AFRICAN POLYGAMY: PAST AND PRESENT

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Abstract. I evaluate the impact of education on polygamy in Africa. Districts of French West Africa that received more colonial teachers and parts of sub-Saharan Africa that received Protestant or Catholic missions have lower polygamy rates in the present. I find no evidence of a causal effect of modern education on polygamy. Natural experiments that have expanded education in Nigeria, Zimbabwe, Sierra Leone and Kenya have not reduced polygamy. Colonial and missionary education, then, have been more powerful sources of cultural change than the cases of modern schooling I consider.

1. Introduction

Polygamy and poverty are both widespread in Africa.[1] Several mechanisms have been proposed linking polygamy to slow growth, including low savings rates (Tertilt, 2005), reduced investment in girls' human capital (Edlund and Lagerlöf, 2006), and diminished labor supply of unmarried men (Edlund and Lagerlöf, 2012). In the "polygamy belt" stretching from Senegal to Tanzania, it is common for more than one third of married women to be polygamous (Jacoby, 1995). Excepting Haiti, polygamy is less common in other developing countries. In all other Demographic and Health Surveys (DHS), at least 92% of married women are reported to be monogamous. This is despite a striking decline in the prevalence of polygamy in Africa over the last half century. In Benin, more than 60% of women in the sample used for this study who were married in 1970 are polygamists, while the figure for

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[1]I deal only with polygyny (multiple wives) in this paper, ignoring polyandry (multiple husbands). Nonspecialists are typically unfamiliar with the term "polygyny."

those married in 2000 is under 40%. This is also true of Burkina Faso, Guinea, and Senegal. Several other countries in the data have experienced similar erosions of polygamy. This is an evolution of marriage markets as dramatic as the rise in divorce in the United States or the decline of arranged marriage in Japan over the same period.

I use DHS data on 494,157 women from 34 countries to ask two questions. First, does increasing education reduce polygamy in Africa? Second, does the answer to this question differ between colonial and modern education? Theories of polygamy exist that suggest it will disappear as education becomes more prevalent (Gould et al., 2008). Similarly, female education and monogamy both correlate with general measures of female empowerment (Doepke et al., 2012); educating women may empower them to pursue more favorable marital outcomes.

I find that colonial education reduces polygamy. I use historic data from Huillery (2009) and Nunn (2014a) to show that schooling investments decades ago predict lower polygamy rates today. Within French West Africa, I show that former colonial districts that had more teachers per capita during the early colonial period display lower rates of polygamy today. This correlation is robust to the inclusion of several district characteristics as controls and to basing inference off comparisons of adjacent districts within narrow geographic neighborhoods. There is only limited evidence that observable characteristics of districts that predict greater receipt of colonial teachers also predict lower rates of polygamy. Similarly, parts of present-day Africa that are in closer proximity to colonial missions have lower rates of polygamy in the present. This is robust to several controls, increasingly stringent geographic fixed effects, different measures of proximity to missions, and comparison with immediately adjacent areas. There is no evidence that missions were located in areas that had less polygamy before colonial rule.

In the modern period, there exists a clear negative correlation between a woman's years of education and whether she is a polygamist. To test if this is causal, I exploit four natural experiments that have increased female education in Nigeria (Osili and Long, 2008), Zimbabwe (Agüero and Bharadwaj, 2014), Sierra Leone (Cannonier and Mocan, 2012), and

Kenya (Chicoine, 2012). The Nigerian Universal Primary Education (UPE) program exposed specific cohorts of children from certain regions of the country to additional primary schooling. While Osili and Long (2008) use a difference-in-difference approach to show that this reduced fertility by age 25, I find no effect on whether women exposed to the program married polygamously. This is robust to state trends and to discarding Lagos from the analysis. Instrumental variables estimates using the intervention do not suggest a negative effect of a woman's education on her later polygamy.

In Zimbabwe, the end of white rule made higher levels of education available to black women. Using a regression discontinuity that compares cohorts just young enough to be treated by this change with their older peers, Agüero and Bharadwaj (2014) show that this improved their knowledge about HIV. By contrast, I find no effect on polygamy. This result is robust to changing the width of the window around the cutoff age and discarding cohorts just around the cutoff. Instrumental variables estimates are insignificant and, at their most negative, still suggest a causal effect that is smaller than the raw correlation observed in the data.

In Sierra Leone, a Free Primary Education (FPE) program benefitted certain cohorts and varied in intensity over space. Cannonier and Mocan (2012) use a difference-in-difference approach to show that the program changed women's attitudes towards health and violence towards women. I find only weak evidence that the program affected polygamy. While my point estimates are consistently negative, they are insignificant and small in most specifications. Some robustness exercises suggest a significant negative effect of the program, but only if variation over space in program intensity is not used for identification. Otherwise, results are robust to alternative specifications of the control group, controlling for violence during the civil war, alternative coding of the treatment group, and restricting the sample to non-migrants.

In Kenya, a reform of the education system during the 1980s lengthened primary school, leading to an average increase in schooling attainment for affected cohorts. Identifying treatment effects using nonlinearities in exposure across cohorts, Chicoine (2012) finds that

the reform led to reduced fertility and delayed marriage. I find no effects on polygamy. Instrumental variables estimates do not suggest a negative effect of years of schooling on polygamy. This is robust to restricting the sample of included cohorts, and there are similarly null effects for placebo cohorts unlikely to have been affected by the reform.

Together, these results suggest that different mechanisms must explain the nonzero effects of historical education and the null effects of modern education. Changes due to historical education accrued through the accumulation of benefits and externalities over time and the pressures for cultural change found in both secular and mission schools. The effects of modern schooling have been muted by a narrower focus, heterogeneous impacts that lack reinforcement, and the low quality of contemporary schooling.

1.1. **Contribution.** My results contribute to our knowledge of the determinants of ethnic institutions. Institutions such as pre-colonial states and land tenure matter for modern incomes (Goldstein and Udry, 2008; Michalopoulos and Papaioannou, 2013). Although an empirical literature has explained national institutions as products of influences such as settler mortality, population, trade, or suitability for specific crops, less is known about the origins of ethnic institutions. Like national institutions, these may have been shaped by geographic endowments such as ecologically-driven gains from trade, by population pressure, or by colonization (Acemoglu et al., 2014; Fenske, 2014; Osafo-Kwaako and Robinson, 2013). I add to this literature by testing hypotheses about the origin of one specific ethnic institution, and by identifying variables that influence its persistence and evolution.

My results also add to our understanding of family structures. Several recent contributions have explained marriage patterns using the gender division of labor created by influences such as the plough (Alesina et al., 2013), animal husbandry (Voigtländer and Voth, 2013), natural resource wealth (Ross, 2008), or deep tillage (Carranza, 2014). Other views link marital rules to risk-sharing arrangements (Rosenzweig and Stark, 1989). Part of this literature has drawn on economic models, history, and anthropology to discuss the causes of polygamy, stressing variables such as male inequality (Barber, 2008; Bergstrom, 1994; Betzig,

1982, 1992, 1995; Kanazawa and Still, 1999), the gender division of labor (Boserup, 1970; Jacoby, 1995), political economy (Anderson and Tollison, 1998; De la Croix and Mariani, 2012; Lagerlöf, 2010), the slave trade (Dalton and Leung, 2014; Edlund and Ku, 2011; Thornton, 1983), the gender ratio (Becker, 1974; White and Burton, 1988), land quality (Korn, 2000), animal husbandry (Adshade and Kaiser, 2008), son-preference (Milazzo, 2014), abundance of land (Goody, 1973, 1976), assortative matching (Siow, 2006), and fertility preferences (Grossbard-Shechtman, 1986). Another branch of this literature has examined the implications of polygamy for outcomes such as cooperation (Akresh et al., 2011) or economic growth (Tertilt, 2005). While many models of polygamy suggest that it benefits women, its prevalence across countries is negatively correlated with other measures of female empowerment (Doepke et al., 2012). In this paper, I uncover a dramatic transition in the continent's marriage markets, and assess one plausible explanation for this change.

Finally, I add to a literature that tests the effects of education on female empowerment. The policy changes I exploit have each been used in previous studies to test whether educating women shapes their later fertility, health, and beliefs (Agüero and Ramachandran, 2010; Bhalotra and Clarke, 2013; Cannonier and Mocan, 2012; Chicoine, 2012; Osili and Long, 2008). Other studies have used similar natural experiments or randomized control trials to test whether educating women improves outcomes for them. In some cases, providing women with education or skills can have large beneficial effects, even compared to the impacts on men (Bandiera et al., 2014; Blattman et al., 2014). Many of these studies, however, have found effects that are weak or heterogeneous (Duflo, 2012; Friedman et al., 2011). This heterogeneity requires explanation. Although polygamy is a marital outcome for millions of African women, I am not aware of any study that has attempted to identify a causal effect of education on polygamy, in either experimental data or through the use of a natural experiment.

The question of historical persistence cuts across all three of these literatures. A large literature has recently demonstrated that several past events, investments, and institutions have had effects that remain visible today (Bruhn and Gallego, 2012; Dell, 2010; Nunn,

2014b). My results provide an unusual example in which not only does history matter, but in which it matters more than policy initiatives in the present day.

I introduce the multiple data sources that I use and describe broad patterns of polygamy and education in section 2. I demonstrate the persistent effects of colonial education in section 3 and test for effects of modern education in section 4. I discuss the mechanisms explaining these effects in section 5. In section 6, I conclude.

2. Data

In this section, I introduce my sources of data. I provide additional details in the online appendix.

2.1. Data sources.

Data are taken from the "individual recode" sections of 90 DHS surveys conducted in 34 sub-Saharan countries between 1986 and 2009. These individual-level samples are nationally representative cross-sections of ever-married women of childbearing age. From these surveys, 494,157 observations are available in which a woman's polygamy status, year of birth, and urban residence are known. A woman is coded as polygamous if she reports that her husband has more than one wife. Latitude and longitude coordinates of the respondent's survey cluster are known for 301,183 of these observations.[2] Year of birth, year of birth squared, age, age squared, dummies for religion, years of schooling, and whether the woman lives in an urban area are taken from these surveys.

Data on historic education are taken from two existing papers. Teachers per capita and other controls from colonial French West Africa from Huillery (2009) are available on her website. Her primary sources are annual local budgets for the period 1910 to 1928. I average the number of teachers for each district in each year over this period and divide by the population in 1925 (the year with the fewest missing values). Additional controls that describe the pre-colonial and geographic characteristics of these districts are also taken from Huillery (2009, 2011), who describes them in greater detail.

[2]Recent DHS surveys add noise to these coordinates. Because this displaces 99% of clusters less than 5 km and keeps them within national boundaries, this adds only measurement error to the geographic controls.

Locations of colonial missions from Nunn (2014a) (originally from Roome (1924)) are available on his website. This map reports precise locations and denominations of missions established by foreign missionaries for indigenous Africans. Distance to a Protestant or Catholic mission is computed using the latitude and longitude coordinates of the mission locations and affiliations recorded by Roome (1924) and the coordinates of the respondent's survey cluster.

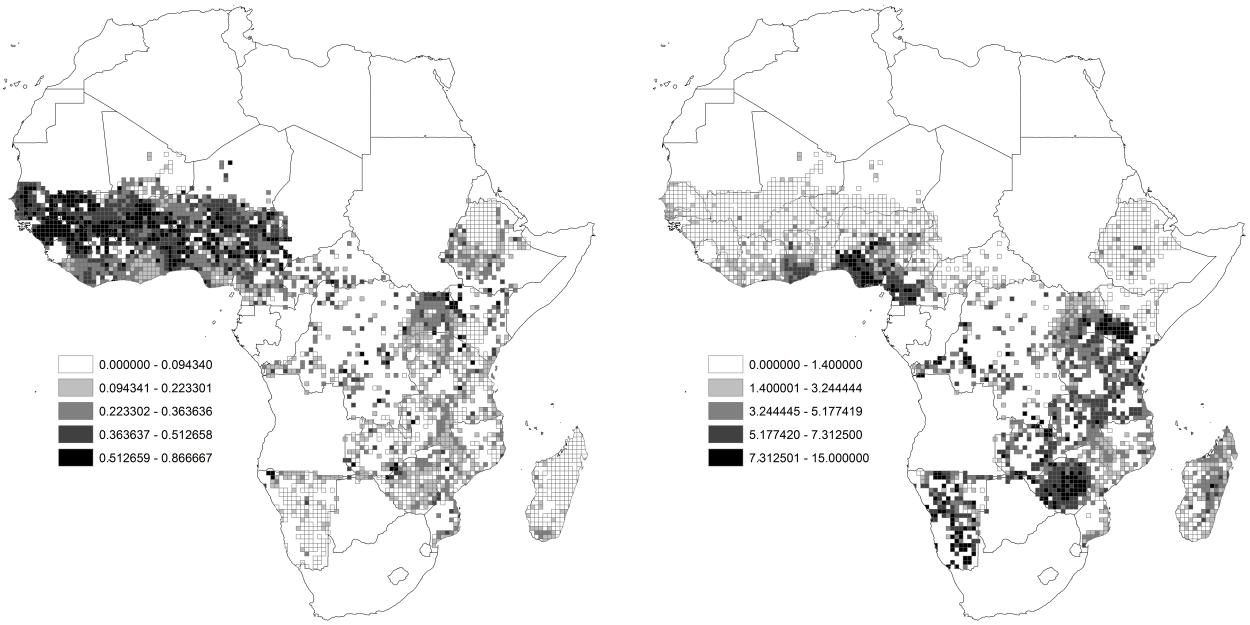
Geographic controls are collected from several sources. For each of these, I assign a survey cluster the value of the nearest raster point. I obtain suitability for rain-fed agriculture and ecological zone from the Food and Agriculture Organization's Global Agro-Ecological Zones (FAO-GAEZ) project. The ecological zones are dummy variables, while the suitability measure ranges from 0 to 7. Elevation is an index that ranges from 0 to 255, taken from the North American Cartographic Information Society. Malaria endemism is from the Malaria Atlas Project, and ranges from 0 to 1. Ruggedness is the Terrain Ruggedness Index used by Nunn and Puga (2012), which ranges from 0 to 1,368,318. Absolute latitude and distance from the coast are computed directly from the cluster's coordinates.

Two of the natural experiments that I exploit require additional data not in the DHS. I use three measures of Nigeria's UPE program from Osili and Long (2008): a dummy for a "high intensity" state, school-building funds in 1976 divided by the 1953 census population estimates, and school-building funds normalized by (unreliable) 1976 population projections. These are reprinted in their paper. I follow Osili and Long (2008) in matching respondents to the Nigerian states that existed in 1976 by matching current states to the historic states from which they were split off.

In order to assess the effect of the Free Primary Education program in Sierra Leone, I require the data on district-level primary-school funding and teachers in 2004 used by Cannonier and Mocan (2012). They have collected this data from the Government of Sierra Leone Budget and Statement of Economic and Financial Policies for the financial year 2004, and have kindly shared it with me in personal correspondence.

Summary statistics are in Table 1.

FIGURE 1. Polygamy and education in Africa



This figure plots the mean polygamy rate and years of education of women in each cell.

2.2. **The distribution of African polygamy and education over space and time.** I map African polygamy and education in Figure 1. I aggregate the women for whom coordinates are available to cells that are 0.5 degrees in latitude by 0.5 degrees in longitude. Darker colors indicate higher rates of polygamy or greater mean years of schooling. Polygamy is concentrated in West Africa, though a high-intensity belt stretches through to Tanzania. Polygamy in the data is largely bigamy: 72% of respondents report that they are the only wife, 19% report that their husband has two wives, 7% report that he has three wives, and fewer than 2% report that he has 4 wives or more (see Table A1 in the online appendix).

It is clear from Figure 1 that areas with high rates of polygamy are also areas in which African women have fewer years of education on average. Further, it is clear that there are correlations across space between polygamy and education that hold within countries. Southern Ghana, Southern Nigeria, and Southwestern Kenya all stand out as more educated and less polygamous than other regions in these countries.

I show the decline of polygamy over time in Figure 2. A raw correlation between year of birth and polygamy will confound time trends with age effects, since a young lone wife may later become a polygamist's senior wife. Thus, I estimate the time trend of polygamy for each country with more than one cross-section. I use the regression:

$$Polygamous_i = f(Age_i) + g(YearOfBirth_i) + \epsilon_i. \tag{1}$$

The functions f and g are quartic. I use the estimated coefficients and survey weights to calculate the predicted probability that a woman aged 30 is polygamous as a function of her year of birth. I present these in Figure 2. Though the speed of the decline has differed across countries, its presence has been almost universal. In the online appendix, I show raw plots of polygamy by age stratified by country and survey year: these tell a similar story. To my knowledge, this is not a trend that has been documented previously.[3]

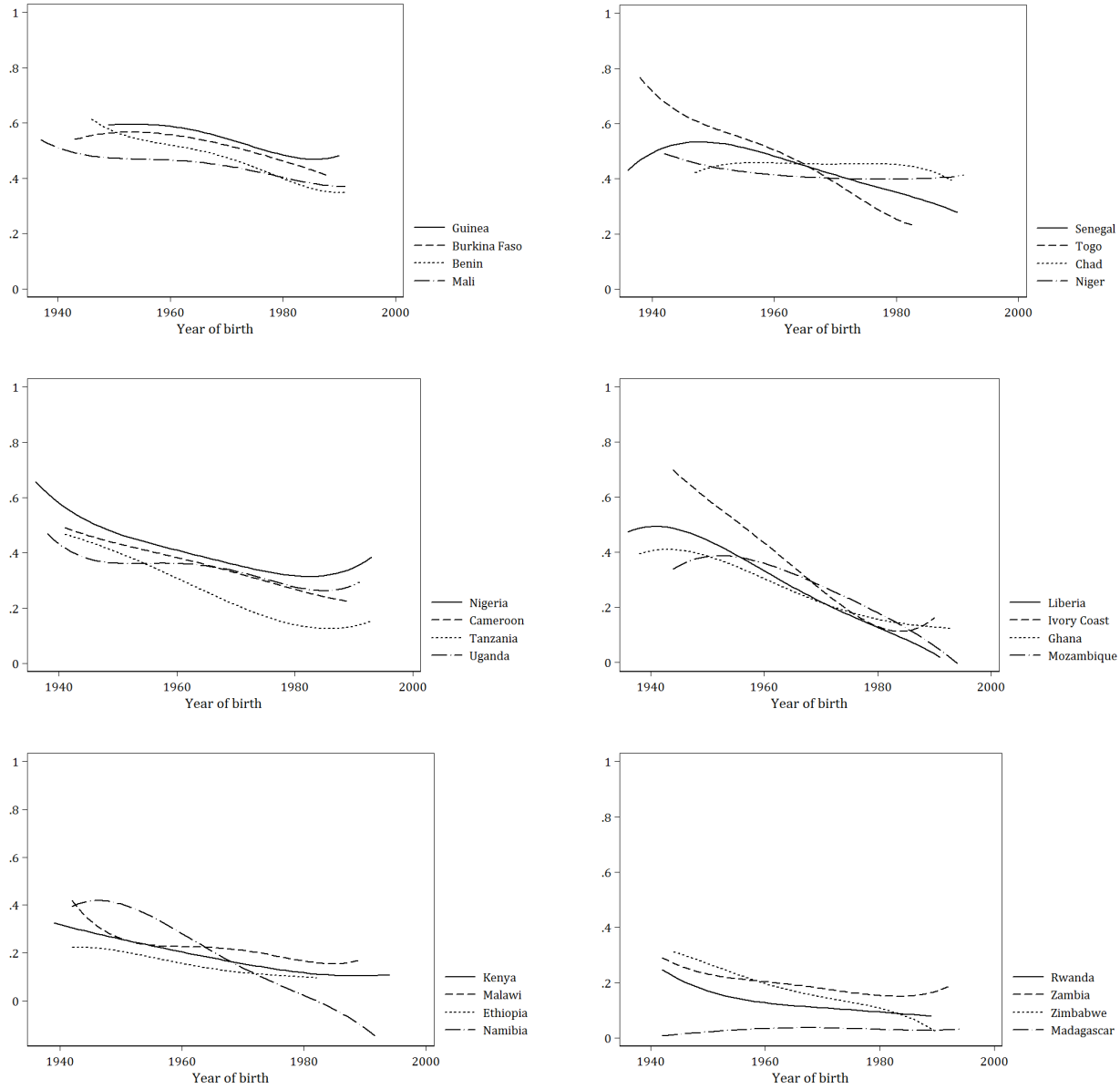
3. African Polygamy: Past

3.1. **Colonial teachers in French West Africa.**

There are 108 districts of French West Africa for which data on both polygamy and colonial teachers are available. The education system established in French West Africa in the early twentieth century was intended to provide rudimentary education to the African masses and a higher level of instruction to a small elite (Gravelle, 2014). Education expanded slowly, from 2,500 primary pupils in 1900 to 62,300 in 1935, mostly in the first two grades, and mostly in public schools rather than mission schools (Gardinier, 1980). Expansion was more rapid after the Second World War, and French West Africa had 381,753 primary pupils in 1957-8 (Gardinier, 1980). The post-independence political class was educated in the interwar period in schools such as the William Ponty School in Senegal (Chafer, 2007).

[3]The data do not permit a similar exercise for men. In the online appendix I also show the evolution of average years of schooling by birth cohort for the women in the sample.

FIGURE 2. Predicted polygamy over time for women aged 30, by year of birth

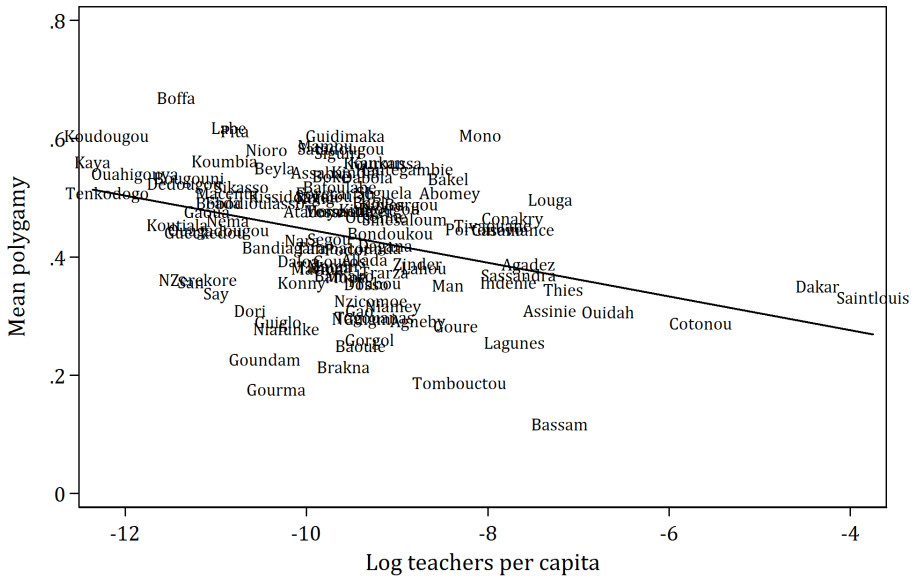


Time trends show predicted polygamy at age 30 by year of birth, as calculated by estimating (1).

Wary of creating an unemployed anti-colonial intelligentsia, the French limited post-primary education (Gardinier, 1980).

Schools in French West Africa operated on the French principle of assimilation, teaching in French and with the French curriculum (Cogneau and Moradi, 2014). Despite post-colonial reform efforts, most teaching in countries such as Senegal remains in French following a curriculum in the French style, with funding skewed towards urban areas (Bolt and Bezemer,

FIGURE 3. Colonial teachers and modern polygamy in French West Africa



This figure plots mean polygamy in each district of colonial French West Africa against log teachers per capita.

2009). Madrassas were suppressed by the colonial government (Bleck, 2013). Although the French were wary of missionary education in the colonies, pragmatic concerns led them to allow missionary schools to receive state funding alongside state schools (Bolt and Bezemer, 2009). Mission schools that did not comply with government rules, however, were shut down (Cogneau and Moradi, 2014).

To test whether former districts of French West Africa that received more teachers in the early colonial period show lower rates of polygamy today, I estimate:

$$Polygamous_{ij} = \beta TeachersPerCapita_j + x'_{ij}\gamma + \epsilon_{ij} \tag{2}$$

Here, $Polygamous_{ij}$ is an indicator for whether woman i in former colonial district j is a polygamist. $TeachersPerCapita_j$ is the average number of teachers per capita in district j over the period 1910 to 1928. In alternative specifications, I replace $TeachersPerCapita_j$ with its natural log. x_{ij} is a vector of controls that includes a constant, the woman's year of birth and its square, dummies for religion and whether the woman lives in an urban area,

and a quadratic in the woman's age. Age and year of birth are not collinear, since not all women are surveyed in the same year. In successive columns, I add controls to x_{ij} so that it matches the controls used by Huillery (2009) as closely as possible. I include measures of the attractiveness of the district to the French, conditions of its conquest, pre-colonial conditions, and geographic variables. Standard errors are clustered by 1925 district. I use the linear probability model throughout this paper, because of my use of fixed effects, my large sample size, and for simplicity.

In Figure 3, I show the raw correlation between the natural log of colonial teachers and the polygamy rate across respondents in the sample. There is a clear negative correlation. I present results of estimating (2) in Table 2. A one standard deviation increase in colonial education reduces polygamy by roughly 1 percentage point (column 1). In columns (2) through (5) I add the controls added by Huillery (2009), and in column (6) I add country-round fixed effects. The result remains robust across columns, and holds whether I use teachers per capita or its natural log.

In each column I also report Altonji et al. (2005) ratios. These compare the coefficient without a restricted set of controls β^r to the coefficient with a full set of controls β^f in the ratio $\beta^f/(\beta^r - \beta^f)$; see Nunn and Wantchekon (2011). For example, a large positive ratio suggests that unobservable variables must have a large effect on β relative to the observed controls in order to explain away the result. Here, column (1) – no controls – is the "restricted" set. A negative ratio exists if additional controls make the estimated effect larger. The ratio is negative or greater than one in all specifications, implying in the first case that controls actually strengthen the results and in the second case that the influence of unobserved variables would have to be greater than that of the observed controls to explain the results.

One benefit of looking only within West Africa is that this removes bias due to the selective colonization of regions of high education demand by particular colonial powers (Frankema, 2012). In Table A3 in the online appendix, I report a robustness exercise similar to one in Huillery (2009). I divide French West Africa into squares of equal size and assign districts

to squares by the latitude and longitude coordinates of the capital city. I then create fixed effects for these squares. I show that, even restricting identification to comparisons of districts within the same 1° by 1° square, districts with more colonial teachers have lower polygamy rates in the present. The most stringent specification (comparison within 1° by 1° squares) assigns 109 districts in the sample across 91 fixed effects.

I also use Table A3 to test whether there is evidence that colonial teachers were stationed in districts whose pre-determined characteristics predict lower rates of polygamy today. More teachers per capita existed in districts that collected more trade taxes before colonial rule, in areas that were conquered earlier and resisted conquest longer, in areas that had a trading post before colonial rule and were more densely settled at the outset, and in drier areas with access to the sea. Few of these characteristics, however, predict polygamy. Access to the sea predicts less polygamy today and average rainfall predicts more polygamy, both of which are suggestive of positive selection. Their effects are small, however. A one standard deviation change in either variable predicts a less than one tenth of a standard deviation change in the mean polygamy rate for a given district. Results survive controlling for these characteristics, and the other correlates of teachers are uncorrelated with polygamy.

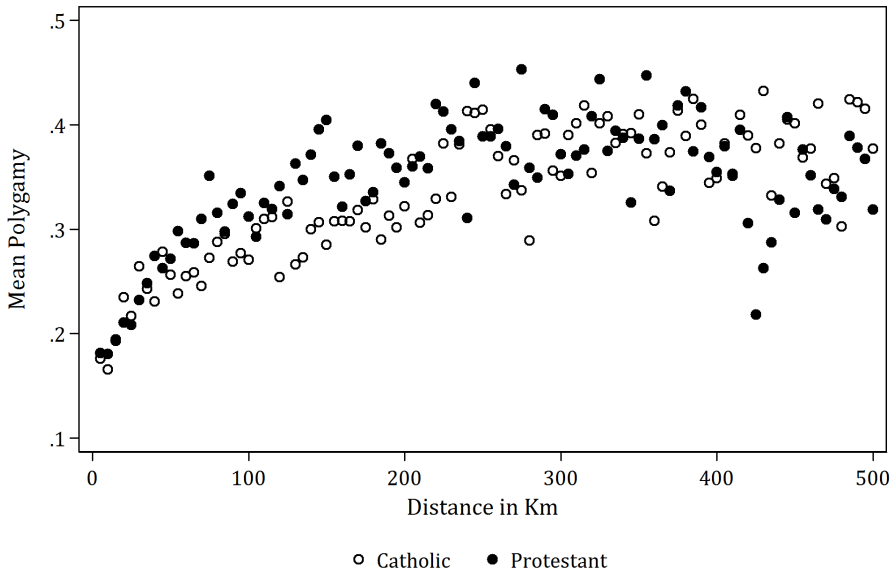
3.2. **Missions.**

In order to test whether a past history of exposure to Christian missionaries reduces polygamy in the present, I estimate:

$$Polygamous_{ij} = \beta \ln(Distance_j) + x_i'\gamma + \epsilon_{ij}. \tag{3}$$

Here, $Polygamous_{ij}$ is an indicator for whether woman i in survey cluster j is in a polygamous marriage. I also report specifications in which a woman's years of education is the dependent variable. $Distance_j$ is the distance of her survey cluster from the closest mission in the Roome (1924) map. I calculate distances separately for Catholic and Protestant

FIGURE 4. Colonial missions and modern polygamy



This figure allots the women in the sample into 5km bins based on their distance from the nearest colonial mission. Each dot plots mean polygamy within a bin against the midpoint of the distance bin.

missions. x_{ij} is a vector of individual and geographic controls that includes a constant, the woman's year of birth and its square, whether the woman lives in an urban area, and a quadratic in the woman's age. I also include several geographic controls: absolute latitude, malaria, suitability for rainfed agriculture, ruggedness, elevation, distance to coast, and dummies for ecological type. In alternative specifications, I will add fixed effects for a variety of geographic regions. ϵ_{ij} is error. Standard errors are clustered by survey cluster.

In figure 4, I show the raw correlation between distance from a mission and polygamy. I divide the sample into 5km bins classified by distance from a mission, and calculate the average polygamy rate for each bin. The positive correlation of distance from a mission and polygamy is clear from the figure. This is also clearly concave, justifying the logarithmic specification.

I present the results of estimating (3) in Table 3. The results indicate that proximity to either Catholic or Protestant colonial missions reduces polygamy in the present and raises a woman's years of education. Moving left to right across columns, I add geographic controls, country-round fixed effects, fixed effects and controls together, and geographic controls with

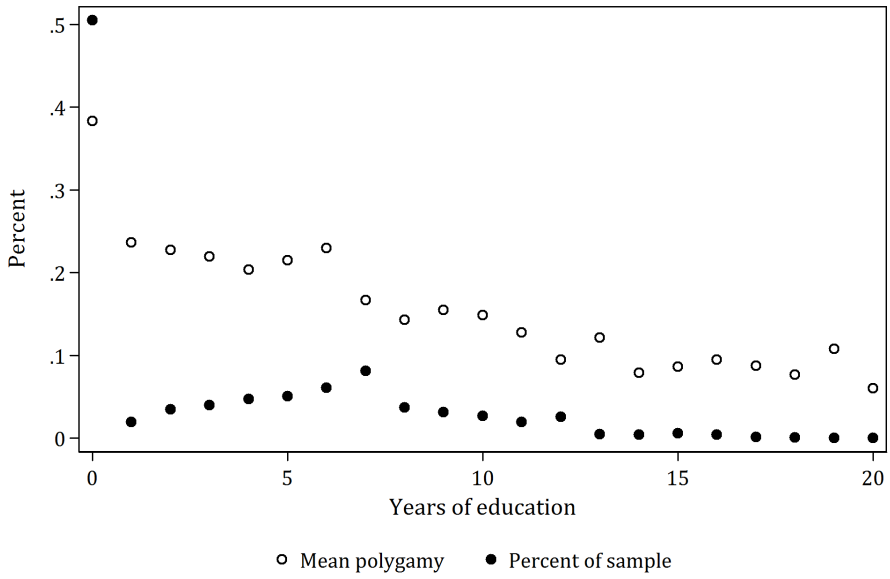
sub-national "region" fixed effects that approximate provinces. Results are robust across columns. As in Table 2, I also report Altonji et al. (2005) ratios. With a single exception, these are greater than one or negative. A one standard deviation increase in access to a Catholic mission reduces polygamy by roughly 2 percentage points with the tightest fixed effects (column 12). For Protestant missions (column 16), the standardized effect is approximately the same. Results are similar if distance from both Catholic and Protestant missions are entered simultaneously (not reported). Though Nunn (2014a) finds effects of missions that differ by denomination, he is constrained by the Afrobarometer sampling frame to use a smaller sample that covers fewer countries.

In the online appendix, I report several robustness exercises. In Table A4, I show that these results survive controlling for respondent religion. Indeed, the point estimates are largely unchanged. Although religion may be a channel through which mission exposure reduced polygamy over the long run, it is by no means a sufficient statistic. I also use this table to follow Nunn (2014a) by adding additional historical controls that may have influenced mission placement. In particular, I control for slave exports in the Atlantic and Indian Ocean slave trade and distance from the nearest route of a nineteenth century European explorer. The results remain robust.

In Table A5, I report three additional checks. In Panel A, I replace the continuous measure of distance from a mission with a dummy variable that equals one if the survey cluster is within 20km of a colonial mission. In Panel B, I keep the same treatment, but restrict my sample to areas within 40km of a mission. In Panel C, I divide the whole of Africa into squares of equal size and assign clusters to these squares using their coordinates. I then add fixed effects for these squares. Even with fixed effects for 1° by 1° squares (approximately 69 miles by 69 miles), there is a positive correlation between distance from a historic mission and modern polygamy.

It is still possible that missions were selectively placed in areas where polygamy was least common. Supporting a causal interpretation, I show in Table A6 that ethnicities recorded in the Murdock (1967) *Ethnographic Atlas* that practiced polygamy received more missions

FIGURE 5. Polygamy by years of schooling



This figure plots mean polygamy for every woman in the sample with a given number of years of schooling against years of schooling. The fraction of the sample with that level of schooling is also shown.

per unit area. Using the Murdock (1959) map of Africa, I count the number of missions in the Roome (1924) map that fell within the territory of each ethnic group. I then merge the ethnic groups in the map to data on polygamy recorded in the *Ethnographic Atlas* using the procedure described by Alsan (2015) and Fenske (2014). Regressing the prevalence of missions on historical polygamy and geographic controls, there is no evidence that missions were more common where polygamy had been less prevalent before colonial rule.

4. African Polygamy: Present

4.1. **Polygamy and own education: Correlations.**

I begin by discussing the raw correlation that exists between education and polygamy in modern data. In Figure 5, I plot the mean polygamy rate for women in my sample against their years of education, while also showing the fraction of the sample that has each level of education. Slightly more than half the sample has no education, and the polygamy rate for this group is nearly 40%. After a sharp decline once any education is achieved, each

additional year of education correlates with a modest decline in the probability of polygamy, so that women who have at least 12 years of education have a less than 20% chance of being polygamous.

This correlation may of course be confounded by omitted variables. I show in Table 4 that the size of the correlation is quite sensitive to controls. I use OLS to estimate:

$$Polygamous_i = \beta YearsOfEducation_i + x_i'\gamma + \epsilon_i. \tag{4}$$

Here, $Polygamous_i$ is an indicator for whether a woman in the sample is polygamous. $YearsOfEducation_i$ is the number of years of education she has received. In alternative specifications, I replace this with a binary indicator for whether she has any schooling at all. x_i contains, in alternative specifications, the individual and geographic controls included in Section 3: a constant, the woman's year of birth and its square, whether the woman lives in an urban area, religious dummies, a quadratic in the woman's age, absolute latitude, malaria, suitability for rainfed agriculture, ruggedness, elevation, distance to coast, and dummies for ecological type. I also add, in different specifications, fixed effects for country-round, sub-national region, and survey cluster. Standard errors are clustered by survey cluster.

This regression does not test for a causal impact of a woman's years of schooling on polygamy, but only uncovers the size of the raw correlation in the data. Without controls, an extra year of schooling appears to have a large correlation with polygamy; one more year of schooling predicts a 2.4 percentage point reduction in the probability of polygamy. This shrinks dramatically as controls and fixed effects are added, falling to 0.6 percentage points in the most conservative specification (column 5). The reduction in probability due to moving from no education to any education is similarly sensitive to controls and fixed effects. This is suggestive evidence that unobservable variables explain much of the correlation between modern education and polygamy, and motivates the use of natural experiments below.

4.2. **Polygamy and parental education: Correlations.** If historical schools proxy for parental education, this could help explain the legacies of colonial education found above,

and the sensitivity of the correlation between polygamy and own education. Because the DHS data do not report parental education, I test whether there is a correlation between parental education and polygamy in three West African datasets – the 2010 Nigerian Living Standards Measurement Study, the first two rounds of the Ghana Living Standards Study (GLSS, 1986-86) and the Ivory Coast Living Standards Measurement Study (LSMS, 1985-88). On each of these datasets, I use OLS to estimate:

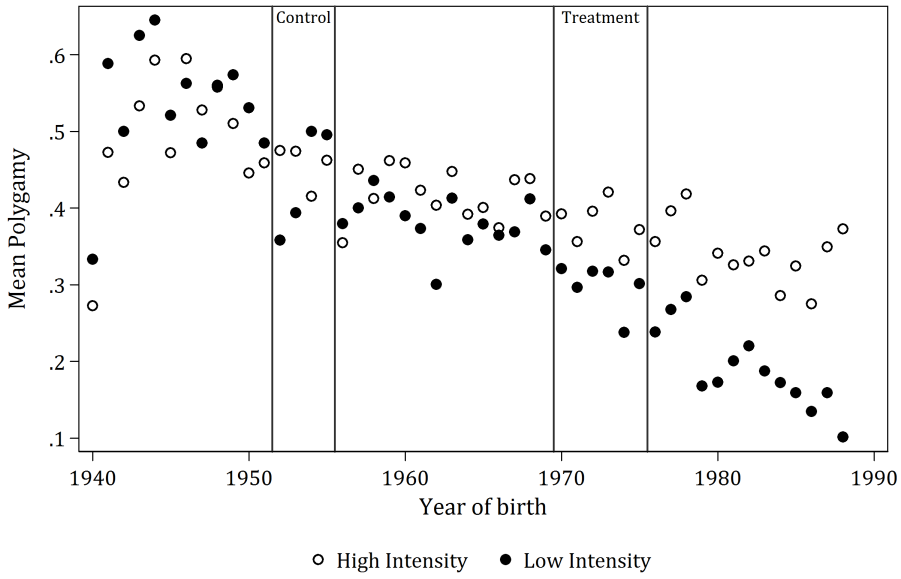
$$Polygamous_i = OwnEducation_i'\alpha + ParentalEducation_i'\beta + x_i'\gamma + \delta_j + \epsilon_i. \tag{5}$$

Here, $Polygamous_i$ is a dummy for whether a woman is a polygamist. $OwnEducation_i$ and $ParentalEducation_i$ are vectors that include dummies for own and parental educational achievement. These vary by country because educational systems vary by country. I collapse uncommon categories together as "other," and use "none" as the omitted category. The controls x_i generally include a constant, dummies for year of birth, religion, and survey round. The fixed effects δ_j are for the smallest administrative units reported in the data – clusters in the Ivory Coast and Ghana, Local Government Areas in Nigeria. Standard errors are clustered at the level of these units.

I show in Table 5 that mother's education does not predict polygamy in any of these samples. Daughters of more educated fathers are less likely to be polygamous in Nigeria and the Ivory Coast, and the negative correlations between own education and polygamy are large and significant only in the Ivory Coast. Father's education correlates with reduced polygamy in the Ivory Coast, but not in Ghana and only uncommon educational categories of father's education correlate with a woman's polygamy in Nigeria.

4.3. **Natural experiments.** To test whether expanded access to education has reduced polygamy in Africa, I exploit four recent natural experiments that have exogenously exposed some cohorts of women to additional education. The original studies that exploited these policy changes have used them to identify effects on outcomes such as fertility, health, and beliefs (Agüero and Bharadwaj, 2014; Cannonier and Mocan, 2012; Chicoine, 2012; Osili

FIGURE 6. Polygamy and Universal Primary Education in Nigeria



This figure plots mean polygamy for each birth cohort, with the sample separated according to whether Osili and Long (2008) classify the woman's state as a "high intensity" or "low intensity" state in the Nigerian Universal Primary Education (UPE) program.

and Long, 2008). Later studies have validated them by testing for effects of education on additional outcomes. Bhalotra and Clarke (2013), for example, use three of them to test whether maternal education reduces maternal mortality.

4.3.1. *Universal Primary Education in Nigeria.*

The first natural experiment that I exploit comes from Nigeria. Between 1976 and 1981, the Nigerian government pursued a Universal Primary Education (UPE) program that provided free primary schooling and increased the supply of classrooms and teacher training initiations nationwide: see Osili and Long (2008) for details. Importantly, investment was targeted towards states in the North and Southeast with low levels of pre-program enrollment and educational inputs. Gross enrollment rates of both boys and girls doubled between 1974 and 1981, and these gains were greatest in states receiving the most new investment.

Osili and Long (2008) use this to test whether female schooling reduces fertility. I use the same regression specification as Osili and Long (2008), and use OLS to estimate:

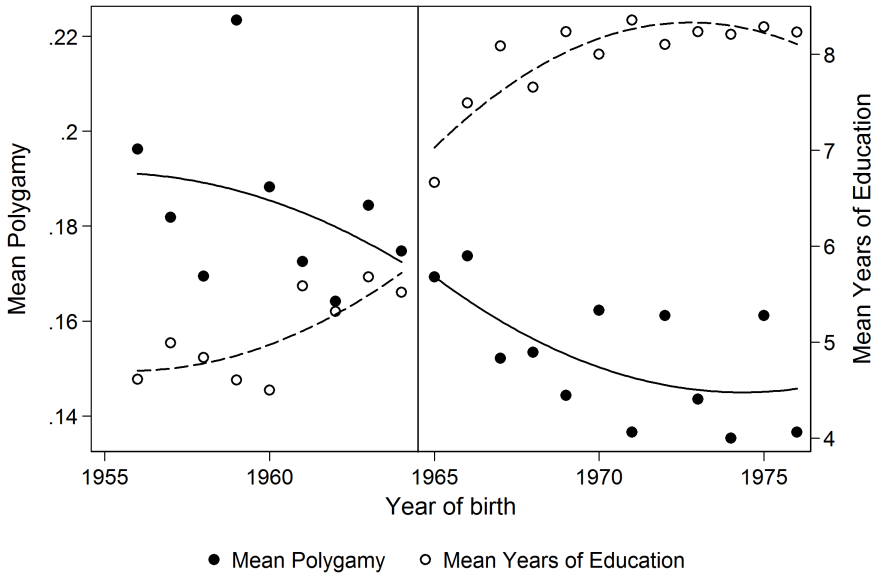
$$Polygamy_i = \beta TreatmentCohort_i \times Intensity_i$$
$$+ \alpha Intensity_i + \lambda TreatmentCohort_i + x_i'\gamma + \epsilon_i \tag{6}$$

Here, $Polygamy_i$ is an indicator for whether woman i is a polygamist. $TreatmentCohort_i$ is a dummy for having been born between 1970 and 1975. $Intensity_i$ will, in different specifications, measure either whether the respondent's state was treated by the program, or spending per capita in the state. The controls in x_i match Osili and Long (2008). These are year of birth, dummies for the three largest Nigerian ethnic groups (Yoruba, Hausa, Igbo), and dummies for the major religions (Muslim, Catholic, Protestant, other Christian, and traditional). The sample includes only women born between 1956-61 and 1970-75. I follow their approach in limiting the sample to individuals aged 25 and over, although I use all five available waves of the Nigerian DHS, rather than the 1999 data on its own.[4] This tests whether the UPE program had a differential effect on the women young enough to be exposed to it as children in the affected states. β is the treatment effect. Standard errors are clustered by the states that existed in 1976.

Figure 6 shows the identifying variation in the sample. I plot the mean polygamy rate by birth cohort separately for women from the high and low intensity states. It is clear that the gap in cohort polygamy rates between the high-intensity states and the low-intensity states is greater between 1970 and 1975 than between 1956 and 1961. While the education gap closed between high-intensity and low-intensity states, polygamy rates widened. This foreshadows the results of estimating (6), reported in Table 6. For two of the three measures of intensity, exposure to the UPE program increases education but has no effect on polygamy. Using the less reliable census figures to normalize spending in columns 3 and 6, there is no effect on either education or polygamy.

[4]If I restrict the sample to the 1999 DHS, I similarly find no effect of the educational reform on polygamy (not reported).

FIGURE 7. Polygamy and Education in Zimbabwe



This figure plots mean polygamy and mean years of education for each birth cohort in the sample. The vertical line at 1964.5 separates those young enough to be treated from the remainder of the sample.

I report several robustness checks in the online appendix. In Table A7, I add state-specific cohort trends, an exercise not performed by Osili and Long (2008). This weakens the effect of the UPE program on education without eliminating it, and there is still no evidence of an effect of the program on polygamy. I also use (6) as the first stage of an instrumental variables specification, in which the first stage dependent variable is years of schooling, the second stage dependent variable is polygamy, and $TreatmentCohort_i \times Intensity_i$ is the excluded instrument. There is again no evidence that greater education led to less polygamy after the UPE program. In Table A8, I show that the results are robust to removing Lagos, a possible source of migrants whose states of birth might be mis-measured.

4.3.2. *The end of white rule in Zimbabwe.*

The end of white rule in Zimbabwe promted educational reforms. These removed the restrictions that prevented black students from progressing to secondary school. Secondary school enrollment increased immediately and dramatically, and access to education was discontinuously increased for black students who were 15 or younger in 1980. Agüero and

Bharadwaj (2014) examine the impacts of this reform on knowledge of HIV, Agüero and Ramachandran (2010) test for intergenerational effects of this education shock, and Bharadwaj and Grepin (2014) estimate the impacts on child mortality. Following Agüero and Bharadwaj (2014), I use IV to estimate:

$$Polygamy_i = \alpha + \beta S_i + x_i'\theta + e_i \tag{7}$$

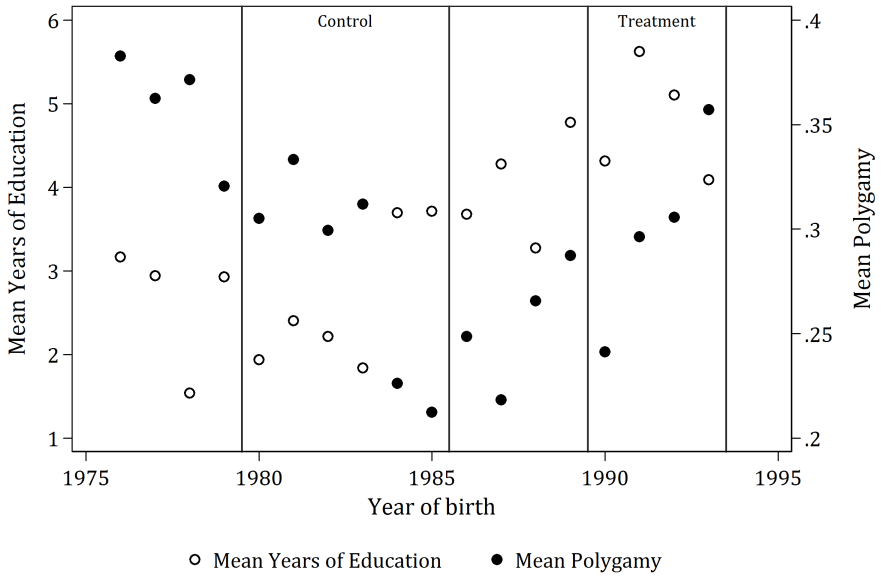
and

$$\begin{aligned} S_i = \beta_1 DumAge_i + \beta_2 DumAge_i \times (Age80_i - 15) \\ + \beta_3(1 - DumAge_i) \times (Age80_i - 15) + x_i'\theta + \epsilon_i \end{aligned} \tag{8}$$

The outcome of interest in the second stage (7) is $Polygamy_i$. It is an indicator for whether a woman is polygamous. S_i is her years of schooling, and β is the coefficient of interest. The first-stage equation (8) exploits the discontinuity in exposure based on age in 1980. $DumAge_i$ is a dummy variable that takes the value one if a woman was aged 15 or less in 1980. This is the excluded instrument. $Age80_i$ is the woman's age in 1980, and so the estimation allows for differential cohort trends on either side of the cutoff. The controls included in x_i in both stages are whether the woman lives in an urban area, dummies for region, and dummies for DHS round. The two trend variables are included in the second stage. I follow Agüero and Bharadwaj (2014) and cluster standard errors by the intersection of region and age in 1980.

Figure 7 shows the identifying variation in the sample, plotting mean years of education and mean polygamy against cohort, where the vertical line separates treated from untreated cohorts. It is clear that those who were still young enough to enter secondary school in 1980 discontinuously increased their educational attainment relative to their slightly older peers. Polygamy rates, by contrast, continue along an almost unbroken trend.

FIGURE 8. Polygamy and Free Primary Education in Sierra Leone



This figure plots mean polygamy and mean years of education for each birth cohort in the sample.

This is confirmed in Table 7, which presents OLS and reduced form estimates of (7) and (8). Treated cohorts gain more than a year's schooling, but there is no reduction in polygamy. Instrumental variables estimates confirm this null effect. In Table A9 in the online appendix, I show that these results are robust to excluding individuals aged 14 and 15 in 1980; this makes the discontinuity in education more sharp, but has no effect on the insignificant break in polygamy.

4.3.3. *Free Primary Education in Sierra Leone.*

Cannonier and Mocan (2012) show that the Free Primary Education (FPE) policy in Sierra Leone affected women's attitudes towards issues such as violence and health. The FPE policy, started in 2001, expanded access to education for all students in schools that were government-owned or government-assisted. This benefitted younger cohorts, but not those who were already old enough to have left primary school. Following Cannonier and Mocan (2012), I estimate:

$$schooling_{ijt} = \beta_0 + \beta_1 FPECohort_i + X_i\Omega + \gamma_j + \epsilon_{ijt}. \tag{9}$$

Here, the years of schooling of woman i, from district j and born in year t, depends on whether she was exposed to the program ($FPECohort_i$), controls X_i, and district fixed effects γ_j. The FPE cohort consists of those born between 1990 and 1993. The sample is women born between 1980 and 1985, and between 1990 and 1993. Controls in X_i are dummies for Christian, Temne, Mende, Urban, Married, Employed, Radio, Fridge, TV, and Wealth Quintiles. Regressions are weighted using weights from the DHS data, and standard errors are clustered by district $\times$ year of birth. In alternative specifications, I estimate a reduced-form equation in which $schooling_{ijt}$ is replaced by an indicator for polygamy, and I use program exposure as an instrument for years of schooling, with polygamy as the outcome.

Alternatively, the effect of the program may vary with its intensity, R_j, suggesting the regression:

$$schooling_{ijt} = \varphi_0 + \varphi_1 FPECohort_i \times R_j + X_i\Psi + \gamma_j + \delta_t + \nu_{ijt}. \tag{10}$$

Again following Cannonier and Mocan (2012), R_j is the logarithm of district-level primary school funding in 2004, divided by the number of teachers and multiplied by 100. The estimation now includes year of birth fixed effects δ_t. As before, I also estimate reduced-form and IV effects on polygamy.

The identifying variation is shown in Figure 8. I plot mean years of schooling and mean polygamy against birth cohorts. While the mean years of schooling is higher for women in the treatment cohort than in the control cohort, there is no clear difference in polygamy rates.

Results are reported in Table 8. I find that, although the FPE program did increase schooling attainment, it had no statistically significant impact on the probability that the women

exposed to the program were polygamous. The instrumental variables estimates, while negative, are insignificant. They are only large in column (7), which does not use program spending as a source of exogenous variation in schooling and has a very low Kleibergen-Paap Wald F statistic, which might indicate a large bias in the estimate. In Table A10 in the Online appendix, I report several robustness checks. First, I show that the results look similar when the control group is replaced with women aged 12 to 16 in 2001. Second, I control for exposure to the violence of the country's civil war by assigning each woman in the control group the number of battle deaths recorded in each respondent's district in the Uppsala Conflict Data Program's database. If I do so, a negative effect of the program emerges on polygamy, but only when I fail to use cross-district variation program spending for identification. Third, I scale the measures of treatment down by one half for women who were aged 12 to 14 in 2001, and thus may have only been partly treated by the reform. Again, a negative effect of the program emerges on polygamy if I only use age to measure treatment. Finally, I show that the results remain similar if the sample is restricted to women who have always lived in their current place of residence.

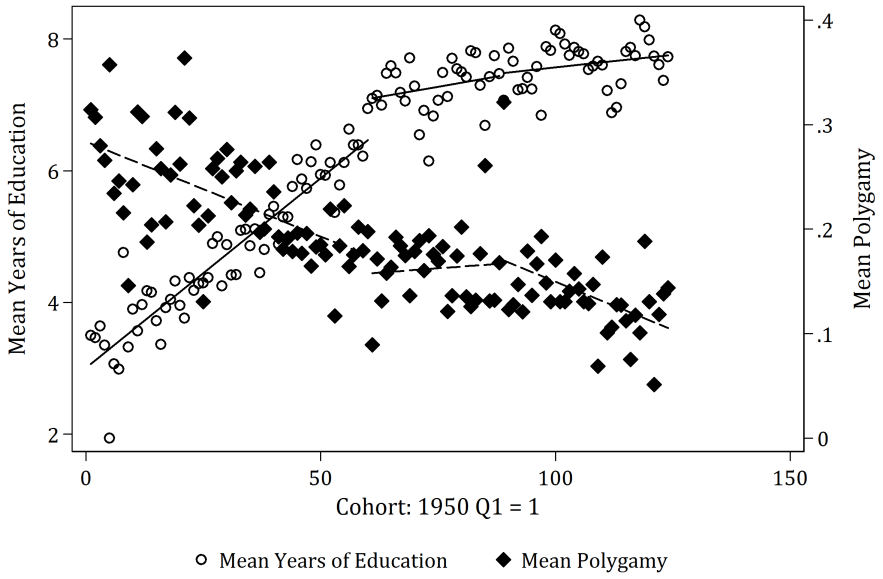
4.3.4. *Education reform in Kenya.*

A Kenyan education reform in January 1985 lengthened primary school from seven years to eight. This tended to increase years of schooling for the affected cohorts; students who left school after primary school now received eight years of education, while those who completed secondary education now finished with twelve years of school, rather than eleven.

Chicoine (2012) uses this reform to test whether additional schooling reduces fertility for women. Following his specification, I use instrumental variables to estimate:

$$Polygamy_{ic} = \alpha + \beta YearsOfEducation_{ic} + x'_{ic}\gamma + \epsilon_{ic}. \tag{11}$$

FIGURE 9. Polygamy and Education in Kenya



This figure plots mean polygamy and mean years of education for each birth cohort in the sample.

Here, $Polygamy_{ic}$ is an indicator for whether woman i from birth cohort c is a polygamist. Birth cohorts are defined by both quarter and age, starting at 1 for women born in the first quarter of 1950. $YearsOfEducation_{ic}$ is woman i's years of schooling. x_{ic} contains a quadratic in cohort, a cubic in age, dummies for ethnicity, dummies for quarter of birth, and dummies for childhood region of residence. I cluster standard errors by year of birth and weight observations using the DHS sample weights. I instrument for $YearsOfEducation_{ic}$ with a variable, $ChicoineInstrument_c$, that measures exposure to the educational reform. It is 0 for cohorts too old to be exposed to the reform (quarters 60 and earlier - i.e. those born before 1965). It is 1 for cohorts exposed to the full reform (quarters 89 and later - i.e. those born after 1971). It increases linearly during the transition period between cohorts 60 and 89. The sample includes women born between 1950 and 1980.

Figure 9 depicts the identifying variation in the data. I plot mean education and polygamy rates against cohorts of birth. It is clear that education increases non-linearly after the reform is enacted, though at a decreasing rate. The declining trend in polygamy, by contrast, slows after the reform is enacted. I report results of estimating (11) in Table 9. Although it is clear that $ChicoineInstrument_c$ predicts education over and above trends in age and birth

cohort, it has no power to predict polygamy. Instrumental variables results similarly show no impact of the reform on polygamy.

I report robustness checks in Table A11, in the online appendix. First, I show that the results are similar if I restrict the sample to women born since 1955. Second, I use two placebos to show that the reform had no effect on women who were unlikely to have been affected by the reform; the variable $ChicoineInstrument_c$ does not predict schooling for women who received less than four years of schooling (quit primary early) or more than 17 years (continued into post-secondary).

5. Discussion

5.1. **Colonial education.** The results above suggest that histories of colonial and missionary education reduce polygamy in the present, but recent schooling expansions have failed to have the same effect. Why?

This is not driven by differences in the set of controls used across the different empirical specifications. I show in Table A12 in the online appendix that using a sparse but consistent set of controls across specifications does not change the general pattern of results.[5]

First, the mechanisms for persistence that Huillery (2009) identifies in French West Africa would have taken years to accumulate: continued use of existing facilities, increased demand due to positive externalities, and the enhanced political power of educated communities that could now better lobby for public goods. Further, externalities from colonial education may similarly have taken time to benefit the descendants of those who were not educated directly

[5]Because not every variable is available for every subset of the data (e.g. geographic controls are not available for DHS surveys without GIS data, Nigerian Free Primary Education intensity is only available for Nigeria) I can only make specifications consistent across tables by controlling for a sparse set of individual controls I use in the baseline: age, age squared, year of birth, year of birth squared, religion dummies, and urban. I present results of Tables 2, 3, 4, 6, 7, 8 and 9 in the appendix using these controls and country-round fixed effects. I do not include Table 5, since this uses entirely different data sources. I use the same clustering and samples as in the baseline, though I no longer weight observations in any specification. For the natural experiments, I include variables that are essential to the identification strategy. For Nigeria, these are the dummies for "high intensity" and "treatment cohort." For Zimbabwe, these are the running variables in year of birth before and after the policy change. For Sierra Leone and Kenya there are no such "essential" variables. The main results continue to hold: colonial education and colonial missions predict reduced rates of polygamy, there is a reduced-form correlation between education and polygamy today, but none of the four natural experiments (in reduced form) predict reductions in polygamy.

(Wantchekon et al., 2013). Early colonial schools, then, capture not only an individual woman's access to education, but also the effects of the access to education experienced by several generations of her ancestors, both male and female. Indeed, controlling for a respondent's years of education in Table 2 weakens the effect of colonial schooling slightly, but it remains significant in all specifications (not reported). The effect of colonial teachers on polygamy is already apparent for the oldest cohorts in my sample, and I have found no evidence that it is diminishing for younger cohorts. Colonial schooling will also have affected the education of a woman's peers; I discuss this below in the context of missions.

The content of education has changed since the period of colonial schools and missions. Colonial administrators believed that Africans should receive instruction with a religious basis, and so some of the religious messages of mission schools were found in secular education (White, 1996). Bolt and Bezemer (2009) characterize colonial education in French Africa as "a dual church-state educational system" in which missionary schools that followed the French curriculum could receive state funding. The French colonial state also opposed polygamy; in Cameroon, for example, it allowed polygamist wives to seek divorce and encouraged monogamy (Walker-Said, 2015). Both colonial and missionary education, then, had effects beyond the simple treatment of providing education. Exposure to the colonial power and to missions had a long series of effects that reduced polygyny.

Much colonial education came through missions; missionaries educated 97% of pupils in Ghana and Nigeria during the 1940s, and operated 96% of the schools in South Africa (Berman, 1974; Nunn, 2014a). Missionaries also made education accessible, bypassing formal rules and teaching in local languages (Frankema, 2012).

Missionaries taught Christian views on marriage that discouraged polygamy. Although the degree of missionary intolerance towards polygamy changed over time, missionaries, particularly Catholics, opposed polygamy as a rule (Falen, 2008). Indeed, prohibition of polygamy by the Catholic Church has pushed Christian polygamists into African independent churches, such as the Cherubim and Seraphim, United Native African, and Apostolic Churches (McKinney, 1992). For missionaries, the sanctity of Christian marriage was an

overarching concern (see e.g. Chanock (1985) for Malawi or Phillips et al. (1953) for the whole of Africa). Kudo (2014) also finds a long-run relationship between missionary activity and polygamy, specifically within Malawi. He interprets this as a causal effect of religion. Polygamous marriages, notably, cannot be blessed in a church. Both my results and his survive controlling for respondent religion, though they are consistent with conversion being an important channel. That the results are similar for both Catholic and Protestant missions suggests, however, that religious doctrine is not a complete explanation.

Missionaries put a heavy emphasis on the Bible and on sermons in their schools, and often had little regard for African customs, actively discouraging tradition (Berman, 1974). Although culture can be persistent despite outside influences (Nunn, 2012; Voigtländer and Voth, 2012), the missionary program of social change was broad. Protestant missionaries translated the Bible into local languages, taught to women and the poor, built voluntary organizations, brought printing presses and newspapers, taught science, supported secondary education, encouraged medicine and public health, promoted cash crops, farming techniques, and industrial and business skills, undermined monopolies, promoted the rule of law, colonial reforms, and democracy, and protected the property rights of Africans (Cagé and Rueda, 2014; Woodberry, 2012; Woodberry et al., 2014).

The relationship of Christian missionaries with Islam is also instructive. Islam was a barrier to the expansion of Christian missions and to the reduction of polygamy. In Northern Nigeria, the colonial government attempted to discourage Christian missionaries, seeing them as a threat to the system of indirect rule (Barnes, 1995). Victorian missionaries believed that Islamic support of polygamy gave it an advantage in Africa (Prasch, 1989). In Malawi, Islamic ethnic groups were more resistant to the effects of missionary teaching on polygamy (Kudo, 2014). How missionaries wrote about Islam also indicates their particular concern with polygamy. Christian missionaries wrote disapprovingly of Islam, citing its support of polygamy as a point of criticism (Freas, 1998; Hassing, 1977).

Controlling for either the average education or shares of each religion within a respondent's survey cluster diminishes the effects of missions by roughly half, though they remain

significant (not reported). This is suggestive evidence that the enduring effects of missions operate because they affect not only the religious beliefs and education of an individual woman, but also those of her peers. This effect differs from the four modern natural experiments I exploit, which only treated specific cohorts of women and so are unlikely to have been strengthened by social reinforcement.

5.2. **Modern education.** Modern education does not fail to reduce polygamy by failing to inform students that polygamy is illegal; in each of Nigeria, Sierra Leone, Kenya and Zimbabwe, polygamy is legal in at least some circumstances, such as a marriage under customary or Islamic law.

Does modern education fail to reduce polygamy because of a different attitude towards religion than existed in mission or colonial schools? While modern African schools do not typically have the same religious focus as colonial mission schools, religion is not absent from modern schools. Schooling in post-independence Africa is heterogeneous. For example, the school system in Mali today is a mix of "public schools, private, Francophone schools, madrassas, and community schools" (Bleck and Guindo, 2013). While private schools (including religious schools) typically educate a quarter or less of primary students in Africa, there are some exceptions: notably 88.1% of Zimbabwean primary students were in private schools in 1999 (Tsimpo and Wodon, 2014).

Despite the secular nature of public (state) schools in many African countries, students in these schools are taught about religion. This is true, for example, in Tanzania (Dilger, 2013), and in the four countries used for natural experiments. In Nigeria, public (state) schools are secular, as they were during the oil boom (Abubakre, 1984). Non-vocational electives include Bible knowledge and Islamic studies (Oluniyi and Olajumoke, 2013). In Zimbabwe, schools do teach "Religious and Moral Education" with a Christian focus (Marashe et al., 2009; Mutepfa et al., 2007). In Sierra Leone, some 75% of schools are faith-based (Nishimuko, 2008), accounting for more than half of enrolment (Wodon and Ying, 2009). An overwhelming majority of teachers believe religion contributes positively to education (Nishimuko, 2008). Faith-based schools disproportionately serve poor students and rural

students (Wodon and Ying, 2009). These religious schools receive government assistance (World Bank, 2007). In Kenya, government schools are secular; indeed, some schools have recently prohibited certain items of religious clothing for girls (of State, 2012). As in the other three cases, religious education does exist in government schools, having been added to the curriculum in the 1990s (Woolman, 2001). The primary curriculum includes Christian religious knowledge and Islamic religious knowledge (Bunyi et al., 2011).

It is unlikely that the bulk of the teachers who taught the students affected by the four natural experiments I use were themselves schooled by missionaries. The exception will be the earliest case in the data (the Nigerian FPE program), which began sixteen years after Nigeria's independence. The changes I exploit in Kenya and Zimbabwe occurred during the 1980s, so that a forty-year-old teacher would have received primary schooling under colonial rule but during the post-1945 expansion of non-missionary schooling that occurred in most colonies (Cogneau and Moradi, 2014). The change in Sierra Leone comes even later.

The lack of an impact for modern education is similar to the finding in Friedman et al. (2011) that educating women does not necessarily create "modern" attitudes. One explanation of the null results I find is that we should not expect a single measure of empowerment (education) to affect a broad spectrum of outcomes without specifying a specific theory linking them. A woman's marriage and fertility decisions, for example, may be made on the basis of several distinct factors, only some of which are affected by education (e.g. Duflo et al. (2012)). Education may have conflicting effects. For example, schooling may increase a woman's distaste for polygamy but also reduce her desired fertility, increasing her husband's incentives to take an additional wife (Grossbard-Shechtman, 1986). The natural experiments that I exploit have found that additional education affected women, but have often failed to find that the characteristics of their husbands changed as a result. In Zimbabwe, Agüero and Bharadwaj (2014) do find that women exposed to additional treatment did marry more educated husbands, but that this is accounted for by the country's educational reform also affecting men. In Kenya, Chicoine (2012) finds no effect on the age of a treated woman's husband or his level of education.

A related explanation for the lack of an effect of modern schooling interventions is that the supposed link between female education and empowerment has generally been overstated due to omitted variables bias. Duflo (2012), for example, cautions that many studies on the impacts of women's education have not accounted for endogeneity; studies that do account for it may find, for example, that educating women does not improve the health of their children or has no greater effect than educating men (Breierova and Duflo, 2004; Chou et al., 2010). My results, then, are not atypical.

Further, the effects of education may be heterogeneous, only affecting marital outcomes for a subset of the population and under certain conditions. The impact of education may be limited by other conservative social norms (e.g. Field et al. (2010)), by other ethnic institutions (e.g. Ashraf et al. (2014)), or by the need for complementary changes in the incentives facing men (e.g. Gould et al. (2008, 2012)).

Another possible explanation of the negligible effects of modern schooling expansions is the low quality of schooling in contemporary Africa. Although I am able to find effects of each of the four natural experiments on literacy (not reported), increased enrollment may have reduced quality by stretching resources more thinly (Chicoine, 2012). Modern curricula in Africa are less demanding than those under colonial rule (e.g Bleck and Guindo (2013)). If an additional year of schooling does not translate into much real education, it may not be expected to have a large effects on a woman's opportunities (Pritchett, 2001).

The literature on teacher characteristics in the four countries under study shows teachers to be, on average, under-motivated and under-trained. In Nigeria, many of the students entering teacher training during the 1970s were motivated by free training and the difficulty of being dismissed (Nwagwu, 1981). In 1971, 31% of primary and secondary school teachers in Nigeria were not professionally qualified to each (Nwagwu, 1977). In Zimbabwe, secondary school teachers often lack training, skills, experience, or even formal schooling (Nyagura and Reece, 1990). In the Nyagura and Reece (1990) sample, the average age of teachers in rural day schools was under 24 years.

In Kenya, teachers are hired mostly through the Ministry of Education (Duflo et al., 2015). Hiring is also nepotistic; one third of contract teacher positions in the Duflo et al. (2015) sample were given to relatives of existing teachers. The civil service teachers in their sample were two thirds female with an average age of 42. Teacher absence is common (Duflo et al., 2011). During the 1970s, more than a third of Kenyan teachers were untrained, while today almost all are trained, though their subject knowledge is poor (Bunyi et al., 2011).

In Sierra Leone, primary school teachers are roughly 30% female (World Bank, 2007). Some 40% of primary teachers teach at a higher level than appropriate given their qualifications, partly due to the departure of qualified teachers during the war (World Bank, 2007). Many teachers are unmotivated due to low salaries (Nishimuko, 2007). Teachers in the Nishimuko (2007) sample frequently had secondary jobs, and 80% had at least five years of experience.

6. Conclusion

In this paper, I have shown that reduced polygamy rates are a legacy of colonial education in Africa, but that recent expansions of education have had no effect on polygamy rates. Women in the former districts of colonial French West Africa that received more teachers before 1928 are less likely to be polygamous today, as are women who currently reside closer to the locations of Catholic and Protestant missions in 1925. Policy changes from Nigeria, Sierra Leone, Kenya, and Zimbabwe that have created natural experiments increasing educational access for some women have not, by contrast, reduced the probability of polygamy for these same women.

These results suggest that ethnic institutions are shaped by history, as are family structures in developing countries. Knowing this, however, does not translate easily into policy. Increasing education in the present is not a perfect substitute for a long history of education, and both the content and quality of education in present-day Africa differ from what was provided by colonial schools and missions. Similarly, education is not a sufficient statistic

for female empowerment; the effects of expanding educational access to women are heterogeneous across women and vary by the outcome measured. The effects of education on fertility and polygamy need not move in the same direction.

Of course, this study does have limitations. There is little data available on heterogeneity across missions or colonial schools that could be used to better illuminate the mechanisms behind their long-run effects. Similarly, I am not aware of other data that would allow the legacies of non-missionary colonial education to be estimated for the rest of Africa. The natural experiments from the modern period are, by construction, very specific events. Each reform differed in which parts of the schooling distribution were shifted. Each comes from a former British colony. All four have yielded similar results, but future research will be needed to demonstrate whether the response of polygamy to education is the same after other educational reforms in other contexts.

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Table 1. Summary statistics

	Mean	s.d.	Min.	Max.	N
Main controls					
Polygamous	0.28	0.45	0	1	494,157
Resp. Education Years	3.27	4.06	0	26	493,829
Age	30.8	8.70	10	64	494,157
Urban	0.30	0.46	0	1	494,157
Year of birth	1,969	10.5	1,936	1,994	494,157
Religion: Animist	0.0071	0.084	0	1	494,157
Religion: Catholic	0.17	0.37	0	1	494,157
Religion: Christian (Other)	0.025	0.16	0	1	494,157
Religion: Missing	0.093	0.29	0	1	494,157
Religion: None	0.044	0.21	0	1	494,157
Religion: Orthodox	0.016	0.12	0	1	494,157
Religion: Other	0.096	0.29	0	1	494,157
Religion: Protestant	0.19	0.39	0	1	494,157
Religion: Spiritual	0.0036	0.060	0	1	494,157
Religion: Traditional	0.025	0.16	0	1	494,157
Religion: Muslim (excluded)	0.34	0.47	0	1	494,157
Ecological zones					
Woodland	0.22	0.42	0	1	301,183
Forest	0.061	0.24	0	1	301,183
Mosaics	0.15	0.35	0	1	301,183
Cropland	0.12	0.33	0	1	301,183
Intensive cropland	0.0011	0.033	0	1	301,183
Wetland	0.0084	0.091	0	1	301,183
Desert/Bare	0.034	0.18	0	1	301,183
Water/Coastal fringe	0.041	0.20	0	1	301,183
Urban	0.0070	0.083	0	1	301,183
Other GIS controls					
Malaria	0.38	0.19	0	0.75	301,183
Elevation	165	10.3	140	195	301,183
Ruggedness	64,238	102,520	0	1.37e+06	301,183
Distance to coast	463	358	0.013	1,771	301,183
Abs. latitude	10.7	5.18	0.0015	28.7	301,183
Rainfed ag. suit.	3.95	2.08	1	8	301,183
Distance to Catholic Mission	0.21	0.20	0.00015	1.17	301,183
Distance to Protestant Mission	0.17	0.19	0.00010	1.22	301,183

Table 2. Colonial education in French West Africa

	Dependent variable: Polygamous					
	(1)	(2)	(3)	(4)	(5)	(6)
Teachers/capita, 1910-1928	-7.227***	-5.148***	-9.192***	-9.206***	-4.163**	-5.314***
	(1.314)	(1.820)	(2.245)	(3.234)	(1.972)	(1.017)
Observations	103,432	103,432	103,432	103,432	103,432	103,432
AET	N/A	2.476	-4.676	-4.652	1.359	2.779
Other controls	None	Attractiveness	Conquest	Precolonial	H-Geographic	Country-Round FE
Clustering	District 1925	District 1925	District 1925	District 1925	District 1925	District 1925
	(7)	(8)	(9)	(10)	(11)	(12)
Ln (Teachers/capita, 1910-1928)	-0.024***	-0.031***	-0.038***	-0.033***	-0.030***	-0.022***
	(0.006)	(0.008)	(0.006)	(0.007)	(0.007)	(0.006)
Observations	103,432	103,432	103,432	103,432	103,432	103,432
AET	N/A	-4.430	-2.694	-3.637	-4.752	14.94
Other controls	None	Attractiveness	Conquest	Precolonial	H-Geographic	Country-Round FE
Clustering	District 1925	District 1925	District 1925	District 1925	District 1925	District 1925

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using OLS. Attractiveness controls are trade taxes in 1914. Conquest controls are date of conquest, length of resistence and its square, and indemnities in 1910. Precolonial controls are the presence of an ancient state, the presence of a European trade counter, and 1925 population density. H-Geographic controls are latitude, longitude, altitude, dummies for the river and coast, and average rainfall from 1915 to 1975.

Table 3. Missions

	Dependent variable: Years of Education			
	(1)	(2)	(3)	(4)
Ln Distance from Catholic Mission	-0.419***	-0.913***	-0.293***	-0.307***
	(0.016)	(0.018)	(0.018)	(0.022)
Observations	301,002	301,002	301,002	301,002
AET	N/A	-1.848	2.336	2.747
	Dependent variable: Years of Education			
	(5)	(6)	(7)	(8)
Ln Distance from Protestant Mission	-0.709***	-0.893***	-0.394***	-0.399***
	(0.014)	(0.015)	(0.016)	(0.021)
Observations	301,002	301,002	301,002	301,002
AET	N/A	-4.860	1.248	1.289
	Dependent variable: Polygamous			
	(9)	(10)	(11)	(12)
Ln Distance from Catholic Mission	0.024***	0.032***	0.017***	0.015***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-4.053	2.277	1.801
	Dependent variable: Polygamous			
	(13)	(14)	(15)	(16)
Ln Distance from Protestant Mission	0.027***	0.028***	0.013***	0.014***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-35.08	0.945	1.125
Other controls	Geo./Ind.	None	Geo./Ind.	Geo./Ind.
Fixed Effects	None	Cntry-rnd	Cntry-rnd	Region
Clustering	Cluster	Cluster	Cluster	Cluster

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using OLS. Geographic controls (Geo.) are absolute latitude, suitability for rain-fed agriculture, malaria endemism, ruggedness, elevation, distance to coast, and ecological zone. Individual controls (Ind.) are year of birth, year of birth squared, age, age squared, and urban. Religious dummies are not included in the controls.

Table 4. The conditional correlation between education and polygamy

	Dependent variable: Polygamous				
Full sample	(1)	(2)	(3)	(4)	(5)
Resp. Education Years	-0.024***	-0.016***	-0.011***	-0.010***	-0.006***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	493,829	493,829	493,829	493,829	493,829
Other controls	None	Indiv.	Indiv.	Indiv.	Indiv.
	Dependent variable: Polygamous				
Sample with geographic controls	(6)	(7)	(8)	(9)	(10)
Resp. Education Years	-0.022***	-0.013***	-0.011***	-0.010***	-0.007***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	301,002	301,002	301,002	301,002	301,002
Other controls	Geog.	Geog./Indiv.	Geog./Indiv.	Geog./Indiv.	Indiv.
	Dependent variable: Polygamous				
Full sample	(11)	(12)	(13)	(14)	(15)
Any Education	-0.201***	-0.121***	-0.070***	-0.059***	-0.032***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	493,829	493,829	493,829	493,829	493,829
Other controls	None	Indiv.	Indiv.	Indiv.	Indiv.
	Dependent variable: Polygamous				
Sample with geographic controls	(16)	(17)	(18)	(19)	(20)
Any Education	-0.188***	-0.098***	-0.070***	-0.062***	-0.034***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Observations	301,002	301,002	301,002	301,002	301,002
Other controls	Geog.	Geog./Indiv.	Geog./Indiv.	Geog./Indiv.	Indiv.
Fixed Effects	None	None	Cntry-rnd	Region	Cluster
Clustering	Cluster	Cluster	Cluster	Cluster	Cluster

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using OLS. Geographic controls (Geog.) are absolute latitude, suitability for rain-fed agriculture, malaria endemism, ruggedness, elevation, distance to coast, and ecological zone. Individual controls (Indiv.) are year of birth, year of birth squared, religious dummies, age, age squared, and urban.

Table 5A. Parental education in three West African samples

	GLSS				
	Means	*Dependent variable: Polygamous*			
	(1)	(2)	(3)	(4)	(5)
Own education					
MSLC	0.13	-0.014			-0.012
		(0.016)			(0.016)
Other	0.036	-0.002			-0.003
Mother's education		(0.024)			(0.026)
MSLC	0.019		0.020		0.030
			(0.034)		(0.035)
Other	0.0081		0.036		0.040
Father's education			(0.067)		(0.069)
MSLC	0.11			-0.021	-0.022
				(0.015)	(0.015)
Other	0.030			-0.002	-0.010
				(0.029)	(0.029)
Observations		3,821	3,821	3,821	3,821
Controls and F.E.		Yes	Yes	Yes	Yes
Clustering		Cluster	Cluster	Cluster	Cluster

	ICLSS				
	Means	*Dependent variable: Polygamous*			
	(6)	(7)	(8)	(9)	(10)
Own education					
CEPE	0.047	-0.120***			-0.112***
		(0.026)			(0.026)
Other	0.038	-0.149***			-0.134***
Mother's education		(0.030)			(0.030)
Any	0.0058		-0.053		0.021
Father's education			(0.055)		(0.053)
Any	0.046			-0.096***	-0.060**
				(0.024)	(0.024)
Observations		6,924	6,786	6,815	6,785
Controls and F.E.		Yes	Yes	Yes	Yes
Clustering		Cluster	Cluster	Cluster	Cluster

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Other controls in the GLSS sample are dummies for religion, year of birth, survey round, and survey cluster. Other controls in the ICLSS sample are dummies for religion of husband, year of birth, survey round, and survey cluster. Other controls in the NLSS sample are dummies for urban, religion, year of birth and Local Government Area (LGA). In all specifications, "none" is the excluded education category.

Table 5B. Parental education in three West African samples

	NLSS				
	Means	*Dependent variable: Polygamous*			
	(1)	(2)	(3)	(4)	(5)
Own education					
Other	0.0059	-0.073			-0.074
		(0.122)			(0.130)
Post-Secondary	0.071	-0.036			-0.038
		(0.104)			(0.107)
Primary/Some Primary	0.41	-0.023			-0.031
		(0.097)			(0.100)
Quaranic	0.15	0.015			0.007
		(0.096)			(0.101)
Secondary/Some Secondary	0.36	-0.045			-0.047
Mother's education		(0.099)			(0.102)
Other	0.034		-0.027		0.021
			(0.037)		(0.038)
Primary/Some Primary	0.094		-0.017		-0.000
			(0.022)		(0.021)
Quaranic	0.20		-0.046		-0.040
Father's education			(0.037)		(0.052)
Other	0.066			-0.076***	-0.065**
				(0.027)	(0.029)
Primary/Some Primary	0.13			-0.011	-0.021
				(0.021)	(0.021)
Quaranic	0.22			-0.032	-0.062
				(0.032)	(0.053)
Observations		2,727	4,828	4,851	2,637
Controls and F.E.		Yes	Yes	Yes	Yes
Clustering		LGA	LGA	LGA	LGA

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Other controls in the GLSS sample are dummies for religion, year of birth, survey round, and survey cluster. Other controls in the ICLSS sample are dummies for religion of husband, year of birth, survey round, and survey cluster. Other controls in the NLSS sample are dummies for urban, religion, year of birth and Local Government Area (LGA). In all specifications, "none" is the excluded education category.

Table 6. UPE in Nigeria

	Dep. Var.: Years of Education			*Dep. Var.: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1970-75 X Intensity	0.933**	0.008**	-0.041	0.026	-0.000	-0.011
	(0.411)	(0.003)	(0.180)	(0.037)	(0.000)	(0.008)
Born 1970-75	-0.216	-0.225	0.664	-0.010	0.016	0.029
	(0.409)	(0.391)	(0.468)	(0.035)	(0.032)	(0.026)
Intensity	-1.903	-0.003	0.609***	-0.010	-0.000	-0.018**
	(1.151)	(0.005)	(0.152)	(0.066)	(0.000)	(0.008)
Observations	15,166	15,166	15,166	15,179	15,179	15,179
Measure of intensity	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.
Other controls	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long
Clustering	1976 State	1976 State	1976 State	1976 State	1976 State	1976 State

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Osili/Long controls are year of birth, and dummies for the three largest Nigerian ethnic groups (Yoruba, Hausa, Igbo), and the major religions (Muslim, Catholic, Protestant, other Christian, and traditional).

Table 7. Zimbabwe

	Dep. Var.: Years of Education (OLS)			*Dep. Var.: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
Age 15 or less in 1980	1.167***	1.054***	1.172***	0.008	-0.000	-0.005
	(0.198)	(0.211)	(0.245)	(0.020)	(0.023)	(0.027)
Observations	6,268	5,260	4,460	6,272	5,264	4,464
Ages in 1980	8 to 21	9 to 20	10 to 19	8 to 21	9 to 20	10 to 19
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region

	Dep. Var.: Polygamous (IV)		
	(7)	(8)	(9)
Years of education	0.006	-0.001	-0.005
	(0.017)	(0.021)	(0.023)
Observations	6,268	5,260	4,460
Ages in 1980	8 to 21	9 to 20	10 to 19
Other controls	Yes	Yes	Yes
Clustering	Cohort X Region	Cohort X Region	Cohort X Region
KP-F	35.18	22.85	21.39

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results are estimated using OLS. Other controls are urban, dummies for region, and dummies for DHS round, and differential trends in Age in 1980 on either side of the cutoff.

Table 8. Free Primary Education (FPE) in Sierra Leone

	OLS					
	Dep. Var: Schooling			*Dep. Var: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
FPE Cohort	0.523**			-0.045		
	(0.239)			(0.050)		
FPE X R		0.705***	0.654***		-0.002	-0.019
		(0.204)	(0.238)		(0.073)	(0.074)
Observations	2,659	2,659	2,721	1,613	1,613	1,648
Other controls	Yes	Yes	No	Yes	Yes	No
Fixed Effects	District	District + Y.O.B.	District + Y.O.B.	District	District + Y.O.B.	District + Y.O.B.
Clustering	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District

	IV		
	Dep. Var: Polygamous		
	(7)	(8)	(9)
Years of schooling	-0.105	-0.002	-0.020
	(0.115)	(0.093)	(0.077)
Observations	1,611	1,611	1,646
Other controls	Yes	Yes	No
Fixed Effects	District	District + Y.O.B.	District + Y.O.B.
Clustering	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District
KP Wald	3.937	6.821	9.044

* p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses are clustered by birth-year X district. The FPE cohort consists is those born between 1990 and 1993. The sample is women born between 1980 and 1985, and between 1990 and 1993. R is the logarithm of district-level primary school funding in 2004, divided by the number of teachers and multiplied by 100. Regressions are weighted using weights from the DHS data. Other controls are dummies for Christian, Temne, Mende, Urban, Married, Employed, Radio, Fridge, TV, and Wealth Quintiles.

Table 9. Kenya

	Dep. Var.: Years of Education	*Dep. Var.: Polygamous*	
	(1)	(2)	(3)
Chicoine Instrument	0.657***	0.016	
	(0.175)	(0.016)	
Years of education			0.032
			(0.034)
Observations	29,270	19,611	19,603
Estimator	OLS	OLS	IV
Sample	Born 1950 to 1980	Born 1950 to 1980	Born 1950 to 1980
KP-F			7.944
Other controls	Yes	Yes	Yes
Clustering	Year of birth	Year of birth	Year of birth

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Other controls are quadratic in cohort, a cubic in age, dummies for ethnicity, dummies for quarter of birth, and dummies for childhood region of residence.

Online appendix. Not for publication.

Appendix A. Data appendix

A.1. **Principal DHS data.** The principal data source for this study is the Demographic and Health Survey (DHS) data, downloaded from `http://www.measuredhs.com/`. The main data are the "individual recodes," which are nationally representative samples of ever-married women between the ages of 15 and 64 for each country.

I use 90 of these datasets for the analysis. These include data from 34 countries. The full list of datasets used is reported in Table A2.

The data include 494,157 observations for which data on polygamy are non-missing. Of these observations, latitude/longitude coordinates are available for 301,183 in separate files similarly downloaded from the DHS website. These coordinates are used to join the data to the other variables listed below. Variables used from the DHS data are:

Respondent's year of birth is the difference between the survey year (v007) and the respondent's age (v012). The survey year required cleaning: adding 1900 if it was less than 100, recoding 1900 as 2000, 1901 as 2001, and, for Ethiopia, recoding 1992 as 2000 and 1997 as 2000.

Urban is an indicator for whether v025 is "urban." If v025 was not asked, v102 was used.

Polygamous is an indicator for whether v505 is greater than 0 if v505 is not missing and is not 98.

Years of schooling is variable v133.

Religion is variable v130.

A.2. **FAO-GAEZ data.** All of these data are downloaded from `http://www.iiasa.ac.at/Research/LUC/GAEZ/index.htm`. The variables that I create are:

Suitability for rain-fed agriculture is plate 46 from the FAO GAEZ data, and measures the suitability for rain-fed agriculture of any type.

Constraints on rain-fed agriculture is plate 28 from the FAO GAEZ data, and measures the sum of soil terrain, slope, and climate constraints on rain-fed agriculture.

Ecological zone dummies are computed from plate 55 of the FAO GAEZ data.

A.3. **Other geographic controls.** *Absolute latitude* is the absolute value of the survey cluster's latitude, as reported in the DHS data.

Distance to coast is calculated using the globdist command in Stata, after merging the survey clusters with a pixelated map of the African coast.

Elevation is average elevation for the ethnic group. Raster data are available at `http://www.naturalearthdata.com/`, and are provided by the North American Cartographic Information Society.

Ruggedness is a measure of terrain ruggedness used by Nunn and Puga (2012). This measures the elevation distance between a raster cell and its neighbors at a fine level. The data are downloaded from `http://diegopuga.org/data/`.

Malaria endemism is an index of the degree to which malaria is endemic, according to the Malaria Atlas Project, downloaded from `http://www.map.ox.ac.uk/data/`.

A.4. **Ethnographic variables.** The *Ethnographic Atlas* can be downloaded from:

`eclectic.ss.uci.edu/~drwhite/worldcul/`

`EthnographicAtlasWCRevisedByWorldCultures.sav`.

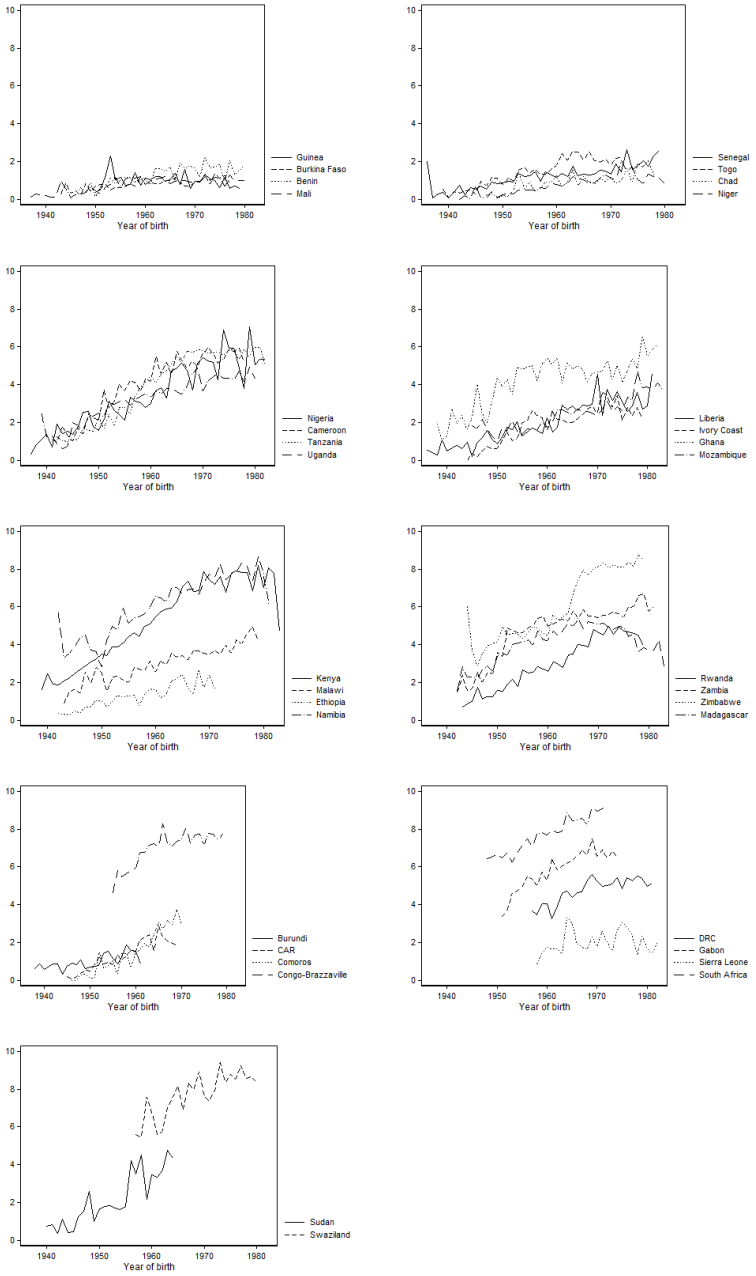
There are several reasons why not all observations could be matched between the DHS data and this source. Ethnicity is only reported for a sub-sample of the DHS data, and some of these entries contain uninformative responses such as "other" or "African." Not all ethnic groups in Africa have entries in the *Ethnographic Atlas*, and not all ethnic groups with entries in the *Ethnographic Atlas* have data on the historic gender division of labor or historic class stratification.

Usual polygyny records whether V9 is 3, 4, 5, or 6.

A.5. **Data taken from other papers.** Three measures of program intensity are taken directly from the text of Osili and Long (2008) – a dummy variable for a "high intensity" state, school-building funds in 1976 divided by the 1953 census population estimates, and school-building funds normalized by 1976 population projections based on the (unreliable) 1963 census.

These variables used to replicate Huillery (2009) are downloaded from `http://econ.sciences-po.fr/elise-huillery`. Because population is not available every year, the denominator for the per capita measures is population in 1925.

FIGURE 10. Predicted education over time for women aged 30, by year of birth



Time trends show mean education by year of birth, for women aged above 25.

APPENDIX B. MEAN EDUCATION OVER TIME FOR WOMEN ABOVE 25 YEARS OLD, BY YEAR OF BIRTH

Appendix C. Mean polygamy by age, survey round, and country

Figure 11. Means and lowess trends, Part 1

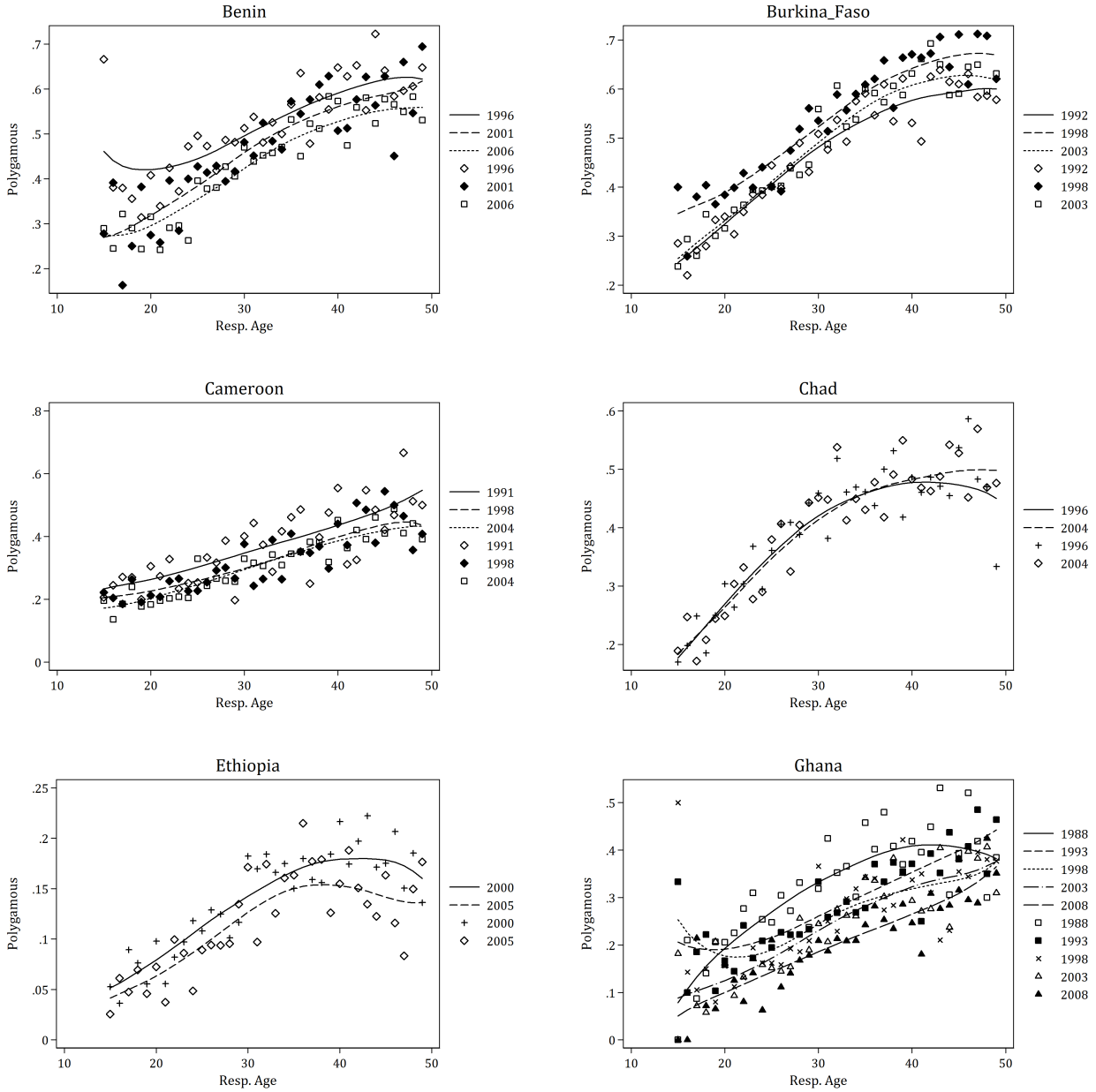


FIGURE 12. Means and lowess trends, Part 2

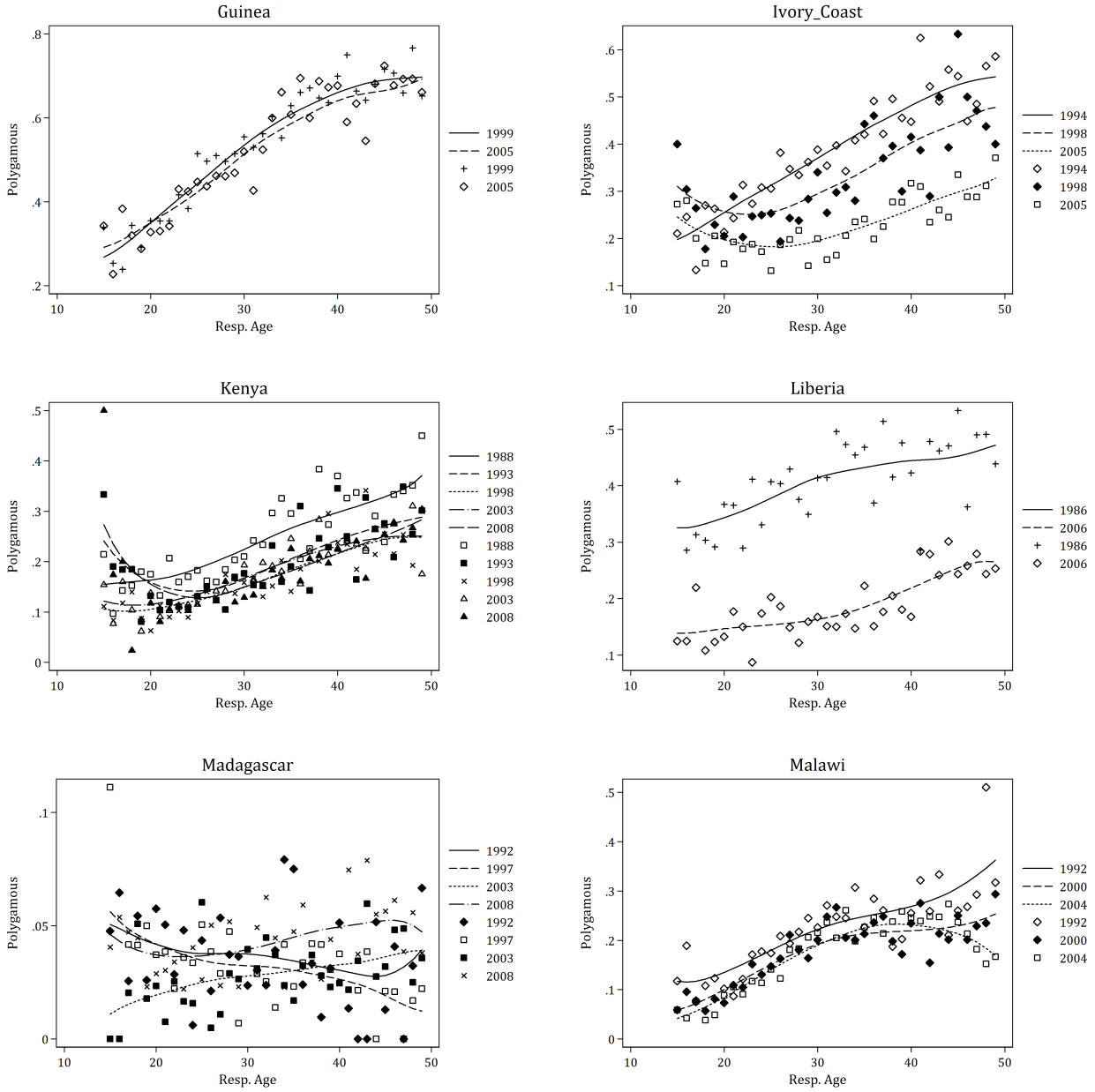


FIGURE 13. Means and lowess trends, Part 3

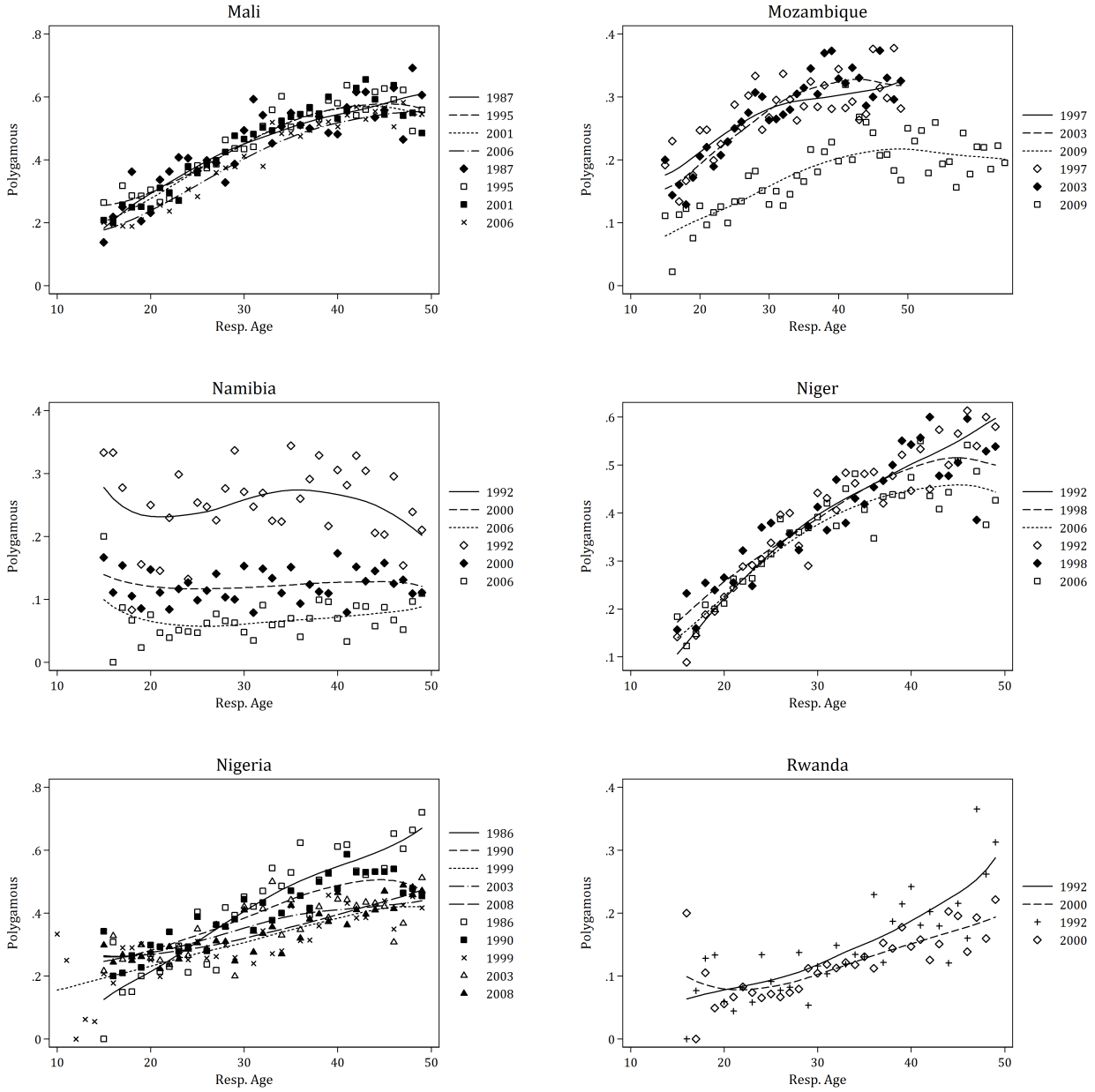


FIGURE 14. Means and lowess trends, Part 4

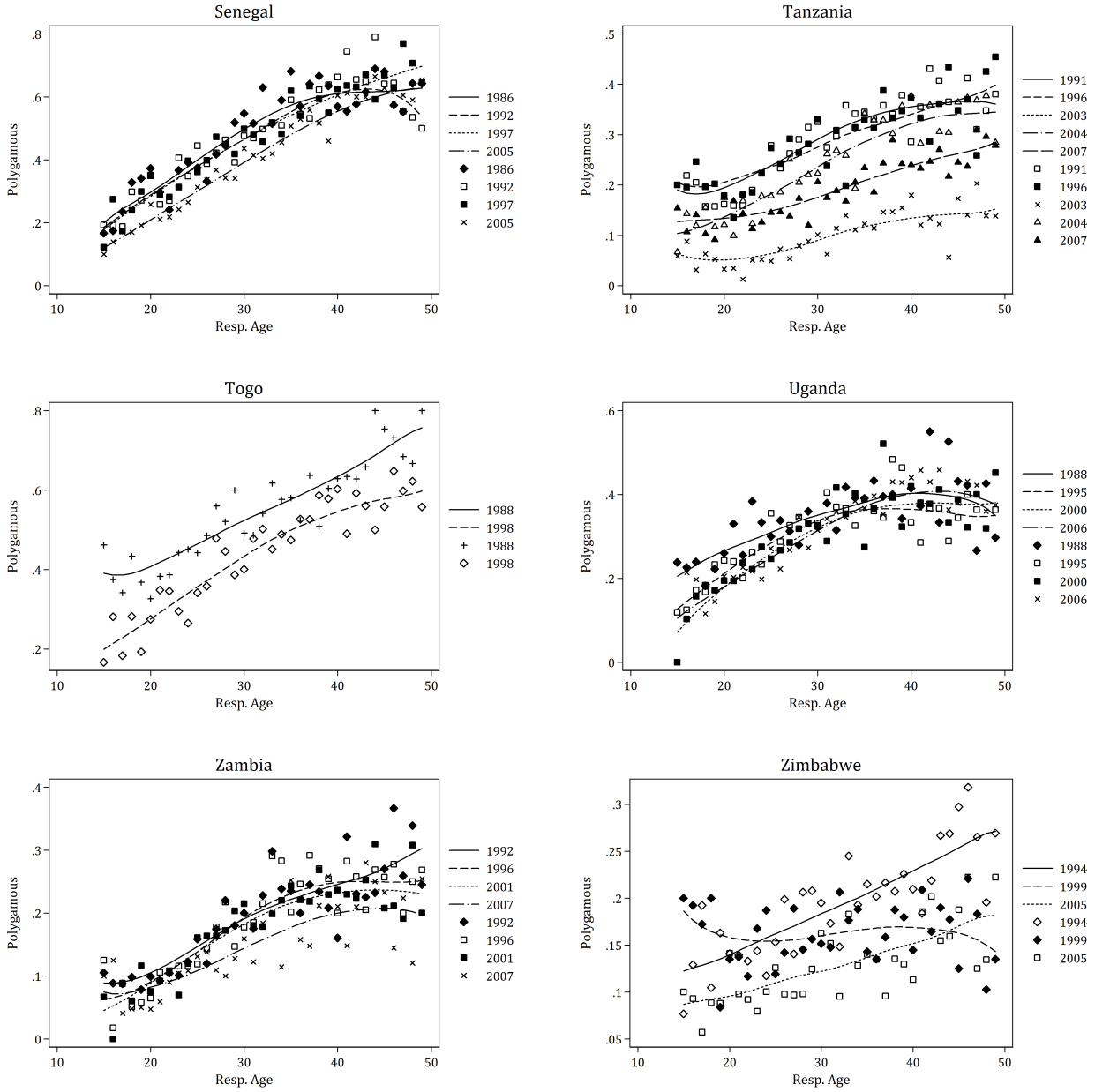


Table A1. The distribution of polygamy

	Husband's number of wives		*Respondent's rank (if polygamous)*	
	Freq	Percent	Freq	Percent
	(1)	(2)	(3)	(4)
1	354,019	(71.98)	49,636	(40.49)
2	92,003	(18.71)	58,680	(47.87)
3	34,272	(6.97)	10,959	(8.94)
4	8,428	(1.71)	2,469	(2.01)
5	1,980	(0.40)	488	(0.40)
6	578	(0.12)	181	(0.15)
7	205	(0.04)	67	(0.05)
8	131	(0.03)	38	(0.03)
9	70	(0.01)	29	(0.02)
10	47	(0.01)	22	(0.02)
Total	491,850		122,586	

Notes: Values above 10 not reported

Table A2. List of individual recodes

Country	*First survey year*	*Dataset*	*Country*	*First survey year*	*Dataset*
Benin	1996	BJIR31FL.DTA	Mozambique	1997	MZIR31FL.DTA
Benin	2001	BJIR41FL.DTA	Mozambique	2003	MZIR41FL.DTA
Benin	2006	BJIR51FL.DTA	Mozambique	2009	MZIR51FL.DTA
Burkina Faso	1992	BFIR21FL.DTA	Namibia	1992	NMIR21FL.DTA
Burkina Faso	1998	BFIR31FL.DTA	Namibia	2000	NMIR41FL.DTA
Burkina Faso	2003	BFIR43FL.DTA	Namibia	2006	NMIR51FL.DTA
Burundi	1987	BUIR01FL.DTA	Niger	1992	NIIR22FL.DTA
CAR	1994	CFIR31FL.DTA	Niger	1998	NIIR31FL.DTA
Cameroon	1991	CMIR22FL.DTA	Niger	2006	NIIR51FL.DTA
Cameroon	1998	CMIR31FL.DTA	Nigeria	1986	OSIR01FL.DTA
Cameroon	2004	CMIR44FL.DTA	Nigeria	1990	NGIR21FL.DTA
Chad	1996	TDIR31FL.DTA	Nigeria	1999	NGIR41FL.DTA
Chad	2004	TDIR41FL.DTA	Nigeria	2003	NGIR4BFL.DTA
Comoros	1996	KMIR32FL.DTA	Nigeria	2008	NGIR51FL.DTA
Congo-Brazzaville	2005	CGIR51FL.DTA	Rwanda	1992	RWIR21FL.DTA
DRC	2007	CDIR50FL.DTA	Rwanda	2000	RWIR41FL.DTA
Ethiopia	2000	ETIR41FL.DTA	Rwanda	2005	RWIR53FL.DTA
Ethiopia	2005	ETIR51FL.DTA	Senegal	1986	SNIR02FL.DTA
Gabon	2000	GAIR41FL.DTA	Senegal	1992	SNIR21FL.DTA
Ghana	1988	GHIR02FL.DTA	Senegal	1997	SNIR32FL.DTA
Ghana	1993	GHIR31FL.DTA	Senegal	2005	SNIR4HFL.DTA
Ghana	1998	GHIR41FL.DTA	Sierra Leone	2008	SLIR51FL.DTA
Ghana	2003	GHIR4AFL.DTA	South Africa	1998	ZAIR31FL.DTA
Ghana	2008	GHIR5HFL.DTA	Sudan	1989	SDIR02FL.DTA
Guinea	1999	GNIR41FL.DTA	Swaziland	2006	SZIR51FL.DTA
Guinea	2005	GNIR52FL.DTA	Tanzania	1991	TZIR21FL.DTA
Ivory Coast	1994	CIIR35FL.DTA	Tanzania	1996	TZIR3AFL.DTA
Ivory Coast	1998	CIIR3AFL.DTA	Tanzania	2003	TZIR4AFL.DTA
Ivory Coast	2005	CIIR50FL.DTA	Tanzania	2004	TZIR4IFL.DTA
Kenya	1988	KEIR03FL.DTA	Tanzania	2007	TZIR51FL.DTA
Kenya	1993	KEIR33FL.DTA	Togo	1988	TGIR01FL.DTA
Kenya	1998	KEIR3AFL.DTA	Togo	1998	TGIR31FL.DTA
Kenya	2003	KEIR41FL.DTA	Uganda	1988	UGIR01FL.DTA
Kenya	2008	KEIR51FL.DTA	Uganda	1995	UGIR33FL.DTA
Liberia	1986	LBIR01FL.DTA	Uganda	2000	UGIR41FL.DTA
Liberia	2006	LBIR51FL.DTA	Uganda	2006	UGIR51FL.DTA
Madagascar	1992	MDIR21FL.DTA	Zambia	1992	ZMIR21FL.DTA
Madagascar	1997	MDIR31FL.DTA	Zambia	1996	ZMIR31FL.DTA
Madagascar	2003	MDIR41FL.DTA	Zambia	2001	ZMIR42FL.DTA
Madagascar	2008	MDIR51FL.DTA	Zambia	2007	ZMIR51FL.DTA
Malawi	1992	MWIR22FL.DTA	Zimbabwe	1994	ZWIR31FL.DTA
Malawi	2000	MWIR41FL.DTA	Zimbabwe	1999	ZWIR42FL.DTA
Malawi	2004	MWIR4CFL.DTA	Zimbabwe	2005	ZWIR51FL.DTA
Mali	1987	MLIR01FL.DTA			
Mali	1995	MLIR32FL.DTA			
Mali	2001	MLIR41FL.DTA			
Mali	2006	MLIR52FL.DTA			

Table A3. Robustness: Colonial education in French West Africa

	Dependent variable: Polygamous				
Panel A	(1)	(2)	(3)	(4)	(5)
Teachers/capita, 1910-1928	-4.792**	-4.424**	-4.108**	-7.745***	-7.569***
	(2.023)	(1.809)	(1.635)	(1.487)	(1.431)
Observations	103,432	103,432	103,432	103,432	103,432
Square Size	1 X 1	2 X 2	3 X 3	4 X 4	5 X 5
Other controls	None	None	None	None	None
Clustering	District 1925	District 1925	District 1925	District 1925	District 1925
	(6)	(7)	(8)	(9)	(10)
Ln (Teachers/capita, 1910-1928)	-0.030**	-0.035***	-0.024***	-0.031***	-0.037***
	(0.012)	(0.009)	(0.008)	(0.005)	(0.007)
Observations	103,432	103,432	103,432	103,432	103,432
Square Size	1 X 1	2 X 2	3 X 3	4 X 4	5 X 5
Other controls	None	None	None	None	None
Clustering	District 1925	District 1925	District 1925	District 1925	District 1925

	Dependent variable: Ln (Teachers/capita, 1910-1928)			
Panel B	(11)	(12)	(13)	(14)
Beginning of the colonial conquest		-0.369***		
		(0.118)		
Number of years of resistance		-0.532*		
		(0.276)		
Duration of resistance sqrd.		0.329		
		(0.230)		
Indemnities 1910		0.120*		
		(0.064)		
Trade taxes 1914	0.587***			
	(0.133)			
Kingdom			-0.051	
			(0.075)	
Trading post			0.528***	
			(0.078)	
Pop. density 1910			0.162**	
			(0.072)	
Latitude of district's main city				-0.460***
				(0.125)
Longitude of district's main city				-0.160
				(0.109)
Indicator of access to the sea				0.570***
				(0.112)
Indicator of a navigable river				-0.080
				(0.081)
Altitude				-0.043
				(0.080)
Rainfall				-0.581***
				(0.146)
Observations	108	108	108	108
R-Squared	0.345	0.197	0.411	0.480

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using OLS. Panel A: This is estimated at the individual level and includes fixed effects for the latitude-longitude squares indicated in each column. Panel B: This is estimated at the district level, and standardized coefficients are reported. Robust standard errors are used in Panel B.

Table A4. Robustness: Missions conditional on respondent religion

	Dependent variable: Polygamous			
Panel A. Add religion dummies	(1)	(2)	(3)	(4)
Ln Distance from Catholic Mission	0.016***	0.032***	0.014***	0.015***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-2.000	9.040	14.74
	Dependent variable: Polygamous			
	(5)	(6)	(7)	(8)
Ln Distance from Protestant Mission	0.012***	0.028***	0.008***	0.012***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-1.723	2.487	-16.81
	Dependent variable: Polygamous			
Panel A. Add historical controls	(9)	(10)	(11)	(12)
Ln Distance from Catholic Mission	0.021***	0.032***	0.016***	0.016***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	300,242	301,183	300,242	300,242
AET	N/A	-3.081	2.885	2.809
	Dependent variable: Polygamous			
	(13)	(14)	(15)	(16)
Ln Distance from Protestant Mission	0.024***	0.028***	0.011***	0.015***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	300,242	301,183	300,242	300,242
AET	N/A	-7.309	0.870	1.543
Other controls	Geo./Ind.	None	Geo./Ind.	Geo./Ind.
Fixed Effects	None	Cntry-rnd	Cntry-rnd	Region
Clustering	Cluster	Cluster	Cluster	Cluster

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using ordinary least squares. Geographic controls (Geo.) are absolute latitude, suitability for rain-fed agriculture, malaria endemism, ruggedness, elevation, distance to coast, and ecological zone. Individual controls (Ind.) are year of birth, year of birth squared, age, age squared, and urban. Religious dummies are also included in the controls.

Table A5. Robustness: Missions with distance bands and matching

Panel A. Full Sample	*Dependent variable: Polygamous*			
	(1)	(2)	(3)	(4)
Catholic Mission within 20km	-0.053***	-0.073***	-0.029***	-0.024***
	(0.005)	(0.004)	(0.004)	(0.004)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-3.750	1.219	0.797
	Dependent variable: Polygamous			
	(5)	(6)	(7)	(8)
Protestant Mission within 20km	-0.076***	-0.093***	-0.048***	-0.038***
	(0.004)	(0.003)	(0.004)	(0.004)
Observations	301,183	301,183	301,183	301,183
AET	N/A	-5.332	1.737	0.989
Panel B. Within 40km	*Dependent variable: Polygamous*			
	(9)	(10)	(11)	(12)
Catholic Mission within 20km	-0.018***	-0.028***	-0.010**	-0.013***
	(0.006)	(0.005)	(0.005)	(0.005)
Observations	62,680	62,680	62,680	62,680
AET	N/A	-2.708	1.494	3.118
	Dependent variable: Polygamous			
	(13)	(14)	(15)	(16)
Protestant Mission within 20km	-0.020***	-0.057***	-0.025***	-0.025***
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	100,098	100,098	100,098	100,098
AET	N/A	-1.524	-4.945	-4.500
Other controls	Geo./Ind.	None	Geo./Ind.	Geo./Ind.
Fixed Effects	None	Cntry-rnd	Cntry-rnd	Region
	Dependent variable: Polygamous			
Panel C. Full Sample	(17)	(18)	(19)	(20)
Ln Distance from Catholic Mission	0.009***	0.018***	0.015***	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	301,183	301,183	301,183	301,183
Fixed Effects	1X1	2X2	3X3	4X4
	Dependent variable: Polygamous			
	(21)	(22)	(23)	(24)
Ln Distance from Protestant Mission	0.013***	0.015***	0.015***	0.015***
	(0.002)	(0.002)	(0.002)	(0.001)
Observations	301,183	301,183	301,183	301,183
Fixed Effects	1X1	2X2	3X3	4X4

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using ordinary least squares. Geographic controls (Geo.) are absolute latitude, suitability for rain-fed agriculture, malaria endemism, ruggedness, elevation, distance to coast, and ecological zone. Individual controls (Ind.) are year of birth, year of birth squared, age, age squared, and urban. Religious dummies are not included in the controls.

Table A6. Robustness: Missions and pre-colonial polygamy

	Dependent variable: Catholic Missions		*Dependent variable: Catholic Missions per unit area*	
	(1)	(2)	(3)	(4)
Any polygamy	0.515***	0.174	0.311**	-0.075
	(0.150)	(0.142)	(0.136)	(0.147)
	Dependent variable: Protestant Missions		*Dependent variable: Protestant Missions per unit area*	
	(5)	(6)	(7)	(8)
Any polygamy	1.382***	1.027	0.869*	0.089
	(0.391)	(0.714)	(0.429)	(0.680)
	Dependent variable: Catholic Missions		*Dependent variable: Catholic Missions per unit area*	
	(9)	(10)	(11)	(12)
Usual polygamy	0.153	-0.058	0.360***	0.154
	(0.257)	(0.282)	(0.115)	(0.160)
	Dependent variable: Protestant Missions		*Dependent variable: Protestant Missions per unit area*	
	(13)	(14)	(15)	(16)
Usual polygamy	-0.405	-0.296	0.500	0.241
	(0.697)	(0.424)	(0.375)	(0.406)
Observations	517	517	517	517
Controls	No	Yes	No	Yes
Clustering	Region	Region	Region	Region

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. All results estimated using ordinary least squares. Controls are constraints on agriculture, distance from coast, elevation, malaria prevalence, precipitation, temperature, distance from Lake Victoria, date of observation, crop dummies, river, ruggedness, and slave trade distances.

Table A7. Robustness: UPE in Nigeria. State trends and instrumental variables

	Panel A: OLS with state trends					
	Dep. Var.: Years of Education			*Dep. Var.: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1970-75 X Intensity	1.288*	0.014**	-0.021	0.009	0.000	0.034*
	(0.615)	(0.005)	(0.221)	(0.045)	(0.000)	(0.017)
Born 1970-75	-0.357	-0.545	0.729	-0.011	-0.030	-0.069**
	(0.465)	(0.441)	(0.578)	(0.033)	(0.034)	(0.029)
Intensity	-35.295	1.638**	54.712	0.000	0.007	5.551**
	(87.174)	(0.736)	(32.627)	(0.000)	(0.060)	(1.984)
Observations	15,166	15,166	15,166	15,179	15,179	15,179
Measure of intensity	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.
Other controls	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long
Fixed Effects	None	None	None	None	None	None
Clustering	1976 State	1976 State	1976 State	1976 State	1976 State	1976 State

	Panel B: IV		
	Dep. Var.: Polygamous		
	(7)	(8)	(9)
Years of education	0.027	-0.004	0.249
	(0.038)	(0.028)	(1.040)
Born 1970-75	-0.002	0.017	-0.136
	(0.030)	(0.027)	(0.636)
Intensity	0.043	-0.000	-0.170
	(0.107)	(0.000)	(0.598)
Observations	15,166	15,166	15,166
Cragg-Donald F	22.92	35.76	0.653
Measure of intensity	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.
Other controls	Osili/Long	Osili/Long	Osili/Long
Fixed Effects	None	None	None
Clustering	1976 State	1976 State	1976 State

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Osili/Long controls are year of birth, and dummies for the three largest Nigerian ethnic groups (Yoruba, Hausa, Igbo), and the major religions (Muslim, Catholic, Protestant, other Christian, and traditional).

Table A8. Robustness: UPE in Nigeria. No Lagos.

	Dep. Var.: Years of Education			*Dep. Var.: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1970-75 X Intensity	0.419	0.006**	0.456***	0.022	-0.000	-0.005
	(0.367)	(0.003)	(0.152)	(0.060)	(0.000)	(0.015)
Born 1970-75	0.235	0.025	-0.100	-0.011	0.010	0.015
	(0.412)	(0.334)	(0.422)	(0.055)	(0.034)	(0.032)
Intensity	-0.077	0.000	0.237	-0.103	-0.000	-0.012
	(0.468)	(0.003)	(0.182)	(0.074)	(0.000)	(0.019)
Observations	14,302	14,302	14,302	14,313	14,313	14,313
Measure of intensity	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.	High / low	Dollars / 1953 pop.	Dollars / 1976 pop.
Other controls	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long	Osili/Long
Fixed Effects	None	None	None	None	None	None
Clustering	1976 State	1976 State	1976 State	1976 State	1976 State	1976 State

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Osili/Long controls are year of birth, and dummies for the three largest Nigerian ethnic groups (Yoruba, Hausa, Igbo), and the major religions (Muslim, Catholic, Protestant, other Christian, and traditional).

Table A9. Zimbabwe without ages 14 and 15

	Dep. Var.: Years of Education			*Dep. Var.: Polygamous*		
	(1)	(2)	(3)	(4)	(5)	(6)
Age 15 or less in 1980	1.591***	1.519***	1.682***	-0.001	-0.008	-0.019
	(0.237)	(0.277)	(0.327)	(0.022)	(0.025)	(0.032)
Observations	5,504	4,496	3,696	5,508	4,500	3,700
Ages in 1980	8 to 21	9 to 20	10 to 19	8 to 21	9 to 20	10 to 19
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region	Cohort X Region

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Other controls are urban, dummies for region, and dummies for DHS round, and differential trends in Age in 1980 on either side of the cutoff. All results estimated using ordinary least squares.

Table A10. Robustness: Free Primary Education (FPE) in Sierra Leone

	Schooling			*Polygamous*		
Panel A. Alternative control groups						
FPE Cohort	-0.282			0.012		
	(0.202)			(0.046)		
FPE X R		0.834***	0.829***		0.004	-0.001
		(0.211)	(0.262)		(0.062)	(0.066)
Observations	2,167	2,167	2,223	921	921	941
Panel B. Control for battle deaths						
FPE Cohort	0.593***			-0.113**		
	(0.214)			(0.050)		
FPE X R		0.710***	0.705***		-0.021	-0.032
		(0.201)	(0.249)		(0.081)	(0.081)
Observations	7,205	7,205	7,360	5,140	5,140	5,241
Panel C. Half treatment						
FPE Cohort	0.626***			-0.095*		
	(0.211)			(0.051)		
FPE X R		0.731***	0.716***		-0.022	-0.032
		(0.205)	(0.251)		(0.081)	(0.081)
Observations	7,205	7,205	7,360	5,140	5,140	5,241
Panel D. Sample of non-movers						
FPE Cohort	0.972***			-0.090		
	(0.237)			(0.063)		
FPE X R		0.482*	0.667**		0.056	0.060
		(0.265)	(0.290)		(0.085)	(0.086)
Observations	3,372	3,372	3,435	2,363	2,363	2,406
Other controls	Yes	Yes	No	Yes	Yes	No
Fixed Effects	District	District + Y.O.B.	District + Y.O.B.	District	District + Y.O.B.	District + Y.O.B.
Clustering	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District	Y.O.B. x District

* p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses are clustered by birth-year X district. The FPE cohort consists is those born between 1990 and 1993. The sample is women born between 1980 and 1985, and between 1990 and 1993. R is the logarithm of district-level primary school funding in 2004, divided by the number of teachers and multiplied by 100. Regressions are weighted using weights from the DHS data. Other controls are dummies for Christian, Temne, Mende, Urban, Married, Employed, Radio, Fridge, TV, and Wealth Quintiles.

Table A11. Robustness. Kenya

	Dep. Var.: Years of Education	*Dep. Var.: Polygamous*	
	(1)	(2)	(3)
Panel A. Born since 1955			
Chicoine Instrument	0.370*	0.029	
	(0.196)	(0.018)	
Years of education			0.165
			(0.263)
Observations	27,044	17,807	17,801
Panel B. Less than 4 years schooling			
Chicoine Instrument	0.110	0.031	
	(0.172)	(0.038)	
Years of education			0.119
			(0.174)
Observations	7,679	6,025	6,025
Panel C. More than 17years schooling			
Chicoine Instrument	0.600	-0.056	
	(0.647)	(0.091)	
Years of education			-0.079
			(0.307)
Observations	290	203	195
Estimator	OLS	OLS	IV
Other controls	Yes	Yes	Yes
Clustering	Year of birth	Year of birth	Year of birth

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Other controls are quadratic in cohort, a cubic in age, dummies for ethnicity, dummies for quarter of birth, and dummies for childhood region of residence.

Table A12. Consistent Controls

	Dependent variable: Polygamy			
	(1)	(2)	(3)	(4)
RHS Variable	-5.317***	-0.022***	0.017***	0.012***
	(1.019)	(0.006)	(0.001)	(0.001)
Observations	103,432	103,432	301,183	301,183
RHS Variable	Teachers/cap	Log teachers per capita	Ln Distance from Catholic Mission	Ln Distance from Protestant Mission
Clustering	District 1925	District 1925	Cluster	Cluster
Compare to	Table 1	Table 1	Table 2	Table 2
	Dependent variable: Years of Education			
	(5)	(6)	(7)	(8)
RHS Variable	-0.011***	-0.070***	0.046	0.000
	(0.000)	(0.002)	(0.036)	(0.000)
Observations	493,829	493,829	15,115	15,115
RHS Variable	Resp. Education Years	Any education	Born 1970-75 X Intensity	Born 1970-75 X Intensity
Clustering	Cluster	Cluster	Cluster	Cluster
Compare to	Table 4	Table 4	Table 6, Col 1	Table 6, Col 2
	Dependent variable: Polygamous			
	(9)	(10)	(11)	(12)
RHS Variable	-0.014	0.014	0.009	0.003
	(0.008)	(0.024)	(0.027)	(0.032)
Observations	15,115	6,267	5,259	4,460
RHS Variable	Born 1970-75 X Intensity	Age 15 or less in 1980	Age 15 or less in 1980	Age 15 or less in 1980
Clustering	Cluster	Region X age	Region X age	Region X age
Compare to	Table 6, Col 3	Table 7, Col 1	Table 7, Col 2	Table 7, Col 3
	Dependent variable: Polygamous			
	(13)	(14)	(15)	
RHS Variable	-0.052	-0.024	0.029*	
	(0.137)	(0.049)	(0.017)	
Observations	1,641	1,641	19,596	
RHS Variable	FPE Cohort	FPE X R	Chicoine Instrument	
Clustering	Y.O.B. x District	Y.O.B. x District	Y.O.B.	
Compare to	Table 8, Col 1	Table 8, Col 2	Table 9	
Other controls	Individual	Individual	Individual	
Fixed Effects	Cntry-rnd	Cntry-rnd	Cntry-rnd	

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Individual controls (Ind.) are year of birth, year of birth squared, age, age squared, religious dummies, and urban. Other controls are "High Intensity" and "Treated Cohort" for Nigeria, and running variables in age for Zimbabwe.