ESSAYS ON THE ECONOMICS OF EDUCATION IN MIDDLE-INCOME COUNTRIES

By

Luis Carlos Carvajal Osorio

Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Economics

May 10, 2024

Nashville, Tennessee

Approved:

Lesley Turner, Ph.D.

Andrew Dustan, Ph.D.

Michelle Marcus, Ph.D.

Felipe Barrera-Osorio, Ph.D.

To my mother, Patricia, and to my father, Carlos, for always keeping hope alive.

ACKNOWLEDGMENTS

My doctoral studies have been an amazing journey of professional development and personal growth. I am very grateful to all the people who have invested in my success and helped me achieve this goal.

First, I would like to thank my advisors for their guidance, their constructive feedback, their patience, and especially for their unwavering support throughout the good and the bad times. I have learned a lot from each and every one of them. I would like to thank Lesley Turner for teaching me how to do applied research, for proposing brilliant solutions to many of the issues I encountered, and for her attention to detail in my work. I am grateful to Andrew Dustan for cultivating my passion for development economics, for always being so generous with his time and his comments, and for his insights on how to be an effective teacher. I thank Felipe Barrera-Osorio for giving me the opportunity to learn to do fieldwork and for his wise advice on how to address professional challenges. I also thank Michelle Marcus for bringing new perspectives and helping me make my work more accessible to a broader audience. All my committee members have been great role models for me and I aspire to follow their steps in my professional career.

I would like to express my gratitude to all the institutions that have helped me gain access to data and institutional knowledge. I would like to thank Lina Cano Olaya, Yeni Ospina Zuluaica, Jubert Palacios Casas, and Sandra Ramírez Ospina at *Fundación EPM* for sharing data and institutional background on the *Agua para la Educación, Educación para el Agua* program, as well as the teachers in the schools I visited during my fieldwork for sharing their experiences with the program. The third chapter of this dissertation would have been impossible without the collaboration of ICFES, the Colombian Institute for the Evaluation of Education. I thank Daniel Cañizares Osorio, Daniel Cuartas Rocha, Luis Fernando Gamboa Niño, Felipe Lizarazo Bonilla, Mónica Ospina Londoño, Lorena Trujillo Quintero, and Iván Velásquez Abril for hosting me during my visit to the ICFES headquarters in Bogotá, for granting me access to the de-anonymized *Saber 11* and *Saber Pro/T&T* data, and for answering my questions related to that data. I gratefully acknowledge funding from Vanderbilt University's Center for Latin American, Caribbean, and Latinx Studies, as well as the Council for Economics Graduate Students.

During the summer of my fifth year, I had the opportunity to visit Mathematica as a Summer Fellow. I would like to thank all the researchers I met there, and in particular Paolo Abarcar, Sarah Liuzzi, and Faraz Usmani, for their warm welcome and their insights on my work.

I would like to thank those who inspired me to pursue a PhD in Economics. I am grateful to the Department of Economics at Universidad EAFIT in Medellin, Colombia, for the excellent education I received there and for encouraging me to embark on this journey. I am particularly grateful to Gustavo Canavire Bacarreza, Juan Camilo Chaparro Cardona, Catalina Gómez Toro, David Jacho Chávez, Michael Jetter, and Mery Patricia Tamayo Plata for introducing me to academic research in economics and for their support during the process of applying to graduate school.

I am also very thankful to all my classmates for making my graduate studies much more productive and enjoyable. I would like to particularly thank my cohort mates Yuan Huang, Rowan Isaaks, Yukun Ma, Charles Michel, Tucker Smith, and Lillian Trotter for their help throughout the different phases of the PhD, their friendship, and the thought-provoking conversations. I am also thankful to all my classmates in other cohorts, and especially Laura Nettuno and João Tampellini, for their willingness to provide feedback on my work, for their support, and for making life in the office both fun and intellectually rewarding. The friendships that I made during the program are some of my most valued accomplishments.

Last – but certainly not least – I would like to thank my family for always being there when I needed them to provide me comfort and encouragement. I would like to thank my uncle Edgar and aunt Patricia for hosting me during my frequent trips to Bogotá. The most special thanks go to my mother, Patricia, and my father, Carlos, because they have been my most fervent supporters, my best teachers, and my most reliable source of love and wisdom. Thank you, mom and dad, for giving me the wings to fly and encouraging me to follow my dreams. *Los amo*.

TABLE OF CONTENTS

List of Tables

List of Figures

Chapter 1

Safe Drinking Water at Schools and Student Outcomes

1.1 Introduction

Poor health and nutrition are major obstacles to human capital accumulation in low- and middle-income countries.[1] To address this challenge, governments around the world have relied on school-based interventions to reduce the incidence of illness and malnutrition among school-age children. One important component of this strategy is improving access to safe drinking water at schools. According to UNICEF (2023), almost 30% of schools worldwide lack access to at least a basic safe drinking water service. Expanding access to safe drinking water at schools could benefit students by reducing the incidence of waterborne diseases that hinder learning. Furthermore, the availability of safe drinking water at schools could provide an incentive for families to enroll their children in school so that they have access to a resource that may not be available at home. In settings where safe drinking water is scarce, gaining access to safe drinking water may turn schools into water suppliers for the entire community. At the same time, schools must balance this new role as water providers with a potentially long list of existing responsibilities. If school resources are limited, the introduction of a new task may require schools to reallocate resources across tasks, which could have unintended consequences in the form of reductions in direct inputs into the education production function.

To explore the importance of these tradeoffs, I investigate the effects of a large-scale program that expanded the availability of safe drinking water to schools in rural Colombia on school enrollment, student achievement, and the health outcomes of the surrounding communities. Since 2011, the "Water for Education, Education for Water" program (AEEA - *Agua para la Educación, Educación para el Agua*) has installed water treatment facilities on the premises of rural schools lacking a potable water supply. AEEA water treatment facilities filter and chlorinate the piped water the school receives, removing pathogen agents and making it safe for human consumption. These facilities are designed to be operated by the school community. The first delivery of a water treatment facility from AEEA took place in 2012 and since then, the program has gradually expanded to almost 800 schools, with a combined enrollment of more than 90,000 students (EPM, 2023). In the state of Antioquia—where the bulk of the intervention has taken place—AEEA has treated 15% of all rural schools which encompass approximately 30% of total rural enrollment. I exploit variation in the timing of delivery of the water treatment facilities in a difference-in-difference design that compares the outcomes of schools that received their facility in a given year with those of schools that will receive their facility in a later year (i.e., not-yet-treated schools). The empirical analysis combines data on the delivery dates

[1] For instance, 443 million school days are lost to water-related diseases every year (UNDP, 2006).

of the water treatment facilities with administrative records such as annual school censuses and student-level test scores to evaluate the effects of the intervention on student enrollment, school choice, and achievement. I also use municipal-level health data to assess whether the installation of water treatment facilities leads to lower incidence of gastrointestinal diseases.

The effect of installing a water treatment facility at school varies across student populations. At the upper secondary level (i.e., grades 10 and 11), improving access to safe drinking water at school leads to a 14% increase in enrollment, although I do not find evidence of an effect of the intervention on test scores in the national high school exit exam. The introduction of a water treatment facility did not affect enrollment at the lower secondary level (grades 6 to 9), whereas at the primary level (grades 1 to 5), I find a 5% decline in enrollment after the delivery of the facility.

I explore mechanisms that may be behind the mixed effect on enrollment by grade level. One potential explanation is that the heterogeneous effects by grade level capture differential effects by school institutional capacity. The schools that have received AEEA water treatment facilities range from small primary-only schools staffed by a single teacher to large schools with hundreds of students enrolled in all grade levels. This variation is particularly pronounced among schools that offer primary level grades. These differences imply that some recipient schools have more resources and are better equipped to operate the facility than others. To examine whether institutional capacity explains the mixed effects on enrollment, I test for heterogeneous treatment effects along several measures of school institutional capacity related to the availability of teachers and the organizational nature of the school. Across all measures, I find large, negative, and statistically significant reductions in enrollment at the primary level for schools with lower institutional capacity—i.e., the schools where the operation of the facility is more likely to crowd out other activities. On the other hand, there are no effects on enrollment at the primary level for schools with higher institutional capacity.

The introduction of a water treatment facility may not only have affected the enrollment counts of the school, but also the composition of the student body. I do not find evidence to suggest this is the case, as the effects on enrollment are similar in magnitude for boys and girls and there is no effect on the percentage of students who belong to an ethnic minority or who are victims of the armed conflict. The changes in enrollment appear to be driven by students who are new to the school rather than by students who are already enrolled in school, as dropout and transfer rates remain stable after treatment. In addition, I do not find an effect on total enrollment at the municipality level, which suggests that the changes in enrollment in treated schools are due to students shifting schools rather than extensive-margin changes in the demand for schooling.

The effects of improving access to safe drinking water could go beyond the recipient schools, as the communities where these schools are located may have benefited from a reduction in the incidence of waterborne diseases. To assess this possibility, I explore the impacts of the introduction of the first water treatment facility in a municipality on the incidence rate of acute diarrheal diseases (ADDs) for the overall population and for specific age groups. The potential for health spillover effects of the installation of water treatment facility at schools for the surrounding communities appears to be limited, as I do not detect an effect on ADD incidence rates for any age group.

The results from this paper indicate that the effectiveness of school-based water treatment facilities for improving access to safe drinking water depends on the extent to which a school can absorb the new tasks needed to keep the facility operational. When schools have adequate institutional capacity, the availability of water induces some families to enroll their children in a school with more amenities. In contrast, the facility may overburden schools with less capacity. For some families, the crowding out of resources towards the operation of the facility may not justify the benefit of having safe drinking water at school and as a result, these families may choose a different school.

This paper contributes to a growing literature on the effects of school infrastructure on human capital outcomes. School inputs—which include the school's physical infrastructure—are one of the factors typically included in the estimation of education production functions (e.g., Todd and Wolpin (2003)).[2] I provide evidence on the effects of a school amenity that is not directly related to instruction but that could induce demand for schooling and facilitate the learning process through improvements in student health. The existing evidence indicates that interventions that improve access to water, sanitation, and hygiene (WASH) in schools have the potential to impact some student outcomes (McMichael, 2019). Adukia (2017) finds that the construction of school latrines in India increased enrollment among pubescent-age girls, although it did not improve achievement. Kazianga et al. (2013) evaluate a program that constructed girl-friendly schools in Burkina Faso that provided, among other facilities, drinking water and gender-specific latrines, finding positive effects on enrollment and achievement. In both contexts, improvements in access to safe drinking water are provided jointly with other WASH interventions. I expand upon these papers by isolating the effect of safe drinking water and by exploring whether these effects vary by school capacity, availability of safe drinking water within the community, and student characteristics.

A common strategy in studies that examine the effect of school infrastructure on student outcomes is to exploit variation in school funding for capital expenditures.[3] The evidence on the effects of infrastructure on student learning is mixed, in part because school physical capital is a broad category that encompasses several types of investments. In contrast to this literature, I explore the effects of a program that provides a *homogeneous* amenity that is delivered to *heterogeneous* schools in terms of size, grades offered, and socioe-

[2] See Glewwe et al. (2021) for a review of the evidence on the effects of school resources on improving educational outcomes in low- and middle-income countries.

[3] See, for example, Hong and Zimmer (2016); Martorell et al. (2016); Conlin and Thompson (2017); Belmonte et al. (2020); Lafortune and Schönholzer (2022), and Biasi et al. (2023).

conomic conditions. I show that the degree to which investments in infrastructure affect student outcomes may be related to the school's ability to utilize and maintain that infrastructure.

The water treatment facilities delivered through AEEA are intended to facilitate access to safe drinking water for the entire community. Previous studies have found that communities that gain access to safe drinking water experience reductions in child mortality and the incidence of waterborne diseases (e.g., Jalan and Ravallion (2003); Cutler and Miller (2005); Galiani et al. (2005); Gamper-Rabindran et al. (2010); Kremer et al. (2011); Beach et al. (2016); Bhalotra et al. (2021b); Beach (2022); Anderson et al. (2022)), as well as long-term gains in outcomes like educational attainment, labor earnings, and cognitive test scores (Zhang and Xu, 2016; Chen et al., 2020; Bhalotra et al., 2021a).[4] I contribute to this literature by exploring the effects of an intervention specifically targeted to schools that also facilitates access to water for the surrounding community. Prioritizing access to safe drinking water in public-use facilities like schools could be a cost-effective way to expand water availability in remote rural communities that do not have the resources to reach every household. However, there are two notable characteristics that set a school-based expansion of safe drinking water apart from other comparable interventions like public taps or boreholes. First, the placement of the water source at schools implicitly signals that the main target of the intervention are school-age children. Second, this intervention designates schools as the water provider for their communities and as such, they are responsible for the operation of the water treatment facility. Hence, the provision of safe drinking water implies a social benefit for recipient communities and a private cost that is borne by the school.

An extensive body of literature documents a strong link between student health and educational outcomes (Glewwe and Miguel, 2007; Glewwe and Muralidharan, 2016). While AEEA is a school infrastructure intervention, one of the main goals of that infrastructure is to improve student health. As such, AEEA is related to a broader family of interventions that aim to improve child health and use schools as a medium of delivery. Some of the goods delivered through these interventions include meals, micronutrient supplementation, immunizations, and deworming treatment. Advocates for school-based health interventions argue that addressing student health in a school setting allows the use of existing infrastructure, which increases take-up and reduces costs (Bundy et al., 2018). However, program effectiveness may be diminished if its operation interferes with the other tasks carried out by the school. This is a version of the multitasking problem first introduced by Holmstrom and Milgrom (1991). Schools' objective is to maximize the total amount of human capital produced subject to a resource constraint and to do so, they must allocate resources between their current activities and the new intervention, which may induce tradeoffs between tasks. This crowding out be-

[4]There are two additional branches of the literature on water and human capital. The first explores the effects of changes in the *quality* of the safe drinking water service leveraging variation from pollution shocks or service interruptions (Currie and Vogl, 2013; Kosec, 2014; Buchmann et al., 2019; Ashraf et al., 2021; Marcus, 2022; Hill, 2022), while the second investigates the effects of more convenient access to water on time allocation (Devoto et al., 2012; Koolwal and Van de Walle, 2013; Meeks, 2017; Nauges and Strand, 2017; Gross et al., 2018).

havior between new interventions and existing duties has been documented in the context of school feeding programs (Vermeersch and Kremer, 2004; Berry et al., 2021), early childhood home-visiting interventions (Bos et al., 2023), and the hiring of contract teaching (Muralidharan and Sundararaman, 2013). This paper adds to the evidence on the importance of assessing the capability that potential implementers have to take on new responsibilities without compromising their normal activities.

The remainder of the paper proceeds as follows. Section 3.2 provides background about public education and water access in rural Colombia and describes the AEEA program, its eligibility conditions, and the process through which beneficiary schools are selected to receive water treatment facilities. Section 3.3 details the data sources and presents summary statistics about the recipient schools. I explain the empirical strategy used to identify the causal effects of AEEA in Section 3.4. Section 3.5 presents the results related to school enrollment and achievement and explores the heterogeneity of the estimated effects as well as the potential mechanisms behind them. Section 1.6 examines whether AEEA is associated with health improvements in the municipalities where AEEA recipient schools are located. Section 3.6 concludes.

1.2 Background

1.2.1 Access to Water in Rural Colombia

Manuscripts consist of four major sections and must be placed in the order listed:

According to the WHO/UNICEF Joint Monitoring Programme for Water Supply, Sanitation and Hygiene, in 2011 72% of households in Colombia had access to a safely managed water service—that is, a service that provides improved water on premises to households at any time that is free of contamination. In rural areas, this percentage is significantly lower (37%) and is below the Latin American average for rural areas (46%). An additional 43% of rural households have access to a basic drinking water service—that is, households whose water service fails to meet at least one of the three conditions of a safely managed water service (accessible on premises, uninterrupted availability, and free from contamination) but who can access a safely managed water service within less than 30 minutes roundtrip from home (WHO-UNICEF, 2023).

Rural households with access to piped water are served by either public utility companies or by informal community-run water networks which have fewer resources and less oversight. Data from the 2011 National Quality of Life Survey indicates that community-run water networks are a much more prevalent source of water for rural households than networks operated by public utilities companies – 36% versus 17%. Other common sources of water include wells (13% of rural households in 2011), rainwater (9%), and rivers and lakes (22%). Water quality is an issue for the majority of rural households in Colombia regardless of their water source. Only 41% of households report being able to drink the water directly from its source without any additional treatment like boiling or filtering (DANE, 2012).

1.2.2 The Colombian Public School System

In rural Colombia, primary and secondary education are almost entirely provided by public schools.[5] Public schools do not charge tuition and do not have formally defined attendance zones. At age five, children are required to enroll in preschool (*educación preescolar*), although they may enroll in pre-kindergarten grades from ages as low as three. At age five or six, after children have completed at least one grade of preschool, they enroll in primary school (*educación básica primaria*), which is also mandatory and comprises five grades. Then, at ages eleven or twelve, children are required to enroll in lower secondary school (*educación básica secundaria*) for four grades. Upon completing lower secondary school, children may enroll in two additional years of upper secondary school (*educación media*), which unlike the three previous grade levels, are not mandatory. The compulsory nature of the primary and lower secondary grade levels has translated into high enrollment rates at these levels. According to data from the Colombian Ministry of Education (2023), in 2011 gross enrollment rates at the primary and lower secondary level were 114.5% and 105.2%, respectively. In contrast, the gross enrollment rate at the upper secondary level (80.3%) was significantly lower than in the other grade levels.[6]

Public schools in Colombia are organized into multi-site school systems. A school system (*establecimiento educativo*) is made up of multiple school campuses (*sede educativa*).[7] One of the school campuses in the system is considered to be the main campus (*sede principal*) while the others are known as affiliated campuses (*sede adscrita*). Main campuses house the school principal and coordinators, tend to have higher enrollment, and must offer at least all ten compulsory grades, with the majority also offering the two grades that make up the upper secondary level. In contrast, affiliated schools are usually smaller campuses which offer only a subset of grade levels—typically primary only, although some affiliated schools also offer lower and upper secondary.[8] Many smaller schools in rural areas operate under a multi-grade system known as *Escuela Nueva*, under which students of different ages are grouped in the same classroom where they learn independently using age-appropriate material and with the supervision of a teacher.

In Colombia, public schools are administered at the local level by authorities known as Certified Territorial Entities (ETC - *Entidades Territoriales Certificadas*). Larger municipalities have their own ETC with jurisdiction over the schools in that municipality only. Schools in municipalities that are not large enough to have their own ETC are administered by a state-level ETC. ETCs are in charge of distributing resources from the central government to the schools in their jurisdiction. Other duties of ETCs include the establishment of

[5]Ninety-eight percent of rural schools are public.

[6]Gross enrollment rates refer to the number of student enrolled in school as a fraction of the school-age population. Since these rates include students of all ages, they may exceed 100% if there are many students who are too old or too young for their grade level. In contrast, net enrollment rates only account for school enrollment of children who are the appropriate age for their grade level. In 2011, net enrollment rates at the primary, lower secondary, and upper secondary level were 89.3%, 72.3%, and 42.5% respectively.

[7]The term “school” in this paper refers to a single school campus, which may be affiliated to a larger school system.

[8]Very few schools offer lower or upper secondary level grades without offering primary level grades.

educational policies and the hiring and promotion of teachers. Local governments in municipalities without their own ETC may complement school funding with their own resources and they also assist ETCs in the oversight and operation of the schools within their municipality. These complementary funds from municipal governments represent less than 10% of total school funding, a fraction lower than in other Latin American countries (Alvarez Martinelli et al., 2018).

1.2.3 The "Agua para la Educación, Educación para el Agua" Program

AEEA is run by *Fundación EPM*, the private foundation of *Empresas Públicas de Medellín* (EPM), a large public utilities company whose operations are mostly concentrated in Medellín—Colombia's second largest city—and in the surrounding state of Antioquia. In 2011, *Fundación EPM* launched AEEA with the purpose of providing safe drinking water to rural public schools in EPM's areas of influence.[9] According to EPM, prior to the launch of AEEA, only 25% of rural schools in Antioquia had access to safe drinking water (EPM, 2023). Through AEEA, *Fundación EPM* installs a water treatment facility on school premises that treats the piped water the school receives from their local water network to make it safe for human consumption. The water treatment facility consists of a filter, a chlorine feeder, and two separate tanks that store untreated and treated water, which are housed in a purpose-built storage room. After the water has been filtered and chlorinated, it is transported to a hydration point (e.g., a water fountain or a sink) where students and the community may have access to it, and to the school kitchen, where it can be used for food preparation. The basic design of the water treatment facilities is constant across schools, although facilities can be adapted to match the expected demand for water. The smallest facility can provide safe drinking water for up to 45 children, whereas the capacity for the other three types of facilities are 100, 300, and 1,000 children. The facility may or may not require a water pump according to the technical conditions of the water supply of the school, the terrain in which the school is located, and the space available at the school.

Through AEEA, *Fundación EPM* installs the water treatment facilities at the school, but they are not in charge of their operation and maintenance, as this is the responsibility of the school community and the municipal government. Even though the facilities have been adapted to the conditions found in remote rural areas, their operation is not automatic and there are some tasks that need to be conducted in order to obtain safe drinking water from them. Filters have to be manually back washed before every use, and chlorine must be added to the water in the correct proportion every two to three days. In addition, water tanks and pipes must be thoroughly washed every few months. Concurrent with the installation of the water treatment facility, *Fundación EPM* provides training to teachers and to community leaders on the correct procedures to operate

[9] Originally, the program was only targeted to rural public schools in Antioquia. In 2016, the program was expanded to all areas in Colombia where EPM has subsidiaries.

and maintain the facility. Schools also receive the equipment necessary to use the facility, like the instruments needed to measure the concentration of chlorine in the water and the first dose of chlorine.

The selection process to receive a water treatment facility starts with the nomination of candidate schools by municipal governments. Then, municipal governments and *Fundación EPM* reach a cost-sharing agreement which specifies the number of intended recipient schools and the share of the cost assumed by each party.[10] Next, *Fundación EPM* visits the candidate schools and assesses whether they meet the technical and legal requirements for receiving a water treatment facility.[11] *Fundación EPM* checks that all the requirements are met before making the final decision on which of the candidate schools will receive a water treatment facility.[12] The particularities of each school and each municipal government imply that the time between agreement and delivery may vary considerably across treated schools.

Once a school is selected for receiving a water treatment facility, *Fundación EPM* organizes a meeting with members of the community to inform them about the project. *Fundación EPM* also works with teachers to help them incorporate topics related to water conservation and adequate use of natural resources into their curriculum. Communities may start receiving safe drinking water from the facilities once the construction crews finish the installation of the equipment. However, the facility is only formally handed over to the school and the municipal governments after *Fundación EPM* conducts two follow-up visits after the completion of the installation to verify the facility is operating properly. These follow-up visits take place one and two months after the completion of the construction. Although the water treatment facilities are located on school grounds, they are not for the exclusive use of the students and staff of the school. In fact, the community is allowed and encouraged to use the facility as a source of safe drinking water and to assist the school in its upkeep.

1.3 Data

To estimate the effect of receiving a water treatment facility on student and health outcomes, I combine information on the timing of the delivery of the facilities with administrative records at the student, school, and

[10]In some cases, *Fundación EPM* covers the entirety of the cost of installation of the facility. To select the municipalities that receive a fully subsidized water treatment facility, *Fundación EPM* runs a contest called "More Water, More Smiles" (MAMS - *Más Agua, Más Sonrisas*) under which municipal governments submit a proposal that highlights the efforts they have taken to meet the Sustainable Development Goals in their municipality. Additional criteria for scoring proposals include municipality-wide school enrollment rates and poverty levels according to the National Population Census. MAMS awards water treatment facilities to up to 10 municipalities every year. I cannot identify in the data whether a school received their facility through MAMS or through a cost-sharing agreement. Likewise, I do not observe the percentage of the cost assumed by each party.

[11]In order for a school to be eligible to receive a water treatment facility, it must have a combined enrollment across all grade levels between 10 and 1,000 students. The school must have a legally obtained connection to a water source with adequate year-round water flow that delivers freshwater free from heavy metals like lead or mercury but that requires treatment to be suitable for consumption. Moreover, the school must be reachable by land or by water for construction crews and it must not be located in an area prone to natural disasters. There must be an available area of at least 9 square meters on the school grounds for the construction of the facility. The land on which the school sits must be municipal property and there must not be plans to close or relocate the school for the duration of the service life of the facility (approximately 10 years).

[12]Because candidate schools are nominated by individual municipal governments, there is no centralized list of schools ever considered for AEEA.

municipality level. This section discusses the data sources and the sample selection and provides descriptive statistics about the schools that receive water treatment facilities and the nearby never-treated schools.

1.3.1 Data Sources

I identify the schools that received a water treatment facility under AEEA using a list of all ever-treated schools provided by *Fundación EPM*. The list includes all the schools that received a water treatment facility from 2012 to 2022. For each treated school, I observe the municipality and district (*corregimiento* or *vereda*) where the school is located, as well as the date of delivery of the water treatment facility. The AEEA program has primarily operated in the northwestern state of Antioquia, with 94% of treated schools – 744 out of 792 – located in the state. The program has delivered water treatment facilities to schools in 116 of the 125 municipalities in Antioquia. The remaining schools are located in 22 municipalities in 5 other states. Because of the geographic concentration of the treated schools and because my identification strategy compares already-treated to not-yet-treated schools, I limit the estimating sample to treated schools in Antioquia.

There are two main data sources for educational outcomes. The first is annual school censuses conducted by Colombia's National Administrative Department of Statistics (DANE - *Departamento Administrativo Nacional de Estadística*), which are publicly available from DANE's website since 2004. In the school census, schools report the number of students enrolled in the academic year broken down by gender, age, and grade level, as well as the number of teachers and staff employed, the number of groups per grade level, and the availability of IT assets. Schools also report the number of students who dropped out or transferred to a different school during the previous academic year. The annual school census takes place in the second semester of the (calendar) year and the reference period for the reported figures is March 31 of the current year.[13]

To measure effects on achievement, I use individual-level records from *Saber 11*, the national high school exit exam. This test is administered annually to all graduating high school students by the Colombian Institute for the Evaluation of Education (ICFES - *Instituto Colombiano para la Evaluación de la Educación*). Conditional on upper secondary graduation, take-up of *Saber 11* is nearly universal, even for students who do not intend to enroll in higher education. Individual-level records of the current format of *Saber 11* are available since 2012. Students receive a combined score and subject-specific scores for the five subjects that make up the test—math, reading, natural science, social science, and English.[14]

[13] The academic year in public schools in Colombia mirrors the calendar year. Classes start in early February and end in late November, with a one-month break between June and July.

[14] While there are other tests for lower grades, they have not been administered on a frequent enough basis to be suitable for the purposes of exploring the effects of AEEA on achievement. From 2012 to 2017, ICFES administered standardized tests to students in third, fifth, and ninth grade. These tests—known as *Saber 3-5-9*—were only administered to a sample of schools and students within the selected schools did not take a test in all subjects assessed. Because *Saber 3-5-9* were not universal, there are not many schools with tested students in the sample that are observed both before and after the delivery of the water treatment facility. A second complication of using *Saber 3-5-9* data is that average test scores are only available at the school system (*establecimiento educativo*) level. At the school (*sede educativa*) level it is only possible to observe the number of students who score in four achievement tiers: insufficient, minimum, satisfactory, and advanced.

Even though AEEA is a school-based intervention, it has the potential to impact the health outcomes of the communities where treated schools are located.[15] To evaluate these impacts, I use municipality-level data on the incidence rate of acute diarrheal diseases (ADD) from Antioquia's Regional Secretariat of Health (DSSA - *Dirección Seccional de Salud de Antioquia*). The DSSA reports the number of cases and the incidence rate of ADD per 100,000 inhabitants in all the municipalities in Antioquia since 2013 broken down by 5-year age groups.

In addition to the sources described above, I use two sources of geographic data. First, I obtain school coordinates from DANE's School Location Information System (SISE - *Sistema de Información de Sedes Educativas*). Second, I use geolocated data from the 2005 and 2018 Population Censuses. These data are are available at the census block (*sector rural*) level and allow me to characterize the number of people living in the vicinity of treated schools as well as the socioeconomic conditions of the surrounding communities.[16]

1.3.2 Sample Selection

The sample for the school-level analysis is made up of 742 schools located in the state of Antioquia that received a water treatment facility between 2012 and 2022.[17] Because the Covid-19 pandemic disrupted normal school operations, I only examine outcomes through the end of 2019.[18] For each treated school, I include up to seven pre-treatment years and when applicable, I include all available post-treatment years through 2019.

The municipality-level analyses are restricted to the 112 municipalities in the state of Antioquia outside the Medellin metropolitan area that have received at least one water treatment facility through AEEA.[19] The first delivery of water treatment facilities at the municipality level ranges from 2012 to 2017. Following the school-level analysis, I exclude all outcome data collected after 2019.

1.3.3 Descriptive Statistics

Figure 1.1 shows the location of all treated schools, while figure 1.2 depicts the number of deliveries by year. The 742 treated schools represent approximately 15% of all public rural schools in Antioquia and their combined enrollment represents approximately 30% of total rural enrollment in the state. The first delivery

[15]The ideal school-level outcome to measure the impact of AEEA on student health would be school attendance. However, it is not possible to assess the effect on attendance because it is not captured in the administrative records.

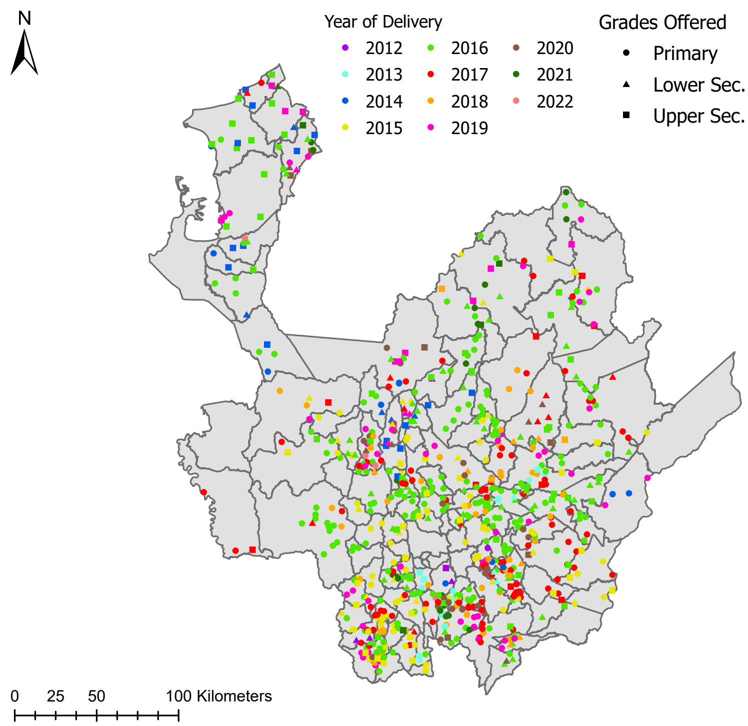
[16]Unless otherwise noted, I define the school's vicinity as all the census blocks that are within a 5-km radius of the school. This definition allows some census blocks to be in the vicinity of multiple treated schools.

[17]Two treated schools were dropped from the sample because they could not be linked to a unique school in the annual school census.

[18]This restriction implies that schools that received their treatment facility after 2019 effectively act as never-treated controls in the difference-in-differences design.

[19]The Medellin metropolitan area is made up of 10 municipalities and AEEA has delivered water treatment facilities in four of them. I do not include these four municipalities in the sample because these are mostly urban municipalities with easier access to the health facilities in Medellin. There are only three municipalities outside of the Medellin metropolitan area that have never received a facility through AEEA.

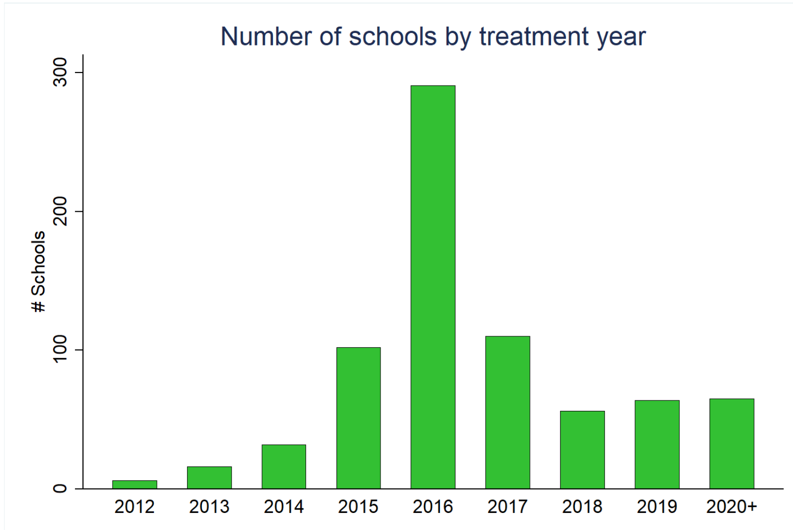
of a water treatment facility took place one year after the launch of AEEA in 2012, and the bulk of deliveries took place after 2015. Treated schools are located in all regions of the state of Antioquia and the delivery schedule does not follow a clear geographic pattern.



Notes: This map present the geographic location of the AEEA recipient schools in the state of Antioquia from 2012 to 2022. Each color represents a year of delivery, while the shape of the marker indicates the highest grade level offered by the school. The lines denote municipal boundaries. Data sources: DANE (SISE), *Fundación EPM*

Figure 1.1: Geographic distribution of water treatment facility deliveries.

Table 1.1 presents the means of various school characteristics in the year immediately before delivery. In general, schools treated in earlier years had similar pre-treatment characteristics to schools treated in later years. To show more formally that schools treated in earlier years are similar in terms on enrollment to schools treated in later years, I regress enrollment in 2011—the year AEEA was launched—on treatment year dummies, with 2012 as the omitted category. Figure 1.3 shows that there is no statistically significant difference in enrollment in 2011 across treatment years. In Appendix 1.8, I present the trends in average enrollment by grade level for the schools in the sample. Throughout the period of study, average enrollment across all grade



Notes: This figure depicts the number of deliveries of water treatment facilities through AEEA in the state of Antioquia from 2012 to 2022. Even though outcome data only goes through 2019, schools treated after 2020 are included in the estimation sample. Data source: *Fundación EPM*

Figure 1.2: Number of water treatment facility deliveries by year.

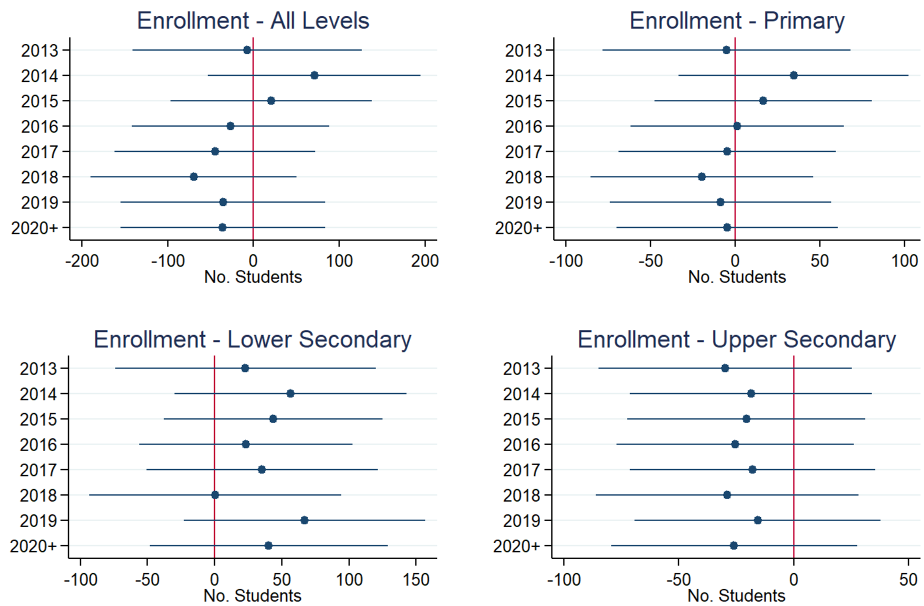
levels in ever-treated schools has been decreasing. The Appendix also shows that this declining trend is not unique to ever-treated schools, as average enrollment has also decreased over time in never-treated schools in the state of Antioquia.[20]

Table 1.1: Descriptive statistics of outcomes of interest for ever-treated schools in the year before receiving the water treatment facility.

	All Schools	Year of Delivery 2012	2013	2014	2015	2016	2017	2018	2019	2020+
% main schools	0.24	0.50	0.27	0.55	0.38	0.22	0.14	0.14	0.23	0.24
% schools offering lower secondary	0.41	0.83	0.56	0.66	0.53	0.40	0.27	0.32	0.41	0.45
% schools offering upper secondary	0.22	0.17	0.31	0.50	0.35	0.19	0.15	0.14	0.22	0.21
Total number of students	101.72	128.67	115.50	193.03	149.07	96.90	84.83	50.88	90.59	76.76
Number of students in primary	57.51	63.50	52.75	89.84	79.26	58.07	55.14	34.16	44.67	36.80
Number of students in lower secondary	87.58	65.40	92.22	124.00	108.81	81.34	92.07	44.56	90.38	64.65
Number of students in upper secondary	36.74	64.00	34.80	37.19	36.94	35.29	45.75	21.00	45.29	29.85
% female students	0.48	0.49	0.48	0.46	0.47	0.48	0.47	0.49	0.48	0.47
% students victims of armed conflict	0.16	0.04	0.14	0.17	N/A	0.20	0.28	N/A	0.19	0.15
Number of teachers	4.65	5.50	5.50	9.00	6.98	4.41	3.64	2.54	4.23	3.66
% female teachers	0.68	0.76	0.86	0.74	0.70	0.69	0.64	0.68	0.61	0.68
% teachers with college degree	0.80	0.92	0.89	0.70	0.78	0.80	0.78	0.79	0.83	0.81
Student-teacher ratio	22.26	24.08	22.96	22.76	22.16	23.93	22.50	20.36	22.49	15.01
Combined score *Saber 11* (SDs)	-0.54	N/A	-0.58	-0.55	-0.28	-0.57	-0.60	-0.71	-0.72	-0.71
Number of schools	742	6	16	32	102	291	110	56	64	65

This table presents the mean by treatment timing group of different school characteristics in the year immediately before the delivery of the water treatment facility. The data on the number of students victims of the armed conflict is unavailable in 2014 and 2017. Prior to 2012, *Saber 11* did not include a combined score. Data sources: DANE (annual school censuses - *Formulario C600*), ICFES (*Saber 11*).

[20]This reduction can also be seen at the national level. Data from DANE (2023) indicates that during most years of the 2010s decade, nationwide enrollment in primary and secondary education fell by 0.5% to 2.5% per year.



Notes: This figure depicts the estimated coefficients of a regression of enrollment in 2011—prior to the launch of AEEA—on treatment year dummies. The omitted category is receipt of a facility in 2012. Point estimates are measured in levels.

Figure 1.3: Estimated coefficients from regression of enrollment in 2011 on year dummies. The omitted category is 2012.

1.4 Empirical Strategy

The staggered timing of the delivery of AEEA water treatment facilities induces a natural experiment. All recipient schools meet the eligibility criteria to receive a water treatment facility. However, some schools received their water treatment facility earlier than others. As shown in the previous section, schools treated in earlier and later years are located in similar areas and are comparable in terms of pre-delivery characteristics. Given these similarities, it is reasonable to assume that in the absence of the program, the outcomes of early and late treated schools would have evolved in a similar way in the time periods between early and late deliveries. Under this parallel trends assumption, a difference-in-difference design that compares the outcomes of early and late treated schools in the time periods before and after the delivery of the facility to early-treated schools should yield a causal estimate of the effects of a water treatment facility on the outcomes of interest.

Goodman-Bacon (2021) notes that in settings where treatment timing varies, the parameter that a two-way fixed effect (TWFE) specification identifies is a weighted average of all possible 2×2 difference-in-differences estimators across time periods and treatment timing groups. The weights of some comparisons—in particular, comparisons that use already-treated units as controls for late-treated units—may be negative, which could complicate the causal interpretation of the TWFE parameter as the average treatment effect.

To overcome this issue, I follow Callaway and Sant'Anna (2021) and estimate group-time average treatment effects. First, I divide schools into treatment timing groups that are defined by the year in which schools received their water treatment facilities. These treatment timing groups are indexed by $g = 2012, 2013, \ldots, 2020+$.[21] Then, for each year t such that $t \geq g$ and for each treatment timing group g, I estimate the following 2×2 difference-in-difference specification:

$$Y_{st} = \alpha + \beta_{gt} \mathbb{1}[G_s = g] \times \text{Post}_t + \gamma \mathbb{1}[G_s = g] + \delta \text{Post}_t + \varepsilon_{st}, \tag{1.1}$$

where $\mathbb{1}[G_s = g]$ is an indicator variable that takes a value of 1 if school s belongs to treatment timing group g, and Post_t is an indicator variable that takes a value of 1 if schools in group g have already received their water treatment facility. The control group for the estimation of equation (1.1) is made up of all the schools that have not yet received a facility by year t—i.e., schools in treatment timing groups $t+1, \ldots, 2022$. The sample used to estimate equation (1.1) includes two observations for each school: one from year $g-1$ (when no school has received a facility yet) and another from year t (when the only schools that have water treatment facilities are those from group g). The parameter β recovers the group-time average treatment effect for group g in year t. Let $\widehat{\beta}_{gt}$ denote the estimate of the average treatment effect for group g in year t. The identifying assumption for each group-time average treatment effect is that in the absence of the water treatment facility, the outcomes of the schools in treatment timing group g in year t (where $t \geq g$) would have followed a similar trajectory to those of the schools that had not yet received their facility by year t.

This procedure yields 33 different group-time average treatment effects.[22] Instead of reporting all 33 estimates, I summarize them using two of the aggregations proposed by Callaway and Sant'Anna (2021). First, I provide an overall average treatment effect across all treatment timing groups g and years t. This aggregate, which Callaway and Sant'Anna (2021) refer to as the simple aggregate, provides a summary of the treatment effect for all units and all time periods. This simple aggregation is given by

$$\hat{\theta}^{S} = \frac{\sum_{g=2012}^{2020+} \sum_{t=2004}^{2019} \mathbb{1}[g \leq t] P(G = g) \widehat{\beta}_{gt}}{\sum_{g=2012}^{2020+} \sum_{t=2004}^{2019} \mathbb{1}[g \leq t] P(G = g)}, \tag{1.2}$$

where $P(G = g)$ denotes the share of treated schools that received their facility in year g.

Second, I combine the group-time average treatment effects into event-study-style aggregates $\hat{\theta}(e)$ that identify the treatment effect for units that have been exposed to treatment for e periods. These dynamic treatment effects are useful for exploring how treatment effects evolve over time:

[21] Schools treated in 2020 or later constitute a single treatment timing group because outcome data are not included after 2019, which makes this group analogous to a never-treated group.

[22] There are 9 treatment timing groups, which means there should be $\binom{9}{2} = 36$ possible combinations. However, 3 of the combinations (2012 vs 2019, 2012 vs 2020+, 2013 vs 2020+) cannot be estimated since the time difference between treated and control groups exceeds 7 years.

$$\hat{\theta}(e) = \sum_{g=2012}^{2020+} \sum_{t=2004}^{2019} \mathbb{1}[t-g+1=e] P(G_g = 1 | t-g+1 \geq e) \widehat{\beta}_{gt}. \tag{1.3}$$

I conduct several heterogeneity analyses to determine whether the effect of receiving a water treatment facility varied across schools with different characteristics. These analyses imply testing the equality of the simple aggregates of average treatment effects for schools belonging to two different subsamples. Letting A and B denote the two subsamples, the parameter of interest is:

$$\Delta_{AB} = \theta_A^S - \theta_B^S, \tag{1.4}$$

where θ_i^S, $i = A, B$ denotes the simple aggregation of group-time average treatment effects for schools in subsample i.

I estimate $\widehat{\Delta}_{AB}$ by replacing the simple aggregations with their sample analogues from equation (1.2). For inference, I follow a pair-bootstrap procedure similar to that in Flynn and Smith (2022). First, I draw a stratified random sample of schools with replacement. This random sample has the same number of schools in each subsample as the original sample. Then, I estimate $\widehat{\Delta}_{AB}$ for this bootstrap sample. I repeat this process 1,000 times and use the estimated $\widehat{\Delta}_{AB}$ from each iteration to calculate bootstrap standard errors as follows:

$$\text{SE}(\widehat{\Delta}_{AB}) = \sqrt{\frac{1}{999} \sum_{n=1}^{1000} (\widehat{\Delta}_{AB,n} - \overline{\widehat{\Delta}}_{AB})^2}, \tag{1.5}$$

where $\widehat{\Delta}_{AB,n}$ is the estimate of the difference in the n-th bootstrap iteration and $\overline{\widehat{\Delta}}_{AB}$ is the mean of the estimated difference across all bootstrap iterations.

Some of the outcomes of interest, such as disease incidence rates, are only measured at the municipality level. For these outcomes, I follow the same method I use for school-level outcomes but with municipalities as the cross-sectional unit. In these specifications, I define that a municipality is treated if by year t, there has been at least one delivery of a water treatment facility in the municipality.[23] Appendix 1.9 shows the variation in treatment timing for municipality-level outcomes.

[23] There are alternative ways of defining treatment at the municipality level. For instance, treatment could be a continuous variable that denotes the percentage of treated schools within the municipality. Alternatively, treatment could be a binary variable that indicates that the fraction of treated schools within the municipality exceeds a given threshold. However, these approaches involve nontrivial econometric concerns. A continuous treatment variable would require assuming parallel trends across *all* treatment intensities (Callaway et al., 2021). On the other hand, a threshold-based binary variable would likely represent a violation of parallel trends since some municipalities may be partially treated from the year of the first delivery to the year in which they finally reach the threshold to be considered treated. These issues do not apply to the specification based on first deliveries.

1.5 Effects on School Outcomes

This section presents the estimated effects of receiving a water treatment facility through the AEEA program on school-level outcomes. First, I show the effects of the program on enrollment and achievement. Then, I explore the potential mechanisms through which the effects of the program may operate. Finally, I conduct robustness checks.

1.5.1 Effects on Enrollment and Achievement

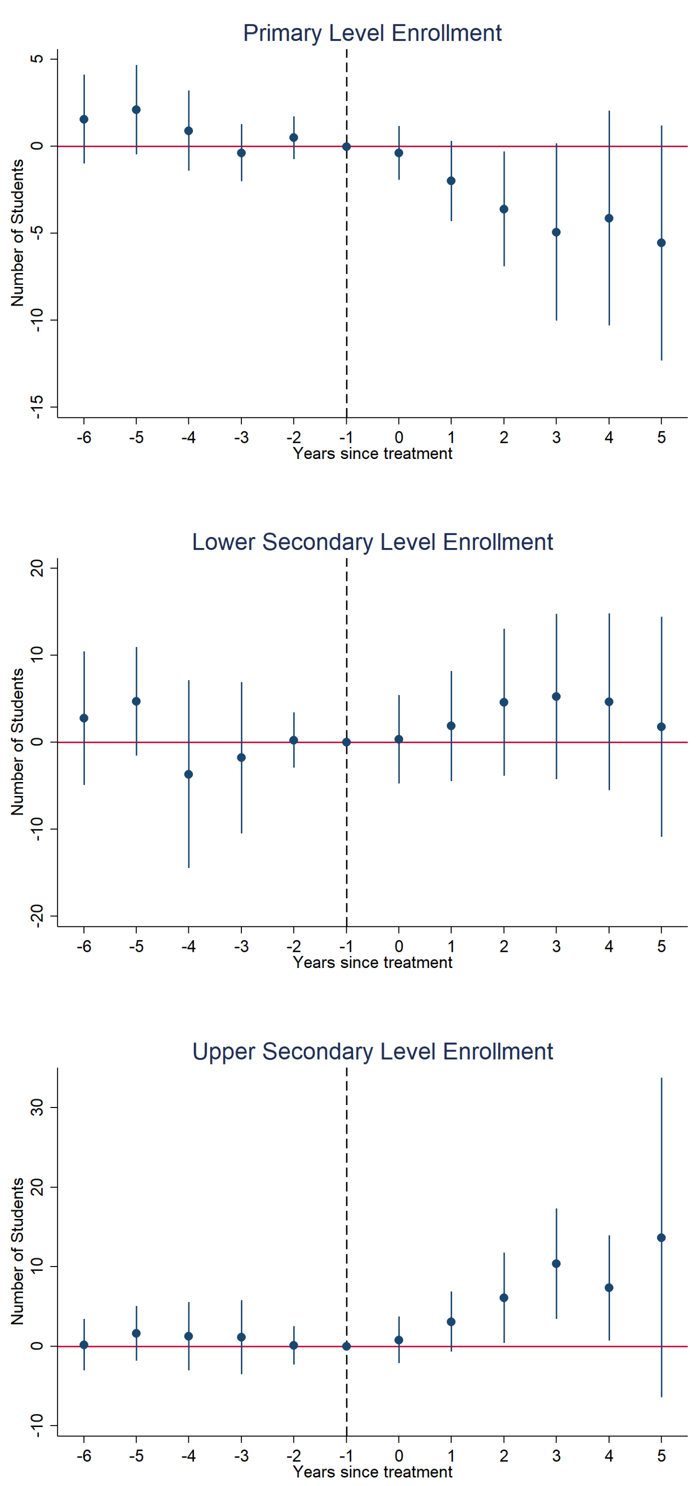
The first outcome I explore is the number of students enrolled, both across all grades and by grade level. The introduction of the water treatment facility may affect enrollment because it alters the bundle of goods offered by the school. All else equal, families should be weakly more likely to enroll their children in a school with more amenities. However, students may value individual school amenities in different ways and the enrollment response will be a function of these valuations. For instance, a student may not change their enrollment decision if they have access to the amenity outside the school.[24] Moreover, adding an amenity that requires resources for its operation – like the water treatment facility – may result in lower levels of other goods and services, especially within relatively resource constrained schools.

Table 1.2 shows the estimated average treatment effects of water treatment facility receipt on school enrollment. On average, total school enrollment remains unchanged. However, these effects vary considerably by grade level. I find that, on average, primary level enrollment (i.e., grades 1 to 5) decreases by 2.6 students, which represent a 5% decrease in relation to the pre-treatment mean. While there is no statistically significant effect on enrollment at the lower secondary level (grades 6 to 9), I find a significant increase of 5.2 students at the upper secondary level (grades 10 and 11), a 14% increase with respect to the pre-treatment mean.[25]

Figure 1.4 shows the estimated treatment effects on enrollment by grade level in the years before and after receipt of a water treatment facility. For all grade levels, the estimates for pre-treatment periods are all indistinguishable from a null effect, lending credence to the identifying assumption that enrollment in early- and late-treated schools would have behaved similarly during the time period AEEA has been in place had they never received a water treatment facility. Both the negative effect on primary level enrollment and the positive effect on upper secondary enrollment start manifesting two years after the delivery of the facility and are present for approximately three more years, after which the estimated coefficients are imprecisely estimated.

[24] Das et al. (2013) document the substitutability of school-provided and home-provided goods in the context of school grants and find that household spending is reduced when households can anticipate increases in school funding.

[25] The effect sizes at the upper secondary level are similar to those found by Adukia (2017) in the context of a school latrine construction program in India. Furthermore, estimating the effects on log enrollment yields qualitatively similar results – a 6% decrease at the primary level and a 10% increase at the upper secondary level. These results can be found in Appendix Table 1.12 and Appendix Figure 1.10.



Notes: This figure depicts the event study aggregation of Callaway and Sant'Anna (2021) estimates (i.e., the average treatment effect for schools that have been treated for $e = t - g$ time periods—see equation (1.3)) of the effect on enrollment in levels at the primary (top), lower secondary (middle), and upper secondary (bottom) levels. The point estimates measure the effect in relation to $g - 1$, the year immediately before the delivery of the facility. The bars represent 95% confidence intervals. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Figure 1.4: Event study aggregation of Callaway and Sant'Anna (2021) estimates of effects on enrollment by grade level.

Table 1.2: Effects on enrollment by grade level.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
Water Treatment Facility	0.20	-2.64*	2.93	5.24***
	(2.31)	(1.36)	(2.70)	(1.97)
N	7,546	7,473	2,894	1,549
Mean Dep. Var. in $g-1$	101.18	57.39	87.45	37.18

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Changes in enrollment may come from two sources: new students who are deciding whether to enroll in a treated school, and current students who are deciding whether to remain enrolled. If the effect on enrollment was driven by existing students, then there should be an effect on the proportion of students who drop out or transfer to another school. Table 1.3 shows that the delivery of a water treatment facility did not affect the number of students who dropped out or transferred during the academic year.[26] Hence, the effect on enrollment can be attributed to new rather than to current students of the school. In Appendix Table 1.14, I estimate heterogeneous effect on enrollment by the age of the student. The findings from that analysis suggest that the effect on enrollment is only significant for students who are young for their grade level—that is, the students who are the most likely to be transitioning to a new grade level.

I now explore the effects on achievement as measured by the scores in *Saber 11*. *A priori*, the effects of safe drinking water on achievement are ambiguous. On one hand, improvements in hygiene could lead to reduced student absenteeism and limit the cognitive impairments associated with dehydration.[27] On the other hand, the increase in enrollment at the upper secondary level associated with the delivery of the water treatment facility could be driven by lower-ability students that would not have otherwise stayed in school, thereby reducing average achievement at the school level.

Table 1.4 presents the effect of water availability at school on the combined and subject-specific scores on *Saber 11*. I do not find any effect on the combined score or on any of the sub-scores. Figure 1.5 shows that the null effect on test scores persists over time. One potential reason for this null effect is that, since *Saber 11* takes place at the end of the upper secondary level, its scores are more reflective of the quality of

[26] Appendix Table 1.13 shows the effects on dropout and transfer rates.

[27] Some examples of studies in the medical literature that link dehydration with cognitive impairments include Bar-David et al. (2005), Fadda et al. (2012), and Cooper-Vince et al. (2017)

Table 1.3: Effects on number of dropouts and transfers by grade level.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
		PANEL A: DROPOUTS	
Water Treatment Facility	-0.90**	0.10	-0.26
	(0.38)	(0.95)	(0.51)
N	7,477	2,891	1,563
Mean Dep. Var. in $g-1$	4.13	9.39	2.08
		PANEL B: TRANSFERS	
Water Treatment Facility	-0.44	0.43	0.38
	(0.38)	(0.88)	(0.53)
N	7,477	2,891	1,563
Mean Dep. Var. in $g-1$	3.76	4.43	1.45

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panel A in this table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on the number of students in each grade level who dropped out of school during the previous academic year. Panel B shows the effect on the number of students in each grade level who transferred out to a different school during the previous academic year. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

education along a student's trajectory than of short term changes. In other words, any potential improvement in learning that may have occurred after the water treatment facility was delivered may not be enough to compensate deficiencies from previous grade levels. In Appendix Tables 1.15 and 1.16, I show that the intervention did not have a differential effect on achievement by gender or by socioeconomic status, and that there is no effect at any percentile of the test score distribution.

1.5.2 Mechanisms

So far, the results indicate that the installation of a water treatment facility at schools is associated with a decrease in enrollment at the primary level, an increase in enrollment at the upper secondary level, and no effects on test scores. Now, I explore potential mechanisms that could explain these effects.

1.5.2.1 The Role of Institutional Capacity

The introduction of the water treatment facility involves a new set of tasks school personnel must carry out to provide safe drinking water to their students and their surrounding communities. Performing these duties requires time and resources that could be used in other activities, such as instruction or other administrative tasks. While some schools may have enough resources to be able to accommodate the new tasks without

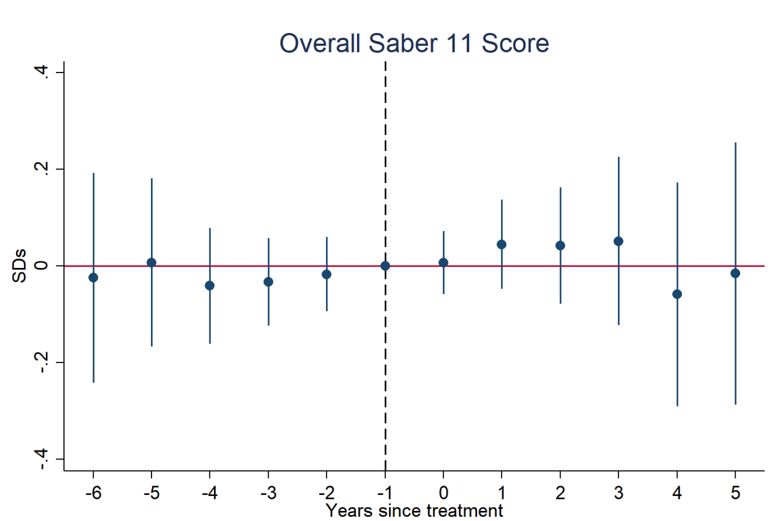
Table 1.4: Effect on mean *Saber 11* scores.

	(1)	(2)	(3)	(4)	(5)	(6)
		Subject-Specific Score				
	Combined Score	Math	Reading	Natural Science	Social Science	English
Water Treatment Facility	0.024	0.036	0.018	0.010	0.040	0.005
	(0.055)	(0.053)	(0.055)	(0.059)	(0.048)	(0.048)
N	23,904	23,904	23,904	23,904	23,904	23,904
Mean Dep. Var. in $g-1$	-0.525	-0.467	-0.432	-0.504	-0.434	-0.510

Standard errors clustered at the school level in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on standardized scores in *Saber 11*. Column 1 presents the effect on the overall (combined) scores, while columns 2-6 show the effects on subject-specific scores. The sample used for these estimates is made up of students from schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. Students in the sample took *Saber 11* between 2012 and 2019. The control group is made up of students enrolled in schools that had not yet received a water treatment facility in year t. Test scores are standardized using the national mean and standard deviation for the relevant academic year. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: ICFES (*Saber 11*), *Fundación EPM*



Notes: This figure depicts the event study aggregation of Callaway and Sant'Anna (2021) estimates (i.e., the average treatment effect for schools that have been treated for $e=t-g$ time periods—see equation (1.3)) of the effect on standardized global *Saber 11* scores. The point estimates measure the effect in relation to $g-1$, the year immediately before the delivery of the facility. The bars represent 95% confidence intervals. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. Data sources: ICFES (*Saber 11*), *Fundación EPM*

Figure 1.5: Event study aggregation of Callaway and Sant'Anna (2021) estimates of effects on standardized global *Saber 11* scores.

compromising the provision of other goods and services, other schools may need to reallocate resources away from their normal activities to operate the water treatment facility. In rural Colombia, there is significant heterogeneity in terms of school institutional capacity. The main schools in a school system often offer all grade levels and have enough students enrolled to be able to have at least one group per grade. The principals for the entire school system are based in the main school, which often enrolls students from multiple communities. In contrast, affiliated schools are more local in nature and offer only a subset of grade levels. These schools employ few teachers, who are also in charge of non-instruction duties that are typically performed by other staff members in larger schools, like feeding the students or cleaning the school.[28] While almost all schools that offer upper secondary level grades are larger main schools with more resources, the schools that offer primary level grades run the gamut from small schools with a single teacher to larger schools with dedicated teachers for each grade level. Thus, heterogeneous effects on enrollment by grade level could come from heterogeneity by school institutional capacity.

I assess whether differences in institutional capacity explain the observed pattern in enrollment by estimating heterogeneous effects on enrollment using two measures of institutional capacity. First, I estimate differential effects by whether the school is a main or an affiliated school. Second, I classify schools by whether their student-to-teacher ratio (STR) in the year immediately before the delivery of the water treatment facility is above or below the sample median (20 students per teacher). Table 1.5 shows the results of the heterogeneity analysis by main campus status whereas table 1.6 shows the results by pre-treatment STR. In schools with low capacity – i.e., affiliated schools and schools with an above-median STR – the introduction of the water treatment facility led to a decline in primary level enrollment. In contrast, I do not find statistically significant declines in primary-level enrollment in schools with higher capacity. At the upper secondary level, I find that the positive effect on enrollment is only present in main schools, whereas the effect in affiliated schools is negative and significant. Appendix Table 1.17 decomposes the effect on enrollment at the primary level by whether the school also offers lower and upper secondary. All else equal, schools that offer more grade levels should have more teachers and more resources than schools that only offer primary level grades. I find that the negative effect on enrollment at the primary level is only statistically significant in primary-only schools. Together, these estimates suggest that the introduction of a water treatment facility in schools with more institutional capacity had positive effects on student demand: enrollment increased at the higher grade levels and it was not affected at the lower grade levels. On the other hand, schools with limited capacity experienced decreases in enrollment after the delivery of the water treatment facility, which would be consistent with tradeoffs between instruction and the operation of the facility.

[28]These schools often rely on volunteers to help with some non-instruction duties. However, these volunteers are not full-time employees of the school and are not part of the school's payroll.

Table 1.5: Effects on enrollment by grade level and main campus status.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
		PANEL A: MAIN CAMPUS	
Water Treatment Facility	3.36	4.35	6.29***
	(3.89)	(4.66)	(2.23)
N	1,843	1,521	1,199
Mean Dep. Var. in $g-1$	117.77	142.62	44.02
		PANEL B: AFFILIATED CAMPUS	
Water Treatment Facility	-3.97***	-1.57	-3.63**
	(1.03)	(2.56)	(1.59)
N	5,599	1,373	309
Mean Dep. Var. in $g-1$	38.62	36.28	19.07
		PANEL C: DIFFERENCE	
Main - Affiliated	7.33	5.92	9.92
	(4.18)	(5.46)	(3.03)
90% CI	[0.66, 14.22]	[-3.02, 14.88]	[5.30, 15.45]
95% CI	[-0.58, 16.14]	[-4.74, 16.78]	[4.36, 16.61]

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panels A and B in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level and by whether the school is the main campus in the school system. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Panel C shows the difference between the effect for main and affiliated schools. The standard errors for this difference are calculated using a pair bootstrap procedure (see equation (1.5)). The 90% (95%) confidence intervals are constructed from the 5th (2.5th) and 95th (97.5th) percentiles of the empirical distribution of the bootstrap estimates for the difference between the point estimates for main and affiliated schools. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Table 1.6: Effects on enrollment by grade level and pre-treatment student-teacher ratio (STR).

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
	PANEL A: BASELINE STR ≤ 20		
Water Treatment Facility	-0.75	7.98***	6.01**
	(1.55)	(2.93)	(2.59)
N	3,146	1,199	619
Mean Dep. Var. in $g-1$	36.70	53.96	24.15
	PANEL B: BASELINE STR > 20		
Water Treatment Facility	-6.51***	-1.24	5.76**
	(1.71)	(4.36)	(2.52)
N	4,168	1,663	895
Mean Dep. Var. in $g-1$	74.13	113.79	46.64
	PANEL C: DIFFERENCE		
Lower STR - Higher STR	5.76	9.22	0.25
	(2.24)	(5.36)	(4.84)
90% CI	[2.09, 9.36]	[-0.01, 18.32]	[-5.83, 10.22]
95% CI	[1.59, 10.14]	[-1.62, 20.05]	[-7.63, 11.37]

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panels A and B in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level and by whether the school has a STR below the sample median of 20 in the year immediately before delivery. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Panel C shows the difference between the effect for schools with below- and above-median pre-treatment STR. The standard errors for this difference are calculated using a pair bootstrap procedure (see equation (1.5)). The 90% (95%) confidence intervals are constructed from the 5th (2.5th) and 95th (97.5th) percentiles of the empirical distribution of the bootstrap estimates for the difference between the point estimates for schools with below- and above-median pre-treatment STR. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

One potential explanation for these heterogeneous results is that the introduction of the facility could have affected the institutional capacity of recipient schools. I evaluate the effects of receiving a water treatment facility on school capacity by estimating the effects on the number and the characteristics of teachers. The results from this analysis (table 1.7) do not support the hypothesis that the water treatment facility induced changes in schools' institutional capacity. I do not find an effect on the number of teachers at any grade level nor on STRs. Likewise, I do not find changes in the proportion of teachers who hold a bachelor's degree nor on the proportion of teachers with degrees in education.

Table 1.7: Effects on the number of teacher and teacher characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of Teachers				Teachers Education		
	All Levels	Primary	Lower Secondary	Upper Secondary	College or Higher (%)	Education Degree (%)	Student-Teacher Ratio
Water Treatment Facility	0.13	-0.07	0.29	0.05	-0.005	-0.021	-0.52
	(0.10)	(0.06)	(0.19)	(0.24)	(0.022)	(0.008)	(0.40)
N	7,547	7,430	2,765	1,211	7,547	7,547	7,547
Mean Dep. Var. in $g-1$	4.67	2.39	4.10	2.40	0.792	0.968	25.08

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on the number of teachers by grade level (columns 1-4), the fraction of teachers with higher education degrees (column 5), the fraction of teacher whose degree is in education (column 6), on on the STR (column 7). The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

1.5.2.2 Changes in the Composition of the Student Body

Improving the availability of safe drinking water at schools may not only have affected the number of students enrolled in recipient schools; it may have also changed the type of students who enroll in these schools. For instance, in many contexts water collection is a task predominantly performed by women and girls (Gross et al., 2018). In such contexts, facilitating access to safe drinking water at schools could have a larger effect on enrollment for girls since they would be more likely to benefit from the time savings that convenient access to water represents. Panels A and B of table 1.8 show that at all grade levels, the effects of safe drinking water at school on enrollment for boys and girls are similar in magnitude. Thus, this intervention does not appear to affect the student gender ratio at recipient schools.[29]

Panels C and D of table 1.8 show the effect on student enrollment by ethnic minority status, while panels E and F show the effect by armed conflict victim status. The changes in enrollment are approximately proportional to the fraction of students in each group prior to the intervention, which suggests that there are no changes in the demographic characteristics of the student body of recipient schools after the installation of the water treatment facility. Appendix Table 1.18 shows that the proportion of students who belong to

[29] Koolwal and Van de Walle (2013) and Nauges and Strand (2017) also found no differential effect by gender on schooling of improved access to safe drinking water.

each demographic group was not affected by the availability of safe drinking water. As a whole, the lack of heterogeneity in the effect on enrollment indicates that observable student characteristics do not predict who are the marginal students who change their enrollment decision after the introduction of the water treatment facility.

Table 1.8: Effects on enrollment by grade level for different types of students.

	(1) Primary	(2) Lower Secondary	(3) Upper Secondary
	PANEL A: BOYS		
Water Treatment Facility	-0.94	0.25	2.27**
	(0.94)	(1.63)	(1.12)
N	6,766	2,703	1,509
Mean Dep. Var. in $t-1$	31.68	45.37	17.52
	PANEL B: GIRLS		
Water Treatment Facility	-1.22	3.13*	3.23**
	(0.79)	(1.80)	(1.29)
N	6,766	2,703	1,509
Mean Dep. Var. in $t-1$	28.25	45.33	20.46
	PANEL C: ETHNIC MINORITIES		
Water Treatment Facility	-0.80	0.96	0.55
	(0.87)	(3.29)	(1.88)
N	6,259	2,375	1,228
Mean Dep. Var. in $g-1$	4.30	9.00	4.39
	PANEL D: NON-ETHNIC MINORITIES		
Water Treatment Facility	-1.21	4.01	5.48**
	(1.62)	(4.23)	(2.73)
N	6,259	2,375	1,228
Mean Dep. Var. in $g-1$	49.50	73.73	32.87
	PANEL E: VICTIMS		
Water Treatment Facility	0.63	2.56	3.86*
	(1.33)	(4.29)	(2.03)
N	5,522	2,099	1,091
Mean Dep. Var. in $g-1$	9.08	17.67	7.36
	PANEL F: NON-VICTIMS		
Water Treatment Facility	-2.80	3.17	2.95
	(1.93)	(5.08)	(2.92)
N	5,522	2,099	1,091
Mean Dep. Var. in $g-1$	46.69	68.16	31.04

Standard errors clustered at the school level in parentheses.

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Notes: Panels A and B in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by gender and grade level. Panels C and D show the effect by ethnic minority status, while panels E and F show the effect by armed conflict victim status. The sample used for the estimates in this table is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data on gender composition and ethnic minority status are unavailable for 2017 for some schools, while data on armed conflict victim status are unavailable for 2014 and 2017 for all schools. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

1.5.2.3 Substitution Between Water at Home and Water at School

The introduction of a water treatment facility implies that schools are now able to offer a new good – safe drinking water – that students may or may not have access to at home. For students without access to safe drinking water at home, the availability of water at school may play a more important role in their school choice than for students who have access to it. If this is the case, the effects of the availability of safe drinking water at school on enrollment should be more pronounced in areas where residential access to safe drinking water is relatively scarce. To test this hypothesis, I use geolocated data at the census block level from the 2005 population census to calculate the proportion of households living within a 5-km radius of the school that have access to piped water.[30] I classify schools by whether the percentage of households in the vicinity of the school with access to piped water is above or below 50%, the national median coverage of water networks in rural areas.

Table 1.9 shows the effects on enrollment disaggregated by the local availability of piped water. I do not find statistically significant differences in the effects on enrollment at any grade level for schools in areas with lower or higher access to residential piped water. These findings suggest that the valuation of safe drinking water as a school amenity may not depend on the local availability of piped water.

1.5.2.4 General Equilibrium Effects on Enrollment in Treated Municipalities

As mentioned in section 1.5.1, the effects of installing a water treatment facility at schools are driven by new rather than by current students. These changes in new enrollment could represent adjustments at the extensive or at the intensive margin. Extensive margin responses correspond to changes in the decision of whether to enroll in school whereas intensive margin responses refer to changes in school choice conditional on enrollment. Since enrollment at the primary and lower secondary level in Colombia is compulsory and nearly universal, changes in enrollment at these grade levels are more likely to reflect changes in school choice. In contrast, changes in enrollment from students who would not have otherwise enrolled in school are more likely to occur at the upper secondary level, where enrollment is optional. To identify whether the observed effects on enrollment correspond to changes at the extensive or the intensive margin, I estimate the effect of the delivery of water treatment facilities on municipality-level enrollment.[31] These estimates are shown in table 1.10. There is no evidence of an effect on municipality-level enrollment at any grade

[30] As the discussion in section 1.2.1 points out, in the Colombian context piped water is not always safe for human consumption. I assume that the percentage of households with access to safe drinking water is directly related to the percentage of households with access to piped water. In any case, the availability of piped water indicates that the household has access to a convenient source of water, even if the water needs to be treated before it is used.

[31] I define treatment at the municipality level as the first year in which a water treatment facility was delivered to a school that offered a given grade level. For instance, suppose that in a given municipality, the first delivery was to a primary-only school in 2013 and that the first recipient school that offers lower secondary level took delivery of their facility in 2016. In that case, the municipality is considered treated since 2013 when estimating the effect on enrollment at the primary level and since 2016 when estimating the effect on enrollment at the lower secondary level.

Table 1.9: Effects on enrollment by grade level and by local availability of piped water.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
	PANEL A: RURAL WATER COVERAGE $< 50\%$		
Water Treatment Facility	-2.24	3.49	6.81***
	(2.24)	(3.58)	(2.27)
N	3,663	1,695	901
Mean Dep. Var. in $g-1$	71.32	101.43	38.84
	PANEL B: RURAL WATER COVERAGE $\geq 50\%$		
Water Treatment Facility	-3.77***	6.50**	6.19
	(1.34)	(3.72)	(3.96)
N	3,778	1,199	646
Mean Dep. Var. in $g-1$	43.67	68.40	34.78
	PANEL C: DIFFERENCE		
Lower Coverate - Higher Coverage	1.54	-3.00	0.62
	(2.59)	(5.50)	(5.24)
90% CI	[-2.38, 6.19]	[-12.11, 5.78]	[-7.86, 9.25]
95% CI	[-3.32, 7.17]	[-14.02, 7.38]	[-9.35, 10.57]

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panels A anb B in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level and by the coverage of piped water in the area surrounding the school. Local coverage of piped water is calculated using data from the 2005 Population Census at the census block level. The percentages are calculated by dividing the number of households living in census blocks within 5 km of the school that were connected to piped water by the total number of households in these census blocks. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Panel C shows the difference between the effect for schools in areas with below- and above-median coverage of piped water. The standard errors for this difference are calculated using a pair bootstrap procedure (see equation (1.5)). The 90% (95%) confidence intervals are constructed from the 5th (2.5th) and 95th (97.5th) percentiles of the empirical distribution of the bootstrap estimates for the difference between the point estimates for schools in areas with below- and above-median coverage of piped water. Data sources: DANE (annual school censuses - *Formulario C600*, 2005 Population Census, SISE), *Fundación EPM* level. This finding suggests that the introduction of water treatment facilities did not induce new demand for schooling; instead, the changes in enrollment can be attributed to changes in school choice.

Table 1.10: Effects on total enrollment at the municipality level by grade level.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
Water Treatment Facility	-59.02	51.21	16.82
	(50.85)	(40.28)	(16.26)
N	1,004	1,140	999
Mean Dep. Var. in $g-1$	2,561.48	1,560.40	465.79

Standard errors clustered at the municipality level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of the first water treatment facility in a municipality on total enrollment in levels in the municipality by grade level. The sample used for these estimates is made up of the 112 municipalities in the state of Antioquia outside of the Medellin metropolitan area where at least one water treatment facility delivery has been delivered through AEEA. For each municipality, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for these estimates is made up of municipalities where no delivery had taken place yet in year t at a school that offers the relevant grade level. Total enrollment includes schools in urban and rural areas. The mean of the dependent variable is measured in the year immediately before the delivery of the first water treatment facility in the municipality. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundacion EPM*

If the effects of safe drinking water availability on enrollment are due to changes in school choice, enrollment in never-treated schools could also be affected by the intervention. While the school-level data I use does not allow me to identify the school in which incoming students were previously enrolled or the school in which outgoing students go on to enroll, it is reasonable to assume that neighboring schools are potential alternatives to treated schools. If the effect on enrollment is driven by changes in school choice between a treated school and a nearby never-treated school, the effect of the introduction of the water treatment facility on enrollment at nearby never-treated schools should be the opposite of the effect on enrollment at treated schools.

To test this hypothesis, I identify the closest never-treated school to each treated school. Then, I compare enrollment in schools nearby an early-treated school to enrollment in schools nearby a late-treated school. I show the results of this analysis in Appendix Table 1.20. Although I do not find any statistical significant effect on enrollment in never-treated schools, the point estimates have the opposite signs of the effects on enrollment in ever-treated schools. A limitation of this analysis is that it assumes that students' school choice sets are made up of *only* the treated school and the closest (never-treated) alternative. In practice, students' school choice sets may encompass more than these two schools and it is not possible to determine the origin or destination of all the demand for schooling that was displaced by the introduction of the water treatment facility.

1.5.3 Robustness Checks

I now present analyses to assess the plausibility of alternative explanations of the observed effects, as well as additional tests to evaluate the robustness of the results to changes in the empirical specification.

1.5.3.1 Using Never-Treated Schools as Controls

The analysis in this paper uses only ever-treated schools and exploits variation in the timing of the delivery of the water treatment facility. Because schools need to meet particular eligibility requirements (as explained in section 1.2.3) not every never-treated school is a suitable control and the evolution of enrollment and achievement outcomes in these schools may differ from that in ever-treated schools. However, it is possible to approximate which schools would be suitable controls by identifying the never-treated schools that are within a given distance of an ever-treated school. Appendix Table 1.19 shows the estimated effect on enrollment using these nearby never-treated schools as controls. These results are similar to the ones obtained using only ever-treated schools. This estimation strategy has an important caveat: a student transferring from say, a control school to a treated school, would be recorded as one more student for the treated school *and* one less student for the control school. These spillovers would bias the estimates away from zero.[32]

1.5.3.2 Demographic Changes as Confounders

The identifying assumption states that in the absence of the delivery of the water treatment facilities, the outcomes of early- and late-treated schools would have evolved in a similar way. An implication of this assumption is that there should not be a factor that predicts both treatment timing and the outcome of interest. One such factor are municipality-level demographic changes. If early deliveries are taking place in municipalities that are experiencing relatively larger declines in fertility or increases in outmigration, the estimated coefficient would conflate the effect of these demographic changes with the effect of the installation of the water treatment facility.

To assess whether these municipal-level demographic pose a threat to the identification of a causal effect, I estimate the effect of the first delivery of a facility in a municipality on the log of total population and the log of school-age population (ages 5-17). I obtain these data from municipality-level population projections that are calculated by DANE using data from the 2005 and 2018 population censuses. I present the results from this analysis in table 1.21 of Appendix 1.10. Demographic changes do not predict the timing of the delivery of the first water treatment facility within a municipality. Therefore, the observed effect on enrollment and achievement are unlikely to be due to population changes.

[32] In principle, double-counting is also possible in the analysis that only uses ever-treated schools. However, it would only occur if a student transfers between ever-treated schools. In contrast, the double-counting issue when never-treated schools are part of the control group is likely to be more prevalent since it can occur when students transfer between ever-treated schools or between ever-treated and nearby never-treated schools.

1.5.3.3 Adding Pre-Treatment Covariates

The Callaway and Sant'Anna (2021) framework allows the inclusion of pre-treatment controls that could predict treatment timing. This change implies a modification to the parallel trends assumption; conditional on pre-treatment characteristics and in the absence of the delivery of a water treatment facility, the outcomes of interest in early and late treated would have evolved in a similar way. I check whether pre-treatment characteristics predict the timing of the delivery of the water treatment facility by estimating the effect on enrollment after controlling for covariates, one covariate at a time. I control for municipality-level characteristics like the coverage rates of water networks and the multidimensional poverty index, and for school-level characteristics like pre-treatment STR and distance to the nearest town. In Appendix Table 1.22, I show that the effects on enrollment after controlling for covariates are qualitatively similar to the effects estimated without covariates.

1.5.3.4 Using Alternative Data Sources for Enrollment

The enrollment data I use for the main analyses come from the annual school censuses carried out by DANE, Colombia's national office of statistics. Enrollment data are also collected by the Colombian National Ministry of Education as part of its Integrated Enrollment System (SIMAT - *Sistema Integrado de Matrícula*). SIMAT consists of individual-level records on enrollment for every student enrolled in any school in the country—public or private— since 2010. The National Ministry of Education publishes annual enrollment counts at the school-by-grade-by-gender level based on aggregations from the SIMAT individual-level data. I use the public-use enrollment counts from SIMAT as an alternative source of data to estimate the effects of AEEA on enrollment. The results of this estimation are presented in Appendix Table 1.23. These estimates are similar to the ones I obtain using the school census data. Moreover, as Appendix Table 1.24 shows, the heterogeneous treatment effects by school institutional capacity are also evident using the SIMAT data. While the results on enrollment do not depend on the data source, using the school census data allows me to be consistent and use the same data source I use for other outcomes like number of teachers, student-teacher ratios, or number of students who drop out or transfer.

1.5.3.5 Estimating Alternative Estimators for Staggered Differences-in-Differences Designs

The Callaway and Sant'Anna (2021) estimator is not the only available alternative to estimate treatment effects in differences-in-differences designs with variation in treatment timing. Borusyak et al. (2024) propose an imputation-based estimator in which not-yet-treated units are used to model untreated potential outcomes. Their approach consists of estimating the following two-way fixed effect model using only observations for not-yet-treated schools s and time periods t:

$$Y_{st} = \alpha_s + \delta_t + \varepsilon_{st}.$$

Then, the predicted values from this regression ($\hat{Y}_{st}$) can be used to infer the untreated potential outcome for each school. The Borusyak et al. (2024) estimator compares the treated outcomes for each school with this imputed potential outcome—i.e., the ATT is given by $Y_{st} - \hat{Y}_{st}$. I estimate the effects of AEEA on enrollment using the Borusyak et al. (2024) estimator and present the results in Appendix Table 1.25. In general, the results using this estimator are comparable in direction and magnitude to the main results of the paper using the Callaway and Sant'Anna (2021) estimator. The key difference between the two estimators is that Callaway and Sant'Anna (2021) compares treated outcomes to the outcome in the time period immediately before treatment whereas the Borusyak et al. (2024) estimator compares treated outcomes to the average imputed outcome in the pre-treatment period. Roth et al. (2023) note that while the use of imputation may improve precision in some cases, it involves imposing a stronger parallel-trends assumption since parallel trends should hold for all periods and all treatment timing groups while the Callaway and Sant'Anna (2021) estimator only requires parallel trends in the time periods after the first unit receives treatment.

1.6 Effects on Health Outcomes

One of the main motivations to improve accessibility to safe drinking water to schools is to reduce the prevalence of waterborne diseases. To assess the effectiveness of AEEA water treatment facilities in improving community health, I use municipality-level incidence rates of acute diarrheal diseases (ADDs).[33] There is a close link between ADD incidence and the consumption of contaminated water. ADDs may cause dehydration and are the second most common cause of mortality among children less than five years old worldwide (WHO, 2017). Even though AEEA is a school-based intervention, it could have affected the health of the entire municipality through two channels. First, the water from the facilities can be used by all members of the community, even if they do not have family members studying or working at the school. Second, improvements in health for the children attending recipient school should reduce the probability of other members of the community getting sick from contagious diseases.

The results of the municipality-level analysis are shown in table 1.11 and figure 1.6. I do not detect an effect of treatment on ADD incidence rates for the overall population or for any age group. However, this null effect is imprecisely estimated given the limited data availability. Even though the existing evidence on the health impacts of AEEA is inconclusive, there are some potential explanations that could rationalize a null effect on community-level health outcomes. First, it could be that although the water treatment facility is

[33] It would be ideal to use a school-level outcome such as student absenteeism or the number of cases of waterborne diseases among students to examine whether AEEA is effective at improving student health. However, these outcomes are not observable in administrative records.

supposed to be used by the community, in practice its use may be limited to the school community. Second, the water consumed at school may represent a small fraction of the total water consumption, with unsafe sources making up the remaining amount of water consumed. In that case, waterborne diseases would still be possible even among the population that routinely uses the water treatment facility. Third, while the water from the facility may be of better quality, community members may prefer to access more convenient sources of water like the one coming from informal water networks.[34] Testing these hypotheses would require data on individual or household-level water consumption and water sources used.

Table 1.11: Effects on ADD incidence rates per 100,000 inhabitants at the municipality level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Rates	Ages 0-4	Ages 5-9	Ages 10-14	Ages 15-19	Ages 20+
First Delivery	-42.1	215.9	-691.3	-43.3	-437.6	98.9
	(435.9)	(1,473.7)	(568.7)	(431.8)	(392.9)	(333.1)
N	408	408	408	408	408	408
Mean Dep. Var. (2013)	3,253.3	11,004.2	3,599.6	2,449.9	2,292.9	2,407.0

Standard errors clustered at the municipality level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

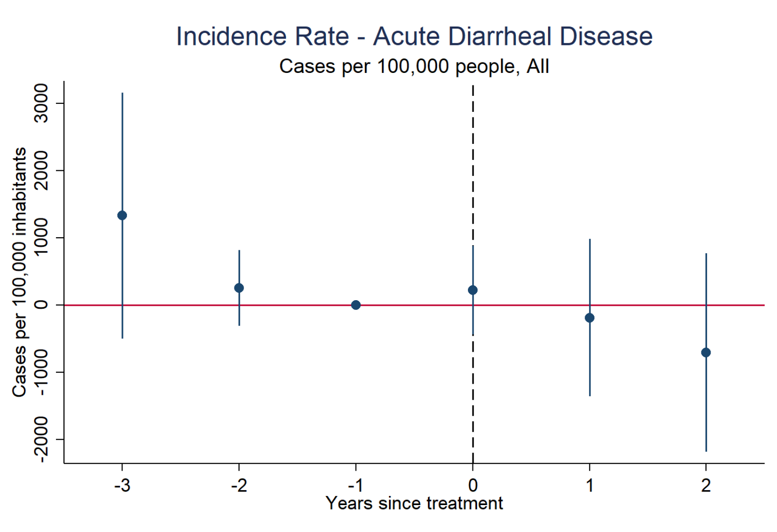
Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of the first water treatment facility in a municipality on the incidence rates of ADD for the overall population (column 1) and for different age groups (columns 2-6). Incidence rates represent the number of cases of ADD per 100,000 inhabitants. The sample used for these estimates is made up of the 112 municipalities in the state of Antioquia outside of the Medellin metropolitan area where at least one water treatment facility delivery has been delivered through AEEA. The control group for these estimates is made up of municipalities where no delivery had taken place yet in year t. The mean of the dependent variable is measured in 2013, the first year with available data. Data sources: DSSA (municipal reports of cases of ADD per 100,000 inhabitants), *Fundación EPM*

1.7 Conclusion

Despite important investments in recent decades, much progress needs to be made in order to achieve universal access to safe drinking water in schools. In this paper, I show that the installation of water treatment facilities at schools may induce changes in school choice, but only in schools with adequate institutional capacity to operate the facility. In schools with insufficient capacity, the introduction of a facility leads to decreases in enrollment at the primary level, likely from students shifting to different schools. These reductions in enrollment suggest that for these schools, the upkeep of the water treatment facility may compromise the provision of other goods and services.

The rationale for using schools to deliver social assistance programs targeted at children is that the use of existing infrastructure reduces costs, increases program take-up, and can complement the process of human capital formation. The findings from this paper suggest that in order to avoid unintended consequences, it is necessary to assess the capacity of the implementing partner to take on new responsibilities without compromising its current duties. A related implication for program design is that the tasks assigned to each

[34] Another possible explanation is that since the data on ADDs incidence rate does not distinguish between urban and rural areas, the null effects may be driven by urban areas.



Notes: This figure depicts the event study aggregation of Callaway and Sant'Anna (2021) estimates (i.e., the average treatment effect for municipalities that have been treated for $e = t - g$ time periods—see equation (1.3)) of the effect on municipality-level incidence rates of acute diarrheal diseases per 100,000 inhabitants. The point estimates measure the effect in relation to $g - 1$, the year immediately before the delivery of the first water treatment facility in the municipality. The bars represent 95% confidence intervals. The control group for these estimates is made up of municipalities where no delivery had taken place yet in year t. Data sources: DSSA (municipal reports of cases of ADD per 100,000 inhabitants), *Fundación EPM*

Figure 1.6: Event study aggregation of Callaway and Sant'Anna (2021) estimates of effects on municipality-level incidence rates of acute diarrheal diseases per 100,000 inhabitants.

agent involved in the implementation of the intervention should be commensurate with their capability to execute them and that it may be necessary to complement the additional responsibilities from the intervention with additional resources.

One of the goals of installing water treatment facilities at schools is to improve access to safe drinking water to the entire community. I do not find evidence to suggest that improved water availability at schools translates into reduced disease incidence. A null effect on health outcomes would be consistent with recipient schools being capacity constrained: if schools are struggling to operate the water treatment facility for their own students, it is unlikely they can act as water suppliers for the entire community. However, given the limitations of the data on disease incidence, more research is needed to assess the potential for health spillovers of a school-based approach to expand access to safe drinking water in rural communities.

There are other questions that also merit further research. The context of this paper is one where access to water is not universal but it is higher than in other regions of the world. In places that are more deprived of safe drinking water, improving access to this resource at school may have larger impacts on human capital accumulation. Another important question is whether access to safe drinking water affects school attendance and learning for younger students. Since institutional capacity seems to be key in the success of school-based interventions to improve water access, it is important to identify the specific school activities that may be displaced by the operation of a water treatment facility in school. Finally, while the discussion in this paper about institutional capacity has focused on resource availability, institutional capacity may also manifest in school management practices that may facilitate or complicate the introduction of new tasks. More research is needed to understand whether school managerial quality translates into better student outcomes.

1.8 Appendix 1: Time Series of Enrollment in Ever-Treated and Never-Treated Schools

Figure 1.7: Average enrollment by grade level in ever-treated schools by year of delivery

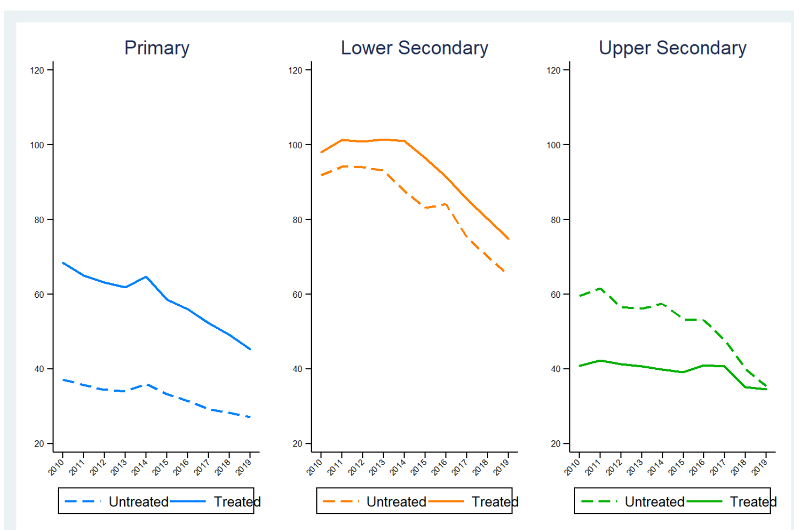


Figure 1.8: Average enrollment by grade level in ever-treated (solid line) and never-treated (dashed line) rural schools in the state of Antioquia

1.9 Appendix 2: Variation in Treatment Timing for Municipality-Level Analysis

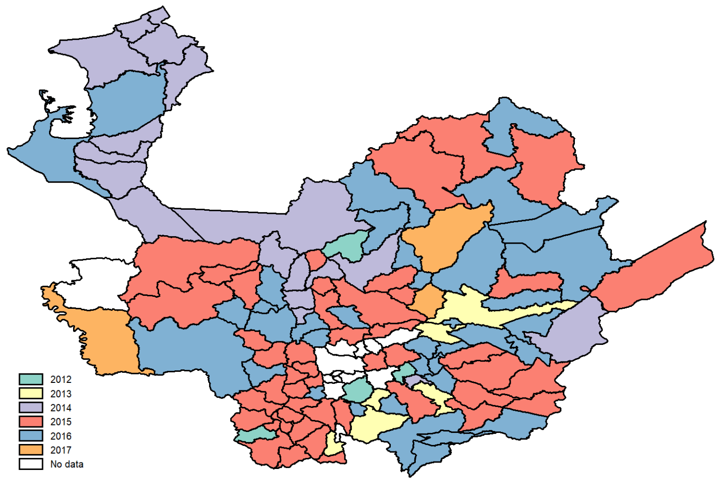


Figure 1.9: Map of municipalities in the state (*departamento*) of Antioquia by first year of delivery of water treatment facility.

1.10 Appendix 3: Additional Results

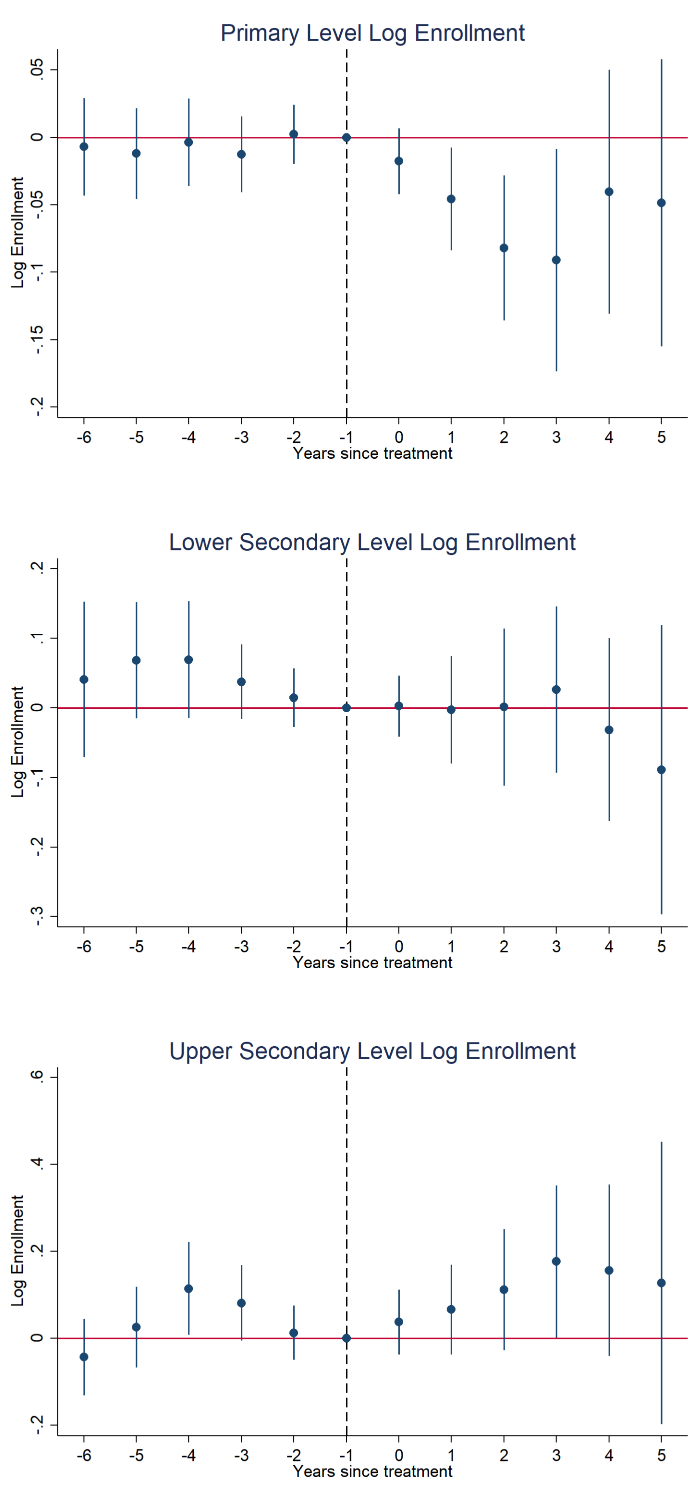
Table 1.12: Effects on log enrollment by grade level.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
Water Treatment Facility	-0.023	-0.054**	-0.000	0.100*
	(0.024)	(0.021)	(0.038)	(0.053)
N	7,546	7,473	2,894	1,549
Mean Dep. Var. in $g-1$	3.99	3.66	4.00	3.33

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment by grade level in logs. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*



Notes: This figure depicts the event study aggregation of Callaway and Sant'Anna (2021) estimates (i.e., the average treatment effect for schools that have been treated for $e = t - g$ time periods—see equation (1.3)) of the effect on log enrollment at the primary (top), lower secondary (middle), and upper secondary (bottom) levels. The point estimates measure the effect in relation to $g - 1$, the year immediately before the delivery of the facility. The bars represent 95% confidence intervals. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Figure 1.10: Event study aggregation of Callaway and Sant'Anna estimates of effects on log enrollment by grade level

Table 1.13: Effects on dropout and transfer rates by grade level.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
	PANEL A: DROPOUT RATES		
Water Treatment Facility	-0.005	-0.003	-0.011
	(0.006)	(0.009)	(0.014)
N	7,304	2,845	1,523
Mean Dep. Var. in $g-1$	0.047	0.088	0.060
	PANEL B: TRANSFER RATES		
Water Treatment Facility	-0.014	0.005	0.006
	(0.008)	(0.009)	(0.012)
N	7,304	2,845	1,523
Mean Dep. Var. in $g-1$	0.057	0.044	0.039

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panel A in this table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on dropout rates (i.e., the number of students who drop out during the previous academic year divided by total enrollment in the previous year). Panel B shows the effect on transfer rates (i.e., the number of students who transfer out to a different school during the previous academic year divided by total enrollment in the previous year). The estimates in this table are weighted by pre-treatment enrollment. The sample used for the estimates in this table is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Table 1.14: Effects on enrollment by age and grade level.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
		PANEL A: YOUNGER STUDENTS	
$\hat{\theta}^{S}$	-2.08*	4.35	2.68**
	(1.14)	(3.40)	(1.15)
N	6,373	1,790	1,097
Mean Dep. Var. in $t-1$	28.45	44.39	10.78
		PANEL B: OLDER STUDENTS	
$\hat{\theta}^{S}$	0.25	3.62	2.63
	(1.23)	(4.31)	(1.87)
N	6,691	2,458	1,489
Mean Dep. Var. in $t-1$	34.50	61.17	30.54

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panels A and B in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level for students that given their age, should be enrolled in the first grades of the respective grade level and are therefore more likely to be new students. Panel B shows the effects on enrollment in levels by grade level for students that given their age, should be enrolled in later grades of their grade level. The age limits between younger and older levels are 8 (primary), 12 (lower secondary), and 15 (upper secondary). Students whose age is at the limit are included in panel A. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Table 1.15: Effect on mean *Saber 11* scores by student characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)
		Subject-Specific Score				
	Global Score	Math	Reading	Natural Science	Social Science	English
			PANEL A: BOYS			
Water Treatment Facility	0.006	0.007	0.028	-0.003	0.014	-0.011
	(0.054)	(0.055)	(0.053)	(0.060)	(0.056)	(0.050)
N	10,464	10,464	10,464	10,464	10,464	10,464
Mean Dep. Var. in $g-1$	-0.478	-0.349	-0.464	-0.429	-0.430	-0.500
			PANEL B: GIRLS			
Water Treatment Facility	0.039	0.063	0.010	0.020	0.058	0.020
	(0.066)	(0.059)	(0.074)	(0.068)	(0.059)	(0.056)
N	13,440	13,440	13,440	13,440	13,440	13,440
Mean Dep. Var. in $g-1$	-0.562	-0.561	-0.407	-0.564	-0.438	-0.517
			PANEL C: LOWER SES			
Water Treatment Facility	0.039	0.066	0.042	0.016	0.037	-0.000
	(0.064)	(0.070)	(0.065)	(0.065)	(0.057)	(0.049)
N	7,803	7,803	7,803	7,803	7,803	7,803
Mean Dep. Var. in $g-1$	-0.663	-0.612	-0.567	-0.623	-0.545	-0.605
			PANEL D: HIGHER SES			
Water Treatment Facility	0.049	0.028	0.019	0.057	0.083	0.029
	(0.059)	(0.053)	(0.051)	(0.070)	(0.067)	(0.064)
N	8,686	8,686	8,686	8,686	8,686	8,686
Mean Dep. Var. in $g-1$	-0.379	-0.325	-0.281	-0.395	-0.306	-0.444

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on standardized scores in *Saber 11* for different types of students. Panels A and B disaggregate the effect by gender, whereas panels C and D disaggregate the effect by socioeconomic status. The disaggregation by socioeconomic status is conducted using the socioeconomic status index developed by ICFES. A student is considered to be lower SES is they are classified in the bottom category of the index. Higher SES students are classified in the remaining three categories of the index. Column 1 presents the effect on the overall (combined) scores, while columns 2-6 show the effects on subject-specific scores. The sample used for these estimates is made up of students from schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. Students in the sample took *Saber 11* between 2012 and 2019. The control group is made up of students enrolled in schools that had not yet received a water treatment facility in year t. Test scores are standardized using the national mean and standard deviation. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: ICFES (*Saber 11*), *Fundación EPM*

Table 1.16: Effect on percentiles of *Saber 11* scores.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mean	Median	10th pctile	25th pctile	75th pctile	90th pctile	Prop. above nat'l mean
Water Treatment Facility	0.024	0.017	0.031	-0.004	0.061	0.052	0.006
	(0.065)	(0.071)	(0.059)	(0.068)	(0.074)	(0.081)	(0.029)
N	1,196	1,196	1,196	1,196	1,196	1,196	1,196
Mean Dep. Var.	-0.525	-0.577	-1.329	-1.008	-0.107	0.383	0.232

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on standardized scores in *Saber 11*. Estimates are weighted by the number of test takers. Column 1 presents the effect on the overall (combined) scores, while columns 2-6 show the effects on subject-specific scores. The sample used for these estimates is made up of students from schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. Students in the sample took *Saber 11* between 2012 and 2019. The control group is made up of students enrolled in schools that had not yet received a water treatment facility in year t. Test scores are standardized using the national mean and standard deviation. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: ICFES (*Saber 11*), *Fundación EPM*

Table 1.17: Effects on enrollment at the primary level by grade levels offered at the school

	(1)	(2)	(3)	(4)
	All Schools	Primary Only	Primary and LS	Primary, LS, and US
Water Treatment Facility	-2.64*	-3.37**	-0.76	1.56
	(1.36)	(1.38)	(2.35)	(3.52)
N	7,473	3,217	4,249	2,366
Mean Dep. Var. $(t-1)$	66.07	44.26	82.33	108.93

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment at the primary level by the grade levels offered by the school. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Table 1.18: Effects on enrollment by grade level for different types of students.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
	PANEL A: FRACTION GIRLS		
Water Treatment Facility	-0.002	0.012	0.017
	(0.008)	(0.014)	(0.018)
N	6,766	2,703	1,509
Mean Dep. Var. in $t-1$	0.466	0.504	0.547
	PANEL B: FRACTION ETHNIC MINORITIES		
Water Treatment Facility	-0.012	0.011	0.001
	(0.019)	(0.037)	(0.045)
N	5,525	2,039	1,006
Mean Dep. Var. in $t-1$	0.035	0.049	0.079
	PANEL C: FRACTION VICTIM OF ARMED CONFLICT		
Water Treatment Facility	0.039	0.022	0.077
	(0.029)	(0.055)	(0.055)
N	4,815	1,768	874
Mean Dep. Var. in $g-1$	0.179	0.201	0.176

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panel A in this table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on the fraction of female students by grade level. Panels B and C show the effect on the fraction of students in each grade level who belong to ethnic minorities or who are victims of the armed conflict. The estimates in these panels are weighted by pre-treatment enrollment. The sample used for the estimates in this table is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data on gender composition and ethnic minority status are unavailable for 2017 for some schools, while data on armed conflict victim status are unavailable for 2014 and 2017 for all schools. Data sources: DANE (annual school censuses - *Formulario C600*), *Fundación EPM*

Table 1.19: Effects on enrollment by grade level using never-treated schools as controls.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
			PANEL A: $<$ 1 KM	
$\hat{\theta}^S$	-4.24**	-3.36***	-3.23	0.05
	(2.16)	(1.06)	(3.87)	(3.17)
N	14,708	14,573	3,494	1,837
Mean Dep. Var. (2011)	86.92	57.30	100.51	45.57
			PANEL B: $<$ 5 KM	
$\hat{\theta}^S$	-5.68***	-5.24***	-0.39	4.44***
	(1.55)	(0.87)	(1.96)	(1.50)
N	39,494	39,149	6,687	2,919
Mean Dep. Var. (2011)	60.90	42.58	89.81	51.72
			PANEL C: $<$ 10 KM	
$\hat{\theta}^S$	-4.91***	-4.46***	-0.32	3.45***
	(1.58)	(0.952)	(1.87)	(1.33)
N	49,361	48,953	8,096	3,565
Mean Dep. Var. (2011)	61.07	42.94	94.64	52.63

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t and of never-treated schools within 1 km (panel A), 5 km (panel B), or 10 km (panel C) of an ever-treated school. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*, SISE), *Fundación EPM*

Table 1.20: Effects on enrollment by grade level in never-treated schools in the vicinity of ever-treated schools.

	(1)	(2)	(3)
	Primary	Lower Secondary	Upper Secondary
Water Treatment Facility	0.90	-2.11	-0.53
	(0.729)	(4.294)	(2.201)
N	20,561	2,241	832
Mean Dep. Var. (t-1)	30.49	87.39	61.48

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level in the closest never-treated schools to ever-treated schools. Treatment timing is assigned based on the year in which the relevant ever-treated school received its water treatment facility. The control group for these estimates is made up of never-treated schools that are nearby an ever-treated school that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*, SISE), *Fundación EPM*

Table 1.21: Effect on municipality population (in logs).

	(1)	(2)
	Log Total Population	Log School-Age Population
Water Treatment Facility	-0.003	-0.005
	(0.004)	(0.007)
N	1,001	1,001
Mean Dep. Var. (t-1)	9.68	8.27

Standard errors clustered at the municipality level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of the first water treatment facility in a municipality on the log of total population (column 1) and on the log of the population ages 5-17 (column 2). The control group for these estimates is made up of municipalities where no delivery had taken place yet in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the first water treatment facility in the municipality. Data sources: DANE (municipality population projections), *Fundación EPM*

Table 1.22: Effects on enrollment by grade level including potential confounders.

	(1)	(2)	(3)	(4)
	PANEL A: ALL GRADE LEVELS			
Water Treatment Facility	-3.76*	-3.77**	-4.73*	-3.29
	(2.11)	(1.89)	(2.51)	(2.08)
N	5,948	5,948	5,948	5,948
Mean Dep. Var.	93.98	93.98	93.98	93.98
	PANEL B: PRIMARY			
Water Treatment Facility	-5.11***	-5.07***	-4.14***	-4.57***
	(1.16)	(1.06)	(1.10)	(1.15)
N	5,923	5,923	5,923	5,923
Mean Dep. Var.	54.13	54.13	54.13	54.13
	PANEL C: LOWER SECONDARY			
Water Treatment Facility	2.69	1.38	0.98	1.41
	(2.22)	(2.24)	(2.84)	(2.49)
N	2,170	2,170	2,170	2,170
Mean Dep. Var.	81.98	81.98	81.98	81.98
	PANEL D: UPPER SECONDARY			
Water Treatment Facility	5.90***	6.81***	7.29***	5.58***
	(2.14)	(2.43)	(1.93)	(1.96)
N	1,122	1,122	1,122	1,122
Mean Dep. Var.	35.84	35.84	35.84	35.84
Confounders				
Water Network Coverage	✓			
Rural Multidimensional Poverty Index		✓		
Pre-Treatment STR			✓	
Distance Nearest Town				✓

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates (i.e., the average treatment effect for all units across all time periods and all treatment timing groups—see equation (1.2)) of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level. Each column controls for one potential time-invariant confounder. The estimating sample only includes schools for which all confounders are observed in the pre-treatment period. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (annual school censuses - *Formulario C600*, 2005 Population Census, SISE), *Fundación EPM*

Table 1.23: Effects on enrollment by grade level using different data sources.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
	PANEL A: SCHOOL CENSUS (C-600)			
Water Treatment Facility	0.20	-2.64*	2.93	5.24***
	(2.31)	(1.36)	(2.70)	(1.97)
N	7,546	7,473	2,894	1,549
Mean Dep. Var. in $g-1$	101.18	57.39	87.45	37.18
	PANEL B: SIMAT			
Water Treatment Facility	1.07	-1.59	3.79	4.98**
	(2.66)	(1.46)	(3.06)	(2.46)
N	6,889	6,820	2,662	1,406
Mean Dep. Var. in $g-1$	108.69	56.03	87.26	37.48

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Panels A uses data from the annual school censuses (C-600), while panel B uses data from administrative student-level records from Colombia's National Ministry of Education (SIMAT). Data sources: DANE (*Formulario C-600*), Colombia's National Ministry of Education (SIMAT), *Fundación EPM*.

Table 1.24: Effects on enrollment by grade level and capacity measures using SIMAT data.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
		PANEL A: ALL SCHOOLS		
Water Treatment Facility	1.07	-1.59	3.79	4.98**
	(2.66)	(1.46)	(3.06)	(2.46)
N	6,889	6,820	2,662	1,406
Mean Dep. Var. in $g-1$	108.69	56.03	87.26	37.48
		PANEL B: MAIN CAMPUS		
Water Treatment Facility	15.76**	4.98	5.12	6.02**
	(7.58)	(4.43)	(5.21)	(2.82)
N	1,683	1,628	1,341	1,071
Mean Dep. Var. in $g-1$	276.43	116.34	145.30	45.12
		PANEL C: AFFILIATED CAMPUS		
Water Treatment Facility	-4.00***	-3.06***	-1.43	-3.23***
	(1.47)	(0.98)	(2.71)	(1.23)
N	5,174	5,159	1,248	276
Mean Dep. Var. in $g-1$	54.71	37.22	37.40	19.34
		PANEL D: BASELINE STR ≤ 20		
Water Treatment Facility	6.92	1.41	10.66***	5.09
	(4.26)	(2.01)	(4.16)	(3.30)
N	2,239	2,222	881	474
Mean Dep. Var. in $g-1$	64.78	32.44	53.47	24.84
		PANEL E: BASELINE STR > 20		
Water Treatment Facility	-6.27**	-5.70***	-1.11	3.78
	(3.13)	(1.47)	(4.13)	(3.69)
N	4,484	4,431	1,676	879
Mean Dep. Var. in $g-1$	131.31	68.26	109.23	45.71

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panels B and C in this table show the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level and by whether the school is the main campus in the school system while panels D and E show the effect by pre-treatment STR. The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years until 2019. The control group for the estimates in this table is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: Colombia's National Ministry of Education (SIMAT), *Fundación EPM*.

Table 1.25: Effects on enrollment by grade level using different data sources.

	(1)	(2)	(3)	(4)
	All Levels	Primary	Lower Secondary	Upper Secondary
	PANEL A: CALLAWAY AND SANT'ANNA			
Water Treatment Facility	0.20	-2.64*	2.93	5.24***
	(2.31)	(1.36)	(2.70)	(1.97)
N	7,546	7,473	2,894	1,549
Mean Dep. Var. in $g-1$	101.18	57.39	87.45	37.18
	PANEL B: BORUSYAK, JARAVEL, AND SPIESS			
Water Treatment Facility	-1.79	-4.21***	1.88	3.35
	(2.41)	(1.38)	(2.70)	(2.05)
N	7,546	7,473	2,894	1,549
Mean Dep. Var. in $g-1$	101.18	57.39	87.45	37.18

Standard errors clustered at the school level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panel A or this table shows the simple aggregation of Callaway and Sant'Anna (2021) ATT estimates of the effects of the introduction of a water treatment facility on school enrollment in levels by grade level. Panel B shows the imputation estimator from Borusyak et al. (2024). The sample used for these estimates is made up of 742 schools in the state of Antioquia that received a water treatment facility between 2012 and 2022. For each school, the sample includes seven pre-treatment years and all available post-treatment years through 2019. The control group for these estimates is made up of schools that had not yet received a water treatment facility in year t. The mean of the dependent variable is measured in the year immediately before the delivery of the water treatment facility. Data sources: DANE (*Formulario C-600*), *Fundación EPM*.

Chapter 2

The Effect of Local Economic Conditions on High School Choice: Evidence from Mexico City

2.1 Introduction

Demand for secondary education in developing economies has risen steadily over recent decades. At the same time, education systems in these countries have expanded to accommodate this increased demand. Today it is not uncommon for students – particularly in urban areas – to be able to consider hundreds of high school options with different characteristics. This is a consequential decision, as the high school a student ultimately attends affects post-secondary and labor market outcomes. Moreover, it is a complicated decision that could be influenced by a number of factors. One of these factors could be the local economic conditions at the time students are transitioning between education levels. Economic shocks might impact school choice directly, by imposing new liquidity constraints that affect the students' ability to afford the direct and opportunity costs of attending particular schools, and indirectly, by altering students' expectations about returns to educational investments. As a result, temporary economic shocks may affect educational decisions that have long-lasting consequences.

In this paper, I explore the effect of changes in local economic conditions on high school choice in Mexico City during the late 2000s. Mexico was one of the countries most heavily impacted by the Great Recession, with households experiencing widespread decreases in employment and earnings.[1] The deterioration of economic conditions caused by the Great Recession did not affect all households to the same extent, as some neighborhoods were more heavily impacted than others. I take advantage of this spatial variation in a two-way fixed effects design in order to examine whether and how exposure to economic shocks affects school choice at the secondary level.

Admissions to public high schools in Mexico City are administered through a centralized assignment mechanism.[2] High school applicants submit a rank-ordered list of schools and are assigned to a high school based solely on their performance on a standardized admission test. There are hundreds of options to choose from, which vary considerably in the type of curriculum and the quality of instruction. Since, in principle, all students can apply any school, I can compare the school choice patterns for students who live in

[1]The Mexican GDP decreased by -5.3% in 2009. In comparison, the GDP of Latin America as a whole fell by -1.9% (World Bank, 2021). See appendix 2.7.2 for time series on unemployment and wages.

[2]The choice of public high schools in Mexico City has been a useful setting for other authors to explore a variety of questions related to school choice in urban areas of middle-income economies. Some of the topics include the risks and benefits of elite school admission (Dustan et al., 2017; Estrada and Gignoux, 2017), the effect of conditional cash transfers on school choice and completion (Avitabile et al., 2017; Dustan, 2020), the socioeconomic stratification of school choice (Ortega Hesles, 2015), the transmission of school information through family networks (Dustan, 2018), the effect of improving public transportation on school choice (Dustan and Ngo, 2018), the effect of violence on academic performance (Chang and Padilla-Romo, 2019), and the updating of beliefs with information on academic ability (Bobba and Frisancho, 2020).

neighborhoods that were more or less exposed to economic shocks and who were selecting schools from a common choice set. In practice, however, there is an important degree of socioeconomic stratification of school choice in Mexico City (Ortega Hesles, 2015). If economic shocks induce changes in school choice, they may reinforce the existing socioeconomic segregation of schools.

In my empirical analysis, I use administrative data from the applications to public high schools in Mexico City from 2005 to 2010, which contain student demographic information and the full set of high school options to which they applied, along with data on the characteristics of the high schools such as type of curriculum, specializations, and fees charged. Since students only select schools once, their school preferences are only observed under a particular state of the local economy, and it is not possible to observe what their choices would have been under different economic conditions. To overcome this issue, I estimate a two-way fixed effects model that leverages the geographic and temporal variation of shocks to neighborhood-level average household income to identify the effect of local economic conditions on school choice. By aggregating shocks at the neighborhood level, I isolate changes in household income that come from a deterioration in local economic conditions from shocks that are unique to any particular household.

I find that, although the transition from middle to high school is a critical moment for students to make important educational decisions, local economic conditions have a small impact on school choice outcomes. Students who live in neighborhoods with the largest decreases in average household income during the Great Recession do not significantly alter the characteristics of their highest ranked schools. However, students do adjust the set of schools listed to which they are most likely to be admitted. In particular, students make small adjustments to their choices for their most preferred school among those that are plausibly reachable given their academic ability to favor schools that are relatively cheaper but farther away from home. In times of economic hardship, students with more educated parents are more likely to select schools that typically cater to college-bound students. On the other hand, I find suggestive evidence that the choices of students from lower parental education households are more sensitive to the monetary cost of schooling in response to local economic shocks.

This paper contributes to several strands of the literature on school choice and on the determinants of schooling in developing countries. First, this paper adds to the debate on the importance of local economic conditions on educational outcomes. While research on the effects of shocks to households such as parental job loss on educational outcomes often focuses on shocks to individual households,[3] there is some evidence (e.g., Rege et al., 2011; Stevens and Schaller, 2011) suggesting that the state of the economy, as measured by

[3]Some of the educational outcomes that have been found to be affected by parental job loss include grade retention (Stevens and Schaller, 2011), academic performance (Rege et al., 2011), high school completion (Tanndal and Päällysaho, 2020), enrollment in post-secondary education (Coelli, 2011; Hilger, 2016), and choice of field of specialization (Huttunen and Riukula, 2019)

indicators like unemployment rates, may also influence these outcomes.[4] These papers use regional indicators to measure economic conditions, which could mask important within-region variation in the local economy. I deal with this concern by exploring shocks at a much more granular level – neighborhoods within a large city in the developing world.

The effects of household shocks on school enrollment and completion in the developing world have been studied extensively (e.g., de Janvry et al., 2006; Duryea et al., 2007; Ferreira and Schady, 2009; de Carvalho Filho, 2012; Glick et al., 2016). Ferreira and Schady (2009) note that the relationship between economic shocks and schooling outcomes in developing countries is ambiguous. On the one hand, a reduction of family income would make it difficult for them to afford educational investments. On the other hand, the opportunity cost of schooling also decreases. These authors note that there is evidence of both procyclical and countercyclical investments in education. In low-income economies, the income effect dominates and investments in education are procyclical, whereas in high- and middle-income economies (such as Mexico), the substitution effect dominates and investments in education are countercyclical. While it is important to establish how school enrollment is affected by economic conditions, it does not imply that households may only respond to economic shocks by changing school enrollment or attendance. Families may also adjust by changing the type of schools their child attends, and these decisions could be as important as these extensive-margin adjustments. This paper contributes to the exploration of other margins of adjustment of educational investments in the presence of economic shocks.

Economic conditions have also been found to affect career choices. Several studies in the US (e.g., Liu et al., 2019; Ersoy, 2020; Blom et al., 2021) and in other high-income economies (e.g., Huttunen and Riukula (2019) in Finland; Bičáková et al. (2021) in the United Kingdom) have explored how the choice of college major varies across the business cycle, finding that in times of recession, students shift to fields that are less likely to be affected by economic fluctuations.[5] Unlike these studies, which focus on choices made in post-secondary education, I am exploring choices at an earlier stage, when students are transitioning from middle to high school. At that point, students may be less certain about their abilities and the returns they would get from particular educational investments. Dustmann et al. (2017) explore the effect of the choice of track in middle school in Germany on long term outcomes like employment or wages and find little effect.

[4]For instance, Rege et al. (2011) use Norwegian data to identify the effects of job losses due to plant closures on the GPA of the children of affected workers and they find that job losses decrease GPA by 11% of a standard deviation, with larger effects in areas with higher unemployment. Stevens and Schaller (2011) use US data from the Survey of Income and Program Participation and find that a 1 percentage point increase in state unemployment is associated with an increase in the probability of grade retention by 0.3 percentage points. These authors note that the effect of parental job loss on grade retention is still present after controlling for state unemployment rates. They interpret this as evidence that the shock to the individual household is capturing an effect additional to the one from the aggregate economic conditions.

[5]The degree of vulnerability of a college major to an economic recession is often measured as the variation of the major-specific employment and wages in different stages of the business cycle. Other classifications are based on characteristics of the major such as math requirements (Blom et al., 2021) or whether the major is related to STEM (Liu et al., 2019).

They attribute this finding to the possibility of switching tracks later in life. On the other hand, a similar study conducted in Sweden (Dahl et al., 2020a) finds that there are important returns to the choice of high school track, which depend on the student's alternative career path and are driven by the ability to access different college majors and occupations. These two studies take place in two high income countries with high tertiary education enrollment rates. Compared to these countries, the college enrollment rate in Mexico is significantly lower, which means that there are many students for whom high school is their terminal degree. For these students, high school choice may be a more consequential decision. The impacts of high school choice need not be limited to educational attainment. Beuermann et al. (2018) find that the choice of secondary school in Trinidad and Tobago has effects not only on student achievement on high-stakes tests, but also on labor market participation and crime.

The rest of the paper is organized as follows. In section 3.2, I describe the process of admission to public high schools in Mexico City. Then, in section 3.3, I describe my data sources and provide some descriptive evidence of the changes in patterns of school choice in Mexico City during my period of study. Section 3.4 lays out the empirical strategy that I use to find the effect of local income shocks on school choice. I present my results in section 3.5, first for the full sample of students and later for specific types of students. I also present a number of robustness tests. Section 3.6 concludes.

2.2 School Choice in Mexico City

The Mexican education system offers three types of high school: academic, technical, and technological. Students typically enter high school at age 14 or 15 and stay in high school for three years regardless of the type of high school. Academic high schools (*Bachillerato General* or *Preparatoria*) provide general education to prepare students to continue with their studies at the university level, while technical high schools (*Bachillerato Técnico*) train students in particular trades or occupations. Technological high schools (*Bachillerato Tecnológico*) have a hybrid curriculum that is designed to prepare students for university while also providing some vocational education.

In the Metropolitan Area of Mexico City (henceforth "Mexico City"),[6] public high schools are grouped into nine subsystems[7] which are affiliated to either a public higher education institution or to the Secretary of Education of the state.[8] There is significant variation in terms of prestige and reputation across the different subsystems. Two subsystems in particular are considered to be elite. The first is the subsystem affiliated to the

[6]For the purposes of this paper, the Metropolitan Area of Mexico City refers to the Federal District (*Distrito Federal*, renamed *Ciudad de México* in 2016) and 22 surrounding municipalities in the State of Mexico (*Estado de México*).

[7]The nine public high school subsystems are *Colegio de Bachilleres (COLBACH)*, *Colegio Nacional de Educación Profesional Técnica (CONALEP)*, *Dirección General de Bachillerato (DGB)*, *Dirección General de Educación Tecnológica Industrial (DGETI)*, *Dirección General de Educación Tecnológica Agropecuaria y Ciencias del Mar (DGETAyCM)*, *Instituto Politécnico Nacional (IPN)*, *Secretaría de Educación del Gobierno del Estado de México*, *Universidad Autónoma del Estado de México (UAEM)*, and *Universidad Nacional Autónoma de México (UNAM)*.

[8]The discussion on elite subsystems and the admission mechanism is based on Dustan et al. (2017).

National Autonomous Univerisity of Mexico (UNAM - *Universidad Nacional Autónoma de México*), which offers 14 high schools that confer academic high school degrees, and the second is the subsystem affiliated to the National Polytechnic Institute (IPN - *Instituto Politécnico Nacional*), which comprises 16 technological high schools whose curricula emphasize science, math, and technical subjects. The good reputation of elite schools in Mexico City is not unfounded: Dustan et al. (2017) find that admission to these schools is associated with large increases in math scores on ENLACE, the national high school exit exam, whereas Estrada and Gignoux (2017) find that elite school graduates have higher earning expectations.

Since 1996, admission to public high schools in Mexico City is determined through a centralized assignment process run by the Metropolitan Commission of Public Institutions of Higher Secondary Education (COMIPEMS - *Comisión Metropolitana de Instituciones Públicas de Educación Media Superior*), a consortium of all nine public high school subsystems in Mexico City. In January of the last year of middle school – ninth grade – students receive information about the high school admission process and all the available high school options. Then, in late February or early March, students submit a registration form that contains demographic information and a rank-ordered list of the high school programs to which they would like to apply. Students may list up to twenty options. In each option, students list a school and in the case of some technical and technological schools, a specialization for vocational training.[9] In June, after students have listed their preferences, they take an admission test made up of 128 multiple-choice questions covering both material from the public middle school curriculum and more general topics on mathematical and verbal reasoning. Their test score is the only criterion taken into account to determine high school assignment.[10]

School assignment is conducted using a serial dictatorship mechanism (Abdulkadiroğlu and Sönmez, 2003). Before the assignment is done, schools set a fixed number of available seats. Once students have taken the admission test, students are sorted by their test score from highest to lowest. Students with a score of 31 or below are disqualified. A computer assigns students to their most preferred school, beginning with the highest scoring students and proceeding in descending order. If there are no seats available in a student's most preferred school by the time their turn arrives, they are assigned to their most preferred school that still has availability. If none of the schools listed by the student have seats open, they are put on a waiting list and then they select from the remaining schools with availability after the computer has gone through all qualifying applicants.

This serial dictatorship mechanism is a special case of the deferred acceptance mechanism (Gale and

[9] I can observe school choice for all years, but I cannot observe the choice of specialization in 2010 for students selecting a DGETI technological school. This is because in that year, DGETI changed their admission policies so that students would select a specialization after being enrolled in a school.

[10] The UNAM and IPN subsystems also require that students have a middle school GPA of 7.0/10.0. Furthermore, UNAM uses a different admission test for students that list an UNAM school as their top choice. The UNAM test is designed to be comparable in content and difficulty to the general admission test, administered by the National Center of Evaluation of Higher Education (CENEVAL - *Centro Nacional de Evaluación de la Educación Superior*).

Shapley, 1962). Under this assignment mechanism, it is a weakly dominant strategy for the student to list their true preferences. This implies that the observed behavior should correspond to the student's actual preferences and not to strategic considerations. Another important consideration is that, due to the differences across subsystems, switching schools is a complicated process that could involve starting anew in a different school. Less than 5% of students who finish high school on time do so in a different subsystem from the one in which they started (Dustan et al., 2017).[11]

2.3 Data

The main data source I use in this paper are student-level administrative records from the COMIPEMS school admission process. For each high school applicant in the dataset, I observe their complete public school choice set, their score in the admission test, the outcome of their application – whether they were admitted to a school in the first round of assignments and if so, the school to which they were assigned – and background information such as gender, postal code of residence, middle school, middle school GPA, parental education, household income, household composition, and opinions about their study habits and the support they receive from teachers and from their parents.

I complement the student-level data with information about the schools in their choice set. For each school in the student's choice set, I have data on the type of high school program (academic, technical, technological), the specialization of the program (for most technical and technological programs), and the admission cutoff from the previous year. I sort the specializations of technical and technological schools into STEM and non-STEM related using the classification from Ngo and Dustan (2019). I measure the cost of attending each school in two ways. First, I calculate the distance from the centroid of the student's postal code to each of the schools in their choice set using geographic data from the National Institute of Statistics and Geography (INEGI). Second, I use data from the annual school census (*Formato 911*) to obtain information on the average educational expenses associated with attending each school (e.g., tuition, uniforms, school supplies).[12]

2.3.1 Sample selection

The dataset spans six years, from 2005 to 2010. I start the analysis in 2005 for two reasons. First, 2005 is the first year in which the National Survey of Employment and Occupation (ENOE - *Encuesta Nacional de Ocupación y Empleo*) was carried out in its current format. This allows me to compare the income reported by

[11] Some subsystems also impose restrictions on changes between schools within the subsystem. For instance, the IPN establishes that students may only request a change in high school program once, subject to capacity constraints and after completing at least one semester in the current high school program (IPN, 1998).

[12] The *Formato 911* data on school fees also includes information on the average voluntary contributions that parents make to schools throughout the year. Since contributions are voluntary (and therefore not paid by all students) and likely to be affected by a deterioration in economic conditions, I exclude them from the total amount of school fees.

the students to the income measures from ENOE, which is the survey used to calculate official labor market statistics. Second, UNAM and its affiliated high schools were affected by a large strike in the early 2000s which made students substitute from UNAM to other subsystems, a pattern which only reversed by 2005 (Dustan and Ngo, 2018). Such behavior could become a threat to a parallel trends identifying assumption. My time frame ends in 2010 because the school cost data are only available until that year.

The empirical strategy exploits differences in the size of shocks to the average household income across postal codes over time. In order to reduce the possibility of measurement error of average income at the postal code level, I restrict the sample to only include students residing in the 812 postal codes with at least 50 applicants per year. This procedure reduces the sample size by approximately 15%. In addition, I drop all observations for which either an outcome or a covariate is missing. The final sample has approximately 150,000 observations per year and more than 900,000 observations in total.[13]

2.3.2 Descriptive statistics

Before presenting the estimation strategy and results, I conduct an exploratory analysis of the data. First, I discuss the magnitude and heterogeneity of the shocks to household income in Mexico City during the Great Recession. Then, I describe the trends in school choice during that time period.

2.3.2.1 Household income

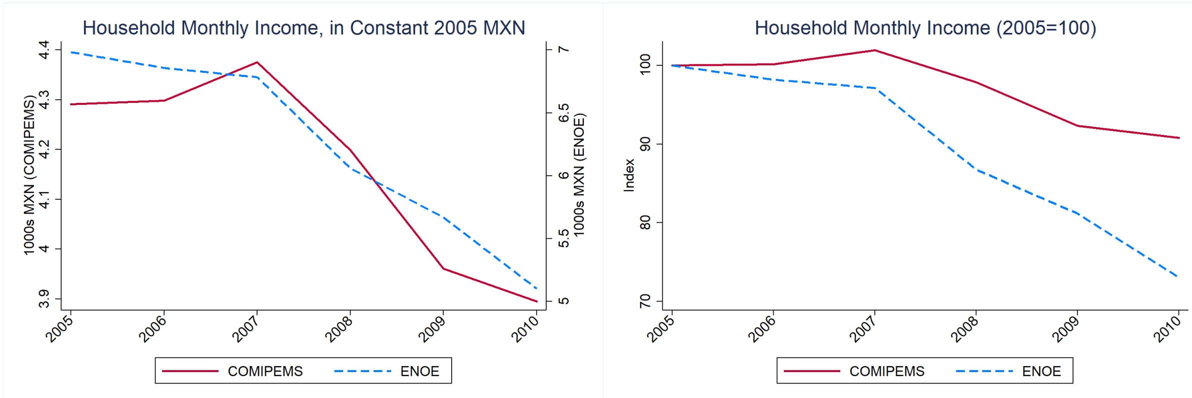
The 2000s in Mexico were characterized by a period of moderate economic growth followed by a severe economic downturn during the Great Recession. In 2009, the Mexican GDP shrunk by -5.3%, the largest GDP reduction since 1995. This contraction in economic activity also affected employment and wages. Figure 2.12 in appendix 2.7.2 shows the evolution of the unemployment rate and real hourly wages in Mexico and in Mexico City from 2005 to 2010.

The deterioration of economic conditions can also be seen in the COMIPEMS data. In the demographic survey that students fill out as part of their application to public high schools, students are asked to report the total monthly income for all members of their household. Income is recorded in 15 bins. Most of the bins are 1,000 Mexican Pesos (MXN) – approximately 85 US Dollars[14] – wide, with wider bins (2,500 MXN or 214 US Dollars) at the top of the income distribution.[15] I use the midpoint of each bin as my measure of household income and adjust for inflation using the national consumer price index. Although a continuous measure of income would be preferable, the binned income measure still captures the decline of average household income during the Great Recession. Figure 2.1 compares household income as reported

[13]Figure 2.11 in appendix 2.7.1 shows the number of observations per year and the number of students who live in large postal codes. This figure suggests that the fraction of students who live in large postal codes remains constant over my period of study.

[14]The average exchange rate for 2005-2010 was 11.7 MXN/USD.

[15]Bins are not adjusted for inflation and remain unchanged for the entire period of study.



(a) Average income in 2005 MXN.

(b) Average real income index (2005=100)

Notes: This figure depicts the average household income for households living in Mexico City with at least one teenager aged 12-17. The figure to the left expresses the average in constant 2005 MXN, while the figure on the right indexes real income in relation to its 2005 levels. Data sources: COMIPEMS, ENOE (INEGI, 2020)

Figure 2.1: Average real monthly household income in Mexico City, 2005-2010.

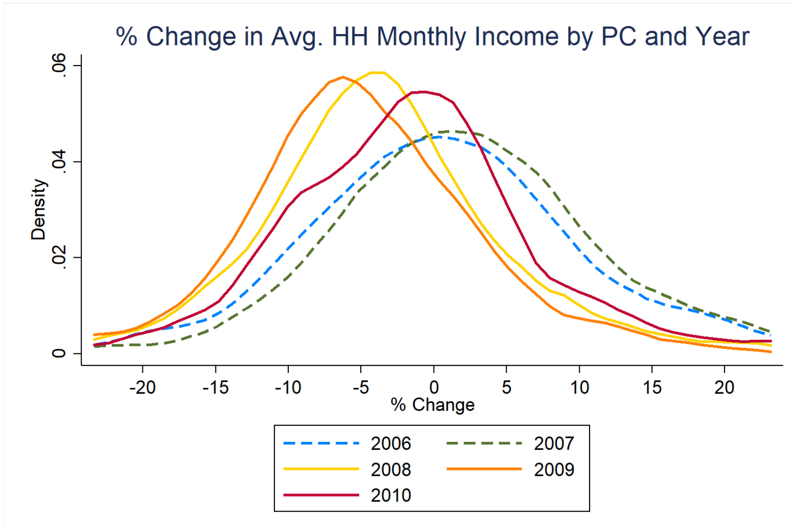
by the students in COMIPEMS with income from the ENOE for households in Mexico City with at least one teenager (ages 12-17). While the levels of both measures are different and the magnitude of the changes in income is different, the general trend of a reduction of household income is consistent across datasets.[16]

The variation in household income during the Great Recession was not evenly spread across neighborhoods in Mexico City. Figure 2.2 shows the density of the year-on-year percentage change of average household income at the postal code level. These densities indicate that there is an important geographic heterogeneity in the magnitude of the income shocks. In appendix 2.7.3 I show that, even though there is considerable residential segregation – relatively wealthy postal codes are close to each other – the size of the income shock seems uncorrelated with the initial levels of household income of the different postal codes.

2.3.2.2 Students' Choices

For the most part, the demographic characteristics of applicants to high school in Mexico City did not change much over time during the period of study. Panel A of table 2.1 shows for every year between 2005 and 2010 the percentage of female applicants, the percentage of applicants living in Mexico State, the average middle school GPA and the average years of schooling of the students' parents. The only noticeable trend is the increase in the average years of schooling of the parents. This would be consistent with a long-term, gradual increase in average schooling, similar to the ones experienced in many urban areas in middle-income

[16] Another potential reason why there could be a discrepancy between the data from ENOE and COMIPEMS is that household income is reported by students and not by the head of the household. Even though students are encouraged to ask an adult for help answering the household income question, they might still disregard the suggestion and fill out the question with their best guess.



Notes: This figure shows the yearly densities of average real monthly household income at the postal code level. This figure only includes the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Income figures are expressed in constant 2005 MXN. Data source: COMIPEMS

Figure 2.2: Yearly densities of postal code-level average real monthly household income.

countries.[17]

Table 2.1: Descriptive statistics by year.

	2005	2006	2007	2008	2009	2010
PANEL A: Demographic Characteristics						
Percent Female	52.38	52.59	52.54	52.78	52.40	52.02
Percent Living in Mexico State	54.81	55.60	56.15	56.39	55.58	53.94
Middle School GPA	8.06	8.07	8.10	8.10	8.12	8.14
Years of Schooling of Mother	9.05	9.16	9.32	9.42	9.51	9.63
Years of Schooling of Father	9.67	9.73	9.85	9.92	9.98	10.05
PANEL B: Additional School Choice Outcomes						
Test Scores	63.90	65.75	65.54	66.81	62.88	65.21
Numbers of Schools Chosen	8.92	9.34	9.51	9.73	9.75	9.98

Notes: This table presents the mean of several socioeconomic and educational characteristics of the applicants to public high schools in Mexico City by year from 2005 to 2010. The estimation sample includes students who live in postal codes with at least 50 applicants per year. GPA is measured on a scale from 0.0 to 10.0. Test scores are on a scale from 0 to 128. The number of schools chosen may be any integer from 1 to 20. "State of Mexico" refers to the 22 municipalities in the State of Mexico that are adjacent to the Federal District and are part of the COMIPEMS catchment area. Source: COMIPEMS.

Figure 2.3 shows the fraction of students who select an academic, technical, or technological school as their most preferred option. These proportions remain fairly stable over the years in my sample, with just small decreases in the share of academic schools and a corresponding increase in technical and technological schools. In a context with a marked socioeconomic stratification of school choice (Ortega Hesles, 2015), the

[17] In 2000, the average years of schooling for all of Mexico was 7.7. By 2010, it had increased to 8.8 (Barro and Lee, 2013)

stability of the share of school type for the most preferred school would suggest that few changes toward socioeconomic integration in public high schools took place in the late 2000s. The slight decrease in the share of academic schools seems to be driven by a decrease in demand for non-elite schools. The increases in the share of technical and technological schools are also small, with the increase in technological schools mostly explained by the demand for the elite IPN schools and the increase in technical schools distributed evenly between STEM and non-STEM specializations.[18] Likewise, the proportion of students listing an elite school did not change between 2005 and 2010 (see figure 2.4). In that period, almost two-thirds of students listed an elite school (belonging to either the IPN or the UNAM subsystems) as their most preferred school.

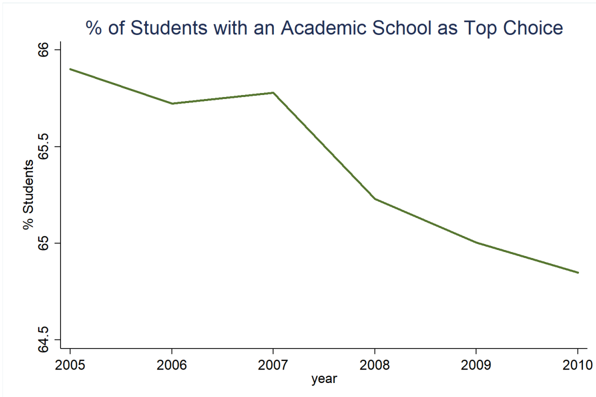
As mentioned in section 3.2, the serial dictatorship mechanism that is used to assign students to public high schools in Mexico City incentivizes the true revelation of preferences for schools. However, preferences alone do not determine the final placement of students to high schools; their performance on the admission test relative to the rest of the applicants also plays an important role. Students are aware that the process of being admitted to some schools may be very competitive, especially since COMIPEMS provides information on the minimum test score that was required to be admitted to every public high school in the previous three years. Fack et al. (2019) note that, under a deferred acceptance mechanism such as the one used in Mexico City, a student-school matching is stable if and only if every student is assigned to their most preferred *feasible* choice.[19] A feasible choice is a school whose cutoff score is below the student's score in the admission test. If students knew their score *before* submitting their list of schools, they would not consider schools that are unreachable. However, that is not the case and students only know if a school is feasible ex post, *after* they have submitted their choices. Nevertheless, students to some extent know ex ante their academic ability and how it compares to their peers' ability and may use this information to guess how likely it is that they are admitted to their most preferred schools.[20]

Assuming that students' expectations on their performance in the admission test are reasonable – that is, that students' expectations are based on their true ability and that, when learning about their score, students do not get surprised by how well they did on the test – I can use their ex post test score to define which schools students may consider to be feasible ex ante, when they are preparing their list of schools. To construct the rule of feasibility, I use the fact that students know previous cutoff scores. The mean of the standard deviation of cutoff scores within schools over time is 3.93 points and the average cutoff score is 44.26 (out of 128). Therefore, the average standard deviation of cutoffs is approximately 8.9% of the average cutoff score. I

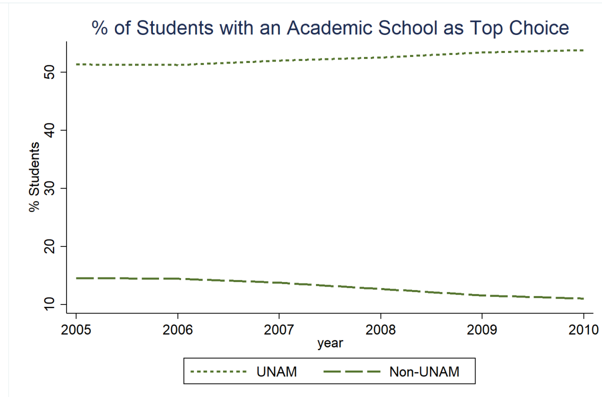
[18]Starting in 2010, students who enter to a DGETI school take the same common core during their first year and then select a specialization in their last two years. Thus, students who choose a DGETI school from 2010 onwards do not state a specialization. This change makes the shares of students selecting a STEM specialization in 2010 not comparable to the share in previous years.

[19]Fack et al. (2019) define the stability of a matching as a scenario in which no student who had preferences for a school and who could have gotten to that school given their priority index – in this case, their test score – is assigned to a less preferred school.

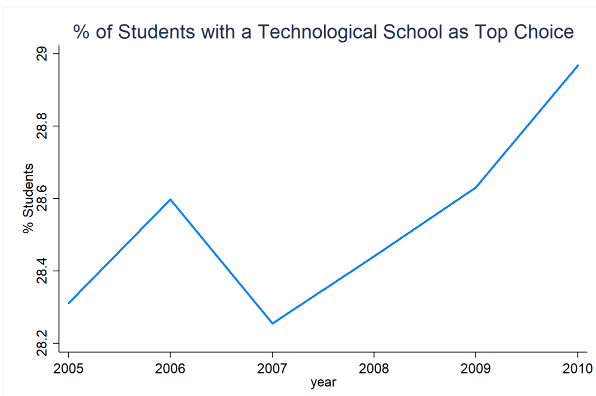
[20]Note that if students are not constrained by the number of options they can list, they should still list unfeasible schools because it allows them to account for the low-probability event that they get a score high enough to be admitted to unfeasible schools.



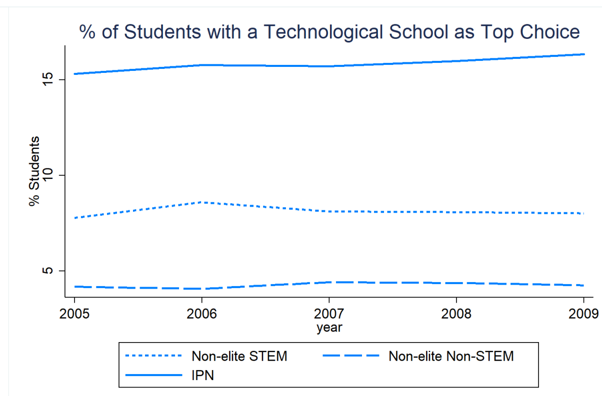
(a) Academic.



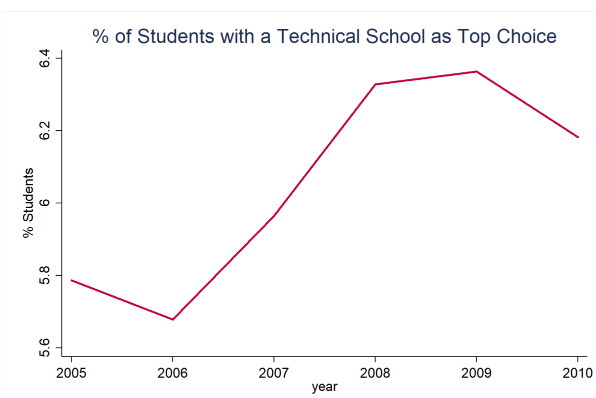
(b) Academic - Detailed.



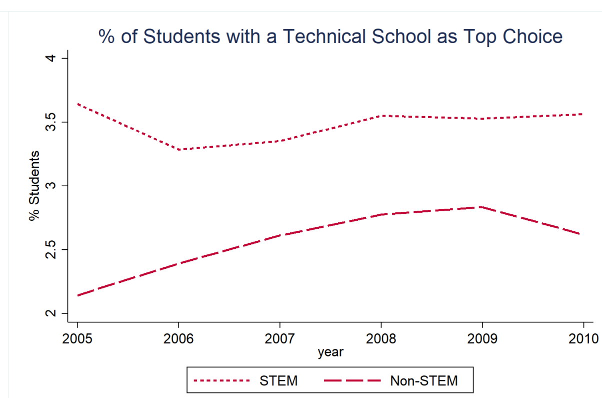
(c) Technological.



(d) Technological - Detail.



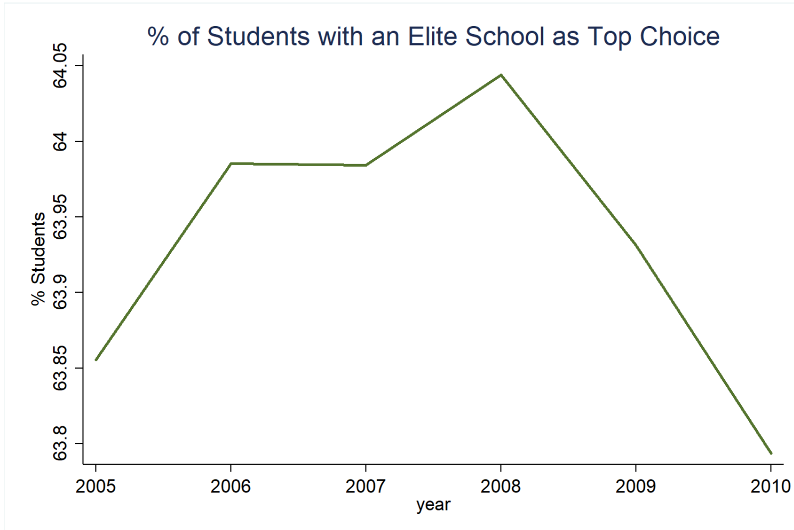
(e) Technical.



(f) Technical - Detail.

Notes: This figure shows the percentage of students by year who selected a particular type of school – academic, technical, or technological – as their top choice. The plots to the left show the total proportion in each school type, while the plots to the right disaggregate this proportion by whether the school belongs to an elite subsystem (UNAM, IPN) or by whether the program of study is considered STEM according to the classification by Ngo and Dustan (2019). This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.3: Percentage of students per type of school in first choice.



Notes: This figure shows the percentage of students by year who selected a school belonging to an elite subsystem (i.e., UNAM or IPN) as their top choice, regardless of school type. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.4: Percentage of students who had an elite school as their top choice.

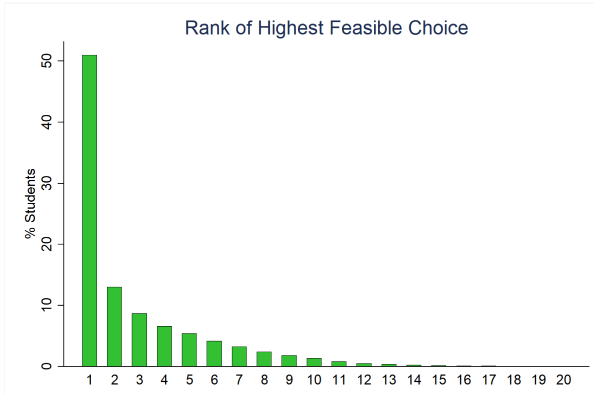
round this percentage to 10% and I assume that, for students, the best case scenario is that the cutoff in the year they are applying is 10% lower than it was the previous year.[21] More succinctly, the rule for defining that school s is feasible for student i who is applying to that school in year t is

$$\text{Feasible}_{ist} = 1 \iff \text{Test Score}_i \geq (1-0.1)\text{Cutoff}_{s,t-1}.$$

Figure 2.5 shows the percentage of students who rank their top feasible choice in each possible rank. For approximately half of the students, their top choice is also their top feasible choice. This raises the question of how far off students are from the lowest possible cutoff they could expect at their top choice. In figure 2.6 I plot the difference between the lowest expected cutoff score ($0.9\times\text{Cutoff}_{s,t-1}$) and the actual test score. On average, students who select an unfeasible school as their top choice have scores that are 18.2 points below the lowest expected cutoff. This gap is not small, considering that the standard deviation of test scores is 19.3. Figures 2.5 and 2.6 underscore the importance of looking beyond first choices to explore the effects of local income shocks on student choices.

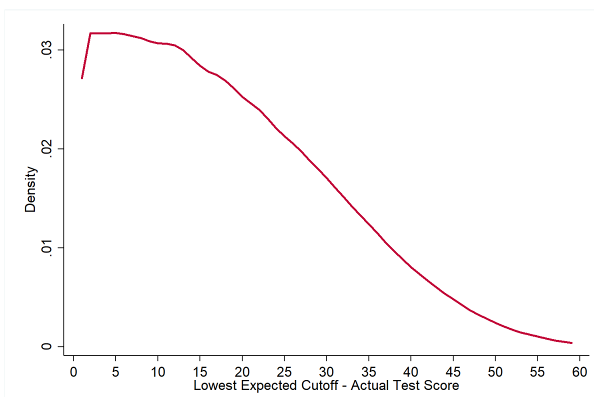
The trends in the types of schools that students select as their top feasible choice (figures 2.7 and 2.8) are not the same as those for their top choice. The decline in the share of students who select an academic school is more pronounced and it is driven by the decline in the share of top feasible schools from UNAM, although by 2010, the shares of UNAM and non-UNAM academic schools as top feasible choices return to their 2008

[21] In section 2.5.5.6 and in appendix 2.7.10 I show that my results are not significantly affected by the use of a different rule of feasibility based on a fixed adjustment to cutoff scores.



Notes: This figure shows the percentage of students by the rank of their highest feasible school option. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.5: Percentage of students by rank of the highest feasible school in their choice set.



Notes: This figure shows the density of the difference between the minimum test score that would make the top option feasible (i.e. $0.9\times\text{Cutoff}_{s,t-1}$) and the student's score in the admission test. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.6: Density of the difference between feasibility cutoff at top choice and actual test score.

values. This reduction in the proportion of feasible UNAM schools explains the decline in the share of elite schools as top feasible choices. The share of technical schools as top feasible choices, on the other hand, increases over this time period, mostly due to increases in STEM-related specializations.

An important dimension that students may take into account in their preferences for specific schools involves the cost of attending that school. I use two measures of cost. First, I use the straight-line distance from the school to the centroid of the postal code where the student resides. Second, I use the total annual fees that a student must pay to attend school. While public schools do not charge tuition, they still report the average cost of uniforms, textbooks, and school supplies. A potential issue with using school fees is that, during economic downturns, schools may adjust their fees. I return to this point in section 2.5.5.5. Since school fees in year t may be affected by both changes in school demand and endogenous adjustments due to economic conditions, I also measure cost using the fees in 2005. This measure of baseline fees should capture the relative price of different schools without including any endogenous adjustment to school fees.

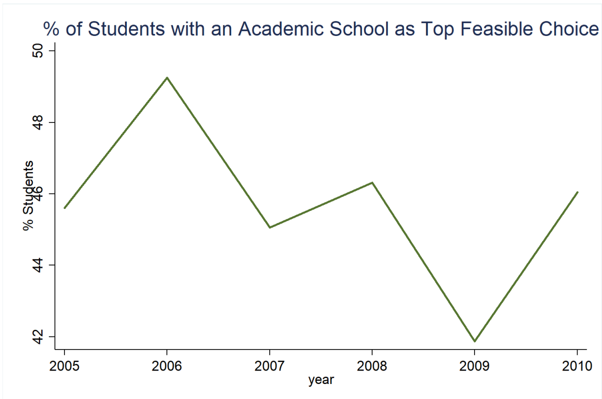
Figure 2.9 shows the average distance to the most preferred school and to the most preferred feasible school over time. While the average distance to the top choice is increasing over time, the average distance to the top feasible choice decreases over time. Next, I look at the evolution of average fees over time in figure 2.10. The present-year fees show a clear decline in the years of the Great Recession, and that is particularly noticeable for the fees of the top feasible choice. This trend, however, is not seen in the baseline fees time series. While the average baseline fees of top choices decreases slightly, the baseline fees of top feasible choices increase over time. One possible interpretation of the two time series of school fees for top feasible choices is that schools that are more likely to be considered feasible by many students are having larger reductions in fees.

2.3.2.3 Other outcomes

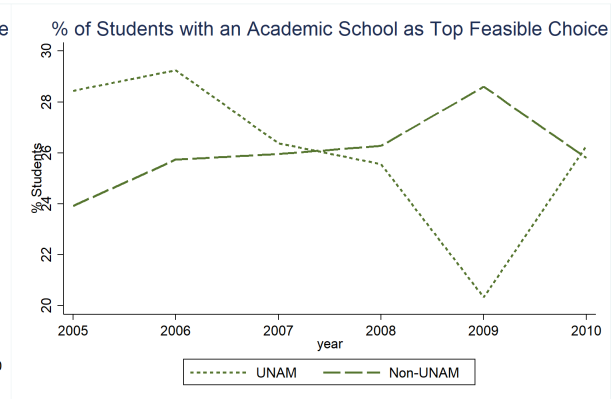
Panel B of table 2.1 presents the average test score in the admission test (on a scale from 0 to 128) and the size of the choice set – that is, the number of options students list. Average test scores improve gradually in the first years of my time frame, but they decrease considerably in 2009 and rebound in 2010. This lower performance in 2009 may explain why the share of elite schools among top feasible choices decreases in that year. In contrast, the average choice set size increases monotonically during the period of study.

2.4 Empirical Strategy

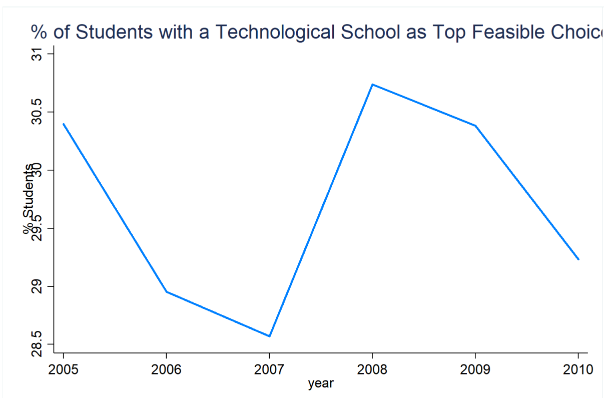
A natural starting point to explore the relationship between local economic conditions and school choice outcomes would be to regress an outcome of interest related to school choice – type of school, distance, school fees, test scores, number of schools chosen – on a variable that captures the intensity to which students



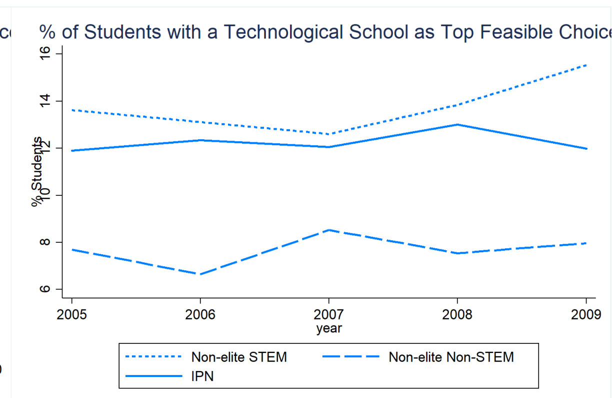
(a) Academic.



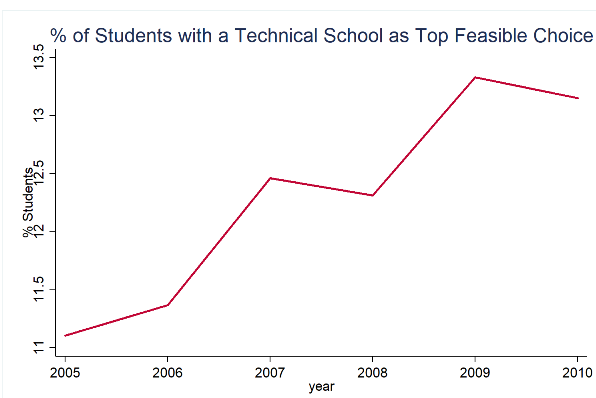
(b) Academic - Detailed.



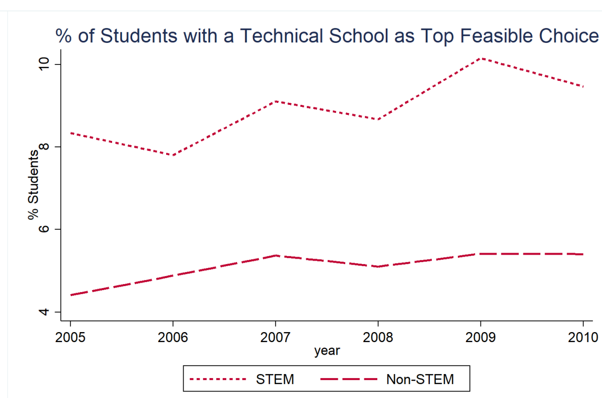
(c) Technological.



(d) Technological - Detail.



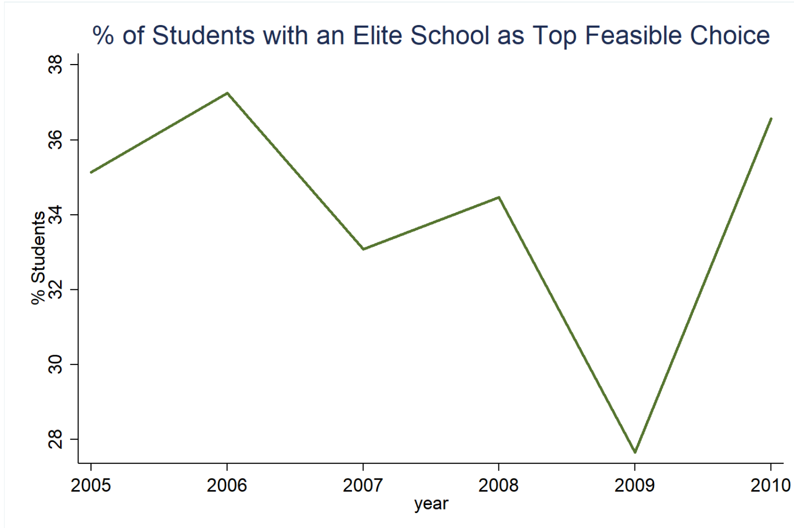
(e) Technical.



(f) Technical - Detail.

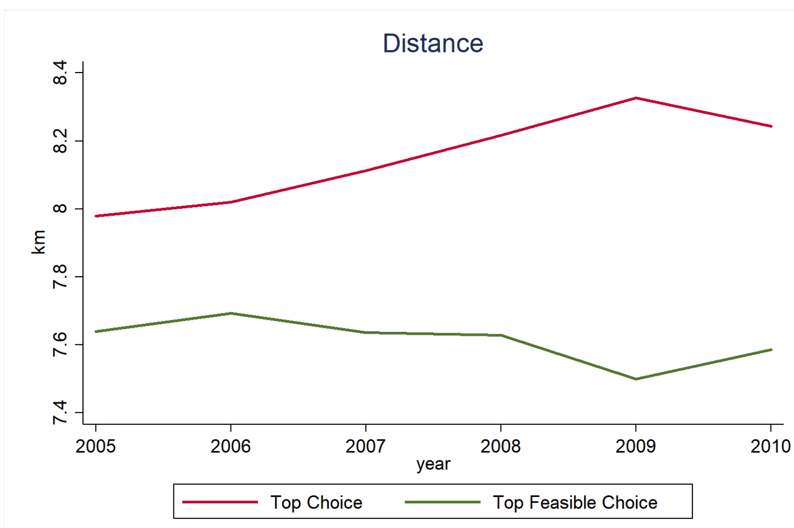
Notes: This figure shows the percentage of students by year who selected a particular type of school – academic, technical, or technological – as their top feasible choice. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. The plots to the left show the total proportion in each school type, while the plots to the right disaggregate this proportion by whether the school belongs to an elite subsystem (UNAM, IPN) or by whether the program of study is considered STEM according to the classification by Ngo and Dustan (2019). This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.7: Percentage of students by type of school in first feasible choice.



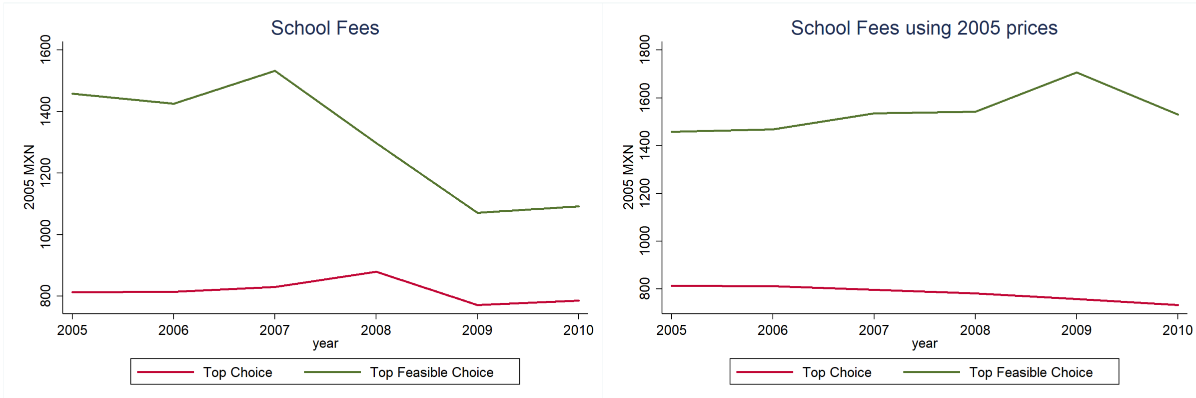
Notes: This figure shows the percentage of students by year who selected a school belonging to an elite subsystem (i.e., UNAM or IPN) as their top feasible choice, regardless of school type. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.8: Percentage of students who had an elite school as their top feasible choice.



Notes: This figure shows the average straight-line distance from the centroid of the postal code where the student resides to their top choice and top feasible choice. Distances are measured in meters. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.9: Average distance to top choices and to top feasible choices



(a) Present Year. (b) Baseline Year (2005).

Notes: This figure shows the average school fees (excluding voluntary fees) for top choices and top feasible choices. Present Year estimates refer to fees in each year, in 2005 MXN, whereas Baseline Year estimates fix school fees to their 2005 levels. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. This figure only includes applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data sources: COMIPEMS, *Formato 911*

Figure 2.10: Average school fees for top choices and top feasible choices.

have been affected by the economic crisis. At first glance, household income seems to be a suitable indicator to gauge the severity of the local economic shock. Figure 2.1 shows that household income fell during the economic recession. The relationship of interest using household income as an indicator of shock severity would be

$$Y_{it} = \alpha \text{HHInc}_{it} + \Phi' X_{it} + \nu_{it}, \tag{2.1}$$

where Y_{it} denotes a school choice outcome for student i in year t, HHInc_{it} denotes household income, and X_{it} is a vector of student-level covariates. However, there are two reasons why the estimates of equation (2.1) may not be interpreted causally. First, there could be unobserved factors that affect both household income levels and school choice outcomes. For instance, consider a household where parents have higher-than-average academic ability. This higher ability would correlate with higher labor income and higher parental preference for academically demanding schools, which in turn could influence the school choice of their children. Since students only choose a high school once – and hence are only observed once – this unobserved heterogeneity cannot be controlled for with an individual fixed effect. Second, there may be measurement error in household income at the individual level, since this variable is recorded in bins and it is reported by students, who may not be very involved with household finances.

Household income may be affected by factors unique to each household or by factors that are common to all households, like local economic conditions. To the extent that households in a neighborhood are similar to one another, I can use the *neighborhood-level average* household income to measure the economic conditions of the neighborhood. In Mexico City, this is a reasonable assumption since, as figure 2.14 in appendix 2.7.3 shows, there is significant income-based residential segregation. However, because the neighborhood-level average is a function the household income of each individual student, it would not be exempt from the endogeneity concerns that prevent a causal interpretation of equation (2.1). To address this issue, I construct leave-one-out averages of household income at the neighborhood level. These leave-one-out averages are defined as

$$\overline{\text{HHInc}}_{-ijt} = \frac{\sum_{k\neq i} \text{HHInc}_{kjt}}{N_{jt}-1} = \frac{\sum_{k=1}^{N_{jt}} \text{HHInc}_{kjt} - \text{HHInc}_{ijt}}{N_{jt}-1},$$

where N_{jt} is the number of high school applicants from postal code j in year t. Intuitively, a leave-one-out average represents the average household income of student i's neighbors. Leave-one-out averages also address the issue of measurement error of individual household income because they average out individual measurement errors.

After defining $\overline{\text{HHInc}}_{-ijt}$ as the measure of local economic conditions, I use it to construct the following two-way fixed effects model

$$Y_{ijt} = \beta\overline{\text{HHInc}}_{-ijt} + \Gamma' X_{ijt} + \delta_j + \lambda_t + \varepsilon_{ijt}, \tag{2.2}$$

where Y_{ijt} denotes a school choice outcome for student i in postal code j in year t as a function of local economic conditions $\overline{\text{HHInc}}_{-ijt}$, a vector of student-level covariates X_{ijt}, and postal code (δ_j) and year (λ_t) fixed effects. Equation (2.2) is my main estimating specification. The vector of covariates X_{ijt} is made up of gender, middle school GPA, parental education, number of siblings, and birth order. In all the estimations, standard errors are clustered at the postal code level.

To interpret the estimates from equation (2.2) as the causal effect of local economic conditions on school choice outcomes, I need to assume that the leave-one-out average household income is uncorrelated with the error term – that is, $E[\varepsilon_{ijt}\overline{\text{HHInc}}_{-ijt}|X_{ijt},\delta_j,\lambda_t] = 0$. The intuition behind this identifying assumption is that conditional on individual characteristics and in the absence of an economic downturn, the trends in school choice outcomes would have been the same across postal codes that experienced different-sized shocks to average income.

The variation I am leveraging in equation (2.2) is in the severity of local income shocks across postal codes over time. Thus, any threat to identification should vary across both time and postal codes. Any time-invariant confounding factor that could affect the choices of students in a postal code is accounted for by the postal code fixed effect δ_j. Likewise, the year fixed effect λ_t accounts for any confounding factor affecting all students who apply for high school in a given year.

2.5 Results

In this section, I present results on how local economic shocks affect two types of school choice. First, I look at the effects on the characteristics of the most preferred school. Second, I explore the effects on the top feasible choice, the highest-ranked school to which students have a reasonably high probability of being assigned. For both sets of results, I characterize the changes in both types of school choice along two dimensions: the type of school – academic, technical, or technological; elite or non-elite – and the cost of attending that school, expressed as distance from the student's postal code of residence to the school, and as the annual fees charged by the school. After presenting these results, I explore the potential for heterogeneous effects and effects on other outcomes. Finally, I conduct several robustness checks.

2.5.1 Top Choice

Table 2.2 shows the results of estimating equation (2.2) for outcomes related to the type of school that students rank as their top choice. The leave-one-out average household income at the postal code level, $\overline{\text{HHInc}}_{-ijt}$, is expressed in thousands of 2005 MXN. Hence, the estimated coefficients should be interpreted as the effect on school choice of changing the average household income in a postal code by 1,000 MXN.[22] For reference, the average reduction of average household income at the postal code level in 2009 was 226 MXN.[23]

Columns 1-3 of table 2.2 show the effect of varying the average household income at the postal code level on the type of school that students list as their most preferred option. A 1,000 MXN decrease in postal code-level average household income increases the share of academic schools as the top choice by one percentage point. Appendix table 2.10 shows that this increase is driven by higher demand for non-elite (i.e., non-UNAM) academic schools.[24] The increase in the proportion of academic schools is counteracted by a decrease of almost the same magnitude – 0.9 percentage points – in the share of technological schools, which is mostly attributed to an increase in the demand for elite (i.e., IPN) technological schools. The results from column 4 show that local household income shocks have no effect on the share of elite schools as a top choice.

Table 2.3 presents the estimates of local household income shocks on the outcomes related to the cost of attending the most preferred school. I find that students do not make any adjustments to their top choice in terms of distance (column 1) or school fees (columns 2 and 3) when affected by a local household income

[22] To interpret the results, I use a 1,000 MXN decrease in leave-one-out average household income at the postal code level. This way of interpreting the results means that the effects I describe in the text have the opposite sign as the coefficients shown in the tables.

[23] In 2009, 28 postal codes (3.4%) experienced reductions in average household income of more than 1,000 MXN.

[24] Here, "demand" is a short-hand way of expressing the share of students who list a school as their first choice. Notice that not all the demand for a school is captured by students who list it as their top choice.

Table 2.2: Effect of changes in average local household income on the type of school of the top choice.

	Type of School			
	(1)	(2)	(3)	(4)
	Academic	Technological	Technical	Elite School
$\overline{\text{HHInc}}_{-ijt}$	-0.0096***	0.0093***	0.0003	0.0036
	(0.0027)	(0.0027)	(0.0014)	(0.0028)
Mean dep. var.	0.654	0.285	0.0605	0.685
N	911,756	911,756	911,756	911,756

Standard errors, clustered at the postal code level, in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top choice. An elite school is a school belonging to either the UNAM or the IPN subsystems. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

shock.[25]

Table 2.3: Effect of changes in average local household income on the distance and the monetary costs of the top choice.

		School Fees	
	(1)	(2)	(3)
	Distance	Fees in present year	Fees in 2005
$\overline{\text{HHInc}}_{-ijt}$	-47.17	-31.30	-4.16
	(47.83)	(24.32)	(10.08)
Mean dep. var.	8,148.3	815.7	782.4
N	911,756	900,758	911,756

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top choice. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

2.5.2 Top Feasible Choice

The results shown so far suggest that the students' top choices are, for the most part, unaffected by the changes in local economic conditions. This does not imply that *all* school choices do not change in times of economic hardship. Students may not see a strong need to adjust their top schools if they feel that it is very unlikely that they get a seat in their most preferred school. Instead, they may adjust their choices only for the schools to which they feel they have a reasonably high probability of being assigned. I now repeat the previous analyses

[25] The sample size for all the regressions that use present-year fees is slightly smaller than in the rest of estimations because of missing data on fees for some schools in 2008.

but focusing on the top *feasible* school.

Table 2.4 presents the effect of local household income shocks on the type of school chosen as the most preferred feasible option. As was the case for the top choice, a reduction in postal code level average household income of 1,000 MXN is associated with a small increase (0.7 percentage points) in the fraction of students who select an academic school as their top feasible choice (column 1). This increase can be attributed to an increase in the proportion of students listing an UNAM school as their top feasible choice (see appendix table 2.11). This explanation is further supported by the results in column 4, which also point to an increase in the share of elite schools as top feasible choices. The effects on the share of technological (column 2) and technical (column 3) schools are not statistically significant.

Table 2.4: Effect of changes in average local household income on the type of school of the top feasible choice.

	Type of School			
	(1)	(2)	(3)	(4)
	Academic	Technological	Technical	Elite School
$\overline{\text{HHInc}}_{-ijt}$	-0.0070**	0.0039	0.0031	-0.0058*
	(0.0032)	(0.0031)	(0.0024)	(0.0031)
Mean dep. var.	0.475	0.367	0.158	0.417
N	768,363	768,363	768,363	768,363

Standard errors, clustered at the postal code level, in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top feasible choice. An elite school is a school belonging to either the UNAM or the IPN subsystems. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

Table 2.5 shows the estimates of equation (2.2) for outcomes related to the cost of attending the highest-ranked feasible school. Column 1 presents the effect on distance to the top feasible school. In contrast to the results for the cost of the first choice, I find that students do adjust their top feasible choice in relation to their cost of attendance when they are affected by a shock to local economic conditions. A 1,000 MXN decrease in the postal code-level average household income leads to students selecting a top feasible choice that is 169 meters farther away from home. Considering that the average distance to school for top feasible choices is 7.59 kilometers, this effect represents a 2.2% increase in distance to school. For a student living in a postal code that experienced an average decline of household income in 2009 (226 MXN), the effect would imply choosing a feasible school that is $0.226 \times 168.8 = 38.1$ meters (0.5%) farther from home.

The rest of the columns of table 2.5 examine the effects of local income shocks on the fees of top feasible choices. I find that, while there is no significant adjustment in terms of present-year fees (column 2), students who experience a 1,000-MXN decrease in local average household income choose a school whose baseline fees are 33 MXN lower (column 3). This represents a decrease of 2.6% with respect to the average baseline fees. Since baseline fees reflect the relative price of schools net of any responses schools may have during times of economic hardship, the results from column 3 suggest that students select relatively inexpensive schools when they are affected by a decrease in local average household income.

Table 2.5: Effect of changes in average local household income on the distance and the monetary costs of the top feasible choice.

		School Fees	
	(1)	(2)	(3)
	Distance	Fees in present year	Fees in 2005
$\overline{\text{HHInc}}_{-ijt}$	-168.8***	-1.88	32.83***
	(42.88)	(26.54)	(12.10)
Mean dep. var.	7,592.2	1,293.2	1,277.0
N	768,363	757,599	768,363

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top feasible choice. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

The results from the analysis of the top feasible choice suggest that students are more likely to make adjustments to the option where they feel they are the most likely to be assigned. For this reason, the rest of the analysis will focus on the decisions regarding the top feasible choice. These adjustments, however, are small in magnitude, which indicates that the changes in choices induced by economic conditions are rather subtle. This is notable, because for students it is much easier to alter their educational choices before entering high school than switching schools after they have already enrolled in high school.

Taken together, the results suggest that students change their top feasible choices during economic downturns mostly along the dimensions related to the cost of attendance and they do so by substituting toward relatively inexpensive options that may represent an additional cost in terms of commuting. It is worth noting, however, that there are two types of costs associated with commuting. First, commuting involves a monetary cost. Second, commuting represents an opportunity cost since students could use the time they spend commuting in other activities. This opportunity cost may be lower during economic downturns if it is more difficult for students to find a job.[26] Another important point is that, given the small magnitudes of the

[26]The proportion of students who work decreases steadily during my period of study, even during the years of the Great Recession. This proportion goes from 6.97% in 2005 to 4.83% in 2010.

effect on distance, the monetary cost of commuting may remain the same.[27] If students perceive commuting as an inferior good, they may become more willing to accept longer commutes (and the inconvenience they represent) when household income decreases.

2.5.3 Heterogeneity

This section explores the effect of changes in local household income on the top feasible choices for different subgroups of students.[28] In each case, I restrict the sample to include only students from a particular subgroup and then reestimate equation (2.2) for each subgroup and for all the outcomes of interest. I divide the sample using three criteria: gender, parental education and academic performance in middle school. For parental education, I define that a student lives in a household with high parental education if at least one of their parents has completed high school, whereas for academic performance, I divide the sample into students with a middle school GPA below or above the median.

Table 2.6 shows the effects of local household income shocks on the type of school chosen as the most preferred feasible school. The increase in the fraction of students who list academic or elite schools as their top feasible choice when there is a decline in local income can be mostly attributed to students with highly educated parents and to students with above-median GPAs.

Table 2.7 presents the estimates of the effect of changes in local household income on the cost of attending the top feasible choice. The results show no statistically distinguishable difference in the effects by gender (panel A), parental education (panel B), or academic performance in middle school (panel C). In spite of this, a comparison of point estimates is suggestive of some potential differences. For instance, the adjustment that students with less educated parents make in terms of the monetary cost of schooling is larger than that of students with higher parental education, whereas the opposite is true with respect to adjustments in terms of distance.

The results from tables 2.6 and 2.7 show that the effect of local economic shocks on school choice varies by socioeconomic status. When affected by a shock, students from households with higher parental education (who are more likely to have a higher socioeconomic status) become slightly more likely to choose an academic school or an elite school. The goal of these schools is to prepare students for continuing their post-secondary studies at a university. Furthermore, they also become more willing to have longer commutes. A possible rationalization of these results is that these students are making countercyclical investments in education, as documented in Ferreira and Schady (2009). However, students from lower socioeconomic status may not be able to afford these additional investments in times of economic hardship and instead they

[27] In Mexico City, some means of transportation like the subway or some bus routes cost the same regardless of the distance traveled while others like private bus lines (*microbuses*) or suburban trains have differentiated fares according to distance traveled.

[28] Appnedix tables 2.12 and 2.13 show the results of the heterogeneity analysis for the top choice.

adjust their choices by selecting less costly schools.

Table 2.6: Effect of changes in average local household income on the type of school of the top feasible choice for subgroups of students.

	Type of School							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Academic		Technological		Technical		Elite School	
	PANEL A: GENDER							
	Boys (B)	Girls (G)	B	G	B	G	B	G
$\overline{\text{HHInc}}_{-ijt}$	-0.0074*	-0.0063	0.0031	0.0043	0.0042	0.0020	-0.0053	-0.0065*
	(0.0040)	(0.0042)	(0.0042)	(0.0040)	(0.0034)	(0.0029)	(0.0038)	(0.0039)
Mean dep. var.	0.423	0.525	0.403	0.332	0.174	0.143	0.446	0.389
N	380,064	388,299	380,064	388,299	380,064	388,299	380,064	388,299
p-value: $\beta^B = \beta^G$	0.842		0.817		0.580		0.803	
	PANEL B: PARENTAL EDUCATION							
	Less than High School (L)	High School or Higher (H)	L	H	L	H	L	H
$\overline{\text{HHInc}}_{-ijt}$	0.0040	-0.0097**	0.0017	0.0082**	-0.0057	0.0015	0.0063	-0.0078**
	(0.0048)	(0.0039)	(0.0052)	(0.0037)	(0.0044)	(0.0023)	(0.0040)	(0.0039)
Mean dep. var.	0.401	0.547	0.385	0.350	0.214	0.104	0.279	0.551
N	378,247	390,116	378,247	390,116	378,247	390,116	378,247	390,116
p-value: $\beta^L = \beta^H$	0.020		0.281		0.121		0.005	
	PANEL C: ACADEMIC PERFORMANCE							
	Below Median GPA (B)	Above Median GPA (A)	B	A	B	A	B	A
$\overline{\text{HHInc}}_{-ijt}$	0.0028	-0.0155***	-0.0030	0.0094**	0.0002	0.0061***	-0.0026	-0.0085*
	(0.0043)	(0.0040)	(0.0041)	(0.0040)	(0.0038)	(0.0023)	(0.0037)	(0.0044)
Mean dep. var.	0.380	0.569	0.392	0.342	0.228	0.0888	0.292	0.540
N	381,754	386,609	381,754	386,609	381,754	386,609	381,754	386,609
p-value: $\beta^B = \beta^A$	0.001		0.016		0.135		0.237	

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top feasible choice for particular subgroups of students. An elite school is a school belonging to either the UNAM or the IPN subsystems. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

2.5.4 Other Outcomes

The decrease in income that many neighborhoods in Mexico City experienced during the Great Recession could have affected the school assignment process through other channels. In this subsection, I will look into two of these other outcomes: the number of schools chosen and the scores in the admission test. Column 1 of table 2.8 presents the effect of local household income shocks on the number of schools students include in their choice set. A 1,000 MXN reduction in local average household income would lead students to list 0.07 more options. This is a very small effect and perhaps not meaningful enough to significantly alter the allocation of students to schools. That said, the negative sign of the point estimate is consistent with an increase in risk aversion in times of economic recession. Column 2 of table 2.8 shows that a decrease in local average household income by 1,000 MXN is associated with a 0.02 standard deviations *increase* in test scores. An effect of this size should not affect the feasible choice set or the school allocation process

Table 2.7: Effect of changes in average local household income on the distance and the monetary costs of the top feasible choice for subgroups of students.

	(1)	(2)	(3)	(4)	(5)	(6)
			School Fees			
	Distance		Present year		2005	
PANEL A: GENDER						
	Boys (B)	Girls (G)	B	G	B	G
$\overline{\text{HHInc}}_{-ijt}$	-164.3***	-170.4***	-0.970	0.313	29.65**	37.49**
	(54.51)	(54.90)	(28.42)	(28.62)	(14.10)	(16.64)
Mean dep. var.	7,673.8	7,512.4	1,248.6	1,336.6	1,204.8	1,347.6
N	380,064	388,299	373,512	384,087	380,064	388,299
p-value: $\beta^B = \beta^G$	0.929		0.950		0.681	
PANEL B: PARENTAL EDUCATION						
	Less than High School (L)	High School or Higher (H)	L	H	L	H
$\overline{\text{HHInc}}_{-ijt}$	-120.2*	-163.2***	-9.624	13.93	47.19**	11.66
	(65.76)	(51.83)	(39.09)	(25.32)	(20.61)	(12.94)
Mean dep. var.	7,182.8	7,989.3	1,544.8	1,048.4	1,547.2	1,014.9
N	378,247	390,116	373,588	384,011	378,247	390,116
p-value: $\beta^L = \beta^H$	0.583		0.535		0.114	
PANEL C: ACADEMIC PERFORMANCE						
	Below Median GPA (B)	Above Median GPA (A)	B	A	B	A
$\overline{\text{HHInc}}_{-ijt}$	-213.8***	-143.3**	-7.943	3.854	27.98*	38.74**
	(52.01)	(64.69)	(31.14)	(27.98)	(15.80)	(15.46)
Mean dep. var.	6,967.2	8,209.4	1,473.6	1,115.0	1,473.4	1,083.0
N	381,754	386,609	376,526	381,073	381,754	386,609
p-value: $\beta^B = \beta^A$	0.378		0.649		0.581	

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top feasible choice for particular types of students. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

considerably.[29]

Table 2.8: Effect of changes in local average household income on other school choice outcomes.

	(1) Number of schools chosen	(2) Test Score
$\overline{\text{HHInc}}_{-ijt}$	-0.068**	-0.024***
	(0.032)	(0.006)
Mean dep. var.	9.536	0.104
N	911,756	911,756

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effects of increasing average household income by 1,000 MXN on the number of school options listed (column 1) and on standardized scores in the COMIPEMS admission test (column 2). The number of schools chosen may be any integer from 1 to 20. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS

2.5.5 Robustness Checks

Having established the main effects of changes in local household income on student choices, I present a series of exercises to assess the validity of my results. First, I examine whether the leave-one-out average household income I use as a proxy for local economic condition is a good predictor of household income. I also explore whether student-reported income data captures the same trends in income variation as other data sources. Next, I address some potential threats to identification: within-city migration, high school dropout, and supply-side responses. Lastly, I show that my two main findings – the changes in top feasible choice with respect to distance and baseline fees – are robust to different definitions of feasibility.

2.5.5.1 Relationship between Household Income and Neighborhood Average Household Income

One of the assumptions needed to use the leave-one-out average household income at the postal code level as a measure of the severity of the income shock to an individual household is that households within a neighborhood are similar to each other and therefore, the average household income should approximate the individual household income. To test this assumption I estimate the following regression.

$$\text{HHInc}_{ijt} = \theta \overline{\text{HHInc}}_{-ijt} + \Pi' X_{ijt} + \xi_{ijt}. \tag{2.3}$$

[29] Even though this result is too small to be economically meaningful, there are some possible explanations for the counter-intuitive negative sign found in the estimations. For instance, students could increase their effort in preparing for the admission test to get a favorable outcome or it could also be the case that students that are not very affected by the recession may consider a better outside option such as private high school. Since I cannot observe outside options, these hypotheses are speculative.

The estimated $\hat{\theta}$ is 0.24 and it is highly significant, which suggests that there is an important correlation between own household income and average household income at the postal code level.[30] The complete results of this estimation are presented in appendix table 2.15.

2.5.5.2 How Accurate Are Students' Estimations of Household Income?

As discussed in section 3.3, the measure of household income is based on the income reported by the students when completing their application to high school. This student-reported income may not be as accurate as the income reported by their parents or other members of the household. This measurement error could bias the results, and since there is not a clear prediction on whether students overestimate or underestimate household income, I cannot predict the sign of the bias.[31] Although averaging at the postal code level should reduce the degree of measurement error, it does not completely eliminate this possibility. Figure 2.1 suggests that the aggregate trends of average household income using COMIPEMS and ENOE data are roughly similar, but even in that case, it could be that student-reported data does not capture the same trends in household income within the different neighborhoods of the city. To assess the accuracy of the student-reported household income, I regress the average household income at the municipality level calculated using COMIPEMS data on the same average calculated using ENOE data.[32] These latter data are based on household surveys which are answered by all employed household members, thus making it a more reliable measure of household income. This regression has the form

$$\text{Inc\_COMIPEMS}_{mt} = \alpha + \beta \text{Inc\_ENOE}_{mt} + \varepsilon_{mt}, \tag{2.4}$$

where $\text{Inc\_}X_{mt}$ denotes the average income in municipality m in year t when using data from $X = \{\text{COMIPEMS, ENOE}\}$.[33] Standard errors are clustered at the municipality level.[34] The estimated coefficient is $\hat{\beta} = 0.465$ and it is highly significant ($SE(\hat{\beta}) = 0.029$), which indicates that the observed trends in average household income at the municipality level using student-reported data are similar to those obtained using worker-reported data. I present the full results of this estimation in appendix table 2.16.

[30] In general, the coefficient of a regression of own outcome on a leave-one-out average of that outcome need not be equal to one. Boozer and Cacciola (2001) and Angrist (2014) note that this coefficient is mechanically determined by the intraclass correlation coefficient. This fact implies that the estimated coefficient may not have a causal interpretation and would make it inappropriate to use the leave-one-out average as an instrument in an instrumental variables setting.

[31] I also cannot assess if this is classical or non-classical measurement error as it is possible that misreporting could be correlated with observable or unobservable characteristics of the student or the household.

[32] I do not observe postal code in the ENOE data. The smallest geographic subdivision I observe in both datasets are municipalities. I further restrict the ENOE sample to include only the 38 municipalities in the COMIPEMS catchment area.

[33] I use nominal averages to estimate equation (2.4) because the inflation adjustment for both measures would be the same for both income measures.

[34] I have data for 38 municipalities, 16 in the Federal District, and 22 in Mexico State. Given this low number of clusters, I also use the wild cluster bootstrap procedure described by Cameron et al. (2008) and implemented in the `boottest` Stata command (Roodman et al., 2019).

2.5.5.3 Neighborhood composition

If the degree to which a neighborhood is affected by economic downturns is correlated with changes in the composition of the neighborhoods, the estimates would pick up the effects of, say, gentrification on school choice. If that was the case, the characteristics of the households that live in a neighborhood should also change over time in a way that is correlated with the severity of the local income shock. To test this, I estimate equation 2.2 using parental education as the dependent variable. Parental education is a predetermined characteristic of the household that approximates its socioeconomic status. I compute separate estimations for the years of schooling of each parent, for the years of schooling of the most educated parent, and for an indicator variable that takes a value of one if at least one parent has completed high school.[35] The results of this exercise are found in table 2.9. None of the estimates of the relationship between income shocks and parental education are statistically significant, which indicates that the results are probably not caused by demographic changes in particular neighborhoods.

Table 2.9: Parental education does not correlate with changes in postal code-level leave-one-out average household income

	Most Educated Parent	Mother		Father		Highly Educated Parent
	(1)	(2)	(3)	(4)	(5)	(6)
$\overline{\text{HHInc}}_{-ijt}$	0.0325	0.0019	0.0089	0.0106	0.0152	0.0020
	(0.0198)	(0.0146)	(0.0189)	(0.0164)	(0.0209)	(0.0026)
Mean dep. var.	10.67	9.347	9.347	9.867	9.867	0.509
Includes other parent's education	N/A	Yes	No	Yes	No	
N	911,756	911,756	911,756	911,756	911,756	911,756

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the relationship between average household income (in 1,000 MXN) and parental education. The reported coefficients should be interpreted as the increase in the number of years of parental schooling (columns 1-5) and on the proportion of households with a highly educated parent (column 6) – i.e., a parent with at least a high school degree. All estimations include postal code and year fixed effects, as well as the following covariates: sex, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

2.5.5.4 School Dropout

If local economic shocks are indeed causing changes in the students' school choices, they may also have an effect on a perhaps more consequential decision: whether or not to enroll in high school. Since I can only observe students who apply to high school, it could be the case that the sample is positively selected. It should be noted, however, that not observing a student in the data does not necessarily mean the student has decided not to continue with their studies. For instance, there are no data for students who go to a private high school or migrate to a city outside of the COMIPEMS catchment area.

The aggregate data on enrollment collected by the Secretary of Public Education do not point to a decrease in enrollment during the years of the Great Recession. In fact, enrollment in public high schools in the Federal

[35]This is the same criterion I use to divide my sample by parental education in section 3.5.3.

District increased in all years of the sample except for 2010, when it dropped by 0.8% (SEP, 2018).[36] These aggregate indicators could be masking some geographic heterogeneity in enrollment that could be related to the severity of local income shocks. Even though I cannot observe whether students who apply to high school end up enrolling, I can use the number of applications as a measure of the intention to continue studying after middle school.[37] If economic conditions affect the probability of applying to high school (and thereby affect school enrollment), the number of high school applicants in heavily-hit areas should increase at a slower rate than in less severely affected areas. I estimate the following regression

$$\text{Applicants}_{jt} = \beta \overline{\text{HHInc}}_{jt} + \delta_j + \lambda_t + \varepsilon_{jt}, \tag{2.5}$$

where Applicants_{jt} denotes the number of students applying to public high school in postal code j in year t and the rest of the variables are defined as in equation (2.2). The results of estimating equation (2.5) are shown in appendix table 2.17. These results show that there is no significant effect of shocks to local household income on the number of students who apply to high school. Furthermore, appendix figure 2.15 shows that there are no differential trends in the number of high school applicants that come from middle schools with above average or below average household income.

2.5.5.5 Supply-Side Responses

If local economic conditions are causing students to respond by changing their school choices, it would be plausible that schools themselves are also affected and that the changes in students' choices are due to these supply-side responses. While a full analysis of the responses of schools to economic shocks is outside of the scope of this paper, I present some exploratory analyses using data from the annual school census (*Formato 911*) to assess the possibility that school changes are the main driver of my results.[38]

Appendix figure 2.16 shows that the average total fees charged by the schools net of any voluntary contribution decreased during the years of the Great Recession. This response complicates the analysis of the results using present-year fees since I cannot separate the effect of lower prices from the effect of preferences favoring cheaper schools. This figure also motivates the inclusion of baseline fees, which should not have any endogenous response to the economic downturn, as an outcome of interest.

Schools do not seem to have adjusted the number of teachers hired. Although the student-to-teacher ratio increased in the years of the Great Recession (appendix appendix figure 2.17, left), the average number of

[36] I cannot calculate enrollment for the Metropolitan Area of Mexico City because I cannot separate the enrollment figures for municipalities in Mexico State in the COMIPEMS area from the total enrollment in the state.

[37] The number of applicants to high school is not necessarily the same as the number of students who actually enroll in high school since some students may drop out of the process after submitting their application.

[38] Schools are required to fill out *Formato 911* twice a year, at the beginning and at the end of the school year. I use data from the beginning of the school year.

teachers also increased (figure 2.17, right). Then, the increase in the student-to-teacher ratio should be due to an increase in the number of students that is larger than the increase of the number of teachers. Such a pattern would be inconsistent with schools decreasing supply during the Great Recession.

2.5.5.6 Defining Feasibility

The definition of what makes a school feasible is based on the variation of cutoff scores within schools over time. I check if the significance of my results hinges on this particular definition of feasibility by estimating equation 2.2 using different feasibility "buffers." The smaller the buffer, the smaller the feasible choice set.[39] In addition to the definitions of feasibility that are based on a percentage of the previous year cutoffs, I also define feasibility using the difference in points between the students test score and the previous year cutoffs (i.e., a school is feasible if the previous year cutoff is within x points of the ex post test score). Appendix 2.7.10 shows the estimated coefficients for distance and baseline fees using different definitions of feasibility. In appendix table 2.18, feasibility is defined using percentages of previous year cutoffs, whereas in appendix table 2.19, feasibility is defined using absolute differences. In all cases, results remain statistically significant and are qualitatively similar.

2.6 Conclusion

Despite the consequential nature of school choice, this paper finds that the effects of local household income shocks on the school choice of students entering high school in Mexico City are small.[40] While students' top choices remain largely unchanged, students do make adjustments to their choices for schools for which they can reasonably expect to be admitted. The margins of adjustment for these feasible choices are modest and mostly related to the cost of attending school. Students in areas that experienced large decreases in income were more likely to choose feasible schools that are relatively inexpensive but farther away from home.

Looking at the effects for subgroups of students reveals an important contrast. Students that come from high parental education households and higher-achieving students change their choices to favor academic and elite schools, even if they are farther away, which reflects additional investments in education. On the other hand, there is suggestive evidence that points to more disadvantaged students being more sensitive to the monetary cost of schooling. Together, these results suggest that a potential consequence of economic

[39] To see this, notice that the feasibility rule for a given buffer z is

$$\text{Feasible}_{ist} = 1 \iff \text{Test Score}_i \geq (1-z)\text{Cutoff}_{s,t-1}.$$

Thus, smaller values of z would make the feasibility cutoff higher.

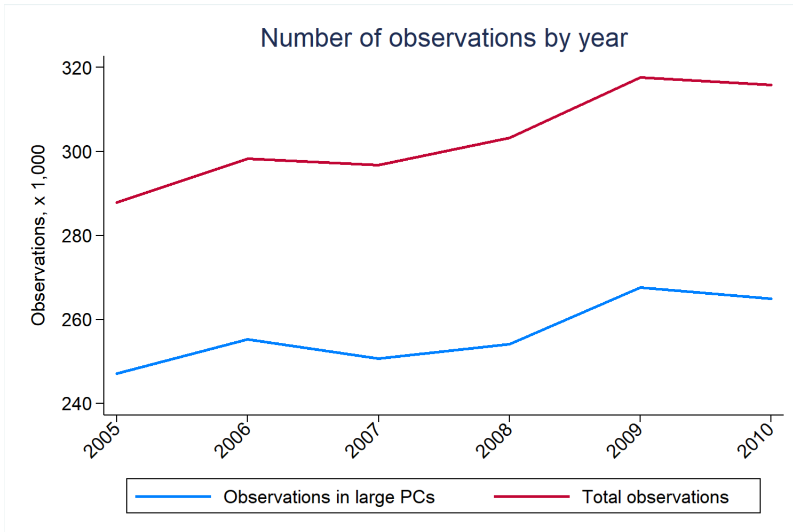
[40] In the context of Mexico City, other authors have found that other factors may have larger effects on school choice than changes to local household income. Dustan (2018) finds that having an older sibling already enrolled in high school increases the probability that a student lists their sibling's school as their top choice by 7.3 percentage points. Avitabile et al. (2017) find that the receipt of Oportunidades, a conditional cash transfer, is associated to an increase of 6.2 percentage points in the probability of listing a technical school as the top choice.

recessions in developing economies might be an increase in education inequality.

Although the estimated effects are small in magnitude, the fact that these are not null effects and that there is significant heterogeneity across subgroups implies that there are some students for whom local economic shocks play no role in their educational choices whereas for others, the same shocks influence their choices to a great extent. Further research could shed some light on the household-level determinants of vulnerability to economic conditions in terms of educational choices and could inform the policy responses required to mitigate any potential adverse effects of economic downturns on individual educational choices and on educational inequality along socioeconomic lines.

2.7 Appendix

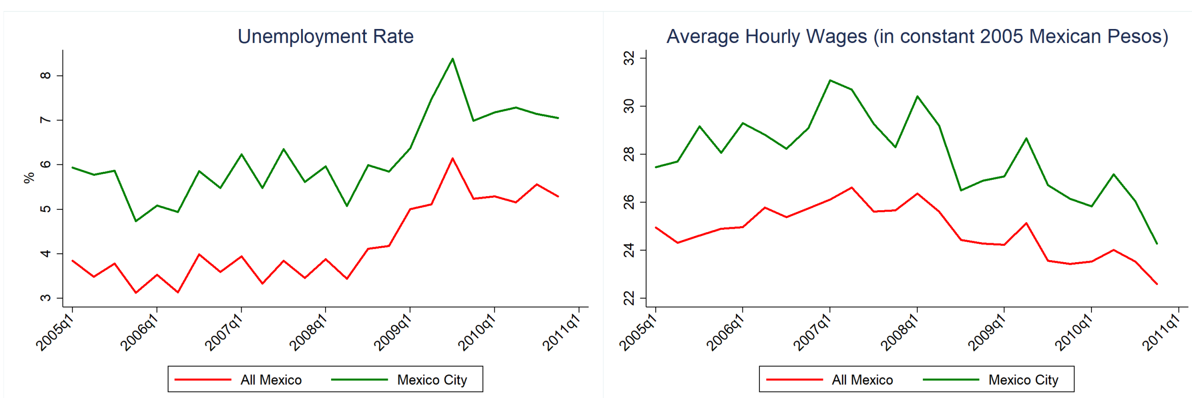
2.7.1 Number of observations by year



Notes: This figure shows the number of observations per year without missing information. The red line represents the total number of applicants, while the blue line shows the number of applicants living in the 812 postal codes included in the sample – i.e., those with at least 50 applicants per year. Data source: COMIPEMS

Figure 2.11: Number of observations by year with nonmissing information

2.7.2 Unemployment and Wages in Mexico City during the Great Recession



(a) Unemployment rate.

(b) Real hourly wages.

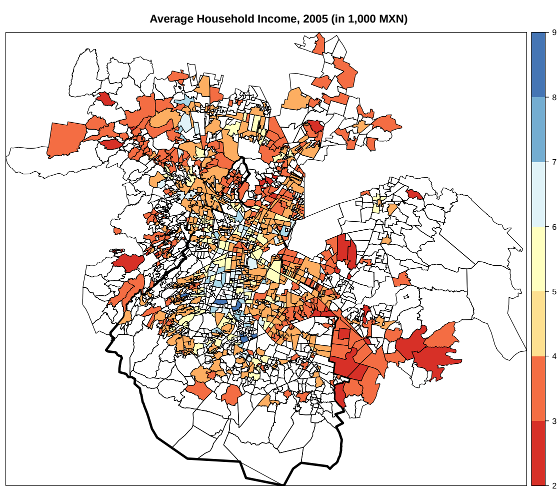
Notes: This figure shows two labor market indicators for Mexico (red line) and Mexico City (green line): unemployment (left) and real hourly wages in 2005 MXN (right). The figure depicts quarterly data from the first quarter of 2005 to the fourth quarter of 2010. Data Source: ENOE (INEGI, 2020).

Figure 2.12: Labor market indicators for Mexico and Mexico City, 2005-2010.

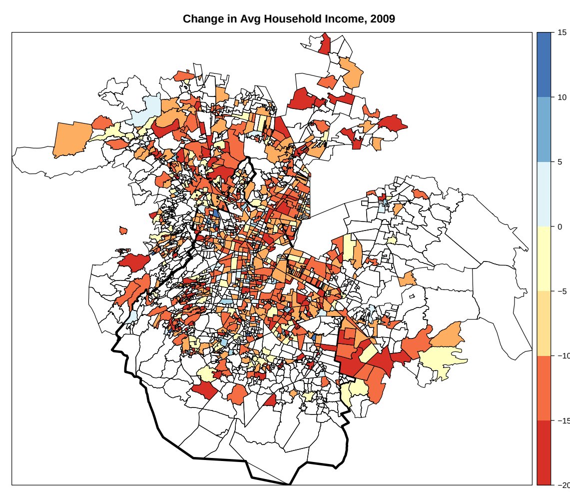
2.7.3 Geographic Heterogeneity of Income Shocks

Notes: This figure compares the average household income at the postal code level in 2005 (x-axis) to the percent change in average household income between 2008 and 2009 (y-axis). This figure only includes data from the 812 postal codes included in the estimation sample. Data Source: COMIPEMS.

Figure 2.13: Average household income by postal code level in 2005 vs change in average household income by postal code level between 2008 and 2009.



(a) Average income by postal code in 2005.



(b) Change in average income by postal code 2008-2009.

Notes: These maps show all postal codes in the COMIPEMS catchment area, which encompasses Mexico City and 22 nearby municipalities in the surrounding State of Mexico. The maps show the average household income at the postal code level in 2005 (left) and the percent change in average household income between 2008 and 2009 (right). This figure only includes data from the 812 postal codes included in the estimation sample. Blank postal codes have less than 50 students per year applying to public high schools. Data Source: COMIPEMS.

Figure 2.14: Measures of income at baseline and during the Great Recession by postal code in the COMIPEMS catchment area.

2.7.4 Detailed Results on the Type of School Chosen

2.7.4.1 Top Choice

Table 2.10: Effect of changes in average local household income on the type of school of the top choice (detailed version)

	Academic		Technological			Technical	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UNAM	Non-UNAM	Non-Elite STEM	Non-Elite Non-STEM	IPN	STEM	Non-STEM
$\overline{\text{HHInc}}_{-ijt}$	-0.00231	-0.00727***	0.000894	0.00193	0.00595***	0.000160	0.0000951
	(0.00280)	(0.00274)	(0.00186)	(0.00124)	(0.00201)	(0.00100)	(0.000922)
Mean dep. var.	0.524	0.130	0.0812	0.0427	0.161	0.0349	0.0256
N	911,756	911,756	756,769	756,769	911,756	911,756	911,756

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top choice. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

2.7.4.2 Top Feasible Choice

Table 2.11: Effect of changes in average local household income on the type of school of the top feasible choice (detailed version)

	Academic		Technological			Technical	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UNAM	Non-UNAM	Non-Elite STEM	Non-Elite Non-STEM	IPN	STEM	Non-STEM
$\overline{\text{HHInc}}_{-ijt}$	-0.00808***	0.00112	-0.00547*	0.00355*	0.00226	0.00369**	-0.000585
	(0.00309)	(0.00265)	(0.00301)	(0.00210)	(0.00198)	(0.00185)	(0.00147)
Mean dep. var.	0.284	0.191	0.134	0.0732	0.133	0.100	0.0576
N	768,363	768,363	768,363	768,363	768,363	768,363	768,363
R^2	0.209	0.160	0.066	0.039	0.082	0.061	0.038

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top feasible choice. A school option is considered to be feasible if the student's score in the admission test is at least 90% of the admission cutoff score for that school option in the previous year. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

2.7.5 Heterogeneity analysis for top choice

Table 2.12: Effect of changes in average local household income on the type of school of the top choice for some subgroups of students.

	Type of School							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Academic		Technological		Technical		Elite School	
	PANEL A: GENDER							
	Boys (B)	Girls (G)	B	G	B	G	B	G
$\overline{\text{HHInc}}_{-ijt}$	-0.0113***	-0.0076**	0.0094**	0.0092***	0.0020	-0.0017	-0.0002	0.0071**
	(0.0038)	(0.0037)	(0.0038)	(0.0035)	(0.0021)	(0.0015)	(0.0034)	(0.0036)
Mean dep. var.	0.585	0.717	0.338	0.238	0.0775	0.0451	0.686	0.683
N	433,539	478,217	433,539	478,217	433,539	478,217	433,539	478,217
p-value: $\beta^B = \beta^G$	0.868		0.810		0.437		0.154	
	PANEL B: PARENTAL EDUCATION							
	Less than High School (L)	High School or Higher (H)	L	H	L	H	L	H
$\overline{\text{HHInc}}_{-ijt}$	-0.0022	-0.0106***	0.0070	0.0094***	-0.0048*	0.0012	0.0110***	-0.0002
	(0.0045)	(0.0031)	(0.0043)	(0.0030)	(0.0026)	(0.0013)	(0.0040)	(0.0033)
Mean dep. var.	0.612	0.695	0.303	0.268	0.0847	0.0372	0.595	0.772
N	447,350	464,406	447,350	464,406	447,350	464,406	447,350	464,406
p-value: $\beta^L = \beta^H$	0.081		0.526		0.034		0.059	
	PANEL C: ACADEMIC PERFORMANCE							
	Below Median GPA (B)	Above Median GPA (A)	B	A	B	A	B	A
$\overline{\text{HHInc}}_{-ijt}$	-0.0060	-0.0135***	0.0062*	0.0126***	-0.0001	0.0009	0.0018	0.0048
	(0.0038)	(0.0037)	(0.0035)	(0.0035)	(0.0022)	(0.0013)	(0.0033)	(0.0039)
Mean dep. var.	0.610	0.700	0.303	0.268	0.0876	0.0324	0.620	0.752
N	463,945	447,811	463,945	447,811	463,945	447,811	463,945	447,811
p-value: $\beta^B = \beta^A$	0.013		0.008		0.847		0.622	

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the probability of listing a particular type of school as the top feasible choice for particular subgroups of students. An elite school is a school belonging to either the UNAM or the IPN subsystems. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data source: COMIPEMS.

Table 2.13: Effect of changes in local average household income on the distance and monetary costs of the top choice for some subgroups of students

	(1)	(2)	(3)	(4)	(5)	(6)
	Distance		School Fees: Present year		School Fees: 2005	
PANEL A: GENDER						
	Boys (B)	Girls (G)	B	G	B	G
$\overline{\text{HHInc}}_{-ijt}$	-63.07	-30.55	-10.04	-49.62**	8.995	-15.11
	(57.48)	(55.01)	(27.37)	(24.29)	(11.86)	(12.36)
Mean dep. var.	8,158.1	8,139.3	847.2	787.2	775.0	789.1
N	433,539	427,717	433,539	478,217	474,551	478,217
p-value: $\beta^B = \beta^G$	0.503		0.054		0.242	
PANEL B: PARENTAL EDUCATION						
	Less than High School (L)	High School or Higher (H)	L	H	L	H
$\overline{\text{HHInc}}_{-ijt}$	-17.10	-55.51	-68.45**	11.80	-15.27	4.720
	(54.99)	(57.49)	(29.28)	(25.39)	(15.03)	(10.85)
Mean dep. var.	7,937.4	8,351.4	981.8	655.5	956.8	614.3
N	447,350	464,406	442,244	458,514	447,350	464,406
p-value: $\beta^L = \beta^H$	0.454		0.007		0.242	
PANEL C: ACADEMIC PERFORMANCE						
	Below Median GPA (B)	Above Median GPA (A)	B	A	B	A
$\overline{\text{HHInc}}_{-ijt}$	-89.97**	-20.97	-35.42	-23.87	-1.798	-3.166
	(43.35)	(69.58)	(24.91)	(26.38)	(11.19)	(13.21)
Mean dep. var.	7,552.1	8,765.9	909.5	718.5	884.7	676.4
N	463,945	447,811	458,397	442,361	463,945	447,811
p-value: $\beta^B = \beta^A$	0.519		0.410		0.774	

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top feasible choice for particular types of students. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

Table 2.14: Effects of changes in household income on other school choice outcomes for some subgroups of students

	(1)	(2)	(3)	(4)
	Number of Schools Chosen		Test Score	
	PANEL A: GENDER			
	Boys (B)	Girls (G)	Boys (B)	Girls (G)
$\overline{\text{HHInc}}_{-ijt}$	-0.0404	-0.0896**	-0.0191**	-0.0300***
	(0.0386)	(0.0368)	(0.00755)	(0.00693)
Mean dep. var.	9.496	9.573	0.216	0.002
N	433,539	478,217	433,539	478,217
p-value: $\beta^B = \beta^G$	0.519		0.410	
	PANEL B: PARENTAL EDUCATION			
	Less than High School (L)	High School or Higher (H)	Less than High School (L)	High School or Higher (H)
$\overline{\text{HHInc}}_{-ijt}$	-0.109**	-0.00440	-0.0115	-0.0166**
	(0.0436)	(0.0342)	(0.00792)	(0.00703)
Mean dep. var.	9.409	9.659	-0.175	0.372
N	447,350	464,406	447,350	464,406
p-value: $\beta^L = \beta^H$	0.650		0.791	
	PANEL C: ACADEMIC PERFORMANCE			
	Below Median GPA (B)	Above Median GPA (A)	Below Median GPA (B)	Above Median GPA (A)
$\overline{\text{HHInc}}_{-ijt}$	-0.0697**	-0.0692*	-0.0128*	-0.0378***
	(0.0354)	(0.0396)	(0.00692)	(0.00790)
Mean dep. var.	9.544	9.529	-0.210	0.429
N	463,945	447,811	463,945	447,811
p-value: $\beta^B = \beta^A$	0.000		0.000	

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effects of increasing average household income by 1,000 MXN on the number of school options listed (columns 1-2) and on standardized scores in the COMIPEMS admission test (columns 3-4) for particular groups of students. The number of schools chosen may be any integer from 1 to 20. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS

2.7.6 How well does the leave-one-out average income approximate household income?

Table 2.15 presents the estimates from estimating the relationship between own household income and leave-one-out average household income at the postal code level (equation (2.3)). Column 1 presents the results when excluding covariates, whereas column 2 presents the results after adding covariates.

Table 2.15: Relationship between own household income and leave-one-out average household income at the postal code level

	(1)	(2)
$\overline{\text{HHInc}}_{-ijt}$	0.306***	0.242***
	(0.0184)	(0.0157)
Covariates	No	Yes
Mean dep. var. (in 1,000 MXN)	4.366	4.366
N	911,756	911,756

Standard errors, clustered at the postal code level, in parenthesis.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the relationship between average household income and individual household incomes. The covariates included in column 2 are sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS

2.7.7 How accurate are students' estimates of household income?

Table 2.16 shows the results of estimating equation (2.4). Column 1 reports the estimated coefficient without covariates and column 2 adds covariates. In both cases, the correlation between average household income from both data sources is high, suggesting that students are capable of identifying changes to household income.

Table 2.16: Comparison of household income across datasets: ENOE and COMIPEMS

	(1)	(2)
$\text{Inc\_}ENOE_{mt}$	0.474***	0.465***
	(0.0262)	(0.0291)
	[0.000]	[0.000]
Year FEs	No	Yes
N	228	228

Standard errors clustered at the municipality level in parenthesis. Wild cluster bootstrap p-values in square brackets.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the relationship between average income at the municipality level from ENOE and from COMIPEMS. Municipalities include the 16 districts (*delegaciones*) of Mexico City and the 22 municipalities in the State of Mexico that are in the COMIPEMS catchment area. Data sources: COMIPEMS, ENOE

2.7.8 Does high school enrollment correlate with local income shocks?

Table 2.17: Relationship between enrollment and local household income shocks

	(1) Enrollment	(2) ln Enrollment
$\overline{\text{HHInc}}_{-ijt}$	-1.322	
	(1.322)	
$\ln[\overline{\text{HHInc}}_{-ijt}]$		-0.0759*
		(0.0414)
N	4608	4608
Mean dep. var.	197.9	4.930

Standard errors clustered at the PC level

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the relationship between average household income and the number of applicants by postal code in each year. Data sources: COMIPEMS

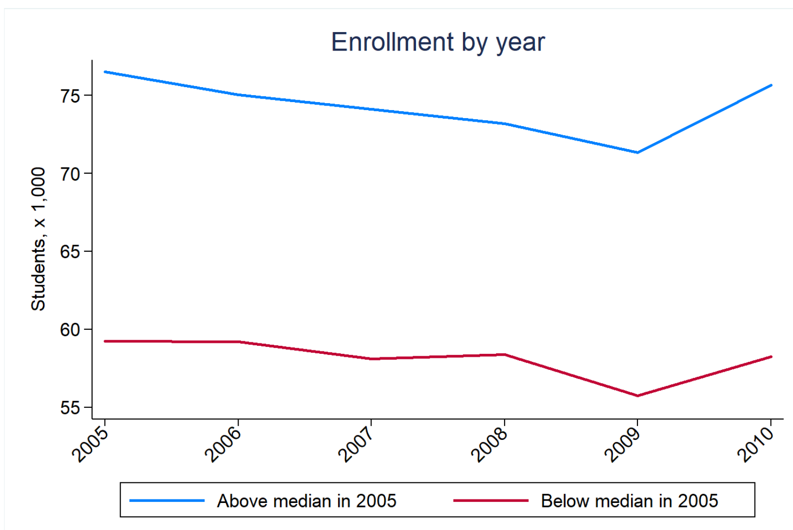
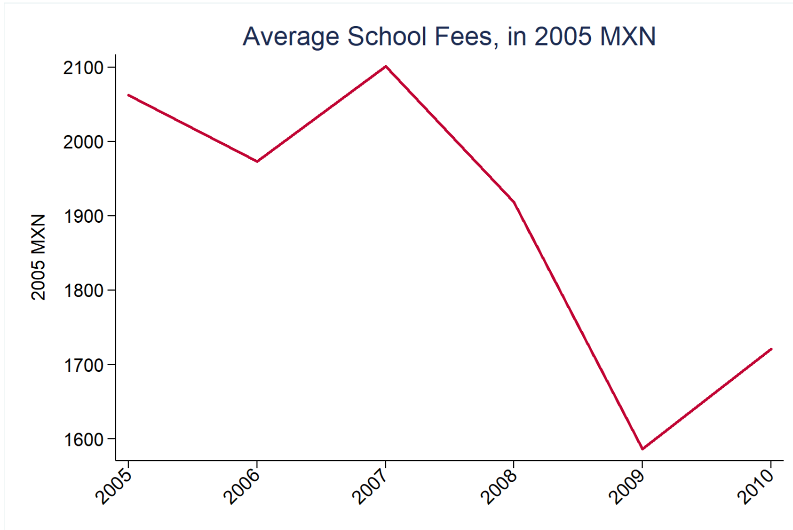


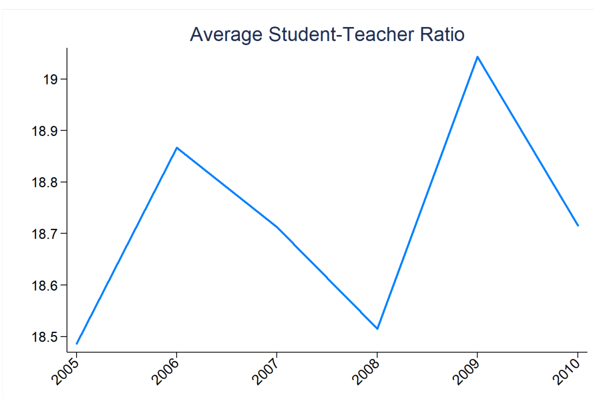
Figure 2.15: Number of observations per year by average household income at the middle school level in 2005

2.7.9 Descriptive Statistics: Supply-Side Responses

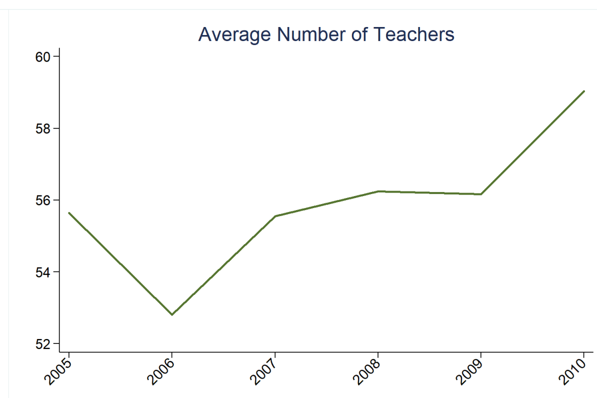


Notes: This figures shows the average annual school fees charged by public high schools in Mexico City, excluding voluntary fees. Data are expressed in constant 2005 MXN. Data Source: *Formato 911*.

Figure 2.16: Average annual real school fees charged by public high schools in Mexico City, 2005-2010.



(a) Average student-teacher ratio.



(b) Average number of teachers.

Notes: The figure to the left shows the average student-teacher ratio in public high schools in Mexico City from 2005 to 2010, whereas the figure to the rights shows the average number of teachers for the same time period. Data Source: *Formato 911*.

Figure 2.17: Indicators for teacher availability in public high schools in Mexico City, 2005-2010.

2.7.10 Results for distance and baseline fees for top feasible choice under different definitions of feasibility

There are two types of definitions of feasibility. The first, based on a percentage adjustment to previous year cutoffs, assumes that a school s is feasible if the ex post test score of student i in year t is greater than the previous year cutoff multiplied by $(1-z)$

$$\text{Feasible}_{ist} = 1 \iff \text{Test Score}_i \geq (1-z)\text{Cutoff}_{s,t-1}.$$

The results of estimating equation (2.2) for distance and baseline school fees of the top feasible choice for different values of z are reported in table 2.18.

Table 2.18: Difference in estimates for distance and school fees in 2005 for different definitions of feasibility based on percentage adjustments

	Distance					School fees in 2005				
z	5%	8%	10%	12%	15%	5%	8%	10%	12%	15%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\overline{\text{HHInc}}_{-ijt}$	-141.4***	-163.3***	-168.8***	-160.6***	-151.9***	28.68**	31.10**	32.83***	32.61***	30.84**
	(43.52)	(43.06)	(42.88)	(43.96)	(43.58)	(11.99)	(12.16)	(12.10)	(11.85)	(12.09)
Mean dep. var.	7,495.6	7,553.8	7,592.2	7,629.8	7,683.1	1,342.3	1,302.9	1,277.0	1,252.4	1,210.9
N	738,096	756,347	768,363	778,594	794,658	738,096	756,347	768,363	778,594	794,658

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top feasible choice for particular types of students. Each column varies the definition of feasibility so that a school option is considered to be feasible if the student's score in the admission test is at least (100-z)% of the admission cutoff score for that school option in the previous year. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

The second definition is based on the difference between the previous year cutoffs and the ex post test score. For a given feasibility buffer w, a school is feasible if the difference between the previous year cutoff and the student's test score is less than w. That is,

$$\text{Feasible}_{ist} = 1 \iff \text{Cutoff}_{s,t-1} - \text{Test Score}_i \leq w.$$

Table 2.19 presents the results of estimating equation (2.2) using this second definition of feasibility for different values of w.

Table 2.19: Difference in estimates for distance and school fees in 2005 for different definitions of feasibility based on absolute point differences

	Distance						School fees in 2005					
w	0	2	4	5	8	10	0	2	4	5	8	10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\overline{\text{HHInc}}_{-ijt}$	-139.4***	-168.3***	-162.2***	-162.7***	-179.6***	-159.3***	33.44***	29.14**	25.90**	30.22**	30.65**	33.67***
	(45.17)	(43.95)	(43.61)	(43.13)	(44.33)	(44.36)	(12.12)	(12.21)	(12.14)	(12.26)	(12.05)	(11.79)
Mean dep. var.	7,411.1	7,459.7	7,508.8	7,533.0	7,607.2	7,659.3	1,397.2	1,368.8	1,337.8	1,322.3	1,272.8	1,237.7
N	712,972	734,142	753,256	761,881	785,279	799,116	712,972	734,142	753,256	761,881	785,279	799,116

Standard errors, clustered at the postal code level, in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the effect of increasing average household income by 1,000 MXN on the distance and monetary costs of the school chosen as the top feasible choice for particular types of students. Each column varies the definition of feasibility so that a school option is considered to be feasible if the difference between the admission cutoff score for that school option in the previous year and the student's score in the admission test is at most w. Distance is measured in meters and school fees are in 2005 MXN. All estimations include postal code and year fixed effects, as well as the following covariates: sex, parental education, number of siblings, birth order, and middle school GPA. Data sources: COMIPEMS, *Formato 911*.

Chapter 3

Sibling Spillover Effects of Financial Aid to High-Achieving, Low-Income Students in Colombia

3.1 Introduction

Many students in low- and middle-income countries do not enroll in higher education because they cannot afford the costs associated with college attendance. These liquidity constraints are difficult to overcome, since there tend to be few alternatives for financial aid and access to credit markets is often limited. Considering that the rates of return to higher education in the developing world are higher than in advanced economies (Psacharopoulos and Patrinos, 2018), policies that aim to remove financial constraints in order to incentivize students to enroll in higher education are likely to yield large benefits to both the student and society as a whole. One of the mechanisms through which these policies may provide societal benefits is through spillovers that occur within the households of beneficiary students. Recent studies (e.g., Goodman et al. (2015); Dustan (2018); Gurantz et al. (2020); Altmejd et al. (2021); Aguirre and Matta (2021); Black et al. (2021)) have shown that siblings play an important role in the determination of educational outcomes. Measuring the extent to which policies designed to reduce the costs of college attendance have an impact on other household members may be helpful to better assess the potential of such policies to improve access to higher education.

In this paper, I explore the sibling spillover effects of *Ser Pilo Paga* (SPP), a large-scale policy in Colombia that provided generous financial aid to low-income, high achieving students. Under SPP, the Colombian government provided forgivable loans to students who could demonstrate high academic preparation and financial need. The target during the first four years of the policy was to award 40,000 loans that students could use to pay for tuition and educational expenses at any high-quality higher education institution (public or private) in the country. Previous research on SPP (Londoño-Vélez et al., 2020; Bernal and Penney, 2019; Laajaj et al., 2022) has found that SPP has been successful in increasing enrollment in higher education and academic achievement among low-income students. Given these large impacts, it is plausible to believe SPP could have affected the educational outcomes of the younger siblings of its beneficiaries.

To estimate the sibling spillover effects of SPP, I use administrative records from the national high school and college exit exams linked to household-level data on living conditions. These data allow me to identify the students who are eligible for SPP and to link eligible students to any other sibling living in the same household. To identify the causal effect of sibling eligibility for SPP, I exploit the score-based eligibility rules of SPP in a regression discontinuity design that compares the outcomes of students whose older siblings were barely eligible for SPP to those of students whose older siblings were close to meeting the eligibility criteria but fell short.

In the context of SPP, there are several reasons to expect sibling spillover effects. First, SPP loans increase the resources available to the household for human capital formation. These households may decide to reallocate the existing resources among siblings to either compensate the non-recipient siblings or complement the SPP resources with their own. Second, because SPP was targeted at low-income students, its beneficiaries are likely to be the first person in their family to access higher education. As such, they may provide first-hand information to their younger siblings about their institution, their program, and the college experience in general. Third, older siblings may become role models to their younger siblings, who see that going to college as a low-income student is not an unattainable goal, and that it is possible to do so by exerting effort and excelling in their studies. Fourth, there could be economies of scale within the household that reduce the marginal cost of attendance for younger siblings, as siblings may share some educational costs like housing or transportation.

While the outlined mechanisms through which a spillover effect might operate may suggest that these effects are always positive, that need not be the case. For instance, if households perceive that SPP is a once-in-a-lifetime opportunity for the recipient child, they may divert resources away from the non-recipient children, thus affecting their own human capital accumulation.[1] Furthermore, if the recipient sibling's experience in college is not satisfactory, they could transmit information that could dissuade their younger siblings from enrolling. The theoretical ambiguity of the direction of the spillover effects provides further justification for the exploration of these effects.

The results from this paper suggest that there is no detectable spillover effect on the academic achievement at the high school level of younger siblings associated with their older sibling becoming SPP eligible. This null result is not due to heterogeneity in the effects as I do not find evidence of non-zero effects for any subgroup of students. The non-existence of spillover effects of SPP cannot be attributed to its lack of direct effects because I find that SPP was successful in increasing the probability of enrolling in higher education and the academic achievement in college of recipient older siblings. These results mirror those in the SPP literature (Londoño-Vélez et al., 2020, 2023). Moreover, I find preliminary evidence that suggests that siblings of SPP-eligible students are not more likely to enroll in college or have better scores in the national college exit exams than siblings of barely ineligible students.

In recent years, several studies have explored the way in which siblings influence human capital decisions.

[1]There is existing evidence that suggests that parents may redistribute resources to either compensate lower-achieving children or to reinforce investments in higher-achieving children. Yi et al. (2015) finds that parents of twins in China make compensatory health investments in children who suffered from early-life health shocks. In contrast, Dizon-Ross (2019) found evidence of reinforcing behavior from an intervention in Malawi that provided parents with better information on their children's academic ability.

The motivation for studying siblings as a special type of peers stems from the fact that unlike other kinds of peers, siblings share genetics, socioeconomic background, and household resources. In addition, the interaction with siblings starts occurring at a younger age than with other peers and it is more frequent. The results from this literature suggest that siblings are influential in many aspects of the human capital accumulation process. For instance, across a variety of contexts like the United States, Chile, Croatia, and Sweden (Altmejd et al., 2021; Goodman et al., 2015; Aguirre and Matta, 2021), it has been found that younger siblings are more likely to enroll in the same higher education institutions as their older siblings. Similarly, it has been observed that in Mexico (Dustan, 2018), Denmark (Joensen and Nielsen, 2018), and Sweden (Dahl et al., 2020b) students tend to choose similar schools and fields of concentration at the secondary level. Another set of papers (e.g., Karbownik and Özek (2019); Nicoletti and Rabe (2019); Gurantz et al. (2020)) explores sibling spillover effects on achievement, finding mostly positive spillover effects. Other related research focuses on the responsibility of siblings in forming the human capital of their siblings (Qureshi, 2018; Jakiela et al., 2020) and on the mechanisms behind these spillover effects (Black et al., 2021).

Many papers in this literature (e.g., Dustan (2018); Altmejd et al. (2021); Aguirre and Matta (2021)) take advantage of discontinuities generated by high school or university admission processes for identification. In contrast, I use the discontinuities related to the availability of financial aid.[2] That way, I can explore whether resource availability within the household plays a role in mediating a potential sibling spillover effect. This is an important consideration in the context I am exploring. In relation to many other settings where sibling effects have been studied, high school graduates in Colombia have more uncertainty with respect to their ability to afford college, as college attendance is costly and credit availability is limited. There is still much research to be done on the impacts of financial aid in developing countries. The existing evidence suggests that the impacts of financial aid programs are large. For instance, in the case of Chile, Solis (2017) finds that access to financial aid increases the probability of a student ever enrolling higher education by 50%, while Barrios-Fernández (2022) find spillover effects among the neighbors and the siblings of beneficiary students.

The evidence from Colombia that uses SPP to explore the effects of financial aid are consistent with the positive effects found in the Chilean case. Londoño-Vélez et al. (2020) find that SPP increased college enrollment among eligible students by between 56.5% and 86.5%, thus reducing the socioeconomic gap among high-achieving students. Bernal and Penney (2019) find that after the introduction of SPP, the test scores of students who meet the need-based eligibility criterion for SPP increased by 0.09 standard deviations. A similar result is found by Laajaj et al. (2022), who also document that the achievement socioeconomic gap at the 90th percentile of the test score distribution declined by 16%. This project adds to the literature on SPP

[2] In particular, the financial aid program that I am studying is not tied to studying a particular subject at a particular institution. I do not require either sibling to enroll in higher education to estimate spillover effects.

by exploring whether the policy had impacts beyond its direct recipients.

The rest of this paper is organized as follows. Section 3.2 provides background on the financial constraints students in Colombia face to enroll in higher education and on SPP as a policy solution to remove these barriers. In section 3.3 I mention the data sources used in the empirical analysis, which is explained in detail in section 3.4. Section 3.5 presents the estimated spillover effects for the entire sample and for particular subgroups. Section 3.6 discusses the results and concludes.

3.2 Ser Pilo Paga

The Colombian higher education sector is composed of both public and private institutions, which offer both academic and technical degrees. The system has expanded rapidly in recent decades, fueled by the increase in high school graduation rates, which went from 20 percent in the early 1990s to 47 percent in the late 2000s (Camacho et al., 2017). College attendance costs remain high, as Bernal and Penney (2019) note that the average tuition cost at an elite private university in Colombia represents approximately 40% of the national income per adult. According to the 2012 Longitudinal Survey of Social Protection, 57.5% of youths in Colombia ages 17-25 who were not enrolled in education did not do so because of the high costs of attendance or the need to work (DNP (2015), p. 65). A consequence of the high costs of attendance was that few low-income students were able to enroll in a higher education institution. According to Melguizo et al. (2016), in 2010 only 10% of students in the poorest quintile enrolled in higher education, whereas the enrollment rate in higher education for students in the richest quintile was 52%. Even though Colombia has one of the highest rates of student loan penetration in Latin America, obtaining a student loan is difficult: in 2013, the percentage of students who had accessed a loan through ICETEX – Colombia's public student loan provider – was 22%, much lower than in countries like the United States, Canada or the United Kingdom (OECD/World Bank, 2012). Moreover, few institutions offer scholarship opportunities.

To address the lack of affordability of elite higher education institutions, the Colombian National Ministry of Education launched SPP in October 2014. The stated goal of the program was to provide 40,000 forgivable student loans to high-achieving, low-income students. The loans would cover 100% of tuition costs for any program at any higher education institution – public or private – that had a high-quality accreditation.[3] In addition, SPP beneficiaries received a stipend to cover their educational and living expenses. The stipend amount varied depending on the place of residence of the students and the location of the higher education institution. The funding for SPP came from the National Ministry of Education and the loans were administered by ICETEX. The only requirement for students to have the loan forgiven was to graduate from a high-quality

[3]The high-quality accreditation is a recognition of the quality of education provided by an institution. These accreditations are awarded by the National Ministry of Education after universities apply to obtain it and are evaluated by a committee made up of academic peers. The accreditation serves as a proxy for the elite status of an institution: accredited institutions have higher test scores and their graduates have higher wages. See Londoño-Vélez et al. (2020) for a more detailed discussion.

higher education institution. That way, students would only need to repay the loan if they dropped out before completing their program.

Students became eligible for a SPP loan if they could demonstrate high academic ability and financial need. To prove academic ability, students had to have a global score in *Saber 11*, the national high school exit exam, that would place them in roughly the top decile of the score distribution.[4] Students proved financial need through their SISBEN score, a household wealth index calculated by the National Department of Planning that is widely used as a proxy-means test to determine eligibility for social services. The SISBEN cutoff scores differed depending on whether the student lived in an urban or a rural area.

In 2018, the Colombian Ministry of Education launched *Generación E*, a policy designed to replace SPP. The rationale for replacing SPP was that this was a costly policy on a per-capita basis and that the resources of SPP were mostly devoted to finance tuition for students at private universities. While *Generación E* still had a component that provided forgivable loans to low-income students on merit grounds, it also provided incentives for beneficiary students to enroll in public universities. The requirements to obtain a merit-based forgivable loan that could be used at any higher education institution under *Generación E* were more stringent than under SPP, and it was expected that private universities and donors would cover part of the tuition costs for students who decided to enroll at a private university. In contrast, *Generación E* subsidized the totality of tuition costs for a larger proportion of students, as long as they enrolled at a public institution.

3.3 Data

The main data source for this paper are individual-level administrative records from the Colombian Institute for the Evaluation of Education (ICFES), which is the institution in charge of administering *Saber 11*. I observe the universe of *Saber 11* test takers from 2014 to 2021. For each student, I observe their demographic characteristics (e.g., gender, student-reported household income, household asset ownership), the high school they attend, the characteristics of that school, their global score in the exam, and their subject-specific scores. The older sibling's global score in Saber 11 is one of the running variables in the regression discontinuity design I use in the empirical exercise.

The other running variable is the student's SISBEN score. These scores are calculated on a scale of 0 to 100, where higher values represent higher wealth. Because SISBEN scores depend on the living conditions of the entire household, the scores are constant within the household. The SISBEN dataset includes all indi-

[4]The cutoff score for *Saber 11* was originally designed so that students in the top decile of the distribution were eligible. As students' performance in Saber 11 improved, cutoff scores were increased to make the cutoff score represent a highest percentile of the distribution. The cutoff scores in the four years of the program were 310/500 (2014), 318/500 (2015), 342 (2016), and 348 (2017).

viduals who are in households that have a SISBEN score, approximately 70% of the Colombian population.[5] I merge the SISBEN scores and the household identifiers to the student records from *Saber 11*. I use the household identifiers to form pairs of siblings. Two students are considered to be siblings when they share the same household identifier and they have the same relationship to the household head.

I use two outcomes to characterize the effect of SPP eligibility of the older sibling on the educational trajectory of their younger siblings. First, I explore the impact on the *Saber 11* score of the younger sibling. Such an effect would be suggestive of motivational effects of SPP within the household. Second, I use data from the national college exit exams – *Saber Pro/T&T* – to examine the impact of older sibling SPP eligibility on the probability of college enrollment, type of degree, and achievement for both siblings. Colombia is one of the few countries that administers national standardized tests in higher education. There are two types of tests: *Saber Pro* is designed for students who are pursuing a university (bachelor's) degree, while *Saber T&T* is designed for students pursuing a technical or vocational degree. By observing which test a student takes, it is possible to infer what type of degree they pursue. Moreover, exploring test scores at a higher level could indicate whether any motivational effect persists over time.

For the analysis, I limit the sample to pairs of siblings in which both siblings took *Saber 11* in different years, with the older sibling taking *Saber 11* between 2014 and 2017. This procedure yields 207,981 sibling pairs. Of these pairs, 176,406 meet the need requirement for SPP while 10,029 have older siblings meeting the merit requirement. 5,275 sibling pairs have an older sibling who is eligible for SPP – i.e., meeting both the need- and the merit-based eligilibity criteria.

3.4 Empirical Strategy

The eligibility rules for SPP induce discontinuities in the probability of SPP receipt for the older sibling. A student needs a high enough *Saber 11* score and a low enough *SISBEN* score. Conditional on meeting the SISBEN score requirement, there are some students whose *Saber 11* score barely exceeds the minimum required for SPP eligibility, while there are others whose score in *Saber 11* is close to the eligibility threshold, but not enough to become eligible. Similarly, among the students who meet the *Saber 11* requirement, there are students whose SISBEN score is close to the maximum without exceeding it, while others' scores are above the SISBEN maximum by a small margin. The regression discontinuity design I am proposing compares the outcomes of the siblings of these barely eligible and barely ineligible students.

Formally, let y_{it} denote an educational outcome for the younger sibling in sibling pair i in year t, let

[5] A household obtains a SISBEN score by requesting a household visit from the National Department of Planning. Households receive a SISBEN score an enumerator has visited them and conducted a survey on household composition, sociodemographic characteristics of household members, income, housing conditions, and asset ownership. Since the process of obtaining a SISBEN score is initiated by the household when they believe they are eligible for social assistance, the households not included in the SISBEN dataset tend to be wealthier than those included in it.

$S11_{i\tau}$ denote the score of the older sibling in *Saber 11* in year $\tau < t$, let $C_{m\tau}$ denote the minimum *Saber 11* score to be eligible for SPP on merit grounds in year τ, and let X_{it} denote a vector of covariates for the younger sibling. The reduced-form equation to estimate the effect of the older sibling crossing the merit-based eligibility threshold is given by

$$y_{it} = \alpha + \beta \mathbb{1}[S11_{i\tau} \geq C_{m\tau}] + f(S11_{i\tau}) + \Gamma' X_{it} + \varepsilon_{it}, \tag{3.1}$$

where $\mathbb{1}$ is the indicator function and $f(\cdot)$ is a polynomial in the older sibling's *Saber 11* score.

The reduced-form equation to estimate the effect of crossing the SISBEN threshold can be constructed in an analogous manner:

$$y_{it} = \alpha + \beta \mathbb{1}[SISBEN_{i\tau} \leq C_{n\tau}] + f(SISBEN_{i\tau}) + \Gamma' X_{it} + \varepsilon_{it}, \tag{3.2}$$

where $SISBEN_{i\tau}$ is the SISBEN score of the household and $C_{n\tau}$ is the SPP SISBEN cutoff score. In the empirical exercise, I estimate local linear regressions (i.e., the polynomials in $f(\cdot)$ and $g(\cdot)$ are of order one) using a triangular kernel. To calculate the optimal bandwidth for the estimation of the local linear regressions, I follow the procedure outlined in Calonico et al. (2014). The vector of covariates includes gender, ethnicity, parental education, employment status, type of school (public/private; morning/afternoon shift), and socioeconomic stratum of the household.[6]

I follow Bettinger et al. (2019) and Londoño-Vélez et al. (2020) and estimate the effects of crossing the two eligibility thresholds separately instead of using a multidimensional regression discontinuity design. Londoño-Vélez et al. (2020) note that crossing each discontinuity identifies a different type of compliers and hence the results are useful to illustrate the effects on different populations. Specifically, the estimates from equation (3.1) identify the spillover effect on a low-income population, whereas the estimates from equation (3.2) identify the effect on a population with high-achieving siblings.

In order to give a causal interpretation to the estimates from equations (3.1) and (3.2), it is necessary to assume that potential outcomes are continuous across the two SPP eligibility thresholds. In other words, the only characteristic that should vary discontinuously across the threshold is the eligibility for SPP. An implication of this assumption is that there should not be an accumulation of mass in the density of the running variables at any side of the discontinuity threshold. This would occur if students could perfectly manipulate either their *Saber 11* or SISBEN scores to become eligible for SPP. This is unlikely to be the case because, even if older siblings exerted additional effort while taking *Saber 11*, they would not be able

[6] In Colombia, households are classified into six strata for the purposes of determining the rates charged by public utility companies. These strata are also widely used as a proxy for socioeconomic status.

to exactly predict their scores. SISBEN scores are also difficult to manipulate since the formula used to calculate them is not in the public domain and households must request a reassessment if they are dissatisfied with their score. Households cannot predict whether the reassessment will be done in time for the updated score to be valid for SPP eligibility. Moreover, every year the eligibility conditions for the new round of SPP loans was released after students took *Saber 11*. Figure 3.1 shows the density of both running variables. In neither figure is there evidence of accumulation of mass in the vicinity of the threshold, which suggests that the continuity assumption holds.

As an additional test to assess the plausibility of the identifying assumption, I estimate equation (3.1) using observable sibling characteristics as the outcome of interest. The assumption of continuity of potential outcomes across the merit threshold implies that the observable characteristics of sibling pairs with barely eligible and barely ineligible older siblings should be similar. Table 3.1 presents the results of these estimations. I do not find differences between sibling pairs on either side of the merit threshold with respect to gender composition, age gap, and type of high school attended. These results further support the assumption that the only characteristic that varies across the merit cutoff is the older sibling's SPP eligibility.

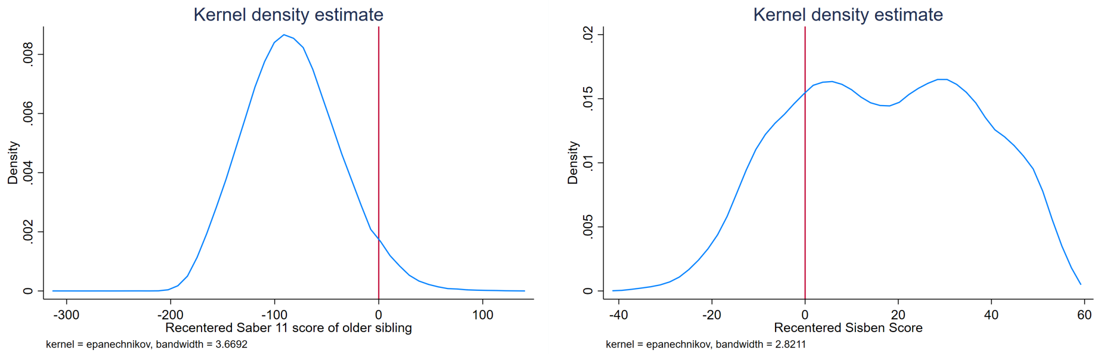
Table 3.1: Effects of older sibling crossing Saber 11 eligibility threshold on sibling pair observable characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Gender Composition						Private School	
	Older M, Younger M	Older M, Younger F	Older F, Younger M	Older F, Younger F	Age Difference	Highly-Educated Parent	Older	Younger
Older Sibling Eligible	-0.000	-0.007	0.001	0.005	-0.061	0.014	-0.009	-0.015
	(0.013)	(0.013)	(0.013)	(0.013)	(0.050)	(0.014)	(0.015)	(0.014)
N	174,768	174,768	174,768	174,768	174,768	174,768	174,768	174,768
CCT Bandwidth	34.037	39.028	32.537	35.579	31.677	39.124	26.800	29.423

This table shows the effects of the older sibling crossing the SPP merit threshold (*Saber 11*) on the observable characteristics of the sibling pairs. The estimating sample is made up of all SISBEN-eligible pairs who took *Saber 11* in different years, with the older sibling taking the test between 2014 and 2017. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

3.5 Results

This section presents the estimated spillover effects on younger siblings' achievement and enrollment in higher education. First, I show that crossing both the *Saber 11* and the SISBEN 11 threshold is associated with a discontinuous increase in the probability of SPP eligibility for the older sibling. Next, I present the results on younger sibling scores in *Saber 11*. I explore these effects in both the overall sample and in particular subgroups. Then, I present the impacts of older sibling eligibility on both siblings' higher education outcomes using the data from *Saber Pro/T&T*.



(a) Saber 11 score of older sibling

(b) Household SISBEN score

Notes: This figure depicts the density function of the two running variables: the *Saber 11* score of the older sibling (left) and the household SISBEN score. Both variables are recentered around the discontinuity cutoff, and SISBEN scores are coded so that eligible scores are positive and to the right of the eligibility cutoff. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

Figure 3.1: Density function of running variables

3.5.1 SPP Eligibility of Older Siblings

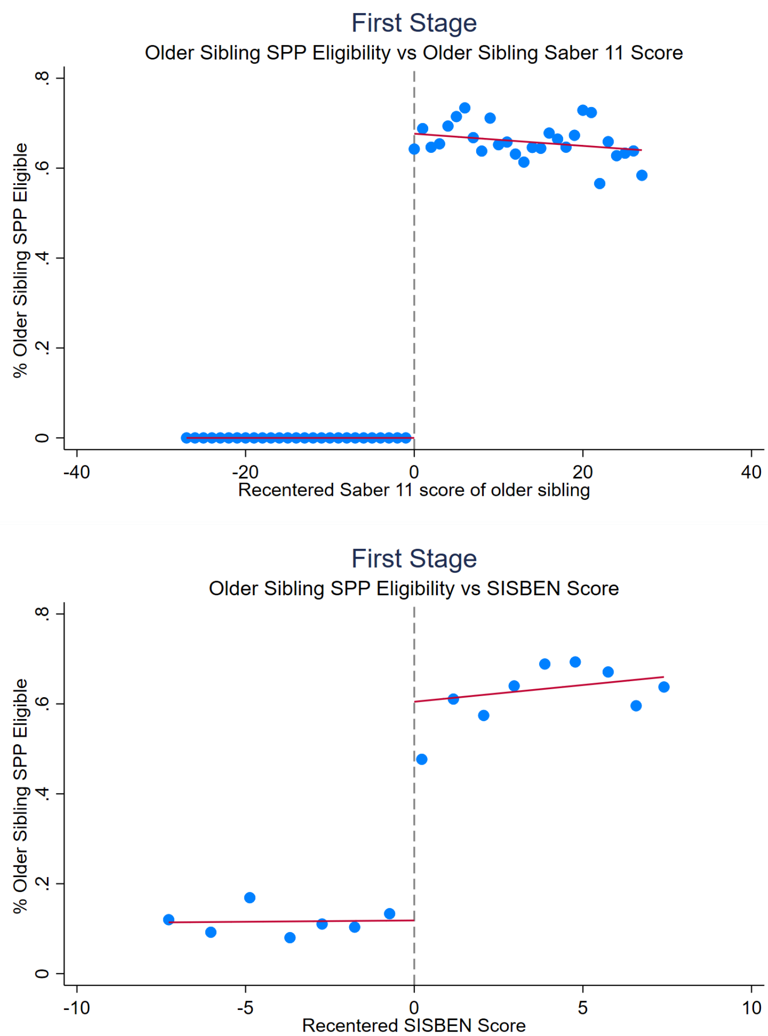
The regression discontinuity design will represent a spillover effect of the eligibility of the older sibling for SPP if indeed the probability of the older sibling being eligible changes discontinuously across the threshold. Table 3.2 and figure 3.2 show the results of estimating equations (3.1) and (3.2) with older sibling SPP eligibility as the outcome of interest. For need-eligible older siblings, crossing the *Saber 11* threshold represents a 67 pp. increase in the probability of eligibility, while for merit-eligible older siblings, crossing the threshold increases the probability of SPP eligibility by 46 pp. The large magnitude of the direct effect on the older sibling's eligibility validate the interpretation of the crossing of the threshold as the effect of having an older sibling who is eligible for SPP.

Table 3.2: First-stage results of older sibling SPP eligibility on the older sibling crossing the SPP thresholds.

	(1)	(2)
Threshold Crossing	0.671	0.463
	(0.011)	(0.030)
Running variable	Saber 11	SISBEN
CCT Bandwidth	26.624	11.039
N	176,406	10,029

Notes: This table shows the effect of the older sibling crossing either eligibility threshold on their own SPP eligibility. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). The sample in column 1 is made up of all SISBEN-eligible pairs who took Saber 11 in different years, while the sample in column 2 is made up of all pairs with merit-eligible older siblings who took Saber 11 in different years. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

The reduced-form estimates from equations (3.1) and (3.2) do not take into account that not every sibling



Notes: This figure depicts the probability of SPP eligibility for the older sibling by the older sibling's *Saber 11* score (top) or by the household SISBEN score (bottom). Both variables are recentered around the discontinuity cutoff, and SISBEN scores are coded so that eligible scores are positive and to the right of the eligibility cutoff. Each dot represents the percentage of SPP-eligible older siblings among those sibling pairs with a given *Saber 11*/SISBEN score. The fit lines are calculated using local linear regression (i.e., the polynomial of the running variable is of order one). Data sources: ICFES (*Saber 11*), DNP (SISBEN)

Figure 3.2: First stage result: older sibling SPP eligibility

who crosses either threshold becomes eligible for SPP. This can be done using a fuzzy regression discontinuity design with crossing the thresholds as instruments for older sibling's SPP eligibility. The first- and second-stage specifications in the case of *Saber 11* are given by

$$\text{Elig}_{i\tau} = \delta + \theta \mathbb{1}[S11_{i\tau} \geq C_{m\tau}] + g(S11_{i\tau}) + \Lambda' X_{it} + \nu_{i\tau}, \tag{3.3}$$

$$y_{it} = \alpha + \beta \widehat{Elig}_{i\tau} + f(S11_{i\tau}) + \Gamma' X_{it} + \varepsilon_{it}, \tag{3.4}$$

where $\text{Elig}_{i\tau}$ denotes older sibling SPP eligibility. Notice that this fuzzy design is instrumenting for *eligibility* and not actual *take-up* of a SPP loan. While take-up would be the ideal measure to estimate the spillover effects of SPP loans, I cannot use it because I do not observe it. According to Londoño-Vélez et al. (2020), in the first cohort of SPP, 59.4% of eligible students took up a loan. This percentage is likely higher for later cohorts since they are aware of the existence of the policy and may account for it when making decisions with respect to higher education enrollment.

3.5.2 Spillover Effects on High School Achievement for Older Siblings

Having established that crossing the thresholds affects the SPP eligibility of the older sibling, I now present the spillover effects of older sibling SPP eligibility on achievement outcomes for the younger sibling at the high school level. I measure the effect on three outcomes: (i) the raw global score of the younger sibling in *Saber 11*, (ii) the percentile rank of the younger sibling in *Saber 11*, and (iii) younger sibling SPP eligibility. Global scores in *Saber 11* range from 0 to 500 points and are standardized to have a mean of 250 points and a standard deviation of 50 points.

Table 3.3 presents the estimated effects when using the older sibling's *Saber 11* score as the running variable. I find that for low income students, having an older sibling crossing the merit eligibility threshold for SPP does not impact their achievement in *Saber 11* or their probability of being SPP eligible. In the case of the younger sibling's test scores, I can rule out increases greater than 0.04 standard deviations (SDs) and decreases greater than 0.07 SDs. This null effect is in line with those found in the Chilean and Croatian contexts by Altmejd et al. (2021). Figure 3.3 depicts the achievement outcomes for the younger sibling as a function of their older siblings' *Saber 11* scores. The positive correlation in test scores among siblings is consistent with the idea that academic ability is partly determined by household inputs and characteristics, and similar relationships have been documented in other contexts (e.g., Goodman et al. (2015) in the United States).

I find a similar null result on younger siblings' *Saber 11* test scores and percentile ranks when using

Table 3.3: Reduced-form and IV results using Saber 11 scores as the running variable.

	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced Form			Fuzzy RDD		
	Score	SPP Elig.	Percentile Rank	Score	SPP Elig.	Percentile Rank
Older Sibling Eligible	-0.683	0.011	-0.476	-1.016	0.002	-0.708
	(1.353)	(0.008)	(0.699)	(2.014)	(0.008)	(1.040)
N	176,406	176,406	176,406	176,406	176,406	176,406
CCT Bandwidth	29.814	31.366	30.004	29.814	24.403	30.004

Notes: Column 1-3 show the reduced-form effect of the older sibling crossing the SPP merit eligibility threshold (*Saber 11* score) on the high school achievement outcomes of their younger sibling. Columns 4-6 show the effect after instrumenting older sibling SPP eligibility with their *Saber 11* score. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). The estimating sample is made up of all SISBEN-eligible pairs who took *Saber 11* in different years, with the older sibling taking the test between 2014 and 2017. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

the household SISBEN score as the running variable (see table 3.4 and figure 3.4). Because this effect is estimated for students with an older sibling who is merit-eligible for SPP, sample sizes are considerably smaller. Therefore, the null effect is less precisely estimated; a non-zero effect, if present, would be between -0.06 and 0.16 SDs. The positive slope seen in figure 3.4 in the relationship between SISBEN scores and achievement in *Saber 11* reflect that students from more disadvantaged households tend to perform worse in *Saber 11* than wealthier students.

Table 3.4: Reduced-form and IV results using SISBEN scores as the running variable.

	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced Form			Fuzzy RDD		
	Score	SPP Elig.	Percentile Rank	Score	SPP Elig.	Percentile Rank
Older Sibling Eligible	2.665	0.103	1.412	6.400	0.122	3.507
	(2.837)	(0.016)	(1.433)	(6.552)	(0.027)	(3.314)
N	10,029	10,029	10,029	10,029	10,029	10,029
CCT Bandwidth	10.554	10.434	10.261	9.156	9.567	9.011

Notes: Column 1-3 show the reduced-form effect of crossing the SPP need eligibility threshold (SISBEN score) on the high school achievement outcomes of the younger sibling. Columns 4-6 show the effect after instrumenting SPP eligibility with the household SISBEN score. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). The estimating sample is made up of all pairs with *Saber 11*-eligible older siblings who took the test between 2014 and 2017 and in a different year than their younger sibling. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

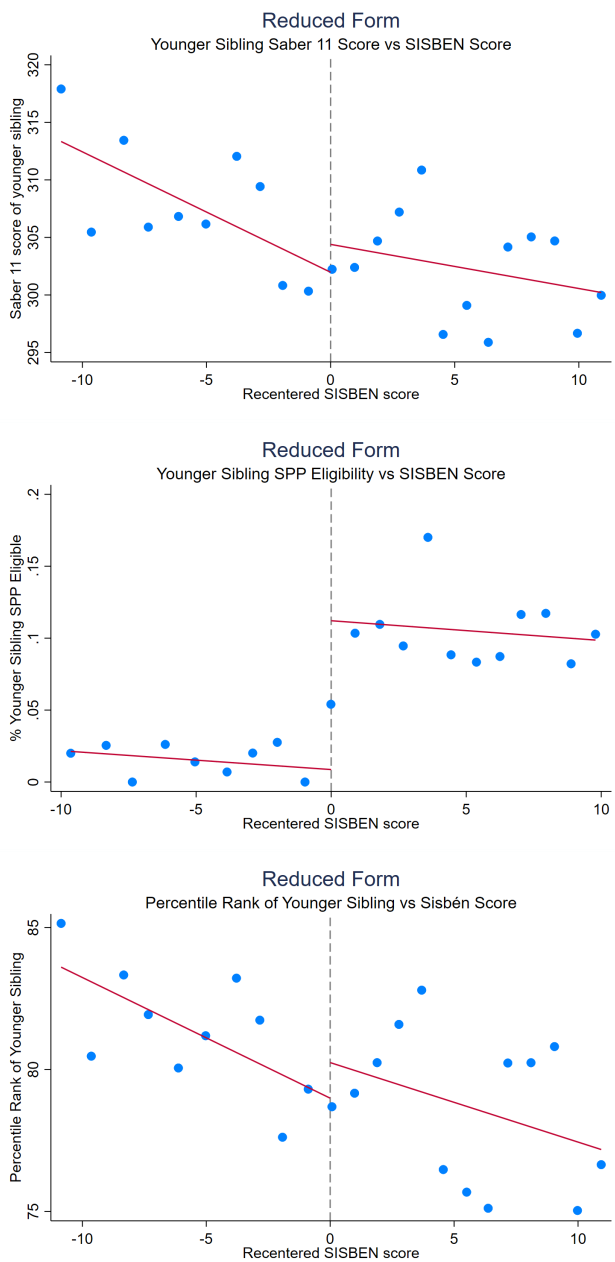
While I do not find an effect on achievement, I do find that crossing the SISBEN threshold increases the probability of SPP eligibility for the younger sibling by 10.3 pp. Given that SISBEN scores are constant within a household, this effect is the combined effect of the direct increase in probability of eligibility that the younger sibling receives by having a low enough SISBEN scores and the indirect effect that operates through the older sibling being eligible for SPP.

Notes: This figure depicts the effects of the older sibling crossing the SPP merit eligibility (*Saber 11* score) on the younger sibling's high school achievement outcomes: their score in *Saber 11* (top), their eligiblity for SPP (middle), and their percentile rank in *Saber 11* (bottom). The *Saber 11* scores of the older sibling are recentered around the discontinuity cutoff. Each dot represents the mean outcome for the younger sibling conditional on their older sibling's *Saber 11* score. The fit lines are calculated using local linear regression (i.e., the polynomial of the running variable is of order one). Data sources: ICFES (*Saber 11*), DNP (SISBEN)

Figure 3.3: Reduced form results: younger sibling high school achievement outcomes by older sibling *Saber 11* score

3.5.3 Heterogeneity in High School Achievement Outcomes

The results presented so far suggest that SPP eligibility of the older sibling has on average a null effect on the younger sibling's SPP eligibility and on test scores in *Saber 11*. This null effect could mask important heterogeneity in the spillover effects among particular subpopulations. I investigate this possibility by re-



Notes: This figure depicts the effects of the household crossing the SPP need eligibility (SISBEN score) on the younger sibling's high school achievement outcomes: their score in *Saber 11* (top), their eligiblity for SPP (middle), and their percentile rank in *Saber 11* (bottom). SISBEN scores are recentered around the discontinuity cutoff and coded so that eligible scores are positive and to the right of the eligibility cutoff. Each dot represents the mean outcome for the younger sibling conditional on the household SISBEN score. The fit lines are calculated using local linear regression (i.e., the polynomial of the running variable is of order one). Data sources: ICFES (*Saber 11*), DNP (SISBEN)

Figure 3.4: Reduced form results: younger sibling high school achievement outcomes by SISBEN score

stricting the sample and re-estimating the spillover effects on achievement and SPP eligibility. I calculate heterogenous effects by gender composition of the siblings, age difference, parental education, and whether siblings attend the same high school. The results for this analysis are in tables 3.5 and 3.6. I do not find a statistically significant effect for any of the analyzed subgroups when using *Saber 11* as the running variable. I do find significant effects for SPP eligibility when using SISBEN as the running variable, although these results represent, as discussed in the last subsection, a combination between the direct effect of the younger sibling becoming need-eligible and the indirect effect from the older sibling becoming need-eligible. There are some non-zero effects associated with crossing the SISBEN threshold on achievement for some groups, like a 25 point (approximately 0.1 SDs) decrease in test scores for sibling pairs with an older sister and a younger brother, and an 11 point increase when the two siblings take the test less than two years apart. For both running variables and for all heterogeneity criteria, I am unable to reject the null hypothesis of differential spillover effects. The evidence from this exercise does not suggest that the null effect on the population is the result of opposite-signed effects offsetting one another.

Table 3.5: RDD results for different subpopulations using Saber 11 as the running variable.

	(1) Score	(2) SPP Eligibility	(3) Percentile Rank
	PANEL A: GENDER COMPOSITION		
Two Brothers	0.057	0.012	0.562
(N=38,709)	(2.985)	(0.015)	(1.447)
Older Brother, Younger Sister	-2.774	-0.005	-0.801
(N=45,069)	(2.393)	(0.013)	(1.183)
Older Sister, Younger Brother	-0.659	0.047	-1.567
(N=40,571)	(3.004)	(0.022)	(1.573)
Two Sisters	1.367	-0.001	0.684
(N=52,057)	(3.016)	(0.015)	(1.484)
	PANEL B: AGE DIFFERENCE		
1-2 years	1.374	0.024	0.550
(N=64,522)	(2.169)	(0.016)	(1.138)
3-4 years	-1.200	0.011	-0.950
(N=76,843)	(2.115)	(0.012)	(1.154)
5+ years	-1.508	-0.007	-1.012
(N=35,041)	(2.733)	(0.009)	(1.497)
	PANEL C: PARENTAL EDUCATION		
Both parents with at most high school	-1.616	0.004	-0.941
(N=129,884)	(1.857)	(0.010)	(0.983)
At least one parent with more than high school	-0.666	0.016	0.283
(N=46,522)	(1.838)	(0.012)	(0.908)
	PANEL D: SAME/DIFFERENT SCHOOL		
Same school	-0.274	0.007	-0.526
(N=108,153)	(1.567)	(0.011)	(0.860)
Different school	-0.194	0.015	-0.450
(N=68,253)	(2.209)	(0.011)	(1.129)

Notes: This table shows the reduced-form effect of the older sibling crossing the SPP merit eligibility threshold (*Saber 11* score) on the high school achievement outcomes of the younger sibling for different types of sibling pairs. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

Table 3.6: RDD results for different subpopulations using SISBEN as the running variable.

	(1)	(2)	(3)
	Score	SPP Eligibility	Percentile Rank
	PANEL A: GENDER COMPOSITION		
Two Brothers	11.314	0.102	5.245
(N=2,738)	(6.134)	(0.032)	(2.735)
Older Brother, Younger Sister	7.208	0.112	2.047
(N=2,776)	(5.859)	(0.024)	(2.836)
Older Sister, Younger Brother	-25.024	0.151	-11.267
(N=2,159)	(8.264)	(0.049)	(3.923)
Two Sisters	-0.513	0.054	0.676
(N=2,356)	(5.768)	(0.027)	(2.871)
	PANEL B: AGE DIFFERENCE		
1-2 years	11.540	0.169	6.063
(N=3,376)	(5.205)	(0.031)	(2.636)
3-4 years	1.002	0.096	0.420
(N=4,067)	(4.565)	(0.025)	(2.207)
5+ years	-4.495	0.022	-1.574
(N=2,586)	(5.933)	(0.018)	(2.717)
	PANEL C: PARENTAL EDUCATION		
Both parents with at most high school	3.743	0.075	1.417
(N=4,006)	(4.416)	(0.021)	(2.262)
At least one parent with more than high school	2.118	0.126	1.451
(N=6,023)	(3.868)	(0.024)	(1.758)
	PANEL D: SAME/DIFFERENT SCHOOL		
Same school	4.458	0.136	1.562
(N=6,259)	(3.469)	(0.021)	(1.818)
Different school	0.298	0.043	0.159
(N=3,770)	(5.364)	(0.022)	(2.662)

Notes: This table shows the reduced-form effect of the household crossing the SPP need eligibility threshold (SISBEN score) on the high school achievement outcomes of the younger sibling for different types of sibling pairs. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*), DNP (SISBEN)

3.5.4 Direct and Spillover Effects on Higher Education Enrollment and Achievement

The sibling spillover effects of SPP could have manifested themselves in dimensions other than high school achievement. As discussed in section 3.3, I cannot directly observe SPP loan take-up or college enrollment for either sibling. I proxy college enrollment by whether the student is observed taking either *Saber Pro* or *Saber T&T*, the national higher education exit exams targeted at students pursuing an academic or technical degree, respectively. In addition to observing whether the student is taking a higher education exit exam, I can observe the type of institution (public or private) from which the student is graduating, and the score in the test. I estimate the effects of older sibling eligibility on both the older siblings – for whom the estimated effects represent the direct effect of becoming SPP eligible themselves – and the younger siblings – who would receive a spillover effect from the SPP eligibility of their older siblings. For brevity, I will only present the reduced-form estimates of the effects of the older sibling crossing the *Saber 11* cutoff.

A caveat of the results presented in this subsection is that they are based on higher education *exit* exams. Therefore, a student is only observed if they enroll in higher education *and* remain enrolled until they take the exam. This would indicate that the sample is positively selected and that would bias the results upward. In addition, I only observe students taking *Saber Pro* or *Saber T&T* until 2021. Considering that the first cohort of SPP beneficiaries would have started their higher education studies in 2015 (if they enrolled immediately after graduation) and that bachelor's degrees in Colombia are designed to take between 4 and 5 years, the window to observe students taking both *Saber 11* and *Saber Pro/T&T* is small, especially for younger siblings of students who received SPP in later years and for students with siblings with a large age gap.

Table 3.7 and figure 3.5 present the results of SPP eligibility for the older sibling. I find that eligible older siblings are 8.3 pp more likely to be observed taking either higher education exit exam. When looking at the probability of observing the student taking a particular test, I find a 13.4 pp increase in the probability of taking *Saber Pro* and a 6.7 pp decrease in the probability of taking *Saber T&T*. Taken together, these results suggest that not only did SPP induce students to enroll in higher education, but also that it incentivized students to enroll in universities and pursue a bachelor's degree instead of a technical or vocational degree. Older siblings who are SPP eligible and who enrolled in a higher education institution are 29.5 pp more likely to enroll in a private institution than need-eligible students who barely fail the merit eligibility criterion. Furthermore, the test scores of SPP eligible older siblings in *Saber Pro* are 2.15 points higher, which represents a 0.07 SD increase in relation to the control group.[7] This result likely reflects the fact that students could only take SPP loans to attend high-quality institutions. These direct effects are similar in magnitude to those found by Londoño-Vélez et al. (2023).

The estimated spillover effects for the younger siblings of the older sibling crossing the *Saber 11* SPP

[7] Very few SPP eligible older siblings are observed taking *Saber T&T*

Table 3.7: Reduced-form results for outcomes related to higher education for the older siblings using Saber 11 scores as the running variable.

	(1) Takes Either Test	(2) Takes Saber Pro	(3) Takes Saber T&T	(4) Private Institution	(5) Score in Saber Pro
Older Sibling SPP Eligibility	0.083	0.134	-0.067	0.295	2.154
	(0.017)	(0.017)	(0.009)	(0.024)	(0.945)
N	174,915	174,915	174,915	18,712	18,669
Mean dep. var.	0.220	0.107	0.124	0.658	150.62

Notes: This table shows the reduced-form effects of the older sibling crossing the SPP merit eligibility threshold (*Saber 11* score) on their own enrollment and achievement outcomes in higher education. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). The sample is limited to all SISBEN-eligible pairs who took Saber 11 in different years. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*, *Saber Pro/T&T*), DNP (SISBEN)

eligibility cutoff are shown in table 3.8 and figure 3.6. The estimated effect for the probability of taking either higher education exit exam is not statistically significant, but the point estimate is positive, consistent with a positive spillover effect. Younger siblings of barely eligible older siblings who enroll in higher education are 12.5 pp more likely to be enrolled in a private institution. This could indicate that SPP is allowing households to redistribute resources to allow non-recipient children to attend a private institution. Just like in the case of the older sibling, the point estimate on test scores for younger siblings of barely eligible older siblings is positive, but unlike the effect on older siblings, it is statistically indistinguishable from zero. Given the previously discussed caveats, the results presented in tables 3.7 and 3.8 should be understood as preliminary evidence of the direct and spillover effects of SPP eligibility on downstream outcomes. These effects should be estimated more precisely as time passes and more data becomes available.

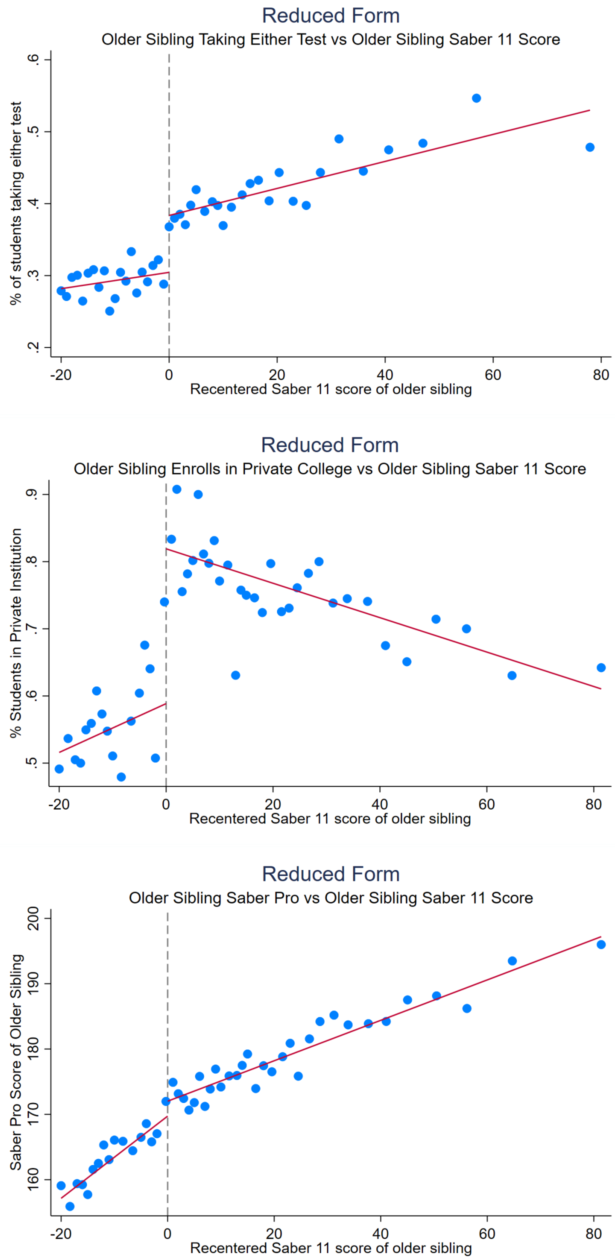
Table 3.8: Reduced-form results for outcomes related to higher education for the younger siblings using Saber 11 scores as the running variable.

	(1) Takes Either Test	(2) Takes Saber Pro	(3) Takes Saber T&T	(4) Private Institution	(5) Score in Saber Pro
Older Sibling SPP Eligibility	0.009	0.007	0.003	0.125	3.856
	(0.007)	(0.007)	(0.005)	(0.067)	(3.474)
N	174,915	174,915	174,915	2,102	2,097
Mean dep. var.	0.037	0.012	0.026	0.716	151.83

Notes: This table shows the reduced-form effects of the older sibling crossing the SPP merit eligibility threshold (*Saber 11* score) on their younger siblings' enrollment and achievement outcomes in higher education. All regressions include the following covariates: gender, ethnicity, parental education, employment status, and type of school (public vs private; morning vs afternoon shift). The sample is limited to all SISBEN-eligible pairs who took Saber 11 in different years. These estimations use a degree 1 polynomial (local linear regression), the optimal bandwidth from Calonico et al. (2014), and a triangular kernel. Robust standard errors in parentheses. Data sources: ICFES (*Saber 11*, *Saber Pro/T&T*), DNP (SISBEN)

3.6 Discussion

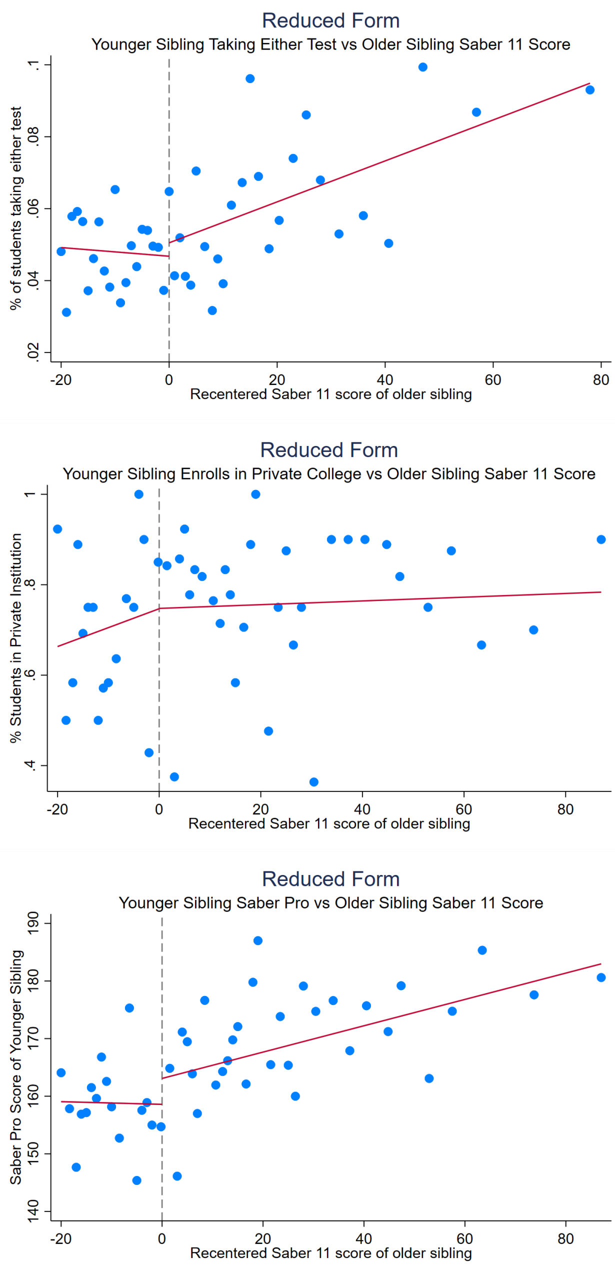
Despite the large body of evidence that suggests that older siblings influence the educational outcomes and trajectories of their younger siblings and that SPP has been successful in increasing the probability of higher education enrollment among low-income students in Colombia, I find no evidence of spillover effects of



Notes: This figure depicts the effects of the older sibling crossing the SPP merit eligibility (*Saber 11* score) on their own higher education enrollment and achievement outcomes: whether they took a college exit exam (top), whether they enrolled at a private institution (middle), and their score in the college exit exam – either *Saber Pro* or *Saber T&T* (bottom). The *Saber 11* scores of the older sibling are recentered around the discontinuity cutoff. Each dot represents the mean outcome for the older sibling conditional on their own *Saber 11* score. The fit lines are calculated using local linear regression (i.e., the polynomial of the running variable is of order one). Data sources: ICFES (*Saber 11*, *Saber Pro/T&T*), DNP (SISBEN)

Figure 3.5: Reduced form results: older sibling higher education enrollment and achievement outcomes by older sibling *Saber 11* score

SPP on younger siblings' achievement in high school and on their higher education enrollment decisions. Even though some of these null results could be due to data limitations – especially those related to higher education outcomes – there are some potential explanations as to why the sibling spillover effects found in



Notes: This figure depicts the effects of the older sibling crossing the SPP merit eligibility (*Saber 11* score) on the following higher education enrollment and achievement outcomes of their younger siblings: whether the younger sibling took a college exit exam (top), whether the younger sibling enrolled at a private institution (middle), and the score of their younger sibling in the college exit exam – either *Saber Pro* or *Saber T&T* (bottom). The *Saber 11* scores of the older sibling are recentered around the discontinuity cutoff. Each dot represents the mean outcome for the older sibling conditional on their own *Saber 11* score. The fit lines are calculated using local linear regression (i.e., the polynomial of the running variable is of order one). Data sources: ICFES (*Saber 11*, *Saber Pro/T&T*), DNP (SISBEN)

Figure 3.6: Reduced form results: younger sibling higher education enrollment and achievement outcomes by older sibling *Saber 11* score

other contexts are not observed in Colombia.

One potential reason for the nonexistence of spillover effects is that SPP targeted students from disadvantaged backgrounds.[8] The resources available to these students in the absence of the policy are limited and in most cases, not enough to cover the costs of college attendance, especially at the elite universities where SPP recipient students were able to attend using the forgivable loans. Given that these households typically have little or no access to credit, younger siblings who were not SPP recipients would be unable to follow the steps of their older siblings even if they desired to do so.

Another important fact is that, as Bernal and Penney (2019) and Laajaj et al. (2022) find, SPP had a strong motivational effect and contributed to improved achievement among low-income students. The increase in motivation may be plausibly constant for siblings of recipients and non-recipients. The null result in achievement would occur because even if achievement among younger siblings of recipients increased, so too did it increase among siblings of non-recipients.

Sibling spillover effects are likely to depend on the experience of the older sibling while in college. While some beneficiary students may thrive in an elite university, some others may struggle because of academic or societal pressure.[9] The heterogeneity in the experience of older siblings in elite universities is largely unobservable, thus making it difficult to assess whether this is a relevant explanation for the observed null effect.

The results presented in this paper are not definitive evidence that SPP had no effect on the educational outcomes of other household members. For instance, the direction of the effects found in the analysis using the higher education exit exams is positive, but the precision of the estimates is hindered by the small sample size, which is a consequence of a strategy based on observing students at the end of their higher education studies in a relatively narrow time window. These spillover effects on enrollment are very relevant from a policy standpoint and merit further exploration. The insights on spillover effects from this paper and from further research on the topic will be helpful in understanding the influence of older siblings on educational outcomes in an environment of financial disadvantage. Research on this topic will also inform the policy efforts to maximize the societal benefits of policies targeted at reducing liquidity constraints that prevent millions of students around the world from enrolling in higher education.

[8]The most closely related paper that looks at spillover effects of financial aid in a middle-income country is Barrios-Fernández (2022). In contrast to SPP, the loan programs explored in that paper are available to a much larger fraction of the population, both in terms of academic merit and household income.

[9]Anecdotal evidence suggests that some SPP recipients have had difficulties adapting to elite universities. As of June 2022, 4,052 SPP recipients had dropped out of higher education and were required to pay their SPP loan (Observatorio de la Universidad Colombiana, 2022). Additional accounts in Spanish can be found in https://www.elespectador.com/educacion/ser-pilo-paga-un-programa-que-se-convirtio-en-una-deuda-impagable-para-mas-de-cuatro-mil-pilos/ or https://www.semana.com/finanzas/trabajo-y-educacion/articulo/el-drama-que-viven-jovenes-que-desertaron-del-programa-ser-pilo-paga/202247/.

Bibliography

Abdulkadiroğlu, A. and Sönmez, T. (2003). School Choice: A Mechanism Design Approach. *American Economic Review*, 93(3):729–747.

Adukia, A. (2017). Sanitation and Education. *American Economic Journal: Applied Economics*, 9(2):23–59.

Aguirre, J. and Matta, J. (2021). Walking in Your Footsteps: Sibling Spillovers in Higher Education Choices. *Economics of Education Review*, 80:102062.

Altmejd, A., Barrios-Fernández, A., Drlje, M., Goodman, J., Hurwitz, M., Kovac, D., Mulhern, C., Neilson, C., and Smith, J. (2021). O Brother, Where Start Thou? Sibling Spillovers on College and Major Choice in Four Countries. *The Quarterly Journal of Economics*.

Alvarez Martinelli, H., Elacqua, G., Piñeros, L., Rivera, M. C., and Santos, H. (2018). ¿Cómo mejorar la eficiencia y la equidad de la inversión educativa en Colombia ante un panorama fiscal restrictivo?: Diagnóstico y propuestas. Technical report, Inter-American Development Bank.

Anderson, D. M., Charles, K. K., and Rees, D. I. (2022). Reexamining the Contribution of Public Health Efforts to the Decline in Urban Mortality. *American Economic Journal: Applied Economics*, 14(2):126–157.

Angrist, J. D. (2014). The Perils of Peer Effects. *Labour Economics*, 30:98–108.

Ashraf, N., Glaeser, E., Holland, A., and Steinberg, B. M. (2021). Water, Health and Wealth: The Impact of Piped Water Outages on Disease Prevalence and Financial Transactions in Zambia. *Economica*, 88(351):755–781.

Avitabile, C., Bobba, M., and Pariguana, M. (2017). High School Track Choice and Liquidity Constraints: Evidence from Urban Mexico. Technical report, IZA Discussion Papers No. 10506. IZA Institute of Labor Economics: Bonn, Germany.

Bar-David, Y., Urkin, J., and Kozminsky, E. (2005). The effect of voluntary dehydration on cognitive functions of elementary school children. *Acta paediatrica*, 94(11):1667–1673.

Barrios-Fernández, A. (2022). Neighbors' Effects on University Enrollment. *American Economic Journal: Applied Economics*, 14(3):30–60.

Barro, R. J. and Lee, J. W. (2013). A New Data Set of Educational Attainment in the World, 1950–2010. *Journal of development economics*, 104:184–198.

Beach, B. (2022). Water Infrastructure and Health in US Cities. *Regional Science and Urban Economics*, 94:103674.

Beach, B., Ferrie, J., Saavedra, M., and Troesken, W. (2016). Typhoid Fever, Water Quality, and Human Capital Formation. *The Journal of Economic History*, 76(1):41–75.

Belmonte, A., Bove, V., D'Inverno, G., and Modica, M. (2020). School Infrastructure Spending and Educational Outcomes: Evidence from the 2012 Earthquake in Northern Italy. *Economics of Education Review*, 75:101951.

Bernal, G. L. and Penney, J. (2019). Scholarships and Student Effort: Evidence from Colombia's Ser Pilo Paga Program. *Economics of Education Review*, 72:121–130.

Berry, J., Mehta, S., Mukherjee, P., Ruebeck, H., and Shastry, G. K. (2021). Crowd-Out in School-Based Health Interventions: Evidence from India's Midday Meals Program. *Journal of Public Economics*, 204:104552.

Bettinger, E., Gurantz, O., Kawano, L., Sacerdote, B., and Stevens, M. (2019). The Long-Run Impacts of Financial Aid: Evidence from California's Cal Grant. *American Economic Journal: Economic Policy*, 11(1):64–94.

Beuermann, D., Jackson, C. K., Navarro-Sola, L., and Pardo, F. (2018). What is a Good School, and Can Parents Tell? Evidence on the Multidimensionality of School Output. Technical report, NBER Working Paper No. 25342. National Bureau of Economic Research: Cambridge, MA, USA.

Bhalotra, S. R., Brown, R., and Venkataramani, A. S. (2021a). The Impact of Access to Clean Water on Cognitive and Physical Development: Evidence from Mexico's Programa de Agua Limpia.

Bhalotra, S. R., Diaz-Cayeros, A., Miller, G., Miranda, A., and Venkataramani, A. S. (2021b). Urban Water Disinfection and Mortality Decline in Lower-Income Countries. *American Economic Journal: Economic Policy*, 13(4):490–520.

Biasi, B., Lafortune, J., and Schönholzer, D. (2023). Effectiveness, Efficiency, and Governance of School Capital Investments Across the U.S. Retrieved from https://www.barbarabiasi.com/uploads/1/0/1/2/101280322/bilaschon_2023.pdf on August 11, 2023.

Bičáková, A., Cortes, G. M., and Mazza, J. (2021). Caught in the Cycle: Economic Conditions at Enrolment and Labour Market Outcomes of College Graduates. *The Economic Journal*, 131(638):2383–2412.

Black, S. E., Breining, S., Figlio, D. N., Guryan, J., Karbownik, K., Nielsen, H. S., Roth, J., and Simonsen, M. (2021). Sibling Spillovers. *The Economic Journal*, 131(633):101–128.

Blom, E., Cadena, B. C., and Keys, B. J. (2021). Investment over the Business Cycle: Insights from College Major Choice. *Journal of Labor Economics*, 39(4):1043–1082.

Bobba, M. and Frisancho, V. (2020). Self-Perceptions about Academic Achievement: Evidence from Mexico City. *Journal of Econometrics*.

Boozer, M. and Cacciola, S. E. (2001). Inside the 'Black Box' of Project STAR: Estimation of peer effects using experimental data. Technical report, Economic Growth Center Discussion Paper No. 832. Yale University: New Haven, CT, USA.

Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting Event Study Designs: Robust and Efficient Estimation. arXiv preprint arXiv:2108.12419. Retrieved from https://arxiv.org/pdf/2108.12419.pdf on February 17, 2024.

Bos, J., Shonchoy, A., S., Ravindran, S., and Khan, A. (2023). Early Childhood Human Capital Formation at Scale. Retrieved from https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3906697 on August 11, 2023.

Buchmann, N., Field, E. M., Glennerster, R., and Hussam, R. N. (2019). Throwing the Baby out with the Drinking Water: Unintended Consequences of Arsenic Mitigation Efforts in Bangladesh. Technical report, National Bureau of Economic Research.

Bundy, D. A., de Silva, N., Horton, S., Jamison, D. T., and Patton, G. C. (2018). Optimizing Education Outcomes: High-Return Investments in School Health for Increased Participation and Learning. *Disease Control Priorities*, 1.

Callaway, B., Goodman-Bacon, A., and Sant'Anna, P. H. (2021). Difference-in-Differences with a Continuous Treatment. *arXiv preprint arXiv:2107.02637*.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics*, 225(2):200–230.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326.

Camacho, A., Messina, J., and Uribe Barrera, J. P. (2017). The Expansion of Higher Education in Colombia: Bad students or Bad Programs? *Documento CEDE*, (2017-13).

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *The Review of Economics and Statistics*, 90(3):414–427.

Chang, E. and Padilla-Romo, M. (2019). The Effects of Local Violent Crime on High-Stakes Tests. Technical report.

Chen, Y. J., Li, L., and Xiao, Y. (2020). Early-Life Exposure to Tap Water and the Development of Cognitive Skills. *Journal of Human Resources*, pages 0917–9031R3.

Coelli, M. B. (2011). Parental Job Loss and the Education Enrollment of Youth. *Labour Economics*, 18(1):25–35.

Conlin, M. and Thompson, P. N. (2017). Impacts of New School Facility Construction: An Analysis of a State-Financed Capital Subsidy Program in Ohio. *Economics of Education Review*, 59:13–28.

Cooper-Vince, C. E., Kakuhikire, B., Vorechovska, D., McDonough, A. Q., Perkins, J., Venkataramani, A. S., Mushavi, R., Baguma, C., Ashaba, S., Bangsberg, D., et al. (2017). Household water insecurity, missed schooling, and the mediating role of caregiver depression in rural uganda. *Global Mental Health*, 4:e15.

Currie, J. and Vogl, T. (2013). Early-Life Health and Adult Circumstance in Developing Countries. *Annu. Rev. Econ.*, 5(1):1–36.

Cutler, D. and Miller, G. (2005). The Role of Public Health Improvements in Health Advances: the Twentieth-Century United States. *Demography*, 42(1):1–22.

Dahl, G., Rooth, D.-O., and Stenberg, A. (2020a). Long-Run Returns to Field of Study in Secondary School. Technical report, NBER Working Paper No. 27254. National Bureau of Economic Research: Cambridge, MA, USA.

Dahl, G. B., Rooth, D.-O., and Stenberg, A. (2020b). Family Spillovers in Field of Study.

Departamento Administrativo Nacional de Estadística (2012). Anexos – Encuesta Nacional de Calidad de Vida. Retrieved from https://www.dane.gov.co/index.php/estadisticas-por-tema/salud/calidad-de-vida-ecv on March 21, 2023.

Departamento Administrativo Nacional de Estadística (2023). Boletín Técnico – Educación Formal (EDUC) 2022. Retrieved from https://www.dane.gov.co/files/operaciones/EDUC/bol-EDUC-2022.pdf on August 12, 2023.

Das, J., Dercon, S., Habyarimana, J., Krishnan, P., Muralidharan, K., and Sundararaman, V. (2013). School Inputs, Household Substitution, and Test Scores. *American Economic Journal: Applied Economics*, 5(2):29–57.

de Carvalho Filho, I. E. (2012). Household Income as a Determinant of Child Labor and School Enrollment in Brazil: Evidence from a Social Security Reform. *Economic Development and Cultural Change*, 60(2):399–435.

de Janvry, A., Finan, F., Sadoulet, E., and Vakis, R. (2006). Can Conditional Cash Transfer Programs Serve as Safety Nets in Keeping Children at School and from Working when Exposed to Shocks? *Journal of development economics*, 79(2):349–373.

Devoto, F., Duflo, E., Dupas, P., Parienté, W., and Pons, V. (2012). Happiness on tap: Piped water adoption in urban morocco. *American Economic Journal: Economic Policy*, 4(4):68–99.

Dizon-Ross, R. (2019). Parents' Beliefs About their Children's Academic Ability: Implications for Educational Investments. *American Economic Review*, 109(8):2728–2765.

Departamento Nacional de Planeación (2015). *Plan Nacional de Desarrollo 2014-2018. Todos por un Nuevo País*. Departamento Nacional de Planeación, Bogotá, Colombia.

Duryea, S., Lam, D., and Levison, D. (2007). Effects of Economic Shocks on Children's Employment and Schooling in Brazil. *Journal of development economics*, 84(1):188–214.

Dustan, A. (2018). Family Networks and School Choice. *Journal of Development Economics*, 134:372–391.

Dustan, A. (2020). Can Large, Untargeted Conditional Cash Transfers Increase Urban High School Graduation Rates? Evidence from Mexico City's Prepa Sí. *Journal of Development Economics*, 143:102392.

Dustan, A., De Janvry, A., and Sadoulet, E. (2017). Flourish or Fail? The Risky Reward of Elite High School Admission in Mexico City. *Journal of Human Resources*, 52(3):756–799.

Dustan, A. and Ngo, D. K. (2018). Commuting to Educational Opportunity? School Choice Effects of Mass Transit Expansion in Mexico City. *Economics of Education Review*, 63:116–133.

Dustmann, C., Puhani, P. A., and Schönberg, U. (2017). The Long-Term Effects of Early Track Choice. *The Economic Journal*, 127(603):1348–1380.

Empresas Públicas de Medellín (2023). Historia - Agua para la Educación, Educación para el Agua. Retrieved from https://www.grupo-epm.com/site/fundacionepm/quehacemos/programas/aguaparalaeducacion-educacionparaelagua/historia on February 22, 2023.

Ersoy, F. Y. (2020). The Effects of the Great Recession on College Majors. *Economics of Education Review*, 77:102018.

Estrada, R. and Gignoux, J. (2017). Benefits to elite schools and the expected returns to education: Evidence from mexico city. *European Economic Review*, 95:168–194.

Fack, G., Grenet, J., and He, Y. (2019). Beyond Truth-Telling: Preference Estimation with Centralized School Choice and College Admissions. *American Economic Review*, 109(4):1486–1529.

Fadda, R., Rapinett, G., Grathwohl, D., Parisi, M., Fanari, R., Calò, C. M., and Schmitt, J. (2012). Effects of Drinking Supplementary Water at School on Cognitive Performance in Children. *Appetite*, 59(3):730–737.

Ferreira, F. H. and Schady, N. (2009). Aggregate Economic Shocks, Child Schooling, and Child Health. *The World Bank Research Observer*, 24(2):147–181.

Flynn, P. and Smith, T. (2022). Rivers, Lakes and Revenue Streams: The Heterogeneous Effects of Clean Water Act Grants on Local Spending. *Journal of Public Economics*, 212:104711.

Gale, D. and Shapley, L. S. (1962). College Admissions and the Stability of Marriage. *The American Mathematical Monthly*, 69(1):9–15.

Galiani, S., Gertler, P., and Schargrodsky, E. (2005). Water for Life: The Impact of the Privatization of Water Services on Child Mortality. *Journal of political economy*, 113(1):83–120.

Gamper-Rabindran, S., Khan, S., and Timmins, C. (2010). The Impact of Piped Water Provision on Infant Mortality in Brazil: A Quantile Panel Data Approach. *Journal of Development Economics*, 92(2):188–200.

Glewwe, P. and Miguel, E. A. (2007). The impact of child health and nutrition on education in less developed countries. *Handbook of development economics*, 4:3561–3606.

Glewwe, P. and Muralidharan, K. (2016). Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. In *Handbook of the Economics of Education*, volume 5, pages 653–743. Elsevier.

Glewwe, P., Siameh, C., Sun, B., and Wisniewski, S. (2021). School Resources and Educational Outcomes in Developing Countries. In McCall, B. P., editor, *The Routledge Handbook of the Economics of Education*. Routledge.

Glick, P. J., Sahn, D. E., and Walker, T. F. (2016). Household Shocks and Education Investments in Madagascar. *Oxford Bulletin of Economics and Statistics*, 78(6):792–813.

Goodman, J., Hurwitz, M., Smith, J., and Fox, J. (2015). The Relationship Between Siblings' College Choices: Evidence from One Million SAT-Taking Families. *Economics of Education Review*, 48:75–85.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics*, 225(2):254–277.

Gross, E., Günther, I., and Schipper, Y. (2018). Women are Walking and Waiting for Water: The Time Value of Public Water Supply. *Economic Development and Cultural Change*, 66(3):489–517.

Gurantz, O., Hurwitz, M., and Smith, J. (2020). Sibling Effects on High School Exam Taking and Performance. *Journal of Economic Behavior & Organization*, 178:534–549.

Hilger, N. G. (2016). Parental Job Loss and Children's Long-Term Outcomes: Evidence from 7 Million Fathers' Layoffs. *American Economic Journal: Applied Economics*, 8(3):247–83.

Hill, E. L. (2022). The Impact of Oil and Gas Extraction on Infant Health. Forthcoming, American Journal of Health Economics.

Holmstrom, B. and Milgrom, P. (1991). Multitask Principal–Agent Analyses: Incentive Contracts, Asset Ownership, and Job Design. *The Journal of Law, Economics, and Organization*, 7(special_issue):24–52.

Hong, K. and Zimmer, R. (2016). Does Investing in School Capital Infrastructure Improve Student Achievement? *Economics of Education Review*, 53:143–158.

Huttunen, K. and Riukula, K. (2019). Parental job loss and children's careers. Technical report, IZA Discussion Papers No. 12788. IZA Institute of Labor Economics: Bonn, Germany.

Instituto Nacional de Estadística y Geografía (2020). Encuesta Nacional de Ocupación y Empleo (ENOE), población de 15 años y más de edad. Retrieved from https://www.inegi.org.mx/programas/enoe/15ymas/ on September 9, 2020.

Instituto Politécnico Nacional (1998). Reglamento Interno del Instituto Politécnico Nacional. Retrieved from https://www.aplicaciones.abogadogeneral.ipn.mx/reglamentos/reglamento-interno.pdf on February 13, 2021.

Jakiela, P., Ozier, O., Fernald, L., and Knauer, H. (2020). Big Sisters.

Jalan, J. and Ravallion, M. (2003). Does Piped Water Reduce Diarrhea for Children in Rural India? *Journal of econometrics*, 112(1):153–173.

Joensen, J. S. and Nielsen, H. S. (2018). Spillovers in Education Choice. *Journal of Public Economics*, 157:158–183.

Karbownik, K. and Özek, U. (2019). Setting a Good Example? Examining Sibling Spillovers in Educational Achievement Using a Regression Discontinuity Design. Technical report, National Bureau of Economic Research.

Kazianga, H., Levy, D., Linden, L. L., and Sloan, M. (2013). The Effects of "Girl-Friendly" Schools: Evidence from the BRIGHT School Construction Program in Burkina Faso. *American Economic Journal: Applied Economics*, 5(3):41–62.

Koolwal, G. and Van de Walle, D. (2013). Access to Water, Women's Work, and Child Outcomes. *Economic Development and Cultural Change*, 61(2):369–405.

Kosec, K. (2014). The Child Health Implications of Privatizing Africa's Urban Water Supply. *Journal of health economics*, 35:1–19.

Kremer, M., Leino, J., Miguel, E., and Zwane, A. P. (2011). Spring Cleaning: Rural Water Impacts, Valuation, and Property Rights Institutions. *The Quarterly Journal of Economics*, 126(1):145–205.

Laajaj, R., Moya, A., and Sánchez, F. (2022). Equality of Opportunity and Human Capital Accumulation: Motivational Effect of a Nationwide Scholarship in Colombia. *Journal of Development Economics*, 154:102754.

Lafortune, J. and Schönholzer, D. (2022). The impact of school facility investments on students and homeowners: Evidence from Los Angeles. *American Economic Journal: Applied Economics*, 14(3):254–289.

Liu, S., Sun, W., and Winters, J. V. (2019). Up in STEM, Down in Business: Changing College Major Decisions with the Great Recession. *Contemporary Economic Policy*, 37(3):476–491.

Londoño-Vélez, J., Rodríguez, C., and Sánchez, F. (2020). Upstream and Downstream Impacts of College Merit-Based Financial Aid for Low-Income Students: Ser Pilo Paga in Colombia. *American Economic Journal: Economic Policy*, 12(2):193–227.

Londoño-Vélez, J., Rodríguez, C., Sánchez, F., and Álvarez, L. E. (2023). Financial Aid and Social Mobility: Evidence from Colombia's Ser Pilo Paga. Retrieved on March 17, 2023 from https://sites.google.com/site/julianalondonovelez/research.

Marcus, M. (2022). Testing the Water: Drinking Water Quality, Public Notification, and Child Outcomes. *Review of Economics and Statistics*, 104(6):1289–1303.

Martorell, P., Stange, K., and McFarlin Jr, I. (2016). Investing in Schools: Capital Spending, Facility Conditions, and Student Achievement. *Journal of Public Economics*, 140:13–29.

McMichael, C. (2019). Water, Sanitation and Hygiene (WASH) in Schools in Low-Income Countries: A Review of Evidence of Impact. *International journal of environmental research and public health*, 16(3):359.

Meeks, R. C. (2017). Water Works: The Economic Impact of Water Infrastructure. *Journal of Human Resources*, 52(4):1119–1153.

Melguizo, T., Sanchez, F., and Velasco, T. (2016). Credit for Low-Income Students and Access to and Academic Performance in Higher Education in Colombia: A Regression Discontinuity Approach. *World Development*, 80:61–77.

Ministerio de Educación Nacional de Colombia (2023). Estadísticas Sectoriales de Educación Preescolar, Básica y Media - Indicadores de Proceso. Retrieved from http://bi.mineducacion.gov.co:8380/eportal/web/planeacion-basica/tasa-de-cobertura-bruta on August 8, 2023.

Muralidharan, K. and Sundararaman, V. (2013). Contract Teachers: Experimental Evidence from India. Technical report, National Bureau of Economic Research.

Nauges, C. and Strand, J. (2017). Water Hauling and Girls' School Attendance: Some New Evidence from Ghana. *Environmental and Resource Economics*, 66(1):65–88.

Ngo, D. and Dustan, A. (2019). Closing the STEM Gender Gap: Choice, Access, and Policies in Mexico City High Schools.

Nicoletti, C. and Rabe, B. (2019). Sibling Spillover Effects in School Achievement. *Journal of Applied Econometrics*, 34(4):482–501.

Observatorio de la Universidad Colombiana (2022). La otra cara de Ser Pilo Paga: Miles de dramas familiares de quienes abandonaron. Retrieved from https://www.universidad.edu.co/la-otra-cara-de-ser-pilo-paga-miles-de-dramas-familiares-de-quienes-abandonaron/ on March 18, 2023.

OECD/International Bank for Reconstruction and Development/The World Bank (2012). *Reviews of National Policies for Education: Tertiary Education in Colombia 2012*. OECD Publishing.

Ortega Hesles, M. E. (2015). *School Choice and Educational Opportunities: The Upper-Secondary Student-Assignment Process in Mexico City*. PhD thesis, Harvard University.

Psacharopoulos, G. and Patrinos, H. A. (2018). Returns to Investment in Education: A Decennial Review of the Global Literature. *Education Economics*, 26(5):445–458.

Qureshi, J. A. (2018). Additional Returns to Investing in Girls' Education: Impact on Younger Sibling Human Capital. *The Economic Journal*, 128(616):3285–3319.

Rege, M., Telle, K., and Votruba, M. (2011). Parental Job Loss and Children's School Performance. *The Review of Economic Studies*, 78(4):1462–1489.

Roodman, D., Nielsen, M. Ø., MacKinnon, J. G., and Webb, M. D. (2019). Fast and wild: Bootstrap inference in Stata using boottest. *The Stata Journal*, 19(1):4–60.

Roth, J., Sant'Anna, P. H., Bilinski, A., and Poe, J. (2023). What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature. *Journal of Econometrics*.

Secretaría de Educación Pública (2018). Sistema Nacional de Información de Estadística Educativa. Retrieved from https://planeacion.sep.gob.mx/estadisticaeducativas.aspx on February 25, 2021.

Solis, A. (2017). Credit Access and College Enrollment. *Journal of Political Economy*, 125(2):562–622.

Stevens, A. H. and Schaller, J. (2011). Short-Run Effects of Parental Job Loss on Children's Academic Achievement. *Economics of Education Review*, 30(2):289–299.

Tanndal, J. and Päällysaho, M. (2020). Family-level stress and children's educational choice: Evidence from parental layoffs. Retrieved from http://juliatanndal.com/JMP.pdf on December 11, 2020.

Todd, P. E. and Wolpin, K. I. (2003). On the Specification and Estimation of the Production Function for Cognitive Achievement. *The Economic Journal*, 113(485):F3–F33.

United Nations Development Programme (2006). Human Development Report 2006: Beyond Scarcity: Power, Poverty, and the Global Water Crisis. *UNDP (United Nations Development Programme)*.

Vermeersch, C. and Kremer, M. (2004). School Meals, Educational Achievement and School Competition: Evidence from a Randomized Evaluation.

World Health Organization and United Nations International Children's Emergency Fund (2023). WHO/UNICEF Joint Monitoring Programme for Water Supply, Sanitation and Hygiene (JMP) Global Dataset – Households. Retrieved from https://washdata.org/data on March 21, 2023.

World Health Organization (2017). Diarrhoeal Disease Fact Sheet. Retrieved from https://www.who.int/news-room/fact-sheets/detail/diarrhoeal-disease on March 23, 2023.

The World Bank (2021). World Development Indicators. Retrieved from https://databank.worldbank.org/source/world-development-indicators on March 6, 2021.

Yi, J., Heckman, J. J., Zhang, J., and Conti, G. (2015). Early health shocks, intra-household resource allocation and child outcomes. *The Economic Journal*, 125(588):F347–F371.

Zhang, J. and Xu, L. C. (2016). The Long-Run Effects of Treated Water on Education: The Rural Drinking Water Program in China. *Journal of Development Economics*, 122:1–15.