Poverty &
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PEP

Réseau de recherche sur
les politiques économiques
et la pauvreté

8th PEP GENERAL MEETING
Dakar, Senegal - June 2010

Assessing the Impact of Argentina's *Ley Federal de Educacion* on Educational and Labor Outcomes

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PIERI2 - Non-Experimental

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Final Research Report

Assessing the impact of Argentina's *Ley Federal de Educación* on educational and labor outcomes *

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May 3rd 2010

Abstract

In the early nineties the Argentina's Congress passed a law aimed at changing some key characteristics of the educational system. Chiefly among them, compulsory education was extended in two years. The timing in the implementation of the reform substantially varied across provinces, providing a source of identification for the causal effect of the law. We estimate difference-in-difference models to assess the impact of the reform on various educational and labor outcomes.

* The authors would like to thank Fabio Veras Soares, Martín Valdivia, Chris Ryan, Verónica Amarante, and participants from the 7th PEP General Conference in Manila, and to Federico Barra for providing GIS data on the location of Argentine schools. We are especially grateful to the assistance of Carolina Garcia Domench, Mariana Marchionni, and Mariana Viollaz who participated at several stages of the project.

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1 Introduction

In 1993 the Argentine Congress passed a law (*Ley Federal de Educación*, LFE henceforth) aimed at changing some important characteristics of the educational system. Chiefly among them, there was a change in the structure of the educational curricula, along with an extension in the years of compulsory education. While in the previous system a child was obliged to attend seven years of primary school, under the new legislation that compulsory educational level was extended to nine years.

By increasing the obligatory number of years of education, the government sought to force mostly poor children to increase their human capital accumulation, and induce some of them to continue studying in the secondary level, and hopefully into college. More educated youths are expected to perform better in the labor market, and hence have a lower probability of falling into poverty.

The impact of this major policy change is still mostly unknown, in part due to the fact that the Argentine government did not implement an explicit strategy to evaluate the policy intervention. The literature on the LFE is scarce, mostly descriptive and based on cursory non-conditional evidence.

The Reform is still under much heated debate. In fact, the government recently passed a law undoing some of the changes in the curricula, and at extending the years of mandatory schooling to twelve years. The lack of causal evidence regarding the impact of the Reform on key educational and labor outcomes forces the discussion among policy makers and political actors to rely merely on existing descriptive studies.

The aim of this paper is to contribute to this debate shedding some light on the effect of the LFE on several educational outcomes and labor market variables. In order to analyze this issue, we exploit the regional heterogeneity in the timing of the reform. Argentina is a federal country where primary and secondary public education are administered and financed at the provincial level. Although the LFE was a federal law to be complied with in all provinces, there was flexibility for state governments to decide on the timing of the reform. While in some provinces the reform was quickly implemented

after the LFE was passed, in others the pace of the changes was slower. In fact, in some districts many central aspects of the reform were never implemented.

Taking advantage of this source of variation in the exposition to the "treatment", we study the impact on different educational and labor market outcomes. In particular, we are interested in evaluating whether poor youngsters who were forced to attend two additional school years have now a higher likelihood of finishing high school. We also analyze whether the quality of the education received by these youths improved because of the LFE. Finally, we look at the effect of the reform on labor market outcomes such as labor income, employment and hours worked.

The main contribution of this paper is to fill a gap on the literature about one of the largest educational reforms in Latin America.

The rest of this report is organized as follows. The Argentina's educational system and the LFE are described in section 2. Section 3 reviews the literature on impact evaluation of school reforms, and the literature on the LFE. Section 4 briefly sketches the methodology and describes the databases used. Sections 5 and 6 are the core of the report, as they present the main results on the effects of the LFE on education and labor variables. Section 7 presents further evidence on the effect of the reform on school scores by exploiting a regression discontinuity design. Section 8 concludes with a summary of the main findings obtained so far, and comments on the future steps of the research.

2 The educational reform

During the 1990's the Argentina's government carried out a large package of structural reforms including trade and financial liberalization, an extended program of privatizations, and deregulation of economic activities. The educational system, which was considered to be in crisis, did not escape the wave of structural reforms. This system experienced substantial changes that were fundamentally imposed by two different laws.

On the one hand, in 1991 Congress commanded the transfer of the administration of all federal secondary schools to the provincial governments (*Ley de Descentralización Educativa*, Law 24049). This transfer progressively took place between 1992 and 1994.

On the other hand, the enactment of the *Ley Federal de Educación* (LFE) on April 14th, 1993 (Law 24195) introduced a second set of reforms, among which a significant change in the structure of the educational curricula and the extension of mandatory education stand out. While in the old system a child was obliged to attend seven years of primary school, under the new legislation that compulsory educational level was extended to nine years.

In fact, the LFE implied the reorganization of the levels in which the educational system in Argentina is divided. The main changes were:

- Pre-primary education for children aged five became compulsory.
- The primary level, which comprised seven years in the previous law, was replaced by a nine-year level named *Educación General Básica* (*EGB*).
- The five years of high school education were replaced by a three-year level called *Polimodal*.

Table 2.1 shows the structure of the educational system before and after the reform. The first column reports the age in which the child/youth is supposed to be attending each level.

One of the main goals of the LFE was reducing the high dropout rate in the initial years of secondary school, especially by poor students (Braslavsky, 1999).[1] Under the new structure youths were "forced" to stay two years more in school. Advocators of the LFE argued that this extension may also induce many of them to complete the, now shorter, high school level, and hopefully to get into the tertiary level. Other authors are more skeptical. Rivas (2003), among others, suggests that the increase in the enrollment rate during mandatory education may be compensated later with a higher dropout rate in the non compulsory stage.

Considering that the implementation of the reform was expected to induce an increasing pressure over the educational facilities, a budget of around US$ 3,000 million was allocated for an extensive program of investment in both educational infrastructure and training.

[1] The year Congress passed the law, the net enrolment rate in secondary school was around 65% for all (urban) Argentina, while it was below 50% in the bottom quintile of the income distribution (CEDLAS, 2007).

In spite of failing to fully accomplish the goals in terms of enrolment rates and school construction, the law implied important achievements.[2] In particular, with respect to the additional mandatory school years, previous work and our exploration of microdata which we will show next, illustrate that net enrollment rates significantly increased after the reform.

An important point for our analysis is that the new legislation was implemented with a substantial variation in terms of timing and intensity across provinces. Argentina is a federal country where primary and secondary public education are administered and financed at the provincial level. Although the LFE was a federal law to be complied with in all provinces, there was flexibility for state governments to decide on the timing of the reform. In fact, provinces were allowed to phase the implementation of the reform along the period 1995-1999.

While in some states the reforms were quickly and massively implemented, in others the changes were put into effect more gradually and involving a much smaller percentage of schools.[3] Moreover, in some districts some central aspects of the reform were never implemented (city of Buenos Aires, and the province of Río Negro). Table 2.2 reports for each province the year of implementation of the LFE and the modality (full, gradual, or null). By year 2000 the majority of the Argentina's provinces were complying with the new legislation.

Although the LFE implied a massive and costly intervention in the educational system, there was not an explicit comprehensive strategy to evaluate its results. The main contribution was the implementation of a yearly evaluation of the students performance, (DINIECE, 2003; Holm-Nielsen and Hansen, 2003). Once a year starting in 1993, a sample of schools was chosen and standardized Math and Spanish tests were administered in both elementary and secondary schools.[4]

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[2] Some authors report increases of up to 20% in the "installed capacity" of the educational system (Gorostiaga et al., 2003; Llach et al., 1999).

[3] See Rivas (2003) and Crosta (2008).

[4] Such evaluations are known as *Operativos Nacionales de Evaluación* (ONE). In 2000, tests were administered to all students in 6th year of EGB, and the last year of Polimodal.

3 Literature Review

The first strand of studies concerned with the quality of education focused on its determinants (Coleman *et al*., 1966; Jencks *et al*., 1972). Initial findings of the literature were somewhat discouraging, with components such as family background and the student environment deemed much more relevant. Such research was based, nonetheless, on a rudimentary methodological framework, which did not account for endogeneity or selection bias.

In the past few years, the literature has turned to focus on estimating effects of educational interventions on student performance. Such contributions have exploited exogenous components of these programs to avoid issues of endogeneity. Webbink (2005) is an excellent reference for such methodologies, reviewing a number of interventions which take into account such factors as class size, teacher training, hours of school, and expenses per student, among others.

In another recent study, Tiongson (2005) documents experiences of educational reforms with some recent empirical findings. Additionally, he realizes a typology of reforms according to their characteristics.

For the particular case of Argentina, some research in educational achievement has been completed using data from the Operativos Nacionales de Evaluación (ONE). Llach and Schumacher (2004) looked at academic results for students in primary education, with their main results pointing at the importance of socioeconomic status and school characteristics in scholarly performance. Taking a different approach, Cervini (2003, 2005) evaluated the differences between attending a public or private school on achievement and on non-cognitive outcomes (for example, attitudes and perceptions of their courses) for high school seniors. Using data from the Program for International Student Assessment (PISA) for 2000, Santos (2007) studied the determinants and distribution of schooling outcomes, finding that materials and school supplies, as well as human resources, are relevant when explaining student performance.

In Argentina, impacts of the reforms have been quantified for the *Ley de Descentralizacion Educativa*. Eskeland and Filmer (2002), using cross sectional data, found that institutional autonomy and parental participation increase performance in

primary school. Habibi *et al.* (2001) find a positive effect of decentralization on provincial net enrollment rates. However, none of these studies take into account possible endogeneity and spurious correlations which may arise. Galiani, Gertler and Schargrodsky (2002) analyze the effect of decentralization using the exogenous variation across provinces imposed by the program. The authors also find positive (negative) effects of the decentralization of educational services on educational outcomes for richer (poorer) provinces.

According to our knowledge, there are only two studies which have focused on the *Ley Federal de Educación*. Berlinski, Galiani and Gertler (2006) find a positive impact of the extension of pre-primary education on primary school achievement by exploiting differences in construction of classrooms for pre-primary schools across provinces. Crosta (2007) carries out a preliminary exploration of the effects of the LFE on access to schools using a basic regression framework.

The seminal contribution for the assessment of public policies in developing countries with fixed effects is Rosenzweig and Wolpin (1988). Among several evaluations that employ fixed-effects identification strategies, our study is particularly linked to the line of research followed by Duflo (2001) and Chou, Liu, Grossman and Joyce (2007), to mention but a few. The former analyzes the impact of an extended school construction program on both schooling and the labor market in Indonesia, using the interaction between cohort indicators and program intensity as an instrument for schooling. The latter paper investigates child health consequences of extending the years of compulsory schooling from six years to nine years in Taiwan, also taking advantage of the differences in the construction rates of new schools across regions.

4 Methodology

The implementation of the LFE was not accompanied by any strategy to evaluate its impact. This forces us to rely on observational data to derive our results. Our analysis seeks to identify the effect of the implementation of the LFE on several educational and

labor outcomes by exploiting the variation in both the timing –early vs. late- and the intensity –full vs. gradual- of the reform across Argentine provinces.

Figures 4.1 to 4.3 help to motivate this strategy. Figure 4.1 shows that while enrollment for children aged 6 to 12 remained almost universal during the period under analysis, enrollment rates for youths aged 13 to 15 substantially increased after provinces started implementing the reform in 1996. Figures 4.2 and 4.3 show enrollment rates for ages 13-15 according to the timing (early vs. late) and degree (massive vs. gradual) of the implementation of the educational reform. Enrollment rates seem to have strongly increased for those youngsters living in areas where the LFE was quickly and fully implemented.

One of the basic points of the paper is to evaluate whether youngsters who were affected by the LFE[5] performed better in certain dimensions (*e.g.* the labor market) than their peers who were not "treated", either because they were born in provinces that did not implement the reform quickly, or because they were not affected by the LFE as they were just leaving primary school when the law was passed.

We use a difference-in-difference approach for most of our estimations. Specifically, we use fixed-effects methods to control for unobserved heterogeneity across both cohorts and urban areas. Essentially, fixed-effects identification strategy uses repeated observations of the unit of analysis to control for unchanged unobservable characteristics (in this case by cohorts or urban areas) that can be correlated with both causal variables and outcomes of interest (Angrist and Krueger, 1998).

Our strategy is similar to that of Duflo (2001), who analyzes the impact of an extended school construction program, using the interaction between cohort indicators and program intensity as an instrument for schooling. Formally, the basic model is:

$$Y_{ijk} = C + \alpha_j + \beta_k + \theta X_i + (P_j * T_i).\gamma + e_{ijk} \qquad (1)$$

where

Y_{ijk} = outcome of interest of individual *i*, living in city *j*, belonging to cohort *k*.

α_j = city fixed effect

[5] As mentioned above, the LFE implied a two year extension of compulsory number of yeas and a change of curricula. Also,

β_k = cohort fixed effect

X_i = individual characteristics (dummies for head of household, spouse, child of the head of household, number of employed individuals in the household, number of household members, maximum years of education in the household)

T_i = treatment variable, equal to 1 if youngsters are treated according to both city and cohort, and 0 otherwise.

P_j = measure of the program intensity in the city

e_{ijk} = error term.

A key issue in this strategy is to identify which is the relevant geopolitical unit in order to determine the level of treatment. After the enactment of the LFE, the Argentina's provinces chose whether to adhere or not to this national law and, in case they decided to implement the Reform, they had to select a starting date. The 23 Argentinean provinces and the city of Buenos Aires (Federal District) are listed in table 2.2, along with the year of implementation of the law.

However, the practical execution of the LFE was not necessarily immediate. In fact, provinces decided whether to make a generalized implementation of the LFE since the beginning, or to follow a gradual strategy (see column (iii) in table 2.2). Although within a certain province, all the cities should have applied the set of reforms according to the provincial government's decision; there is some evidence on heterogeneities between cities in terms of the percentage of educational establishments which implemented the Reform. We consider including these differences as a measure of treatment exposure.

In terms of choosing the cohorts that were exposed to the extension in compulsory education under the new curricula, we compare different age cohorts. We provide a thorough description of such cohorts in section 5. Besides, as we will observe later, there is substantial variability in terms of treatment intensity among the young cohorts. This variability is driven by two main sources. In the first place, differences in the timing of the reform involve that a given cohort could have been exposed to a variable extension in mandatory education according to their city of residence.

Second, differences in school construction rates and/or percentage of schools that implemented the reform are another source of variability in the program exposure.

Children living in two provinces that implemented the reform in the same year (Buenos Aires and Córdoba) could be exposed to considerably different treatment intensities in accordance with the investment program carried out to support these reforms.

Outcome variables

We are interested in measuring the impact of the LFE on human capital accumulation and labor market performance. Clearly, these aspects are strongly linked since more educated youngsters are expected to perform better in the labor market.

Regarding human capital accumulation, we study whether the law was effective in retaining youths in the educational system beyond the compulsory level. As outcome indicators we consider years of formal education and secondary school graduation. With respect to the labor market performance of students, the main outcomes considered are earnings, hours worked, and employment.

We are also interested in analyzing the impact of the LFE on educational quality measured by some standardized tests administered in Argentina.

Data

Our research relies on various data sources. Our primary source of information in order to assess labor market outcomes is the *Encuesta Permanente de Hogares* (EPH) from 2003 to 2006, the main household survey in Argentina. The EPH covers 32 urban areas, with at least one observation in the 24 Argentina's provinces listed in table 2.2. Although the EPH covers only urban population, and hence it is not nationally representative, the share of rural population in Argentina is, unlike most developing countries, very small (13%). The EPH gathers information on individual's socio-demographic characteristics, employment status, hours of work, wages, incomes, type of job, and education. The EPH includes information on about 100,000 individuals.

Though the units of observation in our research are the individuals, the sources of variability in exposition to treatment are both their birthplaces, or place of residence, and cohorts. Data regarding exposition to treatment (percentage of schools that implemented the reform or school construction to allow the extension in compulsory education) are obtained from the administrative records of the Argentine National Education Ministry.

In the section regarding educational quality, the administrative records of the Argentine National Education Ministry are our principal source of data. Among other variables, these records allow computation of the average test score by school, which is our unit of observation. Our dataset is composed of around 5,000 schools in the period 1997-2000.

While in the case of education and labor market outcomes, our unit of analysis is the individual, in the case of the quality effect, we work with data at the school level. Such specifications result from the availability of data. Whereas in the former case we can build a panel at the city-cohort level, there is not such information available for the quality indicators.

Exogeneity

One of the major methodological concerns about the approaches that exploit the regional variability in the timing or intensity of a policy intervention is that the choice of the local governments as to when and how to implement the reform may be correlated with unobservable factors which also affect outcomes. In our case, for instance, one may conjecture that poorer provinces with lower enrollment rates could have been more eager to put into effect the changes, since they will be granted resources from the central government.

In order to better understand the timing of the implementation of the LFE, we estimate a hazard model (Jenkins, 1995) of the probability of implementing the reform. We are interested in examining whether there are factors which could be affecting labor market/educational outcomes and the probability of implementing the reform. In table 4.1 we present our estimates of the hazard model. We model the probability that a province implements the reform at a given period of time as a function of time varying provincial variables. There are several specifications for the dependent variable and for the time variables.[6] Among the explanatory variables we consider proxies for regional GDP (*gdp*), Gini coefficient (*Gini*), unemployment (*Desempleo*), population (*Poblac*), fiscal deficit (*Resultado fiscal*), political party (*polity*), which takes the value of 1 if the province is

[6] We considered "implementation" to several different thresholds: 33% percent of implementation and 90% of implementation of EGB and Polimodal.

governed by the same party than the national government at the time of reform, and the percentage of individual with unmet basic needs (*NBI*).

The only variable that is significant in most of the specifications is the political party, which means that the provinces were more likely to implement the LFE if its ruling party was the same than the national one. Given this, we control for this variable in our estimations. The rest of the variables, which are correlated with economic shocks and could be also correlated with our outcome variables of interest are uncorrelated with the probability of reform. If the reform is uncorrelated with observed time varying factors, it is less likely that it is correlated with unobserved time varying factors which could be affecting also our outcomes of interests.

As mentioned above, our identification assumption is that in the absence of the reform, educational and labor outcome measures would not systematically differ across provinces which implemented and the ones that not. If this is the case, then the difference in outcomes observed between exposed and non exposed cohorts/regions are attributable to the educational reform.

Table 4.2[7] performs some checks in order to support our identification strategy. Based on table 2.2, individuals' ages and region of residence we can split our sample according to exposition to reform. In Panel I we examine the simple difference in years of education between provinces which implemented massively vs. the ones who did not. While young cohorts are the ones exposed to the new law, old cohorts are not since they were born before they could be affected by the educational reform. The double difference between these two groups amounts to 0.91 years of education and it is statistically significant. Panel II and III show differences by the degree of implementation by age cohorts who should not have been affected by reform. In both panels both groups (young and old) are comprised by people not exposed to the reform. The double difference in both panels is not statistically significant. This gives us some support to our claim that our results are driven by the studied reform and not by other factors.

[7] This analysis follows closely the one in Duflo (2001).

5 Education and labor market outcomes

We carry out the estimations using several samples and different cohorts' definitions. The samples we consider are the following: all individuals, males, poor, and poor males.[8] We conduct our estimations for three different definitions of cohorts in order to give more robustness to our estimates. The main rational for the estimation using different cohorts is that you cannot observe exactly the date of birth of each individual in our databases. Argentine law stipulates individuals must enter primary schools with the age of six at June 30th each year. We cannot observe if, for example, an 8 year old person is in second or third grade, so different cohorts are constructed. As left-hand-side variables, we consider a set of education variables - years of education, dummy for incomplete high school, and a dummy for complete high school – and a set of labor variables - labor income, weekly hours worked, and a dummy for employed.

Education outcomes

Tables 5.2 and 5.3 show the results for the education variables from two models; in the first one the reform is introduced through a single binary variable (=1 for those individuals fully exposed to the reform), while in the second one that dummy is interacted with the intensity of the reform, measured by the share of school that had adopted the reform by 1998 in the area where the individual lives. Results vary more across samples than across definitions of cohort. Results do not qualitatively change if we consider other measures for reform intensity. Besides the typical set of controls, we also include political party in the regressions, given its significance in the hazard models of table 4.1.

The LFE seems to have had a significant effect on some basic school enrollment outcomes. The coefficients for years of education are positive and significant in all the samples and for all the definitions of cohorts. Youths fully exposed to the LFE ended up with more years of education than those not fully exposed to the reform. Coefficients range from 0.3 to 0.8 extra years of education as a result of the reform. Moreover, most

[8] We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution.

coefficients are also positive and significant in the case of the binary variable for complete high school. In particular they are positive for poor people, implying at least a partial success of the reform: poor youngsters exposed to the reform ended up with better educational outcomes than those not fully exposed to the reform. The impact, however, seems small: education for those poor youths fully exposed to the LFE increased in about just half a year.

The increase is somewhat larger for the sample of all people. One possibility beyond this result is that the reform convinced some poor teenagers to finish high school, but few of them to go beyond that. Instead, the impact could have been more intense on non-poor youths, who probably live in an environment more prone to education, and have higher opportunities to continue studying.

The impact of the reform on educational outcomes seems to have been higher for males than for females. This is consistent with the fact that in Argentina, as in most of Latin American countries, high-school drop-out rates are higher for men than for women. CEDLAS (2009) reports that in 2006 while 84% of females in secondary school age are attending that educational level, the share for males is 78%.

Figure 5.1 shows coefficients for the interaction of age in year 1996 and treatment when taking years of education as the outcome variable. As expected, coefficients are positive and significant for children aged 8 to 12 years (*i.e.* those fully exposed to the LFE), and become non-significant after the age of 12 (*i.e.* for those youths that were finishing or had finished primary school at the time the reform was implemented).

Labor outcomes

The impact of the education reform on labor outcomes can be studied with the help of tables 5.4 and 5.5. When introducing the reform as a binary variable the results in terms of labor market outcomes are mostly positive and statistically significant.[9]. Youths fully exposed to the reform when they were teenagers have now higher probability of being employed, work more hours and earn higher incomes. Reform increased the probability of employment for all individuals and males. Probability of employment in

[9] Here we must stress that this sample consists of wage earners and self employed and not the whole population affected by the reform.

those groups increases between 4.9 and 7.4%. The effect for poor individuals and poor males is also positive, but not statistically significant ant the conventional levels.

Labor incomes for treated youths are around 10% higher than for their non-treated counterparts. Results are similar for the sample of males, but almost completely vanish in the sample of poor youths. The reform seems to have had no effect on the labor outcomes of income-deprived people. Most results are also non-significant when considering the intensity of treatment (table 5.5). Positive and significant coefficients remain only for the sample of all youths and definition of cohort C. Again, all results for the poor are non-significant. The same pattern observed for employment is observed for hours worked. While the entire sample and all males increased their hours worked per week between 2.28 and 3.53, the effect for poor individuals is negligible and not statistically significant. For an average working week of 35 hours, the increase in hours amounts from 8 to 10%. Finally, results are also positive for labor income, with increases amounting to 20% for some sub-groups.

In summary, the reform seems to have had an overall positive impact on education and labor outcomes. On average, youths fully exposed to the LFE have more years of education, were more likely to have completed secondary school, have higher probability of finding a job, work more hours and earn higher salaries. However, the effects are in general rather small and some of them do not hold in all specifications. In addition, and more important, the impact of the reform on the income-deprived youths is small for education outcomes and null for labor outcomes. Relatively few poor teenagers exposed to the reform managed to increase high-school education, with apparently no impact on their labor outcomes.

One possible explanation for the differences across groups runs as follows. Poor people have very limited access to jobs with high returns to education. Most of them are construction workers, domestic servants, or are self-employed in the commerce sector. The environment where they grow (low social capital, scarce contacts) implies a substantial constraint to the access to jobs where education makes a big difference. As the reform implied only small gains in years of education, the impact on the performance in the labor market for these people was understandably low. In contrast, the educational

gains were larger for the non-poor, and arguably more productive in the labor market, given the types of jobs that these people are more likely to hold (*e.g.* civil servants).

6 Educational quality

In this section we report the results of our analysis of the impact of the LFE on education quality measured by standardized tests. We use the Spanish and Math scores results from the annual quality tests collected by the National Ministry of Education. Such tests are administered to students undergoing the last year of *Polimodal*. We have score tests at the school level. Our database contains information for 5745 schools during the 1997-2000 period.

As stated in Galiani *et al*. (2005), such test do not capture all the dimensions of student performance, but, on the other hand, they are standardized and uniform across provinces, which makes them comparable. Moreover, there is not a stick and carrot policies either to students or teachers, so there is little scope for manipulation of the results.

We consider as "treated" those schools that have students in the last year under the new structure of *Polimodal,* regardless of whether the implementation has been piecemeal or cold turkey.[10] Thus, we will not consider among treated units schools which have already been operating under the new system, but which do not have students in the last year of *Polimodal* yet. Notice that this could harm the validity of a fixed effect assumption, given that the implementation of the *Polimodal* can change the administrative management of the unit affecting student performance, even if the students in the last year are not yet under the new regime. In order to deal with this situation, we use fixed effects at the school level and control for changes in schools resulting from implementing the law.

Table 6.1 present basic statistics for control and treatment groups, both before and after treatment. For both periods, the score for Math and Spanish are higher for the treatment group. The table also exhibits unconditional double differences of program

[10] In 1999 and 2000, there are 631 and 638 schools with students in the last year of Polimodal respectively.

effect. There are both positive results for Math and Spanish scores, but the effect on Spanish scores is higher.

In order to test the effects of the educational reform on the quality of education, and since we cannot observe the counterfactual, we rely on non-experimental methods which simulate a valid counterfactual under a set of assumptions. Our control group is formed by schools which either have not implemented or completed the reform. In the latter group we consider students from the last year of reformed schools who are being educated according to the old regime.

There are some concerns of following this approach. The main problem is that schools in the control and treatment groups are different due to factors which are correlated with test scores, not allowing us to identify the causal effect of reform on test scores. If unobserved heterogeneity across schools is fixed across time, it can be controlled by using differences in differences methods. In this sense, we face the same problem than in the previous section on labor market and educational outcomes, and all previous analysis remains valid.

Formally, the model to be estimated is:

$$y_{ijt} = \alpha + \beta dI_{ijt} + \delta x_{jt} + k_{ijt} + \lambda_t + \mu_i + \xi_{ijt} \qquad (2)$$

where

y_{ijt} = mean test score of school i, in province j at year t

dI_{ijt} = binary variable which equals one if the school i at t has students in the last year of *Polimodal* (the new system) and zero, otherwise

x_{jt} = covariates (per capita regional GPD, unemployment rate and fiscal variables)

k_{ijt} =indicator variable in order to control for changes at the school level resulting from implementing *Polimodal* or not.

λ_t = fixed effect common to all schools at time t

μ_i = school fixed effect. We will also estimate the model using school county fixed effects.

ξ_{ijt} =independent mean zero error

The error term may be correlated over time and across provinces, which underestimates standard errors (Bertrand, 2004). In order to overcome this problem, we clustered the results at the province level.

One possible threat to the validity of our identification strategy is that test contents are biased to the new contents of the curricula after the reform, which would, in principle favor the students of the provinces that implemented the reform. However, the tests are designed taking into account a basic curricula common to both regimes, so the risks that tests are biased are very small.[11] Another concern is the possible composition effect caused by the extension of the compulsory years of education. If, as a result of such extension a higher proportion of students were able to finish high school, then this could result in lower average test at the school level. However, given the timing in the implementation of the reform, this effect would potentially appear only after 2000.[12]

Results

This section presents the results obtained from the estimations under different specifications. Table 6.2 presents the definition of the variables and the data sources while table 6.3 presents the results of the estimations. Model (1) includes only school and year fixed effects and controls at the school level to account for the degree of implementation of the *Polimodal (*variable *Implementación_ciclos)*. The effect is positive and significant for Spanish scores. Model (2) adds county covariates which change over time: geographic per capita GDP (*PBG_pc*), unemployment rate (*Desempleo*), fiscal variables (*Resultado_Fiscal*), and poverty (*Poverty*). Here, we also find a positive and significant effect of the reform on Spanish scores. Model (3) has the same controls than (2), but school fixed effects are replaced by county fixed effects. We also find a positive effect on Spanish scores in this model.

[11] DINIECE (1997, 2003b).

[12] In particular, the first provinces in implementing the EGB, and thus, extending two years of compulsory education were Buenos Aires and Cordoba in 1996. Buenos Aires implemented the new system gradually, so our first affected cohort reached *Polimodal* in 2001. In the province of Cordoba students would have finished *Polimodal* at best in 2000. However, given the fact that highest dropout levels happen in the transition between EGB and Polimodal it is very unlikely that this only year/province cause a composition effect in our estimations. The rest of the provinces implemented the reform either lately or gradually.

As far as the Math scores are concerned, evidence is mixed. Models (1) and (3) suggest a positive effect, which is only significant for (3). The estimation with Model (2) has a negative, but not significant effect of the reform.

Time varying factors

One of the main threats to the validity of our identification strategy is the potential existence of unobserved factors which vary over time and that are correlated with the treatment and outcome variables. There are two main risks that this may be happening. One is the endogeneity of educational reform.[13] In this particular case, there are reasons to believe that such bias is not a problem, even though we cannot test this directly. First of all, the educational reform was a national policy and most of the provinces adhered to the changes with little difference in time. Second, we cannot reject the null hypothesis of independence of Spearman rank correlation between provincial test scores and timing of reform. Third, we estimate a hazard model for the probability of the reform – see section 4- and find that the decision to implement the LFE is not correlated with observed time varying covariates. Finally, 97% of the treated schools in our dataset belong to provinces which implemented the reform massively. The reform was decided at the provincial level and not at the school level, and hence not conditioned to any pre-existing school characteristic.

Our second threat is that omitted time varying factors may be affecting treated and control units differently. However, both control and treated schools within the same jurisdiction are governed by the same administrative authority (the Ministry of Education at the provincial level), and this authority did not make any difference across schools. Yet, for most of the cases, treated and non-treated schools are located in different provinces which could be affected by different shocks. Such problem is reduced by the fact that the control group is large, which makes it less sensible to changes in local conditions. Regrettably, we cannot fully assess whether our estimates are biased because of the second reason above, so we use two different strategies to address this problem. First, we clustered standard errors at the provincial and year level (table 6.4). Results do

[13] This will happen if the government decision as to when is the best time for the implementation of reform is based on the results of the standardized test or other local factors that are related to such results.

not significantly changed compared to table 6.3. We also cluster the standard errors at the county year level, since the number of groups using provinces may be small, but we obtain similar results. Secondly, we estimate the model using province fixed effects. Again, the coefficients are slightly larger than the previous models, but results remain unchanged.

Heterogeneous program impact

One interesting aspect that can be assessed is whether the reform had any heterogeneous impact according to poverty[14] levels. Also, there can be heterogeneous treatment according to the different school characteristics, rural vs. urban and/or private vs. public, for example.

Table 6.5 presents mean variables for control and treatment group respectively. The proportion of school located in high poverty areas and the number of public school is lower for the treatment than for control groups. Also, the size of the schools in the treatment group is smaller. The ratio of urban/rural school is similar in both groups.

Table 6.6 shows estimates that take into account if the school is located in a poor county or not. Model (1) includes a dummy interacting treatment and county poverty, (*d_tratxpobre*). As expected, treatment in poor areas reduces average performance in Spanish (0.75 points) respect to non-treated schools. Again, the effect on Math scores is not statistically significant.[15] Model (2) takes into account a variable interacting treatment with the percentage of population with unmet basic needs[16] (*d_tratxNBI*). Scores in counties where unmet basic needs is higher than the median are lower, and vice-versa. Finally, model (3) includes a variable interacting treatment with school location (urban

[14] In order to measure poverty at the county level, we use percentage of population with Unmet Basic Needs (NBI) in the 2001 national census. The definition of a household with NBI in Argentina is whether a household presents at least one of the following characteristics: (i) more than 4 persons per room (ii) the household lives in "poor" places (e.g. street, shanty towns) (iii) the dwelling is made of low-quality materials (see section 7) (iv) the dwelling does not have access to water (v) the dwelling does not have an hygienic restroom (vi) there are children aged 7 to 11 not attending school (vii) the household head does not have a primary school degree (viii) the household head does not have a high-school degree, and there are more than 4 household members for each income earner (INDEC, 2003). We use a dummy which takes the value of one if the county has 20% or more of the population with NBI.

[15] This result is consistent with findings in Galiani, Gertler y Schargrodsky (2005).

[16] The sample median of NBI is 13.4%, according to the Census of 2001.

vs. rural, *d_tratxrural*). On average, there is an adverse effect of treatment for rural schools.

Table 6.7 shows treatment interacted with school characteristics. Model (1) includes a dummy for public schools interacted with treatment (*d_tratxpublica*). Treatment for public schools is negative for Math (-1.46 points). For Spanish, the effect is the opposite. However, the effect is considerable lower when compared to that of private schools.

Model (2) includes a variable for relative size of schools (*cant_rel_alum*) and its square (*cant_rel_alum2*) to test possible non linearities. Such variable is defined as the difference between the number of students in the last year of *Polimodal* in that school and the sample median. Treatment has a non linear effect on the score average both for Spanish and Math. Relatively small and big schools have negative effects compared to that of the control group. There is positive scale effect for Spanish, which is negative for Math, as we can observe in figure 6.1.

Since the reform implied significant changes in the organization at the school level, it can be conjectured that school with fewer resources and lower managerial capabilities find it more difficult to adapt to the new educational structure. In order to test this issue, we build different indexes of material resources using principal component analysis with information available in the Ministry of Education.[17]

Table 6.8 presents the results using four different indexes of school resources. Column I to IV show a positive effect for different specifications of indexes related to school physical resources (books, blackboards, computers, etc.). It can be seen that performance is higher in schools with better physical resources. Columns V to VIII have indexes indicating resources related to teachers training.

Robustness analysis

As mentioned above, our counterfactual will not be valid if treatment and control schools are affected by different time trends. Given the timing of the implementation of the

[17] In 2000, 19.3% of all schools are scheduled or have already received help from the National Government.

reform,[18] it is very difficult to draw controls from the same counties where we have treated schools, so we redo our estimations looking for control schools with similar characteristics than the treated ones. In table 6.9 we restrict our sample to schools with similar number of students for the year of reference. Results are similar to the ones previously obtained.

We also performed our estimations considering different control groups. As stated in Lee (2005), it is possible to control for unobservable factors confounding our identification by conditioning our analyses to different control groups. If conditional expectation of the outcome variable is different according to the control group chosen, then it is possible to conjecture that our treatment group is also different in terms of unobservables.[19] We considered two alternative control groups: one with the schools where reform was not implemented and a second one with schools where reform has been implemented but there are not yet students in the last year of *Polimodal*. Our results for Spanish scores are invariant to the specification of the control group. Math scores results depend on the specification of the control group.

7 Regression Discontinuity Design

The regional heterogeneity in the implementation of the reform also allows us to exploit another popular identification strategy: the regression discontinuity design. This methodology, introduced during the 1960s by Thistlewaite and Campbell, was not spread in the economic literature until recently, in the late 1990s.

The main idea is simple. In certain contexts the assignment to a treatment is a discontinue function of an observable covariate. The treatment depends on whether the value of this covariate is above or below a certain threshold, and people cannot self select into treatment by altering their behavior to achieve such threshold. Those observations lying close enough to the cutoff point are expected to be quite similar in terms of the relevant attributes, allowing drawing causal inference by comparing them. Frequently,

[18] In general, within a jurisdiction, all schools which implemented the reform did it at the same time. Thus, they are always either inside the control group or not.

[19] This strategy is similar to the one used by Card and Krueger (1994).

these thresholds for treatment arise with the eligibility conditions imposed by governments dealing with scarce resources and the search of transparence in the assignment of social programs. We find many examples of recent studies taking advantage of these kinds of designs. Among them, Lalive (2007) studies the effect of the duration of unemployment benefits in Austria. This study exploits a geographical threshold: there was sharp discontinuity in the assignment of benefits at the border of eligible and control regions. As pointed by the author, contrasting individuals living close to both sides of the border is appealing since the labor market conditions are remarkably similar in tightly defined geographical areas.

In our case, the discontinuity also refers to the geographical dimension. As mentioned above, while the province of Buenos Aires quickly introduced the set of reforms involved in the LFE, the city of Buenos Aires (CABA), which is a federal district, never implemented it. This city is separated from the province of Buenos Aires by an avenue, called General Paz. Although these geographical areas are substantially different in terms of standard of living, poverty and infrastructure, as we get close to the border of these regions (i.e. to the General Paz Avenue), these differences tend to disappear. The neighborhoods, populations, and schools become more similar. In each side of the border, the attributes of those households located a few blocks away from it should be, on average, approximately the same.

One key factor in order to apply RDD techniques is that individuals cannot self select into treatment. One of the main threats to our identification strategy would be the fact that people might choose schools in either district (regardless of the place of residence) if they perceive that education is better in one district. We believe this possibility is not very important as people have priority to send their children to public schools in the jurisdiction they live.

Buenos Aires metropolitan area is the most vastly populated area in Argentina. In terms of political jurisdiction such area is formed by Buenos Aires city (henceforth CABA) and its suburbs (Greater Buenos Aires, henceforth GBA). Even though the city is autonomous politically, GBA belongs to the government of the province of Buenos Aires. While GBA implemented the educational reform cold turkey in 1996, the city never implemented it. The city and its suburbs are separated by the Avenida General Paz,

individuals living close to the border are very similar in most characteristics (see Cruces *et al*., 2008), except for the treatment (being in one reformed area vs. non reformed). We can locate schools within some specific distance from the General Paz (see the map below), considering several cut offs to check for robustness, and use a regression discontinuity design to assess the change in quality measured by test scores.

We have such information for students who went to 6th grade at primary school and 5th year of high school under both systems for the year 2000, with more than 30,000 observations scattered equally within the city and outside the city limits. We estimate the effect of such policy change. This approach is very interesting in the sense that for the group under analysis, quality changes can be studied overcoming some of the issues of endogeneity of reform.

We performed several estimations in order to assess the impact of the reform in both primary schools and high schools. The data we use corresponds again to the ONE 2000. As explained above, from the point of view of the students, we can consider that students going to school "near" General Paz on greater Buenos Aires and in the city of Buenos Aires belong, on average, to similar groups. Moreover, we can consider the treatment to be exogenous, *i.e*: it is highly improbable that students behave strategically and selects into better schools anticipating the success/failure of the reform. However, given to the fact that selection is not completely random, we will control our estimation using household specific and school specific covariates. We performed different cutoff points for the distance from the General Paz: five, ten and twenty blocks. We present results corresponding to twenty blocks, but we found similar results for five and ten

blocks. As it can be observed in the tables below, simple mean of test scores show a significant differences in both Math and Spanish scores. For all the cases considered, scores are lower in GBA, the area where reform was implemented. As it can be observed in table 7.1, students in primary and secondary schools scored higher in both Math and Spanish scores is the schools that were not under the reform, the difference being statistically significant.

We then proceeded to the estimation, starting with OLS estimates, clustering standard errors at the school level. In table 7.2, we show the results for several specifications of the estimated model for students in primary schools. The first and second columns show only a dummy for treatment, and a dummy for treatment and for public school. We then added student household characteristics in column 3 and finally, some school characteristics. For all the specifications, "reform" has a negative and statistically significant sign, both for Spanish and Math scores. While the effect is lower as we add covariates, it is still statistically significant.

All the covariates have the expected sign. The dummy for public schools is negative, and so are male and the fact that the student has repeated the grade in the past. School size has no effect on scores. Past performance in school exams show a positive correlation with scores. Finally, household size and whether the student helps the family by working at home also affect scores negatively.

Table 7.3 shows the results for high schools. For high schools, we found the reform has a negative effect for Spanish scores, but the effect on Math scores vanishes after we control for covariates. Again, all the rest of the coefficients have similar sign to that of the students in primary school.

Finally, we performed some placebo experiment in order to test our results. We randomly allocated school within two kilometers within the city limits and found the coefficients associated with the treatment are not statistically significant.

We then estimated the effect of reform using local linear regression. We want to estimate the value of the regression at a specific cut-off point, which is the boundary point. Following Fan and Gibbels (1996) we estimate a local linear regression. We try to fit linear regression functions to the observations at a distance h or smaller at each side of the point of discontinuity:

$$\min_{\alpha_l:\beta_l} \sum_{i:c-h<X_i<c} (Y_i - \alpha_l - \beta_l(X_i - c))^2,$$

and

$$\min_{\alpha_r:\beta_r} \sum_{i:c<X_i<c+h} (Y_i - \alpha_r - \beta_r(X_i - c))^2,$$

where c is the cutoff value, h is the bandwidth and $\hat{\tau} = \hat{\alpha}_r - \hat{\alpha}_l$ is the local average treatment effect. Several kernels[20] and bandwidths were specified. Figures 1 to 4 show results for primary and secondary schools and for each type of test respectively. The distance from the cutoff value is measured in meters from the avenue dividing GBA and the city of Buenos Aires[21]

When considering all primary schools, (Table 7.6) reform had a negative and significant effect of around 9 points in math scores. This represents a huge drop in math mean scores, given that the mean of non-treated is 67. For Spanish, the effect is also significant and negative, though smaller. However, it is far from negligible, averaging 4 points (for a mean in the non-treated group of 71).

When we estimate the effects for high schools, we found no significant effect for Math scores (see Figure 7.2) and a 5.95 drop in Spanish scores.

There are several issues to be taken into account in order to check the validity of our estimations. First of all, we do not have census information previous to the reform; neither can we identify the location of schools. However, there are several indicators we can look at in order to check the two groups were similar before the reform. As mentioned above, standardized tests scores are administered by the Ministry of Education since 1993, though they are nationally, but not provincially representative, nor do they have census coverage. In such databases, the exact location of schools cannot be identified. However, we can observe sample averages for schools in CABA and in GBA. Though these areas are bigger geographically than the ones we used in the regression discontinuity estimation, it gives some idea that scores were relatively similar before the

20 Cheng et al. 1997 suggest the triangular kernel for regression discontinuity design cases due to being boundary optimal. As a consequence, we present results a using triangular kernel.

21 We present results only for schools at +/- 2000mts from the city limit. However, our results do not change if we further restrict the sample to +/- 1000mts.

reform. Table 7.7 shows average scores for primary school students for CABA and the province of Buenos Aires. As mentioned above, this sample is different to the one used in our estimations, but it can be observed that scores did not differ much in the case of Spanish scores, we do not reject the mean difference test. However, scores do differ for math, being 3.3 point higher in CABA. This difference is statistically significant. Unfortunately, these results are not directly comparable with our samples, neither are statistically significant at the province or city level, but provide some anecdotic evidence that gaps were not existent for Spanish and small in Math.

Another caveat is that our estimates only represent local average treatment effects, and so, though internally valid, they cannot be extrapolated to other regions of the country. In terms of comparing our results with the existing literature on educational reform in Argentina, Galiani et al. (2007) found that decentralization had positive effects on richer districts but negative in poorer ones. This may go in line with our results, since CABA is much smaller and richer than the whole province of Buenos Aires, and so we can consider the quality of administration of the province of somewhat lower quality than that of CABA, even though our samples are similar in most observed characteristics. The fact that we are comparing with no reform scenario raises the question of why the reform had such negative effects. One possible explanation for such decrease in quality is the change in curricula and such change was not adequate. Curricula were supposedly updated to keep up with technological change. The change in curricula was far bigger in high school than in primary ones. Another factor might be the fact that schools started to be administered by regional Ministry of Education and not nationally and thus, poorer administration had lower management and administration capabilities.

8 Conclusions

High dropout rates in developing countries have long motivated changes in educational systems in order to keep individuals in school. . In most developing countries, education still remains an important policy for leveling off different labor market opportunities. While evidence on the (sometimes causal) relationship of time spent at school and improvements in labor market is well established for developed countries, evidence for

developing countries is much scarcer. It is believed, however, that increasing the average years of education for individuals will enhance their labor market opportunities. While it is very difficult to legally enforce the number of compulsory years, it is believed that reforms raising the number of compulsory years and/or school leaving age are effective. Different channels may be behind this. Among them we can mention (Oreopoulos 2009) social norms and the stigma caused by not fulfilling what should be mandatory. For the case of Argentina, we believe that the most pressing concern is the difficulty of finding a job in the formal sector without the legal educational requirements.

Argentina, while still one of the countries with the greater number of years of education in Latin America, still presents a high dropout rate after elementary school, especially for poorer individuals. Henceforth, the reform under analysis here, among other changes, aimed at increasing the average number of compulsory years at school.

In spite of the heated debate about the advantages and disadvantages of the educational reform in Argentina, there has not been solid evidence of its causal effect on educational and labor market outcomes. This paper contributes to the measurement of the impact of the educational reform in different dimensions. Our analysis takes advantage of the variation both of the timing –early vs. late- and the intensity –full vs. gradual- of the reform across Argentine provinces.

Our results suggests positive effects of the reform in some educational outcomes (enrollment rates, Spanish test scores), and labor outcomes (employment, hours and labor income). However, the impact on educational outcomes are rather small and even non-significant in some cases (Math scores) while the impact on labor outcomes are limited to the non-poor. The LFE seems to have been only partially successful, as the effects were small and likely non-significant in the poor population.

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Tables and figures

Table 2.1
Educational structure before and after the reform

	Before the LFE			After the LFE		
Age	Levels	Year	Compulsory?	Levels	Year	Compulsory?
3	Pre-primary	1	No	Pre-primary	1	No
4	Pre-primary	2	No	Pre-primary	2	No
5	Pre-primary	3	No	Pre-primary	3	Yes
6	Primary	1	Yes	EGB	1	Yes
7	Primary	2	Yes	EGB	2	Yes
8	Primary	3	Yes	EGB	3	Yes
9	Primary	4	Yes	EGB	4	Yes
10	Primary	5	Yes	EGB	5	Yes
11	Primary	6	Yes	EGB	6	Yes
12	Primary	7	Yes	EGB	7	Yes
13	Secondary	1	No	EGB	8	Yes
14	Secondary	2	No	EGB	9	Yes
15	Secondary	3	No	Polimodal	1	No
16	Secondary	4	No	Polimodal	2	No
17	Secondary	5	No	Polimodal	3	No

Table 2.2
The process of LFE implementation

Province	Year	Degree
Buenos Aires	1996	F
Catamarca	1999	G
City of Buenos Aires	N.I	
Chaco	1997	G
Chubut	1999	G
Córdoba	1996	F
Corrientes	1997	F
Entre Ríos	1997	F
Formosa	1998	F
Jujuy	1998	G
La Pampa	1997	F
La Rioja	1999	G
Mendoza	2000	G
Misiones	1998	F
Neuquén	1998	G
Río Negro	N.I	
Salta	1998	G
San Juan	1997	F
San Luis	1998	F
Santa Cruz	1998	F
Santa Fé	1997	F
Santiago del Estero	1998	F
Tierra del Fuego	1998	G
Tucumán	1998	F

Source: Crosta (2007)
N.I: not implemented
F: full implementation since the beginning
G: gradual implementation

Table 4.1
Hazard model
Time of implementation

Dependent variable	33% polimodal implemented		90% polimodal implemented		33% EGB implemented		90% EGB implemented	
Variables	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**
pbg	-0.001	-0.001	-0.002	0.000	0.000	-0.000	-0.000	-0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]
gini	0.607	-15,299	4,650	-8,219	-28.235*	-52.549**	-10,541	-27,035
	[11.691]	[14.903]	[14.009]	[18.726]	[14.433]	[23.218]	[15.655]	[21.443]
desempleo	0.074	-0.031	0.137	0.243*	0.120	-0.170	-0.067	-0.216*
	[0.071]	[0.080]	[0.100]	[0.135]	[0.097]	[0.159]	[0.085]	[0.120]
polity	-0.088	0.341	1.705**	2.349**	2.096**	0.509	2.134**	3.019***
	[0.603]	[0.664]	[0.758]	[0.947]	[0.843]	[1.185]	[0.840]	[1.153]
poblac	0.005	0.019	0.005	-0.044	0.026	0.055	0.005	0.024
	[0.018]	[0.016]	[0.029]	[0.037]	[0.036]	[0.035]	[0.017]	[0.026]
Resultado Fiscal	-0.001	-0.001	-0.001	-0.005**	0.000	0.002	-0.002	-0.000
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.004]	[0.002]	[0.003]
lnt	1,224		2,690		1.401**		1.662*	
	[0.772]		[1.763]		[0.621]		[0.858]	
nbi	0.024	0.044	-0.083	-0.080	0.110**	0.125**	0.099*	0.128
	[0.049]	[0.056]	[0.054]	[0.058]	[0.044]	[0.061]	[0.060]	[0.083]
d2		19.936***		18.015**		17,484		18.067**
		[2.135]		[8.230]		[0.000]		[7.813]
d4		18,552		14,998		21.074***		17.409**
		[0.000]		[9.380]		[1.571]		[7.976]
d5		22.078***		19.406**		22.789***		19.997**
		[1.892]		[8.762]		[1.811]		[7.926]
d6		23.186***		21.310**		22.350***		23.514***
		[1.930]		[8.818]		[2.252]		[8.527]
d7		22.620***		21.432**		24.089***		23.913***
		[2.256]		[8.552]		[2.596]		[8.560]
d9		22.508***						23.488**
		[2.468]						[9.174]
Constant	-5,340	-16.769***	-8716	-18830	3328	0.027	-3,284	-13,930
	[4.925]	[6.182]	[7.193]	[0.000]	[5.839]	[10.396]	[5.947]	[0.000]
Observations	141	141	181	181	96	96	131	131

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Model (1) has a time trend
Model (2) has temporal dummies.

Table 4.2
Experiment of interest
Years of education
Panel I

	Intensive	Non-Intensive	Difference
Young	10.183	10.5	-0.317
	[00]	[00]	[0.044]
Old	10.991	12.218	-1.227
	[00]	[0.001]	[0.063]
Difference	-0.809	-1.718	**0.91**
	[0.043]	[0.054]	**[0.077]**

Note: standards erros between brackets

Control experiment 1
Years of education
Panel II

	Intensive	Non-Intensive	Difference
Young	9.425	10.452	-1.026
	[00]	[0.001]	[0.074]
Old	9.118	10.217	-1.098
	[00]	[0.001]	[0.082]
Difference	0.307	0.235	**0.072**
	[0.055]	[0.079]	**[0.11]**

Note: standards erros between brackets
Young= 19 20 21 22 23
Old= 24 25 26 27 28

Control experiment 2
Years of education
Panel III

	Intensive	Non-Intensive	Difference
Young	9.849	11.076	-1.227
	[0.001]	[0.001]	[0.086]
Old	9.49	10.515	-1.026
	[0.001]	[0.001]	[0.096]
Difference	0.36	0.561	**-0.202**
	[0.066]	[0.089]	**[0.129]**

Note: standards erros between brackets
Young= 16, 17, 18
Old= 20, 21, 22

Table 5.1
Cohorts

	Name	Ages
Cohort	A	
Young		8,9,10,11,12
Old		14, 15, 16, 17, 18
Cohort	B	
Young		8,9,10,11
Old		15, 16, 17, 18
Cohort	C	
Young		11, 12, 13
Old		14, 15, 16

Note: age in 1996.

Table 5.2
Impact of educational reform on educational outcomes
Binary variable for reform

	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
	Years of educaion			Complete Highschool		
All	0.733***	0.787***	0.471***	0.023	0.026*	0.034***
	[0.179]	[0.204]	[0.097]	[0.015]	[0.015]	[0.013]
Observations	59449	47339	35850	59996	47797	36084
Males	0.785***	0.845***	0.606***	0.028	0.034*	0.036**
	[0.184]	[0.217]	[0.108]	[0.019]	[0.020]	[0.018]
Observations	29128	23213	17693	29429	23466	17821
All Poor	0.472***	0.527***	0.291***	0.068***	0.076***	0.074***
	[0.134]	[0.160]	[0.070]	[0.019]	[0.022]	[0.015]
Observations	32485	26002	19065	32850	26302	19229
Poor males	0.546***	0.605***	0.368***	0.072***	0.083***	0.082***
	[0.132]	[0.163]	[0.102]	[0.020]	[0.026]	[0.018]
Observations	15521	12446	9085	15722	12612	9176

Clustered standard errors in brackets
* significant at 5%; ** significant at 1%
All regressions include city and cohort fixed effects

Table 5.3
Impact of educational reform on educational outcomes
Intensity of reform=share reformed schools

	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
	Years of educaion			Complete Highschool		
All	0.007***	0.008***	0.005***	0.00	0.00	0.000**
	[0.002]	[0.002]	[0.001]	[0.000]	[0.000]	[0.000]
Observations	59449	47339	35850	59996	47797	36084
Males	0.008***	0.009***	0.006***	0	0	0
	[0.002]	[0.002]	[0.001]	[0.000]	[0.000]	[0.000]
Observations	29128	23213	17693	29429	23466	17821
All Poor	0.005***	0.006***	0.003***	0.001**	0.001***	0.001***
	[0.002]	[0.002]	[0.001]	[0.000]	[0.000]	[0.000]
Observations	32485	26002	19065	32850	26302	19229
Poor males	0.006***	0.007***	0.004***	0.000*	0.001*	0.001***
	[0.001]	[0.002]	[0.001]	[0.000]	[0.000]	[0.000]
Observations	15521	12446	9085	15722	12612	9176

Clustered standard errors in brackets
* significant at 5%; ** significant at 1%
All regressions include city and cohort fixed effects

Table 5.4
Impact of educational reform on labor outcomes
Binary variable for reform

	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
		Employed			hours worked			labor income	
All	0.063**	0.074**	0.049**	3.093***	3.261**	2.280***	0.139***	0.152***	0.135***
	[0.029]	[0.034]	[0.020]	[1.132]	[1.328]	[0.770]	[0.041]	[0.037]	[0.044]
Observations	59998	47799	36083	59998	47799	36083	15703	12390	10427
Males	0.058*	0.065*	0.058**	3.170**	3.239**	3.552***	0.194***	0.223***	0.203***
	[0.030]	[0.037]	[0.023]	[1.272]	[1.504]	[1.204]	[0.038]	[0.038]	[0.050]
Observations	29431	23468	17820	29431	23468	17820	9742	7692	6474
All Poor	0.034	0.03	0.036**	1.063	1.01	0.371	-0.007	0.028	0.031
	[0.026]	[0.029]	[0.018]	[1.035]	[1.151]	[1.067]	[0.046]	[0.052]	[0.061]
Observations	32852	26304	19229	32852	26304	19229	9150	7149	6098
Poor males	0.044	0.032	0.053	1.921	1.656	1.232	0.051	0.118***	0.051
	[0.033]	[0.034]	[0.035]	[1.423]	[1.629]	[1.839]	[0.048]	[0.043]	[0.053]
Observations	15724	12614	9176	15724	12614	9176	5941	4666	3928

Clustered standard errors in brackets
* significant at 5%; ** significant at 1%
All regressions include city and cohort fixed effects

Table 5.5
Impact of educational reform on labor outcomes
Intensity of reform=share reformed schools

	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
		Employed			hours worked			labor income	
All	0.001	0.001	0.001**	0.021	0.021	0.020**	0.001	0.001*	0.001***
	[0.000]	[0.000]	[0.000]	[0.014]	[0.017]	[0.009]	[0.000]	[0.000]	[0.000]
Observations	59998	47799	36083	59998	47799	36083	15703	12390	10427
Males	0.001	0.001	0.001**	0.023	0.02	0.029*	0.001**	0.002***	0.002***
	[0.000]	[0.000]	[0.000]	[0.018]	[0.021]	[0.015]	[0.001]	[0.000]	[0.001]
Observations	29431	23468	17820	29431	23468	17820	9742	7692	6474
All Poor	0	0	0	0.006	0.008	0.001	0	0	0.001*
	[0.000]	[0.000]	[0.000]	[0.014]	[0.016]	[0.014]	[0.000]	[0.000]	[0.001]
Observations	32852	26304	19229	32852	26304	19229	9150	7149	6098
Poor males	0	0	0	0.008	0.006	-0.003	0	0.001*	0.001
	[0.000]	[0.000]	[0.000]	[0.019]	[0.022]	[0.023]	[0.000]	[0.000]	[0.000]
Observations	15724	12614	9176	15724	12614	9176	5941	4666	3928

Clustered standard errors in brackets
* significant at 5%; ** significant at 1%
All regressions include city and cohort fixed effects

Table 6.1
Average Math and Spanish scores

	Pre-treatment		Post-treatment		Difference	
	Math	Spanish	Math	Spanish	Math	Spanish
Control Group (C)	60.25	62.55	61.80	59.59	1.55	-2.96
	(0.142)	(0.126)	(0.142)	(0.138)	(0.201)	(0.186)
Treatment Group (T)	61.52	63.75	64.43	63.50	2.91	-0.25
	(0.319)	(0.285)	(0.338)	(0.330)	(0.465)	(0.436)
Difference (T - C)	1.26	1.20	2.62	3.90	1.36	2.71
	(0.349)	(0.311)	(0.367)	(0.356)	(0.009)	(0.009)

Table 6.2
Variable definitions and data sources

Variable	Definition	Sources
Matemática	School average of correct responses in Math tests	Operativo Nacional de Evaluación Educativa (Mro. De Cultura y Educación de la Nación).
Lengua	School average of correct responses in Spanish tests	Operativo Nacional de Evaluación Educativa (Mro. De Cultura y Educación de la Nación).
d_trat	Dummy variable, equals one if the school has students in the last year of polimodal under the new educational structure, zero otherwise	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).
d_trat x pobre	Interaction of the treatment variable (*d_trat*) and a binary variable (*pobre*) equalling one if the county where the school is located is poor.	Operativo Nacional de Evaluación Educativa, Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación) y Censo Nacional de las personas 2001.
d_trat x NBI	Interaction of the treatment variable (*d_trat*) and a binary variable (*pobre*) equalling one if the county where the school is located has unmet basic needs relative to the country median.	Operativo Nacional de Evaluación Educativa, Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación) y Censo Nacional de las personas 2001.
d_trat x rural	Interaction of the treatment variable (*d_trat*) and a binary variable (*rural)* if the school is located in a rural area	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).
d_trat x pública	Interaction of the treatment variable (*d_trat*) and a binary variable (*publica)* if the school is public	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).
d_trat x cant_rel_alum	Interaction of the treatment variable (d_trat) and a variable (*cant_rel_alum*); which measures the relative quantity of students in the last year relative to the sample median.	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).
d_trat x cant_rel_alum2	Interaction of the treatment variable (d_trat) and the binary variable cant_rel_alum squared;	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).
Resultado Fiscal	Fiscal deficit or surplus relative to GDP	Consejo Federal de Inversiones y Dirección Nacional de Coordinación Fiscal con las Provincias (Mro. De Economía de la Nación).
PBG_pc	Per capita regional GDP.	Consejo Federal de Inversiones e INDEC.
Desempleo	Uneployment rate (average May- October)	Encuesta permanente de hogares (EPH), INDEC
Pobreza	Poverty rate (average May-October)	Encuesta permanente de hogares (EPH), INDEC
Implementación Ciclos	Dummy variable, equals one if the school has students in the last year of polimodal under the new educational structure, zero otherwise	Operativo Nacional de Evaluación Educativa y Relevamientos Anuales 1997-2000 (Mro. De Cultura y Educación de la Nación).

Table 6.3
Impact of LFE on scores

	Model 1		Model 2		Model 3	
Independent variables	Math	Spanish	Math	Spanish	Math	Spanish
d_trat	0.583	2.649***	-1,065	1.182**	3.122**	4.763***
	[0.571]	[0.329]	[0.633]	[0.423]	[1.320]	[1.301]
Resultado Fiscal			0.031	0.013	-0.083	-0.093
			[0.073]	[0.051]	[0.185]	[0.155]
PBG_pc			-0.001	-0.001**	0.001	0.001**
			[0.001]	[0.0009]	[0.001]	[0.0009]
Desempleo			-0.410**	-0.344**	0.076	0.140
			[0.173]	[0.165]	[0.273]	[0.233]
Pobreza			-0.504***	-0.442*	0.379***	0.306***
			[0.178]	[0.255]	[0.056]	[0.053]
Implementación Ciclos	-2.018***	-0.269	-2.301***	-0.539	-1.504*	0.029
	[0.631]	[0.326]	[0.589]	[0.917]	[0.812]	[0.658]
Observations	22089	22087	21675	21678	15986	15985
Number of schools	5745	5745	5643	5643	-	-
Number of counties	-	-	-	-	229	229
School fixed effects	Yes	Yes	Yes	Yes	No	No
County fixed effects	No	No	No	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note (i) All estimations are within and include a constant.
(ii) Standard errors in parenthesis
(iii) Clustered standard errors at the province level
(iv) * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6.4
Impact of LFE on scores

	Model 1		Model 2		Model 3	
Independent variables	Math	Spanish	Math	Spanish	Math	Spanish
d_trat	-1,065	1.182**	-0.899	1.227*	-1,696	1.663**
	[1.114]	[0.529]	[0.786]	[0.630]	[1.850]	[0.681]
Resultado Fiscal	0.031	0.013	0.087*	0.051	-1.880*	-1,114
	[0.073]	[0.053]	[0.048]	[0.042]	[1.005]	[0.681]
PBG_pc	-0.001	-0.001**	-0.001	-0.001**	-0.001	0.005***
	[0.001]	[0.0004]	[0.0004]	[0.0004]	[0.001]	[0.001]
Desempleo	-0.410**	-0.344**	-0.381***	-0.253***	-1.064***	-0.628**
	[0.186]	[0.146]	[0.112]	[0.097]	[0.365]	[0.299]
Pobreza	-0.504**	-0.442**	-0.431***	-0.456***	-0.377***	-0.333*
	[0.248]	[0.172]	[0.152]	[0.137]	[0.115]	[0.181]
Implementación Ciclos	-2.301***	-0.539	-2.304***	-0.549	-1,585	-0.557
	[0.847]	[0.899]	[0.437]	[0.400]	[1.797]	[0.556]
Observations	21675	21678	15986	15985	21675	21678
Number of schools	5643	5643	4109	4109	5643	5643
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No	No
Year and province fixed effects	No	No	No	No	Yes	Yes

Note: (i) Estimations LSDV in model (1) y (2). Estimations within in Model (3). All estimations include a constant.
(ii) Standard errors in parenthesis
(iii) Clustered standard errors at the year-provincial level (Model 1), year-departments (Model 2), at province level (Model 3).
(iv) * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6.5
Group means

Variables	All Sample (1)	Control Group (C) (2)	Treatment Group (T) (3)	Diffrence (C - T) (4) = (2)-(3)
Pobre	0.226	0.249	0.066	0.184
	(0.003)	(0.004)	(0.005)	(0.006)
Rural	0.102	0.100	0.117	-0.017
	(0.002)	(0.002)	(0.006)	(0.007)
Pública	0.571	0.583	0.476	0.107
	(0.003)	(0.003)	(0.010)	(0.010)
Cant_rel_alum	14,570	15.068	10.621	4.447
	(0.310)	(0.332)	(0.837)	(0.901)
Number of Schools	5745	5107	638	

Note: Standard errors in parenthesis

Table 6.6
Heterogeneous impact

	Model 1		Model 2		Model 3	
Variables	Math	Spanish	Math	Spanish	Math	Spanish
d_trat	-0.967	1.400**	-0.971	1.105**	-1,044	1.328***
	[0.736]	[0.501]	[0.715]	[0.513]	[0.650]	[0.376]
d_trat x pobre	0.694	-2.147***				
d_trat x NBI			-0.073***	-0.177***		
			[0.018]	[0.009]		
d_trat x rural					-0.604***	-1.853***
					[0.093]	[0.054]
Resultado Fiscal	0.061	0.033	0.089	0.055	0.034	0.018
	[0.062]	[0.047]	[0.080]	[0.059]	[0.072]	[0.049]
PBG_pc	-0.001	-0.001*	-0.001	-0.001*	-0.001	-0.001**
	[0.001]	[0.0005]	[0.001]	[0.001]	[0.001]	[0.0004]
Desempleo	-0.384**	-0.248*	-0.382*	-0.256	-0.393**	-0.325***
	[0.181]	[0.125]	[0.190]	[0.172]	[0.176]	[0.110]
Pobreza	-0.437**	-0.450***	-0.437	-0.460	-0.500**	-0.448***
	[0.188]	[0.140]	[0.292]	[0.286]	[0.183]	[0.128]
Implementación Ciclos	-2.294***	-0.542	-2.307*	-0.556	-2.408***	-0.678*
	[0.609]	[0.425]	[1.135]	[1.091]	[0.604]	[0.349]
Observations	16104	16106	15945	15945	20935	20936
Number of schools	4141	4141	4098	4098	5401	5401
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Si

Nota: (i) Estimations within. All the specifications include a constant
(ii) Standard errors in parenthesis
(iii) Robust standard errors, clustered at the provincial level.
(iv) * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6.7
Heterogeneous impact

	Model 1		Model 2	
Variables	Math	Spanish	Math	Spanish
d_trat	-0.372	2.058***	-1.137*	0.960**
	[0.653]	[0.382]	[0.643]	[0.397]
d_trat x pública	-1.458***	-1.843***		
	[0.160]	[0.139]		
d_trat x cant_rel_alum			0.021***	0.045***
			[0.003]	[0.004]
d_trat x cant_rel_alum2			-0.00007***	-0.0001***
			[0.00001]	[0.00002]
cant_rel_alum			0.008	0.006
			[0.005]	[0.005]
cant_rel_alum2			-0.00002	0.00001
			[0.00002]	[0.00002]
Resultado Fiscal	0.031	0.013	0.028	0.010
	[0.073]	[0.051]	[0.072]	[0.051]
PBG_pc	-0.001	-0.001**	-0.001	-0.001**
	[0.001]	[0.0003]	[0.001]	[0.0003]
Desempleo	-0.410**	-0.345***	-0.414**	-0.353***
	[0.173]	[0.108]	[0.174]	[0.109]
Pobreza	-0.505***	-0.442***	-0.505**	-0.452***
	[0.178]	[0.139]	[0.180]	[0.146]
Implementación Ciclos	-2.301***	-0.539	-2.315***	-0.562
	[0.589]	[0.376]	[0.592]	[0.387]
Observations	21675	21678	21675	21678
Number of schools	5643	5643	5643	5643
School fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Si

Note: (i) Estimations within. All the specifications include a constant
(ii) Standard errors in parenthesis
(iii) Robust standard errors, clustered at the provincial level.
(iv) * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6.8

Variable	Math scores-Component 1	Spanish sccores - Component 1	Math scores-Component 2	Spanish sccores - Component 2	Math scores-Component 3	Spanish sccores - Component 3	Math scores-Component 4	Spanish sccores - Component 4
Resultado Fiscal	0.028	0.094	0.028	0.094	0.028	0.094	0.028	0.094
	[0.095]	[0.064]	[0.095]	[0.064]	[0.095]	[0.064]	[0.095]	[0.064]
PBG_pc	-0.001	-0.001*	-0.001	-0.001*	-0.001	-0.001*	-0.001	-0.001*
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Desempleo	-0.392**	-0.373***	-0.391**	-0.372***	-0.392**	-0.372***	-0.392**	-0.372***
	[0.162]	[0.112]	[0.162]	[0.112]	[0.162]	[0.112]	[0.162]	[0.112]
Pobreza	-0.444**	-0.355***	-0.444**	-0.355***	-0.444**	-0.355***	-0.444**	-0.355***
	[0.171]	[0.117]	[0.171]	[0.117]	[0.171]	[0.117]	[0.171]	[0.117]
Implementación Ciclos	-2.332***	-0.576	-2.331***	-0.576	-2.332***	-0.576	-2.332***	-0.576
	[0.562]	[0.364]	[0.562]	[0.364]	[0.562]	[0.364]	[0.562]	[0.364]
d_trat	-1,126	1.297***	-1,167	1.120**	-1.205*	1.062**	-1.249*	1.174**
	[0.680]	[0.433]	[0.682]	[0.435]	[0.683]	[0.436]	[0.682]	[0.435]
d_tratxcomp1	0.143***	0.504***						
	[0.014]	[0.011]						
d_tratxcomp2			0.529***	0.553***				
			[0.006]	[0.003]				
d_tratxcomp3					-0.318***	-0.534***		
					[0.007]	[0.005]		
d_tratxcomp4							0.340***	-0.350***
							[0.009]	[0.008]
Observations	13294	13288	13294	13288	13294	13288	13294	13288
Number of schools	3414	3414	3414	3414	3414	3414	3414	3414

Robust standard errors in brackets, clustered at the provincial level
* significant at 10%; ** significant at 5%; *** significant at 1%

Component 1 is linked to school physical infrastructure, 2 is linked to the quality of school materials. Component 3 is related to the availability to educational material (labs, computers, etc.) Component 4 is related to school teachers' activities (courses, training, etc.).

Table 6.9
Robustness analysis

Table 12
Robustness analysis

	Analysis (1) Matching				Análisis (2) Multiple Control Groups			
	Model (1)		Model (2)		Control Group (1)		Control group (2)	
Independent variables	Math	Spanish	Math	Spanish	Math	Spanish	Math	Spanish
d_trat	-1,123	1.174***	-0.930	1.314***	-1.346*	1.480***	1,821	3.425***
	[0.658]	[0.381]	[0.641]	[0.386]	[0.736]	[0.497]	[1.200]	[1.049]
Resultado Fiscal	0.021	0.005	0.011	-0.0004	0.039	0.028	0.100	-0.038
	[0.075]	[0.048]	[0.071]	[0.042]	[0.076]	[0.053]	[0.139]	[0.088]
PBG_pc	-0.001	-0.001**	-0.001	-0.001*	-0.001	-0.001**	-0.002	-0.001
	[0.001]	[0.0003]	[0.001]	[0.0004]	[0.001]	[0.0004]	[0.001]	[0.0005]
Desempleo	-0.391**	-0.320***	-0.396**	-0.313***	-0.435	-0.211	-0.355**	-0.406**
	[0.175]	[0.106]	[0.168]	[0.109]	[0.260]	[0.145]	[0.127]	[0.160]
Pobreza	-0.483**	-0.457***	-0.437**	-0.446***	-0.456*	-0.163	-0.289	-0.337
	[0.189]	[0.128]	[0.204]	[0.121]	[0.225]	[0.116]	[0.259]	[0.198]
Implementación Ciclos	-2.401***	-0.633*	-2.654***	-0.677*			0.433	1.711**
	[0.623]	[0.353]	[0.651]	[0.335]			[0.648]	[0.686]
Observations	20467	20466	15318	15311	12701	12680	11467	11487
Number of schools	5281	5281	3946	3946	3302	3302	2979	2979
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: (i) Estimations within. All the specifications include a constant

(ii) In Analysis (1), Model (1) we eliminate those schools whose size is below the first percentile (for the treatment group) and above the 99 percentile (for the control group) of school size distribution. In model (2) we eliminate schools below the 15th percentile and above the 85th respectively, as in model (1) In Model (3) we eliminate those schools whose predicted probability of receiving subsidies is below the 1st percentile (for the treated units) and above the 99 percentile of school distribution in the control group. Model (4) is the same as above, but the percentiles are 15 and 75 respectively.

(iii) In Analysis (2), Control Group (1) is formed by those schools who have not implemented polimodal in the period 1997-2000. Control Group (2) is formed by those schools who implement some other year of polimodal in the period 1997-2000.

(iv) Standard errors in parenthesis

(v) Robust standard errors, clustered at the provincial level.

(vi) * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7.1: Educational Reform

Summary Statistics

6th grade - Primary Schools				
	City (20 blocks)	GBA (20 blocks)	City (10 blocks)	GBA (10 blocks)
Mean Spanish Scores	72.37	65.98	71.38	67.28
s.d	16.92	19.81	17.28	19.82
Nr of Observations	6,534	4,406	3,358	1,759
Mean Math Scores	67.63	60.50	67.80	60.82
s.d	18.72	20.90	18.56	21.53
Nr of Observations	6,718	4,477	3,513	1,751

5th year - Secondary Schools				
	City (20 blocks)	GBA (20 blocks)	City (10 blocks)	GBA (10 blocks)
Mean Spanish Scores	65.21	59.64	65.49	60.94703
s.d	18.27	18.26	17.99	18.43996
Nr of Observations	3,270	2,807	1,142	1,180
Mean Math Scores	67.72	64.34	67.46567	65.82248
s.d	18.52	19.65	18.73747	18.95619
Nr of Observations	4,034	3,032	1,675	1,228

Table 7.2:

6th grade primary - 20 blocks schools

	Spanish Scores				Math Scores			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Reform	-6.39	-7.13	-4.48	-4.30	-7.13	-7.87	-5.32	-5.21
	-17.53	-5.89	-4.46	-3.01	-18.42	-5.90	-4.48	-2.91
% students repeating grade				-0.08				-0.11
				-2.80				-3.88
teacher_student_ratio								
school_size				0.11				0.00
				1.59				1.29
Public (=1)		-11.06	-7.58	-5.68		-9.84	-6.10	-3.91
		-10.32	-8.30	-4.24		-7.88	-5.40	-2.17
Male (=1)			-3.94	-3.65			2.19	2.26
			-8.53	-6.15			4.47	3.64
repetidor (=1)			-5.29	-3.61			-6.20	-5.04
			-6.79	-3.77			-7.15	-4.81
nota_len99			1.84	1.60			1.19	0.96
			10.38	7.47			6.93	4.07
nota_mat99			1.34	1.47			2.55	2.51
			8.26	6.52			13.73	10.05
household_size			-0.87	-3.96			-0.84	-0.74
			-6.36	-6.21			-6.17	-4.72
help_work_home			-3.88	0.91			-3.86	-3.49
			-8.23	0.15			-8.30	-5.16
_cons	72.37	78.84	61.43	46.34	67.63	73.45	48.80	37.22
	345.82	110.20	33.20	4.86	296.15	85.22	24.60	3.21
Number of observations	10,940	10,940	8,889	4,647	11,195	11,195	9,098	4,725

t statistics are in parenthesis
Standard errors were clustered at the school level

Table 7.3

5th year secondary - 20 blocks schools

	Spanish Scores				Math Scores			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Reform	-5.57	-4.12	-4.05	-5.14	-3.38	-1.83	-1.45	-0.32
	-11.86	-2.40	-2.42	-2.00	-7.32	-0.83	-0.70	-0.13
% students repeating grade				-0.10				-0.06
				-2.69				-1.30
teacher_student_ratio								
school_size				0.01				0.01
				1.41				1.78
Public (=1)		-12.99	-10.37	-12.85		-11.40	-10.49	-15.44
		-7.64	-6.20	-4.24		-5.67	-5.37	-4.70
Male (=1)			-3.57	-3.64			4.96	4.38
			-3.79	-3.51			4.18	3.89
household_size			-0.68	-0.87			-0.72	-0.86
			-2.89	-3.61			-3.19	-3.36
repetidor (=1)			-7.95	-6.56			-7.45	-6.27
			-9.45	-5.84			-8.70	-5.01
nota_len99			1.62	1.59			1.22	1.19
			7.30	5.47			5.14	3.82
nota_mat99			1.04	1.06			1.64	1.57
			6.21	5.20			8.25	7.82
ayuda_trab			-3.56	-3.42			-3.08	-3.05
			-7.10	-5.56			-4.85	-3.75
_cons	65.21	71.79	58.60	57.10	67.72	73.08	54.45	61.57
	204.06	51.74	22.23	3.16	232.23	52.87	16.65	2.46
Number of observations	6,077	6,077	5,132	2,470	6,077	7,066	5,859	2,869

t statistics are in parenthesis
Standard errors were clustered at the school level

Table 7.4

6th grade primary - 20 blocks schools - Placebo experiment

	Spanish Scores		Math Scores	
	(1)	(2)	(1)	(2)
Reform	1.99	2.33	2.65	2.65
	1.01	1.75	1.33	1.33
% students repeating grade		-0.08		-0.09
		-2.38		-2.09
teacher_student_ratio				
				0.01
school_size		0.00	0.01	1.24
		0.66	1.24	-1.86
Public (=1)	-4.08	-3.33	-1.86	-0.98
	-4.33	-2.83	-0.98	2.73
Male (=1)	-3.88	-3.81	2.73	3.35
	-7.47	-5.62	3.35	
repetidor (=1)	-4.94	-3.73	-0.35	-5.62
	-4.17	-2.60	-1.01	-3.94
nota_len99	2.25	2.14	-5.62	1.46
	8.69	6.13	-3.94	4.65
nota_mat99	0.97	1.15	1.46	2.56
	4.20	4.81	4.65	9.49
household_size	-0.77	-0.55	2.56	-0.35
	-3.33	-2.05	9.49	-1.01
help_work_home	-3.17	-3.68	-3.21	-3.21
	-6.72	-5.79	-3.93	-3.93
_cons	58.13	31.81	15.27	15.27
	24.62	2.32	0.63	0.63
Number of observations	4,289	2,258	4,334	2,290

Table 7.5
5th year secondary - 20 blocks schools - Placebo experiment

	Spanish scores	Math scores
Reform	2.82	3.16
	0.86	0.73
% students repeating grade	-0.12	-0.10
	-1.53	-1.08
teacher_student_ratio		
school_size	0.02	0.02
	1.39	1.35
Public (=1)	-17.51	-16.40
	-2.30	-2.10
Male (=1)	-2.94	5.97
	-1.66	3.35
household_size	0.26	0.67
	0.89	1.87
repetidor (=1)	-2.01	-4.00
	-1.19	-2.16
nota_len99	1.25	1.00
	2.67	2.17
nota_mat99	1.14	1.76
	3.78	5.43
ayuda_trab	-4.22	-3.38
	-4.36	-2.91
_cons	71.85	45.14
	1.46	9.00
Number of observations	1,072	1,141

t statistics are in parenthesis
Standard errors were clustered at the school level

Table 7.6

Primary Schools		
	Wald Estimator	Bootstraped standard errors
Math scores	-9.10	1.182
Spanish scores	-5.29	1.037

Secondary schools		
	Wald Estima	Bootstraped standard errors
Math scores	0.50	1.510
Spanish scores	-5.95	1.525

Table 7.7

		Spanish scores	Math scores
CABA(1)	mean	60.77	64.10
	s.d.	14.56	16.53
	# of obs.	255	255
Buenos Aires(2)	mean	60.95	60.60
	s.d.	15.67	16.21
	# of obs.	238	238

(1) considers a sample for all the city for 1993
(2) considers a sample of the whole province of Buenos Aires for 1993

Figure 4.1
Gross enrollment rates by age group

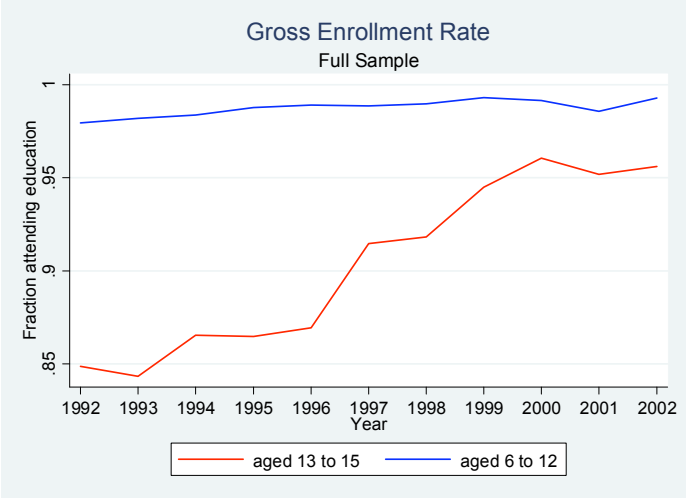


Figure 4.2
Gross enrollment rates by timing of the reform

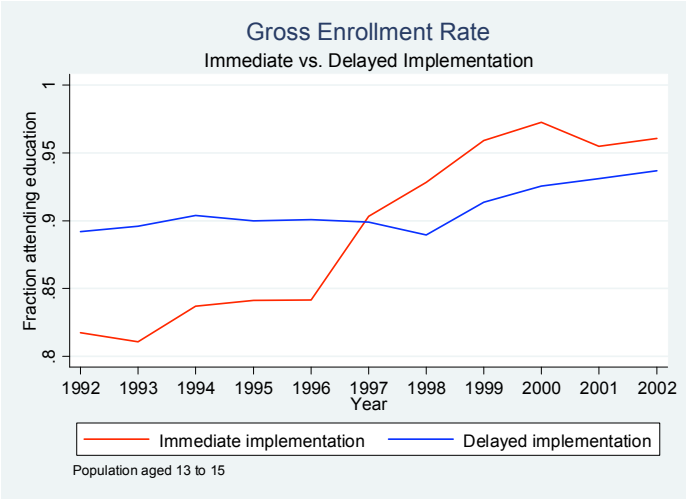


Figure 4.3
Gross enrollment rates by degree of the reform

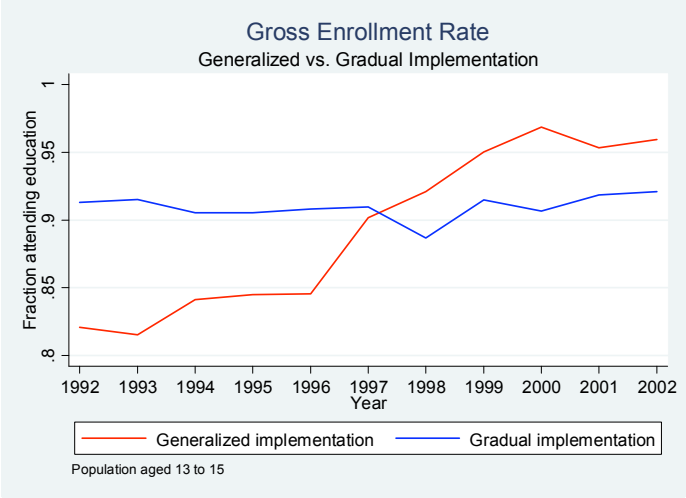


Figure 5.1
Coefficients of the interaction of age in 1996 with program intensity in the province

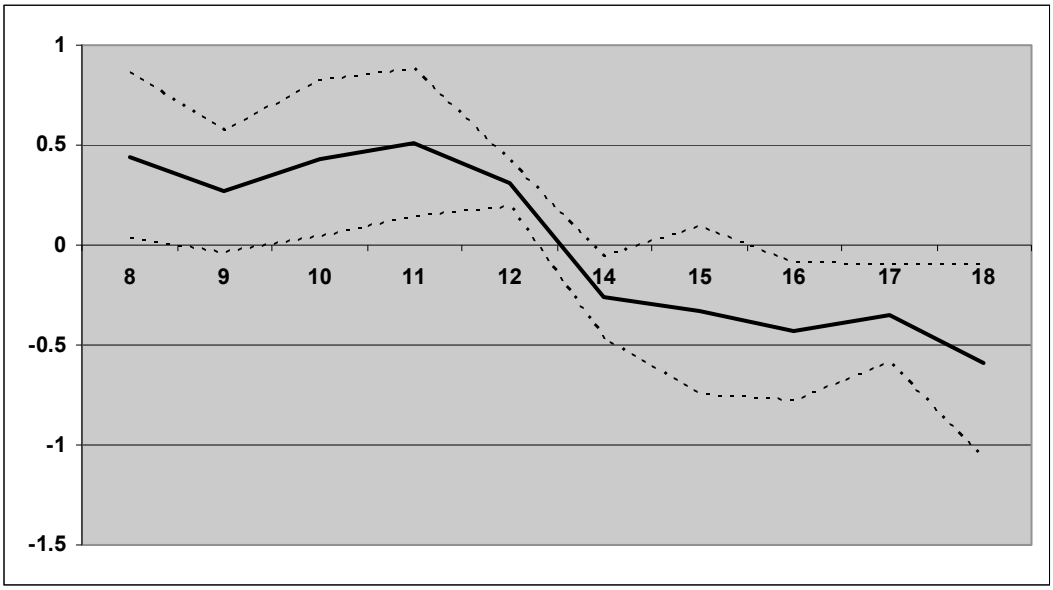


Figure 6.1

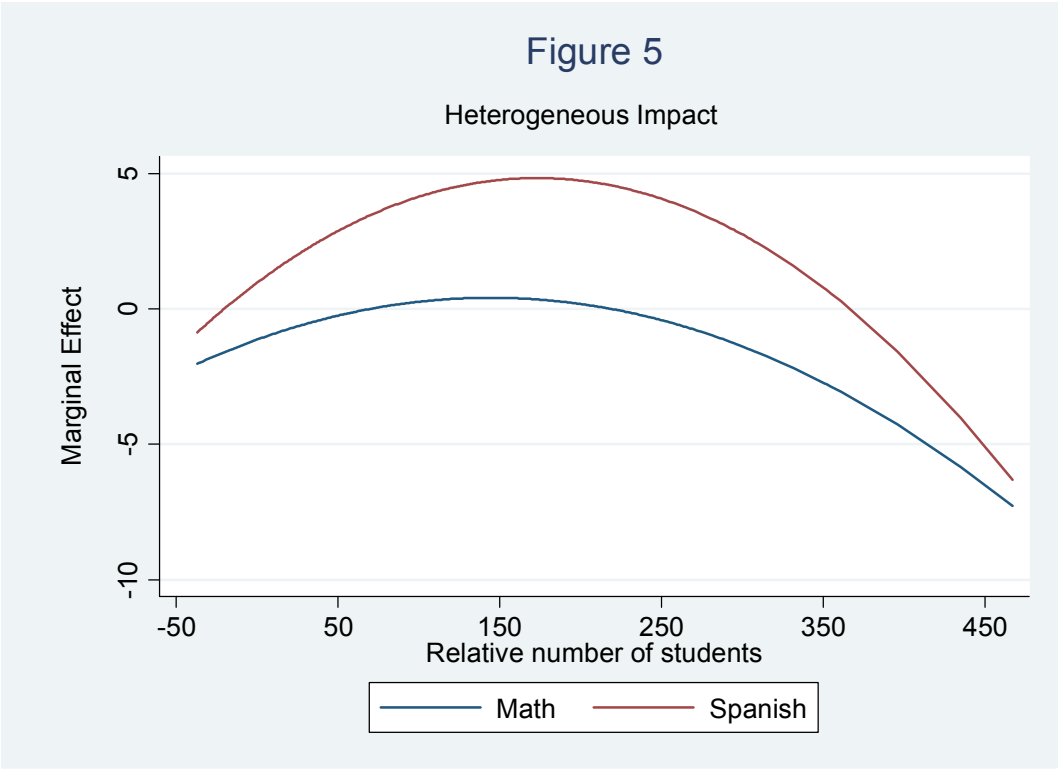


Figure 7.1

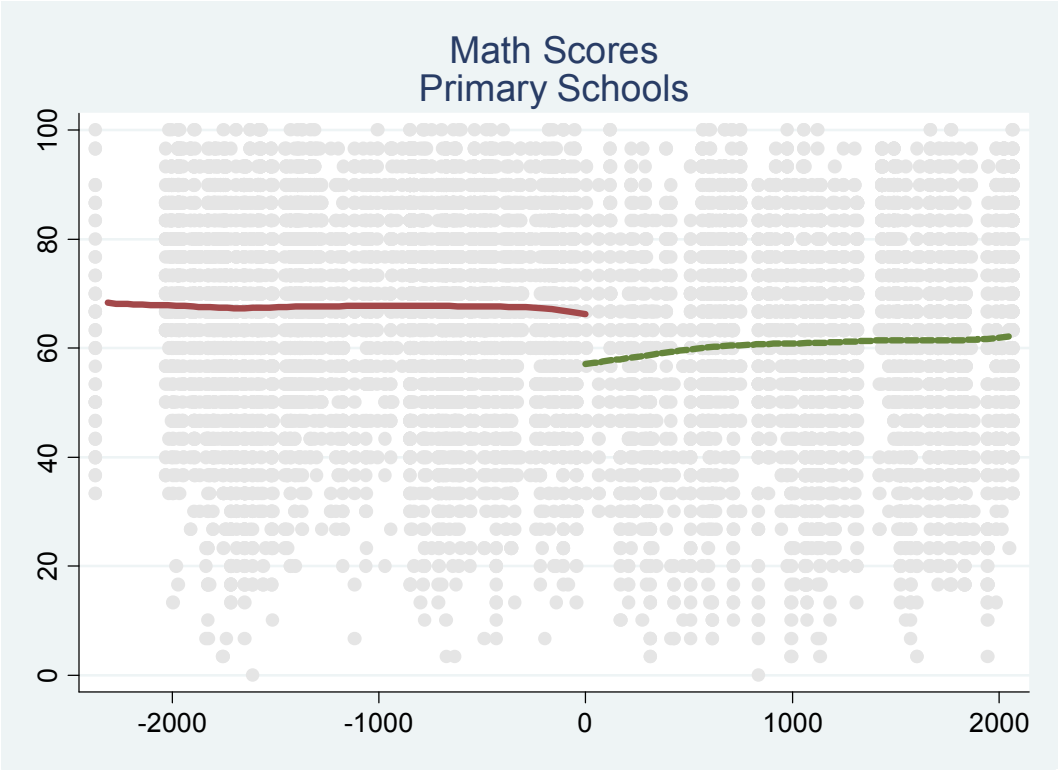


Figure 7.2

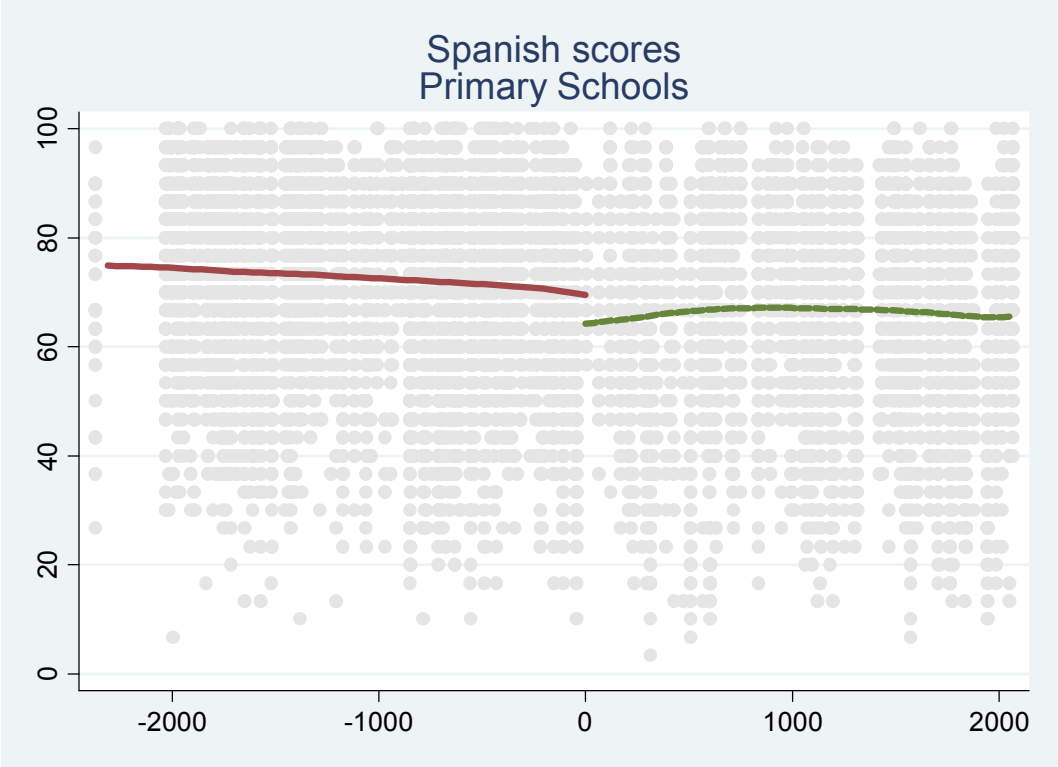


Figure 7.3

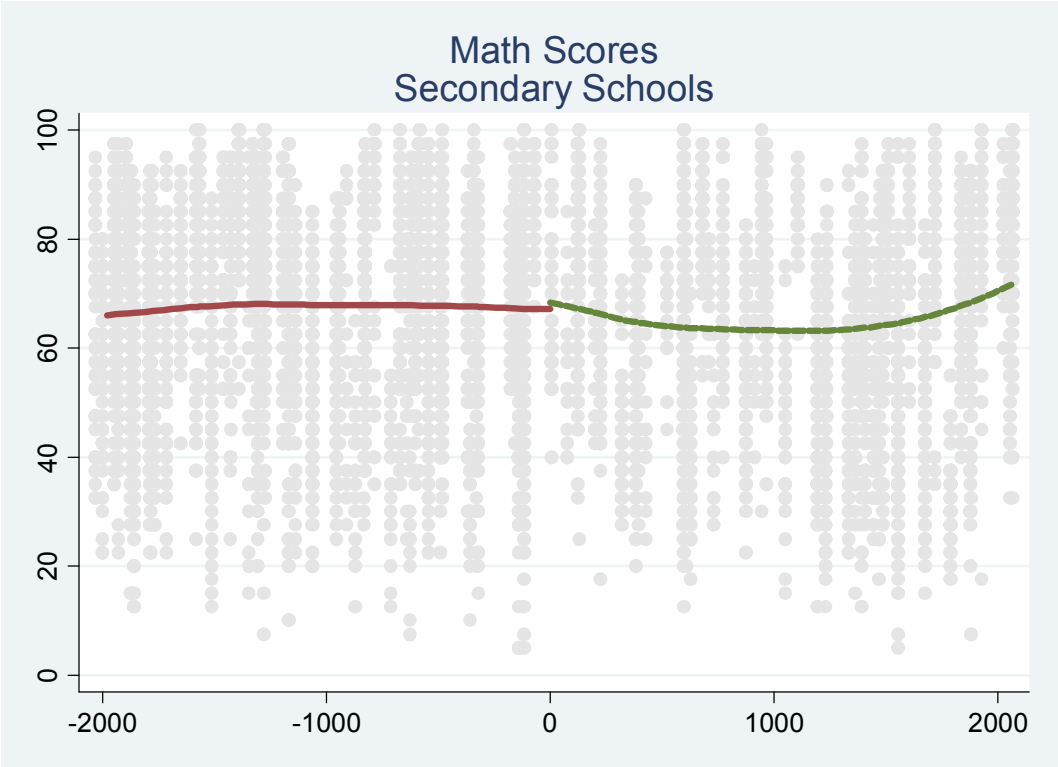
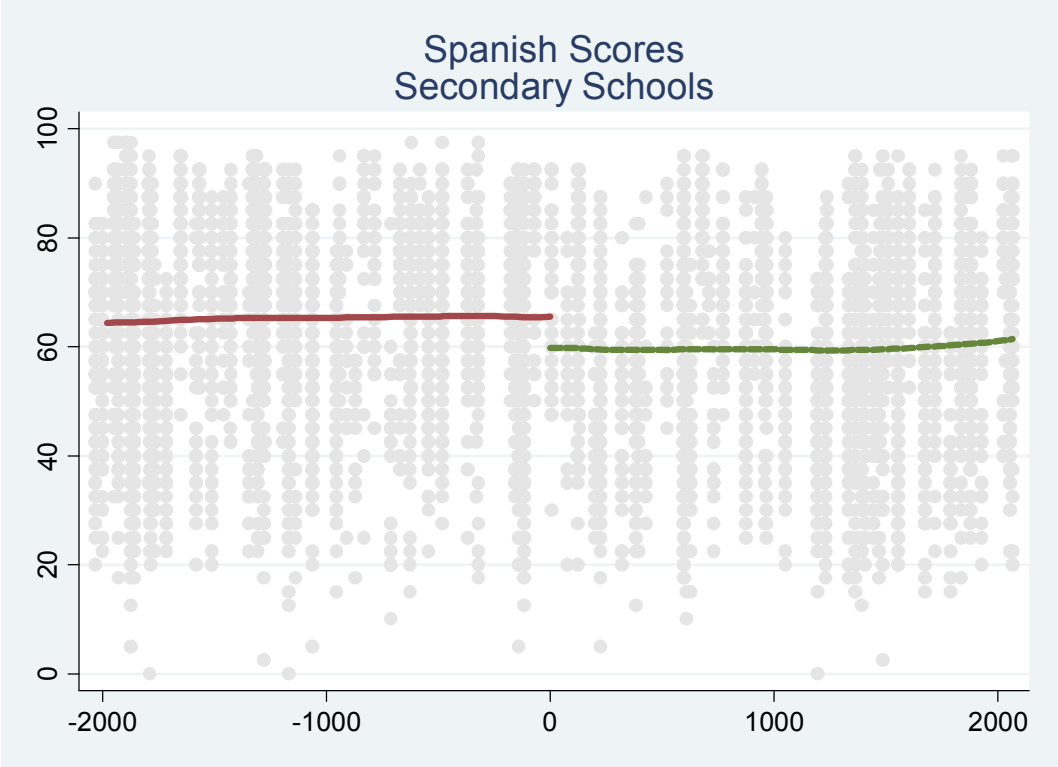


Figure 7.4


Spanish Scores
Secondary Schools
100
80
60
40
20
0
-2000
-1000
0
1000
2000