International Institute for Applied Systems Analysis
IIASA www.iiasa.ac.at

Address: IIASA, Schlossplatz 1, A-2361 Laxenburg, Austria

Email: permissions@iiasa.ac.at

Working paper

The causal effect of primary school reforms on women reproductive behaviors in Ethiopia. Is the expansion in education quantity the primary mechanism?

Endale Kebede
endale.birhanu.kebede@univie.ac.at

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Approved by:

Name Anne Goujon
Program: Population and Just Societies Program
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Abstract

Several studies investigated the causal impacts of Africa's school reform programs on demographic outcomes. Many of the studies attributed the reform's causal effects to the post-reform expansions in the quantity of education. Nonetheless, the observed increases in school enrollment came at the expense of education quality needed to derive economic and social developments. The present study uses a formal mediation analysis framework to quantify and decompose the fertility effect of the 1994 Ethiopian school reform program into the impact through the most widely hypothesized mechanism (years of schooling) and the causal impact of the program net the effect of years of education. The results suggest a fertility-inducing effect of the reform mainly driven by mechanisms other than years of education. The fertility-reducing effects of increased school enrollment were too small to offset the positive impact of the unknown mechanisms. The result points to potential losses in the effectiveness of similar reform programs if adverse effects, such as deteriorated school quality, were not given as much attention.

Keywords. *School reform program. Reproductive behavior. Causal mechanisms. Matching. Mediation Analysis. Ethiopia. Years of education. Education quality.*

About the authors

Endale Kebede is a teaching and research scholar at university of Vienna, the Wittgenstein Centre for Demography and Global Human Capital (IIASA, OeAW, University of Vienna)
(Contact: endale.birhanu.kebede@univie.ac.at).

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1. Introduction

Since the 1990s, many sub-Saharan African countries have undergone school reform programs that substantially increased girls' school participation (Lewin 2009; Majgaard and Mingat 2012; World Bank 2009). A growing number of studies have since taken advantage of the mass education expansion programs to examine the impact of education on fertility and other demographic outcomes. These studies provide mixed results: while most findings reaffirm the well-established negative relationship between education and fertility indicators (Behrman 2015; Ferre 2009; Glick, Handy, and Sahn 2015; Keats 2018) , some report a weaker than expected or even unexpected positive association (Grant 2015; Liu and Raftery 2020; Mensch, Singh, and Casterline 2005; Zanin, Radice, and Marra 2015).

Nonetheless, many of these reform-based studies focus on the quantity dimension of education—as reflected by completed years of schooling. They employ estimation strategies, such as the instrumental variable technique, to identify the causal effects of increased girls' schooling induced by the reform. The key identification assumption behind the estimation approaches is the so-called exclusion restriction - reform exposures would affect outcomes of interest only through its effect on the supposedly primary mechanism variable (years of education). Therefore, the assumption ruled out the contributions of 'other' potential mechanisms on the causal pathway between the reform exposure and demographic outcomes.

However, vast empirical evidence suggests that the reform induced surge in primary school enrollment in sub-Saharan Africa was accompanied by deteriorated school quality (Chimombo 2009; Grogan 2009; World Bank 2009; Zhang 2006), which could affect socio-economic outcomes. Studies further argue that the substantial progress in school attainment made by developing countries does not guarantee improvements in learning outcomes and socio-economic conditions (Hanushek 2013; Filmer, Hasan, and Pritchett 2006; Pritchett 2013). This mismatch between schooling and learning outcomes indicates the possible mechanism effects of adverse consequences of the program and the need to focus beyond expansions of education quantity in developing regions. Indeed, the more recent policy focus for developing countries has shifted from expanding primary education access to improving the quality of education. For example, the United Nations' Sustainable Development Goal 4 emphasizes educational quality at all levels and enhancing work-related skills for youth and adults (UNESCO 2016).

The present study uses a two-stage formal mediation analysis framework developed by Flores and Flores-Lagunes (2009) to estimate the causal mechanism effects of the 1994 Ethiopian school reform program on women reproductive behaviors. The method allows to quantify and decompose the reform's causal impact in to the effects through the most widely hypothesized mechanism (years of schooling) and the causal impact of the program net the impact of years of schooling. The net treatment effect is the causal effect of the reform while 'blocking' the exposure's effect on completed years of schooling. No prior research made such an explicit decomposition while identifying the causal relationship between school reform exposures and fertility behaviors in sub-Saharan Africa.

Ethiopia is one of the first African countries to adopt universal primary education policies. Like many other rapid education expansion policies, the 1994 Ethiopia school reform program has been criticized for trading the quality of education for massive school enrollments (World Bank 2009). Women who were of primary school age (7-14) during the reform implementation were more likely to be affected by the reform. In this analysis, I use this exogenous variation in the probability of exposure to the school reform to form the treated units (age 7-14 in 1994) and control units (age 15-22 in 1994).

Using data from the Demographic and Health Surveys Program (DHS), the present study demonstrates that exposure to the school reform program is causally associated with an early onset of childbearing and larger number of children at age 25. The findings further suggested that the causal impact of the school reform was mainly driven by mechanisms other than years of education. Consistent with the literature, the causal effect of years of education is negative and statistically significant. However, it was too small to offset the positive effects of the primary school reform program through unknown mechanisms. The unidentified mechanisms could include the adverse effects of the program through the deteriorated school quality. However, due to data limitations, the present study could not further decompose it.

1.1. Schooling, learning and fertility

Numerous studies in sub-Saharan Africa attempted to establish a causal relationship between schooling and fertility outcomes. The quasi-experimental studies use instrumental variable and (fuzzy) regression discontinuity techniques that exploit exogenous variations in completed years of schooling following the introduction of universal primary education programs (Behrman 2015; Keats 2018; Makate and Makate 2016; Osili and Long 2008b; Zanin, Radice, and Marra 2015) or change in school systems (Ali and Gurmu 2018; Ferre 2009). It took advantage of the fact that school reforms are exogenous interventions that differently affect women's schooling. This is based on girls' age at the time of the intervention. The studies generally confirmed higher years of schooling of women directly exposed to an intervention, which led to delayed marriage and first birth, a smaller ideal family size, and lower overall fertility.

Expanding access to school could influence women's reproductive behavior in its own right without directly promoting family limitation behaviors and attitudes. Evidence showed that school attendance reduces girls' time and exposure to sexual activities and delays childbearing (Berthelon and Kruger 2011). Similarly, increased average years of schooling could negatively affect overall fertility by changing average group norms (Bicchieri et al. 1997) and social influence (Montgomery and Casterline 1993). However, sufficient fertility reduction needs reproductive decisions to be in the ‘calculus of conscious choice”, which would require empowering women with basic life-supporting skills. Literacy skills acquired in school may influence women's attitude and behavior with long-lasting impacts through the following broad pathways:

First, learning skills acquired in schools could improve efficient utilization of birth control methods (El-Ibiary and Youmans 2007), better use of health care services (Gakidou et al. 2010), improve overall health knowledge (Glewwe 1999), and ability to access, process and comprehend family planning information outside of school (R. A. LeVine et al. 2004; Islam and Hasan 2000).

Second, basic literacy skills enable women to conceptualize future fertility plans. Results from DHSs show that women with more schooling tend to report to questions of 'ideal family size' numerically. In contrast, women with no formal education are more likely to provide non-numerical answers, such as 'it is up to God' (Kebede, Striessnig, and Goujon 2021; Riley, Hermalin, and Rosero-Bixby 1993). The classical demographic transition theories posit that inability to assign numeric values to future childbearing plans reflect a "pre-transition" mindset that fertility is not in the 'calculus of conscious choice' (Caldwell 1976).

Third, skills acquired in school could enhance labor market earnings that would increase the opportunity cost of childbearing (Green and Riddell 2003; Galor 2011). Likewise, studies suggest that women's reading skill is the primary causal pathway between a mother's education and child health outcomes, contributing to the onset of fertility declines in high fertility settings (Smith-Greenaway 2015b; Notestein 1953).

Fourth, knowledge and skills gained through formal schooling and school-based reproductive health education led to higher exposure to different values and ways of thinking (R. LeVine, LeVine, and Schnell 2001; Ajuwon and Brieger 2007; Goldman and Collier-Harris 2012). In the same vein, schools provide an essential platform for disseminating ideas about alternative family norms, birth control methods, and sexual behaviors (Caldwell 1976; 1980).

Table 1 estimated effects of education on fertility outcomes when schooling is not learning

	Cumulative number of children at age 25	
	'Full' sample	*'restricted' sample*
	(1)	(2)
Women's education		
None (reference)		
Primary	-0.033	-0.140***
	(0.026)	(0.039)
Secondary	-0.821***	-0.872***
	(0.032)	(0.032)
Completed secondary or more	-1.20***	-1.230***
	(0.034)	(0.034)
Constant	1.699***	1.685***
	(0.009)	(0.010)
Observation	19,152	16,698
AIC	59,396.25	51,460.38

Note: The table reports the Poisson model regression estimate of the effect of women's education on the cumulative number of children at age 25. The estimates are reported as marginal effect. Standard errors are shown in parentheses. Data are from Ethiopian DHSs and include women aged 24 or above at the time of the interview. The 'full' sample includes all women who were age 7-22 during the implementation of the 1994 school reform. The restricted sample excludes women with at least one year of primary schooling but were not able to read a simple sentence. Both models include controls for age of women at interview date, urban residence, religion, province of residence, and number of siblings. *p<0.1; **p<0.05; ***p<0.01

Examining the effect of education on fertility outcomes for the sample of women included in the present analysis1 revealed that studies based on only 'time served' in school could drastically underestimate the true impact of women's education. Table 1 compares the estimated effect of education on the cumulative number of children at age 25 from alternative samples. Column 1 shows results from the entire sample. The 'restricted 'sample, column 2, excludes women who completed at least one year of primary education but had no reading skills. These exclude about one-third of women with some years of primary schooling but could not read a simple sentence written in their preferred language. The results from the 'full' sample revealed no statistically significant difference, in the number of children at age 25, between women with primary education and those with no formal education. In contrast, in the 'restricted' sample, primary education attainment is associated with a 14 percent reduction in the cumulative number of children at age 25.

Although the findings could not be interpreted as causal effects, it highlighted the importance of distinguishing the learning effect from the pure act of girls attending school. It is particularly essential in sub-Saharan Africa, where schooling has a weak correlation with learning (Smith-Greenaway 2015a). For example, the most recent DHSs showed that 70 percent of Ugandan women who completed third grade could not read a simple sentence written in their preferred language. In Ethiopia, while about 53 percent of women (15-49) have attended formal education, only one-fourth proved reading skills. In countries like Niger, more than one-fifth of women with six years of schooling have no essential reading skills.

This study uses the 1994 Ethiopian school reform program to show the relative causal contribution of time served in school on reproductive behaviors. It hypothesized that the program effect net of the causal effect of years of education mainly reflects the effects of the deteriorated school quality caused by the mass education expansion program. However, the available data does not allow us to test the hypothesis.

2. Primary school reforms in Ethiopia

In the initial post-independence years, many governments in Sub-Saharan Africa were engaging in an ambitious expansion of free primary education and other social services, which creates optimism in the continent (Ezenwe 1993). However, the time of high expectation was soon replaced by an unfortunate period of economic stagnation and political instability related to the mounting government external debt, world oil price shocks, and declining terms of trade (Ezenwe 1993; Bates, Coatsworth, and Williamson 2007). In an effort to “restructure the economy”, in the 1980s, many governments of the region implemented austerity measures, and introduced user fees for education and other public services (World Bank 2009). The indirect and direct cost of education coupled with existing political and socio-economic miseries led to a declining enrollment rates, higher drop outs and stalls in women’s educational progress (Avenstrup, Liang, and Nellemann 2004; Kebede, Goujon, and Lutz 2019; Unicef 1987). In the last three decades, however, many African governments implemented school fee abolishing policies, as part of wider policy reforms (World Bank 2009).

[1] Includes women aged 7-22 at the implementation of the 1994 Ethiopian primary school reform.

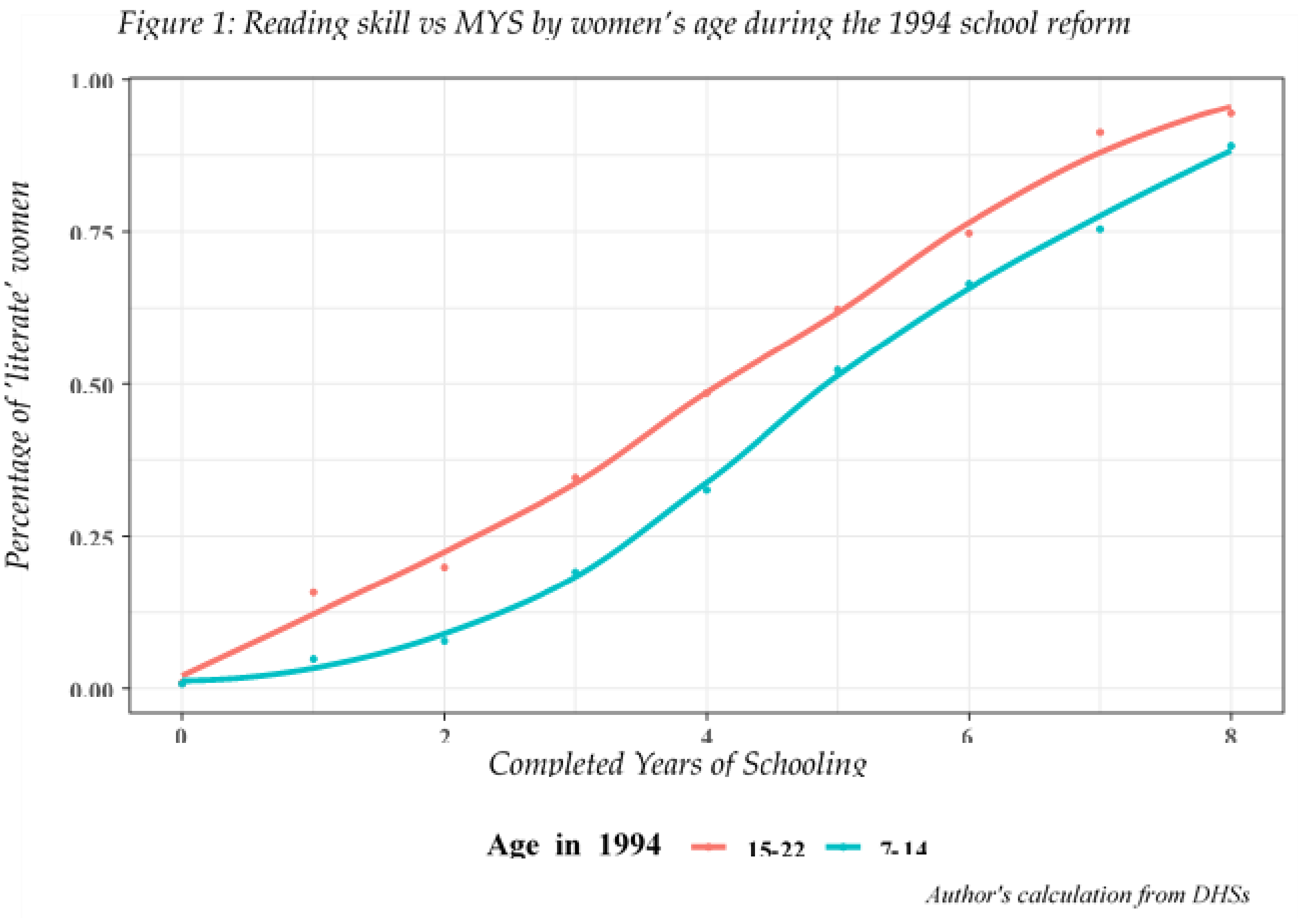
In 1994, with little prior planning, Ethiopia introduced a radically new 'Education and Training' policy. School fees were abolished across all grade levels of primary school (1-8) and the first two years of secondary school (9-10). In addition, the government provides grants to schools (US$ 1.20-US$ 2.35 per pupil per year) to match the pre-existing school fees (World Bank 2009). The immediate impact of the school reform in Ethiopia was a surge in primary school enrollments. In the following year of the reform, 317,000 additional students entered first grade, a 28 percent annual growth (Ministry of Education 1996). Primary school gross enrollment rose from 26.2 percent in 1994 to 79.8 percent in 2004. The improvement was particularly remarkable among girls, rural schools, and disadvantaged regions (World Bank 2009).

Elimination of school fees, though, was only one aspect of the reform. Curriculum reform, school feeding programs, decentralization, and alternative primary education in marginalized regions were instead the main components of the reform. The length of the primary school extends from 6 years to 8 years, with starting age of 7 remaining unchanged. Additionally, the decentralization policy allowed federal state governments to choose the language of instruction in primary schools and give discretion to local authorities in school administrations (Ministry of Education 1994). In the years prior to 1994, several regions decide to switch the language of instructions from Amarigna to local languages (Tigrigna in Tigray, Oromogna in Oromia, Somaligna in Somali region, Sidamigna , Wolayitigna , Hadiyigna , Gedeogna , and Kembatigna in SNNPR region)(Boothe and Walker 1997). Since primary schools were the main focus of the broader policy reform, I assumed primary-school-age women (7-14) in 1994 were exposed to the reform while women older than 14 during the reform were not affected by it.

2.1. Primary school reforms and school quality

An insufficiently discussed aspect of the primary school reforms in sub-Saharan Africa was its adverse effect on school quality. Countries mainly adopt the so-called "big bang" approach and implemented school reforms with little or no prior planning and negligible school infrastructural improvements (World Bank 2009). Though the sectoral budget in Ethiopia has increased with the reform, it could not match the surge in primary school enrollment. Thus, the primary mechanism to accommodate reform-triggered enrollment surges was by reducing resources available per primary school pupil. In Ethiopia, the pupil-teacher ratio in primary schools had declined since 1980 before it rose again with the reform. It increased from 26:1 in 1993 to 67:1 in the year 2000. Beside this, schools were forced to hire unqualified teachers to cope with explosions in enrollment. For example, by the year 2000, about 76 percent of upper primary school (5-8) teachers were not qualified (Education Management Information Systems 2000). Similarly, the mismatch between sectoral budget and enrollment expansions caused a more intensive use of classrooms, textbooks, and other school facilities. Between 1992 and 1996, the number of students per school increased by 90 percent, and an average classroom had to serve 20 more students in 2004 than it used to accommodate in 1994 (Ministry of Education 2000; World Bank 2009)

The deteriorated school quality in Africa was a challenge for cognitive improvement and learning outcomes (Pritchett 2013). The Ethiopian National Baseline Assessment in 1999 and 2004 clearly showed that expansions in enrollment were not translated into learning: the average test results of Grade 4 and 8 students, in all subject areas, were below the minimum passing score of 50 percent. Comparing average reading skill of women in primary school age in 1994 with the preceding cohort, figure 1 revealed the deteriorated school quality following Ethiopia's primary school reform. At each completed year of primary school, the reading skill of women directly exposed to the 1994 school reform while of primary school age was significantly lower than those above school exit age at the reform implementation (15-22). For example, about 78 percent of women with four years of primary school and who were of primary school age in 1994 could not read a sentence written in their preferred language. In contrast, about 44 percent of their counterparts from the previous cohort were 'literates'.



3. Methods and Data

3.1. Identification Strategy

The study seeks to quantify and decompose the impact of the 1994 Ethiopian school reform program on women's reproductive behavior (the outcome) into the effect through the most widely hypothesized mechanism (years of schooling) and the causal impact of the program net the impact of years of schooling. Exposure to the 1994 primary school reform implementation varies by women's age at the reform. While girls in primary school age (7-14) were likely to be affected during the reform implementation, girls above the primary school exit age were not exposed. In this analysis, I exploited the age-specific differences in the probability of exposure to the school reform to form the treated units (age 7-14 in 1994) and control units (age 15-22 in 1994).

To define these parameters of interest, I use the Neyman-Rubin potential outcome framework as suggested by Flores and Flores-Lagunes (2009). The expected causal effect of exposure to the school reform program (T) for a randomly chosen woman (i) that was actually exposed to the program, the average treatment effect on the treated (ATT), can be defined as:

$$ATT = E[Y_i(1) - Y_i(0)/T_i = 1] - - - - - - - - - - - - - - - - - - [\text{E1}]$$

Where, $Y_i(1)$ and $Y_i(0)$ are the potential outcomes for treated woman $(i \in N)$ that can be observed in the presence of the school reform program $(T = 1)$ and under the absence of the program $(T = 0)$, respectively. Thus, the ATT compares the observed outcomes for a treated woman with the counterfactual outcome that would be observed had they not been exposed to the reform.

The fundamental problem in the causal estimate of ATT is that, for women exposed to T, we can observe only $E[Y_i(1)]$. In a random treatment allocation where T is assigned independent of potential outcomes, one can use the observed outcomes of the untreated units, $E[Y_i(0)/T_i = 0]$ as a surrogate for the counterfactual outcomes of the treated women. 2 However, assignments to the school reform program were conditional on a set of observable pre-treatment characteristics; for example, women from disadvantaged regions and at primary school age in 1994 were more likely to be exposed to T. Therefore, an appropriate treatment comparison invokes the following conditional independence assumption:

$\boldsymbol{Assumption}$ **1**. $\quad Y(1), Y(0) \perp T/X$

Which states that the potential outcomes are jointly independent of the treatment assignment conditional on the set of covariates X. Assumption 1 further implies that, once we condition on X, the mean observed outcome for the untreated units is equal the mean counterfactual outcomes for the treated women, had they been exposed to the reform; $E[Y_i(0)/T_i = 0] = E[Y_i(0)/T_i = 1]$. In combination with the overlap assumption specified below, assumption 1 is sufficient for identifying the ATT.

$\boldsymbol{Assumption}$ **2**. $\quad 0 < pr(T = 1/X = x) < 1$

Assumption 2 states that the conditional distribution of the exposed and control units overlap for the vector of covariates or no combination of covariates perfectly predict treatment assignment.

The (net) mechanism effect

The primary purpose of the study involves decomposing the ATT in to the effect through years of schooling (the mechanism effect), and the causal effect of the treatment while 'blocking' the exposure's effect on completed years of schooling (the net treatment effect). Years of schooling can be viewed as an intermediate outcome or a post-treatment mechanism variable in the causal pathway between treatment exposure and the final outcome of interest. Since the exposure T affects the

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[2] The treated and untreated units are just two random samples from the same population

mechanism variable (S), any observed posttreatment mechanism variable could represent two different potential values: $S_i(1)$ for a treated woman and $S_i(0)$ if not exposed to the treatment.

In the same vein, one can derive the potential outcome of interest Y as a function of both T and S, and, for each i, four composite potential outcomes can be considered: (i). $Y_i(1, S_i(1))$, the potential outcome when a woman i is exposed to the reform and the mechanisms variable is affected by the exposure. This is equivalent to the potential outcome $Y_i(1)$. (ii). $Y_i(0, S_i(0))$, the outcome when the woman is not exposed to the reform and the mechanism is not affected by the treatment, and is equivalent to $Y_i(0)$.. (iii). $Y_i(1, S_i(0))$, the outcome when a woman is exposed to the reform but the mechanism is kept at the level in the absence of the exposure. (iv). $Y_i(0, S_i(1))$, is the potential outcome a woman who were not exposed to the reform obtains if she receives the mechanism value as if she were treated.

Using the above composite potential outcomes, the mechanism average treatment effect for the treated (MATT) can be defined as:

$$MATT = E[Y_i\big(1, S_i(1)\big) - Y_i\big(1, S_i(0)\big) / X_i = x, T_i = 1] - - - - - - - - - - - - \text{[E2]}$$

and the Net Average Treatment Effect ($NATT$) can be defined as the average causal effect of school reform exposure on an outcome, blocking the effect of the exposure on the post treatment mechanism (S_i):

$$NATT = E[Y_i\big(1, S_i(0)\big) - Y_i\big(0\ , S_i(0)\big) / X_i = x, T_i = 1] - - - - - - - - - - - - - \text{[E3]}$$

The above equations imply that to estimate the $MATT$ and $NATT$ from non-experimental data, one must estimate the potential outcome for a treated unit under treatment, but blocks the effect of T on Y; $Y_i\big(1, S_i(0)\big)$. This require to fix the observed value of the mechanism variable for each i at $S_i(0)$. In principle, one could adjust the exposure's effect on the mechanism, by simply controlling the observed values of S , in standard analytical approaches such as multivariate regression or matching [with or without controlling for years of education]. However, without satisfying several strong assumptions, simple treatment comparisons 'adjusting' for the observed values of a posttreatment variable lacks causal interpretations (Rosenbaum 1984; Rubin 2005; Flores and Flores-Lagunes 2009).

Principal Stratification

In order to causally interpret our parameters, treatment comparisons were rather made based on the concept of principal stratification developed by (Frangakis and Rubin 2002) [I employed the framework and notations of Flores and Flores-Lagunes (2009)]. The basic idea is to stratify units based on the values of the potential mechanism outcome $S(0), S(1)$, irrespective of treatment status. Women corresponds to a "principal stratum" defined by the joint potential values of a posttreatment variable $\{s(0) = s_0,\ \ s(1) = s_1\}$ are comparable. The advantage of principal stratification over the naive approaches of controlling observed values of the mechanism values is that membership to a principal stratum is not affected by treatment assignment and, hence, principal effects are causal effects.

Adopting the basic concept of principal stratification our estimands can be redefined as:

$MATT =$
$$E\{E[Y_i(1, S_i(1)) - Y_i(1, S_i(0))/s(0) = s_0, s(1) = s_1, X_i = x, T_i = 1]\} - - - - - - - - [E4]$$

and

$$NATT = E\{E[Y_i(1, S_i(0)) - Y_i(0, S_i(0))/s(0) = s_0, s(1) = s_1, X_i = x, T_i = 1]\} - - - - - - - - [E5]$$

Note that, $MATT$ and $NATT$ sum to ATT.

The practical difficulty in the identification and estimation of our parameters as defined above arise from the fact that the principal strata $\{s(0) = s_0,\ s(1) = s_1\}$ is not observable. For example, a treated woman with 4 completed years of schooling may belong to either a stratum $\{s(1) = 4,\ s(0) = 4\}$ or $\{s(1) = 4,\ s(0) = 13\}$. Therefore, in the absence of a counterfactual experiment that would allow us to directly observe $Y_i(1, S_i(0))$, one need to invoke additional conditional independence assumption:

$\boldsymbol{Assumption\ 3.}$ $\quad E[\,S_i(1)/X_i, T_i = 1] = E[\,S_i(0)/X_i, T_i = 0]$

which states that given a rich set of covariates X, treatment is not assigned based on expectation that the posttreatment mechanism outcome will be different under treatment and control conditions. Assumption 3, therefore, would enable us to find suitable counterfactual mechanism values from the other arm of the treatment.

3.2. Estimation strategy

While estimating and decomposing the ATT into the mechanism effect ($MATT$) and the net treatment effect ($NATT$) under the potential outcome framework, I follow a two-stage estimation strategy [a similar strategy was employed by Ferraro and Hanauer (2014)]. In the first stage, I use matching to create comparable treated and control units in terms of observable pretreatment confounding variables. Matching ensures the covariate distribution in the two arms of the treatment assignment looks as they would be in a randomized experiment [hence, assumptions 1 and 3 are satisfied]. By creating balance across covariates, matching provides an estimate of the ATT. In addition, it provides estimates for the counterfactual mechanism values $S_i(0)$ for each treated unit, using the observed mechanism values from the matched untreated units.

I use optimal full matching with a caliper which involves assigning women into a subclass that contains one treated unit and one or more control units or one control unit and one or more treated units. The full matching method provides better covariate balance than pair marching techniques such as 1:1 nearest neighbor propensity score matching and 1:1 Mahalanobis distance matching [Appendix table S.1-S.3 compares covariates balance improvements with alternative matching methods]. Furthermore, full matching presents the additional advantage that the matching discarded no treated units to estimate the ATT effectively. The matched data set consists of 12,398 treated women and 12,201 control women. After matching, I estimated the average treatment effect on the treated (ATT) with regression bias adjustment and matching weights.

In the second stage, I follow the approach suggested by Flores and Flores-Lagunes (2009) to estimate the counterfactual outcome for treated units had treatment not affected the posttreatment mechanism variable, $Y_i\big(1, S_i(0)\big)$. It is a regression-based method based on a simple assumption about the way the mechanism and the covariates affect outcomes in the original and counterfactual treatments. It assumes that the conditional expectation of $Y(1, S(0))$ and $Y(1, S(1))$ in terms of $\{X, S(0)\}$ and $\{X, S(1)\}$, respectively, shares the same functional form.

$\boldsymbol{Assumption\ 4}. Suppose$

$$E\big[\, Y\big(1, S(1)\big)/S(1) = s_1\, , X = x, T = 1\big] = f_1(S(1), X)$$

Then

$$E\big[\, Y\big(1, S(0)\big)/S(0) = s_0\, , X = x, T = 1\big] = f_1(S(0), X)$$

which implies that the covariates and the mechanism have similar effects on the potential outcomes both in the original and counterfactual treatments.

(Flores and Flores-Lagunes 2009) suggested the implementation of the above method by first running a regression of the outcome of interest on the mechanism variable and all of the covariates, using the treated matched units. We then evaluate the counterfactual $Y_i\big(1, S_i(0)\big)$ by plugging an estimated counterfactual mechanism value $\widehat{S_\iota}(0) = E[S_i(0)/T = 1]$ and covariates X into the estimated coefficients. With the above adjustment the NATT can be rewritten as:

$$NATT = E[f_1(S(0), X] - E\{E[Y^{obs}/T = 0, S^{obs} = s_0, X = x]\} - - - - - - - - \text{[E6]}$$

And the $MATT = ATE - NATE$.

The counterfactual mechanism value for each treated unit was estimated using the information from the observed mechanism value of the corresponding control unit(s)3 . First, we estimate a model for

$$E[\, S^{obs}/S(0) = s_0\, , X = x, T = 0] = f_0(X, T = 0) - - - - - - - - \text{[E7]}$$

The coefficients from the above estimation are, then, used to estimate

$$[\widehat{S_\iota}(0)/T = 1] = f_0(X, T = 1)\ - - - - - - - - \text{[E8]}$$

3.3. Data

Data were obtained from consecutive standard demographic and health surveys (DHS) conducted in 2000, 2005, 2011, and 2016. The DHS used a two-stage cluster sampling technique and standardized questionnaires to collect comparable and nationally representative data on population health, living conditions, and household demographics. Data from the four surveys were pooled to provide an adequate sample size for our analysis. The final sample consists of 12,398 women directly exposed to the 1994 primary school reform while of primary school age (7-14) and 12,767 women above primary school age (15-22) in 1994. I restricted the analysis to women who were 19 years old or above and

[3] In cases where there are more than one control matches for a treated unit within common principal stratum, the weighted median mechanism value was taken.

possibly not attending school at the survey time. Table 2 provides summary statistics of key variables of the analysis

The Ethiopian DHSs provide multiple indicators of women's reproductive behavior (outcomes of interest) relevant to the demographic changes in sub-Saharan Africa. In this study, a binary outcome variable-whether a woman started childbearing before age 20-was used as an indicator of the timing of reproductive events. Additionally, to capture the quantum of fertility rather than differences in birth timing, I analyzed the cumulative number of children women had by age 25 y. The choice for the cut-off age of 25 was made due to data limitations. Table 2 revealed that women in the treated group tend to have larger number of children at age 25 and earlier onset of childbearing than their control group counterparts.

Access to schooling is the most widely hypothesized mechanism through which exposure to the school reforms could affect reproductive behaviors (Keats 2018; Pradhan and Canning 2016; Behrman 2015).. Girls exposed to the reform program had an average of 0.37 more years of schooling than girls who were not at primary school age in 1994. Considering the country's overall low level of schooling (55 percent of women age 20-49 in 2016, for example, had no formal education), the observed difference is sizable.

A set of exogenous covariates was included to increase the precision of our estimation. Studies showed that rural residents benefited more from the school reforms than their urban counterparts (World Bank 2009). Thus, I control for the impact of urban residence, as it is defined in DHS (urban vs rural resident). Similarly, the decentralization policy associated with the school reforms allowed administrative regions to implement the reform differently. While some regions changed the language of instructions in primary schools to native languages, in others, even school fees were not eliminated until 1996. Therefore, a categorical variable for the administrative region was included as an exogenous covariate in the above model. Additionally, age at the time of the interview, the number of siblings, and a dummy for religion were controlled. The distribution of these baseline variables considerably varies by women’s exposure to the reform. For example, exposed women were younger at an interview date, more likely to live in urban areas, and grew up with fewer siblings.

Table 2: Summary statistics for key variables

	Control cohort *(age in 1994: 15-22)*	Exposed cohort *(age in 1994: 7-14)*	Full sample
	Mean (SD)	Mean (SD)	Mean (SD)
Years of school	2.62 (4.32)	2.99 (4.57)	2.80 (4.45)
No. Children at age 25*	1.66 (1.33)	1.75 (1.35)	1.70 (1.34)
1st birth before age 20	.502 (.500)	.495 (.499)	.498 (.500)
Age at survey	30.6 (6.23)	26.86 (4.48)	28.75 (5.75)
Urban resident	.307 (.461)	.342 (.474)	.324 (.468)
Muslim	.346 (.475)	.370 (.482)	.358 (.479)
Number of Siblings			
[0,3]	.200 (.400)	.214 (.410)	.207 (.405)
[4,6]	.397 (.489)	.411 (.492)	.404 (.490)
More than 6	.401 (.490)	.374 (.483)	.388 (.487)
Region			
Tigray	.094 (.292)	.087 (.282)	.091 (.287)
Afar	.057 (.232)	.069 (.253)	.063 (.243)
Amhara	.125 (.331)	.115 (.319)	.120 (.325)
Oromia	.146 (.353)	.138 (.345)	.142 (.349)
Somali	.059 (.236)	.066 (.248)	.062 (.242)
Benishangul-Gumuz	.063 (.243)	.072 (.260)	.068 (.251)
SNNPR	.141 (.348)	.125 (.331)	.133 (.340)
Gambella	.055 (.229)	.065 (.246)	.060 (.238)
Harari	.063 (.244)	.062 (.242)	.063 (.243)
Addis Ababa	.121 (.327)	.129 (.335)	.125 (.331)
Dire Dawa	.070 (.255)	.067 (.250)	.068 (.252)
Sample Size	12,767	12,398	25,165

Data were from DHSs including women aged 20 or above at the time of the interview ; *consider only women age 25 or more at interview date

4. Results

4.1. Effect of exposure to primary school reforms

In an attempt to identify the causal effects of exposure to primary school reforms on reproductive behaviors, table 3 presents the estimates from the naïve logistic and Poisson regression models. Column (1) and (2) compares the effects of women's exposure to the reform program on the timing of first birth with and without controlling for relevant exogenous background characteristics, respectively. In a model that does not adjust for confounders, treatment status has no statistically significant association with first birth timing. However, accounting for confounding variables, reform exposure has a strong positive effect on reproductive behaviors; the odds of having a first child before age 20 were about 13 percent higher among those directly exposed to the reform while of primary school age than in the control group.

Table 3. Pre-matching results: Estimated effect of exposure to primary school reforms on women's reproductive behavior

	Reproductive Behavior			
	First birth before age 20		Children at Age 25	
	Odds Ratio		*Rate Ratio*	
	(1)	(2)	(3)	(4)
Reform Exposure	0.97	1.131***	1.049***	1.074***
	(0.006)	(0.031)	(0.011)	(0.012)
Constant	1.008***	0.561***	1.662***	1.530***
	(0.017)	(0.052)	(0.013)	(0.071)
Adjusted for baseline differences	No	Yes	No	Yes
Obs.	25,165	25,165	19,096	19,096

Note: The table reports the conventional logistic and Poisson estimates of the effect of exposure to the 1994 Ethiopian school reform on indicators of reproductive behaviors. Data are from DHSs and includes women aged 20 or above at the time of the interview. Results in columns 2 and 4 controls for age of women at interview date, urban residence, religion, province of residence, number of siblings.

***, **, and * represent significance at 1%,5% and 10% levels, respectively. terms in parentheses are robust standard errors

Similarly, women in the treated group tend to have more children at age 25; compared to older women, the birth rate of a woman of primary school age during the school reform increases by a factor of 1.05-1.07. The treatment effect is again more substantial in a model that controls baseline characteristics such as religion, number of siblings, urban and region of residence, and age at the interview date.

The above comparisons revealed that the distributions of treatment characteristics of the treated and untreated women are different, which calls for matching methods to eliminate the influence of observable confounders. Table 4 displays impact estimates based on the 'full matching' method with post-matching regression bias adjustments. The results indicate that treated women had a differently larger number of children at age 25 and were more likely to have first birth before age 20 than they would have been in the absence of the program. The causal estimate, column 1, shows that treatment exposure increases the likelihood of early childbearing for those exposed to the reform by about four percentage points (or about 10 percent higher than the probability amongst the control group). Similarly, the average number of children at age 25 for a treated woman would have been ~0.11 smaller had she not been in primary school in 1994. This mean difference is equivalent to ~8.5 percent of the average number of ever-born children of control women at age 25.

Table 4. Post-matching results: the average causal effects of exposure to the school reform program on reproductive behavior

	Reproductive Behavior			
	First birth before age 20		Children at Age 25	
	(1)	(2) +	(3)	(4)+
ATT	0.041**	0.052***	0.105***	0.170***
	(0.011)	(0.010)	(0.032)	(0.032)
Constant	0.453***	0.326***	1.250***	1.091***
	(0.010)	(0.041)	(0.016)	(0.149)
Matched Obs.	24,599	24,679	18,530	18,610
Matched treated-units	12,398	12,398	8,590	8,590

Note: The table reports the estimated impacts of the 1994 Ethiopian school reform program on those women exposed to the reform (the average treatment effect on the treated-ATT). The ATT was estimated using 'Full matching' method.
Matching weights with robust standard errors and covariate adjustments were used to estimate the ATT.
Covariate balances were made based on the following pre-treatment variables: age of women at an interview date, urban residence, religion, province of residence, and number of siblings. In Columns 2 and 4, however, years of schooling was included as an additional matching covariate.
Data are from DHSs and include women aged 20/25 or above at the time of the interview.
***, **, and * represent significance at 1%,5% and 10% levels, respectively. terms in parentheses are robust standard errors
+ the mechanism variable (years of schooling) was included as an additional matching covariate

4.2. Mechanism effects

To get an insight into the contribution of the mechanism variable to the estimated ATT, I calculated the net treatment difference (NTD) using the 'full matching' method that matches units based on pre-treatment variables and the mechanism (years of education). As shown in columns 2 and 4 of Table 4, the impact estimates significantly increased when adjusting for years of education (the mechanism); for example, the average treatment impact on the likelihood of early childbearing has substantially improved from about 4.1 percent to 5.2 percent after the adjustment. The estimated differences between the NTD (columns 2 and 4) and the ATT (columns 1 and 3) are suggestive of a fertility reducing mechanism effect (through years of schooling) and a substantial positive net mechanism effect (through unknown mechanisms). However, the estimation by simply adjusting for the posttreatment variable, without further assumptions, lacks causal interpretations as an average treatment effect (Rosenbaum 1984; Flores and Flores-Lagunes 2009).

Table 5. Mechanism effects: Estimated mechanism effects of primary school exposure on the treated women

Outcome	*observed mechanism*	counterfactual mechanism	*ATT*	*NATT*
	(1)	(2)	(3)	(4)
First birth before age 20	2.98	2.62	0.041**	0.052***
			(0.011)	(0.004)
No. children at age 25	2.73	2.10	0.105***	0.154***
			(0.032)	(0.012)
Matched treated- units			12,398	12,398

Note: The table reports decompositions of the ATT in to the MATT and NATT. Data are from DHSs and include women aged 20 or above at the time of the interview. All models include controls for age of women at interview date, urban residence, religion, province of residence, and number of siblings.

***, **, and * represent significance at 1%, 5% and 10% levels, respectively. terms in parentheses are robust standard errors

To have a causal interpretation of the estimates, I adopt the concept of principal stratification and estimation strategies discussed in the method section and evaluate the causal mechanisms underlying the average effects of the reform on the treated women. Table 5 summarizes how much of the estimated effect of the reform exposure was accounted for by the mechanism effect (through increased years of education) and through the net mechanism effects. The reform program increases the average years of schooling for those exposed. For example, for exposed women older than 24 at the interview date, the reform exposure induced 0.63 additional years of schooling. Considering Ethiopia's low level of education, the 30 percent increase relative to the 2.1 average years of schooling in the control group is sizeable. The result further suggested that exposure to the school reform program has a fertility reducing effect through the supposedly primary mechanism (years of education) and opposite fertility inducing effect through the unknown mechanisms. Had the treatment not affected the mechanism, the estimated average number of children at age 25 and the likelihood of early childbearing for the treated women would have increased by about 46 percent and 27 percent, respectively. However, the fertility reducing impacts of the mechanism variable were too small to offset the positive net mechanism effects. Thus, the fertility effects of the program were predominantly driven by factors other than years of education.

4.3. Robustness checks

In the following, I explore the sensitivity of the above results to changes in methodology bandwidth selection and intensity of exposure to the reform. Table 6 displays the exposure effect and the net mechanism effect on the treated women using three alternative matching methods for optimal full matching. The alternative matching methods produce positive and statistically significant fertility effects of the reform on the exposed women. The magnitude of the estimated effects on the cumulative number of children at age 25 is nearly identical to the estimates from the full matching method. Furthermore, the substantial positive net mechanism treatment effect remains robust to change in matching methods.

To investigate the sensitivity of the estimates to bandwidth selection, I re-estimate the specified models for different sizes of birth cohorts on each side of the cut-off (the reform year). The fertility-inducing treatment effect and the main result that the reform's effect was mainly driven by mechanisms other than years of education are robust to varying the bandwidth sizes [see appendix figure S.1].

Table 6. Sensitivity checks: Estimated impacts of the reform exposure using alternative matching methods

	Reproductive Behavior				
	First birth before age 20		*Children at Age 25*		No. matched treated units
	ATT	NATT	ATT	NATT	
1:1 Mahalanobis distance	0.028***	0.034***	0.115***	0.126***	12,398
	(0.006)	(0.004)	(0.019)	(0.014)	
1:1 Propensity score	0.028***	0.035***	0.118***	0.130***	12,398
	(0.006)	(0.004)	(0.019)	(0.014)	
1:1 Propensity score with caliper	0.024***	0.034***	0.093***	0.113***	9,952
	(0.006)	(0.005)	(0.020)	(0.014)	
Full matching	0.041**	0.052***	0.105***	0.154***	12,398
	(0.011)	(0.004)	(0.033)	(0.012)	

Note: ***, **, and * represent significance at 1%,5% and 10% levels, respectively. terms in parentheses are robust standard errors

Another concern is the potential differential impacts of the reform on the treated women based on the intensity of exposure to the program. For example, women closer to primary school entry age during the reform year were exposed longer than those at school exist age in 1994. Appendix Table S.4 compares the reform's effect on women of lower primary school age in 1994 with those of upper primary school age in the same year. The sensitivity check confirms the more substantial fertility-inducing reform effect on women exposed longer to the program while of primary school age in 1994.

Similarly, several regions made additional reforms by switching primary school languages from Amarigna to local languages. Women in these regions were, therefore, exposed differently than women of primary school age in regions with no language changes. A heterogeneity analysis based on the introduction of local languages in primary schools confirms a stronger effect of the reform on women from the language reform regions. [see appendix table S.6].

Finally, I assess how men's exposure to the reform would affect the reform's effect on women's reproductive behavior. About two-thirds of women in the sample are currently married, with an average of eight years age difference between husbands and wives. The considerable age difference between married couples implies that treated women were likely married to husbands who did not expose to the reform themselves. To glimpse the role of men's exposure to the reform, I compared the treatment effect on treated women married to treated men (vs. untreated women) and the treatment effect on treated women married to untreated men (vs. untreated women). The findings revealed a more substantial positive effect of the treatment in the transition to motherhood in the latter group. However, we found no difference in the treatment effect on the number of children at age 25 between the two groups [see appendix table S.5].

Though the findings provide an insight into the role of men in fertility decisions, they should be interpreted with caution: First, potential sample selection and collider problems could emanate from restricting the sample to only currently married women; Women's treatment status could influence the marital status and partner selection. Second, it is unclear whether the differential impact of the reform based on the partner's exposure reflects the fertility reducing impact of the husbands' exposure or the fertility inducing impact of age differences (e.g., through child marriage with older men]. Future work can expand on these findings by decomposing the relative contributions of the two possible factors. Third: the analysis did not consider the pre-treatment characteristics of men.

5. Discussions and conclusions

Many studies have examined the relationship between school reform programs and demographic outcomes in sub-Saharan Africa. These studies exploit exogenous variations in female schooling caused by education reforms to show the causal links between education and desired number of children (Behrman 2015), teenage fertility (Pradhan and Canning 2016; Osili and Long 2008a), and child mortality (Andriano and Monden 2019; Makate and Makate 2016). However, the possible effects of mechanisms other than years of education have been ruled out through the exclusion restrictions that reform exposure could influence outcomes of interest only through its effect on years of education.

This study investigated the causal mechanisms through which exposure to the 1994 Ethiopian school reform program affects women's reproductive behavior. In particular, it considers the reform's effect through mechanisms other than years of schooling (unknown mechanisms). The estimation results revealed positive and sizable reform effects on timing to first birth and the number of children at age 25. The findings further implied that the causal impact of the school reform was mainly driven by mechanisms other than years of education. Consistent with the literature, the causal effect of years of education is negative and statistically significant. However, it was too small to offset the positive effects of the primary school reform program through the unidentified mechanisms.

Consistent with our findings, previous studies have shown lower than expected socio-economic impacts of primary school expansions in sub-Saharan Africa (Liu and Raftery 2020; Grant 2015; Mensch, Singh, and Casterline 2005; Zanin, Radice, and Marra 2015). In Ethiopia, for example, the proportion of women with at least one year of primary education doubled between 2000 and 2016. In contrast, the median age at first birth remained at about 18 years, and the median age at first marriage shows a modest increase from about 15 to 16 years. These seemingly weak associations between primary educational attainment and fertility indicators have led some scholars to call for an extension of the threshold level of compulsory schooling beyond primary education (Maïga 2013), or claim factors other than education for the speed of Africa's fertility decline (Garenne 2012).

Nonetheless, the above results should not be misinterpreted as evidence of the irrelevance of education on fertility in the region. It only reflects the inefficiency of the school expansion programs. The unidentified mechanism effects could include the adverse effects of the school reform programs through deteriorated school qualities. Empirical evidence showed that expansions in the quantity of education in many developing countries came at the expense of the quality needed to derive economic growth and social developments (World Bank 2009; Pritchett 2013). Future studies with richer measures of quality of education (not just years of education) could better quantify the mechanisms through which school reforms affect socioeconomic outcomes. The data from the demographic and health surveys capture only one aspect of women's literacy status- reading skills. However, cognitive differences that alter perception and abstract skills require functional literacy, such as writing, arithmetic, oral communication skills, or abstract thinking.

Our findings should not also be interpreted as evidence against rapid education expansion programs, particularly in sub-Saharan Africa, where one-fifth of girls are still out of school. While the gap between schooling and literacy has recently widened, mass education expansion programs improved education opportunities and average literacy levels. This overall progress in the two education indicators could contribute to fertility declines in sub-Saharan Africa in two ways. First, school participation could delay girls' marriage and the onset of childbearing through the pure "incapacitation' effect. Staying in school delays the onset of childbearing, risk of early marriage, and pregnancy (Lloyd and Mensch 2008). Second, overall improvements in literacy and average years of schooling could influence reproductive behaviors through social interaction effects (Bongaarts and Watkins 1996; Montgomery and Casterline 1996).

To conclude, development efforts in accelerating fertility declines in sub-Saharan Africa must look beyond expanding primary education for all. If not adequately addressed, the increasing learning deficit in the region will continue to undermine the role of education in supporting socio-economic support progresses.

Data and code availability: All the data used in this article is publicly available in the websites of the DHS program: https://dhsprogram.com/privacy-policy.cfm. Replication codes are available from by email request.

Declarations

Conflict of interest: The author declares no competing interests that are relevant to the content of this article.

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Appendix

Table S.1: summary of covariate balance before and after optimal full matching

Covariate	Description	Status	Mean treated units	Mean control units	Absolute Std. mean differences
Age	Age of women at interview date	Pre-matching	26.86	30.89	0.830
		matched	26.86	26.90	0.008
Urban	The unit resides in an urban area	Pre-matching	0.342	0.301	0.074
		matched	0.342	0.330	0.024
siblingNo3	has 3+ siblings	Pre-matching	0.374	0.401	0.049
		matched	0.374	0.364	0.021
Muslim	Religious affiliation is Islam	Pre-matching	0.370	0.346	0.056
		matched	0.370	0.366	0.007
Region	Province of residence*				

*See table 3 for the list of provinces

Table S.2: summary of covariate balance before and after 1:1 nearest neighbor matching

Covariate	Description	Status	Mean treated units	Mean control units	Absolute Std. mean differences
Age	Age of women at interview date	Pre-matching	26.86	30.89	0.830
		matched	26.86	30.23	0.750
Urban	The unit resides in an urban area	Pre-matching	0.342	0.301	0.074
		matched	0.342	0.310	0.068
siblingNo3	has 3+ siblings	Pre-matching	0.374	0.401	0.049
		matched	0.374	0.399	0.038
Muslim	Religious affiliation is Islam	Pre-matching	0.370	0.346	0.056
		matched	0.370	0.351	0.052
Region	Province of residence*				

*See table 3 for the list of provinces

Table S.3: summary of covariate balance before and after 1:1 Mahalanobis distance matching

Covariate	Description	Status	Mean treated units	Mean control units	Absolute Std. mean differences
Age	Age of women at interview date	Pre-matching	26.86	30.89	0.830
		matched	26.86	30.30	0.767
Urban	The unit resides in an urban area	Pre-matching	0.342	0.301	0.074
		matched	0.342	0.312	0.062
siblingNo3	has 3+ siblings	Pre-matching	0.374	0.401	0.049
		matched	0.374	0.388	0.028
Muslim	Religious affiliation is Islam	Pre-matching	0.370	0.346	0.056
		matched	0.370	0.348	0.044
Region	Province of residence*				

*See table 3 for the list of provinces

Table S4: the impact of length of exposure

	(a). women who were 7-10 y in 1994		*(b). women who were 11-14 y in 1994*	
	First birth before age 20	Children at Age 25	First birth before age 20	Children at Age 25
ATT	0.050*** (0.013)	0.141*** (0.039)	0.034*** (0.011)	0.084*** (0.033)
NATT	0.063*** (0.011)	0.202*** (0.033)	0.040*** (0.010)	0.119*** (0.027)
Obs.	5,830	3,892	6,568	4,698

Note: the control group in both cases were married women who were not exposed to the reform.
****, **, and * represent significance at 1%,5% and 10% levels, respectively. terms in parentheses are robust standard errors*
All models include controls for age of women at interview date, urban residence, religion, province of residence, and number of siblings

Table S5: Estimated effect of exposure to primary school reforms on reproductive behavior

	(a). Married women who were exposed to the reform and married to treated men		*(b). Married women who were exposed to the reform but married to untreated men*	
	First birth before age 20	Children at Age 25	First birth before age 20	Children at Age 25
Net Treatment Difference	0.030**	0.172***	0.047**	0.173***
	(0.014)	(0.040)	(0.009)	(0.027)
Constant	0.507***	1.640***	0.579***	1.889***
	(0.011)	(0.033)	(0.007)	(0.020)
Partners' age difference	2.99	2.79	10.53	1.055
Obs.	2,809	2,101	6,168	4,689

Note: the control group in all cases were married women who were not exposed to the reform.
****, **, and * represent significance at 1%,5% and 10% levels, respectively. terms in parentheses are robust standard errors*
All models include controls for age of women at interview date, urban residence, religion, province of residence, number of siblings, and husband's current age

Figure S.1: Treatment effects on the likelihood of early childbearing for varying bandwidths