

Women's Inheritance Rights and Bargaining Power: Evidence from Kenya*

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Abstract

This paper investigates the human capital effects of a legal reform granting Kenyan women equal inheritance rights. I employ a difference-in-differences strategy, exploiting variation in pre-reform inheritance rights across religious groups. I find that women exposed to the reform are more educated, less likely to undergo genital mutilation, more likely to receive prenatal care, and that they delay marriage and childbearing. They also tend to participate more in family decisions, suggesting improved bargaining power as the main channel. These findings suggest that legal recognition of women's inheritance rights can be beneficial for women even in contexts of poor enforcement.

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1 Introduction

In numerous countries across the developing world, particularly in Sub-Saharan Africa, gender discrimination takes the form of unequal inheritance and property rights. As inheritance is one of the main ways for women to acquire and control property, women’s legal inability to inherit property can significantly undermine their economic security and independence, as well as their access to economic opportunities (World Bank, 2011, 2012). Among initiatives aimed at remedying such discrimination, development practitioners and international organizations have been advocating legal reforms establishing equal *de jure* rights in matters of inheritance and family law. This view is illustrated in the 2012 World Bank Gender Equality and Development Report, according to which “the most promising policies to increase women’s voice in households center on reforming the legal framework (...): land laws and aspects of family law that govern marriage, divorce, and disposal of property are particularly important” (World Bank, 2012). Legal reforms are further claimed to have the potential to “improve economic outcomes” and “strengthen women’s economic empowerment” (World Bank, 2011). Quantitative evidence on the effects of reforms of this type remains, however, scant, particularly in Sub-Saharan Africa.¹ *A priori*, it is not obvious that such policy initiatives benefit women: first, legal reform alone may do little to change women’s *de facto* rights, particularly in contexts where legal enforcement is poor and social norms strongly oppose women holding property (UN-HABITAT, 2006; Human Rights Watch, 2003; USAID, 2003). Second, if women are granted improved property rights on physical assets, other household members may respond by engaging in compensatory behavior, putting women in a disadvantage with respect to other margins. For instance, Quisumbing et al. (2003) as well as Rosenblum (2015) find evidence that parents substitute land inheritance and human capital investments, the two major forms of

¹As discussed below, existing evidence on gender-neutral inheritance rules is mostly based on the Hindu Succession Act in India.

intergenerational wealth transfer in developing countries.

This paper studies the human capital effects of a statutory law reform granting Kenyan women equal inheritance rights, exploiting variation in pre-reform inheritance rights across religious groups. Before the 1981 Law of Succession Act, inheritance in Kenya was determined by the customary law of the ethnic group of the deceased, and, in the case of Muslims, by Koranic law. The customary law of virtually all ethnic groups in Kenya denies women any right of inheritance, whereas Islamic law entitles women to half of the inheritance share that goes to each of their brothers. The 1981 reform made inheritance a matter of statutory law, and formally established equal inheritance rights for men and women, regardless of religious affiliation. In 1990 an exemption was established for Muslims, who were allowed to revert to Koranic succession law. I exploit the timing of the 1981 reform and subsequent 1990 amendment, as well as cross-sectional variation in religious affiliation, in order to estimate the causal impact of the reform on a variety of outcomes related to human capital and household decision making, in a difference-in-differences framework. An interesting feature of this setting is that the reform was amended for one of the two groups, creating three distinct regimes throughout the period of study. This allows me to estimate two effects: the impact of the “full reform” - increasing women’s inheritance share relative to men’s from 0 to 1 - and the impact of the “incremental reform” - increasing this share from 0.5 to 1.

Drawing upon a rich set of outcomes from the Kenyan Demographic and Health Surveys, I find significant improvements along several dimensions, particularly in women’s education and health. Women exposed to the reform are more educated, both in absolute terms and relative to males. Switching from a regime with no inheritance rights to equal inheritance rights (i.e. the “full” reform) is associated with a 19 percentage point increase in primary school completion rates for females, and with a 14 percentage point decrease in the same outcome for males. Women exposed to the “full” reform are also 20 percentage points less

likely to undergo genital mutilation, and 7 percentage points more likely to receive medical assistance during pregnancy and childbirth. Moreover, they tend to delay marriage and childbearing.

These findings are compatible with both an investment and a bargaining power mechanism: on the one hand, as women have the ability to inherit physical capital, parents might decide to invest more in their human capital, if they view human and physical capital as complementary. On the other hand, the ability to inherit improves women's bargaining power and shifts human capital investment choices towards their preferences. This latter interpretation is supported by the finding that female genital mutilation rates are reduced for exposed women, since this particular practice does not have any obvious complementarities with the inheritance of physical assets. While I cannot fully disentangle the investment and bargaining power mechanism, I attempt to examine the latter channel explicitly by considering more direct proxies for women's household bargaining power, based on survey questions on decision making and attitudes. I find suggestive evidence that the reform makes women more likely to participate in family decisions, supporting the interpretation that women's bargaining power is indeed enhanced.

My identification strategy is complemented by several additional checks. When available, I exploit information on number of siblings as a source of treatment heterogeneity, showing that the effects of the reform are less pronounced when a woman has a large number of siblings - and hence a smaller potential inheritance. I also estimate the impacts of "placebo" reforms on non-exposed cohorts, finding zero effects. Finally, I show that the impacts of the reform tend to occur across most ethnic groups.

This work relates to two strands of the literature: that on intra-household decision making and that on intergenerational transfers. In a non-unitary household, the allocation of resources between spouses will affect intra-household bargaining and associated socio-economic outcomes. Women's ability to control resources has been shown to translate

into larger investments in children’s education, health and nutrition in a variety of contexts (Duflo, 2003; Qian, 2008; Luke and Munshi, 2011).²

The importance of family law for household bargaining has been emphasized by Chiappori et al. (2002), who view the intra-household distribution of power as affected by outside opportunities, including legislation on the assignment of property rights in case of divorce. A number of empirical papers have examined the impact of family law in developing contexts. Ambrus et al. (2010) study the link between the value of dowry and prenuptial agreements and changes in family laws concerning polygamy and divorce. Carranza (2014) studies how changes in Islamic family and inheritance law in Indonesia have affected fertility behavior and son preference.

Several recent papers have considered specifically gender-progressive changes in inheritance laws, by focusing on the Indian Hindu Succession Act. Most papers in this literature exploit variation in the timing of the reform across states in conjunction with the timing of the death of a woman’s male family members.³ This reform has been found to be associated with an increase in female education (Goyal et al., 2013; Roy, 2015), increased autonomy and labor supply (Heath and Tan, 2015), and greater bargaining power (Mookerjee, 2015). However, other studies find that women were made worse off under other dimensions: Anderson and Genicot (2015) show that the reform led to more suicides and wife beating, and interpret this as resulting from greater conflict over property within the household; Rosenblum (2015) finds evidence of higher female mortality, as parents substitute investments in human capital and land bequests. Overall, it is unclear whether women benefited in net terms from the reform and how this would generalize to other contexts.

²A number of papers focus specifically on women’s physical asset ownership, documenting how this correlates with better health and education outcomes for their children (Katz and Chamorro, 2003; Quisumbing and Maluccio, 2003). In the context of urban China, Wang (2014) shows that transferring ownership rights to women leads to less consumption of male goods in the household.

³Although the Hindu Succession Act affected Muslims and non-Muslims differently, much like the Kenyan Law of Succession Act examined in this paper, the identification strategy in the papers discussed below is mostly based on variation within Hindus.

Inter-generational transfers have been viewed mostly through the lens of the wealth model (Becker and Tomes, 1979) or the strategic bequest model (Bernheim et al., 1985). In the context of developing countries, a number of empirical papers have examined the interaction between traditional kinship systems and inheritance rules (Goetghebuer and Platteau, 2010; La Ferrara, 2007; Mobarak et al. 2013; Platteau and Baland, 2001). Employing an empirical strategy similar to mine, La Ferrara and Milazzo (2017) examine the strategic responses of matrilineal and patrilineal ethnic groups to an amendment to Ghana's Intestate Succession law, finding that parents substitute bequests with education investments.⁴

The rest of the paper is organized as follows. Section 2 describes the 1981 Kenyan Law of Succession and subsequent amendment, and provides additional background information on the Kenyan context. Section 3 outlines a conceptual framework for interpreting the effects of the reform under study. In sections 4 and 5 I present my data sources and empirical strategy. In section 6 I discuss my results. Section 7 concludes.

2 Background

Kenya is a fractionalized country in which ethnic and religious cleavages are salient. According to the 2009 Census, Kenya has a population of 38 million, subdivided in as many as 133 ethnic affiliations, according to the Census disaggregation. The 2009 Census reports that 83% of Kenya's inhabitants are Christians - of which 23% Catholics and 57% Protestants - and 11% Muslims. The remaining 6% is divided among traditional religions, no religion and Hinduism (Kenya National Bureau of Statistics, 2010). These figures have been widely disputed by the Muslim community, who has claimed to be selectively

⁴A similar finding is that of Quisumbing et al. (2001) and Quisumbing and Otsuka (2001), who study the effects of the evolution of land tenure institutions and matrilineal inheritance practices on agricultural and schooling investments in Ghana and Indonesia.

under-reported by the government. A more plausible figure for the Muslim population has been suggested to lie between 20 and 30% (The Daily Nation, 2010). Kenya's Muslims are not a homogeneous group, as they comprise converts from different ethnic groupings, among which notably Somalis and some other nomadic groups, Arabs and people of mixed Arab-African descent. Most Muslims live in the Coastal Province, where their sense of common identity is strongest (Oded, 2000).

Sub-Saharan Africa represents a unique setting for studying women's property rights, as land and family rights are governed by multiple and overlapping legal domains. In Kenya, property rights are defined by a complex interplay of customary law, statutory law and Islamic law. The 1969 Constitution, which is the reference one for the time period considered in this study, embraced legal pluralism recognizing the application of customary law and Islamic law in specific instances. Section 82(4) stated that the customary law of an individual's particular tribe could to be applied in cases of "adoption, marriage, divorce, burial, devolution of property on death or other matters of personal law", an exception with the statutory principle of non-discrimination (Cooper, 2011). In those matters listed under Section 82(4), Islamic law has been applied to Muslims and enforced by Kadhi Courts.⁵ Throughout the history of post-colonial Kenya, until the recent constitutional review process, a tension has persisted between the Muslim community, who sought to reinforce and expand the role of Kadhi courts, and Christian leaders, claiming that Islam should not be afforded special rights (Oded, 2000; Cooper, 2011).

Before the 1981 Law of Succession Act became operational, there were four separate systems of inheritance for Africans, Europeans, Muslims and Hindus. Since the 1897 Order-in-Council, African customary law in matters of succession was to apply to Africans, as long

⁵When the Sultan of Zanzibar allowed the British to administer the coastal province of Kenya as a protectorate in 1895, the British vowed to respect the existing judicial system, including Kadhi Courts. Upon independence in 1963 the governments of Kenya and Zanzibar entered an agreement that guaranteed the preservation of Kadhi Courts in exchange for annexion of Muslim territories to independent Kenya. In compliance with such agreement, the existence of Kadhi courts was established in the Constitution upon independence and the Kadhi Courts Act was passed in 1967.

as it was not “repugnant to justice or morality”, a judgment which courts have typically been unable to make. In 1961, the African Wills Ordinance was passed to enable Africans to make written wills, while intestate succession continued being governed by the respective customary law of the deceased. The 1897 Native Courts Regulations Ordinance proclaimed that the law of succession for Muslims was the law contained in the Quran. This continued to apply until independence when the government reaffirmed the position of the Muslims as part of a constitutional bargain, in order to counter their threat to separate from the rest of Kenya. The government pledged that, under the new constitution, Muslims would be allowed to continue applying Islamic personal law. This guarantee was backed by Section 66 of the Constitution, that established that Kadhi courts would decide matters of personal law for Muslims. Finally, the scant European and Hindu population were ruled by the British Indian Succession Act (1865) and by Hindu customary law, respectively.

In 1967, a commission appointed by the President began looking into marriage, divorce and inheritance law. A Report on the Law of Succession in Kenya was issued in 1968, recommending a uniform code of inheritance. A succession bill eventually passed in 1972 as the Law of Succession Act (Cap 160), but only came into force in 1981. The process of drafting and approving the bill was lengthy and highly contentious (Oded, 2000) for both political and substantial reasons. First, depriving local authorities and courts of legal competence in matters of inheritance was perceived as a threat to the independence of individual ethnic groups, thus altering the already precarious political equilibrium in a highly fractionalized country. The most common reason cited in opposition to the reform was the fear that daughters would be allowed to “inherit too much land”, which may enable their husbands, potentially from other clans, to control the traditional land of the wife’s family. This argument has recently been made again in the occasion of the debate on the new constitution (Cooper, 2011). The 1967 Report on Marriage was ignored, and to date the Married Women’s Property Act of 1882, a remnant of British colonial rule, remains the

only statute to govern married women's right to property acquired during a marriage, applying to all Kenyan marriages.

The Law of Succession Act, operational since July 1st 1981, was passed with the intention of merging and consolidating all the four systems of inheritance law into one uniform statute, applicable to all Kenyans. The Law of Succession Act outlines a Western-style type of succession based on bilateral descent, establishing equal inheritance rights for female and male children, regardless of whether married or unmarried, on their parent's property (Section 38). It is applied automatically in case of intestate succession or by the court, in case there is a will but not reasonable support for any dependents. Most people in Kenya die intestate (Mutongi, 2007).

If there is one surviving spouse and a child or children, the spouse is entitled to an absolute interest in the deceased's personal and household effects, and a life interest in the rest of the estate (e.g. land or businesses), although this cannot be disposed of without a court's permission (Sections 35 and 36). The latter provision was meant to protect widows from eviction or property grabbing. Although non-discriminatory in the treatment of the children of the deceased, there are still the vestiges of discriminatory customs in the provisions concerning spouses: when the surviving spouse is a woman, her interest in the property is invalidated if she remarries, whereas a surviving husband maintains his interest also upon remarriage. Children inherit the estate when a surviving spouse dies and, in a woman's case, remarries. If the deceased did not have a spouse or offspring, the estate is assigned first to the father, then to the mother, in case the father is deceased. If the parents are both deceased, it is assigned to the brothers and sisters if there are any, then to their children. In cases of polygamous unions, the estate is divided among the households according to the number of children in each.

Finally, Section 32 exempts from intestacy provisions of the Act "agricultural land, crops on such land and livestock" in ten specific districts specified by gazette notice: Marsabit,

Narok, Tana River, Samburu, West Pokot, Turkana, Isiolo, Mandera, Wajir and Kajiado. According to Section 3(1) “agricultural land” means land used for agricultural purposes which is not within a municipality or a township or a market, but does not include land registered under the provisions of any written law (UN-HABITAT, 2002). These so-called “gazetted” districts (henceforth: exempt districts) comprise the semi-desertic part of the country, scantily populated and inhabited by nomadic and semi-nomadic pastoral communities. Exempt districts encompass roughly 60% of the territory of Kenya but include only about 15% of the total population according to the 2009 Census (Kenya National Bureau of Statistics, 2010). The reason for exemption is that these are areas where land was owned communally, and as such difficult to attribute to individuals (UN-HABITAT, 2002).

Between 1981 and 1990, there was intense agitation by the Muslims who regarded the passing of the Law of Succession Act as a repudiation of the assurance given at independence. This debate culminated in one of the moments of maximum tension between the Muslim and Christian community in the post-colonial history of Kenya. The Kenyan Muslim community protested through newspaper editorials, petitions and heated public demonstrations in Mombasa (Oded, 2000). The government gave in to the pressure mounted by the Muslims in 1990, as it was keen to have their support in view of the transition to multi-partyism. Section 2 of the Succession Act was ultimately amended by Statute Law (Misc. Amendment) Act No. 2 of 1990 to specifically exclude application to Muslims. The Amendment disappplied the Act to persons who at the time of their death were Muslims, and the Kadhi Courts regained jurisdiction to determine questions relating to Muslim succession (Kenya Law Resource Center, 2011).

Islamic inheritance is clearly pinned down by the Quran. A widow receives 1/4 of her husband’s estate; women in polygamous marriages receive 1/8 if they are childless. What is left is divided among sons and daughters in such a way that sons receive twice as much as

daughters of their father's property. Even if there is no obligation to provide for dependents, only 1/3 of the Muslim's estate can be disposed of by will; at least 2/3 should be dealt with according to Koranic principles i.e. with fixed shares for specific heirs (UN-HABITAT, 2005; Kenya Law Resource Center, 2011).

A comprehensive source for the customary law of Kenya's various ethnic groups is the Restatement of African Law (Cotran, 1968). Virtually all ethnic groups covered do not allow women to inherit land from their parents nor their deceased husbands. The vast majority of Kenyan ethnic groups are patrilineal.

That of inheritance has been perceived as a sensitive and contentious issue from Kenyan independence until the recent constitutional review, and the debate on women's inheritance rights has received over the years considerable attention. For instance, in 2008 the Kenya Law Reform Commission issued a memorandum to civil society organizations to invite feedback on the existing provisions of the Succession Act. This suggests that knowledge of the law is reasonably widespread.

Unfortunately, there are no official or systematic reports on the enforcement of the Succession Law nor quantitative evidence on the evolution of women asset ownership following the reform. According to UN-HABITAT (2005) "while in the majority of cases, the rights enjoyed by women under this Act have been upheld, some incorrect interpretations have also been made" and "courts have on occasion ruled to disinherit married daughters". An ambiguity arises from the fact that legal pluralism formally persisted during the period of analysis in the 1969 Constitution's Section 82(4), which recognized customary law to be applicable in matters of personal law. While no systematic data exists on actual asset ownership by Kenyan women before and after the reform, the qualitative human rights literature reports enforcement problems and emphasizes how local custom strongly opposes women's inheritance (Kameri Mbote, 1995; Cooper, 2011).

3 Conceptual Framework

In this paper I focus on the human capital consequences of allowing women to inherit parental property.⁶ There are primarily two channels through which such a legal change can impact investments in human capital: a bargaining power channel and an “optimal bequest” or investment channel.

First, allowing women to inherit represents a positive shock to wives’ potential asset ownership, that affects the intra-household bargaining process. As property rights, especially on land, are intimately related to an individual’s ability to fulfill subsistence needs outside the family, in the context of a non-unitary household inheritance rights constitute an example of those “distribution factors” (Chiappori et al., 2002) or “extra-marital environmental parameters... that shift the threat point” but that, at least in the short run and to first order, “do not affect prices and non-wage income faced by married individuals.” (McElroy, 1990). Human capital investment choices are affected insofar as the relative bargaining weight of wives increases following the reform. It is worth emphasizing that this bargaining power hypothesis does not rely on women actually realizing their inheritance rights following the reform, but merely on women having the option to claim such rights in a court, based on a codified law. I view inheritance rules as Chiappori et al. (2002) view divorce laws: as “distribution factors that can influence the intra-household balance of power ... even when the marriage does not actually dissolve”. By the same line of reasoning, it is possible to detect a bargaining power effect of inheritance rights and yet observe no realized inheritance in equilibrium.⁷

⁶The Law of Succession also included provisions concerning the ability of widows to inherit from their deceased husbands, but I do not focus on this aspect. First, it is not clear that these provisions should affect the bargaining power of the wife while the husband is still alive. Moreover, these norms should not alter significantly the terms of parents’ bequest decision problem since the assets inherited by the widow will eventually pass onto the children.

⁷In the case of the Indian Hindu Succession Law, Roy (2015) finds that women do not inherit more land following gender-progressive inheritance reform. However, both Roy (2015) and Goyal et al. (2013) find large positive effects on the education of girls.

Holding constant the relative bargaining weights of spouses, the provision of equal inheritance shares for sons and daughters also affects the optimal bequest problem faced by parents. In the context of a wealth model of transfers due to Becker (1974) and Becker and Tomes (1979), altruistic parents maximize a collective utility function, which includes their children's future incomes as well as their own consumption. The income-generating process of children depends on the stock of human capital (health and education) and physical capital (assets) inherited, and could be different for sons and daughters. The model predicts that parents will choose the optimal mix of human and physical capital to bequeath to sons and daughters, given their relative comparative advantages in income-generating activities. The inheritance reform adds an additional constraint to this problem, by introducing a lower bound on the amount of physical capital that should be bequeathed to daughters. The optimal amount of human capital bequeathed to sons and daughters will change, in a direction which depends on whether human and physical capital are complements or substitutes in the income-generating process. For example, human capital in the form of education could be a complement for physical capital in the form of a family business, if more education increases the returns to running such business. Human capital in the form of health and nutrition could be a complement to physical capital in the form of family land, if healthier farmers reap higher returns from agricultural land. In both cases, forcing parents to increase the amount of physical capital bequeathed to daughters would also make them increase the amount of human capital invested in them. Alternatively, human and physical capital could be substitutes. This would yield the opposite prediction: as parents are forced to bequeath more assets to daughters, they substitute human capital for physical capital and disinvest in their daughters' education and/or health.

As this discussion highlights, the effects of improved inheritance rights on human capital are *a priori* ambiguous. Education, for instance, could be affected by the inheritance reform in at least three ways. The first channel is mothers' bargaining power: as mothers

have a greater bargaining weight, intra-household decisions concerning human capital investments will reflect to a larger extent the preferences of women. Since it is well documented that these preferences tend to be tilted towards the well being of children, and especially girls, we should expect outcomes such as health and education to unambiguously improve for girls and possibly boys as well. The second channel is the complementarity of education and physical assets: if education increases the returns to physical capital for daughters, once parents are forced to assign to daughters a larger share of physical capital they will also want to provide daughters with more education. Conversely, as boys receive a smaller share of assets, their education should decrease. A third channel is substitution between human and physical capital: parents might decide to invest less in the human capital of girls and more in that of boys, to compensate the fact that law now forces them to bequeath the same amount of physical capital to both.⁸ How human capital outcomes respond to changes in inheritance rules is thus ultimately an empirical question, which I attempt to address in the next sections.

4 Data sources

All the data used in this study is drawn from the different rounds of Kenyan Demographic and Health Surveys (DHS): 1989 (DHS-I), 1993 (DHS-II), 1998 (DHS-III), 2003 (DHS-IV), and 2008-2009 (DHS-V). DHS are household surveys with large sample sizes (usually between 5,000 and 10,000 households) that provide data for a wide range of monitoring

⁸*A priori* there is another potential mechanism, besides bargaining and bequests, through which the reform affects human capital: one mediated by marriage markets. All else being equal, the ability to inherit physical assets makes a woman a more attractive bride, which would lead her to change her pre-marital investments. For instance, relative to a woman who does not inherit, she may afford investing more in education and postponing marriage. While plausible at the individual level, this mechanism is unlikely to play a major role in the case of an inheritance reform that affects all women of a certain religion and cohort. Given that marriage occurs within religious groups, in this context “treated” women compete on marriage markets primarily with women who are also “treated”. Empirically, the marriage market implications of the reform are difficult to explore with my identification strategy, as I rely on comparisons across religious groups, that do not inter-marry. For these reasons, I choose not to focus on the marriage market channel.

and impact evaluation indicators in the areas of health and demography, with a specific focus on female household members. The core DHS questionnaire is administered to all women aged 15 to 49 in each selected household and contains detailed questions on reproductive and maternal health as well as on the health of the respondent's youngest children. Basic demographic data and information on educational attainment is collected for all other household members as well. In each round, a small sub-sample of households is selected for an additional questionnaire to be administered to males 15-49. Waves IV and V also include a module on gender, with specific questions about household decision making, whereas wave IV includes an additional siblings questionnaire. While waves IV and V are nationally representative, earlier waves exclude the North Eastern province - a semi-desertic area scantily inhabited by nomadic populations, predominantly of Muslim religion. For consistency as well as to avoid potential confounding effects, I exclude households from the North Eastern province from my analysis.⁹

The advantages of DHS data are manifold. First, the relatively large sample size allows me to obtain fairly precise estimates even if the variation I rely on comes from a minority in the population. Second, the high degree of comparability across waves mitigates measurement error problems associated with pooling together different waves. Finally, DHS surveys are among the very few surveys administered in Kenya which report detailed data on religious and ethnic affiliation, information which the National Statistical Office is typically not willing to disclose, given its political sensitivity.

One of the limitations of the data is that all waves are administered post-reform, with the exception of the 1989 wave, which is administered shortly before the 1990 amendment. As a result, I focus on cumulative or past outcomes - such as the accumulated stock of education or the timing of fertility onset - rather than on outcomes measured at the time of

⁹A natural concern is that more recent Muslim cohorts would not be comparable to earlier ones, as they include nomadic and arguably more traditional households. My results are qualitatively unchanged if the North Eastern province is included (results available upon request).

the survey - such as current health measures.¹⁰ Furthermore, I will typically not be able to include any pre-reform household characteristics as controls.

5 Empirical Strategy

My identification strategy exploits within-country variation in pre-reform customary inheritance law across different religious groups. Following Duflo (2001), Bleakley (2010) and, specifically in the case of inheritance, La Ferrara and Milazzo (2017), my benchmark specification relies on a difference-in-differences between cohorts exposed and not exposed to the reform, across Muslims and non-Muslims. The identifying assumption is that, absent the change in inheritance rules, the outcomes of interest would have evolved over time following the same time trend across religious groups. Such a strategy is thus robust to differences in time-invariant characteristics of different religious and ethnic groups.¹¹

The reform under study includes two subsequent legal changes: the 1981 Law of Succession, granting all women a share of parental inheritance equal to that of their brothers, and the 1990 Amendment, exempting Muslims from the rule. This generates three different inheritance regimes, as summarized by Table 1.

[Insert Table 1]

In the pre-1981 regime, non-Muslim women inherit a 0 share of assets, while Muslim women inherit half the share which is entitled to their brothers. In the “post 1” regime, between 1981 and 1990, the Law of Succession applies to both Muslims and non-Muslims

¹⁰One could in principle compare short-term outcomes from the 1989 survey with those in subsequent waves and attempt to estimate the impacts of the reform’s amendment. However, it would not be possible to control for pre-trends having only one cross-section of pre-reform observations. Moreover, given that the 1989 survey was administered shortly before the reform, there could be anticipatory behavior.

¹¹It should be noted that a woman whose parents have died before the reform will not experience any increased bargaining power by changes in inheritance rules, as her potential inheritance has been already realized. Unfortunately, in my data I do not have any information on the timing of parents’ death and I will necessarily consider as “treated” also women who are not affected by the reform given that their parents have already died. This should attenuate my estimates, but not invalidate my identification strategy, to the extent that the timing of parents’ death is not systematically different for Muslims and non-Muslims.

alike and grants women the same inheritance share as their brothers. In the “post 2” regime, after 1990, the Law of Succession continues to apply to non-Muslims, for whom the same share is granted to sons and daughters, but no longer applies to Muslims, who revert to the pre-1981 rule that grants daughters half the share entitled to their brothers. My empirical specification thus includes two different “post” periods: one for the regime in place between 1981 and 1990 (“post 1” or first regime) and one for the post-1990 one (“post 2” or second regime).

I define a cohort’s exposure to the reform and subsequent amendment based on the outcome of interest, depending on how old a cohort would have to be at the time of the reform in order to be affected in that specific outcome. For instance, when considering education, a cohort is considered “treated” when part of the school-going age range overlaps with one of the post-reform regimes. A problem then arises in how to assign treatment status to cohorts that were partially exposed to the first reform and partially to the amendment, for instance because of school-going age in 1990. In my benchmark specification I consider three possible treatment statuses, that are mutually exclusive: exposed to the first regime (1981-1990) only, exposed to both the first and the second regime (post 1990), and exposed to the second regime only, where treatment status is binary (0 or 1) and the omitted group encompasses those too old to be exposed to any of the two reforms.

Consider human capital outcome y of individual i born in year t , belonging to ethnicity e , surveyed in wave w and living in province r , district d at the time of the survey. My benchmark difference-in-differences specification is:

$$\begin{aligned}
 y_{iterdw} &= \alpha + \beta_0 \cdot nonMuslim_i + & (1) \\
 &+ \beta_1 \cdot post1_t \cdot nonMuslim_i + \beta_{12} \cdot post12_t \cdot nonMuslim_i + \beta_2 \cdot post2_t \cdot nonMuslim_i \\
 &+ \lambda \cdot X_{iterdw} + \theta_e + \alpha_r + \eta_w + \mu_t + \varphi_r \cdot t + ASAL_d \cdot t + \varepsilon_{iterdw}
 \end{aligned}$$

where X_{iterdw} is a vector of controls observed in wave w - for instance, urban residence; θ_e , α_r , η_w , μ_t are respectively ethnicity, province, wave and cohort fixed effects; $\varphi_r \cdot t$ is a province-specific time trend, and $ASAL_d \cdot t$ is a time trend specific to Arid and Semi-Arid Lands (ASAL).¹² The key regressors of interest are the interactions between the non-Muslim dummy and the $post1_t$, $post12_t$ and $post2_t$ dummies, corresponding to the three mutually exclusive treatment indicators described above. These dummies are constructed based on birth year and will vary based on the specific dependent variable considered, as detailed in the next sub-sections. Province fixed effects as well as a province-specific linear time trend are included in order to capture region and cohort-specific effects that may be correlated with the error term, for instance variation across regions and over time in the supply of education.¹³ Ethnicity dummies capture time-invariant characteristics of each ethnic group, controlling for different traditions and customs concerning family, marriage and inheritance. Since ethnic boundaries in Kenya are typically coterminous with political and administrative boundaries (Ferré, 2009), ethnic groups can also serve as good proxies for areas of birth. DHS data provide quite detailed information on ethnic affiliation - respondents can choose among 10 different options in earlier waves, 15 in more recent ones.¹⁴ I estimate all my specifications by OLS and cluster standard errors at the religious group times birth cohort level, following La Ferrara and Milazzo (2017).¹⁵

The coefficients of interest are those on the interaction terms β_1 , β_{12} and β_2 . Coefficient β_1

¹²ASAL comprise the poorest areas in the country, which the government has identified as needing specific attention and has occasionally targeted with specific policies. For instance, in 1971 school fees were abolished up to the 4th year of primary school in ASAL districts; this policy was extended to the rest of the country in 1973 (Ferré, 2009).

¹³It is in principle also possible to control for household district of residence. Kenya, however, has almost doubled the number of districts between the first DHS wave (1989) and the last one (2008-09), making it sometimes hard to match new districts with the older, coarser definitions. My results are only marginally altered by including district fixed effects (results available upon request).

¹⁴In order to make ethnicity definitions comparable across DHS waves, I draw on ethnic people trees from the Joshua Project, (<http://www.joshuaproject.net/joshua-project.php>).

¹⁵Results are robust to clustering at the DHS cluster level (available upon request).

captures the difference between Muslims and non-Muslims in differences between those exposed to the first reform and those exposed to no reform; thus, it estimates the impact of the following experiment: allowing women who used to inherit half the share of their brothers to inherit the same share - which can be denoted as the “incremental” reform. Coefficient β_2 captures the difference between Muslims and non-Muslims in differences between those exposed to the second, “full” reform and the control group; therefore, it estimates the impact of the following experiment: allowing women who used to inherit a 0 share to inherit the same share as their brothers. Unless there are strong non-linearities in the effects of inheritance rights, one expects β_1 and β_2 to have the same sign, and β_2 to be larger in magnitude than β_1 . Coefficient β_{12} captures the effects of partial exposure to the first and to the second reform. In practice, a complication arises in the interpretation of coefficient β_2 : cohorts exposed to the “full” reform are in some cases so young, that the previous generation has also been exposed to the reform - specifically, to the “incremental” reform. Given that I typically do not know the year of birth of the mothers of respondents, I cannot exclude these young cohorts from my sample. Thus, for a number of outcomes, the coefficient β_2 will effectively capture a cumulative effect: that of being exposed to the full reform as well as the effect of having parents exposed to the incremental reform.

One of the disadvantages of employing a binary treatment indicator is that an individual who spent only one year under the reform is assigned the same treatment status of an individual whose entire relevant age range - say, school-going age - is spent under the new regime. For robustness, I also consider measures of exposure to the reforms that are continuous as opposed to binary. The intensity of exposure to each reform can be defined as the number

of years spent under each regime. This leads to the following alternative specification:

$$\begin{aligned}
 y_{iterdw} &= \alpha + \beta_0 \cdot nonMuslim_i + \\
 &+ \gamma_1 \cdot post1intensity_t \cdot nonMuslim_i + \gamma_2 \cdot post2intensity_t \cdot nonMuslim_i + \\
 &+ \lambda \cdot X_{iterdw} + \theta_e + \alpha_r + \eta_w + \mu_t + \varphi_r \cdot t + ASAL_d \cdot t + \varepsilon_{iterdw}
 \end{aligned} \tag{2}$$

where $post1intensity_t$ and $post2intensity_t$ indicate the number of years, during the appropriate age range, spent by each cohort under either reform. For a cohort exposed partially to the first and partially to the second reform both variables will thus be positive. The sign of coefficients γ_1 and γ_2 is expected to be the same as for coefficients β_1 , β_{12} and β_2 in the benchmark specification, but the magnitude is to be interpreted as the impact of one additional year under either reform.

The main threat to the identification is related to confounding pre-existing trends across religious groups. In particular, I would be overestimating the positive impacts of the reform on women if non-Muslim women started doing systematically better than Muslims after the reform. In order to address these concerns, I complement my main identification strategy with a number of robustness checks, described in more detail when discussing each specific outcome. First, when sample size allows, I restrict the sample to individuals too old to be affected by the reform and estimate the effects of hypothetical “placebo reforms”, typically finding precise zero effects. Second, I re-estimate my benchmark specification focusing on one ethnic group at a time; specifically, I compare Muslims as a group to non-Muslims belonging to a specific ethnic group rather than pooling them all together in a single category.¹⁶ There is significant heterogeneity in pre-reform outcome levels across non-Muslim individuals belonging to different ethnic groups. I show that, in spite of this heterogeneity, the reform had similar effects across most ethnic groups, regardless of how

¹⁶Given that Muslims are a minority in most ethnic groups, I don not have enough power to replicate the analysis *within* ethnic groups.

their pre-reform outcome levels ranked, relative to those observed among Muslims.

Another threat to the identification stems from systematic differences between Muslims and non-Muslims in the prevalence of practices affecting the extent to which they are effectively exposed to the reform. Such practices include writing wills, *inter-vivos* transfers and enforcement of the reform. Through these practices, a group could limit its *de facto* exposure to the reform, which would change the interpretation of the relevant coefficients. For illustrative purposes, assume that enforcement is systematically laxer among Muslims. If this is the case, then Muslims - my comparison group - are exposed to a “smaller” incremental reform than the one on paper, and through coefficient β_1 I would be overestimating the effects of the “true” incremental reform. Differential enforcement would be less of a concern for the interpretation of coefficient β_2 , since in 1990 Muslims revert to the pre-1981 legislation and, presumably, enforcement standards. The same kind of bias would manifest itself if Muslims are systematically more likely to circumvent the new inheritance rules by writing wills. Assume now that enforcement were systematically laxer among non-Muslims. Then in the “post 1” and “post 2” period non-Muslims would be exposed to a smaller reform than the full one; since treatment is defined as being non-Muslim interacted with the reform, I would be underestimating the impact of both the incremental and the full reform.¹⁷ The same would hold if non-Muslims are systematically more likely to write wills. This second scenario is less worrisome since this kind of bias would tend to go against my findings. Unfortunately, there is no systematic evidence on enforcement of the law nor on the prevalence of wills and *inter-vivos* transfers across religious groups.¹⁸ It is therefore important to keep this caveat in mind when interpreting

¹⁷In the extreme case in which non-Muslims are able to perfectly elude inheritance changes and maintain the same *de facto* inheritance practices throughout the sample period, β_1 will capture the effects of a reduction, rather than an improvement, in women’s inheritance shares, and β_2 should be zero.

¹⁸It should be noted that Koranic law only allows to dispose by will of 1/3 of one’s estate; the rest should be assigned according to Koranic principles. This limits the Muslims’ ability to circumvent the law by writing wills. At the same time, the fact that the Muslim community was strongly opposed to the reform, demanding and ultimately obtaining an exemption, suggests that enforcement of the reform may have been more difficult among Muslims.

the empirical results.

As discussed above, as per Section 32 in the Law of Succession Act, the reform did not apply to particular types of assets, if located in one of a list of “exempt” districts. Given the impossibility to identify the district in which parental assets are located, nor the nature of such assets, it is not possible to identify which individuals are unaffected by the reform due to this exemption. For this reason, information on household district together with the exemption rule cannot *per se* be used as an additional source of identification, and I choose to include observations from all districts, including exempt ones, in my benchmark specifications. However, I also report estimates for households located in exempt districts at the time of the survey, and typically find insignificant treatment effects. Subject to the caveat discussed above, this can be cautiously interpreted as further evidence that I am indeed capturing the effects of the reform.

In the next sub-sections, I describe the construction of my outcome variables from the DHS data and specify the treatment definition for each of those outcomes.

5.1 Education

All DHS waves include information on years of education and educational attainment of all household members, both males and females. I consider both education, measured in years, and educational attainment, defined with binary indicators for whether primary or secondary school was completed. I define the treatment as being between age 5 and 13 during the “post 1” or “post 2” period - this corresponds to the age range of primary school.¹⁹ Specifically, in equation (1) the $post1_t$ dummy is equal to 1 for individuals born

¹⁹From independence in 1964 until 1971, Kenyan children would start school at 6 and graduate from primary school at 13. There would then be 4 years of lower secondary, 2 years of upper secondary and 3 years of university - until the age of 22. In 1985 a new system was created which included 8 years of primary school, graduation from primary school at 14, followed by 4 years of secondary school until age 18, and then 4 years of university. Other relevant changes in the education system include the abolition of school fees up to the 4th year of primary school in ASAL lands in 1971 and its extension to most of the country in 1973 up to the 6th year of primary school (Ferré, 2009).

between 1968 and 1977 (both years included), who were at a minimum 5 years old and a maximum 13 years old during the period 1981-1990; therefore, they spent at least one year of their school-going age range under the “post 1” regime, and none of their school-going years under the “pre” or “post 2” periods. The $post2_t$ dummy is equal to 1 for individuals born in or after 1986, who were 5 years old or older during the post-1990 period; therefore, throughout their school-going age, they were exposed to the “post 2” regime, but not to the “post 1” one. Intermediate cohorts born between 1978 and 1985 (both years included) spent at least one year of their school-going age under the “post 1” regime and at least one year under the “post 2” regime; the dummy $post12_t$ is equal to 1 for those cohorts. The continuous version of the treatment variables, $post1intensity_t$ and $post2intensity_t$, are defined as the number of years in the 5-13 range that an individual spent under the “post1” and “post 2” regimes respectively. Exposure thus ranges from 1 to 9.²⁰ In one of my robustness checks I also consider an alternative definition of the three treatment dummies based on the 5-18 age range, covering also secondary school. According to this specification, the $post1_t$, $post12_t$ and $post2_t$ dummies are equal to 1 for cohorts born, respectively, between 1963 and 1972, between 1973 and 1985, and after 1986.

In all of the education specifications I restrict my sample to individuals above age 20, to ensure they have completed their education and to avoid censoring issues. The resulting sample includes individuals born between 1897 and 1988. Religious affiliation is only available for female respondents. As I am not always able to match males to a female relative whose religion is known, the resulting sample of males that I can use in my education specifications is significantly smaller than that of females.

DHS wave IV also includes a siblings questionnaire, which allows me to retrieve the

²⁰For instance, an individual born in 1968 will be exposed to the first reform for one year; one born in 1977 will be exposed to the first reform for 9 years; one born in 1978 will be exposed to the first reform for 8 years and to the second reform for one year; one born in 1985 will be exposed to the first reform for one year and to the second reform for 8 years; those born after 1986 will be exposed to the second reform for 9 years.

number of siblings of each adult female respondent. Information on the number of siblings can then be exploited as an additional source of variation in the intensity of the inheritance treatment: *a priori*, the effects of obtaining inheritance rights are less pronounced when a woman has a larger number of siblings. The siblings sample includes individuals born between 1906 and 1982, who have limited exposure to the second reform; in fact, the $post2_t$ dummy is zero for all individuals in the sample. Given power limitations preventing me from cleanly estimating the effects of the “full” reform, I consider a simpler definition of treatment based on whether an individual spent part of her school-going age after the 1981 reform. The binary treatment indicator, defined as $post_t$, is equal to 1 for cohorts born after 1968. The corresponding continuous version, $postintensity_t$ is defined as number of school-going age years spent after during the post 1981 regime; as such it ranges from 1, for cohorts born in 1968, to 9, for cohorts born after 1976.

In order to test whether the reform differentially affects female education depending on the number of siblings, I estimate a triple differences specification:

$$\begin{aligned}
y_{iterdw} &= \alpha + \delta_0 \cdot nonMuslim_i + \delta_1 \cdot siblings_i + \delta_2 \cdot nonMuslim_i \cdot siblings_i + \quad (3) \\
&+ \delta_3 \cdot post_t \cdot nonMuslim_i + \delta_4 \cdot post_t \cdot siblings_i + \\
&+ \delta_5 \cdot post_t \cdot nonMuslim_i \cdot siblings_i + \\
&+ \lambda \cdot X_{iterdw} + \theta_e + \alpha_r + \eta_w + \mu_t + \varphi_r \cdot t + ASAL_d \cdot t + \varepsilon_{iterdw}
\end{aligned}$$

where $siblings_i$ represents the number of siblings of respondent i .²¹ The coefficient of interest is δ_5 , which captures the differential impact of the reform for those having one additional sibling.

²¹Since the treatment age range, at its coarsest definition, is 5 to 18 years of age, I exclude siblings who were born after an individual was 18. Results are similar using all siblings (available upon request).

5.2 Female Genital Mutilation

DHS waves III, IV and V include a module on “female circumcision” or Female Genital Mutilation (FGM). Respondents of the core questionnaire - women between 15 and 49 - are asked whether they are themselves circumcised and, if so, their age at circumcision. The same questions are asked about their oldest daughters. I construct my sample by pooling together respondents and their oldest daughters.²² About 96% of circumcised women in the resulting sample underwent FGM between age 2 and age 18. I thus restrict my sample to women above 18 in order to avoid censoring issues. The resulting sample includes females born between 1949 and 1990.

I define the treatment as being between 2 and 18 in a post-reform period and estimate versions of equations (1) and (2). Specifically, the $post1_t$ dummy is equal to 1 for individuals born between 1963 and 1972, the $post12_t$ dummy is equal to 1 for individuals born between 1973 and 1988 and the $post2_t$ dummy is equal to 1 for individuals born in or after 1989. The continuous version of the treatment dummies, $post1intensity_t$ and $post2intensity_t$, are defined as the number of years in the 2-18 range that an individual spent under the “post 1” or “post 2” regimes. Exposure thus ranges from 1 to 17 years.

5.3 Maternal Health

Drawing upon the detailed birth histories provided by DHS respondents, I construct a maternal health sample, in which the unit of observation is a birth. All DHS waves collect information on the births occurred to each respondent in the previous 5 years. For each recorded birth I define two variables: “prenatal care” is a dummy equal to 1 if the mother received prenatal care by a doctor, nurse or midwife; “birth in hospital” is a dummy equal to 1 if delivery took place in a government, private or mission hospital.

Since the earliest DHS wave is from 1989 and the latest DHS wave is from 2008/2009, I

²²I attribute to daughters the same religion, province and ethnicity as their mothers.

have information on births occurred from 1984 to 2009, namely all after the first reform period. With these data I can only compare births which occurred after the 1990 amendment with births occurred before. Consider birth j occurring in year τ to mother i born in year t and denote with v_τ a childbirth year fixed effect. I estimate:

$$\begin{aligned}
y_{j\tau iterdw} &= \alpha + \beta_0 \cdot nonMuslim_i + \\
&+ \beta \cdot post1990_\tau \cdot nonMuslim_i + \\
&+ \lambda \cdot X_{iterdw} + \lambda_1 \cdot \Psi_i + \lambda_2 \cdot \Omega_j \\
&+ \theta_e + \alpha_r + \eta_w + \mu_t + v_\tau + \varphi_r \cdot t + ASAL_d \cdot t + \varepsilon_{j\tau iterdw}
\end{aligned} \tag{4}$$

where $post1990_\tau$ is a dummy equal to 1 if the delivery took place after 1990. Furthermore I control for a vector of mother characteristics Ψ_i (mother’s age and mother’s age squared) and child characteristics Ω_j (birth order, gender, twin birth). The interpretation of coefficient β in this specification is similar to that of coefficient β_1 in equation (1): it captures the impact of the “incremental” reform.

5.4 Nuptiality and Fertility Timing

All DHS waves report the year of marriage of each respondent as well as the year of birth of each of her children. For each woman in the sample I define dummy variables for whether the respondent was married or had become a mother by a given age threshold. I define the treatment as “being of marriageable age in a post-reform period”. Given the distribution of ages at first marriage in my sample, I consider a broad definition of “marriageable age” as between 12 and 22 years of age. I restrict my sample to women above age 22, in order to avoid censoring issues, and drop women who have been in more than one union, as it is not clear whether the reported year of marriage refers to their first union. The resulting sample includes females born between 1939 and 1986.

I estimate versions of equations (1) and (2). Dependent variables are dummies for whether the respondent was married or a mother by age 15, 18 and 20. Given the treatment definition, the $post1_t$ dummy is equal to 1 for individuals born between 1959 and 1968; the $post12_t$ dummy is equal to 1 for individuals born between 1969 and 1978 and the $post2_t$ dummy is equal to 1 for individuals born in or after 1979. The continuous version of the treatment dummies are defined as the number of years in the 12-22 range that an individual spent under the “post 1” and “post 2” regimes. Exposure thus ranges from 1 to 11 years.

5.5 Decision Making, Violence and Attitudes

Self-reported measures of decision making ability, domestic violence and attitudes can be constructed drawing on the module on gender, available for DHS waves IV and V. This module includes questions on who takes decisions in the family on specific issues, on whether the respondent was ever hurt by a family member and on the respondent’s attitudes towards wife beating and refusing sex with one’s husband. As for the previous set of outcomes, I define treatment as being of marriageable age (12-22) after the reform and estimate a version of equation (1). The reference sample in this case comprises all women in DHS waves IV and V above 22 years of age and with only one union. In order to cope with the large number of outcomes and the power issues induced by small sample size, I also report summary indicators for women’s decision making ability and for the prevalence of domestic violence, following the procedure outlined in Kling, Liebman and Katz (2007).

6 Empirical Results

In this section, I present results on the impact of the reform on human capital. The bulk of my empirical analyses concerns the reform’s impact on completed education (section 6.1), as this is an outcome that I can observe directly in the DHS data across all waves.

Moreover, the conceptual framework outlined in section 3 suggests that education could be affected by the reform both through a bargaining power channel and through an “optimal bequest” channel, with education being viewed by parents as a complement or a substitute to inheriting physical assets.

I then consider outcomes related to health. As discussed in section 4, one of the limitations in the data is the inability to observe short-term outcomes before and after the reform.

This constrains me to examine only a limited set of outcomes related to health, all of which reflect past healthcare decisions. Specifically, I consider Female Genital Mutilation (FGM; section 6.2) and maternal health-seeking behavior (section 6.3). I argue that both could be affected by improved female bargaining power after the reform, although they reflect decisions taken by different agents within the household: FGM takes place mostly during childhood and teenage, and as such reflects mostly parental choices; seeking professional prenatal care is likely to reflect choices taken by adult women negotiating with their spouses. It is plausible that these outcomes would be affected primarily through a bargaining power channel, rather than an “optimal bequest” one.

Total fertility is another outcome that the reform is likely to affect, both through a bargaining power channel, with family size plausibly getting closer to the preferences of wives, and through an optimal bequest channel, if parents want to avoid fragmentation in family property. Data limitations discussed in below prevent me from observing total fertility, but I can examine age at marriage and fertility onset (section 6.4) as an imperfect proxy.

Finally, in the attempt to pin down the bargaining power channel with more confidence, I provide some suggestive evidence on household bargaining power, drawing upon self-reported survey questions on decision making and attitudes (section 6.5).

6.1 Education

[Insert Table 2]

Summary statistics from my main education sample are reported in Table 2. The average number of years of education is around 6 for females and 8 for males. The gap in educational attainment across religious groups is sizable. That Muslims have been lagging behind in education is a well-known fact, that has sometimes been blamed on discriminatory practices in missionary schools (Oded, 2000).

[Insert Table 3]

The main results related to education are presented in Table 3. The benchmark specification employing the binary treatment indicator, corresponding to equation (1), is presented in panel A. Columns (1) and (2) refer to years of education as an outcome, and report estimates for females (column (1)) and males (column (2)) considered separately. The coefficient on the “post 1” interaction is positive and significant for females, negative and insignificant for males. As expected, the coefficients on the “post 2” interactions have the same sign and are larger in magnitude, becoming significant also for males. The coefficients on the “post 1,2” interaction are insignificant, but have the same signs. According to these estimates, females receive roughly one more year of education following the “full” reform - going from a zero share to the same share as their brothers - whereas males receive roughly one and a half fewer years of education. This sizable reduction of the gender education gap suggests that parents substitute the education of males for that of females, in a way which is compatible both with a bargaining power channel and with an “optimal bequest” one. Columns (3) to (6) replicate the analysis for two alternative dependent variables related to educational attainment: a dummy for whether an individual has completed primary and secondary school, respectively. The estimates confirm the pattern of columns (1) and (2) and are highly significant for females, noisier for males. A girl exposed to the full reform (“post 2”) is 19 percentage points more likely to complete

primary school and 15 percentage points more likely to complete secondary school.

As a robustness check, panel B reports estimates from equation (2), which employs a continuous treatment indicator. Qualitatively, the patterns are similar to those found in panel A, but estimates are smaller in magnitude. For each additional school-going age year that a girl spends under the full reform regime, she increases her likelihood of completing primary school by about 1.2 percentage points; spending the entire age range - 9 years - under the full reform regime corresponds to an 11 percentage points increase in the likelihood of completion.

[Insert Table 4]

Table 4 reports a number of robustness checks, focusing on years of education as an outcome variable. First, restricting my sample to exempt districts, I find insignificant effects for females, although the negative impact of the reform remains significant for males (columns (1) and (2)). As explained in section 5, the power of this test is somewhat limited so these results should be interpreted with caution.²³ Columns (3) and (4) show that the estimates in Table 3A are robust to the inclusion of a household-level wealth index. While I view this as an endogenous control, it is reassuring to note that the estimated effect of the reform is only slightly affected by controlling for wealth. In columns (5) and (6) I consider a different, coarser treatment definition - being of age 5 to 18 during a “post” period, i.e. being exposed by high-school age. Again, the estimated impacts of the reform are comparable in sign and magnitude as those in the benchmark specification of Table 3A, columns (1) and (2). Finally, in columns (7) and (8), I conduct a falsification test: I restrict the sample to individuals older than 18 in 1981, and thus unaffected by the reform. I then estimate the impact of a “placebo” reform, where the treatment is defined as being born after 1955. I find insignificant impacts for both males and females; in particular, the

²³The power of this test could be especially limited for males if the latter are more mobile than females, and therefore display a greater discrepancy between district of residence and the district in which parental assets are located.

coefficient for females is negative (the opposite sign of the “true” treatment effect) and that of males is a precise zero.

Overall, these estimates suggest a sizable improvement in the education of girls whose schooling decisions were made in the post-reform period, to the expense of boys. These results are in line with those of Goyal et al. (2013) and Roy (2015), who also find an increase in girls’ education following improved inheritance rights with the Hindu Succession Act. On the other hand, my results contrast with those of La Ferrara and Milazzo (2017), who find that the education of boys decreases as their inheritance rights improve. In terms of absolute magnitudes, my estimated effect - up to one and a half year difference - is similar to the effects found in the aforementioned studies.

In the analysis conducted so far, I have compared Muslims and non-Muslim, pooling together all ethnic groups. This masks significant heterogeneity in pre-reform education levels across non-Muslims belonging to different ethnic groups. While Muslims are initially less educated than non-Muslims considered as a group, there are individual ethnic groups for which the gap is more pronounced. In Table A1, I disaggregate the non-Muslim sample by ethnicity - following the 1989 DHS definition - and estimate my benchmark specification (from Table 3A) comparing Muslims, pooled as a group, with non-Muslims belonging to one ethnic group at a time. The pre-reform years of education, measured for cohorts born before 1962, for non-Muslim females and males of each ethnic group are reported at the bottom of each column. As a reference, the pre-reform years of education among the Muslims in this sample is 2.2 for females and 4.8 for males. Qualitatively, my results still hold for most of the sub-samples, except for the two groups at the two extremes: the Kikuyu, the most educated in the sample, and the Mijikenda/Swahili, the least educated in the sample. It is possible that in these two groups education decisions are less sensitive to inheritance considerations.

[Insert Table 5]

In Table 5 I present specifications that exploit sibling composition as a source of variation in treatment intensity.²⁴ The inheritance effect of the reform should be smaller, in absolute terms, for women with a larger number of siblings. Recall from the discussion in section 5.1 that the siblings sub-sample is drawn from DHS wave IV only, consists only of females and does not include cohorts of the “post 2” period. Therefore, the relevant comparison is between cohorts of school-going age during any of the post-reform regimes with cohorts that were not exposed to any reform. Summary statistics for this sample are reported in Appendix Table A2 and show no large differences in the average number of siblings of Muslims and non-Muslims.

In columns (1) and (2) I consider the benchmark binary treatment indicator, and in columns (3) and (4) I consider the continuous version. Before turning to the triple differences specification of equation (3), in columns (1) and (3) I only consider non-Muslims and compare females of pre- and post- cohorts with different number of siblings. The interaction between “post” and the number of siblings is negative and significant in both specifications suggesting that the positive reform effect is attenuated for females with a high number of siblings. The full triple differences specification is reported in columns (2) and (4). The triple interaction coefficient is negative in both specifications and significant for the continuous measure, indicating that a higher number of siblings reduces the reform impact.

Appendix Table A3 reports specifications analogous to those in Table 5, columns (1) and (2), considering separately brothers and sisters. *A priori*, there are several reasons why the effects may be heterogeneous depending on the gender of one’s siblings. For a Muslim woman, the effective increase in inheritance associated with the “incremental” reform - going from half a share under the pre-1981 regime to the full share under the 1981-1990

²⁴Unfortunately, this strategy is only possible when examining education as an outcome variable due to sample size limitations: only one DHS wave contains sibling information, and only education is available for a sufficient number of respondents, across a sufficient number of cohorts.

one - is larger, the more brothers she has, conditional on the total number of siblings. This stems from the fact that there are fewer sisters to share the increment with. By this mechanical effect, one would expect the relative extra-increase in inheritance among non-Muslim women - captured by the interaction between the “post” and the non-Muslim dummies - to be attenuated for women with more brothers. At the same time, the strictness of adherence to inheritance rules could be correlated with the gender mix of the siblings. For instance, a family with many daughters may be more reluctant to comply with the law because parents wish to avoid the fragmentation of parental assets among daughters that are married away. In this case, the impacts of the reforms could be attenuated for women with a large number of daughters, conditional on the total number of siblings. The results in Table A3 are qualitatively consistent with those in Table 5 for both brothers and sisters, but not significant. The attenuating effect of having a large number of siblings appears larger, in absolute terms, in case of sisters.

6.2 Female Genital Mutilation

[Insert Tables 6 and 7]

Female Genital Mutilation (FGM), officially deemed illegal in Kenya in 2011 (IFHRO, 2011), is widespread among women in my sample, and practiced across ethnicities and religious groups. It is particularly interesting to examine FGM as an outcome since this practice does not constitute a parental investment that can be considered an obvious complement or a substitute to physical bequests, like education. Although FGM is considered a valuable trait for a bride among traditional households (Ouedraogo and Koissy-Kpein, 2012),²⁵ it reduces women’s well-being and may entail adverse health consequences. Summary statistics for the FGM sample are reported in Table 6, which

²⁵FGM could in principle be viewed as a substitute for physical assets on the marriage market. This interpretation is related to the marriage market channel discussed in footnote 8. Unfortunately my setting does not allow me to examine the interactions between the reform and marriage markets directly, given that marriages occur within religious groups, and my identification strategy relies on comparisons across religions.

shows that FGM is equally prevalent in the Muslim as well as non-Muslim community; around 32% of the women in the sample underwent FGM. Table 7 presents the benchmark specification in column (1) and the version employing a continuous treatment definition in column (2). The inheritance reform is associated with a highly significant decrease in the probability of mutilation, by as much as 5 percentage points for the incremental reform and 20 percentage points for the full one, according to the specification in column (1).

According to estimates in column (2), one additional year in the 2-18 age range spent under the full reform regime reduces FGM likelihood by about 0.95 percentage points, meaning that spending the full age range under the second reform regime leads to an estimated 15 percentage points reduction in FGM rates. While I am not aware of any other estimate that I can directly compare this figure to, this seems a very sizable impact, as it is more than twice the size of the impact of the urban residence dummy.

An important caveat should be noted: FGM is a sensitive, self-reported outcome and there could be a social desirability bias by which individuals are more or less likely to report undergoing FGM. This is a threat to the identification to the extent that the likelihood of reporting is correlated with the treatment. In particular, one may worry that more educated women, such as those exposed to the reform, are less likely to report undergoing FGM. In column (3) I mitigate this concern by including in the specification of column (1) a battery of controls, including household wealth at the time of the survey and the respondent's years of education. While both controls should be viewed as endogenous to the reform, it is reassuring to note that my estimates are marginally affected by the inclusion of these variables.

When restricting my sample to exempt districts (column (4)), I obtain a precisely estimated 0 effect. A precise 0 effect is also found when restricting the sample to unaffected cohorts and estimating a placebo treatment (column (5)).

Although FGM is practiced among most ethnic groups, there is significant heterogeneity in

FGM prevalence across ethnicities. Table A4 reports the specification in Table 7, column (1), comparing Muslims to non-Muslims in one ethnic group at a time. Pre-reform FGM rates for non-Muslims are reported for the 8 main ethnic groups considered in the first DHS wave. FGM prevalence ranges from 97% among the Kisii to close to 0 among the Mijikenda/Swahili. The reduction in FGM rates is mostly driven by the Kamba, Kikuyu and Meru groups, which all have a pre-reform FGM prevalence between 55 and 72%. Among groups that never practiced FGM, there seems to be if anything a slight increase in FGM rates following the reform. Finally, no significant impact is found when looking at the Kalenjin and Kisii, the groups among which FGM was almost universally practiced to start with (83% and 97% prevalence respectively). A plausible interpretation is that the inheritance reform reduced FGM rates only in contexts in which this practice was not universal to start with, but was not able to induce significant behavioral changes in groups in which FGM was very deep-rooted.

Taken together, these results suggest that the reform induced a reduction in the prevalence of FGM. As FGM arguably reflects choices made by parents during teenage and childhood, the most natural interpretation of these findings is as evidence of improved bargaining power of mothers following the reform, which translates into better health outcomes for their daughters.

6.3 Maternal Health

I next turn to an adult female health outcome: whether a woman received professional medical assistance during pregnancy and labor. In my maternal health sub-sample, the unit of observation is the birth. Recall that the estimated specification (equation 4) includes fixed effects for the year of the birth itself, and also for the mother's birth year. Therefore, it does not simply capture whether different cohorts of women have different practices concerning pregnancy and delivery, but also whether the same cohort of women

behaves differently during pregnancies that occurred before or after the reform.

[Insert Tables 8 and 9]

Summary statistics for this sample are reported in Table 8. On average, 84% of births occurred to Muslim women are preceded by prenatal care, and 27% occur in a hospital. These figures are respectively 81% and 32% for non-Muslims. Table 9 shows that women adopt safer antenatal and birth practices for births occurring after the reform: within a given cohort of mothers, births occurred after the full reform are roughly 7 percentage points more likely to take place in a hospital and to be preceded by professional antenatal care (columns (1) and (4)). These results are only minimally attenuated by the inclusion of controls (columns (2) and (5)). In exempt districts (columns (3) and (6)) the reform is found to have insignificant effects, but this test has to be interpreted cautiously given the large standard errors and small sample.²⁶

In Table A5 I estimate the benchmark specification from Table 9, columns (1) and (4) disaggregating by ethnic group. Pre-reform averages of the dependent variables for non-Muslims are reported at the bottom of each column. Prenatal care is the least prevalent among the Mijikenda/Swahili and the most prevalent among the Meru and Kikuyu. Large improvements occur among the Kamba and Mijikenda/Swahili, two groups with particularly low levels of the two outcome variables before the reform. Conversely, the impacts of the reform are insignificant for the Kikuyu, Kisii and Luo.

Overall, these results are suggestive of greater bargaining power of mothers following the reform, tilting household decisions related to maternal health towards the preferences of females.

²⁶Unfortunately the maternal health sample does not have enough pre-reform years to perform a meaningful falsification test using “placebo” reforms.

6.4 Nuptiality and Fertility Timing

Changes in inheritance rules are likely to affect total fertility. First, the bargaining power channel suggests that post-reform fertility choices will be tilted towards women's preferences - typically involving a smaller number of children at the optimum. In fact, Sen (2001) argues that women's empowerment, including property rights, is a key instrument for reducing fertility rates. Secondly, as parents take the reform into account in their fertility decisions, they could reduce their target fertility in order to prevent the fragmentation of family assets.²⁷ Unfortunately, data limitations do not allow me to observe total fertility, given that cohorts of women exposed to the reform have typically not completed their fertility at the time of the survey. However, I can examine fertility onset and investigate whether there have been shifts in the timing of entry into motherhood and marriage.

[Insert Tables 10 and 11]

Table 10 presents summary statistics for the nuptiality and fertility sample. The timing of childbearing and marriage seems to be overall similar across religious groups, with 51% of Muslim women and 55% of non-Muslim women in the sample entering motherhood before age 20. Table 11 reports results referring to the benchmark specification, employing binary treatment indicators, and considers three sets of outcomes: a dummy for whether a woman was married or had become a mother by ages 15, 18, and 20. According to the estimates in Table 11, women exposed to the incremental as well as the full reform are less likely to get married and bear children before they are 18 and 20. Specifically, the full reform reduces a woman's likelihood to be married and to be a mother by age 20 by 18 and 19 percentage points, respectively. The coefficients are comparable in sign and significance, although smaller in magnitude, for the likelihood of being married or a mother by age 18. It is not

²⁷For instance, it has been frequently argued that the French birth rate dropped very rapidly in the 19th century following the Napoleonic change in the inheritance laws, from primogeniture to equal division of estates among all children (Garner, 1914).

surprising that the patterns of fertility and nuptiality timing are very similar, since age at first birth and age at first marriage are highly correlated.

[Insert Table 12]

In Table 12 I report robustness checks, focusing on the age 20 threshold for the dependent variables. Since the definition of treatment period for these outcomes (“being of marriageable age”, defined as 12-22 years of age) could overlap with one of the possible definitions of treatment in the education regressions (age range 5-18), there is a concern that the coefficients in Table 11 may be purely driven by the mechanical effect of girls staying in school longer as a consequence of the reform, rather than a direct effect of inheritance rights on fertility and nuptiality decisions. My results, however, survive the inclusion of a variety of controls, among which wealth and education (Table 12, columns (1) and (4)). Results are also robust to employing the continuous treatment definition (columns (2) and (5)). Finally, a placebo treatment (“born after 1950”) administered to unexposed cohorts yields a positive and insignificant coefficient (columns (3) and (6)).²⁸

In Table A6 I report the specification of Table 11, column (6) disaggregating by ethnic group. The pre-reform likelihood of being a mother by age 20 ranges from 53% to 74%. Results are consistent across ethnic groups, with the exception of the Meru, among whom the reform has an insignificant impact.

Taken together, these results are suggestive that women exposed to the reform tend to postpone marriage and childbirth. While this could reflect a mere shift in timing, it seems plausible that it would also translate into a lower total fertility rate.

²⁸Unfortunately the nuptiality and fertility sample includes too few observations from exempt districts to perform a meaningful falsification test focusing on those districts only.

6.5 Other Outcomes: Decision Making, Violence, Attitudes

The results discussed so far are consistent with the reform having a bargaining power effect but also possibly an investment effect, with parents complementing physical capital with human capital in their optimal bequests. While it is, in general, difficult to disentangle these two mechanisms, some suggestive evidence on bargaining power can be provided by considering self-reported measures of decision making ability and attitudes from the DHS gender module. If the reform increases women’s bargaining power, one expects that couples formed after the reform should be characterized by a more balanced decision making process, and attitudes more favorable to women. This could result from better marital matching - for example, women exposed to the reform marrying higher-quality husbands - or from implicit pre-marital commitment contracts more favorable to women.²⁹

I investigate the impacts of the reform on these outcomes in Appendix Tables A8 to A10, while summary statistics are reported in Table A7. Treatment status is defined as being of marriageable age (12 to 22 years of age) during one of the reform periods. This is to avoid endogeneity in the timing of marriage, which is affected by the reform, as documented in section 6.4. I report both individual outcome variables - drawn from specific DHS questions - and summary measures, coded such that higher values represent favorable outcomes for women (e.g. more decision making power, or lower domestic violence). Table A8 shows that women exposed to the reform during their marriageable age are significantly less likely to report that their husbands have the final say on a variety of household decisions, including large and daily purchases, spending the wife’s earnings, and the wife’s health. Results for spousal and domestic violence (Table A9) as well as attitudes towards wife beating or refusing sex with one’s husband (Table A10) are generally noisy and the

²⁹As discussed in section 3, I cannot isolate the matching channel with my identification strategy because marriage occurs within religious groups: in this context “treated” women compete on marriage markets primarily with women who are also “treated”. Empirically, it is also difficult to examine measures of husband quality as evidence of marital selection because of the small size of the sample that includes husband characteristics.

summary measures are insignificant, however a few significant patterns arise: the reform appears to reduce the likelihood of severe or sexual spousal violence, and violence perpetrated by the father (Table A9, columns (2), (3) and (5)). Moreover, after the reform women are more likely to consider refusing marital sex “justified” (Table A10, column (3)). While small sample size and concerns related to self-reporting should make one cautious in interpreting these estimates, these results support the interpretation that the reform had a direct bargaining power effect, consistent with the findings of Heath and Tan (2015) and Mokerjee (2015) in the Indian context.

7 Conclusions

In this paper I provide estimates of the impact of an inheritance law reform granting women equal inheritance rights, in the context of Kenya. I exploit variation in inheritance rights across religious groups and cohorts to assess how improved statutory inheritance rights affected a variety of human capital outcomes. I start by considering education and I compare cohorts who were of school-going age before and after the reform. I find that the education of girls improves in absolute terms and relative to that of boys. These effects are attenuated if a woman has a large number of siblings, which supports the interpretation that I am indeed capturing the effects of the inheritance regime change. This is consistent with the reform having a bargaining power effect, but may also reflect an investment channel, with parents complementing physical capital with human capital in their “optimal bequest” choice. I then consider two female health-related outcomes: female genital mutilation and maternal health, proxied by medical assistance during pregnancy and labor. I find a significant decrease in the probability of being mutilated for girls who were children or teenagers after the reform, mostly in ethnic groups among which this practice is not universal to start with. This is particularly interesting since this is an outcome that is most

likely to be affected by a bargaining power mechanism, rather than an investment one. Comparing pregnancy and childbirth practices before and after the reform, I find that improvements in inheritance rights are associated with better maternal health practices. I finally turn to outcomes related to marriage, finding that women who are of marriageable age after the reform tend to postpone marriage and fertility, suggesting that total fertility rates may be lower, and report having more decision making power within the household. Taken together, my results provide a quite coherent picture of a general improvement in women's status, health and education. These results are all consistent with a bargaining power effect, although these improvements can also reflect changes in the mix of human and physical capital that parents bequeath to their children in the post-reform inheritance regime. Given that the reform makes both parents and children become "treated" at the same time, an inherent limitation of my identification strategy is that it is not entirely possible to disentangle these two channels. However, the finding that women participate more in household decision making in unions formed after the reform are suggestive that women's bargaining power is indeed enhanced.

Overall, these findings suggest that legal reform at the statutory level can have an impact even in a context of poor legal enforcement and in spite of the persistence of deep-rooted social norms. As many Sub-Saharan African countries are undergoing pro-woman reform or drafting new constitutions, these results indicate that formal legislation can be an important starting point even in contexts in which customs are perceived to be very hard to change.

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Table 1: Inheritance Regimes

	parental assets inherited by daughters / parental assets inherited by sons		
	<i>pre</i>	<i>post 1</i>	<i>post 2</i>
	pre 1981	1981-1990	post 1990
Muslims	0.5	1	0.5
non-Muslims	0	1	1

Table 2: Summary Statistics, Education Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
<i>Panel A: Females</i>									
Muslim	32885	0.069	0.253	2268	1	0	30617	0	0
Urban	32885	0.245	0.430	2268	0.513	0.500	30617	0.226	0.418
Age	32885	35.771	12.629	2268	36.108	12.969	30617	35.746	12.603
Wealth Index (1 to 5)	26513	3.226	1.426	1950	3.362	1.556	24563	3.215	1.414
Years of education	32885	5.951	4.519	2268	4.063	4.523	30617	6.091	4.487
Completed primary school	32885	0.454	0.498	2268	0.301	0.459	30617	0.465	0.499
Completed secondary school	32885	0.139	0.346	2268	0.086	0.280	30617	0.143	0.350
<i>Panel B: Males</i>									
Muslim	23581	0.069	0.253	1624	1	0	21957	0	0
Urban	23581	0.266	0.442	1624	0.556	0.497	21957	0.245	0.430
Age	23581	38.764	13.826	1624	39.275	14.377	21957	38.726	13.784
Wealth Index (1 to 5)	21831	3.312	1.432	1523	3.491	1.546	20308	3.298	1.422
Years of education	23581	7.625	4.376	1624	6.371	4.836	21957	7.718	4.326
Completed primary school	23581	0.640	0.480	1624	0.538	0.499	21957	0.647	0.478
Completed secondary school	23581	0.245	0.430	1624	0.192	0.394	21957	0.248	0.432

All DHS waves; individuals above 20 years of age.

Table 3 : Education

Dependent variable	Years of education				Completed primary school		Completed secondary school	
	(1)		(2)		(3)		(4)	
	females	males	females	males	females	males	females	males
Panel A								
Post 1 x Non-Muslim	0.511** (0.197)	-0.188 (0.217)	0.0922*** (0.0180)	-0.00564 (0.0250)	0.0837*** (0.0144)	-0.00564 (0.0250)	0.0248 (0.0230)	
Post 1,2 x Non-Muslim	0.0937 (0.189)	-0.270 (0.251)	0.0779*** (0.0205)	-0.0217 (0.0229)	0.0713*** (0.0187)	-0.0217 (0.0229)	-0.0169 (0.0327)	
Post 2 x Non-Muslim	1.041*** (0.373)	-1.591*** (0.195)	0.194*** (0.0186)	-0.138*** (0.0357)	0.154*** (0.0225)	-0.138*** (0.0357)	-0.168*** (0.0519)	
Non-Muslim	0.794*** (0.117)	1.125*** (0.156)	0.0597*** (0.0134)	0.0810*** (0.0148)	0.0522*** (0.00817)	0.0810*** (0.0148)	0.101*** (0.0153)	
Urban	2.296*** (0.0830)	2.182*** (0.0872)	0.226*** (0.00939)	0.180*** (0.0108)	0.163*** (0.0108)	0.180*** (0.0108)	0.199*** (0.00819)	
R-squared	0.415	0.314	0.275	0.215	0.215	0.215	0.208	
Panel B								
Post 1 Intensity x Non-Muslim	0.0182 (0.0221)	-0.0313 (0.0289)	0.00740*** (0.00237)	-0.000923 (0.00337)	0.00896*** (0.00215)	-0.000923 (0.00337)	0.00122 (0.00299)	
Post 2 Intensity x Non-Muslim	0.0387 (0.0364)	-0.0834* (0.0453)	0.0118*** (0.00377)	-0.00833* (0.00432)	0.00960*** (0.00318)	-0.00833* (0.00432)	-0.0103 (0.00668)	
Non-Muslim	0.914*** (0.128)	1.125*** (0.147)	0.0770*** (0.0143)	0.0813*** (0.0143)	0.0636*** (0.00909)	0.0813*** (0.0143)	0.106*** (0.0143)	
Urban	2.297*** (0.0828)	2.182*** (0.0872)	0.226*** (0.00938)	0.180*** (0.0108)	0.163*** (0.0108)	0.180*** (0.0108)	0.199*** (0.00819)	
Observations	32,885	23,581	32,885	23,581	32,885	23,581	23,581	
R-squared	0.415	0.314	0.275	0.215	0.215	0.215	0.208	

All DHS waves; individuals above 20 years of age. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1968 and 1977, between 1978 and 1985, and after 1986 respectively. Post 1 Intensity and Post 2 Intensity are defined as the number of years in the 5-13 range spent under the first and second reform respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Education, Robustness

Dependent variable: years of education

	(1)	(2)		(3)		(4)		(5)		(6)		(7)		(8)
	exempt districts females	males	males	females	females	males	all districts females	males	females	males	all districts females	males	all districts females	males
Post 1 x Non-Muslim	0.321 (0.495)	-1.230* (0.725)	-0.318 (0.232)	0.421** (0.196)	0.431* (0.220)	-0.318 (0.267)	0.431* (0.220)	-0.0830 (0.267)	0.431* (0.220)	-0.0830 (0.267)	0.431* (0.220)	-0.0830 (0.267)	0.431* (0.220)	-0.0830 (0.267)
Post 1,2 x Non-Muslim	-0.214 (0.628)	-2.334* (1.323)	-0.317 (0.241)	-0.191 (0.194)	0.252 (0.162)	-0.302 (0.220)	0.252 (0.162)	-0.302 (0.220)	0.252 (0.162)	-0.302 (0.220)	0.252 (0.162)	-0.302 (0.220)	0.252 (0.162)	-0.302 (0.220)
Post 2 x Non-Muslim	0.513 (0.979)	0.719 (0.979)	-1.761*** (0.205)	0.623* (0.371)	1.074*** (0.378)	-1.610*** (0.199)	1.074*** (0.378)	-1.610*** (0.199)	1.074*** (0.378)	-1.610*** (0.199)	1.074*** (0.378)	-1.610*** (0.199)	1.074*** (0.378)	-1.610*** (0.199)
Placebo: born after 1955 x Non-Muslim														
Non-Muslim	1.049* (0.545)	1.718*** (0.609)	1.203*** (0.158)	0.996*** (0.130)	0.759*** (0.129)	1.139*** (0.162)	0.996*** (0.130)	1.203*** (0.158)	0.759*** (0.129)	1.139*** (0.162)	1.226*** (0.143)	1.098*** (0.178)	1.226*** (0.143)	1.098*** (0.178)
Urban	1.146** (0.447)	0.964* (0.508)	0.479*** (0.0751)	0.437*** (0.104)	2.297*** (0.0828)	2.182*** (0.0874)	0.437*** (0.104)	0.479*** (0.0751)	2.297*** (0.0828)	2.182*** (0.0874)	2.706*** (0.125)	2.591*** (0.152)	2.706*** (0.125)	2.591*** (0.152)
Wealth index				1.104*** (0.0283)	1.032*** (0.0298)		1.104*** (0.0283)	1.032*** (0.0298)						
Observations	1,253	1,025	21,831	26,513	32,885	23,581	26,513	21,831	32,885	23,581	14,296	10,956	14,296	10,956
R-squared	0.445	0.398	0.376	0.495	0.415	0.314	0.495	0.376	0.415	0.314	0.325	0.319	0.325	0.319
Treatment age range	5-13	5-13	5-13	5-13	5-18	5-18	5-13	5-13	5-18	5-18	5-13	5-13	5-13	5-13

All DHS waves; individuals above 20 years of age. Columns (7) and (8): individuals older than 18 in 1981. Columns (1) to (4): Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1968 and 1977, between 1978 and 1985, and after 1986 respectively. Columns (5) and (6): Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1963 and 1972, between 1973 and 1985, and after 1986 respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Education and Sibling Number

	(1)		(2)		(3)		(4)	
	non-Muslim		DDD		non-Muslim		DDD	
Non-Muslim			0.589 (0.388)				0.712* (0.367)	
Num Siblings	0.118*** (0.0326)		0.0487 (0.0616)		0.101*** (0.0304)		0.0365 (0.0633)	
Non-Muslim x Num Siblings			0.0773 (0.0599)				0.0733 (0.0624)	
Post x Non-Muslim			0.346 (0.505)					
Post x Num Siblings			0.0114 (0.101)					
Post x Non-Muslim x Num Siblings	-0.139*** (0.0424)		-0.155 (0.104)					
Post Intensity x Non-Muslim							0.0234 (0.0596)	
Post Intensity x Num Siblings					-0.0162*** (0.00564)		0.00761 (0.0120)	
Post Intensity x Non-Muslim x Num Siblings							-0.0248* (0.0126)	
Urban	2.152*** (0.177)		2.195*** (0.158)		2.151*** (0.177)		2.202*** (0.158)	
Observations	6,187		6,722		6,187		6,722	
R-squared	0.421		0.432		0.421		0.433	

DHS wave 4; females above 20 years of age. Post equals 1 for cohorts born after 1968. Post Intensity is defined as number of years in the 5-13 range spent after or during one of the two reform regimes. Additional controls: age, age squared; ethnicity, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Summary Statistics, FGM Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
Muslim	18363	0.075	0.263	1378	1	0	16985	0	0
Urban	18363	0.297	0.457	1378	0.536	0.499	16985	0.277	0.448
Age	18363	30.646	8.446	1378	30.205	8.170	16985	30.682	8.468
Wealth Index (1 to 5)	18363	3.313	1.439	1378	3.391	1.570	16985	3.307	1.428
Age of household head	18363	41.370	12.729	1378	42.242	13.626	16985	41.299	12.651
Number of household members	18363	5.332	2.621	1378	5.800	3.120	16985	5.294	2.572
Male-headed household	18363	0.659	0.474	1378	0.628	0.483	16985	0.661	0.473
Circumcised	18363	0.325	0.468	1378	0.316	0.465	16985	0.326	0.469

DHS waves III, IV and V; females above 18 years of age.

Table 7: Female Genital Mutilation

FGM

	(1) all districts	(2) all districts	(3) all districts	(4) exempt districts	(5) all districts
Post 1 x Non-Muslim	-0.0496* (0.0283)		-0.0300 (0.0298)	0.00448 (0.0445)	
Post 1,2 x Non-Muslim	-0.114*** (0.0245)		-0.0956*** (0.0254)	-0.0402 (0.0884)	
Post 2 x Non-Muslim	-0.199*** (0.0384)		-0.180*** (0.0355)	-0.104 (0.147)	
Post 1 Intensity x Non-Muslim		-0.00613*** (0.00199)			
Post 2 Intensity x Non-Muslim		-0.00949*** (0.00190)			
Placebo: born after 1955 x Non-Muslim					0.0234 (0.0605)
Non-Muslim	-0.0600*** (0.0221)	-0.0619*** (0.0207)	-0.0552** (0.0236)	-0.251*** (0.0882)	-0.173*** (0.0542)
Urban	-0.0595*** (0.00798)	-0.0589*** (0.00798)	-0.0145** (0.00709)	-0.00687 (0.0300)	-0.0413** (0.0178)
Household head age			-0.00149*** (0.000270)		
Household size			0.00198* (0.00104)		
Male-headed household			0.00629 (0.00474)		
Years of education			-0.0225*** (0.000753)		
Observations	18,363	18,363	18,332	940	3,181
R-squared	0.435	0.435	0.465	0.675	0.528

DHS waves III, IV and V; females above 18 years of age. Column (5): females older than 18 in 1981. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1963 and 1972, between 1973 and 1988, and after 1989 respectively. Post 1 Intensity and Post 2 Intensity are defined as the number of years in the 2-18 range spent under the first and second reform respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Summary Statistics, Maternal Health Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
Muslim	27182	0.068	0.251	1838	1	0	25344	0	0
Urban	27182	0.201	0.401	1838	0.398	0.490	25344	0.187	0.390
Childbirth year	27182	1995	7.528	1838	1999	7.333	25344	1995	7.483
Mother age at delivery	27182	25.903	6.587	1838	25.238	6.440	25344	25.951	6.595
Prenatal care	23777	0.810	0.392	1439	0.837	0.370	22338	0.809	0.393
Birth in hospital	27182	0.318	0.466	1838	0.269	0.443	25344	0.322	0.467

Original note: All DHS waves; births occurred between 1984 and 2009.

Table 9: Maternal Health

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Prenatal care all districts	Prenatal care all districts	Prenatal care exempt districts	Birth in hospital all districts	Birth in hospital all districts	Birth in hospital exempt districts
Birth post 1990 x	0.0681** (0.0282)	0.0674** (0.0284)	0.475 (0.314)	0.0740** (0.0294)	0.0740** (0.0293)	0.0898 (0.204)
Non-Muslim	-0.0728*** (0.0266)	-0.0757*** (0.0271)	-0.779** (0.304)	-0.0642** (0.0303)	-0.0700** (0.0302)	-0.0439 (0.223)
Urban	0.103*** (0.0141)	0.0920*** (0.0137)	0.0713* (0.0395)	0.262*** (0.0124)	0.236*** (0.0123)	0.372*** (0.0576)
Mother age		0.155 (0.226)			0.296*** (0.0532)	
Mother age squared		-0.000147** (5.69e-05)			-4.92e-05 (6.25e-05)	
Birth order		-0.0156*** (0.00181)			-0.0387*** (0.00329)	
Twin		0.0685*** (0.0207)			0.127*** (0.0176)	
Female child		-0.01000*** (0.00368)			-0.0164*** (0.00511)	
Observations	23,777	23,777	967	27,182	27,182	1,364
R-squared	0.290	0.294	0.230	0.228	0.244	0.280

All DHS waves; births occurred between 1984 and 2009. Additional controls: ethnicity, DHS wave, province, respondent year of birth, and childbirth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Summary Statistics, Nuptiality and Fertility Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
Muslim	23437	0.057	0.232	1336	1	0	22101	0	0
Urban	23437	0.252	0.434	1336	0.522	0.500	22101	0.235	0.424
Age	23437	33.209	7.457	1336	32.313	7.180	22101	33.263	7.471
Married by age 15	23437	0.106	0.308	1336	0.172	0.378	22101	0.102	0.302
Married by age 18	23437	0.350	0.477	1336	0.430	0.495	22101	0.345	0.475
Married by age 20	23437	0.551	0.497	1336	0.618	0.486	22101	0.547	0.498
Mother by age 15	23437	0.067	0.250	1336	0.083	0.276	22101	0.066	0.248
Mother by age 18	23437	0.310	0.462	1336	0.324	0.468	22101	0.309	0.462
Mother by age 20	23437	0.544	0.498	1336	0.513	0.500	22101	0.546	0.498

All DHS waves; females above 22 years of age with only one union.

Table 11: Nuptiality and Fertility Timing

<i>Dependent variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Married by age 15</i>	<i>Mother by age 15</i>	<i>Married by age 18</i>	<i>Mother by age 18</i>	<i>Married by age 20</i>	<i>Mother by age 20</i>
Post 1 x Non-Muslim	0.0257 (0.0227)	0.000924 (0.0164)	-0.0506 (0.0328)	-0.0599** (0.0294)	-0.0848** (0.0341)	-0.0854** (0.0348)
Post 1, 2 x Non-Muslim	0.00188 (0.0237)	-0.00485 (0.0165)	-0.108*** (0.0337)	-0.0718** (0.0316)	-0.139*** (0.0356)	-0.137*** (0.0356)
Post 2 x Non-Muslim	0.0242 (0.0294)	-0.0133 (0.0228)	-0.136*** (0.0346)	-0.0914** (0.0391)	-0.176*** (0.0391)	-0.188*** (0.0326)
Non-Muslim	-0.0416** (0.0209)	-0.00836 (0.0133)	0.00423 (0.0260)	-0.000555 (0.0246)	0.0178 (0.0276)	0.0352 (0.0258)
Urban	-0.0339*** (0.00594)	-0.0167*** (0.00456)	-0.131*** (0.00983)	-0.0949*** (0.00820)	-0.171*** (0.0104)	-0.155*** (0.0107)
Observations	23,437	23,437	23,437	23,437	23,437	23,437
R-squared	0.064	0.022	0.117	0.059	0.116	0.074

All DHS waves; females above 22 years of age with only one union. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Additional controls: ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 12 : Nuptiality and Fertility Timing, Robustness

<i>Dependent variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Married by age 20</i>			<i>Mother by age 20</i>		
Post 1 x Non-Muslim	-0.0753*			-0.114**		
	(0.0451)			(0.0454)		
Post 1,2 x Non-Muslim	-0.140***			-0.168***		
	(0.0443)			(0.0432)		
Post 2 x Non-Muslim	-0.190***			-0.229***		
	(0.0438)			(0.0444)		
Post 1 Intensity x Non-Muslim		-0.00653			-0.00318	
		(0.00408)			(0.00481)	
Post 2 Intensity x Non-Muslim		-0.0118***			-0.0124***	
		(0.00331)			(0.00315)	
Placebo: born after 1950 x Non-Muslim			0.0275			0.0166
			(0.0668)			(0.0630)
Non-Muslim	0.0710*	-0.0182	-0.0335	0.111***	-0.0128	0.00324
	(0.0383)	(0.0286)	(0.0589)	(0.0385)	(0.0292)	(0.0535)
Urban	-0.0533***	-0.171***	-0.168***	-0.0474***	-0.155***	-0.125***
	(0.0146)	(0.0104)	(0.0199)	(0.0140)	(0.0108)	(0.0213)
Wealth index	-0.0103**			-0.00702*		
	(0.00438)			(0.00356)		
Years of education	-0.0404***			-0.0389***		
	(0.001000)			(0.00154)		
Observations	18,274	23,437	5,978	18,274	23,437	5,978
R-squared	0.199	0.116	0.076	0.154	0.074	0.048

All DHS waves; females above 22 years of age with only one union. Columns (3) and (6): individuals older than 22 in 1981. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Post 1 Intensity and Post 2 Intensity are defined as the number of years in the 12-22 range spent under the first and second reform respectively. Additional controls: ethnicity, DHS wave, district, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A1: Education across Ethnic Groups

	Dependent variable: years of education																													
	Kalenjin			Kamba			Kikuyu			Kisii																				
	females	males		females	males		females	males		females	males																			
Post 1 x Non-Muslim	1.140*** (0.255)	0.328 (0.318)		1.048*** (0.241)	-0.347 (0.263)		0.144 (0.246)	-0.641** (0.267)		0.820*** (0.294)	-0.569* (0.337)																			
Post 1,2 x Non-Muslim	0.363 (0.254)	0.598 (0.406)		0.471* (0.254)	-0.434 (0.332)		-0.606** (0.287)	-0.334 (0.317)		1.091*** (0.318)	-0.582 (0.401)																			
Post 2 x Non-Muslim	1.532*** (0.321)	-0.395 (0.559)		2.052*** (0.459)	-1.355*** (0.434)		0.729 (0.503)	-2.006*** (0.390)		1.212** (0.496)	-2.629*** (0.396)																			
Non-Muslim	-1.538* (0.833)	0.848 (1.148)		0.208 (0.454)	1.168** (0.457)		0.946** (0.467)	1.295** (0.518)		-1.363 (1.714)	0.504 (0.749)																			
Urban	2.122*** (0.169)	2.251*** (0.206)		1.717*** (0.147)	1.944*** (0.187)		1.803*** (0.123)	2.029*** (0.147)		2.172*** (0.163)	2.464*** (0.186)																			
Observations	5,852	4,509		5,630	3,943		9,334	6,303		4,362	3,203																			
R-squared	0.397	0.308		0.435	0.328		0.447	0.326		0.427	0.338																			
Pre-reform mean	3.5	5.5		4.4	6.7		6.0	7.8		4.3	7.2																			
	Luhya						Luo						Meru						Mijikenda/Swahili											
	females		males				females		males				females		males				females		males				females		males			
Post 1 x Non-Muslim	0.354 (0.225)	-0.385 (0.274)		0.781*** (0.213)	-0.513* (0.268)		0.704** (0.278)	-0.149 (0.284)		0.704** (0.278)	-0.149 (0.284)		-0.0353 (0.227)	0.691** (0.306)																
Post 1,2 x Non-Muslim	-0.0342 (0.254)	-0.454 (0.371)		0.722*** (0.246)	-0.313 (0.339)		0.238 (0.341)	-0.437 (0.423)		0.238 (0.341)	-0.437 (0.423)		-0.0611 (0.314)	-0.265 (0.338)																
Post 2 x Non-Muslim	0.581* (0.302)	-1.228*** (0.455)		1.623*** (0.310)	-2.556*** (0.380)		1.273*** (0.464)	-0.992** (0.425)		1.273*** (0.464)	-0.992** (0.425)		-0.423 (0.288)	-0.863*** (0.272)																
Non-Muslim	1.075*** (0.313)	0.819** (0.390)		0.525 (0.876)	1.162 (1.050)		3.013*** (0.577)	2.323*** (0.515)		3.013*** (0.577)	2.323*** (0.515)		0.257* (0.153)	0.731*** (0.204)																
Urban	2.097*** (0.118)	2.233*** (0.169)		2.094*** (0.120)	2.410*** (0.155)		1.997*** (0.159)	2.304*** (0.201)		1.997*** (0.159)	2.304*** (0.201)		2.319*** (0.190)	2.301*** (0.196)																
Observations	7,093	5,078		6,308	4,469		4,317	3,272		4,317	3,272		3,701	2,531																
R-squared	0.358	0.258		0.400	0.339		0.405	0.283		0.405	0.283		0.336	0.298																
Pre-reform mean	5.0	7.3		4.4	7.2		4.4	6.2		4.4	6.2		1.5	4.8																

All DHS waves; individuals above 20 years of age. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1968 and 1977, between 1978 and 1985, and after 1986 respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Summary Statistics, Education and Siblings Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
Urban	6722	0.339	0.473	535	0.559	0.497	6187	0.320	0.466
Age	6722	36	13.076	535	37	13.810	6187	36	13.009
Years of education	6722	6.919	4.737	535	3.953	4.598	6187	7.175	4.661
Number of siblings	6722	5.182	3.589	535	4.587	3.697	6187	5.234	3.575
Number of brothers	6722	2.617	2.150	535	2.350	2.210	6187	2.640	2.144
Number of sisters	6722	2.565	2.122	535	2.237	2.104	6187	2.594	2.122

DHS wave 4; females above 20 years of age.

Table A3 : Education and siblings composition

VARIABLES	(1) (2) (3) (4)			
	Brothers		Sisters	
	non-Muslim	DDD	non-Muslim	DDD
Non-Muslim	0.146*** (0.0375)	0.130 (0.111)	0.128** (0.0527)	-0.0772 (0.0988)
Num Siblings		0.818** (0.374)		0.497 (0.369)
Non-Muslim x Num Siblings		0.0327 (0.111)		0.211** (0.0991)
Post x Non-Muslim		-0.0214 (0.153)		0.128 (0.171)
Post x Num Siblings	-0.181*** (0.0557)	0.0848 (0.448)	-0.144** (0.0719)	0.257 (0.455)
Post x Non-Muslim x Num Siblings		-0.172 (0.160)		-0.275 (0.179)
Urban	2.156*** (0.177)	2.200*** (0.159)	2.143*** (0.175)	2.188*** (0.157)
Observations	6,187	6,722	6,187	6,722
R-squared	0.420	0.432	0.420	0.432

DHS wave 4; females above 20 years of age. Post equals 1 for cohorts born after 1968. Additional controls: age, age squared; ethnicity, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

TableA4: FGM across Ethnic Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Dependent variable: 1 if woman underwent FGM</i>							
	Kalenjin	Kamba	Kikuyu	Kisii	Luhya	Luo	Meru	Mijikenda/ Swahili
Post 1 x Non-Muslim	0.0367 (0.0447)	-0.167*** (0.0423)	-0.0397 (0.0410)	0.0195 (0.0308)	0.0238 (0.0212)	0.0205 (0.0218)	-0.109*** (0.0451)	0.0451** (0.0217)
Post 1, 2 x Non-Muslim	-0.0329 (0.0543)	-0.296*** (0.0517)	-0.142** (0.0544)	-0.0318 (0.0572)	0.0465* (0.0251)	0.0413 (0.0280)	-0.143*** (0.0573)	0.0577*** (0.0213)
Post 2 x Non-Muslim	0.0258 (0.0747)	-0.274*** (0.0697)	-0.293*** (0.0712)	0.0162 (0.0767)	0.0756** (0.0375)	0.0341 (0.0387)	-0.409*** (0.0947)	0.0783*** (0.0280)
Non-Muslim	0.236** (0.116)	0.241*** (0.0777)	-0.0680 (0.113)	0.0918 (0.214)	-0.0303 (0.0241)	-0.0434 (0.0262)	-0.0492 (0.0886)	-0.147*** (0.0174)
Urban	-0.0703*** (0.0188)	-0.0440** (0.0181)	-0.103*** (0.0169)	-0.0689*** (0.0140)	-0.0287*** (0.0104)	-0.0421*** (0.0108)	-0.0914*** (0.0179)	-0.0671*** (0.0161)
Observations	3,337	3,193	5,272	2,608	4,110	3,564	2,618	2,044
R-squared	0.366	0.331	0.240	0.704	0.591	0.600	0.353	0.598
Pre-reform mean	0.834	0.552	0.595	0.974	0.013	0.014	0.724	0.006

DHS waves III, IV and V; females above 18 years of age. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1963 and 1972, between 1973 and 1988, and after 1989 respectively. Additional controls: age, age squared, ethnicity; DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Maternal Health across Ethnic Groups

Dependent variable:	Kalenjin		Kamba		Kikuyu		Kisii	
	Prenatal care	Birth in hospital	Prenatal care	Birth in hospital	Prenatal care	Birth in hospital	Prenatal care	Birth in hospital
Birth post 1990 x non-								
Muslim	0.109*** (0.0380)	0.0106 (0.0430)	0.140*** (0.0381)	0.126*** (0.0425)	-0.0388 (0.0333)	0.0257 (0.0349)	0.0531 (0.0384)	0.0625 (0.0414)
Non-Muslim	-0.233*** (0.0817)	-0.344*** (0.125)	-0.117*** (0.0432)	-0.128 (0.0899)	0.0649 (0.0797)	0.159* (0.0882)	-0.0799 (0.0669)	-0.270 (0.212)
Urban	0.101*** (0.0198)	0.289*** (0.0211)	0.0692*** (0.0168)	0.277*** (0.0222)	0.0903*** (0.0137)	0.251*** (0.0201)	0.0967*** (0.0186)	0.310*** (0.0207)
Observations	4,434	5,178	3,748	4,441	5,615	6,386	2,976	3,567
R-squared	0.314	0.176	0.398	0.221	0.187	0.295	0.338	0.206
Pre-reform mean	0.630	0.241	0.571	0.238	0.799	0.566	0.619	0.323
	Luhya		Luo		Meru		Mijikenda/Swahili	
Birth post 1990 x non-								
Muslim	0.0874*** (0.0313)	0.0803** (0.0330)	-0.0540 (0.0338)	-0.0122 (0.0384)	0.00807 (0.0406)	0.123*** (0.0463)	0.166*** (0.0360)	0.0720** (0.0362)
Non-Muslim	-0.0696 (0.0464)	-0.121*** (0.0460)	0.158** (0.0765)	0.0278 (0.110)	0.0826 (0.0779)	0.0175 (0.122)	-0.168*** (0.0323)	-0.0502 (0.0339)
Urban	0.107*** (0.0172)	0.267*** (0.0174)	0.104*** (0.0149)	0.254*** (0.0181)	0.0949*** (0.0179)	0.276*** (0.0226)	0.113*** (0.0195)	0.283*** (0.0197)
Observations	5,517	6,518	4,961	5,937	2,702	3,213	2,494	3,068
R-squared	0.352	0.179	0.188	0.262	0.229	0.277	0.393	0.255
Pre-reform mean	0.563	0.311	0.740	0.494	0.806	0.558	0.421	0.156

All DHS waves; births occurred between 1984 and 2009. Additional controls: ethnicity, DHS wave, province, respondent year of birth, and childbirth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A6 : Fertility Timing across Ethnic Groups

Dependent variable: Mother by age 20

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Kalenjin	Kamba	Kikuyu	Kisii	Luhya	Luo	Meru	Mijikenda/ Swahili
Post 1 x Non-Muslim	-0.0469 (0.0389)	-0.141*** (0.0357)	-0.137*** (0.0363)	-0.0935** (0.0415)	-0.145*** (0.0409)	-0.0741** (0.0352)	-0.0243 (0.0456)	-0.0350 (0.0421)
Post 1,2 x Non-Muslim	-0.180** (0.0762)	-0.184*** (0.0469)	-0.189*** (0.0494)	-0.137** (0.0643)	-0.171*** (0.0527)	-0.127** (0.0484)	-0.0676 (0.0682)	-0.160*** (0.0459)
Post 2 x Non-Muslim	-0.277*** (0.0874)	-0.288*** (0.0519)	-0.307*** (0.0561)	-0.290*** (0.0874)	-0.236*** (0.0659)	-0.234*** (0.0512)	-0.0592 (0.0827)	-0.109** (0.0494)
Non-Muslim	0.185 (0.200)	-0.0758 (0.0663)	-0.0889 (0.0728)	-0.388*** (0.102)	0.0587 (0.0642)	0.219 (0.140)	-0.509*** (0.0981)	0.0413 (0.0302)
Urban	-0.122*** (0.0270)	-0.0712*** (0.0258)	-0.135*** (0.0197)	-0.117*** (0.0268)	-0.131*** (0.0213)	-0.144*** (0.0199)	-0.0601** (0.0261)	-0.109*** (0.0262)
Observations	4,030	3,598	6,610	2,895	4,766	4,241	2,868	2,232
R-squared	0.080	0.072	0.074	0.088	0.068	0.109	0.090	0.093
Pre reform mean	0.625	0.624	0.586	0.635	0.672	0.741	0.534	0.610

All DHS waves; females above 22 years of age with only one union. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Additional controls: ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Summary Statistics, Decision Making, Attitudes and Domestic Violence Sample

	Total			Muslims			Non-Muslims		
	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.	No. Obs.	Mean	Std. Dev.
Husband has final say on spending wife's earnings	4442	0.072	0.259	230	0.096	0.295	4212	0.071	0.257
Husband has final say on wife's health	4442	0.208	0.406	230	0.252	0.435	4212	0.206	0.404
Husband has final say on large purchases	4442	0.292	0.455	230	0.326	0.470	4212	0.290	0.454
Husband has final say on daily purchases	4442	0.121	0.327	230	0.222	0.416	4212	0.116	0.320
Husband has final say on visits to family members	4442	0.192	0.394	230	0.300	0.459	4212	0.186	0.389
Husband has final say on food to be prepared	4442	0.037	0.190	230	0.087	0.282	4212	0.035	0.183
Respondent has ever experienced	6335	0.384	0.486	488	0.244	0.430	5847	0.396	0.489
spousal violence	6335	0.127	0.333	488	0.096	0.295	5847	0.130	0.336
minor	6335	0.146	0.353	488	0.088	0.284	5847	0.151	0.358
severe	6335	0.133	0.340	488	0.096	0.295	5847	0.136	0.343
sexual	6335	0.050	0.217	488	0.051	0.221	5847	0.049	0.217
with physical consequences	6335	0.020	0.141	488	0.031	0.173	5847	0.019	0.138
father	6335	0.001	0.028	488	0.004	0.064	5847	0.001	0.023
brother									
father in law									
Respondent has ever been hurt by	8914	0.551	0.497	666	0.590	0.492	8248	0.548	0.498
justified in some cases									
nr of reasons for which									
justified	8914	1.529	1.717	666	1.824	1.850	8248	1.505	1.703
Attitude towards wife beating (*)									
justified in some cases	4195	0.955	0.206	270	0.959	0.198	3925	0.955	0.207
nr of reasons for which									
justified	4195	3.262	1.054	270	3.263	1.046	3925	3.262	1.055
Refusing sex with husband (*)									
justified in some cases									
nr of reasons for which									
justified									

DHS waves IV and V; females above 22 years of age with only one union. (*): only DHS wave V.

Table A8: Decision Making

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Dependent variable:</i>	<i>Husband alone has the final say</i>						
	<i>spending wife's earnings</i>	<i>wife's health</i>	<i>large purchases</i>	<i>daily purchases</i>	<i>visits to family members</i>	<i>food</i>	<i>Decision making: summary measure</i>
Post 1 x Non-Muslim	-0.103** (0.0458)	-0.288*** (0.0666)	-0.249** (0.103)	-0.318*** (0.0667)	-0.231*** (0.0458)	-0.0983** (0.0438)	0.575*** (0.116)
Post 1,2 x Non-Muslim	-0.116*** (0.0294)	-0.263*** (0.0318)	-0.278*** (0.0939)	-0.263*** (0.0647)	-0.180*** (0.0500)	-0.113*** (0.0378)	0.548*** (0.112)
Post 2 x Non-Muslim	-0.0991** (0.0443)	-0.310*** (0.0647)	-0.325*** (0.103)	-0.326*** (0.0788)	-0.186** (0.0712)	-0.125** (0.0526)	0.614*** (0.144)
Non-Muslim	0.0893*** (0.0262)	0.295*** (0.0321)	0.289*** (0.0895)	0.277*** (0.0566)	0.218*** (0.0412)	0.0910*** (0.0298)	-0.556*** (0.100)
Urban	-0.0268*** (0.00702)	-0.0899*** (0.0169)	-0.102*** (0.0184)	-0.0511*** (0.0126)	-0.0694*** (0.0161)	-0.0187** (0.00804)	0.154*** (0.0231)
Observations	4,442	4,442	4,442	4,442	4,442	4,442	4,442
R-squared	0.027	0.093	0.081	0.059	0.091	0.034	0.109

DHS waves IV and V; females above 22 years of age with only one union. Column (7): summary measure a la Kling, Liebman and Katz (2007). Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A9 : Domestic Violence

Dependent variable:	Ever experienced spousal violence				Ever hurt by			Domestic violence: summary measure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	minor	severe	sexual	w. physical consequences	father	brother	father in law	
Post 1 x Non-Muslim	0.175 (0.147)	-0.0147 (0.0303)	-0.0888* (0.0450)	0.0771 (0.140)	-0.0406 (0.0247)	-0.0238 (0.0159)	-0.00796 (0.00605)	-0.0332 (0.132)
Post 1,2 x Non-Muslim	0.149 (0.149)	-0.0831*** (0.0297)	-0.101** (0.0415)	0.0549 (0.140)	-0.0906*** (0.0321)	-0.0240 (0.0165)	-0.00274 (0.00502)	-0.106 (0.133)
Post 2 x Non-Muslim	0.225 (0.148)	-0.0291 (0.0318)	-0.121*** (0.0413)	0.0978 (0.143)	-0.0618* (0.0336)	0.00502 (0.0156)	0.00107 (0.00467)	0.0104 (0.134)
Non-Muslim	-0.114 (0.145)	0.0672** (0.0272)	0.119*** (0.0381)	-0.0639 (0.141)	0.0552** (0.0270)	0.0103 (0.0156)	0.00189 (0.00561)	0.0746 (0.130)
Urban	-0.0540*** (0.0173)	-0.0140 (0.0126)	-0.0174* (0.00998)	-0.0281 (0.0181)	0.00871 (0.00953)	-0.00154 (0.00459)	-0.000427 (0.000931)	-0.0402* (0.0215)
Observations	6,335	6,335	6,335	6,335	6,335	6,335	6,335	6,335
R-squared	0.082	0.054	0.034	0.039	0.030	0.018	0.011	0.065

DHS waves IV and V; females above 22 years of age with only one union. Column (8): summary measure a la Kling, Liebman and Katz (2007). Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Women's Attitudes

<i>Dependent variable:</i>	(1)		(2)		(3)		(4)	
	<i>justified in some cases</i>	<i>Wife beating</i>	<i>nr of reasons for which it is justified</i>	<i>justified in some cases</i>	<i>Refusing sex with husband</i>	<i>justified in some cases</i>	<i>nr of reasons for which it is justified</i>	
Post 1 x Non-Muslim	0.0884 (0.0908)		0.206 (0.465)	0.0470* (0.0240)	0.280 (0.246)			
Post 1,2 x Non-Muslim	0.0315 (0.0931)		0.0427 (0.459)	0.0215 (0.0141)	0.326 (0.239)			
Post 2 x Non-Muslim	0.146 (0.0930)		0.441 (0.461)	0.0655 (0.0473)	0.366 (0.284)			
Non-Muslim	-0.0927 (0.0879)		-0.260 (0.457)	-0.0209 (0.0146)	-0.152 (0.242)			
Urban	-0.150*** (0.0171)		-0.555*** (0.0473)	0.00294 (0.00923)	0.0696 (0.0572)			
Observations	8,914		8,914	4,195	4,195			
R-squared	0.151		0.156	0.106	0.074			

DHS wave V; females above 22 years of age with only one union. Post 1, Post 1,2 and Post 2 equal 1 for cohorts born between 1959 and 1968, between 1969 and 1978, and after 1979 respectively. Additional controls: age, age squared; ethnicity, DHS wave, province, and birth year fixed effects; province x time trend, ASAL x time trend. Standard errors clustered at the religious group x birth year level. *** p<0.01, ** p<0.05, * p<0.1.