ÉQUIPE DE RECHERCHE SUR L'UTILISATION
DES DONNÉES INDIVIDUELLES EN LIEN
AVEC LA THÉORIE ÉCONOMIQUE

Sous la co-tutelle de :
UPEC • UNIVERSITÉ PARIS-EST CRÉTEIL
UPEM • UNIVERSITÉ PARIS-EST MARNE-LA-VALLÉE

Series of ERUDITE Working Papers

N° 15-2019

Title

Education, fertility and childlessness in Indonesia

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Abstract

This paper explores the causal relationship between education and fertility. It examines whether education reduces fertility at the intensive and extensive margins. It also investigates how education impacts age at first birth. We exploit an exogenous variation in education induced by an extension of compulsory schooling in Indonesia in 1994. As this law increased education mainly in regions that were initially lagging behind, a difference-in-differences variable based on women's year and region of birth is used to instrument education. Our results suggest that additional schooling leads to a decrease in childlessness and to a delay in first birth, but no effect is observed on achieved or desired fertility. With regard to the mechanisms, better-educated women are more attractive on the marriage market, which explains why they are less often childless. While no effect is found on the labor market, education increases contraceptive use and women's decision-making authority.

Keywords: Education, childlessness, fertility, labor market, marriage, Indonesia

JEL classification: I20, I25, J12, J13, J24, N35

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1 Introduction

These past decades, fertility has declined in many developing countries. In Indonesia, for example, fertility rate dropped from 5.7 children in 1960 to 2.4 in 2016. At the same time, education has significantly increased. A large and well documented literature, using exogenous sources of variation in education, has shown that female education has a direct impact on fertility. Breierova and Duflo (2004) and Osili and Long (2008), for instance, find that education leads to a delay in fertility in Indonesia and Nigeria. Similarly, in Kenya, Ferre (2009) and Ozier (2015) show that education reduces teenage pregnancy. Chicoine (2012), Dinçer, Kaushal, and Grossman (2014), Handa (2000) and Samarakoon and Parinduri (2015) find that extra schooling reduces overall fertility in Kenya, Turkey, Jamaica and Indonesia, respectively. This rise in education could partly explain demographic transitions.

The relationship between female education and fertility is intricate as education affects fertility through many channels (Basu, 2002). Because of data limitations, empirical studies in developing countries did not investigate all these mechanisms. First, education improves individuals' labor market opportunities which increases the opportunity cost of childbearing (Becker, 1965). While this negative substitution effect could be reinforced by a quantity-quality trade-off (Becker & Lewis, 1973), it could also be partly offset by an income effect. Second, extra schooling can increase women's chances of getting married, which should positively affect fertility. Moreover, due to assortative mating, additional education increases the probability of finding a highly educated partner with a greater potential income (Behrman & Rosenzweig, 2002). Here again, the increase in partner's education has both an income and a substitution effect playing contradictory roles. Third, women who study longer may be more informed about family planning and use contraception more efficiently to reach their desired fertility (Ainsworth, Beegle, & Nyamete, 1996; Rosenzweig & Schultz, 1985, 1989; Samarakoon & Parinduri, 2015; Thomas, Strauss, & Henriques, 1991).

Whilst fertility is often apprehended by its intensive margin (number of children that women have), its extensive margin (the fraction of women who are mothers) has been overlooked by the literature. This is particularly true in developing countries, even though childlessness is not uncommon. In Indonesia, for instance, around 5% of women aged 40-49 are childless.[1] Distinguishing the extensive margin from the intensive one is paramount because their relationships with education may differ. Baudin, De la Croix, and Gobbi (2015) show that, in the United States, while fertility decreases with education, the relationship between childlessness and education is U-shaped.

We exploit an exogenous variation in schooling induced by a law introduced in 1994 in Indonesia that lengthened compulsory schooling by three years. As this law had a greater impact in regions where the level of education was initially lower, we use a difference-in-differences variable defined by both individuals' year and region of birth to instrument education.

If we find no effect on desired or achieved fertility (intensive margin), the extra induced schooling leads to a decrease in childlessness (extensive margin) and to a delayed first birth. Chances of getting married increase with additional education, which partly explains the negative effect on childlessness. Regarding the other mechanisms, while we find no effect on the labor market, education increases contraceptive use and women's decision-making authority on contraception. Our results also confirm the existence of a U-shaped relationship between childlessness and education.

We contribute to the literature in four ways. Firstly, we provide new evidence on the causal effects of education on fertility using an original instrument based on a natural experiment that allows for geographical heterogeneity. Doing so, this article complements related studies which rely on regression discontinuity designs (in developed countries) or on other instruments (in developing countries). Secondly, we study various dimensions of fertility: its extensive and intensive margins as well as its timing. Thirdly, we investigate several potential mechanisms: labor market, marriage and contraception. Fourth, like Breierova and Duflo (2004) and Samarakoon and Parinduri (2015), we study the specific case of Indonesia, a middle-income country. This study therefore brings new insights compared to studies in poorer countries such as Kenya (Chicoine, 2012; Ferre, 2009; Ozier, 2015) or Nigeria (Osili & Long, 2008).

The remaining part of this paper proceeds as follows. Section 2 describes the 1994 compulsory schooling reform and the data. Section 3 presents the empirical strategy. Section 4 discusses the results along with robustness checks. Section 5 concludes.

2 The 1994 reform and data

2.1 The Nine-Year Universal Basic reform

In 1984, in Indonesia, a law introduced six-year compulsory education for primary school age children (7-12 years old). Compulsory basic education was then expanded in 1994 to include junior secondary school (12-15 years old) (Yeom, Acedo, & Utomo, 2002). This policy, known as Nine-Year Universal Basic Education (NYUBE), targeted at getting basic education for all children aged 7 to 15 by 2004 (Yeom et al., 2002). Junior secondary school fees were also made free even though, in reality, parents still have to pay for additional expenditures in schools (school activities, maintenance, etc) (Yeom et al., 2002). This reform was supported by large Junior Secondary Education programs implemented by the World Bank and the Asian Development Bank between 1996 and 2004. During the whole period, 903 schools and 2,153 classrooms were built.

Exposure to the 1994 reform is therefore determined by individuals' year of birth. Individuals aged 15 or more in 1994 (born in 1979 or before) should not, in theory, have been impacted by the reform, contrary to those aged less than 15 in 1994. However, in Indonesia, delayed or early primary school enrollments are not rare[2], which could lead to underestimate the effect of the reform. In the baseline regressions, the control (untreated) and treated groups are reduced to include women aged 16 to 26 and 2 to 12 in 1994, respectively.

2.2 Database

The data used come from the Indonesian Family Life Survey (IFLS). It is a longitudinal survey conducted by the Research and Development Corporation (RAND) that began in 1993 and gathered information in four additional rounds: 1997, 2000, 2007 and 2014. Data were collected in 13 of the 27 provinces of Indonesia and are representative of 83% of the population. IFLS data gather information about educational background, monthly household expenditures, pregnancy and marital histories, contraceptive use and labor market experiences.

We only use the three most recent waves (2000, 2007 and 2014) because they contain information on individuals both affected and not affected by the reform. The initial sample was reduced to keep women over 19 who belong to the old or young cohorts and who provided information on their pregnancy history. To add a geographical dimension to our analysis, we only keep women whose Kabupaten (administrative subdivision of province) of birth is known. Finally, to avoid to artificially increase the number of observations by observing twice or three times the same women, only the last year of observation was kept. The final sample includes 4,597 women.[3]

Descriptive statistics are reported in Table 1. While almost all women have attended and finished primary school, around 76% of them attended junior secondary school. This proportion has been increasing over time (82% for the young cohort). Women have been pregnant on average 1.5 times and report wanting 2.6 children, suggesting that some sample women, especially in the young cohort, will have additional children in the future. This could partly explain why 22% of women are childless. The sample is relatively evenly divided between women who marry up, those who marry down and those who marry a husband with the same level of education. Women in the young cohort tend to marry up less often than those in the old cohort which could suggest that the law disproportionally moved girls into lower secondary school.

Table 1: Descriptive statistics - women

Sample	All	Young cohort	Old cohort	Mean diff. Old-young
Age	30.04	26.61	38.95	12.34***
	(6.69)	(3.19)	(4.98)	(0.12)
Education: went to junior secondary school	0.76	0.82	0.62	-0.20***
	(0.43)	(0.39)	(0.49)	(0.01)

Continued on next page

Following the previous table

Sample	All	Young cohort	Old cohort	Mean diff. Old-young
Education: finish junior secondary school	0.73	0.79	0.59	-0.20***
	(0.44)	(0.41)	(0.49)	(0.01)
Years of education	10.28	10.76	9.05	-1.72***
	(4.02)	(3.77)	(4.38)	(0.13)
No. of pregnancies	1.52	1.20	2.33	1.13***
	(1.33)	(1.00)	(1.70)	(0.04)
Childlessness	0.22	0.25	0.14	-0.11***
	(0.41)	(0.43)	(0.34)	(0.01)
More than one pregnancy[a]	0.54	0.44	0.79	0.35***
	(0.50)	(0.50)	(0.41)	(0.02)
No. of desired children	2.55	2.49	2.69	0.19***
	(1.02)	(0.91)	(1.23)	(0.04)
Age at first birth	23.29	22.64	24.83	2.19***
	(4.24)	(3.26)	(5.66)	(0.15)
Married	0.84	0.83	0.88	0.05***
	(0.36)	(0.38)	(0.32)	(0.01)
Spouse's education	9.85	10.12	9.22	-0.90***
	(4.04)	(3.82)	(4.44)	(0.15)
Marry down	0.32	0.33	0.29	-0.04***
	(0.47)	(0.47)	(0.45)	(0.02)
Marry same	0.37	0.38	0.35	-0.04**
	(0.48)	(0.49)	(0.48)	(0.02)
Marry up	0.31	0.28	0.37	0.08***
	(0.46)	(0.45)	(0.48)	(0.02)
Ever worked	0.86	0.85	0.86	0.01
	(0.35)	(0.35)	(0.34)	(0.01)
Ever used contraceptive method	0.80	0.82	0.77	-0.04***
	(0.40)	(0.39)	(0.42)	(0.01)
Age when first use modern contraceptive method	23.72	22.77	26.02	3.25***
	(4.51)	(3.34)	(5.93)	(0.17)
Involved in deciding whether using contraception	0.75	0.77	0.71	-0.05***
	(0.43)	(0.42)	(0.45)	(0.02)
Observations	4597	3319	1278	4597

Notes: Standard deviations are reported in parentheses except for average differences (column 4) where standard errors are reported in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: Among women with at least one pregnancy.

3 Empirical strategy

3.1 Sources of exogenous variation in years of education

To cope with endogeneity issues, we exploit an exogenous variation in years of schooling caused by the extension of compulsory education in Indonesia in 1994. Several articles, mainly in developed countries, have used compulsory school laws as natural experiments to identify the causal impact of education on fertility (Black, Devereux, & Salvanes, 2008;

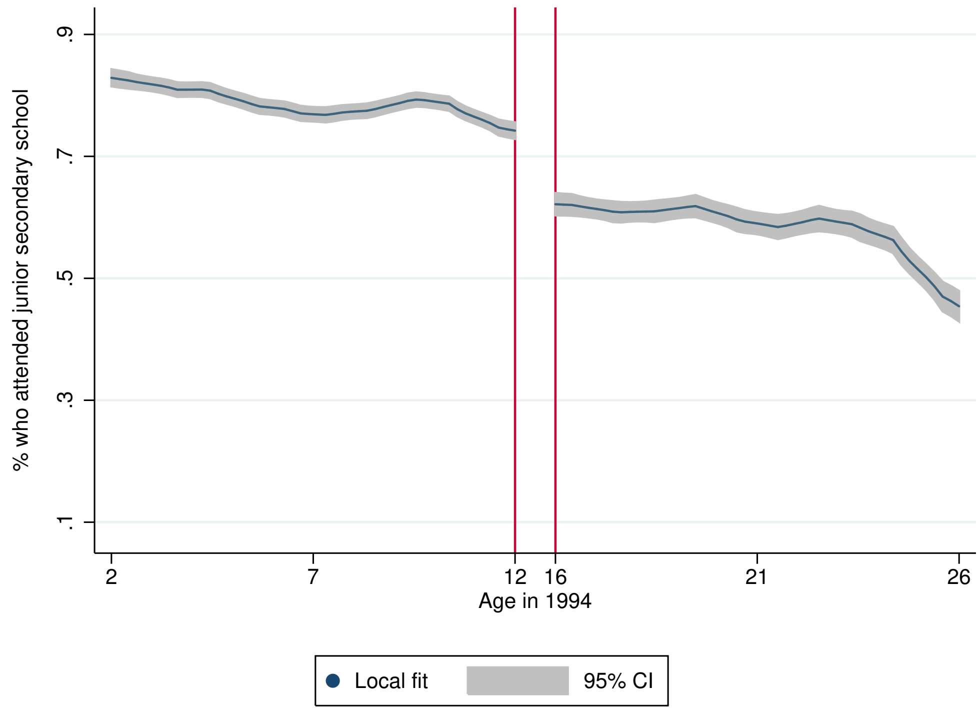
Braakmann, 2011; Cygan-Rehm & Maeder, 2013; DeCicca & Krashinsky, 2015; Dinçer et al., 2014; Fort, Schneeweis, & Winter-Ebmer, 2011; McCrary & Royer, 2011; Monstad, Propper, & Salvanes, 2008). A simple graphical analysis shows that the percentage of women attending junior secondary school increased by 15 percentage points after the reform (Figure 1 (a)). However, when looking at years of education, we do not observe a clear jump but only a slight increase of less than one year of education (Figure 1 (b)).[4] A possible explanation for this absence of discontinuity might be that many individuals were already meeting the requirements of the reform before it was implemented: 59% of women in the old cohort completed junior high school. It prevents us from using a regression discontinuity design.

We therefore consider a second source of variation determining women's exposure to the reform: her region of birth. Intuitively, in regions where studying for nine years was the norm, the reform should not have had any (or a smaller) impact in comparison with regions where individuals were on average studying less than nine years at the time of the reform. The intensity of the program is therefore assumed to vary across regions of birth because of differences in initial level of education. We use the 1993 Indonesian Family Life Survey to compute, by Kabupaten, the average years of education before the reform. This average is computed using all the individuals aged 19 or more in order to ensure that most of them left school or at least have completed secondary school.[5] Using regions of birth, we are able to compute the initial level of education in 13 provinces and 153 Kabupaten.[6] We observe a real heterogeneity across regions (Figure 1.A2, Annex). In regions lagging behind, more effort was made to achieve junior secondary universal education, making it relevant to differentiate regions by their initial level of education. For instance, the Junior Secondary Education Projects implemented by the World Bank and the Asian Development Bank between 1996 and 2004 were more important in these regions.

As expected, the jump in junior secondary school enrollment following the reform is greater in regions where the initial levels were low (Figure 2 (a)). In the most educationally backward regions, enrollment increased by almost 20 percentage points. In comparison, it increased by only 5 percentage points in the most advanced regions. We now observe a slight jump in years of schooling for the regions with the lowest initial level of schooling (Figure 2 (b)). In these regions, education increased by almost one year and a half after the reform. When intermediary cohorts are included, upward trends in educational outcomes quickened after the reform (Figure 1.A3, Annex).

The identification strategy therefore uses two sources of variation: a temporal and a geographical variation. This strategy is illustrated in Table 2. If education has increased overtime, this increase has been lower in regions where the initial level of schooling was higher (negative difference-in-differences).

(a) Junior secondary school



(b) Years of schooling

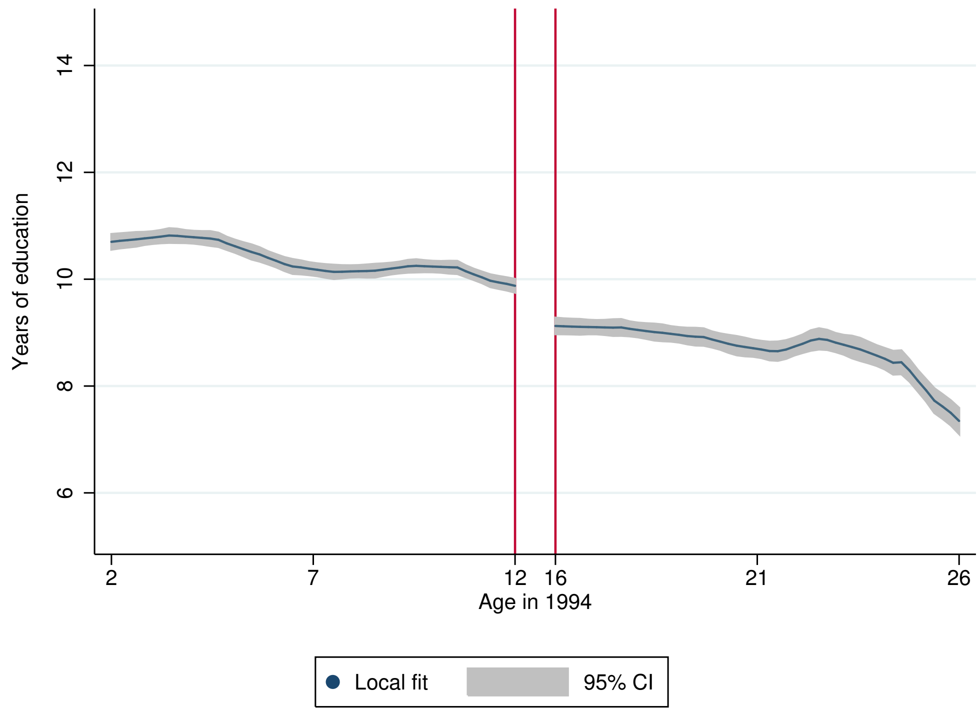
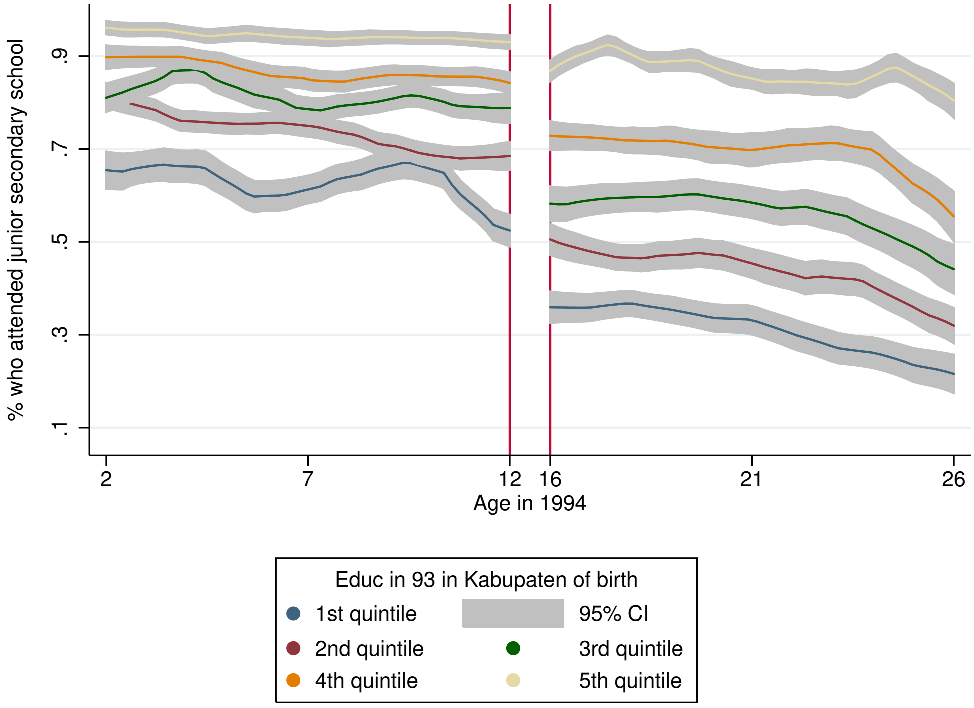


Figure 1: Evolution of schooling

(a) Junior secondary school



(b) Years of schooling

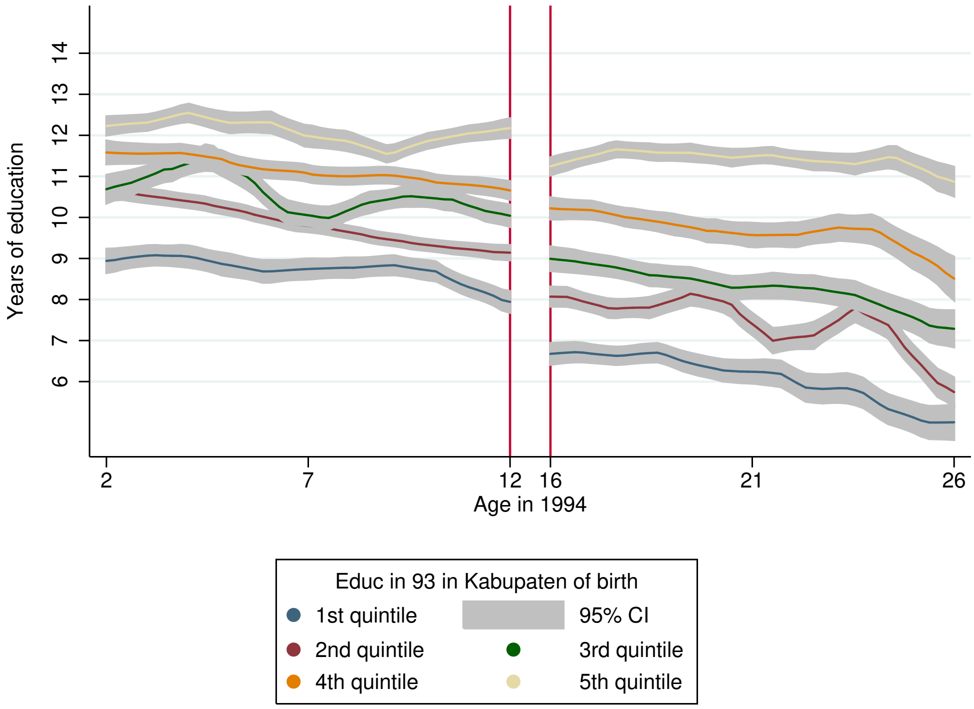


Figure 2: Evolution of schooling by Kabupaten of birth (quintile)

Table 2: Means of education by cohort and region of birth

Sample:	Level of education in Kabupaten of birth: Low	High	Diff
	Women	Women	Women
Aged 2-12 in 94	9.80	12.04	2.24
	(0.19)	(0.26)	(0.32)
Aged 16-26 in 94	7.68	10.90	3.22
	(0.22)	(0.26)	(0.34)
Difference	2.02	1.14	-0.88
	(0.19)	(0.27)	(0.32)

Notes: Standard errors are reported in parentheses. Kabupaten with low level of education in 1993 are Kabupaten with an average of education below 6 years in 1993.

3.2 Empirical model

The 1994 reform is used to investigate the causal effect of an increase in education on fertility behaviors. If a jump in years of schooling was observed when the reform was implemented, we could rely on a fuzzy regression design (Braakmann, 2011; Cygan-Rehm & Maeder, 2013; McCrary & Royer, 2011). However, as suggested before, such a jump is only observed in regions with an initial low level of schooling. We therefore rely on a different method using the reform as an instrument to education while adding a geographical dimension. This empirical strategy is similar to the one employed by Bleakley (2010) to study the effects of malaria-eradication campaigns on labor productivity.

The first-stage equation can be modeled by the following equation:

$$E_{ick} = \beta_0 + \beta_1(Young_c * InitialEduc_k) + \beta_2 a_c + \beta_3 r_k + \beta_4 X_{ick} + v_{ick} \quad (1)$$

Where E_{ick} represents the number of years of education of woman i in cohort c and born in Kabupaten k. $Young_c$ is a dummy variable that indicates whether cohort c was affected by the educational reform. $Young_c$ equals one if the woman was aged 2 to 12 in 1994 (exposed) and zero if she was aged 16 to 26 in 1994 (unexposed). $InitialEduc_k$ represents the initial level of education before the reform in the Kabupaten k (average years of education in 1993). a_c is a vector of year of birth fixed effects that allows to control for temporal trends common to all regions (for instance national development programs). r_k is a vector of region of birth fixed effects that controls for region of birth-specific characteristics that do not change over time (initial regional supply of education, initial development in the region, etc). X_{ick} is a vector of other characteristics potentially affecting E_{ick}. In all specifications, this vector includes current age of the woman. Finally, v_{ick} is the error term. The coefficient β_1 represents the impact of being affected by the reform and being born in regions where the initial level of education increased by one year. The interaction term $Young_c * InitialEduc_k$ can be interpreted as a (continuous) measure of the intensity of the reform. We expect β_1 to be negative: the higher the initial level of education in the region of birth, the lower the impact of the reform.

In the second stage, we estimate the impact of the increases in education induced by the reform on fertility behaviors:

$$Y_{ick} = \alpha_0 + \alpha_1 \widehat{E_{ick}} + \alpha_2 a_c + \alpha_3 r_k + \alpha_4 X_{ick} + u_{ick} \tag{2}$$

Y_{ick} represents different fertility outcomes for woman i in cohort c born in Kabupaten k. Other notations have already been defined. The excluded instrument is the interaction variable ($Young_c * InitialEduc_k$). α_1 measures the effect of increases in education due to the regional impact of the reform on fertility behaviors.

Several assumptions need to be made for this empirical strategy to be valid. First, with regard to the first-stage equation (equation (1)), we assume that, in the absence of the reform, trends in educational outcomes would have been the same in both regions (common trends assumption). This key assumption would be violated if regions where the level of education was low were already catching up before the reform. If so, even in the absence of the reform, education would have increased more rapidly in "treated" regions and the effect of the reform would be overestimated. Placebo tests are implemented to test for the existence of differences in trends before the reform by comparing several untreated cohorts (Table 1.B1, Annex).[7] The results suggest that our estimates are not driven by systematic differences between regions. Moreover, geographical differences are believed to capture something more than a catch-up phenomenon as large programs of school and classroom construction were implemented in regions lagging behind by the World Bank and the Asian Development Bank. We also assume that no time varying and region-specific omitted variables are correlated with the interaction variable. This assumption is violated if other regional programs impacting education were implemented at the same time as the reform (health policies for instance). In this case, the coefficient in equation (1) could capture the effect of these other programs (upward bias). Even though it is not possible to include Kabupaten-specific trends, as a robustness check, we include province-specific trends which capture the effects of other programs implemented at the provincial level.

Concerning the second stage equation (equation (2)), we assume that, in the absence of the reform, similar trends in fertility would have been observed. This assumption is violated if regions lagging behind had a higher initial level of fertility and therefore could have experienced a faster decrease in fertility even in the absence of the program. Placebo tests show that there are no differential trends in fertility between cohorts that were not exposed to the reform (Table 1.B2, Annex).

4 Results

4.1 First-stage regression

The first-stage regressions are reported in Table 3. F-statistics are above 10 for all estimates. As expected, the reform had a greater impact in regions where education was initially low.[8] These results suggest that the reform could have changed the social norms concerning schooling in regions that were initially lagging behind. It is also possible that, in these regions, the law was more strongly enforced or more efforts were made from a supply point of view (construction of schools, increased spending on education, etc). These results suggest that the reform helped the regions that were lagging behind to catch up.[9]

Table 3: First-stage estimates

Estimator:	IV - First stage				
Dep. Var (First stage):	Years of education				
Dep. Var (Second stage)	No. of pregnancies	Childlessness	More than one pregnancy[a]	No. of desired children	Age at first birth
Young cohort*level of educ in birth Kabu in 93	-0.346***	-0.346***	-0.303***	-0.329***	-0.286***
	(0.075)	(0.075)	(0.074)	(0.073)	(0.073)
Observations	4597	4597	3598	3838	3461
R^2	0.27	0.27	0.27	0.27	0.27
Mean outcome	10.28	10.28	9.83	9.98	9.86
No. of clusters	150	150	150	150	150
First stage stat					
F-stat	21.182	21.182	16.662	20.052	15.445
P-value associated with F-stat	0.000	0.000	0.000	0.000	0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01. [a]: sample is restricted to women who have been pregnant at least one.

Following Angrist, Imbens, and Rubin (1996), we can distinguish four behaviors depending on how women would adapt their schooling decisions when compelled by the compulsory education reform. Some of them, the never-takers, would never attend junior secondary school even if the reform is implemented. Others, the always-takers, would attend junior secondary school even in the absence of the reform. A third group, the compliers, would attend junior secondary school only if compelled by the reform. Finally, the last group includes those defying systematically the law: they would attend school in the absence of the reform and would not if compelled by the law. Assuming the absence of defiers, we can estimate the proportion of compliers, never-takers and always-takers in the population (Table 4). Overall, 14% of the women decided to attend junior secondary school because they were compelled by the 1994 reform, a significant proportion compared to similar studies (Acemoglu & Angrist, 2001; Angrist & Krueger, 1991). These overall statistics hide a regional heterogeneity. In regions lagging behind, fewer women would have enrolled if not compelled by the compulsory education reform (40%) and 25% of them decided to enroll because of the reform.

Table 4: Compliers, always takers and never takers

	(1) % always takers	(2) % never taker	(3) % compliers
Sample: Birth Kabupaten			
All	63%	23%	14%
Lowest tercile of educ in 93	40%	35%	25%
Medium tercile of educ in 93	59%	20%	21%
Highest tercile of educ in 93	81%	10%	8%

Notes: Kabupaten belonging to the lowest tercile are Kabupaten with an average of years of education in 1993 below 4.2. Kabupaten belonging to the medium tercile are Kabupaten with an average of years of education in 1993 between 4.2 and 5.9. Kabupaten belonging to the highest tercile are Kabupaten with an average of years of education in 1993 above 5.9.

Table 5: Main results (second-stage)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimator:	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]		No. of desired children		Age at first birth	
Years of education	-0.047***	-0.013	0.020***	-0.054**	-0.009***	-0.026	0.003	0.034	0.314***	1.305***
	(0.007)	(0.091)	(0.002)	(0.023)	(0.002)	(0.030)	(0.005)	(0.059)	(0.022)	(0.412)
Observations	4597	4597	4597	4597	3598	3598	3838	3838	3461	3461
Mean outcome	1.52	1.52	0.22	0.22	0.54	0.54	2.55	2.55	23.29	23.29
No. of clusters	150	150	150	150	150	150	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		21.182		21.182		16.662		20.052		15.445
P-value associated with F-stat		0.000		0.000		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: sample is restricted to women who have been pregnant at least one.

4.2 Fertility, childlessness and age at first birth

Table 5 reports the results for fertility outcomes. For each outcome, the first column reports the results from a simple OLS regression while the second column presents the results from the IV estimates. In line with Osili and Long (2008), the increase in education induced by the reform has neither a significant effect on achieved and desired fertility nor on the probability of having more than one pregnancy. However, we observe an impact on the probability of being childless (no pregnancy). An additional year of education reduces the probability of being childless by around 5.4 percentage points. Once instrumented, the effect of education on childlessness changes from being positive to being negative. This could be due to the omission of an unobserved variable affecting both schooling and the probability of being childlessness positively.[10] For instance, education improves women's labor opportunities and, independently of their education, women with greater employment prospects prioritize their careers over getting pregnant. Better-educated women may also be more informed about contraceptive methods and therefore have more control over their reproductive life. We also observe a significant effect on age at first birth. An additional year of education caused by the reform is associated with a delay in their first birth by more than one year. This result is consistent with Dinçer et al. (2014); Ferre (2009); Osili and Long (2008) and Ozier (2015). OLS estimates give underestimated coefficients. One potential reason for this negative bias could be the omission of household wealth. In Indonesia, where bride price is common (Ashraf, Bau, Nunn, & Voena, 2016), richer men may be able to afford the cost of marriage at a younger age. If they marry women that are themselves more educated, it could explain a potential negative bias.

4.3 Mechanisms

Turning to the mechanisms, an additional year of education increases the likelihood of being married by 6 percentage points (Table 7). Women who get more educated are more attractive on the marriage market, which could explain why they are less often childless. When we consider only married women, the effect on childlessness disappears (Table 1.C1, Annex). As women get more educated, their spouses' profile also changes: they are more likely to marry men who are themselves more educated.[11] However, they are not more likely to marry up probably because they are themselves more educated. We find no evidence suggesting that education increases women's labor market participation.

Turning to contraception (Table 6), extra induced schooling increases women's likelihood of using contraception: one additional year of education is associated with an increase in contraceptive use by 6.5 percentage points (significant at 10% only). This finding is in line with similar studies in Turkey (Dinçer et al., 2014), Sierra Leone (Mocan & Cannonier, 2012) and Indonesia (Samarakoon & Parinduri, 2015). Education also improves women's decision-making authority on contraception: women that have been in school one more year are more likely to be involved in decisions about contraception by 4.8 percentage points.

Table 6: Impacts on contraceptive use

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator:	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	Ever used contraceptive method		Age when first use modern contraceptive methods		Involved in deciding whether using contraceptive	
Years of education	-0.007***	0.065*	0.254***	0.398	-0.002	0.048*
	(0.002)	(0.035)	(0.026)	(0.348)	(0.003)	(0.029)
Observations	3934	3934	2944	2944	3376	3376
Mean outcome	0.80	0.80	23.72	23.72	0.75	0.75
No. of clusters	150	150	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat						
F-stat		19.473		15.636		21.187
P-value associated with F-stat		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01.
Only women for which we know the educational level in the Kabupaten of birth.

Table 7: Impacts on the marriage and labor markets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimator:	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	Married		Spouse's education		Marry down		Marry up		Ever worked	
Years of education	-0.013***	0.063***	0.542***	1.066***	0.034***	0.017	-0.052***	-0.007	0.009***	-0.025
	(0.002)	(0.022)	(0.025)	(0.202)	(0.003)	(0.028)	(0.003)	(0.030)	(0.002)	(0.002)
Observations	4597	4597	3562	3069	3562	3069	3562	3069	4597	4597
Mean outcome	0.84	0.84	9.96	9.91	0.31	0.32	0.31	0.31	0.86	0.86
No. of clusters	150	150	255	150	255	150	255	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spouse's birth Kabupaten FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Spouse's year of birth FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		21.181		15.044		15.044		15.044		21.181
P-value associated with F-stat		0.000		0.000		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01.

4.4 Non linear relationship between childlessness and education

In a recent paper, Baudin et al. (2015) show that the relationship between education and childlessness is not monotonic. Below a certain education threshold, childlessness decreases with education while above it increases. This U-shaped relationship is explained by the reasons for childlessness. Beyond natural sterility, childlessness may be driven by poverty with the poorest women suffering from diseases, pregnancy-related infections, malnutrition or high mortality rates preventing them from having children. Education should relax this poverty constraint and negatively impact childlessness. The second reason for voluntary childlessness lies in a high opportunity cost of child-rearing. As education increases, labor market opportunities and potential earnings rise, as well as the opportunity cost of raising children. Therefore, when education increases, poverty-driven decreases to a minimum and then opportunity-driven childlessness increases. Because the 1994 law expanded education at a relatively level (lower secondary education), the decrease in poverty-driven childlessness may have prevailed on the opportunity-driven one. This assumption is confirmed by the results reported in Table 8. The first column shows the existence of an education threshold below which education and childlessness are negatively correlated. The three other columns confirm that our instrument is only valid for lower levels of education.

Table 8: U-shaped relation between education and childlessness

	(1)	(2)	(3)	(4)
Estimator:	OLS	IV	IV	IV
Dep. Var:	Childlessness			
Sample:Years of education	All	Less than 7	7-11	More than 11
Years of education	-0.051***	-0.084*	-1.668	0.180
	(0.006)	(0.045)	(16.678)	(0.270)
Years of education^2	0.004***			
	(0.000)			
Observations	4597	1103	1013	2481
Mean outcome	0.22	0.14	0.11	0.30
No. of clusters	150	137	140	149
Birth Kabupaten FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes
First stage stat				
F-stat		10.901	0.009	0.892
P-value associated with F-stat		0.001	0.923	0.346

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01. [a]: sample is restricted to women who have been pregnant at least one.

4.5 Robustness checks

When we use the last year of observation for each woman, we compare women aged 22-32 (young cohort) to women between 36-46 (old cohort). This could potentially explain differences in fertility outcomes. To observe both cohorts at the same age, we gather information from the 2000 and 2014 surveys for the old and young cohorts, respectively. All women are now observed when they were aged 22-32. The sample is reduced and first-stage F statistics fall under 10 for fertility at the intensive margin (Table 1.C2, Annex). Previous findings with regard to childlessness and age at first birth are not altered even though, probably because of smaller size of the sample, the coefficients are less significant.

We try other measures of fertility - number of live births, number of miscarriages and stillbirths - and the results confirm that the extra induced education had no effect on completed fertility (Table 1.C3, Annex). Province-specific linear trends are included in order to control for time-varying unobserved characteristics at the provincial level and the positive effect on age at first birth is confirmed (Table 1.C4, Annex).[12]

When intermediary cohorts are added, the results, available on demand, confirm that education reduces childlessness and increases age at first birth. Similarly, excluding individuals who start school before the official age does not change the results.[13] We also add controls for household wealth (measured when the woman was a child) and for mother's education (Table 1.C5, Annex).[14] The sample and the significance of the instrument are considerably reduced, which could explain why no effect is found on the timing of first birth (lack of power). The other results remain unchanged.

Using regions of birth may not be appropriate if households have migrated and were educated in other regions. However, regions of birth and of education are highly correlated with 93% of women who were, at age 12, still living in their Kabupaten of birth. When the sample is restricted to women who were still living in their Kabupaten of birth at age 12, the main results are not altered (Table 1.C6, Annex).

5 Concluding remarks

Compulsory education laws have been used as instrument for education in developed countries. Yet little evidence on their efficiency has been provided, especially in developing countries. In this article, we focus on a compulsory education law implemented in 1994 in Indonesia. Although this reform on average increased educational attainment - with 14% of women who decided to attend junior secondary school - , such an analysis hides a geographical dispersion. As a matter of fact, the compulsory education law had a greater impact in regions that were initially lagging behind. These results emphasize that while such policies can help to reach the Universal Primary Education goal, their effects should not be taken as granted. A particular attention is therefore needed before using them as instruments. In this paper, we do not explore the mechanisms through which such reforms are effective. In the future, it would be interesting to distinguish regions where more investments were made (constructions of schools, etc) or to distinguish regions where the law was more strongly enforced.

Among the significant results, there is no evidence that the extra induced education reduces fertility at the intensive margin but it did impact age at first birth and fertility at the extensive margin. One additional year of education increases age at first birth by more than one year and reduces women's probability of being childless by 5 percentage points. This last result may first appear counterintuitive. It is nevertheless in line with the recent literature (Baudin et al., 2015) showing that the relationship between education and childlessness is U-shaped. Indonesian better-educated women appear to be more attractive on the marriage market, which also partly explains why they remain childless less often.

With regard to other potential mechanisms, there is no evidence that education improves women's labor market participation. In this paper, we do not investigate whether education affects other dimensions of the labor market such as income, work hours, quality of jobs. Exploring these issues requires collecting this information before women start having children. Future research could perhaps investigate this question. Our findings also suggest that education slightly increases contraceptive use and women's decision-making authority on contraception. One more year of schooling increases women's likelihood of using contraception by almost 7 percentage points and the likelihood that women have decision-making authority by 4 percentage points.

Notes

[1]2012 Demographic and Health Survey

[2]41% of sample women started primary school when they were only 6 and 4% started when they were already 8.

[3]94% are observed in 2014.

[4]When cohorts aged 13 to 15 in 1994 are included, it becomes even more obvious that there was no discontinuity following the reform (Figure 1.A1, Annex).

[5]We chose not to use children's education in 1993 because of a lack of data even though the two measures are highly correlated (0.65). The consistency of the indicator we compute is confirmed when comparing it with average years of schooling and secondary school enrollment rates available in the 1994 Demographic Health Survey (high correlations of 0.90-0.91).

[6]The initial level of education by Kabupaten was computed by averaging schooling on 111 individuals in each Kabupaten on average. Indonesia counts 401 Kabupaten.

[7]We also compare trends in regions with the lowest level of education in 1993 (first quintile) with the ones in the highest (fifth quintile). Results, available on demand, show that, even though enrollment rates were increasing at a faster rate in the regions lagging behind, the difference is not statistically significant.

[8]The positive effect of belonging to the young cohort is shown by the positive impact of dummies associated with years of birth 1982-1994 (Figure 1.B2, Annex). When year of birth fixed effects are replaced by a dummy indicating if the woman belongs to the young cohort, this variable is found positive and significant: on average, it increases years of schooling by 3.

[9]These effects do not vary from one cohort to another (Figure 1.B2, Annex).

[10]It could also be that the omitted variable has a negative effect on both variables.

[11]Because women potentially unaffected by the reform (young women born in regions lagging behind) are likely to marry a husband also unaffected (older and born in the same regions), we also include controls for the spouse's year and region of birth.

[12]The effect on childlessness appears to be not significant but it is close to a significance (p-value of 0.11).

[13]Results are available on demand.

[14]We do not add all controls in the same estimates because the sample is significantly reduced.

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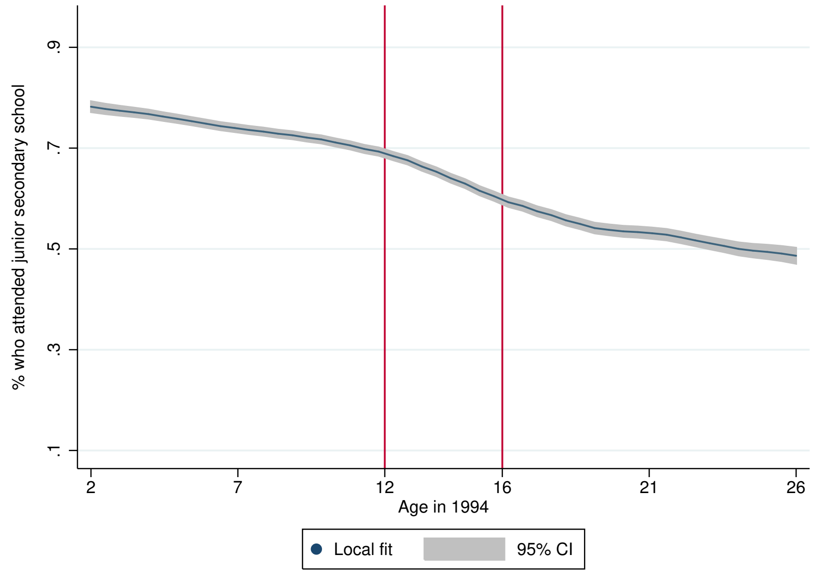
Annexes

Complementary graphical analyses

(a) Junior secondary school



(b) Years of schooling

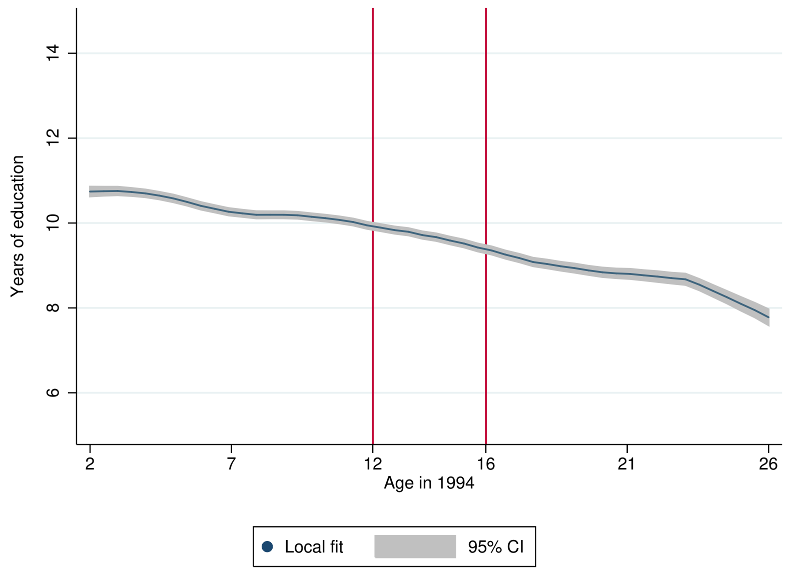
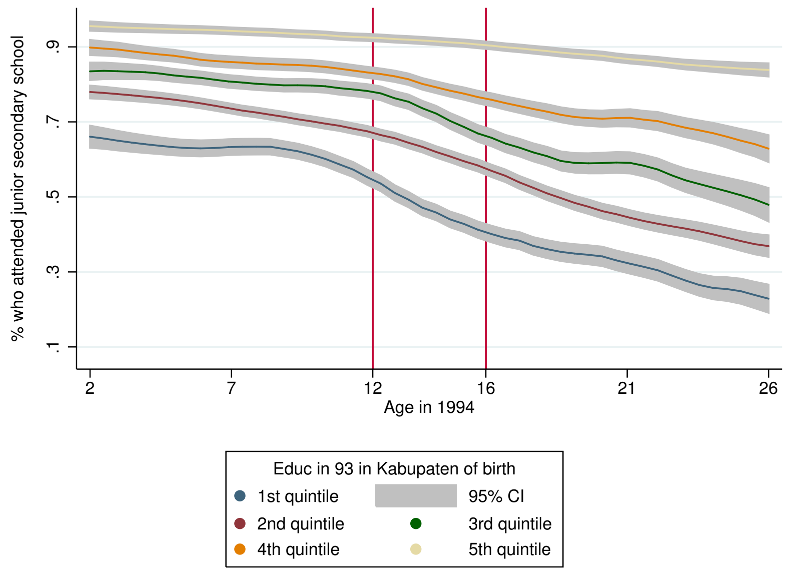


Figure 1.A1: Evolution of schooling

Figure 1.A2: Average years of education in regions in 1993

(a) Junior secondary school



(b) Years of schooling

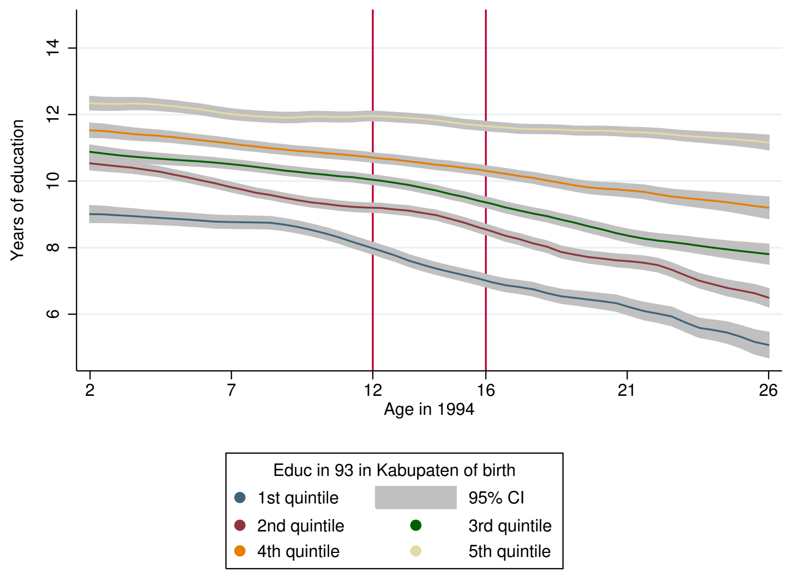


Figure 1.A3: Evolution of schooling by Kabupaten of birth (quintile)

Identifying assumptions

Table 1.B1: Placebo tests

Dep. Var:	Attend junior secondary school		Years of schooling	
Estimator: OLS/DiD	(1)	(2)	(3)	(4)
Young cohort*level of educ in birth Kabu in 93	-0.006 (0.02)	-0.012 (0.01)	-0.233 (0.14)	-0.266* (0.14)
Observations	1278	1278	1278	1278
R^2	0.351	0.351	0.403	0.404
Mean outcome	0.617	0.617	9.045	9.045
No. of clusters	147	147	147	147
Young cohort (treated group): age in 94	16-19	16-18	16-19	16-18
Old cohort (control group):	20-26	19-26	20-26	19-26
Birth Kabupaten FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01.

Table 1.B2: Placebo for number of pregnancies

Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]	
Estimator: OLS/DiD	(1)	(2)	(3)	(4)	(5)	(6)
Young cohort*level of educ in birth Kabu in 93	-0.01 (0.01)	-0.01 (0.01)	0.07 (0.05)	0.06 (0.05)	-0.00 (0.01)	-0.01 (0.02)
Observations	1283	1283	1283	1283	1109	1109
R^2	0.133	0.132	0.179	0.179	0.180	0.180
Mean outcome	0.136	0.136	2.329	2.329	0.785	0.785
No. of clusters	147	147	147	147	147	147
Young cohort (treated group): age in 94	16-19	16-18	16-19	16-18	16-19	16-18
Old cohort (control group):	20-26	19-26	20-26	19-26	20-26	19-26
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a] only women with at least one pregnancy. This table tests for pre-reform region specific trends in fertility outcomes. In each column we compare two cohorts that were not affected by the reform and assess whether the difference in fertility between these two cohorts varies according to the initial level of education of their Kabupaten of birth.

First stage - additional results

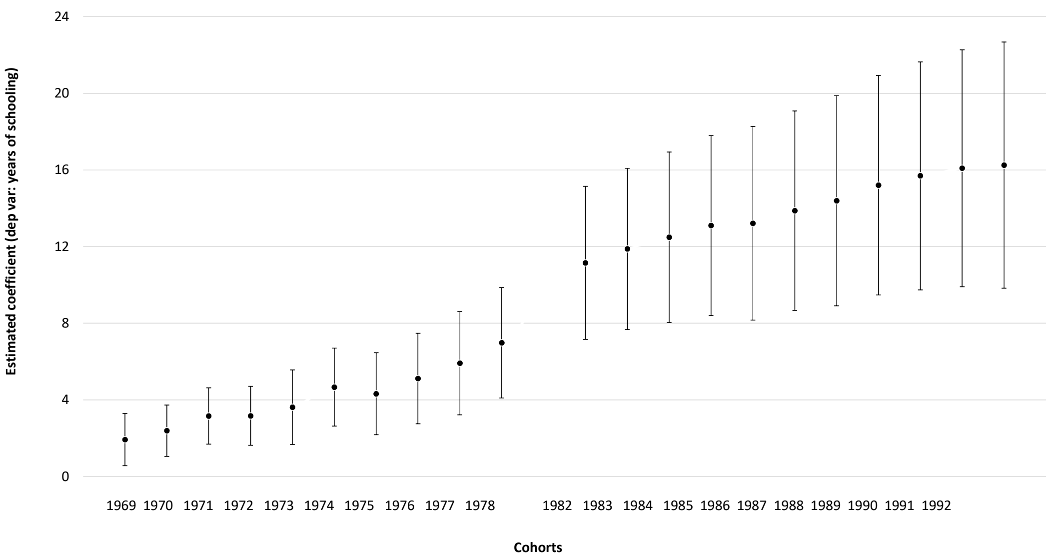


Figure 1.B1: Cohort effects

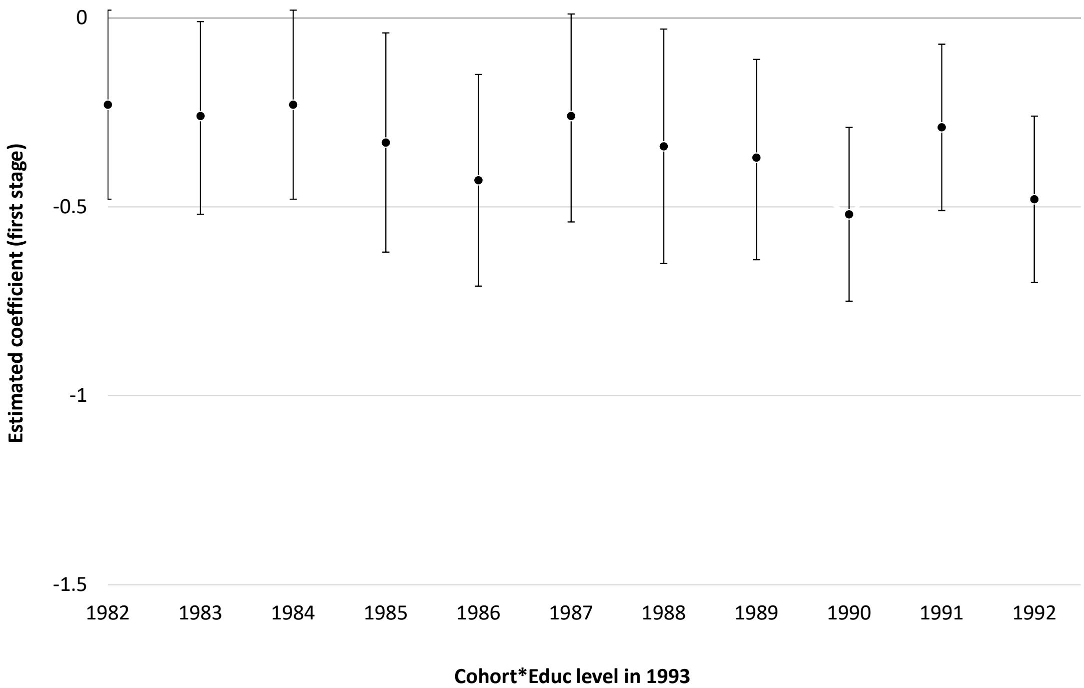


Figure 1.B2: Effect of the reform on years of education by cohort

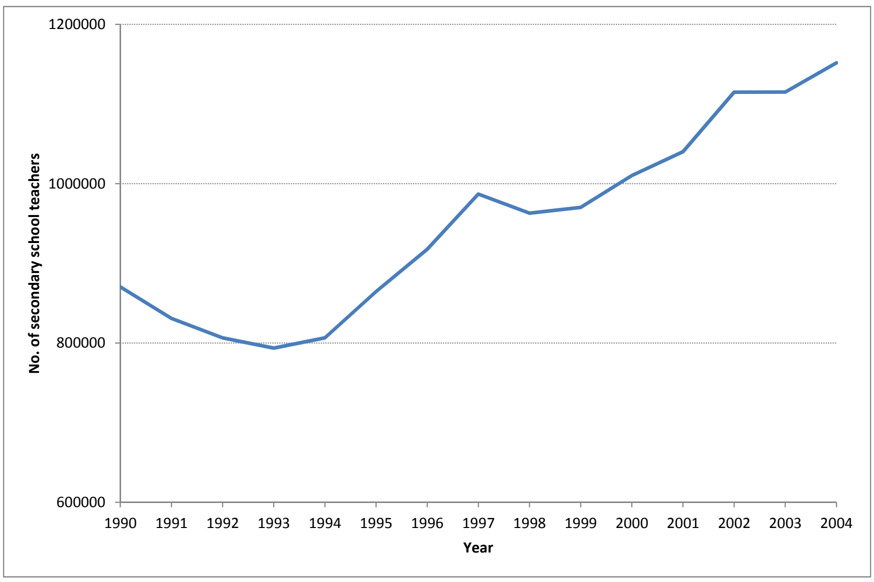


Figure 1.B3: Number of teachers in secondary schools

Source: Author, UNESCO data.

Second stage - additional results

Table 1.C1: Fertility outcomes (only for married women)

Estimator:	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(7) IV	(8) OLS	(9) IV	(10) OLS	(11) IV
Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]		No. of desired children		Age at first birth	
Years of education	-0.032***	-0.098	0.008***	-0.018	-0.009***	-0.028	0.004	0.01	0.339***	1.318***
	(0.007)	(0.104)	(0.002)	(0.019)	(0.002)	(0.029)	(0.005)	(0.065)	(0.023)	(0.487)
Observations	3876	3876	3876	3876	3443	3443	3675	3675	3317	3317
Mean outcome	1.72	1.72	0.11	0.11	0.54	0.54	2.56	2.56	23.32	23.32
5 No. of clusters	150	150	150	150	150	150	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		17.585		17.585		14.759		17.925		13.703
P-value associated with F-stat		0.000		0.000		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: sample is restricted to women who have been pregnant at least one.

Table 1.C2: Women aged 22-32 at the time of the survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimator:	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]		No. of desired children		Age at first birth	
Years of education	-0.059***	0.122	0.026***	-0.173*	-0.015***	-0.053	0.008	0.177	0.264***	1.024*
	(0.006)	(0.146)	(0.002)	(0.097)	(0.003)	(0.089)	(0.005)	(0.138)	(0.019)	(0.588)
Observations	3775	3775	3775	3775	2833	2833	3117	3117	2783	2783
Mean outcome	1.20	1.20	0.25	0.25	0.44	0.44	2.47	2.47	22.64	22.64
No. of clusters	151	151	151	151	150	150	151	151	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		3.792		21.182		3.161		5.088		15.445
P-value associated with F-stat		0.053		0.000		0.077		0.026		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: sample is restricted to women who have been pregnant at least one. Surveys used are the 2000 and the 2014 waves for the old and young cohort, respectively.

Table 1.C3: Other measures of fertility outcomes

	(1)	(2)	(3)	(4)
Estimator:	OLS	IV	OLS	IV
Dep. Var:	No. of live births		No. of miscarriages and stillbirths	
Years of education	-0.047***	0.009	0.001	-0.022
	(0.005)	(0.066)	(0.003)	(0.058)
Observations	4597	4597	4597	4597
R^2	0.26	0.23	0.09	0.07
Mean outcome	1.30	1.30	0.22	0.22
No. of clusters	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes
First stage stat				
F-stat		21.182		21.182
P-value associated with F-stat		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01.

Table 1.C4: Adding linear trends by province

	(1)	(2)	(3)	(4)	(5)	(7)	(8)	(9)	(10)	(11)
Estimator:	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]		No. of desired children		Age at first birth	
Years of education	-0.048***	-0.108	0.020***	-0.057	-0.009***	-0.015	0.003	0.055	0.315***	1.864**
	(0.007)	(0.137)	(0.002)	(0.036)	(0.002)	(0.058)	(0.005)	(0.112)	(0.022)	(0.921)
Observations	4597	4597	4597	4597	3598	3598	3838	3838	3461	3461
Mean outcome	1.52	1.52	0.22	0.22	0.54	0.54	2.55	2.55	23.29	23.29
No. of clusters	150	150	150	150	150	150	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends by province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		21.182		21.182		16.662		20.052		15.445
P-value associated with F-stat		0.000		0.000		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: sample is restricted to women who have been pregnant at least one.

Table 1.C5: Adding additional controls

Estimator:	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV	(9) OLS	(10) IV	(11) OLS	(12) IV
Dep. Var:	No. of pregnancies		Childlessness		Age at first birth		No. of pregnancies		Childlessness		Age at first birth	
Years of education	-0.052***	0.147	0.023***	-0.081**	0.268***	1.050*	-0.044***	0.283	0.018***	-0.089*	0.240***	2.034
	(0.008)	(0.125)	(0.003)	(0.037)	(0.030)	(0.578)	(0.009)	(0.199)	(0.003)	(0.053)	(0.033)	(1.447)
Log of monthly per capita expenditures when child	-0.040*	-0.162*	0.021***	0.085***	0.109	-0.323						
	(0.023)	(0.083)	(0.008)	(0.027)	(0.087)	(0.338)						
Mother's education							-0.014*	-0.148*	0.013***	0.056***	0.037	-0.661
							(0.007)	(0.082)	(0.003)	(0.022)	(0.025)	(0.566)
Observations	2980	2980	2980	2980	2089	2089	2631	2631	2631	2631	1814	1814
Mean outcome	1.38	1.38	0.27	0.27	23.40	23.40	1.29	1.29	0.29	0.29	23.40	23.40
No. of clusters	150	150	150	150	149	149	148	148	148	148	147	147
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat												
F-stat		13.286		13.286		5.694		8.377		8.377		2.923
P-value associated with F-stat		0.000		0.000		0.018		0.004		0.004		0.089

Notes: Robust clustered (on birth region) standard errors in parentheses: * p < .1, ** p < .05, *** p < .01. [a]: sample is restricted to women who have been pregnant at least one.

Table 1.C6: Women who stayed living in Kabupaten of birth at age 12

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimator:	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. Var:	No. of pregnancies		Childlessness		More than one pregnancy[a]		No. of desired children		Age at first birth	
Years of education	-0.042***	0.013	0.019***	-0.062**	-0.009***	-0.019	0.005	0.062	0.296***	1.396***
	(0.006)	(0.097)	(0.002)	(0.025)	(0.003)	(0.033)	(0.006)	(0.064)	(0.022)	(0.452)
Observations	4237	4237	4237	4237	3318	3318	3536	3536	3188	3188
Mean outcome	1.51	1.51	0.22	0.22	0.54	0.54	2.53	2.53	23.31	23.31
No. of clusters	150	150	150	150	150	150	150	150	150	150
Birth Kabupaten FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for current age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage stat										
F-stat		19.104		19.104		15.590		17.412		13.600
P-value associated with F-stat		0.000		0.000		0.000		0.000		0.000

Notes: Robust clustered (on birth region) standard errors in parentheses: * $p < .1$, ** $p < .05$, *** $p < .01$. [a]: sample is restricted to women who have been pregnant at least one.