# Effects of Schooling on Female Genital Cutting
# The Case of Kenya

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Needless to say, any remaining errors are my own.

# Abstract

There is not much conclusive evidence on conditions for female genital cutting to decline (Hayford, 2005). Relying on data from the Kenya Demographic and Health Surveys, I study whether increased educational attainment for a sample of women born between 1950 and 1980 has had any effect on the probability that the genitals of their eldest daughter have been cut. In order to mitigate the problem of omitted variable bias, I utilize an exogenous change in years of schooling as a result of the 1985 restructuring of the education system.

According to the two-stages least-squares results, receiving an additional year of schooling decreases the probability that the eldest daughter of respondents complying with the reform was cut. This change accounts for 11 % of the sample mean. Because educational attainment is measured with some noise, I focus on the intention-to-treat effect by estimating a reduced-form model. On average, the reform led to a decrease in the probability that the eldest daughter of respondents was cut. This compares to 16 % of the sample mean.

I demonstrate that the results might be consistent with an intra-household bargaining narrative. This finding differs from that of UNICEF (2013) where the role of family dynamics is downplayed. Because the intention-to-treat effect is heterogeneous across ethnic groups, the intra-household bargaining narrative might be consistent with convention theories since marriage markets are overlapping. My findings challenge the current practice by the policy community.

# Contents

# 1 Introduction

Female genital cutting is a serious public health issue (WHO, 2012). The practice occurs in many African countries, as well as in some countries of the Middle East and Asia and among immigrants in Europe and North America. While more than 100 million girls and women living today have undergone genital cutting, there is not much conclusive evidence on conditions for the practice to decline (Hayford, 2005).

Although many findings in this literature are not very conclusive, the policy community have, to a large degree, mainstreamed their interventions.[1] It is in the light of this observation that I have chosen to look at female genital cutting. In order to investigate the drivers of female genital cutting, I study the effect of providing mothers with more education on the probability that the genitals of their eldest daughter is cut. A rationale for studying the effect of education on the prevalence of cutting, specifically, is that this will enable me to look into the drivers of this relationship. Because the relationship between education and the prevalence of female genital cutting is likely to be driven by omitted variables (see, e.g., Carr (1997) for a discussion on education and female genital cutting), I undertake a more sophisticated analysis in order to identify a causal relationship.

Kenya is an ideal country to look into because legislation against female genital cutting was adopted as late as 2001 (UNICEF, 2013). Relying on data on a sample of 15443 women born between 1950 and 1980 from the Kenya Demographic and Health Surveys, I verify that increased educational attainment is associated with a decrease in the probability that the genitals of their eldest daughter is cut. This finding corresponds to that in other countries (see, e.g., Boyle et al. (2002), Caldwell et al. (1997), and Yount (2002)).

In order to mitigate the problem of omitted variable bias, I follow Chicoine (2012) by utilizing an exogenous change in years of schooling as a result of the 1985 restructuring of the education system in Kenya. The restructuring of the education system constrained pupils to stay one year longer in school to obtain primary school certificates. I argue that the likelihood of being affected by this reform is exogenous, when controlling for date of birth.

As illustrated by my first-stage results, the reform is a strong predictor of increased years of schooling. According to the two-stages least-squares results, receiving an addi-

[1] A recent example is a report issued by UNICEF (2013) pushing a unified view on why female genital cutting tends to persist. The report was also covered by The Economist (2013). I will discuss this study later.

tional year of schooling led to a 1.4 percentage points decrease in the probability that the eldest daughter of respondents complying with the reform was cut. This accounts for 11 % of the sample mean. Because educational attainment is measured with some noise, I focus on the intention-to-treat effect by estimating a reduced-form model. On average, the reform led to a 2 percentage points decrease in the probability that the eldest daughter of the respondents was cut. This compares to 16 % of the sample mean.

Undertaking robustness checks I argue that the reform only captures the effect on the decision to perform female genital cutting through its effect on schooling. In the placebo tests I argue that Kenya has not experienced a change over the same time period as I study which has led to a decrease in the prevalence of female genital cutting. I further show that the intention-to-treat effect is heterogeneous. The effect is especially so in terms of the ethnic origin of respondents.

Linking my findings to economic theory, I investigate potential mechanisms behind this causal relationship. Although highly suggestive, I find that the results are consistent with an intra-household bargaining narrative. The education reform increased the position of the female respondents relative to their husbands. This is likely to have increased the bargaining power of women. Given that Kenyan women to a larger extent than men prefer female genital cutting to be abolished (see, e.g., Ahlberg et al. (2000), Shell-Duncan and Hernlund (2000), and Simister (2010)), it is plausible that the prevalence declines.

Although this finding is consistent with parts of the literature, it differs from arguments offered by UNICEF (2013) in which the role of family dynamics is downplayed. Because the intention-to-treat effect is quite heterogeneous across ethnic groups, the intra-household bargaining narrative might be consistent with convention theories since marriage markets are overlapping. These findings challenge the policy community.

## 1.1 Contribution to the literature

Following Yount (2002), reasoning around conditions for the decline of female genital cutting are often classified into three branches. These categories include modernization theory, feminist theory and convention theory. While many scholars do not rely on a single theory when investigating the practice, they are often motivated by one tradition (Steinmetz, 2012). Therefore, it is relevant to discuss female genital cutting in light of this categorization of the literature.

Modernization theory links female genital cutting to social and economic development (Boyle et al., 2002; Hayes, 1975). The change from an agrarian economy to an economy

based around the production of manufactured goods and services is accompanied by a change of focus from social responsibility to individual rights. Such societal processes tend to affect people's preferences. Coinciding with the spread of modern values, preferences for female genital cutting will therefore erode. One reason for this could be that education makes women realize the negative health consequences associated with genital cutting.

Feminist theory focuses on women's status as an important driver of social change (Althaus, 1997; Yount, 2002). As women become less dependent upon their husbands, they have a larger say when discussing issues related to, e.g., the bodies of their daughters. In economics an intra-household bargaining problem (see, e.g., Becker (1981)) is often relied on when analyzing issues such as this. If increased educational attainment increases the position of the women relative to that of her husband, her bargaining power will increase. The higher the bargaining power, the more decisive is the women.

Convention theory conceptualizes female genital cutting as a social practice upheld by social norms (Mackie, 1996). Even if group members are opponents to the practice, it persists because of the high social sanctions incurred by deviating. An example of such sanctions is exclusion from the marriage market. Unless it is known that others are against the practice, costs of deviating are too high. It is a coordination problem, and the practice is upheld because information is not sufficiently distributed in society. Cutting will not decline rapidly before a critical mass (following Schelling (1978)) has turned away from the practice.[2]

I find that providing additional education for mothers reduce the probability that they cut the genitals of their eldest daughter. Although highly suggestive, I show that my findings are consistent with an intra-household bargaining narrative. The education reform increased the position of the female respondents relative to their husbands. This is likely to have increased the bargaining power of women. Given that Kenyan women to a larger extent than men prefer female genital cutting to be abolished (see, e.g., Ahlberg et al. (2000), Shell-Duncan and Hernlund (2000), and Simister (2010)), the prevalence is likely to decline. If these arguments is hold, changes in information asymmetries and preferences have not alone facilitated the decline of female genital cutting observed in Kenya.

[2]A related literature within economics is the one viewing female genital cutting as a pre-martial investment (see, e.g., Chesnokova and Vaithianathan (2010) and Rai and Sengupta (2013). Another related literature is viewing female genital cutting as a question about identity (see, e.g. Akerlof and Kranton (2000), Coyne and Coyne (forthcoming) and Coyne and Mathers (2011), and thus complementing the approach by Mackie (1996).

Because the intention-to-treat effect is quite heterogeneous across ethnic groups, the scope of the intra-household bargaining narrative might be consistent with Hayford (2005). Due to large heterogeneity across space, small groups can stop undertaking female genital cutting without generating societal change. Since marriage markets are overlapping (see, e.g., Agorash and Childs (2006), Iyer and Weeks (2009), and Luke and Minshi (2006)) and different subpopulations attach varying degrees of importance to the practice, more than one tipping point may exist. Thus, intra-household bargaining is relevant to the 'process of gathering the critical mass of support that, according to convention theory, is necessary to end female genital cutting' (Hayford, 2005, p. 136).

This suggestive argument is also in line with the between-household variation in female genital cutting found in the Gambia (Bellemare et al., 2014). My findings differs, however, from a recent report issued by UNICEF (2013). Based on anecdotal evidence and some problematic quantitative analysis, they end up downplaying the role of intra-household dynamics.[3]

The structure of the rest of the thesis is the following: In the next section I discuss the 1985 restructuring of the education system and other policy reforms. The data I rely on is presented in Section 3. In the same section, I present alternative ways of measuring educational attainment. In Section 4 baseline results are discussed and the identification strategy is presented. Section 5 presents results from the two-stages least-squares and reduced-form models. I further present robustness checks and placebo tests, as well as assessing heterogeneity and exploring potential mechanisms. Section 6 concludes.

# 2 Education reform

## 2.1 The 1985 restructuring of the education system

Under colonial rule, the provision of education was segregated along racial lines. Pupils of African origin did not have compulsory primary education, while pupils of Asian and European origin had (Eshiwani, 1990). While the colonial history has been incremental for today's interest in universal primary education in Kenya, no major education reforms were adopted in the few years after independence from the United Kingdom in 1963.

[3]More specifically, I argue that UNICEF downplays the role for family dynamics on wrong grounds. Their arguments are based on descriptive statistics, in which they compare respondents that appear to be very different and are survey in different years.

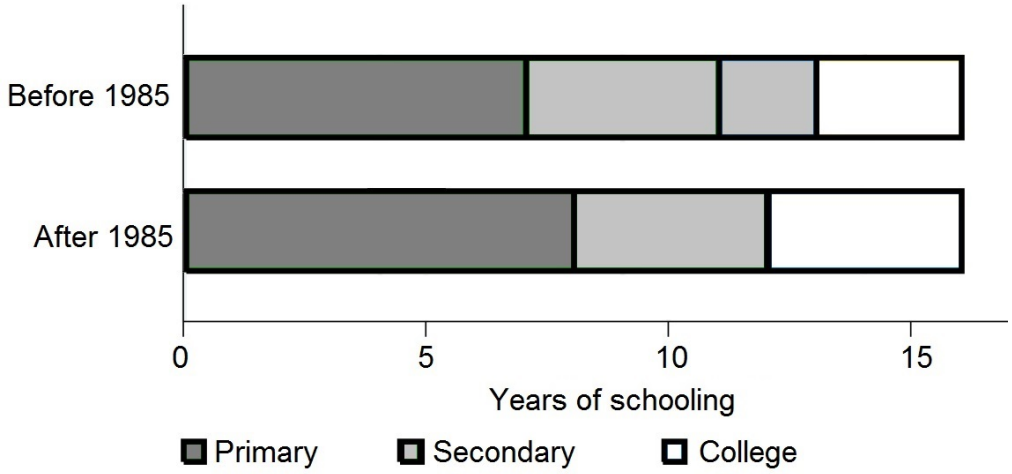


**Figure 1:** The effect of the 1985 reform on the education system.

According to Somerset (2007; 2009), President Jomo Kenyatta, Kenya's first head of state after independence, was elected in parts because he promised universal primary education. For various reasons, however, he did not end up reforming the education system. Based on recommendations from the Ominde Commission (Republic of Kenya, 1964), the educational system established by the former colonial power was continued (see, e.g., Ferre, 2009). This structure is referred to as a 7-4-2-3 system, consisting of seven years of primary school, four years of lower secondary school, two years of upper secondary school, and three years of college.

Primary school attendance increased at a very low rate after independence. These cohorts experienced relatively low drop-out rates. In addition to the low intake of pupils, another challenge was the frequent repetition in the two last grades of primary school in order to qualify for primary school certificates (see, e.g., Somerset, 2009).

In order to increase primary school attendance as well as reduce the amount of repetition, the Government of Kenya reformed the education system in January 1985 (Somerset, 2007). The new education system came to be on a 8-4-4 structure, comprising eight years of primary school, four years of secondary school, and four years of college (Somerset, 2007). The restructuring of the education system constrained pupils to stay one year longer in school to obtain primary school certificates, one year shorter in school to obtain secondary school certificates, while pupils obtaining college degrees needed to stay 16 years across both systems. The restructuring of the education system is illustrated in Figure 1. As a consequence of this policy change, no classes were admitted into secondary school in 1985. The last pre-reform class finished primary school in 1983 (Somerset, 2007). I argue that the likelihood of being affected by this reform is exogenous, when controlling

for date of birth.

Note that the reform might have had undesired effects. First, the policy change might have reduced the amount of pupils staying in school after primary school because of increased attrition (Chicoine, 2012). This may have affected the quantity of pupils in secondary school negatively (Somerset, 2007). Second, the reform put added stress on primary schools. The increase in the number of pupils was only partially compensated by hiring more teachers. This led to an increase in the pupil-teacher ratio (Somerset, 2009). This might have had negative effects on the quality of the additional year of primary education for the treated group. Note also that since no classes were admitted into secondary school in 1985, the reform necessarily left more teachers per pupil in the control group.

## 2.2 Other reforms

Since 1963, other policy efforts have been undertaken in order increase primary school enrollment. These are the Free Primary Education programs of 1974, 1979 and 2003 (Somerset, 2009). While it is not relevant for me to take into account the 2003 program because it was initiated after mothers finished their education, I need to take into account the consequences of the 1974 and 1979 fee bans on the likelihood of being affected by the 1985 education reform. It should further be noted that the issues related to female genital cutting was not put on the curriculum before 2004 (Allen et al., 2013). This was after the mothers I study finished schooling.

An abolishment of formal fees up to the fourth grade for primary schools in arid and semi-arid land was implemented in 1974. From 1973 to 1974, intake of primary school pupils increased by 150 % (Somerset, 2009). This observation correlates well with the fee reduction. The intake reduced sharply in the few subsequent years after 1974, returning to the trend. Another implication of the policy change was that children had a tendency to start primary school at different ages. Note further that drop-out rates doubled over this period (Somerset, 2009).

Non-formal fees were not banned as a part of the 1974 program. According to Somerset (2009), such fees included the construction and maintenance of primary schools buildings and the accommodation of teachers. Because non-formal fees necessarily increased as a result of the higher enrollment of pupils, political will to reform the fee system increased (Somerset, 2009).

In 1979 President Daniel arap Moi, succeeding the late President Kenyatta, extended

the ban on fees. The ban was extended in two dimensions. Rather than covering people in arid and semi-arid areas up to the fourth grade of primary school only, it now included pupils up to the sixth grade in most of the country. In addition, non-formal fees were banned. In consequence, communal work and community fund-raising financed the construction and maintenance of primary schools buildings and the accommodation of teachers.

As a result of this reform, enrollment and the number of drop-outs increased, well in line with the experiences from the 1974 fee ban. In the years after the 1979 reform, enrollment declined rapidly again (Somerset, 2009). This is also in line with the observation from the 1974 reform. It should be noted that this may also in parts be attributed to the adverse effects of the abolishment of non-formal fees. Because communal activities did not raise sufficient funds, parent-based levies were gradually imposed (Somerset, 2007).[4]

The consequences of the 1974 and 1979 reforms were quite similar. Enrollment increased sharply, but then returned to trend-levels. These two reforms therefore affected the timing of when pupils entered primary school and whether they continued education (Chicoine, 2012). I take this into account when estimating the likelihood that a given cohort was affected by the 1985 restructuring of the education system.

# 3 Data

## 3.1 Sources

All data at the individual level comes from the three last rounds of the Kenya Demographic and Health Surveys (KDHS). These survey rounds are undertaken in the years 1998, 2003, and 2008 to 2009. The rationale for not utilizing individual level data from the 1989 and 1993 survey rounds is that questions on female genital cutting were not included. The KDHS dataset I rely on is the individual recode for female respondents. This part of the survey includes information on birth date, religion, age at marriage and education attainments. The survey also includes husband characteristics like age at marriage and educational attainments. In addition, information on whether the eldest daughter of the

[4]The next reform of the fee system is the third Free Primary Education program of 2003. After the defeat of President Moi, President Mwai Kibaki initiated a ban of the parent-based levies which arose in the aftermath of the 1979 program. Although having consequences similar to the 1974 and 1979 reforms, analyses tend to be less pessimistic (see, e.g., Lucas and Mbiti (2012)). It appears like the effects of this fee reduction can be sustained. As noted earlier, however, this reform is not relevant for my study.

respondents have been cut is included.[5] Due to restricting the sample to women reporting on female genital cutting, 3804 out of 15440 observations go missing. The vast majority of respondents coded as missing has only recently given birth to their first-born child. Therefore, they have not yet needed to decide whether to cut their daughters. People who did not respond are on average five and a half years younger, have two years more education, and are slightly more likely to have grown up in urban areas. This difference is mitigated when increasing the age of the respondents.

Demographic characteristics such as ethnicity are included, as well as retrospective information on childhood place of residence. I study women born between 1950 and 1980, aged 20 or more at the time of the survey. Very few respondents are born before 1950. Therefore, there is a rationale for not including these. As I show later, findings are robust to limiting the sample to those born after 1955.

Since the dataset gives information about the month and year of birth, I will be able to take this into account when estimating the probability of being treated by the 1985 reform. More specifically, treatment status is assigned based on the year and quarter of birth of female respondents. No respondents are lost by doing this.

Summary statistics are presented in Table 1. The sample used in producing this table is the same as described above. Women in this sample have an average age of 33 years and an average of seven years of schooling. 46 % has finished at least eight years of schooling. 13 % has had their eldest daughter cut, while it is evident that there is a lot of heterogeneity. It is important to have this in mind when undertaking the data analyses later. While respondents tend to come from different parts of the country, as indicated by the ethnicity data, Kikuyu, Luhya, Kalenjun, and Luo have sample shares larger than 10 %. 86 % of the women are Christian, with the majority of these being Protestants. 80 % of the respondents grew up on the countryside, while fewer than 10 % come from cities.

Table 2 illustrates how the cutting of the eldest daughter vary by the characteristics of the mother. Mothers with low levels of education have a higher tendency to have their daughters cut. For those with no years of schooling, for example, the prevalence is 34 %. For those with eight years of schooling, 5 % of the mothers have cut their eldest daughter, while this number is 9 % for those with seven years of schooling. For those with many years of education, a low share of cutting is observed. Cutting is most common for respondents of Somali, Kisii, and Masai origin, respectively. The prevalence is low for Luhya, Luo, Mijikenda and Swahili, and Kikuyu. 38 % of Muslim mothers in the sample

[5] This information is given explicitly in the 1998 and 2003 survey rounds, but I have needed to construct the variable for the the 2008-2009 survey round.

**Table 1:** Summary statistics for characteristics and the outcome.

| Variable | Mean | Std. Dev. | N |
|---|---|---|---|
| Age in years | 33.137 | 7.660 | 15443 |
| | | | |
| Education in years | 6.974 | 4.410 | 15440 |
| Share with at least 8 years of schooling | 0.463 | 0.499 | 15440 |
| | | | |
| Share of mothers that cut daughter | 0.127 | 0.333 | 11640 |
| | | | |
| Share of Kalenjun | 0.115 | 0.319 | 15434 |
| Share of Kamba | 0.096 | 0.294 | 15434 |
| Share of Kikuyu | 0.207 | 0.405 | 15434 |
| Share of Kisii | 0.061 | 0.239 | 15434 |
| Share of Luhya | 0.142 | 0.349 | 15434 |
| Share of Luo | 0.109 | 0.311 | 15434 |
| Share of Meru and Embu | 0.064 | 0.245 | 15434 |
| Share of Mijikenda and Swahili | 0.078 | 0.268 | 15434 |
| Share of Somali | 0.048 | 0.213 | 15434 |
| Share of Masai | 0.013 | 0.115 | 15434 |
| Share of other ethnicities | 0.067 | 0.250 | 15434 |
| | | | |
| Share of Catholics | 0.235 | 0.424 | 15427 |
| Share of Protestants | 0.625 | 0.484 | 15427 |
| Share of Muslims | 0.108 | 0.311 | 15427 |
| Share with no religion | 0.025 | 0.157 | 15427 |
| Share with other religion | 0.007 | 0.085 | 15427 |
| | | | |
| Share from city | 0.092 | 0.289 | 15427 |
| Share from town | 0.092 | 0.289 | 15427 |
| Share from countryside | 0.800 | 0.400 | 15427 |
| Share from abroad | 0.016 | 0.125 | 15427 |

Source: KDHS 1998, 2003, and 2008-2009.

Mean and std. dev. for city and town differ.

have cut their eldest daughter. The corresponding number is 12 % for Catholics, while being below 10 % for Protestants and mothers without any religious affiliation. The share of mothers that cut their daughters is around 12 % when the childhood place of residence is a town and 14 % for the countryside. The number is also high for those that grew up abroad, while it is relatively low for those from cities.

Finally, I provide a description of other data sources. In order to estimate the likelihood that a pupil proceeded through primary school and into secondary school before the 1985 restructuring of the education system, I utilize the calculations in Chicoine (2012). The calculations are based on enrollment and failure rate data from the World Bank's Education Statistics and UNESCO's Institute for Statistics, information on the age of pupils enrolled in grade one in 1978 and 1979 from Somerset (2007), and transition rates from primary school to secondary school from Ohba (2009). For the enrollment data used as control variables in some of my auxiliary regressions, I rely on Somerset (2007).

## 3.2 Measuring education

The success of my study depends on whether I manage to measure the educational attainments of female respondents in a desirable way. The KDHS survey rounds take a two-question approach to measure educational attainment. There is a discrete variable measuring years of schooling, while there is a categorical variable measuring the highest education level (i.e., primary school, secondary school, and education at a higher level).

While it is evident that a person born after 1971 needed to study for eight years in order to graduate primary school and safe to assume that a person born before 1964 needed to study for seven years, the picture is not as clear for the cohorts between. Therefore, the years of schooling variable proxy educational attainments with some noise for these cohorts.

In addition to relying on the years of schooling variable in my regressions, I construct two additional variables to measure educational attainment. Following Chicoine (2012), I define the variable

$$Education8_i = \begin{cases} 1 & \text{if } education\ level = primary \text{ and } education\ years = 8 \\ 0 & \text{otherwise} \end{cases}$$

This variable captures whether pupil $i$ spent eight years in primary school. Figure 2 illustrates how having spent eight years in school vary by birth cohort. The picture is interesting. The share of women spending eight years in school is low for pre-reform

**Table 2:** Summary statistics on how cutting vary by characteristics.

| Variable | Mean | Std. Dev. | N |
|---|---|---|---|
| Share with 0 years of education that cut | 0.335 | 0.472 | 2187 |
| Share with 1 years of education that cut | 0.189 | 0.394 | 74 |
| Share with 2 years of education that cut | 0.209 | 0.407 | 235 |
| Share with 3 years of education that cut | 0.171 | 0.377 | 356 |
| Share with 4 years of education that cut | 0.121 | 0.327 | 527 |
| Share with 5 years of education that cut | 0.123 | 0.328 | 563 |
| Share with 6 years of education that cut | 0.092 | 0.289 | 869 |
| Share with 7 years of education that cut | 0.092 | 0.288 | 2043 |
| Share with 8 years of education that cut | 0.046 | 0.209 | 1743 |
| Share with 9 years of education that cut | 0.118 | 0.323 | 459 |
| Share with 10 years of education that cut | 0.030 | 0.172 | 394 |
| Share with 11 years of education that cut | 0.061 | 0.240 | 800 |
| Share with 12 years of education that cut | 0.019 | 0.136 | 796 |
| Share with 16 years of education that cut | 0.037 | 0.192 | 27 |
| Share with 20 years of education that cut | 0.040 | 0.200 | 250 |
| | | | |
| Share of Kalenjun that cut | 0.096 | 0.295 | 1434 |
| Share of Kamba that cut | 0.062 | 0.241 | 1097 |
| Share of Kikuyu that cut | 0.048 | 0.213 | 2281 |
| Share of Kisii that cut | 0.561 | 0.497 | 736 |
| Share of Luhya that cut | 0.005 | 0.068 | 1739 |
| Share of Luo that cut | 0.007 | 0.084 | 1254 |
| Share of Meru and Embu that cut | 0.110 | 0.313 | 717 |
| Share of Mijikenda and Swahili that cut | 0.015 | 0.122 | 863 |
| Share of Somali that cut | 0.702 | 0.458 | 601 |
| Share of Masai that cut | 0.397 | 0.491 | 179 |
| Share of other ethnicities that cut | 0.205 | 0.404 | 732 |
| | | | |
| Share of Catholics that cut | 0.123 | 0.329 | 2757 |
| Share of Protestants that cut | 0.087 | 0.281 | 7241 |
| Share of Muslims that cut | 0.376 | 0.484 | 1275 |
| Share with no religion that cut | 0.097 | 0.296 | 300 |
| Share with other religion that cut | 0.069 | 0.256 | 58 |
| | | | |
| Share from city that cut | 0.036 | 0.186 | 918 |
| Share from town that cut | 0.122 | 0.327 | 1011 |
| Share from countryside that cut | 0.137 | 0.344 | 9539 |
| Share from abroad that cut | 0.106 | 0.308 | 161 |

Source: KDHS 1998, 2003, and 2008-2009.

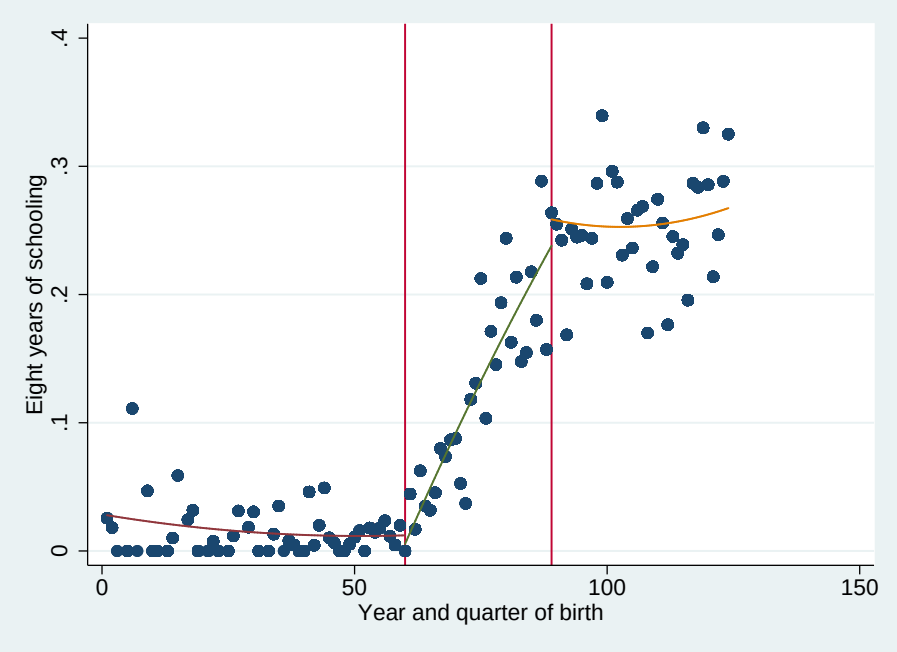


**Figure 2:** The share having spent eight years in school varies by birth cohort. Observations in the plots are local averages and the lines are OLS-fitted polynomials of degree 2, fitted for pre-reform, transition and post-reform cohorts. These cohorts are separated by the vertical lines.

cohorts, increasing for the cohorts in the transition group and slowly increasing for the post-treatment cohorts. This is indicative evidence for the reform's effect on educational attainment.

Another variable I define is

$$Atleast8_i = \begin{cases} 1 & \text{if } education\ level = primary \text{ and } education\ years = 8 \\ 1 & \text{if } education\ level = secondary \text{ and } education\ years \geq 8 \\ 1 & \text{if } education\ level = higher \\ 0 & \text{otherwise} \end{cases}$$

This variable captures if pupil $i$ spent at least eight years in the school system. This is true if the pupil spent eight years in primary school, at least one year in secondary school irrespective of the education system, and necessarily so if the pupil advanced to education at a higher level. Figure 3 illustrates how having spent eight years or more in school vary by birth cohort. While this share is increasing for the pre-treatment cohorts, it is increasing even more for the transition cohorts and becoming less steep for the post-treatment cohorts. This is an indication of the reform's effect on educational attainment.

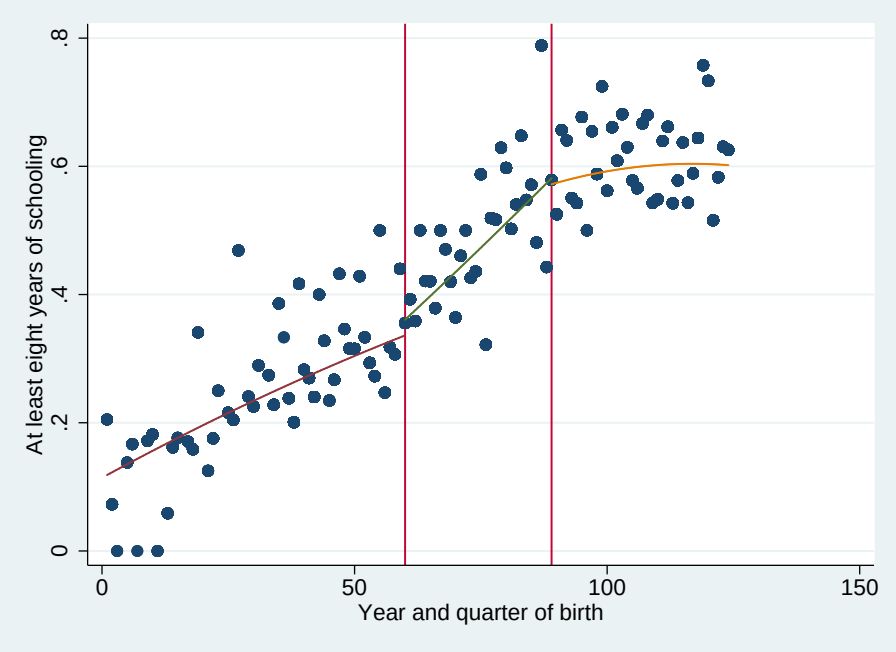


**Figure 3:** The share having spent eight years or more in school varies by birth cohort. Observations in the plots are local averages and the lines are OLS-fitted polynomials of degree 2, fitted for pre-reform, transition and post-reform cohorts. These cohorts are separated by the vertical lines.

# 4 Methodology

## 4.1 Baseline estimation through ordinary least-squares

As stated in the introduction, there is a correlation between a women's educational attainment and the decision to perform genital cutting on daughters. Typically, this correlation is found to be negative, meaning that an additional year of schooling, on average, is associated with lower rates of cutting (see, e.g., Boyle et al. (2002), Caldwell et al. (1997), and Yount (2002)). According to my baseline estimates, this pattern is also evident for the sample of Kenyan women born between 1950 and 1980.

My baseline estimates rely on an ordinary least-squares (OLS) model. I start by measuring educational attainment by years of schooling, $Education_{ic}$. Based on previous discussions, I will also consider other ways of measuring educational attainment. The regression model is

$$Cut_{ic} = \alpha + Education_{ic}\beta + \sum_{p=1}^{3} Age_{ic}^{p}\pi_p + X_{ic}\theta + \varepsilon_{ic} \quad (1)$$

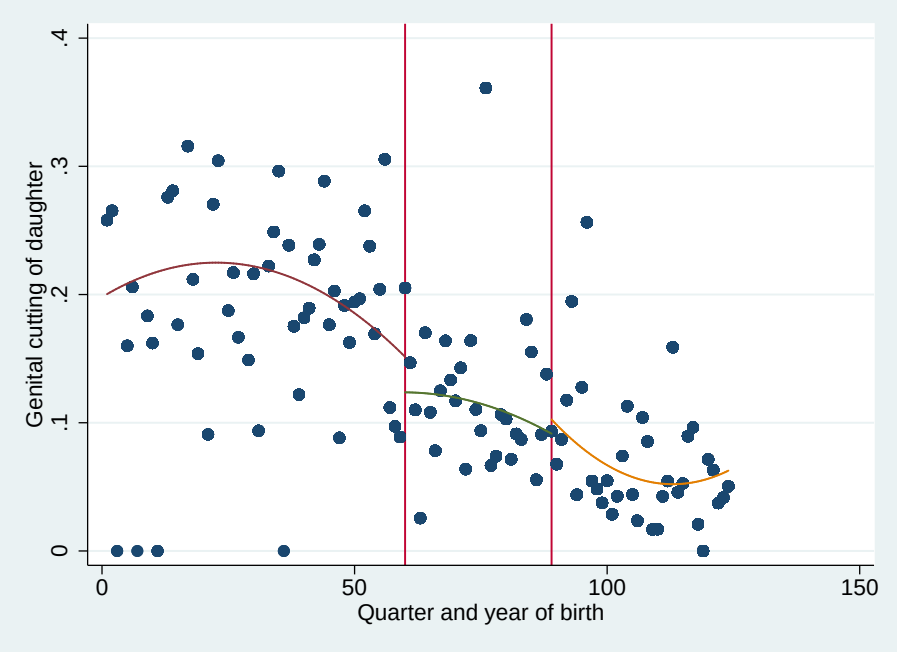


**Figure 4:** The share of cut daughters varies by the birth cohort of mothers. Observations in the plots are local averages and the lines are OLS-fitted polynomials of degree 2, fitted for pre-reform, transition and post-reform cohorts. These cohorts are separated by the vertical lines.

for the outcome $Cut$ for a specific women $i$ being a part of cohort $c$, defined by year and quarter of birth. $Cut_{ic}$ captures whether the respondent ended up cutting the genitals of her eldest daughter. The age (in years) of the respondent at the time of the survey is measured by $Age_{ic}$. I have allowed for squared and cubic terms of this characteristic. $X_{ic}$ is a vector of individual level characteristics that are either constant over time or occurred before the restructuring of the education system. These characteristics include ethnicity, quarter of birth, childhood place of residence, as well as religion. One may worry that religious affiliation might be a post-treatment variable. Therefore, I also display results without including this characteristic. In the regressions, I do not rely on the KDHS sample weights.[6] Standard errors are clustered at the birth cohort level.

Before presenting the OLS results, it may be informative to look into what we may expect. Figure 4 illustrates the relationship between the birth cohort of mothers and whether the genital of her eldest daughter has been cut. While the trend is decreasing, it is deceasing more in magnitude for the cohorts in the transition and post-reform cohorts. This observation fits especially well for the subsamples of people that grew up in the countryside as well as the respondents that are Kisii, Meru and Emba.

[6]Findings are qualitatively the same when utilizing these weights, although the magnitude is higher.

**Table 3:** OLS estimates of years of education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| Education | -0.023*** | -0.010*** | -0.010*** | -0.010*** | -0.010*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Age | | -0.095*** | -0.095*** | -0.095*** | -0.094*** |
| | | (0.019) | (0.019) | (0.019) | (0.019) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 11638 | 11631 | 11631 | 11620 | 11612 |
| $R^2$ | 0.083 | 0.396 | 0.396 | 0.397 | 0.398 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I now investigate this pattern more formally. Baseline estimates are presented in Table 3 for the measure $Education_{ic}$. A negative correlation between mothers' years of education and cutting of eldest daughter seems to hold, also conditioning on a set of control variables. These variables include ethnicity, quarter of birth, birthplace characteristics, and religion. Across different specifications (columns (1) to (5)), the partial correlation tend to be between -2.3 and -1.1 percentage points. These estimates are statistically significant at the 1 % level. This implies that an additional year of schooling, on average, is associated with a 1.1 to 2.3 percentage points decrease in in cutting.

Table 4 shows similar, although a bit weaker, results for the measure $Education8_{ic}$. When not holding constant for other characteristics, having finished exactly eight years of schooling is associated with a 10.5 percentage points lower likelihood of a mother ending up having her daughter cut (column (1)). The estimate is significantly different from 0 at the 1 % level. When controlling for ethnicity (column (2)), the estimate drops to -0.5 percentage points. This estimate is no longer significantly different from 0 at conventional levels. This is because this type of education measure is highly correlated

**Table 4:** OLS estimates of eight years of education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| Education8 | -0.105*** | 0.005 | 0.005 | 0.005 | 0.005 |
| | (0.009) | (0.007) | (0.007) | (0.007) | (0.007) |
| Age | | -0.115*** | -0.115*** | -0.113*** | -0.111*** |
| | | (0.021) | (0.021) | (0.021) | (0.021) |
| Age2 | | 0.004*** | 0.004*** | 0.004*** | 0.004*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 11640 | 11633 | 11633 | 11622 | 11614 |
| $R^2$ | 0.012 | 0.383 | 0.383 | 0.385 | 0.387 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with the ethnicity of the respondent.[7] The estimate is robust to controlling for quarter of birth, birthplace characteristics, and religion (see columns (3) to (5)).

The partial correlations in Table 5 are strong. This table shows regression outcomes for the measure $Atleast8_{ic}$. Basically, we are comparing women that did undertake eight years of education or more with those who did not, conditioned on a set of control variables. On average, respondents that finished eight years of schooling or more tend to have a smaller change of having cut their eldest daughter. While the column (1) estimate of -13.6 percentage points is a large in magnitude, compared to the sample mean, estimates reported in columns (2) to (5) tend to be more informative. When holding constant for characteristics of respondents, the estimates reported are between -5.5 and -5.1 percentage points. All estimates are statistically significant at the 1 % level. This means that mothers that finished eight or more years of schooling, on average, tend to have a 5.1 to 5.5 percentage points smaller chance of having performed genital cutting on their eldest daughter.

The OLS results show that the higher the educational level of the Kenyan mother, the

[7]Because this finding persists across different specifications, I will look into heterogeneity later.

**Table 5:** OLS estimates of at least eight years of education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| Atleast8 | -0.136*** | -0.055*** | -0.055*** | -0.053*** | -0.051*** |
| | (0.009) | (0.006) | (0.006) | (0.006) | (0.006) |
| Age | | -0.102*** | -0.102*** | -0.101*** | -0.100*** |
| | | (0.020) | (0.020) | (0.020) | (0.020) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 11638 | 11631 | 11631 | 11620 | 11612 |
| $R^2$ | 0.040 | 0.389 | 0.389 | 0.390 | 0.392 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

lower is the probability that she performed genital cutting on her eldest daughter. We should, however, be careful in giving this a causal interpretation. This is because there are good reasons to believe that the estimate is biased. More specifically, years spent in school might be correlated with some unobservable characteristic that also affects the decision to cut the daughter. Thus, the relationship might be driven by these omitted variables.[8] To mitigate this issue I rely on an instrumental variable (IV) strategy. I utilize an exogenous change in the years spent in the primary school as a result of the 1985 restructuring of the education system in Kenya.

## 4.2 Identification strategy

Pupils who started secondary school in 1985 or later needed to undertake eight years of primary school. Pupils who obtained their primary school certificates in 1983 only needed to stay seven years in primary school. As noted earlier, primary school was not compulsory at this point in time. In consequence, pupils could enroll primary school at different ages.

[8]Carr (1997) provides a discussion on what variables this might be.

Due to the fact that pupils started school at different ages and that the old educational system was plagued with large repetition rates, the effect of the 1985 restructuring of the education system was different across cohorts. This stands in contrast to, e.g., a study by Ferre (2009) in which a sharp regression discontinuity approach is applied to study the effect of education on fertility decisions.

To be able to capture the differential effect of the reform across birth cohorts, I rely on an instrument developed by Chicoine (2012). Chicoine's instrument is inspired by Angrist and Lavy (1999) in that it estimates the probability of being treated. More specifically, it is an application of Borkum (2010) by relying on pre-reform data on enrollment in grade-one of primary school to predict the probability that an individual born in a given year and quarter is affected by the restructuring of the education system. In addition to taking this information into account, Chicoine (2012) uses data on enrollment and repetition at every grade of primary school as well as transition rates from primary school to secondary school to estimate the probability of treatment.[9] This enables me to take into account the fluctuations in enrollment as a consequence of the 1974 and 1979 fee bans, discussed in Subsection 2.2.

The instrument is defined as the likelihood that an individual from a specific cohort $c$, defined by birth year and cohort, needed to undertake eight rather than seven years of primary school in order to obtain the primary school certificate. This likelihood is estimated according to

$$Inst_c = \begin{cases} 1 & \text{if } c \geq 1972Q1 \\ Pr(\text{Treated}) = 1 - Pr(\text{Secondary before 1985}) & \text{if } 1964Q4 < c < 1972Q1 \\ 0 & \text{if } c \leq 1964Q4 \end{cases}$$

If an individual is born in 1972 or later, it is certain that the person needed to undertake eight years of primary school in order to obtain the certificate. If the individual is born earlier than 1965, we can safely assume that the person needed to undertake only seven years of primary school in order to graduate.[10] In other words, these individuals were not affected by the education reform. If an individual was born between 1965 and 1972, the probability of being affected by the reform is described as 1 - the probability of reaching secondary school before 1985. This probability is defined by

[9] Although trying several times, I have not managed to get hold of the methodology used to calculate the specific values of the instrument.

[10] This assumption is discussed in Chicoine (2012).

$$Pr(\text{Secondary before 1985}) =$$
$$\sum_{a=6}^{1977-y} \sum_{r=0}^{1971-a-y} Pr(\text{Start school})\ Pr(\text{Complete primary} \mid \text{repeat})$$

In which Start school $= a \mid c$, where $a$ is the age at which a child could enroll primary school and $c$ is the birth cohort. Repeating grades is denoted $r$. Consider a person being a part of birth cohort $c$, born in the year $y$. Then the right-hand side of the equation is the probability of this person enrolling primary school at age $a$ multiplied by the probability of completing primary school, summed over the range of ages in which the individual could enroll primary school and the number of times grades could be repeated $r$ while still graduating before being affected by the 1985 reform.

One set-back with the instrument estimated by Chicoine (2012) is that it is not able to fully take into account drop-outs. I have not been able to incorporate this effect. Although discussions offered by Chicoine (2012) are weak, it is in my opinion ambiguous what the effect of this would be. On the one hand, I may overestimate the value of the instrument for cohorts early in the transition period by not accounting for drop-outs. On the other hand, drop-outs may be pupils that would not have enrolled primary school in the first place. If this is the case, my estimates are conservative. This leads to an underestimation of the causal effect. Therefore, it is difficult to assess whether the set-back is likely to be a significant problem for identifying causal effects.

Figure 5 illustrates how the value of the instrument and having finished eight years of schooling vary by birth cohort. Observe that the change in the probability of being affected by the reform fits well with the increase in schooling. This is an indication of a strong first-stage relationship.

## 4.3 The reduced-form and the two-stages least-squares models

In this subsection I present the reduced-form and the two-stages least-squares models. I start by verifying that the instrument predicts a change in education. The first-stage model is defined by

$$Education_{ic} = \alpha_F + Inst_c\beta_F + \textstyle\sum_{p=1}^{3} Age_{ic}^{p}\pi_{Fp} + X_{ic}\theta_F + \varepsilon_{Fic} \tag{2}$$

The variables are the same as described for the OLS model in Equation (1). $Inst_c$ is as

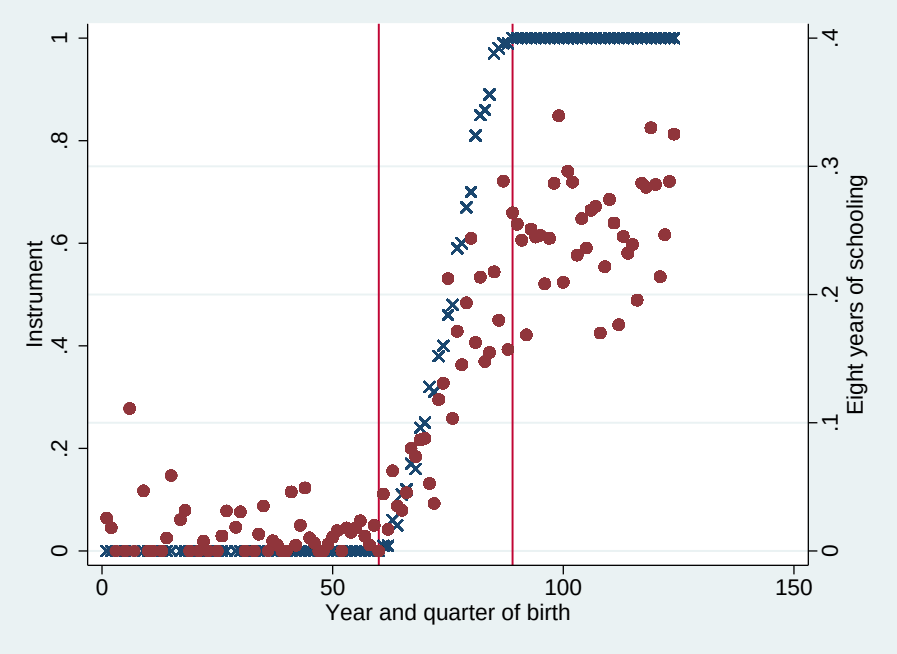


**Figure 5:** The value of the instrument (crosses) and having finished eight years of schooling (dots) vary by birth cohort. Dots are based on local averages. The figure is separated into pre-reform, transition and post-reform. Note that the scaling of the y axes differs.

defined in the previous subsection. The $F$ subscript refers to the parameters as first-stage parameters. Due to drawbacks of the education measure described earlier, I also estimate the same equation using $Education8_{ic}$ and $Atleast8_{ic}$ as outcome variables.

Table 6 displays the first-stage estimates of the instrument on years of education. The 1985 restructuring of the education system appears to be highly correlated with years of schooling. For the different specifications of the first-stage model, the significance level is at the 1 % level. Being fully affected by the reform tends to be associated with between 1.5 and 2.3 years of education, depending on which characteristics I control for (see columns (1) to (5)). The point estimate is close to unaffected by holding constant for religious affiliation.

A similar strong relationship is found for $Education8_{ic}$ in Table 7 as well as for $Atleast8_{ic}$ in Table 8. Being fully affected by the reform tends to increase the probability of achieving eight years of education by between 22 and 24 percentage points (see columns (1) to (5)). All estimates are significantly different from 0 at the 1 % level. Cohorts affected by the 1985 reform have a 24 to 31 percentage points higher chance of having completed eight years or more of schooling (see columns (1) to (5)). All estimates are significantly different from 0 at the 1 % level.

**Table 6:** First-stage estimates using OLS of the instrument on years of education.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Education | Education | Education | Education | Education |
| Instrument | 2.266*** | 1.713*** | 1.713*** | 1.536*** | 1.520*** |
| | (0.178) | (0.196) | (0.196) | (0.190) | (0.183) |
| Age | | 0.586 | 0.586 | 0.488 | 0.501 |
| | | (0.384) | (0.384) | (0.350) | (0.340) |
| Age2 | | -0.015 | -0.015 | -0.012 | -0.012 |
| | | (0.012) | (0.012) | (0.011) | (0.010) |
| Age3 | | 0.000 | 0.000 | 0.000 | 0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 15440 | 15431 | 15431 | 15415 | 15400 |
| $R^2$ | 0.055 | 0.240 | 0.240 | 0.281 | 0.300 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Results from the two-stages least-squares model are presented and discussed in the next section. I regress whether the eldest daughter has been cut on the predicted value of the of the mother's years of schooling from the first-stage

$$Cut_{ic} = \alpha_S + \widehat{Education}_{ic}\beta_S + \sum_{p=1}^{3} Age_{ic}^{p}\pi_{Sp} + X_{ic}\theta_S + \varepsilon_{Sic} \tag{3}$$

The subscript $S$ refers to the second-stage. The parameter $\beta_S$ represents the effect of the predicted value of education on the outcome for those complying with the treatment. Variables are the same as described for the OLS model. I also perform this analysis for the other education measures.

Because the education variable is not able to capture the full impact of the 1985 education reform, I follow Chicoine (2012) by mainly focusing on estimates from the reduced-form model. In the reduced-form model, the outcome, whether the eldest daughter of the respondent has been cut, is regressed on the instrument

$$Cut_{ic} = \alpha_R + Inst_c\beta_R + \sum_{p=1}^{3} Age_{ic}^{p}\pi_{Rp} + X_{ic}\theta_R + \varepsilon_{Ric} \tag{4}$$

**Table 7:** First-stage estimates using OLS of the instrument on eight years of education.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Education8 | Education8 | Education8 | Education8 | Education8 |
| Instrument | 0.235*** | 0.215*** | 0.215*** | 0.217*** | 0.218*** |
| | (0.006) | (0.011) | (0.011) | (0.010) | (0.010) |
| Age | | 0.038* | 0.038* | 0.039* | 0.038* |
| | | (0.022) | (0.022) | (0.022) | (0.022) |
| Age2 | | -0.001* | -0.001* | -0.001* | -0.001* |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | 0.000** | 0.000** | 0.000** | 0.000** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 15443 | 15434 | 15434 | 15418 | 15403 |
| $R^2$ | 0.090 | 0.109 | 0.109 | 0.111 | 0.112 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The subscript $R$ refers to the reduced-form. My focus will be on the parameter $\beta_R$, which represents the effect of the reform on the outcome. While it may be a worry that I pick up the effect of increased attrition, as discussed in Subsection 2.1, there are good reasons for looking at the reduced-form effect. It will enable me to study the direct effect of the education reform on the prevalence of female genital cutting. This is refereed to as an intention-to-treat effect.

# 5 Results and discussion

## 5.1 Two-stages least-squares estimates

In this subsection I present and discuss the estimates from the second-stage of the two-stages least-square model. The model is defined in Equation (3), while the first-stage results are presented in Tables 6 to 8. I discuss the results for the measure $Education_{ic}$, meaning that I am after illustrating the causal effect on cutting of the eldest daughter

**Table 8:** First-stage estimates using OLS of the instrument on at least eight years of education.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Atleast8 | Atleast8 | Atleast8 | Atleast8 | Atleast8 |
| Instrument | 0.308*** | 0.255*** | 0.255*** | 0.242*** | 0.242*** |
| | (0.014) | (0.018) | (0.018) | (0.017) | (0.017) |
| Age | | 0.040 | 0.040 | 0.031 | 0.032 |
| | | (0.041) | (0.041) | (0.038) | (0.038) |
| Age2 | | -0.001 | -0.001 | -0.001 | -0.001 |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | 0.000 | 0.000 | 0.000 | 0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 15440 | 15431 | 15431 | 15415 | 15400 |
| $R^2$ | 0.079 | 0.168 | 0.168 | 0.189 | 0.197 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

by letting a mother receive an additional year of education. As discussed in Subsection 3.2, educational attainment is measured with noise for the transition cohorts. Therefore, we should not fully trust these results. Results are also presented and discussed for the measures $Education8_{ic}$ and $Atleast8_{ic}$.

Results from the second-stage are presented in Table 9 for the measure $Education_{ic}$. What I estimate here is a local average treatment effect, meaning the treatment effect for those complying with the 1985 restructuring of the education system. For the compliers, the effect of an additional year of education on the decision to cut was negative. When not controlling for additional characteristics, giving a mother an additional year of education reduces the likelihood that she cut her eldest daughter by 6.7 percentage points (column (1)). This point estimate is significantly different from 0 at the the 1 % level. When controlling for characteristics of the the mother, such as ethnicity, quarter of birth, birthplace or religious affiliation, the estimated effect drops in magnitude but is still significant at the 5 % level (see columns (2) to (4)). The result is stable at around -1.4 percentage points across these specifications. Therefore, it appears like an additional year of education tends to reduce a mother's propensity to cut her eldest daughter.

**Table 9:** Second-stage estimates of predicted education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| $\widehat{Education}$ | -0.067*** | -0.014** | -0.014** | -0.013** | -0.014** |
| | (0.007) | (0.006) | (0.006) | (0.006) | (0.006) |
| Age | | -0.088*** | -0.088*** | -0.090*** | -0.088*** |
| | | (0.023) | (0.023) | (0.023) | (0.022) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| *N* | 11638 | 11631 | 11631 | 11620 | 11612 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

It is difficult to say, however, how informative the second-stage estimates are. It might be so that it is due to the error is measuring educational attainment that the reported second-stage estimates are larger in magnitude than the corresponding OLS estimates reported in Table 3.

In Table 10, the second-stage estimates are presented for the measure $Education8_{ic}$. While the estimate is very large when no covariates are controlled for (column (1)), it is quite stable across the other specifications (columns (2) to (5)). It ranges from -8.9 to -7.6 percentage points, depending on what I control for. All the estimates are significantly different from 0 at at least the 10 % level. It appears like spending eight years in school tend to reduce a mother's propensity to cut her eldest daughter. As discussed earlier, it is difficult to say how informative the second-stage estimates are. The second-stage estimates are higher than the reported estimates from the OLS model (consult Table 4). It is likely that this is a consequence of the measurement errors.

Second-stage estimates for the measure $Atleast8_{ic}$ are presented in Table 11. In line with the observation before, the point estimate tends to stabilize as long as I control for ethnicity (from column (2) and on). The estimated effect is between -8.5 and -7.5 percentage points, in which all are significantly different from 0 at the 5 % level. This

**Table 10:** Second-stage estimates of predicted eight years of education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| $\widehat{Education8}$ | -0.472*** | -0.089** | -0.089** | -0.076* | -0.081** |
| | (0.048) | (0.042) | (0.042) | (0.041) | (0.040) |
| Age | | -0.101*** | -0.101*** | -0.101*** | -0.101*** |
| | | (0.023) | (0.023) | (0.022) | (0.022) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.000) | (0.000) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| *N* | 11640 | 11633 | 11633 | 11622 | 11614 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

suggests that getting eight or more years of education reduces a mother's propensity to cut the genitals of her eldest daughter. It is difficult to say how informative the second-stage estimates are. Based on the discussion in Section 4, I put more trust in the reduced-form estimates.

## 5.2 Reduced-form estimates

In Table 12 I present the results from the reduced-form model. These estimates show the effect the educational reform had on the decision to cut, holding a set of characteristics constant. The estimated effect of the education reform on cutting is negative and significantly different from 0 at at least the 10 % level across all specifications. This means that mothers affected by the reform to a lesser degree chose to cut the genitals of the eldest daughter. When not holding other characteristics constant, c.f. column (1), the effect of the reform on the decision to cut is -11.6 percentage points. This means that mothers affected by the reform are 11.6 percentage points less likely to cut their eldest daughter. The estimate is statistically significant at the 1 % level. Note that this accounts for most of the sample mean. Thus, this raw correlation does not appear to be very informative.

**Table 11:** Second-stage estimates of predicted at least eight years of education on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| $\widehat{Atleast8}$ | -0.444*** | -0.085** | -0.085** | -0.075** | -0.080** |
| | (0.046) | (0.037) | (0.037) | (0.038) | (0.038) |
| Age | | -0.095*** | -0.095*** | -0.100*** | -0.100*** |
| | | (0.022) | (0.022) | (0.022) | (0.022) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| *N* | 11638 | 11631 | 11631 | 11620 | 11612 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

When taking into account ethnicity, which is done in column (2), the estimated effect of the reform on the decision to cut is -0.021, one-sixth of the sample mean. This implies that mothers affected by the reform have a 2.1 percentage points lower chance of cutting their eldest daughter. The estimate is significant at the 5 % level. The result indicates that it is likely that the reform affected ethnic groups differently.

The face value of this estimate persists when controlling for other characteristics. In column (3), I also control for quarter of birth. The effect of the reform on the decision to cut is still -2.1 percentage points, and still significantly different from 0 at the 5 % level. When taking into account the birthplace of the respondent, as done in column (4), the estimate becomes -1.8 percentage points and the significance level changes to 10 %. In column (5) I additionally take into account the religious affiliation of the women.[11] The effect of the reform on cutting is now -1.9 percentage points and is significantly different from 0 at the 5 % level.

To sum up, the 1985 restructuring of the Kenya education system led to a decrease in in the prevalence of female genital cutting by around 2 percentage points, which accounts for one-sixth of the sample mean. Undertaking robustness checks in Subsection 5.3 and

[11] As noted earlier, this characteristic is not necessarily pre-treatment.

**Table 12:** Reduced-form estimates using OLS of the instrument on cutting.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| Instrument | -0.116*** | -0.021** | -0.021** | -0.018* | -0.019** |
| | (0.012) | (0.010) | (0.010) | (0.009) | (0.009) |
| Age | | -0.103*** | -0.103*** | -0.103*** | -0.101*** |
| | | (0.022) | (0.022) | (0.021) | (0.021) |
| Age2 | | 0.003*** | 0.003*** | 0.003*** | 0.003*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 11640 | 11633 | 11633 | 11622 | 11614 |
| $R^2$ | 0.025 | 0.383 | 0.383 | 0.385 | 0.387 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

placebo tests in Subsection 5.4, I argue that this effect is causal.

## 5.3 Robustness checks

The estimated effect of being affected by the 1985 education reform is only valid as an instrument if it captures the effect on the decision to perform female genital cutting solely through its effect on schooling. In this subsection I perform a range of robustness checks to argue that this probably is the case.

One may worry that there is a trend in women's likelihood to enroll in primary school which is not related to the reform itself. If this is true, the instrument is capturing an endogenous trend in the decision to enroll primary school. Utilizing data provided by Somerset (2007), which is described in Subsection 3.1, I perform a set of auxiliary regressions where I control for grade-one enrollment for each cohort the year in which they turned six years old.[12] In columns (1) and (2) of Table 13, results from the full model are

[12]For simplicity, I have assumed that pupils enrolled primary school at age six when conducting this test.

**Table 13:** Robustness checks of the reduced-form estimates.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Education | Cut | Education | Cut | Education | Cut |
| Instrument | 1.115*** | -0.026** | | | 1.535*** | -0.019** |
| | (0.227) | (0.010) | | | (0.184) | (0.009) |
| Annual instrument | | | 1.573*** | -0.020** | | |
| | | | (0.183) | (0.009) | | |
| Enrollment | 0.001** | 0.000 | | | | |
| | (0.000) | (0.000) | | | | |
| Age | 0.523 | -0.102*** | 0.451 | -0.100*** | 0.651* | -0.106*** |
| | (0.342) | (0.024) | (0.340) | (0.021) | (0.334) | (0.023) |
| Age2 | -0.014 | 0.003*** | -0.011 | 0.003*** | -0.018 * | 0.003*** |
| | (0.010) | (0.001) | (0.010) | (0.001) | (0.010) | (0.001) |
| Age3 | 0.000 | -0.000*** | 0.000 | -0.000*** | 0.000 | -0.000*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | Y | Y | Y | Y | Y | Y |
| *Quarter controls* | Y | Y | Y | Y | Y | Y |
| *Birthplace controls* | Y | Y | Y | Y | Y | Y |
| *Religion controls* | Y | Y | Y | Y | Y | Y |
| $N$ | 14031 | 10378 | 15400 | 11614 | 14717 | 11000 |
| $R^2$ | 0.293 | 0.396 | 0.301 | 0.387 | 0.297 | 0.387 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

presented. Column (1) presents the first-stage estimate. While this is somewhat smaller than what I reported before, it is still significantly different from 0. In column (2) the estimate from the reduced-form model is shown. As is evident, controlling for enrollment in primary school is not affecting the reform's impact on the decision to cut. The result is quite similar to the estimate reported in the previous subsection, although quantitatively larger. These findings indicate that fluctuations in primary school enrollment in the 1970's are not biasing my causal estimate.

In columns (3) and (4) of Table 13 I show that the results reported in Subsection 5.2 are insensitive to relying on an instrument specified at the birth year level rather than at the birth year and quarter level.[13] The first-stage, reported in column (3), is qualitatively similar to the one reported earlier and significant at the 1 % level. The point estimate for

[13]This instrument is constructed by calculating annual averages of the year and quarter values of the instrument described in Subsection 4.2.

the reduced-form model, reported in column (4), is quite similar to the estimated effect obtained before. In conclusion, the estimated effect of the reform tend to be insensitive relying on annual data when constructing the instrument. While not reported, these results are insensitive to controlling for enrollment.

The density of observations is smaller for the for older cohorts. In columns (5) and (6) of Table 13 I show that results are insensitive to restricting the sample to include only those born after 1955. The estimated effect of the reform on educational attainment is similar to before and statistically significant at the 1 % level. The estimated effect of the reform on the decision to cut yields a similar point estimate. This estimate is significantly different from 0 at the conventional level. While not reported, results are insensitive to controlling for enrollment as well as using the instrument specified for year of birth.

## 5.4 Placebo tests

Another worry for my causal narrative is that Kenya may have experienced a change of some kind over the same time period as I study, and that this change led to a decrease in the prevalence of female genital cutting. To assess this plausible objection to my claims, I run similar regressions as before for subsamples of women that most likely were not affected by the reform. Results are presented in Tables 14 and 15.

I look into two subsamples that one would not expect to be affected by the restructuring of the education system. The first group consists of those who undertook fewer than six years of education. Under the assumption that the demand for education was unchanged (see Chicoine (2012)), these cohorts were unaffected by the one-year increase in the length of the primary school. Results for the first set of placebo tests are presented in columns (1) to (5) of Table 14. Although the effect of the reform is significant and large in magnitude when not holding constant for relevant characteristics (see column (1)), this finding is not very robust. When taking into account relevant characteristics, such as ethnicity, quarter of birth, birthplace or religious affiliation, the estimated effect drops in magnitude and is no longer different form 0 at conventional levels (see columns (2) to (4)). This builds a strong case in arguing that the reform truly is exogenous.

The second group I look into consists of those who attended college. Because the years one needed to spend in school to obtain college degrees were 16 in both school systems (consult Figure 1), these pupils are likely to be unaffected by the education reform. Results from the placebo tests are presented in columns (1) to (5) in Table 15. The estimated effect of the reform on the cutting for this subsample is not significantly

**Table 14:** Placebo tests for the subsample with less than six years of education.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Cut | Cut | Cut | Cut | Cut |
| Instrument | -0.085*** | 0.018 | 0.018 | 0.022 | 0.014 |
| | (0.027) | (0.022) | (0.022) | (0.022) | (0.022) |
| Age | | -0.226*** | -0.226*** | -0.228*** | -0.225*** |
| | | (0.047) | (0.047) | (0.047) | (0.047) |
| Age2 | | 0.007*** | 0.007*** | 0.007*** | 0.007*** |
| | | (0.001) | (0.001) | (0.001) | (0.001) |
| Age3 | | -0.000*** | -0.000*** | -0.000*** | -0.000*** |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 3942 | 3941 | 3941 | 3937 | 3935 |
| $R^2$ | 0.008 | 0.418 | 0.418 | 0.419 | 0.422 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

different from 0 at any conventional level. That the reform did not affect this subsample builds a strong case in arguing that an unobserved trend in female genital cutting is not driving my results.

## 5.5 Heterogeneous effects

Having argued that my instrument is valid, I return to the reduced-form model discussed Subsection 5.2. What I have estimated is an intention-to-treat effect. I aim at complementing this analyses by looking into heterogeneity. The rationale for doing this is to say something about who may be those complying with the treatment. In other words, which respondents are likely to have reduced the propensity to cut their eldest daughter because they were affected by the reform? The other subsamples of interest are never-takers and always-takers.[14] Never-takers are those that would not reduce their propensity to cut irrespective of being affected by the reform. Always-takers, on the other hand, are those that

[14]Another interesting subsample is the defiers. These are respondents that increase their likelihood of cutting as a result of being exposed to the reform. It may be so that some respondents do this because increased schooling led to increased attrition. In my analysis, however, I assume that this is not the case.

**Table 15:** Placebo tests for the subsample with education at a higher level.

| | (1) Cut | (2) Cut | (3) Cut | (4) Cut | (5) Cut |
|---|---|---|---|---|---|
| Instrument | -0.046 | -0.031 | -0.031 | -0.020 | -0.010 |
| | (0.031) | (0.030) | (0.030) | (0.028) | (0.026) |
| Age | | -0.109 | -0.109 | -0.188 | -0.136 |
| | | (0.167) | (0.167) | (0.184) | (0.172) |
| Age2 | | 0.003 | 0.003 | 0.005 | 0.004 |
| | | (0.005) | (0.005) | (0.005) | (0.005) |
| Age3 | | -0.000 | -0.000 | -0.000 | -0.000 |
| | | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | Y | Y | Y | Y |
| *Quarter controls* | N | N | Y | Y | Y |
| *Birthplace controls* | N | N | N | Y | Y |
| *Religion controls* | N | N | N | N | Y |
| $N$ | 169 | 169 | 169 | 169 | 169 |
| $R^2$ | 0.015 | 0.241 | 0.241 | 0.274 | 0.335 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

would reduce their propensity to cut irrespective of being affected by the reform. Because neither of these subsamples are observable, my analysis below is necessarily suggestive.

Table 16 presents the reduced-form model estimated for the different categories of childhood place of residence. Across the different categories, I hold constant for the ethnicity, quarter of birth, and religious affiliation of the respondent. Note that the results are qualitatively similar if religious affiliation is not controlled for.[15] First-stage estimates are all positive and significant. I have limited my attention to the reduced-form estimates. It is only for respondents growing up at the countryside (see column (1)) that the estimated effect is significantly different from 0. The estimated effect is statistically significant at the 10 % level. For respondents growing up at the countryside, the reform, on average, led to a 2 percentage points decrease in the likelihood to cut the eldest daughter. This accounts for one-seventh of the subsample mean. For mothers growing up in a town (column (2)), abroad (column (3)), or in a city (column (4)), being exposed to the reform, on average, did not affect the prevalence of female genital cutting. It is difficult to conclude, but respondents growing up at the countryside are good candidates

[15]Results are available upon request.

**Table 16:** Heterogeneity for the reduced-form model based on childhood place of residence.

| | (1) Cut if Countryside | (2) Cut if Town | (3) Cut if Abroad | (4) Cut if City |
|---|---|---|---|---|
| Instrument | -0.020* | -0.004 | -0.082 | -0.027 |
| | (0.010) | (0.023) | (0.102) | (0.021) |
| Age | -0.103*** | -0.150*** | -0.230 | 0.016 |
| | (0.026) | (0.056) | (0.166) | (0.042) |
| Age2 | 0.003*** | 0.005*** | 0.006 | -0.000 |
| | (0.001) | (0.002) | (0.005) | (0.001) |
| Age3 | -0.000*** | -0.000*** | -0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | Y | Y | Y | Y |
| *Quarter controls* | Y | Y | Y | Y |
| *Birthplace controls* | N | N | N | N |
| *Religion controls* | Y | Y | Y | Y |
| $N$ | 9526 | 1010 | 161 | 917 |
| $R^2$ | 0.391 | 0.473 | 0.427 | 0.112 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

for being compliers. Note, however, that it is difficult to distinguish these from never-takers and always-takers. Therefore, I also look into other subsamples in order to say something meaningful.

I now study heterogeneity in light of religious affiliation. Although this characteristic may be post-treatment, I have chosen to discuss the findings. Results are presented in Table 17. Across the different categories, I hold constant for the ethnicity, quarter of birth, and the childhood place of residence of the respondent. The non-reported first-stage estimates are positive and significant for all except the Muslims. For Muslims, getting into primary school seems to be a bigger challenge than increasing the length of primary school. In Table 17 we observe that, on average, respondents affiliated with the largest religions are not affected. One may suggest that these are never-takers. For the subsample without any religious affiliation, the reform led to a 9.3 percentage points decrease in the likelihood of cutting the eldest daughter (column (3)). It is significantly different from 0 at the 5 % level. This point estimate is close to the subsample mean. These may be always-takers, although the pattern of female genital cutting declines neatly with the introduction of

**Table 17:** Heterogeneity for the reduced-form model based on religious affiliation.

| | (1) Cut if Muslim | (2) Cut if Catholic | (3) Cut if No religion | (4) Cut if Protestant | (5) Cut if Other |
|---|---|---|---|---|---|
| Instrument | 0.050 | -0.017 | -0.093** | -0.029*** | -0.001 |
| | (0.039) | (0.019) | (0.043) | (0.010) | (0.032) |
| Age | -0.287*** | -0.110*** | -0.023 | -0.066*** | -0.077 |
| | (0.091) | (0.042) | (0.110) | (0.024) | (0.185) |
| Age2 | 0.009*** | 0.004*** | 0.000 | 0.002*** | 0.002 |
| | (0.003) | (0.001) | (0.003) | (0.001) | (0.005) |
| Age3 | -0.000*** | -0.000*** | 0.000 | -0.000*** | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | Y | Y | Y | Y | Y |
| *Quarter controls* | Y | Y | Y | Y | Y |
| *Birthplace controls* | Y | Y | Y | Y | Y |
| *Religion controls* | N | N | N | N | N |
| $N$ | 1274 | 2754 | 298 | 7230 | 58 |
| $R^2$ | 0.518 | 0.299 | 0.366 | 0.295 | 0.596 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the reform. For the subsample of respondents that are Protestants, the reform led to a 2.9 percentage points decrease in the likelihood of cutting the eldest daughter (column (4)). It is significantly different from 0 at the 1 % level. The point estimate accounts for one-third of the subsample mean. These are candidates for being compliers. Catholics and those affiliated with other religions were not affected, on average (columns (2) and (5)). It appears like respondents that are Protestants or not affiliated with any religion are those that, on average, were affected by the reform. Those not affiliated with any religion are candidates for being always-takers.

In order to be more specific about who the compliers might be, I present the reduced-form estimates by the ethnic origin of the respondents. This is an important exercise, given that results reported in previous subsections tended stabilize when controlling for ethnicity. Estimates are presented in Table 18. Across the different categories, I hold constant for the quarter of birth, childhood place of residence, and religious affiliation of the respondent. Note however, that the results are qualitatively similar if religion is not

**Table 18:** Heterogeneity for the reduced-form model based on ethnicity.

| | (1) Cut if Somali | (2) Cut if Kisii | (3) Cut if Masai | (4) Cut if Other | (5) Cut if Meru and Embu | (6) Cut if Kalenjun | (7) Cut if Kamba | (8) Cut if Kikuyu | (9) Cut if Miji. and Swa. | (10) Cut if Luhya | (11) Cut if Luo |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Instrument | 0.033 | -0.176*** | -0.201 | 0.076 | -0.072** | -0.010 | 0.003 | -0.034*** | 0.014 | -0.005* | -0.008 |
| | (0.077) | (0.062) | (0.137) | (0.062) | (0.034) | (0.022) | (0.028) | (0.012) | (0.015) | (0.003) | (0.006) |
| Age | -0.160 | -0.085 | -0.508* | -0.272** | -0.035 | -0.129** | 0.013 | -0.039 | -0.029 | -0.027** | -0.018 |
| | (0.239) | (0.107) | (0.264) | (0.108) | (0.094) | (0.057) | (0.074) | (0.042) | (0.029) | (0.010) | (0.014) |
| Age2 | 0.007 | 0.005 | 0.016** | 0.008** | 0.001 | 0.004** | -0.000 | 0.001 | 0.001 | 0.001** | 0.001 |
| | (0.007) | (0.003) | (0.008) | (0.003) | (0.003) | (0.002) | (0.002) | (0.001) | (0.001) | (0.000) | (0.000) |
| Age3 | -0.000 | -0.000* | -0.000** | -0.000** | -0.000 | -0.000 | 0.000 | -0.000 | -0.000 | -0.000*** | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Ethnicity controls* | N | N | N | N | N | N | N | N | N | N | N |
| *Quarter controls* | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| *Birthplace controls* | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| *Religion controls* | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| $N$ | 600 | 736 | 179 | 731 | 716 | 1427 | 1096 | 2277 | 862 | 1737 | 1253 |
| $R^2$ | 0.285 | 0.397 | 0.524 | 0.174 | 0.082 | 0.160 | 0.038 | 0.048 | 0.016 | 0.009 | 0.008 |

Source: KDHS 1998, 2003, and 2008-2009.
Standard errors are robust and clustered at the birth cohort level and given in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

controlled for.[16] First-stage estimates are all positive and significant, except for Somali, Masai, and the group of other ethnicities. Most of Somali and Masai respondents have not obtained primary school certificates, according to my data. Therefore, the education reform was not effective for these subgroups. First-stage estimates are not reported.

For Somali in the north of Kenya, reported in column (1), the reduced-form estimate is not significantly different from 0 at conventional levels. As noted above, the reform did not predict an increase in years of schooling for this subsample. The lack of an estimated effect is therefore in line with my causal story. In order to compare with the earlier results, it is so that most Somali are Muslims. Given the above discussion, this suggests that these may be never-takers.

For Kisii in the south, reported in column (2), the reform affected the cutting behavior. The point estimate is -17.6 percentage points, accounting for three-tenths of the subsample mean and being significantly different from 0 at the 1 % level. The majority of these are Christian, in which most are Protestants. Although neighboring to Luo (reported in column (11)), they differ in terms of their sense of community, according to anthropological accounts (Allen et al., 2013). Given the prevalence of cutting, these may be classified as compliers. The decline in cutting follows the reform closely.

Masai, reported in column (3), has a similar story to that of Somali. The reform did not predict more schooling and cutting did not change. It is difficult to conclude whether these are never-takers or compliers. The finding is similar for the group of other ethnicities, reported in column (4). This subsample comprise a wide range of ethnicities, and therefore it is difficult to classify the group.[17]

The ethnicities Meru and Embu (results are reported in column (5)) are closely related to each others, as well as to Kikuyu (Allen et al., 2013). Meru and Embu are located in the center and the east of the country. The first-stage estimate is positive and significant. The estimate from the reduced-form model is -7.2 percentage points and significantly different from 0 at the 5 % level. This point estimate accounts for 65 % of the subsample mean. The majority of Meru and Embu are Protestants, while most others are Catholics. These are likely to be compliers.

For Kalenjun from the Rift Valley area, reported in column (6), the first-stage estimate is positive and significant. Many respondents focused on finishing primary school and were affected by the reform. Very few did not enroll primary school at all. The reduced-form estimate, on the other hand, is not significantly different from 0 at the conventional levels.

[16] Results are available upon request.

[17] The group of other ethnicities includes Borana, Kuria, Pokot, Samburu, Taita, and Tukana.

The majority is Protestants, while most of the others are Catholics. The subsample mean of cutting is about 10 %. I find these respondents difficult to classify. Kamba is reported in column (7). The first-stage estimate is positive and significant, while the reduced-form estimate is not statistically significant, meaning that their story is similar to that of Kalenjun. The ethnicity is based in the south and central parts of the country, and is related to Swahili (Allen et al., 2013). The prevalence subsample mean is 6 %. These are likely to be compliers or always-takers.

Kikuyu, reported in column (8), has a first-stage estimate that is statistically significant. In contrast with Kamba, in which the subsample mean is similar, the reduced-form estimate is negative and significant at the 1 % level. The point estimate of -3.4 percentage points accounts for seven-tenths of the subsample mean. A large majority of these respondents are Protestants. These may be compliers.

The ethnicities Mijikenda and Swahili (results are reported in column (9)) are closely related to each others, as well as Kamba (see column (7)). While the first-stage estimate is positive and significant, the reduced-form estimate is not different from 0 at conventional levels. The majority are Muslims, and they tend to live in the south. Given that the subsample mean is 15 %, the case for arguing that many of these are never-takers is strong.

Luhya is reported in column (10). The first-stage is positive and significant. The reduced-form estimate is -0.5 percentage points and statistically significant at the 10 % level. This point estimate accounts for the whole sample mean of 0.5 %. It is likely that these are always-takers. For Luo, reported in column (11), the non-reported first-stage estimate is positive and significant. The reduced form estimate, however, is not statistically significant. Similarly to Luhya, female genital cutting is not widely practiced in these communities. These may be always-takers.

## 5.6 Exploration of possible mechanisms

This far I have tried to argue that the 1985 restructuring of the education system led to a decrease in the likelihood that mothers cut the genitals of their eldest daughter. In the previous subsection I also looked into the segments of respondents that were most likely to have been affected by the reform. Having established a causal relationship, I now try to explore potential mechanisms behind this relationship. This discussion is closely related to economic theory.

Because I study data at the individual level, I am reluctant to say anything about dy-

**Table 19:** Exploration of gaps in age and years of schooling between the respondent and her husband.

| | Whole sample | | Placebo group 1 | | Placebo group 2 | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Age gap | Educ. gap | Age gap | Educ. gap | Age gap | Educ. gap |
| Instrument | -0.124 | -0.507*** | 0.616 | -0.082 | 0.734 | 0.473 |
| | (0.247) | (0.120) | (0.568) | (0.209) | (0.967) | (0.751) |
| Age | -0.409 | -0.598** | -0.872 | -0.789** | -0.619 | 6.705 |
| | (0.556) | (0.271) | (0.965) | (0.397) | (6.328) | (4.364) |
| Age2 | 0.012 | 0.015* | 0.029 | 0.021* | -0.007 | -0.187 |
| | (0.017) | (0.008) | (0.028) | (0.012) | (0.177) | (0.119) |
| Age3 | -0.000 | -0.000 | -0.000 | -0.000* | 0.000 | 0.002 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.002) | (0.001) |
| *Ethnicity controls* | Y | Y | Y | Y | Y | Y |
| *Quarter controls* | Y | Y | Y | Y | Y | Y |
| *Birthplace controls* | Y | Y | Y | Y | Y | Y |
| *Religion controls* | Y | Y | Y | Y | Y | Y |
| $N$ | 11547 | 13357 | 3681 | 4408 | 219 | 229 |
| $R^2$ | 0.073 | 0.023 | 0.058 | 0.085 | 0.144 | 0.110 |

Source: KDHS 1998, 2003, and 2008-2009.

Standard errors are robust and clustered at the birth cohort level and given in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

namics at the higher level, e.g., changing conventions. The persistence of conventions can be seen as a coordination problem in which information asymmetries uphold the practice. Further, because of the limited quality of the data it is difficult to say something meaningful about changes in attitudes and preferences as a result of the education reform.[18] Therefore, I limit my attention to mechanisms other than the ones related to information asymmetries and endogenous preferences.

I argue that an intra-household bargaining narrative is consistent with my findings, reported in the previous subsections. More specifically, the results reported in Table 19 suggest that the restructuring of the education system might have increased the position of female respondents relative to their husband. Results for the whole sample are reported in columns (1) and (2). I have controlled for the ethnicity, quarter of birth, birthplace and religious affiliations of the respondents. Note, however, that results are essentially the same if I do not control for religious affiliation. While the age gap between the respondent

[18]I have only data available on these issues for one of the three survey rounds. The data is plagued by low response rates. Therefore, I have not been able to use it.

and her husband, on average, is not different for those affected by the reform, the gap in years of schooling is. The education gap is reduced with 0.5 years, compared to the sample mean of 1.2 years. The point estimate is significantly different from 0 at the 1 % level. Columns (3) and (4) report results for the subsample of women with less than six years of education. Neither the age gap nor the education gap between the respondent and her husband are, on average, affected by the restructuring of the education system. The same is true for those with education at a higher level, as reported in columns (5) and (6). The results are essentially the same across the placebo groups when not controlling for religious affiliation. To sum up, it appears like the reform did not change the gap in age between females and their husbands, while it reduced the gap in schooling with 40 %, on average.[19]

The reduction in the education gap is likely to have increased the bargaining power of the female respondents relative to that of their husbands. That I find similar patterns when it comes to chances of being tested for HIV as well as the use of modern contraception, builds a stronger case for such an argument. Therefore, the education reform is likely to have led to women becoming more empowered. This is relevant when it comes to female genital cutting because couples tend to disagree on the practice (see, e.g., UNICEF (2013) for some descriptive statistics).

Under the assumption that women are less supportive of female genital cutting than men, the probability that their eldest daughter is cut should decrease as the mother has a larger say in the household. I am not able to compare attitudes of the respondents and their husbands since I do not possess good data on this aspect. Note, however, that the KDHS 1998 data indicates that female respondents have low support for the practice. Anecdotal evidence from Kenya suggests that female are less in favor of continuing the practice than their male counterparts (see, e.g., Ahlberg et al. (2000), Shell-Duncan and Hernlund (2000), and Simister (2010)).

If the above statements are true, changes in information asymmetries and preferences have not alone facilitated to the decline of female genital cutting observed in Kenya. Note, however, that findings in this subsection are only suggestive. I am not trying to argue that the information asymmetries have not been mitigated or that preferences have not adapted as a result of the 1985 reform.

Because the causal effect is quite heterogeneous across ethnic groups, as argued in the last subsection, it is plausible that small groups can stop undertaking female genital cutting without generating societal change. Since marriage markets may be overlapping

[19]The sample mean for age gap is 7.5 years.

(see, e.g., Agorash and Childs (2006), Iyer and Weeks (2009), and Luke and Minshi (2006)) and different subpopulations attach varying degrees of importance to the practice, more than one tipping point may exist (Hayford, 2005). This story is partially consistent with the theoretical arguments by Mackie (1996), in the sense that intra-household bargaining is relevant to the 'process of gathering the critical mass of support that, according to convention theory, is necessary to end female genital cutting' (Hayford, 2005, p. 136).

Thus, that my results are consistent with an intra-household bargaining narrative does not undermine the role of convention theory. Whether my findings are also consistent with modernization theory depends on the role of education. Hayford (2005) finds evidence that the effect of education in Kenya is status-based rather than content-based. This is consistent with feminist theory rather than modernization theory. I have not been able to investigate this claim.

# 6 Conclusion

Female genital cutting is a serious public health issue (WHO, 2012). While more than 100 million girls and women living today have undergone genital cutting, there is not much conclusive evidence about the practice. Although a range of theories trying to assess conditions for the decline of female genital cutting have been developed over the last decades, there is not much systematic knowledge about the relative merits of competing explanations.

A recent report by UNICEF (2013) offers a resolution by pushing one story for why female genital cutting persists. While arguing that the practice is upheld by conventions, they fail to recognize alternative and complementary theories. This report is currently guiding actions undertaken by the policy community. It is in the light of this development, that I chosen to undertake a robust analysis of female genital cutting.

I study the effect of educating mothers on the prevalence of genital cutting amongst their eldest daughters. To mitigate the omitted variable bias, I rely on an instrumental variable strategy. Inspired by Chicoine (2012), I utilize an exogenous change in the years spent in the primary school as a result of the 1985 restructuring of the the education system in Kenya. More specifically, I estimate the likelihood that a given cohort was affected by the restructuring of the education system. This allows me to make inference about a sample of Kenyan women born between 1950 and 1980.

I verify that the education reform is a strong predictor years of schooling. According

to the two-stage least-squares results, receiving an additional year of schooling led to a 1.4 percentage points decrease in the likelihood that the eldest daughter of respondents complying with the reform was cut. This accounts for 11 % of the sample mean. Because educational attainments are measured with some noise, I focus on the intention-to-treat effect by estimating the reduced-form model. On average, the reform led to a 2 percentage points decrease in the likelihood that the eldest daughter the respondents was cut. This compares to 16 % of the sample mean. Undertaking robustness checks and placebo tests, I argue that this effect is causal.

Linking my findings to economic theory, I investigate potential mechanisms for the causal relationship. I find that my results are consistent with an intra-household bargaining narrative (see, e.g, Becker (1981)). The education reform increased the position of the female respondents relative to their husbands. This is likely to have increased their bargaining power. Given that Kenyan women to a larger extent than men prefer female genital cutting to be abolished, the prevalence is likely to decline. I stress, however, that this argument is highly suggestive. If the above statements are true, changes in information asymmetries and preferences have not alone facilitated the decline of female genital cutting observed in Kenya. This differs from arguments offered by UNICEF (2013). The report fails to recognize intra-household dynamics.

Because the intention-to-treat effect is quite heterogeneous across ethnic groups, the scope of the intra-household bargaining narrative might be consistent with Hayford (2005). Due to large heterogeneity across space, small groups can stop undertaking female genital cutting without generating societal change. Since marriage markets may be overlapping and different subpopulations attach varying degrees of importance to the practice, more than one tipping point may exist. Thus, intra-household bargaining is relevant to the 'process of gathering the critical mass of support that, according to convention theory, is necessary to end female genital cutting' (Hayford, 2005, p. 136).

A potential policy implication of my study is to increase the focus on empowering female members of the household. Examples of such broad programs are to expand education as well as promoting property rights for female. There is, however, a need for further research on these issues. The literature on female genital cutting is still at an early stage. Many of my findings are only of a suggestive character. It may also be a worry that I was not able to account for drop-outs when constructing the instrument. Lastly, it may be so that the sample of Kenyan women differs from that of Kenya today and from that of other countries where female genital cutting persists.

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