Essays on Education: Evidence from a developing country

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Chapter 1

Introduction

Education is a cornerstone of human development that promotes individual advancement and drives social progress. In several economies, educational attainment remains a critical determinant of economic prosperity, providing pathways out of poverty and enhancing prospects for sustainable growth (Barro, 1991); (Card, 1999); (Glewwe, 2002). Furthermore, education promotes equality by paving the way for equal opportunities and curbing future social unrest (Blattman & Miguel, 2010). However, in contexts marred by armed conflict, such as Colombia, the attainment of quality education becomes a formidable challenge. Armed conflicts disrupt communities, destabilize educational infrastructure, and threaten the safety of students and educators alike. Moreover, the forced recruitment of children into non-state armed groups compounds these challenges, depriving young individuals of their right to education and perpetuating cycles of violence (Akresh, 2008).

The Colombian civil conflict has denied countless children and young people access to education, often turning schools into battlefields and educational infrastructure into collateral damage. The prevalence of armed groups in remote areas exacerbates these problems, leading to widespread displacement and school closures. The abhorrent practice of forced recruitment of children into armed groups not only robs them of their childhood but also undermines their education, perpetuating the cycle of violence and undermining long-term prospects for peace.

Children and adolescents, in particular, bear a disproportionate burden in conflict settings. Exposure to violence during critical developmental stages can disrupt their educational trajectories, potentially leading to long-term implications for personal well-being and economic prospects (Akresh, 2008). Of particular concern is the phenomenon of forced recruitment of children by non-state armed groups. Beyond violating international humanitarian laws, this practice fundamentally alters the life course of affected children, shaping their identities in ways that perpetuate violence rather than fostering peace (Haer et al., 2020).

There are several ways in which forced recruitment can impact educational outcomes. Firstly, the fear of abduction, sexual violence, and recruitment while traveling to and from school may lead parents to prevent their children from attending altogether. Secondly, families residing in areas with high incidences of forced armed recruitment of children often experience heightened stress levels, which can hinder children's ability to learn effectively. Lastly, forced recruitment frequently disrupts classes as children are separated from their families and communities for extended periods. This disruption results in missed school days, falling grades, and eventual school dropout.

In recent decades, empirical research has increasingly highlighted the profound and enduring impacts of armed conflict on educational outcomes, particularly underscoring the vulnerability of children and adolescents. Exposure to violence during crucial developmental years not only disrupts regular schooling but also imposes lasting psychological and developmental scars, significantly affecting individuals' future prospects and societal stability (Haer et al., 2020).

Despite the significant implications, empirical studies specifically investigating the effects of forced recruitment on education in the context of Colombia's civil conflict remain sparse. This gap is partly attributed to data limitations at the municipality level and the complex nature of the conflict itself, which lacks clear religious or ethnic divisions but is intricately linked to historical land disputes and illicit drug production. Multiple armed groups vying for territorial control have further compounded the humanitarian toll, with civilians bearing the brunt of the violence.

In Chapter 1, I aim to contribute to the existing literature on the impact of internal conflicts on educational outcomes by examining detailed combined datasets. These include two datasets aggregated at the municipality level and another comprising cross-sectional micro-level data. The latter consists of household survey data from the Colombian Demographic and Health Surveys (DHS) conducted in 2005, 2010, and 2015. The former two datasets are sourced from the Centro Nacional de Memoria Histórica (CNMH) and the municipal Panel Data constructed by the Centro de Estudios Sobre Desarrollo Económico (CEDE) at the Universidad de Los Andes.

I employ an econometric approach that exploits the variation in forced recruitment cases over time (birth year) and across municipalities in Colombia. To construct a measure of exposure to forced recruitment, I match the information on the number of cases (which varies over time and across municipalities) with data on the year and municipality of residence of each child provided in the DHS dataset. This matching allows me to identify whether a child was exposed to forced recruitment during their school-age years (from 6 to 17 years old).

The analysis uses an extensive margin definition of forced recruitment to capture the incidence of recruitment cases rather than their scale and magnitude, thereby minimizing potential biases arising from under-reporting. With repeated cross-sections, I simultaneously control for year of birth fixed effects and flexible child age polynomials. Additionally, detailed municipality-level aggregate variables enable control for time-varying observed local characteristics.

The results suggest a significant negative association between exposure to forced recruitment during compulsory school age and school attendance in over ten cases. Specifically, there is a 2.7 percentage point reduction in the likelihood of school attendance for 10 to 30 cases, a 2.3 percentage point reduction for 30 to 50 cases, and a 2.8 percentage point reduction for more than 50 cases, compared to children who were not exposed to forced recruitment (with 92% of children attending school).

As an additional outcome, I also investigate the impact of exposure to forced recruitment on grade repetition. The results indicate that children are 3.3 percentage points more likely to repeat a grade if they experienced 1 to 10 cases of forced recruitment (significant at the 1% level). Furthermore, the probability of repeating a grade increase by 4.4 percentage points for 10 to 30 cases, by 4.5 percentage points for 30 to 50 cases, and by 4.4 percentage points for more than 50 cases compared to children who were not exposed to forced recruitment.

To understand the direct mechanism that leads to lower school attendance and higher grade repetition among children exposed to forced recruitment during compulsory schooling, I created a dummy variable that takes the value of 1 if a child was exposed to more than 30 cases during compulsory schooling and 0 otherwise. I then interacted it with children's and mothers' characteristics to examine heterogeneous effects. The results indicate that girls are approximately 3.9 percentage points less likely to attend school and 2.8 percentage points more likely to repeat grades compared to boys. One possible explanation for this is parents' concerns about sending their children to school when living among armed gangs, where children inevitably experience various forms of violence. Girls, in particular, may be exposed to gender-based violence. According to (Springer, 2012), child recruitment is viewed as a form of human trafficking, placing girls at high risk of exploitation, including prostitution, early sexual initiation, high pregnancy rates, and abortions.

On the other hand, the impact of forced recruitment on school attendance and grade repetition varies according to the child's age. Children as young as 9 years old can be recruited or used for criminal purposes, but the majority are adolescents aged between 14 and 17 years. This group shows a 4.8 percentage point lower likelihood of attending school and a 3.7 percentage point higher likelihood of repeating a grade. Additionally, there are heterogeneous effects based on maternal education. Compared to children whose mothers have no education, the impact appears to be more pronounced when the mother has completed higher education, with a negative association with grade repetition and a positive association with school attendance. Maternal education seems to have a protective effect against forced recruitment, as children of more educated mothers appear to be less severely affected by exposure to this form of violence in their municipality of residence.

This study contributes to the growing literature in two main ways. Firstly, forced recruitment erodes investment in human capital in Colombia. Exposure to forced recruitment during school age can lead to long-lasting detrimental effects on school progression and exacerbate gender differentials in educational outcomes. Secondly, these results are relevant in the current context where, despite a steady decrease in grave violations against children in Colombia since the signing of the Peace Agreement between the Government and FARC in 2016, children are still being recruited as "child soldiers" by dissident FARC fighters and other armed groups. Moreover, the effects caused by the COVID-19 pandemic, such as the temporary closure of schools, have increased the vulnerability of children in the most critical areas of the country. This reality highlights that challenges for populations in the most remote areas of the country remain complex, not only due to wide social and economic gaps but also because of deteriorating security conditions, which pose greater institutional challenges in preventing this crime.

In grappling with the complexities of post-conflict reconstruction, Colombia faces numerous educational challenges, including efforts to enhance the quality of schooling and narrow the digital divide. Both are critical for fostering sustainable peace and development. Within this context, Chapter 2, titled "Do Longer School Days Have Enduring Academic Effects in Colombia?", examines the impact of full-day school (FDS) policies on the academic achievement of Colombian students. The study employs an identification strategy that adjusts for the staggered implementation of the program. Using school-level data, I apply the difference-in-difference method (DID) across multiple time periods and variations in treatment timing to estimate the aggregate average treatment effect of longer school days on academic achievement at the initial implementation stage. The analysis focuses on the FDS policy introduced by the Colombian government in 2015, aimed at increasing classroom time to enhance educational quality and learning outcomes. This chapter presents novel findings on the impact of FDS on academic achievement in Colombia, utilizing the difference-in-difference approach proposed by (Callaway & Sant'Anna, 2021) to address three primary questions: First, what is the cumulative average treatment effect of FDS across all schools? Second, do schools exposed to FDS in different years exhibit higher or lower average treatment effects on average? Third, how does the effect of implementing the FDS program vary based on the duration of exposure?

I examine the effects of FDS policies on academic performance using an identification strategy appropriate for this framework that considers the staggered implementation of FDS, which was neglected in previous studies. Second, I also present dynamic average treatment effects to examine how the effect of the FDS varies with the length of exposure to the FDS. Next, I explore heterogeneous effects of school characteristics. In particular, I consider the type of school (private or public), the type of education (college or technical school), and the location (rural or urban). Finally, I conduct further analysis using individual-level data to examine the effects of years of exposure to FDS up to the year of the exam, while controlling for a range of student, family, and school characteristics. This alternative approach also enables analyzing the presence of heterogeneous effects of the FDS policy according to students and family characteristics.

To implement the empirical analysis, I combine two different data sources covering the period 2014-2019. The administrative data from the Colombian Institute for the Evaluation of Education (ICFES), the dataset SABER 11, and the school census (C600), collected annually by the "Departamento Administrativo Nacional de Estadística" (DANE, the national statistics office), which contains information on school infrastructure and available material. The SABER 11 is a statewide standardized test administered annually in all public and private schools in Colombia. It is an assessment mechanism developed by the Colombian Institute for Educational Evaluation (ICFES) and the Colombian Ministry of Education to evaluate the knowledge of 11th grade students in order to improve the provision of educational services and provide educational institutions with relevant information about the competencies of applicants for higher education.

Overall, I find evidence that the FDS improves academic performance. Specifically, the adoption of the FDS enhances test scores by 0.04 standard deviations. When examining results across different subjects, I observe varying effects ranging from 0.02 to 0.04 standard deviations in math, English, social studies, science, and reading. Additionally, I present dynamic average treatment effects to analyze how the impact of FDS exposure changes with the duration of school exposure. Generally, these effects are statistically significant and align with conclusions drawn from average treatment effects. However, the effect appears modest when the program is initially implemented in schools in 2016, but it increases noticeably in magnitude one and two years after the introduction of the FDS program.

Further analysis using individual-level data confirms that the introduction of the FDS has a positive impact on test scores. Specifically, students exposed to the FDS program show improved academic performance in general tests, with more pronounced effects observed in mathematics and language compared to social studies, science, and reading. In mathematics, performance improves with the duration since the introduction of the FDS program. Regarding heterogeneous effects, there is no evidence suggesting that exposure to an FDS benefits male students more than females or younger students more than older ones. However, the effect of the FDS appears stronger for students whose fathers completed primary, secondary, or technical education, and it increases consistently with mothers' education levels. Lastly, the policy's impact seems to be greater for students from middle-income families and notably higher for those from low-income backgrounds.

Chapter 2 contributes to the literature on the benefits of extending the school day for academic achievement in developing countries. I build upon previous work by introducing a staggered implementation methodology to better understand the effects of this intervention. The study's findings are valuable for policymakers, particularly as many countries seek to enhance education quality and increase classroom hours. Furthermore, the evidence presented sheds light on the anticipated outcomes of Colombia's ongoing expansion of FDS policies, thereby adding a significant dimension to the debate on the effectiveness of such interventions.

Lastly, In Chapter 3, I examine the conditional relationship between information and communication technology (ICT) access and student performance in compulsory secondary schools in Colombia. I also explore heterogeneous effects based on the characteristics of students and their families, employing unconditional quantile regression to analyze how ICT usage at home and school varies across the entire distribution of test scores, including high- and low-performing students. Moreover, I integrate data from two distinct sources covering the period from 2015 to 2019 administrative data from the Colombian Institute for the Evaluation of Education (ICFES), specifically the SABER 11 dataset, and the annual school census (C600) conducted by the National Statistics Institute (DANE). The latter dataset provides insights into school infrastructure and facilitates the inclusion of ICT access as an independent variable, encompassing both household and institutional levels.

This chapter contributes to the existing literature by demonstrating a positive conditional relationship between ICT at home and better performance on standardized tests both at home and at school. Conversely, a negative relationship was found when students only had access to ICT at school. This phenomenon has been observed in various global contexts but has not yet been extensively documented in Colombia. It suggests that ICT access alone does not guarantee academic improvement, which emphasizes the importance of combining it with teacher training. Gender differences in ICT use appear to have a significant conditional relationship on academic achievement, with male students often benefiting more from ICT use, particularly in the home environment. In addition, parental education plays a crucial role in students' ICT skills; higher levels of parental education are associated with better guidance and monitoring of children's use of electronic devices, which improves academic performance. In addition, the results of the unconditional quantile regression analysis (UQR) show significant effects on students in the lower percentiles (10th and 25th percentiles), especially in subjects such as math, social science and reading, where access to ICT at home and at school has notable effects.

Chapter 2

Does exposure to forced recruitment affect school progression? Evidence from Colombia

2.1 Introduction

Differences in educational achievement play an important role in explaining the economic well-being of people and nations(Barro, 1991);(Card, 1999);(Glewwe, 2002). Education helps individuals to escape poverty and increases their economic prospects. It is also the most important factor to ensure the equality of opportunities and to avoid future social instability (Blattman & Miguel, 2010). Empirical research on the consequences of armed conflict on education have expanded over the past years, giving both the importance-a large number of developing countries currently deal with armed conflict- and the different ways in which a high level of violence can have long term consequences for civilians. One group that has been vulnerable to conflict exposure are children and youngster. Exposure to violence during this important phase of life may lead to educational problems that can have long-term consequences on individuals' well- being and long-term effects on economic growth (Akresh, 2008). In recent years, there has been growing attention on the phenomenon of children being involved in armed groups, not only because it breaches international humanitarian laws but also because recruiting them for military purposes results in serious consequences for an entire society. Children who have been forced to perform as soldiers have spent their formative years constructing their values and identities under the guidance of armed groups, becoming instruments that perpetuate violence rather than citizens who can build stable peace (Haer et al., 2020).

The relationship between the forced recruitment of children by non-state armed groups and education in Colombia has, to date, been neglected in empirical studies on the effects of armed conflicts. This is partly due to the lack of data on educational indicators such as school attendance and repetition at municipality level but also because the characteristics of the conflict impose many challenges, especially when attempting to quantify its effects. The Colombian civil conflict has several features; for example, there are no religious or ethnic factions defining the conflict. Rather, the conflict is associated with the historical processes of land settlement and cocaine production, which has altered the dynamic of the internal confrontation. This has resulted in the formation of more than one illegal group fighting for territory control, leaving the civilian population the principal victim of violence. To the best of my knowledge, no paper has yet empirically researched this topic. To fill this gap in research, this paper estimates the effect of forced recruitment on school attendance and grade repetition among children of school age in Colombia.

There are numerous channels through which forced recruitment could affect educational outcomes. First, the fear of abduction, sexual violence, and recruitment towards children on their way to and from school may result in parents preventing their children from attending. Second, families living in municipalities with high prevalence of forced armed recruitment cases of children most likely leads to increased levels of stress, which may impact their ability to learn. Finally, it often leads to disruption of classes as children are separated from their families and communities for extended periods of time. This can lead to missed school days, falling behind in classes, and eventually dropping out of school.

The remainder of this paper is organized as follows: Section 2 presents the existing literature on civil conflict and educational investments. The historical background of the Colombian civil conflict is presented in Section 3. Data are described in Section 4, and descriptive statistics are reported in Section 5. The empirical strategy and the results are discussed in Sections 6 and 7, respectively, while Section 8 provides a robustness check. Section 9 presents the final remarks.

2.2 Related literature

Empirical research on the economic consequences of civil war and armed conflicts has expanded over recent years, given both the significance and the incidence of the issue (with many developing countries currently involved in a civil war), and the different ways in which a high level of violence can have long-term consequences for civilians. Children and young individuals are the group most likely to be affected by armed conflicts. Early (or any) exposure to violence during this important phase of life may lead to health and educational problems that can have long-term consequences on individuals' well-being and long-term effects on the economic growth of their country(Akresh, 2008).

Several authors have explored the effect of in-utero exposure to violence on birth outcomes and found that exposure during the first trimester of pregnancy increases the risk of low birth weight and prematurity (Camacho, 2008), (Koppensteiner & Manacorda, 2016), (Duque, 2017).The existing empirical evidence on the effects of violent conflict on children´s schooling is diverse, whereby some studies use macro-level data and find that armed conflict has only a small impact on educational outcomes, such as educational attainment and literacy rates (Miguel & Roland, 2011), (Chen et al., 2008).On the other hand, the literature using micro-level data has found that civil conflict has negative effects on educational outcomes.For instance, (Verwimp & Van Bavel, 2014) estimate the impact of civil war on schooling in Burundi from 1993 to 2000 and find that the probability of completing primary schooling is 9 percentage points lower for children exposed to violence. (Shemyakina, 2011) analyzes the impact of Tajakistan civil war in 1992 on school enrollment of children in the mandatory school age group, between 7 and 15 years old using a difference-in-difference methodology and finds a reduction in all educational outcomes, especially for girls. The results indicate that children aged between 7-15 years were about 11 percentage points significantly less likely to be enrolled in school if their household dwelling was damaged during the war in Burundi (which is their measure of violence). Along the same lines, (Chamarbagwala & Morán, 2011) studied how Guatemala´s 36-year-long civil war between 1960 and 1996 affected the human capital accumulation of individuals exposed to civil war and determined the demographic groups that were most affected. Their results show that rural Mayan males and females completed 1.09 and 1.17 years less of schooling, respectively, with a lower likelihood of completing primary school. In line with these papers, (Koppensteiner & Menezes, 2021) , (Brück et al., 2019) , (Caudillo & Torche, 2014), (Dabalen & Paul, 2012), (Akresh, 2008) estimate the causal effects of civil war on educational performance and human capital investment for school-age children exposed to armed conflict and find that violence diminishes the level of educational achievement, reduces exam performance, and increases the risk of grade repetition.

While there is a vast literature in Colombia about the effects of violence on educational achievement, to the best of my knowledge no paper has yet empirically analyzed the relationship between forced recruitment and education. For instance, (Barrera Osorio & Ibáñez Londoño, 2004) study the relationship between violence and educational investments and conclude that violence in Colombia appears to erode investments in human capital. School enrollments in violent municipalities are lower, and the likelihood of school enrollment for children between 7-11 years, 12-17 years, and young adults between 18-22 years decreases as homicide rates increase due to a reduction in family income, a reduction in household income and changes in the rates of return of education. In the same vein, (Rodriguez & Sanchez, 2012) estimate the effect that exposure to armed conflict has on school drop-out rates and labor decisions of Colombian children between the ages of 6-16 years. The authors suggest that violence influences the schooling decisions of children aged 12 years and older, with an emerging trade-off between engaging in child labor and continuing their education. Interestingly, they find that the effect of violence varies predominantly with age rather than household wealth or gender.

Finally, (Fergusson et al., 2020) examine the long-term impact of violence on educational attainment by exploiting within-country variations on the incidence of violence using data from Colombia´s political violence of the mid-20th Century, known as *La Violencia* [1]. Children exposed to political violence, especially younger children, have lower levels of educational attainment (0.2 to 0.3 years of education). Younger cohort, those who were 45 to 49 in 1973(20 to 25 in 1948) have, on average, almost two full years less than the youngest generation born about 25 years later.

On the other hand, the economic and social cost of the internal confrontation in Colombia has been significant. According to (Parra, 1998), a 10 percent reduction in the crime rate would have an impact of 1.2 percent per year on the growth rate, and, if the economic cost of violence were invested in any other activity that produces a valued good or services, the GDP would grow by an additional 1.7 percent. (Querubín, 2004) finds that a 10 percentage points increase in the homicide rate reduced the total GDP per capita by 0.37 percent. According to Norwegian Refugee Council today, Colombia is home to the world´s third largest internally displaced population, surpassed only by DR Congo and Syria. Nearly 5 million Colombians remain displaced, according to Internal Displacement Monitoring Centre (IDC). In the first nine months of 2021, 188 monthly attacks against civilians were registered by United Nations Office for the Coordination of Humanitarian Affairs (OCHA).

2.3 A Brief Historical Background of the Internal Conflict in Colombia

Colombia has been characterized by internal conflict for more than 50 years. Even though the country is considered a democratic society, our history has been defined by violations of civil freedoms, extrajudicial executions, violence against civilians, and civil war. The period of La Violencia was a 10-year civil war in Colombia from 1948 to 1958 between Liberal and Conservative parties. In 1958, the parties signed a common agreement to alternate the presidency every four years, however this agreement did not offer any solution to the problems experienced by the rural sector in Colombia that lacked attention from the government and had minimal participation in governmental decisions.

The absence of government attention, the lack of authority over the territory, and the disgruntled landowners forced the peasants to arm themselves in rural areas. Colombian authorities provided protection and public service in the large urban areas, such as Bogota, Barranquilla, and Medellin, however there remained significant rural areas where public services, law and order did not exist. Instead, illegal groups controlled the political and natural resources (Acemoglu, 2015).

[1]This was a period in Colombia where bipartisan violence intensified after the assassination of the liberal presidential candidate, Jorge Eliécer Gaitán in 1948

In the 1960s, the Revolutionary Armed Forces of Colombia (Fuerzas Armadas Revolucionarias de Colombia, FARC) and the National Liberation Army (Ejercito de Liberacion Nacional, ELN) were formed. During the coca boom of the 1980s, these groups worked together with drug traders, however they soon separated, and the traders went on to create their own self-protection system, the paramilitaries, which generated a new wave of violence in Colombia. The ELN and the paramilitary groups started to fight against the FARC guerilla fighters for coca land cultivation and territory in areas with a lack of state involvement. One of the paramilitary objectives was to perform “social cleansing” [2]; in fact, between 1989 and 1993, there are 1926 documented cases of these types of killing (Leech, 2009). Despite the efforts of the Colombian Government towards a unilateral ceasefire agreement and to end crimes against civilians and drug trafficking, the violence continued. Between 1996 and 2002, the armed conflict reached its most critical level as a consequence of the military strengthening of the guerrillas, the national expansion of paramilitary groups, and the reconfiguration of drug trafficking and its rearrangement within the structure of the armed conflict. However, since 2003, it has been marked by a declining phase because of the State´s military presence, the withdrawal of the guerrillas, and the partial demobilization of paramilitary groups.

In 2010, under the presidency of Juan Manuel Santos, the government started new peace negotiations with the FARC and in 2016 they signed a peace agreement with the following topics as core elements: rural reform, agreements on the bilateral and definitive ceasefire, the cessation of hostilities and the laying down of arms, the reincorporation of the FARC into civilian life, a solution to the problem of illicit drugs, and an agreement regarding the victims of the conflict. The mandatory presidential term of Juan Manual Santos ended, with the implementation and compliance of the agreement the key challenge for the future.

The Peace agreement ended a five-decade-long civil war; but the country is far from being pacified by it. Conflict and violence continue, fueled by confrontation among a left-wing guerrilla Ejército de Liberación (ELN), not part of the Peace agreement, FARC´s dissident groups and former paramilitary right-wings groups turned into drug cartels. In the last years, escalating violence, including forced recruitment, massacres, and murder of social activists, has increased the number of internally displaced people. This violence is also a direct consequence of the government not moving fast enough to guarantee the permanent presence of Colombian institutions in hard-to-reach zones after the peace agreement.

[2] A process of forcing groups of people who are regarded as not desirable to leave an area, in some cases killing them

2.4 Child Soldiers in Colombia

The recruitment of children and adolescents by non-state armed groups is one of the most serious war crimes affecting both minors and families. Engaging children under the age of 15 to be soldiers is prohibited by international humanitarian laws and is defined as a war crime [3] by the International Criminal Court (ICC). However, the recruitment of children has become an important issue for the opposition armed groups, as their survival in a war depends on their involvement.

Among other things, children are used to replace illegal group members who have died or been killed in combat. Indeed, it has been estimated that nearly 300,000 children under the age of 18 are actively involved in armed conflict around the world (Hamilton & Dutordoir, 2009). Since 1999, the direct involvement of children in a violent conflict has been considered one of the worst forms of child labor, because children lose the "essence of childhood" and the opportunity to enjoy a better future as their schooling process is interrupted and the direction of their lives is dramatically altered.

Colombia has a long history of children being forcibly engaged in armed groups, according to the Centro Nacional de Memoria Historica (CNMH)[4]: 16,879 children have been recruited by different armed groups between 1960 and 2016. Among these, 71% were male and 30% female, who also suffer sexual exploitation, mostly from the age of 11 years. Children are used for domestic chores such as cooking, and other roles such as spying, logistic and intelligence work, the recruitment of other children, guarding, and for fighting, the construction and installation of mines, including the use of explosives, for manual work such as coca cultivation and in illegal mining (Villanueva O'Driscoll et al., 2013) . According to the Ombudsman´s Office, during 2014-2016, 87.5% of Colombia departments [5] have been informed of the forced recruitment and use of children and adolescents in violent conflict.

2.5 Data

The empirical analysis presented in this paper is carried out with combined datasets; two of which are aggregated at the municipality level, with the other a cross-section of micro-level data. The latter is household survey data from the Colombian Demographic and Health Surveys (DHS), waves 2005, 2010, and 2015. The former two are from the *Centro Nacional de Memoria Historica* (CNMH), and the municipal Panel Data constructed by the *Centro de Estudios Sobre Desarrollo Económico* (CEDE) at the Universidad de Los Andes.

[3] War crimes include torture, mutilation, corporal punishment, hostage taking and acts of terrorism. This category also covers violation of human dignity such as rape and forced recruitment and prostitution

[4] Una Guerra sin edad. Informe Nacional de Reclutamiento y Utilización de niños, niñas y adolescentes en el conflicto armado colombiano

[5] Currently according to 1991 Political Constitution of Colombia, the country has 32 departments, 1132 municipalities and 5 regions (Andean, Caribbean, Pacific, Orinoquía and Amazonia)

The Demographic and Health Surveys are nationally representative cross-section surveys carried out in developing countries, which collects primary information on women of reproductive age between 13-49 years and their households. This includes several questions about each child living in the household, as well as other characteristics, including gender, age, educational level, current school attendance, and grade repetition status. Moreover, it contains information about the mothers (education, work status, number of children born into the household, marital status, among others) and household characteristics. One limitation of the data is the missing information about the child´s father, so all results in this paper need to be interpreted with this data restriction in mind. Finally, the DHS also includes information about the municipality of residence of children and their families at the time of the survey, which is necessary to ascribe exposure to cases of forced recruitment [6]

The CNMH violence data provides a detailed description of violent events for 1,071 municipalities in Colombia, including cases of forced recruitment, terrorist attacks, firebombs, attacks to the population, clashes, enforced disappearance, massacres, kidnapping, anti-personal landmines, and sexual violence. The database also reports the corresponding date of occurrence, the responsible groups, the municipality in which the violent events occurred, and the number of victims involved in each event. In this paper, I focus on forced recruitment as a violent event. The recruitment of child soldiers into non-state violent armed groups has been divided into two types: the children who are forced into armed groups and the children who join voluntarily. The former, according to the United Nations, is defined as: *"any person under 18 years of age, who is part under coercion of any kind of regular or irregular armed force or armed group in any capacity, including but not limited to cooks, porters, messengers and anyone accompanying such groups, other than family members"*. The latter, is motivated by a range of factors, including domestic violence, poverty, fear, family conflicts, coveting local power and visibility (Brett & Specht, 2004), (Gutiérrez Sanín, 2008).

However, a fully informed choice is rarely an option in any real situation. Children therefore make a range of choices based on the limited information available to them before they are recruited. But this does not indicate that they have the capacity to decide or know the consequences of joining a non-state armed group and the difficulties of reintegrating into society (Bjørkhaug, 2010). The regressor of interest uses a categorical variable that has a value of 1 if a child was not exposed to any reported cases of forced recruitment during compulsory school age (starting from the year during which the child was 6 years old until the year of the survey) according to the municipality of residence, a value of 2 if a child was exposed to one to ten cases, a value of 3 if a child was exposed to ten to thirty cases, a value of 4 if a child was exposed to thirty to fifty cases, and a value of 5 if a child was exposed to more than fifty cases.

[6] Notice, however, that not all Colombian municipalities are covered in the DHS. Indeed, in the 2005 wave there are 232 municipalities out of 1,132; in 2010, 188; and 205 in 2015

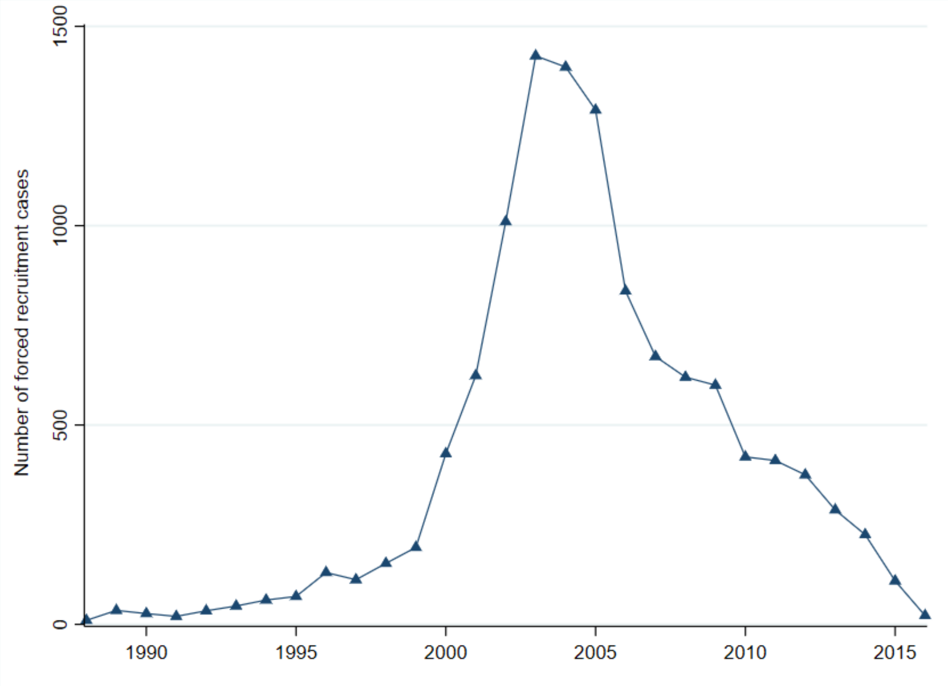
This variable, therefore, captures the exposure to forced recruitment at the municipality level during school age, exploiting variation by birth years and across municipalities.These databases are complemented with a large set of municipality-level characteristics from the annual panel of the CEDE that includes socio-economic and geographical information of all municipalities in Colombia. This information enables me to control for potential time-varying confounders at the municipality level. To control for the general level of economic activity, I use local tax revenue and the total transfer received on education from the central government. To capture other types of violence (other than forced recruitment), I use violent attacks, (the total number of attacks against infrastructure and civilian population), massacres, and deaths rate per 100,000 population undertaken by guerrillas and paramilitary groups in each municipality in each year. Finally, to proxy the municipality's quality of education, I use a measure of the budget execution in education. The main statistics for all these variables are presented in Table 3.1

The data are stacked at the child level, linking children and mothers'/household information using the household identifier, considering data from the Colombian Demographic and Health Survey (DHS) 2005, 2010, and 2015 waves, and only children of school-age (6-17 years) are retained for the empirical analysis. Forced recruitment data by municipality and year of occurrence are assigned to each wave based on the child´s year of birth and the municipality of residence. The variable of interest (exposure to forced recruitment) is equal to the total value of recruitment cases to which a child was exposed during compulsory school age (6 to 17 years).

2.6 Descriptive Statistics

For more than 50 years Colombia has faced an internal armed conflict. Forced recruitment has been a common practice employed by armed groups in this conflict. Figure 2.1 illustrates the variation in forced recruitment cases in Colombia since 1988. As this figure shows, the occurrence of these cases fluctuated between 10 to 193 during the 1990s, until it peaked in 2002, when more than 1,000 forced recruitment cases occurred. Since 2006 the number of cases declined, reaching the lowest level in 2016 due to the demobilization of paramilitary groups and the Peace Agreement. However, despite this steady decrease in forced recruitment cases, children are still being recruited as "child soldiers" by dissident FARC fighters and other armed groups, not part of the Peace Agreement. In addition, the effects caused by the COVID-19 pandemic, such as the temporary closure of schools, has increased the vulnerability of children in the most critical areas of the country.

Figure 2.1: Forced recruitment cases in Colombia 1989-2016



Source: CNMH. Forced recruitment cases in Colombia by the year of occurrence (1989-2016)

In terms of spatial distribution, Figures 2.2, 2.3 and 2.4 show that forced recruitment cases are concentrated in certain areas of the country (Central and Eastern regions). The most vulnerable regions are Antioquia, Nariño, Chocó, Meta, Bolívar, Tolima, Cauca, Valle del Cauca, Casanare, and Putumayo. The municipalities in these regions are strategic corridors and areas of natural resource exploitation. Illegal mining is the aim of the armed groups fighting for control of a territory and power, according to the National Ombudsman Office (*Defensoría del Pueblo*), with illegal gold mining the main source of income for armed groups. In addition, the Naya and Guapí rivers are the corridors through which these groups move cocaine out to sea and onto the Mexican cartels, whose presence has also been detected in the Cauca territory.

Figures 2.2, 2.3 and 2.4 also exhibit the geographic distribution of the occurrence of forced recruitment in municipalities covered by DHS across the three waves (light brown indicates reported cases). In the 625 municipalities included in the DHS (across the three survey rounds), 312 cases of forced recruitment were reported (50% of the municipalities in the sample), (162) in 2005, (90) in 2010, and (60) in 2015. The figures show that most cases occurred are in the mid-2000s and decreased toward the end of the decade, coinciding with the intensity of conflict in the country.

Table 2.1 reports descriptive characteristics for children, their mothers, household, and municipalities for the whole sample and by exposure to forced recruitment. Results show some differences across groups; for instance, children who do not grow up with forced recruitment have more brothers and sisters and tend to be younger, who are by definition, younger children who have had lower exposure to recruitment cases than older children. Hence, as a robustness check I include different polynomial specifications to show that results are not driven by this mechanical correlation. The mothers of children who are exposed to forced recruitment during the ages of compulsory schooling tend to be slightly more educated and are more likely to be single, divorced or widowed compared to other mothers. I also find that children living in municipalities with recruitment cases have less school attendance and a higher percentage of grade repetition. Households in municipalities with forced recruitment cases tend to have better economic conditions and fiscal practices, and more violent events.

Figure 2.2: Spatial distribution of forced recruitment cases by municipalities in 2005

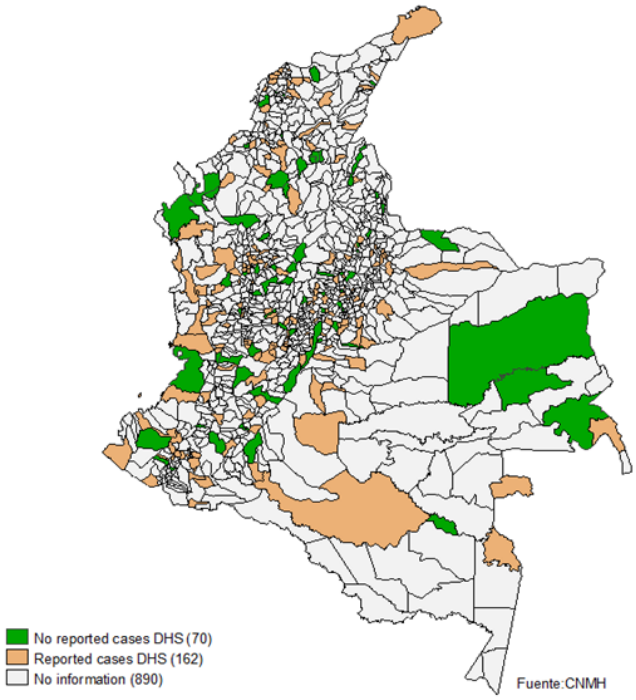


Figure 2.3: Spatial distribution of forced recruitment cases by municipalities in 2010

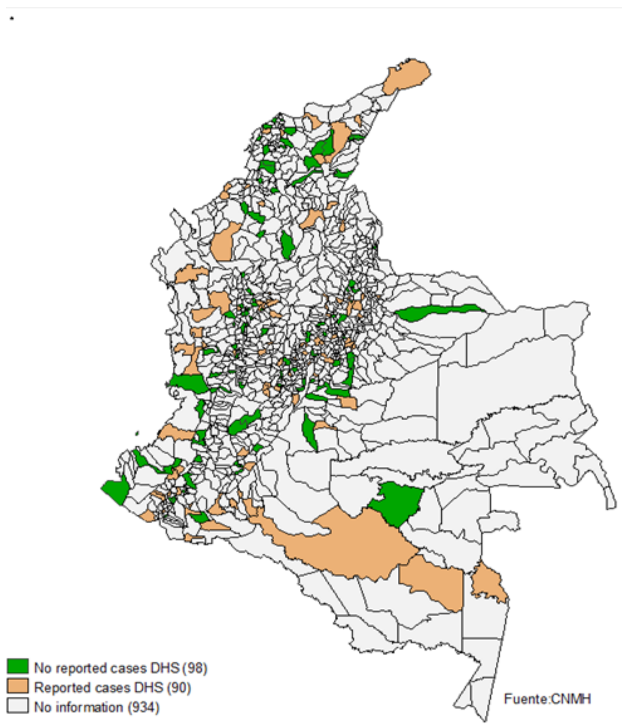


Figure 2.4: Spatial distribution of forced recruitment cases by municipalities in 2015

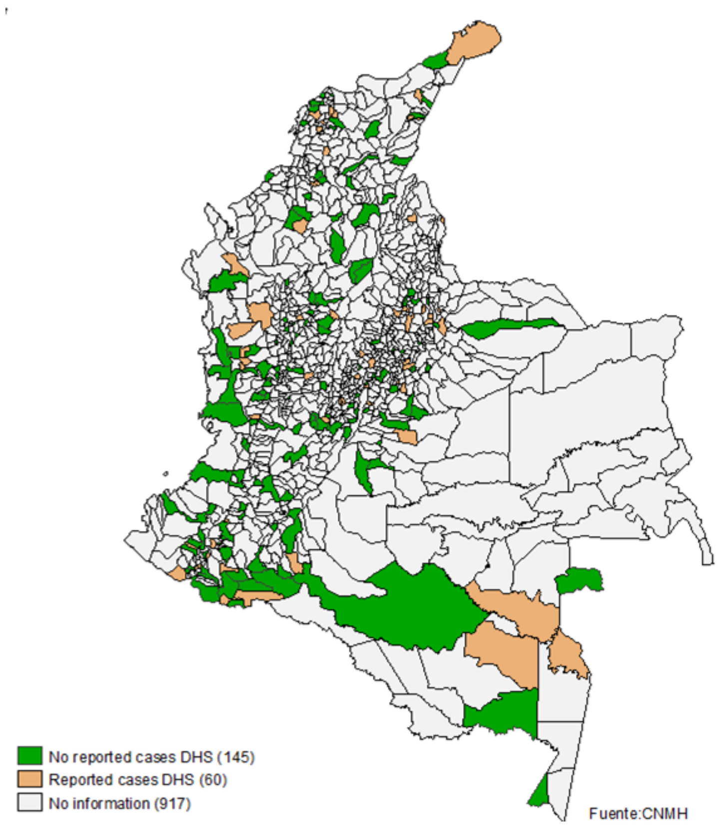


Table 2.1: Descriptive statistics by forced recruitment exposure

	Full sample	Municipalities with forced recruitment	Municipalities without forced recruitment	t-test	p value
Child outcome					
Attending school (%)	92,0	92,1	94,2	11,6	0,00
Grade repetition (%)	46,4	49,9	40,3	-26,5	0,00
Child characteristics					
Child sex					
Female(%)	49,0	49,19	48,77	-1,2	0,00
Male(%)	51,0	50,81	51,23	-1,3	0,00
Age	11,0	11,56	9,92	-74,2	0,00
Birth order	2,5	2,49	2,54	4,4	0,00
Number of siblings	3,6	3,61	3,53	-5,8	0,00
Mother characteristics					
Age	36,2	36,7	34,4	-27,8	0,00
Years of education	7,3	7,4	7,3	-2,2	0,00
Married/in union(%)	76,5	75,0	79,2	14,1	0,00
Working(%)	60,5	62,0	57,7	0,0	0,00
Household characteristics					
Home has electricity	94,3	97,7	93,6	-7,0	0,00
Home has television	87,3	87,9	86,2	-7,0	0,00
Home has refrigerator	69,7	71,2	66,9	-13,4	0,00
Family has a car	8,5	8,8	7,9	-4,4	0,00
Municipality characteristics					
Ln(Tax income)	20,4	21,0	19,4	-81,4	0,00
Ln(Education expenditure)	20,5	21,0	19,5	-83,8	0,00
Ln(Goverment transfer to education)	20,7	21,3	19,8	-83,8	0,00
Number of offensive attacks to population	69,2	88,8	32,3	-60,3	0,00
Forced recruitment cases(%)	14,1	22,8	0,0		
Number of observations	88,729	56,575	32,154		

Sources: DHS, author calculations

2.7 Empirical Strategy

The empirical analysis focuses on differences in school attendance and grade repetition of children living in each municipality who have been exposed (not exposed) to forced recruitment cases during school age. In doing so, I want to partial out the effect of different children, mother, and household characteristics (that are especially important to the proxy partner´s unavailable characteristics), as well as municipality controls to rule the effect of municipality-level confounders. The analysis is based on an extensive margin definition of forced recruitment in order to capture the incidence of recruitment cases rather than their scale and magnitude, but also to minimize possible biases derived from the potential under-reporting of cases. One of the major difficulties in assessing the effects of exposure to forced recruitment on children´s education is the existence of unobserved heterogeneity and endogenous residential sorting.

Indeed, municipalities more likely to be exposed to forced recruitment and other types of violence contrast with less violent municipalities with regard to factors that may also affect educational outcomes. Moreover, endogenous residential sorting across municipalities due to violence may represent a need to identify the effect of forced recruitment on school attendance and grade repetition. The identification strategy used in this paper relies on exploiting the variation in forced recruitment cases over time (birth year) and across municipalities in Colombia. To construct a measure of exposure to forced recruitment, I match the information on the number of cases (which varies over time and across municipalities) to information on the year and municipality of residence of each child provided in the DHS dataset. Matching this data allows me to identify whether a child was exposed to forced recruitment during their school-age; that is, from 6 to 17 years. Given the use of repeated cross-sections, it is possible to simultaneously control for the year of birth fixed effects and the flexible child's age polynomials. Moreover, detailed information about aggregate variables at the municipality level also allows me to control for time-varying (observed) local characteristics. Since I only have information on the years in which the child was supposed to attend school, I must assume that the municipality of residence[7] is the municipality in which the child attends school at the time of exposure to forced recruitment.

The main equation to be estimated takes the following form:

$$\mathrm{Y}_{imtj} = \alpha + \beta_1 \mathrm{X}^{c}_{i\,mt} + \beta_2 X^{p}_{i\,mt} + \beta_3 FR_{mt} + \beta_4 Z^{p}_{mt} + \beta_5 Z^{c}_{mt} + \gamma_m + \delta_t + \pi_j + \epsilon_{imtj} \tag{2.1}$$

[7]Of the total sample of children exposed to forced recruitment more than 50% of mothers stated that they have not changed their residence. Additionally moving to another municipality creates new challenges in terms of access to education. In Colombia families are not allowed to freely choose public schools according to their preferences. It is the Secretary of Education at the municipality level that make the decision based on school demand and supply spots. If no spaces are available, families must wait for their children to return to school

Here Y_{imt} is the educational outcome of interest (binary variable for attending school or course repetition) for individual "i" born in the municipality "m" in year "t". $X^c_{i\,mt}$ is a vector of child characteristics such as age, sex, birth order of the child and the number of siblings. $X^p_{i\,mt}$ is a vector with the following standard mother characteristics: age, marital status, working status, year of education, and level of earning from their employment.The main regressor is FR_{mt}, which is a dummy variable that has a value of 1 if a child was not exposed to any reported cases of forced recruitment during compulsory school age according to the municipality of residence, a value of 2 if a child was exposed to one to ten cases, a value of 3 if a child was exposed to ten to thirty cases, a value of 4 if a child was exposed to thirty to fifty cases, and a value of 5 if a child was exposed to more than fifty cases.

β_3 is the parameter of interest, indicating whether having been exposed to cases of forced recruitment from age 6 until 17 affects the probability of attending school or having repeated a grade, compared to children who were not exposed to forced recruitment during their school age. Z^p_{mt} are a set of household characteristics, such as the presence of a television, a car, and a fridge, as proxies of family income due to lack of information about the child´s father. Z^c_{mt} are local municipalities variables to control for potential time-varying confounders at the municipality level. The controls are tax revenue, educational central government transfer, budget execution in education, and violent attacks on a population, and are defined as the average values regarding when the child was 6 years old up to the present age. Because exposure to forced recruitment varies significantly across municipalities and the child's year of birth, I include year of birth fixed effects δ_t and municipality fixed effect γ_m to control for any time invariant municipality characteristics and shocks common to all children born in the same year. This is required because there are several children in a family for each available DHS surveys year. All regressions are estimated using Linear Probability Model (LPM)[8] with two-way clustered standard errors by municipality and year of birth, which is the level of variation of the variable capturing the exposure to cases of forced recruitment (Cameron & Miller, 2015).

However, the OLS estimation of equation 2.1 can be affected by some issues that may invalidate the interpretation of the results. First, it is possible that the measure of forced recruitment cases is capturing other unobserved factors that correlate with the outcome(s). The inclusion of municipality fixed effects in model 2.1 allows me to account for time-invariant differences in educational achievements across municipalities. Moreover, the availability of time-varying controls at the municipality level (including an alternative measure of violence) should contribute to mitigating the presence of unobserved heterogeneity. Second, due to many internally displaced people, it is often hard to follow their movements during the analyzing period in response to forced recruitment.

[8]The LPM is a regression model where the outcome variable is a binary, and one or more explanatory variables are used to predict the outcome. The Cumulative Distribution Function of the error term is a linear function, and it is possible to apply all the tools for statistical inference for the Ordinary Least Square (OLS)

This could be a problem if the characteristics of displaced children are significantly different from non-movers or, for instance, if the negative effect of forced recruitment in educational outcomes is due to the decision of movers to relocate away from schools in municipalities with a high number of forced recruitment cases. To provide suggestive evidence that endogenous residential sorting issues are not the main drivers of the results, I re-estimate the model after excluding families who were living in different municipalities, as well as after restricting the sample to municipalities with a low displacement expulsion rate. Finally, I also check the results obtained after removing from the main econometric specification the municipalities that have never had forced recruitment cases during the school age.

2.8 Results: the effect of forced recruitment on school attendance and grade repetition

In the first estimation of the model (1), I use as a dependent variable a dummy that takes value 1 if children between 6-17 years attend school and 0 otherwise, with the results reported in Table 2.2. All specifications include the child's year of birth, year of the DHS interview, and the municipalities' fixed effects [9] . Model 1 shows the result of a specification that includes only a parsimonious set of children's characteristics, namely sex and age, and age squared. The estimated coefficient for forced recruitment captures the difference in percentage points in the probability of attending school for children exposed to forced recruitment in their municipality of residence. The results show that in more than ten cases there is a negative and significant association between exposure to forced recruitment during compulsory school age and school attendance. The effect suggests a 2.7 percentage point reduction in the likelihood of school attendance for 10 to 30 cases, a 2.3 percentage points reduction for 30 to 50 cases, and a 2.8 percentage point reduction for more than 50 cases, compared to children who were not exposed to forced recruitment (with 92 % of children attending school). It is also important to comment on the estimates of other control variables as well, which is of independent interest. For instance, being male reduces the probability of attending school by around 2 percentage points. This result can be explained, among other things, by the historical link between gender and violence. Involvement in hostilities is one of the main functions of life in an armed group and given the specific conditions of these activities' characteristics, boys are more representative than girls. These attributes have been considered as an important reason that children who live in municipalities with forced recruitment cases drop out of school.

The relationship between the child's age and school attendance has an inverted U-shape indicating that the probability of school attendance increases with age until a certain point but then decreases. The age at which children have the highest percentage of school attendance is around 10 years old.

[9] I have tried to control for municipality population in a model without fixed effects, as there is not enough temporal variability in population within each municipality

The findings displayed in model 2 include the number of siblings and birth order as additional controls. The results indicate that child´s birth order has a negative and significant relationship with school attendance; children born later are less likely to attend school by 1.8 percentage points than the firstborn. Moreover, as expected, having more siblings is negatively associated with school attendance. Model 3 displays the results obtained controlling for the mother's characteristics, such as education, age, marital status, working status, and type of employment. The education of the mother is strongly significant in understanding school attendance. Children with educated mothers are more likely to attend school even in war environments. Conversely, when mothers do not have a formal education, this factor intensifies the vulnerability of children to be recruited or exploited by armed groups. Furthermore, the age and the mother's level of earnings from her employment are not significant to explain the probability of attending school. For model 4 and 5, I add a set of household and municipality variables to control for household living conditions, since I do not have information about the children´s fathers and for time-varying municipality conditions. Despite the included control variables, the magnitude and significance of the effect of forced recruitment on the probability of attending school remains significant and negative, and very similar in magnitude to the results of model 1.

As an additional outcome, I also investigate the impact of exposure to forced recruitment on grade repetition. That is, I estimate equation 2.1 using as a dependent variable a dummy that takes a value of 1 if the grade of school attendance does not correspond to the standard grade for the child's age and 0 otherwise. All of the specifications include fixed effects for the year of the child's birth, municipalities, and the year of the interview, and they follow the same step-wise inclusion of control variables as for school attendance. The results in Table 2.3 show that there is a positive and significant relationship between exposure to forced recruitment during compulsory school age and grade repetition. The effect indicates that children are 3.3 percentage points more likely to repeat a grade if they experienced 1 to 10 cases of forced recruitment (significant at the 1% level). In addition, the probability of repeating increases by 4.4 percentage points for 10 to 30 cases, by 4.5 percentage points for 30 to 50 cases, and by 4.4 percentage points for more than 50 cases compared with children who were not exposed to forced recruitment.

Child attributes, using male as a dummy variable increases the probability of grade repetition by around 6 percentage points. Younger siblings are more likely to repeat a grade. Having more siblings is strongly and positively associated with grade repetition, by increasing the probability of repeating grade by 2 percentage points. Having an educated mother reduces the probability of repeating a grade by 1.6 percentage points. Coefficients on the mother´s working status and level of earnings are negative but not statistically significant in any of the estimations.

At the household level, children living in a residence with electricity, television, and a refrigerator have a lower probability of repeating a grade which reflect how social and economic conditions determine educational achievements in developing countries. Finally, when I add municipality controls (model 5), the magnitude and the significance of the variable of interest remains significant and stable, which is the same as for school attendance, indicating the overall robustness of the results.

To understand the direct mechanism that leads to lower school attendance and higher-grade repetition among children exposed to forced recruitment during compulsory schooling, I generate a dummy variable that has a value of 1 if a child was exposed to more than 30 cases during compulsory schooling and zero otherwise and interact it with the children´s and mother´s characteristics to check for the existence of heterogeneous effects. The results in Tables 2.4 and 2.5 show that girls are around 3.9 percentage points less likely to attend school and 2.8 percentage points more likely to repeat grades relative to boys. One possible explanation for this is the parents 'concerns about sending their children to school. When living among armed gangs, it is inevitable that children will experience all forms of violence. Girls in particular may be exposed to gender-based violence. According to (Springer, 2012), child recruitment is seen as a form of human trafficking where girls are at high risk of prostitution. They are exposed to early sexual initiation, high pregnancy rates, and abortions.

On the other hand, the impact of forced recruitment on school attendance and grade repetition varies according to the child´s age. Children as young as 9 years old can be recruited or used for criminal purposes, but most of them are adolescents between 14 and 17 years. This group has 4.8 percentage points less likelihood of attending school and a 3.7 percentage points greater likelihood of repeating a grade. Moreover, the impact of forced recruitment increases, in absolute value, for children with more siblings and according to their birth order for both outcomes and are statistically significant at the 1% level of confidence. Children and adolescents being aware of violence or catastrophic events experienced by their brother or sister are vulnerable to increase rate of post-traumatic stress disorder (PTSD), depression, anxiety and externalizing behaviors that can make it difficult for children to learn (Newnham et al., 2015).

Finally, concerning the heterogeneous effects by maternal education, relative to children whose mothers have no education, the effect appears to be stronger when the mother has completed higher education and it is negatively associated with grade repetition and positive associated with school attendance. Maternal education seems to produce a protective effect against forced recruitment, since children of more educated mothers appear to be less severely affected by the exposure to this form of violence in their municipality of residence.

Table 2.2: The effect of forced recruitment on school attendance

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
0 recruitment cases			(reference category)		
1 to 10 recruitment cases	-0.007	-0.007	-0.007	-0.007	-0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
11 to 30 recruitment cases	-0.021**	-0.024**	-0.026**	-0.027**	-0.027**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
31 to 50 recruitment cases	-0.020**	-0.021**	-0.023**	-0.024**	-0.023**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
More than 51 recruitment cases	-0.025**	-0.023**	-0.027**	-0.027**	-0.028**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Male	-0.021**	-0.021**	-0.021**	-0.021**	-0.021**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Child age	0.099**	0.101**	0.099**	0.098**	0.090**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Child age square	-0.005**	-0.005**	-0.005**	-0.005**	-0.005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Child birth order		-0.018**	-0.018**	-0.018**	-0.018**
		(0.001)	(0.001)	(0.001)	(0.001)
Number of sibling		-0.020**	-0.014**	-0.011**	-0.011**
		(0.001)	(0.001)	(0.001)	(0.001)
Mother years of education			0.036**	0.036**	0.036**
			(0.000)	(0.000)	(0.000)
Mother age			0.002	0.002	0.001
			(0.002)	(0.002)	(0.001)
Mother age square			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Married			0.018**	0.015**	0.014**
			(0.003)	(0.003)	(0.003)
Working mothers			0.017**	0.017**	0.017**
			(0.002)	(0.002)	(0.003)
Salaried mothers			0.002	0.003	0.003
			(0.002)	(0.002)	(0.002)
Home has electricity				0.031**	0.030**
				(0.010)	(0.011)
Home has television				0.029**	0.030**
				(0.005)	(0.005)
Home has refrigerator				0.022**	0.022**
				(0.003)	(0.003)
Family has a car				0.001	0.000
				(0.003)	(0.003)
Log(Tax income)					-0.014
					(0.014)
Log(Educational transfers)					0.022
					(0.004)
Log GPS					0.021
					(0.022)
Offensive attacks rate					0.000
					(0.000)
Constant	0.243**	0.277**	0.162*	0.106	0.108
	(0.073)	(0.073)	(0.076)	(0.077)	(0.436)
Observations	91,320	91,320	91,320	91,320	85,770
Adjusted R-squared	0.127	0.137	0.144	0.147	0.123

Two-way clusters standard errors in parentheses. All specifications include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10% .

Table 2.3: The effect of forced recruitment on grade repetition

	Model 1	Model 2	Model 3	Model 4	Model 5
0 recruitment cases	(reference category)				
1 to 10 recruitment cases	0.033*	0.033*	0.033*	0.033*	0.033*
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
11 to 30 recruitment cases	0.044**	0.046**	0.042**	0.042**	0.042**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
31 to 50 recruitment cases	0.051**	0.051**	0.055**	0.056**	0.045**
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
More than 51 recruitment cases	0.059**	0.052**	0.055**	0.054**	0.044**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Male	0.052**	0.052**	0.053**	0.053**	0.056**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Child age	0.399**	0.391**	0.397**	0.398**	0.384**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
Child age square	-0.010**	-0.010**	-0.010**	-0.010**	-0.009**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Child birth order		0.012**	0.024	0.025*	0.024*
		(0.002)	(0.002)	(0.002)	(0.002)
Number of sibling		0.042**	0.022**	0.019**	0.020**
		(0.002)	(0.002)	(0.002)	(0.002)
Mother years of education			-0.017**	-0.015**	-0.016**
			(0.001)	(0.001)	(0.001)
Mother age			-0.011**	-0.010**	-0.010**
			(0.003)	(0.003)	(0.003)
Mother age square			0.000*	0.000*	0.000
			(0.000)	(0.000)	(0.000)
Married			-0.028**	-0.023**	-0.024**
			(0.004)	(0.005)	(0.005)
Working mothers			-0.000	-0.000	-0.001
			(0.004)	(0.004)	(0.004)
Salaried mothers			-0.006	-0.006	-0.006
			(0.003)	(0.003)	(0.003)
Home has electricity				-0.011**	-0.019**
				(0.004)	(0.005)
Home has television				-0.013**	-0.011**
				(0.002)	(0.002)
Home has refrigerator				-0.032**	-0.033**
				(0.005)	(0.005)
Family has a car				-0.025**	-0.026**
				(0.007)	(0.008)
Log(Tax income)					0.038**
					(0.013)
Log(Educational transfers)					-0.006
					(0.027)
Log GPS					-0.015
					(0.041)
Offensive attacks rate					-0.000**
					(0.000)
Constant	-3.939**	-4.012**	-3.569**	-3.547**	-2.631**
	(0.101)	(0.101)	(0.122)	(0.123)	(0.802)
Observations	86,849	86,849	86,849	86,849	81,857
Adjusted R-squared	0.260	0.276	0.295	0.296	0.278

Two-way clusters standard errors in parentheses. All specifications include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

Table 2.4: Heterogeneous effects of forced recruitment on attending school

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
More than 30 recruitment cases	-0.030**	-0.031**	-0.033**	-0.032**	-0.035**	-0.030**	-0.031**	-0.030**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Forced recruitment*child age(6-9 years old)		-0.026***						
		(0.000)						
Forced recruitment*child age(10-13 years old)		-0.034**						
		(0.002)						
Forced recruitment*child age(14-17 years old)		-0.048**						
		(0.002)						
Forced recruitment*male	-0.039**							
	(0.003)							
Forced recruitment*child birth order			-0.038**					
			(0.003)					
Forced recruitment*number of siblings				-0.034**				
				(0.002)				
Forced recruitment*mother education in levels(primary)					-0.019			
					(0.026)			
Forced recruitment*mother education in levels(secondary)					0.030**			
					(0.003)			
Forced recruitment*mother education in levels(higher)					0.044**			
					(0.002)			
Forced recruitment*married mothers						-0.001		
						(0.009)		
Forced recruitment*working mothers							0.006	
							(0.010)	
Forced recruitment*salaried mothers								0.018*
								(0.008)
Constant	-0.108	0.077	-0.107	-0.098	-0.146	-0.112	-0.112	-0.109
	(0.437)	(0.424)	(0.437)	(0.435)	(0.425)	(0.438)	(0.439)	(0.438)
Observations	85,770	85,770	85,770	85,770	85,770	85,770	85,770	85,770
Adjusted R-squared	0.123	0.112	0.123	0.120	0.124	0.122	0.121	0.121

Control variables include: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS) and offensive attacks rate. Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

Table 2.5: Heterogeneous effects of forced recruitment on grade repetition

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
More than 30 recruitment cases	0.035**	0.037**	0.038**	0.034**	0.037**	0.039**	0.031**	0.033**
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Forced recruitment*child age(6-9 years old)		0.013***						
		(0.000)						
Forced recruitment*child age(10-13 years old)		0.027**						
		(0.003)						
Forced recruitment)*child age(14-17 years old)		0.037**						
		(0.004)						
Forced recruitment*male	-0.028**							
	(0.002)							
Forced recruitment *child birth order			0.033**					
			(0.002)					
Forced recruitment *number of siblings				0.039**				
				(0.003)				
Forced recruitment*mother education in levels(primary)					0.017**			
					(0.004)			
Forced recruitment*mother education in levels(secondary)					-0.033**			
					(0.002)			
Forced recruitment*mother education in levels(higher)					-0.045**			
					(0.003)			
Forced recruitment*married mothers						-0.004		
						(0.011)		
Forced recruitment*working mothers							-0.002	
							(0.009)	
Forced recruitment*salaried mothers								0.006
								(0.014)
Observations	81,857	81,857	81,857	81,857	81,857	81,857	81,857	81,857
Adjusted R-squared	0.278	0.240	0.278	0.267	0.276	0.275	0.275	0.275

Control variables include: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS) and offensive attacks rate. Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

2.9 Robustness checks

I perform several robustness checks to address some important issues that may affect the results discussed above. The main concern is the sample selection arising from migration, especially mass migration triggered by forced recruitment and the consequent issue of endogenous residential sorting. The DHS datasets provide information on an individual's place of residence; however, the data does not allow me to establish if some individuals migrate at some point in their life or when this migration has occurred.

To assess whether the bias deriving migration is of serious concern in my results, models 1 and 2 in Table 2.6 present estimates from regressions that exclude from the analysis cases from 76 municipalities with high displacement expulsion rate in a given year. These municipalities have a per capita displacement expulsion rate in 2005, 2010, and 2015 above the mean from the national average in the period 1990-2016. The results are comparable in terms of magnitude, signs, and significance to those obtained using the full sample, suggesting that the main results are not driven by children who are exposed to forced recruitment in high displacement areas. This could be a problem if the characteristics of displaced children are significantly different from non-movers or, for instance, if the negative effect of forced recruitment in educational outcomes is due to the decision of movers to relocate away from schools in municipalities with a high number of forced recruitment cases.

To provide suggestive evidence that endogenous residential sorting issues are not the main drivers of the results, I re-estimate the model after excluding families who were not living in the same municipalities. The results in models 3 and 4 of Table 2.6 show that it does not alter the magnitude of the effect and the association of forced recruitment on school attendance, and that grade repetition remains significant.

Results in 2.7 include an additional sensitivity check, in which I remove those municipalities that have had no reported cases of forced recruitment during the analysis period. Doing so does not alter the conclusions reached so far concerning the effect of forced recruitment on educational achievement. This means that in the main regression, I am not simply capturing the impact of systematic differences between municipalities exposed (not exposed) to forced recruitment in a different way.

An additional concern in the identification strategy is that forced recruitment captures the effect of other types of violence. I control for alternative proxies of violence, such as municipal homicide rates, massacres rates, and death rates (see Table A1 of the end), and the results again remain stable.

Finally, another potential concern is that the effect of forced recruitment is changing according to the period during which a child is exposed to this type of violence measure. Although I use repeated cross-section and include all model fixed effects for the year of birth, one may wonder whether the results are stable to the parametric specification of the effect of age. In the model 1 estimates, I use a quadratic effect of the child's age. However, to test for sensitivity, in Table A2 of the document I use different age polynomials, which always provide very stable results.

Table 2.6: School attendance and grade repetition municipalities with low displacement rates and non-movers

	Municipalities with low displacement rate		Non-movers	
	Model 1 Attending School	**Model 2 Grade repettion**	**Model 3 Attending School**	**Model 4 Grade repettion**
0 recruitment cases	(reference category)			
1 to 10 recruitment cases	-0.001	0.023*	-0.007	0.021*
	(0006)	(0009)	(0006)	(0009)
11 to 30 recruitment cases	-0.023**	0.039**	-0.026**	0.043**
	(0.002)	(0.013)	(0.002)	(0.013)
31 to 50 recruitment cases	-0.021***	0.061**	-0.022***	0.061**
	(0.002)	(0.017)	(0.002)	(0.017)
More than 51 recruitment cases	-0.025***	0.054**	-0.024***	0.057**
	(0.002)	(0.019)	(0.002)	(0.019)
Observations	20,023	18,206	37,662	34,640
Adjusted R-squared	0.14	0.303	0.150	0.292

Control variables include: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS) and offensive attacks rate. Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

Table 2.7: School attendance and grade repetition in municipalities without force recruitment cases during the analysis

	Model 1 Attending School	**Model 2 Grade repettion**
0 recruitment cases	(reference category)	
1 to 10 recruitment cases	-0.002	0.020*
	(0.06)	(0.009)
11 to 30 recruitment cases	-0.023**	0.044**
	(0.002)	(0.013)
31 to 50 recruitment cases	-0.027**	0.061**
	(0.016)	(0.017)
More than 51 recruitment cases	-0.027**	0.051**
	(0.016)	(0.019)
Observations	74,974	69,291
Adjusted R-squared	0.122	0.282

Control variables: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS) and offensive attacks rate. Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities.***significant at 1%,** significant at 5%, * significant at 10%.

2.10 Final Remarks

This study contributes to a growing literature on the effects of early life exposure to violence on human capital formation by providing evidence on the effect of one of the most concerning violations of children´s rights: forced recruitment. Specifically, this paper aims to study the effect that exposure to forced recruitment during school age might have on school progression in Colombia, considering outcomes such as school attendance and grade repetition. Estimating such an effect is vital given the relevance that education has for the well-being of children, for their cognitive and non-cognitive development, and for their future socio-economic performance. In line with the existing literature on the effects of violent conflict on educational outcomes, I find that school-age children who live in municipalities that have experienced cases of forced recruitment are less likely to attend school and face a higher risk of grade repetition, even when controlling for a large set of children, mothers, household, and municipality variables. This is a relevant issue, not only because forced recruitment by itself is a grave violation of children's rights and international humanitarian law, but because it erodes the investment in human capital.

These results are relevant in the current context where, despite a steady decrease in grave violations against children in Colombia since the signing of the Peace Agreement between the Government and FARC in 2016, children are still being recruited as "child soldiers" by dissident FARC fighters and other armed groups. In addition, the effects caused by the COVID-19 pandemic, such as the temporary closure of schools, has increased the vulnerability of children in the most critical areas of the country. This reality shows that the challenges for populations in the most remote areas of the country continue to be complex, not only because of the wide social and economic gaps but also because of the deterioration of security conditions, which translate into greater institutional challenges to prevent this crime. The Colombian national strategy called "Súmate por mí" provides protective environments for children and adolescents in the prevention of recruitment, their utilization as child soldiers, and in the sexual violence against them to reinforce the deterrence of these violations and to strengthen the protection of young people. However, it is critical that the Colombian Government legitimizes the implementation of the strategy in the most vulnerable areas, otherwise armed groups will continue to victimize generations of children and adolescents. Exposure to forced recruitment during school age can lead to long lasting detrimental effects on school progression and exacerbate the gender differentials in educational outcomes.

Strengthening potential institutional actions that seek to reduce these international crimes against children is also imperative and should promote and include early prevention and protection routes in the action plans of each municipality in Colombia. In addition, the Colombian government should guarantee access for the entire population to this fundamental right to achieve a more egalitarian, developed, and peaceful society.

Chapter 3

Do Longer School Days have enduring academic effects in Colombia?

3.1 Introduction

In the last two decades, the Colombian educational system has undergone fundamental transformation. The structure of political change is very similar to changes in other Latin American countries. In the late 1960s, the Colombian government introduced the so called "half-shift school," in which two different groups of students attend the same school, one in the morning and one in the afternoon, to increase student enrollment. One of the problems associated with the half-shift school is the quality of education. In some countries, it has been found that teachers in these institutions, especially afternoon teachers, are less prepared and are absent more often (Linden, 2001). More in general, this type of education policy provides lower quality services, and this quality is as important or more important than quantity when it comes to the impact of education on inequality, growth, and development (Barro, 2001).

In the early 1990s, the Colombian Government attempted to eliminate practices such as the "half-shift school" as a strategy to improve the quality of education, and there was a law mandating full-day school (Law 115, 1994); however, it was suspended in 2002 mainly because of the high cost of its implementation. In 2015, the Ministry of Education announced the implementation of a nationwide full-day school program as one of the most important educational reforms. The reform stipulated that full-day schools should be established in urban areas by 2025 and in rural areas by 2030, and that the national government should finance this expansion.

In 2022, only 1% of students in official educational institutions were enrolled in full-day schools and the target set by the new government is 24% by 2026. One of the most important challenges the country is still facing in terms of human capital formation is improving the quality of education. Implementing effective education policies to increase academic achievement is central to fostering economic development. Inadequate quality of education is likely to raise the risk of grade repetition and school dropout, and definitely reduce skill formation.

This paper provides new evidence on the impact of full day school (hereafter FDS) on academic achievement in Colombia, using the difference-in-difference method for staggered policies developed by (Callaway & Sant'Anna, 2021) to answer three main questions. First, what is the cumulative average treatment effect of FDS across all schools? The second question is: Do schools that were exposed to FDS in different years have on average, higher or lower average treatment effects? The third question is: How does the effect of implementing FDS program vary by the length of exposure?

I analyze the impact of the FDS policy adopted by the Colombian government in 2015, to increase the time students spend in the classroom in order to improve the quality of education and learning outcomes. Proponents of longer school days make three main arguments for the effectiveness of these types of policies. First, by staying in school longer, students are likely to spend more time on academic tasks and learn more. Second, risky behaviors that occur outside of school, such as criminal activity and teen pregnancy, are reduced (Kruger & Berthelon, 2009), (Berthelon et al., 2015). Third, they provide a form of subsidized childcare and therefore can increase parental employment and family income, especially for families with young students (Berthelon et al., 2015).

Schools with longer school days are increasingly common in developing and developed countries. In Latin America, Uruguay and Chile pioneered the introduction of full-day schools in most of their public schools. There are only three studies that have examined the impact of the length of the school day on academic achievement in Colombia. (Bonilla, 2014), for example, evaluated the effects of attending a full-day school on students' performance on the high school exit exam (SABER 11, formerly known as the "ICFES test"), using the educational provision of a full-day school in the municipality of the student´s residence as an instrumental variable. He found a local average treatment effect (LATE) of 2.5% better test scores among students who attended full-day school compared those who attended half-day school. (Hincapie, 2016), on the other hand, found that test scores on SABER 5 and 9, a nationwide standardized test administered every three years in all elementary and secondary schools, were one-tenth of a standard deviation higher in school cohorts attending full-day schools than in cohorts attending half-day schools. Finally, (Vega, 2018) found no effect of full-day school reform on dropout and retention rates and a positive effect in the first two years of implementation on test scores in language in SABER 5 and SABER 9, but a negative effect on test scores in math in SABER 3 (administered to 3rd grade students to assess skills in math and language).

My study differs from this earlier literature because I examine the effects of full-day school policies on academic performance using an identification strategy appropriate for this framework that considers the staggered implementation of FDS, which was neglected in previous studies. Some schools implemented full-day-schooling in 2015 and others in subsequent years, but several schools have not implemented the policy by 2019, the final period in the dataset. Using school-level data, I adopt the difference-in-difference (DID) method of (Callaway & Sant'Anna, 2021) with multiple periods and variations in treatment timing to estimate an aggregate average treatment effect of a longer school day on academic achievement at the time the school first implemented the intervention. I also present dynamic average treatment effects to examine how the effect of the FDS varies with the length of exposure to the FDS. Next, I explore heterogeneous effects of school characteristics. In particular, I consider the type of school (private or public), the type of education (college or technical school), and the location (rural or urban). Finally, I conduct an additional analysis using individual-level data, to examine the effects of the years of exposure to the FDS until the year of the exam, controlling for a set of student, family, and school characteristics. This alternative approach also enables analyzing the presence of heterogeneous effects of the FDS policy according to students and family characteristics.

To implement the empirical analysis, I combine two different data sources covering the period 2014-2019. The administrative data from the Colombian Institute for the Evaluation of Education (ICFES), the dataset SABER 11, and the school census (C600), collected annually by the "Departamento Administrativo Nacional de Estadística" (DANE, the national statistics office), which contains information on school infrastructure and available material. The SABER 11 is a statewide standardized test administered annually in all public and private schools in Colombia. It is an assessment mechanism developed by the Colombian Institute for Educational Evaluation (ICFES) and the Colombian Ministry of Education to evaluate the knowledge of 11th grade students in order to improve the provision of educational services and provide educational institutions with relevant information about the competencies of applicants for higher education.

Overall, I find evidence that the FDS improves academic performance. Specifically, the adoption of the FDS improves test scores by 0.04 standard deviations. Looking at the results for each test subject, I find that the effect ranges from 0.02 to 0.04 standard deviation points in math, English, social studies, science, and reading. The effect sizes reported here are comparable to those described in previous studies examining FDS on academic achievement. For example, (Cerdan-Infantes & Vermeersch, 2007) found that FDS improved math and reading test scores by 0.02 standard deviations, while (Figlio et al., 2018) found that it improved reading test scores by 0.05 standard deviations. I also present dynamic average treatment effects. This estimation allows me to examine how the effect of FDS exposure changes with the duration of school exposure. In general, the effects are significant and lead to conclusions consistent with average treatment effects. However, the effect is small when the school first receives the treatment in 2016, but it is larger one and two years after the introduction of the FDS program. These results are robust to various sensitivity tests.

Finally, the additional specification with individual-level data confirms that the introduction of the FDS has a positive effect on test scores. Specifically, student exposure to the FDS program improves academic performance on general tests. The effects are larger in math and language than in social studies, science, and reading. For test scores in mathematics, the longer it has been since the introduction of the FDS program, the better they perform.

To understand the direct mechanism that leads to higher test scores when students are exposed to FDS program, I interact the variable of interest (years of exposure to the FDS) with student and household characteristics to test for the existence of heterogeneous effects. The results show that there is no evidence that exposure to a FDS is more beneficial for male than for female or younger students. Regarding the heterogeneous effects by parental education, relative to students whose parents have no education, the effect of the FDS appears to be stronger when the father completed primary, secondary or technical education and it increases monotonically with mother's education attainments. Finally, the impact of the policy appears to be higher for students belonging to middle-income and (especially) low-income families.

This paper is a contribution to the literature on the benefits of extending the school day for academic achievement in developing countries. I improve upon a previous work by presenting a staggered implementation methodology to better understand the effects of this type of intervention. The results presented in this study are useful for policymakers as many countries continue to improve the quality of education and increase the amount of time students spend in the classroom. Moreover, the evidence reported in this paper also sheds light on the expected results of the expansion of the FDS policies that is currently taking place in Colombia and provides an additional element to stimulate the debate on the effectiveness of this type of intervention.

The remainder of the paper is organized as follows: Section 2 presents the existing literature on the effects of school lengthening on academic achievement and the historical background of the Colombian education system. The data and empirical strategy are described in Section 3, while Section 4 presents the results and identification tests. Section 5 provides the concluding remarks.

3.2 Background

3.2.1 Evidence of the effect of full day schools on academic achievement

The scant evidence on the effects of full-day schools on academic achievement is mixed. Primarily, the literature suggests a positive effect on math, language, and reading tests (Aucejo & Romano, 2016; Battistin & Meroni, 2016; Bellei, 2009; Huebener et al., 2017; Pires & Urzua, 2010) identified positive effects on math test scores, finding an increase between 0.12 and 1.7 standard deviations and 2.5 standard deviations for language tests. In addition, (Figlio et al., 2018) show a 0.05 standard deviation improvement in reading test scores for low-performing schools in Florida. These results are consistent with those found by (Estrada et al., 2022) in the city of Fortaleza, Brazil. Enrollment in a full-day school increases high school graduation by 11 percentage points and math test scores by 0.22 standard deviations. (Dominguez & Ruffini, 2023) conclude that full-day schooling in Chile increases educational attainment, delays childbearing, and provides earnings gains in young adulthood on the order of 4 to 5 percent. More recently, (Kozhaya & Flores, 2022) examined the effects of extended school days on enrollment, child labor, and spillover effects on other family members in the case of Mexico. The results show that the full-day school program has no effect on enrollment but reduces the probability of child labor by 0.9 percentage points. The results indicate that these spillover effects are due to a substitution effect between child labor and adult labor. Fathers do not adjust their labor force participation, but they marginally increase the weekly hours they spend on housework.

On the other hand, there are studies such as (Agüero & Beleche, 2013; Barrios-Fernández & Bovini, 2021) in Mexico; (Cerdan-Infantes & Vermeersch, 2007) in Uruguay, that find smaller effects on test scores e.g., that additional days of instruction increase test scores in math and reading by 0.02 standard deviations. (Almeida et al., 2016) quantify the impact of a nationwide policy in Brazil called Mais Educao. The analysis shows no impact on school dropout rates and average negative effects on math test scores. In the case of Colombia, there are a few studies that have examined the effects of longer school days on student outcomes. (Bonilla, 2014), for example, used instrumental variables to measure the local average treatment effect (LATE) of full-day schools on test scores SABER 11 and found that students attending full-day schools had 2.5 percentage points better test scores than students attending half-day schools. However, the author assumes that all schools adopted the reform at the same point in time. As I mentioned earlier, the Colombian government adopted the FDS policy in 2015, and some schools have started implementing it this year, while others have not yet. I use a staggered difference-in-differences method (DID) to estimate an aggregate average treatment at the time the school first implemented the policy, which invalidates the method used by (Bonilla, 2014).

(Hincapie, 2016), on the other hand, uses plausible within-school variation in the length of the school day to analyze variation in average test scores. There are positive effects on academic achievement in math and language test scores in grades 5 and 9. Cohorts taught full-day have test scores that are about one-tenth of a standard deviation higher than cohorts taught half- day. Finally, (Vega, 2018) found no impact of full-day schooling on dropout and retention rates in Colombia, but a 2.5 percentage point decline in language test scores in SABER 3, SABER 5, and SABER 9. However, it is difficult to distinguish whether these results are due to differences in policy implementation or changes in study methodology.

3.2.2 Colombia's education system and the full day school program

Colombia´s basic education system is divided into three levels: Primary (grades 1 to 5) includes basic education cycles, lower secondary (grades 6 to 9), and upper secondary (grades 10 to 11). Primary education begins at age six, and all levels up to grade 9 are compulsory. Approximately 80% of students who complete elementary school, and of this group, nearly 90% come from low-income households. Public schools are run at the municipal level and are funded by the central government and from funds that the municipality may allocate according to its financial capabilities and priorities. Private schools receive no public funding and are allowed to operate as for-profit organizations; however, the setting of tuition and pension rates depends on the results of institutional evaluation of the quality of services provided. If a school achieves a high score on its self-assessment or is certified according to NTC ISO 9000[1]or one of the quality management models recognized by the Ministry of Education, it is classified as a regulated freedom and is free to set fees; private schools that achieve a medium score are classified as supervised freedom and may apply the fee rate set by the Ministry of Education within the established value ranges for the service category in which they are classified; and finally, private schools that achieve a low score are classified as controlled regulation and school fees are set by departmental and municipal secretaries of education (Ministerio de educacion, 1995)

In 1994, the Colombian government initiated a large-scale education reform, the General Education Law (Congreso de la República de Colombia, Law 115, 1994), which established the organization of the national education system in all its modalities and developed the principles for the provision of educational services. This law established that all public schools providing basic education have a single full school day (8 hours/day). However, the plan to implement this law was not initiated until years later and was completely neglected until 2002.

[1]The ISO 9000 family of Standards cited below has been developed to assist organizations of all types and sizes in the implementation and operation of effective quality management systems.

In 2015, the Colombian government[2] and the Ministry of Education announced the implementation of a nationwide Full-Day School (FDS) as one of the most important educational reforms, i.e., the strategy aims to transform public schools from a two and a half day shift[3] to a full day shift as a strategic line in the National Development Plan 2014-2018: "All for a New Country".

The FDS guidelines specify how the additional time in school should be distributed among the different activities, but the schools have the flexibility to implement their schedule with the activities indicated in the institutional educational project created by the educational institutions in exercise of the school autonomy defined in Article 77 of Law 115 of 1994. The success of the implementation of the program depends on several components: a) the available educational infrastructure, b) the implementation of the School Lunch Program (PAE), c) the human resources required to extend the school day, d) the development of strategies for the reorganization of educational activities, such as the mobilization of students enrolled in the two half shifts to a full-day shift, and e) the regular and efficient operation of public services in the schools.

An important aspect of the FDS is that neither the schools nor the students or parents can influence the selection of the school for the program. The selection of schools for the program depends on (i) the demand and supply of school seats for the following year, (ii) the availability of appropriate infrastructure for the extended school hours, (iii) the availability of only one shift, and (iv) the coverage of all grade levels in each school.

Schools that have opted into the FDS program receive technical assistance in developing strategies to align the curriculum with the additional hours by providing guidance and training to the appropriate school authorities. Nevertheless, these schools must provide time slots for other activities such as recreation, meals, sports, artistic, social, or cultural activities in addition to regular instructional time. They also receive financial support to cover the costs of extending the school day. The Colombian government estimates that 51.134 classrooms will need to be built by 2030 to ensure 100% implementation of the reform in public schools, representing an investment of 7.3 billion pesos (1.8 billion USD) at constant 2014 prices. According to the Ministry of Education, the FDS program will be implemented gradually. By the end of 2018, the central government had not met its target, as public school enrollment in the FDS ranged from 9% to 13%. In 2022, only 19% of students in official educational institutions were enrolled in full-day schools and the target set by the new government is 24% by 2026.

[2]Established in Article 85 of Law 115 of 1994, as amended by Article 57 of Law 1753 of 2015.

[3]Some schools in Colombia offer a morning and an afternoon shift. Students in the primary age attend the morning shift and students in the secondary age attend the afternoon shift.

3.3 Methods and data

3.3.1 Data

The data used in this study came from two different sources. The administrative data of the Colombian Institute for the Evaluation of Education (ICFES), the dataset SABER 11[4] and and the school census (C600), collected annually by the "Departamento Administrativo Nacional de Estadística" (DANE, the national statistics office). The SABER 11 is a nationwide standardized test administered annually in all public and private schools in Colombia. It is an assessment mechanism developed by the Colombian Institute for Educational Evaluation (ICFES) and the Colombian Ministry of Education to evaluate the knowledge of 11th grade students in order to improve the delivery of educational services and provide educational institutions with relevant information about the competencies of applicants to higher education. The SABER 11 dataset contains test scores derived from a series of 278 single-answer multiple-choice questions, 254 of which assess skills in language, mathematics, critical reading, and social, and natural sciences. The remaining 24 correspond to a socioeconomic questionnaire for research purposes, containing information on the student's year of birth, age, gender, father and mother education, family socioeconomic class, and type of school shift (morning, afternoon, evening, and full-day school).

These data are complemented by the School Census (C600), which is collected annually by the "Departamento Administrativo Nacional de Estadística" (DANE, the National Statistics Office) to compile information on basic school characteristics such as school type (public, private), location (urban or rural), type of school shift, total number of students enrolled, total number of teachers, and school location. In addition, the same administrative dataset also allows the identification of the year in which the school was enrolled in the FDS program. Therefore, I use the school identifiers to merge the two datasets and confirm that the type of school shift in SABER 11 and in the C600 was identical throughout the analysis period (2015-2019)[5].

Table 3.1 shows the main descriptive statistics using school-level information. These data suggest some important differences between treated and non-treated schools and indicate that the FDS program was implemented in schools that were at greater risk for poor academic performance and are consistent with policy efforts to improve educational quality. Schools that participated in the FDS program had lower average test scores on the overall test compared with schools that did not participate, but also lower average test scores in each of the knowledge areas assessed on the test, particularly English, and small differences on the reading comprehension test.

[4]The overall test score is obtained by multiplying the student´s global index by five. This index is calculated as a weighted average of the scores obtained in the tests. The weighting of the English test is one point and that of the other tests is three points. The score is on a scale of 0 to 500 points, with the average being 250 points. Therefore, a score above 250 is satisfactory, but the best results are those above 360 points. The score for each test ranges from 0 to 100 points, and the proficiency levels are insufficient, minimal, satisfactory, and advanced for all areas except English (A-, A1, A2, B1, B+).

[5]This time period for analysis was chosen because test scores from SABER 11 prior to 2014 are not comparable; they were ranked from 0 to 100 and then to 0 to 500.

I also observed a higher proportion of college, private, and rural schools that had never participated in the FDS program, as well as higher student-teacher ratio and computer per student's ratio in the schools that had never been treated.

Table 3.1: Summary statistics for main dataset

	All	Treated	Untreated	Diff	P-value on Diff
Global test score	250.4	248.4	250.8	2.33	0.00
Math	50.0	49.6	50.0	0.46	0.00
English	50.0	49.1	50.1	1.05	0.00
Natural Science	50.1	49.7	50.2	0.44	0.00
Reading Comprehension	51.3	51.0	51.3	0.29	0.00
Social Science	48.9	48.4	48.9	0.46	0.00
College/Technical schools					
College	0.598	0.431	0.621	0.202	0.00
Technical	0.362	0.553	0.336	-0.162	0.00
Male/female schools					
Male	0.006	0.003	0.007	0.004	0.00
Female	0.02	0.03	0.02	-0.010	0.00
Mixed-sex education	0.973	0.968	0.974	0.005	0.00
Urban/Rural schools					
Urban	0.691	0.287	0.311	-0.024	0.00
Rural	0.308	0.713	0.689	-0.015	0.00
Private/Public Schools					
Private	0.344	0.083	0.380	0.296	0.00
Public	0.655	0.916	0.619	0.254	0.00
Students per computer ratio	0.19	0.22	0.19	-0.03	0.00
Students teacher ratio	104.8	96.32	105.94	9.62	0.00
Observations	59.084	7.108	51.976		

Notes: Treated schools include schools that adopted FDS after 2015. The untreated group includes schools that were never exposed to FDS.

Table 3.2 presents summary statistics for all tested students and key variables, broken down by schools that switched to FDS after 2015 and schools that did not. Variables include test scores (overall and by subject), student characteristics (gender and age), household characteristics, including parental education and socioeconomic class, and school characteristics (computer per student ratio and student-teacher ratio). Students in FDS schools have significantly higher test scores than students at other schools. The differences are larger in math and science, by 5.5 and 4.6 points, respectively. In addition, the two groups in Table 3.2 differ on several dimensions: Students in full-day schools are slightly younger than non-FDS students, which could be due to fewer grade repetitions, and a higher proportion of parents tend to be more educated and have better socioeconomic conditions.

At non-FDS schools, the proportion of boys is larger, high school competition is the highest level of parental education, 79% of students live in middle-income households, the computer-student ratio is lower, and the student-to-teacher ratio is higher.

Table 3.2: Summary statistics, all tested students 2015-2019

	All	Treated	Untreated	Diff	P-value on Diff
Global score	254,2	273,0	248,3	-24,7	0.0
Score in math	50,3	55,1	49,6	-5,5	0.0
Score in language	50,6	55,3	49,0	-4,3	0.0
Score in natural science	50,8	54,3	49,6	-4,7	0.0
Score in critics reading	52,2	55,5	51,1	-4,3	0.0
Age	17	17	18	1,0	0.0
Gender					
Male	44,9	45,9	54,9	8,90	0.0
Female	55,1	54,1	45,1	-9,00	0.0
Father Education					
No education	3,7	2,5	4,1	1,6	0.0
Primary	31,3	26,4	32,9	6,5	0.0
Secondary	37,1	31,6	38,8	7,2	0.0
Technical	8,0	9,8	7,5	-2,3	0.0
Higher education	11,7	22,1	8,4	-13,7	0.0
No information	8,1	7,5	8,3	0,7	0.0
Mother Education					
No education	1,8	1,2	2,1	0,9	0.0
Primary	27,5	22,0	29,2	7,2	0.0
Secondary	42,2	34,9	44,6	9,7	0.0
Technical	11,3	13,6	10,5	-3,1	0.0
Higher education	13,0	24,8	9,3	-15,5	0.0
No information	4,2	3,6	4,4	0,7	0.0
Household socio economic stratum					
Lower income	1,9	4,5	1,0	-3,4	0.0
Middle income	74,6	59,3	79,4	20,2	0.0
Higher income	23,6	36,3	19,5	-16,8	0.0
Students per computer ratio	0,17	0,19	0,18	-0,01	0.0
Students teacher ratio	110,0	105,0	112,0	6.8	0.0
Observations	3.035.479	730.244	2.305.235		

Notes: FDS schools include schools that adopted the FDS program after 2015. The Non-FDS includes schools that were never exposed to the FDS program.

3.4 Empirical Strategy

The purpose of this analysis is to determine the impact of adopting the FDS on academic performance. A first approach to determine this effect would be to use a "two-way fixed effects" (TWFE) regression model. However, because the introduction of the FDS program was staggered and possibly not random, as more disadvantaged schools were selected for participation in the program, both observable and unobservable school characteristics are certainly correlated with program participation. Moreover, these characteristics also likely influence test scores, so their omission form the regression leads to a biased estimate of program impact. Controlling for omitted variables is difficult. As we saw in Section 2.2.2, a school had to meet certain requirements for adoption of the FDS program, and these characteristics were necessary, but not sufficient. Including these variables in the regression will not eliminate all bias because additional criteria for selecting schools that meet program requirements are not easily measurable. In addition, in many situations where the timing of treatment varies and treatment effects are dynamic, the TWFE model may not be accurate enough to assess policy changes because the estimation may result in a negative bias, underestimating the program effect.

The specification I use in this paper attempts to minimize the potential bias arising from the selection of schools for participation in the program based on unobservable school characteristics and the different timing of FDS implementation. I estimate a difference-in-difference method (DID) with staggered adoption using the (Callaway & Sant'Anna, 2021) approach to calculate the group-time average treatment effect, which in this case consists of a one-time average treatment effect on those treated (ATT) for a school participating in the FDS program in the same year. This procedure is robust to heterogeneity in treatment effect resulting from differences in timing of the first exposure and the duration of the exposure. All inference procedures use the cluster-robust multiplier bootstrap standard error at the school level proposed in (Callaway & Sant'Anna, 2021). I use the doubly robust estimator for identification because it is based on less stringent modeling conditions and provides additional robustness to model misspecification compared to outcome regression (OR) or the inverse probability weighting (IPW) approach. An advantage of this method is that I can identify a single average treatment effect parameter that has the same interpretation as the ATT in the canonical DID setup.

Following the notation in the work of (Callaway & Sant'Anna, 2021), the traditional DID setup is specified as follows:

$$\mathrm{Y}_{st} = \alpha_t + \delta_g + \beta_{FDS_{st}} + \gamma_{X\,st} + \epsilon_{st} \tag{3.1}$$

where Y_{st} is the standardized score of school s in year t, FDS_{st} is an indicator of whether school s has implemented FDS, and α_t and δ_g are time and group fixed effects. The key building block of this analysis is the group-time average treatment effects, i.e., the average treatment effect for group g at time t, where a "group" is defined by the time in which units (schools) first receive treatment (Callaway & Sant'Anna, 2021). I include α_t to capture general annual shocks, such as the introduction of additional policies that might directly affect the quality of schooling, e.g., the National School and Teachers Evaluation System. $\gamma_{X\,st}$ is a vector of school characteristics that are likely to affect academic performance, including type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computer per students and ratio of students per teacher (as proxies for school facilities), and ϵ_{st} is the error term.

In the above specification, β is the effect of attending an FDS school. However, recent research on heterogeneous treatment effects in DID with variation in treatment timing has shown that one must be very cautious in ascribing a causal interpretation to aggregate parameters (Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Sun & Abraham, 2021). In general, β yields a weighted average of the underlying treatment effect parameters, but some of the weights of these parameters may be negative, implying that equation 3.1 may not be estimate a valid ATT. In contrast, (Callaway & Sant'Anna, 2021) provide a way to directly aggregate group time average treatment effects over different treatment exposure durations. Assuming parallel trends, the Callaway and Sant'Anna methodology allows estimation of the average treatment effect for each group, which can be combined to estimate ATT in post-treatment years for each group.

In my framework, I have five possible treatment periods labeled g (2015-2019). There are also schools that are "never-treated". Thus, "never-treated" schools can serve as different comparison groups. $G_(s, g) = 1$ if school s is treated for the first time at time g, and zero otherwise. Since there can be schools that are treated at the same time, I can refer to all such schools belonging to the same "group" of treated schools. Thus, g is a dummy variable indicating that treatment begins for each school.

Once a school is treated, it remains treated in the following periods. In addition, $Y_st(g)$ is the standardized test score for school s at time t, if that school is treated at time g. $Y_st(0)$ is the untreated potential outcome for a school and Y_st is the observed outcome for school s at time t. Thus, for the schools that are "never- treated", the observed outcome is equal to their untreated potential outcome $Y_st = Y_st(0)$, but, for the treated schools, the observed outcome is their potential outcome when they were "never-treated" and their potential outcome when they join the "treated" group G_s at time t. Therefore, I will estimate the group-time average treatment effects $ATET_(g, t) = E\left[Yt_g - Y_t(0)|G = g\right]$.

This means, for example, that $ATET_(g,t)(g = 2015, t = 2016)$ is the average treatment effect in 2016 for schools that were treated in 2015. (Callaway & Sant'Anna, 2021) show that the assumption of parallel trends is determined based on either "never-treated" or "not-yet-treated" units and is even more plausible when conditional on pre-treatment covariates.

3.5 Results

3.5.1 DID staggered estimates

In this section, I report results based on school-level data under the hypothesis that the assumption of parallel trends conditionally after controlling for observed characteristics holds. I also consider the case where schools in non-FDS (which have never been treated) form the comparison group. The outcomes of interest are the overall test scores, and separate test scores in mathematics, English, social studies, science, and reading comprehension.To account for changes in testing, I standardized test scores by year to obtain z-scores. The first row of Table 3.3 reports the average effect of the FDS for all schools that have ever participated in the FDS, while the following rows report the effect for each group. In doing so, I assume only that schools with the same characteristics would follow the same trend in standardized test scores in the absence of treatment. All inferences use clustered bootstrapped school-level standard errors, and account for autocorrelation of the data. The school characteristics used are school type (public or private), college or technical schools, location (urban or rural), student-teacher ratio, and computer-per-student ratio. I find that, on average, FDS increases overall test score by 0.04 standard deviations. In contrast, the effect in mathematics, English, social studies, science, and reading is between 0.02 and 0.04 standard deviations, although it is larger for mathematics and English (see document, Table A3 and Table A4). The effect sizes reported here are comparable to those described in previous studies examining FDS on academic achievement. For example, (Cerdan-Infantes & Vermeersch, 2007) found that FSD improved test scores in math and reading by 0.02 standard deviations, while (Figlio et al., 2018) found that it improved test scores in reading by 0.05 standard deviation. Results by "group-specific treatment effects" are shown in the second panel of Table 3.3. The column ATT summarizes the average treatment effects by time of FDS adoption, highlighting the heterogeneity of treatment effects. The biggest effects appear to come from schools that implemented the FDS policy in 2016, which has a coefficient of 0.173 standard deviations improvement in test score, while for 2015 the point estimate is 0.168 but imprecisely estimated. The effect of FDS decreases substantially for schools that adopted the policy in 2017 (0.045 standard deviation points) and is null for the last two years (2018 and 2019).

Table 3.3: FDS reform aggregated treatment effect estimates

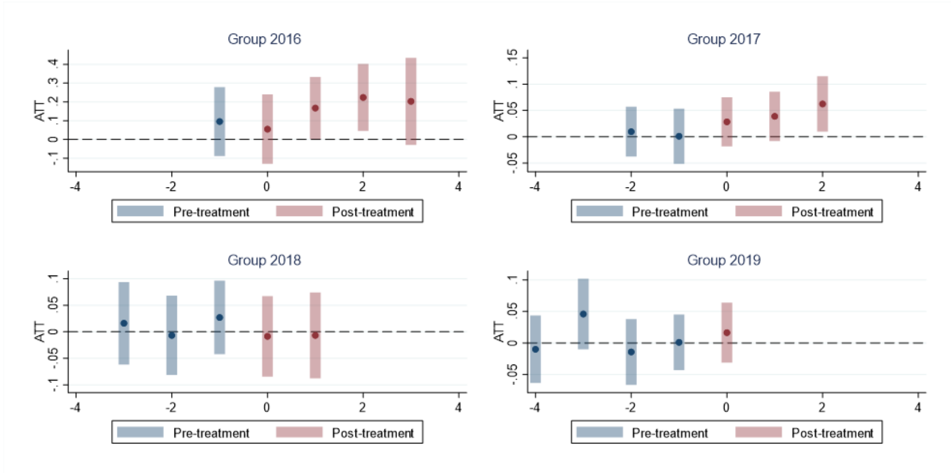
	ATT	**SE**	**95% confidence bands**	
All	0.040	0.010	0.019	0.061
By group				
2015	0.168	0.273	-0.525	0.861
2016	0.173	0.054	0.036	0.310
2017	0.045	0.014	0.010	0.076
2018	0.015	0.021	-0.069	0.039
2019	0.014	0.016	-0.026	0.053
Observations	58.610			

Notes: The table contains aggregate treatment effect parameters assuming conditional parallel-trends and with school-level clustering and asymptotically normal standard errors. The first row reports the weighted average of all available group-time average treatments. Rows by group report group-specific effects by time of FDS adoption. The estimates use the doubly robust estimator.

Figure 3.1 shows the "group-specific treatment effects" on the total test score from Equation 3.1 along with the 95% confidence intervals. Although understanding this type of heterogeneity is relatively less common in applied research than trying to understand dynamic effects (see below), they are useful for understanding whether the effect of treatment participation is larger for groups that receive treatment earlier than for groups that receive treatment later. The graph includes estimates for the pre-treatment period that can be used to "pre-test" the assumption of parallel trends. The group-specific treatment effects support the view that schools that implemented FDS in an early phase (2016 and 2017) have higher standardized test scores. Figure 1 also shows that the effects of FDS program vanishes for schools that first adopted the FDS program in 2018 and 2019, likely because the implementation of FDS slowed down considerably after 2017, which may explain why the effect is not significant in those years.

Similar figures can be found in the document (Figure A1 to Figure A5) for separate test scores. Two main results can be seen from the figures: none of the point estimates in the pretreatment periods (spike line on the left) are significantly different from zero, consistent with the assumption of parallel trends. Second, many effects in the post-treatment periods (spike line on the right) are significant for schools that first implemented the FDS program in 2016 and 2017, suggesting that the FDS program also improves test scores in math, English, social studies, science, and reading comprehension.

Figure 3.1: Overall test score group-time average treatment effects



Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95% confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95% confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant'Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Next, I describe the dynamic effects of implementing the FDS to examine how the effects of treatment participation vary with the length of exposure to the treatment. In other words, I want to determine whether the average treatment effect increases with elapsed treatment time. These results are relevant for two reasons. First, I can observe the overall cumulative effect of the FDS to see if parallel trends exist. Second, I can obtain results that exclude those schools for which the assumption of a parallel trend may not hold. In addition, they are also important because a potential problem with estimating the dynamic effect using the methodology of (Callaway & Sant'Anna, 2021) is that the composition of the sample changes with the duration of exposure . For example, for schools that implemented FDS in 2019, I can only determine the contemporaneous effect (event-time=0), while for schools that implemented FDS in 2016, I can determine the contemporaneous effect and the FDS effects at event-time =1, 2, 3 (2017, 2018 and 2019). The problem with the results presented above is that I include all schools that implemented FDS in the calculation. However, if the impact of FDS varies from year to year, this could lead to confounding dynamics in my event study. A simple alternative that can be used to highlight the dynamics of the treatment effect with respect to the length of exposure to the treatment, which does not suffer from the problem of compositional changes arise from "balancing" the groups with respect to event time.

That is, to calculate the average group-time average treatment effect for units whose event time is equal to two years, which are observed to participate in the treatment for at least five years. A limitation of "balancing" groups with respect to event time is that fewer groups are used to calculate event-study-type estimates, which may lead to less informative inference.

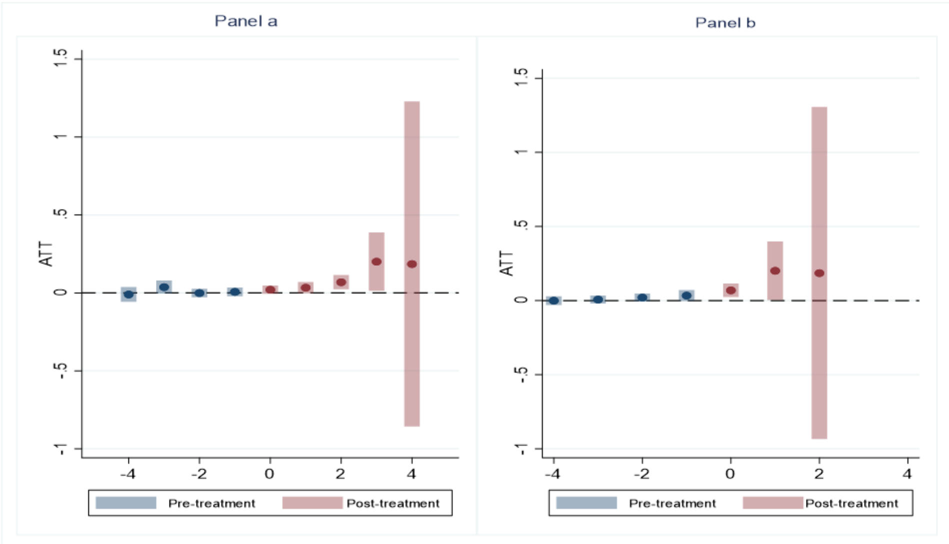
Figure 3.2 shows the dynamic effect of FDS implementation. Panel a includes all schools. Estimates for the pre-treatment period (spike line on the left) are not statistically different from zero and are consistent with the parallel trend assumption. The FDS effects estimated for the post-implementation period are larger and statistically significant for 2016 to 2018, and the magnitude appears to increase with duration of exposure. In other words, schools improve their overall test scores one, two, and three years after implementing the FDS program. Specifically, in the second year that schools implement the FDS reform, overall test scores increase by an estimated 0.07 standard deviations, by 0.20 higher standard deviation in the third year, and by 0.18 higher standard deviations in the fourth year, but these are not significant (see document TableA3). These results are robust when I restrict the sample to include only groups (schools) that implemented the FDS reform for at least two school years (i.e., I keep 2016 and 2017 groups but not 2018 and 2019). In panel b, I estimate that the effect of implementing the FDS on overall test scores is 0.03 and 0.18 higher one and two years after implementation, respectively (see document Table A4). Finally, the document provides dynamic average treatment effects on the separate test scores for each subject (see Figure A6 and Figure A7). The effect of FDS reform implementation appears to be higher for standardized test scores in math and English than for science, reading, and social studies, and it becomes larger the longer schools are exposed to FDS reform, at least in the first three years after program implementation.

3.5.2 Identification Checks

The main threat of my identification strategy is that participation in the FDS program may be correlated with unobserved characteristics. For example, if schools that have adopted the FDS program are simultaneously implementing other initiatives or are affected by other compositional changes that directly or indirectly affect standardized test scores, this would question the validity of my results.

To show that my results are not due to unobserved factors correlated with the implementation of the FDS program, I conducted several robustness tests. First, I present an event study model for the entire school sample. This model allows me to rule out preexisting trends and assess whether effects persist several years after the school´s treatment. The event study plot in Figure A8 in the document shows an increase in standardized test scores. These impacts are still present two years after the school was first classified as treated. The figure also supports the idea that the parallel trends assumption is valid, as most point estimates during the pre-treatment period are not statistically significant.

Figure 3.2: Overall test score dynamic aggregated treatment effects estimates



Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95% confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95% confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant'Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

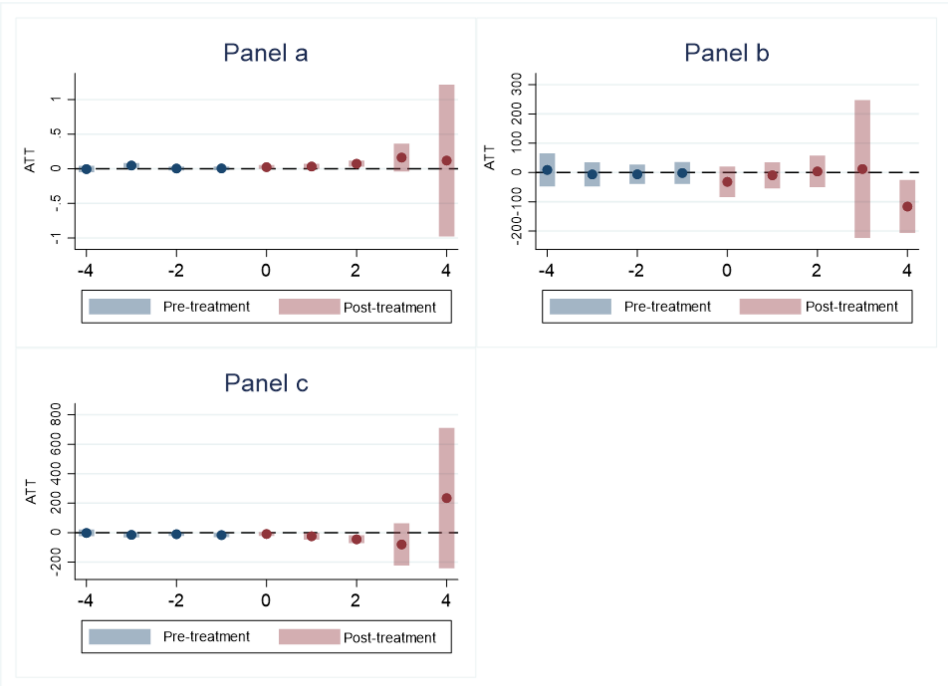
Second, it is possible that the FDS program has a significant effect because of the improved performance of public schools in the five most populous cities in Colombia. Therefore, I repeat the estimation using the methodology of Callaway and Sant'Anna, but exclude schools in Bogota, Medellin, Cali, Barranquilla, and Cartagena from the sample. The results are similar to those reported above (Figure 3.3, panel a), suggesting that the main evidence is not explained by school performance in larger cities.

I also conduct the analysis using the student-teacher ratio as the dependent variable to understand whether the introduction of the FDS may have changed the composition of schools see (Konstantopoulos & Chung, 2009);(Blatchford et al., 2011), among others. In addition, high-performing schools may have more resources to hire teachers to provide additional instructional time, thereby improving test scores. Figure 3.3, panel b shows no significant differences in the pre- and post-treatment periods and a slight but not significant decrease in 2019.

In addition, I used the total number of students to examine whether the positive results of the FDS reform on test scores were due to changes in the composition of test takers. This could confound my results if, for example, the increase in retention rates was related to an increase in the average developed ability level of the larger number of students who stay in school, but also if there was an increase in the number of test takers from groups with traditionally high scores. Panel c in Figure 3.3 shows that there are no significant differences between the pre- and post-treatment periods in the number of students, with the exception of 2019, but the effect is not statistically significant, suggesting that the main results are not due to the favorable school environment and changes in test takers.

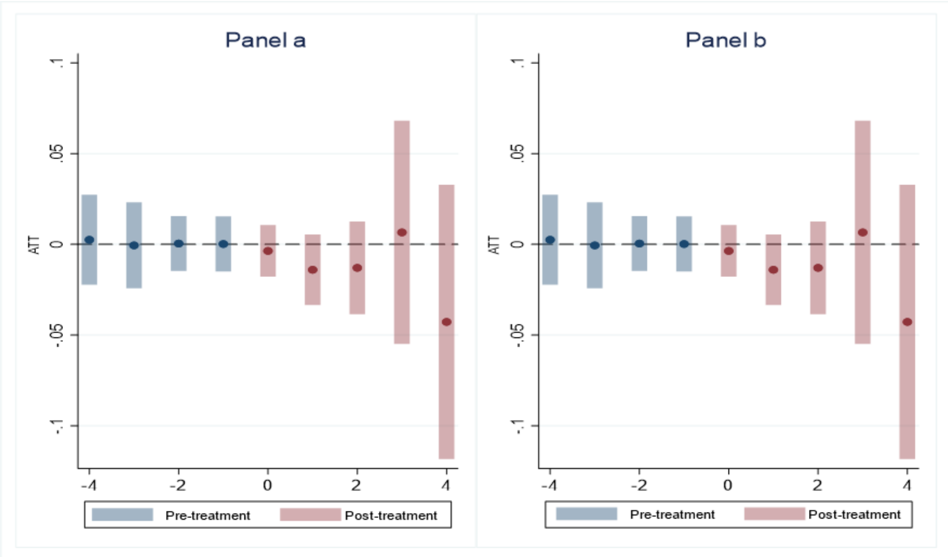
Finally, to assess whether the FDS reform changed the share of parents with higher education, I present the results in Figure 3.4, panels a and b, focusing on parents' education level as the dependent variable. Specifically, I consider the proportion of students for whom have at least one parent completed higher education. I find that the proportion of parents with higher levels of education decreases slightly after the post-treatment period and increases in the third year of FDS implementation, but the effects are not statistically significant. This suggests that my results are not driven by changes in the proportion of parents with higher education.

Figure 3.3: Dynamic average treatment effects estimate robustness checks



Notes: The figure shows the effect of FDS on various compositional changes conditional parallel trends. Point estimates are given along the y-axis and refer to the grade before(after) the introduction of the FDS (x-axis). Spike lines on the left indicate point estimates and associated 95% confidence bands for the pre-treatment period, accounting for school-level clustering. Spike lines on the right indicate point estimates and simultaneous 95% confidence bands for the treatment effect of FDS, accounting for school-level clustering. Panel a. shows the dynamic average treatment effects excluding Bogotá, Medellín, Cali, and Barranquilla from the sample. Panel b. includes the student-teacher ratio as the dependent variable. Panel c shows results focusing on the number of students. I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure 3.4: Dynamic average treatment effects estimate robustness checks



Notes: The figure shows the effect of FDS on various compositional changes conditional parallel trends. Point estimates are given along the y-axis and refer to the grade before(after) the introduction of the FDS (x-axis). Spike lines on the left indicate point estimates and associated 95% confidence bands for the pre-treatment period, accounting for school-level clustering. Spike lines on the right indicate point estimates and simultaneous 95% confidence bands for the treatment effect of FDS, accounting for school-level clustering. Panels a and b show the dynamic average treatment effects for the educational level of the father and mother, respectively. I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

3.5.3 Heterogenous effects by school characteristics

The FDS program might favor certain types of schools more than others, which cannot be detected in the aggregate estimations reported above. To account for possible heterogeneous effects of the policy, I estimate aggregate treatment effects by school characteristics. I consider school type (private or public), type of education (college or technical school), and location (rural or urban). The overall effect reported in the first row of each panel of Table 3.4 is positive and significant for all school types, except for schools with college education.

Results by private or public school are shown numerically in panels a and b. Public schools that implemented FDS in 2016 and 2019 have higher overall standardized test scores. For private schools, the effect is small and only marginally significant for schools that implemented the FDS program in 2017 and 2019. Panels c and d presents results by type of education. The group-time average treatment effect is 0.045 standard deviations of higher standardized test scores for schools with college education. The effect is significant for college and technical schools that implemented the FDS program in 2017 and 2019. The last row (panels e and f) shows the results by school location. The aggregate average treatment effect is larger for rural schools than for urban schools. Rural schools that implemented FDS in 2017 and 2019 have 0.047 and 0.019 higher standardized test scores, respectively. These results highlight the heterogeneity of treatment effects across groups. Rural schools are most likely low-quality, so there may be many schools that benefit academically from implementing the FDS program. Finally, for the six subsamples, I also find a negative and insignificant effect for schools implementing the FDS in 2018 but a positive effect in 2019, which may suggest that the negative effects diminish as schools improve their implementation.

Table 3.4: FDS aggregated treatment effect estimates by school characteristics

	ATT	**SE**	**95% confidence bands**			**ATT**	**SE**	**95% confidence bands**	
	Panel A. Private schools					Panel B. Publics schools			
All	0.038	0.049	-0.057	0.135	All	0.037	0.010	0.016	0.057
By group					**By group**				
2015	0.541	0.560	-0.557	1.639	2015	0.038	0.184	-0.400	0.324
2016	0.262	0.216	-0.161	0.687	2016	0.156	0.056	0.046	0.266
2017	0.007	0.050	-0.092	0.106	2017	0.042	0.013	0.015	0.070
2018	-0.070	0.162	-0.193	0.052	2018	-0.005	0.122	-0.049	0.039
2019	0.040	0.069	-0.095	0.177	2019	0.135	0.016	-0.018	0.045
Observations	20.059				Observations	38.543			
	Panel C. College schools					Panel D. Technical schools			
All	0.027	0.159	-0.003	0.592	All	0.041	0.013	0.021	0.075
By group					**By group**				
2015	-0.206	0.178	-0.556	0.143	2015	0.848	0.264	0.179	1.516
2016	0.140	0.08	-0.028	0.309	2016	0.160	0.063	0.001	0.320
2017	0.040	0.020	0.006	0.080	2017	0.047	0.017	0.003	0.091
2018	-0.020	0.132	-0.084	0.043	2018	-0.005	0.129	0.080	0.068
2019	0.003	0.025	-0.045	0.052	2019	0.019	0.020	-0.033	0.072
Observations	35.149				Observations	21.374			
	Panel E. Urban schools					Panel F. Rural schools			
All	0.036	0.011	0.013	0.059	All	0.045	0.022	0.002	0.088
By group					**By group**				
2015	0.208	0.236	-0.254	0.671	2015	0.002	0.655	-1.283	1.287
2016	0.156	0.056	0.045	0.266	2016	0.252	0.181	-0.103	0.609
2017	0.025	0.015	0.005	0.006	2017	0.047	0.025	0.009	0.111
2018	-0.002	0.022	-0.043	0.043	2018	-0.005	0.054	-0.166	0.047
2019	0.011	0.015	-0.019	0.042	2019	0.019	0.042	-0.079	0.088
Observations	40.564				Observations	18.016			

Notes: The table contains aggregate treatment effect parameters assuming conditional parallel-trends and with school-level clustering and asymptotically normal standard errors. The first row of each panel contains the weighted average of all available group-time average treatment effects. Rows by group report group-specific effects by time of FDS introduction. The estimates use the doubly robust estimator discussed in (Callaway & Sant'Anna, 2021).

3.5.4 Two-way fixed effect model with individual-level data

I apply an additional specification with individual-level data to support the results obtained with Callaway and Sant 'Anna methodology, which also allows for the analysis of heterogeneous effects as a function of student and family characteristics. The effect of FDS on SABER 11 test scores is captured by a variable that measures the number of years a student was exposed to the FDS program before taking the test. The exposure period is the difference between the year in which the FDS reform was implemented at the school and the year in which students take the test ($\mathrm{E}_s t$).

The model to be estimated is described by the following equation:

$$\mathrm{Y}_{ist=}\alpha+\beta\mathrm{E}_{st}+\delta X_{ist}+\gamma Z_{st} + \pi_t+\theta_s + +\epsilon_{ist} \tag{3.2}$$

where Y_{imt} is the test score of students i in school s at the time t, E_{st} is the length of exposure to the full day school for school s at the time t (in years), X_{ist} is a set of student-level controls such as age, gender, parental education, and household socioeconomic class, Z_{st} is a vector of time-varying school characteristics (student per computer and student teacher ratio) π_t and θ_s are time and school fixed effects and ϵ_{ist} is the error term. From this specification, the estimate of β would provide the effect of an additional year of exposure to the FDS on student´s test scores.

3.5.5 Evidence from individual-level models

In the first estimation of model 3.2, I use global standardized test scores as the dependent variable; the results are reported in Table 3.5. All specifications include school and year fixed effects as well as cluster standard errors at the school level. Model 1 shows the result of a specification that includes only a simple set of child characteristics, namely gender and age. The results show that the impact is positive and significant, i.e., overall test scores increase by 0.020 standard deviations for each additional year of exposure to the FDS policy. It is also important to comment on the estimates of the control variables, which are also of independent interest. For example, being male increases test scores by about 0.219 standard deviations, and those who are older relative to their classmates have lower performance on the test, which captures grade repeaters.

Model 2 shows the results obtained by controlling for parental, household, and school characteristics such as father´s and mother´s educational level, social class, and computer-per-student ratio and student-teacher ratios. The estimated coefficient for the years of FDS exposure is only slightly higher than that for the model with the baseline set of student controls (0.028 standard deviations). As for the control variables, both father's and mother's education appear to be relevant predictors of test scores. Family social class also seems to be a strong predictor of test scores. Students from middle-and upper-income households score 0.10 and 0.15 standard deviations higher on the test than students from lower-class households, respectively. For the time-varying school controls, the number of computers per students is positively associated with test scores, but no effect is found for the student-teachers ratio.

For models 3 and 4, I use dummies for the years of exposure to the FDS reform, with the same set of controls included in models 1 and 2, respectively. Overall estimate of the test shows that longer exposure to the reform leads to higher test scores. Two, three and four-year participation in the FDS reform is associated with increases in test scores of 0.04, 0.06, and 0.13 standard deviations, respectively (considering the full specification).

Table 3.5: Regression results: Dependent variables: Overall test score

	Model 1	**Model 2**	**Model 3**	**Model 4**
Years of exposure to FDS	0.020***	0.028***		
	(0.004)	(0.005)		
0 year of exposure to FDS		(reference category)		
1 year of exposure to FDS			0.017***	0.016**
			(0.005)	(0.008)
2 years of exposure to FDS			0.041***	0.041***
			(0.008)	(0.010)
3 years of exposure to FDS			0.057**	0.064***
			(0.027)	(0.009)
4 years of exposure to FDS			0.094	0.134**
			(0.077)	(0.065)
Male	0.219***	0.223***	0.219***	0.223***
	(0.002)	(0.002)	(0.002)	(0.002)
Student´s age	-0.190***	-0.166***	-0.190***	-0.166***
	(0.001)	(0.001)	(0.001)	(0.001)
Father´s education				
Primary		0.034***		0.034***
		(0.003)		(0.003)
Secondary		0.088***		0.088***
		(0.003)		(0.003)
Technical		0.229***		0.229***
		(0.004)		(0.004)
Higher education		0.262***		0.262***
		(0.004)		(0.004)
Mother´s education				
Primary		0.038***		0.039***
		(0.004)		(0.004)
Secondary		0.100***		0.100***
		(0.004)		(0.004)
Technical		0.254***		0.254***
		(0.005)		(0.005)
Higher education		0.276***		0.276***
		(0.005)		(0.005)
Middle income		0.177***		0.156***
		(0.006)		(0.007)
Higher income		-0.015**		-0.020***
		(0.007)		(0.008)
Students per computer		0.020**		0.020**
		(0.008)		(0.008)
Students teacher ratio		0.000		0.000
		(0.000)		(0.000)
Constant	3.238***	2.556***	2.500***	2.556***
	(0.020)	(0.016)	(0.019)	(0.016)
Observations	3,032,445	2,079,662	3,032,445	2,079,662
Adjusted R-squared	0.413	0.427	0.430	0.427

Notes: All specification include fixed effects for child year of birth, year of interview and municipalities. References category (no education and low income)***significant at 1%,** significant at 5%, * significant at 10%.

As an additional result, I also estimated model (2) for test scores in each subject. The results are shown in Table A7 and Table A8 in the document. The largest effects are for English and math, where FDS increase test scores by 0.028 and 0.022 standard deviations,respectively.

For math test scores, the effect is significant and increases with the number of years of exposure. In contrast, the FDS program increases test scores in English in the first two years of exposure. In science, reading, and social studies, the effect is also positive and significant, test scores increase by about 0.01 standard deviations, and the effects are stronger in the second year of FDS program implementation.

Finally, to further investigate the existence of heterogeneous effects of the policy, I interact the variable of interest (student exposure to the FDS program) with student and household characteristics. The results in Table 3.6 show the positive and significant effect of FDS on overall test scores in the first row of each model. However, there is no evidence that exposure to a FDS is more beneficial for male students than for female or younger students (see Models 1 and 2, Table 3.6)

Models 3, 4, and 5 in Table 3.6 show the results for the interaction between years of exposure to the FDS program with parental education and socioeconomic class. Regarding the heterogeneous effects by parental education, relative to students whose parents have no education, the effect of the FDS appears to be stronger when the father completed primary, secondary, or technical education and it increases monotonically with mother's education attainments. Finally, the impact of the policy appears to be higher for students belonging to middle-income and (especially) low-income families.

Table 3.6: Heterogeneous effects results: Dependent variables: Overall test score

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Years of exp to FDS	0.018***	0.020***	0.026***	0.035***	0.048***
	(0.005)	(0.003)	(0.001)	(0.003)	(0.017)
Years of exp to FDS*male	0.003				
	(0.004)				
Years of exp to FDS*age		0.001			
		(0.002)			
Father´s education					
Years of exp to FDS*primary			0.037***		
			(0.010)		
Years of exp to FDS*secondary			0.036***		
			(0.010)		
Years of exp to FDS*technical			0.043***		
			(0.012)		
Years of exp to FDS*higher education			0.020*		
			(0.012)		
Mother´s education					
Years of exp to FDS*primary				0.016	
				(0.012)	
Years of exp to FDS*secondary				0.015***	
				(0.002)	
Years of exp to FDS*technical				0.017***	
				(0.004)	
Years of exp to FDS*higher education				0.022***	
				(0.003)	
Years of exp to FDS*low income					0.075***
					(0.017)
Years of exp to FDS*middle income					0.050***
					(0.017)
Constant	2.556***	2.556***	2.558***	2.557***	2.560***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Observations	2,079,662	2,079,662	2,079,662	2,079,662	2,079,662
Ajusted R-squared	0.427	0.458	0.495	0.478	0.492

Control variables include: student and school controls such as age, gender, parental education, household socieconomic class, student per computer ratio, student teacher ratio, and dummy variables for the year of the test. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%.

3.6 Final remarks

This paper examined the impact of the FDS program on academic performance in Colombia. Using a staggered difference-in-differences approach with SABER11 data, I find a positive and larger effect on test scores for schools receiving earlier treatment than for groups receiving later treatment. However, these aggregate estimates appear to mask the heterogeneity in achievement effects across school characteristics. In particular, the larger positive and significant effects on test scores are reported by rural and technical schools, especially those that implemented FDS in 2017 and 2019. In addition, the effect of FDS reform implementation appears to be higher for standardized test scores in math and English than for science, reading, and social studies, and it becomes larger the longer schools are exposed to FDS reform, at least in the first three years after program implementation.

I also find evidence of a positive effect of the FDS program on test scores using individual-level data, which indicates that the longer students are exposed to the FDS program (two, three, and four years), the greater is the policy effect. For math test scores, the effect is significant and increases with the number of years of exposure. In contrast, the FDS program increases test scores in English in the first two years of exposure. In science, reading, and social studies, the effects are stronger in the second year of FDS program implementation. Regarding the heterogeneous effects by parental education, relative to students whose parents have no education, the effect of the FDS appears to be stronger when the father completed primary, secondary, or technical education and it increases monotonically with mother's education attainments. Finally, the impact of the policy appears to be higher for students belonging to middle-income and (especially) low-income families.

My findings have important policy implications not only for Colombia but also for other Latin American countries where much remains to be done to improve the quality of education. It may be worth considering the introduction of the full-day school program in all public schools in the country, as Colombia allocates significant resources to primary and secondary education compared to the region and to countries with similar income levels and is among the countries with the lowest scores on the PISA test. Expanding the FDS across the country could also help to reduce the gap between rural and urban schools. Large-scale implementation of the FDS program, if targeted, could help improve educational quality and reduce inequalities in human capital formation across the country. However, the targeting mechanism should ensure that program implementation focuses on schools serving disadvantaged populations and on the quality of instruction. In addition, as described in the literature review, this type of policy has long-term benefits for children by increasing the likelihood of timely middle school graduation, high school admissions test scores, and preference for high-quality high schools. In addition, it can simultaneously increase labor market participation of mothers with young children and prevent child labor.

Chapter 4

Exploring the Association of Information and Communication Technology on Student Performance in Compulsory Secondary Education in Colombia

4.1 Introduction

In economically advanced and underdeveloped societies alike, most schools have already integrated Information and Communication Technology (ICT) into teaching to some extent. Digital technology is utilized on a larger scale, and students are acquainted with ICT from an early age. ICT applications have gradually become an integral part of quality teaching (Luo et al., 2021). However, due to differences in economic and technological levels, there exists a significant gap between developing and developed countries regarding ICT in education. On average, across OECD countries in 2022, there were approximately 0.8 computers available at school for educational purposes for every 15-year-old student. In Latin America, the computer-student ratio is only one-fifth that of developed countries (OECD, 2020).

As a developing country, Colombia faces significant challenges in terms of technological infrastructure. According to the latest report from the Program for International Student Assessment (PISA), the average computer-student ratio exceeds 0.5 (OECD, 2020). Furthermore, as of 2021, only 37.9% of households nationwide own a desktop, laptop, or tablet computer. Interestingly, a higher percentage of households in the capital cities, totaling

46.3%, had access to such devices. In contrast, densely populated urban areas and dispersed rural regions had notably lower ownership rates, standing at just 9.7%. However, in recent years, with the adoption of several initiatives, the situation has shown signs of improvement.

The role of ICT in classrooms and its impact on students' performance have been the focus of extensive literature over the last decade. This body of literature shows mixed results. Some studies found potential negative effects of the ICT on students' academic performance such as lack of active participation in extracurricular activities and decreasing classroom attention (Livingstone, 2015), while others point out a positive impact of ICTs on student achievement. The heterogeneity in the results can be attributed to several factors. For example, the impact of ICT on students' learning largely hinges on factors such as access to it, the surrounding environment in which it is utilized, and the specific purposes for its use. Disparities among children in ICT utilization may stem from variations in access to ICT devices and hardware, often influenced by socio-economic status. Additionally, differences in ICT skills and parental attitudes toward ICT can also contribute to these inequalities (Borgonovi & Pokropek, 2021) The study conducted by (Spiezia, 2011) analyses the effect of computer uses on science test scores using data from the 2006 Program for International Student Assessment (PISA) and found that higher frequency of computer use was associated with better PISA test scores in science in all countries, with a stronger effect observed for those who used computers extensively at home than for those who used them mainly at school. The results show a positive correlation between the frequency of computer use and performance in science in all countries studied. Students who reported using computers "once or twice a week" scored on an average of 40 points higher in science than those who used the computer less frequently. Students using the computer "almost every day" scored between 51 and 70 points higher in science than their peers who used computer "once or twice a week." In the same line, (Zhao & Chen, 2023) investigated the effects of the Three Links Project (TLP) in Chinese rural schools and their results indicate that the TLP leads to significant improvements in student performance, both in cognitive and non-cognitive skills, with increases of 0.07, 0.09, and 0.03 standard deviations, respectively. In addition, they observed positive effects of TLP implementation, including greater teacher engagement and enthusiasm, increased parental involvement in education, and positive student responses. (Naik et al., 2020) led a randomized controlled trial involving over 1800 Indian schools, focusing on the use of information technology in the classroom. Their results consistently showed a significant positive relationship between ICT resources in the classroom and students´ academic performance. Remarkably, this relationship persisted even when the number of computers per student was lower, and teachers were less adept at utilizing these resources.

However, some scholars believe that increasing ICT resources in education could be ineffective and even have a negative impact on students´ academic performance. (Gonzalez Vidal, 2021) discovered that ICT use for remote learning often serves purposes other than learning, leading to a detrimental impact on students' achievement. (Wang & Wang, 2023) investigated the relationship between educational ICT resources, student engagement and academic performance. Their results indicate a significant negative relationship between educational ICT resources and students´ academic performance due to inadequate software resources and insufficient teacher professionalization. They also emphasized that while ICT resources in education can effectively promote student engagement online, there is a risk of potential dependency. This could divert students' attention away from the class, ultimately leading to reduced understanding of the material. (Carter et al., 2017) suggest that computer devices may diminish students' assimilation of teaching material acquired throughout the semester. Their findings indicate that allowing computers or laptops in a classroom can lead to a decrease in exam scores by 0.18 standard deviations. This decline is attributed to the multitude of activities students participate in while using computers, including surfing the internet, checking emails, and messaging friends. These distractions can divert their focus from the lesson. (Fernández-Gutiérrez et al., 2020) analyze the impact ICT use in schools on students´ results in compulsory secondary education, focusing on mathematics, reading and science, using data form PISA in different Spanish regions (Autonomous Communities). The results show that while an increase in the use in an Autonomous Community does not have a positive effect on educational outcomes in math and reading, it does not have a negative effect either. However, there is an observed positive association between enhanced ICT use and a notable increase of 6.87 points in PISA scores in science. Finally, (Hall & Lundin, 2024) investigate how such 1:1 program[1] affects school performance in lower secondary school in 26 Swedish municipalities and they found no indication that this type of programs would enhance student performance in either language or mathematics neither in educational outcomes.

In the case of Colombia, there is limited research investigating the impact of Information and Communication Technology (ICT) on student outcomes. A study by (Saldarriaga, 2016) delved into this relationship, focusing on access to ICT and its influence on academic performance in language, mathematics, and English among students taking the SABER 11 exam. Their findings suggest that owning a home computer is associated, on average, with higher performance in language and mathematics but lower scores in English. Another study by (Loza Arenas et al., 2017) examined the effects of digital classrooms on academic achievement among high school students in a public educational institution in Colombia. While the program led to improvements in aspects such as motivation, attention, and participation compared to traditional teaching methods, there was no significant enhancement in test scores when compared to non-participating students.

[1]An educational program where schools provide each student with their own personal learning device.

(Barrios-Fernández & Bovini, 2021) evaluated the impact of home computers and internet access on Saber 11 test results and found a positive effect in Colombia. Specifically, they observed the highest positive impact in English scores. While there was evidence of a positive correlation in mathematics and critical reading, significance was only noted in the latter subject. Additionally, there was a slight decrease of approximately 1 point in scores for social and natural sciences.

My research diverges from previous literature by investigating the conditional relationship between ICT usage both at home and in school settings. I also explore heterogenous effects based on the characteristics of students and their families and employ unconditional quantile regression to examine how ICT usage at home and school differs across the entire distribution of test scores, including high- and low-performing students. Moreover, I integrate data from two distinct sources covering the period from 2015 to 2019: administrative data from the Colombian Institute for the Evaluation of Education (ICFES), specifically the SABER 11 dataset, and the annual school census (C600) conducted by the National Statistics Institute (DANE). The latter dataset provides insights into school infrastructure and facilitates the inclusion of ICT access as an independent variable, encompassing not only household levels but also institutional levels.

This study contributes to the existing literature by indicating a positive conditional relationship between access to Information and Communication Technology (ICT) at home and improved performance on standardized tests both at home and at school. Conversely, a negative relationship was observed when students only had access to ICT at school. This phenomenon has been observed across various global contexts but has not been extensively documented in Colombia. It suggests that ICT access alone does not guarantee academic improvement, highlighting the importance of accompanying it with teacher training. Gender disparities in ICT usage appear significant in influencing academic success, with male students often deriving greater benefits from ICT utilization, particularly in home environments. Furthermore, parental education plays a crucial role in students' ICT skills; higher levels of parental education are associated with better guidance and supervision of children's electronic device usage, which in turn enhances academic achievement. Moreover, findings from unconditional quantile regression (UQR) analysis show significant effects among students in the lower percentiles (10th and 25th percentiles), particularly in subjects such as mathematics, social science, and reading, where access to ICT at home and at school has pronounced effects. Notably, the impact on English remains significant even at higher percentiles. However, potential endogeneity concerns arise because ICT usage may reflect unobserved characteristics of schools, students, or families. Consequently, the findings presented in this paper indicate a conditional association between standardized test scores and ICT access, rather than evidence of causality.

The remainder of the paper is organized as follows: Section 2 and Section 3 described data and empirical strategy respectively, while Section 4 presents the results and section 5 provides the concluding remarks.

4.2 Data

The study utilized data from two primary sources: administrative records from the Colombian Institute for the Evaluation of Education (ICFES), specifically the SABER 11 dataset, and the nationwide school census (C-600) conducted by the National Department of Statistics (DANE) between 2015 and 2019. The SABER 11 exam serves as a high school exit exam, encompassing test scores for all students nearing completion of upper secondary school. It consists of 278 multiple-choice, single-answer questions, with 254 focusing on language, mathematics, critical reading, social sciences, and natural sciences. The remaining 24 questions pertain to a socio-economic questionnaire designed for research purposes, capturing data such as year of birth, age, gender, highest parental education level, family socio-economic status, and home ICT access. Test scores were standardized based on the mean and standard deviation of each test edition.

These datasets are complemented by the annual School Census (C-600), which compiles school-level information including school type (public or private), location (urban or rural), total student enrollment, total number of teachers, and ICT access. One limitation of the data used in this study is the absence of detailed information on the type and intensity of ICT usage. Therefore, in this paper it is not possible to disentangle the effect of the availability of ICT between its type and intensity of use, meaning that all results need to be interpreted with this data restriction in mind.

My study includes a data set of 2,019,874 students who took exams between 2015 and 2019. Students were categorized according to their access to information and communication technology (ICT), distinguishing whether access was exclusively at school, exclusively at home, or both. Table 4.1 shows the main descriptive statistics of all tested students. Approximately 60% of students had access to ICT both at home and at school, while 39% had access only at school. Less than 1% had either no ICT access at home or none at all (see Table 4.1).

Analysis by year of testing showed a predominant trend of students having access to ICT both at home and at school, with less than 1.5% having no or limited ICT access at home. The data shows significant differences between students based on their ICT access at school or at home. Pupils with ICT access both at school and at home scored on average 25 points higher than those without access, with subject-specific scores around 2 points higher than the mean. In addition, females generally have less access to ICT than their male peers, and the average age of the students tested is 17.

Moreover, 4.9% of students came from households whose parents had no formal education. The highest educational level of parents is secondary education, and of the students who have access to ICT at home and at school, 22% of parents have professional or higher education.

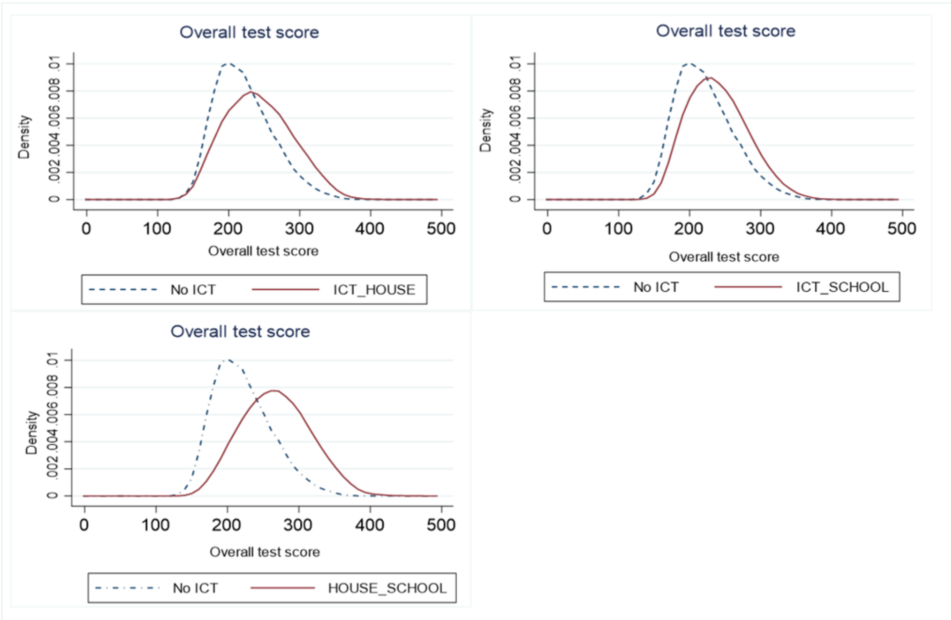
Furthermore, 74.7% of the students tested belong to a middle-income household; this percentage is even higher when the sample is split, and 87% of students without ICT have a lower socioeconomic status. In contrast, less than 3% of students, regardless of their ICT access, came from higher socioeconomic backgrounds. This is due to the fact that I only included public schools in my sample, as 80% of school-age students in Colombia attend public school. Finally, the ratio of students to teachers is 24, which is below the OECD average of 15.

Figure 4.1 illustrates the distribution of overall test scores for students with and without access to ICT using Kernel Density plots. There is an overlap, particularly at the lower end of the distribution. For students without ICT access, the score distribution shifts to the left, indicating a widening gap as scores increase. This emphasizes the importance of estimating the gap not only at the mean but across the entire distribution. Similar patterns are observed in the distributions of test scores in math, reading, English, and social sciences (see document, FigureA9 to FigureA12).

Table 4.1: Descriptives statistics (2015-2019)

	All	**No ICT**	**ICT at home**	**ICT at school**	**ICT at home and school**
No ICT	0,5				
ICT at home	0,4				
ICT at school	39,9				
ICT at home and school	60,3				
Year 2015	21,9	0,4	0,10	37,3	61,2
Year 2016	21,8	0,17	0,06	40,1	59,7
Year 2017	19,1	0,32	0,27	38,3	61,2
Year 2018	19,0	1,17	1,06	38,5	59,3
Year 2019	18,3	0,61	0,43	40,3	58,6
Test score					
Overall	256,5	221,4	242,2	239,9	246,5
Math	51,4	43,9	48,3	47,8	53,8
English	51,1	43,7	48,5	46,8	53,9
Natural Science	51,3	44,3	48,0	48,3	53,3
Reading	52,7	46,7	51,0	49,8	54,7
Social Science	49,9	42,4	46,5	46,4	52,2
Student gender					
Male	54,2	52,4	50,8	56,0	53,3
Female	45,8	47,6	49,2	44,0	46,7
Age	17	18	17	17	17
Mother and Father education					
No education	4,9	10,5	3,7	7,9	2,8
Primary	39,9	57,0	35,7	54,8	29,5
Secondary	39,9	28,3	44,2	32,5	45,4
Professional or more	15,3	4,1	16,4	4,9	22,3
Household socio economic stratum					
Lower income	74,7	87,8	68,6	60,0	64,4
Middle income	23,4	10,6	29,4	9,2	33,0
Higher income	1,9	1,6	2,0	0,8	2,6
Students teacher ratio	24,38	22,46	22,43	24,33	24,43
Number of observations	2.019.874	10.352	7.424	785.838	1.216.260

Figure 4.1: Kernel distribution overall test



4.3 Empirical Strategy

A first approach to estimating the differences in the SABER11 test between students with and without ICT access is an econometric specification with individual-level data, which also allows for the analysis of heterogeneous effects depending on student and family characteristics.

The model to be estimated using OLS is described by the following equation:

$$\mathrm{Y}_{ist=}\alpha+\sum_j \beta_j \mathrm{ICT}_{ist}+\delta X_{ist}+\gamma Z_{st}+\pi_t+\theta_s++\epsilon_{ist} \tag{4.1}$$

where Y_{ist} is the test score(overall, math, English, reading and social science) of students i enrolled in school s at the time t. The ICT_{ist} is a categorical variable indicating whether students have access to ICT at school (j = 1), at home (j = 2), or in both (j = 3), being students without ICT neither at school or at home the reference category (j = 0). The vector X_{ist} is a set of student-level controls such as age, gender, parental education, and household socioeconomic class, Z_{st} is a vector of time-varying school characteristics (student teacher ratio) π_t and θ_s are time and school fixed effects and ϵ_{ist} is the error term. Equation 4.1 was calculated using fixed-effect estimator to remove effects of time-invariant unobserved school characteristics that can influence Saber 11 test score.

In all analyses, standard errors are clustered at the school level to account for correlation between students who attend the same school. I also adopt a 'stepwise inclusion of control variables' approach, starting with age and gender, followed by parental education, socioeconomic status, and finally school characteristic.

Additionally, I interact the ICT dummies with individual and family characteristics to further explore their combined effects.

Furthermore, the estimation of 4.1 provides homogenous or mean effect of ICT use. Therefore, if ICT (at home and/or at school) exerts any effect on students' academic performance at other points of the test score distribution other than the mean, these sources of heterogeneity would be neglected by OLS estimates. To explore the association between ICT use and the test score distribution, I employ Unconditional Quantile Regression (UQR). I choose this methodology over Conditional Quantile Regression because UQR offers a more comprehensive understanding of the distribution of the response variable across its entire range, independent of predictor values. Furthermore, it provides a broader perspective on how changes in predictor variables affect different quantiles of the response variable across the entire population, rather than focusing solely on specific conditional distribution. Thus, UQR provides a more generalized insight into how predictors influence different segments of the distribution.

Specifically, the UQR technique relies on a transformation known as the recentered influence function (RIF) (Alejo et al., 2021).

$$RIF\ (Y;\ q_\tau) = \ q_\tau + \frac{\tau - \Lambda\{Y \leq \ q_\tau\}}{f_y\ (q_\tau)}$$

Where $f_y(.)$ is the marginal density function of Y where is an indicator function. In practice, RIF (Y; q_t) is not observed, hence its sample counterpart is used instead:

$$RIF\ (Y;\ q_\tau) = \ \hat{q}_\tau + \frac{\tau - \Lambda\{Y \leq \ \hat{q}_\tau\}}{\hat{f}_y\ (q_\tau)}$$

Where q_t is the sample quantile and f_y (q_t) is the kernel density estimator, with this transformed variable being used in place of the original dependent variable.

The UQR provides a way to estimate the marginal impact of an additional unit input on SABER11 test performance at a given score percentile of the unconditional distribution. Therefore, it is possible to analyze whether ICT exerts differential influences on academic achievements for low, medium and high performing students. The estimation is carried out for a range of achievement distributions from the 10th to the 90th percentile. The usual inference procedure of OLS is also applicable to the UQR estimates, and I used the cluster-adjusted standard deviation for statistical inference.

This specification is subject to various methodological limitations. Firstly, there may be endogeneity problems, as certain unobservable individual and family variables that influence learning outcomes (e.g., cognitive skills or parents' knowledge and attitudes towards ICT) may also affect the use of information and communication technology (ICT). Secondly, the measurement of ICT use provides an incomplete representation of actual use in educational institutions; however, the school fixed effects capture the effects of time-varying variables, though there could be time-varying heterogeneity at the school level. Finally, there is criticism regarding external validity, as it is challenging to generalize the study results to broader contexts.

4.4 Result

4.4.1 OLS results

In this section, I present results from an analysis of individual-level data. The dependent variable under examination is standardized test scores, with detailed findings provided in Table 4.2 . All specifications include school and year fixed effects, as well as cluster standard errors at the school level. Model 1 depicts the outcomes of a specification containing only a basic set of child characteristics—namely, gender and age. Model 2 introduces parental education (the highest level between the two parents), and Model 3 includes indicators for family socioeconomic status. Finally, Model 4 controls for time-varying school characteristic. Notably, the coefficient for ICT access is significantly positive across all columns, at the 1% and 5% levels of confidence. Students who have ICT access only at home obtain scores 0.06 standard deviation higher, however, this coefficient decreases when controlling for parental education. One possible explanation is that educated parents are better equipped to support their children in various ICT tasks by providing guidance, troubleshooting technical issues, and reinforcing learning objectives.(Drugova et al., 2021) emphasize the importance of parental involvement in helping students develop higher -order ICT skills such as critical thinking and problem-solving, which are crucial for academic success in a digital age. The coefficients for ICT access both at home and school are very similar to those for access only at home. Specifically, referring to the results in Model 4, ICT access increases the average total score in the SABER 11 test by 0.036 standard deviations. These findings align with prior studies confirming the beneficial effects of ICT in education (Bai et al., 2016);(Borgonovi & Pokropek, 2021);(Pagani et al., 2016)

In terms of ICT access at school, there is a significant negative correlation at a 1% confidence level, which remains stable across all columns when control variables are included. This could be attributed to two primary factors: Firstly, the varying socioeconomic ICT backgrounds of students accessing the education system, which may impact their test scores, and this could potentially widen the digital gap, particularly among disadvantaged students (Alderete et al., 2017).

Secondly, the displacement hypothesis suggests that extensive ICT usage for leisure and educational purposes both at home and in school may displace other activities. If the returns on time invested in a particular activity decrease with increased ICT usage, this could result in lower academic achievement (Borgonovi & Pokropek, 2021)

Regarding the control variables,being male increase test score by 0.2 standard deviations and those who are older relative to their classmates have lower performance on the test, which possibly captures grade repeaters. As expected, parental education emerges as a significant predictor of test scores, particularly among students whose parents have attained a professional or higher level of education.

Similarly, family social class demonstrates a notable influence on test performance. Students from middle- and upper-income households score 0.19 and 0.13 standard deviations higher on the test, respectively, compared to their counterparts from lower-class households. However, it seems worth noticing that the coefficients of the highest level of education between the two parents is unaffected by the inclusion of family socioeconomic status as control variable, highlighting that parental education matters for academic performance beyond its impact on socioeconomic conditions. Concerning the time-varying school controls, no significant effects are observed for student-teacher ratio.

As an additional result, I also estimated 4.1 for test scores in each subject. The estimations are shown in Table A9 and Table A10 in the document. The results give no indication that ICT access would enhance student performance in either mathematics or social science, neither at home nor at school. However, ICT availability at home, as well as in both settings (home and school), correlates positively and significantly with scores in English and reading, contrary to the finding of (Saldarriaga, 2016). An increase of one standard deviation in ICT access at home and at school is associated with an increase of 0.07 and 0.04 standard deviations in test scores, respectively. In all test subjects, the finding that ICT access is associated with higher test scores seems to be entirely driven by ICT use at home only.

To further investigate the existence of heterogeneous effects of ICT access, I interacted the variable of interest of student and household characteristics. The results from Table 4.3 to Table 4.6 indicate a positive and significant association between total test scores and ICT access at home, as well as in both home and school settings, as depicted in the first and third rows of each model. ICT appears to be more beneficial for male students than for female students (Models 1) and the effect of ICT is higher for younger students (Model 4).

Models 2 and 3 in Table 4.3, Table 4.4, Table 4.5 and Table 4.6 exhibit heterogeneous results based on parental education and socioeconomic class. Concerning the former, compared to students whose parents have no education, the positive conditional correlation between having ICT at home and academic performance seems stronger when the parents has completed primary, secondary, or technical education, although the differential effect decreases with their level of education.

This might be attributed to a spillover effect on parental involvement in education at home, where parents actively guide and monitor children's use of electronic devices (Zhao & Chen, 2023). Additionally, in contrast to lower-income students, the association between having ICT at home and at school is positive and significant for middle and higher-income families.

Table 4.2: Overall test scores results

	Model 1	**Model 2**	**Model 3**	**Model 4**
No ICT		(referece category)		
ICT at home	0.063***	0.028**	0.033**	0.033**
	(0.013)	(0.013)	(0.013)	(0.013)
ICT at school	-0.033***	-0.034***	-0.035***	-0.035***
	(0.012)	(0.011)	(0.011)	(0.011)
ICT at home and school	0.067***	0.033***	0.036***	0.036***
	(0.012)	(0.011)	(0.011)	(0.011)
Male	0.217***	0.208***	0.209***	0.209***
	(0.002)	(0.002)	(0.002)	(0.002)
Student´s age	-0.180***	-0.169***	-0.169***	-0.169***
	(0.001)	(0.001)	(0.001)	(0.001)
Mother/Father education				
Primary		0.072***	0.069***	0.069***
		(0.003)	(0.003)	(0.003)
Secondary		0.185***	0.185***	0.185***
		(0.003)	(0.003)	(0.003)
Professional or more		0.406***	0.412***	0.412***
		(0.004)	(0.004)	(0.004)
Middle income			0.194***	0.194***
			(0.007)	(0.007)
Higher income			0.136***	0.136***
			(0.006)	(0.006)
Students teacher ratio				0.002
				(0.003)
Constant	3.069***	2.745***	2.559***	2.558***
	(0.023)	(0.022)	(0.023)	(0.023)
Observations	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.419	0.428	0.429	0.429

Notes: All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. Reference category (no education, low income) ***significant at 1%,** significant at 5%, * significant at 10%.

Table 4.3: Heterogeneous effects: students characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**
No ICT		(reference category)		
ICT at home	0.021***	0.064***	0.095***	0.075***
	(0.025)	(0.028)	(0.025)	(0.025)
ICT at school	-0.018	-0.011	-0.016	-0.018
	(0.032)	(0.044)	(0.049)	(0.049)
ICT at home and school	0.019***	0.034***	0.041***	0.045***
	(0.012)	(0.025)	(0.049)	(0.049)
ICT at home*male	0.072***			
	(0.025)			
ICT at school*male	0.091***			
	(0.016)			
ICT at home and school*male	0.118***			
	(0.016)			
Observations	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.429	0.429	0.429	0.429

Control variables include: age, gender, parental education, household socioeconomic class, student teacher ratio and dummy variable for the year of the test. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%.

Table 4.4: Heterogeneous effects: students and family characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**
ICT at home*age				-0.167***
				(0.001)
ICT at school*age				-0.171***
				(0.001)
ICT at home and school*age				-0.167***
				(0.001)
ICT at home*primary		0.098**		
		(0.051)		
ICT at school*primary		0.120		
		(0.020)		
ICT at home and school*primary		0.088		
		(0.063)		
Observations	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.429	0.429	0.429	0.429

Notes: I include student and school controls such as age, gender, parental education, household socioeconomic class, student teacher ratio and dummy variable for the year of the test. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%

Table 4.5: Heterogeneous effects: family characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**
ICT at home*secondary		0.046**		
		(0.023)		
ICT at school*secondary		0.028		
		(0.027)		
ICT at home and school*secondary		0.003		
		(0.042)		
ICT at home *professional or more		0.027**		
		(0.023)		
ICT at school*professional or more		0.006		
		(0.027)		
ICT at home and school*professional or more		0.047		
		(0.042)		
Observations	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.429	0.429	0.429	0.429

Notes: I include student and school controls such as age, gender, parental education, household socioeconomic class, student teacher ratio and dummy variable for the year of the test. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%

Table 4.6: Heterogeneous effects: family characteristics

	Model 1	**Model 2**	**Model 3**	**Model 4**
ICT at home*middle income			0.067	
			(0.077)	
ICT at home*higher income			0.014	
			(0.078)	
ICT at school*middle income			0.046	
			(0.049)	
ICT at school*higher income			0.020	
			(0.052)	
ICT at home and school*middle income			0.227***	
			(0.049)	
ICT at home and school*higher income			0.091***	
Observations	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.429	0.429	0.429	0.429

Notes: I include student and school controls such as age, gender, parental education, household socioeconomic class, student teacher ratio and dummy variable for the year of the test. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%

4.4.2 Results from the Unconditional Quantile Regression (UQR)

This section presents Unconditional Quantile Regression (UQR) estimates. The results indicate significant heterogeneity in the effect of ICT use on global test scores across different quantiles. The largest effects were observed for students at the lower percentiles (10th and 25th). This finding corroborates previous research ((Zhao & Chen, 2023);(Banerjee et al., 2007); (Wang & Wang, 2023)). For example, at the 25th percentile (p25), a one-standard deviation increase in ICT access at home was associated with a 0.02 unit increase in overall test scores, holding all other variables constant. Conversely, the effect was negative when students had ICT access at school, with the smallest effects observed at the highest percentiles. For a student at the 90th percentile (p90), a one-standard deviation increase in ICT access at school was associated with a decrease of 0.015 units in overall test scores.

Furthermore, students with ICT access both at home and at school showed the greatest effects in overall test scores at the lowest percentile (p10), where a one-standard deviation increase ICT access both at home and at school was associated with a 0.04 unit increase. These effects are particularly concentrated among low- and middle-income students. Finally, the impact on test scores appears to diminish at higher percentiles of ICT access.

It is also noteworthy to discuss the estimates of the control variables, which hold independent interest. For instance, on average males achieve higher test scores compared to females by about 22 and 23 standard deviations at the 75 th and 90 th percentiles. This difference could be attributed to socioeconomic status and disparate educational opportunities. Male from wealthier families often have greater access to educational resources such as private tutoring and test preparation courses compared to their female counterparts. Additionally, they tend to attend schools with more resources and have access to a wider array of extracurricular activities, all of which may impact their performance on standardized tests ((Mahoney et al., 2005); (Yang et al., 2013)). Moreover, students who are relatively older than their classmates tend to have lower test scores at the 25 th and 50th percentiles.

Another significant variable for predicting test scores is parental education. Students with parents who have attained professional or higher education levels, particularly those in the 75th and 90th percentiles, tend to achieve higher test scores compared to students whose parents have no education. This underscores the importance of parental involvement in education; higher-income parents may possess greater time, resources, and educational backgrounds to support their children's learning and advocate for their academic success. Furthermore, socio-economic status plays an important role in determining students' achievement, particularly those at the 75th and 90th percentiles. The positive relationship found in the estimates confirms findings from previous studies. For instance, ((Schütz et al., 2008); (Lounkaew, 2013)) have also observed similar trends. A school-level variable that is relevant and significant for students across various percentiles, notably the 10th, 25th, and 50th, is the student-teacher ratio. This metric holds particular importance for students hailing from low socioeconomic backgrounds. A plausible explanation is that these students require greater attention to ensure their academic success.

I also estimate UQR for each subject. The kernel density plots in Figure A9 to Figure A12 highlights the importance of estimating the difference between students with ICT and those without, not only at the mean but also along the distribution. Results are shown in Table A11 to Table A14 in the document and are very similar to the overall test scores, however, the largest effects are observed for math, social science and reading at the lowest percentiles when students have access to ICT at home and school, respectively. For English, the effect is significant and increase at the higher percentiles.

Table 4.7: Results for overall test

	p10	**p25**	**p50**	**p75**	**p90**
No ICT			(reference category)		
ICT at home	0.021***	0.024***	0.013***	0.015***	0.016***
	(0.004)	(0.008)	(0.005)	(0.009)	(0.009)
ICT at school	-0.038***	-0.036***	-0.027***	-0.018***	-0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ICT at home and school	0.047***	0.042***	0.030***	0.031***	0.036***
	(0.037)	(0.031)	(0.025)	(0.026)	(0.032)
Male	0.086***	0.143***	0.200***	0.233***	0.223***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
Student´s age	-0.199***	-0.224***	-0.219***	-0.191***	-0.156***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Mother/Father education					
No Education			(reference category)		
Primary	0.216***	0.174***	0.101***	0.047***	0.009**
	(0.007)	(0.005)	(0.004)	(0.004)	(0.004)
Secondary	0.322***	0.334***	0.300***	0.237***	0.139***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
Professional or more	0.440***	0.548***	0.673***	0.824***	0.908***
	(0.008)	(0.007)	(0.006)	(0.008)	(0.012)
Low income			(reference category)		
Middle income	0.186***	0.167***	0.136***	0.404***	0.713***
	(0.014)	(0.013)	(0.013)	(0.020)	(0.036)
Higher income	0.169***	0.100***	0.218***	0.213***	0.487***
	(0.013)		(0.012)	(0.018)	(0.034)
Students teacher ratio	0.013*	0.014*	0.015*	0.010	0.012
	(0.007)	(0.008)	(0.009)	(0.011)	(0.015)
Constant	1.294***	2.302***	3.171***	3.775***	4.190***
	(0.052)	(0.043)	(0.038)	(0.044)	(0.059)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.110	0.168	0.205	0.205	0.173

Notes: All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses.***significant at 1%,** significant at 5%, * significant at 10%.

4.5 Final remarks

This study provides insights into the complex relationships between Information and Communication Technology (ICT) access and student achievement based on a comprehensive quantitative analysis. The findings highlight the conditional relationship of ICT on students' performance reveal important heterogeneity that should be carefully considered in educational policy and practice.

First, the analysis of ICT access at home shows that there is a positive correlation with standardized test scores, albeit with a diminishing effect when parental education is controlled for. Educated parents play a critical role in fostering their children's ICT skills, which underscores the importance of parental involvement in promoting critical thinking and problem-solving skills that are essential for academic success in the digital age.

Similarly, ICT access in both home and school environments presents a positive effect on test scores, which is according with previous research confirming the role of ICT in improving educational outcomes. However, caution is necessary regarding ICT access in school, as there appears to be a negative correlation. This is likely because using ICT for leisure activities at school might replace other educational activities, thereby diminishing the benefits of time invested and potentially lowering academic achievement. Simply having ICT in schools is not enough it must be accompanied by teacher training, especially in a country like Colombia where the quality of public secondary education and teachers is not optimal.

The study also shows heterogeneous effects based on student characteristics. Male and younger students tend to benefit more from ICT access, especially at home. In addition, When controlling for parental education, the coefficient decreases significantly, highlighting the crucial role of parental involvement in fostering higher-order ICT skills such as critical thinking and problem-solving, which are essential for academic achievement in the digital era. Heterogeneous effects based on student characteristics reveal that male and younger students tend to derive greater benefits from ICT access at both home and school. Moreover, the study finds that when parents have no formal education, the positive conditional relationship between having ICT at home and academic performance appears stronger compared to situations where parents have completed primary, secondary, or technical education.

Additionally, the heterogeneity of the effect of ICT access for high and low performing students is examined through Unconditional Quantile Regression, , showing a more pronounced impact of ICT on lower percentiles compared to higher ones, suggesting varying effectiveness across test distribution. Furthermore, subject-specific analyses highlight differential impacts of ICT: while access to ICT (where, at home and home school) is positively associated with math, social sciences, and reading skills among students in lower percentiles of the unconditional distribution, the effect on English skills is higher at higher percentiles.

Chapter 5

Concluding Remarks

Education is universally recognized as a cornerstone of human development, a fundamental human right, and a powerful driver of social progress. It empowers individuals, fosters economic growth, and promotes societal cohesion. However, in contexts marred by armed conflict, such as Colombia, the attainment of quality education becomes a formidable challenge, impacting generations and perpetuating cycles of violence and poverty.

This thesis has examined critical aspects of human capital formation in Colombia, focusing on the impact of early life exposure to violence and educational policy interventions, as well as the conditional relationship between Information and Communication Technology (ICT) access and academic achievement. By addressing these issues, this thesis contributes to the broader literature on education policy, human capital accumulation and socio-economic development in conflict-affected regions.

The first chapter explored how armed conflict disrupts educational access and attainment, particularly through the lens of forced recruitment into non-state armed groups. The Colombian civil conflict has not only denied countless children and young people access to education but has also turned schools into battlegrounds and educational infrastructure into collateral damage. The prevalence of armed groups in remote areas has exacerbated these challenges, leading to widespread displacement, school closures, and the erosion of educational opportunities for vulnerable populations. It was found that children exposed to forced recruitment during their school years exhibit lower school attendance and higher grade repetition rates. This underscores the severe educational repercussions of armed conflict, highlighting the urgent need for targeted interventions to protect children's rights and mitigate these adverse effects. One limitation of this chapter is the use of an extensive definition of forced recruitment. In future studies, it would be interesting to treat the predictor variable as continuous.

In addition to examining the detrimental effects of armed conflict, this thesis has also explored the potential of educational policies to foster positive educational outcomes. The second chapter of this thesis has critically examined the impact of Colombia's Full Day School (FDS) program on academic performance, utilizing a robust empirical approach with SABER11 data. The findings underscore a significant and positive effect of the FDS program on standardized test scores, particularly in subjects such as math and English. This effect was more pronounced in schools that implemented the FDS earlier, indicating a cumulative benefit with longer exposure to the program.

Moreover, the study revealed important nuances in the program's impact across different school characteristics. Rural and technical schools, which often serve more economically disadvantaged and geographically isolated communities, reported larger and statistically significant improvements in test scores following FDS implementation. This highlights the program's potential to narrow educational disparities between urban and rural schools, thus promoting more equitable access to quality education across Colombia.

Furthermore, the observed enhancements in test scores across different subjects—math, English, science, reading, and social studies—highlighted the multidimensional benefits of the FDS program. The variation in these effects by subject area suggests that the program's impact is context-specific and influenced by curriculum alignment, teacher training, and instructional quality in different academic disciplines.

Further research could offer insights into how changes in the learning environment might affect students' performance. This research would provide a detailed account of how schools have implemented additional instructional hours and how the allocation of these hours across various subjects can vary significantly, potentially influencing students' academic achievements. Additionally, it would be interesting to explore the factors that determine schools' decisions to adopt the Full Day School (FDS) program and to assess the implications of the FDS program on student achievement.

The third chapter of this thesis undertook a comprehensive examination of how ICT (Information and Communication Technology) access influences student achievement in Colombia. The study revealed a nuanced and multifaceted relationship between ICT access and academic performance, highlighting various factors that mediate this association.

Firstly, the study found that ICT access at home exhibited a positive correlation with standardized test scores among students. This suggests that students who have access to ICT resources at home tend to perform better academically, leveraging digital tools for learning and enhancing their educational outcomes. However, the positive impact of ICT access at home diminishes when accounting for parental education levels. Educated parents play a crucial role in supporting their children's digital literacy skills, which are increasingly vital for success in the modern educational landscape dominated by digital technologies. Thus, while ICT access at home can provide significant educational benefits, the level of parental education moderates the extent of these benefits, indicating that parental involvement and support are essential for maximizing the potential of ICT in education.

Conversely, the study found mixed results regarding ICT access in school settings. While ICT access in schools generally showed positive effects on student achievement, there were also indications of potential negative impacts. These negative effects were attributed to leisure-oriented usage of ICT, where students may use digital devices for non-academic activities such as gaming or social media, thereby detracting from their engagement in educational activities. This highlights the importance of thoughtful implementation and management of ICT resources in schools to ensure that they are used effectively to enhance learning outcomes.

Unconditional Quantile Regression (UQR) was employed in this chapter to investigate the varied impacts of ICT (Information and Communication Technology) access on student achievement across different percentiles of academic performance in Colombia. The analysis revealed distinct patterns where ICT access demonstrated significant positive effects, particularly among students in lower percentiles of achievement who typically face greater educational challenges. Moreover, the study highlighted disparities in the effects of ICT access based on socio-economic status, with students from lower-income families benefiting more significantly compared to their peers from higher-income backgrounds. Subject-specific analyses further revealed that while ICT access positively influenced outcomes in subjects like math, social science, and reading for lower percentiles, its impact was more pronounced in English at higher achievement levels.

The econometric specification in this chapter is subject to various methodological limitations. First, there are potential endogeneity issues. Unobservable individual and family characteristics that influence learning outcomes (such as cognitive skills or parents' knowledge and attitudes towards ICT) may also affect the use of information and communication technology (ICT). Second, the measurement of ICT use may not fully capture its actual utilization in educational settings. However, the inclusion of school fixed effects helps to account for time-varying factors, though there may still be time-varying heterogeneity at the school level.

Finally, concerns about external validity arise, making it difficult to generalize the study findings to broader contexts. Consequently, the results presented in this paper suggest a conditional association between standardized test scores and ICT access, rather than establishing causality. Future research could address these limitations by incorporating additional data sources to mitigate endogeneity concerns and to establish causal relationships rather than conditional associations.

Looking ahead, this thesis calls for integrated and evidence-based approaches to address the complex challenges facing education in underdeveloped countries. As Colombia navigates its post-conflict reconstruction phase and continues its pursuit of sustainable peace, addressing educational disparities and promoting inclusive education must remain a priority.

This thesis identifies six key policy implications for Colombia. First, Colombia should prioritize investment in educational infrastructure in remote and vulnerable areas. This includes constructing schools, ensuring adequate provision of resources, and guaranteeing safe and accessible facilities. By enhancing infrastructure, the country can improve educational opportunities for children currently deprived of access.

Secondly, improving teacher training programs is crucial to support inclusive education practices. Teachers require training in diverse teaching methodologies, effective classroom management techniques, and understanding the specific needs of marginalized students. Continuous professional development and support will empower teachers to create supportive learning environments for all children.

Thirdly, Colombia must work towards expanding access to quality education for marginalized populations, such as rural communities, indigenous groups, and internally displaced persons. This involves overcoming barriers such as geographic isolation, language differences, and socio-economic disadvantages. Policies should aim to provide equitable opportunities for all children, irrespective of their background or location.

Fourthly, Colombia should consider implementing full-day school programs, particularly in underserved and vulnerable communities. Full-day schooling offers additional educational opportunities, including extracurricular activities, nutritional support, and access to educational resources beyond regular school hours. This approach not only enhances learning outcomes but also supports working families by providing a safe and enriching environment for children throughout the day.

Fifthly, improving access to Information and Communication Technology (ICT) in public and rural schools is essential to prepare students for the digital age. This includes ensuring reliable internet connectivity, providing access to computers or tablets, and promoting digital literacy among students and teachers. Integrating ICT into education can bridge the digital divide, enhance learning opportunities, and equip students for future employment in a technology-driven world.

Lastly, Colombia should prioritize integrating psychosocial support and trauma-informed education into schools affected by internal civil conflict. Many children and adolescents in conflict zones suffer from trauma, which significantly impacts their learning outcomes and overall well-being. Providing training for educators on trauma-informed approaches and establishing assisting services within schools can help mitigate these effects, improve emotional resilience, and enhance academic achievement. By addressing the psychological and emotional needs of students, Colombia can create a supportive environment that enables all children to thrive academically and personally, despite the challenges posed by conflict.

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Appendix

Table A1: School attendance and grade repetition with an alternative violence measure

	Model 1 **Attending School**	**Model 2** **Attending School**	**Model 3** **Attending School**	**Model 4** **Grade repettion**	**Model 5** **Grade repetition**	**Model 6** **Grade repetition**
0 recruitment cases			(reference category)			
1 to 10 recruitment cases	-0.002	-0.002	-0.002	0.022*	0.021*	0.020*
	(0.06)	(0.06)	(0.06)	(0.009)	(0.009)	(0.009)
11 to 30 recruitment cases	-0.021**	-0.020**	-0.022**	0.045**	0.043**	0.042**
	(0.002)	(0.002)	(0.002)	(0.013)	(0.013)	(0.013)
31 to 50 recruitment cases	-0.023**	-0.025**	-0.026**	0.060**	0.060**	0.064**
	(0.002)	(0.002)	(0.002)	(0.017)	(0.017)	(0.017)
More than 51 recruitment cases	-0.027**	-0.029**	-0.025**	0.052**	0.053**	0.055**
	(0.002)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)
Offensive attacks rate	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Homicide rate	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Massacres rate		-0.000*	-0.000		0.000	-0.000
		(0.000)	(0.000)		(0.000)	(0.000)
Deaths rate			-0.000			0.000
			(0.000)			(0.000)
Constant	0.156	0.146	0.134	-3.379**	-3.374**	-3.336**
	(0.413)	(0.413)	(0.413)	(0.328)	(0.327)	(0.327)
Observations	85,947	85,947	85,947	79,377	79,377	79,377
Adjusted R-squared	0.124	0.124	0.124	0.284	0.284	0.284

Control variables include: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS). Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A2: School attendance and grade repetition by different polynomial degree specification

	Model 1 Attending School	**Model 2 Attending School**	**Model 3 Attending School**	**Model 4 Attending School**	**Model 5 Grade repetition**	**Model 6 Grade repetition**	**Model 7 Grade repetition**	**Model 8 Grade repetition**
10 to 10 recruitment cases	-0.002	-0.002	-0.002	-0.002	0.022*	0.020*	0.020*	0.020*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.009)	(0.009)	(0.009)	(0.009)
11 to 30 recruitment cases	-0.021**	-0.020**	-0.022**	-0.027**	0.045**	0.043**	0.042**	0.040**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.013)	(0.013)	(0.013)	(0.013)
31 to 50 recruitment cases	-0.023**	-0.025**	-0.026**	-0.029**	0.063**	0.060**	0.064**	0.061**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.017)	(0.017)	(0.017)	(0.017)
More than 51 recruitment cases	-0.027**	-0.029**	-0.025**	-0.027**	0.052**	0.053**	0.055**	0.057**
	(0.002)	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)	(0.019)
Child age	-0.009**	0.090**	-0.055**	0.517**	0.207**	0.428**	-0.048	6.434**
	(0.002)	(0.005)	(0.017)	(0.085)	(0.009)	(0.012)	(0.029)	(0.149)
Child age square		-0.004**	0.009**	-0.072**		-0.010**	0.035**	-0.884**
		(0.000)	(0.001)	(0.012)		(0.000)	(0.003)	(0.020)
Child age to the third power			-0.000**	0.004**			-0.001**	0.054**
			(0.000)	(0.001)			(0.000)	(0.001)
Child age to the fourth power				-0.000**				-0.001**
				(0.000)				(0.000)
Constant	1.109*	0.133	0.572	-0.846	-1.226**	-3.397**	-1.949**	-1.927**
	(0.473)	(0.407)	(0.415)	(0.431)	(0.362)	(0.328)	(0.330)	(0.466)
Observations	85,947	85,947	85,947	85,947	79,377	79,377	79,377	79,377
Adjusted R-squared	0.105	0.124	0.125	0.126	0.261	0.284	0.288	0.310

Control variables include: household and municipalities controls such as gender, child age, child birth order, number of siblings, mother years of education, mother age, married, working mothers, salaried mothers, home has electricity, home has television, home has refrigerator, family has a car, log(tax income), log(education transfer), log(GPS) and offensive attacks rate. Two-way clusters standard errors in parentheses. All specification include fixed effects for child year of birth, year of interview and municipalities. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A3: FDS reform aggregated treatment effect estimates

	Math				English			
	ATT	SE	95% confidence bands		ATT	SE	95% confidence bands	
All	0.048	0.011	0.025	0.071	0.047	0.011	0.025	0.068
By group								
2015	0.066	0.180	-0.367	0.501	0.120	0.264	-0.552	0.792
2016	0.212	0.063	0.061	0.364	0.256	0.048	0.134	0.378
2017	0.053	0.015	0.016	0.090	0.045	0.014	0.010	0.080
2018	0.003	0.023	-0.060	0.054	0.010	0.022	-0.065	0.044
2019	0.016	0.018	-0.027	0.059	0.034	0.015	-0.0041	0.071
Observations	52.458				52.458			

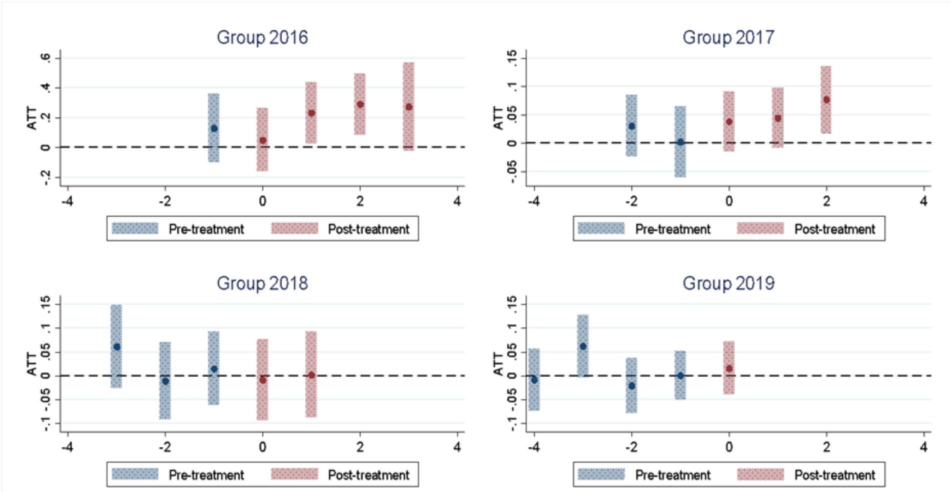
Notes: The table reports aggregated treatment effects parameters under the conditional parallel assumption and with school level clustering. The first row reports the weighted average school-time treatment. The rows by group report the group-specific effects by time of FDS implementation. The estimates use the doubly robust estimator discussed in Callaway and Sant'Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Table A4: FDS reform aggregated treatment effect estimates

	Natural Science				Reading				Social Science			
	ATT	SE	95% confidence bands		ATT	SE	95% confidence bands		ATT	SE	95% confidence bands	
All	0.040	0.011	0.016	0.063	0.035	0.012	0.011	0.059	0.023	0.022	-0.007	0.046
By group												
2015	0.049	0.174	-0.516	0.418	0.141	0.257	-0.502	0.784	0.114	0.281	-0.611	0.840
2016	0.146	0.050	0.013	0.279	0.160	0.068	-0.100	0.330	0.074	0.063	-0.089	0.237
2017	0.050	0.016	0.085	0.092	0.033	0.016	-0.070	0.072	0.029	0.016	-0.011	0.068
2018	0.016	0.028	-0.091	0.059	0.006	0.026	-0.059	0.071	0.016	0.024	-0.077	0.045
2019	0.013	0.018	-0.036	0.061	0.025	0.018	-0.018	0.069	0.005	0.018	-0.041	0.051
Observations	52.458				52.458				52.458			

Notes: The table reports aggregated treatment effects parameters under the conditional parallel assumption and with school level clustering. The first row reports the weighted average school-time treatment. The rows by group report the group-specific effects by time of FDS implementation. The estimates use the doubly robust estimator discussed in Callaway and Sant'Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A1: Math test score group-time average treatment effects



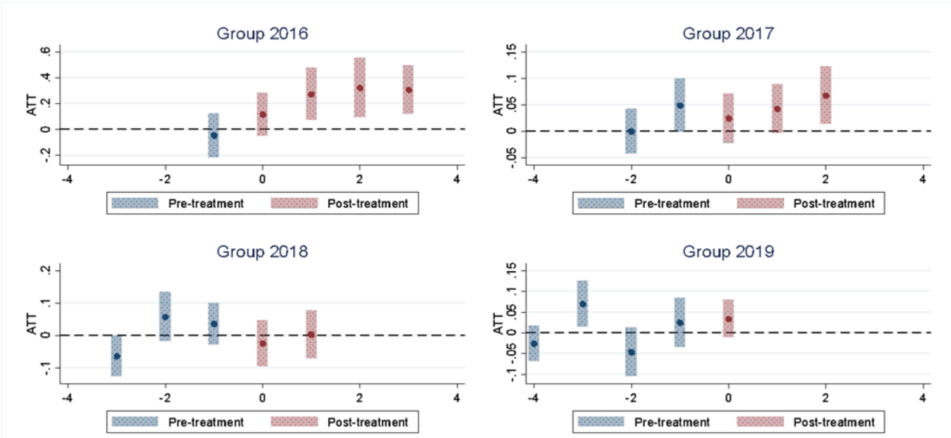
Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95% confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95% confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Table A5: Dynamic estimates of aggregated treatment effects for overall test score

Event Study/Dynamic effects		**SE**	**95% confidence bands**	
e=0	0.02	0.01	-0.006	0.047
e=1	0.03	0.01	-0.003	0.070
e=2	0.07	0.01	0.023	0.117
e=3	0.20	0.08	0.012	0.400
e=4	0.18	0.38	-0.883	1.273
Observations	58.610			

Notes: The "Event Study-Dynamic Effects" row reports the average treatment effects by duration of exposure to the FDS reform. Where e represents the length of exposure. The group-time average treatment effects range from 0.03 to 0.18 standard deviations of higher overall test scores. I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A2: English test score group-time average treatment effects



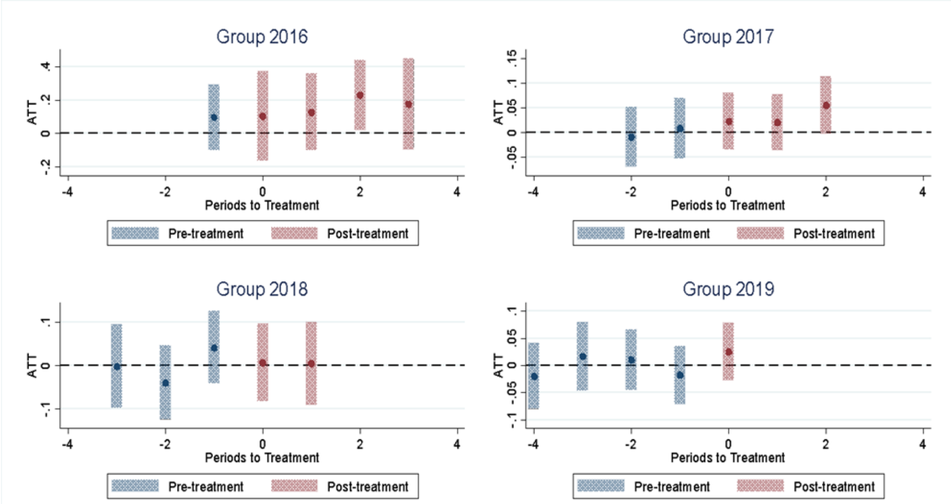
Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Table A6: Dynamic aggregate estimates of treatment effects for groups that completed the FDS for at least two years

Event Study/Dynamic effects		**SE**	**95% confidence bands**	
e=1	0.03	0.01	-0.003	0.073
e=2	0.18	0.05	0.033	0.3239
Observations	28.754			

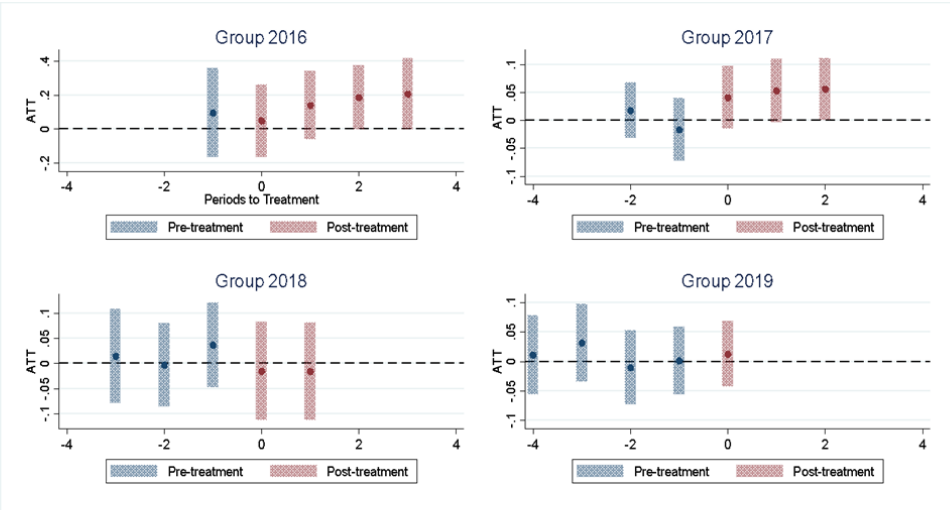
Notes: The "Event Study-Dynamic Effects" row reports the average treatment effects by duration of exposure to the FDS reform. Where e represents the length of exposure. The group-time average treatment effects range from 0.03 to 0.18 standard deviations of higher overall test scores. I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A3: Reading test score group-time average treatment effects



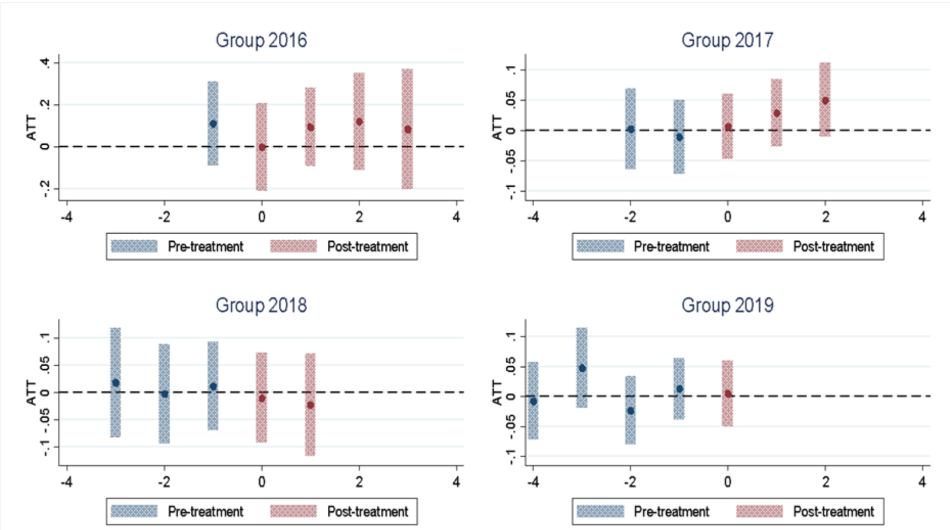
Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A4: Science test score group-time average treatment effects



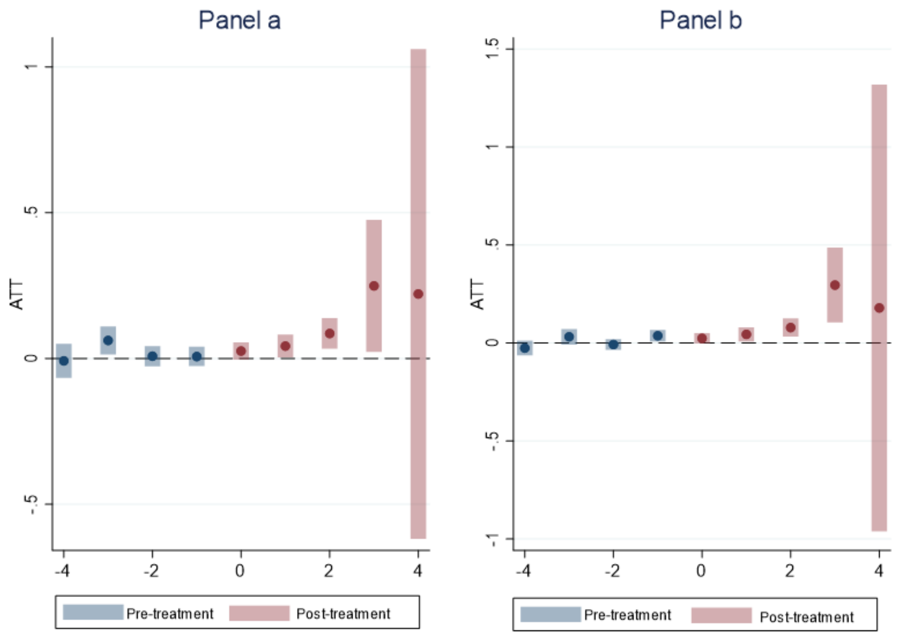
Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A5: Social studies test score group-time average treatment effects



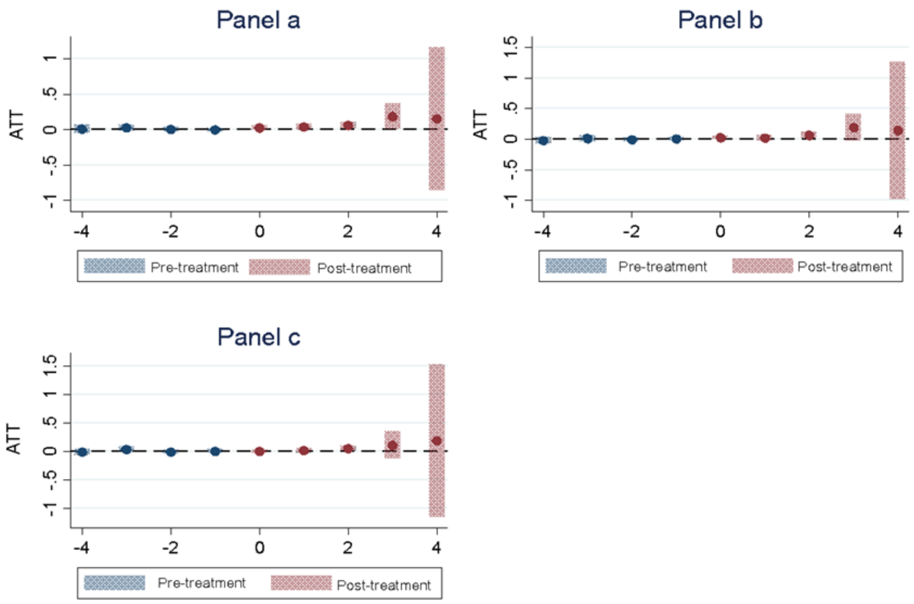
Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A6: Dynamic average treatment effects for Math and English test score



Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A7: Dynamic average treatment effects for science, reading and social studies score



Notes: Figure shows the impact of FDS on standardized test scores estimated assuming conditional parallel-trends and accounting for school-level clusters. Point estimates are on the y-axis (in standard deviation) and are related to grade level (x-axis). The spike lines on the left show the point estimates and associated 95 % confidence bands for the pre-treatment period. The spike line on the right indicates concurrent 95 % confidence bands for the treatment effect of FDS on overall test scores SABER 11. The 2016 group includes schools participating in FDS for the first time this year. The 2017-2019 groups include results for schools that participated in FDS for the first time in 2017 through 2019. The estimate uses the doubly robust estimator discussed in Callaway and Sant' Anna (2021). I include school controls such as type of education (college or technical school), type of school (private or public), location of school (urban or rural), ratio of computers per student, and ratio of students per teacher (as proxies for school facilities).

Figure A8: Event-Study: Impact of FDS program on standarized test score

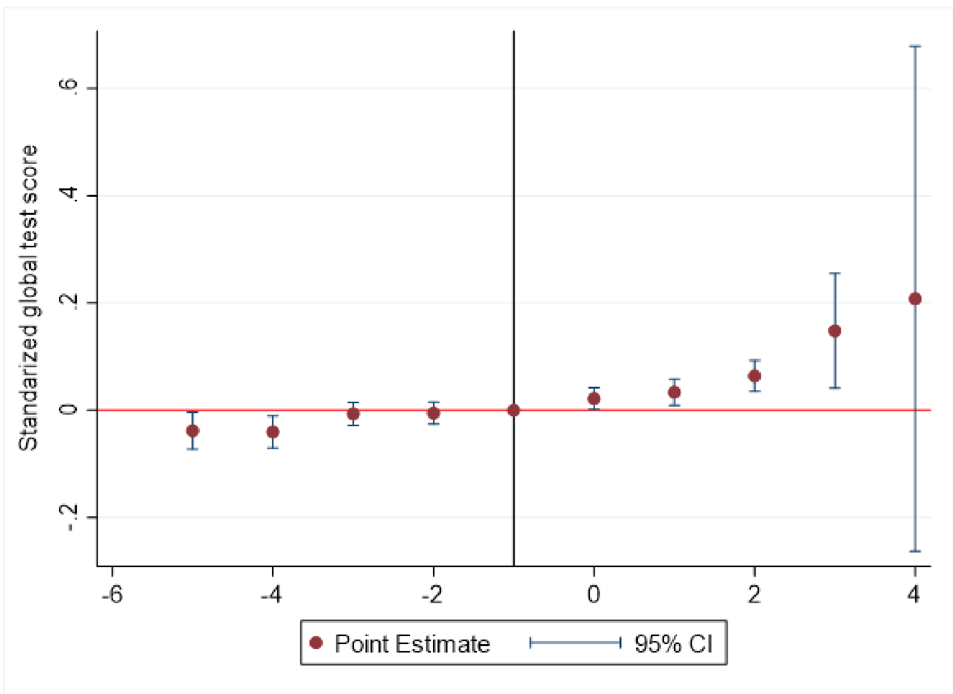


Table A7: Regression results: Math and English test score

	Mathematics test score				English test score			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Years of exposure to FDS	0.022***	0.022***			0.031***	0.028***		
	(0.004)	(0.005)			(0.005)	(0.006)		
0 year of exposure to FDS				(reference category)				
1 year of exposure to FDS			0.020***	0.016**			0.035***	0.035***
			(0.006)	(0.008)			(0.006)	(0.008)
2 years of exposure to FDS			0.046***	0.049***			0.060***	0.054***
			(0.009)	(0.010)			(0.010)	(0.012)
3 years of exposure to FDS			0.056**	0.065*			0.056	0.043
			(0.028)	(0.037)			(0.035)	(0.054)
4 years of exposure to FDS			0.147**	0.185***			0.069	0.076*
			(0.063)	(0.054)			(0.057)	(0.040)
Constant	3.196***	2.596***	3.196***	2.596***	2.394***	1.987***	2.394***	1.985***
	(0.019)	(0.016)	(0.0193)	(0.016)	(0.018)	(0.018)	(0.0179)	(0.018)
Observations	3,032,445	2,079,662	3,032,445	2,079,662	3,032,415	2,005,499	3,032,415	2,079,639
Adjusted R-squared	0.367	0.378	0.367	0.378	0.406	0.432	0.406	0.435

Notes: Control variables include: age, gender, parental education, household socioeconomic class, student per computer and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A8: Regression results: Science, Reading and Social studies test score

	Science				Reading				Social studies			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**	**Model 9**	**Model 10**	**Model 11**	**Model 12**
Years of exposure to FDS	0.015***	0.012**			0.016***	0.017***			0.010**	0.009*		
	(0.004)	(0.006)			(0.004)	(0.004)			(0.004)	(0.005)		
0 year of exposure to FDS							(reference category)					
1 year of exposure to FDS			0.014**	0.010			0.013***	0.012*			0.006	0.006
			(0.006)	(0.009)			(0.005)	(0.007)			(0.005)	(0.008)
2 years of exposure to FDS			0.029***	0.025**			0.036***	0.035***			0.024***	0.020**
			(0.009)	(0.011)			(0.008)	(0.009)			(0.009)	(0.010)
3 years of exposure to FDS			0.052	0.042			0.048**	0.079**			0.039	0.029
			(0.032)	(0.046)			(0.024)	(0.039)			(0.028)	(0.034)
4 years of exposure to FDS			0.109	0.135*			0.028	0.098*			0.046	0.053
			(0.075)	(0.076)			(0.065)	(0.054)			(0.080)	(0.065)
Constant	3.068***	2.414***	3.068***	2.415***	2.813***	2.150***	2.813***	2.150***	2.605***	2.014***	2.605***	2.000***
	(0.020)	(0.017)	(0.0197)	(0.017)	(0.018)	(0.016)	(0.0184)	(0.016)	(0.019)	(0.017)	(0.0191)	(0.016)
Observations	3,032,445	2,079,662	3,032,445	2,079,662	3,032,445	2,079,662	3,032,445	2,079,662	3,032,445	2,005,507	3,032,445	2,079,662
Adjusted R-squared	0.353	0.362	0.353	0.362	0.300	0.311	0.300	0.311	0.297	0.303	0.297	0.309

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Figure A9: Kernel distribution math test

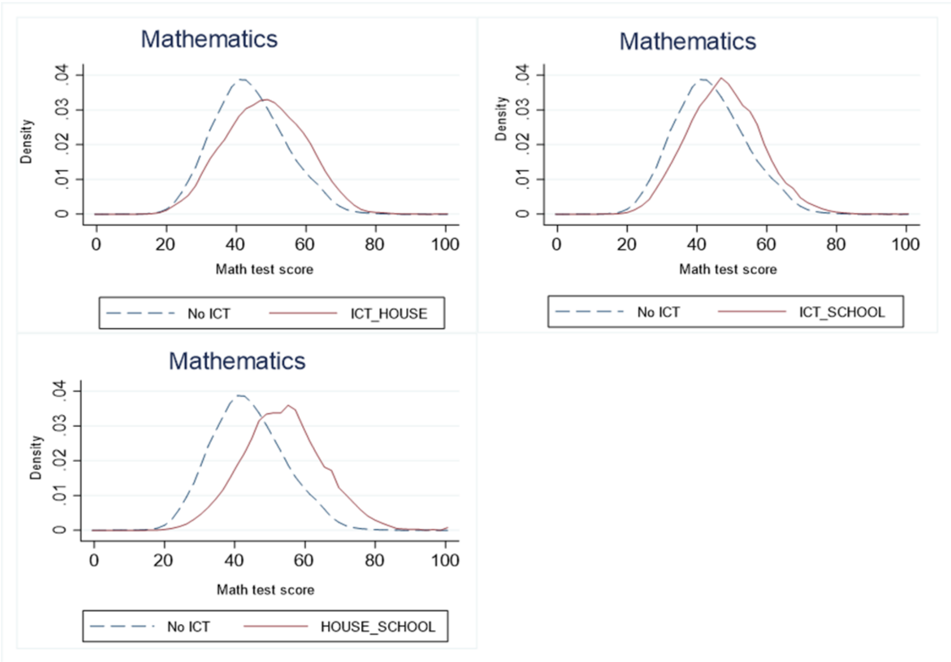


Figure A10: Kernel distribution reading test

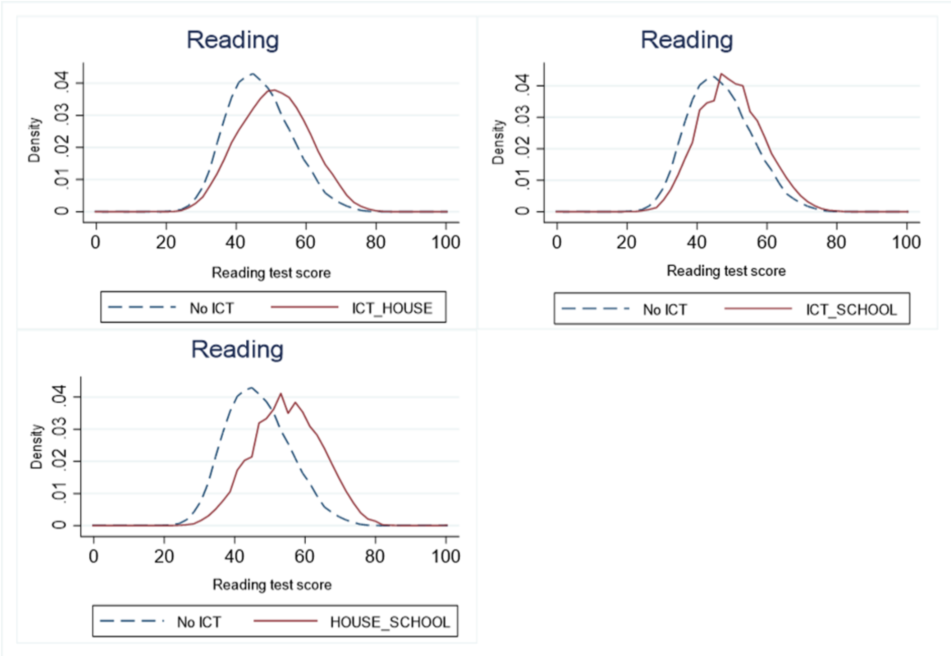


Figure A11: Kernel distribution English test

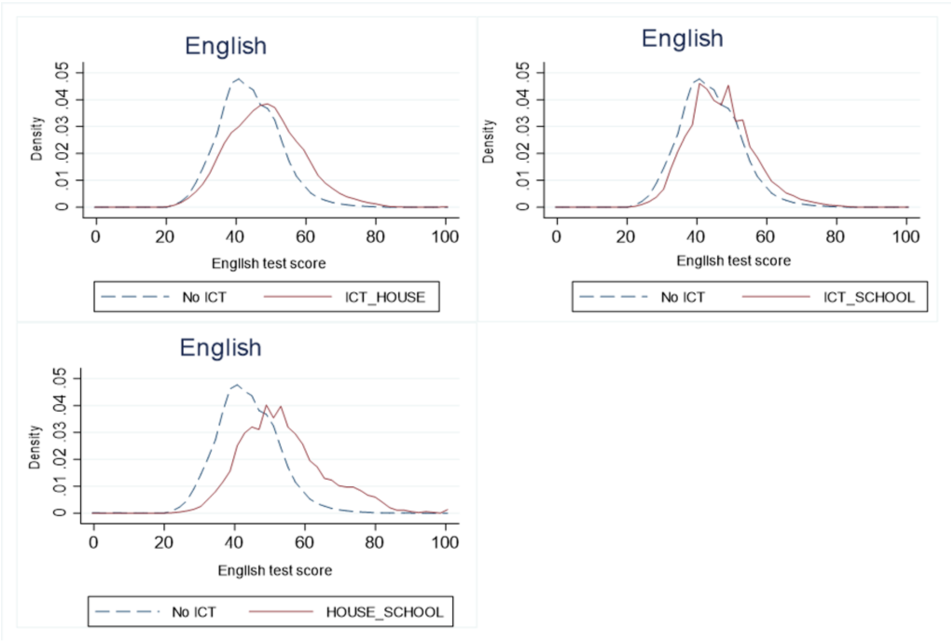


Figure A12: Kernel distribution Social Science test

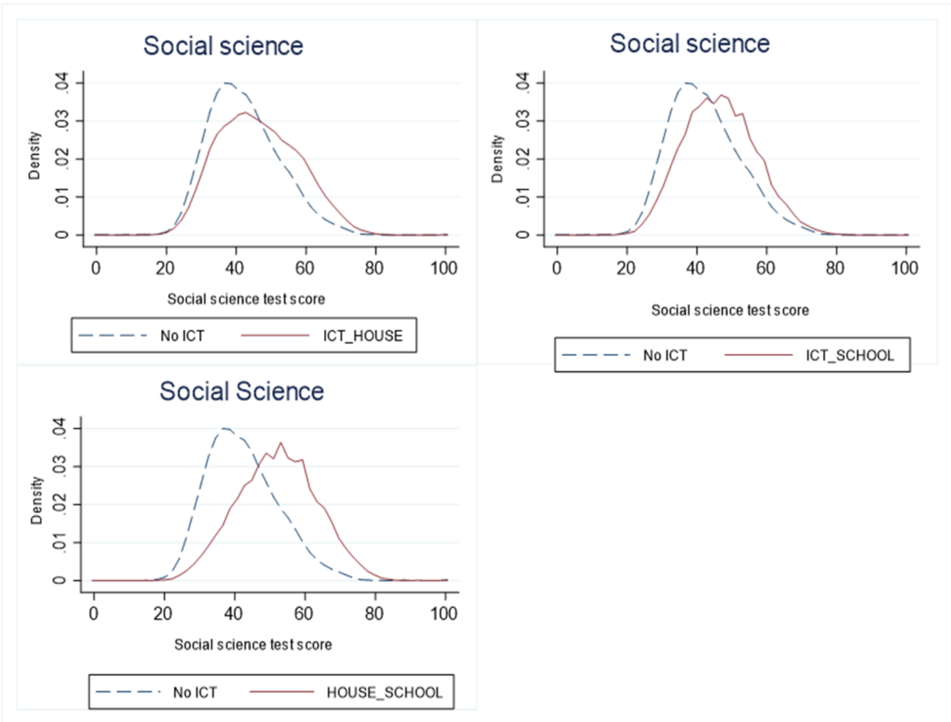


Table A9: Regression results: Math and English test score

	Mathemathics				English			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
No ICT		(reference category)				(reference category)		
ICT at home	-0.030***	0.041***	0.044***	0.044***	0.111***	0.074***	0.072***	0.072***
	(0.029)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
ICT at school	-0.035	-0.019	-0.020	-0.020	-0.019	-0.020	-0.020	-0.020
	(0.023)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
ICT at home and school	0.043***	0.041***	0.044***	0.044***	0.111***	0.074***	0.073***	0.073***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
Constant	3.107***	2.821***	2.664***	2.152***	2.267***	1.949***	1.927***	1.927***
	(0.022)	(0.021)	(0.022)	(0.022)	(0.022)	(0.021)	(0.022)	(0.023)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.379	0.386	0.386	0.379	0.416	0.427	0.427	0.427

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A10: Regression results: Reading and Social studies test score

	Reading				Social Science			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
No ICT		(reference category)				(reference category)		
ICT at home	0.074***	0.041***	0.044***	0.044***	0.056	0.024	0.029	0.029
	(0.014)	(0.014)	(0.014)	(0.014)	(0.025)	(0.025)	(0.025)	(0.025)
ICT at school	-0.018	-0.019	-0.020	-0.020	-0.033	-0.034	-0.035	-0.035
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
ICT at home and school	0.074***	0.041***	0.044***	0.044***	0.058***	0.026**	0.030**	0.030**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Constant	2.624***	2.321***	2.164***	2.162***	2.470***	2.196***	2.034***	2.031***
	(0.022)	(0.021)	(0.022)	(0.022)	(0.023)	(0.022)	(0.023)	(0.023)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.302	0.310	0.311	0.311	0.309	0.317	0.318	0.318

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A11: Regression results for math test

	p10	**p25**	**p50**	**p75**	**p90**
No ICT			(reference category)		
ICT at home	-0.032***	-0.034***	-0.036***	-0.024***	-0.026**
	(0.004)	(0.001)	(0.007)	(0.008)	(0.004)
ICT at school	-0.035***	-0.032***	-0.036***	-0.031***	-0.030***
	(0.005)	(0.003)	(0.007)	(0.006)	(0.002)
ICT at home and school	0.061***	0.051***	0.053***	0.051***	0.056***
	(0.005)	(0.003)	(0.007)	(0.006)	(0.002)
Male	0.205***	0.294***	0.349***	0.359***	0.223***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
Students age	-0.219***	-0.247***	-0.228***	-0.191***	-0.156***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Mother/Father education					
No education			(reference category)		
Primary	0.223***	0.193***	0.117***	0.057***	0.009**
	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)
Secondary	0.333***	0.349***	0.287***	0.208***	0.139***
	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)
Professional or more	0.453***	0.559***	0.621***	0.680***	0.908***
	(0.008)	(0.007)	(0.006)	(0.007)	(0.012)
Low income			(reference category)		
Middle income	0.151***	0.043***	0.131***	0.339***	0.713***
	(0.014)	(0.013)	(0.013)	(0.018)	(0.036)
Higher income	0.140***	0.075***	0.042***	0.208***	0.487***
	(0.013)	(0.012)	(0.012)	(0.017)	(0.034)
Students teacher ratio	0.007	0.012	0.022**	0.017	-0.012
	(0.008)	(0.008)	(0.009)	(0.011)	(0.015)
Constant	1.634***	2.740***	3.371***	3.747***	4.190***
	(0.050)	(0.045)	(0.039)	(0.043)	(0.059)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.093	0.151	0.189	0.184	0.173

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A12: Regression results for English test

	p10	**p25**	**p50**	**p75**	**p90**
No ICT			(reference category)		
ICT at home	0.061***	0.075***	0.076***	0.078***	0.072***
	(0.030)	(0.021)	(0.020)	(0.028)	(0.054)
ICT at school	-0.023***	-0.025***	-0.020***	-0.022***	-0.026***
	(0.009)	(0.002)	(0.002)	(0.006)	(0.009)
ICT at home and school	0.070***	0.071***	0.074***	0.076***	0.073***
	(0.009)	(0.002)	(0.003)	(0.006)	(0.009)
Male	0.029***	0.049***	0.076***	0.101***	0.127***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.007)
Students age	-0.134***	-0.119***	-0.139***	-0.165***	-0.188***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Mother/Father education					
No education			(reference category)		
Primary	0.152***	0.106***	0.077***	0.033***	0.007***
	(0.006)	(0.004)	(0.003)	(0.004)	(0.006)
Secondary	0.258***	0.224***	0.250***	0.258***	0.207***
	(0.006)	(0.004)	(0.004)	(0.005)	(0.007)
Professional or more	0.365***	0.363***	0.525***	0.863***	1.519***
	(0.007)	(0.005)	(0.005)	(0.008)	(0.020)
Lower income			(reference category)		
Middle income	0.057***	0.028***	0.184***	0.616***	1.929***
	(0.010)	(0.008)	(0.009)	(0.018)	(0.059)
Higher income	0.093***	0.040***	0.036***	0.292***	1.313***
	(0.010)	(0.007)	(0.008)	(0.017)	(0.056)
Students teacher ratio	-0.002	0.010**	0.014**	0.022**	-0.021
	(0.007)	(0.005)	(0.006)	(0.011)	(0.024)
Constant	0.558***	0.940***	1.954***	3.389***	5.591***
	(0.039)	(0.027)	(0.029)	(0.044)	(0.092)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.071	0.126	0.193	0.239	0.229

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A13: Regression Social studies test

	p10	p25	p50	p75	p90
No ICT			(reference category)		
ICT at home	0.020***	0.027***	0.029***	0.024***	0.029***
	(0.001)	(0.003)	(0.006)	(0.007)	(0.008)
ICT at school	-0.036***	-0.034***	-0.034***	-0.036***	-0.035***
	(0.003)	(0.009)	(0.004)	(0.003)	(0.004)
ICT at home and school	0.059***	0.050***	0.052***	0.056***	0.060***
	(0.003)	(0.009)	(0.004)	(0.003)	(0.005)
Male	0.007***	0.053***	0.123***	0.177***	0.182***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Students age	-0.146***	-0.187***	-0.184***	-0.155***	-0.112***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Mother/Father education					
No education			(reference category)		
Primary	0.127***	0.128***	0.078***	0.041***	0.002***
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Secondary	0.216***	0.271***	0.253***	0.202***	0.131***
	(0.007)	(0.006)	(0.005)	(0.005)	(0.004)
Professional or more	0.355***	0.515***	0.633***	0.734***	0.716***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.009)
Low income			(reference category)		
Middle income	0.081***	0.055***	0.117***	0.321***	0.500***
	(0.011)	(0.013)	(0.013)	(0.018)	(0.027)
Higher income	0.098***	0.093***	0.020	0.171***	0.342***
	(0.011)	(0.012)	(0.012)	(0.017)	(0.026)
Students teacher ratio	0.026***	0.014*	0.009	0.005	0.012
	(0.006)	(0.008)	(0.008)	(0.009)	(0.011)
Constant	0.724***	1.765***	2.647***	3.178***	3.332***
	(0.043)	(0.041)	(0.036)	(0.038)	(0.044)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.067	0.118	0.157	0.159	0.128

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Cluster standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.

Table A14: Regression reading test

	p10	p25	p50	p75	p90
No ICT			(reference category)		
ICT at home	0.045***	0.047***	0.042***	0.047***	0.049***
	(0.003)	(0.008)	(0.002)	(0.001)	(0.006)
ICT at school	-0.027***	-0.017***	-0.014***	-0.018***	-0.015***
	(0.003)	(0.006)	(0.001)	(0.002)	(0.002)
ICT at home and school	0.064***	0.052***	0.063***	0.061***	0.068***
	(0.003)	(0.006)	(0.001)	(0.002)	(0.002)
Male	0.006**	0.028***	0.043***	0.050***	0.045***
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Students age	-0.178***	-0.188***	-0.166***	-0.143***	-0.119***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Mother/Father education					
No education			(reference category)		
Primary	0.179***	0.143***	0.081***	0.031***	0.005***
	(0.007)	(0.005)	(0.004)	(0.004)	(0.004)
Secondary	0.300***	0.305***	0.258***	0.193***	0.130***
	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)
Professional or more	0.436***	0.519***	0.577***	0.646***	0.679***
	(0.008)	(0.006)	(0.005)	(0.006)	(0.009)
Low income			(reference category)		
Middle income	0.137***	0.035***	0.133***	0.297***	0.455***
	(0.013)	(0.011)	(0.011)	(0.015)	(0.026)
Higher income	0.136***	0.078***	0.026***	0.150***	0.284***
	(0.012)	(0.011)	(0.010)	(0.014)	(0.025)
Students teacher ratio	0.011	0.014*	0.028***	-0.005	-0.017
	(0.007)	(0.007)	(0.007)	(0.008)	(0.010)
Constant	1.038***	1.873***	2.496***	3.023***	3.451***
	(0.047)	(0.038)	(0.032)	(0.034)	(0.043)
Observations	2,019,874	2,019,874	2,019,874	2,019,874	2,019,874
Adjusted R-squared	0.075	0.125	0.158	0.152	0.115

Notes: Control variables include: age, gender, parental education, household socieconomic class and student teacher ratio. All columns control for time and school fixed effects. Clusterd standard errors at school level in parentheses. ***significant at 1%,** significant at 5%, * significant at 10%.