ESTIMATING THE EFFECT OF MATERNAL EDUCATION ON CHILD HEALTH AND FEMALE GENITAL CUTTING USING MICRODATA FROM AFRICA

A Dissertation

Submitted to the National Graduate Institute for Policy Studies (GRIPS)

in Partial Fulfillment of the Requirements for the Degree of

Ph.D. in Development Economics

by

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February 2021

ABSTRACT

This dissertation empirically examines how much an increase in education for African female has advanced child health and reduced female genital cutting on daughters. Identifying the causal effect of maternal education on child outcomes is challenging because unobserved family attitudes/attributes can affect maternal education, the decision to invest in child health, and the decision to have their daughters cut. Thus, identification requires empirical techniques to single out maternal education's variation uncorrelated with family attitudes/attributes.

Previous research has used data from one country and exploited its Free Primary Education (FPE) reform as a source of exogenous variation in maternal education across birth cohorts. However, several issues (e.g., health-promotion or poverty reduction policies under the Millennium Development Goal) might have confounded or invalidated the single-country analysis. Also, that country's previous trends in education and health might have driven at least part of the observed differences across birth cohorts. Furthermore, previous works lack precision due to the small sample size.

This dissertation overcomes the above challenges using data from twenty African countries implementing FPE and using IV methods with a triple difference design. The rollout of the FPE reforms across the continent over the decades provides extra leverage

to fix the country-specific trends (either related to the reform or unrelated but contemporaneous policies). Furthermore, this study incorporates the within-country variation in reform intensity across regions, measured by pre-reform primary school enrollment rates. This identification strategy exploits the triple differences in the reform exposure across countries, birth cohorts, and local reform intensities to tease out the exogenous variation in female school enrollment. This result allows this study to pin down how much maternal education can impact child outcomes, including daughters' genital cutting, using IV methods.

This dissertation discovers exposure to FPE increases female education by approximately one school year. This extra schooling reduces child mortality under age five and decreases stunting, underweight, or fever prevalence. Although similar to previous studies in patterns, most of this study's results are smaller and more precise than the previous estimates. The possible mechanisms through which maternal education affects child health include increased literacy, better health knowledge, and prolonged breastfeeding practices. The extra schooling also significantly reduces daughters' FGC prevalence rates because of changes in women's attitudes toward the practice and increases in women's bargaining power relative to their partners.

ACKNOWLEDGMENT

I wish to express my deepest gratitude to my principal supervisor, Professor Stacey H. Chen for her eruditeness, commitment, motivation, patience, and willingness to assist me throughout the entire dissertation writing.

I would also like to express my sincere gratitude to my other co-supervisors, Professor Stephan Litschig and Professor Chikako Yamauchi, for their constructive comments and suggestions that made me accomplish this dissertation. My gratitude extends to Professor Yoko Kijima, whose comments and suggestions at the beginning of this journey helped me to structure this dissertation.

My sincere thanks to Professors at GRIPS, especially, Yosuke Yasuda, Makoto Hasegawa, Minchung Hsu, Shinsuke Ikeda, Yoichi Arai, Roberto Leon-Gonzalez, Keijiro Otsuka, Chikako Yamauchi, who taught me various courses that greatly helped me in writing this dissertation. Also, special thanks to all the supporting staff, including the security guards, for their administrative support and protection.

I am highly indebted to my colleagues, particularly Mr. Constance Sorkpor and Mr. Paul Owusu Takyi; my friend Ms. Getrude Njokwe for their encouragement during times of difficulties. If it had not been for them, I would not have survived this journey to date. I am also grateful to the Japanese Government for financial support in my five years of stay in Japan.

Finally, my special thanks go to my parents, Christian and Dorothy Asamoah, as well as my brother and sister, Godfred and Evelyn Asamoah, for their encouragement and prayers.

DEDICATION

To my mother, Dorothy Kafui Asamoah

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CHAPTER 1

INTRODUCTION

1.1 Overview

Education is vital to economic development and poverty alleviation; there is indisputable empirical evidence that education has a strong positive association with economic growth and development (Schultz, 1961). Moreover, other empirical studies have demonstrated that education affords economic and social benefits to individuals; specifically, education improves individual productivity and income, reduces civil conflict, improves personal health outcomes, and reduces fertility (Lipset, 1959; Becker, 1991). A strong positive relationship has also been found between education and health status, educational attainment, and labor market outcome of offspring (Haveman and Wolfe, 1995; Strauss and Thomas, 1995).

As a result, several African countries have since independence introduced education policies targeting accessible education for every child irrespective of gender, age, and location. These policies also aim to contribute to the achievement of the Millennium Development Goals (MDGs) and the acceleration of progress towards the Education For All (EFA) plan (Akyeampong, 2009). Access to schooling in Africa has risen immensely over the past two decades due to policy implementation. The Gross Enrollment Rate (GER) at the primary school level now stands at 98 percent (base on *World Bank Open Data*). Also, average years of educational attainment, especially for females aged 15 and above, have increased substantially across Africa, though unevenly. For example, the average education attainment by females increased by 84 percent in North Africa (from 3.5 to 6.5) and by 48 percent in Sub-Saharan Africa (from 3.1 to 4.7) (Barro and Lee, 2013).

In light of the increase in educational attainment and the extensive empirical evidence on the importance of education, many social observers and international organizations have strongly advocated for education, especially maternal education, as a policy tool for tackling some global issues such as poor child health (Veneman, 2007; Ki-Moon, 2010) and for the elimination of harmful practices including a child, early, and forced marriage; and female genital cutting (UNICEF, 2016). A study on the effect of education on child health outcomes and female genital cutting (FGC)[1] in Africa is urgently needed. This dissertation provides a basis for a policy debate on promoting female education, particularly at the primary level, as an inroad to substantial health and social returns to offspring.

1.2 Statement of Problem

Child health and female genital cutting have long been an issue of concern for governments in Africa. Many children in Africa face several harmful traditional practices and are malnourished, exposed to illness, and subsequent death. The prevalence rate of *stunting* among children aged under 5 in 2018 is 30 percent in Africa, substantially above the global average of 22 percent, and triple the level in Latin American and Caribbean countries (based on *World Bank Open Data*). Statistics also show that FGC remains one of the causes of high morbidity and mortality among females in Africa. More than 200 million girls and women alive today have undergone some form of FGC, and 3 million girls are at risk of experiencing some sort of FGC each year (WHO, 2012). An infibulated woman faces a 70 percent higher risk of hemorrhage (the most common cause of maternal mortality) than a woman who has not had any form of FGC (World Health Organization or WHO, 2012, 2016).

[1] Also referred to as Female Genital Mutilation or Female Circumcision; for ease of exposition, I opt for the current term.

Although prevalent, there has been an improvement in child health and reduced FGC prevalence over the past decades. For instance, between 1990 and 2017, child (under-five) mortality decreased from 108 to 52 per 1000 live births (based on World Bank data). Also, between 1990 and 2017, the prevalence rate of FGC among children aged 0–14 decreased from 71.4% to 8.0% in East Africa, 57.7% to 14.1% in North Africa, and 73.6% to 25.4% in West Africa (Kandala, Ezejimofor, Uthman, and Komba, 2018). These vast improvements in child health, the reduction in FGC prevalence, and the increase in female education raise the question of whether the comovement of female education, child health, and FGC is causal.

Obtaining accurate estimates of maternal education's causal impact on child health and FGC prevalence is policy-relevant, especially for Africa. The United Nations' reports have advocated maternal education to be an essential policy tool for child health and eliminating harmful traditional practices such as FGC[2]. Identifying the causal effect of maternal education on child outcomes is challenging because unobserved family attitudes/attributes can affect maternal education, the decision to invest in child health, and the decision to have their daughters cut. Suppose more affluent families tend to invest more in education and child health and less likely to cut daughters. In that case, the observed correlations might be purely an income effect, not necessarily a causal impact of maternal education. Thus, identification requires empirical techniques to single out maternal education's variation uncorrelated with family attitudes/attributes.

[2] See policy reports by, for example, Ki-Moon (2010) and Veneman (2007).

Many researchers have exploited various sources of exogenous variation in maternal education across regions and birth timings to address the omitted variable bias. These sources of exogenous variation include college openings (Currie and Moretti, 2003), school construction (Breierova and Duflo, 2004), and education laws (e.g., Chou, Liu, Grossman, and Joyce, 2010; Lindeboom, Llena-Nozal, and van der Klaauw, 2009; Lundborg, Nilsson, and Rooth, 2014).

Surprisingly, to the best of my knowledge, only a few papers have investigated the causal link between female education and child health and FGC using African microdata[3]. All the existing studies focused on one country and exploited its Free Primary Education (FPE) reform as a source of exogenous variation in maternal education across birth cohorts. Except for the study by Masuda and Yamauchi (2018) and De Cao and La Mattina (2019), in addition to the variation in policy exposure, the authors exploited the differences in reform intensity across districts with varying pre-program rates of completing primary education. However, several issues might have confounded or invalidated the single-country analysis. Even with the RD technique, exceedingly popular recently, confounding factors still arise because contemporaneous policies in a country (e.g., health-promoting or poverty reduction policies under the Millennium Development Goal) often target school-age children, so as the FPE reform. Also, that country's previous trends in education and health might have driven at least part of the observed differences across birth cohorts. Not surprisingly, mixed results appear among various studies using the same education reform and data source. Perhaps strikingly, several single-country analyses have recently suggested maternal education has almost *no* impact on the decision

[3] Important examples include Grepin and Bharadwaj (2015) and Makate and Makate (2018) who focus on Zimbabwe; Makate and Makate (2016) on Malawi; Keats (2018) and Masuda and Yamauchi (2018) on Uganda; Ali and Elsayed (2018) on Egypt .

to cut daughters, contrary to what this study concludes. The no-impact results in the previous work are due to the small sample size and lacking required precision.

1.3 Objectives

This dissertation seeks to provide new evidence of maternal education's causal impact by providing a large-scale empirical analysis that investigates the causal link between maternal educational attainment and child health and the prevalence of daughters' FGC. In this study, I overcome the above challenges using data from twenty African countries implementing FPE and using IV methods with a triple difference design. The rollout of the FPE reforms across the continent over the decades (1976-2007) provides extra leverage to fix the country-specific trends (either related to the reform or unrelated but contemporaneous policies). Furthermore, this study incorporates the within-country variation in reform intensity across regions, measured by pre-reform primary school enrollment rates. This identification strategy exploits the triple differences in the reform exposure across countries, birth cohorts, and local reform intensities to tease out the exogenous variation in female school enrollment.

This result allows this study to pin down how much maternal education can impact child outcomes, including daughters' genital cutting, using IV methods. By doing so, this study provides more precise results that can be generalized to the continent. Besides providing external validity, this study also examines the possible pathways through which maternal education improves child health and reduces the prevalence of FGC in Africa.

1.4 Main Findings

This dissertation's main findings show that the FPE policy substantially increases females' educational attainment in Africa. The education policy leads to approximately *one additional year* of schooling. The effect magnitude is in the ballpark of the previous estimates using country-specific data. Because of the substantially larger sample size, the standard error for the first-stage estimate is, at most, one-third of the existing estimates. For cohorts affected by the FPE policy, I find one extra year of maternal education reduces infant mortality by 24 percent and under-five mortality by 16 percent. Likewise, one additional year of maternal education reduces the likelihood of a child suffering from stunting and underweight by no more than 10 and 5 percent, respectively. Also, one extra year of schooling for mothers decreases the likelihood of their surviving children suffering from fever two weeks before the survey. The 2SLS estimates in this dissertation are typically smaller but more precise than those for the previous studies. However, the estimates are qualitatively similar: the additional year of maternal education significantly reduces child mortality and improves the health status of surviving children. I argue that the effects of additional schooling on women's knowledge and ability to process information, socioeconomic status, and breastfeeding practices may help explain the results.

Regarding the effect of increased schooling on FGC prevalence, the 2SLS regression analysis shows that the extra year of maternal education significantly reduces the prevalence rate of a daughter's FGC by about 10 percentage points. The high precision of these 2SLS estimates reflects the large sample size. However, the point estimates are larger than previous estimates because the past studies considered only the eldest daughter, and using different samples leads to selection bias. Second, while De Cao and La Mattina (2019) exploit variation in reform intensity at the more disaggregated state level, I measure intensity at the region level. I explain the result in this study by looking at the impact of education on women's attitude towards FGC and women's empowerment. The increased educational attainment is likely to improve women's attitudes toward FGC and increase women's position relative to their husbands, leading to increased bargaining power within their marriage settings.

1.5 Organization of the Dissertation

The remaining part of this dissertation is organized as follows: Chapter 2 reviews the relevant literature and clarifies the literature gaps. Chapter 3 describes the institutional background to the education policy, organization of data, and a detailed empirical strategy description. In Chapter 4, I present the results of the causal effects of maternal education on child health. I further examine the causal effects of maternal education on daughters' FGC prevalence in Chapter 5. Finally, Chapter 6 concludes the dissertation and discusses implications for future research and policy debates.

CHAPTER 2

LITERATURE REVIEW

2.1 Introduction

This chapter reviews the literature on education outcomes focusing on child health outcomes and female genital cutting. This chapter's main objective is to highlight the critical gaps in the existing literature that this dissertation seeks to fill. The first part of this section offers a review of empirical studies on maternal education's effects on child health; section 2.3 reviews the literature on maternal education's impact on female genital cutting. The final section summarizes the gaps in the literature that this study seeks to address.

2.2 Previous Evidence of the Impact of Maternal Education on Child Health

The health economics literature offers various explanations of why education might impact health (Grossman, 2006). The two leading descriptions in the literature are the productive and allocative efficiency theories. The productive theory suggests that improved schooling enhances health's marginal product, making educated individuals efficient health producers (Grossman, 1972). According to the allocative efficiency theory, highly educated individuals can choose the right health inputs that maximize their health utility and are therefore more likely to accommodate and process new information faster than their less-educated equals (Grossman, 1972).

Prior research has extensively documented the effect of maternal education on child health using different quasi-experimental designs. Examples include compulsory schooling laws (Lindeboom et al., 2009; McCrary and Royer, 2011; Grytten, Skau, and Sørensen, 2014; Lundborg et al., 2014; Güneş, 2015), school construction programs (Breierova and Duflo, 2004; Chou et al., 2010), and university openings (Currie and

Moretti, 2003). These studies exploit maternal birth timing and regional variations in policy exposure to capture the exogenous variation in education. All these studies suggest a protective and statistically significant impact of maternal education on child outcomes, except for McCrary and Royer (2011). They use age-at-school-entry policies in California and Texas and find that education has small effects on infant health, such as birth weight, prematurity, and infant mortality rate, but does not affect prenatal behaviors such as smoking rates and prenatal care.

Existing evidence from Africa is country by country, using an education reform to instrument maternal education. Examples include the FPE reforms in Malawi (e.g., Behrman, 2015; Makate and Makate, 2016; Andriano and Monden, 2019), Uganda (e.g., Behrman, 2015; Keats, 2018; Andriano and Monden, 2019; Masuda and Yamauchi, 2018), and Zimbabwe (e.g., Grepin and Bharadwaj, 2015; Makate and Makate, 2018). All these studies rely on within-country variation in policy exposure across cohorts for identification.

Two important exceptions are noteworthy. Masuda and Yamauchi (2018) and Andriano and Monden (2019) use Uganda's FPE reform as earlier studies. However, they further exploit the district-level variation in pre-program rates of primary education completion, in addition to the variation in mothers' birth timing. While both studies exclude the partially affected cohorts from the analysis, the latter adds more recent surveys to include younger cohorts in the data. Their difference-in-differences estimates suggest that the FPE significantly increases female education by nine years (SE=2) and 0.002 years (SE=0.0004), respectively. Compared to Behrman's (2015) and Keats' (2018) findings using the same education reform in the same country, the former is approximately tenfold higher, and the latter is less than 20 percent lower.

2.3 Previous Evidence of the Impact of Education on Female Genital Cutting

Among the several theories to explain the slow but persistent declining trend in FGC, the most relevant theories are social convention theory, modernization theory, and feminist theory (Yount, 2002). The social convention theory argues that FGC serves as a positive signal (e.g., morality, integrity, and fidelity) to men looking for wives. If this theory is correct, women with FGC are more likely to get married. While the modernization theory links female genital cutting to social and economic development (Boyle, McMorris, and Gómez, 2002; Hayes, 1975), the feminist theory focuses on women's status as an essential driver of social change (Althaus, 1997; Yount, 2002).

FGC mainly appeared in 30 countries throughout Africa, Asia, the Middle East, and migrants from those who reside in America, North America, and Europe also practice FGC (WHO, 2012). In Africa, FGC is prevalent and profoundly rooted in some ethnic groups' cultures, particularly in the eastern, north-eastern, central, and western regions. The prevalence rates range from less than 10% to a nearly universal prevalence rate of 99% (28TooMany, 2015).

The WHO classifies FGC's practice into four types depending on the severity of the cutting: Type I, often referred to as clitoridectomy, involves the partial or total removal of the clitoris (both the clitoral hood and clitoral glans). Type II FGC, also known as the excision, consists of the partial or total removal of the clitoris and the labia minora, with or without excision of the labia majora. Type III, often described as infibulation, is the more aggressive practice, and it pertains to the narrowing of the vaginal opening through the covering seal's creation. Type IV includes other harmful procedures (such as incising, cauterization, scarring) to the female genitalia for non-medical reasons (WHO, 2012). The type of FGC practiced in Africa differs significantly among countries. Whereas Type

II is the most prevalent type of FGC in West Africa, Type III is performed primarily in eastern African regions.

The age at FGC varies significantly within and across countries, cultures, and ethnic groups, between infancy and 15. Specifically, the FGC prevalence is hump-shaped, peaked at age 15, and spiked at infancy (Chen and Fenta, 2020). Regardless of the type or age, most FGCs in Africa are carried out by traditional practitioners who have little knowledge of the female organ. The practice is mostly performed in an unclean environment using a crude, unsterile instrument (e.g., razor blades, kitchen knives, scissors, broken glass, sharpened rocks, and fingernails) and without anesthetics (Berg, Underland, Odgaard-Jensen, Fretheim, and Vist, 2014; UNICEF, 2013).

Previous evaluations of the causal link between maternal education and a daughter's FGC are few and suggest mixed results. Given Kenya's education reform that extended the length of primary school by one year, Nesje (2014) uses the timing of birth as the only source of exogenous variation in maternal education. The result suggests that the extra year of education decreased the eldest daughters' FGC prevalence by 11 percent. In contrast, De Cao and La Mattina (2019) find that a one-year increase in maternal education (induced by Nigeria's FPE reform) reduces the eldest daughter's FGC prevalence by only 1.4 percent, with standard errors as large as 14 percent of the prevalence rate of FGC. Unlike Nesje (2014), De Cao and La Mattina (2019) capture the exogenous variation in maternal education by adding within-cohort variation in policy intensity across states, not only using the changes in the timing of birth.

2.4 The Niche of this Study

As reviewed in the previous two subsections, the literature has shown large and significant results if using the mother's years of birth as the only source of exogenous variation in maternal education given a single country's education reform. If adding the within-cohort, within-country variation in maternal education across regions, most of the previous results show a dramatic decrease in maternal education's estimated impact on child outcomes. Still, the standard errors for the FGC impact of maternal education are too large to make a definitive conclusion. Additionally, several issues might have confounded or invalidated the single-country analysis. Even with the RD technique, exceedingly popular recently, confounding factors still arise because contemporaneous policies in a country (e.g., health-promoting or poverty reduction policies under the Millennium Development Goal) often target school-age children, so as the FPE reform.

Also, that country's previous trends in education and health might have driven at least part of the observed differences across birth cohorts. Not surprisingly, mixed results appear among various studies using the same education reform and data source. Perhaps strikingly, several single-country analyses have recently suggested maternal education has almost *no* impact on the decision to cut daughters, contrary to what this thesis concludes. The no-impact results in the previous work are due to the small sample size and lacking required precision.

Despite years of policy concerns on the relationship among African children's health, FGC, and maternal education, surprisingly, no empirical analysis uses data from multiple countries and has considered both variations in mother's birth timing and policy intensity across regions within countries. The results using data from one country might not necessarily apply to others.

This dissertation fills this literature gap by providing new evidence for a generalization to the continent by pooling all the African nations with FPE reforms and DHS data. This strategy allows this study to incorporate country-fixed effects and country-specific trends (or region-specific trends), removing all other contemporaneous changes within countries unrelated to the education reform but affecting child outcomes.

Besides providing external validity, this study exploits both cohort and region-level variations in policy exposure within countries. As Larreguy and Marshall (2017) have noted, females in districts with lower pre-reform education levels may receive a greater policy impact on education. Omitting the variation in district-specific policy intensity might understate the effect of the education reform on maternal education, so likely to overstate the causal effect of maternal education on child outcomes. This dissertation also contributes to the literature by incorporating the within-country variation in policy intensity across regions in a triple difference-in-differences design.

CHAPTER 3

INSTITUTIONAL DETAILS, DATA, AND EMPIRICAL SPECIFICATIONS

3.1 Free Primary Education Reforms in Africa

In the past few decades, several countries in Africa have implemented education policy to achieve Millennium Development Goals and Education For All plan. Out of the 54 African countries, 27 have implemented the free primary education policy under various names at different timings over the past five decades.[4] In Table 3.1, I list the 27 implementing countries and their reform timings. For ease of exposition, I refer to all those education reforms as FPE. As shown in Table 3.1 and Figure 3.1, the first country to implement the policy is Tunisia in 1958. From 2000 to 2013, 17 African countries formally abolished tuition fees for primary schools. Before FPE reforms, African parents were responsible for their child's school tuition, and the tuition costs were a significant obstacle to the enrollment and retention of children in schools (Deininger, 2003). The FPE program provides a tuition waiver for public primary schools and sometimes absorbs other costs, such as textbooks, stationery, and sports kits. Several countries also provide free school uniforms to enrolled children to encourage enrollment (UNESCO, 2003).

Like any social policy implemented on a large scale, Africa's FPE policy imposed financial burdens on the implementing countries. Because school construction did not always keep pace with increased enrollment, several countries such as Ethiopia, Ghana, Kenya, Malawi, and Mozambique adopted various measures to accommodate the surge in enrollment (World Bank and UNICEF, 2009). The measures include multi-shift teaching, increased class sizes, increased pupil-teacher ratios, recruitment of untrained or

[4] The reform names include UPE, FBE, FPE, and FCUBE, which refer to universal primary education, free basic education, free primary education, and free compulsory universal basic education, respectively. I include all types of primary education reforms that abolish tuition fees.

retired teachers, and use of temporary facilities (e.g., church buildings or the shadow of a tree) to serve as classrooms. Section 3.3.2 discusses how these measures may bias the estimates and how I overcame this challenge.

3.2 Data Sources, included Countries, Key Variables, Intensity Variable, and Cohort Classification

3.2.1 Data sources

This dissertation's primary data source is the Demographic and Health Survey (DHS), collected and administrated by the *United States Agency for International Development*. I use the DHS because it is nationally repeated representative data and rich in health and demographic information for both mothers and children. Its high degree of standardization across countries and surveys is of great importance for this study. In Chapter 4, some countries have one wave of DHS data; thus, I include Malaria Indicator Survey (MIS) to increase the sample size. Like the DHS data, the *United States Agency for International Development* organizes and collects the MIS data. It is highly standardized across countries, and it is a representative sample of women aged 15–49. This dissertation's main analysis includes all the survey waves *after* the FPE reform in the implementing African countries[5]. The number of included waves per country ranges from 2 to 7, and totally in Chapter 4, I extract 85 surveys (63 from the DHS survey and 22 from the MIS survey), as Table A3.1 shows. In Chapter 5, I pull 23 DHS surveys, as shown in Table A3.2.

As both the *DHS* and *MIS Child Recode File* contain information on children born in the five years preceding the survey, In Chapter 4, I include children below the age of 5

[5] In the Ethiopian DHS, the interview year and respondent's birth year are all in the Ethiopian calendar, so I have to convert it to the Gregorian calendar.

(0–59 months) in analyzing the effect of maternal education on child health. While pooling DHS data across countries, I find that all the 20 included countries have no missing entries for a mother's age or year of birth in the data. However, several variables of interest contain missing values. In Chapter 5, I focused on waves of *Individual Women's Recode File* that has information on FGC, and my sample consisted of mothers aged between 15 and 49 at the time of the survey. The *Women's Recode File* includes a module of questions about the FGC of the respondent and that of her daughters. Women are asked whether they are circumcised, the age at cutting, attitudes toward FGC, if any (eldest) daughter is cut, the daughters' age at cutting, and who performed the procedure.

3.2.2 Included countries and sample size

Since I am using the FPE policy as a natural experiment for identification, I focus on countries that have implemented the policy and have public-use data. Chapter 4 excludes Botswana, Equatorial Guinea, and Guinea-Bissau because they have no public-use data. Also, I exclude countries such as D.R. Congo, Lesotho, and Namibia because the affected cohorts are too young. Lastly, I exclude Tunisia because it implemented the policy quite early. There are no recent DHS data; thus, the old data's affected cohorts are too old to be comparable with other countries. I, therefore, end up with 20 countries for the analysis (Table 3.1). The pooled dataset contains 322,592 children from 110,929 families (Table A3.1).

In analyzing the effect of maternal education on a daughter's FGC, I excluded countries from the southern part of Africa since they do not practice FGC; thus, these countries have no FGC information. Additionally, I excluded countries like Benin, Burkina Faso, Cameroon, and Togo because the waves of data with FGC information are old and do not have the cohorts that have been affected due to the recent implementation of the policy in those countries. As a result, I ended up with 7 countries for the analysis (Table 3.2). While the old survey rounds asked the respondents whether the eldest daughter was cut, questions about FGC were asked about all of the respondents' daughters in recent survey rounds. For the main analysis in this dissertation, I used the sample for all daughters; however, I restrict the sample to the eldest daughter for robustness checks. The pooled dataset consisted of 195,486 observations from 113,958 families (Table A3.2).

3.2.3 Key variables

The sample consists of mothers aged between 15 and 49 at the time of the survey. Because I measure maternal education at the time of the survey, the observed maternal education is likely right-censored among young women. I also measure education using a dummy variable indicating whether the mother has some education or not.

In analyzing the impact of maternal education on child health outcomes, I focus on several early childhood health outcomes: (1) mortality, (2) anthropometric measures, and (3) the occurrence of diseases. First, I include three mortality rates in the analysis: neonatal mortality (death within the first month of life), infant mortality (death before the first birthday), and Under-5 mortality (death before the fifth birthday). Table 3.3 shows that 6 percent of children in our sample died before the first month of life, 7 percent before their first birthday, and 9 percent before their fifth birthday. These figures are almost similar to the mortality rates of the firstborn data from Uganda (Keats, 2018) and Taiwan's mortality rates (Chou et al., 2010).

Second, I investigate three anthropometric measures of early childhood: height-for-age, weight-for-height, and weight-for-age z-scores. To create z-scores, the DHS uses age- and sex-specific growth standards to expresses the anthropometric ratios as several standard deviations below or above the median value of a reference population. The purpose of using a growth reference is to allow the measures comparable across countries over time. This practice can avoid situations where children in a specific country with stable trends in heights and weights appear worsening in nutritional status because children in other countries have been improving more substantially. The DHS derives the growth standards using data from the United States. The reference population for age 0–23 months consists of healthy, Ill-nourished children from the Ohio Fels Research Institute Longitudinal Study, and the reference population for age 2–5 is made up of healthy, well-nourished children using data from cross-sectional representative surveys (De Onis & Blössner, 1997).

The height-for-age z-score reflects all past and present nutritional deficiencies, so it is considered a good measure of chronic conditions (Martorell and Habicht, 1986). The weight-for-height z-score captures the current dietary deficiencies. The weight-for-age z-score appears to be a sensitive measure of child health-related to short-run malnutrition shocks (Foster, 1995). In addition to those continuous anthropometric variables, I construct three additional indicators. Stunting (defined by height-for-age z-score two standard deviations below the average), wasting (defined by weight-for-height z-score two standard deviations below the average), and being underweight (defined by Weight-for-age z-score two standard deviations below the average).

As Table 3.3 shows, African children's anthropometric measures are weak compared with the reference population. The average z-scores for height-for-age and weight-for-age suggest that African children are about 1.2 standard deviations shorter and the 1.0 standard deviation lighter than the reference population. However, less than one-third of children in the sample show stunted growth (i.e., more than two standard deviations below the reference group average). Also, less than one-third are wasted (i.e., more than two standard deviations below the reference group's average) and are underweight (i.e., more than two standard deviations below the reference group's average weight). Finally, I include three dummies indicating whether the child had ever suffered from anemia, diarrhea, and fever to account for common illnesses experienced by children in Africa.

To examine the effect of the increase in schooling years on health knowledge, I constructed comprehensive knowledge about HIV/AIDS. The DHS defines comprehensive knowledge about HIV/AIDS as: knowing that consistent use of condoms during sexual intercourse and having just one uninfected faithful partner can reduce the chance of getting the AIDS virus, knowing that a healthy-looking person can have the AIDS virus, and rejecting the two most common local misconceptions about AIDS transmission or prevention. Therefore, I constructed a dummy equal to 1 if a woman provides a correct answer to all of these questions.

In Chapter 5, the key outcome variable is a dummy equal to 1 if the mother reported that her daughter was cut. Additionally, I measured women's attitudes toward FGC using a dummy variable equal to 1 if the mother replied yes to the question "Female circumcision should continue" and 0 otherwise. A dummy variable equal to 1 if the mother replied yes to the question "Intend to circumcise daughter in future" and 0 otherwise. While the sample size for "Female circumcision should continue" is slightly smaller than the main analysis sample, the sample size for "Intend to circumcise daughter in future" is substantially smaller than the main analysis sample.

Table 3.3 shows 11 percent of the daughters are cut; about 52 percent of the women think FGC should continue, and 57 percent intend to cut their daughter in the future. It is worth mentioning that the number of observations for variables such as

"Female circumcision should continue" and "Intend to circumcise daughter in future" is considerably smaller than the sample for the main analysis because earlier survey waves do not contain these variables. Given this, I replicated the main analysis using the sample for respondents who intend to cut daughter in the future because the sample size is far too small.

Furthermore, the regression analysis in Chapter 4 and Chapter 5 utilizes several variables to capture time-varying country-specific characteristics related to healthcare and school quality. To measure healthcare quality, I use *physician density* and *hospital bed density,* defined by the number of physicians and hospital beds per 1,000 people, respectively, at the survey time. To capture the variation in school quality over time across countries, I include the primary school pupil-teacher ratios for each country prior to the reform. Since our school quality measure began in 1960, I lose several older cohorts for which this measure is unavailable. I derive those country-level covariates using statistics downloadable from the website of the *World Bank Open Data.*

3.2.4 Within-country inequality in school enrollment before FPE

FPE may have a stronger enrollment impact in areas with lower pre-reform enrollment rates. To exploit such variation in *policy intensity* in my analysis, I define policy $Intensity_{rj}$ for each region *r* in country *j,* as the female proportion of the regional population who had not entered the primary school before the reform. As a result, for each region *r* in country *j*, I define a population of female youths aged 15-18. This range of cohorts is an approximation of the population of female youths. Depending on the data availability, this range of cohorts varies from county to country, as listed in Table A3.3.

I construct the intensity measure at the regional level in each country using one DHS wave closest and prior to the reform except for Egypt, Nigeria, Sierra Leone, and

Zimbabwe because these countries have no pre-reform data. For these four countries, I used data immediately after the reform but used the population of female youth too old to be affected by the policy. As a result, I performed a number of robustness checks to ensure that the results from this intensity measure are robust. First, I provided an alternative definition of FPE intensity based on the primary completion rates using the cohorts that had not completed primary school. Second, I dropped these four countries and run the regression for the first stage and present the results in Table 4.10. I define a region as "High intensity" for each country if the region's intensity is above the country's median intensity.

Table A3.3 contains information on the number of observations and policy intensity for each region within each country. The table shows a large dispersion in policy exposure *across and within*-country at the regional level. The table also shows that while the number of observations in each region differs across countries, there is a sufficient number of observations within each region. The regions listed in Table A3.3 are the regions that existed in the pre-reform data. The recent data contains more regions than the ones listed in Table A3.3. It turns out that most of the current data regions were created from the already existing region, which implies that the new regions' cohorts were affected by the policy in the old region. Therefore, to not lose many observations, I recoded the new regions to match the parent regions before merging the intensity data with the children dataset.

3.2.5 Cohort classification

The empirical work begins with cohort classification to prepare for my analysis. The implementing countries experienced a sudden and massive increase in enrollment, especially at the primary level. However, the key to my empirical design is that this increase was mainly driven by enrollment in primary school grade 1. For example, before Ghana implemented the policy in 1996, the primary 1 enrolment between 1990 and 1996 ranged between 405,000 children and 415,000 children. However, enrolment in primary 1 jumped to 490,000 in the new academic year, representing an approximately 20 percent increase in primary 1 intake. Primary 1 enrolment was constant at around 490,000 for the next 3 years and then continued to grow steadily.

Similarly, enrolment in primary 2 and 3 experienced marginal increases of about 20,000 pupils each, i.e., 5 percent and 6 percent, respectively. No significant increase was observed immediately after the reform in upper primary and lower secondary levels (Akyeampong, Djangmah, Oduro, Seidu, and Hunt, 2007). Given this, I compare females born late enough to start primary 1 in the year of the reform or after with females born only a few years too early. Since the school starting age is rarely compulsory in Africa, even after the FPE reform, the official starting age is typically not the point of discontinuity in the primary school enrollment rate. Using the DHS survey just before the reform in a given country, I show in Figure B3.1 that the de facto entry age for girls peaks between ages 6 and 9, about zero to three years after the official starting age (6 or 7). However, I observe that in Liberia, girls' de facto entry age peaks at age 11, about five years after the official entry age. Liberia went through a 14-year civil war that destroyed the education system and damaged schools. School children (both boys and girls) served as child soldiers. Therefore, overaged students are common in primary school classrooms due to war (UNICEF, 2011).

Although most countries have a windfall age immediately after the peak point, others, such as Kenya, Mali, Mozambique, Rwanda, and Uganda, have the fraction of girls entering school gradually decreases with age. I refer to the age with the large fall from the previous age group as the *windfall age*, as indicated by the dash lines.[6]

For each included country, I classify cohorts using the empirical distribution of school entry age among school-age females. I define *the full-exposed cohorts* as those at the windfall age or below the windfall age of school entry before the reform (see Table 3.1 and Figure B3.1) and call those older than the windfall age as of the *partially-exposed cohorts*. I set the number of fully or partially exposed cohorts up to ten, subject to DHS data availability. To balance the numbers of fully and partially exposed cohorts, I limit the number of cohorts to be equal within countries and no more than ten. I test the robustness of this sample selection using the RD design conditional on bandwidth (see Section 3.3.3).

For example, in 1996, Ghana started the reform and set the school-entry age at six. However, the DHS in 1993/94 suggests that the school-entry age would peak at age seven at the reform year, who were born in 1989 (see Figure B3.1). This statistic motivates me to start the fully-exposed cohorts at 1989. Because Ghana's DHS data can accommodate all the cohorts ten years younger and older than the birth cohort in 1989, the fully and partially exposed cohorts are defined as 1979-1988 and 1989-1998, respectively. For countries that implement an FPE program more recently, such as Togo's reform in 2008, I only can include four cohorts in both fully and partially exposed groups (that is, 1995-1998 and 1999-2002, respectively) since the youngest DHS cohort in Togo were born in 2002. For the analysis on FGC in Chapter 5, I could only include five cohorts in both fully and partially exposed groups for Kenya (1995–1999 and 1990–1994, respectively) since the youngest members of a DHS cohort in Kenya were born in 1999.

[6] This empirical strategy is similar in spirit to Keats (2018), who use empirical histogram of completion.

Table 3.1 and Table 3.2 show a considerable cross-country variation in the timing of the policy change and the birth cohorts exposed to the reform. It can also be noted in Table 3.2 that the youngest member of a DHS cohort for Kenya, Sierra Leone, and Tanzania is different in Table 3.1 and 3.2. This difference is due to the differences in the final data for each Table. In Table 3.1, the final data for these three countries is the recent MIS data, while in Table 3.2, the final data in the same countries is the current DHS data.

3.3 Empirical Specifications

To identify maternal education's effect on child outcomes and FGC prevalence, I adopt a triple-differences strategy (DDD), which exploits the exposure to the FPE rollout and the within-country variation in pre-FPE enrollment rates across regions as the additional source of variation in education. I used the enrollment rate as an extra source of variation in education because the reform may substantially affect the school enrollment in regions with lower enrollment rates. However, I begin the specification with a difference-in-differences (DD) design using the FPE rollout across countries as an instrument for education. Finally, I extend the DDD setting to a fuzzy regression discontinuity (RD) design by looking into a narrower range of birth cohorts for robustness checks. I describe the estimation strategy in detail as below and report the findings in the next section.

3.3.1 Initial analysis: difference-in-differences design

During the past four decades, the rollout of FPE across the African countries offers a natural experiment for the changes in education across countries and birth cohorts. I define the FPE rollout dummy, $Z_{cj} = I\{c \leq \bar{c}_j\}$, indicating the birth cohort c younger than or equal to the pivotal cohort $\bar{c}_j$ in country j, as the instrument for maternal education in the initial difference-in-difference setting:

$$S_{icj} = \alpha Z_{cj} + \mu_c + \mu_j + \mu_j(c - \bar{c}_j) + W_{jt}'\varphi + X_{icj}'\delta + e_{icj} \quad (3.1)$$

Where S_{icj} is the education level of that mother, μ_c and μ_j represent cohort fixed effects and country fixed effects, respectively. X_{icj}' represents the child's gender and the full set of dummies for the household's religion, ethnicity, and survey year. The error term e_{icj} potentially captures unobserved family backgrounds that might affect child health and maternal education. By including the country-specific linear cohort trend $\mu_j(c - \bar{c}_j)$, this model allows each country to have its trends in school enrollment, which could be altered by other contemporaneous policies unrelated to the educational reform. In some specifications, I also include time-varying country characteristics W_{jt}' (such as physician density and school quality, both evaluated before the reform year) to capture part of the nonlinear trends.

Key to this difference-in-differences identification strategy is the condition that the variation in exposure to the reform is an exogenous source of the variation in maternal education. In this initial difference-in-differences model, I temporarily assume no spatial variation in pre-FPE enrollment rates within countries. Also, I assume the policy intensity is uniformly valued by either zero or one for all cohorts born before or after the pivotal cohort. I relax both assumptions in the triple-difference settings.

Figure 3.2 shows the unconditional first stage graphs of the effect of the reform on schooling years. Panel A of Figure 3.2 uses the sample for child health, and Panel B uses the sample for FGC. Figure 3.2 suggests that FPE reforms in Africa increase maternal years of education of the pivotal cohort by approximately 1.4 and 1.2 years of schooling in Panel A and B, respectively. Panel B of Figure 3.2 shows that the years of education diverge as the cohorts become younger and younger. This situation is probably due to the following reasons. First, not all included countries have the maximum number of birth cohorts up to 9. Some countries have the maximum number of birth cohorts to be 6. As a result, the cohort size for birth cohorts greater than 6 is small. Second, the divergence in schooling years is because the younger women, who are likely to receive more education, were still very young at the survey time. They are likely to achieve their highest educational attainment later. For instance, the countries with more than 6 birth cohorts have most respondents between 15 to 18, although some respondents are older than 18 years.

Also, I examine the effect of the reform on the probability of having some education and literacy rate. Figure B3.2 in the Appendix also shows that being exposed to the reform increased the likelihood of having some education and literacy rate by roughly 11 percentage points and 10 percentage points, respectively.

3.3.2 2SLS model with a triple-differences design

While using FPE rollout across countries and birth cohorts, the DD design in Equations (3.1) omits the within-country disparity in educational opportunities across regions. Females who grew up in regions initially with lower enrollment rates are likely to receive a greater policy impact when the government eliminates tuition fees. To capture the regional differences in the pre-reform enrollment rate, I construct a *policy intensity* measure, I_{rj} defined by the proportion of females in region r of country j who had not entered the primary school before the reform. I include policy intensity I_{rj} as a third dimension in the DD setting, to form a DDD model as my preferred specification below:

$$S_{icrj} = \alpha Z_{cj} I_{rj} + \mu_{cr} + \mu_{rj} + \theta_{0rj}(c - \bar{c}_j) + \theta_{1rj} Z_{cj}(c - \bar{c}_j) + W'_{jt}\varphi + X'_{icj}\delta + e_{icrj} \qquad (3.2)$$

Here the instrumental variable $Z_{cj}I_{rj}$ captures the within-country variation in policy intensity across regions and birth cohorts. I control for the regional-specific effects for each cohort (μ_{cr}) and for each country (μ_{rj}) in the model. Since the instrumental variable varies across cohorts and regions within a country, I additionally control for region-specific linear cohort trends and allowed these region-specific linear cohort trends during the pre-and postreform periods (θ_{0rj} and θ_{1rj}). The estimated coefficient, α, reflects the impact of FPE on maternal education.

Equation (3.2) shows the impact of FPE on educational attainment, and it can be regarded as the first stage regression in the instrumental variable estimation method where years of education is an endogenous variable.

Next, to obtain an unbiased estimate, I simultaneously estimate the second stage to investigate the causal effect of education on child health and FGC prevalence with the interaction term in Equation (3.2) as the instrument. Specifically, the second-stage equation is:

$$Y_{icrj} = \beta S_{icrj} + \mu_{cr} + \mu_{rj} + \theta_{0rj}(c - \bar{c}_j) + \theta_{1rj} Z_{cj}(c - \bar{c}_j) + W'_{jt}\varphi + X'_{icj}\delta + u_{icrj} \qquad (3.3)$$

where Y_{icrj} is the health outcome of child i of age 0 to 5 and the FGC status of girl child i in country j's region r, born to a mother in year c, and the error terms u_{icj} potentially captures unobserved family backgrounds that might affect child health. The coefficient of maternal education β is our parameter of interest. Figure 3.3 provides evidence that while the FPE reform increased education years, the impact is uneven. The reform differentially influenced years of education across regions with different policy intensities. The impact is high in areas with low pre-reform enrollment rates. As shown in Figure B3.3, the reform differentially influenced the probability of having some education and women's literacy. The reduced form graphs (Figure 3.4) also show the policy differentially reduced child mortality, other health outcomes, and FGC prevalence.

Key to this triple-differences identification strategy is the condition that in the absence of FPE reform, trends in maternal education, child health outcomes, and FGC prevalence would not have differed across regions with different policy intensity. The unconditional first-stage graphs (Figure 3.3) and the reduced form graphs (Figure 3.4) show that the parallel trend assumption is satisfied in child mortality, anthropometric measures, and the occurrence of diseases. However, the probability that a daughter is cut is inconsistent with the parallel trend assumption. Due to the common trend assumption's possible violation, in the regression analysis, I include region-specific cohort trends to allow for differential trends in these outcomes at the regional level within each country. As robustness, I allowed differential trends at the country level, and the result is robust. In Section 4.3 and Section 5.3, I perform a formal test to examine the common trend assumption's robustness by providing a placebo test restricting each country's sample to cohorts too old to be affected by the policy.

In the first stage, which consists of Equation (3.2) with mothers' years of education as an outcome variable, I estimate the effect of UPE on educational attainment. In the second stage, I use the interaction term as the excluded instrument. If the instrument satisfies the exclusion restriction, the 2SLS estimate identifies the causal effect of education. The exclusion restriction would be violated if the reform impacted child health and FGC beyond its impact on completed years of schooling.

One concern about this identification strategy is that parents with scarce resources who are aware of the reform's implementation year might postpone their children's education to benefit from the reform. Such behavior could result in a positive bias, causing us to overstate the causal impact of maternal education on child health and FGC prevalence. To address this concern, I directly examine this possibility. If parents had postponed the education of children whose grade 1 primary education took place close to the policy implementation year, then the gross enrollment ratio would have decreased significantly two or three years before the reform years. In Figure B3.4, I find the female enrollment rates in Ethiopia, Ghana, Tanzania, and Zimbabwe slightly decrease before or during the year of the policy change, while the other included countries have almost no dip in the percentage of female enrollment before the reform year. I derive these country-level gross enrollment rates using statistics downloadable from the World Bank Open Data website. Also, the policy did not filter out any grade; instead, the policy implementation was set out to provide all children with primary education irrespective of grade level. As a result, there is no motivation for parents to postpone the education of their children. However, in Section 4.3, I discuss the results' robustness by excluding data from the four countries (see Table 4.10 for the result).

Another potential violation of the exclusion restriction will be if the policy affects the quality of education. As noted earlier, most countries adopted measures to keep up with the massive increase in enrollments. Although I have not found statistics on the extent to which schools adopted other measures, any measure that potentially reduces teaching quality will discourage female enrollment into primary school and negatively affect child health. This situation causes a positive bias, leading us to *overestimate* the causal link between maternal education on child health. As a result, the regression analysis includes the primary school pupil-teacher ratios at the country level. Furthermore, if these measures reduce school quality, educational attainment gains would not necessarily indicate increased learning (Keats, 2018). Therefore, I examine the impact of the policy on women's literacy level, and the result in Table 4.1 and Figure B3.2 shows the policy substantially increased the literacy level of women.

Also, fertility and sibling effects could be another source of potential threat to the identification. For instance, the first child's poor health status may affect a woman's decision regarding subsequent fertility and child investment choices. The eldest daughter's health status who has undergone FGC may also affect a woman's decision regarding the subsequent daughter. Ideally, to overcome this challenge, I would have to focused on first-time mothers (i.e., first births only). Dealing with this issue in chapter 4, I restrict the sample to firstborns (see Section 4.3.3 for discussion of result), and in Chapter 5, I limit the sample to the eldest daughter (see Section 5.3.3 for discussion of result). Both results are robust to these sample restrictions.

If the instrument satisfies the exclusion restriction, then the 2SLS estimate identifies the causal effect of education on child health and FGC prevalence. I interpret these results as the Local Average Treatment Effect (LATE) of a year increase in education for the women who stayed in school longer because of the policy (compliers). In this study, the treatment group comprises women who were of school entry age or below at the time of the policy; thus, they can be regarded as fully treated cohorts. Since these cohorts might have avoided fees from grade one, the LATE estimates are likely to be more applicable for females from households of lower socioeconomic status.

3.3.3 Robustness checks: Fuzzy regression discontinuity design

As discussed in Section 3.3.2, the analysis so far is based on the symmetric number of cohorts within countries and no more than ten. However, the sample is not necessarily symmetric around the pivotal cohort. There are less than 10 birth cohorts in some countries before and after the reform because either the policy was too late or there are no recent datasets. As a result, this situation could lead to sample selection.

I examine the robustness of the result to the selection of the sample by extending the DDD model to a *local fuzzy RD* design. The 2SLS *fuzzy RD* design is essentially the same as the triple-difference specification in Equations 3.2 and 3.3. However, for the *fuzzy RD* design, I focus only on females who were born *soon before* and *after the pivotal cohort*, $c \in (\bar{c}_j - b, \bar{c}_j + b)$, conditional on a bandwidth *b.* I determine the bandwidth by running a leave-one-out cross-validation using the specification function in Lee and Lemieux (2010) and then pooled all the countries together.

The identifying requirements are: (i) the covariates affecting both years of schooling, child health outcomes, and daughters' FGC vary smoothly across the pivotal cohort, and (ii) individuals are unable to manipulate treatment status. In Section 3.3.2, I have already discussed the threat to the second assumption. The first assumption, smoothness of covariates at the cutoff, is a concern, as it may fail to hold if there are other policies related to education implemented around the same time as the education policy. If this happens, then the estimated results will be biased as they will capture the combined effect of education reform and other policies. I test the validity of the first requirement by examining the covariates' smoothness that may affect both years of schooling, child health outcomes, and FGC prevalence around the pivotal cohort, namely the child's gender, respondent's religion, physician density, and pupils-teacher ratios. Figure B3.5 shows the trend of some of the predetermined covariates, including religion, child's gender, and time-varying characteristics measure before the reform. For these variables, I fail to find a discontinuity in the slope around the pivotal cohort, suggesting that no other policies were implemented around the education policy that affected our outcome variables.

The unconditional first-stage graph (Figure 3.5) displays the actual and fitted cohort profiles of maternal education for all mothers aged 15-49 in the pooled DHS data from 20 countries for the child health study and 7 countries for the FGC analysis in Africa. Each marker indicates the average education level of the mothers born in a given year. The *pivotal cohort* refers to the school-entry age cohorts in the year of the reform given the country and region of residence. Thus, the birth cohorts with a positive distance from the pivotal cohort are *fully exposed* to the reform, and those with negative distance are unexposed or partially exposed cohorts. Figure 3.5 shows that the average years of schooling are higher for the post-reform cohorts, suggesting that FPE reforms in the African countries increase maternal education. The estimated jump in average years of schooling without controls is approximately 0.9 years in Panel A and 1 year in Panel B.

CHAPTER 4

ESTIMATING THE EFFECT OF MATERNAL EDUCATION ON CHILD HEALTH USING MICRODATA FROM 20 AFRICAN COUNTRIES

4.1 Introduction

This chapter presents the baseline regression results for the triple-differences estimation of maternal education's causal effect on child health using microdata from 20 African countries. As discussed in chapter 3, to address the issue of endogenous education, I exploit the rollout of the education reform across 20 implementing countries in Africa. The policy rollout gives variation in reform exposure both within and between cohorts which forms the difference-in-differences setup. Thus, this difference-in-differences is considered as multiple countries with different timing. I then used the within-country variation in policy intensity across regions as the third source of variation to form my triple-differences design. As robustness checks for the triple-differences design, I further adopt a non-parametric local RD design.

I begin the analysis by estimating the impact of the education reform on maternal education. Next, using the reform exposure as an instrument for maternal education, I estimate maternal education's causal effect on child health. Finally, I present a heterogeneity analysis of maternal education's causal impact on child health, the possible mechanisms through which maternal education improves child health, and then discuss the results' robustness.

4.2 Baseline Triple-Differences Estimation Result

4.2.1 Increased maternal education

In this section, I examine the effect of exposure to the FPE reform on maternal education. Table 4.1 presents the first-stage regression results based on Equation (3.2), the preferred model. In this design, I instrument maternal education with an interaction between the indicator for policy exposure and reform intensity. Since the dependent variable maternal education varies across individuals while the instrument Z_{cj} varies by country and birth cohort, I take the different levels of aggregation into account by clustering standard errors by country and cohort, the level of the treatment.

Column 1 of Table 4.1 shows the results of estimating Equation (3.2) with the inclusion of only baseline controls such as child's gender and a full set of dummies for maternal birth cohort, survey year, childbirth year, and region fixed effects. The result indicates that exposure to the education policy has increased schooling years by 0.7 years for the affected cohorts. In columns 2 and 3, I show the results based on the inclusion of socioeconomic controls such as mother's religion and a full set of dummies for ethnicity and time-varying country characteristics such as physician density and pupils-teacher ratio. The inclusion of these controls barely changes the results, both quantitatively and qualitatively. The results consistently indicate that exposure to the education policy is estimated to have significantly increased the years of schooling by approximately 0.7 years for the affected cohorts.

The estimate marginally increased but remained qualitatively the same when I also control for the country-specific linear trend (interactions between the country dummies and birth year fixed effects) in Column 4. The inclusion of the country-specific linear trend addresses the issue of contemporaneous confounding factors within countries. The result indicates that exposure to the education policy is estimated to have significantly increased the years of schooling by approximately one additional year. In Column 5, I replace the country-specific linear trend with the region-specific linear trend to capture within-country changes across regions over cohorts. In Column 6, I allow the region-specific linear trend to change before and after the policy. I included pre-and-post-FPE region-specific linear cohort trends to address the more general parallel trends concern that the results could reflect differential regional-level cohort trends that country-specific cohort trends cannot capture. The inclusion of region-specific linear trend and the pre-and-post-FPE region-specific linear cohort trends slightly weakens the magnitude of the FPE policy's effect on maternal years of education to 0.9 years. The qualitative implication of the result remains the same. In the estimation, I assumed the region-specific trend is linear. However, it is often considered that even within a country, relatively advantaged and less advantaged areas are not good comparison groups. Also, Figure 3.3 shows the trend in years of schooling is non-linear. I checked the validity of this assumption controlling for the functional form of the trends to quadratic. The coefficient indicates that controlling for quadratic trend does not alter the result's qualitative implication, but there is a marginal increase in the point estimate. Thus, exposure to the education policy significantly increased schooling years by approximately one year for the affected cohorts, which suggests my result is robust. Therefore, I used the linear model (Column 6) as my preferred specification. For all estimates, the F-statistic value of the excluded instrument is well above 10, which means that the instrument has a substantial impact on our endogenous variable and can be assumed to be relevant.

Table 4.1 also presents the results for the probability that respondents have some education as a robustness check for years of schooling. The results indicate that exposure to the policy increases the likelihood of obtaining some education by 10 percentage points (or 16 percent). This result implies that the estimates obtained through measuring maternal education by total years of completed education are robust and do not suffer a possible bias caused by the censoring. Table 4.1 also reveals that the reform increased the likelihood of a female being literate by about 11 percentage points (or 23 percent). This result implies that the extra year of schooling does also correspond to improve learning; thus, I conclude that it is not the case that the reform reduced school quality. In comparison with previous studies, I use years of schooling.

The estimate of Africa's education policy's effect using the triple difference estimation strategy is somewhat comparable with previous estimates at a single country level. For instance, for Uganda, Keats (2018), Andriano and Monden (2019), and (Nagashima and Yamauchi (2020) reported that the UPE policy increased years of schooling by about 0.71 years 1.5 years, and 0.3 years, respectively. For Nigeria, Osili and Long (2008), Larreguy and Marshall (2017), and De Cao and La Mattina (2019) found that the FPE policy increased years of schooling by 1.54, 0.60, and 2.1 years, respectively. For Ghana, Boahen and Yamauchi (2018) also reported that the FCUBE policy implemented in 1996 increased schooling years by one year. Overall, the effect magnitude falls within the range of these past magnitudes.

4.2.2 Ordinary Least-Squares and Instrumental Variables Results

4.2.2.1 Impact of maternal education on child mortality

In this subsection, I examine the effect of maternal education on child mortality measures, including neonatal mortality, infant mortality, and under-five mortality. I consider the cross-sectional relationship between maternal education and child health by running simple ordinary least-squares (OLS) regressions of Equation (3.3). OLS estimates have no causal interpretation but are useful for understanding the data structure (Column 1). The result shows that an additional year of schooling is associated with a 0.1 percentage point reduction in the probability of dying within the first month of life. Table 4.2 also shows the causal effects of maternal education on child mortality using the baseline estimation design. The 2SLS estimates are displayed with and without the country-specific linear trend in Columns 4 and 5, respectively. Column 6 replaces the country-specific linear trend with the region-specific linear trend, and Column 7 allows the region-specific linear trend to vary before and after the policy. The inclusion of country-specific linear trend reduces maternal education's causal effect on death before the first birthday, but the result is statistically significant. The result becomes positive but insignificant when I included the region-specific linear trend. Although the result remains statistically insignificant, it becomes correctly signed after allowing the region-specific linear trend to vary before and after the policy. Column 7 (the preferred specification) indicates that an additional year of schooling reduces the probability of dying within the first month of life. In particular, the result shows that attending another year of education reduces the likelihood of death within the first month of life by 0.7 percentage points. Given that, on average, 6 percent of the children in the sample were reported to have died before their first birthday of life, the decrease in neonatal mortality from one year of education represents a decrease of approximately 12 percent, although insignificant.

Table 4.2 also shows the result of death before the first year of life. The OLS result in Column 1 shows that the additional year of schooling is associated with a 0.4 percentage point reduction in death probability before the first year of life. In line with the literature, the estimate of maternal education's causal effect on death before the first year of life suggests that increased female education had a negative impact on infant mortality in Africa (Columns 5, 6, and 7, Table 4.2). The inclusion of the country-specific linear trend (Column 5) slightly decreased maternal schooling years' causal effect on infant mortality. The extra year of schooling reduces the probability that a child dies before the first year of life by 1.9 percentage points, which is slightly lower than the estimate obtained without the country-specific linear trend. However, the effect further decreases when I control for the region-specific linear trend, but the point estimate increases from 0.013 to 0.017 after controlling for the pre-and-post-FPE region-specific linear cohort trends. In particular, in Africa, the likelihood of dying before the first year of life for children of mothers with one additional year of education reduces by 1.7 percentage points (Column 7). This estimate implies that the one-year increase in maternal education reduces the probability that a child dies before the first year of life by 24 percent, evaluated at the mean of 7 percent.

Finally, the probability of death before the age of 5 is significantly associated with maternal education. An additional year of schooling reduces the likelihood of under-five mortality by 0.2 percentage points. Here, too, in line with the literature, the result in Column 7, which accounts for the region-specific linear trend and the pre-and-post-FPE region-specific linear cohort trends, shows maternal education's effect on under-five mortality is statistically significant and correctly signed. A one-year increase in education significantly reduces the probability of dying before age five by about 1.4 percentage points, translating into a decrease of approximately 16 percent, evaluated at the mean of 9 percent.

Other studies such as Makate and Makate (2016) for Malawi and Masuda and Yamauchi (2018) for Uganda found that a year increase in maternal education reduces under-five mortality by 36 and 67 percent, respectively. The dissertation's results suggest that previous studies overestimated the causal impact of maternal education on under-five mortality. The result shows the difference between our estimate and estimates of earlier studies is about 20–51 percent.

Whiles Keats (2018) and Masuda and Yamauchi (2018) do not find a significant effect of maternal education on infant mortality; this study's result is consistent with other studies. For example, for Malawi, Makate and Makate (2016) found that a year increase in maternal education reduces infant mortality by 34 percent.

4.2.2.2 Impact of maternal education on child stunting, underweight, and wasting

Here, I examine maternal education's effect on child stunting, underweight, wasting, and anthropometric measures, including standardized z-scores for HAZ, WAZ, and WHZ. For this section and the subsequent sections, I focus only on the preferred model, which includes region-specific linear trends and the pre-and-post-FPE region-specific linear cohort trends for reporting the results. The OLS results suggest a negative association between years of schooling and the various nutritional measures. The 2SLS results in Table 4.3 show that one additional year of education significantly improves surviving children's dietary measures, such as child stunting and underweight. The baseline regression results indicate that a year increase in education reduces the probability of child stunting and underweight by 2.5 and 1.4 percentage points, respectively. Specifically, given that, on average, 26 percent of the children in our sample are stunted and underweight, the extra year increase in education decreases child stunting, underweight approximately 10 and 5 percent, respectively. The effect on child wasting is statistically insignificant.

These magnitudes are smaller than the results from past studies by about 7 to 27 percent. For instance, Keats' (2018) estimates for Uganda show that an increase in maternal education reduces child stunting of firstborns by 37 percent, and Makate and Makate's (2018) estimate for Zimbabwe indicates that one additional year increase in education reduces child stunting by 17 percent. This study's result contradicts the result obtained by Ali and Elsayed (2018). For instance, Ali and Elsayed (2018), using data for Egypt and an instrumental variable approach based on reduction in primary school length, show that maternal education does not affect nutritional status.

4.2.2.3 Impact of maternal education on anemia, diarrhea, and fever

Table 4.3 also reports the effect of maternal education on additional episodes of illness, including anemia, diarrhea, and fever. Here again, I focus on the model, which includes region-specific linear trends and the pre-and-post-FPE region-specific linear cohort trends for reporting the results. I find that children whose mothers were exposed to the policy are approximately 0.3 percentage points less likely to be anemic, 0.6 percentage points less likely to have diarrhea, and 2.4 percentage points less likely to suffer from fever. Given that, on average, 27 percent are anemic, 26 percent suffer from diarrhea and fever, the result translates into 1 percent, 2 percent, and 9 percent reduction in the probability of a child having anemia, sick with diarrhea or fever, respectively.

Table 4.3 shows that maternal education has a somewhat small effect on these illness episodes. Although maternal education's impact on the probability of a child suffering diarrhea is insignificant, the point estimate is much smaller than previous estimates. While the estimate in the paper shows that an additional year of maternal education decreases the probability that a child suffers from diarrhea by about 2 percent, Masuda and Yamauchi (2018) found that the extra year of maternal education reduces the likelihood that a child will suffer from diarrhea by 53 percent, and their result is also statistically insignificant. In terms of anemia, this paper's estimate is insignificant and minimal compared to the result obtained by Keats (2018), who found that a year increase in maternal education reduces the probability that a child is anemic by 7 percent.

4.2.3 Heterogeneous Analysis

This section explores the possibility of heterogeneous effects of maternal education on child health outcomes across gender. I do so by estimating Equation (3.3) separately for boys and girls, and I present the result in Table 4.4. As expected, this study finds that improved maternal education reduced mortality and improves the health of surviving children for both sexes. However, except for neonatal mortality, the results indicate that the effect is larger for girls than boys. Specifically, the result indicates that female children whose mothers are exposed to the policy are 2 percent less likely to within the first month of life, but male children are 17 percent less likely to die within the first month of life. However, the effect for female children is insignificant. Also, female children whose mothers are exposed to the policy are 26 percent and 24 percent less likely to die before the first birthday or die before the fifth birthday, respectively. On the contrary, male children whose mothers are exposed to the policy are 23 percent and 13 percent less likely to die before the first birthday or die before the fifth birthday, respectively but the estimate for under-five mortality is insignificant.

Also, female children are 16 percent and 13 percent less likely to be stunted or sick with a fever. On the contrary, male children are 7 percent and 6 percent less likely to be sick with a fever. However, the effect is statistically insignificant. This result implies that there seem to be consistent differences in impact across gender. This finding is consistent with Bourne and Walker (1991), who find that hat improved mothers' education reduced mortality at all ages below five years for both sexes. But the effect was found to be greater on the girl than on boy children in India.

4.2.4 Possible Mechanisms

As presented in Section 4.2, the results show a substantial effect of maternal education on child mortality and surviving children's health outcomes. In this section, I examine the impact of a mother's education on some key intermediate outcomes expected to improve child health. I classified these mechanisms into broader categories, as shown in Table 4.5. First, I explore the effects of expanded access to education on women's knowledge and ability to process information. As noted in the literature, education may improve women's literacy and numeracy levels, enhancing a woman's abilities to treat child illnesses (Glewwe, 1999). Table 4.5 reveals that a year increase in maternal education does significantly affect the mother's literacy. A one-year increase in schooling increases maternal literacy by 12 percentage points (or 25 percent). The results in Table 4.5 also show that schooling improves general health knowledge regarding HIV/AIDS transmission, the oral rehydration method used for treating diarrhea in children, modern contraceptive methods, and whether a respondent knows when a woman can get pregnant in the ovulation cycle. This study's result is consistent with some previous studies in Africa, such as Makate and Makate (2016, 2018) and Masuda and Yamauchi (2018). All concluded that an additional year of schooling enhances human capital measured by literacy and other health knowledge. This result implies that the education policy did not affect the quality of education. However, this result contradicts the result found by Ali and Elsayed (2018) for Egypt.

A second possible mechanism is the income effect via women's economic activities. Here, I measure women's economic activities using the probability of working or being employed by others. Table 4.5 shows that schooling increased the likelihood that a woman worked in the last year or be employed by others by approximately 2 percentage points, but the effects are statistically insignificant. This result is consistent with the result obtained by Masuda and Yamauchi (2018). They find that maternal education has no significant impact on the probability of working in any sector or being employed by someone other than a family member. This result implies that education improves human capital measured by literacy, but it has not led to significantly different jobs. This suggests the education effect on child health status through an own income increase is limited.

A third possible mechanism is the income effect via marriage. My result shows that the reform did not lead to any essential differences in the partner's years of schooling and age. This result implies that education does not influence the types of men women marry. I conclude that positive assortment does not play a role in maternal education's effect on child health outcomes in my study. This result is consistent with the findings obtained by other recent studies for African countries (e.g., Keats, 2018; Masuda and Yamauchi, 2018).

Next, I investigate the effect of education on the socioeconomic status of women. I define two dummy variables: one for being in the lowest wealth quintile and another for being in the highest wealth quintile. The result shows that more education results in a lower probability of being in the lowest wealth quintile by approximately 8 percentage points (or 22 percent) and increase the likelihood that these women move into the highest wealth quintile by 5 percentage points (or 12 percent). Additionally, women with more schooling are 2 percentage points (or 6 percent) more likely to reside in urban areas, but it is statistically insignificant.

Furthermore, I examine the impact of increased education on marriage, sexual behavior, and women's fertility preferences. I find that women with more education are more likely to get married by about approximately 4 percentage points—however, the additional year of schooling does delay marriage. The result also indicates that an additional year of schooling does not affect the age at which these women first have sex but increases the age at which they give birth to their first child by 33 percentage points. Table 4.5 also shows that the year increased in education decreases the number of children born, but the result is not significant. I compare the results in this study with some recent studies that have examined the causal effect of maternal education on fertility, especially in developing countries. Osili and Long (2008), using data for Nigeria, and Keats (2018) and Masuda and Yamauchi (2018), using data for Uganda, find that schooling has a negative and significant effect on the number of children ever born and the desired number of children. The point estimates of schooling's impact on the number of children ever born range from –0.09 to –0.52.

Finally, the sixth broader classification of the possible mechanism is mothers' health-seeking behavior necessary for children's survival. Here, I examine maternal education's effect on mothers' health inputs, vaccination, access to basic amenities, and breastfeeding practices. Under mothers' health inputs, the result indicates that maternal education has a statistically significant effect on postnatal care. That is, highly educated mothers are more likely to engage in health-seeking behavior such as postnatal care utilization. I also obtained a positive effect of maternal education on other mothers' health inputs, such as antenatal care but insignificant. Surprisingly, the effect of increased education on the probability of being tested for HIV/AIDS during pregnancy, having institutional delivery, and the likelihood of washing hands before feeding the child is negative although insignificant. Also, I failed to find support for passageways connected to vaccination and access to social to basic amenities.

Lastly, under the mothers' health-seeking behavior mechanism, I examine the effect of increase maternal education on breastfeeding practices. It is estimated that about 22% of neonatal deaths could be prevented if all infants are put to the breast within the first hour of birth (Edmond, Zandoh, Quigley, Amenga-etego, and Owusu-agyei, 2006). The results in Table 4.5 reveal that mothers with an additional year of schooling can breastfeed for an extended period. Thus, a year increase in education increases the duration of breastfeeding by about 3 months. My result indicates that an extra year of education increases the probability that an infant will be put to the breast within the first hour of birth by approximately 4 percentage points (or 11 percent) and reduces the likelihood that an infant will put to the breast after hours by about 3 percentage points (18 percent). It is also evident that increased education decreases the probability of breastfeeding a child after days 1 percentage point (17 percent). Therefore, this study's result reveals that a year increase in maternal education increases breastfeeding duration and ensures timely breastfeeding initiation.

4.3 Robustness

In this section, I conduct a falsification test to check the validity of the assumption. I carry out several robustness checks to demonstrate that the results obtained in this study are due to the FPE policy and not to any confounding factors or policies. Each country rolled out the FPE reforms alongside other social policies aimed at achieving the Millennium Development Goals. As the FPE reforms are only effective because they affect cohorts differently, it might be a concern that the exposure variable could pick up some unspecified time trends or structural changes in each country instead of the exact exposure effect.

4.3.1 Placebo treatment

A significant advantage of my cross-country estimation strategy for Africa is that I can also control for country-specific or region-specific linear time trends in addition to the cohort fixed effects. Supporting the parallel trends assumption, the results are robust to including region-specific quadratic cohort trends. As a result, a large part of any unobserved differences between pre-and-post reform individuals should be minimized or already eliminated. Beyond the inclusion of region-specific cohort trends on either side of the policy, I check the robustness of this assumption by verifying whether the estimation detects any effects for cohorts that should have been unaffected by the education policy by using a placebo test. For each country, I restrict the sample to those born before implementing the policy and defining exposure to the fake reform using these cohorts. I exclude *partially exposed* cohorts (the control group in the main analysis) because they were possibly still in primary school at the time of the reform-thereby guaranteeing that no respondent could have partially benefited from the policy. I test the placebo effect on years of schooling, child mortality, and dietary measures. The placebo reform should have no impact on these variables. As shown in Table 4.6, the results suggest that these cohorts did not experience any significant effects of the reform on years of education and child health outcomes.

4.3.2 Fuzzy regression discontinuity estimation

As explained in Section 3.3.3, due to the concern of sample selection, I examine the robustness of the triple-differences model by adopting the *local fuzzy RD* design. I focus only on females who Were born *soon before* and *after the pivotal cohort*, $c \in (\bar{c}_j - b, \bar{c}_j + b)$, conditional on a bandwidth *b.* I determine the bandwidth by running a leave-one-out cross-validation using the specification function in Lee and Lemieux (2010) and then pooled all the 20 countries together.

Table 4.7 presents the RD regression results for the first stage, and it contains the linear and quadratic function of the running variable, respectively. The policy's impact on years of education is significant at the bandwidth in all the different model specifications. The size of the policy impact on education is sensitive to the different model specifications. However, the result suggests a positive and significant effect of the policy on years of schooling. Using Akaike's information criterion (AIC), I test to see which specification best predicts the data conditional on the bandwidth. The AIC test indicates the linear specification as the best specification. Thus, the policy increased years of education by approximately 1.6 years. This point estimate is marginally higher than the DDD design estimate, and the RD's estimate's lower precision reflects the smaller samples available for the estimation. The RD's 2SLS analyses are based on local linear specifications.

Although the RD result in Table 4.8 further strengthens the triple-difference findings, most of the estimates are insignificant due to the smaller sample size. In some instances, there is a change in the sign for some variables, but insignificant.

4.3.3 Further robustness checks

Next, I consider the issue of fertility and sibling effects as potential threats to the identification. In the sample, some women have one child, and others have more than one. As a result, I compare the health outcomes of first-born children among women who were exposed to the education policy. This comparison of first-born children reduces differences in outcomes that may be due to other siblings' presence in the household. Table 4.9 shows that the results' qualitative and quantitative implications are similar to the results obtained for the full sample; however, the result becomes insignificant in some instances but remains correctly signed.

The primary measure of policy intensity was school enrollment since the introduction of the policy increased enrollment. However, the policy also increased student retention and primary school completion. Following De Cao and La Mattina (2019) and Larreguy and Marshall (2017), I provided an alternative definition of FPE intensity based on the primary completion rates using the control cohorts that had not completed primary school. Table 4.10 shows similar results for the first stage, suggesting that the results, especially the first-stage estimates are robust to alternative policy intensity operationalizations.

There is a concern about the systematic migration of respondents, which might bias our estimates. The data set does not allow me to identify the region in which the respondents received their education. However, one question asks respondents how long (in years) they have been living in their current residence. I created a dummy equal to 1 if the respondent answered "always" and 0 otherwise. As a result, about 30% of our respondents had never moved. Using the sample of *non-movers*, the first stage result (Table 4.10) shows that the point estimate of exposure to the policy on schooling years is almost the same but imprecise. However, the conclusion remains that the reform increased years of education by approximately one year.

Also, I subject the baseline specification to the following four alterations: (i) I drop the time-varying country characteristics; (ii) I introduce a new time-varying country characteristic; (iii) I exclude countries that experienced a dip in enrollment before the reform, and (iv) I exclude from the estimation sample one country at a time. As shown in Table 4.10, the estimated size of the reform's effect on years of education does not significantly change when I drop the time-varying country characteristics or introduce a new time-varying country characteristic.

4.4 Discussion and Conclusion

In this chapter, I study maternal education's role in improving child health in Africa by using exogenous variation in maternal education resulting from FPE reforms implemented in several African countries. I address the issue of contemporaneous confounding factors within countries by pooling all of the DHS data available from Africa and controlling for country-specific or region-specific cohort trends in child health and time-varying country characteristics. I outline the empirical findings as follows. The FPE policy seems to have had a substantial positive effect on years of schooling. Additional schooling can be as high as approximately one additional year for females exposed to the education reform. The estimates suggest that mothers' schooling does reduce child mortality. The mother's education also improves the health outcomes of surviving children, such as stunting, underweight, wasting, and fever. Also, the heterogeneous effects across sexes indicate that there are consistent differences in impact across gender.

I find supporting evidence toward channels through which maternal education may help improve child health. The result implies that mothers with more years of education can enhance their ability to process information. Also, the extra year of school changes the health-seeking behavior of women. After childbirth, women use postnatal care centers, which is necessary to improve child health and reduce child mortality. Additionally, maternal education mechanically increases the probability that a woman will get married, but it does not affect their fertility. Finally, I do not find support for mechanisms that involve positive assortative mating, which suggests that a father's education does not play a role in the effects measured in this paper.

CHAPTER 5

ESTIMATING THE EFFECT OF MATERNAL EDUCATION ON FEMALE GENITAL CUTTING USING MICRODATA FROM 7 AFRICAN COUNTRIES

5.1 Introduction

This chapter investigates the causal effects of maternal education on female genital cutting using microdata from 7 African countries. As in chapter 4, to address the issue of endogenous education, I exploit the roll-out of the education reform across the 7 implementing countries in Africa. This chapter proceeds as follows: In section 5.2, I estimate the policy's impact on education years. Using policy exposure as an instrument for years of education, I estimate maternal education's causal effect on a daughter's FGC prevalence. The regression results presented in section 5.2 are based on the 2SLS specifications in Equation 3.3. In section 5.3, I further investigate the possible mechanisms behind my results. For all estimates in this chapter, I take the different aggregation levels into account by clustering standard errors by country and cohort, the level of the treatment.

5.2 Baseline Triple-Differences Estimation Result

5.2.1 Increased maternal education

This section examines exposure to the FPE reform on maternal education years using data from the 7 African countries. I report the regression results with various specifications; however, I base the discussion in this study on the specification that includes region-specific linear cohort trend and the region-specific linear trend before and after the policy (Column 6 in Table 5.1). The result based on the preferred specification shows that exposure to the reform increased maternal schooling years by about 21 percent, which corresponds to approximately 0.9 years with a standard error of about 0.16. These magnitudes are in previous studies' ballpark using free primary education policy in Africa

(Fenske, 2015; Larreguy and Marshall, 2017; Masuda and Yamauchi, 2018; Osili and Long, 2008). In the first stage, the *excluded instrument's F-statistics* is 34, indicating that it is sufficiently correlated with schooling years.

5.2.2 Impact of maternal education on female genital cutting

Having established a substantial first stage, I next turn to the 2SLS estimates for the effect of maternal years of schooling on the probability that the respondent's daughter undergoes FGC, as shown in Table 5.2. The OLS result shows a negative association between maternal years of education and the daughter's FGC. Specifically, a one-year increase in maternal education reduces daughters' genital cutting by 0.1 percentage points.

The 2SLS estimates show the effect of an additional year of education on the decision to cut is negative and significant. When not controlling for country-specific linear cohort trend, giving a mother an additional year of education reduces the likelihood that her daughter undergoes genital cutting by 7 percentage points (Column 4). This point estimate is significantly different from zero. When controlling for country-specific linear cohort trend, the estimated effect is stable at 7 percentage points and is still negative and significant (see Column 5). The result increased to approximately 9.6 percentage points when I replaced the country-specific linear trend with the region-specific linear cohort trend (Column 6). The result is robust at around 9.5 percentage points when I further allowed the region-specific linear cohort trend to vary before and after the reform (Columns 7). Therefore, based on the preferred model (Column 7), the result indicates that the additional year of education tends to reduce a mother's propensity to cut her daughter by approximately 9.5 percentage points. This result translates into a 86 percent reduction in the daughter's FGC prevalence relative to a base of 11 percent. The higher precision of these 2SLS estimates reflects the larger sample available for the estimation.

This finding in this study is consistent with the negative effect of women's schooling on daughters' FGC obtained by Nesje (2014) and De Cao and La Mattina (2019). Both studies found that a year increase in maternal education reduces the prevalence of female genital cutting by 1.4 percentage points and 0.3 percentage points, respectively. However, the findings differ from those reported by De Cao and La Mattina (2019), who found a nonsignificant effect of maternal education on the probability that a daughter undergoes FGC. There are two main possible explanations for this inconsistency. First, their studies used different samples, which could have biased their estimates. In particular, De Cao and La Mattina (2019) considered only the eldest daughter, and using different samples leads to selection bias. Second, while De Cao and La Mattina (2019) exploit variation in reform intensity at the more disaggregated state level, I measure intensity at the region level.

5.2.3 Possible Mechanisms

To understand the direct mechanism driving this decline in FGC in Africa due to the additional year increased in maternal years of schooling, I examine the role of women's attitude toward FGC and women's bargaining power. The attitude toward FGC is one possible channel to explain why education may reduce FGC. I measured women's attitude toward FGC with a dummy equal to 1 if the respondent thinks FGC should continue; 1 if the respondent intends to cut daughter in the future; and zero otherwise. Table 5.3 shows that one additional year of education improved women's attitude toward FGC. Specifically, the extra year increase in schooling reduced the likelihood that a woman will say FGC should continue by, on average, 12 percentage points (or 22 percent).

One additional year increase in education also reduces the likelihood that a woman intends to cut the daughter in the future by approximately 5 percentage points (or 9 percent). Due to data limitations, I am unable to examine the partner's attitude toward FGC. Overall, the results on attitudes are consistent with the findings on behavior and suggest that exposure to the FPE reform significantly changes women's attitudes toward FGC in Africa. However, De Cao and La Mattina (2019) found no significant effect of education on women's attitudes toward FGC.

It is, however, essential to note that these two measures are self-reported and very sensitive. As a result, the measures of attitude towards FGC may be affected by measurement error, such as non-report and social desirability, which may bias the result. It would have been interesting to provide an objective alternative measure of FGC attitude, but I cannot carry out this exercise due to data limitation.

Next, I discuss how education increases the bargaining power of women in a marriage setting. Couples tend to disagree when negotiating deals, such as cutting daughters (UNICEF, 2013). Under the assumption that women are less tolerant of female genital cutting than men, their daughter's FGC probability will decrease as the mother has a more substantial say in the household. This is because education will empower women financially and vocally to encourage informed decision-making and make their own independent choices to stand up and not be undermined in a marriage setting. As women empowerment measures, I explored the gap in age and schooling years between respondents and their partners. Also, I explore the participation of women in household decision-making.

The results in Table 5.3 suggest that the gap in educational years between the respondent and her partner is statistically different on average for those affected by the reform. Thus, a year increase in female education reduces the partners' education gap by about one year. This estimate also implies that a year increase in female education reduces the age gap between partners by about 0.3 years but insignificant.

Furthermore, schooling had a significant effect on women's participation in household decision-making. Specifically, the year increased in maternal education increased affected women's ability to decide on their health. These women are also more likely to decide on family visits and large purchases within the household. However, these women have no power over their partner's money.

Based on these findings, I posit that the extra year of female education reduces the education gap and, to some extent, the age gap, which is likely to increase the respondents' bargaining power relative to that of their partners.

5.3 Robustness

This section presents falsification tests and several sensitivity checks to demonstrate that the results obtained in this study are due to the FPE policy and not to any confounding factors. In Section 5.3.1, I present the robustness of our estimates to placebo reform. I examine the issue of sample selection by applying the fuzzy regression discontinuity model in Section 5.3.2. Finally, I investigate the robustness of the estimates concerning the intensity measure, fertility and sibling effects, migration, and samples (Section 5.3.3).

5.3.1 Placebo treatment

Each country rolled out the FPE reforms alongside other social policies aimed at achieving the Millennium Development Goals. As the FPE reforms are only effective because they affect cohorts differently, there is the concern that the exposure variable could pick up some unspecified time trends or structural changes in each country instead of the exact exposure effect. I checked the robustness of this by verifying whether the estimation detects any effects for cohorts that should have been unaffected by the education policy. Thus, people who would have been too old (pre-reform cohorts) at the time of policy implementation to benefit from the FPE policy. As shown in Columns 1 and 2 of Table 5.4, the result suggests that the placebo reform did not significantly impact schooling years. As the placebo reform did not affect schooling years, I further used the reduced form estimates to test for a placebo effect. Columns 3 and 4 of Table 5.4 indicates that the placebo reform has no impact on the FGC prevalence. This result implies that other unobserved mechanisms do not drive the estimates.

5.3.2 Fuzzy regression discontinuity estimation

Table 5.5 presents the RD regressions results for both the first-stage and the IV estimation. The regressions include a region-specific trend and allow the region-specific trend to vary before and after the policy and two different specifications of the region-specific trends in cohorts, a linear and a quadratic trend. Panel A of Table 5.5 shows the estimated coefficients of exposure on years of education. Panel B shows the estimated coefficient of maternal education on the probability that a daughter undergoes FGC. The result indicates a positive and significant effect of the policy on years of schooling. Thus, the policy increased years of education by approximately one additional year. This point estimate is the same as the estimate obtained for the DDD design, but these estimates' lower precision reflects the smaller samples available for the estimation. Consistently, the coefficients of the 2SLS regressions show the same sign and almost the same magnitude as the DDD estimates: years of education reduces the prevalence of FGC by approximately 8 percentage points (or 67 percent). Overall, the RD result further strengthens the findings obtained from the triple-difference, and these DDD results are robust to the sample selection.

5.3.3 Further robustness checks

The primary measure of policy intensity was school enrollment since the introduction of the policy increased enrollment. However, the policy also increased student retention and primary school completion. Following De Cao and La Mattina (2019) and Larreguy and Marshall (2017), I provided an alternative definition of FPE intensity based on the primary completion rates using the proportion of control cohorts of the region population not completed primary school. The dependent variable in Panel A of Table 5.6 is the years of education achieved by the individual. Based on the preferred estimation (Column 4), the result shows that the policy increased schooling years by about 1 more year. The 2SLS estimates (Panel B) show one year of education decreases the likelihood of FGC by 13 percentage points. This result shows the estimates obtain in Section 5.2.2 robust to the measure of intensity.

There is a concern about the systematic migration of respondents, which might bias our estimates. The data set does not allow us to identify the region in which the respondents received their education. However, one question asks respondents how long (in years) they have been living in their current residence. I created a dummy equal to 1 if the respondent answered "always" and 0 otherwise. As a result, about 49 percent of the respondents had never moved. Using the sample of *non-movers*, the first stage result (Table 5.7) shows that the reform increased years of education by approximately one extra year; however, this result is less precise than the result obtained in Table 5.2. Table 5.7 also presents 2SLS results obtained from this sample of "*non-movers*." The point estimate of the effect of an extra year of schooling on female genital cutting is not different from what Table 5.2 shows. Still, qualitatively, education reduces the probability that a daughter undergoes FGC by about 9 percentage points.

Next, the attitude measures are available in a sample different from that for the main analysis. The sample for attitude measures, especially the sample for intends to cut the daughter in the future, is considerably smaller than the main analysis sample. As a result, I replicate the first-stage and 2SLS estimates using this smaller sample and present the results in Table 5.8. I confirm the main result in terms of the direction of the effects: the policy exposure increases maternal education and the causal effect of maternal education on FGC prevalence is negative, i.e., education decreases FGC prevalence. The magnitude of policy exposure on maternal education is higher than the estimate in Table 5.1. Still, education's impact on the FGC prevalence is similar to the one estimated using the main sample.

Furthermore, some daughters might be too young and still at risk of being cut at the survey time. Following De Cao and La Mattina (2019), I limited the sample to mothers with at least one daughter older than five years. Table 5.9 presents the results, and the point estimate of the full model (Column 4) is smaller compared to column 7 in Table 5.2. However, the main inference does not change. That is, a year increased in schooling has a negative and significant effect on the FGC prevalence.

I also consider the issue of fertility and sibling effects as potential threats to the identification. In the sample, some women have one daughter, and others have more than one. As a result, I compare the FGC of eldest daughters among women who were exposed to the education policy. This comparison of eldest daughters reduces differences in outcomes that may be due to other siblings' presence in the household. Table 5.10 shows that the results' qualitative implication is pretty much the same as the results obtained for the full sample; however, the magnitudes obtained for this estimation is smaller than the whole sample. Specifically, an extra year of education decreases FGC's prevalence by 5 percentage points, and the effect is statistically significant.

5.4 Discussion and Conclusion

In the past few decades, there has been increasing international interest in FGC due to its diversity and its short- and long-term effects on women. As a result, the practice is recognized as a violation of girls' and women's fundamental human rights. The SDG-5 includes a target to eliminate all harmful practices, including FGC. Education is often advocated as one of the vital steps for eradicating FGC, but empirical evidence on the causal impact of education on FGC is limited.

For this paper, I studied maternal education's role in eliminating FGC in Africa by using the exogenous variation in maternal education that resulted from FPE reforms implemented in several African countries. I addressed the issue of contemporaneous confounding factors within countries by pooling all of the DHS data available from Africa and controlling for trends in education, FGC practice, and time-varying country characteristics. To capture the regional differences in the pre-reform enrollment rate, I constructed a *policy intensity* measure, defined by the proportion of females in region r of country j who had not entered the primary school before the reform. I also allowed the cohort linear trends to vary with reform. The results show that the FPE policy seems to have had a significant positive effect on schooling years. Being exposed to the policy increases years of education by approximately one year for females.

The 2SLS results suggest that an additional year of schooling reduces a daughter's FGC prevalence by approximately 9.5 percentage points. These estimates account for about 86 percent of the sample mean. Also, I investigated mechanisms through which maternal education could help end FGC. My estimates indicated an increase in female education improves women's FGC attitude and increases the female respondents' position relative to their husbands. Having performed a series of robustness checks and placebo tests, I argue that these effects represent the causality of education on female genital cutting.

This study's contribution to the literature is that there is almost no large-scale empirical analysis investigating the causal link between maternal educational attainment and FGC prevalence in Africa. Therefore, this study happens to be the first to show that FPE reform in Africa increased female educational attainment and substantially reduced FGC's prevalence. This study contributes to the scarce literature investigating the use of educational reforms to examine the causal link between education and FGC in Africa.

The results provide a potential policy implication. Thus, this study's results highlight female empowerment's vital role through formal education in discontinuing female genital cutting. Formal education will help to change attitudes toward an entrenched, gender-biased cultural practice such as FGC. This change in attitudes could translate into behavioral change, which could help curb the prevalence of FGC in the next generation. As a result, any educational reforms that affect individuals from low-income households—who are mainly at risk of not attending school—can play a significant role in putting an end to such a harmful practice in Africa.

CHAPTER 6
Conclusion

This chapter summarizes the key findings from chapters 4 and 5 and discusses those findings' policy implications.

6.1 Summary

Education, mostly maternal education, has been regarded by many as an essential policy tool in tackling some of the world's problems, such as improving child health and eliminating all harmful practices, such as child, early and forced marriage, and female genital cutting. This dissertation examines the causal effect of maternal education on two pressing issues in Africa. Specifically, I first estimated the causal effect of maternal education on child health using microdata from 20 African countries. Second, I examined the causal effect of maternal education on the prevalence of female genital cutting using microdata from 7 African countries.

I address endogeneity by exploiting the rollout of the Free Primary Education (FPE) program for the period 1976–2007 across the implementing countries in Africa. I include all types of primary education reforms that abolish tuition fees. The policy rollout induces variation in policy exposure both within and between birth cohorts. Additionally, to capture regional differences in the pre-reform enrollment rate, I construct a *policy intensity* measure that serves as the third source of variation, motivating me to adopt a triple-differences strategy. Furthermore, I address the issue of contemporaneous confounding factors within countries by holding constant several time-varying country characteristics and the country (or region)-specific linear cohort trends.

Chapter 4 of this dissertation presented insightful findings. First, the abolition of tuition fees for primary school appears to have substantially impacted females' educational attainment in Africa. Thus, being exposed to the FPE leads to an increase in schooling by approximately one extra year. Also, I find that a one-year increase in schooling years reduces child mortality under age five and improves the health status of surviving children. However, this study's estimation results are generally smaller but more precise than previous studies in Africa. The mechanism behind the effects is likely that more educated mothers have substantially higher literacy, a high ability to process information, and initiate a timely and more prolonged breastfeeding period.

Chapter 5 of this dissertation offered important results. The two-stage least squares (2SLS) estimates show that the extra schooling year decreased a daughter's FGC's prevalence rate by 10 percentage points. The estimates indicated that the decrease in FGC is produced by improving women's attitude toward FGC and increasing women empowerment and female respondents' position relative to their husbands.

The findings of this study apply to a subset of African countries. Hence, the question naturally arises as to whether the result holds for Africa in general. The evidence of these studies suggests an affirmative answer to this question, given that our sample covers a rather broad group of African countries: drawing on North Africa (Egypt); Central or Middle Africa (Cameroon); Eastern Africa (Burundi, Ethiopia, Kenya, Malawi, Mozambique, Rwanda, Tanzania, Uganda, and Zambia); Western Africa (Benin, Burkina Faso, Ghana, Liberia, Mali, Nigeria, Sierra Leone, and Togo); and Southern Africa (Zimbabwe). In the study reported in chapter 4, our sample covers North Africa (Egypt); Eastern Africa (Ethiopia, Kenya, and Tanzania); and Western Africa (Mali, Nigeria, Sierra Leone). There is no country from southern Africa because southern African countries do not practice FGC. Countries in the south part of Africa engage in a somewhat different practice, *labia pulling.*

6.2 Policy Implications

The findings from this study have substantial policy implications. The results point to the vital role that education reforms can play in promoting formal education attainment by females and, as a result, improving child health and discontinuing the practice of female genital cutting. Formal education helps increase knowledge, process information, and change attitudes toward entrenched, gender-biased cultural practices such as FGC. Such an attitude change would surely help reduce the prevalence of FGC in the next generation. It suggests that if the remaining African countries eliminate primary education fees, then the free primary education for girls could have substantial implications for reducing child mortality, improving the health outcome for surviving children, and eliminating the practice of female genital cutting in Africa. Also, already implementing countries should put in measures that will encourage enrollment and retention in schools.

The results also indicate that education reforms that affect individuals from low-income households—the population mainly at risk of not attending school—can strongly enhance child health and end the harmful FGC practice in Africa. The importance of financial assistance from grade one onward, rather than only in the higher grades, and especially for girls from low-income families, is not ignored.

6.3 Limitations and Future Research

Previous studies such as Breierova and Duflo (2004), Chou et al. (2010), and Ali and Elsayed (2018) have demonstrated that paternal education is essential in improving child health. However, in this dissertation, I focused mainly on maternal education, thereby ignoring paternal education. The omission of paternal education will lead to the overestimation of maternal education's causal effect (Breierova and Duflo 2004; Chou et al. 2010). Although positive assortative mating does not play a role in this study, future research can investigate the causal effect of paternal education on child health and the prevalence of female genital cutting in Africa.

Next, this dissertation used the FPE policy as the source of exogenous variation in maternal education. This policy generated variation in access to schooling only at the primary school level. Given that some countries in Africa have started implementing free secondary education, it will be interesting for future studies to examine continued education's causal effect. It is possible that more schooling at the secondary level may produce a different result.

Finally, in this dissertation, I exploit variation in reform intensity at a higher level (regional level), which might not be close to the respondent. I could not verify the robustness of the intensity variable at a more disaggregated level. As a result, it will be interesting for future studies to define the intensity at a more disaggregated level.

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Table 3.1: Free primary education policy by African countries

Country	Year of reform		Official entry age	Windfall entry age	DHS cohorts Partially exposed	Fully Exposed
Benin	2006	a	6	9	1992-1997	1998-2003
Burkina Faso	2007	b	6	8	1997-1999	2000-2002
Burundi	2005	c	7	10	1990-1995	1996-2001
Cameroon	2000	d	6	8	1989-1992	1993-1996
Egypt	1981	e	6	7	1965-1974	1975-1984
Ethiopia	1996	f	7	11	1976-1985	1986-1995
Ghana	1996	g	6	8	1979-1988	1989-1998
Kenya	2003	h	6	9	1989-1994	1995-2000
Liberia	2001	i	6	12	1980-1989	1990-1999
Malawi	1994	f	6	10	1975-1984	1985-1994
Mali	1992	j	7	9	1974-1983	1984-1993
Mozambique	2005	k	6	11	1986-1994	1995-2003
Nigeria	1976	l	6	8	1959-1968	1969-1978
Rwanda	2003	m	7	11	1985-1992	1993-2000
Sierra Leone	2001	n	6	9	1984-1992	1993-2001
Tanzania	2002	f	7	9	1985-1993	1994-2002
Togo	2008	o	6	10	1995-1998	1999-2002
Uganda	1997	p	6	10	1978-1987	1988-1997
Zambia	2002	f	7	9	1989-1993	1994-1998
Zimbabwe	1980	q	6	8	1963-1972	1973-1982
Other implementing countries not in this study						
Botswana	1980	r	Data not in DHS public domain			
D.R. Congo	2010	s	Affected cohorts are too young			
Equatorial Guinea	2009	t	Data not in DHS public domain			
Guinea-Bissau	2002		No DHS data			
Lesotho	2006	u	Only two post-reform birth cohorts in DHS			
Namibia	2013	v	Affected cohorts are too young			
Tunisia	1958	w	Affected cohorts are too old			

Note: "Windfall entry age" is the age with the largest fall in the number of enrollments in primary school given a country, according to our histogram plots in Figure B3.l using DHS survey data just before the reform.

Sources: a Mcnabb (2018) and Koski et al. (2018). b Kouraogo & Dianda (2008) and Koski et al. (2018). c Heymann and Raub (2014), Travaglianti (2016), and Koski et al. (2018). d Koski et al. (2018). e UNESCO (2006, 2008). f Al- samarrai and Hassan Zaman (2007), Hoogeveen and Rossi (2013), and Koski et al. (2018). g Akyeampong (2009), Boahene and Yamauchi (2018), and Koski et al. (2018). h Lucas and Mbiti (2012). i Waydon, Ying, and Ketter (2016). j Godefroy and Lewis (2018). k Koski et al. (2018). l Osili and Long (2008). m Nkurunziza, Broekhuis, and Hooimeijer (2012) and Koski et al. (2018). n Cannonier and Mocan (2018). o Djahini-Afawoubo (2016). p Deininger (2003), Grogan (2009), Masuda and Yamauchi (2018), Keats (2018), and Koski et al.

(2018). q Grepin and Bharadwaj (2015) and Makate-Matake (2018) r Al-samarrai (2003). s World Bank (2005). t Naseef and Reuter (2013). u Koski et al. (2018) and Urwick (2011). v Iipinge, Likando, Haipinge, and Claassen (2013). w Frini and Muller (2012).

Table 3.2: Cohorts exposed to the reform of free primary education by country

Country	DHS cohorts		Youngest birth cohort exposed	Final DHS data
	Partially exposed	Fully exposed		
Egypt	1965-1974	1975-1984	1984	2014
Ethiopia	1976-1985	1986-1995	1995	2015/16
Kenya	1990-1994	1995-1999	1999	2014
Mali	1974-1983	1984-1993	1993	2018
Nigeria	1959-1968	1969-1978	1978	2018
Sierra Leone	1987-1992	1993-1998	1998	2013
Tanzania	1987-1993	1994-2000	2000	2015/16

Note: These are the cohorts used for the FGC analysis. The cohorts for Kenya, Sierra Leone, and Tanzania are different from those in Table 3.1 because here, I link mothers and daughters. Also, for these three countries, the final data is different. While the final data for these three countries in Table 3.1 is the MIS data as listed in Table A3.1, the final data for Table 3.2 comes from the DHS data.
Source: DHS dataset, listed in Table A3.2.

Table 3.3: Descriptive statistics

Child outcome and maternal education:	Mean	SD	Sample size
Maternal education (years) for child health	4.56	4.56	322,592
Maternal education (years) for FGC	4.27	5.61	195,486
Maternal education (some education)	0.64	0.48	322,592
Child is male	0.51	0.50	322,592
Early childhood mortality:			
Died within first month per 1,000 live births (Neonatal)	0.06	0.24	322,592
Died before the first birthday per 1,000 live births (Infant)	0.07	0.27	322,592
Died before the fifth birthday per 1,000 live births (Under 5)	0.09	0.28	322,592
Child anthropometric measures:			
Height-for-age z-score	-1.23	2.02	248,996
Weight-for-height z-score	-1.08	1.99	249,005
Weight-for-age z-score	-1.04	1.24	249,005
Stunting (height-for-age z-score < –2 SD)	0.26	0.44	248,996
Wasting (Weight-for-height z-score < –2 SD)	0.20	0.40	249,005
Being underweight (Weight-for-age z-score < –2 SD)	0.26	0.44	249,005
FGC measures:			
Daughter is cut	0.11	0.32	195,486
FGC should continue	0.52	0.50	155,264
Intend to cut daughter in the future	0.57	0.50	27,605
Health status of surviving children:			
Anemia	0.27	0.44	285,720
Diarrhea	0.26	0.44	292,685
Fever	0.26	0.44	293,433
Knowledge and ability to process information:			
Literacy	0.49	0.50	322,592
Comprehensive knowledge about HIV/AIDS	0.02	0.14	322,592
Knows oral rehydration method	0.58	0.49	322,592
Knows any modern contraceptive	0.23	0.42	322,592
Knows when in the ovulation cycle a woman can get pregnant	0.14	0.35	322,592
Income effect of education via work:			
Working	0.43	0.50	322,592
Employed by others	0.14	0.35	322,592
Income effect of education via marriage:			
Partner’s completed years of schooling	5.72	5.07	251,734
Partner’s age in the survey year	34.99	9.62	231,815
Socioeconomic status:			
Live in urban	0.27	0.44	322,592
Low wealth	0.38	0.49	322,592
High wealth	0.45	0.50	322,592
Behavior channels- sex, marriage, and fertility:			
Married	0.77	0.42	322,592
Married by age 14	0.14	0.34	322,592

Age at first sex	16.68	3.05	231,323
Age at first birth	18.99	3.51	281,339
Number of children	3.30	2.09	322,592
The ideal number of children	5.38	8.17	253,242
Behavior channels- mother's health inputs:			
Got tested for HIV/AIDS during pregnancy	0.01	0.69	322,592
Institutional delivery	0.66	0.47	322,592
Use antenatal care	0.87	0.34	322,592
Use postnatal care	0.41	0.87	105,765
Wash hands before feeding child	0.08	0.27	322,592
Vaccines:			
Polio	0.45	0.50	322,592
Measles	0.44	0.50	322,592
Tuberculosis	0.60	0.49	322,592
Diphtheria Pertussis Tetanus	0.46	0.50	322,592
Amenity:			
Access to family planning	0.10	0.31	250,256
Access to toilet	0.78	0.42	322,592
Breastfeeding:			
Duration of breastfeeding (in months)	34.91	36.38	274,166
Timely breastfeeding	0.35	0.48	322,592
Breastfed child after hours	0.18	0.38	322,592
Breastfed child after days	0.07	0.25	322,592
Household decision-making:			
Own health decision	0.53	0.50	165,540
Daily purchase decision	0.58	0.49	61,515
Family visit decision	0.56	0.50	165,535
Large purchase decision	0.42	0.49	165,535
The decision about husband money	0.42	0.49	126,919

Notes: DHS provides the z-score for each child's anthropometric measure, using the deviation from the median of the reference group, divided by the standard deviation of the reference group given the same age and sex. The negative z-scores for height-for-age, Weight-for-height, and Weight-for-age suggest that African children are shorter and lighter than the average children in the USA of the same age and sex, in terms of the standard deviation of that African country.

Source: DHS datasets

Table 4.1: Effect of whether exposed to free primary education on maternal education and literacy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable= Years of education [Mean=4.6]						
Intensity*Post-FPE	0.656	0.749	0.749	1.061	0.892	0.929	1.326
	(0.060)***	(0.060)***	(0.060)***	(0.071)***	(0.076)***	(0.083)***	(0.140)***
F-statistics	120	158	158	222	139	126	90
Adjusted R-squared	0.32	0.34	0.34	0.35	0.35	0.35	0.35
	Dependent variable= Some education [Mean=0.64]						
Intensity*Post-FPE	0.150	0.147	0.147	0.115	0.073	0.101	0.158
	(0.007)***	(0.007)***	(0.007)***	(0.008)***	(0.009)***	(0.009)***	(0.016)***
F-statistics	509	500	500	228	71	117	100
Adjusted R-squared	35	37	37	38	38	38	39
	Dependent variable= Literacy [Mean=0.49]						
Intensity*Post-FPE	0.109	0.108	0.108	0.119	0.101	0.114	0.136
	(0.007)***	(0.007)***	(0.007)***	(0.008)***	(0.009)***	(0.010)***	(0.017)***
F-statistics	252	251	251	229	131	139	65
Adjusted R-squared	28	30	30	31	31	31	31
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls		Yes	Yes	Yes	Yes	Yes	Yes
Time-varying country characteristics			Yes	Yes	Yes	Yes	Yes
Country-specific linear trend				Yes			
Region-specific polynomial trend					Linear	Linear	Quadratic
Region-specific polynomial kink						Linear	Quadratic

Notes: Sample size = 322,592. “Basic controls” include region fixed effects, survey year fixed effects, the child’s year-of-birth dummies, and child’s gender. “Socioeconomic controls” include dummies for the household’s religion and ethnicity. “Time-varying country characteristics” include the pupil-teacher ratio and the number of physicians per 1,000 people. In column (5), I replaced the country-specific linear trend with the region-specific linear trend. In columns (6) and (7), I controlled for pre-and-post-FPE region-specific linear and quadratic cohort trends, respectively. For each region within countries, I measure "policy intensity" by the pre-FPE non-enrolment rate of youths aged 15-18 in the most recent pre-FPE DHS. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.2: IV estimated effects of years of maternal education on child mortality

Dependent variables:	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
Neonatal	-0.001	-0.019	-0.014	-0.014	-0.007	0.001	-0.007
[Mean=0.06]	(0.0001)***	(0.005)***	(0.004)***	(0.004)***	(0.004)**	[-0.01, 0.01]	[-0.02, 0.002]
Infant	-0.004	-0.023	-0.020	-0.020	-0.019	-0.013	-0.017
[Mean=0.07]	(0.0001)***	(0.006)***	(0.005)***	(0.005)***	(0.004)***	(0.006)**	(0.006)***
Under-five	-0.002	-0.028	-0.023	-0.023	-0.023	-0.014	-0.014
[Mean=0.09]	(0.0002)***	(0.006)***	(0.005)***	(0.005)***	(0.004)***	(0.006)**	(0.006)**
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes		Yes	Yes	Yes	Yes	Yes
Time-varying country characteristics	Yes			Yes	Yes	Yes	Yes
Country-specific linear trend					Yes		
Region-specific linear trend	Yes					Yes	Yes
Region-specific linear kink	Yes						Yes

Notes: I measure mortality rates per 1,000 live births within the first month of life (neonatal), before the first year (infant), and before the fifth birthday (under-five). “Basic controls” include region fixed effects, survey year fixed effects, the child’s year-of-birth dummies, and child’s gender. “Socioeconomic controls” include dummies for the household’s religion and ethnicity. “Time-varying country characteristics” include the pupil-teacher ratio and the number of physicians per 1,000 people. In column (5), I replaced the country-specific linear trend with the region-specific linear trend, and in column (6), I controlled for pre-and-post-FPE region-specific linear cohort trends. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.), and I report the confidence interval at the 95 significance level in [.]. ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.3: IV estimated effects of maternal education on the health status of surviving children

	(1) OLS	(2) 2SLS
Dependent variables:		
Stunting	-0.004	-0.027
[Mean=0.26]	(0.0002)***	(0.009)***
Underweight	-0.004	-0.014
[Mean=0.26]	(0.0002)***	(0.009)*
Wasting	-0.001	-0.010
[Mean=0.20]	(0.0002)***	[-0.027, 0.007]
Height-for-age z-score	0.013	0.079
[Mean=-1.23]	(0.001)***	(0.041)*
Weight-for-age z-score	0.015	0.054
[Mean=-1.04]	(0.001)***	(0.026)**
Weight-for-height z-score	0.008	0.032
[Mean=-1.08]	(0.001)***	[-0.047, 0.111]
Anemic	-0.002	-0.003
[Mean=0.27]	(0.0002)***	[-0.018, 0.012]
Diarrhea	-0.002	-0.006
[Mean=0.26]	(0.0002)***	[-0.022, 0.010]
Fever	-0.001	-0.024
[Mean=0.26]	(0.0002)***	(0.008)***
Baseline controls	Yes	Yes
Socioeconomic controls	Yes	Yes
Time-varying country characteristics	Yes	Yes
Region-specific linear trend	Yes	Yes
Region-specific linear kink	Yes	Yes

Notes: I define stunting as height-for-age z-score < –2 standard deviation (SD), wasting as weight-for-height z-score < –2 SD, and being underweight as weight-for-age z-score < –2 SD. Sample size = 248,996 for those stunting, 249,005 for underweight and wasting measures, 285,720 for the anemic indicator, and 292,685 for the dummies for having diarrhea, and 293,433 for fever. "Basic controls" include region fixed effects, survey year fixed effects, the child's year-of-birth dummies, and child's gender. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels, and are reported in (.) and I report the confidence interval at the 95 percent significance level in [.]. $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.4: Heterogeneity of the IV estimated effects of maternal education on child mortality and the health status of surviving children

	Boys			Girls		
	Coefficient	SE [CI]	Sample size [Mean]	Coefficient	SE [95% CI]	Sample size [Mean]
Dependent variables:	(1)	(2)	(3)	(4)	(5)	(6)
Neonatal	-0.012	(0.007)*	164,186 [0.07]	-0.001	[-0.015, 0.012]	158,406 [0.06]
Infant	-0.018	(0.007)**	164,186 [0.08]	-0.018	(0.008)**	158,406 [0.07]
Under-five	-0.012	[-0.027, 0.003]	164,186 [0.09]	-0.019	(0.009)**	158,406 [0.08]
Stunting	-0.018	[0.040, 0.003]	125,957 [0.27]	-0.039	(0.014)**	123,039 [0.25]
Underweight	-0.013	[-0.036, 0.009]	125,961 [0.27]	-0.017	[-0.044, 0.010]	123,044 [0.26]
Wasting	-0.008	[-0.028, 0.012]	125,961 [0.21]	-0.015	[-0.040, 0.010]	123,044 [0.20]
Anemic	-0.0001	[-0.082, 0.017]	144,027 [0.28]	-0.007	[-0.031, 0.015]	141,693 [0.27]
Diarrhea	-0.001	[-0.019, 0.020]	148,033 [0.27]	-0.016	[-0.039, 0.007]	144,652 [0.26]
Fever	-0.017	(0.010)*	148,377 [0.27]	-0.033	(0.013)***	145,056 [0.26]

Notes: All variables are defined as before. "All regressions here include basic and socioeconomic controls and time-varying country characteristics. I also control for region-specific linear trend and region-specific linear kink. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.5: IV estimated effects of years of schooling on socioeconomic behaviors

Outcomes:	(1) Coefficient	(2) (SE) [CI]	(3) Mean	(4) Sample size
Knowledge and ability to process information:				
Literacy	0.123	(0.009)***	0.49	322,592
Comprehensive knowledge about HIV/AIDS	0.010	(0.003)***	0.02	322,592
Knows oral rehydration method	0.021	(0.100)***	0.58	322,592
Knows any modern contraceptive	0.023	(0.009)**	0.23	322,592
Knows when in the ovulation cycle a woman can get pregnant	0.020	(0.009)**	0.14	322,592
Income effects of education via work:				
Working	0.019	[-0.037, 0.040]	0.43	322,592
Employed by others	0.016	[-0.008, 0.032]	0.14	322,592
Income effects of education via marriage:				
Partner’s completed years of schooling	0.274	[-0.005, 0.542]	5.72	251,734
Partner’s age in the survey year	-0.284	[-0.825, 0.256]	34.99	231,815
Socioeconomic status:				
Live in urban	0.015	[-0.006, 0.035]	0.27	322,592
Low wealth	-0.082	(0.012)***	0.38	322,592
High wealth	0.052	(0.010)***	0.45	322,592
Behavior channels - sex, marriage, and fertility:				
Married by age 14	-0.001	[-0.019, 0.016]	0.14	322,592
Married	0.036	(0.009)***	0.77	322,592
Age at first sex	0.049	[-0.132, 0.230]	16.68	231,323
Age at first birth	0.334	(0.088)***	18.99	281,339
Number of children	-0.022	[-0.095, 0.050]	3.30	322,592
Ideal number of children	0.124	[-0.429, 0.677]	5.38	253,242

Behavior channels – mother's health inputs:				
Got tested for HIV/AIDS during pregnancy	-0.0002	[-0.002, 0.001]	0.01	322,592
Institutional delivery	-0.007	[-0.027, 0.012]	0.66	322,592
Use antenatal care	0.009	[-0.005, 0.022]	0.87	322,592
Use postnatal care	0.067	(0.033)**	0.41	105,765
Wash hands before feeding child	-0.002	[-0.009, 0.006]	0.08	322,592
Vaccines:				
Polio	-0.010	[0.028, 0.008]	0.45	322,592
Measles	0.011	[-0.007, 0.028]	0.44	322,592
Tuberculosis	-0.004	[-0.022, 0.014]	0.60	322,592
Diphtheria Pertussis and Tetanus	-0.002	[-0.020, 0.015]	0.46	322,592
Amenity:				
Access to family planning	0.012	[-0.003, 0.026]	0.10	250,256
Access to toilet	0.002	[-0.018, 0.022]	0.78	322,592
Breastfeeding:				
Duration of breastfeeding (in months)	3.038	(0.624)***	34.91	274,166
Timely breastfeeding	0.037	(0.009)***	0.35	322,592
Breastfed child after hours	-0.031	(0.008)***	0.18	322,592
Breastfed child after days	-0.012	(0.006)**	0.07	322,592

Notes: Dependent variables are labeled in each panel and column. "Low wealth" indicates whether the household's wealth index falls in quintile 2 or below, while "High wealth" denotes quintile 4 or above. All regressions here include basic and socioeconomic controls and time-varying country characteristics. I also control for region-specific linear trend and region-specific linear kink. Robust standard errors, clustered at both birth cohort–country and survey-cluster levels, are reported in (.). I report the confidence interval at the 95 percent significance level in [.]. ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.6: Placebo test

	(1)	(2)	(3)	(4)
Panel A: First-stage				
	Years of education			
Intensity*Post-placebo	0.237	0.035	0.054	0.147
	(0.095)*	(0.139)	(0.157)	(0.216)
F-statistics	6	0.1	0.1	0.5
Observations	130,073	130,073	130,073	130,073
Panel B: Reduced Form				
	Neonatal mortality			
Intensity*Post-placebo	-0.001	-0.013	-0.011	0.003
	(0.006)	(0.008)	(0.010)	(0.013)
Observations				
	Infant mortality			
Intensity*Post-placebo	0.001	-0.011	-0.007	0.005
	(0.007)	(0.009)	(0.012)	(0.016)
Observations				
	Under-five mortality			
Intensity*Post-placebo	-0.0002	-0.014	-0.010	0.002
	(0.007)	(0.010)	(0.012)	(0.016)
Observations				
	Stunting			
Intensity*Post-placebo	0.010	0.023	0.012	-0.009
	(0.011)	(0.015)	(0.019)	(0.026)
Observations				
	Underweight			
Intensity*Post-placebo	0.006	0.021	0.014	-0.012
	(0.012)	(0.016)	(0.021)	(0.028)
Observations				
	Wasted			
Intensity*Post-placebo	0.003	0.013	0.0004	-0.014
	(0.011)	(0.015)	(0.019)	(0.026)
Observations				
Baseline controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-varying country characteristics	Yes	Yes	Yes	Yes
Country-specific linear trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: This is a placebo test for cohorts too old to be affected by the FPE. "Basic controls", "Socioeconomic controls," and "Time-variant country characteristics" are defined as before. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.7: Effect of whether exposed to free primary education on maternal education-*RD design*

	(1)	(2)
	Dependent variable=Years of education	
Intensity*Post-FPE	1.594	2.958
	(0.157)***	(0.347)***
F-statistics	103	57
Mean of outcome	4.9	4.9
Observation	144,192	144,192
Basic controls	Yes	Yes
Socioeconomic controls	Yes	Yes
Time-variant country characteristics	Yes	Yes
Region-specific polynomial trend	Linear	Quadratic
Region-specific polynomial kink	Linear	Quadratic

Notes: “Basic controls” include region fixed effects, survey year fixed effects, the child’s year-of-birth dummies, and child’s gender. “Socioeconomic controls” include dummies for the household’s religion and ethnicity. “Time-varying country characteristics” include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.8: IV estimated effects of maternal education on child mortality and the health status of surviving children-*RD design*

	(1)	(2)	(3)	(4)
Dependent variables:	Coefficient	SE [CI]	Sample size	Mean
Neonatal	-0.007	[-0.017, 0.003]	144,192	0.06
Infant	0.001	[-0.012, 0.013]	144,192	0.07
Under-five	-0.005	[-0.007, 0.018]	144,192	0.08
Stunting	-0.017	[-0.043, 0.009]	107,428	0.25
Underweight	0.014	[-0.011, 0.038]	107,430	0.26
Wasting	0.011	[-0.012, 0.032]	107,430	0.20
Anemic	0.003	[-0.021, 0.027]	128,451	0.28
Diarrhea	-0.029	(0.012)**	131,204	0.26
Fever	-0.018	[0.041,0.004]	131,758	0.27

Notes: All outcome variables are defined as before. "All regressions here include basic and socioeconomic controls and time-varying country characteristics. I also control for region-specific linear trend and region-specific linear kink. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). I report the confidence interval at the 95 significance level in [.]. ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.9: IV estimated effects of maternal education on child mortality and the health status of surviving children-*Only firstborns*

	(1)	(2)	(3)	(4)
	Coefficient	SE [CI]	Sample size	Mean
Dependent variables:				
Neonatal	-0.011	[-0.026, 0.004]	100,319	0.06
Infant	-0.018	(0.009)*	100,319	0.08
Under-five	-0.003	[-0.021, 0.014]	100,319	0.09
Stunting	-0.056	(0.018)***	71,432	0.27
Underweight	-0.026	(0.016)***	71,437	0.26
Wasting	-0.001	[-0.028, 0.029]	71,437	0.21
Anemic	-0.012	[-0.029, 0.037]	89,366	0.28
Diarrhea	-0.008	[-0.035, 0.019]	90,981	0.29
Fever	-0.029	(0.014)**	91,235	0.28

Notes: All outcome variables are defined as before. "All regressions here include basic and socioeconomic controls and time-varying country characteristics. I also control for region-specific linear trend and region-specific linear kink. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.), and I report the confidence interval at the 95 significance level in [.]. $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 4.10: Robustness checks

	(1)	(2)	(3)
	Years of education		
	Coefficient	SE	F-stat.
Baseline result	0.929	(0.082)***	126
Dropping time-varying country characteristics	0.929	(0.083)***	126
Replacing physician density with hospital beds	1.080	(0.084)***	165
Alternative operationalization of intensity	0.811	(0.058)***	196
Never moved	0.764	(0.149)***	26
Dropping Ethiopia, Ghana, Tanzania, and Zimbabwe	0.962	(0.100)***	93
Dropping Egypt, Nigeria, Sierra Leone, and Zimbabwe	1.043	(0.100)***	109
Nigeria excluded from baseline	0.983	(0.091)***	117
Zimbabwe excluded from baseline	0.924	(0.085)***	118
Egypt excluded from baseline	1.045	(0.088)***	144
Mali excluded from baseline	1.139	(0.098)***	134
Malawi excluded from baseline	0.863	(0.085)***	103
Ethiopia excluded from baseline	1.069	(0.094)***	130
Ghana excluded from baseline	0.917	(0.084)***	119
Uganda excluded from baseline	0.861	(0.087)***	99
Cameroon excluded from baseline	0.915	(0.083)***	122
Liberia excluded from baseline	0.913	(0.083)***	120
Sierra Leone excluded from baseline	0.879	(0.089)***	98
Tanzania excluded from baseline	0.845	(0.083)***	103
Zambia excluded from baseline	0.911	(0.083)***	120
Kenya excluded from baseline	0.926	(0.083)***	125
Rwanda excluded from baseline	0.927	(0.083)***	125
Burundi excluded from baseline	0.922	(0.084)***	120
Mozambique excluded from baseline	0.915	(0.083)***	121
Benin excluded from baseline	0.913	(0.085)***	117
Burkina Faso excluded from baseline	0.928	(0.083)***	126
Togo excluded from the baseline	0.927	(0.083)***	126

Notes: All regressions here include basic and socioeconomic controls and time-varying country characteristics define as before. I also control for region-specific linear trend and region-specific linear kink. Robust standard errors, clustered at both birth cohort–country and survey-cluster levels, are reported in (.). I report the confidence interval at the 95 percent significance level in [.]. ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5.1: Effect of whether exposed to free primary education on maternal education

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable= Years of education [Mean=4.3]					
Intensity*Post-FPE	0.827	0.782	0.782	0.987	1.014	0.921
	(0.115)***	(0.110)***	(0.110)***	(0.116)***	(0.129)***	(0.158)***
F-statistics	52	50	50	73	62	34
Adjusted R-squared	0.32	0.36	0.36	0.37	0.37	0.37
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls		Yes	Yes	Yes	Yes	Yes
Time-varying country characteristics			Yes	Yes	Yes	Yes
Country-specific linear trend				Yes		
Region-specific linear trend					Yes	Yes
Region-specific linear kink						Yes

Notes: "Basic controls" include region fixed effects, a full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include a full set of dummies for the household's religion and ethnicity and a dummy for urban. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.
Source: DHS dataset, listed in Table A3.2.

Table 5.2: IV estimated effects of maternal education on FGC

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
	Dependent variable: Daughter is cut						
Years of education	-0.001	-0.068	-0.071	-0.071	-0.071	-0.096	-0.095
	(0.0002)***	(0.011)***	(0.012)***	(0.012)***	(0.010)***	(0.014)***	(0.019)***
Mean of the outcome	0.11	0.11	0.11	0.11	0.11	0.11	0.11
Observations	195,486	195,486	195,486	195,486	195,486	195,486	195,486
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes		Yes	Yes	Yes	Yes	Yes
Time-variant country characteristics	Yes			Yes	Yes	Yes	Yes
Country-specific linear cohort trend					Yes		
Region-specific linear trend	Yes					Yes	Yes
Region-specific linear kink	Yes						Yes

Notes: “Basic controls” include region fixed effects, a full set of dummies for maternal birth cohorts, and survey year. “Socioeconomic controls” include a full set of dummies for the household’s religion and ethnicity and a dummy for urban. “Time-varying country characteristics” include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.
Source: DHS dataset, listed in Table A3.2.

Table 5.3: IV estimated effects of maternal years of schooling on socioeconomic behavior

	(1)	(2)	(3)	(4)
Dependent variables:	Coefficient	SE [CI]	Mean	Sample size
Attitude towards FGC:				
FGC should continue	-0.116	(0.027)***	0.52	155,264
Intends to cut daughter in future	-0.054	(0.016)***	0.57	27,605
Marriage:				
Husband-wife education gap	-1.049	(0.201)***		172,031
Husband-wife age gap	-0.255	[-0.839, 0.329]		168,303
Empowerment:				
Own health decision making	0.045	(0.016)***	0.53	165,540
Daily purchase decision making	0.019	[-0.004, 0.042]	0.58	61,515
Family visit decision making	0.032	(0.018)*	0.56	165,535
Large purchase decision making	0.035	(0.017)**	0.42	165,535
Decision about husband money	0.039	[-0.021, 0.100]	0.42	126,919

Notes: All regressions here include the same basic and socioeconomic controls and the same time-variant country characteristics as before. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). I report the confidence interval at the 95 percent significance level in [.]. ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.
Source: DHS dataset, listed in Table A3.2.

Table 5.4: Placebo test

	(1)	(2)	(3)	(4)
	Years of education		Daughter is cut	
Intensity* Post-placebo	0.008	0.023	-0.008	-0.009
	(0.106)	(0.160)	(0.007)	(0.011)
Observations	67,936	67,936	66,752	66,752
Basic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-variant country characteristics	Yes	Yes	Yes	Yes
Region-specific linear trend	Yes	Yes	Yes	Yes
Region-specific linear kink		Yes		Yes

Notes: This is a placebo test for cohorts too old to be affected by the FPE. All regressions here include the same basic and socioeconomic controls and the same time-variant country characteristics as before. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.).

Table 5.5: First-stage and IV estimated effects of maternal years of schooling on FGC-*RD design*

	(1)	(2)
Panel A: First-stage result		
	Dependent variable=Years of education	
Intensity*Post-FPE	0.841	1.124
	(0.250)***	(0.473)*
F-statistics	11	6
Observation	143,281	143,281
Mean of outcome	4.4	4.4
Panel B: 2SLS result		
	Dependent variable= Daughter is cut	
Years of education	-0.080	-0.266
	(0.028)***	(0.249)
Observation	143,281	143,281
Mean of outcome	0.12	0.12
Basic controls	Yes	Yes
Socioeconomic controls	Yes	Yes
Time-variant country characteristics	Yes	Yes
Region-specific polynomial trend	Linear	Quadratic
Region-specific polynomial kink	Linear	Quadratic

Notes: "Basic controls" include region fixed effects, a full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include a full set of dummies for the household's religion and ethnicity and a dummy for urban. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5.6: Effect of whether exposed to free primary education on maternal education–Alternative operationalization of intensity

	(1)	(2)	(3)	(4)
Panel A: First-stage result				
	Dependent variable=Years of education			
Intensity*Post-FPE	0.603	0.783	0.753	0.619
	(0.102)***	(0.103)***	(0.109)***	(0.130)***
F-statistics	35	58	47	23
Mean of outcome	4.3	4.3	4.3	4.3
Observations	195,486	195,486	195,486	195,486
Panel B: 2SLS result				
	Dependent variable=Daughter is cut			
Years of education	-0.110	-0.100	-0.129	-0.137
	(0.020)***	(0.015)***	(0.020)***	(0.031)***
Mean of outcome	0.11	0.11	0.11	0.11
Observations	195,486	195,486	195,486	195,486
Baseline controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-varying country characteristic	Yes	Yes	Yes	Yes
Country-specific linear cohort trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: "Baseline covariates" include region fixed effects, the full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5.7: First-stage and IV estimated effects of maternal years of schooling on FGC–*non-movers*

	(1)	(2)	(3)	(4)
Panel A: First-stage result				
	Dependent variable=Years of education			
Intensity*Post-FPE	1.043	0.909	0.785	1.026
	(0.158)***	(0.165)***	(0.182)***	(0.221)***
F-statistics	44	30	19	22
Mean of outcome	4.5	4.5	4.5	4.5
Observations	96,330	96,330	96,330	96,330
Panel B: 2SLS result				
	Dependent variable=Daughter is cut			
Years of education	-0.088	-0.100	-0.150	-0.097
	(0.015)***	(0.021)***	(0.037)***	(0.024)***
Mean of outcome	0.11	0.11	0.11	0.11
Observations	96,330	96,330	96,330	96,330
Baseline controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-varying country characteristics	Yes	Yes	Yes	Yes
Country-specific linear cohort trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: "non-movers" refers to cohorts who have never moved from their current place of residence. "Baseline covariates" include region fixed effects, the full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5.8: IV estimated effect of maternal education on FGC–*using subsample*

	(1)	(2)	(3)	(4)
Panel A: First-stage result				
	Dependent variable=Years of education			
Intensity*Post-FPE	2.021	2.425	2.762	2.304
	(0.319)***	(0.325)***	(0.354)***	(0.453)***
F-statistics	40	56	61	26
Mean of outcome	5.5	5.5	5.5	5.5
Observations	27,605	27,605	27,605	27,605
Panel B: 2SLS result				
	Dependent variable= Daughter is cut			
Years of education	-0.052	-0.047	-0.058	-0.058
	(0.009)***	(0.007)***	(0.008)***	(0.013)***
Mean of outcome	0.08	0.08	0.08	0.08
Observations	27,605	27,605	27,605	27,605
Basic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-variant country characteristics	Yes	Yes	Yes	Yes
Country-specific linear trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: "Subsample" refers to the sample size of respondents who intend to cut daughter in the future. "Baseline covariates" include region fixed effects, the full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

Table 5.9: IV estimated effect of maternal education on FGC–*using daughters older than five years*

	(1)	(2)	(3)	(4)
Panel A: First-stage result				
	Dependent variable=Years of education			
Intensity*Post-FPE	0.529	0.827	0.983	0.933
	(0.129)***	(0.136)***	(0.150)***	(0.191)***
F-statistics	16	37	43	24
Mean of outcome	4	4	4	4
Observations	137,363	137,363	137,363	137,363
Panel B: 2SLS result				
	Dependent variable= Daughter is cut			
Years of education	-0.056	-0.046	-0.075	-0.064
	(0.018)***	(0.011)***	(0.013)***	(0.017)***
Mean of outcome	0.10	0.10	0.10	0.10
Observations	137,363	137,363	137,363	137,363
Basic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-variant country characteristics	Yes	Yes	Yes	Yes
Country-specific linear trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: "Baseline covariates" include region fixed effects, the full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). ∗∗∗, ∗∗, and ∗ indicate significance at the 1, 5, and 10 percent levels, respectively.

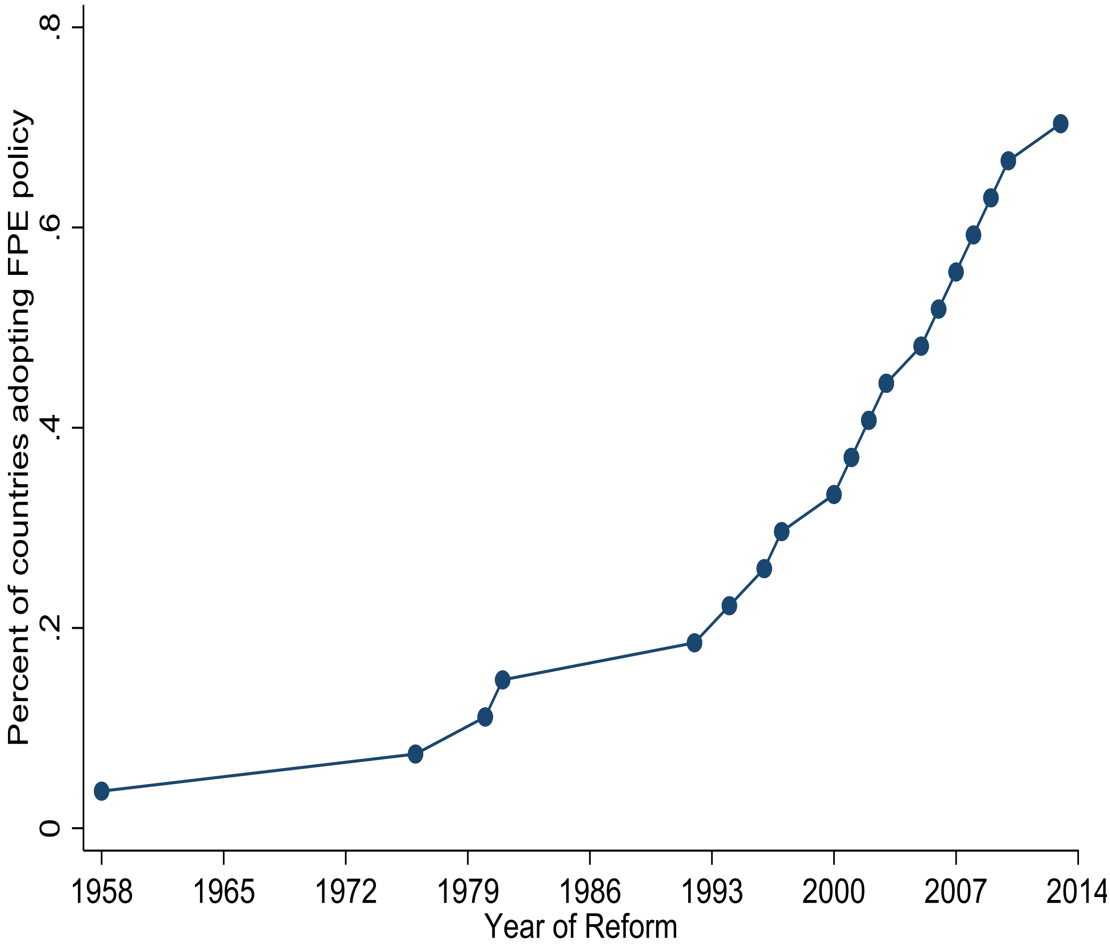
Table 5.10: IV estimated effect of maternal education on FGC–*using eldest daughters*

	(1)	(2)	(3)	(4)
Panel A: First-stage result				
	Dependent variable=Years of education			
Intensity*Post-FPE	1.104	1.162	1.029	0.954
	(0.095)***	(0.101)***	(0.115)***	(0.139)***
F-statistics	135	132	81	47
Mean of outcome	4.8	4.8	4.8	4.8
Observations	115,356	115,356	115,356	115,356
Panel B: 2SLS result				
	Dependent variable= Daughter is cut			
Years of education	-0.021	-0.027	-0.055	-0.047
	(0.005)***	(0.005)***	(0.008)***	(0.010)***
Mean of outcome	0.08	0.08	0.08	0.08
Observations	115,356	115,356	115,356	115,356
Basic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Time-variant country characteristics	Yes	Yes	Yes	Yes
Country-specific linear trend		Yes		
Region-specific linear trend			Yes	Yes
Region-specific linear kink				Yes

Notes: "Baseline covariates" include region fixed effects, the full set of dummies for maternal birth cohorts, and survey year. "Socioeconomic controls" include dummies for the household's religion and ethnicity. "Time-varying country characteristics" include the pupil-teacher ratio and the number of physicians per 1,000 people. I cluster robust standard errors at both birth cohort–country and survey-cluster levels and are reported in (.). $***$, $**$, and $*$ indicate significance at the 1, 5, and 10 percent levels, respectively.

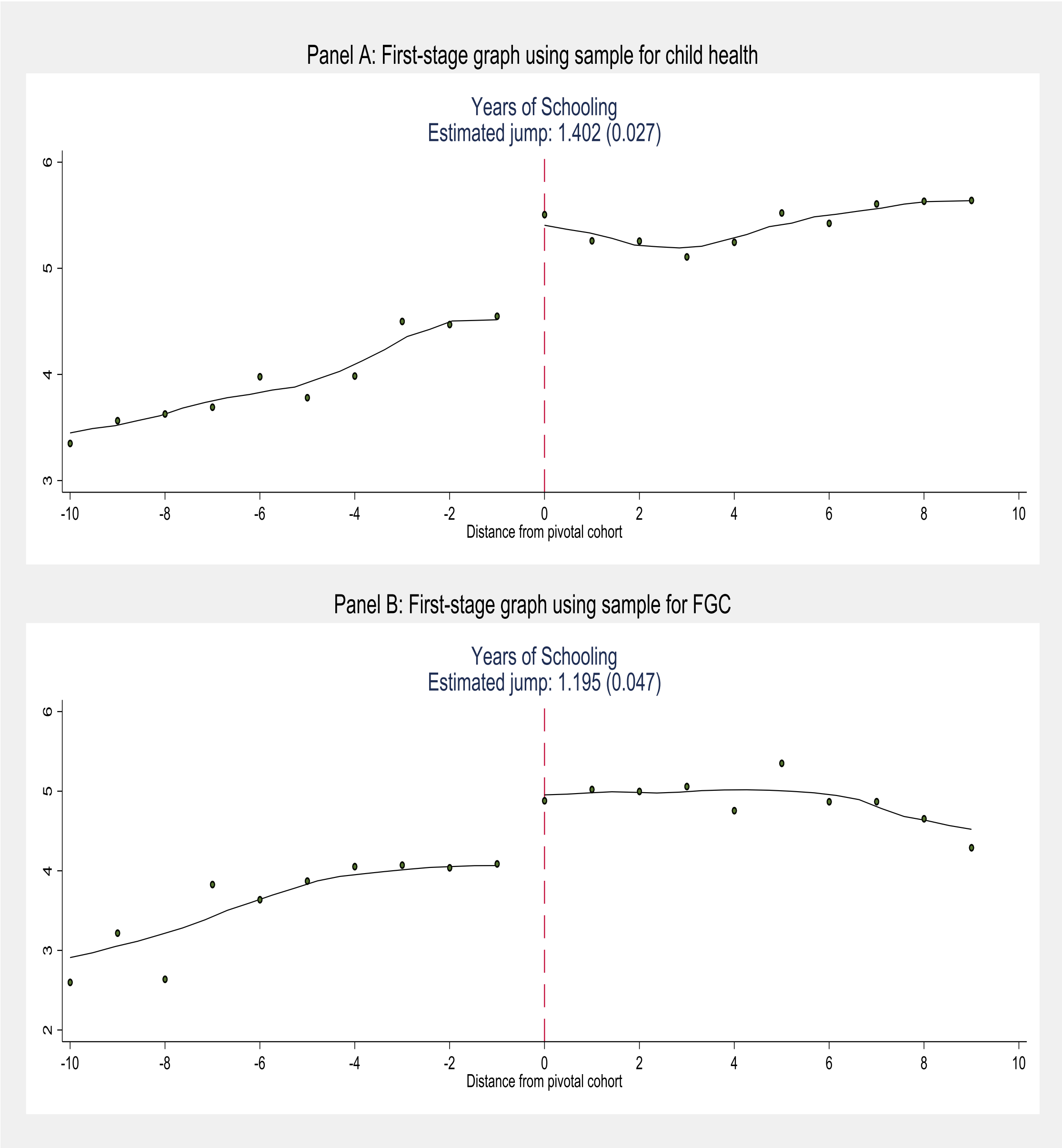
List of Figures

Figure 3.1: Percent of African countries adopting free primary education policies, 1958-2013



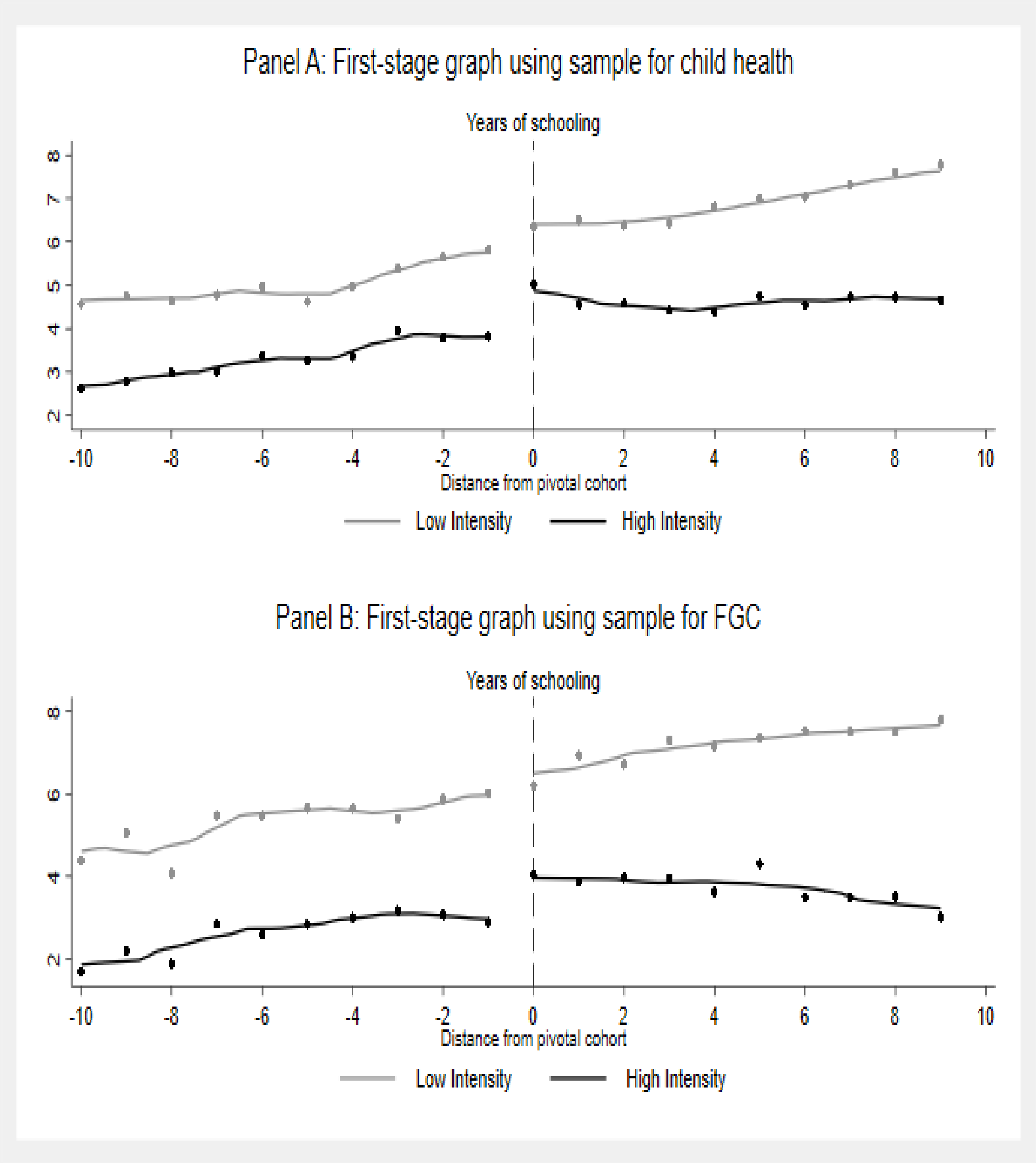
Source: Authors' calculation

Figure 3.2: Impact of whether exposed to FPE on female education



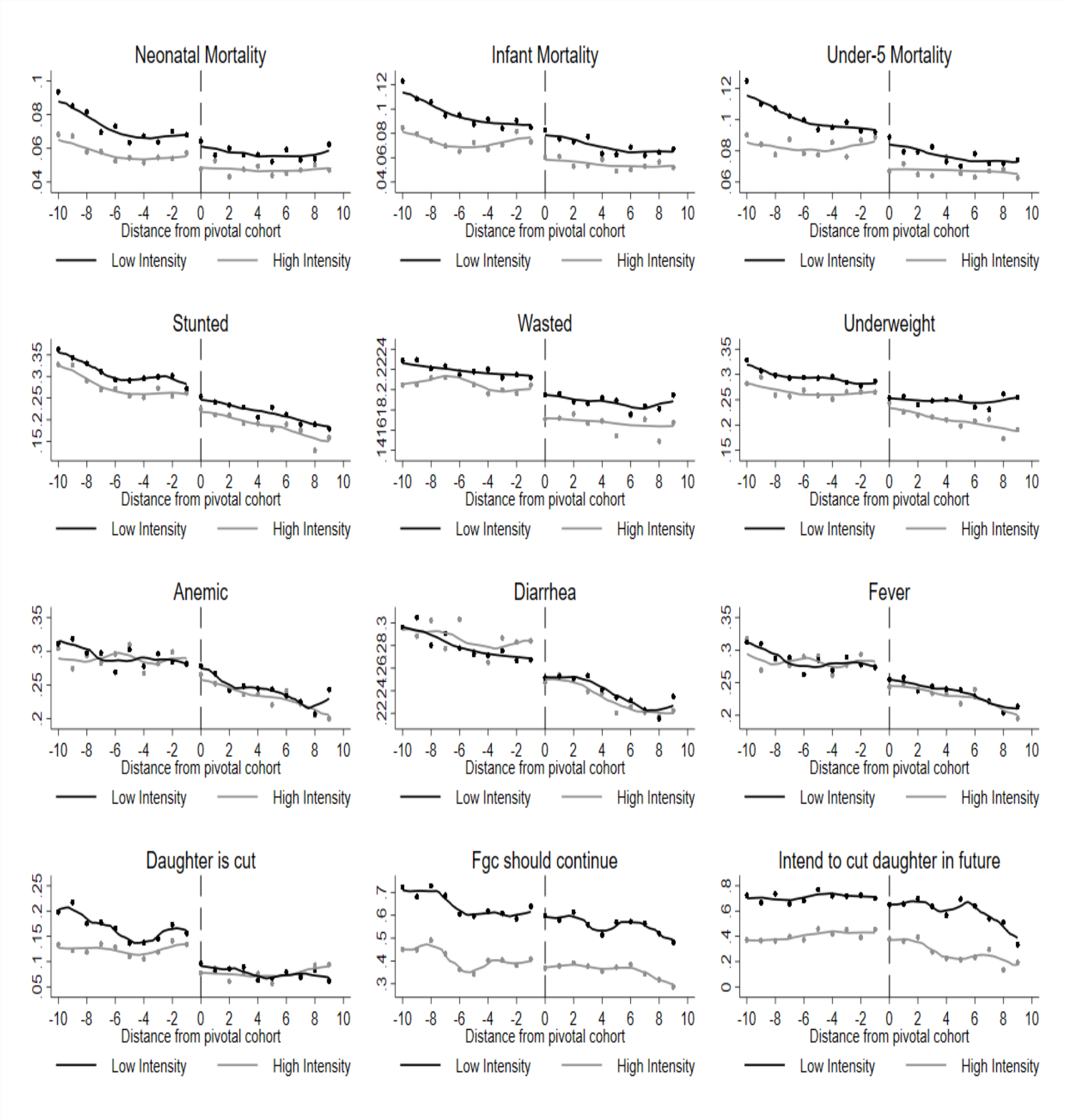
Notes: "Pivotal cohort" refers to the oldest cohort exposed to the reform, listed in Tables 3.1 and 3.2. All the younger cohorts are fully exposed to the reform. Each dot represents the average level of education of a given cohort.
Source: DHS dataset, listed in Tables A3.1 and A3.2

Figure 3.3: Trends in female education



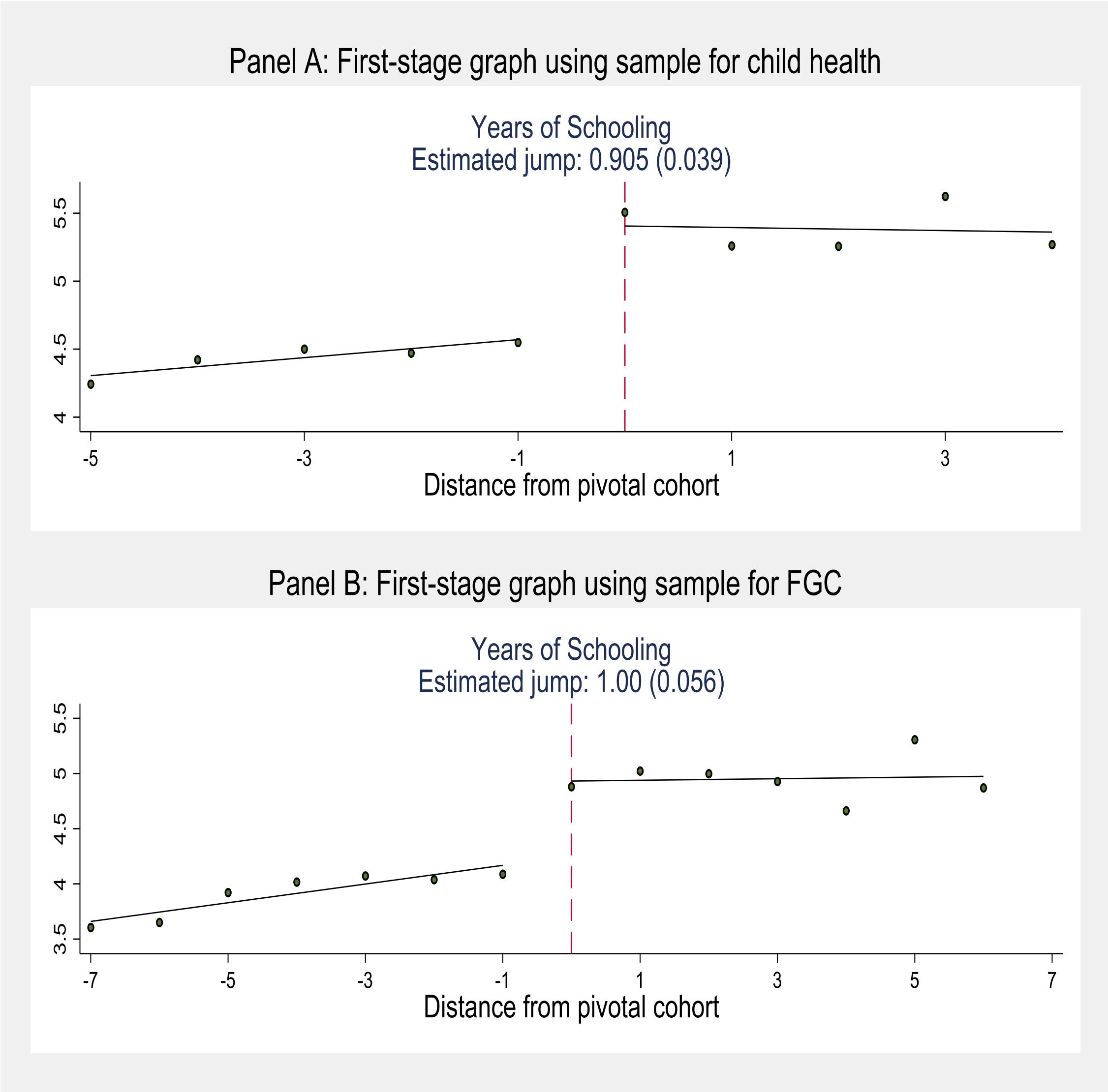
Note: "Pivotal cohort" refers to the oldest cohort exposed to the reform, listed in Tables 3.1 and 3.2. All the younger cohorts are fully exposed to the reform. Each dot represents the average level of education of a given cohort in low- and high-intensity regions. I measure intensity as the female proportion of the regional population who had not entered the primary school before the reform. I define a region as `High intensity' if the region's intensity is above the country's median intensity. The polynomial curve shows the trend in schooling years across intensity regions based on the bandwidth of 10.
Source: DHS dataset, listed in Table A3.1 and A3.2.

Figure 3.4: Trends in child mortality, stunting, wasting, underweight, and FGC prevalence



Note: "Pivotal cohort" refers to the oldest cohort exposed to the reform, listed in Tables 3.1 and 3.2. All the younger cohorts are fully exposed to the reform. Each dot represents the average level of education of a given cohort in low- and high-intensity regions. I measure intensity as the female proportion of the regional population who had not entered the primary school before the reform. I define a region as `High intensity' if the region's intensity is above the country's median intensity. The polynomial curve shows the trend in schooling years across intensity regions based on the bandwidth of 10.
Source: DHS dataset, listed in Table A3.1 and A3.2.

Figure 3.5: Impact of whether exposed to FPE on years of schooling-*RD design*



Notes: Each dot represents the average level of education of a given cohort.
Source: DHS dataset, listed in Table A3.1 and A3.2.

Appendix A

Table A3.1: Summary of DHS data per country for child health analysis

Country	Survey years	Number of surveys	Sample size	Number of families
Benin	2006, 2011/12, 2017/18	3	5,179	3,444
Burkina Faso	2010, MIS2014, MIS2017/18	3	866	699
Burundi	2010/11, MIS2012/13, 2016/17	3	5,210	3,345
Cameroon	2004, 2011	2	2,176	1,542
Egypt	1988/89,1992/93,1995/96, 2000, 2005, 2008, 2014	7	50,992	10,563
Ethiopia	1997, 2003, 2008	3	23,781	7,520
Ghana	1998, 2003, 2008, 2014, MIS2016	5	9,533	3,758
Kenya	2003, 2008/09, 2014, MIS2015	4	8,284	5,570
Liberia	2000/07, MIS2000/09, MIS2011, 2013, MIS2016	5	6,691	3,010
Malawi	2000, 2004/05, 2010, MIS2012, MIS2014, 2015/16, MIS2017	7	48,903	13,478
Mali	1995/96, 2001, 2006, 2012/13, MIS2015, 2018	6	39,131	8,538
Mozambique	2011, MIS2018	2	7,645	4,518
Nigeria	1990, 2003, 2008, MIS2010, 2013, MIS2015	6	26,614	10,879
Rwanda	2005, 2010/11, MIS2013, 2014/15, MIS2017	5	9,271	4,523
Sierra Leone	2008, 2013, MIS2016	3	12,579	6,338
Tanzania	2004/05, 2009/10, 2015/16, MIS2017	4	14,324	5,908
Togo	2013/14, MIS2017	2	646	533
Uganda	2000/01, 2006, MIS2009/10, 2011, MIS2014/15, 2016	6	32,403	9,421
Zambia	2001/02, 2007, 2013/14	3	4,339	3,110
Zimbabwe	1988/89, 1994, 1999, 2005/06, 2010/11, 2015	6	14,025	4,232
Total		85	322,592	110,929

Table A3.2: Summary of DHS data per country for female genital cutting analysis

Country	Survey years	Data file names	Number of surveys	Sample size	Number of families
Egypt	1995/96, 2005, 2008, 2014	EGIR51FL, EGIR33FL, EGIR5AFL, EGIR61FL	4	60,751	33,833
Ethiopia	2004/05, 2015/16	ETIR51FL, ETIR71FL	2	21,377	13,584
Kenya	2003, 2008/09, 2014	KEIR42FL, KEIR52FL, KEIR72FL	3	4,813	3,992
Mali	1995/96,2001, 2006,2012/13, 2018	MLIR32FL, MLIR41FL, MLIR53FL, MLIR6AFL, MLIR7AFL,	5	37,198	23,599
Nigeria	2003, 2008, 2013, 2018	NGIR4BFL, NGIR6AFL, NGIR53FL, NGIR7AFL	4	55,188	24,051
Sierra Leone	2008, 2013	SLIR51FL, SLIR61FL	2	8,053	8,015
Tanzania	2004/05, 2009/10, 2015/16	TZIR4IFL, TZIR63FL, TZIR7BFL	3	8,106	6,884
Total			23	195,486	113,958

Table A3.3: Pre-FPE enrollment rates and Pre-FPE enrollment growth per country

	Pre-FPE enrollment rates	The proportion of the overall sample (%)	Sample size per region	DHS data file	Data year	Potentially enrollment cohorts
Regions	(1)	(2)	(3)	(4)	(5)	(6)
Benin:				2001	BJPR41	1988-1991
Atacora	0.60	0.30	968			
Atlantique	0.29	0.22	697			
Borgou	0.58	0.47	1,487			
Mono	0.46	0.17	543			
Oueme	0.42	0.18	576			
Zou	0.49	0.28	908			
Burkina Faso:				1998	BFPR31	1993-1996
Central south	0.03	0.05	146			
East	0.04	0.05	154			
North	0.06	0.04	140			
Ouagadougou	0.08	0.07	219			
West	0.09	0.06	207			
Burundi:				1987	BUIR02	1971-1974†
Bujumbura	0.81	0.15	474			
Centre-east	0.65	0.37	1,168			
North	0.80	0.56	1,793			
South	0.56	0.23	726			
West	0.30	0.33	1,049			
Cameroon:				1998	CMPR31	1985-1988
Central and south-east	0.05	0.20	648			
North, extreme north and adamaoua	0.56	0.31	989			
Northwest and southwest	0.07	0.09	284			
Littoral	0.02	0.08	255			
Egypt:				1988	EGIR01	1961-1964
Lower-Egypt-rural	0.57	3.97	12,675			
Lower-Egypt-urban	0.24	1.35	4,318			
Upper-Egypt-rural	0.68	5.61	17,909			
Upper-Egypt-urban	0.38	1.81	5,789			
Urban-governorates	0.17	3.23	10,301			
Ethiopia:				1992	ETPR41	1972-1975

Addis Ababa	0.16	0.31	997			
Affar	0.82	0.65	2,081			
Amhara	0.81	0.80	2,591			
Bengumz	0.79	0.62	2,014			
Dire dawa	0.45	0.41	1,315			
Gambela	0.68	0.51	1,630			
Harari	0.34	0.44	1,425			
Oromiya	0.74	1.19	3,836			
Snnp	0.69	1.00	3,217			
Somali	0.91	0.76	2,442			
Tigray	0.81	0.69	2,233			
Ghana:				1993	GHPR31	1975-1978
Ashanti	0.16	0.35	1,113			
Bong Ahafo	0.15	0.32	1,011			
Central	0.17	0.28	884			
Eastern	0.10	0.26	845			
Greater Accra	0.12	0.25	786			
Northern	0.61	0.45	1,447			
Upper east	0.52	0.27	866			
Upper west	0.48	0.25	802			
Volta	0.14	0.27	869			
Western	0.13	0.28	910			
Kenya:				1998	KEPR3A	1985-1988
Central	0.04	0.15	465			
Coast	0.22	0.38	1,209			
Eastern	0.08	0.38	1,205			
Nairobi	0.37	0.07	218			
Nyanza	0.05	0.44	1,418			
Rift valley	0.14	0.92	2,945			
Western	0.08	0.26	824			
Liberia:				1986	LBIR01	1971-974††
Monrovia	0.18	0.46	1,482			
South eastern	0.29	1.63	5,209			
Malawi:				1992	MWPR22	1971-1974
Central	0.37	5.35	17,075			
North	0.11	2.79	8,908			
South	0.36	7.18	22,920			
Mali:				1987	MLIR01	1970-1973
Bamako	0.36	1.50	4,788			
Kayeskoulikoro	0.68	3.81	12,176			
Moptigaotombouсto	0.68	2.82	8,994			
Sikassosegou	0.71	4.13	13,173			
Mozambique:				2003	MZPR41	1982-1985
Cabo Delgado	0.51	0.19	619			
Gaza	0.29	0.21	666			
Inhambane	0.31	0.18	570			
Manica	0.34	0.28	884			

Maputo city	0.26	0.17	537			
Maputo province	0.22	0.19	603			
Nampula	0.54	0.21	679			
Niassa	0.43	0.23	720			
Sofala	0.57	0.27	869			
Tete	0.40	0.23	730			
Zambzia	0.52	0.24	768			
Nigeria:				1990	NGPR21	1955-1958
North-east	0.95	2.38	7,588			
North-west	0.90	3.00	9,582			
South-east	0.58	1.37	4,385			
South-west	0.39	1.58	5,059			
Rwanda				2000	RWPR41	1981-1984
East	0.14	0.73	2,328			
Kigali city	0.09	0.42	1,348			
Kigali ngali	0.13	0.01	16			
North	0.22	0.43	1,374			
South	0.14	0.62	1,987			
West	0.22	0.69	2,218			
Sierra Leone:				2008	SLPR51	1980-1983
Eastern	0.77	0.83	2,666			
Northern	0.82	1.44	4,587			
Southern	0.76	1.08	3,434			
Western	0.43	0.59	1,892			
Togo:				1998	TGPR31	1991-1994
Agglomeration de lome	0.65	0.02	70			
Centrale	0.01	0.04	112			
Kara	0.02	0.03	107			
Maritime	0.11	0.03	101			
Plateaux	0.01	0.04	118			
Savanes	0.002	0.04	138			
Tanzania:				1999	TZPR41	1981-1984
Arusha	0.13	0.23	750			
Coast	0.22	0.12	397			
Dar-es-salam	0.23	0.14	432			
Dodoma	0.27	0.11	336			
Iringa	0.14	0.16	511			
Kagera	0.22	0.17	544			
Kigoma	.25	0.18	573			
Kilimanjaro	0.69	0.07	211			
Lindi	0.33	0.12	381			
Mara	0.06	0.27	850			
Mbeya	0.05	0.13	417			
Morogoro	0.07	0.14	454			
Mtwara	0.27	0.11	338			
Mwanza	0.27	0.44	1,407			

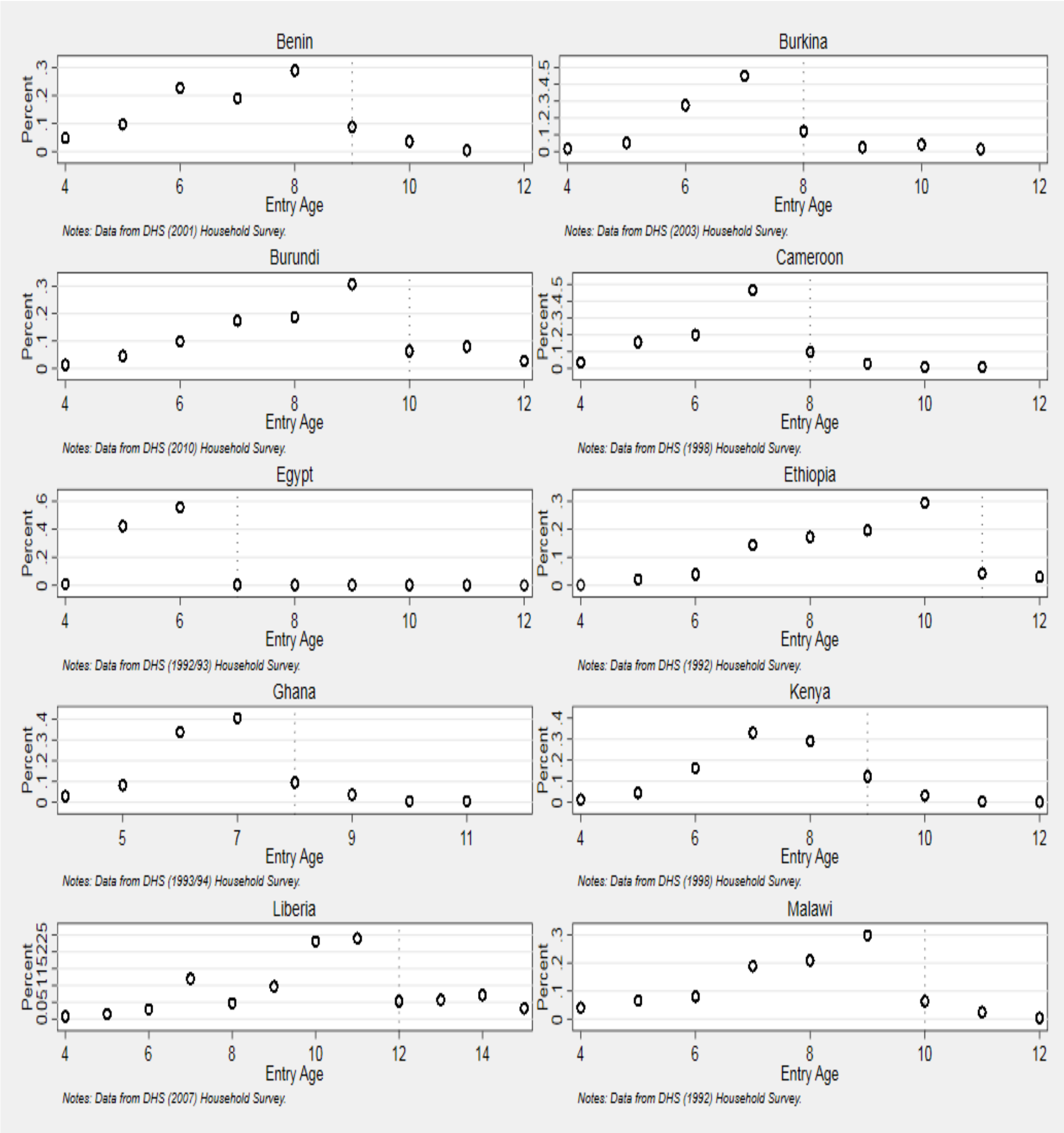
Pemba	0.16	0.22	706			
Zanziba	0.08	0.29	913			
Rukwa	0.44	0.38	1,218			
Ruvuma	0.33	0.14	443			
Shinyanga	0.38	0.53	1,682			
Singida	0.24	0.15	484			
Tabora	0.40	0.29	941			
Tanga	0.09	0.11	336			
Uganda:				1995	UGPR33	1974-1977
Central	0.11	3.00	9,570			
Eastern	0.20	1.96	6,263			
Northern	0.35	2.87	9,161			
Western	0.24	2.32	7,409			
Zambia:				1996	ZMPR31	1985-1988
Central	0.26	0.12	392			
Copperbelt	0.21	0.12	379			
Eastern	0.66	0.18	567			
Luapula	0.50	0.13	419			
Lusaka	0.28	0.13	402			
Northern	0.45	0.14	443			
North-western	0.49	0.27	860			
Southern	0.28	0.15	487			
Western	0.40	0.12	390			
Zimbabwe				1988	ZWIR01	1959-1962
Bulawayo	0.28	0.33	1,044			
Harare	0.28	0.45	1,421			
Manicaland	0.28	0.50	1,612			
Mashonaland-central	0.40	0.46	1,453			
Mashonaland-east	0.18	0.40	1,264			
Mashonaland-west	0.17	0.45	1,438			
Masvingo	0.29	0.48	1,519			
Matabeleland-north	0.46	0.40	1,291			
Matabeleland-south	0.06	0.38	1,218			
Midlands	0.15	0.55	1,765			

Note: The Authors' calculation using the DHS wave before and closest to the reform per countryexcept for Egypt, Nigeria, Sierra Leone, and Zimbabwe (see Section 3.2.4 for discussion). The regions in the pre-reform data are mostly fewer than the regions in the recent surveys. The regions in the recent data were formally part of the regions in the pre-reform data. As a result, to be able to match the regions, I recoded the regions in the recent data to match the pre-reform redgions before merging. However, I dropped regions that could not be matched. I construct the intensity as the fraction of the female youth who had never enrolled in primary school prior to the reform. As a result, for each region *r* in country *j*, I define a population of female youths (aged 15-18). This range of cohorts is an

approximation of the population of female youths. Depending on the data availability, this range of cohorts varies from county to country, as listed in the table.
† the ideal cohorts are 1986-1989, but the data does not allow for these cohorts.
†† the ideal cohorts are 1976-1979, but the data does not allow for these cohorts.

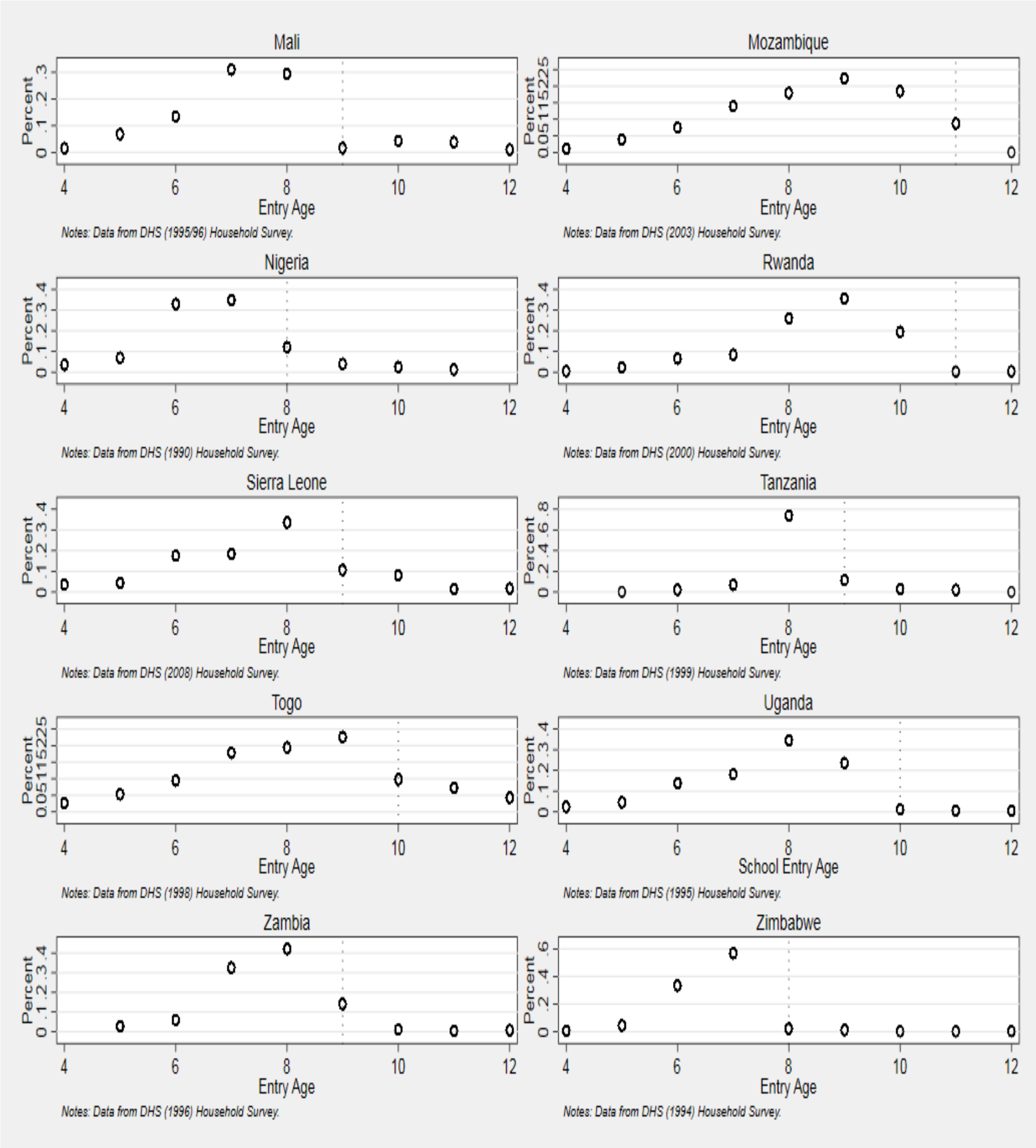
Appendix B

Figure B3.1(1): Distribution of school entry age



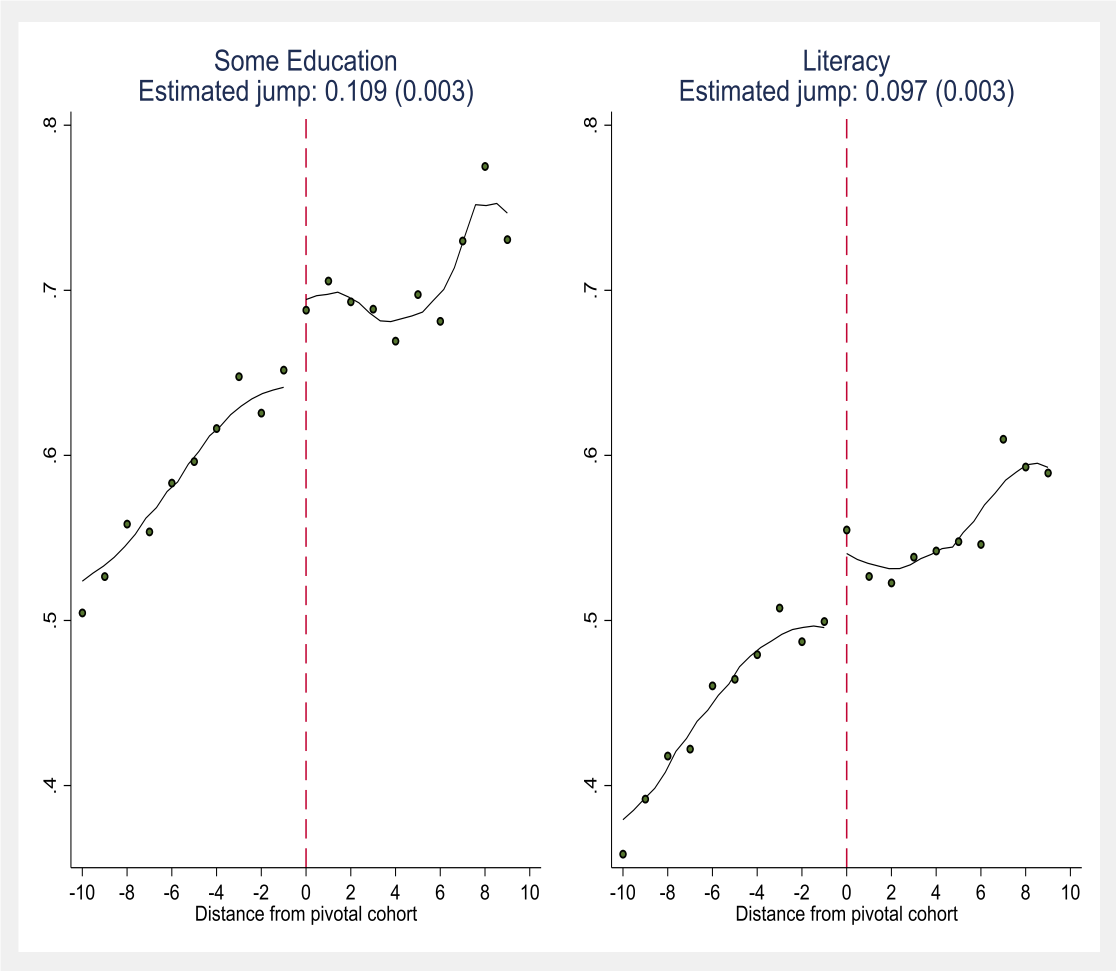
Source: Authors' own calculation

Figure B3.1(2): Distribution of school entry age



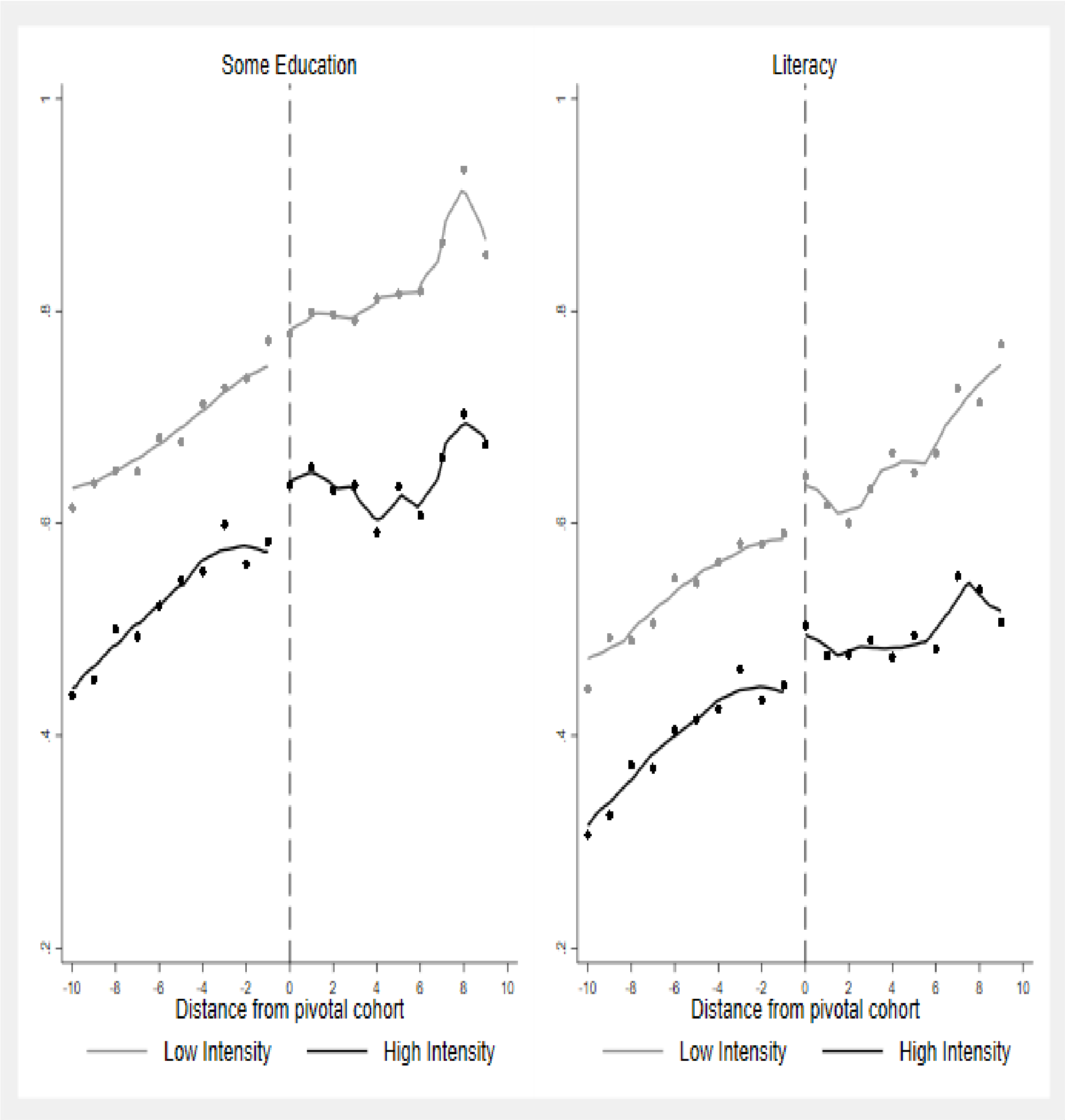
Source: Authors' own calculation

Figure B3.2: Impact of whether exposed to FPE on some education and literacy



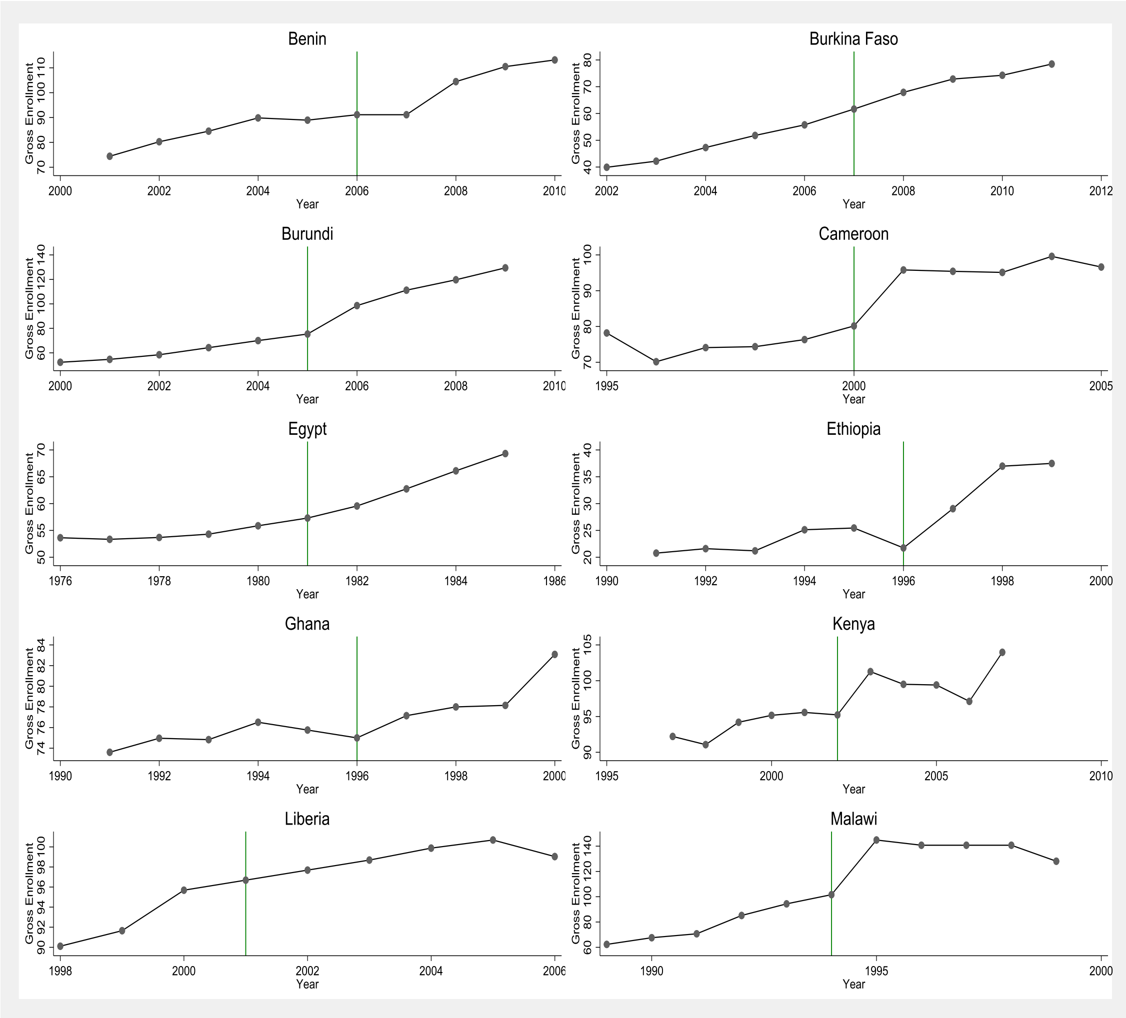
Notes: Each dot represents the average level of education of a given cohort.

Figure B3.3: Trends in the probability of having some education and women's literacy



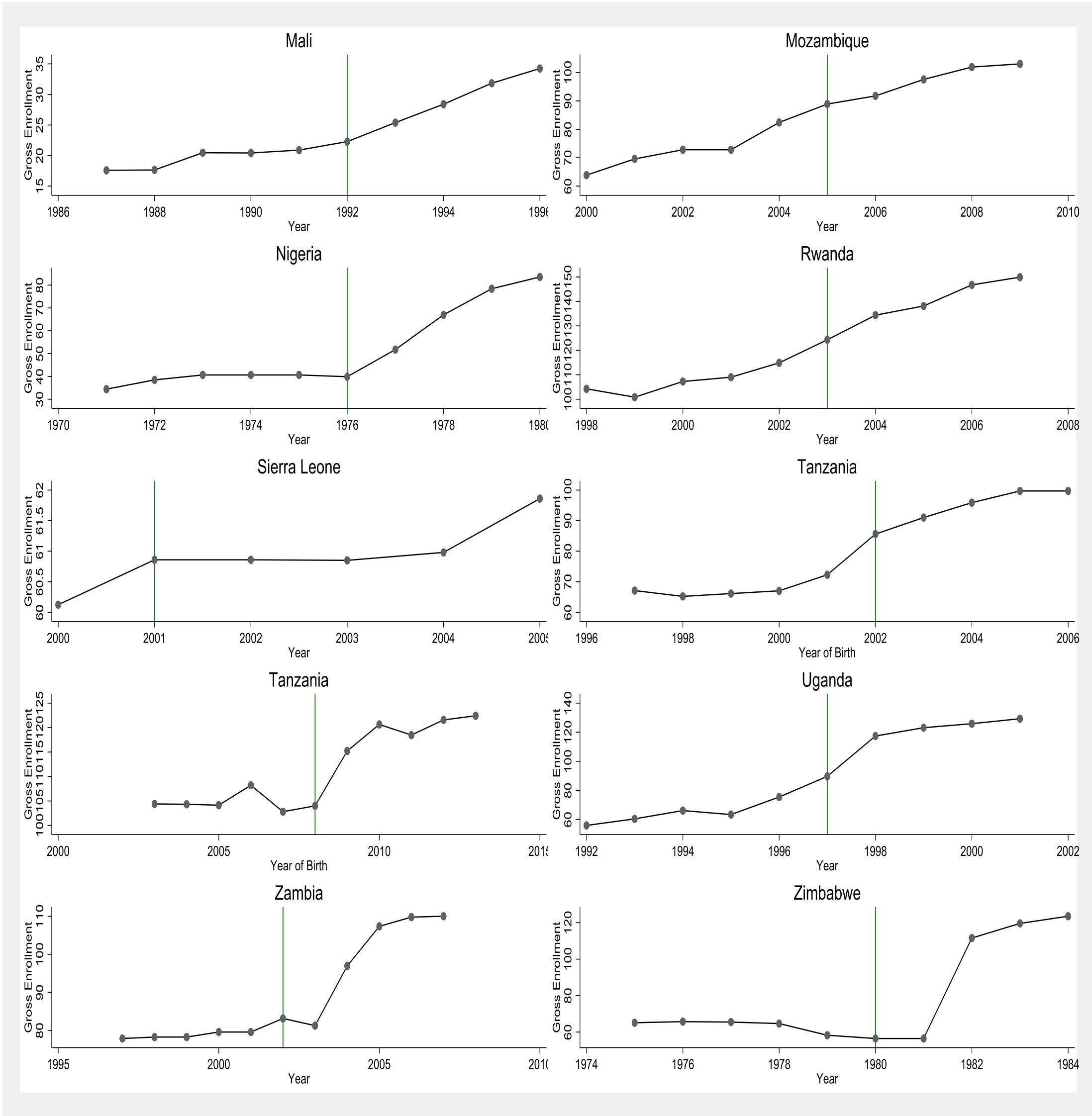
Note: "Pivotal cohort" refers to the oldest cohort exposed to the reform, listed in Tables 3.1 and 3.2. All the younger cohorts are fully exposed to the reform. Each dot represents the average level of probability of having some education and literacy of a given cohort in low- and high-intensity regions. I measure intensity as the female proportion of the regional population who had not entered the primary school before the reform. I define a region as "High intensity" if the region's intensity is above the country's median intensity. The polynomial curve shows the trend in schooling years across intensity regions based on the bandwidth of 10.

Figure B3.4(1): Gross enrollment rate per country



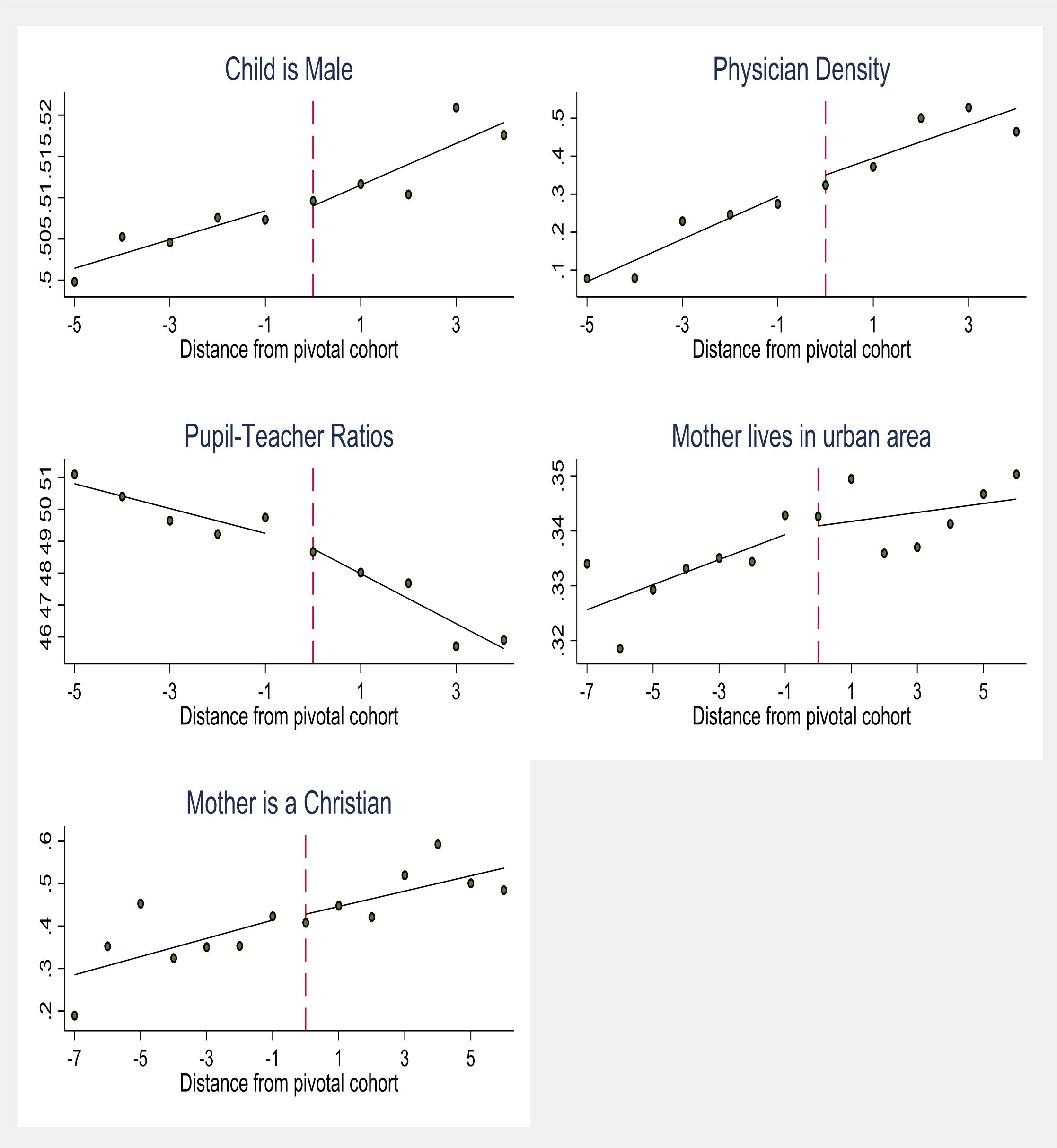
Source: World Development Indicator, 2018.

Figure B3.4(2): Gross enrollment rate per country



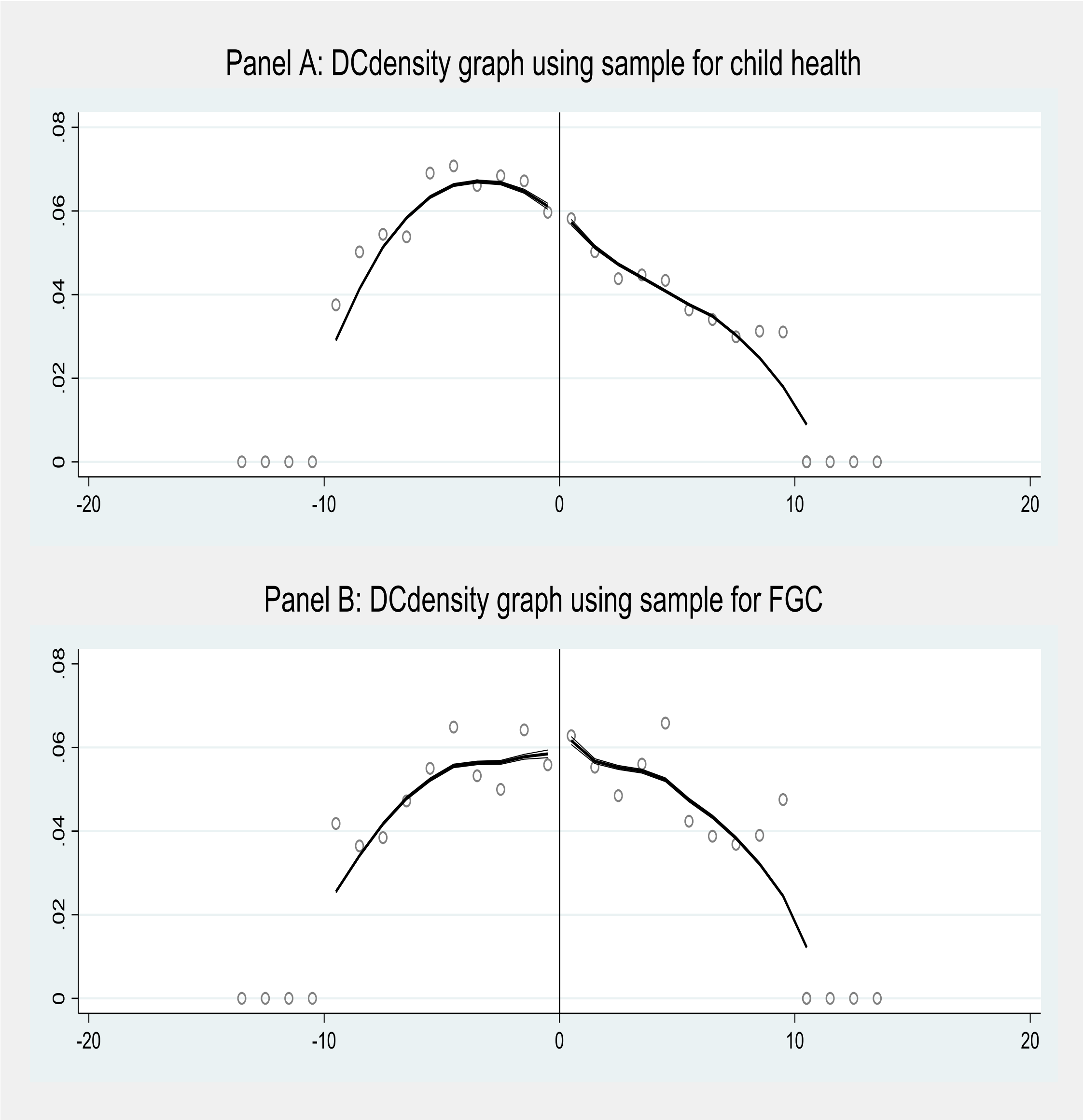
Source: World Development Indicator, 2018.

Figure B3.5: Impact of whether exposed to FPE on covariates for females born in years around the pivotal cohort



Notes: The linear fit shows the trend in the covariates.
Source: DHS dataset.

Figure B3.6: Density function of the birth year



Source: DHS dataset.