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Maternal education and child mortality: Evidence from a quasi-experiment in Malawi and Uganda

Liliana Andriano[a], Christiaan W. S. Monden[a]

[a] Department of Sociology, Nuffield College, University of Oxford

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Abstract

Since the 1980s the demographic literature has suggested that maternal schooling plays a key role in determining children's survival chances in low and middle income countries. However, no studies have successfully distinguished between the causal and non-causal relationship between maternal education and child mortality and to overcome the endogeneity problems inherent in this relationship. In order to identify the causal effect of maternal education on child mortality we explore exogenous variation in maternal education induced by schooling reforms in the second half of the 1990s in Malawi and Uganda, which introduced Universal Primary Education. We use a two-stage residual inclusion approach and the Demographic and Health Surveys data to test if increased maternal schooling reduced the probability of dying before age five across these countries. We find that the reform caused an increase in years and levels of education for the youngest women and validate this assumption. In Malawi, we find that for each additional year and level of education, children have a 66% and 91% lower probability of dying, respectively, while for Uganda the estimates show that the odds of dying for children of women with one additional year and level of education are 0.48% and 0.17% lower, respectively. However, these effects are not statistically significant probably due to the lower quality of education provided as a result of the reform.

1 Introduction

Child survival is a key indicator of social development and a serious challenge to developing countries. Substantial progress has been made in order to increase children survival chances: since 1990 the number of deaths under age five has reduced from 12.7 million to 6.3 million, but still every year over 50 children per 1,000 live births continue to die in developing countries (UNICEF, WHO, The World Bank, & UN, 2012, 2014). All developing regions have more than halved the under-five mortality rate since 1990, with the only exception of Sub-Saharan Africa and Oceania who registered an actual 48% and 28% decline (UNICEF et al., 2014). As the region with the highest mortality rate (i.e. 92 deaths per 1,000 live births in 2013), the Sub-Saharan Africa continent continues to face remarkable challenges, thus, requires an accelerating progress in reducing mortality among children.

The strong negative association between maternal education and child mortality is well known (e.g., Bicego & Boerma, 1990; Caldwell, 1979; Cleland, Bicego, & Fegan, 1992; Gakidou, Cowling, Lozano, & Murray, 2010; Hatt & Waters, 2006) and has built the basis for the enormous investments in the education of young girls. A number of different quantifiable channels through which maternal schooling contributes to the improvement of child health outcomes have been proposed. Socioeconomic factors have been found to be the most important mechanism through which maternal schooling is associated to child health outcomes (Alemayehu Azeze & Huang, 2014; Cleland & Van Ginneken, 1988; Desai & Alva, 1998; Frost, Forste, & Haas, 2005; Grépin & Bharadwaj, 2015; Keats, 2014). Further research suggests that women's education plays a key role in increasing children's health outcomes through modern attitudes about modern health care (Frost et al., 2005), personal illness control (Basu & Stephenson, 2005; Desai & Alva, 1998), maternal knowledge of health (Alemayehu Azeze & Huang, 2014; Aslam & Kingdon, 2012; Frost et al., 2005; Glewwe, 1999; Webb & Block, 2004), autonomy or empowerment (Aslam & Kingdon, 2012; Frost et al., 2005), and place of residence (Bicego & Boerma, 1990; Desai & Alva, 1998).

However, most of these studies use single- or multi-level regression models to derive the relationship between maternal's years of education (or educational-level) and child mortality, after controlling for child-, maternal-, family-, and community-level background variables. But, the negative relationship may arise due to omitted variables such as maternal ability, family background and resources, and community infrastructure, which predict both maternal education and child mortality—the omission of unobserved confounding variables may produce a biased estimate of the association. Hence, studies that consider maternal education as an exogenous variable and assume absence of unobserved confounders cannot assess the causality of the relationship between mother's education and child mortality.

A few studies have explored the omitted variable bias with the use of family or household fixed effects (Horton, 1988; Strauss, 1990; Wolfe & Behrman, 1987). Horton (1988) uses data on siblings in the Philippines and runs within-household regressions, which show that the significance of the correlation between maternal education and current as well as long-run child nutritional status disappears. Strauss (1990) analyses data on extended families living together to control for unobserved characteristics at household level and finds that the association between education and weight given height is attenuated. Wolfe & Behrman (1987) use data on sisters in Nicaragua to remove the family fixed effect and find that once they control for the unobserved family background characteristics shared in common among sisters, the impact of maternal education on child health outcomes completely disappears. These results suggest that it is worthwhile taking the hypothesis that the effect of maternal education on child mortality in absolute value has been overstated.

A way of addressing this issue and accounting for observed and unobserved confounding variables is to use an exogenous source of variation in education that is not related to child mortality. Instrumental variables (IV) exploit quasi-natural experiments that are "situations where the forces of nature or government policy have conspired to produce an environment somewhat akin to a randomized experiment" (Angrist & Krueger, 2001, p. 73). While a growing number of quasi-experimental studies on the relationship between education and fertility-related behaviours and intentions among adolescent girls have provided causal evidence of this link (Behrman, 2015; Breierova & Duflo, 2004; Ferré, 2009; Keats, 2014; Osili & Long, 2008; Zanin, Radice, & Marra, 2015), fewer are the studies identifying the effect of mother's education on child health outcomes in developing countries (Breierova & Duflo, 2004; Chou, Liu, Grossman, & Joyce, 2010; Grépin & Bharadwaj, 2015; Keats, 2014).

Breierova & Duflo (2004) take advantage of a large-scale school construction program in Indonesia to examine the effect of parental schooling on fertility and child mortality. Using regional differences in program intensity and birth cohorts differences as instrument for the average years of education in the household, their results show that an increase in parental education leads to a reduction in child mortality rates. Chou et al. (2010) exploit the Taiwanese expansion of compulsory schooling in 1968 and instrument the consequent variation in access to schooling for education to estimate the causal effects of parental educational attainment on infant birth outcomes. They find a significant reduction in the probability of being born underweight and of dying in the neonatal and postnatal periods because of the increase in both paternal and maternal education. Grépin & Bharadwaj (2015) use the expansion in the access to secondary education in 1980 in Zimbabwe to explore the impact of increased maternal education on child mortality[1]. They find that children born to mothers benefitting from the reform were less likely to die than children born to mothers not benefitting from the reform. Keats (2014) takes advantage of the implementation of the national reform in Uganda aimed at eliminating primary school fees in 1997 as an exogenous change in primary schooling investments to examine whether women exposed to the program invest more in their children (Keats, 2014, p.1). He finds strong evidence that a one-year increase in women's education rises their first-born children's likelihood of receiving immunisations and other preventive care, as well as decreases their first-born children's likelihood of presenting signs of chronic malnutrition. He does not find evidence of significant differences in the probability of dying before age one and five for children of women exposed and not exposed to the reform. However, Keats (2014) attributes the non-significance of maternal education on infant and child mortality to the fact that i) deaths are reported by the mothers thus suffering from measurement error and ii) the most common cause of death among under-five children in Uganda is malaria (31.9%) and maternal education does not seem to increase investments in the prevention and care of malaria (Keats, 2014, p.3-4).

To our knowledge, no studies have used the IV method to study the nature of the relationship between maternal education and child mortality across Sub-Saharan African countries. In the second half of the 1990s, Universal Primary Education (UPE) reforms were implemented in Malawi and Uganda—these mainly consisted of the abolishment of school fees with the aim to provide basic education to all children between 6 and 12 years. We will use the consequent random variation in the access to schooling and the district-level variation in the intensity of the reform as an instrument for maternal's education with the aim to identify the impact of women's schooling on child mortality.
The results show that the effect of the exposure to the program is positively associated with the years and levels of maternal schooling, meaning that girls exposed to the policy have more education than girls not exposed to the policy across all three countries. As for the effect of maternal schooling on child mortality, we find evidence that a one-year and a one-level increase in the schooling of the woman causes a significant reduction in her child's probability of dying in Malawi and Uganda.

[1] In sub-Saharan Africa, the differences between having no education and primary education are extremely important, thus although this study is relevant, it misses most of the population.

However, this effect is not statistically significant and we try to explain why by means of a descriptive analysis concerning the quality of education provided as a result of the reform.

The remaining of the paper is as follows. In Section 2, we provide a review of the UPE reforms in Malawi and Uganda. Section 3 describes the data used for the analysis. In Section 4 and 5 we present the identification strategy and methods, respectively, while in Section 6 we present the results. Section 7 discusses the results and in Section 8 we draw some conclusions of our work.

2 The UPE Reforms

After the Jomtien World Conference on Education for All in 1990[2] confirming that UPE is one of the most beneficial interventions for reducing poverty, policymakers reached the conclusion that in Sub-Saharan Africa "the goal of universal basic education could be reached only by making primary education free" (Avenstrup, Liang, & Nellemann, 2004, p.5). Direct costs of education, including tuition, books, exam, and uniforms fees, as well as community and parent-teacher association contributions, represent a large percentage of households' spending, especially for low-income ones (Morgan, Petrosino, & Fronius, 2012; World Bank, 2009b). During the 1990s, Sub-Saharan governments, with the support of development agencies and non-governmental organizations, began to eliminate primary school fees (World Bank, 2009a).

2.1 Malawi

Malawi was the first country to launch the schooling reform, and its launch coincided with the introduction of multiparty democracy, being a key issue for the governmental election campaign. School fees were abolished grade by grade starting in 1991, but it is in September 1994 that a *big bang* approach was adopted and free primary education was launched and strongly enforced (Avenstrup et al., 2004). Besides the abolishment of tuition fees (between 10 and 22 kwachas per pupil per annum[3]), which were not replaced by a capitation grant to the school, uniforms were made optional and the government paid for basic textbooks and exercise books fees and other direct fees[4,5] (Kadzamira & Rose, 2003; Rose, 2002). Enrolment in primary education increased by 51% in the first year, from 1.9 million in 1993 to 2.9 million in 1994, and the disparities in gross enrolment rates among income quintiles, gender, and areas were no longer existent in 1997 (Al-Samarrai & Zaman, 2002; Inoue & Oketch, 2008).

2.2 Uganda

In Uganda, a UPE policy reform was prepared in 1987, but the lack of resources and focus on primary education, and the political constraints impeded its implementation. In 1996, during the campaign of

[2] The Education for All is an international initiative led by UNESCO and a global commitment to provide quality basic education for all children, youth and adults. It was first launched in 1990 at the Jomtien World Conference, which gathered delegates from 155 countries, as well as representatives from 150 governmental and non-governmental organisations. They adopted both the World Declaration on Education for All, which reaffirmed the fundamental human right of education and urged countries to strengthen efforts to address the basic learning needs of all, and the Framework for Action to Meet Basic Learning Needs, which set targets and strategies to meet them by 2000 (WCEFA Inter-Agency Commission, 1990).

[3] In 1994, 10 and 22 kwachas were equal respectively to 2.35 and 5.18 US dollars. For Malawian households, fees accounted for 15.50% of the total cost of primary education (Chimombo, 1999).

[4] The government provided teachers, classrooms, furniture, teachers' houses, sanitation facilities, and boreholes (World Bank, 2009a).

[5] Private schools were also included in the fee abolition policy.

the first direct elections for President, the eventual winner made a promise to provide free primary schooling and, after being elected in December 1996, he announced the launch of the Free Primary Education reform to be implemented in January 1997. At the beginning, free primary education was provided for up to four children per family and at least two of the four children had to be girls and priority had to be given to disabled children[6]. The government assigned two types of grants to the (government-aided) school: the capitation and the school facilities grants. The former was meant to cover tuition fees (5,000 Ugandan shillings per pupil per annum in the first three years of schooling and 8,100 Ugandan shillings for the fourth to the seventh classes)[7,8] that the school could spend on instructional materials, extracurricular activities, maintenance and utilities, and administration and the latter to assist schools in the construction of classrooms, latrines and teachers' houses, and to procure furniture (Essama-Nssah, Leite, & Simler, 2008; Grogan, 2006). In 2003 the policy was expended in that the four-children rule was eliminated and uniforms were made optional. Primary school enrolment in Uganda rose by 68%, from 3.4 in 1996 to 5.7 million in 1997, and jumped by 240% over six years (Avenstrup et al., 2004). Free primary education has had a positive influence on the enrolment of the poorest quintile: in 1992, prior to the introduction of UPE, the attendance rate for the poorest quintile in the primary school age group was 50% compared to 84% for the richest, but after the introduction of UPE these rates were almost on a par (Avenstrup et al., 2004; Essama-Nssah et al., 2008). Also the inequalities in access associated with gender, parental education and region of residence significantly narrowed after the implementation of the reform (Essama-Nssah et al., 2008).

2.3 Intensity of the program

In order to capture the intensity of the program by administrative unit, this study uses the number of primary schools constructed in the years following UPE. In this sub-section we present the administrative structure Malawi and Uganda so as to identify the unit responsible of the educational sector.

Malawi is divided into three administrative regions: Northern, Central and Southern. In 1994, there were twenty-four districts, five in the Northern Region, nine in the Central Region and ten in the Southern Region, in each district there are Traditional Authorities and Villages. The provision of primary schooling and primary school teacher training is a responsibility of the Ministry of Education, Science and Technology; however, the administrative structure of education was assigned to six education divisions, namely Northern, Central Eastern, Central Western, South Eastern, Shire Highlands, South Western and to the respective 28 district education offices[9,10] (Chimombo et al., 2000; EMIS 2006).

In 1996, Uganda was divided into four statistical (not administrative) regions: Central, Eastern, Northern and Western. The country was further divided into 39 administrative districts[11,12,13],

[6] As a matter of fact, school fees elimination was applied to all primary school students, regardless of how many siblings were into primary school (Grogan, 2006; Riddell, 2003).
[7] In 1997, 5,000 and 8,100 Ugandan shillings were equal respectively to 4.62 and 7.48 US dollars. However, to give some context, in 1999 the salary of a teacher in a government-aided school in Uganda was about 75,000 shillings per month (Uganda Ministry of Education and Sports, 1999 cited in Grogan, 2006).
[8] The cost of putting one child through school amounted to 20% of the average per capita income (Fyfe, 2005).
[9] These included 24 districts and 4 urban cities (Mzuzu city, Lilongwe urban, Zomba urban, and Blantyre urban). In 2000 and 2010 DHS, the data from Mzuzu city and Mzimba district were combined, so we were able to identify 27 districts.
[10] To this day, there are 34 district education offices.
[11] The districts increased to 45 during 1997.
[12] In 2000/01 DHS the Ntungamo district was combined with Mbarara district so we specify 38 districts.
[13] The districts were identified using the GPS data. The clusters in the DHS are geo referenced: their coordinates are collected during the survey sample listing proves using GPS receivers so that one record for each cluster is available. In

subdivided into counties, sub-counties and parishes (and sub-parishes in most cases). Under the local Government Act of 1997, public service (nursery, primary schools, special schools and technical schools) fell under the administration and management of District Councils, which had the authority to formulate, approve, and execute their own development plan, register UPE children, and distribute textbooks. Moreover, monthly remittances for schools from central government are transmitted to local government via the district administration officer, who in turn pass them on to schools (Uganda Ministry of Education and Sports, 2004 cited in Nambalirwa, 2010; Nakabugo, Byamugisha, & Bithaghalire, 2008, p.60).

3 Data

The data for this study arise from the Demographic and Health Surveys (DHS). The DHS are national household sample surveys measuring population, health, socioeconomic and, for most of them, anthropometric indicators, which emphasise maternal and child health in developing countries. DHS provide the most reliable data source on child mortality across developing countries in terms of coverage, comparability, and data quality using extensive interviewer training, standardised measurement instruments and techniques, and instrument pretesting to ensure standardisation and comparability across space and time (ICF Macro, 2009).

In order to realise the datasets, the DHS uses a two-stage stratified design: in the first stage, a number of clusters[14,15] are selected with a probability proportional to the size from a frame list of enumeration areas (EAs) created from the most recent population census and in the second stage, a fixed number of households are selected from the complete list of households in each of the selected EAs. The households in a survey area are stratified according to type of residence (urban-rural) crossed by administrative/geographical regions (Aliaga & Ren, 2006, p.iii). All household members in a certain age group (usually women of reproductive age 15-49[16] and men age 15-59) in the selected households are interviewed. For this paper the household members of interest will be the mother, for which a complete birth and death history of her children was collected, including birth date and death age, when applicable, and the children under age of five, born in the 5-year period before the survey.

For Malawi we use pooled data from the 2000, 2004, and 2010 DHS and for Uganda the DHS surveys from 2000/1, 2006, and 2011 and from the DHS-Malaria Indicator Survey (MIS)[17] from 2009 are pooled. In order to consider only women who have completed their educational we consider those women who were at least 18 and 19 at the time the survey was conducted in Malawi and Uganda, respectively. In addition, we exclude all children who were not living with their mothers at the time of the survey: it would be meaningless to link their health status to their mothers' characteristics. We also exclude visitors to the household and not usual residents.

Table 1 shows the descriptive statistics of the pooled samples.[18] The Malawian sample includes 19,682 children under the age of five and 13,172 mothers between 17 and 36 years; women exposed

order to protect the privacy of the respondents, the GPS data are randomly displaced up to 5 and 2 kilometres respectively in rural and urban areas; however the data is located within the country boundaries and assigned DHS region.

[14] The term cluster is used interchangeably with community.

[15] The number of clusters varies from about 300 to 550 for a sample of 10,000 households for each survey in each country (Vaessen, Mamadou, & Le, 2005, p.500).

[16] We use the term mothers and women interchangeably and we refer to women in reproductive age (15-49).

[17] The MIS was developed by the Monitoring and Evaluation Working Group of Roll Back Malaria, an international partnership coordinating global efforts to fight malaria, and collects national and regional or provincial data from a representative sample of respondents.

[18] The data presented in this table and in the following tables and figures are weighted by sampling weights, that is the inverse of the probability that the observation is included in the sample.

to the reform, on average, have more years of education compared to women not exposed to the reform (4.95 against 3.84). The Ugandan sample includes 11,260 children between 0 and five years and 6,668 mothers between 18 and 34 years; women age 11-15 at UPE are more educated than women age 16-20 at UPE (5.39 against 4.21).

Table 1. Sample characteristics.

A. Malawi

	Overall		*Treatment (age 11-15)*		*Control (age 16-20)*	
	Mean	SD	Mean	SD	Mean	SD
Characteristics of the mother						
Years of education	4.42	3.58	4.95	3.53	3.84	3.55
Age in 1994	15.39	2.82	13.07	1.40	17.97	1.40
Sample size	13,172		7,044		6,128	
Characteristics of the child						
Child is dead	0.11	0.31	0.10	0.30	0.11	0.32
Birth year	2003.0	4.17	2004.0	3.80	2002.0	4.34
Child is female	0.51	0.50	0.50	0.50	0.51	0.50
Birth order	3.04	1.67	2.62	1.41	3.50	1.80
Sample size	19,682		10,316		9,366	

B. Uganda

	Overall		*Treatment (age 11-15)*		*Control (age 16-20)*	
	Mean	SD	Mean	SD	Mean	SD
Characteristics of the mother						
Years of education	4.76	3.64	5.39	3.60	4.21	3.59
Age in 1997	15.66	2.84	13.05	1.48	17.96	1.37
Sample size	6,668		3,163		3,505	
Characteristics of the child						
Child is dead	0.08	0.28	0.07	0.26	0.09	0.29
Birth year	2005.0	3.85	2006.0	2.77	2004.0	4.16
Child is female	0.50	0.50	0.49	0.50	0.51	0.50
Birth order	3.20	1.78	2.74	1.45	3.60	1.94
Sample size	11,260		5,296		5,964	

Using information on the district of residence of the mother, we match individual survey data with the number of primary schools constructed in the district in the years following the reform (see Table 2 for details). These data are taken from the Education Management Information Systems database of the Ministry of Education. We take the difference in the number of primary schools between 1995/96 and 1993/94 for Malawi.[19] In Malawi, there is a negative correlation between the change in the number primary schools and the enrolment rates in 1993/94 (EMIS 1994, 1996). This highlights

[19] We submitted a request to obtain this information for Uganda, but at the time of writing it was not in our possess yet. Hence, the analysis for Uganda have been run without the intensity measure.

the necessity of the government to construct primary schools in districts where the proportion of children in school was low.

Table 2. Characteristics of the district-level information.

	Malawi		*Uganda*	
	Mean	SD	Mean	SD
Number of primary schools constructed per 100 children	0.1815	0.2515	na	na
Gross enrolment rate	100.20	27.18	na	na
Number of districts	27		38	

Note: na = not available yet.
Source: EMIS various years.

These data have some limitations. The Ministry of Education in both countries publishes data from school records submitted by primary school administrations. It has been shown that the estimates on gross enrolment rate are different from the survey estimates either because: i) some school do not respond to the Ministry’s request for information; ii) government school fund allocations are based on enrolment, so school administrators may be motivated to inflate pupil numbers (IOB, 2008; Tamusuza, 2011). However, as for the data on primary schools, we are taking the difference in the number of actual primary schools in the district (as opposed to the number of respondent schools) and if it is true that the number of primary schools might have been understated, it is also true that this might have been understated in both academic years, thus making the difference in this number close to reality.

4 Identification strategy

The mother’s year of birth and the region in which she started and completed primary school together determine the exposure to the reform. As for the mother’s year of birth, we present a separate discussion for Malawi and Uganda.

Malawi operates on an 8-4-4 systems: primary school is made up of eight years (referred to as standard 1 to standard 8) and the entry age is 6 years. There were three school terms a year for primary schools, running generally from September to December, January to April, and April to July. The reform was implemented in September 1994 and tuition fees were abolished for all grades of primary school (standards 1-8). This implies that girls who were 14 and older in 1994 were not exposed to the reform, while girls who were 13 and younger in 1994 were exposed to the reform. Women born in 1980 and before were not exposed and women born in 1981 and after were instead exposed.

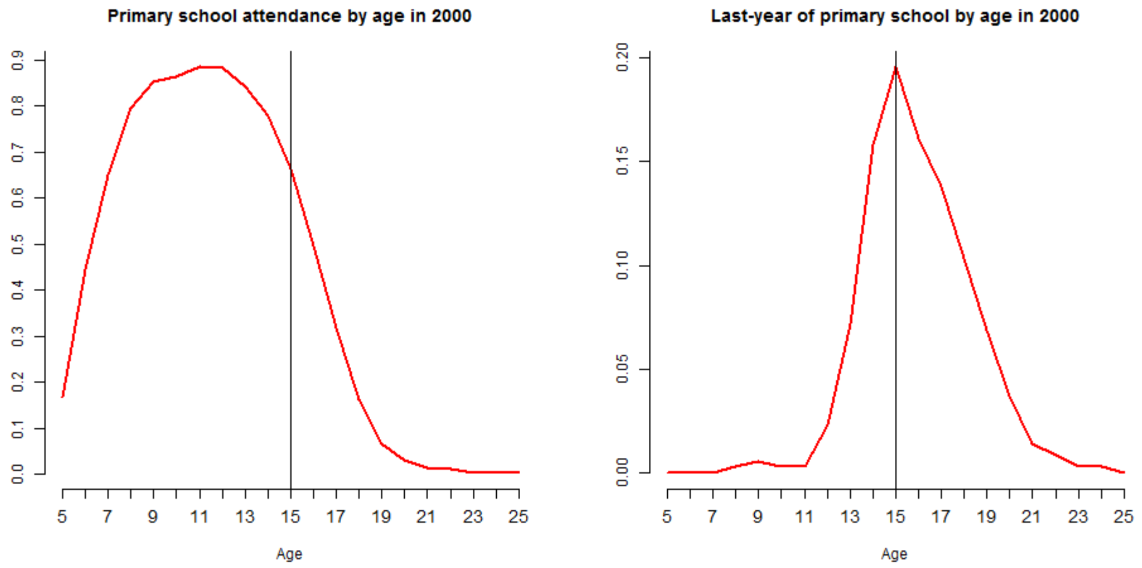
The system of education in Uganda has a structure of 7 years of primary education, 6 years of secondary education (divided into 4 years of lower secondary and 2 years of upper secondary school), and 3 to 5 years of post-secondary education. The academic year starts in February and ends in December and the entry age in primary school is 6 years old, so all children who turn 6 in the calendar year start school: the minimum age of entry is indeed five (UNESCO, 2015). The reform was introduced in January 1997 and according to it, families were exempt from paying primary-school fees for their children. Therefore, girls who were 13 and older at the time of the implementation were not exposed to the reform and girls who were 12 and younger at the time of the implementation were exposed to the reform: women born in 1984 and before were not exposed and women born in 1985 and after were exposed.

This approach requires the strong assumption that there are no grade repetition or late entry into primary school. If this assumption is invalid, our treatment and control groups would be incorrectly specified and our first stage analysis would be unreliable. The evidence shows that this assumption is not plausible in none of the two countries. When examining the role of women's education on fertility and child health outcomes in Uganda, Keats (2014) considers women born in 1982 and before as not exposed and those born in 1983 and after as exposed, thereby setting 1983 as cut-off year. He justifies his choice by arguing that other studies (Grogan, 2006, 2009) have proved that UPE in Uganda had an impact on school starting age: UPE increases the probability of a child entering school before age 8 by 9%. Moreover, other studies (Guarcello, Rosati, Breglia, & Ssennono, 2008; Tamusuza, 2011) analysing the effect of the reform in Uganda show that children attend primary school when they are older than the officially recommended age as a result of grade repetition, long-term absenteeism, and late entry into school and that the exit age from primary school is 16 (Tamusuza, 2011, p.125). Indeed, according to the UNESCO report the entry age for primary education usually varies between five and seven years (UNESCO, 2015). A similar argument applies to Malawi, where the evidence shows us that 50% of girls who are 14 years old (the official age of exit from primary school) are in sixth grade instead of eight grade, which suggests the actual age of exit from primary school to be 16 instead (Malawi Ministry of Education, 2004, p.17). Fig. 1 provides plausible support of this. According to the 2000 MDHS and the 2000/01 UDHS, the primary school attendance of girls started to decline considerably at the age of 15 in the primary school attendance (graph on the left) and that the proportion of girls who were in their last year of primary education is high until the age of 15, after which it starts to decline (graph on the right), thereby suggesting that the actual age of exit from primary school in Malawi and Uganda was effectively 16 (Fig. 1a-b). This leads me to use a treatment and a control group different from those specified above for both countries. The subsequent analysis considers women who were 15 and younger at UPE as exposed and women who were 16 and older at UPE as non exposed, in particular it compares the outcomes for women who were 11-15 years old to the outcomes for women who were 16-20.[20]

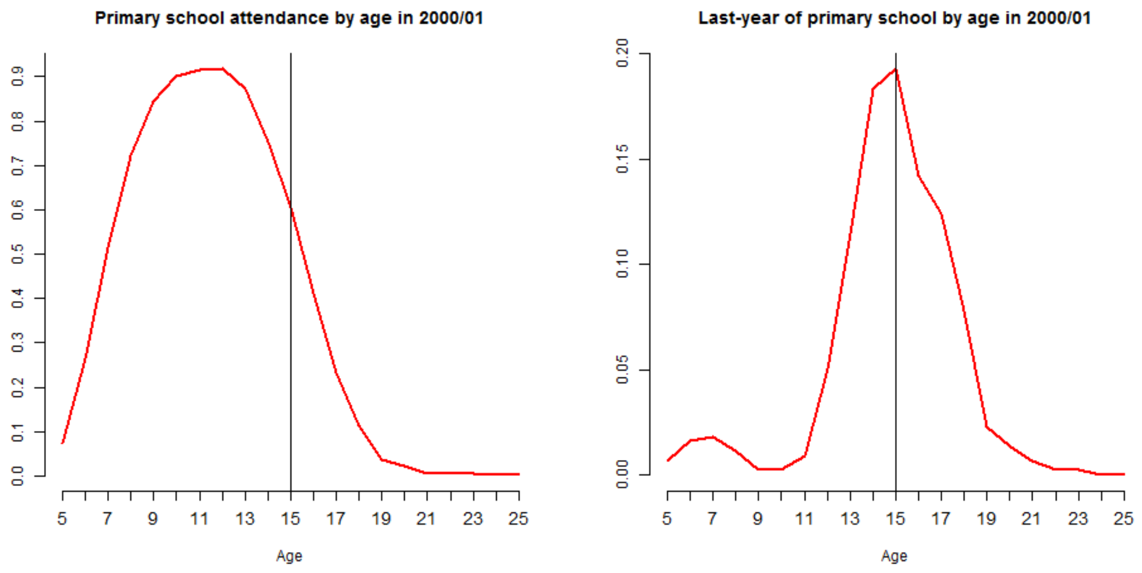
[20] The approach used by Grépin & Bharadwaj (2015) is also very interesting: in analysing the effect of free secondary education in Zimbabwe, they compare the outcomes for women who were 9-13 years old to the outcomes for women who were 16-20 so as to exclude those women aged 14-15 who were partially exposed to the reform. However, if applied to our study, this strategy would compare the outcomes for women too far in time, thereby not ensuring that the women only differ for their exposure to the reform.

Fig. 1. Fraction of girls in primary school by age and fraction of girls in last-year of primary school by age.[21]

a. Malawi



b. Uganda



As for the region in which the mother started and completed primary school, we follow the strategy proposed by Duflo (2001) according to which individuals born in a region where primary school enrolment rates just before the reform were low are also more likely to be educated in the region because exposed to a high-intensity program. Unfortunately, the DHS does not contain questions on the region where the mother started and completed primary school; however it includes data on her region of residence that we use as proxy of the region in which she started and completed primary school under the assumption that she has never moved since the year she started primary school. This

[21] The 2000 MDHS and 2000/01 UDHS Household Surveys were used to produce these graphs.

seems to be a plausible assumption given that the last census in Malawi (2008) and Uganda (2002)[22] shows that the proportion of internal migrants is accounting for respectively 16.2% and 13% of the total population of the country (National Statistical Office Malawi, 2009; Uganda Bureau of Statistics, 2002). However, higher shares of internal migrants are found in the capital cities: 52.2% and 18% of the population Lilongwe and Kampala migrated from other districts. Therefore, although the results displayed in this study will include the capital city, the analysis will be done with and without it in order to account for possible measurement error as robustness check.
In order to capture the intensity of the program by region, we use the change in the number of primary schools between after and before the reform per 100 children by administrative unit. We do not weigh this measure for the change in the population because of unreliable data on it, but we assume that the population has increased proportionally across districts; this appears a plausible assumption given the low rate of migration within the country. The districts that saw an increase in the number of primary schools per 100 children are those that experienced a significant expansion in educational inputs, i.e. high-intensity regions, and are also those that had lower primary school enrolment rates prior to the program.

5 Methods

The objective of this study is to examine the relationship between maternal education and child mortality and its nature. However, doing this by using ordinary least squares may produce a biased estimate. The absolute value of this estimate may be biased upward due to unobserved factors such as household wealth or maternal ability, that influence maternal education positively and child mortality negatively, or biased downward because of measurement errors in the education level. In order to obviate this issue, in this study we take advantage of the timing of the Universal Primary Education reform and instrument mother's education using her exposure to the reform. The location above or below the threshold (age 15) only makes individuals eligible for the treatment, but the decision to take the treatment is mainly up to their family: eligible girls may also not be enrolled in primary school because they had already entered the labour market or marriage market or because of indirect costs associated with education, as opposed to ineligible girls who may have been exposed to the reform because they were still in primary school when UPE was introduced. This non-compliance with the treatment assignment makes this design a fuzzy regression discontinuity design (RDD) (Imbens & Lemieux, 2008).

The fuzzy RDD considers the discontinuity in the probability of exposure to the reform conditional on birth cohort as an instrumental variable for treatment status. The estimate can be obtained using a two-stage least squares (2SLS) estimation strategy. Our outcome variable of interest is the risk of death in childhood (0-59 months), measured as the duration from birth to the age at death, or censored since children who were still alive at the time of the interview were right censored. Therefore, we will use a survival model to account for right censoring in the estimation of exposure time (Allison, 1982). In a linear setting the two-stage residual inclusion (2SRI) approach[23] is identical to the 2SLS (Greene, 2003; Terza, Basu, & Rathouz, 2008) and yields consistent estimates in the context of survival models (Atiyat, 2011). The first stage of 2SRI is the following linear regression model:

$$S_{ik} = c + \alpha_k + d_i\lambda + (P_k * d_i)\gamma + x'_i B + \nu_{ik}$$

where α_k is the mother's region of residence effect, S_{ik} is the endogenous variable, years of education, P_k is the level of the program in the mother's region of birth, d_i is a dummy variable taking value 1

[22] The last census was taken in 2014; however, results are not yet available.
[23] The 2SRI approach was proposed by Hausman (1978) to test for endogeneity in linear models.

if the mother is exposed to the reform and 0 otherwise. x'_i is a vector of control variables: sex of the child, birth order[24], religion, year of survey fixed effects, and child's year of birth fixed effects. After the estimation of the first stage, the residuals $\hat{S}_{v_{ik}}$ are calculated as follows:

$$\hat{S}_{v_{ik}} = S_{ik} - \hat{S}_{ik}$$

In the second stage, the endogenous variable, the exogenous variables, and the residuals are included in the estimation of the Cox proportional hazards model for right censored data (Cox, 1972):

$$h(t; S_{ik}, x'_i, \hat{S}_v) = h_0(t)\exp(\alpha_k + \beta_1 S_{ik} + x'_i B + \beta_2 \hat{S}_v)$$

where $h_0(t)$ is an unspecified baseline hazard function. β_1 is the treatment effect on compliers, also called local average treatment effect. It is effect of schooling on the probability of dying for compliers (i.e., girls going to primary school if eligible, and not going if not eligible). The window of people for our main sample[25] will be specified in the Results Section and will be the same in the first and second stage, but we also test for smaller and larger windows as robustness checks. The standard errors will be calculated using the clustered sandwich estimator so as to control for the dependency of each child within district by specifying to which district the child belongs to. In order to have a valid estimate, two assumptions will have to be met: i) the instrument is correlated with the endogenous variable (relevance); ii) the instrument is uncorrelated with any other determinants of the dependent variable (exclusion restriction). In the next Section, we will present evidence supporting the non-violation of these assumptions.

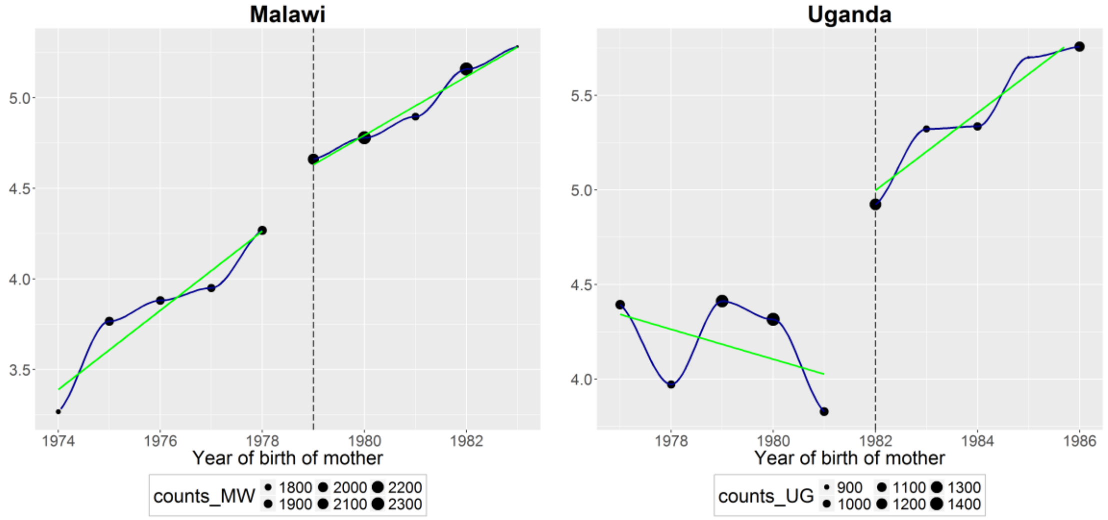
6 Results

Fig. 2 charts the educational attainment by mothers' year of birth; we also plot a linear and polynomial trend line estimated separately for women exposed to the reform and for women not exposed to the reform. Women born after 1979 and 1982 respectively in Malawi and Uganda have more education than women born before. On average, women born after 1979 and 1982 respectively in Malawi and Uganda are more educated compared to women born before the cut-off year.

[24] The DHS data include information on the birth order number, thereby giving the order in which the children were born and for which we will control. If mothers, who are exposed to the reform, have fewer children because they are younger, non-exposed mothers have more children because older. Thus, if for younger mothers we are considering the health outcome of their first births, for older mothers we are considering the health outcome of their last births, which are not necessarily the first births, in fact there are low chances that they are the first births. So, although the outcome of interest is the same for children of exposed and not exposed mothers, there might be differences in outcomes that are a result of the presence of other siblings in the household.

[25] This choice comes with a trade-off: a large window will lead to a larger sample, but it does not ensure that the women only differ for their exposure to the reform; on the contrary, a smaller window will ensure the comparability of the women, but it will also give us a smaller sample.

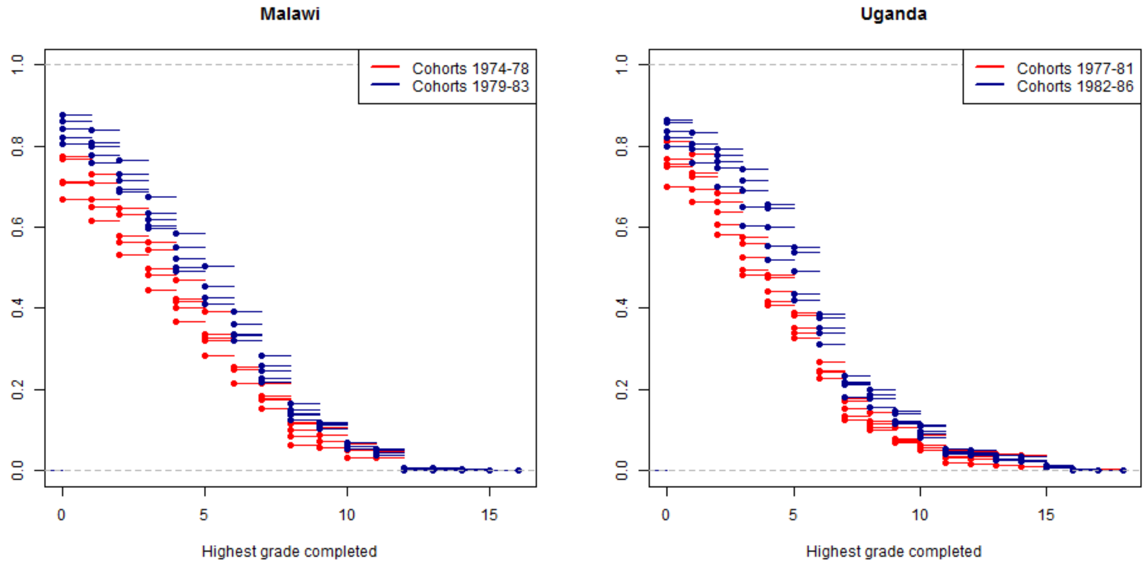
Fig. 2. Number of years of education based on the year of birth of mothers.



I plot the reverse cumulative distribution function of the years of education attained by birth year cohort for the treated and non-treated women (Fig. 3a) and the difference in these functions (Fig. 3b). It is evident that there is a greater change in the distribution for women exposed to the reform compared to women not exposed to the reform and that more women in the first group tend to have more education.

Fig. 3. Reverse empirical cumulative distribution function of education based on the year of birth of mothers.

a. Reverse CDF of education by birth cohort



b. Difference in reverse CDF of education between the groups

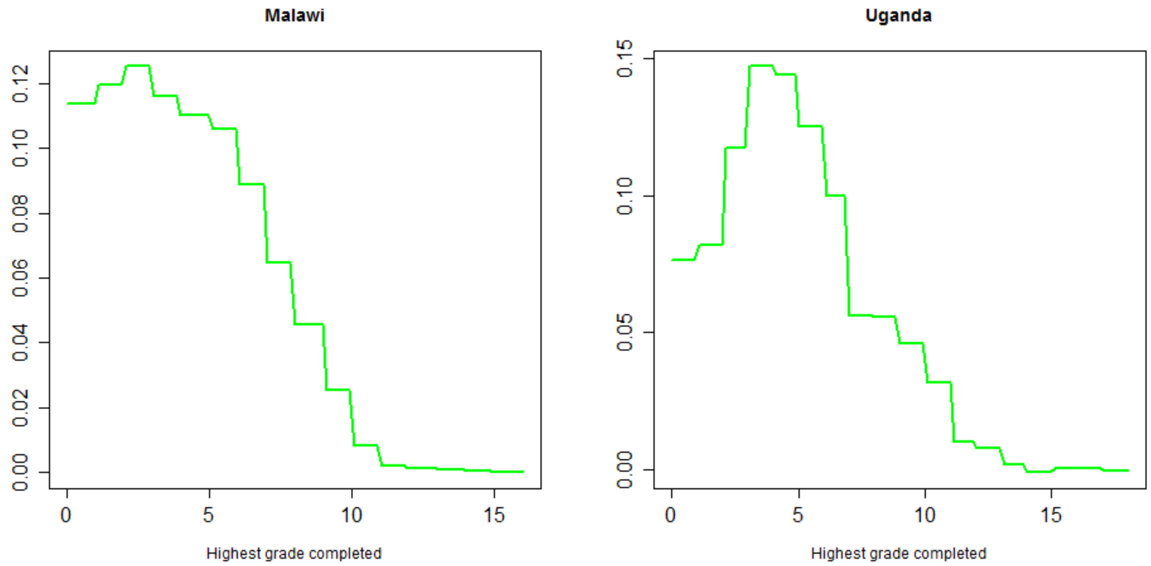


Fig. 4 shows the trend in the probability of dying before age five over the mothers' year of birth. On average, mortality decreases for children of mothers born after 1979 and 1982 in Malawi and Uganda, respectively. We have to keep in mind that this figure only represents the change in child mortality by year of birth of the mother, it does not tell us the change in child mortality due to an increase in education.

Fig. 4. Child mortality based on the year of birth of mothers.

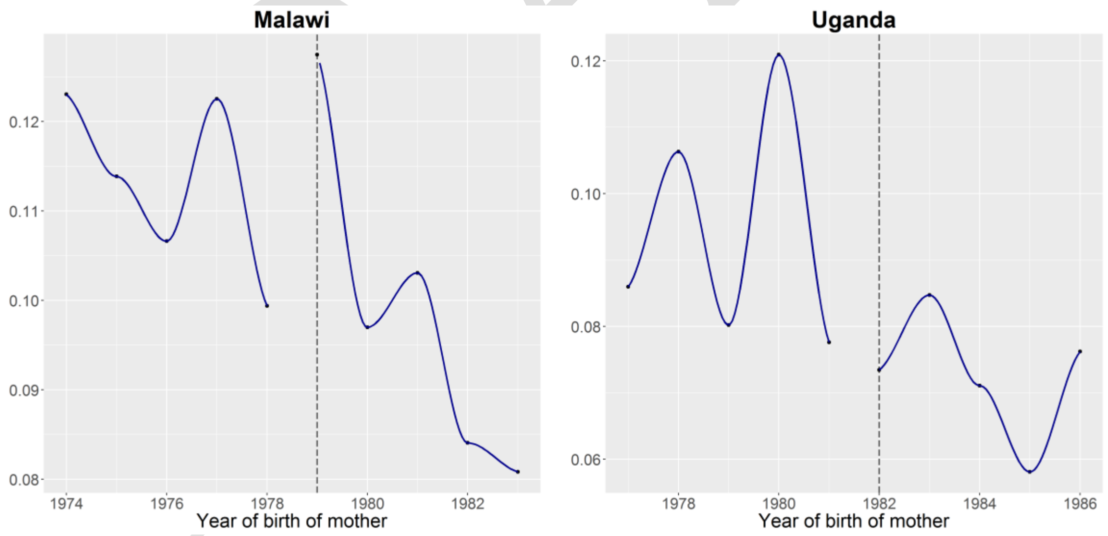
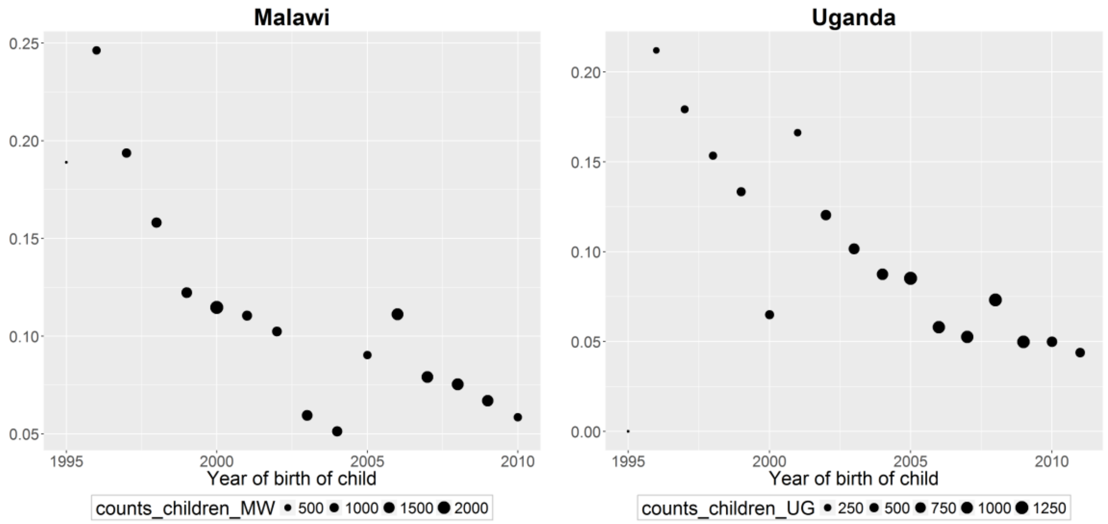


Fig. 5 shows child mortality in Malawi and Uganda by year of birth of the child. These levels are likely to be underestimated because of omission and backward displacement of births, particularly relevant in sub-Saharan Africa (Pullum, Schoumaker, Becker, & Bradley, 2013; Schoumaker, 2011); however, these issues affect recent (in the 5-year period prior the survey) births equally over year of birth, thereby not being a problem here.[26] In both countries, there is a declining trend in child mortality

[26] Schoumaker (2011) finds that omissions of recent births are more common among less-educated women; we will discuss the implications of this finding in the Section on the empirical model.

over year of birth, thus in our analysis we include child's year of birth fixed effects to control for year-specific trends that could have affected children.

Fig. 5. Child mortality based on the year of birth of children.



6.1 First stage

This sub-section includes the results of the first stage estimation, i.e. impact of the reform on maternal education. Table 3 shows different specifications of the empirical model for Malawi and Uganda. The baseline model includes the sex of the child, religion[27], and the birth order, as well as the year of survey fixed effect in order to control for the fact that several surveys per country have been merged and the child's year of birth fixed effect so as to deal with year-specific trends that could have affected children. We also included the mother's district of residence fixed effect in order to control for time-invariant unobserved characteristics at the district level, such as the initial level of economic development of the district. The second specification includes the mother's year of birth fixed effects in order to control for other government policies or trends that occurred during that period that might have affected the educational level of women. The treatment binary variable is null when the mother's year of birth fixed effect is included. In the third specification, we add the initial gross enrolment rate at district level because we expect that educational attainment would grow more in districts where fewer pupils were in school before UPE. The fourth specification includes all variables. The reported standard errors are clustered at district level. Panel A (the top panel) uses years of education as dependent continuous variable, whereas Panel B (the bottom panel) uses the educational attainment[28] as dependent continuous variable. We use these two different definitions of education because the reform might have increased both the number of years of education but also the probability of enrolling in secondary and higher school and thus of achieving higher levels of education.
The results in Panel A for Malawi show that one school per 100 children at the time of the reform increases the years of education of the youngest women (i.e. those who were 11-15 years old at the time of the reform) by 0.42 years (Column 1). The effect slightly increases to 0.44 when the mother's year of birth fixed effect and gross enrolment rate are included (Column 4). The reform has increased the level of education of exposed women by 0.19 (Column 8). In Uganda (Panel B), the reform caused

[27] For Uganda, we could not include religion because in the 2009 DHS-MIS this question was not asked.
[28] In Malawi, the educational levels are: 1 if no education, 2 if lower primary education (1-4 years of education), 3 if upper primary education (5-8 years of education), 4 if lower secondary education (9-10 years of education), and 5 if upper secondary education and higher. In Uganda, the educational levels are: 1 if no education, 2 if primary education, 3 if lower secondary education (8-11 years of education), and 4 if upper secondary education and higher.

an increase in years of education of 0.55 years and in the level of education of 0.07 for the youngest women (i.e. those who were 11-15 years old at UPE).

Table 3. First-stage results in Malawi and Uganda.

A. Malawi[A]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	0.4203*	0.3949	0.4642**	0.4389**	0.1845**	0.1778**	0.2007***	0.1941***
	(0.2549)	(0.2520)	(0.2231)	(0.2164)	(0.0821)	(0.0822)	(0.0651)	(0.0641)
Treatment	0.5048***		0.9420***		0.1297***		0.2911***	
	(0.1178)		(0.3100)		(0.0386)		(0.0970)	
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6911	0.6917	0.6912	0.6919	0.8695	0.8698	0.8696	0.8698
Observations	19,682	19,682	19,682	19,682	19,682	19,682	19,682	19,682

B. Uganda[B]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	NA	NA	NA	NA	NA	NA	NA	NA
Treatment	0.5528***		NA		0.0693**		NA	
	(0.0441)				(0.0294)			
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.7137	NA	NA	NA	0.8988	NA	NA	NA
Observations	11,260	NA	NA	NA	11,260	NA	NA	NA

Note: NA = not applicable.
A) Treatment: age 11-15 in 1994; control: age 16-20 in 1994.
B) Treatment: age 11-15 in 1997; control: age 16-20 in 1997.
* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

6.2 Robustness tests

I also run several robustness tests in order to verify that our results are not driven by inappropriate identification assumptions. Table 4 shows that the results are robust when closing the window data by one year (Panels A-B) and when opening the window data by one year (Panels C-D). For the purpose of this study, the second stage has used the residuals obtained from the estimation in Table 3; however, the second stage was also estimated using the residuals in Table 4, and the results are very similar.

In Table 5 we show the results of the first stage when the capital city is excluded from the sample. This is done since our analysis uses the district of residence as place where the woman was educated. Even if the proportion of internal migrants is low in both countries, the higher shares of internal migrants found in the capital cities might have biased our results. However, this is not the case: the results are robust when excluding the capital cities from the sample.

Our analysis assumes that the reform was the only factor increasing education; however, education might have increased because of other factors and not necessarily as a result of the reform. Thus, the third test aims at controlling that before the reform, education was not increasing faster in high-intensity areas. Based on Duflo (2001), we compare women who were in our control group with women who were even older at the time of the reform. The former are now our treatment group and the latter are our new control group. The results in Table 6 show that in both countries, there is not a significant positive effect of our instrument on years and level of education, thereby confirming that education was not increasing faster in high-intensity areas.

Table 4. First-stage results – Robustness check: different bandwidths.

A. Malawi[A]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	0.4274	0.4244	0.4611*	0.4582*	0.2005**	0.2002***	0.2114**	0.2113***
	(0.2919)	(0.2868)	(0.2637)	(0.2572)	(0.0947)	(0.0621)	(0.0947)	(0.0820)
Treatment	0.4084***		0.7433**		0.1010**		0.2098***	
	(0.1343)		(0.3508)		(0.0440)		(0.0440)	
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6908	0.6911	0.6909	0.6912	0.8698	0.8699	0.8699	0.8700
Observations	16,251	16,251	16,251	16,251	16,251	16,251	16,251	16,251

B. Uganda[B]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	NA	NA	NA	NA	NA	NA	NA	NA
Treatment	0.5363***		NA		0.0787**		NA	
	(0.1663)				(0.0306)			
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.7025	NA	NA	NA	0.8969	NA	NA	NA
Observations	8,987	NA	NA	NA	8,987	NA	NA	NA

C. Malawi[C]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	0.4570*	0.4320*	0.4973**	0.4725**	0.1819**	0.1754**	0.1966***	0.1902***
	(0.2595)	(0.2605)	(0.2318)	(0.2305)	(0.0815)	(0.0823)	(0.0677)	(0.0678)
Treatment	0.5560***		0.9334***		0.1464***		0.2845***	
	(0.1306)		(0.3309)		(0.0414)		(0.1012)	

Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6955	0.6962	0.6956	0.6963	0.8718	0.8720	0.8719	0.8721
Observations	22,841	22,841	22,841	22,841	22,841	22,841	22,841	22,841

D. Uganda[D]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	NA	NA	NA	NA	NA	NA	NA	NA
Treatment	0.5917***		NA		0.0784***		NA	NA
	(0.1495)				(0.0255)			
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.709	NA	NA	NA	0.8975	NA	NA	NA
Observations	13,329	NA	NA	NA	13,329	NA	NA	NA

Note: NA = not applicable.

A) Treatment: age 12-15 in 1994; control: age 16-19 in 1994.

B) Treatment: age 12-15 in 1997; control: age 16-19 in 1997.

C) Treatment: age 10-15 in 1994; control: age 16-21 in 1994.

D) Treatment: age 10-15 in 1997; control: age 16-21 in 1997.

* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

Table 5. First-stage results – Robustness check: no capital city.

A. Malawi[A]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	0.3147	0.2919	0.4002**	0.3781*	0.1406	0.1346	0.1722***	0.1664**
	(0.2669)	(0.2700)	(0.2267)	(0.2239)	(0.0882)	(0.0904)	(0.0666)	(0.0677)
Treatment	0.5486***		0.9236***		0.1432***		0.2817***	
	(0.1146)		(0.3200)		(0.0374)		(0.0991)	
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6904	0.6909	0.6905	0.6911	0.8706	0.8708	0.8707	0.8709
Observations	18,587	18,587	18,587	18,587	18,587	18,587	18,587	18,587

B. Uganda[B]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	NA	NA	NA	NA	NA	NA	NA	NA
Treatment	0.6494***		NA		0.0845***		NA	
	(0.1394)				(0.0156)			
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6895	NA	NA	NA	0.8968	NA	NA	NA
Observations	10,481	NA	NA	NA	10,481	NA	NA	NA

Note: NA = not applicable.
A) Treatment: age 11-15 in 1994; control: age 16-20 in 1994.
B) Treatment: age 11-15 in 1997; control: age 16-20 in 1997.
* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

Table 6. First-stage results – Robustness check: different treatment and control groups.

A. Malawi[A]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	0.2497	0.2834	0.2238	0.2576	0.0510	0.0596	0.0445	0.0531
	(0.2408)	(0.2340)	(0.2401)	(0.2322)	(0.0839)	(0.0826)	(0.0831)	(0.0814)
Treatment	0.4200***		0.1602		0.1416***		0.0765	
	(0.1105)		(0.3013)		(0.0380)		(0.1014)	
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6183	0.6202	0.6183	0.6203	0.8493	0.8501	0.8493	0.8501
Observations	15,370	15,370	15,370	15,370	15,370	15,370	15,370	15,370

B. Uganda[B]

Dependent variable	Years of education				Level of education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Intensity measure	NA	NA	NA	NA	NA	NA	NA	NA
Treatment	-0.1884		NA		-0.0219		NA	
	(0.1219)				(0.0219)			
Mother's year of birth fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Gross enrolment rate	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.6482	NA	NA	NA	0.8913	NA	NA	NA
Observations	12,694	NA	NA	NA	12,694	NA	NA	NA

Note: NA = not applicable.
A) Treatment: age 16-20 in 1994; control: age 21-25 in 1994.
B) Treatment: age 16-20 in 1994; control: age 21-25 in 1997.
* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

6.3 Second stage

This sub-section focuses on the impact of maternal education on child mortality. We estimate four different survival models for each country. The first and second model estimate a simple Cox regression model so as to show the bias in the estimate we would obtain by not taking into account the endogeneity of the variable education. Model 1 includes the gross enrolment rate and Model 2 adds the mother's year of birth fixed effect. The third and fourth model use the IV method by adding the residuals of the first stage estimation in the Cox regression model. Similar to other tables above, all models include sex of the child, birth order, year of survey fixed effect, child's year of birth fixed effect, and mother's district of residence fixed effect. The bandwidth is the same as the bandwidth specified in Table 3 and the standard errors reported here are clustered at district level.

Coherent with the literature, the simple Cox model suggests that increased female education has a negative impact on child mortality in both Malawi and Uganda (Columns 1-2, Panel A-B, Table 7). In particular, in Malawi a one-year increase in education significantly reduces the probability of dying before age 5 by about 4%. The results in Columns 3-4, Panel A, account for the endogeneity of education; they show that the effect is even larger: the odds of dying for children of women with one additional year of education are 66% lower. These results suggest that the simple Cox analysis may underestimate the magnitude of maternal education on child mortality. However, the effect is no longer statistically significant. In Uganda, for each additional year of education, children have a 3% lower probability of dying. However, Column 3, Panel B, shows the results when we control for the endogeneity of the education variable: the causal relationship between maternal education and child mortality is still negative (the odds of dying for children of women with one additional year of education are 0.48% lower), but slightly weaker and not statistically significant.

To sum up, in both Malawi and Uganda there is not a negative statistically significant effect of maternal education on child mortality. But in the former we find that we may incur in an underestimation of the effect of schooling on mortality probably due to measurement errors in education.

Table 7. Second-stage results in Malawi and Uganda: years of education.

A. Malawi

Model	Simple Cox model		Instrumental variable	
	(1)	(2)	(3)	(4)
Years of education	0.9615***	0.9617***	0.3556	0.3411
	(0.0094)	(0.0094)	(1.0100)	(1.0680)
Mother's year of birth fixed effect	No	Yes	No	Yes
Gross enrolment rate	Yes	Yes	Yes	Yes
Observations	19,682	19,682	19,682	19,682

B. Uganda

Model	Simple Cox model		Instrumental variable	
	(1)	(2)	(3)	(4)
Years of education	0.9670***	NA	0.9952	NA
	0.0087		0.1433	
Mother's year of birth fixed effect	No	Yes	No	Yes
Gross enrolment rate	No	Yes	No	Yes
Observations	11,260	NA	11,260	NA

Note: NA = not applicable.

* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

In this analysis, we are assuming that the effect across years of education is homogenous, so that the effects of one additional year of education from 0 to 1 and from 1 to 2 are the same. We run an additional analysis in which we look at whether a one-level increase in education can significantly reduce child mortality. Indeed, a higher level of education attained by the mother can improve her chances of getting a job which in turn can allow her to afford more health services for her and for her children. Table 8 displays the results obtained with educational level as explanatory continuous variable. The simple Cox model (Columns 1-2) suggests that each additional level of education is associated with a significant 12% lower probability of dying before age five in Malawi. In Columns 3-4, Panel A, we show that the effect is even more remarkable: a one-level increase in education causes a decrease in the odds of dying before age five of 91%. These results suggest that the simple Cox analysis may underestimate the magnitude of maternal education on child mortality. However, the effect is not statistically significant. In Uganda a one-level increase in education is significantly associated with a reduction in the probability of dying before age five by about 10%. In Column 3, Panel B, we account for the endogeneity of education and the results show that the effect is smaller (the odds of dying for children of women with one additional level of education are 0.17% lower) and no longer statistically significant.

To sum up, in both Malawi and Uganda there is not a negative statistically significant effect of maternal education on child mortality. But in the former we find that we may incur in an underestimation of the effect of schooling on mortality probably due to measurement errors in education.

Table 8. Second-stage results in Malawi and Uganda: level of education.

A. Malawi

Model	Simple Cox model		Instrumental variable	
	(1)	(2)	(3)	(4)
Level of education	0.8775***	0.8783***	0.0906	0.0870
	(0.0293)	(0.0295)	(2.3450)	(2.4230)
Mother's year of birth fixed effect	No	Yes	No	Yes
Gross enrolment rate	Yes	Yes	Yes	Yes
Observations	19,682	19,682	19,682	19,682

B. Uganda

Model	Simple Cox model		Instrumental variable	
	(1)	(2)	(3)	(4)
Level of education	0.9026**	NA	0.9983	NA
	0.0423		1.1490	
Mother's year of birth fixed effect	No	Yes	No	Yes
Gross enrolment rate	No	Yes	Yes	Yes
Observations	11,260	NA	11,260	NA

Note: NA = not applicable.
* Significant at 0.1 level, ** Significant at 0.05 level, *** Significant at 0.01 level. The standard errors (in parenthesis) are clustered at district level. The baseline model includes sex of the child, birth order (binary), religion (categorised as Catholic, Presbyterian, Muslim, Other Christian, No religion and Other for Malawi; not included for Uganda), year of survey fixed effect, child's year of birth fixed effect, and mother's region of residence fixed effect.

The empirical analysis is suggesting that neither additional years of education nor higher levels of education can significantly reduce child mortality levels. Another interpretation of these results can be that other factors may be necessary preconditions for more education to contribute to improved survival chances. In the next sub-section we discuss the possible explanations of these results.

6.4 Discussion

In Malawi, the government promptly replied to the extraordinary increase in the enrolment rate in 1994/95, with a more than proportional increase in the number of teachers (1.5 as opposed to 1.6 times increase). At first glance, this provision might appear satisfactory. However, about half of new teachers were not trained (compared to 16% in 1993-94) and for the other half, training was not adequate (Avenstrup et al., 2004; Inoue & Oketch, 2008; EMIS, 1994). In addition, in the year of the introduction of UPE, there was a shortage of books, school materials (e.g. maps, mathematical sets, and globes), and permanent classrooms, which led to more intensive usage of existing facilities (Kadzamira & Rose, 2003; Inoue & Oketch, 2008). Furthermore, Kadzamira & Rose (2003) observed that teachers did not feel accountable for their job responsibilities with parents as they were not paying for education anymore. Therefore, the government's answer to the influx of new pupils does not seem to have been effective, making their learning opportunities lower than in the previous cohorts.

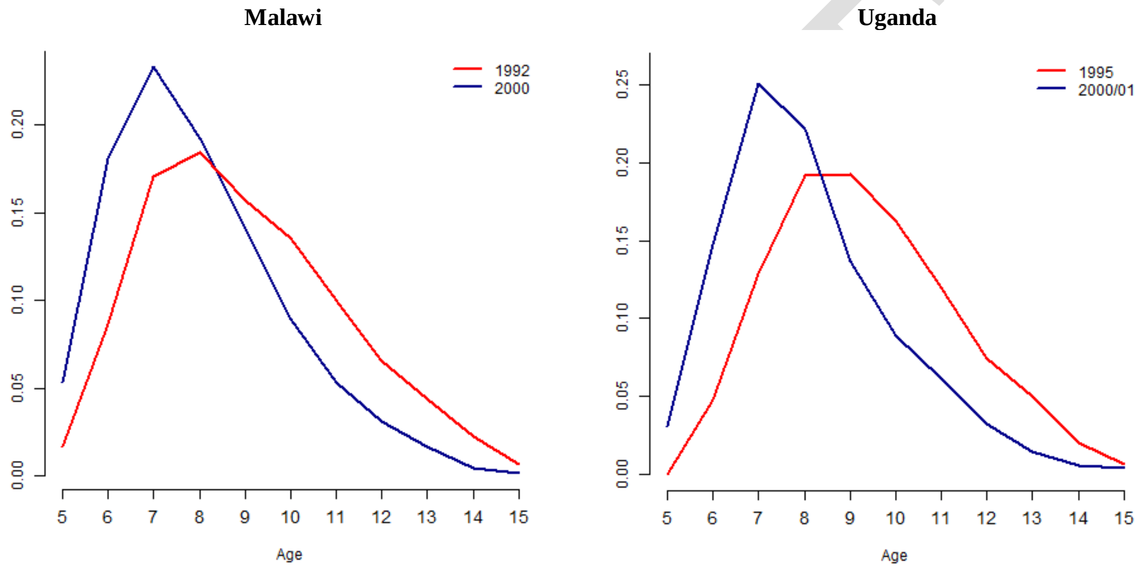
In Uganda, the access shock to education caused an initial decrease in various indicators referring to the resources available to pupils; in particular, the increase in the sectoral budget in the years following the reform was not sufficient to reduce the textbook-pupil and teacher-pupil ratios. According to the National education statistics cited in Avenstrup et al. (2004, p.14), the enrolment rate increased by 140% between 1996 and 2002, but the number of primary teachers and classrooms respectively increased by only 71% and 55% (from 81,564 to 139,484 and 45,115 to 69,990) in the same period. This would suggest that the introduction of the reform decreased the quality of education.

Another indicator of the quality of education is the students' performance. In Malawi, the National Primary School Leaving Certificate Examination pass rate was increasing in the years preceding the reform, i.e. 62.7%, 67.1, and 79.3 respectively in 1991-92, 1992-93 and 1993-94, before decreasing again to 72.6% and 61.7% in 1994-95 and 1995-96, respectively (EMIS various years). In Uganda, two independent national assessment monitoring the learning progress, one carried out by Uganda National Examinations Board and the other funded by UNESCO/UNICEF, indicated a substantial decline in the number of pupils meeting the minimum standard in English and Mathematics compared to pre-UPE levels (World Bank, 2004). Moreover, Grogan (2006), studying the consequences of the introduction of UPE in Uganda, finds that the probability of children exposed to UPE to successfully complete a reading test fell by 10%.

The literature also reports that male and female teachers' expectations over girls' academic ability are found to be lower than those on boys. For example teachers tend to involve boys more than girls because they are perceived to be more intelligent and motivated (Chimombo et al., 2000). This suggests that the quality of education might have been even lower for girls. Moreover, Sey (1997) and Rose (2002) observed that in Malawi girls are given more domestic duties, hence having less time to devote to their studying.

An alternative explanation is offered by the change in the enrolment age and the consequences related to this. In Uganda, it was found that UPE increased the probability of a child entering school before age 8 by 9% (Grogan, 2006, 2009). Fig. 6 also shows that the proportion of children enrolling in primary school earlier has been increasing over time in both Malawi and Uganda. As a result the average age of exiting school lowers, as does the age at which women establish a family. Grant (2015), instrumenting women's education using the exposure to the educational reform in Malawi, finds that a one-year increase in education increases the probability of giving birth by age 18 and 20 by 9% and 11%, respectively. Therefore, it is likely that the advantage acquired by the more years or level of education might have been counterbalanced by the risks related to early childbearing. Finlay et al. (2011) find that in developing countries children born to adolescent mothers have poorer health outcomes and that this gap is persistent across socioeconomic groups. So, even if the treated women are more educated, not only might they start childbearing before, but their higher socioeconomic status resulting from their more education might not exert any protective role.

Fig. 6. Fraction of girls in first-year of primary school by age.[29]



7 Conclusion

In this paper, we investigate the causal effect of maternal education on child mortality. To do so, we take advantage of the educational reforms that introduced free primary education in Malawi and Uganda in 1994 and 1997, respectively. We instrument maternal's years of education and level of education using the mother's exposure status at the time of the reform interacted with the investment made in primary infrastructure (i.e. the number of primary schools per 100 children constructed) in the years following the reform in the district of education of the woman. In this way, we exploit the district variation in the intensity of the reform. In both countries, we find that the reform caused an increase in both the years of education and the level of education, in particular we find that for each additional school per 100 children, younger women experienced a 0.44 significant increase in the years of education and a 0.19 significant increase in the level of education in Malawi. In Uganda, we find that the reform caused an increase in years of education of 0.55 years and in the level of education of 0.07 for the youngest women. The results are robust to different bandwidths, specifications, and tests, so we are confident that the assumption that the reform has increased education is valid.

In the second stage of our analysis, we model child mortality using a Cox model. This study is the first application of the two-stage residual inclusion method to study the effect of maternal education on child mortality. In Malawi, these estimates are higher than the estimates obtained ignoring the

[29] The 1992 and 2000 MDHS and 1995 and 2000/01 UDHS Household Surveys were used to produce these graphs.

endogeneity problem; for each additional year of education, children have a 66% lower probability of dying, but the effect is not significant. In Uganda, the IV estimates show that the odds of dying for children of women with one additional year of education are 0.48% lower; however, this effect is not statistically significant. We also study the effect of educational level on child mortality in order to analyse whether a one-level increase in education has a significant effect on child mortality. In Malawi, we find that the odds of dying for children of women with one additional level of education are 91% lower, but the effect is not statistically significant. For each additional level of education, children have a 0.17% lower probability of dying in Uganda, but again the effect is not statistically significant.

These results are not in conflict with what the less recent literature exploring the causal effect of maternal education on child mortality has shown. Strauss (1990), analysing data on extended families living together, finds that the association between education and weight given height is attenuated when controlling for unobserved characteristics at household level. Horton (1988) and Wolfe & Behrman (1987) use data on sisters respectively in the Philippines and Nicaragua and control for unobserved family background characteristics to show that the impact of maternal education on child health outcomes completely disappears. Our results go in this direction and confirm that it is worthwhile taking the hypothesis that the effect of maternal education on child mortality in absolute value has been overstated.

However, more recent studies exploiting the exogenous access variation in education as result of policy changes (e.g. new school construction or changes in the length of compulsory education) have found that an increase in parental education leads to a reduction in neonatal and child mortality (Breierova & Duflo, 2004; Chou et al., 2010; Grépin & Bharadwaj, 2015). Among these studies, however, Keats (2014) finds strong evidence that a one-year increase in women's education rises their first-born children's health outcomes, but he finds no evidence of a significant effect of maternal education on the probability of dying before age one and five.

In conclusion, even if our results speak in favour of a negative but non-statistically significant effect of maternal education on child mortality in Malawi and Uganda, we have to bear in mind that there might be other factors that have prevented treated mothers to protect their children. In other words, other factors may be necessary preconditions for more education to contribute to improved survival chances. In particular, we have observed that the quality of education provided to women who were exposed to the reform has decreased after the introduction of UPE in Malawi and Uganda. All sub-Saharan African countries that have implemented UPE reforms have experienced a deterioration in the quality due to lack of adequate teacher training, low morale of teachers, inadequate textbooks and learning materials, large class size, and increase pupil-teacher ratios (World Bank, 2009a). In addition, even though girls' participation has been promoted in Malawi through the school fee waiver program for non-repeating girls sponsored by USAID in 1992-93 and in Uganda through the decision to favour marginalised groups (i.e. girls and disabled), girls tend to be discriminated in school, and therefore their learning opportunities and outcomes may be compromised, regardless of any possible effort.

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