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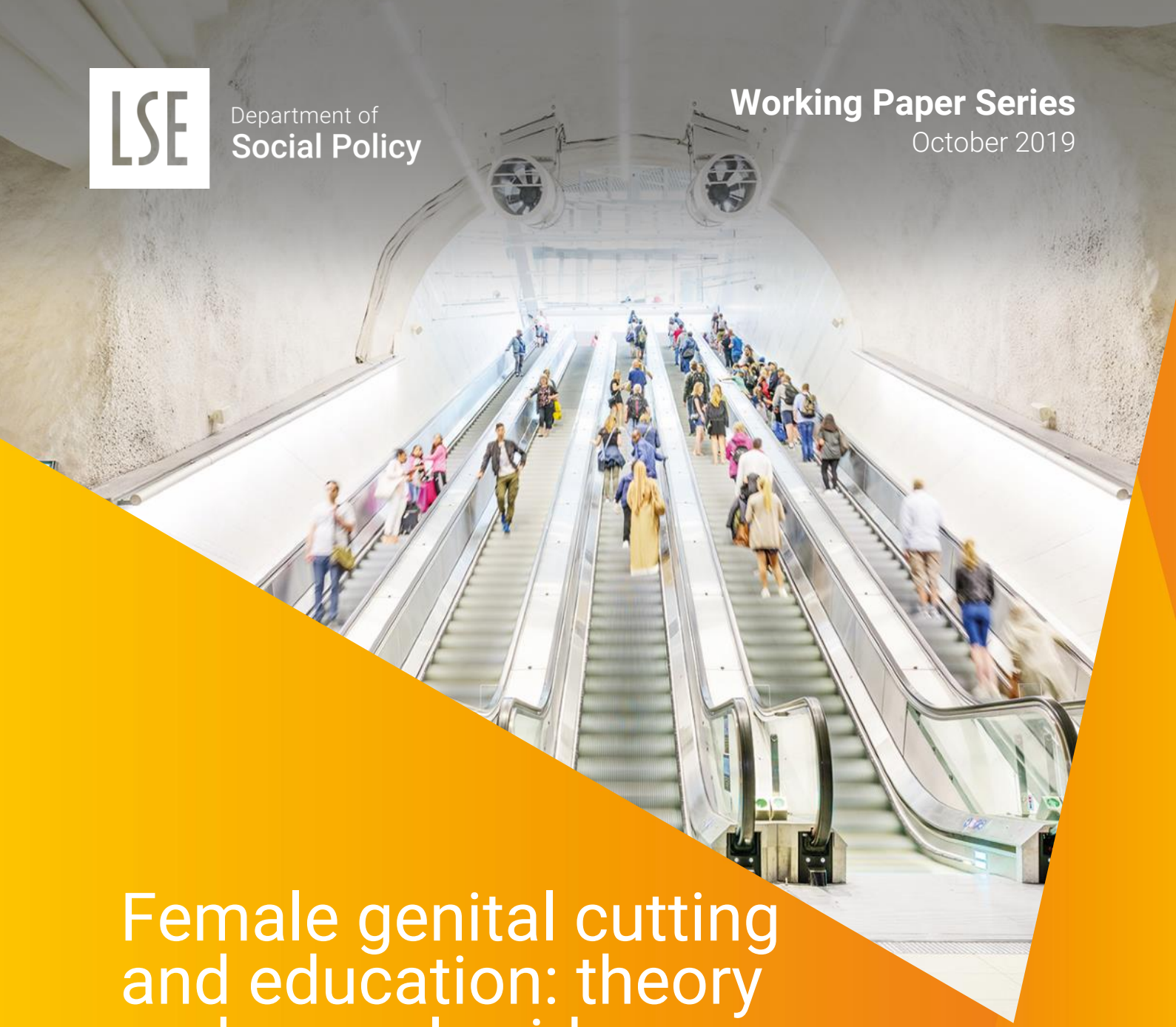

Female genital cutting and education: theory and causal evidence from Senegal

Working Paper 10-19

Jorge García Hombrados and Edgar Salgado

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Abstract

We use across-ethnic-group variation in exposure to a law that banned the practice of female genital cutting (FGC) in Senegal to evaluate the impact of the law and to document the perverse effect that this cultural practice has on girls' education. We find that the law, interpreted as a rise in the cost of FGC, reduced the prevalence of FGC, which later increased educational investments received by girls. To explain this result, we propose a theoretical model where, consistent with previous evidence, both education and FGC lead to better marriage market outcomes. In line with the predictions of the model, the results suggest that education and FGC work as substitutes in the marriage market and that educational investments are affected by the cost of alternative pre-marital investments. Additionally, we rule out alternative mechanisms, such as a broader change in gender norms, better health or fewer adolescent women leaving school to get married, to explain why the law increased educational investments. Keywords: Female genital cutting, education, harmful traditions.

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Introduction

The practice of female genital cutting (FGC)[1] affects 200 million women worldwide (UNICEF, 2016). Although many governments and international organizations have mobilized large amounts of resources in the last decades to fight FGC (UNICEF, 2016), the practice remains a social norm in many countries, affecting around three million girls annually (Bellemare et al., 2015).

Despite the attention gathered from policy-makers and the civil society, there is scarce evidence on the long-term welfare consequences of this cultural practice. This study advances the literature on the consequences of FGC by documenting, for the first time, the perverse effect that this cultural practice has on education. There are two main reasons to study the link between FGC and education. First, efforts to eradicate FGC documented in the literature generally highlight the prominent role of education as an instrument to prevent it or to change attitudes towards it (UNICEF, 2016). Yet, recent evidence shows that education has limited effectiveness in addressing this problem (De Cao and La Mattina, 2019). In this paper, we show that, in our setting, the causal relationship between FGC and education is in the opposite direction, namely FGC causes a decline in education.

Second, current research on the consequences of FGC has mainly focused on its effect on health, documenting a lack of association with health impairment or fertility, while some relation to worse reproductive and sexual health, especially higher prevalence of sexually transmitted diseases, genital sores or hard labour at birth (Berg and Underland, 2013; Berg et al., 2014; Wagner, 2015). However, because the reasons for FGC are socioeconomic rather than medical, and some studies suggest it plays an important role in marriage markets (Mackie, 1997; Kolawole and Van der Kwaak, 2010; Karumbi et al., 2017), we explore its interaction with another marriage market investment: education.

But how would FGC interact with education? Among ethnic groups practicing FGC, women who have undergone FGC are perceived as purer and more loyal. Karumbi et al. (2017) and Kolawole and Van der Kwaak (2010) review several qualitative studies on different sub-Saharan countries and conclude that, in those communities where this practice is common, FGC increases marriage payments received by the parents of the bride, also known as the bride price, and it also seems to improve marriage prospects. Wagner (2015) finds empirical evidence consistent with this argument. Using data from Demographic and Health Surveys (DHS) from 13 African countries, Wagner shows that, conditional on ethnic group, village and age, women who have undergone FGC are more likely to be married. Boyden et al. (2013) reach the same conclusion using qualitative data from Ethiopia. Mackie (1997) documents that, in some societies, FGC works as a precondition of marriage. Chesnokova and Vaithianathan (2010) and Molitor (2014) propose theoretical models where the expectation that FGC will lead to a better marital outcome perpetuates the

[1] Although the practice is also called female genital mutilation, or female circumcision, we opt for the more general term cutting. It is defined as the ritual cutting of some or all of the external female genitalia for reasons unrelated to health (see https://www.who.int/news-room/fact-sheets/detail/female-genital-mutilation)

practice as a social norm. Allen et al. (2015) document that infibulation, the most aggressive type of FGC,[2] is practiced among some ethnic groups of Senegal to ensure a bride's virginity at marriage. Thus, in some aspects, the effects of FGC on marriage market outcomes resemble those reported for education,[3] in particular, those where education has been found to causally increase the bride price, for example, in Zambia and Indonesia (Ashraf et al., 2019), or, more generally, to improve the quality of the match in the marriage market (Boulier and Rosenzweig, 1984; Chicoine, 2018; Hahn et al., 2018). In this context, FGC and education may work as substitutes in the marriage market and, given the high relative price of education, FGC is potentially preferred as an investment.

We use the introduction of a law in January 1999 that banned the practice of FGC in Senegal as a natural experiment to investigate, for the first time in the literature, the causal link between FGC and the education attained by women and girls. In a context where the majority of the cuts occur during infancy and where FGC is deeply rooted in the tradition of some ethnic groups, our difference-in-difference strategy compares women and girls born before and after the introduction of the law who belong to ethnic groups where the practice of FGC is common with those from ethnic groups where FGC has never been practiced. Our results show that the introduction of the law was followed by a reduction in the probability of experiencing FGC and an increase in the extensive margin of education. The exogenous variation in the probability of undergoing FGC, generated by this law, is then used as an instrumental variable to estimate the causal effect of FGC on education. A reduction of 10 percentage points in the prevalence of FGC (mean is 32 percent) led to a reduction of 2.6 percentage points in the probability of never having attended school (mean is 36 percent). However, we find limited effects of FGC on the intensive margin (i.e. years of schooling). While FGC decreases substantially the number of women receiving one or two years of education, it does not seem to have an effect on the probability of achieving three or more years of education.

The results of different falsification tests suggest that estimates are unlikely to be driven by differential trends in FGC or educational investments across different ethnic groups before the introduction of the law or by larger levels of under-reporting of FGC among younger cohorts of girls. The results are also robust to the use of different analytical samples and regression specifications, to the exclusion of areas where a non-governmental organization (NGO) has implemented a large anti-FGC project and to a placebo test examining the effects of the law on the education of boys who have no sister born after the introduction of the law.

To explain the effect of FGC on education, we propose a marriage market model based on previous economics and anthropological studies where FGC and education are pre-marital investments. In the model, parents are imperfectly altruistic and both FGC and education convey a higher bride

[2] Infibulation or Type III FGC is defined as the narrowing of the vaginal opening through the creation of a covering seal. The seal is formed by cutting and sewing over the outer labia, with or without removal of the clitoris or inner labia.
[3] Several studies have documented the role of education as a pre-marital investment in the marriage market. See, for example, the influential work by Chiappori et al. (2009) and Lafortune (2013).

price and better marriage market outcomes. In line with the predictions of the model and the hypothesis that FGC and education work as substitutes in the marriage market, we find that the effect of reducing FGC on education is larger in areas where FGC is more valued in the marriage market.

We then investigate three alternative mechanisms for the result: (1) a broader change in gender norms following the awareness campaigns associated with the intervention; (2) an increase in returns to schooling after improvements in the health conditions of girls; (3) a decrease in early marriage among those girls and women from FGC groups born after the introduction of the law. We do not find any evidence supporting any of these mechanisms, which provides additional support for the substitution hypothesis.

This study makes three contributions. First, we add to the emerging literature that investigates whether legal changes can be effective instruments to address harmful traditional practices in many developing countries (Fenske, 2012; Crisman et al., 2016; Garcia-Hombrados, 2017), a topic that has recently attracted the attention of researchers and policy-makers (Acemoglu and Jackson, 2017). The results suggest that laws banning FGC could contribute to reducing this practice. This finding is of particular interest for policy-making because it can help to explain why FGC has decreased remarkably in some countries, such as Kenya or Burkina Faso, while it has remained constant in many others. It also shows that the success of certain social policies is not independent from their interaction with cultural norms (Ashraf et al., 2019). Second, we explore and document the perverse effect of FGC on education in Senegal, suggesting that reducing this practice could benefit women's human capital accumulation in countries where this practice is prevalent. Finally, we contribute to the literature that explores the nature, origin and persistence of this practice (Bellemare et al., 2015; Efferson et al., 2015; Wagner, 2015; Novak, 2016; Poyker, 2016; Vogt et al., 2016; Becker, 2018; Harari, 2019 and Diabate and Mesple-Somps, 2019) by providing theoretical and empirical insights into how FGC interact with other pre-marital investments.

The rest of the paper is structured as follows. In Section I, we present the conceptual framework and the theoretical model linking education, FGC and marriage markets. Then, in Section II, we discuss the tradition of FGC in Senegal and we summarise the key aspects of the law that banned its practice. Next, we introduce the data used in the analysis in Section III and the empirical strategy in Section IV. In Section V, we present the results of the analyses on the effects of the law and the estimated impact of FGC on educational investments, testing the different predictions of the model. In Section VI, we explore the feasibility of alternative mechanisms for explaining the effect of the law on education. We conclude in Section VII.

Section I: Theoretical Framework

In this section, we extend the theoretical framework depicted in Ashraf et al. (2019) to include FGC as an additional choice variable to maximize bride price and capture material surplus in the marriage market.

In the model, parents enjoy utility from consumption and a child's utility. They are imperfectly altruistic and have only one child in a two-period setting. A bride price (*BP*) is transferred from the groom to bride's parents in the marriage market. This assumption is realistic in the Senegalese context, where, according to the Murdock Ethnographic Atlas, the payment of a bride price is a common practice among the main six ethnic groups in Senegal. A daughter's utility is denoted by *v* and is discounted in the parents' utility by $\gamma \in [0.1]$, while a son's utility is denoted by *u* and is discounted by $\delta \in [0.1]$ in the parents' utility. Parents' income in both periods is y, and *i* and *j* are indices for daughters and sons, respectively.

Investment decisions are made in the first period while marriage decisions take place in the second period. In the first period, the parents of daughters choose consumption c1and decide whether to educate them ($S_i \in [0, 1]$) and/or subject them to FGC ($F_i \in [0, 1]$). This is particularly relevant for Senegal, where FGC typically occurs during infancy and is entirely a parental decision (Yoder and Wang, 2013). Parents with sons also choose how much to consume and whether to educate their sons ($P_i \in 0, 1$). Education has a cost k, which is the same for sons and daughters. FGC has a cost σ.[4] The ability of daughters (a_i) and sons (a_j) is distributed according to a unimodal probability density $g(\bullet)$ with a cumulative distribution $G(\bullet)$. Ability affects the first-period utility of the child interacted with the schooling investment ($a_i S_i$ for daughters and $a_j P_j$ for sons).

In the second period, parents consume c_2, children marry and transfers are made. Additionally, the groom pays BP_e to the parents of the bride as a marriage transfer.

Maximization Problem

Parents choose whether to educate and/or subject their daughter to FGC to maximize their utility:

$$\max_{S_i, F_i \in \{0,1\}, c_1, c_2} \quad c_1 + \frac{c_2}{1+r} + \gamma \left[a_i S_i + \frac{v_2(S_i, BP_e)}{1+r} \right]$$
$$\text{s.t.} \quad c_1 + k \cdot S_i + \sigma \cdot F_i \leq y \quad \text{and} \quad c_2 \leq y + BP_e.$$

[4] The monetary cost of cutting, a fine, or time in prison, which reduces time devoted to work and then to consume.

For a son, parents maximize:

$$\max_{S_j \in \{0,1\}, c_1, c_2} \quad c_1 + \frac{c_2}{1+r} + \delta \left[a_j P_j + \frac{u_2(S_j, BP_e)}{1+r} \right]$$
$$\text{s.t.} \quad c_1 + k \cdot S_j \leq y \quad \text{and} \quad c_2 \leq y + BP_e.$$

Marriage Market

A single man's value in the marriage market is $\boldsymbol{\zeta}\frac{\boldsymbol{m}}{\boldsymbol{j}}$, while a woman's value is $\boldsymbol{\zeta}\frac{\boldsymbol{f}}{\boldsymbol{i}}$. Note that $\boldsymbol{\zeta_{ij}}$ is the marriage value and none of these values depend on ethnicity. Marriage surplus is defined as $\boldsymbol{z_{ij} = \zeta_{ij} - \zeta_i^f - \zeta_j^m}$, which depends on education: $\boldsymbol{z_{S_i P_j} = \zeta_{S_i P_j} - \zeta_{P_j}^m}$. As is standard in the literature, we assume that marriage surplus increases with education and that there is complementarity between spouses' educational attainment. Additionally, we assume that FGC materializes its returns only in marriage, so single FGC women have zero gain from FGC.

We consider the stable equilibrium defined in Chiappori et al. (2009) where there are more educated men than women, which implies that educated men marry only educated women, that some educated men marry uneducated women and that uneducated men marry only uneducated women.

Let $V_{i,e}$ be the material surplus that women or their parents receive in equilibrium in the marriage market. Let $\Delta V_{i,e}^S = V_{i,e}(S_i = 1) - V_{i,e}(S_i = 0)$ be the material surplus in the marriage market generated by education and $\Delta V_{i,e}^F = V_{i,e}(F_i = 1) - V_{i,e}(F_i = 0)$ the material surplus in the marriage market generated by FGC. Total returns to schooling enjoyed by daughters are $v_2(S_i = 1) - v_2(S_i = 0) = R^f + (1 - I_e)\,\Delta V_{i,e}^S$ while FGC returns are $v_2(F_i = 1) - v_2(F_i = 0) = \Delta V_{i,e}^F$.[5] Parents enjoy returns through the bride price for each investment decision: $BP(S_i = 1) - BP(S_i = 0) = \Delta V_{i,e}$ and $BP(F_i = 1) - BP(F_i) = \Delta V_{i,e}^F$.

[5] It would be possible to assume that $\Delta V_{i,e}^F$ enters a daughter's utility discounted by a parameter κ that reflects future costs (i.e. health worsens after FGC): $(1 = \kappa)\Delta V_{i,e}^F$. For simplicity, we assume $\kappa = 0$. This assumption does not change the predictions derived from the model.

FGC and investment in female education

Substituting the budget constraints into the objective function, we find that parents choose to educate their daughter if her ability exceeds a certain threshold:

$$a_i \geq a_{I_e}^*(k,\sigma) = \frac{k}{\gamma} - \frac{\sigma}{\gamma}\left[\frac{\gamma R^f + \Delta V_{i,e}^S\{1+I_e[(1-\gamma)/\gamma]\}}{\Delta V_{i,e}^F\{1+I_e[(1-\gamma)/\gamma]\}}\right] \quad \text{(I.1)}$$

This is the main expression from which we derive our predictions.

First, what is the effect of FGC on female education? If FGC has any value in the marriage market, $\Delta V_{i,e}^F > 0$ and regardless of education's value in the marriage market $\left(\Delta V_{i,e}^S\right)$ and the market returns to education (R^f), our model predicts fewer educated daughters. Let us consider the case that FGC has no value in the marriage market: if $\Delta V_{i,e}^F$ is very close to zero, then the second term in equation (I.1) becomes very large, which leads to a very small ability threshold, in which case, the probability of receiving education is very large. However, for larger values of $\Delta V_{i,e}^F$, the ability threshold $\left(a_{I_e}^*(k,\sigma)\right)$ increases, which reduces the probability that a daughter is educated. This is summarised in our first prediction, as follows:

Prediction 1. *In contexts where FGC has any value in the marriage market, the practice of FGC reduces the probability that a daughter receives an education.*

Cost of FGC and female education

The second prediction derived from our model is based on how the probability of being educated is related to the associated cost of FGC. It is worth noting that the probability that daughter i is educated is given by $\Pr(S_i = 1|k,\sigma,I_e) = \Pr\left(a_i \leq a_{I_e}^*(k,\sigma)\right) = 1 - \mathrm{G}\left(a_{I_e}^*(k,\sigma)\right)$. Thus, an increase in the cost of FGC is associated with a smaller ability threshold: $a_{I_e}^*(k,\sigma_1) < a_{I_e}^*(k,\sigma_2)$. Here, $\sigma_1 > \sigma_2$, which ultimately results in a higher probability of being educated: $Pr(S_i = 1|\sigma_1, I_e, k) > Pr(S_i = 1|\,\sigma_2, I_e, k)$. This can be summarised in our second, and main prediction.

Prediction 2. *A rise in the cost of FGC increases the probability that a daughter is educated.*

Relative gain

The model can also be used to test how the effect of increasing the cost of FGC interacts with the intensity with which the practice is substituted by education. We denote the change in the material surplus in the marriage market derived from the education map into the material surplus derived

from FGC as $\Delta V_{i,e}^{F} = \theta \Delta V_{i,e}^{S}$. If $\theta < 1$, then the gains in the material surplus derived from education are only a fraction of the material surplus derived from FGC. We can re-express equation (I.1) as

$$a_i \geq a_{I_e}^{*}(k,\sigma) = \frac{k}{\gamma} - \frac{\sigma}{\gamma}\left[\frac{\gamma R^{f}}{\theta \Delta V_{i,e}^{S}\{1+I_e[(1-\gamma)/\gamma]\}} + \frac{1}{\theta}\right] \quad \text{(I.2)}$$

To illustrate the point, consider two contexts: one where the material surplus derived from education is exactly the same as the material surplus derived from FGC; and one in which education increases the material surplus just by a fraction of what FGC does. This can be represented by θ = 1 and θ < 1, respectively:

$$\frac{\partial \Pr(S_i = 1|\sigma,(\theta=1),I_e,k)}{\partial \sigma} < \frac{\partial \Pr(S_i = 1|\sigma,(\theta<1),I_e,k)}{\partial \sigma}$$

This is summarised in our third prediction.

Prediction 3. *An increase in FGC cost has a larger effect in contexts where the material surplus derived from FGC is higher than the material surplus derived from education.*

Intuitively, in contexts where FGC is the preferred option to extract material surplus (i.e. areas where the value of FGC in the marriage market is larger), an increase of the cost of FGC has to be compensated by larger investments in education.

Zero gain for daughters

Different studies suggest that women who have undergone FGC are perceived as more suitable for marriage (Kolawole and Van der Kwaak, 2010; Karumbi et al., 2017), which can lead to better outcomes for them in the marriage market. Therefore, the model assumes so far that women who have undergone FGC achieve a higher utility of marriage. However, one can argue that, in the absence of causal literature examining this link, this assumption is not straightforward and FGC might not improve the well-being of a daughter within marriage. In this subsection, we show that FGC can still exist and curb education investments, even if it does not affect a daughter's utility. We assume now that $v_2(F_i = 1) - v_2(Fi = 0) = 0$, but this still conveys a positive payment as a bride price, $BP(F_i = 1) - BP(F_i) = \phi$. Equation (I.1) can be re-expressed as

$$a_i \geq a_{I_e}^{*}(k,\sigma) = \frac{k}{\gamma} - \frac{\sigma \Delta V_{i,e}^{S}}{\gamma \phi} - \frac{R^{f}}{1+r} - \frac{(1-I_e)\Delta V_{i,e}^{S}}{1+r} \quad \text{(1.3)}$$

Equation (I.3) indicates that for any given $\phi \neq 0$, an increase in the cost of σ reduces the ability threshold, which leads to an increase in the probability of receiving an education. This is summarised in our Prediction 4.

Prediction 4. *If FGC has no effect on a daughter's utility, but is still part of a strategy to increase her bride price in the marriage market, then an increase in the associated cost of FGC increases the probability of the daughter receiving an education.*

Based on the roles of FGC and education in the marriage market documented in previous studies, the model provides a set of predictions on the link between education and FGC. Although the predictions are the same even if FGC has no material effect on a daughter's utility or if FGC negatively affects a daughter's utility by affecting her health, the model does not aim to represent every potential channel through which FGC affects women's welfare, but only the most evident ones that act through the marriage market. Nonetheless, in Section VI, we investigate empirically the feasibility of alternative mechanisms for explaining the effect observed, and we find no evidence for any of them.

Finally, despite the availability of qualitative and empirical evidence for other settings suggesting that FGC leads to a higher bride price as well as better outcomes in the marriage market, we lack data to further test this. In particular, we are prevented from estimating the effects of FGC on these outcomes because of the lack of data on bride price, and the fact that most girls and women born after the introduction of the law in Senegal are still very young and remained unmarried by the time of the survey. The availability of more data in the future will allow us to quantify the effects of this practice on the marriage market.

Section II: FGC in Senegal and the Introduction of the Ban

The practice of FGC is widespread in West African countries, where its prevalence ranges between 3 percent in Niger and 99 percent in Guinea. In Senegal, approximately 25 percent of women aged 15–49 have declared that they have undergone FGC (Allen et al., 2015). The practice is embedded in the tradition of many ethnic groups such as the Soninke, Mandingue, Diola and the Poular, while it is sparse among the Wolof and the Serer people. Unlike in most East African countries (e.g. Kenya, Egypt and Tanzania), FGC in Senegal is mainly conducted during infancy or early childhood. Yoder and Wang (2013) show that 61 percent of FGC occurs before the end of the first year of life and that FGC is infrequent after early childhood. In our data, less than 3 percent of the women (accounting for less than 10 percent of the women who had undergone FGC) experienced FGC after the age of 6. These patterns do not change substantially across ethnic groups. The DHS data used in this study show that the first year of life is the most common age for FGC among all ethnic groups in Senegal, and the percentage of women who experienced FGC after the age of 6 across the different ethnic group's ranges between 0 percent and 7 percent.

The World Health Organization (WHO) defines four types of FGC, depending on the extent of genital tissue cut. Type I, or cloridectomy, is the partial or total removal of the clitoris. Type II, or

excision, is the partial or total removal of the clitoris and the labia minora, with or without excision of the labia majora. Type III, or infibulation, is the more aggressive practice, and consists of the narrowing of the vaginal opening through the creation of a covering seal. Type IV includes other harmful procedures to the female genitalia for non-medical procedures. Although most FGC conducted in Senegal could be classified as type II FGC, type III cuttings exist among Soninke and Mandingue women (Allen et al., 2015). Regardless of the specific FGC type, most of the cuts are performed, without anesthetics, by traditional practitioners who have little knowledge of female anatomy and use crude, unsterile instruments (UNICEF, 2013; Berg et al., 2014).

With the increasing awareness of the health risks and discriminatory nature of FGC, tackling this practice has been at the top of Senegal's policy agenda for decades. The flagship measure of the government was the approval of Law No. 99-05 that sanctions those who provoke sexual mutilations or give instructions for their commission with six months to five years in prison, or hard labour for life if FGC results in death. The law was enacted on 29 January 1999, following the anti-FGC speech of US First Lady Hillary Clinton in Senegal, and after nine months of intense campaigning led by different Senegalese civic organizations. Details about the context and enforcement of this law are provided in Shell-Duncan et al. (2013). The study concludes that although some politicians and social groups lobbied against the new regulation, the government made an effort to raise awareness about the law.[6] Indeed, using 2004 data from remote rural locations, Shell-Duncan et al. (2013) find that knowledge of the existence of the law was widespread. Most of the people learned of the law by listening to radio broadcasts, by attending community meetings or by word of mouth, although their knowledge of key aspects of the legislation was superficial.[7] Following the introduction of the law, the government conducted some high-profile arrests and convictions of FGC perpetrators, which received widespread media coverage. Although not many people were prosecuted for breaking the law (Kandala and Komba, 2015), the strategy of the government established a lasting fear of prosecution (Shell-Duncan et al., 2013). Shell-Duncan et al. (2013) also suggest that the law was particularly effective in communities where FGC had been actively contested, whereas in more traditional communities the impact could have been smaller because many households continued the practice underground. We examine the effectiveness of the law empirically in Section V. In Sections IV and V, respectively, we discuss and rule out – as potential limitations of the results – the possibility that the results are simply capturing an eventual effect of the law on misreporting of the FGC status and the existence of anticipation effects of the law.

[6] The introduction of the law was accompanied by a media campaign to raise awareness of the law and of the negative consequences of FGC. Although it is beyond the scope of the study to understand whether the effect was driven by the legal change or by the campaign itself, the existing evidence suggests that mass campaigns promoting behavioural change in developing countries, concerning issues such as sexual practises, fertility, child rearing or gender violence, have little or no effect (Garcia-Moreno et al., 2015; Krishnaratne et al., 2016; Banerjee et a., 2017.

[7] The findings of the study suggest that most individuals do not know key aspects of the law, such as the penalty for ordering or committing FGC.

Section III: Data

The main analysis of the paper is conducted using the 2016, 2015, 2014, 2012 and 2010 cross-sectional rounds of the Senegalese DHS. The DHS are nationally representative surveys that include individual- and household-level modules that collect rich information on health and demographic characteristics. They have been implemented in more than 100 low- and middle-income countries across the world for more than three decades, and the high quality of their data is discussed in Pullum (2008). DHS questionnaires include information on basic demographic and educational outcomes for every member of the household. Furthermore, information on different health outcomes is provided for a sub-sample of the household members, which vary in every survey. The 2010–2016 rounds of the DHS include a module on FGC. This module is designed to collect information on the FGC status of every female aged 0–49.[8] The information on whether a woman has undergone FGC is self-reported for respondents aged 15 or older, whereas this is reported by the mother for girls younger than 15 at the time of the survey. When conducting the survey, enumerators are instructed to seek privacy and to avoid the presence of other members of the family in the same room, although this might not always be possible (ICF, 2018).

While information on FGC is comprehensive, these datasets only include information on anthropometric and health variables for children younger than 5. Also, information on marital status and other marital outcomes is available only for individuals aged 15 or older.

In total, the 2016, 2015, 2014, 2012 and 2010 Senegalese DHS databases include information from 25,256 households. The analysis is, however, based on 28,425 females from these households born since January 1990 who, by the time of the survey, were at least seven years old. We restrict the analysis to females born since 1990 because the accuracy of the information on date of birth is likely to be higher among younger cohorts. However, the selection of the exact birth date used as a threshold for being excluded from the sample is arbitrary and, for this reason, we also examine the robustness of the results to the inclusion of older cohorts of women. There are two reasons to focus the analysis on those girls and women aged 7 or older. First, this is the legal age for starting school in Senegal. Second, because the vast majority of FGC occurs during infancy or early childhood, [9] setting the threshold at the age of 7 avoids the possibility that the reduction in the prevalence of FGC is simply capturing the fact that many girls in the sample have yet to undergo FGC by the time of the survey. In Section V, we also provide evidence that the results do not change in any relevant way when alternative ages are used as thresholds for being included in the analytical sample.

[8] Information on FGS status was not collected in every round of the DHS for the full sample of females aged 0–49. While this information was collected for all females aged 0–49 in the 2014, 2015 and 2016 rounds of the DHS, the 2010 round does not gather FGC information for girls aged 11–14 and the 2012 round does not report the FGC status for women aged 15 or older. Although the 2005 Senegalese DHS also includes a module on FGC, this information is only collected for women aged 15 or older. Because the main analytical sample is restricted to females born since 1990, we do not use the 2005 round of the DHS in the main analysis.

[9] DHS data show that less than 3 percent of the Senegalese women aged 15-49 were cut after the age of 6. This corresponds to less than 10 percent of the women who have undergone FGC, as reported in the DHS data.

Table 1: Summary statistics

	Full sample					FGC	Non-FGC	Diff
	N	Mean	Std dev.	Min	Max	(*N* = 9,293) Mean	(*N* = 19,132) Mean	(FGC – Non-FGC)
FGC	28,425	0.33	0.47	0	1			
Age	28,425	14.49	5.23	7	26	15.31	14.09	1.24***
Year of birth	28,425	1,998.70	5.28	1,990	2,009	1,997.93	1,999.07	−1.14***
Never in school	28,425	0.36	0.48	0	1	0.37	0.36	0.02***
Years of education	28,425	3.16	3.61	0	17	3.17	3.15	0.02
Years of educ≥1	25,817	0.62	0.48	0	1	0.61	0.63	−0.02***
Years of educ≥2	23,346	0.60	0.49	0	1	0.59	0.61	−0.02***
Years of educ≥3	21,382	0.57	0.50	0	1	0.55	0.57	−0.02***
Years of educ≥4	19,283	0.54	0.50	0	1	0.51	0.55	−0.04***
Wealth index	28,424	2.61	1.32	1	5	2.13	2.84	−0.71***
Rural	28,425	0.63	0.48	0	1	0.71	0.60	0.12***
Wolof	28,425	0.32	0.47	0	1	0.01	0.47	−0.46***
Poular	28,425	0.34	0.47	0	1	0.59	0.22	0.37***
Serer	28,425	0.12	0.33	0	1	0.01	0.18	−0.17***
Mandingue	28,425	0.09	0.28	0	1	0.19	0.04	0.16***
Diola	28,425	0.04	0.20	0	1	0.06	0.03	0.03***
Soninke	28,425	0.02	0.13	0	1	0.03	0.01	0.02***
Not a Senegalese	28,425	0.02	0.14	0	1	0.04	0.01	0.03***
Other	28,425	0.05	0.22	0	1	0.06	0.04	0.01***

Note: The number of observations for the variables measuring the probability of receiving at least 1, 2, 3 and 4 years of education is smaller because, for these variables, we only include in the analytical sample women and girls that by the time of the survey were equal or older than 8, 9, 10 and 11 years old respectively, that are the ages at which these girls should have completed 1, 2, 3 or 4 years of education.
*** p < 0.01; ** p < 0.05; * p < 0.1.

Table 1 reports summary statistics for the sample of girls and women included in the analysis. Age ranges between 7 and 26 years. Despite the fact that they are all above the legal age at which they should have started school, 36 percent of them report they have never attended school. The average number of years of schooling is three, although many of these girls and women were still enrolled in school when the survey was conducted.

Furthermore, girls and women who have undergone FGC, who represent 32 percent of the sample, are not a random sample of the Senegalese female population. They are, overall, poorer and older than girls and women who have not undergone FGC, and they are more likely to live in rural areas. They are also significantly more likely to report never having attended school, although the differences between them in terms of years of education are not statistically significant at conventional confidence levels. The lack of significant differences in years of schooling between girls and women who have undergone FGC compared with those who have not contrasts with the

strong negative association between FGC and education observed in Section V when controlling for ethnicity, age and village of residence. This suggests that both education and the prevalence of FGC vary substantially across places of residence and ethnic groups.

Table 2: Prevalence of FGC across ethnic groups in Senegal before the introduction of the law

Ethnic group	FGC prevalence	Share of ethnic group in sample
Wolof	0.017	0.322
Poular	0.642	0.341
Serer	0.020	0.119
Mandingue	0.812	0.086
Diola	0.553	0.039
Soninke	0.679	0.019
Not a Senegalese	0.744	0.023
Other	0.450	0.051

Note: The prevalence of FGC among each ethnic group before the introduction of the law and the share of each ethnic group in the sample are calculated using the sample of women and girls born between 1990 and 1998 who were interviewed in the Senegalese DHS rounds 2016, 2015, 2014, 2012 and 2010.

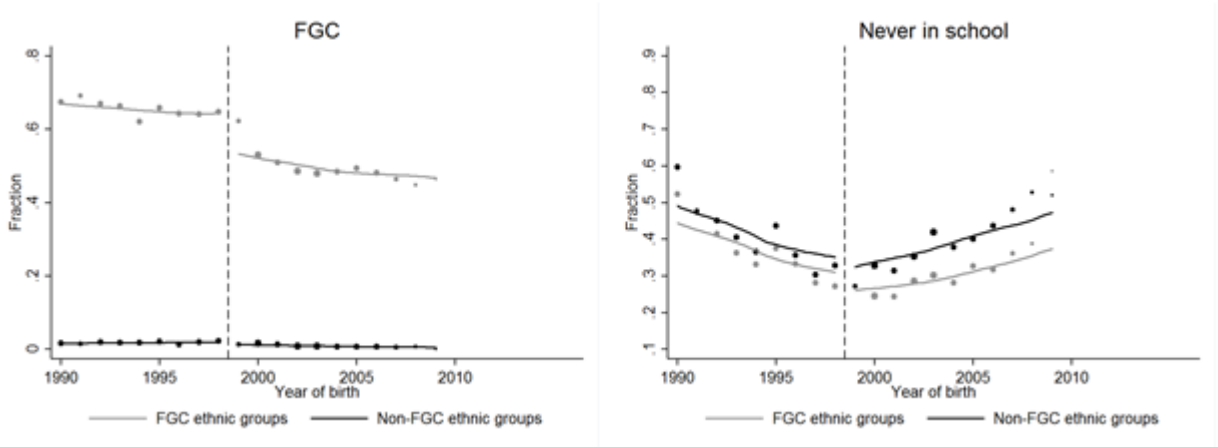
Table 2 reports the prevalence of FGC across ethnic groups, calculated from the group of girls and women born before the introduction of the law.[10] In a country where inter-ethnic marriages are not frequent,[11] the data confirm a wide variation in the prevalence of FGC across ethnic groups, which ranges between 81 percent among Mandingue and 1.7 percent among Wolof.

Figure 1 illustrates how the prevalence of FGC and school outcomes have evolved over time. The left-hand graph shows that while the prevalence of FGC hardly changed across cohorts of girls and women from ethnic groups in which the incidence of FGC is small (e.g. Wolof and Serer), the prevalence of FGC dropped sharply for girls from ethnic groups with a high incidence of FGC who were born after the introduction of the law in January 1999. The right-hand graph reveals that the percentage of girls and women who have never attended school is lower in ethnic groups that practice FGC. Another pattern to highlight is the higher probability of never having attended school among younger cohorts for both FGC and non-FGC ethnic groups. Among the cohorts of girls and women born after the introduction of the law, the upward trend in the probability of never having attended school is mechanically originated by the fact that many girls start school after the age of

[10] We us the information available in DHS rounds 2010-2016 for those women and girls born between 1990 and 1998.
[11] According to the DHS couple recode, only in 24 percent of the marriages are spouses from a different ethnic group. In rural areas, the percentage of inter-ethnic marriages is 17 percent.

7.[12] In Section V, we examine the robustness of the results to the exclusion of the youngest cohorts from the analytical sample, in order to rule out the possibility that the effect is simply reflecting the fact that girls from non-FGC ethnic groups start school at a later age.

Figure 1: FGC and never in school across year of birth cohorts (all ethnic groups)



Note: Each dot represents the average prevalence of FGC and the proportion of girls and women who have never attended school, by year of birth, for ethnic groups in which the practice of FGC is traditional (i.e. Poular, Diola, Mandingue, Soninke, Non-Senegalese and "Other" ethnic groups) and for ethnic groups in which FGC is not traditional (non-FGC; i.e. Wolof and Serer). The size of the dots reflects the quantity of data in our sample. We overlay local polynomial curves (bandwidth of 2) to show trends across FGC and non-FGC ethnic groups.

Section IV: Empirical Strategy

We start by examining whether the introduction of the law, which in the model is interpreted as an increase in the cost of FGC, reduced the prevalence of FGC. We explore whether girls and women from ethnic groups with a larger prevalence of FGC born after the introduction of the law experienced larger reductions in the probability of undergoing FGC compared with girls and women from ethnic groups with a lower prevalence of FGC. For this, we estimate the following regression:

$$FGC_{ikrt} = \propto_0 + \propto_1 (POST_t \times LawIntensity_k) + \propto_2 YearBirth_t + \propto_3 EthnicGroup_k + \propto_4 Village_r + \propto_5 X_i + \mu_{ikrt} \quad \text{(IV.1)}$$

[12] In the DHS data, while starting school after age of 7 is common, school initiation after the age of 10 is extremely rare among every ethnic group. For example, less than 1.5 percent and 0.8 percent of children aged 11 or 12 at the time of the survey started school in that year.

Here, FGC_{ikrt} indicates whether woman i from ethnic group k, living in village r, and born during year t, experienced FGC, and $YearBirth_t$, $EthnicGroup_k$ and $Village_r$ are vectors of dummy variables to indicate year of birth, ethnic group and village of residence, respectively. X_i is a vector of control variables: age at the time of the survey and a dummy variable equal to 1 if FGC status is reported by the individual's mother and 0 if reported by the individual.[13] The term $POST_t \times LawIntensity_k$ is the interaction between the variables $POST_t$, equal to 1 if the girl or woman was born after the introduction of the law (January 1999), and $LawIntensity_k$, which measures the mean prevalence of FGC within ethnic group k calculated using the cohorts of girls and women who were born before the introduction of the law.[14] The parameter $\propto_1$ measures the differential change in the prevalence of FGC after the introduction of the law for ethnic groups with higher and lower levels of FGC before the introduction of the law. In other words, $\propto_1$ yields the effect of the degree of exposure to the law, which is believed to be larger among ethnic groups with a larger prevalence of FGC, on the probability of experiencing FGC. μ_{ikrt} is the error term.

We then follow the same approach to estimate the effect of the law on education:

$$\boldsymbol{Education_{ikrt} = \gamma_0 + \gamma_1(POST_t \times LawIntensity_k) + \gamma_2 YearBirth_t + \gamma_3 EthnicGroup_k + \gamma_4 Village_r + \gamma_5 X_i + \theta_{ikrt}} \quad \text{(IV.2)}$$

Here, $Education_{ikrt}$ measures the education of women i from the ethnic group k living in village r who was born during year t. In the latter regression, the parameter γ_1 yields the effect of the degree of exposure to the law on education attained by girls. This parameter is a direct test for Prediction 2 in the model: a rise in the cost of FGC (brought about by the law banning FGC) increases educational investments received by girls.

Finally, we follow Duflo (2001) and use the interaction term $POST_t \times LawIntensity_k$ in equation (IV.1) as an instrumental variable to investigate whether the reduction in FGC caused by the law increased education. Using a two-stage least-squares (2SLS) procedure, we estimate:

$$\boldsymbol{Education_{ikrt} = \beta_0 + \beta_1 \widehat{FGC_{ikrt}} + \beta_2 YearBirth_t + \beta_3 EthnicGroup_k + \beta_4 Village_r + \beta_5 X_i + u_{ikrt}} \quad \text{(IV.3)}$$

[13] The information on FGC status for each woman or girl is self-reported for women aged 15 or older, while it is reported by the mother of the girls younger than 15.
[14] Between 1990 and 1998.

Where $\widehat{FGC}$ is the predicted probability of FGC estimated from equation (IV.1). The parameter of greatest interest is β_1, which yields the causal effect of FGC on educational investments for those girls and women who did not undergo FGC because they were exposed to the law. This regression also provides a direct test for Prediction 1 in our model.

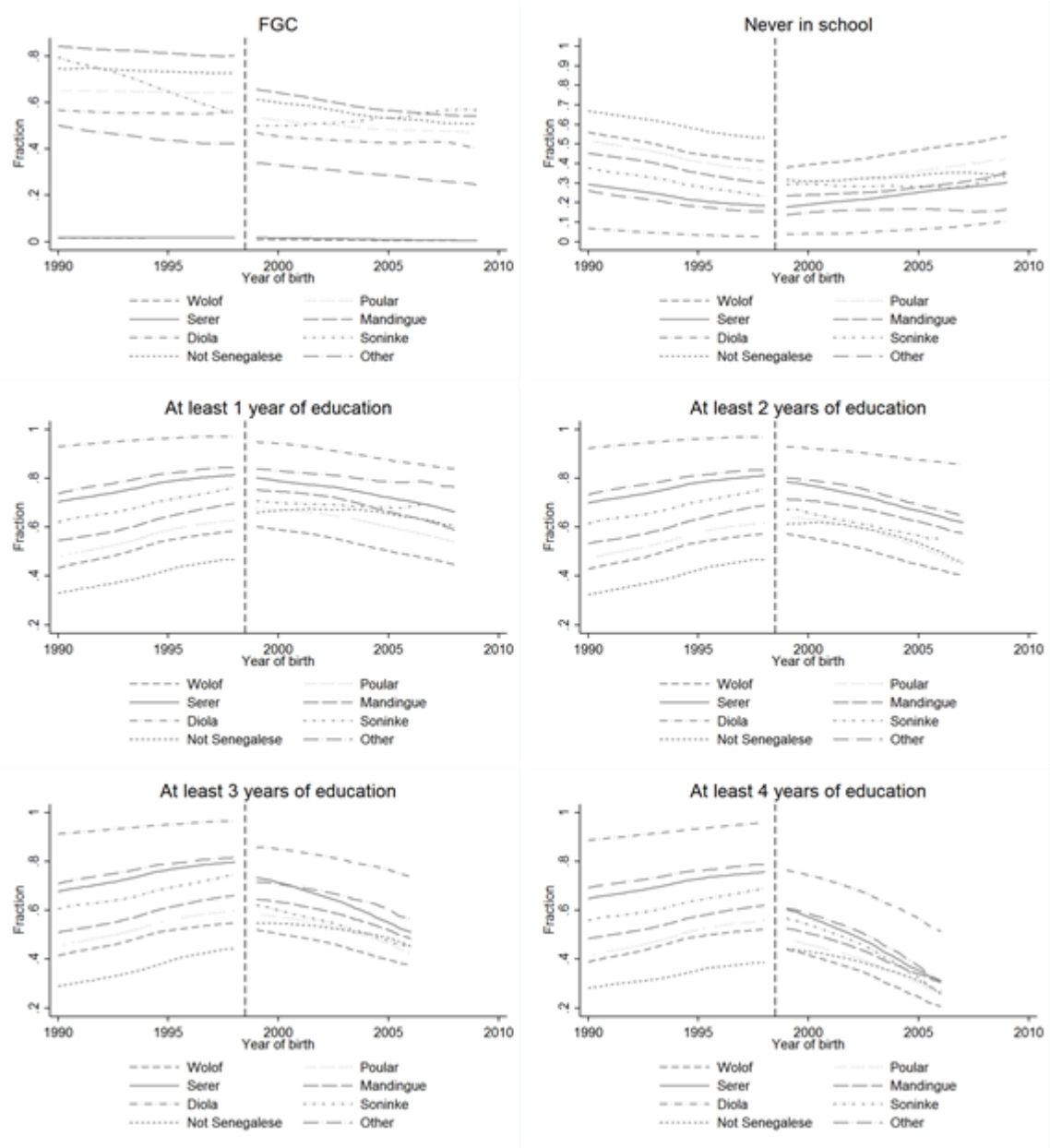
Because the number of ethnic groups in the DHS data is small, we follow Cameron et al. (2008) and estimate equations (IV.1), (IV.2) and (IV.3) using wild bootstrapped standard errors clustered at the ethnic group level.[15] Furthermore, we also estimate the regressions with a vector of regional dummies and region-specific time trends to account for differential trends in FGC or education across regions in Senegal.

One of the main conditions for the causal interpretation of the parameters $\propto_1$, γ_1 and β_1 is that, without the introduction of the law, the evolution of the prevalence of FGC and educational investments over year of birth cohorts should have been the same across ethnic groups. We can test for parallel trends by examining whether the evolution of the prevalence of FGC and educational outcomes across cohorts of girls and women born before the introduction of the law is similar across the different ethnic groups. The latter assumption would be violated, for example, if the prevalence of FGC among FGC ethnic groups started to decline among cohorts born before the introduction of the law, relative to girls from non-FGC ethnic groups. Figure 1 suggests that, for cohorts born before the introduction of the law, the evolution of both the prevalence of FGC and the probability of never having attended school was not very different between FGC and non-FGC ethnic groups.[16]However, the validity of our identification strategy would require parallel trends across every ethnic group used in the analysis. Figure 2 shows the evolution of these variables over cohorts of birth by ethnic group. The evolution of educational outcomes for girls and women born before the introduction of the law was remarkably similar across every ethnic group. However, although the prevalence of FGC remains constant across cohorts of girls and women born before the introduction of the law in six out of the eight ethnic categories in the sample, the prevalence of FGC was already declining among girls and women from Soninke and the ethnic category “Other” who were born before the introduction of the law.

[15] Wild bootstrapping is conducted using the STATA command BOOTTEST presented in Roodman (2015). The programme uses Rademacher weights in the bootstrapping with a number of repetitions equal to 2^n, where n is the number of clusters.

[16] Figure A1 in the Appendix shows the graphs for the rest of the educational outcomes used in the study for these two groups, showing similar patterns.

Figure 2: FGC and educational outcomes for all ethnic groups: continuous treatment



Note: The graphs show local polynomial curves (bandwidth of 2) to show trends across ethnic groups in terms of the prevalence of FGC and educational outcomes in order to assess the conditions of the parallel trends.

In the main analysis, we use two different samples. First, we estimate the above equations restricting the sample to the six ethnic groups for which the prevalence of FGC does not decrease among cohorts born before the introduction of the law. The two ethnic groups removed from this analysis account for 7 percent of the girls and women in the sample and, as can be appreciated in Figures A2 and A3 in the Appendix, excluding these from the analysis ensures the pre-law parallel

trends in terms of educational variables and FGC. Secondly, we also conduct the main analysis using all the eight ethnic groups in the data, reaching the same results.

We further test whether the results could be driven by differential pre-law parallel trends in Section V by estimating the effects of placebo bans introduced in 1993, 1995 and 1997. Consistent with the lack of pre-law parallel trends in FGC or education before the introduction of the law, there are no effects of the false laws on educational outcomes or on the prevalence of FGC.

The second identifying condition for the estimation of α_1, γ_1 and $\beta_1$is that these coefficients are not confounded by the effects of other interventions or programmes that might be differentially affecting the prevalence of FGC or the education of girls and women from FGC groups who were born after the introduction of the law. In the next section, we show that the educational reforms in Senegal that occurred during the last decades and the large anti-FGC programme implemented by the NGO Tostan are unlikely to drive the results.

The correct identification of the 2SLS estimate of the effect of FGC on education, which is the parameter $\beta_1$in equation (IV.3), has two additional conditions. First the validity of this estimation requires that those ethnic groups with a larger pre-law prevalence of FGC experienced larger reductions I the prevalence of FGC following the introduction of the law. This condition would be satisfied if the parameter $\propto_1$ in equation (IV.1) is large and statistically significant. Bound et al. (1995) and Angrist and Pischke (2008) propose that, in order to avoid a problem of weak instruments, the *F*-statistics of the instrumental variables in the first-stage regression should be equal to or large than 10. In Section V, we examine the relevance of our instrumental variable, showing that the statistical significance of $\propto_1$largely satisfies this threshold in most specifications. Second, the 2SLS estimates should also satisfy the exclusion restriction: the introduction of the anti-FGC legislation should only affect educational investments through its effect on the prevalence of FGC. The latter hypothesis would be violated if, for example, the effect of the law on education is caused by a broader effect of the new legislation on gender norms, which affects more intensely girls and women from FGC ethnic groups who were born after the introduction of the law. This possibility is investigated and ruled out in Section V.

A final concern is the accuracy of the information regarding FGC, which in the DHS is either self-reported or reported by mothers. The reporting of the FGC status in settings where this cultural practice has been contested could be problematic (De Cao and Lutz, 2018). In principle, imperfect reporting could challenge our results in two different ways. On the one hand, inaccuracy in the reporting of the FGC status can lead to the problem of measurement error. However, this measurement error should be less problematic when exposure to the law is measured using a dichotomous variable that indicates whether FGC was embedded in the culture of each ethnic group. This dichotomous measure of exposure to the law is unlikely to be measured with error and would provide a straightforward solution for the problem of measurement error described here. The results of this robustness check, reported in Section V, suggest that imperfect reporting does not drive the results of the analysis.

On the other hand, it is also possible that the reporting of the FGC status could be different between girls and women born before and after the introduction of the law. If the degree of under-reporting increases dramatically among those cohorts of girls and women born after the introduction of the law, one could argue that the effects of the law on the reported prevalence of FGC could be also capturing an increase in the degree of under-reporting of the practice and not only a reduction in the *true* prevalence of FGC. The latter may occur if the introduction of the law contributes to the stigmatization of FGC. However, although the law might have contributed to this stigmatization, the latter is unlikely to have had different effects on the reporting of FGC among those girls and women born a few years before or after the introduction of the law, because all the surveys employed in the analysis were collected at least 11 years after the introduction of the law.

Higher levels of under-reporting among girls and women born after the introduction of the law banning FGC would also arise if the fear of confessing a crime led to larger levels of under-reporting among girls and women born after the law was passed than among those born before its introduction. At this point, it is important to highlight that the prevalence of FGC was substantially lower among girls and women born in 1999 and 2000 (after the introduction of the law) than among girls and women born in 1998 or 1997 (before the anti-FGC legislation was passed). We believe this decrease is unlikely to be driven by differential reporting for three reasons. First, enumerators are instructed to explain the confidential nature of the information provided and to conduct the interview without the presence of other household members (ICF, 2018). Second, the superficial knowledge of key aspects of the law documented by Shell-Duncan et al. (2013), which might suggest that parents were unsure about the date in which the law was introduced, indicates that the differences in the reported prevalence of FGC among women born 16 and 17 years ago and those born 18 or 19 years ago are unlikely to be driven by strategic misreporting in order to avoid the reporting of a crime. Finally, although most respondents might not know, the Senegalese law established a ten-year statutory term for criminal offences. Therefore, the parents of girls and women who were subjected to FGC in 1999 or 2000 while they were still infants were not legally accountable for this crime at the time of the surveys.

In the following tests, we investigate further the feasibility that FGC is under-reported because of a fear of the law. We examine whether the magnitude of the decrease in the probability of reporting FGC among those girls and women born after the introduction of the law is affected by who reports the FGC status of a girl in the survey (i.e. the mother of the girl or the girl herself) and by whether the girl or woman lives in the household of her parents. The hypothesis is as follows. If the drop in FGC for cohorts born after the introduction of the law is simply capturing the fear of confessing a crime, then the reporting of the FGC status of girls and women born after the introduction of the law is arguably more likely to be done by a person who is not accountable for the crime or who lives in a different household. Thus, we should observe that the decrease in the prevalence of FGC among girls and women born after the introduction of the law is larger when the FGC status is reported by the mother or when the woman or girl lives in a different household than her parents.

To test this hypothesis, we estimate the following regression:

$$\boldsymbol{FGC_{irt} = \delta_0 + \delta_1 Mother_i + \delta_2 (POST_t \times Mother_i) + \delta_3 YearBirth_t + \delta_4 EthnicGroup_k + \delta_5 Village_r + \delta_5 Age_i + \mu_{ikrt}} \quad \text{(IV.4)}$$

Here, $POST_t \times Mother_i$ is an interaction term between a dummy variable $Mother$, which is equal to 1 if the FGC status is reported by the mother and to 0 is this is reported by the girl of woman herself, and a dummy variable $POST$, which is equal to 1 if the girl or woman is born after the introduction of the law. We estimate a similar specification to check whether the prevalence of the reported FGC status changed differentially for women born after the introduction of the law who do not live with their parents. The results of these analyses are reported in Table A1 in the Appendix. The coefficient of the interaction terms is negative, but small in magnitude and statistically insignificant at conventional confidence levels. These results suggest that the reporting of FGC does not show any relevant difference after the introduction of the law whether it is self-reported or reported by the mother of the girl or whether the daughter does not live with her parents. The lack of significance of δ_2 is consistent with the hypothesis that the decrease in the prevalence of FGC among those girls and women born after the introduction of the law is not simply capturing their stronger reluctance to report FGC.

Finally, even if the lower prevalence of FGC among those cohorts born after the introduction of the law would have been to some extent caused by the fear of confessing a crime, the estimates of the effect of the FGC on educational investments reported in the study would be a lower bound for the true estimates of this effect, which would not compromise the main conclusions of this paper.

Section V: Results

Effect of the law on the prevalence of FGC

We start by estimating the effect of exposure to the law on the prevalence of FGC using the sample that includes girls and women from all ethnic groups. The estimates of interest are presented in Panel A of Table 3, which reports the results of the first-stage regression (equation (IV.1)) using region fixed effects, then adding region-specific time trends and finally using village fixed effects. The coefficient of the variable $\boldsymbol{Intensity \times PostLaw}$ in these regressions shows that girls and women from ethnic groups with a higher pre-law incidence of FGC born after the introduction of the law experienced larger reductions in the probability of undergoing FGC than girls and women from ethnic groups in which FGC was less prevalent. The estimated reduction in the probability of undergoing FGC for girls and women that belong to an ethnic group with a 100 percent pre-law prevalence of FGC is of 16-25 percentage points, depending on the specification. The coefficient measuring the effect of the law is statistically significant at the 5 percent significance level in every

specification and satisfies the relevance condition ($F > 10$) when the regression is estimated using region-specific time trends.

We then re-estimate the regressions excluding girls and women from the two ethnic groups for which the prevalence of FGC started decreasing for cohorts born before the introduction of the law: Soninke and the category “Other” ethnic group. These two groups account for the 7 percent of the girls and women and excluding them from the sample ensures parallel pre-law trends in the prevalence of FGC and educational outcomes across ethnic groups. The results of this analysis are reported in Panel B of Table 3 and they confirm the large effect of exposure to the law on the prevalence of FGC. The magnitude of the impact of the law on FGC is very similar to the one found in Panel A and the statistical significance of this effect largely meets the relevance threshold in the three specifications. These results suggest that the increase in the cost of FGC caused by the introduction of the law led to a reduction in the prevalence of this practice, which is one of the main assumptions in the theoretical model presented in Section I. In Section V, we provide further information on the timing of the effect of the law, ruling out the existence of anticipatory effects.

Table 3: First stage: impact of the law on FGC

Panel A: all ethnic groups

	(1) Prevalence FGC (0/1)	(2) Prevalence FGC (0/1)	(3) Prevalence FGC (0/1)
Intensity × *PostLaw*	−0.238***	−0.157***	−0.247**
	(0.090)	(0.036)	(0.102)
Mean dep. var	0.327	0.327	0.327
Regional dummies	Yes	Yes	No
Regional time trends	No	Yes	No
DHS cluster dummies	No	No	Yes
N	28,425	28,425	28,425

Panel B: restricted sample of ethnic groups

	(1) Prevalence FGC (0/1)	(2) Prevalence FGC (0/1)	(3) Prevalence FGC (0/1)
Intensity × *PostLaw*	−0.236***	−0.164***	−0.245***
	(0.053)	(0.037)	(0.055)
Mean dep. var	0.318	0.318	0.318
Regional dummies	Yes	Yes	No
Regional time trends	No	Yes	No
DHS cluster dummies	No	No	Yes
N	26,517	26,517	26,517

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors clustered at the ethnic group level using wild bootstrapping are reported in parentheses. *** p < 0.01; ** p < 0.05; * p < 0.1.

The effects on educational investments and 2SLS results

In the previous subsection, we have shown that the introduction of the law banning FGC had a large impact on the prevalence of FGC. In this subsection, we first test Prediction 2 by examining whether the ban on FGC had any effect on educational investments. We then test Prediction 1 using a 2SLS estimation to quantify the causal effect of FGC on education of women.

Table 4: Impact of FGC on the extensive margin of educational investments

Panel A: all ethnic groups

Dependent variable: Never in school (0/1)	(1) OLS	(2) RF	(3) SS	(4) RF	(5) SS	(6) RF	(7) SS
Intensity × *PostLaw*		−0.093*		−0.044**		−0.059*	
		(0.049)		(0.018)		(0.031)	
FGC	0.083***		0.392**		0.281***		0.240*
	(0.011)		(0.178)		(0.098)		(0.123)
Mean dep. var	0.364	0.364	0.364	0.364	0.364	0.364	0.364
Regional dummies	Yes	Yes	Yes	Yes	Yes	No	No
Regional time trends	No	No	No	Yes	Yes	No	No
DHS cluster dummies	No	No	No	No	No	Yes	Yes
N	28,425	28,425	28,425	28,425	28,425	28,425	28,425

Panel B: restricted sample of ethnic groups

Dependent variable: Never in school (0/1)	(1) OLS	(2) RF	(3) SS	(4) RF	(5) SS	(6) RF	(7) SS
Intensity × *PostLaw*		−0.099***		−0.052**		−0.064***	
		(0.022)		(0.024)		(0.015)	
FGC	0.079***		0.418**		0.315***		0.262**
	(0.011)		(0.173)		(0.071)		(0.108)
Mean dep. var	0.374	0.374	0.374	0.374	0.374	0.374	0.374
Regional dummies	Yes	Yes	Yes	Yes	Yes	No	No
Regional time trends	No	No	No	Yes	Yes	No	No
DHS cluster dummies	No	No	No	No	No	Yes	Yes
N	26,517	26,517	26,517	26,517	26,517	26,517	26,517

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors clustered at the ethnic group level using wild bootstrapping are reported in parentheses. $***$ $p < 0.01$; $**$ $p < 0.05$; $*$ $p < 0.1$.

The results of the reduced-form equation of the effects of exposure to the law on the extensive margin of educational investments are reported in Columns 2, 4 and 6 of Table 4 using all the ethnic groups (Panel A) and the restricted sample of ethnic groups with parallel pre-law trends (Panel B). The estimates suggest that girls and women born after the introduction of the law that belong to an ethnic group with a 100 percent pre-law prevalence of FGC would experience a reduction in the probability of never having attended school of 4.4–9.9 percentage points, depending on the specification. These magnitudes are consistently negative and statistically significant at conventional confidence levels across specifications and samples, which suggests that a higher degree of exposure to the law decreases the probability of never having attended school. In other words, the estimates on the extensive margin of education suggest that, in line with Prediction 2 of the model, many parents reacted to the rise in the cost of FGC caused by the law by abandoning this practice and investing in their daughters' education.

Columns 1, 3, 5 and 7 of the table provide further details about the link between FGC and the extensive margin of educational investments. The ordinary least-squares (OLS) estimates reported in Column 1 show that, conditional on ethnic group, age, year of birth and village of residence, girls and women who have experienced FGC are nearly 8 percentage points more likely to have never attended school. However, these estimates should not be interpreted as the causal effect of FGC because the statistical association could be driven by unobservable factors, such as attitudes towards tradition. In order to overcome the endogeneity in the link between FGC and educational investments and to estimate the causal effect of FGC on education, we exploit the exogenous variation in the prevalence of FGC caused by across-ethnic group variation in exposure to the law as an instrumental variable, as discussed in the previous section. The results of the 2SLS analysis on the extensive margin of educational investments are reported in Columns 3, 5 and 7 of Table 4. The coefficient of the variable FGC in these regressions indicates that FGC has a large and negative effect on the extensive margin of educational investments. An increase of 10 percentage points in the prevalence of FGC increases the probability of never having attended school by 2.40–4.18 percentage points, depending on the regression specification and sample used. In percentage changes, a decrease by 30 percent in the prevalence of FGC would reduce the probability of never having attended school by approximately 6–11 percent. The coefficient measuring the effect of FGC on the probability of never having attended school is statistically significant at conventional confidence levels in all specifications.

On the other hand, the results of the analysis suggest limited effects of the law on the intensive margin of educational investments. Table 5 reports the estimates of the reduced-form and second-stage regressions using the village fixed effects specification and whether the girl or woman has at least one, two, three and four years of education as dependent variables. The use of these outcome variables imposes constraints on the minimum age of girls who are included in the analysis. For example, for the estimation of the effects on having three or more years of education, we only include in the analytical sample girls who are 10 or older (rather than 7), because this is the age at which girls who started school on time should have had three years of education. Although the relevance of the coefficient measuring the effect of exposure to the law on the prevalence of FGC loses some strength the more restricted the sample is, the results of these regressions suggest that the law increased the probability of receiving one or two years of education. However, the effect of the law on the probability of receiving three or more years of education was much smaller and statistically indistinguishable from 0. We re-estimate the intensive margin analysis using region fixed effects and region-specific time trends for both samples in Tables A4 and A5 in the Appendix. The results of these analyses are consistent with those reported in Table 5, showing statistically significant results only for the probability of receiving one or two years of education; and small and insignificant coefficients for the effect of the ban on the probability of receiving three or more years of education.

Table 5: Intensive margin: impact of FGC on education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of educ≥1		Years of educ≥2		Years of educ≥3		Years of educ≥4	
	RF	SS	RF	SS	RF	SS	RF	SS
Panel A: all ethnic groups								
POST × *Intensity*	0.051**		0.032		0.016		0.006	
	(0.025)		(0.025)		(0.034)		(0.028)	
FGC		−0.222*		−0.161		−0.090		−0.039
		(0.118)		(0.122)		(0.187)		(0.188)
Mean dep. var	0.623	0.623	0.599	0.599	0.567	0.567	0.537	0.537
DHS cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	25,817	25,817	23,346	23,346	21,382	21,382	19,283	19,283
First-stage regression								
POST × *Intensity*		−0.228**		−0.202**		−0.182**		−0.151**
		(0.094)		(0.094)		(0.092)		(0.070)
Panel B: restricted sample of ethnic groups								
POST × *Intensity*	0.055**		0.040*		0.026		0.011	
	(0.026)		(0.021)		(0.034)		(0.031)	
FGC		−0.244**		−0.199**		−0.144		−0.074
		(0.101)		(0.082)		(0.192)		(0.207)
Mean dep. var	0.612	0.612	0.589	0.589	0.556	0.556	0.526	0.526
DHS cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	24,060	24,060	21,726	21,726	19,900	19,900	17,945	17,945
First-stage regression								
POST × *Intensity*		−0.226***		−0.199***		−0.181**		−0.149**
		(0.051)		(0.045)		(0.084)		(0.069)

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and village of residence fixed effects. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping. *** p < 0.01; ** p < 0.05; * p < 0.1.

Heterogeneous effects of FGC on education

We have so far documented the perverse effect that FGC has on the extensive margin of educational investments. Another important question is whether the cultural context can moderate the magnitude of this effect. In this sense, Prediction 3 of the theoretical model indicates that the effect of FGC on educational investments would be larger in those cultural contexts where the value of FGC in the marriage market is larger. This prediction is fairly intuitive: If parents increase educational investments to compensate for worse marriage market outcomes among daughters who have not undergone FGC, we would not observe any effect of FGC on education in contexts where FGC is not linked to marriage markets.

Unfortunately, the 2010–2016 rounds of the Senegalese DHS lack information on the perceived

benefits of FGC. Thus, we investigate this prediction by exploring the heterogeneous effects of FGC on education for different samples that differ in terms of the value of FGC in the marriage market. More specifically, we explore whether the effect of FGC on education is different in rural and urban areas. Because the value of FGC in the marriage market is higher in rural areas (Mackie, 2000; Kandala and Komba, 2015),[17] a larger effect of FGC on educational outcomes in rural settings would be consistent with the substitution hypothesis and the mechanism proposed in the model.

Table 6 shows the results of this analysis. The coefficients reported in Columns 1–4 indicate that while the effect of FGC on the probability of never having attended school is strong in rural areas, the magnitude of the impact is smaller and statistically indistinguishable from 0 at conventional confidence levels in urban areas. However, we do not observe any effect of FGC on the probability of receiving four or more years of education, either for rural or urban households. Taken together, these results are consistent with Prediction 3 of the model and the hypothesis that the effect of FGC on educational investments is driven by parents responding to the law by abandoning FGC and investing instead in the education of their daughters, who are not subjected to FGC, to minimize potential losses in the marriage market.

[17] Mackie (2000) argues that urbanization enlarges marriage opportunities, reducing the negative consequences in the marriage market of not having undergone FGC. Mackie also mentions the chapter written by Orubuloye et al. (2000), who argue that the higher value of FGC in marriage markets in rural Yoruba (Nigeria) relative to urban Yoruba is explained by the fact that social conventions are less enforceable in urban areas. Along this line, Kandala and Komba (2015) argues that the practice of FGC in Senegal is a cultural and social norm embedded predominantly in rural settings.

Table 6: Impact of FGC on education: heterogeneous effects

	Urban sample		Rural sample	
Panel A: Never in school (0/1)				
	(1) FS: Prevalence FGC (0/1)	(2) SS: Never in school	(3) FS: Prevalence FGC (0/1)	(4) SS: Never in school
Intensity × *PostLaw*	−0.314***		−0.211***	
	(0.071)		(0.048)	
FGC		−0.012		0.493**
		(0.069)		(0.248)
DHS cluster FE	Yes	Yes	Yes	Yes
Mean dep. var	0.244	0.193	0.349	0.474
N	9,434	9,434	17,083	17,083
Panel B: years of education≥4 (0/1)				
	(1) FS: Prevalence FGC (0/1)	(2) SS: Years of education≥4	(3) FS: Prevalence FGC (0/1)	(4) SS: Years of education≥4
Intensity × *PostLaw*	−0.201***		−0.124**	
	(0.046)		(0.058)	
FGC		0.050		−0.219
		(0.125)		(0.239)
DHS cluster FE	Yes	Yes	Yes	Yes
Mean dep. var	0.274	0.711	0.396	0.412
N	6,827	6,827	11,118	11,118

Note: Analytical sample restricted to the ethnic groups with parallel trends in terms of FGC among women born before the introduction of the law. All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and village of residence fixed effects. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Robustness checks

In the previous section, we have tested the first three predictions of the model and we have shown that the introduction of the law banning FGC in Senegal decreased the prevalence of FGC and the probability of never having attended school, although the effect on the intensive margin of educational investments was more limited. In this section, we check the robustness of these results to different falsification tests.

First, we run the analysis falsely setting the introduction of the law to January 1995 (rather than 1999) and by limiting the analysis to those girls and women born before the introduction of the *true* law in 1999. This placebo test is an additional robustness check for the parallel trends condition and the lack of anticipation effects. The main results of the study would be compromised if the

estimates of equations (IV.1) and (IV.2) show significant effects of exposure to the law on the prevalence of FGC or on educational investments. The results of this falsification test are reported in Columns 1 and 2 of Table 7 and they support the argument that the results of the study are not driven by pre-law differential trends across ethnic groups in terms of the prevalence of FGC or educational outcomes. We also repeat the analysis, artificially setting the introduction of the law to January 1993 and 1997. The coefficients of the first-stage and reduced-form equations measuring the effects of these placebo laws on FGC and educational investments, reported in Table A2 in the Appendix, are statistically indistinguishable from 0.

Table 7: Robustness checks: FGC and education

	Placebo: law in 1995		Placebo: sample of men	Pre-law period from 1980	
Panel A: never in school (0/1)					
	(1) Prevalence FGC (0/1)	(2) Never in school	(3) Never in school	(4) Prevalence FGC (0/1)	(5) Never in school
Intensity × *PostLaw*	−0.005	−0.014	0.006	−0.272***	−0.097***
	(0.004)	(0.025)	(0.021)	(0.062)	(0.022)
DHS cluster FE	Yes	Yes	Yes	Yes	Yes
N	12,560	12,560	4,924	38,358	38,358
Panel B: years of education≥4 (0/1)					
	(1) Prevalence FGC (0/1)	(2) Years of education≥4	(3) Years of education≥4	(4) Prevalence FGC (0/1)	(5) Years of education≥4
Intensity × *PostLaw*	−0.005	0.015	0.004	−0.172**	0.032
	(0.004)	(0.043)	(0.028)	(0.080)	(0.023)
DHS cluster FE	Yes	Yes	Yes	Yes	Yes
N	12,560	12,560	3,398	29,786	29,786

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures.
*** p < 0.01; ** p < 0.05; * p < 0.1.

Second, we estimate the effect of the law on educational outcomes for boys and men. Because the law might affect the educational investments received by boys and men with sisters exposed to

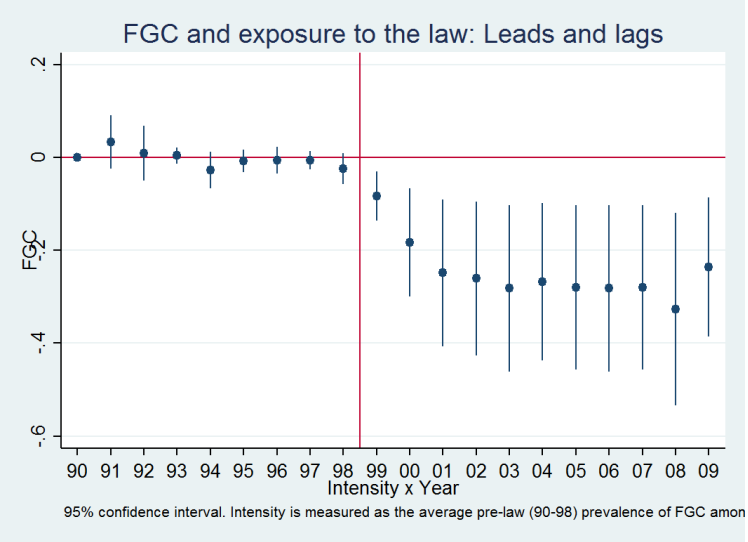
the law, we restrict the analysis to boys and men aged 7 or older, born since 1990 and without any sister born after the introduction of the law. Although, in principle, we cannot dismiss the possibility that the introduction of the law could have had some spill-over effects operating via the job market or the marriage market on the education received by these boys, we believe that this is the purest placebo group. The estimates reported in Column 3 of Table 7 suggest that, for these boys and men, the law did not have any effect on the probability of never having attended school or on the probability of completing at least four years of education.

Third, because the restriction of the analytical sample to those girls and women born since 1990 was to some degree arbitrary, we re-estimate equations (IV.1) and (IV.2) including in the analytical sample those girls and women born since 1980. The results of this exercise are reported in Columns 4 and 5 of Table 7 and show that the magnitudes of the effects of the law on FGC and on educational investments are larger than those found when the pre-law period is limited to those girls and women born since 1990. However, the statistical significance of these coefficients and the main conclusions remain unchanged, confirming the robustness of the results to the inclusion of older cohorts of women in the analytical sample.

Fourth, we also believe that the educational reforms and programmes implemented in Senegal over the last decades are unlikely to confound the results of the study. The main aspects of the Senegalese educational system were established in the law 91-22, which came into force in February 1991. This law set free and compulsory education until the age of 16. Although the expansion of free education might have affected educational outcomes, even the oldest cohort of women in the analytical sample started school a few years after the free primary education policy was established. However, the Senegalese government and different NGOs might have, in the last couple of decades, implemented other educational programmes aimed at improving the quality of education and expanding vocational education (e.g. the Ten-Year Education and Training Programme in 2000). Although these programmes could have affected the educational investments received by women and girls, they would only confound the estimates reported in this study if, conditional on village and year of birth, these programmes affect differently the education of girls and women from different ethnic groups only for cohorts born after the introduction of the anti-FGC law. Furthermore, and given that those girls and women from ethnic groups that traditionally practice FGC are, on average, more educated, the effect is also unlikely to be driven by convergence over time in school attendance among girls and women from ethnic groups that were less likely to attend school.

Fifth, in the last three decades, different NGOs and activists have developed programmes aiming to tackle FGC. The largest anti-FGC intervention in Senegal was conducted by Tostan. Since the early 1990s, this NGO has conducted a community-based programme that aims to inform local communities about the health risks of FGC and to promote community level anti-FGC declarations. Although the geographical scope of the intervention was limited, one may argue that the estimated effect of the law on the prevalence of FGC could be confounded by the impact of Tostan's programme.

Figure 3: Leads and lags: exposure to the law and FGC



In order to assess whether the drop in FGC observed could be driven by Tostan's programme or more generally by other interventions, we study the timing of the decrease in the prevalence of FGC by estimating a leads and lags model interacting the variable measuring the pre-law prevalence of FGC with a set of dummy variables that indicate the year of birth of the woman.[18] The model yields the differential variation in the prevalence of FGC across ethnic groups for each year-of-birth cohort relative to the 1990 cohort. Figure 3 displays the results of this analysis. The figure first shows that the evolution of FGC was not significantly different across ethnic groups for girls and women born before the introduction of the law. However, the figure also reveals that exposure to the law significantly reduced the prevalence of FGC for those girls and women born in the year when the law was introduced, which doubled the following year, increased further during the third year and then remained relatively stable. The timing of the decrease in the prevalence of FGC

[18] The estimated leads and lags model is

$$FGC_{ikrt} = \propto_0 + \sum_{\tau=1991}^{1998} \propto_{1\tau} (YearBirth_t \times LawIntensity_k) + \sum_{\tau=1999}^{2009} \propto_{1\tau} (YearBirth_t \times LawIntensity_k) + \propto_2 YearBirth_t + \propto_3 EthnicGroup_k + \propto_4 Region_r + \propto_5 X_i + \mu_{ikrt}$$

Where $(YearBirth_t \times LawIntensity_k)$ is a vector of variables constructed as the interaction of the pre-law average prevalence of FGC among ethnic group k with each year-of-birth dummy. These interaction variables are known in the literature as the lead and lag variables. In our specification, the lead variables are the interaction between average pre-law FGC prevalence among the ethnic group of the woman and the year of birth for those girls and women from cohorts born before the introduction of the law in January 1999. The lag variables are the interactions terms for those girls and women from cohorts born after the introduction of the law. The coefficients of the lead and lag variables yield the differential variation in the prevalence of FGC between girls and women from different ethnic groups across year-of-birth cohorts relative to those women born in 1990, the omitted category in the regression specification. The estimated coefficients of the lead variables provide an empirical test for the parallel trends condition and the existence of anticipatory effects. If these coefficients are small and statistically indistinguishable from 0, the prevalence of FGC across ethnic groups was arguably following the same trend before the introduction of the law, validating our empirical approach. Furthermore, the coefficients of the lag variables provide useful information on the timing of the effect of the law.

following the introduction of the law provides reassurance that the decrease in the prevalence of FGC observed in the data is caused by the law, also ruling out the existence of anticipatory effects.

We explore further the possibility of confounding interventions by re-estimating the main results of the study, excluding from the sample girls and women living in rural areas of the Kolda, Thiès and Fatick regions, where Tostan conducted the vast majority of its Community Empowerment Programmes (Yoder, 2008). The results of these analyses are reported in Table 8. The estimates, very similar to those reported in the main estimations of the study, confirm that the effects seen in the analysis are not confounded by the anti-FGC work that this NGO has been conducting in Senegal since the 1980s.

Table 8: Impact of FGC on education (excluding rural areas in Kolda, Thiès and Fatick)

	Never in School (0/1)			Years of education≥4		
	(1) Prevalence FGC (0/1)	(2) Never in school	(3) Never in school	(4) Prevalence FGC (0/1)	(5) Years of education≥4	(6) Years of education≥4
Intensity × *PostLaw*	−0.240***	−0.069**		−0.133**	0.027	
	(0.054)	(0.032)		(0.062)	(0.026)	
FGC			0.286**			−0.201
			(0.118)			(0.212)
DHS cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	22,233	22,233	22,233	15,193	15,193	15,193

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Sixth, we estimate the main regressions using a dichotomous measure of exposure to the law. Instead of defining exposure as the mean prevalence of FGC within an ethnic group calculated from the sample of girls and women from this ethnic group born before the introduction of the law, we use a dummy variable that is equal to 1 if the ethnic group has traditionally practiced FGC. In this analysis, the girls and women from the Wolof and Serer ethnic groups, in which the practice of FGC is rare, would constitute the control group (unexposed to the law) while girls and women from the rest of the ethnic groups in Senegal would constitute the treatment group (exposed to the law). The main advantage of this approach is that it provides a straightforward solution for an eventual measurement error problem caused by random reporting errors in the FGC status. Table 9 reports the results of the estimations on both the extensive and intensive margins of educational investments when exposure to the law is defined using FGC versus non-FGC ethnic groups. The results of the analysis measuring the effects of the law are aligned with those obtained in the main

analysis and the magnitudes and statistical significance of the coefficients measuring the effect of FGC on educational outcomes are remarkably similar to those obtained using the continuous measure of exposure to the law.

Table 9: Impact of FGC on education using dichotomous measure of exposure to the law

Panel A: never in school (0/1)	FS	RF	SS
	(1) Prevalence FGC (0/1)	(2) Never in School	(3) Never in School
Intensity × *PostLaw*	−0.160***	−0.038**	
	(0.036)	(0.018)	
FGC			0.239**
			(0.099)
DHS cluster dummies	Yes	Yes	Yes
N	26,517	26,517	26,517
Panel B: years of education≥4 (0/1)			
	(1) Prevalence FGC (0/1)	(2) Years of education≥4	(3) Years of education≥4
Intensity × *PostLaw*	−0.097***	0.007	
	(0.022)	(0.018)	
FGC			−0.074
			(0.184)
DHS cluster dummies	Yes	Yes	Yes
N	17,945	17,945	17,945

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Finally, we examine the robustness of the results to the exclusion of non-Senegalese girls and women from the sample. Foreign girls and women constitute 2.5 percent of the sample. Because Senegalese law only grants citizenship to the children of Senegalese citizens, many of the younger non-Senegalese girls were likely born in Senegal and, therefore, subject to the anti-FGC legislation.

Ideally, we would exclude from the main analytical sample those girls and women who were non-Senegalese and were not born in Senegal. However, as we lack information on birthplace, we include non-Senegalese girls and women in the main analytical sample. Nonetheless, we also test the robustness of the results to their exclusion from the analysis. The results of this robustness exercise, reported in Table A3 in the Appendix, show that limiting the analysis to those girls and women with the Senegalese citizenship hardly changes the magnitudes and the statistical significance of the estimates.

Another potential concern with the estimation of the effect of FGC on the extensive margin of educational investments is that, because the cohorts of girls and women born before the introduction of the law were at least 10 years old at the time of the survey, the effects of FGC on the variable measuring whether a girl or woman has never attended school could be at least partially explained by school initiation at a later age among ethnic groups that did not practice FGC. In other words, girls aged 7–10 in the sample were all born after the introduction of the law and, therefore, the estimates could be capturing a larger prevalence of starting school late among non-FGC ethnic groups rather than a true effect of FGC on the probability of never having attended school. With the objective of testing this hypothesis, we conduct the following analyses. First, we re-estimate the main regressions restricting the sample to girls and women aged 8, 9 and 10 or older. In the data used in the analysis, it is extremely rare among every ethnic group for children to start school after the age of 10.[19] In other words, our data suggest that if a girl has never attended school by the age of 10, it is unlikely that she will start school in the future. The results of this analysis, reported in Table 10, show that removing girls aged 7–9 from the analysis does not make any relevant change to the magnitude of the estimates. Secondly, we re-estimate the main analysis on the probability of never having attended school, also including the 2005, 2006 and 2008 DHS rounds. By adding these datasets, the sample used in the analysis also includes girls aged 7–10 at the time of the surveys, who were born before the introduction of the law, which improves the age-at-survey control variable in the regression. However, the lack of information on the FGC status for girls and women born since 1990 in the DHS rounds 2005, 2006 and 2008 only allows for a reduced-form estimation of the effect of exposure to the law on the probability of never having attended school. The results of this robustness check are reported in Table 11. The estimates of the effect of the law have the correct sign and are all statistically significant at conventional confidence levels. The smaller size of the coefficient in reduced form estimations of the effect of the law using only older cohorts is consistent with the smaller effect of the law on the prevalence of FGC among older cohorts that we previously observed. The results of these two analyses dismiss the hypothesis that the effect of FGC on the probability of never having attended school is simply capturing a larger prevalence of girls and women from non-FGC ethnic groups starting school later.

[19] In our sample, less than 1.5 percent and 0.8 percent of children aged 11 or 12 at the time of the survey started school in that year. This pattern is no different across ethnic groups or across surveys.

Table 10: Impact of FGC on education: sample limited to older girls

Dep var: never in school (0/1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample of girls	≥ 7 years old		≥ 8 years old		≥ 9 years old		≥ 10 years old	
	RF	SS	RF	SS	RF	SS	RF	SS
POST × *Intensity*	−0.064***		−0.059**		−0.048**		−0.043*	
	(0.015)		(0.027)		(0.022)		(0.023)	
FGC		0.262**		0.261**		0.243**		0.239**
		(0.108)		(0.108)		(0.100)		(0.111)
First-stage regression								
POST × *Intensity*		−0.245***		−0.226***		−0.199***		−0.181**
		(0.055)		(0.051)		(0.045)		(0.084)
DHS cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	26,517	26,517	24,060	24,060	21,726	21,726	19,900	19,900

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Table 11: Impact of FGC on education: Using all available DHS data

	Never in school (0/1)			
	(1)	(2)	(3)	(4)
POST × *Intensity*	−0.057**	−0.056***	−0.043***	−0.036***
	(0.026)	(0.013)	(0.010)	(0.008)
Mean dep. var	0.396	0.388	0.388	0.391
DHS cluster dummies	Yes	Yes	Yes	Yes
N	41,869	38,548	35,421	32,980
Sample of girls	≥ 7 years old	≥ 8 years old	≥ 9 years old	≥ 10 years old

Note: Analytical sample restricted to the ethnic groups with parallel trends in terms of FGC among women born before the introduction of the law. All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and village of residence fixed effects. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping.
*** p < 0.01; ** p < 0.05; * p < 0.1.

Section VI: Alternative Mechanisms

There are three alternative mechanisms through which the law could have affected educational investments received by girls. First, the introduction of the law might have contributed to a broader change in cultural preferences and gender norms among ethnic groups practicing FGC, leading to larger investments in the education of girls. If this mechanism is driving the results, then the causal estimates of the second-stage equation providing the effect of FGC on education would not meet the exclusion restriction, and the results of the 2SLS analysis would be invalid.

We argue that the effect on the extensive margin of educational investments is unlikely to be driven by a change in gender roles. The cohorts of girls and women born in the year of the law only started school seven years after the introduction of the law. Thus, several cohorts of girls and women born before the introduction of the law started school after the introduction of the anti-FGC legislation. The lack of an effect for these girls and women suggests that the effect is not driven by the law changing gender norms or educational preferences among FGC ethnic groups. Nonetheless, we explore this hypothesis further by examining the effect of the law on the empowerment outcomes of women who live in households with girls or women exposed to the law. If the effect of the law on educational investments is driven by a change in gender norms in those families of girls and women who were more exposed to the law, then we would also expect an effect on the empowerment level of their older sisters and other female members of their house- holds. The results of this analysis are reported in Table 12 and show that the law did not have any effect on the education and most empowerment indicators of female members of households with girls exposed to the law. The latter indicates that the effects seen in this study are not driven by the law promoting a differential change in gender norms among families of girls and women born after the introduction of the law who belong to ethnic groups in which the practice of FGC is traditional.

A second mechanism that could explain the result is that FGC might also work indirectly on education by affecting the age at which women are married. In the context of a competitive marriage market where parents prefer to marry their daughters as soon as possible after menarche (Moghadam, 2004; Wahhaj, 2015), girls and women who have undergone FGC could be in a better position to marry at a younger age. Because early marriage could lead to early school dropout (Field and Ambrus, 2008; Hicks and Hicks, 2015), it can be argued that FGC could also reduce educational investments by lowering the age at which women are married.

Table 12: Impact of the law on women empowerment in households of girls affected by the law

	(1) Never in School	(2) Years of education≥4	(3) Participate decisions: health care	(4) Participate decisions: hhold purchases	(5) Participate decisions: family visits	(6) Participate decisions: husband earns
Intensity × PostLaw	-0.029	0.061	0.065	0.100*	0.021	0.033
	(0.066)	(0.057)	(0.068)	(0.054)	(0.105)	(0.103)
Mean Dep. Var.	0.548	0.395	0.175	0.146	0.215	0.142
DHS Cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	20,977	20,977	13,725	13,725	13,725	13,564

Note: All the regressions include as control variables a vector of year-of-birth dummies for the girl and the other female members of her households, variables for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

However, although FGC may decrease the age at which marriage occurs, we believe this mechanism is unlikely to drive the observed educational effects of FGC. The results presented in Section V suggest that FGC has a limited effect on the intensive margin of educational investments. Specifically, the estimates reveal no effect of FGC on the probability of receiving three or more years of education, which should in principle be completed by the age of 10. Given the small impact on the intensive margin of education and the fact that only 5 percent of women older than 18 were cohabiting with their partner by the age of 14, it seems unlikely that the increase in educational investments caused by the law is driven by women who have not undergone FGC marrying at older ages.

A third alternative mechanism through which FGC may curb educational investments is its potential perverse effect on health. If health during childhood is a strong determinant of educational achievements and eventually reduces returns to education (e.g. Grantham-McGregor et al., 2007), FGC could also affect educational investments and school dropout rates by severely affecting the health of women and girls. Although the literature on the health consequences of FGC is mixed (e.g. Wagner, 2015), most studies suggest that FGC is associated with worse sexual and reproductive health outcomes, such as a higher prevalence of sexually transmitted disease, genital tissue swelling, painful intercourse and difficulties during childbirth (Berg and Underland, 2013; Berg et al., 2014; Wagner, 2015). In light of the evidence of the health consequences of FGC, we cannot dismiss the possibility that the effect of FGC on education may also be driven by the lasting effects of FGC on health, eventually leading to lower educational achievements.

The most straightforward way to test the health mechanism would be to re-estimate equations (IV.1), (IV.2) and (IV.3) using health outcomes as dependent variables. However, the databases used in the main analysis do not provide health information on anthropometric measurements, incidence of anemia and diarrhea for girls older than 5 years old, implying that we do not observe

key health outcomes for children born before the introduction of the anti-FGC law. To cope with this limitation, we add to the sample rounds 2005 and 1997 of the Senegalese DHS and rounds 2008 and 2006 of the Senegalese Malaria Indicator Survey (MIS). These databases include information on health outcomes for girls born before and after the introduction of the law that banned FGC. However, these rounds of the DHS and the MIS do not include appropriate information on FGC status, which limits the analysis to reduced-form estimations. In order to test the health mechanism, we estimate the effect of the law on health outcomes as follows:

$$\boldsymbol{Health_{ikrt}} = \propto_0 + \propto_1 \boldsymbol{POST_t} \times \boldsymbol{LawIntensity_k} + \propto_2 \boldsymbol{YearBirth_t} + \propto_3 \boldsymbol{EthnicGroup_k} + \propto_4 \boldsymbol{Village_r} + \propto_5 \boldsymbol{X_i} + \boldsymbol{\mu_{ikrt}} \quad \text{(VI.1)}$$

Here, $\boldsymbol{Health_{ikrt}}$ indicates the health status of girl $\boldsymbol{i}$ from the ethnic group $\boldsymbol{k}$, living in village $\boldsymbol{r}$ and born during year $\boldsymbol{t}$. We use different health variables including weight, height, anemia, diarrhea and whether the girl has a health card, aiming to measure health status and investments. The health variables analysed correspond to the health-related questions that are available for girls aged below 5 in all survey rounds included. In equation (VI.1), $\propto_1$ yields the effect on health outcomes of the degree of exposure to the law that banned FGC. If the parameter $\propto_1$ is consistently small and statistically insignificant across the different specifications, the effect of FGC on education is arguably not driven by any effect of FGC on the health outcomes analysed.

The results of this analysis are reported in Table 13. The coefficients that measure the effect of the law on weight, height, anemia and the probability of having a health card are all statistically indistinguishable from 0 at conventional confidence levels. Only the probability of having diarrhea during the last 24 hours is statistically significant at the 10 percent level. Overall, the results suggest that FGC may have limited effects on the health outcomes analysed. However, the young age of most girls and women born after the introduction of the law hampers the assessment of the effect of the law on sexual and reproductive health outcomes, which have been found to be associated with FGC. Indeed, one could argue that the effect of FGC on education is caused by teenage girls who have undergone FGC dropping out of school because of poor sexual and reproductive health outcomes, and this might not be reflected in the health variables analysed. However, most of the effects of FGC on sexual and reproductive health (e.g. irregular menstruation, painful intercourse, etc.) are unlikely to affect girls' education before their teenage years. Hence, the fact that FGC has a large impact on the probability of never having attended school but little or no impact on the probability of finishing three or more years of education suggests that the beneficial effect of reducing FGC on educational investments observed does not seem to be driven by poor sexual and reproductive health among girls and women who have undergone FGC.

Table 13: Impacts of the law banning FGC on health outcomes and investments

	(1) Ln weight (kg 1 dec)	(2) Ln height (cm 1 dec)	(3) Anemia (0/1)	(4) Diarrhea in last 24 h(0/1)	(5) Health card (0/1)
Intensity × *PostLaw*	−0.006	0.014	−0.043	−0.054*	−0.001
	(0.036)	(0.017)	(0.039)	(0.032)	(0.009)
Mean dep variable	177	965	0.328	0.175	0.905
DHS cluster FE	Yes	Yes	Yes	Yes	Yes
N	13,965	13,763	15,158	19,656	18,413

Note: Analytical sample restricted to the ethnic groups with parallel trends in terms of FGC among women born before the introduction of the law. All the regressions include as control variables a vector of year of birth dummies, a quadratic polynomial for age at the time of the survey, a vector of ethnic group dummies, village of residence fixed effects, a dummy indicating whether a women is born after the introduction of the law and a variable indicating 1980–1998 average pre-law incidence of FGC for the ethnicity of the woman. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping. *** p < 0.01; ** p < 0.05; * p < 0.1.

Conclusions

In this paper, we show that the introduction of a law in Senegal that banned the practice of FGC significantly reduced the prevalence of FGC and, as a consequence, the extensive margin of educational investments received by women and girls improved. We contribute to the literature that investigates the effectiveness of legal reforms as policy instruments to tackle harmful traditional practices in developing countries and to the understanding of the interaction of cultural norms and policy design.

Our study provides evidence consistent with previous research that finds that the introduction of laws reduced the prevalence of child marriage in Ethiopia (Garcia-Hombrados, 2017) and FGC in Burkina Faso (Crisman et al., 2016). More specifically, the results support the introduction of anti-FGC legislation in the many countries where the practice is widespread but still not regulated (Allen et al., 2015), highlighting also that these laws could have beneficial consequences in human capital accumulation among women.

The study also provides causal estimates on the effect of FGC on education, documenting the perverse consequences of this practice on the extensive margin of educational investments and suggesting that reducing this practice would also contribute to closing the gender gap in education.

We formalize the link between FGC and education in a model where both are pre-marital investments that improve marriage market outcomes. Although girls and women affected by the law are still very young and the marriage market effects of the law cannot be tested, the empirical results presented in this paper are consistent with the different predictions of the model. The results suggest that many parents perceived FGC and educational investments as substitutes in

the marriage market and they open the debate on whether interventions aiming to lower the value of FGC in the marriage market or to change the relative price of alternative pre-marital investments could be effective strategies to reduce the prevalence of FGC. These results are also consistent with existing theories that argue that individual- and household-level factors and circumstances are key to understanding decisions regarding FGC (Bellemare et al., 2015; Efferson et al., 2015; Novak, 2016).

Although the results of this study are consistent with the predictions of the model and the assumptions of the model are well backed by previous anthropological studies, additional research examining empirically the causal effects of FGC on the marriage market would be needed to test comprehensively the substitution hypothesis. Unfortunately, the lack of suitable data and the low age of girls born after the introduction of the law prevented the estimation in this study of the effects of FGC on marriage market outcomes.

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Appendix A: Additional tables and figures

Table A1: The law and the reporting of FGC status

	(1) Prevalence FGC (0/1)	(2) Prevalence FGC (0/1)
Mother reports	−0.049***	
	(0.011)	
Post× Mother reports	−0.023	
	(0.031)	
No live with parents		0.000
		(0.003)
Post× No live with parents		−0.012
		(0.020)
Ethnic group dummies	Yes	Yes
Year of birth dummies	Yes	Yes
DHS cluster dummies	Yes	Yes
N	28,425	28,425

Note: Regression includes as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies and village-level dummies. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping.
*** p < 0.01; ** p < 0.05;* p < 0.1.

Table A2: Introduction of placebo laws: FGC and education

	Placebo: law in 1993		Placebo: law in 1995		Placebo: law in 1997	
Panel A: never in school (0/1)						
	(1) Prevalence FGC (0/1)	(2) Never in School	(3) Prevalence FGC (0/1)	(4) Never in School	(5) Prevalence FGC (0/1)	(6) Never in School
Intensity × *PostLaw*	−0.019	0.017	−0.005	−0.014	−0.017	−0.011
	(0.016)	(0.043)	(0.004)	(0.025)	(0.012)	(0.033)
DHS cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12,560	12,560	12,560	12,560	12,560	12,560
Panel B: years of education≥4 (0/1)						
	(1) Prevalence FGC (0/1)	(2) Years of education≥4	(3) Prevalence FGC (0/1)	(4) Years of education≥4	(5) Prevalence FGC (0/1)	(6) Years of education≥4
Intensity × *PostLaw*	−0.019	−0.009	−0.005	0.015	−0.017	0.018
	(0.016)	(0.029)	(0.004)	(0.043)	(0.012)	(0.018)
DHS cluster FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12,560	12,560	12,560	12,560	12,560	12,560

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Table A3: Impact of FGC on education (excluding from the sample the ethnic categories "Other", non-Senegalese and Soninke)

	Never in school (0/1)			Years of education≥4 (0/1)		
	(1) Prevalence FGC (0/1)	(2) Never in school	(3) Never in school	(4) Prevalence FGC (0/1)	(5) Years of education≥4	(6) Years of education≥4
Intensity × *PostLaw*	−0.248***	−0.055*		−0.152*	0.001	
	(0.056)	(0.030)		(0.082)	(0.007)	
FGC			0.223**			−0.004
			(0.104)			(0.049)
DHS cluster dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	25,911	25,911	25,911	17,518	17,518	17,518

Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping procedures. *** p < 0.01; ** p < 0.05; * p < 0.1.

Table A4: Intensive margin: impact of FGC on education (with and without regional time trends): restricted sample of ethnic groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of educ≥1		Years of educ≥2		Years of educ≥3		Years of educ≥4	
	RF	SS	RF	SS	RF	SS	RF	SS
Without regional time trends								
POST × *Intensity*	0.086**		0.064**		0.045*		0.023	
	(0.040)		(0.030)		(0.027)		(0.037)	
FGC		−0.391**		−0.322**		−0.249*		−0.158
		(0.162)		(0.133)		(0.141)		(0.244)
Mean dep. var	0.612	0.612	0.589	0.589	0.556	0.556	0.526	0.526
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional trends	No	No	No	No	No	No	No	No
N	24,060	24,060	21,726	21,726	19,900	19,900	17,945	17,945
First-stage regression								
POST × *Intensity*		−0.220***		−0.198***		−0.181***		−0.146***
		(0.050)		(0.045)		(0.041)		(0.033)
With regional time trends								
POST × *Intensity*	0.039*		0.021		−0.000		−0.009	
	(0.023)		(0.026)		(0.000)		(0.060)	
FGC		−0.249**		−0.142		0.000		0.089
		(0.116)		(0.166)		(0.004)		(0.566)
Mean dep. var	0.612	0.612	0.589	0.589	0.556	0.556	0.526	0.526
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	24,060	24,060	21,726	21,726	19,900	19,900	17,945	17,945
First-stage regression								
POST × *Intensity*		−0.157***		−0.144***		−0.134***		−0.106***
		(0.036)		(0.033)		(0.030)		(0.024)

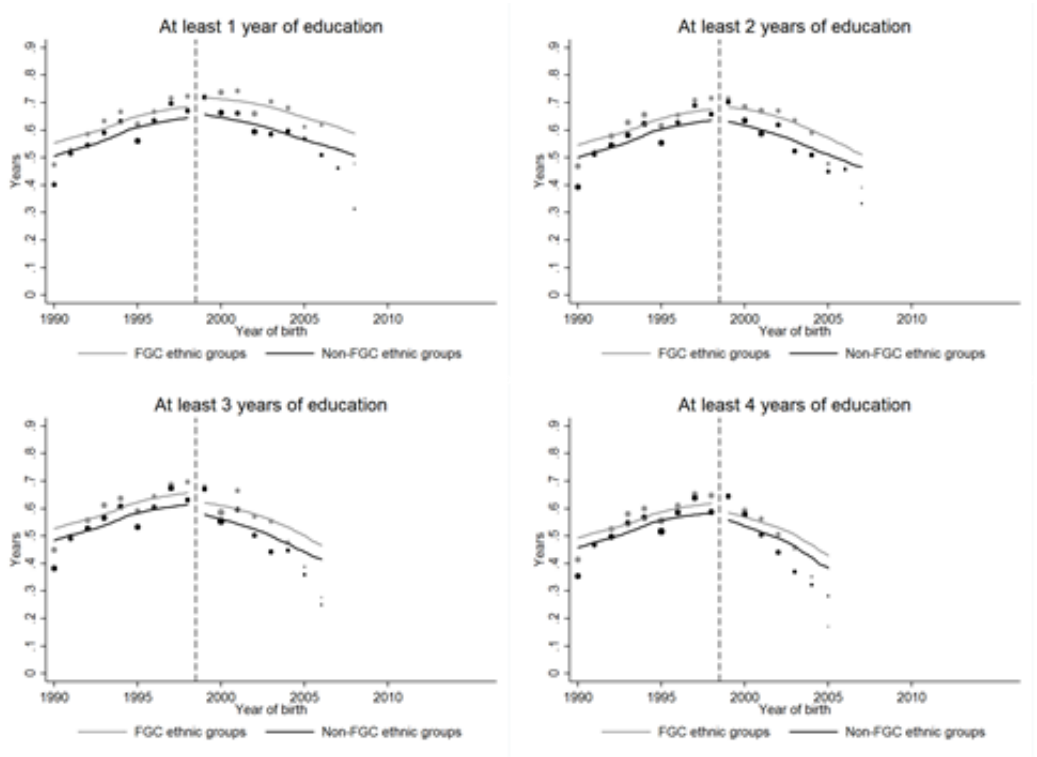
Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. Standard errors, reported in parentheses, are clustered at the ethnic group level using wild bootstrapping. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table A5: Intensive margin: impact of FGC on education (with and without regional time trends): all ethnic groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of educ≥1		Years of educ≥2		Years of educ≥3		Years of educ≥4	
	RF	SS	RF	SS	RF	SS	RF	SS
Without regional time trends								
POST × *Intensity*	0.080*		0.057*		0.037		0.021	
	(0.042)		(0.030)		(0.031)		(0.035)	
FGC		−0.361**		−0.282**		−0.201		−0.139
		(0.182)		(0.131)		(0.166)		(0.230)
Mean dep. var	0.623	0.623	0.599	0.599	0.567	0.567	0.537	0.537
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional time trends	No	No	No	No	No	No	No	No
N	25,817	25,817	23,346	23,346	21,382	21,382	19,283	19,283
First-stage regression								
POST × *Intensity*		−0.222***		−0.201**		−0.184**		−0.151**
		(0.083)		(0.089)		(0.085)		(0.073)
With regional time trends								
POST × *Intensity*	0.033*		0.013		−0.013		−0.015	
	(0.020)		(0.030)		(0.026)		(0.029)	
FGC		−0.216**		−0.093		0.104		0.151
		(0.107)		(0.172)		(0.231)		(0.305)
Mean dep. var	0.623	0.623	0.599	0.599	0.567	0.567	0.537	0.537
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	25,817	25,817	23,346	23,346	21,382	21,382	19,283	19,283
First-stage regression								
POST × *Intensity*		−0.151***		−0.139***		−0.127***		−0.100***
		(0.034)		(0.031)		(0.029)		(0.023)

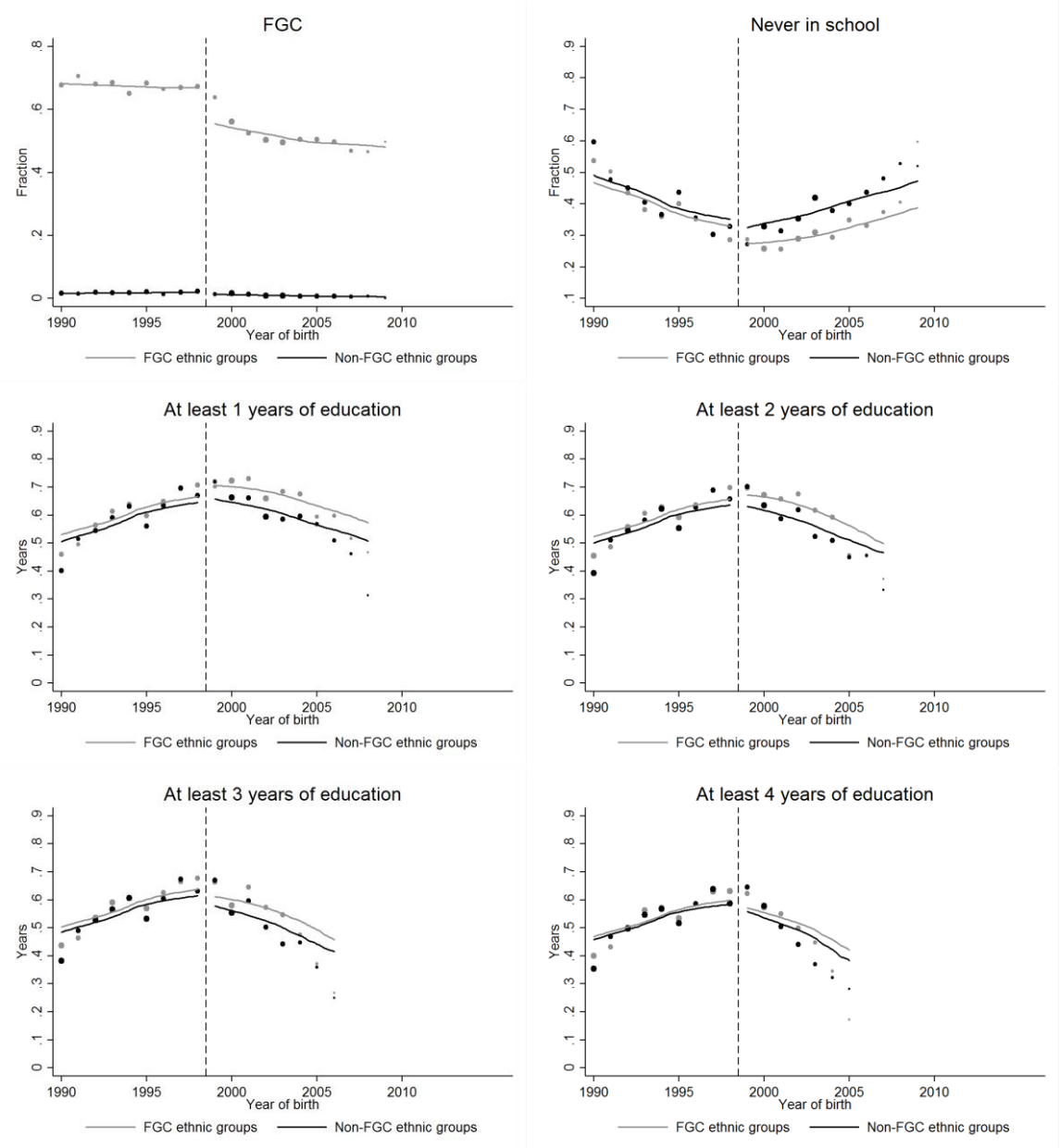
Note: All the regressions include as control variables a vector of year of birth dummies, a variable for age at the time of the survey, a vector of ethnic group dummies, a dummy indicating whether the FGC status is reported by the girl and a vector of dummies for survey rounds. The p-values, based on wild cluster bootstrapping at the ethnic group level, are given in parentheses. *** p < 0.01; ** p < 0.05; * p < 0.1.

Figure A1: FGC and the intensive margin of educational investments (all ethnic groups): dichotomous treatment



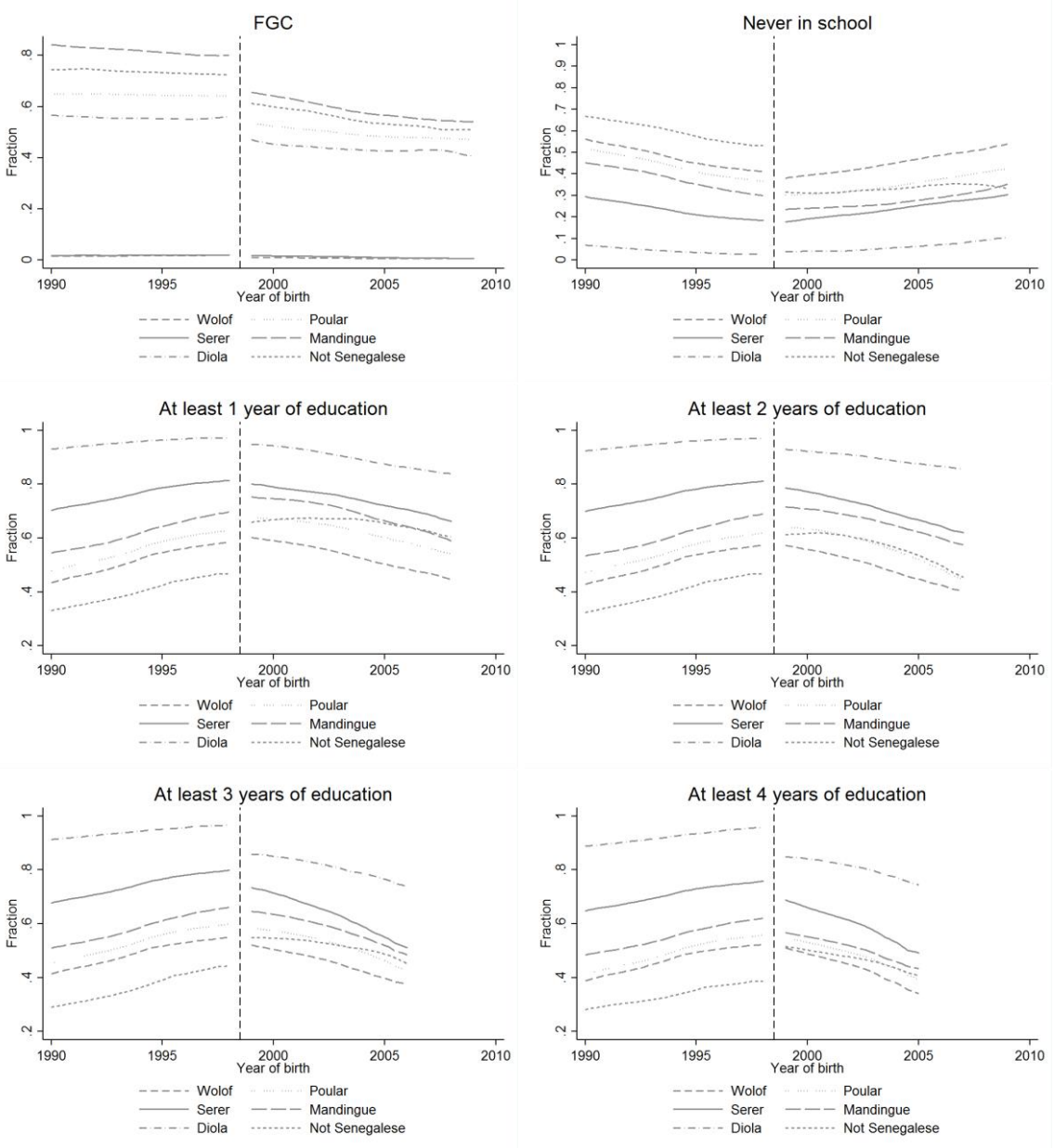
Note: Each dot represents the proportion of girls or women with at least one, two, three and four years of education by year of birth for ethnic groups in which the practice of FGC is traditional (Poular, Diola, Mandingue, Soninke, non-Senegalese and "Other" ethnic group) and for ethnic groups in which FGC is not traditional (Wolof and Serer). The size of the dots reflects the quantity of data in our sample. We overlay local polynomial curves (bandwidth of 2) to show trends across FGC and non-FGC ethnic groups.

Figure A2: FGC and educational outcomes (restricted sample of ethnic groups): dichotomous treatment



Note: Each dot represents the proportion of girls or women with at least one, two, three and four years of education by year of birth for ethnic groups in which the practice of FGC is traditional (Poular, Diola, Mandingue, Soninke, non-Senegalese and "Other" ethnic group) and for ethnic groups in which FGC is not traditional (Wolof and Serer). The size of the dots reflects the quantity of data in our sample. We overlay local polynomial curves (bandwidth of 2) to show trends across FGC and non-FGC ethnic groups.

Figure A3: FGC and educational outcomes (restricted sample of ethnic groups): continuous treatment



Note: The graphs show local polynomial curves (bandwidth of 2) to show trends across ethnic groups in terms of the prevalence of FGC and educational outcomes to assess the parallel trends condition.