Does the Legal Stick Work? Unintended Consequences of a Polygamy Ban in Benin

Shatanjaya Dasgupta
Providence College
sdasgupt@providence.edu

and

Abhilasha Sahay
World Bank
asahay@worldbank.org

PRELIMINARY DRAFT

Abstract

Are legal reforms enough to change deep-seated, socio-cultural conjugal practices? This paper studies the effect of a polygamy ban policy in Benin on marital outcomes. We find that the ban did reduce the incidence of polygamous unions but did not lead to a complete cessation of polygamous practices. Using a difference-in-difference model, exploiting cross-commune variation in prevalence of polygamy before the abolition, we find that, among polygamous marriages, the ban led to an increase in wife rank (by 33 percent) and spousal age gap (by 63 percent) but no change in spousal education difference. Further, women who faced a higher wife rank or larger spousal age gap post-ban, also faced a measurable decline in intra-household decision-making and reported greater acceptance of wife-beating. These results cumulatively point towards a deterioration in marital quality post-ban and reiterate the need to complement legal reforms with effective socialization of policies - including addressing harmful norms- to achieve intended outcomes.

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1. Introduction

Past scholarship demonstrates that polygyny, a form of marriage in which a man has more than one wife, generally has negative effects on women's role in decision-making and household welfare (Munro et. al., 2019), household conflict (Seeley, 2012), and children's health and education outcomes (Han and Foltz, 2015; Arthi and Fenske, 2016; Mammen, 2008). However, there is limited evidence on whether legal reforms such as abolition of polygamy can reduce the incidence of such deep-seated, socio-cultural conjugal practices and alleviate its harmful impacts.

To address this knowledge gap, we investigate the impact of a polygamy ban policy in Benin, where polygamy has been historically rife. As per Benin Census 2013, the average rate of polygamy is 34% among women and 27% among men, the average rate of polygamy is 27% and, in some communes, it is as high as 40%. Polygamy is more common in rural areas compared to urban areas (27 percent versus 19 percent). In 2004, polygamy was abolished in Benin rendered unconstitutional under the *Personal and Family Code of Law*. As per the law, while polygamous unions contracted before the enactment would continue to be recognized, those contracted after, would not be acknowledged and its members would not be entitled to any legal protection and benefits. Its practice was made punishable by six months to three years of imprisonment alongside 300,000 francs (approx. USD 500) fine.

Using data from four waves (2001, 2006, 2011-12 and 2017-18) of the nationally representative Benin Demographic Health Survey, we evaluate the impact of the ban on three sets of outcomes: (i) hazard into marriage, early marriage (between ages 12 and 26), and polygamous marriage, (ii) marital outcomes determined at the time of marriage such as wife rank, spousal age gap and spousal education gap, and (iii) marital quality measured by intra-household decision-making power and attitudes towards wife beating. We identify a causal impact by using a

difference-in-difference model that exploits cross-commune differences in exposure to the ban, measured by pre-ban polygamy rate, and the timing of marriage, i.e., whether the respondent(s) got married before or after the ban.

We first find that the probability of getting into a polygamous union (compared to monogamous marriage) declines post-ban, both among women and women; thereby, providing first-stage evidence on execution of the ban. At the same time, we also find a small reduction in hazard into marriage and early marriage among the women sample, post-ban, but no corresponding change in the men's sample.

Second, we estimate the impact of the ban on primary outcomes of interest, i.e., wife-rank, spousal age gap and spousal education gap. We find that women who are in a polygamous union post-ban and belong to communes with higher rate of polygamy at baseline are more likely to have a higher wife rank, i.e., greater likelihood of becoming a *junior* wife (0.6 increase in rank number or 33 percent increase relative to the control mean) and are more likely to have a larger age gap with their spouses (a 6-year increase or 63 percent increase relative to control mean), than women who are in polygamous unions pre-ban and belong to communes where polygamy was less common. In contrast, we do not find any statistically significant impact of the ban on spousal education gap. Further, we find notable heterogenous effects in treatment by level of literacy at baseline. Specifically, we find that the increase in wife rank and spousal age, post-ban, is smaller among literate women, implying that literate women are better able to weather the negative shock of the ban.

After estimating an impact of the ban on the primary outcomes, we suggestively examine whether the above-mentioned primary impacts have any implications for marital quality, as measured by intra-household decision-making and attitudes to wife-beating. We find that women

who face a higher wife rank (or more *junior* rank) also face a reduction in intra-household decision-making (68 percent decrease relative to control mean) and report increased acceptance of wife-beating (75 percent increase relative to the control mean). Similarly, among women who are in polygamous unions with larger spousal age gap report an increase in tolerance to wife beating (9 percent increase relative to control mean) but no change in decision-making.

Overall, our results show a marginal direct influence of the ban on the likelihood of polygamous marriages among men and women. On the intensive margin, we find significant impacts on marital matching prospects of women who continue to be in polygamous unions since they face a greater likelihood of being a higher ranked wife or have large age gaps with their spouses. This translates to a deterioration in marital quality as measured by decision-making power and attitudes towards wife-beating. These estimates, therefore, showcase the existence of unintended effects of a well-intentioned ban. Our findings reiterate the need to complement legal reforms with effective socialization and communication of policies to also address norms around gender roles and power dynamics among couples

This paper contributes to two broad strands of literature. First it adds to the evidence base on impact of legal reforms - such as ban on child-marriage or increase in minimum legal age of marriage (Wilson, 2022), anti-dowry law (Calvi and Keskar, 2021), women's property rights (Roy 2015), and compulsory schooling laws (Kirdar, Dayioglu and Koc, 2018; Chaijaroen and Panda, 2023) - on marital outcomes. Past scholarship demonstrates that banning child marriage and increasing the minimum legal age of marriage reduces prevalence of early marriage and adolescent fertility, increases educational attainment, and improves employment (see Lee-Rife et al. (2012) for a systematic review; Maswikwa et al. (2015); Wilson 2022). Similarly, legislations around extension of compulsory schooling have also shown to delay marriage and reduce incidence of

adolescent fertility (Kirdar, Dayioglu and Koc, 2018; Chaijaroen and Panda 2023).mThese education policies aren't explicitly aimed at marital outcomes but have shown to have positive spillovers via delay in marriage and fertility.

At the same time, some such legal reforms have shown to have unintended, negative consequences. For example, Calvi and Keskar (2021) document that abolishing or reducing dowries (through anti-dowry laws or taxes) can reduce women's bargaining power and separations, increase incidence of domestic violence, aggravate intra-household inequality and increase women's risk of living in poverty after marriage. Roy (2015) finds that legal reforms aimed at improving women's property inheritance rights in India did not increase daughters' likelihood of inheriting any land; instead, it led to sons being strategically "gifted" parental land so as to circumvent the new law. Daughters, however, were "compensated"; younger daughters were given more education and girls about to get married received higher dowries as a substitute for their share in ancestral property. Our paper contributes to this body of work by providing, to the best of our knowledge, first empirical evidence on the (unintended) impacts of abolishing polygamy.[1]

Second, this paper contributes to the growing body of work on the consequences of polygamy. We show how matching in the polygamy marriage market changes in the presence of legislation that aims to abolish polygamous unions, and the consequences of those changes.

The rest of the paper is divided into 5 sections. Section 2 discusses the context and provides details of the policy. Section 3 and 4 delineate the data and empirical strategy, respectively. Section 5 presents the results and Section 6 concludes.

[1] Notably, Tertilt (2015) provides *theoretical* estimates on impact of polygamy ban. It utilizes a quantitative model of polygyny, which indicates that enforcing monogamy reduced fertility, increases savings and output per capita. However, Tertilt (2015) paper does not provide any empirical evidence of impact of a ban.

2. Context and Intervention

Polygamy in Africa

Polygamy is widespread in Africa, although recent estimates have noted its decline. It's prevalence is highest in the western part of the continent with rates varying within countries as well. The emergence and persistence of this marital practice can be linked to various factors. White and Burton (1988) review several theories of why polygyny exists in certain societies. It can emerge from a reproductive strategy by which men maximize their fertility, higher male mortality due to disease, warfare or dangerous occupations, or the ease of access to resources needed to support large families. Lesthaeghe (1994) contends that polygyny allowed men to have continuous sexual relations in societies that endorsed long periods of post-partum abstinence for women. Religion is also correlated with polygyny. On the one hand, the highest polygyny rates are observed among Muslim populations (Chae and Agardjanian, 2022) and on the other hand, polygyny is forbidden in Christianity (Dalton and Leung, 2014).

Early economic theories based on work by Becker (1974) and Grossbard (1980) focus on male inequality in resources and the marginal productivity of women as the primary determinants of polygyny. They suggest that wealthier men and women with high marginal contributions are more likely to be in polygamous unions. Jacoby (1995) showed empirically that polygyny is high in rural areas where the there is greater reliance on subsistence agriculture and high contributions of women to agricultural production. More recently, Gould, Moav and Simhon (2008) put forth a model to illuminate that rich men in less-developed countries prefer quantity over quality in wives and children and therefore, polygamy is more prevalent in poorer countries. However, the ability of men to marry multiple low-quality women and raise several low-quality children is reduced if their wealth is derived from education or wage incomes rather than non-labor income (Gould,

Moav, and Simhon, 2012). The rise of polygyny has also been linked to the slave trade. Dalton and Leung (2014) finds that polygamy emerged or persisted in regions affected by the Transatlantic slave trades whereas it remained uncommon in regions affected by the Indian Ocean slave trades. This pattern arises because of skewed sex ratios in the marriage market resulting from higher proportion of males being exported from Western Africa in the Transatlantic slave trades in contrast to higher proportion of females being exported from Eastern Africa in the Indian Ocean slave trades. Another determinant of polygyny is historical education systems. Regions that were exposed to colonial or missionary education have lower polygyny rates (Fenske, 2015).

A large body of literature has documented the effects of polygyny on various women's outcomes. Tertilt (2005) finds that polygynous countries are poorer than non-polygynous countries, and that banning polygyny could increase savings and output per capita and decrease fertility. Gender inequality as measured by literacy rates and share of seats occupied by women in parliament is more pronounced in polygamous societies (Tertilt, 2006). Previous research has also shown that polygyny is positively associated with greater acceptance of intimate partner violence (Uthman, Lawoko, and Moradi, 2010), the transmission of sexually transmitted diseases, and anxiety and depression among women (Bove and Valeggia, 2009).

Polygamy in Benin

This study focuses on one such West African country, Benin. While the incidence of polygamy varies by geographical location, estimates indicate that, on average, about one-third of women and a little less than a quarter of men in Benin are in polygamous unions (Benin Census, 2013; Benin Demographic and Health Survey Report, 2018).

In 2004, polygamy was officially abolished in Benin. It was rendered unconstitutional under the *Personal and Family Code of Law*. As per the law, while polygamous unions contracted before the enactment of the law would continue to be recognized, those contracted after would not be acknowledged and its members would not be entitled to any legal protection and benefits. Furthermore, its practice would lead to six months to three years of imprisonment alongside a fine of 300,000 francs (approx. USD 500).

In Benin, as in other West African countries, legal attempts at abolishing polygamy have not been successfully enforced (Tertilt, 2006). Besides, men can also choose traditional or religious marriages to circumvent such laws which only target civil unions (Dauphin, 2013, Chae and Agardjanian, 2022).

Before 2004, men were allowed to have multiple spouses, but women could only have one spouse. This was seen as a violation of the basic principle of equality between men and women, enshrined in Article 26 of the Beninese Constitution. With the motivation of mitigating discriminatory provisions against women, the new code also establishes gender parity regarding marriage age, inheritance of property and penalizes levirate and forced marriage.[2] The policy was first introduced in the constitutional court in 2002 and after 2 years of discussions and deliberations by multiple stakeholders (political parties, women activists, human rights institutions and non-governmental agencies) it was passed in June 2004.

While the law prescribes a blanket ban on the practice, the rate of polygamy in Benin continues to be quite high. Data from the 2013 Benin Census indicates that 34% of women and 23% of men are in polygamous unions. More recently, the latest Demographic and Health Survey (2018) data from Benin shows that 38% of married women are in polygamous unions while around

[2] Based on information from Monograph accessed from Jacob Burns Law Library, International Encyclopedia of Law: Family and Succession Laws

22% men report being polygamous. Fig 1 reports calculated polygamy rates from DHS data for women and for a period of 22 years from 1996-2017. Trends in Figure 1 depict a reduction in rate of polygamy post-ban; thereby, indicating some enforcement of the ban. However, it also indicates that the ban did not lead to a complete cessation of polygamous practices; thereby, suggesting imperfect compliance.

3. Data

We obtain data from four rounds of the Benin Demographic and Health Survey (BDHS). These are nationally representative cross-sectional household surveys and data are collected using a two-stage stratified cluster sampling technique. In the first stage, clusters are drawn from census information and in the second stage a sample of households is chosen within each cluster. Our study draws data from the latest four survey rounds conducted in 2001, 2006, 2011-12 and 2017-18.

Several questionnaires were used to collect the survey data. The Household Questionnaire collected information on the roster of household members and basic socio-demographic and household characteristics. The Women's questionnaire was administered to all women aged 15-49 living in the selected households. In contrast, the Men's questionnaire was administered to men aged 15-64 in a subset of the households surveyed.

Selection of sample:

We consider only the rural sample because polygamy is more prevalent in rural areas than urban areas (27 percent compared to 19 percent) and as noted in the literature, such legal reforms might

be more enforced in urban areas. Since DHS only collects information on year of first marriage, we limit our sample to women who have been married only once to ensure that we can correctly place women into the pre-and post-ban periods.

We evaluate the impact of the ban on three sets of outcomes. The primary analysis includes the sample of women (respondents of the individual recode) who are in polygamous marriages and estimates the impact on marital outcomes determined at the time of marriage (referred to as *marital matching* outcomes), i.e., wife rank, spousal age gap and spousal education gap. The next set of analyses also draws on the sample of women who are in a polygamous marriage and evaluates the impact on measures of marital quality – typically determined post-marriage - such as intra-household decision-making and attitudes towards wife beating. Notably, we could also examine the effect on exposure to intimate partner violence; however, this data was only collected for the most recent wave and not the other waves we use. For both these analyses, we confine the sample to women who are in polygamous *marriages* and not those who are living together (without marriage); we find our primary estimates to be robust to including the living together sample (results available upon request). The women's sample used to estimate the primary impacts includes 3,775 women across 77 communes who got married between years 1996 and 2017 in a polygamous union.

Finally, the third set of analysis includes the full sample of women and men (not only those who are in a polygamous marriage) and evaluates the impact on outcomes at the extensive margin, i.e., hazard into marriage, early marriage and polygamous marriages. The women's sample includes 12,770 women and men's sample includes 4,095 men across 77 communes who got married between years 1996 to 2017.

Outcomes of interest

A woman is identified as being in a polygamous marriage from the variable v505, which enumerates the number of other wives of her husband. A woman is labeled as being in a monogamous marriage if the husband has no other wives and polygamous if the husband has other wives.

Wife rank is determined from variable v506 in the individual women's recode and takes a numeric value between values 1 to 11. Spousal age gap is the difference between the husband's age (v730) and the wife's age (v012) (in years) and spousal education gap is the difference between the husband's years of education (v715) and the wife's years of education (v133).

To measure intra-household decision making, we create an index, which add the number of decision-making domains in which women takes decision independently or jointly. DHS records responses on decision-making across six prescribed domains (v743a-v743f), i.e., large household purchases, own health care, daily needs, visit to family/relatives, food to be cooked and expenditure using partner's income. However, we find that data on three of these domains wasn't collected in recent survey years (2011 wave and 2017 wave), i.e., decision on daily household purchases, food to be cooked, and expenditure on partner's income. Thus, we only use responses from the remaining three domains. For each domain, we create a binary variable, which turns on if the woman makes the final decision on the domain either independently or jointly. Thereafter, we add the 3 binary variables to create a composite index on decision-making. To measure attitudes towards wife beating, we create a binary variable, which turns on if the wife reports that it is justified to beat the wife for ANY one of the following five reasons (v744a-v744e): wife goes out without telling husband, wife neglects the children, wife argues with husband, wife refuses to have sex with husband, wife burns the food, and 0 otherwise.

4. Empirical Strategy

We identify the causal impact of the legal ban on marital quality using a difference-in-difference method, exploiting cross-commune differences in exposure to the ban, measured by pre-ban polygamy rate, and timing of marriage, i.e., whether the woman got married before or after the polygamy ban in 2004. Considering this was a nation-wide ban, and did not exclusively impact a subset of location, we cannot identify a pure treatment and control group. Instead, we identify the effect using a treatment intensity approach where treatment is assigned as a continuum between 0 and 1, based on exposure to the ban, which is measured by rate of polygamy prevalent pre-ban. Communes that had a high rate of polygamy before 2004, are likely more exposed to the ban, after 2004, than communes with a low polygamy rate at baseline. Thus, we estimate the difference in marital quality between women who got married after 2004 and are residing in a commune with high polygamy rate at baseline and women who got married before 2004 (and were not subject to the ban) and were residing in a commune with lower rate of polygamy pre-ban.

$$Y_{icbm} = \beta_1(Post_i \times Basepolygamyrate_c) + \beta_2(Basepolygamyrate_c) + \alpha_c + \delta_m + \theta_b + \boldsymbol{X}_i + \varepsilon_{icbm}$$

(Equation 1)

where, y_{icbm} is the outcome for woman *i*, living in commune *c*, who was born in year *b* and got married in year *m*. $Basepolygamyrate_c$ is the constructed variable of treatment intensity, i.e., proportion of ever-married women in a given commune *c* who reported to be in a polygamous union, averaged across pre-ban years, i.e., 1996-2003.[3] It takes value between 0 and 1. Figure 2 depicts the spatial variation in polygamy rate across communes before the ban on polygamous

[3] We change the exposure window in robustness checks, which we elaborate on in Section 5.6.

marriages. *Post* is a dummy variable that takes value 1 if the interviewed woman got married after 2004, and 0 otherwise. β is the main coefficient of interest, which captures the average difference in marital quality after the ban based on varying levels of pre-ban baseline polygamy rate.

The model accounts for potential statistical confounders. α_c, δ_m, and θ_b are commune, year of marriage and year of birth fixed effects, respectively. Year of marriage fixed effects absorb any time-varying characteristics that may affect marriage, such as adult sex-ratio, conjugal norms. Similarly, year of birth fixed effects account for cohort effects. We include commune specific fixed effects to control for time-invariant local unobservable characteristics, such as geographic, economic, and cultural factors. There are 77 communes in Benin. $\epsilon_{i,c,m,b}$ is the idiosyncratic error term that is clustered at commune level. $\boldsymbol{X}_i$ i is a vector of individual-level control variables and includes dummies for whether the woman is older than her husband and whether she is more educated compared to her husband, ethnicity, religion, wealth index and household-size. Our choice of control variables are largely based on correlates of polygyny as established in previous literature.

5. Results

5.1. Evidence of Ban

Before we discuss the impact of the ban, it is instructive to showcase first-stage evidence on the implementation of the ban, i.e., did the ban change incidence of polygamous unions. Figure 1 provides descriptive evidence of decline in polygamy rate post-ban. In addition, we conduct a more formal check – closely following the empirical approach in Corno, Hildebrandt and Voena (2020) – to estimate whether the ban led to any changes in hazard into polygamous marriage, and relatedly

hazard into marriage and early marriage (marriage between ages 12 and 26). We estimate this for the women's sample (using individual recode) as well as the men's sample (using men's recode).

Similar to Corno et al (2020), we convert our data into person-year panel format. t_0 denotes the age when a woman is first at risk of getting married, and tm, the age when she enters her first marriage. In our sample, t_0 is age 12 (4 percent of women in our sample are married before the age of 12). All women in the sample are married by age 48, therefore women older than 48 years old are right-censored. To estimate the impact on early marriage, women after the age of 26 are right-censored.[4] To estimate the impact on polygamous marriage, the full sample of women is restricted to only those who got into a polygamous marriage and like before, women after the age 48 are right-censored. Thereby, in this data structure, a woman who is married at age t_m contributes $(t_m - t_0 + 1)$ observations to the sample: one observation for each at-risk year until she is married, after which she exits the data.

$$Y_{icbm} = \beta\,(Post_i \times BasePolygamyRate_c) + \alpha_c + \delta_m + \theta_b + \epsilon_{icbm} \quad \text{(Equation 2)}$$

The dependent variable, y_{icbm}, is a binary variable coded as 1 in the year the woman gets married, and zero otherwise. All other variables encapsulate the same interpretation as Equation 1. β is the main coefficient of interest and measures the effect of the ban on the probability of marriage, early marriage, or polygamous marriage.

Estimates from Table 3 (column 3, panel A and panel B) indicate that the probability of getting into a polygamous union (compared to a monogamous marriage) declines post-ban; thereby, providing first-stage evidence of the implementation of the ban. Further, among the women's sample (panel A), we find a small reduction in hazard into marriage (column 1) and

[4] Corno et al (2020) considers 25 years old as the cut-off point for early marriage. In our sample, 95 percent of the sample is married by 26 years of age and 5 percent of the sample is married by age 12. Therefore, we consider the age range 12-26 years old (inclusive) to examine hazard into early marriage.

hazard into early marriage (column 2). However, we find no change in hazard into marriage or early marriage, among the men's sample (panel B, column 1 and 2). The reduction in hazard into marriage and early marriage may occur due to a *marriage squeeze* post-ban caused by *compliers, i.e.*, men who abide with the ban may not take on addition wives; consequently, with limited *supply* of potential husbands, women may face difficulties in finding a match leading to delay in marriage or failure to get married.

Thus, we do find some changes in the composition of people getting married post-ban, but the size and statistical significance of the coefficient estimates (column 3) suggest that the ban primarily affected whether that pool of people getting married chose into a polygamous union versus a monogamous union. This is a noteworthy, intended impact of the policy. The next of results shed light on whether women who did end up in a polygamous union, post-ban, faced a deterioration in marital matching and marital quality versus women who were already in a polygamous marriage.

5.2 Impact of Ban on Marital Matching

We estimate the impact of the ban on three outcomes from the individual women's recode data set. These are determined at the time of marriage and therefore provide some insights into marital matching effects of the ban.

From Table 2, we find that the ban led to a 0.57 increase in wife rank (33 percent increase relative to control mean), a 6.25-year increase in spousal age gap (63 percent increase relative to control mean), but no statistically significant impact on spousal education gap. The finding on wife rank is noteworthy since higher wife rank has been found to be associated with inferior outcomes

for the women and her children (Lardoux and van de Walle, 2003; Gibson and Mace, 2007). In a similar vein, increase in spousal age gap is also correlated with lower bargaining power for women within marriage. This is consistent with our estimates from additional suggestive analysis conducted in Section 5.4.

5.3 Heterogeneity in Impact on Marital Matching

Previous literature suggests that lower quality women become higher ranked wives (Gibson and Mace, 2007; Matz, 2016). Following this finding, we investigate if the ban may have disproportionately affected low quality women. We use literacy and height as the two proxies of quality.

To conduct an empirical investigation of the heterogenous impact of the ban, we utilize the following model:

$$Y_{icbm} = \beta\,(Post_i \times BasePolygamyRate_c \times Quality_i) + \alpha_c + \delta_m + \theta_b + \epsilon_{icbm} \quad \text{(Equation 3)}$$

where the dependent variables are wife rank, spousal age gap and spousal education gap as in equation (1) and $Quality_i$ is a dummy for whether a woman in literate or if a woman is above median height.

The regression results are presented in Table 3. The first row displays the results using literacy as a proxy for quality. Column 1 reveals that literate women in high polygamy areas and in the post-ban period are less likely to have a high wife rank. A similar statistically significant negative association is also obtained for spousal age gap. As with our main results, we find no statistically significant impact of the ban on spousal education gap that varies by literacy status of women.

In contrast, the second set of regressions, where we proxy quality with height, does not yield any statistically significant results.

Overall, these results provide suggestive evidence that higher quality women may be better able to weather the negative, unintended consequences of the ban (in terms of wife rank and spousal age gap) compared to low quality women.

5.4 Association between Marital Matching and Marital Quality

We conduct auxiliary analysis to examine whether there is differential impact of the ban on decision-making and attitudes towards wife-beating among women faced a high wife-rank and larger spousal age-gap, post-ban. We find that women who faced a higher wife-rank post-ban also reported reduction in intra-household decision-making (68 percent decrease relative to control mean) and an increase in acceptance of wife-beating (75 percent increase relative to the control mean). Women who faced a larger spousal age gap, post-ban, report an increase in tolerance to wife beating (9 percent increase relative to control mean) but no change in decision-making. Admittedly, these results on marital quality are, at best, suggestive, given the first-order impact of the ban on wife rank and spousal age gap. Nonetheless, these estimates, in conjunction, indicate a deterioration in marital matching and quality and showcase an unintended effect of a well-intentioned ban of polygamy.

5.5. Identification

To test the parallel trends assumption, we follow Autor (2003) and conduct the following event-study specification:

$$y_{icmb} = \sum_{\substack{m \\ \tau=1}} \mu_{-\tau} \left(T_{m-\tau} \times BasePolygamyRate_c\right) + \sum_{\substack{q \\ \tau=0}} \mu_{\tau} \left(T_{m+\tau} \times BasePolygamyRate_c\right) + \alpha_c + \delta_m + \theta_b + X_i + \epsilon_{icmb}$$

(Equation 4)

All terms follow the same definition as Equation 1. The first two terms represent interaction of treatment intensity with year of marriage-dummies before (m periods) and after (q periods) the ban. Figure 2 indicates no evidence of pre-trends and there is no systematic difference between more and less exposed communes before the ban, as shown by the statistical insignificance of coefficients associated with the lead dummies.

We conduct an additional placebo check to examine whether the effect is stemming from any pre-existing trends. We replicate the primary analysis (Equation 1) with varying the timing of the polygamy ban policy, i.e., we construct "fake policies" in pre-ban years, namely 2000, 2001, 2002 and 2003. Figure 4 shows that none of the placebo estimates are significantly different from zero in the pre-ban years; thereby, indicating that the effect of the ban on wife rank and spousal age gap is not driven by any systematic pre-shock differences in trend of marital outcomes between more and less exposed communes.

5.6 Robustness

Here, we examine if our primary estimates are robust to using alternative measures of exposure to the legal shock. Our analysis is conducted using the base polygamy rate, calculated as the average rate of polygamy in the pre-ban years 1996-2003. In this check, we examine whether the results are similar if the base polygamy rate is constructed by averaging across years: (i) 1997-2003, (ii) 1998-2003, (iii) 1999-2003, (iv) 2000-2003. The results of this exercise, presented in Table 5,

show that our findings on the impact of the ban on wife rank and spousal age gap are robust to changing the exposure measure.

6. Conclusion

This paper investigates the impact of a ban on polygamy in Benin on marital outcomes. We identify an impact by exploiting cross-commune variation in rate of polygamy at baseline, with the prior that communes with higher rate of polygamy at baseline (pre-2003) are likely more exposed to or will be more affected by abolition of polygamy than communes where polygamy wasn't as commonplace, pre-ban. We first find first-stage evidence of reduction in incidence of polygamy, i.e., both men and women have a reduced probability of being in a polygamous union; thereby, suggesting an intended proximate impact of the ban. However, the ban does not lead to an absolute cessation of polygamous practices, indicating imperfect compliance.

Among women who continued to marry into polygamous unions, post-ban, faced a higher (more junior) wife rank and larger spousal age gap. Both these outcomes are shown to be associated with lower bargaining power (Lardoux and van de Walle, 2003; Gibson and Mace, 2007). Consistent with past scholarship, we find that women who take on a more junior wife rank and marry a man with larger age gap, post-ban, also experience lesser say in intra-household decision making and are more accepting of wife beating. These results point towards unintended consequences of a well-intentioned policy and may indicate that legal reforms, in isolation, may not be sufficient to move the needle on deep-seated conjugal practices. Possibly, complementing such legal reforms with effective socialization and communication of the policy and its associated returns – alongside adopting interventions to address harmful norms around marriage and gender roles – is needed to achieve intended outcomes.

The study has a few key limitations, primarily due to limited data. First, the Demographic Health Survey (DHS) does not record information on all co-wives within a household. Consequently, a complete examination of intra-household dynamics among co-wives is beyond the scope of this paper and is an area for future research. Second, in a similar vein, DHS only records information on the year of *first* marriage. Therefore, among the men's sample (used for hazard rate analysis), we are not able to observe information on all marriages a polygamous man may have been in pre- and post-ban periods. Third, while we try to shed some light on heterogeneity in impact of the ban, based on a few baseline covariates, namely, height and literacy (discussed in Section 5.2), we aren't able to shed much light on who selects into polygamous unions post-ban due to limited data on women's native family (for example, parental education, household size, and sibling order etc. The DHS only records information on women's marital family (not her native family). Future data collection efforts can address this gap to better characterize persistence of such conjugal practices despite legal bans and reforms.

While a comprehensive analysis of the policy is beyond the scope of this paper, our findings provide, to the best of our knowledge, one of the first causal evidence on the impact of a ban on polygamy. Considering polygamy is still commonly practices in several parts of the world and is shown to have determinantal effects on women, children and households, examining what works to reduce the incidence of this practice and improve women's status within marriages is critical. Findings from our paper sheds pertinent light on this agenda. Future research can provide more insights on which specific interventions can complement legal reforms to maximize impact and what works to improve policy compliance, especially in developing country contexts where institutional capacity and enforcement may be limited.

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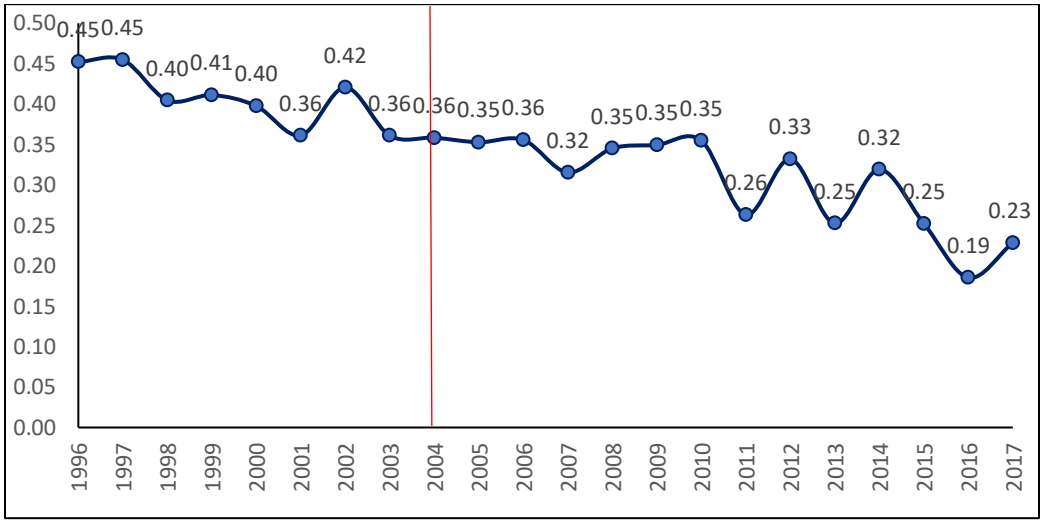
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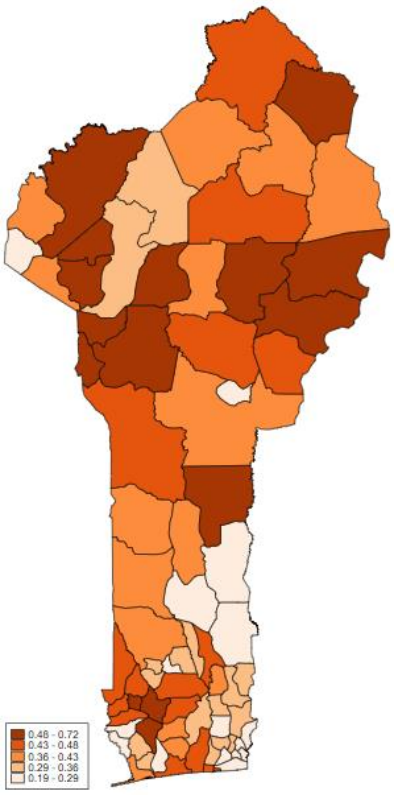
Figures

Figure 1: Polygamy Rate by Year of Marriage



Source: Demographic Health Survey

Figure 2: Baseline Polygamy Rates by Commune

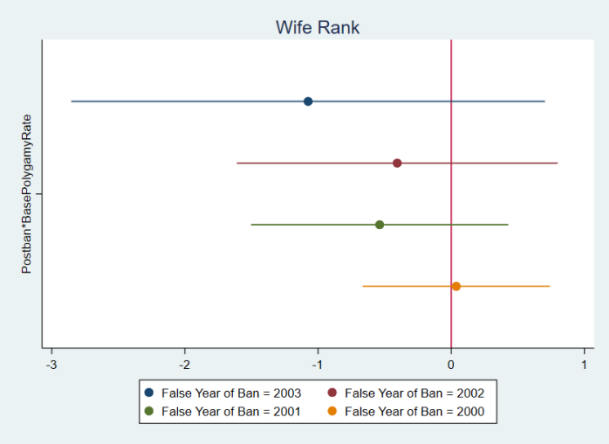


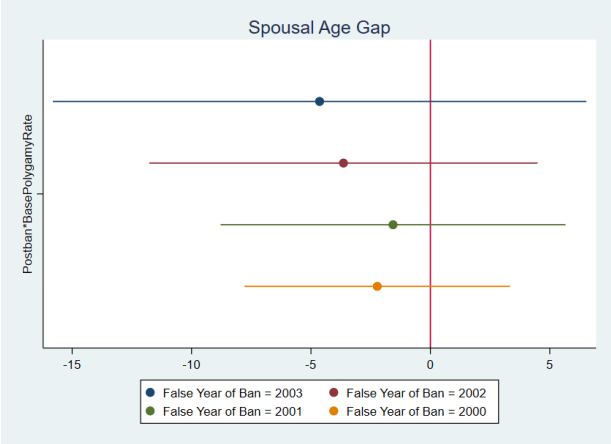
Notes: GIS shapefiles were downloaded from https://international.ipums.org/international/ ; Source: Demographic Health Survey

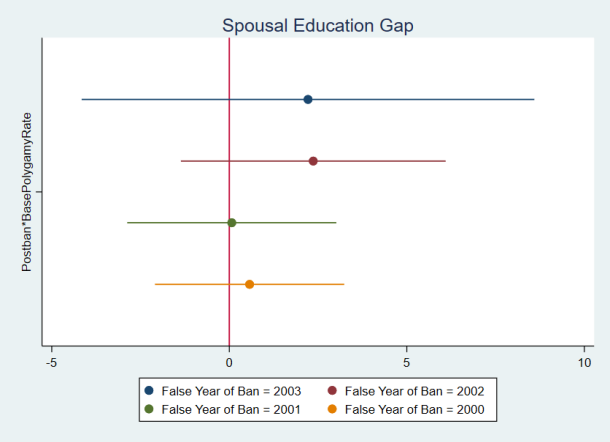
Figure 3: Event Study Graphs



Figure 4: Falsification Checks







Tables:

Table 1: Impact of Ban on Hazard into Marriage and Early Marriage

	(1)	(2)	(3)
	Panel A: Women's Sample		
	Hazard into Marriage	Hazard into Early Marriage	Hazard into Polygamy Marriage
PostBan × BasePolygamyRate	-0.016*	-0.011*	-0.063***
	(0.009)	(0.006)	(0.01)
BasePolygamyRate	0.066***	0.004	0.134***
	(0.008)	(0.004)	(0.007)
Observations	96,121	86,790	96,121
	Panel B: Men's Sample		
	Hazard into Marriage	Hazard into Early Marriage	Hazard into Polygamy Marriage
PostBan × BasePolygamyRate	0.0008	-0.004	-0.042***
	(0.004)	(0.002)	(0.006)
BasePolygamyRate	-0.121	0.56***	0.141***
	(0.013)	(0.072)	(0.024)
Observations	54,395	30,828	54,395

Notes: ***, **, * denote significance at 1%, 5% and 10% level respectively. Robust-clustered standard errors at the commune level are reported in parentheses.

Table 2: Impact of the Ban on Marital Matching

	Wife rank	Spousal age gap	Spousal educ gap
	(1)	(2)	(3)
PostBan × BasePolygamyRate	0.57*	6.25**	−0.37
	(0.31)	(2.52)	(0.90)
BasePolygamyRate	−1.89***	−23.9***	−6.10***
	(0.20)	(1.58)	(0.67)
Observations	3,775	3,672	3,720
Control Mean	1.71	9.93	1.12
R-squared	0.163	0.137	0.359

Notes: ***, **, * denote significance at 1%, 5% and 10% level respectively. Robust-clustered standard errors at the commune level are reported in parentheses.

Table 3: Heterogeneity in Impact of the Ban on Marital Matching

	Wife rank	Spousal age gap	Spousal educ gap
	(1)	(2)	(3)
PostBan × BasePolygamyRate	−1.51*	−18.9*	2.46
× Literacy	(0.85)	(9.68)	(3.27)
Observations	3,688	3,586	3,634
R-squared	0.166	0.143	0.360
PostBan × BasePolygamyRate	−0.36	−1.15	0.85
× AboveMedianHeight	(0.54)	(4.74)	(2.41)
Observations	3,016	2,926	2,973
R-squared	0.194	0.144	0.356

Notes: Each triple interaction coefficient is estimated from a separate regression. ***, **, * denote significance at 1%, 5% and 10% level respectively. Robust-clustered standard errors at the commune level are reported in parentheses.

Table 4: Impact of Ban on Marital Quality, based on heterogeneity by marital matching

	(1)	(2)
	Panel A: Heterogeneity by Wife Rank	
	Decision Making	Attitude towards Wife Beating
PostBan × BasePolygamyRate × Wife Rank	-1.076**	0.314*
	(0.345)	(0.154)
Observations	3775	3682
Control Mean	1.58	0.42
R-squared	0.15	0.153
	Panel B: Heterogeneity by Spousal Age Gap	
	Decision Making	Attitude towards Wife Beating
PostBan × BasePolygamyRate × Spousal Age Gap	-0.078	0.036*
	(0.059)	(0.016)
Observations	3775	3682
Control Mean	1.58	0.42
R-squared	0.15	0.153

Notes: Each triple interaction coefficient is estimated from a separate regression. ***, **, * denote significance at 1%, 5% and 10% level respectively. Robust-clustered standard errors at the commune level are reported in parentheses.

Table 5: Robustness Check: Alternative Measure of Exposure to Shock

	Exposure years 1997-2003			Exposure years 1998-2003			Exposure years 1999-2003			Exposure years 2000-2003		
	Wife rank	Spousal age gap	Spousal educ gap	Wife rank	Spousal age gap	Spousal educ gap	Wife rank	Spousal age gap	Spousal educ gap	Wife rank	Spousal age gap	Spousal educ gap
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PostBan × BasePolygamy Rate	0.62**	6.50***	−0.61	0.50*	5.78**	−0.67	0.59*	4.53**	−0.57	0.69**	6.07***	−0.62
	(0.30)	(2.39)	(0.91)	(0.29)	(2.38)	(0.88)	(0.30)	(2.23)	(0.81)	(0.30)	(2.17)	(0.82)
Observations	3,775	3,672	3,720	3,775	3,672	3,720	3,775	3,672	3,720	3,775	3,672	3,720
R-squared	0.165	0.137	0.359	0.165	0.137	0.359	0.165	0.136	0.359	0.166	0.137	0.359

Notes: ***, **, * denote significance at 1%, 5% and 10% level respectively. Robust-clustered standard errors at the commune level are reported in parentheses. Model 2 estimated.

Appendix

Table A1: Sample Descriptive Statistics

	Full Sample			Pre-Ban Period			Post-Ban Period		
	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome Variables									
Wife rank	1.77	0.88	3,775	1.71	0.89	2,263	1.86	0.86	1,512
Spousal age gap	9.87	7.20	3,672	9.93	7.17	2,191	9.78	7.24	1,481
Spousal educ gap	0.96	2.92	3,720	1.12	2.80	2,239	0.71	3.07	1,481
Control Variables									
Wife more educ	0.09	0.29	3,775	0.06	0.23	2,263	0.14	0.35	1,512
Wife older	0.02	0.15	3,775	0.03	0.16	2,263	0.02	0.13	1,512
Marital HH size	8.72	5.07	3,775	8.81	4.68	2,263	8.59	5.60	1,512
Religion									
Traditional	0.19	0.39	3,775	0.22	0.41	2,263	0.15	0.36	1,512
Islam	0.38	0.48	3,775	0.34	0.47	2,263	0.44	0.50	1,512
Catholic	0.14	0.35	3,775	0.15	0.35	2,263	0.14	0.34	1,512
Protestant	0.05	0.22	3,775	0.05	0.21	2,263	0.05	0.22	1,512
Other Christian	0.16	0.37	3,775	0.16	0.37	2,263	0.16	0.37	1,512
Other	0.01	0.10	3,775	0.01	0.11	2,263	0.01	0.09	1,512
None	0.06	0.24	3,775	0.07	0.25	2,263	0.06	0.23	1,512
Ethnicity									
Adja	0.16	0.37	3,775	0.17	0.38	2,263	0.14	0.35	1,512
Bariba	0.14	0.34	3,775	0.14	0.35	2,263	0.13	0.33	1,512
Dendi	0.06	0.23	3,775	0.05	0.21	2,263	0.07	0.26	1,512
Fon	0.29	0.45	3,775	0.31	0.46	2,263	0.25	0.43	1,512
Yoa and Lokpa	0.05	0.22	3,775	0.05	0.22	2,263	0.05	0.22	1,512
Betamaribe	0.07	0.25	3,775	0.07	0.25	2,263	0.07	0.25	1,512
Peulh	0.13	0.33	3,775	0.10	0.30	2,263	0.16	0.37	1,512
Yoruba	0.08	0.28	3,775	0.08	0.28	2,263	0.08	0.28	1,512
Other Beninoise	0.03	0.16	3,775	0.02	0.14	2,263	0.04	0.19	1,512
Other nationality	0.00	0.07	3,775	0.00	0.06	2,263	0.01	0.08	1,512
HH wealth quintile									
1 (poorest)	0.23	0.42	3,775	0.24	0.43	2,263	0.21	0.41	1,512
2 (poorer)	0.28	0.45	3,775	0.27	0.44	2,263	0.29	0.45	1,512
3 (middle)	0.27	0.44	3,775	0.26	0.44	2,263	0.29	0.45	1,512
4 (richer)	0.17	0.37	3,775	0.17	0.38	2,263	0.16	0.37	1,512
5 (richest)	0.05	0.23	3,775	0.06	0.24	2,263	0.05	0.21	1,512