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The Effect of Schooling on Teenage Fertility: Evidence from the 1994 Education Reform in Ethiopia.

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Abstract
We investigate the effect of female schooling on teenage fertility using an education reform in Ethiopia in 1994 as a natural experiment that led to a jump in female school enrollment and about 0.74 years of additional schooling for the first two exposed cohorts. Using a regression discontinuity approach we find that each additional year of schooling lowers the probability of both teenage marriage and teenage childbearing by about six percentage points. This casual estimate is consistent with the steep gradient of teenage marriage and fertility with education observed in the data.

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1. **Introduction**

Education is a form of human capital that increases worker productivity and wages. Improving girls' education can also lead to large changes the empowerment of women and can have major effects on economic and social development over and above its simple economic benefits (Summers et al. 1992). Educated women tend to have better health, lower fertility and greater bargaining power within the family, leading to higher investment in the health and education of their children. In this paper we focus on the effect of girls' education on their fertility, in particular on teenage fertility—giving birth before age twenty.

Childbearing as a teenager has been linked with poor maternal and child health outcomes and to a reduction in long run economic outcomes for women. Early motherhood prevents women from participating in the formal labor market in the short run and can significantly attenuate their future market wages (Chevalier et al. 2003; D. Klepinger et al. 1999; Miller 2011; Trussell 1976). In addition to its labor market effects, childbearing as a teenager has been linked with poor maternal and child health outcomes. Childbearing at an early age increases the risk of maternal mortality (Conde-Agudelo et al. 2005; Jolly M.C. et al. 2000) and adverse maternal health outcomes (Jolly M.C. et al. 2000; King 2003; Makinson 1985). Adolescent mothers are at risk of early term birth, maternal anemia, and cesarean delivery (Scholl et al. 1994), particularly in developing countries. Also, children born to teenage mothers are more likely to be underweight (Kurth et al. 2010) and have higher mortality. Children born to older mothers, and if they survive they are more likely to have anemia, and be stunted (Branson et al. 2011; Finlay et al. 2011).

Schooling has the potential to affect the probability of adolescent childbearing through several mechanisms. By increasing a woman's earning power, it can increase the opportunity cost of children (Becker 1981; Schultz 1981) and can also increase a woman's relative bargaining position within the household (Mason 1986). It can also reduce adolescent fertility through increase in woman's knowledge of and access to reproductive health services (Rosenzweig and Schultz 1989). Ultimately, however, education must work though one of the proximate determinants of fertility: marriage and sexual activity, contraceptive use, abortion, and infecundity (Bongaarts 1978) if it is to have an effect.

Previous research has shown that schooling is associated with a lower probability of teenage fertility. Early discontinuation of formal education is associated with adolescent pregnancy in rural Nigeria (Okonofua 1995). In cross country analyses of women's schooling and fertility, educated women are less likely to marry or bear children at an early age (Martin 1995). An analysis of 23 Sub-Saharan African found that areas with low infant mortality and high female education had low fertility levels (Kirk and Pillet 1998) .

While there is clearly an association between education and early childbearing, isolating the causal effect is complex because schooling may simply be a marker for a woman's wider family and community background and because having a child as a teenager may affect the mother's educational achievement (Angrist and Evans 1999). Having a child before age 20 reduces schooling by three years in the United States (D. H. Klepinger et al. 1995). In Kwazulu-Natal, South Africa, 74% of young women ages drop out of school within a year of pregnancy (Grant and Hallman 2008) while girls in Cape Town, are 6% less likely to complete high school by age

20 if they have borne a child (Ranchhod et al. 2011). This means we have to be careful when trying to identify a causal effect of schooling on teenage fertility since there is the possibility both of confounding by omitted variables and reverse causality.

Several studies have used variation in policies that affect schooling to trace out the causal effect of girl's education to teenage fertility. An education policy reform in Nigeria significantly affected women's educational attainment and early fertility (Osili and Long 2008) . Intervention programs targeted at lowering barriers to attending school have reduced teenage pregnancy in low and middle income countries (McQueston et al. 2012). An educational reform in Kenya increased female educational attainment and delayed marriage and fertility (Chicoine 2012). Similarly, a school building program in Indonesia increased education levels and delayed fertility (Breierova and Duflo 2004). A randomized controlled trial that reduced the cost of school uniforms in Kenya, found that the cost reduction not only reduced dropout rates, but also reduced teenage marriage and childbearing (Duflo et al. 2006) . Our paper presents further evidence of causal role of education in significantly reducing teenage birth in women by examining the effects of an educational reform in Ethiopia in 1994 that immediately led to increased female enrollment.

The 1994 reform involved abolishing school fees for grades one to ten, a revised school curriculum, the introduction of local languages for instruction, a school feeding program, and was associated with increased public spending on schools. Schools in Ethiopia saw large enrolment increases immediately following the policy change (World Bank and UNICEF 2009). We estimate that the change in policy induced a jump in years of schooling achieved by women who were six or seven years of age at the school entry date in 1994 by about 0.75 years relative to the baseline level of around 2.5 years of schooling for those who were eight or none years old at the time of the reform and who were not exposed to the policy change at the time of their initial primary school enrolment decision.

We use a regression discontinuity approach to estimate the effect of the induced increase in schooling on teenage fertility. The method compares women who were six or seven years old in 1994 and who were affected by the policy and those who were eight or nine years old at the time. The regression discontinuity estimates are based on the idea that unobserved characteristics will be similar between the women in the pre- and post-treatment groups due to the short time window being considered, though we do allow for a linear time trend in education and fertility over the period. We find that each additional year of schooling reduces the probability of a teenage birth by about 0.06. In our cross sectional data; teenage fertility occurs in around 61% of women with no education as compared to 16% of women with completed primary school (8 years or more); our findings suggest that the causal effect of schooling on teenage marriage is sufficiently large to explain this observed gradient.

We also use our regression discontinuity design to investigate the effect of schooling on the probability of marriage and age at first sex being below age twenty. We find that educational attainment reduces the probability of teenage marriage by almost exactly the same probability as it does teenage birth. We find some reduction in reported age at first sex being below twenty with higher levels of education but the effect is not statistically significant. This may be because of a general under reporting and age at first sex due to stigma associated with reporting premarital sexual activity (Curtis and Sutherland 2004).

Our results are consistent with the findings (Portner et al. 2011) that in Ethiopia family planning and education act as substitutes rather than complements; family planning reduces fertility of women who did not go to school, but does not seem to have a significant effect on women with formal education. These results suggest that in Ethiopia education acts as a distal determinant of adolescent fertility by increasing women's autonomous decision-making power and lowering their probability of marriage rather than by increasing their bargaining power and contraceptive use within marriage.

Our results depend on the absence of other policy reforms that could differentially affect the observed teenage reproductive behavior of our cohorts born between 1986 and 1989 that we use to estimate the effect of the educational reform. A large change in the availability of family planning services would be one such variable whose omission might affect our results. However we do not see such changes over the relevant time interval. Abortion to save the life of the mother and protect her physical and mental health has been legal in Ethiopia from at least 1960 onwards. Ethiopia adopted a new population policy in 1993 (The Transitional Government of Ethiopia 1993). This adopted population targets, encouraged social developments that would reduce desired fertility, and removed legal restrictions and improved access to contraception. In 1993 our birth cohorts were between 6 and 10 years old, and so both our treatment and control groups would have all spent their reproductive lives under the post 1993 regime. Access to family planning services in Ethiopia increased smoothly over time after 1993 (Portner et al.

2011); our regression discontinuity model estimates are robust to these changes in availability of family planning since we control for smooth time trends across cohorts.

In the next section we discuss the details of the 1994 educational reform in Ethiopia and the theoretical framework that informs our study. In section 3 we present our dataset which is drawn from the 2011 Ethiopian Demographic and Health Survey and discuss our empirical strategy. We present our results in section 4 and the robustness of our experiment in section 5. We then discuss potential limitations in section 6 and conclude in section 7 with some remarks on policy implications.

2. The 1994 Education Reform in Ethiopia

Education Policy in Ethiopia underwent a radical change with the new Education and Training Policy and Education Sector Strategy introduced in 1994. The government removed school registration and tuition fees for grades one to ten, and introduced curriculum reform, school feeding programs and teaching in vernacular languages. Furthermore, the government also increased its education budget and established new schools and trained more teachers to improve access, especially at the primary school level.

While the policy change in 1994 abolished school registration and tuition fees from grades one through ten, students were still expected to pay other supplemental fees, such as a certificate fee, identity card fee, a sports fee, and a stationary and books fee. Before the policy reform, the amount of registration fee varied according to region and type of school. The registration and tuition fees averaged around 10-15 birrs per student annually. These fees, although small, acted as deterrent to education for children belonging to the poorest quintile in Ethiopia (World Bank and UNICEF 2009). Along with the fees removal, other aspects such as use of local languages, increased education budget may also have been important in increasing enrolment. Before the policy reform, Amharic was the only language of instruction in Ethiopia. Introduction of local languages for instruction in grades 1-8 encouraged enrolment in regions where Amharic was not the local language. After the reform, over 20 languages were used in government schools in Ethiopia (Ministry of Education, Ethiopia 2002).

The increased education budget following the reform was focused on primary education, with funding for establishing primary schools in rural areas and training more teachers, which improved access to primary education. Government expenditure in education increased from 491.9 million Birrs, or 8% of total government expenditure in 1990, to 1337.0 million Birrs, which was 13% of total government expenditure by 1995 (World Bank and UNICEF 2009). Complementing the 1994 policy reform, the Education Sector Development Program started in 1997 introduced vocational opportunities for people with less than high school or secondary school education. Vocational training opportunities were provided to graduates of grade 4, 8 and 10 (Ministry of Education, Ethiopia 2007).

3. Data

Our analysis uses the 2011 Ethiopian Demographic and Health Survey (Central Statistical Agency and ICF International 2012). Since our main dependent variable is birth before 20 (teenage birth), we limit our data to women aged at least 20 years old at the time of the survey for our analysis[1]. Education is measured by years of schooling, with a maximum of 12 years, to avoid the problem that women in their early twenties may not have completed their education. Birth cohorts are coded to reflect women's birth year in terms of eligibility for school enrollment. The start of the school year and usual month of enrolment in Ethiopia is September and children are eligible to enter primary school at age seven. This means our "1987" birth cohort is women born in the period September 1986 to August 1987. This cohort was 7 years old on 1st September 1994 and is therefore the first cohort to reach the normal entry age for school after the education reform.

Using these birth cohort variables, based on the woman's year and month of birth, we construct two dummy variables to measure exposure to the policy change. The first is a full exposure dummy which corresponds to being 7 years old or younger on September 1st 1994 so that all of the woman's potential school years were under the policy reform. The second is as partial exposure dummy which corresponds to being eight to eighteen years in September 1994. These women may have been encouraged to start school after age seven due to the reform, or they may have been in school already and encouraged to stay in school by the reform. In practice we find this partial exposure dummy has little effect on educational attainment.

Data on teenage births is taken from the woman's fertility history. It represents a first birth before age twenty. We use teenage births rather than teenage pregnancies as our main outcome variable because there is evidence of underreporting of pregnancies that result in termination rather than births (Barreto et al. 1992; Casterline 1989; Magnani et al. 1996) with neither

[1] We get similar estimates and confidence intervals for regressions using birth under 18 and marriage under 18 as dependent variables and including all women 18 and over at the time of the survey in our analysis..

pregnancy or termination being reported because of the stigma associated with abortion (Fu et al. 1998; Jones and Forrest 1992; Smith et al. 1999). This means that we do not distinguish between a reduction in fertility due to lower pregnancy rates and one due to higher rates of termination. Our other outcome variables are marriage before age twenty, and age of first reported sex before age twenty. While we report results for sex before age twenty women may misreport their age at first sex due to stigma associated with pre-marital sex (Meekers 1995).

Table 1 shows descriptive statistics, the mean, standard deviation, maximum and minimum for each variable we use in our analysis for women born in the four year window around the critical date for exposure to the reform (two years each side of 1st September 1987) our 1985 to 1988 cohorts. Around 47% of women in our sample have had at least one child in their teenage years. Similarly, 63% of the women were in union by twenty years of age and 67.1% report having had sex by age twenty. The average years of schooling in our sample is 2.97 years, though 53.5% of the women do not have any formal schooling. The largest religious group was Orthodox Christian. The second largest religion in the sample is Islam followed by Protestantism. Ethnicity is coded to reflect the four major ethnicities represented in the sample—Affar, Amhara, Oromo and Tigrie. The number of siblings and birth order of the woman are included because educational attainment might differ in families of different sizes or between the oldest and the youngest sibling. On average, women in the sample have 6 siblings, and are the third child of the family.

Table 2 shows the mean of our outcome variables by education level for the women born in the same 4 year window as Table 1. There is a strong correlation between education level attained and our outcome variables. 60.9% of women with no education have had a teenage birth compared with 15.6% of women with completed primary, or higher (at least 8 years) levels of schooling. A similar gradient with education is seen for teenage marriage and teenage sex.

A key test for regression continuity models is that the explanatory variable should actually be discontinuous at the point of policy change. Figure 1 shows the average years of schooling by birth cohort. There is a marked increase in average schooling years of women born in 1987 or after compared to the women born before. This increase matches exactly the major educational policy change in Ethiopia in 1994 when the 1987 birth cohort was eligible to attend school. Our regression discontinuity approach exploits this major policy change to assign women into different categories of reform coverage. The effect of partial coverage, those birth cohorts that should have already been in school when the policy change occurred, is less clear cut in Figure 1 and it is difficult to see any effect over and above the trend towards higher levels of education over time.

Figure 2 shows the proportion of women that had children before 20, by birth cohort. We observe a steady decline of the proportion of women who give birth before age 20 with later birth cohorts. There does appear to be a decline in teenage childbearing for the cohort fully exposed to the educational reform. In the next section we estimate the size of this decline and test whether this decline is statistically significant using our regression discontinuity model.

4. Results

We use for our main results data from a four-year window, with women born September 1984-August 1988. We want to take a narrow window to make women born to be eligible to enter education just before and after the 1994 reform to be as similar as possible. However we also want to have a reasonable large data set, and to be able to estimate a time trend in teenage birth to be sure that or policy is not just picking up an underlying trend towards higher education. In the appendix we also report results for all women in the sample using the policy as an instrument for education.

The first stage results from the regression discontinuity model with a four-year data window are shown in Table 3. We include only one dummy variable, for full exposure to the policy. The older children are partly exposed in that they were seven years old and eligible for school before the policy reform but the reform may have increased the incentives to attend school later. In this regression discontinuity model we estimate the educational reform increased the years of schooling by 0.76 years in the cohorts that were fully exposed to the policy as opposed to partially exposed. This corresponds to the jump in education attainment with the policy reform seen in Figure 1.

In Table 3 we control a trend in education attainment by birth cohort. We also control for the woman's ethnicity and religion and for the number of siblings the women has and her birth order among these siblings, which may affect her access to education. Women with many siblings are less likely to be educated because the household resources will have to be divided among more children. Similarly, women of lower birth order, or women who are born earlier could have better chance of being educated because the household resources might allow for the education of fewer children. We do not control for variables such as husband's education or measures of current household socioeconomic status, such as the asset index, since these variables may be endogenous and the result of teenage education and fertility. In all models, we cluster the standard errors at the primary sampling unit level, to control for the survey design. We find later birth cohorts tend to have more education. Orthodox Christians tend to have higher levels of education than the other religions groups. The baseline Amhara ethnicity tends to have higher levels of education than other ethnicities. Women with many siblings, and those with high birth order, tend to have lower levels of education.

The reduced form model of the effect of education policy on adolescent reproductive behavior reported in Table 4 shows significant effect of the reform on childbearing before twenty or marrying before 20. Since our outcome variables are binary we estimate a probit model. Rather than report the probit coefficients themselves, which are difficult to interpret, we report the marginal effects on the outcome variable. These marginal effects are very similar to the results of a linear probability model. Women who were fully covered by the new education policy on average were 8 percentage points less likely to bear children early and 6.8 percentage points less likely to marry early. We do not see a statistically significant effect of the policy on reported teenage sex in the reduced form model.

In Table 5 we report the second stage results where we instrument years of schooling with the fitted values from the first stage (as shown in Table 3). Again we show marginal effects from a probit model. The coefficient on years of schooling in Table 5 can therefore be interpreted as the change in probability of the outcome with a one year increase in schooling. In column 1 of table 5 we find significant negative effect of education on probability of teenage birth. Each additional year of schooling decreases the probability of adolescent birth by 0.066. We also find significant effects of ethnicity and birth order on teenage births. The estimated size of the education effect on teenage births is large. But this is consistent with the observation in Table 2 of a large gradient in teenage births with education. According to Table 2 women with completed primary education have a probability of a teenage birth of 0.16 as opposed to a probability of 0.61 for those with no schooling. This reduction of 0.45 in the probability of a teenage birth suggests a year of schooling reduces the probability of a birth by 0.056, which is close to our causal estimate.

We also examine the effect of education on teenage marriage and teenage sex. Column 2 of Table 5 reports the effect of education on the probability of a teenage marriage. Each additional year of schooling reduces the probability of adolescent marriage by 0.060. This is almost exactly the same magnitude as found in column 1 for a teenage births. Column 3 of Table 5 reports estimates of the effect of education on teenage sex. Here we do not find a statistically significant effect.

To check the robustness of our results we run with model on different windows for the birth cohorts around the point of full coverage. The main coefficient of interest, the effect of a year of schooling on the probability of each reproductive outcome, is shown in Table 6. Moving to a shorter two-year window reduces the sample size. We also have to remove the cohort trend since we only have two years of data and a trend is collinear with exposure dummy. We also report results for 6 and 8 year windows which do allow for cohort trends. All the results in Table 6 are very similar to our main results reported in Table 5.

Tables 7 and 8 report the first and 2^{nd} stage of running the model on our full sample of women aged 20 to 49 from the 2011 Ethiopian Demographic and Health Survey. In this model we add a dummy for partial exposure to the policy reform (age below 18 at the time of the reform) and a quadratic term in birth cohort to allow for a nonlinear time trends. We also cluster at the birth cohort level, as well as the primary sampling unit level, to control for unobserved correlations between women born in the same year (Bertrand et al. 2004). We again find significant effects of full exposure to the policy on years of schooling, and of the educational attainment on probability of marrying or bearing a child before 20. We do not however, find a significant effect of partial exposure to the policy. The estimated effect of years of schooling on teenage births and teenage marriage are very similar to those found in our main results in table 5. With this much larger sample we also find a significant effect of education in reducing teenage sex.

While the educational policy change seems to have a significance effect on educational attainment we need this first stage link to be strong in order to avoid the problem of a weak instrument (Murray 2006). We can calculate the Cragg-Donald F-Statistic for the full sample instrumented regression[2] if we have a linear probability model in the 2^{nd} stage. In this case the Cragg-Donald F-Statistic is 41.9, which is well above the Stock-Yogo weak identification test critical values at 10% maximal IV size distortion of 19.9. We therefore take it that our instrument is not weak. Note that it is difficult to undertake this type of weak instrument test with clustering

[2] With standard errors clustered at the birth cohort and the primary sampling unit level

at the birth cohort level with a short window of data; in our four-year window we only have four clusters of birth cohorts and the degrees of freedom in the model are essentially the number of these clusters.

Our preferred model is the four-year window. This has a reasonable sample size, and allows for a time trend in the outcome variable. We can assume that women born in this short time interval face similar unobserved forces on their reproductive behavior. While the full sample gives a much larger data set it comes at the cost of comparing across women who may have experienced very different settings and it is not clear if these differences can be fully captured by a quadratic time trend. For example, older women at the time of the survey, who reached reproductive age prior to the change in population policy in Ethiopia in 1993 may have been in a noticeably different regime in regard to access to family planning services.

5. Conclusions

Our study investigates the causal effect of female education on teenage fertility. Exploiting the Ethiopian education policy change in 1994, we show that the reform was associated with a large increase in schooling for the two cohorts of women that were born in 1987 and 1988 who were exposed to the reform when entering school age relative to the cohort born earlier. We also observe a large decrease in teenage marriages and in teenage births in precisely these cohorts. We estimate a significant effect of schooling on the early reproductive behavior of women.

The estimated effect sizes are quantitatively large, with each year of schooling lowering teenage marriage rates and fertility rates by about 6 percentage points. The results suggest that the large increase in educational attainment in Ethiopia over the last 30 years, with women born in 1960 having around 0.5 years of schooling on average compared to about 4.0 years for those born in the early 1990s has reduced the numbers of women having teenage births by about 20 percentage points. Our result, a strong causal relationship in Ethiopia between female schooling and early marriage and fertility, adds to an expanding body of work finding this relationship to hold in different settings.

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Table 1: Descriptive Statistics for Women born in School Years 1985-1988

Variables	Mean	Standard Deviation	Min	Max
Teenage Birth	0.470	(0.017)	0	1
Teenage Marriage	0.629	(0.017)	0	1
Teenage Sex	0.671	(0.015)	0	1
Years of Schooling	2.978	(0.143)	0	12
Exposure to Policy	1.389	(0.015)	1	2
Religion				
Orthodox	0.466	(0.026)	0	1
Protestant	0.221	(0.022)	0	1
Muslim	0.290	(0.027)	0	1
Other	0.024	(0.005)	0	1
Ethnicity				
Amhara	0.299	(0.019)	0	1
Oromo	0.365	(0.022)	0	1
Tigrie	0.065	(0.005)	0	1
Other	0.272	(0.016)	0	1
Birth Order	3.207	(0.073)	0	10
Number of Siblings	5.977	(0.083)	0	12

For women born September 1984- August 1988.

Table 2: Average Teenage Birth, Marriage and Sexual Activity by Educational Attainment for Women born in School Years 1985-1988

	Sample Mean (percentages)		
Educational Attainment (Years of Schooling)	**Teenage Birth**	**Teenage Marriage**	**Teenage Sex**
None (0)	60.9	79.2	80.9
Incomplete Primary (1-7)	41.4	57.8	64.8
Completed Primary & Higher (8 or more)	15.6	21.9	31.2
Sample Average	47.0	62.5	67.1

For women born September 1984- August 1988.

Figure 1: Average Years of Schooling by Birth Cohort and Reform Coverage

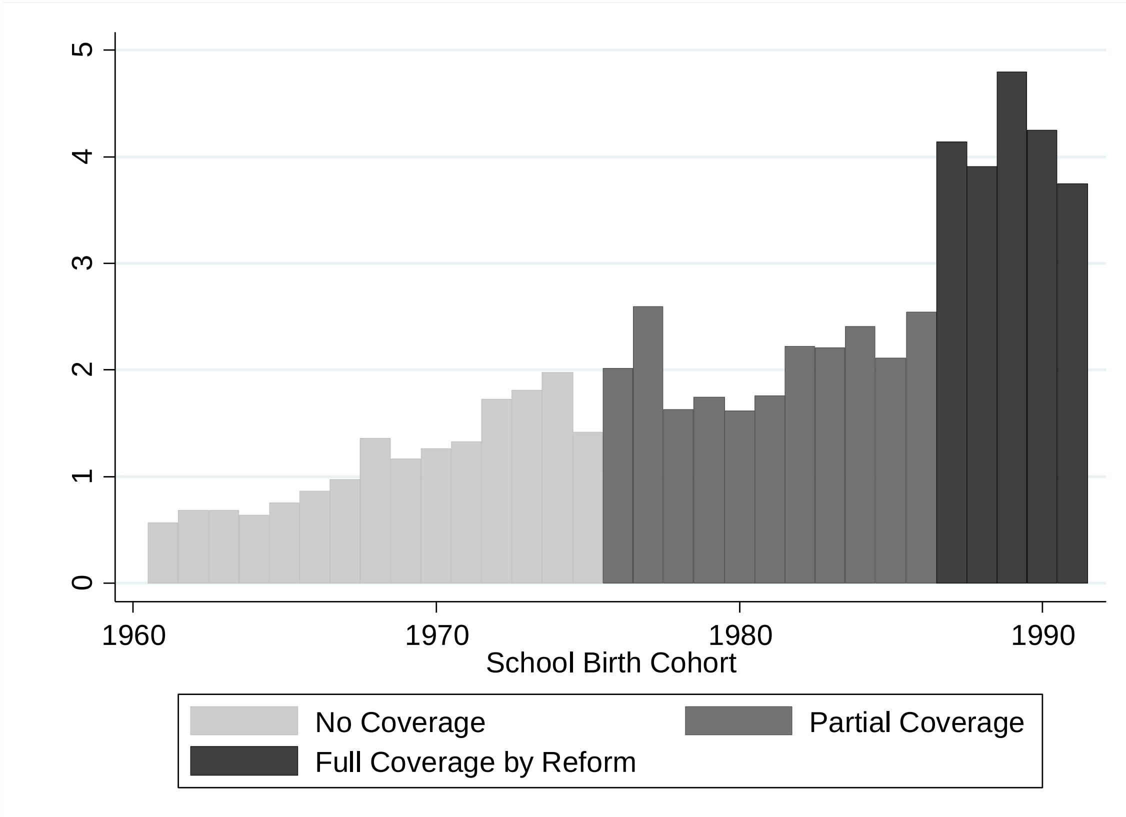

Figure 2: Probability of Childbearing before 20 by Birth Cohort and Reform Coverage

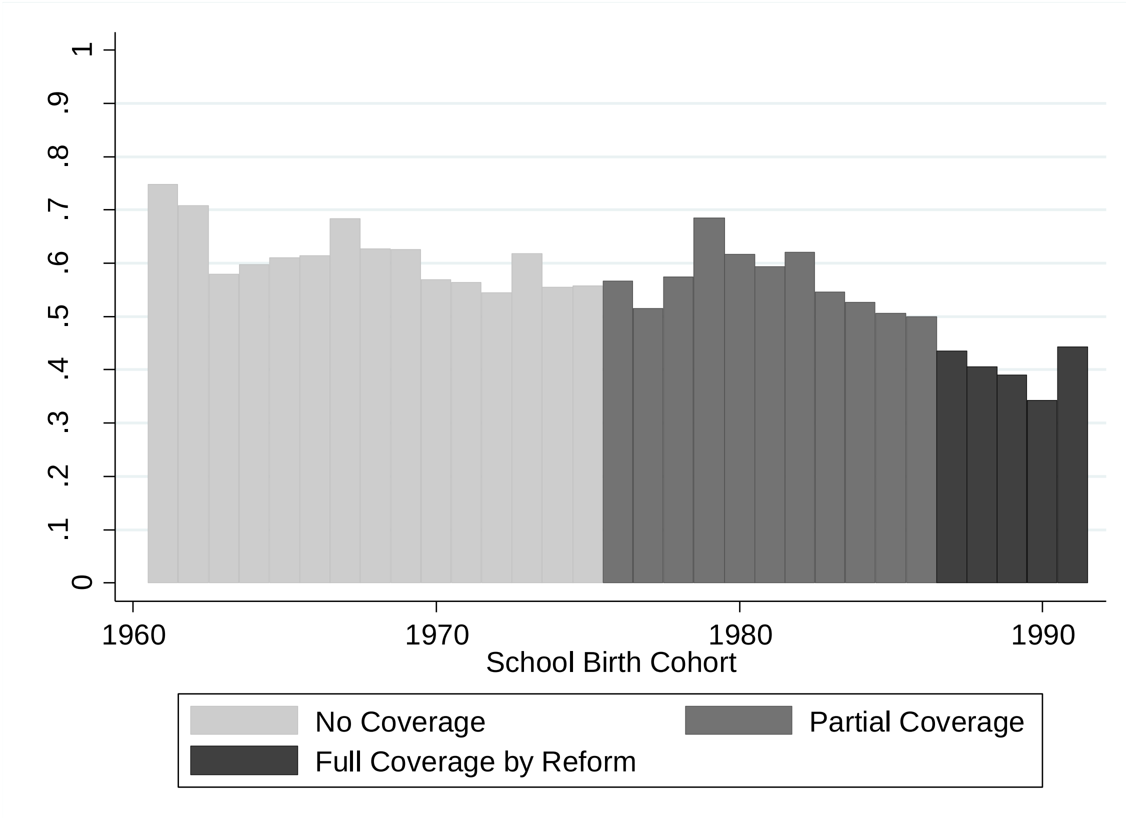

Table 3: First Stage--Effect of the Education Reform on Years of Schooling

Two Year Window around Reform

Dependent Variable: Years of Schooling	
Full Coverage	0.745**
	(0.321)
Cohort Trend	0.327**
	(0.130)
Religion	(Base: Orthodox Christian)
Protestant	-0.446
	(0.350)
Muslim	-2.260***
	(0.278)
Other	-1.703***
	(0.594)
Ethnicity	(Base: Amhara)
Oromo	-0.442
	(0.322)
Tigrie	-0.298
	(0.371)
Other	-1.072***
	(0.347)
Number of siblings	-0.071*
	(0.040)
Birth order	-0.145***
	(0.039)
Constant	-3.181
	(3.394)
Observations	2,740

Estimated for women born September 1984- August 1988. Full coverage means age 7 or younger on 1st September 1994 and thus fully exposed to the education reform. The cohort trend is a linear function of year of birth. Standard errors in parentheses, clustered at primary sampling unit level.

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

Table 4: Reduced Form--Effect of Education Reform on Adolescent Reproductive Behavior

Two Year Window around Reform

	Dependent Variables		
	Teenage Birth	**Teenage Marriage**	**Teenage Sex**
Full Coverage	-0.081**	-0.068*	-0.013
	(0.036)	(0.036)	(0.036)
Cohort Trend	-0.008	-0.005	-0.026*
	(0.015)	(0.014)	(0.014)
Religion	(Base: Orthodox Christian)		
Protestant	0.079**	0.088**	0.062*
	(0.037)	(0.038)	(0.035)
Muslim	0.118***	0.158***	0.101***
	(0.028)	(0.029)	(0.028)
Other	0.105*	0.179**	0.136*
	(0.061)	(0.077)	(0.076)
Ethnicity	(Base: Amhara)		
Oromo	-0.010	-0.092***	-0.062**
	(0.031)	(0.032)	(0.030)
Tigrie	0.071*	-0.062	-0.017
	(0.038)	(0.042)	(0.039)
Other	0.016	-0.048	-0.067**
	(0.032)	(0.033)	(0.031)
Number of siblings	0.011**	0.0035	0.003
	(0.005)	(0.005)	(0.004)
Birth order	0.003	0.014***	0.011***
	(0.005)	(0.005)	(0.004)
Observations	2,740	2,740	2,740

Estimated for women born September 1984- August 1988. Full coverage means age 7 or younger on 1st September 1994 and thus fully exposed to the education reform. The cohort trend is a linear function of year of birth. Standard errors in parentheses, clustered at primary sampling unit level.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Marginal Effects from IV Probit Discontinuity Model—Effect of Education on Adolescent Reproductive Behavior

Two Year Window around Reform

	Dependent Variables		
	Teenage Birth	**Teenage Marriage**	**Teenage Sex**
Years of Schooling	-0.066***	-0.060***	-0.013
	(0.006)	(0.009)	(0.049)
Cohort Trend	0.016	0.016	-0.022
	(0.014)	(0.014)	(0.027)
Religion	(Base: Orthodox Christian)		
Protestant	0.025	0.041	0.057
	(0.036)	(0.042)	(0.039)
Muslim	-0.072	-0.025	0.068
	(0.049)	(0.069)	(0.108)
Other	-0.044	0.023	0.110
	(0.059)	(0.089)	(0.108)
Ethnicity	(Base: Amhara)		
Oromo	-0.036	-0.101***	-0.079*
	(0.024)	(0.036)	(0.041)
Tigrie	0.028	-0.069**	-0.026
	(0.032)	(0.034)	(0.044)
Other	-0.062**	-0.110***	-0.094
	(0.026)	(0.024)	(0.068)
Number of siblings	0.0025	-0.002	0.002
	(0.005)	(0.004)	(0.006)
Birth order	-0.008**	0.002	0.010
	(0.004)	(0.006)	(0.008)
Observations	2740	2740	2740

Estimated for women born September 1984- August 1988. Full coverage means age 7 or younger on 1st September 1994 and thus fully exposed to the education reform. The cohort trend is a linear function of year of birth. Standard errors in parentheses, clustered at primary sampling unit level. The first stage for this instrumental variable regression is shown in Table 3 where the Full Exposure variable is the instrument.

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

Table 6: Robustness--Marginal Effect of Years of Schooling on Adolescent Reproductive Behavior, across models with different Birth Cohorts.

	Dependent Variables		
	Teenage Birth	**Teenage Marriage**	**Teenage Sex**
Two-Year Window	-0.060***	-0.041**	-0.033
(n=1297)	(0.008)	(0.018)	(0.020)
Four-Year Window	-0.066***	-0.056***	-0.013
(n=2740)	(0.006)	(0.009)	(0.049)
Six-Year Window	-0.060***	-0.053***	-0.041*
(n=3684)	(0.010)	(0.013)	(0.024)
Eight-Year Window	-0.059***	-0.047**	-0.031
(n=4933)	(0.009)	(0.017)	(0.021)

Results are the coefficient on years of schooling from the same model as in Table 5 with different windows for the birth cohorts around the start of exposure to the reform. The two year window model does not include a cohort trend which would be collinear with the exposure dummy.

Table 7: First Stage Regressions of Effect of Policy Reform on Years of schooling

Full Sample of Women Aged 20-49 in 2011

Dependent Variable: Years of Schooling	
Coverage	(Base: None)
Partial	0.0755
	(0.2450)
Full	1.441***
	(0.3930)
Cohort Trend	0.138***
	(0.0377)
Cohort Trend Squared	-0.00188
	(0.0014)
Religion	(Base: Orthodox Christian)
Protestant	-0.630**
	(0.2610)
Muslim	-2.208***
	(0.2160)
Other	-1.676***
	(0.3010)
Ethnicity	(Base: Amhara)
Oromo	-0.403
	(0.2420)
Tigrie	-1.456***
	(0.3820)
Other	-1.201***
	(0.2440)
Number of siblings	-0.0509**
	(0.0189)
Birth order	-0.0570**
	(0.0219)
Constant	2.678***
	(0.3540)
Observations	12,680

Full coverage means age 7 or younger on 1st September 1994 and thus fully exposed to the education reform. The cohort trend is a linear function of year of birth. Standard errors in parentheses, clustered at primary sampling unit and birth cohort level.

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

Table 8: Second Stage Estimates of Effect of Schooling on Teenage Reproductive Behavior

Full Sample of Women Aged 20-49 in 2011

	Teenage Birth	Teenage Marriage	Teenage Sex
Years of Schooling	-0.0693***	-0.0530***	-0.0448***
	(0.0137)	(0.0104)	(0.0083)
Cohort Trend	0.00331	0.000918	-0.00087
	(0.0033)	(0.0023)	(0.0016)
Cohort Trend Squared	-4.41E-05	-0.00012	-1.48E-05
	(0.0001)	(0.0001)	(0.0001)
Religion	(Base: Orthodox Christian)		
Protestant	-0.00061	0.0148	0.00568
	(0.0228)	(0.0199)	(0.0194)
Muslim	-0.0639*	0.0118	-0.0206
	(0.0337)	(0.0254)	(0.0237)
Other	-0.110***	-0.00479	-0.0306
	(0.0290)	(0.0329)	(0.0296)
Ethnicity	(Base: Amhara)		
Oromo	-0.0538***	-0.100***	-0.0631***
	(0.0183)	(0.0158)	(0.0148)
Tigrie	-0.0351	-0.0689***	-0.0049
	(0.0359)	(0.0229)	(0.0157)
Other	-0.103***	-0.111***	-0.101***
	(0.0253)	(0.0174)	(0.0157)
Number of siblings	-0.00188	-0.00214	-0.00132
	(0.0023)	(0.0022)	(0.0018)
Birth order	-0.00335**	0.00372**	0.00242
	(0.0016)	(0.0015)	(0.0016)
Constant	0.759***	0.915***	0.898***
	(0.0548)	(0.0400)	(0.0292)
Observations	12680	12680	12680

Full coverage means age 7 or younger on 1st September 1994 and thus fully exposed to the education reform. The cohort trend is a linear function of year of birth. Standard errors in parentheses, clustered at primary sampling unit and birth cohort level.

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