Essays in Education Policies in Latin America

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Submitted in partial fulfillment of the
requirements for the degree
of Doctor of Philosophy
in the Graduate School of Arts and Sciences

COLUMBIA UNIVERSITY
2016

ABSTRACT

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Education is often perceived as a key to development and growth, consequently, in the last decades, many countries have increased education coverage in all education levels. The creation of international education quality measurement programs, such as OECD's Programme for International Student Assessment (PISA) or the Trends in International Mathematics and Science Study (TIMSS), have further led to a focus on improving education quality. For these reasons, the last decades have seen an increase in the importance of education in the political debate. This has been particularly relevant in Latin America, where, additionally, education has been used, sometimes, to fight traditionally high levels of income inequality, with a significant rise in education expenditure and coverage.

The evaluation of education policies that aim to increase education coverage, quality or equity is, however, generally difficult. Many education policies are large-scale policies and are likely to affect all students or workers in the population, even those not directly benefiting from the policy. For example, students not participating in some education policy could still experience changes in their classmate characteristics that could affect their achievement. The presence of possible spillovers may change the direction of the effects of large-scale education policies when all the population is included in the analysis. Therefore, analyzing solely the effects on students participating in the policy may not give a complete picture of the effects of large-scale education policies.

This dissertation focuses on the effects that three large-scale education policies that aimed to improve education equity, quality and coverage, respectively, had on students and workers affected differently by the policies. Particularly, each chapter analyzes the aggregate effects for the population of each education policy and decomposes these effects on the impact suffered by different groups of students or workers.

In Chapter 1, I analyze the effects on test scores of a policy that aimed to increase education equity in Chile. I study the effects of an increase in school choice for low-income students by examining a 2008 reform that made the value of Chile's (previously flat, universal) school voucher

a step function of student income. This policy increased the proportion of private schools that low income, eligible children could access free of charge from 0.5 to 0.7. In order to identify aggregate effects and the impact within groups of students, I combine the introduction of the policy with variation from a date of birth enrollment cutoff for 1st grade. I show that the differentiated voucher lowered the probability that students used public schools by a small fraction and that these students shifted out of low achievement public schools to enroll in low achievement private schools. Nonetheless, private schools where these students enrolled had better test scores and socioeconomic composition at baseline, and less experienced teachers and smaller class sizes than public schools where they would have enrolled in the absence of the program. Despite the improvement in some school observable characteristics, I do not find any increase in test scores for students more likely to move to private schools. Further analysis suggests a rise in test scores for students most likely to stay in public schools. These results suggest that the policy had an overall modest positive effect on test scores, but that this positive effect was caused by responses from public schools instead of by students responding to the increase in school choice.

In Chapter 2, I study the impact on test scores of a policy that aimed to improve education quality by increasing transparency of school performance in Chile. Particularly, I look at the effects of the distribution of school performance information to all families in Chile in 2011. Since I am interested in identifying effects for different groups of students, I define a control group within each group of students by using variation in enrollment year. Due to the presence of a date of birth enrollment cutoff for 1st grade enrollment there is variation in enrollment year for students born a few days apart. I combine this variation together with the timing of the distribution of information to identify the effects of the policy. I show that the distribution of information increased enrollment in high-performing schools, particularly for students in the third quartile of the municipality socioeconomic distribution. Thanks to this policy, students in the third quartile were exposed to a better socioeconomic composition of peers. Test score results suggest that there was an overall positive effect on verbal test scores, particularly for students in the third quartile, seemingly caused by an improvement in peer characteristics. However, there does not seem to be any significant change in test scores for students less likely to change enrollment decisions in response to the new information.

Finally, Chapter 3, examines the effects of a policy that increased tertiary education coverage in

Colombia on wages. I identify the effects on the distribution of wages using two different empirical strategies: the DiNardo, Fortin and Lemieux (1996) reweighting method and a differences-in-differences strategy. My results suggest that the overall distribution of wages remained constant, once labor demand shifts and productivity changes are taken into account. In contrast, wages increased for workers that were not at the margin of studying tertiary education, workers with primary education or less, and the density of wages at high levels of the distribution decreased for high school and tertiary education graduates. However, there were no effects on average wages for workers with any of the education levels. These results suggest that the policy had heterogeneous effects within the wage distribution and between education levels that were not captured by changes in average wages.

These three chapters show that large-scale education policies can, sometimes, have effects on achievement or wages of students that are not participating in the policy, and that these effects are not always visible in the aggregate effects. Therefore, policy-makers and researchers should take into account the presence of spillovers or strategic responses when designing or analyzing large-scale education policies.

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Acknowledgements

I would like to thank my committee, fellow students and faculty and staff at Columbia for the support offered during the writing of this dissertation. First of all, I would like to express my gratitude to my advisors, Miguel Urquiola and Kiki Pop-Eleches for their guidance and encouragement, without their help this dissertation would not have been possible. I would like to thank Miguel for always pushing me to think out of the box and for all his on-point advice and Kiki for guiding me to creative solutions when I did not know what direction to follow. I would also like to thank Miikka Rokkanen, for sharing his knowledge with me and for all his help and support. He made this process, including the job market, much more enjoyable. And I would like to thank Bentley Macleod for all his guidance and support during all my Ph.D., he pushed me to think from another perspective, open my mind and become a better researcher. I am also grateful to Peter Bergman for his valuable comments and helpful feedback. It has been amazing to have the opportunity to work with these amazing professors and people.

I would also like to thank Jonas Hjort, for his invaluable advice and support, Eric Verhoogen, for his sharp comments and ability to detect weaknesses in my research, Supreet Kaur, for helping me become a better empirical researcher, and Suresh Naidu, for his innovative and helpful comments.

Additionally, I would like to thank all the researchers, experts, politicians and friends that took the time to help me understand the policies analyzed in this thesis. I would especially like to thank Nicolás Rojas, Ignacio Monge, Marcela Huepe, Josefa Aguirre, Pablo Egaña, Alejandra Mizala, Miguel Crispi, Sergio Urzúa, Nicolás Muñoz, Carolina Flores, Cristian Bellei, Dante Contreras, Ana Pozo, Alejandro Carrasco, Andrés Hernando, Ricardo Paredes, Verónica Cabezas, Roberto Schurch, Rodrigo Díaz, Francicos Meneses, Sylvia Eyzaguirre, Ana María Gazmuri, Anita Laborde, Maria Jesús Rojas, Teresa del Sol, Ignacio Irarrázaval, Francisco Gallego, Claudia Allende, Juan Saavedra and Juan Pablo Valenzuela. This paper used data from the Chilean *Agencia de Calidad de la Educación* and Ministry of Education. I would like to thank the *Agencia* and the Ministry for generously providing me with the access to the data.

I am also deeply grateful to my amazing classmates and friends. Especially, I would like to thank Andrea Kiguel, Mica Sviatschi and Evan Riehl for their encouragement, motivation and help. Andrea, you have been a sister all these years, the Ph.D. would have been a very different experience without you. I would also like to thank Andrés Ayala, Mariana Garcia Schmidt, Lesley Turner, Hyelim Son, Xing Xia, Shaowen Luo, Valentina Duque, Jorge Mejia-Licona, Chris Hansman, Zach Brown, Andrey Ermolov, Yogita Shamdasani, Nicolás De Roux, Golvine de Rochambeau, Katherine Meckel, Mike Mueller-Smith and all my cohort for all their help and advice during this process.

Finally I would like to thank my family, since they were the ones who made this experience possible. Without the emotional and economic support of my parents, Cristobal Navarro Perona and Maria Mercè Palau Imbern, brother, Xavier Navarro Palau and, especially, my husband, Ruben Andino, this dissertation would not be completed. Thanks for believing in me and for your unconditional love and support.

To Ruben, for all his support.

Chapter 1

Effects of Differentiated School Vouchers: Evidence From a Policy Change and Date of Birth Cutoffs

1.1 INTRODUCTION

The past decades have seen the creation of dozens of school voucher programs. The motivation has been that if there were an increase in school choice and competition, it would improve educational outcomes (Friedman, 1962). Most of these programs remain small in size (Epple et al., 2015), and only in a few cases is their reach national. Consequently, there is still limited evidence on the effects of large school choice programs on achievement. These effects may differ from small-scale programs due, for example, to the possible re-sorting of students across schools (e.g., Hsieh and Urquiola, 2006), or to school responses to the increased competitive pressure (e.g., Neilson, 2013). In addition, the literature on large-scale programs has generally focused on the aggregate effects of such programs or used structural estimation. Thus, effects on different types of students are barely known. Finally, Epple et al. (2015) highlights that much work remains to be done on the design of school vouchers. For instance, should their value be flat or a function of students' income?

In this paper I consider a reform that allowed the value of the voucher to vary with family income in the context of Chile's nationwide voucher system, and I assess the effects of this reform on different types of students. Specifically, this policy allowed public and private schools to potentially receive significantly higher subsidies when they enrolled eligible, lower-income children. In exchange, the schools had to set achievement targets, and to stop charging tuition add-ons for eligible children. The number of private schools free of charge for eligible students increased by about 20 percentage points in the initial years of the policy.

To analyze the effects of this policy, I use the pool of students enrolling in 1$^{\text{st}}$ grade. I match these data to socioeconomic and test score information obtained from a national standardized exam that takes place every year for 4$^{\text{th}}$ grade students.

I combine two sources of variation to identify the impact of this reform. First, I exploit the timing of the policy, which was introduced in 2008. Second, I rely on the fact that, like other countries, Chile has an enrollment cutoff — children born after June 30$^{\text{th}}$ must, in principle, wait a year to enroll in school. Taken together, these facts imply that students born only a few days apart faced potentially different amounts of school choice when they enrolled in 1$^{\text{st}}$ grade.

A comparison of their outcomes in a regression discontinuity (RD) design thus provides an assessment of the effects of increased school choice. Additionally, by using the RD created by the enrollment cutoff for years prior to the introduction of the differentiated voucher, I can identify possible trends and confounders that could be biasing the RD estimates of the effects of the differentiated voucher. Then, taking differences of the RD estimates, I obtain unbiased estimates of the impact of the voucher reform. In contrast to most of the literature on large-scale voucher programs, this identification strategy allows me to analyze the effects of the increased school choice separately for different types of students.

The introduction of the differentiated voucher in 2008 seemingly implies that the increase in school choice will be a one-time event. However, in practice, information on the program seems to have disseminated slowly, thus, in most specifications I apply a similar analysis to subsequent cohorts. This is consistent with evidence that families did not understand their eligibility immediately, and that schools similarly were slow to understand the rules of the program. Additionally, this reflects that families had to research which private schools were participating in the program in order to benefit from the increased school choice.

I use the two sources of variation (i.e., timing of the policy and date of birth) to carry out three exercises. First, I quantify and characterize compliers — students that enrolled in private schools instead of public schools in response to the introduction of the differentiated voucher. Second, I look at changes in enrollment choices for all students, and I compare public schools where compliers would have enrolled in the absence of the program to the private schools where they actually enrolled, at baseline. Finally, I analyze the effects of the increased school choice on current school characteristics and test scores.

To quantify and characterize compliers, I instrument enrollment in private schools with the interaction of the timing of the policy and whether a student was born after the enrollment cutoff. From this first stage, I find that, although about 50 percent of the population was eligible for the new voucher, only a small fraction of students changed their enrollment decisions in response to the increased school choice.

In the second stage, I obtain the average characteristics for compliers. The most relevant result is that around 90 percent of students that switched sectors in response to the new voucher had mothers that completed high school education. Therefore, compliers were not the poorest students within the group of eligible students. In addition, I use the fact that most compliers had mothers with high school education to classify students, since the proportion of eligible students in the population was not stable over the period. Thus, I divide students in groups depending on their mothers' education, given that the resulting groups are fairly constant over the period.

Next, I use the differences in RD estimates to examine the effects on enrollment decisions. My results show that the probability of enrolling in a public school fell slightly for students with mothers that had basic education or less, and for students with mothers that completed high school. This probability remained constant for students with mothers that completed university education. Furthermore, my results show that the probability of enrolling in elite private schools remained constant over the period. These results suggest that wealthier students did not try to avoid differentiated voucher users through enrolling in elite private schools.

Instrumenting enrollment in public and private schools in a two-stage least squares system, I find that, on average, compliers did not enroll in low achievement public schools and enrolled in low achievement private schools instead. Nevertheless, the schools they moved to had better socioeconomic characteristics and average test scores, smaller classes and less experienced teachers at baseline.

Regarding current school characteristics, the results of the difference in RD estimates show that, on average, no group of students classified according to the mother's education experiences changes in the socioeconomic composition of its peers after the introduction of the differentiated voucher. Additionally, students with mothers that completed high school, on average, have smaller class sizes and less experienced teachers, which is consistent with the changes in enrollment choice of compliers. With respect to students with mothers that had basic education or less — who were more likely to stay in public schools — my analysis uncovers one potential channel through which public schools could be responding to the introduction of the differentiated voucher. These students have, on average, less experienced teachers once the differentiated voucher is introduced. This suggests that public schools may have replaced more experienced teachers with less experienced instructors.

Despite the fact that compliers enrolled in schools with generally better characteristics, the difference in RD estimates shows that there are no effects on average test scores for students with mothers that completed high school. This result is consistent with a part of the literature in school choice that finds small or inexistent effects for students transferring to better schools (e.g., Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2014).

In contrast, the average test scores of students with low education mothers — those were more likely to stay in public schools — increased. This is consistent with another strand of the school choice literature which suggests that school choice leads to responses by public schools (e.g., Hoxby, 2003; Chakrabarti, 2008). This impact in test scores could be related to the decrease in average experience of teachers for this group of students. But other mechanisms could be at work as well.

This paper relates to several strands of previous work. First, identification of the effects of large-scale voucher programs is generally difficult. These programs distribute vouchers nationwide to all students who want to use them, and may have effects on non-voucher users through changes in student composition or other school characteristics. Therefore, it is hard to define control groups. Generally, the literature has either focused on the aggregate effects of such programs (e.g., Hsieh and Urquiola, 2006; Bohlmark and Lindahl, 2008) or used structural estimation (e.g., Neilson, 2013; Bravo, Mukhopadhyay and Todd, 2010). In a controlled experimental setting, Muralidharan and Sundararaman (2015) considered the effects of the introduction of targeted school vouchers in India on voucher users, students remaining in public schools and students already enrolled in private schools. My identification strategy allows me to quantify and characterize students responding to the introduction of a national differentiated voucher in Chile, compare schools where responding students would have enrolled and actually enrolled in response to the policy, and analyze the impact of the policy on students affected in different ways.

Since this large-scale program was overlaid on top of the national voucher system, its effects may have been more limited and thus relevant to work on small-scale programs. Thus, I also contribute to a broad literature on school choice that has found mixed results on outcomes of students that transfer to private or charter schools (e.g., Abdulkadiroglu et al., 2011; Deming et al., 2014; Rouse, 1998) or to higher quality schools (e.g., Deming et al., 2014). My estimates suggest that students that enrolled in private schools instead of public schools, due to the differentiated voucher, did not significantly increase achievement, despite the improvement in some school characteristics.

Additionally, students left behind in public schools also have been shown to benefit from voucher programs, especially in public schools likely to be affected by the increased competition (Figlio and Hart, 2014; Hoxby, 2003). In this paper I show that, even though the program did not generate a large re-sorting of students, students more likely to stay in public schools experienced increases in average test scores. Even though many mechanisms could be responsible for this result, I find some evidence of response by public schools through changes in their teaching teams.

The remainder of the paper is organized as follows: the following section describes the differentiated voucher program and the Chilean school system. Section 3 explains the possible effects that the introduction of the differentiated voucher could have generated, then, section 4 discusses the data used in this paper and presents descriptive statistics. Section 5 describes my empirical strategy and the differences in RD design, followed by section 6 that discusses the characteristics of students who switched enrollment decisions in response to the policy. Section 7 presents my results and section 8 discusses some robustness checks. Finally, section 8 concludes this paper.

1.2 BACKGROUND

In 1981, school vouchers were introduced in Chile. These vouchers were universal and nationwide: every student had access to a voucher of equal monetary value.

The introduction of vouchers defined three main school types, on the basis of financing and control, that have remained unchanged since 1981:

1. *Public (or municipal) schools*. These schools are controlled by the municipalities and funded centrally through a per-student payment for every child attending their schools. Public schools can only legally turn away students if they are oversubscribed.
2. *Elite private schools*. These private institutions do not receive public funding and are financed through tuition fees. They account for around 7% of total enrollment and serve mainly high-income households.
3. *Voucher (private) schools*. These schools are privately controlled and up to 1993 were funded exclusively through the same per-student payment as public schools. Since 1993 these schools can charge supplementary tuition.[1] If they choose to do so, the per-student payment is progressively reduced as tuition increases, but this reduction does not offset the tuition revenues

[1] Law 19.247 of September 15, 1993.

except for very high tuition levels. In contrast to public schools, voucher schools were allowed to select students up to 2009.[2]

Supplementary financing for specific types of school was introduced after 1981, such as the *P-900* program that transferred resources to schools with low absolute performance, but the basic features of the school voucher program remained constant until 2008.

In the late 1990s, some observers began to suggest a differentiated per-student voucher that might correct educational inequities. However, the voucher payments remained flat until 2008, when a new differentiated voucher was created through the Preferential School Subsidy Law.[3]

The Preferential School Subsidy Law created a two-step voucher that depends on the socioeconomic characteristics of students. All students still receive the original flat voucher, but now participating schools receive an additional amount for each eligible student enrolled.

Through the new differentiated voucher the government wanted to recognize that educating low socioeconomic status students is more costly than educating wealthier students. Furthermore, it hoped that low income students (eligible students) would be more appealing to selective participating voucher schools due to the additional payment, and that, as a consequence, voucher schools would have greater incentives to locate in low-income neighborhoods.

In order to receive the differentiated voucher, schools must formally join the program by signing a four-year agreement with the Ministry of Education.[4] If they do so, schools receive an additional payment worth around 50% of the flat voucher amount for each eligible student enrolled. Participating schools can also receive an additional subsidy if the school has a high percentage of eligible students.[5] This extra subsidy is much smaller in size than the differentiated voucher, and increases with the proportion of eligible students in the school, with discontinuities at 15, 30, 45 and 60 percent of the total student population in the school.

In exchange, participating schools commit to provide a plan that explains how the differentiated voucher resources will be spent to improve the school. They are held accountable for these

[2] Article 12 in Law 20.370 of September 12, 2009. Carrasco et al. (2014) suggest that voucher schools continue to select students.

[3] *Ley de Subvención Escolar Preferencial, number 20.248.*

[4] *The agreement is called Convenio de Igualdad de Oportunidades.*

[5] *The additional subsidy is called Subvención por Concentración.*

expenses and must set achievement targets in terms of standardized test results with a special focus on increasing test scores for eligible students.[6] Additionally, this agreement forbids selecting or expelling students between kindergarten and 6th grade and finally, it also forbids charging any compulsory fees to eligible students.

This last condition implied that due to the participation of voucher schools in the differentiated voucher program, the proportion of free voucher schools for eligible students jumped from around 0.52 in 2007 to 0.65 in 2008, as shown in Figure 1.1, and then rose up smoothly to 0.71 in 2010. Thus, there was an immediate increase in school choice for eligible students followed by additional subsequent increases.

Enrollment responses were gradual, additionally, because there were difficulties in informing parents about the policy and about eligibility. Initially eligibility information was distributed through letters to families, sometimes sent through the school where the students were enrolled. This information was not always understood by families (Soto Aranda, 2011).

Starting in 2010, the responsibility to inform parents about the differentiated voucher program was transferred to the schools, with the Ministry of Education distributing eligibility information by logging into a website. The computer access requirement may reduce access to eligibility information for the poorest eligible students.

On top of the difficulties to obtain information on the differentiated voucher program and eligibility, parents additionally need to research about voucher school participation in the program if they want to benefit from the increased school choice.

As a result, the effects of the policy on enrollment choices were smooth over time, as parents slowly learned about the differentiated voucher program, eligibility and participating schools. Therefore, in my analysis I will consider that students that enrolled in 2008 and each subsequent year were exposed to a new increase in school choice for each year, as parents learned about the differentiated voucher program and were more able to respond to the increase in school choice.

[6] Minimum increases in test scores were set by the Ministry of Education.

1.2.1 Eligibility

Approximately the poorest 50% of the population is eligible for the differentiated voucher program. Eligibility is determined according to socioeconomic indicators. Specifically, a student is eligible if any of the following conditions are met: (i) the student belongs the lowest third of the distribution of scores of a government instrument that measures the family capability to generate income, (ii) the student participates in a social program that targets the 10% poorest population,[7] or (iii) the student is classified in the poorest group of the National Health Fund (FONASA). Finally, students could be temporarily deemed eligible according to socioeconomic indicators.

In 2010, 55% of eligible students were eligible through criteria (1), 30% were eligible through criteria (2), and the remaining 15% through other criteria.

Unfortunately, the instrument used to identify most eligible students, the *Ficha de Protección Social*, was introduced in 2007, and it was not administered to students who could be eligible for the differentiated voucher until 2009.[8]

Additionally, since this same instrument was used to determine eligibility for most social benefits in Chile, citizens had incentives to reduce their scores. Many of them learned how to game the instrument and became eligible,[9] which led to small or even nonexistent differences between eligible and ineligible students as determined by the instrument.

Consequently, eligibility information is not suited to analyze the effects of the differentiated voucher program. Therefore, to analyze the effects of this policy I characterize students responding to the program, as explained in section 6, and use the education level of the mother to classify students.

1.3 Potential Partial and General Equilibrium Effects

In a regular school voucher program, in which students can use school vouchers to pay for tuition in private schools, there are three types of students depending on voucher use: (1) voucher

[7] *Chile Solidario*.

[8] Herrera, Larrañaga and Telias (2010).

[9] Poblete, J. (2012, April 30) *Detectan 50.512 casos de información falsa en ficha de protección social*, *La Tercera*. retrieved from: http://diario.latercera.com/.

users, students that enroll in private schools instead of public schools using the voucher, (2) students left behind in public schools, and (3) students that were already enrolled in private schools.

Suppose that country A introduces a small-scale voucher program in which, for example, a small number of students is eligible for school vouchers that allow them to enroll in private schools. Assuming that private schools are different from public schools, students in group (1), voucher users, would attend schools with different characteristics with respect to the public school they would have enrolled in if the voucher did not exist. These students may or may not experience increases in test scores.

If the voucher program is very small-scale, the change in student composition in public schools would be minimal, and students remaining in public schools should not be affected by voucher users leaving public schools. Additionally, public schools are unlikely to feel threatened by the increased competition.

Similarly, if voucher users are evenly distributed among private schools, students in private schools receiving these voucher users should not be affected. The number of voucher users would be too small to have an effect on the socioeconomic composition of private schools and to have any negative effect on students already enrolled in private schools.

In summary, the effects of a small-scale voucher program would be dominated by the effects on voucher users, since non-voucher users would be barely affected.

The differentiated school voucher program in Chile is, in contrast, a large-scale program. Approximately 50% of the population were eligible for the differentiated voucher. Additionally, participating voucher schools were not allowed to select students anymore. Almost 70% of eligible students were enrolled in public schools in the pre-policy period and, thanks to the differentiated voucher, could enroll in participating voucher schools for free. However, this program was a reform to the already existing national voucher system. Therefore, its effects on re-sorting the students could be large, but also could be small if all students who wanted to enroll in voucher schools were already enrolled in voucher schools using the existing (flat) voucher.

Suppose that eligible students have a strong preference for voucher schools over public schools, but, before the differentiated voucher was introduced, were prevented from enrolling in voucher schools due to student selection and school prices. If this is the case, in response to the differentiated voucher program, a large number of eligible students could enroll in participating voucher schools instead of public schools. As a consequence, if the number of students switching enrollment is sufficiently large, all 3 groups of students could be affected. Additionally, the possible benefits for voucher users of enrolling in a voucher school may be dampened.

In this case, voucher users would leave public schools to enroll in voucher schools that had, at baseline, better characteristics. However, since the poorest half of the population is eligible for the differentiated school voucher and participating voucher schools are not allowed to select students anymore, participating voucher schools could potentially be flooded with voucher users in a way that could affect the school characteristics. In an extreme case, and assuming no differences in value-added, it is possible that voucher schools accepting voucher users would end up with the same socioeconomic characteristics as public schools and with larger class sizes, causing a decline in achievement for students transferring from public schools.

Alternatively, these students may distribute evenly across schools and may not significantly affect student composition, or simply benefit from more effective teachers in voucher schools and achieve test scores.

Students left behind in public schools may also be affected. If a sufficiently large number of students leaves public schools, they may have smaller class sizes, better teacher per student ratios, and better targeting of instruction (Duflo et al., 2011). Additionally, public schools may actively compete for enrollment, and carry out changes at the school level to improve achievement. Therefore, it is possible that students left behind in public schools are positively affected.

Another possibility is that the best students in public schools leave due to the differentiated voucher, and students in public schools have a worse peer composition. In the case in which teachers do not respond by readapting their teaching targets, and schools do not respond, the negative effect due to the worse peer composition could dominate, leading to a decline in achievement by students left behind in public schools.

Finally, students already enrolled in voucher schools may experience negative consequences caused by the change in student composition in participating voucher schools. If the change is sufficiently large, it could slow teaching pace and lower teaching targets, causing a reduction in learning and test scores for students that were already enrolled in voucher schools.

Also, these students may respond to the change in school characteristics by deciding to enroll somewhere else as, for example, in non-participating voucher schools or elite private schools.

In contrast, another possibility is that as the differentiated voucher program was overlaid on the existing voucher system in Chile, very few eligible students switched enrollment decisions. If this is the case, the impact would be the same as in a small-scale program, with the variation that public schools may respond. Given the characteristics of the program, potential competition for public schools increased. Thus, if the perceived increase in school competition is sufficiently large, public schools may try to improve achievement in order to keep students from transferring to voucher schools.

Consequently, as the differentiated voucher was an addition to the already existing national voucher system, the effects of this program on re-sorting and achievement could be large or small. If the effects on re-sorting are large, all students, including the non-voucher users and ineligible students, could be affected, and changes in achievement could go in either direction. If the effects on re-sorting are small, there could still be effects on achievement if the public schools respond to the increase in potential competition. Therefore, since the effects of the introduction of the differentiated voucher program are theoretically ambiguous, empirical evidence is needed. The remainder of this paper will try to clarify which of these scenarios best fits the effects of the introduction of the differentiated voucher in Chile.

1.4 Data

I rely on two sources of administrative data, the Chilean Ministry of Education and the Education Quality Assurance Agency.

From the Ministry of Education, I use the individual enrollment information for all students in primary education from 2005 to 2014, including date of birth, grade, year and school enrolled in and the municipality of residence. Additionally, I use school information from 2005 to 2014, including type of school, school municipality and total enrollment, school average prices for voucher schools from 2005 to 2013, teacher hiring and firing, and teacher characteristics from 2005 to 2013. Finally, I use information about the differentiated voucher program at the school level, including school participation from 2008 to 2014, and funds received and used from the differentiated voucher at the school level for 2008-2012.

From the Education Quality Assurance Agency I use individual standardized test results for

4^{th} grade of basic education, and survey information on socioeconomic characteristics, including parents' education and family gross monthly income from 2008 to 2013.

I will use the sample of students enrolling in 1^{st} grade each year from 2005 to 2010 to determine the effects of the differentiated voucher on school enrollment choices and achievement. Since there are costs related to school switching, I will focus on the initial school enrollment decision, students enrolling in 1^{st} grade.[10]

1.4.1 Descriptive Statistics

Summary statistics can be found in Table 1.1 and Table 1.2. Table 1.1 shows summary statistics of the sample of students and schools used by year of enrollment in 1^{st} grade, while table 1.2 shows average characteristics of schools by type and year.

As shown in Table 1.1, there was a steady decrease in enrollment in public schools during the period, with a corresponding increase in enrollment in voucher schools. With regard to test scores, average test scores for a standardized exam carried out in 4^{th} grade increased smoothly during the period, and, on average, do not seem to improve significantly more for students that enrolled once the differentiated voucher program had been introduced.

Finally, the last panel of Table 1.1 shows that the number of schools was relatively stable over the period. However, there were changes in the composition of schools in the market, with a reduction in the proportion of public schools and a corresponding increase on the proportion of voucher schools. Almost all public schools participated in the differentiated voucher since the first year, but only approximately 50% of voucher schools participated in the first year of the differentiated voucher program. Voucher school participation in the program increased smoothly in the following years.

Table 1.2 shows changes in school characteristics by school type over time. Average class size is largest in voucher schools and lowest in elite private schools. While class sizes in these two school types remained fairly constant, class size fell in public schools once the differentiated voucher

[10] Approximately 42% of children were enrolled in kindergarten in the school where they attended 1^{st} grade two years in advance, and approximately 55% were already enrolled in the same school on the year before starting 1^{st} grade. However, most of the effect of the differentiated voucher in school choice comes from the 45% of students enrolling in a new school in 1^{st} grade. Changes in school choice can also be observed for kindergarten levels but in a smaller magnitude than for 1^{st} grade enrollment.

program was established. Thus, the number of teachers per student increased in public schools starting in 2008. In terms of average school size, all three school types are quite similar, even though public schools suffered from a decline in total enrollment during the period. Socioeconomic composition of students in the three school types is as expected, with public schools having the lowest socioeconomic status students, voucher schools middle class students and elite private schools high socioeconomic status students.

With respect to teacher characteristics, almost the totality of teachers in all three school types hold a degree in education, but the percentage of educated teachers is slightly higher in public schools. Even so, the proportion of educated teachers increased somewhat for all three types of schools in 2008, when the differentiated voucher was introduced. The most experienced teachers are those in public schools. However, the average teacher experience (and age) fell in public schools in 2008, and it continued decreasing in the following years in public and, to a lower extent, voucher schools. This seems due to a change in teaching teams in public schools and, in a smaller scale, in voucher schools that started once the differentiated voucher was introduced, as can be observed with changes in the proportion of newly hired and terminated teachers.

The average teacher evaluation score,[11] that is compulsory for all teachers in public schools, increased in 2008 once the differentiated voucher program was introduced. This increase may have been caused by a rise in effort by teachers in response to the achievement goals set by the differentiated voucher program, the perceived higher school competition, or to a higher effectiveness of newly hired teachers in public schools.

The most immediate effect of the differentiated voucher program was the increase in per-student expenditures. This rose in public and voucher schools on average, with the largest increase in public schools.

Finally, average standardized scores are highest in elite private schools, followed by voucher schools, then public schools. This is consistent with the differences in socioeconomic characteristics of students among the three school types. However, while the differentiated voucher program

[11] The average teacher evaluation score is the score assigned to teachers in public schools by an institution associated with the Ministry of education based on the analysis of a recording of a class and the teaching materials prepared by the teacher, together with a self-evaluation, an evaluation by a peer teacher and by managers. It intends to evaluate the effectiveness of the teacher and this evaluation is compulsory for teachers in public schools since 2005. Teachers newly hired in public schools are not evaluated on the first year of experience.

increased public schools test scores, the effect on voucher school average test scores was more modest and even somewhat negative in the first year of the program.

1.5 EMPIRICAL STRATEGY

I use two sources of variation to analyze the effects of the differentiated voucher program on enrollment choices, school characteristics, and achievement. The first is the timing of the policy, and the second is exogenous variation on students' 1st grade enrollment year.

The differentiated voucher program was implemented in 2008 but, due to the information issues explained above, the distribution of information was slow. Consequently, enrollment responses were smooth over time, since in practice, parents of eligible students faced a new increase in school choice as they became more informed. This implies that all students who enrolled in 1st grade in 2008 or later could have modified their school choices in response to the introduction of the differentiated voucher. Therefore, the cohorts that enrolled in 2008 or later will be considered "treated" cohorts.

The second source of variation originates in Chile's enrollment cutoff dates for 1st grade. The official enrollment cutoff in Chile is March 31: all students whose sixth birthday is later than March 31 should delay enrollment by one year. However, in practice, a decree from the Ministry of Education allows schools to apply later enrollment cutoffs with the latest possible being June 30. This cutoff is the most common in Chile, as shown in McEwan and Shapiro (2008), and is the one I use in this paper.[12]

The June 30 enrollment cutoff implies that, for example, students whose sixth birthday was between April 1 and June 30, 2007, had a positive probability of enrolling in 1st grade in 2007, while students whose sixth birthday was after June 30, 2007, had essentially zero probability.

As a result, the enrollment cutoff creates a discontinuity in enrollment probability and, consequently, in exposure to the differentiated voucher for students enrolling in 2007/2008. This set up can be used in a Regression Discontinuity Design to determine the effects of the differentiated voucher on enrollment choices and achievement.

[12]Despite the existence of multiple enrollment cutoffs, public and voucher schools do not seem to use the choice of enrollment cutoff strategically. For all three types of schools the largest discontinuity happens on June 30 and the magnitude of the discontinuity is similar between public and voucher schools.

Let $y_i(1)$ be the outcome of interest for student i if he was exposed to the increased school choice caused by the introduction of the differentiated voucher at the time of enrollment, and let $y_i(0)$ be the outcome if he was not exposed at time of enrollment. Ideally, I would compute the average impact of the increase in the number of free voucher schools on enrollment choices and test scores as: $\alpha = E(y_i(1) - y_i(0))$.

Since this is not possible, I will compare students whose sixth birthday was on or before the June 30 enrollment cutoff for 2007, 2008 and 2009 to students whose sixth birthday was after those enrollment cutoffs and hence had to delay enrollment to 2008, 2009 and 2010. Eligible students who had to delay enrollment were more informed and, in practice, exposed to an increase in the proportion of free voucher schools, thanks to the differentiated voucher.

Due to the two enrollment cutoffs within the same year, enrollment is not a deterministic function of date of birth for students who were born between April 1 and June 30. Consequently, date of birth affects the probability that a student is exposed to the increase in school choice at the time of enrollment in the following way: $1 = P(T = 1|B > 0) > P(T = 1|B \leq 0) > 0$, where T is the probability of being exposed to the increase in school choice for eligible students at time of enrollment, and B is the distance from the sixth birthday to the June 30$^{\text{th}}$ enrollment cutoff (see figure 1.2 for the 2007/2008 example).

This implies that the regression discontinuity design estimates that use the date of birth as the running variable are actually intent-to-treat (ITT) estimates. Assuming that birth dates are random and that student characteristics are continuous near cutoffs, [13] in the absence of other confounders, the regression discontinuity estimator would provide unbiased estimates of the effects on the outcome of interest of being born at either side of the cutoff. The intent-to-treat RD estimate for the

[13] Table A.1 in the appendix shows that there are no significant differences in socioeconomic characteristics of students born at either side of the cutoff. However, Table A.2 shows that density of births is not stable around the cutoff. Cesarean sections are widely used in Chile and not performed on weekends. In two of the five cohorts used, July 1 fell on a weekend, which could be causing the fall in density of births. Additionally, seasonality in births could explain changes in density of births. This policy was discussed for the first time in October 2005, therefore, since mothers of students that turned six around the 2007/2008 enrollment cutoff got pregnant in 2000/2001, it is impossible that there was manipulation of day of birth in response to the differentiated voucher program. Nevertheless, since the socioeconomic characteristics of students at either side of the cutoff are similar and changes in the density of births cannot be related to the policy, due to the timing of the policy, this fall in birth densities should not cause any bias to the differences in RD estimates.

differentiated voucher effect is given by:

$$\alpha_{ITT,RD} = lim_{B\downarrow 0}E[y|B] - lim_{B\uparrow 0}E[y|B] \tag{1.1}$$

Which can be obtained by estimating the following equation through ordinary least squares:

$$y_i = \beta + \theta Post_i + f(B_i) + \epsilon_i \tag{1.2}$$

where B is the distance from the sixth birthday to the June 30 enrollment cutoff, $Post_i$ is an indicator variable that equals 1 if the student was born after the enrollment cutoff, and $f(B_i)$ is a flexible parametric specification that includes higher-order polynomials of sixth birthday and can vary on either side of the enrollment cutoff. My main specification uses a linear spline on sixth birthday distance to cutoff interacted with a dummy indicating whether the student falls to the left or the right of the cutoff. The coefficient θ in (1.2) represents the estimate of the intent-to-treat effect of the differentiated voucher, $\hat{\alpha}_{ITT,RD} = \hat{\theta}$.

It is possible that there existed pre-trends on enrollment choices, since as seen in the previous section, enrollment in public schools was already falling before the differentiated voucher. There may also be or that there are "age-at-test" or "enrollment age" effects. For example, McEwan and Shapiro (2008) show increases in fourth grade standardized test scores of more than 0.3 standard deviations due to the differences in age caused by the enrollment cutoffs. In this case, the regression discontinuity estimate from above would also capture these confounders, leading to biased estimates.

Assuming that the confounder is constant over time, the RD estimate for years prior to the differentiated voucher program would converge to λ, where λ is the pre-existing trend on enrollment or the "age-at-test" or "enrollment age" effect, while the RD estimate for the initial years of differentiated voucher existence would converge to $\lambda + \alpha$.

Thus, I take differences of the RD estimators to account for these possible confounders (Carneiro, Løken and Salvanes, 2015). By estimating equation (1.2) for years prior to the differentiated voucher program was in place, I obtain $\hat{\theta}_{pre} \rightarrow \lambda$. Then, I obtain an estimate for $\hat{\theta}_{post} \rightarrow \lambda + \alpha$ through the estimation of (1.2) for the cohorts that enrolled once the differentiated voucher had been implemented, from 2008 on. Finally, I can estimate the effect of the reform as $\hat{\alpha}_{RD-D} = \hat{\theta}_{post} - \hat{\theta}_{pre}$.

The RD-D estimate can also be obtained by interacting a "treated" term in regression (1.2), as follows:

$$y_i = \beta + \theta Post_i + \rho Treated_i + \alpha Post_i \times Treated_i + f(B_i) + u_i \tag{1.3}$$

where $Treated_i$ is an indicator equal to one if the student belongs to the cohort that enrolled in 2007/2008 or later, $f(B_i)$ is additionally interacted with $Treated_i$, and the other variables are defined as above.

The coefficient on the interaction of the dummy for whether the student was born after the enrollment cutoff ($Post_i$) and the indicator for whether the student belongs to the cohort exposed to the program ($Treated$), $\hat{\alpha}$, estimates the intent-to-treat effect of the program.

I will focus my analysis on a 90-day window around the June 30 cutoff, with day 0 representing the last cohort "untreated". Results are robust to the use of alternative bandwidth sizes.

1.6 Complier Characteristics

The introduction of the differentiated voucher increased the number of free private schools for eligible students, as shown in Figure 1.1, and, consequently, incentivized eligible students to enroll in voucher schools accepting the differentiated voucher instead of public schools.

However, as discussed previously, there is indication that information on the differentiated voucher did not disseminate instantaneously. Information about eligibility was hard to understand or to access, since from 2010 on, the information was distributed online, what could prevent the poorest families from accessing it. This implies that less educated families may have had more trouble accessing and understanding eligibility information and therefore, have not been able to respond in their enrollment choices to the program. Additionally, as suggested by Carrasco et al. (2014), voucher schools may still have been selecting students, so not all eligible students may have faced the same school choice at time of enrollment.

This section takes into account these facts and investigates the characteristics of students who took advantage of the increased school choice to enroll in voucher instead of public schools.

Imagine the simplest case in which students can be "induced" to treatment or not, where this is denoted by the binary variable Z_i. Treatment is binary and represented by variable D_i, and X_i is an indicator for whether student i has predetermined characteristic X. Denote D_{0i} the value that D_i would have taken if $Z_i = 0$, if the student was not induced to treatment, and D_{1i} if $Z_i = 1$. Then, extending Abadie (2002), in this simple case I can compute the proportion of compliers that had characteristic X as:

$$P(X_i = 1 | D_{1i} > D_{0i}) = \frac{E[X_i D_i | Z_i = 1] - E[X_i D_i | Z_i = 0]}{E[D_i | Z_i = 1] - E[D_i | Z_i = 0]} \tag{1.4}$$

Following this logic, I can use two-stage least squares to compute the proportion of students with characteristic X that enrolled in voucher schools instead of public schools in response to the differentiated school voucher program with the following system:

$$D_i = \theta + \psi Post_i \times Treated_i + \phi Post_i + \eta Treated_i + g(B_i) + \nu_i \tag{1.5}$$

$$X_i \times D_i = \alpha + \beta Post_i + \rho Treated_i + \gamma D_i + f(B_i) + u_i \tag{1.6}$$

where D_i is an indicator variable equal to one if student i enrolled in a voucher school in 1$^{\text{st}}$ grade, $Post_i$ an indicator variable that equals one if the student was born after the enrollment cutoff, $Treated_i$ an indicator variable for whether the student belongs to the "treated" cohorts, and $g(B_i)$ and $f(B_i)$ are linear splines on distance of sixth birthday to enrollment cutoff, interacted with whether the student was born to the right or left of the cutoff and interacted again with the $Treated_i$ indicator.

The coefficient γ in equation (1.6) gives the proportion of compliers with characteristic X. In this case, students "induced" into treatment would be those born after the enrollment cutoff in the "treated" cohorts, as they faced increased school choice.

Since information disseminated slowly, more parents learned about the increase in school choice every year following the introduction of the differentiated voucher. Therefore, students who turned six after the enrollment cutoff for 2007, 2008 and 2009 had more information and were more likely to benefit from the increased school choice and enroll in voucher instead of public schools than students born before the enrollment cutoff for these years. The slow dissemination of information is consistent with the increase in the proportion of compliers over the period, as shown in Figure 1.3. Thus, I will consider students born around the 2007, 2008 and 2009 enrollment cutoffs as students in the "treated" cohorts.

For this system to identify the characteristics of compliers, the *monotonicity assumption* needs to hold. That means, all students that were exposed to the increase in school choice had to change enrollment decisions in the same direction. Therefore, all students that responded to the increase in school choice had to increase the probability of enrolling in voucher schools only.

I provide suggestive evidence that this assumption holds by looking at the effect of the introduction of the differentiated voucher program on the probability of enrolling in different types of schools in Table 1.3. First of all, results show that even though almost 50% of the population was eligible, the probability of enrolling in a public school only fell in around 1.5%. This amount is the percentage of compliers in the population.

Regarding the monotonicity assumption, results show a significant decrease in the probability of enrolling in public schools in the post-policy cohort with respect to the pre-policy cohort and a corresponding increase in the probability of enrolling in voucher schools. These two coefficients are significant and very similar, indicating that almost all students that did not enroll in public schools enrolled in voucher schools instead. Row (C) shows no significant changes in the probability of enrolling in elite private schools, which additionally suggests that students were not exiting voucher schools to avoid differentiated voucher users. These results give support to the monotonicity assumption and suggest that the system of equations (1.5) and (1.6) should identify the characteristics of students that enroll in voucher schools instead of public schools.

Unfortunately, this approach cannot be used to estimate effects on test scores for students that switched enrollment decisions in response to the increased school choice. Since the differentiated voucher could have induced changes in schools through changes in student composition or increased funds, the *exclusion restriction* does not hold in this case. This means, the introduction of the differentiated voucher could be correlated directly with test scores, independently of changes in enrollment of the individual student.

Results for the characteristics of compliers can be found in column (1) of Table 1.4. Since standard errors are very large, partly due to the small first stage on voucher enrollment, the estimated proportions of students in each group add up to slightly more than one. Regardless, about 90% of compliers belong to the group of students with mothers that completed high school education, while the remainder of compliers had mothers with basic education or less. Additional complier characteristics can be found in Table A.8 in the appendix.

When comparing these proportions to the proportions in the actual population in column (2), it can be observed that "compliers", students that switched enrollment decisions in response to the program, come overwhelmingly from the group of students with mothers that completed high school. These results suggest that, either due to information issues or to selection on the school side, compliers were the wealthiest eligible students in the school market.

As discussed above, another difficulty is that the proportion of eligible students is not stable over time, due to the change of the instrument and to gaming by citizens to gain access to other social benefits. Thus, eligibility information cannot be used to classify students.[14]

In view of the results in Table 1.4, I can use the classification of students by mother education level to define three groups of students depending on their responses to the differentiated voucher program: (1) students with mothers that completed basic education or less, who are eligible but are not highly represented in the group of compliers, (2) students with mothers that completed high school, who represent the majority of compliers and, (3) students with mothers that completed university that are mostly not eligible and in most cases were already enrolled in voucher and elite private schools.[15]

The proportion of students that belong to each group is fairly constant over time, as shown in Table 1.5. Thus, I will use these three categories of students to determine the effects of the differentiated voucher in the next section.

As can be observed in Table 1.6, all of these groups contain students in public and private schools together with compliers (as discussed for Table 1.5). However, enrollment patterns show that students with mothers that had basic education or less generally attend public schools, the group with mothers that completed high school contains students in public and private schools, together with most compliers, and the majority of students with mothers that completed university are enrolled in private and elite private schools.

[14] The instrument used to determine eligibility was introduced in 2007, and it was not administered to students who could be eligible for the differentiated voucher until 2009.

[15] Summary statistics for students in each group in the post-policy period can be found in Table A.9 in the appendix.

1.7 RESULTS

This section presents three sets of results. The first illustrates the effect on 1st grade enrollment decisions of the increase in school choice caused by the introduction of the differentiated voucher program. The second looks at changes in school characteristics encountered by students. Finally, the third examines the effects of the differentiated voucher on standardized test scores.

Results are presented in two different ways, a figure and a table. First of all, differences in individual cohort RD estimates with respect to RD estimates for the last pre-policy cohort (2006/2007) and their 95% confidence intervals are presented for all three groups of mother education in a figure. Then, RD estimates pooling all pre-policy cohorts and all post-policy cohorts and differences in these RD estimates are presented in a table.

In the figure, each one of the differences in RD coefficients represents the increase/decrease in the outcome of interest for students who were born after the enrollment cutoff for that individual cohort once possible stable confounders, such as "age-at-test" effects, are taken into account. Difference in RD coefficients for cohorts that enrolled in 2007/2008, and subsequent cohorts, with respect to the RD coefficient for 2006/2007 estimate the effect of the differentiated voucher once the confounder has been accounted for. Therefore, if the difference in RD coefficient for the cohort that enrolled in 2007/2008 is positive and significant, that means that the effect on the outcome of the differentiated voucher is positive.

With regard to the table, the corresponding table presents RD coefficients of the outcome of interest and differences in the RD coefficients for all three groups of mother education. Column (1) presents estimates of the confounders, that means, RD coefficients pooling all pre-policy cohorts (students within 90 days of the enrollment cutoff for years 2005 and 2006), column (2) RD coefficients using all post-policy cohorts (students within 90 days of the enrollment cutoff for years 2007, 2008 and 2009) and columns (3) shows the RD-D estimates, the difference between the post-policy RD estimate with respect to the pre-policy RD estimate.

As mentioned in the empirical strategy section, all the reported differences in regression discontinuity estimates are differences in intent-to-treat (ITT) estimates, since the probability of enrolling in the next year does not sharply increase from 0 to 1 for students born after the enrollment cutoff. This probability, that would correspond to the first stage in a fuzzy regression discontinuity design, increases by around 0.4 for students born after the enrollment cutoff. Therefore, if we were interested in the average treatment on the treated (ATT) effects, these results would be roughly similar to the presented estimates multiplied by one over 0.4, that means, by 2.5.

1.7.1 Enrollment choices

One of the immediate effects of the differentiated voucher program was an increase in the number of free voucher schools for eligible students in 2008, shown in Figure 1.1. However, as discussed above, due to informational problems, enrollment responses are likely to have happened also in later years, as parents slowly learned about the differentiated voucher program, eligibility and school participation.

In response to the increase in school choice, Figure 1.4 and Table 1.7 show that there was a decrease in the probability of enrolling in public schools with respect to the pre-policy cohorts for students with mothers that had basic education or less and students with mothers that completed high school education of around 1.6%. Given the magnitude of the program, this result is perhaps surprising: even though around 50% of the population was eligible, only around 1.5% of all students switched enrollment decisions in response to the differentiated voucher program in the first three years of the program. However, as the differentiated voucher was a reform to the existing voucher system, it is possible that most eligible students with a strong preference for voucher schools were already enrolling in voucher schools.

Additionally, this fall was significant only for students with mothers that completed high school, which is expected, 90% of students that switched enrollment decisions belonged to this group. Students with mothers that completed high school were more likely to respond in their enrollment decisions than students with less educated mothers for two reasons: (1) better understanding and access to differentiated voucher program and eligibility information, and (2) student selection by voucher schools, even though it was forbidden, as argued by Carrasco et al. (2014).

For this group, the fall in enrollment in public schools already occurred in the first year of the program, but enrollment in public schools fell at about the same rate in the following year and even more rapidly two years after the introduction of the differentiated voucher. This persistent effect in the decrease in enrollment in public schools is consistent with the slow distribution of information over time.

Students that switched enrollment decisions in response to the differentiated voucher program may have enrolled in schools with different characteristics. Using a parallel technique to the previous section, I instrument enrollment in voucher and public schools, in a two-stage least squares system, to compare public schools where students would have enrolled in the absence of the differentiated voucher program (counterfactual public schools) to voucher schools where students actually enrolled (destination voucher schools) at baseline.

Formally, I run the following two-stage least squares system:

$$D_{it} = \theta + \psi Post_i \times Treated_i + \phi Post_i + \eta Treated_i + g(B_i) + \nu_i \tag{1.7}$$

$$S_i \times D_{it} = \alpha + \beta Post_i + \rho Treated_i + \gamma D_{it} + f(B_i) + u_i \tag{1.8}$$

where $D_i t$ is the probability of enrolling in school of type t, S_i is the school characteristic of interest, and $Post_i$, $Treated_i$ and $g(B_i)$ and $f(B_i)$ are as defined above. The coefficient γ in equation (1.8) gives the average of characteristic S for voucher schools where compliers enrolled or for counterfactual public schools, public schools where compliers ceased to enroll.

This method cannot be used to look at current school characteristics since the *exclusion restriction* may not hold for current characteristics. If schools experience changes in characteristics in response to compliers leaving public schools and enrolling in voucher schools, to the increased funds or to more school competition, then the introduction of the differentiated voucher program would be directly correlated with school characteristics. Thus, the two stage least squares system would not only be estimating the changes in school characteristics due to changes in complier choices.

Table 1.8 shows that, at baseline, students that enrolled in voucher schools instead of public schools thanks to the differentiated voucher, enrolled in smaller voucher schools that had, on average, class sizes about five fewer students. These schools had higher test scores and better socioeconomic composition, with a larger average socioeconomic index, about 20% fewer students with less educated mothers, and around 13% more students with highly educated mothers. Schools where students enrolled, however, had around 10% fewer teachers with a completed degree in education with respect to the public schools where they would have enrolled, and teachers had on average around five years less experience and were approximately ten years younger. Finally, schools where students enrolled had higher teacher per student ratios at baseline. Therefore, excluding teacher characteristics, due to the introduction of the differentiated voucher, compliers enrolled in higher quality voucher schools at baseline, in terms of peers and test scores, with respect to the public schools they would have enrolled if the differentiated voucher had not been introduced.

It is possible however, that even though compliers, students that changed their enrollment decisions, enrolled in higher quality schools at baseline, the schools where they actually enrolled were low quality voucher schools. This is analyzed in Table 1.9, that shows information on the position at baseline of public schools where students would have enrolled and voucher schools where students did enroll in the distribution of some characteristics relative to the other schools of the same type in the municipality.

In the absence of the differentiated voucher, compliers would have enrolled in public schools with large class sizes and enrollment with respect to other public schools in the market. Thanks to the increase in school choice, they ended up enrolling in voucher schools with small class sizes and enrollment with respect to voucher schools. These suggest that compliers may have stopped enrolling in large public schools to enroll in small and undersubscribed voucher schools.

With regard to school quality at baseline, both counterfactual public schools and destination voucher schools had test scores below the median for their type in the municipality. This means that compliers enrolled less in low quality public schools to enroll more in low quality voucher schools. However, even though the counterfactual public schools and destination voucher schools were low quality with respect to other schools, compliers enrolled in voucher schools with higher test scores at baseline with respect to counterfactual public schools, as discussed for Table 1.8.

Socioeconomic characteristics of counterfactual public schools and destination voucher schools are not particularly different from socioeconomic characteristics of other public and voucher schools in the market. But these schools do look different in terms of teacher education since both counterfactual public schools and destination voucher schools, had a lower percentage of teachers with a degree in education with respect to other schools of the same type. Finally, voucher schools where compliers enrolled were “cheap” voucher schools, since they were in the bottom half of the distribution of copays in the area.

In conclusion, the increase in school choice caused by the introduction of the differentiated voucher program reduced public school enrollment by around 1.6%. Students switching enrollment decisions enrolled in voucher schools with better socioeconomic characteristics and test scores at baseline with respect to public schools where they would have enrolled in the absence of the program. However, they decided not to enroll in low achieving public schools to enroll in undersubscribed, low-copay, low achieving voucher schools.

1.7.2 School characteristics

The previous section discussed changes in enrollment decisions due to the increased school choice caused by the differentiated voucher program, and changes in school characteristics at baseline due to the decision to enroll in a voucher school instead of a public school. Since it is possible that voucher and public schools experience changes in their characteristics due to compliers changing enrollment decisions, to the increased school resources or to increased competition, this section looks at the current school characteristics that students encountered in the year they enrolled. Changes in school characteristics caused by enrollment responses of compliers, however, are likely to be small, as only about 1.6 percent of students in the lowest groups of mother education responded to the policy.

Unfortunately, the differentiated voucher program is likely to have caused a direct effect on current school characteristics, violating the *exclusion restriction*. Therefore, it is not possible to characterize counterfactual public schools and destination voucher schools using two stage least squares, as in the previous section.

For this reason, this section analyzes changes in school characteristics on average for each of the three groups of students by mother education, pooling together students enrolled in public schools, students enrolled in voucher schools and students that switched enrollment in response to the differentiated voucher.

However, given the enrollment patterns in each of the three groups (shown in table 1.6) and since 90% of compliers belong to the group of students with mothers that completed high school education, students with mothers with less than high school will generally remain in public schools, students with mothers that completed high school include most compliers and students in voucher and public schools, and students with mothers that completed university include students that were usually already enrolled in voucher or elite private schools.

Since the proportion of students that switched enrollment decisions was small, around 1.6%, on average there were no significant changes in average socioeconomic index in schools where students of each group enrolled in 1st grade, as shown in Figure 1.6 and Table 1.11. Public schools where compliers would have enrolled in the absence of the program were barely affected, as were voucher schools where compliers did actually enroll. Table 1.8 showed an improvement in socioeconomic characteristics of schools at baseline for compliers, but since they are so few, these effects are not visible on average, and are probably cancelled out with the very light decrease in socioeconomic characteristics of voucher schools where they enrolled.

With regard to class size, Table 1.8 showed that compliers enrolled in voucher schools that had on average class sizes that were about 5 students smaller at baseline. Again, since they were so few, it is unlikely that they affected class sizes in voucher schools where they enrolled or in public schools where they would have enrolled.

Figure 1.6 and Table 1.11 look at the average effects on current average class size for students in each of the three groups of mother education. Average class size decreased, but insignificantly, for students with mothers that had basic education. These students were primarily enrolled in public schools, where class size fell somewhat, as discussed in the descriptive statistics, but not significantly. Students with mothers that completed high school experienced significant decreases in class size and found, on average, classes that were around half a student smaller. This fall in class size is due to the enrollment by compliers in schools with smaller class sizes, together with the very small decrease in class sizes for students in this group left behind in public schools. Finally, students whose mother completed university education did not experience changes in class size.

Another large difference in characteristics at baseline between counterfactual public schools and voucher schools where compliers actually enrolled were teacher characteristics. Particularly, teachers in voucher schools where compliers enrolled had, on average, five years less experience than teachers in public schools where these students would have enrolled. Consistently, panel B in figure 1.7 and row (C) in Table 1.12 show a significant decrease in teacher years of experience for students with mothers that completed high school, the group that includes most compliers.

Unexpectedly, there is also a fall in teacher years of experience for students with uneducated mothers, which is larger than for students with mothers that completed high school. This suggests that, additionally, there was a decrease in average teacher experience in public schools in 2008, as was already visible in the summary statistics Table 1.2.

This seems related to a replacement of highly experienced teachers, teachers with, on average, 25 years of teaching experience, with less experienced teachers, that had, on average, 10 years of teaching experience, particularly in public schools, as suggested by Figure 1.8 and Table 1.13, and the summary statistics Table 1.2. Figure 1.8 and Table 1.13 illustrate increased hiring of teachers in the first year of the program particularly in schools where most students with uneducated mothers were enrolled. When considering the whole treatment period, this effect is significant and largest for students with mothers that completed high school, since descriptive statistics in Table 1.2 suggest increased hiring in 2009 and 2010 in voucher schools.

This is weak evidence that there were responses on the school side and that schools where these students were enrolling responded to the increased competition or to the introduction of achievement goals by restructuring their teaching teams.

To summarize, on average there were no changes in peer characteristics, but students with mothers that completed high school had smaller class sizes and less experienced teachers. Students with uneducated mothers also had a decrease in average teacher years of experience, which seems related to responses to the differentiated voucher program on the school side, particularly from public schools, and to a replacement of highly experienced teachers by less experienced teachers.

1.7.3 Test scores

Figure 1.9 and Table 1.14 show the effect on average 4th grade test scores in a standardized exam for students in each group of mother education. Test scores for students with uneducated mothers that enrolled in 1st grade in 2008 and later, and took the standardized exam in 2011 or later, increased significantly, by around 0.08 standard deviations. These students are the ones that generally stayed in public schools and that had a decrease in average years of teaching experience in their schools as a result of changes in teaching teams. This result suggests that students with uneducated mothers could have benefited from the increased perceived competition by public schools, and that the introduction of less experienced teachers could be a possible channel through which public schools were responding.

With regard to students with mothers that completed high school education, on average there was a small negative effect on test scores caused by a decrease for the last post-policy cohort included in the sample. However, this result is not robust and Figure 1.9 shows no significant changes in test scores for the first two cohorts affected by the policy. These suggest that there were no significant effects on average test scores for students with mothers that completed high school. This group includes most students that switched enrollment decisions in response to the differentiated voucher. These students enrolled in schools with better peer characteristics and achievement at baseline, and lower class sizes, and younger and less experienced teachers. Nevertheless, they decided not to enroll in low achievement public schools to enroll in low achievement voucher schools. Consequently, the improvement in observable characteristics of schools may not have been sufficient to increase test scores for these students. Additionally, as compliers were so few, it is possible that their test scores increased or decreased somewhat but that these effects are not large enough to be visible on average for the group of students with mothers that completed high school education.

In the case of students in this group already enrolled in voucher schools, changes in school characteristics were very small, given the proportion of compliers, so their test scores may have stayed constant. Finally, regarding students in this group enrolled in public schools, even though socioeconomic characteristics in these schools could not significantly change, they did experience a change in teaching teams. Since their initial test scores were already higher than for students with uneducated mothers, it is likely that changes in test scores were smaller. However, if we believe that there may have been some positive effect on test scores for that group, that would imply that there was some small decrease in test scores for compliers or for students that were already enrolled in voucher schools, as summary statistics in Table 1.2 suggest. Nevertheless, none of these possible effects on test scores is sufficiently large to lead to significant effects on test scores, therefore, there were no observable effects on test scores for students with mothers that completed high school education.

Finally, test scores for students with highly educated mothers did not change either. In this case this is expected, since students in this group did not have any significant change in school characteristics or in enrollment decisions.

In short, there were no obvious direct effects on test scores for students that took advantage of the increased school choice and enrolled in voucher schools instead of public schools. In contrast, there were positive effects on test scores for the poorest students, those with uneducated mothers, that could be related to the change in teaching teams in public schools that took place in 2008. Therefore, on aggregate, the effects on test scores were positive but very small and insignificant, and led by responses to the program by public schools instead of by re-sorting of students.

1.8 Robustness Checks

This section presents results that address possible concerns regarding the main specification used in this paper.

Table A.3 in the Appendix shows results dividing the population of students according to the tercile of the municipality socioeconomic index distribution they belong to instead of mother education. These results are comparable in terms of significance, sign and magnitude, to the results using mother education level discussed throughout the paper, suggesting that the classification of students does not affect the main conclusions of this paper.

As discussed in the Complier Characteristics section, there were delayed enrollment responses to the differentiated voucher policy, caused mostly by the lack of information that families received. For this reason, most of the tables presented pool together three pre-policy cohorts and two post-policy cohorts. Table A.4 in the Appendix addresses possible concerns about biases caused by the use of several cohorts by presenting results for the first post-policy cohort compared to the last pre-policy cohort only. This table uses the first post-policy cohort as the only treated cohort and presents results comparing RD coefficients for the first post-policy cohort (students born around the 2007 enrollment cutoff) to RD coefficients for the last pre-policy cohort (students born around the 2006 enrollment cutoff). Results for this specification are very similar to the results of the main specification, suggesting that the effects of the policy net of possible school responses are comparable to the effects of the policy when considering delayed effects.

Tables A.5 and A.6 in the appendix deal with the fact that there are enrollment cutoffs in Chile other that the main June 30 cutoff used in this paper. As discussed in the Empirical Strategy section, the official enrollment cutoff was on March 31, with schools being able to set enrollment cutoffs as late as June 30. As shown by McEwan and Shapiro (2008), the cutoff with a largest enrollment discontinuity was June 30, but there were other smaller enrollment discontinuities on March 31, April 30 and May 31. Table A.5 presents the main results including only students that were turning six years of age within 30 days of the June 30 enrollment cutoff, including only this enrollment cutoff. Since this bandwidth reduces sample size to a third, there is a loss of significance for most results. However, the signs and magnitudes of the coefficients are similar to those of the main results, suggesting that results are robust to using a smaller bandwidth. Alternatively, the specification in Table A.6 allows the linear spline to change between each enrollment cutoff. Results from this specification are, again, comparable to the main results. These two sets of results suggest that the results obtained from the main specification are not affected by the inclusion of other smaller enrollment cutoffs.

Finally, Table A.7 addresses the assumption that the confounders are constant over time and, thus, the differences in RD get rid of them. This specification allows confounders to follow a linear trend over time. Results show that fitting confounders in a liner trend instead of assuming that they are constant over time does not affect the main results, as results in Table A.7 are comparable in sign, magnitude and significance to the main results.

1.9 Conclusion

Even though the effects of school choice on educational outcomes have been broadly studied, particularly for students enrolling in better schools, there is still limited evidence on the effects of large-scale programs. Large-scale programs could affect the sorting of students, or induce responses on the school side to increases in competitive pressure, leading to different effects on achievement for students switching enrollment, and affecting all students in the market. For that very reason, it is generally difficult to measure the effects of large-scale school choice programs, since there is no group of students unaffected by the program.

The literature has generally looked at aggregate effects of such programs (e.g., Hsieh and Urquiola), or used structural estimation (e.g., Neilson, 2013) to determine the effects on educational outcomes. In this paper, I combine two sources of variation that allow me to identify: (1) the proportion and characteristics of students switching enrollment decisions in response to an increase in school choice caused by a large-scale program, (2) the characteristics at baseline of schools where students switching enrollment decisions enrolled and the characteristics of schools where they would have enrolled in the absence of the policy and (3) changes in enrollment decisions, current school characteristics and test scores for all students in the population, divided in groups that were affected differently by the program.

My first result is that, despite the magnitude of the program, the impact on re-sorting of students was small. Only a small fraction of students switched enrollment decisions. However, this program was a reform to the existing voucher system in Chile, therefore, it is likely that most of the re-sorting had already occurred before the policy.

Additionally, students that switched enrollment decisions were among the wealthiest eligible students, which is consistent with "cream-skimming" in the presence of selection by private schools and with imperfect information, especially for the poorest families. Both seem to be the case in Chile. Therefore, a small implication is that for such a policy to re-sort the poorest students, information should be clear, with all population understanding the program and their eligibility status, and non-selection by private schools should be enforced.

My second result shows that compliers, students switching enrollment decisions, enrolled in schools that had better observable characteristics at baseline than schools where they would have enrolled in the absence of the policy. Regarding current characteristics, schools where the group of students that included most compliers enrolled had smaller class sizes and less experienced teachers (in most cases with at least five years of teaching experience). Despite this improvement in school characteristics, there are no effects on average test scores for the group of students with most compliers. This result would be consistent with either small positive or negative effects on average test scores for compliers, and is in line with the results of some literature on the effects of attending a better school (e.g., Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2014). Additionally, this result fits well with Neilson's (2013) result that most of the effect on test scores of the differentiated school voucher in Chile came through indirect channels, instead of through re-sorting of students.

Finally, a third, perhaps surprising result is that test scores increased for the group of students most likely to stay in public schools. This result could be due to an increase in competitive pressure, as argued by Neilson (2013), and a subsequent increase in quality in public schools (e.g., Figlio and Hart, 2014). My analysis uncovers one potential channel through which public schools could be responding: there is an increase in hiring in public schools in the first year of the program that resulted in lower average experience of teachers in public schools.

Further research is needed to understand what were the actual underlying mechanisms behind the increase in test scores in public schools. Understanding the reasons why public schools succeeded in improving achievement is essential to design strategies to increase learning in all school types.

Overall, the effect of the differentiated voucher program on test scores was positive, but small and not significant. However, it did increase test scores for the poorest students in the population, closing the test score gap. This policy increased public expenditure in education by 8%, close to 0.3% of Chile's GDP. An open question is whether the same test score effects could have been achieved through increased resources and monitoring of public schools, instead of an increase in the school voucher, which would have reduced the cost of the policy.

Even though the particular effects of the program may be specific to Chile, there is a conclusion that can be generalized. Public schools seem to have responded to the increase in potential competition. Consequently, the differentiated voucher program was successful in increasing achievement in public schools, despite the small effects on re-sorting of students. Therefore, education policies may have effects beyond those for targeted students. These general equilibrium effects need to be taken into account when designing policies.

TABLES AND FIGURES

Figure 1.1: Changes in school choice: Proportion of voucher schools that are free for eligible students

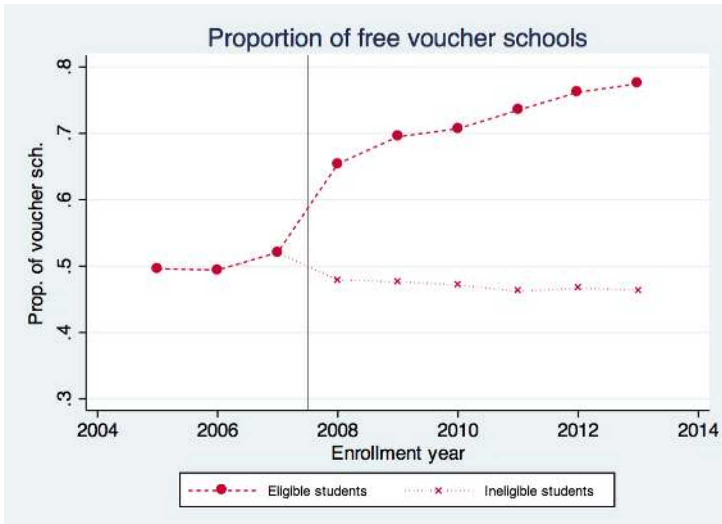


Table 1.1: Sample summary statistics by enrollment year

School year	2005	2006	2007	2008	2009	2010
Total students in 1st grade	260,724	251,602	256,040	248,921	247,575	241,203
Enrolled in public schools	0.488	0.468	0.450	0.430	0.416	0.397
Enrolled in voucher schools	0.447	0.465	0.482	0.499	0.513	0.528
Average 4th grade standardized score	0.005	0.062	0.145	0.167	0.206	0.175
Total number of schools	8,771	8,697	8,680	8,685	8,665	8,598
Proportion of public schools	0.606	0.596	0.589	0.583	0.576	0.570
Proportion of voucher schools	0.343	0.355	0.362	0.368	0.374	0.381
Proportion of voucher participating schools	0	0	0	0.483	0.581	0.612

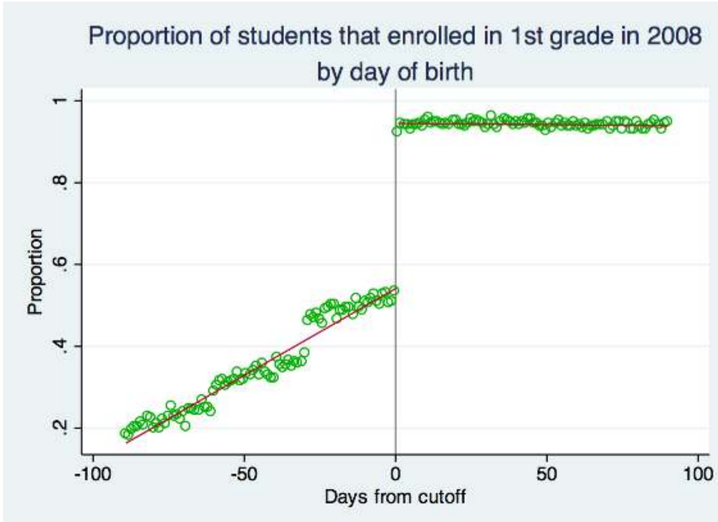
Notes: The chart above lists the descriptive statistics for the sample of students enrolling in first grade in each year. Test scores are comparable over time and normalized with respect to test scores in 2005.

Table 1.2: School summary statistics by enrollment year

School year	2005	2006	2007	2008	2009	2010
Panel A: Public schools						
Number of students in class (1^{st})	30.5	30.0	30.0	29.3	28.8	28.0
Number of teachers per student	0.043	0.044	0.046	0.047	0.049	0.052
Number of students in school (1^{st}-4^{th})	271.4	238.7	223.5	220.2	208.4	201.6
Average school socioeconomic index (standard dev.=100)	-44.2	-43.3	-45.3	-48.2	-47.6	-51.2
Proportion of mothers with university ed. (1^{st})	0.028	0.031	0.032	0.035	0.037	0.038
Proportion of mothers with basic ed. or less (1^{st})	0.345	0.332	0.341	0.355	0.345	0.321
Teachers with a degree in education (%)	95.6	96.1	96.0	96.8	97.1	97.6
Teaching experience in years	22.9	23.4	23.9	21.8	20.8	20.7
Teacher age	49.2	49.3	49.7	48.1	47.5	47.4
Proportion of teachers hired on year	0.087	0.097	0.080	0.145	0.147	0.133
Proportion of teachers terminated on previous year	0.076	0.104	0.081	0.170	0.130	0.126
Average teacher evaluation scores	2.556	2.581	2.598	2.648	2.621	2.592
Male teachers	0.265	0.265	0.261	0.268	0.268	0.27
Per student expenditure of extra voucher funds (1^{st})	0.0	0.0	0.0	34.9	222.1	407.6
Standardized 4^{th} grade average scores	-0.251	-0.189	-0.091	-0.054	-0.016	-0.055
Panel B: Voucher schools						
Number of students in class (1^{st})	34.4	34.3	34.8	34.6	34.3	34.2
Number of teachers per student	0.04	0.04	0.041	0.041	0.042	0.043
Number of students in school (1^{st}-4^{th})	323.5	285.9	272.2	272.7	257.7	274.9
Average school socioeconomic index (standard dev.=100)	16.4	17.4	16.0	11.5	11.0	6.8
Proportion of mothers with university ed. (1^{st})	0.091	0.095	0.101	0.118	0.118	0.125
Proportion of mothers with basic ed. or less (1^{st})	0.146	0.139	0.143	0.154	0.152	0.138
Teachers with a degree in education (%)	90.8	92.2	91.7	94.2	95.3	96.2
Teaching experience in years	13.4	13.7	13.5	13.5	13.3	13.2
Teacher age	41.6	41.4	41.2	41.2	41.0	40.7
Proportion of teachers hired on year	0.162	0.174	0.176	0.177	0.191	0.196
Proportion of teachers terminated on previous year	0.101	0.126	0.131	0.139	0.140	0.144
Male teachers	0.253	0.248	0.246	0.242	0.241	0.239
Per student expenditure of extra voucher funds (1^{st})	0.0	0.0	0.0	36.0	180.5	300.2
Standardized 4^{th} grade average scores	0.137	0.185	0.251	0.234	0.269	0.223
Panel C: Elite private schools						
Number of students in class ($1^{s}t$)	26.1	26.2	26.4	26.4	26.0	26.2
Number of teachers per student	0.072	0.071	0.071	0.071	0.07	0.07
Number of students in school (1^{st}-4^{th})	283.5	253.0	242.9	260.1	232.9	253.0
Average school socioeconomic index (standard dev.=100)	202.0	201.3	197.9	191.3	191.1	181.5
Proportion of mothers with university ed. (1^{st})	0.507	0.45	0.493	0.625	0.645	0.666
Proportion of mothers with basic ed. or less (1^{st})	0.002	0.002	0.002	0.002	0.003	0.005
Teachers with a degree in education (%)	94.7	94.7	94.7	95.2	95.9	96.2
Teaching experience in years	14.1	14.6	14.7	14.8	15.0	15.1
Teacher age	42.0	42.0	42.0	42.1	42.2	42.1
Proportion of teachers hired on year	0.177	0.138	0.134	0.137	0.133	0.128
Proportion of teachers terminated on previous year	0.120	0.115	0.121	0.120	0.116	0.101
Male teachers	0.226	0.223	0.217	0.228	0.228	0.225
Per student expenditure of extra voucher funds (1^{st})	0.00	0.00	0.00	0.00	0.00	0.00
Standardized 4^{th} grade average scores	0.951	0.945	0.925	0.891	0.903	0.852

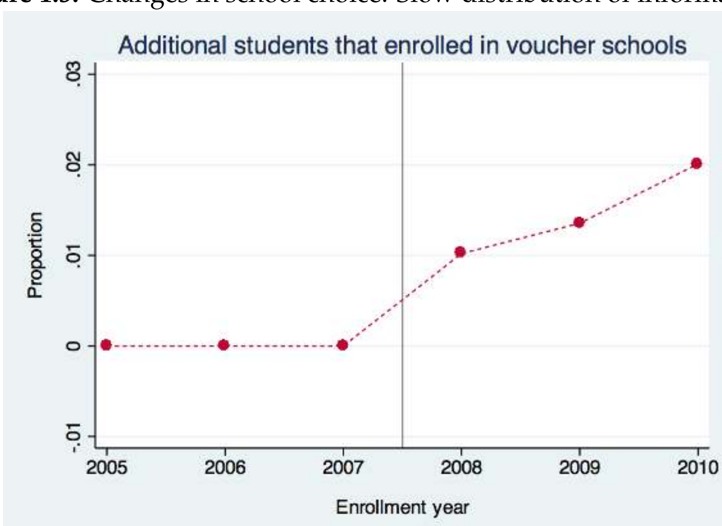
Notes: The chart above lists the descriptive statistics of average school characteristics for each school type and year. Average test scores are shown by enrollment year in 1^{st} grade. They are comparable over time and normalized with respect to test scores in 2005.

Figure 1.2: Probability of enrolling in the next academic year by distance from the date of birth to the June 30th enrollment cutoff



Notes: Each circle above represents the proportion of students that turned six years of age on that day and enrolled in 2008 instead of 2007 within 90 days of the June 30, 2007 enrollment cutoff.

Figure 1.3: Changes in school choice: Slow distribution of information



Notes: This graph represents the proportion of students that enrolled in voucher schools instead of public schools in response to the introduction of the differentiated voucher. Each observation represents the RD coefficient of the probability of enrolling in a voucher school for students born after the enrollment cutoff using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff.

Table 1.3: Check of the monotonicity assumption - Probability of enrolling in each type of school (All students)

	Difference in ITTs
	(1) Post-Pre
(A) Voucher schools	0.0143***
	(0.0052)
(B) Public schools	-0.0146***
	(0.0052)
(C) Elite private schools	0.0004
	(0.0027)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows the differences in the RD coefficients for the post-policy period (2008-2010) with respect to the pre-policy period (2005-2007) including a full set of cohort dummies.

Table 1.4: Complier characteristics - Proportion of compliers with each level of mother education (post-policy period)

	(1) Compliers	(2) Population post-policy period
Mother has basic education or less	0.145	0.22
	(0.228)	[0.41]
Mother has high school education	0.889	0.65
	(0.295)	[0.48]
Mother has university education	0.047	0.13
	(0.211)	[0.34]
Average socioeconomic index	-46.82	0.27
	(65.49)	[99.8]

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses and the standard deviations are in brackets. Each coefficient represents a separate regression. Enrollment in voucher school is instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows the complier characteristics including all years in the sample and a full set of cohort dummies. Column (2) shows the proportion of students in each category in the population for the post-policy years (2008-2010).

Table 1.5: Proportion of students by mother's education and year

	2005	2006	2007	2008	2009	2010
Mothers with basic education or less	0.26	0.24	0.23	0.23	0.22	0.19
Mothers with high school education	0.63	0.64	0.64	0.64	0.65	0.66
Mothers with university education or more	0.11	0.12	0.12	0.13	0.13	0.14
Average years of mother education	11.30	11.50	11.55	11.59	11.55	11.89
Number of observations	260,724	251,602	256,040	248,921	247,575	241,203

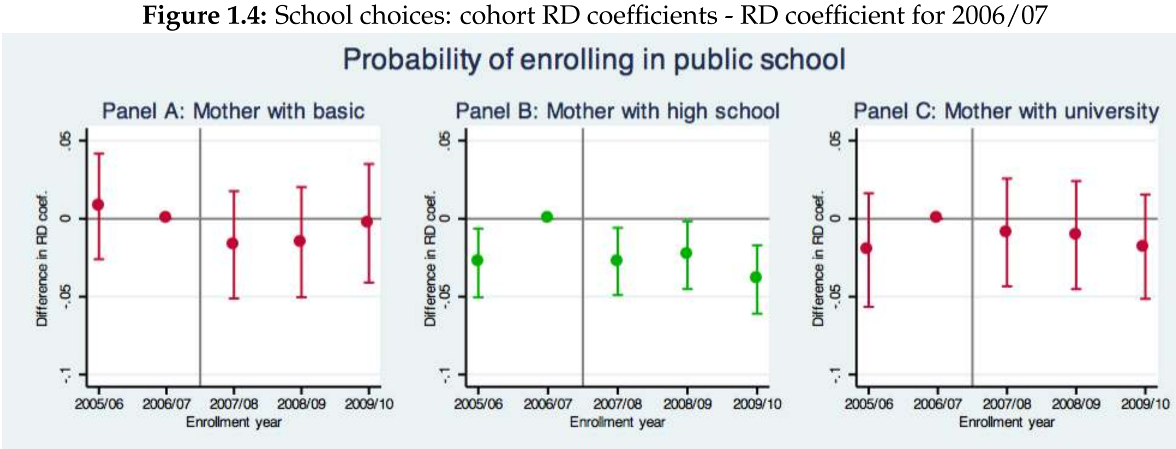
Notes: Includes students in 1st grade.

Table 1.6: Enrollment patterns by mother's education and year

	2004	2005	2006	2007	2008	2009	2010
Panel A: Mothers with basic education or less							
Public schools	0.71	0.71	0.68	0.68	0.65	0.65	0.64
Voucher schools	0.29	0.29	0.32	0.32	0.35	0.35	0.36
Elite private schools	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Panel B: Mothers with high school education							
Public schools	0.45	0.44	0.42	0.41	0.39	0.38	0.36
Voucher schools	0.50	0.52	0.54	0.55	0.57	0.58	0.60
Elite private schools	0.05	0.04	0.04	0.04	0.04	0.04	0.04
Panel C: Mothers with university education							
Public schools	0.17	0.16	0.16	0.14	0.14	0.13	0.12
Voucher schools	0.46	0.48	0.50	0.50	0.52	0.52	0.52
Elite private schools	0.37	0.36	0.34	0.36	0.35	0.36	0.36

Notes: Includes students in 1st grade.

Figure 1.4: School choices: cohort RD coefficients - RD coefficient for 2006/07



Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.7: School choices: Probability of enrollment in public schools by mother education level

	ITT estimates		Difference in ITTs
	(1)	(2)	(3)
	Pre-policy	Post-policy	Post-Pre
(A) All students	-0.0123***	-0.0270***	-0.0146***
	(0.0040)	(0.0033)	(0.0052)
(B) Mother with basic education or less	0.0018	-0.0137*	-0.0160
	(0.0086)	(0.0078)	(0.0116)
(C) Mother with high school education	-0.0118**	-0.0271***	-0.0156**
	(0.0056)	(0.0045)	(0.0072)
(D) Mother with university education	-0.0107	-0.0133*	-0.0025
	(0.0093)	(0.0068)	(0.0115)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Table 1.8: School characteristics at baseline for compliers

	(1) Counterfactual public	(2) Destination voucher	(3) Difference
Class size	34.50	29.55	-4.95
	(4.20)	(3.90)	
Total enrollment	337.1	106.6	-230.5
	(75.26)	(123.8)	
Standardized 4th grade average score	-0.427	-0.006	0.421
	(0.164)	(0.201)	
Socioeconomic index (standard dev.=100)	-33.25	34.06	67.31
	(15.70)	(24.90)	
Proportion of mothers with basic or less	0.298	0.099	-0.199
	(0.084)	(0.079)	
Proportion of mothers with university	0.025	0.157	0.132
	(0.018)	(0.051)	
Teachers with education degree (%)	97.81	88.67	-9.14
	(2.60)	(4.18)	
Average teacher experience in years	16.83	6.84	-9.99
	(1.05)	(1.78)	
Average teacher age	46.22	35.96	-10.26
	(2.31)	(1.87)	
Average teacher per student	0.031	0.062	0.031
	(0.009)	(0.011)	

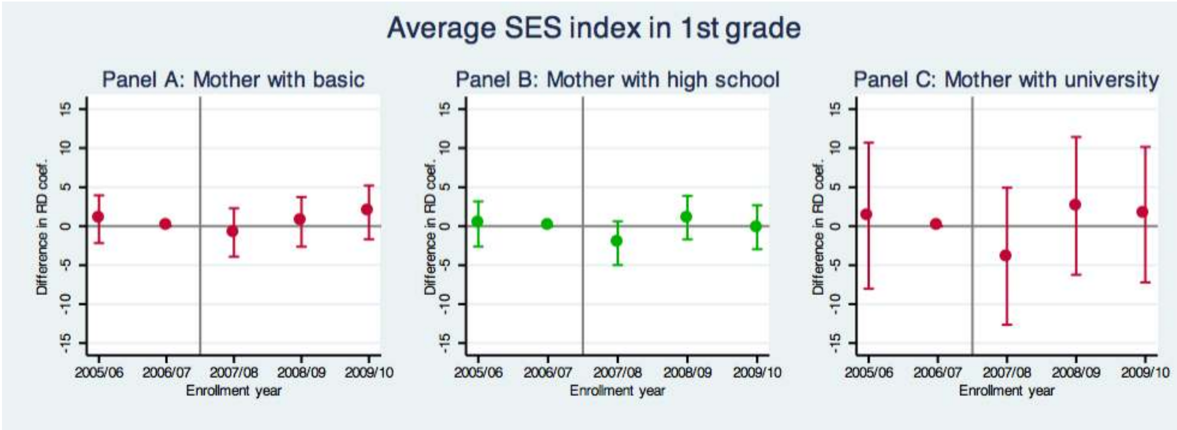
Notes: Robust standard errors are in parentheses. Each coefficient represents a separate regression. Enrollment in voucher and public school are instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts. The regressions above use a window of 90 days, include all years and a full set of cohort dummies. They also include a linear spline of distance from birthday to enrollment cutoff.

Table 1.9: School characteristics at baseline for compliers with respect to schools of the same type in the municipality

	(1) Counterfactual public schools	(2) Destination voucher schools
(A) Class size		
Below the median	0.372	0.811
	(0.209)	(0.217)
Above the median	0.628	0.189
	(0.209)	(0.217)
(B) Total enrollment		
Below the median	0.317	0.751
	(0.214)	(0.214)
Above the median	0.683	0.249
	(0.214)	(0.214)
(C) Standardized 4th grade average score		
Below the median	0.723	0.806
	(0.200)	(0.213)
Above the median	0.277	0.194
	(0.200)	(0.213)
(D) Socioeconomic index (%)		
Below the median	0.554	0.565
	(0.197)	(0.203)
Above the median	0.446	0.435
	(0.197)	(0.203)
(E) Proportion of mothers with basic or less		
Below the median	0.465	0.606
	(0.201)	(0.206)
Above the median	0.535	0.394
	(0.201)	(0.206)
(F) Proportion of mothers with university		
Below the median	0.614	0.589
	(0.198)	(0.205)
Above the median	0.386	0.411
	(0.198)	(0.205)
(G) Teachers with education degree (%)		
Below the median	0.923	1.060
	(0.176)	(0.227)
Above the median	0.077	-0.060
	(0.176)	(0.227)
(H) Copay amount		
Below the median	-	0.858
	-	(0.213)
Above the median	-	0.142
	-	(0.213)

Notes: Robust standard errors are in parentheses. Each coefficient represents a separate regression. Enrollment in voucher and public school are instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts.The regressions above use a window of 90 days, include all years and a full set of cohort dummies. They also include a linear spline of distance from birthday to enrollment cutoff.

Figure 1.5: Peer characteristics: cohort RD coefficients - RD coefficient for 2006/07



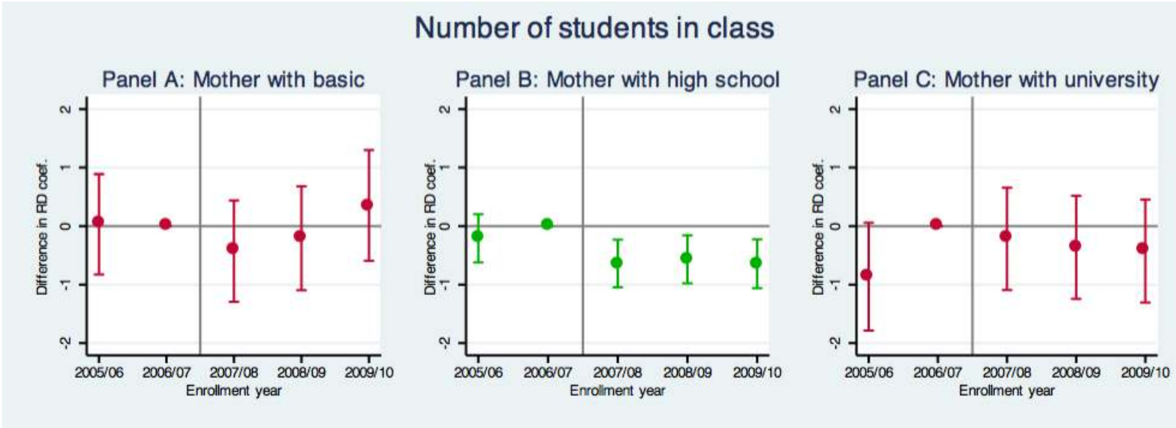
Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.10: School characteristics: Average socioeconomic index in 1st grade (normalized nationally within grade - mean 0, standard deviation 100)

	ITT estimates		Difference in ITTs
	(1) Pre-policy	(2) Post-policy	(3) Post-Pre
(A) All students	4.479***	4.380***	-0.097
	(0.639)	(0.520)	(0.824)
(B) Mother with basic education or less	1.097	1.040	-0.070
	(0.782)	(0.697)	(1.047)
(C) Mother with high school education	3.682***	3.076***	-0.587
	(0.739)	(0.574)	(0.936)
(D) Mother with university education	7.191***	6.555***	-0.626
	(2.385)	(1.748)	(2.957)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Figure 1.6: School characteristics: cohort RD coefficients - RD coefficient for 2006/07



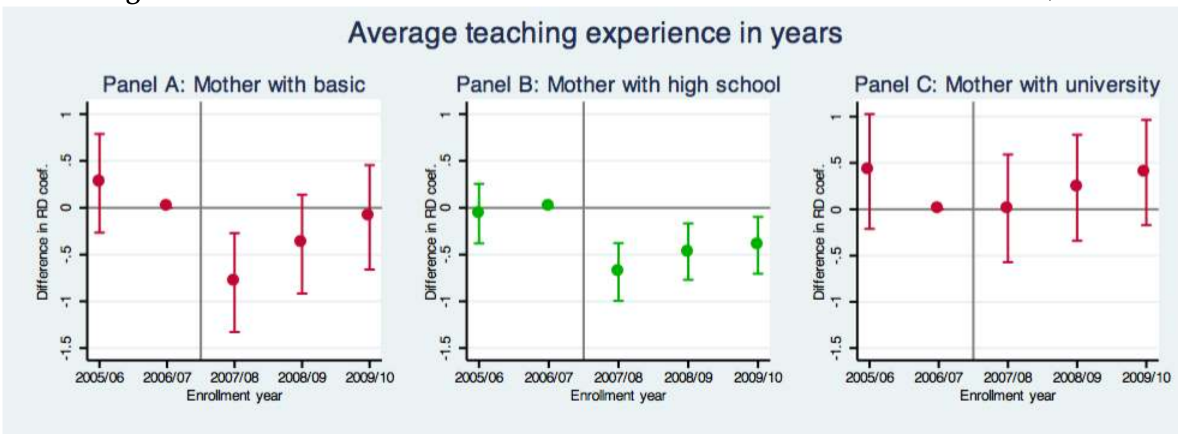
Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.11: School characteristics: Number of students in the class by mother education level

	ITT estimates		Difference in ITTs
	(1) Pre-policy	(2) Post-policy	(3) Post-Pre
(A) All students	0.471***	0.137**	-0.332***
	(0.083)	(0.068)	(0.107)
(B) Mother with basic education or less	0.472**	0.321*	-0.154
	(0.219)	(0.193)	(0.292)
(C) Mother with high school education	0.493***	-0.0317	-0.515***
	(0.105)	(0.086)	(0.136)
(D) Mother with university education	0.231	0.321*	0.091
	(0.235)	(0.179)	(0.295)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Figure 1.7: School characteristics: cohort RD coefficients - RD coefficient for 2006/07



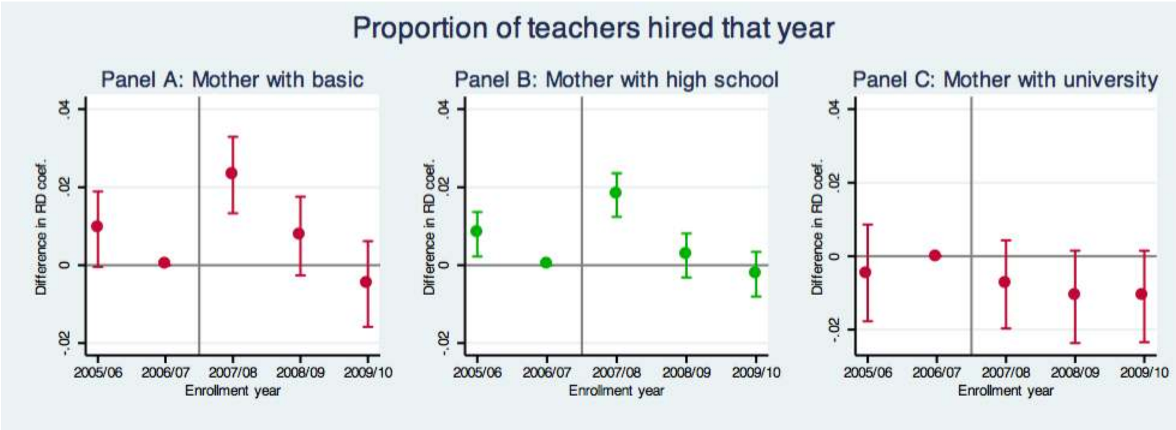
Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.12: School characteristics: Average teacher experience in years in the school by mother education level

	ITT estimates		Difference in ITTs
	(1) Pre-policy	(2) Post-policy	(3) Post-Pre
(A) All students	0.0726	-0.343***	-0.411***
	(0.058)	(0.044)	(0.073)
(B) Mother with basic education or less	0.117	-0.468***	-0.590***
	(0.134)	(0.113)	(0.175)
(C) Mother with high school education	0.143*	-0.361***	-0.494***
	(0.081)	(0.060)	(0.101)
(D) Mother with university education	0.058	0.068	0.010
	(0.158)	(0.112)	(0.194)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Figure 1.8: School characteristics: cohort RD coefficients - RD coefficient for 2006/07



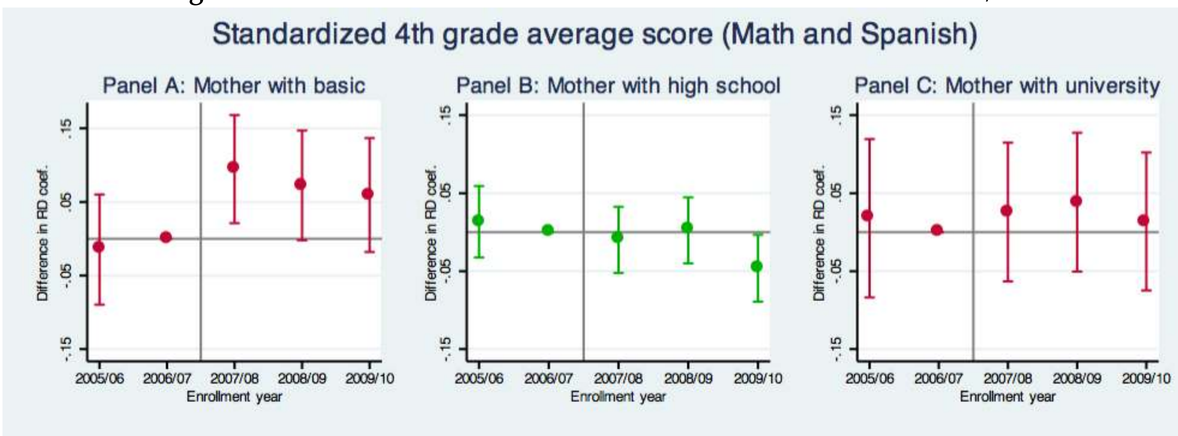
Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.13: School characteristics: Proportion of teachers hired in the current year

	ITT estimates		Difference in ITTs
	(1) Pre-policy	(2) Post-policy	(3) Post-Pre
(A) All students	0.0011	0.0034***	0.0023*
	(0.0011)	(0.0009)	(0.0014)
(B) Mother with basic education or less	0.0024	0.0075***	0.0051
	(0.0025)	(0.0023)	(0.0034)
(C) Mother with high school education	0.0015	0.0040***	0.0025
	(0.0015)	(0.0012)	(0.0019)
(D) Mother with university education	0.0043	-0.0033	-0.0076*
	(0.0034)	(0.0026)	(0.0042)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Figure 1.9: Test scores: cohort RD coefficients - RD coefficient for 2006/07



Notes: Each observation represents the difference in the RD coefficient with respect to the RD coefficient for the last pre-policy cohort (2006/07) together with its 95% confidence interval using a window of 90 days around the enrollment cutoff and including a linear spline of distance from birthday to enrollment cutoff. Each RD coefficient can be interpreted as the increase/decrease in the outcome for students born after the enrollment cutoff.

Table 1.14: Test scores: Average Math and Spanish standardized 4^{th} grade test scores

	ITT estimates		Difference in ITTs
	(1) Pre-policy	(2) Post-policy	(3) Post-Pre
(A) All students	0.199***	0.201***	0.003
	(0.009)	(0.007)	(0.012)
(B) Mother with basic education or less	0.147***	0.228***	0.084***
	(0.019)	(0.016)	(0.025)
(C) Mother with high school education	0.233***	0.205***	-0.024*
	(0.012)	(0.009)	(0.014)
(D) Mother with university education	0.100***	0.117***	0.017
	(0.026)	(0.017)	(0.031)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for years 2005 and 2006, while column (2) for years 2008, 2009 and 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Chapter 2

The Effects of Distributing School Performance Information: Evidence from the Chilean "Traffic Lights"

2.1 INTRODUCTION

In the last decade, there has been an increase in the use of student performance measures to evaluate education systems and, especially, individual school effectiveness. Most research shows a positive impact of school accountability[1] on student achievement (e.g., Rockoff and Turner, 2010; Figlio and Rouse, 2006) as schools respond to the (explicit or implicit) rewards and/or sanctions of accountability systems. For instance, if school quality information is made public, parents can reward or sanction schools through their enrollment choices (e.g., Hastings and Weinstein, 2008). Enrollment responses, however, can also directly affect future school evaluations if they lead to non-random sorting of students across schools. For this reason, it is important to understand parents' enrollment responses to school accountability in order to correctly interpret changes in school performance.

In this paper I analyze the effects on enrollment choices and student achievement of the distribution of simplified school performance information in Chile in 2010 for students in different points of the socioeconomic distribution. Specifically, the policy provided all families in Chile with a map that included all schools in their municipality color-coded in three categories depending on the previous' year school performance in a national standardized test.

I use administrative information for all the students that enroll in 1st grade and match this information to standardized test scores for 4th grade and individual socioeconomic information.

To carry out this analysis, I use two sources of variation. First, I exploit the timing of the policy: information was distributed in 2010 and could affect 2011 enrollment choices. Second, I use the fact that Chile, like many countries, has an enrollment cutoff – children that turn six years of age after June 30th must wait one year to enroll in 1st grade by law. Taken together, these facts imply that students at either side of the enrollment cutoff potentially had different levels of school performance information when they chose a school in 1st grade.

This difference in the exposure to the school performance information around the enrollment cutoff allows me to compare student enrollment choices and test scores in a regression discontinuity (RD) design. Additionally, by using the RD created by the enrollment cutoff for years prior to

[1]School accountability refers to the evaluation of schools using student performance measures.

the distribution of information, I can identify possible trends and confounders that could be biasing the RD estimates. Finally, taking differences of the RD estimates, I obtain unbiased estimates of the impact of distributing school performance information. Further, this identification strategy allows me to identify effects within groups of students at different points of the socioeconomic distribution, which makes it possible to decompose the aggregate effects of the policy on student responses and outcomes.

Since I find stronger effects in larger school markets, I focus the analysis on the largest school market in Chile: Santiago's metropolitan area. There, I perform three different exercises. First I characterize compliers – students that respond to the school performance information by switching enrollment choices. Second, I look at changes in enrollment choices for students at different points of the socioeconomic distribution, and compare the characteristics at baseline of schools where compliers enroll to schools where they would have enrolled in absence of the policy. Finally, I analyze the effects of the increased school choice on current school characteristics and test scores.

To characterize compliers, I instrument enrollment in high-performing schools with the interaction of the timing of the policy and whether a student was born after the enrollment cutoff. I find that, in response to the new information, less students enrolled in average-performing schools and more students enrolled in high-performing schools (as defined by their absolute scores). There were no changes in enrollment in low-performing schools.

In the second stage, I obtain the average characteristics of compliers. I find that most students responding to the policy belong to the third quartile of the municipality socioeconomic distribution and only around 35% of compliers belong to the bottom half of the municipality socioeconomic distribution. Since the school information included elite private schools and expensive and selective subsidized private schools, these schools were overrepresented in the high-performing category of schools. Thus, students in the bottom half of the socioeconomic distribution were less able to respond to the information by switching enrollment to high-performing schools. In contrast, most students in the top quartile of the socioeconomic distribution were already enrolled in high-performing schools. For this reason, there are no compliers that belong to the fourth quartile.

Then, using differences in RD estimates I examine the impact on enrollment decisions. Consistent with the results of the complier characterization I find a decrease in enrollment in average-performing schools and an increase in enrollment in high-performing schools, particularly for students in the top half of the municipality socioeconomic distribution and in the third quartile.

I instrument enrollment in average-performing and high-performing schools in a two-stage least squares system and find that, in response to the distribution of information, compliers enroll less in public schools and more in subsidized and elite private schools and switch enrollment towards schools with a better socioeconomic composition at baseline.

The analysis of current school characteristics shows that students in the group that contains most compliers, the third quartile of the municipality socioeconomic distribution, end up in schools that have a better socioeconomic composition and with higher segregation levels, while there are no significant changes in teacher or other school characteristics.

Finally, the differences in RD show no significant changes in Math standardized test scores but suggest a significant and relatively large increase in Spanish standardized test scores for the whole student population in Santiago and particularly for students in the third quartile of the municipality socioeconomic distribution.[2] This result, however, loses significance and magnitude when I include the whole student population in Chile. It is possible that some school market particularities in Santiago, such as, for example, a larger level of school choice, are the trigger behind this effect.

The overall increase in Spanish test scores in Santiago suggests that a low-cost policy, such as distributing simplified school performance information, could have real effects on achievement by sorting students to better-performing schools. However, this increase was particularly strong for students in the top half of the socioeconomic distribution, while scores for students in the bottom half did not significantly change. This suggests that this policy could have also induced a decrease in education equity by improving education opportunities for higher-income students while keeping those of lower-income students constant.

This paper relates to several strands of literature. First, it contributes to the literature on school accountability by analyzing student sorting responses to the availability of school performance information. School accountability has been shown to have a positive impact on student achievement (e.g., Rockoff and Turner, 2010; Figlio and Rouse, 2006), to trigger gaming behaviors by

[2] These results are consistent with Zimmerman (2003) that shows how peers' verbal scores are more strongly linked to student achievement than math peers' scores.

schools and teachers (e.g., Figlio and Getzler, 2006; Figlio, 2006; Jacob, 2005), to affect housing prices (Black, 1999; Figlio and Lucas, 2004), to affect parents' contributions to schools (Figlio and Kenny, 2009) and to modify school practices (Goldhaber et al, 2013). In this paper I show that disclosing school performance information can, additionally, induce enrollment responses and increase sorting of students across schools with, in this case, higher-income students enrolling in higher-perfomance schools.

Second, this paper adds to the literature on responses to school performance information. Literature on parents' responses to school quality information shows mixed results (e.g., Hastings and Weinstein, 2008; Andrabi, Das and Kwaja, 2015; Mizala and Urquiola, 2010) as enrollment responses are affected by how the information is presented and distributed, and by the school's ability to select students.[3] For instance, the ability of parents to respond to school performance information will depend on the set of schools included (e.g., only schools in the choice set or all schools), the way information is presented (e.g., adjusted by socioeconomic composition or not, by categories of achievement or the full rank of schools) and on how the information is distributed (e.g., parents receive reports cards or information has to be accessed online). Low-income families may not be able to respond to information that simply states that elite private schools are the top-performing schools in their area. This paper adds to this literature by identifying the effects of the distribution of simplified school performance information when this information is not adjusted by school socioeconomic composition and schools out of students' school choice sets are included.

Finally, this paper relates to the literature on aggregate effects of education policies (e.g., Hsieh and Urquiola, 2006; Muralidharan and Sundararaman, 2015). My identification strategy allows me to characterize students responding to the distribution of school performance information, understand how their enrollment choices change, and analyze changes in current school characteristics and test scores for all students in the population, including students that are not responding to the policy but could be affected by students changing enrollment decisions. Thus, I can decom-

[3] Hastings and Weinstein (2008) analyzed policies that provided low and middle-income parents with test score information for public schools (only) in their transportation zone. They found switching in enrollment towards higher-performing schools. Andrabi, Das and Kwaja (2015) provided parents of children enrolled in school with report cards that included test score rank information for all schools in the village, including public and private schools, their results suggest some switching of students out of low-scoring private schools. Mizala and Urquiola (2010) looked at the effects on enrollment choices for public and private subsidized schools of being categorized as a top-performing school with respect to schools with similar socioeconomic composition. These results are largely disseminated in the media, however, they found no effects on enrollment switching.

pose the effects of the policy by looking at the effects on students that were differently affected by the distribution of information, that means, students that responded, and students that did not respond but may have faced changes in school characteristics due to the policy.

The remainder of the paper is organized as follows: the following section describes the Chilean school system and the distribution of school quality information. Section 3 discusses the data used in this paper and presents descriptive statistics, then, section 4 describes my empirical strategy and the differences in RD design, followed by section 5 that presents my results and section 6 that discusses some robustness checks. Finally, section 7 concludes this paper.

2.2 Background

Chile introduced universal school choice, through school vouchers, in 1981. This education reform defined three types of schools based on the financing of the school:

1. *Public (or municipal) schools*. These schools are controlled by the municipalities and funded centrally through a per-student payment for every child attending their schools. Public schools can only legally turn away students if they are oversubscribed.
2. *Voucher (private) schools*. These schools are privately controlled and up to 1993 were funded exclusively through the same per-student payment as public schools. Since 1993 these schools can charge supplementary tuition.[4] If they choose to do so, the per-student payment is progressively reduced as tuition increases, but this reduction does not offset the tuition revenues except for very high tuition levels. In contrast to public schools, voucher schools were allowed to select students up to 2009.[5]
3. *Elite private schools*. These private institutions do not receive public funding and are financed through tuition fees. They account for around 7% of total enrollment and serve mainly high-income households.

In 2010, Sebastián Piñera's Government built on the education market's mechanisms by distributing simplified school performance information. In July 2010, all families in Chile received a

[4] Law 19.247 of September 15, 1993.

[5] Article 12 in Law 20.370 of September 12, 2009. Carrasco et al. (2014) suggest that voucher schools continue to select students.

map of their municipality which included all schools (including voucher and elite private schools) color-coded as a traffic light depending on their performance in a national standardized test in 2009.

Specifically, schools with a score lower than the national average of the test were colored in red in the map, schools with scores around the average of the test were colored in yellow, and schools with scores higher than the average of the test were colored in green. Families received this information for schools in their own municipality, and all schools were included, regardless of whether they were selective or their tuition fee amount. Additionally, schools' test scores were not adjusted by the socioeconomic composition of schools.

Through the distribution of this information the government intended to increase families' responsiveness to school performance measures and increase schools' pressure to improve quality. However, the policy was not well-received by education experts and many claimed that the maps were simply a picture of inequality in Chile. Since test scores were not adjusted by socioeconomic composition,[6] many families, and specially those in lower-income neighborhoods, would not be able to respond to the information by enrolling their children in a higher-performing school, therefore, experts argued that the only outcome of the policy would be to stigmatize schools.[7]

This policy was first announced during President Piñera's speech on May 21st 2010, and the distribution of information started on June 4th. By July 23rd 85% of schools had delivered the information,[8] which included the school performance municipality map together with letters from the President, Sebastián Piñera, and the Minister of Education, Joaquín Lavín, and advice to parents to improve their children's education.[9] Since the school year starts in March, the school performance information could have affected school enrollment decisions in 2011. This policy was discontinued in later years due to the pressure of the media and education experts.

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[6]Mizala, Romaguera and Urquiola (2007) show that school rankings based on test score information in Chile are very similar to school rankings based solely on students' socioeconomic status.

[7]See for example Jaime Retamal , "Como política pública es una medida muy simplista," *El Mostrador*, July 19, 2010, http://www.elmostrador.cl/noticias/pais/2010/07/19/%E2%80%9Ccomo-politica-publica-es-una-medida-muy-simplista%E2%80%9D/ (accessed May 2, 2016); or Alejandra Mizala, "El Simce, una vez más," *La Segunda*, June 10, 2010.

[8]United Press International, "Mineduc fiscalizará en colegios la entrega de mapas Simce", *La Tercera*, July 23 2010, http://www.latercera.com/contenido/679_278698_9.shtml (accessed May 2, 2016).

[9]For more information, see Allende (2011).

2.3 DATA

This chapter uses the same data that Chapter 1, please, refer to Chapter 1 Section 1.4 for more detail.

To carry out the analysis I use the sample of students enrolling in 1^{st} grade in 2009, 2010 and 2011. I focus on enrollment in 1^{st} grade since there are no costs of switching the initial enrollment decision.[10]

2.3.1 School Classification

As part of the policy, schools were classified in three categories depending on their performance on a standardized test in 2009. Unfortunately, there is no information regarding the actual classification of schools or the detailed procedure used to classify schools. Thus, I use 2009 standardized test scores for 4^{th} grade and allocate schools to categories as follows:[11]

- *Low-performing (red) schools* are those with an average standardized 4^{th} grade score in 2009 more than one standard deviation lower than the 4^{th} grade standardized average score in Chile for 2009.
- *Average-performing (yellow) schools* are those with an average standardized 4^{th} grade score in 2009 less than one standard deviation away from the 4^{th} grade standardized average score in Chile for 2009.
- *High-performing (green) schools* are those with average standardized 4^{th} grade score in 2009 more than one standard deviation higher than the 4^{th} grade standardized average score in Chile for 2009.

Table 2.1 presents school characteristics at baseline for schools in each one of the categories in Santiago.[12] The largest category of schools is the one for schools within one standard deviation of

[10]In omitted results I found no effects on transfers for students in higher grades. Enrollment in later grades responds less to new information due to the existence of costs related to switching schools.

[11]Results are robust to alternative categorizations of schools, as shown in Table B.6 in the Appendix for Allende's (2011) definition.

[12]Heterogeneity results show stronger responses to the policy in larger school markets. Therefore, as discussed in Section 2.5.1 in this chapter, this analysis focuses on the largest school market in Chile, Santiago metropolitan area, to determine the effects of the distribution of school performance information.

the average score (average-performing schools), and the smallest group is the one corresponding to low-performing schools.

There is a high correlation between the type of school and its classification by performance. Public schools are overrepresented in low-performing schools, voucher schools in average-performing schools, and elite private schools in high-performing schools. Similarly, average voucher school copay and school selectivity increase with school performance.

Regarding socioeconomic composition of schools, as Mizala, Romaguera and Urquiola (2007) showed, there is a high correlation between school performance and socioeconomic composition. Higher performing schools have higher-income students with higher-educated mothers. Students eligible for other targeted social programs are mostly enrolled in low-performing schools.

Class size increases from low-performing to average-performing schools, and decreases from average-performing to high-performing schools. This is consistent with Urquiola and Verhoogen (2009) who show an inverted-U relationship between class size and household income. As they discuss, lower-quality schools struggle to fill their classrooms, while higher-quality schools reduce class sizes to appeal to richer families.

The ratio of teachers per student is consistent with average class sizes, with a significantly larger ratio of teacher per student in high-performing schools. Teacher experience and age do not vary much between the three categories of schools, although, on average, teachers are younger and slightly less experienced in high-performing schools. Finally, the average level of education for teachers in the three types of schools is fairly similar, with a slight increase in the percentage of educated teachers for high-performing schools.

2.4 Empirical Strategy

The empirical strategy used in this chapter is methodologically equivalent to the one used in Chapter 1. Please, refer to Chapter 1 Section 1.5 for more detail.

I use two sources of variation to analyze the effects of the distribution of school performance information on enrollment decisions, school characteristics and achievement: the timing of the policy and exogenous variation on students' 1st grade enrollment year caused by an enrollment cutoff.

The policy of interest was carried out in June 2010, which implies that could have affected enrollment choices for the first time for students who enrolled in 2011.[13] Thus, the cohort of students that enrolled in 2010/2011 is the "treated" cohort.

The second source of variation originates in Chile's enrollment cutoff. As discussed in Chapter 1, in Chile all students that turn six years of age after June 30th have to wait until the next year to enroll in 1st grade by law. This creates a discontinuity in the exposure to the school performance information at the time of school choice. Figure 2.1 illustrates the discontinuity in the probability of enrolling in 2011 (instead of 2010) for students born after the enrollment cutoff. Since other enrollment cutoffs exist, the probability of enrolling in 2011 was already increasing for students born before the June 30th enrollment cutoff, but the June 30th cutoff is the largest discontinuity in the probability of enrolling in 2011.

I will use the discontinuity in the probability of enrolling in 2011, caused by the enrollment cutoff, in a Regression Discontinuity (RD) design to identify the effects of the distribution of school performance information on enrollment choices, school characteristics and test scores.

It is possible that there existed enrollment pre-trends or, in the case of test scores, that there were "enrollment age" or "age-at-test" effects (McEwan and Shapiro, 2008). Thus, I use the discontinuity created by the enrollment cutoff for the year prior to the policy to estimate these possible confounders. Then, I take differences in RD coefficients to estimate the effects of the distribution of school performance information.[14]

The differences in RD estimate can be obtained by interacting a "treated" term in an RD regression, as follows:

$$y_i = \beta + \theta Post_i + \rho Treated_i + \alpha Post_i \times Treated_i + f(B_i) + u_i \tag{2.1}$$

[13] The school year in Chile starts in March and admissions start in September of the previous year.

[14] Table B.2 in the appendix shows that there are no significant differences in socioeconomic characteristics of students born at either side of the cutoff. However, Table B.3 shows that density of births is not stable around the cutoff. Seasonality in births could explain changes in density of births. This policy was discussed for the first time in May 2010, therefore, since students that turned six around the 2010/2011 enrollment cutoff were conceived in 2003, it is impossible that there was manipulation of day of birth in response to the school performance information policy. Since the socioeconomic characteristics of students at either side of the cutoff are similar and changes in the density of births cannot be related to the policy, due to the timing of the policy, this fall in birth densities should not cause any bias to the differences in RD estimates.

where y_i is the outcome of interest, $Post_i$ is an indicator variable that equals 1 if the student was born after the enrollment cutoff, $Treated_i$ is an indicator equal to one if the student belongs to the cohort born around the 2010 enrollment cutoff (and that enrolled in 2010/2011), B is the distance from the sixth birthday to the June 30$^{\text{th}}$ enrollment cutoff, and $f(B_i)$ is a flexible parametric specification that includes higher-order polynomials of sixth birthday and can vary on either side of the enrollment cutoff. My main specification uses a linear spline on sixth birthday distance to cutoff interacted with a dummy indicating whether the student falls to the left or the right of the cutoff and with whether the student belongs to the "treated" cohort. I focus my analysis on a 90-day window around the June 30$^{\text{th}}$ cutoff, with day 0 representing the last cohort "untreated".

The coefficient α in regression (2.1) represents the estimate of the intent-to-treat effect of the distribution of school performance information.

2.5 Results

This section presents five sets of results. First of all, it shows heterogeneity in responses to the distribution of school performance information by school market characteristics. Since school market size is highly correlated with responses to the policy, I focus my analisys on Chile's largest school market: Santiago's metropolitan area. Second, it looks at the characteristics of students switching enrollment decisions in response to the distribution of school performance information. The third set of results illustrates responses in 1$^{\text{st}}$ grade enrollment to the policy for different student types, and compares the average characteristics of schools where compliers enroll in response to the policy to characteristics of schools where compliers would have enrolled in absence of the policy at baseline. Then, the fourth set of results presents results on the current school characteristics that students encounter after the resorting of students caused by the policy. Finally, the fifth set of results shows the effects on 4$^{\text{th}}$ grade standardized test scores for students that enrolled after school performance information was distributed.

Results for most outcomes are presented in a table that follows always the same structure. Column (1) presents estimates of the possible confounders, that is, the RD coefficient for the last pre-policy cohort (students born within 90 days of the 2009 enrollment cutoff and that enrolled in 2009 and 2010). Column (2) shows RD estimates for the treated cohort, students born within 90 days of the 2010 enrollment cutoff and that enrolled in 2010 and 2011. Finally, column (3) shows the differences in the RD estimates for column (2) minus column (1), that is, the unbiased estimate of the effect of distributing school performance information on the outcome.

Since, as shown in Figure 2.1, the probability of waiting one year to enroll increases by less than 1 for students born after the enrollment cutoff, the RD estimates in this case are intent-to-treat (ITT) estimates. This probability, that would correspond to the first stage in a fuzzy RD design, increases by 0.4 for students born after the enrollment cutoff. Thus, the average treatment on the treated effects (ATT) would be roughly equal to the presented estimates multiplied by one over 0.4, this means by 2.5.

2.5.1 Heterogeneity

Since the policy of interest consisted on the distribution of information, it is likely that responses to the policy varied depending on the characteristics of the school market. For this reason, Table 2.2 looks at the effect of the policy on the probability of enrolling in high-performing schools (green) for the full sample (column (1)) and at how the effects of the policy varied with different school market characteristics (columns (2)-(4)).

Coefficient $Post_i \times Treated_i$ in Table 2.2 represents the difference in RD coefficients for the treated cohort with respect to the pre-policy cohort. Column (1) shows that for the full sample, on average, there was no significant effect on the probability of enrolling in a high-performing school for students exposed to the school performance information. However, column (2) shows that this probability increases with the number of students enrolled in the municipality ($Post_i \times Treated_i \times$ Number of students in municipality), and column (3) shows that, similarly, it increases with the number of schools in the municipality ($Post_i \times Treated_i \times$ Number of schools in municipality). Finally, column (4) illustrates that the probability of enrolling in a high-performing school is also positively affected by the proportion of this type of schools in the municipality ($Post_i \times Treated_i \times$ Proportion of "green" schools in municipality).

Given the results in columns (2) and (3), I will focus my analysis on the largest school market in Chile: Santiago's metropolitan area. This school market also contained a higher proportion of high-performing schools than the average, as illustrated in Table 2.3. Thus, we should expect that the distribution of school performance information had a stronger effect in Santiago metropolitan area than in other municipalities in Chile.

2.5.2 Complier Characteristics

This section looks at the characteristics of students who switched enrollment decisions in response to the distribution of school performance information. Given the design of the information, and the fact that test scores were not adjusted by socioeconomic composition, it is possible that not all students in a municipality could respond to the information. Particularly, since high-performing schools were generally selective and expensive, it is likely that lower income students in municipalities were not able to adjust their enrollment choices to the school performance information. Additionally, it is possible that the highest income students in municipalities were already enrolled in the highest performing schools and, for this reason, could not respond to this policy. Thus, the characterization of compliers will help understanding what students were able to readjust their enrollment decisions in response to the information.

Compliers, in this case, are students that enrolled in high-performing schools in response to the distribution of school performance information. In order to obtain the average characteristics of compliers, I use a method equivalent to the one described in Chapter 1.[15] I compute the proportion of compliers with characteristic X_i through the following two-stage least squares system:

$$D_i = \theta + \psi Post_i \times Treated_i + \phi Post_i + \eta Treated_i + g(B_i) + \nu_i \tag{2.2}$$

$$X_i \times D_i = \alpha + \beta Post_i + \rho Treated_i + \gamma D_i + f(B_i) + u_i \tag{2.3}$$

where D_i is an indicator variable equal to one if student i enrolled in a high-performing school in 1st grade, $Post_i$ an indicator variable that equals one if the student was born after the enrollment cutoff, $Treated_i$ an indicator variable for whether the student belongs to the "treated" cohort, and $g(B_i)$ and $f(B_i)$ are linear splines on distance of sixth birthday to enrollment cutoff, interacted with whether the student was born to the right or left of the cutoff and interacted again with the $Treated_i$ indicator.

The coefficient γ in equation (2.3) gives the proportion of compliers with characteristic X. In

[15]For greater detail, please, refer to Chapter 1 Section 1.6.

this case, students "induced" into treatment in the first stage would be those born after the enrollment cutoff in the "treated" cohort, as they were exposed to the school performance information before choosing a school.

This system can only identify the characteristics of compliers if the *monotonicity assumption* holds. All students that were exposed to the distribution of school performance information had to change enrollment decisions in the same direction. Table 2.4 provides suggestive evidence that the monotonicity assumption holds: the difference in RD estimate in row (B) column (3) shows that the probability of enrolling in an average-performing school decreased with the distribution of school performance information, while the corresponding estimate for row (C) shows a similar and significant increase in enrollment in high-performing schools. The difference in RD estimate for low-performing schools is small and imprecise, which suggest that most switches in enrollment decisions were between average-performing schools and high-performing schools.

Table 2.5 shows the average characteristics for students that enrolled in high-performing schools in response to distribution of school performance information.[16] The first panel shows that the average socioeconomic index for compliers is 72.[17] The average socioeconomic index in Santiago is 0.29, therefore, compliers are significantly higher socioeconomic status than the average student in Santiago metropolitan area.

The second panel shows the proportion of compliers by quartile of the socioeconomic index distribution in the municipality. As the school performance information was distributed at the municipality level, we should expect responses for students in different points of the municipality socioeconomic distribution to differ. Results of the complier characterization go in this direction and show that most compliers (around 65% of students switching enrollment to high-performing schools) belong to the third quartile of the municipality socioeconomic distribution. This is consistent with high-performing schools being selective and with students in the top quartile being already enrolled in these high-performing schools, which serve the highest socioeconomic status students. Finally, consistent with these results, the last panel shows that most compliers belong to

[16] Since the proportion of students by the level of education of the mother is not stable over time (see Table B.4 in the Appendix) the results of the complier characterization by mother education are not informative and, for this reason, excluded.

[17] The socioeconomic index is computed using the coefficients of a regression of normalized math SIMCE score in 4$^{\text{th}}$ grade within test year on dummies for family income categories and mother's years of education.

the top half of the municipality socioeconomic index distribution.

Thus, the complier characterization suggests that not all students had the same ability to respond to the school performance information, as high-performing schools were mostly selective and more expensive than other schools. Students in the top half of the municipality socioeconomic distribution and, particularly, in the third quartile were the ones that used the school performance information and switched to higher-performing schools.

2.5.3 Enrollment choices

The rationale behind the distribution of school performance information is to help parents choose a more effective school. Therefore, the immediate effect of distributing school performance information should be a change in enrollment decisions. These changes in enrollment decisions may differ for students with different socioeconomic characteristics since, as discussed in the previous section, high-performing schools were usually selective and expensive. This section presents results on enrollment choices for students in different quartiles of the municipality socioeconomic distribution.

The difference in RD estimates presented in column (3) of Table 2.6 show that there were no significant changes in the probability of enrolling in low-performing schools for students in any quartile once families received school performance information. Most low-performing schools were public school while higher performing schools were mostly voucher and elite private schools. Therefore, since many voucher and elite private schools select students, it is likely that students that enrolled in low-performing schools would not have been admitted in higher performance schools or could not afford the tuition fees of these schools. For this reason, it is not surprising that students kept enrolling in low-performing schools despite the fact that these schools were now labeled as "bad" schools.

Enrollment in average-performing schools, however, did significantly decrease, as shown in column (3) of Table 2.7. The difference in RD estimate shows a significant decrease in the probability of enrolling in average-performing schools for students in the first and third quartiles.[18]

Finally, column (3) in Table 2.8 shows that there was a significant increase in the probability of

[18]This result, however, is not very robust as these coefficients lose significance in some of the robustness checks.

enrolling in high-performing schools, consistent with Hastings and Weinstein (2008), particularly for students in the third quartile of the municipality socioeconomic distribution. This increase is of similar magnitude to the decrease in enrollment in average-performing schools, which suggests that students in the third quartile of the municipality socioeconomic distribution enrolled less in average-performing schools and more in high-performing schools in response to the distribution of school performance information. This is consistent with about 65% of compliers belonging to this socioeconomic quartile and with the fact that high-performing schools were more expensive and selective, preventing lower socioeconomic status students from enrolling in this category of schools.

In order to determine how different the schools where compliers enrolled to were from schools where compliers would have enrolled to in absence of the policy I use a parallel technique to the one described in the previous section. I instrument enrollment in average-performing and high-performing schools in the following two-stage least squares system to obtain average school characteristics for compliers at baseline.

$$D_{it} = \theta + \psi Post_i \times Treated_i + \phi Post_i + \eta Treated_i + g(B_i) + \nu_i \tag{2.4}$$

$$S_i \times D_{it} = \alpha + \beta Post_i + \rho Treated_i + \gamma D_{it} + f(B_i) + u_i \tag{2.5}$$

where D_{it} is the probability of enrolling in school of type t, S_i is the school characteristic of interest, and $Post_i$, $Treated_i$ and $g(B_i)$ and $f(B_i)$ are as defined above. The coefficient γ in equation (2.5) gives the average of characteristic S for high-performing (destination "green") schools where compliers enrolled or for average-performing (counterfactual "yellow") schools where compliers ceased to enroll.

Unfortunately, this method cannot be used to compare current characteristics, as the distribution of school performance information may directly affect current school characteristics, for instance, through changes in student sorting. In this case, the *exclusion restriction* would not hold.

Table 2.9 shows the results from the two-stage least square system. Column (1) shows the average characteristics at baseline of average-performing ("yellow") schools where compliers would have enrolled to in absence of the policy, column (2) presents the average characteristics at baseline of high-performing ("green") schools where compliers enrolled to in response to the policy.

Finally, column (3) shows the differences in average characteristics at baseline for schools where compliers enrolled to with respect to schools where compliers would have enrolled to in absence of the policy. As illustrated by column (3), in response to the distribution of school performance information compliers enrolled less in public schools and more in voucher and, particularly, elite private schools. They enrolled in more expensive schools at baseline, larger schools with larger class sizes[19], and schools that had higher baseline test scores, which is consistent with the higher performance category of the school.

The third panel shows the differences in teacher characteristics at baseline for schools where compliers enrolled to with respect to schools where they would have enrolled to in absence of the policy. Column (3) shows that compliers enrolled in schools with teachers that, on average, had 4.5 less years of teaching experience, with more teachers per student and with a lower proportion of teachers with an education degree at baseline.

Finally, the last panel shows that, thanks to the distribution of school performance information, compliers enrolled in schools with a better socioeconomic composition. High-performing schools where compliers enrolled to had a higher average socioeconomic index at baseline, a higher proportion of students with mothers that completed university education, and lower proportions of students with mothers that had lower levels of education at baseline. This is consistent with students enrolling in higher performing schools since, as shown by Mizala, Romaguera and Urquiola (2007), school test scores are highly correlated to school socioeconomic composition.

2.5.4 School Characteristics

Results from the previous section showed that, in response to the school performance information, students, and particularly students in the third quartile of the municipality socioeconomic distribution, enrolled less in average-performing schools to enroll more in high-performing schools. This sorting of students could lead to changes in the school characteristics faced by the group that contains most compliers, the third quartile, and, if the the number of students switching enrollment was large enough, it could even imply changes in school characteristics for students in

[19]This is consistent with Urquiola and Verhoogen (2009) who show an inverted-U relationship between class size and household income. Compliers may be leaving undersubscribed lower quality schools to enroll in oversubscribed higher quality schools.

other quartiles. This section presents results on changes in current school characteristics faced by students in different quartiles of the municipality socioeconomic distribution.

Table 2.10 shows changes in average class size encountered by students in different points of the municipality socioeconomic distribution. Column (1) shows the RD coefficient for the pre-policy cohort, column (2) presents the RD coefficient for the treated cohort, and column (3) shows the differences in RD coefficients of column (2) minus column (1), that is, the effect of the policy on current class size for different groups of students. Estimates in column (3) show a positive but not significant increase in class size for students in the third and fourth quartiles, and a positive and significant increase in current class size for students in the top half of the municipality socioeconomic distribution. As shown in the previous section, compliers enrolled in schools that, at baseline, had larger class sizes. This partly explains the positive coefficient in class size for students in the third quartile. Additionally, compliers were enrolling in high-performing schools that were serving, mostly, students in the third and fourth quartiles.[20] Thus, the changes in enrollment decisions also affected class sizes of students in the top two quartiles through increasing enrollment in high-performing schools and class sizes at these schools. The positive and significant coefficient for students in the top half of the municipality socioeconomic distribution shows a combination of these two effects.

Column (3) in Table 2.11 shows that, resulting from the changes in enrollment decisions for students in the third quartile, these students ended up in schools with a better socioeconomic composition. This is consistent with compliers enrolling in schools that had a better socioeconomic composition at baseline, as shown in Table 2.9. In contrast, enrollment responses of compliers do not have an effect, on average, on the socioeconomic composition in schools where students in the fourth quartile enrolled. Similarly, results in column (3) of Table 2.12 suggest that, on average, students in the third quartile of the municipality socioeconomic distribution exposed to the school performance information ended up in schools with higher levels of socioeconomic segregation.[21] These two results suggest that, on average, students most likely to respond to the school

[20]See Table B.5 in the Appendix.

[21]The school socioeconomic integration index is the heterogeneity measure used in Urquiola (2005) and Alesina et al. (2004). Let $q = 1, 2, 3, 4$ denote the quartiles of the municipality socioeconomic distribution, and define P_{qs} as the proportion of students from quartile q in school s. The school socioeconomic integration index is then $I_s = 1 - \Sigma_{q=1}^{4} P_{qs}$. If the school is segregated and most of its students are from the same socioeconomic quartile, then the index will be

performance information, students in the third quartile, did encounter a better and more cohesive socioeconomic group of peers.

Finally, Table 2.13 explores possible changes in teacher characteristics. In omitted results, I found that there were no significant changes on the number of teachers per student or on the proportion of teachers with a degree in education for students in any quartile. The differences in RD coefficients in column (3) of Table 2.13 suggest, however, small but significant decreases in teacher experience for students in the second and fourth quartiles. In contrast, there are no significant changes on the years of experience of teachers for the group that contains most students switching enrollment decisions, students in the third quartile. This shows that, on average, teacher characteristics did not significantly change for students most likely to respond to the distribution of school performance information.

These results imply that students most likely to enroll in high-performing schools in response to the policy did find a better socioeconomic composition of students in these high-performing schools. However, on average, there were no significant changes in any other of the school characteristics analyzed.

2.5.5 Test Scores

The previous sections have shown that there were changes in enrollment choices, particularly for students in the third quartile of the municipality socioeconomic distribution, in response to the distribution of school performance information. Students most likely to respond ended up with a better socioeconomic composition of peers, but there were no other significant changes in school observable characteristics. This section analyzes the effects of the policy (and having the option to respond to the information by switching enrollment decisions) on standardized test scores for a test that is carried out in 4^{th} grade, three years after enrollment choices.

First of all, Tables 2.14 and 2.15 show that there were no effects on overall test scores neither in Math or Language when the whole student population in Chile is included. However, there is a small positive effect on Language test scores for students that belong to the third quartile of

close to zero, while if the school has a mixed composition of students, the index will approach 0.75, the maximum in this case, since there are only 4 groups of students. This index can be interpreted as the probability that two students picked at random are from two different quartiles of the socioeconomic distribution.

the municipality socioeconomic distribution, the same group that responded to the distribution of school performance information by switching enrollment choices.[22] As discussed in the Heterogeneity subsection, characteristics of the school market could affect responses to the policy and, consequently, effects on test scores. For example, families in school markets with little variation in school performance categories were not as able to respond to the information. Thus, Tables 2.16 and 2.17 focus on the largest school market in Chile, the Santiago metropolitan area.

The differences in RD estimates in column (3) of Table 2.16 show that, even though there were some increases in Math test scores for students exposed to the information, particularly for the first and third quartiles, these effects were not significant.

Table 2.17 analyzes the effects on Language test scores. Results in column (3) show that there was an overall positive and significant effect on Spanish test scores. This effect is strongest and significant for students in the top half of the municipality socioeconomic distribution and, particularly, for students most likely to enroll in high-performing schools in response to the policy, students in the third quartile. Additionally, this effect is pretty large, it is around one third of the effect of enrolling one year older.[23] However, since these results lose significance for the full sample and in one of the robustness checks, they can only be considered suggestive.

These results suggest that a better socioeconomic composition of peers may have stronger effects on verbal achievement than on math achievement. This could be consistent with Zimmerman (2003), since he finds a higher correlation of own achievement with peers' verbal scores than math scores.

Additionally, these results show that there were no negative effects for any group of students caused by the distribution of school performance information, despite the possible stigmatization of schools. Therefore, from an efficiency perspective, results suggest that this policy had positive effects, as there was an overall increase in Spanish test scores. However, from an equity perspective, since only students in the third quartile of the municipality socioeconomic distribution were able to respond to the information, this could improve education opportunities only for students in this quartile. There is the possibility that this led to small increases in inequality in the medium

[22] See table B.1 in the Appendix.

[23] As estimated by McEwan and Shapiro (2006).

run due to an unequal improvement in education opportunities.

2.6 Robustness Checks

This section presents results that address possible concerns regarding the main specification or the assumptions used in this paper.

As discussed in the Data section, there is no official description of the criteria used to determine the school classification. Therefore, Table B.6 shows that the enrollment results of this analysis are in general robust to using Allende's (2011) classification of schools, despite a loss of significance in the coefficients corresponding to the probability of enrolling in average-performing schools.[24]

Another possible concern is that the last pre-policy cohort may not be representative of the existing pre-trends. For this reason, Table B.7 includes, additionally, the cohort of students born around the 2008 enrollment cutoff (that enrolled in 2008 and 2009) in the pre-policy sample.[25] Results in this case are generally consistent both in magnitude and significance, except for the case of Spanish test scores. In this case, the coefficient for students in the third quartile is still positive and the largest of all student groups, but it loses significance. For this reason, I consider the results in Spanish test scores suggestive.

Finally, a last concern is the possibility that the choice of bandwidth or the existence of other enrollment cutoffs may be biasing the estimates. Table B.8 addresses this concern by repeating the analysis using a 45 day bandwidth, instead of the 90 day bandwidth used in the main specification. In this case, since the sample size is reduced in half, several results lose significance. In spite of this, the sign of the coefficients is the same as in the main specification and the magnitude of the estimates is very similar to the magnitude of the results in the main specification. Therefore, my results are robust to the use of a smaller bandwidth and the choice of bandwidth does not seem to be biasing my results.

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[24] Allende (2011) divides schools in three groups depending on the availability of test score results for 4th and 8th grades. Then, she classifies schools within each group in the three performance categories.

[25] Since in 2008 there was a major school reform that had delayed effects, it is likely that students that enrolled in 2008 and 2009 were still affected by this policy. For this reason, I do not include students born around the 2008 enrollment cutoff in the pre-policy cohort in my main specification.

2.7 Conclusion

School accountability has been expanding in the last decades and particularly in the last few years, with a significant increase in the number and frequency of standardized student testing. One possible consequence of school accountability, if school performance information is available, is changes in enrollment choices, which could, in turn, affect school future performance. Consequently, it is necessary to understand how student sorting interacts with school accountability systems in order to correctly interpret changes in school performance indicators.

There is some evidence on enrollment responses to school performance information (e.g., Hastings and Weinstein, 2008), but results in this area are mixed. This suggests that the format and distribution channel of the information play an important role in order for the information to be effective.

In this paper, I analyze the effects of the distribution of simplified school performance information that was not adjusted by socioeconomic composition of schools and that included all schools, even elite private schools. Additionally, using two sources of variation, I characterize students responding to the information and decompose the aggregate effects of the policy by looking at effects on students at different points of the municipality socioeconomic distribution.

Results show that not all students were able to respond to the information, since high-performing schools were generally selective and expensive. Thus, the group of students that switched enrollment decisions belonged, mostly, to the third quartile of the municipality socioeconomic distribution. This implies that the immediate effect of the policy was to allow medium-high income students to enroll in higher-performing schools, while keeping lower income students in the same schools.

Thanks to the school performance information, students in the third quartile had peers from a higher socioeconomic status, while other observable school characteristics did not significantly change. Test score results suggest that there was an overall increase in verbal test scores for students in the Santiago metropolitan area, driven by improvements for the third quartile and no negative effects for other quartiles, while math test scores did not significantly change. This could indicate that peer effects could be stronger in verbal than math achievement, which is consistent with results in Zimmerman (2003).

From an efficiency point of view this is an optimistic result, since a low-cost policy, such as sending families school performance information, allowed students to sort into higher-performing schools and led to an increase in achievement. However, not all students in the population benefited equally from the policy, as lower-income students were not able to respond to the information. As a consequence, the policy increased education opportunities and achievement for students in the third quartile while not directly affecting lower income students, reducing education equity.

Additionally, these results show that the way the information is presented and distributed matters. Since the information was not adjusted by school socioeconomic composition, and socioeconomic status is highly correlated with performance (Mizala, Romaguera, Urquiola, 2007), schools in the high-performing category were mostly out of reach for students in the lower half of the municipality socioeconomic distribution. This policy may have been more effective had school performance information been adjusted by school socioeconomic composition.

Regarding the distribution of information, Mizala and Urquiola (2010) found no effects of receiving school quality information through the media. In this policy, all parents received the school information directly at home together with a letter from the President and the Minister of Education. Thus, parents may respond more to school performance information if the information is directly addressed to them.

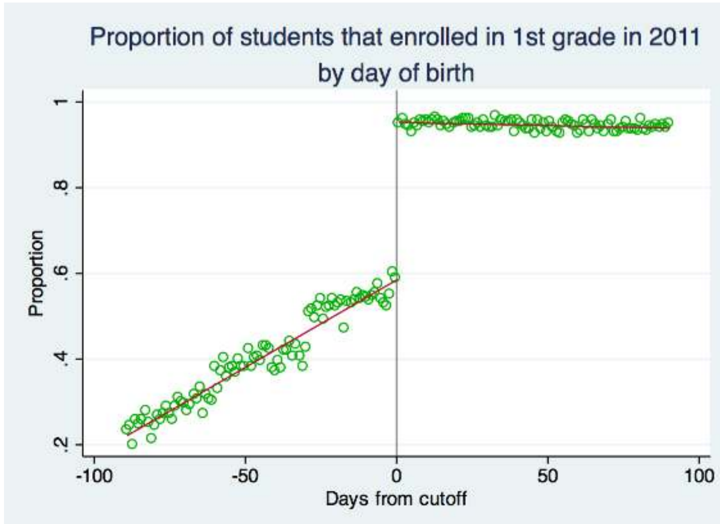
In conclusion, this paper shows that the distribution of school performance information could lead to sorting of students into higher-performing schools and to an improvement in achievement. However, policy-makers need to be careful regarding the format and distribution of the information, since parents may respond differently to information presented in different ways, and this could potentially lead to a reduction in education equity.

Tables and Figures

Table 2.1: School characteristics by category - year 2009

School clasification in 2009 (traffic light)	Low-perfoming schools (red)	Average-performing schools (yellow)	High-performing schools (green)
Proportion of schools in category	0.167	0.567	0.267
Proportion of public schools	0.594	0.307	0.060
Proportion of voucher schools	0.402	0.647	0.479
Proportion of elite private schools	0.005	0.046	0.462
Proportion of schools participating in SEP	0.863	0.639	0.182
Average copay (2013 USD)	2.9	14.7	42.9
Prop. of students that claim school was selective	0.68	0.768	0.896
Average school socioeconomic index (standard dev.=100)	-54.4	0.1	122.2
Proportion of mothers with basic ed. or less (1^{st}-4^{th})	0.272	0.145	0.027
Proportion of mothers with university ed. (1^{st}-4^{th})	0.019	0.074	0.318
Proportion of Chile Solidario students (1^{st}-4^{th})	0.307	0.169	0.054
Proportion of prioritized students (1^{st}-4^{th})	0.667	0.449	0.154
Proportion of male students (1^{st}-4^{th})	0.549	0.525	0.512
Standardized 4^{th} grade Math score	-0.703	-0.133	0.538
Number of students in class (1^{st})	29.1	32.2	27.4
Number of students in school (1^{st}-4^{th})	161.5	223.5	242.4
Number of teachers per student (%)	6.6	6.4	11.8
Teaching experience in years	17.4	15.4	14.7
Teacher age	46.0	43.4	42.3
Teachers with a degree in education (%)	93.9	94.2	94.8

Figure 2.1: Probability of enrolling in the next academic year by distance from the date of birth to the June 30th enrollment cutoff



Notes: Each circle above represents the proportion of students that turned six years of age on that day and enrolled in 2011 instead of 2010 within 90 days of the June 30, 2010 enrollment cutoff. Since the policy was planned and implemented after the 2010 school year had started, there is no possibility of startegically delaying enrollment to obtain the information before making a choice.

Table 2.2: Heterogeneity by School Market Characteristics – Changes in the probability of enrolling in a high-performing schools (green)

	(1) Baseline	(2) Number of students	(3) Number of schools	(4) Proportion of "green" schools
$Post_i \times Treated_i$	0.0019	-0.0135	-0.0164	-0.0120
	(0.0073)	(0.0107)	(0.0124)	(0.0093)
$Post_i \times Treated_i \times$ Number of students in municipality	–	1.64e-06*	–	–
		(8.61e-07)		
$Post_i \times Treated_i \times$ Number of schools in municipality	–	–	0.0004*	–
			(0.0002)	
$Post_i \times Treated_i \times$ Prop. of "green" schools in municipality	–	–	–	0.0678**
				(0.0329)
Observations	235,081	235,081	235,081	235,081
R-squared	0.000	0.002	0.003	0.145

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each column represents a separate regression with the first coefficient representing the difference in the RD coefficients for the post-policy period with respect to the pre-policy period. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. In columns (2)-(5) the linear spline is additionally interacted with the variable with respect to which heterogeneity is measured.

Table 2.3: Proportion of Schools by Category in 2009

	Chile	Santiago
Proportion of low-performing schools (red)	0.114	0.123
Proportion of average-performing schools (yellow)	0.642	0.578
Proportion of high-performing schools (green)	0.239	0.296

Notes: Each coefficient represents the proportion of schools that belong to that performance category weighted by enrollment.

Table 2.4: Check of Monotonicity Assumption: Probability of enrollment in school by classification (1^{st} grade)

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
(A) Low-performing schools (red)	-0.012*	-0.003	0.009
	(0.007)	(0.007)	(0.010)
(B) Average-performing schools (yellow)	-0.007	-0.038***	-0.032**
	(0.011)	(0.011)	(0.016)
(C) High-performing schools (green)	0.017*	0.044***	0.027*
	(0.010)	(0.010)	(0.015)
Observations	33,864	32,995	66,859

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Table 2.5: Complier characteristics - Proportion of compliers by level of mother education and position in socioeconomic index distribution

	Compliers
Average socioeconomic index	72.06
	(72.00)
1^{st} quartile of Socieconomic index in municipality	0.263
	(0.254)
2^{nd} quartile of Socieconomic index in municipality	0.082
	(0.305)
3^{rd} quartile of Socieconomic index in municipality	0.664
	(0.393)
4^{th} quartile of Socieconomic index in municipality	-0.008
	(0.397)
Below median of Socieconomic index in municipality	0.345
	(0.342)
Above median of Socieconomic index in municipality	0.655
	(0.342)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses and the standard deviations are in brackets. Each coefficient represents a separate regression. Enrollment in voucher school is instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts. The regressions above use a window of 90 days and include only students attending school in Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows the complier characteristics including all years in the sample and a full set of cohort dummies.

Table 2.6: School choices: Probability of enrollment in low-performing schools - red (1st grade)

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	-0.0123*	-0.0033	0.0090
	(0.0070)	(0.0068)	(0.0097)
1st quartile of Socioeconomic index in municipality	-0.0302	-0.0014	0.0288
	(0.0193)	(0.0183)	(0.0266)
2nd quartile of Socioeconomic index in municipality	-0.0175	0.0025	0.0200
	(0.0155)	(0.0144)	(0.0212)
3rd quartile of Socioeconomic index in municipality	-0.0006	-0.0236**	-0.0230
	(0.0104)	(0.0111)	(0.0152)
4th quartile of Socioeconomic index in municipality	-0.0238**	-0.0227**	0.0010
	(0.0106)	(0.0102)	(0.0147)
Below median of Socioeconomic index in municipality	-0.0237*	-0.0010	0.0227
	(0.0125)	(0.0117)	(0.0171)
Above median of Socioeconomic index in municipality	-0.0107	-0.0231***	-0.0124
	(0.0075)	(0.0076)	(0.0107)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.7: School choices: Probability of enrollment in average-performing schools - yellow (1st grade)

	ITT estimates		Difference in ITTs
	(1)	(2)	(3)
	2009/10	2010/11	Post-Pre
All students	-0.0067	-0.0382***	-0.0316**
	(0.0109)	(0.0111)	(0.0155)
1st quartile of Socioeconomic index in municipality	0.0532**	-0.0038	-0.0570*
	(0.0235)	(0.0245)	(0.0340)
2nd quartile of Socioeconomic index in municipality	-0.0410*	-0.0388	0.0022
	(0.0237)	(0.0253)	(0.0347)
3rd quartile of Socioeconomic index in municipality	-0.0141	-0.0790***	-0.0649*
	(0.0244)	(0.0266)	(0.0361)
4th quartile of Socioeconomic index in municipality	-0.0365	-0.0412	-0.0047
	(0.0274)	(0.0283)	(0.0394)
Below median of Socioeconomic index in municipality	0.0062	-0.0229	-0.0291
	(0.0167)	(0.0177)	(0.0243)
Above median of Socioeconomic index in municipality	-0.0235	-0.0612***	-0.0376
	(0.0183)	(0.0195)	(0.0267)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.8: School choices: Probability of enrollment in high-performing schools - green (1st grade)

	ITT estimates		Difference in ITTs
	(1)	(2)	(3)
	2009/10	2010/11	Post-Pre
All students	0.0173*	0.0438***	0.0266*
	(0.0102)	(0.0105)	(0.0146)
1st quartile of Socioeconomic index in municipality	-0.0202	0.0144	0.0346
	(0.0176)	(0.0199)	(0.0265)
2nd quartile of Socioeconomic index in municipality	0.0546***	0.0398*	-0.0148
	(0.0208)	(0.0238)	(0.0316)
3rd quartile of Socioeconomic index in municipality	0.0095	0.1030***	0.0939***
	(0.0241)	(0.0263)	(0.0356)
4th quartile of Socioeconomic index in municipality	0.0540*	0.0606**	0.0067
	(0.0276)	(0.0286)	(0.0398)
Below median of Socioeconomic index in municipality	0.0169	0.0302*	0.0134
	(0.0137)	(0.0157)	(0.0208)
Above median of Socioeconomic index in municipality	0.0284	0.0831***	0.0547**
	(0.0183)	(0.0194)	(0.0267)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.9: School characteristics at baseline (2009) for compliers

	(1) Counterfactual "yellow" schools	(2) Destination "green" schools	(3) Difference
Enrolled in public schools	0.249	-0.255	-0.504
	(0.321)	(0.247)	
Enrolled in voucher schools	0.668	0.775	0.107
	(0.326)	(0.367)	
Enrolled in elite private schools	0.083	0.480	0.397
	(0.090)	(0.333)	
Enrolled in voucher schools with copay	0.677	0.715	0.038
	(0.350)	(0.353)	
Average school copay in 2013 USD	-9.99	17.67	27.66
	(24.67)	(42.47)	
School av. class size (1^{st}-4^{th})	28.94	32.48	3.54
	(5.45)	(5.04)	
Enrollment (1^{st}-4^{th})	376.9	535.2	158.3
	(166.4)	(157.2)	
Average score 4^{th} grade SIMCE score	-0.177	0.542	0.719
	(0.209)	(0.224)	
Average teacher age (school)	41.68	42.25	0.57
	(3.72)	(2.87)	
Years of teaching experience (school)	14.56	9.90	-4.66
	(3.97)	(4.04)	
Number of teachers in class (school)	34.61	94.43	59.82
	(15.75)	(34.52)	
School teachers per student (%) (school)	5.005	7.698	2.693
	(1.766)	(2.836)	
Proportion of male teachers (school)	0.083	0.207	0.124
	(0.114)	(0.093)	
Teachers with degree in education (%) (school)	98.17	90.91	-7.26
	(4.65)	(4.10)	
Av. socioeconomic index (2009 - 1^{st}-4^{th})	-2.37	120.40	122.77
	(32.35)	(53.38)	
Proportion of mothers with basic (1^{st}-4^{th})	0.120	0.055	-0.065
	(0.065)	(0.034)	
Proportion of mothers with high school (1^{st}-4^{th})	0.664	0.383	-0.281
	(0.083)	(0.149)	
Proportion of mothers with university (1^{st}-4^{th})	0.047	0.252	0.205
	(0.054)	(0.147)	

Notes: Robust standard errors are in parentheses. Each coefficient represents a separate regression. Enrollment in voucher and public school are instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts. The regressions above use a window of 90 days use the sample of students attending schools in Santiago's metropolitan area and include all years. They also include a linear spline of distance from birthday to enrollment cutoff.

Table 2.10: School characteristics: Average Class Size (1st grade)

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	-0.018	0.038	0.056
	(0.177)	(0.179)	(0.252)
1st quartile of Socioeconomic index in municipality	-0.072	-0.123	-0.052
	(0.378)	(0.406)	(0.555)
2nd quartile of Socioeconomic index in municipality	0.088	0.037	-0.051
	(0.398)	(0.410)	(0.572)
3rd quartile of Socioeconomic index in municipality	-0.330	0.375	0.704
	(0.400)	(0.418)	(0.578)
4th quartile of Socioeconomic index in municipality	-0.317	0.687	1.004
	(0.430)	(0.447)	(0.620)
Below median of Socioeconomic index in municipality	0.005	-0.037	-0.042
	(0.275)	(0.288)	(0.398)
Above median of Socioeconomic index in municipality	-0.325	0.520*	0.845**
	(0.293)	(0.305)	(0.423)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.11: School characteristics: Average Socioeconomic Index (1^{st}-4^{th} grades)

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.028	0.052***	0.025
	(0.019)	(0.019)	(0.027)
1^{st} quartile of Socioeconomic index in municipality	0.001	-0.019	-0.019
	(0.028)	(0.032)	(0.043)
2^{nd} quartile of Socioeconomic index in municipality	0.060	0.051	-0.009
	(0.037)	(0.043)	(0.057)
3^{rd} quartile of Socioeconomic index in municipality	0.023	0.154***	0.131**
	(0.046)	(0.047)	(0.066)
4^{th} quartile of Socioeconomic index in municipality	0.069	0.089*	0.020
	(0.045)	(0.045)	(0.064)
Below median of Socioeconomic index in municipality	0.029	0.024	-0.005
	(0.024)	(0.028)	(0.036)
Above median of Socioeconomic index in municipality	0.043	0.123***	0.081*
	(0.033)	(0.033)	(0.046)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.12: School characteristics: School Integration Index (1st grade, in %)

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	-0.225	-0.479	-0.254
	(0.327)	(0.339)	(0.471)
1st quartile of Socioeconomic index in municipality	-0.232	1.084	1.316
	(0.720)	(0.742)	(1.034)
2nd quartile of Socioeconomic index in municipality	0.431	-0.048	-0.479
	(0.503)	(0.592)	(0.777)
3rd quartile of Socioeconomic index in municipality	0.389	-1.452**	-1.840*
	(0.636)	(0.740)	(0.976)
4th quartile of Socioeconomic index in municipality	-1.848*	-0.921	0.927
	(1.059)	(1.052)	(1.493)
Below median of Socioeconomic index in municipality	0.076	0.597	0.522
	(0.447)	(0.478)	(0.655)
Above median of Socioeconomic index in municipality	-0.560	-1.185*	-0.625
	(0.593)	(0.636)	(0.869)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.13: School characteristics: Teachers Experience

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.019	-0.251**	-0.270
	(0.116)	(0.124)	(0.170)
1st quartile of Socioeconomic index in municipality	-0.258	-0.475*	-0.217
	(0.270)	(0.286)	(0.394)
2nd quartile of Socioeconomic index in municipality	0.179	-0.519*	-0.699*
	(0.263)	(0.286)	(0.389)
3rd quartile of Socioeconomic index in municipality	0.203	-0.230	-0.433
	(0.240)	(0.278)	(0.367)
4th quartile of Socioeconomic index in municipality	0.396	-0.390	-0.786*
	(0.268)	(0.304)	(0.405)
Below median of Socioeconomic index in municipality	-0.040	-0.522**	-0.482*
	(0.189)	(0.203)	(0.278)
Above median of Socioeconomic index in municipality	0.298*	-0.300	-0.598**
	(0.179)	(0.206)	(0.273)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.14: School outcomes: Math Scores in 4th Grade Standardized Test – Full sample

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.165***	0.158***	-0.00742
	(0.0130)	(0.0132)	(0.0185)
1st quartile of Socioeconomic index in municipality	0.153***	0.131***	-0.022
	(0.027)	(0.027)	(0.039)
2nd quartile of Socioeconomic index in municipality	0.206***	0.202***	-0.004
	(0.026)	(0.027)	(0.038)
3rd quartile of Socioeconomic index in municipality	0.164***	0.193***	0.029
	(0.026)	(0.027)	(0.038)
4th quartile of Socioeconomic index in municipality	0.135***	0.127***	-0.008
	(0.026)	(0.027)	(0.038)
Below median of Socioeconomic index in municipality	0.175***	0.162***	-0.013
	(0.019)	(0.019)	(0.027)
Above median of Socioeconomic index in municipality	0.153***	0.165***	0.012
	(0.019)	(0.019)	(0.027)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Test scores are comparable over time and normalized with respect to test scores for 2012 (corresponding to students who enrolled in 2009). Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.15: School outcomes: Spanish Scores in 4th Grade Standardized Test – Full sample

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.157***	0.179***	0.022
	(0.013)	(0.013)	(0.018)
1st quartile of Socioeconomic index in municipality	0.140***	0.168***	0.028
	(0.026)	(0.026)	(0.037)
2nd quartile of Socioeconomic index in municipality	0.192***	0.218***	0.025
	(0.026)	(0.027)	(0.037)
3rd quartile of Socioeconomic index in municipality	0.140***	0.206***	0.066*
	(0.025)	(0.027)	(0.037)
4th quartile of Socioeconomic index in municipality	0.147***	0.139***	-0.009
	(0.025)	(0.026)	(0.037)
Below median of Socioeconomic index in municipality	0.162***	0.188***	0.026
	(0.019)	(0.019)	(0.026)
Above median of Socioeconomic index in municipality	0.146***	0.177***	0.031
	(0.018)	(0.019)	(0.026)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Test scores are comparable over time and normalized with respect to test scores for 2012 (corresponding to students who enrolled in 2009). Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.16: School outcomes: Math Scores in 4th Grade Standardized Test – Students in Santiago metropolitan area

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.074***	0.108***	0.033
	(0.025)	(0.025)	(0.035)
1st quartile of Socioeconomic index in municipality	0.049	0.118**	0.069
	(0.054)	(0.053)	(0.075)
2nd quartile of Socioeconomic index in municipality	0.093*	0.072	-0.021
	(0.052)	(0.052)	(0.073)
3rd quartile of Socioeconomic index in municipality	0.055	0.149***	0.094
	(0.049)	(0.052)	(0.071)
4th quartile of Socioeconomic index in municipality	0.058	0.072	0.014
	(0.054)	(0.053)	(0.075)
Below median of Socioeconomic index in municipality	0.070*	0.099***	0.029
	(0.037)	(0.037)	(0.053)
Above median of Socioeconomic index in municipality	0.054	0.112***	0.058
	(0.036)	(0.037)	(0.052)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Test scores are comparable over time and normalized with respect to test scores for 2012 (corresponding to students who enrolled in 2009). Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table 2.17: School outcomes: Spanish Scores in 4th Grade Standardized Test – students in Santiago metropolitan area

	ITT estimates		Difference in ITTs
	(1)	(2)	(3)
	2009/10	2010/11	Post-Pre
All students	0.046*	0.12***	0.075**
	(0.024)	(0.025)	(0.034)
1st quartile of Socioeconomic index in municipality	0.025	0.128**	0.103
	(0.052)	(0.052)	(0.073)
2nd quartile of Socioeconomic index in municipality	0.065	0.093*	0.028
	(0.051)	(0.051)	(0.072)
3rd quartile of Socioeconomic index in municipality	0.031	0.154***	0.124*
	(0.047)	(0.052)	(0.070)
4th quartile of Socioeconomic index in municipality	0.020	0.100*	0.080
	(0.053)	(0.053)	(0.075)
Below median of Socioeconomic index in municipality	0.045	0.114***	0.070
	(0.036)	(0.034)	(0.052)
Above median of Socioeconomic index in municipality	0.024	0.128***	0.104**
	(0.035)	(0.037)	(0.051)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Test scores are comparable over time and normalized with respect to test scores for 2012 (corresponding to students who enrolled in 2009). Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Chapter 3

Educational Shocks and the Distribution of Wages. Evidence from Colombia

3.1 Introduction

The relationship between the proportion of college graduates and the college wage premium has been extensively studied, especially for the US (e.g., Katz and Murphy, 1992; Teulings and van Rens, 2008). The evidence supports a strong negative relationship between the proportion of educated workers and the wage premium, keeping demand constant (e.g., Card and Lemieux, 2001; Angrist, 1995). However, a decrease in the wage premium can indicate a decrease in average wages for college graduates, an increase in average wages for high school graduates, or both. Similarly, an increase in the proportion of college graduates can affect differently workers at different points of the wage distribution. Thus, in order to understand the effects on wages of a change in the educational structure of the labor force, the whole wage distribution needs to be analyzed.

In this paper I study the effects of an expansion in the tertiary education system in Colombia on the distribution of wages for workers with different education levels. In 1995, in response to the educational reform carried out through the "Ley 30" (December 1992), there was a significant expansion of the tertiary education sector in Colombia. This led to a rise in the proportion of young workers with tertiary education from around 25% for workers who accessed the labor market prior to the reform to over 40% for workers exposed to the reform.

I use education and labor market information from household surveys for 2001, 2002, 2011 and 2012 to carry out this analysis. I address the question by using two different empirical strategies: a reweighting method and a differences in differences strategy.

First, I exploit the timing of the policy and use the reweighting method developed by DiNardo, Fortin and Lemieux (1996) to build a counterfactual distribution of wages. I focus on workers between 27 and 34 years of age, since these were the workers exposed to the expansion in the tertiary education system in the post-policy sample (the 2011/2012 sample). These workers had not entered the labor market yet in the pre-policy sample (the 2001/02 sample), thus, I reweight the distribution of wages for workers between 27 and 34 years in the pre-policy sample (the 2001/02 sample) to mimic the distribution of characteristics of the post-policy sample. The difference between the post-policy distribution of wages and the reweighted pre-policy distribution of wages shows the changes in the structure of wages. Unfortunately, this method does not allow to control for labor demand shifts and productivity changes, which could lead to positive biases in my results, for this reason, I use a second empirical strategy.

In my second strategy, I assume that workers of different ages are imperfect substitutes (Card and Lemieux, 2001), and I define a "control" group within each sample: older workers. This way I can address the question in a differences-in-differences setting using the "treated" and "control" groups, workers between 27 and 34 years and workers between 37 and 44 years, respectively, in the pre-policy (2001/02) and post-policy (2011/12) samples. Then, I take differences-in-differences in wage distributions, which allows me to control for changes in the wage structure that affected also the "control" group, such as possible changes in productivity that increased the whole distribution of wages or shifts in the labor demand. In contrast, this method does not control for changes in the composition of workers in each education group, which could lead to negative biases in my results within education levels, as "lower ability" workers were completing higher levels of education. Finally, I use differences-in-differences regressions to analyze changes in average wages.

The first stage results of this analysis show a general increase in education in the population, with the density of workers by years of education shifting to higher values. Regarding the wage distribution, the reweighting method shows a shift towards higher wages for workers exposed to the increase in the proportion of educated workers, but this increase is reduced when I control for the economic conditions at the time of entering the labor market for the first time. The results of the differences-in-differences strategy show some decrease in dispersion, but no change on the distribution of wages, which suggests that productivity factors or labor demand shifts may have been causing the shift in the wage distribution.

Since the results of the reweighting method may be overestimated due to possible changes in labor demand and productivity,[1] this result could be interpreted as an upper-bound. In contrast, the results of the differences-in-differences strategy may be downward biased, due to changes in composition. The expansion of the tertiary education system led to "lower ability" workers to complete higher levels of education, which worsened the composition of workers by education levels. Consequently, these results can be interpreted as a lower-bound on the analysis. Therefore, the two sets of results interpreted together will give suggestive evidence of changes in wage

[1]The period studied was a period of economic growth in Colombia with annual GDP growth of over 3% for most years. Therefore, if there were any labor demand shifts, they should be positive and have a positive effect on wages.

distributions for workers with different levels of education.

The reweighting method and the differences-in-differences strategy point towards the same result for workers with low-levels of education: there was some shift in the distribution of wages towards higher wages, although this shift was small. Wages for workers with high school education seem to concentrate in both strategies, with a reduction in the density in high wages and an increase in the density of wages around the mode. Finally, the density of wages for workers with tertiary education decreases at high levels of wages using both strategies, with a shift of the wage distribution towards lower wages.

Results of the differences-in-differences regressions show no significant changes on average wages for any education group in response to the change in the educational structure of the labor force. Despite the insignificant results, the coefficients seem to go in the expected direction given the results from the wage distributions, with an increase in average wages for workers with primary education or less and a decrease in wages for workers with tertiary education.

This paper relates to several strands of literature. First of all, it contributes to the literature on how shocks to the proportion of educated workers affect wages of non-educated workers. This literature is composed by research on Comparative Advantage and on Signaling and is mainly theoretical. Thus, this paper provides an empirical assesment of these two literatures, which have opposing conclusions.

On the one hand, with a very old tradition that started with Roy's paper in 1951, the Comparative Advantage literature predicts the sorting of workers into the education level in which they are comparatively better. Therefore, easing the access to tertiary education implies a transfer of "low quality" high school graduates to "low quality" tertiary education graduates. Then, as a response to an increase in the proportion of educated workers, average wages for tertiary education graduates should decrease, while average high school wages should increase (e.g., Rosen and Willis, 1979; Carneiro and Lee, 2005). On the other hand, based on Spence (1973), a branch of the Signaling literature predicts exactly the opposite effect on uneducated wages. In the presence of credit constraints, the initial pool of uneducated workers is composed by low ability workers and workers with high ability and low financial resources. Once education costs are reduced, high ability workers enroll in tertiary education leaving only low ability workers in the uneducated pool of workers. As a consequence, wages for uneducated workers are reduced (e.g., Willen, Hendel and

Shapiro, 2005; Balart, 2012).

My results suggest that both, the Comparative Advantage hypothesis and the Signaling hypothesis are holding in this case. Workers with primary education or less experience increases in their wage distribution, which is consistent with the Comparative Advantage literature, i.e. the "best" uneducated workers being left in the pool of uneducated workers. In contrast, workers at the top of the high school graduate wage distribution experience decreases in wages. This could be explained by the "high ability" high school graduates completing tertiary education in the post-policy period and employers taking high school education as a "signal" that the worker is "low ability".

Additionally, this paper contributes to the Skill-Biased Technological Change hypothesis (SBTC) literature by looking at the effects on wages of educated workers. According to the Skill-Biased Technological Change hypothesis (SBTC), technological advancement implies a rise in the relative demand for educated workers (e.g., Goldin and Katz, 1996; Autor, Krueger and Katz, 1998) and in their relative wages. My results show a decrease in wages for workers with tertiary education while there were no shifts on wages with high school education. This implies that the increase in the proportion of educated workers due to the expansion of the tertiary education system was sufficient to satisfy the increase in the demand for educated workers, actually reducing wages for this group.

Finally, this paper also contributes to the literature on general equilibrium effects of education policies on wages (e.g., Bianchi, 2016; Duflo, 2004; Abbot et al., 2013). I find an increase in wages for students with primary education or less in response to the rise in the proportion of workers with tertiary education, which implies that this policy not only affected the wage premium for tertiary education, but also wages for lower levels of education.

The organization of this paper is as follows: section 2 includes an explanation of the shock in the proportion of educated workers suffered by Colombia, section 3 describes the data used, section 4 describes the empirical strategy, section 5 shows the results and, finally, section 6 concludes.

3.2 Background

In 1992, Colombia's government regulated the higher education system for the first time through the "Ley 30". This law was a very comprehensive law that created several control and advice institutions for higher education, regulated the functioning of the public higher education system, and planned a substantial increase in the public expenditure on higher education. Additionally it created the National System of Accreditation for higher education institutions,[2] explicitly allowed the entry of private higher education institutions and reinforced the role of the ICETEX (i.e. the Colombian institution that awards scholarships and student loans). The "Ley 30" expanded the resources that the ICETEX would be responsible for, established the criteria according to which scholarships and student loans would be granted, and gave the ICETEX the role of guarantor of those student loans awarded by the financial sector to students with low economic resources.

As a result of the "Ley 30", there was an unprecedented entry of private higher education institutions (see Figure 3.1), and a higher availability of public resources for higher education. These two facts, together with the larger proportion of population that completed secondary education, led to a trend break in tertiary education enrollment that took place in 1995 (see Figure 3.2).

Despite some cohort crowding that resulted in a lower probability of graduation (Herrera Prada, 2011), this expansion in the tertiary education system led to an increase in the proportion of educated workers for the cohorts exposed to the policy. This can be observed in Figure 3.3 panel C, the proportion of workers younger than 35 with tertiary education was higher in the post-policy sample than in the pre-policy sample. These workers exposed to the policy faced a higher proportion of workers with tertiary education in the labor market, which could have affected their wages.

Lower levels of education were not affected by the expansion in the tertiary education sector. Panels A and B in Figure 3.3 show that there was a general decrease in the proportion of workers with primary education or less for students of all ages between the pre-policy and the post-policy sample and an increase in the proportion of workers with high school education for the generation not exposed to the policy only. This suggests that, while there was a general trend towards

[2] *Sistema Nacional de Acreditación.*

completing high school education instead of dropping out after primary education, the policy led to an increase in the proportion of workers with tertiary education while keeping workers with high school education constant.

The tertiary education system in Colombia is composed by three different levels of education: technical education, technological education and university education. Technical education is oriented to operational and instrumental occupations and usually lasts around 2 years. Technological education is slightly larger, with a duration of around 3.5 years, and offers further education on occupations and professional training. Finally, university education takes around 5 years, although there are students that take up to 8 years to complete it, and takes place at Universities, which are institutions that carry out scientific or technological research. Unfortunately, the data used for this analysis does not allow to disaggregate tertiary education in these three levels.

3.3 Data: Colombia's Household Surveys 2001-2011

This paper uses data from the Continuous Household Survey[3] for 2001 and 2002 and for its newer version, the Integrated Household Survey[4] for 2011 and 2012. These household surveys are administered by Colombia's National Statistical Administrative Department[5] and the survey microdata is public and accessible. They contain information on wages, hours of work, education level and other demographic characteristics and represent the whole population of Colombia.

Since the expansion in the tertiary education system took place in 1995, I will define the 2001 and 2002 household surveys (ECH) as my pre-policy sample and the 2011 and 2012 household surveys (GEIH) as my post-policy sample. Wages in the post-policy sample for young workers exposed to the shock include changes in the wage structure caused by the change in the educational structure of the labor market.

Due to possible heterogeneous effects on wages by sex or by residence, I limit the sample to men who live in urban areas. Additionally, I focus the analysis on workers who are between 27 and 34 years of age at the time of the survey, since these workers belong to the cohorts exposed to

[3] *Encuesta Continua de Hogares* (ECH).

[4] *Gran Encuesta Integrada de Hogares* (GEIH).

[5] *Departamento Administrativo Nacional de Estadistica* (DANE).

the expansion of the tertiary education system when they completed high school education. The lower limit of 27 years of age is chosen to allow individuals to complete tertiary education, since there are some workers who take up to 8 years to complete university education.

I exclude from the sample self-employed individuals, and individuals working without pay. Furthermore, I exclude those workers with wages lower than the 5th percentile and higher than the 95th percentile, as I consider these observations outliers. In order to be able to compare across individuals, I consider wages per hour. Finally, I deflate wages to the 2001 Colombian pesos value so wages can be compared across survey years.

Table 3.1 shows summary statistics for the full sample, the sample of individuals in the pre-policy sample (ECH 2001/02), and the sample of individuals in the post-policy sample (GEIH 2011/12). There are no large differences in terms of demographic characteristics between the two samples, with a similar proportion of male workers and a small decrease in the proportion of married workers. However, consistent with the policy studied, there are some differences in the proportion of workers by level of education. There is a decrease in the proportion of workers in the post-policy sample that completed primary education or less or high school and a corresponding increase in the proportion of workers that completed tertiary education.

Regarding job market indicators, there are no differences in the proportion of workers employed in the two samples and there is a small increase in the proportion of self-employed workers in the post-policy sample. Finally, wages per hour seem to increase on average in the post-policy sample.

3.4 Empirical strategy

I use two different empirical strategies to identify the effects of the expansion in the tertiary education system on wage distributions for workers with different levels of education. First of all, I use the DiNardo, Fortin and Lemieux (2006) reweighting method, and then I use a differences-in-differences strategy. The two subsections below provide the details of each one of these strategies.

3.4.1 Reweighting method

In this first strategy I use one source of variation: the timing of the policy. Since the expansion in the tertiary education system started in 1995, it affected mostly students finishing high school around that time, this means, students that were around 18 years in 1995. Thus, students exposed to the policy were 25 or younger in the pre-policy sample and 35 or younger in the post-policy sample.

Since my analysis focuses on workers between 27 and 34 years of age, workers exposed to the policy at the time of deciding whether to pursue tertiary education are excluded of the pre-policy sample. This means that he 2001/02 sample is not affected by the policy, while workers between 27 and 34 years in the post-policy sample were affected by the policy. Comparing distributions of wages for workers in these two samples, however, would lead to biased results. First of all, it is possible that the composition of workers in these two samples differs. Also, a simple comparison of wage distributions would not account for changes in education of the labor force. As a consequence, I need to build a counterfactual wage distribution that replicates the composition and educational structure of the post-policy wage distribution but keeps the wage structure of the pre-policy wage distribution.

To do so, I use the reweighting method developed by DiNardo, Fortin and Lemieux (1996). Following their method, I rescale the observations of the sample that was in the labor market before the shock to mimic the distribution of characteristics of the sample affected by the shock.

Assuming that the structure of wages after the shock does not depend on the distribution of characteristics, the hypothetical density of wages before the shock with the distribution of characteristics and education corresponding to the sample after the shock can be written as:

$$\begin{aligned} f(w; t_w = pre, t_x = post) &= \int f(w|x, t_w = pre) dF(x|t_x = post) && (3.1) \\ &\equiv \int f(w|x, t_w = pre) \psi_x(x) dF(x|t_x = pre) && (3.2) \end{aligned}$$

where

$$\psi_x(x) \equiv \frac{dF(x|t_x = post)}{dF(x|t_x = pre)} \tag{3.3}$$

Applying Bayes' rule, this weight can be written as

$$\psi_x(x) = \frac{P(t_x = post|x)}{P(t_x = pre|x)} \times \frac{P(t_x = pre)}{P(t_x = post)} \tag{3.4}$$

where $P(t_x = t|x)$ can be estimated using the following probit model:

$$P(t_x = t|x) = P(\epsilon > -\beta' x) = 1 - \Phi(-\beta' x) \tag{3.5}$$

where $\Phi(.)$ is the cumulative normal distribution. The unconditional probability $P(t_x = t)$ is simply estimated by the proportion of observations that belong to the sample of the year t.

Once these weights have been calculated, I use them in a kernel density estimation to obtain the wage distribution that would have prevailed for workers in the pre-policy sample (not exposed to the expansion in the tertiary education sector) if the distribution of characteristics and education levels had been the same as the observed in the post-policy sample.

$$\hat{f}(w; t_w = pre, t_x = post) = \Sigma_{i \in t = pre} \frac{1}{h} \hat{\psi}_x(x) K\left(\frac{w - W_i}{h}\right) \tag{3.6}$$

where $K(.)$ is the Epanechnikov kernel, and h is the bandwidth.

Additionally, since it has been shown that the economic conditions at the moment of entering the labor market affect wages substantially (Oreopoulos, Heisz and Von Wachter, 2012), I estimate the wage distributions controlling for the economic circumstances at the moment of entering the labor force.

To do so, I estimate the density of the residual obtained from a regression of the logarithm of wage on constant GDP growth at the moment of entering the labor force. The resulting wage densities estimated using this residual are net from the shifts in the wage structure caused by shocks to the economic conditions.

The counterfactual wage densities represent the wage density that would have prevailed if the individual characteristics and education levels had been at their post-policy level and workers had been paid according to the pre-policy wage structure. A comparison of the estimate of the wage distribution for the post-policy and for the reweighted pre-policy sample shows the change in the wage structure net from the change in sample composition and education.

However, since productivity changes and labor demand shifts may also be shifting the wage distribution, the evidence obtained from the estimate of the reweighted wage distribution is not sufficient to conclude that the policy shifted wages in a given direction. For this reason, I use a second empirical strategy that allows me to control for market-wide labor demand shifts and productivity changes.

3.4.2 Differences-in-differences

The second empirical strategy uses two sources of variation: the timing of the policy, the fact that the pre-policy sample was not affected by the policy, and worker age. As Card and Lemieux (2001) show, workers of different ages are imperfect substitutes in the labor market. This implies that wages of workers that were not exposed to the expansion in the tertiary education system when they finished high school but that belong to the post-policy sample should be less affected than wages of workers that were exposed to the expansion in tertiary education. Thus, I can define a "control" group of workers within each one of the samples: older workers. Particularly, I define workers between 37 and 44 years old as the "control" group.[6] This control group is not perfect, since wages for older workers could still be affected. If this is the case, wages for the "treatment" and "control" groups would move in the same direction and my estimates would be underestimated.

This definition of a "control" group allows me to define four groups of workers in a differences-in-differences setting: young workers ("treated") in the post-policy sample, these workers are the ones directly affected by the shock, older workers in the post-policy sample, young workers ("treated") in the pre-policy sample and older workers in the pre-policy sample.

Since the distribution of wages for older workers may differ to that of younger workers, the first difference consists in subtracting the kernel density estimate of the wage distribution of older workers to the estimate of the wage distribution for younger workers for both, the post-policy and pre-policy samples. This first difference for the post-policy sample includes changes in the wage structure due to experience or age and due to exposure to the expansion on tertiary education.

Then, the second difference consists on subtracting the difference for the pre-policy sample

[6] Figure C.1 in the Appendix shows no visible changes in how average wages evolve with age between the pre-policy and the post-policy samples for the current definition of the "control" group.

from the difference for the post-policy sample. This allows to control for usual changes in the wage structure for older workers and also for pre-trends such as the ongoing decrease in the proportion of workers with primary education.

In summary, I will take differences of the kernel density estimates of wage distributions as follows:

Post-policy sample young ("treated") cohort - Post-policy sample older ("control") cohort	Pre-policy sample young ("treated") cohort - Pre-policy sample older ("control") cohort
Difference Post-policy - Difference Pre-policy = **Estimate of policy effects**	

This strategy allows to control for changes in the wage structure of younger workers with respect to the wage structure of older workers that are constant between the two periods. Additionally, it allows to control for demand shifts and productivity shocks that affected equally older and younger workers, which was the main potential bias in the results of the reweighting method. I will also control for the possible effects that labor market initial conditions had on wages, as shown by Oreopoulos, Heisz and Von Wachter (2012), by estimating the density of the hourly wage net of the effects of this initial conditions, that is, using the residual of the regression of log hourly wage on GDP growth on the year of entering the labor market for the first time. In contrast, this strategy does not allow to control for changes in the composition of workers in each one of the education levels caused by the policy.

Since the policy eased the access to tertiary education, it is plausible to assume that the composition of workers in all three education levels worsened after the expansion in the tertiary education system. Thus, if changes in the composition of workers introduce a bias in my results, this bias should be negative and results should be underestimated.

Finally, I use the differences-in-differences strategy to carry out some regressions of the effects of the expansion on the tertiary education system on average wages.

$$log(wage_i) = \beta_1 + \beta_2 post_i + \beta_3 treated_i + \beta_4 post_i \times treated_i + g(exp_i) + \gamma X_i + \phi_{region} + \epsilon_i \quad (3.7)$$

where $log(wage_i)$ is the logarithm of the hourly gross wage for individual i, $post_i$ is a dummy variable equal to one if the observation of the worker belongs to the post-policy sample (GEIH 2011/12), $treated_i$ is an indicator that equals one if the workers belongs to the cohort affected by the expansion in the tertiary education system (the workers is between 27 and 34 years of age), $g(exp_i)$ is a cubic polynomial on estimated experience,[7] X_i are economic and sociodemographic controls, specifically, the real GDP growth rate in the year of entry into the labor market, and dummies for marital status, and ϕ_{region} are region fixed effects.

The coefficient β_4 in regression 3.7 captures the average effect on wages of being exposed to the expansion of the tertiary education system.

Given that the period between the pre-policy and the post-policy samples was a period of high economic growth for Colombia, with annual GDP growth rates larger than 3% in most years, labor demand shifts, if present, should be positive and lead to larger wages. Thus, my results from the reweighting method, if biased, should be overestimated. In contrast, since the main source of bias in the differences-in-differences strategy is the changes in the composition of workers within level of education, these results are likely to be underestimated. These imply that the results from these two strategies can be interpreted as an upper and a lower bound on the effects of the policy on the wage distribution, respectively. Therefore, only the combination of the results of these two strategies will be informative regarding the direction of the changes in the wage distributions for workers with different education levels.

3.5 Results

This section presents results for the two empirical strategies discussed in the previous section. The last subsection interprets how the two sets of results go together.

3.5.1 Reweighting method

As discussed in the Background section, the expansion of the tertiary education system led to an increase in the proportion of young workers with tertiary education. Figure 3.4 looks, additionally, at changes in the distribution of workers by years of education. In panel A, the actual

[7] Estimated experience is computed as age - years of education - modal age of entering primary school (Card, 1999).

distributions are showed for the pre-policy and the post-policy cohorts, while panel B shows the reweighted pre-policy cohort (excluding education variables in the weight computation) and the actual post-policy cohort. The first impression is that the two panels are very similar. The weights can only account for changes in composition of the sample that are captured by the variables included during the computation of the weight. Since the data I am using does not contain many individual characteristics, unfortunately, the reweighting method does not seem to be very successful at adjusting changes in composition of the sample.

Figure 3.5 shows the differences between the post-policy and the pre-policy densities for the actual density estimates in graph A, and for the reweighted density in graph B. Graph B in Figures 3.4 and 3.5 show that there was a decrease in the density of workers with low education levels and an increase in the density of workers with higher education levels, particularly of workers with eleven or more years of education. The increase in the density for workers with more than 12 years of education represents the rise in the proportion of workers completing tertiary education. These results show that the probability of finding a worker with a high number of years of education is higher for the post-policy sample and that the educational change affected, in fact, the distribution of education characteristics in the labor force.

The effects on the wage distribution caused by these changes in the educational structure of the labor force are shown in Figure 3.6. Panel 1 graph A shows the wage density estimate for the post-policy and pre-policy samples. Graph B shows the reweighted pre-policy estimate of the density of wages, accounting for changes in education, together with the post-policy estimate of the density of wages. The reweighted pre-policy estimate of the density of wages represents the wage distribution that would have prevailed if the wage structure had remained as in the pre-policy sample but labor force characteristics had been those of the post-policy sample. Graph C, additionally, accounts for changes in the economic conditions at the moment of entering the labor market.[8] This last adjustment allows to compare the post-policy sample wage distribution to the wage distribution that would have prevailed if the wage structure had remained as in the pre-policy sample, the labor force characteristics had been as in the post-policy sample and the economic conditions when entering the labor market had remained constant. Thus, the pre-policy

[8]Oreopoulos, Heisz and Von Wachter (2012) show that economic conditions at the time of entering the labor market have a persistent impact on wages.

density estimates for graphs B and C are estimates of the counterfactual distribution of wages if workers with the characteristics of the post-policy sample were subject to the pre-policy wage structure.

Additionally, graphs A and B contain two vertical lines that represent the level of the minimum hourly wage in 2001 and 2012, respectively. As shown in panel 1 of Figure 3.6, the minimum hourly wage was not binding in Colombia in any of the two samples. There are two reasons for that: first of all, since the minimum wage is set in terms of monthly wage, it is possible that some workers are working more than the average hours of work and being paid the minimum monthly wage, thus, receiving a lower hourly wage. Second, Colombia has a large informal sector that is not subject to the minimum wage. Around 45-50% of the population was working in the informal sector during the period studied, thus, it is possible that these workers were receiving hourly wages below the minimum wage.[9]

Figure 3.7 shows the differences between different versions of the pre-policy density estimate for the wage distribution and the post-policy wage distribution. Panel 1 graph A shows the estimate for the actual distribution of wages in the pre-policy sample, graph B reweights the pre-policy sample by changes in composition and graph C additionally controls for GDP growth at the time of entering the labor market. These differences show that, once the distribution of wages for the post and the pre-policy samples are comparable, there is a shift in the distribution of wages towards higher wages. This shift is reduced, though, when initial conditions in the economy are taken into account. Thus, this suggests that the fact that Colombia was in an economic expansion had a positive impact on the distribution of wages. However, the positive effect on wages remains after controlling for economic conditions, which means that there were further increases in wages. Since the reweighting method does not allow to control for labor demand shifts or productivity increases, controlling by GDP growth may not be completely capturing these factors and these results may be positively biased. Finally, the increase in the minimum wage seems to be shifting the distribution of wages towards higher values, with the mode of the distribution moving to the value of the minimum wage in 2012.

Densities in panels 2 to 4 of Figures 3.6 and 3.7 replicate the same structure as the previous

[9] See, Peña (2013) for more information on the size of the informal sector in Colombia.

figures dividing workers by education level. First of all, panel 2 in Figures 3.6 and 3.7 look at changes in the distribution of wages for workers with low levels of education, those with primary education or less. These workers experience a shift to higher wages after the expansion of the tertiary education system. According to Peña (2013), over 80% of workers in this education level work in the informal sector, for this reason, the increase in the minimum wage is not likely to be the main driver behind the shift in the wage distribution. In contrast, the shift in the wage distribution is consistent with a Comparative Advantage interpretation in which the highest-skilled workers for uneducated jobs remain uneducated after an expansion in higher levels of education. It should be noted that other factors, such as productivity increases or labor demand shifts, could also have pushed the wage distribution up for this level of education.

Panel 3 in Figures 3.6 and 3.7 shows changes in the wage distribution estimates for workers with high school education. Graphs A and B in both figures suggest a shift towards higher wages, consistent with the increase in the minimum wage,[10] and a small decrease in wages for workers at the top of the wage distribution for high school graduates. However, once I control for the economic conditions at the time of entering the labor market, in graph C, the fall in density for the highest wages becomes larger. This suggests that, thanks to the economic expansion, wages for high school graduates increased. However, once economic conditions are kept constant, wages for high school graduates seem to be concentrating around the new minimum wage, with a decrease in the density of low and high wages in the distribution.

The fall in the density of high wages is consistent with the Signaling hypothesis. Since it is now easier to access tertiary education, workers left in the high school graduate pool may be perceived as less-skilled by employers and offered lower wages. The fall in density for low levels of wages can be explained by the increase in the minimum wage, the 40% of high school graduates working in the formal sector may now be receiving higher wages.

Finally, panel 4 in Figures 3.6 and 3.7 show changes in the wage structure for workers that completed tertiary education. In this case, all three graphs, even without reweighting by changes in composition, show the same results. The increase in the proportion of tertiary education grad-

[10] In the case of high school graduates, around 60% of them worked in the informal sector and 40% in the formal sector. This 40% could have been affected by the increase in the minimum wage and shifted the wage distribution towards the new minimum wage.

uates led to a decrease in wages of tertiary education graduates for workers at the top of the wage distribution. Particularly, the density of wages appears to be bunching around the new minimum wage. Since most workers with tertiary education work in the formal sector (over 60%), the minimum wage was binding for their employers, creating an accumulation of the density just over the minimum wage. This suggests that "highly-skilled" educated workers received lower wages after the policy. Since tertiary graduates labor supply shifted up, the decrease in wages suggests that labor demand for this level of education either did not shift up or, if it did, it shifted up by less than the labor supply, reducing the price of labor, wages.

In summary, these results suggest that after the increase in the proportion of workers with tertiary education, there was an increase in wages for workers with low levels of education, a decrease in dispersion for workers with high school education, and a decrease in wages for tertiary education graduates at the top of the wage distribution, with an increase in the density of wages just over the minimum wage. Workers at the top of the wage distribution for high school and tertiary education graduates seem to have worsened wages in response to the policy, while workers at the bottom of the wage distribution for primary education or less and high school graduates seem to have benefited from the policy with an increase in wages.

3.5.2 Differences-in-differences

The second empirical strategy consists on using an older cohort within each one of the samples as a "control" group and carry out differences-in-differences in estimates of the wage density. In this case, each panel in the Figure of interest contains three graphs. Graph A shows the density estimate for the "treated" cohort (workers between 27 and 34 years) and the "control" cohort (workers between 37 and 44 years) in the post-policy sample, graph B shows the same two densities for workers in the pre-policy sample. Finally, graph C shows the differences-in-differences of the wage densities, that means, it representes the difference in the wage density for the "treated" and the "control" cohorts in the post-policy sample not explained by regular changes in the wage structure. Since, in this case, I am using wages net of the effect of the labor market conditions at the time of entering the labor market, I cannot plot the minimum wage levels.

Figure 3.8 looks at changes in the density of workers by years of education. Results using the differences-in-differences strategy are very similar to those using the reweighting method: there is a fall in the density at low levels of education and an increase in the density at higher levels of education, particularly at eleven or more years of education.

Changes in the wage distribution are showed in Figure 3.9. Graphs A and B in panel 1 show almost identical wage density estimates for the "treated" and the "control" cohorts. Consequently, the differences-in-differences of the wage estimate show very small and noisy changes in the wage distribution for the cohort exposed to the expansion in the tertiary education system.

Since the reform implied easing the access to tertiary education, it is likely that the composition of workers in each of the education levels worsened, as "lower-ability" students were completing higher levels of education. For this reason, the wage density estimate for the "treated" cohort in the post-policy sample within levels of education may be affected by these changes in composition and, thus, downward biased. This needs to be taken into consideration when interpreting the following results.

With regard to the distribution of wages for workers with low levels of education, primary education or less, panel 2 in Figure 3.9 shows an imprecise shift towards higher wages, since there was a lower difference between the wage distributions for "treated" and "control" workers in the post-policy sample than in the pre-policy sample.

The distribution of wages for high school education graduates, however, shows smaller changes, as shown in panel 3 in Figure 3.9. Also, wages for workers with this level of education seem to be concentrating, with a fall in the density at high wages and some opposing effects for wages at lower levels of the distribution. Since this strategy is not controlling for changes in worker composition, the fall in the density of high wages is consistent, in this case, with an easier access to tertiary education levels for "high-skilled" high school graduates, reducing the density of "high-skilled" workers in the group of high school graduates.

Regarding wages for workers with tertiary education, the first result when looking at panel 4 in Figure 3.9 is that wages for this education level increase more rapidly with experience than wages for other education levels, as shown by the lower wage density for the "treated" cohort with respect to the "control" cohort in both samples. Once this change in the educational structure is taken into account, graph C shows that the density of wages for workers exposed to the expansion in the tertiary education system shifted towards lower wage levels.

Summarizing, this strategy shows no significant changes in the overall wage distribution, some shift towards higher wages for workers with primary education or less, a small decrease in the density at the top wages for workers with high school education, and a decrease in wages for workers at the top of the wage distribution for tertiary education graduates.

Finally, table 3.2 shows the results for the differences-in-differences regression 3.7. This table shows changes in average wages for workers exposed to the expansion in the tertiary education system in each one of the education levels. Results in columns (1) and (2) show that the average decrease in wages of younger workers with respect to older workers was larger in the post-policy cohort, which may indicate that experience was more rewarded in the post-policy cohort for all levels of education. However, once I control for job market experience, in columns (3)-(5), results are not significant any more, suggesting that there were no significant effects on average wages. This is consistent with the results discussed above, as in most cases there were small changes in wage densities within the existing wage distribution, instead of clear shifts to higher or lower wages.

Despite the lack of significance, the signs of the coefficients in columns (3)-(5) are generally consistent with the changes observed in the wage distribution. Average wage for workers with primary education or less increase, while average wages for workers with tertiary education decrease.

3.5.3 Reconciling results

As discussed in the Empirical Strategy section, both strategies used in this paper are subject to biases. However, the direction of the possible biases differs for both strategies. In the case of the reweighting method, while this method adjusts for changes in the composition of workers, results are likely to be upward biased due to the favorable economic conditions that could have implied labor demand shifts towards higher wages. Additionally, this method does not allow to control for productivity shocks that, again, could have increased wages during the period.

In contrast, the differences-in-differences strategy can account for these biases as long as they affected equally the wage structure for the “treated” and “control” cohorts. Even though this advantage of the differences-in-differences method, this strategy does not allow to control for changes in the composition of workers in general and, in particular, changes in the composition of workers in each one of the education levels. As the reform studied eased the access to tertiary education levels and, additionally, there was an ongoing expansion of the high school education level, it is likely that changes in the composition of workers by level of education were negative. That means, it is likely that workers that did not progress to higher levels of education were those of lower ability that would have received wages in the bottom of the wage distribution. Thus, if there is a bias in the differences-in-differences estimate for changes in the wage distribution, this bias is likely to be negative.

Since these two strategies are exposed to biases that go in opposite directions, only results that indicate shifts in the same direction for both strategies can be trusted. For this reason, it is important to interpret the two sets of results together.

The overall distribution of wages seems to shift to higher wages with the reweighting method, partly caused by the increase in the minimum wage, while results are unclear with the differences-in-differences strategy. In this case, since the composition of workers for the complete labor force is less likely to change than the composition of workers within levels of education, I expect this second method to lead to results with a smaller bias. Therefore, these results suggest that there were no effects on the overall distribution of wages caused by the expansion in the tertiary education system, but, there were increases in the wage distribution caused by labor demand shifts or productivity changes that cannot be accounted for in the reweighting method, as well as an increase in the minimum wage level.

Wages for workers with primary education or less increase using both strategies, despite the fact that this shift is smaller for the differences-in-differences strategy. This result suggest that the Comparative Advantage hypothesis may be holding in this case. The "best" uneducated workers are the ones that stayed uneducated once there was an expansion in educational opportunities.

With regard to wages for workers with high school education, both strategies showed a decrease of the density at high levels of wages. Since the reweighting method is accounting for changes in composition of workers, this cannot be explained by "highly-skilled" high school graduates completing tertiary education in the post-policy sample. A possible explanation is the Signaling hypothesis. As a result of the policy, it is cheaper and easier to access tertiary education in the post-policy sample. Thus, workers that do not complete tertiary education are perceived as signaling their "lower-ability" by employers and offered lower wages. This may have a stronger impact on workers at the top of the wage distribution.

Finally, both strategies show a decrease in the estimated density at the top wages of the tertiary education wage distribution, with an increased bunching around the new minimum wage. This is consistent with the largest participation of this group of workers in the formal labor market. The decrease in the density of high wages can be explained simply in a supply and demand framework. The expansion in the tertiary education system implied an increase in the supply of tertiary education workers. In the case that the demand for these workers did not increase, or increased less than the supply, wages for these workers should decrease. This result suggests that the increase in the proportion of workers with tertiary education was sufficient to fulfill the ongoing increase in the demand of educated workers caused by technological change, actually decreasing tertiary education wage premium. Additionally, according to my results, workers at the top of the wage distribution were the most affected by the negative change in wages.

3.6 Conclusions

The effects of an increase in the proportion of college graduates on the college wage premium have been extensively studied (e.g., Katz and Murphy, 1992; Teulings and van Rens, 2008). However, changes in the college wage premium may indicate changes in college graduate wages, high school graduate wages, or both. Additionally, the effects on wages may be heterogeneous at different points of the wage distribution.

In this paper I use an expansion in the tertiary education system in Colombia in the mid-1990s to identify the effects of a sudden increase in the proportion of workers with tertiary education on the distribution of wages for workers with different levels of education. I use the timing of the policy and the fact that workers of different ages are not perfectly substitutable (Card and Lemieux, 2001) to carry out two different analysis. In the first one I build a counterfactual distribution of wages replicating the composition of workers in the post-policy sample through the DiNardo, Fortin and Lemieux (1996) reweighting method. In the second, I take differences-in-differences in wage density estimates.

None of the two strategies are free from biases. However, since the main sources of bias in each strategy go in opposite directions, the results from the first strategy are likely to be overestimated while the results of the second strategy are likely to be underestimated. Thus, the interpretation of the results of these two strategies together allow to reach conclusions regarding the effects of the expansion in the tertiary education system on the wage distribution.

My results show that labor demand shifts and productivity changes were the main drivers behind the general increase in wages during the period. When looking at wage distributions by levels of education, the only group of workers that benefited from the change in the educational structure of the labor force were the least educated workers, workers with primary education or less. This is consistent with a Comparative Advantage hypothesis in which only the "best" uneducated workers remain "uneducated" and, thus, their wages increase. There is some decrease in the density of wages at the top wage levels for workers with high school education. A possible explanation is the Signaling hypothesis. Once the cost of accessing tertiary education is reduced, "high ability" high school graduates can now complete tertiary education. Therefore, not completing tertiary education is perceived by employers as a signal of not being "high ability", and wages for high school graduates, and particularly those at the top, are reduced. These results show that within a labor market, both, the Comparative Advantage hypothesis and the Signaling hypothesis can hold for workers with different levels of education.

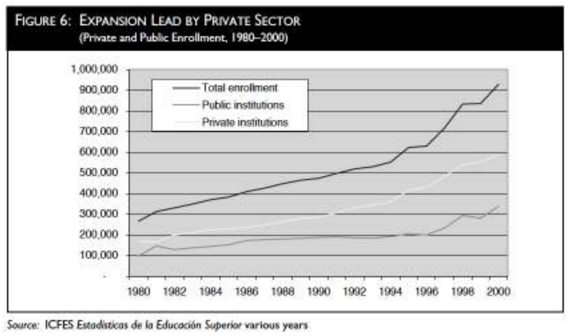
Regarding wages for tertiary education graduates, my results show a decrease in the density at high levels of wages and an increase in bunching around the minimum wage. This suggests that the growth in the demand of educated workers suggested by the Skill-Biased Technological Change Hypothesis (e.g., Goldin and Katz, 1996; Autor, Krueger and Katz, 1998) was not large enough to absorb the increase in educated workers in the labor market, implying a decrease in wages for tertiary education graduates.

Finally, results on average wages show no significant changes for workers in any of the education levels. This means that workers at different points of the wage distribution were affected, as shown by the shifts in wage distribution estimates, but that these shifts did not affect average wages.

These results imply that education policies may have heterogeneous effects for workers at different points of the wage distribution and may, additionally, affect workers that were not at the margin of studying the education level that is expanded. Therefore, an analysis of average wages or wage premia will give an incomplete picture of the actual effects of education policies on the wage distribution. Finally, since the whole labor market can be affected by education policies, as shown in this paper, the possible effects on workers not directly exposed to the education policy should also be taken into account.

TABLES AND FIGURES

Figure 3.1: Origin of expansion in tertiary enrollment



Source: The World Bank, 2003. *Tertiary Education in Colombia. Paving the Way for Reform*

Figure 3.2: Enrollment in tertiary education

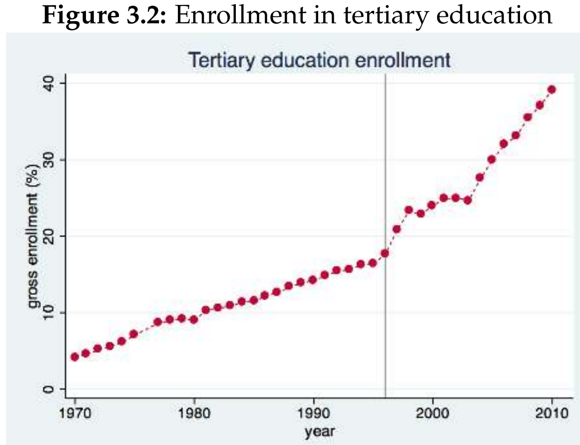


Figure 3.3: Proportion of workers by age and education level in ECH 2001/0202 and GEIH 2011/12

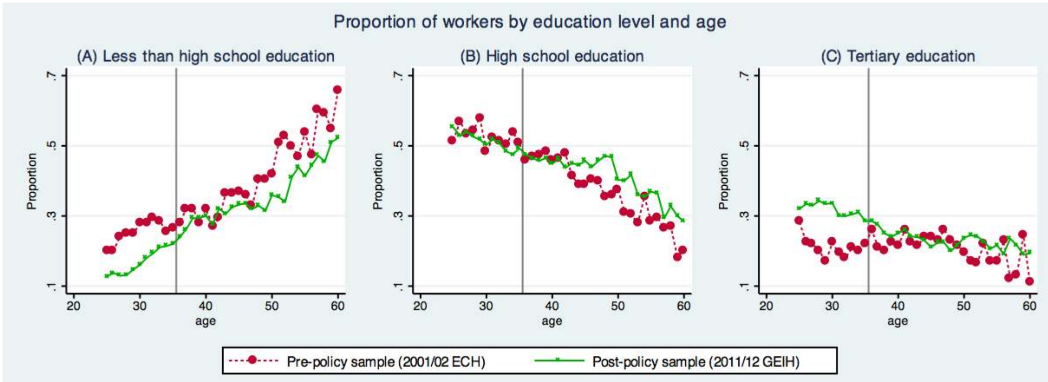


Table 3.1: Summary statistics

	Full sample	Pre-policy sample (ECH 2001/02)	Post-policy sample (GEIH 2011/12)
Age	29.66	30.06	29.50
	(2.04)	(1.99)	(2.04)
Male	0.554	0.564	0.550
	(0.497)	(0.496)	(0.497)
Married	0.591	0.610	0.584
	(0.492)	(0.488)	(0.493)
Basic education or less	0.166	0.241	0.136
	(0.372)	(0.427)	(0.343)
Secondary education	0.498	0.527	0.486
	(0.500)	(0.499)	(0.500)
Tertiary education	0.335	0.228	0.378
	(0.472)	(0.420)	(0.485)
Employed	0.987	0.987	0.987
	(0.113)	(0.113)	(0.113)
Self-employed	0.350	0.341	0.353
	(0.477)	(0.474)	(0.478)
Wage per hour	2,232.14	1,800.37	2,380.72
	(1,721.00)	(1,488.86)	(1,769.78)
Number of observations	80,056	19,135	60,921
Wage observations	40,992	9,637	31,355

Notes: Data for workers between 27 and 34 years of age for the pooled sample and by subsample, using sampling weights. Standard deviations in parentheses. Observations of wage per hour over the 95th percentile or under the 5th percentile were dropped. Wage per hour deflated to the 2001 value in Colombian pesos.

Figure 3.4: Estimated distribution of years of education for workers between 27 and 34 years of age

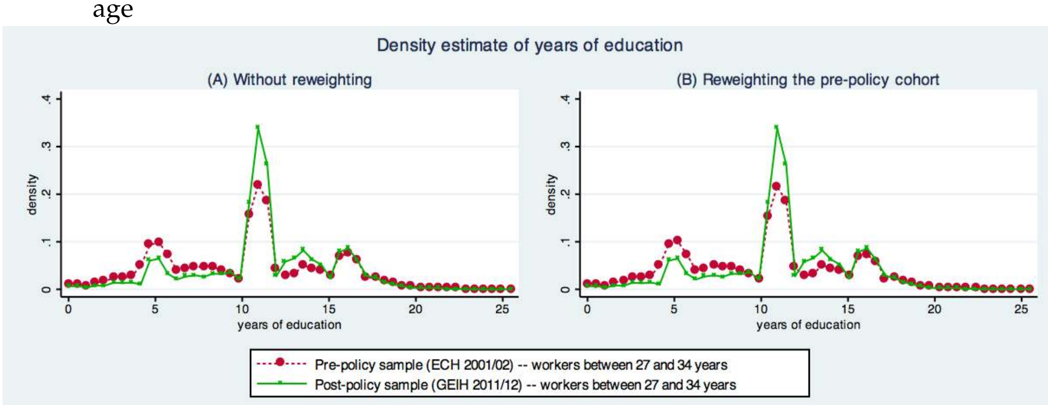


Figure 3.5: Differences in the distribution of years of education for workers between 27 and 34 years of age

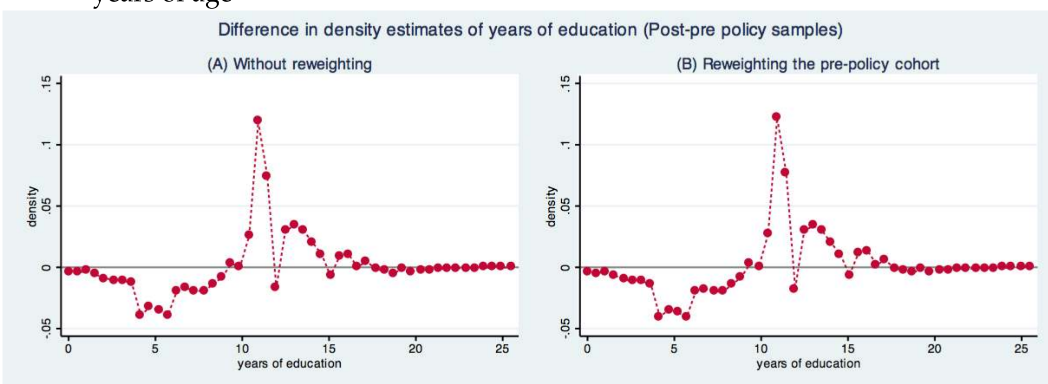


Figure 3.6: Estimated distribution of wages for workers between 27 and 34 years of age

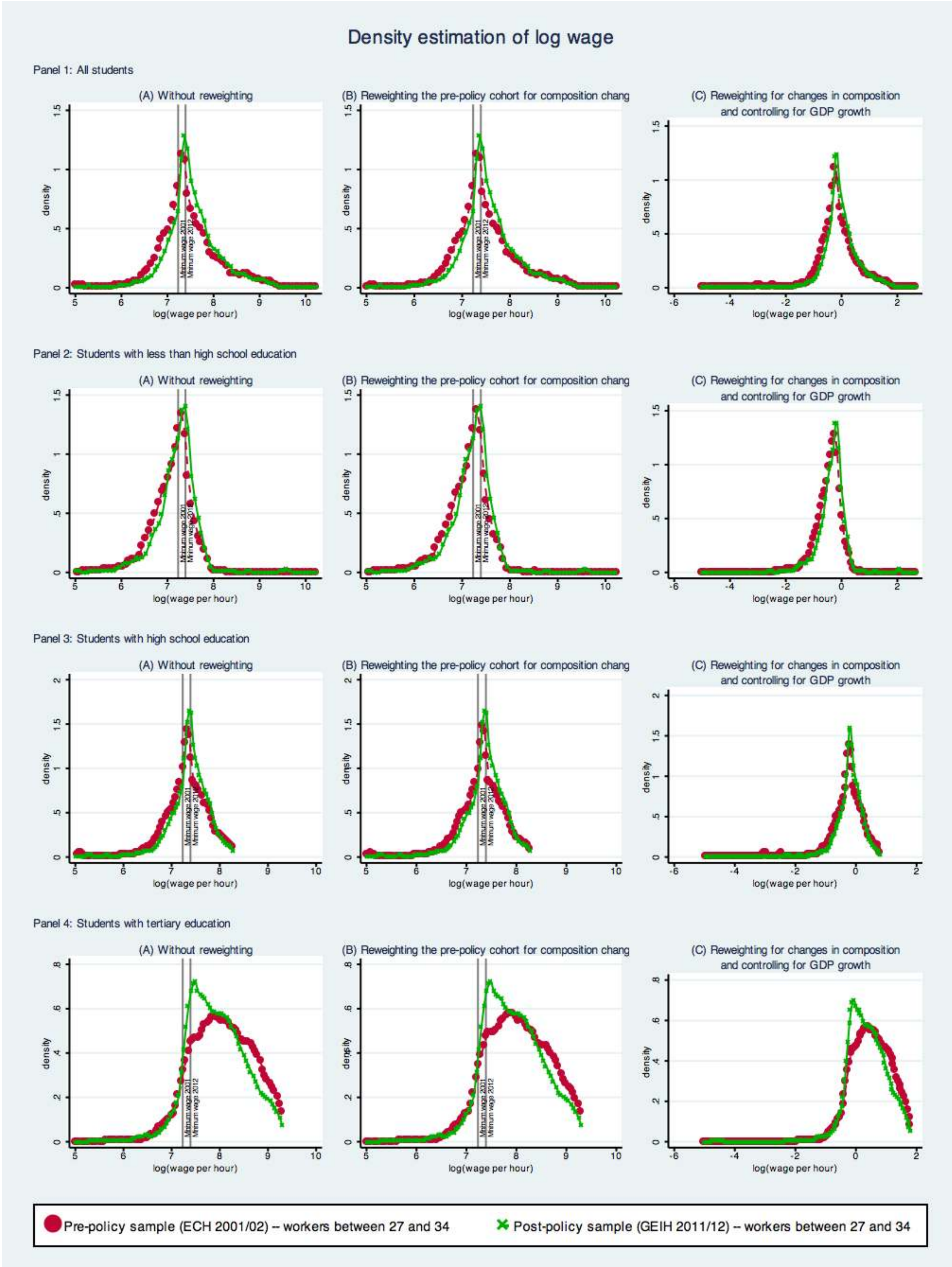


Figure 3.7: Differences in the distribution of wages for workers between 27 and 34 years of age

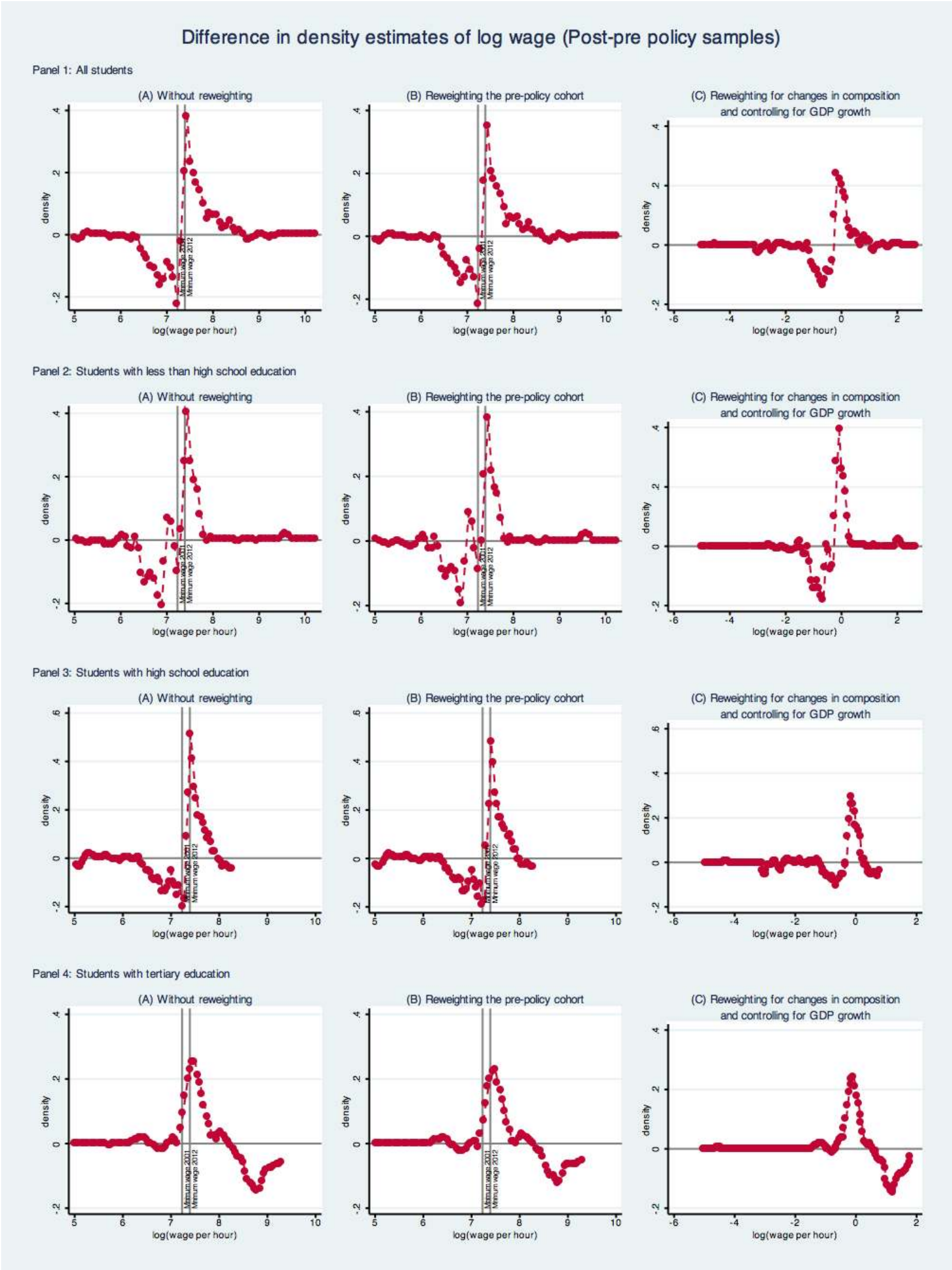


Figure 3.8: Changes in distribution of years of education (Treated sample, Control Sample, Differences)

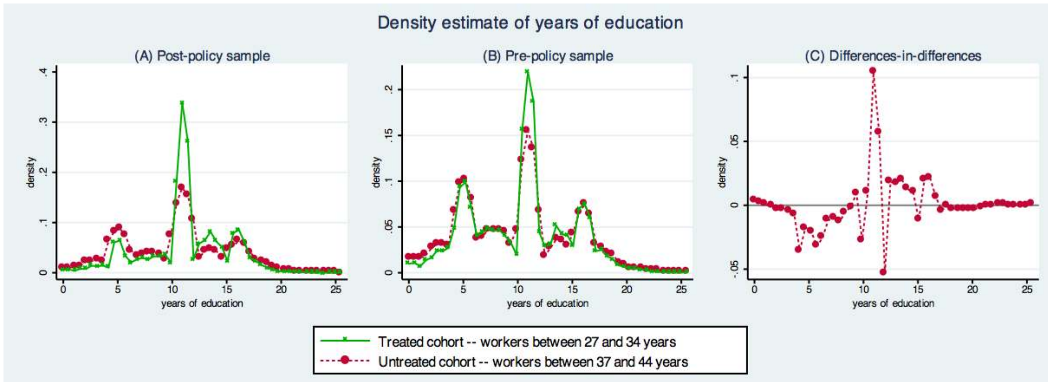
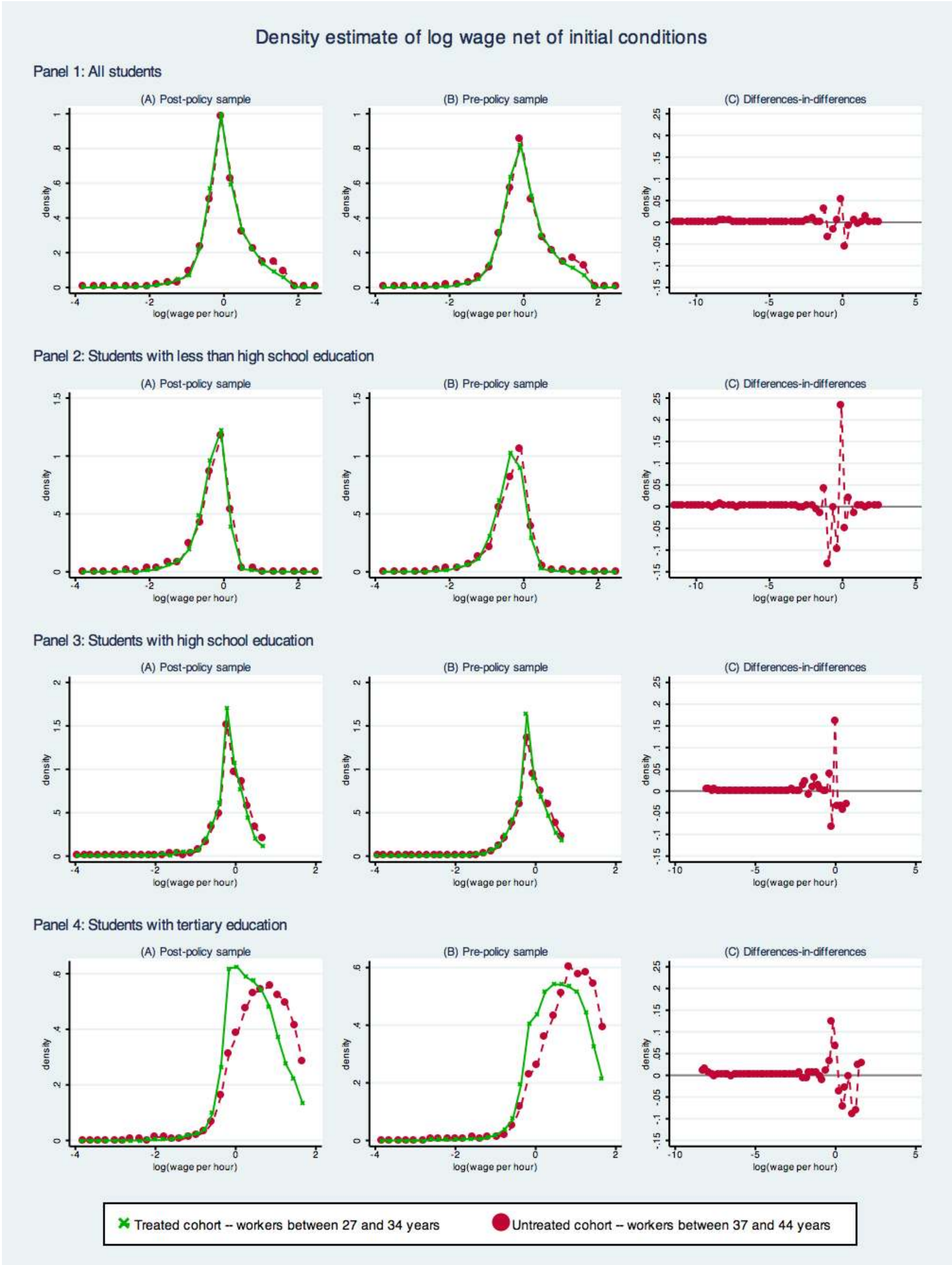


Figure 3.9: Changes in distribution of wages (Treated sample, Control Sample, Differences)



Notes: Density estimate of the residual of a regression of log hourly wage in 2001 Colombian pesos on the GDP growth on the year the worker entered the labor market for the first time, this means, net of the effects of initial labor market conditions.

Table 3.2: Differences-in-difference regressions of average log wage per hour

Dependent variable: log(wage per hour)	(1)	(2)	(3)	(4)	(5)
Panel A: All students					
Post × Treatment	-0.0607***	-0.0621***	-0.0117	-0.0139	-0.0111
	(0.0180)	(0.0179)	(0.0348)	(0.0347)	(0.0355)
Observations	90,455	90,455	90,400	90,400	90,390
R-squared	0.195	0.200	0.208	0.213	0.213
Panel B: Students with primary education or less					
Post × Treatment	-0.00179	-0.00629	0.0191	0.0144	0.118
	(0.0367)	(0.0374)	(0.0999)	(0.0982)	(0.0907)
Observations	17,609	17,609	17,609	17,609	17,609
R-squared	0.006	0.027	0.043	0.052	0.058
Panel C: Students with high school education					
Post × Treatment	-0.0180	-0.0186	0.0392	0.0369	0.0417
	(0.0161)	(0.0158)	(0.0251)	(0.0251)	(0.0257)
Observations	43,731	43,731	43,731	43,731	43,731
R-squared	0.003	0.023	0.025	0.032	0.032
Panel D: Students with tertiary education					
Post × Treatment	-0.0792*	-0.0797*	-0.0227	-0.0209	-0.00543
	(0.0475)	(0.0484)	(0.0878)	(0.0876)	(0.0899)
Observations	29,060	29,060	29,060	29,060	29,050
R-squared	0.008	0.013	0.016	0.021	0.021
Region FE	No	Yes	Yes	Yes	Yes
Experience polynomial	No	No	Yes	Yes	Yes
Demographic controls	No	No	No	Yes	Yes
Economic controls	No	No	No	No	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. Includes data for workers between 27 and 34 years of age and 37 and 44 years of age for the pre and post-policy samples.

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Appendix

Appendix for Chapter 1

Table A.1: RD coefficient for changes on student socioeconomic characteristics around the June 30th cutoff

	(1) Pre-policy	(2) Post-policy
(A) Socioeconomic index	0.011	0.011
	(0.010)	(0.008)
(B) Mother with basic education or less	-0.0001	-0.0038
	(0.004)	(0.003)
(C) Mother with high school education	-0.004	-0.002
	(0.004)	(0.004)
(D) Mother with university education	0.003	0.006**
	(0.003)	(0.003)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. These regressions include all cohorts. They use window of 90 days and include a linear spline of distance from birthday to enrollment cutoff.

Table A.2: McCrary test – RD coefficient for changes on density of birthday around the June 30th cutoff

	Jump at cutoff	
	(1) Pre-policy	(2) Post-policy
(A) Log share of students	-0.103***	-0.060*
	(0.001)	(0.001)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. These regressions include all cohorts. They use window of 90 days and include a linear spline of distance from birthday to enrollment cutoff.

Table A.3: Robustness – Alternative Student Classification – By municipality socieconomic index terciles

Variables	(1) 1^{st} SES tercile	(2) 2^{nd} SES tercile	(3) 3^{rd} SES tercile	(4) All students
Probability of enrollment in public schools	-0.0207**	-0.0337***	-0.00528	-0.0146***
	(0.0102)	(0.0108)	(0.00864)	(0.00520)
Socioeconomic index (1^{st} grade)	0.691	2.327**	1.150	-0.0972
	(0.874)	(0.957)	(1.763)	(0.824)
Class size	-0.215	-0.530***	-0.244	-0.332***
	(0.246)	(0.198)	(0.190)	(0.107)
Average years of teaching experience (1^{st}-8^{th})	-0.819***	-0.519***	-0.145	-0.411***
	(0.152)	(0.152)	(0.135)	(0.0725)
Average Math and Spanish standardized 4^{th} grade test scores	0.0534***	0.000167	0.00561	0.00347
	(0.0205)	(0.0199)	(0.0191)	(0.0115)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to enrollment cutoff and a full set of cohort dummies.

Table A.4: Robustness – Jump only in first year of policy

Variables	(1) Mothers with basic	(2) Mothers with high school	(3) Mothers with university	(4) All students
Probability of enrollment in public schools	-0.0168	-0.0274**	-0.00883	-0.0108
	(0.0175)	(0.0110)	(0.0176)	(0.00805)
Socioeconomic index (1^{st} grade)	-0.803	-2.181	-3.850	-1.969
	(1.583)	(1.425)	(4.489)	(1.269)
Class size	-0.427	-0.638***	-0.216	-0.573***
	(0.443)	(0.207)	(0.447)	(0.165)
Average years of teaching experience (1^{st}-8^{th})	-0.800***	-0.687***	0.0101	-0.535***
	(0.270)	(0.157)	(0.296)	(0.114)
Average Math and Spanish standardized 4^{th} grade test scores	0.0946**	-0.00986	0.0258	0.0147
	(0.0374)	(0.0216)	(0.0454)	(0.0174)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to enrollment cutoff and a full set of cohort dummies. Including students around the enrollment cutoffs for 2006 and 2007.

Table A.5: Robustness – Smaller bandwidth (30 days)

Variables	(1) Mothers with basic	(2) Mothers with high school	(3) Mothers with university	(4) All students
Probability of enrollment in public schools	-0.0191	-0.0195	-0.0170	-0.0148
	(0.0203)	(0.0126)	(0.0203)	(0.00909)
Socioeconomic index (1^{st} grade)	2.360	-0.290	9.848*	1.770
	(1.820)	(1.641)	(5.205)	(1.441)
Class size	0.272	-0.210	-0.233	-0.210
	(0.509)	(0.237)	(0.523)	(0.187)
Average years of teaching experience (1^{st}-8^{th})	-0.989***	-0.653***	-0.413	-0.581***
	(0.306)	(0.176)	(0.339)	(0.127)
Average Math and Spanish standardized 4^{th} grade test scores	0.0551	-0.0185	0.0732	0.0107
	(0.0434)	(0.0252)	(0.0546)	(0.0202)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to enrollment cutoff and a full set of cohort dummies.

Table A.6: Robustness – Linear spline changes between enrollment cutoffs

Variables	(1) Mothers with basic	(2) Mothers with high school	(3) Mothers with university	(4) All students
Probability of enrollment in public schools	-0.0158	-0.0155**	-0.0025	-0.0146***
	(0.0116)	(0.0072)	(0.0115)	(0.0052)
Socioeconomic index (1^{st} grade)	-0.086	-0.587	-0.643	-0.099
	(1.047)	(0.936)	(2.957)	(0.824)
Class size	-0.156	-0.515***	0.091	-0.332***
	(0.292)	(0.136)	(0.295)	(0.107)
Average years of teaching experience (1^{st}-8^{th})	-0.589***	-0.494***	0.012	-0.411***
	(0.175)	(0.101)	(0.194)	(0.073)
Average Math and Spanish standardized 4^{th} grade test scores	0.0835***	-0.0241*	0.0174	0.0034
	(0.0248)	(0.0144)	(0.0309)	(0.0115)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to the closest enrollment cutoff and a full set of cohort dummies.

Table A.7: Robustness – Fitting confounders in a linear trend

Variables	(1) Mothers with basic	(2) Mothers with high school	(3) Mothers with university	(4) All students
Probability of enrollment in public schools	-0.0185 (0.0153)	-0.0245*** (0.00945)	0.0102 (0.0149)	-0.0161** (0.00686)
Socioeconomic index (1^{st} grade)	-0.345 (1.385)	0.0403 (1.222)	-7.954** (3.812)	-0.678 (1.086)
Class size	-0.591 (0.386)	-0.816*** (0.179)	-0.0204 (0.381)	-0.581*** (0.141)
Average years of teaching experience (1^{st}-8^{th})	-1.268*** (0.232)	-0.993*** (0.132)	-0.0973 (0.250)	-0.821*** (0.0955)
Average Math and Spanish standardized 4^{th} grade test scores	0.124*** (0.0324)	0.0212 (0.0186)	-0.00963 (0.0390)	0.0445*** (0.0149)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to enrollment cutoff, a full set of cohort dummies and a linear trend.

Table A.8: Complier characteristics - family income and socioeconomic index distributions

	(1) Municipality distribution	(2) National distribution
Panel A: Family income		
1^{st} quartile	0.434	0.183
	(0.319)	(0.316)
2^{nd} quartile	0.729	1.438
	(0.367)	(0.638)
3^{rd} quartile	-0.130	-0.307
	(0.330)	(0.374)
4^{th} quartile	0.106	-0.729
	(0.341)	(0.590)
Panel B: Socioeconomic index		
1^{st} quartile	0.209	0.334
	(0.263)	(0.262)
2^{nd} quartile	0.882	0.290
	(0.415)	(0.286)
3^{rd} quartile	-0.173	0.406
	(0.365)	(0.310)
4^{th} quartile	0.305	-0.246
	(0.309)	(0.403)
Average	-46.82	-46.82
	(65.49)	(65.49)

Notes: Robust standard errors are in parentheses. Each coefficient represents a separate regression. Enrollment in voucher and public school are instrumented with the interaction of the dummy of turning six after the enrollment cutoff and the indicator for belonging to the treated cohorts.The regressions above use a window of 90 days, include all years and a full set of cohort dummies. They also include a linear spline of distance from birthday to enrollment cutoff.

Table A.9: Descriptive statistics by mother education level - Enrolled in post-policy period

	Mothers with basic education	Mothers with high school education	Mothers with university educ.
Panel A: Characteristics			
Proportion of actual eligible students	0.58	0.34	0.11
	(0.49)	(0.47)	(0.31)
Average socioeconomic index	-114.4	6.0	166.5
	(36.5)	(64.3)	(59.3)
Attend urban school	0.72	0.91	0.97
	(0.45)	(0.28)	(0.17)
Average category of family income	2.4	4.1	9.0
	(1.5)	(2.7)	(3.6)
Median monthly family income in category (2013 USD)	220	515	1,619
Panel B: National SES distribution			
1^{st} quartile	0.833	0.109	0.000
	(0.373)	(0.312)	(0.000)
2^{nd} quartile	0.159	0.342	0.001
	(0.366)	(0.474)	(0.028)
3^{rd} quartile	0.006	0.362	0.030
	(0.075)	(0.481)	(0.172)
4^{th} quartile	0.002	0.200	0.969
	(0.040)	(0.400)	(0.174)
Panel C: Municipality SES distribution			
1^{st} quartile	0.772	0.143	0.006
	(0.419)	(0.350)	(0.080)
2^{nd} quartile	0.198	0.319	0.056
	(0.398)	(0.466)	(0.231)
3^{rd} quartile	0.027	0.337	0.148
	(0.163)	(0.437)	(0.355)
4^{th} quartile	0.003	0.234	0.885
	(0.056)	(0.423)	(0.319)
Panel D: Enrollment choices			
Proportion enrolled in public schools	0.65	0.37	0.12
	(0.48)	(0.48)	(0.32)
Proportion enrolled in voucher schools	0.35	0.59	0.52
	(0.48)	(0.49)	(0.50)
Proportion enrolled in elite private schools	0.00	0.04	0.36
	(0.03)	(0.19)	(0.48)
Panel E: School characteristics at baseline			
Average copay in schools in 2005 (2013 USD)	2.5	12.2	31.1
	(8.0)	(19.8)	(31.1)
Average Math test scores in school in 2005	-0.23	0.08	0.58
	(0.43)	(0.47)	(0.46)
Number of observations	124,205	373,142	76,359

Notes: Standard deviations are in parentheses. The descriptives above include all students in 1^{st} grade in the post-policy years (2008-2010). The average socioeconomic index is a weighted average of mother years of education and family income normalized by municipality with mean equal to 100.

APPENDIX FOR CHAPTER 2

Table B.1: School choices: 3rd quartile of the municipality socioeconomic distribution – Full smaple

	ITT estimates		Difference in ITTs
	(1) 2009/10	(2) 2010/11	(3) Post-Pre
All students	0.019	-0.251**	-0.270
	(0.116)	(0.124)	(0.170)
Probability of enrolling in a low-performing school	-0.003	-0.019***	-0.015*
	(0.006)	(0.006)	(0.009)
Probability of enrolling in an average-performing school	-0.023*	-0.033**	-0.010
	(0.013)	(0.014)	(0.019)
Probability of enrolling in a high-performing school	0.025**	0.056***	0.032*
	(0.012)	(0.013)	(0.018)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1) including a full set of cohort dummies.

Table B.2: RD coefficient for changes on student socioeconomic characteristics around the June 30^{th} cutoff

	(1) Jump at discontinuity
(A) Socioeconomic index	2.140
	(1.893)
(B) 1^{st} quartile of Socioeconomic index in municipality	-0.00309
	(0.00809)
(C) 2^{nd} quartile of Socioeconomic index in municipality	0.00625
	(0.00810)
(D) 3^{rd} quartile of Socioeconomic index in municipality	0.00111
	(0.00808)
(E) 4^{th} quartile of Socioeconomic index in municipality	-0.00427
	(0.00754)
Below median of Socioeconomic index in municipality	0.00316
	(0.00919)
Above median of Socioeconomic index in municipality	-0.00316
	(0.00919)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. These regressions include all cohorts. They use window of 90 days and include a linear spline of distance from birthday to enrollment cutoff.

Table B.3: McCrary test – RD coefficient for changes on density of birthday around the June 30^{th} cutoff

	Jump at cutoff	
	(1) Pre-policy	(2) Post-policy
(A) Log share of students	-0.0497***	-0.0707***
	(0.00386)	(0.00427)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. These regressions include all cohorts. They use window of 90 days and include a linear spline of distance from birthday to enrollment cutoff.

Table B.4: Students by category

School year	2009	2010	2011
Total students in 1st grade	70,847	69,055	68,074
Average socioeconomic index (standard dev.=100)	29.5	30	28.2
Mothers with basic ed. or less	0.118	0.101	0.036
Mothers with high school or professional ed.	0.512	0.517	0.408
Mothers with university education	0.135	0.151	0.248
1st quartile of Socioeconomic index in municipality	0.266	0.262	0.258
2nd quartile of Socioeconomic index in municipality	0.248	0.251	0.272
3rd quartile of Socioeconomic index in municipality	0.275	0.27	0.254
4th quartile of Socioeconomic index in municipality	0.212	0.216	0.216
Below median of Socioeconomic index in municipality	0.513	0.513	0.53
Above median of Socioeconomic index in municipality	0.487	0.487	0.47

Table B.5: School demographics

School year	2009	2010	2011
Low-achieving schools (red)			
Total students in 1st-4th grades	35,376	32,342	29,939
Average socioeconomic index (standard dev.=100)	-56.3	-56.8	-58.4
1st quartile of Socioeconomic index in municipality	0.508	0.511	0.514
2nd quartile of Socioeconomic index in municipality	0.266	0.268	0.27
3rd quartile of Socioeconomic index in municipality	0.157	0.152	0.149
4th quartile of Socioeconomic index in municipality	0.069	0.069	0.067
Below median of Socioeconomic index in municipality	0.775	0.779	0.785
Above median of Socioeconomic index in municipality	0.225	0.221	0.215
Average-achieving schools (yellow)			
Total students in 1st-4th grades	166,472	160,456	155,242
Average socioeconomic index (standard dev.=100)	-2.8	-3.8	-5.9
1st quartile of Socioeconomic index in municipality	0.3	0.301	0.303
2nd quartile of Socioeconomic index in municipality	0.279	0.281	0.285
3rd quartile of Socioeconomic index in municipality	0.247	0.247	0.245
4th quartile of Socioeconomic index in municipality	0.173	0.171	0.167
Below median of Socioeconomic index in municipality	0.579	0.581	0.588
Above median of Socioeconomic index in municipality	0.421	0.419	0.412
High-achieving schools (green)			
Total students in 1st-4th grades	85,090	86,566	87,020
Average socioeconomic index (standard dev.=100)	123.8	121.4	117.1
1st quartile of Socioeconomic index in municipality	0.116	0.117	0.118
2nd quartile of Socioeconomic index in municipality	0.181	0.18	0.192
3rd quartile of Socioeconomic index in municipality	0.352	0.352	0.343
4th quartile of Socioeconomic index in municipality	0.351	0.35	0.347
Below median of Socioeconomic index in municipality	0.298	0.298	0.31
Above median of Socioeconomic index in municipality	0.702	0.702	0.69

Table B.6: Robustness – Alternative school classification – Probability of enrollment in average-performing schools (yellow) and in high-performing schools (green), 1st grade

	Average-performing schools			High-performing schools		
	(1) 2009/10	(2) 2010/11	(3) Post-Pre	(4) 2009/10	(5) 2010/11	(6) Post-Pre
All students	-0.00762	-0.0427***	-0.0351**	0.0187*	0.0421***	0.0234
	(0.0109)	(0.0111)	(0.0156)	(0.0102)	(0.0105)	(0.0147)
1st quartile of Socioeconomic index in municipality	0.0133	-0.0178	-0.0311	0.00682	0.00245	-0.00437
	(0.0238)	(0.0249)	(0.0344)	(0.0182)	(0.0206)	(0.0275)
2nd quartile of Socioeconomic index in municipality	-0.0419*	-0.0219	0.0199	0.0445**	0.0245	-0.0200
	(0.0238)	(0.0253)	(0.0347)	(0.0212)	(0.0238)	(0.0319)
3rd quartile of Socioeconomic index in municipality	-0.0233	-0.0813***	-0.0579	0.0151	0.102***	0.0874**
	(0.0244)	(0.0266)	(0.0361)	(0.0242)	(0.0263)	(0.0358)
4th quartile of Socioeconomic index in municipality	-0.0123	-0.0566**	-0.0444	0.0227	0.0660**	0.0432
	(0.0274)	(0.0281)	(0.0393)	(0.0276)	(0.0285)	(0.0397)
Below median of Socioeconomic index in municipality	-0.0143	-0.0209	-0.00665	0.0254*	0.0163	-0.00916
	(0.0168)	(0.0178)	(0.0244)	(0.0140)	(0.0159)	(0.0212)
Above median of Socioeconomic index in municipality	-0.0180	-0.0696***	-0.0516*	0.0179	0.0851***	0.0673**
	(0.0183)	(0.0194)	(0.0267)	(0.0183)	(0.0195)	(0.0267)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses. Each coefficient represents a separate regression. The regressions above use a window of 90 days and include only students attending schools in the Santiago metropolitan area. They include a linear spline of distance from birthday to enrollment cutoff. Column (1) shows RD estimates using student observations within 90 days of the enrollment cutoff for year 2009, while column (2) for year 2010. Column (3) shows the differences in the RD coefficients for column (2) with respect to column (1).

Table B.7: Robustness – Including 2008/09 cohort in pre-policy years

Variables	(1) All students	(2) 1^{st} quart. SE	(3) 2^{nd} quart. SE	(4) 3^{rd} quart. SE	(5) 4^{th} quart. SE	(6) Below SE median	(7) Above SE median
Probability of enrollment in low-performing schools	0.00360	0.00733	0.0187	-0.0333**	-0.00973	0.0115	-0.0231**
	(0.00855)	(0.0233)	(0.0186)	(0.0136)	(0.0128)	(0.0150)	(0.00945)
Probability of enrollment in average-performing schools	-0.0451***	-0.0336	-0.0302	-0.0922***	-0.0268	-0.0337	-0.0628***
	(0.0137)	(0.0301)	(0.0310)	(0.0322)	(0.0350)	(0.0216)	(0.0237)
Probability of enrollment in high-performing schools	0.0448***	0.0357	0.0153	0.129***	0.0373	0.0288	0.0878***
	(0.0129)	(0.0238)	(0.0285)	(0.0319)	(0.0353)	(0.0188)	(0.0237)
Average class size (1^{st} grade)	-0.0902	-0.230	-0.377	0.444	0.524	-0.295	0.492
	(0.506)	(0.512)	(0.550)	(0.353)	(0.375)		
Socioeconomic index (1^{st} grade)	0.0753***	0.00527	0.0579	0.234***	0.115**	0.0396	0.180***
	(0.0240)	(0.0388)	(0.0515)	(0.0589)	(0.0568)	(0.0330)	(0.0413)
School integration index (1^{st} grade in %)	-1.045**	0.695	-0.696	-2.141**	-1.491	0.0750	-1.866**
	(0.421)	(0.914)	(0.708)	(0.894)	(1.328)	(0.585)	(0.783)
Average years of teaching experience	-0.345**	-0.414	-0.671*	-0.573*	-0.623*	-0.564**	-0.605**
	(0.151)	(0.350)	(0.347)	(0.330)	(0.364)	(0.247)	(0.245)
Math standardized 4^{th} grade test scores	-0.0284	-0.00590	-0.0811	0.0250	-0.0580	-0.0375	-0.0132
	(0.0302)	(0.0643)	(0.0629)	(0.0619)	(0.0644)	(0.0453)	(0.0447)
Spanish standardized 4^{th} grade test scores	0.00915	0.000652	-0.0374	0.0545	0.0455	-0.0125	0.0498
	(0.0297)	(0.0628)	(0.0623)	(0.0612)	(0.0637)	(0.0445)	(0.0442)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. The regressions above use a window of 90 days, include a linear spline of distance from birthday to enrollment cutoff. Includes students around the enrollment cutoffs for 2008, 2009 and 2010.

Table B.8: Robustness – Smaller bandwidth (45 days)

Variables	(1) All students	(2) 1st quart. SE	(3) 2nd quart. SE	(4) 3rd quart. SE	(5) 4th quart. SE	(6) Below SE median	(7) Above SE median
Probability of enrollment	-0.0176	0.0175	-0.0127	-0.0618***	-0.0187	-5.99e-05	-0.0430***
in low-performing schools	(0.0139)	(0.0377)	(0.0299)	(0.0213)	(0.0217)	(0.0242)	(0.0152)
Probability of enrollment in	-0.0197	-0.0736	0.000217	-0.0452	0.0391	-0.0379	-0.00889
average-performing schools	(0.0221)	(0.0484)	(0.0493)	(0.0519)	(0.0562)	(0.0346)	(0.0382)
Probability of enrollment in	0.0370*	0.0642*	0.0106	0.108**	-0.0165	0.0410	0.0541
high-performing schools	(0.0208)	(0.0381)	(0.0451)	(0.0512)	(0.0567)	(0.0298)	(0.0381)
Average class size (1st grade)	0.347	0.329	-0.0365	0.609	0.861	0.162	0.761
	(0.359)	(0.786)	(0.817)	(0.837)	(0.878)	(0.567)	(0.607)
Socioeconomic index	0.0511	-0.0279	0.0766	0.201**	0.0333	0.0346	0.128*
(1st grade)	(0.0382)	(0.0613)	(0.0807)	(0.0940)	(0.0910)	(0.0518)	(0.0660)
School integration index	-0.202	1.963	-0.717	-2.132	-0.655	0.780	-1.558
(1st grade in %)	(0.669)	(1.483)	(1.101)	(1.382)	(2.100)	(0.936)	(1.227)
Average years of teaching	-0.472*	0.0434	-1.012*	-0.813	-1.363**	-0.516	-1.075***
experience	(0.242)	(0.560)	(0.551)	(0.529)	(0.584)	(0.395)	(0.393)
Math standardized	0.0413	0.0283	0.0374	0.121	-0.0461	0.0406	0.0510
4th grade test scores	(0.0496)	(0.106)	(0.102)	(0.102)	(0.107)	(0.0741)	(0.0737)
Spanish standardized	0.0723	0.0977	0.0568	0.109	0.0128	0.0820	0.0715
4th grade test scores	(0.0486)	(0.103)	(0.101)	(0.0990)	(0.107)	(0.0728)	(0.0726)

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses. The regressions above use a window of 45 days, include a linear spline of distance from birthday to enrollment cutoff.

Table B.9: Enrollment within type of student

School year	2009	2010	2011
(A) All students			
Enrolled in low-achieving schools (red)	0.12	0.108	0.104
Enrolled in average-achieving schools (yellow)	0.572	0.566	0.563
Enrolled in high-achieving schools (green)	0.303	0.316	0.321
(B) 1st quartile of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.202	0.18	0.159
Enrolled in average-achieving schools (yellow)	0.655	0.663	0.659
Enrolled in high-achieving schools (green)	0.136	0.147	0.173
(C) 2nd quartile of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.113	0.104	0.084
Enrolled in average-achieving schools (yellow)	0.662	0.645	0.609
Enrolled in high-achieving schools (green)	0.22	0.239	0.297
(D) 3rd quartile of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.065	0.051	0.045
Enrolled in average-achieving schools (yellow)	0.526	0.522	0.503
Enrolled in high-achieving schools (green)	0.403	0.415	0.439
(E) 4th quartile of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.034	0.03	0.027
Enrolled in average-achieving schools (yellow)	0.477	0.445	0.434
Enrolled in high-achieving schools (green)	0.487	0.516	0.524
(F) Below median of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.159	0.143	0.121
Enrolled in average-achieving schools (yellow)	0.658	0.654	0.633
Enrolled in high-achieving schools (green)	0.177	0.192	0.236
(G) Below median of Socioeconomic index distribution in municipality			
Enrolled in low-achieving schools (red)	0.051	0.041	0.037
Enrolled in average-achieving schools (yellow)	0.505	0.488	0.471
Enrolled in high-achieving schools (green)	0.439	0.46	0.478

APPENDIX FOR CHAPTER 3

Figure C.1: Average wages by age

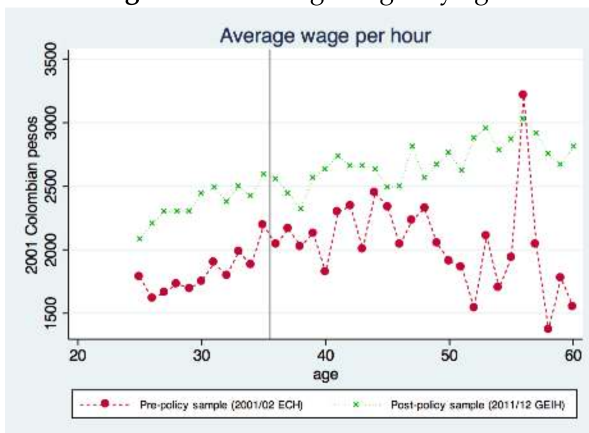

Figure C.2: Average wages per hour by age and education level in ECH 2001/0202 and GEIH 2011/12