Drug Battles and School Achievement: Evidence from Rio de Janeiro's Favelas*

Joana Monteiro
CID - Harvard University

Rudi Rocha
UFRJ

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Abstract

This paper examines the effects of armed conflicts between drug gangs in Rio de Janeiro's favelas on student achievement. We explore variation in violence that occurs across time and space when gangs battle over territories. To identify the causal effect of violence on education, we rely on the fact that armed conflicts between drug gangs are triggered by factors often exogenous to local socioeconomic conditions, such as the imprisonment or release of a gang leader, betrayals and revenge. Within-school estimates indicate that students from schools exposed to violence score less in math. The violence effect increases with conflict intensity and duration; decreases with the distance between the school and the conflict location; and increases when the conflict occurs in the months just before the exam. There is no evidence that the effect of violence persists over time. Finally, we find that violence impact on school resources is an important mechanism behind our results. Armed conflicts are significantly associated with higher teacher absenteeism, principal turnover and temporary school shutdown.

JEL: D74, O18, I2, K42
Key words: favelas, slum, violence, drug trafficking, student achievement.

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1 Introduction

Drug-related violence perpetrated by criminal gangs is a widespread phenomenon in many developed and developing countries, particularly in urban areas. Violence involving drug trafficking organizations has notoriously reached unprecedented levels in Mexico and Central America (Geneva Declaration [2011]). In France, conflicts between drug dealers using combat weapons caused Marseille to experience in 2012 one of the most extreme periods of violence in its history (New York Times [2012a]). In the US, gang activity has increased and become more lethal. The US Department of Justice [2011] points out that neighborhood-based gangs and drug crews pose the most significant criminal threat in larger cities and suburban counties. In these areas, retail drug trade and the distribution activities are routinely associated with lethal violence and violent disputes over control of drug territory and enforcement of drug debts (FBI [2011]). Yet, although the negative consequences of drug-related violence may go far beyond the casualties of those directly involved in the criminal activity and its victims, little is known about whether this phenomenon has other detrimental impacts on areas exposed to conflicts.

This paper studies the negative spillovers of conflicts between drug gangs in Rio de Janeiro by analyzing how they affect the academic achievement of children attending schools located in the proximities of conflict areas. In recent decades, several favelas scattered across the city have been dominated by drug gangs, which have used the territory to sell drugs and hide from police (Misse [1999], Silva et al. [2008]). Local violence skyrockets when gangs fight against each other to dispute favelas. These conflicts are extremely violent due to the arsenal used, which often include heavy weaponry, such as grenades and modern military machine guns. As an immediate consequence, the risk of being caught in the crossfire and real threat to life substantially increase for those who live or work in areas under dispute.

The estimation of the causal effects of drug-related violence on educational outcomes is not a trivial exercise because of two main empirical challenges. First, conflict-prone areas are markedly different from non-violent ones in terms of hard-to-measure individual and community characteristics, which confound cross-section analysis that aims to identify the violence effects. We circumvent this problem by exploring variation over time and space in drug-related conflicts. Most of the disputes occur because gangs have no access to legally enforceable contracts or property rights and, therefore, rely on violence as the primary tool to resolve disputes. Indeed, our data suggest that drug gang conflicts are not rare events: in 61 percent of the

days between 2003 and 2009, there was at least one favela under dispute in Rio de Janeiro. Such frequency in conflicts supports the view that the equilibrium of power among gangs is very unstable. The qualitative evidence indicates that conflicts in Rio de Janeiro are triggered by factors that are exogenous to local socioeconomic conditions, such as the imprisonment or release of a gang leader, betrayals or revenge. Similar factors have been pointed out by studies on street gangs in the US and drug gangs in Mexico. Levitt and Venkatesh [2000] suggest that social/nonpecuniary factors are likely to play an important role in explaining why gangs initiate conflicts, and emphasize that the decision-making of gang members cannot be reconciled with that of optimizing agents. In addition, they point out that a single member of a gang can easily initiate a dispute to show toughness, and that once such violence occurs, it is difficult for the opposing gang not to retaliate. Topalli et al. [2002] found in interviews with active drug dealers that vengeance and the maintenance of reputations for dangerousness are reported as motives for gang violence in St Louis, Missouri. Guerrero-Gutierrez [2011] argues that alliances between drug trafficking organizations in Mexico have been highly unstable during the past five years and that, within drug trafficking organizations, most decisions about day-to-day operations are decentralized. Our empirical strategy allows us to estimate the causal effect of violence on education since it explores idiosyncratic variation in turf violence rather than cross-sectional differences in neighborhood chronic violence or even in the persistent presence of drug gangs. By doing so, our strategy disentangles the effects of violence from other types of socioeconomic disadvantages that correlate with educational outcomes.

The second empirical challenge relates to data availability. Exposure to drug-related conflicts varies dramatically across time and space. Thus, any analysis of violence effects requires finer data on when and where conflicts take place. In order to track these events, we built a novel dataset based on thousands of anonymous reports of drug gang conflicts to a police hotline over the period between 2003 and 2009. We then read and geocoded these reports at the favela level, and matched this information with educational data by exploring distances between schools and favelas. The final dataset includes educational outcomes and exposure to local violence over time, either at the school or student level.

We focus our analysis on young students (5th graders) from schools located inside or on the borders of favelas. We show evidence that students from schools that are exposed to violence perform worse in standardized math exams. Conflicts during the academic year are associated with a decrease of 0.05 standard deviations in math test

scores. The violence effect increases with conflict intensity and duration, when the conflict occurs in the months just before the exam. The effect rapidly decreases with the distance between the school and the conflict location, which supports the view that the negative spillovers on education are geographically very localized. Also important, there is no evidence that the effect of violence persists over time. The results are not driven by student selection and are robust to placebo tests. In particular, we find no association between violence that occurs *after* the exam and performance *at* the exam. Finally, we find that violence impact on school resources is an important mechanism behind our results. Armed conflicts are significantly associated with higher teacher absenteeism, principal turnover and temporary school shutdown.

A growing number of studies examine negative spillovers of disputes for drug markets. Fryer et al. [forthcoming] suggest that the expansion of crack cocaine markets in the US led to adverse consequences such as an increase in homicide rates and low birth weight among blacks just after the introduction of the drug, when property rights were not established and the profitability of the business was high. Evans et al. [2012] argue that the introduction of crack cocaine in the US and the consequent spike in violence altered the behavior of young black males by decreasing their life expectancy, and decreasing their high-school graduation rates. Contrary to us, they argue that drug markets impact educational outcomes through changes to the returns to education, while our results emphasize the school resources channel. The mechanism emphasized by Evans et al. [2012] should play a smaller role in our context since we evaluate the effect on younger cohorts (around 11 years old), whose choices regarding education are still made by their parents. In addition, the fact that we find stronger effects for high-performing students, who usually take more advantage from school quality, reinforces that the impacts on the school routine are an important mechanism behind our results.

Other recent papers look at spillover effects of drug-related violence on economic outcomes. Frischtak and Mandel [2012] show that the removal of drug traffickers' rule from favelas in Rio leads to an increase in property values and reduces the inequality among residential prices.[1] Dell [2011] analyzes the drug war in Mexico and finds suggestive evidence that drug trafficking presence exerts negative economic effects on the general population. She argues that the negative correlation between drug trafficking presence and lower informal sector wages and female labor force participation

[1]Similarly, but focusing instead on terrorism violence, Besley and Mueller [2012] exploit within-region variability in violence and house prices over time in Northern Ireland to show that the peace process resulted in an increase in housing prices.

is consistent with the qualitative evidence that drug trafficking organizations engage in widespread extortion, particularly of poorer citizens. In our setting, we show that the negative spillovers arise because turf violence impacts the school resources, in particular, teachers' and principals' behavior.

To the best of our knowledge, there is no causal estimate available in the literature that unequivocally attributes to violence a negative effect on student achievement. The existing literature relies on cross-sectional analysis and faces difficulties in disentangling neighborhood violence from other types of socioeconomic disadvantage that have also had negative impacts on children's education, such as poverty, domestic violence and parental education (for instance, Aizer [2007], Severnini and Firpo [2009], Grogger [1997]). Though less related to our work, there is a strand of literature that evaluates whether more disruptive forms of conflicts, such as civil wars, affect education. This literature finds that school attainment decreases for those cohorts exposed to conflicts at school age (Akresh and Walque [2008], Shemyakina [2011], León [2009], Chamarbagwala and Morn [2011]). These conflicts, however, often cause economic and political chaos, and institutional and infra-structure disruption. The mechanisms that operate in our context are likely different and more specific. In addition, we are able to shed light on how violence affects education by exploring information on school resources and student mobility.

This paper is organized as follows. Section 2 describes the institutional background, while section 3 presents the data on violence and primary education in Rio de Janeiro. Section 4 presents a conceptual discussion and our identification strategy. In sections 5 and 6 we show the results. Section 7 concludes.

2 Institutional Background

2.1 Drug Gangs and Violence in Rio de Janeiro

In 2009, 2,155 people were murdered in the city of Rio de Janeiro, which is equivalent to a homicide rate of 32 per 100,000 habitants. This rate is comparable to those of the most violent cities in the United States, such as Detroit (40 murders per 100,000 habitants), Baltimore (37) and Newark (26).[2] This record, already high for international standards, masks striking differences of violence exposure within the city. Poor neighborhoods in the Northern zone of the city experienced 60.3 deaths per 100,000 inhabitants in 2009. On the other hand, the rich neighborhoods of the

[2]Source: FBI's Uniform Crime Reporting (UCR) Program.

Southern zone recorded a homicide rate of approximately 6.6.[3]

Violence in Rio de Janeiro took off in the early 1980s. This period is marked by the foundation of Comando Vermelho (CV), the first organized major drug gang of Rio de Janeiro (Dowdney [2003]). Drug dealers relied on the marijuana trade network already established in Rio de Janeiro's favelas to sell cocaine. Control over the favelas' territory became crucial to protect the illicit trade. The favelas' geography, marked by tiny streets and corners, as well as a lack of formal rules within its boundaries, make them an important market for drugs and a strategic place to hide from police (Silva et al. [2008]). The higher profitability of cocaine trade changed drug trade dynamics and soon led to increasing quarrels among gang members. As a result, some members left Comando Vermelho and created Terceiro Comando (TC) in the late 1980s (Misse [1999]). In the 1990s, two other gangs, Amigos dos Amigos (ADA) and Terceiro Comando Puro (TCP), were created by dissidents of the two former gangs. This fractionalization led to more armed conflicts over the favelas, and to an increasing militarization of the drug gangs (Misse [1997]). The arsenal used in the conflicts has often included heavy weaponry, such as grenades and modern military machine guns (for instance, M-16, AK-47, AR-15, .30 and .50 caliber machine guns).[4]

There is scant research on what triggers these conflicts. We gathered qualitative evidence from research in sociology, media coverage and conversations with the Intelligence Unit of the Military Police in order to better characterize drug gangs' behavior and understand the determinants of conflicts. Overall, we find evidence supporting the view that the conflicts between drug gangs are not strategically planned and often respond to idiosyncratic triggers, such as the imprisonment or release of a gang leader, betrayals and revenge. According to Misse [1997] and Souza [2001], the Rio de Janeiro's drug gangs do not have a hierarchical structure ruled by a drug baron in the models found in Colombia or in the Italian mafia. Dowdney [2003] defines the drug gangs of Rio de Janeiro as "networks of affiliated independent actors", while Baptista et al. [2000] emphasize that the gangs are controlled by a group of independent leaders who are inexperienced and young. Though some coordination may occur among leaders within gangs,[5] each favela has a local boss who runs the operations independently and decides how to defend the territory and whether to attack the rivals.

[3]Source: Instituto de Seguranca Publica do Estado do Rio de Janeiro (ISP).

[4]According to the 2009 ISP Annual Report, about 30% of all illegal weapons collected through police operations in 2009 in the State of Rio de Janeiro were classified as weapons of "high destructive power", such as large-caliber machine guns.

[5]See Lessing [2011] for details on how gang affiliation matters within prisons.

Case studies and conversations with officers of the Intelligence Unit of the Military Police support the view that there is an unstable equilibrium of forces among local leaders. The local boss controls the favela and maintains the order until the "peace" is broken by the imprisonment or release of a gang leader, betrayals, honor-related violence or assassinations of gang members. In the words of a favela resident quoted in Perlman [2010], "things are quiet here when a gang is in control. But if the leader is killed or imprisoned, all hell breaks loose - there is a war over who will control the turf". The newspaper coverage also supports that conflicts are triggered by very specific reasons. Some fragments from newspaper articles exemplify this argument:

Fragment 1: Three people died and eight were wounded after Vila dos Pinheiros invasion by Baixa do Sapateiro drug dealers...The invasion was led by Nei da Conceição Cruz, known as Facão (machete), the main leader of Terceiro Comando Puro (TCP). The conflict began at 10 pm and lasted the whole night. The operation was supported by Matemático (mathematician) (...). Facão and Matemático left the jail last month after winning in Court the right to work outside the jail and come back to sleep. Both criminals did not return to jail after the first day under the new sentence. (Source: Meia Hora, 5/31/2009)

Fragment 2: Drug dealers from Morro dos Macacos reobtained the control of three favelas in Água Santa with the support of drug dealers from Rocinha and São Carlos (...). The area was under militia control since last year. The conflict lasted five hours. According to the police department, the invasion was led by Luciano de Oliveira Felipe, known as Cotonete (cotton swab), who is the former favela traffic manager. He was deposed one year ago and was hidden in Morro dos Macacos. (Source: Meia Hora, 6/12/2009)

Fragment 3: ...in July, Marcus Vinicius Martins Vidinhas Júnior, known as Palhaço (clown), betrayed his father-in-law, Celsinho da Vila Vintém, who is in jail but is still the favela drug baron. Palhaço killed 13 drug gang members in order to control drug trade slots. Two days later, Celsinho's allies deposed Palhaço, who ran away with guns and R$ 1 million. (Source: Meia Hora, 9/22/2009)

In Appendix A we transcribe other articles that support the role of idiosyncratic

factors as conflict triggers. These fragments also indicate how violent these events are. People who live in conflict areas or near them are the most affected. Freedom of movement is drastically restricted while the chance of being hit by a stray bullet is imminent. People who are associated with a drug gang can be evicted from their homes or murdered when a new gang assumes control. In addition, the transcriptions show that conflict duration can vary greatly. Conflicts to depose a gang can take hours or days and are usually followed by attempts to reconquer the territory. This effort to regain control may occur in the same week or a few months later, depending on how much support the deposed gang can gather from other leaders. Therefore, when a conflict begins, it is hard to predict when it is going to end. The impact of these conflicts on city daily routine is also attested to with the answers from a victimization survey carried out in 2007. Fear of a stray bullet (60%) and being caught in the crossfire (44%) were mentioned as the violent events which respondents were most afraid of, followed by robberies (37%).[6]

The police force in Rio de Janeiro is subordinated to the State Secretariat of Security. Policemen are underpaid, have a long history of corruption, and are outarmed by the drug gangs (Perlman [2010]). The police do not always intervene in the gang conflicts. When they intervene, however, it is usually after the first battles, and in particular when the conflict reaches larger proportions and public attention. Until recently, the interventions were aimed only to interrupt the conflict, and not to definitively remove the drug dealers' control over the favelas.[7]

2.2 The Favelas of Rio de Janeiro

Most of the conflicts occur in favelas, which does not imply that all favelas are controlled by drug gangs, or are constantly under conflict. The Rio de Janeiro's City Plan defines favelas as those areas used mainly for housing, characterized by tiny and irregular streets, irregular plot size, poor urban services and a lack of the usual legal procedures of formal licensing and construction.[8] There are 979 favelas in Rio de Janeiro according to Instituto Pereira Passos, which concentrate 1.093 million people or 19 percent of the city population (2000 Census data) in those areas. Figure

[6]This survey was carried out by DATAUFF and interviewed 4,000 people in the Rio de Janeiro metropolitan area. The percentage shown corresponds to answers from people who live in the city of Rio de Janeiro.

[7]The police force strategy started to change only by the end of 2008, when the state government implemented the first Pacifying Police Unit (UPP), which aims to remove permanently the drug traffickers rule from the favelas.

[8]See Article 147 of Rio de Janeiro's Plano Diretor (Law number 16/1992).

1 shows the map of the city of Rio de Janeiro with the favelas' locations. As we can see, the favelas are quite widespread across the city.

Although favelas are poverty enclaves, not all families that live in favelas are poor, nor do all the urban poor live in favelas (Perlman [2010]). Access to urban infrastructure, especially water and electricity distribution, has improved in the favelas in the last two decades, and nowadays they are not markedly different from other areas of the city (Vianna [2008]). Yet, social inequalities are still persistent. According to Neri [2010], in 2007-2008 earnings and education amongst the favela inhabitants were significantly lower in comparison to the earnings and education levels found among non favela inhabitants (average earnings were 49 percent lower, while years of education were 3.5 lower amongst favela dwellers).

2.3 The Municipal Education System

The municipal administration is the main elementary school provider in Rio de Janeiro. The municipal system of Rio is one of the largest in Brazil, comprising 1,063 elementary schools and 550,000 students. First to fifth graders, which are the focus of our analysis, correspond to 46 percent of the students enrolled in the system. There are no school districts in the city and students can choose any school to attend. Some schools have more demand than others, which implies that some students do not end up in their first school choice.[9] The public school network is complemented by the private system, although private coverage is low among poor students. Only 2.5 percent of favela inhabitants attend private schools, while 12.7 percent of other city inhabitants study in the private system (Neri [2010]).

About 36,000 teachers work in the municipal school system. All professionals are hired through public contests. Wages are the same across schools but vary with seniority and additional duties. Recently hired teachers are allowed to choose among open placements across different regions, but do not have control over the specific school where they are going to work in the chosen region. There is mobility across schools between years, but it depends on seniority. After three years working in the system, professionals can apply to transfer to another school. Conversations with professionals suggest that some teachers indeed manage to move away from violent areas between years. Within years, however, teachers can only respond to episodes of violence with absenteeism and attrition.

[9]See Costa et al. [2010] for a discussion of the process of registration in public schools in Rio de Janeiro.

Figure 1 shows that the distribution of schools in the city is widespread. This feature, along with the fact that 98% of the children at school age attend school in Rio de Janeiro, indicates that school coverage is not a main concern in the city. However, there are several issues related to school quality. An assessment made by the Municipal Secretariat of Education in 2009 showed that 15% of students (28,000) at the 4th, 5th and 6th grades were actually functionally illiterate (Prefeitura [2009]). In addition, inequalities across the city are still persistent. Neri [2010] shows that favela inhabitants study less than 1 hour and 15 minutes per week compared to other city inhabitants, due to a combination of higher dropout rates, lower school load and higher absenteeism.

3 Data

3.1 Data on Violence

The understanding of the consequences of Rio de Janeiro's armed conflicts requires detailed information on where and when conflicts take place. This is necessary because violence exposure varies across and within neighborhoods. Official crime data, which is provided by Instituto the Segurança Pública (ISP), cannot track differences in violence exposure since it records information gathered by police stations and then aggregates it into 18 major city areas. In addition, ISP does not track information on when and where conflicts happen, but only on homicides, which is one of the outcomes of these conflicts. To overcome the lack of finer data, we build a novel dataset based on anonymous reports to Disque-Denúncia, a crime hotline open to the public for the reporting of any problems associated with security or public order which require government intervention.

Disque-Denúncia (DD) is a NGO created in 1995, and sits inside the Police Authority of the State of Rio de Janeiro. The calls received by the hotline are directly forwarded to Civil and Military police, who decide whether and how to respond to each report. All the reports are anonymous and are neither recorded nor tracked. DD works 24 hours a day, 7 days a week and its phone number is broadly disclosed across the city (e.g. on supermarket bags and bus signs).

The reports are registered in a database which contains the date, location and description of each event. People call to report any kind of crime and irregularities, such as assaults, the location of criminals and corpses, or simple noise complaints. We get from DD all reports that mention an armed conflict among drug gangs between

2003 and 2009 in the city of Rio de Janeiro. We read all reports to make sure they described a gunfight and to standardize the addresses provided. The address and the description of the events allow us to associate most of the reports with a specific favela. We then aggregated the data per favela and year by counting the number of days per year when at least one report of armed conflict is reported to Disque-Denúncia. Appendix B details how the dataset is built.

Table 1 provides descriptive statistics for the reports about armed conflicts. There were 4,365 reports registered as 'gunfights between drug gangs' from January 1st, 2003 to December 31st, 2009. However, the analysis of the database showed that 523 reports do not describe a gunfight, which led us to exclude them from our analysis.[10] In addition, we exclude another 315 reports that we were not able to associate with a specific favela, leading to a final sample of 3,527 reports.[11]

The matching of 92% of the reports to favelas confirms the idea that favelas are the main conflict battlefield. However, favelas are not a synonym of armed conflicts. Table 1 shows that more than one-third of the favelas (338 out of 979) experienced at least one conflict between 2003 and 2009 according to Disque-Denúncia. We refer to this group as violent favelas. We see that the average number of reports in violent favelas is 1.5 per year or 10 reports between 2003 and 2009. In our analysis we use the number of days with conflicts in each favela rather than the number of reports in order to deal with the fact that one person can call several times to report the same conflict. The mean value of this variable in violent favelas is 1.2 per year and the standard deviation is 3. The dynamics of these events in the ten most violent favelas are exemplified in Figure 3. This Figure indicates that violence peaks in different years depending on the favela.

3.2 Educational Data

In order to determine the impact of drug-related violence on education, we use three educational databases that provides information at student-, school- and teacher-level. Our main outcome variable consists of student-level scores in Prova Brasil, a national standardized exam applied to all fifth graders in 2005, 2007 and

[10] The reports that were excluded mention the threat of conflicts among drug gangs, the location of drug dealers, or else they complement previously reported information. They are excluded because they do not mention that an armed conflict took place on the specific date.

[11] We were not able to localize the other 315 reports because they do not provide a specific address, or they mention a street that is not inside a favela or close to a favela border.

2009.[12] All students from public schools that had more than 30 students enrolled in the fifth grade in 2005, or more than 20 in 2007 and 2009, were supposed to take this exam. The exam is composed of two tests that measure math and language (Portuguese) skills. In addition, students answer a survey about their socioeconomic profile, while teachers and principals provide information on their experience and school conditions. In 2007 and 2009, the principals answered specific questions about school problems, which we use to understand how violence affects school routine. The Prova Brasil micro-data set is provided by Instituto Anisio Teixeira (INEP).

Panel A of Table 2 provides summary statistics about fifth graders who take the Prova Brasil exam. Our benchmark sample comprises 76,131 students from 336 elementary schools that participated in at least two Prova Brasil editions between 2005 and 2009, and are located within 250 meters from at least one favela. We include the full sample of schools in the heterogeneity analysis and robustness checks, which consists of all the 736 schools of the municipal system that participated in at least two Prova Brasil editions. According to our data, 47% of the municipal schools are within 250 meters of at least one favela, while 73% are within 500 meters. Table 2 shows school averages for the whole sample, and separately for schools exposed and not exposed to violence. We define the schools exposed to violence as those located within 250 meters of favelas that experienced two or more days of conflicts during the academic years (March-November) throughout the 2003-2009 period. This definition of violence exposure is fully detailed in section 4. According to this definition, violence affects 45 percent of the schools in our sample (152 schools). The data indicates that there are marked differences between schools exposed and not exposed to violence. The former present worse performance in terms of Prova Brasil scores. However, it is not clear whether the worse performance can be attributed to violence, since students from households of low socioeconomic status are over-represented in schools exposed to violence - which have a higher share of students with illiterate mothers, a lower share of whites and a higher share of students who have already repeated a grade or previously dropped out.

We complement the Prova Brasil dataset with administrative data from Rio de Janeiro's Secretaria Municipal de Educação (SME) from 2003 to 2009. This data covers all students enrolled in municipal schools and provides additional background information. In particular, the data set tracks information on student mobility within

[12]Prova Brasil is also applied to ninth grade students. However, we do not explore this exam because we want to avoid reverse causality. More drug conflicts can lead to more demand for soldiers (older boys), which might impact students' schooling decisions.

the system. This information includes all the municipal schools that each student has attended to in the past, the grade in which they are enrolled, and if and when they transferred across schools. These data allow us to calculate indicator variables for whether the student leaves or enters a school within the school year, or between academic years. Based on this data set, Panel B of Table 2 shows statistics for all the students from pre-school to fifth grade in our benchmark sample of schools. We observe that 17 percent of the students leave schools between academic years, which includes students who change schools and the ones who drop out. This number, however, is inflated by the fact that only a small share of schools provide the 6th or later grades. Thus, fifth graders usually change schools by the end of the academic year. Student mobility reduces to 7 percent within the academic year. The difference between the percentage of students leaving and entering schools suggest that dropout rates (or attrition, more generally) are around 10 percent between academic years, and 4 percent within years. Schools exposed to violence have higher mobility rates than others. Panel B also indicates that each school enrolls on average 800 students over the year, though enrollment in schools exposed to violence is higher, which may reflect high population density in areas surrounded by favelas. Interestingly, schools exposed to violence also have a higher proportion of students who study near their homes, which indicates that proximity to their households can be an important reason why parents choose to enroll their children in these low-performing schools.

The SME also provides administrative records on teachers' absenteeism and medical leaves from the 2007 though 2009 period, which allows us to calculate absenteeism rates, separately for unexcused absences and medical leaves. Panel C of Table 2 indicates that 16 percent of the teachers were absent from work at least one day during the academic year. Interestingly, this rate is lower for schools exposed to violence. We use the Educational Census (INEP) to obtain information on school infrastructure from 2003 and 2009. The data indicate that almost all schools provide free lunch, while only 41 percent have a computer lab, and 10 percent have a science lab. We observe that schools exposed to violence are usually those with worse infrastructure conditions. Finally, Panel D of Table 2 reports some stylized facts from a survey answered by the principals in the 2007 and 2009 Prova Brasil editions. This survey investigates several aspects of the school routine, including a long list of problems faced by the administration.

3.3 Other Data

This work relies heavily on geocoded information, which was provided by Instituto Pereira Passos (IPP). Key information is the favela borders, which are based on satellite pictures. This information is not only precise but also quite detailed, since it defines different favela borders even within large favela areas. As a result, the given definition led to 979 favelas (rather than about 300 given by other definitions), which allows us to better localize each event of violence. The IPP also provides shape files with municipal schools' location, Rio de Janeiro's main roads and neighborhood limits. Based on these shape files, we used GIS tools to calculate the area and population density of Rio de Janeiro's neighborhoods and distances from favelas to schools and main roads. In order to understand the determinants of conflicts, we gathered from IPP income per capita, Gini index, and population, calculated at neighborhood level based on the 2000 IBGE Census. We also obtained information on the favela areas for 1999 and 2004. The NASA website provided gridpoint information on Rio de Janeiro's elevation, which allowed us to calculate favela steepness. Finally, we get from IPP a list with the favelas' alternative names, which is necessary to match more precisely the DD reports to each favela.

4 Empirical Model

In this paper we analyze very localized but extremely violent events of armed conflict within the city. Once a conflict is triggered, safety concerns and threats to individuals' lives dramatically increase in the conflict's location. In this setting, we expect two main potential connections between violence and our main outcome variable, student achievement. First, violence may impact the school's human resources, for example, by increasing teacher attrition and absenteeism, by causing interruption of classes and school shutdown, as well as by possibly increasing workplace stress and principal turnover. Second, violence exposure may directly affect student learning through mental health and psychological impacts. We discuss these two channels in section 4.1, which provides the conceptual background from which we develop our empirical strategy (section 4.2) and that helps identify the potential caveats (section 4.3).

4.1 Conceptual Discussion

Violence may have substantial effects on learning through the school resources. As mentioned in Grogger (1997), the theory of compensating differentials predicts that teachers (or the school staff more generally) would demand a premium wage in order to accept work in a school at risk of violence. The author indeed finds evidence that violence at school is positively correlated with teachers' salaries in a nationwide sample of schools in the US, in the early 1980s. In our setting, as salaries are given, violence may lead to higher teacher attrition and absenteeism. As a result, it is straightforward to assume that student achievement will suffer as classes are discontinued. We also hypothesize that violence may have disruptive effects on the school routine and management. As supported by several reports in the media, extreme events of gang conflicts can affect the school routine by causing temporary school shutdown and interruption of classes. Additionally, principal turnover may also rise since the cost of being in charge of the school management is expected to increase.

The consequences of violence exposure may extend rather beyond the school resources channel. Research conducted by psychologists and psychiatrists has recognized the potential harmful effects of neighborhood violence on children's mental health. As revealed by the meta-analysis in Fowler et al (2009), the most notorious findings suggest that children exposed to community violence are at a greater risk for developing post-traumatic stress disorder (PTSD) symptoms.[13] In addition to PTSD, violence exposure would also be associated with internalizing symptoms in young children, such as depression and anxiety (Buckner et al 2004; Fitzpatrick, 1993; Weistet al 2001).[14] Research contrasting subtypes of violence suggests that the effect of violence exposure on negative outcomes may increase with the physical proximity of children to the violent event (Nader et al 1990; Fitzpatrick, 1993). Family

[13]This body of research usually refers to exposure to community violence as parent or child reports of victimization, witnessing, and hearing about violence experienced by youths outside of their homes. As defined in Fowler et al (2009, p.229), victimization by community violence refers to having been the object of intentional acts initiated by another person to cause harm, which include being chased, threatened, robbed, beaten up, shot, stabbed, or otherwise assaulted; witnessing refers to eye-witnessing an event that involves loss of property, threat of physical injury, actual injury, or death; hearing about community violence is learning of another person's victimization by neighborhood violence.

[14]This is consistent with two different, but not competing, views. First, younger children lack the mature coping skills that could prevent the development of internalizing problems (Farver et al., 2005). Second, though older children may develop initial internalizing symptoms in reaction to new or unusual exposure to violence, their symptoms might be expected to abate if they are continuously exposed to community violence over time. In this case they may become desensitized and suppress feelings of sadness or anxiety (Farrell and Bruce, 1997; Fitzpatrick, 1993; Terr, 1991)

support seems to attenuate the consequences of violence exposure on children.[15] Yet, parents who have been traumatized are also more likely to have children who feel unsafe or develop PTSD symptoms (Linares and Cloitre, 2004; Yehodaet al 2001). In this case, parents would transmit the consequences of the violent events to their children.

It is important to note that if families make decisions after observed changes to school inputs, parents might increase investments in children human capital in order to compensate for unexpected violence costs. First, parents' investment may moderate mental health consequences. Second, parents may also change their input decision rule in education, for instance, spending more time teaching their children at home, or even transferring them to a more distant public or private school. In this case, student attrition and absenteeism are also potential outcomes of exposure to violence.

Although there are different potential connections between local violence and children learning, there is no causal estimate available in the literature that unequivocally attributes to violence a negative effect on student achievement. The existing results in the research conducted by psychologists and psychiatrists have limitations, as identified by psychiatrist Joy Osofsky (1999, cited in Aizer 2009). One important shortcoming relates to the fact that neighborhood violence is generally correlated with other types of socioeconomic disadvantage (poverty, parental education, domestic violence), which in turn has been shown to have negative impacts on children's education. Thus, since the literature has not been able to disentangle violence from other detrimental confounding factors, the existing estimates possibly overstate the role of the violence effect (Aizer 2009). Another limitation arises from the difficulty defining or characterizing neighborhood violence, which leads to measurement error. Both of these shortcomings - omitted variables and measurement error - can be extended to previous studies in economics, as recognized by their authors (see, for instance, Grogger 1997, Firpo and Severinini 2007, and Aizer 2009).

4.2 Empirical Strategy

This section describes how we explore our data to avoid identification problems found in previous research and achieve a causal estimate of the violence effect on learning. The modeling of the production function for cognitive achievement is often

[15]Though not at all margins. For instance, Overstreet et al (1999) present suggestive evidence that availability of family support attenuates the negative effects of exposure to community violence on internalizing symptoms, though PTSD does not seem to respond to this moderator.

based on theoretical notions that children's development is a cumulative process, dependent on the history of family and school inputs and on innate ability (Todd and Wolpin, 2003). Rather than attempt to estimate a tightly specified education production function, we propose a reduced-form strategy which explores the fact that variations in conflicts within favelas are orthogonal to any other past and contemporaneous latent determinants of learning. We estimate the following equation.

$$A_{ist} = \beta V_{st} + \mu_s + \gamma_t + Z'_{ist}\alpha + X'_{st}\psi + \varepsilon_{ist} \tag{1}$$

where A_{ist} is the learning outcome of student i, enrolled in the 5th grade at school s, in year $t \in \{2005, 2007, 2009\}$. Learning is measured by standardized test scores in math and language, available in the 2005, 2007 and 2009 Prova Brasil editions. The variable of interest is V_{st}, a dummy that indicates whether the school s is exposed to violent events throughout the academic period in year t. More precisely, we define this variable as

$$V_{st} = 1 \text{ if } \sum_j \mathbb{1}\{D_{sj} < B\}\vartheta_{jt} \geq n, \text{ and 0 otherwise} \tag{2}$$

Where ϑ_{jt} is the number of days with a recorded report of gang conflict in favela j throughout the academic year t. In our benchmark specification, this period includes the months between March through November (the month in which the Prova Brasil exam is taken). The term $\mathbb{1}\{D_{sj} < B\}$ is a function that indicates whether the linear distance D_{sj} between the school s and the favela j' border is smaller than B meters. Our benchmark specification sets the buffer $B = 250$, at which value the variable V_{st} captures only the conflicts that take place near the school, i.e., in favelas located up to 250 meters from the school. The benchmark specification also sets $n = 2$. In this case, the variable V_{st} captures whether the school experienced two or more days of violence within $B = 250$ meters of distance during the school period. By defining $n = 2$, we exclude isolated and unimportant events of shooting that may add noise our analysis. The formula (2) consists of a straightforward and flexible way of measuring violence. We can easily compute this variable at different values for the parameters B and n, which enables us to better characterize the violence effect (by distance B and intensity n) and perform robustness checks.

The terms γ_t and μ_s in equation (1) are year and school fixed effects, respectively.

Year fixed effects capture common time trends, such as macroeconomic and labor market conditions at the municipal level, political cycles, and common educational policies. School dummies control not only for unobserved heterogeneity at the school level, but also for fixed neighborhood characteristics around the school. For most students this also controls for neighborhood characteristics around their households, since 79 percent of them live within 15 minutes walking distance to their schools. The within-schools estimator eliminates the cross sectional variation in violence levels and captures idiosyncratic shocks driven by conflicts. Thus, we remove the effects of the *presence* of a drug gang in the locality (and therefore remove the cross-sectional variation in socioeconomic disadvantages correlated with chronic violence), and keep only the effect of the violence resulting from a fight *between* drug gangs.

The term Z_{ist} includes student socioeconomic characteristics in order to absorb within-school heterogeneity and limit potential selection bias in the pool of students taking the Prova Brasil exam. Here we include students' gender, race, level of mother's education, age fixed effects, and dummy variables for whether the child has ever repeated a grade or dropped out. The term X_{st} absorbs confounding effects driven by within-school heterogeneity in classroom size and composition (which includes the number of students and the averages for the students' socioeconomic characteristics mentioned above), as well as by school physical infrastructure (dummy variables for whether the school has a computer lab, science lab, principal's office, teachers' offices, free lunch and kitchen).

We focus on young children (5th graders) in order to avoid potential endogeneity driven by reverse causality - lower school quality leading to children soldiering and higher violence. Our benchmark sample includes only the students enrolled in schools located within 250 meters from at least one favela, which retains 336 schools, or 45% of the total number of municipal schools that participated in at least two Prova Brasil editions. This restriction makes treatment and control groups of students more similar as it accounts for the fact that schools near favelas are possibly exposed to higher levels of chronic violence, and are also attended by students from more disadvantaged households (who perform less well in school, on average, than their peers from more advantaged backgrounds). We nevertheless confirm that our results are robust to sample selection.

Identification relies on the assumption that, conditioned upon school and time fixed effects, as well as on students and school observed characteristics, unexpected and severe conflicts between drug gangs within favelas are uncorrelated with any latent determinant of children's education. Under this assumption, we are able to

identify the causal impact of violence on student achievement given that our variable of interest V_{st} should be orthogonal to the error term ε_{ist} in equation (1). In all specifications at the student level, we use robust standard errors clustered by school, the level at which we measure violence. The coefficient of interest β captures a reduced-form effect, which includes the impacts transmitted through all the main potential channels likely to be at work in our setting (as discussed in section 4.1). Though it is not possible to disentangle the relative importance of each potential mechanism, we provide suggestive evidence on the role of the school resources channel. We are also able to identify whether the parents respond to the conflicts in terms of student mobility and absenteeism. In the following section we discuss additional strategies and some stylized facts that help us validate our empirical strategy.

4.3 Validating the Empirical Strategy

A first potential problem that pervades our analysis concerns student selection at the Prova Brasil exam. In our setting, students are not constrained to study at schools near their homes. Parents' choices may therefore lead to students' attrition. In particular, if high-performing students move from a school exposed to violence towards another located in a non-exposed area, the estimated effect of violence on achievement at the end of the year may capture the worsening of the pool of students, and not the causal impact of violence on learning.

We perform two tests for selection. First, we explore the SME administrative records on student enrollment. Given that we can follow a student's enrollment number over time and across schools, we can fully identify all her movements within the system (transfers between schools) or out of the system (in this case, if her enrollment number disappears from the records). We are therefore able to examine whether violence impacts student mobility and attrition. Second, we also test selection at the Prova Brasil exam by examining whether exogenous socioeconomic characteristics of the students who take the exam are correlated with the violence during the academic year, after conditioning on school and year fixed effects. We find that the within-school variation in violence is orthogonal to mobility and other determinants of student achievement, which suggests that our estimates are not biased by self-selection of students into or out of a particular school.

A second potential concern relates to measurement error. We measure violence from anonymous reports, and propensity to report may vary within regions, and over time. Given that we explore within-school variation, our estimates are at risk

if the propensity to report in some neighborhoods changes due to factors also correlated with student outcomes. In order to test for bias in violence measurement, we cross-check the Disque-Denúncia data with official homicide data which is only disaggregated into 18 major regions of the city. When we aggregate our reports following the same division in homicide data, we observe that trends in both series are remarkably similar. In addition, we plot the relationship between the homicide rates and the number of days with reports by region, separately for each year, and observe that regional propensity to over- or under-report is constant over time. These tests are detailed in Appendix C. For the interested reader, we also present in Appendix C a comparison between the frequency of DD reports, by favela and year, with newspapers coverage of the conflicts. We find that the DD reports provide a much more complete picture about the conflicts than the media does.

Finally, Appendix D presents a further characterization of the conflict dynamics based on DD data by examining their socio-demographic determinants and their time-series properties. We first show that the cross-sectional variation in conflicts at the favela level correlates only with specific geographical fixed-effects, such as the favela's steepness and area size. More important to our analysis, factors that are usually associated with crime, such as income levels and inequality, do not explain variation in violence. Second, panel data extensions of the Dickey-Fuller unit root tests reject the null that the conflicts are non-stationary at the favela-month level over the period we analyze. Also important to mention, estimates of partial autocorrelation functions provide evidence that the conflicts follow either a very weak AR(1) or a white noise process at the favela-month level. These time-series properties eliminate any concern related to the presence of spurious correlation driven by non-observable trends or breaks in the data.

5 Results

5.1 Impact on Student Achievement

Table 3 displays the results for our baseline specification, equation (1). Panel A shows the effects of violence on math test scores, and Panel B reports the violence impact on language achievement. Column 1 presents our simplest specification, which includes only year fixed effects. In column 2 we add controls for student, classroom and school characteristics in order to absorb confounding effects driven by observed heterogeneity in students' background, school infrastructure, classroom

size and composition. Column 3 is our full specification. It adds school fixed effects and presents our within-school estimates. The sample includes students from schools located within 250 meters of at least one favela. The variable of interest captures whether the school experienced two or more days of violence during the school period within a radius of 250 meters of the school.

In Panel A, column 1 shows that there is a significant negative correlation between violence and math achievement, though this result is conditioned only on year fixed effects. When we move from column 1 to column 2, in which specification we include cross-section controls, the point estimate reduces only slightly. This result indicates that the heterogeneity in students, classroom and school characteristics plays a limited role in generating the observed correlation between violence and math achievement. Column 3 reports our within-school estimates. Within-group estimators used to control for fixed effects may isolate omitted variable bias, but they also typically remove much of the useful information in the variable of interest. In our case, deviations from means eliminate cross sectional variation in violence levels. As we move from column 2 to column 3, the correlation indeed drops a little more in magnitude, but nevertheless remains statistically significant at 5%. As we discuss in section 4, this effect can be regarded as causal since, conditional upon time and school fixed effects, the remaining variation in the variable of interest is plausibly idiosyncratic. In the following sections we strengthen this view by showing that our results are neither driven by student selection, nor by different ways of restricting the sample or defining the variable of interest.

Panel B repeats the same sequence of specifications for language test scores. The coefficient drops relatively more as we move from column 1 to 3, where it remains negative but is no longer statistically significant. This result suggests that both observed and unobserved heterogeneity tend to fully absorb the relationship between violence and language achievement shown in column 1. This pattern is not surprising given the common view that performance in language is expected to be strongly associated with household background.

Column 3 from Table 3 is our preferred specification and is the one we use in the remainder of the paper. The magnitude of the coefficient on the violence indicator we find in this column is quantitatively important. Each episode of violence triggered by drug gangs leads to a reduction of 0.054 standard deviations in math test scores. This effect represents 50% of the coefficient estimated in a regression of math test scores on a dummy indicating low-educated mother (with no or only primary education).

In the following sections we further characterize the violence effect on achievement

and present robustness tests. Given the results from Panel B, the remainder of the paper focuses on achievement in math. First, we examine whether the violence effect varies with the distance between the school and the conflict location. In section 5.3 we test whether the violence effect is sensitive to the conflict intensity and length. In section 5.4 we study the specific timing of the violence impact. Section 5.5 explores heterogeneity in the violence effect by students characteristics. Finally, section 5.6 examines the effect of violence on students' mobility among schools within the school year, and between years, and tests for the presence of bias in student selection at the Prova Brasil exam. Throughout these sections we provide evidence that, irrespective of how we measure violence or restrict the sample, we detect a negative and statistically significant impact of violence on student achievement. We also rule out selection bias in different ways.

5.2 Distance to Favelas and Sample Selection

In this section we examine how the relationship between violence and student achievement varies with the distance between the school and the conflict location. In the first five columns of Table 4 we report regressions that include all the schools of the municipal system that participated in at least two Prova Brasil editions, irrespective of their distance from favelas. In the first column, the variable of interest considers only the conflicts in favelas within a radius of 5 meters of the school. In the second column we add the violence that takes place in favelas within a radius of 250 meters of the school. We keep expanding this buffer in the following 3 columns, until we reach the violence that occurs in favelas within 1000 meters of the school. Since these indicators are not mutually exclusive, they capture the differential effects of violence on learning as we increase the distance of the school to the conflict location.

We observe in the first five columns of Table 4 that violence has a very local impact. We detect a significant negative relationship between violence and achievement only for those episodes of violence that take place near the school, more specifically, up to around 250 meters of the school. The magnitude of the point estimate at the 250 meters cutoff in columns 2 through 5 is only slightly larger than our benchmark coefficient of 0.054. Columns 6 through 9 further test these findings. Column 6 includes only those schools located inside favelas (within 5 meters). In this case, the control group includes only the schools located in favelas, but not exposed to gang conflicts. Despite the small sample, we find a higher coefficient (0.130), significant at 5%. Columns 7 through 9 show that our results are not sensitive to the 250 meter

cutoff. In these columns we restrict the sample to those schools within 300 meters of a favela. In column 7 the variable of interest considers only the violence in favelas within a radius of 200 meters of the school. In columns 8 and 9 we expand this buffer to 250 and 300 meters, respectively. As we move from column 7 through 9, the coefficient drops monotonically, which supports the view that the violence effect reduces with distance.

5.3 Intensity

Our benchmark measure of violence is a dummy variable that indicates whether the school experienced two or more days of conflict during a certain span of time (the academic period), and within a certain distance from the school (the buffer of 250 meters is the benchmark). In this section we further characterize the relationship between violence and achievement by varying the number of days with conflict during the academic year, within the buffer of 250 meters. In other words, we test whether violence impacts vary with conflict intensity by assuming that violence intensity increases with the number of days of conflict.

We perform two tests. First, we compute different violence indicators by varying n in equation (2), the number of days of conflict during the school period, that occur within a radius of $B = 250$ meters from the school. The first column of Table 5 presents the effect of violence on math achievement, where the violence indicator is defined for $n \geq 1$. The second column presents our benchmark result, where $n \geq 2$. Columns 3 and 4 show the results for $n \geq 7$ and $n \geq 9$, respectively. As shown in these four regressions, the effect of violence on student achievement increases with violence intensity. In column 1 we observe that the effect on achievement of one or more days of conflict is not statistically different from zero. The second column presents our benchmark estimate. Columns 3 and 4 show that the impact doubles when we consider 7 and 9 or more days of conflict, respectively. Figure 4 complements Table 5 by plotting the coefficients of nine different regressions of student achievement on the violence indicator, each computed for a distinct $n \in (1, 9)$. We see a clear negative relationship between the effect of violence and violence intensity, captured by n. We also observe that the confidence intervals around the estimated coefficients (at 5% and 10%) tend to increase with n, which is a consequence of the small number of very intense conflicts used to detect the effect of violence for larger values of n.

We also perform a second test, in which the violence indicator is calculated in two alternative ways. In column 5 of Table 5, the variable of interest indicates whether

the school experienced two or more days of conflict within 14 contiguous days during the school period. In column 6, conversely, the variable of interest indicates whether the school was exposed to two or more days of conflict within the school period, but more than 14 days apart. We assume that two or more days of conflict within a lengthy but not large span of time indicates that the conflict has continued over time and, for this reason, can be regarded as a more disruptive event. Though the coefficients are not significantly different, the comparison of the results in columns 5 and 6 is supportive of the view that the effect of long-lasting conflicts is higher than the impact of episodes of violence sporadically distributed over the school period.[16]

5.4 Timing

Another important aspect of the effect of violence on student achievement is the specific timing of the impacts. The question of timing has at least two relevant dimensions. First, the extent to which student achievement by the end of the year varies with the moment of the violence shock during the school year. Second, the extent to which violence has either persistent or transitory effects on learning.

In order to explore the timing of the violence effect, we perform two tests. First, we break the computation of the violence indicator into three different periods of the calendar year: (i) the violence in the March through June period, the first school term; (ii) August through November, the second term and the months just before the Prova Brasil exam; and (iii) December through February, the months just after the exam. This procedure gives us three new indicators of violence, each of them for a specific period of the year.

The first column of Table 6 reports a regression of math achievement on these three indicators of violence. The coefficient for the December through February period provides a natural placebo test in within-group estimation. As expected, it indicates that the violence that occurs *after* the exam is not significantly associated with performance *at* the exam. Only the effect associated with the second term, which precedes the exam, is significantly different from zero (only at 10%).

In the remainder columns of Table 6, we complement the analysis by exploring the relationship between achievement and our benchmark measure of violence, but computed either for the previous or the following school year. Column 2 reports the regression of achievement on the violence computed in the following year. As

[16]We alternatively considered windows of 7 and 21 contiguous days of conflict, and the results are qualitatively similar.

expected in this alternative placebo test, we observe no association between violence during the following academic year and performance in the current year.[17]

In the third column of Table 6 we regress student achievement on the violence that occurred in the previous school year. Since learning is a cumulative process, this regression tests whether violence has any persistent effect on student achievement. As a result, we find no significant association between achievement and past violence. This is consistent with other studies that also find that treatment effects on education fade away rapidly (see Kane and Satiger [2008], Jacob et al. [2010], Rothstein [2010], Banerjee et al. [2007], Andrabi et al. [2011], Herrmann and Rockoff [2010]).

5.5 Heterogeneity

Table 7 examines the heterogeneity in the violence effect by students' socioeconomic characteristics. We split the data by student gender, race and age, by level of mother's education, and by the indicators of whether the student has ever repeated a grade or dropped out. The first two columns of Table 7 show that the coefficient for girls' achievement is larger than that for boys. In columns 3 and 4 we find a larger coefficient for white students, roughly twofold that estimated for non-whites. Columns 5 and 6 show that the coefficient is larger for students with high-educated mothers. The coefficients by age look similar in columns 7 and 8, though slightly larger for younger students (aged 11 or less, i.e., at correct age for 5th grade). Columns 9 and 10 show that the coefficient for students who have never repeated a grade is larger. In columns 11 and 12 we find a larger coefficient for those students who have dropped out before. Overall, though these differences are not statistically significant, they indicate that violence is more detrimental to high-performing students (those with high-educated mothers, whites, at the correct age for grade, and that never repeated a grade). The only exceptions are the students that have dropped out before. Despite being low-performers, they seem particularly susceptible to violence shocks.

If high-performing students in particular benefit from instruction at school, the results in Table 7 support the view that school resources likely work as a significant link between violence and learning. Section 6 provides more direct evidence in support of this view.

[17]Note that our sample drops since we do not have information on violence for 2010, the year after the latest Prova Brasil edition, in 2009.

5.6 Student Mobility and Selection

In our setting, students are not constrained to study at schools near their homes. A major concern regarding our empirical strategy, therefore, is student selection. The observed correlation between violence and student achievement may be spurious if violence is also associated with student mobility. In particular, if high-performing students move from a school exposed to violence towards another located in a less violent area, the estimated effect of violence on achievement at the end of the year may be capturing the worsening of the pool of students, and not a causal impact of violence on achievement.

In order to examine whether violence affects student mobility across schools, we explore the SME administrative records on students enrollment. Given that we can follow the student enrollment number over time and across schools, we are able to fully identify all her movements within the system (transfers between schools) or out of the system (in this case, if her enrollment number disappears from the records).

Table 8 presents regressions at the student-year level that explore the relationship between violence and student mobility. All columns follow a within-school specification, which controls for students characteristics, grade, year and school fixed effects. To make these regressions comparable to our previous results, we restrict the sample to schools located within 250 meters of at least one favela, while the variable of interest considers only the episodes of violence that occur in favelas within a radius of 250 meters of the school. Panel A considers all students enrolled in the 1st through 5th grades, and Panel B includes only the 5th graders. The sample covers the 2003 through 2009 period.

In the first column of Table 8, the dependent variable is an indicator of whether the student moves out of her school within the school year. This includes either movements to other schools or dropouts. We observe in both panels that violence is not significantly associated with a higher probability of observing a student moving out of the school. As shown in column 2, the violence effect on new entries to the school during the academic year is also not statistically significant. Finally, columns 3 and 4 test whether violence affects student mobility between academic years. We see that there is also no evidence that violence is associated with a higher probability of transferring between years. One likely explanation for these results is that parents may expect the conflicts (and their consequences) to be temporary, which in turn would increase the opportunity cost of moving their children to another school. This is consistent with the evidence provided in section 5.4, which suggests

that the violence effect on learning is not persistent.

We complement this analysis by testing student selection at the Prova Brasil exam. We regress the socioeconomic characteristics of the students that take the exam on the violence during the school year. The first column of Table 9 follows our benchmark specification, in which we regress on violence a dummy variable indicating gender equal to male. We see that the violence during the school year is not significantly associated with a higher probability of observing a male in the pool of students taking the exam by the end of the year. In the following columns, we repeat the same specification, but for other binary dependent variables - race (non white), age (12 or more), mother's education (low), ever repeated or dropped out. As a result, we find no systematic association between violence and student selection.[18]

6 The Impact on School Resources

Throughout the previous sections we followed a reduced-form strategy in order to identify and characterize an average effect of violence on learning. This effect possibly includes the impacts transmitted through the different channels likely to be at work in our setting. Since we are able to observe teacher and principal behavior, the final effort of this paper focuses on the identification of specific mechanisms linking violence and school resources.[19]

6.1 Impact on School Routine

In Table 10 we examine whether violence affects the school routine. We rely on a survey answered by principals in the 2007 and 2009 Prova Brasil editions. This survey investigates several aspects of the school routine, including a long list of problems faced by the administration. We regress an indicator variable for whether the principal mentioned a given problem on the indicator of violence. The regressions include the full set of student and school controls, plus school and year fixed effects.

Panel A of Table 10 indicates that, in schools exposed to violence, principals

[18]The number of observations varies across the columns because of missing values in the Prova Brasil survey. In order to test whether this problem affects our results, we regressed, for each student characteristic, a dummy indicating missing value on the violence indicator. We find no association between missing observations and violence.

[19]As already discussed in section 4.1, in our setting, violence exposure may also affect learning through mental health symptoms. Although we acknowledge the potential role played by this mechanism, we are not able to empirically test whether or to what extent it helps generate the observed relationship between violence and achievement.

were 7.7 percentage points more likely to report that there was a threat to teachers' live, which represents 40% of the sample mean. There is also evidence that violence impacts teacher turnover, which increases by 5 percentage points (13% the average) in violent years according to principals' reports, though this result is only significant at 0.14 (p-value). Panel B looks at differential impacts depending on whether violence is experienced in contiguous or non-contiguous days. Column 1 shows that principals are 24 percentage points more likely to report that interruption of classes (temporary school shutdown) was a problem in the school in years with contiguous days of violence. This implies that schools are twice as likely to shut down in years with conflicts of long duration. This is consistent with several articles in Rio de Janeiro's main newspapers that mention that schools temporarily shut down during the conflicts in order to avoid teachers and students getting caught in the crossfire.

Also interesting, column 5 indicates that the administration of the schools are more likely to change in years with contiguous violence. The schools that are exposed to this type of event are 12 percentage points more likely to have a principal that is less than two years on the job, which represents a 31% increase in the sample mean. This is a reasonable finding considering the tremendous stress that principals face in managing schools during a conflict period.

Finally, we find no significant association between violence and student absenteeism, as also reported by the principals. It is reasonable to assume that principals may not consider absenteeism in periods marked by interruption of classes or unusual stress. The result nevertheless suggests that student absenteeism does not systematically increase in years when conflicts take place.

6.2 Teacher Absenteeism

Table 11 examines teachers behavior in terms of absenteeism and medical leaves. We use three years of data (2007-2009) to evaluate violence effects on both the extensive (percentage of teachers) and the intensive margins (average number of days of absence). Column 1 indicates that in years with episodes of violence, the percentage of teacher absences increases by 5.8 percentage points, which represents 38% of the sample mean. Panel B indicates that the effect is qualitatively similar for both contiguous and non-contiguous violence indicators, though only in the former is the coefficient statistically different from zero. Column 2 indicates that the variable of contiguous violence is associated with an increase in absenteeism at the intensive margin. There is no evidence that violence affects medical leaves.

The results of Table 11 are qualitatively relevant since they associate violence with a great number of days without instructors in the classrooms because of teacher absenteeism. This result can be regarded as either a lower bound of the violence effect on absenteeism or evidence on teacher turnover. Unexcused absences are reported by the school's principal and are subjected to endogeneity since the principal may under-report absenteeism in response to violence and safety threats. In fact, long-lasting absences, which are generally followed by employment resignation, are likely to be better reported.[20] Thus, the violence effect on unexcused absences may be interpreted as a combination of impacts on short and lengthy absenteeism, where the latter is possibly a combination of absenteeism and turnover.

7 Conclusion

This study provides evidence that drug-related violence causes negative spillovers on the the population that lives and works in areas surrounded by violence by showing that it affects student achievement and education supply. Despite the fact that such incidents of violence have become more preponderant in diverse regions in the world, we have a limited understanding of its consequences due to the challenges of empirically estimating their effects. As previous research has shown and our data suggests, violence correlates with poverty and a simple cross-section analysis that aim to assess its impact would be subject to omitted variable bias. We circumvent this challenge by exploring variation over time in armed conflicts between drug gangs that are plausibly exogenous to local socio-economic conditions.

We show that students from schools located close to conflict areas perform 0.05 standard deviations lower in violent years relative to their peers in the same schools in peaceful years. In addition, we are able to point out one mechanism through which violence affects student achievement. In particular, we provide evidence that violence decreases instructional time and affects individuals working decisions by increasing principal turnover and teachers' absenteeism. Interestingly, we do not find that students respond to these conflicts by leaving schools exposed to violence. Although we can only speculate about the reasons for that, we believe that mobility costs and difficulty in evaluating alternatives explain why students do not leave schools after violent events.

In order to carry out this study, we built a novel dataset that contains detailed in-

[20] As revealed by informal conversations with the administrative staff of the SME Department of Human Resources.

formation on the dynamics of drug-related conflicts in Rio de Janeiro. This database is an additional contribution of this paper. It provides the first detailed picture of conflicts in Rio de Janeiro, and offers a methodological contribution by showing that records of a crime hotline can be used to track crime and violence in a cost-effective way.

Our findings have important implications for education and social policy. They provide strong empirical evidence that exposure to violence has disruptive effects on children's outcomes. Our results show that combating violence should be a priority policy since its effects have a far-reaching impact that goes beyond the great number of casualties. This paper supports the view that violence accentuates the poverty trap, since it is particularly acute in poor areas. By decreasing the quality of learning in these areas, it makes the way out of poverty even harder for children from disadvantaged households. The results here also indicate that schools in areas marked by violence require special policies to compensate for exposure to violence. These may include financial rewards to retain principals and extended school periods to compensate for lost classes.

It is worth emphasizing that our analysis estimates the effect of experiencing extreme violence but does not take into account the cross-section variation in violence and the impact of being under the rule of drug dealers. Consequently, one might reasonably interpret our estimates as a lower bound for the impact of violence on student achievement.

A final comment relates to whether what we learn from Rio de Janeiro's conflicts helps us to understand the consequences of violent conflicts in general. Although the levels of territorial control by drug gangs and the weaponry involved in these conflicts are unique to Rio de Janeiro, some characteristics of the conflicts under analysis resemble what we find in other urban environments. For instance, experts consider that the upsurge in violence in Mexico in the last decade was caused by the crackdown on drug trade that led drug trade organizations to break up into smaller groups and engage in turf wars (Felbab-Brown [2012], Rios [2012]). The city of Marseille, in France, is currently under distress due to conflicts between drug gangs. Experts and police officers consider the spike in violence to be caused by the increasing use of automatic and semiautomatic rifles and by the enlargement of drug networks (New York Times [2012b]). And in Chicago, gang violence and retaliation in specific neighborhoods of the city are the driving forces behind the surge in murders in 2012 (Chicago Tribune [2012]).

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Appendix A - Triggers of Armed Conflicts

This appendix provides more transcripts gathered from Plantão de Polícia and Casos de Polícia blogs. Our aim is to provide evidence that drug battles follow a unique dynamic that depends on betrayals, revenge, the imprisonment or release of a gang leader and others.

Six bodies were found in Morro do Juramento. These people were killed in an 11-hour conflict that took place last Tuesday. CV drug dealers tried to reconquer the area, which is dominated by Terceiro Comando Puro (TCP). Last month TCP overthrew the area from ADA. (Source: Meia Hora, 9/20/2009)

...in July, Marcus Vinicius Martins Vidinhas Júnior, known as Palhaço, betrayed his father-in-law, Celsinho da Vila Vintém, who is in jail but is still the favela drug baron. Palhaço killed 13 drug gang members in order to control drug trade slots. Two days later, Celsinho allies deposed Palhaço, who ran away with guns and R$ 1 million. (Source: Meia Hora, 9/22/2009)

An intense gunfight took place yesterday night at Morro do Dendê. Chorrão (ADA) and Pixote attempted to conquer the favela, which is dominated by Fernandinho Guarabu (TCP). Pixote is a former member of Guarabu gang. (Source: Meia Hora, 10/11/2009)

In addition, several reports to Disque-Denúncia also provide examples on what triggers conflicts:

Informs that at the given address it is possible to find fugitives and drug dealers, who yesterday were involved in a gun conflict. Today, the mother of one of the boys was shot to death in the Estrada Porto Nacional. This group is part of Pipa's gang, who was recently murdered in jail. Pipa's death explains the attempt against his supporters. It concludes by mentioning that the school Piquet Carneiro received an order to close. Date: 3/26/2004 2:19 PM

Reports that the favela mentioned and Morro do Timbau, which are controlled by Facão, were invaded today by more than 80 drug dealers. Some of them are known as 'Noquinha', 'Sassá', 'Alex Churrasquinho', 'Nelsinho', 'Daniel do Lava Jato', 'Ilton',

(...). There are others from Morro do São Carlos. They are from ADA gang, are heavily armed, are led by Gan Gan and aim to kill Desviado, the leader of Baixa do Sapateiro, and the drug trade manager Tico. The gun fight began at noon and these drug dealers are still around the favela, shooting without a specific target and leaving favela inhabitants in panic. Date: 1/11/2004 5:20 PM

Inform that in Parque Alegria favela a gun fight is taking place right now among drug dealers. Yesterday, during the day, the drug dealers Nêgo Dengo and Araketu killed a person and this is the reason for the current gun conflict. Drug dealers connected with the person who died invaded the favela to take revenge. Demands intervention because several people are being shot by stray bullets. Date: 12/12/2006 3:37 PM.

Appendix B - Coding Disque-Denúncia reports

This appendix explains how we used Disque-Denúncia reports to construct violence indicators. We gathered from Disque-Denúncia (DD) all reports classified as 'gun fight between drug-gangs' (tiroteio entre facções) registered between 2004 and 2009 in the city of Rio de Janeiro. The content of each report varies a lot but in all cases it contains the date of the call, a location reference and a description of the event. Most of the reports are simple as the one below:

Inform that drug dealers from the referred favela are currently in a battle with rival drug dealers. The gunfight is intense and people are worried. Demand police intervention. Address provided: Morro da Mangueira.[21]

Other reports are incredibly rich, provide important information for the police (eg. the location of a drug dealer) and show how violent these events are:

Report that today (10/26/2005), at 7:00AM, there was a gunfight in front of the school Vicente Mariano between drug leaders from Timbau favela and Vila do Pinheiro favela. A man was killed and five children were shot. ... The traffic leader had intentionally shot in the school direction. This guy, whose nickname is Night, is currently located at rua Capivari, 55. Address provided: Maré favela.[22]

[21]Original report: 'Relata que traficantes do morro citado se encontram nesse momento trocando tiros com traficantes rivais. Informa que a troca de tiros é intensa e os moradores estão preocupados. Sem mais, pede policiamento para o local."

[22]Original report: 'Informa que hoje (26/10/2005), as 07h, ocorreu um tiroteio na favela da Maré,

The two examples above also show that although DD always asks for the full address (street name, number and zip code), people do not always provide it in detail. In both cases, just the name of the favela was provided. The exact location of the second event was even harder to identify since the person mentioned Maré, which is the name of a favela complex. In order to deal with these issues, we relied on a combination of addresses provided, the name of the favela (when it was mentioned) and the content of each report to identify where the described event took place. Based on that information, we associated each report to a city favela by using the favela shape file provided by Instituto Pereira Passos (IPP). In some cases, this association was not straightforward due to three reasons. First, many times the name of a favela was not mentioned in any part of the report. In this case, we opened the favela shape file on Google Earth and added the address or other information provided in the report (for instance, in the second example, we added the address of school Vicente Mariano). In case the address was within a favela or close to its border, the report was associated with the respective favela. The addresses far away from a favela were classified as 'paved area' (asfalto) and were excluded from our sample. Another challenge is the fact that people use different names to refer to the same favela and the favela name used by IPP does not always match the one most used by the population. For instance, the favela popularly called Parada de Lucas or just Lucas is registered in IPP as 'Parque Jardim Beira Mar'. Fortunately, IPP also provides a list with alternative names for the same favela, which allows us to match the names used by the population with the ones in IPP's shape file.[23] Finally, some reports mentioned that a gunfight occurred in places that are not officially favelas but rather housing projects or irregular settlements, which are not marked in IPP's favela shape file. For instance, several reports mentioned a conflict in Conjunto Guaporé, Cidade Alta or Conjunto Fumacê, which are housing projects. To keep from losing that information, we used Google Earth and the addresses provided in the reports to draw borders for these areas and incorporated them in the favela shape file.[24]

em frente ao Brizolão Colégio Vicente Mariano, confronte entre o tráfico do morro do Timbau e Vila dos Pinheiros onde causou a mote de um adulto e o ferimento de cinco crianças (não identificados), estudantes do colégio supra citado, que encontram-se no hospital geral de Bonsucesso em estado grae. Relata que o chefe do tráfico do morro do Timbau, identificado como Night, foi o responsável pelos disparos, pois direcionou sua arma para o colégio atirando impiedosamente, provocando este acidente. Declara que Night pode ser encontrado neste exato momento, em uma casa, no alto do morro, na rua capivari, próximo ao numero 55, no local onde existe uma placa informando tratar-se do beco da escolinha. Sem mais, pede providências."

[23]In the cases that the IPP list didn't have the favela name provided in the DD report, we used the address provided and Google Earth to make the match.

[24]We added 14 borders in IPP's favela shapefile which represents the following housing projects

In addition to standardizing the address, we read the content of each report to guarantee that each one indeed describes a gunfight that took place on the date and at the address registered. Hence, we marked the reports that mention the threat of a gunfight or the location of bodies and drug dealers but did not mention that a gunfight occurred at that place and date. We exclude these reports from our sample. In addition, some reports provide an address, but the content refers to a conflict that happened in another place. In this case, we corrected the address to guarantee that it informs where the event happened. For instance, the report below was registered as Baixa do Sapateiro, but the content led us to change it to 'Avenida Canal', which is the official name of Vila do Pinnheiro favela, and where the conflict took place according to the report.

Inform that drug dealers from the favela mentioned, which are part of Terceiro Comando gang, invaded Pinheiro favela, which is dominated by ADA. Both favelas are located in Maré... Address: Baixa do Sapateiro.[25]

A similar adjustment was necessary for the dates. Sometimes people call and report that a gunfight occurred three days before and DD registers the call date. We corrected the dates to guarantee that they refer to when the event took place.

This procedure generated a favela list containing the dates on which a gunfight took place. We then aggregated the data per favela and year by counting the number of days that at least one report of armed conflict was registered in Disque-Denúncia. Table 1 provides the descriptive statistics of Disque-Denúncia reports.

Bellow, we give more examples of original reports and how we classified them in

or irregular settlements (neighborhood indicated in parenthesis): Vila do Pinheiro (Maré), Vila do João (Maré), Conjunto Guaporé (Brás de Pina), Conjunto Alvorada (Santa Cruz), Conjunto Cezarão (Santa Cruz), Favela do Rola (Santa Cruz), Guandu II (Santa Cruz), Morro das Pedrinhas (Santa Cruz), Cidade Alta (Cordovil), Vila Alice (Laranjeiras), Cruzada São Sebastião (Leblon), Conjunto Mangariba (Paciência), Conjunto Cavalo de Aço (Senador Camará) e Conjunto Fumacê (Realengo)".

[25]Original report: 'Informa que traficantes (não identificados) da favela em questão, que pertencem a facção criminosa Terceiro Comando, invadiram a favela do Pinheiro, que pertence a facção ADA, ambas situadas no complexo da Maré, Afirma que a invasão ocorreu sábado a tarde, por volta as 18hs, com intuito dos traficantes assumirem os pontos de boca de fumo da favela rival. Menciona que a invasão aconteceu devido a retirada das viaturas que ficavam frequentemente na entrada da favela do Pinheiro, que tem acesso pela linha amarela. Segundo informações, traficantes da favela em questão, teriam pago aos policiais (no identificados) lotados no 22 BPM, para se retirarem do local para assim eles poderem invadir a favela rival com mais facilidade. Disse que ontem (09/11) todos os estabelecimentos da favela acima estavam com as portas fechadas com a ordem passada pelo tráfico, pois provalmente algum indivíduo teria sido morto pela guerra das facções. Pode que o policiamento retorne ao local."

order to clarify our methodology.

Informs that this avenue is one of the access points to Morro do Cajueiro, which will be invaded today at night by people from Morro da Serrinha. These people want to revenge the death of three colleagues that were killed by the rival gang. The attempt to invade the favela has been planned since these guys began to steal cars in the neighborhood. Address: Avenida Ministro Edgard Romero. Date: 10/22/2004.

Morro do Cajueiro is an alternative name for Morro do Sossego, which is the name in IPP's shape file. This report was not included in our sample because it mentions only the threat of a conflict.

Reports that in the mentioned road, close to the school Chiquinha Gonzaga, several drug dealers were seen yesterday around 10 pm with the possession of heavy guns and motorbikes. There was an intense gun fight and a car was severely shot. The gun fight took one hour and the group escaped to Vila Aliança, close to Beira Rio store (.....) Demands police intervention in the region. Address: Estrada do Engenho, Bangu. Date: 10/31/2006.

We changed the date of this report to the day before (10/30/2006), when the conflict actually happened, but we ended up not using this report because it was not close to a favela.

Reports that in this street, which is the entrance to Favela Boogie Woogie, is the location of school Olga Benário, where it is possible to find several drug dealers from Terceiro Comando. One of them is known as 'Grilo' and he is the son of a school employee. Drugs are sold inside the schools during class breaks. Yesterday, at 4:30 pm, drug dealers from Comando Vermelho tried to invade the school. There was an intense gun fight. Address: Rua Dante Santoro, Cacuia, Ilha do Governador. Date: 8/22/2003. This report mentions the proximity to favela Boogie Woogie, whose official name is Bairro Nossa Senhora das Graças. Therefore, we associated this report to this last favela name. In addition, we changed the day of the report to the previous day (8/21/2003), when the event took place.

Report that in the mentioned street is the location of Guaporé housing project. A gun fight is taking place right now between drug dealers from rival gangs. A senior lady and a young boy were wounded. Address: Rua Carbonita, Brás de Pina. Date: 8/14/2004.

We drew the border of Guaporé housing project using Google Earth and added it to IPP's shape file in order to incorporate this and other reports in our analysis.

Appendix C - Disque-Denúncia Reports as a Measure of Violence

In this paper we define as violence the number of days with conflicts according to Disque-Denúncia reports. Therefore, we measure reported violence rather than track actual violence. In this appendix, we provide evidence that Disque-Denúncia reports are indeed a good measure of armed conflicts.

One way to check the validity of the Disque-Denúncia data is to cross-check it with official homicide data. Figure 5 shows how the number of homicides in the city of Rio de Janeiro and levels of violence documented in Disque-Denúncia reports changed between 2003 and 2009. Note that we are interested in understanding the trends in both variables, rather than comparing levels of violence. The trends in both series are remarkably similar. Both indicate that 2004 was the most violent year; that after 2004, violence declined; but that violence had peaked again by 2009. The largest difference between the two variables occurs in 2006, when a reduction in the number of reports was not followed by a decrease in the number of homicides. Figure 6 shows the yearly correlation between the number of homicides and the number of days with conflicts, aggregated per AISP (the city division used by the police department). We observe that in all years, there is a strong correlation between the two measures, which vary from 0.48 in 2004 to 0.74 in 2006 and 2007. Therefore, comparing the number of homicides to Disque-Denúncia shows that Disque-Denúncia data provide a reasonable picture of variations in violence across time and space.

We measure violence from anonymous reports, and that propensity to report may vary within regions, over time. Given that we explore within-school variation, our estimates are at risk if the propensity to report in some neighborhoods changes due to factors also correlated with student outcomes. In order to investigate this issue, we first cross-check the Disque-Denúncia data with official homicide data, which is only disaggregated into 18 major regions of the city (AISPs). When we aggregate our reports following the same division in homicide data, we observe that trends in both series are remarkably similar. In addition, we plot the relationship between the homicide rates and the number of days with reports by region, separately for each year, and observe that regional propensity to over or under-report is rather constant

over time. Figure 6 indicates that each AISP consistently tends to be situated above or below the prediction lines of homicide rates based on reports. Table 12 formalizes this finding by showing the actual and predicted homicide rate based on the number of days with reports in each AISP and year, and on whether the region over or under-reported violence each year. This exercise indicates that 11 AISPs always over-report violence, i.e., have a predicted homicide level greater than the actual number, while five AISPs always under-report. Only AISPs 14 and 31 demonstrate changes in their propensity to report over time.[26] These two AISPs are located in Rio de Janeiro's Western Zone, a region which was marked during the period under analysis by increasing militia dominance. There is evidence that the militia intimidates the local population (see Cano and Ioot [2008] and Soares et al. [2010]), which can change the propensity to report conflicts. Although it is not clear what the militia's effect on student outcomes might be, we replicate our exercises excluding Rio de Janeiro's Western Zone from the sample and obtain similar estimates (results available upon request).

A final way of checking the validity of our measure of violence is to compare Disque-Denúncia reports with media coverage. We performed a web search in Rede Globo website,[27] which is the main media network in Rio de Janeiro and contains information on every report which was disseminated in the press, on websites and on TV. We carried out an automatic search for each favela name plus gunfight (tiroteiro), drug dealers (traficantes), favela and year (from 2003 to 2009). We use the number of hits of each search as a proxy for whether any gun conflict between drug dealers in a specific favela and year took place. Table 13 shows the comparison of Disque-Denuncia data and this web search and confirms one important feature of the Disque-Denuncia dataset. Disque-Denuncia provides a much more complete picture of gang conflicts than Globo. Out of 867 favelas on which we carried out the web search,[28] 298 favelas or 34% experienced a conflict between drug gangs between 2003 and 2009 according to Disque -Denuncia. During the same period, Globo network

[26] AISP 14 includes the following neighborhoods: Anchieta, Guadalupe, Parque Anchieta, Ricardo de Albuquerque, Campo dos Afonsos, Deodoro, Jardim Sulacap, Magalhães Bastos, Realengo, Vila Militar, Bangu, Gericinó, Padre Miguel and Senador Camará. AISP 31 includes Barra da Tijuca, Camorim, Grumari, Itanhangá, Joá, Recreio dos Bandeirantes, Vargem Grande and Vargem Pequena

[27] `http://g1.globo.com/`

[28] We did not include in this analysis the favelas whose names are very common (e.g. Funcionarios (employees) or Rio), or indicate a name of a street or a date. The reason is that we could easily find hits for these names that are not associated with the favela itself. This is a conservative way to carry out this search but the numbers and correlations shown in Table 13 are very similar if we include these favelas in the list.

mentioned only 177 favelas' names along with "tiroteiro" and "traficantes" words. We interpret this difference as evidence that Globo does not cover all violent events because many of them happen in poor and restricted areas and, therefore, do not attract the attention of the largest share of the population.

We recognize that this is a very rough exercise since we did not read all Globo's articles to confirm that they indeed refer to a drug gang conflict that happened in the respective favela and year.[29] But even with this caveat in mind, we believe that this analysis, together with the evidence in this Appendix, provides compelling evidence that Disque-Denuncia data provides a good proxy for violence and a richer picture of Rio de Janeiro's drug conflicts than other available databases.

Appendix D - Cross-Sectional and Time-Series Properties

Our identification strategy relies on the assumption that the conflicts between drug gangs are uncorrelated with any latent determinant of children's education. In particular, to overcome the omitted variables problem, we need to be sure that the conflicts are as good as random within schools or neighborhoods. We perform two tests. First, we carry out an exercise to examine whether the variation in conflicts correlates with favela and neighborhood characteristics. More specifically, we regress, at the favela level, the variance of the total number of days with conflicts on (i) favela characteristics, such as steepness, area and distance to main road; and (ii) neighborhood characteristics, for example, population density, the logarithm of total population, share of population between 13 and 19 years old, income per capita and Gini index (all these variables at the neighborhood level, from the 2000 Census). The results (available upon request) indicate that the only predictors of variation in conflicts are favela steepness and area. These features make the favela a strategic place, where gang members can easily hide and protect themselves. More important to our analysis, factors that are usually associated with crime, such as income levels and inequality, do not explain variation in conflicts.

The second test explores the time series properties of our data on conflicts. First,

[29]The fact that we did not double-check the content of each article implies that this is a noisy measure of Globo. On one hand, our tool overstates Globo's coverage because it is possible to find an article with all the entered key words that refers to a past event or that mentions the name of the favela in another context. On the other hand, our method may understate Globo's coverage because it does not take into account synonyms of our key words.

based on panel data extensions of the Dickey-Fuller unit root tests, we reject the null that the conflicts are non-stationary at the favela level (results available upon request). This eliminates any concern related to spurious correlation driven by non-observable trends or breaks in the data. We also estimate partial autocorrelation functions (PACF) based on a favela-month panel of data over the period 2004-2009 in order to model conflict dynamics. Figure 7 plots the correlogram for the PACF estimated up to the 15th lag, where the number of months is $T = 84$, and in which regressions we include month, year and favelas' fixed-effects. The results suggest that the conflicts may follow either a very weak AR(1) or a white noise process at the favela-month level.

Table 1: Disque-Denúncia Database Summary Statistics

Panel A - Statistics of reports			
Total number of reports between 2003-2009		4,365	
Reporting gunfight		3,842	
	on favelas	3,527	92%
	other places	315	8%
Number of favelas		979	
	with at least one report of gunfight	338	35%
	without reports of gunfight	641	65%

Panel B - Favelas with conflicts	Number of reports		Number of days	
	per year	2003-2009	per year	2003-2009
mean	1.5	10	1.2	9
sd	4	20	3	15
p50	0	3	0	3
p90	4	28	3	25
max	85	163	41	111

Notes: This Table provides summary statistics of Disque-Denuncia dataset. Total number of reports indicates the number of text entries that Disque-Denuncia provided that were classified as 'gunfight between drug-gangs' (tiroteio entre faccões). We consider a report as refering to gunfight if the text provided indeed mentions that a gunfight between drug gangs took place. Reports on favelas indicate addresses that fall within favelas' boundaries. We refer to 'favelas with conflicts' as the ones that have at least one report of gunfight in 2003-2009 period. See Appendix B for more information on how we coded Disque-Denuncia reports.

Table 2: Education Summary Statistics

	All schools			Exposed		Not Exposed		Years
	N	mean	sd	mean	sd	mean	sd	
A - Student-level variables (Prova Brasil takers - 5th graders)								
Language score	76131	180.9	42.5	177.0	41.9	182.8	42.7	05,07,09
Math score	76131	195.2	42.9	191.2	42.1	197.2	43.2	05,07,09
Mean age	67195	11.37	1.11	11.40	1.11	11.36	1.11	05,07,09
% of boys	73197	0.497	0.500	0.498	0.500	0.497	0.500	05,07,09
% non-white	70048	0.745	0.436	0.754	0.431	0.740	0.438	05,07,09
% low educated mother	42869	0.455	0.498	0.487	0.500	0.439	0.496	05,07,09
% work	71856	0.120	0.325	0.127	0.333	0.117	0.321	05,07,09
% failed a grade in the past	71673	0.288	0.453	0.303	0.460	0.281	0.449	05,07,09
% dropped out school in the past	72068	0.090	0.286	0.094	0.291	0.088	0.283	05,07,09
B- Student-level variables (Pre-school to 5th graders)								
% of students who leave (between academic year)	1499049	0.173	0.378	0.173	0.378	0.172	0.378	03 to 09
% of students who enter (between academic year)	1499049	0.068	0.252	0.070	0.256	0.065	0.246	03 to 09
% of students who leave (within academic year)	1521402	0.097	0.296	0.098	0.297	0.095	0.293	03 to 09
% of students who enter (within academic year)	1521402	0.058	0.234	0.058	0.234	0.058	0.234	03 to 09
Number of students	1521402	800.3	358.8	825.2	358.4	758.8	355.5	03 to 09
Mean age	1459230	8.214	2.410	8.177	2.417	8.275	2.397	03 to 09
% of boys	1459433	0.520	0.500	0.518	0.500	0.523	0.499	03 to 09
% non-white	1215895	0.660	0.474	0.667	0.471	0.650	0.477	03 to 09
% low educated mother	1179357	0.837	0.369	0.855	0.353	0.808	0.394	03 to 09
% live close to school	1271173	0.787	0.409	0.816	0.387	0.738	0.440	03 to 09

Notes: This table provides summary statistics of students, schools and teacher characteristics. The sample is comprised of all schools located within 250 meters of at least one slum. Violent schools are the schools which were exposed to two or more days of violence within 250 meters of the favelas during the academic years (March-November) from 2003 to 2009. There are 152 violent schools and 184 non-violent schools in our sample according to this definition.

Table 2: Education Summary Statistics (continuing)

	All schools			Violent		Non-violent		Years
	N	mean	sd	mean	sd	mean	sd	
C- School-level variables								
Number of days with violence	2352	1.219	3.082	2.608	4.172	0.071	0.256	03 to 09
Number of days with contiguous violence	2352	0.718	2.740	1.587	3.901	0.000	0.000	03 to 09
% with kitchen	2352	0.980	0.141	0.970	0.171	0.988	0.111	03 to 09
% with principal's room	2352	0.848	0.359	0.818	0.386	0.873	0.333	03 to 09
% with science lab	2352	0.079	0.269	0.059	0.236	0.095	0.293	03 to 09
% with computer lab	2352	0.412	0.492	0.396	0.489	0.425	0.494	03 to 09
% with free lunch	2352	0.980	0.140	0.979	0.142	0.981	0.138	03 to 09
% with teachers' room	2352	0.817	0.387	0.812	0.391	0.821	0.384	03 to 09
Number of teachers (1st -5th gr)	1004	14.3	6.5	14.8	6.5	13.8	6.358	07 to 09
% of teachers with absences	1004	0.155	0.195	0.145	0.188	0.163	0.201	07 to 09
Days of absence per teacher	1004	0.991	2.598	1.003	2.807	0.981	2.414	07 to 09
% of teachers on medical leave	1004	0.759	0.355	0.699	0.339	0.808	0.361	07 to 09
Days on medical leave per teacher	1004	28.8	23.0	25.2	20.7	31.7	24.3	07 to 09
D- Principal reported problem with								
School shutdown	626	0.276	0.448	0.340	0.475	0.224	0.417	07,09
Students' absence	623	0.361	0.481	0.377	0.486	0.348	0.477	07,09
Teachers' turnover	628	0.123	0.328	0.145	0.353	0.104	0.306	07,09
Principal turnover	623	0.185	0.388	0.179	0.384	0.190	0.392	07,09
Threat to teachers' life	612	0.031	0.174	0.029	0.169	0.033	0.178	07,09
Threat to students' life	620	0.016	0.126	0.011	0.103	0.021	0.142	07,09

Table 3: Violence Effects on Student Achievement: Benchmark Results

Dependent Variable: Student Test Scores in Math and Language			
	(1)	(2)	(3)
	Panel A: Math		
Violence	-0.116	-0.093	-0.054
	(0.026)***	(0.021)***	(0.027)**
	Panel B: Language		
Violence	-0.103	-0.079	-0.030
	(0.025)***	(0.020)***	(0.027)
Common Specification:			
Observations	76,084	76,084	76,084
Number of Schools	336	336	336
Year FE	Yes	Yes	Yes
Student, Class and School Controls	No	Yes	Yes
School FE	No	No	Yes

Notes: Dependent variable is student achievement test scores in math (Panel A) and language (Panel B) for 5th graders in the 2005, 2007 and 2009 Prova Brasil editions. Test scores are expressed in standard deviations. All regressions include year fixed effects. Student characteristics include sex, race, age fixed effects, dummies for levels of mother's education, and dummies indicating if students have ever repeated a grade or dropped out. Classroom composition includes share of boys and whites, average age, share of students that have previously repeated a grade or dropped out. School controls are dummies indicating whether there is a computer lab, science lab, free lunch, teachers' offices, principal's office and kitchen. The variable of interest (violence) is a dummy indicating at least two days of conflict within the school year in a favela within 250 meters of the school. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Violence Effects and Distance Between Schools and Conflict Location

	Dependent Variable: Student Test Scores in Math								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Violence within	-0.065	-0.018	-0.018	-0.019	-0.018	-0.130			
5 meters	(0.078)	(0.082)	(0.081)	(0.081)	(0.081)	(0.056)**			
Violence within		-0.051	-0.067	-0.067	-0.066				
250 meters		(0.029)*	(0.034)**	(0.034)**	(0.034)**				
Violence within			0.020	0.002	0.003				
500 meters			(0.023)	(0.032)	(0.032)				
Violence within				0.022	0.032				
750 meters				(0.025)	(0.029)				
Violence within					-0.013				
1000 meters					(0.021)				
Violence within							-0.066		
200 meters							(0.030)**		
Violence within								-0.053	
250 meters								(0.027)*	
Violence within									-0.034
300 meters									(0.024)
Sample of Schools:	All	All	All	All	All	Inside	Within 300m	Within 300m	Within 300m
Observations	162,999	162,999	162,999	162,999	162,999	5,700	87,437	87,437	87,437
Number of schools	736	736	736	736	736	24	388	388	388
Year ans School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student, Class and School Characts	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is student achievement test scores in math for 5th graders in the 2005, 2007 and 2009 Prova Brasil editions, expressed in standard deviations. All regressions include year and school fixed effects. For student, classroom and school controls, see notes from Table 3. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Violence Effects by Conflict Intensity and Length

	Dependent Variable: Student Test Scores in Math					
	(1)	(2)	(3)	(4)	(5)	(6)
1 Day of Violence	-0.009					
	(0.025)					
2 or More Days of Violence		-0.054				
(benchmark)		(0.027)**				
7 or More Days of Violence			-0.091			
			(0.048)*			
9 or More Days of Violence				-0.104		
				(0.046)**		
Contiguous: 2 or More Days					-0.053	
of Violence Within 2 Weeks					(0.032)*	
Non-Contiguous: 2 or More Days						-0.043
of Violence More Than 2 Weeks Apart						(0.029)
Observations	76,084	76,084	76,084	76,084	76,084	76,084
Number of schools	336	336	336	336	336	336
Year and School FE	Yes	Yes	Yes	Yes	Yes	Yes
Student, Class	Yes	Yes	Yes	Yes	Yes	Yes
and School Characts						

Notes: Dependent variable is student achievement test scores in math for 5th graders in the 2005, 2007 and 2009 Prova Brasil editions, expressed in standard deviations. All regressions include year and school fixed effects. For student, classroom and school controls, see notes from Table 3. The dummy for contiguous days of violence indicates that the school experienced two or more days of violence within a 14 day window. Conversely, non-contiguous dummy refers to two or more days of conflict that were more than 14 days apart. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: The Timing of the Violence Effect on Student Achievement

Dependent Variable: Student Test Scores in Math			
	(1)	(2)	(3)
Violence in Dec-Feb (Just After Exam)	-0.016		
	(0.039)		
Violence in Aug-Nov (Just Before Exam)	-0.051		
	(0.030)*		
Violence in Mar-Jun (1st Semester)	-0.035		
	(0.034)		
Violence: Current Year (Benchmark)		-0.053	-0.054
		(0.041)	(0.027)**
Violence: Next Year (Lead, Year + 1)		-0.017	
		(0.043)	
Violence: Past Year (Lag, Year - 1)			-0.006
			(0.028)
Observations	76,084	53,503	76,084
Number of schools	336	336	336
Year and School FE	Yes	Yes	Yes
Student, Class and School Characts	Yes	Yes	Yes

Notes: Dependent variable is student achievement test scores in math for 5th graders in the 2005, 2007 and 2009 Prova Brasil editions, expressed in standard deviations. All regressions include year and school fixed effects. For student, classroom and school controls, see notes from Table 3. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Heterogeneity in the Violence Effect by Students' Socioeconomic Characteristics

	Dependent Variable: Student Test Scores in Math											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Violence	-0.038	-0.072	-0.038	-0.086	-0.041	-0.069	-0.053	-0.058	-0.033	-0.072	-0.098	-0.054
	(0.030)	(0.030)**	(0.029)	(0.032)***	(0.031)	(0.035)*	(0.033)	(0.029)**	(0.031)	(0.031)**	(0.056)*	(0.028)*
Sample	Boys	Girls	Non-White	White	Mother Educ Low	Mother Educ High	Age >= 12	Age <= 11	Repeated Before	Never Repeated	Dropped Out Before	Never Dropped Out
Observations	36,393	36,804	52,169	17,879	19,525	23,344	22,384	53,700	20,648	51,025	6,458	65,610
Number of schools	336	336	336	336	336	336	336	336	336	336	336	336
Year and School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student, Class and School Char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is student achievement test scores in math for 5th graders in the 2005, 2007 and 2009 Prova Brasil editions, expressed in standard deviations. All regressions include year and school fixed effects. For student, classroom and school controls, see notes from Table 3. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Violence Effects on Student Mobility

	Mobility Within the School Year		Mobility at the End of School Year	
	Move Out	Move In	Move Out	Move In
	(1)	(2)	(3)	(4)
		Panel A - All Grades (1st to 5th)		
Violence	0.0034	0.0012	-0.0018	0.0010
	(0.0027)	(0.0022)	(0.0036)	(0.0033)
Dep Var Mean	0.0968	0.0582	0.173	0.0683
Students (Obs)	1,521,402	1,521,402	1,499,049	1,499,049
		Panel B - Only 5th Grade		
Violence	0.0025	-0.0036	0.0104	0.0197
	(0.0044)	(0.0028)	(0.0147)	(0.0162)
Dep Var Mean	0.0771	0.0457	0.522	0.209
Students (Obs)	269,524	269,524	261,580	261,580
Common Specification:				
Year and School FE	Yes	Yes	Yes	Yes
Student Characts	Yes	Yes	Yes	Yes

Notes: Dependent variable is a dummy indicating whether the student moved out (columns 1 and 3) or moved in to the school (columns 2 and 4) in the referred period. We define that a student entered the school if her identifier appears for the first time in the school records in a specific month. Conversely, we define that the student moved out from the school if her number disappears from school records. Columns 1 and 2 consider movements between April and November and columns 3 and 4 consider movements from December to March of the following year. Panel A includes all students from first to fifth grade that are enrolled in the school. Panel B includes only fifth graders. All regressions include year and school fixed effects. Student characteristics include gender, race, age, grade, dummies for levels of mother's education, and dummy indicating if the student lives close to the school. Robust standard errors clustered at the school level in parentheses, significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Student Selection at the Prova Brasil Exam

	Boys	Nonwhite	Mother Educ Low	Repeated	Dropped Out	Age >=12
	(1)	(2)	(3)	(4)	(5)	(6)
Violence	0.000	0.000	0.005	-0.003	-0.001	0.008
	(0.001)	(0.001)	(0.011)	(0.003)	(0.001)	(0.008)
Observations	73,197	70,048	42,869	71,673	72,068	67,195
Number of schools	336	336	336	336	336	336
Year and School FE	Yes	Yes	Yes	Yes	Yes	Yes
Principal, Class and School Characts	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables are dummies indicating the characteristics of the student taking the Prova Brasil exam in the 2005, 2007 and 2009 Prova Brasil editions. All regressions include year and school fixed effects. For student, classroom and school controls, see notes from Table 3. Robust standard errors clustered at the school level in parentheses *** p¡0.01, ** p¡0.05, * p¡0.1

Table 10: Channels: Violence Effects on School Routine

	Interruption of Classes (1)	Students Absenteeism (2)	Teachers Turnover (3)	Principal Turnover (4)	Threat Against Teachers' life (5)	Threat Against Students' life (6)
			Panel A			
Violence	0.064	-0.052	0.127	0.051	0.077	0.011
	(0.096)	(0.085)	(0.086)	(0.079)	(0.041)*	(0.010)
			Panel B			
Violence	0.246	0.037	0.046	0.126	0.029	0.022
(contiguous)	(0.105)**	(0.075)	(0.081)	(0.073)*	(0.031)	(0.025)
Violence	-0.073	-0.045	0.070	-0.021	0.057	-0.008
(non-contiguous)	(0.097)	(0.087)	(0.078)	(0.083)	(0.035)	(0.021)
Sample Mean	0.27	0.48	0.32	0.38	0.17	0.01
Observations	637	637	637	637	637	637
Number of schools	319	319	319	319	319	319
Year and School FE	Yes	Yes	Yes	Yes	Yes	Yes
Principal, Class and School Characts	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables are dummies indicating whether the school principal mentioned that the problem listed in the column occurred in the school during the academic year. All regressions include school and year fixed effects, school characteristics (see notes in Table 3 for the list), student average characteristics, and principals' characteristics (age, gender, and dummies for graduate and undergraduate degrees). The sample comprises 2007 and 2009 years.

Table 11: Violence Effects on Teachers' Absenteeism and Medical Leaves

	% of Absent Teachers (1)	Average Number of Days of Absences per Teacher (2)	% Teachers Took Medical Leave (3)	Average Number of Days on Medical Leave (4)
		Panel A		
Violence	0.058	0.357	-0.012	2.116
	(0.020)***	(0.302)	(0.024)	(1.984)
		Panel B		
Violence (contiguous)	0.041	0.315	-0.010	-0.607
	(0.024)*	(0.163)*	(0.027)	(2.055)
Violence (non-contiguous)	0.030	0.198	-0.021	0.157
	(0.019)	(0.325)	(0.023)	(2.327)
Sample Mean	0.15	0.99	0.75	28.7
Observations	956	956	956	956
Students and Teachers Characts	Yes	Yes	Yes	Yes
School characteristics	Yes	Yes	Yes	Yes
Year and School FE	Yes	Yes	Yes	Yes

Notes: Dependent variables are the total number of teachers' absences (columns 1 and 2) and medical leaves (columns 3 and 4) normalized by the number of teachers on duty in the school. Column 1 and 3 indicate the percentage of teachers that miss classes, while columns 2 and 4 indicate the average length of absence. All regressions include school and year fixed effects, school characteristics (see notes in Table 3 for the list), student average characteristics, number of teachers in the school, and teachers' average profile (age, gender, and dummies for graduate and undergraduate degrees). The period of analysis is 2007-2009.

Table 12: Testing for Under-Reporting

AISP		2004	2005	2006	2007	AISP		2004	2005	2006	2007
1	Homicide rate	118	83	71	74	16	Homicide rate	129	149	150	170
	Pred homicide	184	266	205	174		Pred homicide	217	165	180	183
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
2	Homicide rate	36	20	33	23	17	Homicide rate	80	49	59	38
	Pred homicide	97	100	66	58		Pred homicide	151	150	107	54
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
3	Homicide rate	153	135	166	199	18	Homicide rate	138	150	133	123
	Pred homicide	221	215	252	322		Pred homicide	88	105	66	72
	Under-reporting	0	0	0	0		Under-reporting	1	1	1	1
4	Homicide rate	45	33	43	22	19	Homicide rate	16	19	11	12
	Pred homicide	82	70	60	63		Pred homicide	91	77	86	72
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
5	Homicide rate	38	55	42	37	22	Homicide rate	209	137	110	115
	Pred homicide	76	60	55	49		Pred homicide	140	80	81	91
	Under-reporting	0	0	0	0		Under-reporting	1	1	1	1
6	Homicide rate	54	67	79	88	23	Homicide rate	37	41	33	28
	Pred homicide	186	155	143	169		Pred homicide	101	140	91	63
	Under-reporting	0	0	0	0		Under-reporting	0	0	0	0
9	Homicide rate	617	532	480	454	27	Homicide rate	238	182	232	231
	Pred homicide	178	205	273	345		Pred homicide	155	80	164	151
	Under-reporting	1	1	1	1		Under-reporting	1	1	1	1
13	Homicide rate	17	14	14	18	31	Homicide rate	50	46	51	38
	Pred homicide	68	57	45	49		Pred homicide	68	57	45	49
	Under-reporting	0	0	0	0		Under-reporting	0	0	1	0
14	Homicide rate	372	368	414	339	39	Homicide rate	305	326	344	327
	Pred homicide	412	301	444	294		Pred homicide	136	123	102	77
	Under-reporting	0	1	0	1		Under-reporting	1	1	1	1

Notes: This Table presents the actual and predicted homicide rate of each Área Integrada de Segurança Pública (AISP), which is a division of Rio de Janeiro used by Police Authority to provide crime statistics. In order to calculate predicted homicide, we run yearly regressions of homicide rates on the number of days with reports about armed conflicts. We then used the estimated coefficient to generate predicted homicide. Under-reporting indicates whether the predicted homicide rate is lower than the actual homicide rate.

Table 13: Comparison between Disque-Denuncia and Newspaper coverage on drug conflicts

		Disque-Denuncia			Newspaper			Correlations	
Year	Number of favelas	favelas with reports	%	number of reports	favelas with articles	%	number of articles	between (2) and (5) ρ	se
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2003	867	158	0.18	567	47	0.05	70	0.09	(0.02)***
2004	867	147	0.17	810	58	0.07	101	0.13	(0.02)***
2005	867	133	0.15	568	45	0.05	76	0.16	(0.02)***
2006	867	106	0.12	333	93	0.11	303	0.31	(0.03)***
2007	867	78	0.09	330	94	0.11	288	0.27	(0.04)***
2008	867	84	0.10	270	79	0.09	170	0.26	(0.03)***
2009	867	99	0.11	410	70	0.08	143	0.26	(0.03)***
Total	867	298	0.34	3288	177	0.20	1151	0.19	(0.01)***

Notes: This Table shows how many favelas and reports appear in Disque-Denuncia and in Globo media, related with favelas' name, drug dealers (traficantes), and gunfight (tiroteio) in a specific year. Columns 3 and 5 indicate, respectively, the number of favelas which have at least one report of gunfight between 2003 and 2009 in Disque-Denuncia and in Globo. Columns (8) and (9) show the correlation and standard errors between dummies that indicate whether the favela was mentioned in Disque-Denuncia and whether it was mentioned in Globo media network.

Figure 1: Favela and School Distribution

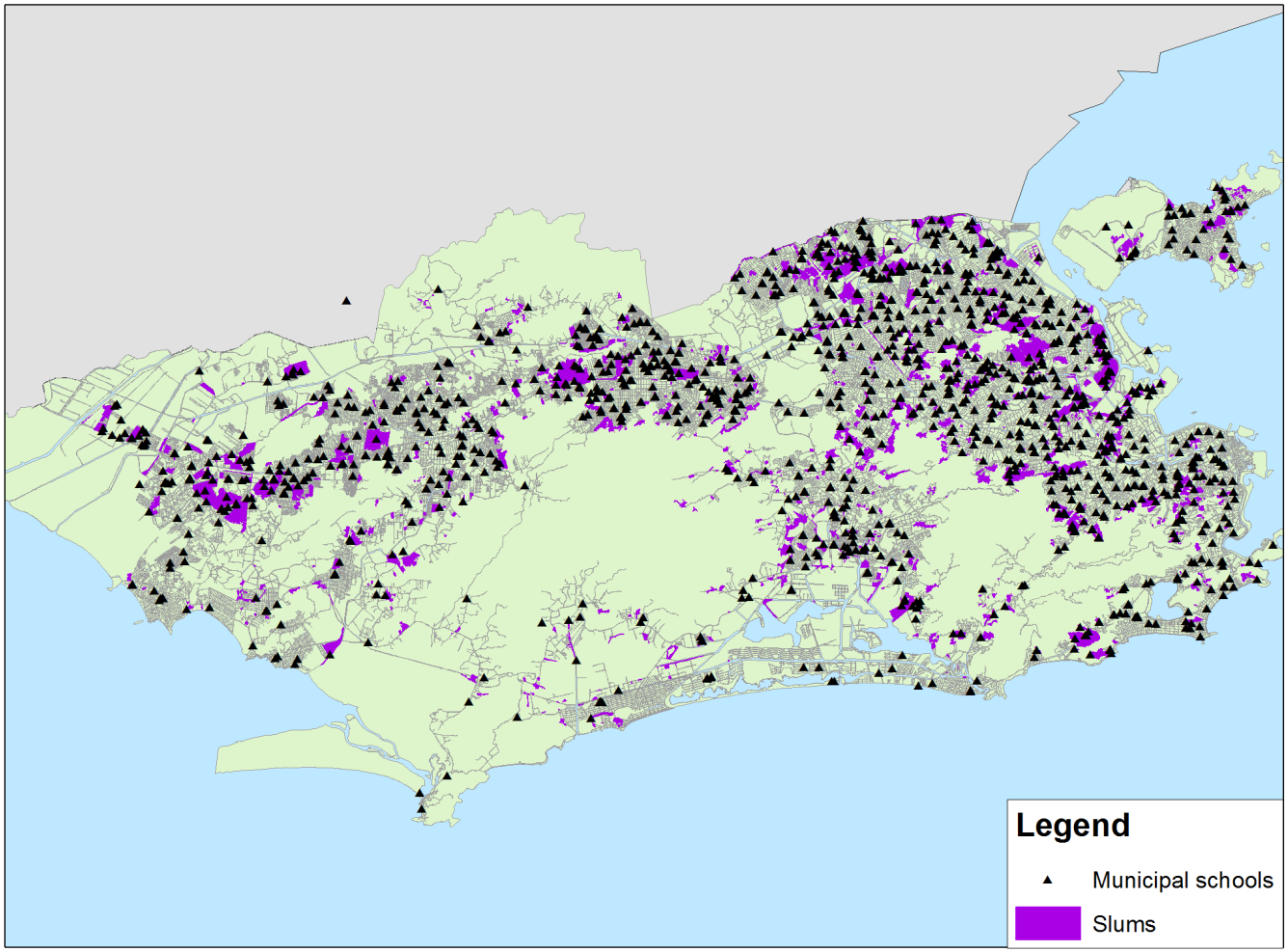


Figure 2: Number of Days with Reports of Gunfights 2003-2009

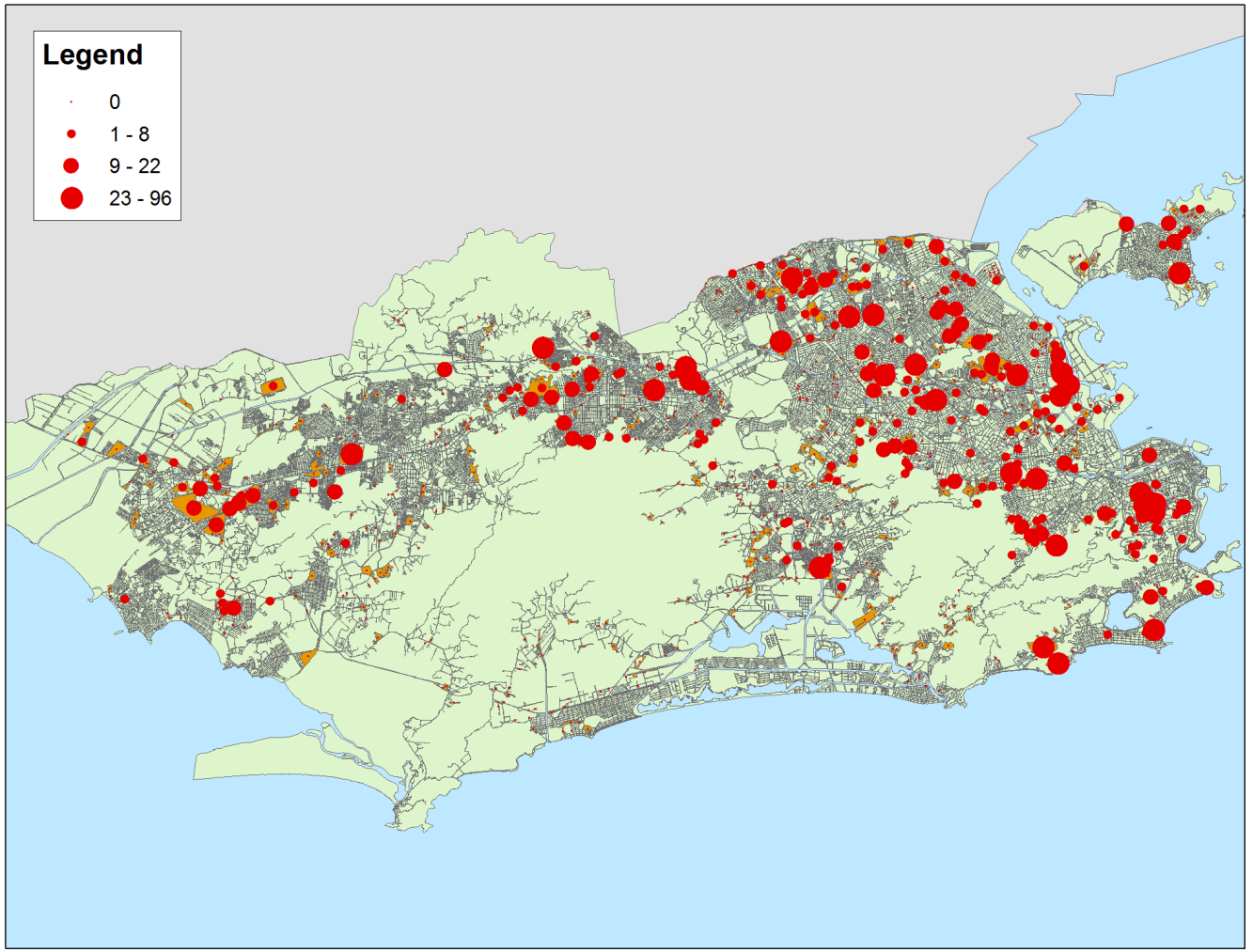


Figure 3: Number of Days with Reports of Gunfights per Year in Selected Favelas 2003-2009

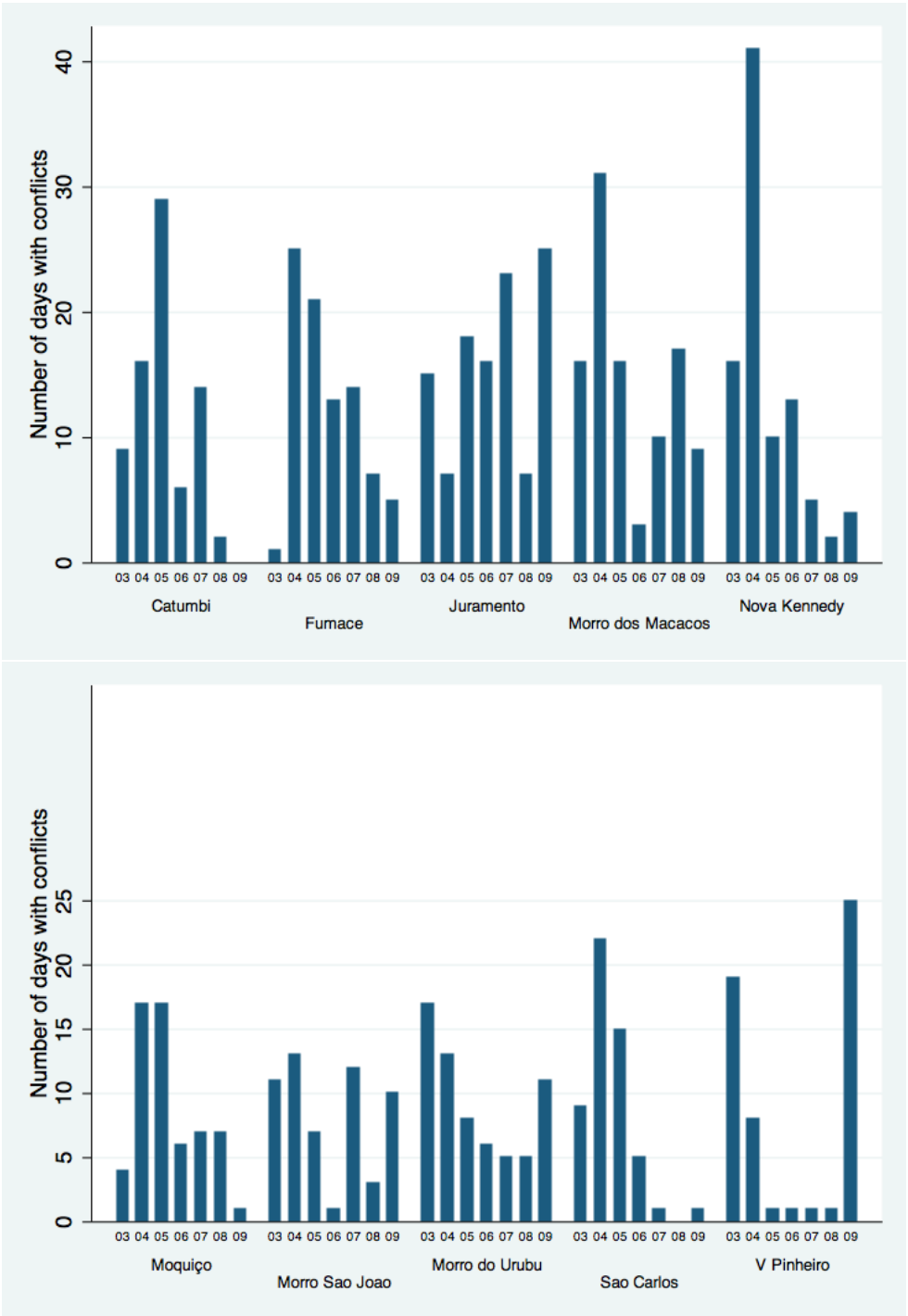


Figure 4: Impact on Student Achievement by Violence Intensity (number of days during the school period)

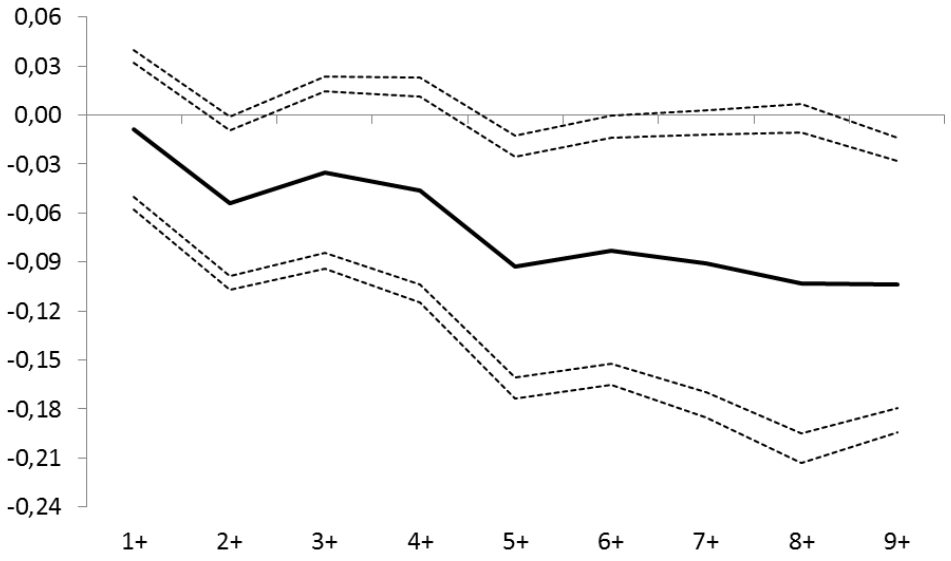
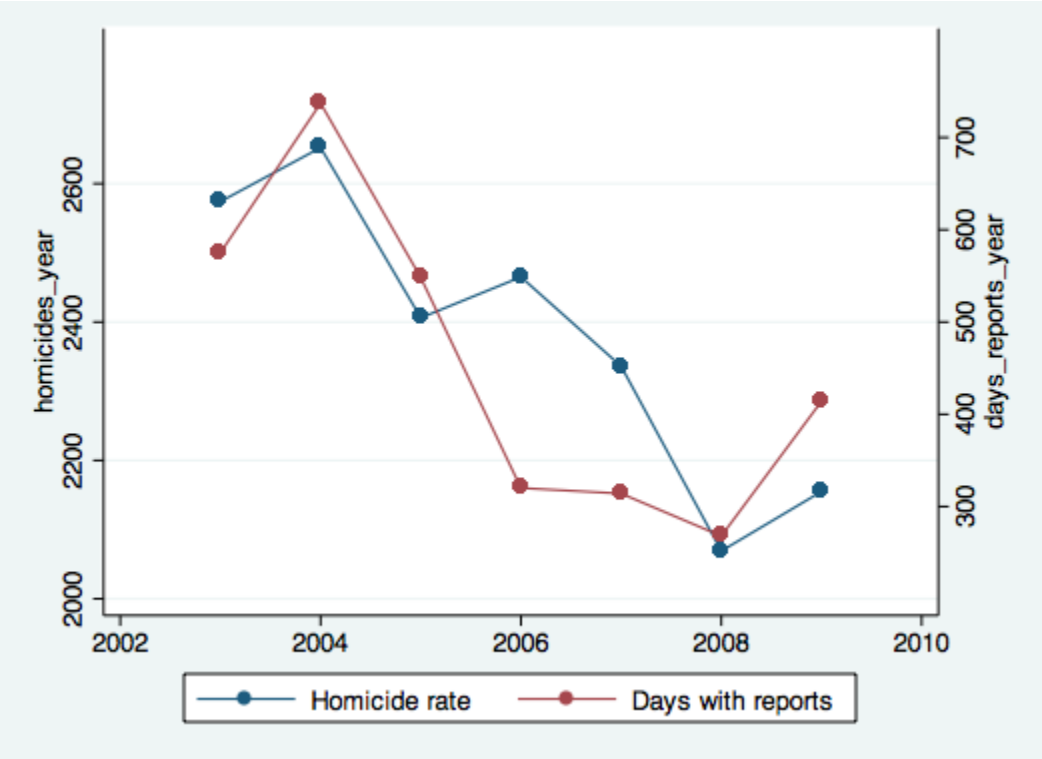
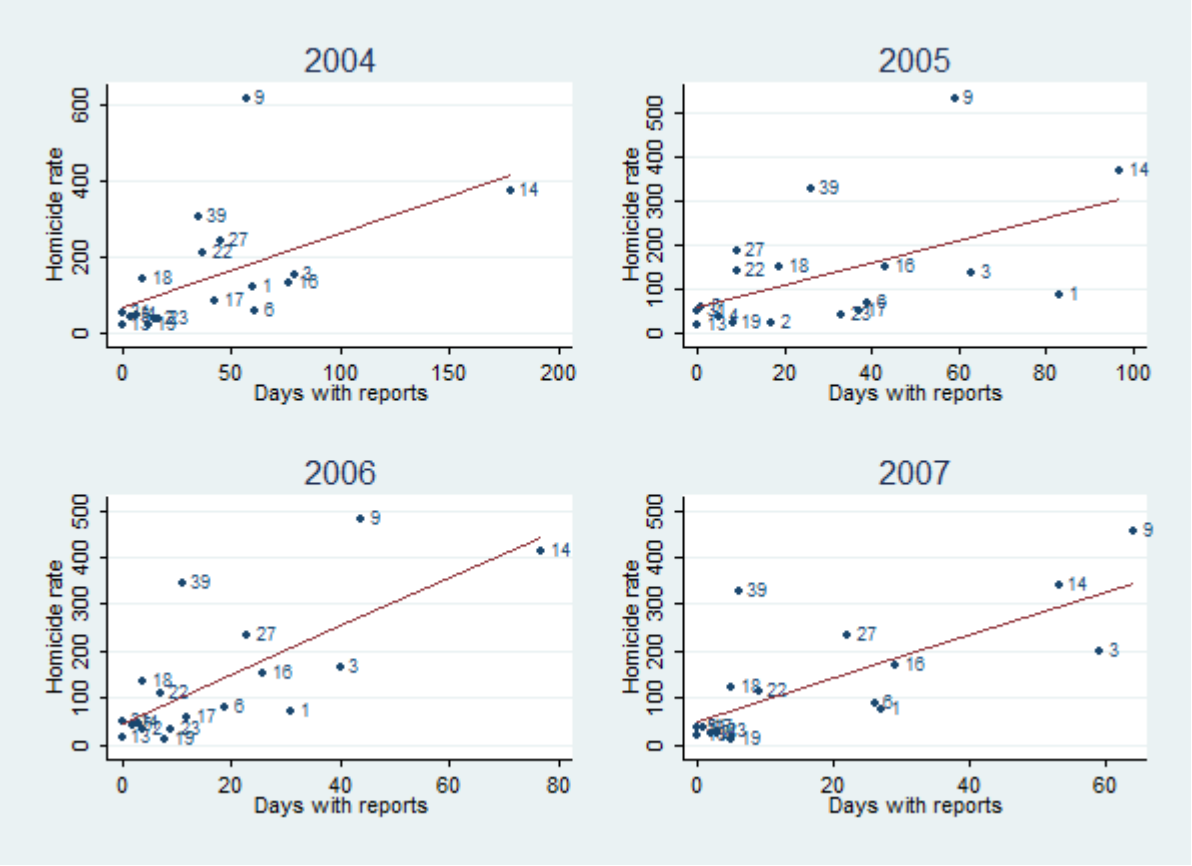


Figure 5: Homicides and Number of Days with Conflicts 2003-2009



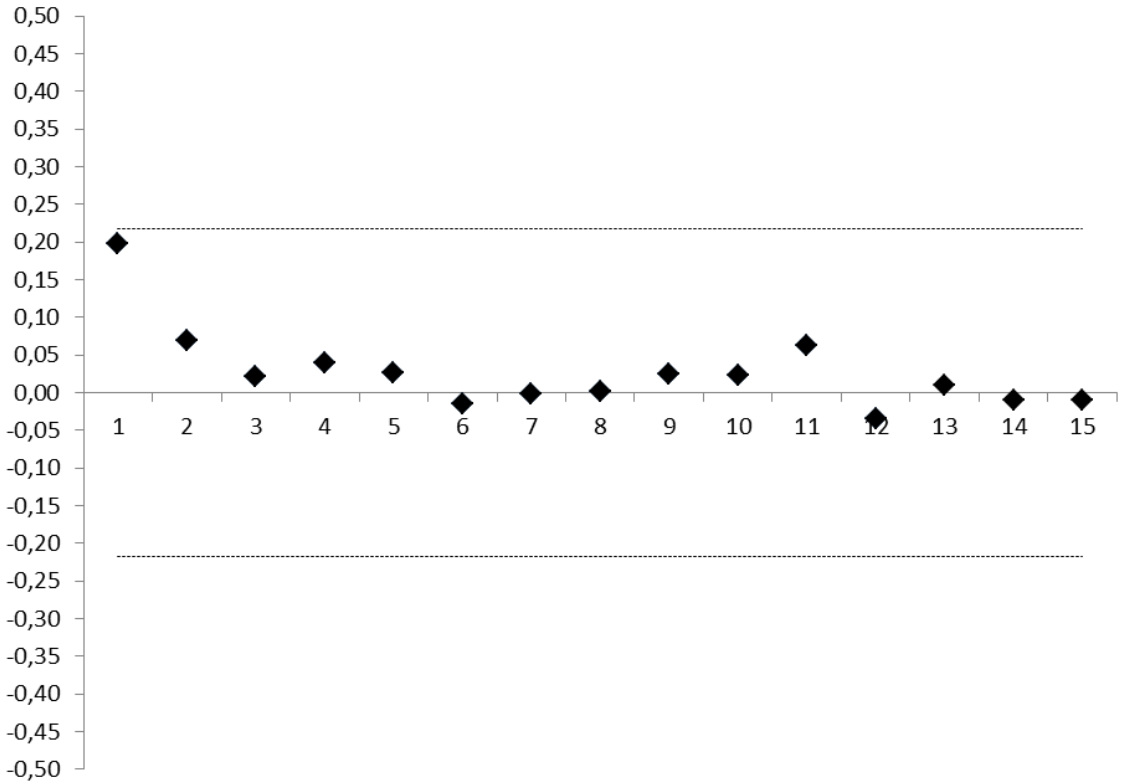
Notes: This figure compares the number of homicides and the levels of violence documented in Disque-Denúncia reports between 2003 and 2009. The left y-axis indicates the number of homicides in the city of Rio de Janeiro. The right y-axis indicates the sum of the number of days with reports about gunfight in all Rio de Janeiro's favelas.

Figure 6: Homicides and Number of Days with Conflicts per AISP



Notes: This figure shows the correlation between the number of homicides in the city of Rio de Janeiro and the number of days with conflicts in Rio de Janeiro's favelas. Both measures are aggregated per AISP (the city division used by the police department). Each panel indicates a different year.

Figure 7: Conflict Dynamics at the Favela-Month Level: Correlogram for the PACF up to the 15th Lag



Notes: This Figure plots the correlogram for the PACF estimated up to the 15th lag, where the number of months is $T = 84$, and in which regressions we include month, year and favelas' fixed-effects.