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**EDUCATION REFORM AND LABOR MARKET OUTCOMES:
THE CASE OF ARGENTINA'S *LEY FEDERAL DE
EDUCACIÓN***

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In the nineties Argentina implemented a large education reform (*Ley Federal de Educación* – LFE) that mainly implied the extension of compulsory education in two additional years. The timing in the implementation substantially varied across provinces, providing a source of identification for unraveling the causal effect of the reform. The estimations from difference-in-difference models suggest that the LFE had a positive impact on years of education and the probability of high school graduation. The impact on labor market outcomes —employment, hours of work and wages— was positive for the non-poor youths, but almost null for the poor.

JEL classification codes: I2, I3

Key words: education, reform, Argentina, employment, wages, poverty

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I. 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. Chief among them was an extension in the years of compulsory education, along with a change in the structure of the educational curricula. 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 then, 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. There are, however, scenarios in which these links may be weak. The return to an additional year of education could be very small for the poor if there exist complementarities with other educational investments, if the poor are less likely to work in the formal sector, or if there are large network effects (see for instance Cunha, Heckman and Schennach 2010; Almond and Currie 2011; and Zimmerman 2013).¹ Only the empirical evidence can settle the issue of the relationship between time spent at school and improvements in labor market outcomes. While evidence on this relationship is well established for developed countries, evidence for developing countries is much scarcer (Duflo 2001).

In this paper we evaluate the impact of a large education reform in Argentina (the LFE) on several educational and labor outcomes by exploiting 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 provincial governments to decide on the timing of the reform. While in some provinces the reform was quickly implemented after

¹ For instance, Cunha, Heckman and Schennach (2010) develop a human capital model with dynamic complementarities between educational investments. In their model, key cognitive and non-cognitive skills are developed early in life, and these skills considerably increase the productivity of the educational investments carried out later in life. Hence, as a corollary of this model, a given educational investment during high school might be less productive for disadvantaged populations.

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. We take advantage of this source of variation in the exposure to the reform to study its impact on different educational and labor market outcomes. In particular, we are interested in evaluating whether poor youngsters who had to attend two additional school years were more likely to finish high school, and performed better in the labor market.

The rest of this paper is organized as follows. The education reform is described in Section II. Section III presents the methodology and describes the data sources. Section IV presents the main results on the effects of the LFE on education and labor market variables. Finally, section V closes with some remarks.

II. The education reform

In the early nineties Argentina decentralized the provision of schooling services, previously in hands of the federal government.² The enactment of the *Ley Federal de Educación* (LFE) on April 14th, 1993 (Law 24195) introduced a second set of reforms, among which the extension of mandatory education stands out.³ 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: (i) pre-primary education for children aged five became compulsory; (ii) the primary level, which comprised seven years in the previous law, was replaced by a nine-year level named *Educación General Básica (EGB)*; and (iii) the five years of high school education were replaced by a three-year level called “*Polimodal*”. Table 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.

² Decentralization has been one of the main recent institutional innovations in developing countries. Galiani, Gertler and Schargrodsky (2007) find that decentralization in the provision of schooling in Argentina increased (decreased) test scores in richer (poorer) districts. Madeira (2006) and Rodriguez (2010) find that decentralization had a positive effect on test scores in Brazil and Colombia, respectively.

³ This is a somehow different change from the one observed in developed countries, which increased the age for which school is mandatory. The policy in Argentina was to increase the number of compulsory years, regardless of age.

Table 1. Educational structure before and after the reform

Age	Before the LFE			After the LFE		
	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

Note: LFE=Ley Federal de Educación; EGB=Educación General Básica.

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).⁴ Under the new structure youths were encouraged to stay two years more in school. Advocators of the LFE argued that this extension might also induce many of them to complete the, now shorter, high school level, and hopefully to get into the tertiary level. Other authors were more skeptical about the enforcement of the law and its actual effects. 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.

The increase in the years of compulsory education was accompanied by other institutional changes also aimed at keeping youths at school for longer. The change

⁴ 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 2012).

in the curricula included several specializations in the *Polimodal* which could be chosen by students according to their preferences, a system that could make the transition to higher education easier. Also, 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.

Table 2. Year and degree of implementation of LFE by province

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 (2008). N.I: not implemented. F: full implementation since the beginning. G: gradual implementation.

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 provincial 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 provinces the reforms were quickly and massively implemented, in others the changes were put into effect more gradually, involving a much smaller percentage of schools (Rivas 2003; Crosta 2008). 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 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.

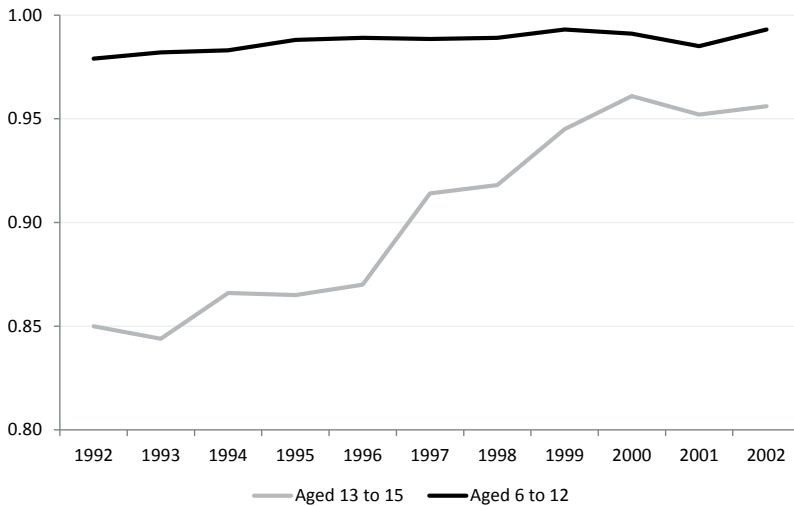
The main objective of the reform was to reduce the high dropout rates in the first years of high school and to contribute to improve labor market outcomes. There is a great deal of literature studying the effects of additional schooling on subsequent gains later in life, related mainly to labor market outcomes (Angrist and Kruger 1991 and Acemoglu and Angrist 2000 for the US, Harmon and Walker 1995 for the UK and Oreopoulos 2006 for Canada). Also, there are some studies that look at other outcomes such as crime (Lochner and Moretti 2004) and teen pregnancy (Black, Devereux and Salvanes 2005). However, as argued by Oreopoulos (2006), these studies look at changes in compulsory schooling laws that took place many decades ago in developed countries and the studies affecting dropout at that time may be different from the ones affecting dropouts today. Furthermore, the above-mentioned studies look at changes in the age a student should remain in school. Our paper looks at a somehow different change, since we examine the number of years that the individual must remain at school, regardless of her age. Our paper is more closely related to Duflo (2001), who studies the effect of a large school construction program in Indonesia —aimed at increasing primary school enrollment in poor areas during the seventies— on labor market outcomes.

III. Methodology

The implementation of the LFE was not accompanied by any strategy to evaluate its impact. This situation forces us to rely on observational data to derive our results. Our analysis seeks to identify the effect of the LFE on several educational and labor outcomes by exploiting the variation in the implementation of the reform across Argentine provinces.

Figures 1 and 2 help to motivate this strategy. Figure 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.

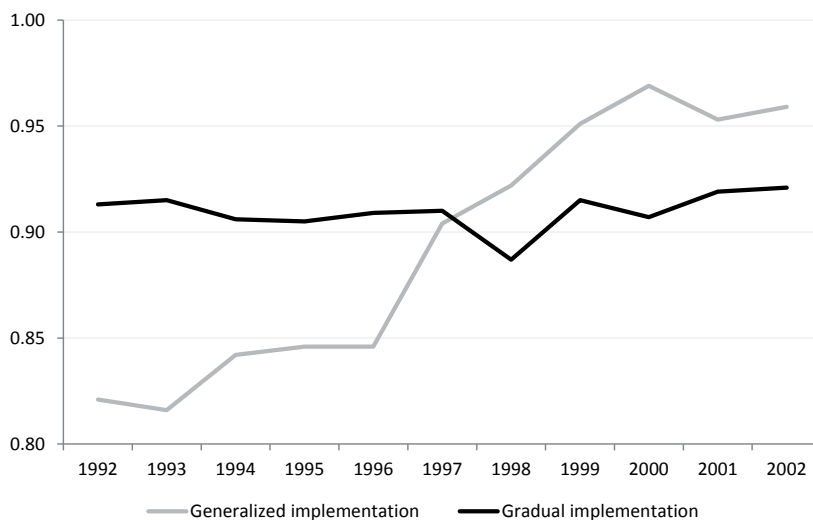
Figure 1. Gross enrollment rates by age group



Note: The gross enrollment rates measure the share of each age group attending school. Source: own calculations based on microdata from EPH (INDEC).

Figure 2 shows enrollment rates for ages 13-15 according to the 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.

Figure 2. Gross enrollment rates by degree in the implementation of the reform



Notes: The gross enrollment rates measure the share of each age group attending school. The provinces that followed each modality of implementation (*Gradual* or *Generalized*) are listed in Table 2. Population aged 13 to 15. Source: own calculations based on microdata from EPH (INDEC).

One of the basic points of the paper is to evaluate whether individuals who were affected by the LFE performed better in certain dimensions (e.g., the labor market) than their peers who were not affected, 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 (diff-in-diff) approach for 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 that can be correlated with both causal variables and outcomes of interest. 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:

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

where Y_{ijk} is the outcome of interest of individual i , living in city j , belonging to cohort k ; α_j is a city fixed effect, β_k is a cohort fixed effect and X_i represents a vector of individual characteristics. T_i is a treatment variable, equal to 1 if the individual is young enough to have been exposed to the reform, while P_j is a measure of the program intensity in the city.

Treated age cohorts are defined in terms of the likelihood to be fully exposed to the reform (see below for further discussion on the definition of cohorts). Besides, there is substantial variability in terms of treatment intensity among the young cohorts. Differences in the timing of the implementation of the new law imply that a given cohort could have been exposed to a variable extension in mandatory education according to the city of residence. In practice, we introduce the intensity of the reform P_j through a single binary variable equal to 1 for those individuals living in a city that fully implemented the LFE (instead of gradually or not implemented), according to the classification in Table 2.

We are interested in measuring the impact of the LFE on human capital accumulation and labor market performance (our left-hand-side variables Y). As outcome indicators for education we consider years of formal education and a dummy for secondary school graduation. With respect to the labor market performance, the main outcomes considered are employment, hours worked and wages.

A. Data

Our primary source of information 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 from Argentina's twenty-four provinces listed in Table 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, small (13%). In addition, the available evidence drawn from other data sources suggests only small differences between urban and rural areas in terms of poverty and other social variables (Gasparini 2005).

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 exposure to treatment are both the city of residence⁵ and the cohort.

B. 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 that could be both affecting labor market/educational outcomes and the probability of implementing the reform. In Table 3 we present the 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 left-hand-side variable.⁶ Among the explanatory variables we consider proxies for regional GDP per capita, the Gini coefficient, the unemployment rate, population, fiscal deficit, poverty (percentage of individuals with unmet basic needs), and a political party dummy which takes the value 1 if the province is governed by the same party than the national government at the time of the reform. All these covariates were significantly different for the gradual and the full implementers, but they do not evolve differently over time between both groups.

⁵ If migration is important, the region of residence used for the estimations may be different from the region the individual was living in at the time of the reform. Internal migration is, however, relatively low in Argentina. Since the EPH has information on migration during the past five years, the estimations were replicated using the previous residence: all the results in the paper hold under this alternative.

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

Table 3. Hazard model: time of implementation

Variables	Dependent variable											
	33% polimodal implemented		90% polimodal implemented		33% EGB implemented		90% EGB implemented		33% EGB implemented		90% EGB implemented	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
GDP per capita	-0.001 [0.001]	-0.001 [0.001]	-0.002 [0.001]	0.000 [0.001]	0.000 [0.000]	-0.000 [0.001]	-0.000 [0.001]	-0.000 [0.001]	-0.000 [0.001]	-0.000 [0.001]	-0.000 [0.001]	-0.000 [0.001]
Gini coefficient	0.607 [11.691]	-15.299 [14.903]	4.650 [14.009]	-8.219 [18.726]	-28.235* [14.433]	-52.549** [23.218]	-10.541 [15.655]	-27.035 [21.443]	-10.541 [15.655]	-27.035 [21.443]	-10.541 [15.655]	-27.035 [21.443]
Unemployment rate	0.074 [0.071]	-0.031 [0.080]	0.137 [0.100]	0.243* [0.135]	0.120 [0.097]	-0.170 [0.159]	-0.067 [0.085]	-0.216* [0.120]	-0.067 [0.085]	-0.216* [0.120]	-0.067 [0.085]	-0.216* [0.120]
Political party	-0.088 [0.603]	0.341 [0.664]	1.705** [0.758]	2.349** [0.947]	2.096** [0.843]	0.509 [1.185]	2.134** [0.840]	3.019*** [1.153]	2.134** [0.840]	3.019*** [1.153]	2.134** [0.840]	3.019*** [1.153]
Population	0.005 [0.018]	0.019 [0.016]	0.005 [0.029]	-0.044 [0.037]	0.026 [0.036]	0.055 [0.035]	0.005 [0.017]	0.024 [0.026]	0.005 [0.017]	0.024 [0.026]	0.005 [0.017]	0.024 [0.026]
Fiscal deficit	-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.002]	-0.005** [0.002]	0.000 [0.002]	0.002 [0.004]	-0.002 [0.002]	-0.000 [0.003]	-0.002 [0.002]	-0.000 [0.003]	-0.002 [0.002]	-0.000 [0.003]
Time trend	1.224 [0.772]		2.690 [1.763]		1.401** [0.621]		1.662* [0.858]		1.662* [0.858]		1.662* [0.858]	
Poverty	0.024 [0.049]	0.044 [0.056]	-0.083 [0.054]	-0.080 [0.058]	0.110** [0.044]	0.125** [0.061]	0.099* [0.060]	0.128 [0.083]	0.099* [0.060]	0.128 [0.083]	0.099* [0.060]	0.128 [0.083]
Constant	-5.340 [4.925]	-16.769*** [6.182]	-8.716 [7.193]	-18.83 [0.000]	3.328 [5.839]	0.027 [10.396]	-3.284 [5.947]	-13.930 [0.000]	-3.284 [5.947]	-13.930 [0.000]	-3.284 [5.947]	-13.930 [0.000]
Year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	141	141	181	181	96	96	131	131	96	96	131	131

Notes: Each panel of this table reports a hazard model for the probability of implementing the reform under alternative definitions of "implementation". The alternative definitions differ either in the thresholds employed (i.e. percentage of schools that implemented the reform) or in the educational level considered (EGB or polimodal). The explanatory variables include the following time-varying covariates at the provincial level: GDP per capita, Gini coefficient, unemployment rate, population, fiscal deficit, poverty (percentage of individuals with unmet basic needs), and a political party dummy which takes the value 1 if the province is governed by the same party than the national government at the time of the reform. Source: own calculations based on microdata from EPH (INDEC).

The only variable that is significant in most of the specifications is the political party, which means that a province was more likely to implement the LFE if its ruling party was the same as the national one. Given this situation, we control for this variable in our estimations. As mentioned above, 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 that could be also affecting our outcomes of interest.

Table 4 performs some checks in order to support our identification strategy. Based on individuals' ages and region of residence we split our sample according to exposure to the reform.

Table 4. Double differences between groups in years of education

Panel A. Experiment of interest			
	Intensive	Non-intensive	Difference
Young	10.183	10.500	-0.317
	[0.001]	[0.001]	[0.044]
Old	10.991	12.218	-1.227
	[0.001]	[0.001]	[0.063]
Difference	-0.809	-1.718	0.910
	[0.043]	[0.054]	[0.077]
Young= 8, 9, 10, 11, 12, Old=14, 15, 16, 17, 18.			
Panel B. Control experiment 1			
	Intensive	Non-intensive	Difference
Young	9.425	10.452	-1.026
	[0.001]	[0.001]	[0.074]
Old	9.118	10.217	-1.098
	[0.001]	[0.001]	[0.082]
Difference	0.307	0.235	0.072
	[0.055]	[0.079]	[0.110]
Young= 19, 20, 21, 22, 23, Old=24, 25, 26, 27, 28.			
Panel C. Control experiment 2			
	Intensive	Non-Intensive	Difference
Young	9.849	11.076	-1.227
	[0.001]	[0.001]	[0.086]
Old	9.490	10.515	-1.026
	[0.001]	[0.001]	[0.096]
Difference	0.360	0.561	-0.202
	[0.066]	[0.089]	[0.129]
Young= 16, 17, 18; Old=20, 21, 22.			

Notes: Each panel reports for different regions ("Intensive" or "Non-intensive") and cohorts ("Young" or "Old") the average number of years of education for the subsample of males. "Intensive" and "Non-intensive" refer to whether the provinces implemented massively the reform or not, respectively. In panel A, "Young" denotes cohorts that were young enough to be exposed to the reform (8 to 12), whereas "Old" denotes cohorts that were slightly older, and hence not exposed (14 to 18). In Panel B and C, both cohort groups (young and old) are comprised by people not exposed to the educational reform. The numbers in bold in each panel correspond to simple difference-in-difference estimates. Standard errors in brackets.

In Panel A we examine the simple difference in years of education between provinces that implemented massively vs. the ones that did not, for the subsample of males. While young cohorts (ages 8 to 12) are the ones exposed to the new law, old cohorts (ages 14 to 18) are not, since they were born before they could be affected by the education reform. The double difference between these two groups amounts to 0.91 years of education and is statistically significant.

To contribute to the assessment of the likelihood of our identification assumption, in panels B and C we run false experiments or *placebos*, in which we evaluate the program impact over age cohorts that were not affected by the program. In both panels, both groups (young and old) are comprised by people not exposed to the educational reform. The double difference in both panels is not statistically significant, which supports our claim that our results are driven by the reform under study and not by other factors.

IV. The results

We carry out the estimations using several samples and different cohorts' definitions. Since Argentina has a high rate of individuals who graduate at a later age, we build the cohorts using several age ranges for robustness (see Table 5).⁷ While cohort A includes a broader age range, cohort C considers that all individuals graduate on time.

The number of observations under the definition A of cohorts is 60,825, while it drops to 48,486 for definition B and 36,522 for definition C. The number of observations used for the estimations slightly decreases due to missing variables for some individuals (see Tables 6 and 7 below). Table A1 in the Appendix reports the mean values for the set of variables used in the empirical analysis.

We carry out the analysis for four samples: all individuals, males, poor, and poor males. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. We performed the estimations using Unmet Basic Needs as definition of poverty and the results hold.

⁷ We define the cohorts with different age ranges due to two reasons. First, we cannot observe the exact date of birth of the individual, so we do not know the exact age at which she should have entered school. Secondly, we cannot observe grade promotion/repetition, so our measure of exposure has some noise.

As left-hand-side variables, we consider two measures of educational attainment—years of education and a dummy for complete high school—and a set of labor variables related to the labor market—employment (measured by a dummy of being employed), weekly hours worked and labor income (log of hourly wages).

Table 5. Cohort definitions

	Age
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

Notes: Age in 1996. For each cohort definition, “Young” refers to the cohorts exposed to the reform, whereas “Old” refers to control cohorts.

A. Educational outcomes

The impact of increasing mandatory years of schooling on actual years of formal education may not be straightforward. While the extension in the number of years is mandatory and punished by law, such laws are difficult to enforce (Angrist and Krueger 1991). That is especially true in a context of high labor informality and credit constraints to the access to education. Poor individuals may be secluded into working in the informal sector, where returns to education are usually smaller, and proof of mandatory schooling is not required. Also, if credit markets have frictions, individuals may not go to school even when is compulsory.

Table 6 shows the results of the impact of the reform on educational variables. As explained above, the effect is captured by the interaction of a dummy identifying the “young cohorts” (i.e., those individuals young enough to be exposed to the reform) with a dummy variable that measures the intensity of the reform (=1 for those individuals living in a city that massively implemented the LFE). Besides the

typical set of controls including socioeconomic and demographic characteristics, we also include political party in the regressions, given its significance in the hazard models of Table 3. Standard errors shown in the table are clustered at the province level. We report the results using different cohort definitions according to Table 5, and different samples. Results vary more across samples than across definitions of cohort.

Table 6. Impact of educational reform on educational outcomes

	Years of education			Complete high school		
	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
All	0.895*** [0.211]	0.920*** [0.235]	0.756*** [0.132]	0.024 [0.015]	0.027* [0.015]	0.035*** [0.013]
Observations	59449	47339	35850	60002	47799	36089
Males	0.838*** [0.195]	0.887*** [0.248]	0.704*** [0.127]	0.029 [0.019]	0.035* [0.020]	0.037** [0.018]
Observations	29128	23213	17693	29432	23466	17824
All Poor	0.779*** [0.198]	0.859*** [0.227]	0.575*** [0.122]	0.078*** [0.021]	0.087*** [0.025]	0.083*** [0.017]
Observations	32485	26002	19065	32852	26304	19230
Poor males	0.856*** [0.198]	0.924*** [0.236]	0.614*** [0.129]	0.083*** [0.022]	0.095*** [0.030]	0.093*** [0.019]
Observations	15521	12446	9085	15722	12612	9176

Notes: Standard errors clustered at the province level in brackets. * significant at 10%, ** significant at 5%, ***significant at 1%. This table reports the reduced form estimation of the impact of the reform on educational outcomes (i.e., the parameter γ of equation (1)) for alternative samples and cohorts definitions. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 5. The educational outcomes include: (a) years of formal education, and (b) a dummy variable that equals 1 if the individual completed high school education.

The LFE seems to have had a significant effect on some basic school enrollment outcomes. The coefficients of the treatment variable in the regressions for years of education are positive and significant for all samples and cohort definitions. Youths fully exposed to the LFE ended up with more years of education than those not fully exposed to the reform. Coefficients range from 0.58 to 0.92 extra

years of education as a result of the reform. Most 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.⁸

The increase in years of education is somewhat larger for the sample of all people than for the poor. One possibility behind this result is that the reform caused 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 after high school.

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 Latin American countries, high-school drop-out rates are higher for men than for women. CEDLAS (2012) reports that in 2006 while 84% of females in secondary school age are attending that educational level, the share for males is 78%.

B. Labor market outcomes

The educational reform under analysis had mainly the objective of facilitating permanence of young individuals within the schooling system. By achieving higher levels of education it was believed that labor market perspectives would improve. We find that the results of the reform in terms of labor market outcomes are mostly positive and statistically significant (Table 7). Youths fully exposed to the reform when they were teenagers have now higher probability of being employed, work more hours and earn higher wages. The probability of employment in the sample of all individuals increases between 4.3% and 7.2%.

⁸ The larger effect in the group of poor youths compared to the rest is mainly driven by the fact that high school graduation rates in this group are substantially lower.

Table 7. Impact of educational reform on labor outcomes

	Employed			Hours worked			Labor income		
	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
	All	0.060** [0.028]	0.072** [0.034]	0.043** [0.019]	2.980*** [1.118]	3.181** [1.318]	1.997*** [0.747]	0.157*** [0.042]	0.169*** [0.038]
Observations	60006	47803	36089	60006	47803	36089	15705	12391	10428
Males	0.056* [0.030]	0.063* [0.036]	0.055** [0.023]	3.074** [1.266]	3.168** [1.486]	3.371*** [1.222]	0.205*** [0.034]	0.230*** [0.038]	0.218*** [0.050]
Observations	29434	23468	17823	29434	23468	17823	9742	7692	6474
All Poor	0.025 [0.026]	0.022 [0.028]	0.029 [0.018]	0.774 [1.021]	0.712 [1.143]	0.087 [1.044]	0.015 [0.047]	0.051 [0.053]	0.051 [0.066]
Observations	32854	26306	19230	32854	26306	19230	9151	7150	6098
Poor males	0.031 [0.033]	0.019 [0.034]	0.043 [0.035]	1.366 [1.442]	1.142 [1.642]	0.768 [1.800]	0.064 [0.046]	0.129*** [0.043]	0.065 [0.053]
Observations	15724	12614	9176	15724	12614	9176	5941	4666	3928

Notes: Standard errors clustered at the province level in brackets. * significant at 10%, ** significant at 5%, *** significant at 1%. This table reports the reduced form estimation of the impact of the reform on labor market outcomes (i.e., the parameter γ of equation (11)) for alternative samples and cohorts definitions. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 5. The labor market outcomes include: (a) a dummy variable that equals 1 if the individual was employed, (b) weekly hours worked, and (c) labor income (log of hourly wages).

The effect for poor individuals and poor males is also positive, but not statistically significant at the conventional levels. The same pattern applies to hours worked: while in the entire sample hours worked per week increased between 2 and 3 hours,⁹ the effect for poor individuals is negligible and not statistically significant. Labor incomes for treated youths are around 16% higher than for their non-treated counterparts. The impact is higher for the sample of males, but almost completely vanishes in the sample of poor youths. The reform seems to have had no effect on the labor outcomes of income-deprived people.

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. In contrast, the impact of the reform on the labor outcomes of poor youths turns out to be almost null. Poor teenagers fully exposed to the reform apparently did not experience improvements in their labor outcomes, compared to their counterparts in the control group.

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. In contrast, the gains were larger for the non-poor given the types of jobs that these people are more likely to hold (e.g., civil servants).

C. Returns to education

Our identification assumption allows us to estimate the impact of the program; if in addition we assume that the effect of the program on wages comes only through higher educational attainment, we can use the program to construct instrumental

⁹ For an average working week of 35 hours, the increase in hours amounts from 5.7% to 8.5%.

variables to estimate the impact of additional years of education on wages (Duflo 2001). In reality, the reform could have had an impact on both the quantity and the quality of education, and hence wages could have been affected through both channels. The impact of the education reform on quality has been studied by Galiani et al. (2007) and Bet (2008). The evidence is mixed: while the first paper shows that quality increased (decreased) in richer (poorer) districts, the second paper shows no change in quality as measured by standardized tests, other than an almost negligible improvement in Spanish scores. In what follows we assume that the change in quality was on average very small, so we can interpret our estimations as returns to education.

In our estimations we use equation (1) as a first stage in a Two Stages Least Squares estimation of the returns to education. We use the instrumented years of education in equation (2):

$$w_{ijk} = A + \phi_j + \beta_k + \delta X_i + \theta S_i + \eta_{ijk}, \quad (2)$$

where w_{ijk} are log of hourly wages of individual i of city j of cohort k , X_i are individual characteristics, S_i are schooling years and η_{ijk} is the error term. Returns to education are presented in Table 8.

Point estimates for the whole sample are between 15.8% and 17.6%; they increase to 20.4% to 24.1% in the case of males. These results seem high but are in line with the ones estimated by Lopez Bóo (2010) for Argentina. Consistent with our previous findings, the returns are much lower for the group of poor individuals. The substantial difference between returns for poor and non-poor can be explained by the fact that still the majority of poor individuals do not finish high school and are severely limited to find a job in the formal labor market, where wages are higher.

Table 8. Returns to education

	Cohort A	Cohort B	Cohort C
All	0.158*** [0.042]	0.169*** [0.047]	0.176*** [0.056]
Observations	15657	12359	10396
Males	0.204*** [0.045]	0.238*** [0.058]	0.241*** [0.069]
Observations	9709	7672	6450
All Poor	0.013 [0.049]	0.048 [0.050]	0.053 [0.093]
Observations	9116	7126	6075
Poor males	0.074 [0.063]	0.149*** [0.057]	0.088 [0.085]
Observations	5917	4652	3910

Notes: * significant at 10%, ** significant at 5%, ***significant at 1%. This table reports the implicit returns to education under the assumption that the reform only affected hourly wages by increasing the years of formal education (i.e., parameter θ of equation (2)). This was estimated with Two Stages Least Squares, using the models estimated in table 6 as the first stage for each sample and cohort definition. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 5. Standard errors clustered at the province level in brackets.

V. Concluding remarks

High dropout rates in developing countries have 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.

In spite of the heated debate about the educational reform in Argentina, there has not been solid evidence on its causal effect over educational and labor market outcomes. This paper contributes to the measurement of the impact of the reform, by taking advantage of the variation in the implementation of the reform across provinces. Using a diff-in-diff methodology, we show the effect of the reform on several educational and labor outcomes. We also perform some robustness checks to argue that our estimates can have a causal interpretation.

When we look at the complete sample of individuals affected by the reform, our results suggest positive effects in some educational outcomes (years of education and high school completion) and labor outcomes (employment, hours and wages).

Results also hold for the sub-sample of males. The same can be said for returns to education, which are high, but in line with previous literature for Argentina. However, the reform seems to have been only partially successful, as the impact on labor outcomes for the poor was almost null, possibly as a consequence of a very limited access of poor youths to jobs with high returns to education.

Appendix

Table A1. Descriptive statistics

Variables	Exposure to treatment			
	Non-Intensive		Intensive	
	young	old	young	old
Years of education	10.794 [2.268]	12.628 [3.13]	10.436 [2.431]	11.319 [3.434]
Incomplete high school	0.48 [0.5]	0.153 [0.36]	0.474 [0.499]	0.189 [0.392]
High school graduate	0.112 [0.316]	0.206 [0.404]	0.151 [0.358]	0.224 [0.417]
Some college	0.315 [0.464]	0.409 [0.492]	0.233 [0.423]	0.283 [0.451]
College graduate	0.007 [0.081]	0.129 [0.335]	0.005 [0.073]	0.079 [0.27]
log(labor income)	5.712 [0.951]	6.304 [0.904]	5.734 [0.938]	6.161 [0.84]
log(wages)	0.931 [0.839]	1.229 [0.8]	0.893 [0.754]	1.107 [0.726]
Hours worked	8.59 [17.323]	25.693 [23.977]	9.874 [18.986]	23.94 [24.462]
Employed	0.259 [0.438]	0.653 [0.476]	0.277 [0.448]	0.611 [0.488]
Male	0.498 [0.5]	0.479 [0.5]	0.495 [0.5]	0.483 [0.5]
Household head	0.04 [0.196]	0.199 [0.399]	0.044 [0.205]	0.193 [0.394]
Spouse	0.026 [0.161]	0.144 [0.351]	0.036 [0.187]	0.171 [0.376]
Children	0.802 [0.399]	0.523 [0.499]	0.793 [0.405]	0.524 [0.499]
Number of people employed in the household	1.678 [1.048]	1.393 [1.093]	1.652 [1.086]	1.37 [1.089]
Household members	4.997 [2.19]	4.181 [2.209]	5.12 [2.271]	4.371 [2.093]
Maximum years of education (head/spouse)	18.668 [4.987]	13.14 [9.495]	18.388 [5.445]	12.734 [9.619]

Note: definition of young and old cohort according to criterion A (see Table 5). Standard errors in brackets. Source: own calculations based on microdata from EPH (INDEC).

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