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

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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. To identify the causal effect of violence on education, we explore variation in violence that occurs across time and space when gangs battle over territories. The evidence indicates that these battles 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 exams. The effect of violence increases with conflict intensity, duration, and proximity to exam dates; and decreases with the distance between the school and the conflict location. There is no evidence that the effect of violence persists for more than one year. Finally, we find that school supply is an important mechanism driving the achievement results; armed conflicts are significantly associated with higher teacher absenteeism, principal turnover, and temporary school closings.

JEL: I25, K42, O12
Key words: favelas, slum, violence, drug gangs, 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. In recent years, violence involving drug trafficking organizations has notoriously reached unprecedented levels in Mexico and Central America (Rios [2012], Geneva Declaration [2011]). Conflicts between drug dealers using combat weapons caused Marseille, France, to experience one of the most extreme periods of violence in its history in 2012 (New York Times [2012]). In the US, retail drug trade and the distribution activities are routinely associated with violent, and often lethal, disputes over control of drug territory and enforcement of drug debts (FBI [2011]). 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 the population of affected areas. In particular, violence can have serious welfare consequences both in the short and in the long run if it impacts education production, children's schooling, and accumulation of human capital.

This paper studies the negative spillovers of conflicts between drug gangs in Rio de Janeiro by analyzing how they affect educational outcomes of children attending schools located in and around conflict areas. In recent decades, several *favelas* (slums) scattered across the city have been dominated by drug gangs, who use the territory to sell drugs and hide from police (Misse [1999], Silva et al. [2008]).[1] When gangs fight to gain territory local violence skyrockets. These conflicts are extremely violent and rely on heavy weaponry, such as grenades and modern military-grade machine guns. As a consequence, once a conflict is triggered, safety concerns and threats to individuals' lives dramatically increase in the conflict's location. In this setting, we may expect potential connections between violence and our main outcome variable, student test scores. For instance, violence may disrupt the school routine, increase teacher and student absenteeism, and cause major psychological distress.

The estimation of the causal effects of drug-related violence on educational outcomes is not a trivial exercise due to 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, confounding cross-section analysis that aims to identify the violence effects. We circumvent this problem by exploring varia-

[1]We use *favela* in the remainder of the paper to refer to Rio de Janeiro's slums. We provide a detailed definition of this term in the Background section.

tion in drug-related conflicts over time and space. Most of the disputes occur because gangs have no access to legally enforceable contracts or property rights and, therefore, typically rely on violence as the primary tool to resolve disputes. Indeed, our data suggest that drug gang conflicts are not rare events: on 65 percent of the days between 2003 and 2009, there was at least one favela in conflict in Rio de Janeiro. Such high conflict frequency 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 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 temporal variation in 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 the effects of violence requires fine-grained data on when and where conflicts take place. In order to track these events, we built a novel dataset which aggregates 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, both 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 provide evidence that students from schools which are exposed to violence perform worse on standardized math exams. Conflicts during the academic year are associated with a decrease of 0.054 standard deviations in math test scores. The violence effect increases with conflict intensity and duration, and 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 localized. Although there are significant short-run impacts, 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 the impact of violence on school supply is an important mechanism driving our results. Armed conflicts are significantly associated with higher teacher absenteeism, principal turnover and temporary school shutdown.

To the best of our knowledge, there is no causal estimate in the literature that provides unequivocal evidence linking violence and educational outcomes. The existing literature relies on cross-sectional analyses and faces difficulties in disentangling violence from other types of socioeconomic disadvantage that also have negative impacts on children's education, such as poverty, domestic violence and parental education (as in Grogger [1997], Aizer [2007], Severnini and Firpo [2009]). For instance, Grogger [1997] documents that violence within schools, measured with data from principal reports, is negatively correlated with the likelihood of high school graduation and the probability of college attendance. However, violent schools are more likely to be in poorer neighborhoods, where families may suffer from other forms of disadvantage. It may be the differences in family backgrounds, not in school violence per se, that are responsible for the results.

Though less related to our work, there is also 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, disrupting institutions and infrastructure. The mechanisms that operate in our context are likely different and more specific. We use information on school supply and student mobility to shed light on the mechanisms through which violence affects education.

A growing number of studies examines negative spillovers of disputes in 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. Evans et al. [2012] argue that the introduction of crack cocaine in the US and the consequent spike in violence lowered life expectancy of young black males and decreased their high-school graduation rates. Contrary to our results, they find that drug markets impact educational outcomes through changes in the returns to education, while our results emphasize the school supply channel. Dell [2011] analyzes the drug war in Mexico and finds suggestive evidence that drug trafficking presence is associated with lower informal sector wages and female labor force participation. Frischtak and Mandel [2012] show that the removal of drug traffickers' rule from favelas in Rio is correlated with an increase in property values.[2]

The remainder of the 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 provide the results of the analysis. 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, resulting in 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).[3] This record, already high by international standards, masks striking differences in exposure to violence within the city. In 2009, poor neighborhoods in the Northern zone of the city experienced 60.3 deaths per 100,000 inhabitants, while rich neighborhoods in the Southern zone recorded a homicide rate of approximately 6.6 per 100,000 habitants.[4]

Violence in Rio de Janeiro increased rapidly in the early 1980s. This period is marked by the foundation of Comando Vermelho (CV), the first major organized drug

[2] Similarly, but focusing instead on terrorism, 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.

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

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

gang in Rio de Janeiro (Dowdney [2003]). During this time, drug dealers utilized the marijuana trade network already established in Rio de Janeiro's favelas to sell cocaine. Control over the favelas' territory became crucial to protecting the illicit and lucrative trade. The favelas' geography, with tiny streets and crowded corners, as well as a lack of enforcement of formal rules within its boundaries, make them an important market for drugs as well as 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 an increasing disputes 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 additional 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 control of 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), leading to high death tolls even among those not directly involved in the drug trade.[5]

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 instead 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 as 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,[6] each favela typically has a local boss who runs the operations independently and who decides how to defend the territory and whether to attack his rivals.

Case studies and conversations with officers of the Intelligence Unit of the Military Police support the view that there is an unstable equilibrium among local drug traffickers. The local boss controls the favela and maintains the order until the

[5]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.

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

"peace" is broken by the imprisonment or release of a gang leader, betrayal, 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 in a quasi-random manner. For example:

Excerpt 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)

Excerpt 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)

Excerpt 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 excerpts also indicate how violent these events are. People who live in conflict areas or near them are heavily affected. Freedom of movement is drastically restricted during these periods given the increased likelihood of being hit by a stray bullet. People who are associated with a drug gang can be evicted from their homes or murdered when a new gang assumes control. In addition, as the excerpts show, conflict duration can vary greatly. Conflicts to depose a gang can take anywhere for a few hours to multiple days. Though incumbents often succeed the battle, cases of deposal are usually followed by attempts to reconquer the territory further extending the period of violence. 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 will end. The impact of these conflicts on the daily routine of the Rio de Janeiro's habitants is also attested to with the responses from a victimization survey conducted 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%).[7]

The police force in Rio de Janeiro has low wages, a long history of corruption, and less effective weapons than the drug gangs (Perlman [2010]). Hence, the police does 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 attempted only to interrupt the conflict, and not to definitively remove the drug dealers' control over the favelas.[8]

2.2 The Favelas of Rio de Janeiro

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.[9] 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 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.

While most of the conflicts occur in favelas, not all favelas are controlled by

[7]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.

[8]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 permanently remove the drug traffickers rule from the favelas.

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

drug gangs or are constantly under conflict. Favelas are also not a synonym of poverty. Although favelas typically have high poverty rates, 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 (Vianna [2008]). Yet, social inequalities still persist. According to Neri [2010], in 2007-2008 earnings and education amongst the favela inhabitants were significantly lower than the earnings and education levels of 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 correspond to 46 percent of the students enrolled in the system. There are no school districts in the city and students have some choice of which school they would like to attend. However, some schools have more demand than others, so some students are not able to enroll in their first choice.[10] The public school network is complemented by a private system, although private school enrollment 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 school employees are hired through public exams. 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 within the chosen region. Mobility across schools between years depends on seniority. After three years working in the system, employees can apply to transfer to another school. Conversations with professionals suggest that some teachers do move away from violent areas between school years. Within years, however, it is not possible to transfer and teachers can only respond to episodes of violence with absenteeism and attrition.

Figure 1 shows that schools are widely distributed across the city. This feature, along with the fact that 98% of the children of school age living in the city attend

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

school in Rio de Janeiro, indicates that school coverage is not a main concern in the city. However, school quality is highly variable. 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 spend 1 hour and 15 minutes less per week in school 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

Understanding the consequences of Rio de Janeiro's armed conflicts requires detailed information on where and when conflicts take place. This is necessary because exposure to violence varies between and within neighborhoods. Official crime data, which is provided by Instituto the Segurança Pública (ISP), does not provide sufficiently fine grained information on differences in violence because it records information gathered by police stations, which are not evenly distributed across space. In addition, ISP does not track information on when and where conflicts happen. Instead, they track only on homicides, which is a noisy outcome of these conflicts. To overcome the lack of finer data available from the police, 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), an NGO created in 1995, 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 disseminated across the city (e.g. on supermarket bags and bus advertisments).

The reports are recorded in a database which contains the date, location and description of each event. Residents may call to report any kind of crime, or irregularities, the location of criminals or corpses, or to report simple complaints such as noise disturbances. DD has provided us with 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 locations provided. The location and the description of the events allow us to associate 92% of the reports with a specific favela. We then sort the data by favela and by year and count the number of days per year when at least one report of armed conflict is reported to Disque-Denúncia in that favela-year. Appendix B details of how the dataset was 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.[11] In addition, we exclude an additional 315 reports that we were not able to associate with a specific favela, leading to a final sample of 3,527 reports.[12]

Although 92% of the reports of drug related violence occur in favelas, not all favelas are exposed to conflicts. Table 1 shows that just over 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 a total of 10 reports between 2003 and 2009. In our analysis we use the number of days with conflicts in each favela rather than the total number of reports because one person may call several times in the same day 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 displayed in Figure 2. This figure demonstrates that violence peaks in different years depending on the favela. Figure 3 presents a map with the distribution of the total number of days with reports between 2003 and 2009 across favelas. We observe that conflicts are widespread across the city.

3.2 Educational Data

In order to determine the impact of drug-related violence on education, we use three educational databases that provide information at the level of the student, the school, and the teacher. Our main outcome variable is student scores on Prova Brasil,

[11] 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.

[12] 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.

a national standardized exam given to all fifth graders in 2005, 2007 and 2009.[13] All students from public schools with more than 30 students enrolled in the fifth grade in 2005, or more than 20 in 2007 and 2009, were required to take this exam. The exam has two portions: math and language (Portuguese) skills. In addition, students respond to a survey about their socioeconomic profile, and 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 for fifth graders who take the Prova Brasil exam. Our benchmark sample is comprised of 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 a favela. We include the full sample of schools, which consists of all the 736 schools of the municipal system that participated in at least two Prova Brasil editions, in the heterogeneity analysis and in robustness checks. 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 to 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) in any year between 2003 and 2009. This definition of exposure to violence 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 to and schools not exposed to violence. The former has significantly lower Prova Brasil scores. However, it is not clear whether the worse performance is attributable to violence, since students from households of low socioeconomic status (students with illiterate mothers, non-white, and students who have previously repeated a grade or dropped out school) are over-represented in schools exposed to violence.

We complement the Prova Brasil test scores 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 demographic information. In particular, the dataset contains information on student

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

mobility within the system. This information includes all of the municipal schools that each student has attended in the past, the grade in which they were enrolled, and if and when they transferred between schools. These data allow us to generate indicator variables for whether the student leaves or enters a school during the school year, or between academic years. Based on this data set, Panel B of Table 2 shows statistics for all 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 students 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 declines to 7 percent within the academic year. The difference between the percentage of students leaving and entering schools suggest that the dropout rate (or attrition, more generally) is around 10 percent between academic years, and 4 percent within years. Schools exposed to violence have higher mobility rates than schools not exposed to violence. Panel B also indicates that each school enrolls an average of 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 of teachers' absenteeism and medical leaves from 2007 though 2009, allowing us to calculate absenteeism rates, both for unexcused absences and for 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. 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). The favela borders, which are based on satellite pictures, are key to our analysis. This information is not only precise, but also quite detailed, with much more finely grained favela definitions than other datasets. As a result, IPP's classification identifies 979 favelas (rather than about 300 given by other definitions), which allows us to better localize each event of violence. In order to match more precisely the DD reports to each favela, we use a list with the favelas' alternative names computed by IPP. The IPP also provides shape files with municipal schools' locations, and we used GIS tools to calculate the distances from favelas' borders to schools.

We also collect information on favela and neighborhood socio-geographic characteristics in order to conduct robustness checks and understand the cross-section determinants of conflicts. We show this analysis in Appendix C. We gathered from IPP income per capita, Gini index, and population, calculated at neighborhood level based on the 2000 IBGE Census, as well as shape files with Rio de Janeiro's main roads and neighborhood limits. 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.

4 Empirical Model

In this paper we analyze highly 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 test scores. First, violence may impact the school's human resources, for example, by increasing teacher attrition and absenteeism, by causing interruption of classes and school closing, or by increasing workplace stress and principal turnover. Second, exposure to violence may directly affect student learning through mental health and psychological impacts. We discuss these two channels in section 4.1, both to provide the conceptual underpinnings from which we develop our empirical strategy (section 4.2) and to help identify the potential caveats (section 4.3).

4.1 Conceptual Discussion

Violence may have substantial effects on learning through school supply. As in Grogger [1997], the theory of compensating differentials predicts that teachers (or the school staff more generally) would demand a wage premium in order to accept work in a school at risk of violence. Indeed, Grogger [1997] 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 fixed, violence may lead to higher teacher attrition and absenteeism. As a result, it is straightforward to predict that student achievement will suffer as classes are taught sporadically or discontinued. We also hypothesize that violence may have disruptive effects on 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 closings and interruption of classes. Additionally, principal turnover may also rise since managing a school in an area with high conflict is likely to be difficult as well as risky.

The consequences of exposure to violence may extend beyond the school supply channel. Research conducted by psychologists and psychiatrists has recognized the potential harmful effects of neighborhood violence on children's mental health. Fowler et al. [2009]'s meta analysis even suggests that children exposed to community violence are at a greater risk for developing post-traumatic stress disorder (PTSD) symptoms.[14] In addition to PTSD, exposure to violence can also be associated with depression and anxiety in young children (Buckner et al. [2004]; Fitzpatrick [1993]).[15] Research contrasting subtypes of violence suggests that the effect of exposure to violence on negative outcomes may increase with the children's physical proximity to the violent events (Nader et al. [1990]; Fitzpatrick [1993]). Family support appears

[14] 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.

[15] 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]

to attenuate the consequences of exposure to violence on children.[16] Yet, parents who have been traumatized are also more likely to have children who feel unsafe or who develop PTSD symptoms (Linares and Cloitre [2004]). In this case, parents could 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 their children's human capital in order to compensate for the unexpected cost of violence. 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 a number of paths connecting local violence and childrens' learning, there is no causal estimate available in the literature that unequivocally attributes a negative effect on student achievement to violence. The existing results in the research conducted by psychologists and psychiatrists have limitations, as identified by psychiatrist Osofsky [1999] (cited in Aizer [2007]). 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 impact of violence on test scores (Aizer [2007]). 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 - are also concerns in previous studies in economics, as recognized by their authors (see, for instance, Grogger [1997], Severnini and Firpo [2009], and Aizer [2007]).

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 effect of exposure to violence on learning. The modeling of the production function for cognitive achievement is often based on the idea that a child development is a cumulative process,

[16] Though not at all margins. For instance, Overstreet and Dempsey [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.

dependent on the history of family and school inputs as well as on innate ability (Todd and Wolpin [2003]). In this paper, we do not attempt to estimate a tightly specified education production function given that we do not observe past inputs and test scores. Instead, we propose a reduced-form strategy which rely on the evidence that variation in conflicts within favelas over time is 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 Portuguese, 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 year 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's 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 shootings that may add noise to our analysis. Formula (2) is 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 to 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 students live within 15 minutes walking 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.[17] Given that our analysis does not take into account the cross-sectional variation in violence and the impact of being under the rule of drug dealers for extended periods, one might reasonably interpret our estimates as a lower bound for the impact of drug-related violence on student achievement.

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, mother's education, age fixed effects, and dummy variables for whether the child has ever repeated a grade or dropped out in previous years. The term X_{st} indicates a set of variables that absorb 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 differential school physical infrastructure (we add dummy variables for whether the school has a computer lab, science lab, principal's office, teachers' offices, free lunch, and a kitchen).

We focus on young children (5th graders) in order to avoid potential endogeneity driven by reverse causality - lower school quality may lead to children to become involved in drug trafficking and to higher violence. Our benchmark sample includes only the students enrolled in schools located within 250 meters from at least one

[17]Though incumbents often succeed the battle, the eruption of a conflict will eventually result in the entry of a new gang into the territory if the incumbent gang is deposed. In this case, the flux of gang entry and exit out of the territory may have effects on student performance through other types of violence besides those specifically generated by the conflict. For instance, the new gang may impose widespread psychological fear and life threat among favela residents and teachers through extortions and evictions.

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 generates more comparable treatment and control groups because it accounts for the fact that schools near favelas are possibly exposed to higher levels of chronic violence, and typically have more students from disadvantaged households. We nevertheless confirm that our results are robust to sample selection.

Identification relies on the assumption that, conditional 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. If this assumption holds, 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 regarding the importance of the school supply 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 to be considered in 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 over time and across schools, we can fully identify all her movements within the system (transfers between schools) or out of the system (if her enrollment number disappears from the records). We are therefore able to examine whether violence impacts student mobility and dropout. Second, we also test selection at the Prova Brasil exam by examining whether 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 and show in Appendix C that trends in both series are remarkably similar. In addition, we aggregate our reports into 18 major regions of the city for which homicide data is available. We plot the relationship between the homicide rate and the number of days with reports by region, separately for each year, and observe that the 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 geographic features, such as the favela's steepness and 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. Finally, 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 a 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 impact of violence 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 reports our full specification. It adds school fixed effects and presents our within-school estimates. For each of these regressions, the sample includes students from schools located within 250 meters of at least one favela. The variable of interest, violence, 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 declines 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 in magnitude, but nevertheless remains statistically significant at the 5% level. Clustering standard errors one-level-up, allowing for unrestricted residual correlation within neighborhoods, provides similar results (standard errors drop marginally from 0.027 to 0.025). 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 the evidence 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 indicates 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 of 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. Exposure to violence triggered by drug gangs leads to a reduction of 0.054 standard deviations in math test scores. This effect is equivalent to 1/3 of the magnitude of the coefficient estimated in a regression of math test scores on a dummy indicating the child's mother had low education (none or only primary education). In comparison to the importance of other determinants of academic performance, the effect of violence is equivalent to 1/4 to 1/2 of the drop in test scores associated with a one standard deviation decrease in teacher quality, as documented in the related literature (Rivkin et al. [2005], Rockoff [2004]).

In the following sections we further characterize the violence effect on achievement and present robustness tests. Given the results in Panel B, the remainder of the paper focuses on achievement in math. First, we examine whether the effect of violence varies with the distance between the school and the conflict location. In Section 5.3 we test whether the effect of violence is sensitive to the conflict intensity and length. In section 5.4 we study the specific timing of the effect of violence. Section 5.5 explores heterogeneity in the effect by student 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 selection bias in student composition 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 column of Table 4 we report a regression that includes only schools located in favelas. The variable of interest considers only the conflicts in the favela where the school is located. In this case, the control group includes only the schools located in favelas, but not exposed to gang conflicts. In the following column we expand the sample to include only schools located within a radius of 100 meters from a favela, while the variable of interest considers only the violence in favelas within a radius of 100 meters from the school. We continue expanding this buffer in the following 4 columns, until we reach the violence that occurs in favelas within 450 meters of the school, for those schools situated within 450 meters from a favela. We observe that violence has a very local impact.[18] In column 1, which focuses only on schools located in favelas, despite the small sample, we find a higher coefficient (0.130), significant at 5%. The coefficient drops as we move to the second column, but increases again and is robust at 5% level in the 200 and 250 meters specifications.[19] We observe in the remainder columns that the point estimate tends to decreases as the buffers continue to increase. Figure 4 complements Table 4 by plotting the coefficients of ten different regressions of student achievement on the violence indicator, each computed for a distinct buffer of distance from the school to the conflict location. Figure 4 confirms that violence mainly affects students from schools located within 250 meters from the violence epicenter.

Finally, the last column of Table 4 presents a different test by including simultaneously three indicators of violence. These variables compute, respectively, (i) the violence that occurred in the favela where the school is located (if the school is located in a favela); (ii) the violence that occurred in favelas within 250 meters or (ii) 500 meters of distance from the school. The sample includes schools within 500 meters from a favela. 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. Column 7 indicates that students from schools located within 250 meters from favelas exposed to conflicts have an average score 0.067 standard-deviations lower than students from schools which are between 250 and 500 meters from favelas with conflicts. It also indicates that students from schools inside and in the border of favelas (up to 250m distance) are equally affected by violence, while students from schools located farther than 250 meters are not affected.

[18] As reference point, the standard city block in Manhattan is about 80 by 270 meters

[19] Point estimates should drop for shorter distances (less than 100 meters) because in these cases the control group includes schools that are also exposed to conflicts (for instance, the ones located within 100 to 250 meters of distance from a favela exposed to conflicts).

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 benchmark 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.

To accomplish this, we perform two tests. First, we compute a series of 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 5 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.[20]

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: (i) the extent to which student achievement by the end of the year varies with the moment of the violence shock during the school year and; (ii) the extent to which violence has either persistent or transitory effects on learning.

In order to explore the timing of the effect of violence, we perform two tests. First, we break the computation of the violence indicator into three different periods of the calendar year: (i) 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 vacation months that follows 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 the vacation period (December through February) 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. The other two coefficients in the first column are quantitatively more important, but only the effect associated with the second term, which immediately precedes the exam, is significantly different from zero (only at 10%). Overall, the point estimates suggest that the relevant timing of the events of violence corresponds to the months just before the exam, though the breaking of the violence indicator into three new variables adds noise to the estimation and does not allow us to reject the null hypothesis that they are statistically equal.

In the remaining 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

[20] 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.[21]

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 result is consistent with other studies that also find that treatment effects on test scores 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]). The interpretation that the effect of violence is only transitory, however, should be taken with caution. First, test score impacts of educational interventions often fade out over time even when its effects on knowledge does not (Cascio and Staiger [2012]). Second, even if test scores effects fade, it is possible that there are lasting effects on personality skills such as through the deterioration of externalizing behaviors and future academic motivation (see Heckman et al. [Forthcoming]).

5.5 Heterogeneity

Table 7 examines heterogeneity in the effect of violence 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 more negative than that for boys. In columns 3 and 4 we find a larger coefficient in absolute value for white students, roughly twofold that estimated for non-whites. Columns 5 and 6 show that the coefficient is larger for students with highly educated mothers. The coefficients by age are 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, the coefficients provide suggestive evidence that violence is more detrimental to girls and high-performing students (those with highly educated mothers, whites, at the correct age for grade, and that never repeated a grade), with the only exception being drop outs. On the one hand, this is consistent with experimental ev-

[21] 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.

idence on early childhood educational interventions which indicates that treatment effects are stronger for girls, in particular through enhanced academic motivation (see Heckman et al. [Forthcoming] for evidence from the Perry Preschool program). On the other hand, if high-performing students in particular benefit from instruction at school, the results in Table 7 support the view that school supply likely work as a significant link between violence and learning. This evidence should be taken with caution, however, given that none of the estimated differences are statistically significant. Section 6 provides more direct evidence in support of this view.

5.6 Student Mobility and Selection

In this 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, rather than a causal impact of violence on achievement.

In order to examine whether violence impacts 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 (if the student 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 during the school year. This includes both movements to other schools and 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 effect of violence 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. Another explanation is that parents may find it difficult to evaluate alternatives due to the quasi-random nature of conflicts. These findings are consistent with the evidence provided in section 5.4, which suggests that the effect of temporary episodes of violence on learning is not persistent.

We complement this analysis by testing for student selection at the Prova Brasil exam. We regress the socioeconomic characteristics of the students who 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, and ever dropped out in previous years. In any of these regressions we find systematic association between violence and student selection.[22]

6 The Impact on School Supply

Throughout the previous sections we followed a reduced-form strategy in order to identify and characterize an average effect of violence on test scores. This effect can be driven by a variety of channels likely to be at work in our setting. Since we are able to observe teacher and principal behavior, the final section of this paper focuses on the identification of specific mechanisms linking violence and school supply.[23]

[22]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.

[23]As already discussed in section 4.1, in our setting, exposure to violence may also affect learning through mental health symptoms. Although we acknowledge the potential role played by this

6.1 Teacher Absenteeism

There are many mechanisms through which teacher absenteeism and attrition may reduce student achievement (see Miller et al. [2007]). It may reduce instructional intensity, create discontinuities of instruction and disruption of regular routines and procedures of the classroom. It may also undermine common planning time, which can inhibit attempts by school faculties to implement practices across classrooms and grades. Consistent with this view, many studies have found a negative relationship between teacher absenteeism, turnover, and student achievement (Miller et al. [2007], Clotfelter et al. [2007], Ronfeldt et al. [2013]).

Table 10 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, teacher absences increase by 5.8 percentage points (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 contiguous violence is associated with an increase in absenteeism on the intensive margin. There is no evidence that violence affects medical leaves.

The results of Table 10 are qualitatively relevant since they link violence with a larger number of days without instructors in the classrooms. This result can be regarded as either a lower bound of the violence effect on absenteeism and/or teacher turnover. Unexcused absences are reported by the school's principal and are subject 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.[24] Thus, the effect of violence on unexcused absences is a combination of the impact on both short and lengthy absenteeism, where the latter is possibly a combination of absenteeism and of turnover.

Overall, these findings are consistent with the evidence that non-pecuniary factors and work environment characteristics are significant determinants of teacher transfers and retention (Hanushek et al. [2004], Boyd et al. [2005], Jackson [2009]). Table 10

mechanism, we are not able to empirically test whether or to what extent it helps generate the observed relationship between violence and achievement.

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

provides new causal evidence in support of this link, and contributes to the literature by documenting the detrimental role played by neighborhood violence in local labor markets dynamics.

6.2 Impact on School Routine

In Table 11 we examine whether violence affects school routine. In this analysis, we rely on a survey answered by principals in the 2007 and 2009 Prova Brasil editions. This survey investigates multiple aspects of the school's routine, including an extensive list of problems faced by the administration. We regress an indicator variable for whether the principal mentioned a given problem on the indicator for violence. The regressions include the full set of student and school controls, as well as school and year fixed effects.

Panel A of Table 11 indicates that, in schools exposed to violence, principals were 7.7 percentage points more likely to report that there was a threat to teachers' lives, an effect equivalent to 40% of the sample mean. There is also evidence that violence impacts teacher turnover, which increases by 5 percentage points (13% of the average) in violent years according to principals' reports, though this result is only marginally significant (p-value 0.14). Panel B examines 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 an interruption of classes (temporary school closing) in years with contiguous days of violence. This implies that schools are twice as likely to close temporarily in years with conflicts of long duration. This finding is consistent with several articles in Rio de Janeiro's main newspapers which mention that schools temporarily shut down during the conflicts in order to avoid teachers and students being caught in the crossfire.

Also of interest, column 5 indicates that the administrative staff are more likely to turnover 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 (a 31% increase in the sample mean). This finding is straightforward given 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 possible that principals may not carefully track absenteeism in periods marked by interruption of classes or unusual stress. However, the result nevertheless suggests that student absenteeism does not increase in a noticeable way in years when conflicts take place.[25]

7 Final Comments

This study provides evidence that drug-related conflicts have negative spillovers on the population living and working in conflict areas by demonstrating that violence affects both student achievement and education supply. Such episodes of violence have become a pervasive problem in many parts of the World. However, there is only limited understanding on the causal effects of violence due to identification challenges. As acknowledged in previous research, violence typically correlates with poverty and other local economic conditions. Simple cross-section analysis is therefore subject to measurement error and omitted variable bias. We circumvent this endogeneity by exploring variation over time in armed conflicts between drug gangs that are plausibly exogenous to local socioeconomic conditions.

We show that students from schools located close to conflict areas score 0.054 standard deviations less in violent years relative to their peers in the same schools in peaceful years. We also find that the violence effect increases with conflict intensity and duration, and 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. Thus, though substantially disruptive, the negative spillovers of episodes of violence on education seem geographically localized.

We are able to provide evidence for one mechanism through which violence affects student achievement; violence decreases instructional time and affects school human resources by increasing temporary school closing, principal turnover and teacher absenteeism. Interestingly, we find that students do not respond to these conflicts by leaving schools exposed to violence. Difficulty in predicting violence and evaluating alternatives may possibly explain this finding.

It is worth emphasizing that our analysis estimates the effect of exposure to extreme but temporary episodes of violence, and does not take into account the cross-sectional variation in violence and the impact of being under the rule of drug dealers for extended periods. Consequently, one might reasonably interpret our estimates as a lower bound for the impact of drug-related violence on student achievement. The fact that the magnitude of this lower bound is quantitatively important supports

[25]We also gathered administrative data on student absenteeism that indicate high levels of attendance and little variation.

the view that the costs of drug-related violence may go far beyond the casualties of those directly involved in the criminal activity and its victims. In this case, violence spillovers should be regarded as a relevant policy concern in conflict areas.

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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.[26]

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.[27]

[26] 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."

[27] 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.[28] 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.[29]

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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.”

[28] 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.

[29] 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.[30]

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)".

[30]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 6 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 7 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 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 7 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.[31] 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,[32] 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,[33] 298 favelas or 34% experienced a conflict between drug gangs between 2003 and 2009 according to Disque -Denuncia. During the same period, Globo network

[31] 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

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

[33] 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 this analysis.

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.[34] 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's 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 shown in Table 14 indicate that the only predictors of variation in conflicts are favela's 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,

[34]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 8 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)
Violence within favela	-0.130						-0.018
	(0.056)**						(0.081)
Violence within 100 meters		-0.034					
		(0.034)					
Violence within 200 meters			-0.067				
			(0.029)**				
Violence within 250 meters				-0.054			-0.067
				(0.027)**			(0.034)**
Violence within 300 meters					-0.034		
					(0.024)		
Violence within 450 meters						-0.006	
						(0.019)	
Violence within 500 meters							0.020
							(0.023)
Sample of Schools: Buffer in meters	Within favelas	Within 100m from a favela	Within 200m from a favela	Within 250m from a favela	Within 300m from a favela	Within 450m from a favela	Within 500m from a favela
Observations	5,700	37,947	62,484	76,084	87,437	112,288	162,999
Year and School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student, Class and School Characts	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: 1st Semester	-0.035		
	(0.034)		
Violence: 2nd Semester	-0.051		
(Just Before the Exam)	(0.030)*		
Violence: Vacation Months	-0.016		
(Just After the Exam)	(0.039)		
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 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 (1)	Nonwhite (2)	Mother Educ Low (3)	Repeated (4)	Dropped Out (5)	Age >=12 (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: 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 11: 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 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

Year	Number of favelas	Disque-Denuncia favelas with reports	%	number of reports	Newspaper favelas with articles	%	number of articles	Correlations 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.

Table 14: Cross-section determinants of variation in the number of days with conflicts during the academic year

Dependent variable:	Variation in the number of days with conflict during the academic year (2003-2009)
	(1)
Favela's characteristics:	
Steepness	0.032
	(0.011)***
Distance to main road	-0.183
	(0.144)
Area (1999)	0.002
	(0.001)**
Neighborhood characteristics:	
Population density	17.878
	(15.965)
Population (log)	0.134
	(0.120)
% youngsters on population	-5.531
(13-19 years)	(16.038)
Income pc	-0.841
	(0.611)
Gini index	3.643
	(2.670)
Constant	-1.449
	(2.776)
Observations	294
R-squared	0.113

Notes: This Table indicates favela's and neighborhood characteristics that correlate with the standard deviation in the number of days with conflicts during the academic years for 2003-2009 period. The regression is at favela level.

Figure 1: Favela and School Distribution

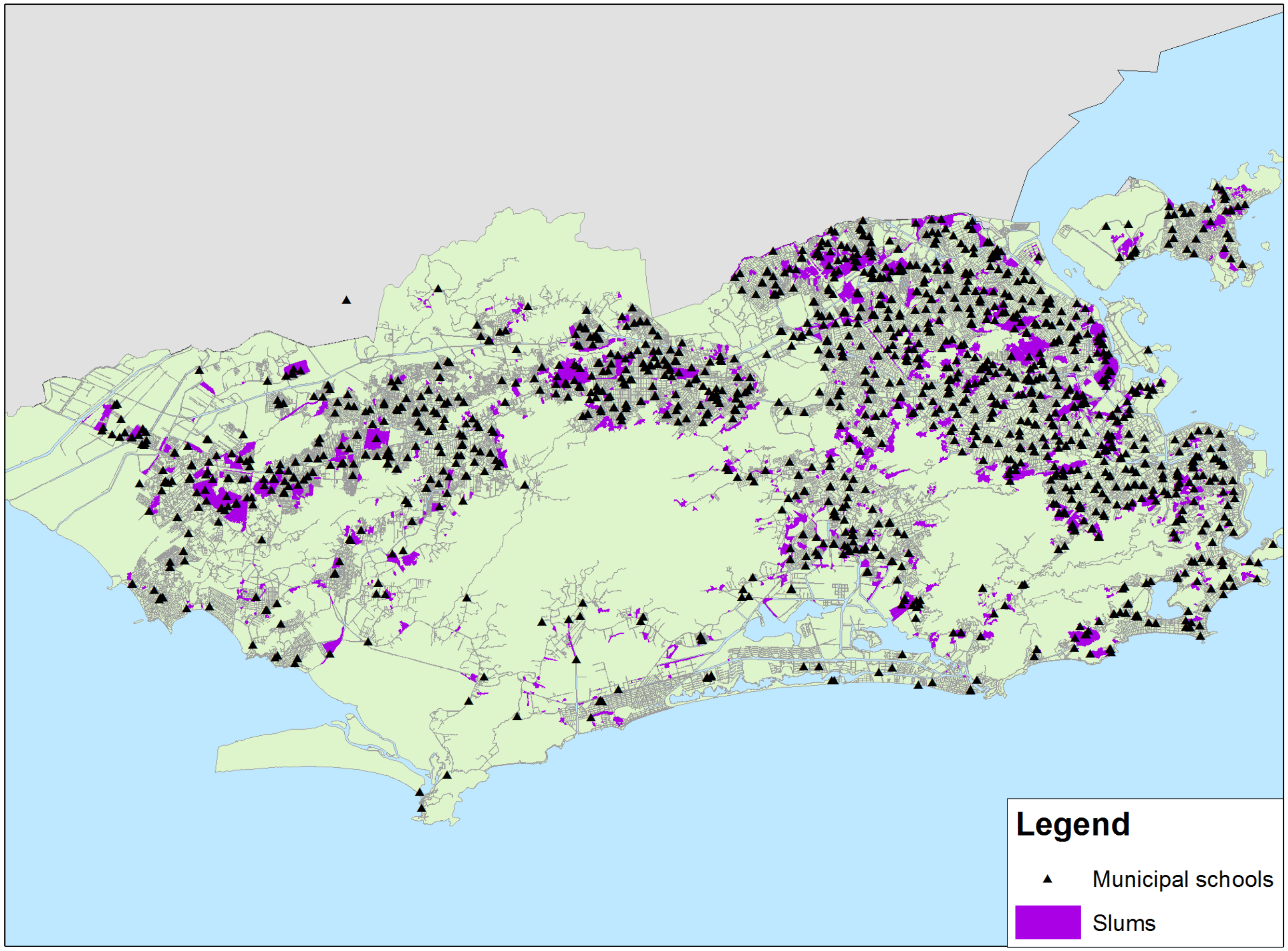


Figure 2: Number of Days with Reports of Gunfights per Year in Selected Favelas 2003-2009

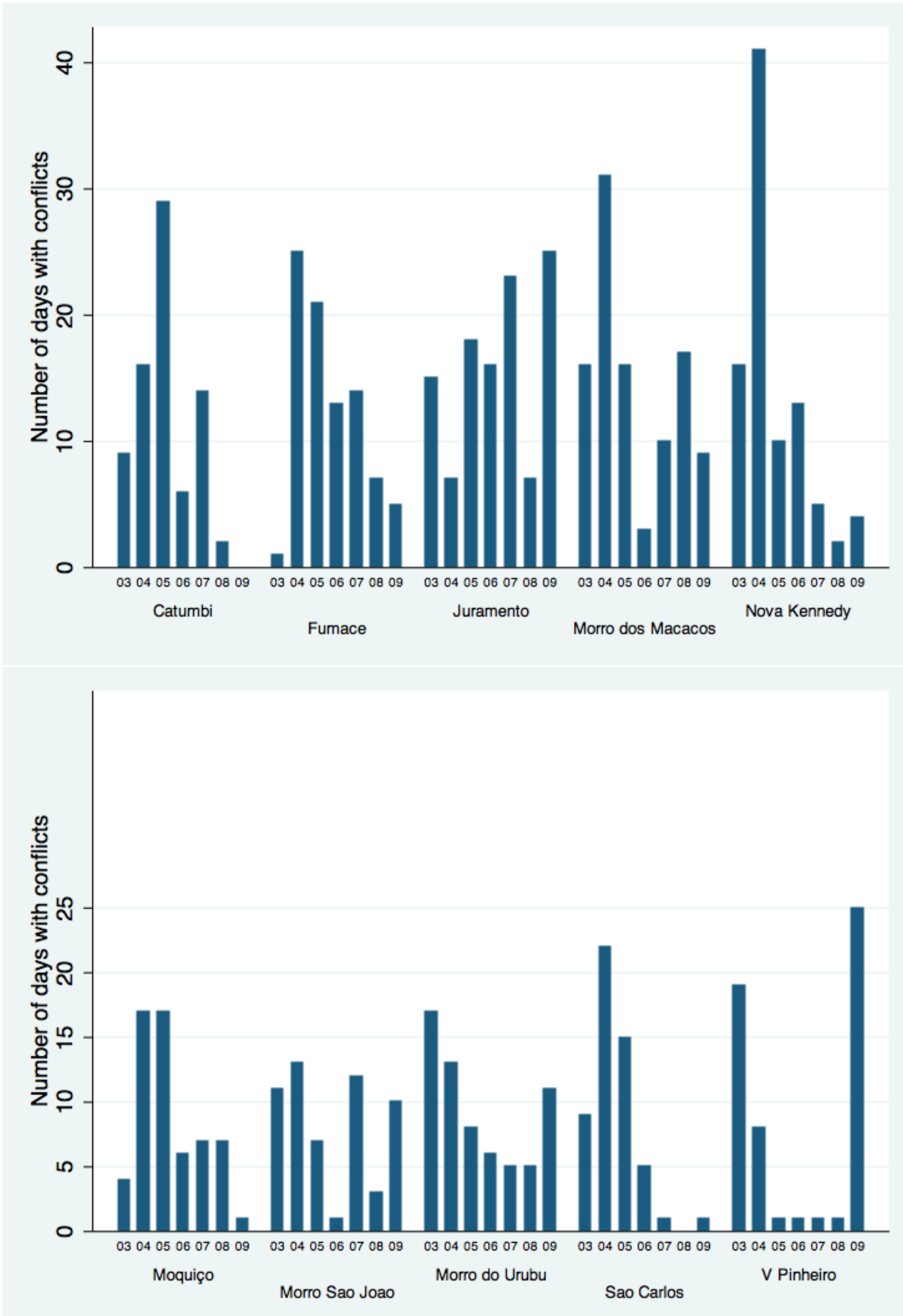


Figure 3: Number of Days with Reports of Gunfights 2003-2009

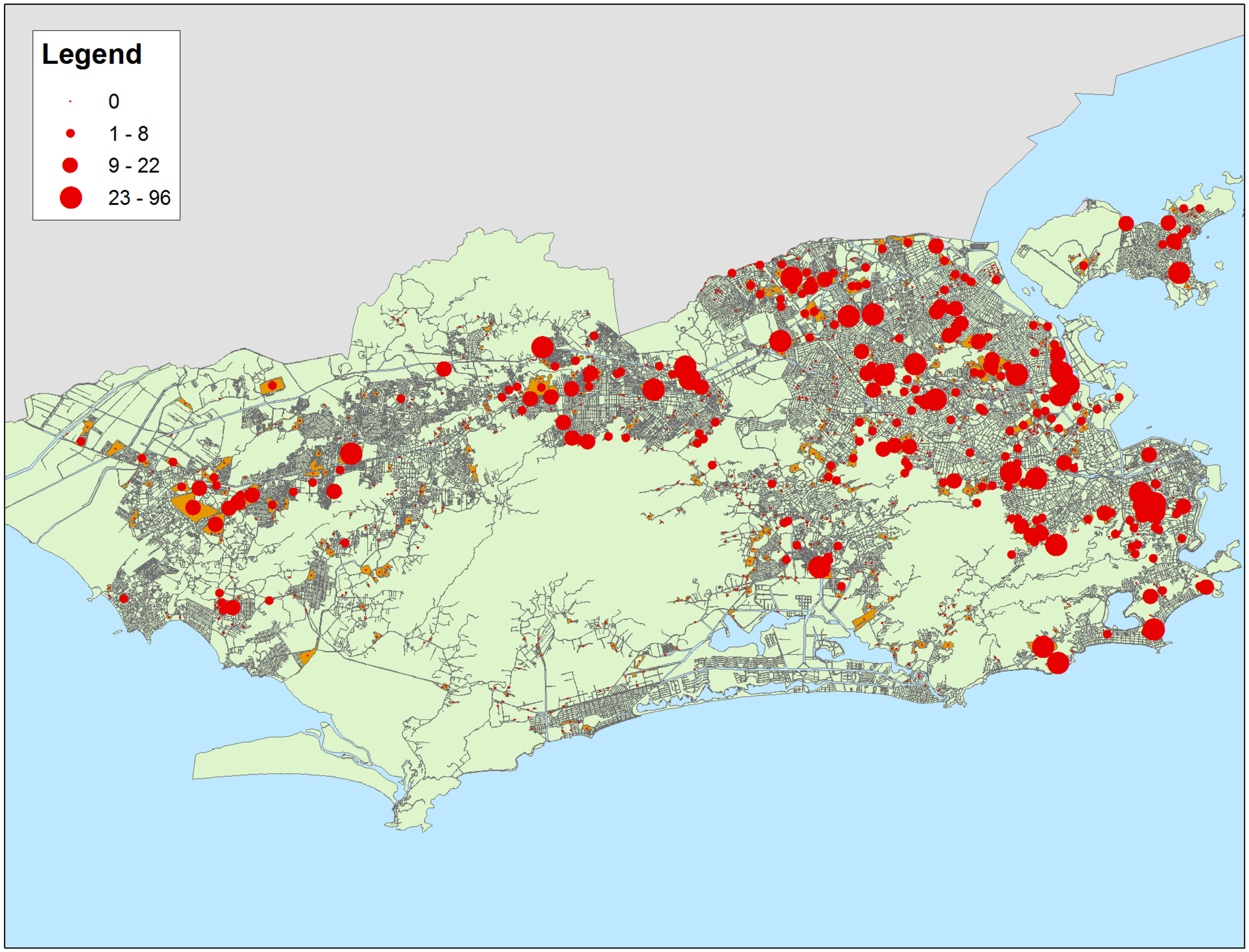


Figure 4: Impact on Student Achievement by Violence Distance (buffer of distance from the school to the conflict location, in meters)

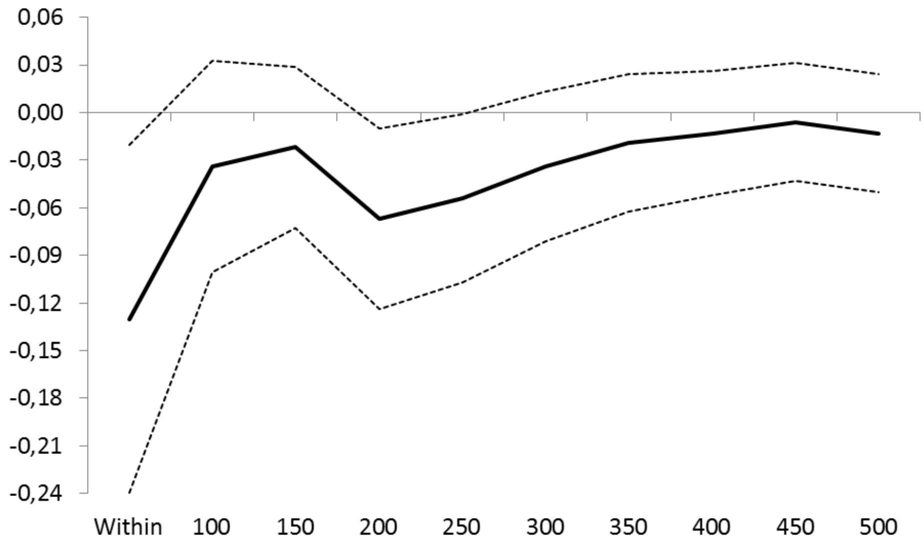


Figure 5: Impact on Student Achievement by Violence Intensity (number of days during the school period)

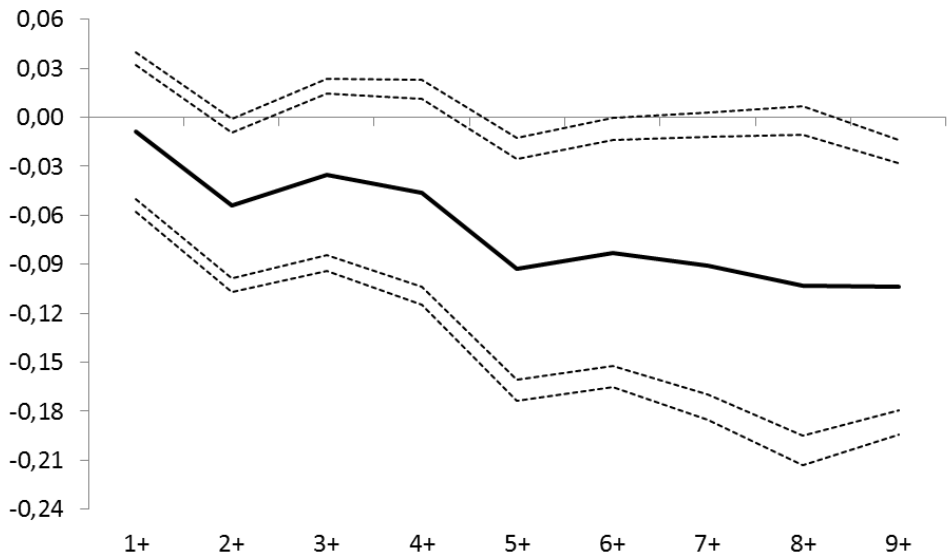
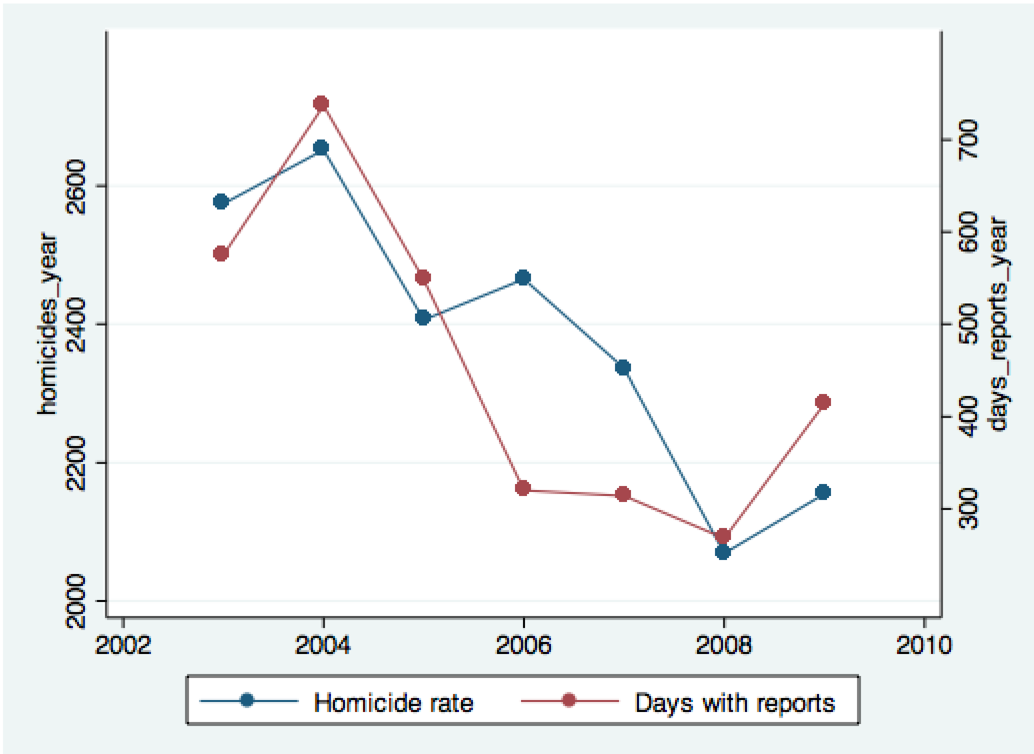
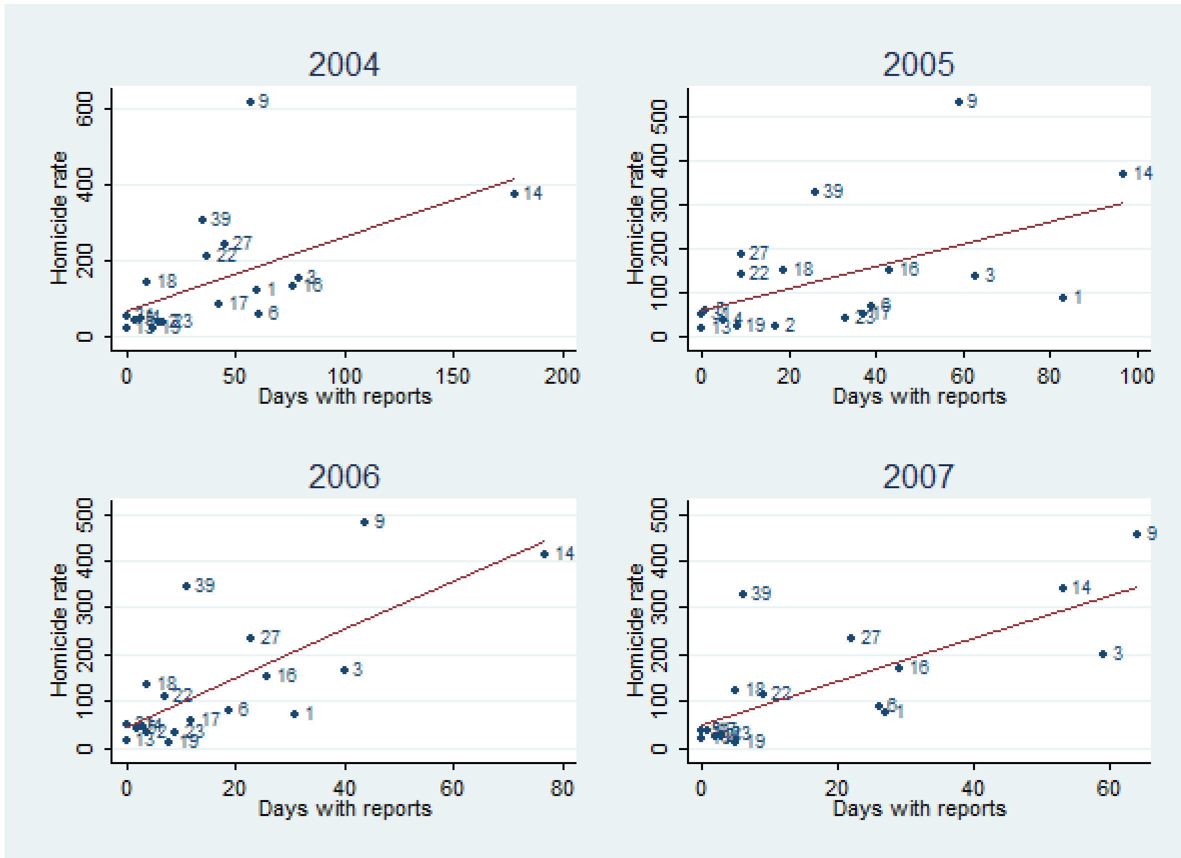


Figure 6: Homicides and Number of Days with Conflicts 2003-2009



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 7: 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 8: 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.