

# 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 0.07 percentage points less likely to be chronically malnourished have increased 0.167 deviations in height for age Z-scores.<sup>1</sup>

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# 1 Introduction

Increasing women’s schooling has been a central goal for many developing countries and governments.<sup>2</sup> 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.<sup>3</sup>

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 regions 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 wellbeing. 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

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<sup>2</sup>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.

<sup>3</sup>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.<sup>4</sup> 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.

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).<sup>5</sup> 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 of fever, diarrhea, coughing, and wasting, 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).

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

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<sup>4</sup>The list includes school attainment, child health, and fertility outcomes among others.

<sup>5</sup>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 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).<sup>6</sup> 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.

I find that maternal education decreases the likelihood of being stunted by 2.6 percentage points. Children of more educated mothers report an increase of 0.11 and 0.12 standard deviations in their height and weight zscores, respectively. Conversely, I find less evidence of the effects of maternal education on acute conditions. My findings suggest a decrease of two percentage points on the likelihood of reporting diarrhea and wasting but no significant effects on the likelihood of experiencing coughing or fever.

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 analyzes the results and Section 7 the robustness checks. Section 8 presents the threats to validity and section 9 presents conclusions.

## 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).<sup>7</sup> As a result, by the beginning of the 1970 school year, there were large disparities in opportunities, which was reflected by

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<sup>6</sup>There is also evidence in more developed countries like the US and Norway (Black, Devereux, and Salvanes, 2008 and Monstad, Proper, and Salvanes, 2008).

<sup>7</sup>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.

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.<sup>8</sup> These fees created a significant burden for households.<sup>9</sup>

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.<sup>10</sup> 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 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.<sup>11</sup>

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<sup>8</sup>Fees usually did not include any text books, which had to be rented elsewhere.

<sup>9</sup>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.

<sup>10</sup>The policy encouraged community support for schools, but it prohibited per pupil fees.

<sup>11</sup>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.

### 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.

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 region's pre-reform levels of education. The concept is that in regions where enrollment levels had been high prior to the reforms, eliminating school fees did not have the same level of impact as in regions 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 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).<sup>12</sup>

#### 3.1 Instrumental variable

Following Chicoine (2019), I first create the maximum potential magnitude of the reform for each region-cohort 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  $g$ ,  $F_{zg}$ .<sup>13</sup> The equation can be summarized as follows:

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<sup>12</sup>In Chicoine (2021), the author presents a thorough justification for this assumption using census data.

<sup>13</sup>For example,  $F_{z0}$  is the group of women who never attended school in region  $r$ , and  $F_{z1}$  is the fraction of women who left after the first grade.

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

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.<sup>14</sup> For the cohorts, the magnitude is equal to:

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

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 region, the magnitude can be modified to account for 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:

$$\sum_{a=6}^{12} S_{z,a} = 1 \quad (3)$$

Next, the specific magnitudes are combined with the starting age probabilities to construct a reform intensity for each region ( $r$ ) 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:

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



$$\sum_{p=6}^7 S_{z,a} \text{Magnitude}_{z,1986+a-7} \quad (4)$$

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:

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

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:

$$\begin{aligned} \text{Intensity}_{z,1986} = & \sum_{p=6}^7 S_{z,a} \text{Magnitude}_{z,1987-a+7} + \text{Magnitude}_z^{Max} \cdot \sum_{p=8}^{12} S_{z,a} \\ & + [(10)F_{z,0}] \frac{1}{e^{8-7}} \sum_{a=6}^7 (S_{za} - S_{za,pre}) \end{aligned} \quad (6)$$

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:

$$\text{Intensity}_{z,1988} = \sum_{p=6}^{12} S_{z,a} \text{Magnitude}_z = \text{Magnitude}_z \sum_{p=6}^{12} S_{z,a} = \text{Magnitude}_z^{Max} \quad (7)$$

The last important fact that needs to be considered to calculate the intensity of the reform relates to the timing of implementation across each region of the country. Several regions 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 regions, and between 1979 and 1989 for later-implementing regions.

Figure 3 presents the evolution of the measure of intensity for the regions 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 regions 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:

$$YrsSchl_{mzy} = \theta_0 + \theta_1 Intensity_{zy} + \sum_{p=1}^3 \theta_2^p Age_{mzy}^p + \tau_y + \sigma_z + \nu_y Trend_y + \theta_3 x_{mzy} + \epsilon_{mzy} \quad (8)$$

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 ( $\tau_y$  and  $\sigma_z$ ) are added to capture any cohort and region characteristics.<sup>15</sup>  $Trend_y$  is a set of region-specific squared linear trends that captures secular changes over time within each zone of Ethiopia.<sup>16</sup> 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:

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<sup>15</sup>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.

<sup>16</sup>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.

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

Where  $Health_{mzy}$  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 mothers 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.

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.<sup>17</sup> 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.<sup>18</sup>

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

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<sup>17</sup>Alternatively, migration might have changed the distribution of the students across regions. 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.

<sup>18</sup>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.

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 regions. 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 containing 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.<sup>19</sup> As I describe in section 3.1, the instrument accounts for the set of pre-intervention enrollment probabilities.

### 4.2 Child health outcomes

The child health outcomes are selected because the combination provides a description of the overall health status. Following Keats (2018), I classify the outcomes into acute conditions and chronic conditions. Each group of outcomes has differing consequences

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<sup>19</sup>These data were later made available in the Integrated Public Use Microdata Series (IPUMS) International by the Minnesota Population Center.

for the child’s well-being and productivity. First, short-term wellbeing, measured by acute conditions, approximates daily health care provisions and the daily routine of the household. The set has also been directly linked to income disparities (Victora, Vaughan, Barros, Silva, & Tomasi, 2000). The set includes reports on diarrhea, coughing and fever in the two weeks prior to the survey, and the prevalence of wasting.

In contrast, the second set of conditions deals with malnutrition and anthropometric measures (stunting, body mass index, and height-for-age and weight-for-age z-scores). 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).<sup>20</sup> 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).<sup>21</sup>

### 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.

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<sup>20</sup>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.

<sup>21</sup>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).

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 0.102 years of schooling. Moreover, the F-statistic for the instrument is 13.90. In column 2, I add the health facilities variables. The coefficient is equal to 0.100 additional years of womens schooling (the F-statistic is 12.81).

To further show the effects of the policy, Figure 4 graphically demonstrates the average yearly coefficient for the first-stage estimates for mothers by year of birth and zone.

### 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).<sup>22</sup>

As shown in Table 3, the children of more educated mothers are 0.070 points (0.037 SE,  $p < 0.1$ ) less likely to be stunted.<sup>23</sup> 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 0.167 (0.085 SE,  $p < 0.5$ ) 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 0.022, 0.039, 0.041 and -0.014, respectively. Neither coefficient is statistically significant.

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.<sup>24</sup> Next,

<sup>22</sup>Table C.2 documents the general Ordinary Least Squares (OLS) estimates for the relationship between years of schooling and the outcomes.

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

<sup>24</sup>Across all estimates, the OLS model finds a negative and statistically significant relationship between

Table C.3 includes the reduced form estimation coefficients where the main independent variable is the measure of the intensity of the reform. Finally, Table C.4 contains the two stage least square estimation for the firstborn children.

## 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 intensity. In other words, it is important to test the difference-in-differences assumption. If regions 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 regions 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.

### 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 were directly affected by the UPE reform, also benefited from an increase in health care access. As shown in Figure 6, the national health expenditure rose significantly in 1995.<sup>25</sup> 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 region. I calculate the number of publicly provided health facilities (hospitals, health stations, health posts, and health centers) per thousand inhabitants.<sup>26</sup> 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

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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. Nevertheless, 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.

<sup>25</sup>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.

<sup>26</sup>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 0.022 (0.026 SE), 0.038 (0.026 SE), 0.038 (0.035 SE) and -0.011 (0.020 SE) but they are not statistically significant.

### 6.3 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.<sup>27</sup> 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:

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

where  $Z$  is equal to 1 if the mother was born after 1986 and zero otherwise.<sup>28</sup>

The results described in Table 5 are consistent with the findings in Table 3 for chronic conditions. 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

<sup>27</sup>The classic deterministic model where exposure to UPE is a deterministic and discontinuous function of birth cohort (Handa et al., 2016) will not work in the Ethiopian setting because grade repetition, long-term absenteeism, and late entry into school were frequent.

<sup>28</sup>In this model, the treatment effect is a local average treatment effect. The main coefficient captures the effect of schooling on the health outcome for those that would get an additional year of schooling of school fees were eliminated only if the fees school are eliminates (the compliers) measured at the cutoff. There are three important assumptions that 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 be 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.



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 Conclusions

This paper provides evidence that a universalization of primary school in Ethiopia not only led to an increase in women's education, but also had spillover effects that improved the long-term health of their children. Mothers who benefited from the reforms and, thus, attended more school as a result of the elimination of school fees, gave birth to children who were less likely to be stunted and have better height-for-age and weight-for-age Z-scores on average. The evidence associated with conditions that affect the short-run health of the child is mixed. Maternal education had an impact in the likelihood of reporting having had an episode of coughing or being wasted, but not on diarrhea or fever incidence.

The identification strategy proposed by Chicoine (2019) and Chicoine (2021) exploit 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 results are consistent with the neoclassical model of trade-off between the quantity and quality of children. It is also a consistent idea that education has such a great impact that has visible effects, even when teachers confront greater numbers of students in their classrooms, and when school construction fails to keep up with increasing enrollment.

This paper's main contribution is to add empirical evidence to the literature concerning the strong relation 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. The implementation of this type of policy has been common in Sub-Saharan Africa since the mid-70's and aimed to promote primary school enrollment drastically. Therefore, it is particularly important to assess not only short-run effects, but also possible spillovers into the next generation.

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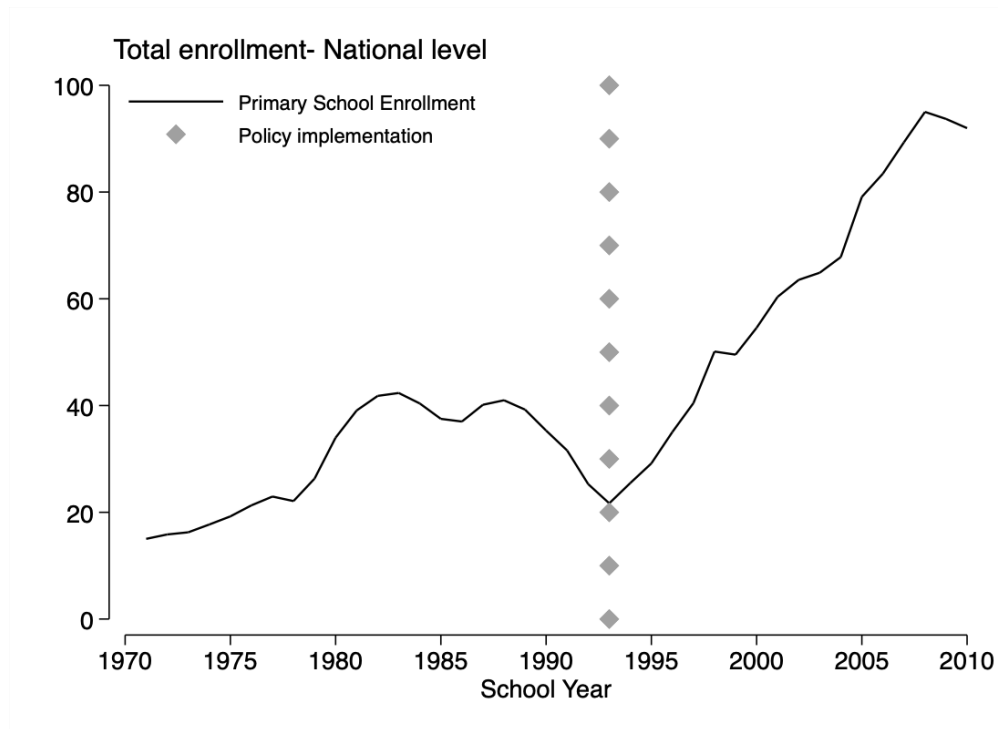
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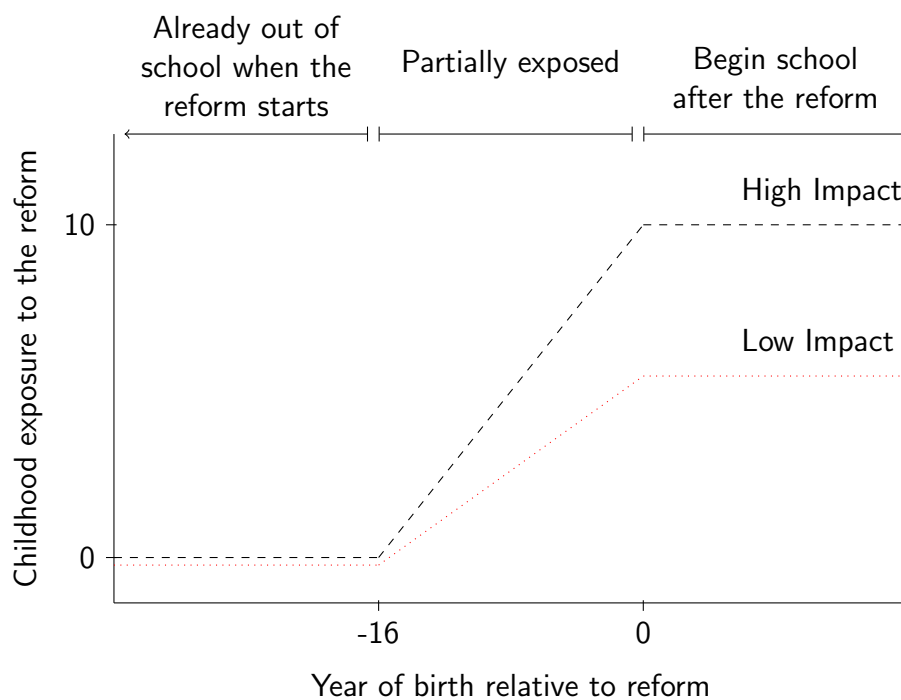
## A Graphs and Tables

Figure 1: Primary school enrollment - National level



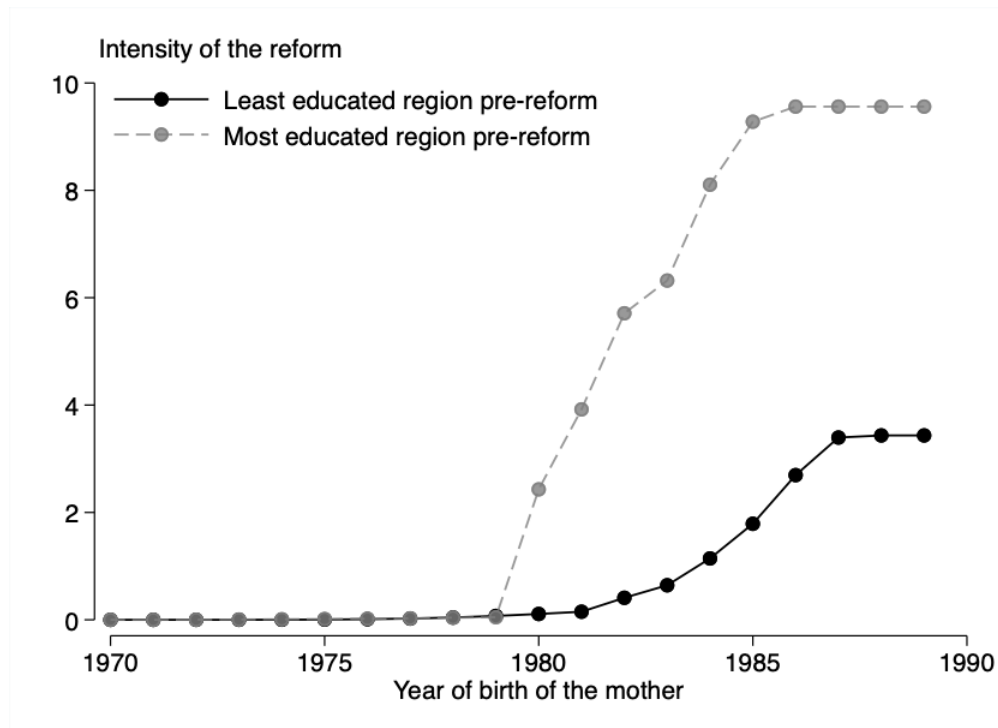
Source: World Development Indicators. Observations: 40

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.

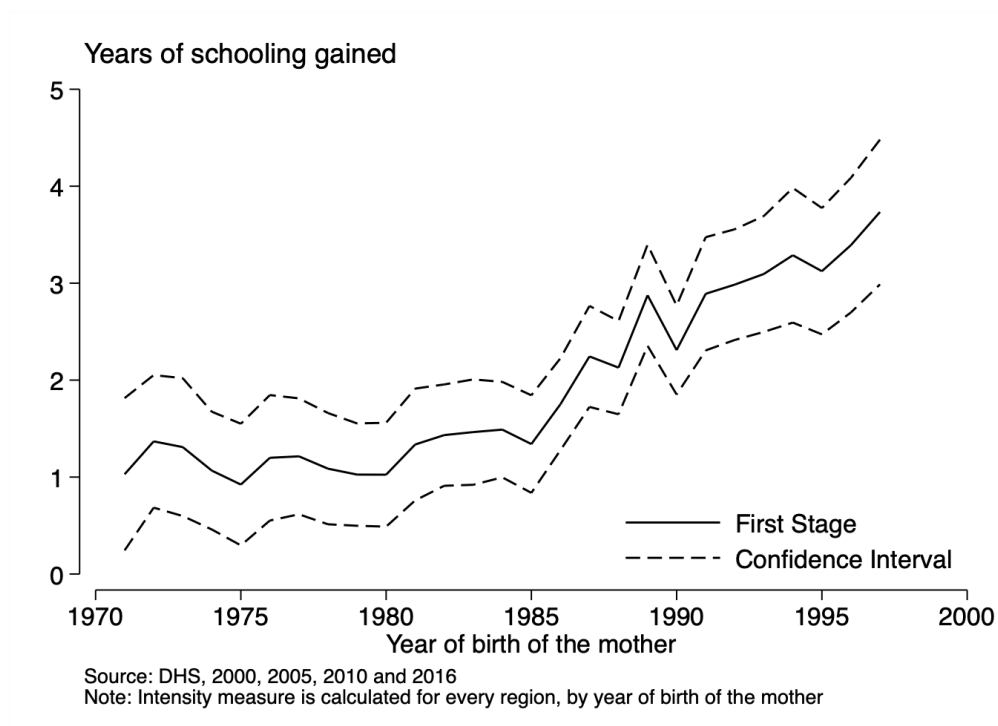
Figure 3: Intensity measure for highest and lowest regions, by birth year



Source: 1994 Ethiopian census data. Observations: 2,509,888

Notes: Intensity measure is calculated for every region, by year of birth of the mother

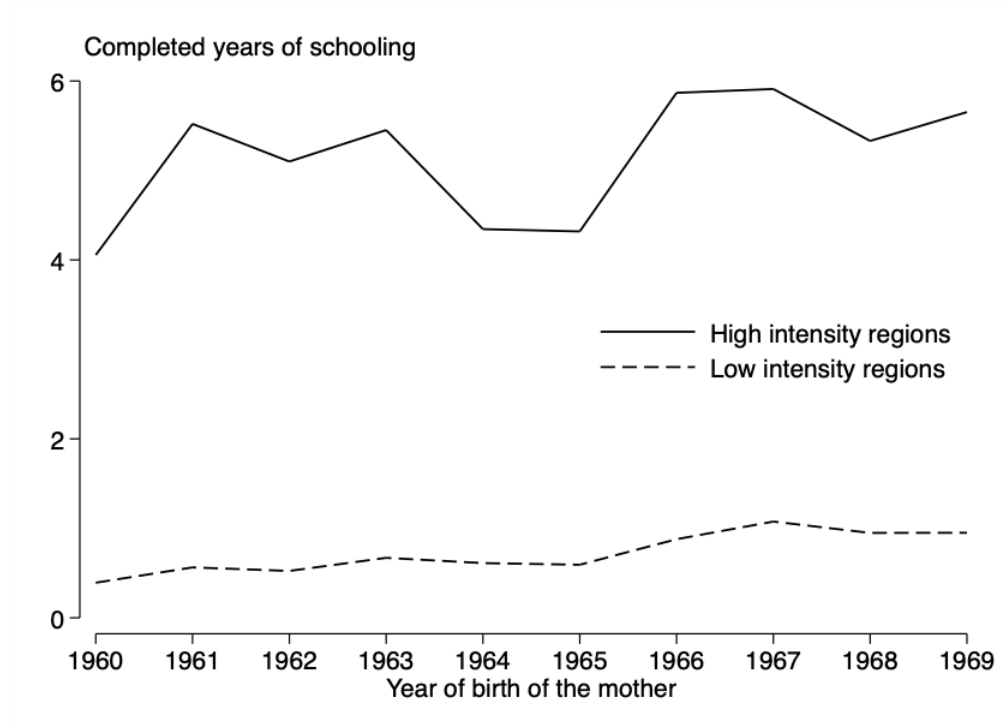
Figure 4: First stage estimates by year of birth of the mother



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

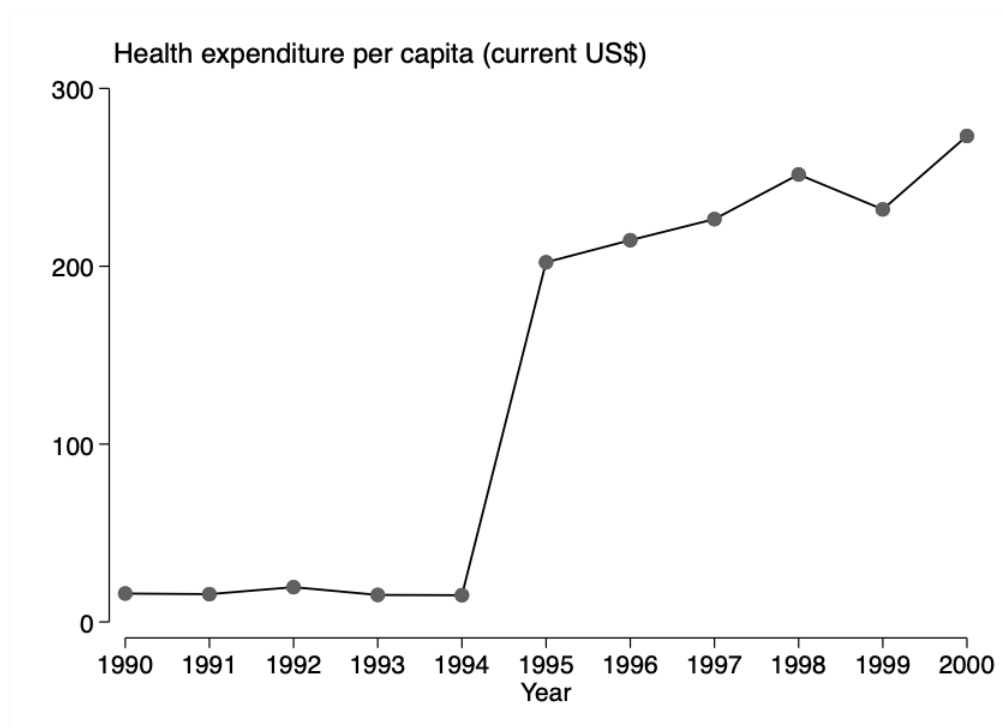


Figure 5: Pre-treatment trends of maternal schooling



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

Figure 6: Health Expenditure - National level



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

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 (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
<b>Mother level variables</b>						
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	
<b>All children sample</b>						
Age of the child	2.496	1.406	2.405	1.443	2.161	1.425
Male	0.856	0.351	0.837	0.369	0.808	0.394
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. Note: 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 childrens data.

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

	Years of Schooling	
	Intensity of the Reform	
	(1)	(2)
$Intensity_{zy}$	0.102*** (0.0274)	0.100*** (0.0287)
Observations	22,528	22,528
Adjusted R <sup>2</sup>	0.001	0.001
F- statistic	13.90	12.81
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.	R-Sq	N	Mean dep. var. Before intervention
	(1)	(2)	(3)	(4)	(5)
<b>Chronic conditions</b>					
Stunting	-0.070*	(0.037)	22,528	0.001	0.457
BMI Zscore	-0.053	(0.085)	22,528	0.027	-0.381
Height-for-Age Zscore	0.167**	(0.085)	22,528	0.154	-1.676
Weight-for-Age Zscore	0.075	(0.072)	22,528	0.149	-1.356
<b>Acute conditions</b>					
Diarrhea	0.022	(0.026)	22,528	0.020	0.200
Fever	0.039	(0.025)	22,528	-0.019	0.246
Cough	0.041	(0.036)	22,528	-0.026	0.255
Wasting	-0.014	(0.020)	22,528	0.064	0.319
First stage F-statistic	16.42				
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.  $\widehat{YrsSchl_{mry}}$  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 $\widehat{YrsSchl}_{mry}$	S.E.	Facilities per 1000 hab.	S.E.	R-sq	N	Mean dep. var.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Chronic conditions</b>							
Stunting	-0.066*	(0.035)	-0.666***	(0.219)	22,528	0.014	0.457
BMI Zscore	-0.051	(0.087)	-0.339	(0.780)	22,528	0.028	-0.381
Height-for-Age Zscore	0.150*	(0.080)	3.107***	(0.882)	22,528	0.164	-1.676
Weight-for-Age Zscore	0.065	(0.073)	1.681***	(0.524)	22,528	0.152	-1.356
<b>Acute conditions</b>							
Diarrhea	0.022	(0.026)	0.018	(0.160)	22,528	0.020	0.200
Fever	0.038	(0.026)	0.139	(0.255)	22,528	-0.016	0.246
Cough	0.038	(0.035)	0.476*	(0.260)	22,528	-0.016	0.255
Wasting	-0.011	(0.020)	-0.508***	(0.156)	22,528	0.066	0.319
First stage F-statistic	12.81						
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.  $\widehat{YrsSchl}_{mry}$  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	S.E.	N	Mean dep. var. Before intervention
	(1)	(2)	(3)	(4)
<b>Chronic conditions</b>				
Stunting	-0.202***	(0.000)	7,192	0.413
BMI Zscore	0.004***	(0.001)	7,192	-0.387
Height-for-Age Zscore	0.844***	(0.001)	7,192	-1.487
Weight-for-Age Zscore	0.453***	(0.001)	7,192	-1.239
<b>Acute conditions</b>				
Diarrhea	0.055***	(0.000)	7,192	0.173
Fever	-0.033***	(0.000)	7,192	0.215
Cough	-0.051***	(0.000)	7,192	0.225
Wasting	-0.096***	(0.000)	7,192	0.279
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.  $\overline{YrsSchl_{mry}}$  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 region 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:

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

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.



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

## 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
1977	Born			1978	Born			1979	Born			1980	Born		
1978	0			1979	0			1980	0			1981	0		
1979	1			1980	1			1981	1			1982	1		
1980	2			1981	2			1982	2			1983	2		
1981	3			1982	3			1983	3			1984	3		
1982	4			1983	4			1984	4			1985	4		
1983	5			1984	5			1985	5			1986	5		
1984	6			1985	6			1986	6			1987	6		
1985	7	G1		1986	7	G1		1987	7	G1		1988	7	G1	
1986	8	G2		1987	8	G2		1988	8	G2		1989	8	G2	
1987	9	G3		1988	9	G3		1989	9	G3		1990	9	G3	
1988	10	G4		1989	10	G4		1990	10	G4		1991	10	G4	
1989	11	G5		1990	11	G5		1991	11	G5		1992	11	G5	
1990	12	G6		1991	12	G6		1992	12	G6		1993	12	G6	
1991	13	G7		1992	13	G7		1993	13	G7		1994	13	G7	
1992	14	G8		1993	14	G8		1994	14	G8		1995	14	G8	
1993	15	G9		1994	15	G9		1995	15	G9		1996	15	G9	
1994	16	G10		1995	16	G10		1996	16	G10		1997	16	G10	
1995	17			1996	17			1997	17			1998	17		
1996	18			1997	18			1998	18			1999	18		
1997	19			1998	19			1999	19			2000	19		
1998	20			1999	20			2000	20			2001	20		
1999	21			2000	21			2001	21			2002	21		
2000	22			2001	22			2002	22			2003	22		
2001	23			2002	23			2003	23			2004	23		
2002	24			2003	24			2004	24			2005	24		
2003	25			2004	25			2005	25			2006	25		
2004	26			2005	26			2006	26			2007	26		
2005	27			2006	27			2007	27			2008	27		
2006	28			2007	28			2008	28			2009	28		
2007	29			2008	29			2009	29			2010	29		
2008	30			2009	30			2010	30			2011	30		
2009	31			2010	31			2011	31			2012	31		
2010	32			2011	32			2012	32			2013	32		
2011	33			2012	33			2013	33			2014	33		
2012	34			2013	34			2014	34			2015	34		
2013	35			2014	35			2015	35			2016	35		
2014	36			2015	36			2016	36			2017	36		
2015	37			2016	37			2017	37			2018	37		
2016	38			2017	38			2018	38			2019	38		
2017	39			2018	39			2019	39			2020	39		
2018	40			2019	40			2020	40			2021	40		
2019	41			2020	41			2021	41			2022	41		
2020	42			2021	42			2022	42			2023	42		
2021	43			2022	43			2023	43			2024	43		
2022	44			2023	44			2024	44			2025	44		
2023	45			2024	45			2025	45			2026	45		
2024	46			2025	46			2026	46			2027	46		
2025	47			2026	47			2027	47			2028	47		
2026	48			2027	48			2028	48			2029	48		
2027	49			2028	49			2029	49			2030	49		
2028	50			2029	50			2030	50			2031	50		
2029	51			2030	51			2031	51			2032	51		
2030	52			2031	52			2032	52			2033	52		
2031	53			2032	53			2033	53			2034	53		
2032	54			2033	54			2034	54			2035	54		
2033	55			2034	55			2035	55			2036	55		
2034	56			2035	56			2036	56			2037	56		
2035	57			2036	57			2037	57			2038	57		
2036	58			2037	58			2038	58			2039	58		
2037	59			2038	59			2039	59			2040	59		
2038	60			2039	60			2040	60			2041	60		
2039	61			2040	61			2041	61			2042	61		
2040	62			2041	62			2042	62			2043	62		
2041	63			2042	63			2043	63			2044	63		
2042	64			2043	64			2044	64			2045	64		
2043	65			2044	65			2045	65			2046	65		
2044	66			2045	66			2046	66			2047	66		
2045	67			2046	67			2047	67			2048	67		
2046	68			2047	68			2048	68			2049	68		
2047	69			2048	69			2049	69			2050	69		
2048	70			2049	70			2050	70			2051	70		
2049	71			2050	71			2051	71			2052	71		
2050	72			2051	72			2052	72			2053	72		
2051	73			2052	73			2053	73			2054	73		
2052	74			2053	74			2054	74			2055	74		
2053	75			2054	75			2055	75			2056	75		
2054	76			2055	76			2056	76			2057	76		
2055	77			2056	77			2057	77			2058	77		
2056	78			2057	78			2058	78			2059	78		
2057	79			2058	79			2059	79			2060	79		
2058	80			2059	80			2060	80			2061	80		
2059	81			2060	81			2061	81			2062	81		
2060	82			2061	82			2062	82			2063	82		
2061	83			2062	83			2063	83			2064	83		
2062	84			2063	84			2064	84			2065	84		
2063	85			2064	85			2065	85			2066	85		
2064	86			2065	86			2066	86			2067	86		
2065	87			2066	87			2067	87			2068	87		
2066	88			2067	88			2068	88			2069	88		
2067	89			2068	89			2069	89			2070	89		
2068	90			2069	90			2070	90			2071	90		
2069	91			2070	91			2071	91			2072	91		
2070	92			2071	92			2072	92			2073	92		
2071	93			2072	93			2073	93			2074	93		
2072	94			2073	94			2074	94			2075	94		
2073	95			2074	95			2075	95			2076	95		
2074	96			2075	96			2076	96			2077	96		
2075	97			2076	97			2077	97			2078	97		
2076	98			2077	98			2078	98			2079	98		
2077	99			2078	99			2079	99			2080	99		
2078	100			2079	100			2080	100			2081	100		
2079	101			2080	101			2081	101			2082	101		
2080	102			2081	102			2082	102			2083	102		
2081	103			2082	103			2083	103			2084	103		
2082	104			2083	104			2084	104			2085	104		
2083	105			2084	105			2085	105			2086	105		
2084	106			2085	106			2086	106			2087	106		
2085	107			2086	107			2087	107			2088	107		
2086	108			2087	108			2088	108			2089	108		
2087	109			2088	109			2089	109			2090	109		
2088	110			2089	110			2090	110			2091	110		
2089	111			2090	111			2091	111			2092	111		
2090	112			2091	112			2092	112			2093	112		
2091	113			2092	113			2093	113			2094	113		
2092	114			2093	114			2094	114			2095	114		
2093	115			2094	115			2095	115			2096	115		
2094	116			2095	116			2096	116			2097	116		
2095	117			2096	117			2097							

## Appendix C    Appendix Tables

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

Ordinary Least Squares Estimation				
	Coefficient (1)	S.E. (2)	N (3)	R-Squared (4)
<b>Chronic conditions</b>				
Stunting	-0.012***	(0.002)	22,528	0.109
BMI Z-score	0.026***	(0.003)	22,528	0.053
Height-for-age Z-score	0.053***	(0.005)	22,528	0.198
Weight-for-age Z-score	0.007	(0.008)	22,528	0.148
<b>Acute conditions</b>				
Diarrhea	-0.003**	(0.001)	22,528	0.055
Fever	-0.001	(0.001)	22,528	0.055
Cough	-0.002	(0.001)	22,528	0.053
Wasting	-0.010***	(0.001)	22,528	0.070

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)	R-Sq (3)	N (4)
<b>Chronic conditions</b>				
Stunting	-0.008***	(0.003)	22,528	0.105
BMI Z-score	-0.007	(0.008)	22,528	0.050
Height-for-age Z-score	0.018*	(0.009)	22,528	0.191
Weight-for-age Z-score	0.007	(0.008)	22,528	0.148
<b>Acute conditions</b>				
Diarrhea	0.002	(0.003)	22,528	0.054
Fever	0.004	(0.002)	22,528	0.055
Cough	0.004	(0.003)	22,528	0.053
Wasting	-0.002	(0.002)	22,528	0.066
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						
	Effect of Schooling				N	Mean dep. Var. Before intervention (6)
	2SLS		OLS			
	Coeff	S.E.	Coeff	S.E.		
	(1)	(2)	(3)	(4)		
<b>Chronic conditions</b>						
Stunting	-0.058*	(0.035)		15,446	0.059	0.457
BMI Z-score	-0.019	(0.084)		15,446	0.044	-0.381
Height-for-age Z-score	0.185**	(0.094)		15,446	0.181	-1.676
Weight-for-age Z-score	0.106	(0.083)		15,446	0.161	-1.356
<b>Acute conditions</b>						
Diarrhea	0.007	(0.027)		15,446	0.036	0.200
Fever	0.055*	(0.028)		15,446		0.246
Cough	0.041	(0.034)		15,446		0.255
Wasting	-0.017	(0.021)		15,446	0.081	0.319

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.  $\widehat{YrsSchl}_{mry}$  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. 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).