DISCUSSION PAPER SERIES

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ABSTRACT

Do Compulsory Schooling Laws Always Work? A Study of Youth Crime in Brazilian Municipalities*

We examine if compulsory schooling laws (CSL) necessarily lower crimes. We focus on violent youth crime (homicides by assault and guns) among 15-19 years age group in all Brazilian municipalities over 2000-13, taking advantage of the 2009 Brazilian Constitutional Amendment that required introduction of compulsory high schooling of 15-17-year-olds by 2016. Only about 53% municipalities adopted the Amendment by 2013. Difference-in-difference estimates with municipality fixed effects to account for the endogenous adoption of the Amendment by municipalities show small treatment effects for homicides, but insignificant effects for homicide rates in the full sample. In the absence of any significant increase in income/employment among this age group, we attribute this to the incapacitation effect of CSL, which was, however, weakened by overcrowding in day and night schools in treated municipalities after 2009. In contrast, poorer treated municipalities witnessed increased class size, worse school performance and increased crime too. The crime reduction effects of CSL thus crucially depend on whether/how it affects class size and school quality especially in less promising jurisdictions.

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1. Introduction

Many researchers advocate for the efficacy of compulsory schooling laws (CSL) to fight crime (Lochner and Moretti, 2004; Machin, Marie, and Vujić, 2011; Hjalmarsson, Holmlund, and Lindquist, 2015), dictated by the success of the policy primarily in the US, UK, Sweden and other developed countries. This literature, however, remains largely silent about the preconditions of the success of CSL. Naturally, the success of CSL would depend on whether adequate resources are available so that classes are not over-crowded, students are supervised closely, the teaching and learning environment is not compromised after the introduction of CSL. Otherwise schools can be a place of crime as documented in the neigbourhood effects literature (Jacob and Lefgren, 2003; Billings et al., 2016), highlighting the role of social interactions on crime (e.g., Glaeser et al., 1996). We therefore ask whether CSL will *necessarily* reduce crime and look at one of the least promising jurisdictions, namely, Brazil, a country with one of the highest incidence of crime in general and youth crime in particular (WHO 2016; Murray, Cerqueira, and Kahn, 2013).

Using the 2009 Brazilian Constitutional Amendment 59 that introduced compulsory high schooling of 15-17-year-olds as a natural experiment, we examine the impact of CSL on selected youth crime indices in Brazilian municipalities. In doing so, we depart from the existing literature in a number of ways. We consider *violent* youth crime rather than those pertaining to overall crime as in most existing studies, especially those on Brazil (Chioda et al. 2016). Second, we use municipality-level data from *all* 5560 municipalities rather than specific regions like Sao Paulo (Chioda et al. 2016). This allows us to exploit the striking regional variations in youth crime across Brazil with special focus on the most deprived north-eastern region. Third, we use a variant of CSL which is different from Chioda et al. (2016) who

considered the 2008 reform that extended compulsory schooling of 16-17 year olds under Bolsa Familia programme in Brazil that offered cash transfers for attending schools. Berthelon, and Kruger (2011) considered the impact of an extended school day in Chile.

What was the constitutional Amendment 59 in Brazil? The 2009 Amendment increased the duration of compulsory schooling from 6-14 years to 4-17 years, requiring that the states and municipalities complete the progressive extension by 2016. A key feature of the Amendment was therefore the introduction of compulsory high schooling of 15-17-year-olds from 2010 onwards and there was no cash incentives to do so as in Chioda et al. (2016). We build a unique municipality-level annual panel data from all 5560 Brazilian municipalities over 2000-2013. Using the 2009 Amendment as a natural experiment, we attempt to resolve the circularity between high schooling and youth crime in our analysis in different ways. One key identifying assumption is that the Amendment introduced by the Federal government and implemented by the state authorities (and therefore beyond the influence of municipality government) is unlikely to be correlated with the youth crime indices because the municipal and state government education secretaries implementing the Amendment do not have any influence on the police force generally governed by the state.[1] Second, only about 53% municipalities have adopted the reform by 2013 indicating that the Amendment was not adopted randomly. In order to address the issue of endogenous adoption, we use the municipality fixed effects within a difference-in-differences framework.[2] There may still remain some unobservables that could bias the estimates. We provide evidence to suggest that

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[1] With the exception of the 13% of all municipalities who maintain their own police force. Later we test our results by dropping these municipalities from the estimation sample.

[2] Since the population Census gives data for the following age groups: 5-9, 10-14, 15-19, 20-24 etc. at the municipality level, we focus on 15-19 age group that includes the targeted 15-17-year-olds. Further, given high degree of age grade distortion, late completion of high schools is common (Bruns, Evans, and Luque 2012), many 18-19 year olds still attend high schools in Brazil. We thus define a municipality to be treated if it experienced an increase in mean high school enrolment among 15-19-year-olds over 2010-13[2] relative to that in year 2009 in our sample (see further discussion in Section 3.1).

this is not the case in our sample: (i) pre-Amendment trends in crime are parallel between treated and control municipalities (see Table 4 and Figure A1). (ii) We control for a large set of time-varying municipality characteristics including 15+ illiteracy rate, local GDP per capita, Mayor being aligned with the President, distance of the municipality from the state capital (a proxy for being closely monitored by the state authority implementing the Amendment) to absorb the possible differences between the treatment and control municipalities while determining crime indices. In order to account for any remaining time-varying unobservables. we control for state-level time trends as the Amendment was administered by the state.[3] (iii) We also eliminate the possibility that our results are not confounded by other crime fighting policies adopted by some municipalities/states. (iv) Placebo tests confirm that the treatment effects for targeted 15-19 age group do not hold before the Amendment was made effective or for another age group.

To avoid the problems of unreliable crime data from state secretaries and the Ministry of Security (Cardia, Adorno, and Poleto, 2003), we use the Brazilian Health Ministry data on violence-related deaths among 15-19-year-olds as our crime indicators instead of data. In particular, we consider the number of deaths (by guns, by assaults and also the sum of the two as total violent deaths) as well as the death rates (as share of the 15-19 population) in the municipality.

Ceteris paribus, the treatment has been associated with a small reduction in gun and assault death incidence (measured in logarithm of total deaths on each account) though its effect on death rates (although still negative) remains largely insignificant in our sample. In terms of logarithm of deaths, the treated municipalities experienced 3-4 percentage points lower deaths (relative to control municipalities) due to gun or assault after the adoption of the Amendment. The size of the treatment effect is somewhat smaller when we consider the

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[3] Given the large sample size, Stata did not allow us to include municipality-level time trends.

treatment intensity defined in terms of higher high school enrolment rather than the binary indicator Treated. Results are robust when we drop the 13% municipalities with own policing, Rio municipalities with additional crime fighting policies in place. We document that these treatment effects do not hold for 10-14 age group or for pre-2009 years. Further, we find differential effects of the Amendment in treated poor and non-poor municipalities: compulsory high schooling seems to lower crime only in the non-poor municipalities, but yields no effect or somewhat higher incidence of violent crime rates in the treated poorer municipalities, especially when we focus on municipalities located in the north-eastern region of the country.

What explains these results? Unlike Chioda et al. (2016), there is no evidence of an increase in employment or income among 15-19 year olds in the treated municipalities after the 2009 Amendment; neither there was any evidence of more policing in these treated municipalities. Consequently, we can attribute the observed treatment effects only to the incapacitation arising from the CSL. Further we show that the incapacitation effect was weakened by a sudden increase in class size in both day and night schools in the treated municipalities after 2009 which in turn is likely to affect teaching and learning environment in schools and therefore school performance too. The harmful effects of bigger class size after the adoption of the Amendment were particularly worse in the treated poorer municipalities especially those in the northeastern region, leading to greater crime rates in the region.

Our analysis integrates various strands of the literature. The literature on economic deterrents of crime has heavily focused on the role of education. While most studies on US/UK/Sweden/Australia find a crime reduction effect of CSL (Lochner and Moretti, 2004; Machin, Marie, and Vujić, 2011; Meghir et al., 2012; Hjalmarsson et al. 2015; Beatton et al., 2016), education may also increase the earnings from crime and the tools learnt in school may be inappropriately used for criminal activities (e.g., Levitt and Lochner, 2001). Using US data from Census years 1980-2010, Bell, Costa, and Machin (2016) report different causal effects

of compulsory schooling laws for black and white people, crime reduction effect being greater for the black. While we use similar CSL, but obtain somewhat mixed results from Brazil's emerging economy that instigates us to explore the factors driving the efficacy of CSL.

Focusing on the emerging economy literature, we find different variants of CSL used to study the impact on crime. Berthelon, and Kruger (2011) finds a lower incidence of adolescent risky behaviour after a reform that extended the school day in Chile. Surely an extension of school day for the existing students does not impose any pressure for school places, thus explaining the results. Similarly, Chioda, De Mello, and Soares (2016) report that the 2008 extension of Bolsa Família to include 16-17-year-olds reduced crime in the Sao Paulo municipality in Brazil. While this reform would generate only a marginal increase in the number of 16-17 year olds on Bolsa Familia in each school after the 2008 reform, it still reports a robust crime reduction effect, which is attributed to the cash transfers associated with Bolsa Familia rather than the incapacitation. In contrast to both these studies, the 2009 Amendment required a sudden inclusion of *all* 15-17 year olds in high schools, thus delivering a much larger shock to the already crowded public school system (Cavalcanti, Guimarães, and Sampaio, 2010; Bruns, Evans and Luque, 2012; Nardi et al., 2012) in the country. Our analysis documents that the Amendment has led to an increase in class size in both day and night schools, resulting in a significant deterioration of teaching and learning environment in the short run over 2010-13.

Our results also links closely to the literature on neighbourhood effects that highlights the role of neighbourhood interactions on crime (Glaeser, Sacerdote, and Scheinkman, 1996; Moreira at al. 2013; Damm and Dustmann, 2014). Applying the neighbourhood effects to schools, Jacob and Lefgren (2003) showed that incapacitation does not necessarily lower all sorts of crimes because of lack of supervised school activities. Using public school choice lottery in the US, Deming (2011) showed that better school quality is associated with fewer

serious crimes and fewer days spent in incarceration in the long run. Billings, Deming, and Ross (2016) further showed that concentration of disadvantaged youths in the same school may breed criminal networks within school premises and therefore higher crime. Conversely, crime may decline, where chronically underperforming schools are closed in Philadelphia (Steinberg et al., 2019). Our finding that the introduction of CSL has in fact increased crime rates in the poorer disadvantaged areas after 2009 can be explained by this literature in that crime may breed within overcrowded school premises with poor school quality. We thus integrate the literature on economic deterrents of crime and that on the neighborhood effects/social interactions on crime to show that the success of CSL in reducing crime is crucially contingent on how CSL affects class size and school quality, thus extending the literature on economic deterrents of crime. Although this is a study of Brazil, results from our analysis are likely to shed new light on the potential limits of compulsory schooling in other emerging economies with overcrowded public schools too.

The paper is developed as follows. Section 2 provides the background information, while Section 2 describes the data and explains the empirical strategy. Section 4 discusses the results and the final section concludes.

2. Background

2.1. Public secondary school system in Brazil

The Federal Government of Brazil regulates its educational system through the Ministry of Education. The central government provides each level with funding and educational guidelines, while the individual states are responsible for implementing and enforcing these (with a low degree of decentralisation to the municipalities). High schools are for three years, meant for children aged 15-17 years of age though age-grade distortion is very common in

secondary schools in Brazil. Since the end of November 2009, with the Amendment 59, it is mandatory for 15-17-year-olds to attend high schools.

Most Brazilian schools, especially, the public funded ones, are open in shifts to cope with the numbers of children. Most students attend classes for only five hours a day. Given the pressure on day schools, night schools are common especially at the high school level to permit schooling of working students. Usually, a Brazilian secondary school may run classes in three shifts; morning, afternoon and evening. Support facilities are rudimentary at best, walls are covered with graffiti, with desks too small for teenaged bodies, and the halls are unevenly lighted (Bruns, Evans, and Luque 2012). Teachers, hurrying from their day jobs, may arrive late, and like many of their students may be exhausted.

While Brazil has been able to widen access to primary and high school education, the quality of its schools is low - it remains a public policy challenge to improve it (Kuenzer 2010). Common problems include overcrowding in classes, double school shifts (day and night; the quality of night schools is known to be particularly inferior where grade repetition is common)[4] with shorter school days than in other countries, drug use and anti-social behaviour, exposition of youths to the violence in poor communities (Moreira et al., 2013). Significantly lower PISA (Programme for International Student Assessment) Maths scores for the state sector (relative to those in the private secondary sector) seem to be dictated by the higher % of night schools in the state sector.

Despite high initial enrolments, barely 60 percent of Brazilian youth complete secondary schools, compared to 80 percent in OECD countries. 40 percent of state secondary schools in Brazil qualify as "dropout factories"; in five states, over 50 percent demonstrate this

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[4] About 28% of 9th graders attend night schools because of lack of space in day schools; as high as 33 percent of students enrolled in upper secondary education are enrolled in night schools because of the need to work in 2013. Grade repetition is common among night school students; also poor are over-represented among the repeaters. Source http://educacaosec21.org.br/wp-content/uploads/2013/07/EM-noturno.pdf

abysmal level of performance. Yet, around 44 percent of Brazilian secondary school students are at least two years overage for their grade (OECD 2010). Nothing like these patterns is observed in OECD countries or in other LAC and middle-income countries (Bruns, Evans, and Luque 2012).

Despite ensuring significant expansion of the access to education between 1976 and 2008 (Paim et al., 2011) and reduction in income inequality, incidence of youth crime in Brazil has not shown much signs of remission. Tackling crime, especially youth crime, thus remains a policy priority of the government in Brazil.

2.2. Constitutional Amendments

The 1998 Brazilian Constitution has decentralised primary and secondary education to the municipalities and states, respectively; states were given the responsibility for the high school (to be decentralised to the municipalities latter). The process, however, started only after 1998, following the Law 9424 that originated from the FUNDEF (Fund for Maintenance and Development of Elementary Education and Valorisation of Teaching) in 1996, a source of resources from the central government to finance the municipalities and states. The FUNDEF, however, was replaced in 2006 by the FUNDEB (Fund for Maintenance and Development of Basic Education and for enhancing the value of the teaching profession) following Amendment 53. The replacement allowed increasing the resources available for public education since the major part of localities had not enough resources for funding education.

The Constitution offered successive legal provisions for securing the rights to access primary and secondary education. First, the law 11.274 of 2006 introduced 9 years of compulsory education from age 6-14 years replacing the former 8 years (age 7-14) compulsory programme. Second, the Amendment 59 of 2009 increased the duration of compulsory

schooling to age 4 to 17 years, requiring that the states and municipalities complete the progressive extension by 2016. While states were still in charge of delivering high school education, the process of decentralisation of the high school governance from the state to the municipalities started in the beginning of 2010s. Since our sample of all Brazilian municipalities covers a period of 2000-2013, we can account for the evolving nature of the state and municipal bodies delivering high school education over this period.

Adoption of Amendment 59 necessitated directing additional resources to implement the compulsory schooling among 4-17-year-olds, which meant a change in all budget. Before the introduction of the Amendment 59, the central government was willing to reduce some education resources earmarked for "linked expenses". After the introduction of Amendment 59, the central government altered the policy and instead promised more resources for compulsory basic and high school using the resources set aside for "linked expenses". Accordingly, the municipalities that adopted the policy of mandatory education of 4-17-year-olds are entitled to receive additional federal transfers marked to be spent on education in the post-Amendment years.

To ensure the resources for the implementation, the Federal Government also enforced the Amendment through monitoring. The Public ministry is required to do a Census to audit if public schools are obeying the compulsory schooling rule by providing free education to 4-17-year-olds. The punishment for the municipalities is regulated by the Constitution of 1988 (art. 208, § § 1º e 2º), the Law of guidelines and base of 1996 (Law no 9,394 of December 20 of 1996), and the Law no 12,796 of 2013 that included changes of Amendment 59.

Also, there have been mechanisms to punish the parents (Código Penal Brasileiro (art. 246), Law no 11.114, of 2005, for mandatory primary education among 6 years old; and the new writing of Law no 12.796, of 2013, for mandatory education from 4 years. Also, the Child and Adolescent Statute (Law no 8.069, of 1990) and the Brazilian penal code indicate

punishment for the parents in case of not enrolling their children in schools. The Law no 12.796 of 2013, however, that punish public institutions and parents in case of failure to abide by the Amendment 59 started applying only after 2016.

3. Data and Empirical Strategy

3.1. Data

We compile an annual dataset of 5560 Brazilian municipalities over a period of 2000-2013 from several official sources (see Appendix Table A1 for data sources). Since, the crime data from the Security Ministry is unreliable (Cardia, Adorno, and Poleto 2003), we use the information about violent youth deaths from the Health ministry. Our indices of violence-related deaths among 15-19-year-olds are classified as follows: (a) death by assaults, (b) death by guns. (c) We further aggregate (a) and (b) to construct a composite index of violent deaths incidence. We also convert (a)-(c) into the corresponding violent youth crime rates per 10000 of 15-19 year population. Appendix Table A1 summarises variable definitions and summary statistics.

Since we could not get population and crime data specifically for 15-17-year-olds, we focus on crime numbers and rates among 15-19-year-olds; this is because the Census data on youth population projection ranges from 15-19. The underlying idea is that any effect of the Amendment regarding compulsory high schooling of 15-17-year-olds would be reflected among 15-19-year-olds. Especially given age-grade distortion in Brazil, often many 18-19-year-olds continue to attend high schools (Bruns, Evans, and Luque, 2012).

The 2009 Amendment required the states and municipalities to adopt the reform by 2016 – so all the municipalities did not adopt the reform immediately; rather it was a case of staggered adoption by municipalities unrelated with youth crime. Naturally, excluding 2014-

2016 allow us to distinguish between treated and non-treated municipalities – by 2016 all municipalities will be treated. Since there is no administrative data indicating the timing of the adoption of the Amendment by each municipality, we make use of the target age cohort 15-17 years as laid down by the Amendment to define the treatment and the control groups. We consider the increases in high school enrolment among 15-19-year age group, during 2010-13 (relative to year 2009),[5] if any, to identify the treatment group. Accordingly, we generate a binary variable '*Treated*' that assumes a value 1 if the mean high school enrolment rate among 15-19-year-olds of a municipality during 2010-2013 is greater than its corresponding value in 2009; it is 0 otherwise. Using this criterion, around 53.5% of sample municipalities were identified as treated during 2010-2013. The rest were treated as control municipalities. We also find a direct correspondence between population share of 15-19-year-olds and 15-19 high school enrolment (see columns 1-2 of Table 1), as reflected in the positive and statistically significant Post*population share 15-19 coefficients in the treatment sample. The latter highlights the transparency of the treatment after the introduction of the Amendment 2009.

Table 2 compares the mean high school enrolment, municipal educational expenses and dropout rates after middle schools for treated and non-treated municipalities before and after the Amendment. By definition, the treated municipalities had significantly higher high school enrolment; they also had lower dropout rates after the middle school. Further, the treated municipalities received significantly higher educational funds, as these adopting municipalities were granted federal transfers to enact the Amendment, which again suggests the transparency of the Amendment. Since the expenditure per student is kept similar, mean difference between the treated and non-treated communities is found to be statistically insignificant as expected.

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[5] Note that until 2005, states and municipalities could inflate the student enrolment numbers to get more federal resources. However, as of 2006 the central government changed the way that the municipalities and states need to report the number of enrolments in order to receive federal transfers: in addition to the annual number of students, municipalities were required to report grades and other indicators. subsequently, the reported student numbers kept falling for the next two years, stabilizing from around 2009. This is why we choose year 2009 as a reference year for identifying the treatment municipalities.

Figure 1 (panel a) illustrates the trend in 15-19 enrolments in treated and control groups in our sample over 2000-2013. After the new rule for disclosing detailed school information to secure federal transfers was introduced in 2006 (see footnote 5), high school enrolments gradually stabilized around 2008 for both the treatment and control groups and the trend was parallel between the treatment and control group of municipalities; then, high school enrolment started increasing in the treatment group (relative to the control group) from 2009 onwards.[6] Panel (b)-(d) show the plots of the three crime indices in treatment and control municipalities. Treatment municipalities had lower crime number relative to control municipalities since 2007 onwards and the gap widened from 2009 onwards.

We trace back the evolution of crime-rates among 10-14-year-olds during 2006-09 to those among 15-19-year-olds during 2010-13 after the Amendment. This is because 10-14-year-olds during 2006-09 would turn into 15-19-year-olds during 2010-13. It follows from Figure 2 that 10-14 violent youth crime (sum total of gun and assault deaths) is generally stable in our sample. In general, 15-19 crime is higher than 10-14 crime and also that two trended parallelly in the pre-2010 years. However, from 2010 onwards, 15-19 crime started diverging from 10-14 crime trend noted during 2006-09, thus reflecting the evolution of crime from 10-14 cohort during 2006-09 to 15-19 cohort after 2009.

Since the Amendment guaranteed funding for its implementation, we cross-check if the treated municipalities experienced an increase in education spending after the 2009 Amendment (see column 1 of Table 3). Since we do not have the data on municipality education spending by schooling level, e.g., primary, middle, high, we consider the logarithm of total education spending of the municipality. We find that total municipal education spending increased in the treated municipalities during 2006-08 and then again from 2010

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[6] A part of it can be attributed to the Bolsa Familia expansion to enforce poor 16-17 year olds to be in schools to be eligible for the cash transfers (see Chioda et al. 2016). But we see a much larger effect from 2010 onwards when all 15-17 year olds need to attend high schools; the latter can be attributed to the 2009 Amendment.

onwards. While the 2006-08 increase in education spending in the treated municipalities is attributed to the 2006 reform (that brought age 6 years old children under compulsory schooling), 2010-13 increase in education spending in the treated municipalities is attributed to the 2009 constitutional Amendment introducing compulsory schooling of 15-17-year-olds. The latter confirms that changes in educational spending of the municipality are related to the ongoing educational reforms. Further we test if there is a direct correspondence between increase in high school enrolment and education spending in the treated districts after the 2009 Amendment. Regression with all controls as in Equation (1) shows a statistically significant estimated coefficient of the log of total education spending equal to 0.012 with a t-statistic of 2.218.

3.2. Empirical Strategy

We build our estimation method on the basis of a clear identification strategy on several fronts. First, we use a difference-in-differences model to control for all time-invariant heterogeneity across municipalities, a necessary condition for causal inference. Common determinants of youth crime include childhood deprivation, abuse and psychological disturbances, which are unobservable and remain fairly unchanged over a short period among the targeted group of 15-19 year olds. Second, in order to mitigate the estimation problems posed by endogenous adoption by municipalities, we include municipality fixed effects in all difference-in-difference models that we estimate. We also control for a large number of observed time-varying municipality characteristics to absorb the difference between treatment and control municipalities in our sample as far as possible. There may still remain other time-varying unobservables. Given that the Amendment was implemented by the state authority, we also control for state-level time trends; we were, however, unable to include municipality-level trends (given the large sample). Third, we test that the pre-Amendment trends in crime indices are parallel between treated and control municipalities, an essential condition for consistency

of difference-in-difference estimates. Fourth, we eliminate the possibility that our results are not confounded by other suspects, e.g., crime fighting policies adopted by some municipalities/states. Fifth, we conduct two Placebo tests to examine that the same treatment effects are neither observed for any other age group but 15-19 year olds nor for another fake treatment date. Finally, we also explore the heterogeneous impact, if any, of the Amendment between poorer and richer municipalities in the full sample as well as in the most deprived north-eastern region of the country (see discussion in Section 4.4). The underlying idea is to see what drives the full sample results.

Accordingly, we estimate the following crime Equation (1) to determine C_{it} in municipality i in year t with municipality fixed effects (FE) within a difference-in-differences (DD) framework:

$C_{it} = \alpha_0 + \alpha_1\ Treated_{it} + \alpha_2\ Post + \alpha_3\ TreatedxPost + \alpha'X_{it} + M_i + S_ixT_t + u_{it}$ (Equation 1)

where C_{it} is the index of deaths among 15-19-year-olds - gun deaths, assault deaths, and also violent deaths (sum total of gun and assault deaths). In this respect, we alternatively consider death rates (as shares of 10000 of 15-19 population) and the logarithm of deaths of a particular type. *Treated*=1 if there is an increase in enrolment in i-th municipality in year t where $t>=2010$ and 0 otherwise. *Post* is a second binary variable that takes a value 1 for year>=2010 and 0 otherwise. Inclusion of municipality fixed effects (*M_i)* would control for the unobserved municipality-level factors along with other controls X (see below) that may influence the outcomes of interest as well. Further we include state-level time-trends S_ixT_t.

In order to test the robustness of our treatment group defined by *Treated*, we also construct alternative treatment variables. First, we consider *Treated Median* that takes a value 1 if the *median* high school enrolment rate for the 15-19 age group during 2010-2013 is greater than its corresponding rate in 2009, and 0 otherwise, resulting in 53.7% of the municipalities treated by 2013. Median is considered better because it is less sensitive to outliers. By doing

so, we find a match with about 96% of municipalities using Treated by mean. Second, we construct a net treatment variable, *Treated Net*, that takes a value one if there is an increase in the high school enrolment net of high school dropout during 2010-13 relative to the year 2009, and 0 otherwise. Finally, we construct a *Treatment Intensity* variable which considers the size of the annual increase in enrolment in the treated municipalities during 2010-13 relative to 2009. The latter allows us to identify the marginal effect of increased enrolment on crime.

The set of control variables **X** includes various time-varying municipality characteristics: if mayor is male, if mayor is graduate, size of municipal population, GDP per capita, Gini index, presence of public inter-municipal transport, presence of municipal internet services, number of public health clinics, number of public libraries, municipal police, and number of public sport facilities. As 99% of youth violent deaths pertain to males in Brazil, gender difference is unlikely to arise here. We also control for the possible drivers of early adoption of the Amendment. First, we include if mayor's party is the same as the President's party with the expectation that this would account for the key differences between treatment and control groups. Second, we include 15-plus illiteracy rate expecting that municipality with higher illiteracy may be more likely to adopt the Amendment. Finally, we control for a set of municipality level fixed effects (M_i) that account for the unobserved municipality-level time-invariant factors (e.g., distance of the municipality from the state capital as a proxy for close monitoring by the state authority, proxies for culture including urbanization, religious adherence, as well as social cohesion) that may also influence the selected crime indices. Controlling for all these factors, we take the key explanatory variable *Treated* to be exogenous because it was caused by the 2009 Constitutional Amendment, which was beyond the influence of individual municipalities. The coefficient of interest for us is α_3, which yields the differential effect of high schooling on selected crime indices among treated (relative to control) municipalities, ceteris paribus.

All standard errors are clustered at the municipality level to minimize any autocorrelation of errors across years for a given municipality. Since most high school education is governed at the state level, we also test if our baseline results remain unchanged when we cluster standard errors at the state level in an alternative formulation (see further discussion in Section 4).

3.3. Tests of some identifying assumptions

Before presenting the DD estimates, we examine some identifying assumptions. To this end, we consider the trend in high school enrolment and municipal policing among treated (as opposed to control) municipalities in our sample with a view to eliminate some competing explanations of our results; we also test the assumption of parallel trends which is key to the success of the DD model.

In particular, we regress the treatment dummy *Treated*, year dummies (2001-2013) and the interaction between *Treated* and year dummies on high school enrolment rate of 15-19-year-olds with additional controls for state dummies and state*year dummies. These estimates are shown in Table 3. Results in column 1 of Table 3 show that the interaction dummies *Treated*xyear are statistically insignificant generally for the years 2002-07, thus suggesting that there was no significant pre-reform trend in high school enrolment between treatment and control groups during 2002-07. Evidently, the interaction terms are significant from 2008 onwards: while it is negative and statistically significant for 2008 and 2009, it turns out to be positive and statistically significant from 2010 onwards after treated municipalities started adopting the Amendment, thus highlighting the differential impact of the Amendment on treatment (relative to control) municipalities from 2010 onwards.

Second, we check that municipal policing did not change in the treated municipalities in the post-Amendment years; we do this with a view to eliminate the possibility that increased

policing did not affect crime in the treated municipalities after 2009 Amendment. The municipal police dummy is defined as follows: it takes a value 1 if the municipality is concerned about security and has its own municipal police force; it is zero otherwise. Since security is usually under the state control, having municipal police is not mandatory and only 13% of all sample municipalities maintained their own municipal policing. The last has a distinct role as compared to the state police: usually policing specific buildings and not fighting crime on streets. Municipalities with own policing tend to be more violent (both in terms of violent crime numbers and rates) than those without. These estimates, column 3 of Table 3, suggest that *Treated*Year$_t$*, $t= 2001….2013$ dummies remain insignificant for all the sample years and confirms that changes in municipality policing could be a factor influencing crime indices in the treated communities in the post-reform years.

Since the consistency of DD estimates is based on the assumption of parallel trends, we finally examine if this assumption holds in our sample. Table 4 compares the mean of number of youth crimes between treatment and control groups before and after the Amendment. Panel A shows the comparisons using *Treated* as the treatment group. Mean youth crime numbers were not significantly different in treated and control municipalities before the Amendment; but these mean differences turned out to be significant after the Amendment and the crime rates are generally lower in the treated municipalities (though not always significant). Similar trend is also reflected in Figure 1. Overall, there is no suggestion that more crime-prone municipalities were more likely to adopt the 2009 Amendment. The latter supports our argument that the treatment is largely independent of the crime indices because policing in a municipality is a responsibility of the state secretary of security while the decision to adopt the Amendment is largely determined by the municipality and state education secretary which does not work together with the secretary of security.

Following McCrary (2008) we also ran regressions of selected youth crime indices on

Treated, year dummies and also their interactions TreatedxYeari, i=2001, 2013. Insignificance of the interaction coefficients TreatedxYeari for the pre-Amendment years confirm the parallel trends between the treatment and control municipalities. Estimated coefficients and the associated confidence intervals are shown in Appendix Figure A1 for each year for determining log(gun deaths), log(assault deaths) and log (violent deaths) respectively that depict the estimated interaction coefficients with the associated confidence intervals. Insignificance of the estimated coefficients of the interaction dummies in the pre-2009 years for all these crime indices confirm the presence of parallel trends; the latter suggest that the treated municipalities were generally comparable to the control municipalities in the years immediately before the 2009 Amendment with respect to all these youth crime indices.

4. Empirical Findings and discussion

4.1. Baseline estimates

Our baseline OLS and FE youth crime estimates of equation (1) are summarised in Table 5 respectively in the upper and the lower panels. For each index, we provide two sets of DD estimates – crime *rates* per 10,000 15-19 year olds (columns 1-3) and logarithm of *total number of crimes* (columns 4-6). Ceteris paribus, we focus on the estimated coefficient of α_3 of the interaction term, *Treated* Post*, that captures the effect of high schooling on crime indices in the treated municipalities after the Amendment. We show the ols estimates in the upper panel and the corresponding municipality fixed effects estimates in the lower panel. On the whole the signs and significance of the estimated interaction coefficients are rather comparable for both sets of estimates though their sizes differ. In particular, the estimated coefficients of log crime indices are all negative and statistically significant both for OLS and fixed effects estimates. However, the fixed effects estimates tend to be smaller, indicating the biases of the ols estimates attributable to the potential municipality-level time-invariant omitted variables. In particular, the treated municipalities experienced 3-4 percentage points lower crime related

deaths due to gun or assault after the adoption of the Amendment. Looking at the crime rate indices, however, we find that treatment effect is weaker in that the effect is only significant for the violent death rates due to assault (column 2) though the estimated interaction coefficient estimates remain negative for all three rates shown in columns (1)-(3). In particular, we note that the treated municipalities experienced a reduction of 2 assault deaths per 10000 15-19-year-olds.

In our baseline estimates all standard errors are clustered at the municipality level, however, since the high schooling is largely provided by states, we also test that the baseline results shown in Table 5 remain robust even when we cluster the standard errors at the state level (see Appendix Table A3).

Taken together, the OLS treatment effects of the Amendment introducing compulsory high schooling are not only small (relative to other available estimates available largely for various OECD countries), but remain rather weak, especially for the youth crime rates indices in Brazilian municipalities. In view of the potential omitted variable bias in ols estimates, we prefer the fixed effects OLS estimates.

To verify the robustness of our treatment effects we employ alternative treatment variables respectively using *Treated Median* (Table 5B, two top panels), *Treated Net* (Table 5B two middle panels), and *Treated Intensity* (Table 5B two bottom panels). All FE treatment estimates for log crime indices confirm the robustness of our baseline estimates shown in Table 5: ceteris paribus, the estimated coefficient of the interaction term is negative and statistically significant in columns (4)-(6), thus suggesting significant crime reduction effect in the treated municipalities after the 2009 Amendment. We get very similar effects when we consider the Treated net variable (see the middle panels, Table 5B). Considering the estimates using treatment intensity, a comparison of Table 5 and 5B estimates reflects that the size of the treatment effect is somewhat smaller (see Table 5B, bottom panels): the larger the increase in

high school enrolment, the smaller is the drop in subsequent youth crime. We attribute it to the problem of overcrowding in classes that may weaken the incapacitation effect of compulsory schooling in municipalities experiencing greater enrolment after the 2009 Amendment (see further discussion in Section 4.3).

Given that high school enrolment data was likely to be inflated before 2006, we also test the robustness of our estimates by dropping observations for the pre-2006 years. The resultant estimates are shown in Table 5C; the upper panel shows the OLS estimates and the lower panel the corresponding municipality fixed effects (FE) estimates. Focusing on the FE estimates, we confirm that these estimates are very similar to those in Table 5. Treated municipalities experienced significantly lower incidence of crime (columns 4-6) after the Amendment; as before, the corresponding estimates remain insignificant when we consider the FE estimates for the crime rates (columns 1-3).

4.2. Eliminating competing explanations

We now ensure that our estimates are not biased because of any confounding events.

First, 13% of the sample municipalities have their own police force which could be correlated with local crime. Column (3) of Table 2 show that the estimated coefficient of Treated*Post is insignificant in determining municipal policing; in other words, there is no evidence to suggest that there has been any change in municipal policing in the treated municipalities after 2009.

Further, we drop these municipalities with own police force from our sample in a bid to remove any potential endogeneity of policing with crime in the municipality. Results shown in Table 6 confirm the similarity of these estimates with those in Table 5: as before, the estimated interaction coefficients are negative and statistically significant in determining logarithm of crime incidence (columns 4-6), but not the crime rates (columns 1-3).

Second, it is necessary to control for possible noises in specific places that may

influence our crime estimates. For decades, many of Rio de Janeiro's favelas have been controlled by gangs of armed drug traffickers. Beginning with the launch of the Police Pacification Unit (UPP for short) that was implemented in Dona Marta in 2008, many of Rio's major favelas had received pacifying police forces, as well as innovative and aggressive actions to deal with the urban cycle of violence. Since there are 21 municipalities drawn from Rio de Janeiro in our sample, we test if our baseline results shown in Table 5 hold after dropping the Rio municipalities from our sample. These estimates shown in Table 7 confirm the similarity with the baseline estimates shown in Table 5.

We also experiment with alternative placebo tests. First we consider a placebo for the years 2006-09 taken together, which are the years before the actual introduction of the Amendment 59. Controlling for all other factors, the estimated interaction coefficients as shown in Table 8 are virtually zero and statistically insignificant too for all youth crime indices. The latter validates that similar treatment effects as shown in Table 5 were not generated for the years before 2009 constitutional Amendment 59.

Second, the Amendment focused on 15-17-year-olds which pertains to 15-19 age cohort in our analysis. We now construct an fake treatment group for the age group 10-14. In particular, we construct a fake treatment group using information on increase enrolment of 10-14 year olds rather than 15-19 year olds as we did for Table 5. Municipality FE results summarized in Table 8B suggest that the fake treatment group pertaining to the 10-14 age cohort failed to generate any effect at all as the interaction term Treated10-14XPost2009 remained statistically insignificant for both crime incidence and crime rates estimates including municipality fixed effects. The latter confirms that the baseline treatment effects observed in Table 5 cannot be generated by considering the fake treatment group, further validating our key results.

4.3. Possible explanations

In this section we consider the possible explanations of the small and weak treatment effects of compulsory high schooling among 15-19 age cohort after the 2009 Amendment, especially for youth violent crime rates among the treated municipalities in our sample.

First, it appears from columns 4 of Table 2 that under-17 employment rates did not increase significantly in the treated municipalities after the Amendment in our sample, thus ruling out the possibility of incentive effects among the target age group that compulsory high schooling would lower crime because it had increased youth employment opportunities in the treated municipalities.[7]

In the absence of an income/incentive effect, the observed reduction in crime, if any, in the treated municipalities after the Amendment can solely be attributed to the incapacitation effect associated with compulsory high schooling. This is supported by the fact that the high school enrolment rate has gone up in the treated municipalities (by definition) only after the Amendment (column (1) of Table 2). Table 1 further shows that the latter has been geared by higher share of the target population (15-17 as per the Amendment) in the municipality (Table 1). In other words, the Amendment has induced more 15-19 year olds to get enrolled and also to attend schools, thus taking them off the streets for the duration of the school. This "incapacitation effect" is likely to reduce crime (but not crime rates).

Further, we envisage that the incapacitation effect associated with compulsory high schooling is likely to be weaker because of the sudden overcrowding in classes in day schools after the introduction of the Amendment, which in turn may put pressure on night school enrolment in the treated municipalities after the adoption of the Amendment. Using the class size information available from INEP (National Institute for Educational Studies and Research "Anísio Teixeira"), we compare the average class size in treated and control municipalities.

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[7] We get similar results for Table 2 when we use the alternative treatment variables *Treated_median* or treated_intensity.

The left panel of Figure 3 shows that the average class size is significantly higher in the treated municipalities that adopted the Amendment, thus implying that the adoption of the Amendment has given rise to larger class sizes in the treated municipalities in our sample.

Panel A of Table 9 summarises the treatment effects on selected school quality indices. This includes (1) class size (number of secondary students per class), (2) number of secondary night school students as share of total secondary students, (3) fail rates at secondary level, and (4) age-grade distortion at the secondary level, which is an index of grade repetition in Brazil. While the treatment effect after the Amendment as captured by the estimated coefficient of the interaction term *Treated*Post* is not significant for (3) and (4) for the full sample, it is significant for (1)-(2) in Table 9. There is, therefore, confirmation that the treated municipalities in the full sample had experienced a significantly higher class size in both day and night schools after the Amendment, which is likely to adversely affect teaching and learning environment in the high school classes; note however that the failure rate is not significantly different in full sample after the Amendment. The evidence from North American studies, in particular, the large state-funded experiments, tend to demonstrate an association between class size and pupil achievement such that pupil attainment rises as class sizes fall though (Finn and Achilles 1999). More importantly, greater class size is likely to breed crime through social interaction as well as conflict if disadvantaged youth come together in the same school after the Amendment (Billings, Deming, and Ross 2016; Steinberg et al., 2019), and also if there are less supervised activities in schools (Jacob and Lefgren, 2003).

Further in an attempt to directly assess the role of overcrowding in classes after the Amendment, we consider the FE estimates of crime (both incidence and rates) for cases when class size is below and above its median value 29 in our sample. These results (see Appendix Table A4) suggest that the treatment effects on crime are positive and statistically significant in columns (1)-(3) when class size is above its median value; but the corresponding treatment

effects turn out to be negative but insignificant when class size is equal to or lower than its median value. As before, these results are significant only for the log crime incidence, but not for crime rates and confirm that the crime reduction benefits of compulsory high schooling are crucially dependent on class size.

4.4. Heterogeneous effects

Finally, we consider the heterogeneous impact, if any, of the Amendment on violent youth crime indices in poor and non-poor municipalities in our sample.

We follow the Health Ministry's definition to classify a municipality to be poor if its income per capita is less than half of the minimum national wage. It is non-poor otherwise. Over 60% of these poorer municipalities are located in the north-eastern region of Brazil, which is particularly an underdeveloped and disadvantaged region relative to the rest of the country.

The estimates summarised in Table 10 highlight the differential effects of the Amendment in poor and non-poor municipalities. Focusing on the fixed effects estimates, the effect of the mandatory high schooling is negative and statistically significant for crime indices in the non-poor municipalities, irrespective of whether we consider crime rates (upper panel) or log of crime indices (lower panel). In contrast, the treatment effect is generally positive though not always significant for the poor municipalities: it is statistically significant for violent death rates only, but remains positive, though statistically insignificant for logarithm of violent crime incidence in poor municipalities, thus highlighting the failure of the Amendment to lower crime in poor municipalities. Taken together, the full sample treatment effects of the Amendment on the selected violent crime indices as observed in Table 5 (baseline models) are essentially driven by the effects experienced by the non-poor municipalities while the Amendment fails to generate similar crime reduction effects among the poorer municipalities in our sample.

Table 11 further shows the crime estimates for poor/non-poor municipalities located in the deprived north-east of the country. Focusing on the preferred FE estimates, the interaction coefficient is positive and statistically significant for all crime rate indicators in the poorer municipalities, after controlling for all other factors; in contrast the interaction coefficient estimates turn out to be positive, but statistically insignificant for the logarithm of crime estimates in the poorer municipalities. For the non-poor municipalities, however, the effect is negative irrespective of the choice of crime indices, but these effects are statistically significant only when we consider violent crime rates.

As with the full sample, we attribute the observed treatment effects for the poorer municipalities to increase in class size and worsening school quality after the Amendment and these problems are worse than in the full sample. First, the right-hand panel of Figure 3 indicates that the class sizes grew larger in the treated poorer municipalities after the Amendment. Second, Appendix Table A2 indicates that poor and non-poor municipalities also tend to differ significantly in terms of selected governance indices. Relative to the non-poor municipalities, poorer ones are less likely to have a municipal education board that monitors performance of schools under their jurisdictions. Poorer municipalities are also less likely to have a safety board that oversees the overall safety and security issues of the municipalities. Third, poorer municipalities (including those in the north-eastern region) also tend to suffer from lower income/employment opportunities and hence lower returns to high schooling, even after the Amendment, thus limiting the incentive effects of the Amendment.

Finally, we conduct a regression analysis to assess the impact of the Amendment on selected school quality indices (e.g., class size in day and enrolment shares in night schools, age-grade distortion and failure rates) in treated poor municipalities, controlling for other factors as before. Results summarised in panel B of Table 9 highlight that the treatment effects on crime (as reflected in the estimates of *Treated*Post*) are positive and statistically significant

in all columns in the poorer municipalities. Thus, the poorer treated municipalities experienced significantly higher classes size, greater night school enrolment rates, greater failure rates and also greater age-grade distortion, thus crowding out the benefits of compulsory high schooling on crime.

If school quality deteriorates in the poorer municipalities after the introduction of the Amendment in 2009, the crime reduction effects of compulsory high schooling arising from the incapacitation effect is likely to be limited, thus explaining the positive (or insignificant) effect of the Amendment on crime indices in our sample. Thus, an important implication of our results is that the effectiveness of compulsory high schooling on crime is crucially contingent on its impact on class size and school quality after the Amendment.

5. Concluding remarks

Using the annual municipality-level data over 2000-2013 drawn from all Brazilian regions, we assess the impact of compulsory high schooling of 15-17-year-olds on selected youth crime indices, exploiting the Brazilian Constitutional Amendment 59 as a natural experiment. The Amendment was adopted in a staggered fashion triggered by higher proportion of 15-19 year olds leading to their greater enrolment over 2010-13. About 53% municipalities adopted the Amendment during 2010-13 and before the 2016 deadline, indicating the aspect of potential endogenous adoption of the Amendment by the municipalities. Also the adoption of the 2009 Amendment was likely to be independent of the violent youth crime indices because policing of municipalities is generally under the jurisdiction of the states and not directly regulated by the municipal government.

We use a difference-in-difference model with municipality fixed effects to exploit the variation in crime (level and rates) across the municipalities and over time to identify its causal impact on selected crime indices, thus minimizing any municipality-level omitted variable bias. These estimates generally indicate a small and rather weak treatment effect of the Amendment

on violent youth crime indices and more so for crime rates. Further analysis shows that the Amendment worked primarily through the process of incapacitation associated with compulsory high schooling of 15-17-year-olds. There is no evidence that the Amendment gave rise to better incentives, income/employment for the youth in the treated municipalities. More importantly, we document that the incapacitation effect of compulsory high schooling is weakened by overcrowding in high schools (both day and night schools) after the Amendment such that the beneficial effect of the Amendment vanishes when class size is greater than its median value. Further, we find a heterogeneous impact of the Amendment on crime in poor and non-poor municipalities: treated non-poor municipalities tend to benefit from the Amendment (both in terms of violent crime incidence and rates) while the poorer ones either experienced significant increases in violent youth crime rates or no effect at all. We document that the latter can be attributed to larger class size in day and night schools that had led to worsening school quality indices and therefore higher crime in poorer municipalities after the 2009 Amendment.

An important finding of the present study is that the success of compulsory schooling is crucially contingent on its effect on class size and school quality in emerging economies. Compulsory schooling laws can ensure youth will stay in school longer and will earn higher wages as adults, and commit fewer crimes only if schools are made to function. An effective implementation of the Amendment would therefore necessitate authorities to consider not just its immediate costs, but also the kind of school infrastructure that needs to be in place if the Amendment is to succeed. It would surely require adequate classroom space as well as quality teachers, among other complementary teaching inputs including class size and supervised activities. Care must also be taken to ensure that schools with large concentrations of poor students especially in deprived areas are not overcrowded and do not end up with lower-quality teachers due to the limited supply of well-prepared new teachers coming into the system or the

migration of best teachers to more attractive school systems after the Amendment.

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Tables

Table 1: 2009 Constitutional Amendment and transparency of the treatment

VARIABLES	(1) Treated	(2) Treated
Population share 15-19	-1.0611***	-1.1897***
	(0.072)	(0.084)
Post 2009	0.0541***	-0.0199*
	(0.010)	(0.012)
Post*Population share 15-19	0.4272***	0.5678***
	(0.097)	(0.100)
Constant	0.4199***	0.6475***
	(0.017)	(0.036)
Other controls	No	Yes
State dummies	Yes	Yes
Year dummies	Yes	Yes
Observations	37,126	33,222
R-squared	0.257	0.299

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Other controls included are mayor gender, mayor graduate, mayor party is the same of President's party, population, GDP per capita, illiteracy rate, Gini index, public inter-municipal transport, municipal internet services, municipal policing, number of public health clinics, number of public libraries. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 2. Treatment and control municipalities: Mean comparisons of depend variables

Variables	2000-2009			2010-2013		
	Treated=1	Treated=0	T-stat	Treated=1	Treated=0	T-stat
Number of enrolments at middle school	2844.86	2587.86	2.09**	2634.91	2248.13	2.11**
Number of enrolments at high school	1600.86	1523.83	0.91	1601.13	1376.45	1.88*
Rate of enrolment at high school and population between 15-19 years old	0.4231	0.4622	-27.83***	0.4755	0.4477	18.40**
Age lag (school delay)	43.29	46.75	-16.72***	33.38	35.72	-9.47***
Share of dropout after middle school	0.00037	0.000442	-6.84***	0.000235	0.000288	-7.30***
Total education spending	2273844	2142740	1.19	3126076	2903411	1.88*
Education spending per student	357,679	349,703	1.41	762,518	793,464	-1.55

Note: Treated=1 are the municipalities that adopted the reforms and Treated=0 are those that did not. Significance level: *** p<0.01, ** p<0.05, * p<0.1

Table 3. Time trend in high school enrolment, municipal policing, and Under 17 employment

Variables	Logarithm of Education expenditure	Enrolment rate for 15-19-year-olds	Dummy of municipal police	Under 17 employment share
Treated	0.0439	-0.0306***	0.0101	-0.0437
	(0.032)	(0.005)	(0.007)	(0.038)
treatx2001	0.0033	-0.0064**	0.0008	
	(0.015)	(0.003)	(0.003)	
treatx2002	0.0013	0.0011	-0.0012	-0.0328
	(0.019)	(0.004)	(0.004)	(0.033)
treatx2003	0.0091	0.0005	0.0015	-0.0288
	(0.015)	(0.004)	(0.005)	(0.032)
treatx2004	0.0192	-0.0017	0.0045	-0.0448
	(0.015)	(0.005)	(0.005)	(0.030)
treatx2005	0.0404**	-0.0015	0.0006	-0.0138
	(0.018)	(0.005)	(0.006)	(0.028)
treatx2006	0.0484**	-0.0007	-0.0031	-0.0350
	(0.022)	(0.005)	(0.006)	(0.027)
treatx2007	0.0407**	-0.0032	-0.0038	-0.0259
	(0.020)	(0.005)	(0.007)	(0.026)
treatx2008	0.0362*	-0.0192***	-0.0060	-0.0246
	(0.019)	(0.004)	(0.007)	(0.024)
treatx2009	0.0296	-0.0236***	-0.0075	-0.0086
	(0.019)	(0.004)	(0.007)	(0.022)
treatx2010	0.0320*	0.0253***	-0.0062	-0.0142
	(0.019)	(0.004)	(0.008)	(0.019)
treatx2011	0.0325*	0.0417***	-0.0045	Dropped
	(0.019)	(0.004)	(0.008)	
treatx2012	0.0302**	0.0498***	-0.0058	Dropped
	(0.015)	(0.004)	(0.008)	
treatx2013	0.0318*	0.0519***	-0.0040	-
	(0.019)	(0.004)	(0.008)	-
Constant	13.6587***	0.2523***	-0.0076	6.2554***
	(0.112)	(0.016)	(0.005)	(0.125)
Observations	78,190	77,180	83,460	55,640
R-squared	0.330	0.253	0.161	0.341

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Other Controls included: state dummies, year dummies, and its interactions. Cluster-robust standard errors by municipality: Significance level: *** p<0.01, ** p<0.05, * p<0.1

Table 4. Mean comparisons of number of youth crime for treatment and control municipalities before/after 2010

	Before 2010			After 2010		
			Treatment by mean			
Variables	Treated=1	Treated=0	Diff	Treated=1	Treated=0	Diff
Assault deaths	2.06	1.94	0.12	1.88	2.69	-0.81***
Gun deaths	1.87	1.91	-0.04	1.75	2.62	-0.87***

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Difference-in-difference estimates for crime indicators in rates and levels (Baseline Model)

	Crime rates (15-19-year-olds) as a share of 15-19 municipal population*10,000			logarithm of number of youth crimes (15-19 years old)		
Variables	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
			OLS			
PostXTreated	-0.1343	-0.2024*	-0.3248	-0.0280**	-0.0315***	-0.0358**
	(0.112)	(0.119)	(0.236)	(0.012)	(0.011)	(0.016)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.095	0.089	0.108	0.493	0.512	0.507
			Municipality Fixed Effects			
PostXTreated	-0.1683	-0.2246*	-0.3707	-0.0336***	-0.0342***	-0.0441***
	(0.112)	(0.120)	(0.238)	(0.011)	(0.010)	(0.015)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.050	0.043	0.059	0.085	0.078	0.087
Number of municipalities	3,307	3,713	3,140	3,307	3,713	3,140

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Controls included are Treated, Post, mayor gender, mayor graduate, mayor party is the same of President's party, population, GDP per capita, illiteracy rate, Gini index, public inter-municipal transport, municipal internet services, municipal policing, number of public health clinics, number of public libraries, state dummies, year dummies, and state*year dummies. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 5B. Difference-in-difference estimates of crime indicators using alternative treatment variable

Variables	Crime rates (15-19 years old) as a share of 15-19 municipal population			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS - Treated Median						
PostXTreated	-0.1423	-0.2282*	-0.4032*	-0.0356***	-0.0377***	-0.0487***
	(0.112)	(0.119)	(0.237)	(0.012)	(0.011)	(0.016)
Municipality FE - Treated Median						
PostXTreated	-0.1818	-0.2576**	-0.4686*	-0.0404***	-0.0403***	-0.0565***
	(0.113)	(0.120)	(0.239)	(0.011)	(0.010)	(0.015)
OLS - Treated Net						
PostXTreated	-0.0688	-0.1546	-0.2075	-0.0278**	-0.0326***	-0.0413***
	(0.112)	(0.118)	(0.237)	(0.012)	(0.011)	(0.016)
Municipality Fixed Effects - Treated Net						
PostXTreated	-0.0890	-0.1557	-0.2054	-0.0294***	-0.0316***	-0.0426***
	(0.113)	(0.119)	(0.241)	(0.011)	(0.011)	(0.015)
OLS - Treated Intensity						
PostXTreated	-0.0688	-0.1546	-0.2075	-0.0278**	-0.0326***	-0.0413***
	(0.112)	(0.118)	(0.237)	(0.012)	(0.011)	(0.016)
Municipality Fixed effects - Treated Intensity						
PostXTreated	-0.0890	-0.1557	-0.2054	-0.0294***	-0.0316***	-0.0426***
	(0.113)	(0.119)	(0.241)	(0.011)	(0.011)	(0.015)

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Cluster-robust standard errors by municipality. Significance level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Other controls are the same as in Table 5.

Table 5C: Crime estimates for 2006-2013

Variables	Crime rates (15-19 years old) as a share of 15-19 municipal population			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
	OLS					
PostXTreated	-0.7021	0.8149*	-1.9151*	-0.1438**	0.0963	-0.2178**
	(0.480)	(0.433)	(1.021)	(0.056)	(0.066)	(0.090)
Observations	25,139	28,255	23,882	25,139	28,255	23,882
R-squared	0.098	0.082	0.104	0.494	0.510	0.502
	Municipality Fixed Effects					
PostXTreated	-0.1423	-0.1662	-0.3286	-0.0262**	-0.0288***	-0.0384**
	(0.134)	(0.144)	(0.283)	(0.011)	(0.011)	(0.016)
Observations	25,139	28,255	23,882	25,139	28,255	23,882
R-squared	0.033	0.027	0.037	0.051	0.043	0.049
Number of municipalities	3,287	3,691	3,120	3,287	3,691	3,120

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Estimates of youth crime indices – Municipalities without own police

Variables	Crime rates (15-19 years old) as a share of 15-19 municipal population			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS estimates						
PostXTreated	-0.0674	-0.1130	-0.1625	-0.0187	-0.0205*	-0.0280*
	(0.121)	(0.130)	(0.258)	(0.012)	(0.011)	(0.017)
Observations	34,164	38,900	32,254	34,164	38,900	32,254
R-squared	0.055	0.055	0.068	0.364	0.390	0.384
Municipality Fixed Effects						
PostXTreated	-0.0843	-0.1290	-0.1729	-0.0190*	-0.0205**	-0.0294*
	(0.122)	(0.131)	(0.260)	(0.011)	(0.010)	(0.016)
Observations	34,164	38,900	32,254	34,164	38,900	32,254
R-squared	0.034	0.031	0.042	0.061	0.058	0.066
Number of municipalities	2,919	3,311	2,758	2,919	3,311	2,758

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Cluster-robust standard errors by municipality. Significance level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7. Estimates of youth crime indices excluding Rio de Janeiro Metropolitan area

Variables	Crime rates (15-19-year-olds) as a share of 15-19 municipal population*10,000			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS estimates						
PostXTreated	-0.1373	-0.2054*	-0.3317	-0.0292**	-0.0324***	-0.0373**
	(0.112)	(0.119)	(0.237)	(0.012)	(0.011)	(0.016)
Observations	41,526	46,578	39,465	41,526	46,578	39,465
R-squared	0.092	0.087	0.106	0.480	0.501	0.496
Municipality Fixed Effects						
PostXTreated	-0.1684	-0.2251*	-0.3708	-0.0339***	-0.0346***	-0.0447***
	(0.113)	(0.120)	(0.239)	(0.011)	(0.010)	(0.015)
Observations	41,526	46,578	39,465	41,526	46,578	39,465
R-squared	0.050	0.043	0.059	0.085	0.078	0.087
Number of municipalities	3,287	3,693	3,120	3,287	3,693	3,120

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1. Other controls are the same as in Table 5.

Table 8. Placebo Test – using fake post treatment years 2006-09

Variables	Crime rates (15-19-year-olds) as a share of 15-19 municipal population*10,000			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS						
Post 2006-09XTreated	-0.0193	-0.0350	-0.0895	-0.0050	-0.0021	-0.0013
	(0.100)	(0.098)	(0.191)	(0.009)	(0.008)	(0.012)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.095	0.068	0.080	0.493	0.512	0.507
Municipality Fixed Effects						
Post2006-09XTreated	-0.0067	0.0232	0.0143	-0.0030	0.0009	0.0004
	(0.099)	(0.107)	(0.205)	(0.008)	(0.008)	(0.012)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.049	0.043	0.058	0.085	0.077	0.087
Number of municipalities	3,307	3,713	3,140	3,307	3,713	3,140

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1. Other controls are the same as in Table 5.

Table 8B. Placebo Test using fake treatment group 10-14-year-olds

Variables	Crime rates (15-19 years old) as a share of 15-19 municipal population			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS						
PostXTreated10-14	0.0703	-0.0396	0.1315	-0.0053	-0.0088	-0.0011
	(0.125)	(0.134)	(0.264)	(0.013)	(0.012)	(0.017)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.095	0.089	0.109	0.494	0.513	0.508
Municipality Fixed Effects						
PostXTreated10-14	-0.0076	-0.1122	-0.0213	-0.0056	-0.0062	-0.0007
	(0.126)	(0.135)	(0.267)	(0.013)	(0.012)	(0.017)
Observations	41,745	46,797	39,684	41,745	46,797	39,684
R-squared	0.049	0.043	0.058	0.085	0.077	0.087
Number of municipalities	3,307	3,713	3,140	3,307	3,713	3,140

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Cluster-robust standard errors by municipality. Significance level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Other controls are the same as in Table 5.

Table 9. Treatment effects on school quality indices

Panel A	All municipalities: school quality at high school level			
Variables	Class size	Share of night students	Fail rate	Age-grade distortion
PostXTreated	0.931***	0.019***	0.189	0.603085
	(0.092)	(0.004)	(0.127)	(0.380)
Observations	44,348	44,317	36,399	62,627
R-squared	0.399	0.19	0.264	0.585
Panel B	Poor municipalities: school quality at high school level			
Variables	Class size	Share of night students in total students	Fail rate- more than 50% night enrolments	Age-grade distortion
PostXTreated	1.103***	0.015**	1.147*	2.194***
	(0.199)	(0.007)	(0.676)	(0.833)
Observations	10,646	10,631	2,030	18,490
R-squared	0.490	0.238	0.240	0.473

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Cluster-robust standard errors by municipality. Significance level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Other controls are the same as in Table 5.

Table 10. Heterogeneous impact on youth crime in poor and non-poor municipalities

Variables	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
Crime rates (15-19 years old) as a share of 15-19 municipal population*10,000						
	Non-Poor			Poor		
	OLS					
PostXTreated	-0.2237	-0.3291**	-0.5475*	0.2848*	0.2440	0.5962*
	(0.139)	(0.148)	(0.295)	(0.171)	(0.185)	(0.362)
	Municipality Fixed Effect					
PostXTreated	-0.2701*	-0.3669**	-0.6182**	0.2788	0.2581	0.6222*
	(0.139)	(0.149)	(0.296)	(0.174)	(0.186)	(0.366)
Logarithm of number of youth crimes (15-19 years old)						
	OLS					
PostXTreated	-0.0375**	-0.0403***	-0.0468**	0.0067	-0.0022	0.0055
	(0.015)	(0.014)	(0.020)	(0.016)	(0.015)	(0.023)
	Municipality Fixed Effect					
PostXTreated	-0.0426***	-0.0426***	-0.0543***	0.0050	0.0050	0.0045
	(0.014)	(0.014)	(0.019)	(0.016)	(0.016)	(0.023)
Observations	28,579	32,038	27,139	13,166	13,166	12,545

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 11. Heterogeneous impact in poor and non-poor municipalities, North-Eastern Region

Variables	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
Crime rates (15-19 years old) as a share of 15-19 municipal population*10,000						
	Non-Poor			Poor		
	OLS					
PostXTreated	-0.4743	-0.6944**	-1.0365	0.4620**	0.4742**	0.9261**
	(0.340)	(0.350)	(0.723)	(0.205)	(0.211)	(0.425)
	Municipality Fixed Effect					
PostXTreated	-0.4610	-0.7350**	-0.9811	0.4590**	0.4715**	0.9375**
	(0.347)	(0.351)	(0.732)	(0.205)	(0.211)	(0.424)
Logarithm of number of youth crimes (15-19 years old)						
	OLS					
PostXTreated	-0.0141	-0.0269	-0.0270	0.0286	0.0245	0.0457
	(0.038)	(0.037)	(0.048)	(0.019)	(0.019)	(0.028)
	Municipality Fixed Effect					
PostXTreated	-0.0192	-0.0390	-0.0369	0.0264	0.0264	0.0445
	(0.036)	(0.035)	(0.046)	(0.019)	(0.019)	(0.028)
Observations	7,054	7,606	6,716	8,534	8,534	8,146

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Figures

Figure 1. 15-19 enrolment and crime rates in treated and control municipalities, full sample

Panel a. 15-19 enrolment rates

Panel b. 15-19 log(gun deaths)

Panel c. 15-19 log(assault deaths)

Panel d. 15-19 log(violent deaths)

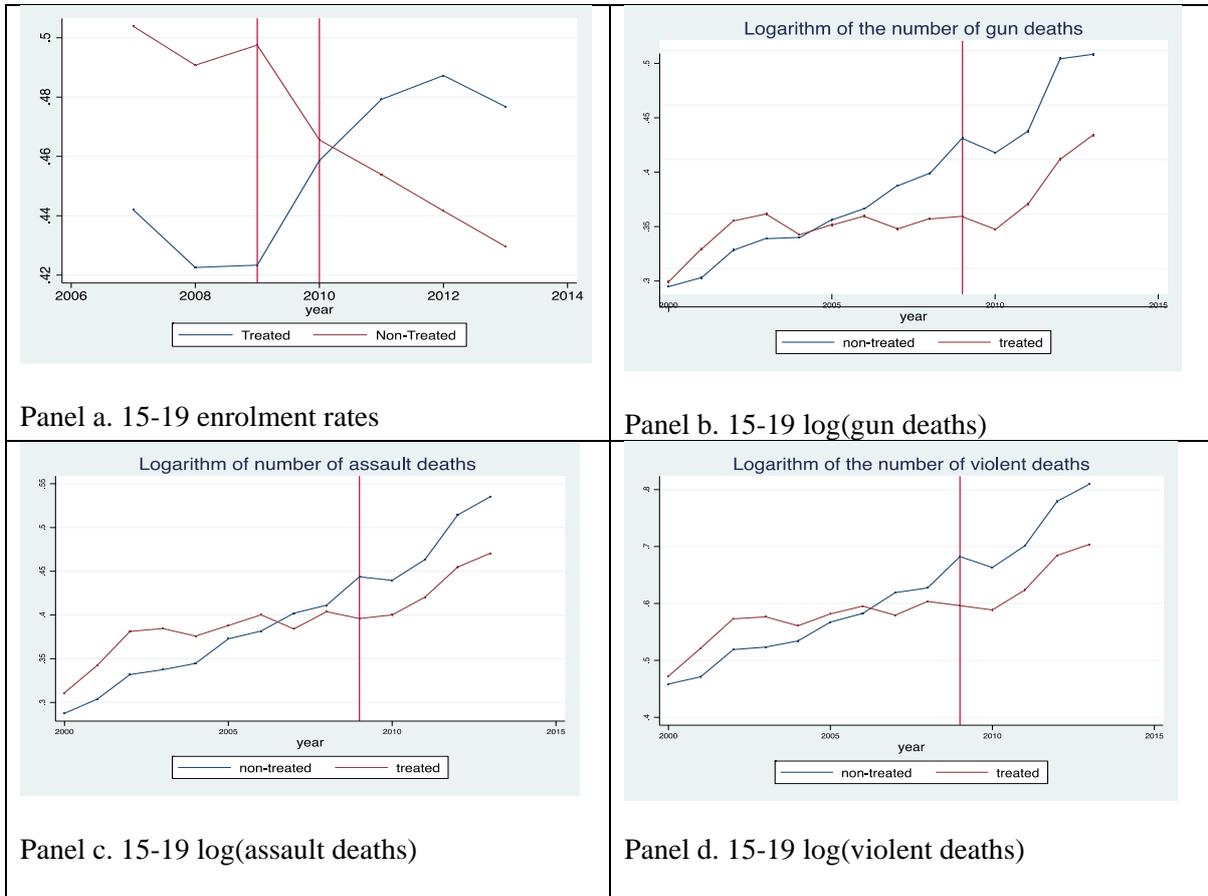Figure 2. Trend in violent youth crimes among 10-14 and 15-19 year olds over time
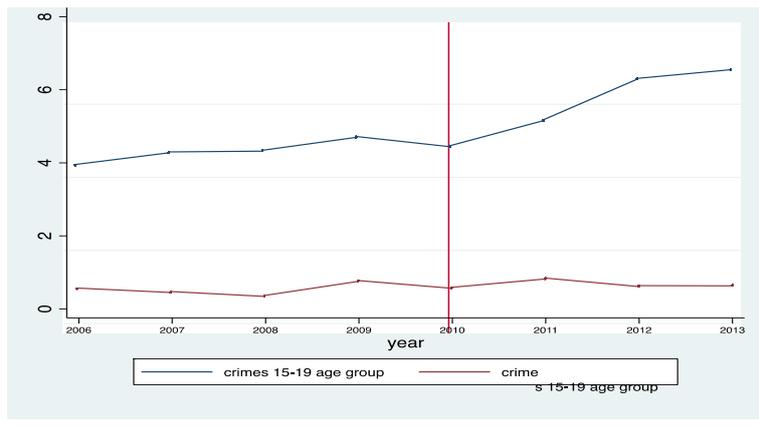
Figure 3. Comparison of class size in treated and control municipalities- full sample and poor regions
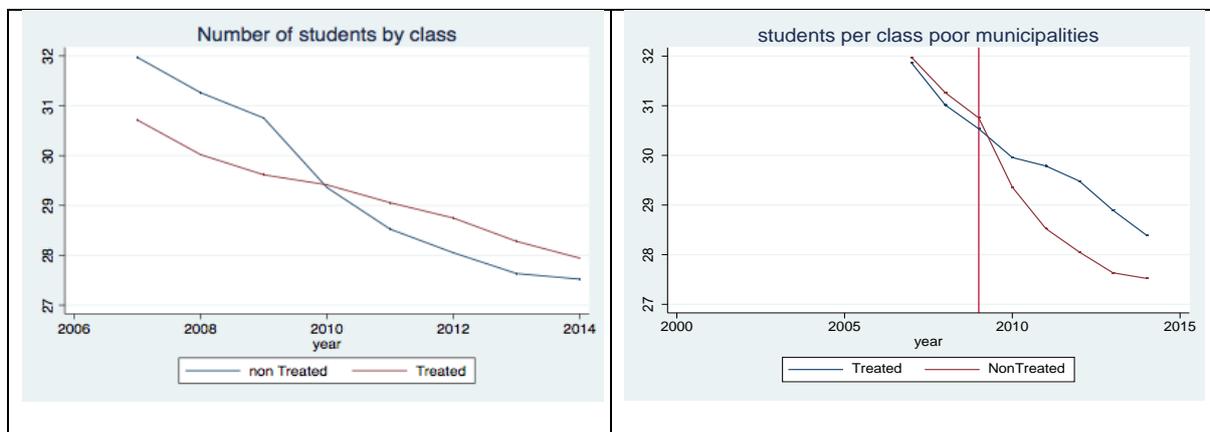
Appendix Figure

Figure A1. Test of parallel trends – coefficient estimates of the interaction term (TreatedxPost) and the associated confidence intervals by year for determining log(crime) indices
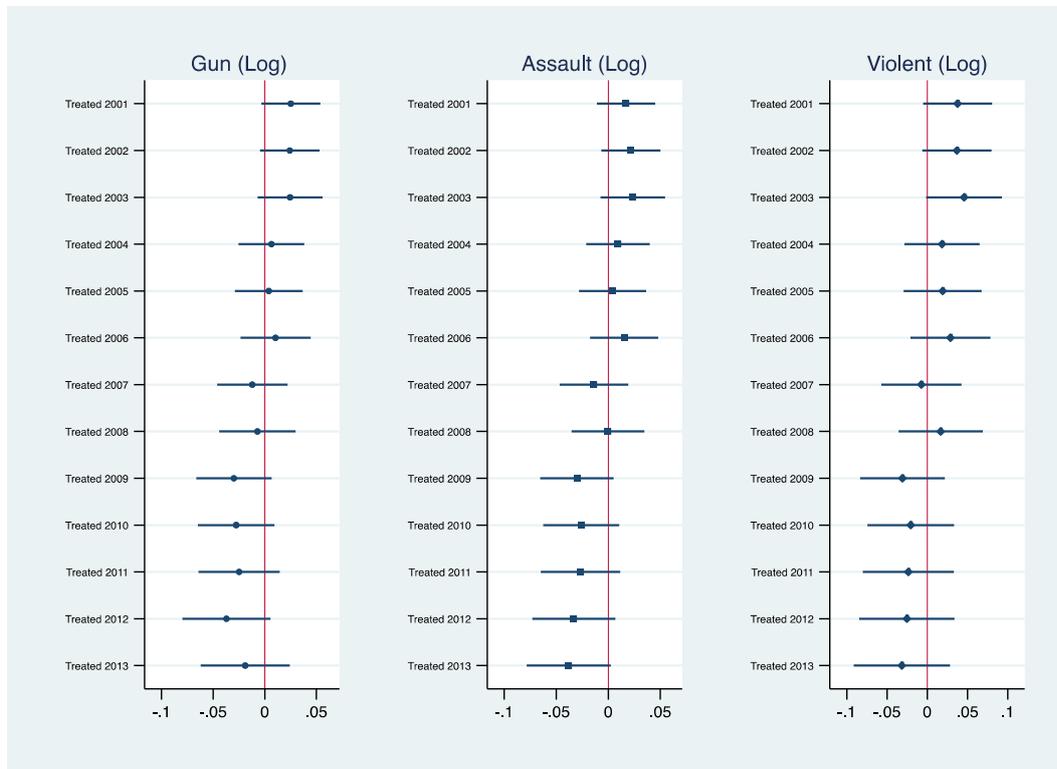
Each column shows the coefficient estimates of TreatedXYear$_t$, t=2001, 2002,…., 2013, (along with the confidence intervals) respectively for determining log(gun deaths), log(assault deaths) and log(violent deaths) from left to right.

Table A1. Variable definitions, Sources and Summary Statistics

Variable	Description	Obs	Mean	Std. Dev.	Source
Rate of gun deaths 15-19	Rate of number of youth deaths by guns and resident youth population (15 to 19 years old)*10,000	46,843	1.9316	4.5066	DATASUS
Rate of Assault deaths 15-19	Rate of number of youth deaths by assault and resident youth population (15 to 19 years old) *10,000	52,640	2.3449	5.1198	DATASUS
Rate of Violent deaths 15-19	Rate of number of violent youth deaths and resident youth population (15 to 19 years old) *10,000	44,453	4.5325	9.1892	DATASUS
Gun deaths 15-19	Logarithm of number of deaths by guns - people from 15 to 19 years old	46,858	0.36625	0.76776	DATASUS
Assault deaths 15-19	Logarithm of number of deaths by assault (aggressions) - people from 15 to 19 years old	52,653	0.396	0.769	DATASUS
Violent deaths 15-19	logarithm of sum of deaths by gun and assault -people from 15 to 19 years old	44,463	0.599	1.00014	DATASUS
Treated	It takes a value 1 if average of number of enrollments in high school per population of 15-19 years old is greater than this rate in 2009; 0 c.c.;	83,460	0.5350	0.49877	INEP
GDP per capita	logarithm of municipal gross domestic product per capita - prices of 2000	75,294	1.47	0.77	IBGE
Mayor education	binary variable: 1 = mayor graduated at a college; 0=the opposite	73,762	0.41	0.49	TCU
Aligned with President	Mayor Party Aligned and Supports President Party	83,880	0.10	0.30	TCU
Gini index	Gini index by municipalities, according to the residents' distribution of income	77,896	0.52	0.08	IBGE
Population	Municipal resident population	80,860	31193	187024	TCU
Population 15-19 years old	15-19 years old resident population	72,259	3129	16577	TCU
Cash transfer program 'Bolsa Família'	Number of people receiving cash transfers Bolsa Família	50,040	2003	6090	Brazilian Social Ministry
Class size	Number of enrolments at high school	44,372	29.3	6.17	INEP
Night school enrolment rate	Number of students in night schools over total of high school students	44,341	0.39	0.24	INEP
Fail rate	Number of all high schools students who failed the grade	44,352	9.78	5.98	INEP

Age-grade distortion	Number of students older than the ideal age for the current grade over total of high school students	42,984	42.48	23.05	INEP
High-school enrolments	Number of enrolments at high school	77,205	1545	9661	INEP
Dropout rate after middle school	Dropout rate at elementary and middle school	44,248	2.83	3.58	INEP
Mayor party	Mayor party	83,880			TCU
President party	president party	83,880			TCU
Poor municipality	Municipality with less than half minimum wage as income per capita	83,460	0.28	0.45	IBGE
Mayor gender	Mayor gender	79,480	2.16	0.55	TCU
Education Board	Municipality has an education board	83,460	0.23	0.42	IBGE
Employment /Income index	Composite index of employment and income	82,999	0.45	0.14	FIRJAN
Education Board	Municipality has an education board	83,460	0.23	0.42	IBGE
Public transport	Existence of public transport to/from other municipalities in 2008	83,460	0.817	0.39	IBGE
Internet services	Public services on Internet of municipal government in 2009	83,460	0.772	0.42	IBGE
Public clinics	Number of public clinics in 2009	83,460	11.371	26.67	IBGE
Public libraries	Number of public libraries	83,460	1.267	2.74	IBGE

DATASUS – Health Informatics Department of the Brazilian Ministry of Health.
INEP – National Institute for Educational Studies and Research "Anísio Teixeira".
IBGE – Brazilian Institute of Geography and Statistics.
TCU - Tribunal de Contas da União - Federal court accounts.
FIRJAN – Federation of industries of Rio de Janeiro.

Table A2. Comparison of selected characteristics across poor and non-poor municipalities

Brazil	Poor	Non-poor	T-statistics
Population	15220	39553	-4.2060***
Logarithm of GDP per capita in Reais of 2000)	8.40	16.40	-17.6002***
Share of Bolsa Família households	0.12	0.07	11.0000***
Distance from capital (km)	276	244	22.3607***
Northeast	0.59	0.21	29.0864***
Gini index	0.52	0.49	-13.1231***
Employment/income index	0.41	0.51	-26.6006***
Mayor professional	0.31	0.35	-2.5085***
Mayor graduate	0.39	0.45	-3.6893***
Class size	30	28	8.3571***
Dropout rate (%)	12.6	11.2	24.3162***
Fail rate (%)	9.6	9.8	-2.9641**
Night students rate (%)	0.405	0.378	6.8656***
Age-grade distortion rate (%)	50.686	39.235	61.6640***
Graduate teachers	0.83	0.92	20.9397***
Have an education board	0.2	0.25	-14.6320***
Have a safety board	0.04	0.09	-26.2972***
Have not been audited	0.96	0.97	-5.1055***
PCA Composite index [1]	-0.06	0.02	25.4693***
Northeast	**Poor**	**Non-poor**	**T-statistics**
Have an education board	0.18	0.26	26.0107***
Have a safety board	0.02	0.10	-37.8170***
Have not been audited	0.97	0.98	-4.8948***
PCA Composite index [1]	-0.08	0.04	-40.6054***
Employment/income index	0.36	0.40	- 23.2809***

We use the principal component analysis to derive the composite index of governance using the information on having an education board, safety board and not being audited. *** p<0.01, ** p<0.05, * p<0.1

Table A3. Difference-in-difference estimates for selected crime indices, standard errors clustered by State

Variables	Crime rates (15-19 years old) as a share of 15-19 municipal population			logarithm of number of youth crimes (15-19 years old)		
	Gun deaths	Assault deaths	Violent deaths	Gun deaths	Assault deaths	Violent deaths
OLS						
PostXTreated	-0.1343	-0.2024	-0.3248	-0.0280**	-0.0315**	-0.0358*
	(0.090)	(0.140)	(0.213)	(0.014)	(0.015)	(0.018)
Observations	41,526	46,578	39,465	41,526	46,578	39,465
R-squared	0.092	0.087	0.106	0.480	0.501	0.496
FE						
PostXTreated	-0.1683	-0.2246	-0.3707	-0.0336**	-0.0342**	-0.0441**
	(0.102)	(0.147)	(0.224)	(0.013)	(0.013)	(0.017)
Observations	41,526	46,578	39,465	41,526	46,578	39,465
R-squared	0.050	0.043	0.059	0.085	0.078	0.087
Number of municipalities	3,287	3,693	3,120	3,287	3,693	3,120

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table A4. Heterogeneous impact on crime indices by class size: Municipality Fixed Effects estimates

	Crime rates					
	Class size<=29			Class size>29		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	gun death	assault death	violent death	gun death	assault death	violent death
Treated*post	-0.2209	-0.2809*	-0.4289	-0.1908	-0.2292	-0.6566
	(0.142)	(0.152)	(0.300)	(0.399)	(0.391)	(0.843)
Observations	33,570	37,297	32,136	8,175	9,500	7,548
R-squared	0.068	0.060	0.079	0.023	0.021	0.027
Number of muni	3,278	3,681	3,116	1,963	2,251	1,826
	log crime indices					
	Class size<=29			Class size>29		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	gun death	assault death	violent death	gun death	assault death	violent death
Treated*post	-0.0142	-0.0113	-0.0327	-0.0359**	-0.0388**	-0.0420**
	(0.018)	(0.017)	(0.030)	(0.016)	(0.015)	(0.021)
Observations	33,570	37,297	32,136	8,175	9,500	7,548
R-squared	0.068	0.060	0.079	0.023	0.021	0.027
Number of muni	3,278	3,681	3,116	1,963	2,251	1,826

Treated=1 are the municipalities that adopted the 2009 Amendment and Treated=0 are those that did not. Post=1 if year>=2010 and 0 otherwise. Other controls are the same as in Table 5. Median class size is 29 in our sample. Cluster-robust standard errors by municipality. Significance level: *** p<0.01, ** p<0.05, * p<0.1.