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Effects of school day time on homicides:
The case of the full-day high school program in Pernambuco, Brazil

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Abstract

This paper estimates the effects of an educational program that lengthen the high school day on the homicide rates of 15-19 years olds. The state government converted existing 5 hours high schools into 10 hours. Starting in 2004, the expansion of the program occurred over several years, and in 2014 nearly 40% of high school enrollment were in these full-day schools. We use the staggered adoption of the program to implement a difference in differences with multiple periods of time. Our estimates indicate the program reduced the municipal homicide rates by 12 percentage points, which translates into a 30-50% reduction in the average homicide rates. The results are robust to placebo tests with different age ranges and the addition of a never treated control group. This paper contributes to the scarce literature of potential interventions to reduce homicides among juveniles, especially in developing countries where researchers have pointed out that a large number of homicides among males and youth is an important public health problem.

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Introduction

The capacity of staying alive may be the most obvious indicative of welfare. It is not a coincidence that the poorest regions, such as Latin America, are also those with highest levels of violence and lowest life expectancy (Briceño-León et al., 2008). In Brazil, the country we analyze in this study, nearly 30,000 youths deaths each year have homicides as the leading cause. The homicide rates of youth in Brazil is the third highest when compared to other 85 countries (Waiselfisz, 2017). Moreover, it does not show large improvements over years. In fact, the likelihood of mortality among youths increased in the last 40 years (Ward et al., 2021; Masquelier et al., 2021). From several perspectives, homicide of Brazilian youth can be considered an epidemic.

Although studies have been documenting these trends, we still know relatively little about the impact of potential social interventions to change these patterns in developing countries (Ward et al., 2021). The literature that studies the effects of educational interventions on crime among youths, for example, is mostly restricted to developed countries.

In this paper we contribute to this literature, presenting evidence on how education affects the homicide rates of youths in high school age. More specifically, we study an intervention that lengthen the school-day time and estimate their effects on homicide rates of males aging 15-19 years old. The intervention (*full-day high school policy* hereafter) took place in a Brazilian state and consisted in the conversion of five hours into ten hours high schools. Although the change in school time is noteworthy, the intervention also changed the curriculum that students received. Aiming to increase the education quality and the engagement of students, policymakers filled the extra time with academic and non academic activities. Also, the state we analyze is known as extremely violent, and homicide rates for young males are among the highest across Brazil.

To study the effects of this intervention, we rely on detailed information on the expansion of the program and annual homicide data for a period of nearly 20 years. The program started in 2004 as a pilot, and the state government expanded it over the years. This

implementation process generated variation over space and time, allowing us to implement a difference-in-differences with multiple time periods (Callaway and Sant'Anna, 2021) and estimate the average treatment effect on treated (ATT). Our main dependent variable is the annual homicide rate of males aging 15-19 years old for each municipality studied. This information was generated using administrative records from local governments. From 2002 to 2018, we count for each of the 271 municipalities used in our analytical sample, the number of deaths by type of death, age of the individuals and year.

Our findings suggest that the full-day high school program had a negative effect on homicide rates of 15-19 years old. The effects are large when compared to the average homicide rates for this age. Over a period of 3 years after the policy starts in the municipality, the homicide rates across 15-19 years old reduced by 32%. If we average the effects over 8 years after the intervention starts in the municipality, our main estimates indicate a a reduction of 50% in the homicide rates.

We perform four checks to evaluate these results. First, we run our main difference in differences analysis using different control groups. We present separately results using the municipalities in Pernambuco that were *not yet treated* as control group and, alternatively, we use municipalities in other states but bordering Pernambuco as a *never treated* control group. Estimate effects are very similar for both specifications. Second, we estimate the effects making the parallel trends conditional on covariates, adding relevant socieconomic characteristics of municipalities in our models. The results are very similar before and after their inclusion, suggesting that our findings are not being driven by wealth or development differences between these municipalities. Third, we present results in a event-study set-up, which allows us to examine the dynamics of the intervention. Results indicate that before the entrance of a full-day school in a municipality differences in homicide rates between treatment and control groups are null. After the start of the intervention, point estimates of the differences between treatment and control groups always appear below zero and drop over time. Finally, we run the same models for a group of individuals (20-24 years old) not

affected directly by the policy which we should not expect the same effects. The estimates for this group indicate null effects.

Considering the full-day high school intervention as comprised of the time students stay in school and the quality of education they receive, there are at least two potential mechanisms we should consider when hypothesizing the directions of the effects of the intervention on homicides among youths. First, keeping students inside school and assuming that schools are safer than the streets, this solution leads to a contemporaneous reduction in victimization by what the literature calls an incapacitating effect (Bell et al., 2022; Lochner, 2010). Staying longer inside school can be particularly effective for youth, since this is a period in their lives when individuals are more prone to engage in criminal activities and, therefore, put themselves in endangering situations (Levitt and Lochner, 2001; Deming, 2011).[1]

Although this channel seems trivial and might explain our findings, we could also think about alternative driving forces behind the change in homicide rates. Since the intervention mandates students to stay longer in schools, it could be that some of the students opted to drop out from high school. If this is the case, and the students that drop out are those with a greater likelihood to engage in criminal or violent activities, then we could expect an increase in the homicide rates within this age range. Therefore, finding negative effects of the full-day high school policy on homicide rates mitigates some concerns that offering costly education interventions like this one can have a reverse effect in inequality.

Second, considering that the full-day high school policy changes the quality of education provided to students, human capital accumulation can also change the violence among youth. This positive shock in human capital leads to potentially better job opportunities with higher wages in the future, making crime, a highly dangerous activity, less attractive (Becker, 1964). It also leads to more opportunities to live outside a dangerous neighborhood, reducing vulnerability and the chances of juvenile imprisonment [2]. It follows that more time in school

[1] For evidence for Brazil, see Carvalho and Soares (2016).

[2] See Eren and Mocan (2021) for an ex-post analysis on the effects of juvenile incarceration in further recidivism and school achievements in the US, and Lotti (2020) for a discussion on how tough should young criminal offenders be punished.

can raise the costs of future punishment, making crime less attractive in the present and keeping youths safe.[3] Altogether, since the policy is still recent and this seems to be a less immediate effect, in this study we consider more likely to be identifying the incapacitating mechanism.

This paper is aligned with a broad literature that relates education with criminal activity in several ways. Fu et al. (2020), for instance, investigates the driven forces that leads teenage students to crime through the development and estimation of an dynamic model in the context of Chile, and find family background as the most important factor in this decision. With counterfactual analysis, the authors suggests that free tuition and improvements in schooling opportunities would generate a relevant gain in preventing teenagers arrests and in enrollment through primary to secondary school.

Our study relates to a the literature that analyzes whether school hours affects violence. Jacob and Lefgren (2003) examine the short-term effects of school attendance in juvenile crime. The authors use as source of exogenous variation days where teaching staff are required to attend work and students are not required to be at school. The authors find a decrease in property crime and an increase in violent crime in such days. The authors argue that the latter effect can be explained due to extended social interactions among crime-prone students.[4] In Luallen (2006), the author uses teacher strikes as a source of exogenous variation in order to measure the incapacitation effect of schools on crime. His findings reinforce the previous results: the absence of the school incapacitation effect reduces property crime and at the same time increases violent crime. The author also shows that these effects stronger highly urbanized areas and for repeat offenders. Fischer and Argyle (2018) examines the adoption of a four-day school week in the state of Colorado. This experiment is particularly interesting since the number of weekly school hours was kept constant between schools who adopted the four-day school week and schools who did not.

[3]See, for example, Lochner and Moretti (2004), Machin et al. (2011) and Hjalmarsson et al. (2015) for supporting evidence on this mechanism.

[4]For a thorough explanation on the social nature of crime behavior see Glaeser et al. (1996).

The authors find that places with schools which adopted the four-day week had a 20% increase in juvenile criminal offenses. Moreover, the effect they find is stronger for property crimes.

More recently, Bell et al. (2022) show that an additional year in mandatory high school education reduced criminality in the US states. The authors uses administrative data for different crimes by age groups from 1980 to 2010, and compared the change of the dropout age regulations across states. Even though the authors acknowledge the long run human capital accumulation mechanism that may impact criminality outcomes, they understand that the act of staying physically in class rooms for longer periods as the main reason for that change – i.e., a dynamic incapacitation effect.

In the context of Brazil, Chioda et al. (2016) analyses the effect of the expansion of *Bolsa Família* conditional cash transfer program in 2008 to cover adolescents aged between 16 and 17 on schools local criminality in the city of São Paulo. The cash transfer for poor families have an additional monetary value for each kid attending school mandatory hours. The authors find a reduction in criminality around schools neighborhoods, specially for property crimes. The authors argue that the driven mechanism for this change was not the incapacity effect, since they don't find significant difference in crimes between school hours and free hours. They suggest the driven force may be the income effect of the program or a peer effect from the hours spent in class. Since *Bolsa Família* covers around 10% of students and the change focused in a small age group, we believe the study of the full-day high school come as an important complement to this paper as it gets to impact 40% of all students enrolled in high school for an entire state.

In this paper we add further evidence to this literature relating education and crime. By taking advantage of the rapid expansion of the full-day high school program and its broad coverage, we evaluate if longer hours in school have an impact on exposure to lethal violence. A more extreme violence event, compared to previous work.

The remainder of the paper is organized as follows. Section 1 provides basic background

information on the state we analyze and the full-day high school program. Sections 2 and 3 describe the data and present the research design to identify the effects of the full-day high school program on homicides. We present the results in Section 4. Section 5 concludes the paper.

1 Institutional background: homicides and the full-day high school program

1.1 Homicides

The program we analyze was part of an educational policy in the state of Pernambuco, Brazil. Pernambuco is in the northeastern region of Brazil, where are located its poorest states. For instance, Brazil has a GDP per capita near US$ 7 thousand, while Pernambuco's is of US$ 4 thousand, 40% lower, and still accounts for the greatest GDP per capita between northeastern states.

In 2019, the average homicide rate in the state was approximately 36 per 100 thousand inhabitants, compared to 21 per 100 thousand in Brazil. It was the 10th highest homicide rate among the 27 Brazilian states. As in other parts of the world, homicides are very concentrated among young men (UNODC, 2019). While 4% of all deaths have homicides as the leading cause in 2019, they were the leading cause for 62% of males that were between 15 and 19 years old. Homicide rates for this age group were near 81.1 per 100 thousand inhabitants in 2018, 2.7 times higher than the average homicide rates in Pernambuco.

The distribution of homicide rates in Pernambuco changed over time without a clear trend, as shown in Figure 1. Between 2002 and 2013 the rates declined by approximately 30% and returned to the same level of 2002 in 2017. These rates vary by geographic area as well. The homicide rates of males that were 15-19 years old in 2019 were 50% higher in municipalities with more than 50 thousand inhabitants than municipalities with less than

10 thousand inhabitants.[5]

[Figure 1 about here.]

1.2 The full-day high school program

The full-day high school program we analyze was a relevant education policy in the State of Pernambuco, Brazil. In 2004,the state's government implemented the pilot of the program which converted regular schools into full-day high schools. The program was exclusive to high schools, and was based in reorganizing the existing structure and providing resources to enable an increase in the the number of school day hours. The program did not open or close any schools, but converted the 5 hours arrangement into 10 hours in some of the preexisting high schools. Furthermore, together with the change in school hours, the department of education delivered changes in: (i) the curriculum of the school; (ii) the workforce (teachers and principals); (iii) the structure of the school building.[6]

According to local authorities, there was a political goal which aimed at providing at least one full-day high school in all municipalities of the state. Moreover, state authorities chose the schools they would convert based on convenience. If one school had the minimum infrastructure required (labs and sport court) and was in a municipality that did not have a full-day school yet, then the school had a higher likelihood to be converted.[7]

In the implementation process, we observe variation in the exposure of students to the

[5]While Pernambuco constantly ranks in official reports as one of Brazil's most violent states in terms of homicide rates, from 2007 and 2011, the state experienced a 26% drop in homicide rates, with an average decrease of 5.25% per year. The state introduced a program to reduce violence: *Pacto pela vida* (Ratton et al., 2014). This program was presented as a coordinated effort to reduce violence, with a focus on improving police activity (Ratton et al., 2014). The program did not target one specific age group and the full-day high school program was not part of this coordinated action from government. In fact, one public manager reported the following in an interview: "Today, Pernambuco has the biggest full-time education programme in Brazil. Nobody talks about that. In fact, not even those involved with the Pact for Life talk about this, and they should" (Ratton et al., 2014).

[6]Rosa et al. (2022) present detailed information on the components of the program.

[7]This information was obtained from interviews with state authorities. Rosa et al. (2022) checked this information and the results match with the authorities' reports. Having a science lab at the school (proxy for school structure) and being a school located in a municipality without any full-day school predict school conversion.

program over time and geographically. The government smoothly scaled up the program across years and municipalities, starting as a pilot in 2004 and after ten years (in 2014) including all municipalities of the state with at least one full-day high school.

[Figure 2 about here.]

More than the coverage in the territory, the full-day high school was a large scale program when it came to the number of students affected by it. Figure 3 shows that in 2014, near 40% of students starting high school attended a full-day school building. This seems a radical change in terms of structure of schooling. For example, studies that analyze the age reforms and effects on crime indicate that increases in education attainment are close to 4% (Bell et al., 2022).

[Figure 3 about here.]

2 Data and summary statistics

This study combines multiple data sources to test whether the full-day high school program had an impact on homicides in the state of the Pernambuco. The primary data source is from DATASUS (*Departamento de Informática do Sistema Único de Saúde*), an administrative dataset from the Ministry of Health. DATASUS contains individual level data of all deaths. From this data we can observe the age of the individual, gender, municipality the individual lived, and the cause of death. For the purpose of this study, we focus on males that were 15-19 years old and that had homicides as the leading cause of death.[8] We aggregate the data by municipality, age group, and year.

To compute the homicide rates, we use the estimated population also available in DATASUS. For each municipality, the Ministry of Health estimates the population by age range

[8]Death causes are classified according to the International Classification of Diseases and Related Health Problems (ICD-10). We use the same ICD-10 codes from Costa et al. (2018), which account for misreporting and undereporting as suggested by Cerqueira (2013).

and gender. We use the estimated population for 15-19 years old from 2002 to 2018.[9] Even though the mandatory age for school attendance is 17 years old in Brazil, we decided to include 18 and 19 years old in the age range as a

Our treatment is defined at the level of the municipalities. In our definition, a treatment starts in a given municipality when the first full-day high school is converted there. To define these dates we use administrative records from the Department of Education in Pernambuco. These records indicate the school which was converted and the year of conversion.

To account for the wealth, development level, size and geographic region of the municipalities, we also supplement our dataset with information on the annual municipal GDP per capita per municipality, the human development index (HDI), and the latitude and longitude of municipalities.

We gather all data for Pernambuco municipalities and for all municipalities that border the state line of Pernambuco, but are in other states jurisdictions (Piauí, Alagoas, Ceará, Paraíba, and Maranhão). Figure 4 shows the municipalities cited.

[Figure 4 about here.]

Finally, our restrictions to data were minimal. We exclude only 3 municipalities from Pernambuco – Recife, Bezerros and Fernando de Noronha. We exclude the first two municipalities because they were the only municipalities starting the program in a given year.[10] And we exclude the third one because it is one island not located in the continental Pernambuco territory. In any case, we show our results are robust to the exclusion of this municipalities by using the municipalities in the state border (Recife is not one of them) and showing results with an alternative treatment definition.

[9] To analyze the effect of an education intervention on violence, it would be natural to use less extreme events than homicides (e.g., crime). Unfortunately, as of this version of the paper, we do not have access to administrative data at the municipal level from the state police of Pernambuco. Previous research consider homicide rates as a reliable crime statistics for developing countries. In Soares (2004), the author argues that in such contexts where less serious offenses can be widespread and nonrandomly underreported, homicide rates can be used as a proxy measure for violent crimes.

[10] For Recife, which is the capital and the largest municipality of Pernambuco, it seems imprecise to define the municipality as treated after the single first school opened, given the municipality proportions.

[Table 1 about here.]

In Table 1 we provide the summary statistics for our main variables, where the unit of observation is a municipality in a year. Homicide rates are the ratio between the number of homicides and the population on a given year, multiplied by 100,000. The share of converted schools is given by the ratio between the students enrolled in schools which were converted into full-day schools and the number of students in the municipality. GDP per capita is in thousands of BRL and employment rate is the ratio between the number of formally employed individuals in the city and the population.

3 Research Design

We define the treatment in the municipality level. A municipality i is treated ($D_i = 1$), when the first full-day high school starts in year g. In years t before this point the municipality is not treated ($D_i = 0$for all $t < g$), and after g the municipality i is stay treated thereafter ($D_i = 1$for all $t \geq g$).

We aim to estimate the effect of introducing the full-day high school on the homicide rates of municipalities. In a potential outcome framework, we could write the Average Treatment Effect on the Treated (ATT) as the difference of treated ($Y_t(1)$) and untreated potential outcomes ($Y_t(0)$), on average, for units in the treated group ($D = 1$), or:

$$ATT = E[Y_t(1) - Y_t(0)|D = 1].$$

For each municipality in an year t, we observe only one potential outcome. Thus, we need to rely on a group of municipalities that do not adopted the program as a comparison group. An option would be to use the canonical difference in differences and, relying on the parallel trends assumption, compare municipalities that have adopted the program with those that did not adopt, before and after the experiment. This framework would fit if we had only two groups of municipalities and the introduction of the program did not vary on time. As

we discussed in Section 1, municipalities introduced the program in different years, starting in 2004 and ending 2014. Because of that, we need a more flexible method to accommodate the staggered adoption. It is common to transpose the difference in differences with multiple periods in a regression fashion, which is called two-fixed effects model. However, doing it introduces bias on the estimates (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

Considering that, we estimate the effects of the full-day high school policy using the difference in differences with multiple time periods introduced by Callaway and Sant'Anna (2021), hereafter C&S. Their difference in differences allow us to use the features of the full-day high school policy implementation in Pernambuco, comparing the homicide rates of municipalities that adopted the policy with those that had not adopted it yet in different years. Furthermore, with the C&S approach we can include covariates and make the parallel trends conditional on them.

The estimation of C&S difference in differences follows a two step process. First, for each group of municipalities that adopted the full-day high school program in year g, we estimate a separated difference in differences, for each year t, using as comparison group municipalities that have not adopted the program yet, which means the control group is changing period by period to accommodate the units that become treated at each t:

$$ATT(g,t) = E[Y_t - Y_{g-1}|G = g] - E[Y_t - Y_{g-1}|D_t = 0, G \neq g].$$

When using the *not yet treated group*, for each group of municipalities treated in year g, the following parallel trends assumption is key for the causal interpretation of our results:[11]

[11]Here we focused the discussion in the parallel trends assumption. In accordance with C&S there are three other important assumptions: (i) irreversibility of treatment; (ii) random sampling; (iii) limited treatment anticipation. We verify (i) in our data, and when a school was converted, it stays as full-day school for the entire period we can observe. We address (ii) by using panel data, as C&S suggest. Finally, we believe that (iii) is addressed by the fact the municipal government had no control about their treatment status, it was the state government that decided the conversion of schools.

$$E[Y_t(0) - Y_{t-1}(0)|X, G = g] = E[Y_t(0) - Y_{t-1}(0)|X, D_s = 0, G \neq g].$$

Second, we aggregate these parameters into different effects. We are particularly interested on the effect of participating in the treatment across all groups, which is given by:

$$\theta_S^O := \sum_{g=2}^{\mathcal{T}} \left[\frac{1}{\mathcal{T} - g + 1} \sum_{t=2}^{\mathcal{T}} \mathbf{1}\{g \leq t\} ATT(g,t) \right] P(G = g).$$

In addition, we are interested on looking at the dynamic effects, or the average effect of participating in the treatment for the group of units that have been exposed to the treatment for exactly e time periods:

$$\theta_D(e) := \sum_{g=2}^{\mathcal{T}} \mathbf{1}\{g + e \leq \mathcal{T}\} ATT(g, g+e) P(G = g | G + e \leq \mathcal{T}).$$

One limitation of using only Pernambuco's own municipalities in our estimates is that all municipalities became treated by 2014. Therefore, when using the *not yet treated* as a comparison group, we mechanically restrict our data for a shorter period of time (2003-2013) as the number of units shrink to the point when all of them are treated. Also, when using covariates in our estimates, we need to address what C&S calls *Overlap* assumption. This assumption requires that for each municipality starting their treatment in period g we can find a similar municipality in terms of propensity score in the period t. Addressing this assumption is challenging when using the *not yet treated* as a control group since the number of municipalities decrease over time. Especially when we use a saturated model, there are few units that overlap in different treatment condition.

To address these issues, we present in our main results a second control group, coined "never-treated" group by C&S, that gathers comparable units not treated during the study time window (2003-2018). We chose to use the municipalities located in other states that border the state limit of Pernambuco as the *never-treated* group. The fact that these municipalities were not targeted by any full-day school policy, the geographic proximity, and

similarities between socioeconomic characteristics of the states ensures the comparability with their neighboring treated counterparts. When using this *never treated* group, we repeated the two steps process described above and the assumptions are analogous to the previous assumptions discussed.[12]

4 Results

Is this section, we show that the full-day high school policy has a negative effect on the homicide rates of males that were 15-19 years old, or in high school age.

Tables 2 and 3 present the main results. Table 2 uses all Pernambuco municipalities and the control group are the municipalities that were *not yet treated* in a particular year. Because we use the *not yet treated* as a control group, the period of analysis is restricted to 2002-2013. Column (1) shows the *Average Treatment Effect on the Treated* (ATT) estimates using no covariates (municipality characteristics). The results indicate the homicide rates drop by 15 p.p. over a period of 6 years. Column (2) shows that the estimates do not change when we include latitude and longitude as covariates. Also, aggregated results come from ATTs to all periods. Column (3) includes the Human Development Index as an additional covariate, the point estimates drop to 12 p.p., and we cannot compute the ATTs for some specific groups and years. In Column (4) we add the GDP per capita as a covariate, the point estimate increase to 13 p.p., and we cannot address the overleap condition for 11% of our estimates. Column (5) adds the log of the population as an additional covariate, the results are close to zero. However, these results are hard to compare with previous specifications. This is because we cannot estimate several ATTs (23%) since introducing population makes impossible to address the overlap condition.[13]

[12]The parallel trend assumption is the weaker since it does not imposes any restriction in pre-treatment trends across groups. (Marcus and Sant'Anna, 2021) highlight this approach present a potential loss of efficiency compared to the not yet treated parallel trend assumption, since it does not exploit restrictions on pre-treatment trends across groups, which leads to a trade-off between efficiency and robustness. Because we present separated analyses using *not yet treated* and *never treated* as control groups, we see both results separately as complementary to check the robustness of our findings.

[13]Giving the number of municipalities and the time of the program, adding covariates to our case is not

[Table 2 about here.]

Table 3 uses a never treated group to present estimates for the effects of the full-day high school program on homicide rates of 15-19 years old males. Doing that, we aim to provide additional evidence about the effects of the program using a more stable comparison groups and compute the ATTs more consistently. Columns (1)-(5) presents the analysis in the same fashion than Table 2. Importantly, our estimates are based on a more extensive period of time (2002-2018) compared to the not yet treated group (2002-2013). Our findings indicate a negative effect of the full-day high school program on homicide rates. The point estimates are stable, ranging from -10.7 to -6.7. Moreover, these aggregated estimates are coming from the stable ATTs. In other words, we do not observe the restrictions on the overleap condition we observed previously on Table 2.

[Table 3 about here.]

4.1 Dynamic effects

In Figure 5 we present the effect of the full-day high school entrance on homicide rates of 15-19 years old males by length of treatment exposure. This analysis uses the *never treated group* as our control group and include all covariates listed in Table 3 - Column (5).[14] Results in Figure 5 have wide confidence interval. This is because we are computing a separated ATT for each period. If we focus our discussion on the point estimates, Figure 5 suggests there is no specific trends in the homicide rates when comparing treatment and control groups before the treatment. Point estimates pre-treatment are very close to zero oscillated between positive and negative.

straightforward. The (Callaway and Sant'Anna, 2021) follows a matching approach in terms of propensity score. In each year, new treated units need to be paired with not yet treated. As long as the time goes, the possibilities to match treated and *not yet treated* decreases. For example, when using the saturated model in Table 2 (Column 5), we can use only one group (municipalities treated in a specific year g) to compute the ATT for three years after the policy.

[14] In the appendix, we present the dynamic effects with all specifications. The conclusions did not change.

After the treatment starts, we observe consistent negative effects. As shown in Figure 5, the point estimates are below zero for all years after the treatment. Moreover, the point estimates suggest moderated dynamic effects. The average ATT for the first four years of the program is -7.3 p.p., between the fifth and eighth year the program started the average ATT changes to -15.8 p.p., and in the average is -20.7 p.p. for the last five ATTs have average.[15] Ignoring the confidence intervals, these point estimates suggest the effects of the program interact with the full-day high school expansion, increasing when a large mass of individuals attend the program.

[Figure 5 about here.]

4.2 Aggregated effects on alternative age range

To show the negative effects of full-day high school on homicide rates of 15-19 years old males are not not capturing other violence directed policies, we estimate the results on one alternative group: males between 20 and 24 years old in the study period. These age group is formed by those who have already leaved the high school when the program started and more general alternative policies aiming to reduce homicides among youth would probably target this age group as well.

[Table 4 about here.]

Table 4 shows the estimate using this alternative homicide rates. The results indicate the homicide rates for 20-24 males after the full-day high school started are not different across treatment and control (*never treated*) groups. Estimates are positive and close to zero, ranging from 0.1 to 1.9 p.p..

[15] Importantly, these differences have wide confidence intervals and they overleap. Confidence intervals for the periods cited are available in the Appendix.

4.3 Incapacitating versus long-term effects

The group of males aging 20-24 years old would be entirely exposed by the full-day high school program years ahead. If we believe the improvement in education quality caused by the program (Rosa et al., 2022) translates into long term opportunity that reduces criminality across years, then we should observe differential effects for the age 20-24 years old across exposure of municipality to the program. Figure 6 presents the estimates by length of exposure to the group of 20-24 years old males. The point estimates do not show a clear pattern. Effects on homicide rates of males aging 20-24 years old of municipalities exposed to the program for more than 10 years are negative (CI -44;-0.04). However, because this heterogeneous analysis include only a subgroup of the municipalities analyzed, we should interpret these estimates cautiously.

[Figure 6 about here.]

5 Discussion

By analyzing a full-day high school policy, this paper presents the first evidence to show that lengthen the school-day affect homicide rates of youths. Focusing on the entrance of full-day high schools in municipalities of Pernambuco (Brazil) from 2006 to 2014, a difference in differences with multiple periods framework is used to show that homicide rates for young men (15-19 years old) fall by around 30%-50%, on average, as a result of this educational reform. Effects are similar when using different control groups. Furthermore, they are larger when the municipality have more time of exposure to the program.

The results are consistent with incapacitation effects since we do not observe the same trends in close non-high school age groups, such as 20-24 men. Nevertheless, further research is needed to understand whether the intervention has long term effects. The time horizon and the aggregated data used in this study are limited to answer whether the effects are only being driven by incapacitating effects. For further research one might explore a direct

connection between the policies and individuals' behavior. This connection can allow to establish more categorically whether students attending full-day high schools reduced their likelihood of committing crime (or being homicides victims) through more consistent long-run effects, like human capital increases and dynamic incapacitation.

When placed into the health public discussion, the findings of this paper is a contribution to better understanding interventions that might mitigate the mortality among youth in developing countries, specially in Latin America. Homicides of young men are a serious concern for policymakers and researchers in Brazil and particularly in the Brazilian state we analyzed. This is because homicide rates among these group is persistently high over the years. Therefore, impact evaluations that highlight preventive policies, such as the full-day high school program, might be a valuable asset to understand the benefits of such policies.

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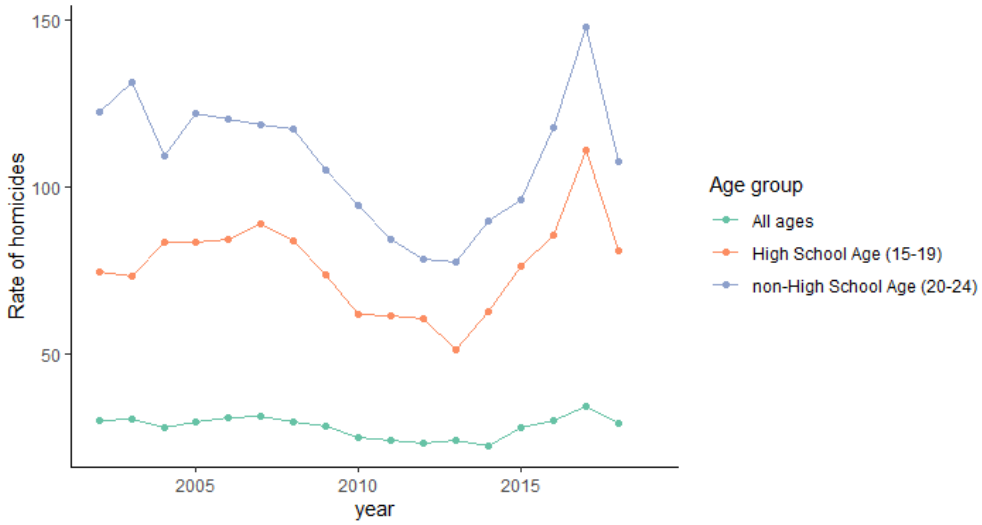
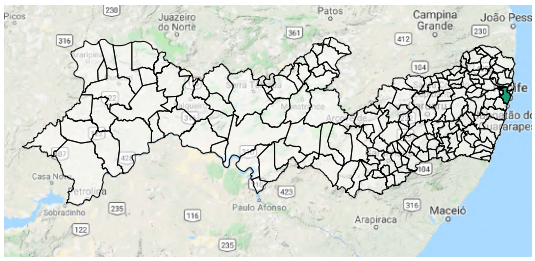
and national mortality among young people aged 10–24 years, 1950–2019: a systematic analysis for the global burden of disease study 2019. *The Lancet 398*(10311), 1593–1618.

Figure 1: Homicide rates in Pernambuco state between 2002 and 2018 by age group (in 100,000)

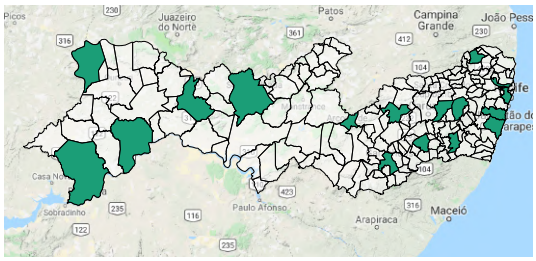


Source: DATASUS homicides and population data.

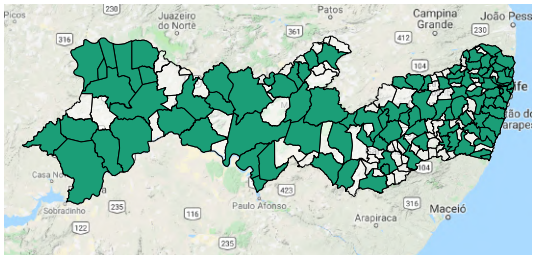
Figure 2: Geographical expansion of the full-day high school program in Pernambuco, 2004-2014



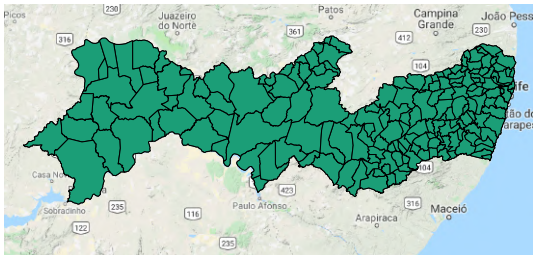
(a) Municipalities with a full-day school in 2004



(b) Municipalities with a full-day school in 2007



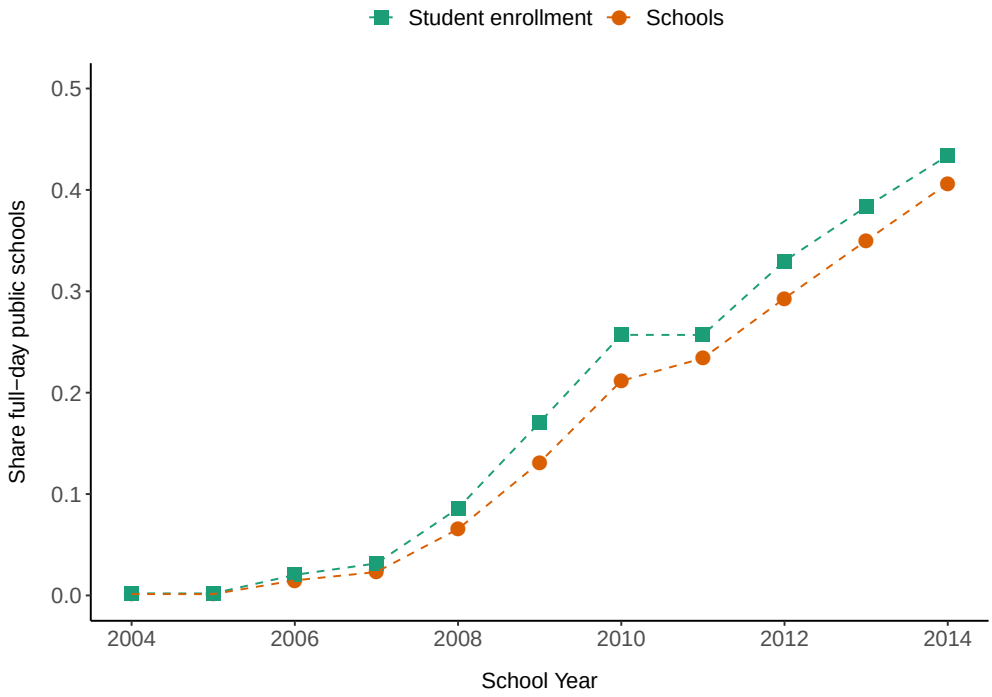
(c) Municipalities with a full-day school in 2010



(d) Municipalities with a full-day school in 2014

Notes: These maps report the expansion of the full-day public high school program across Pernambuco's municipalities. Black borders indicate municipality limits. Dark (green) colors indicate the municipalities with at least one full-day school. Source: Official documents from Pernambuco state.

Figure 3: Participation of full-day in the enrollment of public high schools, 2004-2014



Source: Rosa et al. (2022)

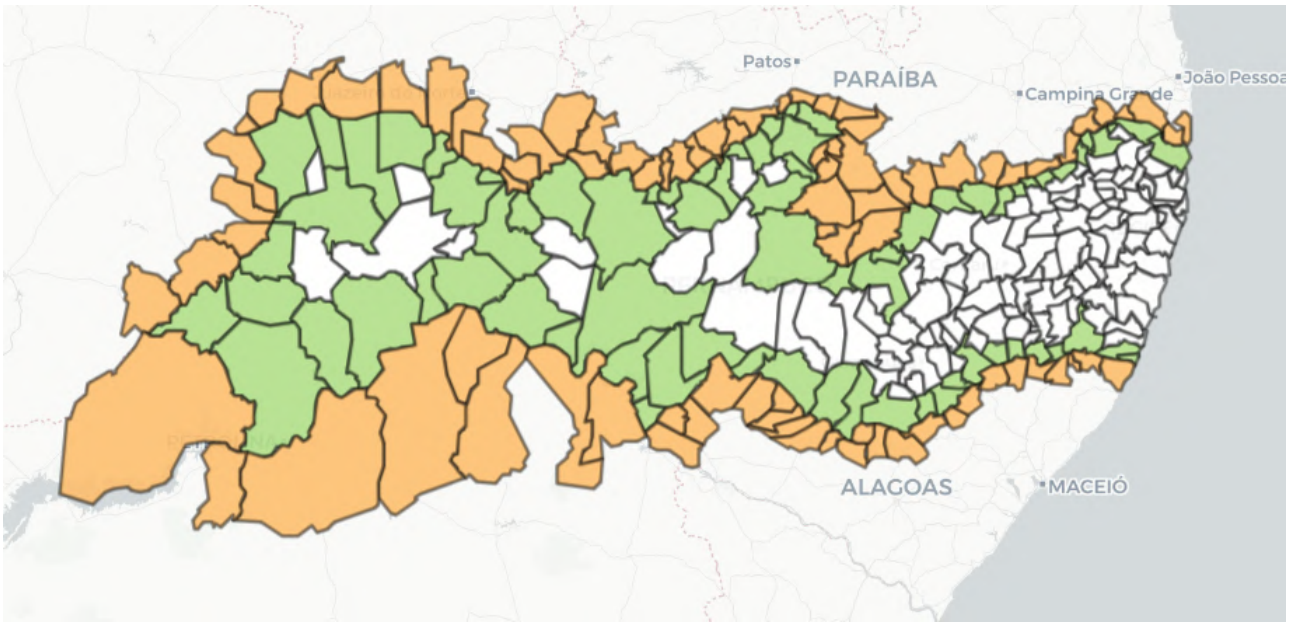


Figure 4: Pernambuco and out of state municipalities

Notes: This figure presents the municipalities used in the analysis. White and green municipalities are part of Pernambuco jurisdiction. Green municipalities are in the border of Pernambuco. Orange municipalities are the *never treated* control group, they are out of Pernambuco and bordering Pernambuco state.

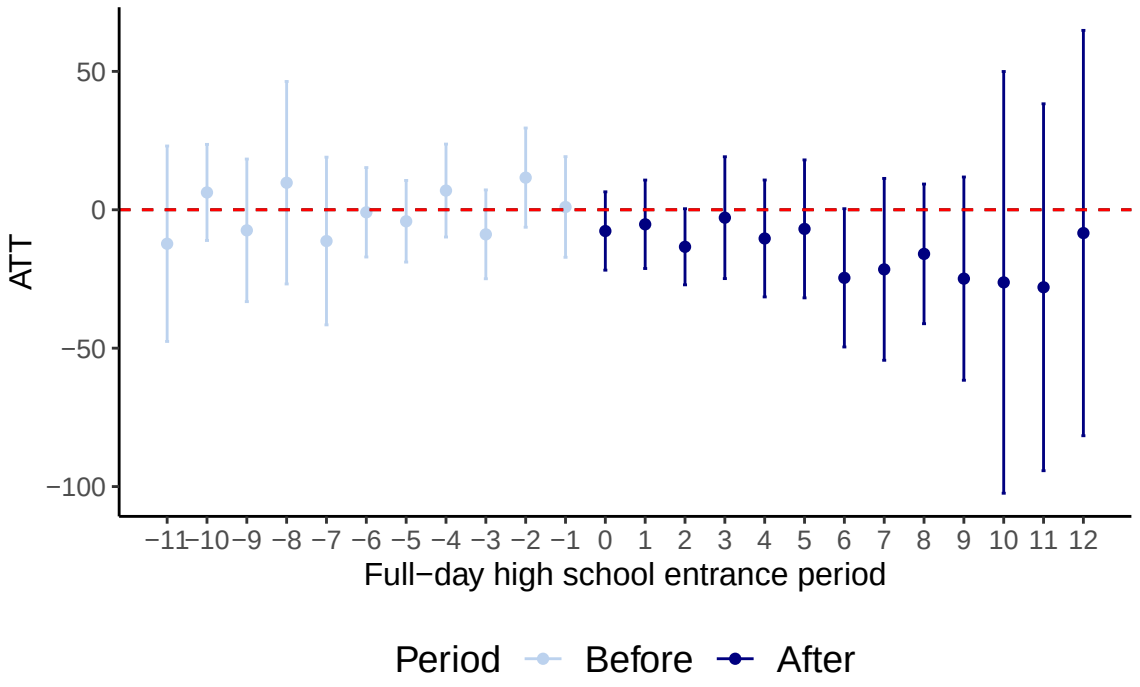


Figure 5: Effects of the full-day high school by length of exposure (*never treated as control group*)

Notes: This figure reports the Average Treatment Effects on Treated using the method proposed by (Callaway and Sant'Anna, 2021). The dependent variable is the homicide rates of males and 15-19 years old. It uses as control group municipalities in the Pernambuco's boundary but in other states that were not treated in the period. Each point present a different ATT and the 90% confidence interval for the estimates.

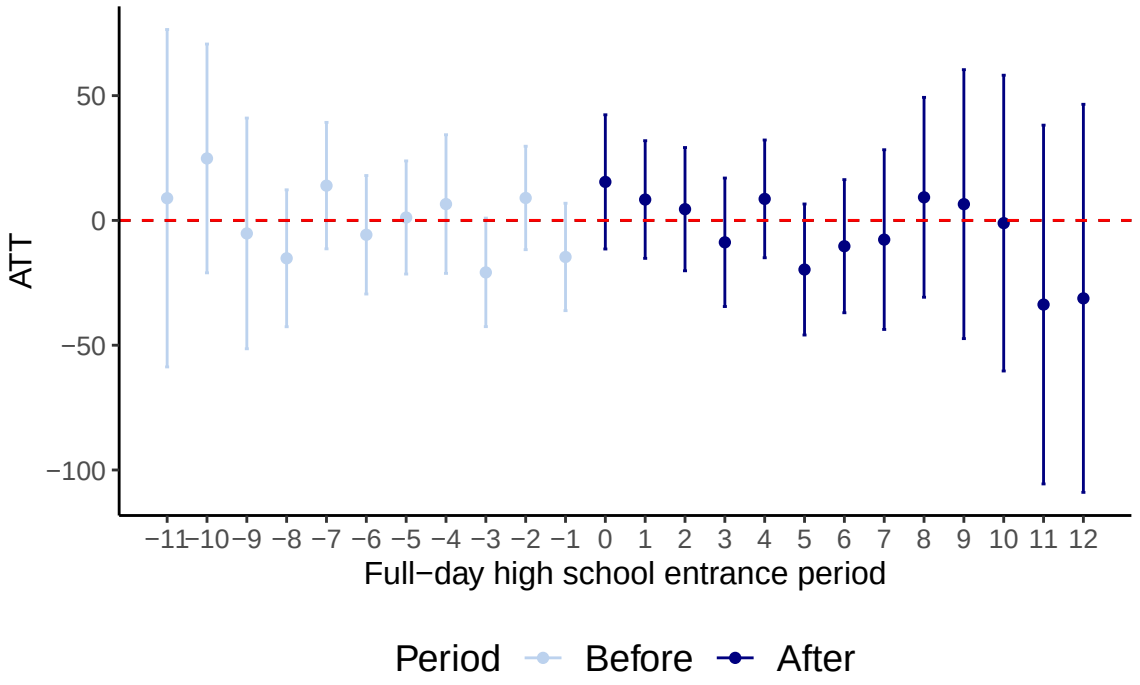


Figure 6: Effects of the full-day high school by length of exposure on 20-24 years old males

Notes: This table reports the Average Treatment Effects on Treated using the method proposed by (Callaway and Sant'Anna, 2021). The dependent variable is the homicide rates of males and 20-24 years old. It uses as control group municipalities in the Pernambuco's boundary but in other states that were not treated in the period. Each point present a different ATT estimate and the 90% confidence interval.

Table 1: Summary statistics

		Pernambuco municipalities		Out-state municipalities
		All	Border	Border
		(1)	(2)	(3)
Homicide rates for males 15-19 years old		31.553	20.786	11.219
		[41.249]	[35.223]	[47.802]
Homicide rates for males 20-24 years old		66.475	43.128	25.941
		[71.618]	[61.303]	[59.630]
GDP per capita (BRL)		4,730	4,495	4,613
		[3,316]	[3,452]	[3,988]
Human Development Index		0.473	0.468	0.437
		[0.058]	[0.056]	[0.056]
Population		37,187	27,755	18,059
		[63,144]	[30,762]	[24,550]
Number of Municipalities		182	74	86
Municipalities by treatment year				
	2006	10	4	-
	2007	5	3	-
	2008	18	6	-
	2009	43	19	-
	2010	24	10	-
	2011	13	5	-
	2012	22	11	-
	2013	22	8	-
	2014	25	8	-
Years used in the analysis		2002-2014	2002-2018	2002-2018
Number of observations		2,184	1,258	1,462

Notes: This table reports summary statistics for Pernambuco municipalitites and municipalities in other states that are part of our control group. Means and standard deviations (square brackets) of homicide rates, GDP per capita, HDI, and Population are from the year of 2002, before the full-day high school program started in Pernambuco.

Table 2: Effects of full-day high school on municipal homicide rates of males and 15-19 years old - *not yet treated as control group*

	(1)	(2)	(3)	(4)	(5)
ATT	-16.5	-16.4	-17.5	-13.3	-0.6
	[-24.3, -8.7]	[-24.5, -8.3]	[-26.2, -8.8]	[-22.7, -3.8]	[-13.9, 12.7]
Average homicide rate	35.3				
Latitude and longitude	No	Yes	Yes	Yes	Yes
GDP per capita	No	No	Yes	Yes	Yes
IDH	No	No	No	Yes	Yes
Population	No	No	No	No	Yes
Number of municipalities	182	182	182	182	182
Number of start years (G)	8	8	8	8	8
Number of years (T)	12	12	12	12	12
ATTs computed	88	88	83	78	68

Notes: This table reports the Average Treatment Effects on Treated using the method proposed by (Callaway and Sant'Anna, 2021). It uses as control group municipalities in Pernambuco that were not yet treated. Square brackets present the 90% confidence interval for the estimates. Average homicide rate was computed using the period from 2002 to 2014. The row ATTs computed indicated the number of ATTs calculated by the algorithm. ATTs computed in the first column indicate the maximum ATTs that can be recovered using the sample. These values change whether it is not possible to match treatment groups with no treated units.

Table 3: Effects of full-day high school on municipal homicide rates of males and 15-19 years old - *never treated as control group*

	(1)	(2)	(3)	(4)	(5)
ATT	-6.7	-6.5	-6.7	-10.7	-12.3
	[-16, 2.5]	[-15.4, 2.3]	[-15.5, 2]	[-20, -1.3]	[-21.7, -2.9]
Average homicide rate	24.2				
Latitude and longitude	No	Yes	Yes	Yes	Yes
GDP per capita	No	No	Yes	Yes	Yes
IDH	No	No	No	Yes	Yes
Population	No	No	No	No	Yes
Number of municipalities	160	160	160	160	160
Number of start years (G)	9	9	9	9	9
Number of years (T)	17	17	17	17	17
ATTs computed	144	144	144	144	144

Notes: This table reports the Average Treatment Effects on Treated using the method proposed by (Callaway and Sant'Anna, 2021). It uses as control group municipalities in the Pernambuco's boundary but in other states that were not treated in the period. Square brackets present the 90% confidence interval for the estimates. Average homicide rate was computed using the period from 2002 to 2018. The row ATTs computed indicated the number of ATTs calculated by the algorithm. ATTs computed in the first column indicate the maximum ATTs that can be recovered using the sample. These values change whether it is not possible to match treatment groups with no treated units.

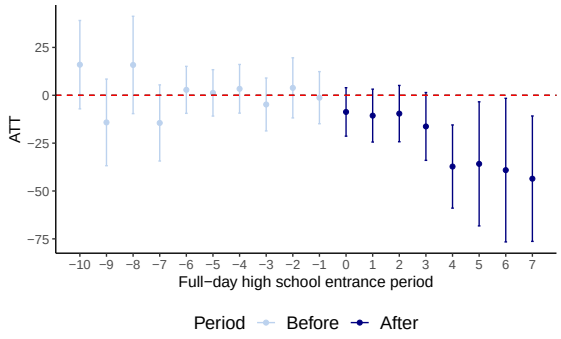
Table 4: Effects of full-day high school on municipal homicide rates of males and 20-24 years old - *never treated as control group*

	(1)	(2)	(3)	(4)	(5)
ATT	1.7	0.4	1.9	1.1	0.1
	[-8.8, 12.1]	[-10, 10.8]	[-8.8, 12.5]	[-10.9, 13.1]	[-12.6, 12.8]
Average homicide rate	39.9				
Latitude and longitude	No	Yes	Yes	Yes	Yes
GDP per capita	No	No	Yes	Yes	Yes
IDH	No	No	No	Yes	Yes
Population	No	No	No	No	Yes
Number of municipalities	160	160	160	160	160
Number of start years (G)	9	9	9	9	9
Number of years (T)	17	17	17	17	17
ATTs computed	144	144	144	144	144

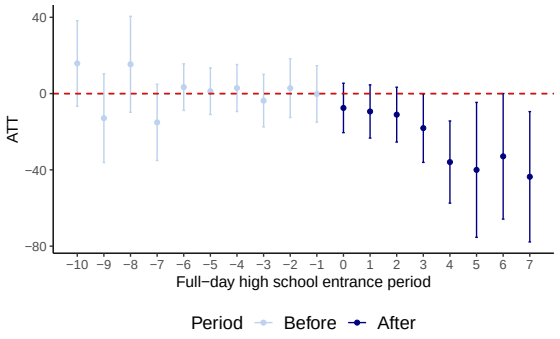
Notes: This table reports the Average Treatment Effects on Treated using the method proposed by (Callaway and Sant'Anna, 2021). The dependent variable is the homicide rates of males and 20-24 years old. It uses as control group municipalities in the Pernambuco's boundary but in other states that were not treated in the period. Square brackets present the 90% confidence interval for the estimates. Average homicide rate was computed using the period from 2002 to 2018. The row ATTs computed indicated the number of ATTs calculated by the algorithm. ATTs computed in the first column indicate the maximum ATTs that can be recovered using the sample. These values change whether it is not possible to match treatment groups with no treated units.

Appendix A

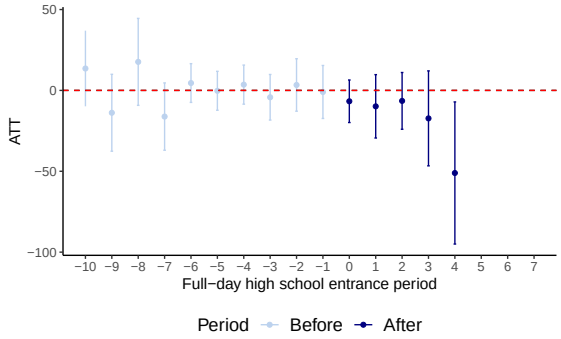
Figures



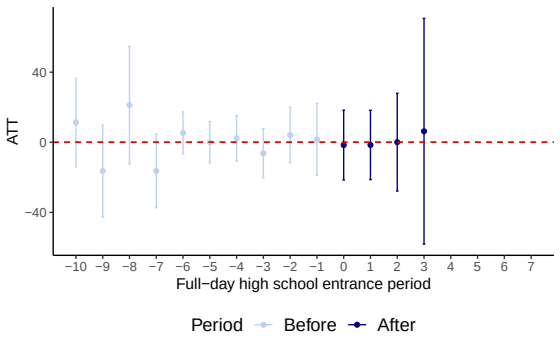
(a) Covariates: None



(b) Covariates: Latitude and Longitude

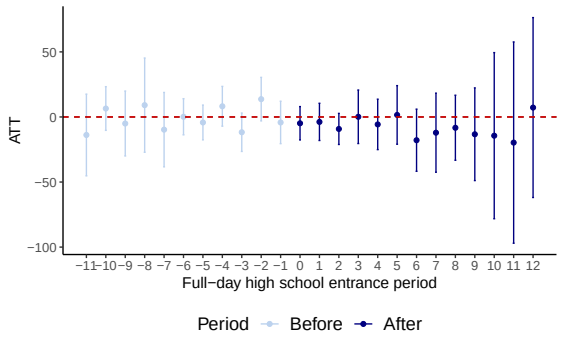


(c) Covariates: Latitude, Longitude, HDI, and GDP per capita

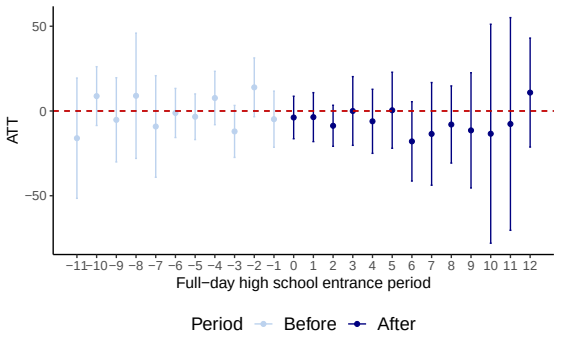


(d) Covariates: Latitude, Longitude, HDI, GDP per capita, Population

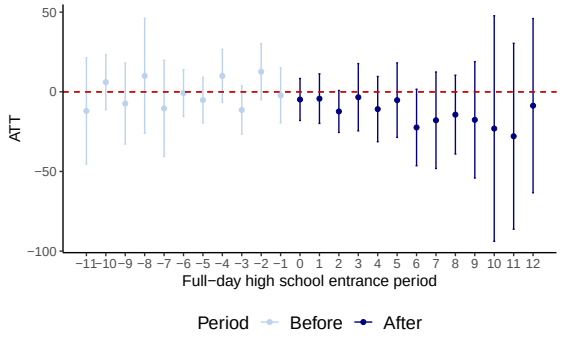
Figure A.1: Effects of the full-day high school by length of exposure (*not yet treated as control group*)



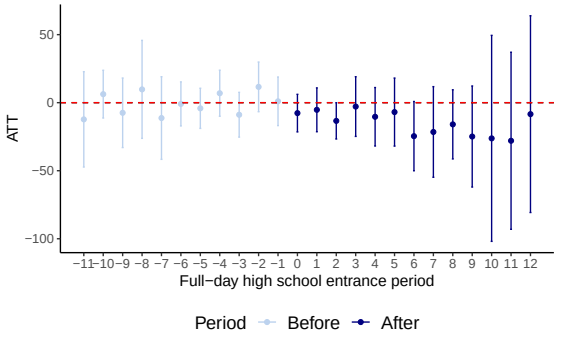
(a) Covariates: None



(b) Covariates: Latitude and Longitude



(c) Covariates: Latitude, Longitude, HDI, and GDP per capita



(d) Covariates: Latitude, Longitude, HDI, GDP per capita, Population

Figure A.2: Effects of the full-day high school by length of exposure (*never treated as control group*)