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CULTURAL PRACTICES AND WOMEN'S LIVES‡

Does Maternal Education Decrease Female Genital Cutting?†

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Female genital cutting (FGC) is a violation of women's physical integrity and a harmful custom with potentially negative long-term health consequences (Berg and Underland 2013). FGC, also called female genital mutilation or female circumcision, includes all procedures involving the removal of external female genitalia for nonmedical reasons. Although the prevalence of FGC has been declining, it is estimated that at least 200 million girls and women have undergone the practice worldwide (UNICEF 2016).

Besides a vast anthropological literature on the existence of FGC (Shell-Duncan et al. 2011), there is still little understanding of the reasons why it persists. Recent studies show that both individual preferences and normative forces play a role, and contribute in large portion to the persistence of the practice (Bellemare, Novak, and Steinmetz 2015; Efferson et al. 2015; Vogt et al. 2016). Education is often depicted as an effective instrument for abandoning the practice, but strikingly, causal evidence is scant (International Center for Research on Women 2016).[1]

In this paper we study the causal effect of maternal education on the probability that daughters undergo FGC. We focus on Nigeria, a country where 20 million girls and women are estimated to be circumcised, representing 10 percent of the global total. To establish causation, we consider the introduction of the Universal Primary Education (UPE) program as a natural experiment (Osili and Long 2008; Bhalotra and Clarke 2013; Fenske 2015; Larreguy and Marshall 2017).

Using data from the 1999 Nigeria Demographic and Health Survey, we document a small negative association between mothers' education and the probability that their daughters undergo FGC. We confirm that UPE significantly increases years of education for women in the exposed cohorts, but we find no significant impact of the reform on the probability that their daughters are cut. As a potential mechanism for the absence of a significant causal effect, we examine whether UPE affects women's attitudes toward FGC. If education does not reduce a mother's support for FGC, it might as well not change her decision to have her daughter cut. Indeed we find no evidence that mothers' education alters their attitudes toward FGC.

I. Universal Primary Education Program

The Universal Primary Education program, which was introduced in 1976, was a large-scale,

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[1] Nesje (2014) estimates the causal effect of education on FGC in Kenya by exploiting an education reform that extended the length of primary school by one year. However, since this identification strategy exploits exogenous variation in education only along one dimension (timing of birth) in an instrumental variable framework, it does not allow full control for differences across cohorts.

nationwide program designed to expand primary education in Nigeria. The main objective of UPE was to provide tuition-free education. The reform supplied six years of free primary education starting from age six for all children (Larreguy and Marshall 2017).

To implement the reform, the government invested substantially in the construction of new classrooms, teacher training, and teaching equipment. Investments varied considerably across states reflecting the different prior enrollment rates. In particular, states in the northern and eastern regions received the highest levels of funding (Osili and Long 2008).

UPE ended in 1981 and subsequently most states reintroduced school fees (Osili and Long 2008). Nevertheless, the enrollment continued to increase after UPE ended (Larreguy and Marshall 2017).

II. Data

In this paper we use micro-level household survey data from the Nigerian Demographic and Health Survey (DHS) for the year 1999 (National Population Commission Nigeria 2000). The sample is designed to be representative of women aged 10–49.[2]

The DHS includes a module of questions about FGC. Women are asked whether they are circumcised, the age at cutting, attitudes toward FGC, if their eldest daughters are cut, the daughters' age at cutting, and who performed the procedure. Our main outcome is a dummy variable that equals one if the mother reports that her eldest daughter is cut.[3] As an additional outcome we use a dummy variable that equals one if the mother replied yes to the question "Female circumcision should continue" and zero otherwise.

III. Empirical Framework

To identify the effect of education on FGC, we exploit the plausibly exogenous variation in access to schooling generated by UPE using a difference-in-differences (reduced form) and instrumental variable approach as in Larreguy and Marshall (2017).

The first source of variation is differential exposure to the reform across cohorts (years of birth). Children who had not yet started school in 1976 (born after 1969) benefited from the reform the most.

The second source of variation is across states, using the fact that UPE had larger effects in states where primary enrollment was low before 1976. Following Larreguy and Marshall (2017), we measure reform intensity as the proportion of the female state population born between 1960 and 1969 that had not completed primary education.[4] These proportions are derived from the 2009 Harmonized Nigeria Living Standard Survey (HNLSS), and merged to the DHS via the respondent's state of residence.

We estimate the following regression model:

$$(1) \quad y_{ics} = \beta\big(Post\_UPE_c \times Intensity_s\big) + X_i'\gamma + \delta_c + \lambda_s + \eta_s\big(\lambda_s \times c\big) + \epsilon_{ics},$$

where i indicates the individual, c year of birth, and s state. y_{ics} is our outcome of interest (a dummy for whether the eldest daughter is cut or a dummy for reporting that FGC should continue). β is the reduced form estimate of the effect of UPE. $Post\_UPE_c$ is a binary indicator that equals one for cohorts born after 1969. $Intensity_s$ is the proportion of female state population born 1960–1969 that had not completed primary school. X_i' includes controls for the main ethnic groups, religion, and living in an urban area. δ_c and λ_s are cohort and state fixed effects, while η_s are state-specific cohort trends. Standard errors are clustered at the state level (37 states). We also report wild bootstrap

[2] The 2003, 2008, and 2013 DHS are not considered because in those surveys, differently from the 1999 DHS, the FGC module is asked only to women who are aware of FGC. In those years, about 30–40 percent of respondents declare not to know what FGC is, potentially leading to a selected sample.

[3] Due to an error in the 1999 questionnaire, only women who had sexual intercourse were asked the FGC module. This is not problematic given that we must necessarily focus on mothers to look at daughters' FGC status.

[4] Larreguy and Marshall (2017) exploit variation in reform intensity at the more disaggregated Local Government Areas (LGAs) level. As LGAs are not identified in the 1999 DHS, we measure intensity at the state level. Our results are robust to using alternative measures of reform intensity as in Larreguy and Marshall (2017) (results not reported).

p-values to account for the limited number of clusters (Cameron, Gelbach, and Miller 2008).

The main identifying assumption is that, in the absence of the UPE reform, trends in education and FGC would have been similar across states with different reform intensity. To control for differential pre-trends, we include state-specific cohort trends in all the specifications. Additionally, as in Larreguy and Marshall (2017), we provide a placebo test restricting the sample to those born before 1965, and using being born after 1959 to define exposure to a fake reform. We exclude cohorts 1965–1969 that might be partially treated because they were possibly still in primary school at the time of the reform (Osili and Long 2008). In online Appendix Table A1, we find a *negative* and statistically significant effect of the placebo reform on years of education (column 1), that is an opposite sign relative to the expected effect of the reform, and a positive but statistically insignificant effect on the probability that the eldest daughter is cut (column 3). When state-specific cohort trends are included, the coefficient on the placebo reform is statistically insignificant for both outcomes (columns 2 and 4).

While the reduced form estimates inform us about the effect of UPE on FGC, we are ultimately interested in the effect of schooling on FGC, which we estimate using an instrumental variables approach (two-stage least squares (2SLS)). In the first stage, which consists of equation (1) with mothers' completed years of education as outcome, we estimate the effect of UPE on educational attainment. In the second stage, we use $Post\_UPE_c \times Intensity_s$ as the excluded instrument. If the instrument satisfies the exclusion restriction, the 2SLS estimate identifies the causal effect of education, and can be interpreted as the local average treatment effect of one year increase in education for the women who stayed in school longer because of the reform (compliers). The exclusion restriction would be violated if the reform had an impact on FGC beyond its effect on completed years of education, for instance, if health or sexual education were included in the curriculum. Although the UPE curriculum was quite ambitious and included topics such as citizenship education and moral training (Achor 1977), we found no evidence that it included health, hygiene, or sexual education. Another potential violation to the exclusion restriction would be if UPE also improved the quality of education, but that does not seem to be the case as reported by Oyelere (2010).

Ideally we would like to calculate the reform intensity for the state where women attended primary school, but the DHS only provides the state of residence. There could be a concern that endogenous migration might bias our estimates. Osili and Long (2008) show that female migrants and non-migrants are similar in their educational level. To further rule out selective migration, we consider only cohorts born before 1976 when UPE was introduced (Duflo 2001; Larreguy and Marshall 2017).[5] We also exclude cohorts born before 1950 because they are too old (Larreguy and Marshall 2017).

IV. Results

Table 1 reports the main results. The first-stage estimate indicates that the UPE reform increases women's completed years of education by 2.1 years, and the effect is statistically significant at the 1 percent level (column 1). Column 2 shows a negative and statistically significant association between a mother's educational attainment and the probability that her eldest daughter undergoes FGC. The reduced form estimate of the effect of UPE on FGC is negative, but not statistically significant (column 3). In column 4, the 2SLS estimate shows that an additional year of education decreases the probability that the eldest daughter is cut by 0.3 percentage points, but the estimate is not statistically different from zero. In columns 5–7, we examine the effect of education on attitudes. More educated mothers are less likely to say that FGC should continue, but the reduced form and 2SLS estimates are not statistically different from zero. Overall, the results on attitudes are consistent with the findings on behavior and suggest that UPE did not significantly change the practice of FGC in Nigeria.

In the remainder of this section we discuss threats to validity and robustness checks. First, since only women who have at least a daughter are included in the sample, changes in fertility induced by UPE may lead to selection into our sample. Osili and Long (2008) show that

[5] Our main results are robust to adding women born in or after 1976 (see Table A2 in the online Appendix).

Table 1—The Impact of UPE and Schooling on FGC

	Years of education	Eldest daughter is cut			FGC should continue		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years of education		−0.007		−0.003	−0.010		0.049
		(0.001)		(0.031)	(0.002)		(0.030)
		[0.000]		[0.926]	[0.001]		[0.117]
Post-UPE × intensity	2.105		−0.006			0.103	
	(0.561)		(0.065)			(0.070)	
	[0.000]		[0.933]			[0.185]	
Observations	3,818	3,818	3,823	3,818	3,816	3,821	3,816
Mean of outcome	4.079	0.220	0.220	0.220	0.199	0.199	0.199
First-stage *F* test	14.09						
Model	OLS	OLS	OLS	2SLS	OLS	OLS	2SLS

Notes: The sample includes only women who respond to questions on daughters' FGC status and are born in the years 1950–1975. Every column includes the controls: urban area of residence, dummies for religion and ethnicity, cohort fixed effects, state of residence fixed effects, and state-specific linear trends in year of birth. Standard errors are clustered at the state level and reported in parenthesis, wild bootstrapped *p*-values are reported in brackets.

UPE decreases the number of children born to a woman before age 25. Nonetheless, we find no evidence that UPE changes the probability of having at least one child or at least one daughter before age 24, which is the age threshold relevant for our sample (Table A3 in the online Appendix).[6]

Second, some daughters might be too young and still at risk of being cut at the time of the survey. As a robustness check, we restrict the sample to mothers who have at least one daughter older than five and the results are not altered (columns 3–4, Table A3 in the online Appendix).[7]

Finally, misreporting may bias our estimates if more educated women have different likelihoods of reporting daughters' FGC status relative to less educated women. However, if that were the case, we would also expect mothers' attitudes regarding FGC to vary with their education, which is not the case. Moreover, at the time of the survey there were no laws against FGC, which should reduce the potential underreporting due to a change in the legality of the practice (De Cao and Lutz 2018).

[6] We focus on fertility before age 24 to avoid censoring problems given that our treated cohorts are 23–29 years old at the time of the survey (born 1970–1975). Results are very similar when we use 25 as the threshold.

[7] Data from the 2013 DHS show that 80 percent of women who were born in the same years as the daughters in our sample (1962–1999) were cut by age five (conditional on being cut).

V. Discussion and Conclusion

Sustainable Development Goal Number Five includes a target to eliminate all harmful practices including FGC. Education is often advocated as one of the vital steps for the eradication of FGC, but empirical evidence on the causal impact of education on FGC is scarce. This paper uses the introduction of Universal Primary Education in Nigeria as a natural experiment to identify the impact of maternal education on the probability of having the eldest daughter cut. We find no significant evidence that an increase in primary schooling decreases FGC or the support for the practice.

The estimates suggest that it might be hard to change behaviors without changing attitudes. Understanding the benefits that parents attribute to cutting is therefore necessary to identify policies that may help reduce FGC (Efferson et al. 2015; García Hombrados and Salgado 2019).

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