Free Primary Education, Schooling, and Fertility: Evidence from Ethiopia

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October 2016

Abstract

This paper investigates the causal relationship between women's education and fertility by exploiting variation generated by the removal of school fees in Ethiopia. The increase in schooling caused by this reform is identified using both geographic variation in the intensity of the reform's impact and the temporal variation generated by the implementation of the reform. The model finds that the removal of school fees in Ethiopia led to an increase of over 1.5 years of schooling for women. A two-stage least squares approach is used to measure the impact of the exogenous increase in schooling on fertility. Each additional year of schooling led to a reduction of 0.2 births, a delay in sexual activity, marriage, and the timing of both the first, second and third births. There is also evidence that the increase in schooling led to improved labor market outcomes, and a reduction in the desired number of children. Additionally, there is evidence of strategic use of hidden forms of contraception, only after family size becomes sufficiently large or after two sons have been born.

JEL classification: O55, J13, I25, I26

Keywords: free primary education; Ethiopia; schooling; fertility

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1 Introduction

As evidenced by its prominent position among the Millennium Development goals, universal primary education has been a central goal of the international development effort for a number of years. As far back as the 1970s, the most readily available policy tool to promote enrollment has been the removal of schooling fees. This type of policy was implemented in Kenya and Nigeria in the 1970s, in Zimbabwe and Tanzania in the 1980s, in Ethiopia, Malawi, and Uganda in the 1990s, and has been aggressively pursued as a key tool in achieving the goal of universal primary education by international development organizations as evidenced by over a dozen more countries removing schooling fees since the turn of the millennium (World Bank, 2009; Kattan and Burnett, 2004). Data from the UNESCO Institute for Statistics (UIS) show that, on average, these reforms increase enrollment by about 25 percent in the following year, but also increase pupil-teacher ratios by 10 percent. Work by Pritchett (2001) has called into question the efficacy of increases in schooling in developing parts of the world, and the potential reduction in quality generated by these "big-bang" education reforms is a prime example of an environment in which a deterioration of the quality of schooling could fail to increase learning and improve the lives of the newly enrolled students. However, evidence of the returns to reductions in these user fees is lacking, as highlighted by the recent systematic education review from the International Initiative for Impact Evaluation (3ie – Snilstveit et al., 2016).

This paper evaluates the returns to a nationwide free primary education program in Ethiopia by examining the relationship between the schooling generated by the removal fees and women's fertility decision, and the potential mechanisms driving the relationship. In light of recent findings by Fort et al. (2016), that increases in education generated by 20th century compulsory schooling laws did not reduce fertility rates in continental Europe, our ex-ante expectation of a universally negative relationship between schooling and fertility has been called into question. However, using a two-stage least squares (2SLS) model to identify the causal relationship, each additional year of schooling in Ethiopia is found to lead to a reduction of 0.214 births. The increase in schooling is identified by combining two dimensions of variation, the timing of the reform, and geographic variation in education outcomes for cohorts who completed their schooling prior to the reform. The identification strategy employed in this paper uses age, schooling, and location data that are readily available for countries in most parts of the world. Motivated directly by the work from Bleakley (2010); Lucas (2010, 2013); Lucas and Mbiti (2012a,b), the identification utilizes the concept that although the policy itself is applied uniformly across the country, the intensity of the reform in a specific location depends on the pre-existing characteristics of that area. In this setting, removing school fees from an area of high educational attainment will have a small impact relative to removing the same fees in an area that had low pre-reform educational attainment.

Investigating the mechanisms underlying the ways in which schooling impacts fertility decisions yields an increasing understanding of how and why women choose to change their behavior. This analysis finds that women's reduction in fertility is associated with a postponement in first birth through a delay in sexual activity in their late teens and reduced rates of marriage from the ages of 19 to 21. Beyond the age of 21, there is evidence that women continue to delay their second and third birth, and unsurprisingly, there is a reduced impact on first birth at these later ages following the dissipation of effect on sexual activity and marriage.

The additional schooling generated by the removal of school fees is also found to lead to an increased likelihood of receiving cash payment for their work, and being less likely to work in subsistence farming and also in agriculture more generally. The increase in labor market productivity found in these results likely led to an increase in the opportunity cost of raising children, and the model finds evidence of a reduced demand for children.

Finally, although there is no evidence of an average increase in contraception use for the full sample, there is evidence that the increase in schooling led to a specific and strategic use of contraception. As family size grows, the increase in schooling leads to larger increases in contraception use, specifically with the use of contraception not visible to husbands. In addition to the evidence that women continue to delay the birth of their second and third child through the age of 24, this evidence suggests escalating changes in behavior aimed at preventing future pregnancies, making it unlikely that births were simply delayed to later ages. The increased of hidden contraception is also found in Zambia by Ashraf et al. (2014), but is also associated with increased stress within the woman's relationship with her husband. This is likely due to the short experimental time frame within which the women needed to make their decision. The increased use of hidden contraception in Ethiopia begins only after the second child is born, and for families with fewer than four children, only occurs if the family has two or more boys. These results suggest a specific and strategic behavior exhibited by women, who likely understand the preference for children that exists within their household, and may allow them to avoid the negative externality of contraception use found in Ashraf et al. (2014).

Earlier literature (Ainsworth et al., 1996; Lam and Duryea, 1999; Schultz, 1994, 1997) documents the negative relationship in the data that exists between schooling and fertility. To identify the effect of education, McCrary and Royer (2011) exploit discontinuities at school-starting ages in California and Texas, Osili and Long (2008) use school construction in Nigeria, and Keats (2014) uses a discontinuity around the implementation of free primary schooling in Uganda. All three of these papers find that education led to a reduction in fertility, and the two papers examining schooling reforms in Africa find evidence of a reduction between 0.2 to 0.3 births for each additional year of school, similar to the effect size found in this paper.

However, a number of other papers in the recent literature find evidence of an initial delay in fertility, but no change in total fertility (Black et al., 2008; Monstad et al., 2008; Fort, 2012; Geruso et al., 2014), and the previously mentioned paper by Fort et al. (2016) finds a positive causal relationship between schooling and fertility in continental Europe and a negative relationship in the UK. This literature finds surprisingly mixed results, suggesting the need for a greater understanding of the linkages between schooling and fertility.

These results are significant in three ways. First, the paper presents an example of an application of an identification strategy that can be applied to settings to study national level reforms with a minimal amount of variables needed for identification. Measuring the impact of national level removal of user fees was one of the key categories found to need further study in the 3ie systematic review of education policy. Second, the paper finds strong evidence of a negative relationship between schooling and fertility, and the evidence of the pathways through which this relationship exists. Third, the paper finds significant evidence of positive returns to schooling, through both the reduction in fertility and improvement in labor market outcomes. This suggests that in the ways that matter most in developing parts of the world, the increased schooling generated by the reform, even in the presence of increasingly crowded classrooms, led to increases in education that bettered women's day-to-day life.

The paper proceeds as follows. The reform is described in greater detail in Section 2, and the strategy to identify the increase in schooling is outlined in Section 3. The empirical model is discussed in Section 4. Section 5 details the data used in the analysis, as well as the summary statistics of the sample. The results are discussed in Section 6, and Section 7 concludes.

2 Background and Education Reform

After 17 years of military and communist rule a transitional government took control in 1991. During the process of decentralizing power over the next few years this government made significant changes to the Ethiopia's education policy (Ofcansky and Berry, 1993). The first of two reforms that impacted education policy was Proclamation No. 41, released in 1993. This law delegated the local provision of primary and secondary education to each of nine newly formed regional authorities and two independent administrations located in the country's largest cities(Negash, 1996; UNESCO, 2007; Tewfik, 2010).[1] The government's official Education and Training Policy (ETP) was then published the following year, 1994, and was officially forwarded to the regional governments prior to the 1995 school year. The release of the transitional government's official education policy, the ETP, was largely known at the time of Proclamation No. 41. This

[1]The decentralization process led to an increase in teaching using local languages. Zenebe Gebre (2014) finds that the introduction of mother tongue education had a negative impact on enrollment and years of schooling. These findings suggest a downward pressure on schooling that would make finding a positive impact of the removal of school fees more difficult.

general knowledge, and the proclamation's decentralization of power, led to the functional implementation of the ETP at different times in different parts of the country between 1993 and 1996 (Negash, 1996; Oumer, 2009; World Bank, 2009; UNESCO, 2007). This variation will be part of what is taken into account in the paper's empirical strategy.

The central consequence of the ETP was that it required that public education be fee-free for grades one through ten. The identification strategy used in this paper will focus on the removal of these fees, and the idea that the fraction of students completing these grades during the pre-reform period is likely inversely related to the magnitude of the reform's eventual impact. The ETP did not change the school starting age, which stayed consistent at 7 years old, but did extend primary school from six to eight years, and set junior and senior secondary school to each be two years (Oumer, 2009; World Bank, 2009).

These reforms, both Proclamation No. 41 and the ETP, led to significant increases in enrollment in Ethiopia. Evidence of this increase can be seen using grade one enrollment data from UIS. As seen in Figure 1, there was a decline in enrollment in the late 1980's that coincides with the conflict that led to the eventual overthrow of the ruling government, but no immediate reversal following the end of the conflict in 1991.[2] The initial increase in enrollment began in 1993 following the implementation of Proclamation No. 41; grade one enrollment increased by 45 percent, over 280,000 students. The second largest year-to-year growth was in 1995 and coincided with the implementation of the ETP; grade one enrollment increased by 317,000 relative to the previous year, an increase of 28 percent. These reforms ultimately led to an increase in schooling of more than one full year, at least a six percentage point increase in passing the grade eight exam, and a roughly ten percentage point increase in the literacy rate (Chicoine, 2016).

There are a number of common critiques of this type "big-bang" of reform. First and foremost, decrees restricting local school's ability to levy tuition fees are difficult to enforce, and likely are not fully implemented. This type of a compliance problem only makes finding estimates more difficult. To the extent that fees are removed, this can cause a significant shortfall in the finances of local schools for which the central authorities are often not able to fully compensate. Although there were significant capital investments in Ethiopia that accompanied the reforms (World Bank, 2005), pupil-teacher ratios increased by 40 percent between 1992 and 1995, and by 60 percent if the time frame is extended to 1996. Growth in the number of students per school grew at an even faster rate, by 75 percent between 1992 and 1995 and 90 percent through 1996 (Ministry of Education, 1995, 1996, 2000). Although there has been some evidence of learning that coincided with the increase in enrollments, as discussed above, these types of difficulties following the rapid expansion of schooling are not unique to the Ethiopian experience (World Bank, 2009).

[2] Ethiopia uses their own calendar that begins its new year, and academic year, around the second week of the Gregorian (Western) September; the numerical Ethiopian year is either seven or eight years behind the Gregorian calendar depending on the time of year. The year referenced in the text is the Gregorian year in which the academic year began.

3 Identification Strategy

The intensity of the Ethiopian education reform is jointly determined by both location within Ethiopia, and the timing of the reform's implementation. Although the intensity of the reform does not vary within Ethiopia, the ETP removed all school fees for grades one through ten, the potential magnitude of the reform's impact does depend on local pre-reform characteristics. This concept is similar to the Bleakley (2010); Lucas (2010, 2013) strategy that utilizes pre-eradicaiton levels of malaria to identify local variation the impact of eradication programs, Lucas and Mbiti (2012a,b) more directly applied this concept to the post-2000 removal of school fees in Kenya. The central idea being that following the removal of school fees in Ethiopia there were 10 years of fee-free schooling available to every single person. However, prior to the reform some portion of these years were already being completed. Therefore, in areas of the country where a higher fraction of the newly available fee-free schooling were already being obtained the removal of school fees would have a smaller impact relative to parts of the country where very few students attended school in the pre-reform period. Across Ethiopia this pre-reform level of education will be evaluated for each of 60 zones, the second administrative level, similar to counties in the United States.

The maximum potential magnitude of the reform's impact, $\overline{M_z}$, is calculated for each zone of Ethiopia. Conceptually, $\overline{M_z}$ is the maximum number of additional years of schooling that the reform could generate in zone z; which is simply the number of free years newly available; this will be roughly calculated as 10 minus the average pre-reform education level in the zone. To standardize this value relative to the length of pre-reform primary school, which was six years, the difference is then divided by six.[3] This will allow for future comparisons to reforms in other settings, and a magnitude value of one to be consistently interpreted as the "removal of primary school fees." It is important to note that the maximum impact of the reform, $\overline{M_z}$, will only fully impact cohorts who were born late enough that no matter what age they entered grade one they were able to do so in the post-fee environment.

The year that an individual is born combines with this magnitude measure to determine the intensity reform's impact. The year an individual is born determines the set of possible starting ages at which they could enter grade one prior to the implementation of school fees; alternatively, the school entry decision is made in the pre-reform environment. The probability of entering school at age 6, one year early, to age 12, five years late, are estimated for each zone. The the pre-reform entry probabilities are taken from the data, the relative probabilities across all ages are held constant, and the sum of the post-reform entry probabilities are set to equal one assuming every child could potentially enter school in the fee-free environment. If a child is born in 1989 or later, even with a positive probability of starting school a year early, they will only face

[3] The calculation of the maximum magnitude of the reform: $\overline{M_z} = \frac{1}{6}\left(10 - Avg_z\right)$. Where Avg_z is the average pre-reform level of schooling in zone z.

schooling decisions in the post-reform, fee-free, time period, and be assigned a zone specific reform intensity of $\overline{M_z}$. Children born in 1988 will face a pre-reform probability of starting school at age 6, the fraction of students that enter at this age will not have the benefit of the reform in grade 1, but only in grades 2 through 10. The remainder of the 1988 cohort, entering school at ages 7 to 12, will make their entry decision in the post-reform period and receive all 10 years of benefit. Then for the 1987 cohort, entry at both ages 6 and 7 are pre-reform. Age 6 entrants start school two years prior to the reform and will only see the benefit from grades three to 10, and age 7 entrants will only begin to receive the benefit when making the decision to continue grade 2. The fraction of the cohort who did not enter at 6 and 7 could then enter between 8 and 12, and would receive the full benefit across all 10 years. This continues to iterate back to the 1973 cohort where children who enter school as late as possible in the calculation, at age 12, would have only completed nine years of school in the pre-reform period, would then have the opportunity to continue to a fee-free grade 10 in the post-reform environment. Students born in 1972, are the first cohort not to be affected by the reform.

The intensity of the reform varies between the 1972 cohort, the final pre-reform cohort, to the 1989 cohort, the first fully post-reform cohort.

$$I_{zy} = \begin{cases} 0 & \text{if } y \leq 1972 \\ 0 < M_{zy} < \overline{M_z} & \text{if } 1973 \leq y \leq 1988 \\ \overline{M_z} & \text{if } y \geq 1989 \end{cases} \cdot \tag{1}$$

The intensity of the reform, I_{zy}, is determined for zone (z) and birth year (y), and in each zone, and ranges between 0 and $\overline{M_z}$. An individual's location determines the magnitude of the reform's impact through both the zone's pre-reform level of schooling and school entry age probabilities, as discussed in the above paragraph.[4] Location is matched using today's place of residence, in the dataset utilized in this paper information on place of birth is not available. However, data from 2014 Ethiopian Living Standard and Measurement Survey (LSMS) find that over 80 percent of adult respondents still live in their region of birth, suggesting that migration is not overly prevalent. Furthermore, Chicoine (2016) shows that analysis of the reform's impact on years of schooling using the LSMS data yield estimates similar to those using the DHS data from this paper, and that the estimates are not sensitive to three separate strategies to take into account the available migration data in the LSMS dataset. This is further evidence that the distinction between today's zone of residence and place of birth are unlikely to significantly impact the results.

The final piece of the intensity calculation is to identify the timing of the reform in each region of Ethiopia,

[4] The explicit equation used to calculate the full set of intensity measures (I_{zy}) are discussed in detail in the appendix of Chicoine (2016).

this must be done because a portion of the reform was the decentralization of the provision of education (World Bank, 2009). Pre-reform (1989 to 1992) grade one enrollment data from the Ministry of Education (1995, 1996, 2000)is compiled for each of the country's 11 regions is used to predict enrollment over the next four years (1993 to 1996). The annual grade one enrollment level is then compared to the predicted level in each region for the years between 1993 and 1996. Each region is set to be fully implemented at the year furthest above the trend, and fraction below that level in the previous year's sets the level of partial implementation.[5]

4 Estimation Strategy

The central estimating model is a 2SLS model. The first-stage is defined by the following equation:

$$YrsSchl_{izy} \quad = \quad \theta_0 + \theta_1 I_{zy} + \sum_{p=1}^{3} \theta_2^p age_{izy}^p + \delta_z + \tau_y + \delta_z Trend_y + \nu_{izy}. \tag{2}$$

The dependent variable is $YrsSchl_{izy}$, the years of schooling for person i, from zone z, and born in year y, and I_{zy} is the zone and birth year specific estimated intensity of the reform described in Section 3. The value of I_{zy} is scaled to equal one when six years of school, the pre-reform length of primary school, are provided fee-free; therefore, first stage estimates of θ_1 can be interpreted as the impact of providing free primary education. A third order polynomial in age is included to take into account that two waves of the DHS survey are used, and τ_y is a set of birth year specific fixed effects that capture any cohort specific effects of the reform. δ_z is a vector of zone specific fixed effects that capture any time invariant characteristics of the different areas throughout Ethiopia, and $\delta_z Trend_y$ is a set of zone specific linear trends that captures secular changes over time, within each zone of Ethiopia.[6] To ensure that there are a sufficient number of observations on either side of the increasing portion of instrument, the paper's main analysis includes three fully pre- and post-reform cohorts, yielding a sample of women born between 1969 and 1992. This first-stage equation is then used to estimate the exogenous increase in schooling generated by the removal of school fees in Ethiopia. The predicted increase in schooling can then be used in the second-stage to estimate the causal

[5]Estimates removing the partial implementation are also shown in Table 3 and Table 4. Across the regions the reform is set to be fully implemented in the following years: Tigray 1993, Afar 1993, Amhara 1995, Oromiya 1995, Somalia 1993, Benishangul Gumuz 1993, Southern Nations, Nationalities, and Peoples' Region 1995, Gambella 1993, Harari 1995, Addis Ababa 1995 and Dire Dawa 1996.

[6]The set of fixed effects and trends is similar to the empirical strategy used by a number of previous studies evaluating education reforms. These studies include Black et al. (2005); Bleakley (2010); Lucas and Mbiti (2012a,b); Fort et al. (2016); Holmlund et al. (2011); Lundborg et al. (2014).

relationship between schooling and births, or any other outcome of interest.

$$B_{izy} \quad = \quad \alpha_0 + \beta_1 \widehat{YrsSchl}_{izy} + \sum_{p=1}^{3} \alpha_2^p age_{izy}^p + \phi_z + \mu_y + \phi_z Trend_y + \varepsilon_{izy}. \tag{3}$$

The dependent variable B_{izy} is the outcome of interest, initially number of births, for person i, from zone z, and born in year y. The second-stage equation uses the same set of control variables as equation (2), and the coefficient on the predicted years of schooling, β_1, captures the causal impact of one additional year of schooling exogenously generated by the education reform. Standard errors are clustered by zone to allow for within zone correlation Bertrand et al. (2004).

The ordinary least squares relationship (OLS) between schooling and fertility can be studied using a modified version of equation (3), with the predicted level of schooling being replaced with each individual's actual level of schooling, $YrsSchl_{izy}$. However, the OLS estimates are likely if schooling is correlated with unobservable characteristics that also affect the number of children women choose to have. If women who are more likely to obtain higher levels of schooling also have higher career ambition and lower levels of desired fertility, the OLS estimates would be biased. This suggests that the negative relationship that is expected to be found in the OLS relationship could be generated by an omitted variables bias, that requires an alternative identification strategy, such as that proposed in this paper.

The central assumption underlying this identification strategy is that the removal of school fees in Ethiopia only impacts these woman's fertility decision through its effect on their level of schooling. This requires that their year of birth, and their location of schooling are orthogonal to their exposure to the reform. While the timing of birth provides little concern, the zone of residence and schooling would be the dimension more likely to trouble this assumption. This would be problematic if women and families relocated at the time of the reforms implementation in such a way that higher ability students sorted into areas with higher predicted intensity of the reform. However, this type of sorting is unlikely to occur in this setting. The intensity measure is explicitly designed to predict a greater impact of the reform in areas with lower initial levels of schooling. A violation of this assumption would then require the unlikely scenario that higher ability families were moving to areas were worse off at the time of implementation. Furthermore, as mentioned above previously, (Chicoine, 2016) finds evidence that estimates adjusting for migration using three different techniques yield results similar to the baseline estimates, and also finds no evidence of a similar effect on education in settings where no reform occurred (Kenya, Tanzania, Zambia, and Mali).

An alternative concern could be that the benefit or quality of schooling changed at the time of the reform, if this were the case β_1 would still be capturing the impact of the reform, but not explicitly the effect of an additional year of schooling. A potential confounding factor would be if the reform led to the government increasing resources to the parts of the country that had lower pre-reform levels of education in an effort to further increase quality in these regions. Examining the correlation between pre-reform education levels and the change in regional spending on education could provide insight into how funding was allocated following the implementation of the reform; finding a strong negative correlation would suggest a disproportion increase in funding to areas with lower pre-reform levels of education, and would be problematic. However, regional per student spending data in 1993, the first year data are available, exhibits a strong positive correlation with pre-reform education levels. Then looking at the change in spending relative to 1996, as the reform is implemented, and in 2001, well after the implementation, yields correlations of 0.01 and 0.17, respectively (World Bank, 2005). This suggests very little relationship between pre-reform education levels and the investment decisions of the regional governments. It is more likely that the reform actually led to reduction in school quality. In addition to the negative impact of mother tongue education found by Zenebe Gebre (2014), the number of students per teacher increased over 50 percent in the years following implementation of the reform. This likely deterioration of quality, which accompanies many of these large reforms, likely puts a downward pressure on the estimates of schooling, making identification more difficult.

5 Data

The geographic variation used in the construction of the paper's intensity measure is calculated using data from the 1994 Ethiopian Census. These data were collected by the Ethiopian Central Statistical Agency, and made available as part of the Integrated Public Use Microdata Series (IPUMS) International by the Minnesota Population Center (2015). A sample of over 200,000 women born between between 1968 and 1972 is constructed to estimate the pre-reform level of education in each of the zones across Ethiopia. Data from the census for children age 6 to 12, a sample of over 500,000 observations, are also used to calculate zone specific school entry likelihoods for each age. Using birth year and region of residence information the instrument calculated with these data is merged with individual outcome data to estimate the effect of the Ethiopian reform, and the eventual impact of the exogenous increase in schooling on fertility rates.

Individual level outcome data for Ethiopian women are from 2005 and 2011 rounds of the Ethiopian Demographic and Health Survey (DHS). The data used in this paper are from the merged individual women and birth history datasets. The individual women dataset includes detailed information regarding birth date, district of residence, education, health, contraceptive use, employment, household wealth, characteristics of their husbands, and age at first marriage, sexual intercourse and birth. The birth history data includes retrospective information on the woman's age at the time of birth for each of her children. Not only does this data allow for the measurement of how an exogenous increase in schooling can impact the total number of births, but also allows for the identification of the change in number of births specifically at any given age.

The summary statistics of the DHS data used in paper are presented in Table 1. The table provides information for women in pre-reform cohorts, born between 1969 and 1972, and late- and post-reform cohorts, born between 1986 and 1992. The later sample was selected to include one cohort above the age of 25. Although later cohorts are younger, they have a higher level of education and far fewer births. For this reason it can be informative to examine the number of births at each age, ages 15, 17, 20, 22 and 25 are shown in the table for six key variables. These samples only include women older than the stated age, and allow for an apples to apples comparison, for example, for the number of births by age 20. For all six of these measures a consistent pattern emerges, the women in the later cohorts have fewer children at each age, and postpone sexual activity, marriage, and birth to later ages than the earlier cohorts. The portions of these differences that can be explained by the increase in education generated by the reform will be examined in the following section.

6 Results

6.1 Ordinary Least Squares

To begin the analysis the OLS relationship documents the general correlation seen in the data. This is done by estimating equation (3) using the years of schooling data, not the predicted level from first stage. A negative relationship between fertility and schooling has been well documented in the literature (Ainsworth et al., 1996; Lam and Duryea, 1999; Schultz, 1994, 1997), and is also found in the data from Ethiopia. The OLS estimates are shown in Table 2. The estimate in column (1) is for total number of all births for women 15 years of age and above, the model estimates a negative and statistically significant relationship between schooling and fertility.

Additionally, it could be interesting to examine how schooling impacts fertility rates across different ages, and although a cubic in age is included in the model, estimating the relationship between schooling and fertility at specific ages has the added benefit of comparing women at different ages at a fixed point in their life. The estimates in columns (2) through (11) use the total number of births by the stated age as the dependent variable, for a sample of women who were at least one year older at the time of the survey. Across all 10 estimates the OLS model again finds a negative and statistically significant relationship between years of schooling and fertility. However, as discussed previously these estimates are unlikely to describe a causal relationship between schooling and fertility if unobserved characteristics that impact a woman's schooling also affect their fertility decision. To address this concern an exogenous increase in schooling generated by the education reform in Ethiopia is identified, and an instrumental variables technique it used to examine the impact of this increase in education on women's fertility.

6.2 First-Stage Estimates: Effect of Reform on Years of Schooling

Before linking the schooling to fertility, it is important to examine whether exposure to the education reform in Ethiopia did generate an identifiable increase in years of schooling. The impact of the education reform is estimating by regressing years of schooling on the zone and birth year specific intensity measure described in Section 3; the first-stage estimating equation is described by equation (2). The results from five different specifications are shown in Table (3).

All estimates in Table (3) include a cubic control for age, a set of zone fixed effects, and a set of birth year fixed effects. Estimates in column (4) use region- (i.e. province) specific trends, while the estimates in the other four columns use more localized zone-specific time trends. The model in column (1) allows for an incremental implementation of the education reform, as described at the end of Section 3, and the sample includes all women born between 1969 and 1992. This is the preferred intensity measure and sample for the paper. The first-stage estimate described in column (1) finds that the reform led to an increase in schooling just over 1.6 years of school, the F-statistic is 17.15. The largest change in the estimated coefficient on schooling is in column (2) when the reform is forced to be implemented uniformly within each region; however, the model still estimates an increase of nearly 1.25 years of schooling, and and F-statistic of 12.19. Column (3) uses the baseline sample with region-specific trends, columns (4) and (5) estimate the model using alternative sample. The final three estimates are similar to the baseline model. All five estimates find strong evidence that the education reform in Ethiopia led to a significant increase in years of schooling of well over one additional year, with an F-statistic of at least 12. The three models that include allow for incremental implementation and include zone-specific trends – columns (1), (4) and (5) – all find an increase in schooling of over 1.6 years with an F-statistic of over 17.

6.3 2SLS: Effect of Years of Schooling on Fertility

The results from the first-stage demonstrate the strength in the instrument's ability to identify the increase in schooling generated by the removal of school fees in Ethiopia. Estimating the second stage of the 2SLS model will analyze the relationship between birth rates and the predicted level of education, as described in equation (3). The results in Table 4 use the same five variations of the model used to estimate the first-stage, and all five estimates find a statistically significant and negative relationship between schooling a woman's lifetime number of births. The estimates across all five specifications also yield similar point estimates; each additional year of schooling leads to about one-fifth (-0.2) fewer births. This estimate is about 70-percent larger than the corresponding OLS estimate in Table 2.[7]

In addition to looking at the relationship between schooling and total number of births, the number of births at specific ages, from 15 to 24, are again examined in Table 5.[8] At the younger ages, 15 and 16 there is no effect of schooling on births. Not only is the pre-reform average number of births at these ages relatively low, about 0.14, but the average years of schooling in the pre-reform sample is only 2.77, which combined this the small estimates at these younger ages makes it unlikely that any type of incarceration effect of women physically being in the classroom is affecting the results in the paper. Beginning at 17 the point estimate becomes increasingly negative, and statistically significant at the 90 percent level at age 18. Each additional year of schooling reduces the number of births a woman has by the age of 18 by 0.086 births, 27 percent off of the pre-reform average of 0.316. At the age of 20 the magnitude of the point estimate again doubles, becoming increasingly negative, and then takes another jump of about an additional reduction in fertility of another 0.1 births at the age of 22. At the last two ages that can be examined the effect continues its downward trend, becoming increasingly negative while remaining statistically significant. Although women are likely to have more children beyond the age of 24, the negative trend is the first piece of evidence that suggests it is likely effect becomes increasingly negative at later ages. Furthermore, the effect at the age of 24 is already more negative than the results seen in Table 4, when younger women are included in the sample. As seen in Table 5, the increase in schooling does not significantly impact the fertility rate for these younger women, suggesting that the effect of schooling for women older than 24 will have to remain increasingly negative to balance out their zero result. This evidence, as well as later evidence that finds an increased likelihood of contraception use, suggests a persistent effect of the increase in schooling that persists through later ages.

All three of the large jumps in the effect of schooling seen in Table 5 are found to be associated with changes in behavior generated by the increase in schooling. The three ages at which the effect exhibits a large jump are 17, 20 and 22. Figure 2 provides an interesting insight into the changes at 17 and 20. There are three sets of results plotted in the figure; the stripped bars denote the effect on first birth, first sexual intercourse is shown using the white bars, and first marriage using the black bars.The vertical axis represents the change in the likelihood of each event occurring by the relevant age generated by a one year increase in schooling. The timing in this discussion will be based on the assumption that, on average, having sex or marrying and attempting to start a family at any specific age will lead to giving one year later. For each

[7]The magnitude of both the OLS estimates and the 2SLS estimates are similar to those from Osili and Long (2008) in Nigeria, and Fort et al. (2016) in the United Kingdom.

[8]The first-stage F-statistic is not strong enough to take meaningful inference away from the results beyond 24.

additional year of schooling, the model estimates a reduction in the likelihood of having sex by age 16 of 6.7 percentage points. This is the first age at which the decline is found, and at age 17, for the first time, there is a large and statistically significant decline in the likelihood of a woman having her first child of 6.9 percentage points. This corresponds to the initial timing of the fertility effect seen in Table 5. The reduction in sexual activity continues for the next two ages, 17 and 18, before beginning to dissipate as women age into their 20s. However, at 19 the effect of schooling on marriage begins to bite, each additional year of schooling reduces the likelihood that a woman is married by the age of 19 by 4.4 percentage points. The age of 20 was the next age that at which the large reduction in fertility was seen in Table 5, and the first age at which the reduction in sexual activity was complemented by a reduced likelihood in marriage. The postponement of marriage persists slightly longer than the reduction in sexual activity, through the age of 21. These means at age 21 the postponement of marriage effect is the only effect driving the reduction in fertility in Figure 2, and this can be seen affecting fertility through the relatively small and statistically insignificant effect on first birth at the age of 22. This leaves the increasingly negative effect on fertility at age 22 seen in Table 5 unexplained.

Figure 3 presents a similar picture to the previous figure, but with information regarding the relationship between schooling the timing of a women's first birth (dashed bars), second birth (white), and third birth (black). For comparison, the first birth information is same as Figure 2, and the scale on the vertical axis is held constant. This figure shows that through the age of 21 women's postponement of their first birth is the driving factor reducing birth rates. Even at 20 and 21, ages where the effect on first birth is measured with more precision, the matching decline between first and second birth suggests that the delay in the second birth could largely be driven by postponement of having the first child. However, at age 22, the last age at which we saw the large decline in overall birth rate in Table 5, the decline in the second birth becomes the driving factor. At this age the reduced likelihood of the second and third birth are now similar suggesting that the postponement of having the second child could be cascading into a reduced likelihood of having the third by the age of 22. This pattern persists through the remaining age cutoffs, as would be expected from the persistent estimated reduction in number of births at these ages in Table 5. The results outlined in Figure 2 and Figure 3 suggest that the increase in education in Ethiopia generated by the reform reduced fertility rates through women's postponement of sexual activity in their late teens, the postponement of marriage around the age of 20, and the delay in expanding the size of their family in their mid-20s.

6.4 Additional Mechanisms

The results in the previous section find that the increase in education in Ethiopia generated by the reform led to a reduction in fertility, and described the behavior through which this reduction occurred. However, this does not explain why these women decided to change their behavior. There are two broad, but not mutually exclusion, possibilities that could explain why education led to a reduction in fertility rates. The general economic theory for why there could be a negative relationship between schooling and fertility rates is that increases in schooling should increase worker productivity. If this occurs this reform would not merely be generating an increase in schooling, but an actual increase in education, in learning, that is allowing these women to become more productive. This increase in productivity would generate an increase in the cost of the women's time, and of their opportunity cost of raising children. This increase in opportunity cost would manifest itself in a reduced demand for children, and smaller ideal family size. A second channel through which the increase in education could lead to an increased level of empowerment in their personal decision making ability. This could be through an increase in the level of labor market potential, an standing relative to their husband, or by simply generating a better understanding of their opportunities and rights. Women generally want fewer children than their husbands, and an increase in relative power in the decision making process would likely lead to smaller family size.[9] Alternatively, some combination of these two possibilities could occur.

The results in Table 6 begin to examine both of these possibilities. Specifically, results (1) through (6) examine the effect schooling on women's labor market outcomes and their ideal family size. Result (1) finds that each additional year of schooling reduces a woman's ideal number of children by 0.247, a magnitude similar to the estimated reduction in number of births in Table 4.[10] Although income data is not included in the DHS, the next five results show evidence of that an additional year of schooling led to large increases in the likelihood of working, earning cash, not working in subsistence agriculture, not working in agriculture more broadly, and in household wealth. Four of the five estimates are statistically significant, while estimate on currently working is not far from being statistically significant at the 90 percent level. All of these measures yield strong evidence of an increase in productivity for women following the reform, and the expected result that the increase in the opportunity cost of their time is associated with a smaller ideal family size.

The final four results in Table 6 attempt to shed some light on whether the increase in education led to an increase in woman's ability to make household decisions, and potentially impact the number of their

[9]One-in-three pre-reform women report their husband wanting relatively more children, while only nine percent report that they would like to have a larger family than their husband.

[10]Importantly, there was no statistically significant effect on the non-numeric response of "up to god"; only 6.5 percent of pre-reform women gave this response.

number of children through a change in the household bargaining process. Result (7) finds evidence that each additional year of schooling increases the likelihood that a woman reports no justification for a husband beating their wife by 5.4 percentage points.[11] Although this result suggests evidence of an increase in empowerment, there is no change in women's age (8) or education (9) relative to their husband, and there is no change in married women reporting that they have a say in their own health care decisions, large household purchases, the use of their earnings, or ability to visit family or relatives. Finally, result (10) shows the estimated the effect of schooling on the health care decision that would most enable women to better control their fertility decision, their use of modern contraception, and finds no evidence of an effect.

However, further analysis of contraception use yields an interesting, and important pattern. Ashraf et al. (2014) find that when women are able to privately make their decision regarding the use of contraception they use contraception not visible to their husband, and that it creates increased level of stress in their relationship, a potential drawback. This complication is largely due to the short window in which the experiment operates, and the women are required to make their decision. If women have this type of a preference, it is possible that they understand their environment enough to be able to take up contraception in a way that avoids the difficulty found in Ashraf et al. (2014).

The important take away from the following results is the pattern that they demonstrate; as the sample is cut into smaller groups the power in the first-stage is significantly reduced. The interesting piece of initial evidence is outlined in Figure 4. The gray bars show the effect of an additional year of schooling on hidden contraception, and the black bars the effect on any type of modern contraception technology.[12] The full sample result is the same shown for any modern contraception in Table 6, and the results for the full sample and for women with at least one child are estimated zeros. However, when women have their second child the point estimates of the effect of an additional year of schooling increases to 7.5 (hidden) and 9.9 (any modern) percentage points, and women with at least three children see a slightly larger and more precisely estimated effect. The positioning of this increase is not random. It will be shown that women significantly change their behavior regarding contraception use following the birth of their second son, which is obviously only possible after having at least two children. Following the birth of their fourth child, there is a large jump in the likelihood of contraception use; the effect becomes statistically significant, with each additional year of schooling leading to a 25 percentage point increase. Also, important to notice is that the effect on hidden methods consistently tracks with the change in overall use of any method, suggesting that women at generally making this decision on their own.

The estimates in Table 7 are generated using samples defined by whether a woman has at least two

[11] Estimating the model using a sample of married women yields a similar point estimate of a 6 percentage point reduction, with a p-value of 0.136.

[12] Hidden contraception is defined as either IUD or injections. Which are not visible, and do not need to be taken daily.

boys. It is important to note that in Ethiopia, irrespective of a family's current sex mix of children, boys are consistently 52 percent of births. The key take away from Table 7, is that the estimates on the top half of the table, for samples with at least two boys, find a consistently large increase in the use of contraception. The estimates on the bottom of the table, for women with fewer than two boys, find relatively small and more often statistically insignificant estimates. However, the estimates in the bottom half of the table to increase with family size. The results in columns (1) and (2) use the most broadly defined sample, ignoring birth order. When the woman has at least two sons, each year of schooling increases her use of modern (and hidden) contraception by over 20 percentage points. These results find striking evidence that increased schooling does lead to higher rates of contraception use, as family size grows, and following the birth of their second son. Taken in conjunction with the Ashraf et al. (2014) findings, and the matching increases in hidden contraception use found in this paper, the evidence suggests that women are acting very strategically to avoid the negative intra-household difficulties associated with contraception use found in Ashraf et al. (2014). Before increasing their use of hidden contraception women are letting their families become sufficiently large, or having what they seem to believe is an acceptable number of sons. The behavior seems to be targeted to very specific family sizes, and sex mixes, suggesting some type of optimization between desired family size and household tranquility.

7 Conclusion

This paper finds evidence that free primary education led to an increase in schooling in Ethiopia, and that the increase in schooling led to a significant reduction in number of births for Ethiopian women. This reduction was generated through an delay in sexual activity, marriage, and birth. Evidence of a reduced demand for children was also found to be associated with increased labor market returns. Finally, the higher level of schooling also led to an increase in the strategic use of contraception as family size grows, and when the women had at least two sons. Following recent results in the literature that call into question the general understanding of a universally negative relationship between schooling and fertility, these findings become increasingly important. Not only the top line result that fertility declined, but these estimates also yield useful insight into the mechanisms through which schooling impacts fertility.

The identification strategy used in this paper is able to causally identify the returns to increased levels of schooling using only pre-reform schooling and location information. This strategy allows for the identification of national level reforms by exploiting variation in pre-existing levels of education. This can be a powerful tool to examine returns to free primary education in any number of countries, an area of research highlighted by 3ie (Snilstveit et al., 2016) as area in which more study is needed. Furthermore, this paper shows strong evidence of a positive return to schooling, even in the presence of large increases in pupil-teacher ratios. This result suggests that large increases in enrollments are able to outweigh the possible negative impact of declining education quality. This is important because the removal of school fees is a policy lever that has been successfully used in many parts of the world, and consistently generates strong enrollment responses. It will be of great benefit if similar reforms are also shown to generate the same type positive returns to schooling.

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Figures

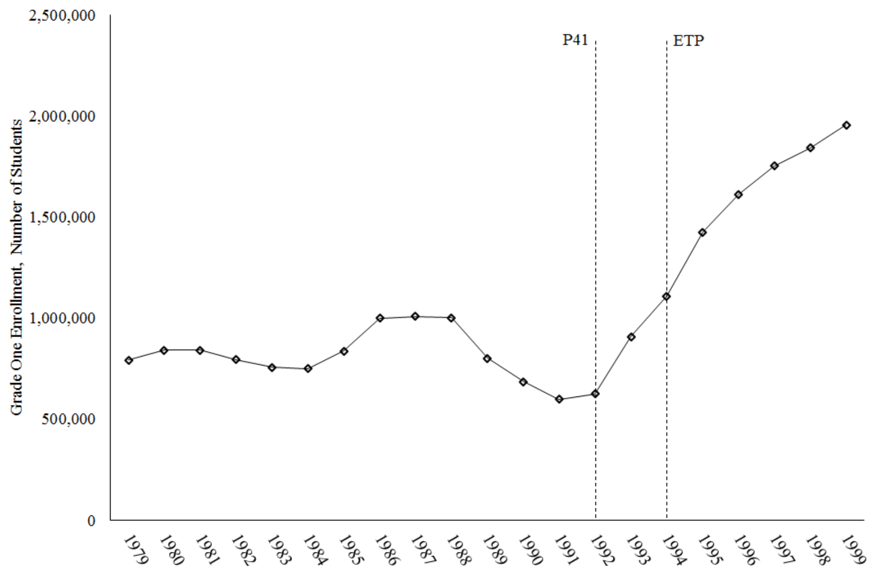

Figure 1: Grade One Enrollment, By Academic Year

Note: P41 refers to Proclamation No. 41, and ETP to the Education and Training Policy.
Source: UNESCO Institute for Statistics.

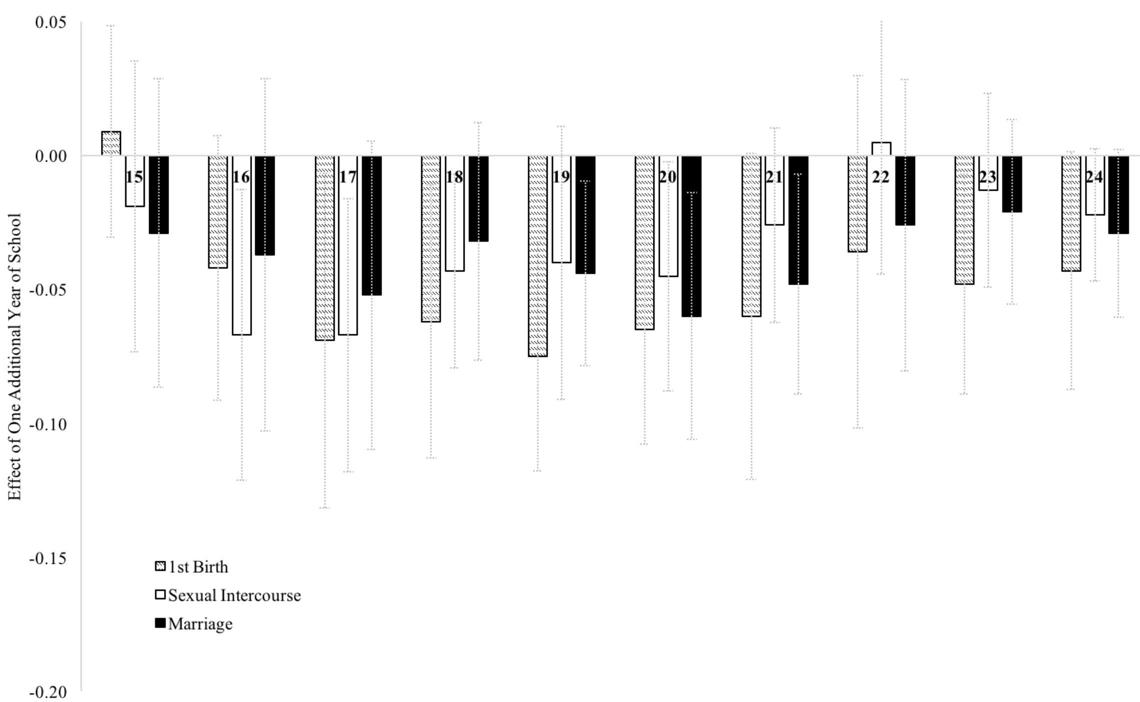

Figure 2: Effect of Years of Schooling on Age at First Birth, Sexual Intercourse, and Marriage (90% C.I)

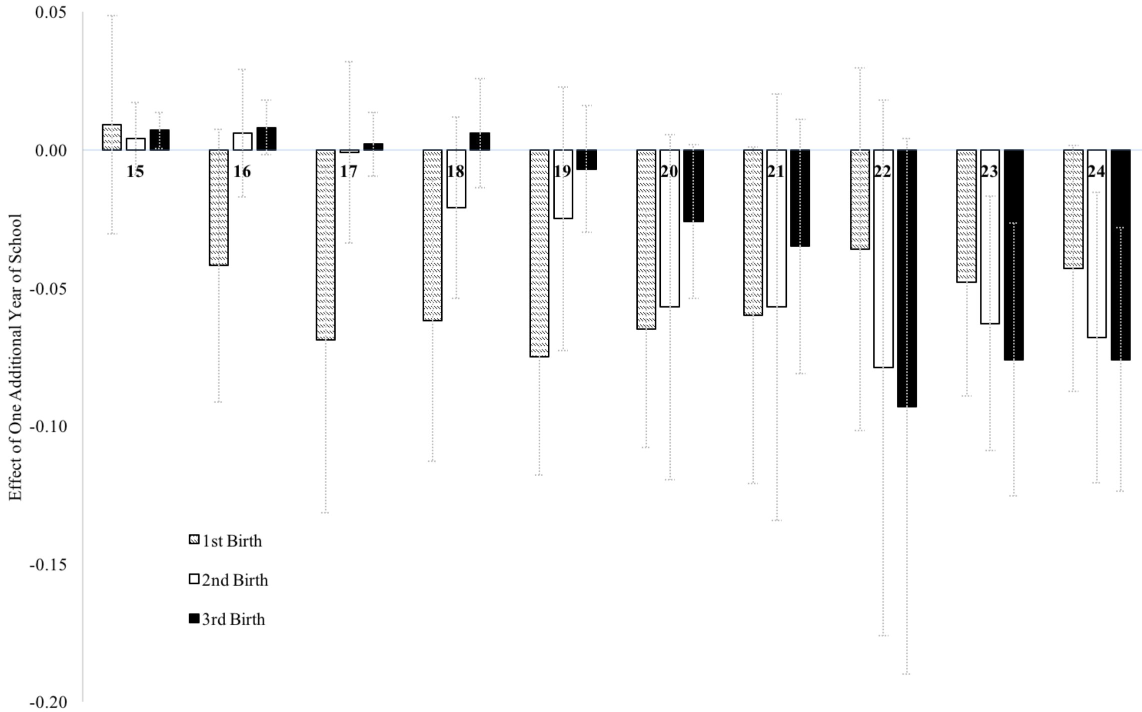

Figure 3: Effect of Years of Schooling on Age at First, Second, and Third Birth (90% C.I)

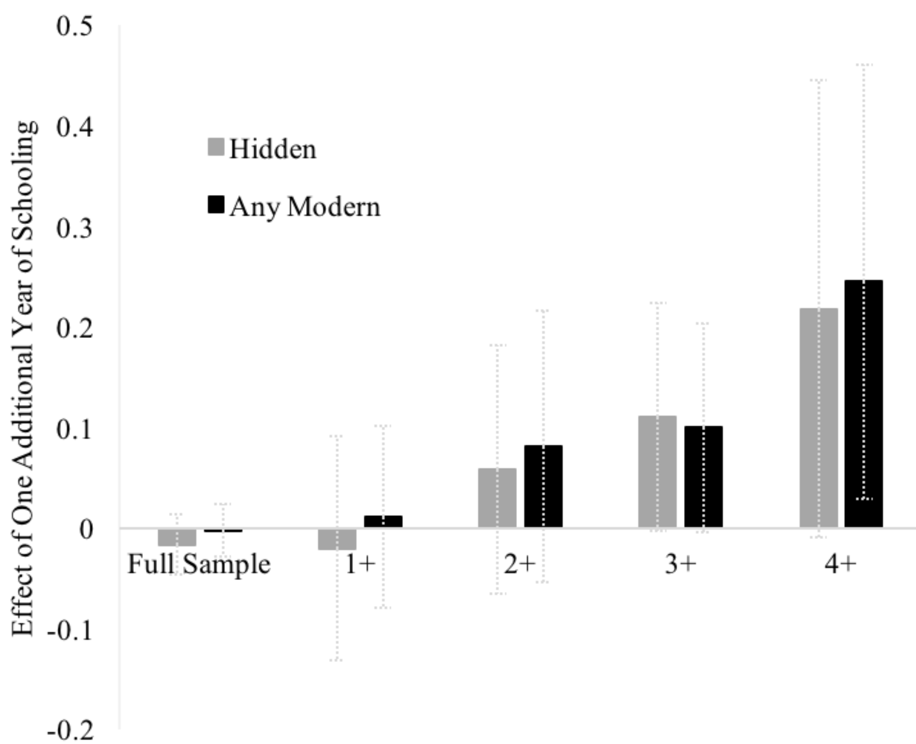

Figure 4: Effect of Years of Schooling on Use of Contraception, by Family Size (90% C.I)

Tables

Table 1: Summary Statistics

Birth Cohorts:	1969 – 1972		1986 – 1992	
	N	*Mean*	*N*	*Mean*
Years of Schooling	2,894	2.774	11,476	4.625
Number of Births	2,894	5.039	11,477	0.459
Ideal Number of Children	2,603	5.630	10,922	3.685
Number of Births				
Age 15	2,894	0.140	10,661	0.042
17	2,894	0.316	9,212	0.132
20	2,894	0.704	4,219	0.394
22	2,894	1.027	2,819	0.651
25	2,894	1.547	217	0.994
First Sexual Intercourse				
Age 15	2,894	0.285	10,661	0.141
17	2,894	0.429	9,212	0.279
20	2,894	0.685	4,219	0.544
22	2,894	0.784	2,819	0.675
25	2,894	0.896	217	0.806
First Married				
Age 15	2,894	0.270	10,661	0.130
17	2,894	0.393	9,212	0.237
20	2,894	0.595	4,219	0.433
22	2,894	0.700	2,819	0.558
25	2,894	0.819	217	0.717
No Justification for Beating Wife	2,810	0.406	10,909	0.411

Birth Cohorts:	1969 – 1972		1986 – 1992	
	N	*Mean*	*N*	*Mean*
First Birth				
Age 15	2,894	0.118	10,661	0.036
17	2,894	0.225	9,212	0.113
20	2,894	0.432	4,219	0.295
22	2,894	0.552	2,819	0.447
25	2,894	0.717	217	0.634
Second Birth				
Age 15	2,894	0.025	10,661	0.004
17	2,894	0.086	9,212	0.022
20	2,894	0.210	4,219	0.106
22	2,894	0.309	2,819	0.194
25	2,894	0.415	217	0.286
Third Birth				
Age 15	2,894	0.002	10,661	0.001
17	2,894	0.016	9,212	0.004
20	2,894	0.096	4,219	0.032
22	2,894	0.171	2,819	0.082
25	2,894	0.310	217	0.174
Working	2,892	0.595	11,464	0.482
Earning Cash	2,894	0.412	11,477	0.335
Non-Subsistance Ag.	2,876	0.303	11,412	0.321
Non-Agriculture	2,876	0.235	11,412	0.233
Difference with Husband:				
Age	1,302	-9.383	2,821	-7.323
Education	1,530	-1.186	3,232	-0.964

Note: Data are for women in the 2005 and 2011 rounds of the Ethiopian DHS.

Table 2: OLS Estimates of the Effect of Schooling on Fertility, by Age

		Number of Births at Age									
	Total Births	15	16	17	18	19	20	21	22	23	24
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$Years\ of\ Schooling_{izy}$	-0.124***	-0.009***	-0.018***	-0.027***	-0.042***	-0.055***	-0.067***	-0.080***	-0.092***	-0.109***	-0.120***
	(0.011)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)	(0.008)	(0.009)
N	15,569	14,993	14,488	14,022	12,961	12,218	10,843	10,256	9,506	8,833	8,217

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is number of births. All samples includes women in birth cohorts from 1969 to 1992, but only observations above the stated age in columns (2) through (11). All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.

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

	Baseline	Uniform Implementation	Regional Trends	Alternative Samples 1971 to 1990	Alternative Samples 1967 to 1994
	(1)	(2)	(3)	(4)	(5)
I_{zy}	1.631***	1.246***	1.605***	1.718***	1.719***
	(0.394)	(0.357)	(0.449)	(0.395)	(0.392)
F-Statistic	17.15	12.19	12.77	18.96	19.24
N	15,569	15,569	15,569	13,712	17,048

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. All samples includes women in birth cohorts from 1969 to 1992. I_{zy} is the estimated intensity of the reform in zone z, for women born in year y. All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.

Table 4: 2SLS - Effect of Years of Schooling on Fertility

	Baseline	Uniform Implementation	Regional Trends	Alternative Samples 1971 to 1990	Alternative Samples 1967 to 1994
	(1)	(2)	(3)	(4)	(5)
$\widehat{Years\,of\,Schooling}_{izy}$	-0.214**	-0.207**	-0.206**	-0.160**	-0.202**
	(0.085)	(0.096)	(0.088)	(0.073)	(0.080)
First Stage F-Statistic	17.15	12.19	12.77	18.96	19.24
N	15,569	15,569	15,569	13,712	17,048

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is number of births. All samples includes women in birth cohorts from 1969 to 1992. $\widehat{Years\,of\,Schooling}_{izy}$ is the predicted level of schooling, instrumented with the reform intensity measure, I_{zy}. All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.

Table 5: 2SLS - Effect of Schooling on Fertility, by Age

	15	16	17	18	19	20	21	22	23	24
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\widehat{Years\,of\,Schooling}_{izy}$	0.011	-0.011	-0.071	-0.086*	-0.084	-0.150*	-0.170	-0.270*	-0.260***	-0.284***
	(0.032)	(0.045)	(0.056)	(0.050)	(0.056)	(0.079)	(0.108)	(0.162)	(0.089)	(0.094)
First Stage F-Statistic	14.55	11.28	10.40	11.43	10.23	7.58	6.43	4.63	11.19	11.41
N	14,993	14,488	14,022	12,961	12,218	10,843	10,256	9,506	8,833	8,217

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is number of births. All samples includes women in birth cohorts from 1969 to 1992, if the observation is above the stated age in each column. $\widehat{Years\,of\,Schooling}_{izy}$ is the predicted level of schooling, instrumented with the reform intensity measure, I_{zy}. All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.

Table 6: 2SLS - Effect of Schooling on Preferences, Labor Market, and Marriage

		Labor Market Details			
	Ideal Number of Children	Currently Working	Currently Earning Cash	Non-Subsistence Agriculture	Non-Agriculture
	(1)	(2)	(3)	(4)	(5)
$\widehat{Years\,of\,Schooling}_{izy}$	-0.247*	0.039	0.048**	0.070**	0.056***
	(0.142)	(0.024)	(0.023)	(0.029)	(0.021)
First Stage F-Statistic	19.32	17.28	17.15	17.58	17.58
N	14,353	15,556	15,569	15,500	15,500
	Household	No Justification	Diff. with Husband (Married Only)		Using Modern
	Wealth (Std.Dev.)	for Beating Wife	Age	Education	Contraception
	(6)	(7)	(8)	(9)	(10)
$\widehat{Years\,of\,Schooling}_{izy}$	0.081**	0.054**	-0.253	0.313	-0.002
	(0.033)	(0.022)	(0.733)	(0.308)	(0.016)
First Stage F-Statistic	17.15	20.31	7.65	8.15	17.15
N	15,569	14,905	9,539	9,471	15,569

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is described above each of the ten results, and is a dummy variable for results (2) through (7) and (10). A negative sign for results (8) and (9) denotes an increase in the wife's value relative to the husband. All samples includes women in birth cohorts from 1969 to 1992, but only married women for results (8) and (9). $\widehat{Years\,of\,Schooling}_{izy}$ is the predicted level of schooling, instrumented with the reform intensity measure, I_{zy}. All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.

Table 7: 2SLS - Effect of Reform and Schooling on Contraception Use

	At Least Two Children				At Least Three Children		At Least Four Children	
	Using Modern Contraception (1)	Using Hidden Contraception (2)	Using Modern Contraception (3)	Using Hidden Contraception (4)	Using Modern Contraception (3)	Using Hidden Contraception (4)	Using Modern Contraception (3)	Using Hidden Contraception (4)
	A. At Least Two Boys		A. Of First Two Children Both Boys		A. Of First Three Children At Least Two Boys		A. Of First Four Children At Least Two Boys	
Reduced Form								
I_{zy}	0.307**	0.266***	0.226	0.215*	0.197*	0.180	0.317*	0.244
	(0.115)	(0.077)	(0.164)	(0.127)	(0.116)	(0.118)	(0.164)	(0.166)
2SLS								
Years of Schooling$_{izy}$	0.235*	0.203	0.362	0.345	0.106*	0.096	0.309	0.238
	(0.136)	(0.127)	(0.654)	(0.655)	(0.059)	(0.063)	(0.214)	(0.210)
	N	4,609	*N*	2,053	*N*	2,901	*N*	2,818
	First Stage F-Statistic	1.308* 3.08	*First Stage F-Statistic*	0.623 0.25	*First Stage F-Statistic*	1.870*** 8.09	*First Stage F-Statistic*	1.025 1.91
	B. Fewer Than Two Boys		B. Of First Two Children Fewer Than Two Boys		B. Of First Three Children Fewer Than Two Boys		B. Of First Four Children Fewer Than Two Boys	
Reduced Form								
I_{zy}	0.061	0.031	-0.013	-0.027	0.083	0.166	0.475*	0.535*
	(0.065)	(0.069)	(0.071)	(0.065)	(0.134)	(0.131)	(0.265)	(0.298)
2SLS								
Years of Schooling$_{izy}$	0.076	0.039	-0.012	-0.025	0.054	0.109	0.127*	0.143
	(0.123)	(0.105)	(0.066)	(0.061)	(0.092)	(0.115)	(0.077)	(0.089)
	N	4,998	*N*	5,419	*N*	2,588	*N*	1,128
	First Stage F-Statistic	0.795 0.75	*First Stage F-Statistic*	1.080 1.54	*First Stage F-Statistic*	1.528 1.27	*First Stage F-Statistic*	3.736*** 8.67

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in odd numbered columns is a dummy equal to 1 if the respondent reports using any type of modern contraception, and in even numbered columns the dummy variable is equal to 1 if the respondent reports using a hidden form of contraception (IUD or injection). All samples includes women in birth cohorts from 1969 to 1992, which the specified family characteristics. $\widehat{Years\,of\,Schooling}_{izy}$ is the predicted level of schooling, instrumented with the reform intensity measure, I_{zy}. All regressions include a cubic in age, birth year and zone fixed effects, and zone-specific linear trends. Each estimate is from a unique regression, weighted using weights provided by the DHS, and standard errors are clustered at the zone level.