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FROM SCHOOL HOURS TO LIFE OUTCOMES: THE ROLE OF FULL-TIME SCHOOLING IN IMPACTING ADOLESCENT SUICIDES, PREGNANCIES, AND HOMICIDES

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Adolescent health and well-being have become central to social development efforts, yet challenges such as teenage suicide and pregnancy remain persistent worldwide. In Brazil, adverse adolescent outcomes occur at nearly twice the rate of other upper-middle-income countries (UN Inter-agency Group for Child Mortality Estimation, 2023; UN Population Division, 2023). Educational reforms are often viewed as key policy tools to address these issues. This study uses Pernambuco's full-time schooling reform, initiated in 2008, to examine whether structural changes to schooling can influence students' socio-behavioural outcomes. Applying two empirical strategies – a two-way fixed-effects model and a multiperiod Differences-in-Differences event-study framework following Callaway & Sant'Anna (2021) – the analysis estimates the causal impact of the reform. The study contributes to the literature by providing the first causal evidence on the relationship between schooling hours and adolescent suicides, while confirming existing links for full-time schooling on pregnancy and homicide rates in other contexts. The results indicate no significant effect on suicides in the full sample, though municipalities with higher baseline suicide levels exhibit a positive link. The reform appears to increase adolescent fertility rates, particularly in urban areas. Finally, the analysis finds no evidence that extended schooling hours affect adolescent homicide rates.

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AI Declaration

For this thesis, AI has been utilised to support the generation of econometric STATA code, focusing on debugging and syntax corrections. Furthermore, generative AI has been used to enhance the grammar, punctuation, and spelling of this thesis to improve readability. For all the aforementioned tasks, ChatGPT has been utilised in the free OpenAI model.

Table of Contents

1. Introduction **7**
2. Literature Review **10**
2.1. Suicide and Youth Mental Health 10
2.2. Adolescent Pregnancy and Fertility 11
2.3. Homicide and Crime 12
3. Context **15**
3.1. Pernambuco 15
3.2. Escola em Tempo Integral 16
3.3. The ETI reform and risky adolescent behaviour 17
4. Data **19**
5. Empirical Strategy **21**
5.1. Two-Way Fixed-Effects Regression 21
5.1.1 Identification 22
5.2. Multi-period Differences-in-Differences model 23
5.2.1 Identification 24
5.3 Potential Source of Biased Coefficients 25
6. Results **28**
6.1. Descriptive Results 28
6.2. Empirical Results 30
6.2.1. Adolescent Suicide 31
6.2.2. Adolescent Fertility 32
6.2.3. Adolescent Homicide 32
7. Heterogeneity Analysis **34**
7.1. Contrasting Dynamics of Urban and Rural Municipalities 34
7.2. Contrasting Dynamics in High- and Low-Vulnerability Municipalities 37
8. Robustness Checks **40**
8.1. Robustness of Regression Coefficients to Alterations in Outcome Variables 40
8.2. Patterns in High School Continuation Rates 42
9. Discussion and Conclusion **44**
9.1. Discussion 44
9.2. Conclusion 46
References **49**
Appendix **54**

List of Tables and Figures

List of Tables

Table 1: Descriptive Statistics **20**

Table 2: Balance Table of Outcome Variables for Early versus Late Adopters **26**

Table 3: Regression Output of the ETI Program on Outcome Variables **30**

Table 4: Regression Output of Two-Way Fixed-Effects Regression for Urban and Rural Areas **35**

Table 5: Regression Output of Two-Way Fixed-Effects Regression for above- and below-median Outcomes **38**

Table 6: Regression Outputs for Top-coded Outcome Variables **40**

Table 7: Regression Outputs for High School Dropout Rates **43**

List of Figures

Figure 1: Share of high schools with full-time eligibility at baseline in 2007 **25**

Figure 2: Geographical trends of municipalities adopting full-time high schools over time **28**

Figure 3: Trends in full-time schooling **29**

Figure 4: Event study plots of the school day extension program on adolescent socio-behavioural outcomes **30**

Figure 5: Event study plots of the school day extension program on adolescent socio-behavioural outcomes for urban and rural municipalities **36**

Figure 6: Event study plots of the school day extension program on top-coded adolescent socio-behavioural outcomes **41**

List of Abbreviations

ATT	Average Treatment Effect on the Treated
DiD	Differences-in-Differences (approach)
ETI	*Escola em Tempo Integral*
FT	Full-time
GTATT	Group-time average treatment effect
TWFE	Two-way fixed-effects (regression)

1. Introduction

Adolescent health and well-being have been at the focus of social development efforts, with issues such as teenage suicides and pregnancies being persistent challenges globally. The probability of dying among adolescents is almost twice as high in Brazil as in the average upper-middle-income countries, likewise for teenage pregnancy rates (UN Inter-agency Group for Child Mortality Estimation, 2023; UN Population Division, 2023). These statistics reflect structural inequalities persistent in Brazil, including poverty, limited educational opportunities, and insufficient access to mental and reproductive health services. Suicide is now the third leading cause of death among Brazilian youth and has increased by over 120% between 2000 and 2022, contrasting decreasing trends at the global level (Corassa et al., 2025). Early pregnancy is associated with school dropouts, worse labour market opportunities and limited social mobility, posing various threats to adolescents' well-being and life prospects. Despite policy efforts, rates remain high, with suicide showing a concerning ongoing upward trend. These patterns underscore the need for integrated and informed policies addressing health and education outcomes simultaneously. Understanding how educational interventions can influence adolescents' socio-behavioural outcomes is both of educational and political relevance, particularly in regions characterised by high poverty and limited social mobility, to design informed and effective interventions.

Pernambuco was the first state in Brazil to launch a full-time schooling reform in 2008, known as *Escola em Tempo Integral* (ETI hereafter). It was initially designed to address poor state-wide educational performance by extending the school day by 80% from part-time to full-time (FT hereafter), adapting the school curriculum, pedagogical methods, staff structures and school infrastructures. The program integrated improved academic instruction with extracurricular activities and socio-emotional training. By keeping students in a structured educational environment longer, ETI sought to enhance cognitive performance and strengthen educational attainment. Implementation was gradual, expanding across municipalities, and over the years, hundreds of schools adopted the model, inspiring similar initiatives nationwide. Existing evaluations of the reform show positive impacts on academic attainment and overall school attendance. Yet, its potential effects on socio-behavioural outcomes beyond the classroom have remained less explored. It is essential to understand these broader effects to assess if structural change to the number of schooling hours is a valuable tool to impact students' risky socio-behavioural choices.

While the relationship between education and youth outcomes has been widely studied, most empirical research has focused on academic performance, labour market prospects or cognitive skills,

particularly in high-income countries. Evidence suggests that schooling interventions can influence adolescent behaviour through increasing aspirations, time use, and improving self-esteem (amongst others: Duflo et al., 2015; Favara & Sanchez, 2019; Novella & Ripani, 2016; Berthelon & Kruger, 2011). Yet, there is limited insight on the extent to which schooling-hour extensions affect adverse socio-behavioural outcomes, particularly in low- and middle-income countries. In Brazil, studies have shown that higher educational attainment and school engagement are associated with reduced adolescent fertility and delayed motherhood (De Araújo Benevides et al., 2024), but causal assessments of structural school interventions and their socio-behavioural outcomes are still limited. Evaluations of Pernambuco's ETI program have concentrated on academic indicators, offering limited insight into its broader social impact. Some findings highlight schools' role in reducing adolescent crime and improving life prospects (Rosa et al., 2022). However, research about the effects of the ETI program on adolescent suicide and fertility rates has not yet been conducted. Since schooling hour extension programs reduce students' spare time, may improve their human capital, and consequent life prospects, and foster more social interaction, they may affect students' choices to engage in risky behaviour, and thus their socio-behavioural outcomes. This highlights the need for a comprehensive assessment of full-time schooling and its relationship to potential risks that compromise adolescent health and life trajectories.

This thesis investigates whether and to what extent Pernambuco's full-time schooling intervention has influenced teenage socio-behavioural outcomes – specifically suicide, fertility, and homicide rates – seeking to identify broader social and health effects of extended schooling. The findings contribute to the growing literature by examining effects on suicide and extending existing evidence on schooling-hour extensions and adolescent fertility and homicide. The central research question asks what effects the expansion of full-time education generates with respect to adolescent well-being beyond the classroom. The study compares municipalities with greater or earlier ETI exposure to those that have limited or delayed implementation. Using two empirical frameworks that analyse intervention timing and intensity, the thesis examines the causality between the relationship of schooling hours and adolescent suicide and verifies links with fertility and homicide. It also explores heterogeneity across outcome intensity, vulnerability to outcomes, and urban-rural divides, recognising that the benefits of full-time schooling may not be uniformly distributed. By addressing these questions, the research contributes to a deeper understanding of the non-academic dimensions of educational reforms and their potential as a cross-sectoral public policy tool.

To assess the causal effect of Pernambuco's ETI on socio-behavioural outcomes, this study employs a quasi-experimental design exploiting the staggered rollout of full-time schools across municipalities. Comparing trends between early and late adopters, it applies the Callaway and Sant'Anna (2021)

Difference-in-Differences framework to isolate program impacts from broader temporal and regional influences. Furthermore, differences in the intensity of adoption, measured by the share of students in full-time education, allow causal analysis of how varying exposure levels to the interventions influence adolescent outcomes, controlling for time-invariant municipal characteristics and broader temporal trends. The study integrates administrative education, health, and demographic data to build a comprehensive panel capturing policy exposure and outcomes over time. The findings aim to provide causal evidence on the relationship between education policy and youth well-being in a middle-income setting and offer practical insights for policymakers designing integrated strategies linking education, mental health, and reproductive health interventions.

The remainder of this thesis is structured as follows. Section 2 reviews existing literature on educational interventions and adolescent socio-behavioural outcomes. Section 3 maps out the study context and full-time intervention, while Section 4 describes data sources and key variables. Section 5 presents the methodological framework and identification strategy. Section 6 reports and interprets the main results, and Section 7 explores heterogeneous effects of the intervention across socioeconomic and geographic dimensions. Section 8 provides robustness checks of the findings. Finally, Section 9 concludes by summarising the key findings, highlighting the limitations and suggesting further research directions.

2. Literature Review

Traditionally, educational reforms are evaluated by their impact on students' test scores and school completion rates. The effects of full-time schooling on improving academic performance are well investigated and found to be generally positive (Rosa, Bettinger, et al., 2022; Bellei, 2009). School-hour interventions have been shown to reduce dropout rates while increasing grade repetition, suggesting lasting positive effects on students' school participation and life trajectories (Vidigal & Vidigal, 2021). More recently, research has begun to investigate the socio-behavioural outcomes of schooling interventions, which will be discussed in the following three sections in greater detail while focusing on the driving effects of these outcomes. Expanding schooling quality, access, and setup can affect students in multiple ways: spending more time in school leads to having less spare time engaging in risky behavioural choices; it also alters the opportunity cost of choices by increasing human capital. Changing the duration that youths spend with peers increases social exposure and can enhance certain risky choices that lead to adverse socio-behavioural outcomes. These factors are particularly relevant to understand for choosing and designing successful policies intended to reduce adverse socio-behavioural outcomes and local and global disparities.

2.1. Suicide and Youth Mental Health

The link between schooling interventions and youth mental health is still an underexplored area in economic literature. Suicide is now the third leading cause of death among Brazilian adolescents and has increased by over 120% between 2000 and 2022, underscoring the mental health crisis among teenagers. This rapidly increasing trend contrasts with declining adolescent suicide trends on the global level (Corassa et al., 2025). Deaths of Despair have been a growing global concern, initially in the United States but now in many developed economies, with growing mortality rates in certain groups that experience increasing levels of distress due to their deteriorating social and economic status (Case & Deaton, 2020). Within the *'deaths of despair'* strand of economic literature, Gomes de Carvalho et al. (2024) document the premature death crisis in Brazil, highlighting social and socioeconomic factors as the main drivers of higher mortality rates.[1] Yet the potential role of schools, which affects young people's daily lives, and their association with mental health, remains vastly overlooked.

Suicide risk is affected by social, environmental, family, individual, and psychological factors, with adolescents from disadvantaged socio-economic backgrounds being particularly vulnerable (Liu & Wang, 2024). Some of these critical factors associated with youth suicide may be altered by extended

[1] In 2022, Brazil launched a mental health reform that expanded community care to address the growing mental health crisis in Brazil. The reform did not reduce mental health mortality and increased violent crimes (Dias & Fontes, 2024).

school programs through increasing supervision, providing counselling, and fostering community belonging. Evidence from related educational settings points to potential protective effects. Research on the starting time of school has delivered some evidence suggesting improved mental health outcomes due to longer sleeping hours and, consequently, better school performance for students (Wheaton et al., 2016). Furthermore, a mildly positive link has been found between mental health promotion programs and students' well-being (Kimber et al., 2007; Hall, 2010; O'Reilly et al., 2018). School-based suicide prevention programs in the United States are linked to decreased adolescent suicide rates, suggesting that they might be an effective tool to reduce adolescent suicide rates (Zhang et al., 2023). Existing studies on structural schooling interventions on the Brazilian compulsory schooling law from 2009 found no significant causal effect on youth suicide rates (Nishijima & Pal, 2023). The law led to students being kept in schools longer, providing tentative evidence against the incapacitation effect – students spending more time in schools and thus having less time to engage in risky behaviour – having a vital role in the reduction of adolescent suicide rates. There is some evidence suggesting that suicide rates vary with respect to the academic calendar, with higher incidents during teaching periods. It further finds that the return to in-person classes after the COVID pandemic increases adolescent suicide rates as opposed to digital classes (Hansen et al., 2023). This research documents the potential negative pressure of schooling periods on mental health outcomes in adolescents.

While intensive, targeted interventions show success in improving students' mental health, the efficacy of large-scale structural school reforms in preventing completed suicide remains unknown, not only in the Brazilian context but also globally. With the *deaths of despair* crisis growing in many upper- and middle-income countries, understanding what structural interventions affect students' suicide behaviour is crucial. This study delivers the first causal estimates of the effect of full-time schooling interventions on adolescent suicide rates in Brazil and closes a relevant research gap. The delivered evidence can help support policymakers to design and implement structural schooling reforms that ideally help improve students' mental well-being and reduce adolescent suicides structurally.

2.2. Adolescent Pregnancy and Fertility

Globally, studies using randomised or quasi-experimental designs consistently find that increasing educational attainment – through access to schooling or structural schooling changes – causally reduces fertility. Lowering barriers to school attendance in a randomised evaluation in Kenya increases enrollment, which is associated with a 17% reduction in adolescent fertility (Duflo et al., 2015). Similarly, other studies find a negative association between attending higher-achieving schools and adolescent fertility rates (Dobbie & Fryer, 2015).

Evidence from Latin America reinforces these findings. Berthelon and Kruger (2011) provide strong causal evidence that the school day extension program in Chile reduces the probability of adolescent motherhood by 3.3%, particularly among poor, urban women via the incapacitation effect. In a later study, they show that adolescent motherhood negatively affects educational outcomes as well as labour outcomes of non-poor women (Berthelon & Kruger, 2016). In Ceará, Brazil, full-day schooling lowers adolescent pregnancy rates by 1.18-1.95 percentage points, depending on exposure (De Araújo Benevides et al., 2024). The mechanisms behind this documented effect, as assessed in the Peruvian context, include increased opportunity cost, empowerment, and greater access to knowledge (Favara & Sanchez, 2019), pointing towards the human capital effect playing a crucial role in youth's fertility choices.

Other interventions targeting school participation yield similar results. Conditional cash transfers in Colombia reduced adolescent fertility rates, but only when the benefits were conditioned on school success or had a binding duration limit (Cortés et al., 2015). Likewise, a soft skills program in the Dominican Republic, designed to improve life planning and self-esteem, was associated with lower adolescent fertility rates for young non-mother women, primarily by strengthening expectations and soft skills (Novella & Ripani, 2016). Beyond short-term outcomes, Chile's full-day schooling reform led to long-term benefits, including higher educational attainment, delayed childbearing, lower adolescent fertility, and improved labour market outcomes (Domínguez & Ruffini, 2020).

To summarise, the current research on schooling interventions and adolescent fertility rates suggests an overall negative link. This is particularly true for programs targeting vulnerable groups. The mechanisms behind this are a human capital effect by increasing the opportunity cost of early motherhood, strengthening female empowerment and self-esteem, and greater access to knowledge, as well as the incapacitation effect by keeping girls in schools longer and therefore reducing the time in which they can engage in risky behaviour. Despite strong causal evidence from neighbouring Ceará and Chile, the impact of Pernambuco's ETI program specifically on teenage pregnancy rates has not been established in the literature. My study will directly contribute to this missing causal estimate for the ETI, thereby consolidating the causal evidence for extended schooling reforms as an adolescent fertility prevention strategy in Northeast Brazil.

2.3. Homicide and Crime

A robust literature connects education with lower crime rates, with the extension of dropout ages being a key policy. Various studies find that increasing the dropout age in secondary school reduces juvenile crime rates (Beatton et al., 2017; Brilli & Tonello, 2018; Bell et al., 2022). Studies assessing the effect of an additional year of schooling also suggest a negative relationship with adult crime

(Cano-Urbina & Lochner, 2019; Lochner & Moretti, 2004). However, there is evidence reporting that increasing minimum dropout ages increased school crimes in states adopting the law, finding contrasting evidence to the established negative link (Gilpin & Pennig, 2015).

A related strand of research investigates school quality and attendance and its relationship to adolescent crime. Attending higher-achieving schools via lottery reduces crime among high-risk youth, pointing towards the human capital effect (Deming, 2011; Dobbie & Fryer, 2015; Cullen et al., 2006). Full-time schooling programs have also been linked to lower crime rates. Early research established that additional time in school reduces opportunities for juvenile crime through the incapacitation effect. Later evidence from Latin America finds similar effects, even though results remain inconclusive. While some authors (De Carvalho, 2019; Nishijima & Pal, 2023) find no significant effect of schooling-hour extensions on homicide, others (Jacob & Lefgren, 2003; Berthelon & Kruger, 2011; Luallen, 2005; Lochner, 2004) report notable reductions in adolescent crime, including property and violent offences.

Rosa et al. (2022) examine Pernambuco's full-time schooling expansion, including its pilot phase (2004-2014), using a Differences-in-Differences approach and find a 30-50% reduction of male youth crime rates driven by the incapacitation effect and the human capital effect, whereby higher-quality education improves labour market prospects and raises the opportunity cost of committing crimes.

While extensive research has been conducted on the link between crime rates and educational reform, only scattered evidence addresses this link in the Brazilian context. With adolescent crime being a pressing issue in Brazil, particularly in the Northeast, understanding the link to schooling is crucial. Rosa et al. (2022) attempted to address this gap by delivering evidence on the effect of Pernambuco's full-time schooling intervention on adolescent male homicides. However, their focus on male outcomes limits understanding of how the reform affects all adolescents – including female youth – representing a key research gap this study addresses. This is particularly relevant since schools are typically mixed gender. Understanding how and to what extent changing schooling hours can affect adolescents' choices to engage in deadly crime is relevant for designing successful policies aimed at reducing local and global disparities.

To conclude, although there exists scattered evidence on the effects of schooling-hour expansion interventions in middle-income countries, insights on its link to socio-behavioural outcomes in Brazil remain scarce. First, there are no insights into the effects of full-day schooling on suicides this far, which this research aims to provide the first causal evidence for. Second, it will also deliver insights on

the existence of the link between schooling hour extension programs and adolescent fertility and homicide rates, as has been established in other contexts by scientific literature.

3. Context

3.1. Pernambuco

The state of Pernambuco is located in the northeastern coastal region of Brazil. Its capital is Recife, where roughly 17% of Pernambuco's approximately 9.5 million inhabitants reside. Pernambuco is among the poorest states in Brazil, with an average monthly household income of 255€[2] in 2022 (Instituto Brasileiro de Geografia e Estatística, 2022a). In 2008, the state's GDP per capita was around 3,551€, highlighting an economic gap of nearly 50% compared to Brazil's national GDP per capita (Instituto Brasileiro de Geografia e Estatística, 2010). By 2022, Pernambuco's GDP per capita had increased to 5,331€, yet it remained about 43% below the national average (Instituto Brasileiro de Geografia e Estatística, 2022a). In 2021, Pernambuco contributed only 2.5% to Brazil's GDP, despite accounting for nearly 4.5% of the country's population, further emphasising the economic challenges many inhabitants face. The state remains economically underdeveloped in comparison to the national average. In 2007, 830.406 adolescents aged 15-19 resided in the state, representing roughly 5% of all adolescents of that age group in Brazil (Ministério da Saúde, n.d.). By 2022, around 7.5% of Pernambuco's population were aged 15-19, totalling approximately 685.000 individuals (Instituto Brasileiro de Geografia e Estatística, 2022b).

Historically, Pernambuco has had some of the lowest educational performances in Brazil. In 2005, illiteracy rates were high - 5.3% for 15-19-year-olds and 25.9% for those over 25. Approximately 90% of students in grade five scored at an unsatisfactory level in both mathematics and Portuguese (World Bank Group, 2017). Between 2005 and 2017, the score on the national high school student achievement indicator (IDEB) increased from 2.7 to 4.0, placing Pernambuco among the top four performing states by 2016. By 2022, the illiteracy rate for 15-19-year-olds in Pernambuco had fallen to 1.5%, while the adult illiteracy rate decreased to 13.4% (Instituto Brasileiro de Geografia e Estatística, 2022b). Although Pernambuco's full-time schooling program has received considerable attention with regard to its effect on academic achievement and even inspired the national full-time schooling program launched in 2023, limited insights exist on its effects beyond academic performance.

Beyond education, adolescents in Pernambuco face significant social challenges. Suicide has become a pressing concern: it is now the third leading cause of death among Brazilian adolescents and has increased by over 120% between 2000 and 2022 (Corassa et al., 2025). Suicides are closely correlated with social inequality and higher unemployment (Jaen-Varas et al., 2019). In 2007, Brazil's adolescent suicide rate was approximately 3.4 per 100,000 inhabitants, increasing by 88% by 2019 (World Health

[2] All currency values have been converted from Brazilian Reais to Euro and deflated to 2025 Euro value. All values have been rounded to the nearest integer.

Organization, 2024). Even though the Northeast shows comparatively lower suicides, rates across Brazil have been growing since the early 2000s (Institute for Applied Economic Research [IPEA], 2022). In Pernambuco, the average teenage suicide rate was 3.8 in 2007, decreasing by 10.5% to 3.4 by 2010, and then sharply increasing by 91.2% to 6.5 by 2021 (Tecnologia da Informação a Serviço do SUS [DATASUS], 2025).

Adolescent fertility also remains high in Pernambuco. While Brazil reduced its average adolescent pregnancy rates by 40.7% between 2000 and 2019, the northeastern region continues to show the second-highest rates in the country. In Pernambuco, the rate decreased from 9.0% to 5.3% over this period - a total reduction of 40.9% - but remained 10.4% higher than the national rate of 4.8% in 2019. Compared to the global average decrease of 11.6% during the same period, Brazil achieved substantial progress, yet adolescent fertility rates in Pernambuco remain comparatively elevated (Monteiro et al., 2021).

Finally, adolescent homicide and youth criminality present significant risks to adolescents' life prospects. In 2007, Pernambuco had the second-highest youth homicide rate (ages 15-29), with 109 deaths per 100,000 inhabitants, only lower than Alagoas, the state bordering Pernambuco to the south. By comparison, Brazil's national youth homicide rate averaged 50 per 100,000 inhabitants, meaning that Pernambuco's rate was 118% higher. By 2010, the rate decreased to 80 per 100,000 inhabitants, ranking sixth nationally, still 45.5% above the national average of 55 deaths. The northeastern region displays the highest adolescent homicide rates, highlighting persistent regional disparities. These differences existed before the full-time schooling reform and remained over time; adolescent homicide rates in 2021 were nearly three times higher in Pernambuco than in the southeastern region of the country (Institute for Applied Economic Research [IPEA], 2022).

3.2. Escola em Tempo Integral

Responsibility for education and its rollout in Brazil is shared among the federal, state, and municipal administrations. While Brazil had a national education plan anchored in *Lei Nº 10.172, de 9 de Janeiro de 2001*, a nationwide rollout of full-time schooling did not occur until 2023, following the approach pioneered in Pernambuco. In 2007, Brazil launched its *Mais Educação* program, which directed the implementation of optional extracurricular courses to students, which Pernambuco adopted in 2010 (Centro de Referências em Educação Integral, 2018). Thus, state-level, instead of nation-level, policies played a directive role in shaping the setup of Pernambuco's schooling system in the 2000s, particularly those that altered educational agendas, extended mandatory schooling hours, and introduced new teaching methods (Cunha & Araújo, 2021).

Pernambuco was one of the first states in Brazil to implement full-time schooling as a state-wide educational program. The initial design for the *Escola em Tempo Integral* (ETI) program was developed between 2002 and 2004. In the subsequent two years, some schools were selected to pilot the new full-time program, publicly known as *Centros de Educação Experimental*. After the ETI program was formally established as a state-wide initiative in 2008 through *Lei Complementar Nº 125, de 10 de Julho de 2008*, the rollout was conducted gradually. The initial goal was to implement at least one full-time school in each of Pernambuco's 185 municipalities.[3] Schools had to apply to participate in the program, and the selection of the applications was directed by the Ministry of Education. However, there is no documentation about the exact selection process of schools and no official guidelines. Thus, research concludes that schools were selected based on convenience, provided they had the necessary infrastructure (laboratories and sports facilities), and preference was given to municipalities that had not yet had a full-time school (Contreras & Lepe, 2023). The program was focused on converting existing schools; it neither closed nor opened new schools (Rosa, Bettinger, et al., 2022).

The full-time intervention's goals were the empowerment of students' ability to take a protagonist role in their life, the support in citizenship, and a comprehensive education by implementing a new pedagogical approach, thus putting enablement and holistic education as the centre of its design. The ETI program increased the average weekly school duration from 25 hours to 45 hours in the full-time model (or to 35 hours for the semi-full-time model), keeping students in school for the full day instead of the half-day previously. In addition, the program required modifications to both the curriculum and school infrastructure to accommodate the extended schedule as well as new pedagogical methods that targeted the program's goals, putting students' learning and empowerment at the focus (Coelho Vieira Selva et al., 2022).

3.3. The ETI reform and risky adolescent behaviour

Although the primary goal of the ETI reform in Pernambuco was to improve the students' academic achievements throughout the state, the changes in time allocation, curriculum and teaching methods can potentially affect household and individual choices. Under full-day schooling, students stayed in class for almost 80% longer than in the part-day system. The reform exogenously alters the time students spend in the classroom, but also the way they organise their daily routines and how students spend their time. It further affects their families and how they organise their daily lives, as well. For instance, full-day schooling increases the number of hours that adolescents spend in school, reducing the amount of time they could work or help out with household chores like watching younger siblings. This could limit the number of students from poor socio-economic backgrounds – whose families are

[3] This included the island municipality of Fernando de Noronha in the list of Pernambuco's municipalities.

dependent on their income generated after school – enrolling in ETI schools. Even though Brazil has launched an extensive number of financial aid programs to incentivise secondary school attendance (e.g. *Bolsa Familia*), these programs are not directly tied to the ETI reform. This could lead to a self-selection bias where students from wealthier backgrounds opt to enrol in full-time schools in contrast to students from less wealthy backgrounds, introducing potential bias into my analysis.

Increasing the time that adolescents spend in school can also affect their decision-making and opportunities to engage in risky behaviours. As previously discussed, economists distinguish between two relevant effects: the incapacitation effect and the human capital effect. The incapacitation effect, also called the time substitution effect between crime and schooling, has been primarily found for property crime and theft (Jacob & Lefgren, 2003), less so for involvement in deadly crime. Longer schooling time and better schooling quality increase students' human capital, which can affect adolescent preferences for involvement in crime and early parenthood, as well as their prospects in life, which may reduce suicide choices, particularly when consulting the *deaths of despair* strand of literature (Berthelon & Kruger, 2011; Gomes de Carvalho et al., 2024). It is also important to note that having larger peer exposure at school through extended schooling hours can alter adolescents' behavioural choices in adverse directions. For instance, more social interaction could increase adolescent fertility rates through engaging in risky sexual choices. Likewise, more peer exposure could increase the opportunity for crime, as has been shown by Dobbie and Fryer (2015). It could also lead to increased social stress levels in students, potentially causing adverse mental health outcomes. Lastly, it is important to also consider the implementation of the schooling reform. If certain municipalities experience lower-quality implementation of the reform – through resource and managerial limitations – links to socio-behavioural outcomes would partially reflect unequal execution of the program. To summarise, schooling hour interventions can alter students' behaviour through three effects: the incapacitation effect, the human capital effect, and the social exposure effect, whereby the incapacitation effect and the human capital effect are expected to mitigate adverse socio-behavioural outcomes and the social exposure effect to deteriorate outcomes. Potential adverse effects may reflect unequal execution of the program and not only a link to socio-behavioural outcomes.

4. Data

For this research, data were sourced from public Brazilian databases covering the state of Pernambuco and its 185 municipalities. The main sources include: (1) INEP - Brazilian School Census retrieved from the National Institute of Educational Studies and Research Anísio Teixeira; (2) SINASC - information on life births retrieved from the Ministry of Health of Brazil; (3) DATASUS - data from the Brazilian Ministry of Health on residential and death records; (4) IPEA - *Atlas da Violência* providing information on adult homicides from the Institute for Applied Economic Research; and (5) IBGE - information on municipal codes and GDP per capita from the Brazilian Institute of Geography and Statistics. Table I in the Appendix lists all variables used in the analysis alongside their respective data sources.[4]

As the primary data source for tracking the rollout of the full-time schooling program in Pernambuco, the INEP school census was used for the time period 2007-2021. Before 2007, the census was collected using different questionnaire questions and measures which are not directly comparable to later editions. Therefore, although earlier data are available, they were excluded from the statistical analysis to avoid inconsistencies in measurement. The final sample includes all non-private high schools in 166 of the state's 185 municipalities for the period 2007-2021, excluding municipalities that had previously received treatment through pilot full-time schooling programs in non-private secondary schools. Descriptive statistics for relevant variables are displayed in Table 1.

Some different measures are used to assess potential channels of effect and to test the robustness of the regression results. First, as explanatory variables tracking the rollout of Pernambuco's ETI program, I employ two distinct indicators: (1) the share of high school students attending a public full-time high school in each municipality, (2) the year in which a municipality first adopted the ETI program in a secondary school. These measures aim to capture all relevant dimensions of change at the municipality level - the adoption of the ETI program and the accessibility of full-time schooling (through (2)), and the student participation (through (1)) resulting from the program expansion.

To capture the effect of the schooling expansion on their lives beyond the classroom, I focus on three distinct socio-behavioural outcomes. First, to estimate the effect of the ETI program on students' mental health, I use the teenage suicide rate, defined as the number of youth suicides per 1,000 adolescent inhabitants. As shown by Daly and Wilson (2009), well-being and mental health can be proxied using suicide rates. Second, to analyze the channel of adolescent pregnancies and, by extension,

[4] Throughout the thesis, tables and figures in the Appendix are numbered with Roman numerals, whereas tables and figures in the main text use Arabic numerals.

early motherhood, I use the following measures: (1) and (2) the birth rates by adolescent mothers aged 15-19 and aged 15-18 respectively with a first-degree lead; (3) a birth exposure variable defined as the number of teenage births (ages 15-19) in a given year, plus the number of teenage births in the following year; and (4) the average age at childbearing. The rationale for focusing on the 15-19 age group is that, given the average duration of pregnancy, some girls may become pregnant at age 18 while still in high school and give birth to their children at age 19. Therefore, I analyse birth rates for both underage girls (ages 15-18) and teenage girls (ages 15-19). Since the typical age in the final year of high school in Pernambuco is 17-18, I include 19-year-olds in measure (3). In order to capture possible changes in the timing of childbearing, I also use the average maternal age at birth. These variables aim to capture the effect of ETI schooling on the likelihood of high school-aged girls becoming pregnant, as well as potential timing effects with respect to childbearing. Third, to evaluate the effect of the ETI program on youth involvement in serious crime, I use the teenage homicide rate, defined as the number of youth homicides per 1,000 adolescent inhabitants. This rate measures the number of youth being murdered. Both female and male youths are included in this measure. Due to the lack of high-quality data on female homicides, I opted to use a combined measure for all teenagers rather than more granular gender-specific indicators. The variable serves to capture the degree of youth engagement in severe criminal activity.

***Table 1**: Descriptive Statistics*

Regression Variables	**Count**	**Mean**	**SD**	**Min**	**Max**
Share of FT HS (%)	2490	0.54	0.39	0.00	1.00
Share of FT Students (%)	2490	0.24	0.18	0.00	0.50
Teenage Suicide Rate (%)	2395	0.04	0.16	0.00	1.81
Teenage Pregnancy Rate (%)	2212	0.07	0.02	0.01	0.15
Teenage Homicide Rate (%)	2485	0.50	0.68	0.00	5.46
Other Variables					
Baseline Eligibility (%)	2490	0.16	0.29	0.00	1.00
Population	2487	35,667	69,455	2,674	711,330
Share of Teenagers (%)	2487	0.09	0.01	0.06	0.15
GDP per Capita (R$)	2490	8,605	7,748	2,288	132,715
Adult Crime Rate (%)	2468	0.35	0.22	0.00	1.67
# Students per HS	2490	588	277	43	1,849

Notes: Descriptive statistics covering the count, mean, standard deviation, minimum, and maximum from a sample of 166 municipalities in Pernambuco that adopted the ETI program. The top rows correspond to the regression variables, where the first two rows capture the explanatory variable and the subsequent rows the outcome variable. The summary statistics for other relevant variables are shown in the Other Variables section. Data sources are provided in the Data Appendix, and the data set covers the years 2007-2021.

5. Empirical Strategy

The following section establishes the empirical strategy used to analyse the causal effects of the full-time schooling program in Pernambuco. It is organised as follows: I describe two different econometric designs - first, the two-way fixed-effects model following the research by Callaway et al. (2024), and second, the Difference-in-Differences approach with multiperiod treatment as established by Callaway and Sant'Anna (2021). Subsequently, I will discuss potential sources of bias in my coefficients and assess the need for integrating covariates into my empirical design.

5.1. Two-Way Fixed-Effects Regression

There are two ways that treatment can be defined in the context of the full-time schooling intervention: (1) continuous treatment measured as the share of full-time students in each municipality over the duration of the intervention; or (2) a binary, staggered treatment variable, indicating whether a municipality offered at least one full-time secondary school and is thus being considered treated. First, I establish the methodology used to assess the effect of treatment using the continuous definition. In the following section, I elaborate on the methodology for the binary definition of treatment.

The empirical strategy to assess the causal effects of the policy intervention follows a classical two-way fixed-effects regression (TWFE), which implements a Differences-in-Differences (DiD) approach with continuous treatment. This approach allows for the municipalities to be treated at different periods and for treatment intensity to vary. Recent econometric literature has noted that classical two-way fixed-effects estimates may have limited causal interpretation, particularly because they do not differentiate between treatment and comparison groups and because of problems around the weights assigned to observations (De Chaisemartin & D'Haultfœuille, 2022; Callaway et al., 2024). To address some of these issues, I will follow the identification strategy established by Callaway et al. (2024). Nevertheless, I acknowledge potential threats to causal interpretation and will supplement these findings with additional econometric results based on the method described in section 5.2.

According to Callaway et al. (2024), the potential outcome variable is denoted as $Y_{t,m}(i)$, where i reflects the intensity of treatment and t corresponds to the time periods. Therefore, the treatment effect is extended from a binary response to a continuous treatment effect function that stretches over multiple periods. The counterfactual from having received treatment thus extends to (1) not having received treatment at all ($Y_t(0)$) or (2) having received treatment with a higher intensity at time t ($Y_t(i_j)$ - $Y_t(i_{j-1})$), such that the causal response measures the treatment effect of a marginal increase

of intensity. This extension of the treatment effect leads to a set of potential threats to causal interpretation – including weighting and selection bias, and potential violations of the parallel trends assumption – that I intend to partially solve with the identification strategy as proposed in their paper and discussed in detail in the following subsection.

The regression specification is:

$$Y_{t,m} = \alpha + \beta\, Intensity_{t,m} + \mu_m + \lambda_t + \epsilon_{t,m}$$

where $Y_{t,m}(i)$ is the outcome of interest (i.e. adolescent suicide rate, pregnancy rate, or homicide rate) at time t in municipality m. $Intensity_{t,m}$ captures the degree to which each municipality has adopted the full-day school extension, and β represents the parameter of interest. Municipality fixed-effects (μ_m) and time fixed-effects (λ_t) control for location- and time-specific effects. Finally, $\epsilon_{t,m}$ is the error term. Standard errors are clustered at the municipality level.

5.1.1 Identification

Callaway et al. (2024) outline five identification requirements for a continuous treatment DiD to ensure some causal interpretability of the parameter of interest:

(1) Random Sampling – observations are independent and identically distributed;
(2) Continuous or Multi-Valued Discrete Treatment – there is at least one pre-treatment period when no unit is treated, and after that, there is continuous treatment of some units in the sample;
(3) No-Anticipation and Observed Outcomes – units do not anticipate future treatment, and there are observable, untreated outcomes for all units in the pre-treatment period;
(4) Parallel Trends Assumption – the trends that not-yet-treated groups show are the same that treated groups would have shown in the absence of treatment;
(5) Strong Parallel Trends Assumption – the average response of outcomes of all units if they experienced treatment intensity i is equal to the response that the treatment intensity group i actually experiences (restriction of treatment effect heterogeneity).

When all five assumptions are satisfied, the regression is identified and allows some causal interpretation of the parameter of interest (for proof, see Callaway et al., 2024). There remain some potential issues with the weighting that cannot be solved through identification, which may introduce potential threats to the causal interpretation of the results.

Given the availability of data on all municipalities and all time periods, I consider condition (1) satisfied. While the conversion of high schools appears to be related to the prior existence of a full-time school in the municipality – as indicated by a negative correlation of -0.27 – this does not violate the random sampling assumption.[5] Random sampling requires that observations are independently drawn from the population, not that treatment assignment is random or uncorrelated with observed characteristics. The observed correlation reflects program uptake patterns rather than systematic selection into the sample. The continuous definition of treatment – measured by the share of full-time students or schools – satisfies condition (2). Since conversion to full-time schools was assigned by the Ministry of Education, treatment anticipation is limited, and outcomes are observed, satisfying condition (3). Finally, I consider conditions (4) and (5) reasonably satisfied, given the support of the parallel trends assumption – in the absence of diverging pretrends – provided by the event study graphs and the assumed restriction on treatment effect heterogeneity. To conclude, all identification criteria are reasonably satisfied, allowing for some causal interpretation of the coefficient of interest while acknowledging potential threats to identification as discussed in the previous section.

5.2. Multi-period Differences-in-Differences model

Using the second definition, treatment is a binary variable indicating whether a municipality offered at least one full-time high school and is therefore considered as treated. The empirical strategy to assess the effects of the ETI program under binary treatment follows the methodology introduced by Callaway and Sant'Anna (2021). This approach is particularly appropriate when treatment timing varies across units, treatment occurs over multiple periods, and the parallel trends assumption may hold only when conditioning on covariates. Rosa, Bettinger, et al. (2022) have applied this methodology to study the ETI reform's effect on test scores, demonstrating its effectiveness in estimating its causal effects.

Similar to the two-way fixed-effects approach, recent econometric literature highlights that, in a classical DiD with more than two time periods, heterogeneous treatment effects, and staggered adoption, the estimated regression coefficient represents a weighted average of the treatment effect across units and periods (De Chaisemartin & D'Haultfœuille, 2022; Callaway et al., 2024). Therefore, the standard DiD coefficient may not accurately capture the causal effect of the policy intervention.

The method proposed by Callaway and Sant'Anna (2021) addresses this issue by estimating the *group-time average treatment effect (GTATT),* which provides a causal interpretation of the parameter of interest. In their framework, the GTATT is defined as "the average treatment effect for group g at

[5] This is statistically significant at the 1% level.

time t, where a 'group' is defined by the time period when units are first treated." The average treatment effect on the treated (ATT) for each group is defined as:

$$ATT_{g,t} = E[Y_t(1) - Y_t(0) | G = g]$$

Where g indicates the group at time t. Note that g=1 corresponds to the last period before treatment. Essentially, treated groups are compared to not-yet-treated groups.

GTATTs can be aggregated into overall treatment effect parameters by averaging the effects for each group across all time periods, and then averaging across all groups, yielding an overall causal estimate of the program's impact. Callaway and Sant'Anna (2021) propose multiple aggregation methods; here, I adopt the most straightforward method, defined as follows. Let the GTATT be defined as:

$$\theta_s(g) = \frac{1}{\tau - g + 1} \sum_{t=2}^{\tau} 1\{g \leq t\} ATT(g,t)$$

and aggregate them as:

$$\theta_s = \sum_{g=2}^{\tau} \theta_s(g) \, P(G = g)$$

Where θ_s represents the overall effect of adopting the full-time schooling reform across all participating groups. Practically, I will run group-specific DiD regressions for each cohort and then aggregate them into a single overall treatment effect parameter.

5.2.1 Identification

Callaway and Sant'Anna (2021) outline five conditions for the identification in a multi-period, staggered treatment DiD model:

(1) Irreversibility of the treatment – once treated, units do not revert to pre-treatment status;
(2) Random sampling – observations are independent and identically distributed;
(3) Limited treatment anticipation – treated units cannot fully anticipate the intervention;
(4) Parallel trends – the parallel trends assumption holds at least when conditioning on covariates;
(5) Overlap – for every treated group, we must find a not-yet-treated group based on covariates.

In the context of the ETI program, when a high school adopts the full-time schedule, it receives state funding to expand infrastructure and support program implementation, ensuring that schools do not revert to a pre-treatment status within the same period. Data confirm no instances of schools switching

back after treatment, satisfying condition (1). While schools voluntarily opt into participating in the program, the Ministry of Education ultimately allocates resources based on school eligibility criteria and the presence of existing full-time secondary schools in the municipality. Consequently, conditions (2) and (3) – random sampling and limited treatment anticipation – are reasonably satisfied. The evidence delivered in the event study plots supports the validity of the parallel trends assumption, satisfying condition (4). The last cohort to adopt the full-time schooling schedule is in 2014, and to satisfy condition (5), I use this group as the final not-yet-treated comparison for the 2013 cohort; for all earlier periods, a comparable not-yet-treated group is available. To conclude, the identification criteria for this empirical framework are reasonably satisfied, allowing for causal interpretation of the coefficients of interest.

5.3 Potential Source of Biased Coefficients

A possible source of bias in the estimates in both empirical methods stems from the mechanism for allocation of public funding for the intervention and municipal characteristics. According to local authorities, high schools were eligible to participate in the program if they met minimum infrastructure requirements, including a science lab and a sports court. As discussed before, each municipality was intended to have at least one full-time secondary school. The distribution of high schools meeting the infrastructure requirements was uneven across the state, as displayed in Figure 1. By 2014, all municipalities offered at least one full-time high school, even though some of them still had no high school meeting the eligibility requirements.

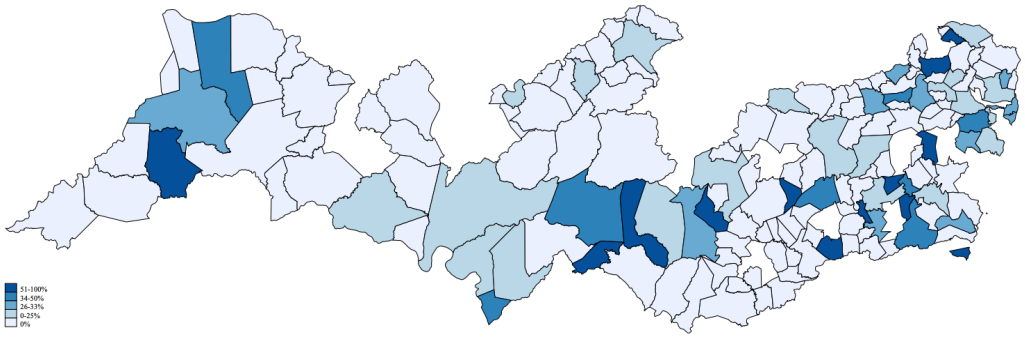


***Figure 1**: **Share of high schools with full-time eligibility at baseline in 2007**.* Eligibility for conversion is defined as meeting the minimum infrastructure requirements (sports court and science lab) for full-time schools. The map includes all municipalities in Pernambuco, excluding the island of Fernando de Noronha, and omits municipalities that already had full-time schools. Data sources are listed in the Data Appendix.

To estimate whether baseline eligibility of conversion is a source of bias and should potentially be included as a covariate, I ran a balance test for early versus late adopters of the program. Evidently, eligibility alone does not fully predict whether a municipality received treatment, and, as shown in Table 2, it does not hold significant explanatory power for the conversion of secondary schools. In

practice, school conversions seem to have been driven more by convenience than by strict eligibility criteria, making the assignment of treatment effectively random.

***Table 2**: Balance Table of Outcome Variables for Early versus Late Adopters*

	Early Adopters		Late Adopters		Pairwise t-test	Pairwise t-test
Outcome Variables	N	Mean/(SE)	N	Mean/(SE)	Mean difference	Mean difference
Teenage Suicide Rate (%)	51	0.02	451	0.03	-0.006	-0.007
		(0.09)		*(0.14)*		
Teenage Pregnancy Rate (%)	50	0.07	423	0.07	-0.001	-0.001
		(0.02)		*(0.02)*		
Teenage Homicide Rate (%)	51	0.35	452	0.35	0.000	-0.012
		(0.51)		*(0.54)*		
Selected Controls						
Baseline Eligibility	51	0.20	452	0.13	0.065	
		(0.30)		*(0.01)*		
Population	51	59,871	452	19,207	40,664***	40,313***
		(114,554)		*(635)*		
Share of Minors (%)	51	0.11	452	0.10	0.003*	0.003*
		(0.01)		*(0.00)*		
GDP per capita (R$)	51	4,502	452	4,648	-145	-202
		(2,598)		*(118)*		
Adult Crime Rate (%)	50	0.35	452	0.33	0.020	0.014
		(0.23)		*(0.01)*		
# Students per HS	51	500	452	465	35	29
		(252)		*(10)*		
Covariate						
Eligibility						✔

Notes: Balance table comparing Early Adopters of the ETI program (2008) to Late Adopters (2009–2014) among 166 municipalities in Pernambuco that eventually adopted the program. All variables are measured in pre-treatment periods only: outcomes for Early Adopters are observed in 2007, while outcomes for Late Adopters are observed prior to their own conversion and thus serve as not-yet-treated controls. Selected Controls report baseline socio-economic characteristics. The last two columns show pairwise t-tests without and with conditioning on baseline eligibility for ETI. Entries report means with standard errors in parentheses. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As can be observed based on the pairwise t-test, there are no significant differences in the outcome variables between early and late adopters of the treatment. Early adopters are defined as those municipalities adopting treatment in the first year, which corresponds to roughly 30% of the sample,

while late adopters include all other municipalities adopting treatment after 2008.[6] To further rule out other potential confounders, a set of controls is included in the balance table. The results indicate that neither a municipality's level of poverty, measured by GDP per capita, nor its average school size, measured by the number of students per high school, differs significantly between early and late adopters. This suggests that treatment timing was unlikely to be systematically driven by economic conditions or educational infrastructure. While economic development and schooling capacity could, in principle, influence adoption through administrative capacity, policy prioritisation, or resource availability, the results indicate that neither factor played a significant role.

Similarly, the level of crime in a municipality does not differ between early and late adopters, which is important since treatment assignment could have been correlated with crime if policymakers prioritised or avoided certain areas. The balance results also suggest that larger municipalities in terms of population tended to adopt treatment earlier, whereas smaller municipalities switched in later stages. This pattern implies a prioritisation strategy where policymakers targeted municipalities in which treatment would reach the largest number of students. Early adopters also had, on average, a slightly higher share of minors, indicating that they, on average, had more children compared to the number of adults in the municipality, but the difference is negligible, as it is very close to zero. Since the outcome variables are calculated as rates, which already account for population size, I do not include population as a covariate in the causal analysis. Having established that early and late adopters are largely balanced across relevant covariates, I proceed not using covariates in the main analysis.

[6] In the first conversion cohort 51 out of the 166 municipalities in the sample adopted the program, representing roughly 30% of all municipalities.

6. Results

The following results will deliver some evidence that the full-time schooling intervention affected socio-behavioural outcomes in adolescents. As indicated, the three relevant socio-behavioural outcomes this study focuses on are adolescent suicide, fertility and homicide rates. The primary findings I deliver are, arguably, attributable to the reduced time students have to engage in risky behaviour, the incapacitation effect, since I assess the rollout and accessibility of full-time schools in Pernambuco over time. Adopting the ETI schedule increases the time students spend in classrooms and thus reduces the number of hours they can spend on engaging in risky behaviour. By splitting the sample to assess the heterogeneity of effects in the subsequent section, I will deliver some insights into how the human capital effect affects outcomes through higher opportunity costs to risky behaviour. Likewise, I will discuss the degree to which increased social interactions lead to the peer exposure effect contributing to the observed outcomes.

This section is organised as follows. In the first part, I describe and track the gradual rollout of the ETI program in Pernambuco, and in the second part, I deliver and assess the empirical findings on the causal effect of the full-time schooling rollout on socio-behavioural outcomes of adolescents.

6.1. Descriptive Results

The implementation of the *Escola em Tempo Integral* (ETI) program in Pernambuco followed a gradual rollout, resulting in substantial variation across the municipalities in both timing and intensity of treatment – that is, the number of full-time high schools established.

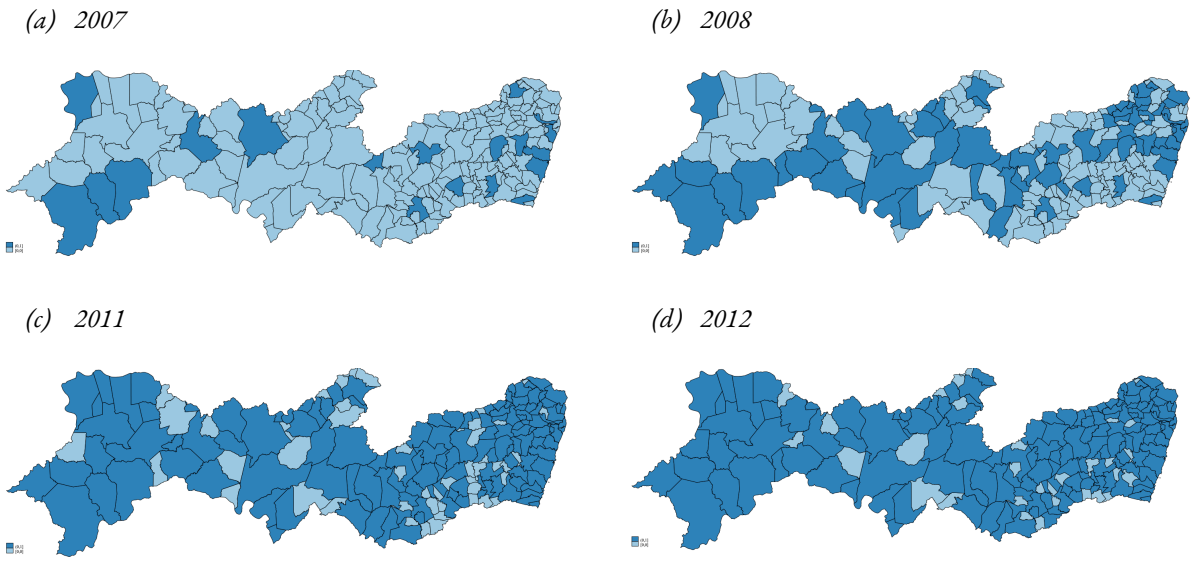


Figure 2: Geographical trends of municipalities adopting full-time high schools over time. Municipalities that adopted the

schooling-hour extension in at least one public high school are highlighted in dark blue. The map includes all municipalities in Pernambuco, excluding the island of Fernando de Noronha. Data sources are listed in the Data Appendix.

As shown in Figure 2, less than 20 municipalities introduced full-time secondary schools as early as the early 2000s through pilot initiatives, prior to the formal launch of the ETI program in 2008. These municipalities have been excluded from the following empirical analysis, leaving 166 municipalities in the sample. When the program was officially implemented in 2008, 105 schools in 51 municipalities from the sample adopted the full-time schooling model. By 2014, the initial target of the ETI reform was achieved: every one of Pernambuco's 185 municipalities operated at least one full-time high school.

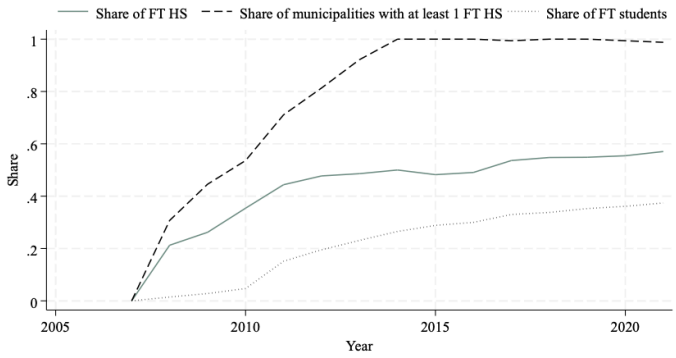


Figure 3: ***Trends in full-time schooling.*** The graph shows municipalities with at least one full-time (FT) high school, the share of schools that are FT, and the share of students attending FT schooling over time in Pernambuco. Shares are calculated for the 166 municipalities participating in the ETI program from 2008 onwards, including only non-private schools. Data sources are listed in the Data Appendix.

The rollout occurred gradually over multiple years (2008-2014), rather than simultaneously across the state. Moreover, once a municipality adopted the full-time model, it remained in treatment, reflecting the irreversibility of the policy and the consistency of implementation over time. Before 2008, only a small portion of students attended full-time high schools, consistent with the limited availability of pilot schools in selected municipalities. Even after the official ETI launch, enrollment in full-time secondary education remained modest in 2008 and 2009. In 2011, over 15% of all high school students were enrolled in full-time schools, marking a substantial increase. The share of students in full-time education continued to grow steadily after that, reaching almost 40% by 2025. In 2008, approximately 19% of high schools in Pernambuco operated on a full-time schedule. This share rose gradually with the ETI expansion, reaching about 46% of all high schools by 2015. Within the sample, the share of secondary schools operating on a full-time schedule increased sharply between 2008 and 2011, reaching approximately 44% of all secondary schools. Thereafter, the pace of school conversion slowed, reaching nearly 57% by 2021 (see Figure 3). Following the COVID-19 pandemic and the state government's reform of the full-time school plan in 2021, the proportion of full-time schools increased sharply to approximately 60% (see Figure I in the Appendix).

6.2. Empirical Results

The regression estimates from the two-way fixed-effects specification, along with the Callaway and Sant'Anna (2021) Difference-in-Differences results, are reported in Table 3 and illustrated in Figure 4. The subsequent three sections present and discuss the findings for each outcome variable in turn.

Table 3: *Regression Output of the ETI Program on Outcome Variables*

	Teenage Suicide Rate	**Teenage Pregnancy Rate**	**Teenage Homicide Rate**
	(1)	(2)	(3)
Two-Way Fixed-Effect	b/(SE)	b/(SE)	b/(SE)
Share of FT Students (%)	0.018	0.012**	-0.045
	(0.048)	*(0.004)*	*(0.185)*
Observations	2,395	2,212	2,485
Multiperiod DiD			
ATT	0.018	0.005**	0.001
	(0.017)	*(0.002)*	*(0.055)*

Notes: Regression results from two-way fixed-effects (TWFE) models and group-time ATTs estimated using the Callaway & Sant'Anna (2021) Difference-in-Differences approach. ATTs are aggregated using weighted averages by group size. TWFE specifications include municipality and year fixed effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students enrolled in full-time high schools. Columns (1)-(3) report adolescent suicide, pregnancy, and homicide rates, respectively. Coefficients are reported first, with standard errors in parentheses. Adolescent pregnancy rates correspond to births in the following year, accounting for average pregnancy duration. Data sources are listed in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01.

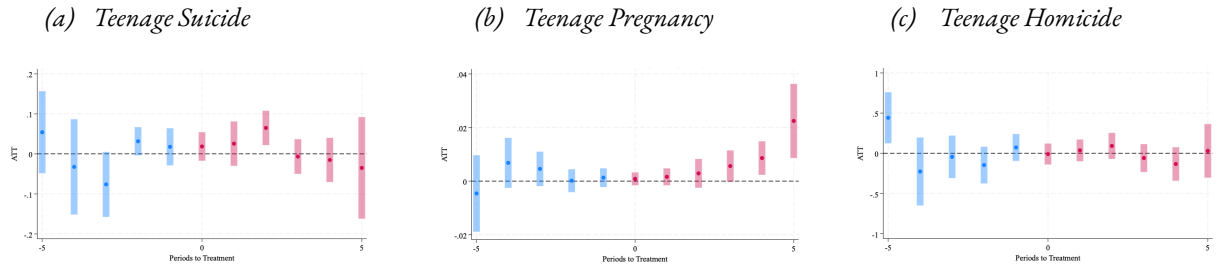


Figure 4: ***Event study plots of the school day extension program on adolescent socio-behavioural outcomes.*** Event-study estimates using the Callaway and Sant'Anna (2021) DiD framework for the 2008-2014 rollout, comparing treated and not-yet-treated municipalities. Group-level estimates are aggregated across cohorts using group-size weights. Pre-treatment coefficients (blue) and treatment/post-treatment coefficients (pink) are shown with 95% confidence intervals. Data sources are displayed in the Data Appendix.

6.2.1. Adolescent Suicide

I find that having greater access to full-time schooling has no statistically significant link to adolescent suicide rates in the full sample. The two-way fixed-effects results are reported in Table 3, accounting for unobserved heterogeneity across municipalities and year-specific effects, with standard errors clustered at the municipality level. A higher intensity of treatment – measured by a larger fraction of students in full-time schooling or the share of full-time high schools – does not have a measurable effect on teenage suicide rates (see Table II in the Appendix). The aggregated ATTs across treatment cohorts support the lack of a causal link, also being statistically insignificant from zero. In other words, in the full sample, extending the school day appears to have no detectable impact on students' mental health as captured by adolescent suicide rates.

Assessing the dynamic effects of the intervention on adolescent suicide rates yields similar results but reveals some insights into long-term responses to the intervention. First, they deliver evidence supporting the validity of the parallel trends assumption by showing no evidence of divergent pretrends since all pre-treatment coefficients are statistically insignificant (see Table III in the Appendix). In both specifications – with and without controlling for baseline eligibility – the second post-treatment coefficient is statistically significant, suggesting an increase in teenage suicide rates of approximately 0.062 - 0.065 per 1,000 inhabitants. This finding implies a delayed, short-term rise in teenage suicide rates, emerging two periods after treatment. Only when controlling for baseline eligibility, the pattern reverts in the fifth post-treatment coefficient, becoming significant and negative, indicating a modest reduction of 0.07 per 1,000 inhabitants as a long-term response to the program. This pattern suggests that, while the school day extension may initially be associated with higher teenage suicide rates, its long-term effects are statistically insignificant from zero or potentially even mitigating initial increases. The observed dynamic effects pose a puzzle and are thus far difficult to explain given existing research. One possible explanation could be a stress effect. Students who enrol in the full-time schooling program upon its launch might experience a sudden increase in stress and academic pressure, due to poor or insufficient integration of the program, leading to a short-term increase in mental health issues. The long-term insignificance of the coefficients could be associated with better implementation of the program in schools over time, not affecting students' mental health anymore. This puzzle poses an interesting direction for future research and could help in understanding how to implement structural schooling interventions without harming students' well-being.

6.2.2. Adolescent Fertility

The schooling hour intervention in Pernambuco has a statistically significant and positive link with adolescent fertility rates, indicating that a higher treatment intensity – through an increased share of students attending full-time schooling – is associated with a 1.2 percentage point increase in adolescent fertility rates (from column (2) of Table 3). This suggests that municipalities with larger shares of students in full-time education experience higher adolescent fertility, with the effect strengthening as the proportion of students in full-time schooling increases. The DiD results support this immediate causal effect of the intervention on teenage pregnancy rates, as indicated by the statistically significant aggregated ATT at approximately 0.005 percentage points, significant at the 5% level. This corresponds to a roughly 6.5% increase from the average adolescent fertility rate at baseline after adopting treatment.

The event study graphs deliver supportive evidence in favour of the validity of the parallel trends assumption since the pre-treatment coefficients are statistically insignificant and thus indistinguishable from zero. The dynamic effects reveal a pattern of the increase emerging a few periods post-intervention, as displayed in the event study in Figure 4: the fourth and fifth post-treatment coefficients are statistically significant and positive. These results are robust to conditioning on covariates, with the fifth post-treatment period, indicating an increase of 0.022 percentage points in adolescent fertility rates five periods after the intervention, corresponding to approximately a 28% increase from average baseline fertility rates (see Table III in the Appendix). This points towards a long-term adjustment of adolescents' behaviour in later cohorts instead of a short-term behavioural shock. Overall, the empirical results contradict the previous findings in scientific literature (e.g. Berthelon & Kruger, 2011; De Araújo Benevides et al., 2024), by finding a positive causal relationship between the school day extension and adolescent fertility instead of a negative one. This indicates that the incapacitation effect is outweighed by another mechanism driving fertility rates up. These findings deliver some tentative evidence in favour of the social interaction effect, where students' fertility rates increase due to larger peer exposure. Instead of students having less spare time to engage in risky behaviours leading to pregnancy, increased peer exposure could arguably lead to higher adolescent fertility rates over time. In the heterogeneity analysis, I find further evidence supporting this mechanism.

6.2.3. Adolescent Homicide

Finally, I examine whether the already established link between full-time schooling and crime outcomes (e.g. Jacob & Lefgren, 2003; Bertelon & Kruger, 2011) is replicable in my sample. The coefficient on the share of students in full-time schools is not statistically significant, which remains the case when

using the share of full-time high schools (see Table II in the Appendix). Supplementing these findings with the results from the DiD, both the ATT and post-treatment coefficients are statistically insignificant from zero. This suggests that the extension of schooling hours has no causal impact on adolescent crime, as measured by homicide rates.

The event study graph from Figure 4 delivers evidence supporting this, as well as the validity of the parallel trends assumption. Given that the four pre-treatment coefficients are statistically insignificant, this suggests that treated and not-yet-treated municipalities followed similar pre-trends, supporting the parallel trends assumption. Similar results are obtained when the regressions are estimated with conditioning on covariates (see Table III in the Appendix). Together, these findings indicate that there is no significant causal relationship between the ETI program and teenage homicide rates. This is also true when using the male homicide rate for adolescents, alike Rosa et al. (2022) did, highlighting that the pilot period of the ETI, which is included in their study, drives their findings of a significant negative causal link to homicide rates (see Table IV in the Appendix). These findings highlight that the previously found incapacitation effect – which is not only relevant for undertaking risky behaviour concerning fertility choices, but also concerning crime behaviour – is not a driving factor in the context of Pernambuco's full-time schooling reform and adolescent crime.

7. Heterogeneity Analysis

Following the aggregate results presented in the previous section, this chapter explores the heterogeneous effects of the full-day schooling reform to better understand what subgroups are particularly vulnerable to the observed outcomes. This section also delivers some insights into what effects might drive the observed outcomes. So far, this study assessed the extent to which full-time schooling impacts socio-behavioural outcomes through the incapacitation effect. In this section, I will deliver some suggestive evidence with regard to the human capital and the social interaction effect by leveraging the heterogeneous effects of the reform on certain subgroups.

This section is structured as follows. First, I assess whether the effects of the intervention differ between urban and rural municipalities. Differences in settlement type are not merely associated with population density but also, in principle, affect students' life prospects and daily routines, for instance, through access to fewer future job opportunities and/or longer commutes. Second, I examine whether municipalities with above- or below-median baseline outcomes experienced distinct effects. Areas with higher baseline vulnerability to adverse socio-behavioural outcomes may be affected differently from other municipalities. Understanding this can generate valuable insights for policymakers, particularly to avoid and mitigate adverse outcomes and to help shape useful and effective interventions.

7.1. Contrasting Dynamics of Urban and Rural Municipalities

The rural-urban divide reflects substantial differences in employment prospects, wage levels, and access to formal labour markets. Because these structural conditions influence the returns to education, the impact of full-time schooling may vary across rural and urban areas. Examining whether – and to which extent – the reform generates heterogeneous effects along this dimension can provide valuable insights into the operation of the human capital channel highlighted in previous research (e.g., Duflo et al., 2015; Favara & Sanchez, 2019). Urban areas have, arguably, higher returns to education due to better labour market prospects, whereas rural areas have fewer opportunities and thus, lower returns to education.

To examine whether the effects of the ETI intervention vary by settlement type, the sample of 166 municipalities was divided at baseline according to the share of rural high schools: municipalities above the sample median were categorised as *rural*, and those below as *urban*. Using the same empirical frameworks as in the previous section, I estimate the causal effects of the intervention on adolescent socio-behavioural outcomes. The results are reported in Table 4.

***Table 4**: Regression Output of Two-Way Fixed-Effects Regression for Urban and Rural Areas*

	Teenage Suicide Rate	Teenage Pregnancy Rate	Teenage Homicide Rate
	(1)	(2)	(3)
	b/(SE)	b/(SE)	b/(SE)
Rural Municipalities			
Share of FT Students (%)	0.003	0.012*	0.298
	(0.088)	*(0.006)*	*(0.215)*
Observations	1,175	1,110	1,240
Urban Municipalities			
Share of FT Students (%)	0.018	0.014*	-0.346
	(0.046)	*(0.006)*	*(0.291)*
Observations	1,220	1,102	1245

Notes: Regression results from two-way fixed-effects (TWFE) models and group-time ATTs estimated using the Callaway & Sant'Anna (2020) Difference-in-Differences approach for urban and rural municipalities. ATTs are aggregated using weighted averages by group size. TWFE specifications include municipality and year fixed-effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students enrolled in full-time high schools. Columns (1)-(3) report adolescent suicide, pregnancy, and homicide rates, respectively. Coefficients are presented first, with standard errors in parentheses. Adolescent pregnancy rates correspond to births in the following year, accounting for the average pregnancy duration. Data sources are listed in the Data Appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

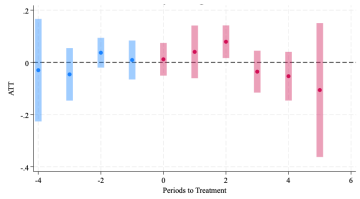
Consistent with the findings on the full sample, there is no statistically significant relationship between the full-time schooling intervention and adolescent suicide rates in either urban or rural areas. However, the event study plots in Figure 5 reveal a pattern similar to the aggregate findings – an initial increase in suicide rates followed by a gradual decrease until the post-treatment coefficients are statistically indistinguishable from zero. This dynamic appears only in rural municipalities, whereas urban municipalities are entirely unaffected with respect to their suicide rates. These findings highlight the same puzzle as previously. Extending the explanation of the stress effect to these results: rural municipalities – where schools might be worse equipped and have access to fewer resources to properly implement the reform – exhibit an initial deterioration of students' mental health. After proper implementation of the intervention, rural municipalities eventually show non-significant effects on suicide rates. These results offer causal evidence that the intervention may have adversely affected mental health outcomes among youth in rural municipalities in the short term. The lack of a statistically significant link to the intervention might be – at least partially – attributable to unequal execution of the program, not that the program intrinsically harms students' mental well-being.

Urban municipalities are more vulnerable to increases in adolescent fertility due to the intervention. The coefficients of the two-way fixed-effects regression are both statistically significant at the 10% level

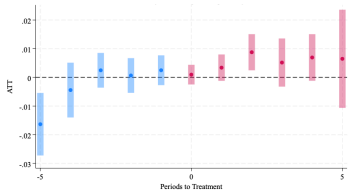
– 0.012 for rural and 0.014 for urban municipalities – indicating that higher shares of students in full-time secondary schools are associated with higher adolescent fertility rates, particularly in more densely populated areas (see column (2) of Table 4). The aggregated ATT for adolescent fertility in urban municipalities is statistically significant and positive, suggesting a 0.005 percentage point increase, roughly a 6,4% increase, with the effects emerging in the long-term (from Table V in the Appendix). In contrast, rural municipalities experience only a short-term increase in adolescent fertility rates, which does not last longer than three periods after intervention, with their ATT being statistically insignificant. The combined results deliver evidence in favour of a social interaction mechanism that is stronger in urban municipalities, suggesting that an increase in peer exposure due to extended school hours might facilitate more frequent social interactions, leading to a higher likelihood of adolescent fertility. With the incapacitation effect not driving adolescent pregnancy rates down – as highlighted and shown in the previous section – the social interaction effect could arguably drive this increase in rates. Furthermore, these findings deliver no evidence supporting a human capital mechanism. Returns to education are higher in urban areas where job opportunities are more accessible, and life prospects are brighter for adolescents, yet urban youths show significant increases in pregnancy rates rather than rural youths. This highlights that the opportunity cost of early motherhood, expressed through the human capital effect, may not be a driving factor in the fertility outcomes of Brazilian youth.

Rural Municipalities

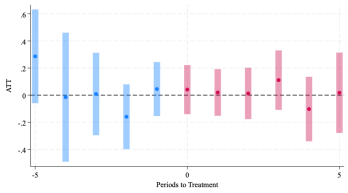
(a) Teenage Suicide Rates



(b) Teenage Pregnancy Rates

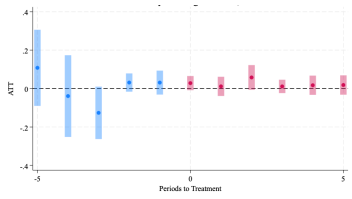


(c) Teenage Homicide Rates

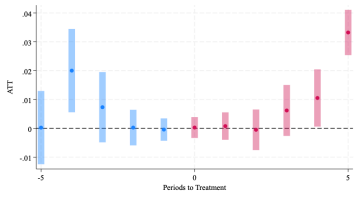


Urban Municipalities

(d) Teenage Suicide Rates



(e) Teenage Pregnancy Rates



(f) Teenage Homicide Rates

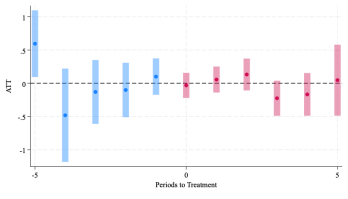


Figure 5: Event study plots of the school day extension program on adolescent socio-behavioural outcomes for urban and rural municipalities. Event-study estimates using the Callaway and Sant'Anna (2021) DiD framework for the 2008-2014 rollout, comparing treated and not-yet-treated municipalities. Group-level estimates are aggregated across cohorts using group-size weights. Pre-treatment coefficients (blue) and treatment/post-treatment coefficients (pink) are shown with 95% confidence intervals. Data sources are listed in the Data Appendix.

Lastly, the results for adolescent homicide rates align with the previous section. None of the coefficients in column (3) of Table 4 are statistically significant, and the event study plots show no post-treatment trends, reinforcing that the ETI program has no detectable causal effect on youth crime. This is also true when using adolescent male homicide rates (Table VI in the Appendix), delivering further evidence suggesting that (1) the schooling hour extension intervention had no causal link to adolescent homicide rates, and (2) that the previous findings by Rosa et al. (2022) were driven by the pre-treatment pilot years of the ETI program.

7.2. Contrasting Dynamics in High- and Low-Vulnerability Municipalities

Areas with higher baseline vulnerability to adverse socio-behavioural outcomes may be affected differently from other municipalities. Understanding this can generate valuable insights for policymakers, particularly to avoid and mitigate adverse outcomes and can help shape useful and effective policy interventions. To further explore this, I divide municipalities into groups with above- and below-median baseline levels of each outcome variable. Since the median suicide rate is zero, high suicide rates are concentrated among a few municipalities; the sample split is not balanced for this outcome variable. For the other two outcome variables, the sample split is balanced. I then re-estimate the same empirical models introduced in the methodology section, results are delivered in Table 5.

First, in contrast to the findings for the full sample, I find a statistically significant effect of the schooling hour extension program on adolescent suicide rates for areas with above-median rates at baseline. For those municipalities, the rollout of the full-time program is associated with a statistically significant increase of 0.205 deaths per 1,000 inhabitants, corresponding to a 51% increase from baseline average suicide rates for this group, as captured by the ATT. This suggests a potential worsening effect of longer schooling hours on students' mental health in already high-risk areas. The coefficient of the TWFE is not statistically significant. Municipalities with zero suicide rates[7] at baseline exhibit no significant causal effect upon the adaptation of the schooling hour intervention.

Second, municipalities with above-median adolescent fertility rates at baseline appear particularly susceptible to increases in teenage pregnancies following the schooling reform. Both the results from the TWFE, as well as the aggregate ATT results, are statistically significant, suggesting an increase in

[7] Since the median is zero, all municipalities with below-median suicide rates consequently have a zero suicide rate at baseline in 2007.

fertility rates after the program's rollout. The results indicate an increase of 0.6 percentage points corresponding to a roughly 7% increase from baseline average for this group following the reform. The effect only exists in municipalities with already high vulnerability towards increased pregnancy rates. These are important findings for policymakers since municipalities that could be targeted by the reform to improve adverse socio-behavioural outcomes end up being harmed by the intervention in the form of deteriorated adolescent fertility rates.

Table 5: *Regression Output of Two-Way Fixed-Effects Regression for above- and below-median Outcomes*

	Teenage Suicide Rates	**Teenage Pregnancy Rates**	**Teenage Homicide Rates**
	(1)	(2)	(3)
Two-Way Fixed-Effect	b/(SE)	b/(SE)	b/(SE)
Above-Median Rate			
Share of FT Students (%)	-0.197	0.017**	-0.099
	(0.171)	*(0.006)*	*(0.350)*
Observations	220	1,170	1,060
Below-Median Rate			
Share of FT Students (%)	0.027	0.008	0.109
	(0.050)	*(0.006)*	*(0.202)*
Observations	2,175	1,042	1,425
Multiperiod DiD			
Above-Median Rate			
ATT	0.205*	0.006**	0.048
	(0.088)	*(0.002)*	*(0.10)*
Below-Median Rate			
ATT	0.017	-0.002	0.017
	(0.017)	*(0.002)*	*(0.055)*

Notes: Regression results from two-way fixed-effects (TWFE) models and group-time ATTs estimated using the Callaway & Sant'Anna (2021) Difference-in-Differences approach for municipalities with above- and below-median baseline outcomes. ATTs are aggregated using weighted averages by group size. TWFE specifications include municipality and year fixed effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students enrolled in full-time high schools. Columns (1)-(3) report adolescent suicide, pregnancy, and homicide rates, respectively. Coefficients are reported first, with standard errors in parentheses. Adolescent pregnancy rates correspond to births in the following year, accounting for average pregnancy duration. Data sources are listed in the Data Appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Supplementing my previous findings, there is no statistically significant relationship between the schooling hour extension and adolescent homicide rates, further confirming that the ETI program does not affect youth criminality. Both regression outputs – the aggregated ATTs and the two-way

fixed-effects coefficients – support this conclusion. Possible explanations for this include the ineffectiveness of the incapacitation effect in the Brazilian full-time context, which I have found multiple evidence for, or possibly self-selection, where at-risk youth may choose part-time schooling or drop out of school altogether. This finding is relevant for policymakers to design interventions targeting the reduction of adolescent crime in the Brazilian context effectively. Understanding that time alone – and with that opportunity – is not a driving factor in youths' choice to engage in deadly crime might help to develop more effective policies that can help keep adolescents safe.

To summarise, the heterogeneity analysis examined differences in outcomes along the rural-urban divide as well as for areas with above- and below-median baseline vulnerability to outcome variables. I find no evidence in favour of the human capital effect for any of the outcomes. Furthermore, I find evidence of the same puzzle regarding the effect of the intervention on suicide rates. Rates in rural municipalities and those with high baseline vulnerability increase two years post-intervention and then decrease again until they are statistically insignificant. In the absence of individual-level data, I cannot draw definite causal conclusions tying the reform to these mechanisms. For future research, it would be highly relevant to understand individual-level mechanisms and gender-specific effects by using individual-level data. Furthermore, in order to rule out the previously mentioned potential source of selection bias through self-selection of richer students into the program, a split between different socio-economic backgrounds would be highly insightful. This would also be interesting in understanding whether the increase in pregnancy rates is caused by poor or rich individuals, and would allow definite causal conclusions about the relevance of the human capital effect in this context.

8. Robustness Checks

The main focus of the robustness checks is on the outcome variables. The definition of outcome variables and their sensitivity to extreme outliers may heavily impact the causal link drawn to the schooling intervention. First, to assess the robustness to outliers, I re-estimated the models using top-coded outcome variables. The results are reported in the first part of this section. Second, to evaluate the gravity of an important source of potential bias – the dropout rate of adolescents between middle and high school, I assess the effect of the schooling hours intervention on high school continuation rates, particularly for the *rural/urban* split. The results are displayed and discussed in the second part of this section.

8.1. Robustness of Regression Coefficients to Alterations in Outcome Variables

To assess the robustness of my regression results to extreme outliers, I conducted the same statistical analysis using top-coded outcome variables. Specifically, all observations above the 99th percentile were replaced with the 99th-percentile value to mitigate the influence of extreme outliers towards the top. This is particularly relevant since some of the outcome variables – suicide and homicide – are comparatively few incidents, and for which an additional case affects the rates substantially more than for fertility rates.

***Table 6**: Regression Outputs for Top-coded Outcome Variables*

	Teenage Suicide Rate	**Teenage Pregnancy Rate**	**Teenage Homicide Rate**
	(1)	(2)	(3)
	b/(SE)	b/(SE)	b/(SE)
TWFE Output			
Share of FT students (%)	-0.003	0.013**	-0.044
	-0.054	*(0.004)*	*-0.175*
Observations	2,490	2,490	2,490
DiD Output			
ATT	0.050*	0.002	0.01
	(0.020)	(0.002)	(0.055)

Notes: Regression results from two-way fixed-effects (TWFE) models and group-time ATTs estimated using the Callaway & Sant'Anna (2021) Difference-in-Differences (DiD) approach. TWFE specifications include municipality and year fixed effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students enrolled in full-time high schools. ATTs are aggregated using weighted averages by group size. Columns (1)-(3) report adolescent suicide, pregnancy, and homicide rates, all top-coded at the 99th percentile. Adolescent pregnancy rates correspond to births in the following year, accounting for the average pregnancy duration. Data sources are listed in the Data Appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results, reported in Table 6, closely mirror those obtained from the original outcome variables. Contrary to my previous findings, lacking statistical significance on the link between the schooling hour extension program and adolescent suicide rates, the ATT using top-coded suicide rates is positive and statistically significant. This suggests that after removing outliers from the dataset, there is a positive and statistically significant increase in suicide rates due to the intervention. These results supplement my findings from the heterogeneity analysis, suggesting that some municipalities – those with higher levels of baseline vulnerability – showed adverse effects in students' mental health as captured by adolescent suicide rates. This is a relevant finding indicating that the incapacitation effect, in fact, is not sufficient in order to reduce adolescent suicide rates.

The estimated effect of the schooling hour extension on adolescent fertility rates is even stronger in the top-coded data for the two-way fixed-effects output, suggesting that the main findings are not driven by a few extreme observations. The lack of statistical significance of the ATT can be explained by balancing effects post-treatment: an initial, not statistically significant, decrease in adolescent fertility rates followed by noisy increases in fertility rates that become statistically significant five periods post-treatment (see Figure 6). The trend observed in the main measures, as well as in the top-coded outcome variables, is very similar, supporting the findings on a positive link between the schooling hour intervention and adolescent fertility rates.

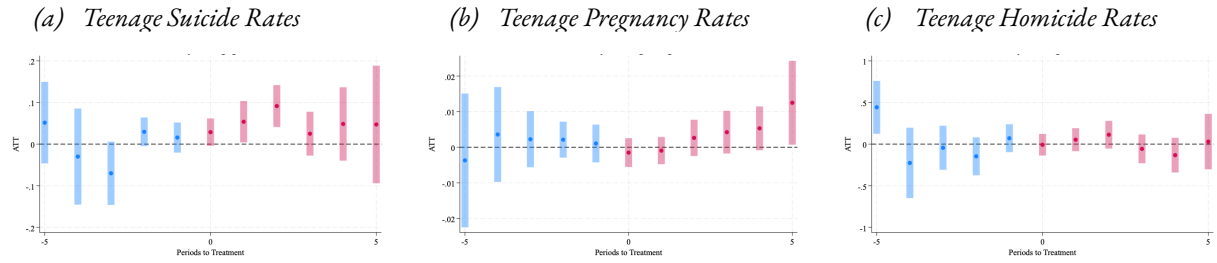


Figure 6: Event study plots of the school day extension program on top-coded adolescent socio-behavioural outcomes. Event-study estimates using the Callaway and Sant'Anna (2021) DiD framework for the 2008-2014 rollout, comparing treated and not-yet-treated municipalities. Group-level estimates are aggregated across cohorts using group-size weights. Pre-treatment coefficients (blue) and treatment/post-treatment coefficients (pink) are shown with 95% confidence intervals. Coefficients are top-coded at the 99th percentile. Data sources are listed in the Data Appendix.

The findings are also robust to different definitions of adolescent fertility rates. Specifically, I restricted the fertility measure to births among 15-18-year-old women, thereby excluding 19-year-olds who might introduce potential confounding effects due to differing life circumstances and educational trajectories. This narrower age range provides a cleaner identification of the reform's impact on younger adolescents, who are most directly affected by changes in schooling schedules and ensures an exclusion of those girls who might already have graduated from school from the sample. In addition, I constructed a birth exposure variable that sums the number of teenage births in the current and

following year. This measure captures the delayed fertility response, accounting for the temporal lag between conception and birth. Both measures deliver positive and statistically significant results, indicating a positive causal link between the schooling hour intervention and adolescent fertility rates (see Table VI in the Appendix).

When looking at average maternal age at childbearing as an alternative measure to adolescent pregnancy rates, I find no statistically significant link between the schooling reform and average maternal age. Previous research on full-time schooling reforms (e.g., Domínguez & Ruffini, 2020) has found evidence that extended school hours can delay childbearing. My findings, however, indicate an increase in adolescent fertility rates linked to the intervention, prompting the question of whether women simply shift births to earlier ages rather than increasing total fertility. My findings, presented in Table VII in the Appendix, suggest that the ETI program does not influence the timing of motherhood, despite its association with higher adolescent fertility rates. This is evidence in favour of an earlier starting time of motherhood rather than a general shift of childbearing ages for teenage girls.

Consistent with previous analyses, there is no evidence of a causal relationship between the intervention and homicide rates in the top-coded dataset, reinforcing the validity of earlier results.

8.2. Patterns in High School Continuation Rates

When evaluating the reform's broader socio-behavioural effects, it is also essential to examine whether students' educational progression was affected. It is reasonable to argue that the reform might lead to a self-selection of students into schools and dropouts depending on their life trajectories. In the absence of individual-level data, the way to test whether more students dropped out of school between middle and high school upon launch of the ETI program is to assess the link to high school continuation rates. Table 7 presents two-way fixed-effects regression results for high school dropout rates as a function of the share of students not continuing from ninth grade to tenth grade. Due to limited enrollment data for each year of school, the measure is a proxy using the average number of students in a cohort in middle school and high schools, respectively and calculating the continuation rates based on that.

The results indicate that the schooling reform reduced dropout rates between middle and high school. The estimated coefficients are statistically significant in the full sample as well as in both the urban and rural subsamples, suggesting that the reform did not lead to higher dropout rates – in fact, it appears to have improved school continuation between middle and high school. Furthermore, the Callaway and Sant'Anna (2021) Difference-in-Differences estimates yield positive and statistically significant ATT for the full sample as well as the *rural/urban* subsample, as reported in Table VIII of the Appendix. These results provide additional evidence that the ETI program fostered student retention, reducing dropout

rates by 7.6% - 8.2% depending on the subsample, mitigating concerns that the extended school day could have increased dropout risks. This is true for all municipalities regardless of their population density. However, dropout rates between middle schools and high schools remain high in the sample, with a baseline average of roughly 68%[8] introducing a potential source of bias in the results.

These findings support my previous results by suggesting that (1) students continued to attend secondary schools even after the launch of the intervention, suggesting that the increased rates were not caused by students simply dropping out of school.

***Table 7**: Regression Estimates for High School Dropout Rates*

	High School Dropout Rates
	(1)
	b/(SE)
Full Sample	
Share of FT Students (%)	-0.414***
	(0.029)
Observations	2,490
Rural Municipalities	
Share of FT Students (%)	-0.357***
	(0.036)
Observations	1,245
Urban Municipalities	
Share of FT Students (%)	-0.468***
	(0.044)
Observations	1,245

Notes: Estimates from two-way fixed-effects regressions for rural and urban municipalities. All specifications include municipality and year fixed effects, with standard errors clustered at the municipality level. Coefficients are reported with standard errors in parentheses. The dependent variable is the dropout rate between middle and high school, and the continuous treatment variable is the share of students enrolled in full-time high schools. Results are presented for the full sample and separately for urban and rural municipalities. Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01.

[8] This includes those students who exit the public schooling system, those who drop out entirely and those who move states. The rate merely captures those students continuing within the public system.

9. Discussion and Conclusion

9.1. Discussion

As various other empirical investigations (e.g. Rosa et al., 2022; Berthelon & Kruger, 2011; Jacob & Lefgren, 2003) have examined, this study assesses the relationship between structural education interventions and life outcomes of students. However, it contrasts previous findings in various ways by (1) delivering evidence of the extension program increasing adolescent fertility rates - particularly amongst municipalities with higher baseline vulnerability – refuting findings on other Latin American countries proving their programs reduced adolescent fertility rates; and (2) finding no significant causal link between the schooling hour intervention and adolescent homicide rates it contradicts previous research on the ETI intervention (Rosa et al., 2022) by focussing solely on the time frame of the official implementation itself. Additionally, this study delivers the first causal evidence on the link between structural schooling interventions and adolescents' suicide rates, suggesting an initial deterioration of mental health of students upon the launch of the intervention, particularly in those municipalities with higher baseline suicide rates, and a later improvement of it until the effect of the intervention is indistinguishable from zero, posing a puzzle. One possible explanation for this effect is a stress effect where poor implementation of the reform leads to increased stress in adolescents in the first two years after the launch.

Several limitations must be acknowledged: First, the study relies on municipality-level aggregated data, which may mask individual-level mechanisms and within-municipality heterogeneity. Without microdata, it is not possible to determine whether the observed mental health and fertility effects stem from changes in individual behaviour, peer dynamics, or broader community trends. Furthermore, in the absence of individual-level data, it is impossible to draw direct causal links between full-time schooling attendance of an individual and their individual-level probability for life outcomes. There are several threats to identification that need to be acknowledged. First, as previously mentioned, a possible selection bias with respect to individuals from poorer socio-economic backgrounds might occur. Due to the high opportunity cost, families who are highly dependent on children's support at home might opt not to enrol their children in full-time schooling. Given the data limitations, there is currently no possibility to test whether this is, in fact, true and to consider it in my empirical design. Another possible threat to identification are movers and those who are registered in one municipality but attend school in another municipality, causing potential spill-over effects. This factor I cannot rule out in the absence of individual-level data and school assignment rules in Pernambuco. Another threat might be students who drop out of school. Given the data limitations, it is impossible to draw conclusions about whether rates are elevated by risky choices of those students who are still in school or of those who

dropped out of school. I delivered some evidence suggesting that the intervention led to increased continuation rates in high school, supporting the validity of my findings. However, with no year-specific dropout rates from high school, the possibility remains that the outcome variables are inflated by cases associated with students who are high school dropouts. Furthermore, the ETI reform may have changed which students are observed in the data, which might affect socio-behavioural outcomes at the municipal level. For instance, before the reform, some individuals – those with high risk levels – might have dropped out of schools, potentially partially migrated, and the incidents of suicide and fertility rates would have occurred in another state. After the implementation of the reform, adolescents remain enrolled and resident in Pernambuco, with their outcomes entering municipality-level statistics. This could lead to aggregate rates rising even though school attendance improves individual-level prospects. Given current data limitations, I am unable to test or capture the existence of this effect.

Second, despite extensive robustness checks – including top-coding and some alternative measures of outcome variables – the empirical results cannot fully rule out unobserved confounding factors related to treatment timing. Although balance tests and robustness analyses have delivered supportive evidence of my identification strategy, other cofounders are possible. Despite evidence supporting the validity of the parallel trends assumption, the possibility of policy endogeneity and treatment anticipation remains. In the absence of detailed public documentation of the program rollout procedure, I cannot fully exclude non-randomness of program assignment. However, the balance test results suggest that no systematic factors other than the population of a municipality were associated with program assignment. With the exclusion of private secondary schools from the sample, a fraction of students are not accounted for in the explanatory variables, whereas they might be counted towards the dependent variable, introducing another potential factor of bias, particularly if those students changed their behaviour significantly due to other factors at the same time as the intervention.

Third, data constraints limit the analysis of certain mechanisms: the lack of information on schooling quality, curriculum changes, and reproductive health education prevents a more precise interpretation of the findings. Effects of the ETI attributed to the extension of schooling hours might also be affected by other alterations – like staff changes, new teaching methods, and curriculum changes – due to the intervention. Essentially, changes may be attributable to *how* students spend their time in school rather than *how much* time is spent in school. This may be particularly relevant to understand the observed links to health outcomes that may be associated with other educational interventions on the school level – particularly those focusing on reproductive and mental health education. Similarly, the absence of gender-specific disaggregated outcomes prevents drawing conclusions on gender-specific dynamics. Some previous research drew conclusions between full-time interventions and students'

socio-economic backgrounds (e.g. Berthelon & Kruger, 2011), which allowed for relevant insights into individual-level mechanisms tied to the reform.

Fourth, with Brazil launching numerous social programs since the early 2000s, i.e. *Bolsa Familia* (conditional cash transfer), *Programa Saúde na Escola* (health program in schools) or *Fome Zero* (food service at school), there might be effects or spillovers attributed to ETI that were causally linked to other interventions. However, none of these programs started in 2008 – most of them were rolled out from 2003 onwards, others in 2007. Thus, merely their long-term effects might be counted towards the link between schooling hour extension and life outcomes. However, since links to *Bolsa Familia* on the three outcome variables are generally found to be negative (Olson et al., 2018; Alves et al., 2018; Machado et al., 2018), the effects of the ETI program are likely to be underestimated.

Finally, the findings might be context-specific, thus lacking external validity. Pernambuco's ETI program represents a unique case of a large-scale, state-led schooling reform, and its effects may not generalise to other contexts and educational systems. Furthermore, the socioeconomic context in Pernambuco is quite critical and social mobility is low; these factors might also limit the generalizability of the findings.

9.2. Conclusion

This study examined the causal effect of the ETI schooling hour extension program and socio-behavioural life outcomes of students in Pernambuco. Using a two-way fixed-effects regression and supplementing the findings with a staggered Differences-in-Differences approach following Callway and Sant'Anna (2021), I leveraged the gradual rollout of the intervention across municipalities to identify both average and heterogeneous treatment effects, allowing me to draw some conclusions on particularly vulnerable groups and potential effects driving these results.

Overall, the results indicate that the ETI program has no statistically significant causal impact on adolescent suicide and homicide rates in the full sample. These findings contrast with previous work from Rosa et al. (2022), who found reductions in homicide rates associated with the ETI full-time schooling program. This suggests that the incapacitation effect is not relevant in reducing adolescent suicides and homicides in the Brazilian context. However, I find a statistically significant and positive link between the intervention and adolescent fertility rates. Municipalities with larger shares of students in full-time schooling experience higher teenage pregnancy rates, suggesting that while the reform is successful in expanding educational attainment, it leads to some unintended social outcomes. This particularly concerns municipalities with higher baseline fertility and, thus, with higher vulnerability and urban municipalities. This finding contrasts with previous work finding significant

reductions in adolescent fertility rates due to schooling hour extensions (e.g. De Araújo Benevides et al., 2024; Berthelon & Kruger, 2011). The dynamic effects reveal that the fertility response emerges gradually, with positive coefficients appearing four and five years post-intervention. This suggests that the increase in fertility rates is not a short-term shock but rather a long-term behavioural adjustment. This is evidence that the incapacitation effect is not a relevant factor keeping adolescents from engaging in risky choices. The suicide results, while statistically weaker, indicate an initial noisy increase in suicide rates followed by a modest decline in later periods, hinting at complex mental health dynamics possibly related to adjustment periods following the schooling reform. This poses a puzzle which is thus far difficult to explain, given existing research and opens up interesting paths for future research. Homicide rates, in contrast, remain unaffected throughout, suggesting that the ETI reform did not alter youth's involvement in severe crime.

The heterogeneity analysis provides further insights into which groups are most affected and delivers some suggestive insights into the relevance of the human capital and social exposure effect. Urban municipalities show greater vulnerability towards an increase in adolescent fertility rates, the more students are enrolled in full-time high schools, while rural municipalities exhibit similar but less pronounced patterns. Furthermore, dynamic effects on urban municipalities suggest an emerging increase in adolescent fertility rates over time. Likewise, municipalities with higher baseline suicide rates also exhibit larger vulnerability to an increase in suicide rates due to the intervention. This is suggestive evidence in favour of the reform's social effects varying according to local socioeconomic contexts and preexisting vulnerability levels. Importantly, the ETI program successfully reduced dropout rates between middle school and high school, highlighting its educational benefits despite its unintended social outcomes.

Despite its limitations, as discussed in the previous section, the findings yield several relevant policy implications.

First, while the ETI program appears to have achieved its educational purpose – and pioneered the full-time schooling reform in Brazil – policymakers should acknowledge the unintended socio-behavioural outcomes associated with the intervention, particularly those around adolescent fertility. This underscores the need for complementary interventions targeted at improving reproductive and sexual health education, and expanding access to contraception and reproductive health facilities, as well as health counselling services targeted towards adolescents - particularly in municipalities that have been identified as vulnerable (as suggested by Novella & Ripani, 2016; Liu & Wang, 2024). Second, the results suggest that schooling interventions cannot solely be evaluated based on academic outcomes. Extended schooling hours alter socio-behavioural responses for adolescents

through changes in their daily routines and social environment, impacting their broader life choices. Integrating mental health services and awareness campaigns might be a necessary step to mitigate negative spill-overs on well-being and suicide rates (as introduced and assessed by Hall, 2010; Zhang et al., 2023; and Kimber et al., 2007). Third, the absence of significant effects of the intervention on crime rates suggests that school-based time substitutions may not be sufficient to influence youth's behavioural outcomes in this domain. Effective prevention strategies may require a combination of school**,** community**,** and family-based interventions, rather than relying solely on structural extensions of schooling time – especially since the incapacitation effect has been found not to play a significant role in altering youth's choices for risky behaviour. Furthermore, the heterogeneity results across urban and rural municipalities suggest the need for localised policy interventions. Tailoring the program intervention to local socioeconomic realities while considering specific vulnerabilities could help balance academic benefits with the social and psychological needs of students. It is also important to ensure proper implementation of the program, as poor program execution could lead to adverse effects on socio-behavioural outcomes in adolescents. Finally, in the absence of insights regarding to potential cannibalisation effects of the program for younger students, this possibility must be acknowledged and prevented. Ensuring that schooling quality and access remain the same for younger students, even with the rollout of the ETI program, is crucial not only to provide equal access to education but also to avoid unintended displacement effects that could undermine learning outcomes and long-term human capital formation among cohorts not directly targeted by the intervention.

In sum, this study provides novel empirical evidence on the socio-behavioural outcomes of a full-time schooling intervention in a developing-country context. While the ETI program successfully improves educational outcomes and reduces drop-out rates, it also appears to have some unintended socio-behavioural effects on adolescent fertility and suicide rates, with heterogeneous effects between regions and demographic groups. These findings highlight the importance of integrating education policy with social and health policy in order to ensure the reform promotes academic achievement while safeguarding the well-being of adolescents.

Future research should build on these findings by employing microdata to assess individual-level mechanisms and gender-specific trends driving these patterns and to understand the mechanisms behind the short-term effect on suicide rates. Such evidence could help design targeted policy interventions and more holistic school reforms that enhance both educational attainment and adolescent welfare.

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Appendix

I. Data Appendix

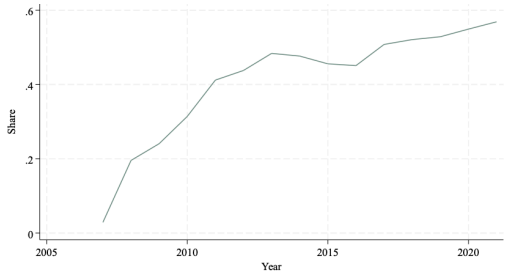
***Table I**: Data Appendix*

Variable	Description	Database
Explanatory Variables		
Share of students in FT high school (%)	Number of students enrolled in public full-time high schools in municipality m/ total number of students enrolled in public high schools in municipality m	INEP
Treatment Year	Year in which a municipality first adopted full-time high schools	INEP
Share of FT high schools (%)	Number of public full-time high schools in municipality m / total number of public high schools in municipality m	INEP
Dependent Variables		
Teenage Homicide Rate (%)	Total number of homicides (age 15-19) in municipality m/ number of adolescents aged 15-19 in municipality m	DATASUS
Teenage Suicide Rate (%)	Total number of suicides (age 15-19) in municipality m/ number of adolescents aged 15-19 in municipality m	DATASUS
Teenage Pregnancy Rate (%)	Total number of births by mothers aged 15-19 in municipality m/ number of female adolescents aged 15-19 in municipality m	SINASC, DATASUS
Underaged Pregnancy Rate (%)	Total number of births of underaged mothers (age 15 - 18) in municipality m/ number of female adolescents aged 15-18 in municipality m	SINASC, DATASUS
Births exposure variable (year n)	Number of teenage births (age 15 - 19) in year n and municipality m + number of teenage births (age 15 - 19) in year n+1 and municipality m	SINASC
High School Continuation Rates (%)	1 - (Average number of students in grade 10 in municipality m/ average number of students in grade 9) in municipality m	INEP
Average Maternal Age	Average maternal age at childbirth in municipality m	SINASC
Adolescent Male Homicide Rate (%)	Number of adolescent male homicides in municipality m/ number of adolescents in municipality m	DATASUS

Controls and Other Variables		
Baseline Eligibility (%)	Number of high schools with full-time eligibility in municipality m/ total number of high schools in municipality m	INEP
Population	Number of residents in municipality m	DATASUS
Share of Minors (%)	Number of adolescents in municipality m/ number of adults in municipality m	IPEA
GDP per capita (R$)	GDP per capita in Brazilian Real in municipality m	IBGE
Adult Crime Rate (%)	Number of Homicides in municipality m/ number of residents in municipality m	IPEA, DATASUS
# Students per School	Number of high school students in municipality m/ number of high schools	INEP
Share of Rural High Schools (%)	Number of public rural high schools in municipality m/ number of public high schools in municipality m	INEP
Notes: Description of all variables used in the empirical analysis examining the causal link between schooling-hour extensions and students' socio-behavioural outcomes. Data are aggregated to the annual level for the period 2007-2021. All data are publicly available from official sources. The first column presents the variable name, the second column details the calculation method, and the third column indicates the data source.		

II. Full-Time Schooling Program Rollout

(a) Share of full-time high schools in Pernambuco over time



(b) Number of full-time high schools in Pernambuco over time



Figure I: Statistics on full-time schooling in Pernambuco over time. Panel (a) depicts the share of high schools adopting full-time schooling over time, while panel (b) shows the absolute number of full-time secondary schools. The sample includes all municipalities in Pernambuco from 2007 to 2021. Data sources are provided in the Data Appendix.

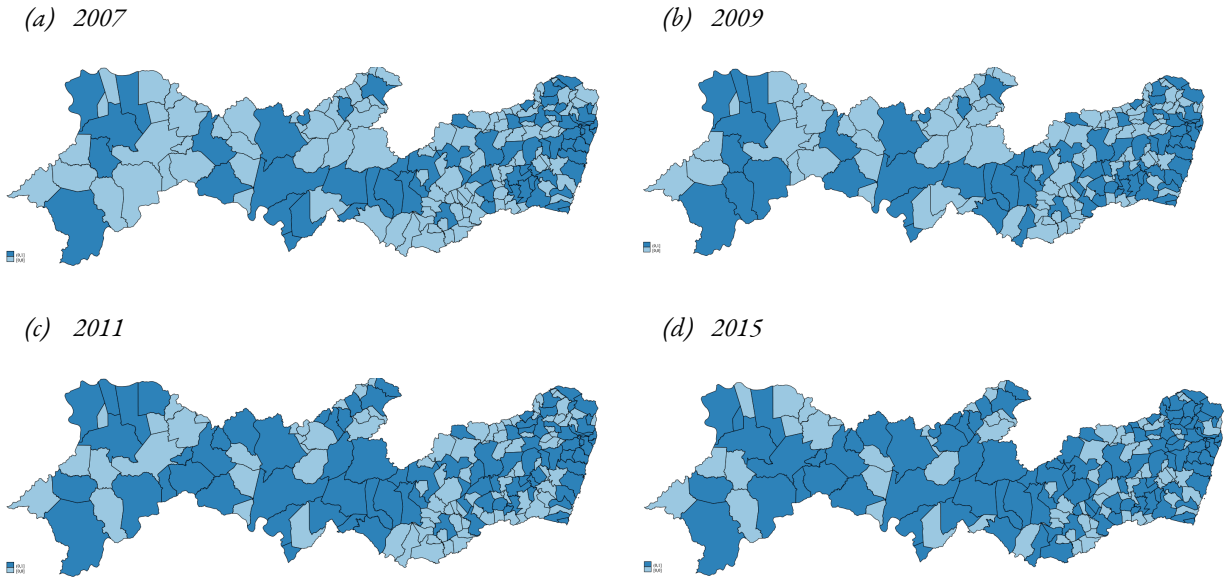


Figure II: Eligibility for conversion to full-time schooling over time. The figure shows the geographic distribution of municipalities in Pernambuco with at least one high school meeting the eligibility criteria for full-time conversion between 2007 and 2015. Eligibility requires minimum infrastructure, including a sports court and a science lab. Darker blue indicates the presence of at least one eligible school. Data sources are provided in the Data Appendix.

III. Variation in Socio-Behavioural Outcome Variables

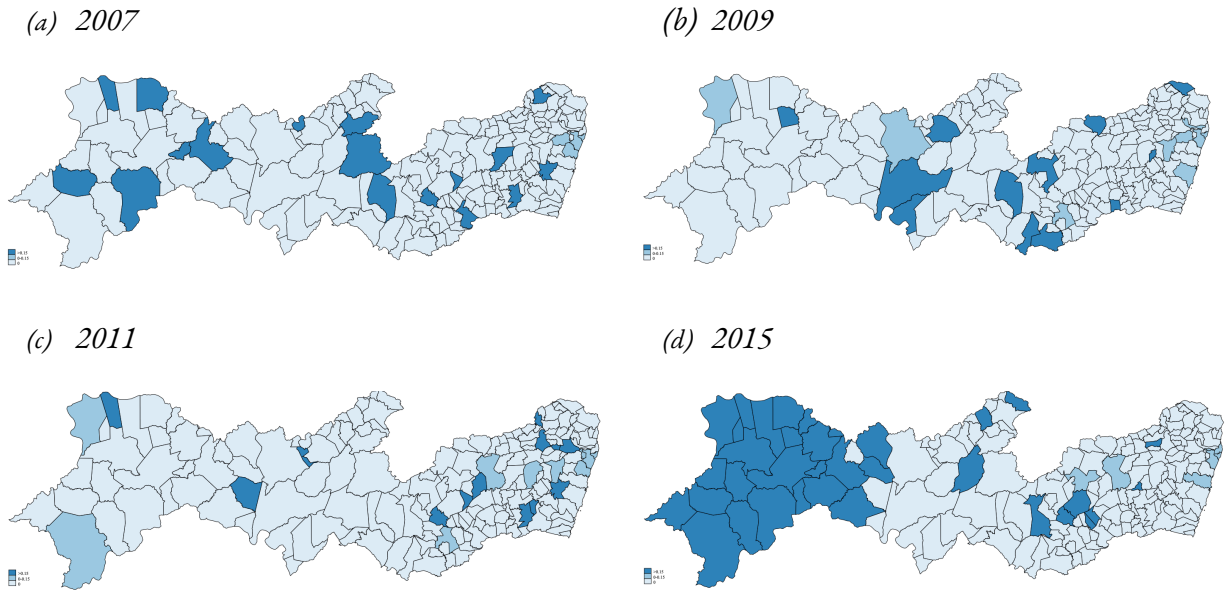


Figure III: Adolescent Suicide Rates over time. The figure illustrates the geographic variation in adolescent suicide rates in Pernambuco from 2007 to 2015, measured as the number of suicides per 1,000 inhabitants. Darker blue shades indicate higher rates. Data sources are provided in the Data Appendix.

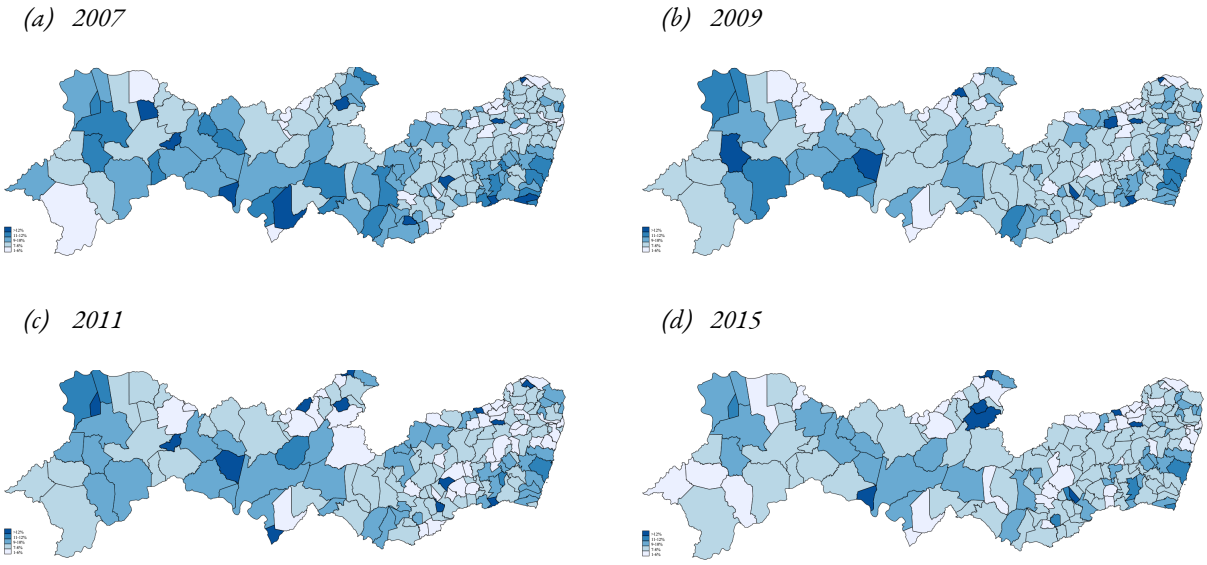


Figure IV: Adolescent Pregnancy Rates over time. The figure illustrates the geographic variation in adolescent pregnancy rates in Pernambuco from 2007 to 2015, measured as the number of births to mothers aged 15-19 per girls in the same age group. Darker blue shades indicate higher rates. Data sources are provided in the Data Appendix.

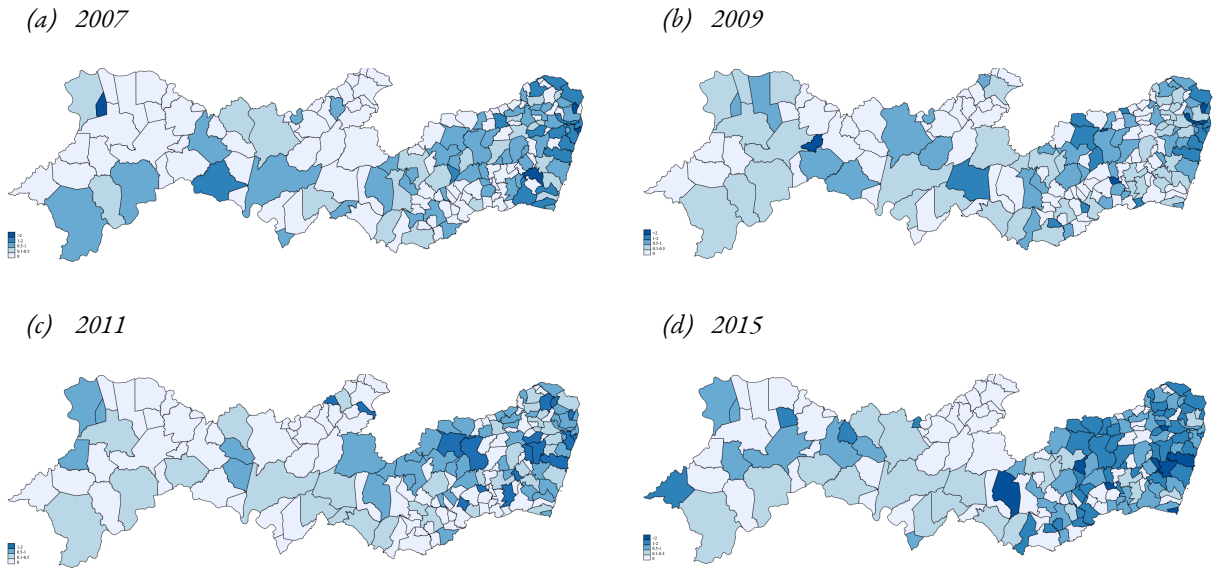


Figure V: Adolescent Homicide Rates over time. The figure depicts the geographic variation in adolescent homicide rates in Pernambuco from 2007 to 2015, measured as the number of homicides per 1,000 inhabitants. Darker blue shades indicate higher rates. Data sources are provided in the Data Appendix.

IV. Supplementary Regression Outputs

***Table II**: Output of Two-Way Fixed-Effects Regression using Share of FT High Schools*

	Teenage Suicide Rate	**Teenage Pregnancy Rate**	**Teenage Homicide Rate**
	(1)	(2)	(3)
	b/(SE)	b/(SE)	b/(SE)
Share of FT HS (%)	0.012	0.001	-0.029
	-0.015	*-0.001*	*-0.053*
Constant	0.035***	0.067***	0.515***
	-0.008	*-0.001*	*-0.029*
Observations	2,395	2,212	2,485

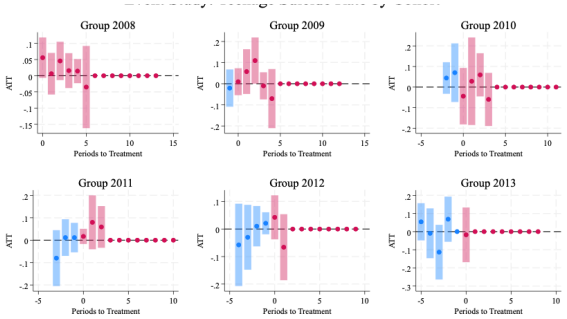
Notes: Regression output from the two-way fixed-effects (TWFE) specification, including municipality and year fixed-effects. Standard errors are clustered at the municipality level. Columns (1)-(3) correspond to the outcomes adolescent homicide, suicide, and pregnancy rates, respectively. Each entry reports the coefficient first, followed by the standard error in parentheses. The continuous treatment variable is the share of students enrolled in full-time high schools. Adolescent pregnancy rates reflect births in the following year, capturing the average pregnancy duration. Data sources are provided in the Data Appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

***Table III**: Event-Study Estimates of School Day Extension on Teenage Outcomes*

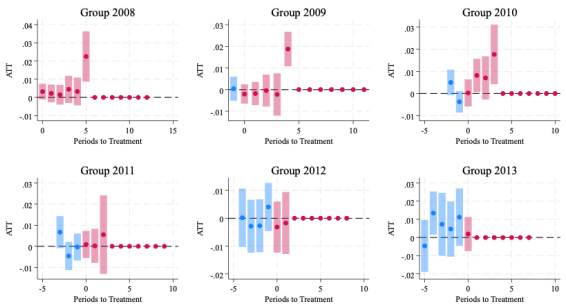
	Teenage Suicide Rate		**Teenage Pregnancy Rate**		**Teenage Homicide Rate**	
	(1)	(2)	(3)	(4)	(5)	(6)
	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Pre-Treatment Average	-0.001	-0.004	0.002	0.002	0.02	0.012
	(0.006)	*(0.006)*	*(0.002)*	*(0.002)*	*(0.042)*	*(0.041)*
Post-Treatment Average	0.009	0.008	0.007**	0.007**	-0.007	-0.008
	(0.021)	*(0.013)*	*(0.002)*	*(0.002)*	*(0.058)*	*(0.056)*
Tm5	0.054	0.054	-0.005	-0.003	0.442**	0.411*
	(0.052)	*(0.052)*	*(0.007)*	*(0.007)*	*(0.162)*	*(0.173)*
Tm4	-0.033	-0.034	0.007	0.008	-0.226	-0.239
	(0.061)	*(0.062)*	*(0.005)*	*(0.005)*	*(0.216)*	*(0.211)*
Tm3	-0.076	-0.064	0.005	0.004	-0.044	-0.099
	(0.041)	*(0.037)*	*(0.003)*	*(0.003)*	*(0.135)*	*(0.159)*
Tm2	0.031	0.024	0.000	-0.001	-0.147	-0.062
	(0.018)	*(0.017)*	*(0.002)*	*(0.002)*	*(0.116)*	*(0.126)*
Tm1	0.018	0.000	0.001	0.002	0.073	0.05
	(0.024)	*(0.027)*	*(0.002)*	*(0.002)*	*(0.085)*	*(0.088)*
Tp0	0.018	0.033	0.001	0.001	-0.009	-0.028
	(0.018)	*(0.020)*	*(0.001)*	*(0.001)*	*(0.066)*	*(0.068)*
Tp1	0.025	0.039	0.002	0.001	0.037	0.026
	(0.028)	*(0.029)*	*(0.002)*	*(0.002)*	*(0.069)*	*(0.072)*
Tp2	0.065**	0.062**	0.003	0.003	0.092	0.09
	(0.022)	*(0.021)*	*(0.003)*	*(0.002)*	*(0.083)*	*(0.083)*
Tp3	-0.007	-0.001	0.006	0.005	-0.059	-0.06
	(0.022)	*(0.017)*	*(0.003)*	*(0.003)*	*(0.088)*	*(0.091)*
Tp4	-0.015	-0.018	0.009**	0.008**	-0.133	-0.134
	(0.028)	*(0.021)*	*(0.003)*	*(0.003)*	*(0.106)*	*(0.103)*
Tp5	-0.035	-0.070*	0.022**	0.022**	0.03	0.056
	(0.065)	*(0.029)*	*(0.007)*	*(0.007)*	*(0.170)*	*(0.150)*
Covariate						
Baseline Eligibility		✔		✔		✔

Notes: Regression output from a Difference-in-Differences (DiD) specification following Callaway and Sant'Anna (2021). Standard errors are clustered at the municipality level. Columns (1)-(6) correspond to individual regression outputs, with some models conditioned on baseline eligibility for full-time school conversion. The outcome variables are adolescent suicide, pregnancy and homicide rates. Adolescent pregnancy rates reflect births in the following year, capturing the average duration of pregnancy. Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01.

(a) Teenage Suicide Rates



(b) Teenage Pregnancy Rates



(c) Teenage Homicide Rates

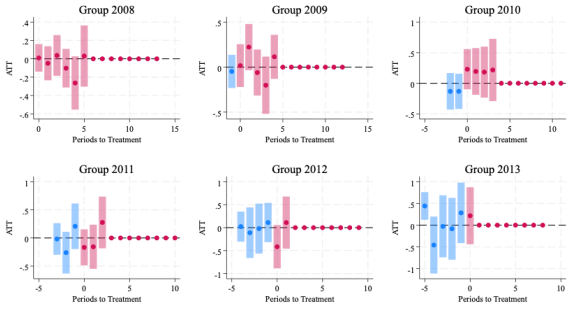


Figure VI: Event Study on socio-behavioural outcomes by full-time schooling conversion cohort. Following the multiperiod Difference-in-Differences framework of Callaway and Sant'Anna (2021), event studies are calculated separately for each conversion group and displayed above. Pre-treatment coefficients and their confidence intervals are shown in blue, while treatment and post-treatment coefficients are shown in pink. Data sources are provided in the Data Appendix.

***Table IV**: Estimates on Adolescent Male Homicide Rates*

	Male Homicide Rates
	(1)
Two-Way Fixed-Effect	b/(SE)
Full Sample	
Share of FT Students (%)	0.006
	(0.190)
Observations	2,415
Rural Municipalities	
Share of FT Students (%)	0.363
	(0.215)
Observations	1,185
Urban Municipalities	
Share of FT Students (%)	-0.334
	(0.302)
Observations	1,230
Multiperiod DiD	
Full Sample	
ATT	0.02
	(0.054)
Rural Municipalities	
ATT	0.043
	(0.067)
Urban Municipalities	
ATT	0.000
	(0.078)

Notes: Regression output of the two-way fixed-effects (TWFE) model and ATTs from a Callaway & Sant'Anna (2021) Difference-in-Differences approach. ATTs are aggregated using a weighted average by group size. The TWFE specification includes municipality and year fixed-effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students in full-time high schools. Each output reports the coefficient followed by the standard error in parentheses. The outcome variable is male adolescent homicide rates. Data sources are provided in the Data Appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

***Table V**: ATT Results for Rural versus Urban Municipalities*

	Teenage Suicide Rate	Teenage Pregnancy Rate	Teenage Homicide Rate
	(1)	(2)	(3)
	b/(SE)	b/(SE)	b/(SE)
Rural Municipalities			
ATT	0.012	0.005	0.023
	(0.031)	*(0.002)*	*(0.067)*
Urban Municipalities			
ATT	0.026	0.005*	-0.025
	(0.016)	*(0.003)*	*(0.079)*

Notes: Average treatment effects on the treated (ATTs) of full-time school conversion on adolescent outcomes, estimated using a Callaway and Sant'Anna (2021) Difference-in-Differences design. ATTs are aggregated as a weighted average by group size. Columns (1)-(3) show adolescent suicide, pregnancy, and homicide rates. Pregnancy rate reflects births the following year, capturing the average pregnancy duration. Results are presented for urban and rural municipalities. Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01

***Table VI:** Regression Outputs for Alternative Fertility Measures*

	Underaged Pregnancy Rate (15-18)	Birth Exposure
	(1)	(2)
	b/(SE)	b/(SE)
TWFE Output		
Share of FT Students (%)	0.013**	79.584***
	(0.004)	*(19.295)*
Observations	2,214	2,490
DiD Output		
ATT	0.005**	-5.797
	(0.002)	(3.980)

Notes: Regression output of the two-way fixed-effects (TWFE) model and Callaway and Sant'Anna (2021) Differences-in-Differences (DiD) approach. TWFE includes municipality and time fixed-effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students in full-time high schools. The ATTs are aggregated using the standard method. Outcome variables are the pregnancy rate of girls aged 15-18 (births the following year, capturing average pregnancy duration) and a birth exposure variable (sum of births to girls aged 15-19 in the current and following year). Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01

***Table VII**: Regression Results for Average Maternal Age*

	Average Age at Birth
	(1)
	b/(SE)
TWFE Output	
Share of FT Students (%)	0.014
	(0.165)
Constant	25.209***
	(0.039)
N	2,490
DiD Output	
ATT	0.097
	(0.084)

Notes: Regression output of the two-way fixed-effects (TWFE) model and Callaway and Sant'Anna (2021) Difference-in-Differences (DiD) approach. TWFE includes municipality and time fixed-effects, with standard errors clustered at the municipality level. The continuous treatment variable is the share of students in full-time high schools. The ATTs are aggregated using the group-size weighted average. The outcome variable is average age at birth. Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01

***Table VIII**: ATT Estimates of Middle School to High School Dropouts*

	Full Sample	**Urban Municipalities**	**Rural Municipalities**
	(1)	(2)	(3)
	b/(SE)	b/(SE)	b/(SE)
ATT	-0.056***	-0.055***	-0.054***
	(0.009)	*(0.013)*	*(0.014)*

Notes: Average treatment effect on the treated (ATT) of full-time school conversion on dropout rates, estimated using a Callaway and Sant'Anna (2021) Difference-in-Differences design. ATTs are aggregated using the group-size weighted average. Columns (1)-(3) report the same outcome variable across different samples. The outcome of interest is the dropout rate between middle and high school. Data sources are provided in the Data Appendix. * p < 0.1, ** p < 0.05, *** p < 0.01