The Impact of Weather Shocks on the Effectiveness of Child Marriage Bans

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Abstract

Despite legal reforms, child marriage remains prevalent in Sub-Saharan Africa, where 31% of women aged 20–24 are married before the age of 18. This persistence suggests that legislation alone is insufficient to eliminate the practice. Economic and social pressures jointly sustain child marriage, with financial constraints playing a particularly critical role in regions where the bride's family receives a financial transfer from the groom upon marriage (a "bride price"). Marrying off daughters early can serve as an economic coping strategy, especially during times of income shock, as it accelerates the receipt of marriage payments. This paper examines whether economic hardship undermines the effectiveness of child marriage bans. Combining household survey data with rainfall shocks and ethnographic information across 12 African countries, I estimate the impact of droughts, used as a proxy for negative income shocks, on the effectiveness of these bans. Using a difference-in-differences approach that leverages variation in exposure to bans across subnational regions and birth cohorts, I find that child marriage bans reduce the probability of marriage before ages 15 and 18. However, these effects are concentrated in urban areas. In rural areas, the bans do not lower the incidence of child marriage but delay the age at first marriage and childbirth. In areas where bride price is practiced, the bans decrease early marriage in both rural and urban settings under stable economic conditions, but their impact is significantly weakened when girls experience droughts during adolescence. These findings highlight the importance of addressing economic vulnerability alongside legal reform to effectively combat child marriage.

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1 Introduction

Child marriage remains a pressing global concern, impacting 640 million women worldwide. Although its occurrence is decreasing in South Asia, Sub-Saharan Africa now has the highest rates, with 31% of women aged 20-24 married as children. Each year, 12 million girls marry before turning 18, with Sub-Saharan Africa accounting for an increasing share as population growth intensifies the problem and subsequently raises the number of child brides. If the pace of decline remains unchanged, the world is at least 300 years away from eradicating child marriage (United Nations Children's Fund, 2023). International organizations emphasize the urgency of addressing child marriage to promote women's empowerment and support global development because child marriage has severe consequences, including restricted labor market participation, increased domestic violence, poor health outcomes, and lower education levels, perpetuating cycles of poverty and disempowerment.[1] However, weak enforcement of laws banning child marriage is common, with an estimation of 7.5 million girls unlawfully married each year (Wodon et al., 2017), with most countries showing limited improvements as regards enforcement (Collin & Talbot, 2023). Families may continue to marry their daughters early for social reasons, such as preserving their virginity or complying with community norms. Nevertheless, recent economic research also underscores the role of financial factors in the persistence of child marriage, especially in Sub-Saharan Africa, where the groom traditionally makes a payment to the bride's family upon marriage (the bride price). In such contexts, households can resort to child marriage as a strategy to cope with negative economic shocks, marrying daughters at younger ages to expedite the receipt of marriage payments and smooth consumption (Corno et al., 2020). As a result, early marriage practices can be particularly difficult to shift when complying with legal bans imposes financial strain on families. This suggests that legal measures may be less effective in settings where child marriage serves as a risk-coping mechanism in times of financial hardship. It underscores the importance of considering not only the direct effects of child marriage legislation but also how its impact is shaped by traditional practices and the economic environment in which marriage decisions are made.

[1] Chari et al. (2017); Field & Ambrus (2008); Garcia-Hombrados (2022); Hicks & Hicks (2019); Jensen & Thornton (2003); Parsons et al. (2015); Raj (2010); Sekhri & Debnath (2014); Sunder (2019)

To address this question, I study the impact of exogenous income shocks, approximated by weather shocks, on the effectiveness of child marriage bans in 12 African countries. Following the UNICEF, I define child marriage as any formal marriage or informal union between a child under the age of 18 and an adult or another child. I combine rainfall data from the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS) between 1981 and 2022 with marriage information from Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS). I gathered data on the age of marriage and the history of rainfall shocks for approximately 160,000 women. To justify the use of rainfall shocks as a proxy for income shocks, I merge the CHIRPS data with historical crop yield data from the Food and Agriculture Organization (FAO) and Iizumi & Sakai (2020) and examine the impact of rainfall shocks on agricultural output. I demonstrate that droughts, characterized by rainfall during the rainy season falling below -1 or -2 standard deviations from the local rainfall distribution, significantly reduce agricultural production, making it a reliable proxy for an adverse income shock. Data on legislative changes in the minimum legal age of marriage come from the WORLD Policy Analysis Center, which gathered various sources to establish the minimum age at which a girl can be married (WORLD Policy Analysis Center, 2023). I complement this analysis with information on historical marriage payment practices based on Murdock (1967)'s *Ethnographic Atlas* and Murdock (1959)'s *Tribal Map of Africa* to evaluate the differential impact of droughts depending on varying cultural practices.

I employ a within-country difference-in-difference-like analysis that exploits sub-national and cohort variation in exposure to child marriage bans to measure the effect of these laws. A woman's exposure to a ban is determined by her age at the time of the ban and the incidence of child marriage in her region before the reform. This regional component, referred to as the "ban intensity," is defined as the average pre-ban rate of child marriage, with or without weighting for how far below age 18 marriages occurred. This measure captures the fact that raising the minimum legal age of marriage to 18 has a varying treatment intensity across regions, depending on the baseline average age at marriage. After controlling for region and cohort of birth fixed effects, the interaction between the dummy variable indicating that the girl was younger than 18 years old at the time of the ban and the intensity of child marriage in her region before the law is a plausibly exogenous variable. Analogous strategies were employed in settings with similar natural experiments (Card, 1992; Duflo, 2001; García-Hombrados & Salgado, 2023; Wilson, 2022). Estimates compare the change in the likelihood of child marriage associated with the timing of the ban in regions where the pre-ban age at marriage was low to the synchronous change in regions where it was high. For an omitted factor to explain these findings, it must change at the same time as the legislation in each country and change differentially in regions with different pre-ban ages at marriage. In addition, I interact this continuous measure of ban intensity with weather shocks that occurred during the years a girl is at risk of entering child marriage (*i.e.* adolescence) to identify whether exogenous income shocks hamper the effectiveness of bans. This analysis relies on the assumption that the timing of weather shocks is orthogonal to potential confounding factors.

My findings indicate that child marriage bans reduce the likelihood of child marriage in urban areas. A one standard deviation increase in the ban intensity reduces the likelihood of child marriage by almost 3 percentage points, or approximately 13% compared to the sample mean. In regions where the pre-law prevalence was 45% (pre-ban regional mean), the estimated reductions are 5.22 percentage points for marriage under 18 and 3.33 percentage points for marriage under 15, corresponding to declines of 23.7% and 55.5%, respectively, relative to the baseline mean. The event study analysis shows that the younger the girl is at the time of the ban, the greater the effect. In rural areas, while the bans do not significantly reduce the likelihood of child marriage, they increase the average age at first union and first birth. Specifically, a one standard deviation increase in ban intensity delays the age at first union by 3.5 months and the age at first childbirth by 4 months. In urban areas, the same increase in ban intensity delays the age at first union and first childbirth by 8 months. Bans also have a positive impact on education in urban areas, with a one standard deviation increase in ban intensity extending education by 5.4 months. Consequently, the bans help reduce the age and education gaps within couples.

In regions where bride price has been historically practiced, child marriage bans decrease early marriage in both rural and urban areas when no drought occurs during adolescence. A one standard deviation increase in ban intensity reduces the likelihood of child marriage by 1.9 percentage points in the absence of drought, lowering the rate from 28% to 26.1%. This corresponds to a 6.8% reduction in the likelihood of child marriage. However, this effect is significantly weakened when girls are exposed to droughts during adolescence. As a result, the overall impact of the ban is diluted, with child marriage decreasing by only 1 percentage point on average. The more severe the drought, the greater its capacity to undermine the effectiveness of the ban. By contrast, positive weather shocks enhance the effect of the law. Among non–bride price groups, child marriage bans are generally ineffective, and droughts do not affect their effectiveness.

The interaction between droughts and child marriage bans is specific to adolescence, as droughts occurring during early childhood (ages 0 to 9) or adulthood (ages 18 to 25) do not affect the effectiveness of bans. The income shock mechanism is absent when banning child marriage for boys. My results are robust across several tests, including the exclusion of migrant women, the exclusion of countries where women might be more likely to manipulate their age at first union after the ban, alternative measures of ban intensity and income shocks, and placebo ban years. Additionally, I present evidence for a distinct mechanism related to state capacity. Both droughts and geographic remoteness reduce the effectiveness of child marriage bans, but through different channels. Droughts highlight the role of economic vulnerability, while remoteness reflects mainly weaker enforcement capacity. These factors do not appear to interact, indicating that economic hardship and limited state presence act as separate constraints on the success of legal reforms.

While child marriage is undeniably harmful to girls and women, families facing financial stress continue to resort to it as a coping mechanism, even after the practice is made illegal. These findings highlight the critical need to complement child marriage bans with targeted anti-poverty measures to address the issue more effectively. Potential interventions include expanding access to credit markets, strengthening social safety nets, and promoting the adoption of insurance against negative weather shocks. Nevertheless, even in the absence of financial hardship, bans alone have proven insufficient to fully eliminate the practice. This underscores the importance of also addressing the structural, social, and cultural norms that sustain early marriage.

This article makes several contributions to the existing literature. Firstly, it expands on the body of research documenting how the effectiveness of institutions depends on economic prosperity. The literature presents that economic prosperity impacts institutions by shaping individual behaviors and by influencing the state's capacity to enforce laws. A well-established stream in the literature demonstrates that economic conditions influence crime, beginning with Becker (1968), who framed crime as a rational response to economic needs (Danziger & Wheeler, 1975; Dix-Carneiro et al., 2018; Draca & Machin, 2015; Fajnzylber et al., 2002a;b; Mocan & Rees, 2005; Rosenfeld & Fornango, 2007). Likewise, Sen (1999) argues that financial constraints can lead individuals to engage in illegal practices, such as corruption. More recently, Banerjee & Duflo (2011) show that poverty often limits choices, forcing individuals to make drastic decisions in response to negative shocks. Alongside these individual effects, economic downturns can also weaken governance, notably by increasing corruption and eroding trust in political systems (Rhein & Jansesberger, 2024; Wenzel, 2021; World Meteorological Organization, 2022).[2] This article contributes to this literature by demonstrating that the effectiveness of institutions in curbing harmful practices, such as child marriage, is weakened by financial constraints, underscoring the role of economic stability in supporting individual compliance and state enforcement of policies.

Secondly, this paper contributes to the debate on the effectiveness of bans as a top-down approach to addressing child marriage. To date, there is no clear consensus on the effectiveness of laws banning child marriage, with evidence generally observing limited or no impact, especially outside urban areas (Batyra & Pesando, 2021; Collin & Talbot, 2023; McGavock, 2021; Wilson, 2022). I contribute to this literature by showing that the continued use of child marriage as a coping mechanism in response to economic shocks helps explain the limited impact of these bans among communities that practice the bride price.

Thirdly, I provide evidence on the malleability and flexibility of marriage as a coping mechanism. The existing literature offers conflicting evidence on how changes in economic incentives influence marriage behaviors (Autor et al., 2019; Brandt et al., 2016; Corno et al., 2020;

[2]Studies have shown that public trust in political institutions tends to decrease following droughts (Rhein & Jansesberger, 2024), and that corruption levels rise during economic hardship (Wenzel, 2021). The World Meteorological Organization (2022) also highlights that climate shocks weaken governance by straining resources, disrupting infrastructure, and intensifying social vulnerabilities.

Corno & Voena, 2023; Hoogeveen et al., 2011; Hotte & Lambert, 2023; Molotsky, 2019; Muchomba, 2021; Schaller, 2013). However, as early as Rosenzweig & Stark (1989)'s seminal paper, marrying off daughters was viewed as an insurance mechanism to mitigate agricultural income risks. Consistent with these findings, declining household incomes due to weather shocks have been shown to increase the likelihood of child marriage in households practicing bride price (Corno et al., 2020; Hoogeveen et al., 2011; Molotsky, 2019; ?).[3] I contribute to this literature by showing that this coping strategy remains active even after the introduction of legal bans, underscoring the limits of legal reform in the absence of economic support.

Finally, I contribute to the literature that emphasizes the role of ethnic and cultural identity in shaping responses to economic changes and policy environments (Ashraf et al., 2020; Chen, 2010; Corno et al., 2020; McGavock, 2021; McGavock & Novak, 2023). My findings reveal heterogeneous responses to droughts under child marriage bans across groups with differing cultural practices. This also augments the broader literature on the interaction between ethnocultural norms and economic conditions (Ashraf et al., 2020; Bau, 2021; Becker, 2023; Corno et al., 2020; Corno & Voena, 2023; La Ferrara & Milazzo, 2017; McGavock & Novak, 2023).

The rest of the paper is organized as follows: Section 2 provides background information on child marriage, legal bans, and marriage payments. Section 3 details the data used in the analysis. Section 4 explains the empirical strategy. Sections 5 and 6 present the main empirical findings and robustness checks. Finally, Section 7 offers concluding remarks.

2 Institutional Background

2.1 Child Marriage Bans

The 1948 *Universal Declaration of Human Rights*, established by the United Nations, is the foundational international legal basis for prohibiting child marriage. This declaration asserts that men

[3] However, this mechanism does not hold uniformly, especially in contexts where multiple marriage payment systems coexist and generate conflicting incentives, or where other economic constraints are at play (Hotte & Lambert, 2023).

and women of legal age possess equal rights in marriage and that marriage requires the free and full consent of both parties (United Nations, 1948). Subsequent international agreements have further reinforced this legal framework. The 1962 *United Nations Convention on Consent to Marriage, Minimum Age for Marriage, and Registration of Marriages* mandates that signatory nations establish a legal minimum marriage age and ensure the registration of all marriages (United Nations, 1962). Article 16(2) of the 1979 *United Nations Convention on the Elimination of All Forms of Discrimination Against Women* (CEDAW) declares that "The betrothal and the marriage of a child shall have no legal effect," and calls for all necessary actions, including legislation, to specify a minimum marriage age and make marriage registration compulsory (United Nations, 1979). The 1989 *United Nations Convention on the Rights of the Child* formally defines a child as an individual under the age of 18 (United Nations, 1989). Lastly, the 1995 *Beijing Declaration and Platform for Action* urged nations to enforce a legal minimum age for marriage, reaffirming 18 as the minimum age (United Nations, 1995). Following these initiatives, many countries introduced laws banning child marriage. In 1995, more than 80% of countries allowed marriage for girls under 18; by 2019, this figure had dropped to 40% (WORLD Policy Analysis Center, 2023).

Despite these agreements, global support remains inconsistent. Only 72 of the 193 United Nations member states participate in the 1962 *Convention on Consent to Marriage, Minimum Age for Marriage, and Registration of Marriages* (United Nations, 2024a). Similarly, while 189 member states are party to the 1979 *Convention on the Elimination of All Forms of Discrimination Against Women*, many have issued declarations and reservations regarding minimum age at marriage provisions, often signaling non-compliance with the United Nations' standard of setting the legal minimum age of marriage at 18 for females (United Nations, 2024b).

Moreover, the mere enactment of legislation banning child marriage does not guarantee effective enforcement. Both qualitative and quantitative evidence reveal significant gaps in implementation, particularly in rural areas. Wodon et al. (2017) estimate that at least 7.5 million girls are unlawfully married every year. Wilson (2022) show that child marriage bans are more effective in urban areas, and Collin & Talbot (2023) note that many countries display weak enforcement of these bans, with little improvement over time.

Nonetheless, while criminalization measures—such as fines and imprisonment—may force individuals to delay their daughters' marriages, legal bans can also lead people to reconsider their beliefs and voluntarily comply with the law. This idea is supported by the findings of Fernández et al. (2024), who identified such pattern in the context of same-sex relationships in the U.S. Their research demonstrates that when individuals are repeatedly exposed to public debates and awareness around an issue, they are more likely to reassess and change their beliefs.

2.2 Determinants of Child Marriage

Several factors contribute to the persistence of child marriage, ranging from cultural norms to economic hardship. For many parents, early marriage is perceived as a way to protect their daughters from risks such as sexual assault or unwed pregnancy, both of which can harm a girl's reputation and diminish her future marriage prospects (Myers, 2013). Cultural preferences for younger brides—often rooted in beliefs about fertility, obedience, and social control—also sustain the practice (Field & Ambrus, 2008).

At the same time, economic conditions are deeply intertwined with child marriage, particularly among disadvantaged households (Harper et al., 2014; Klugman et al., 2014). While the past 25 years have seen a decline in child marriage across Sub-Saharan Africa, this progress has been uneven: rates have dropped significantly among wealthier families but have stagnated or even increased among the poorest (United Nations Children's Fund, 2023). These patterns highlight the importance of understanding the economic dimensions of child marriage.

In what follows, I examine these economic determinants in greater depth, beginning with the role of marriage payments. I then turn to other financial pressures before exploring the cultural and social norms that shape marriage decisions.

2.2.1 Marriage Payments

In Africa, marriage payment customs provide financial incentives for early marriage. Most communities on the continent follow the bride price tradition, where the groom's family makes payments to the bride's family in the form of livestock, money, or other valuables.[4] Evidence documents that the bride price initially rises and then declines with the bride's age, making early marriage financially advantageous for parents (Hoogeveen et al., 2011). Moreover, Corno et al. (2020) document that negative income shocks increase the likelihood of child marriage, as parents marry off their daughters earlier to secure the bride price sooner, leading to more child marriages during economic downturns. Their findings show that negative income shocks, approximated by droughts, increase the likelihood of child marriage in Sub-Saharan Africa by 3%, with the rise concentrated among ethnic groups that practice bride price payments.[5] ? add that daughter marriage, used to mitigate negative income shocks in provinces with a high prevalence of bride price, is not limited to the poorest countries and remains part of household strategies in Turkey. Qualitative evidence from NGOs further reinforces these findings, highlighting how recent droughts in Southern and Eastern Africa have placed increased pressure on families, leading many to marry off their daughters in exchange for resources (ActionAid, 2024; Parry, 2025).

Corno et al. (2020) developed a theoretical model to explain the mechanisms at play in bride price systems. They demonstrate that under such a custom, the supply of brides increases as aggregate income decreases, as more families feel financial pressure to marry off their daughters. Meanwhile, the demand for brides decreases because fewer families can afford to pay the bride price. As a result, the model predicts that the bride price decreases when aggregate income falls. Consequently, negative shocks lead to more marriages for two reasons: first, more families are willing to marry off a daughter in case of a negative income shock because it provides financial relief to the bride's family through the receipt of a bride price, even if the amount is smaller. Second, some men are still able and willing to marry during economic downturns because the

[4] Appendix A1.3 explains the historical origins of the bride price.

[5] In Corno et al. (2020), a drought is defined as an annual rainfall realization below the 15th percentile of the local rainfall distribution. Using the historical practice of the women's reported ethnicity, they find that droughts significantly reduce the likelihood of child marriage among girls from ethnic groups that practice bride price, while they have no significant effect on girls from groups that do not. However, the null hypothesis of equality between the two coefficients cannot be rejected.

bride price is reduced, making marriage more affordable. This mechanism is further reinforced by differences between households. Families willing to marry off daughters during negative shocks differ from grooms who are still willing to marry under such conditions. Intuitively, the poorest households want to receive the bride price, and the richest ones are willing to pay it.[6]

Nevertheless, this mechanism is not observed in all contexts where bride price is practiced. Hotte & Lambert (2023) find that in Senegal, where most marriages involve a bride price, the lack of rain during the agricultural season tends to postpone weddings. While the literature observes that, on average, droughts reduce the likelihood of child marriage in Africa (Corno et al., 2020), this overall effect likely masks important disparities, driven by other underlying financial pressures and non-economic factors that influence marriage decisions.

2.2.2 Other Financial Factors

Beyond bride price, families often consider a range of financial factors when deciding whether—and when—to marry off their daughters.

For many poor households, the cost of educating a daughter can be prohibitively high. Direct expenses like school fees, uniforms, and supplies, as well as indirect costs such as the loss of a girl's labor at home, make schooling economically difficult. Marrying a girl off early removes these education expenses. Evidence shows that when education is made more affordable or free, families are more likely to keep girls in school and delay marriage, indicating that high schooling costs push girls into early marriage (Malhotra & Elnakib, 2021a; United Nations Children's Fund, 2021a).[7] In contexts with no access to credit or limited economic opportunities for girls and

[6] Corno et al. (2020) explain that when the supply and demand for brides are equally responsive to changes in income and price, negative income shocks reduce prices but do not affect the quantity of marriages. However, if the supply of brides is more sensitive to income changes than the demand, lower income leads to more marriages. This asymmetry occurs because, under virilocality, sons continue to contribute to their parents' household after marriage, unlike daughters. Thus, marriage payments have a smaller impact on a man's family's continuation utility compared to a woman's family.

[7] Reducing education costs—through tuition fee elimination (Blimpo et al., 2019; Brudevold-Newman, 2021; Duflo et al., 2021), provision of supplies such as bicycles (Fiala et al., 2022; Muralidharan & Prakash, 2017), or conditional cash transfers such as scholarships (Benhassine et al., 2015; Filmer & Schady, 2014; Giacobino et al., 2024; Giordono & Pugatch, 2017; ?)—can increase girls' school participation and delay marriage, highlighting that high schooling costs contribute to early marriage.

women, early marriage can be used as a strategy to reduce household expenditures on education.

Early marriage can also reduce the pressure on a girl's family by transferring the responsibility for her care and daily needs to her husband's household. In many settings, particularly in patrilocal societies where daughters move to their husband's household after marriage, this transition means one fewer person to feed, clothe, and support. As a result, the everyday costs of the girl's upkeep shift away from her natal family. If the family expects little economic gain from the girl's labor or future earnings, poor households may perceive postponing a girl's marriage as a lost savings.[8] In contrast, marrying her off can bring an instant economic benefit (like bride price or reduced household burden). However, at the same time, any domestic labor or caregiving she was providing at home is transferred to her marital household. In the end, there is an implicit arbitrage depending on whether the girl is primarily a consumer or a contributor within her natal household: if she contributes more—through domestic work or care—than she consumes, the family loses a source of labor; if she consumes more than she contributes, early marriage alleviates a net burden.

These economic motivations are heightened in times of crisis. Negative income shocks—such as droughts, job losses, or surging food prices—make it harder for families to cover school expenses or support an additional member at home. In these situations, early marriage may be seen as a necessary coping strategy. Conversely, when girls are net providers, and their economic contributions remain stable despite a negative income shock, families may prefer to delay marriage in order to retain their support. Moreover, in times of economic hardship, the cost of marriage ceremonies can become prohibitive, leading families to postpone marriages for financial reasons.[9] As a result, different incentives can pull in opposite directions, and the decision to marry off a daughter early or not may ultimately depend on which economic logic prevails within the household.

[8] Sabbe et al. (2015) show that one of the most quoted factors leading to a forced marriage in Morocco is financial dependence on the father, or the household in general, which plays a larger role in poor families. Moreover, Ahmed et al. (2019)'s qualitative research in Bangladesh suggests that marrying out a daughter reduces household expenses. This finding is further corroborated by research with Palestine refugees which found that girls were eager to get married at a young age to escape their natal home in part due to crowded and poor living conditions, but also due to a desire to relieve the natal family from financial burden (Knox, 2017)

[9] In the context of Senegal, Hotte & Lambert (2023) show that negative weather shocks tend to delay weddings, likely because families are unable to afford the associated ceremonial expenses.

Positive income shocks, by contrast, can reduce financial stress and make it more feasible for families to invest in girls' education and delay marriage. Yet at the same time, they may also give households the financial margin needed to afford marriage ceremonies. As with negative shocks, the net effect depends on the balance of competing motivations within the family.

How prospective husbands respond is shaped by similar economic considerations: their decisions to marry or postpone marriage depend on individual exposure to shocks, wealth, and perceived costs and benefits of union formation. Yet, empirical evidence on how men adjust their marital behavior in response to economic shocks remains limited.[10]

2.2.3 Cultural and Social Factors

Child marriage is not driven by economic factors alone. Research documents that early marriage is also rooted in gendered expectations, concerns about family honor, community pressure, perceptions of protection, and religious or traditional beliefs (Malhotra & Elnakib, 2021a;b; Nour, 2006; 2009; Petroni et al., 2017). These social and cultural factors are typically rooted in long-standing norms and beliefs, and are not expected to adjust directly in response to short-term economic shocks or financial constraints.

Girls are often valued primarily for their roles as wives and mothers, rather than for their potential in education or paid work. This perception reduces incentives to delay marriage and results in limited investments in girls' schooling or autonomy. Marriage at a young age is also seen as a way to socialize girls into these gendered roles. Younger brides are often expected to adapt more easily to household responsibilities and to conform to subservient roles within their marital households, including toward in-laws (Women, 2013).

Families may also seek to avoid social stigma by ensuring their daughters marry before reaching an age where premarital relationships become a concern. In these contexts, a girl's sexuality is closely tied to her family's reputation, and early marriage is viewed as a way to preserve both (Ertem & Kocturk, 2008; Sabbe et al., 2015). These concerns are reinforced by beliefs about

[10] Tapsoba (2024) shows that in Sub-Saharan Africa, droughts increase the marriage market share of young men seeking a first wife, while reducing that of older men seeking a junior wife.

fertility: some families believe that delaying marriage or childbirth may increase the risk of infertility or lead to shame if a girl cannot demonstrate fertility shortly after puberty (Henry et al., 2015). Social norms at the community level further shape these decisions. In areas where early marriage is widely practiced, it becomes a shared expectation, and families may feel social pressure to conform. Parents often fear that delaying a daughter's marriage could attract criticism, even if they would prefer to postpone it themselves.[11]

In high-risk settings, such as conflict zones or areas with widespread gender-based violence, early marriage is sometimes considered a protective strategy. Families may arrange marriages to reduce girls' exposure to sexual violence or insecurity, believing that a husband can provide physical safety and social legitimacy (Hutchinson et al., 2016). In some cases, early marriage is also viewed as a way to prevent sexually transmitted infections, particularly HIV/AIDS, by limiting girls' sexual activity to within marriage (Nour, 2006).

Finally, religious and traditional norms play an important role. While no major religion explicitly mandates child marriage, certain interpretations of Islamic and Christian teachings are used to justify it. These interpretations often emphasize the importance of preventing premarital sex and ensuring that childbearing occurs within a sanctioned union (Addai, 2000). Beyond religious justifications, some families continue to arrange child betrothals as a way to reinforce kinship ties and forge family alliances.[12] In addition, cultural rituals—such as initiation ceremonies or female genital mutilation—can influence the timing of marriage by marking a girl's transition to womanhood and triggering social expectations of marriage soon after (United Nations Children's Fund, 2021b).

[11]Qualitative evidence from Cameroon indicates that parents fear their daughters remaining unmarried while peers marry, reflecting anxiety about deviating from community standards (Cislaghi et al., 2019).

[12]Qualitative evidence from Sudan highlights that child marriages are triggered by a strong preference for intra-kinship and intra-village marriages (Dean et al., 2019).

3 Data

3.1 Individual Data

The primary data source for this analysis is the DHS surveys, which are household-level surveys conducted in developing countries and are representative at the national, regional, and residence (urban-rural) levels. I compiled all publicly available DHS data from African countries that had enacted laws prohibiting female child marriage[13] and where surveys with geocoded data were available both before and after the implementation of these laws.

Geo-referenced data within the DHS datasets are critical because they provide the geographical coordinates of each DHS cluster, which consists of groups of villages or urban neighborhoods. These coordinates are essential for identifying pre-ban child marriage intensity, rainfall shocks, and historical marriage payment practices in a woman's place of residence, which are key variables in my analysis.[14] DHS region boundaries are used as the primary geographical unit of analysis to aggregate local information on weather shocks and child marriage intensity before bans, as the surveys are representative at the regional level. This aggregation also minimizes the risk of measurement error related to migration, as women in Africa primarily migrate for marriage, and marriage markets are typically concentrated within geographic areas smaller than the DHS region.[15] This data collection effort resulted in the inclusion of 24 surveys from 12 countries, covering 210 regions.[16] A detailed breakdown of the countries and survey periods included in the analysis is presented in Appendix Table A.1.

Surveys date first unions using the year and month when women report having started

[13] I focus on laws banning female child marriage because this practice predominantly affects girls, and historically, the legal minimum age of marriage for men has been 18 or higher in most countries.

[14] Using the geolocation of clusters instead of relying solely on the declared region of residence or ethnicity helps address issues related to missing data and inconsistencies in data harmonization. Regional boundaries can change between survey years, and some surveys do not include information on individual ethnic groups. Furthermore, there is no perfect correspondence between DHS ethnic groups and historical data on marriage payment practices.

[15] In cases where boundaries have changed over time, priority is given to the delineations that correspond to the pre-ban survey, ensuring the representativeness of pre-ban child marriage intensities at the regional level.

[16] In Malawi, child marriage was banned in 2015, but no DHS survey is available for the post-ban period. I therefore use the most recent MICS survey instead. As this 2020 MICS round and the 2010 pre-ban DHS survey are representative at both the district and urban/rural levels, I align them with district boundaries.

living with their first husband or partner. This cohabitation date is the one I use to define the first union. Dating marriages this way is especially relevant when studying child marriage, as it captures the actual onset of marital life—the point at which the social and economic consequences for girls typically begin. Since child marriage laws aim to prevent early unions and their effects, cohabitation provides a more accurate measure of the behaviors and outcomes these policies are meant to influence.

Descriptive statistics on child marriage from post-ban surveys are presented in Table 1 and Figure 1. On average, 33% of women surveyed were married before the age of 18, 9% before 15, with the average age at first union being 19.[17] Women are, on average, 8 years younger than their husbands. Pre-ban child marriage rates vary widely across countries, ranging from 26% in Kenya to 69% in Guinea, as shown in Appendix Table A.1. At the regional level, child marriage rates range from 10% to 92% (Appendix Table A.12). Figure A.1 shows the distribution of the age at first union by country. In most countries, the distribution is centered around age 18. In Ethiopia, Guinea, and Nigeria, the distribution is instead centered around age 15.

Table A.2 examines the relationship between rural residence and marriage outcomes across the 12 countries in the sample. Rural residence is strongly associated with earlier union formation: on average, 39% of women in rural areas marry before the age of 18, compared to 21% in urban areas. Women in rural areas enter their first union about two years earlier than their urban counterparts, and their partners also do so approximately 1.5 years earlier. The average age gap between spouses remains relatively stable across settings, at around 7.5 years. These patterns hold consistently across all countries in the sample.

3.2 Legislative Changes in Minimum Legal Age of Marriage

Data on changes in the minimum legal age of marriage are sourced from the WORLD Policy Analysis Center, which conducted an extensive review of various legal repositories to determine

[17] Data on women's age at first marriage is collected retrospectively during interviews, where participants are asked to recall the age, month, and year of their first cohabitation. Interviews are conducted with women aged 15 to 49 in the household, regardless of their marital status. The analysis focuses on women aged 18 to 40 at the time of the interview to diminish recall bias.

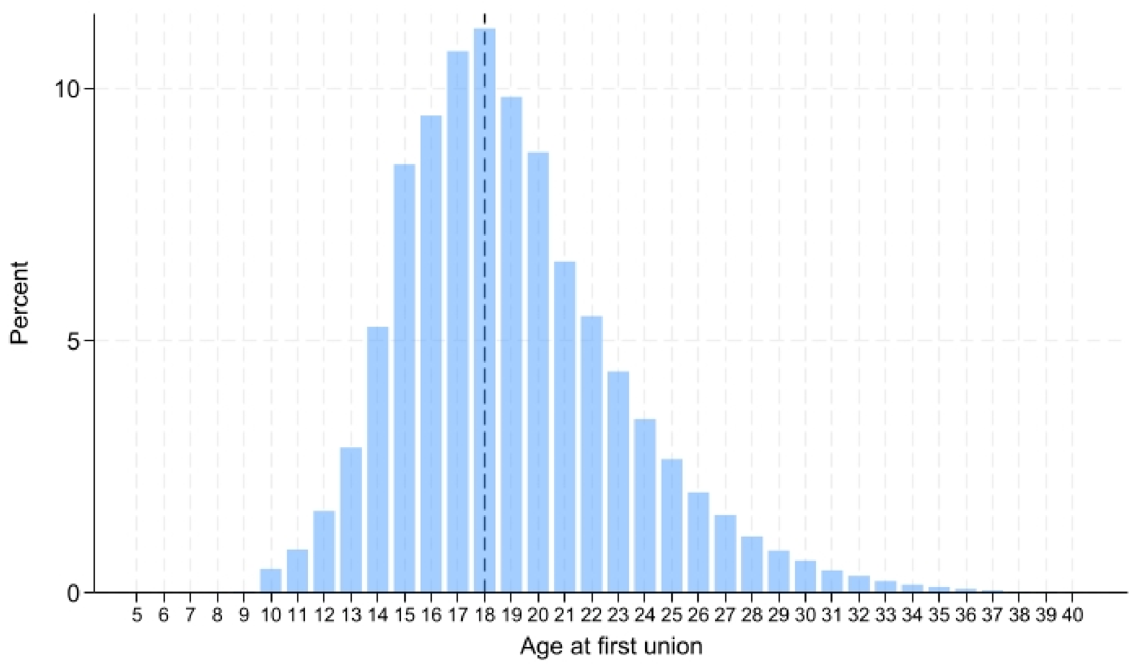
Table 1: Descriptive statistics on individual data

Variable	Obs	Mean	Std. dev.
Rural	162,629	0.64	0.48
Age	162,631	28.03	6.57
Ever in union	162,630	0.82	0.39
Married before 18	162,631	0.33	0.47
Married before 15	162,631	0.09	0.29
Age at first union	132,957	19.01	4.23
Age at first birth	131,344	19.46	3.81
Years of education	145,140	6.15	5.18
Partner's characteristics			
Age	107,992	36.57	9.11
Age difference	107,992	7.60	6.65
Years of education	107,767	6.86	5.49
Age at union	107,629	26.50	7.20

Notes: Sample of women aged 18-40 years old.

Sources: DHS and MICS (post-ban surveys)

Figure 1: Distribution of the age at first union



Notes: This histogram shows the distribution of ages at first union. The red vertical dashed line indicates age 18. The part of the distribution to the left of the red line represents girls who entered their first union before turning 18, while the part to the right represents those who entered their first union at 18 or later. Each bar reflects the percentage of women who experienced their first union at a given age. Sample of women aged 18-40 years old.
Sources: DHS and MICS (post-ban surveys)

the minimum age at which boys and girls can marry across 193 countries as of May 31st, 2023 (WORLD Policy Analysis Center, 2023).[18] The database captures national-level legislation, and in cases where marriage age regulations are set at the state or provincial level, the coding reflects the state or province with the lowest minimum age provisions.

The legal age of marriage and the average age of marriage are lower for females than males in most countries. Thus, I focus on changes in the legal minimum age at marriage for females. The historical trend illustrates a significant shift: in 1995, around 60% of countries prohibited child marriage for boys, while only about 20% had similar provisions for girls. By 2020, nearly 80% of countries had enacted laws banning child marriage for boys and approximately 60% for girls (Appendix Figure A.2).

The sample does not include countries that did not allow female child marriage as of 1995. Unfortunately, the dataset lacks information on the legal minimum age at marriage before 1995, which prevents the identification of legislative changes implemented before 1996. Likewise, countries that had not prohibited child marriage for girls as of May 31st, 2023, are not included in the analysis due to the same limitation.

3.3 Weather Data

Weather shocks are documented using data from the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS). The main advantages of CHIRPS are the extensive temporal coverage, spanning over 35 years from 1981 to the near-present, and high spatial resolution of 0.05 degrees, covering all land areas. I aggregate precipitation data at the DHS region level by summing monthly regional average precipitation across years and seasons, producing observations at the $region \times year$ and $region \times season$ levels.[19] In my main analyses, I use rainfall during

[18]Sources include the Lexadin World Law Guide, Foreign Law Guide, International Labour Organization (ILO)'s NATLEX database, Pacific Islands Legal Information Institute, Asian Legal Information Institute, JaFBase, and official country websites. For Malawi, I use 2015 as the year of the law instead of 2017, based on the enactment of the Marriage, Divorce and Family Relations Bill (2014), which preceded the constitutional amendment passed in 2017.

[19]Precipitation data is aggregated at the DHS region level to align with child marriage intensity measures, which are also calculated at the regional level due to the representativeness of surveys at this geographical scale. This aggregation minimizes the risk of measurement error related to migration, as women in Africa primarily migrate for marriage, and marriage markets are typically concentrated within geographic areas smaller than the DHS region.

the rainy season to capture drought conditions, but I conduct robustness checks using annual rainfall as well as rainfall during the dry season. Appendix Figure A.3 illustrates the timeline by which yearly and seasonal rainfall realizations are associated with calendar years.

Drought is defined as annual or seasonal rainfall falling below a threshold ranging from -1 to -2 standard deviations from a region's long-term rainfall distribution. This standardized anomaly measure captures deviations from the historical regional average,[20] focusing on proportional changes relative to typical variation rather than absolute magnitude. Although there is no well-established best practice for defining droughts with monthly data, I adopt this definition as it accounts for the correlation between the long-run level and variability of rainfall with present-day income, as highlighted by the literature on the effects of climate on development (Dell et al., 2014).

This approach ensures that all locations have an equal probability of experiencing a shock in any given year or season, assuming rainfall realizations are independent and identically distributed (i.i.d.). Consequently, this measure of shock is inherently uncorrelated with unobserved local characteristics. Although each region has an equal probability of experiencing a shock in any given year or season, rainfall varies over time within each region, causing the identifying variation to arise from the random timing of these shocks.

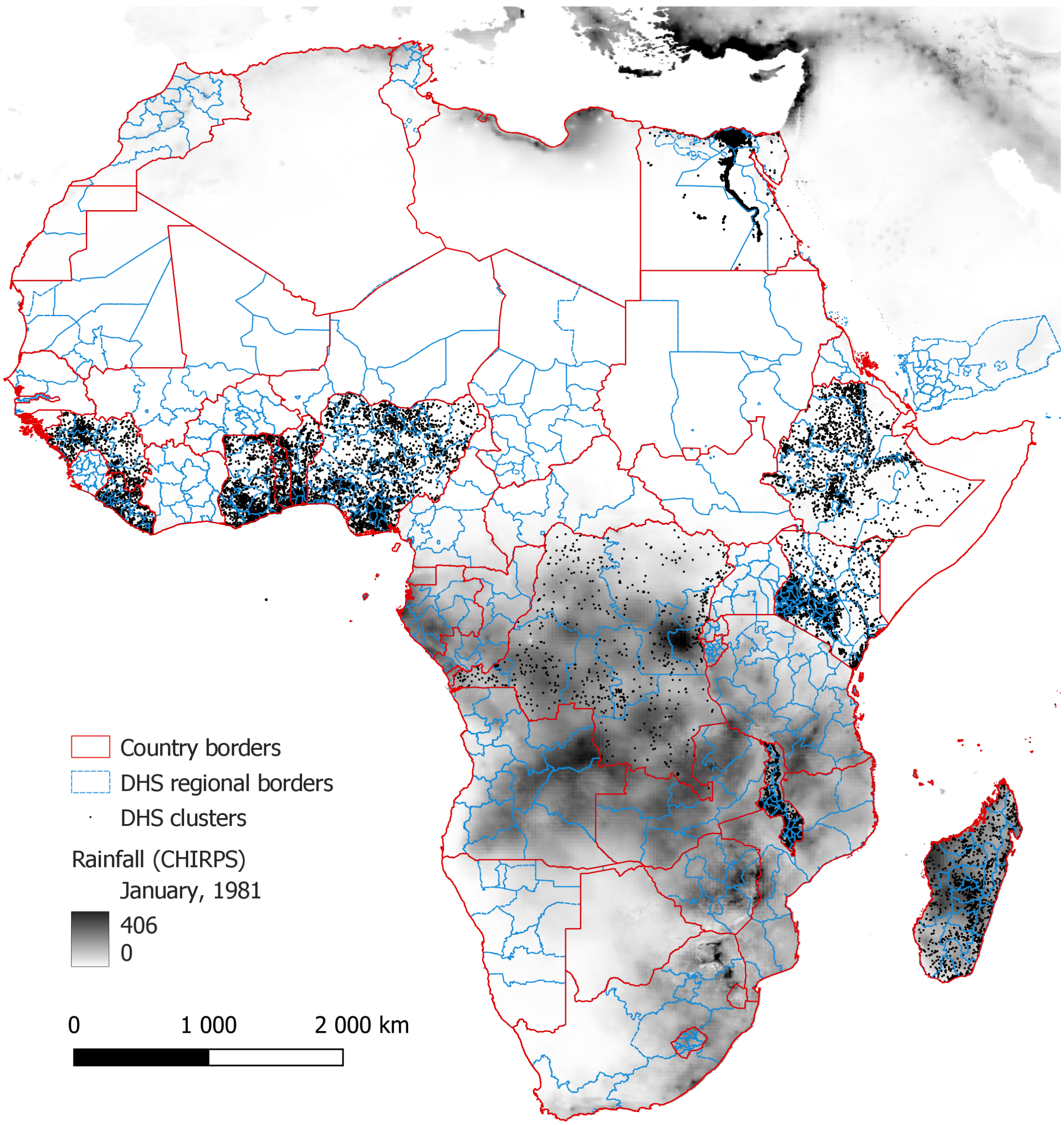
Figure 2 presents the geographical compilation of national and subnational administrative boundaries, survey clusters, and precipitation rasters. Table A.3 reports the average yearly share of regions experiencing droughts during the rainy season across the 12 African countries in the sample. Droughts are defined using three alternative thresholds: rainfall falling below -1, -1.5, and -2 standard deviations from a region's long-term rainfall distribution. On average, 13% of regions experience a mild drought (defined as rainfall <-1SD) in a given year, while only 6% and 2% experience moderate (<-1.5SD) or severe (<-2SD) droughts, respectively.[21]

[20] The historical average is calculated over the 1981-2023 period, consistent with the available CHIRPS data.

[21] While these averages are relatively stable across countries due to the method used to define a drought, there is some heterogeneity. For instance, Togo and Benin report the highest exposure to mild droughts (18% and 17% of regions per year, respectively), while Kenya and Madagascar experience the lowest exposure to severe droughts, with almost no regions affected under the -2 standard deviations threshold every year. In contrast, countries such as the DRC and Egypt exhibit a relatively smaller decline in drought exposure as the definition becomes tighter, indicating that a larger share of their affected regions experience severe rainfall deficits when droughts occur.

Descriptive statistics of weather shocks during adolescence (ages 10 to 17) under various drought definitions are reported in Appendix Table A.4. 55% of women experienced at least one seasonal drought during their adolescence, with a drought defined as a rainy season rainfall realization falling below -1 standard deviation of the region's historical distribution. This percentage decreases to 20% when defining a drought as rainfall below -1.5 standard deviations and to 4% when the threshold is set at -2 standard deviations.

Figure 2: Geographical compilation of datasets



Sources: CHIRPS, DHS regions, DHS and MICS clusters

3.4 Marriage Practices

I rely on historical marriage practices to classify women according to a customary marriage payment system. Specifically, I use the marriage payment tradition of the ethnic group whose historical territory geographically overlaps with the woman's survey location. This allows me to distinguish between bride price–practicing and non–bride price–practicing groups across the African continent.

To do this, I use a digitalized version of the "Tribal Map of Africa," originally published as a fold-out map in the book *Africa: Its Peoples and Their Culture History* (Murdock, 1959)[22]. This map is cross-referenced with data from the *Ethnographic Atlas* (Murdock, 1967)[23]. The *Ethnographic Atlas* provides cultural information on ethnic groups, including details on kinship structure, post-marital residence patterns, marriage payments, and other socio-economic characteristics. One key feature it documents is the "prevailing mode of obtaining a wife" or "mode of marriage," which includes seven categories—one of them being the bride price.[24]

I categorize marriage practices into two groups: bride price-practicing and non-bride price-practicing areas. Figure 3 maps these marriage practices across Africa, showing that bride price is prevalent, although other practices exist, notably in Madagascar, Southern Africa, Ethiopia, Malawi, and the "Matrilineal Belt." After geographically merging the survey clusters with the "Tribal Map of Africa" (Murdock, 1959), I find that approximately 52% of women reside within tribal boundaries where bride price is practiced, and around 30% of women reside within tribal boundaries not characterized by the *Ethnographic Atlas*. The remaining women live in areas where another mode of obtaining a wife prevails (Table A.5, panel B). Only 0.08% of observations are located in places where the alternate mode of obtaining a wife, as characterized by the *Ethnographic Atlas*, is the bride price (Table A.7).

The share of women belonging to ethnic groups that traditionally practice bride price

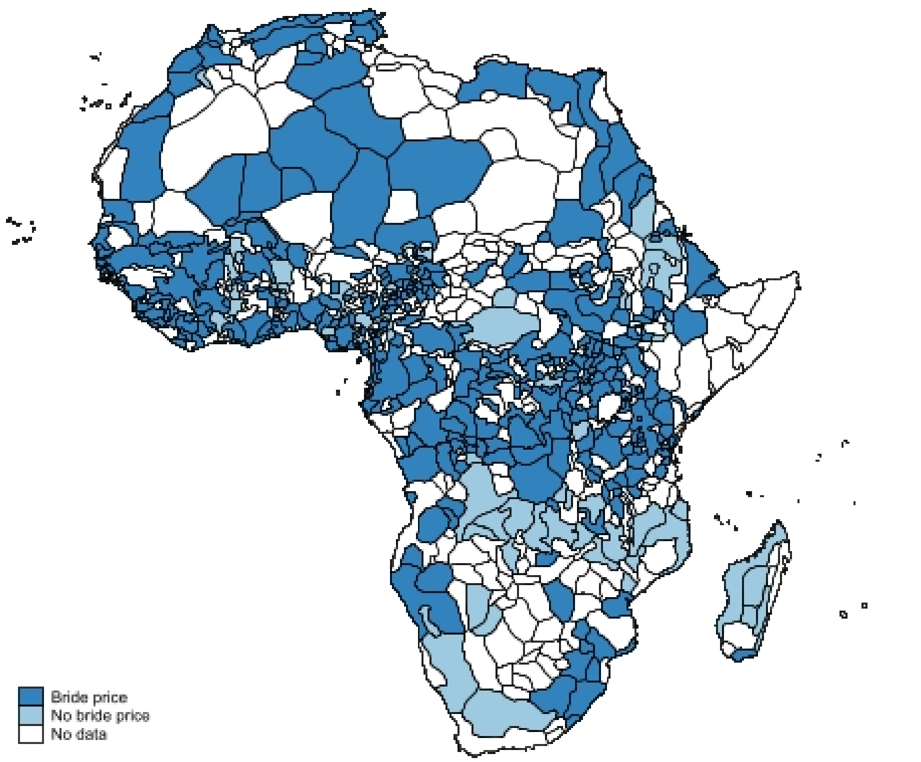
[22] Shapefile accessed from https://nathannunn.sites.olt.ubc.ca/files/2022/02/DATA-2222The-Long-Term-Effects-of-Africas-Slave-Trades2222-Murdock.zip

[23] Data available at https://hub.arcgis.com/datasets/nga::africa-hwm-ethnicity/ or https://africageoportal.maps.arcgis.com/home/item.html?id=d495496a57c1428eaee838f785a163c3

[24] These options are further detailed in Appendix A1.4.

varies substantially across countries: it exceeds 80% in Egypt and the DRC, but falls to just 5% in Madagascar and 15% in Malawi, where token bride price and bride service are more common, respectively (Table A.6). In some countries, such as Ethiopia and Liberia, a significant portion of the population cannot be matched to an ethnographic classification in the *Ethnographic Atlas*, underscoring the limitations of this cross-country anthropological data.

Figure 3: Prevailing mode of obtaining a wife within historical ethnic boundaries



Sources: Murdock (1959; 1967)

Tribes that are mapped but not characterized in the *Ethnographic Atlas* are classified as having an unknown historical marriage payment and, therefore, are excluded from both the bride price- and non-bride price-practicing groups. For these mapped but uncharacterized tribes, I conduct robustness checks following the method of Lowes (2017), which matches them with culturally similar tribes listed in the *Ethnographic Atlas*. This approach uses the cultural group categorizations in the *Ethnographic Atlas* to match uncharacterized tribes with others from the same cultural group. However, this method is an imperfect predictor of a tribe's marriage payment practices, as over 40% of bride price-practicing tribes share their cultural group with at least one tribe that uses a different marriage payment practice. As a result, this method is not used in the main specification (Table A.8).

Table A.2 examines how these customs relate to marriage outcomes. The relationship between bride price customs and child marriage is nuanced. On average, child marriage is less prevalent among women from bride price-practicing groups (27%) than among those from groups with other customs (39%) or from groups not covered by the ethnographic mapping (39%). However, this pattern varies across settings. In Benin, Kenya, Nigeria, and Togo, the prevalence of child marriage is lower among bride price groups than in other or unclassified groups. In contrast, in Egypt, Liberia, and Madagascar, child marriage is more common in bride price groups. In Ghana and Malawi, differences across groups are small or negligible.

I also use information on post-marital residence patterns as characterized in the *Ethnographic Atlas* to test alternative economic incentives related to marriage. Appendix Section A1.5 describes the various residence types, and Table A.10 presents their distribution in the sample.[25] For the analysis, I group ethnicities into two categories, matrilocal and non-matrilocal, based on the assumption that the economic "post-marital residence" incentive to marry off a daughter in response to negative shocks stems from whether the daughter is expected to leave the household after marriage. Consistent with global patterns, no matrilocal ethnic group in this sample practices bride price. Among ethnic groups that do not practice bride price, 22.44% are matrilocal and 77.56% are non-matrilocal (see Table A.11).

Using historical data rather than contemporary reports of marriage practices addresses significant empirical challenges. First, data on actual marriage customs are not available in the DHS or other large-scale, cross-country datasets. Second, as shown by Corno et al. (2020), observed marriage payments are influenced by economic conditions, making them endogenous to outcomes such as early marriage. By using historical classifications, I mitigate this endogeneity concern.

Nevertheless, it is important to acknowledge the limitations of this approach. The

[25]Less than 5% of women in the sample belong to ethnic groups historically classified as matrilocal (matrilocal or uxorilocal). Nearly 60% live in areas identified as patrilocal or virilocal, while about 7% fall under other categories, such as avunculocal or neolocal. The remaining 30% reside in areas where the post-marital residence pattern could not be identified from the *Ethnographic Atlas*.

Atlas relies on broad categories such as "bride price," which may mask variation in how the custom is practiced and how widely it is followed, and omits to mention several additional marriage payments that can coexist. Furthermore, the data reflect ancient observations, and practices may have evolved.

For instance, data collected by Hotte & Lambert (2023) in Senegal show that 85% of marriages that occurred between 1996 and 2006 involved a bride price. This finding aligns with the classifications in Murdock's *Ethnographic Atlas*, which documents the prevalence of bride price among most ethnic groups in Senegal. However, the Senegalese case described by Hotte & Lambert (2023) also illustrates the complexity that such categories may conceal: marriage transfers often flow in multiple directions. In addition to the bride price paid to her family, the bride typically receives a personal transfer from her husband and brings into the household her own set of resources, akin to a wedding trousseau.

In support of the continued relevance of these historical classifications, Ashraf et al. (2020) show that *Ethnographic Atlas* classifications align with more recent—though scarcer—ethnographic sources. I further document this in Appendix A1.6 using Malawi as a case study, showing that historical practices still closely align with contemporary ones. This reinforces the validity of using these historical norms to analyze the influence of traditional practices on marriage outcomes.

4 Empirical strategy

4.1 Impact of Child Marriage Bans

The effectiveness of child marriage bans is estimated using a within-country difference-in-difference-like approach, similar to strategies used in other natural experiment settings, where an individual's exposure to a law or reform is determined by their date of birth and region of residence (Card, 1992; Duflo, 2001; García-Hombrados & Salgado, 2023; Wilson, 2022). By combining cohort and regional variations in ban exposure, this empirical strategy assesses whether child marriage declines more after an increase in the minimum legal age of marriage in regions where the law is likely to have had more bite.

The year of birth in a country is the first factor that determines an individual's exposure to a child marriage ban.[26] All women who were 18 or older at the time the law was enacted were not exposed to the ban. For younger women, exposure to the ban is an increasing function of their date of birth. The younger a woman was at the time of the ban, the less likely she was to have been married before the law took effect. As a result, the impact of the law is expected to be zero for women who were 18 or older at the time of its implementation, and it should increase for younger women. Therefore, women who were younger than 18 at the time of the ban are the "exposed cohort," while those 18 or older comprise the "unexposed cohort."

The second dimension of variation is the region's ban intensity, defined as the average pre-ban rate of child marriage, weighted by how far below age 18 marriages typically occurred. This measure is based on the idea that the potential impact of the ban should be greater in regions where early marriage was more common and occurred at younger ages. In contrast, we do not expect the bans to have any effect in regions where child marriage was virtually absent. My interest lies precisely in assessing the effectiveness of the bans where the issue is present. Accordingly, the intensity of the treatment—the legal reform—is considered higher in areas where the pre-ban average age at marriage was lower, as these regions have more scope for behavioral change in response to the law. Equation 1 below describes the empirical strategy:

$$Outcome_{i,t,r,c} = \beta_0 + \beta_1 Exposed_{t,c} \times Intensity_r + FE_r + FE_{t,c} + \varepsilon_{i,t,r,c} \tag{1}$$

where the variable $Outcome_{i,t,r,c}$ is a binary indicator for whether girl i, born in year t, residing in region r and country c, was married before the age of 18. Alternative regressions also examine other outcomes, which are the age at first union, the age at first birth, and the number of years of education. The coefficient of interest is β_1, which is associated with the interaction between $Exposed_{t,c}$ and $Intensity_r$. $Exposed_{t,c}$ is a binary variable that indicates whether

[26]This is because the minimum legal age of marriage is raised at the national level, and different countries implement such reforms at different times.

girls born in year t in country c were younger than 18 at the time the child marriage ban was implemented in country c. $Intensity_r$ captures pre-ban heterogeneity in child marriage across regions, serving as a measure of "treatment intensity" or "ban intensity." It reflects how much bite the law is expected to have in each region based on historical marriage patterns. The coefficient β_1 compares changes in child marriage likelihood following the ban in regions where the pre-ban average age at marriage was low (*i.e.*, where child marriage was more prevalent) to changes in regions where the average age at marriage was higher. Intuitively, this coefficient estimates the impact of the ban on child marriage for girls who could benefit most from the law, as they live in regions with a low pre-ban average age at marriage. This approach ensures that estimates are independent of the initial average age at marriage.

$FE_{t,c}$ are country-specific year-of-birth fixed effects, which account for cohort effects and capture aggregate changes in marital behaviors over time. These fixed effects also control for other country-specific time trends, such as shifts in women's labor force participation, the rule of law, women's legal rights, educational attainment, and cultural norms, all of which might influence marriage patterns. FE_r are sub-national region fixed effects, which control for region-specific time-invariant unobserved heterogeneity in the outcome, such as state capacity disparities. They notably account for the fact that $Intensity_r$ is inherently correlated with the outcome, as individuals in regions where child marriage is more common are likely to marry at a younger age. Standard errors are robust, and because the region is the geographic unit for measuring the treatment intensity, I address serial correlation by clustering standard errors at the regional level, following the approach of Cameron et al. (2008).

Intensity measures are constructed following the methodology of Wilson (2022). The main measure of sub-national ban intensity treats the ban as having a greater intensity in terms of potential impact in regions where the pre-ban average age at marriage was lower.[27] To model this effect, the main measure of ban intensity for region r is defined as follows:

[27]For example, in a region where half of the females married at age 17 and the other half at age 18, the pre-ban average age at marriage would be 17.5, or 0.5 years below 18. Under universal compliance, the ban would raise the average age of marriage by 0.5 years in that region. Conversely, in a region where half of the females married at age 16 and the other half at age 17, the pre-ban average age at marriage would be 16.5. In this case, the ban would increase the average age of marriage by 1.5 years.

$$Intensity_r = \sum_{i=1}^{N} \frac{I(married\, before\, 18)_{i,r} \times (18 - marriage\ age_{i,r})}{cohort\ size_r} \tag{2}$$

where $Intensity_r$ is the summation across all females in the pre-ban cohorts recorded by the pre-ban surveys, which are representative at the national, regional, and urban-rural levels. The denominator, $cohort\ size_r$, is the total number of females in the pre-ban cohorts within region r. $I(married\, before\, 18)_{i,r}$ is a binary variable equal to 1 if individual i married before age 18. The numerator is zero for respondents who were unmarried by age 18. For women who married before 18, the numerator captures the difference between 18 (the legal minimum age of marriage) and their actual marriage age. This measure captures sub-national pre-ban heterogeneity in child marriage along two dimensions: the *extensive margin*, reflecting the likelihood of marriage before age 18, and the *intensive margin*, which reflects how far below 18 a marriage took place. The higher the value of $Intensity_r$, the greater the potential impact of the ban in increasing the average age at marriage.

To enable a more nuanced analysis of the differing impacts of child marriage bans across various contexts and to capture how these effects may diverge between urban and rural populations, I also construct a measure at the $region \times zone$ (r, z) level. This approach allows for differing "ban intensities" between urban and rural areas within the same region.[28]

An alternative measure of regional ban intensity treats the ban as having greater intensity in regions where child marriage *rates* were higher. This measure focuses on the *extensive margin* of child marriage, differing from the main measure by ignoring variation in the pre-ban intensity on the intensive margin (*i.e.*, the extent to which marriages occurred well below age 18). The alternative measure is defined as follows:

$$Extensive\ Intensity_r = \sum_{i=1}^{N} \frac{I(married\, before\, 18)_{i,r}}{cohort\ size_r} \tag{3}$$

[28]The intensity for each region and zone is measured as follows:
$Intensity_{r,z} = \sum_{i=1}^{N} \frac{I(married\, before\, 18)_{i,r,z} \times (18 - marriage\ age_{i,r,z})}{cohort\ size_{r,z}}$

This alternative measure is notably useful to ensure that the construction of the main intensity measure is not driving the results, such as through sensitivity to outliers.

Appendix Figure A.6 presents the distributions of the different pre-ban child marriage intensity measures, revealing a slight right-skew. Appendix Table A.12 provides descriptive statistics of these measures. To facilitate the interpretation of results, the $Intensity$ variables are standardized to have a mean of 0 and a standard deviation of 1. This standardization helps compare the relative impact of each measure and mitigates the effects of differing scales.

Table A.13 presents key statistics for the three intensity measures by country. The data reveal substantial cross-country heterogeneity in both levels and dispersion.[29] Importantly for the interpretation of results in terms of standard deviation, in none of the intensity measures is a country's maximum value lower than the standard deviation of that measure on the entire sample.

4.2 Impact of Weather Shocks on the Effectiveness of Child Marriage Bans

The impact of weather shocks on the effectiveness of child marriage bans is computed according to the following Equation 4:

$$\begin{aligned} Outcome_{i,t,r,c} &= \beta_0 + \beta_1 Exposed_{t,c} \times Intensity_r + \beta_2 Shock_{t,r} \\ &+ \beta_3 Exposed_{t,c} \times Shock_{t,r} + \beta_4 Shock_{t,r} \times Intensity_r \\ &+ \beta_5 Shock_{t,r} \times Exposed_{t,c} \times Intensity_r + FE_r + FE_{t,c} + \varepsilon_{i,t,r,c} \end{aligned} \tag{4}$$

The empirical strategy builds upon the approach used to evaluate the effectiveness of

[29]For example, Ethiopia and Guinea stand out with the highest average values for the main region-level intensity indicator (2.07 and 2.15, respectively), while Egypt (0.94), Ghana (0.95), and Kenya (0.91) report much lower levels. A similar pattern appears for the intensity variable computed at the subregional (region $\times$ zone) level. For the alternative measure, including only the extensive margin of child marriage, average values range from 0.32 in Kenya to 0.70 in Guinea. There are also notable differences in dispersion. Countries like Ghana and Liberia show relatively low standard deviations across all three indicators (e.g., 0.15 and 0.28 for the main intensity variable, respectively), indicating more homogeneity in exposure. In contrast, Nigeria, Madagascar, and Ethiopia show much greater within-country variation, with standard deviations above 0.75 and reaching as high as 1.18 in Nigeria, highlighting strong subnational heterogeneity in intensity levels.

child marriage bans, incorporating a triple interaction with the variable $Shock_{t,r}$, which indicates whether an extremely dry rainy season (drought) occurred during a girl's adolescence (ages 10 to 17).[30] This analysis assumes that the timing of weather shocks is orthogonal to potential confounding factors. The definition of shocks, based on low rainfall realizations relative to each region's specific rainfall distribution, ensures that droughts are not systematically more or less likely in regions where child marriage is more or less prevalent. Additionally, region fixed effects (FE_r) control for time-invariant regional patterns in marriage practices, while country-specific year-of-birth fixed effects ($FE_{t,c}$) account for broader country-level trends in marriage timing and rainfall shocks.

4.3 Identifying Assumption

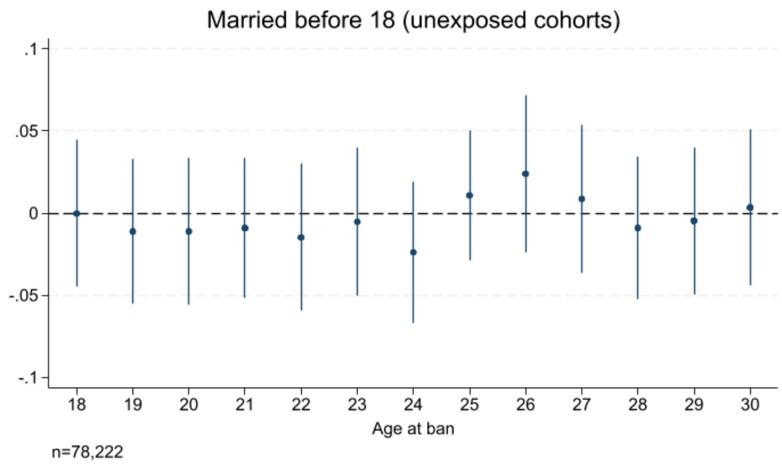
The results can be interpreted as the causal effect of child marriage bans under the assumption that, in the absence of a ban, the decline in the likelihood of child marriage would not systematically differ between regions with varying child marriage intensities.

A key implication of this assumption can be tested. Women aged 18 or older at the time of the ban were not exposed to its effects, so the evolution of child marriage likelihood among these women should not differ systematically across regions. This identification assumption should be approached with caution, as pre-law changes in child marriage likelihood might differ systematically across regions due to factors such as mean reversion. For example, regions with lower average ages at marriage before the law may naturally experience a faster decline over time, regardless of the ban, which could bias the results if not properly accounted for.

To assess the validity of the parallel trends assumption underlying my identification strategy, I implement the robust OLS pre-trend test proposed by Borusyak et al. (2024). This approach relies exclusively on untreated observations—in my case, women who were already 18 or older at the time the law was enacted and thus not exposed to the legal reform. Among this

[30] Robustness checks are conducted by considering the number of droughts during adolescence instead of whether at least one rainy season was extremely dry. Furthermore, alternative specifications compute droughts over different time periods, such as summing monthly precipitation over years or dry seasons, to assess the consistency of findings across varying definitions of drought timing and intensity.

Figure 4: Absence of differential pre-trends in child marriage likelihood



Notes: The dependent variable is an indicator equal to 1 if the respondent married before age 18. The coefficients plotted are β_k, coefficients associated with the interaction between the age at the time of the ban and the measure of ban intensity computed at the $region \times zone$ level, along with 90% confidence intervals.

Sources: DHS, MICS, WORLD Policy Analysis Center, Author's calculations

group, I compare outcome trends for women aged 18–30 (who are closer in age to the treated cohorts) to those aged 31–40, who serve as the reference group.[31] Specifically, I estimate the following specification 5:

$$Outcome_{i,t,r,c} = \beta_0 + \sum_{k=18}^{30} \beta_k 1(\alpha_t = k)_{t,c} \times Intensity_r + FE_r + FE_{t,c} + \varepsilon_{i,t,r,c} \quad (5)$$

where α_t denotes the respondent's age at the time of the ban. β_k captures the interaction between being age k at the time of the ban and the ban intensity in region r. Figure 4 plots the coefficients β_k and reveals no differential pre-trends in the likelihood of child marriage between low- and high-intensity regions before the bans.[32]

Therefore, for omitted variables to bias $\widehat{\beta_1}$, they would need to vary systematically

[31] Because this test is conducted entirely on untreated cohorts, it offers a more reliable assessment of whether regions with varying prevalence of child marriage would have followed parallel trajectories in the absence of the reform.

[32] To reinforce the plausibility of the assumption that no pre-ban trends existed in child marriage, I verify this implication by complementing my difference-in-differences-like regressions with event studies.

by region and correlate systematically with the timing of the bans. One possibility is that other policies, such as those related to education, labor markets, or children's rights, were implemented alongside child marriage bans. If, in each country, child marriage bans were accompanied by other reforms that could also influence child marriage and had a systematically stronger impact in regions with a lower average age at marriage before the ban, then my estimates would be biased. In this case, I would not be capturing the isolated effect of child marriage bans but rather the combined impact with these additional reforms.[33]

[33]To assess this possibility, Appendix Figure A.7 plots the adoption years of legislative changes setting the minimum legal age for marriage at 18 alongside increases in the minimum years of education. The plot reveals no systematic relationship between the timing of these two types of legal reforms, implying that educational reforms were not consistently adopted alongside child marriage bans. This finding mitigates concerns that the observed effects are driven by simultaneous policy changes rather than the bans themselves.

5 Results

5.1 Effectiveness of Child Marriage Bans

5.1.1 Effects on Early Marriage

Table 2 presents the results on the effectiveness of child marriage bans. Panel A shows the results with the dependent variable being an indicator for marriage before age 18, and Panel B for marriage under 15.

For marriage under 18, the coefficient estimate in column 1 indicates that, on average, the bans do not significantly reduce the likelihood of child marriage. In contrast, the estimates in columns 2, 4, and 6 show that a one standard deviation increase in local ban intensity is associated with a decrease in the likelihood of child marriage by approximately 2.5 percentage points in urban areas, with the effect ranging from 2.1 to 2.8 percentage points, depending on the measure of local ban intensity (all with p-values < 0.01). This represents a decline ranging from 9.5% (column 6) to 12.7% (column 4) relative to the level observed among the unexposed cohorts for each additional standard deviation in ban intensity.[34]

Panel B of Table 2 presents results for marriages occurring before age 15. On average, child marriage bans have no significant effect on marriage under 15 in rural areas but lead to a significant reduction in urban areas. A one standard deviation increase in ban intensity lowers the probability of marriage under 15 by about 1.5 percentage points in urban areas.[35] This effect is robust across various definitions of ban intensity. Given that the baseline prevalence of under-15 marriage in urban areas is 6%, it represents a 25% relative decline.

The alternative measure of intensity is more directly interpretable, as it corresponds to the share of women married before age 18 in the woman's region before the ban. With an

[34] In comparison, using a sample of 17 low- and middle-income countries, Wilson (2022) found that a one standard deviation increase in the local intensity of child marriage bans was associated with a significant 7.1 percentage points (31%) reduction in the likelihood of child marriage in urban areas, while the effect in rural areas was not statistically significant.

[35] The effect ranges between 1.4 percentage points in columns 2 and 6 and 1.5 percentage points in column 4.

average pre-ban local child marriage rate of 0.45 and a standard deviation of 0.18 (Table A.12), results imply that if the pre-ban child marriage rate is 45% rather than 27% (one standard deviation difference), laws reduce marriage under 18 by 2.1 percentage points and marriage under 15 by 1.4 percentage points in urban areas (column 6).

Table A.14 reports the results using the unstandardized measures of pre-ban child marriage intensity. The estimates indicate that an additional percentage point in a region's pre-ban child marriage rate is associated with a 0.12 percentage point additional decrease in the likelihood of marriage under 18, and with a 0.74 percentage point additional decrease in marriage before age 15 (column 6). In regions where the pre-law prevalence was 45% (pre-ban regional mean), the estimated reductions are 5.22 percentage points for marriage under 18 and 3.33 percentage points for marriage under 15, corresponding to relative declines of 23.7% and 55.5%, respectively.

While child marriage bans are effective in urban areas, they consistently fail to produce a significant impact in rural settings (columns 3, 5, and 7 of Table 2). The urban-rural divide could reflect differences in access to information, enforcement mechanisms, or economic pressures that vary between these areas, potentially undermining the bans' effectiveness in rural settings.

The estimated effects of child marriage bans on the likelihood of child marriage remain robust when the standardized continuous variable *Intensity* is replaced with a dummy variable, set to 1 if the standardized *Intensity* is positive and 0 if negative (Table A.15). Appendix Section A3.2 presents country-level estimates.

Figure 5 illustrates the dynamic effects of the bans, showing the coefficients associated with the impact of bans by age at the time of the ban, separately for urban and rural areas. It reveals no differential pre-trends in the likelihood of child marriage between low- and high-intensity regions and highlights that, in urban areas, the younger a girl is at the time of the ban, the greater the effect of the law.

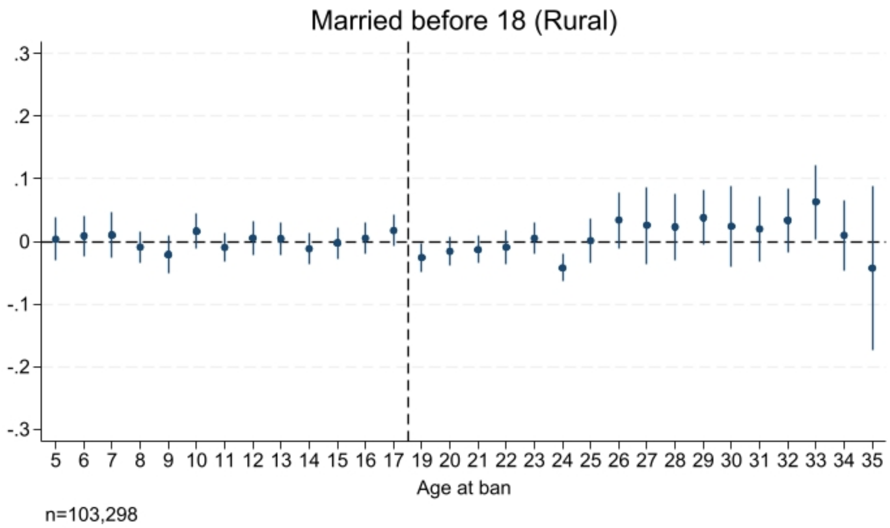
Table 2: Effectiveness of bans: child marriage

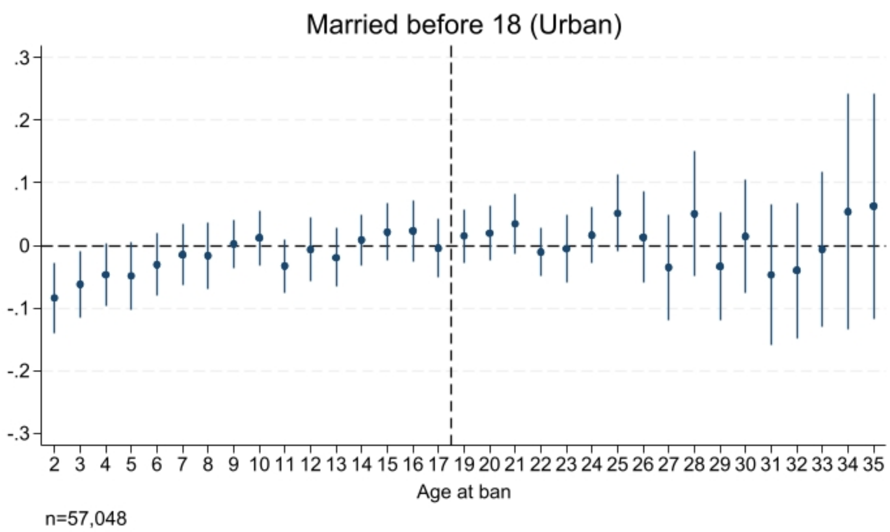
Sample	(1) Country	(2) Urban	(3) Rural	(4) Urban	(5) Rural	(6) Urban	(7) Rural
Panel A: Married before 18							
Exposed × Intensity (std)	0.002	-0.024***	0.010	-0.028***	0.010	-0.021***	0.007
	(0.006)	(0.007)	(0.009)	(0.009)	(0.009)	(0.006)	(0.008)
Mean Y if not exposed	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	162,630	59,298	103,329	57,048	103,298	59,298	103,329
R^2	0.125	0.097	0.121	0.097	0.121	0.096	0.121
Panel B: Married before 15							
Exposed × Intensity (std)	-0.005	-0.014***	-0.002	-0.015**	-0.001	-0.014***	-0.002
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)	(0.004)
Mean Y if not exposed	0.09	0.06	0.11	0.06	0.11	0.06	0.11
Observations	162,630	59,298	103,329	57,048	103,298	59,298	103,329
R^2	0.056	0.047	0.060	0.047	0.060	0.047	0.060
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r ×z	r ×z	r ext	r ext

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable *Intensity* is standardized with mean 0 and standard deviation 1. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Columns 1, 2 and 3 report results using the pre-ban child marriage intensity measured at the region level, taking into account both the intensive and extensive margins of child marriage. Columns 4 and 5 use a similar measure of intensity but computed at the $region \times zone$ level (rural and urban areas have different intensity levels within the same region). Columns 6 and 7 report results using the intensity measure computed at the regional level and only including the extensive margin of child marriage. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Figure 5: Event study - Dynamic effects of bans on child marriage likelihood





Notes: The dependent variable is an indicator equal to 1 if the respondent married before age 18. The coefficients plotted are β_k, coefficients associated with the interaction between the age at the time of the ban and the continuous measure of ban intensity computed at the $region \times zone$ level, along with 90% confidence intervals. The reference category is women aged 18 when the law was enacted.

Sources: DHS, MICS, WORLD Policy Analysis Center, Author's calculations

5.1.2 Impact on Other Marriage-Related Outcomes

Table 3 provides additional insights into the effects of child marriage bans, offering a more nuanced interpretation of their impact. Although the bans do not significantly reduce the likelihood of child marriage in rural areas, they lead to significant increases in the average age at first marriage and first birth, albeit with weaker effects in rural areas compared to urban areas. Among ever-married women, a one standard deviation increase in local ban intensity raises the average age at marriage by approximately 0.67 years in urban areas and 0.29 years in rural areas, equivalent to increases of 8 months or 3.2% in urban areas and 3.5 months or 1.5% in rural areas (p-values < 0.01).[36]

For women who have ever had a child, columns 3 and 4 show that a one standard deviation increase in ban intensity is associated with a 8-month (0.67 year) or 3.2% increase in the age at first birth in urban areas and a 4.1-month (0.34 year) or 1.7% increase in rural areas (p-values < 0.01).

In terms of educational outcomes, a one standard deviation increase in local ban intensity results in an increase of 5.4 months (0.45 year) or 5.6% of education in urban areas (p-value < 0.01). However, the bans do not have a significant impact on educational attainment in rural areas. These results remain robust when computing the local ban intensity measure at the regional level (Table A.16).

The alternative measure of pre-ban child marriage intensity documented Table A.17 shows that, when the baseline child marriage rate is 45% rather than 27% (*i.e*, one standard deviation difference), laws increase the age at first marriage by 6.7 months (0.56 years) or 2.7% in urban areas and 4 months (0.33 years) or 1.7% in rural areas, and the age at first birth by 6.5 months (0.54 years) or 2.5% in urban areas and 4.4 months (0.37 years) or 1.9% in rural areas. The magnitude of these effects is comparable to that of a conditional cash transfer program on school attendance in Malawi, which increased these outcomes by 0.43 and 0.27 years, respectively (?).

[36] In comparison, using a sample of 17 low- and middle-income countries, Wilson (2022) found that a one standard deviation increase in the local intensity of child marriage bans was associated with an average increase of 0.63 years in age at marriage—rising to 1 year in urban areas and 0.52 years in rural areas.

Table 3: Effectiveness of bans: other outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Age first marriage		Age first birth		Years of education	
Sample	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	0.667***	0.288***	0.670***	0.339***	0.454***	-0.064
	(0.116)	(0.076)	(0.112)	(0.064)	(0.110)	(0.065)
Region FE	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X
Definition of intensity	r×z	r×z	r×z	r×z	r×z	r×z
Mean Y if not exposed	21.09	19.04	21.26	19.51	8.18	4.42
Observations	42,262	89,050	41,600	87,998	54,770	88,114
R^2	0.184	0.159	0.190	0.147	0.260	0.424

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. The variable *Intensity* is standardized with mean 0 and standard deviation 1. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location taking into account both the intensive and extensive margins of child marriage. The table reports results using the pre-ban child marriage intensity measured at the $region \times zone$ level (rural and urban areas have different intensity levels within the same region). The number of years of education is missing for Malawi. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

This signifies that while the bans may not significantly reduce child marriage in rural areas, they still have positive, economically and statistically significant effects on delaying marriages and childbirth.

5.2 Impact of Economic Shocks on Ban Effectiveness

This subsection explores how negative weather shocks, such as droughts, may influence the effectiveness of child marriage bans in Africa.

In Appendix A3.3, I explore the relationship between rainfall shocks and aggregate crop yields in Africa by combining meteorological data with agricultural statistics. The analysis demonstrates that droughts significantly reduce agricultural output, validating the use of droughts as a reliable proxy for negative income shocks. Conversely, excessive rainfall is shown to increase agricultural yields.

5.2.1 Main Results: Droughts and Ban Effectiveness

Table 4 presents the effects of child marriage bans on the likelihood of marriage under 18, conditional on whether women experience a drought during adolescence. In the full sample (column 1), the bans do not significantly reduce early marriage, and their effectiveness does not appear to vary with drought exposure.

In areas where bride price is historically practiced, child marriage bans are effective in reducing early marriage, but only when no drought occurs during adolescence (column 2). In the absence of extreme drought (–2 SD), a one standard deviation increase in ban intensity reduces the likelihood of child marriage by 1.3 percentage points (4.6%). When at least one extreme drought occurs during adolescence, the impact of the ban weakens significantly by 4.6 percentage points. Using a slightly less severe drought definition (–1.5 SD), the estimated effect of bans in the absence of any severe or moderate drought is stronger and reaches 1.9 percentage points (6.8%). Results are robust to the use of the alternative measure of pre-ban child marriage intensity (Table A.24).

The fact that the coefficient on the triple interaction term ($Exposed \times Intensity \times Drought$) in bride price areas exceeds the main effect of the ban in absolute value is consistent with the underlying dynamics of early marriage and economic shocks. On average, child marriage bans reduce the likelihood of marriage under 18 by 1.9 percentage points among girls who do not experience a drought during adolescence (Table 4, Column 2, Panel B). Taking the case of droughts more severe than –1.5 standard deviation, approximately 21% of girls are exposed to such shocks during adolescence, while the remaining 79% are not (as shown in Table A.4). Among girls exposed to a drought, the ban's effectiveness is 3.4 percentage points weaker, resulting in a net increase in child marriage of 1.5 percentage points in this subgroup. A simple calculation helps illustrate this heterogeneity. Before the ban, the average child marriage rate is 28%. Applying this to a cohort of 1,000 girls, 280 would marry as children. After the ban, among the 790 girls not exposed to a drought, the marriage rate falls to 26.1%, resulting in 206 child marriages. Among the 210 girls exposed to drought, the rate rises from 28% to 29.5%, leading to 62 child marriages. In total, 268 girls in the cohort marry before 18 following the ban, implying a reduction of only

12 marriages on average, or 1.2 percentage points.[37] While the ban is effective in the absence of economic stress, its average effect is diluted by its sharp ineffectiveness among drought-exposed girls.

This result reflects a key mechanism: in the absence of economic shocks, the ban delays or prevents early marriage, thereby increasing the stock of girls who remain unmarried during adolescence. When a drought occurs after the implementation of the ban, this enlarged pool of unmarried girls becomes newly exposed to the risk of marriage, as households under economic pressure may resort to early marriage as a coping strategy. In contrast, under pre-ban conditions, many of these girls would have already been married by the time a drought hits, limiting the number of additional marriages the shock could induce. In this way, the existence of the ban changes the composition of the population at risk of early marriage, and paradoxically, it can amplify the observable response to an economic shock.[38] Overall, this pattern underscores that while legal bans can be effective under stable conditions, they do not buffer households from adverse economic events.

In regions where the bride price is not historically practiced, bans are ineffective, and droughts do not influence their impact (column 3).[39] This contrast between bride price and non–bride price regions suggests that economic constraints influence marriage decisions in different ways, and that, in the absence of shocks, households face distinct incentives to comply with the ban. In bride price areas, households may have stronger economic reasons to delay marriage when no shock occurs, as postponing the union can increase the expected bride price. In regions without bride price, where such financial returns are absent, the law alone is less likely to change behavior. Indeed, when households expect higher bride prices for more educated daugh-

[37] $0.79 \times 0.261 + 0.21 \times 0.295 = 0.2682$

[38] This dynamic does not necessarily dissipate after one episode. In years without droughts, the pool of unmarried girls continues to grow, as fewer early marriages take place. When a drought eventually occurs, it may prompt a larger-than-usual wave of early marriages, drawing from this accumulated pool. In the absence of economic stress, the ban causes the stock of unmarried girls to increase cohort by cohort. When a drought eventually hits, it draws from a larger base of girls who would have been married earlier in a pre-ban context. Hence, the mechanism operates cyclically: the pool of unmarried girls grows during stable years and is partially depleted during periods of economic hardship.

[39] Tribes that are mapped but not characterized in the *Ethnographic Atlas* are classified as having an unknown historical marriage payment and, therefore, are excluded from both the bride price- and non-bride price-practicing groups. They are, however, included in the country average effect column 1.

ters, delaying marriage can be economically beneficial. Evidence from Zambia shows that bride price increases with the education of the bride, creating a financial return to delaying marriage (Ashraf et al., 2020).[40] Yet in practice, this decision may be constrained by social norms, intra-household bargaining dynamics, or uncertainty about future returns. In such cases, the ban acts as an external constraint that facilitates delay, allowing households to align behavior with long-term financial gains. However, when a drought occurs, the household's ability to wait is undermined. The anticipated future gains from delaying marriage are no longer sufficient to outweigh the immediate need for resources, prompting households to marry daughters early for a lower but immediate bride price. In this context, the economic incentive to delay marriage is still present but no longer affordable, and the ban loses its effectiveness. In regions without bride price, column 3 of Table 4 shows that bans have no significant effect on the likelihood of child marriage, nor do droughts alter their effectiveness. This may reflect that competing financial incentives offset each other, or that non-financial factors—unaffected by weather shocks—play a predominant role in limiting the impact of legal reforms in these regions.

Table A.25, which reports the impact of child marriage bans on educational attainment by marriage payment system and the occurrence of shocks, lends support to this mechanism. A natural implication of the argument that groups practicing bride price have a stronger incentive to delay marriage in order to educate their daughters and secure a higher bride price is that, in the absence of climate shocks, girls' education levels should be higher in these groups than in others, and should increase more rapidly following the implementation of the ban. Consistent with this prediction, Table A.25 shows that prior to the bans, and in the absence of shocks, education levels were higher in bride-price areas. Moreover, in the absence of shocks, the bans do not significantly raise educational attainment among groups that do not practice bride price, but they do lead to a substantial improvement where bride price is practiced. By contrast, during periods of drought, the bans have no significant effect on education in either group.

[40] Similarly, Hotte & Lambert (2023) observe a higher bride price among more educated women in Senegal. This difference in bride price is only significant for women with secondary education. However, only 12% of their sample had this level of education, so such achievement may be more indicative of the family's social standing than of the woman's own qualities (human capital).

Table 4: Impact of droughts on the efficiency of child marriage bans

	(1)	(2)	(3)
Dependent Variable		Married before 18	
Sample	Country	Bride Brice	Not Bride Price
Panel A: Droughts <-2 SD			
Exposed × Intensity (std)	0.002	-0.013**	0.000
	(0.006)	(0.007)	(0.008)
Exposed × Intensity × Drought	0.034	0.046**	0.045
	(0.020)	(0.023)	(0.036)
Mean Y	0.33	0.28	0.42
Observations	162,630	84,272	30,529
R^2	0.126	0.114	0.074
Panel B: Droughts <-1.5 SD			
Exposed × Intensity (std)	0.001	-0.019***	0.003
	(0.006)	(0.007)	(0.009)
Exposed × Intensity × Drought	0.005	0.034**	-0.044
	(0.015)	(0.016)	(0.027)
Mean Y	0.32	0.28	0.41
Observations	162,630	84,272	30,529
R^2	0.125	0.114	0.074
Region FE	X	X	X
Year of birth FE	X	X	X
Definition of intensity	r	r	r

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. "Not Bride Price" areas exclude women located within tribal boundaries not included in Murdock (1967) Ethnographic Atlas. The "Mean Y" is computed on unexposed cohorts for which no drought occurred during adolescence. I also estimate a regression with a four-way interaction from the following Equation to test whether the slope of $Exposed \times Intensity \times Drought$ is the same in bride price and non bride price groups: $Outcome_{i,t,r,c} = \beta_0 + \beta_1\ Exposed_{t,c} \times Intensity_r + \beta_2\ Shock_{t,r} + \beta_3\ BP_i + \beta_4\ Exposed_{t,c} \times Shock_{t,r} + \beta_5 Exposed_{t,c} \times BP_i + \beta_6\ Intensity_r \times Shock_{t,r} + \beta_7\ Intensity_r \times BP_i + \beta_8\ BP_i \times Shock_{t,r} + \beta_9\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} + \beta_{10}\ Exposed_{t,c} \times Intensity_r \times BP_i + \beta_{11}\ Intensity_r \times Shock_{t,r} \times BP_i + \beta_{12}\ Exposed_{t,c} \times BP_i \times Shock_{t,r} + \beta_{13}\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} \times BP_i + \mathrm{FE}_r + \mathrm{FE}_{t,c} + \varepsilon_{i,t,r,c}$. β_{13} is positive and significant at the 1% level in Panel B and positive and not significant in Panel A.

Sources: DHS, MICS, CHIRPS, author's calculations.

5.2.2 Sensitivity Analysis Across Drought Thresholds

Figure 6 illustrates the main coefficients for the interaction between child marriage bans and droughts across various definitions of drought intensity. In line with previous results, Figure 6a confirms that, on average, bans are ineffective outside urban areas and that droughts do not significantly alter the effectiveness of child marriage bans. There is a slight pattern suggesting that droughts may reduce the effectiveness of bans in urban areas, but the interaction terms remain mostly statistically insignificant.

When the analysis is restricted to regions with a historical practice of bride price (Figure 6b left), a more nuanced pattern emerges. In these areas, bans are effective in reducing child marriage when no drought occurs during adolescence. However, as droughts become more severe, their negative impact on ban effectiveness increases. The results also hold when the sample is divided between urban and rural areas (Figure 6c). In both settings, child marriage bans are effective in reducing child marriage in bride price areas when no droughts occur. However, as the severity of droughts increases, the effectiveness of the bans diminishes more sharply. This outcome is not surprising, as both urban and rural populations in Sub-Saharan Africa often depend on agricultural income, and droughts can drastically reduce agricultural output. Such decline often leads to rising prices and economic hardship, which, in turn, can drive early marriage as a coping strategy even for non-agricultural households. The effects are, however, less precisely estimated in urban areas, possibly due to smaller sample sizes or because urban households have access to a wider range of coping mechanisms, such as credit networks or remittances.

Figures A.9, A.10, and A.11 present sensitivity analyses for the four key outcomes — child marriage likelihood, age at first union, age at first birth, and years of education—in bride price areas overall, as well as separately for urban and rural settings. The coefficients are broadly similar across urban and rural areas for most outcomes, with the main exception being education. In urban areas, child marriage bans significantly reduce the likelihood of child marriage and increase the age at first union, age at first birth, and educational attainment. Droughts weaken the effects of the bans on child marriage and the timing of first union and first birth, but have no discernible impact on education. In rural areas, the effects largely follow the same pattern for marriage-related outcomes. However, for education, most coefficients are not statistically significant. Still, the estimates suggest that in the absence of drought, bans improve educational attainment, whereas droughts diminish this effect.

In non-bride price practicing areas (Figure 6b right), child marriage bans are ineffective, and in most cases droughts do not alter their impact. For drought thresholds ranging from −1.4 to −1.7 standard deviation, the interaction coefficient with droughts is negative and weakly significant. This may reflect heterogeneity in behaviors and incentives within the "Non Bride Price" category, which encompasses ethnic groups with diverse marriage customs, some of which may generate incentives opposite to those observed in bride price areas.

5.2.3 Positive Income Shocks

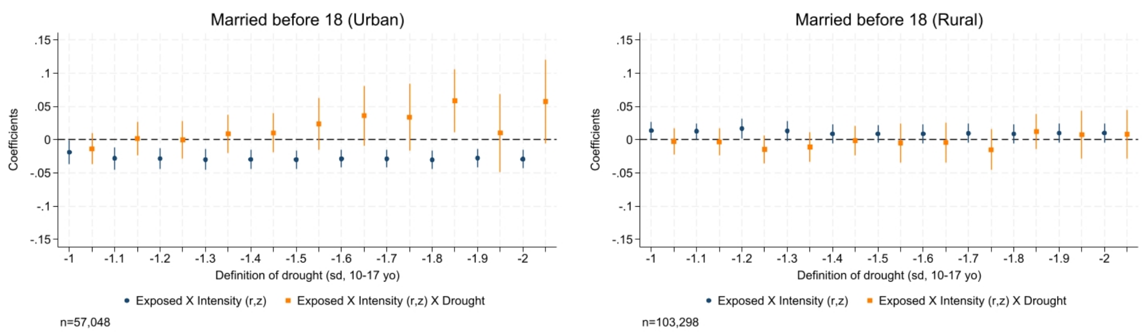
I examine the differential effects of high versus low rainfall realizations (droughts) on the effectiveness of child marriage bans. The results presented in Table A.23 indicate that low rainfall realizations significantly reduce agricultural yields, while high rainfall realizations increase yields, though to a lesser extent. Correspondingly, low rainfall realizations are shown to weaken the effectiveness of child marriage bans, and high rainfall realizations enhance the effectiveness of the bans, as depicted in Figure 7.

Subfigure 7c shows that the positive impact of high rainfall on the effectiveness of child marriage bans is concentrated in rural areas, where increased agricultural yields from abundant rainfall likely improve the affordability of delaying marriage. In contrast, in urban areas, high rainfall does not enhance the effectiveness of the bans, possibly because adverse effects such as flooding offset the potential economic benefits of increased agricultural yields. Because urban soils are less porous, excess rainfall may cause greater disruption to infrastructure and economic activity.

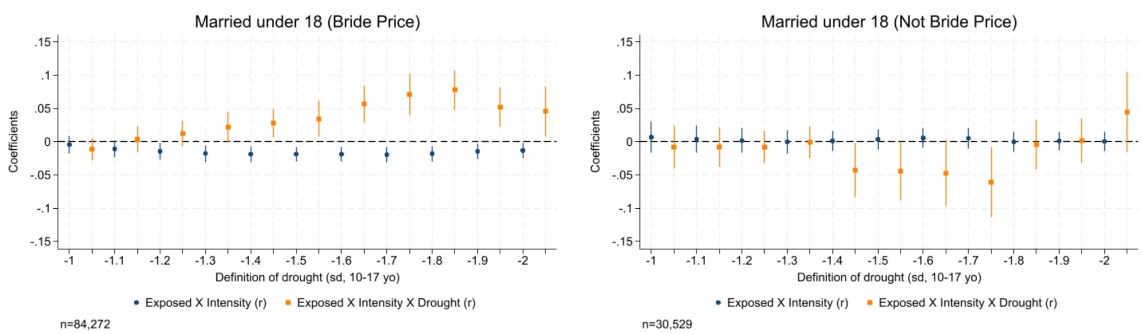
It is important to note that while high rainfall realization is, on average, positive, it can also have negative implications in both urban and rural areas, particularly in contexts prone to flooding and extreme weather events. Heavy rainfall can lead to devastating floods, harming

Figure 6: Sensitivity of the impact of droughts on ban effectiveness

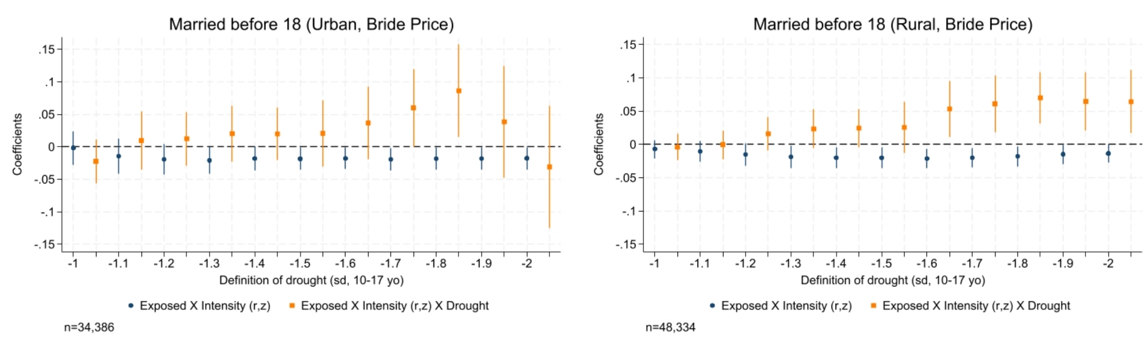
(a) Main sample by zone of residence



(b) Main sample by marriage payment practice



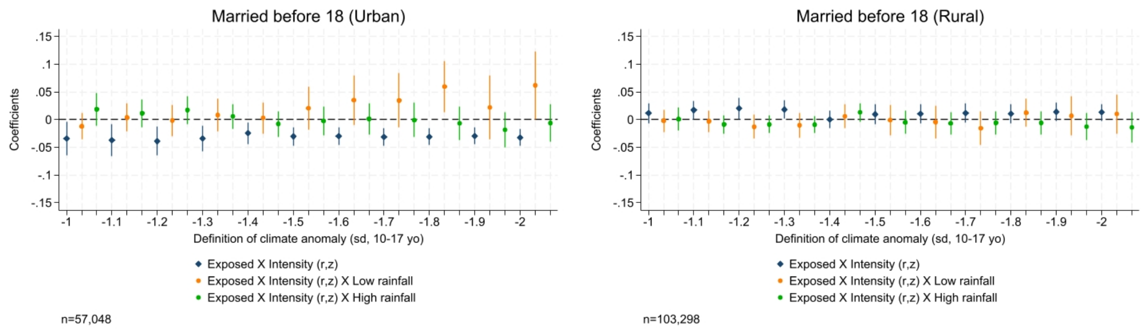
(c) Bride Price areas by zone of residence



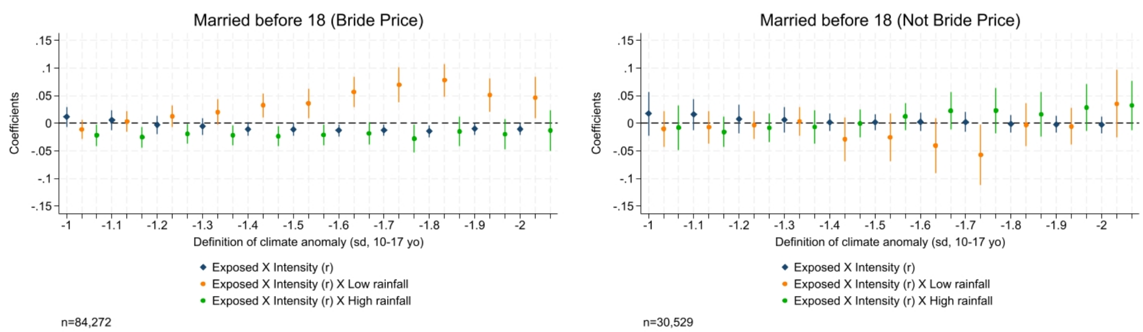
Notes: The dependent variable is a dummy equal to 1 if the woman was married before 18. Navy blue lines plot the coefficient on the interaction between *Intensity* (pre-ban child marriage intensity) and *Exposed* (equal to 1 if the woman was under 18 when the national ban passed). Orange lines add the triple interaction with *Drought* (equal to 1 if at least one rainy season during adolescence, age 10–17, was extremely dry). 90% confidence intervals clustered at the regional level. Regressions limited to women aged 18–40. Subfigures a, b, and c show heterogeneity by urban/rural location, marriage payments, and their interaction.
Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations.

Figure 7: Sensitivity of the impact of climate anomalies on ban effectiveness

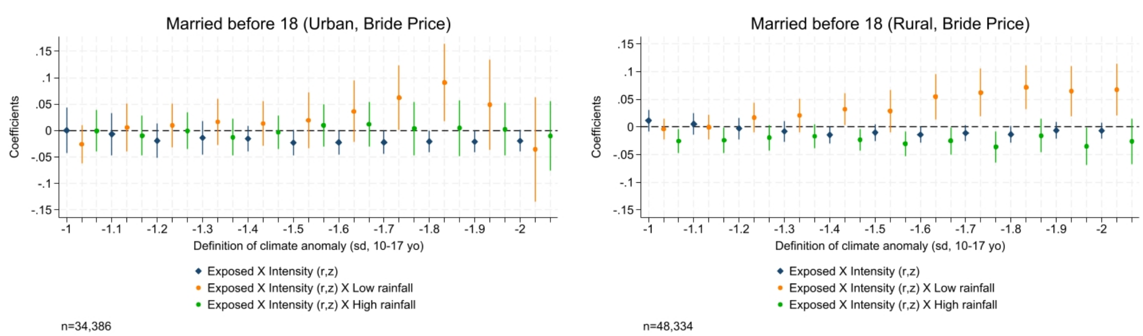
(a) Main sample by zone of residence



(b) Main sample by marriage payment practice



(c) Bride Price areas by zone of residence



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18, 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Low rainfall*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. In green is plotted the triple interaction with the variable *High rainfall*, which is a dummy variable equal to 1 if a rainy season was extremely rainy during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40. Subfigure a provides results for urban and rural areas. Subfigure b provides results by historical marriage payment practice of the area. c provides results by zone of residence for areas with a historical use of Bride Price.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations.

agricultural productivity, displacing communities, and increasing financial pressures on households in both rural and urban areas. Such events can undermine the intended economic benefits associated with rainfall, imposing additional burdens on families. Moreover, with climate change intensifying, extreme weather events, including severe flooding, are becoming more frequent and severe, exacerbating economic vulnerabilities. Given these dynamics, it would not be surprising to observe with very recent data, and increasingly in the future, that high rainfall realizations increase the likelihood of child marriage and weaken the effectiveness of laws banning this practice in both urban and rural settings.

5.3 Additional Mechanisms and Heterogeneity

5.3.1 Weather Shocks and Marriage Timing

The impact of weather shocks on marriage timing has been rigorously examined by Corno et al. (2020), who found that negative income shocks, proxied by droughts, increase the probability of child marriage by 3% in Sub-Saharan Africa.[41] Following their specification, I estimate a discrete approximation of a duration model, adapted from Currie & Neidell (2005). The details of this empirical framework are provided in Appendix A3.5.

Figure A.12a in the Appendix shows how negative rainfall shocks influence the timing of marriage for women. In regions with a tradition of bride price, droughts increase the likelihood of marriage, as families may turn to marriage as a way to access financial resources. In contrast, in areas without bride price, droughts reduce the likelihood of marriage, likely because the ceremony costs make marriage less affordable during economic hardship. These patterns align with existing theory and evidence highlighting that the effect of economic shocks on marriage depends on local customs. While droughts tend to accelerate marriage in bride price regions, they delay it elsewhere. Interestingly, the effect is stronger in non–bride price regions, which may indicate that the bride price buffers and reverses the impact of financial strain by providing an incentive to marry during downturns. Nevertheless, as observed in Table 4 and Figure 6, the influence of these

[41] In Corno et al. (2020), a drought is defined as an annual rainfall realization below the 15th percentile of the local rainfall distribution.

other financial incentives on marriage likelihood in non-bride price areas does not consistently interact with legal bans. Conversely, bride price interacts more strongly with the prohibitions: it creates incentives to delay marriage in stable periods but undermines the effectiveness of the bans during economic hardship.

For men, the results show that droughts reduce the likelihood of marriage in regions where bride price is practiced, while severe droughts increase marriage rates in areas without this tradition (Figure A.12b). In bride price regions, men bear the cost of marriage payments, which become harder to afford during economic shocks, leading to delays in marriage. In contrast, in non–bride price regions—most of which are also patrilocal (Table A.11)—men may be more likely to marry during times of hardship.[42] Nevertheless, the effect is only positive when using drought thresholds of -1.9 and -2 standard deviations, and it is statistically significant only at the -1.9 threshold, so this result must be taken with caution.

5.3.2 Partners' Characteristics

This section explores the effects of child marriage bans and weather shocks on the characteristics of a woman's partner in areas that practice the bride price. The relevant findings are displayed in Figure A.13.

Women exposed to child marriage bans tend to marry older husbands. This result aligns with expectations: when bans delay girls' marriages, prospective husbands must wait longer to marry, which increases their age at marriage. While this effect is not statistically significant overall, the coefficients suggest that droughts may amplify the impact of the bans on the partner's age at first union. One possible explanation is that during times of economic hardship, families may prefer older men who are still able to afford the bride price.

Since the bans on female child marriage tend to delay women's age at first marriage more than that of their partners, the age gap between spouses narrows following the implementation of child marriage bans. Droughts decrease the impact of child marriage bans on the spousal

[42] Without bride price obligations, and with the prospect of a wife contributing labor or providing care for children and elders, marriage can become a strategy to reinforce household capacity during a crisis.

age gap. This result is expected since droughts decrease the positive impact of bans on women's age at first union, and they slightly increase it for men.

Additionally, women exposed to child marriage bans tend to marry more educated partners. However, the bans have a stronger effect on women's own educational attainment than on that of their spouses. Since women typically have lower levels of education than men, the bans contribute to narrowing the educational gap within couples. Droughts do not significantly alter the impact of the bans on the education levels of either women or their partners, so the effect of bans on the spousal education gap is not affected by droughts.

5.3.3 Alternative Economic Mechanism

Other marriage-related practices can also shape the financial incentives to marry, particularly during times of economic hardship. One such practice is the mode of post-marital residence. In the absence of a marriage payment, families may be inclined to marry off a daughter simply to reduce household pressure if the daughter consumes more than she contributes. If marriage entails the daughter leaving the parental home, as is the case in patrilocal or neolocal systems, scarce resources can then be shared among fewer household members, easing the economic burden. Conversely, parents may prefer to keep the daughter in the household if she is a net contributor.

I use post-marital residence patterns from the *Ethnographic Atlas* (Murdock, 1967) to test this alternative economic incentive for early marriage. As detailed in Section A1.5 and Table A.10, fewer than 5% of women in the sample belong to matrilocal ethnic groups, while nearly 60% are from patrilocal or virilocal ones. About 7% fall into other categories, and 30% have no identifiable residence pattern. For the analysis, I classify groups as either matrilocal or non-matrilocal, based on the assumption that the incentive to marry off daughters in response to economic shocks depends on whether the daughter is expected to leave the household after marriage.[43] As expected, no matrilocal group in the sample practices bride price. Among groups without bride price, 22.4% are matrilocal and 77.6% are non-matrilocal (see Table A.11).

Table A.26 presents results for three subsamples: bride price–practicing societies (column 1), non-bride price non-matrilocal societies (column 2), and non-bride price matrilocal societies (column 3). The findings indicate that, among non-bride price and non-matrilocal groups, child marriage bans are generally ineffective, and their effectiveness is not significantly affected by droughts. In matrilocal societies, bans also appear ineffective on average. However, the interaction with droughts cannot be estimated due to limited variation in ban and drought exposure.[44]

These findings imply that while post-marital residence patterns may theoretically influence household incentives around marriage timing during periods of economic stress, this

[43]Groups with an unknown post-marital residence pattern are excluded from the subgroup classification.

[44]Specifically, nearly all matrilocal observations come from Malawi (except one region in Nigeria), where no treated cohorts are exposed to an extreme (<-2SD) drought, and only a single region includes treated women exposed to a -1.5 SD drought.

mechanism does not appear to affect ban's effectiveness. The strongest and most consistent effects of droughts on the effectiveness of child marriage bans are observed in bride price–practicing societies, where marriage involves a direct financial transfer to the bride's family. In contrast, among non–bride price societies—regardless of whether they are matrilocal or not—the bans are ineffective, and there is no robust evidence of interaction with economic shocks. This highlights that the primary economic incentive driving response to child marriage bans in times of hardship is the bride price payment itself, rather than the post-marital relocation of the bride.

5.3.4 State Capacity Mechanism

In the following subsection, the triple interaction term $Shock_{t,r}$ in Equation 4 is replaced with a variable measuring the distance, in degrees, between a woman's residence and the capital city. Table A.27 presents the results. In the capital city (urban areas where the distance to the capital is zero), child marriage bans significantly reduce the likelihood of early marriage. A one standard deviation increase in ban intensity leads to a decline in child marriage likelihood of 2.7 to 3.5 percentage points, depending on the measure of intensity used. These effects correspond to reductions of 12% and 16% relative to the pre-ban child marriage rate, respectively. The effectiveness of the bans does not appear to decline with distance from the capital in urban areas.

In rural areas near the capital, increases in ban intensity are also associated with reductions in child marriage. A one standard deviation increase in ban intensity is associated with a 1.5 to 1.9 percentage points decline in child marriage likelihood, which corresponds to decreases of 3.8% and 4.9% relative to pre-ban rates. However, these coefficients are not statistically significant. Importantly, the interaction between ban intensity and distance from the capital is significant in rural areas: for each additional degree of distance from the capital (approximately 111 km at the equator), the effectiveness of the bans declines by 0.7 percentage point. Beyond a distance of 2.1 degrees, or roughly 239 kilometers, the effect of the bans becomes null. Hence, child marriage bans are less effective in remote rural areas, possibly due to weaker state presence or reduced enforcement capacity.

Figure A.15 presents the coefficients of interest from a regression that includes a four-way interaction between exposure to child marriage bans, droughts during adolescence, and distance from the capital in areas practicing the bride price.[45]. The results indicate that the negative impact of droughts does not worsen with greater remoteness. This implies that distance from the center of power and exposure to climatic shocks act as separate constraints on the effectiveness of legal reforms in rural settings. The effect of distance from the capital is not observed in urban areas.

5.3.5 Bans on Child Marriage for Boys

In this subsection, I examine the effectiveness of child marriage bans for boys, focusing on four countries that implemented such bans after 1995—Ghana (1999)[46], Kenya (2014)[47], Liberia (2012)[48], Malawi (2015)[49], and Nigeria (2003)[50]. In these countries, the child marriage rates for boys, as recorded in the last survey before the law, were relatively low: 5%, 2%, 6%, 4% and 5%, respectively, which are significantly lower than female child marriage rates.

Figure A.14 shows that, on average, child marriage bans do not have a statistically significant effect on boys. Most interaction terms with drought are also not significant, but the estimated coefficients suggest a different pattern than for girls: the bans appear to reduce early marriage among boys only when they experience a negative economic shock during adolescence, in both bride price– and non–bride price–practicing areas. However, this result is sensitive to the drought definition used. While some thresholds yield statistically significant effects, others do not, indicating limited robustness.

[45] Coefficients are estimated from the following Equation: $Outcome_{i,t,r,c} = \beta_0 + \beta_1\ Exposed_{t,c} \times Intensity_r + \beta_2\ Shock_{t,r} + \beta_3\ Distance_i + \beta_4\ Exposed_{t,c} \times Shock_{t,r} + \beta_5 Exposed_{t,c} \times Distance_i + \beta_6\ Intensity_r \times Shock_{t,r} + \beta_7\ Intensity_r \times Distance_i + \beta_8\ Distance_i \times Shock_{t,r} + \beta_9\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} + \beta_{10}\ Exposed_{t,c} \times Intensity_r \times Distance_i + \beta_{11}\ Intensity_r \times Shock_{t,r} \times Distance_i + \beta_{12}\ Exposed_{t,c} \times Distance_i \times Shock_{t,r} + \beta_{13}\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} \times Distance_i + \mathrm{FE}_r + \mathrm{FE}_{t,c} + \varepsilon_{i,t,r,c}$.

[46] Children's Act, 1998

[47] The Marriage Act (2014)

[48] Liberian Children's Act, 2011

[49] Marriage, Divorce and Family Relations Bills, 2014

[50] Child's Right Act 2003

6 Robustness Checks and Additional Tests

6.1 Validity of Identification Strategy

6.1.1 Placebo Tests

Table A.28 presents a placebo test where the ban implementation year in each country is artificially advanced by 5 and 10 years, treating women younger than 23 and 28 at the time as if they were exposed to the ban. The estimated effects of this placebo test are close to zero and not statistically significant, providing strong evidence that flawed identification assumptions do not drive the observed effects of the actual bans.

6.1.2 Heterogeneous Treatment Effects

A key aspect of the identification strategy involves comparing regions with high versus low treatment intensities based on pre-ban levels of child marriage. By construction, such comparisons can introduce negative weights when using standard two-way fixed effects (TWFE) estimators. To assess the severity of this issue in my context, I compute the implicit TWFE comparison weights following the approach of De Chaisemartin & d'Haultfoeuille (2020). The results, reported in Table A.29, show that the sum of negative weights is very low, signifying that the potential for bias is highly limited.

6.2 Data Quality and Measurement Issues

6.2.1 Migration Concerns

A potential concern for the identification strategy is the reliance on ban intensity and weather shocks based on a respondent's location at the time of the survey rather than at the time of marriage. This limitation arises because DHS surveys record a woman's current residence but not her location at the time of her first marriage. If the respondent's current residence differs from where she lived during adolescence, it could lead to inaccuracies in measuring rainfall shocks and ban intensity. This discrepancy might occur for two reasons. First, due to the custom of virilocality, where a woman moves to her husband's household upon marriage, her survey location may differ from her natal home. Second, families may migrate after marriage but before the survey. In this subsection, I explain why measurement error from migration is unlikely to bias my estimates.

In Sub-Saharan Africa, most migration is rural-to-urban and predominantly involves men. Men tend to migrate temporarily for work but maintain permanent rural residences, where their families remain (Baker & Aina, 1995; Henry et al., 2004b; Lucas, 2021). The common practice of patrilocality leads women to migrate for marriage. However, evidence indicates that these moves are typically short-distance, as marriage markets are geographically concentrated within areas smaller than regions, closer to a district, or smaller scale (Lucas, 2021). For instance, data from Senegal show that married women live an average of 20 km from their natal home (Mbaye & Wagner, 2017). By calculating rainfall shocks and ban intensities at the sub-national region level—significantly larger than districts—I minimize the risk of misassigning weather shocks and ban intensity due to migration.[51]

Long-distance migration following droughts could affect the estimation of drought's impact on the effectiveness of child marriage bans. However, research shows mixed results on drought-induced migration (Millock, 2015). Droughts can trigger migration within and between regions toward areas with better agricultural or non-agricultural opportunities. Nevertheless, migration incurs costs, and moving between regions is generally more expensive than within-region migration, which may limit long-distance migration during droughts (Henry et al., 2004a;b). Studies show that droughts modestly increase migration and that most drought-related marriage migration occurs within the same region, with minimal impact on cross-region migration (Becerra-Valbuena & Millock, 2021). This is consistent with existing literature observing that most women migrate for marriage within their region of origin (Beegle & Poulin, 2013). Hence, although DHS surveys do not report the distance between a woman's natal home and current location, most women are likely assigned the correct rainfall shock and ban intensity based on their

[51] Appendix Table A.30 provides migration patterns for the countries analyzed.

current location, even if they moved. Additionally, rainfall shocks tend to be spatially correlated, meaning that women would need to move beyond neighboring regions to experience drastically different weather conditions (Figure A.16).

Finally, if migration in response to negative shocks does occur beyond neighboring regions, it is likely to bias the results toward zero rather than inflate them. This is because women migrating due to drought are more likely to move from areas experiencing adverse conditions to areas less affected by drought. As a result, exposure to shocks measured at the survey location would underestimate the actual exposure experienced during adolescence, attenuating the estimated effects. Therefore, any measurement error arising from migration patterns in response to shocks would work against finding significant results, reinforcing the robustness of the observed effects.

In conclusion, I do not believe migration-induced measurement error is substantial enough to significantly bias my estimates. The results remain robust when excluding women who moved after their 10^{th} birthday, as it is uncertain whether their region at the time of the survey matches their pre-marriage residence (Table A.33 and Figure A.17). Additional tests are provided in Appendix A4.2.

6.2.2 Survey Response Manipulation

Another potential threat to the estimation arises from measurement errors in women's recollections of their age and year of marriage. Such errors could introduce imprecision in the estimates and potentially bias the results if the manipulation of responses shifts around the time the ban was implemented, and these shifts vary systematically across regions with different levels of pre-ban child marriage intensity.

Validation studies of age data in the DHS conclude that these measures are generally accurate, mitigating concerns about substantial bias from recall errors (Pullum, 2006). However, measurement errors can arise after legal changes. If women systematically under-reported being married before the age of 18 due to the presence of a ban or rising social disapproval of child marriage, we would expect to see differences in reported child marriage rates among women born in the same year but interviewed before or after the law's implementation. Specifically, for women born in the same year and country, those interviewed after the ban would likely report lower rates of child marriage compared to those interviewed before. This concern is alleviated by the fact that Figure A.18 shows no systematic differences in child marriage rates across survey waves for women born in the same year and country in most countries.

There are, however, three exceptions: in Egypt and Guinea, women appear to under-report child marriage in post-ban surveys compared to pre-ban surveys, while in Ethiopia, the youngest cohorts in the pre-ban survey appear to under-report child marriage compared to the post-ban survey. This inconsistency likely stems from the timing of fieldwork. In Ethiopia, interviews were conducted between early February and mid-June 2000, while the revised Family Code banning child marriage was issued in July 2000. Since child marriage was already a subject of debate during the interviews, it likely led women to under-report marriages before the age of 18. I conduct a robustness check excluding these three countries (Table A.34). The results remain consistent—and slightly larger—when these countries are omitted, demonstrating that potential under-reporting of child marriage following the bans is unlikely to be driving the main findings.

Moreover, if individuals can manipulate their reported age at first marriage, particularly in response to bans, it is plausible that other outcomes, such as age at first birth, years of education, or marriage under 15, are less prone to such manipulation. To address this concern, I also conduct regressions on these outcomes to ensure that the observed effects of bans on the likelihood of child marriage are not driven by respondents altering their answers. The fact that the bans are found to significantly increase both the age at first birth and the number of years of education and decrease marriage under 15 is reassuring, as these variables are less likely to be manipulated in response to legal changes.

Furthermore, if individuals manipulate their age or year of marriage to conceal illegal behavior, it is unlikely that they would alter their responses differently depending on whether negative shocks occurred years before the survey. Thus, any manipulation is likely orthogonal to the interaction between the bans and shocks, minimizing concerns about potential confounding effects arising from such behavior.

6.2.3 Time Window for Drought Exposure

Figure 1 illustrates the distribution of age at first union, showing that child marriage can occur as early as 7 years old, with a considerable number of marriages happening at or after age 10. To account for shocks that might influence marriage decisions before age 18, I use a time window that includes those occurring between ages 10 and 17. A potential limitation of this approach is the inclusion of shocks that may be irrelevant, such as those that happen after a girl has already entered her first union before turning 18.

However, excluding years when a woman was married but still under 18 could introduce bias and potentially reverse the sign of the estimated coefficients. This is because women who marry before 18 would have fewer "adolescent years" to observe weather shocks, systematically leading to fewer droughts being detected during their adolescence. Conversely, including irrelevant shocks (*i.e.*, those occurring after marriage) would likely attenuate the estimates, as these shocks would have no impact on marriage likelihood, biasing the results toward zero. Given this trade-off, I prefer to include potentially irrelevant shocks to avoid introducing bias from systematically fewer observed shocks in early-married cohorts.

To ensure robustness, I conduct placebo tests to evaluate the impact of droughts occurring at times when they are not expected to influence the likelihood of child marriage. Droughts that occurred during early childhood (ages 0 to 9) and adulthood (ages 18 to 25) show no significant impact on the effectiveness of child marriage bans, as documented in Figure A.19. This finding strengthens the validity of the main results, confirming that the observed interactions between droughts and child marriage bans are specific to the adolescent period.

6.2.4 Knowledge of Child Marriage Bans

Another potential concern is that awareness of the national child marriage ban may vary across households and regions, potentially influencing its observed impact. The estimated effects of the bans might, therefore, reflect differences in knowledge about the ban rather than its actual effectiveness. However, I argue that awareness is a fundamental component of a ban's effectiveness, not a confounding factor. In other words, the impact of a legal reform such as a child marriage ban is partly determined by the extent to which individuals are aware of the new law. Therefore, variation in awareness should be considered an intrinsic element of the ban's implementation and impact rather than a separate confounding factor. Moreover, region fixed effects account for any time-invariant local characteristics that might influence awareness of the ban.

Additionally, the timing of weather shocks is assumed to be orthogonal to differences in ban knowledge. As a result, even if there are intra-region variations in awareness, such differences should not affect the interaction between weather shocks and the ban's effectiveness. Therefore, differences in ban knowledge do not confound the analysis.

6.3 Alternative Specifications and Definitions

6.3.1 Drought Definition Robustness

The main specifications measure droughts during the rainy season. Table A.23 shows that droughts, whether measured over the rainy season, the calendar year, or the dry season, consistently reduce agricultural yields. The negative effect is strongest when droughts are measured during the rainy season, slightly weaker when using calendar-year averages, and much smaller when measured during the dry season. Consistent with this, droughts measured over calendar years (Figure A.21) reduce the effectiveness of child marriage bans in areas practicing the bride price. However, only dry seasons with extremely low rainfall, defined as below -2 standard deviations from the regional historical average, significantly decrease the effectiveness of the bans (Figure A.22).

Additionally, the results remain robust when using the number of droughts during adolescence rather than a binary indicator for experiencing at least one drought, thus reinforcing the consistency of the main findings (Figure A.20).

6.3.2 Marriage Payment Practice Classifications

Lastly, Figure A.23 presents results using the imperfect *Ethnographic Atlas* match developed by Lowes (2017), showing comparable effects and significance. Moreover, when tribes with a historical use of token bride price—defined as a small or symbolic payment—are included in the bride price group, the results remain consistent but the magnitudes are slightly smaller, as expected (Figure A.24). This consistency across different specifications strengthens the conclusion that the observed effects of child marriage bans and weather shocks are robust and not driven by specific definitions of the bride price practice.

7 Conclusion

Marriage decisions are shaped by a complex interplay of social, cultural, and economic factors, particularly in low-income settings where marriage often entails significant inter-household transfers. One such transfer is the bride price—a payment made by the groom's family to the bride's family, common in many parts of Sub-Saharan Africa. In these contexts, child marriage can function as an economic coping strategy: during periods of financial hardship, families may marry off daughters at younger ages to access bride price payments earlier and stabilize household consumption. Consequently, legal bans on child marriage may be more difficult to enforce when compliance imposes financial costs on households. This suggests that such laws are likely to be less effective in settings where early marriage functions as an economic safety net. It highlights the need to consider not only the direct impact of legislation but also how its effectiveness is shaped by traditional norms and the broader economic environment in which households make marriage decisions.

To investigate this, I examine how child marriage bans interact with exogenous income shocks, approximated by droughts, in 12 African countries. The findings reveal a nuanced picture: while bans reduce child marriage in urban settings, their impact in rural areas is limited. In regions where bride price customs prevail, bans reduce child marriage when no negative shock occurs during a girl's adolescence, with a significant effect in both urban and rural areas, but negative shocks nullify the effectiveness of these laws. Among non–bride price groups, child marriage bans are generally ineffective.

While child marriage is undeniably harmful to girls and women, families facing financial stress continue to resort to it as a coping mechanism, even after the practice is made illegal. These findings highlight the critical need to complement child marriage bans with targeted anti-poverty measures to address the issue more effectively. Potential interventions include expanding access to credit markets, strengthening social safety nets, and promoting the adoption of insurance against negative weather shocks. By addressing the financial pressures that drive child marriage, this approach will not only enhance the effectiveness of legal bans but also promote greater economic stability and opportunities for households.

However, the lack of effectiveness of child marriage bans in regions where the bride price is not customary underscores the need to better understand the factors hampering the effectiveness of legal reforms in these areas. Moreover, in bride price regions, even when households do not experience negative shocks, the bans are partially effective but not sufficient to eliminate the practice. This highlights that other structural, social, or cultural forces continue to sustain early marriage and must be addressed to enhance the impact of legal reforms.

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A Appendices

A1 Data

A1.1 Individual Data and Legislative Changes

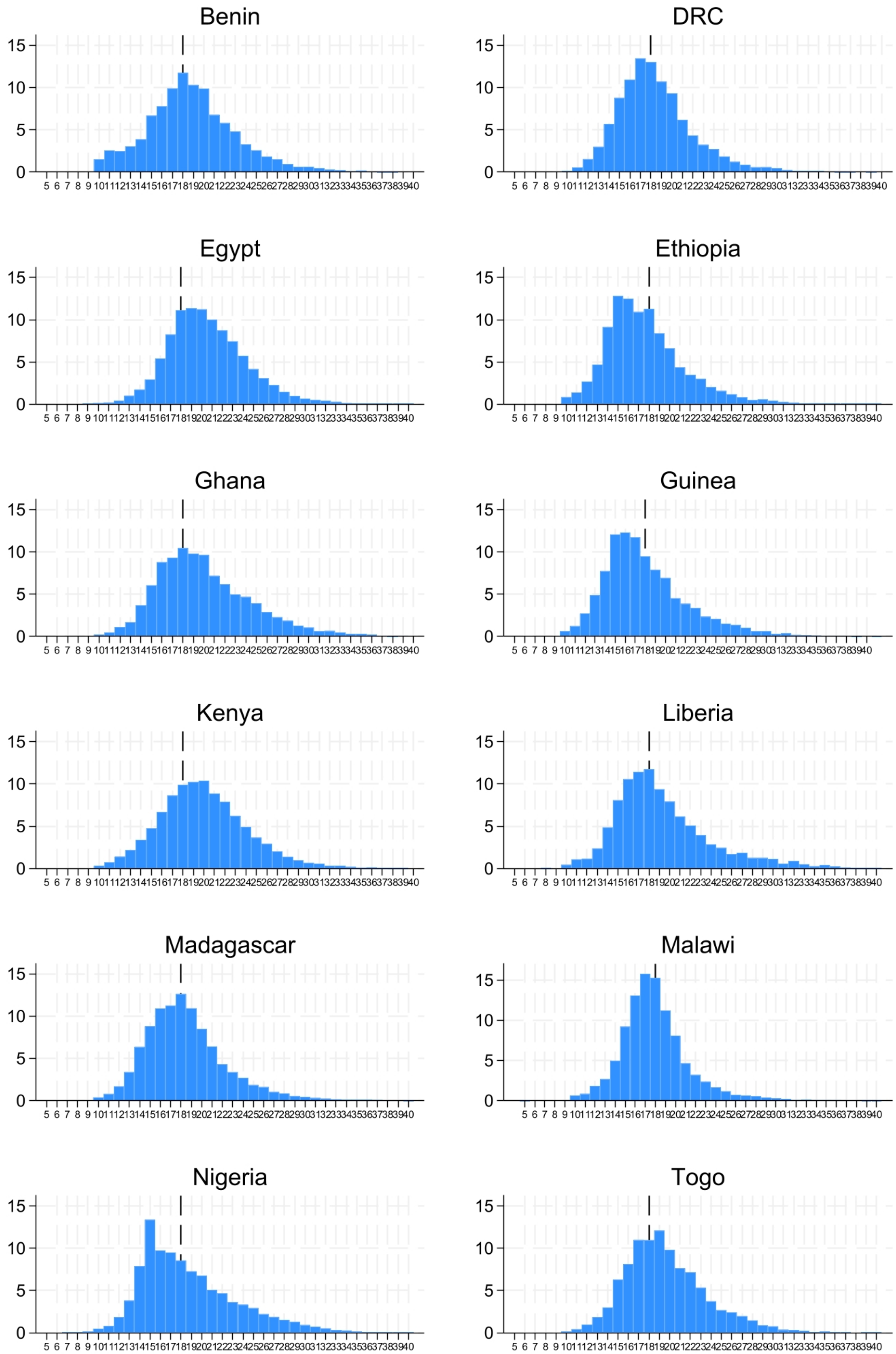
Table A.1: Survey years

Country	Before	Law	After	Child marriage rate before ban
Benin	2001	2004	2017	35%
DRC	2007	2009	2013	35%
Egypt	2005	2008	2014	32%
Ethiopia	2000	2000	2016	55%
Ghana	1998	1999	2014	32%
Guinea	2005	2008	2018	69%
Kenya	2008	2014	2022	26%
Liberia	2007	2012	2019	39%
Madagascar	1997	2007	2021	39%
Malawi	2010	2015	2020	46%
Nigeria	2003	2003	2018	47%
Togo	1998	2007	2013	34%

Notes: Child marriage rates are computed from pre-ban DHS on cohorts aged 18-49.

Sources: WORLD Policy Analysis Center, DHS, MICS.

Figure A.1: Distribution of age at first union by country



Notes: This histogram shows the distribution of ages at first union by country. The red vertical dashed line indicates age 18. The part of the distribution to the left of the red line represents girls who entered their first union before turning 18, while the part to the right represents those who entered their first union at 18 or later. Each bar reflects the percentage of women who experienced their first union at a given age. Sample of women aged 18-40 years old.

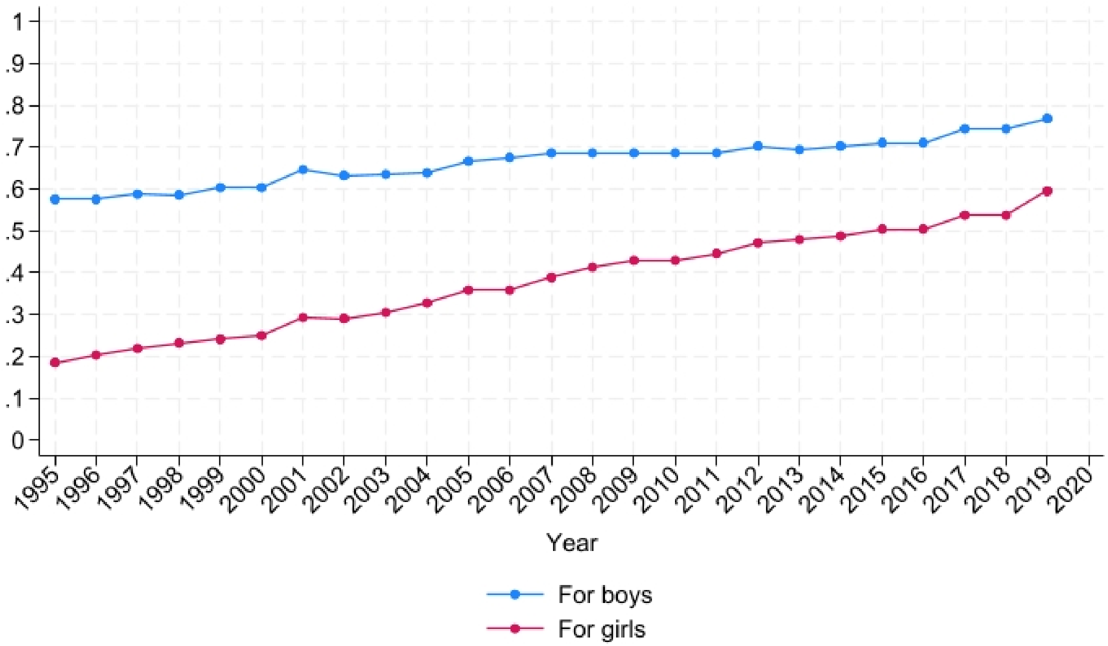
Table A.2: Means of marriage indicators by country, rural status, and bride price custom

			Zone of residence		Prevailing mode of obtaining a wife		
		Total	Urban	Rural	Bride Price	Other	NA
Benin	Married under 18	0.32	0.25	0.37	0.25	0.40	0.38
	Married under 15	0.11	0.09	0.13	0.09	0.12	0.14
	Age at first union	18.87	19.66	18.30	19.44	18.10	18.35
	Age of partner at first union	26.21	26.70	25.86	26.44	26.59	25.86
DRC	Married under 18	0.36	0.29	0.40	0.36	0.33	0.42
	Married under 15	0.09	0.07	0.10	0.09	0.07	0.11
	Age at first union	18.48	19.01	18.20	18.51	18.57	18.18
	Age of partner at first union	25.43	26.61	24.84	25.51	25.09	24.93
Egypt	Married under 18	0.20	0.13	0.25	0.21	0	0.13
	Married under 15	0.04	0.02	0.04	0.04	0	0.02
	Age at first union	20.58	21.55	19.87	20.45	0	21.49
	Age of partner at first union	26.65	27.43	26.08	26.60	0	26.98
Ethiopia	Married under 18	0.44	0.26	0.54	0.49	0.50	0.39
	Married under 15	0.15	0.09	0.18	0.15	0.20	0.13
	Age at first union	17.66	19.21	17.02	17.49	16.92	18.08
	Age of partner at first union	25.26	26.61	24.75	25.28	24.28	25.64
Ghana	Married under 18	0.22	0.15	0.29	0.22	0	0.23
	Married under 15	0.05	0.03	0.07	0.05	0	0.06
	Age at first union	20.05	21.21	19.05	20.25	0	19.49
	Age of partner at first union	27.42	28.42	26.53	27.47	0	27.29
Guinea	Married under 18	0.44	0.31	0.52	0.42	0.31	0.51
	Married under 15	0.14	0.09	0.18	0.12	0.07	0.19
	Age at first union	18.06	18.89	17.68	18.14	19.43	17.79
	Age of partner at first union	31.01	31.04	30.99	30.04	32.35	32.28
Kenya	Married under 18	0.21	0.15	0.25	0.19	0	0.25
	Married under 15	0.06	0.04	0.07	0.05	0	0.08
	Age at first union	20.01	20.85	19.48	20.29	0	19.45
	Age of partner at first union	26.33	26.53	26.22	26.17	0	26.64
Liberia	Married under 18	0.29	0.21	0.34	0.32	0	0.26
	Married under 15	0.07	0.05	0.09	0.08	0	0.06
	Age at first union	19.31	20.06	18.89	19.12	0	19.54
	Age of partner at first union	26.32	27.74	25.51	25.95	0	26.77
Madagascar	Married under 18	0.36	0.21	0.41	0.41	0.34	0.39
	Married under 15	0.10	0.05	0.12	0.14	0.09	0.13
	Age at first union	18.51	19.90	18.12	17.85	18.64	18.31
	Age of partner at first union	23.66	24.74	23.33	25.63	23.38	24.04
Malawi	Married under 18	0.42	0.28	0.44	0.42	0.42	0.41
	Married under 15	0.09	0.05	0.10	0.08	0.10	0.09
	Age at first union	17.94	19.01	17.80	18.01	17.89	18.06
	Age of partner at first union	23.06	24.52	22.88	23.87	22.85	23.11
Nigeria	Married under 18	0.38	0.23	0.49	0.27	0.34	0.56
	Married under 15	0.12	0.07	0.16	0.08	0.11	0.19
	Age at first union	18.79	20.45	17.85	20.05	18.93	17.12
	Age of partner at first union	28.80	30.09	28.06	29.26	28.65	28.23
Togo	Married under 18	0.25	0.16	0.31	0.21	0.33	0.28
	Married under 15	0.05	0.03	0.07	0.04	0.07	0.05
	Age at first union	19.63	20.92	18.97	20.22	18.73	19.15
	Age of partner at first union	27.63	28.61	27.14	27.95	27.39	27.03
Total	Married under 18	0.33	0.21	0.39	0.27	0.39	0.39
	Married under 15	0.09	0.06	0.11	0.07	0.11	0.12
	Age at first union	19.01	20.31	18.37	19.66	18.22	18.39
	Age of partner at first union	26.50	27.74	25.89	27.03	24.41	26.81

Notes: This table reports key early marriage statistics by country, zone of residence, and main marriage practice for women aged 18-40. The prevailing mode of obtaining a wife is based on the ethnicity within whose boundaries a woman is located in, as documented by the Murdock Ethnographic Atlas (EA) and Tribal Map of Africa. The zone of residence is informed in DHS surveys.

Sources: DHS, MICS, IPUMS, Murdock (1959), Murdock (1967).

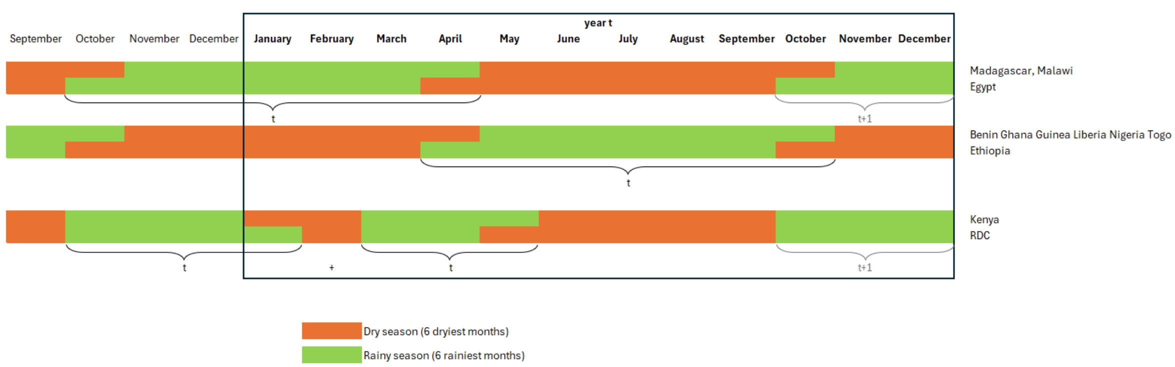
Figure A.2: Share of countries banning child marriage



Sources: WORLD Policy Analysis Center, authors calculation.

A1.2 Weather Data

Figure A.3: Association of rainy seasons to calendar years



Notes: The rainy season is defined as the 6 rainiest months of the year. For countries with one rainy season which begins year *t-1* and end year *t*, precipitation during the rainy season is summed up and associated to the calendar year *t*. For countries with one rainy season which begins year *t* and end year *t*, precipitation during the rainy season is summed up and associated to the calendar year *t*. For countries with two rainy seasons, precipitation of rainy season months belonging to the second half of the year *t-1* is summed up with precipitation of rainy season months belonging to the first half of year *t* and associated to the calendar year *t*.

Table A.3: Mean share of regions affected by droughts per year, by country and drought definition

Country	<-1sd	<-1.5sd	<-2sd	Number of regions
Benin	0.17	0.07	0.02	12
DRC	0.07	0.06	0.05	11
Egypt	0.07	0.03	0.03	4
Ethiopia	0.17	0.07	0.02	11
Ghana	0.15	0.07	0.03	10
Guinea	0.16	0.05	0.01	8
Kenya	0.12	0.02	0.00	47
Liberia	0.15	0.06	0.01	15
Madagascar	0.11	0.04	0.03	23
Malawi	0.11	0.05	0.02	28
Nigeria	0.15	0.07	0.03	37
Togo	0.18	0.07	0.02	6
Total (avg)	**0.13**	**0.06**	**0.02**	**17.67**

Notes: This table represents the average proportion of regions affected by a drought per year, by country and drought definition.

Sources: DHS regions, CHIRPS.

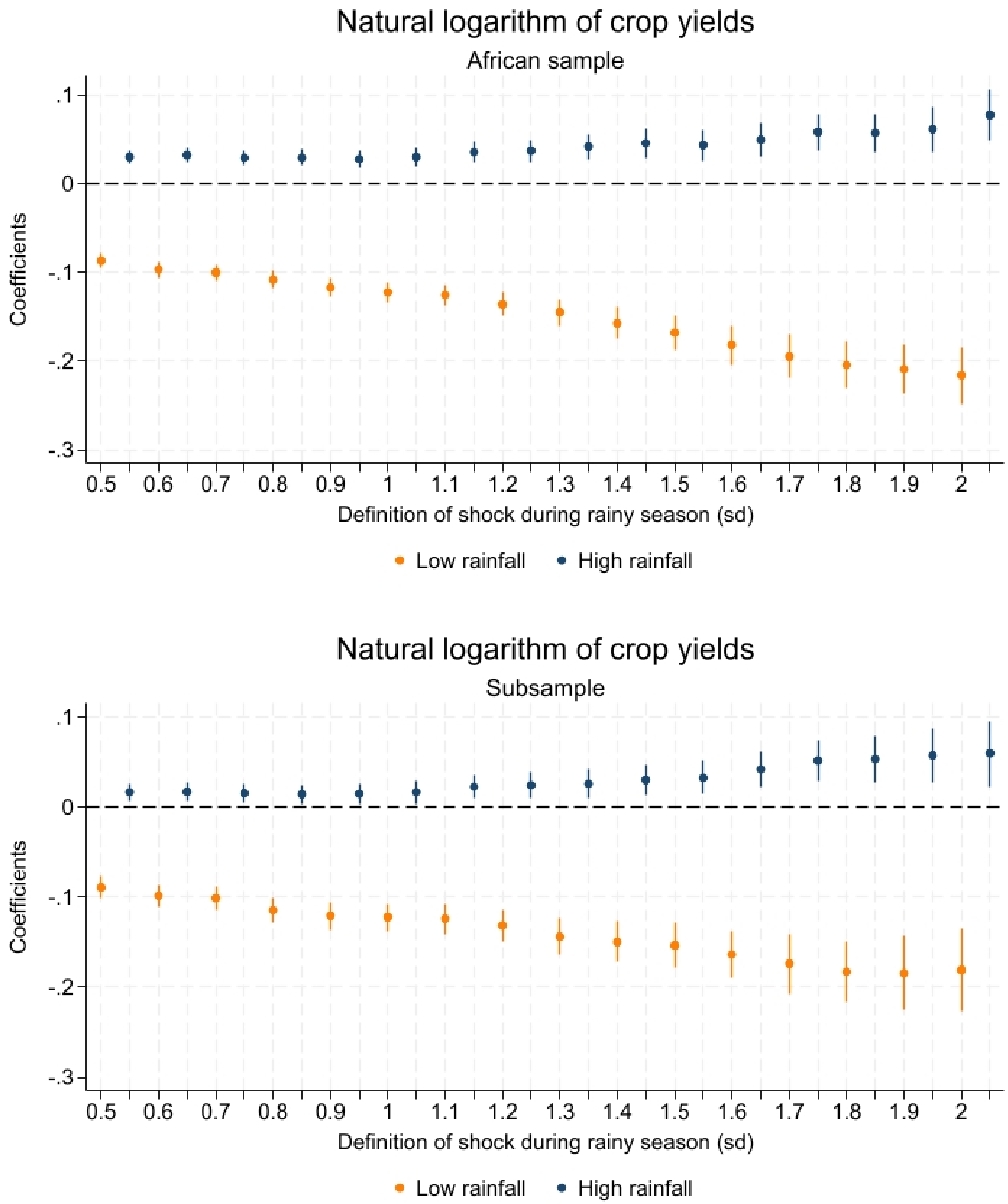
Table A.4: Share of women with at least one drought during adolescence (10-17yo), by country

Country	<-1sd	<-1.5sd	<-2sd
Benin	0.66	0.28	0.00
DRC	0.20	0.13	0.02
Egypt	0.07	0.00	0.00
Ethiopia	0.88	0.51	0.14
Ghana	0.70	0.15	0.04
Guinea	0.68	0.38	0.13
Kenya	0.68	0.08	0.00
Liberia	0.75	0.36	0.02
Madagascar	0.36	0.06	0.00
Malawi	0.67	0.17	0.00
Nigeria	0.51	0.31	0.09
Togo	0.87	0.22	0.01
Total (avg)	**0.55**	**0.20**	**0.04**

Notes: Restricts the sample to adult women aged 18 to 40 years old.

Sources: DHS, MICS, CHIRPS, author's calculations.

Figure A.4: Impact of rainy season climate anomalies on the log of agricultural yields



Notes: Robust standard errors in parentheses clustered at the region level. 90% confidence interval are represented. Regressions are run of maize, wheat, soybean, and rice. The African sample of columns 1 to 3 includes Angola, Benin, Burkina Faso, Burundi, CAR, Cameroon, Chad, Comoros, Congo, DRC, Cote d'Ivoire, Egypt, Eritrea, Ethiopia, Gabon, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mozambique, Namibia, Niger, Nigeria, Rwanda, Sao Tome and Principe, Senegal, Sierra Leone, South Africa, Sudan, Tanzania, The Gambia, Togo, Tunisia, Uganda, Zambia, and Zimbabwe for which we have geographical boundaries of DHS regions. The subsample includes Benin, DRC, Egypt, Ethiopia, Ghana, Guinea, Kenya, Liberia, Madagascar, Malawi, Nigeria, and Togo.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Sources: CHIRPS, DHS regional boundaries, Toshichika Iizumi & Toru Sakai (2020), author's calculations.

A1.3 Historical Origins of Marriage Payments

The prevailing economic view on marriage payments is rooted in the work of Becker (1991). When a woman's shadow price in the marriage market is lower than her contribution to household resources, a bride price may emerge to encourage her participation in the marriage market. Boserup (2007) proposed a hypothesis based on women's agricultural productivity and contributions to household resources. She argued that in regions like Africa, where plough technology was historically absent but light tools were extensively used, female labor was highly valued. In such contexts, marriage payments shifted toward the bride's family, as women contributed more to the household than they consumed. This hypothesis is empirically supported by Giuliano (2015), who found a positive correlation between women's agricultural roles and the direction of marriage payments. Alesina et al. (2021) also provided empirical evidence for this hypothesis, observing a positive correlation between ethnic groups that practice bride price and agricultural practices historically reliant on female labor. Corno et al. (2020) extended this literature with an economic model. In most societies, women traditionally move to the groom's family upon marriage (virilocality) and contribute to its finances. Where women were historically productive, losing a daughter could be economically disadvantageous. In such cases, bride price payments would be necessary to compensate the bride's family for their loss. This implies that, for a marriage to occur, the bride price must exceed the woman's contribution to her parents' household. Intuitively, the poorer households want to receive the marriage payment, and the richest ones want to pay it.

A1.4 Prevailing Modes of Obtaining a Wife in Murdock (1967)

Murdock's *Ethnographic Atlas* (Murdock, 1967) documents the "prevailing mode of obtaining a wife" or "mode of marriage," with seven options:

- Bride-price or bride-wealth, i.e., transfer of a substantial consideration in the form of livestock, goods, or money from the groom or his relatives to the kinsmen of the bride;
- Dowry, i.e., transfer of a substantial amount of property from the bride's relatives to the

bride, the groom, or the kinsmen of the latter;

- Gift exchange, i.e., reciprocal exchange of gifts of substantial value between the relatives of the bride and groom, or a continuing exchange of goods and services in approximately equal amounts between the groom or his kinsmen and the bride's relatives;
- Absence of any significant consideration, or bridal gifts only;
- Bride-service, i.e., a substantial material consideration in which the principal element consists of labor or other services rendered by the groom to the bride's kinsmen;
- Token bride-price, i.e., a small or symbolic payment only;
- Exchange, i.e., transfer of a sister or other female relative of the groom in exchange for the bride.

Table A.5: Prevailing mode of obtaining a wife (V6)

	Perfect EA match		Imperfect EA match	
Mode of marriage	Freq.	Percent	Freq.	Percent
Absence of consideration	6,284	3.86	6,345	3.90
Bride price or wealth to bride's family	84,272	51.82	106,730	65.63
Bride service to bride's family	13,563	8.34	16,731	10.29
Dowry	881	0.54	987	0.61
Sister or female relative exchanged	2,149	1.32	2,556	1.57
Token bride price	7,653	4.71	8,042	4.94
Not in EA	47,828	29.41	21,239	13.06
Total	162,630	100.00	162,630	100.00

Notes: "Perfect EA match" means that information on the mode of marriage is found directly associating Murdock (1959)'s *Tribal Map* with the *Ethnographic Atlas* (EA) (Murdock, 1967). Since not all tribes are described in the EA, I use Lowes (2017)'s method to match tribes with the most physically proximate group described in the EA. The distribution is drawn from women aged 18-40.

Sources: Murdock (1959; 1967), DHS, MICS

Table A.6: Main descriptive statistics by country

Country	Absence of consideration	Bride Price or wealth to bride's family	Bride Service to bride's family	Dowry	Sister or female relative exchanged for bride	Token bride price	Ethnicity not in EA
Benin	0	0.51	0.07	0	0	0.01	0.41
DRC	0	0.84	0.01	0	0.01	0.04	0.10
Egypt	0	0.88	0	0	0	0	0.12
Ethiopia	0.12	0.19	0	0.08	0.02	0.03	0.57
Ghana	0	0.74	0	0	0	0	0.26
Guinea	0	0.60	0	0	0.05	0	0.35
Kenya	0	0.67	0	0	0	0	0.33
Liberia	0	0.53	0	0	0	0	0.47
Madagascar	0.21	0.05	0	0	0	0.48	0.26
Malawi	0.12	0.15	0.53	0	0	0	0.20
Nigeria	0	0.53	0.08	0	0.02	0.01	0.36
Togo	0	0.58	0.12	0	0.10	0.01	0.18
Total	0.04	0.52	0.08	0.01	0.01	0.05	0.29

Notes: This table reports the share of women aged 18-40 by the prevailing mode of obtaining a wife of the ethnicity within whose boundaries they are located in, as documented by the Murdock Ethnographic Atlas (EA) and Tribal Map of Africa.

Sources: DHS, MICS, IPUMS, Murdock (1959), Murdock (1967)

Table A.7: Alternate mode of obtaining a wife (V7)

Mode	Frequency	Percent
Dowry, to bride from her family	506	0.31%
Token bride price	8,144	5.01%
Bride service, to bride's family	18,929	11.64%
Bride price or wealth, to bride's family	134	0.08%
No alternate mode	87,090	53.55%
Missing/Not in EA	47,828	29.41%
Total	162,631	100.00%

Notes: This variable describes the alternative types of transfer or exchange at marriage, or supplementary practices. Information on the mode of marriage is found directly associating Murdock (1959)'s *Tribal Map* with the *Ethnographic Atlas* (EA) (Murdock, 1967). The sample is restricted to women aged between 18 to 40 years old in the subsample of 12 African countries.

Sources: Murdock (1959; 1967), DHS, MICS

Table A.8: Homogeneity of bride price practice among ethnicities sharing the same cultural group (CG)

	Freq.	Percent
Non-Bride Price ethnicities sharing CG with only non-Bride Price ethnicities	33	3.96
Bride Price ethnicities sharing CG with non-Bride Price ethnicities	339	40.70
Bride Price ethnicities sharing CG with only Bride Price ethnicities	461	55.34
Total	833	100.00

Sources: Murdock (1959; 1967)

Table A.9: Share of girls exposed to droughts during adolescence, by bride price tradition

	Drought size		
Marriage payment	<-1sd	<-1.5sd	<-2sd
Bride Price	0.53	0.21	0.04
Not Bride Price	0.56	0.16	0.04
Total	0.54	0.19	0.04

Notes: Restricts the sample to adult women aged 18 to 40 years old.

Sources: DHS, MICS, CHIRPS, author's calculations.

A1.5 Prevailing Profile of Marital Residence

Murdock's *Ethnographic Atlas* (Murdock, 1967) documents the prevailing profile of marital residence in the society with several options:

- Avunculocal, i.e., normal residence with or near the maternal uncle or other male matrilineal kinsmen of the husband;
- Ambilocal, i.e., residence established optionally with or near the parents of either the husband or the wife, depending upon circumstances or personal choice, where neither alternative exceeds the other in actual frequency by a ratio greater than two to one;
- Optionally uxorilocal or avunculocal. This may be the case in a uxorilocal society where many men marry a MoBrDa and thus, in fact, live avunculocally;
- Optionally patrilocal (or virilocal) or avunculocal;
- Matrilocal, i.e., normal residence with or near the female matrilineal kinsmen of the wife;

- Neolocal, i.e., normal residence apart from the relatives of both spouses or at a place not determined by the kin ties of either;
- Nonestablishment of a common household, i.e., where both spouses remain in their natal households, sometimes called ”duolocal” or ”natolocal” residence;
- Patrilocal, i.e., normal residence with or near the male patrilineal kinsmen of the husband;
- Uxorilocal. Equivalent to ”matrilocal” but confined to instances where the wife's matrikin are not aggregated in matrilocal and matrilineal kin groups;
- Virilocal. Equivalent to ”patrilocal” but confined to instances where the husband's patrikin are not aggregated in patrilocal and patrilineal kin groups;
- Ambilocal, with a marked preponderance of uxorilocal practice (i.e., uxorilocal option exceeds virilocal option in actual frequency by a ratio greater than two to one);
- Ambilocal, with a marked preponderance of virilocal practice (i.e., virilocal option exceeds uxorilocal option in actual frequency by a ratio greater than two to one).

Table A.10: Mode of post-marital residence (V12)

Mode	Frequency	Percent
Matrilocal (4.21%)		
Matrilocal	6,800	4.18%
Uxorilocal	51	0.03%
Patrilocal (58.31%)		
Patrilocal	84,902	52.21%
Virilocal	9,916	6.10%
Other (7.50%)		
Avunculocal	9,121	5.61%
Neolocal	293	0.18%
No common residence	709	0.44%
Optionally patrilocal	1,753	1.08%
Optionally uxorilocal or avunculocal	309	0.19%
Missing/Not in EA	48,777	29.99%
Total	162,631	100.00%

Notes: This variable describes the prevailing profile of marital residence in the society. Information on the mode of marriage is found directly associating Murdock (1959)'s *Tribal Map* with the *Ethnographic Atlas* (EA) (Murdock, 1967). The sample is restricted to women aged between 18 to 40 years old in the subsample of 12 African countries.

Sources: Murdock (1959; 1967), DHS, MICS, IPUMS

Table A.11: Cross-tabulation of matrilocality and bride price practices

		Not BP	BP	Total
Not Matrilocal	Count	23,680	83,323	107,003
	Row %	22.13%	77.87%	100.00%
	Column %	77.56%	100.00%	93.98%
Matrilocal	Count	6,851	0	6,851
	Row %	100.00%	0%	100.00%
	Column %	22.44%	0%	6.02%
Total	Count	30,531	83,323	113,854
	Row %	26.82%	73.18%	100.00%
	Column %	100.00%	100%	100.00%

Notes: This variable describe the prevailing profile of marital residence and the mode of post-marital residence in the society. Information is found associating Murdock (1959)'s *Tribal Map* with the *Ethnographic Atlas* (EA) (Murdock, 1967). The sample is restricted to women aged between 18 to 40 years old in the subsample of 12 African countries. Matrilocal ethnicities include matrilocal and uxorilocal ethnicities. Non matrilocal ethnicities include patrilocal, virilocal, avunculocal, neolocal, no common residence, optionally patrilocal and optionally uxorilocal or avunculocal ethnicities.

Sources: Murdock (1959; 1967), DHS, MICS

A1.6 The Case Study of Malawi

In Malawi, customary marriages are broadly classified into two types: patrilineal and matrilineal. In patrilineal systems—predominantly found in the northern regions—the wife relocates to her husband's village after marriage. A central feature of this system is the payment of bride price, known as *lobola*, from the groom to the bride's father or guardian. This transfer legitimizes the marriage, grants the husband rights over the children and household property, and permits the relocation of his wife and children to his village. As noted by Mofokeng (2005), *lobola* is "an agreement between the family group of the prospective husband and the family group of the prospective wife that on or before the marriage ceremony, there would be the transfer of property from the family group of the husband, to the family group of the wife in respect of the marriage." Under this tradition, a marriage that proceeds without *lobola* negotiations is considered void. Additional customs also exist. For example, in the Northern district of Karonga, the *Nyakyusa* tradition includes *Kupimbira*, a practice in which families may give a daughter in marriage in exchange for a loan, a cow, or money from a wealthier household (Beegle & Poulin, 2013).

Matrilineal marriage systems are more common in the central and southern regions and can take two main forms: *chikamwini* and *chitengwa*. In *chikamwini* marriages, the husband moves into the wife's village and holds limited rights there. In *chitengwa* marriages, the wife resides in the husband's village, but children still belong to the woman's matrilineal lineage. In both cases, a marriage is not considered valid unless a traditional engagement ceremony, known as *chinkhoswe*, has taken place. This ceremony often occurs months or even years prior to the actual wedding (Mwambene, 2010).

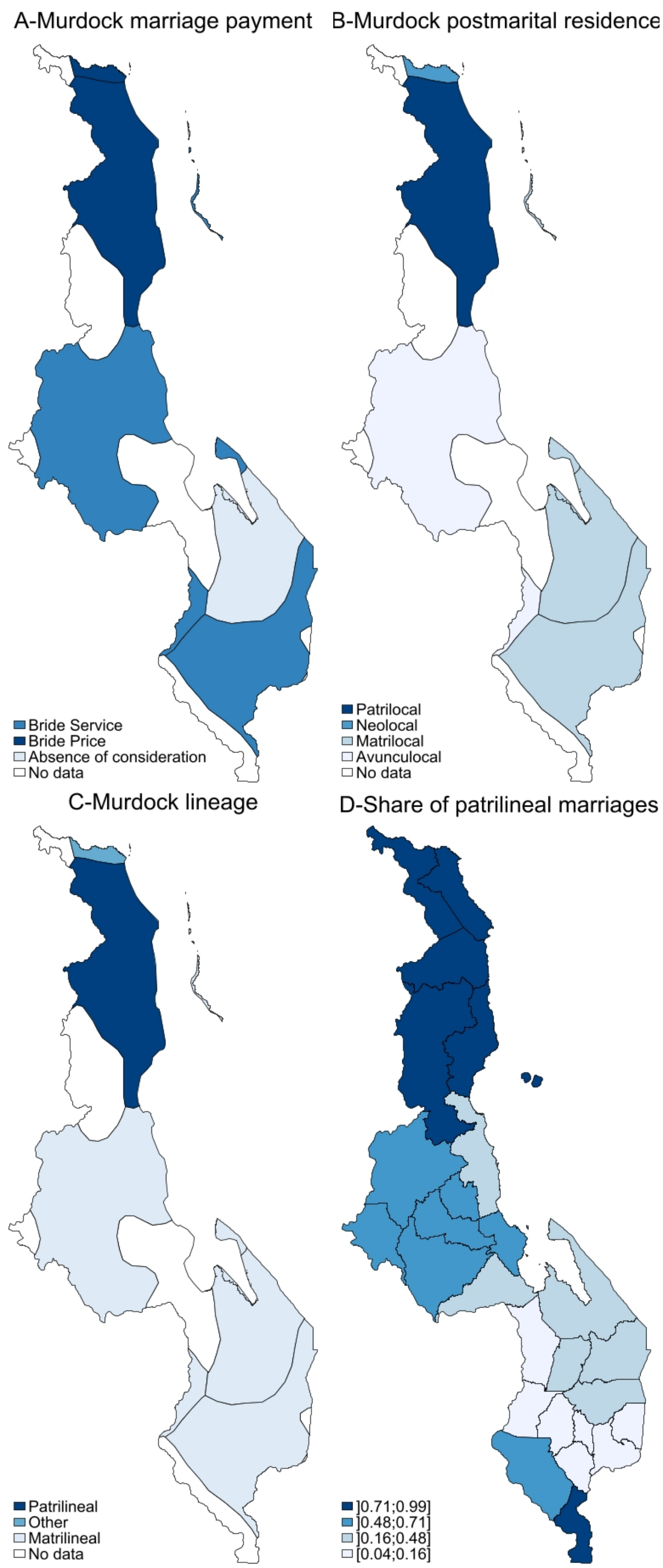
Although traditionally matrilineal, some ethnic groups have shifted toward patrilineal practices over recent decades. Notably, the Chewa—one of the largest ethnic groups practicing matrilineal but patrilocal customs—have increasingly adopted patrilineal norms, including lobola, particularly in certain southern districts (Beegle & Poulin, 2013).

Recent data confirm the regional variation in marriage practices. A survey conducted by Makwemba et al. (2019) shows that bride price remains highly prevalent in the north, where only 4.69% of households report not paying it. In contrast, the practice is less common in the south, where 73.21% of households report not paying a bride price. It is also more widespread in rural areas (with 38.04% of households not paying) than in urban areas (50.92% not paying). These patterns align with lineage systems: 89.10% of individuals in the north are part of patrilineal groups, compared to 34.5% in central Malawi and 25.48% in the south. Dowry, by comparison, is marginal, with only 10% of households in the north and just 2% in the central and southern regions reporting such payments.

Figure A.5-D displays data from the 2019–2020 MICS survey, mapping the marriage customs under which couples marry across Malawi. Patrilineal customs are dominant in the north, where over 70% of couples report marrying under this system. The practice is less common in the central and southern regions, with the exception of one district in the south, which also reports high rates of patrilineal marriage—a finding consistent with the documented evolution of customs in that area.

Importantly, these contemporary patterns strongly reflect the spatial distribution of historical lineage systems. The mapping of ethnic groups based on Murdock's classification (Murdock, 1959; 1967) shows a similar regional pattern of patrilineality—except in the aforementioned southern district—further supporting the notion that historical practices continue to predict current marriage customs in Malawi.

Figure A.5: Recent and historical marriage practices in Malawi

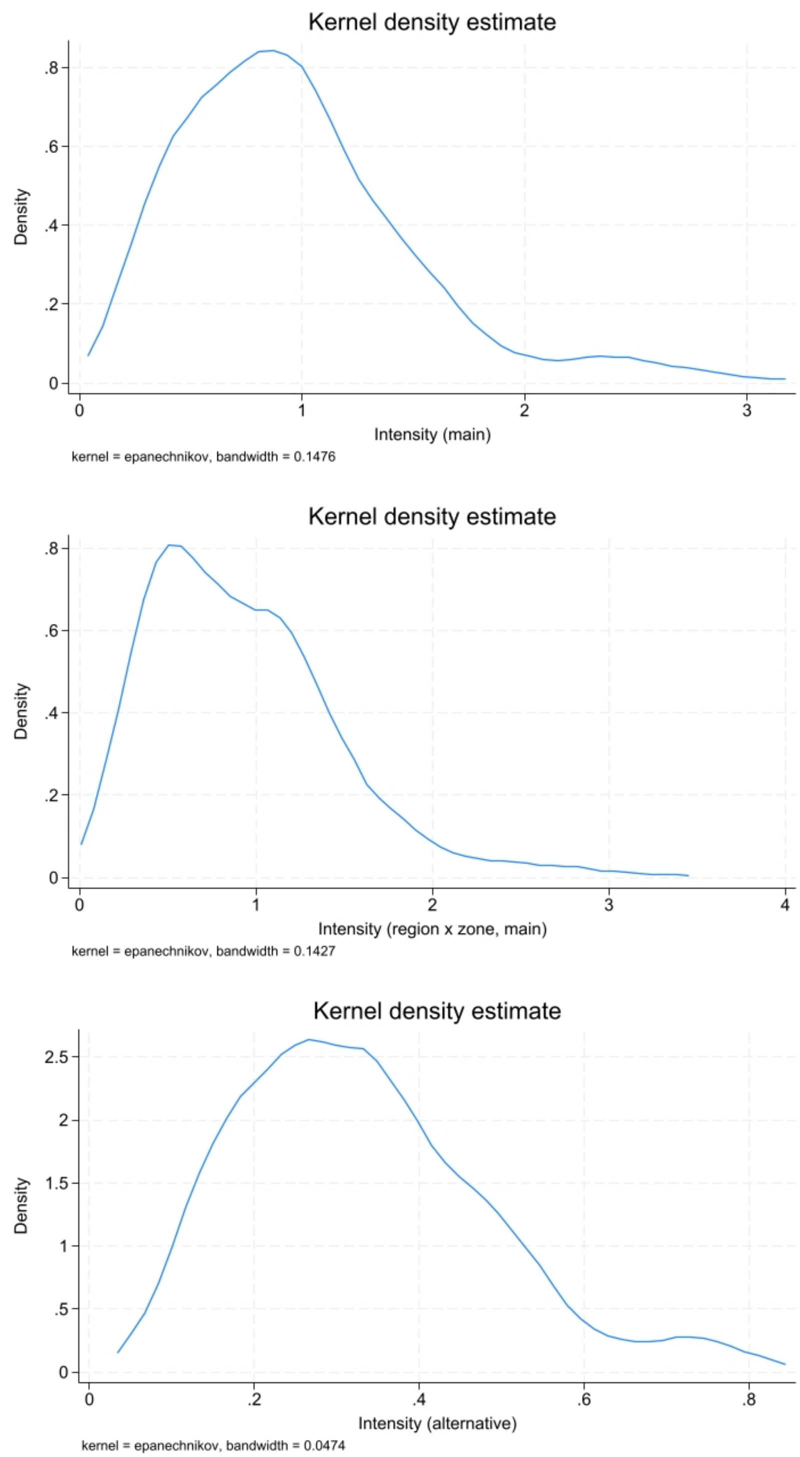


Notes: Figure A, B and C map over Malawi Murdock's ethnicities depending on the prevailing marriage payment, the post-marital residence, and lineage system respectively. Figure D maps districts based on the share of marriages following the patrilineal custom based on MICS 2019-2020 data.

Sources: MICS 2019-2020, Murdock (1959; 1967)

A2 Empirical Strategy

Figure A.6: Distributions of continuous intensity measures among regions



Sources: DHS, author's calculation

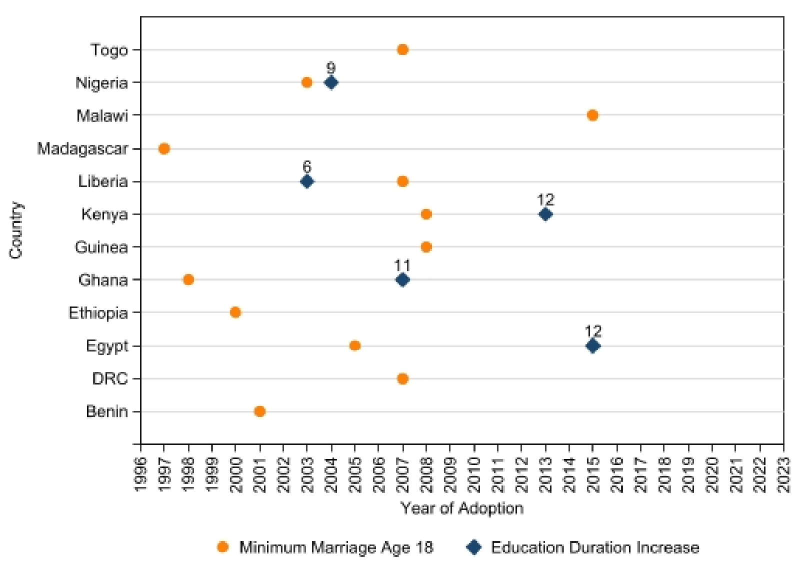
Table A.12: Descriptive statistics of intensity measures

Variable	Obs	Mean	Std. dev.	Min	Max	Sample
Country, women's distribution						
Intensity (Region)	162,630	1.40	0.80	0.27	4.08	Women
Intensity (Region × zone)	160,347	1.40	0.84	0.00	4.23	Women
Intensity Alternative (Region)	162,630	0.45	0.18	0.10	0.92	Women
Urban, women's distribution						
Intensity (Region)	59,299	1.17	0.75	0.27	4.08	Women
Intensity (Region × zone)	57,049	0.99	0.66	0.00	3.46	Women
Intensity Alternative (Region)	59,299	0.39	0.18	0.10	0.92	Women
Rural, women's distribution						
Intensity (Region)	103,329	1.53	0.81	0.27	4.08	Women
Intensity (Region × zone)	103,298	1.63	0.85	0.33	4.23	Women
Intensity Alternative (Region)	103,329	0.49	0.17	0.10	0.92	Women
Country, regions' distribution						
Intensity (Region)	210	1.35	0.76	0.27	4.08	Region
Intensity (Region × zone)	396	1.29	0.77	0	4.233	Region
Intensity Alternative (Region)	210	0.44	0.18	0.10	0.92	Region

Notes: Country, Urban and Rural "women's distribution" are drawn from the sample of 162,631 women aged 18-40 in the 12 countries included in the analysis. Statistics from the "Country, region's distribution" panel are drawn from the sample of 210 regions and 397 subregions (region × zone).

Sources: DHS, MICS, author's calculation

Figure A.7: Timeline of legal reforms by country



Sources: UNESCO Institute for Statistics (2024), WORLD Policy Analysis Center

Table A.13: Descriptive statistics of intensity measures by country

Country	Intensity (Region)	Intensity (Region × zone)	Intensity Alternative (Region)
Benin	1.20	1.17	0.43
	(0.47)	(0.52)	(0.13)
	[0.34–1.80]	[0.34–1.95]	[0.14–0.59]
	N=11,345	N=11,345	N=11,345
DRC	1.14	1.16	0.42
	(0.30)	(0.33)	(0.10)
	[0.60–1.58]	[0.49–1.69]	[0.23–0.54]
	N=12,876	N=12,628	N=12,876
Egypt	0.94	0.93	0.35
	(0.38)	(0.44)	(0.12)
	[0.39–1.38]	[0.38–1.57]	[0.17–0.48]
	N=16,348	N=16,317	N=16,348
Ethiopia	2.07	2.06	0.59
	(0.93)	(0.93)	(0.18)
	[0.87–4.05]	[0.61–4.20]	[0.31–0.85]
	N=11,519	N=11,519	N=11,519
Ghana	0.95	0.94	0.37
	(0.15)	(0.22)	(0.06)
	[0.60–1.14]	[0.53–1.45]	[0.25–0.47]
	N=6,627	N=6,627	N=6,627
Guinea	2.15	2.11	0.70
	(0.39)	(0.50)	(0.10)
	[1.45–2.74]	[1.20–2.82]	[0.49–0.82]
	N=7,738	N=7,738	N=7,738
Kenya	0.91	0.86	0.32
	(0.47)	(0.51)	(0.13)
	[0.27–2.04]	[0.00–2.37]	[0.11–0.66]
	N=22,942	N=21,346	N=22,942
Liberia	1.32	1.31	0.46
	(0.28)	(0.33)	(0.08)
	[0.92–1.83]	[0.67–1.88]	[0.33–0.58]
	N=5,424	N=5,409	N=5,424
Madagascar	1.48	1.48	0.47
	(0.75)	(0.79)	(0.15)
	[0.41–3.24]	[0.16–3.29]	[0.19–0.75]
	N=13,264	N=13,055	N=13,264
Malawi	1.37	1.37	0.52
	(0.30)	(0.32)	(0.07)
	[0.91–2.02]	[0.62–2.06]	[0.39–0.66]
	N=17,480	N=17,452	N=17,480
Nigeria	1.93	1.97	0.51
	(1.18)	(1.22)	(0.26)
	[0.27–4.08]	[0.16–4.23]	[0.10–0.92]
	N=30,012	N=29,856	N=30,012
Togo	1.07	1.08	0.38
	(0.37)	(0.40)	(0.11)
	[0.68–1.74]	[0.52–1.84]	[0.23–0.54]
	N=7,055	N=7,055	N=7,055
Total	1.40	1.40	0.45
	(0.80)	(0.84)	(0.18)
	[0.27–4.08]	[0.00–4.23]	[0.10–0.92]
	N=162,630	N=160,347	N=162,630

Note: Standard deviations in parentheses. Min–Max in brackets. N indicates the number of observations.

Sources: DHS, MICS, author's calculation.

A3 Results

A3.1 Effectiveness of Child Marriage Bans

Table A.14: Effectiveness of bans: child marriage (intensity not standardized)

Sample	(1) Country	(2) Urban	(3) Rural	(4) Urban	(5) Rural	(6) Urban	(7) Rural
Panel A: Married before 18							
Exposed × Intensity	0.003	-0.030***	0.013	-0.033***	0.011	-0.116***	0.038
	(0.008)	(0.008)	(0.011)	(0.010)	(0.010)	(0.034)	(0.041)
Mean Y if not exposed	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	162,630	59,298	103,329	57,048	103,298	59,298	103,329
R^2	0.125	0.097	0.121	0.097	0.121	0.096	0.121
Panel B: Married before 15							
Exposed × Intensity	-0.007	-0.018***	-0.002	-0.018**	-0.001	-0.074***	-0.009
	(0.004)	(0.006)	(0.006)	(0.007)	(0.006)	(0.022)	(0.024)
Mean Y if not exposed	0.09	0.06	0.11	0.06	0.11	0.06	0.11
Observations	162,630	59,298	103,329	57,048	103,298	59,298	103,329
R^2	0.056	0.047	0.060	0.047	0.060	0.047	0.060
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r ×z	r ×z	r ext	r ext

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Columns 1, 2 and 3 report results using the pre-ban child marriage intensity measured at the region level, taking into account both the intensive and extensive margins of child marriage. Columns 4 and 5 use a similar measure of intensity but computed at the $region \times zone$ level (rural and urban areas have different intensity levels within the same region). Columns 6 and 7 report results using the intensity measure computed at the regional level and only including the extensive margin of child marriage. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.15: Effectiveness of bans: binary intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Married before 18						
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Panel A: Married before 18							
Exposed × HI	-0.002	-0.043***	0.005	-0.020	0.000	-0.035***	0.011
	(0.010)	(0.014)	(0.013)	(0.014)	(0.011)	(0.012)	(0.012)
Mean Y if not exposed	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	162,630	59,298	103,329	59,298	103,329	59,298	103,329
R^2	0.125	0.096	0.121	0.096	0.121	0.096	0.121
Panel B: Married before 15							
Exposed × HI	-0.012**	-0.024***	-0.009	-0.021**	-0.012**	-0.018***	-0.002
	(0.005)	(0.008)	(0.007)	(0.009)	(0.006)	(0.007)	(0.006)
Mean Y if not exposed	0.09	0.06	0.11	0.06	0.11	0.06	0.11
Observations	162,630	59,298	103,329	59,298	103,329	59,298	103,329
R^2	0.056	0.047	0.060	0.046	0.061	0.047	0.060
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r ×z	r ×z	r ext	r ext

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable HI is a dummy variable equal to 1 if the ban intensity is above the mean, 0 otherwise. The econometric specification used is the following: $ChildMarried_{i,b,r,c} = \delta_0 + \delta_1 Exposed_{b,c} \times HI_{r,c} + FE_{r,c} + FE_{b,c} + \varepsilon_{i,b,r,c}$. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.16: Effectiveness of bans: other outcomes (regional intensity)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Age first marriage		Age first birth		Years of education	
Sample	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	0.585***	0.284***	0.572***	0.342***	0.426***	-0.078
	(0.083)	(0.079)	(0.077)	(0.064)	(0.089)	(0.066)
Region FE	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X
Definition of intensity	r	r	r	r	r	r
Mean Y if not exposed	21.07	19.04	21.24	19.51	8.18	4.42
Observations	43,871	89,081	43,313	88,025	56,994	88,145
R^2	0.184	0.159	0.188	0.147	0.259	0.424

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. The variable *Intensity* is standardized with mean 0 and standard deviation 1. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location taking into account both the intensive and extensive margins of child marriage. The table reports results using the pre-ban child marriage intensity measured at the $region$ level (rural and urban areas have the same intensity levels within the same region). *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.17: Effectiveness of bans: other outcomes (alternative intensity)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Age first marriage		Age first birth		Years of education	
Sample	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	0.565***	0.331***	0.544***	0.371***	0.395***	-0.041
	(0.077)	(0.077)	(0.072)	(0.067)	(0.083)	(0.074)
Region FE	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X
Definition of intensity	r ext	r ext	r ext	r ext	r ext	r ext
Mean Y if not exposed	21.07	19.04	21.24	19.51	8.18	4.42
Observations	43,871	89,081	43,313	88,025	56,994	88,145
R^2	0.184	0.160	0.188	0.147	0.260	0.424

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. The variable *Intensity* is standardized with mean 0 and standard deviation 1. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location taking into account only the extensive margin of child marriage. The table reports results using the pre-ban child marriage intensity measured at the $region$ level (rural and urban areas have the same intensity levels within the same region). *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

A3.2 Country-Level Heterogeneity

Tables A.18 and A.19 present country-level estimates. The bans do not appear effective in Benin[52], Guinea, the DRC, Ethiopia[53], or Madagascar. In contrast, they significantly reduce child marriage in urban areas of Ghana, Kenya, Nigeria, and Togo, and in rural areas of Egypt, Liberia, and Malawi. The *leave-one-country-out* analysis shown in Figure A.8 indicates that Nigeria drives most of the urban effect. When Nigeria is excluded, the average impact of bans in urban areas becomes statistically insignificant. Conversely, the null effect observed in rural Nigeria bias toward zero the average impact in rural areas. Once Nigeria is excluded, the bans significantly reduce marriages under age 15 in rural settings. In some cases, bans may have unintended adverse effects, which would need further examination. This is notably the case in rural Benin (significant at the 10% level) and urban Guinea (significant at the 5% level). Results for rural Egypt should be interpreted with caution, as there is evidence of increased underreporting of child marriage following the ban, raising concerns about data reliability (see Figure A.18). Tables A.20 and A.21

[52] Batyra & Pesando (2021) also show that changes in minimum-age-at-marriage laws were not effective in curbing early marriage in Benin.

[53] In comparison, McGavock (2021) found that in Ethiopia, the legal reform reduced the likelihood of marriage before age 16 by approximately 3.5 percentage points (a 10% decline relative to the pre-reform baseline). Conversely, the probability of marrying at age 18 or later rose by about 2.24 percentage points.

report estimates using the alternative measure of ban intensity, confirming the robustness of the main findings.

Importantly, even though the effectiveness of child marriage bans is most sensitive to the exclusion of Nigeria, removing this country from the main specification—where I assess how droughts affect the bans' effectiveness depending on the practice of bride price—does not alter the results (see Table A.22).

Table A.18: Effectiveness of bans by country

Sample	(1) Benin	(2) RDC	(3) Egypt	(4) Ethiopia	(5) Ghana	(6) Guinea	(7) Kenya	(8) Liberia	(9) Madagascar	(10) Malawi	(11) Nigeria	(12) Togo
Panel A: Entire country												
Dep. Var.	Married under 18											
Exposed	0.030	0.037	-0.025***	-0.012	0.016	0.069	-0.011	-0.047	0.012	-0.038*	0.006	-0.071**
$\times$ Intensity	(0.060)	(0.131)	(0.148)	(0.203)	(0.785)	(0.003)	(0.210)	(0.179)	(0.228)	(0.077)	(0.149)	(0.003)
	[0.208]	[0.432]	[0.000]	[0.578]	[0.802]	[0.264]	[0.474]	[0.138]	[0.186]	[0.080]	[0.704]	[0.048]
R^2	0.060	0.045	0.053	0.108	0.029	0.034	0.076	0.025	0.049	0.027	0.243	0.045
Obs.	11345	12876	16348	11519	6627	7738	22942	5424	13264	17480	30012	7055
Intensity	r	r	r	r	r	r	r	r	r	r	r	r
Dep. Var.	Married under 15											
Exposed	-0.004	-0.014	-0.021***	-0.011	-0.017	0.005	-0.027**	0.028	0.004	-0.052***	-0.001	-0.000
$\times$ Intensity	(0.706)	(0.353)	(0.008)	(0.105)	(0.592)	(0.751)	(0.000)	(0.166)	(0.492)	(0.000)	(0.802)	(0.993)
	[0.704]	[0.370]	[0.000]	[0.506]	[0.718]	[0.862]	[0.024]	[0.222]	[0.620]	[0.000]	[0.888]	[0.970]
R^2	0.029	0.014	0.013	0.044	0.012	0.018	0.042	0.018	0.038	0.024	0.078	0.014
Obs.	11345	12876	16348	11519	6627	7738	22942	5424	13264	17480	30012	7055
Intensity	r	r	r	r	r	r	r	r	r	r	r	r
Region FE	X	X	X	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X	X	X	X

Notes: p-values in parentheses are based on standard errors. Wild bootstrap p-values are shown in square brackets below (1000 repetitions). Regressions are restricted to women aged between 18 and 40. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Significance levels are based on the wild bootstrap p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.19: Effectiveness of bans by country and residence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample	Benin	RDC	Egypt	Ethiopia	Ghana	Guinea	Kenya	Liberia	Madagascar	Malawi	Nigeria	Togo
Panel B: Urban areas												
Dep. Var.	Married under 18											
Exposed	-0.009	0.001	0.033	0.024	-0.105**	0.188**	-0.020	0.004	-0.013	-0.113	-0.039***	-0.233***
$\times$ Intensity	(0.722)	(0.976)	(0.471)	(0.378)	(0.023)	(0.001)	(0.281)	(0.939)	(0.598)	(0.137)	(0.000)	(0.001)
	[0.804]	[0.982]	[0.340]	[0.798]	[0.046]	[0.032]	[0.298]	[0.936]	[0.738]	[0.164]	[0.000]	[0.000]
R-squared	0.055	0.069	0.034	0.101	0.025	0.022	0.064	0.029	0.031	0.039	0.149	0.031
Obs.	4990	4652	6891	4134	3355	2914	7766	2186	3186	2273	11912	2790
Intensity	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z
Dep. Var.	Married under 15											
Exposed	-0.022	0.005	0.004	0.003	-0.028	0.010	-0.036**	-0.024	0.025	-0.048	-0.018*	-0.052*
$\times$ Intensity	(0.208)	(0.804)	(0.833)	(0.845)	(0.223)	(0.775)	(0.000)	(0.348)	(0.060)	(0.208)	(0.000)	(0.101)
	[0.338]	[0.650]	[0.366]	[0.856]	[0.466]	[0.788]	[0.024]	[0.518]	[0.180]	[0.114]	[0.052]	[0.088]
R^2	0.028	0.016	0.005	0.037	0.008	0.016	0.042	0.028	0.015	0.012	0.051	0.010
Obs.	4990	4652	6891	4134	3355	2914	7766	2186	3186	2273	11912	2790
Intensity	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z
Panel C: Rural areas												
Dep. Var.	Married under 18											
Exposed	0.059*	0.008	-0.039***	-0.028	0.224	-0.015	0.011	-0.151**	0.016	-0.055**	0.025*	-0.070
$\times$ Intensity	(0.016)	(0.841)	(0.089)	(0.013)	(0.037)	(0.702)	(0.368)	(0.018)	(0.185)	(0.028)	(0.000)	(0.052)
	[0.058]	[0.892]	[0.000]	[0.282]	[0.346]	[0.512]	[0.670]	[0.040]	[0.222]	[0.040]	[0.068]	[0.122]
R^2	0.047	0.024	0.047	0.052	0.026	0.022	0.093	0.014	0.045	0.024	0.252	0.019
Obs.	6355	7976	9426	7385	3272	4824	13580	3223	9869	15179	17944	4265
Intensity	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z
Dep. Var.	Married under 15											
Exposed	0.006	-0.017	-0.032***	-0.015	0.022	-0.009	-0.019	0.041	0.005	-0.052***	0.008	0.031
$\times$ Intensity	(0.717)	(0.508)	(0.004)	(0.099)	(0.715)	(0.770)	(0.008)	(0.281)	(0.549)	(0.001)	(0.060)	(0.109)
	[0.668]	[0.436]	[0.000]	[0.492]	[0.698]	[0.698]	[0.154]	[0.426]	[0.774]	[0.000]	[0.280]	[0.264]
R^2	0.031	0.010	0.019	0.034	0.016	0.020	0.048	0.015	0.040	0.025	0.076	0.010
Obs.	6355	7976	9426	7385	3272	4824	13580	3223	9869	15179	17944	4265
Intensity	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z	r $\times$ z
Region FE	X	X	X	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X	X	X	X

Notes: p-values in parentheses are based on standard errors. Wild bootstrap p-values are shown in square brackets below (1000 repetitions). Regressions are restricted to women aged between 18 and 40. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Significance levels are based on the wild bootstrap p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.20: Effectiveness of bans by country (alternative intensity)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Var.							Married under 18					
Sample	Benin	RDC	Egypt	Ethiopia	Ghana	Guinea	Kenya	Liberia	Madagascar	Malawi	Nigeria	Togo
Panel A: Entire country												
Dep. Var.	Married under 18											
Exposed	0.022	0.025	-0.018***	-0.001	0.031	0.071	-0.020	-0.029	0.013	-0.042*	0.002	-0.051**
× Intensity	(0.080)	(0.126)	(0.163)	(0.903)	(0.383)	(0.001)	(0.006)	(0.326)	(0.229)	(0.046)	(0.540)	(0.004)
	[0.206]	[0.404]	[0.000]	[0.934]	[0.778]	[0.354]	[0.210]	[0.198]	[0.170]	[0.098]	[0.840]	[0.046]
R^2	0.060	0.045	0.053	0.108	0.029	0.035	0.077	0.025	0.049	0.027	0.243	0.045
Obs.	11345	12876	16348	11519	6627	7738	22942	5424	13264	17480	30012	7055
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Dep. Var.	Married under 15											
Exposed	-0.004	-0.010	-0.016***	-0.004	-0.003	0.006	-0.023***	0.019	0.007	-0.047***	-0.002	-0.006
× Intensity	(0.646)	(0.329)	(0.008)	(0.602)	(0.885)	(0.707)	(0.000)	(0.272)	(0.301)	(0.000)	(0.523)	(0.502)
	[0.660]	[0.340]	[0.000]	[0.764]	[0.786]	[0.840]	[0.008]	[0.328]	[0.384]	[0.000]	[0.626]	[0.614]
R^2	0.029	0.014	0.013	0.044	0.012	0.018	0.042	0.018	0.038	0.024	0.078	0.014
Obs.	11345	12876	16348	11519	6627	7738	22942	5424	13264	17480	30012	7055
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Region FE	X	X	X	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X	X	X	X

Notes: p-values in parentheses are based on standard errors. Wild bootstrap p-values are shown in square brackets below (1000 repetitions). Regressions are restricted to women aged between 18 and 40. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Significance levels are based on the wild bootstrap p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

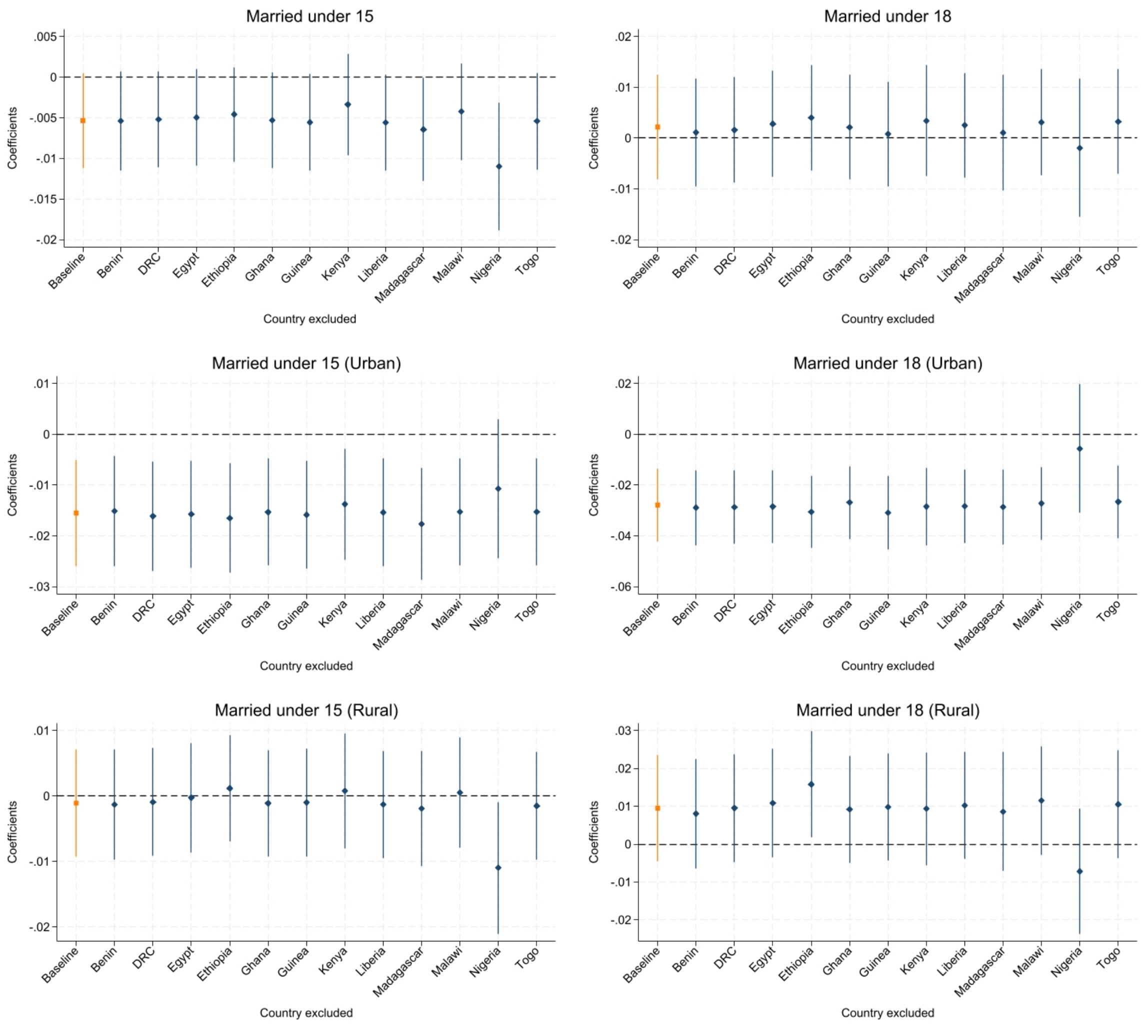
Table A.21: Effectiveness of bans by country and residence (alternative intensity)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. Var.							Married under 18					
Sample	Benin	RDC	Egypt	Ethiopia	Ghana	Guinea	Kenya	Liberia	Madagascar	Malawi	Nigeria	Togo
Panel B: Urban areas												
Dep. Var.	Married under 18											
Exposed	-0.006	0.003	0.006	0.007	-0.039	0.086*	-0.021*	-0.012	-0.034	-0.110*	-0.034***	-0.087**
× Intensity	(0.695)	(0.894)	(0.714)	(0.673)	(0.310)	(0.001)	(0.027)	(0.762)	(0.045)	(0.014)	(0.000)	(0.001)
	[0.756]	[0.922]	[0.562]	[0.704]	[0.512]	[0.062]	[0.070]	[0.768]	[0.130]	[0.086]	[0.000]	[0.038]
R^2	0.055	0.069	0.034	0.101	0.024	0.022	0.061	0.030	0.031	0.039	0.147	0.031
Obs.	4990	4900	6891	4134	3355	2914	9362	2201	3395	2299	12068	2790
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Dep. Var.	Married under 15											
Exposed	-0.019	-0.001	0.003	0.004	-0.020	0.016	-0.025***	-0.020	0.001	-0.042**	-0.017**	-0.022*
× Intensity	(0.065)	(0.907)	(0.699)	(0.746)	(0.304)	(0.318)	(0.000)	(0.324)	(0.898)	(0.062)	(0.000)	(0.069)
	[0.120]	[0.814]	[0.352]	[0.824]	[0.614]	[0.408]	[0.006]	[0.658]	[0.936]	[0.028]	[0.032]	[0.086]
R^2	0.028	0.016	0.005	0.037	0.007	0.016	0.040	0.028	0.015	0.014	0.051	0.010
Obs.	4990	4900	6891	4134	3355	2914	9362	2201	3395	2299	12068	2790
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Panel C: Rural areas												
Dep. Var.	Married under 18											
Exposed	0.052**	0.016	-0.032	-0.033	0.222*	-0.014	-0.007	-0.056	0.019	-0.048*	0.021	-0.062*
× Intensity	(0.030)	(0.554)	(0.104)	(0.039)	(0.003)	(0.800)	(0.511)	(0.234)	(0.185)	(0.046)	(0.000)	(0.069)
	[0.020]	[0.558]	[0.128]	[0.386]	[0.054]	[0.522]	[0.840]	[0.306]	[0.154]	[0.068]	[0.108]	[0.080]
R^2	0.047	0.024	0.047	0.052	0.027	0.022	0.093	0.013	0.045	0.024	0.252	0.019
Obs.	6355	7976	9457	7385	3272	4824	13580	3223	9869	15179	17944	4265
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Dep. Var.	Married under 15											
Exposed	0.007	-0.019	-0.029	-0.010	0.060	-0.056***	-0.018*	0.066	0.006	-0.046***	0.007	0.025
× Intensity	(0.672)	(0.259)	(0.002)	(0.437)	(0.156)	(0.177)	(0.005)	(0.020)	(0.562)	(0.002)	(0.147)	(0.177)
	[0.694]	[0.256]	[0.104]	[0.642]	[0.140]	[0.000]	[0.058]	[0.260]	[0.768]	[0.002]	[0.344]	[0.302]
R^2	0.031	0.010	0.018	0.034	0.017	0.020	0.048	0.016	0.040	0.025	0.076	0.010
Obs.	6355	7976	9457	7385	3272	4824	13580	3223	9869	15179	17944	4265
Intensity	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext	r ext
Region FE	X	X	X	X	X	X	X	X	X	X	X	X
YOB FE	X	X	X	X	X	X	X	X	X	X	X	X

Notes: p-values in parentheses are based on standard errors. Wild bootstrap p-values are shown in square brackets below (1000 repetitions). Regressions are restricted to women aged between 18 and 40. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Significance levels are based on the wild bootstrap p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Figure A.8: Effectiveness of child marriage bans : leave-one-country-out analysis



Notes: 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Author's calculations

Table A.22: Impact of droughts on the efficiency of child marriage bans - Excluding Nigeria

	(1)	(2)	(3)
Dependent Variable		Married before 18	
Sample	Country	Bride Price	Not Bride Price
Panel A: Droughts <-1.5 SD, including Nigeria			
Exposed × Intensity (std)	0.001	-0.019***	0.003
	(0.006)	(0.007)	(0.009)
Exposed × Intensity × Drought	0.005	0.034**	-0.044
	(0.015)	(0.016)	(0.027)
Mean Y	0.32	0.28	0.41
Observations	162,630	84,272	30,529
Panel B: Droughts <-1.5 SD, excluding Nigeria			
Exposed × Intensity (std)	-0.003	-0.020*	0.000
	(0.007)	(0.012)	(0.009)
Exposed × Intensity × Drought	0.005	0.038**	-0.044
	(0.018)	(0.018)	(0.028)
Mean Y	0.32	0.28	0.41
Observations	132,618	68,332	27,162
Region FE	X	X	X
Year of birth FE	X	X	X
Definition of intensity	r	r	r

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. "Not Bride Price" areas exclude women located within tribal boundaries not included in Murdock (1967) Ethnographic Atlas. The "Mean Y" is computed on unexposed cohorts for which no drought occurred during adolescence.

Sources: DHS, MICS, CHIRPS, author's calculations.

A3.3 Weather Shocks as a Proxy for Exogenous Income Shocks

The relationship between weather shocks and agricultural productivity has been extensively investigated in the literature (Burke et al., 2015; Jayachandran, 2006; Kaur, 2019; Shah & Steinberg, 2017). In this subsection, I explore how rainfall shocks affects aggregate crop yields in Africa by combining meteorological data with yield statistics. Yield data is from Iizumi & Sakai (2020), who updated the global dataset of historical yields (GDHY).[54]

I estimate the impact of droughts on the natural logarithm of yields of the four staple crops. The yield data is averaged at the DHS regional level.[55] Droughts are defined as calendar year or season rainfall falling below between -1 to -2 standard deviations from the region's long-run rainfall distribution. Symmetric positive rainfall shocks are also included. All estimates control for year t, region r, and crop c fixed effects, as detailed in Equation 6 below:

$$log(yields)_{c,r,t} = \beta_0 + \beta_1 Low_{r,t} + \beta_2 High_{r,t} + FE_r + FE_c + FE_t + \varepsilon_{c,r,t} \qquad (6)$$

with $log(yields)_{c,r,t}$ the log of the mean production of crop c per unit harvested area in region r in year, rainy season or dry season t. As shown in Table A.23, a rainfall deficit significantly decreases agricultural yields, while an excess of rainfall leads to an increase. This effect is consistent across the African continent (columns 1 to 3) and in the subsample of countries considered in the main analysis (columns 4 to 6), regardless of the definition of rainfall shock or time aggregation (covering rainy seasons, dry seasons, and years). A deficit of rainfall during the rainy season has the most substantial impact on agricultural yields. In the subsample of 11 countries, a low rainfall realization during a rainy season reduces yields by 15% to 18%, while a high rainfall realization increases yields by 2% to 7% (columns 4 to 6, panel A). Appendix Figure A.4 plots the coefficients of rainfall realization dummies on the natural logarithm of crop yields.

[54] This dataset integrates agricultural census statistics and satellite remote sensing data to cover the 36-year period from 1981 to 2016 with a spatial resolution of 0.5 degree. It comprises historical yields for four major crops: maize, rice, wheat, and soybean. Comparisons with different global yield datasets and published results demonstrate that this dataset is a reliable information source for yields (Iizumi & Sakai, 2020).

[55] Since I compute droughts during adolescence in a girl's region of residence to approximate income shocks during the years she is at risk of child marriage, it is essential to verify that droughts at this specific regional level is a reliable proxy for income shocks at the same geographical scale.

Overall, my measure of drought strongly reduces agricultural output, making it a good proxy for negative income shocks.

Table A.23: Impact of weather shocks on agricultural yields

Variable	(1)	(2)	(3)	(4) log Yields	(5)	(6)
Panel A: Shocks during rainy season						
Low rainfall	-0.123***	-0.168***	-0.216***	-0.120***	-0.152***	-0.183***
	(0.007)	(0.012)	(0.019)	(0.009)	(0.015)	(0.027)
High rainfall	0.030***	0.043***	0.078***	0.023***	0.042***	0.074***
	(0.006)	(0.010)	(0.017)	(0.008)	(0.012)	(0.023)
Mean Y if no shock	2.20	2.18	2.18	2.45	2.43	2.41
R^2	0.206	0.204	0.203	0.390	0.388	0.386
Panel B: Shocks during dry season						
Low rainfall	-0.025***	-0.035***	-0.031	-0.044***	-0.130***	-0.271***
	(0.007)	(0.013)	(0.029)	(0.010)	(0.020)	(0.057)
High rainfall	0.011**	0.007	0.012	0.015**	0.028***	0.045***
	(0.005)	(0.007)	(0.010)	(0.007)	(0.010)	(0.014)
Mean Y if no shock	2.15	2.18	2.17	2.38	2.42	2.41
R^2	0.201	0.201	0.201	0.385	0.386	0.385
Panel C: Shocks during year						
Low rainfall	-0.086***	-0.121***	-0.213***	-0.083***	-0.129***	-0.220***
	(0.006)	(0.009)	(0.018)	(0.010)	(0.013)	(0.025)
High rainfall	0.027***	0.046***	0.051***	0.016**	0.020**	0.006
	(0.006)	(0.009)	(0.014)	(0.007)	(0.010)	(0.016)
Mean Y if no shock	2.20	2.18	2.18	2.48	2.44	2.42
R^2	0.204	0.203	0.203	0.387	0.387	0.387
Region FE	X	X	X	X	X	X
Crop FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Observations	37251	37251	37251	15357	15357	15357
Country sample	Africa	Africa	Africa	Subsample	Subsample	Subsample
Definition of shock	1sd	1.5sd	2sd	1sd	1.5sd	2sd

Notes: Robust standard errors in parentheses clustered at the region level. Regressions are run of maize, wheat, soybean, and rice. The African sample of columns 1 to 3 includes Angola, Benin, Burkina Faso, Burundi, CAR, Cameroon, Chad, Comoros, Congo, DRC, Cote d'Ivoire, Egypt, Eritrea, Ethiopia, Gabon, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mauritania, Mozambique, Namibia, Niger, Nigeria, Rwanda, Sao Tome and Principe, Senegal, Sierra Leone, South Africa, Sudan, Tanzania, The Gambia, Togo, Tunisia, Uganda, Zambia, and Zimbabwe for which we have geographical boundaries of DHS regions. The subsample includes Benin, DRC, Egypt, Ethiopia, Ghana, Guinea, Kenya, Liberia, Madagascar, Nigeria, and Togo. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Sources: CHIRPS, DHS regional boundaries, Toshichika Izumi & Toru Sakai (2020), author's calculations.

A3.4 Impact of Economic Shocks on Ban Effectiveness

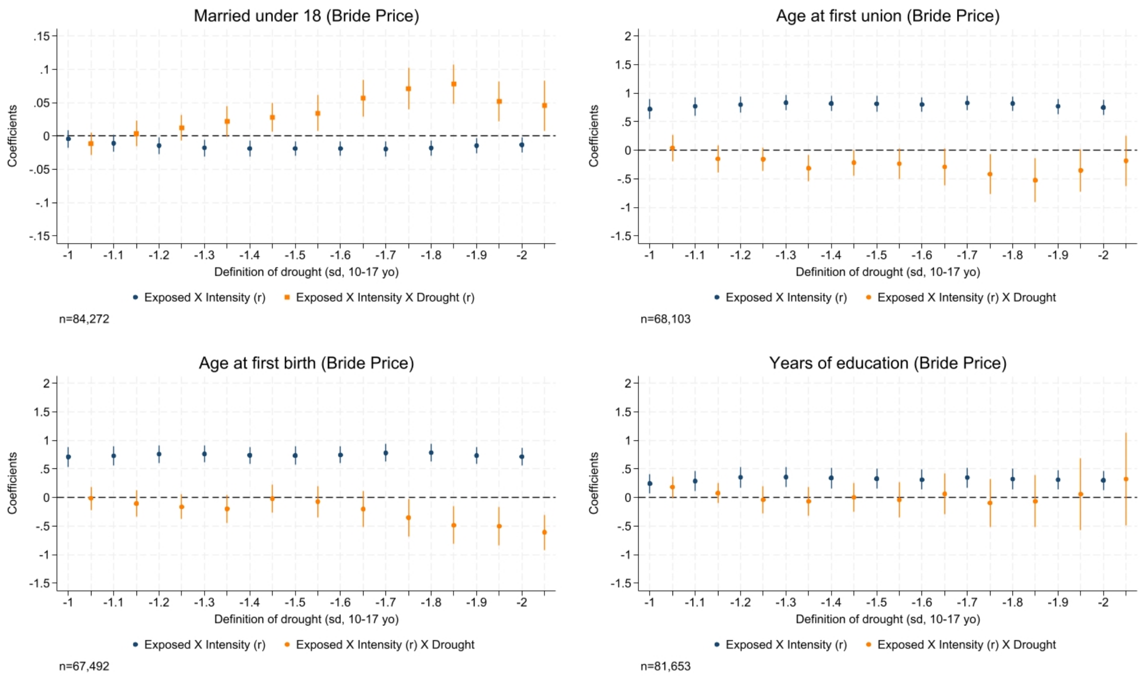
Table A.24: Impact of droughts on the efficiency of child marriage bans (alternative intensity)

	(1)	(2)	(3)
Dependent Variable	Married before 18		
Sample	Country	Bride Price	Not Bride Price
Panel A: Droughts <-2sd			
Exposed × Intensity (std)	-0.000	-0.011*	-0.003
	(0.005)	(0.006)	(0.010)
Exposed × Intensity × Drought	0.030	0.024	0.036
	(0.018)	(0.016)	(0.032)
Mean Y	0.33	0.28	0.42
Observations	162630	84272	30529
R^2	0.126	0.113	0.074
Panel B: Droughts <-1.5sd			
Exposed × Intensity (std)	-0.001	-0.015**	-0.000
	(0.005)	(0.007)	(0.010)
Exposed × Intensity × Drought	0.009	0.024**	-0.042
	(0.012)	(0.012)	(0.027)
Mean Y	0.33	0.28	0.42
Observations	162,630	84,272	30,529
R^2	0.125	0.114	0.074
Region FE	X	X	X
Year of birth FE	X	X	X
Definition of intensity	r ext	r ext	r ext

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. "Not Bride Price" areas exclude women located within tribal boundaries not included in Murdock (1967) Ethnographic Atlas.

Sources: DHS, MICS, CHIRPS, author's calculations.

Figure A.9: Sensitivity of the impact of droughts on ban effectiveness - Bride Price areas



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18, 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

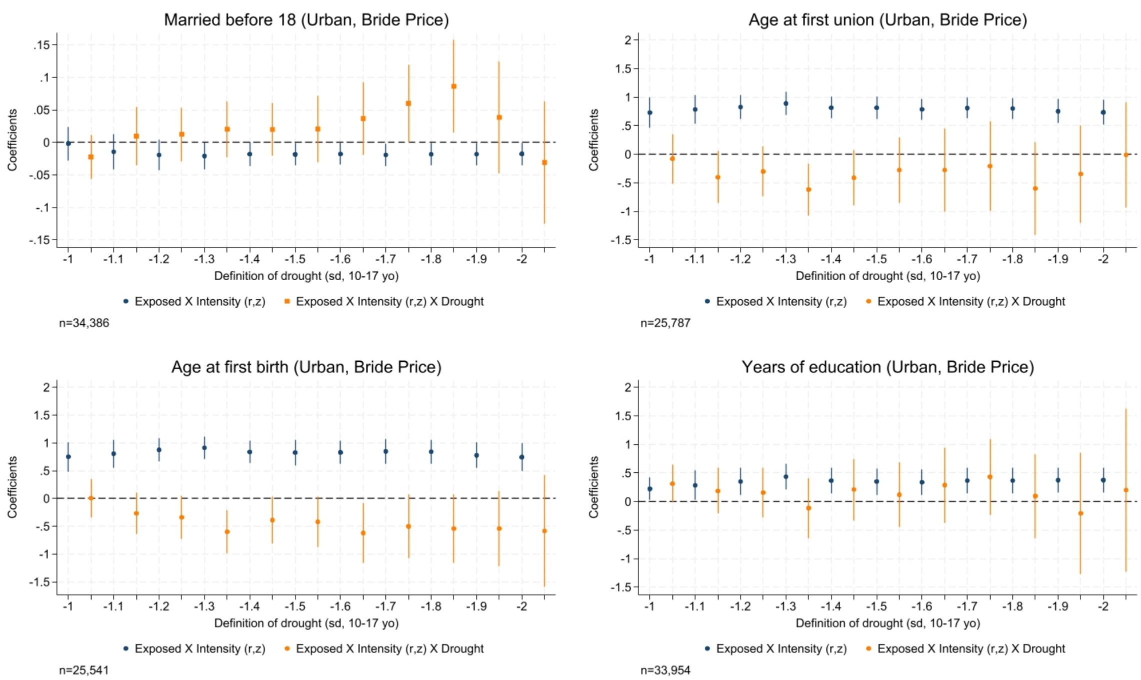
Table A.25: Impact of droughts on educational attainment, heterogeneity by marriage payment and drought occurrence

	(1)	(2)
Dependent Variable	Years of education	
Sample	No shock	Shock (<-1.5SD)
Exposed × Intensity (std)	-0.143	1.029
	(0.121)	(0.724)
Exposed × Intensity × Bride Price	0.444***	-0.464
	(0.165)	(0.617)
Observations	80,937	19,950
R^2	0.32	0.41
Mean Y (untreated, not BP)	4.04	3.28
Mean Y (untreated, BP)	6.96	5.28

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable *Intensity* is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. "Not Bride Price" areas exclude women located within tribal boundaries not included in Murdock (1967) Ethnographic Atlas.

Sources: DHS, MICS, CHIRPS, author's calculations.

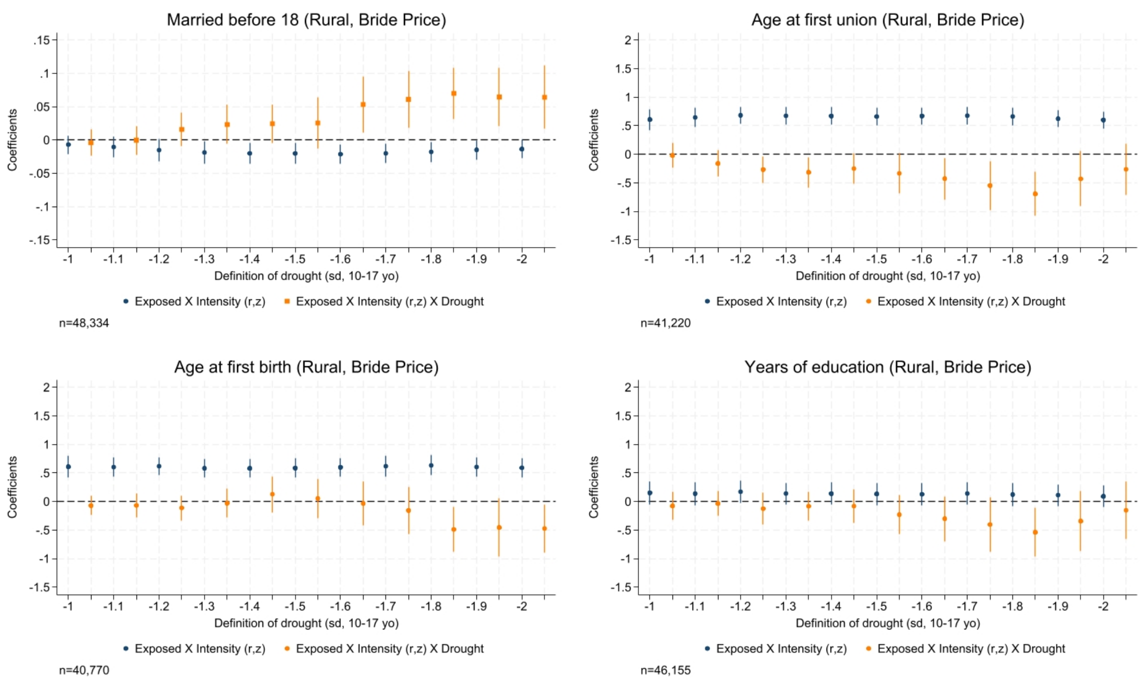
Figure A.10: Sensitivity of the impact of droughts on ban effectiveness - Bride Price Urban areas



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18, 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

Figure A.11: Sensitivity of the impact of droughts on ban effectiveness - Bride Price Rural areas



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18, 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

A3.5 Weather Shocks and the Timing of Marriage

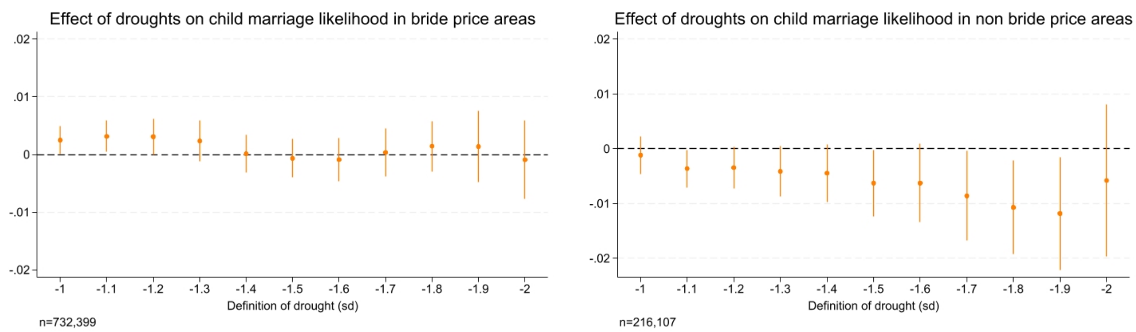
The duration of interest is the time between t_0, the age when a woman is first at risk of getting married, and t_m, the age when she enters her first marriage. In this analysis, t_0 is age 10, which is the minimum age at which a non-negligible number of women in my sample report getting married for the first time. I convert my data into person-year panel format. Hence, a woman who is married at age t_m contributes $(t_m\text{-}t_0 + 1)$ observations to the sample: one observation for each at-risk year until she is married, after which she exits the data. I merge these individual data with my rainfall data at the rainy season level. Using this sample, I estimate the probability of marriage of woman i living in region r in country c born year t and entering her first marriage at age a as follows:

$$Marriage_{i,t,r,c,a} = \beta_0 + \beta_1 Shock_{r,t,a} + FE_r + FE_t + FE_a + FE_{c\times t} + \varepsilon_{i,t,r,c,a} \quad (7)$$

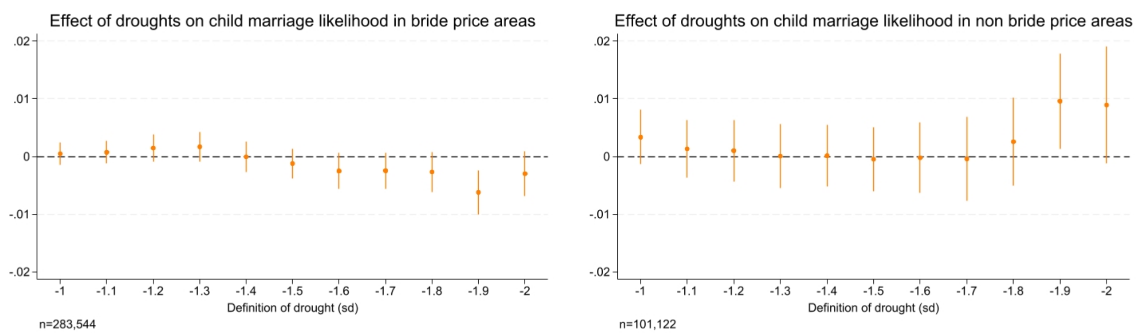
The dependent variable, $Marriage_{i,t,r,c,a}$, is a binary variable coded as 1 in the year the woman gets married and zero otherwise. I examine data on women until age 24. Thus, women married after age 24 are right-censored. The variable $Shock_{r,t,a}$ is a time-varying measure of weather conditions in region r during the year in which the woman born in year t is age a. Specifically, included in $Shock_{r,t,a}$ is a dummy indicator for a drought occurring in a given year. β_1 is the main coefficient of interest and measures the effect of rainfall shocks on the probability of marriage. FE_a is a vector of age fixed effects, which controls for the fact that marriage has a different probability of occurring at different ages. I include location-specific fixed effects, FE_r, to control for time-invariant local unobservable characteristics, and year-of-birth fixed effects FE_t to account for cohort effects. I estimate regressions with standard errors clustered at the region level to allow for serial correlation in the error terms across women in the same region. The impact of weather shocks on marriage hazard is identified from variations in weather shocks and marriage outcomes within regions and within years of birth. To control for cohort-specific changes in marriage behavior at the country level, such as a change in the legal age at marriage, I also include control for country fixed effects interacted with year of birth fixed effects $FE_{c\times t}$.

Figure A.12: Weather shocks and the timing of marriage

(a) Sample of women



(b) Sample of men

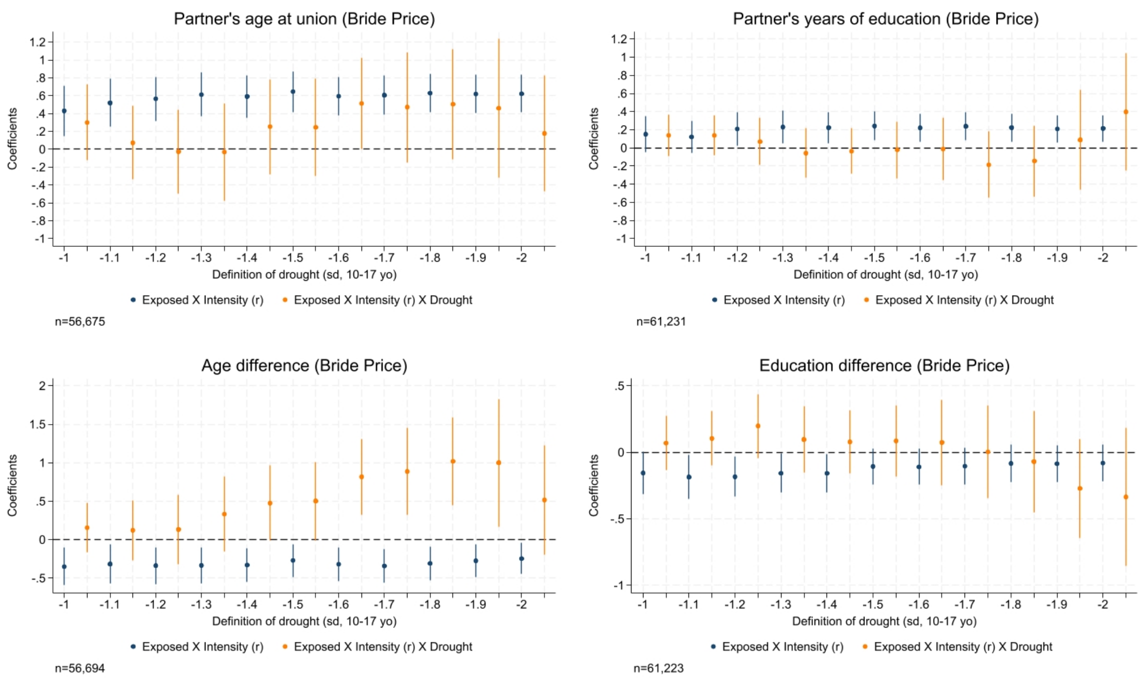


Notes: Figures show OLS regressions for women aged 25 or older at the time of interview. Observations are at the level of person × age (from 10 to 24 or age of first marriage, whichever is earlier). Observations are at the level of person × age (from 10 to 24 or age of first marriage, whichever is earlier). The dependent variable is a binary variable for marriage, coded to 1 if the woman married at the age corresponding to the observation. Standard errors (in parentheses) are clustered at the region level. A drought is defined as an annual rainfall realization below -1sd to -2sd of the local rainfall distribution.

Sources: DHS, MICS, CHIRPS, Murdock (1959; 1967), Author's calculations

A3.6 Additional Mechanisms and Heterogeneity

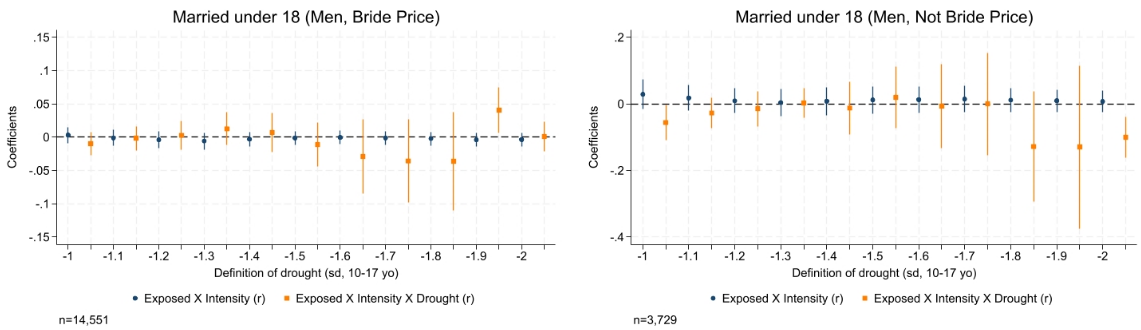
Figure A.13: Partner's characteristics, bans, and droughts in bride price areas



Notes: The dependent variables are the partner's age at marriage, the partner's years of education, the age difference between the spouses, and the education difference in years between the spouses. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a dry season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

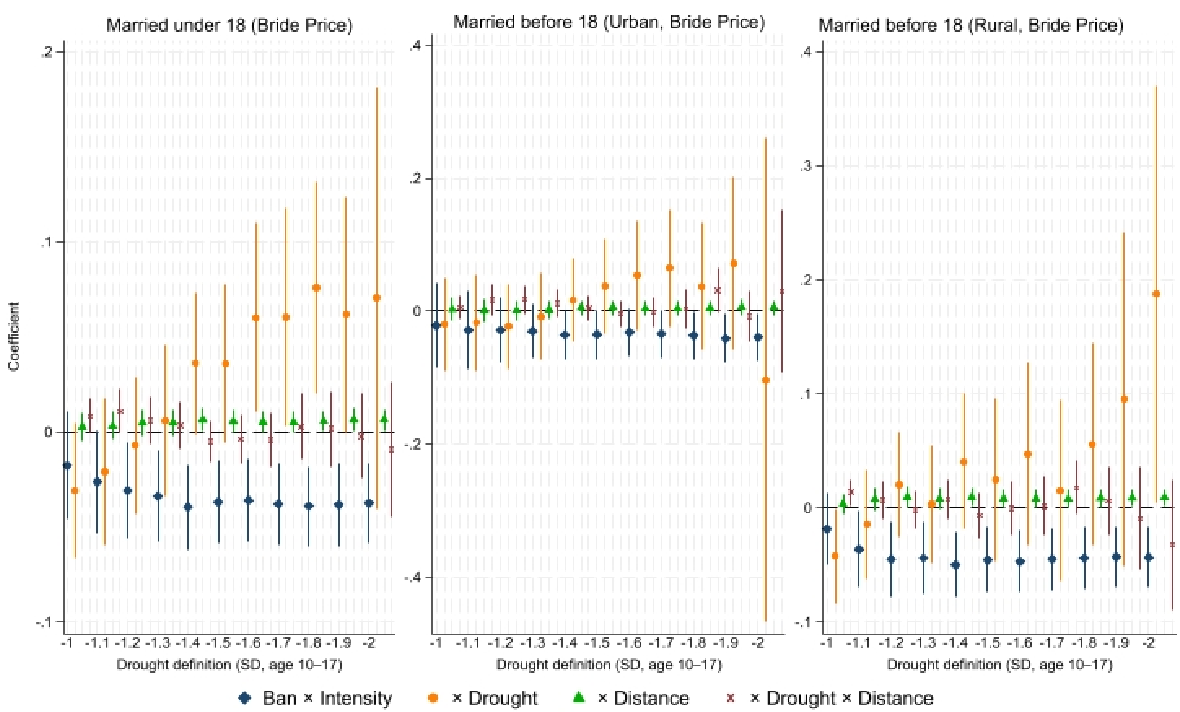
Figure A.14: Bans on boy's child marriage and droughts



Notes: The dependent variables are a dummy variable equal to 1 if the man was married before 18 and 0 otherwise and the age at first marriage for ever-married men. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional boy child marriage intensity) and the dummy variable *Exposed* equal to 1 if the man was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the boy (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to men aged between 18 and 40 and only include four countries that banned child marriage for boys after 1995 (Egypt, Ghana, Kenya, Liberia, Malawi, and Nigeria).

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

Figure A.15: Effects of child marriage bans by exposure, drought, and distance to capital city



Notes: Coefficients are from the following equation:

$$Outcome_{i,t,r,c} = \beta_0 + \beta_1\ Exposed_{t,c} \times Intensity_r + \beta_2\ Shock_{t,r} + \beta_3\ Distance_i + \beta_4\ Exposed_{t,c} \times Shock_{t,r} + \beta_5 Exposed_{t,c} \times Distance_i + \beta_6\ Intensity_r \times Shock_{t,r} + \beta_7\ Intensity_r \times Distance_i + \beta_8\ Distance_i \times Shock_{t,r} + \beta_9\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} + \beta_{10}\ Exposed_{t,c} \times Intensity_r \times Distance_i + \beta_{11}\ Intensity_r \times Shock_{t,r} \times Distance_i + \beta_{12}\ Exposed_{t,c} \times Distance_i \times Shock_{t,r} + \beta_{13}\ Exposed_{t,c} \times Intensity_r \times Shock_{t,r} \times Distance_i + \mathrm{FE}_r + \mathrm{FE}_{t,c} + \varepsilon_{i,t,r,c}$$

The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, Author's calculations

Table A.26: Heterogeneous effects of child marriage bans by weather shock, marriage practice and residence pattern

	BP not matrilocal	Not BP not matrilocal	Not BP matrilocal
Panel A: Droughts < -2SD			
Exposed × Intensity	-0.013**	0.004	0.024
	[0.044]	[0.542]	[0.446]
	(0.011)	(0.668)	(0.503)
Exposed × Intensity × Drought	0.042**	0.047	0
	[0.030]	[0.332]	[.]
	(0.118)	(0.413)	(.)
R-squared	0.110	0.070	0.032
Observations	83,323	23,680	6,850
Definition of intensity	r	r	r
Panel B: Droughts < -1.5SD			
Exposed × Intensity	-0.019***	0.007	0.023
	[0.008]	[0.428]	[0.558]
	(0.001)	(0.510)	(0.532)
Exposed × Intensity × Drought	0.036**	-0.040	0
	[0.042]	[0.242]	[.]
	(0.015)	(0.118)	(.)
R-squared	0.110	0.070	0.032
Observations	83,323	23,679	6,845
Definition of intensity	r	r	r

Notes: p-values in square brackets are from wild bootstrap standard errors clustered at the region level (1,000 replications). Parentheses show conventional p-values. *** p<0.01, ** p<0.05, * p<0.1. All regressions include region and year of birth fixed effects. The dependent variable is an indicator for child marriage. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1.

Sources: DHS, MICS, CHIRPS, author's calculations.

Table A.27: Impact of distance to capital city on the efficiency of child marriage bans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				Child married			
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	-0.016	-0.027*	-0.015	-0.035*	-0.015	-0.029**	-0.019
	(0.010)	(0.016)	(0.014)	(0.021)	(0.013)	(0.013)	(0.012)
Exposed × Intensity	0.005*	0.000	0.007*	0.002	0.007*	0.002	0.007**
× Distance to capital	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.003)
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r × z	r × z	r ext	r ext
Mean Y	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	160,584	58,555	102,026	56,343	101,995	58,555	102,026
R^2	0.127	0.098	0.123	0.098	0.123	0.098	0.123

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable *Intensity* is standardized with mean 0 and standard deviation 1.*** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

A4 Robustness Checks and Additional Tests

A4.1 Validity of Identification Strategy

Table A.28: Effectiveness of bans: placebo ban

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				Married before 18			
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Panel A: Placebo ban 10 years before							
Exposed × Intensity (std)	0.005	-0.001	0.023	-0.023	0.025	-0.001	0.024*
	(0.014)	(0.019)	(0.015)	(0.031)	(0.015)	(0.014)	(0.012)
Mean Y if not exposed	0.33	0.21	0.40	0.21	0.40	0.21	0.40
Observations	78222	27799	50420	26654	50396	27799	50420
R^2	0.103	0.102	0.090	0.102	0.090	0.102	0.090
Panel B: Placebo ban 5 years before							
Exposed × Intensity (std)	-0.001	-0.008	0.007	-0.014	0.007	-0.008	0.010
	(0.005)	(0.007)	(0.007)	(0.009)	(0.007)	(0.006)	(0.007)
Mean Y if not exposed	0.32	0.20	0.40	0.20	0.40	0.20	0.40
Observations	78222	27799	50420	26654	50396	27799	50420
R^2	0.103	0.102	0.090	0.102	0.090	0.102	0.090
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r × z	r × z	r ext	r ext

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to unexposed women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Table A.29: Negative weights in the effectiveness of bans with a continuous treatment intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable				Married before 18			
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r,z	r,z	r ext	r ext
Observations	162,630	59,298	103,329	57,048	103,298	59,298	103,329
Robustness to HTE							
$\sum$ negative weights	-0.0149	-0.0219	-0.0243	-0.1289	-0.0243	-0.0265	-0.0206
$\sum$ all weights	1	1	1	1	1	1	1

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1.

Sources: DHS, MICS, CHIRPS author's calculations.

A4.2 Migration Concerns

The estimation of the effects of child marriage bans may be compromised by migration only in cases of long-distance movement—an occurrence I previously argued to be rare—and if migration is not orthogonal to ban exposure. While a negative correlation between child marriage and migration is observed in both urban and rural areas (Appendix Table A.31), there is a positive correlation between ban intensity and migration after the 10^{th} birthday in rural areas, which is not observed in urban areas (Appendix Table A.32). Therefore, migration is unlikely to introduce bias in the estimates of urban areas. In rural areas, however, estimates could be biased if the higher likelihood of migration among highly exposed women involves systematic long-distance movement to regions with different ban intensities. Nevertheless, this scenario remains improbable, as women primarily migrate for marriage within their region of origin, as discussed earlier (Beegle & Poulin, 2013; Mbaye & Wagner, 2017). In the rare cases of long-distance migration, the negative correlation between child marriage and migration, coupled with the positive correlation between ban exposure and migration, could bias the estimated effect of the bans towards negative values. In such a case, given that the estimated $\hat{\beta}_1$ is null, the unbiased value of β_1 would likely be positive, resulting in child marriage bans increasing child marriage likelihood. However, this potential bias is unlikely to explain the null effect of child marriage bans in rural areas, as my findings are consistent with the existing literature, which uses alternative empirical strategies and similarly finds no significant effect of bans in rural settings (Batyra & Pesando, 2021; Collin & Talbot, 2023).

Table A.30: Marriage migration in Africa

Variable	Percent
Never migrated	56.5
Migrated before marriage	10.4
Migrated at marriage	15.2
Migrated after marriage	17.2
Migrated after 10 yo	40.3

Notes: This table shows how long ever-married women have lived in their current place of residence (village, town, or city where she is interviewed). *Migrated at marriage* includes women who report migrating to their current place of residence within one year of getting married.

Sources: DHS, MICS, author's calculation.

Table A.31: Migration and child marriage in Africa

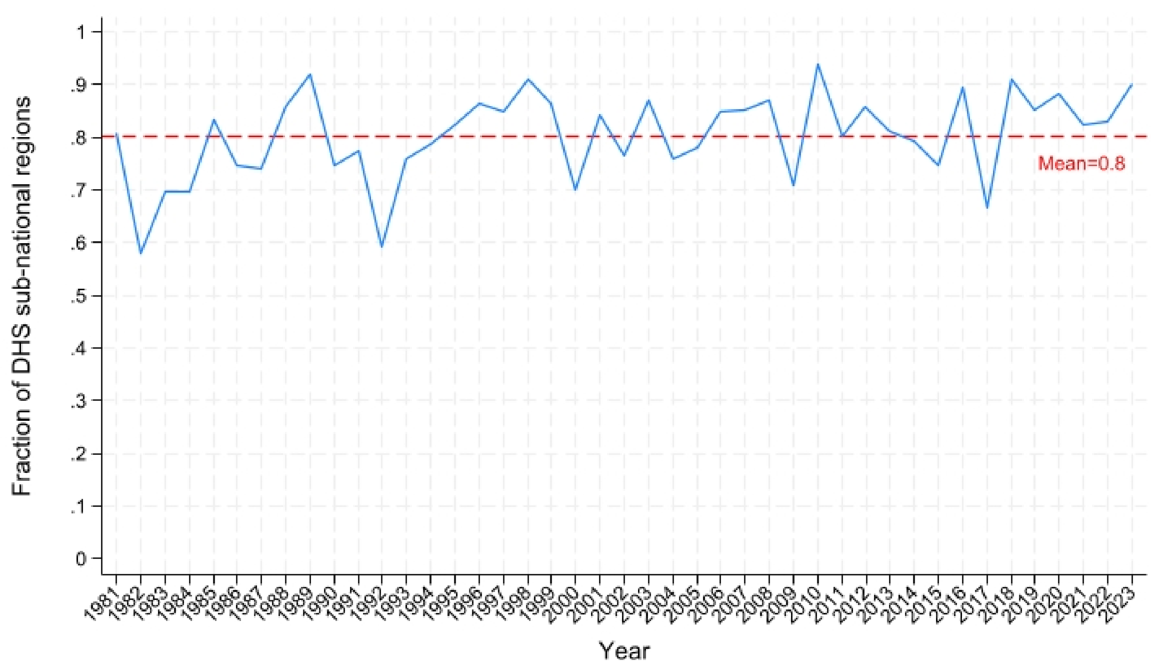
		Group variable: Ever moved				
		No		Yes		
Variable	Sample	Obs	Mean	Obs	Mean	Mean Diff
Child married	Total	49890	0.360	38470	0.306	0.053***
Child married	Rural	33087	0.423	19825	0.401	0.023***
Child married	Urban	16803	0.234	18645	0.206	0.028***

		Group variable: Moved after 10 yo				
		No		Yes		
Variable	Sample	Obs	Mean	Obs	Mean	Mean Diff
Child married	Total	52714	0.355	35646	0.309	0.046***
Child married	Rural	34466	0.422	18446	0.402	0.019***
Child married	Urban	18248	0.229	17200	0.209	0.019***

Notes: This table shows how long women have lived in their current place of residence (village, town, or city where she is interviewed), according to whether they first married before or after 18 years old.

Sources: DHS, MICS, author's calculation.

Figure A.16: Fraction of DHS sub-national regions in Africa with all neighboring regions experiencing identical shocks



Sources: DHS, CHIRPS, Author's calculations

Notes: This graph shows, by year, the share of DHS sub-national regions for which all neighboring regions experience an identical shock - taking the usual threshold of -1 sd to define a drought. The sample includes all African countries for which DHS sub-national regions are available.

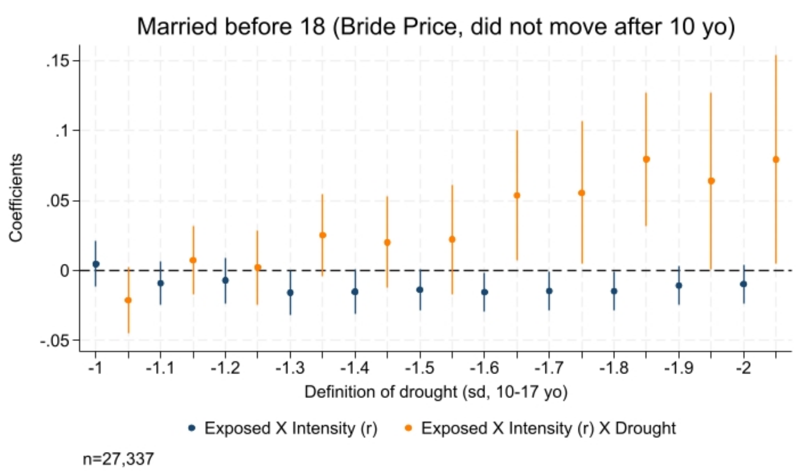
Table A.32: Correlation between ban intensity and migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Did not move after 10 yo						
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	-0.018***	-0.007	-0.017***	-0.010	-0.016***	-0.007	-0.019***
	(0.005)	(0.007)	(0.005)	(0.009)	(0.005)	(0.007)	(0.006)
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r × z	r × z	r ext	r ext
Mean Y if not exposed	0.57	0.45	0.62	0.45	0.62	0.45	0.62
Observations	119096	41120	77973	39129	77973	41120	77973
R^2	0.129	0.148	0.128	0.145	0.128	0.148	0.128

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. The sample excludes the DRC, Egypt, Ghana, and Togo, as this variable is not available in the dataset for these countries.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Figure A.17: Child marriage, bans, and droughts, for women who did not move after 10 yo



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40. The sample excludes the DRC, Egypt, Ghana, and Togo, as this variable is not available in the dataset for these countries.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

Table A.33: Effectiveness of bans: excluding women who moved after 10 yo

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Married before 18						
Sample	Country	Urban	Rural	Urban	Rural	Urban	Rural
Exposed × Intensity (std)	0.005	-0.032***	0.014	-0.035***	0.014	-0.031***	0.012
	(0.006)	(0.009)	(0.009)	(0.011)	(0.009)	(0.008)	(0.009)
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r × z	r × z	r ext	r ext
Mean Y if not exposed	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	72072	21112	50959	20026	50959	21112	50959
R^2	0.144	0.130	0.145	0.132	0.145	0.130	0.145

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable $Intensity$ is standardized with mean 0 and standard deviation 1. *** p<0.01, ** p<0.05, * p<0.1. The sample excludes the DRC, Egypt, Ghana, and Togo, as this variable is not available in the dataset for these countries.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

A4.3 Survey Response Manipulation

Table A.34: Effectiveness of bans: child marriage - Excluding Egypt, Ethiopia, Guinea, and Madagascar

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Country	Urban	Rural	Urban	Rural	Urban	Rural
Variable	Married before 18						
Exposed × Intensity (std)	0.002	-0.031***	0.019*	-0.036***	0.018*	-0.029***	0.012
	(0.007)	(0.007)	(0.010)	(0.009)	(0.010)	(0.007)	(0.009)
Region FE	X	X	X	X	X	X	X
Year of birth FE	X	X	X	X	X	X	X
Definition of intensity	r	r	r	r × z	r × z	r ext	r ext
Mean Y if not exposed	0.33	0.22	0.39	0.22	0.39	0.22	0.39
Observations	113761	41964	71794	39923	71794	41964	71794
R^2	0.132	0.101	0.131	0.102	0.131	0.101	0.131

Notes: Robust standard errors in parentheses clustered at the regional level. Regressions are restricted to women aged between 18 and 40. Variable *Intensity* is standardized with mean 0 and standard deviation 1. The dependent variable $Exposed \times Intensity$ is the interaction between a dummy variable equal to 1 if the woman was younger than 18 at the time of the ban and 0 otherwise, with a measure of pre-ban child marriage intensity of the location. Columns 1, 2 and 3 report results using the pre-ban child marriage intensity measured at the region level, taking into account both the intensive and extensive margins of child marriage. Columns 4 and 5 use a similar measure of intensity but computed at the $region \times zone$ level (rural and urban areas have different intensity levels within the same region). Columns 6 and 7 report results using the intensity measure computed at the regional level and only including the extensive margin of child marriage. *** p<0.01, ** p<0.05, * p<0.1. This sample excludes Egypt, Ethiopia, Guinea, and Madagascar.

Sources: DHS, MICS, WORLD Policy Analysis Center, author's calculations.

Figure A.18: Rate of child marriage by year of birth and survey

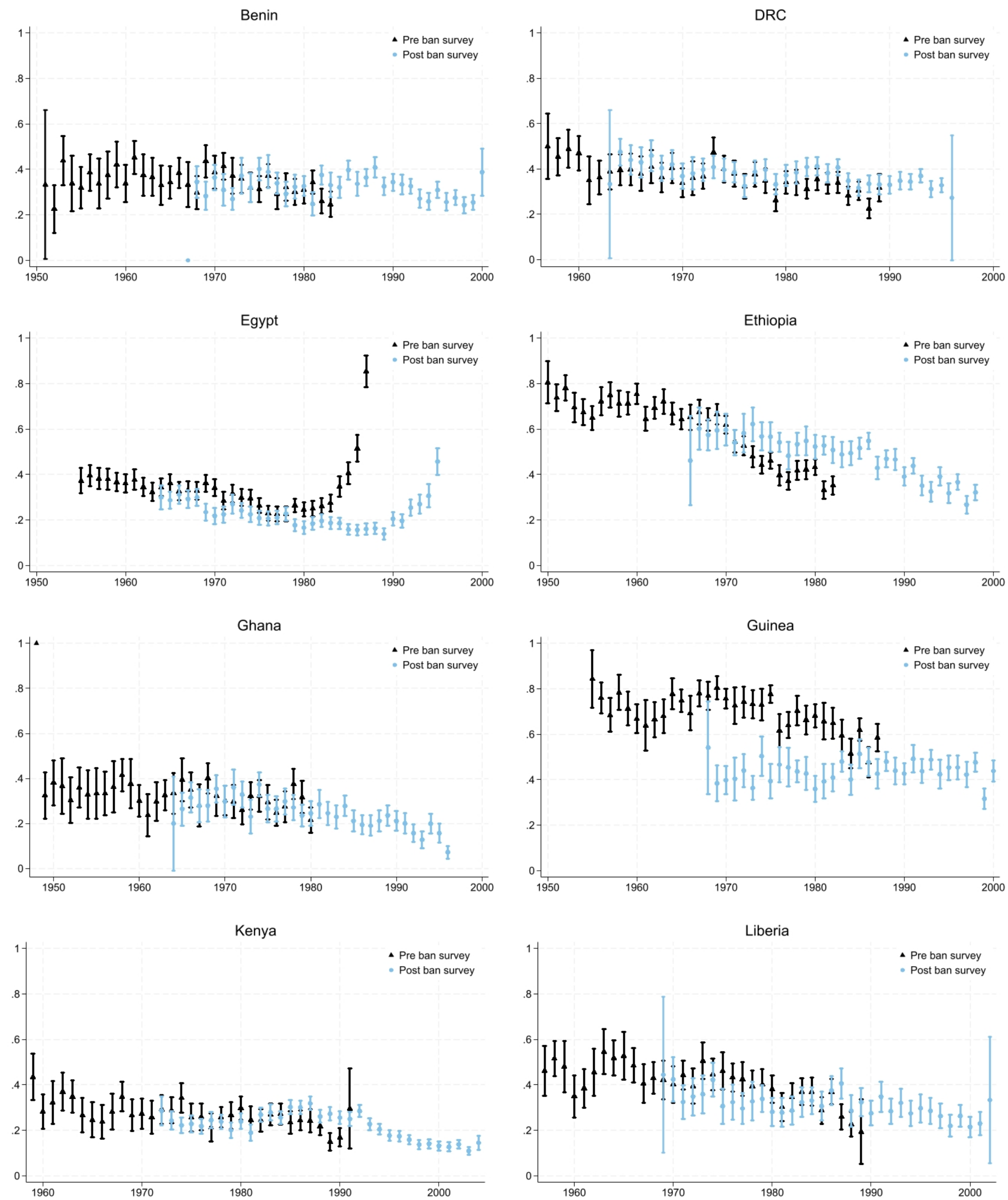
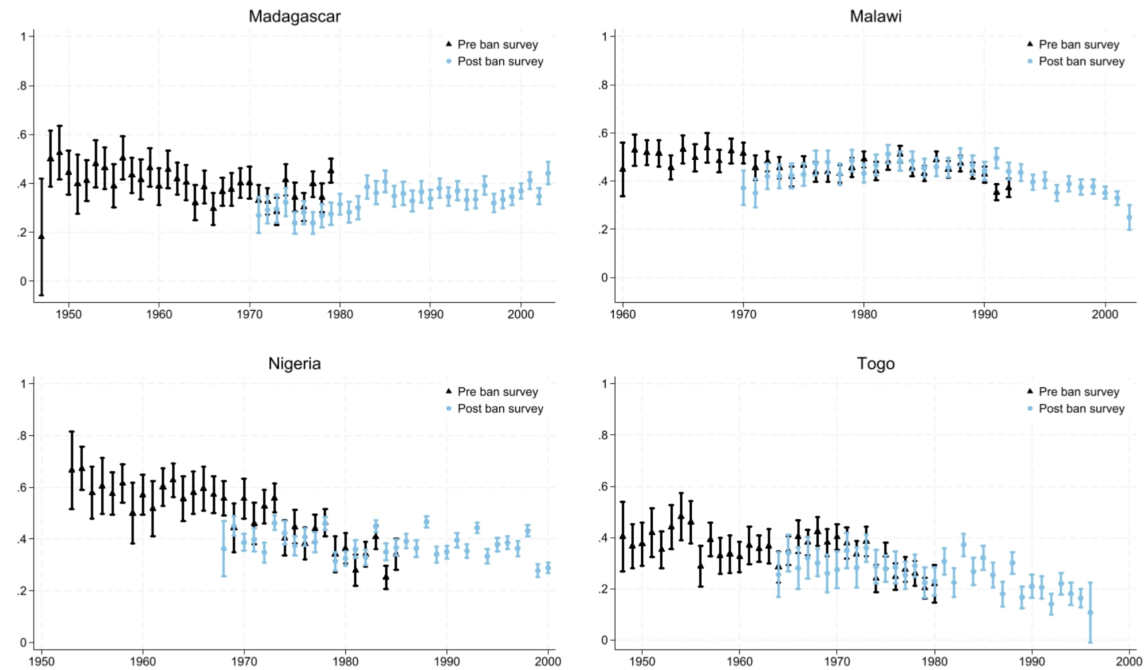


Figure A.18: Rate of child marriage by year of birth and survey (cont.)

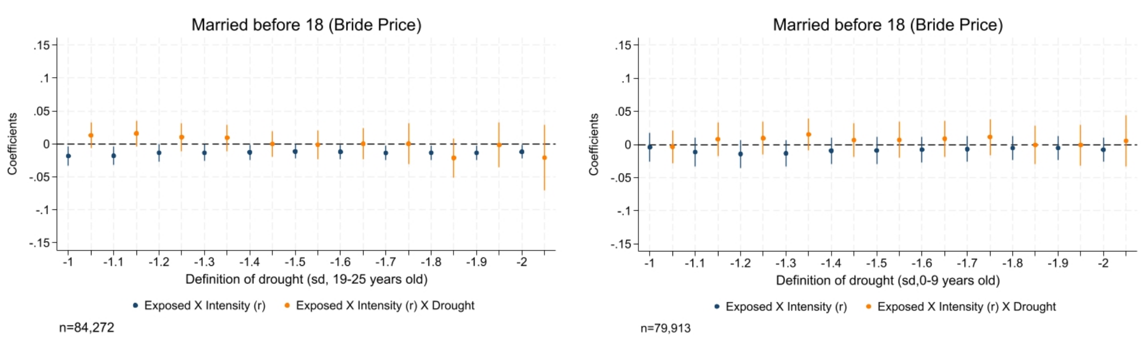


Notes: 95% confidence intervals shown. Each survey for each country is represented as a different series on the graphs.

Sources: DHS, MICS, WORLD Policy Analysis Center, Author's calculations

A4.4 Time Window for Drought Exposure

Figure A.19: Child marriage likelihood, bans, and droughts at other timings

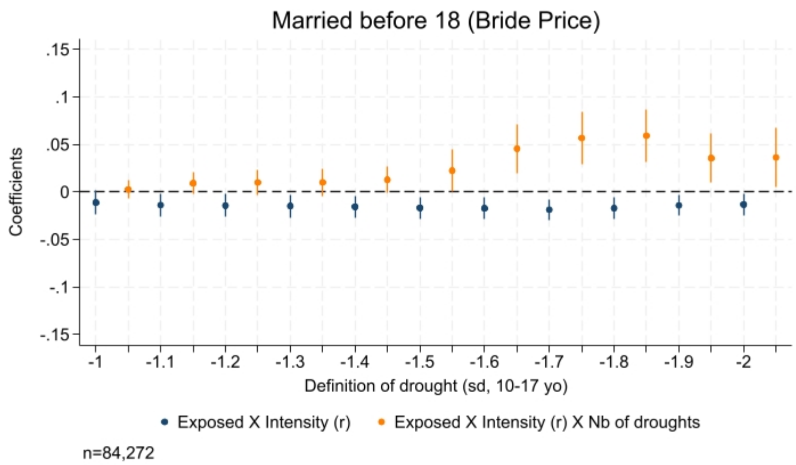


Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18, 0 otherwise. Figures plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry (a) when the woman was a kid (0 to 9 yo) and (b) when the woman was already adult (19 to 25 yo). Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

A4.5 Alternative Specifications and Definitions

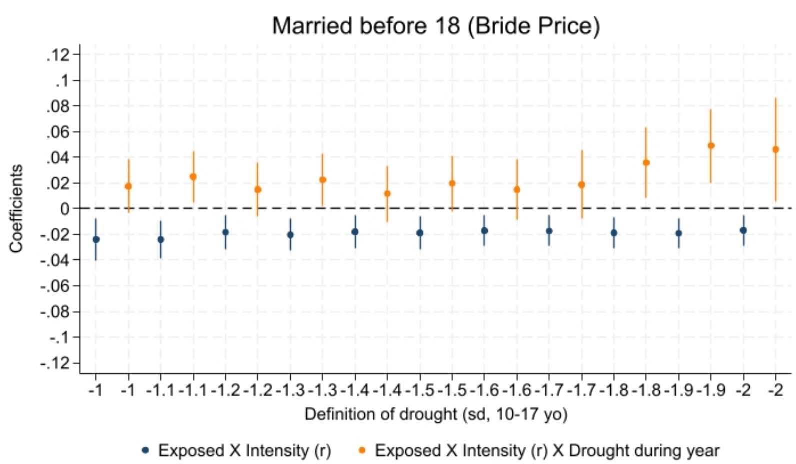
Figure A.20: Child marriage likelihood, bans, and number of droughts



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. The Figure plots in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a continuous variable counting the number of extremely dry rainy seasons during the adolescence of the girl (10 to 17 yo). Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

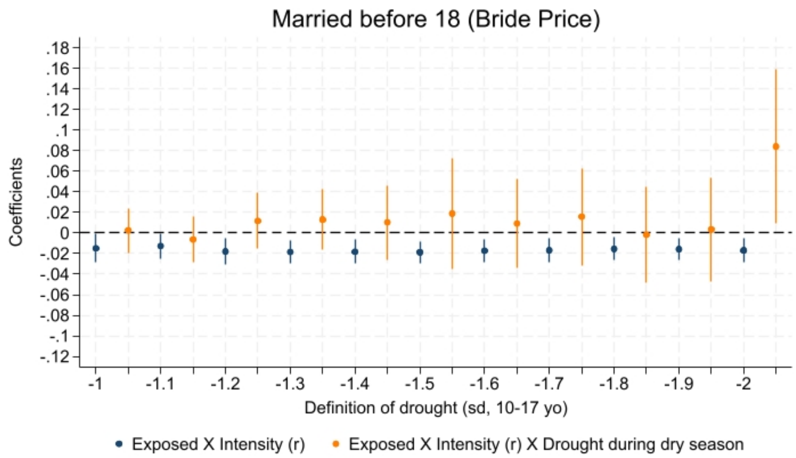
Figure A.21: Child marriage likelihood, bans, and yearly droughts



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. The Figure plots in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a year was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

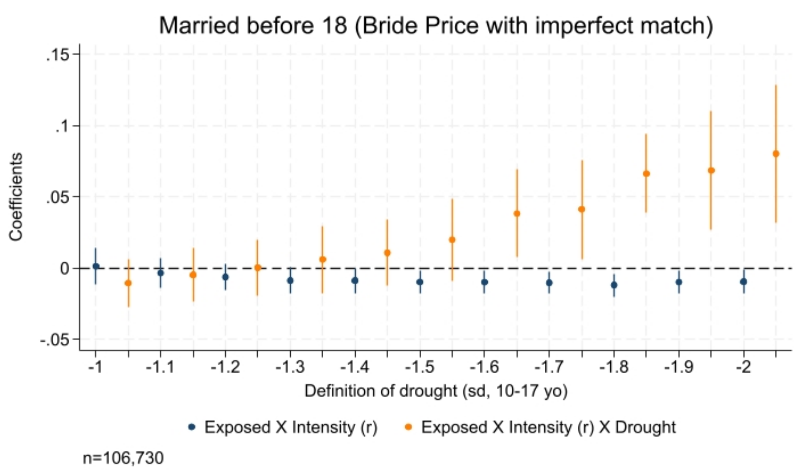
Figure A.22: Child marriage likelihood, bans, and droughts during dry seasons



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. The Figure plots in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a dry season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

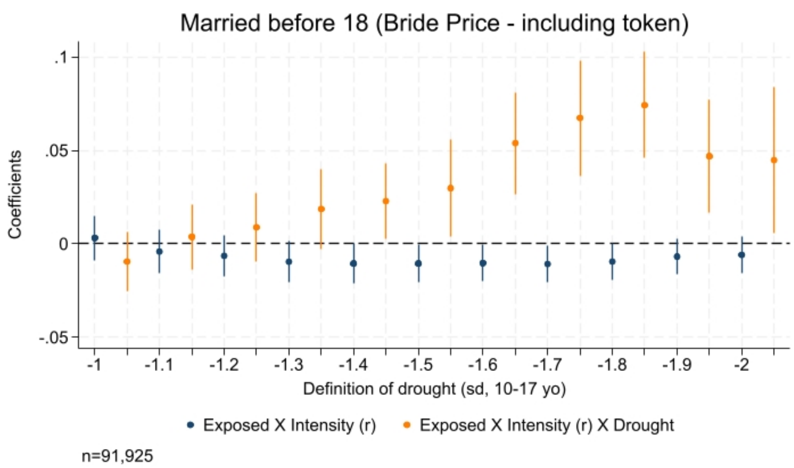
Figure A.23: Child marriage likelihood, bans, and droughts, with imperfect EA match



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. The Figure plot in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40. DHS clusters are matched with marriage payment practices according to Lowes (2017)'s imperfect EA match method.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations

Figure A.24: Child marriage likelihood, bans, and droughts, in Bride Price and Token Bride Price areas



Notes: The dependent variable is a dummy variable equal to 1 if the woman was married before 18 and 0 otherwise. The Figure plots in navy blue the coefficient associated with the interaction between the variable *Intensity* (standardized with mean 0 and standard deviation 1, describing the pre-ban regional child marriage intensity) and the dummy variable *Exposed* equal to 1 if the woman was below 18 yo at the time of the national ban, 0 otherwise. In orange is plotted the triple interaction with the variable *Drought*, which is a dummy variable equal to 1 if a rainy season was extremely dry during the adolescence of the girl (10 to 17 yo), 0 otherwise. Robust 90% confidence intervals clustered at the regional level are drawn. Regressions are restricted to women aged between 18 and 40.

Sources: DHS, MICS, CHIRPS, WORLD Policy Analysis Center, Murdock (1959; 1967), Author's calculations.

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