Alzúa and Velázquez *IZA Journal of Development and Migration* (2017) 7:7
DOI 10.1186/s40176-017-0100-8

IZA Journal of Development and Migration

ORIGINAL ARTICLE Open Access

The effect of education on teenage fertility: causal evidence for Argentina

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Abstract

This is the first study exploring the causal effect of education on teenage fertility in Argentina. We exploit an exogenous variation in education from the staggered implementation of the 1993 reform, which increased compulsory schooling from 7 to 10 years. We find a negative overall impact of education on teenage fertility rates, which operates through two complementing channels: a human capital effect (one additional year of schooling causes a decline of 30 births per 1000 girls) and a weaker 'incapacitation' effect (a rise of one percentage point in enrollment rate reduces 3 births per 1000 girls).

Keywords: Teenage fertility, Education, Instrumental variables, Compulsory schooling laws, Latin America and the Caribbean, Argentina

JEL Classification: I25, I28, J13, J16

1 Introduction

Early motherhood represents a major challenge for policymakers in those countries committed to the Millennium Development Goals (Jiménez et al. 2011; Williamson 2013). In particular, Latin America and the Caribbean (LAC) is the second region (following Africa) with the highest teenage fertility rate in the world, with 68 births per 1000 women between the ages of 15 and 19 (United Nations 2013). While in most LAC countries teenage fertility rates declined in the last decade, in Argentina, rates increased sharply since 2003 as indicated in Fig. 1. Data also indicate high heterogeneity across provinces: rates for Argentina's northeast region are nearly equal to those in Sub-Saharan Africa, as it can be observed in Fig. 2.

Teenage pregnancy is associated with several adverse consequences for child health (Azevedo et al. 2012). These risks include low birth weight, pre-term delivery, and neonatal and infant mortality. These consequences are more severe when the mother is young (14 years of age or less). Table 1 shows these effects for Argentina, according to the age of the mother.

Early childbearing also corresponds to adverse intra-generational socioeconomic consequences for the mothers (lower educational achievement and poorer labor market outcomes) and inter-generational negative socioeconomic consequences for the child (engagement in risky behaviors). In addition, being born to a teenage mother is associated with higher risk of a teenage birth (binding inter-generational poverty traps). Beyond the individual costs associated with the phenomenon, teenage pregnancy has a

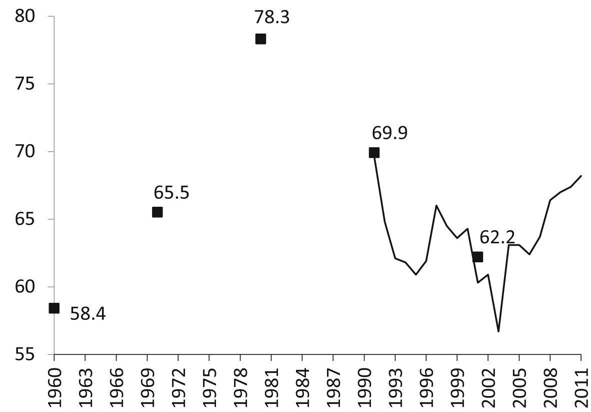

Fig. 1 Teenage fertility rate. Argentina, 1960–2011 (number of live births per 1000 women aged 15–19). Source: Data for 1960, 1970, 1980, 1991, and 2001 from Pantelides and Binstock (2007). The *solid line* corresponds to information provided by Office of Health Statistics DEIS

considerable public cost: there are significant health and welfare costs, as teen mothers are more likely to participate in social programs and become dependent on social assistance income (Azevedo et al. 2012).

Education is a key determinant of fertility choices[1], and its effects may occur through different channels. The first causal channel relies on human capital theory and understands education as an investment for the future: education raises future earnings and, ultimately, the opportunity cost of early childbearing (Becker 1960, 1981).[2] The negative effect of female education on fertility could be stronger under positive assortative mating (Behrman and Rosenzweig 2002).[3] Second, education may also operate through a delay of first births during the teenage years[4] through a pure 'incapacitation' or 'incarceration' effect: keeping teenagers in school, under adult supervision, limits their time/opportunities to engage in risky behavior like unprotected sex. Such birth postponement may also be related to the role incompatibility of enrollment in the educational system and motherhood (Black et al. 2008).

Human capital and incapacitation effects are by no means the only channels by which education could affect fertility. Education not only enhances women's knowledge about contraception and reproductive health (via curricula) but also teaches reasoning skills which foster knowledge after leaving school, i.e., education is associated with higher productivity in the production of health (Grossman 1972). Furthermore, the experience of going to school provides women with greater confidence and skills in accessing modern institutions, including the health care system and family planning services. Education also serves as a socialization process that shapes attitudes, values, and aspirations, providing greater awareness of alternative lifestyles. Schooling may empower women's sense of control over their body and destiny by giving them greater autonomy in domestic decision-making (including the use of contraception) and increasing reliance on science and technology (Cleland 2002).

Although a vast empirical literature shows a negative relationship between female education and early fertility (for international references, see Azevedo et al. 2012; for LAC countries, see Flórez and Núñez 2001; for Argentina, see Fig. 3), it is difficult to establish a causal relationship due to endogeneity problems. Human capital accumulation and reproductive decisions are either joint decisions, which result in a potential

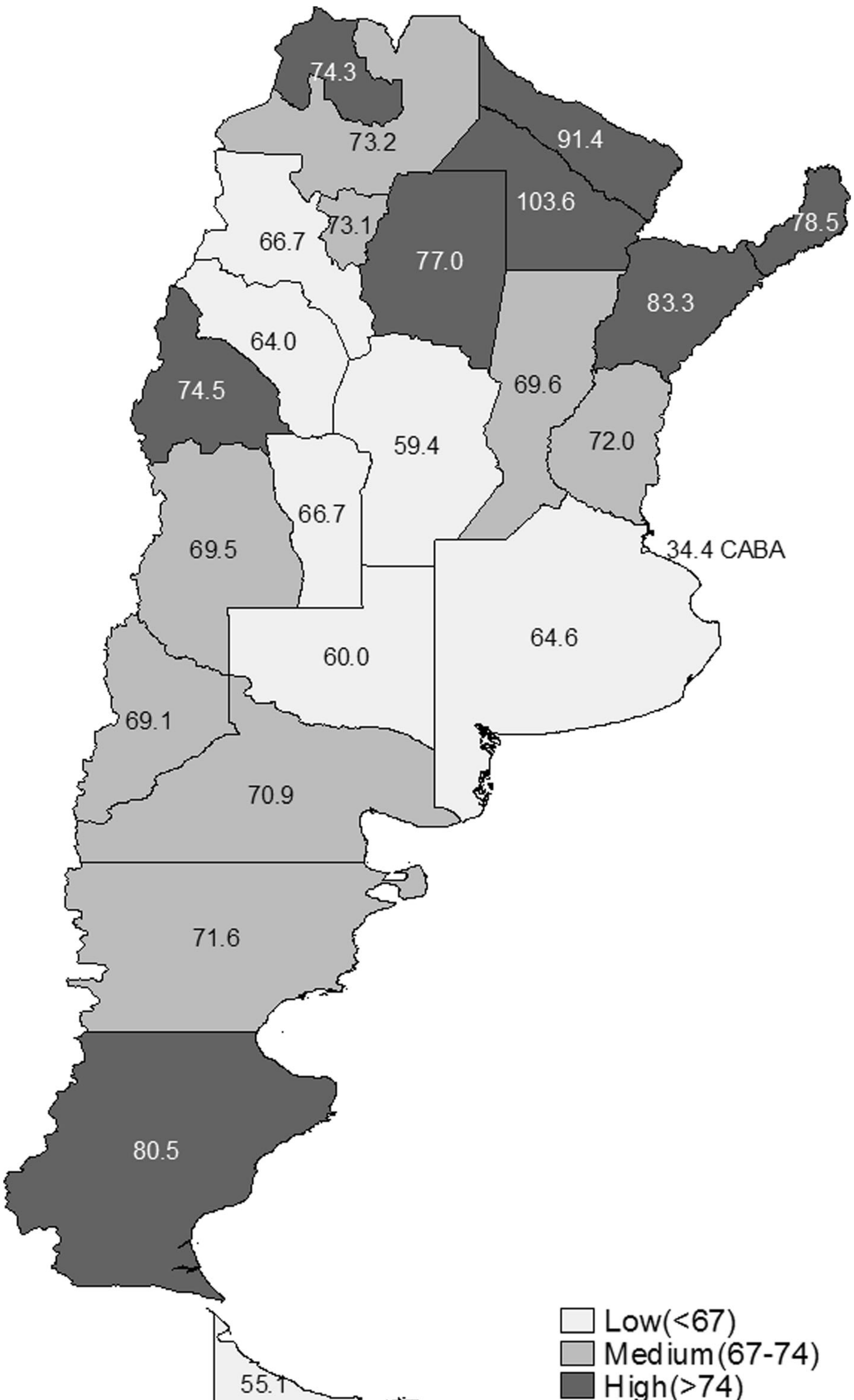

Fig. 2 Teenage fertility rates by province. Argentina, 2011 (number of live births per 1000 women aged 15-19). Note: *CABA* City of Buenos Aires (Federal District). Source: Office of Health Statistics DEIS; own calculations

Table 1 Health and demographic indicators by age of the mother. Argentina, 2012

	Mother's age (years)				Total
	10–14	15–19	20–34	35–49	10–49
Gestational length (weeks)	38.27 (0.044)	38.59 (0.006)	38.62 (0.003)	38.35 (0.006)	38.58 (0.002)
Premature (<37 weeks gestation)[a]	12.73 (0.61)	9.28 (0.09)	7.89 (0.04)	10.31 (0.09)	8.49 (0.03)
Birth weight (grams)	3088 (10.58)	3202 (1.66)	3289 (0.78)	3257 (1.81)	3270 (0.66)
Very low birth weight (<1500 g)[a]	2.17 (0.27)	1.35 (0.03)	0.98 (0.01)	1.37 (0.03)	1.10 (0.01)
Low birth weight (1,500 - 2,499 grams)[a]	9.27 (0.53)	6.78 (0.08)	5.60 (0.03)	7.24 (0.08)	6.04 (0.03)
Infant mortality rate (<1 year)[b]	15.56 (2.25)	9.72 (0.29)	6.61 (0.11)	7.04 (0.25)	7.19 (0.1)
Neonatal mortality rate (<28 days)[b]	9.93 (1.8)	6.56 (0.24)	4.54 (0.09)	5.01 (0.21)	4.94 (0.08)
Maternal mortality rate[c]	3.31 (3.31)	2.97 (0.52)	3.00 (0.24)	6.58 (0.77)	3.54 (0.22)
Live births	3020	111,272	502,625	110,872	727,789

Notes: standard error of mean in parentheses. Source: *Estadísticas Vitales* 2012 (DEIS); own calculations
[a]Per 100 live births
[b]Per 1000 live births
[c]Per 10,000 live births

reverse causality problem or are both affected by unobservable factors, causing selection bias.

Some recent studies used different methodological approaches to overcome selection bias and reverse causality problems, thus isolating the 'pure' effect of education on teenage fertility. Experimental or quasi-experimental research designs have exploited policies that reduce schooling costs (direct and opportunity costs) and increase enrollment (i.e., Conditional Cash Transfer programs and enrollment subsidies), differences in age-at-school-entry policies, reforms that extended the length of the school day, and reforms that extended compulsory schooling. Empirical evidence for OECD countries (Black et al. 2008; Silles 2011; Cygan-Rehm and Maeder 2013)[5] and Africa (Baird et al. 2011; Duflo et al. 2015) supports the hypothesis that education reduces fertility among youth. However, evidence for LAC countries is mixed. On the one hand, the cross-country analysis of Alzúa et al. (2016) based on 22 LAC countries[6] finds that education has no impact on teenage fertility for the region. On the other hand, there is evidence that education negatively affects teenage fertility for Colombia, Chile, and the Dominican Republic (Cortés et al. 2010, 2016; Berthelon and Kruger 2011; and Novella and Ripani 2016).

The evidence gap for LAC countries may be attributed to the country-specific factors analyzed by Cortés et al. (2010, 2016) for Colombia, Berthelon and Kruger (2011) for

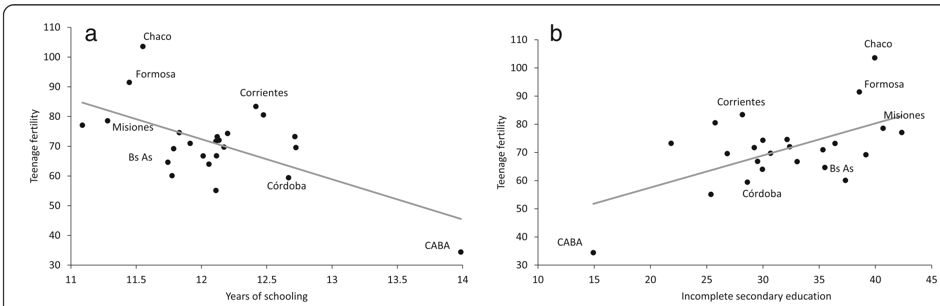

Fig. 3 Teenage fertility rate (number of live births per 1000 women aged 15–19) and education (women aged 25–40), by province. Argentina, 2011. **a** Years of schooling (mean). **b** Incomplete secondary education (%). Note: *CABA* City of Buenos Aires (Federal District). Source: Office of Health Statistics DEIS and Argentina Household Survey EPH - SEDLAC (CEDLAS and The World Bank); own calculations

Chile, Novella and Ripani (2016) for the Dominican Republic, and Alzúa et al. (2016) for the entire LAC region. Also, due to methodological differences, the impacts they intend to capture are different. While Alzúa et al. (2016) and Cortés et al. (2010, 2016) try to capture the impact of an increase in years of education, Berthelon and Kruger (2011) capture the impact of an increase in the length of school day and Novella and Ripani (2016) capture the impact of non-formal education (from a job training program). Although both Cortés et al. (2010, 2016) and Alzúa et al. (2016) attempt to estimate the impact of years of schooling, they exploit different sources of exogenous variation in education with different levels of compliances. In Cortés et al. (2010, 2016), compliance is very high because they exploit a CCT program for which the cash transfer is conditional on school attendance (and academic performance); Alzúa et al. (2016) rely on mandatory schooling laws, which are difficult to enforce (particularly in economies with high informality, such as LAC countries).

This paper provides empirical evidence on the impact of education on teenage fertility for Argentina and helps to shed light on the conflicting evidence for LAC. We exploit a natural experiment: an education reform (*Ley Federal de Educación*, 1993) that, among several features, increased compulsory schooling from 7 to 10 years. Although it was a national reform, its actual implementation was driven by political reasons and varied substantially across provinces (Alzúa et al. 2015). Differences in the timing and degree of implementation provide a source of identification for unraveling the causal effect of education on teenage fertility using an Instrumental Variables approach. To the best of our knowledge, this is the first study exploring the case of Argentina.

We use an annual panel dataset at the birth-cohort/province level for the period 1995-2006. Results provide evidence for a positive impact of education reform on educational outcomes (*first-stage* relationship). In fact, the implementation of the reform (extensive margin) had a statistically significant and positive effect range, from 0.24 to 0.27 additional years of schooling. It also produced an increase in school enrollment rates in the range of 2.6 to 3 percentage points. However, the reform's progress and expansion (intensive margin) showed no impact on human capital or enrollment.

We find evidence for a statistically significant negative overall impact of education on the fertility decisions of teenagers. This overall effect is found to operate through two complementing education channels: a human capital effect (one additional year of schooling reduces teenage fertility rate by roughly 26.9 to 35.5 per thousand points) and a weaker 'incapacitation' effect (a rise of one percentage point in enrollment rate reduces the teenage fertility rate by roughly 2.4 to 3.3 per thousand points). Crosta (2007) found that the 1993 education reform reduced repetition rates, which may explain the weak 'incapacitation' effect (similar to the case of Malawi; Grant 2015).

Our results are in line with those reported by Cortés et al. (2010, 2016), Berthelon and Kruger (2011), and Novella and Ripani (2016), but they contradict recent evidence from Alzúa et al. (2016). The conflicting evidence may be due to Argentina's specific characteristics, which are different from the rest of the region. It may also be due to a delay between the time congresses passed the educational law and its actual implementation. While we exploit the effective implementation of the education reform, Alzúa et al. (2016) rely on laws passed in different years across different countries. Although the estimated effects are very large, we should interpret the results as a local average treatment effect (LATE) for the group complying with the reform (i.e., for young people who did not leave school after 7 years because of the reform). This group is not necessarily representative of the overall population.

The rest of the paper is organized as follows. Section 2 presents a description of the compulsory schooling changes used for identification, describes the data used in this study, and lays out the methodology. Section 3 presents the main findings and Section 4 concludes.

2 Empirical strategy

We identify the causal effect of education on fertility by applying an Instrumental Variables approach to deal with the endogeneity of education. Following Black et al. (2008), Silles (2011), Cygan-Rehm and Maeder (2013), and Alzúa et al. (2016), we use an education reform that extended the number of years of compulsory schooling in Argentina (*Ley Federal de Educación*) as an instrument for education. Our identification strategy takes advantage of an exogenous variation in education generated from the staggered implementation of the reform, which was driven by political reasons uncorrelated with fertility trends.

2.1 The educational reform

Passed into law in April 1993, the *Ley Federal de Educación* (henceforth LFE)[7] provided the legislative framework for an increase in the number of years of compulsory schooling, from 7 to 10, in Argentina. It also introduced a significant change in the structure of the educational curricula. Specifically, it replaced 7 years of primary school and 5 years of secondary school with a 9-year uniform cycle called the *Educación General Básica* (EGB) and a 3-year specialized cycle named *Polimodal*. Pre-primary education for children aged five and EGB were made mandatory. The law applies to both public and private schools in every province. Table 2 shows the structure of the educational system before and after the reform. Table 2 also shows how the change in mandatory schooling affects teenagers aged 14 or younger, showing the age at which children are supposed to reach each level. However, in Argentina, the over-age rates are quite high (18.8% in primary level and 38.1% in secondary education, according to DINIECE 2011).

One of the main goals of the LFE was to reduce the high dropout rate in the initial years of secondary education. Indeed, implementation of the reform increased access to secondary education and reduced dropout and repetition rates (Crosta 2007). In addition, LFE had other unintended effects: a positive impact on labor outcomes (Alzúa et al. 2015) and a reduction in youth crime (López 2012).

As preliminary evidence of the effects of LFE reform on education, Fig. 4 visually represents the effect that this legislation had on increasing average years of schooling in formal education (panel A) and school enrollment (panel B). Enrollment rates responded faster than years of schooling. The average number of years of education for youths aged 10–14 has increased over the decade, from 5.4 years in 1995 to about 5.9 years in 2005. For older teenagers, aged 15–19 years, average years of schooling increased from 9.2 in 1995 to 9.9 in 2005. Enrollment rates for youths

Table 2 Educational structure before and after the reform

Before LFE		Age	After LFE	
Level	Year		Level	Year
Pre-primary	1º	3	Pre-primary	1º
	2º	4		2º
	3º	5		3º
Primary	1º	6	EGB 1	1º
	2º	7		2º
	3º	8		3º
	4º	9	EGB 2	4º
	5º	10		5º
	6º	11		6º
	7º	12	EGB 3	7º
Secondary	1º	13		8º
	2º	14		9º
	3º	15	Polimodal	1º
	4º	16		2º
	5º	17		3º

Notes: Areas shaded in gray indicate compulsory schooling. Source: DINIECE, Ministry of Education
EGB Educación General Básica

aged 10–14 neared 100%, increasing from 96% in 1995 to 98% in 2001. For youths aged 15–19, after LFE school enrollment increased by 13 percentage points, up from the pre-reform level of 63%.

Since 1991, primary and secondary public education are administered and financed at the provincial level (previously secondary schools were in the hands of the federal government). It was expected that the implementation of the reform would increase pressure on educational facilities. Provincial governments required larger budgets for investing in infrastructure and teachers' wages, i.e., provinces required federal government cash transfers. For that reason, provinces were more likely to implement the reform early and massively if its governing political party was the same as the national governing party (Alzúa et al. 2015).

The timing and degree of the implementation of the law differed substantially across provinces (see Table 3). Between the years 1996 and 2000, provincial governments triggered the education reform. While in some provinces the reform was quickly and massively implemented, in others, the changes were put into practice more gradually, involving a much smaller percentage of schools and students (pilot program). Moreover, in two districts (City of Buenos Aires and Río Negro), the reform was never implemented. Furthermore, the reform was applied gradually as cohorts reached the age of EGB3 entry (12 years old). Thus, it took a considerable time for the *Polimodal* to be implemented (Crosta 2007). For all of these reasons, exposure to the reform depended on birth-cohort and province of residence.

2.2 Data

Since exposure to the reform depended on age and province of residence, we construct a yearly panel dataset at birth-cohort/province level for the period 1995–2006. In 2007, a new education reform (Law 26206) aimed to return to the old structure of the educational curricula and raise the age of compulsory schooling again (from 10 to 13 years); we decided to exclude this period from the analysis.

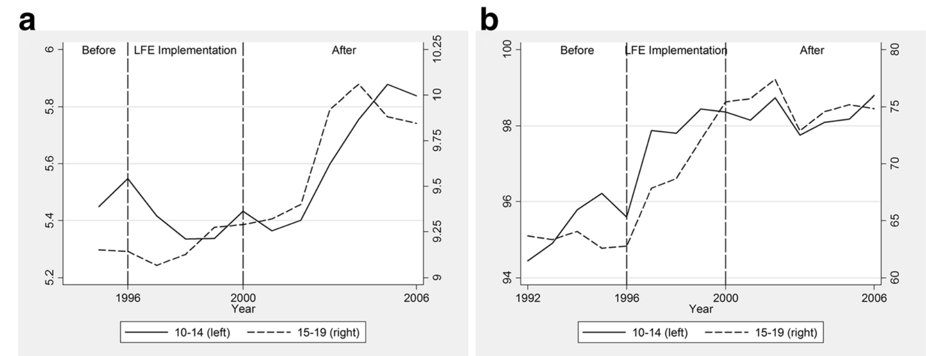

Fig. 4 Argentina, 1995–2006. **a** Years of schooling (mean). Argentina, 1992–2006. **b** Gross enrollment rates by age (%). Notes: **a** Years of schooling in formal education available since 1995; **b** share of a given population attending any educational level. LFE: *Ley Federal de Educación* (Law 24195). Source: Argentina Household Survey EPH - SEDLAC (CEDLAS and The World Bank); own calculations

We assume that most women who become pregnant during a given school year (March–December) t, will give birth the following year $t + 1$. Hence, fertility outcomes, such as births, reflect choices taken a year previous. Birth-cohorts run from 1977 to 1994, but they are included only for the teenage years (12–18 years old).

Teenage fertility rates are the number of live births[8] per 1000 girls. We include in the analysis fertility information for girls ages 13–19.[9] The number of live births by age and the province of residence of the mother were provided by the Office of Health Statistics DEIS. The National Statistics Institute INDEC provided data on female population, available by province of residence and 5-year age groups (we assigned the data into single years of age using Sprague's multipliers; Siegel and Swanson 2004).

In addition, we use two different measures for education: average years of schooling (as a proxy of human capital effect) and enrollment rates (as a proxy of 'incapacitation' effect). Unfortunately, we cannot capture the other mechanisms we briefly discussed above. Education outcomes by sex, age, and province of residence were calculated from the Argentina Household Survey EPH using SEDLAC database (CEDLAS and the World Bank). The survey sample includes 15 provinces for the entire period and 8 provinces since 1998 (see Table 4 for details). Unfortunately, for the period 1995–2006, EPH has no information from the province of Río Negro, where the reform was never implemented.

Two complementary indicators measure the implementation of the reform. First, we use Crosta (2007) as a source for the timing of the reform in provinces and we assigned compulsory attendance laws defining a dummy variable on the basis of province of residence and the year when the girl was 14 years old (see Table 4 and Fig. 7 in the Appendix). Second, we use information from the DINIECE (Ministry of Education) to calculate the share of students in *Polimodal* for each province (following Alzúa et al. 2015; and López 2012). This second indicator captures (i) differences in school construction rates and/or percentage of schools that implemented the reform across provinces; (ii) the fact that the reform was applied gradually, as cohorts reached the age of 12 (the age of EGB3 entry), with considerable additional time to reach *Polimodal*. We will interpret the dummy variable as the extensive margin of the reform, and the share of students in *Polimodal* as an indicator of the reform's progress and expansion (intensive margin).

Table 3 Year of implementation of LFE by province

Province	Year
Buenos Aires	1996
CABA	NI
Catamarca	1999
Chaco	1997
Chubut	1999
Córdoba	1996
Corrientes	1997
Entre Ríos	1997
Formosa	1998
Jujuy	1998
La Pampa	1997
La Rioja	1999
Mendoza	2000
Misiones	1998
Neuquén	1998
Río Negro	NI
Salta	1998
San Juan	1997
San Luis	1998
Santa Cruz	1998
Santa Fe	1997
Santiago del Estero	1998
Tucumán	1998
Tierra del Fuego	1998

Source: Crosta (2007)
NI not implement, *CABA* City of Buenos Aires (Federal District)

Finally, we use economic cycle indicators (provincial GDP and unemployment rates) and public policy indicators (public expenditure on education and health, and *Plan Nacer*[10] beneficiaries) as covariates.

In summary, we have an unbalanced panel dataset with 1764 observations at birth-cohort/province level for the period 1995–2006, with information about fertility of women ages 13–19, education outcomes for women ages 12–18, implementation of the LFE, and other covariates which capture economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries. Table 4 summarizes the indicators we will use for our estimations and their sources.

2.3 Identification strategy

For our identification strategy, the following equations are used:

$$\mathrm{FR}_{c,\,j,\,t+1} = \beta \mathrm{Educ}_{c,j,t} + \gamma X_{j,t} + \mu_{c,j} + \delta_t + \varepsilon_{c,j,t} \tag{1}$$

$$\mathrm{Educ}_{c,\,j,\,t} = \phi \mathrm{LFE}_{c,j,t} + \psi \mathrm{Polimodal}_{j,t} + \alpha X_{j,t} + \eta_{c,j} + \lambda_t + \nu_{c,j,t} \tag{2}$$

$$\mathrm{FR}_{c,\,j,\,t+1} = \xi \mathrm{LFE}_{c,j,t} + \tau \mathrm{Polimodal}_{j,t} + \pi X_{j,t} + \rho_{c,j} + \sigma_t + \omega_{c,j,t} \tag{3}$$

where $FR_{c,j,t+1}$ in Eq. 1 is the fertility rate in year $t+1$ of teenagers living in province j,

Table 4 Data

Indicator		Period		Source	Notes
Live births	By age, 13–19	1995–2006	Yearly	Office of Health Statistics DEIS	
Female population	By age, 13–19	1995–2006	1995–2006	National Statistics Institute INDEC	Forecasts based on 2001 Census. Results by five-year age group are then disaggregated into single years of age using Sprague's multipliers (Siegel and Swanson 2004)
Teenage fertility	By age, 13–19	1995–2006	Yearly	Own elaboration based on DEIS and INDEC	$\frac{\text{Live births}}{\text{Female population}} \times 1000$
Years of schooling	By age, 12–18	1995–2006	1995–2002 yearly 2003–2006 1st half	Household Survey, SEDLAC (CEDLAS and The World Bank)	Average years of schooling
School enrollment		1995–2006	1995–2002 yearly 2003–2006 1st half		$\frac{\text{Students enrolled}}{\text{Population}} \times 100$
LFE	LFE implementation (dummy variable)	1996–2000 (year of implementation)		Crosta (2007)	= 1 for the birth-cohort who were 14 years old at the time their province of residence implemented the reform and younger birth-cohorts; 0 otherwise
	% Polimodal students[a]	1998–2006	Yearly	DINIECE (Ministry of Education)	$\frac{\text{Polimodal students}}{\text{Total students}} \times 100$
Gross Regional Product[a]		1995–2006	Yearly	Statistics offices at provincial level	In million Pesos, at 1993 constant prices
Unemployment[a]		1995–2006	1995–2002 yearly 2003–2006 1st half	Household Survey, SEDLAC (CEDLAS and The World Bank)	Unemployment rate (>15 years old)
Public expenditure[a]	Education Health	1995–2006	Yearly	Ministry of Economy MECON	In million Pesos, at 1993 constant prices (implicit price deflator for GDP)
Plan Nacer[a]	Women and children	2004–2006	Yearly	*Plan Nacer* (Ministry of Health)	$\frac{\text{Beneficiaries}}{\text{Population}} \times 100$
Provinces					
1995–2006: Buenos Aires, CABA, Chubut, Cordoba, Entre Rios, Jujuy, La Pampa, Neuquen, Salta, San Juan, San Luis, Santa Cruz, Santa Fe, Santiago del Estero and Tierra del Fuego. 1998–2006: Catamarca, Chaco, Corrientes, Formosa, La Rioja, Mendoza, Misiones and Tucuman.					

Source: own elaboration
[a]Indicators vary only at the provincial level. The rest of indicators vary at birth-cohort/province level

belonging to birth-cohort *c.* $Educ_{c,j,t}$ represents two different measures of teenagers' educational attainment in year t: average years of schooling and enrollment rate. $X_{j,t}$ represents other covariates (economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries); δ_t is a set of dummy variables indicating the year (to control for aggregate shocks); $\mu_{c,j}$ is a set of dummy variables indicating birth-cohort/province fixed effects; and $\varepsilon_{c,j,t}$ standard errors clustered at the birth-cohort/province level.

When estimating Eq. 1, we should bear in mind that the error term may be correlated with education due to two kinds of endogeneity: selection bias and reverse causality.

Selection bias is related to the fact that education and fertility decisions are both affected by unobservable factors, such as social and cultural norms, religious beliefs, and family background. Usually, these factors are established and shaped during childhood and remain constant over time. A major advantage of panel data is that we can remove any time invariant components, including the unobserved heterogeneity related to norms and religion, by using the within estimator. However, there are other unobservable factors that affect both education and fertility and vary over time. For example, preferences for risky behaviors (during adolescence, the predisposition to engage in risky behavior changes) imply both a higher probability of becoming pregnant and higher probability of educational failures.

Reverse causality is related to the fact that reproductive decisions and human capital investment are joint decisions.

Ordinary least squares (OLS) estimates of β in Eq. 1 which do not account for both endogeneity problems could overstate in absolute value the true effect of schooling on fertility.

In order to solve this problem, we will apply an Instrumental Variables approach (IV), using an instrumental variable that induces exogenous variation in schooling but is uncorrelated with other characteristics, which affect teenage childbearing. As mentioned above, we use the 1993 education reform that extended the number of mandatory years of schooling in Argentina (*Ley Federal de Educación, LFE*) as an instrument for schooling (following Black et al. 2008; Silles 2011; Cygan-Rehm and Maeder 2013; and Alzúa et al. 2016). In that sense, the LFE affects the decision to remain and move through the educational system but it should affect fertility decisions only through the educational channel.

To calculate IV estimates we use the method of two-stage least squares (2SLS). First, we estimate Eq. 2 where $LFE_{c,j,t}$ indicates if teenagers living in province *j,* belonging to birth-cohort c were affected by LFE in year t (extensive margin); and $Polimodal_{j,t}$ indicates the share of students in *Polimodal* in province *j*, in year t (intensive margin). 2SLS allows us to combine more than one instrument in one indicator; in this case, the number of instruments (two) is bigger than the number of endogenous variables (one), so we estimate an overidentified model. The vector of covariates $X_{j,t}$ is exactly the same as in Eq. 1; we also include year fixed effects (λ_t) and birth-cohort/province fixed effects ($\eta_{c,j}$). Equation 2 allows us to isolate the exogenous change in education as a response to the reform, obtaining $\hat{Educ}_{c,j,t}$. In the second stage, we replace $\hat{Educ}_{c,j,t}$ in Eq. 1, obtaining β: the average causal effect of $\hat{Educ}_{c,j,t}$ on $FR_{c,j,t+1}$ for those women whose educational attainment is changed by the reform.

Equation 3 can be derived by substituting the first-stage equation (Eq. 2), into the causal relation of interest (Eq. 1), obtaining the reduced form. The reduced form regression is important because, as Angrist and Krueger (2001)[11] note, if you cannot see the causal relation of interest in the reduced form, it is probably not there. We will estimate the within transformation of Eq. 3 by OLS.

2.4 Internal validity

2SLS estimates can be interpreted as causal effects for those individuals whose educational attainment is changed by the reform instruments (named *compliers*),[12] given that four conditions are fulfilled (Imbens and Angrist, 1994)[13]:

(i) *Independence*, reform exposure is as good as random, conditional upon the controls included.
(ii) *Exclusion restriction*, the education reform should only affect fertility through its effect on schooling choices.
(iii) *First-stage*, the reform must, on average, affect educational attainment in order for it to be used as a source of exogenous variation in schooling. It is also important that the effect on educational attainment be quite strong.
(iv) *Monotonicity*, rules out the existence of individuals that reduce their investments in schooling as a result of the LFE (called *defiers*).

In summary, an instrument, which is as good as randomly assigned, affects the outcome through a single known channel, has a first stage, and affects the causal channel of interest only in one direction, can be used to estimate the average causal effect for students who were induced by the increased schooling requirements to receive more education (*compliers*). This parameter is called the local average treatment effect (LATE). We will now discuss potential threats to the validity of these four assumptions.

The *Independence* assumption would be challenged if there was a correlation between the implementation (timing or intensity) of the reform and pre-reform teenage fertility rates in the provinces. As already mentioned, implementation of the reform was expected to induce an increasing pressure on educational facilities, i.e., provinces would require resources from the central government. For that reason, provinces were more likely to implement the reform early and massively if their governing political party was the same as the national governing party (Alzúa et al. 2015).[14] This suggests that the timing and intensity of the reform both depended on political affinity between central and provincial governments, and were thus not correlated with pre-reform fertility or enrollment rates. Figure 5 provides some evidence supporting the exogeneity of reform implementation and women's fertility decisions. In most provinces, reforms took place when teenage fertility rates were either decreasing or remaining stagnate, suggesting the timing of the reforms was not driven by fertility trends in particular. Figure 6 supports the exogeneity of reform implementation and schooling choices.

Second, selective regional mobility may constitute a threat to the exogeneity of the instrument (violating the *Independence* assumption) if parents of school children would have moved to another province in response to the progress of the reform. Because available data has no retrospective information, our instrument is based on current

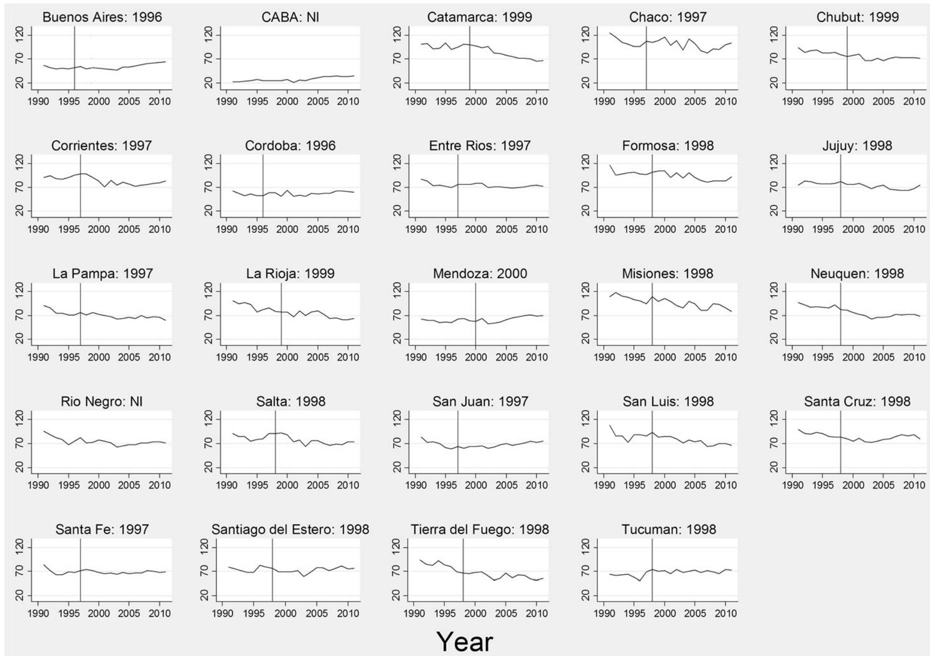

Fig. 5 Teenage fertility rates and implementation of education reform, by province. Argentina, 1991–2011 (number of live births per 1000 women aged 15–19). Notes: *NI* not implement, *CABA* City of Buenos Aires (Federal District). *Vertical lines* indicate year of implementation of LFE by province. Source: Office of Health Statistics DEIS and Crosta (2007); own calculations

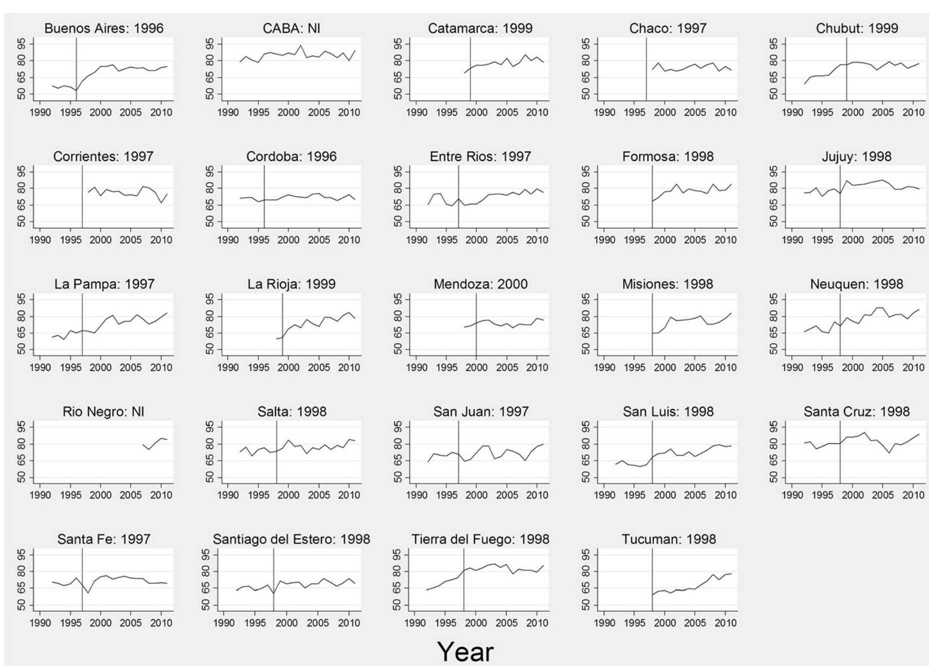

Fig. 6 Gross enrollment rates and implementation of education reform, by province. Argentina, 1992–2011 (teenagers aged 15–19 attending any educational level, %). Notes: *NI* not implement, *CABA* City of Buenos Aires (Federal District). *Vertical lines* indicate year of implementation of LFE by province. Source: Argentina Household Survey EPH - SEDLAC (CEDLAS and The World Bank) and Crosta (2007); own calculations

province of residence and may be therefore partly an outcome of the reform. However, data from the Argentine Household Survey shows that, in 2000, only 3.5% of women aged 13–19 have moved recently (in the past 5 years) from one city to another (this is an upper bound, because it includes people who move within the same province). This evidence suggests that regional mobility should not be a major concern.

Regarding the *Exclusion restriction*, if the education reform is correlated with changes in school quality, and school quality is an omitted variable in Eq. 1, this identification strategy may fail (Holmlund et al. 2011). We do not believe that the education reform was accompanied by a substantial change in quality. Bet (2008) estimates the impact of the LFE reform in Argentina on quality of secondary education, our relevant group. He finds that, on average, the reform did not improve performance in mathematics. Although he finds a small improvement in reading performance, the effect is quite heterogeneous and depends on school characteristics. In order to mitigate this concern, we include the public expenditure on education as a covariate. Although it is well-known that public expenditure on education is not a good proxy of school quality, the information available does not allow us to include a better proxy.

Another concern is that the education reform could affect teenage fertility by other channels related to labor market (violating the *Exclusion restriction*). First, extending compulsory schooling could increase parents' labor supply which, in turn, might affect teenage pregnancy. We do not believe this is a plausible concern, as the reform affected primarily disadvantaged individuals aged 13 to 15 years old, or even older students, taking into consideration the over-age of students. Given this, it seems plausible to assume that parents did not modify the amount of hours they worked due to the reform. Second, the education reform could affect labor market opportunities for teenagers, affecting fertility choices. Regarding this concern, Alzúa et al. (2015) found evidence that the reform had no effect on labor market outcomes for the poor. The authors found an effect only for the non-poor youths. Third, the education reform might also affect parental and children's aspirations and expectations about labor market outcomes, which in turn might cause a delay in pregnancy. With respect to the change in aspirations and expectations, as mentioned above, Bet (2008) found no major change in the quality of education. Since the change in curricula was not very important and there were not many opportunities in the labor market for the poor population, we think that no major change in expectations took place. In order to minimize any omitted variables related to labor market, we include in our regressions control variables, such as the unemployment rate, in line with the variables used by Black et al. (2008) for Norway.

The precondition for using the reform as an instrument for schooling is the existence of a strong *First-stage* relationship between LFE and educational attainment, which is verified in Section 3.2. Besides, Crosta (2007) and Alzúa et al. (2015) showed evidence for this hypothesis.

Finally, the *Monotonicity assumption* fails if there are individuals who are induced by the increased schooling requirements to drop out of school (*defiers*). This seems counter-intuitive, so it seems plausible to assume there are no *defiers*.

We have discussed the internal validity of the two LFE indicators as instruments for schooling in the fertility model. But one concern remains regarding the predictive value of the LATE, in a different context: the external validity. LATE identifies causal effects for students who were induced by the increased schooling requirements to receive more education (*compliers*). In order to make inferences for other populations, we need to assume a constant (homogeneous) causal effect across individuals, which is a rather restrictive assumption. In other words, compulsory schooling laws affect the schooling decisions of a subset of individuals who differ from representative agents, because they would not have otherwise pursued a higher level of education.

3 Results

3.1 The effect of educational reform on fertility

Table 5 reports estimations of the reduced equation (Eq. 3) of the effect of educational reform (extensive in column 1, intensive in column 2, and combined in column 3) on the teenage fertility rate. As can be observed, the educational reform had a statistically significant negative extensive effect (LFE) on the teenage fertility rate for all cohorts that were affected: a decline in the annual fertility rate of 7.5 (or 7.6) births per thousand girls ages 13–19. We can also observe that the proportion of students in *Polimodal* (a 3-year specialized cycle, after mandatory education, created by the reform) had no statistically significant effect on teenage fertility.

These results are robust to the addition of several covariates that capture economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries (columns 4 to 6). The LFE coefficient does not change qualitatively, but it is slightly lower, ranging from 7.3 to 7.4. Again, the proportion of students in *Polimodal* showed no statistically significant effect.

Reduced form estimates indicate that implementation of educational reform (extensive margin) is relevant to explain teenage fertility decisions, while its progress and expansion is not (intensive margin). The causal relations of interest may be hard to identify using the proportion of students in *Polimodal* as an instrument for schooling. For that reason, we will report just-identified estimates using only the dummy variable that captures the implementation of the reform (LFE).

3.2 The effect of the education reform on education

The reform, in order to be a valid instrument, must have a strong effect on educational attainment. We first investigate this by considering the regression results for Eq. 2, which are presented in Tables 6 and 7.

The reform increased education by 0.27 years on average; specifically, it increased women's education by 0.24 years (columns 1 and 2, panel A, Table 6). These results are robust to the addition of covariates, coefficients do not change qualitatively, but they are slightly larger (columns 3 and 4, panel A, Table 6). The *F*-statistic on the excluded instrument (*LFE*) is above the rule of thumb value of 10 in all cases (a first-stage *F*-statistic less than 10 indicates weak instruments, according to Stock, Wright and Yogo, 2002).[15] However, the partial R^2 is extremely low in all cases, that is, the variability of LFE does little to explain the variability of years of schooling, and this result could lead to imprecise estimates in the second stage.

Table 5 The effect of educational reform on fertility (reduced form regressions). OLS estimates of Eq. 3 (within transformation)

	(1)	(2)	(3)	(4)	(5)	(6)
LFE	−7.518 *** (2.446)		−7.632 *** (2.43)	−7.333 *** (2.368)		−7.415 *** (2.346)
Polimodal		0.060 (0.038)	0.061 (0.039)		0.057 (0.038)	0.058 (0.038)
Covariates[a]	No	No	No	Yes	Yes	Yes
R^2	0.925	0.924	0.925	0.926	0.926	0.926
Obs	1764	1764	1764	1764	1764	1764

Robust standard errors clustered at the birth-cohort/province level in parentheses. All regressions include year and birth-cohort/province fixed effects
Source: own calculations
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$
[a]Other covariates included are: economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries

Results from the over-identified model (panel B, Table 6) do not differ much from the case in which LFE is the only instrument: LFE coefficients range from 0.25 to 0.27 additional years of education. However, the proportion of students in *Polimodal* had no statistically significant effect on years of education. The *F*-statistic is above the informal threshold of 10 in columns 1 and 3, but it is below that threshold for the female population (columns 2 and 4) indicating the presence of weak instruments. However, the null hypothesis that instruments are jointly non-significant in all cases is rejected. Once again, the partial R^2 is very low in all cases.

Results for the enrollment rate are presented in Table 7. The reform increased the enrollment rate of teenagers by 2.6 percentage points (p.p.); specifically, it increased women's enrollment rate by 2.9 p.p. (columns 1 and 2, panel A, Table 7). These results are robust to the addition of covariates, coefficients do not change qualitatively, but they are slightly bigger (columns 3 and 4, panel A, Table 7). The *F*-statistic is below 10 (except in column 4), but the null hypothesis is rejected in all cases. The partial R^2 is extremely low in all cases, that is, the variability of LFE does little to explain the variability of enrollment rates.

Table 6 The effect of educational reform on years of schooling. 2SLS estimates—first stage (Eq. 2)

	(1)	(2)	(3)	(4)
PANEL A—IV (identified)				
LFE	0.27 *** (0.048)	0.24 *** (0.064)	0.27 *** (0.048)	0.25 *** (0.062)
Partial R^2	0.0136	0.0080	0.0138	0.0083
F-statistic	31.4 ***	14.8 ***	32.7 ***	16.0 ***
PANEL B—IV (over-identified)				
LFE	0.27 *** (0.048)	0.25 *** (0.063)	0.27 *** (0.047)	0.25 *** (0.062)
Polimodal	−0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)
Partial R^2	0.0149	0.0095	0.0157	0.0100
F-statistic	16.6 ***	8.2 ***	17.4 ***	8.8 ***
Covariates[a]	No	No	Yes	Yes
Obs	1718	1718	1718	1718

Robust standard errors clustered at the birth-cohort/province level in parentheses. All regressions include year and birth-cohort/province fixed effects. In columns (1) and (3) the dependent variable is average years of schooling; in columns (2) and (4) the dependent variable is average years of schooling of females. Source: own calculations
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$
[a]Other covariates included are: economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries

Table 7 The effect of educational reform on enrollment rate. 2SLS estimates—first stage (Eq. 2)

	(1)	(2)	(3)	(4)
PANEL A—IV (identified)				
LFE	2.6 *** (0.88)	2.9 *** (0.925)	2.7 *** (0.875)	3.0 *** (0.9)
Partial R^2	0.0042	0.0036	0.0044	0.0038
F-statistic	8.6 ***	9.8 ***	9.3 ***	11.1 ***
PANEL B—IV (over-identified)				
LFE	2.6 *** (0.876)	2.9 *** (0.924)	2.7 *** (0.871)	3.0 *** (0.897)
Polimodal	−0.012 (0.013)	−0.015 (0.015)	−0.011 (0.013)	−0.013 (0.014)
Partial R^2	0.0052	0.0047	0.0053	0.0047
F-statistic	5.3 ***	6.1 ***	5.6 ***	6.7 ***
Covariates[a]	No	No	Yes	Yes
Obs	1718	1718	1718	1718

Robust standard errors clustered at the birth-cohort/province level in parentheses. All regressions include year and birth-cohort/province fixed effects. In columns (1) and (3) the dependent variable is enrollment rate; in columns (2) and (4) the dependent variable is female enrollment rate. Source: own calculations

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

[a]Other covariates included are: economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries

Results from the overidentified model (panel B, Table 7) do not differ much: LFE increased enrollment rates by about 2.6 to 3 p.p. The instrument related to the share of students in *Polimodal* had no statistically significant effect on enrollment rates. Although the *F*-statistic does not reach the informal threshold of 10 in any case (indicating the presence of weak instruments), the *F*-test rejects the null hypothesis in all cases. Finally, the partial R^2 is very low in all cases.

To sum up, first-stage estimates indicate that the reform, in its extensive margin, had a statistically significant positive effect ranging from 0.24 to 0.27 additional years of schooling, and an effect on enrollment rates ranging from 2.6 to 3 p.p. However, the progress and expansion of the reform (intensive margin) had no statistically significant effect on education (neither on average years of schooling nor on enrollment rates). These results are robust to the addition of several covariates (coefficients do not change qualitatively), which capture economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries. The *F*-test allows us to reject the null hypothesis that instruments are jointly non-significant in all cases. However, the *F*-statistic is below the rule of thumb value of 10 in some cases (indicating the presence of weak instruments) and the partial R^2 is extremely low in all cases, leading to imprecise estimates in the second stage.

3.3 The effect of education on teenage fertility

One additional year of education reduced the number of births per 1000 women aged 13–19 by 27.7. If we consider one additional year of education for the female population, the reduction in the number of births is larger: 30.8 (panel A, Table 8). The results from the over-identified model (panel B, Table 8) are similar, although coefficients are larger in absolute terms. Including covariates in the regressions reduces the coefficients estimated, but it does not modify the statistical significance or the direction of these effects.

In the over-identified model, the Sargan-Hansen test allows us to analyze whether the results are statistically different when using the *Polimodal*, as compared to results

Table 8 The effect of years of schooling on teenage fertility rates (human capital effect). 2SLS estimates—second stage (Eq. 1)

	(1)	(2)	(3)	(4)
PANEL A—IV (identified)				
Schooling (years)	−27.72 *** (8.804)	−30.76 *** (11.397)	−26.89 *** (8.557)	−29.56 *** (10.98)
PANEL B—IV (over-identified)				
Schooling (years)	−31.97 *** (9.083)	−35.53 *** (12.196)	−30.82 *** (8.684)	−33.63 *** (11.421)
Sargan-Hansen (*p* value)	0.252	0.447	0.356	0.511
Covariates[a]	No	No	Yes	Yes
Obs	1718	1718	1718	1718

Robust standard errors clustered at the birth-cohort/province level in parentheses. All regressions include year and birth-cohort/province fixed effects. In columns (1) and (3) the instrumented explanatory variable is average years of schooling; in columns (2) and (4) the instrumented explanatory variable is average years of schooling of females. Source: own calculations

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

[a]Other covariates included are: economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries

obtained when using LFE as the only instrument. In all cases, the null hypothesis cannot be rejected, providing evidence of the validity of the instruments.

Results for the enrollment rate (Table 9) show that an increase of one p.p. in the enrollment rate reduces the number of births per 1000 women aged 13–19 by 2.9. If we consider an increase of one p.p. in the female enrollment rate, the reduction in the number of births is smaller: 2.6 (panel A, Table 9). Once again, results from the overidentified model (panel B, Table 9) are similar, although coefficients are larger in absolute terms. The Sargan-Hansen test cannot reject the null hypothesis in any case, thus indicating the validity of the instruments.

In summary, results provide evidence for a statistically significant negative impact of education on the fertility decisions of teenagers. This negative effect operates through a human capital effect (one additional year of schooling reduces the teenage fertility rate by roughly 26.9 to 35.5 per thousand points) and a weaker 'incapacitation' effect (a rise of one p.p. in enrollment rate reduces teenage fertility rate by roughly 2.4 to 3.3 per thousand points). Education reform LFE reduced repetition rates (Crosta 2007), which may explain the weak 'incapacitation' effect. For instance, after an education reform in Malawi, the average age of

Table 9 The effect of enrollment rates on teenage fertility rates ('incapacitation' effect). 2SLS estimates—second stage (Eq. 1)

	(1)	(2)	(3)	(4)
PANEL A—IV (identified)				
Enrollment rate	−2.904 ** (1.181)	−2.600 *** (1.001)	−2.743 ** (1.094)	−2.450 *** (0.921)
PANEL B—IV (over-identified)				
Enrollment rate	−3.318 *** (1.18)	−2.934 *** (1.006)	−3.125 *** (1.1)	−2.780 *** (0.936)
Sargan-Hansen (*p* value)	0.591	0.664	0.554	0.599
Covariates[a]	No	No	Yes	Yes
Obs	1718	1718	1718	1718

Robust standard errors clustered at the birth-cohort/province level in parentheses. All regressions include year and birth-cohort/province fixed effects. In columns (1) and (3) the instrumented explanatory variable is enrollment rate; in columns (2) and (4) the instrumented explanatory variable is female enrollment rate. Source: own calculations

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

[a]Other covariates included are: economic activity, unemployment, public expenditure on education and health, and *Plan Nacer* beneficiaries

students declined, due to lower rates of grade repetition; and the reduction in the time girls remain in school substantially weakened the 'incapacitation' effect (Grant 2015).

Although the estimated effects are very large, we should remember that LATE identifies causal effects for the group complying with the reform (i.e., young people who did not leave school after 7 years because of the reform and, otherwise, would not have pursued a higher level of education). This group is not necessarily representative of the overall population.

4 Conclusions

This paper provides empirical evidence on the impact of education on teenage fertility for Argentina by applying an Instrumental Variables approach, using a 1993 education reform that increased compulsory schooling from 7 to 10 years (*Ley Federal de Educación*) as an instrument for education. Our identification strategy takes advantage of an exogenous variation in education generated by the staggered implementation of the reform, which responds to political affinity between central and provincial governments (Alzúa et al. 2015).

The education reform seems to have had a significant and positive effect on educational outcomes. Implementation of the reform generated an increase in years of schooling by 0.24 to 0.27 additional years, and an increase in school enrollment rates by 2.6 to 3 percentage points (extensive margin). However, the reform's progress and expansion showed no impact on the stock of human capital or enrollment (intensive margin).

Results provide evidence of a statistically significant negative impact of education on the fertility decisions of teenagers. This negative effect operates through a human capital channel (one additional year of schooling reduced the teenage fertility rate by roughly 26.9 to 35.5 per thousand points) and a weaker 'incapacitation' effect (an increase of one percentage point in the enrollment rate reduced the teenage fertility rate by roughly 2.4 to 3.3 per thousand points). Education reform LFE reduced repetition rates (Crosta 2007), which may explain the weak 'incapacitation' effect (similar to the case of Malawi; Grant 2015).

Although the estimated effects are very large, we should interpret the results as the local average treatment effect (LATE) for the group complying with the reform (i.e., for young people who did not leave school after 7 years because of the reform). This group is not necessarily representative of the overall population.

Reducing early motherhood is a major policy concern due to its adverse consequences on the child and the mother's health, on socioeconomic variables (intra- and inter-generational) and for its public cost. There is evidence that education plays a significant role in fertility decision-making processes among teenagers (Duflo et al. 2015; Black et al. 2008; Baird et al. 2011; Cortés et al. 2010, 2016; Berthelon and Kruger 2011; Silles 2011; Cygan-Rehm and Maeder 2013; and Novella and Ripani 2016). This research contributes to the literature for the case of Argentina, where investing in education may be a powerful tool to reduce teenage fertility.

Endnotes

[1]Literature on fertility distinguishes between direct (proximate) and indirect (distal) determinants. Proximate determinants are biological and behavioral factors (marriage, contraception, abortion and postpartum infecundity) through which education and others socioeconomic, cultural and environmental 'background' variables affect fertility (Bongaarts 1978 and 1982).

[2]Higher earnings raise the opportunity costs of leaving the labor market to bear a child (negative 'substitution effect'); but higher earnings should be positively related to fertility because families can afford more children (positive 'income effect'). However, the substitution effect dominates the income effect under the usual assumption that parents with higher income prefer to invest more in each child (quantity-quality tradeoff).

[3]Under positive assortative mating a woman's education is causally connected to her mate's education, so that the effect of education on household permanent income is augmented through a multiplier effect.

[4]Birth postponement may be temporary and does not necessarily affect completed fertility.

[5]An exception is McCrary and Royer (2011) who do not find any causal effect of education on fertility behavior for the United States. The seemingly conflicting evidence could be due to differences in the type of intervention involved. While in all studies the number of years of schooling increases, the intervention examined by McCrary and Royer (2011) affected school entrance decisions whereas Black et al. (2008), Silles (2011) and Cygan-Rehm and Maeder (2013) investigate reforms that affected school exit decisions. The latter type of intervention is likely to matter more for women who desire to have children early in life but who wish to avoid violating compulsory schooling laws. Prolonging compulsory school by one year will possibly lead such women to postpone childbearing by the same amount of time. Compared to interventions that manipulate school entry age, school exit policies are therefore likely to capture not only the effect of increased human capital, but also that of the mechanical delay related to the woman's desire not to violate the law.

[6]Argentina, Bahamas, Belice, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, and Venezuela.

[7]Law 24195, 14 April 1993.

[8]We would like to determine the share of pregnant teenagers (i.e., teenage pregnancy rate), regardless of the pregnancy outcome. However, we only have information for one pregnancy outcome: live births. We will use fertility rates as a proxy of pregnancy rates. The difference between these two variables is explained by stillbirths, spontaneous abortion, and induced abortion. Due to the illegal nature of induced abortion, the information is scarce, making it difficult to measure its magnitude. Official data indicate that in 2013 there were about 8200 hospital discharges for teenage abortions (15-19 years old). *"Information on hospital discharges due to abortion has several limitations, since it only reflects the public subsector and does not include care in the private system or the consultations by guard, which, considering the increasing use of abortion with medication and the resolution of the consultations of incomplete abortion by guard without hospitalization, would imply an underregistration of the number of women who consult the health system after an abortion"* (Binstock and Gogna 2013). These figures constitute a "floor" for the number of abortions in adolescents, which would raise the 2013 fertility rate from 64.9‰ to 69.6‰ for young women between 15 and 19 years old.

[9]Usually these indicators include women aged 10-19, but the number of mothers younger than 13 years old was very low, even zero for some year/provinces.

[10]*Plan Nacer is* a targeted public health insurance program available for uninsured women who were pregnant or had recently given birth (up to 45 days post-delivery), and children under 6 years of age. The program began in 2004, and uses results-based payments at the provincial and health facility level. It includes a specific package of services which enrolled individuals receive free of charge. Provinces are paid a

capitation fee for enrolling qualified beneficiaries, and health facilities receive fee-for-service payments for providing services.

[11]Cited in Angrist and Pischke (2009).

[12]We can divide the population into four subgroups, defined by their reactions to the instrument: those induced by the increased schooling requirements to receive more education (*compliers*); those who will attend school with or without the LFE (a*lways-takers);* those who will not attend college, even after the law (*never-takers);* and those who reduce their investments in schooling as a result of the LFE *(defiers*).

[13]Cited in Angrist and Pischke (2009).

[14]Alzúa et al. (2015) estimate a hazard model (Jenkins, 1995) of the probability of implementing the reform. The only variable that was significant in most of the specifications was the political party. 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 the outcomes of interest (labor market/educational outcomes).

[15]Cited in Angrist and Pischke (2009).

Appendix

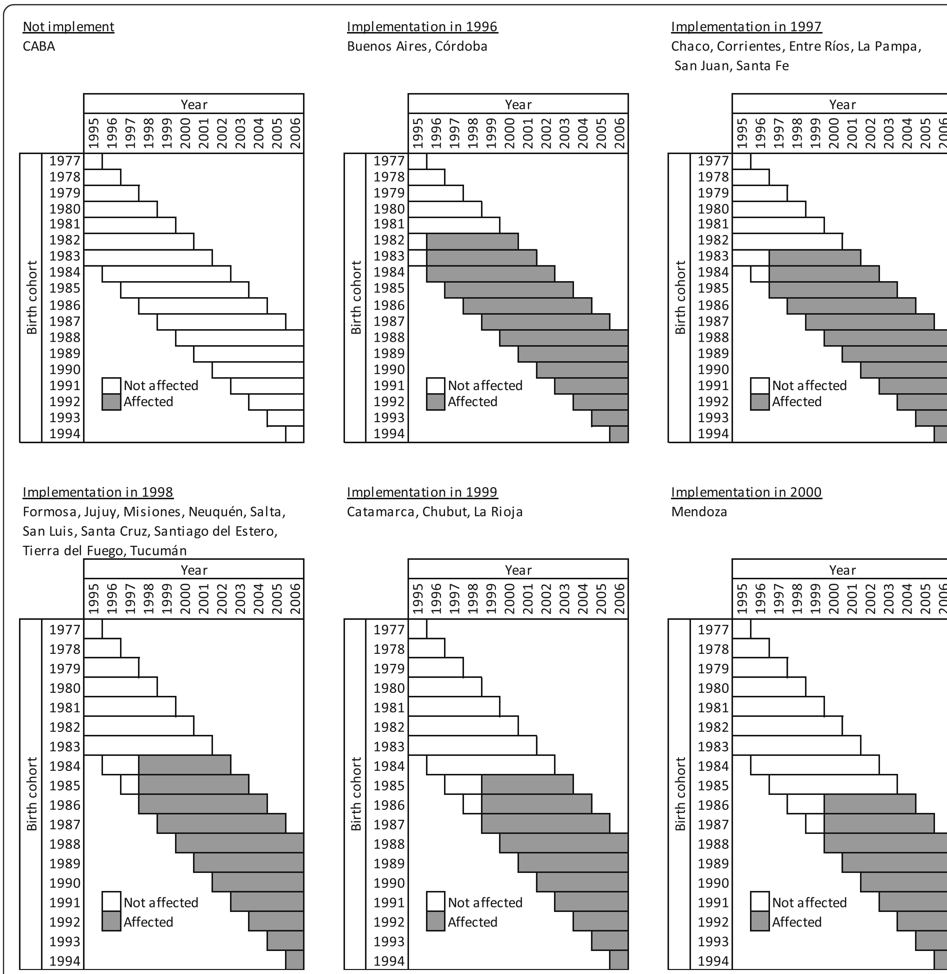

Fig. 7 Birth-cohorts affected by the reform. Note: *CABA* City of Buenos Aires (Federal District). Source: Crosta (2007), own elaboration

Acknowledgements
This paper is based on Cecilia Velázquez's PhD dissertation at Universidad Nacional de La Plata (UNLP). María Laura Alzúa (UNLP) and Wanda Cabella (Udelar) are the thesis advisors. The authors would like to thank, without implication, Wanda Cabella (Udelar) for her invaluable comments, support and suggestions that helped to improve the quality of this work; and Verónica Amarante (CEPAL) and Marcelo Perera (CINVE) for helpful discussions. The authors would also like to thank the Academic Committee of the PhD for valuable feedback, an anonymous referee and the editor for the useful remarks. The usual disclaimer applies.
Responsible editor: David Lam

Competing interests
The IZA Journal of Development and Migration is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles.

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Received: 7 October 2016 Accepted: 20 December 2016
Published online: 14 July 2017

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