# Identifying National Level Education Reforms in Developing Settings: An Application to Ethiopia

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**Abstract**

In developing parts of the world, significant increases in primary school enrollment are often generated by large national level programs, which can simultaneously promote overcrowding and reductions in education quality. To analyze this trade-off one must first identify and evaluate the effect of the reform on schooling. This paper provides a method with which a reform's impact can be identified in developing settings using both temporal and geographic variation, and readily available data. The method is applied to an early 1990s reform in Ethiopia based around the release of the Education and Training Policy, which removed schooling fees from grades one to ten. The model finds that the reform generated an increase in schooling of 1.2 years. Further evidence demonstrates the additional enrollment also led to a higher rate of literacy, suggesting an increase in learning.

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# 1 Introduction

According to the World Bank's World Development Indicators, only 60 percent of primary school age children in Sub-Saharan Africa attended school in the year 2000; a ten percentage point improvement relative to 1990. This was not a problem that went unnoticed. In the 1990's there were three major national initiatives to provide free primary education (FPE) in Ethiopia, Malawi, and Uganda (Deininger, 2003). Following the adoption of the second Millennium Development Goal of universal primary education, and at least 15 countries additional implemented their own FPE reforms since the turn of the millennium. The goal of these efforts is to boost enrollment in primary school, and in the year following the removal of primary school fees grade one enrollment increased an average of 25 percent over the previous year (UNESCO Institute for Statistics). Accompanying this increase in enrollment was a 10 percent increase in pupil-teacher ratios, which policy makers worry may lead to a reduction in the quality of education following FPE implementation (Hillman and Jenkner, 2004; Tiongson, 2005). This increase in class size has the potential to exacerbate the dichotomy between schooling, and the worrying lack of education and learning in many parts of the world (Pritchett, 2001; Pritchett et al., 2013; Watkins, 2013). However, a recent systematic review by the International Initiative for Impact Evaluation (3ie - Snilstveit et al. 2016) concluded that there is currently insufficient evidence to form a conclusion regarding how the removal of school fees may impact school attendance and learning. This paper aims to help fill this void by proposing a novel method to identify the effect of national level reforms using data that are contained in most household datasets. This technique is then applied to an education reform in Ethiopia from the early 1990s; the effect of the reform on both schooling and education is estimated by using years of schooling, literacy, and the completion of schooling beyond the post-grade eight exam.

Building on the conceptual framework of Lucas and Mbiti (2012a,b), this paper focuses on the construction of an identification strategy that can quantify the potential magnitude of national level reforms at any level of education using both geographic and temporal variation. Applying this method, the effect of the removal of school fees in Ethiopia is estimated using three different post-reform data sources: the 2007 round of the census, 2005 and 2011 rounds of the Demographic and Health Survey (DHS), and the 2014 Living Standards and Measurement Study (LSMS). It is shown that the reform led to an increase of between 0.71 and 1.4 years of schooling. The reform also led to an increase in literacy of 15 percentage points, a 50 percent increase relative to baseline levels, and an increase in the number of students continuing beyond the post-grade eight exam. This is suggestive evidence that the increased level of schooling did lead to increased learning, outweighing any potential quality declines. Placebo reform measures are also constructed using data from four countries (Kenya, Tanzania, Zambia, and Mali) that did not remove school fees in the 1990s.

The estimates for all four countries yield no evidence of large increases in schooling during the same time period. These results help to rule out that the estimates from Ethiopia are capturing any type of secular trend in education.

In developed settings, identification strategies are generally more straightforward. The literature often utilizes a dichotomous reform indicator variable denoting one cohort as pre-reform and the next cohort post-reform, such as studies in Norway (Black et al., 2005, 2008; Monstad et al., 2008), Italy (Fort, 2012), the U.K. (Geruso et al., 2014), Sweden (Holmlund et al., 2011; Lundborg et al., 2014), and the U.S. using the exact day of birth (McCrary and Royer, 2011). However, due to issues such as late school entry and high repetition rates in developing countries, entire birth cohorts do not enter and progress through school in such a regimented fashion, generating an effect that gradually builds across a number of cohorts. This non-discrete implementation makes it is more difficult to find and exploit variation using a dichotomous pre- and post-reform technique in developing settings, although Agüero and Bharadwaj (2014) and Grépin and Bharadwaj (2015) showed that it is possible in certain cases. The more common approach has been to exploit two separate dimensions of variation, the timing of the reform and geographic variation in the implementation. For example, Duflo (2001, 2004) used variation in the construction of primary schools to examine the relationship between education and wages in Indonesia, and Osili and Long (2008) exploited similar variation in Nigeria to examine how education impacts fertility. The general policy change in an FPE reform is to remove compulsory school fees in all primary schools across the country (the extent of fee removal varies from reform to reform); therefore, there is no geographic variation in the implementation of the reform itself. Previous work examining the impact of malaria eradication exploited pre-intervention levels of the disease to generate variation in the expected impact of the anti-malaria programs (Bleakley, 2010; Lucas, 2010, 2013). The benefit to this type of strategy is that it does not depend on variation in the implementation, but the geographic component of the variation is determined solely on the pre-intervention characteristics of each geographic district.

Lucas and Mbiti (2012a,b) applied a similar strategy using variations in pre-reform grade completion rates to generate geographic variation. They examined the effect of the 2003 FPE reform in Kenya on gender differences in schooling and sorting into private schools in 2007. This analysis identified the effect of the reform's intensity by using the concept that Kenyan districts with more students dropping out prior to taking their post-grade eight exam have more room for improvement following the removal of school fees. However, because of the short-term nature of the study, all students made their school entry decision prior to the reform's implementation. This paper adopts the central concept of this approach, that the removal of school fees is expected to have a greater impact on areas that have relatively lower initial levels of schooling, and extends it to account for the impact of the removal of school fees on the school entry decision. This method is described in a general way that can be applied to any setting with pre-reform information on the fraction of the population completing each year of schooling and starting at each age at some sub-national level, and because it captures the entirety of the reform, it can be used to study long-term consequences of the reform.

In this paper, the identification strategy is used to evaluate an education reform in Ethiopia, the removal of school fees in grades one through ten. Data from the 1994 Ethiopian census are used to estimate the level of expected impact of the reform separately for each of the 60 zones (i.e. counties) across Ethiopia. To generate both geographic and temporal variation, these zone-specific measures are then interacted with the timing of the reform and zone-specific data on school starting age to calculate the maximum intensity with which the reform could have affected an individual born in a specific year in a specific zone. This can then be matched with the birth year (or age) and the location of respondents from a post-reform survey, and the full effect of the national education reform estimated. In Ethiopia, the reform is found to have led to a significant increase in schooling. The estimates are not sensitive to changes in the sample, or alternative assumptions in the calculation of the intensity measure. Although precise zone of birth is not available, three alternative strategies using region (i.e. state or province) of birth information in the LSMS find similar estimates to the baseline model. Furthermore, 82 percent of respondents in the LSMS sample live in their region of birth. Importantly, this method only finds an increase in schooling in Ethiopia, and does not capture secular increases in education in four alternative countries from Sub-Saharan Africa.

The relevant history of Ethiopia and the education reform is outlined in Section 2. The estimation of the potential magnitude of the reform's impact, and the combination of this measure with the timing of the reform's implementation is described in Section 3. The data are summarized in Section 4, along with the paper's estimating model. The effect of the reform on years of schooling in Ethiopia is explored in Section 5. The paper is concluded in Section 6.

## 2 Background and Education Reform

Beginning in 1974 Ethiopia was governed by a military council, a period that was briefly followed by four years of a single party communist rule, which ended in 1991 (Ofcansky and Berry, 1993). Under these governments, students entered school at age seven, a starting age that remains unchanged today, primary school lasted six years, junior secondary school two years, and senior secondary four years (World Bank, 2009). Grade 1 enrollment declined roughly 25 percent between 1979 (first year of available data) and the fall of the communist government, and the fraction of the national budget spent on education fell nearly 50 percent (Oumer, 2009). During this time period, there was no set tuition fee charged for school attendance;

however, public primary and secondary schools often imposed per-student fees to cover the cost of attendance. Fees were significantly lower in government run schools, and these schools educated more than 90 percent of primary school students (World Bank, 2009).[1]

In 1991, the Ethiopian Peoples Revolutionary Democratic Front took power and moved quickly to establish a transitional government and decentralize authority. The transitional government established nine regional governments and two independent administrative councils in the country's two largest cities. These 11 regions were largely set up along historical ethnic lines, elected their administrations in the summer of 1992, and each established an independent Education Bureau. The decentralization of education policy in Ethiopia occurred through two main declarations. The first was Proclamation No. 41 of 1993. This proclamation clarified the division of power between central and regional authorities, and regional authorities became responsible for the administration and provision of primary and secondary education. The proclamation functioned as the "cornerstone" for the education policy that was being drafted, and acted as the de facto initial implementation of the forthcoming policy. The official Education and Training Policy, the second major education declaration, was eventually published in 1994, and was officially forwarded to the regional governments prior to the 1995 school year. The key aspect of this proposal was that it required that education be fee-free for grades one through ten. The decentralization of power and lack of official fees in place slowed the implementation of this policy in some areas, but by 1996 a majority of the country had complied with the decree (Negash, 1996; UNESCO, 2007; Oumer, 2009; World Bank, 2009).[2] This reform is unlikely to have made school "free" for all students, in fact, the government acknowledged that community support remained an important part of financing education, but schools were no longer allowed to request a per-student fee (Oumer, 2009; World Bank, 2009). Falling short of fully removing the cost of attending school only makes finding an impact of the policies more difficult to identify; therefore, what is directly being measured is the effect of the intention of the government to remove school fees. Additionally, the Education and Training Policy extended the length of primary school to eight years, with an additional two years in both junior and senior secondary school. Importantly, there is a cutoff after eight years of schooling in both the pre- and post-reform periods; this will be exploited later in the paper.

Administrative enrollment data from the UNESCO Institute for Statistics can be used to determine whether there is initial evidence that the two reforms issued by Ethiopia's central government impacted school attendance. Grade one enrollments are likely to be the most responsive to the fee reductions; it is at this margin there are the largest number of students who could potentially be affected by the reform. The

[1] All statistics are from the World Bank's World Development Indicators, and the UNESCO Institute for Statistics.

[2] Proclamation No. 41 also enabled regions to teach in local languages. Zenebe Gebre (2014) finds that the introduction of mother tongue education actually had a significant negative impact on enrollment, negative 6 percentage points, and schooling, a reduction of 0.4 years of school. This downward pressure on schooling should only make it more difficult to find a positive impact of fee-free schooling.

initial impact of Proclamation No. 41 should first be seen in the 1993 school year, and the impact of the Education and Training Policy in the 1995 academic year. Grade one enrollments are shown in Figure 1 from 1979, the first year data are available, to 1999.[3] In the 14 years leading up to 1993, and Proclamation No. 41, grade one enrollment was relatively flat. The number of primary school entrants had actually declined by nearly 20 percent from 1979 levels, and roughly 40 percent from peak levels. The fall in the late 1980s coincides with the conflict that eventually led to the overthrow of the military government.[4] The 1993 and 1995 school years represent the two largest increases in grade one enrollment both in terms of the number of additional students entering school and in percentage growth. The introduction of Proclamation No. 41 is associated with more than 280,000 additional students entering grade one relative to the previous year, a 45 percent increase. Then in 1995, grade one enrollment grew another 317,000 students following the dissemination of the Education and Training Policy, a year-to-year growth of 28 percent. These are the largest two increases in annual enrollment. Between 1992 and 1995 pupil-teacher ratios in primary school increased 40 percent, the increase was more than 60 percent from 1992 to 1996, the year when most of the country had fully implemented fee-free education. The growth in the number of students per school is even greater, growing over 75 percent between 1992 and 1995, and 90 percent through 1996 (Ministry of Education, 1995, 1996, 2000). This is evidence that the increase in enrollment is not being generated by an increase in the supply of education, but an increase in demand driven by the reduced out-of-pocket cost of school. These trends are common following this type of national reform, and often pointed out by critics of these large-scale education reforms. This suggests that Ethiopia conforms to the pattern that is often observed after a country implements a "big-bang" education expansion; large-scale growth in access to education, and an expansion of enrollment that typically outpaces the supply of teachers and classrooms. This is evidence of both the potential effectiveness of these policies, and that Ethiopia is an appropriate context in which to demonstrate a technique to identify and measure the impact of the reform on schooling.

# 3 Methodology

## 3.1 Generalized Calculation of Zone Specific Magnitude and Timing

Quantifying the potential magnitude of the reform is essential to estimating the predicted impact, and doing this separately for each zone in Ethiopia generates the necessary geographic variation. To allow for future

[3]Ethiopia uses their own calendar that begins its new year, and new academic year, around the second week of the Gregorian (Western) September. The numerical Ethiopian year is either seven or eight months behind the Gregorian year, depending on the month. The year referenced in the text is the Gregorian year in which the academic year began. For example, 1993 references the 1993 to 1994 academic year that began in September of 1993.

[4]Note that the timing of reform implementation that will be identified in this paper occurs differentially across the country between two and five years following the end of the armed conflict, suggesting that the peace that saw the Ethiopian Peoples Revolutionary Democratic Front take power is an unlikely explanation for the paper's findings.

cross-country comparisons, the potential magnitude of the reform is calculated in reference to the pre-reform duration of primary school, six years. Motivated by work from Lucas and Mbiti (2012a,b), the underlying concept is that the magnitude of the reform's impact is inversely related to the pre-reform level of schooling. In each zone, $z$, some fraction of students never enter school ($F_{z,0}$), and removing school fees for grades one through ten has a maximum potential impact of ten years of additional schooling for these students. The maximum effect in zone $z$, for this group of students, is then equal to the product of the fraction of students in the zone who never attended school and the maximum potential impact of the reform ($10 \cdot F_{z,0}$). In this district there also exists some fraction of the population that would have dropped out after completing the first grade ($F_{z,1}$), these students received a maximum nine years of additional schooling following the removal of school fees ($9 \cdot F_{z,1}$). The same is true for students who would have dropped out after each successive year, following grades two through nine. Students completing at least ten years of school are assumed to be unaffected by the removal of school fees in grades one through ten. Therefore, to capture the magnitude of the maximum potential effect of the reform in zone $z$, $M_z(G)$, the impact must be summed across grade levels zero through nine,

$$M_z(G) = \frac{1}{6}\sum_{g=G}^{9}(10-g)\cdot F_{z,g}. \tag{1}$$

The equation is divided by six to scale the maximum impact of the reform in reference to the original length of primary school, and set $G = 0$ to capture the impact of the reform across all grades. As previously stated $F_{z,g}$ is the fraction of the population in zone $z$ that left school following grade $g$, and $(10-g)$ is equal to the number of additional years of schooling they could complete after the removal of school fees. The maximum value of the potential magnitude would occur in a zone where no student attended school prior to the reform ($F_{z,0} = 1;\ F_{z,g} = 0\ \forall\ g \in [1,9]$); every student would receive ten additional years of schooling. The maximum potential magnitude of the reform is then 10/6. If every student completed at least ten years ($F_{z,g} = 0\ \forall\ g \in [0,9]$), then the predicted impact of the reform would be zero.

For individuals entering school following the reform's implementation, the reform impacts their schooling decision beginning with whether or not they should enter school at all. They are affected by the full magnitude of the reform, and the $G$ parameter from equation (1) is equal to zero ($G = 0$). However, for students who had completed four years of school when the reform was put into place, the removal of school fees would not affect their decision to attended grades one through four, but will impact their decision to drop out after grade four and each subsequent level. For these students, the impact of the reform begins following the fourth grade, and equation (1) is calculated with $G = 4$.[5] How the predicted magnitude is

[5]The magnitude equation with $G = 4$: $M_z(4) = \frac{1}{6}\sum_{g=4}^{9}(10-g)\cdot F_{z,g}$

assigned to observations from the data is determined by the timing of the reform, and by the respondent's birth year. If every student started on time, at age 7, it would be straightforward to assign each observation the appropriate magnitude equation. However, the data from Ethiopia, like much of the developing world, show that this is not the case; delayed entry is common.

Zone specific starting age probabilities are calculated for ages 6 to 12. This takes into account the possibility that students enters grade one anytime from one year early, at age six, to five years late, at age 12.[6] An intensity measure, $I_{zy}$, is used to appropriately assign the magnitude across birth cohorts. An individual born in 1972 or later, even starting school at age 12, will progress through tenth-grade prior to the reform being put into place. Therefore, individuals born in 1972 or earlier will not be affected by the removal of school fees. On the other extreme, the reform will be implemented prior to individuals born in 1988 entering school, even if they start a year early. All individuals born in 1988 or later are fully impacted by the reform, corresponding to an $M_z(0)$ magnitude. This distribution can be seen in the following equation,

$$I_{zy} = \begin{cases} 0 & \text{if } y \leq 1972 \\ \sum_{a=6}^{12} S_{zya} \cdot M_z\left(G_{y+a-7}\right) & \text{if } 1973 \leq y \leq 1987 \\ M_z\left(0\right) & \text{if } y \geq 1988 \end{cases} . \tag{2}$$

The intensity of the reform for individuals from zone $z$, born in year $y$, is denoted $I_{zy}$. As noted above, this is zero for all individuals born in 1972 and before, and the maximum zone specific magnitude, $M_z(0)$, for individuals born in 1988 and later. $S_{zya}$ is the probability of starting school at age $a$, for individuals from zone $z$, and born in year $y$. The intensity of the reform's impact for individuals born between 1973 and 1988 is the sum of a set of seven zone-specific magnitudes weighted by starting age probabilities. To calculate starting age probabilities, data are used to estimate the fraction of grade one entrants at each age, 6 to 12. Assuming a constant relative relationship across ages, two starting probabilities are calculated. Pre-reform starting probabilities are calibrated to sum to one minus the fraction of students never entering school in each zone, these are assigned to birth year and starting age combinations that would have had to make the decision to enter school prior to the removal of fees. Post-reform starting probabilities are calibrated to sum to one, the maximum impact of every student entering school. The combination of starting age and birth year determines the magnitude equation with which the starting age probability is paired. The starting age probability at age seven for any given birth year $y$ is always paired with the magnitude equation for the same birth year, $S_{zy,7} \cdot M_z(G_y)$. However, the starting age nine probability is then paired with the magnitude

[6]Due to high rates of students never entering school in the pre-reform period, the largest margin along which the reform affects schooling is entry into grade one. Although grade repetition occurs, data are not available at the zone level, and the magnitude of its impact on the distribution of the reform's timing is likely to be modest relative to school entry.

equation for the birth cohort born two years later, $S_{zy,9} \cdot M_z(G_{y+2})$. These products are calculated for each age, 6 to 12, and then summed together to construct the intensity measure for each birth year from 1973 to 1987.[7] Finally, the intensity measure is also calculated separately for men and women to allow for sex specific estimates of the reform's impact on schooling.

### 3.2 Ethiopian Specific Regional Timing

As discussed in Section 2, the implementation of the reform is staggered across Proclamation No. 41 (1993), the Education and Training Policy (1995), and possible delayed implementation (1996) due to the decentralized government structure put into place by the transitional government (World Bank, 2009). This consideration is specific to the Ethiopian reform policy. An objective method is constructed to identify the timing of the reform's implementation in each of the country's 11 regions. Pre-reform (1989 to 1992) regional level enrollment data are used to construct predicted levels of grade one enrollment for the next four years (Ministry of Education, 1995, 1996, 2000). For each region, the annual grade one enrollment level is then compared to the predicted level of enrollment; the post-reform period is set to begin in the year with the largest increase of this relative measure.[8] To ensure that the estimates seen in the following section are not dependent on this definition of implementation, additional estimates are shown using a staggered implementation of the reform. The estimates that allow for gradual implementation use the same distance above the predicted enrollment level for the years 1993 to 1996, and assume no implementation prior to 1993 and full implementation no later than 1996. With these adjustments, the latest entirely pre-reform cohort is the 1970 birth cohort, and the earliest completely post-reform cohort is 1989.

The reform intensity measure, the outcome of the calculations from Section 3.1, is plotted in Figure 2. Each point denotes the average value within each birth cohort, assuming complete adoption at the time of implementation and the evolution of the reform intensity is plotted separately for regions with 1993 and 1995 implementations, as determined by the method described above. Across both groups, the initial changes in the instrument occur slowly for birth cohorts from 1970 to the 1979. In the construction of the intensity measure, all school entrants for these birth cohorts are assumed to be making their school entry decision prior to the reform; therefore, the impact of the reform is entirely dependent on keeping would-be dropouts in school longer. In the reform intensity model, the first cohort possibly making their entry decision following the 1993 implementation of the reform is the 1980 cohort, and the 1982 cohort in the 1995 regions. The reform intensity calculation suggests that we would expect for the impact of the reform to occur gradually across

[7] For further detail, a table listing the affected grade levels by birth year, additional details regarding starting age calculations, and the explicit equation used to calculate the intensity measure for each birth year are included in the appendix.

[8] For the eleven regions the reforms are set as follows: Tigray 1993, Afar 1993, Amhara 1995, Oromiya 1995, Somalia 1993, Benishangul Gumuz 1993, Southern Nations, Nationalities, and Peoples' Region 1995, Gambella 1993, Harari 1995, Addis Ababa 1995, and Dire Dawa 1996.

cohorts, not in a discontinuous fashion. Furthermore, the post-implementation period yields a relatively constant return to the reform, once all students enter school in the post-reform period.

Plotting the reform intensity separately by implementation date also illustrates predicted differences in how the reform's effect will evolve in each of the two sets of regions. First, as seen in Figure 2, 1993 implementation regions increase prior to the 1995 adopting regions, beginning in 1980. Furthermore, the increase in the early adopting regions completely occurs between 1980 and 1985, while the late adopting growth is roughly evenly split on either side of 1985. *Second*, the increase in schooling in the early adopting regions should be more rapid, and should also have a larger overall increase once the reforms are fully in place. The *third* prediction can be seen in the post-implementation flattening of the reform intensity for the five cohorts between 1985 to 1989 in the early adopting regions, and the three separated but still increasing cohorts in the regions that adopted the reform two years later, in 1995. These three predictions are compared to the changes in schooling as the data are described in the following section. The congruence of these predictions with the pattern seen in the schooling data, which are not affected by regional timing in any way, provides support for the adjustments made to account for the timing of reform's implementation.

# 4 Data and Estimation

## 4.1 Data

This study uses individual-level data from three main sources. Ethiopian census data for the years 1994 and 2007 include information on 5 million and 7.4 million Ethiopians, respectively. These data were collected by the Ethiopian Central Statistical Agency, and made available as part of the Integrated Public Use Microdata Series (IPUMS) International by the Minnesota Population Center. Data from the 1994 census are used to construct baseline education levels and starting age probabilities in each zone. Data from the 2007 census are used to estimate the paper's main education results, and are supplemented with data from the 2005 and 2011 rounds of the Demographic and Health Survey (DHS), and the 2014 Living Standards and Measurement Study (LSMS). Each of these datasets includes information that enables household location to be determined at the second administrative level of Ethiopia, the zone. Census data include time consistent definitions of household location within each zone; household zones using the DHS and LSMS surveys are determined by mapping survey cluster GPS location onto maps of zone-level boundaries.[9] This information is further cross-referenced with recorded cluster location in the LSMS; zones are not explicitly recorded in the Ethiopian DHS. Each dataset also includes information on individual's years of schooling, sex, and age; this is the only

[9]Matching to map boundaries is done with two sources. Zone level boundaries are obtained from IPUMS International (2015) and the Food and Agriculture Organization GeoNetwork's Global Administrative Unit Layers (GAUL) maps (2016).

information needed to identify and estimate the impact of the reform.

Each of the datasets has a unique set of strengths, and testing that the effect of the education reform can be identified across all of the surveys provides important evidence that the model is capturing a change that occurred in Ethiopia. Although the 2007 census only collects a small amount of information on each individual, it collects the information necessary to measure the impact of the reform on schooling, and includes thousands of observations in every zone. The census and LSMS do not explicitly ask for birth year, it is inferred using the respondent's age and the year of the survey. The DHS explicitly records birth year, cross checks this information with other dates and ages in the survey to correct for errors, and records the depth of the respondent's knowledge of their birth year. Only 73 percent of respondents are able to report their birth year; it is imputed using age information for the remaining respondents. Due to the importance of the timing of when an individual was born to this study, it is extremely beneficial to have a subsample of respondents with information that has a higher likelihood of being accurate. In the census and DHS, the place of one's childhood education is not explicitly known, but region of birth data in the LSMS suggest that today's location is likely a good approximation. Data from the LSMS demonstrate that 82 percent of respondents born between 1970 and 1989 still live in their region of birth. This information is used to show that estimates for the full sample are similar to those for respondents who remain in the region in which they were born. Furthermore, this information is used to adjust for migration in an alternative construction of the identification variable, again yielding similar estimates.

### 4.2 Summary Statistics

Data from all three equally weighted sources (Census, DHS, and LSMS) are used to calculate relevant summary statistics, presented in Table 1. Data are shown for the 1970 birth cohort, the last entirely pre-reform cohort, and the 1989 birth cohort, the first fully post-reform cohort. These cohorts bookend the range included in the baseline set of estimates in the following sections. The average intensity measure is shown in the first row, for the full sample on the left-hand side of the table, and then separately for men and women. The intensity measure is always zero for the pre-reform 1970 cohort, and larger for women in the 1989 cohort. As can be seen in the second row, education levels in the pre-reform period are exceedingly low. From an average of just over one year of schooling for women born in 1970, to more than two and a half years of schooling for men born in the same year. These low levels of schooling lead to a large potential impact of the reform, and average full intensity estimates of 1.47 and 1.23 for women and men, respectively. Over this time period, education increased for women and men both, an increase of 2.33 years for women, and 1.65 years for men. Because average education rates are so low in the pre-reform period, increasing average years of schooling in this range could have a significant effect on literacy rates. An initial examination of the data reinforces this understanding. Early literacy rates were much lower for women in the earlier cohorts (0.15 vs. 0.43), but more than doubled during this time period. The increase in literacy for men was not as large, but the literacy rate still increased by 51 percent. In addition to a basic measure of learning, such as the literacy rate, there is consistent growth across the entire sample for the fraction of individuals completing eighth grade (12 percentage points), as well as an increase in the completion of ninth grade (nine percentage points).

The same data are used to plot the years of schooling variable across cohorts. This is done separately for 1993 and 1995 implementing regions, and the output is shown in Figure 3. The data are first displayed in levels, using the average of each birth cohort from 1971 to 1989, in Figure 3a, and growth relative to 1979 is shown in Figure 3b. After a period of stagnation, the schooling data in the early implementation areas begins a steady increase in 1980, as predicted in Figure 2. This increase is slightly larger than the increase in the 1995 implementation regions, and much more sustained. The early increase in the 1995 regions could be suggestive of some adoption in these regions that coincides with Proclamation No.41 in 1993, a robustness test allowing for this type of staggered implementation is show in Table 3. As seen in the reform intensity figure, the early adopting region's growth in schooling is entirely between 1980 and 1985. The late adopting region's growth is roughly divided on either side of 1985; a 24 percent increase between 1979 and 1984, and a total increase of 56 percent through 1989.

Evidence of the second prediction is visible in Figure 3b. Growth in the early implementing regions accelerates after 1980, and remains larger through the end of the sample period. . Finally, the plateauing of growth in schooling in the 1993 regions matches the prediction from the reform intensity calculation. The five post-reform cohorts, beginning in 1985, have a relatively similar level of schooling. For the 1995 adopting regions, the post-reform pattern again matches the intensity prediction. There is a jump prior to the final three cohorts, and then an increase in schooling across the last three cohorts. Similar to the reform intensity prediction, the growth in these late adopting regions falls just short of the growth in the early adopting areas. Although not exact, the variation in schooling across these two groups matches well with the pattern predicted by the reform intensity calculation. This includes the timing and relative growth in the final post-reform cohorts.

### 4.3 Estimating Equation

The central model of this paper examines the relationship between the estimated intensity measure from equation (2), and years of schooling. The model exploits both the geographic and temporal variation built into the construction of the intensity measure to identify gains in schooling from a national level reform in a developing setting. The estimates used in this paper are from an ordinary least squares model that is defined by the equation

$$Y_{izy} = \alpha + I_{zy}\beta + \tau_y + \delta_z + \delta_z Trend_y + X_{izy}\theta + \epsilon_{izy}. \tag{3}$$

The dependent variable is $Y$, generally years of schooling, for individual $i$, born in year $y$, from zone $z$. $\beta$ is the coefficient of interest, and $I_{zy}$ is the zone and birth year specific estimated intensity of the reform which is calculated by exploiting geographic variation in the zone-specific pre-reform education levels, and the timing of the reform. $I_{zy}$ is scaled to equal one when six years of school, the length of pre-reform primary school, are provided free of fees; therefore, $\beta$ can be interpreted as the increase in years of schooling generated by providing free primary education. $\tau_y$ is a set of birth year fixed effects that capture any secular changes occurring across Ethiopia in any given year, and $\delta_z$ is a set of zone-specific fixed effects capturing any time invariant characteristics that impact schooling, and is a set of zone-specific linear trends that controls for secular changes over time, separately for each zone in Ethiopia.[10] $X_{izy}$ is a vector of individual level covariates that include a cubic for age when multiple survey rounds are part of the sample, a dummy for sex when both men and women are included in the sample, or region of birth when examining the potential impact of migration in Table 4. It is important that these covariates are either constant over an individual's lifetime or are determined prior to schooling decisions being made. Estimates are weighted using sampling weights provided by the surveys, and standard errors are clustered by zone to allow for within zone correlation (Bertrand et al., 2004).

# 5 Results

## 5.1 Baseline Estimates

Baseline estimates of the effect of the education reform on years of schooling in Ethiopia are shown in Table 2. All nine samples include birth cohorts from 1970 to 1989, the narrowest possible band of birth cohorts that fully captures the reform. The reform intensity is constructed using both male and female observations to estimate the results in Panel A, and using only the respective sex specific data for the estimates in Panel B and Panel C. Using three separate sources of data, all nine estimates yield positive point estimates, seven are statistically significant at the 95 percent confidence level. Estimates for the full sample are shown in Panel A; the model estimates that fee-free primary school led to an increase of between 0.71 and 1.4 years of school; the average estimate across the three datasets is 1.2. Estimating the model separately for both men

[10] The set of fixed effects and linear trends are similar to the estimation strategy used by Black et al. (2005); Bleakley (2010); Lucas and Mbiti (2012a,b); Fort et al. (2016); Holmlund et al. (2011); Lundborg et al. (2014); Monstad et al. (2008).

(Panel B) and women (Panel C) again yields evidence of an increase in schooling generated by the reform in Ethiopia. On average the estimates for men are larger (1.4 vs. 0.75), but less precise for two of the three data sources.

Three additional observations come from the results in Table 2. First, precision of birth year matters. The intensity measure assigned to an observation is partially dependent on the timing of their birth. The DHS survey specifically attempts to accurately measure year of birth information, and flags imputed results. Additionally, the estimates in column (2) include only individuals that were able to identify their year of birth.[11] Estimates for the DHS sample are generally larger, other than the LSMS estimate for men, suggesting that measurement error in the year of birth may be biasing results towards zero. Although larger, this is a reason to think that the DHS estimates may be more accurate. The second observation is that both the census and DHS estimate similar returns for both men and women. The estimate for the overall sample from the LSMS is similar to the DHS; however, caution is warranted when interpreting the breakout by gender. Finally, F-statistics are shown for all nine estimates in the table. Although developing this straightforward identification is an important addition to the literature, this method can be useful beyond its applications to measuring the impact of education reforms if the identification is strong enough to be used as an instrument in a two-stage least squares model. The F-statistics shown in Table 2 demonstrate that the intensity measure would be a strong instrument when using the full sample from any of either the census or the DHS, as well as the female sample of either of these datasets. This suggests that this method, when studying Ethiopia, can be used to examine how education affects a number of important outcomes, especially those relating to women and their families.

## 5.2 Alternative Intensity Measure and Samples

Using the full sample, estimates in Table 3 demonstrate that the results are not dependent on the assumption that the reform is fully implemented at a single point in time within each region or the sample selection. Estimates in Panel A use data from the Census, the estimates from the DHS are shown in Panel B, and those from the LSMS in Panel C. The estimates in column (1) allow for the possibility that the education reform was implemented gradually between 1993 and 1996. The pace of implementation is determined using the method described in Section 3.2, and calculated separately for each region. This adjustment allows for the possibility that the 1980 increase in schooling seen in Figure for the 1995 implementing regions was due to some early implementation in these regions. The baseline reform intensity measure is used in the remaining columns, but the sample is expanded by one pre- and post-reform year in each column.

[11] Of the DHS sample, 74.6 percent are able to report their year of birth information. Estimates using the full DHS sample are included in Appendix Table A4, and consistently yield slightly smaller point estimates.

Across all three datasets and five columns the estimates are statistically significant at the 95 percent confidence level, and at the 99 percent level in 14 of the 15 estimates. The estimates using the staggered implementation in column (1) are similar to those in Panel A of Table 3, although more precise and slightly larger for the DHS and LSMS data.

The inclusion of the linear time trend in the estimating equation could be capturing some of the effect of the reform during the partial implementation period. A possible solution to this would be to include additional post-reform years, which would allow for additional identification using the full implemented reform intensity measure. As seen in columns (2) through (5), expanding the range of cohorts included in the sample does lead to increased precision in the estimates. Furthermore, this increased precision comes with little change in the magnitude of the point estimates, especially for the DHS and LSMS, suggesting that the zone-specific trends are capturing any additional secular growth occurring in these years. The estimates in Table3 demonstrate that the results shown for the baseline model, in Table 2, are not reliant on the specific assumptions used to construct the intensity measure, or the sample range employed.

## 5.3 Migration Adjustments

As one of the three data sources used to test the robustness of the intensity measure, the LSMS adds significant value due to its inclusion of birth region information. An intensity measure constructed using zone of birth data would be ideal. However, LSMS data provide evidence that internal migration is not overly prevalent during this time period; more than four-fifths of the population in the sample lives in their region of birth. In addition to this, Ethiopia is split across strong ethnic and linguistic divides that provide an additional barrier to movement. Important information available from the LSMS can still be used to examine whether internal migration may be biasing the previous estimates.

Estimates shown in Table 4 are generated using the full sample of LSMS data. Estimates in column (1) simply add a set of region of birth fixed effects to the original model. The estimates in column (2) include only individuals who remain in their region of birth, 82 percent of the sample. The estimates in the third column utilize a migration adjusted intensity measure. To construct this new measure, province level intensity calculations were made following the same process described in section 2, and allocated using the following three rules: (1) the intensity value is birth zone if zone of birth is a major urban zone (Addis Ababa, Harari, or Dire Dawa), (2) value is region of birth average if currently living in a major urban zone that is not zone of birth, or if living in a zone that does not the border region of birth, (3) value is the original reform intensity if living in the region of birth or a zone bordering region of birth.[12]

[12]Rule 1 is prioritized, and Rule 2 is prioritized over Rule 3. Note that Addis Ababa, Harari, and Dire Dawa function as both zones and regions. If born in these regions, then zone-specific intensity measure is known for zone of birth.

All three estimates in Table 4 are statistically significant, and very similar to the baseline estimate from column (3) of Panel A in Table 2. The largest difference is 0.146, only about ten percent of the original estimate from Table 2. The similarity of the estimates is evidence that it is unlikely that the previous results are being significantly biased by misallocation of the intensity measure due to migration within Ethiopia.

### 5.4 Literacy and Advancing Through Grade 8 and Grade 9

A consistent point of contention regarding this type of "big-bang" reform is that while it may improve the quantity of education provided, it has a detrimental impact on the quality. This sentiment is likely true, but positive changes generated by bringing new students into the classroom may outweigh reductions in quality to the existing education. Literacy can be used to test whether the increase in years of schooling found in the previous results led to increased levels of education. Especially at the lower levels of education seen in the pre-reform period, it seems that increasing schooling off a pre-reform base of two years could potentially lead to large increases in literacy. Estimates using literacy as the dependent variable in equation (3) are shown in Panel A of Table 5. They indicate that the reform led to an increase in the literacy rate of between 8.8 and 21.5 percentage points, and all three estimates are statistically significant.[13] The median estimate is roughly a 50 percent increase in the literacy rate compared to the baseline level of the 1970 cohort. This is initial suggestive evidence that the post-reform quality of education remained high enough to lead to a significant increase in learning that would have not otherwise occurred.

An additional way to examine this question of education quality is to exploit a characteristic of the reform in Ethiopia. Progression past grade eight required completion of an exam both before and after the reform. This provides a second setting in which the quantity-quality balance can be seen and evaluated. The dependent variable in Panel B of Table 5 is an indicator equal to one if the respondent completed at least eight years of school, and in Panel C the dependent variable is equal to one if the respondent completed at least nine years of schooling. Estimates are again calculated using the model described in equation (3). Estimates in Panel B demonstrate that the reform increased the likelihood of completing at least eight years of school by between 5.6 and 10.9 percentage points.

The increase in grade eight completion can be thought of as the expected "quantity effect" that has been evident throughout the paper. In both states of the reform, these students then had to pass an exam to be eligible to continue to, and potentially complete, grade nine. If post-reform students perform better on the exam (increased quality), any increase in grade nine completion should be larger than the increase in the grade eight completion rate. If the opposite is true, and the increase is smaller for grade nine, this would

[13] Marginal probit estimates are larger for all three datasets ranging from 0.104 to 0.323, and remain statistically significant at least at the 95 percent level.

be evidence that the quality of the education may be declining. However, this then raises an important secondary question, has completion of grade nine increased due to the reform ("quantity effect" > "quality effect"), or declined ("quantity effect" < "quality effect")? Even in the presence of a decline in quality (a smaller effect on grade nine relative to grade eight), it remains possible that the overall effect of the reform is positive and the increased enrollment outweighs a decline in quality. The estimates in Panel C of Table 5 help provide insight to the answer for these questions. Across all three datasets, the increase in grade nine completion is less than the increase in grade eight completion. This suggests a higher failure rate on the post-grade eight exam, a lower quality.[14] At the very least, there is no evidence of a decline in quality that is so extreme that it outweighs the increase in the number of students entering and progressing through school. Both the literacy and grade completion estimates provide important suggestive evidence that a sufficient level of quality persists in the post-reform period.

## 5.5 Placebo Estimates

As a general trend, education levels throughout Africa have been increasing in recent years. Although the inclusion of two dimensions of variation and district specific trends help alleviate concerns that the estimates for Ethiopia are only capturing this secular increase in education, some level of concern could remain. To address this, an intensity measure is calculated for four additional countries for whom recent data are available, where no school fee removal occurred during the 1990s, but following the equations that define the timing of the Ethiopian reform's implementation. This simulates the same reform measured in Ethiopia being implemented in four alternative settings. Finding similar estimates would suggest that the results shown in this paper are driven by general increases in education that are not specific to Ethiopia. Intensity measures from Kenya (which borders Ethiopia to the southwest), Tanzania, Zambia, and Mali are calculated using census data from each country.[15] Kenya was chosen due to its proximity to Ethiopia, Mali for its similar pre-1990s education level, and Tanzania and Zambia were added as additional checks that fit the necessary conditions mentioned above. These intensity measures are then paired with the most up to date datasets available, the 2008/9 and 2014 DHS from Kenya, the 2007/8 (AIDS Indicators Survey) and 2010 DHS from Tanzania, the 2010 census from Zambia, and the 2009 census from Mali; equation (3) is used to estimate whether the placebo intensity measures predict increases in schooling similar to those found in Ethiopia.

The estimates are shown in Table 6. Estimates in Panel A simulate the split implementation in Ethiopia

[14] Marginal probit point estimates suggest grade eight results between 0.051 and 0.073, and grade nine estimates ranging between 0.031 and 0.035. Both estimates for using Census data are statistically significant at the 99 percent level, the DHS grade 8 estimate at the 90 percent level, and all three grade 9 estimates suggest a similar increase. Estimates in all three datasets follow the same pattern seen in Table 5; grade eight estimates are between 0.19 and 0.38 percentage points larger.

[15] Census data are from Minnesota Population Center's IPUMS (2015).

by following the pattern seen in Figure 2 by assigning regions with below median schooling to the 1993 implementation, and those above the median to the 1995 implementation. Panel B includes estimates for a simulated 1993 implementation, and Panel C for a simulated 1995 implementation. There is no evidence of a large increase in years of schooling linked to the misplaced intensity measures in any of the four countries, for any of the pseudo implementations dates. The largest positive estimate is 0.397, from the Zambian Census with a 1995 implementation. However, none of the other 11 estimates are positive and statistically significant, and the estimate for Zambia is only about half of the size of the corresponding estimate using the Ethiopian census. Furthermore, Mali is the most comparable setting in terms of the initial level of schooling; in Mali the average level of schooling in the 1970 cohort was 1.67 years of schooling. The coefficient estimates for Mali are consistently negative. This could signify an increased unlikelihood for areas that have extremely low levels of initial schooling, like Ethiopia in the early 1990s, to have convergence in education levels without such a significant effort to improve the system. Most importantly, there is no evidence from these regressions that the model is capturing general trends in schooling improvements; large positive estimates were only found in Ethiopia, in the presence of an education reform.

## 6 Conclusion

Using both temporal and geographic variation to identify the effect of the education reform in Ethiopia, the model shows that the removal of school fees led to large increases in schooling. The reform is found to have increased the average Ethiopian's schooling by more than one additional year. The estimates are shown to be robust across a number of specifications and migration adjustments. The reform is also found to have increased literacy rates by 50 percent. The method of identification is replicated in four alternative African countries, countries in which no contemporaneous education reform took place. In none of these countries is there evidence of a similar increase in education, further isolating the Ethiopian reform as the cause of the increase in years of schooling.

The magnitude of the increase in schooling that occurred in Ethiopia due to the reform is difficult to overstate. The reform increased schooling by more than 70 percent relative to the baseline level of the 1970 cohort. To put this into perspective, the size of external intervention necessary to cause this kind of national change is unimaginable. In 1995, there were 26 million Ethiopian children under the age of 14. Consider a scenario in which it is possible to construct an education program that would guarantee every student who entered would complete eight years of primary school, and assume that the pre-reform level of schooling was an average of 1.7 years. Although the program itself is unrealistic, no program can promise completion, the scale of this type of program would need to be impossibly large. If this program were to be implemented in

Ethiopia, and it had a target of increasing schooling by 1.2 years (the average estimate from Table 2), on average, for all 26 million children, the program would have to enroll 4.95 million children.[16] In other words, the magnitude of the reform that occurred in Ethiopia is equivalent to a program that brings nearly one out of every five children in Ethiopia all the way through eight years of primary school.

This paper presents a novel strategy, using regularly available data, to identify national level schooling reforms in developing settings. This strategy is applied to Ethiopia, and the estimates indicate that the reform led to significantly more schooling. Initial results also show that the increased quantity of schooling led to higher levels of literacy and ninth grade completion. This is evidence of increased learning, and that the increased enrollments generated by the reform outweigh any potential quality loss in education. Future work can expand on this conclusion by utilizing this method of identification to examine whether the fee removal of the early 1990s, and the increased schooling it generated, led to welfare gains for the people of Ethiopia.

[16]This calculation comes from solving for $x$ in the following equation. $2.9 = \frac{26-x}{26} \cdot 1.7 + \frac{x}{26} \cdot 8$. The $26 - x$ million students not involved in the program remain at two years of school, and $x$ students enter the program and receive eight years of schooling. The goal is to increase the average years of schooling by 1.2 years, from the baseline level of 1.7 years, to 2.9 years of school.

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# Figures

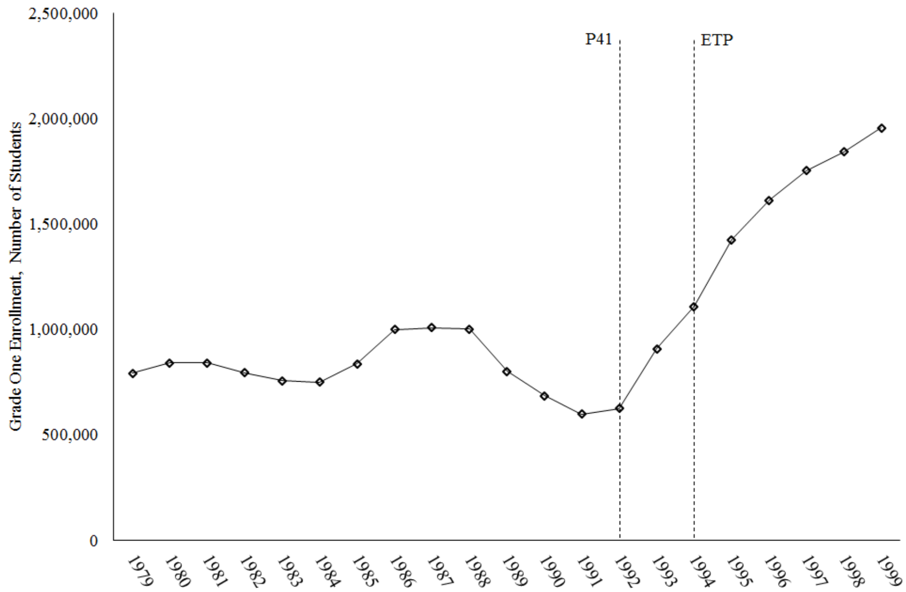



Figure 1: Grade One Enrollment, by Academic Year

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

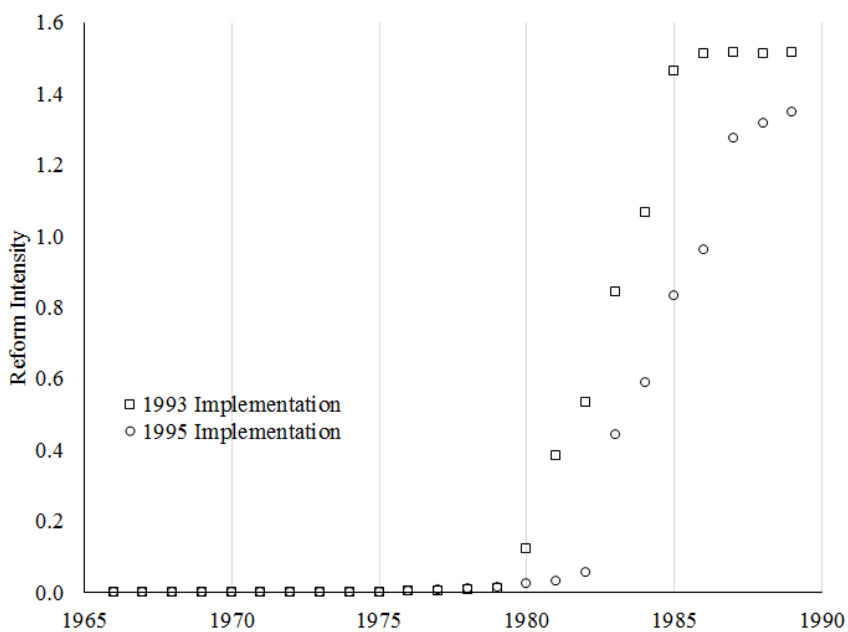
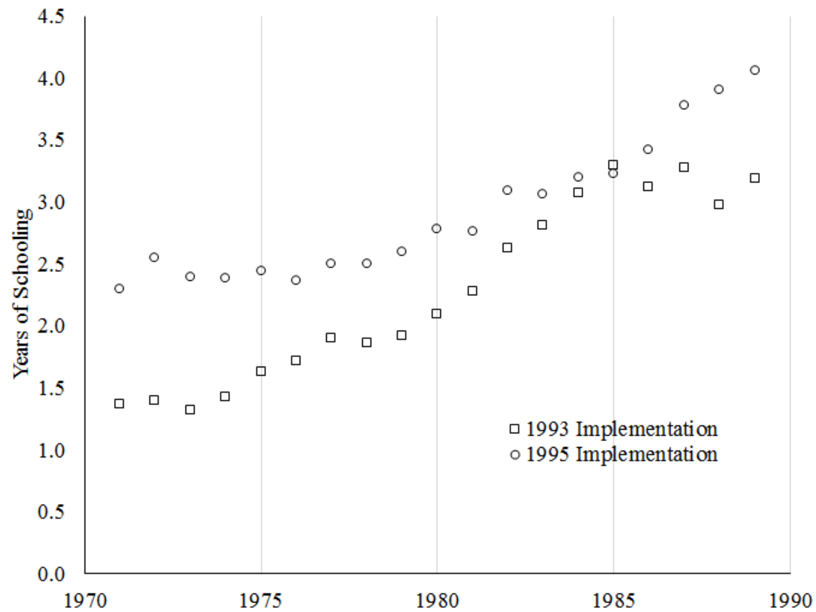



Figure 2: Reform Intensity Measure for 1993 and 1995 Implementing Regions, by Birth Year




(a) Years of Schooling


(b) Growth Relative to 1979 Birth Cohort

Figure 3: Years of Schooling for 1993 and 1995 Implementing Regions, by Birth Year

Note: Data are from the 2007 Ethiopian Census, the 2005 and 2011 waves of the Demographic and Health Survey, and the 2014 Living Standards and Measurement Study; each source is equally weighted.

# Tables

Table 1: Summary Statistics

| | Full Sample | | Men | | Women | |
|---|---|---|---|---|---|---|
| Birth Year | 1970 | 1989 | 1970 | 1989 | 1970 | 1989 |
| Reform Intensity | 0.00 | 1.36 | 0.00 | 1.23 | 0.00 | 1.47 |
| Years of Schooling | 1.73 | 3.83 | 2.67 | 4.32 | 1.15 | 3.48 |
| Literacy | 0.26 | 0.55 | 0.43 | 0.65 | 0.15 | 0.47 |
| Completed at Least 8 Years | 0.08 | 0.20 | 0.11 | 0.23 | 0.06 | 0.18 |
| Completed at Least 9 Years | 0.06 | 0.15 | 0.08 | 0.16 | 0.05 | 0.14 |

Note: All data for schooling variables are from the 2007 Ethiopian Census (Minnesota Population Center, 2015), the 2005 and 2011 waves of the Demographic and Health Survey, and the 2014 Living Standards and Measurement Study; each source is equally weighted. Completed at least 1 year, literacy, completed at least 8 years, and completed at least 9 years are expressed as fractions of the relevant population group.

Table 2: Effect of Education Reform on Years of Schooling

| | Census (1) | DHS (2) | LSMS (3) |
|---|---|---|---|
| | | A. Full Sample | |
| Reform Intensity$_{zy}$ | 0.714*** | 1.414*** | 1.340*** |
| | (0.160) | (0.441) | (0.469) |
| F-Statistic | 19.87 | 10.25 | 8.15 |
| N | 392,702 | 22,139 | 5,280 |
| | | B. Men | |
| Reform Intensity$_{zy}$ | 0.647** | 1.145 | 2.407** |
| | (0.245) | (0.783) | (1.038) |
| F-Statistic | 6.99 | 2.14 | 5.38 |
| N | 185,848 | 8,754 | 2,439 |
| | | C. Women | |
| Reform Intensity$_{zy}$ | 0.606*** | 1.207*** | 0.427 |
| | (0.164) | (0.325) | (0.990) |
| F-Statistic | 13.69 | 13.75 | 0.19 |
| N | 206,854 | 13,448 | 2,841 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. All samples include birth cohorts from 1970 to 1989; in Panel B and Panel C sex specific data are used to calculate the reform intensity. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. An indicator equal to one for all male observations is included in Panel A regressions, and DHS estimates, which use data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table 3: Effect of Education Reform on Years of Schooling:
Alternative Specifications

| | Staggered Implementation (1) | Birth Years | 1969 1990 (2) | 1968 1991 (3) | 1967 1992 (4) | 1966 1993 (5) |
|---|---|---|---|---|---|---|
| | | | A. Census | | | |
| Reform Intensity$_{zy}$ | 0.698*** | | 0.815*** | 0.846*** | 1.049*** | 1.018*** |
| | (0.188) | | (0.161) | (0.166) | (0.209) | (0.218) |
| F-Statistic | 13.81 | | 25.54 | 26.09 | 25.25 | 21.86 |
| N | 392,702 | | 426,256 | 461,414 | 528,450 | 561,373 |
| | | | B. DHS | | | |
| Reform Intensity$_{zy}$ | 1.548*** | | 1.417*** | 1.492*** | 1.406*** | 1.439*** |
| | (0.345) | | (0.435) | (0.413) | (0.378) | (0.374) |
| F-Statistic | 20.11 | | 10.58 | 13.04 | 13.75 | 14.73 |
| N | 22,139 | | 24,156 | 25,528 | 26,952 | 28,282 |
| | | | C. LSMS | | | |
| Reform Intensity$_{zy}$ | 1.579*** | | 1.331** | 1.442*** | 1.355*** | 1.375*** |
| | (0.550) | | (0.534) | (0.449) | (0.449) | (0.403) |
| F-Statistic | 8.23 | | 6.22 | 10.33 | 9.11 | 11.62 |
| N | 5,280 | | 5,808 | 6,254 | 6,871 | 7,297 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. All samples include both male and female observations. All regressions include birth year and zone fixed effects, an indicator equal to one for observations that are male, and zone-specific linear trends. DHS estimates, which use data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Estimates in column (1) assume gradual implementation as explained in 3.2 and use the baseline 1970 to 1989 cohorts, estimates in columns (2) through (5) expand the sample by one additional pre- and post-reform cohort in each column. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table 4: Effect of Education Reform on Years of Schooling: Migration Adjustments

| | (LSMS Only) | | |
|---|---|---|---|
| | Birth Region Fixed Effects | Only Non-Movers | Migration Adjusted Intensity |
| | (1) | (2) | (3) |
| Reform Intensity$_{zy}$ | 1.268*** | 1.194** | 1.486** |
| | (0.470) | (0.553) | (0.583) |
| F-Statistic | 7.26 | 4.66 | 6.49 |
| N | 5,276 | 4,581 | 5,276 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. The sample includes birth cohorts from 1970 to 1989. All regressions include birth year and zone fixed effects, an indicator equal to one for observations that are male, and zone-specific linear trends. The estimate in column (1) also includes region of birth fixed effects. The sample in column (2) only includes respondents living in their region of birth, and column (3) is estimated using a migration adjusted intensity. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table 5: Effect of Education Reform on Literacy and Completing At Least Eight or Nine Years

| | Census (1) | DHS (2) | LSMS (3) |
|---|---|---|---|
| | A. Literacy | | |
| Reform Intensity$_{zy}$ | 0.088*** | 0.137*** | 0.215** |
| | (0.016) | (0.057) | (0.065) |
| N | 398,509 | 21,789 | 5,290 |
| | B. Complete At Least Eight Years of Schooling | | |
| Reform Intensity$_{zy}$ | 0.056*** | 0.109*** | 0.067 |
| | (0.014) | (0.040) | (0.042) |
| N | 392,702 | 22,202 | 5,280 |
| | C. Complete At Least Nine Years of Schooling | | |
| Reform Intensity$_{zy}$ | 0.035*** | 0.070** | 0.052 |
| | (0.012) | (0.028) | (0.041) |
| N | 392,702 | 22,202 | 5,280 |

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is an indicator equal to one if the respondent is literate in Panel A, and if years of schooling is greater than or equal to eight (Panel B), or nine (Panel C). The sample includes birth cohorts from 1970 to 1989. All regressions include birth year and zone fixed effects, an indicator equal to one for individuals that are male, and zone-specific linear trends. DHS estimates, using data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table 6: Effect of Education Reform on Years of Schooling:
Placebo Estimates

| | Kenya<br>(1) | Tanzania<br>(2) | Zambia<br>(3) | Mali<br>(4) |
|---|---|---|---|---|
| | A. Both 1993 and 1995 Implementation Regions | | | |
| Reform Intensity$_{zy}$ | 0.144 | -1.406** | -0.189 | -0.200 |
| | (0.287) | (0.673) | (0.144) | (0.196) |
| N | 37,855 | 13,097 | 338,175 | 267,179 |
| | B. 1993 Implementation | | | |
| Reform Intensity$_{zy}$ | 0.293 | -1.681*** | -0.192 | -0.849*** |
| | (0.305) | (0.541) | (0.140) | (0.234) |
| N | 37,855 | 13,097 | 338,175 | 267,179 |
| | C. 1995 Implementation | | | |
| Reform Intensity$_{zy}$ | 0.162 | -1.781*** | 0.397*** | -0.761*** |
| | (0.300) | (0.582) | (0.146) | (0.114) |
| | 0.592 | 0.003 | 0.009 | 0.000 |
| N | 37,855 | 13,097 | 338,175 | 267,179 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. The sample includes birth cohorts from 1970 to 1989. All regressions include birth year and zone fixed effects, an indicator equal to one for observations that are male, and zone-specific linear trends. To simulate the pattern seen in Figure 2, in Panel A regions below the median level of pre-reform schooling are assigned pseudo 1993 implementation, and regions above a 1995 implementation. In Panel B (1993) and Panel C (1995), all regions are assigned to have the same pseudo implementation date. Kenya data are from 2008/9 and 2014 rounds and Tanzania data are from 2007, 2008, and 2010 rounds of the DHS; estimates also include a cubic control for age. Zambia and Mali data are from 2010 and 2009 census rounds, respectively. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level. Kenya includes fewer than 50 zones, wild-bootstrap p-value is substituted for standard error (Cameron et al., 2008).

# A Appendix

## A.1 Timing and Intensity Calculation: Example of 1995 Implementation

The following uses a 1995 implementation of the Ethiopian education reform as an example. Appendix Table A8 outlines the year-grade level combinations that are considered to be affected by the reform. The table is organized by birth year, age at the beginning of the calendar year, and grade of attendance if starting and matriculating on time.[17] Students born in 1987 or later, and starting on time, enter school in the post-reform, fee-free, period. However, students entering school before this time must pay the prevailing school fees to enter grade one. These divergent decisions yield two separate probabilities of entering school at a given age, one pre-reform, and one post-reform. School starting age probabilities are calculated using census data for ages 6 to 12 for each zone in Ethiopia; it is assumed that the relative start probabilities across the different ages remain constant. For example, if a student is twice as likely to enter at age seven relative to age six, this ratio is consistent across both states of the reform. For each zone, pre-reform period start probabilities ($S_{z,pre}$) are rescaled to ensure that the sum of the start probabilities for ages 6 to 12 is equal to one minus the fraction of students who never enter school. Post-reform start probabilities ($S_{z,post}$) are scaled by setting the sum of the age 6 to 12 start probabilities equal to one, the maximum potential impact of the reform leading full attendance. The above assumption regarding the relative start age probabilities and these two definitions ensure that the post-reform start probability at any given age, $a$, is larger than the pre-reform start probability ($S_{z,post,a} - S_{z,pre,a} > 0$). Note that this leaves a number of students who would enter by age 12 in the post-reform state, but not in the pre-reform state. This will be taken into account in the following equations.

The equations for the calculation of each birth year are listed below. These equations take into account the pre and post start probabilities explained above, and magnitude calculation from equation (1). Any birth year and age combination that would have an individual graduating grade 10 prior to the implementation of the reform, would yield zero benefit from the reform, and is not included in the following equations. From Appendix Table A8, this is analogous to a student being born in 1977 or earlier, and entering grade one no later than 1985. Furthermore, a student born in 1972, that enters school as late as allowed by this model, at age 12, would start grade one in 1985, and therefore not be impacted by the reform,

$$I_{z,1972} = 0.$$

Then a student born the next year, in 1973, and entering school at age 12 would receive a single year of

[17] Ethiopian school begins at the beginning of the Ethiopian year (in September of the Gregorian calendar).

additional schooling from the reform,

$$I_{z,1973} = S_{z,pre,12} \cdot M_z\left(9\right).$$

Those born in 1974 and starting at age 12 would receive two additional years, and starting at 11 would receive one additional year, and this iteration continues in a similar fashion through the 1981 birth year:

$$I_{z,1974} = S_{z,pre,12} \cdot M_z\left(8\right) + S_{z,pre,11} \cdot M_z\left(9\right),$$

$$I_{z,1975} = S_{z,pre,12} \cdot M_z\left(7\right) + S_{z,pre,11} \cdot M_z\left(8\right) + S_{z,pre,10} \cdot M_z\left(9\right),$$

$$I_{z,1976} = S_{z,pre,12} \cdot M_z\left(6\right) + S_{z,pre,11} \cdot M_z\left(7\right) + S_{z,pre,10} \cdot M_z\left(8\right) + S_{z,pre,9} \cdot M_z\left(9\right),$$

$$I_{z,1977} = S_{z,pre,12} \cdot M_z\left(5\right) + S_{z,pre,11} \cdot M_z\left(6\right) + S_{z,pre,10} \cdot M_z\left(7\right) + S_{z,pre,9} \cdot M_z\left(8\right) + S_{z,pre,8} \cdot M_z\left(9\right),$$

$$I_{z,1978} = S_{z,pre,12} \cdot M_z\left(4\right) + S_{z,pre,11} \cdot M_z\left(5\right) + S_{z,pre,10} \cdot M_z\left(6\right) + S_{z,pre,9} \cdot M_z\left(7\right) + S_{z,pre,8} \cdot M_z\left(8\right) + S_{z,pre,7} \cdot M_z\left(9\right),$$

$$\begin{aligned} I_{z,1979} = {} & S_{z,pre,12} \cdot M_z\left(3\right) + S_{z,pre,11} \cdot M_z\left(4\right) + S_{z,pre,10} \cdot M_z\left(5\right) + S_{z,pre,9} \cdot M_z\left(6\right) \\ & + S_{z,pre,8} \cdot M_z\left(7\right) + S_{z,pre,7} \cdot M_z\left(8\right) + S_{z,pre,6} \cdot M_z\left(9\right), \end{aligned}$$

$$\begin{aligned} I_{z,1980} = {} & S_{z,pre,12} \cdot M_z\left(2\right) + S_{z,pre,11} \cdot M_z\left(3\right) + S_{z,pre,10} \cdot M_z\left(4\right) + S_{z,pre,9} \cdot M_z\left(5\right) \\ & + S_{z,pre,8} \cdot M_z\left(6\right) + S_{z,pre,7} \cdot M_z\left(7\right) + S_{z,pre,6} \cdot M_z\left(8\right), \end{aligned}$$

$$\begin{aligned} I_{z,1981} = {} & S_{z,pre,12} \cdot M_z\left(1\right) + S_{z,pre,11} \cdot M_z\left(2\right) + S_{z,pre,10} \cdot M_z\left(3\right) + S_{z,pre,9} \cdot M_z\left(4\right) \\ & + S_{z,pre,8} \cdot M_z\left(5\right) + S_{z,pre,7} \cdot M_z\left(6\right) + S_{z,pre,6} \cdot M_z\left(7\right). \end{aligned}$$

For the 1981 birth cohort, the latest starters, age 12, begin school in 1986, and still have to make the decision to enter school during the pre-reform period. This changes beginning with the 1982 birth cohort; students who are born in 1982 and enter grade one at age 12 are entering during the post-reform period. Furthermore, there are a set of students who would have entered at earlier ages, 6 to 11, only if fees were removed, but now have the opportunity to enter at age 12 in a fee-free environment. However, many of these students in the delayed entry group may be taking part in other activities that makes school entry infeasible. To take this into account delayed entrants are discounted by the following equation: $\frac{1}{e^{(a-7)}}$. This assumes that entrance for this set of students who would have entered at younger ages in a post-reform state becomes increasingly less likely the longer the delay:

$$I_{z,1982} = \left[ S_{z,post,12} + \frac{1}{e^{12-7}} \sum_{a=6}^{11} \left( S_{post,a} - S_{pre,a} \right) \right] M_z (0) + S_{z,pre,11} \cdot M_z (1)$$
$$+ S_{z,pre,10} \cdot M_z (2) + S_{z,pre,9} \cdot M_z (3) + S_{z,pre,8} \cdot M_z (4) + S_{z,pre,7} \cdot M_z (5) + S_{z,pre,6} \cdot M_z (6) .$$

In the 1983 cohort, similar to the 1982 cohort, 11 and 12 year old grade one entrants enter school in the post-reform period. Ages 6 through 10 still must make the decision in the pre-reform period, leaving a large stock of potential future entrants. The fraction of remaining entrants is again defined by the $(S_{z,post,a} - S_{z,pre,a})$ difference; the students who would have entered at each age had they been entering in the post-reform period, but had to make the decision pre-reform. However, again the size of this group is discounted to take into account potential productive activity that precludes their ability to enter school at a later time. This pattern is continued through the later cohorts, as well;

$$I_{z,1983} = \left[ \sum_{a=11}^{12} S_{z,post,a} + \frac{1}{e^{11-7}} \sum_{a=6}^{10} \left( S_{z,post,a} - S_{z,pre,a} \right) \right] M_z (0)$$
$$+ S_{z,pre,10} \cdot M_z (1) + S_{z,pre,9} \cdot M_z (2) + S_{z,pre,8} \cdot M_z (3) + S_{z,pre,7} \cdot M_z (4) + S_{z,pre,6} \cdot M_z (5) ,$$

$$I_{z,1984} = \left[ \sum_{a=10}^{12} S_{z,post,a} + \frac{1}{e^{10-7}} \sum_{a=6}^{9} \left( S_{z,post,a} - S_{z,pre,a} \right) \right] M_z (0)$$
$$+ S_{z,pre,9} \cdot M_z (1) + S_{z,pre,8} \cdot M_z (2) + S_{z,pre,7} \cdot M_z (3) + S_{z,pre,6} \cdot M_z (4) ,$$

$$I_{z,1985} = \left[ \sum_{a=9}^{12} S_{z,post,a} + \frac{1}{e^{9-7}} \sum_{a=6}^{8} \left( S_{z,post,a} - S_{z,pre,a} \right) \right] M_z (0)$$
$$+ S_{z,pre,8} \cdot M_z (1) + S_{z,pre,7} \cdot M_z (2) + S_{z,pre,6} \cdot M_z (3) ,$$

$$I_{z,1986} = \left[ \sum_{a=8}^{12} S_{z,post,a} + \frac{1}{e^{8-7}} \sum_{a=6}^{7} \left( S_{z,post,a} - S_{z,pre,a} \right) \right] M_z (0) + S_{z,pre,7} \cdot M_z (1) + S_{z,pre,6} \cdot M_z (2) ,$$

$$I_{z,1987} = \left[ \sum_{a=7}^{12} S_{z,post,a} + \frac{1}{e^{7-7}} \left( S_{z,post,6} - S_{z,pre,6} \right) \right] M_z (0) + S_{z,pre,6} \cdot M_z (1) ,$$

$$I_{z,1988} = \left[ \sum_{a=6}^{12} S_{z,post,a} \right] M_z (0) = M_z (0) .$$

For the 1989 birth cohort, all individuals, even the early starters, enter school completely in the post-reform period.

## A.2 Examining Post-Reform Investment in Primary Education

If at the same time school fees were removed the government increased investment in the poorest performing areas, this would be a potential confounding factor. This would lead to a greater increase in schooling in the areas furthest behind, a result matching the key identifying assumption used in this paper. This concern is examined below, and no evidence of increased investment in areas with lagging pre-reform schooling is found. It is also important to note that this type of pattern would only be problematic for the interpretation of the results. If such additional efforts were occurring as part of the reform, the estimates could still be interpreted as returns to the education reforms, instead of returns to the removal of school fees.

To examine whether significant investments were being made in other aspects of the education process pre-reform schooling levels are compared to growth in inflation adjusted spending and pupil-teacher ratios (PTR) in Ethiopian primary schools following the implementation of the reform. It would be problematic if pre-reform schooling levels (1960 to 1970 birth cohorts) are negatively correlated with the growth in post-reform spending and class size. This would suggest that areas with lower levels of pre-reform schooling saw larger growth in investments in education following the implementation of the reform. However, if there is a positive correlation, this would suggest that regions of Ethiopia which had been previously successful in schooling completion were the same regions that continued to invest relatively more in their students. This would only put downward pressure on estimates of the impact of fee removal, possibly leading to an understatement of the effect. Spending and pupil-teacher ratios are from World Bank (2005).

First to ensure that this methodology matches with the paper's general assumption of the evolution of primary school enrollments, I examine the correlation between pre-reform schooling levels and growth in primary school enrollment. There is a correlation of -0.348 between initial schooling levels and enrollment growth between 1993 and 1996. This demonstrates that primary enrollment was generally larger in areas with lower levels of pre-reform schooling. The same relationship exists when the period is extended to 2001, there is a correlation coefficient of -0.498. This is consistent with the central identifying assumption of school fee removal leading to larger growth in areas with lower levels of pre-reform schooling. However, it is important to examine if increases in spending on primary education also exhibit this pattern.

I calculate regional growth in both real total spending and real per-student spending on primary education between 1993, the earliest year of data availability, and 1996, and then from 1993 to 2001. Over this time period, real total spending on primary education did increase in nine of 11 regions, but this increase has a correlation of 0.115 (1993 to 1996) and -0.022 (1993 to 2001) with pre-reform education levels. There is no evidence of poorly performing regions disproportionately increasing investment; if anything, higher performing regions had the larger initial investments in the 1993 to 1996 period.

Finally, World Bank (2005) also includes information on pupil-teacher rations separated by rural and urban regions. In 1993 the two areas had similar PTRs, although rural areas generally had lower initial schooling levels. Following the education reforms, the PTR in rural parts of the country increased by roughly 130 percent, and only by 40 percent in urban areas. This again suggests that there was not a similar per-pupil investment in the areas that had lower initial levels of schooling (rural areas), but that primary enrollments did dramatically increase in these areas.

To summarize, there is no evidence of a relationship between spending and pre-reform education levels. This suggests there was no concentrated effort to invest in regions lagging behind in schooling attainment during the reform's implementation. This conclusion is reinforced by the more than doubling of class size in rural areas, an increase that did not occur to nearly the same level in the more educated urban areas. This further suggests that it was not investment in the areas with lower pre-reform education that led to increases in enrollment. However, even without this link to investment, enrollment increases were larger in areas with lower levels of initial schooling, consistent with the key identifying assumption of the paper.

## A.3 Appendix - Figures and Tables

Table A1: Effect of Education Reform on Years of Schooling:
Census, DHS, and LSMS Combined

| | A. Baseline Sample | | | |
|---|---|---|---|---|
| | Full Sample | | | |
| | Baseline (1) | Staggered Implementation (2) | Men (3) | Women (4) |
| Reform Intensity$_{zy}$ | 1.032*** | 1.080*** | 0.779* | 0.926*** |
| | (0.259) | (0.221) | (0.451) | (0.203) |
| F-Statistic | 15.89 | 2.98 | 20.83 | 23.97 |
| N | 414,841 | 194,602 | 220,302 | 414,904 |
| | **B. Alternative Samples** | | | |
| Birth Years | 1969 1990 | 1968 1991 | 1967 1992 | 1966 1993 |
| Reform Intensity$_{zy}$ | 1.170*** | 1.248*** | 1.293*** | 1.312*** |
| | (0.238) | (0.238) | (0.228) | (0.230) |
| F-Statistic | 24.13 | 27.50 | 32.26 | 32.54 |
| N | 456,220 | 493,196 | 562,273 | 596,952 |
| | **C. Education Outcomes** | | | |
| | Literacy | Grade 8 | Grade 9 | |
| Reform Intensity$_{zy}$ | 0.114*** | 0.082*** | 0.051*** | |
| | (0.033) | (0.024) | (0.018) | |
| N | 420,236 | 414,841 | 414,841 | |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling in Panel A and Panel B, and is an indicator for the specified outcome in Panel C. Unless otherwise indicated, samples include birth cohorts from 1970 to 1989. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. An indicator equal to one for all male observations is included in all regressions that include observations of both sexes, and DHS estimates, which use data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A2: Effect of Education Reform on Years of Schooling:
Alternative Specifications - Men Only

| | Staggered Implementation | Birth Years | 1969 1990 | 1968 1991 | 1967 1992 | 1966 1993 |
|---|---|---|---|---|---|---|
| | (1) | | (2) | (3) | (4) | (5) |
| | | | **A. Census** | | | |
| Reform Intensity$_{zy}$ | 0.612** | | 0.711*** | 0.691** | 0.786*** | 0.712** |
| | (0.249) | | (0.254) | (0.266) | (0.271) | (0.282) |
| F-Statistic | 6.04 | | 7.83 | 6.75 | 8.42 | 6.36 |
| N | 185,848 | | 202,813 | 221,066 | 254,166 | 271,435 |
| | | | **B. DHS** | | | |
| Reform Intensity$_{zy}$ | 1.336** | | 1.13 | 1.092 | 0.883 | 0.988 |
| | (0.621) | | (0.730) | (0.682) | (0.616) | (0.625) |
| F-Statistic | 4.63 | | 2.40 | 2.56 | 2.06 | 2.50 |
| N | 8,754 | | 9,581 | 10,233 | 10,909 | 11,508 |
| | | | **C. LSMS** | | | |
| Reform Intensity$_{zy}$ | 2.742*** | | 2.184** | 2.520*** | 2.312*** | 2.328*** |
| | (0.970) | | (0.856) | (0.914) | (0.835) | (0.791) |
| F-Statistic | 7.99 | | 6.51 | 7.60 | 7.66 | 8.66 |
| N | 2,439 | | 2,678 | 2,887 | 3,184 | 3,376 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. All samples include only male observations. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. DHS estimates, which use data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Estimates in column (1) assume gradual implementation as explained in 3.2 and use the baseline 1970 to 1989 cohorts, estimates in columns (2) through (5) expand the sample by one additional pre- and post-reform cohort in each column. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A3: Effect of Education Reform on Years of Schooling:
Alternative Specifications - Women Only

| | Staggered Implementation (1) | Birth Years | 1969 1990 (2) | 1968 1991 (3) | 1967 1992 (4) | 1966 1993 (5) |
|---|---|---|---|---|---|---|
| | | | **A. Census** | | | |
| Reform Intensity$_{zy}$ | 0.833*** | | 0.710*** | 0.764*** | 1.027*** | 1.030*** |
| | (0.228) | | (0.170) | (0.155) | (0.244) | (0.234) |
| F-Statistic | 13.40 | | 17.53 | 24.23 | 17.72 | 19.29 |
| N | 206,854 | | 223,443 | 240,348 | 274,284 | 289,938 |
| | | | **B. DHS** | | | |
| Reform Intensity$_{zy}$ | 1.675*** | | 1.272*** | 1.360*** | 1.292*** | 1.315*** |
| | (0.361) | | (0.339) | (0.352) | (0.343) | (0.341) |
| F-Statistic | 21.49 | | 14.09 | 14.89 | 14.22 | 14.87 |
| N | 13,448 | | 14,638 | 15,358 | 16,106 | 16,837 |
| | | | **C. LSMS** | | | |
| Reform Intensity$_{zy}$ | 0.914 | | 0.331 | 0.272 | 0.315 | 0.408 |
| | (1.168) | | (1.043) | (0.990) | (0.791) | (0.755) |
| F-Statistic | 0.61 | | 0.10 | 0.08 | 0.16 | 0.29 |
| N | 2,841 | | 3,130 | 3,367 | 3,687 | 3,921 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. All samples include only female observations. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. DHS estimates, which use data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Estimates in column (1) assume gradual implementation as explained in 3.2 and use the baseline 1970 to 1989 cohorts, estimates in columns (2) through (5) expand the sample by one additional pre- and post-reform cohort in each column. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A4: Effect of Education Reform on Years of Schooling: Including Imputed Birth Years – DHS

| | Full (1) | Men (2) | Women (3) |
|---|---|---|---|
| Reform Intensity$_{zy}$ | 1.251*** | 0.826 | 1.152*** |
| | (0.408) | (0.777) | (0.306) |
| F-Statistic | 9.39 | 1.13 | 14.13 |
| N | 29,673 | 10,682 | 18,991 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. The sample includes birth cohorts from 1970 to 1989; in column (2) and column (3) the estimated reform intensity is calculated using the relevant sex specific data. All regressions include birth year and zone fixed effects, a cubic control for age, zone-specific linear trends. An indicator equal to one for all male observations is included in the column (1) regression. All DHS observations that report not knowing their exact year of birth are added to the original sample. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A5: Effect of Education Reform on Literacy and Completing At Least Eight or Nine Years – by Sex

| | Census | | DHS | | LSMS | |
|---|---|---|---|---|---|---|
| | Men (1) | Women (2) | Men (3) | Women (4) | Men (5) | Women (6) |
| | | | A. Literacy | | | |
| Reform Intensity$_{zy}$ | 0.084*** | 0.073*** | 0.166* | 0.091** | 0.258** | 0.082 |
| | (0.027) | (0.014) | (0.099) | (0.044) | (0.125) | (0.087) |
| N | 190,142 | 208,367 | 8,576 | 13,213 | 2,445 | 2,845 |
| | | | B. Complete At Least Eight Years of Schooling | | | |
| Reform Intensity$_{zy}$ | 0.044* | 0.046*** | 0.105 | 0.079*** | 0.135 | 0.017 |
| | (0.022) | (0.015) | (0.087) | (0.029) | (0.126) | (0.086) |
| N | 185,848 | 206,854 | 8,754 | 13,448 | 2,439 | 2,841 |
| | | | C. Complete At Least Nine Years of Schooling | | | |
| Reform Intensity$_{zy}$ | 0.022 | 0.034*** | 0.054 | 0.066** | 0.108 | 0.031 |
| | (0.019) | (0.012) | (0.080) | (0.027) | (0.094) | (0.073) |
| N | 185,848 | 206,854 | 8,754 | 13,448 | 2,439 | 2,841 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is an indicator equal to one if the respondent is literate in Panel A, and if years of schooling is greater than or equal to eight (Panel B), or nine (Panel C). The sample includes birth cohorts from 1970 to 1989, for the specified sex. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. DHS estimates, using data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A6: Effect of Education Reform on Literacy and Completing At Least Eight or Nine Years: No Addis Ababa

| | Census | DHS | LSMS |
|---|---|---|---|
| | (1) | (2) | (3) |
| | A. Literacy | | |
| Reform Intensity$_{zy}$ | 0.089*** | 0.146* | 0.212** |
| | (0.020) | (0.076) | (0.090) |
| N | 376,311 | 18,805 | 4,875 |
| | B. Complete At Least Eight Years of Schooling | | |
| Reform Intensity$_{zy}$ | 0.049*** | 0.134*** | 0.069 |
| | (0.018) | (0.048) | (0.058) |
| N | 370,646 | 19,209 | 4,867 |
| | C. Complete At Least Nine Years of Schooling | | |
| Reform Intensity$_{zy}$ | 0.031* | 0.084** | 0.042 |
| | (0.016) | (0.035) | (0.055) |
| N | 370,646 | 19,209 | 4,867 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is an indicator equal to one if the respondent is literate in Panel A, and if years of schooling is greater than or equal to eight (Panel B), or nine (Panel C). The sample includes birth cohorts from 1970 to 1989; all observations from Addis Ababa are dropped. All regressions include birth year and zone fixed effects, an indicator equal to one for individuals that are male, and zone-specific linear trends. DHS estimates, using data from the 2005 and 2011 surveys, also include a cubic control for age. Census (2007) and LSMS (2014) estimates include only a single survey round, age is therefore jointly determined by birth year and survey year. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level.

Table A7: Effect of Education Reform on Years of Schooling:
Placebo Estimates – by Sex

| | Kenya | | Tanzania | | Zambia | | Mali | |
|---|---|---|---|---|---|---|---|---|
| | Men | Women | Men | Women | Men | Women | Men | Women |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | A. Both 1993/1995 Implementation Regions | | | | | | | |
| Reform Intensity$_{zy}$ | 0.244 | -0.028 | -3.343 | -0.750 | -0.055 | -0.180 | -0.192 | -0.174 |
| | (0.437) | (0.293) | (2.856) | (0.533) | (0.220) | (0.168) | (0.220) | (0.166) |
| N | 10,331 | 27,524 | 1,080 | 12,017 | 161,990 | 176,185 | 122,683 | 144,496 |
| | B. 1993 Implementation | | | | | | | |
| Reform Intensity$_{zy}$ | 0.972 | -0.030 | -5.753* | -1.174** | 0.069 | -0.203 | -0.962*** | -0.521** |
| | (0.766) | (0.241) | (2.997) | (0.482) | (0.276) | (0.175) | (0.285) | (0.199) |
| N | 10,331 | 27,524 | 1,080 | 12,017 | 161,990 | 176,185 | 122,683 | 144,496 |
| | C. 1995 Implementation | | | | | | | |
| Reform Intensity$_{zy}$ | 0.780 | -0.140 | -3.205 | -1.371*** | 0.598** | 0.332** | -0.507*** | -0.772*** |
| | (0.507) | (0.286) | (3.156) | (0.493) | (0.295) | (0.159) | (0.177) | (0.219) |
| | 0.132 | 0.627 | 0.312 | 0.007 | 0.048 | 0.042 | 0.006 | 0.001 |
| N | 10,331 | 27,524 | 1,080 | 12,017 | 161,990 | 176,185 | 122,683 | 144,496 |

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is years of schooling. The sample includes birth cohorts from 1970 to 1989, for the specified sex. All regressions include birth year and zone fixed effects, as well as zone-specific linear trends. To simulate the pattern seen in Figure 2, in Panel A regions below the median level of pre-reform schooling are assigned pseudo 1993 implementation, and regions above a 1995 implementation. In Panel B (1993) and Panel C (1995), all regions are assigned to have the same pseudo implementation date. Kenya data are from 2008/9 and 2014 rounds and Tanzania data are from 2007, 2008, and 2010 rounds of the DHS; estimates also include a cubic control for age. Zambia and Mali data are from 2010 and 2009 census rounds, respectively. Each estimate is from a unique regression, weighted using weights provided by the survey, and standard errors are clustered at the zone level. Kenya includes fewer than 50 zones, wild-bootstrap p-value is substituted for standard error (Cameron et al., 2008).

## Table A8: Timing of Post-Reform Year and Grade Level: 1995 Implementation

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1977 | | Born | |
| 1978 | | 0 | |
| 1979 | | 1 | |
| 1980 | | 2 | |
| 1981 | | 3 | |
| 1982 | | 4 | |
| 1983 | | 5 | |
| 1984 | | 6 | |
| 1985 | G1 | 7 | |
| 1986 | G2 | 8 | |
| 1987 | G3 | 9 | |
| 1988 | G4 | 10 | |
| 1989 | G5 | 11 | |
| 1990 | G6 | 12 | |
| 1991 | G7 | 13 | |
| 1992 | G8 | 14 | |
| 1993 | G9 | 15 | |
| 1994 | G10 | 16 | |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1978 | | Born | |
| 1979 | | 0 | |
| 1980 | | 1 | |
| 1981 | | 2 | |
| 1982 | | 3 | |
| 1983 | | 4 | |
| 1984 | | 5 | |
| 1985 | | 6 | |
| 1986 | G1 | 7 | |
| 1987 | G2 | 8 | |
| 1988 | G3 | 9 | |
| 1989 | G4 | 10 | |
| 1990 | G5 | 11 | |
| 1991 | G6 | 12 | |
| 1992 | G7 | 13 | |
| 1993 | G8 | 14 | |
| 1994 | G9 | 15 | |
| 1995 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1979 | | Born | |
| 1980 | | 0 | |
| 1981 | | 1 | |
| 1982 | | 2 | |
| 1983 | | 3 | |
| 1984 | | 4 | |
| 1985 | | 5 | |
| 1986 | | 6 | |
| 1987 | G1 | 7 | |
| 1988 | G2 | 8 | |
| 1989 | G3 | 9 | |
| 1990 | G4 | 10 | |
| 1991 | G5 | 11 | |
| 1992 | G6 | 12 | |
| 1993 | G7 | 13 | |
| 1994 | G8 | 14 | |
| 1995 | G9 | 15 | Post |
| 1996 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1980 | | Born | |
| 1981 | | 0 | |
| 1982 | | 1 | |
| 1983 | | 2 | |
| 1984 | | 3 | |
| 1985 | | 4 | |
| 1986 | | 5 | |
| 1987 | | 6 | |
| 1988 | G1 | 7 | |
| 1989 | G2 | 8 | |
| 1990 | G3 | 9 | |
| 1991 | G4 | 10 | |
| 1992 | G5 | 11 | |
| 1993 | G6 | 12 | |
| 1994 | G7 | 13 | |
| 1995 | G8 | 14 | Post |
| 1996 | G9 | 15 | Post |
| 1997 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1981 | | Born | |
| 1982 | | 0 | |
| 1983 | | 1 | |
| 1984 | | 2 | |
| 1985 | | 3 | |
| 1986 | | 4 | |
| 1987 | | 5 | |
| 1988 | | 6 | |
| 1989 | G1 | 7 | |
| 1990 | G2 | 8 | |
| 1991 | G3 | 9 | |
| 1992 | G4 | 10 | |
| 1993 | G5 | 11 | |
| 1994 | G6 | 12 | |
| 1995 | G7 | 13 | Post |
| 1996 | G8 | 14 | Post |
| 1997 | G9 | 15 | Post |
| 1998 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1982 | | Born | |
| 1983 | | 0 | |
| 1984 | | 1 | |
| 1985 | | 2 | |
| 1986 | | 3 | |
| 1987 | | 4 | |
| 1988 | | 5 | |
| 1989 | | 6 | |
| 1990 | G1 | 7 | |
| 1991 | G2 | 8 | |
| 1992 | G3 | 9 | |
| 1993 | G4 | 10 | |
| 1994 | G5 | 11 | |
| 1995 | G6 | 12 | Post |
| 1996 | G7 | 13 | Post |
| 1997 | G8 | 14 | Post |
| 1998 | G9 | 15 | Post |
| 1999 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1983 | | Born | |
| 1984 | | 0 | |
| 1985 | | 1 | |
| 1986 | | 2 | |
| 1987 | | 3 | |
| 1988 | | 4 | |
| 1989 | | 5 | |
| 1990 | | 6 | |
| 1991 | G1 | 7 | |
| 1992 | G2 | 8 | |
| 1993 | G3 | 9 | |
| 1994 | G4 | 10 | |
| 1995 | G5 | 11 | Post |
| 1996 | G6 | 12 | Post |
| 1997 | G7 | 13 | Post |
| 1998 | G8 | 14 | Post |
| 1999 | G9 | 15 | Post |
| 2000 | G10 | 16 | Post |

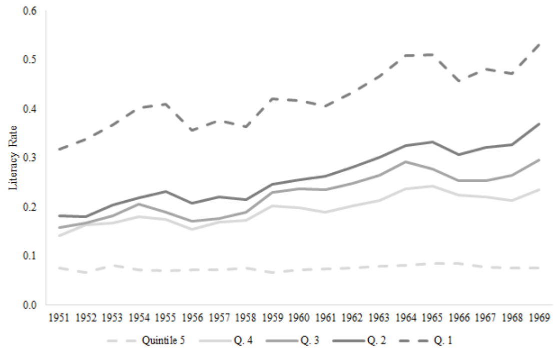
| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1984 | | Born | |
| 1985 | | 0 | |
| 1986 | | 1 | |
| 1987 | | 2 | |
| 1988 | | 3 | |
| 1989 | | 4 | |
| 1990 | | 5 | |
| 1991 | | 6 | |
| 1992 | G1 | 7 | |
| 1993 | G2 | 8 | |
| 1994 | G3 | 9 | |
| 1995 | G4 | 10 | Post |
| 1996 | G5 | 11 | Post |
| 1997 | G6 | 12 | Post |
| 1998 | G7 | 13 | Post |
| 1999 | G8 | 14 | Post |
| 2000 | G9 | 15 | Post |
| 2001 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1985 | | Born | |
| 1986 | | 0 | |
| 1987 | | 1 | |
| 1988 | | 2 | |
| 1989 | | 3 | |
| 1990 | | 4 | |
| 1991 | | 5 | |
| 1992 | | 6 | |
| 1993 | G1 | 7 | |
| 1994 | G2 | 8 | |
| 1995 | G3 | 9 | Post |
| 1996 | G4 | 10 | Post |
| 1997 | G5 | 11 | Post |
| 1998 | G6 | 12 | Post |
| 1999 | G7 | 13 | Post |
| 2000 | G8 | 14 | Post |
| 2001 | G9 | 15 | Post |
| 2002 | G10 | 16 | Post |

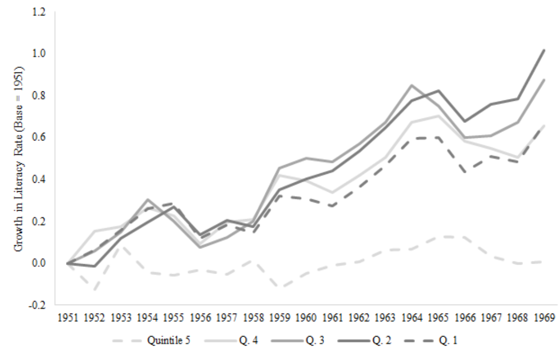
| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1986 | | Born | |
| 1987 | | 0 | |
| 1988 | | 1 | |
| 1989 | | 2 | |
| 1990 | | 3 | |
| 1991 | | 4 | |
| 1992 | | 5 | |
| 1993 | | 6 | |
| 1994 | G1 | 7 | |
| 1995 | G2 | 8 | Post |
| 1996 | G3 | 9 | Post |
| 1997 | G4 | 10 | Post |
| 1998 | G5 | 11 | Post |
| 1999 | G6 | 12 | Post |
| 2000 | G7 | 13 | Post |
| 2001 | G8 | 14 | Post |
| 2002 | G9 | 15 | Post |
| 2003 | G10 | 16 | Post |

| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1987 | | Born | |
| 1988 | | 0 | |
| 1989 | | 1 | |
| 1990 | | 2 | |
| 1991 | | 3 | |
| 1992 | | 4 | |
| 1993 | | 5 | |
| 1994 | | 6 | |
| 1995 | G1 | 7 | Post |
| 1996 | G2 | 8 | Post |
| 1997 | G3 | 9 | Post |
| 1998 | G4 | 10 | Post |
| 1999 | G5 | 11 | Post |
| 2000 | G6 | 12 | Post |
| 2001 | G7 | 13 | Post |
| 2002 | G8 | 14 | Post |
| 2003 | G9 | 15 | Post |
| 2004 | G10 | 16 | Post |

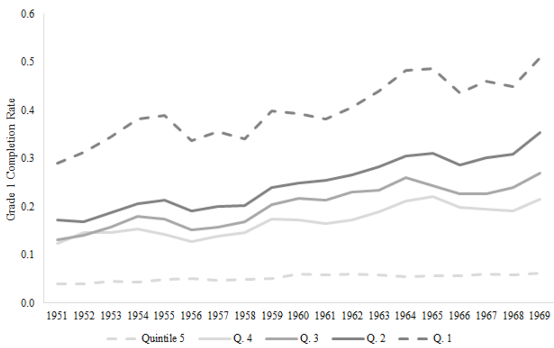
| Birth Year | Grade | Age | Reform Status |
|---|---|---|---|
| 1988 | | Born | |
| 1989 | | 0 | |
| 1990 | | 1 | |
| 1991 | | 2 | |
| 1992 | | 3 | |
| 1993 | | 4 | |
| 1994 | | 5 | |
| 1995 | | 6 | |
| 1996 | G1 | 7 | Post |
| 1997 | G2 | 8 | Post |
| 1998 | G3 | 9 | Post |
| 1999 | G4 | 10 | Post |
| 2000 | G5 | 11 | Post |
| 2001 | G6 | 12 | Post |
| 2002 | G7 | 13 | Post |
| 2003 | G8 | 14 | Post |
| 2004 | G9 | 15 | Post |
| 2005 | G10 | 16 | Post |



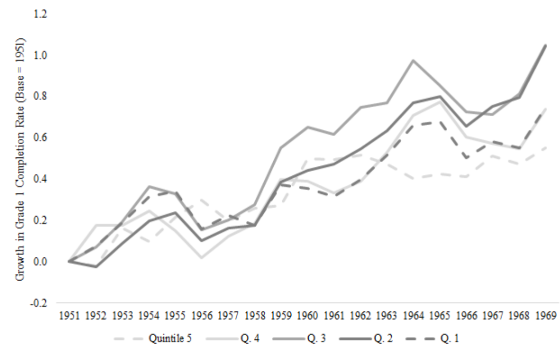

(a) Pre-Reform Literacy Rate; by Birth Year



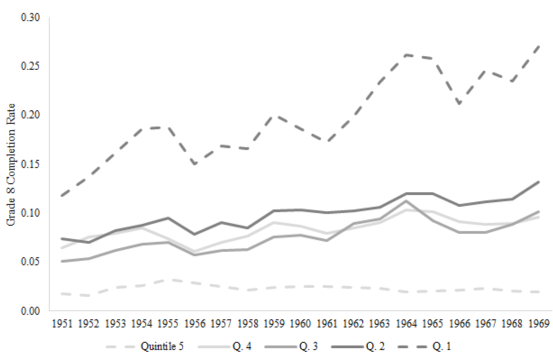

(b) Growth in Pre-Reform Literacy Rate



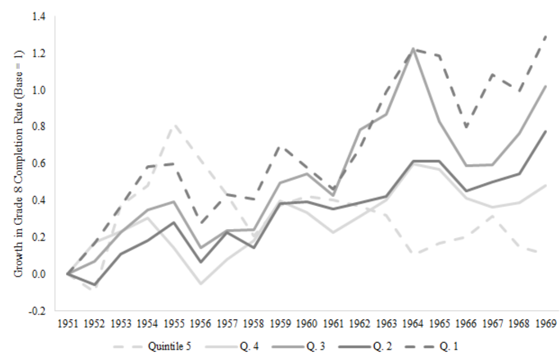

(c) Pre-Reform Grade 1 Completion Rate; by Birth Year



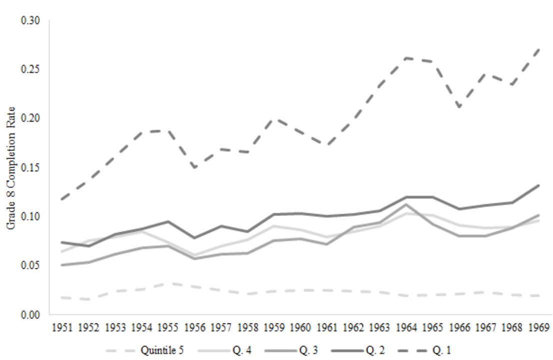

(d) Growth in Pre-Reform Grade 1 Completion Rate



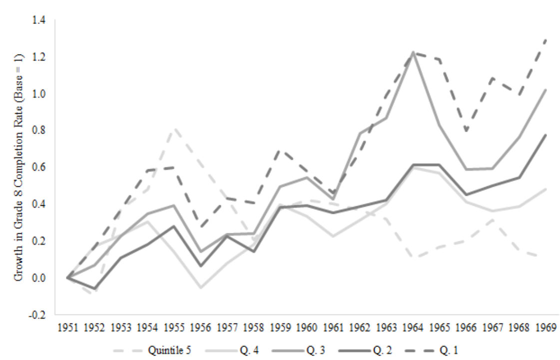

(e) Pre-Reform Grade 8 Completion Rate; by Birth Year




(f) Growth in Pre-Reform Grade 8 Completion Rate




(g) Pre-Reform Grade 8 Completion Rate; by Birth Year




(h) Growth in Pre-Reform Grade 8 Completion Rate

Figure A1: Pre-Reform Trends by Pre-Refrom Schooling Quintiles (Q.5 = Low; Q.1=High)

Note: Data are from 2007 Ethiopian Census.