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Education, Teenage Fertility and Labour Market Participation, Evidence from Ecuador

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Using a representative sample of Ecuadorian young women's households, this paper focuses on the role played by education in shaping fertility choices and labor market participation. Education, which is found to be endogenous with respect to teenage childbearing, is instrumented by a reform that took place in 1977. Then, in a model where the choices to be a mother and to be in the labor force are considered simultaneously, we find evidence that schooling is positively related to women's labor market participation rate and negatively to early motherhood. The last section concludes stressing the potential intergenerational effects of changes in the age at first birth, showing that firstborn children born to older mothers have better educational outcomes than those born to younger ones. We find that educational policies improve women's conditions, lowering the risk of teenage childbearing and increasing labor market attachment.

JEL: I21, I28, J13, J20
Keywords: schooling, education policy, teenage fertility, labor force.

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1 Introduction

This paper analyzes the effect of education on teenage motherhood and on labor market participation in the case of Ecuador, where early fertility rate is high and women average educational level low. As a consequence women are unskilled and underpaid or even unemployed. It is widely acknowledged by the literature that early fertility has detrimental effects of on both women and children in terms of income, health, education, working opportunities and generally well being (see for example Strauss and Thomas 1995, Ribar 1999, Levine and Painter, 2003, and Francesconi 2008, Gordon 2005, Hoffman et al. 1993, Breierova and Duflo 2004, Behrman and Wolfe, 1989). Research shows that too young mothers are less prone to invest in their children than older mothers with respect to both education and health and the risks associated to teenage motherhood are severe in countries where poverty is widespread. There is evidence that in Ecuador children's conditions are deprived and sometimes miserable and investment in human capital is low, especially among poor families. Children drop out of school and work in order to increase their family income (ILO, 2004). They are exploited in the formal and informal labor market, working for a pay which is far below the minimum wage (ibidem). Governamental commitment to prevent minors' abuse and exploitation is not efficacely pursued and much of the effort for promoting policies in favor of children is left to NGOs.

This work aims to provide further evidence in favor of policies that promoting education discourages early fertility and support labor supply. Moreover for developing countries the relationship between early fertility and labor supply has not been analyzed exhaustively yet and here we propose a model that takes into account early childbearing and labor market participation as interdependent decisions.

From a policy perspective promoting education is relevant, as benefits from an increased educational attainment spread over fertility choices and enhance labor opportunities; this in turn might positively affect further future generations, helping to break self-sustaining poor conditions.

Unobserved characteristics, such as ability and preferences, determine simultaneously fertility choices and investment in human capital and thus complicate the empirical estimation of the effect of education on fertility. This problem has been solved using an instrumental variable approach, by means of a reform of compulsory education that took place in Ecuador in 1977.

The outline of this paper is as follows. The next section reviews the main contributions on the effect of schooling on teenage fertility and on women labor supply. Section 3 presents the data and Ecuador's background. The empirical model and estimates' results are presented in section 4. Section 5 concludes.

2 Education, teenage motherhood and labor supply in the literature

The consensus on the role played by female schooling on fertility is wide for both developing and developed countries: women education causes a postponement of fertility and generally its decline (see among others Osili and Long, 2008, Monstad et al., 2008, Silles, 2011, and Fort, 2009).

While for developing countries most of the researches analyzes the effect of education and fertility on human capital outcomes and only marginally on labor market perspectives, for developed countries, most of the current research is focused on the latter in a dynamic perspective, showing that an increase in women's schooling level raises their expected wages and so the opportunity cost of having a baby, expressed in missed earnings. From the seminal theoretical contribution by Becker (1960), many studies explored empirically life cycle models of fertility where education plays a crucial role in explaining both fertility *quantum* and/or *tempo*. Heckman and Walker (1990) estimate a reduced-form neoclassical life cycle model for fertility in which lower fertility rates and a delayed times to all conceptions are associated to higher female wages. In

Walker (1995) the price associated to children is expressed by the time spent with the child, which is generally the opportunity cost represented by female wages, the direct cost of rearing a child and the investments in human capital that a household forgoes. Gustaffson (2001) reviews many empirical contributions to explain the fertility patterns observed in Europe. He points out that the main factor driving the slow down of the observed population growth is the women's career costs: those include the wage loss and the lower human capital investment caused by time spent out of work. Other researches analyze empirically fertility and labor market participation taking into account bivariate and trivariate models that allow to jointly estimate fertility choices and labor supply (Di Tommaso, 1999 , Colombino and Di Tommaso, 1996, Di Tommaso and Weeks, 2000, Bratti, 2003, Cruces and Galiani, 2007, Ekert-Jaffe and Stier, 2009). These works show that the two decisions are strongly interdependent and again that the relationship between fertility and labor supply is negative.

However, an increase in educational level has an impact on fertility that goes beyond the labor market outcomes cited above. There are many channels through which education influences reproductive behavior and moreover this relationship is not static over the demographic transition. Education improves women's ability to process information regarding contraceptive technologies: Rosenzweig and Schultz (1985), Rosenzweig and Seiver (1982), Ainsworth et al. (1996), Florez and Nuñez (2002) provide evidence that education have a positive effect on the use of contraceptives.[1]

Glewwe (1999) and Schultz (1993) find that mothers' education has positive effect on the health status of their offspring so that child mortality and thus fertility is reduced. Chou et al. (2007) find evidence of decreasing child mortality with increasing parents' educational level, instrumented by a reform on compulsory education in Taiwan. The work by Lam and Duryea (1999) on Brazil provides evidence that the sharp decline in fertility observed when schooling increases is linked to improvements in children well being rather than to an upward shifts of women labor supply, as education promotes investments in children's quality, as opposed to quantity.

Even if birth controls methods are diffused and relatively cheap, husband disapproval can hamper women to use them, as shown by Kamal (2000) in a study on Bangladesh. So if education affects the decision making processes inside the family in favor of the women, it can have a role in decreasing fertility (Jejeeboy, 1995).

So education triggers changes in expected wages and cultural values that modify fertility choices. Other studies also point out that the time spent in school itself discourages unwanted pregnancies. A recent work by Kruger et al. (2009) analyzes the effect of a school reform that increased hours spent in school in Chile and show that teen motherhood is reduced by access to full-day education. Also the so called incarceration effect plays a role in the case analyzed by Black et al (2008) even if other forces are at work.

This work shows that the relationship between schooling, early fertility and labor supply is causal, thus providing evidence in support of policies that promoting education are a tool to sustain development and growth processes.

3 Data and background

This work makes use of data from the 1990 census provided by the *Instituto Nacional de Estadistica y Censos, Ecuador*, and harmonized by IPUMSI. It contains information on households' composition and on the dwellings' general characteristics. Each woman in our representative sample (10%) is matched to her children and to all the people currently living in the household. So data provide two kinds of information: a) on the house's status: number of rooms, availability of the

[1] Increased education does not imply a reduction in fertility rates; in fact at the beginning of the demographic transition, education is associated to an increase in the number of children, because of their health improvement and higher survival rate. Notwithstanding, the empirical analysis, as in Schultz (1993) shows that when infant mortality goes down also fertility is reduced. as soon as it gets fully controlled and desired family size is smaller than the actual one.

kitchen, phone line, sewage system, water supply, and b) on households members: parental rule, link to household's head, age, sex, province of birth, place of residence, educational level, employment status and occupation. According to the 1990 census 20% of the women below age 30 gave birth before age 19, and 15% before age 18. Only 68% of the women between age 15 and 30 completed primary schooling.

Figures 1 clearly suggests the correlation between education, teen fertility and labor market participation. Education expressed in levels (no schooling or some, primary completed and some secondary and above) is positively related to participation in the labor market and inversely to early fertility, proxied by being a mother before age 19.

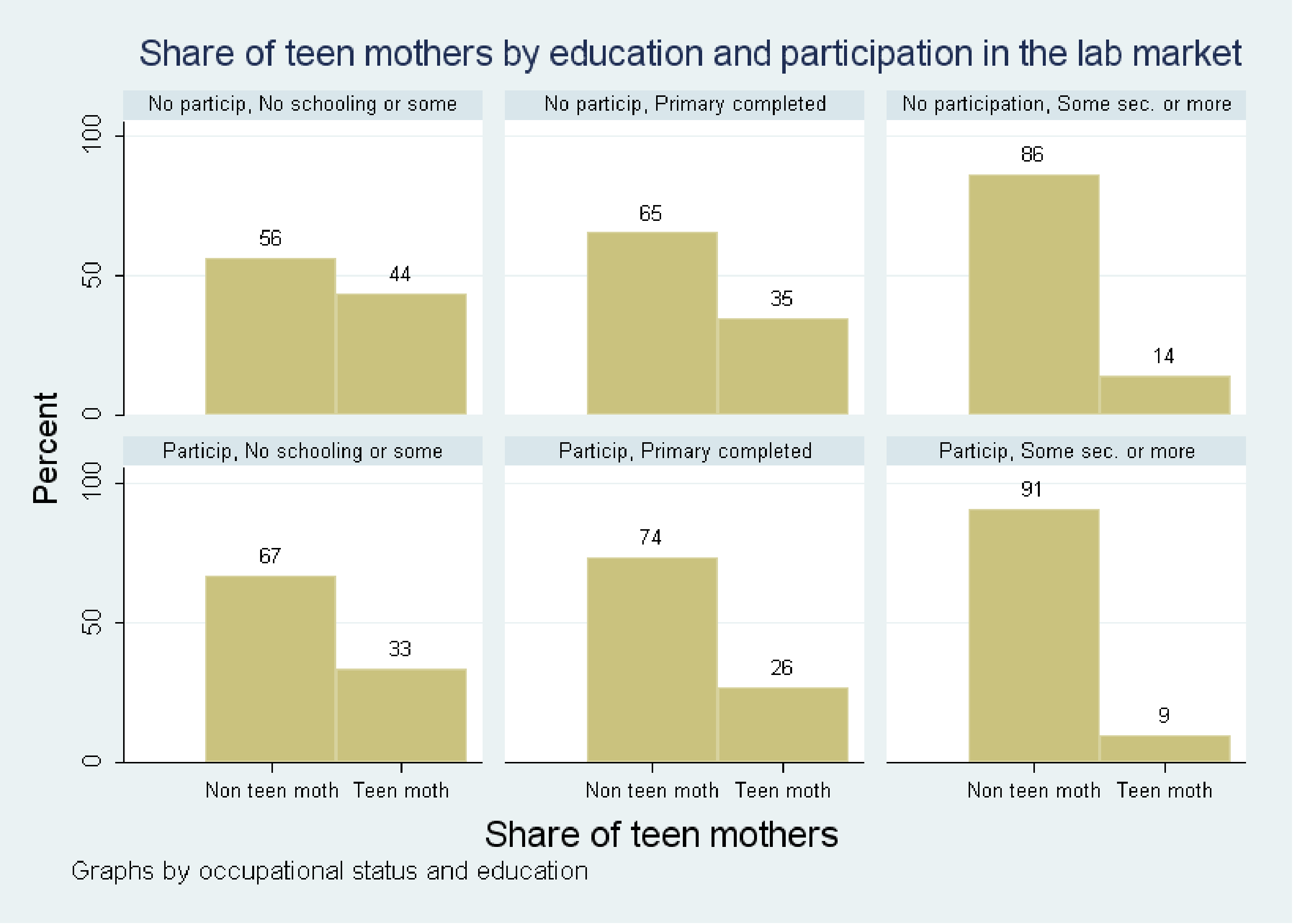

Figure 1 Proportion of teenage mothers in 1990, by educational level and participation in the labor market; source: IPUMSI 1990.

What needs to be uncovered is the causal relation between education, teenage motherhood and labor supply. Changes in compulsory schooling laws provide a useful source of exogenous variation in educational attainment and have been used widely in the literature as an instrument for schooling (see among others the works by Black et al. 2008, Monstad et al. 2008, and Osili and Long 2008). In 1977 a reform - *Ley de Educación y Cultura* - extended compulsory education from six years to nine years, making it freely available to all.[2] With the reform minimum school leaving age was set to 15, so that every pupil under the age of 15 must be enrolled. The Ecuadorian educational system is divided into primary education that last from age 6 to 12, followed by secondary education that last other 6 years. Secondary schooling is composed by two three-year cycles. So the law made compulsory the completion of lower secondary education. In Table 1 we explore the mean differences in years of schooling, teenage fertility and labor supply between the

[2] http://www.tau.ac.il/eial/X_1/ossenbach.html

cohort targeted and non targeted by the reform. The treated group attend on average 0.5 years of schooling more than the not treated group and there are also significant differences in terms of teenage fertility and labor supply. Those that are treated show a lower fertility rate and a stronger labor market attachment.

In the next section we present the econometric models that explore the relationship between education, early childbearing and working status.

DIFFERENCES BETWEEN TREATED AND NON TREATED GROUPS

	Treated group	Non-treated group	Difference (treated minus non-treated)
Average years of schooling	8.7	8.2	.52
	(.038)	(.042)	(.057)
Share of women that give birth before age 19	.26	.28	-.012
	(.004)	(.004)	(.005)
Share of working women	.40	.38	.02
	(.004)	(.004)	(.006)

Standard errors in parentes. Sample of women aged 24-31, from 1990 census (IPUMSI)

Table 1 Means of years of schooling, teenage fertility and labor participation by treatment status

4 Methods and results

Here we first present the variables that are used throughout the paper; the identification strategy and the empirical models follow.

The variable that is exclusively referred to education is a dummy that identifies the individuals targeted by the reform (R_i). The x_i group of variables includes the other covariates: the share of married woman in each *canton* is meant to capture social norms that might influence women's fertility behavior;[3] the share of doctors and nurse in each *canton* can indicate that information about (reproductive) health are easier to acquire; age; a household wealth index built with partial correlation analysis that contains information on the socio-economic conditions of the household and on the household's assets;[4] a dummy variable for urban/rural status; a dummy variable that controls if the woman live in Quito or Guayaquil, the two biggest cities in Ecuador (this controls for the fact that living in a big city can increase educational and labor opportunities); province of birth fixed effects. Descriptive statistics are in Table 5 in the Appendix.

4.1 Models for the relationship between schooling and fertility

We define as teenage mothers all the women that became mothers before age 19 and we infer age at first birth by looking at the age of the eldest son currently living in the household, as in Black et al. (2008). This strategy is pursued because age at first birth is not recorded in the census.

Our major concern is the loss of information about the age at first birth if children are abandoned or leave soon in the lifetime. It is reassuring that, if education promotes investment and care for children, more educated mothers do separate from their young children less frequently, so that the effect of education on teenage fertility would be at most downward biased (if the probability to abandon a child is decreasing with education). Anyway, our strategy might be

[3] Controlling for each woman marital status would surely explain better fertility but can potentially bias the estimation, since it is not clear if marriage is a consequence of a cause of pregnancy. In this way we have a more indirect measure of marital status.

[4] The variables included here are: the availability of electricity, of sewage system, of water supply, of kitchen, of toilet and number of rooms per person, similarly to what La Ferrara et al. (2008) do. Since these variable are discrete it has been used polychoric partial correlation analysis, following Kolenikov and Angeles (2004).

complicated if children leave their parental home before the age of 16.[5] We address this specific issue inspecting the difference between the stated number of children ever born and the number of children that are currently leaving in the household at the time of the census. This difference is close to zero when a mother is around the age of 20 and then it is increasing with mother's age, as expected; 8% of the woman in the sample under analysis is not classified as teenage mother, but nevertheless has at least one child that is not currently leaving with them. We don't know how many of these women became in fact mothers before age 19, but we know how this share of potential wrongly classified women is distributed across age. At 24 (which is the youngest cohort considered) the share of potential misclassified non-teenage mothers is equal to 0,08, while at 32 (the oldest cohort) it is 0,15. This signals that even very young kids do not leave with mothers, so that teenage motherhood is likely to be underreported. Anyway, the attrition bias is at most leading to underestimate the effect of schooling on teenage motherhood, since education is instrumented by a reform that target the younger cohorts, for which the misclassification problem is less relevant than for older women.[6]

The model for the relationship between schooling and teenage motherhood can be written as follows:

$$T_i = \beta_1 + \gamma s_i + x_i^{'}\beta_2 + \eta_i \qquad (1)$$

where T_i is a dummy variable which is equal to 1 if woman *i* had her first child before age 19; s_i is years of schooling; η_i is the error term. Given that the dependent variable is dichotomous we estimate the model using maximum likelihood probit; marginal effects are reported in Column 3 of Table 3. As expected the effect of schooling on teen fertility is negative: each additional year of schooling decreases the probability of being a teenage mother by 2%.

The effect of schooling on teen fertility can be biased if the residuals η_i are correlated with s_i: there might be unobserved characteristics, such as ability and preferences, that determine both fertility choices and investment in human capital. Intuitively education can be endogenous to early motherhood if women with strong labor market preference invest more in education and less in children, and vice versa; moreover it is not clear if the decision to quit school is a cause or an effect of fertility decisions, for example if teenagers get pregnant and they drop out of school (Glewwe, 2002, and Ribar, 1994).

In order to solve this problem we refer to a reform in compulsory education to instrument schooling, provided the reform is correlated with schooling and not directly with fertility. Acemoglu and Angrist (2000) and Angrist and Krueger (1991) refer to reforms in compulsory education law as to the ideal instrument for education. Education has also been instrumented and found negatively related to fertility in the works by Silles for Great Britain and Ireland (2011), by Black et al. for the US and Norway (2008), by Monstad et al. (2008) for Norway, by León (2004) for US, by Fort (2009) for Italy and by Tayfur et al. (2008) for Turkey.

Schooling equation

The effect of the 1977 reform is estimated following a regression discontinuity (RD) designs approach, first introduced by Thistlethwaite and Campbell (1960). This strategy is appropriate to all the non-experimental settings where individuals are assigned to the treatment depending on the value taken by a specific variable over which they do not have control (see also Lee and Lemieux 2010). The RD designs requires that a threshold in an observed characteristic defines the treated and the non-treated groups and assumes that individuals close to the cut-off point are identical in all the characteristics but the assignment (observable) variable.

[5] This threshold considers that women can be classified as mothers from the age of 15 and in the analysis the oldest cohort is aged 31 (16+15=31).
[6] So younger women are more likely to be reported as teenage mothers than older ones.

The outcome variable of interest is represented by s_i, years of schooling of woman *i*. Assignment to the treatment is determined by age so that the dummy variable R_i is equal to 1 if individual is younger than a cut-off age and 0 otherwise, $R_i \equiv I(age_i \leq \overline{age_j})$. Let $s_i(1)$ and $s_i(0)$ denote the potential outcomes for each woman being exposed and not exposed to the reform respectively, so that the outcome that we want to estimate is $E[s_i(1)] - E[s_i(0)]$.

We assume that all other individual's characteristics do evolve smoothly around the cut-off age, so that the average outcome of those just above the threshold age could be used as a counterfactual for the one of those just below (that are treated by the reform).

Since the reform was implemented in 1977 and extended compulsory schooling minimum leaving age to 15, we can split the sample of women in two: those that in 1977 were 14 or younger and those that are older and thus not targeted by the reform. Since only the oldest cohorts in the sample could have had completed the highest level of education, we censor the outcome variable s_i (years of schooling) at 17 and we estimate the model using maximum likelihood tobit.[7]

Since s_i is a latent variable, we have that:

$$s_i^* = \begin{cases} s_i & if\ s_i \leq 17 \\ 17 & if\ s_i > 17 \end{cases}$$

and $s_i = \beta_o + \beta_1 R_i + x_i^{'} \beta_2 + \varepsilon_i$ (2)

The error term ε_i is clustered so to allow for spatial correlation (at province level) and for treatment status. In order to test the efficacy of the reform on educational attainment we estimate various specifications of the model using different age cohorts: ±2, ±3, ±4 and ±5 years wide. We include a polynomial in age of order 2 so to absorb cohorts trends.[8]

SCHOOLING EQUATIONS

VARIABLES	(1) Cohorts ±2 years	(2) Cohorts ±3 years	(3) Cohorts ±4 years	(4) Cohorts ±5 years	(5) "Shifted" reform 1	(6) "Shifted" reform 2	(7) Randomized reform
Reform	0.29**	0.27**	0.32***	0.29***			
	(0.131)	(0.116)	(0.101)	(0.101)			
Fake reform					0.05	0.05	0.03
					(0.094)	(0.115)	(0.052)
Observations	13,948	22,845	29,162	36,284	28,272	26,658	29,162
Pseudo R-squared	0.071	0.072	0.071	0.070	0.072	0.073	0.071

Robust standard errors in parentheses, clustered at Province-treatment level. *** p<0.01, ** p<0.05, * p<0.1

Table 2 Schooling equations: different age cohorts on each side of the reform pivotal year (columns 1-4). Robustness checks (columns 5-7). Complete results in the Appendix

Results (Table 2) show that the effect of the reform is stable across the different specifications, until 5 years away from the reform on each side: the reform increased schooling by 0.29-0.32 years, according to the specification. Increasing further the time span considered can not be done safely as we might pick up other changes that are not of our interest and whose effects can not be ruled out simply including a polynomial in age.[9]

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[7] The youngest cohort in our sample includes women aged 24. At that age they could have potentially completed only 17 years of schooling, as children start going to school at 6.

[8] Higher order polynomials have been tested but do not improve the fit of the data. The second order provide a good fit of the data and is parsimonious. Higher order polynomials are suitable only for model where the cohorts compared are wide enough.

[9] Complete results are in the Appendix.

In the proceeding of the paper we will identify the treated/non-treated groups by looking a the cohorts ±4 years away from the reform, as in this specification the reform impact is the greatest. The graph in Figure 2 offers a visual representation of the reform effect: we plot the residuals of a linear regression of schooling against all the explanatory variables in equation (1) but the reform. For the treated cohorts there is a clear jump in schooling level close to 0.4 additional years of schooling, consistently with our previous findings.

In order to inspect the robustness of the treatment (reform) we perform two additional checks. First we pretend the reform was implemented in 1976 (column 5 of Table 2) and 1975 (column 6), thus shifting by one and two years its date and consequently the treated/non treated cohorts. In none of these two cases the fake reform is affecting educational attainment, leaving unaltered the other controls effects. So if we shift the treatment variable to the first two non-treated cohorts the reform's effect turn out to be not different from zero. Next we randomize the treatment among those that are 4 years away from the reform on each side and again the reform's effect is not statistically different from zero (column 7).[10]

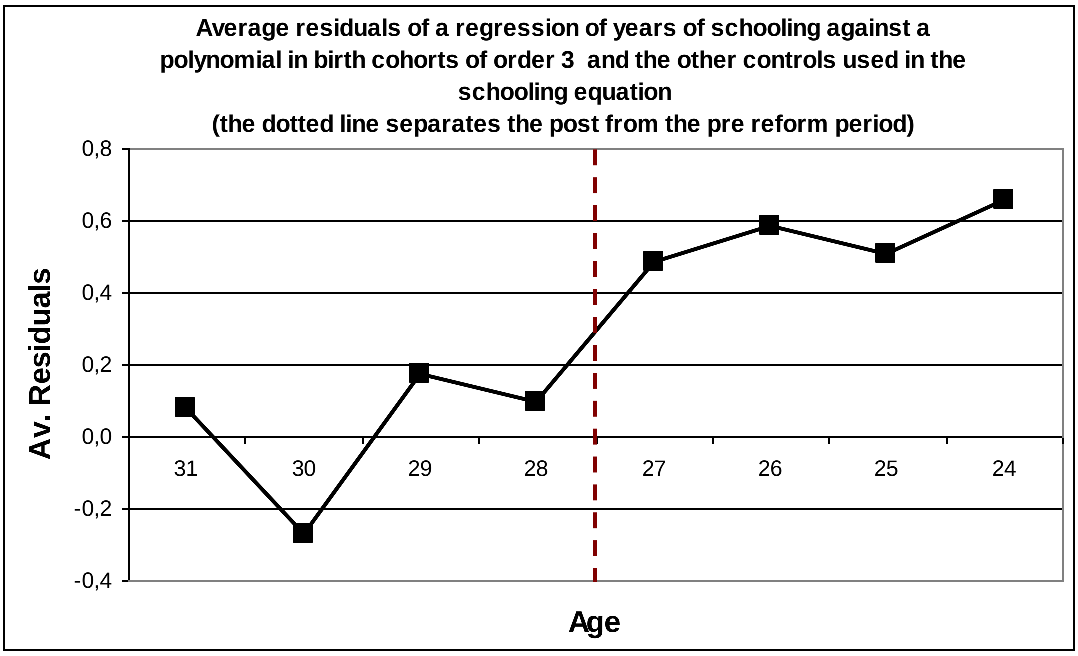

Figure 2 The effect of the reform on years of schooling

We also explore if other potential confounding factors (other major political changes) influence schooling attendance along with the reform. In Ecuador different military dictatorships had been in power from the beginning of the sixties until 1979, when democratic elections were held, after a period of state-lead modernization and growing foreign debt. After that year governments were unable to promote serious structural reforms, so that poverty and unemployment condition worsened, with increasing popular discontent (The Economist Intelligence Unit, various years). So the political context was unstable, even if it didn't end up in severe violence episodes. According to the information available there is no reason to believe that the end of dictatorship caused an increase in educational attainment, since the country's general conditions worsened.

Since next we implement an IV strategy for the relationship between education and teenage motherhood, we explore whether the reform and the control for teachers availability have a direct impact on the probability to give birth before age 16, which is one year after the minimum school leaving age after the law change. We estimate the following:

[10] We simply attribute the treatment to each women drawing the reform dummy from uniform distribution: if the drawn value is greater than 0.5 the woman is assigned to treatment.

$$T_{i,<16} = \beta_0 + \beta_1 R_i + x_i^{'} \beta_2 + \eta_i$$

where as before T_i is a latent variable and $T_i^* = I(T_i > 0)$ is estimated by maximum likelihood probit ($I(\cdot)$ is the indicator function). The dummy is equal to 1 if age at first birth is lower or equal to 16. Scholars refer to this as to an 'incarceration' effect: women are not involved in 'risky behaviors' because they are enrolled in a school program (Black et al., 2008, and Kruger and Berthelon, 2009). So we regress the probability of becoming mother before age 16 against the reform and the occurrence of such early pregnancies is not influenced directly by the reform (Column 1 of Table 3). We argue that the reform has an impact on early fertility only through schooling.

IV estimation

Our IV strategy consists of two equations (teenage fertility (1) and schooling (2)), where education enters endogenously fertility and is instrumented by the reform. We test the relationship between schooling and teenage fertility defining as teenage mothers women that gave birth before age 19 (Table 3).[11] As before the schooling equation is modeled by tobit, while the fertility equation is a probit. The two equations are estimated by full information maximum likelihood using conditional mixed process, the flexible procedure illustrated by Roodman (2010). This estimation technique is compatible with recursive models with a bounded endogenous explanatory variable. ρ measures the endogeneity of schooling into the fertility equation: $\rho = corr(\varepsilon_i, \eta_i)$.

For those that were induced to stay longer at school by the reform (complier women, in the language of Angrist et al. 1996), these estimates suggest that each year of schooling decreases the risk of teenage motherhood by 1% (column 6, Table 3). Given that the average teenage fertility rate in our sample is 27%, this corresponds to a decrease of 3,7% in the probability of early childbearing.[12]

MODELS FOR TEENAGE MOTHERHOOD

VARIABLES	(1) Marg. Eff being a mother before age 16 (incarceration)	(2) Marg.eff being a mother before age 19 (exclusion)	(3) Marg.eff. being a mother before age 19 NON-IV	(4) Coefficients being a mother before age 19 (2nd stage)	(5) Years of schooling (1st stage)	(6) Marg eff. being a mother before age 19 (2nd stage)
Reform	-0.00	0.00			0.28***	
	(0.008)	(0.012)			(0.073)	
Years of schooling		-0.02***	-0.02***	-0.03***		-0.01***
		(0.001)	(0.001)	(0.003)		(0.001)
Corr(fert,schooling)				-0.20***	-0.20***	-0.20***
				(0.017)	(0.017)	(0.017)

Robust standard errors in parentheses. Errors are clustered at Province of birth-treatment level. Obs 29,162
*** p<0.01, ** p<0.05, * p<0.1

Table 3 Fertility equations for teenage motherhood: column (1) direct impact of the reform on the probability to become mother before the end of compulsory schooling; column (2) probit estimates including the reform, column (3) standard probit estimates; column (4-6) IV estimates, coefficients and marginal effects.. Complete results in the Appendix

[11] We also test the effect of schooling on the probability of being a mother before age 18 and 20 using both the probit and the IV strategy; results are stable over these alternative specifications.
[12] (-0,01*100)/0,27=-3,7

In this IV setting the coefficient of the excluded instrument is of the expected sign and strongly significant. ρ is statistically different from zero and negative, meaning that there is a latent factor that increases schooling and decreases the probability of being a teenage mother (or viceversa), as expected. So this IV framework is appropriate and education can not be considered exogenous with respect to adolescent childbearing.[13]

The IV model represents the effect of schooling on fertility only for those women that without the reform would have chosen a lower level of education and IV results can not explain the fertility behavior of the whole population. So even though we can not generalize our result, nevertheless we get estimates that are not biased by unobserved heterogeneity. The impact of schooling in the IV framework is smaller than in the standard probit estimates (column 3), suggesting that women targeted by the reform might be less prone to modify their fertility decision with respect to an average woman (instead in Silles, 2011, results show a greater reaction for the compliers). Anyway, since we can not measure the *real* average schooling effect because exogeneity is rejected, the difference between the probit and the IV estimates for the impact of education has to be ascribed to unobserved heterogeneity and possibly also to a different behavior of the compliers from a population average.

We check whether the reform is correctly excluded from the fertility equation or not and it turn out not to be directly related to fertility (column 2).[14] So the effect of the reform acts only through schooling.

We check the robustness of the relationship using different cohorts replicating the exercises that were performed for the schooling equation. The effect of schooling on teen fertility remains stable using cohorts from 3 to 5 years on either side from the reform's year. This is not surprising since also the reform effect in the schooling equation didn't show great variability in the different specifications considered (results not reported).

One possible confounding factor would be if along with the reform there were significant changes in the availability of contraceptives. According to what we are aware of, we can not assume that there were dramatic changes in the birth controls used by Ecuadorian women caused by exogenous shocks, such as massive family planning campaigns that in fact in those years were reported as 'weak'; so the use of contraceptives relies on women educational level and not on their availability (Weinberger et al., 1989, and Lapham and Mauldin, 1984). According to the DHS survey (1987) the majority of Ecuadorian women do not use any contraception method: schooling can have a role in promoting access to modern methods for births control, even if in this work the channel through which educated women avoid early pregnancies can not be properly spelled out.[15] This study would surely benefit of information about women family planning choices that instead here we can only assume, as well as of other information such as each woman desired family size or her parental background (in terms of both education and fertility history).

So the reform considered here, causing an increase in schooling level, decreases the probability of early motherhood. Since having children when adolescent could negatively affect their future development, this policy can be thought as a tool to promote education on the current generation with potential effects on the next one (this issue will be taken into account in the last section).

4.2 Schooling, fertility and labor supply

[13] We also regress teenage motherhood against the instruments and the other covariates (excluding years of schooling) to check whether the two excluded instruments are influencing directly the probability of becoming a teenage mother or not; they both turn out to be not significantly related to early motherhood.

[14] We include in the fertility equation also the reform and its impact it is not statistically different from 0.

[15] The Demographic Health Survey is a program that collects (representative) data on population health and fertility behaviour. It is funded by the US Agency for International Development (USAID) as well as by other donors and participating countries (www.measuredhs.com).

We explore the impact of schooling on fertility choices and on labor supply taking these decisions as interdependent using a trivariate model of simultaneous equations. The advantage of considering such a model is to explicitly calculate if the three choices are correlated and how. So even if fertility does not enter the labor equation we are able to test if there is a trade off between fertility and labor supply.

Teen pregnancy and labor market participation are likely to be interdependent as they reflect the solution of a lifetime utility maximization problem. Also in the works by Moffit (1984) and Di Tommaso (1999) schooling, fertility and labor supply decisions result from a common process of lifetime utility optimization. It is evident that teenage fertility decisions and current labor supply are not simultaneous choices, as none of the women of the sample is a teenager, but education, labor and fertility are chosen so to maximize an intertemporal preference function defined over consumption, labor supply and family size, subject to budget and time constraints.

Since we rely on cross sectional data, the real timing of labor supply and teenage fertility decisions is not explicitly modeled, as we would need a dynamic setting in order to have consistent estimates. We simply consider a framework that allows interdependence among these choices and estimate their reciprocal dependence allowing the error terms to be correlated.

Before estimating the trivariate model we explore the relationship between schooling and women labor supply in a simple probit model and in a bivariate framework identical to the one used in the previous section for fertility. This will allow us to test if the inclusion and exclusion of the instrument is correct and will guide us to better understand the effect of the endogeneity bias if it is in place. Labor market participation is modeled as follows:

$$L_i = \gamma s_i + x_i^{'}\beta + \iota_i \qquad (3)$$

where L_i is a latent variable as we only observe if woman *i* is in the labor market or not, so we actually estimate $L_i^* = I(L_i > 0)$. Results are in column 1 of Table 8 in the Appendix. This estimation might suffer from endogeneity bias if unobserved heterogeneity affects both labor supply and schooling decisions. So we instrument schooling with as we did for fertility and consider labor supply in an IV framework, using full information maximum likelihood for equation (1) and (3) (columns 3-5 of the same Table). Results show that in fact endogeneity can not be rejected as $\rho = corr(\iota_i, \eta_i)$ is 0,26 and strongly significant. Again the reform can be excluded from the labor supply equation as it does not impact directly labor supply: the reform increased labor supply only through schooling (column 2).

Thus, we can estimate a three equations model for schooling (equation 1), fertility (equation 2) and labor supply (equation 3), using simulated maximum likelihood. We take into account the two probit models and the schooling tobit model by means of the algorithm provided by Roodman (2010) that produces consistent estimates using simulations techniques, as the cumulative normal distributions needed are above dimension two.[16] We assume that the error terms $\varepsilon_i, \eta_i, \iota_i$ are distributed as a multivariate normal, with mean of zero and a variance-covariance matrix V, with unitary variance and symmetric:

$$V = \begin{bmatrix} 1 & \rho_{12} & \rho_{13} \\ \rho_{21} & 1 & \rho_{23} \\ \rho_{31} & \rho_{32} & 1 \end{bmatrix}$$

and $\rho_{12} = \rho_{21} = corr(\iota_i, \eta_i)$, $\rho_{13} = \rho_{31} = corr(\iota_i, \varepsilon_i)$, $\rho_{23} = \rho_{32} = corr(\eta_i, \varepsilon_i)$.

The results of the trivariate model are shown in Table 4.

A one year increase in schooling increases the probability to participate in the labor market by 1% and at the same time decrease the probability to be a teenage mother by 1%, as we already showed. Schooling at the same time diminishes the likelihood to be a teenage mother and affect

[16] The algorithm used to estimate the cumulative distributions follows a Monte Carlo technique that for each observation minimizes the correlation between the draws.

positively women's labor supply. So since 39% of the women in the sample are working, schooling increases the probability to be in the labor force by 11 % for compliers ((0,01 * 100)/0,39).

TRIVARIATE MODEL FOR SCHOOLING, TEENAGE MOTHERHOOD AND LABOR SUPPLY

	(1)	(2)	(3)	(4)	(5)
		Coefficients		Marginal effects	
VARIABLES	Labor equation	Fertility equation	Schooling equation	Labor equation	Fertility equation
Years of schooling	0.02***	-0.03***		0.01***	-0.01***
Reform			0.25***		
rho(lab,fert)	-0.15***	-0.15***	-0.15***		
rho(lab,schooling)	0.26***	0.26***	0.26***		
rho(fert, schooling)	-0.20***	-0.20***	-0.20***		

Robust standard errors in parentheses, clustered at Province of birth-treatment level. Obs: 29,162
*** p<0.01, ** p<0.05, * p<0.1

Table 4 Estimation of a trivariate model for labor supply, teenage fertility and schooling. Complete results in the Appendix

The correlation between labor, fertility and schooling decisions, the rhos, are estimated and give a measure of the existing relationship between the three choices. Latent factors not captured by the model affect positively labor participation and education and, in opposite directions, fertility and labor supply and fertility and education: these results point to an existing trade off between education and labor market participation on one side and teenage motherhood on the other.

Following the interpretation of Walker (1995), women with a high discount rate have children early in the life time, forgo education and stay out the labor market. On the other hand, when the intertemporal discount rate is lower, children's 'opportunity cost' is high, so women delay their 'consumption' in order to get educated and participate in the labor market. Our results are consistent with this interpretation: women with strong labor market preferences tend to invest more in education and on their working career and less in (early) fertility.

We don't know what are the fertility patterns for women when older, it might be the case that completed fertility levels would be the same for more and less educated women. In this context we only observe that the investment in education and not in adolescent motherhood are positively related to labor supply.

We could in principle give more complete results if we could use more detailed information about religion and about the ethnicity of people, but unfortunately those information are not collected. Also it would be relevant to know at what age the women in the sample were conceived, in order to test if there is intergenerational transmission of fertility behavior and if so this could be a stronger instrument for early fertility. In general any information about the original family would help to give more precise estimates, but with census data this is not possible, since the majority of women involved in the analysis are not living with their parents anymore.

Another aspect that is not taken into account is the kind of job: we don't know if the working woman is exploited in some informal labor market or if she has a fair wage. So this model could be improved in various ways conditional on having more information about each household's characteristics. Anyway, the results obtained here are reasonable are fully consistent with standard interpretations.

According to our results education plays an important role in limiting teenage motherhood and increasing the likelihood to access the labor market. With respect to the previous model these estimates also point out that there is a trade off between fertility and labor supply showing that the two choices are interdependent.

4.3 *Age at birth and children's schooling*

The first empirical model presented in this work shows that women's educational attainment, increased by a compulsory schooling reform, has a role in delaying fertility. From a policy perspective it is interesting to see whether women's fertility choices influence children's outcomes, so we test if there is a relationship between the age of first pregnancy and each woman children's educational attainment. Since universal education is a goal pursued by many policies, it is relevant to understand if benefits of an increased educational level spill over on future generations.

Various papers in the literature showed the positive effects of avoiding early pregnancies on children education, morbidity and mortality as already reviewed. While for developed countries the evidence is sometimes limited and not significant, for developing countries benefits of parents' educational attainment on children outcomes are less blurred and definitely positive. So campaigns encouraging women's education by the World Bank or the UN for example find their support.[17]

Here we can not explicitly model the channel through which older mothers' children have a different school attachment with respect to younger mothers' ones, because we don't have any kind of information about mothers' behavior. We simply refer to changes in children's education associated with variation in mothers' age at first birth, so to get some insights about the sign of the correlation between mothers' age at birth and children's educational outcomes.

From the census data we retrieve information on all households' members and we regress years of schooling of first born children against their mothers' age when at birth, so to observe whether kids born to older mothers have different educational attainment with respect to those born to younger ones. Given that education is increasing with age, the dependent variable considered here refers to an index that counts years of school on schooling age as follows:

$$index_i = \frac{edu_i}{age_i - 6}$$

where edu_i indicates years of education of first born child i. This index is bounded between 0 and 1, where 1 means that a child has the maximum possible level of schooling given his age.

This model is estimated by ordinary least square:

$$index_i = \beta_0 + \sum_{k=13}^{K} A_{ki}\beta_{ki} + C_i'\beta + u_i \qquad (4)$$

where A_i is first born child i's mother age at birth, C_i is a matrix of other controls for child's family characteristics which includes: sex, number of siblings currently living in the household, family size (and its square to capture crowding effects), the mother's working status, a dummy that indicates if the father lives in the same household or not, the educational level of the household's head, the number of teachers relative to school age people in each *canton*, the wealth index, a dummy for living in rural or urban area and controls for province of birth. We also analyze if currently working and non working mothers behave differently, but this is not the case and results are not shown.

We explore the effect of mother's age at first birth on the sample of firstborns and we include kids from age 7 to age 17. K in equation 4 is equal to 25. [18]

The results confirm that when a woman decides to postpone her first birth, the firstborns' educational outcome improves. The schooling index increases significantly if the first pregnancy is delayed from age 17 onwards, and after age 20 the relationship flattens. We can interpret this as the

[17] See the review by Strauss and Thomas (1995).

[18] This choice takes into account that for mothers aged 32 and over it is not possible to retrieve information on age at first birth correctly since after that age the share of those with children that already left the household is too high. So until age 32 we can reasonably assume that even the youngest eldest child in the household is actually the first born. Note that we can count schooling years only of people aged 7 and over (which is exactly why we choose age 25 as the maximum mother's age at first birth). We also from the sample all the households where the declared number of children and the children that are currently living in the household diverge.

evidence indicating that women postponing their first birth do invest more in their children, even if in this model the relationship is not causal.

The graph below (Figure 3) represents the coefficients of mother age at birth on child's schooling obtained by the estimation (the base category for mothers age at first birth is 12).

This evidence confirms children benefits if motherhood is shifted away from early adolescence: giving birth at age 20 rather then at age 12 is associated to a 10% increase in children educational level, no matter their current grade (complete results are in Table 10 in the Appendix). This suggests that avoiding early childbearing could promote children enrollment and literacy.

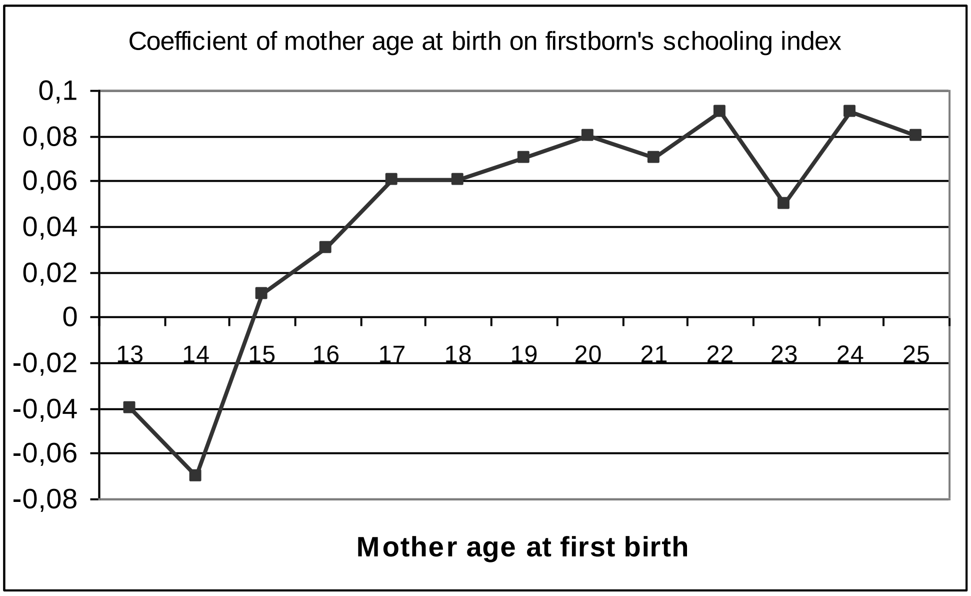

Figure 3 Estimated coefficients of mothers' age at birth on firstborns education index

5 Conclusions

This work sheds light on the effects of education on women early fertility choices, labor market participation rates and their children outcomes. Education is found to be endogenous with respect to fertility behavior and so it has been instrumented with a reform that increased women's educational level.

Results show that schooling negatively affects women's risk of being teenage mothers and encourages women labor supply. When age at first birth is delayed after adolescence children schooling is improved. However, further evidence is needed in order to estimate precisely the size and the duration of the intergenerational effects.

So from a policy perspective it is evident that a reform on compulsory education is a good tool not only to promote schooling attendance itself, but also to increase women control over family planning choices and participation in the labor market. This is particularly relevant in a country where gender differences in both the labor market and educational attainment are pronounced, and where there is evidence that children enrollment is not yet satisfactory and their exploitation in the labor market still high.

6 Appendix

Summary statistics for the sample of women aged 24-31	
Average years of schooling	8.5
Percentage of women targeted by the reform	53
Percentage of teenage mothers	27
Percentage of working women	39
Non working, non teenage mothers	*41*
Working, non teenage mothers	*32*
Non working, teenage mothers	*20*
Working, teenage mothers	*7*
Average Age	27.3
Percentage of women living in a city	82
Average wealth index	0.46
Share of doctors and nurse by 1000 people, by canton	2.56
Percentage of women living in Guayaquil or Quito	38
Percentage of married women by canton	72
Observations	29162

Table 5 Summary statistics for the sample of women aged 24-31; source: IPUMSI, Ecuador 1990.

	SCHOOLING EQUATIONS						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Cohorts ±2 years	Cohorts ±3 years	Cohorts ±4 years	Cohorts ±5 years	"Shifted" reform 1	"Shifted" reform 2	Randomized reform
Reform	0.29**	0.27**	0.32***	0.29***			
	(0.131)	(0.116)	(0.101)	(0.101)			
False reform					0.05	0.05	0.03
					(0.094)	(0.115)	(0.052)
Age	-1.48	1.79***	0.70**	0.66***	0.38	-0.56**	0.70
	(1.562)	(0.644)	(0.352)	(0.233)	(0.323)	(0.238)	(0.452)
age2	0.03	-0.03***	-0.01**	-0.01***	-0.01	0.01*	-0.01*
	(0.028)	(0.012)	(0.006)	(0.004)	(0.005)	(0.004)	(0.008)
Share of doctors and nurse by 1000 people, by canton	0.17***	0.19***	0.22***	0.22***	0.21***	0.20***	0.21***
	(0.065)	(0.059)	(0.047)	(0.040)	(0.049)	(0.046)	(0.047)
Share of married women, by canton	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Wealth index	2.07***	2.05***	2.02***	1.99***	2.07***	2.10***	2.02***
	(0.079)	(0.068)	(0.067)	(0.060)	(0.061)	(0.059)	(0.066)
Urban vs. rural status	0.57***	0.52***	0.59***	0.62***	0.56***	0.54***	0.59***
	(0.110)	(0.107)	(0.103)	(0.088)	(0.093)	(0.083)	(0.104)
Living in Guayaquil or Quito	0.22	0.19	0.08	0.03	0.14	0.18	0.09
	(0.248)	(0.238)	(0.206)	(0.180)	(0.200)	(0.178)	(0.207)
Controls for province of birth	yes	yes	yes	yes	yes	yes	yes
Constant	26.82	-17.68**	-3.26	-2.58	2.15	15.75***	-2.22
	(21.658)	(8.755)	(4.784)	(3.301)	(4.640)	(3.631)	(6.158)
Sigma	4.21***	4.21***	4.18***	4.13***	4.22***	4.24***	4.18***
	(0.041)	(0.041)	(0.043)	(0.047)	(0.037)	(0.032)	(0.043)
Observations	13,948	22,845	29,162	36,284	28,272	26,658	29,162
Pseudo R-squared	0.071	0.072	0.071	0.070	0.072	0.073	0.071

Robust standard errors in parentheses. Errors are clustered at Province-treatment level
*** p<0.01, ** p<0.05, * p<0.1

Table 6 First stage schooling equation: different age cohorts on each side of the reform pivotal year (columns 1-4). Robustness checks (columns 5-7): false treatment is not affecting schooling.

MODELS FOR TEENAGE MOTHERHOOD

	(1)	(2)	(3)	(4)	(5)	(6)
	Marg. Eff	Marg.eff	Marg.eff.	Coeff		Marg eff.
Dependent variable:	being mother before age 16 (incarceration)	being mother before age 19 (exclusion)	being mother before age 19 NON-IV	being mother before age 19	Years of schooling	being mother before age 19
Reform	-0.00	0.00			0.28***	
	(0.008)	(0.012)			(0.073)	
Years of schooling		-0.02***	-0.02***	-0.03***		-0.01***
		(0.001)	(0.001)	(0.003)		(0.001)
Age	-0.01	0.03	0.03	0.05	0.72**	0.02
	(0.028)	(0.042)	(0.042)	(0.134)	(0.300)	(0.043)
age2	0.00	-0.00	-0.00	-0.00	-0.01**	-0.00
	(0.001)	(0.001)	(0.001)	(0.002)	(0.005)	(0.001)
Share of doctors and nurse by 1000 people, by canton	-0.00	-0.00	-0.00	-0.01	0.19***	-0.00
	(0.002)	(0.004)	(0.004)	(0.013)	(0.040)	(0.004)
Share of married women, by canton	0.00	0.00***	0.00***	0.00***	-0.02***	0.00***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.003)	(0.000)
Wealth index	-0.02***	-0.02***	-0.02***	-0.14***	1.65***	-0.05***
	(0.001)	(0.004)	(0.004)	(0.015)	(0.056)	(0.005)
Urban vs. rural status	0.01	0.02***	0.02***	0.04	0.49***	0.01
	(0.004)	(0.009)	(0.009)	(0.028)	(0.087)	(0.009)
Living in Guayaquil or Quito	-0.00	-0.01	-0.01	-0.05	-0.01	-0.02
	(0.005)	(0.013)	(0.013)	(0.038)	(0.202)	(0.012)
Controls for Province of birth	yes	yes	yes	yes	yes	yes
Sigma					1.28***	
					(0.008)	
Corr(fert,schooling)				-0.20***	-0.20***	-0.20***
				(0.017)	(0.017)	(0.017)
Observations	29,162	29,162	29,162	29,162	29,162	29,162

Robust standard errors in parentheses. Errors are clustered at Province of birth-treatment level

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

Table 7 Fertility equations: column (1) direct impact of the reform on the probability to become mother before the end of compulsory schooling; column (2) probit estimates for the probability of being mother before age 19 with the reform (to test exclusion restriction); column (3) probit estimates for the probability of being mother before age 19; (4) – (5) IV estimates: coefficients; (6) IV estimates: marginal effects.

MODELS FOR LABOR SUPPLY

VARIABLES	(1) labor supply NON IV	(2) labor supply Exclusion	(3) IV Coeff labor supply	(4) IV Coeff years of schooling	(5) IV Marg eff labor supply
Years of schooling	0.03***	0.03***	0.02***		0.01***
	(0.002)	(0.002)	(0.005)		(0.002)
Reform		0.01		0.25***	
		(0.010)		(0.074)	
Age	0.02	0.02	0.09	0.75**	0.03
	(0.029)	(0.027)	(0.083)	(0.308)	(0.032)
age2	-0.00	-0.00	-0.00	-0.01**	-0.00
	(0.001)	(0.000)	(0.002)	(0.006)	(0.001)
Share of doctors and nurse by 1000 people, by canton	0.02***	0.02***	0.05***	0.19***	0.02***
	(0.005)	(0.005)	(0.013)	(0.041)	(0.005)
Share of married women, by canton	-0.00***	-0.00***	-0.01***	-0.02***	-0.00***
	(0.000)	(0.000)	(0.001)	(0.003)	(0.000)
Wealth index	0.04***	0.04***	0.21***	1.65***	0.08***
	(0.005)	(0.005)	(0.012)	(0.056)	(0.005)
Urban vs. rural status	-0.01	-0.01	0.01	0.49***	0.00
	(0.013)	(0.013)	(0.035)	(0.086)	(0.013)
Living in Guayaquil or Quito	0.03*	0.03*	0.08	-0.01	0.03
	(0.016)	(0.016)	(0.047)	(0.202)	(0.018)
Controls for province of birth	yes	yes	yes	yes	yes
Constant			-1.19	-3.30	
			(1.171)	(4.208)	
Sigma				1.28***	
				(0.008)	
Corr(lab,schooling)			0.26***	0.26***	
			(0.016)	(0.016)	
Observations	29,162	29,162	29,162	29,162	29,162

Robust standard errors in parentheses. Errors are clustered at Province of birth-treatment level

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

Table 8: Labor market participation models for the probability of working: column (1) probit estimates; column (2) probit estimates with the reform (to test exclusion restriction); (3) –(4) IV estimates: coefficients; (5) IV estimates: marginal effects.

TRIVARIATE MODEL FOR SCHOOLING, TEENAGE MOTHERHOOD AND LABOR SUPPLY

	(1)	(2)	(3)	(4)	(5)
		Coefficients		Marginal effects	
VARIABLES	Labor equation	Fertility equation	Schooling equation	Labor equation	Fertility equation
Years of schooling	0.02***	-0.03***		0.01***	-0.01***
	(0.005)	(0.003)		(0.002)	(0.001)
Reform			0.25***		
			(0.069)		
Age	0.08	0.05	0.73**	0.03	0.02
	(0.083)	(0.134)	(0.306)	(0.032)	(0.043)
age2	-0.00	-0.00	-0.01**	-0.00	-0.00
	(0.002)	(0.002)	(0.005)	(0.001)	(0.001)
Share of doctors and nurse by 1000 people, by canton	0.05***	-0.01	0.19***	0.02***	-0.00
	(0.012)	(0.013)	(0.041)	(0.005)	(0.004)
Share of married women, by canton	-0.01***	0.00***	-0.02***	-0.00***	0.00***
	(0.001)	(0.001)	(0.003)	(0.000)	(0.000)
Wealth index	0.21***	-0.14***	1.65***	0.08***	-0.05***
	(0.012)	(0.015)	(0.056)	(0.004)	(0.005)
Urban vs. rural status	0.01	0.04	0.49***	0.00	0.01
	(0.035)	(0.028)	(0.086)	(0.013)	(0.009)
Living in Guayaquil or Quito	0.08	-0.05	-0.01	0.03	-0.02
	(0.047)	(0.038)	(0.203)	(0.018)	(0.012)
Controls for province of birth	yes	yes	yes	yes	yes
Constant	-1.17	-1.43	-2.99		
	(1.172)	(1.802)	(4.212)		
Sigma			1.28***		
			(0.008)		
rho(lab,fert)	-0.15***	-0.15***	-0.15***		
	(0.015)	(0.015)	(0.015)		
rho(lab,schooling)	0.26***	0.26***	0.26***		
	(0.016)	(0.016)	(0.016)		
rho(fert, schooling)	-0.20***	-0.20***	-0.20***		
	(0.017)	(0.017)	(0.017)		
Observations	29,162	29,162	29,162	29,162	29,162

Robust standard errors in parentheses. Errors are clustered at Province of birth-treatment level
*** p<0.01, ** p<0.05, * p<0.1

Table 9 Estimates for the trivariate model of labor supply, fertility and schooling. Columns (1)-(3) report coefficients, while columns (4) and (5) marginal effects.

CHILDREN'S SCHOOLING EQUATION

	(1)	(2)
VARIABLES	Dependent variable: Firstborns education	
	coefficients	st. err
Mother's age at first birth 13	-0.04	(0.040)
Mother's age at first birth 14	-0.07**	(0.033)
Mother's age at first birth 15	0.01	(0.028)
Mother's age at first birth 16	0.03	(0.028)
Mother's age at first birth 17	0.06**	(0.027)
Mother's age at first birth 18	0.06**	(0.026)
Mother's age at first birth 19	0.07***	(0.026)
Mother's age at first birth 20	0.08***	(0.028)
Mother's age at first birth 21	0.07***	(0.027)
Mother's age at first birth 22	0.09***	(0.029)
Mother's age at first birth 23	0.05*	(0.030)
Mother's age at first birth 24	0.09***	(0.032)
Mother's age at first birth 25	0.08***	(0.031)
Reference category: mother's age at first birth 12		
Female vs. male	0.01**	(0.005)
Number of own siblings	-0.01***	(0.003)
Number of own siblings <5	-0.02***	(0.004)
Family size	0.01*	(0.006)
Family size^2	-0.00	(0.000)
Mother works	-0.02***	(0.006)
Father lives in the same house	0.01	(0.007)
Education of household head	0.01***	(0.001)
Share of teachers per 100 pop, by canton	0.01*	(0.003)
Share of doctors and nurse by 1000 people, by canton	0.00	(0.002)
Wealth index	0.03***	(0.004)
Urban vs. rural status	0.01	(0.009)
Controls for province of birth	yes	
Constant	0.69***	(0.034)
Observations	12,759	
R-squared	0.145	

Robust standard errors in parentheses

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

Errors are clustered at Canton-level

Table 10 Children's schooling equation.

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