Spillovers from Conditional Cash Transfer Programs: *Bolsa Família* and Crime in Urban Brazil*

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Abstract

This paper investigates the impact of Conditional Cash Transfer (CCT) programs on crime. Making use of a unique dataset combining detailed school characteristics with geo-referenced crime information from the city of São Paulo, Brazil, we estimate the contemporaneous effect of the *Bolsa Família* program on crime. We address the endogeneity of CCT coverage by exploiting the 2008 expansion of the program to adolescents aged 16 and 17. We construct an instrument that combines the timing of expansion with the initial demographic composition of schools to identify plausibly exogenous variation in the number of children covered by *Bolsa Família*. We find a robust and significant negative impact of *Bolsa Família* on crime. Incapacitation from time spent in school does not seem to be an important driving force behind the results, leaving as most likely mechanisms income effect and changed peer-group.

Keywords: Conditional Cash Transfer, *Bolsa Família*, Crime, Education, Schooling, Brazil
JEL codes: I28, I38, K42

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1. Introduction

Conditional Cash Transfer (CCT) programs, in which families receive government payments upon fulfillment of schooling and other requirements, are today among the most popular and celebrated social support programs in the world. Various versions of CCTs have been adopted in countries as diverse as Argentina, Bangladesh, Colombia, Indonesia, Jamaica, Kenya, Mexico, Turkey, and the US, among innumerous others. There is substantial evidence from some of these settings on the positive effect of CCTs on enrollment rates, preventive health care, and nutrition (for reviews of the literature, see Rawlings and Rubio, 2005, and Fizbein and Schady, 2009). Brazil, in particular, has one of the first and the largest CCT program in the world, currently named *Bolsa Família*. It covers over 11 million families and costs close to 0.4% of the country's GDP. Evidence suggests that *Bolsa Família* has had substantial impact on enrollment rates, school progression, extreme poverty, and inequality, though there are questions related to its social rate of return and effectiveness in urban settings (Soares, 2012, and Glewwe and Kassouf, 2012).

This paper uses school and crime data from the city of São Paulo, Brazil, to present one of the first pieces of evidence on the effect of *Bolsa Família* – or, for that matter, of any CCT program – on crime.[1] To our knowledge, the only other paper available on the effect of CCT on crime is Loureiro (2012). The author uses a panel with state level data from Brazil, where unobservables are likely to be a serious issue (Brazil has only 27 states). In his paper, identification comes from state level deviations between actual and planned expansions of the program. He finds a strong effect of *Bolsa Família* on poverty, but only mild and non-robust effects on crime (with no significant impact detected in his most complete specifications).

In this paper, we make use of a unique dataset combining detailed school characteristics with geo-referenced crime information. We estimate the contemporaneous impact of the number of children covered by *Bolsa Família* within a school on crime in the school neighborhood. The number of children covered by CCT in an area at a moment in time is determined by the incidence of poverty and unemployment and by other socioeconomic characteristics, all likely to be correlated with crime. This precludes the interpretation of the correlation between *Bolsa Família* coverage and crime as causal. We overcome this problem by exploiting the 2008 expansion of the program to adolescents aged 16 and 17, from an initial setting where maximum age of coverage was 15. We construct an instrument that combines the timing of expansion and the initial demographic composition of schools to identify plausibly exogenous variation in the number of children covered by the CCT. This instrument allows us to estimate the causal impact of *Bolsa Família* on crime. We find a robust and significant negative impact of *Bolsa Família* transfers on crime. Our estimate suggests that the expansion of *Bolsa Família* to 16 and 17 year-olds after 2008 caused a 6.6% reduction in crime in school neighborhoods (45 fewer crimes per school per year, or 2 fewer crimes per student covered). If we make the heroic assumption that

[1] We became aware of the work of Loureiro (2012) while writing the first complete version of this paper in early 2012.

the coefficient estimated with variation at the 16-17 year-old margin can be applied to all age groups, the estimated coefficient implies that the total expansion in *Bolsa Família* coverage between 2006 and 2009 – 63 additional students covered per school – led to a reduction of 18% in crime.

The remainder of the paper is organized as follows. Section 2 contains the conceptual framework and a literature review. Section 3 provides an institutional background, by briefly describing the *Bolsa Família* CCT program and Brazilian educational and law enforcement systems. Section 4 presents and discusses the data, while section 5 describes our methodology and identification strategy. Finally, section 6 presents the results and is followed by concluding remarks in section 7.

2. Conceptual Framework and Literature Review

The relevance of CCTs as crime reducing instruments should be expected, given that youth account for a disproportionately high fraction of crimes, and that there is an intimate relationship between education, socioeconomic conditions, and crime. In the United States, for example, Levitt and Lochner (2001) document that 20% of the arrests for violent crimes involve individuals between ages 15 and 19. In our data for São Paulo, among crimes for which the age of the suspected offender is known, between 20% and 25% of robberies, thefts, and motor vehicle crimes are supposedly committed by individuals below age 18. In fact, Schwartz and Abreu (2007), when discussing the impact of CCT programs on vulnerable youth, highlight their potential crime reducing effect.

There are various potential channels through which education affects crime, and vice-versa. There is evidence on the effect of violence and crime on schooling and learning (see, for example, Grogger, 1997, Aizer, 2009, Rodríguez and Sanchez, 2009, Chambargwala and Morán, 2010, and Monteiro and Rocha, 2011) and also on the effect of schooling on involvement with crime and violence (see review by Lochner, 2010). Schooling may have long-term effects on criminal behavior through wages and preferences (discount rates and risk aversion), and, therefore, through the relative attractiveness of criminal activities and the cost of expected punishment (Becker and Mulligan, 1997, Lochner and Moretti, 2004, Lochner, 2010, Machin et al, 2010, and Deming, 2011). More importantly for this paper, schooling also has short-term impacts through the incapacitation effect, since time spent in school reduces the opportunity for certain types of crimes and risky behavior, though possibly also increasing the interaction among youth and the likelihood of violence. Anderson (2011), for example, documents declines in juvenile crime rates at the time of increase in minimum dropout ages across US states, while Berthelon and Kruger (2011) report reductions in crime and teenage pregnancy following a school reform in Chile that increased weekly school hours from 32 to 39. Jacob and Lefgren (2003) and Luallen (2006), the former exploiting variations in teacher in-service days and the latter teacher strikes, document that property crimes decline but violent crimes rise when youth are in school (see also Snyder and Sickmund, 1999, and Gottfredson and Soulé, 2005).

CCT programs also transfer money to families on a monthly basis, and these transfers may have direct impacts on criminal behavior. The enhanced ability to buy certain goods may reduce the incentive or "need" to engage in economically motivated crimes. Additional income from welfare transfers may also alter households' routines in a manner that exposes them to less risk of victimization and/or opportunities for delinquency, such as by affording parents more time for supervision (Heller et al, 2010). Indeed, there is evidence that welfare transfers affect crime rates. De Franzo (1996, 1997), Zhang (1997), Hannon and De Franzo (1998), and Foley (2008) study the effect of the amount and timing of welfare payments, finding impacts on total number of crimes (negative) and also on the distribution of crimes through the month. Jacob and Ludwig (2010) analyze a housing voucher program in Chicago that transferred the equivalent of 50% of household income to beneficiaries, reporting a decline of roughly 20% in both violent and overall arrests, which implies an income elasticity of -0.4.

A final channel through which CCTs may affect crime is social interaction. If the network or reference group of youth is affected by school enrollment and attendance, then staying in school may have positive peer effects (Glaeser et al, 1996, and Lochner, 2010), despite the higher probability of attrition between youths. In fact, available evidence suggests that keeping youth at school – or keeping certain troubled youth at school – tends to shift part of violence from the streets into the schools (see, for example, Jacob and Lefgren, 2003, and Anderson, 2011). Still, the mechanism discussed here would hold as long as the school network were on average better than the peer group that youth would have in case they dropped out (or did not attend school).

The main challenge faced by this paper is to isolate the channel through which *Bolsa Família* affects crime. Though we present robust evidence on the effect of *Bolsa Família* transfers on crime, we are not able to directly address the issue of channel. Nevertheless, by looking at the heterogeneity of effects across days and types of crimes, we do provide some suggestive evidence on the channels that are likely to be at work in our setting. We find that the estimated effects of CCT on crime are very similar across school and non-school days, suggesting that the incapacitation mechanism extensively discussed in the United States literature is not the driving force behind our results. We also find the effects to be quantitatively more important for robberies, though there is also some evidence of reductions in drug-related crimes. So the results indicate that the income component of *Bolsa Família* is likely to be an important determinant of the reductions in crime, though social interactions through changed peer groups and reorganization of family routines cannot be ruled out.

Our paper relates to three strands of literature. First, the paper adds to the numerous studies analyzing the impact of CCTs by showing that spillovers onto other social phenomena, not directly targeted in the initial design of this type of program, may be significant and relevant from a policy perspective (see review in Fizbein and Schady, 2009). Second, it relates to the literature on educational policies and crime, and presents a context in which there is a reduction crime, but where incapacitation and future returns to schooling do not seem to be the main thrust. And third, the paper also speaks to the discussion on the socioeconomic determinants of crime. Various authors have documented a robust correlation between inequality and crime (see, for example, Fajnzylber et al, 2002, Bourguignon et al, 2003, Soares, 2004, or the discussion in Soares and Naritomi, 2010). To the extent that *Bolsa Família* has had a substantial impact on inequality in Brazil (Soares, 2012), and that the income effect is likely to be an important force behind our results, the evidence presented here may be seen as an additional piece of information on the relationship between socioeconomic conditions and crime (in line with the evidence from welfare payments discussed before).

3. Institutional Background

3.1 The *Bolsa Família* Conditional Cash Transfer Program

Bolsa Família (or "Family Allowance") is Brazil's federal CCT program, created in 2003. Its creation was, in fact, the consolidation of various social support programs coupled with a redesign of Brazil's first federal CCT program, *Bolsa Escola*, which had been in operation since 2001. *Bolsa Escola*, in turn, was inspired by a series of pilot projects in various Brazilian cities, dating back to 1995 in the capital Brasília, and in Campinas and Ribeirão Preto, both cities in the state of São Paulo. Soares (2012) provides a detailed history of *Bolsa Família* and its institutional design, as well as references to various impact evaluation studies.

Bolsa Família provides different benefits for families at different income levels and has various conditionalities. In 2009, the Fixed Benefit was a payment of R$68.00 for families with monthly per capita (p.c.) income below R$70.00. In addition, there was the Variable Benefit, according to which families with monthly p.c. income below R$140.00 received R$22.00 per child under age 15 (up to the maximum of 3 children). Finally, starting in 2008, benefits were extended to adolescents, through the creation of the Variable Youth Benefit. The Variable Youth Benefit paid R$33.00 per adolescent aged between 16 and 17 (up to a maximum of 2 adolescents), for families with monthly p.c. income below R$140.00.[2] For our later discussion of the empirical strategy, it is worth mentioning that the introduction of the Variable Youth Benefit was scheduled for March of 2008 (Ministério do Desenvolvimento Social e Combate à Fome, 2008), but the actual inclusion of adolescents in the program was only consolidated after July of that same year (Soares, 2012).

The maximum benefit value of *Bolsa Família* in 2009 was R$200.00 per family, for families with monthly p.c. income below R$70.00, 3 children under age 15, and 2 adolescents aged between 16 and 17. Throughout the history of the program, there were also schooling and health conditionalities attached to the payments, though recently it has been claimed that they were not always enforced. Program participation

[2] On January 2012, the exchange rate between the Brazilian Real and the US Dollar was 1.76 R$/US$.

requires school enrollment and 85% attendance for children between 6 and 15, and 75% attendance for adolescents between 16 and 17. In addition, conditionalities include fulfillment of a vaccination and growth and development calendar for children under 7, prenatal care for pregnant women, and health monitoring for lactating women.

It is important to mention that, during the period of our analysis, there was a second CCT program in operation in the city of São Paulo: the municipal *Renda Mínima* (or "Minimum Income"). São Paulo's *Renda Mínima* is a minimum family income program created in 2006. In order to be eligible for the program, families must have lived in the city of São Paulo for at least 2 years, have monthly p.c. income below R$175.00, and have at least one child under age 16. The conditionalities are school enrollment and minimum attendance of 85% for children between ages 6 and 15, and fulfillment of a vaccination calendar for children under 7. *Renda Mínima*'s benefits can complement those of *Bolsa Família*. The maximum benefits allowed by *Renda Mínima* are (total value = *Bolsa Família* + *Renda Mínima*): R$140.00 for families with 1 child, R$170.00 for families with 2 children, and R$200.00 for families with 3 or more children.

Renda Mínima is sometimes used as a complement to *Bolsa Família*, and sometimes families receive only one of the two benefits. Our CCT data, to be discussed in further detail below, contain information on the number of students per school receiving *Bolsa Família* and *Renda Mínima* (with no information on value). The correlation of the number of *Bolsa Família* and *Renda Mínima* recipients across schools is above 0.75, making it very difficult to separate the independent effect of each program. In addition, we have an institutional change in *Bolsa Família* that allows us to identify its causal impact, so we concentrate our analysis on the impact of the federal CCT program (also, the number of students covered by *Bolsa Família* is 18% higher than that of *Renda Mínima*). We do however check whether results change if we incorporate *Renda Mínima* in the analysis and, more importantly, we use *Renda Mínima* as a way to validate our empirical strategy.

Regarding the operational aspects of the program, *Bolsa Família* is federally funded and jointly managed by the federal and municipal governments. Budgets for the program for each municipality are determined by the federal government, based on estimates of the number of poor families. Applications of families, eligibility verification, and final decisions on enrollment are managed by the municipality, conditional on the budget determined by the federal government (Glewwe and Kassouf, 2012 and Soares, 2012). Schools play no direct role in operating or funding the program, apart from recording children's attendance. Payments are made by the Ministry of Social Development directly to individual family accounts, which can be accessed using bank cards distributed to families. Monitoring of schooling conditionalities was very mild during the first years of the program, but has become increasingly strict since October 2006. The federal government (Ministries of Social Development, Education, and Health) created an official monitoring system that is supposed to have increased monitoring coverage from 62% of children in 2006 to 85% in 2008 (Soares, 2012).

In our context, it is very unlikely that CCT allocation takes into account differences in violence within the city of São Paulo. However, the program targets poor families, and areas with different concentration – or dynamics – of poverty are likely to have different levels – or dynamics – of crime, be it due to socioeconomic conditions, to other policies, or to different intensity and strategy of policing.

3.2 Law Enforcement

Law enforcement in Brazil is primarily the responsibility of state governments. Executive and administrative authority rests with the state secretariats of public security (*Secretarias Estaduais de Segurança Pública*), which are appointed and respond directly to the governor, who also allocates the budget to the secretariats. Some strategic decisions are determined by law. For example, by constitutional mandate, the number of policemen in the state of São Paulo has to be roughly constant in per capita terms across cities. The execution of enforcement is shared between two corporations that respond to the secretariats: the military police, responsible for ostensive patrolling and repression, and the civil police, which plays judiciary and investigative roles. The commanders of the two police forces are appointed by the governor. There are also municipal police forces (*Guardas Civis Municipais*), which are not mandatory by federal law but a choice of the municipality. These can be used to strengthen ostensive patrolling, traffic and crowd control, but have less authority and a reduced scope in comparison to the state polices. The city of São Paulo has its municipal police force. Crime recording and monitoring of public security conditions are, nevertheless, the responsibility of the state polices.

3.3 Education

Primary and secondary schooling in Brazil is typically run by cities and states. In general, cities focus on elementary and middle schools (pre-school to 8th grade), and states focus on middle and high schools. In the city of São Paulo, 98% of municipal schools offer middle school grades and none offer high school, while, among state schools, 93% offer middle school grades and 99% offer high school.

Pupil allocation to schools is based on place of residence. There are no official school districts, but the city and state secretariats form a committee that allocates the pupil to the closest available school. Once the child is enrolled in a school, she stays there unless the family moves from the original neighborhood. Parents cannot choose schools, except for a couple of high-performance flagship schools – which have enrollment rationed through entrance exams – and for decisions on neighborhood of residence. There is no evidence that public schooling is an important dimension in neighborhood choice in Brazil.

Given the system of assignment of pupils to schools, it is unlikely that students with particular characteristics (more or less violent, for example) are deliberately sent to schools with more *Bolsa Família* coverage. However, again, students from poorer families will be clustered in schools in poorer neighborhoods, which are likely to have higher coverage of *Bolsa Família* and higher crime rates. Similarly, changes in local economic conditions and other policies may simultaneously affect eligibility of families to the CCT program and the incidence of crime in a certain area. To give a concrete example, consider the minimum wage increase from R$300 to R$465 between 2006 and 2009, corresponding to a 55% gain in nominal terms and a 30% gain in real terms. To the extent that minimum wages impact mostly low-income areas, this change could generate a spurious correlation between CCT coverage and crime. This is the type of concern we have in mind when developing our empirical strategy in section 4.

4. Data

4.1 Crime Data

Our crime data are from the INFOCRIM database, a COMPSTAT-like crime tracking system from the Secretariat of Public Security of the State of São Paulo (*Secretaria de Segurança Pública do Estado de São Paulo*), the state law enforcement agency. We have time and geo-referenced data for all reported crimes in the city of São Paulo, from 2006 through 2009. INFOCRIM contains all information available from the police reports. It includes place of occurrence (latitude and longitude), type of crime, estimated time of occurrence, and, sometimes, characteristics of the suspected offender (such as age and gender). Unfortunately, age of the suspected offender is available only for a very small fraction of the data. Therefore, due to concerns about sample selection, we do not use this specific information. We concentrate our analysis on thefts, robberies, acts of vandalism, violent crimes, and drug-related offenses.[3] Records related to these offenses encompass a total of 1,473,939 crimes over the 4 years in our sample.

4.2 School Data

Our school and student data cover the period between 2006 and 2009 and come from two sources. From the Secretariat of Education of the City of São Paulo (*Secretaria de Educação da Cidade de São Paulo*), we have data on state and municipal schools identifying the type of school, school location (longitude and latitude), and number of students covered by conditional cash transfer programs (separate information on number of students receiving *Bolsa Família* and *Renda Mínima*). From the Brazilian School Census, we gather additional information on demographic characteristics of teachers and students and school infrastructure. The characteristics are: (i) number and education of teachers; (ii) number, gender, race, and current grade of students; and (iii) number of classrooms in the school, availability of sanitation, and number of computers.

[3] Thefts and robberies are defined in the usual way (burglaries are subsumed within both categories, according to the use of force). The other categories include the following definitions of crimes from the police records (our translation): (i) violent crimes include assaults, attempted homicides, attempted rapes, homicides, rapes, random acts of violence, and threats; (ii) drug-related crimes include association with/for drug-trafficking, drug-trafficking (sale), manufacturing of drugs, possession of drugs, and use of drugs; and (iii) vandalism includes cruelty to animals, damage to property, obscene writing, disturbance of the public order, causing turmoil, and vagrancy.

4.3. Unit of Analysis

Given the data and the phenomenon we are studying, geographic areas associated with schools should be the natural unit of analysis. Unfortunately, as mentioned before, São Paulo does not have a strict geographic definition of school districts. Still, students in the city are allocated to slots available in the closest school, so there is a high correlation between school location and place of residence. Therefore, we create artificial school districts by assigning to a given school the area that is closer to it than to any other school.

This is a key step that allows us to link crimes happening in a given location to a particular school. Obviously, children do not circulate only in these areas and do not necessarily live there, but they are likely to live close to the school and spend part of their day there. If there is a high probability that children are around the school for a significant amount of time, this approach should be adequate. Evidence from the United States supports this strategy, since it reveals a high concentration of crimes committed by youth in periods immediately after school hours, when children/adolescents are still likely to be near the school (Snyder and Sickmund, 1999, Jacob and Lefgren, 2003, and Gottfredson and Soulé, 2005). The system of allocation of students to schools in São Paulo also supports our approach, as children are expected to study in the closest school available. Hence, our measure of crime per school is the number of crimes that were committed closer to a given school than to any other school (these crimes are "assigned" to that particular school).[4]

The construction of our dependent variable hinges, therefore, on the set of schools considered. Generally, the number of crimes assigned to a given school will vary with this set. Given the typical age of students, we focus on high schools (from 9th to 11th grade), but we also present results considering two other sets of schools: only middle schools and middle schools and high schools together.[5] This allows us to check the robustness of the results to different formulations and also to assess how the response of crime varies with the age group of children considered. In any case, high schools – with older adolescents – seem to be the more adequate unit for our analysis.

4.4 Descriptive Statistics

Table 1 provides descriptive statistics for our definition of high school districts in the city of São Paulo. We present data on different types of crime, number of students receiving *Bolsa Família*, total number of students, and fraction of students between ages 16 and 17. The dataset contains 581 high schools, observed during the four years between 2006 and 2009, comprising a total of 2,324 observations.

The average number of crimes per year occurring around a high school is 634. Robbery is, by far, the most commonly reported crime: almost 70% of the crimes reported are robberies. There is also a sizeable

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[4] For each crime, we calculate the distance from the place of occurrence to every school. We subsequently assign that crime to the closest school.

[5] Though we refer to middle schools and high schools as if they were separate institutions, strictly we should be writing "schools offering middle school grades" and "schools offering high school grades," since the same school can offer middle and high school simultaneously. We keep the terms used in the text as shorthands for these.

number of violent crimes reported, while the number for thefts, vandalism, and drug crimes tend to be very small. The low number of thefts in comparison to robberies and violent crimes indicates a considerable degree of underreporting for lesser crimes. The numbers in the table also suggest that, given the small number of registered occurrences, it may be difficult to estimate the impact of *Bolsa Família* for crime categories such as thefts, vandalism, and drug crimes.

High schools have on average 1,360 students, with 9% covered by *Bolsa Família*. There is substantial variation in the number of children covered by the CCT across schools, reflecting both different numbers of students and different socioeconomic conditions across areas. Table 1 also presents descriptive statistics for the fraction of students aged between 16 and 17 in 2006, which plays an important role in the construction of our instrument and the identification of the causal effect of *Bolsa Família* (see next section). In high schools, 29% of pupils were between 16 and 17 years-old in 2006, with a standard deviation of 11%. This is important for our purposes because our main strategy relies on the presence and variation of 16 and 17 year-olds across schools. In middle schools, for example, the fraction of 16 and 17 year-olds is smaller, as expected (not shown in the table).

5. Empirical Strategy

The main challenge in the identification of the causal effect of CCTs on crime comes from the socioeconomic targeting of this type of program. Targeting implies that CCT coverage is correlated with socioeconomic conditions, which, in turn, can be correlated with crime. This potential problem is present in both the cross section – as when poorer areas have higher CCT coverage and higher crime – and the time series – as when areas experiencing economic improvements see reductions in CCT coverage and reductions in crime. Fixed-effects, despite being important for controlling for unobserved characteristics, do not solve the endogeneity problem. In short, the fraction of children covered by the program each year is likely to be a direct function of socioeconomic conditions in the area at that point in time, which may in turn have an independent impact on crime rates. One could find a positive correlation between CCTs and crime simply because increased coverage is driven by deteriorating socioeconomic conditions.

In addition to controlling for school fixed-effects and several variables related to infrastructure and teacher and student characteristics, we adopt an instrumental variables approach to try to overcome this problem. As mentioned before, starting on July 2008 *Bolsa Família* extended its benefits to adolescents between ages 16 and 17, through the Variable Youth Benefit. From then on, families with monthly p.c. income below R$140.00 would receive R$33.00 per family member between ages 16 and 17, up to a maximum of 2. Relying on the different demographic composition of schools, we can construct an instrument for CCT coverage that exploits the interaction of number of students between 16 and 17 in the first year of the data (2006) with the timing of program expansion. If there is an impact of CCTs on crime, then when *Bolsa Família* is expanded one should expect a larger reduction in crime around schools that had a higher number of students between 16 and 17 before program expansion.

Our benchmark specification is the following:

$$ln(crime_{it}) = \alpha + \beta(CCT_{it}) + \gamma' X_{it} + \theta_i + \delta_t + \varepsilon_{it}, \quad (1)$$

where $crime_{it}$ denotes the number of crimes in school i and year t; CCT_{it} is the number of students receiving CCT; X_{it} is a set of school variables related to infrastructure and teachers and students characteristics; and θ_i and δ_t are, respectively, school and year fixed-effects. In all specifications, standard errors are clustered at the school level, so that the error term is allowed to have an arbitrary correlation within schools over time.

Our instrument for CCT_{it} is the interaction of number of students between ages 16 and 17 in 2006 with a variable indicating the expansion of the program. Since program expansion only started in the middle of 2008, the variable indicating program expansion is set equal to 0.5 in 2008 and 1 in 2009. We use the number of students between ages 16 and 17 to construct the instrument because the initial demographic composition of schools is likely to be exogenous to the change in the program and also somewhat persistent. If these assumptions hold, the expansion in program coverage due to the regulatory change after 2008 should be correlated with the initial demographic composition of schools. This is the first stage of our empirical strategy.

As we do not have a measure of local population, which would be the natural way to normalize the number of crimes in a given area, we use the natural logarithm of number of crimes as the dependent variable. The number of crimes is also typically used as dependent variable in the United States literature (see, for example, Jacob and Lefgren, 2004, and Luallen, 2005). In contexts of count data, such as this, there are concerns related to the functional form of the estimating equation, given the possibility of an excessive number of zeros and over-dispersion. In our setting, excessive zeros do not seem to be a serious issue, but over-dispersion may be a problem (see Figure A.1 in the Appendix). For this reason, in addition to linear regressions with the dependent variable in natural logarithms (substituting undefined values by zero), we also estimate Poisson and negative binomial models. In all three cases, coefficients can be interpreted as semi-elasticities. In order to understand the channels through which *Bolsa Família* affects crime, we estimate our benchmark specification for different types of crime, for days with and without classes, and for different hours of the day.

We control for the total number of students in the school, since students are both potential offenders and victims. In addition, our instrument makes use of the number of students between 16 and 17 in 2006 and one may think that changes in the number of students in this age group are directly related to crime. The literature on the demographic determinants of crime, such as Levitt (1999) and Jacobson (2004), would indeed raise this concern. So we also control for the number of students between 16 and 17 in each year. Furthermore, given that children from different schools interact and migrate across boundaries of school districts, in some specifications we control for the number of children in neighboring schools. To construct this variable, we count the number of students enrolled in schools within two kilometers of a given school. Finally, we control for various characteristics of teachers, students, and schools, all of which are likely to be correlated with local socioeconomic conditions. Our full set of controls includes the following: number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, percentage of non-white students, percentage of delayed students, a dummy for the availability of computers for students, and the proportion of high school students in the school, and, in some specifications, number of students enrolled in schools within 2 kilometers. These are intended to capture, above all, socioeconomic conditions of the area where the school is located (or, similarly, of the students' families) and impacts of other competing policies that may have similar effects to those expected from CCTs.

One remaining concern in our empirical strategy refers to the dynamics of crime across areas. If schools with a higher initial number of adolescents display a dynamics of crime that is intrinsically different from other areas, than our instrumental variable approach is not valid. If this were the case, the exclusion restriction would not apply, since the initial fraction of students aged 16 and 17 in 2006 would be related to changes in crime through other channels, apart from the expansion of the *Bolsa Família* program. Note that this would have to hold conditional on school fixed-effects and on the contemporaneous number of students in that age group, which are used as controls in the main specification (so the simultaneous correlation between demographic composition and crime does not constitute a challenge to identification). The issue of differential pre-existing crime trends in schools with more adolescents is particularly dangerous because we have a panel with a short time-spam in the pre-treatment (before the *Bolsa Família* expansion). Thus we do not have a sufficiently large number of periods to estimate school specific trends with any precision. Still, we perform several exercises that, if not perfect substitutes for that, are informative about how serious this handicap is.

In order to assess whether this issue seems to be a problem, we adopt two alternative approaches. First, we include in the basic specification a linear trend interacted with initial socioeconomic conditions, allowing areas with different socioeconomic background to have different dynamics of crime. Most importantly, we include a time trend interacted with the proportion of delayed students (that are behind their correct grade) at the baseline, which captures social fragility at the school level. Also, the presence of 16-17 year-olds depends on how many students lag behind at the school because 17 year-olds should be graduating. Thus, controlling for the interaction of a time trend with the proportion of students that lag behind also accounts, admittedly imperfectly, for differences in crime trend directly related to the variation we use to estimate the causal impact of *Bolsa Família*. We also include interactions of linear time trends with the presence of (number of students covered by) *Bolsa Família* and the level of violence (overall crime) in the baseline. Second, we test for the presence of heterogeneous pre-existing trends in crime across schools with different demographic compositions. We do that by including placebo treatments that replicate our basic exercise for 2007, one year before the expansion of the program. Statistical significance of these placebo treatments would indicate that there were different trends in crime across schools with different demographic compositions already before the expansion of *Bolsa Família*.

In order to anticipate some of the discussion that will come up when analyzing the results, Table 2 presents correlations between our instrument and various controls. It shows that the instrument is not correlated with observable characteristics. We present both raw and conditional correlations, i.e., the correlation of observables with the residual of a regression of the instrument on year and school dummies. Considering unconditional correlations, 3 out of 8 are below 0.10 in modulus, 4 out of 8 below 0.20, and no correlation is larger than 0.40 in modulus; 6 out of 8 are significant at the 10% level. All conditional correlations, in turn, are below 0.10 in modulus and only one is significant at the 10% level. The only significant conditional correlation is with the number of teachers, with a low correlation coefficient of -0.075. For what it is worth, this suggests that, conditional on time and school dummies, the instrument is not correlated with observable characteristics. If unobservables do not behave very differently from observables, this would also suggest that the instrument is unlikely to be strongly correlated with unobservable characteristics.

6. Results

6.1 First Stage

The key identifying assumptions in our empirical strategy are that: (i) the age composition of schools in 2006 was associated with changes in CCT coverage after 2008; and (ii) there was no other connection between initial age composition and variations in crime apart from that working through CCT coverage.[6] We start by presenting our first stage results, which address the first of these assumptions. We present evidence to support the second assumption in the following sections of the paper, after we discuss our benchmark results.

Table 3 presents the first stage results of our instrumental variable strategy. In order to clarify the type of variation identified by the instrument, we present three specifications: the first one is a simple regression of *Bolsa Família* coverage on the instrument and time dummies; the second one includes school fixed-effects; and the last one, which is the actual first stage used in the paper, includes all controls. The most important point from Table 3 is that in column 1, before we include school fixed-effects, there is no correlation between the instrument and CCT coverage. This comes from a negative cross-sectional correlation between number of students aged 16 and 17 and *Bolsa Família* coverage, given that the main focus of the program is children

[6] Notice that a direct connection between age composition and level of crime does not threaten identification, since we have fixed-effects and control for the number of students aged between 16 and 17. The only threat to identification would be an independent connection between initial age composition and subsequent variations in crime.

below age 15. But once school fixed-effects are introduced in column 2, we see a positive and significant relationship between the instrument and CCT coverage. The within school change in coverage is significantly affected by the initial demographic composition because of the regulatory change introduced in 2008. The coefficient rises in magnitude and becomes estimated more precisely when we introduce additional controls in column 3, meaning that the expansion in coverage captured by our instrument is not reflecting socioeconomic changes, which might constitute a problem for the identification strategy. The point estimate implies that 100 more students between 16 and 17 in 2006 were associated with 5.52 more students covered by *Bolsa Família* in 2009, corresponding to a 5.52% coverage in this age group after expansion (which seems to be in line with the overall coverage of 9% from Table 1). This final specification is the one used in our instrumental variable approach. The F statistic displayed in the table shows that the instrument plays an important role in explaining variation in CCT coverage in the regressions with fixed-effects, suggesting that we do not have a weak instrument problem.

6.2 Benchmark Specification

Table 4 presents the results of our benchmark specification for the effect of *Bolsa Família* on crime. Panels A to F display the same set of regressions for different types of crime: all crimes, robberies, thefts, violent crimes, and drug crimes. In order to clarify the type of variation contained in the data and the role played by our instrument, each panel consists of five columns: the first one displays the result of a simple regression of log of crimes on number of students receiving *Bolsa Famíla* and time dummies, with no additional controls; the second column adds our set of controls to this specification (students, teachers, and school characteristics); the third column adds school fixed-effects; the fourth column presents a reduced form regression of log of crimes on our instrument and the full set of controls (the instrument is the interaction of number of students between ages 16 and 17 in 2006 with a variable indicating the expansion of the program in 2008 and 2009); and the last column presents our IV estimate, where number of students receiving *Bolsa Família* is treated as endogenous.

Qualitative results are very similar across the three first columns for the different types of crime. Looking at column 1, there is a negative correlation between number of students receiving *Bolsa Família* and crime in the school neighborhood. In most cases, this correlation is not associated with observable characteristics of schools, teachers, and students, since the coefficients remain with the same sign and similar magnitude when we add controls in column 2. But in column 3, when we add school fixed-effects, the negative correlation between CCT coverage and crime disappears, with the estimated coefficient becoming very small and non-significant in some cases, and positive and statistically significant in others (thefts and drug crimes). The first three columns suggest that there is some cross-sectional variation in *Bolsa Família* coverage that is negatively associated with crime, but that the within school variation bears either a positive or no relationship with crime. This pattern highlights that one of the main concerns leading to the adoption of our instrumental variable strategy – that variation in coverage within a school responds to neighborhood conditions – seems indeed to be an issue. As mentioned before, this would be expected if improvements in local economic conditions were associated with reduced *Bolsa Família* eligibility and reduced crime. The within school variation in CCT coverage seems to isolate the most endogenous dimension of variation in our independent variable.

Therefore, if we want to use school-fixed effects to control for unobserved neighborhood characteristics, we need to use our instrument for *Bolsa Família* coverage. Column 4 presents a reduced form regression of crime on our instrument, showing that the instrument is negatively correlated with crime, with the exception of thefts and vandalism. In short, schools with a higher number of students between ages 16 and 17 in 2006 experienced larger declines in crime in 2008 and 2009, when the CCT coverage was expanded to include these age groups.

In column 5, we use the instrument to isolate the supposedly exogenous dimension of variation in *Bolsa Família* coverage. The coefficients are negative and statistically significant, again with the exception of thefts and vandalism. In the case of all crimes and robberies, coefficients are slightly smaller than those presented in columns 1 and 2, where we did not use school fixed-effects. In the case of violent crimes and drug crimes, the coefficients are larger than those estimated in columns 1 and 2. In any case, if endogeneity generates a positive correlation between CCT coverage and crime, the use of an adequate instrument should lead to a more negative coefficient, precisely as we observe when we move from column 3 (fixed-effects) to column 5 (fixed-effects with instrumental variable).

The pattern of results from Table 4 deserves some comments. There is a remarkable difference between the fixed-effects and the IV fixed-effects estimates, as well as some similarity between the IV fixed-effects and the between estimates from columns 1 and 2. As mentioned before, we interpret this pattern as evidence that the within variation is particularly endogenous: after discarding all common time-series variation, within variation in crime is most likely driven by differences in socioeconomic dynamics across neighborhoods. Our instrument corrects for that.

We do not have data on number of students receiving *Bolsa Família* by age, which would be the natural number to use in order to explore the quantitative implications of the estimated coefficients. But our first stage result implies that an average of 22 additional students per school would be covered by the program due to its expansion. The coefficient from column 5 in Panel A implies that this expansion would have led to a reduction of 7.6% in crime in the school neighborhood, or 48 fewer crimes per school per year (6.4% of a standard deviation), or 2.1 fewer crimes per student covered.[7]

[7] Manipulation of the equation leads to:

The numbers from the previous paragraph come directly from our empirical strategy, representing therefore the reduction in crime that could be attributed to the expansion of program coverage to an older age group in 2008. A natural question that follows is what the estimated coefficient implies in terms of the overall effect of increased coverage during the 2006-2009 period. In order to asses this question, one would have to make the heroic assumption that the coefficient estimated with variation at the 16-17 year-old margin can be applied to all ages. In that case, we could simply look at the total variation in students covered by *Bolsa Família* between 2006 and 2009 and apply the estimated coefficient to calculate the total causal effect of the change in coverage. The total expansion in program coverage corresponded to 63 additional students covered per school. Following the same logic of the previous exercise, this expansion would have led to a reduction of 20% in crime around schools, or 129 fewer crimes per school per year (18% of a standard deviation), or 1.96 fewer crimes per student covered.

It is useful to recall once more the margin along which we are estimating the parameter. With heterogeneous treatment effects, the IV estimate represents the average reduction in crime among the areas most affected by the expansion in *Bolsa Família*, i.e. areas with lots of 16 and 17 year-olds and, most likely, with more disadvantaged students (as suggested by the evidence from Li, 2006). In all likelihood, these are precisely the youth who are most at risk of involvement in crime and violence. Therefore, it does not seem reasonable to suppose that the response of crime to CCTs would be the same when considering younger age groups. If so, the overall impact of the program on crime would fall somewhere in between the 7.6% and 20% mentioned before.

The magnitudes reported here seem plausible. According to the Brazilian Census Bureau, the average per capita income (inclusive of benefits) of *Bolsa Família* recipient households was R$175 per month in 2006 (data for the Southeast of Brazil, the wealthiest region in the country, which includes São Paulo; see IBGE, 2008). These households had on average 4.7 members, implying that *Bolsa Família* transfers would amount to between 14% and 32% of aggregate household income (based on a back of the envelope calculation for, respectively, those with just one teenage child and those receiving the maximum benefit). Just the conditional transfer to youth would correspond to close to 20% of household per capita income for the average family in this group. Jacob and Ludwig (2010), for example, estimate an income elasticity of crime of the order of -0.4 when analyzing a housing voucher program in Chicago.

Finally, looking at the table from the perspective of different types of crime reveals whether impacts are larger on economically motivated crimes or on "behavioral" crimes, shedding light on the mechanism behind the results. There are significant negative impacts of *Bolsa Família* on robberies, violent crimes, and drug-related offenses. Thefts are seemingly unaffected, though this category is poorly measured, suffering from

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$\ln(Crime|BF_{16-17}=22)-\ln(Crime|BF_{16-17}=0)=-0.00356\times 22 \rightarrow \ln\left(\frac{(Crime|BF_{16-17}=22)}{(Crime|BF_{16-17}=0)}\right)=-0.080 \rightarrow \frac{(Crime|BF_{16-17}=22)}{(Crime|BF_{16-17}=0)}=$ 0.923.

serious underreporting problems. Point estimates are larger for drug crimes, but the recorded incidence of these crimes is much lower than that of robberies (3 per school per year, as opposed to 433). This implies that the quantitative impact of the CCT on all crimes comes almost exclusively from the effect on robberies. There is also a negative impact on violent crimes, but the coefficient is smaller in magnitude and only borderline statistically significant (violent crimes are also quantitatively much less important than robberies).

Overall, the evidence suggests that economically motivated crimes (robberies) are the main driving force in the quantitative response of crime to CCT coverage. Still, given the evidence on the response of violent crime and drug-related offenses, one cannot entirely rule out some effect through peer groups and social interactions, or an indirect income effect through changed household routines (as suggested by Heller et al, 2010). In the remainder of the paper, we concentrate our robustness tests and additional empirical exercises on robberies, given the dominant role played by this crime category in our estimation.

6.2 Alternative Samples and Functional Forms

Our focus throughout the paper is on high schools, since they contain a higher proportion of teenagers. Still, one might wonder how results would change if we considered other schools. To address this concern, we compute the number of crimes per school considering two alternative sets of schools: only middle schools and middle schools and high schools together. This allows us to check the robustness of the results to different formulations and also to assess how the response of crime varies with the group of children considered. We follow the same strategy described in Table 4 and concentrate on robberies. Results are presented in Table 5. The results across the two alternative sets of schools are qualitatively very similar to those from Table 4, but have some noticeable quantitative differences. First and most importantly, the estimated effect of *Bolsa Família* on crime is stronger for high schools than for the other two sets of schools. Since high schools have older children and adolescents, which indeed should be expected to be more actively involved in crime, this should be expected. The IV coefficient for high schools is at least 200% larger than the analogous one for middle schools and for middle schools and high schools together (column 5 in Table 5). These results confirm our expectation that high schools would be the more adequate unit of analysis. So, in the following subsections, we concentrate our analysis on robberies, considering only high schools, and adopting the specification from column 5 in Table 4.

Following, we address potential concerns related to the functional form of the estimating equation. Our dependent variable is a non-negative integer, and the number of crimes in some areas is small. Count data models are arguably more adequate for estimation in this context, where an excessive number of zeros or over-dispersion may raise specification problems (Wooldridge, 2002). In our case, when robberies are considered, over-dispersion seems to be potentially important, but an excessive number of zeros is not an issue.[8] Still, we re-estimate our benchmark specification using Poisson and Negative Binomial models. For the case of Poisson, we estimate all specifications corresponding to columns 1 to 5 in Table 4. For the Negative Binomial, we estimate only specifications corresponding to the instrumental variable regressions from columns 4 and 5 in Table 4. Given the non-linearity present in all these models, the instrumental variable strategy is implemented using control functions and standard errors are bootstrapped.[9] Coefficients in count models can be interpreted as semi-elasticities, so they can be directly compared to the results obtained before.

The results are displayed in Table 6. The first five columns reproduce the columns from Table 4 using the Poisson instead of the log-linear model. Qualitative results are identical, though point estimates are a little different and vary more from specification to specification. Most notably, the results of the simple Poisson models tend to be larger than the corresponding log-linear specifications, while that of the instrumental variable strategy is somewhat lower. The last two columns present the results when we use the instrumental variable strategy with the Negative Binomial. The Negative Binomial coefficient, which accounts for over-dispersion, is larger than the corresponding Poisson result, coming closer to the point estimate from Table 4. In any case, issues related to functional forms and count data do not seem to influence the main results presented before. So we stick to the log-linear specification because of the limitations associated with instrumental variables in non-linear settings.

6.3 Robustness

We now address some potential concerns related to the benchmark specification. We first analyze whether incorporating the municipal CCT program (*Renda Mínima*) affects the results, then address issues related to pre-existing trends and differential behavior of crime across areas, and finally consider potential spillovers of crime across school neighborhoods. In addition, the municipal CCT program also provides us an opportunity to validate the instrument.

As mentioned in section 2, *Renda Mínima* is a municipal CCT program that sometimes complements and sometimes replaces *Bolsa Família*. We do not have an institutional change analogous to the expansion of *Bolsa Família* coverage that would allow us to estimate the independent causal effect of *Renda Mínima*. Therefore, it is not obvious how the municipal CCT program should be incorporated in our analysis. One alternative would be to sum the total number of children in each school receiving either *Bolsa Família* or *Renda Mínima* to create an aggregate variable for CCT recipients, and then instrument this variable with the same

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[8] The average numbers of crimes around high schools and middle schools are, respectively, 634 and 377. Zeros occur in 0.17% and 0.26% of observations in each case.

[9] In both cases we use the fixed-effect transformation (subtracting the mean overtime for each cross section unit), and bootstrap standard errors re-drawing and re-estimating both the first-stage and the second stage including the estimated residual. Under certain assumptions, this procedure will eliminate the fixed-effect even in non-linear models such as the Negative Binomial and the Poisson. See Cameron and Triverdi (2005).

instrument that we used for *Bolsa Família*. The problem with this procedure is the possibility of double counting, since for some families the municipal program complements the federal one. Another alternative would be simply to include *Renda Mínima* coverage as an additional control in our estimation. The problem with this procedure is that *Renda Mínima* suffers from the same endogeneity discussed before, so that its coefficient – and potentially also the other estimated coefficients – would be biased. Since there is no clearly dominant strategy between these two, we apply both.

In the first three columns from Table 7, we present the results from these exercises. In the first column, we present the result from the first stage regression when we consider the sum of recipients of *Bolsa Família* and *Renda Mínima* as the total CCT coverage per school, while in the second column we present the second stage of this same exercise. Results from both the first and second stages are very similar to those obtained before, though the coefficients are estimated slightly less precisely. In the third column, we present the results when we include *Renda Mínima* as an additional control. Though the coefficient on the municipal CCT appears as positive and statistically significant, the coefficient on the *Bolsa Família* variable (instrumented) remains very similar to that obtained before. Given our previous discussion of the endogeneity of CCT coverage, the positive coefficient on *Renda Mínima* should be no surprise (even more so conditional on *Bolsa Família* coverage). The important point from these first three columns is that, irrespective of how one incorporates *Renda Mínima* in the estimation, there is no substantial change in the main result.

But the municipal CCT program also offers an unusual opportunity for us to validate the instrument. One potential concern is that the initial demographic composition of schools might be correlated with specific neighborhood characteristics, which, in turn, might be associated with the evolution of socioeconomic conditions and crime rates over time. If that was the case, our second and first stage results from Tables 2 and 3 would be spurious, reflecting simply the differential dynamic behavior of neighborhoods with a large number of teenagers around ages 16 and 17. Under this scenario, we should expect our strategy to deliver similar results when, instead of considering the *Bolsa Família* program, we consider the *Renda Mínima*. The latter was not expanded to encompass 16 and 17 year-olds, so our instrument should not work when we consider *Renda Mínima* alone and ignore *Bolsa Família*. If it did work in this setting, it should raise serious doubts regarding our identification strategy.

In columns 4 and 5 of Table 7 we conduct this exercise. We ignore *Bolsa Família* coverage and concentrate solely on *Renda Mínima*. We then run our most complete specification, corresponding to column 5 in Table 3. Column 4 in Table 7 shows that there is no significant correlation between our instrument and *Renda Mínima* coverage. In reality, the point estimate is negative but far from statistically significant. In column 5, we show that the second stage of this exercise also delivers non-significant results, which do not carry much meaning given the failure of the first stage. Using our instrument based on institutional changes to the *Bolsa*

Família CCT program, we are not able to generate exogenous variation or to identify any causal effect of the *Renda Mínima* CCT program, which was not subject to the same institutional change. These results should be expected if the assumptions implicit in our instrumental variable approach were met. This indeed seems to be the case.

The last two columns in Table 7 rule out the possibility that our first stage is capturing unobserved neighborhood characteristics linking initial demographic composition and future expansion of CCT coverage. This addresses concerns regarding spurious correlation in the first stage of our empirical strategy, but these concerns may still be present in relation to our second stage. This could be problematic if areas with a higher initial number of teenagers were intrinsically different from other areas, displaying a particular dynamic behavior of crime. We adopt two different strategies to deal with this possibility. First, we include as additional control a linear time trend interacted with three different socioeconomic variables: 1) the fraction of students lagging behind at school in 2006, a measure of socioeconomic fragility; 2) the penetration of *Bolsa Família* in 2006 at the school, a second measure of fragility; and 3) baseline violence, which allows violence to evolve differently according to how violent the place was to begin with. The three interactions allow areas to deviate differentially from the common non-linear time trend (time fixed-effects) according to their initial characteristics. An alternative would be to interact a linear time trend with the initial demographic composition of schools, but the time dimension in our data is somewhat short, so this alternative would suck up all the variation in our instrument (the four-year panel is not enough to distinguish a linear trend conditional on initial demographic composition from the 2008 institutional change, which was also a function of this same composition). Following, we introduce as additional control a placebo variable reproducing our instrument for 2007. Statistical significance of this variable would suggest that areas with a higher number of students aged 16 and 17 in 2006 were experiencing differential changes in crime already in 2007, before the change in *Bolsa Família* coverage took place. This would raise concerns in relation to the exclusion restriction in our second stage. We also apply a similar strategy restricting the data to 2006 and 2007, and instrumenting *Bolsa Família* coverage with our 2007 placebo variable. In other words, we evaluate whether the same approach applied to the 2008 expansion of the program would lead to any significant impact when the data is restricted to the pre-expansion period and a placebo treatment for 2007 is used.

For each of the strategies outlined, we present results for a reduced form specification (where crimes are directly regressed on the instrument and/or the placebo variable) and the IV specification (where crimes are regressed on *Bolsa Família* coverage, either instrumented correctly or with the placebo). Results are presented in Table 8. Columns 1 and 2 in Panel A show that, when we control for the linear time trend interacted with initial fraction of student lagging behind, the results remain very similar to those in Table 4. In columns 3 and 4 we add the baseline penetration of *Bolsa Família*. The coefficient associated with the trend is positive, meaning that violence decreased less steeply in places that had a greater presence of *Bolsa Família* in the baseline. Results again remain very similar to those in Table 4. Finally, we include a trend interacted with the baseline violence, allowing the outcome variable to have a different linear trend according to its own baseline. As expected, the coefficient associated with the trend is negative and highly significant: violence dropped more where is it was higher in the baseline, reflecting a greater scope for reduction where it was higher in the baseline. Not surprisingly, the reduced-form coefficient on the instrument and the IV coefficient on *Bolsa Família* are smaller, but still significant at the 10% level. Notice that this specification is extremely demanding on the data, since we have three dimensions of socioeconomic conditions interacted with time trends, in a setting where there are only four units of observation across time. That the results are similar in magnitude and statistically significant in this specification is reassuring.

In columns 1 and 2 of Panel B we include the pre-treatment placebos, which are simply an interaction of the initial number of students aged 16 and 17 with a 2007 dummy. In neither case the placebos are statistically significant. In addition, both in the reduced form and in the IV specification, the coefficients of interest become larger when the placebo is included, coming close to the initial results from Table 4. Finally, in the last four columns, we restrict the data to 2006 and 2007, and reproduce our benchmark exercise with the placebo 2007 treatment, including and not including the trends interacted with the baseline characteristics (same as in Panel A). Again, both in the reduced form and in the IV specifications the placebo treatment does not lead to a significant coefficient. In all cases reported in the table, pre-existing trends and differential behavior of crime do not seem to threaten identification. This suggests that the exclusion restriction in the estimation of our second stage is valid.

A final concern addressed here is related to the geographic dimension of the problem and the possibility of migration of youth across school neighborhoods. Areas surrounded by a large number of schools with several students may be subject to more crime because a larger number of students circulate and interact with each other. We address this issue by calculating the total number of students in schools within a certain distance from a given school and including it as an additional control in our benchmark specification. The variables used are the total number of students in schools within two kilometers of a given school, the total number of students within this radius receiving *Bolsa Família*, and the total number of students within this radius receiving *Renda Mínima*. We simply control for the number of CCT recipients in adjoining neighborhoods, and do not instrument these variables, since we would not be able to construct an instrument for *Renda Mínima*. We see these mainly as controls for socioeconomic conditions in the areas surrounding the school. Table 9 presents the result of a regression that controls for these additional variables, each at a time in columns 1 to 3 and all simultaneously in column 4. The point estimate on the *Bolsa Família* variable (instrumented) remains again very similar to that obtained before. So, accounting for the geographic distribution of schools and the possibility of migration of youths across school neighborhoods makes virtually no difference to our initial results. Interestingly, we find that the number of students within two kilometers of a given school has a positive and significant impact on the number of crimes in the school neighborhood. But the impact is quantitatively very small: a 1 standard deviation increase in the number of students within two kilometers (5,963) would be associated with 0.5 more crime per year in the school neighborhood. The variables indicating number of students receiving *Bolsa Família* and *Renda Mínima* in the school neighborhood do not appear as statistically significant and are very small in magnitude in all specifications.

6.3. Channels

The last challenge of this paper is to identify the channels through which *Bolsa Família* affects crime. As mentioned, there are at least three potential channels in this relationship: incapacitation from time spent in school, income effects from government transfers, and social interactions from changed peer group.

One way to assess whether incapacitation seems to be at work is by looking at the effect of *Bolsa Família* on crime by day and time of occurrence. Finding larger effects on school days would in principle suggest an important role for the incapacitation effect, whereby time spent in school "crowds-out" opportunities for delinquency. We classify each crime in our sample as having occurred on a school or on a non-school day, corresponding to days with and without classes (non-school days are defined as weekends, holidays, vacations, etc). We also classify crimes as having occurred in the morning (6:00am-12:00pm), afternoon (12:00pm-6:00pm), evening (6:00pm-12:00am), and night (12:00am-6:00am). In this classification, high school classes are typically held in the morning or in the afternoon. Following, we run our benchmark instrumental variable specification for crimes that occurred in different days and at different times. Results are presented in Table 10.

The overall effect of *Bolsa Família* on crime is similar across school and non-school days, but slightly larger for days with classes. It is also more precisely estimated for days with classes, but, given the relative size of the coefficients, one cannot reject the hypothesis of equality across school and non-school days. This suggests that incapacitation effects are not a relevant channel behind the impact of CCT on crime. In terms of the hour of occurrence, the effects of *Bolsa Família* are concentrated in the morning and the afternoon in days with classes, while there are significant effects only in the evening in days without classes. But coefficients are estimated with less precision when we look at crimes by time of the day, so one should not attach too much weight to these results. In any case, no point estimate displays a positive sign, indicating that there is no evidence of displacement of crime across hours within a day.

Together with the results on types of crime, this evidence suggests that the income effect plays an important role in the relationship between CCT and crime. Still, changed peer groups and reorganization of household routines – due to an indirect income effect – could also be important. Additional research is therefore needed to further clarify the channels through which CCT affect crime.

If the operative channel is indeed income, we would expect the impact of *Bolsa Família* on crime to be stronger when *Bolsa Família* represents a larger fraction of household income. We do not have a measure of income at the school level, but we use the fraction of student lagging behind at school, our measure of socioeconomic fragility, to assess whether this relationship seems to be present in our results.

We estimated the reduced-form including not only the instrument but also the instrument interacted with the number of delayed students (lagging behind in grade).[10] Figure 1 depicts the estimated effect of the instrument according to the proportion of students lagging behind. Results come out as expected: the impact of *Bolsa Família* on crime is driven by schools in arguably poorer areas, where transfers are more important. In fact, the reduced form result is completely driven by schools above the 25th percentile of the proportion of delayed students.

7. Concluding Remarks

This paper combines detailed crime data from the city of São Paulo with information on *Bolsa Família* coverage per school to provide one of the first pieces of evidence on the effect of CCT on crime. We overcome the problem of endogeneity of CCT coverage by exploiting an institutional change to the *Bolsa Família* program that expanded coverage to older age groups. Combining the initial demographic composition of schools with the timing of institutional change, we construct an instrument that identifies an exogenous dimension of variation in the number of children covered by the CCT. This instrument allows us to show that CCT coverage in a school has a negative impact on crime in the neighborhood. The evidence also indicates that the reduction in crime is not concentrated in school days and seems to be mostly driven by economically motivated crimes (robberies). So it is likely that the incapacitation effect from time spent in school is not a particularly relevant mechanism.

Our paper speaks directly to the literature on the impact of CCT by showing that the reductions in poverty and inequality associated with these programs have broader social consequences, which should be taken into account in program design and evaluation. Narrow impact evaluations of these interventions, focused on very specific dimensions, should therefore be taken with a grain of salt. The results also contribute to the literature on determinants of crime, by presenting an additional piece of evidence on the relationship between socioeconomic conditions and crime: *Bolsa Família* has been heralded as an effective and low cost instrument to fight inequality; our results suggest that the reduction in inequality determined by the program was accompanied by reduced crime rates, reinforcing the connection between inequality and crime stressed before in the literature.

[10] We cannot explore the heterogeneity in the complete IV specification, since this would require more than one instrument and would bring in the limitations typical of IV estimation in non-linear settings.

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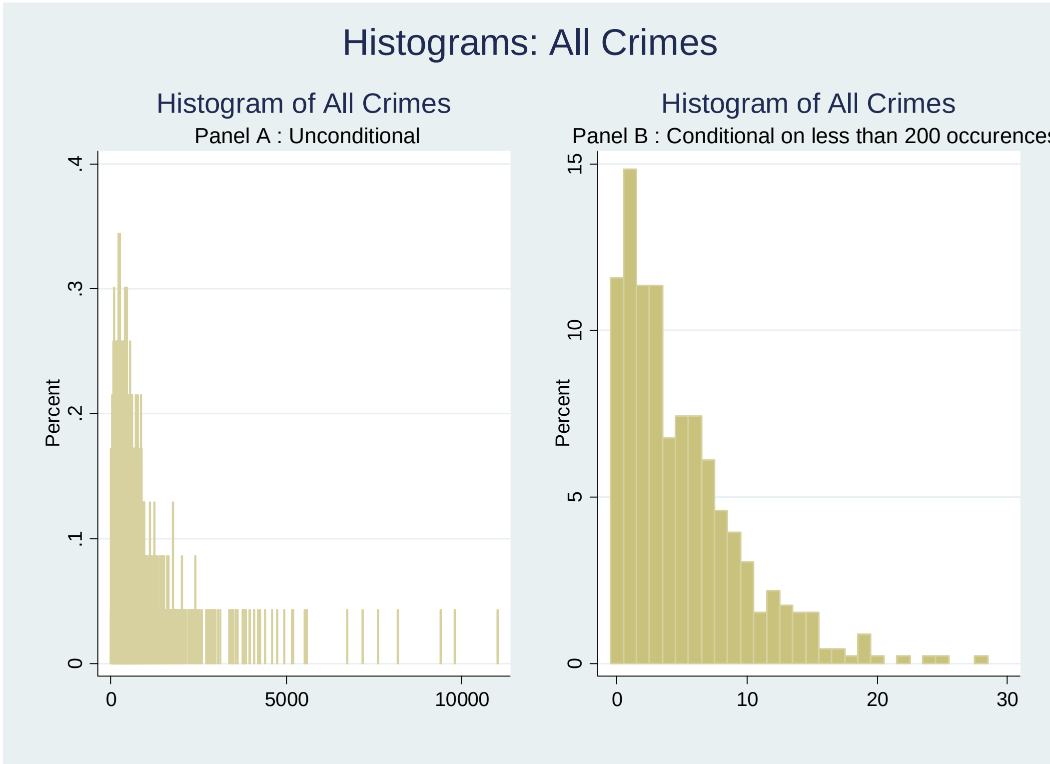
APPENDIX A

Figure A.1 - Distribution of Number of Crimes per School



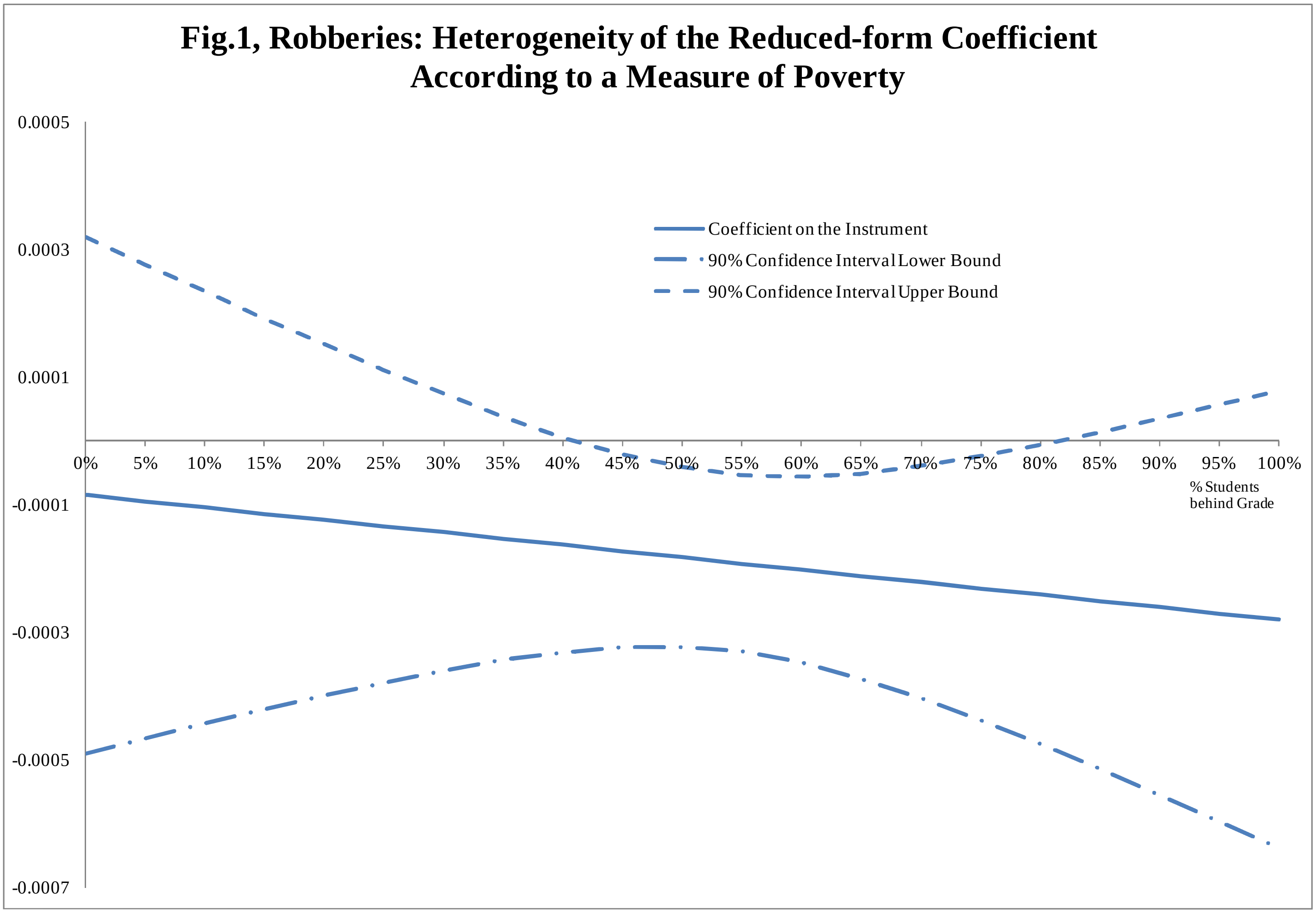


Fig.1, Robberies: Heterogeneity of the Reduced-form Coefficient
According to a Measure of Poverty
0.0005
0.0003
0.0001
-0.0001
-0.0003
-0.0005
-0.0007
Coefficient on the Instrument
90% Confidence Interval Lower Bound
90% Confidence Interval Upper Bound
0%
5%
10%
15%
20%
25%
30%
35%
40%
45%
50%
55%
60%
65%
70%
75%
80%
85%
90%
95%
100%
% Students behind Grade

Table 1 - Summary Statistics: Crime and School Characteristics, High Schools, São Paulo, 2006-2009

Variables:	Mean	Std Deviation	25th percentile	Median	75th percentile	# Schools	# Obs
All Crimes	634.2	761.5	235	447	767	581	2324
Robberies	433.5	530.1	138	291	537	581	2324
Thefts	55.6	139.4	10	23	52	581	2324
Violent Crimes	126.2	104.8	65	103	159	581	2324
Vandalism	11.4	15.5	3	8	14	581	2324
Drug Crimes	2.5	9.6	0	0	1	581	2324
# receiving *Bolsa Família*	124.2	95.6	57	102	168	581	2324
% 16-17 in 2006	0.29	0.11	0.22	0.29	0.35	581	581
# students	1360	499	983	1345	1722	581	2324

Note: Tabulations based on data from Secretaria de Segurança do Estado de São Paulo, Secretaria Municipal de Educação da Cidade de São Paulo, and Ministério da Educação. Only schools that existed in 2006 included in the sample.

Table 2: Unconditional and Conditional Correlation of the Instrument with Observables

	Unconditional	*Conditional*
Total Number of Students	0.2198*	-0.0380
Schooling of Teachers (in years)	0.1018*	-0.0309
Number of Student per Class	0.0304	-0.0459
Sewage in School?	-0.0319	0.0202
Total Number of Teachers	-0.3916*	-0.0749*
Proportion Behind in Grade	-0.1727*	0.0191
Proportion of Non-Whites	-0.0923*	-0.0119
Total Number of 16-17 year-olds	0.3472*	-0.0478

Notes: * = significant at the 10% level. Unconditional: raw correlations. Conditional: correlation with residuals of a regression of the instrument on year and school dummies.

Table 3 - First Stage: *Bolsa Família* Regressed on Instrument, OLS Regressions, High Schools, São Paulo, 2006-2009

	(1)	(2)	(3)
Instrument	-0.00215	0.0333***	0.0552***
	[0.0197]	[0.00981]	[0.00933]
Controls?	No	No	Yes
School Fixed Effects?	No	Yes	Yes
N Obs	2,233	2,233	2,233
R^2	0.081	0.913	0.926
F-statistic of Instrument	0.0119	11.51	35.03

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. There are 2,233 observations in a balanced panel. The unit of observation is school – year.. Dependent variable is the number of *Bolsa Família* recipients in the school. Controls include: year fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for the availability of computers for students, and the proportion of high school students in the school. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009.

Table 4 - Main Estimates: Effect of *Bolsa Família* on Crime, High Schools, São Paulo, 2006-2009

	OLS without controls† or school f.e.	OLS with controls	OLS with controls and school f.e.	Reduced-form (indep. var. = instrument)†	IV with controls and school f.e.
	(1)	(2)	(3)	(4)	(5)
Panel A: All Crimes					
Bolsa Família	-0.00387***	-0.00326***	8.07e-05	-0.000196***	-0.00356***
(or Instrument, in column 4)	[0.000392]	[0.000547]	[0.000281]	[6.70e-05]	[0.00114]
16-17 year students	0.00142***	-0.000979**	8.05e-05	-9.59e-05	-0.000128
	[0.000175]	[0.000435]	[0.000258]	[0.000266]	[0.000254]
R^2	0.222	0.313	0.965	0.965	
Panel B: Robberies					
Bolsa Família	-0.00476***	-0.00391***	0.00028	-0.00020**	-0.00358**
(or Instrument, in column 4)	[0.000477]	[0.000641]	[0.000435]	[8.53e-05]	[0.00140]
R^2	0.135	0.317	0.961	0.961	
Panel C: Thefts					
Bolsa Família	-0.00593***	-0.00429***	0.00108**	-0.000122	-0.00156
(or Instrument, in column 4)	[0.000549]	[0.000767]	[0.000490]	[0.000111]	[0.00228]
R^2	0.165	0.318	0.933	0.932	
Panel D: Violent Crimes					
Bolsa Família	-0.00164***	-0.00165***	0.000151	-0.000169**	-0.00305**
(or Instrument, in column 4)	[0.000321]	[0.000452]	[0.000332]	[7.69e-05]	[0.00129]
R^2	0.050	0.210	0.911	0.911	
Panel E: Vandalism					
Bolsa Família	-0.00405***	-0.00265***	0.000536	-2.33e-06	-4.22e-05
(or Instrument, in column 4)	[0.000388]	[0.000522]	[0.000612]	[0.000152]	[0.00236]
R^2	0.133	0.253	0.805	0.805	
Panel F: Drug Crimes					
Bolsa Família	-0.00030***	-9.14e-05	0.00118	-0.00070***	-0.01270***
(or Instrument, in column 4)	[0.000107]	[0.000216]	[0.000739]	[0.000155]	[0.00303]
R^2	0.586	0.600	0.721	0.724	

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. There are 2,233 observations in a balanced panel. The unit of observation is school – year. Dep. var. is the log of the # of crimes in the school neighborhood. †Only 16-17 year-old students include when the dependent variable is all crimes; for all other crime categories, no controls included. Controls include: year fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for the availability of computers for students, and the proportion of high school students in the school. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. Columns 4 and 5 include all controls and fixed-effects. Sample is restricted to schools that have high-school grades.

†: In the reduced-form, dependent variable is regressed on exogenous covariates and the instrument.

Table 5 – Alternative Samples: Effect of *Bolsa Família* on Robberies, Middle Schools and All Schools Together, São Paulo, 2006-2009

	OLS without controls or school f.e.	OLS with controls	OLS with controls and school f.e.	Reduced-form (indep. var. = instrument)†	IV
	(1)	(2)	(3)	(4)	(5)
Panel A: Middle Schools					
Bolsa Família	-0.00271***	-0.00205***	-2.85e-05		-0.000936***
	[0.000297]	[0.000404]	[0.000269]		[0.000332]
Instrument				-0.000178**	
				[7.25e-05]	
N Obs	3,840	3,840	3,840	3,840	3,840
R^2	0.064	0.173	0.944	0.944	
Panel B: Middle Schools and High Schools Together					
Bolsa Família	-0.00299***	-0.00176***	-4.93e-05		-0.00115***
	[0.000294]	[0.000391]	[0.000265]		[0.000320]
Instrument				-0.000189***	
				[5.98e-05]	
N Obs	3,958	3,958	3,958	3,958	3,958
R^2	0.077	0.186	0.946	0.946	

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school-year. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, and a dummy for availability of computers for students, and the proportion of high-school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. Columns 4 and 5 include all controls and fixed-effects. Panel A includes only schools that have middle-school grades. Panel B includes all schools.

†: In the reduced-form, dependent variable is regressed on exogenous covariates and the instrument.

Table 6 - Count Models: Effect of *Bolsa Família* on Robberies with Alternative Functional Forms, High Schools, São Paulo, 2006-2009

	Poisson without controls or school f.e.	Poisson with controls	Poisson with controls and school f.e.	Reduced-form Poisson (indep. var. = instrument)†	IV – Poisson‡	Reduced-form Negative Binomial (indep. var. = instrument)†	IV – Negative Binomial‡
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bolsa Família	-0.00525***	-0.00601***	-7.70e-06		-0.00205**		-0.00316 ***
	[0.00055]	[0.00188]	[0.00020]		[0.00085]		[0.00071]
Instrument				-0.00011**		-0.00015***	
				[4.72e-05]		[5.18e-05]	
Residual First Stage					0.00224***		0.00343***
					[0.00087]		[0.00077]
N Obs	2,233	2,233	2,233	2,233	2,233	2,233	2,233

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for availability of computers for students, and the proportion of high-school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. Columns 4-7 include all controls and fixed-effects.

†: In the reduced-form, dependent variable is regressed on exogenous covariates and the instrument.

‡: IV Poisson and Negative Binomial implemented via Control Function (standard errors are bootstrapped with 1,000 replications).

Table 7 - Incorporating *Renda Mínima*: Effect of *CCT* on Robberies, High Schools, São Paulo, 2006-2009

	Bolsa Família and *Renda Mínima*			*Renda Mínima*, ignoring *Bolsa Família*	
	1st stage for CCT = *Bolsa Família + Renda Mínima*	2nd stage for CCT = *Bolsa Família + Renda Mínima*	CCT = *Bolsa Família; Renda Mínima* as control	1st stage for CCT = *Renda Mínima*	2nd stage for CCT = *Renda Mínima*
	(1)	(2)	(3)	(4)	(5)
CCT		-0.00393** [0.00169]	-0.00340*** [0.00131]		0.0396 [0.0415]
Renda Mínima as Control			0.00194*** [0.000710]		
Instrument‡	0.0502*** [0.0129]			-0.00499 [0.00585]	
N Obs	2,233	2,233	2,233	2,233	2,233
R^2s	0.925			0.881	

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for availability of computers for students, and the proportion of high school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. All specifications include all controls and fixed-effects.

Table 8 - Accounting for Differential Trends: Effect of *CCT* on Robberies, High Schools, São Paulo, 2006-2009

Panel A	Including Socioeconomic Conditions Trend (*Linear Time Trend* × *Variable*)					
	Reduced-form (indep. var. = instrument)†	IV	Reduced-form (indep. var. = instrument)†	IV	Reduced-form (indep. var. = placebo)†	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Bolsa Família		-0.00348** [0.00155]		-0.00341** [0.00150]		-0.00226* [0.00138]
Instrument	-0.00019** [7.83e-05]		-0.00019** [7.78e-05]		-0.00013* [7.60e-05]	
Trend × % Students Behind in baseline	-0.0552 [0.103]	-0.0291 [0.110]	-0.051 [0.103]	-0.022 [0.107]	-0.0196 [0.102]	0.00389 [0.104]
Trend × Bolsa Família in baseline			0.000259* [0.000138]	0.000466** [0.000181]	0.000175 [0.000138]	0.000308* [0.000174]
Trend × Violence in baseline					-3.37e-05*** [1.12e-05]	-3.67e-05*** [1.32e-05]
N Obs	2,233	2,233	2,233	2,233	2,233	2,233
R^2*s*	0.961			0.962	0.961	

Panel B	Including also 2007 Placebo (*# of Students 16-17 × 2007 Dummy)*		Restricting to 2006-2007 and Using Placebo as Instrument (*Pseudo Treatment in 2007*)			
	Reduced-form (indep. var. = instrument)†	IV	Reduced-form (indep. var. = instrument)†	IV	Reduced-form (indep. var. = placebo)†	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Bolsa Família		-0.00336* [0.00187]		0.00860 [0.01377]		0.00227 [0.00263]
Instrument	-0.000192** [9.61e-05]					
Placebo (# Stud. 16-17 × 2007 D.)	-0.000111 [8.20e-05]	-0.000121 [9.02e-05]	-9.03e-05 [0.000115]		-7.21e-05 [0.000106]	
Trend × % Students Behind in baseline	-0.0118 [0.101]	0.0212 [0.102]			-0.116 [0.342]	0.10048 [0.27854]
Trend × Bolsa Família in baseline	0.000178 [0.000139]	0.000365* [0.000193]			0.000767 [0.000496]	0.00019 [0.00032]
Trend × Violence in baseline	-3.31e-05*** [1.12e-05]	-3.82e-05*** [1.45e-05]			-5.02e-05** [2.53e-05]	-0.00009 [0.00006]
N Obs	2,233	2,233	1,131	1,131	1,131	1,131
R^2*s*	0.962		0.966		0.977	

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for availability of computers for students, and the proportion of high school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. All specifications include all controls and fixed-effects.
†: In the reduced-form, dependent variable is regressed on exogenous covariates and the instrument.

Table 9 - Accounting for Neighboring Schools: Effect of *Bolsa Família* on Robberies, IV Regressions, High Schools, São Paulo, 2006-2009

	(1)	(2)	(3)	(4)
Bolsa Família	-0.00346**	-0.00359**	-0.00358**	-0.00350**
	[0.00136]	[0.00142]	[0.00140]	[0.00140]
# Students within 2 Kms	6.94e-05**			5.11e-05*
	[3.30e-05]			[3.04e-05]
# Students receiving BF within 2 Kms		0.000115		0.000106
		[7.29e-05]		[8.97e-05]
# Students receiving RM within 2 Kms			2.80e-05	-3.96e-05
			[4.47e-05]	[5.75e-05]
N Obs	2,233	2,233	2,233	2,233

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for availability of computers for students, and the proportion of high school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. All specifications are IV estimates including all controls and fixed-effects.

Table 10 - Day and Time: Effect of *Bolsa Família* on Robberies by Day and Time of Occurrence, IV Regressions, High Schools, São Paulo, 2006-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	School Days					Non-School Days				
	All Day	Morning	Afternoon	Evening	Night	All Day	Morning	Afternoon	Evening	Night
Bolsa Família	-0.00325**	-0.00160	-0.00449***	-0.00224*	-0.00286	-0.00347***	-0.00249	-0.00182	-0.00336***	-0.00613***
	[0.00133]	[0.00163]	[0.00163]	[0.00136]	[0.00186]	[0.00118]	[0.00158]	[0.00169]	[0.00122]	[0.00213]
N Obs	2,233	2,233	2,233	2,233	2,233	2,233	2,233	2,233	2,233	2,233

Note: Standard errors in parentheses robust to clustering at the school level. * significant at 10%; ** significant at 5%; *** significant at 1%. Unit of observation is a school. Dep. var. is the log of the # of robberies in the school neighborhood. Controls include: year and school fixed-effects, number of students, number of students between ages 16 and 17, average years of schooling of teacher, teacher-student ratio, students per classroom, a dummy for access to sanitation in the school, % of non-white students, % of delayed students, a dummy for availability of computers for students, and the proportion of high school students. Instrument is the number of 16 and 17 year-olds enrolled in the school in 2006 interacted with a variable equal to 0.5 in 2008 and equal to 1 in2009. All specifications are IV estimates including all controls and fixed-effects..