Does maternal schooling lead to improvements in child health? Evidence from Ethiopia

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Abstract

This paper examines the role of women's education on child health by analyzing the second generation impact of a nationwide reform that eliminated primary school fees in Ethiopia in 1995. I exploit the timing of the implementation of Universal Primary Education (UPE) policy in Ethiopia, as well as regional differences in implementation, as a natural experiment and a Two Stage Least Square analysis of the effect of schooling on their children's health. Analysis of key health outcomes among children whose mothers were educated at the time of the reforms' implementation shows better long-run health outcomes among the children of women who received more schooling. The children of women with more schooling are 0.08 percentage points less likely to be chronically malnourished have increased 0.163 deviations in height for age Z-scores.

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1 Introduction

Increasing women's schooling has been a central goal for many developing countries and governments.[1] As a result, the average number of years of education for Sub-Saharan girls increase from 3.3 in 1970 to 8.8 in 2015 (Montoya, 2019). From a theoretical perspective, the neoclassical model predictions of a quantity-quality trade-off in women's fertility decisions (Becker & Lewis, 1973). The intuition behind this idea is that more educated will lower their fertility and allocate more resources on the children (Currie & Moretti, 2003). However, while the literature in developing countries has mostly examined the effects of education increases on fertility decisions (Osili and Long, 2008 and Chicoine, 2021), fewer studies have considered the effects on the health outcomes of second generation children in developing countries.[2]

This paper studies the effect of female education on children's health outcomes by examining the results of a nationwide UPE reform in Ethiopia that eliminated primary school fees. The policy, implemented in 1995, provides a natural experimental setting in which to analyze the effects of female education by comparing child health outcomes across cohorts and zones who had varying pre-reform enrollment levels of girls. Using data from Demographic and Health Surveys (DHS), I show the reform led to an increase in school enrollment and an increase in child well-being. From a broader perspective, the elimination of the school fees was a major policy intervention with effects that transcended the targeted generation.

To establish the causal relationship between maternal education and child health outcomes, I use a two-stage least-squares approach. The increase in schooling is identified by exploiting two sources of variation: the cohort of birth and geographic location of women (Chicoine, 2021). First, although the policy eliminated school fees for children in the first 10 grades (first through tenth grades) all at once, children who had already begun primary school, received fewer years of tuition free schooling. Therefore, most benefits accrued to younger children who were eligible for more years of free schooling. Second, though the elimination of school fees was uniform across the country, its impact depended on the pre-existing conditions. Greater effects were observed in areas where pre-reform schooling levels were low, compared to those areas where most of the eligible population was already attending school.

Strategies based on pre-intervention and cohort differences have been utilized by a number of papers to study the impact of policies that were implemented at a national level. Lucas (2010, 2013) and Bleakley (2010) examine the effects of nationwide

[1] Free universal primary school policies were implemented in Kenya and Nigeria in the mid-1970s; in Zimbabwe and Tanzania in the 1980s; and in Ethiopia, Malawi, and Uganda in the 1990s.

[2] Keats (2018), for example, finds a positive relationship between maternal health and chronic child outcomes in Uganda. More generally,Breierova and Duflo (2004), Chou, Liu, Grossman, and Joyce (2010), and Currie and Moretti (2003) examine find a positive association between parental education and child health in Indonesia, Taiwan and the United States, respectively. Other studies, such as those of McCrary and Royer (2011), do not find any significant effects in the United States.

malaria eradication campaigns in several countries (US, Colombia, Bolivia, Sri Lanka and Bolivia) on a variety of outcomes.[3] Overall, the literature has found a decrease in fertility and an increase in education and health. Osili and Long (2008) used a difference in difference strategy based on cohorts and regions to estimate the effects of a UPE policy in Nigeria. Lucas and Mbiti (2012) analyze a free primary schooling reform in Kenya using a regression discontinuity design and find that it does not find increased learning. Chicoine (2019) consider the impact of the Ethiopian UPE reform and the inclusion of mother tongue instruction. The authors find significant effects on literacy, knowledge of family planning and HIV, and the likelihood of knowing a location for HIV testing. Finally, in this study, I follow the approach created by Chicoine (2021) where the author exploits cohort and geographic differences to identify the impact of the Ethiopian reform on fertility and labor force participation.

To confirm the results from the instrumental variable approach, I utilize a second strategy that relies on the Ethiopian reform's increase in schooling across birth cohorts. Specifically, there is a discontinuous jump in the completed education for the cohort of women born in 1986 or later. The implementation of this strategy follows Behrman (2015) and Keats (2018). Women who were younger than ten years old younger in 1995 were disproportionately more likely to benefit from the reform, while older women were not.[4] Hence, because the year of birth is exogenous for women, and since women who were affected are similar to those who did not benefit from it are identical, in this strategy, I use the schooling fee elimination reform as an exogenous variation in attainment across age cohorts.

The relationship between women's education and improved early child health outcomes could arise in several ways. First, health care utilization is positively correlated with maternal education in developing countries (Mekonnen and Mekonnen, 2003; Glei and Rodriguez, 2003; Tann et al., 2007; and Jewell, 2009).[5] A second potential pathway is via an income effect that changes women's roles in the household, via increases in female wages or marriages to wealthier men (Behrman and Rosenzweig, 2002; Currie and Moretti, 2003; Black, Devereux, and Salvanes, 2005; Chicoine, 2021). Alternatively, the rise in literacy and numeracy provided by the additional years of primary schooling may be directly responsible for the acquisition of the skills necessary to implement better family planning and health care, suggesting improved children's outcomes are the result of having more knowledgeable parents (Glewwe, 1999; Grossman, 2006).

The outcomes I study relate to the short and long run health status of the children of the women affected by the reform. Acute conditions, such as reports on the prevalence

[3]The list includes school attainment, child health, and fertility outcomes among others.

[4]These women could have dropped out or continued to higher education.

[5]A vast number of studies have considered the relationship between female education and health care utilization in developing countries. For example, in Ethiopia, Mekonnen and Mekonnen (2003) look at the factors that influence the use of pre- and post-natal care and find that more educated women are more likely to use them. Also, Tann et al. (2007) in Uganda; Glei and Rodriguez (2003) in rural Guatemala; Jewell (2009) in South America.

of fever, diarrhea, and coughing provide evidence of the overall well-being of the child that reflects daily health care provisions. These conditions approximate the current health status of the child and may have an impact on the child's daily routine. In contrast, the anthropometric measures (height-for-age, weight-for-age z-scores, and body mass index) relate to the long-run health status. These conditions are chronic and have the potential to impact the child's adult life, not only directly through their effect on health but also through their education attainment (Alderman, Hoddinott, and Kinsey, 2006) and the children's labor market outcomes (Hoddinott, Maluccio, Behrman, Flores, and Martorell, 2008; Carba, Tan, and Adair, 2009). I also consider the likelihood of infant mortality.

The selected outcomes should be studied with-in the literature of the impact of female education on fertility. A growing experimental and quasi-experimental number of papers have found that education decreases the number of children in several developing countries, including Nigeria (Osili & Long, 2008), Malawi (Baird, Chirwa, McIntosh, & Özler, 2010), Kenya (Ferré, 2009; Ozier, 2015 and Duflo, Dupas, and Kremer, 2015), Uganda (Keats, 2018), Turkey (Kırdar, Dayıoğlu, & Koç, 2018) and Indonesia (Breierova & Duflo, 2004).[6] Likewise, the two papers that study the Ethiopian reform, Behrman (2015) and Chicoine (2021), find that women have a smaller number of children. The evidence, therefore, hints to the potential existence of selection bias in the sample of women. Since the sampled women were between 15 and 50 years of age, later surveys are more likely to have interviewed women who became mothers at younger ages. The fact leads the estimates to be biased towards zero.

This paper's findings suggest that maternal education decreases the likelihood of being stunted by eight percentage points. Children of more educated mothers report an increase of 0.163 standard deviations in their height Z-score. Conversely, I do not find a statistically significant link between reports on cough and the prevalence of diarrhea. However, the instrumental variable approach renders a positive effect of the reform on fever. Third, maternal education has an impact on infant mortality. Overall, the results are consistent with the notion that women have fewer children, but the ones women do have are more likely to survive and have better health in the long run.

This paper contributes to the literature on the effects of maternal education on child health in two ways. First, I add to the small number of studies have considered the link between maternal education and child health in developing countries (Glewwe, 1999; Breierova and Duflo, 2004; Chou et al., 2010; and Güneş, 2015). Second, by studying anthropometric measures of the children, I provide evidence that female education has impacts perceived in the long term.

This paper is organized as follows: section 2 provides the background of Ethiopia's reforms. Section 3 outlines the empirical methodology, and describes the instrument. Section 4 details a and the summary statistics. Section 5 presents the results. Section 6

[6] There is also evidence in more developed countries like the US and Norway (Black, Devereux, and Salvanes, 2008 and Monstad, Propper, and Salvanes, 2008).

presents the robustness checks, including the regression discontinuity design. Section 7 discusses the potential mechanisms and in Section 8, I discuss the findings. I conclude the investigation in Section 9.

2 Background: the Ethiopian schooling system

Between 1950 and 2000, budgetary problems and great inequalities of access characterized the Ethiopian schooling system. The central Government was the primary entity responsible for the financing of the public education, but it relied heavily on community contributions and school fees. Between 20 percent and 30 percent of the financing was covered by school fees (Birger & Craissati, 2009).[7] As a result, by the beginning of the 1970 school year, there were large disparities in opportunities, which was reflected by the fact that 76 percent of the population between the ages of 7 and 18 years of age were outside the schooling system. Participation rates differed greatly between regions. For example, Addis Ababa's enrollment rates reached 67 percent while in Afar it was only around four percent (Birger & Craissati, 2009).

Ethiopia faced a military coup in 1975, which was followed by a 17-year-long rule by a communist government. Authorities reshaped the schooling system and the lack of resources problem was deepened. Though no official policy was enacted regarding school fees, each school was allowed to charge students supplemental fees to cover expenses.[8] These fees created a significant burden for households.[9]

The communist government rule ended in 1991, when a transitional government was established by the Ethiopian Democratic People's Revolutionary Democratic Front. To tackle disparities in the education system, the government established its Education and Training Policy (ETP) prior to the 1995 school year. The policy eliminated student fees for the first 10 grades.[10] However, implementation of the elimination of fees did not take place all at once. Because the details of the policy had been previously known, five regions had previously decided to eliminate the fees when the authority over public primary schools was delegated to their local education bureaus in 1993. The policy that changes authorities is known as Proclamation 41.

The elimination of school fees led to exponential growth of school enrollment in all grades and across the country. Figure 1 shows the decrease in enrollment that took place before the end of the conflict in 1991, and the subsequent increase in the wake of the implementation of policies that eliminated school tuition fees in 1995. Following

[7] Until the 1950s, education was free of charge to every household at all facilities in Ethiopia. Learning materials, such as books and exercise books, were also free of charge. In the 1960s enrollment increased significantly, with no greater budgetary allocation; hence, resources per student became more limited.

[8] Fees usually did not include any text books, which had to be rented elsewhere.

[9] Primary schools include the first six years of schooling, secondary schools include the next four years of schooling. Children are encouraged to start first grade at age seven, but they could start anytime between the ages of six and 12. Public primary schools do not offer any kindergarten classes.

[10] The policy encouraged community support for schools, but it prohibited per pupil fees.

the roll-out of the policy eliminating tuition fees, primary school enrollment soared, with a staggering 336,929 new primary school entrants, representing a 28.62 per cent increase (Ministry of Education, 2000). It is also important to note that the intake of pupils was greater in the first four primary grades, as school enrollment increased from 54 percent to 83 percent across all grades in less than a year (Birger & Craissati, 2009). In terms of individual gains in schooling, Chicoine (2021) found that these reforms generally increased schooling by more than one full year in total.[11]

3 Identification strategy

To identify the causal impact of maternal education on child health, I use a Two Stage Least Square model where female schooling is instrumented using the measure of the girls' exposure to the reform created by Chicoine (2021). As shown in Figure 2, to determine how much the sampled women were actually exposed to the nation wide reform, I exploit two sources of variation: cohort and geographical variations at zone levels.

First, the cohort variation is rooted in the fact that even though ten years of schooling were made available for everyone, older students had already completed a number of them. In other words, younger girls received more years of education than older cohorts. For example, a girl enrolled in grade one could gain up to nine years of free years of education. However, a girl enrolled in grade seven could gain up to three years of free years of education.

Second, although the policy eliminated school fees uniformly across all the regions in the country; the magnitude of the reform's impact will differ according to each zone's pre-reform levels of education. The concept is that in zones where enrollment levels had been high prior to the reforms, eliminating school fees did not have the same level of impact as in zones where the probability of attending primary school was low. For example: if a girl lived in Addis Ababa, the average number of years she would have complete before the reform was six but in a poorer region (like Oromia) she would have completed three years of education.

The strategy requires the assumption that the entry probability remains the same after the reform. That is, the relative age distribution within each region is constant over time. Specifically, even though all ages are more likely to enter school in the post-reform period, if a seven-year-old is twice as likely to enter school relative to a

[11] These types of universalization policies were common in Sub-Saharan Africa and Asia as part of the United Nations Millennium Development Goals. Therefore, it is worth considering the benefits and the problems associated with such an abrupt change in the educational system. Pupil-teacher ratios and pupil-section ratios (PTR and PSR), used as proxies for the relationship between supply and demand of education, show that in the aftermath of the Education and Training Policy, the PTR increased from 33:1 in 1994/95 to 66:1 in 2004/05, and the PSR grew from 48:1 to 69:1 in the same period (Birger & Craissati, 2009). School infrastructure also grew, but it struggled to accommodate the new enrollment. The government targeted building 2,426 new schools, but only 1,387 were built between 1994, the year of the reform and 2000.

six-year-old in the census data, then a seven-year-old remains twice as likely to enter school in both the pre- and the post-reform states of the world. This assumption is also made across several papers in the literature, like Chicoine (2021) and Lucas and Mbiti (2012).[12]

3.1 Instrumental variable

Following Chicoine (2019), I first create the maximum potential magnitude of the reform for each region-zone combination ($Magnitude_{zy}$). To avoid endogeneity, the magnitude is calculated using census data from women born between 1965 and 1969, a significant time before the reform. The maximum potential magnitude for each zone ($Magnitude_z^{Max}$) is then calculated as the product of the number of potential additional years of schooling and the fraction of the population that dropped out after grade g, F_{zg}.[13] The equation can be summarized as follows:

$$Magnitude_z^{Max} = \sum_{g=0}^{9}(10-g)F_{zg} \tag{1}$$

In this equation, I identify for each zone z the number of free years of schooling that were made available by the reform beyond what was being completed before the implementation. Therefore, $Magnitude_z^{Max}$ is defined for the cohorts born after 1987, who faced the one time decision to begin schooling after the reform. The cohorts born prior to 1987 benefited from less years of free primary schooling. For example, the 1986 cohort received one less year of free schooling and the 1985 cohort two less years. Finally, individuals born in 1977 or earlier completed all ten grades before 1995 so they could gain zero years.[14] For the cohorts, the magnitude is equal to:

$$Magnitude_{zy} = \begin{cases} \sum_{g=0}^{9}(10-g)F_{zg} = Magnitude_z^{Max} & \text{for } y \geq 1987 \\ \sum_{g=0}^{9}(10-g)F_{zg} & \text{for } 1977 \leq y \leq 1986 \\ 0 & \text{for } y \leq 1976 \end{cases} \tag{2}$$

Second, the main advantages of the instrument created by Chicoine (2021) is that it does not explicitly specify the age at which the person entered the first grade. Although the official first-grade entrance age is seven, late entry is common in Ethiopia and it is allowed until the age of 12. Since I have assumed that the relative age distribution remains constant over time in each zone, the magnitude can be modified to account for

[12] In Chicoine (2021), the author presents a thorough justification for this assumption using census data.

[13] For example, F_{z0} is the group of women who never attended school in zone z, and F_{z1} is the fraction of women who left after the first grade.

[14] Following, Chicoine (2019), entrance is assumed to be a one time occurrence, and progression continuous until drop-out.

the possibilities of starting school between the ages of six (a year early) and 12 (five years late). The starting school probabilities, $S_{z,a}$, are assumed to sum to 1:

$$\sum_{a=6}^{12} S_{z,a} = 1 \tag{3}$$

Next, the specific magnitudes are combined with the starting age probabilities to construct a reform intensity for each zone (z) and cohort (y) combination. The geographic variation is captured in the magnitude measure described in equation 1, and the temporal variation is defined by the year of birth. In the particular case of the 1986 cohort, for example, a portion of girls ($S_{z,7}$) began their schooling on time in 1993. Thus, the assigned magnitude is equal to $S_{z,7} \cdot Magnitude_{z,1986} = \sum_{g=2}^{9}(10-g)F_{z,g}$. Another group will likely begin school at the age of six ($S_{z,6}$). For those, the magnitude is equal to $S_{z,6} \cdot Magnitude_{z,1985} = \sum_{g=3}^{9}(10-g)F_{z,g}$. In summary:

$$\sum_{p=6}^{7} S_{z,a} Magnitude_{z,1986+a-7} \tag{4}$$

Then, because women entering school at the age of eight in the 1986 cohort get all 10 years of available free primary education, I multiply the entry probability times the maximum potential magnitude (equation 2). I also proceed in the same manner as Chicoine (2019) and account for the marginal entrant. Those in this group would have entered school between the ages of 6 and 8 if it was for free but did not because it was not free. At each age a, the group is identified by $(S_{z,a} - S_{z,a,pre})$ where $S_{z,a,pre}$ is a set of starting ages scaled to equal the fraction of students who entered school in the pre-reform environment, $\sum_{a=6}^{12} S_{z,a,pre} = (1 - F_{z0})$. The magnitude for post-reform entrants and the probable entrants in the 1986 cohort is equal to:

$$Magnitude_z^{Max} \sum_{p=8}^{12} S_{z,a} + [10F_{z,0}] \frac{1}{e^{8-7}} \sum_{a=6}^{7} (S_{za} - S_{za,pre}) \tag{5}$$

In this equation, I assume that individuals make the decision to enter one time. Also, following Chicoine (2021), I account for the fact that the marginal children are likely to face a greater opportunity cost. They, could for example, have more more responsibilities at home with the term $\frac{1}{e^{a-7}}$ where a is the age considered (in the 1986 cohort example is equal to eight). Note that this number is greater for older cohorts, which reflects the fact that it is harder to begin school at older ages.

Combining the magnitudes for the entry at ages six and seven with Equation 5:

$$\begin{aligned} Intensity_{z,1986} = & \sum_{p=6}^{7} S_{z,a} Magnitude_{z,1987-a+7} + Magnitude_z^{Max} \cdot \sum_{p=8}^{12} S_{z,a} \\ & + [(10)F_{z,0}] \frac{1}{e^{8-7}} \sum_{a=6}^{7} (S_{za} - S_{za,pre}) \end{aligned} \tag{6}$$

All the equations for the cohorts between 1972 and 1987 are presented in Appendix Section A. Finally, the intensity variable when all children are considering entering school after the reform (the 1988 cohort onward) is defined by:

$$Intensity_{z,1988} = \sum_{p=6}^{12} S_{z,a} Magnitude_z = Magnitude_z \sum_{p=6}^{12} S_{z,a} = Magnitude_z^{Max} \tag{7}$$

The last important fact that needs to be considered to calculate the intensity of the reform relates to the timing of implementation across each zone of the country. Several zone were early adopters, which lead them to eliminate fees in 1993; the rest of the regions adopted the reforms in 1995. Therefore, the intensity measure is shifted two years to account for correct reform probabilities. Consequently, cohorts affected partially by the policy are born between 1977 and 1987 for fast-implementing zones, and between 1979 and 1989 for later-implementing zones.

Figure 3 presents the evolution of the measure of intensity for the zones with most and least potential impact, according to their pre-trend education levels. The measure of the impact of the reform is very close zero through the decade of the 1970s due to the low probability and the small number of years women are likely to have been affected by the reform. As the following decade progresses, the intensity expands until it reaches the maximum of 10 in the areas in which schooling enrollment was low. On the other hand, higher-educated regions are less likely to be affected by the policy. Across zones and within cohorts, there is geographical and temporal variation. Therefore, there are no discontinuous jumps in the measure of the intensity of the reforms, but rather, a steady increase in the impact of the reforms on women's educational attainments.

3.2 Estimation Strategy

The central econometric model is a two stage least squares model. The first stage in this equation is represented by:

$$YrsSchl_{mzy} = \theta_0 + \theta_1 Intensity_{zy} + \sum_{p=1}^{3} \theta_2^p Age_{mzy}^p + \tau_y + \sigma_z + \nu_y Trend_r y + \theta_3 x_{mzy} + \epsilon_{mzy} \tag{8}$$

Where $YrsSchl_{mzy}$ is the completed years of schooling for women m, from zone z, and born in year y. $Intensity_{zy}$ is the zone and cohort specific instrument that captures the exposure to the reform (described in Section 3.1). Year of birth and geographical fixed effects (τ_y and σ_z) are added to capture any cohort and region characteristics.[15] $Trend_y$ is a set of region-specific squared linear trends that captures secular changes over time within each zone of Ethiopia.[16] Finally, x_{mzy} is a vector of household-specific variables that account for the mother's religion, birth order indicator for the child variables, and whether the household is rural.

Next, to estimate the second stage effects, I use the predicted levels of schooling of the women estimated in Equation 8, $\widehat{YrsSchl_{mzy}}$, in the right hand side of the following equation:

$$Health_{mzy} = \alpha_0 + \beta_1 \widehat{YrsSchl_{mzy}} + \sum_{p=1}^{3} \beta_2^p Age_{mzy}^p + \tau_y + \sigma_z + \nu_y Trend_r y + \beta_3 x_{mzy} + \xi_{mzy} \tag{9}$$

Where $Health_{mzry}$ is the health outcome of a child whose mother is denoted by m (and was born in year y) and the remaining variables follow the same definition as in the first stage. Standard errors are clustered at the zone level.

3.3 Threats to validity

The key identifying assumption is that the removal of the school fees affected child's health only through its effects on their mother's schooling. Thus, changes in public health policy, migration and other contemporaneous phenomena should not be correlated in the same way as the UPE reform with year of birth and the levels of schooling pre-reform. A woman's year of birth and the place where she attended school must be orthogonal to her exposure to the reform. I explore these possibilities further in section 6.

An additional threat to the identification would arise if geographic distribution of teaching quality, or the number of schools changed relatively after the reform. It is possible that the ETP or Proclamation 41 caused poorer local governments to allocate more resources to education than took place in other districts with better pre-reform enrollment levels. However, Birger and Craissati (2009) showed that although the enrollment disparities between regions diminished after both policies were enacted, investment across local governments did not reverse the schooling expenditure distribution and teacher quality. Evidence of this is provided by the fact that out of the targeted number of new schools, only around half were constructed.

[15] Women's year of birth fixed effects are added in the same way as Güneş (2015), who considers the impact on child health outcomes in the wake of reform regarding compulsory years of schooling reform in Turkey.

[16] Time trends are also used by Black et al. (2005), Bleakley (2010), Lucas and Mbiti (2012), and Lundborg, Nilsson, and Rooth (2014) among many others.

Second, the measure of intensity for women in each cohort and zone is assigned using current location of the individual, not necessarily the place of birth. Therefore, if women chose to move to a different location after finishing her schooling, then the instrument would not precisely capture the effects of the school fee elimination.[17] Luckily, migration is unlikely to be problematic in the current setting. Women in this study report that they had resided at their current location for the most part.[18]

Third, the strategy relies on the assumption that pre-reform trends in the years of education are similar across regions with different levels. Thus, a challenge would arise if areas with higher levels of reform intensity, had higher increases in enrollment prior to the implementation of the reform. To test the assumption, I present the pre-trends of women's education in Section 6. No significant differences were found between groups.

Fourth, health access benefited greatly in the mid 1990's from an important budgetary increase. Therefore, the geographic distribution of the access to health services changes across zones. In areas where accessibility to health posts, stations, and centers was greater, women are likely to have had more access to child care. Thus, if women in some regions had more access to health care, then improvements in child health would likely be affected by factors other than maternal education. More specifically, these estimates would still capture the effect of the reform, but not explicitly the effect of the removal of school fees, and the resulting increase in women's education. To deal with this possibility I conduct a robustness check in Section 6.

4 Data

4.1 Data Sources

In this paper, I use two sources of data to analyze pre-and post-reform women's characteristics and their children's outcomes. The first source of post-reform information on women's characteristics and their children's health outcomes are the 2000, 2005, 2011, and 2016 rounds of the Ethiopian Demographic and Health Survey (DHS). Each of these datasets contains detailed demographic information about a representative sample of women between the ages of 15 and 49 and about their children. The surveys provide the required information on the women's year of birth, zone and district of residence, education level, health, employment, and household wealth. Additionally, DHS databases detail acute and chronic child health outcomes for children born in the five years preceding the survey.

I combine four rounds of the DHS surveys to create a repeated cross section con-

[17] Alternatively, migration might have changed the distribution of the students across zones. In this case, the instrument would capture the re-configuration of the schooling system. For example, if higher-ability students were sorted into areas with higher predicted intensities of the reform, the estimates would be biased.

[18] More than 80 percent of the surveyed women reported living in the same district where they were born and less than 7 percent report having moved five years prior to the interview.

taining mother's and child's household characteristics, year of birth of both, mothers and their children, and the health outcomes of the children. This enables me to consider households containing women born between 1970 and 2000, and their respective children. The final sample is comprised of 18.557 women and 25.440 children.

Second, the instrument relies on data from the Ethiopian Central Statistical Agency in 1994.[19] As I describe in section 3.1, the instrument accounts for the set of pre-intervention enrollment probabilities.

4.2 Child health outcomes

In this paper, I analyze child health outcomes measures to describe the short- and long-term chances and infant mortality (Keats, 2018; Chou et al., 2010). Each group of outcomes has differing consequences for the child's well-being and productivity. First, short-term well-being, measured by acute conditions, approximates daily health care provisions and the household's daily routine. The set has also been directly linked to income disparities (Victora, Vaughan, Barros, Silva, & Tomasi, 2000). The collection includes reports of diarrhea, coughing, and fever in the two weeks prior to the survey. Importantly, acute health outcomes have been linked to early child mortality (Keats, 2018).

In contrast, more chronic outcomes describe with malnutrition and anthropometric measures (stunting, body mass index, and height-for-age, weight-for-age z-scores and wasting). The conditions have been linked to long-term consequences for welfare and educational attainment. Malnourished children begin school later and perform relatively poorer on tests of cognitive achievement (Glewwe, Jacoby, and King, 2001; Alderman et al., 2006 and Handa, Peterman, Seidenfeld, and Tembo, 2016).[20] Additionally, poor anthropometric measurement in early life have also been linked to worse labor market outcomes (Hoddinott et al., 2008 and Carba et al., 2009).[21] Finally, I consider the likelihood of a child dying during the first year.

[19] These data were later made available in the Integrated Public Use Microdata Series (IPUMS) International by the Minnesota Population Center.

[20] Alderman et al. (2006), for example, finds that improvements in preschool nutritional status, as measured by height for a given age, are associated with beginning school at an earlier age, a greater number of grades of schooling completed, and increased height as a young adult in Zimbabwe. Similarly, Handa et al. (2016) test whether the effects of malnutrition on children persist over time, or, alternatively, children undergo a "catching-up" experience. The authors used data from Nicaragua, South Africa and China and found that catch-up highly dependent on household behaviors.

[21] Hoddinott et al. (2008) find that a reduction in the incidence of malnutrition in early childhood (between zero and seven years of age) caused an increase in men's wages. Likewise, an increase in the height-for-age Z-score at the age of two decreases the likelihood of being employed in the informal sector substantially for Philippine women (Carba et al., 2009).

4.3 Descriptive statistics

Table 1 contains the summary statistics of the women sampled in the DHS database. The first two columns in this table provide information about the women in the pre-reform cohorts, who were born between 1971 and 1973. The following set of columns display characteristics of those women who were partially impacted by the reforms – that is, those women who were born between 1974 and 1986. The last two columns present the data of those women born after 1987 – the group of women who obtained the full extent of the policy's benefit. The Table is also divided into a top panel containing mothers' characteristics and a bottom panel with children's health data.

Women in the youngest cohorts have higher levels of education and fewer children. Across the three groups of women, more than 90 per cent report being married, and most of them live in rural areas. Additionally, three out of the four rounds of data (2000, 2005, and 2016) contain information regarding the number of years spent in the current residence. Most of these women report that they lived in the same residence for more than 18 years, which means that they very likely attended primary school at the same location. In the sample, women fully exposed to the reform have children at a younger age.

The lower panel describes the children of the mothers in each of these groupings. Because the DHS surveys contains health outcomes of the children born five years prior to the survey, children are reported to have the same age on average. Interestingly, the sampled children are mostly male. The children that belong to the younger mother's group have smaller stunting, diarrhea and wasting rates. They also display slightly smaller height-for-age z-score.

5 Results

5.1 First-stage coefficients

The first-stage coefficients are presented in Table 2. The coefficient of interest is estimated by regressing the completed number of years of education of the mother on the zone and year of birth measure of intensity. The first-stage estimate shows that each free year of schooling made available by the program led to an additional 0.088 years of schooling. Moreover, the F-statistic for the instrument is 18.68. In column 2, I add the health facilities variables. The coefficient is equal to 0.085 additional years of women's schooling (the F-statistic is 16.31).

To further show the effects of the policy, we estimate the fitted values of the first stage linear regression by year of birth of the mother. Figure 4 graphically demonstrates these fitted coefficients for the first-stage estimates. As expected, the slope of the trend is positive as the years of education grow with the level of exposure to the reform. The slope increases for those mothers fully exposed to the reform.

5.2 Effect of women's schooling on child health

The second stage coefficients estimation are presented in Table 3. Each row displays a coefficient for a separate regression in which I present the coefficients for chronic conditions (top panel) and acute diseases (bottom panel).[22]

As shown in Table 3, the children of more educated mothers are 0.077 points (0.036 SE, p<0.5) less likely to be stunted.[23] The mother's education is also statistically linked to the increases in the height z-score, since the estimates for the years of education have a significant effect equal to 0.155 (0.093 SE , p<0.90) points. There are no statistically significant effects of mothers' education on the BMI z-score and weight z-score.

Next, I examine the regression coefficients of the four following acute conditions: diarrhea, fever, cough, and a measure of child wasting.The coefficient for diarrhea, fever, cough, and wasting are 0.023, 0.043, 0.048 and -0.012, respectively. Neither coefficient is statistically significant.

Taken together, the results in this section provide evidence that maternal education is linked to their offspring's long-term health outcomes while it has fewer effects on acute health outcomes.

In the appendix section, I present a set of additional estimation tables. First, Table C.2 presents the OLS relationship between women's education and child health. Since unobserved characteristics that impact a woman's schooling may also affect the next generation's health, these estimates are not likely to describe a causal relationship. However, I have included the estimation in the Appendix to consider the direction of the bias and underscore the necessity of the instrumental variable approach to deal with the endogeneity issue.[24] Next, Table C.3 includes the reduced form estimation coefficients where the main independent variable is the measure of the intensity of the reform. These results provide insight into the direct effects of the UPE reform on child health outcomes. Following the instrumental variable approach, the reduced form estimates a negative and statistically significant relationship between the reform and the likelihood of being stunted.

6 Robustness checks

6.1 Pre-Trends

As I mentioned in Section 3.3, the identification strategy used in this paper relies on the assumption that pre-reform trends are similar across different levels of reform in-

[22]Table C.2 documents the general Ordinary Least Squares (OLS) estimates for the relationship between years of schooling and the outcomes.

[23]A child is defined as stunted if his height-for-age Z-score is less than -2 standard deviations from the median.

[24]Across all estimates, the OLS model finds a negative and statistically significant relationship between women's years of schooling and the likelihood of being stunted and anthropometric height and weight for age and body mass index all present positive and significant coefficients.

tensity. In other words, it is important to test the difference-in-differences assumption. If zones with poorer levels of pre-reform schooling (and thus, are assigned a higher intensity level) also experienced rapid increases in schooling prior to the reform; then the exclusion restriction would be violated. To examine this possibility, I divide the zones by their intensity level, and examine the trends in enrollment.

The trends are examined graphically in Figure 5, for the 1960 and 1969 cohorts. Although levels are substantially different, the patterns are similar over this time period. Since the trends are consistent, there is no evidence of divergent pre-reform trends. Similarly, in Appendix Figures D.1 and D.2, I graphically examine the pre-trends for the likelihood of stunting and the height for age of the children of the women affected by the reform.

In the second place, to further test the potential consequences of the onset of the reform, I conduct an event study analysis, as shown in Figure 6. The event is defined as being born in the year 1986, which is the cohort that will fully benefit from the reform.[25] The estimation includes five lags, five leads, and controls for age, time, and year-fixed effects. In Appendix Figures D.3 and D.4, I provide the event study coefficients for two outcome variables: the likelihood of stunting and height for age of the children of the women affected by the reform. As shown, no pre-trends can be found for any variable.

6.2 Differential access to health services

Another important challenge to the causal interpretation of the impact of maternal education on child health relates to the fact that the cohorts that where directly affected by the UPE reform, also benefited from an increase in health care access. As shown in Figure 7, the national health expenditure rose significantly in 1995.[26] Therefore, child health improvements may be related to increases in maternal education or might be a consequence of health access.

To account for the increase in access to health care, I use reports from the Ministry of Health (2000, 2003, 2005, 2008) to proxy access levels at the time of birth of the children in each zone. I calculate the number of publicly provided health facilities (hospitals, health stations, health posts, and health centers) per thousand inhabitants.[27] I then re-estimate the results presented in Table 3, accounting for the increase in health care. Results are presented in Table 4.

Column 1 includes the estimates the effect of maternal education and in Column 2, I present the coefficients for the new control for the number of facilities. As shown in

[25] The same year will be utilized as the discontinuity point for the Regression Discontinuity Design in Section 6.4.

[26] Local governments also gained autonomy over their budgetary allocations, including the educational expenditure. Construction of health station, for example, grew steadily after the reform in two regions.

[27] The growth of private health facilities failed to keep pace with the population growth of around 3 percent in the same period.

the table, the effects of maternal education remain similar in magnitude and statistical significance. Even after the inclusion of health care variables, the effects of maternal education on child health persist.

Among the acute conditions the coefficient for diarrhea, fever,cough and wasting are equal to 0.023 (0.031 SE), 0.043 (0.029 SE), 0.048 (0.040 SE) and -0.012 (0.023 SE) but they are not statistically significant.

6.3 Differential Selection

As established by previous literature on the effects of UPE policies, the increase in completed years of education leads women to have fewer children (CITE). When considering the health quality of the children they have, two issues might arise. First, a significant concern relates to the potential for differential selection. Secondly, the DHS data on child health outcomes describe children born five years before the survey.

To deal with the challenge of incomplete birth histories, I analyze the impacts of additional years of schooling on only the first-born child. This approach only partially solves the issue because I would need survey data from women who obtained the maximum age at which women have a first child. Although biased, estimating the causal impact of maternal education on health outcomes may provide a lower bound for the desired impact for firstborn children. The two sage least square estimation coefficients are provided in Table C.4. The results are similar to the full sample in magnitude and statistical significance.

6.4 Regression Discontinuity Design

As an alternative identification strategy to assess the effects of maternal education on child health, I propose a fuzzy regression discontinuity design. This strategy was used by Keats (2018) in Uganda and by Behrman (2015) in Ethiopia to study the effects of female education on child health and desired fertility, respectively. The regression discontinuity empirical strategy is such that the reform is treated as a random event for girls just below primary school exit age (age 11–13) but not girls who were slightly older (age 14–16) at the time of policy implementation.

Econometrically, I exploit a discontinuity in the probability of exposure to UPE conditional on birth cohort such that the discontinuity is used as an instrumental variable for treatment status. Given the Ethiopian setting, I follow Behrman (2015) and deal with potential noncompliance using a fuzzy regression discontinuity design.[28] The running variable is the year of birth of the mother, the treatment is each additional year of schooling and the cutoff point is the 1985 year of birth (which I will define as c). I then conduct a two-stage least squares specification:

[28]The classic deterministic model, where exposure to the reform is a deterministic and discontinuous function of the birth cohort, will not work in the Ethiopian setting because grade repetition, long-term absenteeism, and late entry into the school were frequent at the time (Handa et al., 2016).

$$\begin{aligned} YrsSchl_{mzy} &= \alpha_0 + \theta_1 Z_m + g(\text{Year Birth} - c) + \theta_2 x_{mzy} + \epsilon_{mzy} \\ Health_{ir} &= \alpha_0 + \beta_{FRD}\widehat{YrsSchl_{mzy}} + g(\text{Year Birth} - c) + \beta_2 x_{mzy} + \xi_i ry \end{aligned} \tag{10}$$

where Z is equal to 1 if the mother was born after 1986 and zero otherwise.[29]

I present the regression discontinuity in completed years of education of the mother in Figure 8. Next, the results described in Table 5 are consistent with the findings in Table 3 for chronic conditions. The first stage coefficient for the estimation is estimated to be 0.430 years (0.150 SE, $p < 0.001$). The estimates for the BMI Z-score and weight for age Z-score are 0.004 (0.001 SE, $p<0.1$) and 0.453 (0.001 SE, $p<0.1$) standard deviations, respectively. More educated mothers have children that are -0.202 (0.000 SE, $p< 0.1$) percentage points less likely to be stunted and the height for age Z-score is equal to 0.844 (0.01 SE, $p < 0.1$).All of the coefficients for the acute conditions are statistically significant.The coefficient for diarrhea, fever, cough and wasting are equal to 0.055 (0.000 SE, $p< 0.01$), -0,033 (0.000 SE, $p < 0.01$), -0.051 (0.000 SE, $p < 0.01$) and -0.096 (0.000 SE, $p < 0.01$) respectively.

7 Mechanisms

Many pathways could link maternal education to child health outcomes. The list includes issues related to increased investments in children (Becker and Lewis (1973) and Keats (2018)), women's reproductive behaviors (Behrman, 2015) and the possibility of assortative mating. Chicoine (2021) found that women affected by the Ethiopian reform were more likely to be employed in the skilled and professional sector. Thus, I begin by testing if the reform whether women change their investment in inputs to infant health. In particular, I consider whether women who obtain a higher level of schooling invest more in their children's health and if they seek appropriate care during and after pregnancy.

As shown in Table 6, maternal education increases the number of antenatal visits by 0.190 appointments. The additional years of schooling have an almost 9-point negative impact on the likelihood of a woman delivering her child at home rather than at a hospital or clinic (significant at the 1 percent level, row 3). Table 6 also suggests that an additional year of education translates into a four-point increase In the likelihood of being attended by a doctor or midwife during the delivery.[30] The coefficient represents

[29] In this model, the treatment effect is a local average treatment effect. The main coefficient captures the impact of schooling on the health outcome for those who would get an additional year of education if school fees were eliminated only if the school fees are eliminated (the compliers) measured at the cutoff. Three critical assumptions need to be satisfied for the fuzzy regression discontinuity design to provide valid estimates. First, the outcomes at the cutoff change solely as a result of the mothers' additional schooling. Next, individuals must not manipulate their treatment status. Third, we need to assume that the covariates that affect both the schooling decision and outcomes of interest vary smoothly across the cutoff.

[30] While the many DHS rounds contain survey questions on these dimensions, postnatal care is only

an 11 percent increase as only 37 percent of women have a trained professional at the birth of their child.

In contrast, using the instrumental variable approach, I do not find statistically significant evidence that women's schooling increases the probability that children are immunized against diseases. Children are fully vaccinated if, in their first year, they receive immunization for measles, tuberculosis, polio, and the DPT (diphtheria, pertussis, and tetanus). As shown in Panel B, women's education does not increase the probability that a child who is one year of age or older is fully immunized.

A second set of mechanisms that may affect child health outcomes relates to outcomes likely fixed before women had children. In particular, I analyze reproductive behaviors and cohabitation outcomes, including the onset of sexual activity, the timing of cohabitation, and the age at first birth. Even though women who obtain more education are just as likely to have sex at a young age, they, on average, report that they first cohabitate with a spouse at a later age.

The third possibility relates to the fact that women in Ethiopia married men with higher levels of schooling. The education reform caused a significant change in the educational attainment. As a result, women impacted by the reform potentially sought out partners with higher levels of education than they would have before the reform. This is because some men who would have previously married women unaffected by the reform were no longer eligible. If both treated and untreated women were to have partners with different levels of education, it would be impossible to separate the effects from fathers and mothers. I test this possibility by estimating the husband's education in the first stage. I do not find a strong relationship between the teaching of the husband and the instrumental variable for their spouses' potential years of schooling. These estimates are not available upon request.

8 Discussion

This paper provides evidence that the universalization of primary school education in Ethiopia not only led to an increase in women's education but also had spillover effects that improved their children's long-term health. As Chicoine (2021) points out, the Ethiopian UPE reform resulted in additional years of schooling that reduced fertility by over 0.4 births. The findings in that paper also indicate that the pathway through which education affects the number of children relates to an increased likelihood of being employed in higher-quality jobs and a reduction in women's ideal number of children. Given this evidence, Ethiopia is a perfect scenario to examine the neoclassical idea that there is a trade-off in women's fertility and the quality of their children (Becker & Lewis, 1973).

To test the neoclassical prediction regarding fertility, I use an instrumental variable

available for the 2003 and 2008 survey rounds.

approach to assess the impact on the desired outcomes. The identification strategy proposed by (Chicoine, 2019,2021) exploits the temporal and regional variations in pre-existing levels of education to estimate the potential impact of the reform without relying on the supply or quality of schooling. The strategy sets individuals into three groups: those who did not benefit from the reform because they were already out of school by the time of implementation, a faction who gained some years of education with some probability, and, finally, a group that was able to take advantage of the added schooling fully. Furthermore, I also test the effects of maternal education through a fuzzy regression discontinuity design to confirm the results and examine the outcomes around the threshold between the second and third groups of women.

Results in this paper indicate that mothers who benefited from the reforms and, thus, attended more school due to the elimination of school fees gave birth to children who were eight percentage points less likely to be stunted and had 0.16 standard deviations better height-for-age Z-scores on average. The coefficients resulting from the regression discontinuity analysis are consistent for the variables associated, if slightly higher.[31] Both measures are related to the long-term effects of malnutrition and chronic conditions. The findings are smaller but close to the results of Keats (2018) and Currie and Moretti (2003) in Uganda and the US, respectively. Even though these effects seem small, the consequences accumulate over time and significantly impact the long run (Alderman et al., 2006). Improvements in height have been found to be linked to better economic outcomes later in life (Cinnirella, Piopiunik, & Winter, 2011).

Conversely, maternal education does not appear to influence child weight or wasting, typically linked to more recent episodes of malnutrition or illness. The result is consistent across the instrumental variable and the regression discontinuity design. Nor does maternal education appear to be linked to the likelihood of reporting an episode of coughing or diarrhea in the two weeks before the survey. Although reporting a fever is found to be positive and slightly significant using the instrumental variable identification strategy, the statistical significance does not survive the inclusion of hospital controls or the regression discontinuity design.

Third, the evidence regarding the impact of maternal education on infant mortality is mixed. While the instrumental variable approach renders a five percentage points decrease in the probability that the child died before one year old, the regression discontinuity strategy yields a coefficient almost double in size. The effects contrast those found by Keats (2021), who does not find a difference in the probability that the first-born child died in Uganda. However, the effects are smaller than those of Chou et al. (2010) and Breierova and Duflo (2004), who find a significant reduction in child mortality in Taiwan and Indonesia, respectively.

Finally, I consider the pathways that explain the relationship between maternal education and the improvements in child health. I find that controlling for the num-

[31] Probably because of local nature of the identification strategy

ber of health facilities in the zone, women with more schooling are less likely to give birth at home. In contrast, education increases the probability of being assisted by a professional attendant at birth. The results contrast with the findings by McCrary and Royer (2011), whose effects on maternal behavior are minor and of mixed signs. In the more similar context of Uganda, however, Keats (2018) finds similar evidence regarding maternal-child investment decisions.

Taken together, this paper's results related to height and mortality are consistent with the trade-off model between the quantity and quality of children. Specifically, they validate the theoretical predictions by (Becker & Lewis, 1973) and support the empirical evidence provided by authors such as Chou et al. (2010), Breierova and Duflo (2004), and Keats (2018) regarding the role maternal education can have in the quality of life of children. The results in this paper are consistent with the idea that education has such a significant impact that it has visible effects, even when teachers confront more significant numbers of students in their classrooms and when school construction fails to keep up with increasing enrollment. Mass schooling is essential in shifting health and health behaviors beyond the targeted generated.

9 Conclusion

This paper examines the second-generation returns to a nationwide free primary education reform in Ethiopia. To identify the effect of removing school fees, I take advantage of the temporal and geographic variations in the prior education levels and estimate the probability of obtaining additional school years. Using this identification strategy and a regression discontinuity design, I find that children of more educated women have better outcomes associated with height and infant mortality. Thus, the results of this paper confirm the hypothesis that more educated women impact fertility decisions and have a vital role in child health and health behaviors.

This paper's main contribution is to add empirical evidence to the literature concerning the strong relationship between female education and child outcomes in a developing-country setting. Policymakers have viewed maternal education as a tool to obtain greater levels of development. Implementing this type of policy has been standard in Sub-Saharan Africa since the mid-'70s and aimed to promote primary school enrollment drastically. Therefore, it is essential to assess not only short-run effects but also possible spillovers into the next generation.

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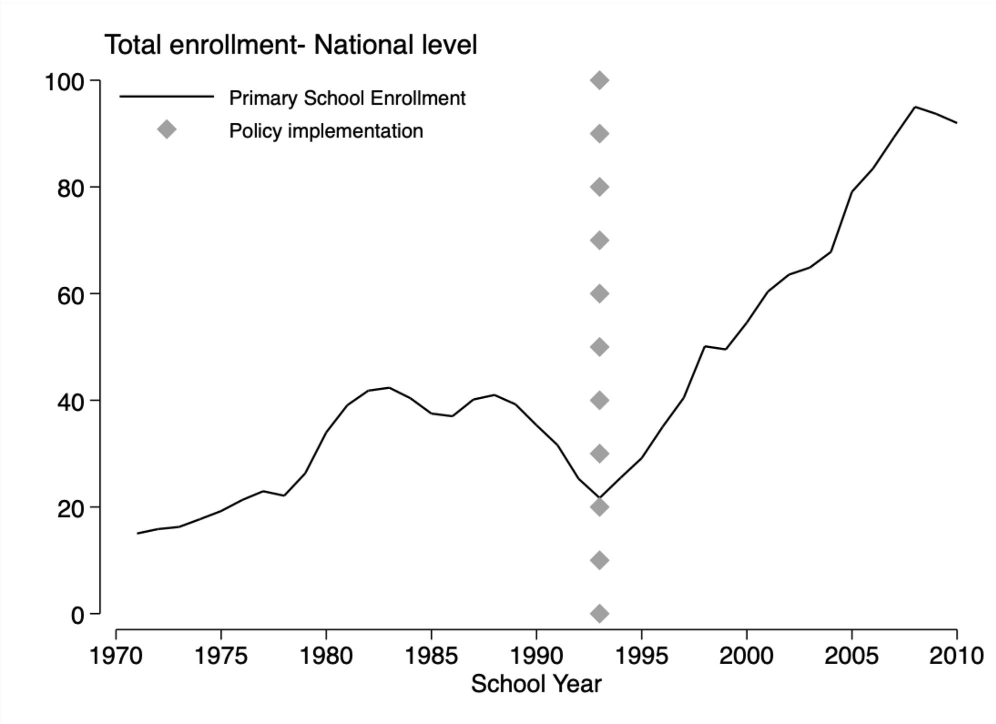
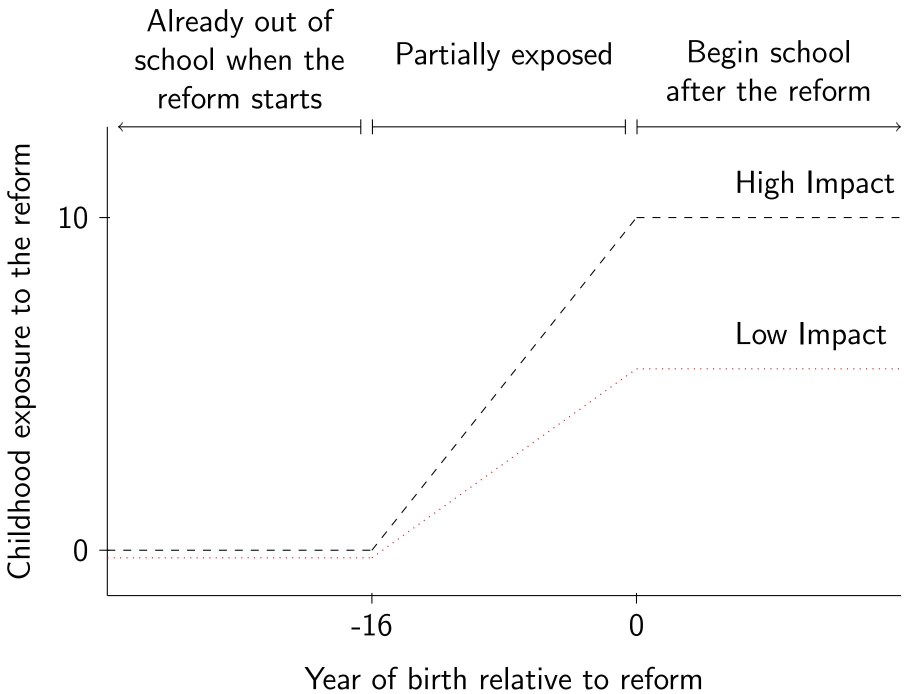
A Graphs and Tables

Figure 1: Primary school enrollment - National level



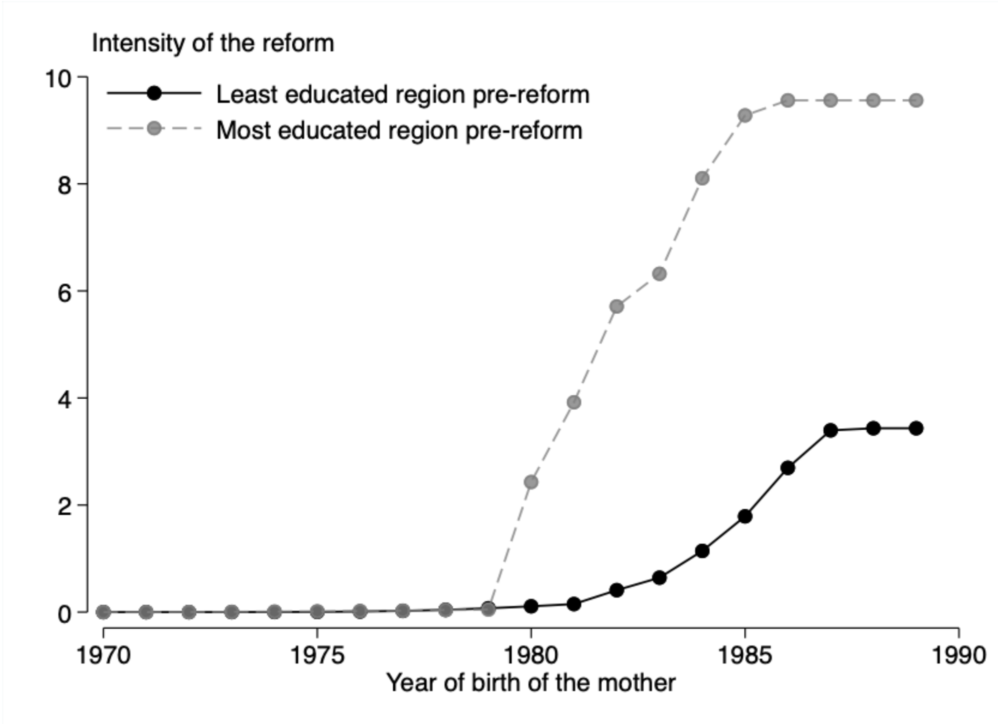
Source: World Development Indicators. Observations: 40

Figure 2: Identification strategy - Cohort and zone variations



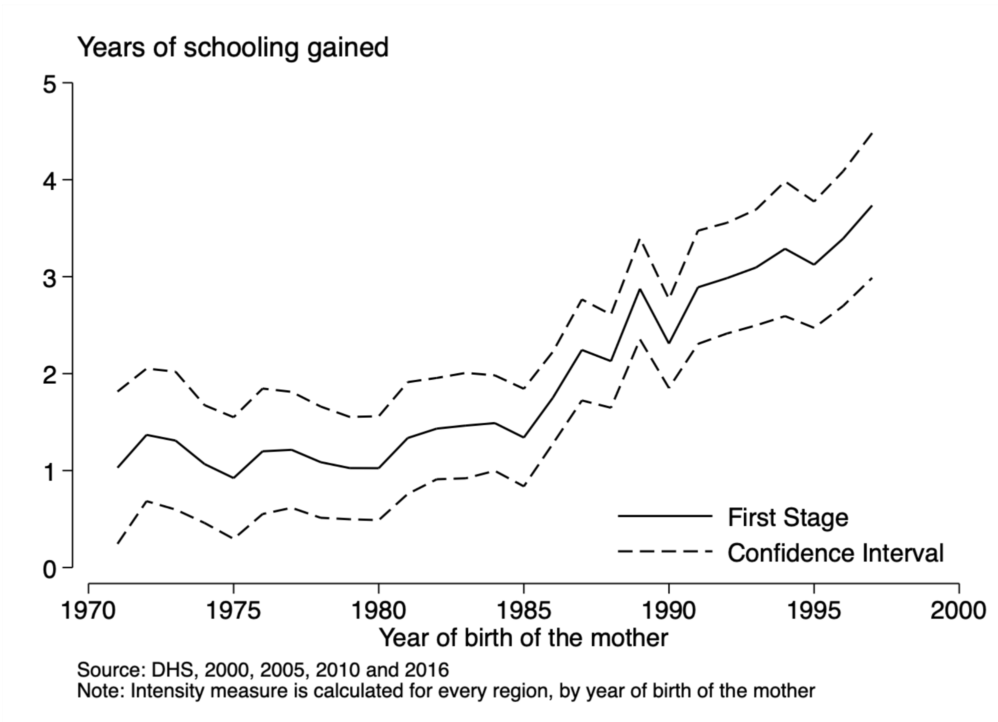
Notes: In Figure 2, I present the fraction of years girls are exposed to a hypothetical free primary education reform. The number of years gained is calculated as a function of the year of birth minus the start year of the reform. Whether the girls live in a high or low exposure region depends on the pre-trends in education levels of women in each region. The patterns are reflected in the data, as shown in Figure 3.

Figure 3: Intensity measure for highest and lowest regions, by birth year



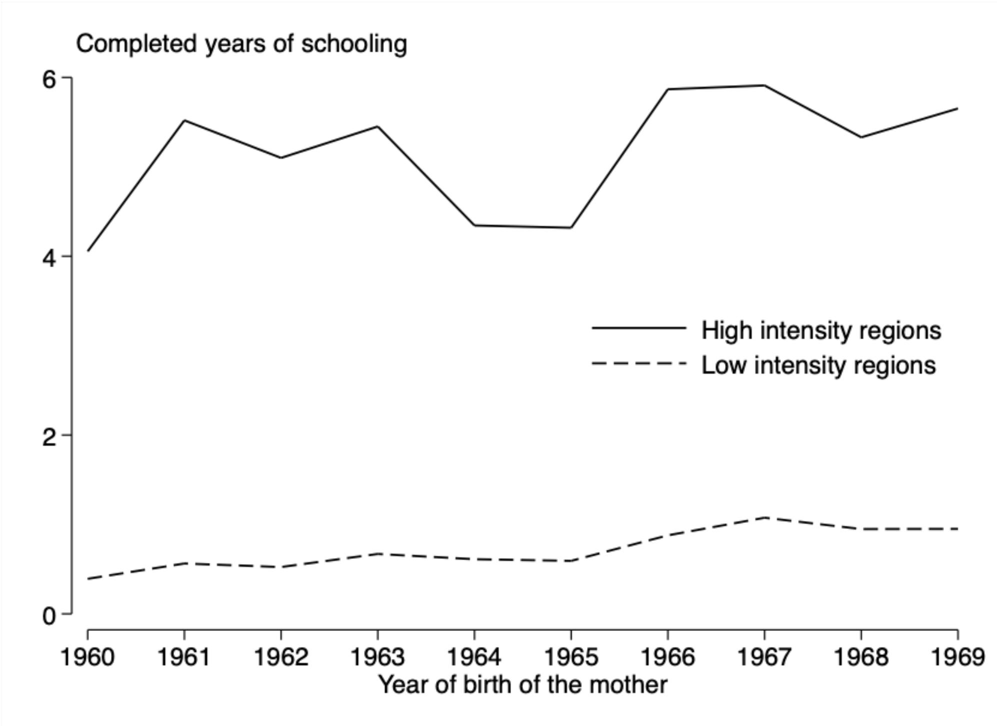
Source: 1994 Ethiopian census data. Observations: 2,509,888
Notes: Intensity measure is calculated for every region, by year of birth of the mother

Figure 4: First stage estimates by year of birth of the mother



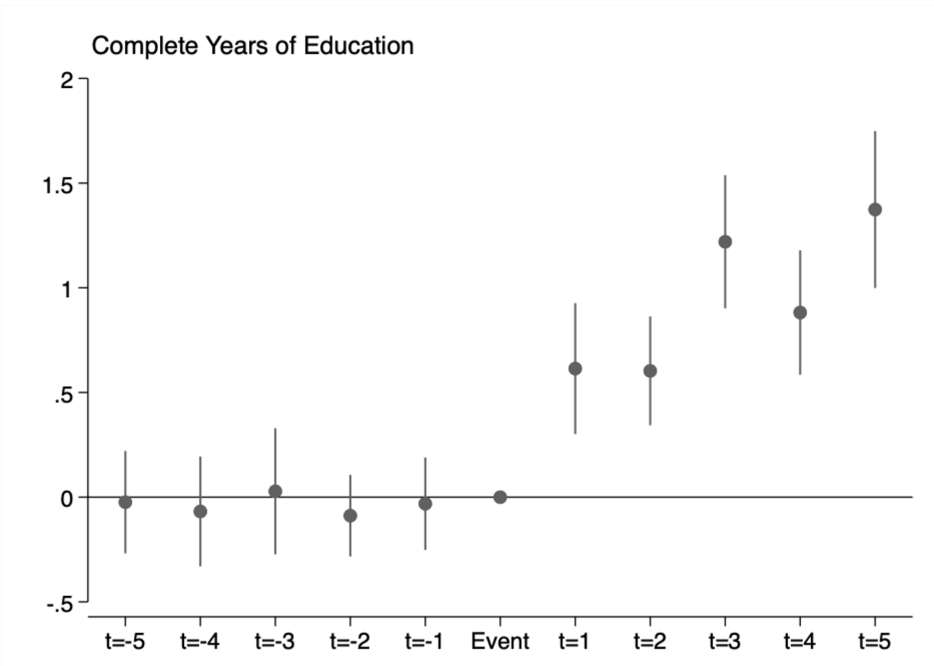
Source: Ethiopia DHS 2000, 2005, 2010 and 2016. Observations: 25,271

Figure 5: Pre-treatment trends of maternal schooling



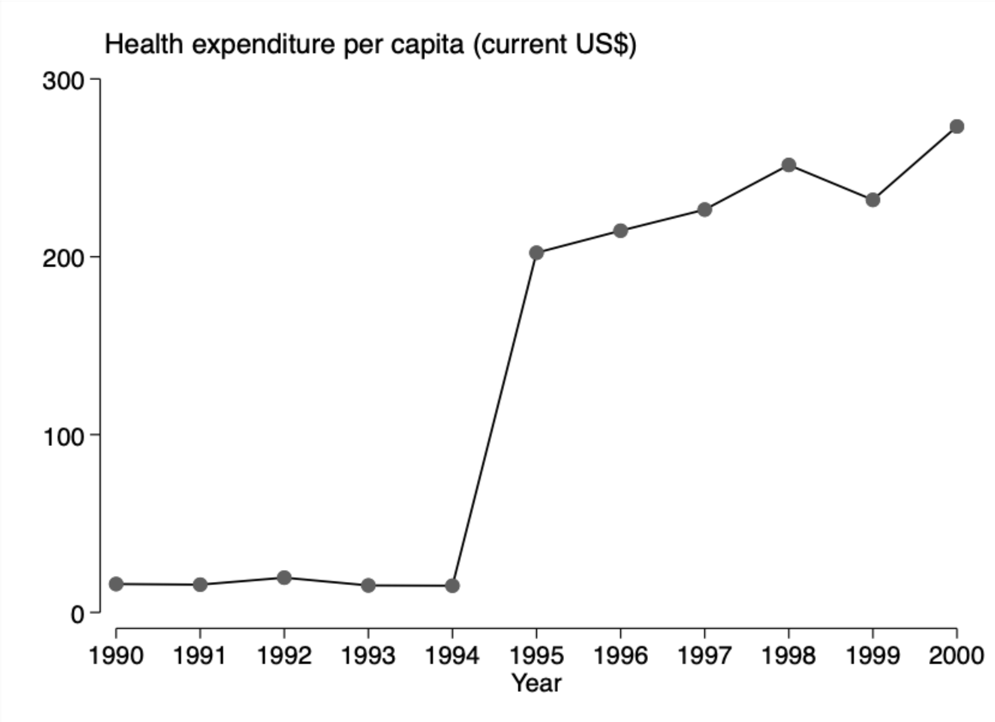
Source: Ethiopia DHS 2000, 2005, 2010 and 2016. Observations: 50,199

Figure 6: Event Study Analysis - Years of Completed Education



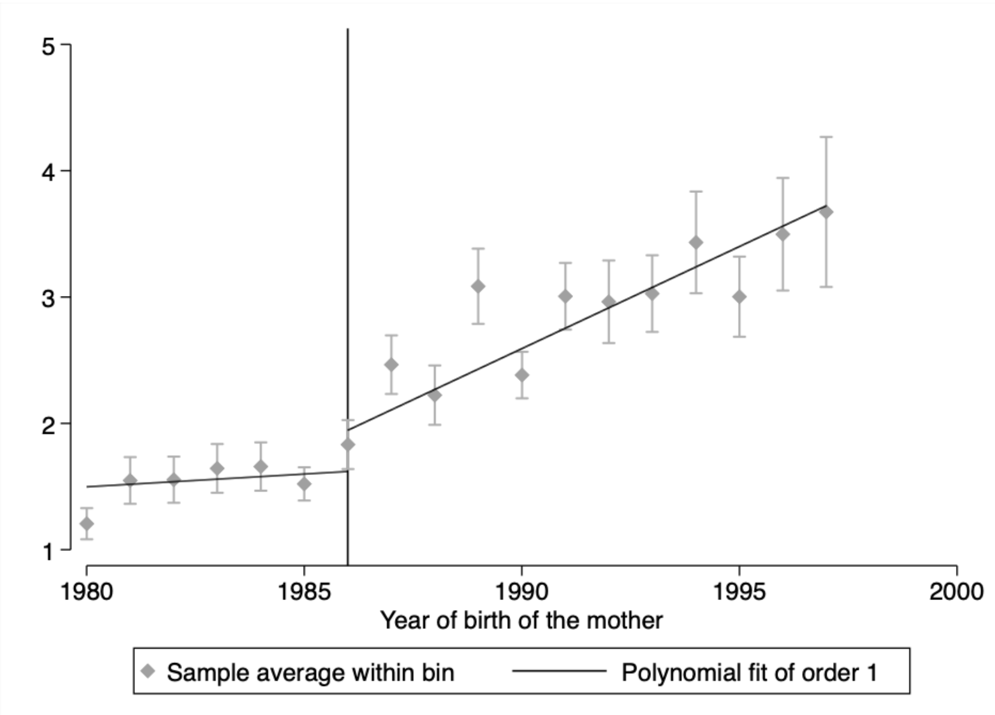
Source: Ethiopia DHS 2000, 2005, 2010 and 2016. The "event" is defined as the women born in 1986, that fully benefited from the FPE policy. Each lead and lag corresponds the number of years before or after the reform, respectively. Observations: 22,258

Figure 7: Health Expenditure - National level



Source: World Bank Historical data - Retrieved from https://tradingeconomics.com/ on April 7th, 2020

Figure 8: Discontinuity in Years of Education of the Mother



Source: Ethiopia DHS 2000, 2005, 2010 and 2016. Sample restricted to women born between 1980 and 1997.

Table 1: Summary statistics – By level of intensity - Mother and children data

	No impact cohorts 1971-1973		Partial impact cohorts 1974-1986		Full impact cohorts 1987- 1997	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Mother level variables						
Mother's age	31.677	5.561	28.586	5.371	23.619	3.331
Age at first birth	19.483	3.728	18.919	3.591	18.161	2.793
Married	0.915	0.279	0.908	0.288	0.915	0.278
Number of Children	4.876	2.483	4.089	2.276	2.612	1.525
Years in district	21.950	11.196	21.203	10.959	18.520	9.317
Rural Household	0.828	0.378	0.831	0.375	0.819	0.385
Years of schooling	1.588	3.277	1.415	3.100	2.675	3.885
Literacy	0.183	0.387	0.131	0.337	0.180	0.385
N	2317		13424		6787	
All children sample						
Age of the child	2.496	1.406	2.405	1.443	2.161	1.425
Male	0.505	0.500	0.505	0.500	0.506	0.500
Stunting	0.452	0.498	0.400	0.490	0.339	0.473
BMI Z-score	-0.384	1.275	-0.392	1.318	-0.420	1.283
Height-for-age Z-score	-1.341	1.291	-1.208	1.302	-1.052	1.282
Weight-for-age Z-score	-1.649	1.718	-1.435	1.755	-1.161	1.772
Child has diarrhea	0.195	0.396	0.168	0.374	0.134	0.341
Wasting	0.308	0.462	0.270	0.444	0.228	0.420
N	2317		13424		6787	

Notes: Source: Ethiopia DHS 2000, 2005, 2011 & 2016. The table considers mothers with at least one child. The cohorts were selected such that the intensity of the reform is null in the first set of columns, greater than zero and less than the maximum possible in the next set or the greatest possible in the last set. The top panel includes mother characteristics and the bottom panel displays children's data.

Table 2: First stage estimates: Effect of the Reform on Years of schooling

	Years of Schooling	
	Intensity of the Reform	
	(1)	(2)
$Intensity_{zy}$	0.0880***	0.0852***
	(0.0204)	(0.0211)
Observations	22,528	22,528
Adjusted R^2	0.001	0.001
F- statistic	18.68	16.31
Fixed Effects	Yes	Yes
Facility controls		Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Source: Ethiopia DHS 2000, 2005, 2011 and 2016. The dependent variable is the years of schooling of the mother. In columns 1 and 2, the intensity measure for each regression is the estimated intensity of the reform in region r for women born in year y. The sample includes women in birth cohorts from 1971 to 1997. All regressions include birth year fixed effects and household controls. Regressions also include region linear trends. Standard errors in parenthesis, clustered at the zone level.

Table 3: Effect of mother's schooling on child health outcomes

	Two Stage Least Squares Estimation				
	Coeff	S.E.	N	R-Sq	Mean dep. var. Before intervention
	(1)	(2)	(3)	(4)	(5)
Chronic conditions					
Stunting	-0.080**	(0.038)	22,528	-0.038	0.457
BMI Zscore	-0.072	(0.098)	22,528	0.016	-0.381
Height-for-Age Zscore	0.163*	(0.095)	22,528	0.161	-1.676
Weight-for-Age Zscore	0.060	(0.083)	22,528	0.151	-1.356
Wasting	-0.014	(0.022)	22,528	0.064	0.319
Acute conditions					
Diarrhea	0.024	(0.031)	22,528	0.021	0.200
Fever	0.047*	(0.028)	22,528	0.019	0.246
Cough	0.057	(0.040)	22,528	-0.085	0.255
Mortality					
Infant Mortality	-0.049*	(0.028)	33,672	0.159	0.350
First stage F-statistic		18.68			
Year of birth and geographic Fixed effects		Yes			
Controls		Yes			

Note: *** p<0.01, ** p<0.05, * p<0.1. Source: Ethiopia DHS 2000, 2005, 2011 and 2016. The dependent variable for each regression is the health outcome of the child. $\widehat{YrsSchl_{mzy}}$ is the predicted level of schooling, instrumented using $Intensity_{zy}$, which measures the exposure of the woman born in year y in region r to the reform. Columns 1 and 2 present the two stage least square coefficients and standard errors. All regressions include birth year fixed effects, and household controls. Regressions also include region linear trends. Standard errors in parenthesis, clustered at the zone level.

Table 4: Effect of mother's schooling on child health outcomes

Two Stage Least Squares Estimation - Health facilities controls

	Coeff $\widehat{YrsSchl_{mzy}}$ (1)	S.E. (2)	Facilities per 1000 hab. (3)	S.E. (4)	N (5)	R-sq (6)	Mean dep. var. (7)
Chronic conditions							
Stunting	-0.077**	(0.036)	-0.621***	(0.228)	22,528	-0.023	0.457
BMI Zscore	-0.064	(0.102)	-0.269	(0.786)	22,528	0.021	-0.381
Height-for-Age Zscore	0.155*	(0.093)	3.032***	(0.891)	22,528	0.164	-1.676
Weight-for-Age Zscore	0.060	(0.087)	1.679***	(0.545)	22,528	0.153	-1.356
Wasting	-0.012	(0.023)	-0.502***	(0.163)	22,528	0.066	0.319
Acute conditions							
Diarrhea	0.023	(0.031)	-0.000	(0.167)	22,528	0.021	0.200
Fever	0.043	(0.029)	0.113	(0.262)	22,528	-0.030	0.246
Cough	0.048	(0.040)	0.441	(0.269)	22,528	-0.050	0.255
Mortality							
Infant Mortality	-0.043	(0.028)	-0.750***	(0.169)	33,672	0.173	0.350
First stage F-statistic			16.31				
Year of birth Fixed effect			Yes				
Controls			Yes				

Note: *** p<0.01, ** p<0.05, * p<0.1. Source: Ethiopia DHS 2000, 2005, 2011 and 2016. The dependent variable for each regression is the health outcome of the child. $\widehat{YrsSchl_{mzy}}$ is the predicted level of schooling, instrumented with the intensity measure. Columns 1 and 2 present the two stage least square coefficients and standard errors. In all regressions, "Facilities per 1000. hab." is added as a control in the which measures the number of health access facilities per 1000 inhabitants in each specific region and for each year of birth of the child. All regressions include birth year fixed effects, and household controls. Regressions also include region squared linear trends. To deal with the small number of clusters (there are 11 regions), cluster p-values are created using the boottest command in Stata 16 (Roodman, Nielsen, MacKinnon, & Webb, 2019).

Table 5: Effects of mother's schooling on child health outcomes

Regression Discontinuity Design	Coefficient (1)	S.E. (2)	N (3)	Mean dep. var. Before intervention (4)
Chronic conditions				
Stunting	-0.099*	(0.049)	12,839	0.413
BMI Zscore	-0.034	(0.128)	12,839	-0.387
Height-for-Age Zscore	0.481**	(0.190)	12,839	-1.487
Weight-for-Age Zscore	0.259*	(0.133)	12,839	-1.239
Wasting	-0.053	(0.037)	12,839	0.279
Acute conditions				
Diarrhea	-0.053	(0.038)	12,839	0.173
Fever	-0.024	(0.038)	12,839	0.215
Cough	-0.057	(0.047)	12,839	0.225
Mortality				
Infant Mortality	-0.094***	(0.034)	16,518	0.350
First-Stage Regressions				
Coefficient	0.430***	(0.150)	12,839	
Year of birth Fixed effect		Yes		
Controls		Yes		

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors in parenthesis, clustered at the zone level. $\widehat{YrsSchl_{mzy}}$ is the independent variable is the estimated effect of the instrument on the years of schooling of the mother. All regressions include the birth order, the religion of the mother, the age of the mother, the age squared, the age the mother's birth year and zone fixed effects. Regressions also include zone linear trends.

Table 6: Effect of mother's schooling on child Health - Mechanisms

Two Stage Least Squares Estimation

	Coeff	S.E.	N	R-Sq	Mean dep. var. Before intervention
	(1)	(2)	(3)	(4)	(5)
Health Care Seeking Behavior					
Number of Prenatal Visits	0.190*	(0.099)	15,379	0.404	1.784
Trained Prenatal Attendant	0.006	(0.029)	15,688	0.274	0.356
Childbirth at Home	-0.081***	(0.027)	22,527	0.269	0.873
Trained Assistant at Birth	0.042**	(0.019)	15,688	0.373	0.106
Attended Postnatal Care	0.037	(0.031sdz)	17,090	0.077	0.0998
Children's Vaccination Status					
Tuberculosis	0.006	(0.039)	19,373	0.205	0.563
Diphtheria	-0.043	(0.043)	19,287	0.115	0.552
Polio	0.050	(0.062)	19,434	-0.069	0.797
Measles	-0.015	(0.042)	19,129	0.240	0.404
Women's Reproductive Behaviors and Cohabitation					
Age at First Sex	-0.005	(0.005)	18,812	-0.095	0.994
Age at First Birth	0.354	(0.358)	22,528	0.592	19.40
Age at First Cohabitation	0.377*	(0.200)	22,369	0.426	16.85
Year of birth Fixed effect		Yes			
Controls		Yes			

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parenthesis, clustered at the zone level. $\widehat{YrsSchl_{mzy}}$ is the independent variable is the estimated effect of the instrument on the years of schooling of the mother. All regressions include the birth order, the religion of the mother, the age of the mother, the age squared, the age the mother's birth year and zone fixed effects. Regressions also include zone linear trends.

Appendix A Reform Intensity Equations

In this section, I present the equations used to calculate measure of the intensity of the reform for each cohort y in region r (Section 3.1). As a reminder, the main variables are the magnitude (which I will shorten to M_{zy} in this section for simplicity) and the starting school probabilities at age a (S_{za}). Women born in the 1972 cohort or earlier will not benefit from any free schooling because even if they enter school at the age of 12 would still complete all ten years of schooling early. Therefore, the measure of intensity for the 1972 cohort is equal to zero:

$$Intensity_{z,1972} = 0$$

Those born in 1973 who enter school at the age 12 could potentially receive one year if they are enrolled until grade 10. Next, those born in 1974 and starting at 12 could potentially have up to two free years of schooling, only if they have completed the first eight grades, and if begin school at age 11. The following equations iterate the equations between 1973 and 1981:

$$\begin{aligned}
Intensity_{z,1973} =& S_{z,12} \cdot M_{z,1978} \\
Intensity_{z,1974} =& S_{z,12} \cdot M_{z,1979} + S_{z,11} \cdot M_{z,1978} \\
Intensity_{z,1975} =& S_{z,12} \cdot M_{z,1980} + S_{z,11} \cdot M_{z,1979} + S_{z,10} \cdot M_{z,1978} \\
Intensity_{z,1976} =& S_{z,12} \cdot M_{z,1981} + S_{z,11} \cdot M_{z,1980} + S_{z,10} \cdot M_{z,1979} + S_{z,9} \cdot M_{z,1978} \\
Intensity_{z,1977} =& S_{z,12} \cdot M_{z,1982} + S_{z,11} \cdot M_{z,1981} + S_{z,10} \cdot M_{z,1980} + S_{z,9} \cdot M_{z,1979} \\
& + S_{z,8} \cdot M_{z,1978} \\
Intensity_{z,1978} =& S_{z,12} \cdot M_{z,1983} + S_{z,11} \cdot M_{z,1982} + S_{z,10} \cdot M_{z,1981} + S_{z,9} \cdot M_{z,1980} \\
& + S_{z,8} \cdot M_{z,1979} + S_{z,7} \cdot M_{z,1978} \\
Intensity_{z,1979} =& S_{z,12} \cdot M_{z,1984} + S_{z,11} \cdot M_{z,1983} + S_{z,10} \cdot M_{z,1982} + S_{z,9} \cdot M_{z,1981} \\
& + S_{z,8} \cdot M_{z,1980} + S_{z,7} \cdot M_{z,1979} + S_{z,6} \cdot M_{z,1978} \\
Intensity_{z,1980} =& S_{z,12} \cdot M_{z,1985} + S_{z,11} \cdot M_{z,1984} + S_{z,10} \cdot M_{z,1983} + S_{z,9} \cdot M_{z,1982} \\
& + S_{z,8} \cdot M_{z,1981} + S_{z,7} \cdot M_{z,1980} + S_{z,6} \cdot M_{z,1979} \\
Intensity_{z,1981} =& S_{z,12} \cdot M_{z,1986} + S_{z,11} \cdot M_{z,1985} + S_{z,10} \cdot M_{z,1984} + S_{z,9} \cdot M_{z,1983} \\
& + S_{z,8} \cdot M_{z,1982} + S_{z,7} \cdot M_{z,1981} + S_{z,6} \cdot M_{z,1980}
\end{aligned}$$

The first cohort for which the post-reform entry probabilities must be considered is 1982. A correction is added to account for the stock of students that would have entered if schooling was free but did not ($S_{z,a} - S_{z,a,pre}$). For the small group of "late entrants" in the 1982 cohort, they may have other responsibilities that keep them away from entering school.

$$
\begin{aligned}
Intensity_{z,1982} =& S_{z6}\cdot M_{z,1981} + S_{z7}\cdot M_{z,1982} + S_{z8}\cdot M_{z,1983} + S_{z9}\cdot M_{z1984} + S_{z10}\cdot M_{z1985}\\
&+ S_{z11}\cdot M_{z,1986} + S_{z11}\cdot M_z^{Max} + [(10)F_{z,0}]\,\frac{1}{e^{12-7}}\sum_{a=6}^{11}(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1983} =& S_{z6}\cdot M_{z,1982} + S_{z7}\cdot M_{z,1983} + S_{z8}\cdot M_{z,1984} + S_{z9}\cdot M_{z1985}+\\
&S_{z10}\cdot M_{z1986} + M_z^{Max}\sum_{a=11}^{12} S_{z,a} + [(10)F_{z,0}]\,\frac{1}{e^{11-7}}\sum_{a=6}^{10}(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1984} =& S_{z6}\cdot M_{z,1983} + S_{z7}\cdot M_{z,1984} + S_{z8}\cdot M_{z,1985} + S_{z9}\cdot M_{z1986} + M_z^{Max}\sum_{a=10}^{12} S_{z,a}\\
&+ [(10)F_{z,0}]\,\frac{1}{e^{10-7}}\sum_{a=6}^{9}(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1985} =& S_{z6}\cdot M_{z,1984} + S_{z7}\cdot M_{z,1985} + S_{z8}\cdot M_{z,1986} + M_z^{Max}\sum_{a=9}^{12} S_{z,a}\\
&+ [(10)F_{z,0}]\,\frac{1}{e^{9-7}}\sum_{a=6}^{8}(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1986} =& S_{z6}\cdot M_{z,1985} + S_{z7}\cdot M_{z,1986} + M_z^{Max}\sum_{a=8}^{12} S_{z,a} + [(10)F_{z,0}]\,\frac{1}{e^{8-7}}\sum_{a=6}^{7}(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1987} =& S_{z6}\cdot M_{z,1986} + M_z^{Max}\sum_{a=7}^{12} S_{z,a} + [(10)F_{z,0}]\,(S_{z,a}-S_{z,a,pre})\\
Intensity_{z,1988} =& M_z^{Max}
\end{aligned}
$$

Appendix B Timing of the Reform

In the following table, I provide an additional representation of cohort variation exploited in the identification strategy. The table is a variation from the one presented by Chicoine (2021). The goal is to show how many years of free primary school were made available by the reform for a girl born in each year between 1977 and 1988.

Table B.1: Timing of the reform by cohort and on time entry

Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status
1977	Born			1978	Born			1979	Born			1980	Born			1981	Born			1982	Born		
1978	0			1979	0			1980	0			1981	0			1982	0			1983	0		
1979	1			1980	1			1981	1			1982	1			1983	1			1984	1		
1980	2			1981	2			1982	2			1983	2			1984	2			1985	2		
1981	3			1982	3			1983	3			1984	3			1985	3			1986	3		
1982	4			1983	4			1984	4			1985	4			1986	4			1987	4		
1983	5			1984	5			1985	5			1986	5			1987	5			1988	5		
1984	6			1985	6			1986	6			1987	6			1988	6			1989	6		
1985	7	G1		1986	7	G1		1987	7	G1		1988	7	G1		1989	7	G1		1990	7	G1	
1986	8	G2		1987	8	G2		1988	8	G2		1989	8	G2		1990	8	G2		1991	8	G2	
1987	9	G3		1988	9	G3		1989	9	G3		1990	9	G3		1991	9	G3		1992	9	G3	
1988	10	G4		1989	10	G4		1990	10	G4		1991	10	G4		1992	10	G4		1993	10	G4	
1989	11	G5		1990	11	G5		1991	11	G5		1992	11	G5		1993	11	G5		1994	11	G5	
1990	12	G6		1991	12	G6		1992	12	G6		1993	12	G6		1994	12	G6		1995	12	G6	UPE
1991	13	G7		1992	13	G7		1993	13	G7		1994	13	G7		1995	13	G7	UPE	1996	13	G7	UPE
1992	14	G8		1993	14	G8		1994	14	G8		1995	14	G8	UPE	1996	14	G8	UPE	1997	14	G8	UPE
1993	15	G9		1994	15	G9		1995	15	G9	UPE	1996	15	G9	UPE	1997	15	G9	UPE	1998	15	G9	UPE
1994	16	G10		1995	16	G10	UPE	1996	16	G10	UPE	1997	16	G10	UPE	1998	16	G10	UPE	1999	16	G10	UPE

Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status	Year	Age	Gr.	Status
1983	Born			1984	Born			1985	Born			1986	Born			1987	Born			1988	Born		
1984	0			1985	0			1986	0			1987	0			1988	0			1989	0		
1985	1			1986	1			1987	1			1988	1			1989	1			1990	1		
1986	2			1987	2			1988	2			1989	2			1990	2			1991	2		
1987	3			1988	3			1989	3			1990	3			1991	3			1992	3		
1988	4			1989	4			1990	4			1991	4			1992	4			1993	4		
1989	5			1990	5			1991	5			1992	5			1993	5			1994	5		
1990	6			1991	6			1992	6			1993	6			1994	6			1995	6		
1991	7	G1		1992	7	G1		1993	7	G1		1994	7	G1		1995	7	G1		1996	7	G1	UPE
1992	8	G2		1993	8	G2		1994	8	G2		1995	8	G2		1996	8	G2	UPE	1997	8	G2	UPE
1993	9	G3		1994	9	G3		1995	9	G3	UPE	1996	9	G3	UPE	1997	9	G3	UPE	1998	9	G3	UPE
1994	10	G4		1995	10	G4	UPE	1996	10	G4	UPE	1997	10	G4	UPE	1998	10	G4	UPE	1999	10	G4	UPE
1995	11	G5	UPE	1996	11	G5	UPE	1997	11	G5	UPE	1998	11	G5	UPE	1999	11	G5	UPE	2000	11	G5	UPE
1996	12	G6	UPE	1997	12	G6	UPE	1998	12	G6	UPE	1999	12	G6	UPE	2000	12	G6	UPE	2001	12	G6	UPE
1997	13	G7	UPE	1998	13	G7	UPE	1999	13	G7	UPE	2000	13	G7	UPE	2001	13	G7	UPE	2002	13	G7	UPE
1998	14	G8	UPE	1999	14	G8	UPE	2000	14	G8	UPE	2001	14	G8	UPE	2002	14	G8	UPE	2003	14	G8	UPE
1999	15	G9	UPE	2000	15	G9	UPE	2001	15	G9	UPE	2002	15	G9	UPE	2003	15	G9	UPE	2004	15	G9	UPE
2000	16	G10	UPE	2001	16	G10	UPE	2002	16	G10	UPE	2003	16	G10	UPE	2004	16	G10	UPE	2005	16	G10	UPE

Appendix C Appendix Tables

Table C.2: Effect of Maternal Education on Chronic Child Health Outcomes

Ordinary Least Squares Estimation				
	Coefficient	S.E.	N	R-Squared
	(1)	(2)	(3)	(4)
Chronic conditions				
Stunting	-0.011***	(0.002)	22,528	0.104
BMI Z-score	0.025***	(0.003)	22,528	0.054
Height-for-age Z-score	0.048***	(0.006)	22,528	0.190
Weight-for-age Z-score	0.049***	(0.004)	22,528	0.156
Wasting	-0.009***	(0.001)	22,528	0.066
Acute conditions				
Diarrhea	-0.002*	(0.001)	22,528	0.053
Fever	-0.000	(0.001)	22,528	0.053
Cough	-0.001	(0.001)	22,528	0.056
Mortality				
Infant Mortality	-0.003***	(0.001)	33,672	0.228

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Source: Ethiopia DHS 2000, 2005, 2011 and 2016. The dependent value is the child's health outcome. The set of independent variables include the years of education of the mother and all regressions include birth year and region fixed effects, and regional trends. Every regression also contains household controls not dependent on the reform. Additionally, standard errors are clustered at the zone level.

Table C.3: Reduced form Estimation - Child Health Outcomes

	Coeff (1)	S.E. (2)	N (3)	R-Sq (4)
Chronic conditions				
Stunting	-0.007***	(0.003)	22,528	0.100
BMI Z-score	-0.006	(0.008)	22,528	0.052
Height-for-age Z-score	0.014	(0.009)	22,528	0.185
Weight-for-age Z-score	0.005	(0.008)	22,528	0.146
Wasting	-0.001	(0.002)	22,528	0.064
Acute conditions				
Diarrhea	0.002	(0.003)	22,528	0.053
Fever	0.004	(0.003)	22,528	0.053
Cough	0.005	(0.003)	22,528	0.056
Mortality				
Infant Mortality	-0.003**	(0.001)	33,672	0.227
Year of birth Fixed effect		Yes		
Controls		Yes		

Note: *** p<0.01, ** p<0.05, * p<0.1. Source: Ethiopia DHS 2000, 2005, 2011 and 2016. The dependent variable for each regression is the health outcome of the child. The main independent variable is $Intensity_{zy}$, which is defined as the reform's intensity measure. Columns 1 and 2 present the two-stage least-squares coefficients and standard errors. All regressions include birth year fixed effects, and household controls. Regressions also include region linear trends. Standard errors in parenthesis, clustered at the zone level.

Table C.4: Effect of Years of mother's Schooling on Health Outcomes of the First-born Child

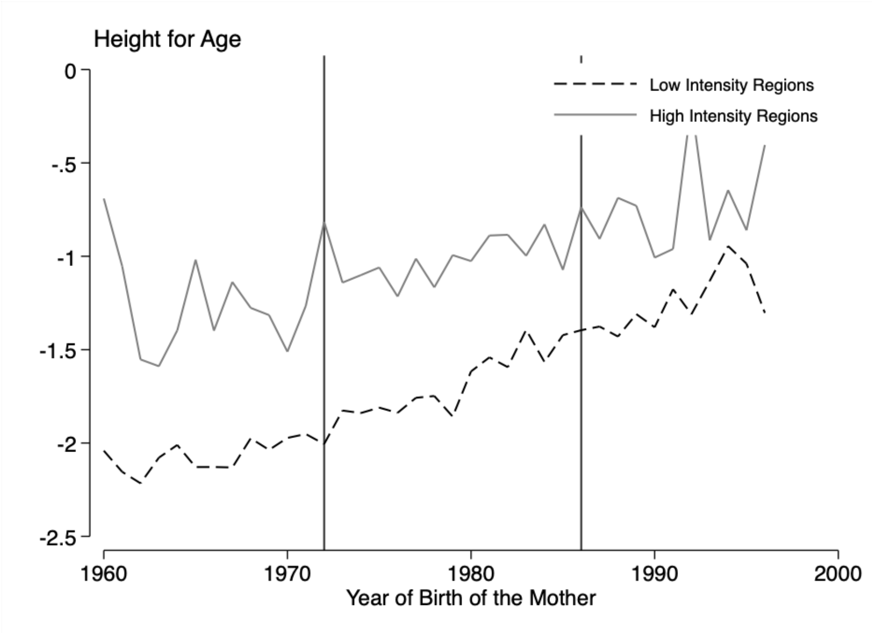
Two Stage Least Squares Estimation

	Coeff (1)	S.E. (2)	N (3)	R-Sq (4)
Chronic conditions				
Stunting	-0.053	(0.037)	15,446	0.083
BMI Z-score	-0.014	(0.080)	15,446	0.052
Height-for-age Z-score	0.159	(0.102)	15,446	0.207
Weight-for-age Z-score	0.092	(0.084)	15,446	0.172
Wasting	-0.015	(0.020)	15,446	0.084
Acute conditions				
Diarrhea	0.005	(0.027)	15,446	0.040
Fever	0.055**	(0.027)	15,446	-0.085
Cough	0.044	(0.034)	15,446	-0.030
Mortality				
Infant Mortality	0.010	(0.014)	16,835	0.059

Note: *** p<0.01, ** p<0.05, * p<0.1. Source: Ethiopia DHS 2000, 2005,2016. The dependent variable for each regression is the health outcome of the first-born child. $\widehat{YrsSchl_{mzy}}$ is the predicted level of schooling, instrumented with the reform intensity measure. Columns 1 and 2 presents the two stage least squares coefficients and standard errors of the estimated Years of education of the mother. All regressions include birth year fixed effects, and child controls for ethnicity. Regressions also include region squared linear trends. Additionally, standard errors are clustered at the zone level.

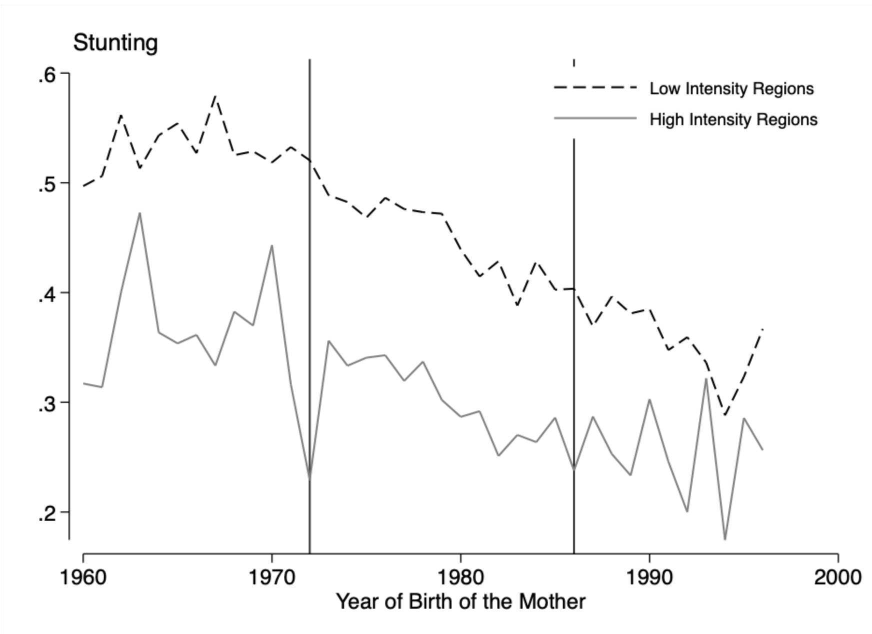
Appendix D Figures

Figure D.1: Pre-treatment trends for Height for Age



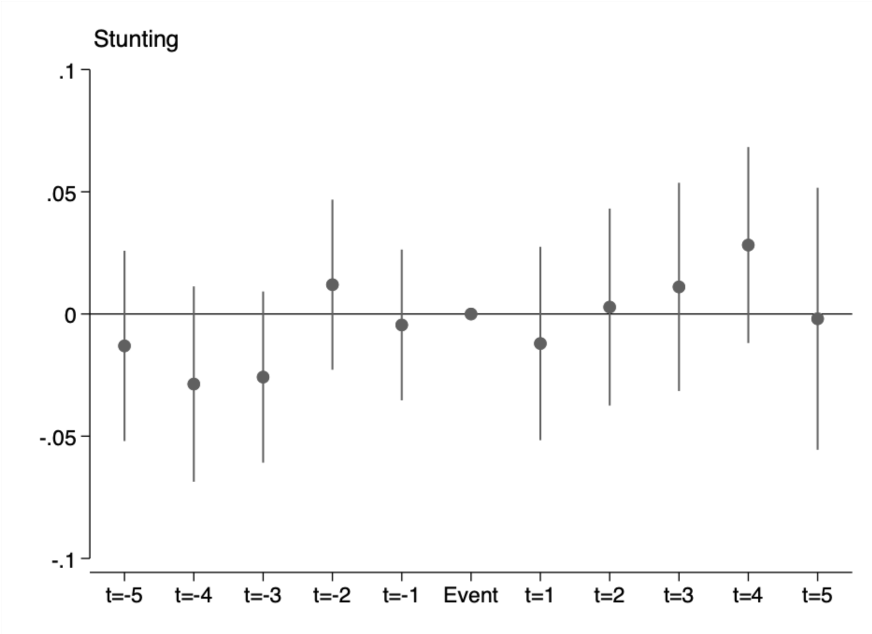
Source: Ethiopia DHS 2000, 2005, 2010 and 2016.

Figure D.2: Pre-treatment trends for Stunting



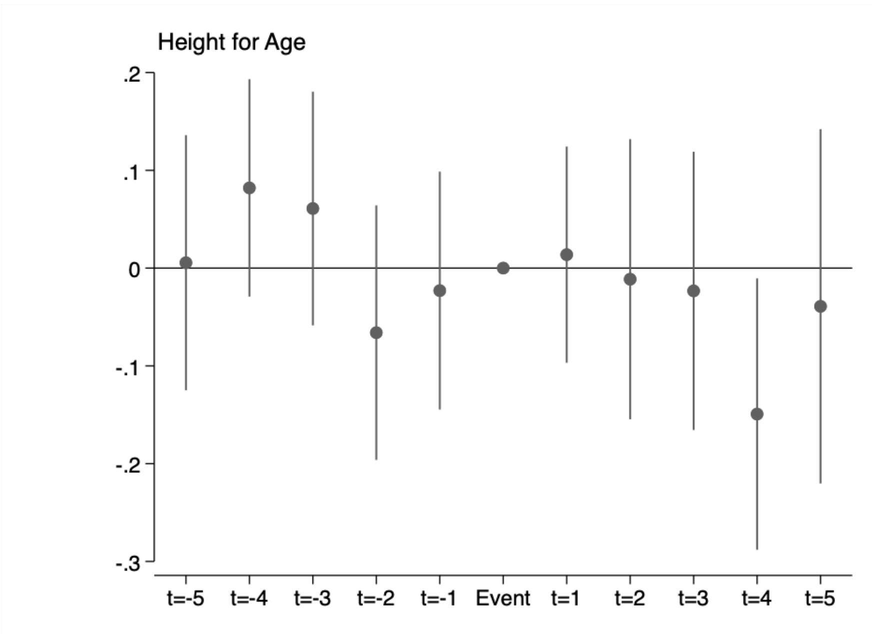
Source: Ethiopia DHS 2000, 2005, 2010 and 2016.

Figure D.3: Event Study Analysis - Stunting



Source: Ethiopia DHS 2000, 2005, 2010 and 2016.

Figure D.4: Event Study Analysis - Height for Age



Source: Ethiopia DHS 2000, 2005, 2010 and 2016.