Gender Norms and the Quantity–Quality Tradeoff: Evidence from Free Primary Education in Africa*

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Abstract: This paper investigates the impact of free primary education (FPE) on parents' fertility decisions in sub-Saharan Africa and how these effects depend on prevailing gender norms. Leveraging the staggered rollout of FPE across 17 countries and a within-ethnicity design, we find that the reform reduced completed fertility by 0.25 births per woman. Reduced fertility coincides with both improved child survival and school enrollment. Linking these data with ethnic-level cultural norms of male dominance, we show that these effects arise exclusively among groups with low male dominance. Consistent with fertility reductions increasing women's autonomy, FPE also strengthens women's outside options and intra-household bargaining power. In low male-dominance settings, this is reflected in higher divorce probabilities, lower acceptance of intimate partner violence, greater participation in household decisions, and increased media engagement—changes absent in high male-dominance cultures. A simple model embedding the quantity–quality tradeoff within a limited-commitment household bargaining framework, where gender norms determine women's veto power over fertility, rationalizes these findings. Overall, the results underscore the central role of gender norms in shaping the effects of policy interventions on fertility and broader development outcomes.

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1 Introduction

Sub-Saharan Africa is on a demographic path that differs fundamentally from that of the rest of the world. While fertility has fallen sharply elsewhere, many African countries continue to experience persistently high fertility rates, with projections indicating that by the early twenty-second century the region will account for more than half of all global live births (Bhattacharjee et al., 2024). These demographic patterns carry wide-ranging implications: rapid population growth exerts pressure on education systems, labor markets, health services, and environmental resources (Das Gupta et al., 2011). Understanding the determinants of fertility behavior in sub-Saharan Africa is therefore central to both local development trajectories and the global demographic landscape.

A central idea in economics is that fertility decisions respond to the costs and returns to investing in children. The classic quantity–quality framework (Becker, 1960; Becker and Lewis, 1973) predicts that reducing the cost of child quality—such as by improving access to schooling or lowering its price—induces parents to substitute toward fewer but better-educated children. This mechanism is thought to be especially salient in developing countries, where scarce high-quality public schooling and the persistence of child labor make the opportunity cost of education particularly high (Doepke, 2015). Yet whether the quantity–quality tradeoff operates in sub-Saharan Africa remains unclear (Vogl, 2022). When women lack autonomy over contraceptive use and fertility decisions—a common feature of societies with strong patriarchal norms—household decisions may reflect male preferences for large families more than the economic incentives facing the household, potentially rendering fertility less responsive to reductions in the cost of child quality (Ashraf et al., 2014; Doepke and Kindermann, 2019; Cassidy et al., 2021).

This paper provides new evidence on how reducing schooling costs affects fertility behavior in sub-Saharan Africa, and how these effects are shaped by deep-rooted gender norms. Our analysis is motivated by a remarkable sequence of education reforms: beginning in the mid-1990s, several African countries abolished primary-school tuition fees by introducing free primary education (FPE). These reforms substantially increased school enrollment (Osili and Long, 2008; Lucas and Mbiti, 2012; Keats, 2018; Valente, 2019), and represent one of the most ambitious human-capital policy shifts in recent African history. However, despite the centrality of children in these reforms, their immediate effects on parents—and in particular on parents' fertility decisions—remain largely unexplored.

Against this backdrop, we examine three central questions. First, does the introduction of FPE alter parental fertility behavior in sub-Saharan Africa? Second, are these effects moderated by local gender norms? Third, insofar as fertility declines are observed, do they coincide with increases in women's autonomy and decision-making capacity? Answering these questions provides a more comprehensive understanding of how large-scale education reforms shape demographic transitions and intra-household allocation.

To address these questions, we assemble several data sources. First, we use women's complete birth histories from the Demographic and Health Surveys (DHS) to construct an annual panel tracking fertility from age 15 through the interview date. Second, we link each woman's self-reported ethnic affiliation to the Male Dominance Index (MDI) developed by Guarnieri and Tur-Prats (2023), which draws on pre-industrial traits from the Ethnographic Atlas (Murdock, 1967) that historically shaped gender roles and continue to influence contemporary behavior. Third, we compile detailed information on the timing of FPE adoption across 17 sub-Saharan African countries.

Our identification strategy exploits the staggered rollout of FPE and addresses two key challenges. First, because ethnic groups span multiple countries, we use within-ethnicity variation in exposure, allowing us to control for deep-rooted, time-invariant cultural characteristics—such as lineage systems, customary laws, or traditional gender roles—that might otherwise confound the estimates. Second, treatment-effect heterogeneity and dynamics can bias conventional difference-in-differences estimators. We therefore employ the estimator proposed by Borusyak et al. (2024), which provides unbiased average treatment effects on the treated (ATT) and enables us to test the key identification assumptions—parallel trends and absence of anticipation effects—separately from ATT estimation.

Our first set of results examines the effects of FPE on fertility and child outcomes, with a particular focus on heterogeneity by gender norms. We begin by showing that the introduction of FPE leads to a reduction in fertility: the total number of children per woman—including those who have not yet completed fertility—declines by 0.1. When restricting to women aged 35 and above, and thus closer to having completed their fertility, the effect increases in magnitude to -0.25, which corresponds to a 5.6 percent decline relative to the sample mean. To put this effect in context, a sustained reduction of one birth per woman over forty years has been estimated to yield a population nearly half a billion smaller in sub-Saharan Africa (United Nations, 2024). The demographic implications are therefore profound. Our fertility estimates are robust to the inclusion of individual-level controls and to alternative difference-in-differences strategies.

We next show that fertility declines coincide with improvements in child outcomes. Child survival rises by 3 percentage points, and school enrollment increases by 7 percentage points. These findings are consistent with the predictions of the quantity–quality theory, whereby lower fertility leads to higher human capital investment per child.

A striking feature of the results is the substantial heterogeneity by gender norms. Fertility reductions arise entirely among ethnic groups with low male dominance: fertility falls by 0.2 children per woman, an effect rising to -0.35 for women closer to completed fertility. In high male-dominance groups we find no effect of FPE on fertility. Improvements in child outcomes likewise concentrate among low-MDI groups. To verify that this heterogeneity reflects gender norms rather than correlated cultural or institutional factors,

we compare high- and low-MDI ethnicities along several characteristics linked to fertility behavior: old-age support (multigenerational family structures), the economic value of children (age specialization in subsistence activities), inheritance regimes (partible versus impartible), and potential returns to education (ancestral economic and political complexity). Across all dimensions, we find no systematic differences between high- and low-MDI groups, indicating that the heterogeneity we observe is driven by gender norms rather than alternative cultural or institutional traits.

Our second set of results examines whether the fertility reductions induced by FPE are accompanied by gains in women's autonomy and decision-making capacity. Because DHS empowerment indicators are measured only cross-sectionally, we exploit within-ethnicity variation in the length of exposure to FPE to study these outcomes. We begin with fertility-related behaviors. Among low-MDI groups, FPE increases both women's use of contraception and their reported influence over contraceptive decisions. These changes may facilitate the lower fertility we document, but they may also reflect some shift in women's agency in fertility decision-making.

Beyond these effects, the fertility decline induced by FPE may free women's time for employment and other non-care activities, thereby strengthening their outside options and intra-household authority (Duflo, 2012). Consistent with this hypothesis, we find that FPE raises divorce probabilities and reduces acceptance of intimate partner violence among low-MDI groups—patterns consistent with bargaining models linking improved outside options to reduced domestic violence (Aizer, 2010; Anderberg et al., 2016). In addition, FPE increases women's employment rates and their participation in major household decisions, including large purchases, their own healthcare, and visits to relatives, with these responses again concentrated primarily among low-MDI groups. Further shifts in women's roles are reflected in greater engagement with media—such as higher newspaper readership and radio listenership—among women from low male-dominance ethnic groups.

Finally, to the extent that FPE reduces households' reliance on large numbers of children, it may also affect the prevalence of polygyny. Although the share of women in polygynous unions is smaller in our sample than in the region as a whole, we find that FPE reduces the likelihood of being in such unions among women in low-MDI ethnic groups. This shift may coincide with another, more indirect source of empowerment: as households move toward monogamy, strategic motives for co-wife competition could weaken (Rossi, 2018).

To interpret these findings, we develop a simple model that embeds the canonical quantity-quality tradeoff in a limited-commitment household bargaining framework (Doepke and Kindermann, 2019), with culture captured through women's veto power over fertility decisions. In high male-dominance groups, women lack veto authority, so men's preferred (and typically higher) fertility prevails. In low male-dominance groups,

women possess veto power and can block high-fertility choices. The model's predictions align closely with our empirical patterns: fertility falls in response to lower schooling costs only where women hold effective agency, and when this is the case, reductions in fertility are accompanied by improvements in women's bargaining outcomes.

Our paper contributes to research on the economic determinants of demographic transitions (Doepke, 2005; Cervellati and Sunde, 2015; De Silva and Tenreyro, 2020; Delventhal et al., 2021) and to recent work on fertility patterns within sub-Saharan Africa. Prior studies highlight the roles of education (Vogl, 2022), female labor supply (Zipfel, 2024), and inheritance institutions (Fontenay et al., 2024), as well as less-explored dynamics such as poverty-induced childlessness (Baudin et al., 2020) and population homeostasis (De la Croix and Gobbi, 2022). We contribute by identifying a policy-induced channel of fertility change and by showing that the canonical quantity-quality mechanism operates only under specific cultural conditions.

Our study also relates to work on how policy shapes fertility decisions. Some of this literature examines colonial-era interventions—such as family planning mandates or forced labor migration—and their long-run demographic legacies (Guirkinger and Villar, 2022; Canning et al., 2024; Dupas et al., 2023). Other contributions document how contemporary policies alter fertility incentives, including social pensions in Namibia (Rossi and Godard, 2022) and tuition-fee reforms in Uganda (Burlando and Bbaale, 2022). Experimental evidence highlights the roles of household bargaining, education subsidies, media exposure, and maternal health information (Ashraf et al., 2014; Duflo et al., 2015; Glennerster et al., 2021; Ashraf et al., 2022). Our paper provides novel evidence from a diverse range of countries and cultural contexts on the impacts of a policy that reduces the costs of having children. More broadly, our results align with research demonstrating that the effects of policy can be moderated by cultural norms (Bau, 2021; Bau and Fernández, 2023; La Ferrara and Milazzo, 2017; Ashraf et al., 2020).

Finally, we contribute to the literature evaluating free primary education programs. Existing work documents substantial improvements in enrollment but mixed impacts on learning (Lucas and Mbiti, 2012; Valente, 2019; Zipfel, 2021; Moussa and Omoeva, 2020; Fatoké Dato, 2022; Taylor and Spaull, 2013). Studies from Nigeria and Uganda find that FPE increased girls' educational attainment and reduced their fertility in adulthood (Osili and Long, 2008; Keats, 2018). We extend this literature by showing that FPE also affects the parents of targeted children, altering contemporaneous fertility behavior and reshaping women's intra-household bargaining power across diverse cultural contexts.

2 Primary Education in Sub-Saharan Africa

In most countries in sub-Saharan Africa, children are expected to enrol in primary school at age 6 or 7, with primary school comprising 6 to 8 grades. Most countries in the region

now offer free primary education, with many having introduced it in recent decades. Existing evidence overwhelmingly shows that FPE led to significant increases in enrollment and educational attainment.[1]

User fees are a key barrier to accessing many public services in the region, including primary education (Bentaouet Kattan and Burnett, 2004; Kremer and Holla, 2009). Although information on primary school user fees are difficult to ascertain, estimates of yearly tuition fees prior to FPE include US$16 in Kenya (Lucas and Mbiti, 2012), US$5-8 in Uganda (Keats, 2018), US$3.30-4.60 in Tanzania (Valente, 2019) and US$3.20 in Benin (Fatoké Dato, 2022).[2] In our analysis, we consider all FPE policies that eliminated tuition fees, although households may have continued to incur other education-related costs. For example, FPE in Ethiopia entailed the removal of fees for tuition and uniforms, while fees for textbooks and sports activities remained (Bank, 2009). In Kenya, FPE only removed fees for tuition, but not for exams and uniforms (Riddell, 2003). In Mozambique, FPE initially waived tuition fees only up to 5th grade before later extending coverage to 8th grade. In Uganda, the policy first applied to only four children per household, and was expanded to include all children six years later.

Many countries financed the introduction of FPE through international aid. Donor funding from international development agencies contributed 40% of educational spending in Malawi, 50% in Uganda and Zambia, and 60% in Tanzania. Other countries were able to finance FPE via the national budget. For example, Nigeria funded FPE using increased oil revenues in the 1970s (Osili and Long, 2008). This could pose a threat to our analysis if FPE coincided with economic expansions that affect fertility through other channels. In Section 5.3 we show that, prior to the introduction of FPE, there was no divergence in macroeconomic indicators of development between treatment and control countries.

Increases in enrollment following FPE could lead parents to transfer their children to private primary schools in order to avoid overcrowded classrooms (Lucas and Mbiti, 2012; Tessa Bold and Sandefur, 2014). If that was the case, such families would still be faced with tuition fees, which may attenuate our estimates. However, data suggest that FPE did not lead to widespread increases in private school enrollment in the countries in our sample: in the five years prior to FPE, the average proportion of enrolled students in

[1]Table C.1 in the Appendix lists all the 17 countries included in our analysis alongside the year of introduction of FPE. Studies documenting increased enrollment in these countries include Lucas and Mbiti (2012) for Kenya, Keats (2018) for Uganda, Osili and Long (2008) for Nigeria, Zipfel (2021) for Ghana, Malawi, and Uganda, and Moussa and Omoeva (2020) for Malawi and Uganda. These are complemented by similar findings for other countries such as Tanzania (Valente, 2019; Zipfel, 2024) and Ethiopia (Moussa and Omoeva, 2020).

[2]To put these costs into perspective, consider that women in our sample had an average of 5.12 children at the end of their reproductive period. According to median per capita income or consumption estimates from the World Bank's Poverty and Inequality Platform processed by Our World in Data (World Bank Poverty and Inequality Platform, 2024), sending five children to primary school before the introduction of FPE would have cost the equivalent of 10 days of median income in Benin, 20 days in Tanzania, 25 in Uganda, and up to 30 days in Kenya.

private primary schools was 17.7%; in the five years after FPE, this figure fell to 13.6% (UNESCO Institute for Statistics, 2025).

3 Data

We construct a novel dataset that combines a variety of sources on women's fertility histories, the introduction of free primary education and ethnographic characteristics. This section provides an overview of the main data sources and how we construct the sample used for analysis.

3.1 Demographic and Health Survey

Data come from the Demographic and Health Survey (DHS). The DHS is a household survey administered in developing countries across the world. The DHS includes a household questionnaire, which is completed by one adult member of each household and collects information on all household members, their health status and educational attainment, as well as the household's living standards. The DHS also includes a woman's questionnaire, which is given to all women between the ages of 15 and 49 residing in the household. The woman's questionnaire collects background information on each respondent, such as her age, ethnicity, education, employment and marital status.

Importantly for our analysis, the woman's questionnaire includes a fertility history module, where respondents are asked about their entire birth history. For every child born to the respondent, information is collected on whether the child was part of a single birth or a twin/triplet birth, the child's sex, date of birth, whether the child is alive at the time of the survey, whether the child lives with the respondent and, for children who are not alive, the child's age at death. This information is collected for each birth, starting with the respondent's first birth. To ensure every birth is accounted for, after collecting information on each birth, respondents are asked whether they had any other births between a given birth and the preceding one, including any children who died after birth. Respondents are also separately asked for the total number of sons and daughters they have had and how many of them are alive at the time of the survey. The answers to these questions are cross-checked to ensure accuracy.[3] In addition, women are asked about the ideal number of children they would like to have.

3.2 Free Primary Education

We gather data on the introduction of free primary education from various national and international sources. As detailed in Section 2, we define our treatment variable as a

[3]Some more recent surveys ask about each pregnancy, rather than each birth, capturing information on any miscarried or aborted pregnancies. Older surveys ask whether women have ever had a terminated pregnancy, but do not collect information on all pregnancies. We therefore focus only on births.

binary indicator equal to 1 if any form of FPE is present in a given country and year. We then match this indicator to the DHS data by country and year.[4]

3.3 Male Dominance Index

To incorporate gender norms into the analysis, we match respondents' self-reported ethnic affiliation to the Male Dominance Index (MDI) of Guarnieri and Tur-Prats (2023). The MDI aggregates ten pre-industrial ethnic characteristics from the Ethnographic Atlas (EA) that theory and empirical evidence have linked to the formation of gender (in)equality. Ancestral family structures (such as lineage and post-marital residence), marriage practices, and subsistence activities have historically determined women's roles in the economy, family, and society. Ethnic traits that increased women's autonomy, economic participation, or support from kin tended to generate more gender-equal norms, whereas traits that limited women's contributions or heightened male control tended to produce more unequal norms.

As described in Guarnieri and Tur-Prats (2023), lineage and residence rules determine the kinship support available to women, the flow of resources, and their protection within the household. In matrilineal societies, descent and inheritance flow through women, giving them greater support from kin networks and stronger household bargaining power (Schneider and Gough, 1961). Bride price is considered by anthropologists as a practice recognizing women's value, and found to be more common where women contribute significantly to agricultural production (Vroklage, 1952). The same is true for polygyny. Although correlated with lower gender equality in contemporary cross-country analyses (Tertilt, 2005), anthropologists highlight that ancestral polygyny traditionally arose where women displayed high productivity in subsistence agriculture (Boserup, 1970; Jacoby, 1995; Goldschmidt and Kunkel, 1971; Bau and Fernández, 2023). As for post-marital residence patters, nuclear residence leaves women without protection or support from extended kin; conversely, extended households can deter abuse within the home and facilitate women's labor-force participation (Khalil and Mookerjee, 2019; Tur-Prats, 2019). Subsistence activities shape the gender division of labor and thus gender norms: shifting cultivation, foraging, fishing, and non-herding animal husbandry involve substantial female labor and elevate women's economic and political status, while plough agriculture and pastoralism concentrate productive control in men and foster norms that restrict women's mobility, sexuality, and autonomy (Boserup, 1970; Alesina et al., 2013; Martin and Voorhies, 1975; Becker, 2025).

Guarnieri and Tur-Prats (2023) group these ethnic characteristics into an index using principal component analysis. Consistently with the literature summarized above, the first principal component (the MDI) loads negatively on "gender-equal" traits and posi-

[4]Appendix Table C.1 provides more information on when each country in the data introduced FPE and the sources from which the information was collected.

tively on "male-dominant" traits.[5] The index ranges between 0 and 1, with higher values denoting higher degrees of male dominance. Because it is based on ancestral characteristics observed prior to European contact and industrialisation, the MDI pre-dates modern institutions and policies, including the introduction of FPE. This feature of the MDI is key for the purpose of the analysis.

To merge the MDI to our dataset, we assign a Murdock ethnic group—as defined in the Murdock Map (Murdock, 1959)—to DHS respondents' self-reported ethnicity. To achieve this, we employ the *Linking Ethnic Data from Africa* (LEDA) R package developed by Müller-Crepon et al. (2022), which exploits information on the languages spoken by members of each ethnic group. The LEDA algorithm establishes links between ethnic groups across datasets if their languages share at least one node at a specified level of the linguistic tree. We specify "dialect" as the level on which to match DHS ethnicities to Murdock ones. Appendix C provides a detailed description of the matching procedure.

Finally, we categorize each ethnic group as either "male-dominant" or "gender-equal" based on whether its MDI is above or below the median MDI of all the ethnicities in the sample, respectively.

3.4 Sample Construction

As the DHS includes detailed information on the timing of birth for all respondents' children, we are able to construct an annual panel dataset of female fertility. Each observation in our sample represents a calendar year for one woman and includes information on the total number of children each woman has given birth to by the end of that calendar year. We are able to construct a panel covering the total fertility of each woman from age 15 to the age at which she is observed in the DHS.

After assigning individuals' self-reported ethnicity to the Murdock Map using LEDA, we restrict the sample to individuals from ethnic groups that are present in multiple countries, with at least one of those countries having implemented FPE. We designate the first country to introduce FPE as the treatment country and we restrict the panel to ten years before and after the introduction of FPE. In cases where the control country within an ethnic group introduces FPE less than ten years after the treatment country, we exclude observations from that ethnic group from this point onward. This is because there exists no suitable control group remaining for that ethnic group.

We also create a sample of children born to mothers in our sample in the 10 years before and after the introduction of FPE. From the woman's survey, we observe children's year of birth and whether they have survived to the time of the survey. From the household

[5]For additional information on the index, see Guarnieri and Tur-Prats (2023). The MDI is available for ethnic groups included in both the EA and the Murdock Map of African ethnic homelands (Murdock, 1959). The latter lists a larger number of ethnic groups than the EA. For a comprehensive description of the differences between the two sources and how the ethnic characteristics provided by the EA can be linked to the Map, see Lowes (2021).

survey, we observe whether a child is enrolled in school and their completed years of schooling.[6]

3.5 Descriptive Statistics

We identify 20 instances of FPE affecting members of an ethnic group that is split by an international border. These instances affect 19 different ethnic groups present in 17 different countries. Figure 1 presents a map of the ethnic groups and countries included in our data.

The resulting dataset contains 663,439 observations from 67,230 women observed in 58 DHS surveys.[7] Panels A and B of Table 1 present descriptive statistics for women in our sample. The average woman is born in 1973 and interviewed in 2005. She is observed at age 24 and has 2.16 children. 65% of women have at least one child. Our panel tracks women from age 15 to the age at which they are observed by the DHS. We have an average of just under 10 observations per woman, and 22% of our observations are treated by free primary education.

Our children's sample comprises 142,548 children, for which summary statistics are presented in Panel C. The average child was born in 1996 and is observed at age 10. 83% of children survive to the time of the survey. 56% of children are enrolled at the time of the survey and the average child has 2.36 years of education.

4 Empirical Strategy

We use a within-ethnicity difference-in-differences design to identify the causal effect of free primary education on fertility. Our strategy employs a staggered difference-in-differences setup exploiting variation in the introduction of FPE across countries inhabited by individuals of the same ethnicity and over time. Were we to implement our empirical strategy using two-way fixed effects, we would do so by estimating the following equation:

$$y_{icet} = \beta \cdot FPE_{ct} + \lambda_{ec} + \gamma_{et} + \mathbf{B}'_{ie}\theta + \varepsilon_{icet} \tag{1}$$

where y_{icet} represents the total number of children born to woman i in country c, of ethnicity e by the end of calendar year t. FPE_{ct} is an indicator equal to 1 if free primary education has been introduced in country c by year t. λ_{ce} are ethnicity-specific country fixed effects, which account for time-invariant determinants of fertility that vary across countries within an ethnic group. γ_{et} are ethnicity-specific year fixed effects, which account for common trends in fertility within an ethnic group across countries. To account for any changes to fertility patterns across birth cohorts, we include a vector of ethnicity-specific

[6] As early DHS surveys did not collect information on children's education, we have a reduced sample size when examining these outcomes.

[7] Appendix Table C.3 presents a list of surveys included in our sample.

birth cohort fixed effects, $\mathbf{B}_{ie}$. Standard errors are clustered at the DHS sampling cluster level as this is the level at which households are sampled by the DHS. These correspond to villages or city blocks.[8]

Due to the staggered implementation of FPE, estimating equation 2 would lead to biased estimates. An ever-growing literature on difference-in-differences estimation has shown that, when treatment is staggered in time, estimated treatment effects are biased by the presence of one or both of heterogeneous and dynamic treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024).

Because of this threat to identification, we use the difference-in-differences estimator proposed by Borusyak et al. (2024), which produces unbiased estimates of the average treatment effect on the treated (ATT) under a staggered treatment rollout. We choose this estimator due to the efficiency advantages associated with it, in comparison to other alternative robust difference-in-differences estimators (Borusyak et al., 2024). This is an imputation estimator, based on the following procedure. First, ethnicity-specific country, time and birth cohort fixed effects are fitted using only untreated observations. Second, these fixed effects are used to impute the untreated potential outcomes for each treated observation, and thus the respective estimated treatment effect. The average of these treatment effects produces the ATT.[9] Event studies are estimated in a similar manner, whereby estimated treatment effects are aggregated separately for each period after treatment.

When examining the effect of FPE on children's outcomes, we conduct the analysis at the level of the child, exploiting variation in the timing of birth before and after the introduction of FPE. In addition to ethnicity-specific country and birth year fixed effects, we include ethnicity-specific age fixed effects to account for the fact that children are observed at different ages. When examining child survival, these are replaced with fixed effects for potential age, were all children to survive.

Difference-in-differences estimation does not require that treatment is randomly assigned, but rather that outcomes in the treatment and control groups would have followed parallel trends in the absence of treatment. One potential violation of this condition in this setting could arise due to selection into treatment. For example, countries may introduce FPE after a sustained period of economic growth, when public finances are in a strong position. If longer-term growth leads to declines and delays in fertility, as shown by Chatterjee and Vogl (2018), or to better outcomes for children, then treated countries

[8] In Section 5.3, we show that the results are robust to clustering at either the ethnicity-country-year or ethnicity-country levels.

[9] In our setting, this approach is equivalent to estimating separate difference-in-differences models for each ethnicity (each of which possesses no within-group variation in treatment timing) and then aggregating the resulting ATTs, where each ethnicity-specific ATT is weighted by the share of post-treatment observations in each ethnic group.

may have experienced changes in fertility trends (or children's outcomes) even before the introduction of FPE. This would constitute a violation of the key identifying assumption. We provide two key pieces of evidence supporting the validity of our design. First, in Section 5, a visual inspection of pre-trends from the event studies shows that there does not appear to be any differential trends in fertility or children's outcomes before the introduction of FPE. Second, in Section 5.3, we employ the same empirical design to show that there did not appear to be diverging trends in key country-level macroeconomic indicators in the years prior to the introduction of FPE.

5 Results

Table 2 reports estimates of the average effect of FPE on fertility and on children's outcomes, obtained using Borusyak et al.'s (2024) procedure. The estimate in column 1 indicates that the introduction of FPE is associated with a significant reduction in fertility: women exposed to FPE experience a 3.7 percent reduction in total fertility, equivalent to 0.092 fewer children per woman.

Next, we turn to examining whether the FPE-induced reduction in the quantity of children occurs concurrently to an improvement in children's outcomes, as would be expected in the presence of a quantity-quality tradeoff. To explore this, we leverage women's birth histories and categorize as treated those children who were born after the introduction of free primary education in their country. We find evidence of increased investments in children. Column 2 indicates that treated children are 3 percentage points more likely to survive to the survey compared to their counterparts unaffected by the introduction of FPE. When compared to the pre-treatment sample mean, this effect amounts to a 3.4 percent higher likelihood of survival. While no sizeable or significant effect can be found on children's years of education, the introduction of FPE increases the likelihood that children are enrolled in school at the time of the survey. Taken together, these results suggest that FPE generates economic incentives for parents to prioritize child quality over quantity.

These results hinge on the assumption that, had FPE not been introduced, treated countries would have exhibited the same trends in fertility and child outcomes as control countries. Our empirical design—which exploits within-ethnicity, cross-country variation in FPE exposure—accounts for unobservable ethnic-specific, time-invariant determinants of fertility and children's outcomes, thus making the common-trends assumption more likely to be satisfied. Another related pre-requisite for the validity of our estimates is the absence of anticipatory effects: we should not observe that parents in treated countries change their fertility or child investment behaviors in anticipation of the arrival of free primary education.

To provide supporting evidence for the validity of the common-trends assumption

and test for the absence of anticipatory effects, we estimate an event-study specification following the Borusyak et al. (2024) methodology described in Section 4. Figure 2 reports the event-study graphs for fertility and child outcomes. Panels on the left (A, C, E and G) depict pre-treatment differences in each outcome between the treated and control groups, using t=−10 as the reference period. Panels on the right (B, D, F and H) present dynamic effects of FPE relative to the average over the entire pre-treatment period. The estimates for the pre-treatment period reveal no significant diverging trends in fertility or child outcomes in the years leading up to the introduction of FPE. This provides evidence for the validity of our design, thus warranting a causal interpretation of the estimates in Table 2.

The event-study specification also allows us to delve into the timing of the effects. Panel B of Figure 2 shows that the fertility response occurs immediately after the introduction of free primary education. Instead, the beneficial effects of the policy on children's outcomes tend to materialize only after a few periods, as shown in Panels D and H.

5.1 Heterogeneity by Gender Norms

The observed effects of FPE on the quantity and quality of children mask stark heterogeneities according to pre-existing, deep-rooted gender norms. Table 3 reports the estimates separately for women and children belonging to ethnic groups characterized by cultural norms with low versus high degrees of male dominance. The fertility-reducing effect of FPE can only be found among women in gender-equal societies: estimates in column 1 of Panel A indicate a 0.199, or 8 percent reduction in the number of children per woman. In contrast, there is no effect on fertility for women in male-dominant ethnic groups (see estimates in Panel B) after the introduction of FPE.

In line with these heterogeneous effects on fertility, we find that only children born in gender-equal societies experience an increase in their human capital following FPE implementation. Columns 2-4 of table 3 show that, for children born to mothers in gender-equal groups, FPE is associated with a 31 percent (or 16 percentage points) increase in the likelihood of being enrolled in school at the time of the survey.

Figure 3 displays the respective event-study graphs separately for women in more male-dominant (in red) and more gender-equal (in blue) ethnicities. Panels on the left (A, C, E and G) depict pre-treatment differences in each outcome between the treatment and control groups, using t=−10 as the reference period. The trends in Panel A indicate that treated women from male-dominant ethnicities experienced a rising trajectory in fertility compared to their control counterparts; conversely, there is no evidence of diverging trends for women in gender-equal ethnicities. Panel B—which present effects relative to the average over the *entire* pre-treatment period—demonstrates that the introduction of FPE had immediate impacts on fertility among gender-equal groups, leading to a significant fertility decline relative to the pre-treatment period.

Looking at children's outcomes, as can be seen in Panels C, E, and G, both male-dominant and gender-equal treated units display similar trends in all outcomes compared to their untreated counterparts in the periods leading to the introduction of FPE. The stark effect on enrollment illustrated in column 4 of Table 3 can be observed in Panel H: while no effect on enrollment can be detected among children in male-dominant groups, there is a visible significant positive effect among children in more gender-equal groups.

5.2 Effects on Completed Fertility

The results in Tables 2 and 3 are based on a sample of women with an average age of just under 24 and average of 2.16 children at the time of the survey. As many women in the sample are still in their reproductive years, these estimates do not capture the full effect of FPE on lifetime fertility. We therefore estimate the effect of FPE on the total number of children by restricting the sample to women who, at the time of the survey, are likely to be closer to completed fertility, i.e., aged 35 or older. As shown in Table 4, we find much larger effects on fertility which again are concentrated in low-MDI groups. We find a reduction in fertility of 0.346, equal to 8.4 percent of the sample mean.

Figure 4 displays the corresponding event study estimates. Panels A-B present the estimates for all women age 35 or older at the time of the survey. Panels C-D present the estimates separately for women in more male-dominant (in red) and more gender-equal (in blue) ethnicities. We do not find evidence of non-parallel trends for either sub-group. As above, we find a sharp drop in fertility after the introduction of FPE for women in more gender-equal groups.

5.3 Robustness Tests

Our baseline findings are robust to alternative estimators, specifications, and samples. Appendix Table A.1 reports the robustness of the estimates to employing alternative difference-in-difference estimators. As discussed in Section 4, our preferred estimator is the one proposed by Borusyak et al. (2024) (BJS). Column 1 reports our baseline results obtained by using this estimator. Columns 2, 5 and 8 report estimates obtained using the estimator proposed by Deb et al. (2024), which also accounts for treatment effect heterogeneity. Reassuringly, this estimator produces results that closely align with our baseline estimates across all outcomes. Columns 3, 6 and 9 report estimates obtained by estimating the naïve two-way fixed effects (TWFE) specification described in equation 1. As documented by Goodman-Bacon (2021), TWFE estimators often produce negatively-biased estimates in the presence of staggered treatment adoption, sometimes to the extent that estimated effects have the wrong sign. For fertility among gender-equal groups and children's outcomes, the TWFE estimates are indeed biased downwards, but remain qualitatively similar to our main results. As shown in column 9, we only find a large discrepancy

between BJS and TWFE for the effect on fertility among male-dominant ethnic groups. Nevertheless, the fact that estimates are consistent across the two difference-in-differences estimators that account for variation in treatment timing confirms that the patterns we document are not an artifact of our chosen estimation approach.[10]

Inference remains unchanged when clustering standard errors at the ethnicity-country-year level (or ethnicity-country-birth year for the children's sample) or at the ethnicity-country level, as shown in Tables A.2 and A.3, respectively. Estimates are also robust to an alternative specification where we include a set of individual, time-invariant controls such as place of residence (urban vs rural) and religion (Table A.4).

A potential concern is that some women in our sample may have been directly exposed to FPE as students. In many SSA contexts, girls aged 15–17 may still be enrolled in primary school, raising the possibility that younger cohorts of mothers were themselves treated. If FPE increased mothers' own educational attainment, the interpretation of our findings could change: fertility reductions might reflect extended schooling and delayed childbearing, whereas improvements in children's outcomes could result from enhanced economic conditions. To assess this possibility, we re-estimate our main specifications using the subsample of women who were 18 or older at the time of FPE introduction, and therefore not directly exposed to the policy during their school years. Table A.5 shows that the results are similar in magnitude to our baseline estimates, indicating that our findings are not driven by direct educational effects of FPE on young women.

Finally, we provide further evidence in support of the main identifying assumption behind our difference-in-differences design. As detailed in Section 4, one potential violation arises if there is selection into treatment, e.g., if countries systematically implement FPE following sustained economic growth and such growth independently leads to declines in fertility or improvements in child outcomes. Even though our event studies present no evidence of pre-trends in our outcomes of interest, we consider a set of time-varying country-level indicators: (log) GDP per capita, the United Nations Human Development index and Gender Development index. We then use the event-study specification to test whether countries that adopted FPE earlier were on a differential trend in any of these dimensions before the policy change, compared to control countries. Figure A.1 illustrates that this is not the case, lending further support to the validity of our identification strategy.[11]

[10] An alternative class of estimators, including those proposed by De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2021), involve the estimation and aggregation of a series of two-period, two-group difference-in-differences models. In this setting, this would involve estimating the effect of FPE relative to the last period before treatment, separately for each ethnic group and for each period after treatment. As we include high-dimensional birth-year fixed effects, many of these individual models cannot be estimated, and the results would therefore not be comparable to our main estimates.

[11] Consistent with increased government spending on education, the introduction of FPE coincides with modest improvements in GDP per capita and the Human Development Index.

5.4 The Role of Other Ethnic Characteristics

Cultural and institutional traits are often interrelated. If the MDI systematically correlates with other ethnic traits that influence fertility behavior, our heterogeneity analysis may capture the moderating role of these alternative factors, rather than that of gender norms alone. We therefore assess the extent to which ethnic groups characterized by above- versus below-median male dominance in our sample differ along other other ethnic-based characteristics that have, in the literature, been shown to be relevant for fertility decisions. Table A.6 presents the results of regressing a set of ethnic characteristic on high versus low MDI. We do not find that high- vs low-MDI groups in our sample differ significantly along this set of cultural and institutional traits.

We start by considering the old age security motive, one of the most commonly cited drivers of fertility and one that may play a particularly important role in sub-Saharan African countries, most of which lack public pension systems (Rossi and Godard, 2022). To measure old-age support at the ethnicity level, we use a Murdock EA variable indicating the presence of multigenerational families in a society, which reflects the degree of intergenerational co-residence and, by extension, the reliance on children for care in later life.[12] If high MDI groups exhibit a stronger need for family-based old-age support, the absence of a fertility response to FPE could reflect this underlying motive, rather than restrictive gender norms. To assess whether our heterogeneity analysis by MDI captures variation in the old-age security motive, we examine whether high-MDI ethnicities differ from low-MDI ethnicities along this dimension. As can be seen in Table A.6, this does not seem to be the case: the difference in the prevalence of multigenerational families across low vs high MDI groups is small and statistically insignificant.

We also explore whether the MDI correlates with the presence of child labor. If in high-MDI groups children are more likely to contribute to household production and income, larger families may be economically advantageous and households might be insensitive to a policy like FPE. We proxy for the presence of child labor in a society using information from the EA on whether junior age specialization existed in subsistence activities during pre-industrial times.[13] We then test whether the lack of a fertility response to FPE among high MDI ethnicities can be attributed to children's differential productive value. If anything, however, Table A.6 shows that age specialization is less prevalent among high-MDI groups, even though this difference is not statistically significant.

Next, we turn to inheritance patterns. In the context of sub-Saharan Africa, Fontenay et al. (2024) show that fertility and inheritance are linked: societies where inheritance is impartible, i.e., transmitted to a single heir, exhibit higher fertility rates compared to so-

[12]This information is reported in variable v8 in the EA, which captures the prevailing form of domestic or familial organization.

[13]This information is reported in variables v61-v65 in the EA, which indicate whether a given subsistence activity (gathering, hunting, fishing, animal husbandry, or agriculture) is largely performed by boys and/or girls before the age of puberty.

cieties where inheritance is partible, i.e., divided equally among all children. Reassuringly, Table A.6 illustrates that inheritance patterns are uncorrelated with our MDI indicator among the ethnic groups included in our sample.[14] Therefore, our heterogeneity analysis cannot be driven by differences in inheritance practices.

Finally, we consider the ancestral economic and political complexity of a society, which might shape potential returns to education and thus parents' willingness to trade child quantity for child quality. We proxy for political complexity using the EA variable that captures the levels of jurisdictional hierarchy, and for economic complexity using a variable on sedentary versus nomadic settlement patterns.[15] Table A.6 does not indicate that high-MDI ethnic groups are characterized by lower economic or political complexity compared to their low-MDI counterparts. Taken together, all these findings reinforce our interpretation that gender norms—rather than other cultural or institutional traits—moderate the fertility response to FPE.

We also consider the role of polygyny, a key distinguishing feature of African marriage markets that may shape fertility decisions and economic outcomes (Tertilt, 2005; Rossi, 2018). Ancestral polygyny is one of the components of the MDI (see Section 3.3 for the rationale behind its inclusion), but there is no variation in ancestral polygyny in our sample: this practice is traditionally present among all the ethnic groups in our sample.[16] This implies that ancestral polygyny cannot be driving the observed heterogeneity by MDI.

We acknowledge, however, that a woman's polygynous status is an outcome that may respond to the introduction of FPE, either through men's decision to take additional wives or through women's marriage choices following a reduction in the cost of education. In the next section, we extend our analysis to examine how FPE affects a broader set of outcomes related to women's status within and beyond the household, including polygyny itself.

6 Effect of FPE on Additional Outcomes

In this section, we assess whether the fertility declines induced by FPE come with corresponding gains in women's autonomy and decision-making capacity. To this end, we leverage rich information provided by the DHS on contraceptive use, women's decision making power, fertility preferences, participation in the labor market, and other measures of female empowerment. Because these outcomes are only measured at the time of the survey—and countries are not surveyed consistently over time, as shown in Table C.3—the

[14] As in Fontenay et al. (2024), we leverage variable v75 in the EA to measure partible vs impartible inheritance.

[15] These are v33 and v30, respectively.

[16] Note that we take the MDI directly from Guarnieri and Tur-Prats's (2023) dataset; polygyny happens to not exhibit variation in our sample even though it contributes to the cross-ethnic variation in the original global dataset.

empirical design employed thus far, which relies on a panel dataset structure, is no longer appropriate. To circumvent this issue, we leverage a similar source of within-ethnicity, cross-country variation in exposure to FPE as described in detail in the next subsection.

6.1 Empirical Strategy

To examine these outcomes, a difference-in-differences approach is not feasible because, for most ethnic groups in our sample, we lack surveys from both treatment and control countries covering periods before and after the introduction of FPE. Instead, to explore the effect of FPE on outcomes that are measured cross-sectionally, we estimate the following model:

$$y_{icet} = \beta \; FPE_{ect} + \delta_e + \gamma_e b_i + \kappa_e t_i + \varepsilon_{icet} \tag{2}$$

where y_{icet} is an outcome for woman i, residing in country c, of ethnic group e, and observed in year t. FPE_{ect} is a treatment indicator that takes a value of 1 if both the following conditions are met: (i) country c where ethnicity e is located introduced FPE earlier than the neighbouring country c', where other members of ethnic group e reside; and (ii) members from ethnic group e in country c are surveyed in a year t that falls after the introduction of FPE. Appendix Table C.4 provides a data extract illustrating how the FPE_{iect} indicator is constructed. δ_e is a vector of ethnicity fixed effects. $\gamma_e b_i$ and $\kappa_e t_i$ respectively capture trends for birth year and interview year, interacted with ethnicity indicators. We cluster standard errors at the DHS sampling cluster level.

This specification compares women from ethnic group e and exposed to FPE in country c—the *early adopter*—to a control group comprising women from the same ethnic group who: (i) reside in country c but were surveyed before FPE was implemented or (ii) reside in country c', the *late adopter*. Our design leverages the fact that women in the early adopter country have been exposed to the policy for a longer period than those in the late adopter country. Consequently, our treatment indicator classifies the latter group of women in country c' as part of the control group, even if their interviews occur after the implementation of FPE. The estimated treatment effect may therefore represent a lower bound of the policy's true impact on these empowerment outcomes.

Before proceeding with the analysis, we assess the validity of our treatment indicator by ensuring that it is a strong and significant predictor of the number of years a respondent has been treated. Appendix Table A.7, column 1 reports the results of estimating the model in equation 2 using the number of years since FPE implementation as outcome variable. We find that FPE_{iect} is associated with 4.16 years longer exposure to FPE on average, equivalent to a 102 percent effect compared to the sample mean. Next, we assess whether the baseline fertility results replicate using this estimation strategy. Column 2 shows that this empirical design yields estimates that are qualitatively similar to our

baseline findings.

6.2 Results

We start by examining the effect of FPE on fertility-related behaviors. Table 5 illustrates the impact on contraception use (columns 1-4) and fertility preferences (columns 5-6). As should be expected, declines in fertility as a result of FPE are paired with FPE-induced increases in average contraceptive use (Panel A). Consistent with heterogeneous fertility effects by gender norms, this average increase is entirely driven by ethnicities with gender-equal norms. Women in gender-equal societies are 15 percentage points (or 32 percent compared to the sample mean) more likely to have ever used any form of contraception and 5 percentage points (or 21 percent) more likely to be currently using family planning methods (Panel B). Modern methods of contraception—as opposed to traditional or folkloric methods—are those accounting for the observed larger contraceptive use among women in gender-equal societies. This subgroup of women also report a 9 percentage points (or 10 percent) higher likelihood of making joint or sole decisions over the use of family-planning methods. This increased agency over contraceptive use suggests that FPE may already be enhancing women's autonomy at the point when decisions over fertility are made. Conversely, none of these effects can be detected among women in male-dominant societies, whose decision-making power over contraceptive use as well as actual use, if anything, is lower in earlier-treated countries (see Panel C).

We also test whether a shift in fertility preferences underlies the observed FPE-induced fertility reduction, and whether these preferences shift for women and men alike. Columns 5 and 6 of Table 5 report the effect of FPE on women's desired number of children and the probability that husbands desire more children than their wife. Consistent with our findings on fertility and children's outcomes, we uncover opposing effects on preferences depending on the underlying gender norm. For gender-equal ethnic groups, we find a 0.235 (or 5 percent) lower number of desired children among women in countries that adopted FPE earlier. We also find a larger likelihood of a within-couple gap in desired fertility, with husbands being 3.2 percentage points (or 16 percent) more likely to desire more children than their wives. Coupled with the fertility-reducing effects of FPE in gender-equal societies, these results suggest that women's preferences play a more significant role in households' fertility decisions as a result of the policy, a finding that points toward increased female bargaining power. Conversely, in male-dominant ethnic groups, FPE is associated with a 0.373, or 7.6 percent higher number of desired children among women, and a 17 percent lower likelihood of husbands desiring more children than their wives.

Our next set of results, reported in Table 6, indicates that the increase in women's agency induced by the policy is not limited to fertility-related decisions but extends to broader domains of female empowerment, both within and beyond the household. We provide two pieces of evidence that FPE-induced fertility reductions are accompanied by

improvements in women's outside options. First, we estimate that women in gender-equal groups exposed earlier to the reform are 5.6 percentage points more likely to have ever been divorced, corresponding to a 20 percent increase relative to the control mean (column 1, Panel B). Second, we construct an index of acceptance of intimate partner violence (IPV), defined as the average response to five questions regarding whether wife-beating is justified in specific scenarios.[17] Among earlier-treated women in gender-equal groups, we find a 0.03 reduction in the IPV acceptance index, amounting to a 14 percent decline relative to the control mean (column 2, Panel B).

As can be seen in column 3 of Panel A, women are also 7 percentage points (or 9 percent) more likely to participate in the labor market in earlier-treated countries. This effect is not moderated by cultural norms, suggesting that FPE frees up women's time and allows them to replace some of their care-giving duties with economic activities, irrespective of the underlying degree of male dominance (see column 3 in Panels B and C). Yet, our baseline results suggest that only for women in more gender-equal groups does this FPE-induced shift in labor market participation go hand in hand with a larger say in fertility decisions. For this subgroup of women, we indeed find that the larger decision-making power over fertility extends to other domains such as household purchases, women's own healthcare, and visits to relatives (see columns 4, 5, and 6 of Panel B).

Table 7 shows that earlier-treated women in more gender-equal groups also report higher engagement with various forms of media, thus presenting additional evidence of FPE's impact on reshaping women's roles. Earlier exposure to FPE is associated with a 4.2, 8.3 and 13.6 percentage points higher likelihood of reading a newspaper, watching television and listening to the radio at least once per week, respectively. We also find a 9.1 percentage point (or 40 percent) higher likelihood of listening to the radio almost every day. These results may reflect women's increased leisure time or greater social engagement.

Finally, we examine women's polygynous status. In our sample, the prevalence of polygynous unions among women is 19 percent, which is lower than the 25 percent estimated for sub-Saharan Africa by Arthi and Fenske (2018). As documented in Section 5, FPE reduces fertility in gender-equal societies, a pattern consistent with a decline in the value that households place on large families. Given that polygyny can function as a means for men to increase their offspring, as noted in anthropological work (Zeitzen, 2020), FPE may consequently weaken men's incentives to enter polygynous unions. The results in Table 8 are consistent with this: FPE reduces women's likelihood of being in a polygynous marriage, the number of their co-wives, and their husband's total fertility (defined as the number of children ever born to all the husband's wives). Mirroring the fertility baseline results, these effects are entirely concentrated among gender-equal ethnic

[17] These scenarios include arguing with her husband, going out without informing him, neglecting the children, burning food, or refusing sex.

groups. In countries that adopted FPE earlier, women in these groups are 6.7 percentage points (or 37 percent) less likely to be in a polygynous union (Panel B, column 1), and their number of co-wives is reduced by half (Panel B, column 2). Correspondingly, husbands' total fertility declines by 9 percent in this subgroup (Panel B, column 3). Taken together, these findings point to an additional, more indirect channel through which FPE may enhance women's empowerment: by shifting households toward monogamy, FPE may reduce co-wife competition for resources (Rossi, 2018).

Given the cross-sectional nature of this analysis, these results may not lend themselves to a causal interpretation. Reassuringly, however, the results are robust to alternative specifications. Because the probability of being treated jumps discontinuously at the country border, the empirical design employed in this section resembles a spatial regression discontinuity design (RDD). In Appendix Tables A.8 to A.11 we show that these estimates are robust to controlling for the distance to the national border in km (i.e., the running variable in a standard RDD) interacted with ethnicity indicators. Results are shown in columns 1-3 in each table, which display the average effect as well as heterogeneity by gender norms. Alternatively, we allow the slope of the distance coefficient to differ on either side of the border. As shown in columns 4 to 6, results remain largely unchanged.

7 Conceptual Framework

Two empirical patterns call for a theoretical explanation: fertility responds to FPE only in low male-dominance groups, and these responses are accompanied by substantial gains in women's empowerment. To interpret these findings, we develop a simple model linking free education, parental fertility decisions, and intra-household roles, with gender norms shaping these interactions. The formal framework is presented in Appendix B. The model is built around the canonical quantity–quality tradeoff, motivated by Doepke (2015)'s observation that this mechanism is central for understanding fertility behavior in developing countries. As he emphasizes, in environments with limited access to high-quality public schooling and widespread child labor, the opportunity cost of education is high, making the quantity–quality margin particularly salient.

The second building block is household bargaining. We first embed the Beckerian quantity–quality mechanism (Becker, 1960) in a full-commitment framework. The model features two partners, each with separate utility functions featuring the quantity–quality tradeoff, who negotiate *ex ante* over fertility, educational investments, and consumption. As schooling costs fall, parents optimally substitute toward child quality and reduce fertility—consistent with our average empirical effects. Yet this framework cannot account for the heterogeneous fertility responses by gender norms, nor can it generate the female empowerment gains we document.

We therefore extend the model to a limited-commitment environment following Doepke

and Kindermann (2019). Partners cannot commit to future consumption or educational investments when making fertility choices, creating a two-stage structure. In the first stage, the couple chooses fertility; in the second stage, after fertility is realized, they renegotiate over consumption and investment in children. Each partner's outside option in this renegotiation depends on prior fertility choices. We interpret the outside option as non-cooperation within an ongoing relationship: both partners enjoy utility from children, but consumption and educational investments are chosen independently, subject to individual budget constraints.

Fertility choices in the first stage therefore reflect the utilities partners expect to obtain in second-stage renegotiation. Following Doepke and Kindermann (2019), we adopt a veto-power framework in which equilibrium fertility equals the preferred fertility of the partner who wants fewer children. This assumption fits contexts where both partners hold comparable influence over reproductive decisions. In more patriarchal settings, however, cultural constraints—such as spousal opposition to contraception or stigma surrounding female reproductive autonomy—limit women's ability to veto their partner's preferred fertility choice (e.g., Ashraf et al., 2014; Cassidy et al., 2021; Ashraf et al., 2022). We therefore distinguish two cultural regimes: under high male dominance, women lack veto power and fertility reflects the man's preference; under low male dominance, fertility reflects the minimum of the two partners' preferred levels.

The model generates asymmetric fertility responses to FPE across these regimes. Under limited commitment, husbands optimally choose the biologically feasible maximum fertility, even though their utility function feature the quantity–quality tradeoff. The intuition is straightforward: higher fertility ex ante shifts wives toward traditional household production ex post, weakening their outside options in subsequent intrahousehold bargaining and allowing husbands to capture a larger share of the marital surplus. Crucially, this incentive is invariant to reductions in the cost of education—the principal channel through which FPE would otherwise activate the quantity–quality tradeoff. Hence, in high–male-dominance settings—where women cannot block husbands' desired fertility—FPE leaves fertility unchanged. By contrast, when women retain veto power, fertility reflects the more education-responsive female preference and therefore declines as FPE reduces schooling costs.

Two implications follow. First, fertility responds to FPE only in low male-dominance settings, where women can block high-fertility choices—precisely the pattern observed in the data. Second, because ex ante fertility choices shape women's ex post outside options, the model explains why FPE-induced reductions in fertility are accompanied by rising female bargaining power. In sum, a simple model that combines the quantity–quality tradeoff with limited-commitment household bargaining—and embeds gender norms through women's veto power over fertility decisions—offers one explanation for the empirical patterns we document.

Our model operationalizes culture through women's veto authority over fertility. When this authority is absent, men strategically choose high fertility to shift women into household roles and thereby increase their own share of the marital surplus. This effectively neutralizes the quantity–quality tradeoff. However, gender norms may dampen the tradeoff through alternative mechanisms. For example, Spolaore and Wacziarg (2022) develop a framework in which social or moral stigma surrounding fertility control raises the perceived costs of adopting it. In their model, economic forces that would normally lead households to prefer fewer children may be insufficient to produce an actual fertility decline when traditional cultural norms increase these perceived costs. To the extent that prevailing gender norms constitute an important component of such traditional beliefs and attitudes, a model of this type—augmented to incorporate the quantity–quality tradeoff—could likewise account for many of the patterns we document.

Both of the above frameworks omit a key feature of African family systems, namely polygyny. Our evidence shows that the introduction of FPE leads to reductions in both the prevalence of polygynous unions and the number of children fathered by men. Several, not mutually exclusive, mechanisms may account for these patterns. First, in low-MDI settings, FPE triggers the quantity-quality tradeoff; that is, free education reduces the economic motivation for maintaining large families. If the demand for many children falls, men's incentives to take additional wives could correspondingly weaken, given arguments that polygyny functions in part as a means for men to increase their number of offspring (Zeitzen, 2020). Second, a related but more nuanced explanation could arise from the fertility–polygyny link highlighted by Rossi (2018), who shows that competition among co-wives may raise fertility because children enhance a wife's claim over her husband's resources. By lowering the resources required per child, FPE may reduce the marginal gains from childbearing for women and therefore weaken incentives for co-wife competition. In turn, diminished fertility-related returns from additional wives may reduce men's incentives to enter polygynous unions. These responses are likely to be most salient in low-MDI cultures, where gender roles may be less rigid and thus more responsive to shifts in the economic returns to fertility and marriage.

8 Conclusion

This paper provides new evidence on how reducing the cost of schooling affects parental fertility behavior in sub-Saharan Africa, a region where high fertility rates remain central to long-run demographic and development challenges. Motivated by the open question of whether the quantity–quality tradeoff operates in settings with limited female autonomy, we show that the fertility response to free primary education (FPE) depends critically on deep-rooted gender norms.

Exploiting within-ethnicity variation in exposure to FPE across 17 countries, we find

that FPE reduces fertility and increases child survival and school enrollment, but only in ethnic groups with low male dominance. Among these groups, FPE also raises contraceptive use, expands women's decision-making power, and strengthens their outside options—reflected in higher divorce probabilities and lower acceptance of intimate partner violence.

A simple model embedding the quantity–quality tradeoff within a limited-commitment bargaining framework helps interpret these findings: fertility declines only where women possess the cultural veto power needed to realize lower desired fertility when schooling becomes cheaper. Where gender norms restrict women's agency, the classic mechanism linking schooling costs and fertility remains inactive.

FPE was designed to promote long-run development by expanding access to education, yet our results show that its effects are more immediate and far-reaching than originally anticipated. Beyond improving children's schooling outcomes, FPE generates meaningful spillovers for mothers. Evaluating education reforms solely through their effects on children therefore overlooks a key channel through which such policies shape household behavior and welfare.

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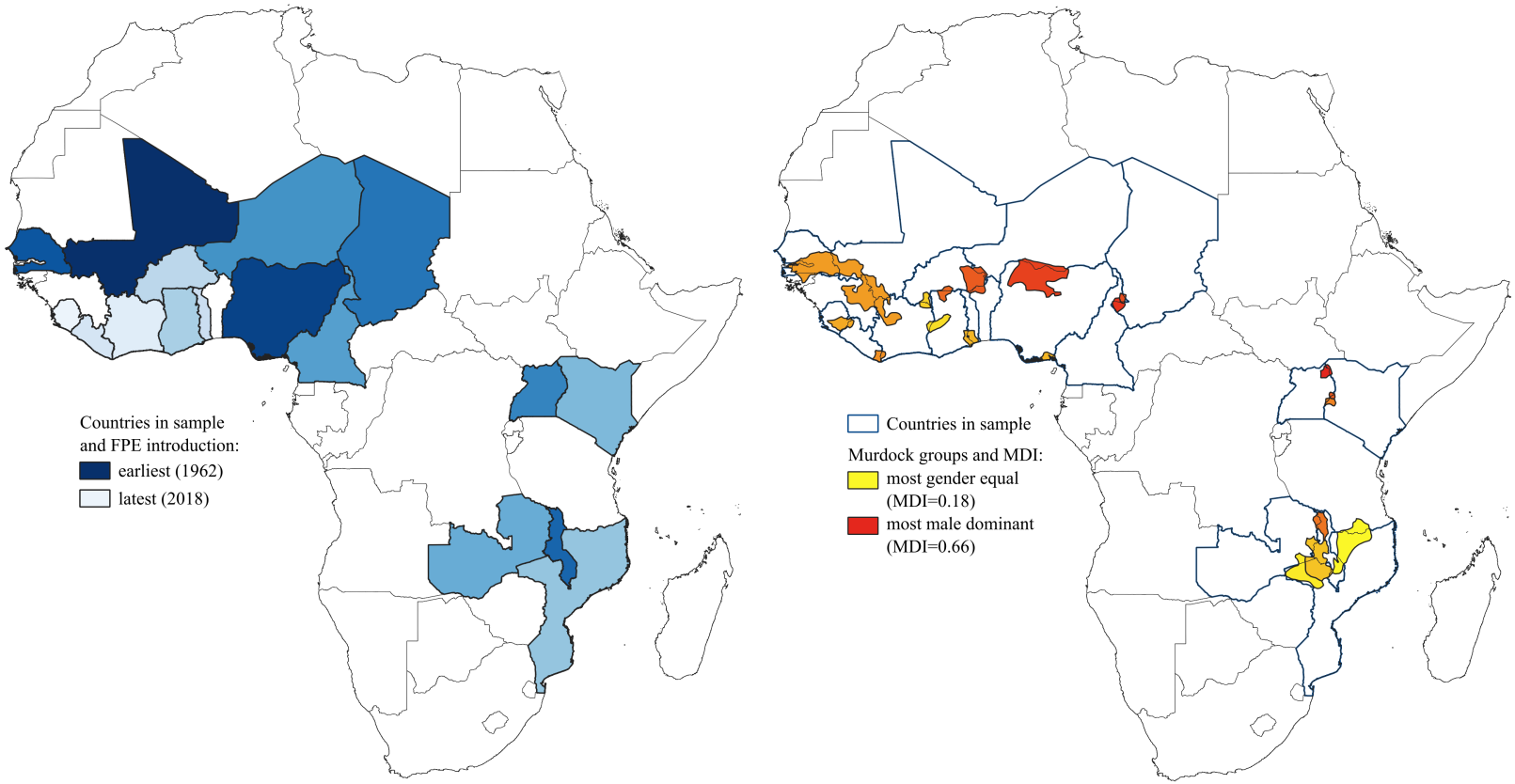
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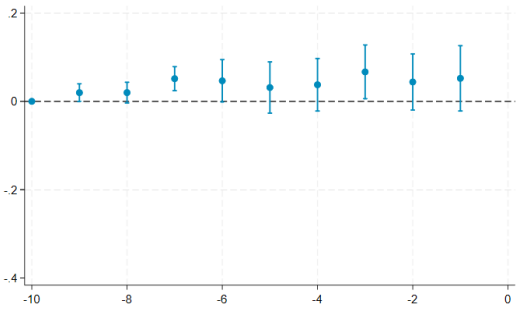
Figures and Tables

FIGURE 1: *Countries and Murdock Ethnic Groups Included in the Analysis*

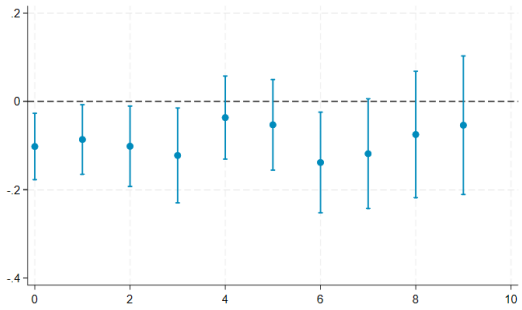


Notes: The map on the left illustrates the countries included in our estimation sample. Different shades of blue indicate the timing of FPE introduction from the earliest (dark blue) to the latest (light blue). Table C.2 reports detailed information on the year when FPE was introduced in each country. The map on the right illustrates the homelands of the Murdock ethnic groups included in the estimation sample. Different shades indicate gender norms, from the most gender-equal (yellow) to the most male-dominant (dark orange). Sources: see Appendix Tables C.1 and C.2, Murdock (1959), and Guarnieri and Tur-Prats (2023).

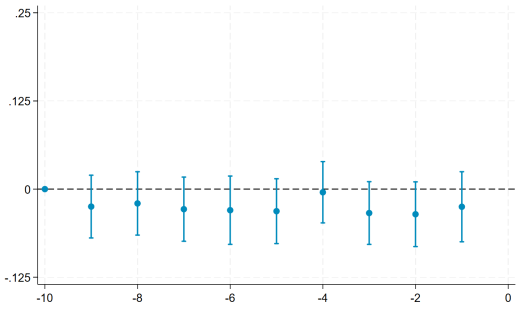
FIGURE 2: *Event Studies*



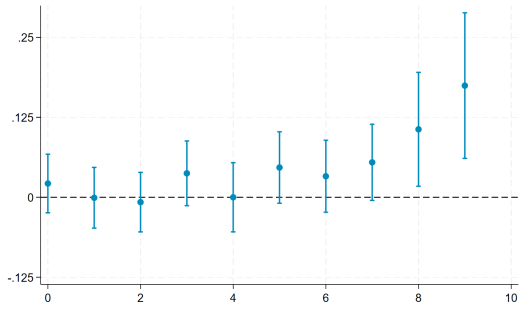
(A) *Total fertility, pre-treatment*



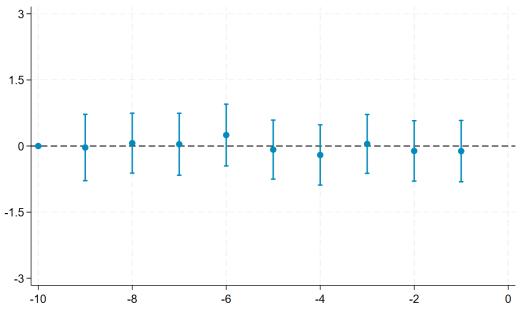
(B) *Total fertility, post-treatment*



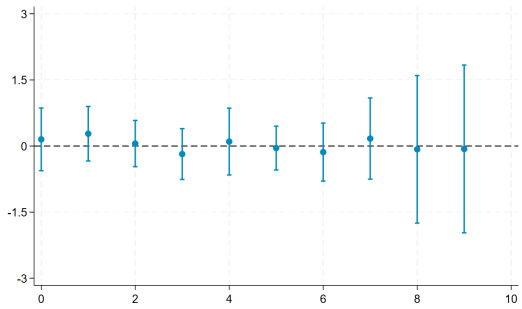
(C) *Survival to survey, pre-treatment*



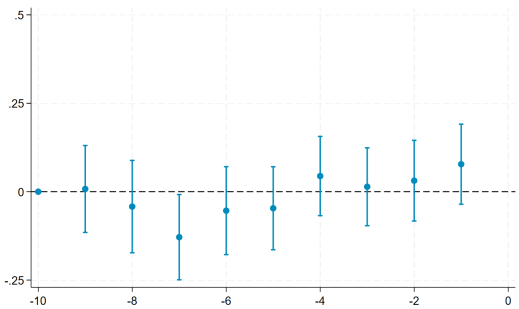
(D) *Survival to survey, post-treatment*



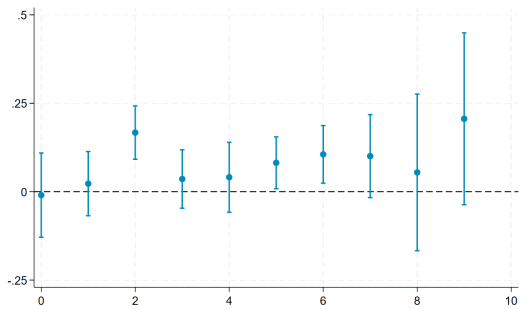
(E) *Years of schooling, pre-treatment*



(F) *Years of schooling, post-treatment*



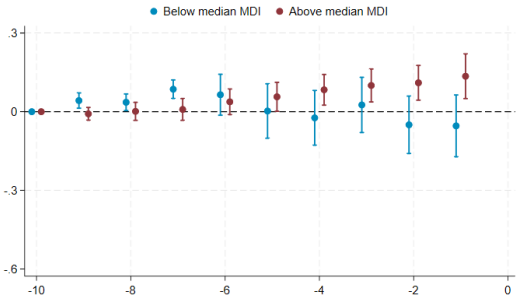
(G) *Enrolled in school at survey, pre-treatment*



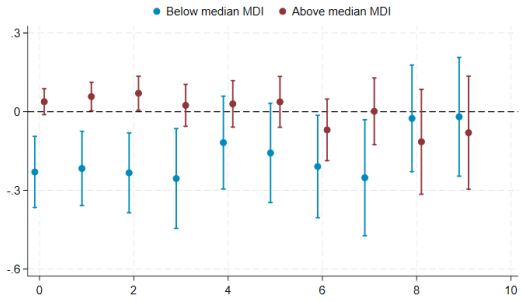
(H) *Enrolled in school at survey, post-treatment*

Notes: The figures plot coefficient estimates using Borusyak et al.'s (2024) method for ten years before (graphs on the left) and ten years after (graphs on the right) the introduction of FPE. Following Roth (2024), the estimates are presented separately for ease of interpretation: for the pre-treatment period, estimates are relative to the reference period of t=−10; for the post-treatment period, estimates are relative to the reference period of $t \in [-10, -1]$, inclusive. In Panels A-B, the outcome variable is the total number of children. In Panels C-D, the outcome variable is a binary indicator capturing whether a child has survived until the time of the survey; in Panels E-F, the outcome variable is the number of years of schooling; in Panel G-H, the outcome variable is an indicator capturing whether a child is enrolled in school at the time of the survey. Vertical bars denote 95 percent confidence intervals. Standard errors are clustered at the DHS sampling cluster level.

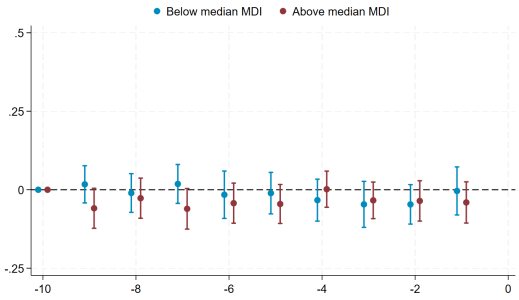
FIGURE 3: *Event Studies by MDI*



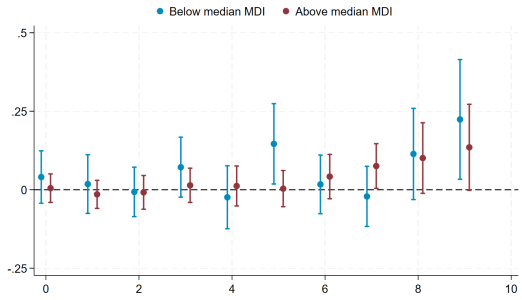
(A) *Total fertility, pre-treatment*



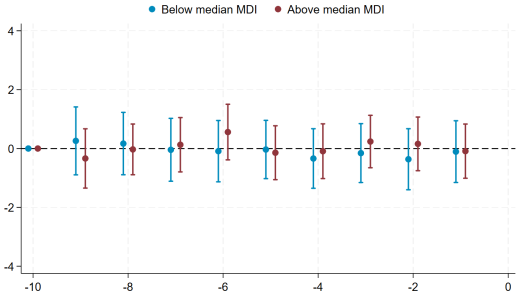
(B) *Total fertility, post-treatment*



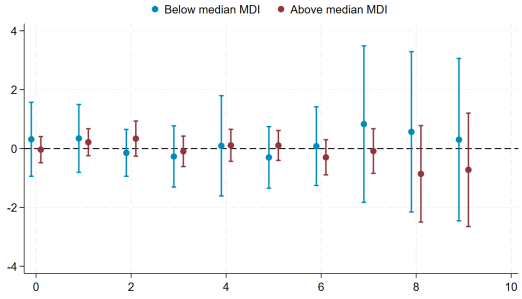
(C) *Survival to survey, pre-treatment*



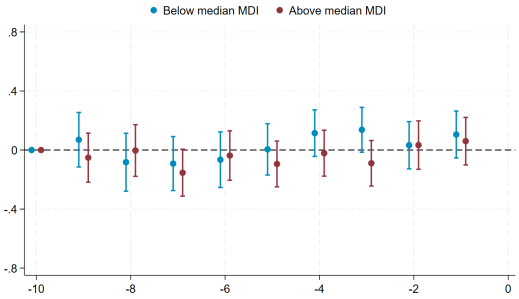
(D) *Survival to survey, post-treatment*



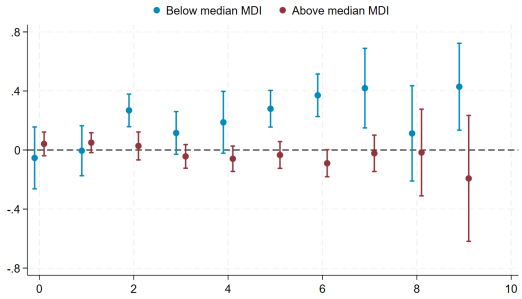
(E) *Years of schooling, pre-treatment*



(F) *Years of schooling, post-treatment*



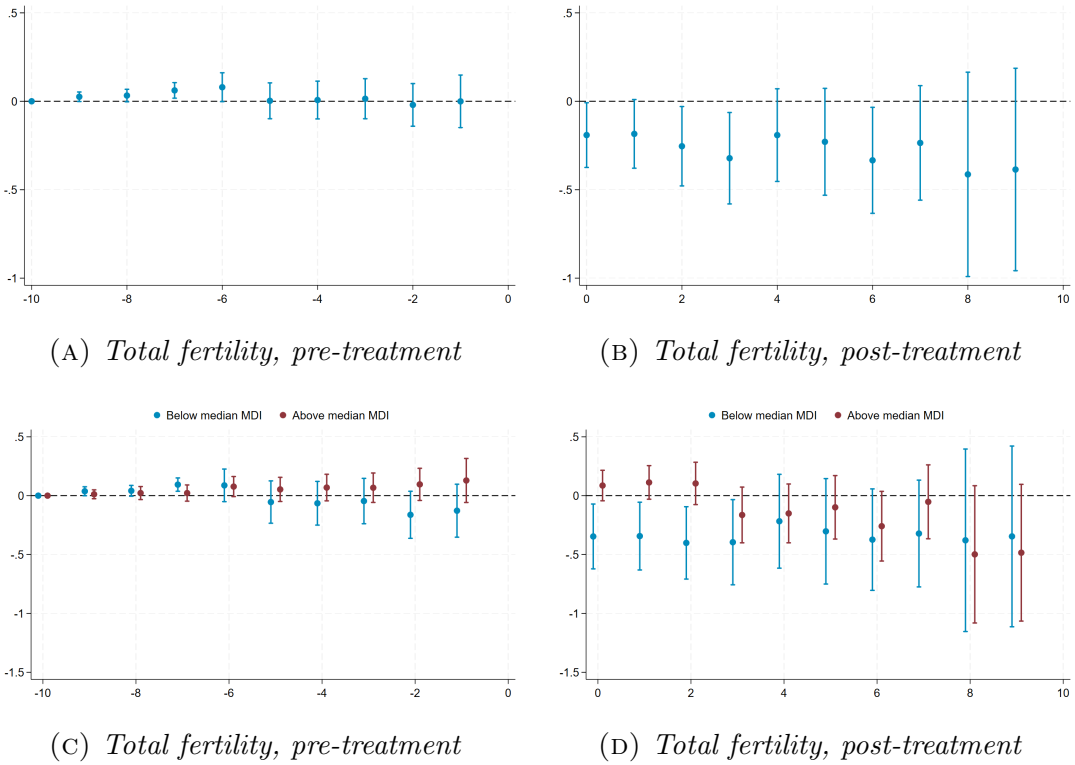
(G) *Enrolled in school at survey, pre-treatment*



(H) *Enrolled in school at survey, post-treatment*

Notes: The figures plot coefficient estimates using Borusyak et al.'s (2024) method for ten years before (graphs on the left) and ten years after (graphs on the right) the introduction of FPE, separately for women and children in gender-equal (in blue) and male-dominant (in red) ethnic groups, respectively. Following Roth (2024), the estimates are presented separately for ease of interpretation: for the pre-treatment period, estimates are relative to the reference period of t=−10; for the post-treatment period, estimates are relative to the reference period of t ∈ [−10, −1], inclusive. In Panels A-B, the outcome variable is the total number of children. In Panels C-D, the outcome variable is a binary indicator capturing whether a child has survived until the time of the survey; in Panels E-F, the outcome variable is the number of years of schooling; in Panel G-H, the outcome variable is an indicator capturing whether a child is enrolled in school at the time of the survey. Vertical bars denote 95 percent confidence intervals. Standard errors are clustered at the DHS sampling cluster level.

FIGURE 4: *Event Studies: Completed Fertility*



(A) *Total fertility, pre-treatment*

(B) *Total fertility, post-treatment*

(C) *Total fertility, pre-treatment*

(D) *Total fertility, post-treatment*

Notes: The figures plot coefficient estimates using Borusyak et al.'s (2024) method for ten years before (graphs on the left) and ten years after (graphs on the right) the introduction of FPE. In Panels A-B, results are presented for all women in our sample observed at age 35 or older. In Panels C-D, results are presented separately for women observed at age 35 or older in gender-equal (in blue) and male-dominant (in red) ethnic groups, respectively. Following Roth (2024), the estimates are presented separately for ease of interpretation: for the pre-treatment period, estimates are relative to the reference period of t=−10; for the post-treatment period, estimates are relative to the reference period of t $\in [-10, -1]$, inclusive. In all Panels, the outcome variable is the total number of children. Vertical bars denote 95 percent confidence intervals. Standard errors are clustered at the DHS sampling cluster level.

TABLE 1: *Descriptive Statistics*

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.
Panel A: Women in our sample				
Survey year	2005.80	8.35	1986.00	2019.00
Birth year	1973.06	10.71	1937.00	1996.00
Age	23.93	7.41	15.00	49.00
Any fertility	0.65	0.48	0.00	1.00
Total fertility	2.16	2.41	0.00	16.00
Male Dominance Index	0.51	0.09	0.18	0.66
Observations	67,230			
Panel B: Panel dataset				
Survey year	2007.44	7.66	1986.00	2019.00
Year	1997.33	8.75	1953.00	2013.00
Age	25.23	7.35	15.00	49.00
Treated	0.22	0.41	0.00	1.00
Any fertility	0.73	0.45	0.00	1.00
Total fertility	2.49	2.46	0.00	16.00
Male Dominance Index	0.50	0.11	0.18	0.66
Observations	663,439			
Panel C: Children's sample				
Survey year	2007.53	7.81	1986.00	2019.00
Birth year	1996.58	9.21	1952.00	2017.00
(Potential) Age	10.56	6.90	0.00	37.00
Treated	0.18	0.39	0.00	1.00
Survival to survey	0.83	0.37	0.00	1.00
Years of education	2.36	3.09	0.00	20.00
Enrolled in school at survey	0.56	0.50	0.00	1.00
Male Dominance Index	0.52	0.11	0.18	0.66
Observations	142,548[†]			

[†]Sample sizes for two variables, years of education and whether a child is enrolled at the time of the survey, are smaller as the earliest DHS surveys did not collect information on children's educational attainment. The sample sizes for these variables are 69,026 children.

TABLE 2: *Effect of Free Primary Education*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Free primary education	–0.092**	0.028**	0.045	0.068**
	(0.045)	(0.014)	(0.315)	(0.030)
Mean of dep. var.	2.49	.831	2.36	.562
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes
Observations	663,439	142,548	69,026	69,026
Number of clusters	11,482	10,076	6,270	6,270

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman/child resides in a country that has introduced free primary education by a specific year, and 0 otherwise. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 3: *Free Primary Education and Fertility: Heterogeneity by Gender Norms*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Panel A: Ethnicities with gender-equal cultural norms				
Free primary education	–0.199**	0.041*	0.097	0.162***
	(0.079)	(0.024)	(0.622)	(0.056)
Mean of dep. var.	2.42	.853	1.64	.523
Observations	338,976	61,852	38,226	38,226
Number of clusters	4,377	3,935	3,284	3,284
Panel B: Ethnicities with male-dominant cultural norms				
Free primary education	0.016	0.019	–0.002	–0.018
	(0.040)	(0.017)	(0.207)	(0.026)
Mean of dep. var.	2.56	.815	3.25	.611
Observations	324,463	80,696	30,800	30,800
Number of clusters	7,717	6,538	3,292	3,292
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. The sample in Panel A includes women and children from ethnic groups with a below-median MDI, while the sample in Panel B includes women and children from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4: *Free Primary Education and Completed Fertility*

	Dependent variable: Total children		
		Ethnicities with:	
	All	gender-equal cultural norms	male-dominant cultural norms
	(1)	(2)	(3)
Free primary education	–0.248**	–0.346**	–0.056
	(0.121)	(0.175)	(0.099)
Mean of dep. var.	4.42	4.13	4.84
Observations	206,330	123,470	82,860
Number of clusters	5,595	2,943	2,771
Ethnicity-by-year fixed effects	yes	yes	yes
Ethnicity-by-country fixed effects	yes	yes	yes
Ethnicity-by-birth cohort fixed effects	yes	yes	yes

Notes: The unit of observation is a woman-year. The dependent variable captures the number of children a woman has had by a given year. To capture completed fertility, the sample is restricted to women observed at age 35 or over. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. The sample in Column 2 includes women from ethnic groups with a below-median MDI, while the sample in Column 3 includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 5: *Free Primary Education, Use of Contraception, and Fertility Preferences*

	Dependent variable:					
	Contraception use:					
	ever	currently	modern method	joint decision	Number of desired children	Husband wants more children
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average effects						
Free primary education	0.099***	0.022**	0.015*	0.049***	–0.054	0.008
	(0.013)	(0.010)	(0.009)	(0.015)	(0.062)	(0.015)
Mean of dep. var.	.5	.269	.228	.885	4.75	.219
Observations	42,048	42,048	42,048	8,049	39,740	29,283
Adjusted R-squared	.111	.0543	.06	.012	.198	.0376
Panel B: Ethnicities with gender-equal cultural norms						
Free primary education	0.149***	0.050***	0.035***	0.087***	–0.235***	0.032*
	(0.016)	(0.012)	(0.010)	(0.019)	(0.078)	(0.019)
Mean of dep. var.	.46	.239	.198	.878	4.7	.205
Observations	30,474	30,474	30,474	5,036	29,481	21,677
Adjusted R-squared	.0562	.016	.0265	.018	.195	.0424
Panel C: Ethnicities with male-dominant cultural norms						
Free primary education	–0.013	–0.038**	–0.031*	0.005	0.373***	–0.044*
	(0.021)	(0.017)	(0.016)	(0.023)	(0.099)	(0.024)
Mean of dep. var.	.608	.349	.307	.896	4.91	.259
Observations	11,574	11,574	11,574	3,013	10,259	7,606
Adjusted R-squared	.209	.0996	.0891	.00253	.204	.0165

Notes: The unit of observation is a woman. The dependent variable in column 1 is a binary indicator that takes value 1 if a woman has ever used contraceptives and 0 otherwise; in column 2, the dependent variable is a binary indicator that takes value 1 if a woman is currently using contraceptives and 0 otherwise; in column 3, the dependent variable is a binary indicator that takes value 1 if a woman is currently using a modern method of contraception and 0 otherwise; in column 4, the dependent variable is a binary indicator that takes value 1 if decisions over contraception are taken jointly by the couple or mainly by the woman, and 0 otherwise; in column 5, the dependent variable captures the number of desired children; in column 6, the dependent variable is a binary indicator that equals 1 if the husband wants more children than the wife, and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a respondent lives in a country that first introduced free primary education and is interviewed after the introduction of FPE. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. Panel B includes women from ethnic groups with a below-median MDI, while the sample in Panel C includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6: *Free Primary Education and Female Empowerment*

	Dependent variable:						
				Participation in decisions over:			
	Ever divorced (1)	IPV acceptance (2)	Currently working (3)	large purchases (4)	own healthcare (5)	visits to relatives (6)	respondent's earnings (7)
Panel A: Average effects							
Free primary education	0.052***	–0.029***	0.068***	0.072***	0.131***	0.136***	–0.016
	(0.009)	(0.009)	(0.011)	(0.016)	(0.015)	(0.015)	(0.010)
Mean of dep. var.	.265	.23	.731	.505	.537	.642	.871
Observations	41,957	39,008	41,105	32,051	32,051	32,051	15,987
Adjusted R-squared	.0924	.151	.129	.124	.115	.104	.0772
Panel B: Ethnicities with gender-equal cultural norms							
Free primary education	0.056***	–0.031***	0.054***	0.108***	0.168***	0.149***	0.005
	(0.011)	(0.010)	(0.012)	(0.019)	(0.016)	(0.016)	(0.011)
Mean of dep. var.	.279	.216	.739	.518	.531	.645	.881
Observations	30,410	28,543	30,404	23,134	23,134	23,134	11,996
Adjusted R-squared	.107	.148	.149	.107	.109	.0961	.0826
Panel C: Ethnicities with male-dominant cultural norms							
Free primary education	0.043***	–0.023	0.102***	–0.004	0.049	0.110***	–0.087***
	(0.015)	(0.018)	(0.023)	(0.026)	(0.030)	(0.032)	(0.023)
Mean of dep. var.	.226	.27	.707	.474	.552	.635	.841
Observations	11,547	10,465	10,701	8,917	8,917	8,917	3,991
Adjusted R-squared	.0387	.143	.0734	.165	.131	.123	.0588

Notes: The unit of observation is a woman. The dependent variable in columns 1-3 are a binary indicator that equals 1 if the woman if a woman has ever been divorced and 0 otherwise, an IPV acceptance index index calculated as the average of answers to questions on whether IPV is justified in five scenarios, and a binary indicator that equals 1 if the woman is currently working; in columns 4-7, the dependent variable is a binary indicator that equals 1 if the woman participates in decisions over large purchases, own healthcare, visits to relatives or her earnings, respectively, and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a respondent lives in a country that first introduced free primary education and is interviewed after the introduction of FPE. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. The sample in Panel B includes women from ethnic groups with a below-median MDI, while the sample in Panel C includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7: *Free Primary Education and Media Engagement*

	Dependent variable:			
	Reads newspaper weekly	Watches television weekly	Listens to radio weekly	Listens to radio daily
	(1)	(2)	(3)	(4)
Panel A: Average effects				
Free primary education	0.006	0.038**	0.086***	0.073***
	(0.008)	(0.015)	(0.012)	(0.012)
Mean of dep. var.	.093	.249	.521	.243
Observations	41,965	41,992	40,523	42,006
Adjusted R-squared	.0458	.179	.138	.28
Panel B: Ethnicities with gender-equal cultural norms				
Free primary education	0.042***	0.083***	0.136***	0.091***
	(0.010)	(0.020)	(0.015)	(0.017)
Mean of dep. var.	.0777	.269	.499	.229
Observations	30,407	30,434	29,131	30,442
Adjusted R-squared	.0406	.218	.141	.27
Panel C: Ethnicities with male-dominant cultural norms				
Free primary education	–0.073***	–0.061***	–0.024	0.033**
	(0.013)	(0.022)	(0.021)	(0.016)
Mean of dep. var.	.133	.198	.577	.281
Observations	11,558	11,558	11,392	11,564
Adjusted R-squared	.0418	.0426	.121	.297

Notes: The unit of observation is a woman. The dependent variables in columns 1-4 are binary indicators that equal 1 if the woman reports that she reads a newspaper at least once per week, watches television at least once per week, listens to the radio at least once per week, and listens to the radio almost every day, respectively. *Free primary education* is a binary variable that equals 1 if a respondent lives in a country that first introduced free primary education and is interviewed after the introduction of FPE. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. The sample in Panel B includes women from ethnic groups with a below-median MDI, while the sample in Panel C includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8: *Free Primary Education and Polygyny*

	Dependent variable:		
	In polygynous marriage	Other wives	Husband's total children
	(1)	(2)	(3)
Panel A: Average effects			
Free primary education	–0.038***	–0.127***	–0.311**
	(0.011)	(0.024)	(0.150)
Mean of dep. var.	.193	.351	5.46
Observations	41,073	30,183	10,121
Adjusted R-squared	.0694	.0719	.277
Panel B: Ethnicities with gender-equal cultural norms			
Free primary education	–0.067***	–0.166***	–0.458***
	(0.011)	(0.026)	(0.166)
Mean of dep. var.	.183	.325	5.2
Observations	30,267	21,932	7,639
Adjusted R-squared	.0762	.0674	.281
Panel C: Ethnicities with male-dominant cultural norms			
Free primary education	0.027	–0.046	–0.014
	(0.023)	(0.050)	(0.308)
Mean of dep. var.	.222	.42	6.28
Observations	10,806	8,251	2,482
Adjusted R-squared	.0496	.0709	.229

Notes: The unit of observation is a woman. The dependent variable in column 1 is a binary indicator that equals 1 if the woman reports that she is in a polygynous marriage/union; the dependent variable in column 2 captures her number of other wives; the dependent variable in column 3 captures the total number of children of her husband. *Free primary education* is a binary variable that equals 1 if a respondent lives in a country that first introduced free primary education and is interviewed after the introduction of FPE. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. The sample in Panel B includes women from ethnic groups with a below-median MDI, while the sample in Panel C includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

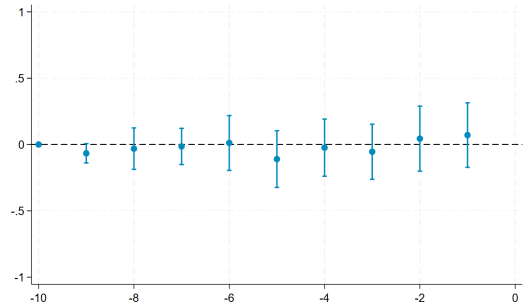
Appendix for Online Publication

"Gender Norms and the Quantity–Quality Tradeoff: Evidence from Free Primary Education in Africa"

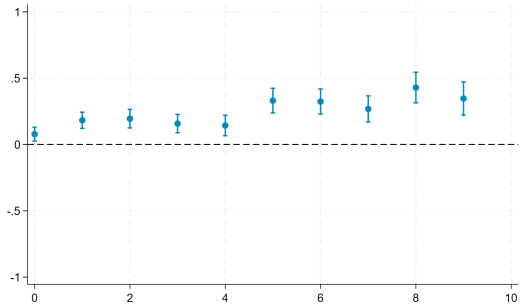
by Matthew Collins, Eleonora Guarnieri, and Helmut Rainer

A Additional Figures and Tables

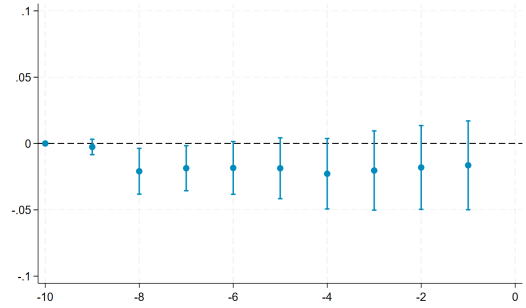
FIGURE A.1: *Trends in Time-varying Country-level Indicators*



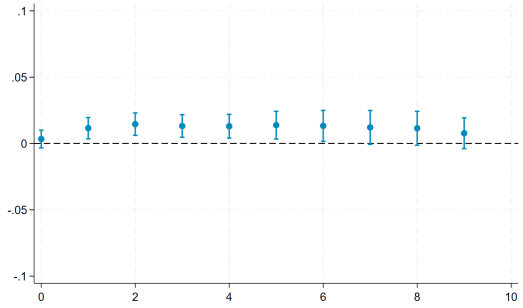
(A) *ln(GDP per capita), pre-treatment*



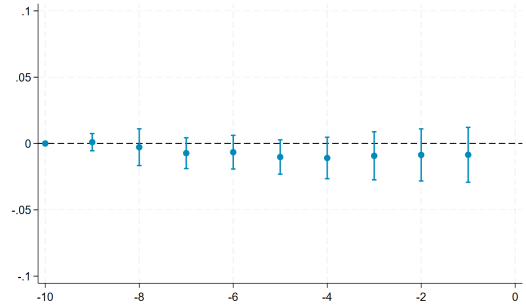
(B) *ln(GDP per capita), post-treatment*



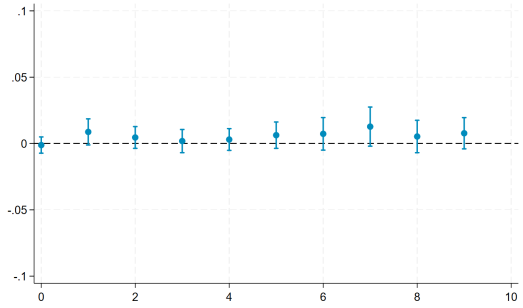
(C) *Human Development Index, pre-treatment*



(D) *Human Development Index, post-treatment*



(E) *Gender Development Index, pre-treatment*



(F) *Gender Development Index, post-treatment*

Notes: This figure presents event study graphs for a set of time-varying country-level indicators—ln(GDP per capita), the UN Human Development Index and the UN Gender Development Index.

TABLE A.1: *Robustness to Alternative Difference-in-difference Estimators*

Sample:	Full sample			Gender-equal			Male-dominant		
Estimator:	BJS	DNWZ	TWFE	BJS	DNWZ	TWFE	BJS	DNWZ	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total children	–0.092**	–0.111**	0.052	–0.199**	–0.234***	–0.182***	0.016	0.013	0.185***
	(0.045)	(0.045)	(0.032)	(0.079)	(0.079)	(0.054)	(0.040)	(0.040)	(0.037)
Survival to survey	0.028**	0.021	0.012	0.041*	0.039	0.004	0.019	0.010	0.015
	(0.014)	(0.014)	(0.009)	(0.024)	(0.024)	(0.013)	(0.017)	(0.016)	(0.012)
Years of education	0.045	0.129	0.005	0.097	0.260	–0.061	–0.002	0.044	0.049
	(0.315)	(0.319)	(0.104)	(0.622)	(0.696)	(0.130)	(0.207)	(0.273)	(0.152)
Enrolled at survey	0.068**	0.065**	0.024	0.162***	0.181***	0.043*	–0.018	–0.011	0.011
	(0.030)	(0.029)	(0.017)	(0.056)	(0.060)	(0.025)	(0.026)	(0.029)	(0.023)

Notes: The unit of observation is a woman-year (first row) or a child (second, third, and fourth row). In row 1, the outcome variable is the total number of children. In row 2, the outcome variable is a binary indicator capturing whether a child has survived until the time of the survey; in row 3, the outcome variable is the number of years of schooling; in row 4, the outcome variable is an indicator capturing whether a child is enrolled in school at the time of the survey. Column headers denote the estimator used. "BJS" refers to the estimator proposed by Borusyak et al. (2024). "DNWZ" refers to the estimator proposed by Deb et al. (2024). "TWFE" refers to two-way fixed effects. Columns 1-3 employ the full sample of ethnicities, columns 4-6 only those with below-median MDI, and columns 7-9 those with above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.2: *Robustness to Clustering at the Ethnicity-country-(birth)year Level*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Panel A: Average effects				
Free primary education	–0.092***	0.028***	0.045	0.068**
	(0.015)	(0.010)	(0.165)	(0.032)
Mean of dep. var.	2.49	.831	2.36	.562
Observations	663,439	142,548	69,026	69,026
Number of clusters	588	554	502	502
	(1)	(2)	(3)	(4)
Panel B: Ethnicities with gender-equal cultural norms				
Free primary education	–0.199***	0.041**	0.097	0.162***
	(0.026)	(0.017)	(0.293)	(0.059)
Mean of dep. var.	2.42	.853	1.64	.523
Observations	338,976	61,852	38,226	38,226
Number of clusters	228	208	206	206
	(1)	(2)	(3)	(4)
Panel C: Ethnicities with male-dominant cultural norms				
Free primary education	0.016	0.019**	–0.002	–0.018
	(0.010)	(0.009)	(0.112)	(0.019)
Mean of dep. var.	2.56	.815	3.25	.611
Observations	324,463	80,696	30,800	30,800
Number of clusters	360	346	296	296
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. The sample in Panel B includes women and children from ethnic groups with a below-median MDI, while the sample in Panel C includes women and children from ethnic groups with an above-median MDI. Standard errors clustered at the ethnicity-country-year level for the women's sample and ethnicity-country-birth year level for the children's sample are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.3: *Robustness to Clustering at the Ethnicity-country Level*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Panel A: Average effects				
Free primary education	–0.092***	0.028***	0.045	0.068***
	(0.019)	(0.006)	(0.147)	(0.023)
Mean of dep. var.	2.49	.831	2.36	.562
Observations	663,439	142,548	69,026	69,026
Number of clusters	40	37	35	35
Panel B: Ethnicities with gender-equal cultural norms				
Free primary education	–0.199***	0.041***	0.097***	0.162***
	(0.027)	(0.005)	(0.037)	(0.012)
Mean of dep. var.	2.42	.853	1.64	.523
Observations	338,976	61,852	38,226	38,226
Number of clusters	16	14	14	14
Panel C: Ethnicities with male-dominant cultural norms				
Free primary education	0.016	0.019***	–0.002	–0.018***
	(0.013)	(0.003)	(0.090)	(0.007)
Mean of dep. var.	2.56	.815	3.25	.611
Observations	324,463	80,696	30,800	30,800
Number of clusters	24	23	21	21
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. The sample in Panel B includes women and children from ethnic groups with a below-median MDI, while the sample in Panel C includes women and children from ethnic groups with an above-median MDI. Standard errors clustered at the ethnicity-country level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.4: *Robustness to Individual-level Controls*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Panel A: Average effects				
Free primary education	–0.050	0.025*	–0.045	0.058**
	(0.042)	(0.014)	(0.291)	(0.029)
Mean of dep. var.	2.49	.831	2.36	.562
Observations	663,439	142,548	69,026	69,026
Number of clusters	11,482	10,076	6,270	6,270
Panel B: Ethnicities with gender-equal cultural norms				
Free primary education	–0.131*	0.038	–0.000	0.149***
	(0.072)	(0.025)	(0.579)	(0.053)
Mean of dep. var.	2.42	.853	1.64	.523
Observations	338,976	61,852	38,226	38,226
Number of clusters	4,377	3,935	3,284	3,284
Panel C: Ethnicities with male-dominant cultural norms				
Free primary education	0.019	0.017	–0.087	–0.026
	(0.040)	(0.017)	(0.194)	(0.025)
Mean of dep. var.	2.56	.815	3.25	.611
Observations	324,463	80,696	30,800	30,800
Number of clusters	7,717	6,538	3,292	3,292
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. Control variables include a set of individual, time-invariant controls, specifically place of residence (urban vs rural) and indicators for religion (Christianity, Islam, Traditional religion). The sample in Panel B includes women and children from ethnic groups with a below-median MDI, while the sample in Panel C includes women and children from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.5: *Robustness to Excluding Women Who Were Under 18 at the Introduction of FPE*

Sample:	Women	Children		
Dependent variable:	Total children	Survival to survey	Years of education	Enrolled at survey
	(1)	(2)	(3)	(4)
Panel A: Full sample				
Free primary education	–0.077	0.027*	–0.139	0.075***
	(0.057)	(0.014)	(0.305)	(0.027)
Mean of dep. var.	2.66	.837	2.36	.563
Observations	596,936	130,349	63,742	63,742
Number of clusters	9,818	9,189	5,893	5,893
Panel B: Ethnicities with more gender-equal cultures				
Free primary education	–0.177*	0.046*	–0.240	0.164***
	(0.096)	(0.025)	(0.583)	(0.048)
Mean of dep. var.	2.53	.853	1.66	.523
Observations	317,224	58,685	36,174	36,174
Number of clusters	4,180	3,808	3,171	3,171
Panel C: Ethnicities with more male-dominant cultures				
Free primary education	0.044	0.014	–0.041	–0.011
	(0.048)	(0.017)	(0.208)	(0.026)
Mean of dep. var.	2.81	.823	3.29	.615
Observations	279,712	71,664	27,568	27,568
Number of clusters	6,157	5,716	2,980	2,980
Ethnicity-by-country fixed effects	yes	yes	yes	yes
Ethnicity-by-year fixed effects	yes	no	no	no
Ethnicity-by-birth year fixed effects	yes	yes	yes	yes
Ethnicity-by-age fixed effects	no	yes	yes	yes

Notes: The unit of observation is a woman-year in column 1, and a child in columns 2-4. In column 1, the dependent variable captures the number of children a woman has had by a given year. The dependent variable in column 2 is a binary indicator that takes value 1 if a child survived to the survey and 0 otherwise. In column 3, the dependent variable captures the years of education of a child at the time of the survey. In column 4, the dependent variable is a binary indicator that takes value 1 if a child is enrolled in school at the time of the survey and 0 otherwise. *Free primary education* is a binary variable that equals 1 if a woman resides in a country that has introduced free primary education by a specific year, and 0 otherwise. The sample in Panel B includes women and children from ethnic groups with a below-median MDI, while the sample in Panel C includes women and children from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.6: *Male Dominance and Other Ethnic Characteristics*

Variable	Sample mean (1)	Above-median MDI (2)
Multigenerational families	0.600	–0.042
		(.235)
Economic level	6.300	–0.750
		(.564)
Impartible inheritance	0.667	0.000
		(.267)
Age specialisation	0.050	–0.125
		(.101)
Societal complexity	2.400	0.042
		(.353)

Notes: The unit of observation is an ethnic group. Column 1 presents sample means. The explanatory variable in column 2 is a binary indicator that takes value 1 if an ethnic group is categorised as having an above-median MDI value. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.7: *Effect of FPE on Additional Outcomes: Validation*

	Dependent variable:		
	Years treated (1)	Total fertility (2)	Total fertility (Age $\geq$ 35) (3)
Panel A: Average effects			
Free primary education	4.159***	–0.002	0.108
	(0.117)	(0.058)	(0.088)
Mean of dep. var.	4.09	3.65	5.34
Observations	42,048	42,048	18,821
Adjusted R-squared	.988	.484	.127
Panel B: Ethnicities with gender-equal cultural norms			
Free primary education	3.482***	–0.159**	–0.199*
	(0.163)	(0.067)	(0.102)
Mean of dep. var.	2.25	3.34	5.09
Observations	30,474	30,474	12,371
Adjusted R-squared	.986	.478	.124
Panel C: Ethnicities with male-dominant cultural norms			
Free primary education	5.642***	0.341***	0.502***
	(0.135)	(0.114)	(0.151)
Mean of dep. var.	8.94	4.47	5.82
Observations	11,574	11,574	6,450
Adjusted R-squared	.991	.436	.0913

Notes: The unit of observation is a woman. The dependent variable in column 1 captures the number of years between the introduction of free primary education and observation; in column 2, the dependent variable is the number of children the woman has given birth to. *Free primary education* is a binary variable that equals 1 if a respondent lives in a country that first introduced free primary education and is interviewed after the introduction of FPE. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. The sample in Panel B includes women from ethnic groups with a below-median MDI, while the sample in Panel C includes women from ethnic groups with an above-median MDI. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.8: *Free Primary Education, Fertility Preferences, and Use of Contraception: Robustness Checks*

	Distance to border			Varying slopes		
	Average effect (1)	Gender equal (2)	Male dominant (3)	Average effect (4)	Gender equal (5)	Male dominant (6)
Ever used contraception	0.108***	0.167***	0.004	0.114***	0.179***	0.011
	(0.015)	(0.019)	(0.021)	(0.015)	(0.019)	(0.021)
Using contraception	0.036***	0.075***	–0.034**	0.039***	0.081***	–0.030*
	(0.011)	(0.014)	(0.017)	(0.011)	(0.014)	(0.017)
Using modern method of contraception	0.034***	0.064***	–0.019	0.037***	0.070***	–0.016
	(0.010)	(0.013)	(0.017)	(0.010)	(0.013)	(0.017)
Joint decision over contraception	0.042***	0.088***	–0.002	0.041**	0.083***	–0.000
	(0.016)	(0.023)	(0.022)	(0.016)	(0.023)	(0.023)
Number of desired children	–0.128*	–0.312***	0.223**	–0.154**	–0.382***	0.243**
	(0.075)	(0.101)	(0.100)	(0.072)	(0.098)	(0.098)
Husband wants more children	0.003	0.032	–0.049**	0.005	0.036	–0.046*
	(0.017)	(0.022)	(0.025)	(0.017)	(0.022)	(0.024)

Notes: The unit of observation is a woman. The dependent variable in row 1 captures the number of desired children; in row 2, the dependent variable is a binary indicator that equals 1 if the husband wants more children than the wife, and 0 otherwise; in row 3, the dependent variable is a binary indicator that takes value 1 if a woman has ever used contraceptives and 0 otherwise; in row 4, the dependent variable is a binary indicator that takes value 1 if a woman is currently using contraceptives and 0 otherwise; in row 5, the dependent variable is a binary indicator that takes value 1 if a woman is currently using a modern method and 0 otherwise; in row 6, the dependent variable is a binary indicator that takes value 1 if decisions over contraception are taken jointly by the couple or by the woman alone, and 0 otherwise. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. Columns (1)-(3) additionally control for ethnicity-specific trends for the distance to the international border, and columns (4)-(6) allow this trend to vary on either side of the border. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.9: *Free Primary Education and Female Economic Empowerment: Robustness Checks*

	Distance to border			Varying slopes		
	Average effect (1)	Gender equal (2)	Male dominant (3)	Average effect (4)	Gender equal (5)	Male dominant (6)
Ever divorced	0.056***	0.059***	0.051***	0.058***	0.064***	0.050***
	(0.010)	(0.013)	(0.016)	(0.010)	(0.013)	(0.015)
IPV acceptance	–0.022**	–0.022*	–0.021	–0.022**	–0.024*	–0.020
	(0.010)	(0.012)	(0.019)	(0.010)	(0.012)	(0.019)
Currently working	0.078***	0.063***	0.106***	0.079***	0.069***	0.096***
	(0.013)	(0.014)	(0.025)	(0.013)	(0.015)	(0.025)
Decisions: large purchases	0.109***	0.158***	0.015	0.105***	0.152***	0.028
	(0.017)	(0.021)	(0.027)	(0.018)	(0.022)	(0.028)
Decisions: own healthcare	0.170***	0.217***	0.066**	0.166***	0.212***	0.092***
	(0.017)	(0.019)	(0.032)	(0.018)	(0.019)	(0.032)
Decisions: visits to relatives	0.152***	0.164***	0.115***	0.154***	0.168***	0.133***
	(0.017)	(0.019)	(0.034)	(0.017)	(0.019)	(0.033)
Decisions: respondent's earnings	–0.022*	–0.007	–0.064**	–0.024*	–0.008	–0.064**
	(0.012)	(0.013)	(0.026)	(0.012)	(0.013)	(0.026)

Notes: The unit of observation is a woman. The dependent variable in rows 1-3 are: a binary indicator that equals 1 if the woman if a woman has ever been divorced and 0 otherwise; an IPV acceptance index index calculated as the average of answers to questions on whether IPV is justified in five scenarios; and a binary indicator that equals 1 if the woman is currently working. In rows 4-7, the dependent variable is a binary indicator that equals 1 if the woman participates in decisions over large purchases, own healthcare, visits to relatives or her earnings, respectively, and 0 otherwise. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. Columns (1)-(3) additionally control for ethnicity-specific trends for the distance to the international border, and columns (4)-(6) allow this trend to vary on either side of the border. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.10: *Free Primary Education and Media Engagement: Robustness Checks*

	Distance to border			Varying slopes		
	Average effect (1)	Gender equal (2)	Male dominant (3)	Average effect (4)	Gender equal (5)	Male dominant (6)
Reads newspaper weekly	0.018**	0.062***	–0.059***	0.015*	0.059***	–0.056***
	(0.008)	(0.010)	(0.013)	(0.008)	(0.010)	(0.013)
Watches television weekly	0.061***	0.116***	–0.034	0.057***	0.114***	–0.035
	(0.017)	(0.023)	(0.023)	(0.016)	(0.022)	(0.022)
Listens to radio weekly	0.088***	0.136***	0.006	0.094***	0.143***	0.017
	(0.015)	(0.019)	(0.022)	(0.014)	(0.019)	(0.022)
Listens to radio daily	0.074***	0.082***	0.060***	0.076***	0.088***	0.057***
	(0.014)	(0.020)	(0.018)	(0.014)	(0.020)	(0.017)

Notes: The unit of observation is a woman. The dependent variables in rows 1-3 are binary indicators that equal 1 if the woman reports that she reads a newspaper at last once per week, watches television at least once per week, and listens to the radio at least once per week, respectively. The dependent variable in row 4 is a binary indicator that equals 1 if a woman listens to the radio almost every day. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. Columns (1)-(3) additionally control for ethnicity-specific trends for the distance to the international border, and columns (4)-(6) allow this trend to vary on either side of the border. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A.11: *Free Primary Education and Polygyny: Robustness Checks*

	Distance to border			Varying slopes		
	Average effect (1)	Gender equal (2)	Male dominant (3)	Average effect (4)	Gender equal (5)	Male dominant (6)
In polygynous marriage	–0.074***	–0.103***	–0.021	–0.070***	–0.100***	–0.020
	(0.013)	(0.015)	(0.023)	(0.013)	(0.016)	(0.022)
Other wives	–0.191***	–0.235***	–0.129**	–0.191***	–0.235***	–0.122**
	(0.030)	(0.039)	(0.052)	(0.032)	(0.042)	(0.049)
Husband total children	–0.403**	–0.413*	–0.389	–0.446**	–0.476**	–0.402
	(0.181)	(0.222)	(0.304)	(0.180)	(0.224)	(0.301)

Notes: The unit of observation is a woman. The dependent variables in rows 1-3 are a binary indicator that equals 1 if the woman reports that she is in a polygynous marriage/union, her number of other wives, and the total children of her husband, respectively. All specifications include ethnicity fixed effects and ethnicity-specific trends for interview year and the birth year of respondents. Columns (1)-(3) additionally control for ethnicity-specific trends for the distance to the international border, and columns (4)-(6) allow this trend to vary on either side of the border. Standard errors clustered by DHS sampling cluster are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B Conceptual Framework

B.1. Household Bargaining with Commitment: Basic Predictions of the Quantity-Quality Model

We consider a household consisting of two partners: a female partner (f) and a male partner (m). Each partner, $g \in \{f, m\}$, derives utility from their individual consumption (c_g), the number of children (n), and the quality of those children (q). Fertility is bounded from above by the maximal number of children a mother can give birth to ($\bar{n}$). Child quality is determined by the production function $q = (\theta + e)^{\gamma}$, where e represents the educational investments made by the partners per child, $\gamma \in (0, 1)$ reflects the returns to education, and $\theta > 0$ denotes a child's baseline human capital in the absence of parental educational investments.

The preferences of partner g regarding individual consumption c_g, fertility n, and child quality q are represented by the following quasi-linear utility function:

$$u_g(c_g, n, q) = c_g + \delta_g v(n, q),$$

where δ_g is a preference parameter. For tractability, we assume that $v(\cdot)$ takes a logarithmic form, specifically $v(n, q) = \log(nq)$.

The female partner earns a wage w_f, while the male partner earns a wage w_m. Both partners have a total time endowment of 1, which they divide between labor and child-rearing. We assume that the male partner dedicates his entire time endowment to market work, whereas the female partner allocates a fixed amount of time ϕ per child to child-rearing. As a result, the female partner's earned income is $y_f(n) = (1 - \phi n)w_f$, and the male partner's earned income is $y_m = w_m$. For simplicity, the couple's total earned income will be denoted by $y(n) = y_f(n) + y_m$. The cost of education investment e is denoted by p. The household's budget constraint can be expressed as:

$$c_f + c_m + pen = y(n), \tag{3}$$

Bargaining with commitment assumes that the partners jointly decide on fertility, education expenditures and consumption *ex ante*, before any children are born. The couple engages in a (Nash) bargaining process, with an outside option where no children are born, and each partner consumes only their respective wage. The utility from this outside option, in the absence of an agreement, is $\hat{u}_g = w_g$. If partner g has a bargaining weight of σ_g (with $\sigma_f + \sigma_g = 1$), the couple maximizes the generalized Nash product:

$$\max_{c_f, c_n, n, e} = \left[c_f + \delta_f \left(\log(n) + \gamma \log(\theta + e)\right) - \hat{u}_f\right]^{\sigma_f} \left[c_m + \delta_m \left(\log(n) + \gamma \log(\theta + e)\right) - \hat{u}_m\right]^{\sigma_g} \tag{4}$$

subject to the budget constraint (3). The following proposition characterizes the bargain-

ing outcome for fertility and education:

Proposition 1 *Let $\Delta = \delta_f + \delta_m$. Under commitment, the couple's optimal choices for fertility and education are:*

$$n^* = \frac{\Delta(1-\gamma)}{\phi w_f - p\theta} \quad \text{and} \quad e^* = \frac{\gamma\phi w_f - p\theta}{p(1-\gamma)}.$$

From a comparative statics perspective, the introduction of free primary education (FPE) can be interpreted as a reduction in the effective price of education, p. In the model, a decline in p reduces fertility (since $\partial n^*/\partial p > 0$) and increases educational investment per child (since $\partial e^*/\partial p < 0$). This reflects one of the central insights of the quantity-quality model: as the price of education falls, parents will prioritize child quality over quantity.[18]

Proof of Proposition 1. We define the Nash product-relevant composite term for each parent $g \in \{f, m\}$ as

$$T_g = c_g + \delta_g \left(\log(n) + \gamma \log(\theta + e)\right) - \hat{u}_g.$$

The household's optimal allocation of consumption, fertility, and educational investment solves the maximization of the Nash product given by equation (4), subject to the budget constraint in equation (3). The corresponding first-order conditions are:

$$\sigma_f T_f^{\sigma_f - 1} T_g^{\sigma_g} - \lambda = 0 \tag{5}$$

$$\sigma_g T_f^{\sigma_f} T_g^{\sigma_g - 1} - \lambda = 0 \tag{6}$$

$$\sigma_f \frac{\delta_f}{n} T_f^{\sigma_f - 1} T_g^{\sigma_g} + \sigma_g \frac{\delta_g}{n} T_f^{\sigma_f} T_g^{\sigma_g - 1} - \lambda(pe + \phi w_f) = 0 \tag{7}$$

$$\sigma_f \frac{\gamma\delta_f}{\theta + e} T_f^{\sigma_f - 1} T_g^{\sigma_g} + \sigma_g \frac{\gamma\delta_g}{\theta + e} T_f^{\sigma_f} T_g^{\sigma_g - 1} - \lambda p n = 0 \tag{8}$$

Substituting the expressions for λ from equations (5) and (6) into (7) and (8), and simplifying, we obtain a system of two equations characterizing the optimal fertility level n^* and educational investment per child e^*:

$$\frac{\delta_f + \delta_m}{n} - (pe + \phi w_f) = 0$$

[18] FPE may also affect other parameters of the household problem. For example, by reducing the time intensity of childrearing, FPE could lower the maternal time cost ϕ. A decline in ϕ raises fertility (since $\partial n^*/\partial \phi < 0$) and reduces educational investment per child (since $\partial e^*/\partial \phi > 0$), while simultaneously increasing the time available for maternal labor supply. In addition, FPE may improve women's labor market opportunities—by increasing productivity or easing transitions into higher-quality employment—which can be represented in the model as an increase in the female wage, w_f (e.g., Clark et al., 2019). Higher wages lower fertility (since $\partial n^*/\partial w_f < 0$) and raise educational investment per child. Taken together, if FPE operates through these three channels, reductions in p and increases in w_f tend to depress fertility, whereas declines in ϕ may partially offset these effects.

$$\frac{\gamma(\delta_f + \delta_m)}{\theta + e} - pn = 0$$

Solving this system yields the closed-form expressions for n^* and e^* stated in the proposition.

B.2. Household Bargaining Without Commitment: The Potential Role of Culture

We now consider a simple extension of this model to demonstrate that the realization of these effects may depend on prevailing cultural norms. We assume that partners *cannot* commit to future consumption and educational investments when making fertility decisions, which changes the model from a single-period decision problem to a two-stage process. In the first stage, partners decide on fertility n, while in the second stage, they renegotiate their expenditure plans, determining how to allocate resources between consumption c_f, c_m and educational investments e.

During the bargaining in the second stage, each partner's outside option now depends on the fertility decision made in the first stage. We interpret the outside option as non-cooperation within an ongoing relationship, meaning that both partners continue to derive utility from children but make individual consumption decisions non-cooperatively, based on their respective budget constraints. To fully define each partner's outside option, we must specify how the couple, under non-cooperation, decides on the educational investment e per child. Building on the "separate spheres" bargaining idea introduced by Lundberg and Pollak (1993), we assume that the female partner determines the level of educational investment, with the resulting costs split between the partners according to shares χ_f and χ_m, with $\chi_f + \chi_m = 1$.[19] Under these assumptions, the female partner's non-cooperative choices for consumption and education, taking n as given, are:

$$\{\hat{c}_f(n), \hat{e}(n)\} = \arg\max_{c_f, e} \left[c_f + \delta_f \left(\log(n) + \gamma \log(\theta + e)\right)\right] \quad \text{s.t.} \quad c_f + \chi_f pen = y_f(n). \tag{9}$$

The partners' outside options are therefore:

$$\hat{u}_f(n) = \hat{c}_f(n) + \delta_f \left[\log(n) + \gamma \log(\theta + \hat{e}(n))\right] \tag{10}$$

$$\hat{u}_m(n) = \hat{c}_m(n) + \delta_m \left[\log(n) + \gamma \log(\theta + \hat{e}(n))\right] \tag{11}$$

where $\hat{c}_g(n)$ represents partner g's earned income y_g, net of their share χ_g of the education

[19] An alternative assumption would be to define child quality through the production function $q = (\theta + e_f + e_m)$, where each partner g non-cooperatively allocates their income between individual consumption c_g and educational investment e_g. Under this setup, and keeping all other assumptions unchanged, the Nash equilibrium of the resulting non-cooperative resource allocation game would lead to only one partner——specifically, the one who values child quality more highly——contributing to educational investments, while the other spends their entire income on individual consumption. If we assume mothers place a higher value on child quality, all our results would remain unchanged under this alternative scenario. Descriptive evidence from sub-Saharan Africa indeed suggests that mothers more important to a child's education than fathers (e.g., Lloyd and Blanc, 1996).

costs $p\hat{e}(n)n$.

Since the female partner decides on e non-cooperatively, disregarding how her choice affects her partner's utility, there are gains from cooperation in the second stage of the couple's decision-making problem. Therefore, we assume that the couple engages in (Nash) bargaining over consumption and education to realize these gains, using each partner's utility under non-cooperation as their threat point. Taking fertility n as given, the joint optimization problem is:

$$\max_{c_f, c_n, e} = \left[c_f + \delta_f \left(\log(n) + \gamma \log(\theta + e)\right) - \hat{u}_f(n)\right]^{\sigma_f} \left[c_m + \delta_m \left(\log(n) + \gamma \log(\theta + e)\right) - \hat{u}_m(n)\right]^{\sigma_g}$$

subject to the budget constraint (3). The solution can be derived in two steps. First, under our assumption of quasi-linear utilities, the couple's optimal choice for education maximizes their joint utility and is given by:

$$e^*(n) = \arg\max_e \left[c_f + c_m + (\delta_f + \delta_m)\left(\log(n) + \gamma \log(\theta + e)\right)\right] \quad \text{s.t.} \quad c_f + c_m + pen = y(n). \tag{12}$$

The couple's joint utility from cooperation is therefore:

$$U^*(n) = c_f^*(n) + c_m^*(n) + (\delta_f + \delta_m)\left[\log(n) + \gamma \log(\theta + e^*(n))\right] \tag{13}$$

where $c_f^*(n) + c_m^*(n)$ represents the couple's total earned income y, net of the education costs $pe^*(n)n$.

Second, the utilities that the partners obtain in the bargaining solution are:

$$u_f^b(n) = \sigma_m \hat{u}_f(n) + \sigma_f U^*(n) - \sigma_f \hat{u}_m(n) \tag{14}$$

$$u_m^b(n) = \sigma_f \hat{u}_m(n) + \sigma_m U^*(n) - \sigma_m \hat{u}_f(n), \tag{15}$$

where $U^*(n)$ and $u_g^b(n)$ are defined in equations (13) to (15). These outcomes reflect a simple "split-the-difference" rule, where each partner obtains the utility from their outside options plus a share σ_g of the gains from cooperation.

Going back to the first stage of the couple's decision problem, the partners determine the fertility level n, taking into account the utilities they expect to receive *ex post* in the bargaining solution. Consistent with evidence from industrialized countries, which shows that disagreements between partners over whether and how many children to have lead to lower fertility, Doepke and Kindermann (2019) model these fertility decisions by introducing the notion of veto power for each partner. However, the veto power assumption relies on the idea that women have at least as much influence over fertility decisions as men, which may not be the case if cultural norms give greater weight to men's preferences. For example, norms of male dominance may prevent women from blocking

their partner's preferred fertility outcome by limiting their access to contraception, either directly through opposition from husbands or indirectly through societal stigma (see, e.g., Ashraf et al., 2014; Cassidy et al., 2021). To account for this, we consider two culture-based scenarios: under high male dominance norms, women are assumed to lack the ability to block men's preferred fertility choices, while under low male dominance norms, women are assumed to have veto power. Veto power in our model is defined such that the couple's equilibrium fertility choice is determined by the partner with the lower preferred fertility outcome, that is, $n^* = \min\{n_f, n_m\}$.

The following proposition outlines the fertility choices of couples under these two scenarios.

Proposition 2 *In the absence of commitment, a couple's fertility decision depends on the extent of veto power held by women:*

- *(a) When women have the ability to block men's preferred fertility outcome (low male dominance), the equilibrium number of children is:*

$$n_L^{**} = \frac{\delta_f(1-\gamma)}{\phi w_f - \chi_f p\theta}.$$

- *(b) When women cannot block men's preferred fertility outcome (high male dominance), the equilibrium number of children is the maximal number of children a mother can give birth to:*

$$n_H^{**} = \bar{n}.$$

The key implication of the model is that, under limited commitment, fertility responses to FPE are asymmetric across low- and high–male-dominance regimes. In particular, a reduction in the cost of education, p, affects fertility only in environments in which women can block the fertility choices preferred by their partners. In such settings, a decline in p lowers fertility, as in the full-commitment benchmark. One distinction, however, is that under limited commitment equilibrium fertility—denoted n_L^{**}—is determined exclusively by women's preferences, whereas under full commitment fertility reflects the joint preferences of both spouses. This distinction yields the following prediction: in settings characterized by low male dominance—where women exert greater control over fertility decisions—actual fertility choices under FPE reflect more strongly the preferences of women, which is what we have documented empirically. By contrast, in high male-dominance contexts, equilibrium fertility—denoted n_H^{**}—corresponds to the maximum number of children a woman can physically bear and is invariant to changes in p and other model parameters.

Two key factors can explain this result. First, the marginal costs associated with choosing fertility n are entirely borne by women, reflected in a reduced future earnings

potential, ϕw_f. Second, when determining their preferred fertility outcome, each partner g seeks to not only maximize their own outside option under non-cooperation (weighted by 1-σ_g) and the couple's joint utility under cooperation (weighted by σ_g), but also to minimize their partner's outside option (also weighted by σ_g). For women, this involves balancing the tradeoff between reduced future earnings potential and the private benefits of having n children of quality q. For men, however, the perceived marginal costs of choosing fertility n influence their preferred fertility outcome through two opposing forces: while an increase in n reduces the couple's joint consumption possibilities under cooperation (which would normally limit their preferred fertility outcome), it also lowers their partner's outside option under non-cooperation by reducing her future earnings potential (which incentivizes higher fertility). That is, a key incentive for men to favor higher fertility lies in its effect on their partner's outside option: by reducing her future earnings potential, high fertility weakens her bargaining position within the household. In our stylized model, these opposing forces exactly offset one another, leaving men's preferred fertility outcome insensitive to the cost of fertility. As a result, men optimally choose the biologically feasible maximum number of children.

Proof of Proposition 2. We begin by solving equation (9) for the woman's non-cooperative educational investment as a function of fertility:

$$\hat{e}(n) = \frac{\delta_f \gamma}{\chi_f p n} - \theta. \tag{16}$$

Substituting this expression into equations (10) and (11), and differentiating each resulting utility expression with respect to n, yields:

$$\frac{\partial \hat{u}_f(n)}{\partial n} = -\phi w_f + \chi_f p \theta + \frac{\delta_f(1-\gamma)}{n}, \tag{17}$$

$$\frac{\partial \hat{u}_m(n)}{\partial n} = \chi_m p \theta + \frac{\delta_m(1-\gamma)}{n}. \tag{18}$$

Similarly, from equation (12), the educational investment that maximizes the partners' joint utility is:

$$e^*(n) = \frac{\gamma(\delta_f + \delta_m)}{pn} - \theta. \tag{19}$$

Substituting this into equation (13) and differentiating with respect to n gives:

$$\frac{\partial U^*(n)}{\partial n} = -\phi w_f + p\theta + \frac{(1-\gamma)(\delta_f + \delta_m)}{n}. \tag{20}$$

We now use equations (17), (18), and (20) to compute the derivatives of the bargaining

utilities $u_f^b(n)$ and $u_m^b(n)$, as defined in equations (14) and (15). These yield:

$$\begin{aligned}\frac{\partial u_f^b(n)}{\partial n} &= \sigma_m \frac{\partial \hat{u}_f(n)}{\partial n} + \sigma_f \left[\frac{\partial U^*(n)}{\partial n} - \frac{\partial \hat{u}_m(n)}{\partial n}\right] \\ &= -\phi w_f + \chi_f p\theta + \frac{\delta_f(1-\gamma)}{n}, \qquad (21) \\ \frac{\partial u_m^b(n)}{\partial n} &= \sigma_f \frac{\partial \hat{u}_m(n)}{\partial n} + \sigma_m \left[\frac{\partial U^*(n)}{\partial n} - \frac{\partial \hat{u}_f(n)}{\partial n}\right] \\ &= \chi_m p\theta + \frac{\delta_m(1-\gamma)}{n}. \qquad (22)\end{aligned}$$

Setting equation (21) equal to zero and solving for n yields the woman's preferred fertility choice under limited commitment, denoted n_L^{**}, as stated in the proposition. In contrast, equation (22) remains strictly positive for all $n \in [0, \bar{n})$, indicating that a man's preferred fertility level coincides with the biological upper bound, $n_H^{**} = \bar{n}$.

Appendix C Data Appendix

C.1. Merging DHS respondents to the Murdock Map

In this section, we provide an overview of the procedure we adopted to match each DHS respondent's self-reported ethnicity or language to ethnic groups in the Murdock Map (Murdock, 1959). This match serves two key purposes: it allows us to (i) select respondents belonging to ethnic groups split by country borders and implement our within-ethnicity design and (ii) assign the male dominance index to women and children, and thus conduct the heterogeneity analysis presented in Section 5.1.

Our within-ethnicity design relies on ethnic groups that have been split across country borders during the Scramble for Africa. The only way to determine whether a respondent belongs to such an ethnic group is by overlaying the Murdock Map—which depicts ancestral ethnic homelands prior to European contact—to contemporary international borders (Michalopoulos and Papaioannou, 2016). Therefore, a crucial requirement of the empirical specification described in Section 4 is that each respondent is matched to at most one Murdock ethnicity. To achieve this goal, and obtain our final estimation sample, we employ the following four-step procedure.

Step 1. In a first step, we use the *Linking Ethnic Data from Africa* (LEDA) matching algorithm developed by Müller-Crepon et al. (2022). The LEDA algorithm exploits the structure of linguistic trees to establish links between ethnic groups across datasets. Linguistic trees—taken from the Ethnologue database (Eberhard et al., 2024)—capture the genealogical relationship between languages, tracing them back to a common ancestor. By leveraging information on the languages spoken by ethnic group members, LEDA establishes a link between a given pair of ethnic groups in two different datasets—say, group A in the DHS and group B in the Murdock Map as in the present case—if their associated languages share at least one node at a specified level of the linguistic tree. LEDA allows the researcher to match ethnic groups at the level of either "language" or "dialect." To maximize precision, we choose the latter, so that LEDA links groups A and B to one another if they are associated with at least one common dialect in the language tree. If one were to specify "language" as the preferred level, the number of groups linked to A would tend to increase and the links would become less precise (albeit more likely to occur in the first place).[20] After running the algorithm on all available DHS survey rounds in 39 sub-Saharan African countries, we successfully pair 928 self-reported ethnic groups/languages out of 1,310 (i.e., 70 percent of ethnic groups, comprising 84.5% of respondents).[21]

[20] Additional details and a useful illustration of the procedure can be found in Müller-Crepon et al. (2022), accessible here: `https://github.com/carl-mc/LEDA/blob/master/docs/LEDA_paper.pdf` (for an illustration, see Figure 2).

[21] This set of 928 groups in the DHS includes many instances where the same ethnicity is reported by different respondents and survey waves with slightly different spellings or variations (for instance,

Step 2. The goal of this step is to assign a single Murdock ethnic group to each respondent. In the majority of cases, the LEDA algorithm assigns multiple Murdock groups to a given DHS group, as there might be multiple ethnicities in the Murdock Map whose language shares a common dialect with the language associated with a DHS respondent's ethnicity. We therefore retain all the DHS-to-Murdock LEDA links sharing the highest precision. Conveniently, LEDA provides us with the number of overlapping linguistic-tree nodes in each DHS-Murdock group pair, which can be considered as a measure of precision of the match. For each DHS ethnic group, we keep the link(s) with the highest number of overlapping nodes.

Step 3. After implementing Step 2, 86 instances remain where a DHS ethnic group is assigned to multiple Murdock ethnicities. To resolve these cases, we apply the following criteria:

(i) *Priority for Murdock groups split across country borders.*

(ii) *Name similarity.* For example, we assign the Murdock group *Ewe* rather than *Fon* to the DHS group *Adja-Ewe*.

(iii) *Availability of MDI data.* When one match lacks MDI data, we prioritize the alternative. For instance, we assign *Acholi* to the DHS group *Ethur* because MDI data is unavailable for *Labwor*, the other match suggested by LEDA.

(iv) *Additional sources.* The alternative name for the DHS group *Janjo* is *Jen*, as suggested by the Joshua project (`https://joshuaproject.net/people_groups/12321/NI`); since *Jen* is also one of the Murdock ethnic groups that LEDA assigns to *Janjo*, we adopt this match.

Reassuringly, in many cases, the final choice has minimal impact, as the matched Murdock groups exhibit similar MDI values.

Step 4. In a last step, we obtain our estimation sample by only keeping respondents matched to the Murdock ethnicities whose homelands were split into different countries during the Scramble for Africa according to Michalopoulos and Papaioannou (2016). Our estimation sample includes 63 DHS ethnic groups matched to 19 groups in the Murdock Map.

"betamaribe" and "betamaribe & related"). In addition to these 928 ethnicities, we manually pair 74 additional DHS groups not included in the LEDA package due to discrepancies in the spelling or the presence of special characters. The majority of unmatched instances include cases where respondents report "Other" or "None" when asked about their ethnic affiliation.

TABLE C.1: *Data Sources and Information on the Introduction of Free Primary Education: Part 1*

Country	Year of introduction	Source	Relevant Extract
Burkina Faso	2007	Kouraogo and Dianda (2008)	(Page 24) "In 2007 the government launched a general education reform that should bring among other innovations an extension of basic education from the current six years to ten years, and generalise progressively free compulsory education for children aged 6 - 16."
Cameroon	2000	Kamga (2011)	(Page 176) "... the principle of free primary education was underlined by the government's order of February 1996 that organises education in the country, and was translated into the Finance Law 2000/8"
Chad	1996	United Nations (2007)	(paragraph 220) "Articles 35 and 36 of the Constitution of 31 March 1996 proclaim the right to education. It is stipulated that public education shall be secular and free and that the State shall create the conditions and institutions to provide and guarantee the education of children."
Côte D'Ivoire	2015	Oyeniran (2018)	(Page 26) "... in 2015-2016, the state enacted grants free tuition in the basic education and colleges in the country. 'Free' education denotes that tuition fees will be waived through government funding"
Ghana	2005	World Bank (2009)	(table 1A.1) "Approach and year of fee abolition: 2005; scaling up of pilot started in 2003 for deprived districts." Which grades?: Grades 1-10. Which types of fees?: All types"
Kenya	2003	Riddell (2003)	(Table 1) "FPE Provisions: January 2003-free tuition and no school levies-costs remain: uniforms, exam fees"
Liberia	2011	Government of Liberia (2011)	(Section 4.4.1) "This level of education, which consists of full-time formal schooling that is provided for children from age six (6) to age twelve (12), and constituting grades 1-6, shall be free and compulsory for all children of the age range for such school level, and shall be free for all pupils within the public school system"
Malawi	1994	Riddell (2003)	(Table 1) "FPE Provisions: October 1994-free tuition, books, stationary -uniforms not compulsory"

TABLE C.2: *Data Sources and Information on the Introduction of Free Primary Education: Part 2*

Country	Year of introduction	Source	Relevant Extract
Mali	1962	United Nations (1997)	(paragraph 58) "The educational reform carried out in 1962 made education compulsory, mixed and free of charge."
Mozambique	2004	World Bank (2009)	(table 1A.1) "Approach and year or fee abolition: Decision in 2003 became effective in 2004 after testing. Phased implementation of direct support to schools 2004–06. Which grades?: First grades 1-5, then 1-7. Which types of fees?: All types"
Niger	1998	United Nations (2001)	(paragraph 273) "Act No. 98-12 of 1 June 1998, ... sets forth the right of the child to education and the obligation of the State to make primary education compulsory and free."
Nigeria	1976	Olatunji (2018)	(page 137) "The program of free education in Nigeria ... became a national project on September 6, 1976 when the then military head of state, Lieutenant–General (later General) Olusegun Obasanjo, launched the Free Universal Primary Education Scheme as a national project."
Senegal	1993	Ndiaye (2012)	(page 6) "... (Sylla, 1993:376). This new law was also meant to be democratic in the sense that it was meant to provide all citizens with free education, recognize their rights to equal educational opportunities (Sylla, 1993) and ensure that the curriculum would be relevant to the citizens' lived experiences."
Sierra Leone	2018	Government of Sierra Leone (2018)	(Page II) "...the MEST has decided to make basic education 'free and compulsory'..."
Togo	2008	Hoogeveen and Rossi (2019)	(Page 11) "Encouraged by the success of the 2007 elections and the new government's reform platform,which included the abolition of school fees starting in the 2008/2009 school year..."
Uganda	1997	Riddell (2003)	(Table 1) "FPE Provisions: January 1997 -free tuition (6-12 yr-olds) -costs remain: clothing, school feeding,scholastic materials, and school fund contributions"
Zambia	2002	Riddell (2003)	(Table 1) "FPE Provisions: February 2002 -user fees abolished -uniforms not compulsory -fees can be levied by PTAs and boards, but no student can be denied an education because of cost"

TABLE C.3: *Surveys Included in our Sample*

Country	Surveys
Burkina Faso	1992-93; 1998-1999; 2003; 2010
Cameroon	1998; 2018
Chad	2014-15
Côte D'Ivoire	1998-99; 2011-12
Ghana	1998-99; 2003; 2008; 2014
Kenya	1988-89; 1993; 1998; 2003; 2008-09; 2014
Liberia	2013
Malawi	2000; 2004-05; 2010; 2015-16
Mali	1987; 1995-96; 2001
Mozambique	2011
Niger	1992; 1998; 2006
Nigeria	2008
Senegal	1986; 1992-93; 1997; 2005; 2010-11; 2012-13; 2014; 2015; 2016; 2017; 2018, 2019
Sierra Leone	2008; 2013; 2019
Togo	1998; 2013-14
Uganda	1988; 1995; 2011; 2016
Zambia	1992; 1996; 2001-02; 2007; 2013-14

TABLE C.4: *Cross-Sectional Analysis: Data Extract*

Ethnicity	**Country ID**	**Interview Year**	**FPE**$_{ect}$	**Year FPE implementation**
e	12 (early adopter)	1998	0	2004
e	12 (early adopter)	1999	0	2004
e	12 (early adopter)	2003	0	2004
e	12 (early adopter)	2008	1	2004
e	12 (early adopter)	2014	1	2004
e	34 (late adopter)	1998	0	2007
e	34 (late adopter)	2013	0	2007
e	34 (late adopter)	2014	0	2007

Notes: This table illustrates the logic behind the treatment indicator FPE$_{ect}$ adopted in the design described in Section 6. The table illustrates a generic ethnicity e located in two countries: country 12, the *early* FPE adopter, and country 34, the *late* FPE adopter.

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