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Community Police Forces and Education: Evidence from Rio de Janeiro UPP Program.[1]

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Abstract

In December 2008, Rio de Janeiro state government inaugurated the first Unidade de Polícia Pacificadora (UPP) in the hill Santa Marta, a small slum (favela) located in a middle class neighborhood in Rio. Training focused on decrease brutality and relationship with communities was applied to police men allocated in these areas, and the number of police staff in these regions climbed. This paper uses a difference-in-difference approach to measure the effect of this intervention on educational outcomes. We find that UPPs improved math learning by 3%. The impact was verified only in 9th grade children, with no effects on 5th grade students learning. However, we did find an impact of UPPs on drop out rates of children from the first cycle of primary education (first to fifth grade).

Key words: UPP, Police, Education.
JEL codes: .

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1. Introduction

Drug illicit trafficking is a huge problem in many developing and developed countries. Several times, the trafficking is organized by violent crime organizations, bringing terrible consequences for the regions where the drug gangs are acting and practicing the illegal activities. Some of these consequences are homicides, fear of the population about retaliation against any denounce, anonymous or not, and battles for territory control, whether against the police or against other rival gangs.

These drug gangs battles have, in general, adverse externalities. The stressful and violent environment may causes worsening in school achievements and health problems. Monteiro and Rocha (2017) show that armed conflicts reduce student math results in standard test scores and that the mechanism is by increases in teacher absenteeism, principal turnover and temporary school closings. At the same time, territory dispute, may affect the access to public health and educational services. Finally, psychological literature has documented the effect of violence on mental health, which may ultimately affect the development of human capital.

The economics literature on police and crime shows that increase the presence of police on the streets substantially reduces crime (Di Tella and Schargrodsky, 2004; Draca et al., 2011). However, in many Latin American countries, the institutional capacity of conduce police interventions is much smaller and researchers have investigated the effects of hot spot policing on crime reduction and population satisfaction and has founded mix results (Blattman et al., 2017; Collazos et al., 2019).

This paper analyzes the effect of a police intervention based on the idea of the interactions between police and community on educational outcomes. The intervention took place in the favelas (slums) of Rio de Janeiro, which are locations with many peculiar characteristics. In these favelas, drug gangs dispute the domain of the territory, and use the region to sell drugs and hide themselves from the police. Many times, these territory disputes end up with an abrupt armed and violent conflict, altering the children school routine, impacting teaching absenteeism and disrupting psychological stress that could, ultimately, affect learning (Monteiro and Rocha, 2017).

In 2008, the Rio de Janeiro state government launched a program called Unidade de Polícia Pacificadora (UPP). The idea of the program was to create permanent police stations located inside the favelas of Rio de Janeiro with qualified personnel who received different salary bonuses and conditions in career. The police men and women allocated to work in these police stations made frequent police patrols in the region, bringing the idea of police occupation in substitution of drug gangs in the favelas, creating, in theory, some ties of coexistence between the police and the community. This kind of relationship between the police and the community represents a very huge change in the way the police acts in the favelas of Rio de Janeiro. Before UPPs, the police made special operations in favelas to fight against the drug gangs, but these operations were day operations, in which the police entered only to seize drugs and weapons, arrest drug dealers, and leave the favelas after operation has been finalized. With the installation of UPPs police station, the police stay in the favelas seven days in a week, doing police patrols and with an approximate coexistence with the local population.

Vaz (2014) measures the effects of UPPs on violence. He finds that the intervention reduces violent crimes and has a small but significant effect on robbery. He also finds an important result of a reduction in death resulting from resistance to police activity, which usually comes from battles between the police and the drug gangs every time the police entered in the favelas for a day operation, like the operations mentioned above.

In this paper we analyze if this reduction in crimes had externalities in educational outcomes. Specifically, we calculate the impact of UPPs intervention on school evasion, school passing rates, and student school achievements, measured by standardized test scores.

We use school panel data organized by the Ministry of Education covering the period from 2005 to 2015 to run a difference-in-difference model, exploring spatial localization and time of the intervention to compare schools affected by the policy with schools in similar locations not affected by the intervention (favelas without UPPs). Our results show that, for students from the 9th grade, UPPs has no impact on school evasion and passing rates, but increases student math score. The results show that students in schools located near to UPPs improve their math score 3% more (or 0.13 standard deviation) than students in schools located near to favelas without UPPs. At the same time, drop out rates in the first cycle of primary education (first to fifth grade) decrease 50% (or 0.26 standard deviation) with the intervention. No impact in student 5th grade performance in test scores was found.

This evidence contributes to the literature, showing that a police intervention in a developing country with high crime rates focusing policing on problematic places can, not only reduce crime, but also have positive externalities, improving investment in human capital and learning. The literature has advanced on the question of this type of program affecting the reduction of crime without negative spillovers in the surrounding areas (Braga et al., 2012; Blattman et al., 2017; Collazos et al., 2019). Our paper advance in this literature showing an indirect impact of this kind of intervention on education.

The characteristics of UPP intervention meet the aspects of these interventions in hot spots, but are also related to interventions based on community policing. Several papers have studied the effect of this kind of policy on reduce violence and the problems with their implementation. Maguire et al. (2019) show that community police intervention in Trinidad and Tobago increases safety perception by the population. Alves and Arias (2012) analyzes the Fica Vivo homicide control program in Belo Horizonte and encountered significant success in reducing homicides, as a result of its innovative two-tiered structure in which community-oriented policing units operated in conjunction with state-administered social programmes led by civil servants at each of the programme sites. Alves and Arias (2013) show trough a randomized control trial that increase proximity between police and community can helps to reduce violence. Colombia National police, in 2010, randomized the Plan Nacional de Vigilancia Comunitaria por Cuadrantes in eight major cities of the country. In some small cuadrantes they randomly select six police men to train and improve interpersonal skills and establishing new patrolling protocols with more community contact. The overall reduction of crime in these areas was of about 20%. We contribute to this literature showing that community policing has positive externalities on education and learning.

Finally, this paper contribute to the literature that makes an effort to calculate the impact of violence on education. Many authors use different contexts to calculate this relationship, such as violence within schools, domestic violence and other socioeconomic disadvantages related to violence and their effect on education (Grogger, 1997; Aizer, 2008; Severnini and Firpo, 2009). Using the same context as us, Monteiro and Rocha (2017) uses variation in the exposure of violence in favelas of Rio de Janeiro to measure the impact of violence on education. We add a piece in this literature showing that an intervention to reduce the type of violence coming from drug gangs can also improve educational achievement.

There are some working papers analyzing the effect of UPPs on educational outcomes. However, these working papers, either explore outcomes different from ours (Conceição, 2017), or make methodological choices, such as choosing specific years sample cuts and specific UPPs, and coming to results very different from the results presented here (Butelli, 2012; Teixeira, 2017). We use in the sample all UPPs created since 2008 and all years available to the analysis, having, therefore, a more reliable result about the average effect of UPPs on educational outcomes than the previous studies.

The rest of this paper is divided in five more sections. In Section 2 we describe the UPP intervention. In Section 3 we describe the data used. In Section 4 we explain the identification Strategy. In Section 5 we present the results and in Section 6 we conclude the paper

2. Institutional Context

In 2008, the city of Rio de Janeiro recorded a homicide rate of 46.2 deaths per 100,000 inhabitants, at a time when police incursions in the hills of Rio de Janeiro, dominated by drug traffickers, generated countless shootings, fear for the population in general and fatal victims. In this scenario, social pressure for innovations to improve security conditions in the state was very high, even more because the government elected in the previous year promised a new cycle of local development, in which the World Cup in 2014 and the Olympics in 2016, to be held in Rio de Janeiro, would be a milestone. In this context, in December 2008, was inaugurated the first UPP in the hill Santa Marta, a small favela located in Botafogo, a middle class neighborhood in Rio.

The UPP program was implemented in an experimental and heuristic way, in which the first institutional document of the government that established doctrinally what would be the UPP was a law decree[2] promulgated by the governor only in 2011, or more than two years after the program began.

Basically, this law decree said that the UPP was a "Proximity Police", in which the central objectives consisted in: a) consolidating state control in communities with a strong influence of ostensibly armed crime; and (b) to restore to the local population the public peace and tranquillity necessary for the exercise of full citizenship that guarantees both social and economic development.

To achieve the objectives, as defined in the decree, the pacification program had four stages, which began with the "Tactical Intervention", when there was a resumption of the territory by a large contingent of police officers, usually belonging to groups of operations special. Then came the stage of "stabilization," which included tactical actions and siege of criminals to prepare the ground for deployment. The "deployment of the UPP" occurred then, when specifically designated and trained officers for the function occupy the site. There would still be a stage of "evaluation and monitoring", which never occurred.

As there was a perception by the high command of public security that the designation of former police officers in the corporation for the UPPs would cause serious problems with the communities, since they would be addicted to a culture of confrontation and brutality, a series of public competitions and short training cycles for the hiring and mobilization of new police officers was put into practice. Considering that the police density in the territories of the UPPs was almost nine times higher than in the rest of the city[3], a strong hiring and training work was required, which meant that, in August 2017, 20.9% of the of the state military police were crowded into the UPPs[4].

[2] Law Decree n. 42,787 of January 6, 2011.

[3] According to Cano et al. (2012, p.21): "for the state of Rio de Janeiro as a whole the ratio is 2.3 Police men per 1,000 inhabitants, [whereas] for the set of these 13 first UPPs the level is 18.2 MPs per 1,000 inhabitants. "

[4] While the number of police men working at Military Police in the state of Rio de Jnaeiro was 45,463, there was 9,543 allocated in the UPPs.

This was a notable change in the pattern of policing in Rio de Janeiro, a city in which, since the 1980s, the objective of public security actions was to combat drug trafficking in actions to combat crime, marked by invasions of territories, shootings and the use of police violence in its highest degree, which helped to boost violence in the state.

In fact, the UPP program has reduced recurrent community shootings, as well as homicides caused by the police, creating a more secure environment, which was positively received by the majority of residents of the communities and by the entire population of Rio de Janeiro, according to several surveys of opinion recorded [see Cano et al. , 2012].

3. Data

In this section, we explain the data used in this paper. We divide the section in two parts. In the first part, we explain the data coming from the Ministry of Education. In the second part, we explain the geographic data used in the paper.

3.1. Educational data

We use data from the Ministry of education, available in the National Institute of Educational Research (INEP). They have two types of data bases. The first database used in this paper is the Prova Brasil, which is a standardized test applied to primary education students of all public schools with more than 50 students enrolled. The exam is applied to students at 5th grade and 9th grade and measures the knowledge in mathematics and language. It is also applied a socio-economic questionnaire, where the students answer about not only their socio-economic conditions but also about some school activities.

The Prova Brasil started in 2005 and happens every two years. Then, we have data from 2005 to 2015 in alternate years. We use the test scores in math and language as our main dependent variables. The 2005 database in language was missing, then, for language test scores we use data from 2007 to 2015. We also use the characteristics of the students coming from the socio-economic questionnaire as control variables. We choose the control variables so that we do not lose too many observations because of missing data.

The Ministry of education provides data on educational indicators per school which come from the School Census, such as passing rate, drop-out rate. The is from 2007 to 2015 and we have data from every year. Finally, we use School Census from 2005 to 2015 to get school characteristics and construct some control variables.

3.2. Geographical Data

The municipal secretariat of education of Rio de Janeiro provides the shape-files with the location of all schools they manage. The same thing is done by the state secretary of education of Rio de Janeiro. In Brazil, usually, the municipality is responsible for manage primary education schools and the states are responsible for high schools. However, some states have also primary education schools, which is the case of Rio de Janeiro. We use, then, in our analysis, municipal and state schools located in the Rio de Janeiro municipality that have primary education.

The Municipal Institute of Urbanism (Instituto Pereira Passos, IPP), provides the geographical polygons with the area covered by each favela of Rio de Janeiro. We cross this data with the data from the Institute of Public Safety (ISP) of Rio de Janeiro, which provides the shape-files with the polygons covered by the UPPs. We use the crossed data to define our sample. We keep in the sample only schools that are within 100 meters from any favela with or without UPP.

Then, for the sample in Prova Brasil we stay with 85 schools with 9th grade test score information and 210 schools with 5th grade test score information. For the sample of Census data, where we measure the effect on drop out and passing rates, we stay with 254 schools with information about first cycle of primary education (first to fifth grades) and 162 schools with information about second cycle of primary education (6th to 9th grades).

We, then, use the distance to UPPs to define our treatment group, which in the main strategy is composed by schools within 100 meters from any UPP area. We provide a better discussion on that later in the Section 4.

3.3. descriptive statistics

In this section we present some descriptive statistics for the schools used in our benchmark sample and treatment definition, which means, schools within 100 meters from some favela are in the sample and schools within 100 meters from UPPs are considered treated schools.

First, we grouped the schools in two groups for this section. The first group are represented by the schools never been a treated school in all sample years. The second group is the group of schools that was treated in some year of the sample. As only one of the thirty eight UPPs was created in 2008, we consider 2009 as an year before the intervention for these descriptive statistics.

Figure 1 shows the evolution of students 9th grade math test scores in treated and control schools. It is possible to see that from 2007 to 2009 the trends were very similar, with control group worsening a little more than treated group from 2005 to 2007 and improving a little more than treated group from 2007 to 2009. However, the improvement of treated group in 2013 and 2015 is remarkable higher than the improvement in control group.

Figure 2 shows the evolution of students 5th grade math test scores in treated and control schools. Again, it is possible to see that from 2005 to 2009 the trends were very similar. However, after treatment, there is a small decrease in treated schools performance. We will see in Section 5 that this decreased is non-significant. The small decrease can also be explained by the fact that, as will be shown in Section 5, the drop out rates decreased in first cycle of primary education (1rst to 5th grades) in treated schools keeping at the schools worse students until the year they do the Prova Brasil. We can see this in Figure 3. This pattern was not observed by drop out rates in the second cycle of primary education (6th to 9th grades), in which we see no different evolution in drop out between treatment and control groups, as shows Figure 4.

Finally, we present in Table 1 and Table 2 the descriptive statistics of the data in Prova Brasil and in School Census, respectively. This descriptive statistics will be important to interpret the coefficients found in the regressions.

4. Empirical Strategy

In this section, we explain the strategy used to identify the causal effect of the UPP intervention on educational outcomes. We divide the section in two parts. First, we explain how we measure the effect of the intervention on standardized test scores. In this part, we also explain the robustness checks. In the second part of the section, we explain the regressions made to calculate the impact of UPPs on drop out rates and schools passing rates.

4.1. The Impact on Test Scores

In this subsection, we explain how we calculate the impact of the UPPs on math and language test scores. We use the difference-in-difference approach exploring the timing and the spatial relationship between the UPPs and the schools of the city of Rio de Janeiro.

In our main identification strategy we keep in the sample only schools within 100 meters from any favela with or without UPP. To define the treatment group, we then choose the schools that were within 100 meters from some area covered by UPPs. We exploring the timing each UPP were implemented to have cross sectional and time data variation. As explained in Section 2, to create the UPP, the state police first invade the favela, stay some time with the help of special operation forces, and only after that, they launch the UPP. However, as the police stay in the place since the date of invasion, we use this date as the time that region started to be considered a treatment region. Finally, because of administrative criterion, some schools do not participate of every Prova Brasil Edition. Following Monteiro and Rocha (2017), we keep in the sample only schools that participate in Prova Brasil at least two years in the sample period.

So, the main idea is to compare schools near to favelas without UPPs with schools near to favelas with UPPs and the evolution of the educational outcomes after each intervention (as we have 38 UPPs, each been created in different dates). However, the choice of where to allocate the UPPs itself could be endogenous, as the government chose problematic places with drug gangs to implement the program. This problem is eased by three facts. We have variation in the time each UPP was created. There are some violent favelas also with drug gangs that were not invaded. And there are many favelas with milícias, which is composed by corrupt police men, retired and active that control some favelas in the same way as the drug gangs, exploring some commercial services in the region using the violence, such as gas for cooking, illegal cable television connection, etc. The choice of the state government was invade only the favelas with the drug gangs and lay aside the favelas with milícias. Then, the other favelas of Rio de Janeiro is problably a better control group than would be if their had invaded all violent favelas.

Still, it is possible that some unobservable socio-economic characteristics be different in control and treatment groups. Many of these non-observables are, however, fixed over time. To minimize this problem, we control for school fixed effects. But the school characteristics and students distribution between schools can also change over time and is probably related to performance. Fortunately, as explained in Section 3, we have the students socio-economic questionnaire and use their answer to control for socio-economic characteristics. We also have information about the schools, such as if their have computer room, science room, library, etc. We use this information to control for possible interventions in the schools that are not because of the UPPs, although could be correlated with it.

Then, we have a panel of schools and information on test scores of the students of theses schools, although we have not the student many times in the sample, only the schools. We run the following difference-in-difference model, then:

$$Y_{ist} = \alpha_s + \phi_t + \beta_1 UPP_{st} + \sum_{k=1}^{K} \lambda_k X_{kst} + \sum_{k=1}^{K} \gamma_k Z_{kist} + \epsilon_{it} \tag{1}$$

where Y_{ist} is the test score of student i, from school s in time t, α_s is a school fixed effect, and ϕ_t is a time fixed effect. UPP_{st} assumes one if the school s was within 100 meters from an UPP in time t and zero oerwhise. So β_1 represents the average treatment effect on the treated. X_{kst} is a set of school timing-vary variables, Z_{kist} is a set of students timing-vary variables, and ϵ_{it} is the error term. To have standard errors robust to serial auto-correlation and heteroskedasticity, we cluster standard errors at school levels.

In order to verify if all the effect is coming from the arbitrary decision of use the 100 meters distance as our definition of treatment we run the same model varying the definition of treatment. We continue to keeping only schools within 100 meters of any favela, but now we vary the definition of treatment to 50, 200, and 500 meters of distance from an UPP. We expect that the effect decrease with the distance.

To allow the effect to be flexible in time of exposure to treatment, we also run a regression in which we build a dummy for every year after UPP has been implemented, using the definition that a school is a treatment school if it is within 100 meters of distance from an UPP. The model used is:

$$Y_{ist} = \alpha_s + \phi_t + \sum_{k=0}^{K} \beta_k UPP_{s,t-k} + \sum_{k=1}^{K} \lambda_k X_{kst} + \sum_{k=1}^{K} \gamma_k Z_{kist} + \epsilon_{it} \tag{2}$$

where $UPP_{s,t-k}$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in $t-k$ periods. These dummies measure the effect of the time of intervention on educational outcomes. For example, $UPP_{s,t}$ is a dummy that represents if the school is in a region in which UPP has been implemented in year t. $UPP_{s,t-1}$ is a dummy that represents if the school is in a region in which UPP has been implemented in year $t-1$, and so on. The other variables are the same as in Equation 1.

The condition that our model has to satisfy to be capturing the causal relation between UPPs and educational outcomes is the traditional condition in difference-in-difference models. That means, UPPs are not related to timing-vary non-observables. To increase confidence that this holds, the very first exercise we have to do is check for parallel trends. It is possible that schools where UPPs were implemented had different trends compared to schools where they were not. To test for this problem of parallel trends, we run a flexible model, where we build a dummy for every year before and after UPP is implemented. The model is:

$$Y_{ist} = \alpha_s + \phi_t + \sum_{k=2}^{K} \omega_k UPP_{s,t+k} + \sum_{k=0}^{K} \beta_k UPP_{s,t-k} + \sum_{k=1}^{K} \lambda_k X_{kst} + \sum_{k=1}^{K} \gamma_k Z_{kist} + \epsilon_{it} \tag{3}$$

where $UPP_{s,t+k}$ represent a set of dummies which are one if the school s will be in a place within 100 meters from an UPP in $t+k$ periods. Notice that these are placebo tests. The coefficients of these variables should be zero if the trends are the same in treatment and control groups. In other words, that school, which will be near to an UPP in, for example, $t+2$ periods should not be affected by UPPs yet, and consequently, should not have different test score means compared to control group. In the other hand, $UPP_{s,t-k}$ is as in Equation 2. Noticed that we keep the year immediately before UPP has been implemented as our comparison year and no dummy for this year is created.

Another possible problem is that schools with low average educational outcomes could be the ones that naturally improve more their performance. We saw in the descriptive statistics that, in fact, the schools located at localities that will be treatment in some year of the sample had low average math test scores. Then, it is possible that we are confounding a convergence process which would happening even without the intervention with the effect of the program. Then, to control for this possibility, we run the model 2 again, but now controlling for a linear time trend multiplied by the average test scores of that school in Prova Basil 2005 and 2007 (the two years with Prova Brasil before the first UPP be implemented). Than, if schools near to UPPs had already a natural tendency to improve their test scores only because they had lower performance at the baseline, the coefficient of UPP should not be significant any more, as this effect will be captured by the trend. We do the same in the case of drop out rates. In this case, we multiply the linear time trend by the average educational outcome of that school from 2005 to 2007, including the year 2006 (which were not possible in the case of Prova Brasil, because Prova Brasil is every two years).

In summary, our goal is to prove that the trends of both group of schools, the ones that will be treatment at some point in the sample and the ones that will be not are the same. If this is true, the fact that they are slightly different in levels before 2008 will be not a problem, specially because we control for school fixed effects and for time-varying observables.

4.2. Other Educational Outcomes

In this section we describe the empirical strategy used for measure the impact of UPPs on other educational outcomes, specifically drop out rates and passing rates. For this part, we use the same strategy as in Equation 1, changing only the dependent variables. Another difference is the sample used. For this part of the work, we have annual information about the schools. So, our predictive power increase and we can get the average effect for the whole system.

There is still the problem that some schools can close and open and do not appear every year in the sample. We keep only schools that are in the sample at least two years in the studied period.

5. Results

In this section we describe the main results found in the paper, and the robustness checks made. We begin describing the results on 9th grade test scores. After that, we describe the effects on 5th grade test scores, the robustness checks and the results on other educational outcomes.

5.1. Impact of UPP on Test Scores

Table 3 presents the results for the impact of UPPs on 9th grade students math and language test scores. Column 1 shows the impact on math test score using our benchmark specification in which the schools are considered treated if are within 100 meters from an UPP. The result shows that students in treated schools increase in average, 6.32 points in the test score (the average was 240), which means an increase of 3% in the test score. They are statistically significant at 5% levels. In other words, it also means an increase of 0.13 standard deviation in math test score.

Columns 2, 3, and 4, repeat the exercise with math scores varying the distnce from UPPs in the definition of treatment. In Column 2, we consider only schools within 50 meters from an UPP, as a treated school. In Column 3, we increase the minimum distance to 200 meters and in Column 4, to 500 meters. The results show that the coefficient continues to be positive, but, as expected, decrease with the distance and becomes non-significant since 200 meters.

Columns 5 to 8 repeat the same exercises using language test scores as dependent variable. Coefficient is still positive, but, in this case, statistically non-significant. Smaller effects on language are expected if the UPP has had a greater impact on schools environment than in family environment. Interventions at the school, usually, have greater impact on math learning than on language skills.

Table 4 presents the results of the impact of UPPs on 5th grade students math and language test scores. The order of the columns is the same as in the previous table. Firsts four columns present the effect on math score and columns 5-8 present the effect on language score. In the case of 5th grade test scores all the coefficients are statistically non-significant and very close to zero.

In general, the results point to the fact that UPPs had a higher effect on learning in older children than in younger children. It seems that the type of problems the drug gangs have caused in the favelas of Rio is affecting more the learning in older ages, where children start to have more acquaintanceship to people related to these gangs.

5.2. timing

In previous tables we considered the average effect of UPPs on learning. However, it is possible that the effect is becoming higher with the time of intervention. As we mentioned before, the UPPs is implemented in many stages and the effect could last some time until be felt. Then, it is possible that many of the UPPs effects is only observed many years after the UPP has been implemented.

To verify the heterogeneity of the effect through time, we rerun the previous models, but now allowing the effect to be different through time, using a model represented by the Equation 2. In this case, we use our benchmark definition of treatment (within 100 meters from an UPP).

Table 5 presents the results. Column 1 shows the impact through time on math test scores for students in 9th grade. Column 2 does the same, but now showing the impact on language test scores. Columns 3 and 4 repeat theses exercises for students in 5th grade. The results show again that the effect of UPPs is concentrated on 9th grade students. No effect was found on 5th grade students test scores. However, the effect on 9th grade test scores now appears also on language. It is possible to see that the effect is increasing with time, but not for every year. That happens because Prova Brasil is not every year, but every two years. Even having upp entering every year since 2008, some UPPs are bigger than others and include more schools. Then, for example, for the dummy UPP in t-2 if only less important UPPs was implemented two years before an year with Prova Brasil, then, the variance of the estimates will be very high for this dummy and then the coefficient is less reliable. Even so, it is possible to see that, for math test scores, the dummies for UPP in t-1, UPP in t-3, UPP in t-5, and UPP in t-6 have the coefficient positive and statistically significant. Also, for these dummies that are statistically significant, the effect is increasing with time.

That means that the intervention has scale gain with time. Communities often take time to adapt to the presence of police officers in close contact with them, which may explain theses results. In addition, the intervention itself is improving over time, learning from mistakes and making adjustments that make the community environment more stable.

5.3. robustness on test scores

Having measured the effect of UPPs on test scores we need to confirm that our estimates are getting the causal relationship between them. In Section 3.3 we showed that the treated schools were slightly different in level from the control schools. This is not necessarily a problem as we are controlling for school fixed effects. But we have to check if both group have parallel trends.

We do, therefore, two exercises to guarantee that we are not confound the effect of trends with the effect of the program. First, we run the model presented in Equation 3 to check if the non-linear trends in both control and treatment group are parallels. The idea is that if the trends are similar, all dummies UPP_{t+k} have to be non-significant.

Second, we run an exercise in which we control for an interaction between a linear trend and the school educational outcome before 2008 (year of the implementation of the first UPP). It is possible that learning at worse schools had a natural tendency to converge to learning at better schools, and we can being confounding the effect of the program with natural process. Controlling for this interaction between a linear trend and the outcome used in dependent variable in the baseline sample, we are controlling for a time trend that depend on the initial levels of this outcome. Therefore, if the worse schools are converging to better schools, this natural process will not be confounded with UPPs effect, as will be captured by this trend. In these two exercises we use only the test scores of 9th grade, the grade we found some effect of UPPs. We also use our benchmark definition of treated school (within 100 meters from an UPP).

Table 6 presents the results for this two exercises. Columns 1 and 2 test for parallel trends, the exercise using Equation 3. Column 1 test for parallel trends in math test score regression and Column 2 in language test score regression. In Columns 3 and 4, we add the trend depending on baseline educational outcomes. Column 3 does this for math regression and Column 4 for language regression. Results show that the effect of UPPs on math and language are robust to these exercises. In columns 1 and 2, all the dummies UPP_{t+k} are statistically non-significant to explain math learning and language learning. And the dummies UPP_{t-k} continue to be positive and significant to explain learning. In columns 3 and 4, in which we control for the trend depending on initial levels of the educational outcome, we see that the effect of the program is robust to this inclusion.

5.4. The Effect of UPPs on Drop out and Passing Rates

Now that we measured the effect of UPPs on test scores, we can calculate the effect of the intervention on drop out and passing rates. In this part, we use only the benchmark definition of treated schools and the flexible model presented in Equation 2. Table 7 presents the results.

In columns 1 and 2, we test the effect of the intervention on drop out rates in the first cycle of primary education (1rst to 5th grades) and in the second cycle of primary education (6th to 9th grades), respectively. In columns 3 and 4 we do the same for passing rates. The results show that the UPP intervention has no effect on passing rates and has an effect on drop out in the first cycle of primary education after 3 years of intervention. Considering the average of the coefficients that was statistically significant we have that schools near to an UPP decrease drop out rates in first cycle of primary education by 0.9 percentage points. As the average is 1.8%, this means a decrease of 50% in the drop out rate. This also means a decrease of 0.26 standard deviations.

The curious fact in these results is that, contrary to the case of test scores, in the case of drop out rates, the intervention had effect only on younger children. As drop out at that age is really a phenomenon more related with their parents life, it seems that UPPs have some effect on the stability of the region and parents drop their children from the school less.

This effect on drop out rates of first cycle of primary education can also explain the fact that we did not find effect on 5th grade test scores. Usually, the worst students are the ones that drop out more. As the schools near to the UPPs reduce their drop out rates, worse students arrive at the end of the first cycle and was able to do the Prova Brasil test, thus pushing the average in this standardized test down.

5.5. Robustness on Drop Out

In this subsection we do the same robustness exercises as in Section 5.3, now with the drop out rates as our dependent variable. First, in the placebo exercise, we add dummies UPP_{t+k} in the model and verify if the trends in treatment and control groups were the same before the UPP implementation. Second, we interact the average school drop out rate from 2005 to 2007 with a linear time trend to avoid confound the effect of the program with a natural convergence process.

The results are presented in Table 8. In column 1 we show the placebo exercise and in Column 2 we control for the trend depending on baseline drop out. The results of Column 1 show that all dummies UPP_{t+k} are statistically non-significant to explain drop out, which means that the trends before the treatment were the same, while the coefficients on the dummies UPP_{t-k} show that three years after UPP has been implemented drop out rate decrease more in schools near to a UPP than in control group, and this effect is statistically significant.

Column 2 show that, even controlling for a trend depending on the baseline average school drop out, UPPs continue to be relevant to explain drop out reduction, meaning that we are getting not only the natural process of convergence between worse and better schools, but the program effect on drop out.

Summarizing, it seems that UPPs have a small impact on 9th grade students learning, who increase their test score means in 3%, or 0,13 standard deviation. This effect on learning, was not found in the 5th grade students, pointing for the fact that the learning process of older students have more to do with the type of intervention UPP brought. Peer effect, for example, could have more influence on this age.

However, another possible explanation for the absence of effect on learning to younger children is that results show a decrease in drop out for children at the first cycle of primary education (younger children). This drop out reduction could allow the worst students to finalize the cycle and, then, do Prova Brasil, pushing the average of that schools down in this test score.

6. Final Remarks

We investigate the effect of a community police intervention in Rio de Janeiro on educational outcomes. The intervention, called Unidade de Polícia Pacificadora (UPP), increase the police personal in some specific areas in Rio de Janeiro and gave training to these police men improve their relationship with the community, reducing brutality. These areas were slums (favelas) with high presence of drug gangs. Vaz (2014) shows that this intervention had an effect on violence reduction in these areas. Monteiro and Rocha (2017) show that violence reductions caused educational improvements. We test if the violence reductions caused by this community intervention, the UPPs, were enough to improve educational outcomes.

Our results show that UPPs increased average math test scores in 9th grade by 3%, or 0.13 standard deviation. We find however no effect on 5th grade students test scores. We interpret these results as a sign that older children learning is more affect for this type of intervention through, for example, peer effect.

In the other hand, we find an effect on drop out reduction of 50%, or 0.26 standard deviation for children from first grade to fifth grade, which means that the worst students are reaching the year in the school in which they have to do Prova Brasil, and then could be pushing scores in theses schools down.

Further research is necessary to understand which mechanism is more intense to explain the quantitative results we found in this paper.

At the same time, we also show that for some outcomes, the effect of UPPs comes only after some time of UPPs implementation. Also, even for the outcomes in which the effect was felt immediately, this effect increases with time, which makes clear the importance of the program continuation to improve increasingly the educational outcomes.

With the decadence of the program in Rio de Janeiro, it is possible that the educational gains seen in schools near to UPPs would be lost in the next rounds of Prova Brasil, which will be also subject of our research when the data is available.

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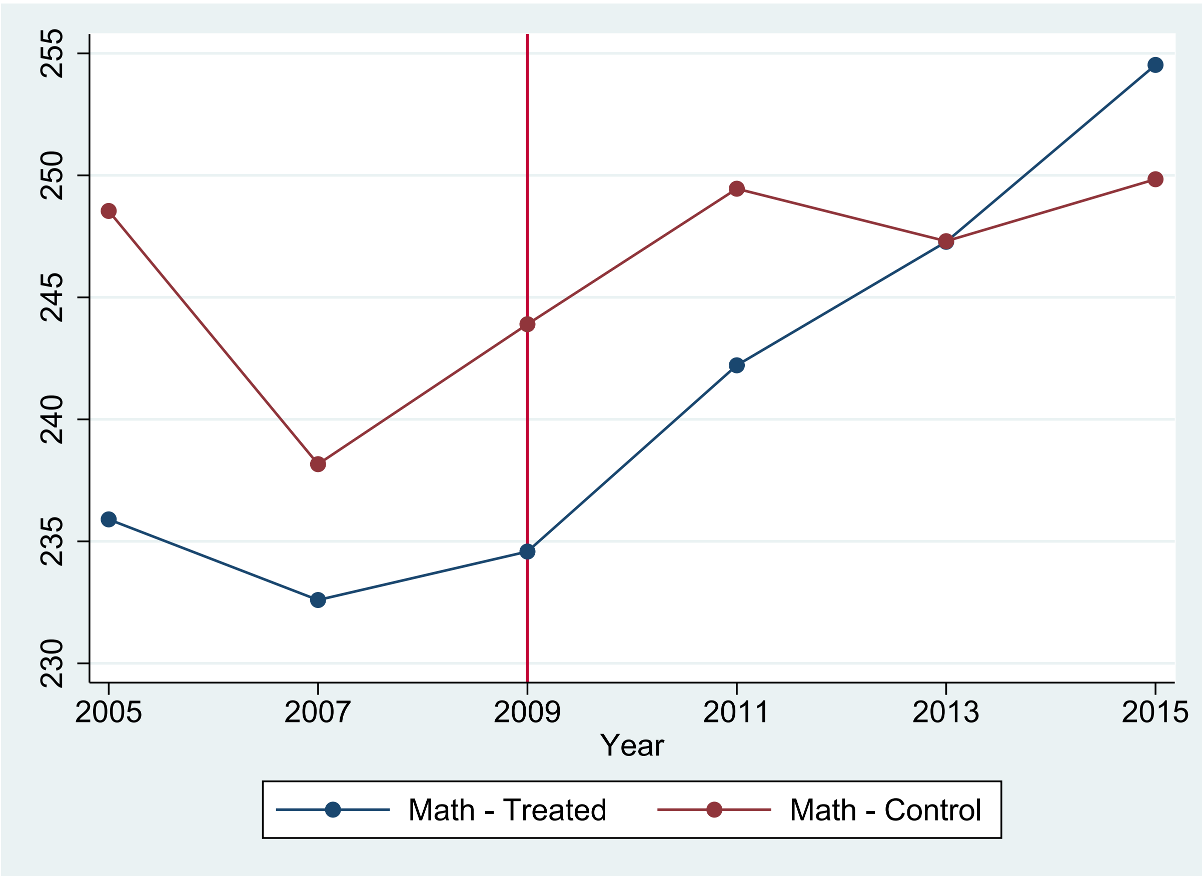
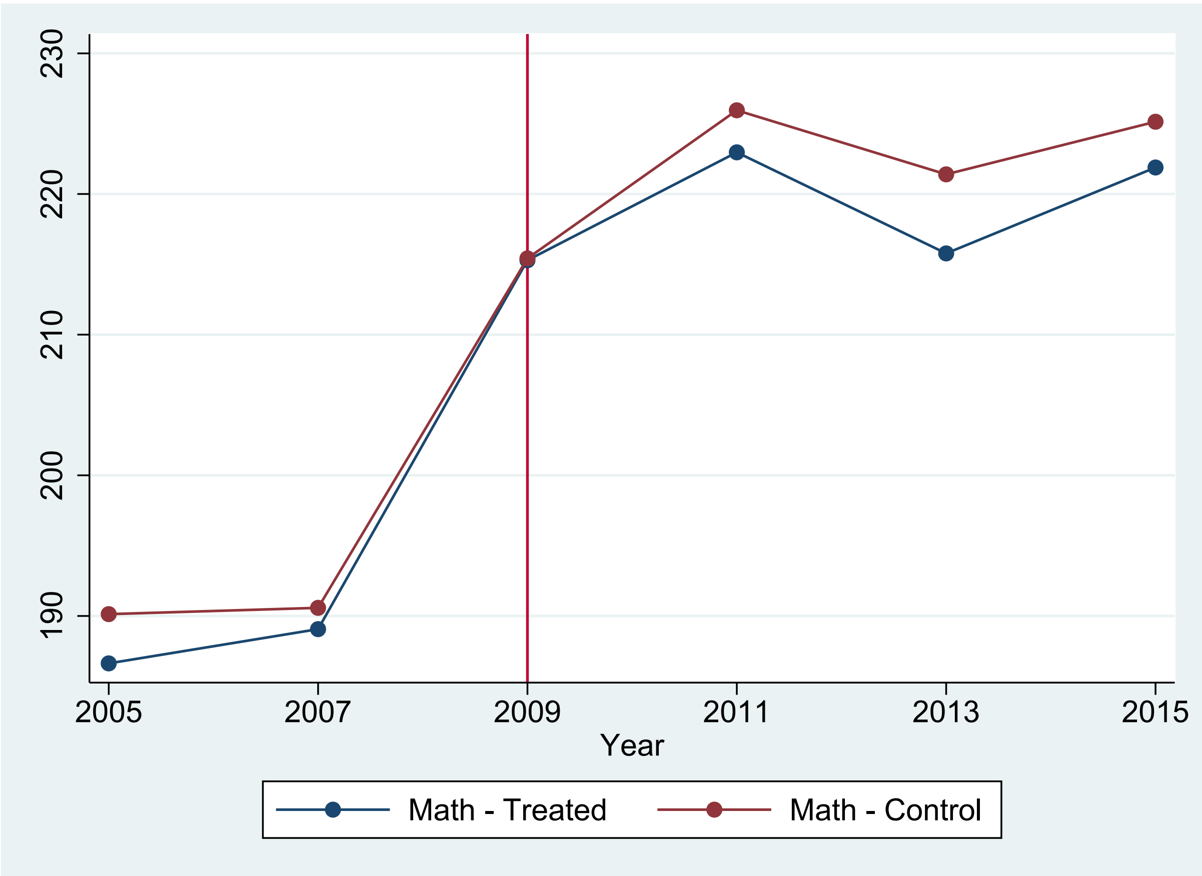
Figure 1: Math Average Test Score, 2005-2015, 9th grade



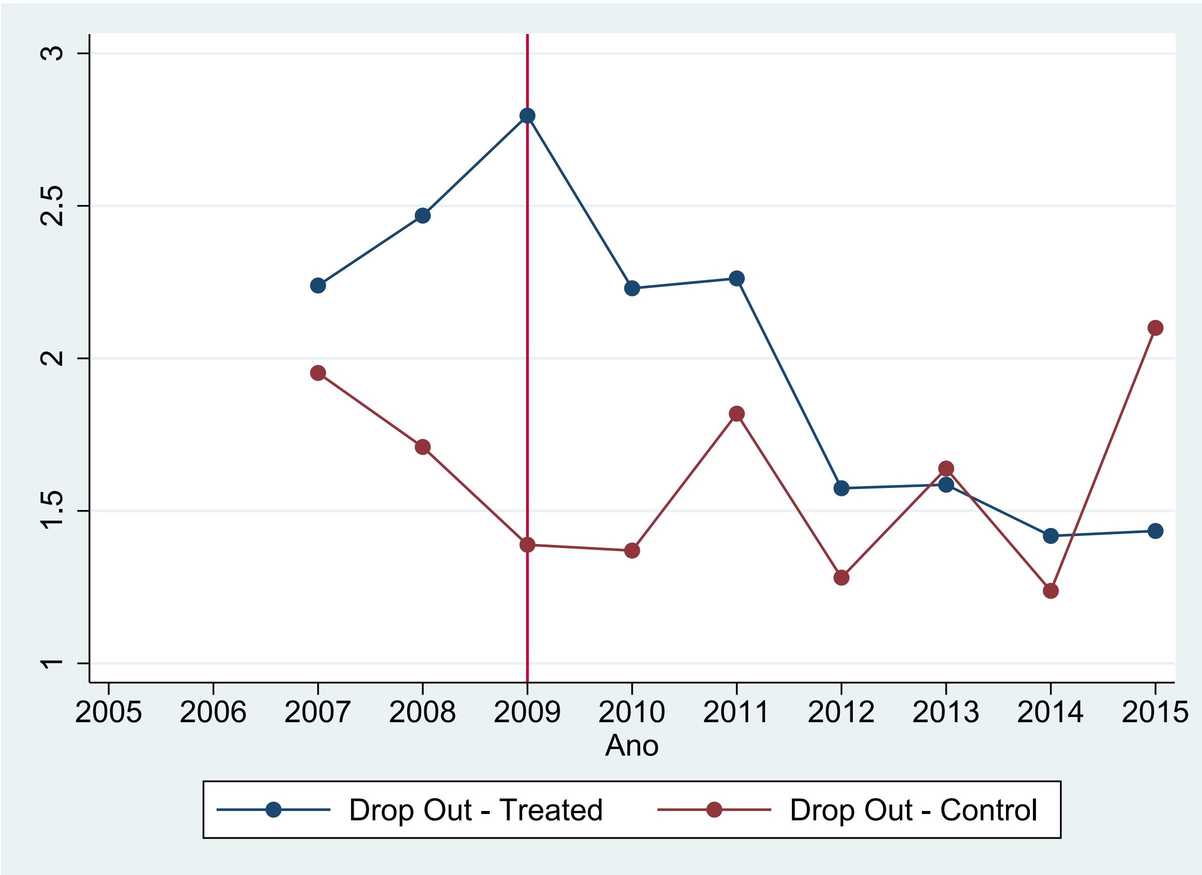
Notes: Figure shows 9th grade students average math test score from 2005 through 2015 in schools that are within 100 meters of some favela. Treated schools are those that in some time of the sample will be within 100 meters from an UPP. As only one UPP was created in 2008, for this graph, we consider 2009 before the intervention.

Figure 2: Math Average Test Score, 2005-2015, 5th grade



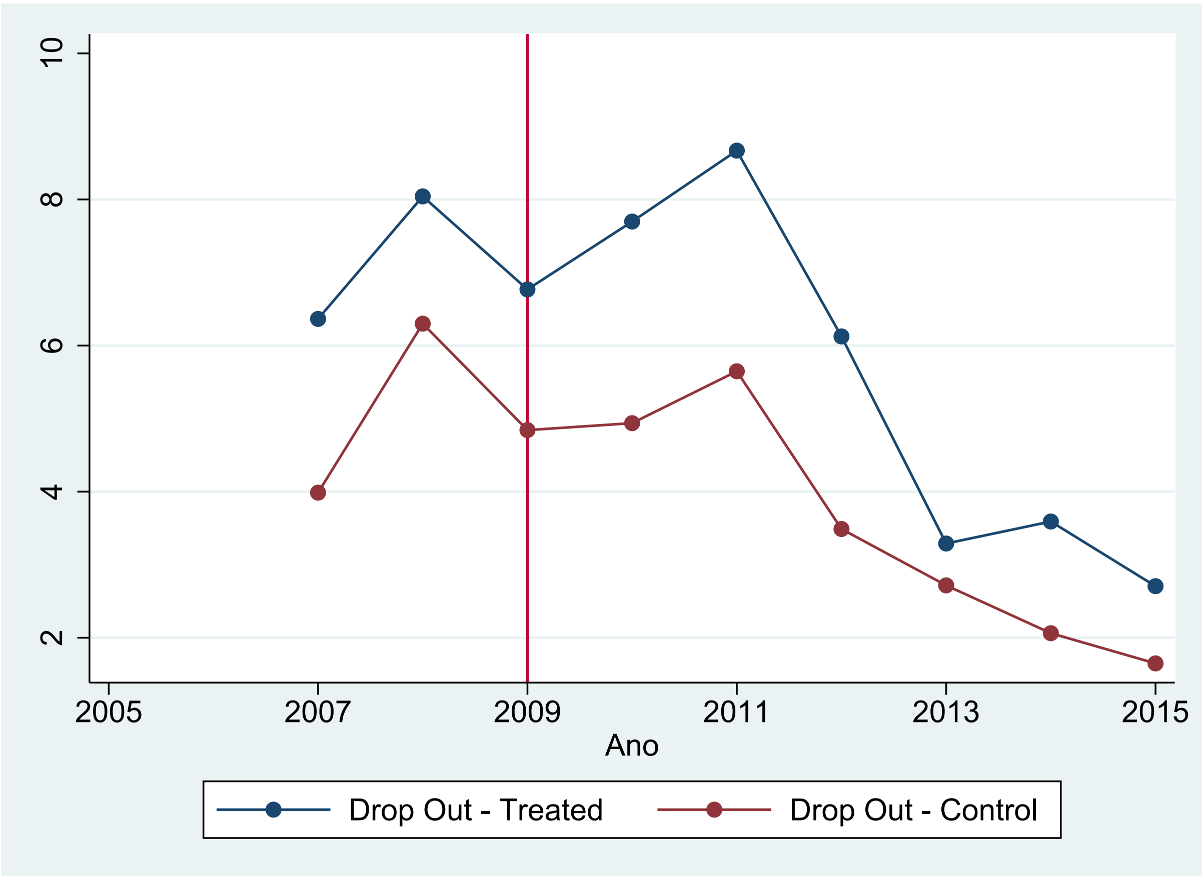
Notes: Figure shows 5th grade students average math test score from 2005 through 2015 in schools that are within 100 meters of some favela. Treated schools are those that in some time of the sample will be within 100 meters from an UPP. As only one UPP was created in 2008, for this graph, we consider 2009 before the intervention.

Figure 3: Drop Out Rate, 2007-2015, Firt Cycle



Notes: Figure shows Drop Out rates at the first cycle of primary education from 2007 through 2015 in schools that are within 100 meters of some favela. Treated schools are those that in some time of the sample will be within 100 meters from an UPP. As only one UPP was created in 2008, for this graph, we consider 2009 before the intervention.

Figure 4: Drop Out Rate, 2007-2015, Second Cycle



Notes: Figure shows Drop Out rates at the second cycle of primary education from 2007 through 2015 in schools that are within 100 meters of some favela. Treated schools are those that in some time of the sample will be within 100 meters from an UPP. As only one UPP was created in 2008, for this graph, we consider 2009 before the intervention.

Table 1: Descriptive Statistics - Prova Brasil, 9th grade and 5th grade, 2005-2015

	2005	2007	2009	2011	2013	2015	Total
9th Grade Students							
Math Score	244.5723	236.3848	241.0424	246.9100	247.2943	251.5759	244.8633
	(43.7829)	(42.4133)	(43.6921)	(47.0505)	(45.8811)	(42.1568)	(44.5408)
Language Score	.	230.5082	241.1178	242.1773	244.1252	247.0543	241.3910
	(.)	(43.5929)	(45.3810)	(47.6895)	(48.7276)	(48.2286)	(47.1508)
Men	0.5036	0.4466	0.4783	0.4520	0.4592	0.4545	0.4646
	(0.5001)	(0.4972)	(0.4996)	(0.4977)	(0.4984)	(0.4980)	(0.4988)
Has a TV	0.9827	0.9933	0.9899	0.9924	0.9818	0.9799	0.9867
	(0.1305)	(0.0815)	(0.1001)	(0.0866)	(0.1337)	(0.1403)	(0.1143)
Has a Car	0.1824	0.2738	0.2643	0.2862	0.3190	0.3445	0.2829
	(0.3862)	(0.4460)	(0.4410)	(0.4520)	(0.4661)	(0.4752)	(0.4504)
Has a Computer	0.3489	0.5130	0.6647	0.7860	0.8520	0.7701	0.6758
	(0.4767)	(0.4999)	(0.4721)	(0.4101)	(0.3551)	(0.4208)	(0.4681)
Works	0.1534	0.1325	0.1520	0.1165	0.1279	0.1123	0.1316
	(0.3604)	(0.3391)	(0.3591)	(0.3209)	(0.3340)	(0.3158)	(0.3380)
Age	15.3210	15.2614	13.4442	16.1365	15.0961	15.0922	15.0258
	(1.0889)	(1.0895)	(0.8673)	(0.8464)	(0.7636)	(0.6999)	(1.2307)
Never Failed	0.6645	0.6453	0.6727	0.7069	0.7289	0.7472	0.6970
	(0.4722)	(0.4785)	(0.4693)	(0.4552)	(0.4446)	(0.4347)	(0.4596)
5th Grade							
Math Score	188.7417	189.9669	215.3725	224.7759	219.0650	223.7676	210.9460
	(37.2568)	(41.8807)	(42.5921)	(42.8842)	(46.3824)	(42.2947)	(45.0776)
Language Score	.	173.4111	193.6698	199.3324	201.9762	209.3624	194.8996
	(.)	(40.9005)	(42.4766)	(43.0651)	(46.5489)	(43.5152)	(45.0683)
Men	0.5137	0.5030	0.4851	0.4665	0.4923	0.4912	0.4910
	(0.4998)	(0.5000)	(0.4998)	(0.4989)	(0.5000)	(0.4999)	(0.4999)
Has a TV	0.9629	0.9724	0.9706	0.9678	0.9449	0.9397	0.9599
	(0.1890)	(0.1637)	(0.1689)	(0.1765)	(0.2282)	(0.2380)	(0.1963)
Has a Car	0.2715	0.2460	0.2648	0.2821	0.3306	0.3448	0.2899
	(0.4448)	(0.4307)	(0.4412)	(0.4501)	(0.4705)	(0.4753)	(0.4537)
Has a Computer	0.2387	0.3913	0.5766	0.7028	0.7956	0.7099	0.5831
	(0.4263)	(0.4881)	(0.4941)	(0.4571)	(0.4033)	(0.4538)	(0.4931)
Works	0.1265	0.1196	0.1093	0.0991	0.1641	0.1219	0.1231
	(0.3324)	(0.3245)	(0.3120)	(0.2988)	(0.3704)	(0.3272)	(0.3286)
Age	11.3934	11.4098	11.1063	11.1629	11.0272	10.9308	11.1665
	(1.2083)	(1.1129)	(0.8109)	(0.7762)	(0.8669)	(0.9939)	(0.9805)
Never Failed	0.6725	0.6927	0.8036	0.7565	0.7330	0.7222	0.7318
	(0.4693)	(0.4614)	(0.3973)	(0.4292)	(0.4424)	(0.4480)	(0.4430)

Table 2: Descriptive Statistics - School Census, 1rst and 2nd cycle, Primary education, 2007-2015

	2007	2008	2009	2010	2011	2012	2013	2014	2015	Total
Drop Out Rate - First Cycle	2.0691	2.0239	1.9517	1.7260	2.0026	1.4018	1.6168	1.3095	1.8375	1.7779
	(2.4945)	(2.2965)	(4.4612)	(1.9298)	(3.7592)	(1.7785)	(2.0660)	(1.4435)	(7.0219)	(3.4353)
Drop Out - Second Cycle	4.9267	7.0043	5.6556	6.1076	6.9510	4.7140	2.9944	2.7639	2.0904	4.6308
	(6.6863)	(10.7860)	(8.0678)	(7.5683)	(12.4718)	(6.9441)	(4.8078)	(3.5181)	(2.6116)	(7.6423)
Passing Rate - First Cycle	92.5013	91.4265	88.3167	87.1762	88.4328	90.4239	88.5245	88.0678	89.5428	89.3990
	(5.3760)	(5.5065)	(6.9862)	(6.5421)	(7.5718)	(5.4821)	(6.5239)	(6.9161)	(8.4819)	(6.8389)
Passing Rate - Second Cycle	85.5174	81.4085	69.4000	73.3815	80.3314	85.4430	87.0492	87.6893	87.6078	82.5404
	(15.3802)	(16.6254)	(13.6240)	(14.8031)	(17.2469)	(12.0384)	(9.4251)	(9.5891)	(10.6433)	(14.5276)

Table 3: The Impact of UPPs on 9th Grade Test Scores

VARIABLES	(1) Math Benchmark	(2) Math	(3) Math	(4) Math	(5) Language Benchmark	(6) Language	(7) Language	(8) Language
UPP, 100 Meters	6.232**				1.810			
	(2.432)				(2.699)			
UPP, 50 Meters		6.632**				3.499		
		(2.666)				(3.059)		
UPP, 200 Meters			4.039				0.192	
			(2.611)				(2.662)	
UPP, 500 Meters				3.310				-0.218
				(2.444)				(2.454)
Observations	30,036	30,036	30,036	30,036	26,169	26,169	26,169	26,169
R-squared	0.083	0.083	0.083	0.083	0.084	0.084	0.084	0.084
Number of cod_escola	85	85	85	85	85	85	85	85
Sample	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters
Treatment	100 Meters	50 Meters	200 Meters	500 Meters	Meters	50 Meters	200 Meters	500 Meters

Notes: The dependent variable is Prova Brasil test score in math or language. All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and participate of Prova Brasil at least two times. Schools within 100 meters from some UPP are considered treated in the benchmark regression used in other tables, and schools within 50, 200 and 500 meters are used to check if the results are dependent on the treatment choice. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 4: The Impact of UPPs on 5th Grade Test Scores

VARIABLES	(1) Math Benchmark	(2) Math	(3) Math	(4) Math	(5) Language Benchmark	(6) Language	(7) Language	(8) Language
UPP, 100 Meters	-0.143				-0.468			
	(1.689)				(1.603)			
UPP, 50 Meters		-0.106				-0.705		
		(1.822)				(1.716)		
UPP, 200 Meters			0.057				0.220	
			(1.681)				(1.543)	
UPP, 500 Meters				-0.528				-0.315
				(1.649)				(1.500)
Observations	61,853	61,853	61,853	61,853	54,652	54,652	54,652	54,652
R-squared	0.172	0.172	0.172	0.172	0.165	0.165	0.165	0.165
Number of cod_escola	210	210	210	210	210	210	210	210
Sample	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters	100 Meters
Treatment	50 Meters	50 Meters	200 Meters	500 Meters	Meters	50 Meters	200 Meters	500 Meters

Notes: The dependent variable is Prova Brasil test score in math or language. All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and participate of Prova Brasil at least two times. Schools within 100 meters from some UPP are considered treated in the benchmark regression used in other tables, and schools within 50, 200 and 500 meters are used to check if the results are dependent on the treatment choice. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 5: The Impact of UPPs on 9th and 5th Grades Test Scores

VARIABLES	(1) Math 9th grade	(2) Language 9th grade	(3) Math 5th grade	(4) Language 5th grade
UPP in t	-0.789	-6.270	1.672	-2.445
	(3.540)	(3.851)	(2.988)	(2.988)
UPP in t-1	9.952***	6.704*	-1.954	-1.159
	(3.079)	(3.466)	(1.928)	(1.902)
UPP in t-2	3.687	-1.553	2.617	0.713
	(3.556)	(5.728)	(3.192)	(2.759)
UPP in t-3	11.312***	7.482**	-1.041	1.434
	(2.804)	(3.121)	(2.315)	(2.136)
UPP in t-4	-0.669	-5.123	1.476	2.268
	(7.144)	(8.872)	(3.692)	(3.381)
UPP in t-5	12.444*	8.421	0.745	0.054
	(6.817)	(6.700)	(2.623)	(2.845)
UPP in t-6	15.950***	20.421**	7.131	8.538
	(4.701)	(8.564)	(4.542)	(5.327)
UPP in t-7	0.254	3.703	1.363	-1.426
	(4.253)	(5.778)	(5.760)	(2.968)
Observations	30,036	26,169	61,853	54,652
R-squared	0.085	0.085	0.172	0.165
Number of cod_escola	85	85	210	210
Sample and Treatment	100 Meters	100 Meters	100 Meters	100 Meters

Notes: The dependent variable is Prova Brasil test score in math or language. All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and participate of Prova Brasil at least two times. Schools within 100 meters from some UPP are considered treated. UPP in t-k represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in period $t-k$. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Robustness exercises

VARIABLES	(1) Placebo Math	(2) Placebo Language	(3) Trend Math	(4) Trend Language
UPP in t	-1.311	-4.933	-3.504	-6.288
	(4.296)	(9.414)	(3.489)	(3.848)
UPP in t-1	9.907***	6.686*	7.691***	6.702*
	(3.015)	(3.388)	(2.435)	(3.468)
UPP in t-2	3.230	-0.258	-1.642	-1.548
	(4.712)	(12.276)	(3.538)	(5.725)
UPP in t-3	11.050***	7.562**	9.080***	7.471**
	(2.792)	(3.422)	(2.452)	(3.120)
UPP in t-4	-1.229	-3.739	-6.055	-5.130
	(7.659)	(12.725)	(7.972)	(8.874)
UPP in t-5	12.126*	8.541	8.938	8.434
	(6.581)	(6.809)	(5.857)	(6.707)
UPP in t-6	15.522***	21.959*	7.825*	20.379**
	(5.778)	(12.488)	(4.027)	(8.488)
UPP in t-7	-0.250	3.784	-5.385	3.727
	(4.224)	(5.727)	(4.090)	(5.775)
UPP in t+2	0.198	2.104		
	(3.101)	(9.465)		
UPP in t+3	-1.795	0.714		
	(3.897)	(5.218)		
UPP in t+4	-0.346	0.216		
	(3.471)	(7.312)		
UPP in t+5	0.393	-0.915		
	(3.711)	(5.336)		
UPP in t+6	-1.777			
	(3.194)			
UPP in t+7	1.324	-0.929		
	(3.705)	(6.701)		
UPP in t+9	2.170			
	(4.934)			
Observations	30,036	26,169	29,054	26,169
R-squared	0.085	0.086	0.089	0.086
Number of cod_escola	85	85	80	85
Sample and Treatment	100 Meters	100 Meters	100 Meters	100 Meters

Notes: The dependent variable is Prova Brasil test score in math or language. All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and participate of Prova Brasil at least two times. Schools within 100 meters from some UPP are considered treated. *UPP in* $t-k$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in t-k. And *UPP in* $t+k$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in t+k (the placebo exercise). In Columns 3 and 4, we add an interaction between a linear time trend and the average test scores before 2008. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 7: The Impact of Upps on Drop out and Approval rates

VARIABLES	(1) Drop out Primary 1st cycle	(2) Drop out Primary 2nd cycle	(3) Passing Primary 1st cycle	(4) Passing Primary 2nd cycle
UPP in t	-0.004	0.520	-0.739	-1.264
	(0.167)	(1.015)	(0.545)	(1.320)
UPP in t-1	0.006	-0.405	-0.007	0.640
	(0.277)	(0.834)	(0.670)	(1.578)
UPP in t-2	-0.275	0.103	-0.233	2.277
	(0.226)	(1.479)	(0.733)	(2.139)
UPP in t-3	-0.600**	1.399	-0.756	1.287
	(0.273)	(1.985)	(0.883)	(3.016)
UPP in t-4	-0.562	-1.884	0.025	2.195
	(0.359)	(1.977)	(1.094)	(3.225)
UPP in t-5	-0.950**	-2.189	0.041	3.094
	(0.390)	(2.557)	(1.299)	(4.062)
UPP in t-6	-1.440**	2.196	0.331	-2.643
	(0.561)	(2.465)	(1.475)	(3.540)
UPP in t-7	-1.488	-0.297	-4.049	-1.525
	(0.990)	(2.848)	(3.161)	(3.321)
Observations	2,028	939	2,028	939
R-squared	0.023	0.082	0.156	0.321
Number of cod_escola	254	162	254	162
Sample and Treatment	100 Meters	100 Meters	100 Meters	100 Meters

Notes: The dependent variables are dropout and approval rates in 1st to 5th grade (first cycle) and 6th to 9th grade (second cycle). All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and appear in the sample at least two times. Schools within 100 meters from some UPP are considered treated. $UPP\ in\ t-k$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in t-k. In Columns 3 and 4, we add an interaction between a linear time trend and the average test scores before 2008. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 8: Robustness exercises, dropout

VARIABLES	(1) Placebo	(2) Trend Drop out
UPP in t	0.028	0.058
	(0.183)	(0.160)
UPP in t-1	0.058	0.102
	(0.310)	(0.265)
UPP in t-2	-0.190	-0.158
	(0.247)	(0.203)
UPP in t-3	-0.493*	-0.374*
	(0.295)	(0.204)
UPP in t-4	-0.440	-0.290
	(0.375)	(0.286)
UPP in t-5	-0.795**	-0.616**
	(0.397)	(0.303)
UPP in t-6	-1.299**	-0.967***
	(0.582)	(0.268)
UPP in t-7	-1.363	-0.705*
	(1.023)	(0.422)
UPP in t+2	0.409	
	(0.421)	
UPP in t+3	0.276	
	(0.247)	
UPP in t+4	-0.990	
	(0.605)	
UPP in t+5	-0.275	
	(0.374)	
UPP in t+6	-0.081	
	(0.501)	
UPP in t+7	-0.168	
	(0.531)	
Observations	2,028	1,992
R-squared	0.035	0.114
Number of cod_escola	254	233
Sample and Treatment	100 Meters	100 Meters

Notes: The dependent variables are dropout rates in 1st to 5th grade (first cycle) and 6th to 9th grade (second cycle). All regressions include school and year fixed effects. The sample includes all schools that are within 100 meters from some favela and appear in the sample at least two times. Schools within 100 meters from some UPP are considered treated. $UPP\ in\ t-k$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in t-k. And $UPP\ in\ t+k$ represents a set of dummies which are one if the school s were in a place within 100 meters from an UPP in t+k (the placebo exercise). In Columns 3 and 4, we add an interaction between a linear time trend and the average test scores before 2008. All regressions includes students socio-economic characteristics (gender, age, employment status, and if the student has never failed), household characteristics (if someone in the household owns a car, television and computer), and schools characteristics (if there is school lunch, computer lab, science lab, kitchen, teacher room and principal room). Robust standard errors are clustered at the school level. Significance: *** p<0.01, ** p<0.05, * p<0.10.