Extra Resources for Poor Schools: Impact on Teachers and Students

José María Cabrera*
Dinand Webbink**
September 30th, 2014.

Abstract
Countries around the world have tried to improve educational outcomes of students in deprived neighborhoods. We study the impact of a program that gave extra resources to public schools that work with vulnerable populations, using an administrative panel of all Uruguayan primary schools in the period 1992-2010. An interesting feature of the program is that it offered a salary increase for teachers. We use a sharp regression discontinuity design and panel data techniques to identify the effects of the program. With both strategies we show consistent evidence that treated schools were able to hire teachers with more years of experience. In the RD analysis we conclude that the monetary incentives for teachers caused an increase of three years in average experience (¾ of a s.d. increase). Panel data analysis shows a positive and significant effect, but of less magnitude, that lasts for three years. Extra funding had no consistent impact on students attendance, grade retention nor dropouts. Our analysis is a contribution to the literature on school resources, teacher experience and their effect on students outcomes in the context of a developing country.
JEL: H52, I2

---

* jmcabrera@um.edu.uy
** webbink@ese.eur.nl
Financial support from ANII (POS_EXT_2012_1_9992) to José María Cabrera is appreciated.
We thank the staff of the Departamento de Investigación y Estadística Educativa de ANEP, in particular Alejandro Retamoso, their coordinator, for providing the data and for useful guidance. Several officials from ANEP also provided us with information and feedback: Hector Bouzón, Luis Petrelle and Rosario Boggio. We are grateful to Juan Dubra, Martin Rossi, Alvaro Forteza and Giorgio Chiovelli for their comments and suggestions. Participants from XXIX Jornadas Anaules de Economía del Banco Central del Uruguay and Universidad de Montevideo also gave use useful feedback. All errors remain our own.

I. Introduction

Socioeconomic background is strongly associated with student performance. For example, across OECD countries, on average the performance difference between students from advantaged (the top quarter of socio-economic status) and disadvantaged (the bottom quarter of socio-economic status) backgrounds is 90 score points in PISA: the equivalent of more than two years of schooling. In Uruguay this difference is 107 points. Another way to look at the relation between students socioeconomic background and performance is that in OECD countries more than 60% of the performance differences between schools is explained by the socioeconomic backgrounds of students. In Uruguay 74% of schools variation in performance is so explained (OECD, 2013).

To mitigate the handicap that poor students bring from home, governments usually implement policies to close the gap between students from poor and rich backgrounds. One way to achieve the goal of providing equitable learning opportunities is to give extra resources to schools that work with underserved populations. We will study on of such programs, implemented in Uruguay: the *Contexto Socio Cultural Crítico (CSCC) program.* Unfortunately, policies that provide schools with extra resources don't always have a positive impact on students outcomes. Hanushek (2006) review indicates that there is little consistent relationship between resources to schools and student achievement, and that results are similar across both developed and developing countries.

It is also generally acknowledged that teachers play a main role in helping students (Chetty, Friedman and Rockoff, 2014), although how to detect who is a good teacher (with observable characteristics) is challenging (Rivkin, Hanushek and Kain, 2005). Placing better teachers in poor schools may be a way to overcome the gap. Without resting importance to the vocational inspiration or the desire to face challenges that moves some experienced teachers to work in underperforming schools, monetary incentives can play a role in attracting teachers to work with poor students.

In this paper we will try to answer two questions: can schools "buy" more teacher experience?; and, does extra resources for poor schools improve students attendance and reduce grade retention and dropouts? We will provide answers using an administrative panel of all public primary schools in Uruguay, spanning 1992-2010. One interesting feature of the program is that it changed the incentives teachers face when selecting where to work: they were given extra money if they decided to teach in poor schools.

When analyzing these types of education policies there is a clear endogeneity issue: poor schools have, on the one hand, underperforming students, and, on the other hand, are the ones who are treated by the programs. So there is a risk of attributing a negative impact of the program on students' performance, when it may be entirely due to selection. We employ two main identification strategies to try to give causal interpretation to our findings: a regression discontinuity design (RD) and panel data regressions with school fixed effects. In the RD strategy, the schools that form the control group to test the impact of the program are the ones located in the neighborhood of the cutoff: by random chance ones were barely above the cutoff, and so received the treatment, while similar schools just below the cutoff didn't participate. In the panel data strategy, using school and year fixed effects, the control is the same school, so we get rid of the individual (fixed) unobserved heterogeneity. Both methodologies have a different set of assumptions and help us answer our research questions from different points of views and using different subpopulations.

In the year 2005 the *CSCC* program had a clear cut-off rule to assign the extra resources and it had perfect compliance: all schools with a score above a certain threshold entered the program, and no one at the left of the cutoff was treated. The score was based on students family characteristics. In the years previous to 2005, the allocation rule was not perfectly enforced, so we try to test our hypothesis using a fuzzy-RD design.

During the period 1995-2010 there were 1.005 reallocations of schools, in and out of the program. We use this variation to perform a panel data analysis. With this second empirical strategy we can overcome one of the main limitations of RD, namely the local interpretation of the impact: although RD has a strong internal validity, is becomes challenging to extrapolate results away from the cutoff (low external validity).

The evidence we provide consistently shows a positive and significant impact of monetary incentives on teacher experience at the school level. The impact on students is small and generally non-significant. Specifically, we find that in 2010 and using a sharp RD design, teachers experience at treated schools increased in approximately 3 years (depending on the specification). This is a big increase: ¾ of a standard deviation of the mean of teacher experience, or more than moving a school from the poorest quintile to the richest quintile in 2010. It also implies that treated schools can increase the percentage of teacher that have at least 9 year of experience in around 22%. There was no significant effect on teachers tenure. These results are replicated using information at the teacher level. In the panel data analysis we can use the variation in schools that are away from the cutoff, so we can estimate the impact for a larger population (and for more years). This analysis shows that schools that receive the extra resources may hire teachers with more than one year of experience. This result is robust to the inclusion of school and year fixed effects. There is also a positive impact on teachers tenure at treated schools. The current impact is still visible after three years (but decreases over time). The effect of the program on students is zero using the RD analysis and the panel data structure from 1992-2010. However, it is interesting to notice that the panel data results (by accounting for schooled fixed unobservable heterogeneity) are drastically different from a simple OLS (negative impact of the program due to selection of poor schools into the treatment). Finally, using information at the teacher level we confirm the panel data estimates with more precision. Similarly, employing data at the grade level (more than 87.000 observations), we confirm that the zero impact on students is not due to a lack of power, but that the program had really no effect on them.

Our paper contributes to the literature on the effects of school resources and to the literature on teachers (experience). Previous studies that look at the relation between extra resources and students achievement have shown mixed results. Card and Payne (2002) use legislatively-induced school finance reforms in the USA and find that increased funding of low-income districts leads to a narrowing of test score outcomes across family background groups. Chay, McEwan and Urquiola (2005) study the impact of a program in Chile that in 1990 allocated extra resources to schools with low test score results. They show that the program had a positive impact on students, but smaller than the one estimated with a difference-in-difference framework. A regression discontinuity design allows them to circumvent the problem of noise (transitory shocks) and mean reversion in test scores that were employed for the assignment to the treatment, and are also the main outcomes of interest. Bénabou, Kramarz and Prost (2009) present evidence that the French program (zones d'éducation prioritaire) had no impact on 6^{th} and 7^{th} grade students academic achievement (between 1982 and 1992). Van der Klaauw (2008) studies the impact of Title I funding for high-poverty schools in New York City. He finds a negative impact of the extra resources in the early years of the program (1993 and 1997) and no effects later on (2001). Leuven, Lindahl, Oosterbeek and Webbink (2007) find that a program in the Netherlands that gave extra resources (personnel and computers) to schools with disadvantaged pupils had zero or even a negative impact on students achievement.

Regarding teachers, experience seems to be the only observable teachers characteristic that matters for performance, and only to some extent. Rivkin, Hanushek and Kain (2005) have shown that only the first years of experience make a difference in students outcomes: the learning curve for teachers appears to be quite steep in the first year or two of teaching, before flattening out. So, for Texas public schools, beginning teachers perform significantly worse than more experienced teachers, but after that period, experience is not related with teacher quality. A recent study on teacher experience by Wiswall (2013), using data from North Carolina, indicates that teacher experience is not only important in the first years of the career but also in later years. Also Harris and Sass (2011) find that teacher productivity increases with experience (informal on-the-job training), but not with formal professional development training. The largest gains from experience, for teachers in Florida, occur in the first few years, but they find continuing gains beyond the first five years of a teacher's career.

We contribute to the literature with two main results: 1) teachers react to monetary incentives, so schools can have a more experienced staff by offering higher salaries; 2) extra resources for schools don't have a strong impact on students outcomes (across the dimensions we measure). We also contribute by answering the questions using two different methodologies: a sharp regression discontinuity design and panel data with school fixed effects, which have different identification assumptions and external validity. Finally, our analysis for a developing country adds to the literature since most previous studies have been performed for rich countries.

The rest of the paper is organized as follows. In section II we present the institutional background of the Uruguayan educational system, and the characteristics of the *CSCC* Program. In section III we describe the data, and in section IV we explain the empirical strategy. The program evaluation is performed in sections V (with a sharp regression discontinuity design) and in section VI (with panel data with fixed effects). We present the effects on teachers and students, along with robustness checks and variations in the main specifications. In section VII we provide a discussion of the implications of the analysis, and we conclude in section VIII.

II. Institutional background and the *CSCC* Program

Primary education in Uruguay has a universal coverage and the majority of children attend schools form the public system (85.4% between 1993 and 2010). There were roughly more than 2,000 primary schools operating in the country in each year in the period under analysis. Public schools are grouped in 5 main categories: Rural (*Escuelas Rurales*), Standard Urban (*Urbanas Comunes*), CSCC (*Contexto Socio Cultural Crítico*), Dobule Shift (*Tiempo Completo*) and Practice (*Habilitadas de Práctica y Práctica*). These schools cover 2 years of preschool education *(Educación Inicial)* and 6 years of primary school (*Escuela Primaria)*[1]. In Graph 1 and Table 1 we show the composition and evolution of the number of schools in each category. Rural Schools are very small schools located in the countryside. Although they are the majority of school facilities (56%), they cover a very small fraction of students (7.2% in the period under analysis). The typical rural schools has on average only 1.4 teachers in charge of the 6 grades of primary education, and has on average 18 students aged 6 to 12 years old. The majority of Uruguayan primary school students attend Standard Urban schools. These schools

[1] In the last years, and in some schools, there is also a special program for handicapped students (Educación Especial) that operates in special schools, or integrating these students to the common schools.

had, in the period, 344 students on average (44 in preschool and 300 in primary), 10.1 teachers in grades 1-6th, and an average class size of 25.5 students in primary. These *Standard Urban* schools are the ones that could receive the extra resources from the program that we will evaluate. They have steadily decreased in number, from 792 schools in 1992 to 400 in 2010. The same building facility of the *Standard Urban* school was used to implement one of the other types of schools that started to operate in that decade: *CSCC* schools, *Double Shift* and *Practice*.

The program that we will study is called *Contexto Socio Cultural Crítico (CSCC* that stands for Critical Socio-Economic Context)[2]. The schools that are assigned to this program are located in deprived and poor areas of the country. We will explain now the dynamics of the program, and in the next section how schools receive the resources and what these extra resources were. The assignment criteria to the program have changed during the years, but it retains the goal: compensate the handicap that students with a poor family background have. A mechanism to increase these students performance is to provide those poor schools located in disadvantaged environments with extra resources.

In the year 1995 the first 155 schools started to participate in the *CSCC Program* (Table 1). The great majority (147) were schools that were already functioning when they entered the program. In that first year of operation, schools were assigned based on school indicators of poor performance (i.e. grade retention) and characteristics of the houses in the neighborhood were each school was located (based on information from census from the National Institute of Statistics). This first allocation of schools didn't use direct measures of socioeconomic characteristics of the students attending the schools that were going to be treated (ANEP 2005).

The program had an expansion between 1998 and 1999. In 1998 we have 28 new schools in the program, and in the following year there was a big jump: 171 poor schools received the extra resources, and the program reached 273 schools (Table 1 and Table 2). In the 1999 reallocation, schools entered or left the program based on three indicators: grade retention, insistences of students in 1st grade, and the percentage of students in 6th grade whose mothers had primary education as the highest level of formal education (with data from 1996) (ANEP 2005).

In 2001 the program was reduced to 106 schools. In the 2002 reallocation, the program had a 32% net increase in the number of schools participating, when 85 new schools jointed the program (80% increase considering the schools in the previous year), and 51 schools were dropped (48% of the schools in the previous year). The criteria used for the assignment to the *CSCC* program were based exclusively on socioeconomic variables of students[3].

From 2003 to 2005 the number of schools participating was around 150. In 2005 a new categorization of schools was made, and in the following years new schools entered the program, and others left because families living in the neighborhoods in which they were located improved their socioeconomic status. The increase in the coverage rate of the program that started in 2006 ended in 2009,

[2] The compensation program changed its name during the period. Between 1995 and 1999 it was called *Requerimiento Prioritario*; from that year until 2011 it was named *Contexto Socio Cultural Crítico*. Since 2011 the compensation program for poor schools is named A.PR.EN.D.E.R. (in Spanish means "to learn"), and is an acronym for "Atención Prioritaria en Entornos con Dificultades Estructurales Relativas". Throughout the document we will always label the Program *CSCC*.

[3] The variables used by the central authorities were the percentage of children: (i) with unemployed household heads (or doing very informal jobs); (ii) whose mothers didn`t achieve primary education; (iii) that were allowed to receive free lunch at school; and (iv) that lived in overcrowded houses. Each state in the country had a fixed slot of schools that were going to receive the extra resources, based in the number of students in the state and the percentage of children between 4 and 12 years old in the poorest quintile of the distribution of income (ANEP 2005).

when 285 schools were receiving the extra resources, and the number of schools participating reached it's maximum in 10 years. The 2005 re-categorization (schools moving in and out of the program) used indicators of human capital (level of education of mothers or, in her absence, the adult in charge of the children), socioeconomic level[4], and an index of social integration[5]. All this information was summarized (using factor analysis for data reduction) in a single measure that described the context of each school (cfr ANEP 2005). All the schools were then ranked according to this unique index, which is the forcing variable that we will use to perform the RD analysis.

In Table 2 we show the year and the number of schools that were transformed to each category, and from which categories they came from, between 1992 and 2010[6]. Most schools that entered the *CSCC* Program were previously Standard Urban Schools (Graph 2 and Table 2).

The overall picture of Table 1 and Table 2 shows variation in the *CSCC* treatment during 16 years, and we can argue that this variation, to a great extent, was exogenous to the decisions at the school level management. There were 1.005 school reallocations (schools considered in our analysis that entered or left the program). This high rotation is also reflected in the fact that from the 155 initial school that inaugurated the program in 1995, only 19 were part of the program during 16 consecutive years (during the whole period 1995-2010). This within school variation will be the identification strategy in the panel data section.

The *CSCC* Program

In the previous section we have explained the evolution of the number of schools participating in the program. Now we will explain the functioning and content of the program. It has been implemented since 1995 at the national level, and there are *CSCC* schools in every state of the country. To enter the program, the schools didn't need to apply. Treated schools were determined by the education system central administration: individual schools didn't have autonomy neither to opt in the program nor refuse to participate. The program was targeted to the schools in more disadvantaged neighborhoods. There was an entering rule that varied in the years, as we have already explained.

The Program had several channels to increase resources in poor schools. It gave them more equipment and didactic materials and improved lunchrooms. It also included more time for coordination between teachers (aimed at institutional development activities, curriculum planning, coordination on program content and evaluation criteria, etc.) and (voluntary) training sessions for teachers. These components of the program are similar to the ones implemented in many countries, and in particular to the Chilean experience studied in Chay, McEwan and Urquiola (2005). Unfortunately, like in their data, there is no administrative record of what each individual school received[7]. So we cannot

---

[4] Poverty mas measured with an unmet basic needs index, constructed with information on overcrowded homes, the materials used to construct the house, where families obtain water to drink, and the sanitary services of the house.

[5] This index was constructed with information of integration to the territory (percentage of students living in illegal land), integration to the education system of brothers and sisters of the students; and integration of the household head to the labor market.

[6] For example, in the year 2006 39 schools jointed the program, and 4 were dropped, in 2007 these numbers were 81 and 45, respectively, in 2008: 61 and 2; in 2009: 13 and 8. In 2010 there were no changes, and 285 schools were participating.

[7] It is also difficult to reconstruct the information of how much extra money was spent in each year (on top of the usual resources that the schools receive). Each year the Parliament assigns the resources for education (and other areas of public expenditure). The money for the CSCC Program comes from the general budget allocated to the public education system, and <u>also</u> from special amounts of money that are directly assigned to the Program (this is done in order to have more control

disentangle the effect of each individual component. One difference with their treatment is that in the Uruguayan program there was a monetary incentive for teachers working at treated schools[8]. This monetary incentive was delivered to teachers monthly during all the years of the intervention (is key a feature of the program that is present in every year). Finally, there was no specialized curriculum, nor was the length of the school week altered or other characteristics of the time at school (elements that could result in different impact on students at treated and control schools).

Schools are not given the money in cash and are not free to decide how to spend it. The incentive for teachers is determined by the central authority, so this reduces discretionality and variations due i.e. to differences on what principals decide to do, so the program was homogeneously implemented. This monetary compensation is equivalent to a 25.7% of the basic salary for a teacher of first grade in the payment scale[9]. The policy of monetary incentives had the explicit aim of encouraging the establishment of more experienced and better qualified teachers in poor schools. The extra salary was not tied to teacher performance.

In Table 3 we present teachers salaries from 1997 to 2010, for three representative grades of the payment scale. When a new teacher enters the system in Grade 1, she earns for 20hs of work, approx. two minimum national wages (monthly nominal salary in 2010 was us$615). The wage structure along the teaching career is relatively flat (as it happens in other countries[10]). The incentive to work in a *CSCC* school is a 25.7% increase in the base salary for a 1st Grade Teacher with 20hs (the base salary is then increased with several adjustments –that have varied with the years-, that increase the salary in around 70%; so the incentive is around 15% of the full salary)[11]. Although the incentive decreases as a percentage of the salary across the categories (since it is a fixed amount), it still represents a sizable percentage increase for every category, given the flat structure of wages. Another way to look at the magnitude of the incentive is that every four years a teacher can advance one step in the teaching

on whether those resource were spent exclusively in the Program). Own calculations based on the yearly budget directly allocated by the Parliament to the Program account for aprox. 58 million dollars for the period 2007-2010. This figure sets a floor to the extra money that was spent in this program, since the money that comes from the general Budget of public education is not included in that figure. To have another dimension of the Program, in the year 2010 there were paid 4.899 *monthly* extra salary compensations, and in the previous years: 2009: 5.121, 2008: 5.152, etc.

[8] In the French case studied by Bénabou, Kramarz and Prost (2009), ¼ of the resources were used to pay bonuses to all employees of the schools (mostly teachers). But, unlike our case, teacher bonus was very small: approximately equal to 2% of the average teacher wage (in 1990–91).

[9] For principals of the *CSCC* schools, the incentive is equivalent to 15% of the salary in Grade 2 of the Principals payment scale (cfr. ANEP 2004).

[10] This is not an exclusive feature of the Uruguayan education system, since this flat structure of salaries is very common across several countries. Teacher salaries in the seventh grade (after 24 years of work) are equivalent to 6 minimum wages (approx.), and in the first grade they were approx. 4 minimum wages (considering a work week of 40hs). Teachers can increase their salaries when they move to technical positions outside the classroom, or by working in other dependencies of the Government, since primary school teachers are public officials. There are also salary increases when a teacher reaches 25, 28 or 32 years of activity (aprox 18%, 23% and 28%, respectively, increase in salary compared to a Grade 7).

Teachers at *CSCC* schools are required to attend one or two coordination meetings (workshops) a month on Saturdays (it varied with the years). But this extra day of coordination was voluntary during many years, and in the years that it was mandatory to attend, the monetary incentive more than compensated this extra work, since it was specifically designed as an incentive to attract more experienced teachers.

[11] These percentages are approximations, since the calculation of teacher salaries is not very transparent (there is little information for the period under analysis, and currently there are more than 20 additions and complements over the base salary). The information we presented in Table 3 includes food complements and other additional, but the incentive is not calculated on top of that; still the percentages are informative and indicate that the incentives are big in relative terms.

scale. After 12 years of work, she can reach the 4th grade: this represents an average increase of 15% in her base salary[12]. So, a teacher has a clear incentive to work in *CSCC* at the cutoff (were treated and non-treated schools are very similar), and this mechanism will drive our main result (treated schools could increase their average teaching staff experience).

Only certificated teachers can work in primary education. So with the extra salary schools couldn't "hire" teachers from outside the official system, but only from the existent pool of certified teachers[13]. It is worth noting that schools don't hire directly their teachers, but is the central education authority who hires them. Teachers are the ones that choose at which school they will work, and individual schools don't have autonomy over their staff. More experienced teachers are (usually) the first to select from the available teaching slots at schools (we explain the process in greater detail in section VII). They consider commuting distance, school amenities, and insider information on the rest of the teaching staff, principal's character and student's performance. Better schools are located in better neighborhoods and have students with more favorable backgrounds. Younger teachers are usually the last ones to choose school, so they end in the less desirable locations. A feature of the system is that students who need more support (because they have a socioeconomic handicap) end up having less experienced teachers. The *CSCC Program* tried to change incentives teachers face when choosing schools by offering the monetary compensation we have just described.

III. Data

The *Monitor Educativo de Enseñanza Primaria* is an administrative registry produced by the Department of Research and Statistics of the National Administration of Public Education (ANEP). It is the official (and main) source of information on the public education system of the country. It delivers statistics in a regular base, which are aimed at providing information to guide policymaking (at the school, regional and national level). This database has been compiled throughout a long period of time (since 1992), has various consistency checks and it has been produced in a standardized way across the years. It is based on regular administrative registries, annual questionnaires to school principals (since 2002), and surveys to parents. It has information on education processes (enrollment, average group size, students per teacher), educational outcomes (insufficient attendance, repetition and dropouts), human resources at each school (number of teachers and other staff, teacher experience and tenure), material resources (library, lunchroom, other infrastructure, school equipment), and social context of each school. There is information for all the public schools in the country (both rural and urban) (ANEP 2008) [14].

Schools that participated in the *CSCC* program are different in several dimensions from the Standard Urban Schools *(UC)*. In Table 4 we show these differences[15]. Standard Urban schools are present in all years of the database [1992-2010], while the *CSCC* started to operate in 1995. There are 14.531 school-year observations, 21% of them are from schools participating in the program. The aver-

[12] For example, in the whole population of the country, the average wage increases a 66% after 12 years of work (considering age group [23,26] compared with [35,38] in 2006 National Household Survey).

[13] It is different in high school, were a candidate doesn't need to have studied the official career to become a teacher: she can have studied another career (i.e. engineering) and be in charge of a classroom (i.e. mathematics).

[14] The administrative registry has a 100% answer rate. The questionnaire to schools principals, has an answer rate of 98%.

[15] In web appendix 1 we present descriptive statistics were averages are weighted by the number of students in each school. This is to obtain means that are consistent with the national statistics at the country level, were a big school receives more weight to calculate, for example, the grade retention rate at the country level.

age number of schools per year is 601 *Standard Urban* and 194 *CSCC*. As we have shown in Table 2, schools can enter and leave the program. A typical *CSCC* remained 9.5 years in the program. If we look at *UC* schools, they were part of the program in 2.17 years on average during the period. There are 555 schools that were never *CSCC* (55.17%), 9 schools participated only 1 year (0.89%) and 19 schools (1.89%) participated during 16 years; the rest of the schools (42.05%) were part of the program between 2 and 15 years. There are relatively more CSCC located outside the capital city, Montevideo. *CSCC* schools are 7% bigger in terms of students. They have on average 16.69 more students, and almost one more teacher (0.98) and almost an extra group (0.82), in grades 1st to 6th. There is no difference in the existence of groups with more than 35 students (45% of the school-year observations have at least one group of big size)[16]. Poor schools have fewer computers for educational use and there is no difference in regular extra staff (teachers and technicians who are not in charge of a group). But there is a sizable difference in another resource: *maestro comunitario:* 88% of treated schools have it, while only 18% of *UC* participated in that program. Finally, we present two measures of violence at both types of schools. Principals at poor schools report in 59% of the cases that verbal violence between students is a moderate to serious problem (vs 45% in *UC* schools), and they report in 57% of the cases that physical violence is also a concern (vs 40%).

We now turn to look at teachers and students characteristics, which will be outcomes of the analysis. Teachers outcomes (experience and tenure) are measured from 2002-2010 and students outcomes (insufficient attendance, grade retention and dropouts) from 1992-2010[17]. Teachers in *CSCC* schools have 12.28 years of experience; they have slightly less experience than teachers at *UC* schools (and the difference is statistically significant). There is no difference when we look at the proportion of teachers with more than 9 years of experience (the measure provided in the official education statistics): around half of the teachers have at least 9 years of experience. Teachers in poor schools remain a (slightly) shorter tenure at the school: 4.5 years vs 4.96.

The main outcomes for students are insufficient attendance, grade retention and dropouts[18]. They are indicators of human capital production. Unfortunately, there is no uniform test that is applied to all students in the country (there are some formative evaluations that monitor student learning and provide feedback to teachers, but there is no summative assessment to evaluate learning for each student). So we don't have test scores outcomes for all students[19]. But attendance, grade retention and dropouts are important outcomes since they are correlated with learning. These outcomes should capture improvements in human capital of the students, through and increase in the *quantity* of education. For example, regarding dropouts and retention, they are negatively correlated with years of schooling, and the literature on returns to education finds that more years of schooling are correlat-

[16] The number of groups with more than 35 students has been constantly decreasing over time. In 2004 there were 1,247 while in 2010 there were only 118 left. Cfr ANEP (2011) for a description of the evolution of the system over the years.

[17] In some years teacher experience and tenure are measured in interval categories. In 2002 5-9 years are in one category (and measured in exact number of years form 2003-2010). Before 2006 years of experience and tenure are grouped between 10 and 19 and of 20 or more.

[18] Insufficient Attendance is defined as the percentage of students who attended more than 70 day of classes, but less than 140 days in the academic year. Grade retention is the percentage of students that didn't approve the course and were retained in the same grade. Dropout is defined as the percentage of students that didn't attend at least 70 days in the year.

[19] When Manacorda (2012) studied the cost of grade retention, also with Uruguayan data for Junior High School, students subsequent outcomes were not test scores, but grade failures and dropouts, since there is not a summative assessment for all students in the country neither for Primary Schools (our case) nor for Junior High School.

ed with higher salaries later in life. Assistance is also an important input in learning[20]. More hours of instructional time have a positive and significant effect on test scores (Lavy, 2014). So these outcomes may be indicators of learning and of future life achievements.

Students at poor schools present significantly worst outcomes: more insufficient attendance, more grade retention and more dropouts[21]. This is a reason why the program was targeted at poor schools: to try to improve poor students outcomes. These students lag behind largely because they have a poor household educational background (human capital). Socioeconomic characteristics of their parents were used as an assignment variable to the program, since it was believed that the school had to make an extra effort to reduce differences. So in the last block of variables in Table 4 we present family characteristics.

Mothers of students at *CSCC* schools have significantly less education. For 1996 (the program started in 1995) we have information on an index of mothers education that is constructed by taking the percent of students whose mothers education level is primary or less and subtracting the percentage that finished secondary education. A value of 100 indicates that not even a single mother has finished secondary education, and a value of -100 indicates that all mothers have completed high school. There is a 33% difference between *CSCC* and *UC* schools, which implies that *CSCC* students have mothers with worse education background. For 2002, 2005 and 2010 we have the percent of mothers that have a very low level of education: primary or less. Mothers of students in poor schools have significantly less human capital: for example, in 2002 three quarters of mothers of students in poor schools had primary education (or less) as the maximum grade attained. It is worth noticing that the education level of students mothers increased during the last decade, but the gap between poor schools and better ones remained constant over time. Also household heads of students in poor schools have a higher unemployment rate. Finally, children in *CSCC* schools have unmet basic needs and participate in a conditional cash transfer program in greater proportion that students of schools located in more favorable backgrounds.

IV. Empirical Strategy

Our main strategy to identify the effects of the CSCC program on teachers and students will be a sharp Regression Discontinuity Design (RD). So we will explain in some detail the validity of this design to answer our hypothesis. Then we will add a fixed effects panel data strategy, that is based on other identification assumptions and that will employ a longer period of time. The RD is only valid as a research design for the last years of the implementation of the Program were a clear cutoff point was used to assign schools to treatment.

In the regression discontinuity analysis we will look at the impact of the 2005 assignment to the Program on outcomes in 2010. The CSCC program had been running for a decade, but before 2005 the entrance of schools was not determined by a clear discontinuity rule (so it was subject to some discretion which would have introduced selection bias in the analysis). As we have explained in section II, in 2005 a new categorization of schools was made. The assignment criterion to the CSCC Program

[20] Regarding insufficient attendance, the General Director of Primary Education in Uruguay, Héctor Florit, recently declared that "absenteeism is the most serious problem for education, since it implies that no educational policy makes sense if children do not attend classes" (El Observador, 25th August 2014).

[21] Grade retention is a major problem in 1st grade of primary education. Almost one in four children is retained in the first grade in poor schools.

in 2005 was well enforced by the administrators of the public education system using a very clear cut-off rule. This type of assignment allows us to identify the effect of the program with a sharp regression discontinuity design. The assignment rule tried to proxy the socioeconomic status of the students and was based on a continuous score[22]. There were no elements that could be affected by schools decisions (like grades or retention rates), so there was no perverse incentive for schools to reduce effort in order to underperform and receive the treatment. Once the score was calculated, each school was ranked. The Central Authority (ANEP) had to assign the extra resources, subject to a limited budget. The criterion they implemented was this ranking based on the socioeconomic status of students: the extra resources would be assigned starting from the poorest schools, until they run out of extra money to assign to more schools. When looking at the new ranking, it became clear that some schools that had been receiving the extra resources before 2005 had improved the socioeconomic status of their students, so they should be dropped out of the Program, in order to assign that budget to schools that weren't receiving the monetary compensation but had worsened the socioeconomic "quality" of the intake of students. This reassignment of schools into an out of the Program was done gradually: between 2005 and 2009 a total of 261 schools changed their treatment status (the number of schools that entered or left the program in each year is shown in Table II).

We will call $s_{i,}$ the index of the socioeconomic status of school students that was used to allocate the extra resources. Let the CSCC treatment be denoted by the dummy variable T_i so that $T_i = 1$ if $s_i \geq s_0$ and $T_i = 0$ if $s_i < s_0$. A school will be part of the program if her poverty score is above the threshold s_0. We will normalize the score so that the threshold is zero $(s_0 = 0)$[23]. The causal effect of the extra resources is the parameter $\boldsymbol{\delta}$ of the following regression

$$Y_i = \alpha + \beta_1 s_i + \beta_2 s_i^2 + \cdots + \beta_p s_i^p + \delta T_i + \beta_1^* T_i s_i + \beta_2^* T_i s_i^2 + \cdots + \beta_p^* T_i s_i^p + \boldsymbol{X}_i^{\prime} \boldsymbol{\rho} + \varepsilon_i$$

estimated with observations located in a bandwidth h of the cutoff, so that $-h < s_i > h$. Our outcome variable is Y_i: (experience and tenure for teachers, and insufficient attendance, grade retention and dropouts for students). In some specifications we will include a set of school level pre-treatment controls $\boldsymbol{X}_i$. The main model includes interactions between the treatment status (T_i) and the polynomial terms s_i^p. This specification has the attraction that it imposes no restrictions on the underlying conditional mean functions (Angrist and Pishke, 2008). In the robustness section, we will also present results with (a) no interaction terms, (b) using different polynomial orders (between $p = 0$, that reduces the model to a comparison of means at both sides of the cutoff, up to a fourth order polynomial), and (c) different bandwidths.

We use the value of the score s_i in 2005 and look at outcomes Y_i in 2010 when the Program was fully operational in 285 treated schools (every school that had a poverty score bellow the cutoff received the extra resources and no rich school that had been previously treated was participating in the program). The schools that could enter the program in 2005 were of two types: *Urbanas Comunes*

---

[22] As we have explained in section II, the score includes indicators of human capital (level of education of mothers), socioeconomic level (unmet basic needs, overcrowded homes, etc), and social integration (household head unemployment, among others).

[23] Subtracting the cutoff value from the original index provided by the authorities (the assignment variable) makes it easier to interpret the results from the regression. The score has the following descriptive statistics: mean = -.0897; s.d. = 1.000; min = -2.350; max = 3.191.

(the standard type of urban schools) and *Habilitadas de Práctica*[24]. *Tiempo Completo* schools (double shift), *Prácticas* (practice) nor *Rurales* (rural schools) could enter the program. For the analysis, we will focus only on Standard Urban schools, that are the main type of school that entered the program[25]. There are 640 schools for the analysis, which were *Standard Urban* or *CSCC* in 2005 and that in 2010 belong to one of those categories[26]. In 2010, 42.7% of them were receiving extra resources. In this first analysis we look at teachers and students outcomes in 2010 and use the 2005 score as the running variable in an RD design.

In Graph 3 we show the assignment rule. Each dot represents the mean of the treatment of the schools that belong to the same bin (of a fixed width of 1 decimal point in the assignment scale, with no overlap between bins). The probability of the treatment jumps from 0 to 1 at the cutoff: there was perfect compliance and all the schools that had a score above the threshold were treated with extra resources, and no one at the left of the threshold participated in the program[27].

One threaten to this identification strategy would be that schools could precisely manipulate the assignment variable to receive the extra resources of the program. That seems very unlikely, since manipulation would imply falsification of the questionnaires sent to the parents, and it should be done precisely knowing the value of the cutoff (that they didn't knew it). The assignment rule in 2005 was not based in what schools could manipulate (teachers effort, students grades, retention, etc). If schools can't precisely control the assignment variable near the cutoff, then the treatment variation is located as good as random in the neighborhood of the cutoff point (Lee 2008).

To test if there was manipulation on the running variable, we show that there are no jumps in the density of the forcing variable (score) at the cutoff point[28]. Results are shown in Graph 4 and Table 5 (in web appendix 4 we also present McCrary (2008) test).

---

[24] These Habilitadas de Práctica schools are a very small category of the standard urban schools. There were 15 schools of this type in 2010. They are schools were undergraduate students of teaching careers could make their pedagogical practices, but they didn't have all the requirements to become a Practice school (100 in 2010) that are the official type of schools were students make their practices.

[25] For several reasons we would not consider the *HP* schools that could enter the program. a) In the database, these schools are in the same category as *PR* schools; b) They are a small and special group of schools; c) From the 642 observations of schools that entered the *CSCC* program (Table 2), 628 had been previously *UC* (98%).

[26] We don't use schools that were *UC* (*Urbanas Comunes*) or *CSCC* in 2005 but in 2010 were participating in other programs. So we look only at the *Standard Urban* schools that entered the *CSCC* Program, and compare them only to *UC* schools in 2010 (schools that had not entered the *CSCC* Program nor they had been transformed in other type of school, ie: *Double Shift*, and *HP*). This is a way to make the analysis of *CSCC* schools cleaner, since we don't mix the impact of different programs

We drop 2 schools that didn`t follow the score rule. As we have explained, although the score was the assignment criteria, the education authorities wanted to have at least one treated school in each state of the country, even if it didn't reach the minimum score to participate (their students weren't poor enough). Results are unchanged if we include those two observations and repeat the entire analysis with instrumental variables (fuzzy RD).

[27] A school is an administrative unit, not a physical facility: a school can function in the morning and another school (different principal, teachers and students) in the afternoon, in the same building. Just over a quarter of the Standard Urban schools (249 in total in 2010) share the building with another school in a second shift. In these cases there are two different administrative units (two schools) operating in one building. So, in our analysis, treated and non-treated schools could be *so similar* that they even share the same building, but one of them received students with slightly better background than the other, so they have different assignment index, and different treatment (cfr. ANEP 2011).

[28] Intuitively, the test looks at the number of schools (frequency) in each bin, and tests if there is a jump at the cutoff: ie, that there aren't many observations in one side of the cutoff compared to just the other side.

Another test to check the validity of the RD design is to see if, in the neighborhood of the cutoff, schools are similar in their baseline characteristics: this is a consequence of the random assignment of the treatment near the threshold. We show that there is no jump at the cutoff in several pretreatment variables so that they evolve smoothly when crossing the threshold (Graph 5)[29]. In what follows, we will describe each variable, to understand the differences and similarities between the two types of schools. These graphs complement the descriptive statistics presented in Table 4, since they allow us to look at the differences in 2005 across the distribution of the score. By looking at entire picture, we can make conjectures on the external validity of the RD results, which will have a local interpretation.

Mothers education index: we can see that it steadily decreases with the assignment variable (this is expected, since the score used to assign schools to treatment is constructed with information on the education of student's mothers). There is no jump at the cutoff.

The outcomes for teachers are their average *Experience* and *Tenure*. Schools with students from more favorable background have on average teachers with more experience. Teachers in schools in the first quintile of the distribution of the assignment index (rich schools) have on average 15.1 years of experience; the median teacher experience is 12.2 years and schools in the last quintile of the distribution of the assignment index have on average teachers with 11.5 years of experience.

Teachers Tenure is the second outcome for teachers. At the left of the cutoff (better schools) teachers remain more time on average at the same school[30]. Teachers in schools in rich neighborhoods remain on average 5.9 years at the same schools, they remain 4.8 years in the median school, and 4.3 years in the last quintile of the distribution of schools. So rich schools can retain more time their teaching staff, but from one point onward all schools face the same teachers rotation (this is a consequence of how teachers choose were to work, as we have explained in section II). There is neither a jump in *teacher experience* at the cutoff.

The next variables that we will describe are related to students. *Grade retention* shows no jump at the cutoff, and, as expected, it increases steadily: the big picture is that schools from better backgrounds have almost zero retention, schools at the cutoff have 10% grade retention, and poor schools have 20% of retention. The other two outcome variables at the student level are *Insufficient Attendance (Truancy)* and *Dropouts*[31]. Poor schools also show more truancy and dropouts. We also present the pretreatment balance for other (non outcome) variables. *School size*: poor schools have slightly more students (328.4 students on average, with a maximum of 1120, vs 322.3 with a maximum of 833 students in rich schools). *Group size* is 26 students on average at the cutoff. Poor schools

---

[29] Although treatment started in 2005, we show the balance in this year because we have information on more variables than in the previous year (2004). Very few schools entered the program in 2005 (Table 1). Results for 2004 are shown in web appendix 2 and 3 have the same pattern as the results in the main text.

In the graphs we present a scatter of the average outcome for each bin of the assignment variable. We also plot a linear regression and a third order polynomial regression of the outcome on the assignment variable, the treatment variable, and their interaction, using the averages for each bin, to have a visual aid to interpret the scatterplot. Finally, we also plot the confidence intervals of a linear regression using the *individual* school information (and not the bins as in the plotted lines): we want to have at this point a formal sense of the results using the regressions that we will present in Table 6. Since the confidence intervals don't correspond to the linear regression using the bins, the plotted line can lie (in some cases) outside the confidence intervals constructed with individual observations.

[30] This indicator is tricky in the best schools, were teacher tenure is usually shorter. This is due to the fact that teachers close to retirement are the ones who have on the one side more experience (so they are the first to choose were to work, and they choose mainly better schools), and on the other side they will have a shorter tenure at that school, because they are closer to the retirement age. Another caveat is that in new schools, teacher tenure is short by default.

[31] All the averages are calculated at the school level with the final enrollment.

have more variation (some groups of more than 30 students and others of less than 24). The next three variables measure students and their families' background. *Mothers Education* is measured as percentage of mothers with only primary education or less. There is a lot of variation in this indicator: form almost zero mothers whose maximum level of education is primary or less, to more than 80% in poor schools. *Unemployment of Households Head* increases, also as expected, in poor schools. The last measure of students and their families poverty background is the percentage of students with no unmet basic needs, that decreases from 100% (all students at rich schools have no unmet basic needs) to 20-30% of students with all their basic needs met in poor schools. One of the components of the program is called *Programa Maestro Comunitario*. Rich schools have zero *maestro comunitario,* and poor schools are much more likely to have at least one[32].

The existence of jumps at the cutoff is formally tested with local linear and polynomial regressions, and results are shown in Table 6. Of the 96 estimated coefficients, only six are significant at the conventional levels (two at the 5% level and four at the 10%).

Now we will briefly describe our second empirical strategy to identify the causal effects of the program on teachers and students. We will try to answer the same questions using a longer time period and a different technique: panel data with school fixed effects[33]. This second strategy will complement and enrich the analysis by overcoming some limitations of the RD technique: mainly the difficulty to extrapolate the results away from the neighborhood of the cutoff (high internal validity but low external validity). In the RD analysis we won't use the treatment variation of the poorest and richest schools (the ones which are far from the cutoff)[34]. So it will be interesting to see if the results of the RD estimations for 2010 are similar in different years and using the whole sample of schools.

The model that we will estimate is:

$$Y_{it} = \alpha_i + \lambda_t + \delta T_{it} + \boldsymbol{X}_{it}^{'}\boldsymbol{\rho} + \varepsilon_{it}.$$

The impact of the CSCC treatment will be the estimation of the parameter $\boldsymbol{\delta}$. The inclusion of school fixed effects (α_i) eliminates the confounding influence of unobserved school fixed confounders (such as neighborhood or school organization)[35]. The inclusion of a full set of year fixed effects (λ_t) eliminates the influence of system-wide factors that affected all the schools at a given year that may be correlated with students or teachers outcomes: changes in retention policies that may have become more or less lenient with the years; changes in the number of effective school days that may affect insufficient attendance; changes in curricula and programs; changes in the country's economic conditions that may affect teachers employment decisions and students dropouts, etc. Controls ($\boldsymbol{X}_{it}$) are

[32] There is also balance in the number of years that a school participated in the program before 2005 (web appendix 5). We also checked for balance in the state (*departamento*) were the schools are located near the cutoff. In Montevideo the richest and poorest schools are located in greater proportion. In the neighborhood of the cutoff (-1 to 1 points in the score), only 20% of the 418 schools are located in Montevideo.

[33] To identify the impact of the program in the years before 2005, we also tried another strategy: a fuzzy RD. We looked at the other re-categorizations of schools into and out of the program (in 1999 and 2002, as explained in section II). In those years, the assignment doesn`t present perfect compliance (or the rule was not clear enough or well enforced), so we used a Fuzzy RD design, that is essentially an instrumental variables approach. But the methodology had small power (first stage) and turned out not to be a good instrument to perform the analysis. Notwithstanding, we make this exercise available as web appendix 8.

[34] And the schools near the cutoff were located mainly outside Montevideo, the capital city.

[35] A Haussman test strongly leads us to a fixed effects model instead of a random effects one.

related to the size of the schools and include: number of students in the school, number of students in the first and sixth grade, total number of groups in the school and number of groups in first and sixth year. Standard errors will always be clustered at the school level.

It is important to notice that, in a fixed effect framework, constant characteristics of the schools can't be included in the analysis (i.e: the city where the school is located, or some baseline characteristics of schools before the start of the program). With this in mind, the impact of the program will be identified using the variation in those schools that changed status (schools that entered or left the program, see Table 2). If a school doesn't change her *CSCC* status (always in the program or has never participated), then the treatment is collinear with the school fixed effect. Only within-school variation is used to identify the effects. As we have already mentioned, teachers outcomes are measured from 2002-2010 and students outcomes from 1992-2010, so we will have more observations when we look at the impact on students.

Finally, we will complement the analysis with the impact of the program after several years, looking at the treatment effect on teachers and students for different time horizons (from one to four years ahead).

$$Y_{it} = \alpha_i + \lambda_t + \delta T_{it-n} + \boldsymbol{X}'_{it\text{-}n}\boldsymbol{\rho} + \varepsilon_{it},$$

with n = 1, 2, 3, 4.

V. Results: Regression Discontinuity Analysis for 2010

Effect on Teachers

One of the main objectives of the *CSCC* Program was to generate incentives for teachers to choose to work in poor schools. It seems to have worked, since treated schools have in 2010 on average teachers with more experience. We present results (graph 5 and table 7) with different bandwidths, with linear and third order polynomial functions[36], and with two different measures of experience[37]. The mean impact of the Program across the different specifications is an increase of approximately 3 years in teachers experience[38]. This is a big increase: ¾ standard deviations of the mean of teacher experience: or more than moving a school from the poorest quintile to the richest quintile in 2010. When we look at the estimated coefficient with Local Linear Regressions, we find that the impact is bigger the closer we are to the cutoff. Regarding *Teachers Tenure* the estimated coefficients have a positive sign, but

[36] We use a third grade polynomial both in the graphs and in these tables, since it seems visually adequate to fit the data. But as we will see in the robustness section, it is in general preferable the adjustment of a linear regression model (in small bandwidths) or a smaller order polynomial as bandwidth increases.
To improve visualization we excluded a few outliers before graphing: *years of teacher experience*, we drop one outlier bin that has more than 25 years of experience and it is far away of the cutoff (so it doesn't affect the local estimation, but it improves visualization). We did the same with a bin that had *dropouts* of more than 13% (when the following worst one has 4%). Those observations are included in the regressions.

[37] There are 8 schools, in the whole sample, that don't have information on these variables for 2010. We tested that schools are missing at random (not correlated with the assignment variable and other variables).

[38] We will present many variations and alternative estimations of the value of this causal impact of the program. For now, we can say that we should rely more on the estimated impact of the LLR when the bandwidth is small, rather than the polynomial regression, that fits the data better when we include more observations using a bigger bandwidth.

the impact of 0.7 years on average is not statistically significant. Teachers at treated and control schools remain the same spell at the school (3.8 years on average at the cutoff).

We also present results with measures of teachers experience and tenure that are the ones reported in the official country statistics of the education system: *Teacher Experience* defined as the percentage of teachers at the school that have more than nine years of experience, and *Teacher Tenure* defined as the percentage of teachers at the school that have more than four years at the same school[39]. With this alternative measure of experience, the mean impact across specifications is estimated to be 24%, so in treated schools the percentage of teachers that have at least 9 years of experience increases 1.14 standard deviations. On the other hand, *Teacher Tenure* remains the same in treated and control schools, or increases slightly (the coefficient has always a positive sign and in some specifications it is significant, but it is not a strong result)[40].

In the database we have information on experience and tenure for each individual teacher in each school. So we check the RD results at the school level using observations at the teacher level, which provides more variability. There is information on experience for 8,560 teachers in 2010 from treated and control schools. Their average experience is 12.3 years (sd=8.9; min=0; max=38). Information on tenure is available for 8,641 teachers, who have an average of 4.9 years working at the same school (sd=5.9; min=0; max=35). Results using this detailed information are totally consistent with the RD results at the school level. Estimations are more precise and show an increase in teachers experience of 3.2 years on average at treated schools, and no significant effect on teacher tenure (Table 8).

Effect on Students

Improving students outcomes (insufficient attendance, grade retention and dropouts) was an objective of the program[41]. But it was not fulfilled, since there was no effect of extra resources on those outcomes (Table 7 and Graph 7). Some individual coefficients are significant, but there is not a strong pattern in the graphs.

[39] This is a relevant measure of tenure, since every four years teachers can be re-assigned to a higher category of the seven levels of the teaching track, and it is more likely that they move to a better school if they have a higher category, as we explain in section VII.

[40] Some coefficients estimated with the 3rd grade polynomial present a strange pattern when a small bandwidth is used. This is due to the fact that fewer observations are used, and that a greater curvature is allowed than in the linear regression case. So, even if some results show a significant coefficient of big magnitude, they should be interpreted with care, considering also if the graphical analysis shows a jump (or what pattern appears), looking if there is consistency with the linear estimations, and the sensitivity to different bandwidths (which trade-off bias and variance). It is the case, for example, of the results for teacher tenure or students insufficient attendance, when the estimation is performed with the polynomial regression in a small bandwidth. As bandwidth increases, the polynomial estimation becomes more reliable. With a small bandwidth we prefer the local linear estimation (which can be viewed as a nonparametric estimation with a rectangular -uniform- kernel). As Lee and Lemieux (2010) suggest, the estimation shouldn't rely on a particular method or specification, and results are more reliable if they are stable across specifications.

[41] Improving retention grade and dropouts was explicitly stated as an objective of the Program (i.e. when reporting the execution of the budget balance; cfr. Rendición de Cuentas de ANEP 2007, 2009, etc.).
Grade retention is a decision variable at the school level. But we think that, although it is endogenous, we can still provide results since the repetition criterion is centralized (with directives from the central authority) and schools have little autonomy. If there is no systematic difference in the application of retention policies between *CSCC* and the rest of the schools at the cutoff, the results should have a causal interpretation.

One explanation of the absence of an impact on students can be that since schools full compliance was achieved in 2009 and students outcomes are measured one year after, there was little time to see the changes. However we have shown that there was a very large effect on the experience of teachers, and we will see that there were no effects on students using more years of data in the panel data analysis.

Robustness, variations and other results

We will present some sensitivity analysis to show if results for teachers are robust to several variations. Generally speaking, in the case of the local linear regressions, the results can be sensitive mainly to the bandwidth used. Local linear regression adjusts better with small bandwidths, and it can be misleading when more observations are used, if they don't have a linear pattern. In the case of the polynomial regression, it can better fit the curvature of data in a wider bandwidth, but it may be sensitive to the order of the polynomial. We have already presented the results for different bandwidths. Now we will look at the effect of different polynomial orders (Table 9). In order to test the goodness of fit of the several models presented, we will include in the regressions a full set of dummy variables indicating the bin in which each observation is located[42]. The bins are the ones used to perform the graphical analysis (Graph 5), and by including this unrestricted set of means of the outcome variables by bins, we provide a nonparametric alternative to the polynomial regressions. A test on the joint significance of those dummy variables is then performed. If we reject the null hypothesis that all the bin dummies are equal to zero, then some bins exhibit an outcome that is far away from the prediction. This can mean that the model is not well specified (a higher order term is needed in the polynomial) or that there is a discontinuity at points other than the cutoff (Lee and Lemieux, 2010). To choose between the different models, we present the Akaike maximum likelihood information criterion of model selection (AIC). It trades off goodness of fit and complexity (a smaller value of information loss is preferred)[43].

The first column of Table 9 shows the estimation of a polynomial of order zero, which is a simple comparison of means at both sides of the cutoff. The second column is a linear regression, and the following ones are polynomials of the indicated order with interactions. The estimated impact of the program with a comparison of means in the neighborhood of the cutoff is an increase in teacher experience of 2.2 years. The p-value indicates that we don't reject that dummies for bins are jointly equal to zero (so the model is well specified). Finally, this simple model (in a small bandwidth from the cutoff) is the one that has better Akaike Information Criterion. The general pattern is that a simple comparison of means works better in very narrow bandwidths, but not when using more observations (distant from the cutoff point): for example, with a bandwidth of 1.5 and 539 observations, p-value <0.01 indicates that some bins are far from the median value, and the AIC comparison is also the worst of the five models evaluated. On the other hand, when more observations are used, higher

---

[42] Two dummies are droped, due to colineality with the constant and with the treatment dummy. The dummies excluded are the ones closer to the cutoff (one in each side).

[43] To show how different are two models, we present a row in the table labeled "AIC comparison" that uses the formula: $\exp((\text{AICmin}-\text{AICi})/2)$. A value of 1 as in the first estimation of Table 9, indicates that the model is the one that minimizes the information loss. The second model is 0.24 times as probable as the first one to minimize the information loss.

order polynomials are preferred. But it was surprising to find that in general a third or fourth order polynomial don't do a good job, even when using almost the entire sample[44].

In web appendix 6 we present a summary of the estimated coefficients, indicating the models that better fit the data. In an ad-hoc calculation, if we weight the estimated coefficient by the AIC comparison row, we find that the estimated impact of the program on teacher experience is 2.6 years and 22.6% increase in the percentage of teacher working at the school that have more than 9 years of experience. We also show graphically the estimated coefficient for the entire range of bandwidths with local linear regressions (Graph 8)[45]. It shows that the impact is always positive, across the entire range of bandwidths, and that it has more variation when the bandwidth is smaller[46]. The estimated impact with local linear regressions for several bandwidths smaller than one is 3.1 years and 24.3% increase in the percentage of teachers with more than 9 years of experience[47].

We also check if including interaction terms between the treatment dummy and the polynomial terms (as we have done) has different implications than excluding them (there is not a consensus in the literature on this point). Finally, we check if including controls to the regressions has an impact (Table 10). Including covariates shouldn't change the estimated results if treatment was randomly assigned near the cutoff, but it can reduce standard errors, increasing the efficiency of the estimators. It is mainly a check of the RD design rather than an improvement in the estimations (Lee and Lemieux, 2010).

In columns 1, 3, 7 and 9 of Table 10 we display the basic results from Table 7 to have them as a benchmark. When we include controls, we find that estimations are more precise and the estimated coefficients are (in general) slightly smaller in magnitude than without controls[48]. When we exclude the interaction terms, the estimated impact of the program becomes bigger in size than the one that we presented in the main results. The general pattern is that across the different new specifications the results remain clearly consistent with the evidence presented so far, that shows a positive effect of the program on teachers average experience at the school.

We also tested for differences in retention across grades, and between sexes. Students in 1st grade of primary school faced more retention in 2010 (14%) than students in 6th grade (1.9%). Boys

---

[44] The only difference between the selected model using the p-value or the AIC criterion is with a polynomial of order one and bandwidth=1.5, were the model has the smallest AIC but the p-value of 0.044 rejects the goodness of fit assessed with the test of joint significance of bin dummies. But it can also be a statistical artifact since when estimating many coefficients, some can be significant by random chance.

[45] This analysis is similar to the graphical display of results in Card, Dobkin and Maestas (2009). The graph shows the estimated impact when we gradually increase the bandwidth. It is done by plotting the coefficient of several regressions, each one with a different bandwidth. The confidence intervals are the ones obtained from theses regressions.

[46] Bandwidth selection is a trade-off between bias and variance. If bandwidth is small, fewer observations are used, which increases variance but reduces bias (since we are comparing observations that are more similar to each another). When we move away from the cutoff, variance is reduced but bias is increased since are comparing observations that aren't as similar as before.

[47] Although we present in Graph 8 the results for all the bandwidths in the range (0,3), we highlight the average impact for bandwidths smaller than 1 because local linear regressions adjust better in small regions. We had shown in Table 9 that with a bandwidth of 1, local linear regressions (polynomial of order one) were well specified, but with a bandwidth of 1.5 they were not (p-val<0.05). Within a bandwidth of one, 65% of the school observations are located.

[48] Controls are pretreatment variables, measured in 2004: insufficient attendance, grade retention, dropouts, number of students and group size. The first three controls are the main outcomes for students, but since they are measured previously to the treatment, they can serve as controls for they are predetermined. They are clearly correlated with the treatment. Results are unchanged if we include controls measured in 2005, instead of 2004.

across all grades face more retention than girls: 7.6% vs 5.1%. The program had no impact across these dimensions[49].

One can think that schools can trade-off resources to hire more teacher or better (more expensive) teachers. But schools had no discretion when deciding salaries. So we tested that extra resources were not directed to hiring more teachers. The program increased the *quality* of the teachers (measured in years of experience), but not their *quantity* at treated schools. The number of teachers at the school (in grades 1° to 6°) and the number of students for each teacher remained unchanged[50]. So we can say that the program led the schools to "buy" more *quality* and not more *quantity* of teaching staff[51]. The CSCC aimed at reducing group size (so hiring more teachers) and incorporating more teachers in the *maestro comunitario* program. Neither of them shows an impact at the cutoff[52].

VI. Results: Panel Data Analysis

As a benchmark we present the results from pooled OLS regressions that do not control for the unobserved individual heterogeneity of schools (column 1 in Table 11) but account for the correlation of unobservables within a school[53]. As we will see, in many cases the estimated coefficient does not only change in magnitude, but also reverses its sign. An estimated negative and significant impact of the program in the cross section of schools may change to a positive impact of the extra resources on several outcomes when we use panel data. This highlights the importance of making a propter account of causality to inform public policy regarding this education program.

In the case of *Teacher Experience*, the impact of the program using pooled information for 6,195 school-year observations is statistically non significant different from zero. When we turn to panel data with fixed effects, it is estimated to be between 1.3 and 1.1 depending on the specification. The estimated coefficient changes little when we include time dummy variables and controls. So when a school receives the extra resources from the Program it is able to increase the average experience of their teachers in more than 1 year. We had estimated that <u>at the cutoff</u>, the increase in 2010 was of 3 years of experience. Now, we are including a couple more years and using information for all the schools, so we can overcome the local limitation of the RD analysis. The panel data result, that uses variation farther from the cutoff point, shows a smaller impact than RD, but it is consistent with the fact that the estimated coefficient of the RD regressions became smaller as more schools were included in the analysis (cfr. Column 1 of Table 7).

The estimation of the impact on *Teacher Tenure* using the pooled regression shows a significant impact of -0.29 years on average. But when we turn to the results using fixed effects, the estimated impact is 0.45 years: when a school receives the extra resources, it can retain their teachers a longer period of time. When time dummies and controls are included, the effect decreases its magni-

[49] Results available upon request.

[50] Results available upon request.

[51] Years of experience is not really a good proxy of teacher *quality*, since quality should be measured by how do they improve students outcomes.

[52] Web appendix 7. This doesn't mean that group size hasn't decreased or that the *maestro comunitario* program was not implemented. Both were implemented but not in a different way at the cutoff. Group size has decreased steadily between 2005 and 2010, both at treated and control schools. The *maestro comunitario* program was implemented in the majority of *CSCC* schools, but also in some non treated schools..

[53] All regressions (pooled and panel) report robust standard errors clustered at the school level.

tude. So, using panel data there is evidence that the program had a positive impact on teachers tenure, and not only in teachers experience (that was a clear result of both RD and panel data analysis).

We turn now to look at the impact on students. The most striking result is that the interpretation of the effects of the program changes dramatically when we account for the individual unobserved heterogeneity. The simple (pooled) OLS regressions (column 1, Table 11) shows that treated schools present significant more insufficient attendance (3.6%), more grade retention (3.3%), and significant more dropouts (0.85%)[54]. But this is a misleading picture that has no causal interpretation. This seemingly negative impact of the program on student's outcomes is based on the selection of the poorest schools into the treatment. When we use fixed effects (columns 2-4), we get rid of this selection bias, and look at what happened to the same school when she received the treatment (or when it left the program). In these specifications we find that the program had no negative impact on students, or even some positive effect (decreases insufficient attendance and grade retention). When we include year fixed effects, the impact on students becomes non-significant. This can be explained by considering time trends: a period in which the number of treated schools increased that took place simultaneously with a decrease in repetition at the national level.

Given the panel structure of the data, we can also look ahead of the current impact, and see what happens in the years following the beginning of the transfer of extra resources (Table 12). As we had done before, we don't include the number of years in the program, since it was not randomly assigned: poor schools remain more years in the program, and because they are poor, they have worse results[55].

After one and two years, treated schools were able to retain more experienced teachers. The impact dilutes over time, and four years after the school entered the program there is no impact on teacher experience (we should notice that during that time, the school may have left the program, so it is less likely that it can retain the more experienced teachers)[56]. The same happens to *teacher tenure*: treated schools are able to retain more time their teaching staff after one and two years; then de effect becomes non-significant. There are no effects on student's outcomes in the medium term (there were no clear effects in the short run neither).

Panel Data with information at the teacher and grade level

In this section we will take advantage of information at the individual teacher level, and also for each one of the six grades in the school. We don't have a unique identifier for each teacher across the years, so it's not a panel of teachers. There is information for almost 80,000 teachers experience and tenure at 842 schools during 9 years. With this individual information we had constructed the averages at the

[54] The mean of the control group in each of these measures is: 6.62%, 9.03%, and 1.10%, respectively, so the magnitudes of the differences are large.

[55] We will look at the effect of the program after one year, after two years and so on. Although it may seem somehow similar to including number of years in the program as a control, it answers a different question. We look -for all the schools- what happen after 3 years, *whether they are in the program or not*. The selection bias that caused the endogeneity of "the first ones to enter (that have more years in the program) are poorer and have worst results", is not present in this panel data model, since we are comparing the school to herself in a fixed effect model. The variation used for identification is within school, across the years.

[56] In columns 13-16 of Table 12, that present the analysis of the impact after four years, the time period used is 1998 to 2006. In that period, 301 schools left the program (Table 2). Also notice that the average number of years in the program between 1998 and 2006 is 1.6 in the whole sample (966 *standard urban* or *cscc* schools), and 4.1 for the 383 schools that participated at least in one year in that period.

school level. So these results should be in line with previous ones, but estimated with more precision[57]. Results from these regression (Table 13) show that the impact of the *CSCC* program on teachers experience and tenure is now slightly bigger in magnitude, and estimated with more precision. The program caused an increase of 1.3 years in teacher experience and 0.3 years in average tenure at schools (a 10% and 7% increase respectively from baseline values).

In the case of outcomes for students, in the previous sections information from the six grades was averaged at the school level. Now we will use more than 87.000 observations at the grade level (from 1st to 6th grade), in 1006 schools, between 1992 and 2010. Results in the three last panels of Table 13 show that there was no impact of the extra resources on students insufficient attendance, grade retention nor dropouts. The sample size leads us to conclude that there was certainly no impact, rather than not rejecting Ho) because of lack of power.

VII. Discussion

We will discuss the results of the extra resources program for both teachers and students. The main result for teachers is that the monetary incentives of the *CSCC* Program moved teachers with more experience to poor schools at the cutoff; but it had no effect on teacher tenure (RD design) or a small positive effect (panel data analysis). We present a very simple diagram to rationalize these two findings (Diagram 1). As we have explained, each year teachers have to choose in which school they want to work. Suppose that the difference in experience between two adjacent teachers in the ranking is three years of experience. The first ones to choose are the teachers in a higher category, i.e.: with more experience[58].

The first teacher to choose a school to work selects herself to the best school (with students from more favorable backgrounds, which are usually located in the best neighborhoods). The following teacher finds the first and best slot occupied, and chooses the next available slot. At one point (near the cutoff), a teacher may find that the schools she has to choose are quite similar, but in one of them there are extra resources, and an increase in monthly salary. So she doesn't choose the *standard urban* school, but decides to work in the *cssc schools.* The next teacher may find that the school located in the 4th place of the ranking is much better than the one located in the 6th place, and that the monetary incentive is not worth moving to a poorer neighborhood. In the following year, when the oldest teacher retires, the remaining active teachers advance one step in the ranking. So the difference between average experience at the cutoff remains at three years, but there is no difference in average tenure at the school. Obviously the process is much more complicated, with thousands of teachers

[57] We haven't presented these results in the main section since treatment was delivered at the school and not the teachers level, so the relevant unit of analysis is the school. But information on individuals is valuable for the analysis (is similar to analyzing a policy at the state level using data from Household Surveys aggregated at the state level, or using the data at the individual level if it is available).

[58] The system is more complex than the diagram that is a stylized model. There are effective, temporary and substitute slots; total or partial dedication; the ranking of teachers into seven categories or grades (e*scalafón*) is not only (but mainly) based on years of experience (art. 13, art. 38, art. 39 of the *Estatuto del Funcionario Docente (Ordenanza 45))*; every four years a teacher can advance one category in that professional scale, an effective teacher has to be at least two years in his working place before he can choose another school, etc. The central authorities of the education systems annually, at the beginning of the teaching year, make the appointments, for the direct and indirect teaching slots (art 13). They also publish annually a list of charges and hours that are not assigned to teacher with effectiveness, and calls for applications in a contest to supply them permanently, provided there are no effective teachers with spare hours (art 24). The creation of permanent positions for teachers in a school (without the need to annually compete for the place) is still on debate (*profesor cargo*).

selecting schools considering different attributes and there are hundreds of schools at the cutoff, but the underlying process that drives the main results for teachers may be similar to this simple diagram

We have to make a subtle precision in the causal interpretation of our findings. We can't claim that more teacher experience has no effect on students outcomes. We have provided a causal link between the *CSCC* program and students outcomes, not between teacher experience and students outcomes. One way to see this is that we haven't done a quasi-natural field experiment by randomly providing some students with mote experienced teacher. What has been randomly delivered (at the cutoff) was an extra resources program (that had an impact on teachers experience at treated schools). So the causal link is between the program and students outcomes on the one hand, and between the program and teachers outcomes on the other hand.

The analysis is suggestive on the point that teacher experience is not a measure of teacher quality and that increasing teacher experience has little effect on students, but not a causal answer to the question of teacher experience and students outcomes[59]. In the above diagram we can think of a pool of teachers located in the 4th place of the ranking, instead of a single teacher. Suppose that there are two *types* of teachers: low quality and high quality ones. Low quality teachers have low impact on students outcomes; suppose that they prefer extra money with underperforming students rather than working in a better school. When the time comes for those teachers in the 4th pool to choose schools, the *low type* teachers would select themselves to *CSCC* schools. But those 3 more years of experience won't translate into better results (relative to students in the 4th school). So heterogeneity in the type of teachers may be a hypothesis that hinders a causal interpretation of the claim that teacher experience has no impact on students outcomes. Our causal result is on the impact of the program. With the data we have, we can't test this hypothesis of different *types* of teachers.

If we suppose that there is no heterogeneity in the type of teachers who choose a *CSCC* school at the cutoff (or that heterogeneity is balanced at the cutoff), then there is an open question on why experience did not lead to an increase in students performance. One explanation can be that 3 years of experience make a difference in some parts of the distribution of teachers experience but not in others. In fact, teacher experience has an impact on students outcomes in the first few years of teaching: comparing a teacher with zero experience with one that has a couple of years (Rivkin, Hanushek and Kain, 2005; Harris and Sass, 2011; Wiswall, 2013). There is also a big difference between a teacher with many years of experience relative to a first year inexperienced teacher[60]. But maybe the difference in returns to experience from a teacher with 10 years of experience relative to one with 13 years is not statistically different form zero[61]. Indeed, Wiswall (2013) estimates show that returns to experience increase with years of experience, but a difference of 3 years of experience is not statistically significant after the first few years. So, the value added by a teacher with 3 more years of experience is only significant in the first years of the career. If this is the case, then it should not be a surprise that our

[59] Hanushek (2008) claims that commonly purchased inputs to schools- class size, teacher experience, and teacher education- bear little systematic relation to students outcomes, implying that conventional input policies are unlikely to improve achievement.

[60] Wiswall (2013) estimated that a teacher with 30 years of experience has over 1 standard deviation higher measured quality than new, inexperience teachers, and about 0.75 standard deviations higher measured mathematics effectiveness than a teacher with 5 years of experience. Harris and Sass (2011) estimated that experience effects in elementary and middle school are quantitatively substantial, with achievement gains of 0.16 of a standard deviation for a teacher with 15–24 years of experience (relative to a first-year teacher).

[61] These values are roughly our estimates in the RD section.

estimated impact on students is zero, given that, although the increase in experience was large, the gains were in a point of the distribution of experience were it makes no difference on students.

VIII. Conclusion

We have studied the effects of a program that gave extra resources for public primary schools in Uruguay, starting in 1995 through 2010. During those years schools moved into and out of the program. In 2005, the rule for treatment assignment was based on a clear cut-off rule. Using a sharp Regression Discontinuity design, we find that after the 2005 assignment, teachers experience at treated schools increased in approximately 3 years (depending on the specification). This is valid using information at the school or the teacher level. A related result is that treated schools could increase the percentage of teachers that have at least 9 year of experience in around 22%. There was no significant effect on teachers tenure. So, more experienced teachers were located in poorer schools. This is a positive result, and one of the main objectives of the program. But, unfortunately, the program had no impact on students outcomes.

The RD results have high internal validity, but are difficult to extrapolate to schools located away from the cutoff. So we complement the analysis with panel data estimates that confirm the above results for a longer period of time and for more schools. The school fixed effects framework uses the within school variation in the treatment, controlling for fixed unobserved heterogeneity. The evidence we provide consistently shows a positive and significant impact of monetary incentives on teacher experience at the school level (more than one extra year of experience). This current impact is still visible after three years (but decreases over time). The impact on students from 1995 to 2010 is small and generally non-significant. This zero impact on students is confirmed using data at the grade level (more than 87.000 observations), so we think that the zero impact is not due to a lack of power, but that the program had really no effect on students outcomes.

Our results are a contribution to the literature on the effects of school resources and to the literature on teachers (experience). The empirical strategies that we have employed help to overcome the selection bias (treated schools are poor by design, and are also the ones that have worst outcomes). We are confident that our results can have a causal interpretation: teachers react to monetary incentives so schools can buy more teacher experience; and that extra resources for poor schools don't have a significant impact on students insufficient attendance, grade retention nor dropouts.

References

ANEP. "Ordenanza Nº45, Estatuto del Funcionario Docente." Administración Nacional de Educación Pública.

--- **(2004).** "Monitor Educativo de Educación Primaria, Segunda Comunicación de Resultados (Escuelas Públicas 2002)." Administración Nacional de Educación Pública.

--- **(2005).** "Panorama de la Educación en Uruguay: 1992-2004 una Década de Transformaciones." Administración Nacional de Educación Pública.

--- **(2007).** "Relevamiento 2005 de Características Socioculturales de las escuelas públicas del Consejo de Educación Primaria." Administración Nacional de Educación Pública.

--- **(2008).** "Monitor Educativo de Enseñanza Primaria 2002 a 2006. Documentación Metodológica." Departamento de investigación y estadística de la Administración Nacional de Educación Pública.

--- **(2011).** "Monitor Educativo de Enseñanza Primaria. Estado de Situación 2010." Administración Nacional de Educación Primaria.

Angrist, Joshua D. and Jörn-Steffen Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press.

Bénabou, Roland, Francis Kramarz and Corinne Prost (2009). "The French zones d'éducation prioritaire: Much ado about nothing?" *Economics of Education Review 28: 345–356.*

Card, David and A. Abigail Payne (2002). "School finance reform, the distribution of school spending, and the distribution of student test scores." *Journal of Public Economics, 83: 49-82.*

Card, David, Carlos Dobkin and Nicole Maestas (2009). "Does Medicare Save Lives?" *The Quarterly Journal of Economics, 124 (2): 597-636.*

Chay, Kenneth Y., Patrick J. McEwan and Miguel Urquiola (2005). "The Central Role of Noise in Evaluating Interventions That Use Test Scores to Rank Schools." *American Economic Review, 95(4): 1237-1258.*

Chetty, Raj, John N. Friedman and Jonah E. Rockoff (2014). "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review, 104(9): 2633-2678.*

Hanushek, Eric A. (2006). "School Resources." *Handbook of the Economics of Education, volume 2, chapter 14.* Elsevier.

Harris, Douglas N. and Tim R. Sass (2011). "Teacher training, teacher quality and student achievement." *Journal of Public Economics, 95: 798-812.*

Lavy, Victor (2014). "Do Differences in Schools' Instruction Time Explain International Achievement Gaps? Evidence from Developed and Developing Countries." *Economic Journal, forthcoming.*

Lee, David S. (2008). "Randomized Experiments from Non-random Selection in U.S. House Elections." *Journal of Econometrics, 142(2): 675-697.*

Lee, David S. and Thomas Lemiux (2010). "Regression Discontinuity Designs in Economics." *Journal of Economic Literature, 48: 281-355.*

Leuven, Edwin, Mikael Lindahl, Hessel Oosterbeek and Dinand Webbink (2007). "The Effect of Extra Funding for Disadvantaged Pupils on Achievement." *Review of Economics and Statistics, 89(4): 721-736.*

Manacorda, Marco (2012). "The Cost of Grade Retention." *Review of Economics and Statistics, 94(2): 596-606.*

McCrary, Justin (2008). "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics, 142(2): 698-714.*

OECD (2013). "PISA 2012 Results: Excellence Through Equity: Giving Every Student the Chance to Succeed (Volume II)." PISA, OECD Publishing.

Rivkin, Steven G., Eric A. Hanushek and John F. Kain (2005). "Teachers, Schools, and Academic Achievement." *Econometrica, 73(2): 417–458.*

van der Klaauw, Wilbert (2008). "Breaking the link between poverty and low student achievement: An evaluation of Title I." *Journal of Econometrics, 142: 731–756.*

Wiswall, Matthew (2013). "The dynamics of teacher quality" *Journal of Public Economics, 100: 61–78.*

Appendix

Graph 1

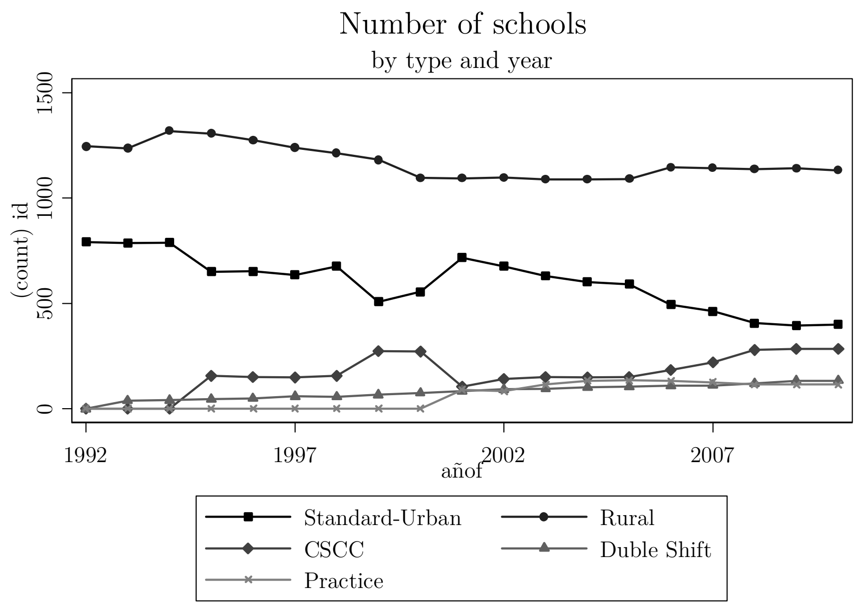


Table 1

The Primary Education System in Uruguay

Year	Standard Urban		Rural		CSCC		Double Shift		Practice		TOTAL	
	Schools	Students	Schools	Students	Schools	Students	Schools	Students	Schools	Students	Schools	Students
1992	792	274,956	1,246	28,390	-	-	-	-	-	-	2,038	303,346
1993	787	271,990	1,237	27,236	-	-	39	4,106	-	-	2,063	303,332
1994	789	271,922	1,320	31,004	-	-	40	4,813	-	-	2,149	307,739
1995	651	223,658	1,307	30,881	155	51,291	46	6,552	-	-	2,159	312,382
1996	653	232,336	1,275	31,379	151	51,755	49	7,848	-	-	2,128	323,318
1997	636	231,138	1,240	27,514	149	54,220	58	9,418	-	-	2,083	322,290
1998	677	248,982	1,214	24,823	156	56,440	57	9,492	-	-	2,104	339,737
1999	507	173,639	1,183	23,066	273	107,819	66	11,875	-	-	2,029	316,399
2000	555	167,036	1,095	18,807	271	110,333	75	15,217	-	-	1,996	311,393
2001	718	236,610	1,094	18,686	106	43,540	84	18,869	90	41,183	2,092	358,888
2002	676	233,582	1,098	19,392	140	49,530	92	21,419	84	38,685	2,090	362,608
2003	631	214,804	1,089	19,985	151	55,412	95	22,451	114	51,892	2,080	364,544
2004	602	205,394	1,089	20,101	148	54,366	102	24,900	133	60,487	2,074	365,248
2005	592	200,035	1,092	20,282	150	54,345	104	25,160	135	60,296	2,073	360,118
2006	495	179,129	1,146	24,132	185	67,290	109	26,528	132	58,107	2,067	355,186
2007	463	158,537	1,143	23,534	221	86,166	111	26,256	126	53,547	2,064	348,040
2008	406	138,898	1,137	23,384	280	102,123	120	28,945	115	48,074	2,058	341,424
2009	395	132,817	1,142	23,486	285	101,438	132	31,359	114	46,881	2,068	335,981
2010	400	130,005	1,133	21,902	285	98,171	134	31,313	115	46,396	2,067	327,787

Authors own calculations based on Monitor Educativo Educación Primaria (ANEP)

Graph 2

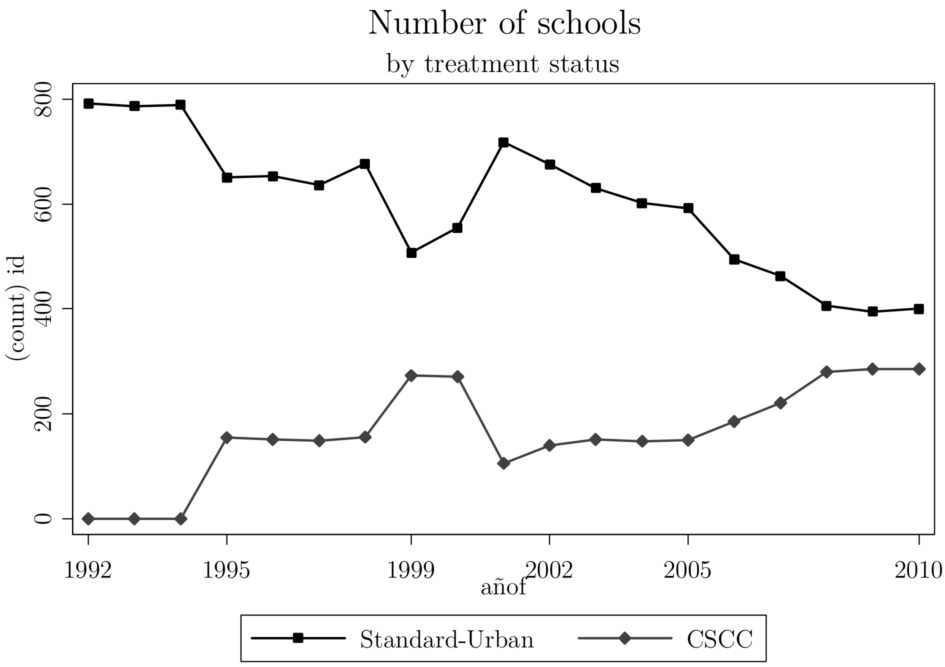


Table 2

Recategorization of schools into diferent types												
	School Changes to:						School Changes from:					
Year of change	Standard Urban	Rural	CSCC	Double Shift	Practice	TOTAL	Standard Urban	Rural	CSCC	Double Shift	Practice	TOTAL
1993	4	0	0	11	0	15	11	4	0	0	0	15
1994	0	0	0	2	0	2	2	0	0	0	0	2
1995	1	0	147	4	0	152	150	1	0	1	0	152
1996	3	0	0	3	0	6	1	0	4	1	0	6
1997	19	0	1	9	0	29	8	18	3	0	0	29
1998	41	0	28	0	0	69	27	19	22	1	0	69
1999	62	0	171	9	0	242	178	13	51	0	0	242
2000	86	1	0	3	0	90	2	86	2	0	0	90
2001	159	0	0	12	90	261	90	2	166	3	0	261
2002	70	15	85	4	11	185	111	5	51	1	17	185
2003	9	7	11	2	34	63	54	6	0	0	3	63
2004	5	3	0	7	24	39	32	0	2	0	5	39
2005	7	10	5	2	3	27	18	5	3	0	1	27
2006	19	63	39	5	12	138	116	3	4	0	15	138
2007	48	0	81	2	2	133	79	1	45	0	8	133
2008	7	0	61	8	0	76	63	0	2	0	11	76
2009	1	0	13	10	0	24	14	1	8	0	1	24
2010	7	0	0	1	2	10	3	6	0	0	1	10
TOTAL	548	99	642	94	178	1,561	959	170	363	7	62	1,561

Table 3

Teacher salaries

Year	Grade 1	Grade 4	Grade 7
1997	9,365	11,235	16,827
1998	9,453	11,346	17,004
1999	10,787	12,144	18,242
2000	10,697	12,044	18,090
2001	10,494	11,814	17,746
2002	11,101	12,519	18,300
2003	8,990	10,138	14,819
2004	8,983	10,186	14,784
2005	9,230	10,424	14,512
2006	9,728	11,033	15,389
2007	10,441	11,927	16,746
2008	11,760	13,568	19,221
2009	12,777	14,918	21,201
2010	13,001	15,182	21,593

Nominal wage (with food complements) for 20hs teachers in Grades 1, 4 and 7 of the payment scale, in constant uruguayan pesos of February 2011. Data from January in each year. Source: Area de Estadística y Análisis- Dirección Sectorial de Programación y Presupuesto - CODICEN- ANEP

Table 4 - Descriptive Statistics

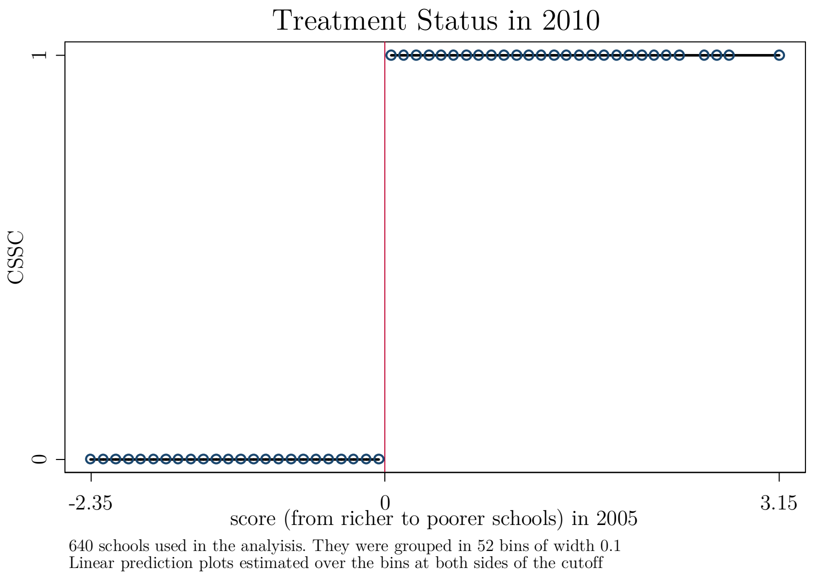
	(1)	(2)	(3)	(4)
	Standard Urban	CSCC	difference (2-1)	t-stat
General				
1 *Years in the sample*	1992-2010	1995-2010	-	-
2 *Number of observations*	11,425	3,106	-	-
3 *Schools per year*	601	194	-	-
Schools				
4 *Average Years as CSCC*	2.17	9.5	7.33	[89.83] ***
5 *Proportion in Montevideo*	27%	33%	6%	[6.53] ***
6 *Number of students*	299.71	316.4	16.69	[4.66] ***
7 *Number of teachers*	10.71	11.69	0.98	[8.48] ***
8 *Number of groups*	11.27	12.09	0.82	[7.87] ***
9 *Students per teacher*	26.89	26.14	-0.75	[6.98] ***
10 *Students per group*	25.53	25.43	-0.1	[0.77]
11 *At least one group of more than 35 students*	45%	46%	1%	[1.1]
12 *Has computers for educational use*	50%	46%	-4%	[3.48] ***
13 *Has extra staff*	63%	66%	3%	[1.74] *
14 *Has Maestro Comunitario*	18%	88%	70%	[62.11] ***
15 *School climate: verbal violence*	45%	59%	14%	[3.69] ***
16 *School climate: physical violence*	40%	57%	17%	[4.41] ***
Teachers				
17 *Experience (in years)*	12.68	12.28	-0.4	[3.16] ***
18 *Experience (% with more than 9 years)*	52%	53%	1%	[1.15]
19 *Tenure (in years at current school)*	4.82	4.53	-0.29	[4.13] ***
20 *Tenure (% more than 4 years at current school)*	36%	33%	-3%	[4.48] ***
Students				
21 *Insufficient Attendance in 1st grade*	11%	16%	5%	[21.53] ***
22 *Insufficient Attendance in 1st-6th grade*	7%	11%	4%	[27.31] ***
23 *Grade Retention in 1st grade*	17%	23%	6%	[27.93] ***
24 *Grade Retention in 1st-6th grade*	9%	12%	3%	[26.49] ***
25 *Dropouts in 1st grade*	2%	3%	1%	[9.02] ***
26 *Dropouts in 1st-6th grade*	1%	2%	1%	[12.36] ***
Families				
27 *Mothers education index 1996*	22%	55%	33%	[18.42] ***
28 *Mothers with primary education or less 2002*	56%	74%	18%	[12.69] ***
29 *Mothers with primary education or less 2005*	45%	62%	17%	[12.59] ***
30 *Mothers with primary education or less 2010*	40%	58%	18%	[60.72] ***
31 *Unemployed Household Head in 2010*	8%	13%	5%	[5.96] ***
32 *Children with unmet basic needs in 2005*	37%	60%	23%	[15.2] ***
33 *Children in PANES (conditional cash transfer)*	17%	31%	14%	[11.73] ***

The difference in means is calculated with an OLS regression with robust standard errors. T-statistics in square brackets.

*** p<0.01, ** p<0.05, * p<0.1.

Variables definition: *Montevideo* : proportion located in Montevideo, the capital city, number of students, teachers and groups are for 1st to 6th grade of primary education, period: [1992-2010]. Group of more than 35: is an dummy variable that takes the value of one if there is at least one group in the school that has more than 35 students. Computers for educational (==1) if the school has at least one computer for educational use ([1992-2009], expect for 2006). Has Extra Staff (==1) if the school has teachers and technicians who are not in charge of a group [2005-2010]. Maestro Comunitario is a program that gives an extra teacher as explained in the text [2005-2010]. Verbal violence (==1) if intimidation, mocking or verbal abuse among students is a moderate or serious problem [2009]. Physical violence (==1) if physical violence between students is a moderate or serious problem [2009]. Teacher variables are measured for [2002-2010]. Students variables are measured for[1992-2010]. Dropouts are measured as those students that didn´t attend 70 days of classes in the school year. Insufficient Attendance is measured as those students that attended between 70 and 140 days. Mothers education index is constructed by taking the percent of students whose mothers education level is primary or less, and subtracting the percentage that finished secondary education.

Graph 3



Graph 4

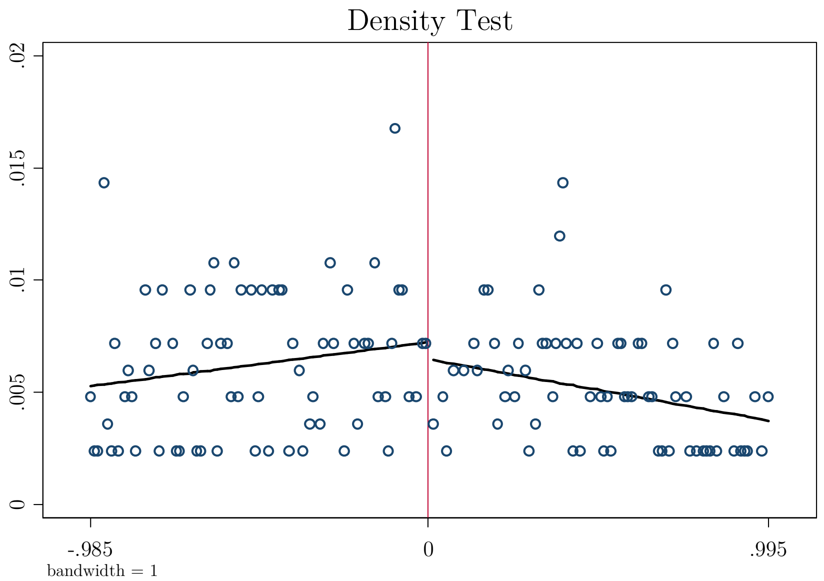


Table 5

Density Test of Manipulation

	Bandwidth		
	h = 0.5	h = 1	h = 1.5
cscc	-0.004*	-0.001	-0.001
	[0.002]	[0.001]	[0.001]
score05	0.009	0.002	0.001***
	[0.006]	[0.001]	[0.001]
score05_cscc	-0.004	-0.005***	-0.003***
	[0.008]	[0.002]	[0.001]
Bins	92	174	242
R-squared	0.04	0.076	0.116

Robust standard errors in brackets

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

Table 6

Table 6 - Pre Treatment Balance																								
	Mothers education index		Teachers Experience		Teachers Tenure		Insuficient Attendance		Grade retention		Dropouts		School size		Group size		Mothers Primary Educ or less %		Unemployed Hh Head %		Students without UBNeeds		Maestro Comunitario	
	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.
Panel A: Bandwidth = 0.25																								
CSCC Program	-2.379	9.867	2.072	1.559	0.799	1.107	-0.954	1.325	0.741	-2.57	0.018	-0.059	-56.264	-12.401	-1.934	-0.737	1.663	-5.649	1.876	1.497	-4.133	11.686	-20.361	-12.883
	[4.393]	[6.059]	[2.246]	[4.453]	[0.866]	[1.478]	[1.658]	[2.512]	[1.643]	[2.995]	[0.407]	[0.583]	[60.364]	[99.709]	[2.865]	[5.737]	[3.721]	[5.059]	[2.987]	[6.361]	[4.614]	[7.266]	[18.596]	[36.003]
Observations	125	125	117	117	118	118	125	125	125	125	125	125	125	125	125	125	125	125	125	125	125	125	125	125
Panel B: Bandwidth = 0.5																								
CSCC Program	-2.49	5.103	0.723	2.641	-0.69	1.225	-0.976	-0.49	1.071	-0.468	0.037	-0.076	-89.378**	-34.48	-2.289	-1.342	1.585	-5.67	1.722	3.951	-3.533	-0.953	-16.464	-16.071
	[3.371]	[5.206]	[1.393]	[3.114]	[0.613]	[1.183]	[1.166]	[2.133]	[1.108]	[2.290]	[0.360]	[0.494]	[44.809]	[81.329]	[1.934]	[4.125]	[2.826]	[4.320]	[2.152]	[4.232]	[3.367]	[5.693]	[12.719]	[25.910]
Observations	238	238	225	225	228	228	238	238	238	238	238	238	238	238	238	238	238	238	238	238	238	238	238	238
Panel C: Bandwidth = 1.0																								
CSCC Program	-4.405*	1.194	-0.037	2.516	-0.284	-0.048	-0.787	-1.926	-0.287	0.74	-0.087	-0.138	-70.324**	-93.843	-1.225	-2.587	3.951*	-1.624	1.48	3.115	-1.936	-4.481	-7.603	-25.031
	[2.523]	[4.528]	[0.918]	[2.150]	[0.436]	[0.874]	[0.837]	[1.670]	[0.801]	[1.670]	[0.275]	[0.441]	[32.825]	[62.998]	[1.392]	[2.917]	[2.091]	[3.683]	[1.532]	[2.998]	[2.406]	[4.573]	[8.907]	[18.443]
Observations	418	418	393	393	398	398	418	418	418	418	418	418	418	418	418	418	418	418	418	418	418	418	418	418
Panel C: Bandwidth = 1.5																								
CSCC Program	-0.768	-1.083	-0.181	0.91	-0.431	-0.316	0.44	-0.724	0.042	0.649	0.163	0.335	-48.806*	-94.607*	-0.652	-2.711	1.675	0.389	0.614	2.132	-1.9	-2.924	-0.738	-20.03
	[2.191]	[3.960]	[0.777]	[1.734]	[0.372]	[0.726]	[0.722]	[1.391]	[0.679]	[1.356]	[0.212]	[0.389]	[28.511]	[54.012]	[1.133]	[2.334]	[1.810]	[3.261]	[1.258]	[2.489]	[1.955]	[3.874]	[7.368]	[14.822]
Observations	547	547	515	515	521	521	547	547	547	547	547	547	547	547	547	547	547	547	547	547	547	547	547	547

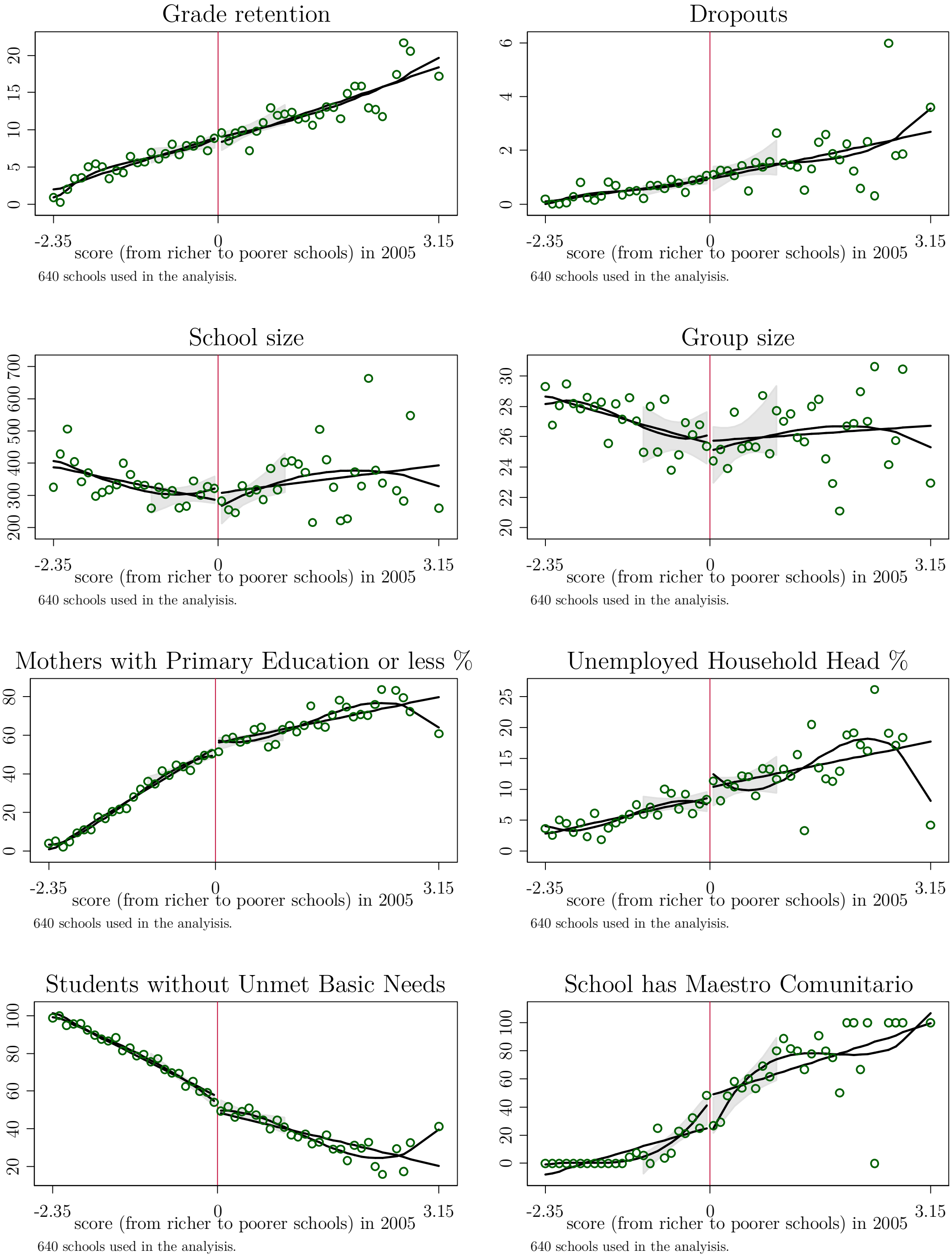
*** p<0.01, ** p<0.05, * p<0.1 Year 2005 3erd order polynomial with interaction terms between polynomial terms and treatment

Graph 5

Pre treatment characteristics for 2005

Outcomes for 2005. Shools grouped in 52 bins of width 0.1.
Linear regression plot and third order polynomial reg with interaction terms estimated over the bins.
Shade areas are 95% CI for the linear regression estimated over school observations (bandwidth = 1).

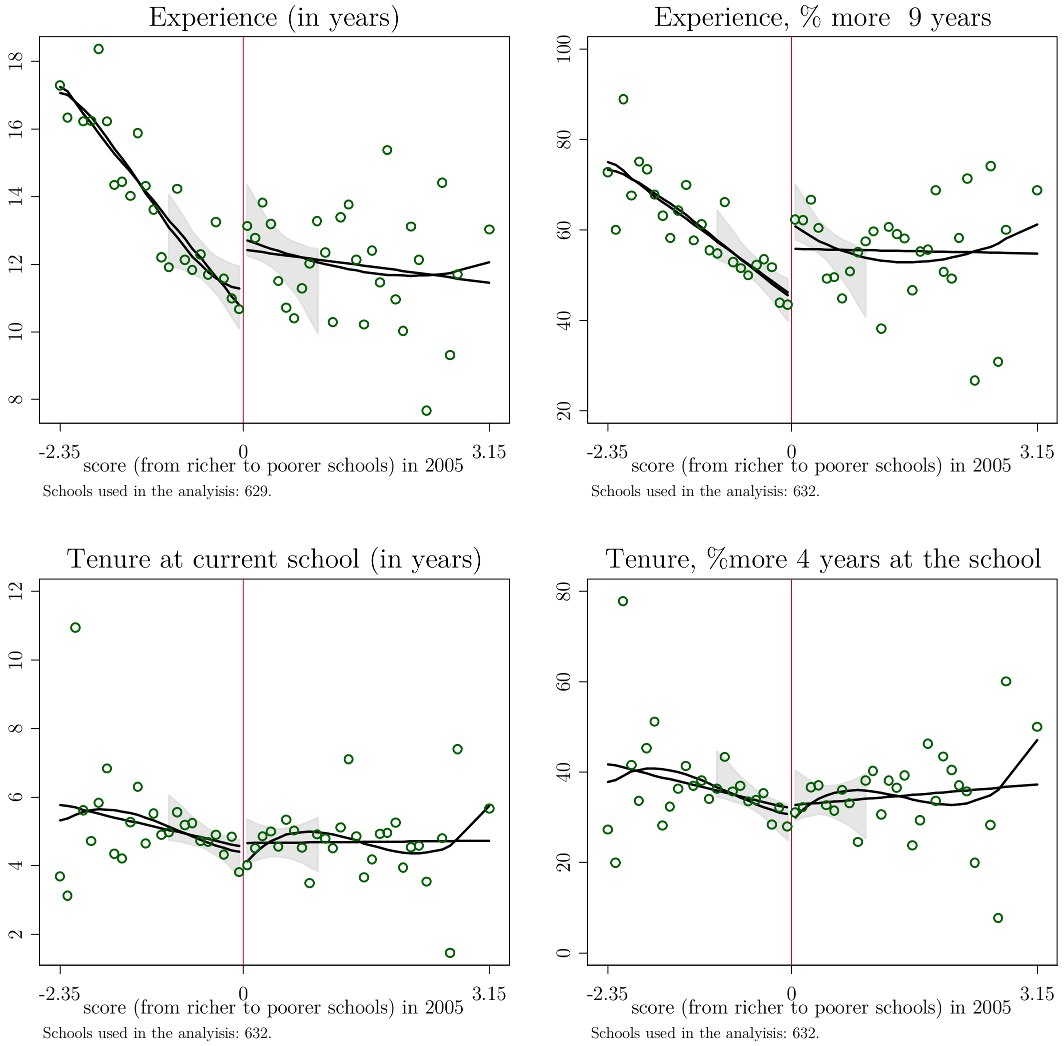
Graph 5 (cont.)



Outcomes for 2005. Shools grouped in 52 bins of width 0.1.
Linear regression plot and third order polynomial reg with interaction terms estimated over the bins.
Shade areas are 95% CI for the linear regression estimated over school observations (bandwidth = 1).

Graph 6

Impact of CSCC Program on Teachers



Outcomes for 2010, treatment started in 2005. Schools grouped in 52 bins of width 0.1.
Linear regression plot and third order polynomial reg with interaction terms estimated over the bins.
Shade areas are 95% CI for the linear regression estimated over school observations using a bandwidth of one.

Table 7

Impact of CSCC Program on Teachers and Students														
	Teachers Experience (in years)		Teacher Experience (% more 9 years)		Teachers Tenure (in years)		Teacher Tenure (% more 4 years)		Insuficient Attendance		Grade retention		Dropouts	
	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.	LLR	Polynom.
Panel A: Bandwidth = 0.25														
CSCC Program	3.467**	2.119	28.031***	18.131	0.156	2.319*	2.717	36.713***	1.491	6.318**	0.428	-2.781	-0.383	0.195
	(1.738)	(4.087)	(9.274)	(22.045)	(0.854)	(1.303)	(7.829)	(12.874)	(1.969)	(2.819)	(1.843)	(2.507)	(0.392)	(0.684)
Observations	124	124	124	124	124	124	124	124	125	125	125	125	125	125
Panel B: Bandwidth = 0.5														
CSCC Program	3.075***	4.436	25.530***	30.052**	0.207	1.538	4.302	14.041	-1.052	4.976**	0.897	-0.142	-0.015	-0.843
	(1.141)	(2.711)	(6.159)	(14.637)	(0.632)	(1.068)	(5.27)	(10.095)	(1.448)	(2.443)	(1.24)	(2.358)	(0.363)	(0.553)
Observations	235	235	235	235	236	236	236	236	238	238	238	238	238	238
Panel C: Bandwidth = 1.0														
CSCC Program	2.541***	3.196*	21.007***	25.370***	0.549	0.253	6.682*	4.888	-2.481**	0.284	-0.225	0.15	-0.423	-0.267
	(0.769)	(1.79)	(4.211)	(9.599)	(0.455)	(0.852)	(3.79)	(7.649)	(1.022)	(2.134)	(0.827)	(1.847)	(0.283)	(0.417)
Observations	412	412	412	412	413	413	413	413	418	418	418	418	418	418
Panel C: Bandwidth = 1.5														
CSCC Program	2.257***	3.728***	17.413***	27.000***	0.29	0.166	3.687	4.466	-1.033	0.31	0.21	0.111	-0.219	0.016
	(0.648)	(1.409)	(3.514)	(7.551)	(0.39)	(0.729)	(3.091)	(6.293)	(0.85)	(1.72)	(0.693)	(1.504)	(0.234)	(0.43)
Observations	539	539	539	539	540	540	540	540	547	547	547	547	547	547

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

Year 2010. LLR: local linear regression estimation. Polynom.: 3erd order polynomial with interaction terms between polynomial terms and treatment. Robust standard errors in parenthesis

Impact of CSCC Program on Teachers

(observations at the teacher level)

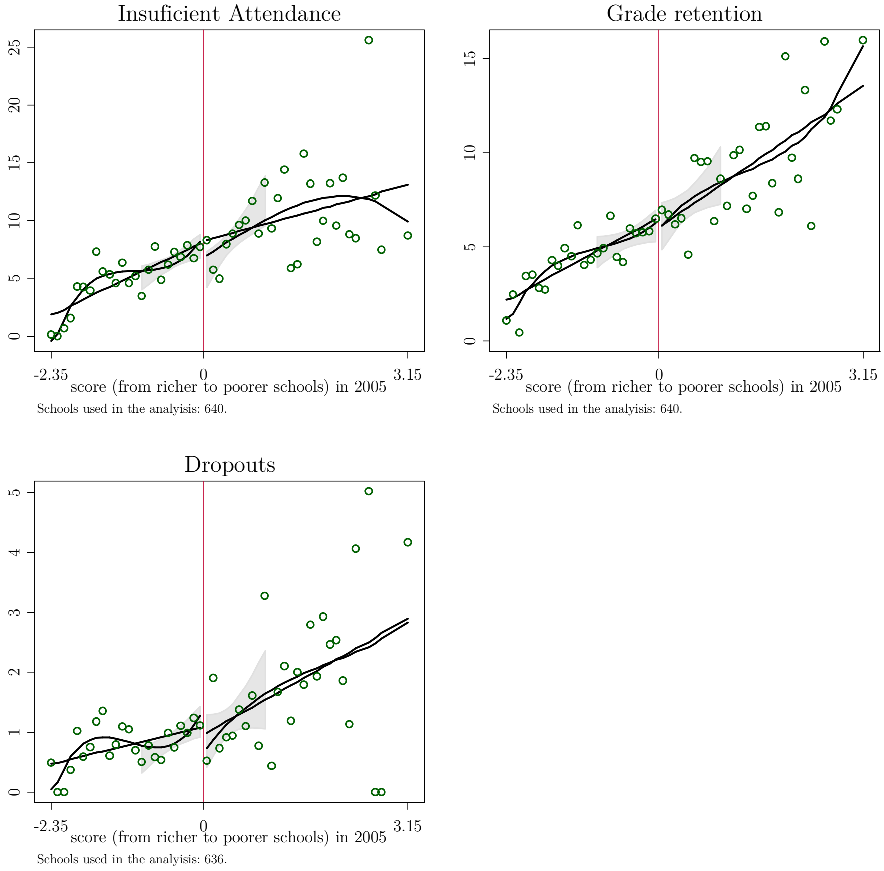
	Teachers Experience (in years)		Teachers Tenure (in years)	
	LLR	Polynom.	LLR	Polynom.
Panel A: Bandwidth = 0.25				
CSCC Program	3.525**	3.611	0.45	2.034
	[1.477]	[3.086]	[0.754]	[1.228]
Observations	1582	1582	1610	1610
Panel B: Bandwidth = 0.5				
CSCC Program	2.735***	4.759**	0.257	1.684*
	[1.025]	[2.160]	[0.570]	[0.968]
Observations	3,021	3,021	3,080	3,080
Panel C: Bandwidth = 1.0				
CSCC Program	2.349***	3.471**	0.428	0.41
	[0.685]	[1.535]	[0.417]	[0.717]
Observations	5,309	5,309	5,383	5,383
Panel C: Bandwidth = 1.5				
CSCC Program	2.255***	3.392***	0.213	0.275
	[0.588]	[1.246]	[0.361]	[0.634]
Observations	7,200	7,200	7,290	7,290

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

Year 2010. LLR: local linear regression estimation. Polynom.: 3erd order polynomial with interaction terms between polynomial terms and treatment. Robust standard errors in parenthesis clustered at the school level.

Graph 7

Impact of CSCC Program on Students



Outcomes for 2010, treatment started in 2005. Schools grouped in 52 bins of width 0.1.
Linear regression plot and third order polynomial reg with interaction terms estimated over the bins.
Shade areas are 95% CI for the linear regression estimated over school observations using a bandwidth of one.

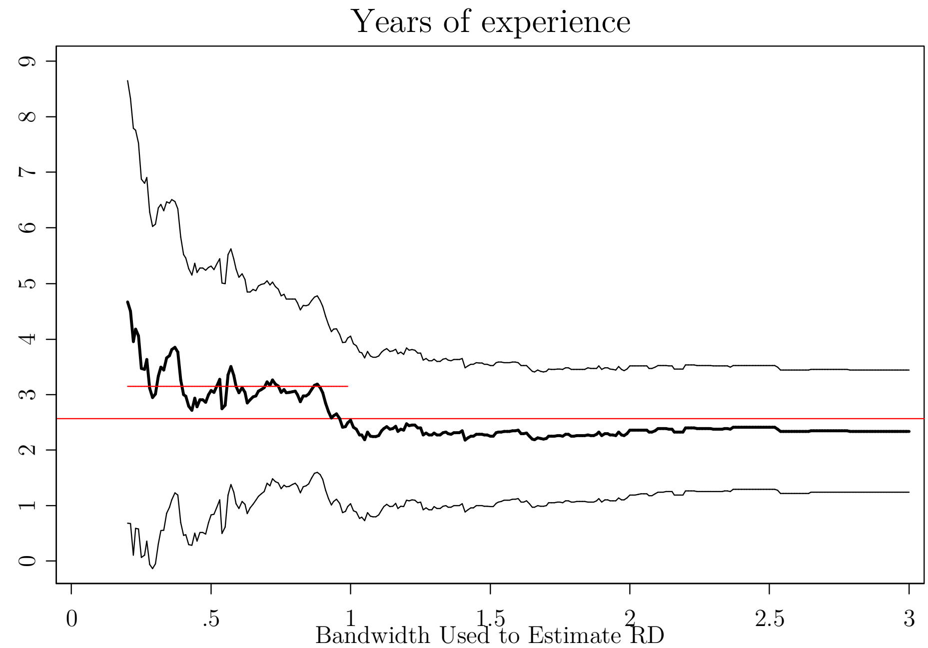
Table 9

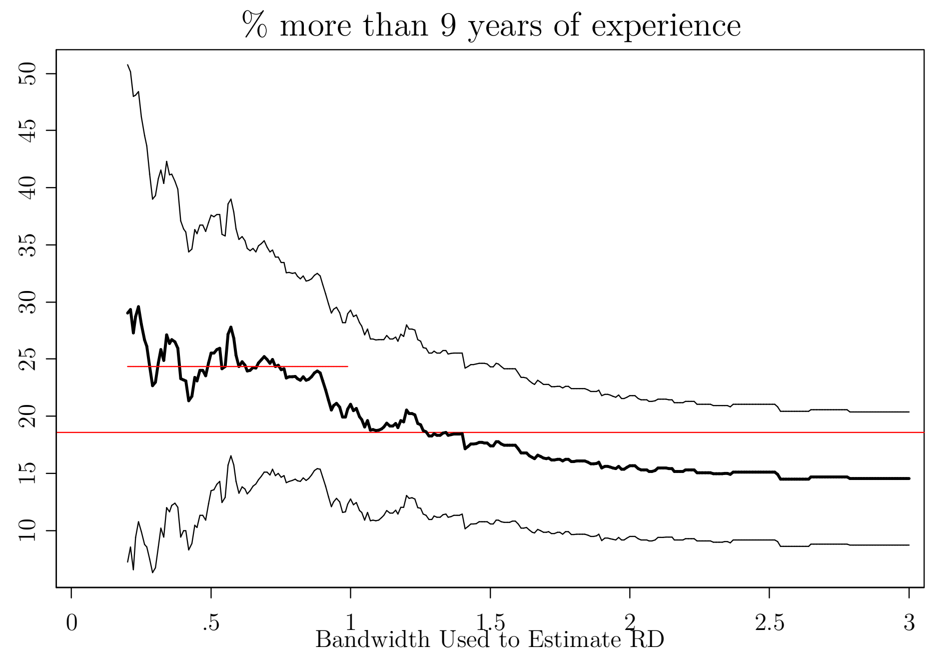
Impact of CSCC Program on Teachers										
Different polynomial orders and goodnes of fit tests										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Teachers Experience (in years)					Teacher Experience (% more 9 years)				
Polynomial of order:	zero	one	two	three	four	zero	one	two	three	four
Panel A: Bandwidth = 0.25										
CSCC Program	2.177***	3.467**	4.526	2.119	3.903	16.766***	28.031***	23.415	18.131	32.426
	(0.723)	(1.738)	(2.933)	(4.087)	(4.842)	(3.982)	(9.274)	(15.746)	(22.045)	(26.076)
Observations	124	124	124	124	124	124	124	124	124	124
pval	0.925	0.165	0.225	0.136	0.231	0.335	0.300	0.399	0.281	0.490
AIC	696.2	699.1	702.2	704.4	707.2	1113.3	1114.0	1116.9	1120.6	1122.5
AIC comparison	1.00	0.24	0.05	0.02	0.00	1.00	0.71	0.17	0.03	0.01
Panel B: Bandwidth = 0.5										
CSCC Program	1.514***	3.075***	3.503*	4.436	3.007	12.692***	25.530***	22.419**	30.052**	19.131
	(0.526)	(1.141)	(1.884)	(2.711)	(3.564)	(2.86)	(6.159)	(10.155)	(14.637)	(19.263)
Observations	235	235	235	235	235	235	235	235	235	235
pval	0.284	0.495	0.342	0.518	0.019	0.138	0.701	0.838	0.742	0.165
ACI	1321.5	1321.9	1324.5	1325.8	1328.7	2114.3	2111.3	2113.5	2115.5	2116.5
AIC comparison	1.00	0.81	0.23	0.12	0.03	0.22	1.00	0.33	0.12	0.07
Panel C: Bandwidth = 1.0										
CSCC Program	0.542	2.541***	3.657***	3.196*	3.838	5.941***	21.007***	27.030***	25.370***	23.047*
	(0.415)	(0.769)	(1.26)	(1.79)	(2.397)	(2.249)	(4.211)	(6.827)	(9.599)	(13.01)
Observations	412	412	412	412	412	412	412	412	412	412
pval	0.062	0.243	0.303	0.055	0.074	0.006	0.342	0.390	0.116	0.219
AIC	2341.3	2336.0	2338.5	2341.0	2344.0	3721.9	3707.6	3709.7	3711.8	3712.9
AIC comparison	0.07	1.00	0.29	0.08	0.02	0.00	1.00	0.35	0.12	0.07
Panel C: Bandwidth = 1.5										
CSCC Program	0.015	2.257***	2.972***	3.728***	3.634**	2.821	17.413***	24.991***	27.000***	26.238***
	(0.359)	(0.648)	(0.982)	(1.409)	(1.848)	(1.974)	(3.514)	(5.341)	(7.551)	(9.863)
Observations	539	539	539	539	539	539	539	539	539	539
pval	0.000	0.044	0.127	0.200	0.176	0.000	0.048	0.159	0.201	0.142
AIC	3048.3	3030.7	3032.2	3034.0	3036.8	4863.6	4839.6	4838.5	4841.6	4845.2
AIC comparison	0.00	1.00	0.48	0.20	0.05	0.00	0.58	1.00	0.20	0.03

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

Year 2010. Polynomial of order zero is a comparison of means at both sides of the cutoff. Polynomial of order one and above, are linear regression with polynomial terms, and their interaction with treatment dummy. Robust standard errors in parenthesis. P-val is a test of the joint significance of a set of bin dummies. A small value (p<0.05) indicates that the model is not well specified. AIC is the Akaike maximum likelihood information criterion of model selection. A smaller value indicates that the model is preferredt. AIC comparison si done with the formula: exp((AICmin−AICi)/2).

Graph 8





Each point of the graph is the estimated outcome for teachers, estimated with different RD local linear regressions by gradually increasing the bandwidth (so in each regression we include more schools to estimate the impact of the program). Confidence intervals of 95% are also plotted. The two horizontal lines show the mean impact with a bandwidth of one and with the entire sample of schools.

Table 10

Impact of CSCC Program on Teachers

Variations: controls and interaction terms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Teachers Experience (in years)						Teacher Experience (% more 9 years)					
	LLR	LLR	Polynom.	Polynom.	Polynom.	Polynom.	LLR	LLR	Polynom.	Polynom.	Polynom.	Polynom.
Panel A: Bandwidth = 0.25												
CSCC Program	3.467**	3.262*	2.119	4.457*	4.571*	1.961	28.031***	26.854***	18.131	24.240*	24.959*	17.58
	(1.738)	(1.688)	(4.087)	(2.571)	(2.407)	(3.795)	(9.274)	(9.017)	(22.045)	(13.815)	(13.275)	(21.439)
Observations	124	124	124	124	124	124	124	124	124	124	124	124
Interaction terms	n/a	n/a	YES	NO	NO	YES	n/a	n/a	YES	NO	NO	YES
Controls	NO	YES	NO	NO	YES	YES	NO	YES	NO	NO	YES	YES
Panel B: Bandwidth = 0.5												
CSCC Program	3.075***	2.536**	4.436	3.558**	3.160**	4.073*	25.530***	23.236***	30.052**	23.776***	22.418***	28.540**
	(1.141)	(1.133)	(2.711)	(1.62)	(1.545)	(2.464)	(6.159)	(6.159)	(14.637)	(8.607)	(8.367)	(13.578)
Observations	235	235	235	235	235	235	235	235	235	235	235	235
Interaction terms	n/a	n/a	YES	NO	NO	YES	n/a	n/a	YES	NO	NO	YES
Controls	NO	YES	NO	NO	YES	YES	NO	YES	NO	NO	YES	YES
Panel C: Bandwidth = 1.0												
CSCC Program	2.541***	2.200***	3.196*	3.389***	3.058***	2.514	21.007***	19.345***	25.370***	24.811***	23.244***	22.285**
	(0.769)	(0.766)	(1.79)	(1.076)	(1.061)	(1.778)	(4.211)	(4.238)	(9.599)	(5.693)	(5.678)	(9.603)
Observations	412	412	412	412	412	412	412	412	412	412	412	412
Interaction terms	n/a	n/a	YES	NO	NO	YES	n/a	n/a	YES	NO	NO	YES
Controls	NO	YES	NO	NO	YES	YES	NO	YES	NO	NO	YES	YES
Panel C: Bandwidth = 1.5												
CSCC Program	2.257***	2.067***	3.728***	2.559***	2.342***	3.203**	17.413***	16.613***	27.000***	21.987***	21.140***	24.756***
	(0.648)	(0.627)	(1.409)	(0.85)	(0.84)	(1.377)	(3.514)	(3.449)	(7.551)	(4.533)	(4.539)	(7.411)
Observations	539	539	539	539	539	539	539	539	539	539	539	539
Interaction terms	n/a	n/a	YES	NO	NO	YES	n/a	n/a	YES	NO	NO	YES
Controls	NO	YES	NO	NO	YES	YES	NO	YES	NO	NO	YES	YES

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

Year 2010. LLR: local linear regression estimation. Polynom.: 3erd order polynomial with interaction terms between polynomial terms and treatment. Robust standard errors in parenthesis.
Control: the following pre-treatmen (2004) variables: insufficient attendance, grade retention, dropouts, number of students and group size.

Table 11 - Panel Data Analysis I				
	(1)	(2)	(3)	(4)
	Current Impact			
	Pooled	Fixed Effects	Fixed Effects	Fixed Effects
Effect on Teacher Experience				
CSCC Program	-0.401	1.272***	1.129***	1.117***
	[0.253]	[0.169]	[0.183]	[0.184]
Observations	6,195	6,195	6,195	6,195
Years	2002/2010	2002/2010	2002/2010	2002/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Teacher Tenure				
CSCC Program	-0.288**	0.449***	0.200*	0.195*
	[0.131]	[0.106]	[0.108]	[0.109]
Observations	6,226	6,226	6,226	6,226
Years	2002/2010	2002/2010	2002/2010	2002/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Insuficient Attendance				
CSCC Program	3.597***	-0.317*	-0.273*	-0.238
	[0.263]	[0.173]	[0.165]	[0.165]
Observations	14,525	14,525	14,525	14,525
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Grade Retention				
CSCC Program	3.312***	-1.749***	-0.085	-0.015
	[0.248]	[0.187]	[0.165]	[0.161]
Observations	14,529	14,529	14,529	14,529
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Dropouts				
CSCC Program	0.853***	0.006	0.003	-0.004
	[0.096]	[0.095]	[0.098]	[0.099]
Observations	14,525	14,525	14,525	14,525
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES

*** p<0.01, ** p<0.05, * p<0.1. Standard Errors clustered at the school level.

Controls include: number of students in the school, number of students in the first and sixth grade, total number of groups in the school and number of groups in first and sixth year.

Table 12

Panel Data Analysis II																
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Impact after 1 year				Impact after 2 years				Impact after 3 years				Impact after 4 years			
	Pooled	Fixed Effects	Fixed Effects	Fixed Effects	Pooled	Fixed Effects	Fixed Effects	Fixed Effects	Pooled	Fixed Effects	Fixed Effects	Fixed Effects	Pooled	Fixed Effects	Fixed Effects	Fixed Effects
Effect on Teacher Experience																
CSCC Program	-0.361	1.147***	1.058***	1.064***	-0.540**	0.668***	0.642***	0.648***	-0.748***	0.15	0.285*	0.275*	-0.833***	0.01	0.23	0.221
	[0.248]	[0.150]	[0.163]	[0.164]	[0.247]	[0.133]	[0.143]	[0.144]	[0.255]	[0.154]	[0.160]	[0.160]	[0.263]	[0.147]	[0.151]	[0.151]
Observations	6,103	6,103	6,103	6,103	6,038	6,038	6,038	6,038	5,944	5,944	5,944	5,944	5,835	5,835	5,835	5,835
Years	2001/2009	2001/2009	2001/2009	2001/2009	2000/2008	2000/2008	2000/2008	2000/2008	1999/2007	1999/2007	1999/2007	1999/2007	1998/2006	1998/2006	1998/2006	1998/2006
School Fixed Effects	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Controls	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
Effect on Teacher Tenure																
CSCC Program	-0.242*	0.462***	0.274***	0.272***	-0.290**	0.209***	0.195**	0.187**	-0.392***	-0.073	0.114	0.103	-0.429***	-0.098	0.093	0.083
	[0.128]	[0.091]	[0.092]	[0.093]	[0.126]	[0.076]	[0.079]	[0.079]	[0.125]	[0.078]	[0.074]	[0.074]	[0.130]	[0.081]	[0.079]	[0.078]
Observations	6,134	6,134	6,134	6,134	6,068	6,068	6,068	6,068	5,975	5,975	5,975	5,975	5,866	5,866	5,866	5,866
Years	2001/2009	2001/2009	2001/2009	2001/2009	2000/2008	2000/2008	2000/2008	2000/2008	1999/2007	1999/2007	1999/2007	1999/2007	1998/2006	1998/2006	1998/2006	1998/2006
School Fixed Effects	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Controls	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
Effect on Students Insuficient Attendance																
CSCC Program	3.607***	-0.276	-0.254	-0.248	3.611***	-0.239	-0.169	-0.177	3.704***	-0.012	-0.002	-0.006	3.635***	0.17	0.066	0.071
	[0.266]	[0.181]	[0.174]	[0.173]	[0.274]	[0.172]	[0.166]	[0.164]	[0.290]	[0.170]	[0.170]	[0.168]	[0.313]	[0.171]	[0.178]	[0.176]
Observations	13,392	13,392	13,392	13,392	12,390	12,390	12,390	12,390	11,425	11,425	11,425	11,425	10,462	10,462	10,462	10,462
Years	1992/2009	1992/2009	1992/2009	1992/2009	1992/2008	1992/2008	1992/2008	1992/2008	1992/2007	1992/2007	1992/2007	1992/2007	1992/2006	1992/2006	1992/2006	1992/2006
School Fixed Effects	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Controls	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
Effect on Students Grade Retention																
CSCC Program	3.412***	-1.510***	-0.15	-0.099	3.520***	-1.365***	-0.327*	-0.299	3.630***	-1.147***	-0.305	-0.279	3.524***	-1.120***	-0.302	-0.287
	[0.246]	[0.191]	[0.165]	[0.164]	[0.254]	[0.204]	[0.185]	[0.185]	[0.261]	[0.217]	[0.202]	[0.202]	[0.272]	[0.232]	[0.212]	[0.211]
Observations	13,393	13,393	13,393	13,393	12,391	12,391	12,391	12,391	11,425	11,425	11,425	11,425	10,462	10,462	10,462	10,462
Years	1992/2009	1992/2009	1992/2009	1992/2009	1992/2008	1992/2008	1992/2008	1992/2008	1992/2007	1992/2007	1992/2007	1992/2007	1992/2006	1992/2006	1992/2006	1992/2006
School Fixed Effects	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Controls	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES
Effect on Students Dropouts																
CSCC Program	0.844***	0.01	0.01	0.011	0.886***	0.095	0.126	0.125	0.836***	0.048	0.045	0.045	0.795***	0.036	0.014	0.017
	[0.101]	[0.108]	[0.105]	[0.107]	[0.104]	[0.108]	[0.115]	[0.116]	[0.104]	[0.082]	[0.092]	[0.092]	[0.110]	[0.085]	[0.094]	[0.093]
Observations	13,392	13,392	13,392	13,392	12,390	12,390	12,390	12,390	11,425	11,425	11,425	11,425	10,462	10,462	10,462	10,462
Years	1992/2009	1992/2009	1992/2009	1992/2009	1992/2008	1992/2008	1992/2008	1992/2008	1992/2007	1992/2007	1992/2007	1992/2007	1992/2006	1992/2006	1992/2006	1992/2006
School Fixed Effects	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
Controls	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES	NO	NO	NO	YES

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

Standard Errors clustered at the school level

Controls include: number of students in the school, number of students in the first and sixth grade, total number of groups in the school and number of groups in first and sixth year.

Table 13 - Panel Data Analysis

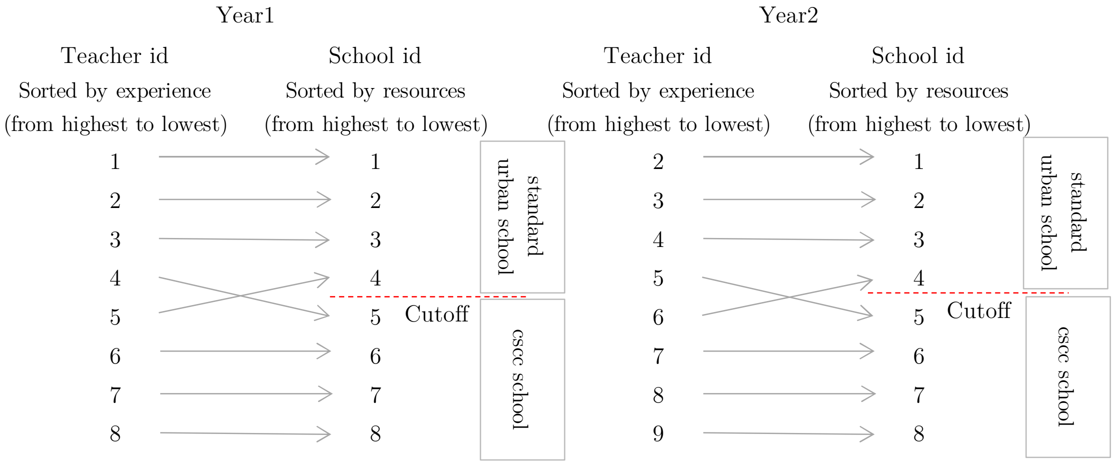
Individual Teachers and Grades observations

	(1)	(2)	(3)	(4)
	Pooled OLS	Fixed Effects	Fixed Effects	Fixed Effects
Effect on Teacher Experience				
CSCC Program	-0.944***	1.472***	1.313***	1.280***
	(0.256)	(0.155)	(0.168)	(0.17)
Observations	78,300	78,300	78,300	78,300
Years	2002/2010	2002/2010	2002/2010	2002/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Teacher Tenure				
CSCC Program	-0.453***	0.615***	0.310***	0.297***
	(0.125)	(0.093)	(0.097)	(0.098)
Observations	79,383	79,383	79,383	79,383
Years	2002/2010	2002/2010	2002/2010	2002/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Insuficient Attendance				
CSCC Program	3.394***	-0.143	-0.199	-0.197
	(0.251)	(0.169)	(0.16)	(0.16)
Observations	87,023	87,023	87,023	87,023
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Grade Retention				
CSCC Program	2.918***	-1.424***	0.049	0.074
	(0.223)	(0.172)	(0.154)	(0.153)
Observations	87,044	87,044	87,044	87,044
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES
Effect on Students Dropouts				
CSCC Program	0.810***	0.034	0.022	0.018
	(0.091)	(0.093)	(0.095)	(0.097)
Observations	87,023	87,023	87,023	87,023
Years	1992/2010	1992/2010	1992/2010	1992/2010
School Fixed Effects	NO	YES	YES	YES
Year Fixed Effects	NO	NO	YES	YES
Controls	NO	NO	NO	YES

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

Standard errors in parenthesis clustered at the school level. Controls include: number of students in the school, number of students in the first and sixth grade, total number of groups in the school and number of groups in first and sixth year

Diagram 1


Year1
Teacher id
Sorted by experience
(from highest to lowest)
School id
Sorted by resources
(from highest to lowest)
1
2
3
4
5
6
7
8
1
2
3
4
5
6
7
8
standard urban school
Cutoff
cscc school
Year2
Teacher id
Sorted by experience
(from highest to lowest)
School id
Sorted by resources
(from highest to lowest)
2
3
4
5
6
7
8
9
1
2
3
4
5
6
7
8
standard urban school
Cutoff
cscc school

Web Appendix for

Extra Resources for Poor Schools: Impact on Teachers and Students

José María Cabrera
Dinand Webbink

September 29th, 2014

Web Appendix 1

Descriptive Statistics (weighted)				
	(1)	(2)	(3)	(4)
	Standard Urban	CSCC	difference (2-1)	t-stat
General				
Years in the sample	1992-2010	1995-2010	-	-
Number of observations	11,425	3,106	-	-
Schools per year	601	194	-	-
Schools				
Average Years as CSCC	2.17	9.5	7.33	[89.83] ***
Proportion in Montevideo	27%	33%	6%	[6.53] ***
Number of students	299.71	316.4	16.69	[4.66] ***
Number of teachers	10.71	11.69	0.98	[8.48] ***
Number of groups	11.27	12.09	0.82	[7.87] ***
Students per teacher	28.04	27.07	-0.97	[10.4] ***
Students per group	27.12	26.84	-0.28	[2.64] ***
At least one group of more than 35 students	52%	53%	1%	[1.13]
Has computers for educational use	51%	44%	-7%	[5.42] ***
Has extra staff	68%	71%	3%	[1.5]
Has Maestro Comunitario	24%	96%	72%	[66.1] ***
School climate: verbal violence	47%	60%	13%	[2.89] ***
School climate: physical violence	41%	59%	18%	[3.98] ***
Teachers				
Experience (in years)	12.56	11.62	-0.94	[7.64] ***
Experience (% with more than 9 years)	51%	49%	-2%	[2.46] **
Tenure (in years at current school)	4.96	4.51	-0.45	[7.02] ***
Tenure (% more than 4 years at current school)	37%	34%	-3%	[5.35] ***
Students				
Insufficient Attendance in 1st grade	11%	16%	5%	[24.41] ***
Insufficient Attendance in 1st-6th grade	7%	11%	4%	[29.12] ***
Grade Retention in 1st grade	17%	24%	7%	[35.44] ***
Grade Retention in 1st-6th grade	9%	13%	4%	[31.53] ***
Dropouts in 1st grade	2%	3%	1%	[10.74] ***
Dropouts in 1st-6th grade	1%	2%	1%	[14.75] ***
Families				
Mothers education index 1996	17%	53%	36%	[19.43] ***
Mothers with primary education or less 2002	51%	71%	20%	[14.31] ***
Mothers with primary education or less 2005	40%	61%	21%	[15.3] ***
Mothers with primary education or less 2010	37%	57%	20%	[67.06] ***
Unemployed Household Head in 2010	8%	12%	4%	[5.22] ***
Children with unmet basic needs in 2005	35%	59%	24%	[15.97] ***
Children in PANES (conditional cash transfer)	17%	30%	13%	[10.39] ***

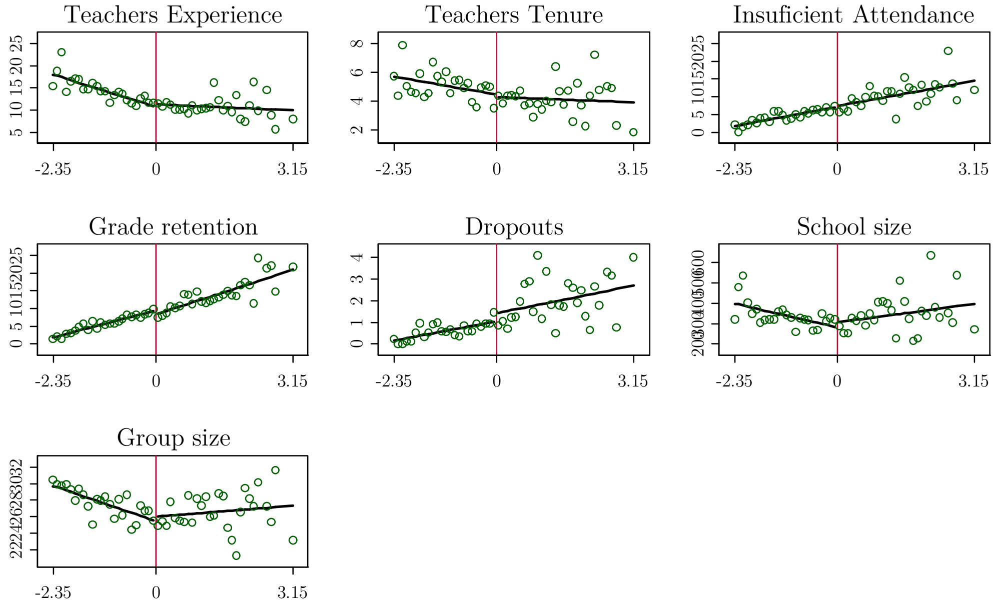
The difference in means is calculated with an OLS regression with robust standard errors. T-statistics in square brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Variables definition: *Montevideo* : proportion located in Montevideo, the capital city, number of students, teachers and groups are for 1st to 6th grade of primary education, period: [1992-2010]. Group of more than 35: is an indicator variable that takes the value of one if there is at least one group in the school that has more than 35 students. Computers for educational use is a dummy variable (==1) if the school has at least one computer for educational use ([1992-2009], expect for 2006). Has Extra Staff is equal to one if the school has teachers and technicians who are not in charge of a group [2005-2010]. Maestro Comunitario is a program that gives an extra teacher as explained in the text [2005-2010]. Verbal violence dummy variable equal to one if intimidation, mocking or verbal abuse among students is a moderate or serious problem [2009]. Physical violence is a dummy variable equal to one if physical violence between students is a moderate or serious problem [2009]. Teacher variables are measured in [2002-2010]. Students variables are measured in [1992-2010]. Dropouts are measured as those students that didn´t attend 70 days of classes in the school year. Insufficient Attendance is measured as those students that attended between 70 and 140 days. Mothers education index is constructed by taking the percent of students whose mothers education level is primary or less, and subtracting the percentage that finished secondary education.

Exepct for the first 8 variables, all the averages are weighted: lines 9, 17-20 by number of teachers; line 10 and 11 by number of groups; lines 12-16 and 21-33 by school size (number of students).

Web Appendix 2

Pre treatment characteristics 2004



Outcomes for 2004. Schools used in the analyisis: 638, grouped in 52 bins of width 0.1.
Linear regression estimated over the bins at both sides of the cutoff.

Web Appendix 3

Pre Treatment Balance (2004)

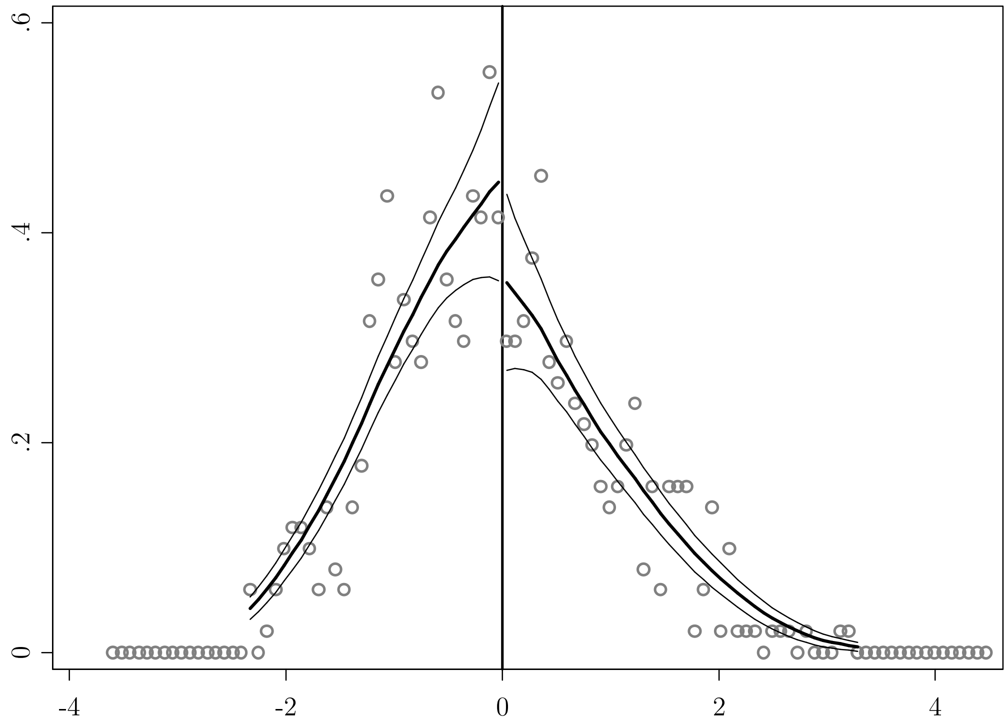
	Teachers Experience		Teachers Tenure		Insuficient Attendance		Grade retention		Dropouts		School size		Group size	
	LLR	3erd order poly	LLR	3erd order poly	LLR	3erd order poly	LLR	3erd order poly	LLR	3erd order poly	LLR	3erd order poly	LLR	3erd order poly
Panel A: Bandwidth = 0.25														
CSCC Program	1.237	-2.211	1.309*	0.868	-0.719	2.517	-2.296	-1.702	-0.266	1.193	-44.628	16.805	-0.663	1.336
	[1.623]	[3.690]	[0.751]	[1.227]	[1.379]	[2.352]	[1.675]	[3.408]	[0.522]	[0.977]	[60.357]	[95.613]	[2.793]	[5.240]
Observations	116	116	117	117	124	124	124	124	124	124	124	124	124	124
Panel B: Bandwidth = 0.5														
CSCC Program	-0.544	1.344	-0.447	2.284**	-1.54	0.337	-2.340**	-1.64	-0.56	-0.002	-88.635**	-21.415	-1.885	-0.486
	[1.090]	[2.393]	[0.581]	[0.939]	[1.168]	[1.725]	[1.121]	[2.450]	[0.347]	[0.697]	[44.694]	[81.067]	[1.943]	[3.936]
Observations	222	222	222	222	237	237	237	237	237	237	237	237	237	237
Panel C: Bandwidth = 1.0														
CSCC Program	0.131	0.142	0.311	0.529	-1.253	-1.269	-2.207***	-3.376**	-0.867***	-0.604	-69.517**	-94.616	-0.777	-2.079
	[0.784]	[1.614]	[0.425]	[0.780]	[0.894]	[1.471]	[0.785]	[1.715]	[0.297]	[0.575]	[32.660]	[63.182]	[1.383]	[2.865]
Observations	395	395	395	395	417	417	417	417	417	417	417	417	417	417
Panel B: Bandwidth = 1.5														
CSCC Program	-0.41	-0.207	0.064	-0.247	-0.284	-1.03	-0.934	-2.779**	-0.155	-0.525	-52.797*	-94.955*	-0.554	-2.082
	[0.695]	[1.312]	[0.366]	[0.677]	[0.767]	[1.306]	[0.672]	[1.410]	[0.243]	[0.506]	[28.451]	[54.168]	[1.131]	[2.315]
Observations	520	520	520	520	546	546	546	546	546	546	546	546	546	546

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

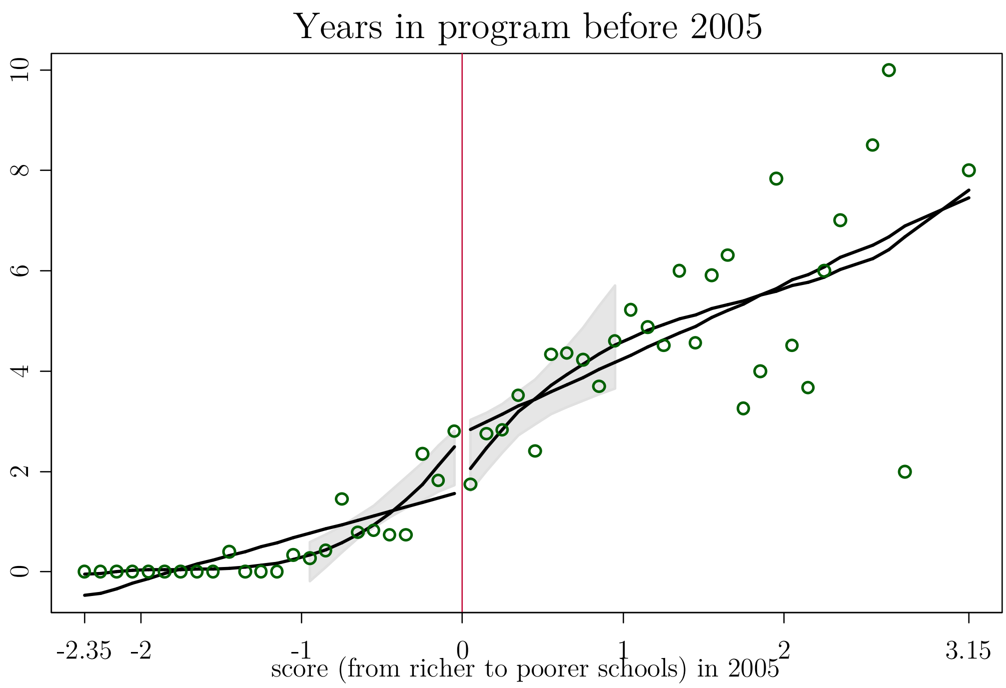
3erd order polynomial with interaction terms between polynomial terms and treatment

Web appendix 4

McCrary (2008) test (and code).



Web appendix 5



640 schools used in the analyisis.

Web appendix 6

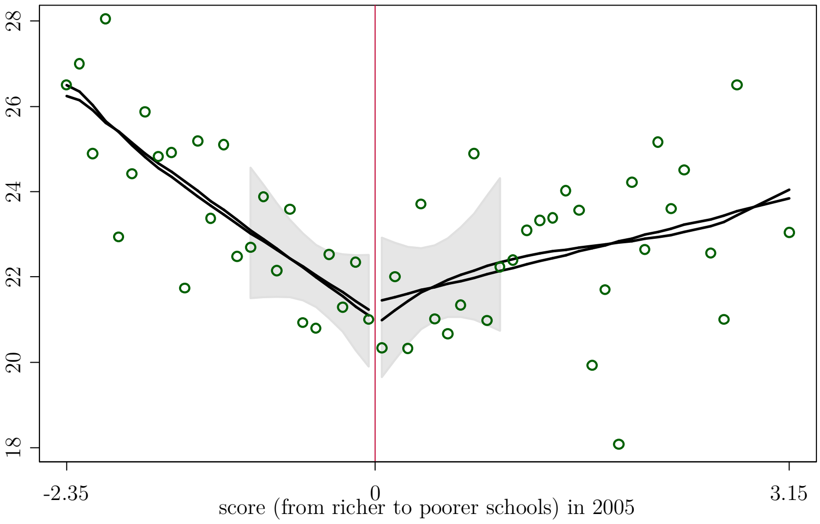
	exp((AIC$_{\min}$−AIC$_i$)/2)									
Outcome:	Teachers Experience (in years)					Teacher Experience (% more 9 years)				
Polynomial of order:	zero	one	two	three	four	zero	one	two	three	four
Bandwidth 0.25	1.00	0.24	0.05	0.02	0.00	1.00	0.71	0.17	0.03	0.01
Bandwidth 0.5	1.00	0.81	0.23	0.12	0.03	0.22	1.00	0.33	0.12	0.07
Bandwidth 1.0	0.07	1.00	0.29	0.08	0.02	0.00	1.00	0.35	0.12	0.07
Bandwidth 1.5	0.00	1.00	0.48	0.20	0.05	0.00	0.58	1.00	0.20	0.03

Outcome:	Teachers Experience (in years)					Teacher Experience (% more 9 years)				
Polynomial of order:	zero	one	two	three	four	zero	one	two	three	four
Bandwidth 0.25	2.177***	3.467**	4.53	2.12	3.90	16.766***	28.031***	23.42	18.13	32.43
Bandwidth 0.5	1.514***	3.075***	3.503*	4.44	3.01	12.692***	25.530***	22.419**	30.052**	19.13
Bandwidth 1.0	0.54	2.541***	3.657***	3.196*	3.84	5.941***	21.007***	27.030***	25.370***	23.047*
Bandwidth 1.5	0.02	2.257***	2.972***	3.728***	3.634**	2.82	17.413***	24.991***	27.000***	26.238***

Dark grey highlights the coefficient of the model with lowest AIC criterion, and light grey indicates the second option.

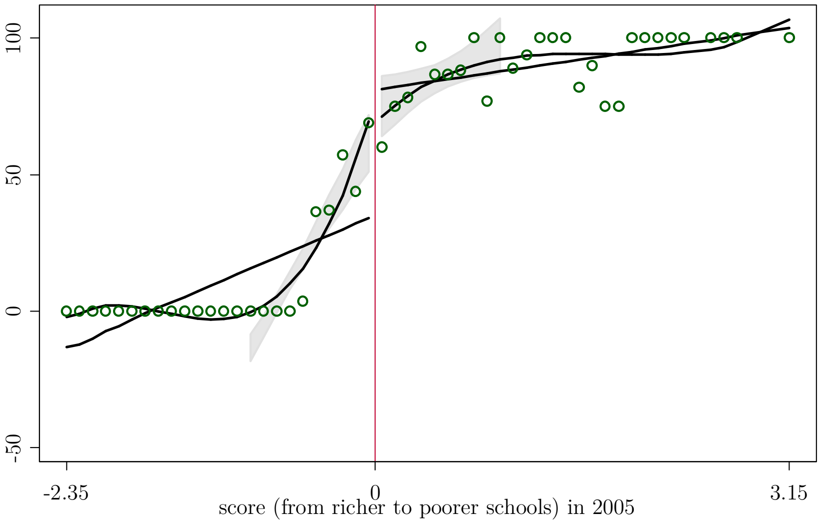
Web appendix 7

Group size



Schools used in the analyisis: 640.

School has Maestro Comunitario



Schools used in the analyisis: 640.

Fuzzy Regression Discontinuity

In a Fuzzy RD we need a discontinuity rule that has imperfect compliance. We suspect that this is what happened in the previous years. We find that not every school that crosses the threshold receives treatment (and some schools that weren't poor received the extra resources). But crossing the threshold should increase the treatment probability, even if other factors play a role. So we can employ an instrumental variable strategy, and look in a first stage the effect of crossing the threshold on the probability of receiving the *CSCC* treatment (a relevant first stage)[62]; and then looking at the effect of crossing the threshold on the outcomes of interest. We will now study the two reallocations of schools that were done before 2005[63]:

In the 1999 assignment of schools to the program, the education authorities employed three indicators: the percentage of children in 6th grade whose mother maximum education level attained is primary school, the repetition rate and insufficient attendance in 1st grade of primary school (ANEP 2004). We have a proxy for the first indicator that shows a discontinuity. This proxy is a socio-education index that is constructed by taking the percent of students whose mothers education level is primary or less, and substracting the percentage that finished secondary education[64]. We present this result in Graph A1. There are bins at the right with a strange pattern: some of the poorest schools didn't receive the treatment (but they don't affect the probability at the cutoff). They are schools that are (mainly) not located in the capital city, Montevideo[65]. The two other indicators that were used for the assignment in 1999 (repetition and insufficient attendance in 1st grade of primary school) don't present a clear discontinuity (Graph A2 with a scatter using different bandwidths)[66]. In some way this is good news, since repetition and insufficient attendance (measured at the school level), are outcome variables.

The estimated impact at the cutoff is a 20% increase in the probability of receiving the treatment (first stage of the 2SLS IV estimation with the entire sample, with t-stat of 4.85). But when we get closer to the threshold with a smaller bandwidth (thus using fewer observations), the power of the first stage estimation is not enough to get results of the im-

---

[62] The instrument should not be correlated with the error term in the second stage (reduced form). This exclusion restriction is met since the treatment is randomly assigned in the neighborhood of the cutoff.

[63] We don't have the data on the assignment variable to perform an RD analysis of the 1995 initial allocation of schools. In that year, the program was called *Requerimiento Prioritario*. The allocation wasn't performed with a socioeconomic characterization of the students. It was based, inter alia, on data from the neighborhood were the school was located, constructed with information from the National Households Surveys (and not directly from surveys to the students as in the following years) (ANEP, 2007).

[64] The socioeconomic index was measured in 1996 with questionnaires to the parents of students. It runs from -100 to 100. It is used in the official statistics to classify the schools into different socioeconomic contexts.

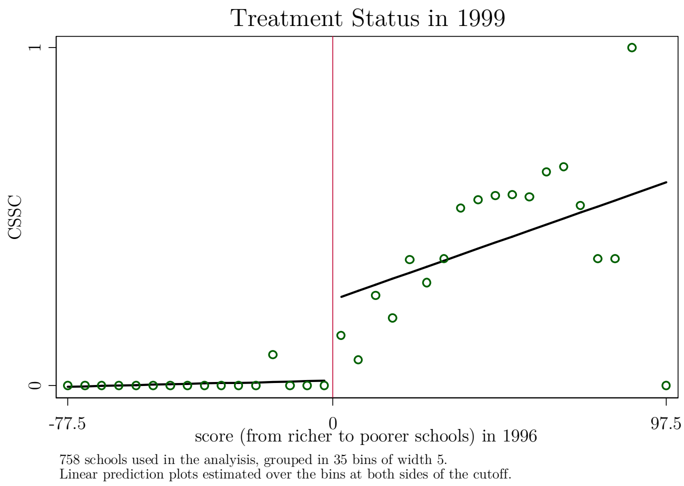
[65] There are 100 schools that have a score of more than 50 points (poor schools), and didn't receive treatment. Only 8 of them are located in Montevideo. At the cutoff there are no differences in the locations of schools between Montevideo and the rest of the country.

[66] The results are the same if we use data from 1996 or 1998.

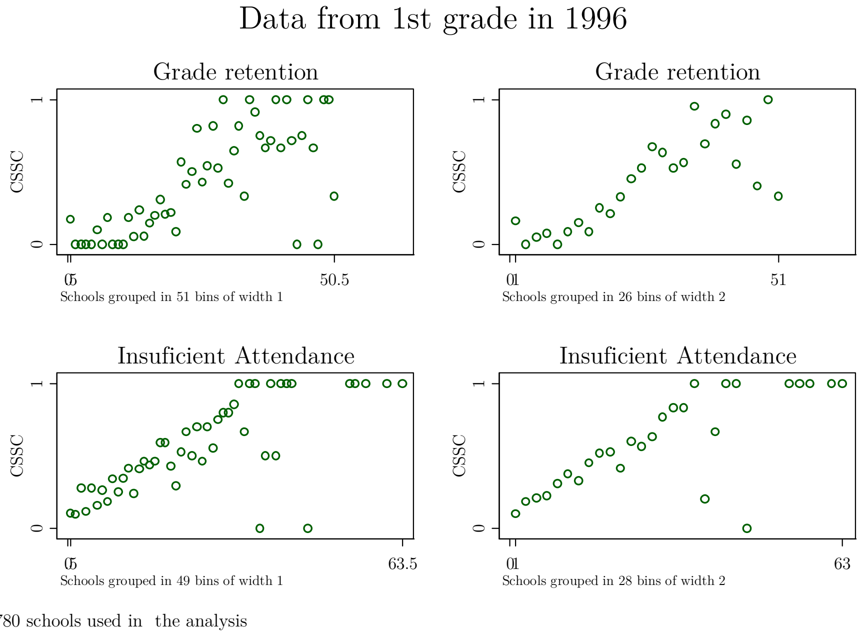
pact of the program[67]. The result of the second stage is that the program had no impact on the students outcomes (insufficient attendance, grade retention and dropouts). But we can't claim that there was no impact of the program because the estimations were imprecise (we don't reject the hypothesis of an estimated impact of zero with tight confidence intervals)[68].

The next re-categorization of schools was performed in 2002, and the indicators used focused exclusively on socioeconomic variables of students (ANEP 2007). We didn't find a clear discontinuity in the assignment variables when we perform the graphical analysis. So to identify the effects of the program we will rely on panel data techniques.

Graph A1



Graph A2



---

67 In the neighborhood of ± 10 points of the score, and using 107 observations, the jump in the probability of receiving the treatment is estimated to be 17% with a t-statistic of 1.93.

68 Full results for this section are available upon request.