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Do Longer School Days Improve Student Achievement? Evidence from Colombia

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Inter-American Development Bank
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Abstract*

This paper analyzes the impact of longer school days on student achievement in Colombia. To identify the impact of longer schools days, this study exploits plausibly exogenous within school variation in the length of the school day. Using test score data from 5th and 9th graders in 2002, 2005, and 2009, along with school administrative data, this research uses school fixed effects models to estimate variation in average test scores across cohorts for schools that switched from a half school day to a full school day or vice versa. I find that cohorts exposed to full school days have test scores that are about one tenth of a standard deviation higher than cohorts that attended half school days. The impact of attending full school days is larger for math than for language, and it is larger for 9th grade than for 5th grade. Effects are largest among the poorest schools and those in rural areas. The results suggest that lengthening the school day may be an effective policy for increasing student achievement, particularly for the lowest-income students in Colombia and other developing countries.

JEL Classification: I21, I28, H43
Keywords: quality of education, student achievement, instructional time, school day

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1. Introduction

There are large differences in the time students spend in school, both across and within countries. Are these variations in schooling contributing to the low achievement levels and large student achievement gaps that we observe in many countries today? Does increasing the time that students spend in school have an impact on student learning? There is a wide debate among researchers regarding the answer to these questions. And while there is a large literature that provides evidence about the effect of school inputs in the education production function, there is only limited evidence on the effects of time in school on student outcomes, and even less evidence showing that interventions that increase the length of the school day will improve learning outcomes.

Advocates of longer school days argue that these could benefit students in three ways: First, by spending more time in school, students will likely devote more time to learning. Second, they will spend less time alone at home or outside their home doing other activities that are not necessarily related to or beneficial for learning. And third, they will spend less time in the streets, exposing themselves to different risks (Kruger and Berthelon, 2011; Jacob and Lefgren, 2003). Therefore, having a full school day might have a positive impact on student achievement because students might be able to learn more things and improve their skills, while being less exposed to risky behaviors.

On the other hand, critics of extending the length of the school day argue that it could have a negative impact on student achievement if instruction during those extra hours is of poor quality. For example, if less prepared teachers are brought to schools in order to be able to extend the number of school hours, then the additional time spent in school will not necessarily translate into more learning for the students attending full school days. Alternatively, it may be the case that higher-quality schools have greater demand and are therefore "split" into a morning and an afternoon shift in order to meet that higher demand. In this case, the full day schools might end up having lower test scores than the half day schools because they were of relatively lower quality in the first place. Additionally, some critics argue that extending the length of the school day could have no impact on student achievement if the content of the extra hours in the school is irrelevant for math and language learning. For example, additional time devoted to sports, or more playing time may have desirable effects on other skills, such as discipline, team work, persistence, etc., which are valued in the labor market but many not affect math and language test scores.

Policies aimed at increasing the length of the school day have been implemented in several countries, particularly in Latin America. Arguing that extending the length of the school day might increase the time students devote to learning, and in that way could have a positive impact on student outcomes, some countries, such as Chile and Uruguay, have already implemented full school day programs in most of their public schools. Colombia implemented a full school day reform in 1994, but it was not enforced and was later rescinded, mainly because of the increasing demand for school slots that required having two half-day shifts in some schools. A new plan to extend the school day in public schools was adopted by the government in 2014.[1] Research on the impact of these reforms generally supports the idea that increasing the length of the school day has a positive impact on student achievement (Holland, Alfaro and Evans, 2015; Patall, Cooper and Batts Allen, 2010).[2] However, there are methodological difficulties in isolating the impact of instructional time that raise questions about the strength of much of this evidence.

This study contributes to the literature by analyzing the causal link between longer school days and student achievement in Colombia, using an identification strategy that exploits plausibly exogenous within school variation in the length of the school day, which allows mitigating some of the most critical selection and endogeneity problems found in the literature. As many countries and cities continue to experiment with programs and policies to extend the length of the school day, this study also aims to contribute to the debate about the effectiveness of increasing the length of the school day.

After the government of Colombia rescinded the 1994 full school day reform, municipalities were given the flexibility to choose the length of the school day for their schools. As a result, some schools have a full school day (usually 7 hours),[3] while others offer half school

[1] In 2014 President Santos announced the implementation of a national full school day program as the central part of his education strategy. This program is known as "jornada unica" or "unique school day". However, this paper focuses on what happened after the 1994 reform, which ended up allowing schools to change the length of the school day, and before the new 2014 reform was announced.
[2] I present a detailed review of the literature in section 2.
[3] In Colombia, the longer school day has been known as "complete day", and under the most recent reforms, as "unique day", given that the aim is to have the same (longer) school days in all public schools.

days (or two separate 4 or 5 hour shifts).[4] Cross-sectional analyses comparing student test scores in schools with full and half school days will likely be biased since schools that have a full school day may have unobservable characteristics that are related to student achievement (e.g., more educated parents, or more motivated principals and teachers). However, in Colombia, thousands of schools switched from half school day to a full school day (and vice versa) between 2002 and 2009. I therefore use this plausibly exogenous variation in the length of the school day to estimate the impact of attending longer school days by comparing the test scores of cohorts of students in the same school that were exposed to full or half school days. This strategy can effectively control for all unobserved time-invariant characteristics of the school that may bias cross-sectional estimates. I draw on data from the 2002, 2005, and 2009 SABER to implement this approach. The SABER is a nationwide standardized test administered every three years in all primary and secondary schools in Colombia. It evaluates 5^{th} and 9^{th} grade students' skills in both mathematics and language.

I find that among schools that changed the length of the school days between 2002 and 2009, the cohorts exposed to full school days have test scores that are about one-tenth of a standard deviation higher than cohorts that attended half school days. The impact of a full school day is larger for math than for language (e.g., 0.138 v. 0.110 of a standard deviation respectively, for 9th graders) and it is larger for 9th grade than for 5th grade (e.g., 0.138 v. 0.082 of a standard deviation respectively, for math test scores). Using different subsamples to estimate the impact of a full school day, I find that the effect of attending a full school day is largest among rural schools and among the poorest schools in the country. Overall, the results suggest that lengthening the school day may be an effective policy for increasing student achievement, particularly for 9^{th} grade students, and for the lowest-income students in Colombia.

The rest of the paper is organized as follows: Section 2 reviews the most relevant literature on time in school. Section 3 describes the background on the full school day in Colombia. Section 4 discusses the empirical strategy. Section 5 describes the data. Section 6 presents results, and Section 7 concludes.

[4] In schools that have a half school day, two different groups of students attend the same school, with one group attending the morning shift, and the other one the afternoon shift.

2. Literature review

i. The length of the school day in developed countries

Most of the literature analyzing the length of the school day has focused on developed countries, and particularly in the United States. A large proportion of these studies have looked at the impact of extending the length of kindergarten. In a meta-analysis of the literature, Cooper et al. (2010) found that attending a full-day kindergarten was positively associated with academic achievement, compared to a half-day kindergarten, but the effect generally seemed to fade-out by third grade. They also found a significant correlation between attending a full day and the child's self-confidence and ability to work and play with others. However, they found that children attending a full-day kindergarten did not have as positive an attitude toward school, and had more behavioral problems. Rathburn (2010) explored the relationships between full-day kindergarten program factors and public school children's gains in reading test scores. She found evidence that children who attended kindergarten programs that devoted a larger portion of the school day to academic instruction (particularly reading instruction) made greater gains in reading over the school year than children who spend less time in such instruction. However, the effects of a full-day kindergarten seem to fade out beyond the kindergarten year, something that might be related to summer learning loss (Redd et al., 2012; Cooper, Nye and Charlton, 1996).

Although there is a large literature on the impact of school time in early education programs, little is known about the impact on K-12 education. This is partly due to the small variation in the length of the school day across schools districts (in the US), or municipalities (in other countries), or to variations in length that are directly related to resources, which makes it difficult to estimate the impact of longer school days or longer school years. Some of the studies that have used identification strategies that allow them to establish a causal impact of school time on student outcomes have shown that spending more time in school could improve academic achievement and could benefits students, families and societies by reducing teen pregnancies (Kruger and Berthelon, 2011) and crime rates (Jacob and Lefgren, 2003).

There are a few papers that have analyzed variations in the length of instructional time across countries. Using data from the 2006 Programme for International Student Assessment

(PISA)[5] for over 50 countries, Lavy (2015) found evidence that instructional time was positively and significantly related to test scores both in developed and developing countries. However, the estimated effect for developing countries was much lower than the effect size in developed countries. Rivkin and Schiman (2015) looked at the relation between instructional time and the 2009 PISA test scores results for 72 countries. They also found that achievement increases with more instructional time, and that the increase varies by the amount of time and classroom environment.

The impact of more instructional time on student outcomes tends to be larger for students from poorer backgrounds or the most at-risk children. For example, Olsen and Zigler (1989) found that in general, extended-day programs in kindergarten seemed to increase standardized test scores in the short term, particularly for disadvantaged, bilingual, or "least-ready" for school children. Cooper et al. (2010) compared evaluations of full-day kindergarten programs in urban versus nonurban settings, and concluded that these programs had a significantly stronger association with higher academic achievement for children attending in an urban setting than those in non-urban communities. They suggested that children attending in urban settings were more likely to come from poorer backgrounds, so this could be taken as indirect evidence of a potentially greater impact of full-day kindergarten for poorer children. Harn, Linan-Thompson, and Roberts (2008) analyzed the impact of intensifying instructional time on early literacy skills for the most at-risk first graders. They concluded that the students receiving more intensive interventions (more instructional time) made significantly more progress across a range of early reading measures.

ii. The full school day reform in Latin America

Growing evidence in Latin America suggests that increasing the length of the school day has a positive impact on student achievement. Some studies have exploited natural experiments given by the implementation of a full school day program in schools that previously had half school days. Valenzuela (2005) did one of the first impact evaluations of the Chilean full school day program. Implemented in 1997, the goal of this program was to increase by 30% the number of

[5] The PISA is an international study jointly developed by the Organization for Economic Cooperation and Development (OECD) countries to evaluate education systems worldwide. Its main component comprehends testing the mathematics, science, and reading skills and knowledge of 15-year-old students in schools in participating countries or economies.

daily hours in public and voucher schools in the country. Taking advantage of the differences in the time in which schools implemented the program, the author implemented a Difference-in-Difference model at the school and county levels. He found that the program had a significant and positive effect on schooling outcomes, but the impact was different depending on socioeconomic characteristics of the treated schools and also among individuals. The results also showed a positive impact of the program at the municipal level.

Bellei (2009) also analyzed the Chilean full school day program, and found that it had positive effects on students' academic achievement in both mathematics and language, and these effects were constant over time (i.e. the impact were very similar for participants who entered the program in 1999-2000 and in 2002). He also suggested that the program had larger positive effects on rural students, students who attended public schools, and students in the upper part of the achievement distribution. Pires and Urzua (2015) analyzed the effect of time spent in school on schooling attainment, cognitive test scores, socio-emotional variables, labor market outcomes, and social behavior in Chile. They concluded that enrollment into a full-day school had positive effects on academic outcomes and cognitive test scores, and reduced adolescent motherhood, but there was no effect on employment or wages at age 25-26. Cerdan-Infantes and Vermeersch (2007) analyzed the impact of a program that lengthened the school day from a half day to a full day in poor urban schools in Uruguay. They found that students in very disadvantaged schools improved test scores by 0.07 of a standard deviation in math, and 0.04 of a standard deviation in language.

Llach, Adrogue, and Gigaglia (2009) analyzed the long-term impact of attending longer school days in Buenos Aires, Argentina. They found that students that attended full-day primary schools had a secondary school graduation rate 21 percent higher than those that attended half-day primary schools, and this was mostly driven by students coming from low socioeconomic backgrounds. However, they did not find enduring effects on income and employment. This research also found evidence of heterogeneous effects for different types of students, caused by variations across schools in the quality of instruction and of the content taught during the extra hours. For example, they suggest that the content of the additional hours was probably more important for academic achievement than increasing the number of hours. Their analysis suggests that the impact on academic results could be very different depending on whether the extra hours are just an extension of the current curriculum, or whether they allow the disadvantaged students to develop their skills and abilities through instruction in the areas in which their more advantaged schoolmates usually learn and practice.

There are only two studies that have previously evaluated the impact of longer schools days on student outcomes in Colombia. In the first one, Bonilla-Mejia (2011) evaluated the impact of attending a full school day on student performance on a high school exit exam (SABER 11, previously known as the "ICFES test"). He used the number of students enrolled in full school days to instrument the probability that a student has of attending a full school day. He found that students attending full school days have average test scores that are 2.5 percentage points larger than those attending half school days. Additionally, he found that the impact was larger when compared only to students attending an afternoon shift. In the second study, Garcia, Fernandez, and Weiss (2013) used family fixed effects to analyze the impact of changing from a half-day to a full school day on student outcomes. They found that a full school day reduces the probability of early dropout and grade repetition.

3. School day in Colombia

In Colombia, there is a long standing debate on what is the appropriate length of the school day. Policy makers and parents have usually advocated for increasing the length of the school day. For policy makers, this is a politically popular policy, arguing that implementing this type of reform can reduce children's exposure to common risky behaviors when they are not in school (e.g., street crime), and can increase female labor supply. Parents usually also favor a longer school day reform because they think that having their children attend school for more hours would allow them to work for longer periods of time, and will decrease the time that their children spend without any adult supervision, which usually exposes them to higher risks. Teachers, on the other hand, frequently oppose to increases in the length of the school day, because they equate it with more hours of work, and probably not getting paid well for the additional work. Other critics argue against increasing the length of the school day because of its (low) cost-effectiveness, mainly due to the high implementation costs of these reforms.

In 1994, the government of Colombia passed the General Education Law (Law 115), which established that all public schools should have a single full school day (7-hour). However, plans to implement this law did not begin until years later and were completely abandoned by 2002. This happened partly due to a substantial increase in demand for public schooling that was difficult to meet because of low physical capacity (fewer schools than the ones needed to meet the demand) and scarce resources (low capacity to hire enough administrators and teachers for a full day). More recently, some of the largest cities in the country (e.g. Bogotá and Cali), implemented pilot programs to increase the length of the school day, and in 2014, President Santos announced the implementation of a national scale full school day program as the central education reform for his second term (2014-2018).

The argument for implementing a longer school day in Colombia is that more time spent in school will translate into better outcomes for students. However, except for a few recent studies that have found a positive impact on student achievement (Bonilla-Mejia, 2011) and other student outcomes (Garcia, et al., 2013), there is not a lot of evidence suggesting a causal link between the length of the school day and student outcomes.

Establishing the impact of having a full school day on student outcomes is methodologically difficult. If students were randomly assigned to different school shifts, in principle one could analyze the impact of attending a full school day by estimating the difference in student achievement between students attending a full school day and students attending a half school day. However, a student's application and allocation to a school, and the decision process on the length of the school day and the number of school shifts a school has, is neither simple nor random.

First, the Secretary of Education of each municipality assesses the expected demand for school slots for the following year, and the capacity that schools in that municipality have to offer school slots (i.e., supply). Depending on that information, he or she determines whether it might be necessary to have two half-day shifts (i.e. a morning and an afternoon shift) instead of a full school day in certain schools, in order to increase the supply of school slots (Ministerio de Educación Nacional, 2006). As a result, the process of deciding the length of the school day for the municipality's schools is a function of many variables. These include the number of people who are at school age in the municipality, the actual demand for school slots (i.e.; the number of students already enrolled in the education system, and the applications filled out by parents for each school), the number of available places in each grade in each school, and the amount of education resources in the municipality (e.g., to hire more teachers). In some cases, the Secretary of Education might decide to implement half school days in schools with a higher demand in order to be able to accept more students in those schools, and to implement full school days in schools with a lower demand.

Second, in Colombia, parents cannot directly choose a public school for their children: When they want to enroll their children for the first time into a public school, or want to transfer their children into a different school because they moved to a new area, they need to complete an application with the municipality's Secretary of Education. In this application, parents are able to express their preference for some schools, but they cannot express their preference for a particular shift. The Secretary of Education then makes the decision on where to assign the new or transferred students whose parents have applied for a school spot, based on the demand and supply of schools slots for each grade. Ideally, this decision matches the parents' expressed preferences, but it is possible that children are assigned to a school that is not in their parents' preferences if there are not enough schools slots in the parents' preferred schools.[6]

4. Empirical strategy

There are methodological difficulties in identifying the causal effect of longer school days in a credible way, because there might be systematic selection of students to schools with a particular shift, or of schools with particular characteristics into a type of shift. For this reason, a cross-sectional OLS strategy will yield biased estimates of the effect of the full school day.

The following sub-sections describe the two main estimation strategies I implement in order to address these selection concerns, and analyze the extent to which they can address them. In every model, the dependent variable is the average SABER test score for each grade, subject, and year for each school. I standardize the schools' test scores by grade, subject, and year, so that each of these samples of school test scores has a mean of zero and a standard deviation of one. This allows me to interpret the coefficients in the regressions as standard deviations from the mean. The key independent variable in this study is an indicator for the type of day that a school

[6] If that is the case, the municipality will try to allocate the students to the school closest to their place of residence. However, it might be possible that they are assigned to a school in the other side of the city if that is the only school where there are available slots, although this is not frequently the case.

has in each year: Full school day (from now on, FSD) is equal to one if the school has a full school day in a year, or zero if it has half school day (i.e. it has morning and/or afternoon shift).[7]

i. Estimation Strategy 1: Municipality fixed effects

The first estimation strategy uses a municipality fixed effects model, which estimates the impact of attending a full school day by comparing test score results of schools with full and half school days within the same municipality. I include dummy variables for each year to control for any observed or unobserved policy changes or events that affected all schools equally in each year. The model is specified as follows:

$$\mathrm{SABER}_{st} = \beta_0 + \beta_1 \mathrm{FSD}_{st} + \beta_2 \mathrm{X}_{st} + \mathrm{d}_t + \mathrm{d}_m + \varepsilon_{st} \qquad (1)$$

Where $SABER_{st}$ is the average test scores in SABER for school *s* in year *t*; FSD_{st} is a dummy variable equal to 1 if the school has a full school day and 0 if it does not (i.e. if it is has half-school-day); d_t are year fixed effects; d_m are municipality fixed effects; and X_{st} is a vector of school control variables.

ii. Estimation Strategy 2: School fixed effects

The problem with the previous estimation strategy is that it is possible that the observed differences between the schools with full and half school days might simply reflect pre-existing differences between the schools or their students, or reflect the effect of unobserved characteristics that are correlated with having both a full school day and higher test scores. For example, if the demand for each school is related to its quality (e.g., higher demand for relatively higher quality schools, and lower demand for relatively lower quality schools), this could possibly cause a downward bias in the estimates of the impact of a full school day, because schools with a full school day will have other unobserved characteristics systematically related to test scores.

[7] In a relatively few cases, a school reports information for two different shifts, and describes one as full school day and the other as a morning or afternoon shift. This seems unlikely to happen, and it is more likely a mistake that someone makes when filling out the C600 forms. My main sample drops observations for these schools (1,140 observations, or 0.6% of the C600 sample).

In order to mitigate this potential problem, I implement a school fixed effects model, which exploits the fact that some schools switched from having a half school day to a full school day, or vice versa. The school fixed effects focuses on analyzing within-school variation in mean school test scores across different cohorts (time), allowing me to break the correlation between unobserved school characteristics and having a full school day, and therefore eliminating the bias created by schools systematically selecting the length of the school day (Bifulco, Fletcher, and Ross, 2011; Hoxby, 2000). To implement this strategy I estimate the following specification:

$$\mathrm{SABER}_{st} = \beta_0 + \beta_1 \mathrm{FSD}_{st} + \beta_2 \mathrm{X}_{st} + \mathrm{d}_t + \mathrm{d}_s + \varepsilon_{st} \quad (2)$$

The dependent variable is the SABER test score for the school in each period. It is a function of time-varying school characteristics (X), the length of the school day in each period (FSD), and time (dt) and school (ds) fixed effects.

The identifying assumption for the school fixed effect model is that schools that switched from having a full school day to a half school day, or vice versa, did so for plausibly exogenous reasons that are not related to the test scores. That is, the reason for the switch must be unrelated to test scores or to unobservable characteristics that are correlated with test scores. I argue that because switching the length of the school day is a municipality-level decision typically based on policy or on the demand and supply for school slots, it is exogenous from the point of view of the school, the parents and the students, and unlikely to be correlated with year to year (i.e., across cohort) variations in test scores.

Additionally, given that the Secretary of Education mandates that in the process of allocating slots each year, priority should be given to students that are already enrolled in the schools, parents of students in 5^{th} and 9^{th} grade are not likely to request a change to a school with a particular type of shift because it was switching to a different type of shift, given that they would risk losing their school slot. This means that 5^{th} graders and 9^{th} graders did not seek to be treated, or alter their condition (enrollment in their current school) in order to get the treatment (enrollment in a school with a full school day).

This is my preferred specification and, in my view, the best estimation of the causal impact of having a full school day. By controlling for all observed and unobserved time-invariant school characteristics, this specification decreases the possible bias caused by characteristics that do not change over time within the school, although some endogeneity may remain from time-varying characteristics. For example, there might be some selection issues if the Secretary of Education chooses school shifts based on school attributes, and particularly if these school attributes are related to school performance in the SABER. For example, if very motivated principals or Parents' Associations systematically push and convince the Secretary of Education to implement a full school days in their schools, the estimate of the impact of having a full school day might be biased, because this type of schools (with more motivated principals or Parents' Associations) are also more likely to have a full school day and to perform better in SABER. There might also be selection issues if the Secretary of Education assigns students to schools with a certain type of shifts, based on parents' or students' socioeconomic characteristics, for example, if all relatively poor students are assigned to half-day schools.

There might also be endogeneity from time-varying characteristics if students systematically switch schools that are changing the length of the school day. For example, if more educated parents systematically apply for schools that have switched to a full school day, or if they move to an area where most schools have a full school day. However, this might not be the case, because in Colombia parents do not have much choice of schools or shifts, and in contrast to the US, they do not tend to move because of the schools in the area. Although this is an empirical question that I am not able to test directly from my data, other authors have cited evidence from the 2007 Quality of Life Survey in Bogota, suggesting that of the 21% of households that move within a 2 year period, just 4% of those cite education or health considerations as the reason for the move (Bonilla-Angel, 2011).

5. Data

To implement the strategies described above, this study exploits an extensive and relatively new school panel dataset that contains standardized test score results from SABER 5th and 9th for three periods: 2002, 2005, and 2009.[8] SABER 5th and 9th are nationwide standardized

[8] More precisely, the SABER 2002 was administered in different moments in 2002-2003, but most tests for 5th grade and 9th grade were conducted in 2002, so I refer to this test as SABER 2002. In the same way, the SABER 2005 was administered in different moments in 2005-2006, but I refer to this test as SABER 2005. All SABER 2009 tests were administered in 2009.

assessments administered every three years to all students in 5th grade and 9th grade in Colombia.[9] The SABER panel contains test scores at the individual level and some basic identification information.

I merge the SABER school panel dataset with school data from the C600, a survey used annually by the Departamento Administrativo Nacional de Estadística (DANE, the national statistics agency) to collect information on basic school characteristics. This dataset contains information on the schools' type (public or private), location (urban or rural), and school characteristics such as enrollment, information on number of transfers, dropouts, grade repetition and promotion in the previous academic year, number of groups or classes by grade, administrative staff, and some information about teachers (e.g., grade they teach, and their educational level). Even though the C600 is collected at the school-shift level (i.e. it collects information separately for each shift in each school), the data from SABER does not allow identifying the type of shift that each student attends. Therefore, in order to match these two datasets I have to aggregate the C600 data at the school level.[10]

The SABER dataset contains information for each student that took the test, but given that the data is not representative at the student level, and that I only have additional data on school level characteristics, I first aggregate the SABER data at the school level. With both the SABER and C600 panels at the school level, I use the school identification codes to merge them and create a school-level panel. I create separate samples for 5th and 9th grade tests, including only schools with non-missing math and language scores in the given grade. Each of these samples contains the school's average SABER math and language test scores for that grade and its corresponding school characteristics from the C600.[11]

To be part of the panel, a school has to have information from both SABER and C600 for a given year, but it does not necessarily have to have information for the three periods. That is, it

[9] The 2012 application of SABER was extended to evaluate third grade students, but data for this round is not included in this dataset.

[10] Because schools that do not have a full school day, or a morning or afternoon shift are very uncommon (mostly night and weekend shifts, and about 5.5% of all shift-level observations), and their student characteristics might be so different than those of students attending traditional "week-day day schools," before aggregating at the school level I drop night and weekend shift observations from the dataset, and exclude them from the analysis.

[11] Separating schools into four sub-samples by grade and subject (and therefore including schools that are missing one subject test per grade) does not change the results (available on request).

is an unbalanced panel.[12] For every year, there are a significant number of schools in the C600 that cannot be matched with any school in the SABER dataset. In some cases this is due to problems matching the school identification codes in the two datasets (because of mistakes when typing-in the data, or because of changes in the codes). Additionally, the SABER school panel left out a large number of school observations that could not matched across years, and were not included in the dataset.[13] Therefore, they could not be matched to C600 either.

Table 1 contains descriptive statistics of the school panel for the 5th grade and 9th grade samples. In the 5th grade sample, most schools in the sample are located in rural areas, while most schools in the 9th grade sample are located in urban areas. In both the 5th and 9th grade samples, there is a high concentration of the poorest schools in the school panel. In the 5th grade sample, around one third of schools have a full school day, while this percentage about one fifth of the schools in the 9th grade sample have a full school day.

Table 2 presents the unconditional mean and standard deviation of test scores, and test scores at different percentiles of the distribution, for schools in the 5th grade and 9th grade samples.[14] In general, the unconditional mean of all test scores goes down in time. The unconditional mean in both grades and both subjects got worse between 2002 and 2005, but the largest decrease was for the 5th grade math test scores, where there was a tenfold decrease in the mean test score (from -0.01, to -0.10). The smallest decrease was in 9th grade math test scores, with test scores going from -0.09 to -0.12 of a standard deviation. The average 9th grade test scores got even worse in 2009: the language test scores for this grade went from -0.22 in 2005, to -0.28 in 2009, and the math test scores went from -0.12 in 2005, to -0.26 in 2009. In contrast,

[12] As part of the robustness checks, I also run my models using a balanced panel with schools that appear in the three periods in the database.

[13] There are three main reasons why schools could not be matched across time, and are therefore not included in the SABER school panel. First, although most of the SABER tests are comparable across time, a substantial number of the SABER 2002 tests could not be made comparable with SABER 2005 and SABER. Second, some school identification codes could not be matched across time because some small schools were merged into larger "education institutions" with a single administration, changing their identification codes. Finally, there was evidence of cheating in some schools, especially in 2002 and 2005, which made impossible to make these schools' test scores comparable across time (ICFES, 2011).

[14] Test scores were standardized by grade, subject, and year, in the complete SABER dataset (i.e., including private schools and schools with night and weekend shits, all of which were excluded from the final panel; and also before losing some observations when merging the SABER dataset with the C600 school data). For this reason, the unconditional means in this table are different from zero, and the standard deviations are different from one.

the average language test scores improved in the 5th grade language sample: it went from -0.16 in 2005, to -0.12 in 2009. The standard deviation narrowed down in time in the four samples.

In Table 3, I look at the differences in observable characteristics between schools that have a full school day and those that have half school days. To do so, I estimate separate regressions of school characteristics as a function of FSD, cohort (time) fixed effects, and school fixed effects. Table 3 shows the estimated coefficient for FSD for each of these regressions. These coefficients tell us if there is a statistically significant difference in each of the school characteristics in column 1, between cohorts exposed to a full school day and cohorts exposed to half shifts, in the same school. As expected, there is a statistically significant difference in school enrollment between schools exposed to full and half school days, given that schools with double shifts are likely to have a larger enrollment. There is also a statistically significant difference in the percentage of teachers with a professional degree in the 9th grade sample. The negative coefficient suggests that schools with a full school day have a lower proportion of professional teachers. Further, the absence of statistically significant estimates for the other schools characteristics suggests that the observable characteristics of the school are not highly correlated with the length of the school day.

Since my identification strategy relies on schools that switched shifts (i.e. changed the length of the school day), Table 4 shows the number of schools that switched by type of change (from half to full school day, and from full school day to a half school day), and classified by the year in which the switch takes place. In total, 2,262 schools in the 5th grade sample and 464 schools in the 9th grade sample switched shift at some point between 2002 and 2009, which corresponds to 12 percent and 10 percent respectively, of the schools that exist in at least 2 periods in the panel.[15] The number of schools that switched shift in each sample corresponds to the schools that are used in the school fixed effect model to identify the effect of FSD.

[15] There are 237 schools in the 5th grade sample, and 74 schools in the 9th grade sample, that switched from half to full school day in 2005 and then back to half in 2009, or that switched from full to half school day in 2005, and then back to full school day in 2009. These schools are excluded from the calculations in this table, but they are included in the sample used in the regressions

6. Results

i. Main Results

Table 5 presents the main findings for 5th grade test scores by subject, using the panel specifications (2) and (3) described in section 4.[16] The municipality fixed effects specification is reported in columns 1 and 3, and the school fixed effects specification is reported in columns 2 and 4. As mentioned above, the latter is my preferred specification, because most of the characteristics and confounding factors that could bias the estimates of FSD originate from differences between schools with and without a full school day. Therefore, by comparing variations in test scores within a school, across cohorts (before and after a scheduling switch), many of the selection and endogeneity concerns are mitigated.[17]

The first row of Table 5 presents the estimates for FSD, the impact of having a full school day on test scores. FSD is statistically significant at the 1 percent level for the panel model for 5th grade language, and across the two specifications for 5th grade math. Looking at the language results first, the estimate of FSD in the municipality fixed effects specification shows that having a full school day increases language test scores by 0.055 of a standard deviation, compared to schools without a full school day in the same municipality and year (column 1). In the school fixed effects specification (column 2), the estimate of FSD (0.04) is only slightly lower than in the panel specification, but it is no longer statistically significant.

Columns 3 and 4 in Table 5 show the set of findings for 5th grade math. The coefficient for FSD in both the municipality and the school fixed effects specifications is 0.082 and statistically significant, and in both cases, it is larger than the respective specification for language (e.g., 0.082 v. 0.055, using the municipality fixed effects specification). In the school fixed effects model (column 4), the estimate for FSD can be interpreted as the within-school impact of switching to a full school day on math test scores. In this case, among schools that switched shifts, the cohorts exposed to a full school day have test scores that are 0.082 of a

[16] I ran regressions of a base model that only includes FSD, and year and municipality fixed effects as explanatory variables. The results for this model tell us the test scores differences between school with a full school day and schools with a half school day: Schools with a full school day have language test scores that are on average 0.285 of a standard deviation higher, and math test scores that are 0.274 of a standard deviation higher, than schools with a half school days.

[17] Because the school fixed effects only captures the changes in test scores that can be explained by time-varying variables, time-invariant variables are excluded from this regression.

standard deviation higher than cohorts that attended half shifts. The estimated coefficients are very similar across specifications for 5th grade math (0.082), and they do not change much across specifications for the 5th grade language (0.055 in the municipality fixed effects, and 0.044 in the school fixed effects). The fact that the school fixed effects do not substantially alter the estimates, suggest that most of the selection occurs at the municipality level.

Table 6 presents the findings for 9th grade test scores by subject. The first row shows that there is a statistically significant (at the 1% level) difference in test scores between schools that have a full school day and schools that have a half school day. The municipality fixed effects results reveal test score differences of 0.162 for language (column 1) and 0.137 for math (column 3). The school fixed effects results show that among schools that switched shifts, there is also a statistically significant difference in test scores between cohorts exposed to a full school day and cohorts that attended half school days. The estimated effect is 0.110 for language (columns 2) and 0.138 for math (column 4), suggesting stronger effects in math, as in the 5th grade sample.

Most notably, all the coefficients for FSD in the 9th grade sample are larger than the respective coefficients for 5th grade (and the difference between these coefficients is statistically significant), suggesting that having a full school day has a larger positive impact on 9th graders than on 5th graders. The 9th grade estimates are more than double the 5th grade estimates in language, and about 50 percent larger in math.

In sum, the most reliable estimates of the impact of a full school day, which come from the school fixed effects specifications, show that among schools that switched shifts, the cohorts exposed to full school days have higher test scores than cohorts that attended half school days, and the impact ranges from 0.082 of a standard deviation for 5th grade math test scores, to 0.138 of a standard deviation for 9th grade math test scores.

ii. Heterogeneous effects and alternate samples

In Table 7, I analyze if there are heterogeneous effects when comparing FSD to different shifts. The main estimations, which are reported in Tables 5 and 6, look at the impact of attending a FSD compared to attending a half day shift, regardless of the type of half day shift (i.e. morning or afternoon shift). These are presented in Panel A of Table 7 for easier reference. Panel B presents the school fixed effects estimates of the impact of FSD, but in this case cohorts exposed to FSD are compared to cohorts exposed to morning shifts. The estimates and statistical significance are very similar to those in Panel A, which suggests that most of the schools that switched went from a morning shift to a full school day. Panel C reports the school fixed effects estimates for the impact of FSD, comparing cohorts exposed to FSD with cohorts exposed to afternoon shifts. In this case, FSD was only statistically significant for the 9th grade language sample. This means that cohorts exposed to a full school day have language test scores that are one fourth of a standard deviation higher than cohorts exposed to afternoon shifts. Notably, the coefficient is twice as large as the corresponding coefficient on Panel A, suggesting that the impact of switching to a full school day is much larger for 9th grade students in schools that previously had afternoon shifts. This has important policy implications, because 9th grade students are usually exposed to more risks when they are not in school, and these results suggest that when they attend full school days instead of afternoon shifts, their test scores are substantially better.

In Table 8, I test for heterogeneous effects by estimating the school fixed effects specification using different subsamples: only rural schools, only urban schools, and only schools classified in each of the four socioeconomic statuses (SES). Columns 1 to 4 present the results for each grade and subject, while each row represents the coefficient of FSD for the specific subsample. Although the smaller samples reduce the power to detect effects, I find a statistically significant impact of FSD for the rural schools in the 5th grade math sample: cohorts exposed to full school days in rural schools have test scores that are almost one tenth of a standard deviation higher than cohorts that attended half school days in rural schools. There is also a marginally statistically significant (10%) impact of being exposed to a full school day in schools in urban areas for the 9th grade language sample.

Splitting the sample by socioeconomic status in the lower rows of Table 8, reveals that the impact of having a full school day is statistically significant only for the subsample of schools with the lowest socioeconomic status (SES=1), and it is not statistically significant for any of the subsamples containing schools with higher socioeconomic statuses (SES=2, 3, or 4). Among the poorest schools, cohorts exposed to a full school day have test scores that are 0.067 of a standard deviation higher in 5th grade language, and 0.093 of a standard deviation higher in 5th grade math, than the cohorts that attended half school days. There is an even larger impact of attending a full school day for the poorest schools in the 9th grade math sample: cohorts exposed to a full school day have test scores that are one fifth of a standard deviation higher that cohorts exposed to half school days. This provides more evidence supporting what has been found in previous studies: having longer school days has heterogeneous impacts on student achievement, and the impact is generally larger for the poorest schools.

Table 9 shows the results of estimating the school fixed effects specification using another set of subsamples. In the first two rows, I run the model using a subsample with only schools that switched from half to full school day (first row), and a subsample with only schools that switched from full to half school day (second row), in order to analyze if there are symmetric effects of switching from half to full school day, and from full school day to half. FSD is only statistically significant for the subsample of schools that went from full school day to half: for these schools, the cohorts exposed to a full school day have 0.214 of a standard deviation higher test scores in 5th grade math, and 0.306 of a standard deviation higher test scores in 9th grade math. The regressions reported in the third row use only the schools that exist throughout the whole period (i.e., using a balanced school panel). In this case, FSD is only statistically significant for the 9th grade language sample (at the 10% level), and the impact is smaller than for the complete sample of schools (0.089 v. 0.11 (Table 6, column 2)).

Finally, the bottom two rows of Table 9 examine "treatment" intensity. I ask whether the impact of attending a full school day is different for schools that were exposed to a full school day for longer/shorter lengths of time. The fourth row presents the results for regressions that used a subsample of only schools that switched from a half to full school day between 2002 and 2005, and kept the full school day in 2009. These schools could be considered "early adopters" and were exposed for longer to the "treatment" (i.e., full school day). In this case, at least two cohorts within the school were exposed to a full school day. It could also mean that students in these schools were exposed to a full school day for more than one grade, although it is not possible to know for sure if students changed schools over this period. Despite the very small sample size (444 schools), the results show that cohorts exposed to a full school day in schools that were treated for longer have test scores that are 0.274 of a standard deviation higher in 9th grade language, and 0.292 of a standard deviation higher in 9th grade math, than cohorts that attended half school days. The bottom row reports the regressions results using a subsample of only schools that switched from half to full school days after 2005. That is, it contains schools that had a shorter duration of a full school day and a longer duration of a half school days in the time period studied. Essentially, students in these schools were "late adopters," and were exposed to the treatment for a shorter period. There is no statistically significant impact of being exposed to a full school day for the schools in this subsample.

7. Conclusions and policy implications

This study provides some of the first evidence that longer school days have an impact on 5th grade and 9th grade academic achievement in Colombia. By using a school fixed effect model that exploits within-school changes in the length of the school day, I am able to control for observed and unobserved time-invariant characteristics of schools, mitigating some of the most critical selection and endogeneity problems that commonly occur when comparing different types of schools. I find that there is a positive impact of having a full school day (approximately 2-3 additional hours) on school achievement in 5th grade math, and 9th grade math and language SABER test scores. Results from the school fixed effects model show that among schools that changed the length of the school day between 2002 and 2009, the cohorts exposed to full school days have test scores that are about one tenth of a standard deviation higher than cohorts that attended half school days. This corresponds to approximately a 2.6 percent increase in test scores with respect to the mean for each grade, subject and year.

To put the magnitude of this effect in perspective, the impact of longer school days is smaller than other popular education interventions in Colombia, namely the PACES voucher program, and the contractual schools in Bogota. The PACES voucher program, which has received a lot of attention in the literature because of the use of a lottery to allocate vouchers, had an impact of about 0.2 standard deviations on student test scores relative to students who did not win the lottery (Angrist et al., 2002). While contractual schools in Bogota, which were traditional public schools whose administration was contracted out to reputed, not-for-profit private schools and universities, had an impact of 0.6 and 0.2 standard deviations in math and verbal tests, respectively, relative to traditional public schools (Bonilla-Angel, 2011). However, the magnitude of the impact estimated in this study is similar to other school interventions such as reducing class size by 4 students (Krueger, 1999), or the impact of charter schools on reading test scores (Angrist et al., 2012), and considering that two-tenths of a standard deviation is roughly the score gain associated with one additional school year (Cole, Trent, and Wadell, 1993), it is a sizable effect. Overall, the results suggest that lengthening the school day may be an effective policy for increasing student achievement.

Further, I find that the impact of having a full school day is larger for math test scores than for language test scores in both 5th and 9th grade (e.g., 0.138 v. 0.11 respectively, for 9th graders). This result is not surprising, since, as other research has found, schools may play a larger role in teaching math relative to language, since language skills are often shaped by the home environment.

More notable is that the positive effects of a longer school day are stronger for 9th grade students than for 5th grade students (e.g., 0.138 v. 0.082 respectively, for math test scores). This makes sense since adolescents may be more likely to engage in risky behaviors outside of school than younger children. Even if they are not engaging in academic endeavors during the extra school hours, 9th graders are certainly spending less time exposed to risk factors outside of school, which ultimately translates into better academic achievement.

Finally, my results suggest that the effects of full school days are heterogeneous. The impact of being exposed to a full school day for 9th grade students is larger when compared to cohorts exposed to afternoon shifts. And, like many other interventions in developing countries, the impact of full school days on test scores is largest among the poorest schools and those in rural areas.

Overall, my findings complement those of previous studies in Colombia, which have found a positive impact of attending a full school day on dropout and grade repetition in primary (Garcia et al., 2013), and a positive impact of attending a full school day on graduation-exit tests (Bonilla-Mejia, 2011). Together, all these findings suggest that longer schools days have a positive impact on academic achievement and other student outcomes in Colombia. Therefore, lengthening the school day may be an effective policy for increasing student achievement, particularly for the lowest-income students in Colombia and other developing countries.

One argument against increasing the length of the school day is that it is not cost-effective, mainly due to the personnel costs which will significantly increase because of the need to hire new teachers, and to the high investments in infrastructure that are needed. However, in the Colombian case, there are some adjustments that could allow extending the school day (at least in some schools) without a significant increase in costs. First of all, in some schools a different group of teachers serve different school shifts, so a change to a full school day would imply a salary increase for current teachers, but will not necessarily require hiring new teachers (Garcia et al., 2013). Similarly, there a large number of schools that have only a morning or an afternoon shift (Bonilla-Mejia, 2011). This means that there is an opportunity for increasing the length of the school day at least in those schools, without high investments in new infrastructure or constructing new schools.

A caveat with the main empirical strategy used in this paper, the school fixed effects model, is that it is not able to control for time-variant characteristics that could be correlated with both the change in the length of the school day and test scores. Although I argue that these are not likely to be large, future research should consider expanding the empirical work to include other quasi-experimental estimation strategies or, if possible, a randomized controlled trial to more definitively assess the impact of longer school days on student achievement.

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Table 1. Descriptive Statistics of School Panel

	5th grade			9th grade		
	2002	**2005**	**2009**	**2002**	**2005**	**2009**
Total schools	14419	15006	24767	2970	4214	6428
% urban	18.8%	25.4%	21.1%	56.4%	60.0%	51.1%
Average socioeconomic status (SES)	1.38	1.41	1.39	1.80	1.89	1.77
% of poorest schools (SES=1)	73.5%	72.8%	72.9%	49.7%	46.0%	52.4%
% of richest schools (SES=4)	2.2%	2.8%	2.4%	7.4%	8.2%	7.0%
Average school enrollment	195	258	205	609	766	673
Average 5th/9th grade enrollment	22	30	24	62	82	73
Average primary/secondary enrollment	129	153	118	321	387	342
Primary/secondary teacher-student ratio	0.04	0.04	0.05	0.07	0.06	0.06
% primary/secondary teachers with professional degree	59.4%	68.3%	67.0%	86.5%	91.7%	90.0%
% primary/secondary teachers with pedagogic training	60.5%	67.8%	67.0%	83.8%	86.9%	83.5%
% schools with a full school day	27.0%	34.2%	27.1%	21.5%	23.9%	19.9%
% schools that switched to/from full school day	14.1%	10.4%	9.8%	15.3%	11.4%	8.2%

Source: Author's calculations based on SABER and C600.

Note: School socioeconomic status (SES) is an index that goes from 1 (poorest schools) to 4 (richest schools).

Table 2. Distribution of Test Scores by Year

	Language			Mathematics		
	2002	**2005**	**2009**	**2002**	**2005**	**2009**
Percentile			**5th grade**			
5%	-1.53	-1.63	-1.49	-1.48	-1.53	-1.45
25%	-0.76	-0.78	-0.74	-0.84	-0.79	-0.77
Median	-0.14	-0.21	-0.21	-0.10	-0.24	-0.23
75%	0.64	0.38	0.39	0.73	0.46	0.40
95%	1.75	1.61	1.64	1.77	1.80	1.71
Mean	**-0.04**	**-0.16**	**-0.12**	**-0.01**	**-0.10**	**-0.11**
St. deviation	**1.01**	**0.96**	**0.94**	**1.02**	**1.00**	**0.95**
Percentile			**9th grade**			
5%	-1.37	-1.63	-1.59	-1.12	-1.06	-1.43
25%	-0.73	-0.75	-0.78	-0.73	-0.61	-0.77
Median	-0.26	-0.24	-0.24	-0.38	-0.34	-0.29
75%	0.30	0.28	0.21	0.19	0.06	0.16
95%	1.73	1.28	0.95	2.16	1.78	1.08
Mean	**-0.13**	**-0.22**	**-0.28**	**-0.09**	**-0.12**	**-0.26**
St. deviation	**0.94**	**0.89**	**0.80**	**0.99**	**0.94**	**0.80**

Source: Author's calculations based on SABER and C600.

Note: Test scores are standardized by grade, subject and year.

Table 3. Differences in Observable Characteristics between Full and Half School Days

	5th grade	9th grade
	(1)	(3)
Enrollment	-0.143***	-0.454***
	(0.018)	(0.091)
Teacher student ratio	0.001	-0.000
	(0.001)	(0.003)
% teachers with professional degree	0.001	-0.025**
	(0.010)	(0.011)
% teachers with pedagogic training	-0.005	-0.014
	(0.010)	(0.013)

Source: Author's calculations based on SABER and C600.
Notes: Standard errors in parenthesis. * significant at 10%, ** at 5 %, *** at 1%. All regressions include school and year fixed effects. Each column reports the coefficient for the variable FSD, which shows the differences in school characteristics (the dependent variable in the first column) between FSD and half schedule schools, controlling for school and year.

Table 4. Number of Schools that Switched Schedule by Type of Change and Year

	5th grade	9th grade
Total number of schools*	19546	4570
Number of schools that switched schedule	**2,262**	**464**
Percent of schools that switched schedule	12%	10%
From half to Full school day	**1398**	**250**
half in 2002, FSD in 2005 and 2009	409	148
half in 2002 and 2005, FDS 2009	120	50
half in 2002, FSD in 2005**	47	8
half in 2002, FSD in 2009**	723	25
half in 2005,FSD in 2009**	99	19
From FSD to half schedule	**864**	**214**
FSD in 2002, half in 2005 and 2009	173	75
FSD in 2002 and 2005, half in 2009	89	38
FSD in 2002, half in 2005**	14	3
FSD in 2002, half in 2009**	220	33
FSD in 2005, half in 2009**	368	65

Source: Author's calculations based on SABER and C600.
Notes: Schools that switched from half to FDS in 2005 and then back to half in 2009, or that switched from FSD to half in 2005, and then back to FSD in 2009 are excluded from the total number of schools that switched schedule (237 schools in the 5th grade sample, and 74 schools in the 9th grade sample).*Total number of schools that exists in 2 or 3 periods. **These schools only exist in 2 periods.

Table 5. Regression Results. Dependent Variable: 5th Grade Test Scores.

	Language		Mathematics	
	Municipality fixed effects (1)	**School fixed effects (2)**	**Municipality fixed effects (3)**	**School fixed effects (4)**
FSD	0.055**	0.044	0.082***	0.082**
	(0.024)	(0.034)	(0.027)	(0.033)
Dummy for urban schools	-0.101***	--	-0.181***	--
	(0.022)		(0.024)	
School socioeconomic status	0.115***	--	0.078***	--
	(0.016)		(0.014)	
School enrollment	-0.002	0.007	-0.009***	0.004
	(0.001)	(0.006)	(0.003)	(0.006)
Primary teacher-student ratio	1.271***	0.509	0.791***	-0.123
	(0.277)	(0.468)	(0.264)	(0.470)
% teachers with professional education in primary	0.033	-0.036	0.039	-0.016
	(0.026)	(0.047)	(0.025)	(0.046)
% teachers with pedagogical training in primary	0.067***	0.048	0.052**	0.041
	(0.026)	(0.046)	(0.024)	(0.045)
Observations	54,192	54,192	54,192	54,192
R-squared	0.161	0.611	0.185	0.618
Year fixed effects	YES	YES	YES	YES
Municipal fixed effects	1093	NO	1093	NO
School fixed effects	NO	26919	NO	26919

Source: Author's calculations based on SABER and C600.
Notes: Standard errors in parenthesis. * significant at 10%, ** at 5 %, *** at 1%. FDS is a dummy variable equal to 1 when the school has a Full school day. School socioeconomic status (SES) is an index that goes from 1 (poorest schools) to 4 (richest schools). All standard errors are robust.

Table 6. Regression Results. Dependent Variable: 9th Grade Test Scores.

	Language		Mathematics	
	Municipality fixed effects (1)	**School fixed effects (2)**	**Municipality fixed effects (3)**	**School fixed effects (4)**
FSD	0.162***	0.110**	0.137***	0.138**
	(0.033)	(0.055)	(0.034)	(0.063)
Dummy for urban schools	-0.040	--	-0.074***	--
	(0.029)		(0.025)	
School socioeconomic status	0.269***	--	0.168***	--
	(0.021)		(0.017)	
School enrollment	0.001	-0.016***	-0.001	-0.008
	(0.002)	(0.006)	(0.002)	(0.007)
Secondary teacher-student ratio	0.230	0.032	0.548*	0.451
	(0.296)	(0.573)	(0.319)	(0.503)
% teachers with professional education in secondary	0.036	-0.028	-0.014	-0.076
	(0.052)	(0.091)	(0.052)	(0.097)
% teachers with pedagogical training in secondary	-0.005	-0.103	-0.034	-0.056
	(0.047)	(0.090)	(0.051)	(0.097)
Observations	13,612	13,612	13,612	13,612
R-squared	0.287	0.661	0.212	0.611
Year fixed effects	YES	YES	YES	YES
Municipal fixed effects	1,083	NO	1,083	NO
School fixed effects	NO	6725	NO	6725

Source: Author's calculations based on SABER and C600.
Notes: Standard errors in parenthesis. * significant at 10%, ** at 5 %, *** at 1%. FDS is a dummy variable equal to 1 when the school has a Full school day. School socioeconomic status (SES) is an index that goes from 1 (poorest schools) to 4 (richest schools). All standard errors are robust.

Table 7. Heterogeneous Effects: Comparing Impacts for Different Half Day Shifts

	5th grade		9th grade	
	Language	**Mathematics**	**Language**	**Mathematics**
Panel A: Full school day v. half day shift (morning or afternoon)				
FSD	0.044	0.082**	0.110**	0.138**
	(0.034)	(0.033)	(0.055)	(0.063)
Observations	54,192	54,192	13,612	13,612
Number of schools	26919	26919	6725	6725
Panel B: Full school day v. morning shift				
FSD_morning	0.044	0.083**	0.108**	0.138**
	(0.034)	(0.033)	(0.055)	(0.063)
Observations	54,049	54,049	13,509	13,509
Number of schools	26877	26877	6701	6701
Panel C: Full school day v. afternoon shift				
FSD_afternoon	0.046	0.030	0.264*	0.175
	(0.113)	(0.107)	(0.155)	(0.172)
Observations	25,216	25,216	8,705	8,705
Number of schools	13214	13214	4551	4551

Source: Author's calculations based on SABER and C600.

Notes: Standard errors in parenthesis. * significant at 10%, ** at 5 %, *** at 1%. FSD is a dummy variable equal to 1 when the school has a full school day and 0 when it has a half school day (either a morning or afternoon shift). FSD_morning is a dummy variable equal to 1 when the school has a full school day, and 0 when the school has a morning shift FSD_afternoon is a dummy variable equal to 1 when the school has a Full school day, and 0 when the school has an afternoon shift. All standard errors are robust.

Table 8. Heterogeneous Effects: School Fixed Effects Model Using Alternate Samples

	5th grade		9th grade	
	Language (1)	**Mathematics (2)**	**Language (3)**	**Mathematics (4)**
Rural schools	0.056	0.097***	0.116	0.175*
	(0.037)	(0.036)	(0.092)	(0.103)
Observations	*42,446*	*42,446*	*6,121*	*6,121*
Urban schools	0.005	0.009	0.104*	0.106
	(0.068)	(0.077)	(0.063)	(0.073)
Observations	*11,746*	*11,746*	*7,491*	*7,491*
Poorest schools (SES=1)	0.067*	0.093**	0.138	0.200**
	(0.039)	(0.038)	(0.084)	(0.093)
Observations	39,571	39,571	6,785	6,785
Middle-to-poor (SES=2)	0.006	0.087	0.103	0.102
	(0.077)	(0.075)	(0.081)	(0.094)
Observations	*9,298*	*9,298*	*3,564*	*6,785*
Middle-to-rich (SES=3)	0.072	-0.028	0.027	0.001
	(0.134)	(0.143)	(0.152)	(0.162)
Observations	*4,012*	*4,012*	*2,251*	*3,564*
Richest schools (SES=4)	-0.169	-0.132	0.121	-0.029
	(0.142)	(0.177)	(0.170)	(0.330)
Observations	*1,311*	*1,311*	*1,012*	*2,251*

Source: Author's calculations based on SABER and C600.
Notes: Standard errors in parenthesis. * significant at 10%, ** at 5 %, *** at 1%. FDS is a dummy variable equal to 1 when the school has a Full school day. School socioeconomic status (SES) is an index that goes from 1 (poorest schools) to 4 (richest schools). Only the coefficient for FDS is reported. All regressions control for school enrollment, teacher student ratio, percentage of teachers with a professional degree, percentage of teachers with pedagogic training, and include school and year fixed effects. All standard errors are robust.

Table 9. School Fixed Effects Model Using Alternate Samples

	5th grade		9th grade	
	Language (1)	**Mathematics (2)**	**Language (3)**	**Mathematics (4)**
Switchers from half to FSD	0.085	0.055	0.017	-0.263
	(0.114)	(0.113)	(0.155)	(0.180)
Observations	*3,325*	*3,325*	*698*	*698*
Switchers from FSD to half	0.054	0.214*	0.079	0.306*
	(0.128)	(0.126)	(0.167)	(0.179)
Observations	*1,990*	*1,990*	*541*	*541*
Schools in 3 periods (balanced panel)	-0.024	-0.017	0.089*	0.096
	(0.041)	(0.039)	(0.053)	(0.063)
Observations	*23,181*	*23,181*	*6,951*	*6,951*
Switchers with longer treatment	0.101	-0.055	0.274***	0.292***
	(0.076)	(0.077)	(0.103)	(0.105)
Observations	*1,227*	*1,227*	*444*	*444*
Switchers with shorter treatment	0.020	0.134	-0.036	-0.030
	(0.121)	(0.134)	(0.207)	(0.182)
Observations	360	360	150	150

Source: Author's calculations based on SABER and C600.

Notes: Standard errors in parenthesis. All standard errors are robust. * significant at 10%, ** at 5 %, *** at 1%. FSD is a dummy variable equal to 1 when the school has a Full school day. School socioeconomic status (SES) is an index that goes from 1 (poorest schools) to 4 (richest schools). Only the coefficient for FSD is reported. All regressions control for school enrollment, teacher student ratio, percentage of teachers with a professional degree, percentage of teachers with pedagogic training, and include school and year fixed effects. Switchers with longer treatment are schools that were half in 2002, switched to FSD in 2005 and kept being FSD in 2009. Switchers with shorter treatment are schools that were half in 2002 and 2005, and switched to FSD only in 2009.