No. 61

OCTUBRE DE 2017

Documentos CEDE

ISSN 1657-7191 Edición electrónica.

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CEDE
CENTRO DE ESTUDIOS SOBRE DESARROLLO ECONÓMICO

Universidad de
los Andes
Facultad de Economía

Serie Documentos Cede, 2017-61
ISSN 1657-7191 Edición electrónica.
Octubre de 2017

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Impreso en Colombia – Printed in Colombia

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Longer school days, less teenage mothers:

Evidence from Colombia

Simón Borrero Escobar[†]

Abstract

This study analyzes the impact of longer school days on teenage fertility. Using administrative data of school enrollment and a national system of beneficiaries for social programs (Sisbén), I am able to identify teenagers who attended a Full School Day (7-hour schedule) or a half-day (4-hour schedule), and whether these teenagers gave birth between 2006 and 2009. I exploit plausibly exogenous within-school variation in the type of school day and estimate panel regressions with school and student fixed effects. Results indicate that attending a full-day instead of a half-day reduces teenage fertility rates by 0.6 p.p. (a 25% reduction from the mean), and that this effect is only evident in urban schools. Also, more years spent in FSD increasingly reduced teenage fertility.

JEL codes: J12, J13, I28.

Keywords: Teenage fertility, Full school days, Within-school variation.

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Jornada escolar completa y las madres adolescentes:

Evidencia para Colombia

Simón Borrero Escobar[†]

Resumen

Este estudio busca determinar el impacto de la jornada escolar completa sobre la fecundidad adolescente. Usando registros administrativos de matrícula escolar y del sistema nacional de beneficiarios de programas sociales (Sisbén), puedo identificar adolescentes que atienden a una jornada completa (7 horas) o media jornada (4 horas), y si estos adolescentes tuvieron un hijo entre 2006 y 2009. Utilizo variación plausiblemente exógena dentro del colegio en la jornada a la que atienden los estudiantes y estimo regresiones de panel con efectos fijos de colegio y estudiante. Los resultados indican que atender una jornada completa en vez de jornada media reduce la fecundidad adolescente en 0.6 p.p. (una reducción de 25% de la media), y que este efecto se da sólo a través de colegios urbanos. Adicionalmente, más años asistiendo a jornada completa reducen progresivamente la fecundidad adolescente.

Códigos JEL: J12, J13, I28.

Palabras clave: Fecundidad adolescente, Jornada completa, Variación intra-colegio.

[†] Simón Borrero Escobar, Universidad de los Andes: s.borrero1041@uniandes.edu.co. Quiero agradecerle a Adriana Camacho por su valiosa asesoría en la elaboración de este documento, al igual que a Alejandra Corchuelo y Raquel Bernal por sus comentarios como jueces de la sustentación. También quiero agradecer a Rafael Santos por su retroalimentación a versiones anteriores de este documento, y a Miguel Jiménez y Diego Amador por su colaboración con algunos de los datos usados en esta investigación.

1. Introduction

Teenage¹ fertility is a worrying situation in Colombia. According to the Demographic and Health Survey (DHS) in 2010, 19.5% of teenagers in Colombia were either pregnant or already mothers. Figure 1 compares teenage fertility rates of selected countries in Latin America and the Caribbean, showing that Colombia's is amongst the highest in the region, while also showing a significant improvement in the last number of years. Teenage births will most likely negatively affect teenagers' income and social outcomes in the future (Fletcher & Wolfe, 2008; Wolff, 2009). In addition, teenage childbearing perpetuates poverty cycles (Ulanowicz, 2006), as poor women are also more likely to become pregnant (Flórez & Soto, 2006).

Figure 1. Teenage fertility in Latin America and the Caribbean.

Source: World Development Indicators.

In reference to the above, it is of immense importance to study how different public policies may affect teenage fertility rates. With that in mind, this paper specifically evaluates the

¹ From now on, I will refer to teenagers as those individuals between 15 and 19 years old.

effect that full school days (henceforth to be abbreviated as FSD) may have on early childbearing, striving to prove that more time spent in school reduces the teenagers' likelihood of giving birth, which may happen through three channels (Black et al, 2008). First, reduction of time exposure to different risk factors, or "incarceration effect". This channel implies that as students spend more time in school, they spend less time without adult supervision, reducing their exposure to risk factors associated to teenage pregnancy. Such factors are well documented by Flórez & Soto (2013) and include contextual, interpersonal and intrapersonal factors. Amongst the interpersonal factors, there are those which include family-related, one of which is parental supervision. For instance, Jacob & Lefgren (2006), show that when teenagers are provided with a supervised environment, they are less likely to engage in certain anti-social behaviors such as committing crimes.

Both the second and third channels through which longer school days may have an impact on reducing early motherhood occur as a result of increased quality and quantity of education. First, education may increase the teenagers' conscience of the negative consequences of early childbearing, while simultaneously providing better knowledge on contraceptive methods. Consequently, education could result in better reproductive decisions. Second, increased exposure and quality of education increases expected lifetime income, thus increasing the opportunity cost of becoming a mother. The first two channels described operate in the short term, while the latter operates in the long term.

To the best of my knowledge, only four papers have assessed this issue empirically, most likely because data on teenage pregnancy is extremely sensitive information and that stating causality is a difficult task. While McCrary and Royer (2006) found no significant effects of age of entry to school policies on teenage pregnancy in the United States, Black et al. (2008) found that compulsory school laws reduced the likelihood of teenage pregnancy in two different institutional settings (United States and Norway). The only two papers assessing specifically, the effect of FSD on teenage pregnancy are Krueger & Berthelon (2009), and Pires & Urzua (2014), both of which took advantage of an exogenous growth in FSD enrollment from a Chilean educational reform in 1997. The first proxied FSD supply with municipality enrollment of FSD and concluded that increased access to FSD reduces the municipality's teenage pregnancy rates. The latter, used retrospective surveys to compare cohorts of individuals before and after the

reform which increased FSD enrollment. This research found that studying in FSD reduces teenage motherhood only amongst less educated families.

This paper contributes to the literature in at least three ways. First, it helps to understand the potential benefits of longer school days. While some research has found positive impacts on test scores (Benavot & Gad, 2007; Cerdan-Infante & Vermeersch, 2007; Abadzi, 2009; Bellei, 2009; Hincapie, 2016), early school dropout and repetition (Garcia, Fernandez & Weiss, 2012), graduation (Llach, Adroque & Gigaglia, 2009), estimated effects are small. Other papers found no impacts on employability and wages (Llach et al, 2009; Pires & Urzua, 2014). Second, this research sheds light on the expected results of the FSD policy expansion, which is currently taking place in Colombia, proving to be an additional element of discussion on this controversial topic.

Third, my research complements the scarce literature related to the specific effects of longer school days on the risk factors faced by students, in this case, teenage motherhood. In fact, this paper builds on previous literature by using individual information from administrative records, which allows for a better identification of causality and provides greater external validity. Most importantly, this is the first paper that, to my knowledge, attempts to disentangle the channels through which FSD impacts reproductive behavior.

By using administrative data containing every student registered in public schools from 2005 to 2010 and the family composition from *Sisbén*² 2010, I will deduce whether teenagers gave birth in that period of time and the type of school day this teenager attended. I will exploit plausibly exogenous variation on how schools and school grades are assigned to a full-day or a half-day. I will argue that this variation is exogenous to the individuals' reproductive decisions and that there is no systematic selection into FSD within the school, thus allowing me to identify the causal effect of FSD on early motherhood. By estimating panel regressions with municipality, year, school and student fixed effects, I find evidence suggesting that attending FSD reduces teenage fertility rates by 0.6 p.p. (25%), and that this effect is seen only in urban schools. In addition, there is no conclusive evidence that indicates whether the effect of FSD on teenage fertility is as a result of an incarceration channel or an education channel. I will also show that

² Sisbén is the System of Identification of Potential Beneficiaries of Social Programs (as per its Spanish acronym). By using socioeconomic variables, Sisbén assigns a score to each household, with which households are categorized to be beneficiaries of certain programs. The lower the Sisbén score, the poorer the household is considered.

more years attending FSD have an incremental effect on reducing teenage fertility. I will characterize the effects that the event of pregnancy and birth have on schooling decisions, and I will finally show additional exercises that support the assumption that within school variation of the school day is exogenous to students' reproductive decisions.

The rest of this document proceeds as follows: Section 2 describes the institutional setting of FSD in Colombia, Section 3 describes the empirical methodology and Section 4 describes the data used its source. Section 5 presents the main results, while Section 6 addresses robustness checks, threats to validity and heterogeneous effects. Finally, Section 7 briefly presents some concluding remarks and Section 8 lists some specific policy recommendations.

2. Full School Days in Colombia

Implementation of FSD is a controversial topic amongst Colombian policy makers and academics. Considerations of the costs associated with the expansion of FSD and the expected results of this expansion are at the center of this debate. While Colombia has not implemented an official extension of the school day towards achieving universal FSD enrollment, some municipalities have made efforts in this direction (García, Fernandez & Weiss, 2012). Therefore, while there was no specific FSD policy before 2015, there are several schools that had FSD.

In 1994, Law 115 was enacted to improve education quality in Colombia. This law mandated every school to implement FSD. However, implementation was slow and the law was derogated in 2002, largely due to its costly implementation. Nevertheless, certain schools and school grades have FSD, and around 12% of students attend FSD. At the time, schools offering FSD designated this additional time to educational, cultural and sporting activities, and not necessarily with additional courses.

In 2014, the National Development Plan (articles 57 and 60) stated that all schools should gradually implement FSD. Universal FSD enrollment should be achieved by 2025 in urban areas and by 2030 in rural areas and the National Government must finance this expansion. These articles were further regulated through Decree 501 of 2016. By implementing this FSD expansion, the Ministry of Education argues that one of the objectives of this policy is to reduce students' exposure to risk and vulnerability factors in their free time. Thus, one explicit result of implementing FSD could be the reduction of crime and teenage fertility. Finally, I must

stress that this paper does not intend to evaluate the specific FSD expansion policy that started in 2014.

As Hincapié (2016) states, the selection of schools to participate in FSD is a very complex process. Currently, each municipality has some freedom on determining schools' supply of FSD spots, depending on the "expected demand for school spots, and the capacity that schools in that municipality have to offer school spots" (pg. 7). Additionally, each municipality's Secretary of Education must consider not only children currently enrolled, but also those who are not, to increase overall enrollment (Ministerio de Educación, 2006).

Lastly, parents also participate in the determination of the length of the school day. Through a registration process, they express their preferences between morning and afternoon schedules as well as their preferences for two schools. While they cannot explicitly express preference for a full school day, they may have information on which school usually offers FSD spots. Parents' preferences are considered and the Secretary of Education finally assigns students to their respective schools, which may or may not reflect the initial parental preference of school. As a result of this complex process, there is a lot of variation of FSD enrollment at municipality level. In addition, not all school grades in each school may be assigned FSD, meaning that not only is there variation in FSD between schools, but within schools and school grades as well.

3. Empirical strategy

Identifying the causal effect of FSD on teenage fertility is not a trivial task since there might be endogeneity problems arising from non-random selection into FSD. Students with certain observable and unobservable characteristics are more likely to attend a full day school. For instance, it may be that proactive parents make a bigger effort to get their children into a FSD school and these parents are also better at educating their children into avoiding early childbearing. It may also be the case that these parents have a busier schedule, which may also indicate carelessness. In addition, there may also be selection at the school level, since schools may select into FSD while also confounding the probability of their students of getting pregnant and giving birth. For example, one municipality's Secretary of Education may select one school to turn into a full day school based on the quality of their staff, and this difference may also influence their students' reproductive decisions. Consequently, ordinary least squares estimates

may capture some of this selection, yielding biased estimates of the causal effect of FSD on teenage motherhood.

In an attempt to solve this selection issue, I will utilize within-school variation of FSD amongst grades. As a result of the process through which municipalities assign FSD spots, within the same school, some school grades may have a half day while others may have a full day. The underlying assumption is that variations of FSD amongst different school grades in the same school is exogenous to individuals' reproductive decisions. This assumption is plausible considering that FSD spots are usually assigned based on decisions of each municipality's Secretary of Education, and mostly reflect supply and demand factors. I will demonstrate that variations in FSD within the school can be considered exogenous to both the individual and the school itself. Specifically, since only variations within the same school is considered, the bias that arises from selection into the school is eliminated.

This empirical strategy has been previously used by Clotfeld, Ladd & Vigdor (2010), Rivkin, Hanushek & Kain (2005) and Hanushek & Rivkin (2008) to cleanse their studies from the biases that emerge from selection into schools. The first studies how teacher credentials affect student achievement, the second study analyzes the impact of school and teacher quality on academic achievement, and the latter studies how schools affect the black-white achievement gap. The first study uses school and student fixed effects, and explain how school fixed effects mitigate the bias that arises from non-random selection across schools, while student fixed effects deal with the biases emerging from non-random selection within schools. The last two studies estimate a school and student fixed effects model, and argue that remaining differences in their outcome variables can be explained by differences in teachers and schools. They mention that school-by-grade fixed effects would control "any systematic differences across grades in a school that are common to all years, including elements of curriculum [...]" while school-by-year fixed effects captures systematic year-to-year differences that are common to all grades, while also addressing systematic variation over time (Hanushek & Rivkin, 2008, pg 14). Together, these fixed effects will account for systematic differences amongst schools, such as schools' institutional background. Also, student fixed effects address unchanging student attributes such as motivation, or family and cultural background, while municipality fixed effects will control for the municipalities' size, as well as social, cultural and religious differences;

Similarly, I estimate a school, year and student fixed effect model. In order to rule out selection into schools and to account only for within-school variation of FSD, only students that didn't switch schools will be considered in the estimations. I also compliment this specification by adding municipality fixed effects and time varying controls. This specification is depicted in equation (1).

$$(1) \quad b_{i,t} = \delta FSD_{i,t} + \beta X_{i,t} + \vartheta M_{i,t} + \gamma S_{i,t} + \omega_m + \theta_i + \rho_s + v_t + \varepsilon_{i,t}$$

In this equation, $b_{i,t}$ is the probability that a teenager gives birth in a given year t . $FSD_{i,t}$ is a dummy variable that is equal to one, if the school-grade that individual belongs to is a full day. $X_{i,t}$ is a set of individual controls. $M_{i,t}$ controls at the municipality level. $S_{i,t}$ are school controls, ω_m , θ_i , ρ_s , v_t are municipality, student, school and year fixed effects, respectively. $\varepsilon_{i,t}$ is an error assumed to be distributed $N(0, \sigma^2)$, which conditional on the explanatory variables and fixed effects should be uncorrelated with the FSD variable. My parameter of interest will be δ , which should be interpreted as the impact of studying in a full-day grade instead of a half-day, on the likelihood of giving birth as a teenager. I will be sequentially including the individual fixed effects to check how my parameter of interest changes upon their inclusion, since the individual fixed effect absorbs many of the selection bias. As an additional estimation, I will include school-by-grade and school-by-year fixed effects instead of school and year fixed effects as proposed by (Hanushek & Rivkin, 2008). For this research, this specification represents a great imposition on the data, since I would be demanding FSD variation within the same grade, which rarely happens. Finding a significant effect through all the specifications will give me stronger evidence that FSD is indeed impacting teenage fertility rates.

In this regression, I will be controlling for individual characteristics at 2005, prior to the period of analysis in the estimations. By doing so, these characteristics will not be determined by reproductive or schooling decisions during the 2006-2009 period. I will also be controlling time-varying observable variables at both the school and municipality level. Nevertheless, I may be omitting unobservable variables of the individual, the school or the municipality that may confound with FSD treatment or reproductive decisions of the individual which may bias my estimations. These differences, if time invariant, will be controlled by the fixed effects included. I will try to address these issues as best as possible.

Individual controls to be included are age, age squared, zone of residency and Sisbén score. Amongst them, the most important control is age, as it is a determining factor of fertility. This is intuitive and consistent with the literature, because as the teenager grows, she is more exposed to risky sexual behavior and less under the control of her parents, which results in a higher implied probability of pregnancy (Flórez & Soto, 2013). Controlling for zone of residency, addresses the fact that the quality or quantity of FSD may be different amongst rural and urban areas. Additionally, Sisbén score works as a wealth control, which may have a negative effect on fertility, most likely through increased access to contraceptive methods.

Municipality controls also respond to an important economic rationality. I include: the number of schools in the municipality, taxing income, health and education transferences from the national government per capita, and child mortality and fertility rate. Number of schools control for the availability of education to students, while the number of teachers helps address differences in educational quality between municipalities. Both quality and quantity may confound the effect that FSD has on reproductive decisions. Taxing income per capita is a good proxy for the municipalities' wealth; that and the municipalities' transferences for health and education help to account for different public policies that may be related to FSD or reproductive decisions. This helps to rule out the possibility that the estimated coefficient may be capturing the effect of a different public policy. Child mortality and fertility rate account for municipalities' differences in child-bearing and child care.

Observable school characteristics I will be controlling for include: the number of students, teacher-student ratio, male-female students' ratio, proportion of teachers for each level of education, the standardized mean score for the school in the ICFES³ test, and dichotomous variables for urban and mixed schools. The number of students in the school controls school size, which may influence interpersonal relationships and teachers' control over students. Teacher-student ratio also controls for teachers' control over students but most importantly for school quality. Male-female ratio proxy for interactions amongst opposite sex classmates. The proportion of teachers by each level of education control for school quality, as does the standardized score in the ICFES test. These time-variant characteristics will help address

³ The ICFES test, currently named SABER test, is a standardized evaluation that every student must take prior to graduation. This test is intended to measure student achievement and school quality.

differences in the quality of the school that may determine some selection into the school or that may confound the effect of FSD on early childbearing.

4. Data

This paper uses two main sources of administrative data. The quality of the data and how each variable is constructed are important features of this research since they give credibility to the results. First, I used the R166 database from 2005-2009, which contains census information on all enrolled students in public schools in the country starting in 2005. Second, I used the Sisbén database on 2005 and 2010. In the next two subsections I will describe this information, as well as characterize the sources of variation I will exploit in the estimations. This paper benefits from using these sources of administrative data since it improves the estimations' efficiency while also providing greater external validity to the results.

4.1. *Census of students in public schools*

The Ministry of Education collects information on all students enrolled in the country since 2005. This database, known as the R166⁴, allows me to identify my variable of interest: the type of school day. Besides the type of school day, the R166 database provides information on the student's national identification number, age, current school grade, socio-economic strata, and zone of residency. I will omit 2005 from the econometric analysis since, being the first year of the report, the quality of the information is not trustworthy.

Figure 1 reports national enrollment and the percentage of schools by type of school day. Around 72% of schools have a half-day, while 26% of schools have a full-day. The remaining 2% are mixed within half and full days; in fact, these schools will provide the source of variation that I will be utilizing.

⁴ In 2004, the Ministry of Education decreed the *Resolución 166*, which mandated that all education institutions must report, amongst other information, all their students enrolled individually. The R166 is currently known as SIMAT (National Enrollment System, as per its Spanish acronym).

Figure 1. Overall enrollment and schools by type of school day.

Source: Own calculations based on R166.

Table 1 shows several characteristics that help to understand the sources of variation described in Section 3. This table quantifies how many students stayed in FSD, half-day and moved from FSD to half-day school or the other way around, for those students who didn't change school in the period of analysis. Column (3) and (4) portray the exogenous variation described in the previous section. From this information, one can confirm that students are indeed switching their school schedules inside the same school. Students switching from half-day to full-day or from full-day to half-day within the same school represent around 2.0% of all students.

Table 1. School-day variation amongst students who stayed in the same school.

YEAR	Stayed in FSD	Stayed in Half Shift	Half Shift->FSD	FSD->Half Shift	Total
2006	456,746 12.7%	3,039,652 84.7%	41,695 1.2%	50,221 1.4%	3,588,314 100.0%
2007	506,640 13.4%	3,231,983 85.5%	17,056 0.5%	22,661 0.6%	3,778,340 100.0%
2008	453,651 12.9%	2,970,393 84.6%	16,867 0.5%	68,720 2.0%	3,509,631 100.0%
2009	495,376 12.1%	3,545,408 86.3%	53,600 1.3%	15,572 0.4%	4,109,956 100.0%

Source: Own calculations based on R166.

4.2. Sisbén.

The Sisbén is often thought of as a census of the poor. Sisbén aims to provide a technical mechanism with which to identify every potential beneficiary of social programs. This is done through surveys that contain very detailed questions about households' socioeconomic characteristics, with which Sisbén creates a score through which households are ranked and assigned to different social programs. This paper uses information from a survey scan that started in 2008 and ended in 2011, since this was the latest Sisbén database available to me. In addition, I must stress that the external validity of the results only apply to Sisbén households.

The main objective of using Sisbén is to use their detailed information about family composition in order to create the teenage birth rate variable. Defining whether a woman gave birth in her teenage years is not a trivial task, since the Sisbén does not specifically ask reproductive questions. I create the teenage pregnancy variable from household composition information. Specifically, the survey has questions for every household member, whether that household member is a child of someone else in the household and of whom. With that information, I can deduce which women in the household are mothers, and based on the age of the child, I am able to identify when the woman gave birth to her child and flag them as a teenage mother if that is the case.

Several considerations must be made with regards to this variable. First, this variable would show if the teenager gave birth in a given year, and does not reflect whether that teenager was pregnant in that same year, had been pregnant before, or had a miscarriage or abortion. As

such, this variable should be interpreted as the teenage fertility rate⁵, and should not be confused with the teenage pregnancy rate. This variable should be interpreted as the probability that a teenager gives birth in a given year. Second, I could be omitting births from a woman in the household, whose children are not a member of the household at the moment of the survey, or children who were assigned to another person in the household different to the biological mother. This becomes an issue when dealing with Sisbén because there may be incentives for the teenager to report their child as if it was their parents' in order to fake higher dependency ratios, get lower Sisbén scores and therefore be beneficiated by more social programs. This may also happen as a mistake from the administrator of the poll, since, for simplicity of the survey, they may be assigning the teenager's child to the household head instead. Third, most of the surveys were carried out during 2009 and 2010; therefore, I could be omitting births that took place after the survey in that same year. This means that the estimated fertility rates are more reliable from 2009 backwards.

To address the robustness of this variable, Table 2 compares the teenage fertility rate I estimated using Sisbén, with the one calculated from the Demographic and Health Survey (DHS) for Colombia in 2005 and 2010. In addition, I also obtained a trimmed subsample of the DHS, limited to lower and middle income households. This will provide higher comparability with the Sisbén households. In 2005, my estimated variable differs from the DHS and DHS trimmed by only about 0.1 and 0.5 p.p., respectively. Whereas, the 2010 variable differs by 5.2 and 6.7 p.p. from the DHS and DHS trimmed variable, respectively. For this reason, I will omit 2010⁶ in my estimations. Despite this, measurement error of the dependent variable is still a concern. However, such measurement error is most likely uncorrelated with the FSD variable, and this issue would not bias my estimations and would only result in bigger standard errors and lower statistical significance.

⁵ Teenage fertility rate is the percentage of births during teenage years (15-19) over the number of women in the same age group. Teenage pregnancy rate is the percentage of teenagers that are or have been pregnant in a given year. Although conceptually different, these variables are highly correlated. The estimated Pearson correlation coefficient for these variables using data from the World Bank is 0.919, significant at the 1% level.

⁶ As mentioned in the beginning of this subsection, several interviews were carried out throughout 2010, so I will be omitting births that happened later that year. This is the reason for the severe sub report in the 2010 teenage fertility rate.

Table 2. *Sisbén and DHS teenage fertility rate.*

Year	SISBEN	DHS	DHS (trimmed)
2005	8.27%	8.17%	8.79%
2006	8.12%		
2007	8.08%		
2008	7.94%		
2009	5.71%		
2010	1.61%	6.85%	8.32%

Source: Own calculations based on Sisbén 2005 and 2010, and DHS 2005 and 2010.

As complementary sources of information, I use the municipality panel from CEDE⁷-Universidad de Los Andes and the C600⁸ school panel. After merging the aforementioned databases, I am able to compare students attending half-days with those attending full-days.

4.3. Descriptive statistics.

Table 3 presents a mean difference test over several variables for all the individuals that were both in the R166 and Sisbén. Here, I will be comparing students from the three groups shown in Figure 1: those attending schools who always had a half-day, those who always had a full-day, and those who had within school variation of the length of their school day. This comparison will allow me to show two things. First, that there is indeed selection into FSD, which is why the empirical strategy described in the previous section becomes so relevant. Second, that those students in school with within-school variation also represent a selected sample.

There is a significant difference in the unconditional mean of the students' reproductive rate between full-day and half-day schools, which provides preliminary evidence that FSD may be reducing teenage pregnancy rate. In addition, these differences show that FSD students are older, live in more rural areas (as seen through the individual's zone of residency and the municipality's rurality index), are wealthier and live with less educated and older household heads. FSD schools outscore half-day schools in the ICFES test, despite having less teachers per

⁷ CEDE, at Universidad de Los Andes, compiles a wide range of information from different administrative datasets at municipality level.

⁸ The C600 is a census of schools provided by DANE (National Department of Statistics, as per its Spanish acronym).

student and less teachers with postgraduate degrees. These differences elicit that there is indeed selection into FSD.

Table 3. Mean difference test.

		Half-shift		Full-shift		Mixed		Difference	
VARIABLE		N	Mean	N	Mean	N	Mean	Mixed-HS	Mixed-FSD
Individual	Birth	228,196	0.024	93,529	0.022	24,640	0.028	0.004***	0.006***
	Age	228,196	16.547	93,529	16.569	24,640	16.533	-0.014**	-0.036***
	Urban	228,196	0.588	93,529	0.519	24,640	0.616	0.028***	0.097***
	Sisben score	228,196	15.290	93,529	16.467	24,640	14.268	-1.022***	-2.199***
	Adults in the household	228,196	2.395	93,529	2.375	24,640	2.353	-0.042***	-0.022***
	Children in the household	228,196	1.086	93,529	1.129	24,640	1.112	0.025***	-0.018**
	Household head age	228,196	44.397	93,529	44.733	24,640	43.816	-0.581***	-0.917***
	Household head years of education	228,196	5.740	93,529	5.425	24,640	5.703	-0.038	0.278***
	Household head woman	228,196	0.250	93,529	0.237	24,640	0.262	0.013***	0.025***
School	Gender mixed school	227,769	0.971	93,452	0.978	24,513	0.984	0.012***	0.006***
	Urban school	227,769	0.781	93,452	0.757	24,513	0.719	-0.062***	-0.038***
	Proportion of teacher with graduate degrees	227,347	0.312	92,840	0.305	24,471	0.234	-0.078***	-0.071***
	Standardized ICFES score	227,769	0.139	93,452	0.160	24,513	0.198	0.059***	0.038***
	Propotion of male students	218,010	0.416	90,428	0.419	23,833	0.432	0.016***	0.013***
	Teachers per student	227,769	0.704	93,452	0.568	24,513	0.695	-0.009*	0.127***
Municipality	Education transferences per capita	225,329	66.412	92,667	55.442	24,279	64.292	-2.120	8.85***
	Health transferences per capita	225,329	211.794	92,667	247.607	24,279	98.658	-113.135***	-148.948***
	Taxing income per capita	228,196	0.126	93,529	0.118	24,640	0.182	0.057***	0.064***
	Schools per capita	228,196	0.002	93,529	0.002	24,640	0.002	0.000***	-0.001***
	Rurality index	228,196	0.470	93,529	0.534	24,640	0.397	-0.072***	-0.137***
	Child mortality rate	228,196	18.656	93,529	18.692	24,640	18.822	0.166***	0.130***
	Fertility rate	228,196	0.015	93,529	0.015	24,640	0.015	0.000*	0.000***

Source: Own calculations based on R166, Sisbén, C600 and CEDE's municipality panel.

Those students in mixed schools are also systematically different to those in FSD and mixed schools. They have a higher fertility rate, live in more urban areas and poorer households, outscore other schools despite having less educated teachers. Also, these schools are located in apparently wealthier and more urban municipalities. Differences between these groups, if unattended, would render biased estimates. In fact, these differences may result in lack of overlap amongst the treatment and control groups, resulting in imprecise estimates of the average treatment effects. I follow Crump et al. (2009), to deal with this issue and estimate the propensity score of attending FSD. I discard all observations outside the [0.1, 0.9] range, obtaining a trimmed (more balanced) sample which approximates an optimal selection rule.

5. Results.

To implement the empirical strategy described in Section 3, one must first demonstrate that FSD variation inside the school is indeed exogenous to the individuals' characteristics. To do so, I estimate a regression where FSD is explained by all the available control variables restricted to those students who didn't switch schools during the period of analysis⁹. This sample will be used in all further estimations. Results exhibited in Table 4 indicate that by only taking into consideration students who remained in the same school, there is no systematic selection into FSD based on observable characteristics of the student and their household.

Table 4. Selection of FSD within the school.

VARIABLES	(1) FSD	(2) FSD	(3) FSD
Age	0.00999 (0.00659)	0.00208 (0.0109)	0.00924* (0.00492)
Age squared	-0.000282 (0.000192)	-3.75e-05 (0.000269)	-0.000266* (0.000142)
Urban zone	-0.000144 (0.000500)	8.178 (968.3)	4.11e-05 (0.000323)
Sisben score	3.66e-05 (2.42e-05)	-0.0986 (76.13)	1.28e-05 (1.69e-05)
Number of adults in the household	0.000372* (0.000196)	0.697 (619.6)	0.000161 (0.000130)
Number of children in the household	-0.000116 (0.000175)	-1.093 (229.2)	1.20e-06 (0.000116)
Household head age	4.16e-07 (2.00e-05)	0.00934 (13.79)	1.39e-05 (1.54e-05)
Household head years of education	9.59e-05 (6.90e-05)	-0.376 (119.8)	5.71e-05 (5.79e-05)
Household head gender	7.43e-05 (0.000565)	-5.591 (2,175)	-0.000263 (0.000476)
Observations	326,306	271,360	323,680
R squared	0.954	0.978	0.985
Municipality fixed effects	X	X	X
Year fixed effects	X	X	X
School fixed effects	X	X	
School-by-grade fixed effects			X
School-by-year fixed effects			X
Individual fixed effects		X	

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. In addition to the individuals' controls shown, the regression controls for schools and teacher per capita, taxing income, health and education transferes per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies.

⁹ Since this sample only considers students who never switched schools, the number of observations differs from those shown in Table 3.

Table 5 presents the results of estimating equation (1). Through these estimations, different fixed effects are included. Column 1 presents a regression with municipality, year and school fixed effects. Column 2 adds individual fixed effects, and Column 3 includes school-by-grade and school-by-year fixed effects instead of school fixed effects. All standard errors are clustered at the school level.

All the specifications yield negative impacts of FSD on teenage fertility, specifications shown on Column 1 and 3 yield significant effects, while Column 2 yields a marginally significant effect. Altogether, results suggest that attending FSD is indeed reducing teenage fertility through either of the described channels. Estimated effects range from 0.58 to 1.64 p.p. reduction of teenage fertility rate, which represents a reduction of 25% to 73% over a 0.023 mean in teenage fertility rate¹⁰. This is a noteworthy reduction, considering that from 2000 to 2010, teenage fertility was reduced by 29.4% (from 8.5% to 6% in the overall rate).

Table 5. Impacts of FSD on teenage fertility rate.

VARIABLES	(1) Birth	(2) Birth	(3) Birth
FSD	-0.00583** (0.00287)	-0.00567 (0.00358)	-0.0164*** (0.00544)
Observations	326,306	271,360	323,680
R squared	0.071	0.433	0.138
Mean of dependent variable	0.0234	0.0206	0.0233
Percentual effect	-25.0%	-28.0%	-73.4%
Individual controls	X	X	X
School controls	X	X	X
Municipality controls	X	X	X
Municipality fixed effects	X	X	X
School fixed effects	X	X	
School-by-grade fixed effects			X
School-by-year fixed effects			X
Individual fixed effects		X	

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All regressions control for age and age squared, zone of residency, sisben score, schools and teacher per capita, taxing income, health and education transferences per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies.

¹⁰ After merging the databases, teenage fertility rate is lower compared to the rates presented in Table 1. This happens because the year teenagers become pregnant, they are less likely to be enrolled and thus will not be observed in the R166 database.

Beforehand, my preferred specification is depicted in Column 1, as it utilizes within-school variations of FSD and does not require individual variations of FSD and teenage births. Column 2 shows the clearest specification, as it includes individual fixed effects. As mentioned in Section 3, individual fixed effects will control for selection given by time-invariant characteristics that may confound the probability of giving birth and attending FSD. However, these fixed effects will be using variation of the school day and the birth variable at individual level, representing a strong imposition on the data, which makes it difficult to identify a significant effect in this specification. In addition, the fact that this specification yields the smallest estimate indicates that the selection being controlled by individual fixed effects biases the estimate for FSD upwards. The specification in Column 3 uses variation within the school-grade, which also has a strong imposition on the data. Here, I compare students in the same school, one which is attending a half-day with one attending a full-day; thus the high estimated coefficient. For the reasons above, this paper will base its conclusions on the results depicted in Column 1.

Finally, one must argue the validity of these results. For the reasons discussed thoroughly in Section 3, these estimations rule out most sources of endogeneity, providing confidence in the internal validity of the results. However, the external validity is somewhat more limited. These estimations were made only about students belonging to Sisbén households, which belong to the lowest distribution of income. These households may be systematically different to wealthier households, and these results should not be extrapolated to such households.

6. Robustness checks.

6.1. Potential channels.

Results presented in the previous section provide evidence indicating that FSD is indeed reducing teenage fertility. However, the channels through which this happens remain unveiled. To shed light on whether the channel through which this effect is occurring is either incarceration or education, I estimate the approximated date of conception¹¹ and whether the conception took place during vacation or school time. This approximation indicates that 71.3% of the conceptions took place during school time, while 75.1% of the year is school time. This

¹¹ The date of conception is built from the date of birth lagged 9 months.

indicates that the number of conceptions that take place in school time is not proportional to the amount of time spent in school, indicating that school may cause an incarceration effect that reduces pregnancies.

This approximation has at least two issues. First, pregnancies may occur on weekends during school time. Since it is impossible to identify the exact day of conception, I cannot correct for this. As a result, conceptions that took place during school days could be severely overreported. Second, all pregnancies may be underestimated in the panel, since one cannot account for abortions. These issues may be biasing the amount of pregnancies that are actually taking place during school time and vacations. To elicit the differences of FSD between these pregnancies, I estimated a modified version of equation (1) that includes an interaction term between FSD and the conception during vacation variable (CDV), as depicted in equation (2).

$$(2) \quad b_{i,t} = \delta FSD_{i,t} + \vartheta FSD_{i,t} * CDV + \pi CDV + \beta X_{i,t} + \vartheta M_{i,t} + \gamma S_{i,t} + \omega_m + \theta_i + \rho_s + v_t + \varepsilon_{i,t}$$

The intuition here is that if the incarceration effect is dominating, then FSD should reduce teenage pregnancies that took place during school time and not during vacations ($\vartheta = 0, \delta < 0$). If the only channel operating is the education channel, then one would expect $\vartheta < 0, \delta = 0$. If both type of pregnancies are being reduced by FSD ($\vartheta < 0, \delta < 0$), then one cannot rule out any of the named channels. The results of these estimations are shown in Table 6. Results indicate that FSD is reducing both type of pregnancies. This evidence is not conclusive about which of these channels is dominating, suggesting that both channels may be assisting FSD to reduce teenage fertility.

Table 6. FSD and pregnancy during vacation.

VARIABLES	(1) Birth	(2) Birth	(3) Birth
FSD	-0.00501** (0.00255)	-0.00573 (0.00390)	-0.0144*** (0.00437)
FSD*CDV	-0.0694*** (0.00811)	-0.0583*** (0.0129)	-0.0836*** (0.00807)
Observations	326,306	271,360	323,680
R squared	0.106	0.439	0.164
Mean of dependent variable	0.0234	0.0206	0.0233
Municipality fixed effects	X	X	X
Year fixed effects	X	X	X
School fixed effects	X	X	
School-by-grade fixed effects			X
School-by-year fixed effects			X
Individual fixed effects		X	

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All regressions control for age and age squared, zone of residency, sisben score, schools and teacher per capita, taxing income, health and education transferences per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies.

6.2. Intensity of treatment.

One important notion regarding the effect of FSD is that more years spent in FSD will incrementally reduce teenage fertility; therefore, one could estimate the intensity of treatment. I estimate a regression using years of exposure to FSD dummies. These estimators reflect the marginal effect of having studied t years in FSD, compared to the base of zero years of FSD. If the estimators become progressively bigger for more years of FSD, this would provide evidence in favor of an accumulated effect of FSD on teenage fertility.

One should also differ out students that were observed in the panel in different years. Therefore, I control for the cohort in which the student is first observed in the panel. This specification is shown in equation (3), where T_i is the cohort in which the student was first observed in the panel; results are shown in Table 7. Estimated results provide evidence of a non-linear effect of FSD; additional years attending a full day school progressively reduce teenage fertility. Results for the clearest specification (Column 2) indicate that while one year attending FSD reduces teenage fertility by 0.7 p.p., studying for four years in FSD reduce it by 1.4 p.p.

$$(3) \quad b_{it} = \delta \sum_{t-T}^t FSD_{i,t} + \beta X_{it} + T_i + \vartheta M_{i,t} + \gamma S_{i,t} + \omega_m + \theta_i + \rho_{s,t} + v_{g,s} + \varepsilon_{it}$$

Table 7. Intensity of treatment.

VARIABLES	(1) Birth	(2) Birth	(3) Birth
1 year in FSD	-0.00628*** (0.00230)	-0.00779** (0.00372)	-0.00787*** (0.00241)
2 years in FSD	-0.0161*** (0.00242)	-0.00992** (0.00391)	-0.0191*** (0.00244)
3 years in FSD	-0.0228*** (0.00293)	-0.0104** (0.00438)	-0.0259*** (0.00285)
4 years in FSD	-0.0327*** (0.00498)	-0.0148** (0.00618)	-0.0372*** (0.00479)
Observations	510,562	504,473	505,433
R squared	0.097	0.332	0.185
Mean of dependent variable	0.0403	0.0401	0.0400
Municipality fixed effects	X	X	X
Year fixed effects	X	X	X
School fixed effects	X	X	
School-by-grade fixed effects			X
School-by-year fixed effects			X
Individual fixed effects		X	

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All regressions control for age and age squared, zone of residency, sisben score, schools and teacher per capita, taxing income, health and education transferences per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies, and cohort dummies.

6.3. Heterogenous effects by area.

Schools work very differently in urban and rural areas. Urban schools are usually more densely populated and have better qualified teachers. Consequently, it is important to understand how the effect of FSD on teenage pregnancy works in both areas. To do so, I estimate differentiated regressions between both areas; results are shown in Table 8. These estimations indicate that the effect of FSD on teenage fertility is driven entirely by urban schools, which represent 70% of the FSD enrollment. From this result, it could be inferred that there are many other channels, besides incarceration and education, that are driving teenage fertility in rural areas. Thus, studying FSD is not significant enough to change such fertility trends in rural areas.

Table 8. Heterogenous effects by school area.

VARIABLES	(1)	(2)	(1)	(2)
	Urban areas		Rural areas	
FSD	-0.00790** (0.00346)	-0.00364 (0.00445)	-0.000976 (0.00468)	-0.00794 (0.00581)
Observations	255,478	212,752	70,792	58,199
R squared	0.067	0.434	0.105	0.434
Mean of dependent variable	0.0225	0.0198	0.0268	0.0232
Municipality fixed effects	X	X	X	X
Year fixed effects	X	X	X	X
School fixed effects	X	X	X	X
Individual fixed effects		X		X

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All regressions control for age and age squared, sisben score, schools and teacher per capita, taxing income, health and education transferences per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies.

6.4. Correcting for misallocations of children in the household.

As explained in Section 4.2, misallocations of children inside the household, whether if it is from a survey mistake or an incorrect report from the household head, is a concern in this research. Therefore, I attempt to solve this issue by getting rid of households which have presumably made this mistake. To do so, I take those births from women aged 40 or more, who live in a household with another woman more likely to have given birth to that child, specifically aged 15-27. As a result of this process, I found 89,363 possibly mistaken households in the Sisbén database, which represent 353,762 people. From these people, 6,094 were students in the sample being used in estimations, which represent 18,747 observations. After dropping these observations, I estimated equation (1). Results are depicted in Table 9. As shown, while the estimated coefficients are smaller than those shown in Table 5, the effects remain the same. Therefore, results are not driven by these presumably mistaken households.

Table 9. Estimations correcting for wrong assignments of children in the household.

VARIABLES	(1) Birth	(2) Birth	(3) Birth
FSD	-0.00483* (0.00268)	-0.00656 (0.00475)	-0.0119** (0.00473)
Observations	307,559	252,613	304,933
R squared	0.074	0.447	0.137
Mean of dependent variable	0.0226	0.0215	0.0225
Percentual effect	-21.4%	-30.5%	-52.9%
Municipality fixed effects	X	X	X
Year fixed effects	X	X	X
School fixed effects	X	X	
School-by-grade fixed effects			X
School-by-year fixed effects			X
Individual fixed effects		X	

Standard errors clustered at the school level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All regressions control for age and age squared, zone of residency, sisben score, schools and teacher per capita, taxing income, health and education transferences per capita, municipality's child mortality and fertility rate, number of students per school, teacher-student ratio, male-female ratio, teachers' education level, ICFES score, urban and mixed school dummies.

6.5. Schooling effects of teenage pregnancy.

Previous sections have discussed how studying for a full-day influences reproductive decisions. However, it is also very important to understand how reproductive decisions affect schooling. I will try to characterize the group of students who were pregnant in any of the years considered in this research, the year they became pregnant (t), the year before the conception ($t - 1$), and the year after the pregnancy, which will most likely be the year of birth ($t + 1$). I will categorize the schooling status of these students in a given year as; not studying¹², attending a half day or attending a full day.

The characterization depicted in Table 10 shows that there is a noteworthy change in the schooling status of students in the year they become pregnant and that this change accentuates the year they give birth. The year before the pregnancy, 14.2% of the students considered were already not enrolled, 73% were attending a half-day and 12.8% were attending a full-day. The year they became pregnant, those students not enrolled represented 23.3%, the amount of those attending a half-day fell to 65.8% and the amount of those in full-day school fell to 10.9%. One

¹² The data doesn't distinguish students not studying from those who graduated. To take this into account, these calculations were made for students whose age was seventeen and a half or less in the given year, considering that this is the usual age of graduation.

year later, most likely the year of birth, the scenario was even worse. 54.2% of the students considered were not enrolled, 40.1% were attending a half-day and 5.7% were attending a full-day. This evidence indicates that 9.1% of students had to leave school because of the pregnancy and an additional 30.9% left because of the birth. In addition, I performed a mean difference test for each schooling status between the years t and $t+1$. This test also suggests that the increase in students not studying in the year of pregnancy and birth compared to the year prior to the pregnancy is significant at the 1% level. Analogously, the decrease in students attending a half and full day of school is also significant.

Table 10. Schooling decisions characterized by fertility events.

Schooling status	Pregnancy		
	t-1	t	t+1
Not studying	14.2%	23.3%	54.2%
Half Shift	73.0%	65.8%	40.1%
Full Shift	12.8%	10.9%	5.7%

Source: Own calculations based on R166 and Sisbén. Only students who were pregnant at least once in the period of the study were considered.

6.6. Threats to validity.

To show that the estimations account for a causal effect of FSD on teenage fertility, one must discuss the possible threats on validity that the empirical strategy faces. Specifically, there are two key discussions worth having regarding the exogeneity of the variation used in this research. First, one must explore whether schooling decisions are truly exogenous to reproductive decisions. In the previous section, Table 10 showed that *ex-post*, schooling decisions are not exogenous to fertility decisions. Table 11 seeks to demonstrate that *ex-ante*, schooling decisions are indeed exogenous to reproductive decisions. Here, I compare those individuals that were pregnant at least once in the panel, the year prior to their pregnancy (also shown in Table 10) with those individuals who were never pregnant in the panel. This comparison suggests that, prior to the pregnancy, schooling decisions amongst these individuals are very similar, supporting the idea that FSD variation within the school is clear of selection bias.

Table 11. *Ex-ante exogeneity of schooling decisions.*

Schooling status	Year prior to pregnancy	Never pregnant			
		2006	2007	2008	2009
Not studying	14.2%	5.7%	14.3%	18.6%	11.8%
Half Shift	73.0%	80.8%	73.3%	69.9%	75.4%
Full Shift	12.8%	13.5%	12.4%	11.5%	12.8%

Source: Own calculations based on R166 and Sisbén.

A second consideration worth giving regard to is the variation used in this paper and whether FSD variation within schools follows a specific pattern or rule. If that was the case, then I may not be capturing the effect of FSD but of some other phenomenon inside schools. For instance, it could be the case that schools always switched to FSD in the final grades, or that every second grade in the school had FSD, then this could be biasing my results.

Table 12 characterizes school day behavior between grades in those schools that have FSD variation within. These calculations show the 2007-2009¹³ averages of the three school day statuses for each grade. Overall, there are no distinguishable patterns in this variation. FSD variation goes both ways, from full to half days and the other way around, throughout all the school grades. In fact, one can only notice two significant breaks. The most notable shift happens in sixth grade, when many school switch their school days, although they shift in both directions. The other important shift happens in tenth grade, when around 7% of these schools switched from a half to a full day. Nevertheless, the conclusion from this characterization remains that within school variation of FSD does not follow distinguishable patterns.

¹³ This table shows transitions compared to the previous year, thus the first year of the panel (2006) is not usable in the calculations.

Table 12. School day behavior in schools with FSD variation.

Grade	Stayed in the same shift	Full-Shift -> Half-Shift	Half-Shift -> Full-Shift	Total
2	58.7%	23.5%	17.7%	100%
3	62.8%	21.3%	15.9%	100%
4	64.7%	19.8%	15.5%	100%
5	65.2%	18.4%	16.4%	100%
6	38.2%	35.1%	26.6%	100%
7	61.3%	19.1%	19.5%	100%
8	65.2%	18.0%	16.8%	100%
9	66.9%	15.4%	17.7%	100%
10	60.1%	16.5%	23.4%	100%
11	67.1%	15.5%	17.4%	100%

Source: Own calculations based on R166. These calculations only consider schools that showed FSD variation within.

7. Concluding remarks.

Implementation of Full-School Days in Colombia is still a controversial topic amongst policy makers and academics. On one hand, several studies argue that attending FSD improves student achievement, however, the evidence is not conclusive. On the other hand, criticism for the cost of implementing FSD is strong. One of the strongest arguments in favor of implementing FSD is that of reducing the time that students are exposed to risk factors; this study sheds some light in this direction. By utilizing plausibly exogenous variation in the assignation of FSD inside schools, I obtained evidence suggesting that attending FSD is indeed reducing teenage fertility by 25%. However, whether this effect is a consequence of an incarceration effect, or a result of increased exposition and better quality of education is still inconclusive. This evidence suggests that the effect may be a combination of both channels. What seems to be more conclusive is that Full-School Days impact students in more areas than just student achievement and test scores, which has been the focus of previous studies. Evidence shown throughout this paper suggests that FSD reduces teenage fertility in urban but not rural areas, and that more years attending a full-day school have an incremental effect on this reduction. Finally, a simple characterization of schooling decisions between different

reproductive scenarios emphasizes the importance of reducing teenage pregnancy and fertility, since students facing such scenarios have considerably higher desertion rates.

8. Policy recommendations

While this research devoted a serious discussion to the internal and external validity of the results, intending to demonstrate that there is a causal effect between attending FSD and lower teenage fertility rates, it is also worth discussing how the results shown throughout this paper lead to specific policy recommendations. I will list some of the conclusions that this paper provides for policy makers. These recommendations point in two directions: first, to longer school day policies, and second, to the information that would be required to study teenage reproductive behavior amongst students.

1. Longer school days can be applied as a complementary strategy to reduce teenage fertility. As shown, attending FSD reduced teenage fertility rates by around 25%, a reduction comparable to the overall reduction of the teenage fertility rate between 2000-2010 in Colombia.
2. This estimation represents a lower bound of the effect of the FSD policy that Colombia is currently implementing. This is because this policy, which started in 2015, includes well designed courses to cover the additional time that students would spend in school. This element will enhance the education channel that is contained in all the estimations.
3. While reducing teenage fertility rates is not the only, nor the most important argument in favor of implementing full school days, it is a very positive externality of the program. In fact, this should complement the arguments presented by those in favor of longer school days, helping to mitigate the concerns associated with the costs of its implementation.
4. Results indicate that rural teenage fertility is not reduced by attending FSD. It seems that the teenage fertility rate in rural areas is determined by other factors such as poverty or life goals, to name a few. Therefore, FSD policies should first focus on increasing FSD enrollment in urban areas.
5. Continuous attendance in school days which are longer will have a stronger effect on reducing teenage fertility rates. Therefore, this policy should aim to provide students with as many years of FSD as possible.

6. Students spend much of their time in schools, thus it is important to understand how their interactions in school influence other factors of their life, such as sexuality. There is no robust administrative record that includes reproductive variables with schooling elements. Such a record should be created, whether it is as a new administrative record or as a complement to an existing record.

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Appendix 1. Comparisons between the R166 and the C600 database.

Section 4 mentioned how 2005 was omitted from the estimations because it was the first year of report of the R166, thus the quality of information is not so trustworthy. Not doing this may induce measurement error in the main dependent variable, FSD_{it} , which will bias the estimations. There may be additional concerns regarding the quality of information of forthcoming years in the R166, especially 2006. Therefore, I address this issue from two points of view. First, I must mention that starting in 2007, the R166 report was audited to ensure the quality of its information. Therefore, from 2007 onwards this is not a concern. Second, I compared basic aggregates at the school level between the R166 and the C600, which is an older- and presumably more robust- administrative record. The three aggregates that the R166 allows me to compare are (1) whether that school is registered to the same municipality in both databases, (2) whether the length of the school day registered is the same in both databases, (3) how different the number of students is, as measured by the percentual difference between the number of students in the R166 versus the C600.

Results from this comparison are evident in Table 13. As shown, there is a significant amount of schools that appear in different municipalities in both databases. However, upon closer inspection, most of these cases occur as a result of the R166 municipalities being reported in more detail and considering small and new municipalities, whereas the C600 municipality codes may assign schools in small and new municipalities to bigger cities, such as capital cities. Secondly, there is a small difference in how schools report the length of their school days. In fact, around 10% of schools report different school days in the R166 and the C600. One can notice that there is a decreasing trend of these inconsistencies. Finally, I calculated the percentual difference between the students who were reported in the R166 compared to those reported in the C600. There is an average difference of 15.5% in the number of students reported in these records. As a final remark, I must reiterate that the R166 is the official administrative record for student enrollment in the country, and as a result schools are more careful with this report.

Table 13. Comparison between R166 aggregates and the C600.

Year	Difference in municipality	Difference in school shift	Difference in the number of students
2005	16.5%	10.0%	11.4%
2006	30.1%	13.0%	-3.9%
2007	24.0%	9.4%	19.3%
2008	36.2%	9.2%	25.4%
2009	53.0%	7.9%	23.3%

Source: Own calculations based on R166 and C600.