang 'corporatization' presented in Taylor (1990).

Since the 1980's the city and parts of the state of Rio de Janeiro have had drug gangs operating in areas of the territory. They are located in *favelas* (shantytowns) in the hills of the city of Rio de Janeiro and poor neighborhoods in the metropolitan region of the city. According to the Pereira Passos Institute (IPP), the official research institute of the city of Rio, there were 1,018 favelas in the city in 2016. Census data (IBGE, 2010)[3] shows that almost 1.4 million citizens live in favelas, more than a fifth of the population of the city. The drug traffickers usually control the social and economic activities of these communities, impose their laws and judge conflicting cases (Dowdney, 2003). Due to the adverse geography of these places, police incursions are at disadvantage in clashes with criminals, fortifying the command of the drug gangs over the slums (World Bank, 2012).

There are three major drug gangs - Red Command (CV), Friends of Friends (ADA), and Pure Third Command (TCP) – that started acting as youth gangs (Skaperdas, 2001; Akerlof and Yellen, 1993) and, then, became criminal enterprises displaying a high degree of criminal governance in the favelas (Blattman et al., 2022) – and militia groups operating in Rio. Lessing (2015) shows that these gangs organize themselves and project their power to the favelas within the prison system. The members in the upper part of the chain of com-

[3]Census has a different methodology to define favelas as *Aglomerados subnormais*. According to this definition, there were 763 favelas in the city of Rio de Janeiro in 2010.

mand are leaders ('owners') of *favelas* and use the gangs to validate their power in these localities, create common symbols (shared beliefs), and find mutual assistance (Penglase, 2008).

Additionally, different than other prison gangs in Brazil (Hirata and Grillo, 2017), Rio's gangs don't have a vertical structure in the organization. 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. The leaders display wide degrees of autonomy and might form coalitions to protect the territories or invade other places with other leaders[4] (Dowdney, 2003)

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, 2013). Different than terrorists or insurgent groups, the gangs' leaders' objective function is mainly an economic one (Lessing, 2008, 2015). 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 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 than70%) 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 one time.

The other major group in Rio's criminal market is the militia[5]. Initially, the militia groups were formed by off-duty state officers, such as policemen 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, 2012). Different than

[4]Joana Monteiro, former president/director of the Institute of Public Security (ISP), told me that Red Command operates as a franchise company. Similar to the structure of the drug gang analyzed in Levitt and Venkatesh (2000), the franchise in Rio is given to the owner of a favela. A policeman allocated in the Intelligence Unit told me that the leaders might act as veto players if the owner of the favela violates a Red Command's 'rule' or threatens the strategic interests of the faction.

[5]The term is a translation of the Portuguese word *milicias*. Different than other definitions of militia (Jentzsch et al., 2015; Staniland, 2015), the action of *milicias* doesn't reflect responses to political cleavages or clear connections to the government such as paramilitaries groups.

the drug factions, their actions are similar to a mafia as described in Gambetta (1993), involving racketeering and 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[6].

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).

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 pacification policy by the installation of Pacifying Police Units in some favelas in 2009, 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.

The last group in this market is the Police. 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 for favela incursions when considered necessary. There are several reports of police corruption as an equilibrium solution for the game played between the drug factions and the Military Police. Lessing (2016) argues that corruption is a stable mechanism of illicit rent extraction in his model of violent corruption and Misse (2010) and Soares (2006) show that this strategy is widespread in the Military Police.

2.2 Pacifying Police Units Program (UPPs)

The Pacifying Police Units program (UPPs) was launched in December 2008 and had as its main goals to end the armed control of the territory by drug gangs and to introduce a community-oriented police[7]. The program was not focused on eradicating drug trafficking but on restoring State control in some selected favelas. Cano et al. (2012) suggest

[6] 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 2018.

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

the program was a paradigm shift in Police strategies against drug criminals. Rather than the Military Police performing armed incursions in the favelas seeking drugs, guns, and criminals, the Pacifying Police Unit program focused on establishing a permanent base of operations in the community, breaking the territorial domination of the drug gangs.

The program was gradually expanded to other slums. Figure 1 shows the evolution of the program over time. Between 2009 and 2014, 38 Pacifying Police Units were installed, and 9,000 newly trained and hired police agents[8] were allocated to the program. Official reports claim that UPP reached 264 communities and 1.5 million citizens. The rise in the number of policemen allocated in these localities implies an increase in law enforcement and, also, in the cost of doing drug business in the treated favelas (Willadino et al., 2018).

The policymakers intended to use the UPP experience as the beginning of institutional reform in Rio's Military Police (Prado and Trebilcock, 2018). The goal was to gradually replace the former 'logic of war' with police close to citizens' needs. Indeed, the UPP program introduced financial incentives to reduce police killings and homicides and encouraged social actions from police actors in treated places that would, ideally, increase citizens' trust in this 'new' police (de Souza et al., 2020).

Lessing (2017) suggests that the UPP program changed the logic of police intervention in the favelas: from unconditional to conditional repression. He argues that police actions after UPP in treated favelas focus on avoiding homicides and that State actors target law enforcement towards individuals who commit these crimes. That is, UPP introduced targeted, conditional repression of specific types of crimes instead of the previous strategy of intermittent police raids, which did not have a clear rationale.

The Public Security Secretary used two 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. Given the spatial distribution of drug trade organizations, these are loose criteria. Monteiro (2013) shows that Pacifying Police Units were significantly more likely to be installed in steeper areas with high population density. The number of days with shootings between 2003-2008, income per capita, and closeness to major highways are weakly correlated with the installation of UPPs. Besides, Monteiro (2013) shows that 11 out of 30 most violent favelas in Rio received pacifying police units. She concludes that UPPs were installed in problematic areas but affluent areas of the city.

[8] Although we cannot rule out that police agents from other places of the city and state were reallocated to the UPPs because we don't have data on that, the former Secretary of Public Security, Jose Mariano Beltrame, emphasized in a book about the program the importance of the allocation of newly hired policemen in these areas because they were not shaped by the neither the corruption's strategies nor the institutional vices of the Military Police (Beltrame, 2014). Several anecdotal pieces of evidence support the claim of the allocation of newly hired policemen in the UPPs.

Other researchers even discuss that the program was created as Rio's strategy for the mega events that would 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)). Therefore, they argue that UPP's goal was to protect World Cup and Olympics areas, and, therefore, not systematically correlated to crime dynamics in Rio's favelas[9].

In terms of drug factions exposed to this program, Misse (2011), Zaluar (2012), and Muggah (2017) suggest most of the Pacifying Police Units were installed in areas from the Red Command, which caused an unanticipated economic shock to this faction. Until 2009, Red Command was strongest in South Zone and the North Zone of Rio and most of the Pacifying Police Units locate in these areas (Rodrigues, 2013)[10]. The militias received only one UPP in their territory. Anecdotal evidence suggests that the only reason for UPP installation in the militia's area was because their members tortured journalists in this place[11].

The UPP policymakers designed a social arm of the program, called UPP *Social*. This arm would be responsible for mapping communities' demands and for 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[12], UPP *Social* never worked as initially intended[13] and very few social policies were implemented in treated areas (Magaloni et al. (2018); Dias (2017); Couto et al. (2016)).

The program gained 'hearts and minds' in the beginning. Between 2009 and 2012 there was almost a consensus about the project. After this period, ethnographic work shows an increase in police corruption, police misconduct, and a rise of clashes between police units and drug traffickers in some 'pacified' slums (Menezes, 2015; Silva (2017)). In June 2013, a citizen was tortured and killed by police officers from the Pacifying Police Unit in Rocinha. Menezes (2015) argues that this broke the consensus around UPPs and polarized opinions about the capacity of the UPP program to fulfill its objectives. Interestingly, Ribeiro and Vilarouca (2018), using survey data until 2016, showed that 3 out of 4 citizens in the treated area believed that the UPP program should continue to exist in their favelas. That is, back

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

[10] Figure A4 shows the spatial distribution of the drug factions in 2006, before the beginning of the program.

[11] https://extra.globo.com/noticias/rio/jornalistas-sao-torturados-por-milicianos-no-rio-equipe-de-dia-foi-espancada-por-7-horas-na-zona-oeste-519747.html . Accessed in September 2018.

[12] In meeting a former coordinator of UPP *Social*, she mentioned that UPP created an important political surplus and that several political actors were fighting over this. If policymakers were to concentrate the social policies in one technical area, this would make it harder for other political actors to claim part of the surplus. Then, political actors did not give political support to implement these social policies.

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

in 2016, even with the raising concerns about UPP efficiency, most citizens wished the program to continue.

It is important to note that the program had heterogeneous impacts on citizens' perceptions. 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) have a higher likelihood of approving the program while individuals in late-treated places have more concerns. They hypothesize that the rapid expansion of the program leads to problems related to its sustainability, which lowers the legitimacy levels in late-treated areas. Therefore, I should take this treatment heterogeneity into account in the empirical strategy.

A major fiscal crisis in the city and the State of Rio de Janeiro from 2015 onwards reduced the public security budget[14] (Muggah, 2017) and increased the crisis in the Pacifying Police Units program. Clashes between drug traffickers and police forces became routine again, violence returned to its pre-UPP level and trust in the police decreased again (Silva, 2017).

3 Conceptual Discussion

The UPP program is a place-based policy that altered one important aspect of treated communities: exposure to violence. While arguably keeping unobservable variables constant such as social networks and informal institutions in the favela, by reducing exposure to violence in treated neighborhoods, other observables socioeconomic factors that influence children's outcomes can change, such as income and the presence of drug traffickers in the favelas. Given that the main driver of these changes is the reduction in violence, I consider the UPP program as a bundled treatment. Although it may be difficult to disentangle all the treatment prospects, I will address some of these possibilities.

Consider a very simple framework in which an outcome y for individual i who lives in place p and goes to school in s is a function of innate ability and preferences θ_i, student's effort e_{ipt}, characteristics at individuals' household h_{ipt}, environmental characteristics of the place they live that have a direct impact on school outcomes λ_{pt} such as exposure to violence, peer effects, presence informal and formal institutions, schools quality $q_{i(s)t}$, a

[14] Muggah (2017) shows that the public security budget decreased around 32% in 2016. To avoid the fiscal crisis as a confounder of the stylized facts, we will restrict the sample until 2015.

contemporaneous effect that impacts all individuals, γ_t, and other non-observable ϵ_{ipt}:

$$y_{ipt} = F(\theta_i, e_{ipt}, h_{ipt}, \lambda_{pt}, q_{i(s)pt}, \gamma_t, \epsilon_{ipt})$$

where F is an unknown function to the researcher.

The object I am interested in: $\frac{\partial \mathbb{E}[y_{ipt}|p=\overline{p},t\geq t^*]}{\partial \lambda_{\overline{p}t^*}}$, that is, the marginal impact of changing an aspect of neighborhood $\overline{p}$ at time t^* that can directly impact students' outcomes, $\lambda_{\overline{p}t^*}$ on the mean outcome after the change on individuals who live in that place, while holding constant other factors that may influence the outcome such as household characteristics or school quality. However, altering the neighborhood environment can influence these other factors. I discuss some paths in which a decrease of violence can affect the independent variables:

First, UPP might increase parental income. Parents would have more mobility to search for jobs in territories controlled by other drug gangs, the stigma of living in a favela could be reduced because of the political discourse of 'integration' between the favela and the city, or parents could exploit jobs created in the favela because of investments that could have happened after UPP or an increase in tourism. Since parental income is correlated with school outcomes (Connolly et al. (2020); Chetty et al. (2016); Black and Devereux (2010)), students might do better at school because parental income has increased.

Second, the reduction in violence can increase the time horizon planning and induce human capital investment in cognitive skills (Prem et al. (2021); Duque et al. (2019)), Third, by decreasing the presence of drug traffickers in treated favelas, UPP might have changed the impact criminals exert on children and teenagers. It could be by reducing the labor demand for drug trafficking or by altering the symbolic role that criminals have in these places (Zaluar, 2012). Then, UPP could impact children who would join drug traffickers had the program did not happen or it could reduce the negative influence criminal peers have on other youngsters. Fourth, since UPP dislodged drug traffickers, students could have had more mobility in the territory and, then, they could chase better study alternatives such as better schools or internships outside the favela. Then, students could either change their effort or the quality of the school they attend after the treatment. In all of these cases, the program can change the effort individuals exert at school.

Fifth, by decreasing exposure to violence, children and teenagers could be exposed to lower stress levels or better mental health prospects. Studies find that stress and mental illness caused by the constant fear of violence could lead to worse school and life outcomes (Jácome (2022); Heissel et al. (2018); Sharkey et al. (2014)). Or, less exposure to violence could alter their preferences towards risk (Brown et al. (2019); Jakiela and Ozier (2019); Callen et al. (2014)). So, θ_i could also be impacted by the place-based polity. Finally, UPP can affect affect $q_{i(s)pt}$. The policy could have induced investments in school infrastructure or increased the teachers' quality in treated schools, which generate better life prospects for treated students (Rose et al., 2022).

Given these possible channels that treatment can impact the outcome, I want to highlight that although the theoretical object of interest is the partial derivative, in the empirical exercises I estimate a result more related to the total derivative of $\mathbb{E}[y_{ipt}|p = \overline{p}, t \geq t^*]$ with respect to $\lambda_{\overline{p},t^*}$. I provide evidence for changes in some of these channels to bring the empirical estimator closer to the theoretical object of interest and I comment on the limitations of my analysis.

4 Data

I employ violence data from official records, a repeated cross-section data for school outcomes, and linked data from identified administrative datasets to show that: (i) UPPs reduce violence in treated places; (ii) schools in treated areas perform better in national standardized exams and (iii) students who attended these schools have a higher probability to be in the formal labor market and a lower probability to be in prison in 2018. In this section, I provide more information about the datasets I use.

4.1 Complexes of Favelas

First, I show the main geographical definition I use to define treated and control areas. As discussed above, the UPPs were installed in large favelas in the city of Rio de Janeiro, known as 'Complexes of Favelas'. These neighborhoods congregate several smaller favelas, have a sizeable population, were plagued by the presence of drug traffickers since the 1980s, and, importantly, they were the planning spatial unit in which Rio's City Hall historically developed and implemented public policies, which engendered a common sense of belonging to the neighborhood (Matiolli, 2016). I use the untreated complexes of favelas as the geographical definition for the control group and complexes of favelas with UPPs for treated areas.

UPP and favelas boundaries The official shapefile with the precise UPPs boundaries are from the Institute of Public Security (ISP-RJ) and the shapefile for Complexes of Favelas is from MPRJ In Loco [15], a data aggregator project from Rio de Janeiro's Public Attorney

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

Office. Figure A1 and A2 show the spatial distribution of the complexes of favelas and the UPPs. There are 53 complexes of favelas in the city of Rio de Janeiro where more than 1.2 million citizens live[16]. These complexes are spread all over the city. Figure A5 displays the spatial and temporal evolution of the UPP program in the city.

Covariates I use Census 2010 data at the census tract level to construct the socioeconomic characteristics of these complexes of favelas. I define that a census tract belongs to a complex of favela if its centroid is within the boundaries of the complex. Then, I aggregate the census tracts up to the complex of favela level.

Frischtak and Mandel (2012) and Magaloni et al. (2018) argue that UPPs were installed in favelas close to the Summer Olympics' venues. To test this hypothesis, I geocode the Olympic venues from a map made by Towle (2013) and calculate the minimum distance from a favela to an Olympic venue. The average distance from a treated complex of favela to an Olympic venue is 3.5 kilometers while untreated favelas are more than 8 kilometers distant on average.

Table A1 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 and ii) the number of residents who earn more than 10 times the minimum wage. Although the share is small (less than 2%), treated favelas have almost 4 times more citizens earning more than 10 times the minimum wage than untreated favelas. This happens because some of the treated favelas are located in the richest zone of the city. The average income in favelas in this area is higher than in other zones of the city.

4.2 Violence

The data about crimes comes from the Institute of Public Security (ISP-RJ). ISP-RJ consolidates crime incident information and makes the data monthly available at two spatial dimensions. First, at the UPP level, the Institute provides crime and population variables from 2007 to 2019. Second, at the police precinct level, a more coarse geographical unit, data is available from 2003 onwards.

To show that UPPs decreased violence in treated areas and that these areas did not display an increase in violence before the beginning of treatment, I use data at the UPP level. For this exercise, I define the timing of treatment as the month when the favela was occupied by the army and the police and I exploit the timing of treatment of each UPP compared to

[16]I calculate this number using Census 2010. I collapse census tracts at complexes of favelas level and, then, I sum the number of residents in each complex of favela.

the not-yet treated places[17]. Figures A6 to A9 display the time series for crime outcomes per 100,000 citizens for areas with UPPs and for the city of Rio de Janeiro for 2007 to 2019. I choose this time range to show that crime outcomes perform a U-shape evolution over time: they decrease between 2009 to 2015 and increase after this period.

There are 37 UPPs in the city of Rio de Janeiro. Using the 2010 Census, the population in treated areas is 658,699 citizens, more than 10% of the population of the city of Rio de Janeiro. The monthly average homicide rate per 100,000 citizens before the beginning of the program, the sum of homicides and police killings, was 4.23 for treated places and 4.19 for the city of Rio de Janeiro, while the monthly police killings rate was almost twice in favelas with UPP than in the city of Rio: 2.34 and 1.06 per 100,000 citizens respectively. Table A2 displays the summary statistics for violence outcomes for periods before and after treatment and the places with UPP and the city of Rio de Janeiro.

4.3 Educational outcomes

In this part, I present the data I use to estimate the impact of Pacification on school outcomes. There are two main sources of information I employ for this goal: data that comes from SAEB/Prova Brasil and School Censuses. Besides, I use the shapefile of schools' locations from Institute Pereira Passos (IPP), Rio's City Hall Research Institute, to restrict the sample to schools that are within 100 meters of distance to a treated or an untreated complex of favelas[18].

SAEB/Prova Brasil These are national standardized exams designed by the National Institute of Educational Research from the Ministry of Education (INEP-MEC) on Reading (Portuguese Language) and Mathematics that happen every two years for students in the 5th and 9th grades studying in public schools with more than 20 students enrolled in that school-grade. The goal of these exams is to evaluate nationwide the educational performance of schools and design actions to improve learning. 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.

This data is composed of test scores and socioeconomic surveys at the students' level and of teachers' and principals' surveys that provide demographic information of these groups and that describe the learning environment at school. In particular, there are ques-

[17]In the Second Chapter of the thesis, I exploit the data from the more coarse geographical areas to construct never-treated units based on which police precinct corresponds to each of the complexes of favelas.

[18]I select this threshold of 100 meters because the focus of the paper is to explore the local effects of Pacification on school outcomes. Then, I want to select schools that are located within treated or control favelas. I allow a 100-meter buffer to deal with any geocoding issue that may arise in the spatial organization of the data.

tions in the surveys that allow me to explore the impact of Pacification on the decrease of violence within the school and to test if the composition of students and teachers change after the beginning of the policy.

Students' and teachers' identifiers do not allow me to follow either students or teachers over time. Thus, the data is a repeated cross-section of students' test scores and survey answers, i.e., for each exam wave, I observe a different set of students in the schools.

I use Prova Brasil exams from 2007 to 2015 in the main sample. There are two reasons for this time range. First, data is not consistent at the individual level for the 2005 wave of the exam and, then, I choose not to incorporate this year in the main sample. Second, as Ferraz et al. (2015) and Willadino et al. (2018) argue, the UPP program was able to successfully decrease violence until after the 2014 World Cup. Figures A6 to A9 show that violence levels return to pre-treatment in treated areas after 2015. I opt to analyze the impacts of Pacification while the policy was effective in decreasing exposure to violence and, therefore, I don't include more recent Prova Brasil exams.

I keep only schools that are within a 100-meter buffer from a complex of favela and 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. Figure A10 presents the sample of schools in treated and control areas in which I use the main empirical exercises.

I define a school as treated in that exam wave if the school is located in a favela pacified at least three months before the exam. 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 school is occupied by a UPP in an even year, the treated year for that school is the year after the occupation. For example, if UPP occupies a favela in 2010, I consider the first treated exam wave to be 2011.

The main outcomes of interest are the test scores for Math and Reading. Figures A11 and A12 show the evolution of raw data test scores for treated and control areas. To facilitate the interpretation of the estimates below, I standardized the test scores by the topic of the exam (Math or Reading), the year of the exam, and the grade of the student (5th or 9th grade), using only treated or control schools.

School Censuses These are annually updated datasets collected by the National Institute of Educational Research from the Ministry of Education (INEP-MEC) that contain information about the universe of schools in Brazil. The variables encompass school characteristics such as the number of enrollments, employees, infrastructure variables, and students 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.

The educational indicators present data for all grades, including high school grades. To be consistent with the data on standardized test scores discussed above, I drop observations related to grades in high school. Then, the final sample has information for all grades of Elementary school (1st grade to 5th grade) and Middle school (6th grade to 9th grade).

I keep the same years as the sample above by restricting the years to between 2007 and 2015. However, I exploit the fact that I have yearly information, in this case, to define a treated school as a school within a 100-meter buffer from a treated favela in that year. The year of treatment is defined by the year that the UPP was introduced in that favela.

Table A3 presents summary statistics for school infrastructure and composition of students in treated and control areas for 2007.

4.4 Medium-run outcomes

These datasets are used mainly to give information that allows me to link individuals over time and to have a good estimate of where they lived before the beginning of the program. Moreover, I use the administrative data from the Municipal Secretary of Education to test if the composition of students in treated schools changes after the treatment compared to schools in control areas.

For the linkage among the different datasets, I use students' names and dates of birth and apply the algorithm described in Appendix B. In a nutshell, I calculate the Jaro-Winkler distance for students' names and I conservatively define a match: if the Jaro-Winkler distance is above 0.95 and if the observations have the same date of birth.

Municipal Secretary of Education These administrative datasets provide information about the universe of students enrolled in municipal public schools (from pre-school to 9th grade) from 1997 to 2014 and they can be linked by a unique identifier. The first piece of data provides information about the students' and parents' names, date of birth, and socioeconomic characteristics such as age, gender, race, parental education if the students or the responsible receive cash transfer, the profession of the parents if the student attended pre-school and if the students live with their parents.

In the second piece of data, I observe the history of students' moves in the Municipal Schooling System. This 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, if a student changes classroom, goes to another municipal or private school, drops out, or enrolls in the same school for a new academic year, I observe this movement as a new observation in the data.

Composition of students in a school x year I employ this second piece of data to construct a dataset that shows the school attended by the student in each academic year[19]. In Appendix D I discuss how I construct the data. With this data, I can analyze the average composition of students in each school x year and test if there is any change caused by the treatment.

I also characterize the students who move in and out of treated schools after treatment and the students who stay in a treated school throughout the treatment period. Then, I can test if the students who are moving out are different than the students who stay and how different the students who transfer to a treated school are.

RAIS – Brazilian Matched Employer-Employee The RAIS data captures the universe of formal labor market relationships in a year. I have access to this administrative data for the State of Rio de Janeiro in the year 2018. The data contains the name and date of birth of individuals that have a formal jobs.

Incarceration It is a 2020 snapshot of confidential data from Rio de Janeiro's District Attorney with all individuals incarcerated at that moment. The data has good quality for the names and dates of birth of the individuals. Other variables such as the crime committed or prison length are not well reported and the empirical analysis won't rely on them.

Cadunico - Social Protection Register The Single Registry (Cadunico) provides information about poor and extremely poor families in Brazil. The information is used to design and target Federal, State, and Municipal levels public policies, such as *Bolsa Familia*, an important conditional cash transfer program in Brazil. I observe individuals registered in Cadunico in 2018, their names and dates of birth.

4.4.1 Sample - Linkage

The goal is to understand how individuals that lived in treated neighborhoods around the treatment time respond differently in terms of medium-run outcomes than students who live close to control areas. To construct the sample to link with the medium-run outcome I exploit the fact that most students live a 15-minute walking distance of the school they attend. So, I use the location of the school a student attends as a proxy for the place they live. Also, students display a low probability of changing the neighborhood they live in

[19] That is, the data contains unique entries at the student x academic year x school level.

over their study years. Indeed, less than 13 % of students have information in two or more different address over their years in the Municipal School System [20].

In addition, I restrict the sample to analyze only students who have at least one appearance in a treated or control school in data after 2008, i.e., I drop students whose last appearance in a treated or control school in the data was before 2008. The idea is to use school attendance as a proxy for where students live. Then, it can be harder to use this as a proxy for students who left the data. To concentrate on children, I keep students who were born between 1992 and 2000[21].

One may wonder why I choose 2008 as the threshold for the beginning of the program for all treated units in this exercise. The objective here is to use school attendance as a proxy for where students live. The main reason to use 2008 as the threshold is that some students finish 9th grade and, therefore, leave the sample, before the beginning of the treatment in the locality. However, by the hypotheses and facts above, it is very likely they still live in the treated neighborhood.

With this set of restrictions, 74,123 students appear in treated or untreated schools after the beginning of the UPP program: 27,559 attend one of the 17 treated schools and 46,564 in 20 untreated schools[22]. I find around 30% of these students in the formal labor market in 2018, 3% in jail in 2018, and 52% registered in the Social Protection Register (Cadunico).

5 Empirical Framework

In this section, I present the empirical strategies for the impacts of UPP on violence, school results and medium-run outcomes. I exploit the staggered introduction and the presence of not-yet-treated and never-treated units to use a Difference-in-Differences estimator for the two initial specifications, and, for the medium-run outcomes, I adopt a two dimensional cross-section estimator in which cohort by year of birth and neighborhood where the student lives define the two dimensions explored.

[20]To construct this share, I rely on the fact that any information change related to school or address engender a new observation in the dataset that contains students characteristics. I clean the address variable and I analyze how many different addresses a student has over time. Even after the variable is clean, the same address can be written differently for the same student if she has a new entry in a different school, for example. I code this as a possible address change. Therefore, the 13 % is an upper bound for the share of students who have more than one address over their study cycle in municipal schools.

[21]I will finish geocoding students' addresses soon. Then, I will restrict students who live in a treated or control favela.

[22]Due to computational reasons in the linkage process, in this draft, I restrict the sample even further and I consider only schools who appear in all exam waves for 9th grade. That is, I do not consider schools that appear in all waves for the 5th grade. I will include the whole sample in the next draft.

5.1 Violence

Estimation Strategy

The impact of the UPP program on violence levels in treated places can be seen as the 'first stage' of the estimation. Had not the program reduced violence exposure, I couldn't hypothesize that short or medium-run impacts of the policy would be through violence reduction. Ideally, I would have event data for all – treated and untreated – complexes of favelas in the city of Rio de Janeiro. However, the lowest geographical level for official crime data is at the police station level, a coarser unit than complexes of favelas. The Institute of Public Security (ISP-RJ) provides crime statistics at the UPP level, that is, only for treated complexes of favelas. Therefore, I am able to use not-yet treated units as the comparison group.

The Two-Way Fixed Effects estimation equation is:

$$Y_{it} = \alpha + \lambda_i + \delta_t + \beta D_{it} + \epsilon_{it} \tag{1}$$

where, i denotes the UPP and t month; λ_i and δ_t are the place and time fixed effect, respectively. D_{it} is a dummy that turns one for months after the month 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 hypothesis that $\beta \neq 0$. The standard error ϵ_{it} can be to correlated to other observations within the same UPP, i and, therefore, they are clustered at UPP level.

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

$$Y_{it} = \alpha + \lambda_i + \delta_t + \sum_{\tau=-12}^{-2} \gamma_\tau D_{i\tau} + \sum_{\tau=0}^{12} \beta_\tau D_{i\tau} + \epsilon_{it} \tag{2}$$

where, $D_{i\tau}$ is dummy that turns one if $t = T_i^* + \tau$ and T_i^* is the month that UPP started in favela i. 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 implementation of the program. If the program was able to reduce violence in treated places, I would expect the lag coefficients to be negative. The leads show the effects of the program 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 leads coefficient are not statistically different than zero.

As I discussed before, there is enough qualitative evidence to be concern of heterogeneous treatment effects *a priori*. Thus, I also evaluate the impact of the program using the estimator proposed by Borusyak et al. (2022).

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 effect only non-treated observations. The next step is 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 actually test if pre-trends estimates are different than zero, while other estimator rely on placebos to shed some light on pre-trends.

Identification In this empirical exercise, due to data restrictions, the sample is composed only by ever-treated units. Thus, any selection concern such as treatment related to the most violent places is not a first order problem. The identification relies on the parallel trends and no anticipation assumption. That is, hadn't the treatment occur, the trajectory of treated areas would follow a similar path than not-yet treated units.

To identify the treatment effect, I exploit the fact that the timing of pacification was arguably exogenous, once controlled by UPP fixed effect. First, there was no official disclosure of information about potential treated areas. Second, the date of occupation was released to the public only a week before the beginning of the treatment.

Threats to identification

The main threat to identification is the violation of SUTVA (Rubin, 1986) due to spatial spillovers to not-yet treated units. This could occur if, for example, drug traffickers migrate from treated areas to not-yet treated neighborhoods, and violence levels increase in their new location. I extensively discuss this possibility in the first chapter of the thesis. I don't observe displacement of crime to other areas in the city of Rio de Janeiro[23].

Another concern may be raised if treatment is correlated with a contemporaneous violence shock. That is, if treatment time is correlated with increase in violence before treatment caused by a temporary shock. Given the focus on pacifying areas close to Olympic venues, I shouldn't expect that this would be a concern *a priori*. Moreover, drug traffickers in not-yet treated area tended to reduce the display of violence after the beginning o the program aiming to decrease the probability of receiving the UPP in their favelas (Cano

[23] As a suggestive evidence, figures A6 to A9 show that violence indicators also decrease in the city of Rio de Janeiro.

et al., 2012). In any case, if the introduction of a UPP in a place correlates with an increase in violence before, the pre-treatment estimators in an 'event study' estimation strategy would be significant, an hypothesis that I test.

5.2 Educational outcomes

Estimation Strategy I employ the same estimators as above with an important difference that, in this empirical exercise, there are never-treated units defined by untreated complexes of favelas. Also, I observe the data at student level. Then, the Two-Way Fixed Effects estimation equation is:

$$Y_{isjt} = \alpha + \lambda_s + \delta_t + \beta D_{isjt} + \phi' X_{isjt} + \epsilon_{isjt} \tag{3}$$

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 for 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 hypothesis that $\beta \neq 0$. The standard error ϵ_{isjt} are robust to correlations within the same complex of favela, j and, therefore, they are clustered at the complex of favela level.

The treatment occurs at the complex of favela dimension and I define a school to be treated if the occupation happens until three months before the exam, that usually happens in November. If the school is treated less than the three months to the date of the exam, the school is considered treated in the next exam wave. I perform robustness exercises that change this definition and shed light on the intensity of treatment by exploiting the differential impact for schools that were treated in the year before and in the same year of the exam.

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

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

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 same concerns of treatment heterogeneity and possible biases caused by this apply here as well. Thus, I also calculate the impact of the program using Borusyak et al. (2022) imputation estimator. I accommodate the repeated cross-section nature of the data, that is, different samples of students over time who belong to the same school, by estimating 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 school level, $\alpha_{(s)i}$. For the main empirical exercises, I also perform robustness tests using De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

Some of the educational outcomes are available only at school level. For these cases, I run the regressions at school level, controlling for school and time fixed effects, weighting by the number of students who take the exam, and clustering the standard errors at complex of favela level.

Identification To estimate a true causal parameter with the estimators discussed above, educational outcomes in treated places would have followed the same common trend as never-treated and not-yet treated units if these treated units hadn't been treated.

Threats to identification I observe the same threats of identification for educational outcomes as for the section before. Violation of SUTVA (Rubin, 1986) due to migration of drug traffickers to other not-yet and never-treated favelas, contemporaneous shocks that lead to an intervention and selection into treatment.

5.3 Medium-run outcomes

I exploit the variation induced by the staggered introduction of the program and how old individuals were when Pacification Police Units were introduced in the favelas they lived. The timing of individual exposure is plausibly exogenous after controlling cohort and place where students live fixed effects. I employ a cohort-place fixed effects strategy (Bailey et al. (2021); Hoynes et al. (2016); Duflo (2001)), in which the individual is considered treated if she was below a certain age threshold when Pacification started in the place she lives.

Note that, since UPP is a place-based policy, there is no age threshold for individuals in treated areas to be treated. However, based on an extensive literature of place-based effects (Chetty et al. (2016); Sviatschi (2022)), I hypothesize that younger individuals are more affected policies that change the local environment. I, first, I assume a age threshold of 13 years old, that is, if a student is below 13 years old is it 'eligible' to the treatment, while older individuals are not. I show how the results change if different thresholds and I also perform an 'event-study' type specification in which I discuss how the effects change by age when treatment started.

The identification assumption is that in the absence of the program, potential outcomes for cohorts in treated and control areas would evolve similarly. Therefore, the analogous pre-trends for this assumption is that older individuals in treated areas behave similarly as older citizens in control areas.

Estimation Strategy The main econometric specification is:

$$Y_{icp} = \alpha + \delta_p + \lambda_c + \beta \mathbb{1}\{p \text{ is treated}\} \mathbb{1}\{age \leq 13\} + \epsilon_{icp} \tag{5}$$

where, i indexes the individual, c cohort (year of birth) and p the favela where the person lives; age refers to individual's age when treatment started in that favela, δ_p is a favela fixed effect and λ_c is a cohort fixed effect. The standard errors are clustered at favela level.

Additionally, I estimate a more flexible specification that sheds light on the "pre-trends" assumption:

$$Y_{icp} = \alpha + \delta_p + \lambda_c + \sum_{\tau=8}^{12} \delta_\tau \mathbb{1}\{p \text{ is treated}\} \mathbb{1}\{age = \tau\} + \sum_{\tau=14}^{19+} \beta_\tau \mathbb{1}\{p \text{ is treated}\} \mathbb{1}\{age = \tau\} + \epsilon_{icp} \tag{6}$$

where, age is age individuals are when treatment started in the place they live. The omitted category is agents with 13 years old when treatment stared. Then, δ_τ and β_τ capture the impact of being age τ in the beginning of the treatment relative to be 13 years old in treated areas.

Figures A17a and A17b show graphically the variation in treatment age when treatment started in the favela the individual lives. 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 was born in 1998 and lives in a favela that was 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.

It is possible that treatment effects are heterogeneous by cohorts, which could invalidate standard interpretations of TWFE estimators (De Chaisemartin and d'Haultfoeuille, 2020). Borusyak et al. (2022) estimator deals with the potential pitfalls caused by treatment heterogeneity and allows the possibility to estimate two-dimensional cross-sectional empirical specification. Moreover, I can test if older cohorts in treated places evolve similar to untreated places. Interestingly, I can re-define treatment in equation (5) to be in Borusyak et al.'s terms. Let p indexes a complex of favela and c the cohort defined by year of birth. Define treatment $D_{pc} = \mathbb{1}\{c + 13 \geq T_p\}$, where T_p is the year when treatment started in place p.

6 Results and Discussion

6.1 Short-run outcomes

6.1.1 Violence

Table 1 shows the effects of UPP on several crime indicators. In the preferred specification, the UPP program reduced the monthly homicide rate per $100,000$ citizens to more than 50%. Police killings have an even bigger decrease: 60%. I also observe a strong reduction in auto thefts in treated places.

Figures 2 and 3 display the dynamic Difference-in-Differences estimation. There is a sustained decrease in homicides, police killings, and auto theft after the introduction of the UPP program compared to not-yet-treated areas. Importantly, there is no evidence of differential trends in these outcomes before the beginning of the treatment. The results are robust to the estimator I use.

In the Second Chapter of the thesis, I provide a thorough discussion about the impact of UPP on crime outcomes and I address possible concerns about spatial spillover to untreated areas.

6.1.2 School outcomes

Standardized test scores Table 2 presents the main results for the impact of UPP on Math and Reading test scores. The Pacification causes an increase of 0.1 standard deviation for the Math exam and 0.07 standard deviation for the Reading. These results are in line with the literature that discusses crime prevention strategies at the school level (Monteiro and Rocha (2017);). The point estimates are quite robust to the introduction of students and school covariates.

The preferred specification is column (1), in which I don't control for covariates, for two main reasons: (i) treated and untreated favelas and schools in treated or untreated places are already similar in almost all of the covariates, as shown in tables A1 and A3, and (ii) *a priori*, the composition of students or 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 case, school and student covariates would be *bad controls*, that is, the UPP policy can directly influence these variables.

Figures 4 and 5 exhibit the estimation for periods before and after treatment for TWFE and Borusyak et al.'s estimator. 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. Due to the plausibility of treatment heterogeneity discussed above, I prefer the DD imputation estimator from Borusyak et al. (2022) to the TWFE estimator. In any case, figure A13 shows that the results are robust to the choice of different Difference-in-Differences estimators designed to deal with treatment heterogeneity.

I provide results from split sample regressions in table 3. The point estimate for boys is bigger than for girls, although they are not statistically different from each other, and the UPP program seems to impact more white individuals than non-whites. The estimates are similar for students who take the exam in 5th grade or 9th grade, but only significant for students in Elementary school (5th grade).

However, the heterogeneity of dynamic effects by grades shows an interesting pattern. Figure A14 exhibits these results. The effects for 5th grade persist until cohorts that take the exam up to three waves after the treatment, while the impact lasts at most for one cohort after the treatment for students in the 9th grade. Although I cannot track individuals over time, students who take the exam in the 5th grade should take the exam again in the 9th grade four years after the first test. In this paper's framework, students in the 5th grade in places treated in 2009 or 2011[24] would take the test in 9th grade in the years 2013 and 2015, respectively. By analyzing the dynamic effects for 9th grade, I observe that treated places don't present differential results for exams that happen two or more waves after the treatment. The only treatment cohorts that I can detect effects of two or more waves after UPP are places treated in 2009 and 2011[25]. Moreover, these are the waves that possible students treated four years before while in the 5th grade appear as taking the 9th-grade test. That is, students who show a positive treatment effect in the 5th grade seem to have a null result four years later, in the 9th grade. I consider this empirical fact as suggestive evidence that the effect of UPP on school outcomes is temporary.

In table 4, I discuss few concerns related to the intensity of treatment. Since *Prova Brasil*, the national standardized exam, happens every two years, some schools may be treated

[24]These years refer to years when the exam happen. I discuss how I construct attribute the calendar timing of treatment to these years in Section 4.

[25]This happens mechanically. A place treated in 2009 exhibits dynamic effects for three waves after the treatment in 2011, 2013, and 2015, while for a favela that received UPP in 2011 I can note the impacts in two waves after 2013 and 2015. For the other treated cohorts, I observe at most one dynamic effect.

in the same wave of the exam but one school is treated in the year before the exam and the other in the same year as the exam. I provide evidence that schools treated for a longer period are more impacted by the UPP program. Moreover, schools in neighborhoods that had a higher reduction in violence after the treatment can be differently impacted than schools in a less peaceful environment. I use confidential police data that classifies each UPP regarding their operational risk[26].Indeed, the impacts are stronger for schools located in low-risk areas.

I address issues related to the robustness of the result in table 5. A possible concern is that the results are driven by changes in the composition of students. For example, high-achieving students move to treated schools after the treatment while low-achieving students leave these schools. I discuss the composition effects in detail below but to address this concern regarding the main regression, I winsorized the dependent variable in columns 'p10-p90' and 'p20-p80' to show that the results are not driven by the tails of students' test scores.

Another concern is related to students that attend schools in the South Zone of the city may be richer than students living in other parts of Rio. I show that South Zone does not drive the results. There might be a concern about spatial spillovers to areas ruled by the militias in the West Zone of the city. Since the UPP targeted mainly drug traffickers in the South, North, and Center of the city, the program might have created an opportunity window for militia members to expand their control over favelas in the West Zone, which could expose the students in this region to more violence. If that is the case, then, school outcomes in this region would be lower and I would overestimate the impact of UPP on school outcomes. Column "West zone" displays that the main estimates are not impacted by the exclusion of this zone of the city[27]. Finally, drug traffickers possibly migrated to the untreated Complex of Mare after the UPP installation in treated favelas (Silva, 2017). I also show that the results hold after excluding this region.

I also demonstrate that the results are robust to different standard error choices. In table A7, I use the *leave-out* method discussed in Borusyak et al. (2022) to deal with issues raised by few clusters and I cluster the standard errors at school level. Last, I present the results controlled by proxies of students' income in table A8. This table describes that even if students in treated areas are becoming richer, the results are robust to controlling by variables that proxy wealth. Then, 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

[26]Operational risk refers to perceived risk to police action in favelas due to the possible presence of drug traffickers. Then, the lower the operational risk the less likely is that a conflict will emerge.

[27]Figure A3 displays definitions of Zones in the city of Rio de Janeiro.

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 housekeeper works in his or her house. Alternatively, I construct an income index 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 the exam and predict its results. The main estimates stay similar to the preferred estimation.

Composition Figure A15 shows that the number of students who take the standardized exam grows over the exam waves for treated places relative to the control group. I investigate if this growth also changes the composition of students in treated schools. The composition of students may help me understand the reasons 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 could exploit educational opportunities outside the favela.

In tables A9 and A10 I analyze some of the students' characteristics available from the Students' Survey in Prova Brasil. The only variable that is statically and economically significant is the share of students who ever attended a private school before. After the treatment, there is an increase in 15% of students enrolled in treated public schools who attended a private school before. However, other variables that predict higher income don't seem to change. Thus, I don't find strong evidence for composition change in this data.

I further investigate this composition concern by using data from the Municipal Secretary of Education (SME)[28]. I analyze if students' composition changes in treated schools after the treatment relative to control schools. Table A12 displays this empirical exercise. I don't observe any meaningful changes after the treatment. I want to highlight, in particular, the results for columns (3) and (4), that provide the changes for students who have or whose parents have a Cadunico entry, and, therefore, can receive social benefits from the federal government such as Bolsa Familia. Using these as a proxy for income, I don't notice evidence that treated schools have poorer or richer students after the treatment.

Other outcomes It is possible to use School Census data to calculate variables related to enrollments and students' flow. I test if the UPP program caused any change in the number of enrollments, number of classes and for approval, failed, drop out and age-distortion

[28] I construct a dataset at student x school x year, in which every student is linked to the school she attended in that year, as I describe in Appendix D. Then, I merge students' characteristics and I collapse the data at the school x year level. So, I analyze composition changes in average students' characteristics in treated versus untreated schools.

rates relative to untreated schools in complexes of favelas. Table A11 displays the results for these outcomes. In line with the increase of students taking Prova Brasil, I find that the number of enrollments in treated schools increases after the UPP program. Interestingly, I don't observe changes in either drop out or age-grade distortion rates.

I also look at the number of students who are enrolled and students who left treated and untreated schools using administrative data from the Municipal Secretary of Education (SME). Moreover, this data allows me to analyze the causes of movements in and out of school. Table A13 displays the reduced-form estimation for the impact of UPP program on enrollment in and out schools and figure A16 shows these impacts over time. UPP increased enrollments in treated schools by almost 10% but the policy did not change enrollments out of these schools. Reassuringly, the results are similar to other datasets such as Prova Brasil and School Census.

In tables A14 and A15 I consider the causes of in and out-movements. Most of the in-enrollments either come from students who move from other public municipal schools or appear for the first time in the administrative data. I don't detect changes in either dropouts or transfers to other schools private or public. These results are in line with the results I find using School Census data. There is an interesting reduction in students who drop out due to working causes. On average, one student leaves every year for this cause. I find that the UPP program can almost fully mitigate dropouts by this cause. However, these cases are rare in the data (less than 1% of the out-movements), so I don't stress the result a lot.

Channels I explore some plausible channels that may explain the increase in standardized test scores after the UPP program. First, given that treated neighborhoods are less violent, better teachers may start working on treated schools. I test this hypothesis by analyzing the composition of teachers in treated and untreated schools after the treatment. I use data from the Teachers' Survey in Prova Brasil and information provided by the School Census. Tables A16 and A17 present the results. I don't find evidence for major changes in teachers' composition.

Another possibility is that schools in treated areas receive more investments. Although I don't have school investment information, I try to proxy this by using the evolution of school infrastructure over time in treated and untreated complexes of favelas. Table A18 displays theses results. There is an increase in schools with a library but a decrease in schools with computer labs after the treatment. Given the limitation of the data, I would affirm that I did not find any reasonable evidence for changes in school infrastructure after treatment.

Finally, I exploit the fact that there are questions about future students' expectations and exposure to violence in the Teachers' Survey. I find suggestive evidence that teachers' belief about future high school graduation for children in elementary school increases by almost 23% while exposure to violence within the school for older children in middle school reduces by more than 30%. These results are plausible with the story that students in older grades are less exposed to violence within the school while younger children may find a better learning environment.

6.2 Medium-run outcomes

Table 6 shows the main results for the probability of students who attend a treated school after 2008 to being in the formal labor market, in prison, or registered in the Social Protection Register in 2018 relative to a student who attended a school in a control area after 2008. I split the results by gender to test if the results are different for boys or girls. I find that boys are 5% more likely to be in the formal labor market and almost 46% less likely to be in jail in 2018. There is also a lower likelihood for girls in the treated area to be in jail in 2018.

Figures 6 displays the results by age when treatment started. In Panel A, I observe an interesting pattern for the effect: younger individuals when treatment started to have a higher probability to be in the formal labor market and a lower probability to be in jail in 2018. However, the effects on the labor market vanish if the student has more than 12 years old at the beginning of the treatment, while effects on prison last until kids are 13 years old when UPP started in their favelas. I don't observe a clear pattern for girls.

I perform split sample regressions to shed light on heterogeneities. Table A20 shows the estimates. Concerning presence in the formal labor market, the only difference is that the results seem to be driven by boys below 13 years old when treatment started in their favelas. Besides, there is no heterogeneity by race. Given that the impact of UPPs on schooling affects more white children, this can be evidence that the mechanism that links short to medium-run results is not through better school prospects or, human capital accumulation. Besides, if the mechanism was through schooling, we should expect no differential results in the formal labor market for boys and girls because both groups are positively impacted by the UPP program in standardized test scores.

The effects on prison outcomes exhibit a different pattern: point estimates are similar and significant for almost all of the specifications. The main difference is that the results for boys are 10 times larger than for girls. This fact would be consistent with the story that treated individuals, especially boys, were less exposed to the presence of drug traffickers in general, without regard to specific characteristics. This reduction in criminal presence would decrease the probability that children enroll in criminal gangs. Alternatively, drug traffickers could have left treated favelas or abandoned criminal jobs. In any case, this would reduce their criminal influence and decrease the exposure of children to criminal peers.

To further analyze this possibility, I run separate regressions for each level of operational risk a favela receives from the police before the intervention. I show these results in table A21. The estimates suggest that individuals below 13 when treatment started in a high-risk favela drive the medium-run UPP impact. These places were expected to have more drug traffickers a priori. The higher the operational risk the more exposed to drug traffickers' presence youngsters were. Therefore, after the treatment, I expect that high-risk areas would have a higher decrease in exposure to criminals. If that is the case, these results corroborate the story of the last paragraph.

6.3 Limitations

The study presents a few caveats. First, I discuss the data limitations. The identified administrative datasets are not consistent with each other: there is no common identifier for subjects, the addresses and zip codes are not standardized and the data are not cleaned. I choose to be the most conservative in the process of cleaning the data and I opt to keep only reliable information. I do not have information about high-school attendance and grades, college admissions, or teenagers in contact with the Juvenile Justice system. These pieces of information would allow me to provide a more complete view of schooling outcomes and explore other mechanisms related to criminal involvement.

Second, there is a concern about internal validity in the medium-run empirical strategy. Since UPP was a place-based policy, it treated all agents who live in a treated favela, regardless of their age. Moreover, I restrict the sample to students who were born between 1992 and 2000 and were enrolled for at least one year in a treated or control school after 2008. I choose this restriction mainly for computational reasons in the linkage algorithm. Therefore, all individuals in the treated areas in the sample are possibly treated while in Elementary or Middle School. Based on the literature that shows that younger children are more affected by policies that change violence exposure (Chetty et al., 2016) and anecdotal evidence that suggests that teenagers are the most dissatisfied with the UPP program (Musumeci, 2017), I test if younger individuals display different results than older individuals (above 13 years old). However, if older individuals are affected by the policy, SUTVA may be violated. I show that older cohorts in treated places perform similarly to control places.

Third, in the medium-run results, I define a treated person if she attends a treated school. Although most of the students indeed live within a 15-minute walking distance of schools, I could be defining students who live outside of a treated favela as treated. 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. In future work, I will expand this definition to consider a treated person if she *lives* in a treated place. I prefer to be conservative in this case to avoid any geocoding issues that could arise due to the fact the addresses are not standardized.

Fourth, for the linkage algorithm, I consider that two names match only if the names are very similar (Jaro-Winkler distance above 0.95) and agents have the same date of birth. I choose these strict criteria to minimize the chance of finding a false-positive match, even though I may exclude true matches that do not satisfy the conditions. However, there may be measurement error in the dependent variable, which can downward bias the estimates.

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 suffered from crime migration. Therefore, future research needs to incorporate these externalities and costs while evaluating UPP's impact.

7 Concluding Remarks

In this paper, I analyze the short- and medium-run consequences of a place-based public policy that reduced 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 modest higher probability of having a formal labor market job, and a significant decrease in the probability of being incarcerated. I provide evidence that a place-based policy may be a plausible policy 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 significant human and financial costs. 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 common that citizens are caught in the crossfire and several public services are 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 that it pays off.

Future research needs to disentangle if these results are driven by students who live or by students who attend a school in the treated areas but live outside the treated areas. After geocoding all the addresses, I will be able to shed light on this issue. Another interesting extension is to understand how the drug trafficking criminal workforce changed after the UPP, and, then, discuss how this change may alter peer effects within the classroom. I am assembling a Juvenile Delinquency dataset linked with administrative schooling data which will allow me to test some of the mechanisms I discuss in this paper.

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Figures and Tables

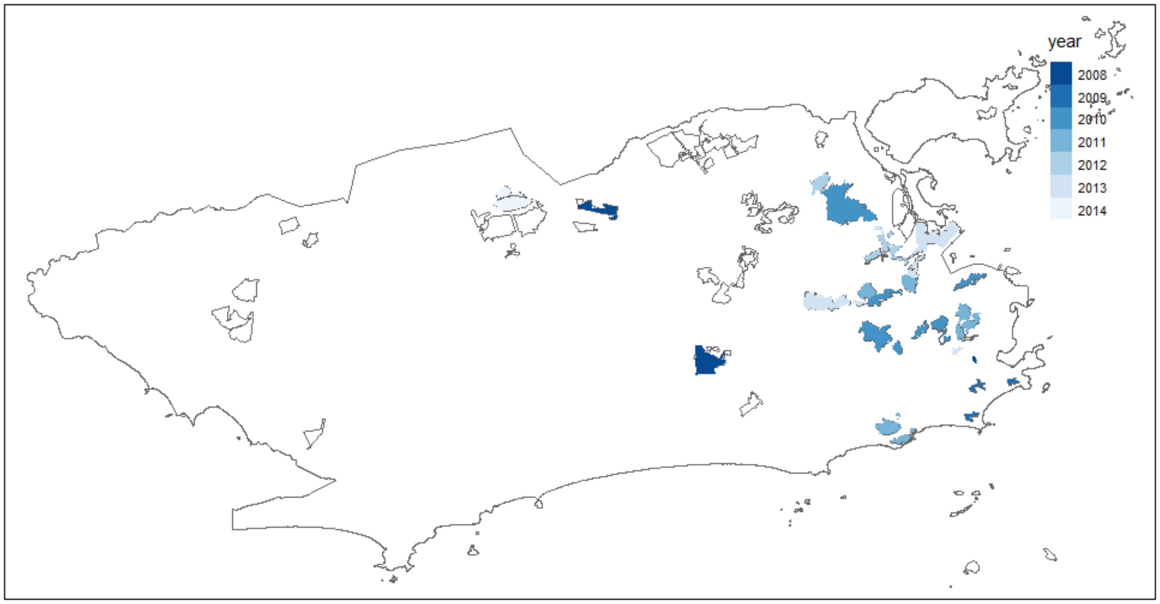


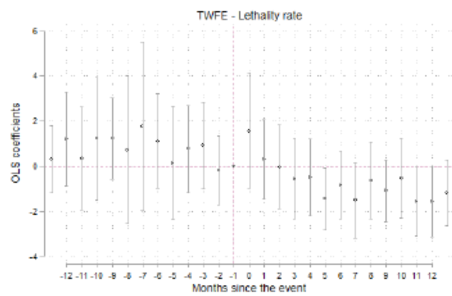
Figure 1: Complexes of favelas by treatment year

Table 1: Impact of UPP treatment on criminal outcomes - monthly rates per 100,000 citizens

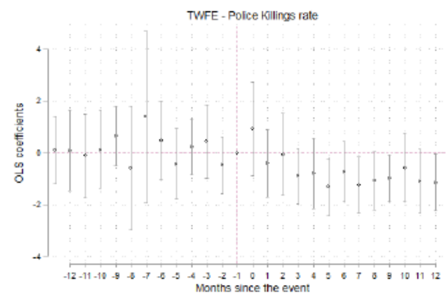
	Homicides	Police Killings	Street Theft	Auto Theft
Panel A: TWFE				
Treat	-1.34	-0.95	3.05	-1.64
	(0.44)***	(0.24)***	(1.59)*	(0.72)**
Obs.	3,996	3,996	3,996	3,996
Panel B: Borusyak et al. (2022)				
Treat	-1.70	-1.06	-1.01	-4.44
	(0.21)***	(0.18)***	(0.77)	(0.57)***
Obs.	3,182	3,182	3,182	3,182
Month FE	Yes	Yes	Yes	Yes
UPP FE	Yes	Yes	Yes	Yes
Mean before treat.	3.31	1.74	10.92	5.77

Notes: Table shows the results for regression equation (1) and for Borusyak et al. (2022) imputation estimator. The dependent variables are monthly rates per 100,000 citizens. Both regressions control for Month and UPP fixed effects, have standard errors clustered at UPP level and use population as analytical weights. Borusyak et al. (2022) estimator uses less observations because it drops observations in which all units are treated. * significant at 10%; ** significant at 5%; *** significant at 1%.

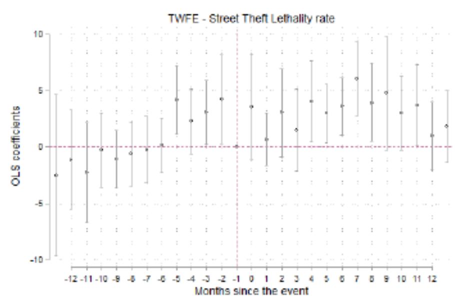
Figure 2: Dynamic TWFE estimation for crime outcomes



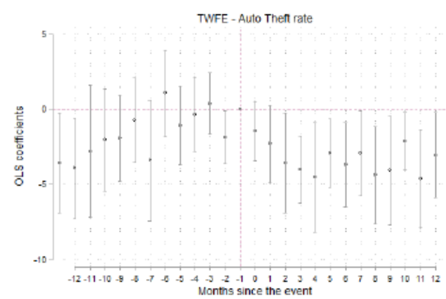
(a) Homicides



(b) Police Killings



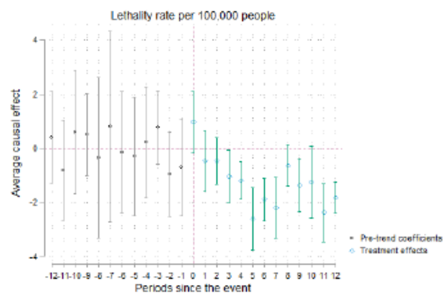
(c) Street Theft



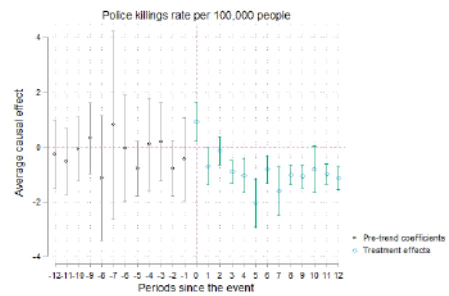
(d) Auto Theft

Note: This figure shows the OLS estimates for equation (2). I collapsed any month before or after 12 months to the same time period, respectively and I normalize the month before the beginning of treatment to zero.

Figure 3: Dynamic Borusyak et al. (2022) estimation for crime outcomes

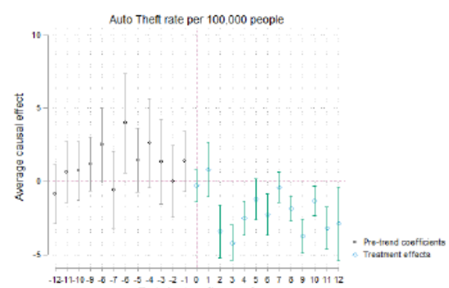


(a) Homicides



(b) Police Killings

(c) Street Theft



(d) Auto Theft

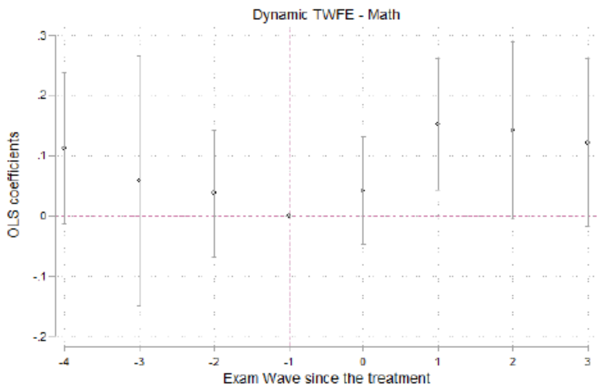
Note: This figure shows the estimates for Borusyak et al. (2022) DD imputation estimator, controlling for month and neighborhood fixed effects.

Table 2: DD estimation for school outcomes

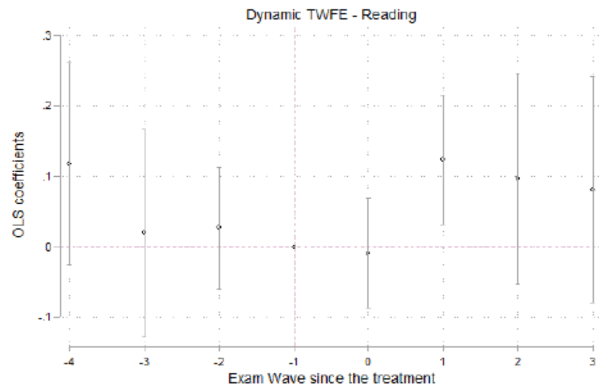
	(1)	(2)	(3)	(4)
Panel A: Math				
TWFE	0.07	0.06	0.07	0.06
	(0.04)*	(0.04)	(0.04)*	(0.04)
DD imputation	0.106	0.099	0.095	0.085
	(0.039)***	(0.038)***	(0.038)**	(0.037)**
Panel B: Reading				
TWFE	0.03	0.03	0.04	0.03
	(0.04)	(0.04)	(0.04)	(0.03)
DD imputation	0.074	0.070	0.064	0.058
	(0.038)**	(0.036)**	(0.036)*	(0.035)*
Obs.	62,051	54,879	62,051	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 TWFE (equation (4)) and DD imputation (Borusyak et al., 2022) regressions. 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 4: Dynamic TWFE estimation for school outcomes



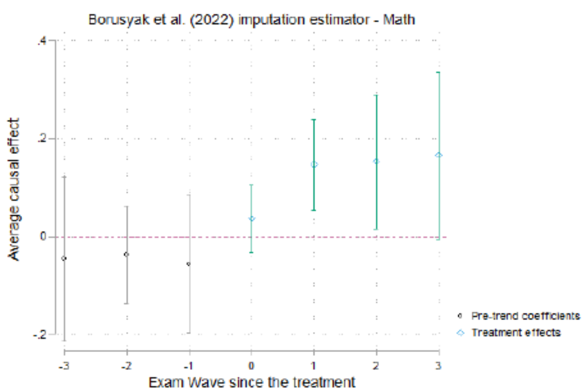
(a) TWFE - Math



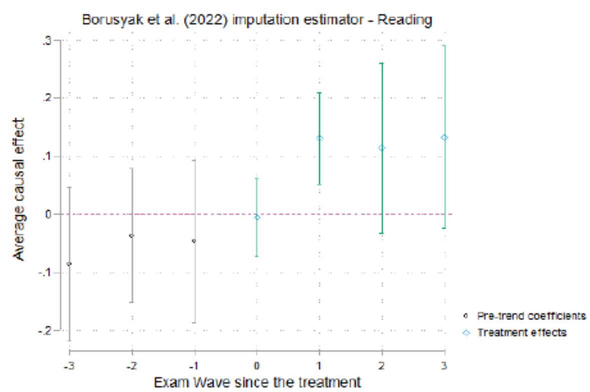
(b) TWFE - Reading

Notes: Figure shows the coefficients estimates for equation (4), controlling for school and wave of exam FE.

Figure 5: Borusyak et al. (2022) DD imputation estimator for school outcomes



(a) DD imputation - Math



(b) DD imputation - Reading

Notes: Figure shows the coefficients estimates for DD imputation estimator, controlling for school and wave of exam FE. Coefficients were calculated by Stata package did_imputation.

Table 3: Heterogeneity - Borusyak et al. (2022) DD imputation estimator for school outcomes

	Elementary	Middle	Girls	Boys	Non-white	White
Panel A: Math						
Treat	0.10	0.10	0.07	0.12	0.07	0.21
	(0.05)**	(0.06)	(0.04)*	(0.05)***	(0.04)*	(0.05)***
Panel B: Reading						
Treat	0.07	0.08	0.07	0.07	0.04	0.14
	(0.04)*	(0.07)	(0.04)	(0.04)*	(0.03)	(0.05)***
Observations	43,429	18,622	29,924	30,417	45,620	14,480
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Each column is a separate regression using samples restricted to the characteristic analyzed. Elementary refers to school tests for 5th grade and Middle to the 9th grade. The other characteristics were retrieved form Students' Surveys. 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 4: Heterogeneity - Borusyak et al. (2022) DD imputation estimator for school outcomes

	Main	Treated year before	Treated same year	Low risk	High risk
Panel A: Math					
Treat	0.11	0.12	0.09	0.13	0.07
	(0.04)***	(0.05)***	(0.06)	(0.04)***	(0.04)*
Panel B: Reading					
Treat	0.07	0.09	0.05	0.07	0.08
	(0.04)**	(0.04)**	(0.06)	(0.04)	(0.05)*
Observations	62,051	53,570	47,241	52,463	48,348
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Each column is a separate regression using samples restricted to the object analyzed. Column "Treated year before" restricts the sample to never-treated schools and for school that were treated in the year before the exam, that is, schools treated in even years (2008,2010,2012 and 2014). Column "Treated same year" uses never-treated schools and schools treated in the same year of the standardized national exam (2009, 2011, 2013 and 2015). I use police classified information for the operational risk Police faces in each UPP to construct the samples for "Low risk" and "High risk". Operational risk refers to perceived risk to police action in favelas due to possible presence of drug traffickers. 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 5: Robustness - Borusyak et al. (2022) DD imputation estimator for school outcomes

	Main	p10-p90	p20-p80	South zone	West zone	Mare
Panel A: Math						
Treat	0.11	0.09	0.07	0.10	0.12	0.10
	(0.04)***	(0.03)***	(0.02)***	(0.04)**	(0.04)***	(0.04)**
Panel B: Reading						
Treat	0.07	0.07	0.05	0.07	0.07	0.08
	(0.04)**	(0.03)**	(0.02)**	(0.04)*	(0.04)*	(0.04)**
Obs.	62,051	62,051	62,051	60,658	35,208	58,295
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

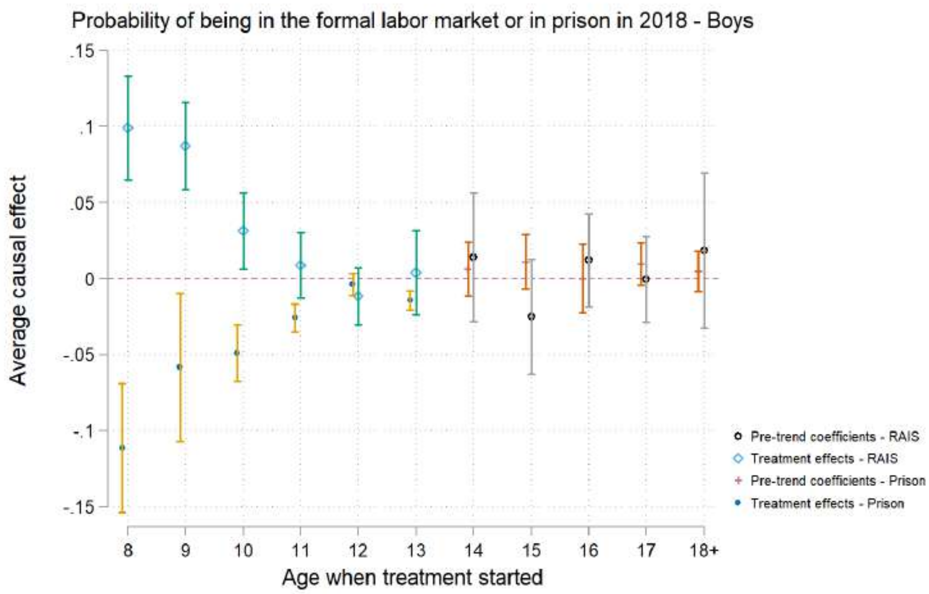
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Each column is a separate regression using different dependent variables or samples restricted to the object analyzed. The second and third columns, "p10-p90" and "p20-p80" show winsorized dependent variables at percentiles 10 and 90 and 20 and 80, respectively. The last three columns display estimations for restricted samples: first, dropping school in the South zone of the city, excluding schools in the West zone and, finally, in the Complex of Maré. 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 6: DD estimation for medium-run outcomes

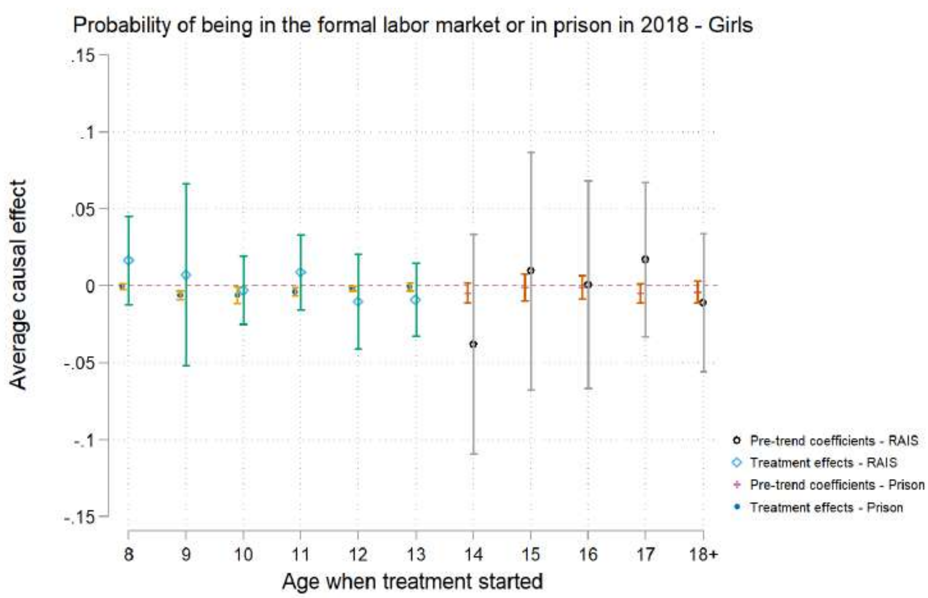
	RAIS	Prison	Cadunico
Panel A: Boys			
TWFE	0.001	-0.016	0.011
	(0.013)	(0.009)*	(0.018)
DD imputation	0.017	-0.028	-0.006
	(0.009)**	(0.006)***	(0.013)
Obs.	36,161	36,161	36,161
Mean Dep. Var	0.306	0.061	0.523
Panel B: Girls			
TWFE	-0.003	-0.002	0.009
	(0.013)	(0.001)	(0.014)
DD imputation	-0.002	-0.003	0.010
	(0.011)	(0.001)**	(0.011)
Obs.	34,890	34,890	34,890
Mean Dep. Var	0.277	0.003	0.532
Year FE	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*

Notes: Table shows the results of TWFE (equation (5)) and DD imputation (Borusyak et al., 2022) regressions. The dependent variable is a dummy that turns one if the student appears in the formal labor market (RAIS), prison or in the Social Protection Register (Cadunico) in 2018. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 6: Dynamic DD imputation estimator for medium-run outcomes



(a) Boys



(b) Girls

A Additional Figures and Tables

Figure A1: Complexes of favelas in the city of Rio de Janeiro

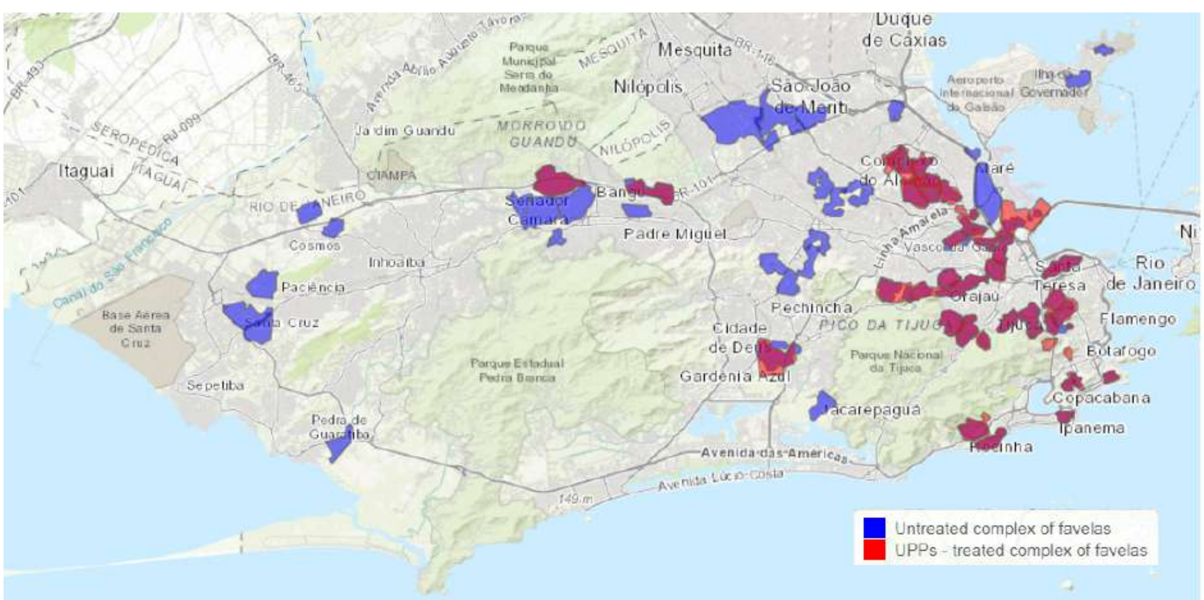


Figure A2: UPPs and untreated complexes of favelas in the city of Rio de Janeiro

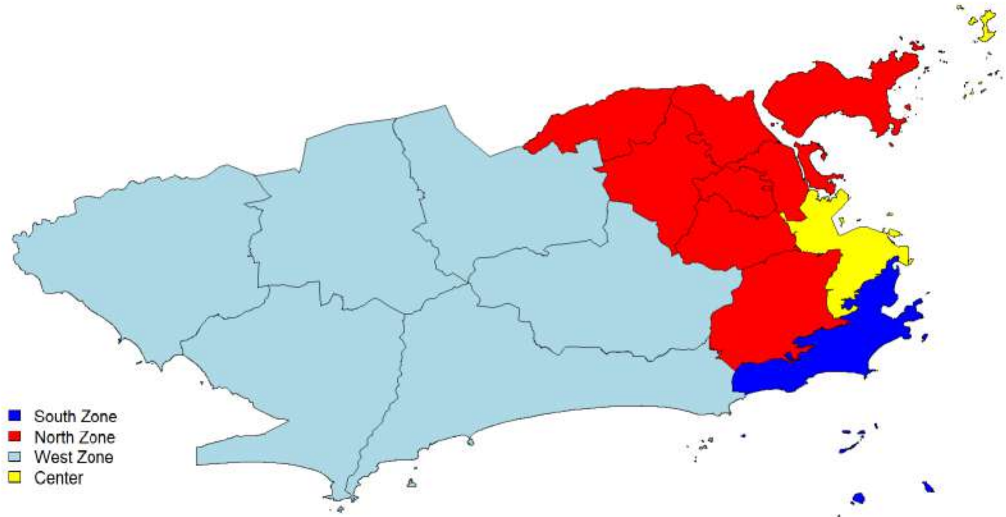


Figure A3: Zones in Rio de Janeiro

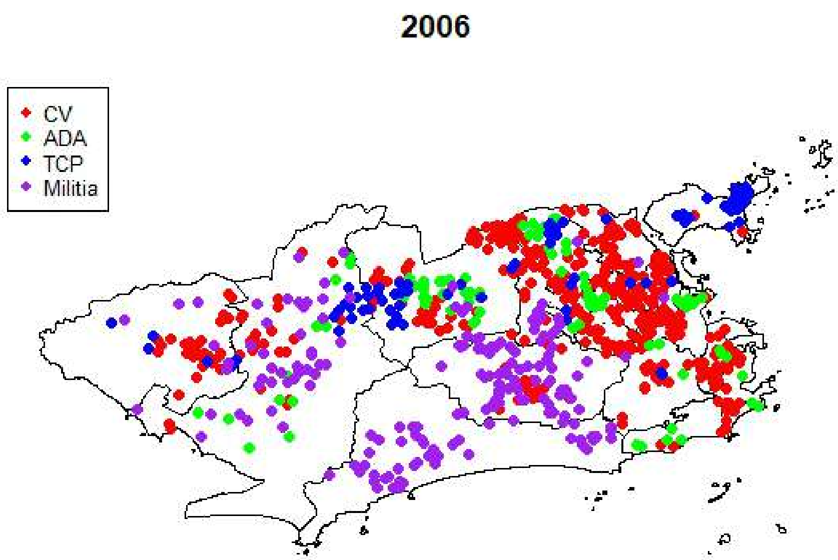


Figure A4: Spatial Distribution of Drug Factions in Rio de Janeiro in 2006

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

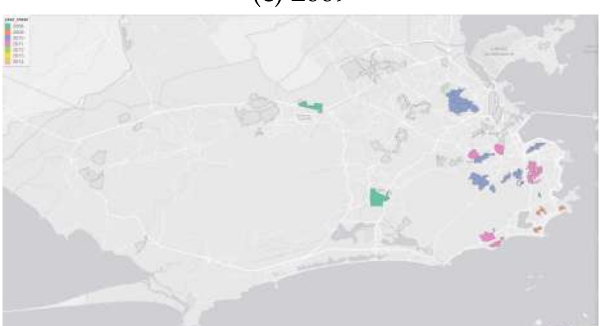
Figure A5: Spatial and Temporal Evolution of UPPs in the city of Rio de Janeiro

(a) Before Treatment

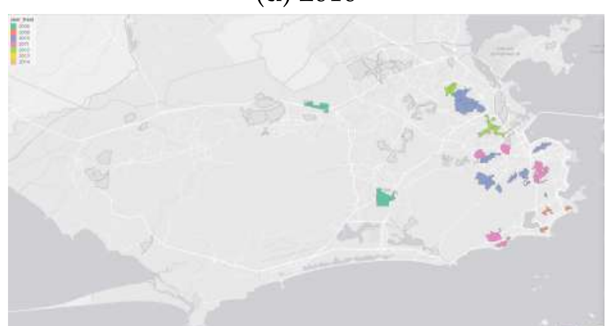
(b) 2008

(c) 2009

(d) 2010



(e) 2011



(f) 2012

(g) 2013

(h) 2014

Table A1: 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 serice	97.56	98.55	0.15	0.54
Avg. bathrooms hh	0.38	0.36	0.06	0.38
Iliteracy 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 complex of 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 complex of 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 A2: Monthly Average for crime outcomes per 100,000 citizens

	UPP 2007-2008	UPP 2009-2015	Rio 2007-2008	Rio 2009-2015
Homicide rate	4.23	1.60	4.19	2.46
Police Killings rate	2.34	0.60	1.07	0.46
Street Theft rate	12.55	6.87	60.71	51.13
Auto Theft rate	7.06	2.48	26.24	16.56
Population	658,699	658,699	6,291,744	6,291,744

Notes: This table shows summary statistics for selected crime monthly crime rates per 100,000 citizens for ever-treated places and for the city of Rio de Janeiro for periods before and after the beginning of the program. To Data comes from Institute of Public Security (ISP-RJ).

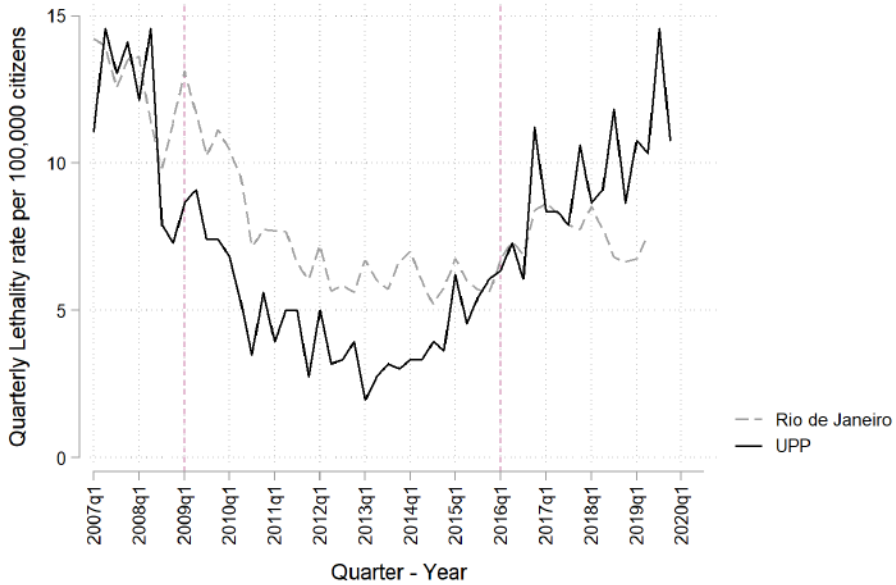


Figure A6: Quarterly Homicide rate per 100,000 citizens

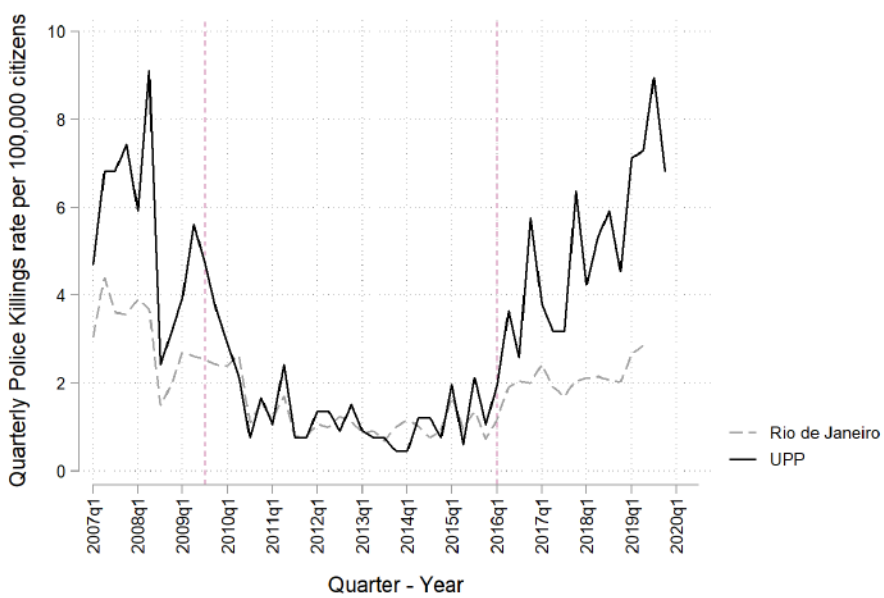


Figure A7: Quarterly Police Killings rate per 100,000 citizens

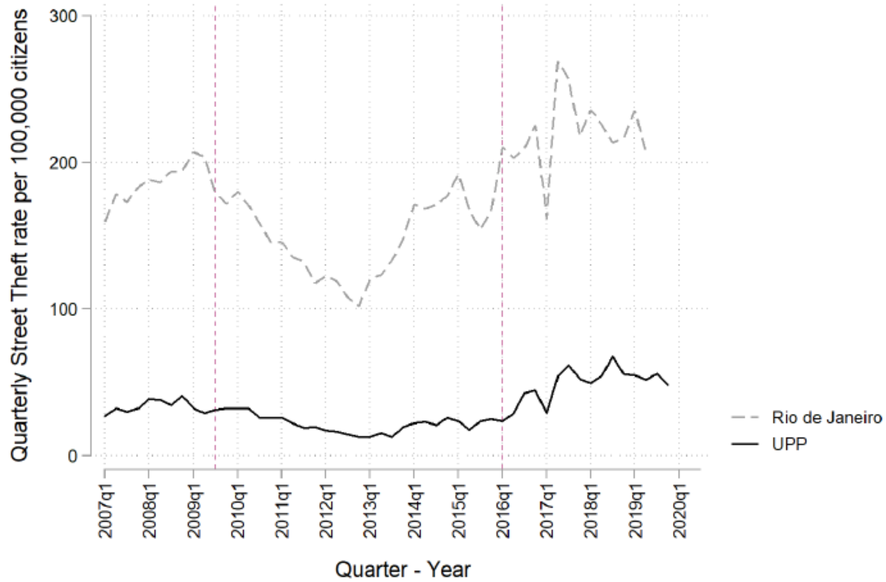


Figure A8: Quarterly Street Theft rate per 100,000 citizens

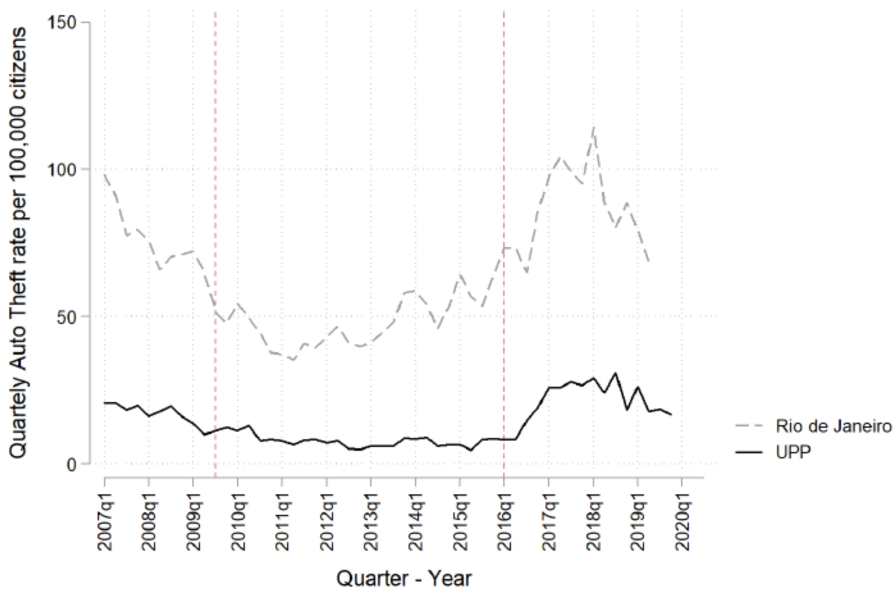


Figure A9: Quarterly Auto Theft rate per 100,000 citizens

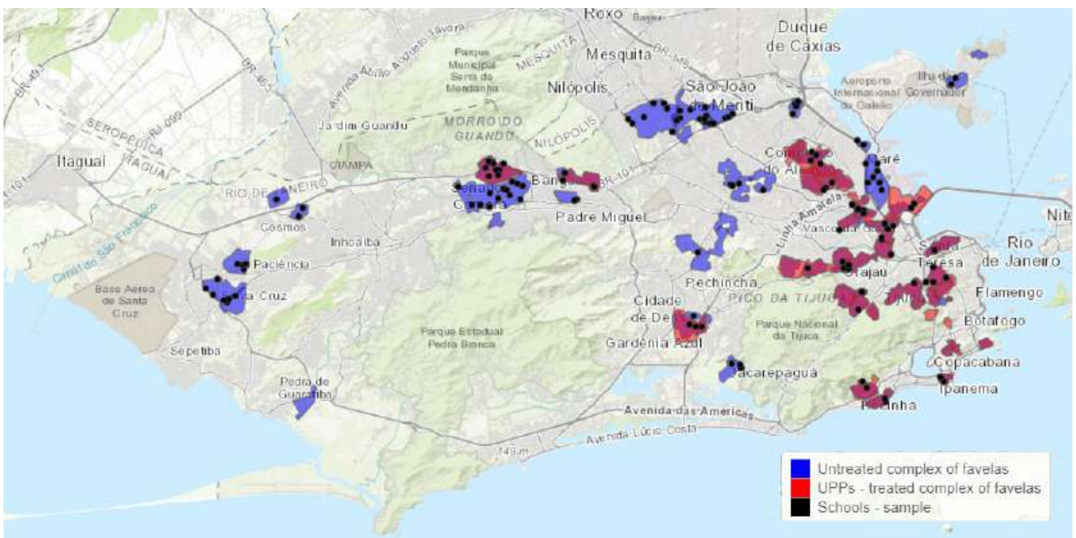


Figure A10: Sample of schools in treated and control areas

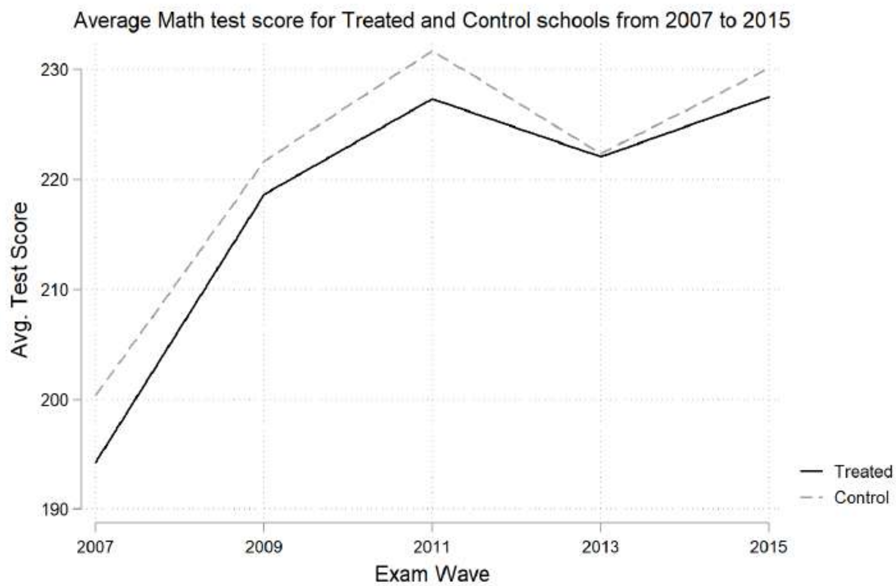


Figure A11: Math

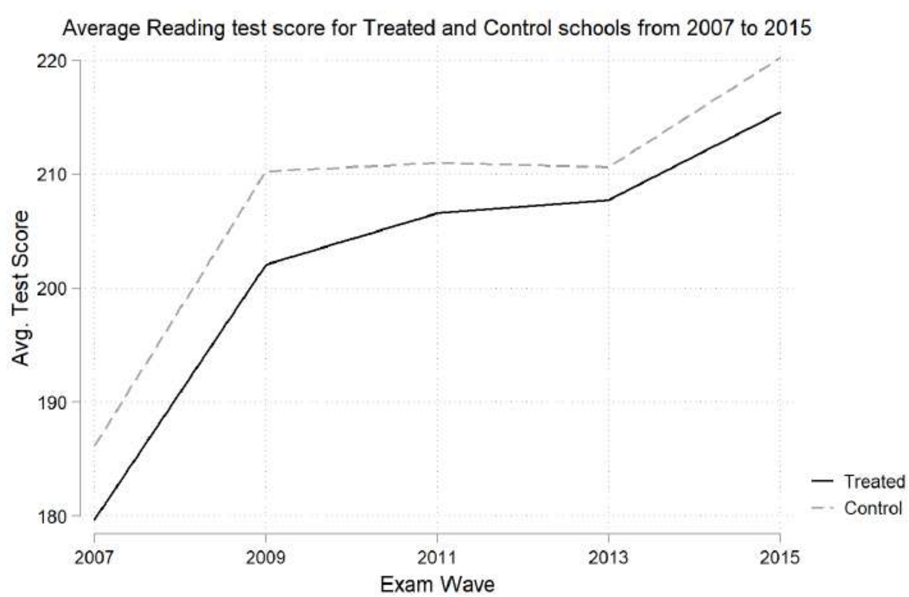


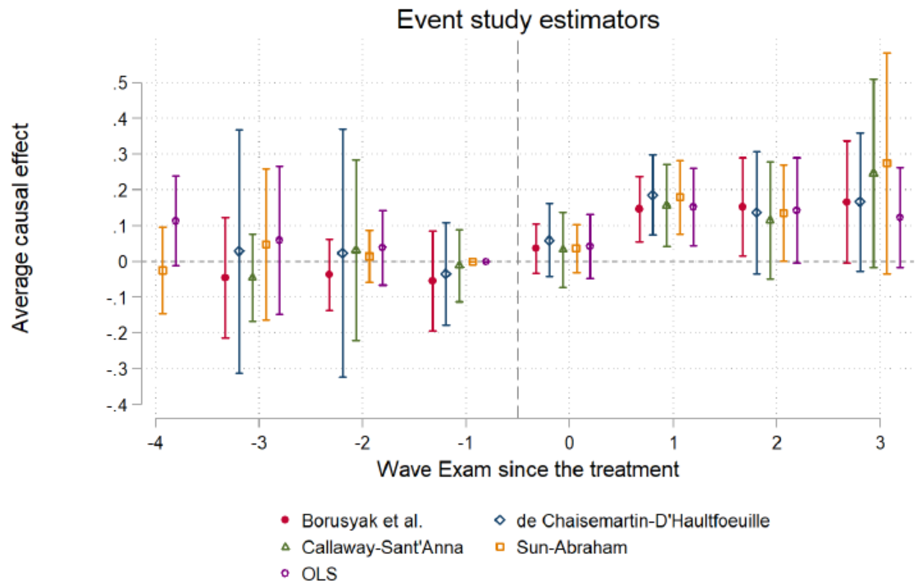
Figure A12: Reading

Table A3: Summary statistics for Treated and Control schools from School Census 2007

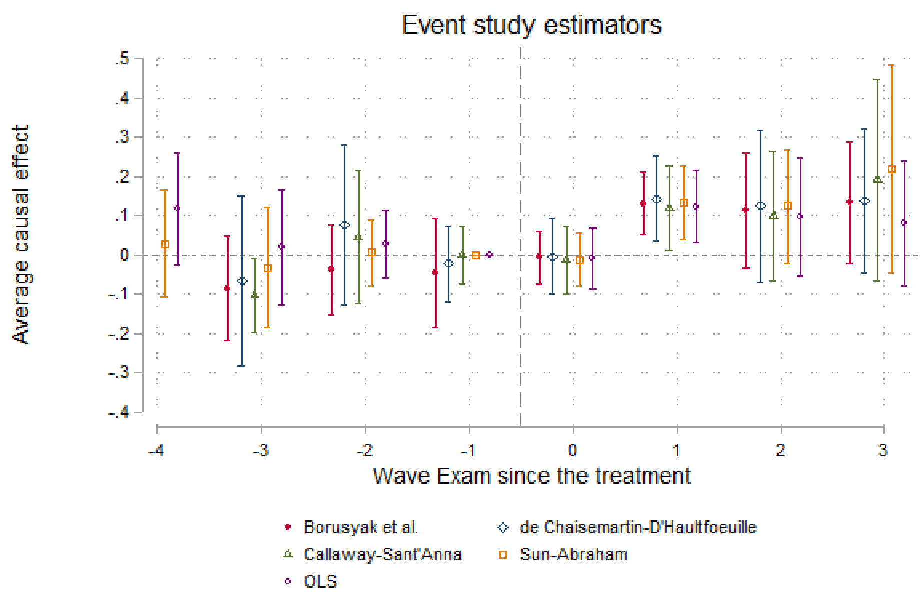
Variables	Mean - Treat	Mean - Control	T-test	Kolmogorov-Smirnov
Computer Lab.	0.58	0.38	0.02	0.14
Science Lab.	0.12	0.14	0.68	1.00
Sports Court	0.55	0.68	0.12	0.62
Kitchen	1.00	0.99	0.38	1.00
Library	0.68	0.81	0.09	0.67
Recreation Area	0.43	0.27	0.04	0.32
Washroom outside the building	0.10	0.13	0.61	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
Female	0.49	0.48	0.66	0.99
Non-white	0.57	0.59	0.33	0.10
Avg. students' age	10.49	10.86	0.40	0.51
Use public transit	0.02	0.01	0.76	1.00

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.

Figure A13: Dynamic DD for selected estimators



(a) Dynamic DD - Math



(b) Dynamic DD - 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.

Table A4: Dynamic DD imputation estimation for school outcomes

	(1)	(2)	(3)	(4)
Panel A: Math				
τ_{-3}	-0.05	-0.04	-0.04	-0.04
	(0.09)	(0.08)	(0.08)	(0.08)
τ_{-2}	-0.04	-0.01	-0.03	-0.01
	(0.05)	(0.06)	(0.05)	(0.06)
τ_{-1}	-0.06	-0.02	-0.05	-0.01
	(0.07)	(0.08)	(0.07)	(0.08)
τ_0	0.04	0.03	0.03	0.02
	(0.04)	(0.03)	(0.03)	(0.03)
τ_1	0.15	0.14	0.14	0.13
	(0.05)***	(0.04)***	(0.04)***	(0.04)***
τ_2	0.15	0.14	0.14	0.12
	(0.07)**	(0.07)*	(0.07)**	(0.07)*
τ_3	0.17	0.15	0.16	0.14
	(0.09)*	(0.11)	(0.09)*	(0.11)
F-stat	0.435	0.0923	0.409	0.0721
p-value	0.729	0.964	0.748	0.975
Panel B: Reading				
τ_{-3}	-0.09	-0.07	-0.08	-0.07
	(0.07)	(0.07)	(0.07)	(0.07)
τ_{-2}	-0.04	0.01	-0.04	0.01
	(0.06)	(0.08)	(0.06)	(0.08)
τ_{-1}	-0.05	-0.00	-0.05	0.00
	(0.07)	(0.09)	(0.07)	(0.08)
τ_0	-0.01	-0.01	-0.02	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)
τ_1	0.13	0.12	0.12	0.11
	(0.04)***	(0.03)***	(0.04)***	(0.03)***
τ_2	0.11	0.11	0.10	0.09
	(0.07)	(0.08)	(0.07)	(0.07)
τ_3	0.13	0.12	0.13	0.12
	(0.08)*	(0.13)	(0.08)	(0.13)
F-stat	0.626	0.389	0.640	0.385
p-value	0.602	0.761	0.593	0.764
Obs.	62,051	54,879	62,051	54,879
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*
Controls	*No*	*Students*	*Schools*	*All*
DF	43	43	43	43

Notes: Table shows the results for DD imputation (Borusyak et al., 2022) estimator considering three pre-treatment and four post-treatment periods. The coefficients $\{\tau_j\}_{-3}^{3}$ refer to the impact of treatment j periods before or after the beginning of treatment. F-stat and p-value relate to the test statistic and its p-value for the hypothesis testing that all pre-treatment coefficients are zero. DF display the degrees of freedom of this test. 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 A5: Intensity of treatment

	(1)	(2)	(3)	(4)
Panel A: Only schools treated year before				
Math				
Treat year before	0.12	0.12	0.11	0.11
	(0.05)***	(0.04)***	(0.04)**	(0.04)**
Reading				
Treat year before	0.09	0.10	0.08	0.08
	(0.04)**	(0.04)**	(0.04)**	(0.04)**
Obs.	53,570	47,380	53,570	47,380
Panel B: Only schools treated same year				
Math				
Treat same year	0.09	0.07	0.08	0.05
	(0.06)	(0.06)	(0.06)	(0.06)
Reading				
Treat same year	0.05	0.03	0.04	0.02
	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	47,241	41,722	47,241	41,722
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. Panel A restricts the sample to never-treated schools and for school that were treated in the year before the exam, that is, schools treated in even years (2008,2010,2012 and 2014). Panel B uses never-treated schools and schools treated in the same year of the standardized national exam (2009, 2011, 2013 and 2015). 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 A6: Intensity of treatment

	(1)	(2)	(3)	(4)
Panel A: Low and Medium Operational Risk				
Math				
Treat	0.13	0.12	0.12	0.11
	(0.04)***	(0.04)***	(0.04)***	(0.04)**
Reading				
Treat	0.07	0.06	0.06	0.05
	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	52,463	46,478	52,463	46,478
Panel B: High Operational Risk				
Math				
Treat	0.07	0.07	0.06	0.06
	(0.04)*	(0.04)*	(0.04)*	(0.04)*
Reading				
Treat	0.08	0.08	0.07	0.06
	(0.05)*	(0.04)*	(0.04)	(0.04)
Obs.	48,348	42,624	48,348	42,624
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. Panel A restricts the sample to never-treated schools and for school that were treated in the year before the exam, that is, schools treated in even years (2008,2010,2012 and 2014). Panel B uses never-treated schools and schools treated in the same year of the standardized national exam (2009, 2011, 2013 and 2015). 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 A7: Robustness standard errors - DD estimation for school outcomes

	(1)	(2)	(3)	(4)
Panel A: Math				
DD imputation	0.106	0.099	0.095	0.085
	(0.039)***	(0.038)***	(0.038)**	(0.037)**
	[0.054]**	[0.052]*	[0.053]*	[0.053]
	{0.049}**	{0.048}**	{0.049}*	{0.048}*
Panel B: Reading				
DD imputation	0.074	0.070	0.064	0.058
	(0.038)**	(0.036)**	(0.036)*	(0.035)*
	[0.047]	[0.047]	[0.047]	[0.048]
	{0.045}*	{0.043}	{0.045}	{0.043}
Obs.	62,051	54,879	62,051	54,879
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*
Controls	*No*	*Students*	*Schools*	*All*

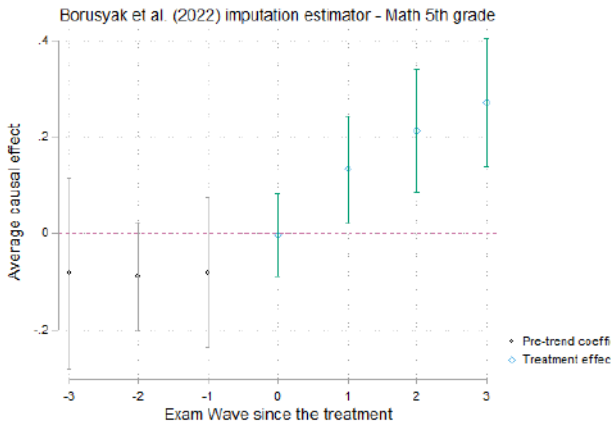
Notes: Table shows the results for DD imputation (Borusyak et al., 2022) regressions with different methods to calculate the standard errors. First, I cluster at the treatment level, that is, at favela level. Second, I use the *leave-out* method discussed in Borusyak et al. (2022) to deal with issues raised by few clusters. Third, I cluster the standard error at school level. 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 A8: Robustness proxy for students' income - DD estimation for school outcomes

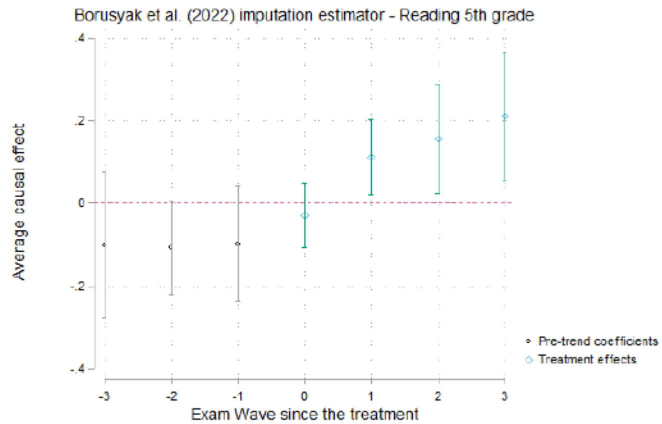
	(1)	(2)	(3)	(4)
Panel A: Math				
DD imputation	0.106	0.104	0.107	0.105
	(0.039)***	(0.039)***	(0.038)***	(0.039)***
Panel B: Reading				
DD imputation	0.074	0.075	0.077	0.075
	(0.038)**	(0.035)**	(0.034)**	(0.036)**
Obs.	62,051	50,538	50,538	50,538
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*
Controls	*No*	*Students − All*	*All*	*All − 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 column, I present the results for the main specification; in the second column results for all students controls, including the proxies for income; in the third, I control for all students' and schools' covariates and, in the last column, I replace the variables that are proxies for income to the income index. 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%.

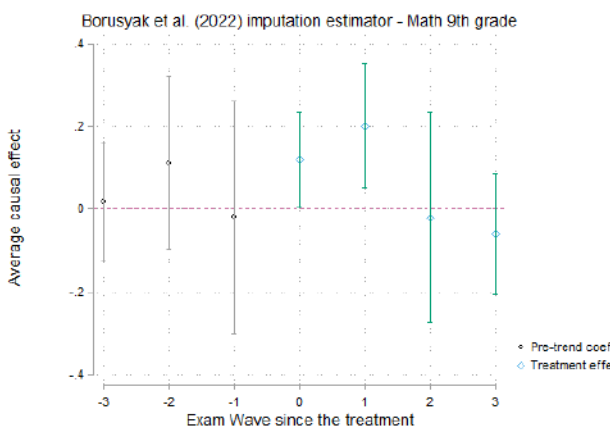
Figure A14: Heterogeneity of UPP treatment effects on schooling by grades - Dynamic effects



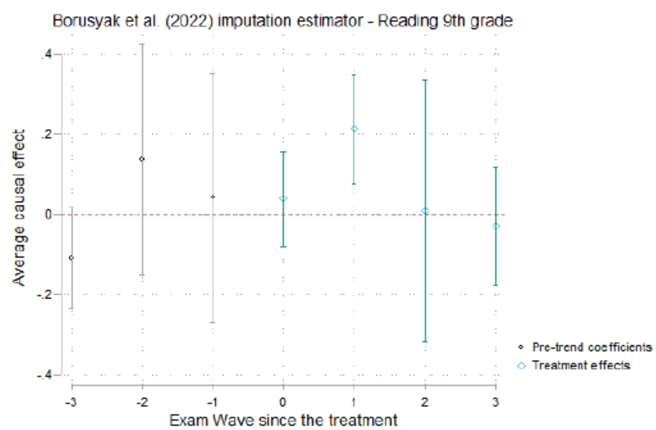
(a) Math - 5th grade



(b) Reading - 5th grad



(c) Math - 9th grade



(d) Reading - 9th grade

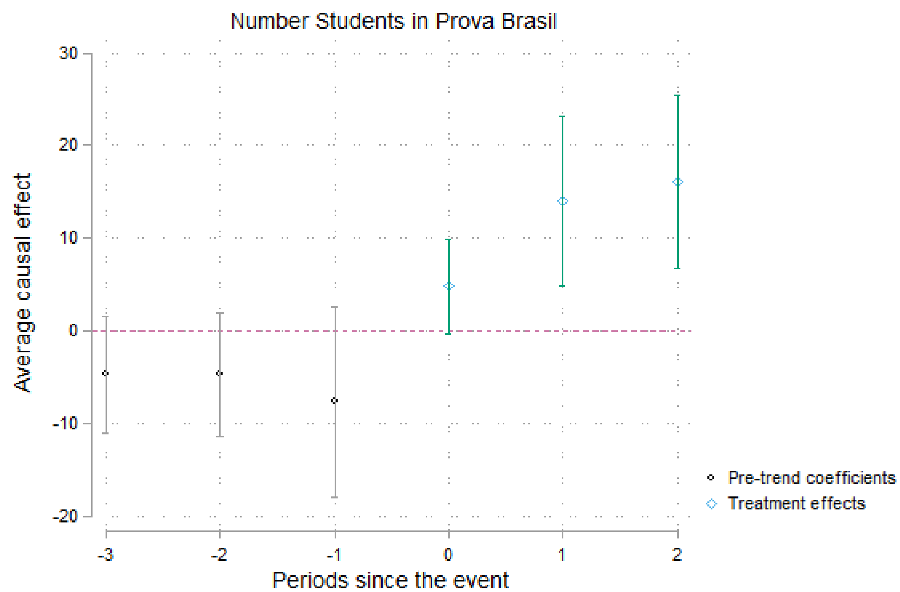


Figure A15: Number of students who take *Prova Brasil*

Table A9: Student Composition - *Prova Brasil*

	(1) Fem.	(2) Non-white	(3) Lives mother	(4) Lives parents	(5) Mother's lit.	(6) Mother above Middle	(7) Failed	(8) Dropout
DD imputation	-0.014	0.008	-0.009	-0.003	-0.002	-0.011	0.023	0.011
	(0.011)	(0.006)	(0.007)	(0.009)	(0.007)	(0.012)	(0.017)	(0.010)
Observations	60,341	60,100	60,099	58,926	59,527	37,805	59,634	59,879
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.496	0.759	0.884	0.491	0.944	0.582	0.297	0.0872

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 his or her mother, if the student lives with both parents, if the mother is literate, if the mother has education above middle school, if the student has failed or drop out before. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A10: Student Composition - *Prova Brasil*

	(1) Car	(2) Private	(3) Doesn't Work	(4) # Bathrooms	(5) # Rooms	(6) # TV	(7) Freezer	(8) Laundry	(9) Computer	(10) Maid	(11) Inc. Index
DD imputation	0.001	0.032	0.024	-0.019	-0.075	-0.052	0.007	-0.017	0.011	0.010	-0.043
	(0.012)	(0.011)***	(0.008)***	(0.013)	(0.022)***	(0.022)**	(0.012)	(0.011)	(0.013)	(0.010)	(0.037)
Observations	60,599	59,513	59,047	60,584	60,111	60,100	60,099	60,372	60,658	59,829	53,210
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	0.300	0.192	0.872	1.254	1.934	1.862	0.355	0.807	0.656	0.089	0.000

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: if student's parents have a car, if the student has studied in a private school before, if the student doesn't work outside home, the number of bathrooms, rooms and TVs in the house, if there is a freezer, a laundry machine, at least one computer at student's home, if a maid works in her house and a income index. I construct the income indez based on the variables discussed above. 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 A11: Flow - School Census

	(1) Enrollment	(2) # classes	(3) Approval	(4) Failed	(5) Aban.	(6) Age-Grade distortion
DD imputation	62.94	0.83	0.34	-0.86	0.52	0.05
	(14.40)***	(0.53)	(0.59)	(0.68)	(0.43)	(1.10)
Observations	1,242	1,242	1,242	1,242	1,242	1,242
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	776.20	26.76	87.81	9.93	2.26	25.10

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from annual School Census data. Equations (3) to (6) are weighted by the number of enrollments at the school. Since I have yearly data, I define treatment as a school being treated in that year. Dependent variables are the number of enrollments at school, the number of classes offered in the school, the approval, 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%.

Table A12: Student Composition - SME

	(1) Fem.	(2) Non-white	(3) NIS - child	(4) NIS - parent	(5) Father dead	(6) Mother dead	(7) Lives mother and father	(8) Appear father	(9) Mother middle	(10) Father middle	(11) Father profession	(12) Mother housekeeper
DD imputation	-0.004	-0.004	-0.006	-0.007	-0.005	-0.005	-0.014	0.003	0.015	0.017	-0.012	-0.001
	(0.002)*	(0.006)	(0.014)	(0.014)	(0.004)	(0.004)	(0.011)	(0.004)	(0.019)	(0.018)	(0.014)	(0.012)
Observations	1,518	1,518	1,518	1,518	1,518	1,518	1,518	1,518	1,518	1,518	1,518	1,518
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.0332	0.680	0.413	0.389	0.0332	0.0134	0.432	0.897	0.586	0.598	0.651	0.388

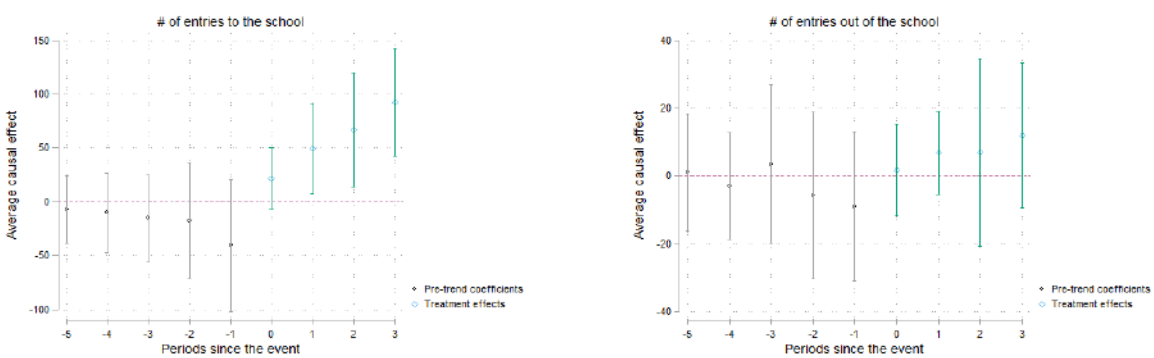
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from a dataset containing the universe of students enrolled in municipal schools from 2004 until 2014. I restricted the sample to the same schools used in Prova Brasil's regressions. Since I have yearly data, I define a school treated if it is within a treated favela in that calendar year. I construct a dataset at student x year x school, in which I observe the school a student attended in a year. Then, I collapse this data at school x year. I discuss more about the data in Appendix D. The outcomes are the share of students who are female, non-white students, have Cadunico number, whose parents have a Cadunico number, the father is dead, the mother is dead, lives with both parents, father appears in the administrative data, mother has education above middle school, father has education above middle school, father has a profession described in admin data and mother works as a housekeeper. I weight the regressions by the number of students in each school x year. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A13: DD imputation estimation for in and out-enrollments

	(1) In	(2) Out
DD imputation	63.81	11.75
	(19.23)***	(7.35)
Observations	1,518	1,518
Year FE	Yes	Yes
School	Yes	Yes
Mean Dep. Var.	685.3	192.3

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Any change is students' status is an entry in the flow (or movement) administrative data from the Municipal Secretary of Education (SME). I restrict the movements for the years 2004 until 2014 and from grades 1st to 9th grade. I drop all the movements not related to an in or out-enrollment, such as classrooms changes . I define an in-enrollment if the entry refers to the first entry in the system, enrollment renewal, transfers from other schools and if the student returns to school after a drop out. I characterize out-enrollments as a dummy that turns one if the entry refers to transfers to other schools, drop out due to specific cases (illness, death, need to work) or just drop out. I also restrict the schools' sample to be the same schools used in Prova Brasil's regressions. Since I have yearly data, I define a school treated if it is within a treated favela in that calendar year. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A16: Dynamic DD imputation in and out-enrollments



(a) In-enrollments (b) Out-enrollments

Table A14: DD imputation for causes in-enrollment

	(1) 1st entry	(2) Transfer public	(3) Transfer private	(4) Return	(5) Renewal	(6) Reclassification
DD imputation	6.34	40.92	1.28	-1.99	16.29	0.50
	(2.27)***	(8.90)***	(0.84)	(1.49)	(13.02)	(0.37)
Observations	1,518	1,518	1,518	1,518	1,518	1,518
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	12.28	143.4	9.806	18.25	498.7	2.652

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Any change is students' status is an entry in the flow (or movement) administrative data from the Municipal Secretary of Education (SME). I restrict the movements for the years 2004 until 2014 and from grades 1st to 9th grade. I drop all the movements not related to an in or out-enrollment, such as classrooms changes . I define an in-enrollment if the entry refers to the first entry in the system, enrollment renewal, transfers from other schools and if the student returns to school after a drop out. I characterize out-enrollments as a dummy that turns one if the entry refers to transfers to other schools, drop out due to specific cases (illness, death, need to work) or just drop out. I also restrict the schools' sample to be the same schools used in Prova Brasil's regressions. Since I have yearly data, I define a school treated if it is within a treated favela in that calendar year. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A15: DD imputation for causes out-enrollment

	(1) Drop out	(2) Disease	(3) Death	(4) Work	(5) Reclass.	(6) Transfer public	(7) Transfer private
DD imputation	4.80	-0.04	-0.04	-0.72	0.43	7.84	-1.15
	(4.85)	(0.05)	(0.03)	(0.36)**	(0.29)	(5.82)	(0.79)
Observations	1,518	1,518	1,518	1,518	1,518	1,518	1,518
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	34.12	0.212	0.173	1.037	1.747	144.3	9.363

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Any change is students' status is an entry in the flow (or movement) administrative data from the Municipal Secretary of Education (SME). I restrict the movements for the years 2004 until 2014 and from grades 1st to 9th grade. I drop all the movements not related to an in or out-enrollment, such as classrooms changes . I define an in-enrollment if the entry refers to the first entry in the system, enrollment renewal, transfers from other schools and if the student returns to school after a drop out. I characterize out-enrollments as a dummy that turns one if the entry refers to transfers to other schools, drop out due to specific cases (illness, death, need to work) or just drop out. I also restrict the schools' sample to be the same schools used in Prova Brasil's regressions. Since I have yearly data, I define a school treated if it is within a treated favela in that calendar year. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A16: Composition Teachers - *Prova Brasil*

	(1) Fem.	(2) Non-White	(3) Age < 30	(4) College	(5) Graduate	(6) Tenure < 2	(7) Tenure at school < 2	(8) Work other school
DD imputation	-0.032	0.089	0.022	-0.052	0.003	-0.030	-0.021	-0.004
	(0.029)	(0.052)*	(0.033)	(0.030)*	(0.056)	(0.027)	(0.046)	(0.042)
Observations	2,282	2,262	2,271	2,245	2,191	2,246	2,269	2,264
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	0.867	0.446	0.0991	0.822	0.399	0.0775	0.239	0.399

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from Teacher's Survey from Prova Brasil. Equations are at teacher's level. Columns reflect the share of female, non-white, teachers with less than 30 years old, wih a college degree, if any graduate degree (specialization, master or PhD), tenure below 2 years, tenure at school below 2 years and if the teacher also work in another school, respectively. Standard errors are clusters at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A17: Composition Teachers - *School Census*

	(1) # Teachers	(2) Fem.	(3) Non-White	(4) College	(5) Graduate	(6) Avg. classes	(7) Age
DD imputation	0.991	-0.011	-0.007	0.026	0.007	0.167	0.261
	(0.664)	(0.013)	(0.027)	(0.024)	(0.016)	(0.104)	(0.370)
Observations	1,242	1,242	1,175	1,242	1,228	1,242	1,242
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No
Mean_DepVar	29.16	0.804	0.400	0.687	0.294	3.207	41.89

Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from School Census. Regressions (2) to (7) 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, wih a college degree, if any gradute degree (specialization, master or PhD), the average number of classes they teach and their mean age, respectively. Standard errors are clusters at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A18: School infrastructure

	(1) # employees	(2) # classrooms	(3) # computers	(4) Computer Lab.	(5) Science Lab.	(6) Sports Court	(7) Library	(8) Recreation Area	(9) Infra Index
DD imputation	1.106	0.308	-0.094	-0.159	0.020	-0.016	0.126	0.017	-0.029
	(2.001)	(0.215)	(1.650)	(0.071)**	(0.038)	(0.043)	(0.061)**	(0.062)	(0.103)
Observations	690	690	690	690	690	690	690	690	690
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No	No
Mean_DepVar	50.01	13.94	15.40	0.775	0.132	0.716	0.486	0.446	2.109

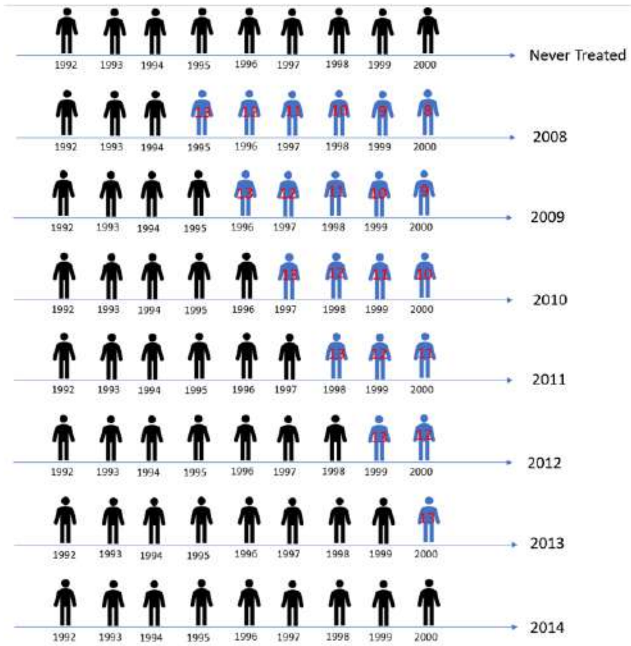
Notes: Table shows the results of regression for Borusyak et al. (2022) imputation estimator. Outcomes come from School Census and regressions are run at the school level. The sample is restricted to years 2007, 2009, 20011, 2013 and 2015. Columns reflect the number of employees and classrooms at school in a year, the share of schools that have a Computer or Science Lab., Sports Court, Library or Recreation Area. The Infra Index is the sum of Computer Lab, Science Lab, Sports Court and Library. These variables would reflect schools' characteristics that may influence the time a student spends at school. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A19: DD estimation for expectations and violence outcomes

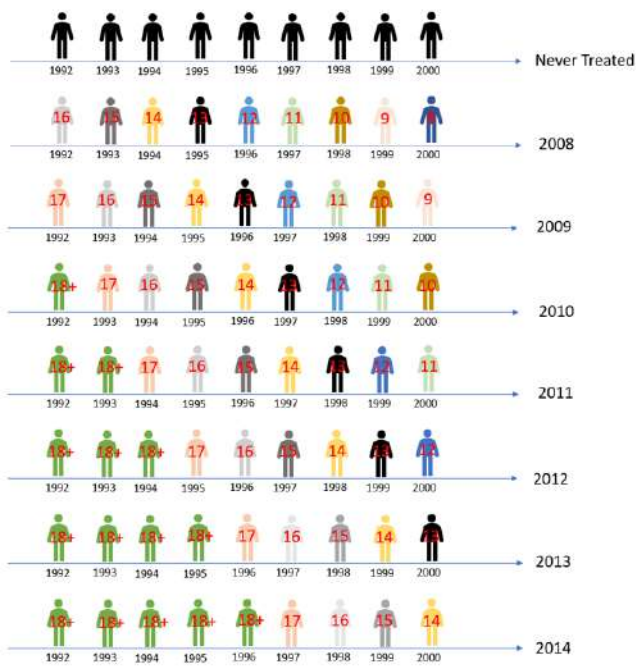
	Graduate High School	Attend College	Violence Teachers	Violence Students
Panel A: Elementary School				
DD imputation	0.163	0.054	0.057	0.071
	(0.053)***	(0.055)	(0.042)	(0.048)
Obs.	1,596	1,591	1,606	1,582
Mean Dep. Var	0.727	0.263	0.633	0.668
Panel B: Middle School				
DD imputation	0.101	-0.087	-0.139	-0.251
	(0.076)	(0.051)*	(0.068)**	(0.063)***
Obs.	603	605	633	621
Mean Dep. Var	0.713	0.126	0.821	0.779
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
School FE	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Table shows the results of DD imputation (Borusyak et al., 2022) regressions for outcomes related to teachers' expectations to students and exposure to violence within school. Answers come from Teachers' Survey from Prova Brasil. Graduate High School refers to expectation the teacher has that more than half of their students will graduate at high school; Attend College is to the belief that more than have of the students will attend college; Viole.ce Teachers relates to violent events against teachers during the year and Violence Students asks if the teacher has seen violent events between students in the last year. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A17: Treated and control cohorts in medium-run empirical strategy



(a) Age below 13 when treated by cohorts x year of treatment in the favela individual lives.



(b) Age when treated by cohorts x year of treatment in the favela individual lives.

Notes: The figures display 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.

Table A20: Heterogeneity medium-run outcomes

	Fem.		Non-White		Cadunico		Mother's Educ.		Lives Mother	
	0	1	0	1	0	1	0	1	0	1
Panel A: Formal Labor Market										
DD imputation	0.019	-0.001	0.010	0.012	0.009	0.012	0.021	0.004	0.010	0.008
	(0.008)**	(0.010)	(0.010)	(0.008)	(0.008)	(0.011)	(0.011)*	(0.008)	(0.012)	(0.009)
Mean Dep. Var	0.308	0.280	0.314	0.282	0.301	0.284	0.275	0.310	0.279	0.300
Panel B: Prison										
DD imputation	-0.024	-0.002	-0.012	-0.012	-0.011	-0.016	-0.013	-0.013	-0.018	-0.012
	((0.006)***	0.001)**	(0.005)**	(0.005)***	(0.005)**	(0.003)***	(0.004)***	(0.003)***	(0.006)***	(0.003)***
Mean Dep. Var	0.0605	0.00272	0.0233	0.0358	0.0296	0.0360	0.0363	0.0276	0.0368	0.0302
Observations	36,417	37,706	20,964	47,098	44,917	29,206	23,331	36,137	18,569	54,996
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table shows the results for DD imputation (Borusyak et al., 2022) estimator for split sample regressions for presence in formal labor market and in prison outcomes. The columns refer to dummies of the variables stated above: if individuals are female, non-white, if she or the parent are register in Cadunico, if mother's education is above middle school, and if the individual lives with her mother. The value 0 means "No" and 1 means " Yes". Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A21: DD estimation for medium-run outcomes - heterogeneity for operational risk

	Low risk	High risk
Panel A: RAIS		
DD imputation	0.001	0.020
	(0.009)	(0.008)**
Mean Dep. Var	0.295	0.298
Panel B: Prison		
DD imputation	-0.001	-0.023
	(0.001)	(0.002)***
Mean Dep. Var	0.0297	0.0289
Observations	63,494	56,952
Year FE	*Yes*	*Yes*
School FE	*Yes*	*Yes*

Notes: Table shows DD imputation (Borusyak et al., 2022) for split sample regressions. The dependent variable is a dummy that turns one if the student appears in the formal labor market (RAIS) or prison in 2018. Operational risk refers to perceived risk to police action in favelas due to possible presence of drug traffickers. Then, the lower the operational risk the less likely is that a conflict will emerge. I keep never treated units in the both samples. Standard errors are clustered at favela level. * significant at 10%; ** significant at 5%; *** significant at 1%.

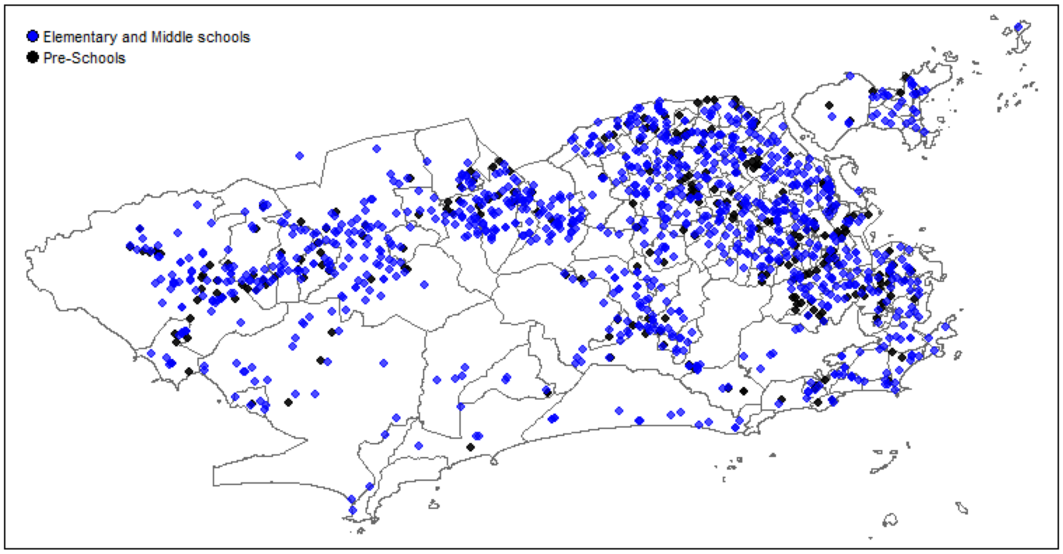


Figure A18: Elementary and Middle schools and Pre-school in Rio de Janeiro.

B Linkage

B.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 operationalize the linkage I use the package "RecordLinkage" in software R.

C Municipal Schooling System

Public schooling provision is constitutionally divided in Brazil in the following way: (i) Municipalities (cities) provide pre-school, elementary and middle school education and Youth and Adult Education; (ii) States supply high school education. In the city of Rio de Janeiro, there are 1540 municipal public schools spread all over its territory that attends individuals from pre-school to Youth and Adult Education.

Figure A18 exhibits the spatial distribution of Elementary and Middle schools and Pre-schools in Rio. There are, on average, 600k students in each year in these schools[29]. Most of them attend Elementary and Middle Schools. As of 2022, there are around 50k employees working in these schools, in which almost 40k are teachers.

Regarding the enrollment for Elementary or Middle School, 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[30].

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

[30] More information in `https://carioca.rio/servicos/matricula-nas-escolas-e-creches-municipais/`. Accessed in June, 2022.

D Panel student x year x school

First, I maintain only movements related grades between the 1st grade and the 9th grade. This choice drops entries associated with Youth and Adult Education (EJA)[31] and with pre-school movements[32] The main reason for this choice is that these students attend separate classrooms with different curriculum 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.

[31] 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.

[32] 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.