Impact of Minimum Marriage Age Law on Child Marriage: Evidence from Nigeria's Child Rights Act*

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Abstract

Child marriage, defined as marriage before the age of 18, remains a challenge worldwide. This study examines whether Nigeria's Child Rights Act, which included a minimum marriage age of 18, has reduced early marriage since its introduction and staggered adoption across states in Nigeria. Using four rounds of the Nigeria Demographic and Health Survey and a staggered difference-in-differences design that compares states before and after they adopt the law, the analysis constructs a state-year panel of marriage outcomes for women aged 15-29 years old. Our results show that the Child Rights Act led to a 2.3 percentage point reduction in child marriage, and a 0.121-year increase in the average age at first marriage. The effects strengthen gradually rather than appearing immediately, consistent with slow diffusion of the law and differences in enforcement capacity across states. The findings suggest that the Child Rights Act played a meaningful role in delaying marriage among young women in Nigeria, although its full impact may depend on sustained enforcement and other complementary interventions to support girls.

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1 Introduction

Child marriage refers to any formal marriage or informal union between a child under the age of 18 and an adult or another child (UNICEF, 2023a). Early marriage often forces girls to leave school earlier, which restricts them from accessing opportunities to acquire the education and skills necessary for their personal and professional growth. These vulnerabilities - being young, often poor and undereducated - affect the resources and assets they can bring into their marital household, thus reducing their decision-making ability (Parsons et al., 2015).

The consequences of early marriage are far-reaching. It is associated with poor socioeconomic and female empowerment outcomes among women (Corno et al., 2020; Field and Ambrus, 2008; Wilson, 2022). By lowering educational attainment, it diminishes the expected returns from participation in paid employment, while higher lifetime fertility often increases unpaid household responsibilities (Klasen and Pieters, 2012; Wodon et al., 2017). This reduced participation in the labor force exacerbates household poverty, further deepening the cycle of disadvantage (Parsons et al., 2015).

Child marriage remains a critical issue in Nigeria, where 24.38 million (out of a total of 110 million) girls and women are married before the age of 18, and Nigeria has the highest number of child brides in Africa (UNICEF, 2023b). However, the prevalence of child marriage varies significantly across the country, with rural areas and northern regions recording substantially higher rates compared to urban areas and southern states (Commission et al., 2019). These disparities are deeply rooted in the plurality of legal traditions around marriage, as well as cultural, religious, and socioeconomic factors, which make addressing this issue particularly challenging (Ikpebe, 2022; Fayokun, 2015; Ogunniran, 2010). In Nigeria, where 63% of the population lives in multidimensional poverty, these effects are devastating, as evidence shows that child marriage not only affects girls' personal development but also undermines broader national efforts to achieve sustainable economic growth and gender equality (National Bureau of Statistics; Wodon et al., 2017; Yousif, 2025).

In Nigeria, the enactment of the Child Rights Act (CRA) in 2003 was a critical step towards addressing child rights, including early marriage. The CRA, which prohibits the marriage of anyone under the age of 18 among other provisions, represents a legislative effort to protect children's rights and promote their well-being. However, its implementation has been uneven across the country, with significant variations in enforcement and acceptance, particularly in regions where cultural and religious practices conflict with the law. This paper examines the impact of the CRA on child marriage in Nigeria. Given the asynchronous domestication of the CRA across states, this study employs a staggered difference-in-differences

(DiD) approach developed by Callaway and Sant'Anna (2021) (hereafter CS2021) to assess the effectiveness of this minimum marriage age law on the prevalence of child marriage.

Using four rounds of data from the Nigeria Demographic and Health Survey (DHS), we find that the CRA reduced early marriage, though the effect is modest. Our estimate is a 2.3-percentage-point decline in the share of women marrying before age 18, with effects that are small right after enactment but grow over time. This translates into a roughly 3.5 percent reduction of child marriage using the baseline child marriage rate we calculated around 65 percent. Stricter cutoffs show similar results for marriages before ages 17, 16, and 15, the rates fall by 3.8, 3.1, and 2.8 percentage points. The average age at first marriage increases by about 0.12 years (roughly six weeks). The parallel trend assumption is satisfied for the child marriage and marriage age outcomes. For stricter cutoffs, the pre-trends are less flat, so we are cautious in treating those estimates as supportive. Taken together, these results highlight both the promise and the limits of the legislation in shifting entrenched social practice. The CRA marks an important milestone, but its ability to change behavior seems to depend on more than just passing a law. It also requires implementation capacity and societal acceptance. Our estimates are consistent with the view that the impact strengthens gradually as the law diffuses and is enforced.

This paper makes the following contributions. First, it contributes to the literature on marriage laws and child marriage in developing countries by focusing on Nigeria. While there are studies on how child marriage laws affect women in countries like India, Bangladesh, Mexico, and Ethiopia (Arthur et al., 2018; Batyra and Pesando, 2021; Bellés-Obrero and Lombardi, 2023; Dhamija and Roychowdhury, 2020; Field and Ambrus, 2008; McGavock, 2021), this paper uniquely contributes to the limited literature on Nigeria.

Second, we contribute to the merging empirical literature evaluating the effectiveness of CRA on child marriage in Nigeria. Despite the enforcement of the CRA since 2003, robust evidence on its direct impact on child marriage remains limited and contested. Previous studies have primarily focused on related outcomes. For instance, Gninafon (2024) examines the CRA impact on intimate partner violence against women while Mahar (2024) explores its effects on children's health outcomes such as stunting. This paper addresses contributes to this literature by providing a rigorious evaluation of whether the CRA has led to measureable changes in child marriage rates across Nigeria's diverse regions, explicitly addresing recent debates on the law's efficacy.

A recent working paper (Vergili, 2025) studying the CRA reports opposite-signed average effects using individual-level DHS data, interpreting the results as a "backlash" driven by conservative local norms. Our analysis differs in four fundamental ways that likely explain this divergence. First, we restrict our sample to women aged 15–29, focusing specifically on the cohort most plausibly exposed to the law around the time of adoption, whereas the recent study includes older cohorts (they included all women aged 15–49) whose marriage timing predates the policy. Second, we address the five-year survey spacing DHS data (collected only every five years) by constructing a state-year panel that aligns treatment timing with survey measurement waves, matches the temporal resolution available consistently across waves, and reduces sensitivity to retrospective timing error. Third, we enforce strict identification by using only not-yet-treated adopting states as controls and fixing covariates at the pre-treatment baseline to avoid endogeneity, whereas the recent study relies on contemporaneous controls and includes never-adopting states, which differ systematically from adopting states and might threaten overlap and lack common support. Finally, we rigorously validate our identification strategy using formal parallel trend tests within the CS2021 framework, rather than relying on visual inspection of trends. Consequently, our estimates should be interpreted as the causal effect of CRA domestication on the target demographic in adopting states—a finding that is complementary to, but methodologically distinct from, evidence on broader population-average effects.

Finally, this study also contributes to understanding the challenges of policy implementation. The CRA's varying levels of acceptance across states, especially in regions with strong cultural and religious norms that often contradict formal legislation, highlight the gap between law enactment and enforcement.

The rest of this paper is organized as follows. Section 2 provides background information and literature on child marriage in Nigeria and CRA enforcement. Section 3 describes our data and provides summary statistics. Section 4 presents the empirical strategy that we use to investigate the effect of CRA on child marriage. Section 5 presents our main results. Section 6 provides a variety of robustness checks and section 7 concludes the paper.

2 Background and Literature Review

2.1 Marriage and Early Marriage in Nigeria

Marriage in Nigeria is deeply rooted in cultural, religious, and socioeconomic practices that vary across the country's diverse regions (Walker, 2012). Four types of marriage are widely recognized in Nigeria: African customary marriage, Islamic marriage, Christian marriage, and statutory marriage. Notably, it is common for couples to contract their marriages under two or three of these laws; for example, Christian couples may have a traditional wedding, a church wedding, and a court wedding (Imam-Tamim et al., 2016). This is important because, although Christian and statutory marriages are monogamous, customary and Islamic marriages are potentially polygamous.

This coexistence of multiple marriage laws has implications, not just for monogamy and polygamy but also for the practice of early marriage. For example, while statutory laws require both parties to be legal adults, Islamic marriage laws do not have a specific age associated with adulthood or readiness for marriage (Oraegbunam and Udezo, 2012). Instead, a child's maturity is discerned through physical indicators of puberty, such as menstruation, the development of breasts, and pubic hair. This distinction becomes particularly relevant when examining the reluctance of several northern states in Nigeria to enact a minimum marriageable age policy of 18 years (Braimah, 2014).

In addition to the religious and legal traditions that allow early marriage in Nigeria, there are also economic drivers. In earlier decades, across most cultures, marriages were arranged by families, often with little or no input from the child involved. These arrangements are frequently influenced by economic considerations, as the payment of a bride price is a widespread custom (Princewill et al., 2019). The bride price, which varies significantly based on cultural and regional norms, is seen as compensation to the bride's family and can include money, livestock, or other goods (Braimah, 2014). However, in recent times, children have become more involved in the choice of their partners with relatively less input from parents. In all, the tradition of the bride price still remains and stands as the foundation of the marriage rites. Especially in rural areas where poverty levels are higher and educational opportunities are more limited, child marriage is often viewed as a way to secure the economic future of girls and reduce the financial burden on families (Nigeria Health Watch, 2021; Parsons et al., 2015). These sociocultural and socioeconomic dynamics make addressing child marriage in Nigeria particularly challenging, as efforts to enforce legal protections such as the CRA often conflict with long established traditions and widely understood economic realities.

All these legal, religious, and economic factors have led Nigeria to have the highest number of child brides in Africa, with about 24.38 million (out of a total of 110 million) girls and women getting married before the age of 18 as of 2023 (UNICEF, 2023b). Although the trend in child marriage has slowed in recent years, the rate of child marriage in the country is still significantly high, which we present as summary statistics in Section 3.

2.2 Child's Rights Act 2003

To challenge the practice of child marriage in Nigeria, the Child's Rights Act (CRA) was enacted in July 2003 and took effect from September 2003. Nigeria's CRA was a domestication of the United Nations Convention on the Rights of the Child and had the same objective of addressing issues related to child rights. Some key provisions of the CRA include the right to survival and development, the right to protection, the right to participation, education, healthcare, protection from exploitation, juvenile justice, and the registration of births (Child's Rights Act, 2003)[1].

Section III, 21 & 22 (Appendix Figure A8) specifically prohibits either the marriage or betrothal of anyone under the age of 18. Such unions are expressly considered null and void (Child's Rights Act, 2003). A contravention of either section 21 or section 22 results in a fine of 500,000 Naira[2] or imprisonment for a term of five years or to both a fine and imprisonment (Appendix Figure A9). The use of the conjunction 'or' may be deemed worrisome because it suggests that those who partake in child marriage may escape jail by only paying the stipulated fine. However, the option to pay a fine for child marriage appears to be the least of the CRA's problems, while the major problem is its differing level of acceptance and enforcement (Braimah, 2014).

Passing the legislation does not guarantee the execution of CRA. Each state in Nigeria has to enact the Act under its own state law before it is enforceable, which indicates that before CRA was passed as state law, child marriage could still be practiced legally.

The effectiveness of CRA implementation remains a question. So far, there is no evidence to show the extent to which CRA affects child marriage in Nigeria.[3] This research is able to provide evidence to shed light on the effectiveness of the law to protect children in Nigeria from getting married before reaching legal maturity.

3 Data

3.1 Data Sources and Coverage

This paper uses the Nigeria DHS data aggregated by IPUMS (Boyle et al., 2019). The DHS is a repeated cross-sectional, national sample survey that provides information on demographics and health indicators. The standard DHS is collected every five years. This study uses the following years' of the standard DHS data: 2003, 2008, 2013, and 2018[4]. The paper uses the

[1] See more details about the different sections of CRA in Appendix A.

[2] The equivalent of around 345 USD in December 2025 exchange rate.

[3] Mahar (2024) studies the impact of the staggered roll out of the CRA on child healthcare related outcomes.

[4] There was a standard DHS survey for DHS-IV phase collected in 1999, however, the survey dataset is not distributed because as indicated on the website: "DHS program was not centrally involved in this survey and cannot stand behind the quality of this survey. Data collected for women 10-49, indicators calculated for women 15-49". This research does not include the 1999 survey round for pre-trend checks because the DHS survey states borders have also changed from 1990 to 2003. All the state borders remain the same since the 2003 survey. The 2023 DHS data was not ready when this study was conducted, therefore, it could not be used either.

women survey sample for the study. The targeted groups for the DHS women's survey were women aged 15 to 49 years in randomly selected households in Nigeria.[5]

3.2 Marriage Trend in Nigeria

In the DHS survey, age at first marriage is defined as the age at which the respondent began living with his/her spouse/partner (age at first cohabitation). To understand the marriage trend in Nigeria, we follow McGavock (2021) and prepare Figure 1. It shows that the average age of marriage 10 years prior to the first CRA enactment and 10 years after the CRA enactment increased from below the age of 18 to over 19 years old. To identify where in the age distribution delays in marriage occurred, Figure 2a shows cumulative distribution functions (CDF) of the age of marriage for women married during 1993-2002 (solid blue), and 2003-2012(dashed red). All the years are inclusive. The figure shows that the probability of marriage at all ages less than 29 years old decreased over time. In particular, the probability of marriage by age 18 dropped from nearly 65% to less than 55%. It is possible that this change can be attributed to Nigeria's development in general that brought other gains in human capital and women's status. The question we are asking is then: how much does CRA contribute to this decrease in child marriage rate, or decrease in women's age of marriage in general?

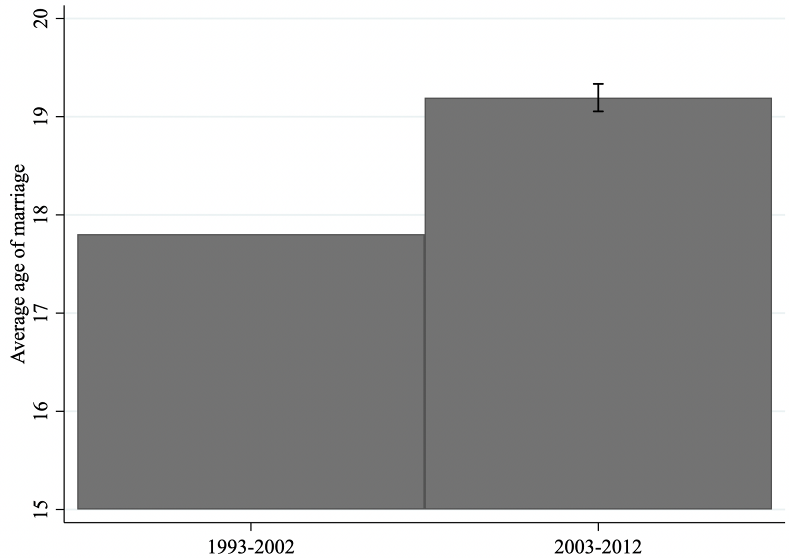
If we break the CDFs into four five-year periods, see Figure 2b, we can tell that the CDFs for the two five-year period prior to the CRA - 1993-1997 (solid gray) and 1998-2002 (solid blue) - are more similar to each other than they are to the five years immediately following the CRA enactment (dashed red). This provides us evidence suggesting that the decrease in child marriage in these five or ten-year periods right before and after CRA enactment was not solely due to Nigeria's development in general in combating child marriage.

3.3 Analytical Sample and Age Restriction

The goal of the CRA is to make child marriage (marriage under the age of 18) less likely. We restrict our analysis to women aged 15-29 at the time of the DHS survey for our main analysis. The reasons are as follows: first, women in this group are most likely to have experienced marriage decisions during the period when the CRA was enacted and gradually enforced across states. Including older women (30+) would introduce marriages that occurred long before CRA implementation, reducing the relevance of the treatment effect and potentially

[5] In the women's survey, the following information was collected: reproduction, contraception, pregnancy and postnatal care, child immunization, child health and nutrition, marriage and sexual activities, fertility preferences, husband's background and women's work, HID/AIDS, other health issues, female genital cutting/mutilation, fistula, adult and maternal mortality, and domestic violence.

Figure 1: Average Age of Marriage Pre- and Post- CRA.

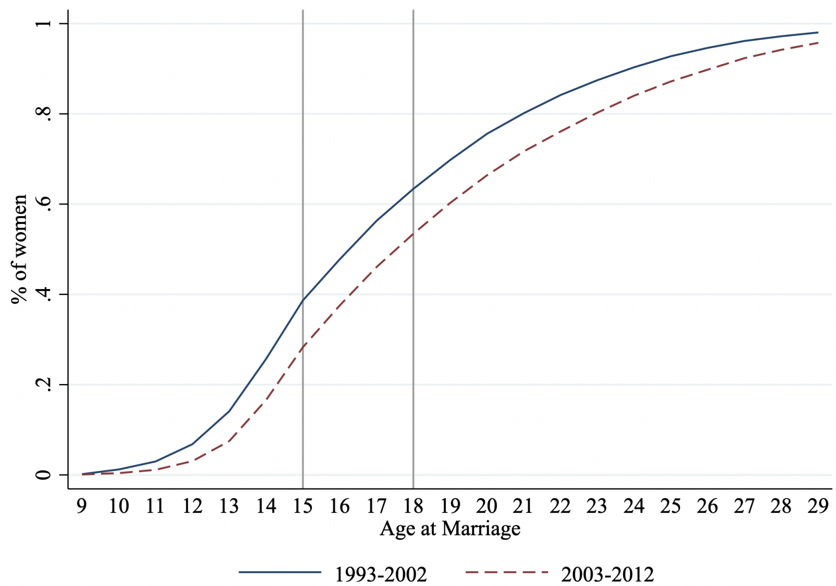


Source: Author's calculation from Nigeria DHS (2003, 2008, 2013, 2018).
Note: This figure shows the average age of marriage across the decade immediately before and immediately after the CRA.

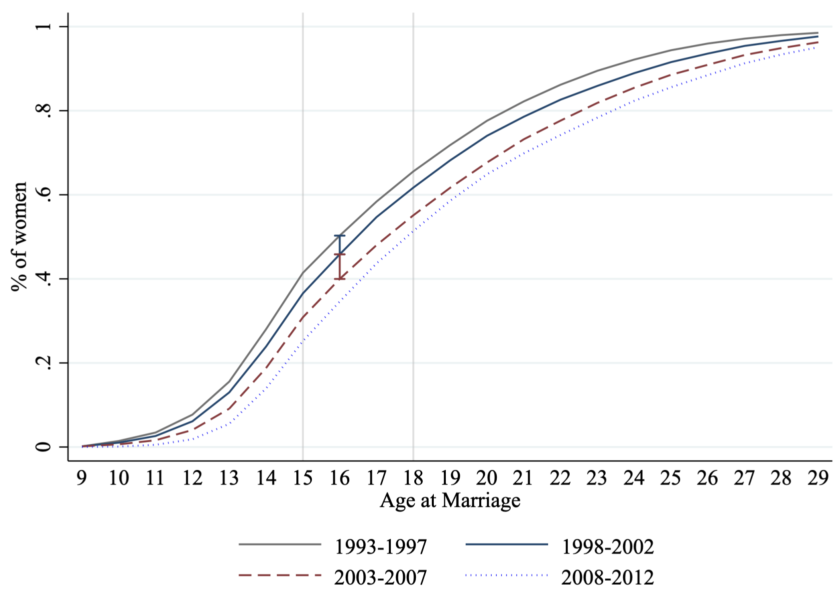
biasing our estimates because older women's marriage decisions largely predate the CRA enactment period. Second, focusing on 15-29 years old ensures that our main outcome, child marriage, is measured for individuals whose marriage timing overlaps with the policy rollout, making them the population most directly exposed to the CRA enactment. Choosing a younger age group (e.g., 15-24) may tighten exposure but shrink our sample size and cross-state support for the estimation, and choosing an older age group (e.g., 15-34) adds observations that are mostly never at risk post-CRA, biasing our estimates. We argue that the 15-29 age group balances precision and interpretability.

Empirically, the timing of marriage in Nigeria is concentrated in adolescence and early adulthood. In our DHS-based calculation, Table 1 summarizes the percentage of women married before age 18 by a five-year age group across the four DHS survey rounds. It shows that women aged 15-17 are still "at-risk", and those aged 20-29 have just passed the threshold, so their marriage timing is closely tied to any policies or economic changes that might impact the marriage timing. 93% of women aged 15-19 and 65% of those aged 20-24 were married before age 18, compared to 51% for ages 25-29 and 50% for ages 30-34. These patterns confirm that the CRA's intended beneficiaries are girls under 18 years old and it would be best captured by focusing on women ages 15-29, whose marriage decisions plausibly overlap with the policy rollout. We re-estimate our results using age groups 15-24 and 15-34 as robustness checks for our main results.

Figure 2: CDFs of Age at First Marriage



(a) by 10 years



(b) by 5 years

Source: Author's calculation from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Figure (a) shows the data by two periods: the decades before and after the CRA in 2003, and Figure (b) shows the data by four periods: the period before and after the CRA in 2003, every five years. The youngest age reported for marriage was 4 years old, and the oldest age at marriage was 48. This figure kept the age of marriage in the range of 9 to 29 years old, as over 90% of women married before the age of 29.

Table 1: % of Women Married < 18 by Age Group & Year

Age Group	Average Percentage				
	Overall (1)	2003 (2)	2008 (3)	2013 (4)	2018 (5)
15-19	92.63	92.90	93.03	91.87	93.07
20-24	64.67	67.72	63.95	63.98	65.30
25-29	51.40	53.51	50.46	53.93	49.33
30-34	50.58	64.54	50.66	52.26	46.85
35-39	47.57	64.84	50.33	49.69	41.07
40-44	50.66	65.58	53.56	50.82	45.62
45-49	53.74	71.25	55.88	55.86	47.30

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: The table shows the weighted percentages of women married before age 18 by age-at-survey group and DHS round (2003, 2008, 2013, 2018). Estimates use DHS person weights.

We discuss the construction of our outcome variables in detail and conduct balance checks for covariates in Section 4.

3.4 CRA Enactment Timing

We compile the state-level CRA enactment timing from the Rule of Law and Empowerment Initiative (Partners West Africa Nigeria, 2025). For each state, we record the year the CRA was enacted and the state's geopolitical zone. Appendix Table A2 lists the state-by-state enactment year. Appendix Table A3 summarizes enactments by DHS data collection windows (2003, 2008, 2013, 2018) and by zone: South West (SW), South East (SE), South South (SS), North Central (NC), North West (NW), and North East (NE). Three patterns stand out in Appendix Table A3 and A2. First, enactment occurred earlier in the southern zones and in the NC, while states in the north, especially in the NW, enacted later. Second, there are long intervals with no new enactment, including the years 2011 through 2016, 2017, and 2019 through 2020. Third, several zones display clustered timing, with many states enacting in similar periods, particularly NC, NW, SW, and SS. Out of 36 states plus the Federal Capital Territory (FCT), only Gombe in the NE has not enacted the CRA as of 2025.

4 Empirical Strategy

This paper leverages the staggered enactment of the CRA in each state in Nigeria since 2003 and employs an event study design to estimate the effect of CRA enactment on child marriage in Nigeria. Treated states are defined as states that enacted CRA after a certain year. For example, while the CRA has been enacted at the federal level since 2003, if a state also domesticated the CRA in 2008, the state is regarded as treated since 2008.

In order to use a DiD design with staggered rollout and two-way fixed effects to estimate a causal relationship, our model needs to satisfy three main assumptions. The first assumption is the assumption of parallel trends: in the absence of treatment, the evolution of the outcome among treated groups is on average the same as the evolution of the outcome among control groups. We test the parallel trend assumption in our analysis.

The second assumption is the "no-anticipation" assumption. We must assume that state-level child marriage rates were not affected by the CRA expansion before the CRA was adopted, thus we observe untreated potential outcomes before the CRA adoption takes effect. This assumption also helps us define effective treatment dates. For instance, if the announcement of CRA affects child marriage rates before its actual adoption, "treatment" begins when the policy is announced rather than when it is adopted/implemented. Note that violation of this assumption biases our estimates towards finding no effect, which merely strengthens the robustness of our statistically significant estimated effects, even if their estimated magnitude may be biased towards zero.

The third assumption is the overlap assumption: the weighted conditional probability of belonging to the treatment groups, given observed covariates, that are determinants of untreated potential outcome growth, is uniformly bounded away from zero and one. We construct and carefully select our data in such a way that the overlapping condition is satisfied, and we provide event-time support robustness checks in Section 6.3.

This paper uses the "not-yet-treated" every-adopter states as the control states in the study. We use the CS2021 set-up for the DiD because in such a set-up, we could study the average treatment effects in the DiD design with:

1. Multiple time periods;
2. Variation in treatment timing (but with staggered treatment adoption);
3. The parallel trends assumption holds after conditioning on observed covariates.

Some unique features of the CS2021 setup are beneficial for our study because CS2021 attempts to make minimal parallel trends assumption to identify the average treatment on the treated (ATT), allowing for covariates in a flexible form, and proposes different estimation procedures based on the regression adjustment (RA), inverse propensity weighting (IPW) and double robust (DR) methods. They also provide aggregation schemes to further summarize the effects of the treatment. In this paper, we use the DR procedure because it adds additional "protection" against modeling assumptions compared to RA and IPW (Baker et al., 2025). Under CS2021 staggered DiD framework, it is important to make sure that once a unit is treated, it remains treated. So far, there is no evidence that any state has domesticated and then repealed the CRA.

We go into details about how we construct the state-level data for the analysis and our estimand and identification.

4.1 Constructing a state-year panel (2003-2018)

Using only the four DHS survey rounds, 2003, 2008, 2013, and 2018, would leave our analysis with very limited temporal variation in treatment exposure. Many states enacted the CRA between 2004 and 2010, meaning that their first observable post-treatment period in the DHS calendar would not occur until 2013. Even times around enactment would therefore have extremely thin support, producing noisy and unstable dynamic estimates. In addition, by 2018, most of the states were already treated, leaving very few not-yet-treated comparison states and weakening our identification. Moreover, relying solely on survey-round timing combines short and long post-enactment intervals where "period 0" could represent anywhere from a few months to nearly five years after the law. This would make it difficult for us to interpret short-run dynamics.

Constructing an annual state-year panel from women's reported marriage timing allows us to align each state's enactment onset precisely with its enactment year, maintaining balanced support across pre- and post-periods, therefore producing more reliable estimates of both immediate and long-run effects. We therefore create an annual state-year panel from 2003 to 2008.

Starting from the IPUMS-DHS women's survey for 2003, 2008, 2013, and 2018, and using each women's survey year, current age, and women's age at first marriage[6], we can backfill the data to create the annual state-year panel from 2003 to 2018. We restrict our sample to women aged 15 to 29 (the population most exposed to policy-relevant marriage age thresholds). We test the robustness of restricting the sample to age 15-24 in the robustness check Section 6.

[6]Age at first marriage is defined at the first year that the woman started living or cohabiting with her partner.

For each state s and calendar year t, we compute the following outcomes at the state-year level as shares, and all of them are weighted using the DHS person sample weight.

- $y_{st}^{<18}, y_{st}^{<17}, y_{st}^{<16}, y_{st}^{<15}$: share of women married before age 18, 17, 16, or 15, respectively;
- $y_{st}^{agefrstmar}$: mean age at first marriage.

Pre-treatment "baseline year" mapping: we construct cohort-baseline state covariates from the last DHS round strictly before CRA enactment and hold them fixed over time. For each state s that eventually enacts the CRA in calendar year g_s, we set its baseline (pre-treatment) year to the last DHS round strictly before enactment. We align our covariates' timing to actual measurement because our DHS waves are discrete (2003/2008/2013/2018). For example, if a state enacted CRA in 2011, the last observed pre-treatment covariates come from 2008, not 2011.

- If $g_s \in [2004, 2008]$, baseline $b_s = 2003$
- If $g_s \in [2009, 2013], b_s = 2008$
- If $g_s \in [2014, 2018], b_s = 2013$
- If the state never enacted in our window or enacted after 2018, it is a never-treated state in 2003-2018. We therefore take $b_s = 2018$ as its reference. However, since we use not-yet-treated ever adopters as our comparison group, never-treated states are automatically dropped from our final analysis.
- The 2003 early adopter (FCT) has no observed pre-treatment in our window, so b_s is missing for pre-trend diagnostics.

We then construct our baseline covariates X_s^{pre} using the DHS state-level means in year b_s and hold them fixed for all t. These covariates enter the conditional parallel-trend adjustment in the DiD. Mapping g_s to the nearest prior DHS makes covariates empirically attainable and consistent. It also helps us preserve the comparability across cohorts. States enacting in different calendar years all draw pre-treatment characteristics from the same DHS round with their cohort bin $2003, 2008, 2013, 2018$. This reduces noise from survey to survey design changes and ensures balanced conditioning across treated and not-yet-treated states.

We consider the following X_s^{pre}: percentage of population living in the rural area, percentage of population living in each wealth quintile[7], percentage of population that is Muslim[8]. We check the balance of these variables because they capture the key pre-existing structural

[7]poorest, poorer, middle, richer, richest, and we drop the poorest group in the analysis.
[8]we dropped other religious groups such as Catholic, Protestant, other Christian in the analysis.

and cultural characteristics that shape both child marriage practices and the timing of the CRA enactment across states. Rural residence and household wealth are strongly correlated with poverty and educational access, two well-documented drivers of early marriage in Nigeria. And the religious composition, in particular as the share of the Muslim population, reflects the differences in the customary and religious law systems that directly influence the marriage norms and the legal adoption of the CRA as we mentioned in section 2. Ensuring that treated and not-yet-treated ever-adopter states are comparable along these dimensions helps assess whether differences in child marriage outcomes can be attributed to the CRA itself rather than to long-standing socioeconomic or cultural disparities.

We report the averages of these variables by not-yet-enacted every-adopter states vs. ever-adopter states with population weights. We report a measure of imbalance that is comparable across variables: the normalized difference in means between the comparison group (not-yet-enacted states) and the treatment (enacted states) (Imbens and Rubin (2015), Chapter 14).

$$\text{Norm. Diff}_{\omega} = \frac{\bar{X}_{\omega,T} - \bar{X}_{\omega,C}}{\sqrt{\left(S^2_{\omega,T} + S^2_{\omega,C}\right)/2}}$$

where $\bar{X}_{\omega,T}$ and $\bar{X}_{\omega,C}$ are the sample weighted average for the treatment and comparison groups, respectively, and $S^2_{\omega,T}$ and $S^2_{\omega,C}$ the corresponding weighted variance. Following Imbens and Rubin (2015) (page 277), normalized differences exceeding 0.25 in absolute value indicate a potentially problematic imbalance. According to Table 2, we find a meaningful imbalance in all of the baseline measures we used in this table: later-adopter states are more rural, poorer, and have a much higher Muslim population share than earlier-adopter states. We therefore include those imbalanced baseline measures as controls in our DiD specification.

Table 2: Covariates Balance Statistics

Variable	Early adopters (1)	Later adopters (2)	Norm. Diff (3)
Lives in rural	0.61	0.66	-0.28
Household wealth index quintiles			
Poorest	0.18	0.14	0.29
Poorer	0.15	0.21	-0.57
Middle	0.15	0.28	-1.93
Richer	0.24	0.21	0.25
Richest	0.28	0.17	0.60
Religion			
Muslim	0.26	0.47	-0.62
Catholic & other religions	0.24	0.13	0.53

Source: Nigeria DHS (2003, 2008, 2013, 2018).
Note: This table reports the averages and standardized differences of each covariate comparing CRA early adopters ($b_s = 2003$) vs. later adopters ($b_s \in \{2008, 2013\}$) states. All variables are in percentages. All measures are weighted.

4.2 Treatment timing and DHS-grid cohorts

We work with an annual state-year panel (2003-2018) that we construct from the four Nigerian DHS rounds (2003, 2008, 2013, 2018). Using each woman's reported age at first marriage and survey year, we recover the woman's marriage year and whether the marriage occurred before the age we defined (we use ages 15, 16, 17, and 18). We then defined the individual outcome as a dummy that turns from 0 to 1 in the calendar year she marries $<$ 15, 16, 17, or 18 and stays 1 thereafter. After aggregating the DHS data to the state-level annual shares of married under 15, 16, 17, or 18 and average age of first marriage with person weight, our outcomes become available yearly, not just at the DHS survey years.

Since covariates are observed only in survey rounds, we freeze each state's covariates at the last DHS wave strictly before enactment (Section 4.1) to avoid post-treatment bias and carry those baseline values to all years. Even with annual outcomes, we could align the first treated cohort to the DHS grid for treatment timing. Let $gvar_s$ be state s's enactment year. We map it to the first observed DHS round at/after enactment:

$$
g_s^{\star} = \begin{cases} 2003 & \text{if } gvar_s \leq 2003, \\ 2008 & \text{if } 2004 \leq gvar_s \leq 2008, \\ 2013 & \text{if } 2009 \leq gvar_s \leq 2013, \\ 2018 & \text{if } 2014 \leq gvar_s \leq 2018, \\ 0 & \text{if } gvar_s > 2018 \end{cases} \tag{1}
$$

g_s^* indicates never treated group if never-enacted or enacted after 2018[9]. See the adoption states by early, middle, late and never adoption status in Appendix Table A4. We then define treatment at the state-year level as $D_{st} = 1\{t \geq g_s^*\}$.

The reasons why we decide to use DHS-grid cohorts despite having constructed annual outcomes are as follows: first, many of the enactments occur between 2004 and 2010. The first observed post-period in our data is then the next DHS year (2013). Aligning g_s^* to that year ensures the "first post" used in estimation matches an actual observation point for both outcomes and baseline covariates, keeping the treatment onset on the same measurement grid as the covariates on which we condition. Second, if we do not use the DHS-grid cohorts, our raw-year cohorts split event time into many thin cells with few not-yet-treated ever-adopters comparison groups, generating omissions and noise into our estimation, and these estimates may end up not having enough common support. Binning to the DHS cohort (2003, 2008, 2013, 2018) concentrates comparisons at a small set of shared event times (e.g., -2, -1, 0, +1, ...) in the annual panel, substantially improving overlap. Lastly, the constructed annual outcome leverages retrospective marriage timing, which is well-captured in DHS, but ages can exhibit heaping[10]. Evaluating dynamics around survey-year anchors mitigates spurious long-lead patterns driven by thin support or reporting noise.

By our design, the 2003 early adopter (FCT) has no observed pre-trend within 2003-2018; therefore, it contributes to post periods but not to lead diagnostics. States that enacted CRA after 2018, e.g., $g_s > 2018$, are coded as never-adopter states within our

[9] Appendix Table A2 for CRA enactment year by states.

[10] Age heaping is a phenomenon where survey respondents tend to round their age to the nearest 0 or 5, leading to an inaccurate age distribution with peaks at these numbers instead of a smooth curve. Indeed, DHS's own methods report, Pullum and Staveteig (2017), documents systematic rounding in ages and in ages-at-event (such as age at first sex/marriage). Nigeria is one of the countries with the highest level of heaping with at least 10% heaping in reported age from the woman's interview. According to Pullum and Staveteig (2017), "age heaping is measured with Myers' Blended Index, which can be interpreted as the percentage of cases that would have to be shifted from over-represented final digits to under-represented final digits, with an adjustment to take account of the general gradient in the age distribution.". Fayehun et al. (2020) also found prominent age heaping in all three rounds of the Nigeria DHS survey in 2003, 2008, and 2013. Although Nigeria was not one of the countries in the study, Neal and Hosegood (2015) found that women aged 15-19 were much less likely to report marriages and first births before age 15 than were women from the same birth cohort when asked five years later at ages 20-24.

window and are dropped from our study because we use not-yet-treated every-adopter states as the comparison group. We deliberately exclude never-adopter states because baseline covariates differ significantly and violate overlap (Appendix Table A6): never-adopters are substantially more rural, poor, and more Muslim than ever-adopters. The absolute value of the normalized difference is much larger than 0.25 and beyond the threshold to include them as control variables. Including them yields noisy late-event cells, and implausibly flat dynamics (Appendix Figures A1). Using not-yet-treated ever-adopters preserves common support around each event time and aligns with CS2021 identification with "not-yet-treated" controls. Our figures and tables therefore use not-yet-treated ever-adopters as controls, and we truncate event time where cell support is thin as a robustness check in Section 6.3.

Anchoring to the DHS grid changes when a state is considered to be treated, but it does not collapse our time variable. When we keep annual year = 2003 . . . 2018, our event time is:

$$e = t - g^*, t \in \{2003, \dots, 2018\}, g^* \in \{2003, 2008, 2013, 2018\}$$

With our DHS-grid cohorts construction, two adoption cohorts contribute to the dynamics observed between 2003 and 2018. States that enacted CRA between 2003 and 2008 are grouped into the 2008 cohort, which yields possible event times from $e = -5$ (2003-2008) to $e = +10$ (2018-2008)[11]. States that enacted CRA between 2014 and 2018 form the 2018 cohort, contributing pre-treatment leads up to $e = -15$ (2003-2018), and there are no post-treatment periods because 2018 is the last DHS round. Therefore, the overall support range across cohorts is $-15 \leq e \leq +10$.

Although the constructed annual state-year panel yields a theoretical even-time range from $e = -15$ to $e = -10$, the tails of this distribution are supported by very few cohort-year combinations. Table 3 documents that while event times $e = -10$ and $e = 10$ still have at least two adoption cohorts present on the DHS-grid, the corresponding average treatment effect on the treated (ATT) cells are very sparse: $e = -10$ is based on a single lead period for a small number of earlier adopters, and $e = +10$ is effectively identified from one earlier adopter cohort with a single late-adopter cohort as comparison. We therefore restrict our main event-time study graphs and aggregation to the range $e \in [-9, +9]$. You can find the detailed event-time mapping by calendar year t in Appendix Table A5.

[11]Under the cohort mapping rule in Equation 1, states that enacted the CRA in 2004–2008 are assigned to the 2008 grid, while those who did so in 2003 or earlier are assigned to the 2003 grid. Only the FCT enacted in 2003. Because it is treated throughout the entire observation window, it is excluded from estimation. Hence, the earliest effective cohort in our sample is 2008.

Table 3: Event-time support across adoption cohorts

Event-time window $e = t - g_s^*$	Cohorts contributing g^*	Not-yet-treated adopters as comparison group	Support Quality
$[-15, -11]$	2018	Yes	Very thin (single cohort)
$[-10, -6]$	2013, 2018	Yes	Moderate (two cohorts)
$[-5, 0]$	2008, 2013, 2018[a]	Yes[b]	Strong (three cohorts)
$[1, 5]$	2003, 2008, 2013	Yes[c]	Strong (three cohorts)
$[6, 10]$	2003, 2008	Yes (2013, 2018 cohorts not yet treated)	Moderate (two cohorts)
$[11, 15]$	2003	No (all ever-adopters treated)	Fails (no overlap)

Notes: Event time is $e = t - g_s^*$, where t is calendar year (2003–2018) and $g_s^* \in \{2003, 2008, 2013, 2018\}$ is the DHS-grid cohort: the first DHS round at or after CRA enactment in each state. The 2003 cohort is treated throughout our pre-period and contributes to only post-treatment observations. A valid not-yet-treated comparison group requires at least one cohort with $g_s^* > t$. This fails for $e \geq 11$, since by 2018 all ever-adopting cohorts have been treated. We therefore retain the range $e \in [-9, +9]$ in our main analysis, where (i) at least two treatment cohorts contribute and (ii) not-yet-treated comparison states are still available. (a) At $e = 0$, the 2003 cohort also contributes but has no pre-treatment support. (b) At $e = 0$, comparison exists only for $t < 2018$. (c) For $e \in [1, 5]$, 2018 cohort remains not-yet-treated.

4.3 Estimand and identification

Let $g_s^* \in \{2003, 2008, 2013, 2018\}$ be the cohort for state s (first DHS wave at or after CRA enactment). We define treatment at the state-year level as $D_{st} = 1\{t \geq g_s^*\}$ (Section 4.2). Outcomes, $Y_{st}(d)$, are annual state-level shares (or mean) constructed from women aged 15-29 using DHS person weights (Section 4.1): percentage of women married under age 15 (y_{st}^{15}), 16 (y_{st}^{16}), 17 (y_{st}^{17}), 18 (y_{st}^{18}), and the average age of marriage ($y_{st}^{agefrstmar}$). X_s^{pre} denote the pre-treatment (last-DHS) state covariates held fixed over time (Section 4.1).

Following CS2021, the building block is the group-time average treatment effect on the treated (ATT):

$$\text{ATT}(g, t) = E\left[Y_{st}(1) - Y_{st}(0) \mid g_s^* = g\right], \quad t \geq g \tag{2}$$

The average effect in year t for states whose first treatment is in cohort g.

Let $\mathcal{C}_t = \{s : g_s^\star > t$ be the not-yet-treated group of states in year t. We assume (i) no anticipation (effects begin weakly at $t = g_s^*$) and (ii) conditional parallel trends in untreated outcomes with covariates:

$$E[Y_{s,t}(0) - Y_{s,t-1}(0) \mid g_s^* = g, X_s^{pre}] = E[Y_{s,t}(0) - Y_{s,t-1}(0) \mid s \in \mathcal{C}_t, X_s^{pre}] \tag{3}$$

We implement the CS2021 Doubly-Robust Inverse Probability Weighted (DR-IPW) estimator using not-yet-treated ever-adopter states as the comparison states and cluster the standard errors at the state level.

Let event time be $e = t - g$. We display even-study effects by averaging $\widehat{ATT}(g,t)$ across timing groups that contribute at each e[12], and report a windowed post-t average over a specified set event window $\mathcal{K}$ (e.g., $e \in \{0, \cdots, 9\}$:

$$\widehat{ATT}^{W} = \sum_{(g,t):t-g\in\mathcal{K}} \omega_{g,t}\widehat{ATT}(g,t) \tag{4}$$

with weights $\omega_{g,t}$ proportional to the size of the g^* cohort contributing at year t. All estimation is clustered at the state level. One can also report the ATT across calendar years, cohort, or event plus cohort.

For expositional purposes only, the event time is mapped into the following equation:

$$Y_{st} = \alpha_s + \delta_t + \sum_{e\in\mathcal{K},e\neq-1} \beta_e \mathbf{1}\left\{t - g_s^\star = e\right\} + \Gamma^\top X_s^{\text{pre}} + \varepsilon_{st} \tag{5}$$

We do not estimate this with a naive two-way fixed effects (TWFE) model. Instead, the plotted β_e is interpreted as the event-time aggregation at a single e, i.e., $\widehat{ATT}^{es}(e) = \sum_{(g,t):t-g=e} \widehat{\omega}_{g,t}\widehat{ATT}(g,t)$, with not-yet-treated ever-adopters as comparison and DR-IPW. This avoids the well-known negative weights and heterogeneity biases of standard TWFE in staggered designs (Borusyak et al., 2024; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021).

5 Results

Figure 3 plots the dynamic treatment effect using DHS-grid cohorts and not-yet-treated states as comparison with DR-IPW weighting. Standard errors are asymptotic and clustered at the state level. We see that for the share of women married before age 18 ($Y_{st}^{<18}$), the pre-trend is flat and statistically indistinguishable from zero, and the post-period coefficients are negative and grow in magnitude over event time. We report a simple average of all non-negative event times as a summary of the overall ATT in Table 4, similar to what has been done in Baker et al. (2025). We also report the joint pre-trend test.

The joint pre-trend test cannot reject the null hypothesis that all pretreatment ATTs are statistically equal to zero (p=0.807), indicating that our parallel trend assumption is indeed satisfied for this result. Aggregating all non-negative event times, ATT implies a reduction of 2.3 percentage points (S.E. 0.55, p=0.002) of child marriage. Given the baseline

[12] This is similar to Baker et al. (2025) eq (5.13).

child marriage rate is around 65 percent (Figure 2a), this indicates a roughly 3.5 percent reduction of child marriage[13].

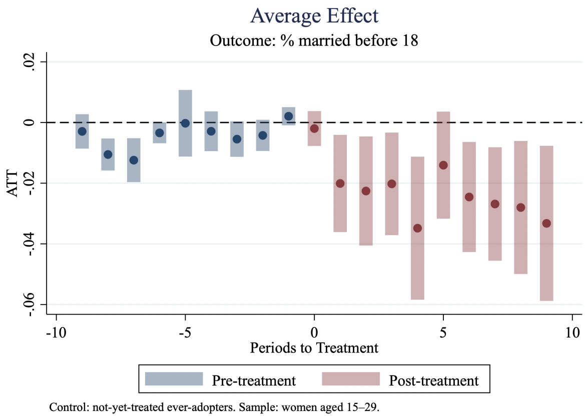
After we tighten the age cut-off, the drop gets bigger. After the CRA enactment, the share of women first married before age 17 falls by around 3.8 percentage points, the share of women first married before age 16 falls by around 3.1 percentage points, and the share of women first married before age 15 falls by around 2.8 percentage points (all statistically significant at 1% level). The effect also grows over time, and it is smaller right after the law a nd larger a few years later. This suggests that the law takes time to spread and be enforced at the local level. As a complementary intensive margin outcome, the average age at first marriage rises by 0.121 years post-enactment, and the result is statistically significant at 5% level.

One caution we shall pay to these results is that for the stricter cutoffs (<17, <16, and <15), the pretrends aren't perfectly flat, and parallel trend assumptions are violated as shown in the joint pretrend test in column (1) of Table 4. It does not mean that these results are not meaningful. We shall put more weight on child marriage before age 18, and the shift in the average marriage age results.

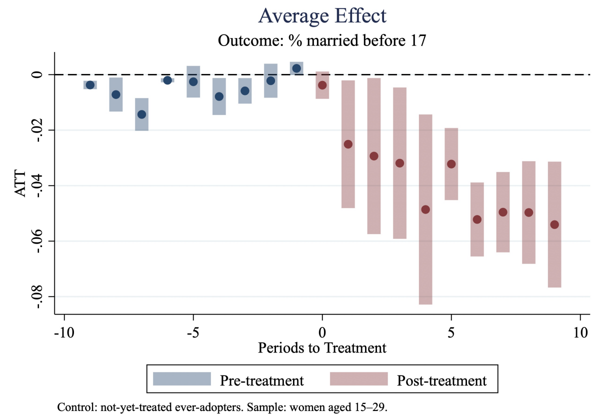
Overall, we find that the CRA enactment is associated with fewer early marriages and a slight delay in marriage timing. The effect of the law grows over time, indicating a gradual enforcement, awareness of, and adaptation to the law rather than immediate legal compliance.

[13] $2.3/65 \approx 3.5$ percent.

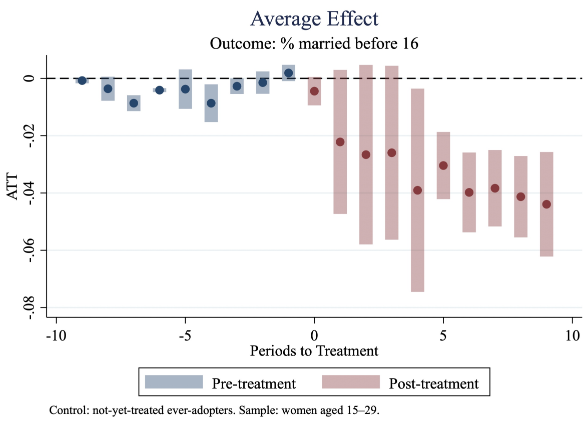
Figure 3: ATT of % Married Under Certain Age



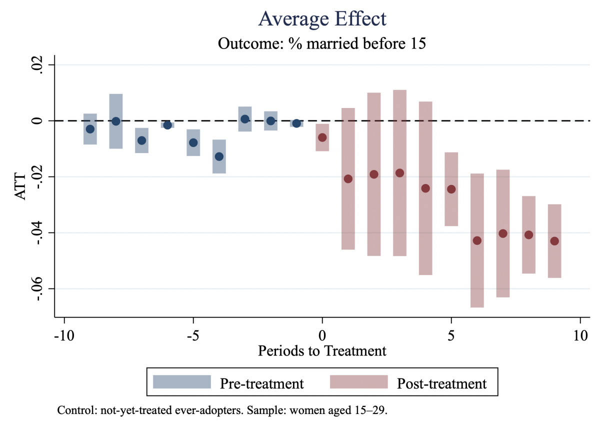
(a) % Married Under Age 18



(b) % Married Under Age 17



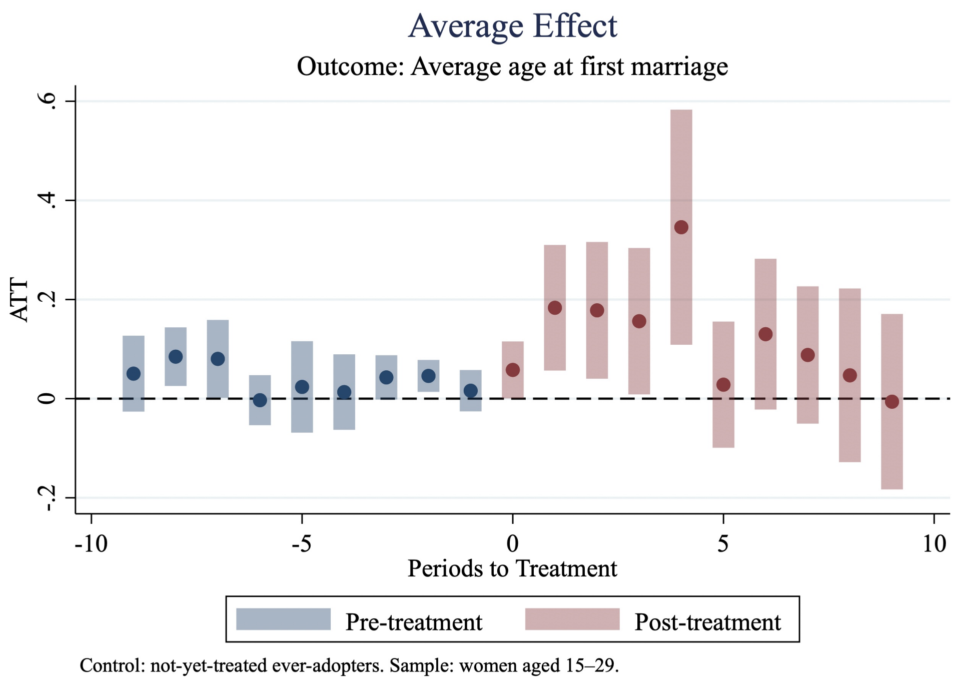
(c) % Married Under Age 17



(d) % Married Under Age 15

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Figure 4: ATT of Mean Age at First Marriage



Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Table 4: Pretrend Tests and Aggregated ATT at Event Time

	Pre-trend	ATT (event time)			
Outcome	P-value	Coefficient	SE	95% CI	P-value
	(1)	(2)	(3)	(4)	(5)
% married before 18	0.807	-0.023	0.007	[-0.037, -0.008]	0.002
% married before 17	0.032	-0.038	0.008	[-0.053, -0.022]	0.000
% married before 16	0.012	-0.031	0.007	[-0.045, -0.018]	0.000
% married before 15	0.000	-0.028	0.005	[-0.037, -0.019]	0.000
Average age at first marriage	0.274	0.121	0.056	[0.011, 0.231]	0.031

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Outcomes are state-level shares (in percentage points) married before age 15, 16, 17, or 18, and the average age at first years in years, computed with DHS person weights. We restrict our sample to women aged 15-29 years. Estimates use the staggered DiD developed by Callaway and Sant'Anna (2021) with the DR-IPW method, clustering at the state level. Controls are not-yet-treated ever-adopter states. Never-adopters are excluded from the analysis. Covariates are fixed to the most recent pre-enacement DHS

round for each state including percentage woman living in rural area, women's household wealth quantile shares (Q2-Q5), and percentage of women that are muslin. Column (1) reports the p-value from the joint pre-trend hypothesis test of null hypothesis as all pretreatment ATTGT's are statistically equal to zero. Column (2)-(5) report the simple average of all non-negative event times as the summary of the overall ATT, its standard error, 95% confidence interval, and p-value, respectively. Unless noted otherwise, standard errors are asymptotic and clustered by states.

6 Robustness Checks

To test the robustness of our main results, we run robustness checks by using different age cutoff samples, using the raw enactment year instead of a state-year panel, and trimming the event-time support window.

6.1 Different age cutoff

6.1.1 Re-estimate for women aged 15–24

We first restrict our analytical sample to women aged 15 to 24 years at the time of the DHS survey. The results in Table 5 show that the estimated effects are smaller in magnitude and statistically less precise compared to our main specification using women aged 15 to 29. The average treatment effect for the share of women married before age 18 is -0.8 percentage points, and the pre-trend test fails to reject the null of parallel trends (with a p-value of 0.516), indicating that the parallel trend assumption is satisfied. Although the coefficients remain negative for all marriage-age cut-offs, they are not statistically significant. This weaker pattern likely reflects a smaller sample size and reduced across-state variation, as well as the fact that some younger women had not yet reached marriageable age by the survey year. Overall, the re-estimated results using women aged 15 to 24 suggest that the direction of the effect remains consistent with the main findings, but the magnitude of the estimated decline in child marriage is attenuated when we focus only on the youngest age group. We include the staggered DiD figures in Appendix A, Figure A2 and Figure A3.

Table 5: Pre-trend Tests and Aggregated ATT at Event Time (Women Sample Aged 15-24)

	Pre-trend	ATT (event time)			
Outcome	P-value	Coefficient	SE	95% CI	P-value
	(1)	(2)	(3)	(4)	(5)
% married before 18	0.516	-0.008	0.025	[-0.057, 0.040]	0.743
% married before 17	0.487	-0.037	0.031	[-0.098, 0.025]	0.242
% married before 16	0.016	-0.036	0.030	[-0.095, 0.022]	0.226
% married before 15	0.063	-0.020	0.013	[-0.046, 0.005]	0.122
Average age at first marriage	0.275	0.071	0.231	[-0.382, 0.524]	0.759

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Outcomes are state-level shares (in percentage points) married before age 15, 16, 17, or 18, and the average age at first years in years, computed with DHS person weights. We restrict our sample to women aged 15-24 years. Estimates use the staggered DiD developed by Callaway and Sant'Anna (2021) with the DR-IPW method, clustering at the state level. Controls are not-yet-treated ever-adopter states. Never-adopters are excluded from the analysis. Covariates are fixed to the most recent pre-enacement DHS round for each state including percentage woman living in rural area, women's household wealth quantile shares (Q2-Q5), and percentage of women that are muslin. Column (1) reports the p-value from the joint pre-trend hypothesis test of null hypothesis as all pretreatment ATTGT's are statistically equal to zero. Column (2)-(5) report the simple average of all non-negative event times as the summary of the overall ATT, its standard error, 95% confidence interval, and p-value, respectively. Unless noted otherwise, standard errors are asymptotic and clustered by states.

6.1.2 Re-estimate for women aged 15–34

We also expand our sample to include women aged 15 to 34 years to test whether the CRA effect is sensitive to a broader definition of the exposure. As shown in Table 6, the estimated coefficients are similar in direction to our main results, and in some cases, they are even slightly larger in absolute value. The share of women married before the age of 18 falls by 2.2 percentage points, and the result is statistically significant at 1% level, and the pre-trend test also shows that the parallel trend assumption is satisfied with a p-value of 0.472. The estimates for stricter cut-offs (married before age 17, 16, and 15) are also negative and statistically significant at 1%, 1%, and 5%, respectively. The average age at first marriage shows a smaller and less precise increase. Including older women, by expanding our age cut-off from 29 to 34 years old, adds statistical power to our analysis but may incorporate some marriages that occurred before CRA enactment. Nevertheless, the results confirm that our main conclusion is robust, and states that enacted CRA experienced measurable declines in early marriage rates, and the law's influence persists even under a broader age-group definition. We include the staggered DiD figures in Appendix A, Figure A4 and Figure A5.

Table 6: Pre-trend Tests and Aggregated ATT at Event Time (Women Sample Aged 15-34)

Outcome	Pre-trend P-value	ATT (event time) Coefficient	SE	95% CI	P-value
	(1)	(2)	(3)	(4)	(5)
% married before 18	0.472	-0.022	0.007	[-0.035, -0.009]	0.001
% married before 17	0.000	-0.031	0.007	[-0.046, -0.017]	0.000
% married before 16	0.000	-0.022	0.006	[-0.034, -0.010]	0.000
% married before 15	0.000	-0.014	0.006	[-0.026, -0.002]	0.023
Average age at first marriage	0.008	0.083	0.054	[-0.022, 0.189]	0.123

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Outcomes are state-level shares (in percentage points) married before age 15, 16, 17, or 18, and the average age at first years in years, computed with DHS person weights. We restricted our sample to women aged 15-34 years. Estimates use the staggered DiD developed by Callaway and Sant'Anna (2021) with the DR-IPW method, clustering at the state level. Controls are not-yet-treated ever-adopter states. Never-adopters are excluded from the analysis. Covariates are fixed to the most recent pre-enacement DHS round for each state including percentage woman living in rural area, women's household wealth quantile shares (Q2-Q5), and percentage of women that are muslin. Column (1) reports the p-value from the joint pre-trend hypothesis test of null hypothesis as all pretreatment ATTGT's are statistically equal to zero. Column (2)-(5) report the simple average of all non-negative event times as the summary of the overall ATT, its standard error, 95% confidence interval, and p-value, respectively. Unless noted otherwise, standard errors are asymptotic and clustered by states.

6.2 Raw enactment year

As an additional robustness check, we replace the DHS-grid cohort timing with the state's actual CRA enactment years to ensure that our results are not driven by the cohort regrouping procedure that we had done earlier. Using the raw enactment year aligns the timing of the treatment precisely with when the law was passed in each state, rather than the nearest DHS survey wave. Results in Table 7 show that the overall pattern remains qualitatively similar to our main specification, though the magnitude and precision of the estimates are reduced. The share of women married before age 18 continues to fall after the CRA enactment, but the coefficient becomes smaller and statistically insignificant, while stricter cutoffs (before age 17, 16, and 15) remain negative and statistically significant at 1% level. The effect on the average age at first marriage is still positive, but less precisely estimated.

Importantly, the pre-trend appears to be more stable under the raw enactment specification, indicating that aligning treatment timing to the actual enactment year improves the credibility of the parallel-trend assumption. By using the actual year of enactment, we capture more accurately the point when the CRA would begin to affect behavior. This helps us avoid the grouping that happens when states with different enactment years are forced into the same DHS survey round. As a result, the pre-treatment patterns are smoother and closer to zero, giving us more confidence that the parallel-trend assumption is satisfied after controlling for our selected covariates. However, the statistical precision of the post-treatment estimates decreases slightly, primarily because the number of not-yet-treated ever-adopter comparison states declines quickly after 2010 as more states adopt the CRA. This limits overlap in certain years and increases estimation noise, especially for later event times. Re-grouping the cohorts as we do for our main results helps reduce the estimation noise and strengthen our identification for the reasons we mentioned in Section 4.2: better overlap, cleaner comparison, and more stable inference we can have.

Overall, the results based on the actual enactment year reinforce the robustness of our main conclusion that the CRA contributes to a reduction in early marriage and an increase in the average age at first marriage at the state level.

Table 7: Pretrend Tests and Aggregated ATT at Event Time (Raw Enactment Year)

	Pre-trend	ATT (event time)			
Outcome	P-value	Coefficient	SE	95% CI	P-value
	(1)	(2)	(3)	(4)	(5)
% married before 18	0.939	-0.005	0.009	[-0.022, 0.012]	0.573
% married before 17	0.999	-0.026	0.006	[-0.038, -0.013]	0.000
% married before 16	0.995	-0.027	0.007	[-0.042, -0.013]	0.000
% married before 15	0.997	-0.032	0.009	[-0.049, -0.015]	0.000
Average age at first marriage	0.766	0.037	0.050	[-0.062, 0.136]	0.462

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Outcomes are state-level shares (in percentage points) married before age 15, 16, 17, or 18, and the average age at first years in years, computed with DHS person weights. We restrict our sample to women aged 15-29 years. Estimates use the staggered DiD developed by Callaway and Sant'Anna (2021) with the DR-IPW method, clustering at the state level. Controls are not-yet-treated ever-adopter states.

Never-adopters are excluded from the analysis. Covariates are fixed to the most recent pre-enacement DHS round for each state including percentage woman living in rural area, women's household wealth quantile shares (Q2-Q5), and percentage of women that are muslin. Column (1) reports the p-value from the joint pre-trend hypothesis test of null hypothesis as all pretreatment ATTGT's are statistically equal to zero. Column (2)-(5) report the simple average of all non-negative event times as the summary of the overall ATT, its standard error, 95% confidence interval, and p-value, respectively. Unless noted otherwise, standard errors are asymptotic and clustered by states.

6.3 Event-time support trimming

A well-known requirement in staggered DiD design is that the ATT at each time must be supported by multiple treatment cohorts and a non-empty set of not-yet-treated comparison group (Baker et al. (2025), CS2021). In our setting, event-time support is jointly determined by the DHS-grid cohort construction in Section 4.2 and the timing of the CRA enactment across states. As shown in Table 3 and the full mapping in Appendix Table A5, the outer tails of the event-time distribution rely very thin support and in some cases lack valid not-yet-treated adopters comparison groups. To test the robustness of our results using event-time $e \in [-9, 9]$, we can restrict the event time to $e \in [-5, 5]$ and see if the pre-trend test and aggregated ATT at event time results still hold. Within $e \in [-5, 5]$, we have three cohorts g^* that contribute overlapping observations (Table 3).

We re-estimate the aggregated ATT using the trimmed event-time window $e \in [-5, 5]$. Results in Table 8 show that the main patterns are robust. The CRA continues to reduce the share of women married before ages 18, 17, 16, and 15, and increases the mean age at first marriage. The magnitudes are slightly smaller for the extensive-margin outcomes, but remain statistically significant and consistent with the main specification. Importantly, trimming improves precision for several outcomes, especially the average age at first marriage, which increased by 0.158 years within the trimmed window with a p-value of 0.007 (as compared to the main result's 0.121 years with a p-value of 0.031).

Table 8: Pretrend Tests and Aggregated ATT at Event Time (Trim to $e \in [-5, 5]$)

Outcome	Pre-trend P-value (1)	ATT (event time) Coefficient (2)	SE (3)	95% CI (4)	P-value (5)
% married before 18	0.481	-0.019	0.007	[-0.033, -0.005]	0.010
% married before 17	0.004	-0.028	0.010	[-0.049, -0.008]	0.005
% married before 16	0.001	-0.025	0.011	[-0.046, -0.004]	0.020
% married before 15	0.000	-0.019	0.010	[-0.038, 0.000]	0.051
Average age at first marriage	0.077	0.158	0.058	[0.044, 0.273]	0.007

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).
Note: Outcomes are state-level shares (in percentage points) married before age 15, 16, 17, or 18, and the average age at first years in years, computed with DHS person weights. We restrict our sample to women aged 15-29 years. Estimates use the staggered DiD developed by Callaway and Sant'Anna (2021) with the DR-IPW method, clustering at the state level. Controls are not-yet-treated ever-adopter states. Never-adopters are excluded from the analysis. Covariates are fixed to the most recent pre-enacement DHS round for each state including percentage woman living in rural area, women's household wealth quantile shares (Q2-Q5), and percentage of women that are muslin. Column (1) reports the p-value from the joint pre-trend hypothesis test of null hypothesis as all pretreatment ATTGT's are statistically equal to zero. Column (2)-(5) report the simple average of all non-negative event times as the summary of the overall ATT, its standard error, 95% confidence interval, and p-value, respectively. Unless noted otherwise, standard errors are asymptotic and clustered by states. All aggregation used even time from -5 to 5.

Overall, the trimmed results reinforce the conclusion that the CRA contributed to a decline in early marriage. By focusing on the interval where cohorts overlap strongest, the trimmed estimates rely on higher-quality identifying variation and therefore, provide us with a complementary validation of our main findings.

7 Discussion and Conclusion

This paper shows that the CRA contributes to a gradual decline in early marriage in Nigeria, although the size and timing of the effects strongly suggest that passing a law is only part of the story. The reduction we find in marriage before the age of 18 is a small but meaningful increase, which points to the CRA having a real influence on marriage decisions. However, these changes unfold slowly, and the early post-enactment years look very different from the long-run pattern. In almost all the results, the effect grows over time. This suggests that the CRA did not immediately reshape behavior. Rather, the law seems to have taken root gradually as awareness spread and enforcement improved.

The staggered rollout across states helps explain the dynamic of the impacts. As the CRA Tracker data show (Table A2, A3), southern and central states were quicker to adopt the law, while many northern states waited years or decades before enacting it. These differences are not purely random. Late-adopter states tend to be more rural, poorer, and have larger Muslim populations, and all of these factors are closely tied to both marriage norms and political willingness to formalize a minimum marriage age. This uneven rollout creates a setting where the same law interacts with very different social and institutional environments. The fact that we detect an overall decline despite this heterogeneity underscores that the CRA has gained some ground, though its influence is arguably shaped by broader structural constraints.

Another important point is that the sizes of the effects of the CRA on child marriage are modest. A 3.5 percent reduction in child marriage represents some progress, but it is far from transformative. Our results are consistent with evidence from other countries: minimum marriage age laws rarely eliminate early marriage on their own. Families respond not only to legal rules but also to economic conditions, social expectations, and the perceived benefits of delaying marriage. In regions where early marriage remains tied to economic vulnerability or long-established cultural practices, legislation without strong enforcement or complementary interventions tends to move behavior only at the margin. Importantly, our estimates speak to contexts where the CRA was domesticated and therefore plausibly implemented to some degree. Within adopting states and among younger cohorts (ages 15–29), we find improvements in marriage outcomes rather than deterioration. This pattern suggests that, conditional on adoption, the key policy constraint may lie less in immediate behavioral backlash and more in the broader gap between legal domestication and effective implementation capacity.

These findings also speak to the emerging debate on "policy backlash." While theoretical work emphasizes that progressive legal reforms can provoke counter-reactions in conservative settings (Acemoglu and Jackson, 2017), our results provide little support for backlash within the set of adopting states we study. By design, we do not estimate effects in never-adopting states, where political and social opposition to domestication may be strongest. Taken together with evidence from broader samples that reports opposite-signed average effects (Vergili, 2025), one interpretation is that estimates may differ because they weight fundamentally different policy environments (adopting vs. non-adopting settings) and cohorts. A useful direction for future work is to explicitly reconcile these approaches on harmonized samples and to examine heterogeneity by baseline proxies for local norms.

The fact that the CRA effects strengthen over time and remain robust is encouraging. The law appears to work as a foundation that might gradually shift some expectations, whether through school continuation, parental decisions, or maybe the willingness of local authorities to intervene in underage unions. Overall, the results suggest a hopeful picture of the decline in child marriage in Nigeria. One of the contributions that the CRA makes might be that it sets a precedent for other initiatives and laws that could enhance women's well-being.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the authors used ChatGPT (OpenAI) in order to assist with language refinement and improve readability of the manuscript. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

Declaration of competing interests

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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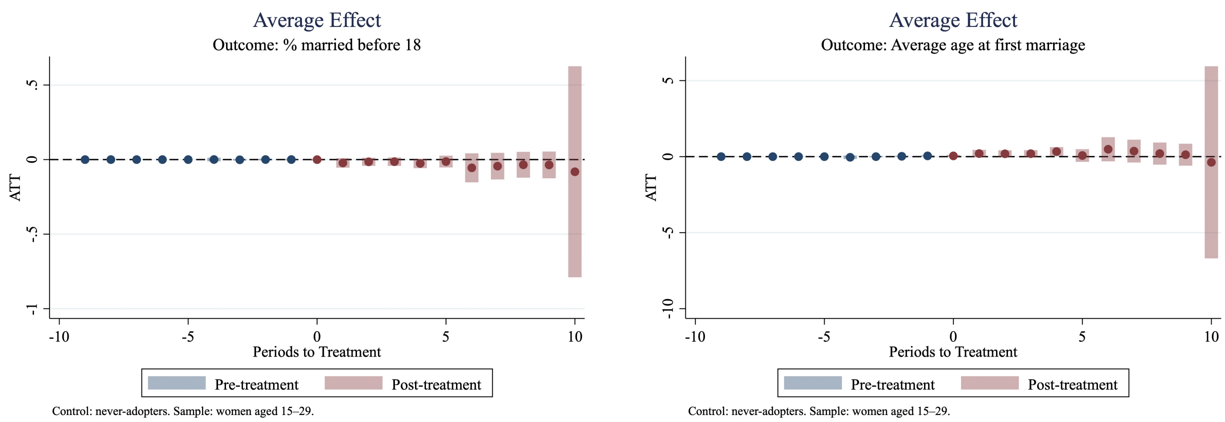
Appendix

Appendix A Figures

Figure A1: ATT using Never-Adopting States as Comparison Group

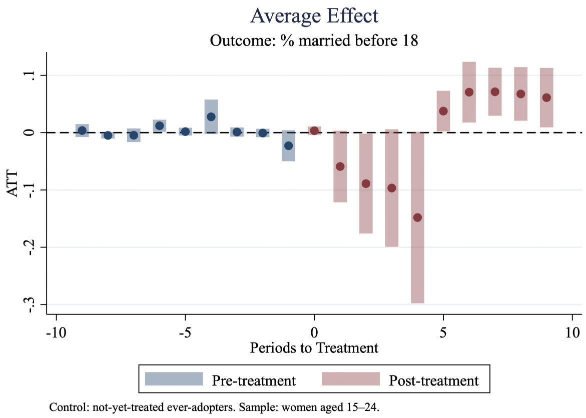


(a) % Married Under Age 18

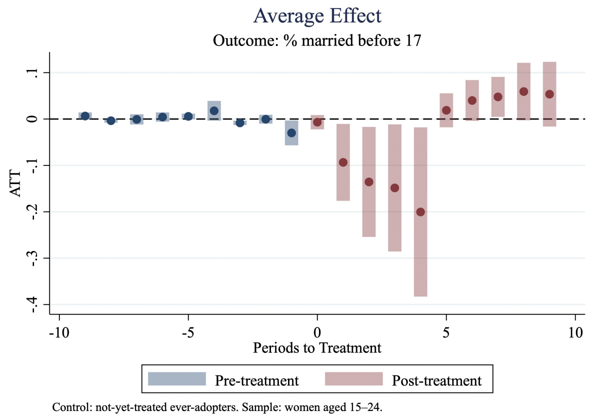
(b) ATT of Mean Age at First Marriage

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

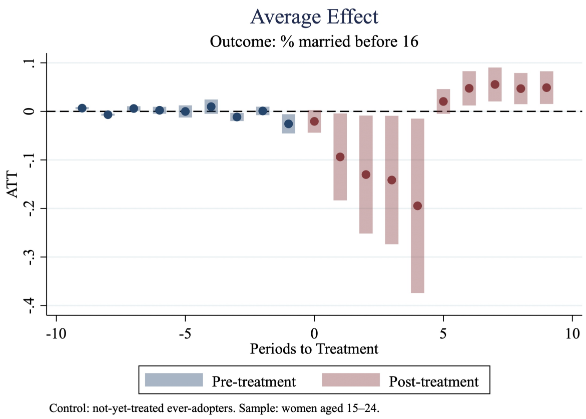
Figure A2: ATT of % Married Under Certain Age Using Women Sample Aged 15-24



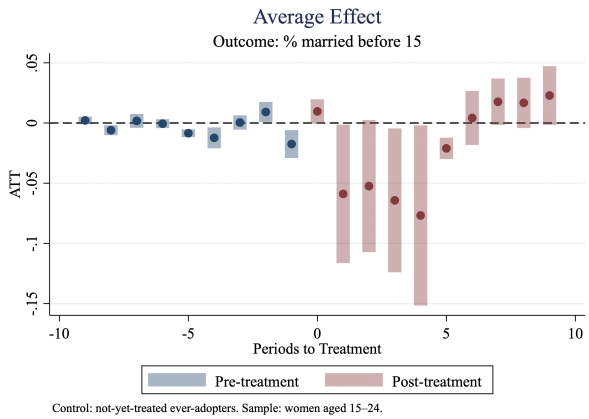
(a) % Married Under Age 18



(b) % Married Under Age 17



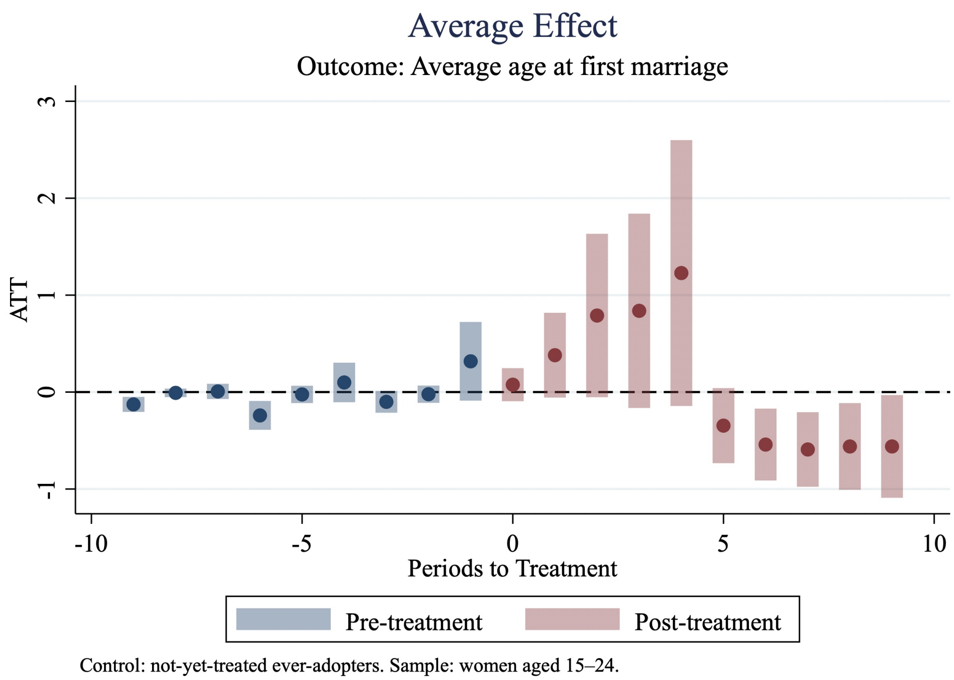
(c) % Married Under Age 17



(d) % Married Under Age 15

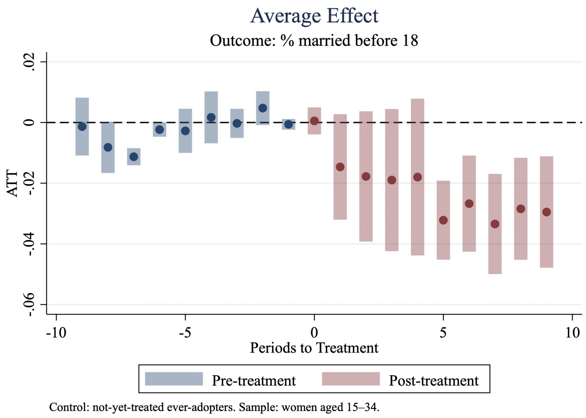
Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Figure A3: ATT of Mean Age at First Marriage Using Women Sample Aged 15-24

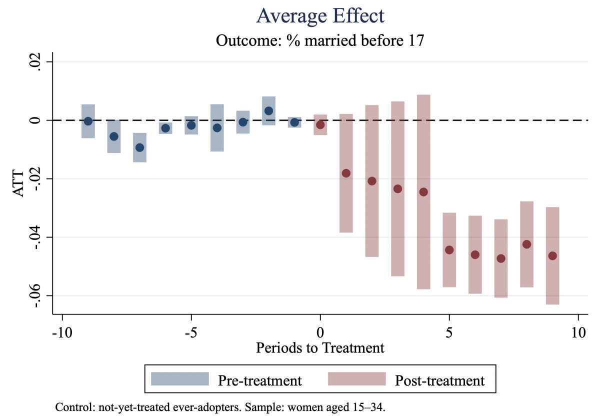


Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

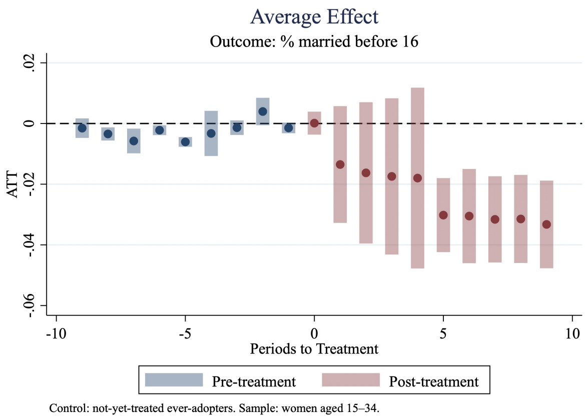
Figure A4: ATT of % Married Under Certain Age Using Women Sample Aged 15-34



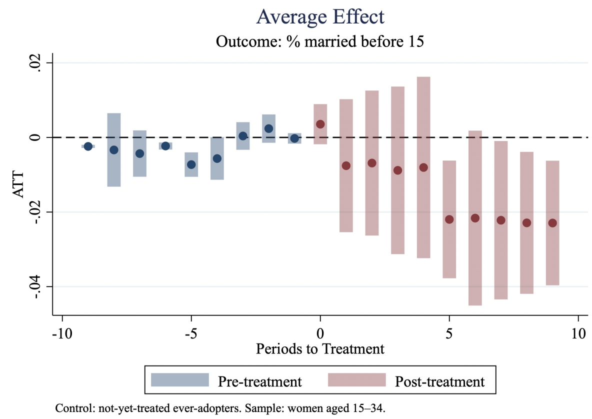
(a) % Married Under Age 18



(b) % Married Under Age 17



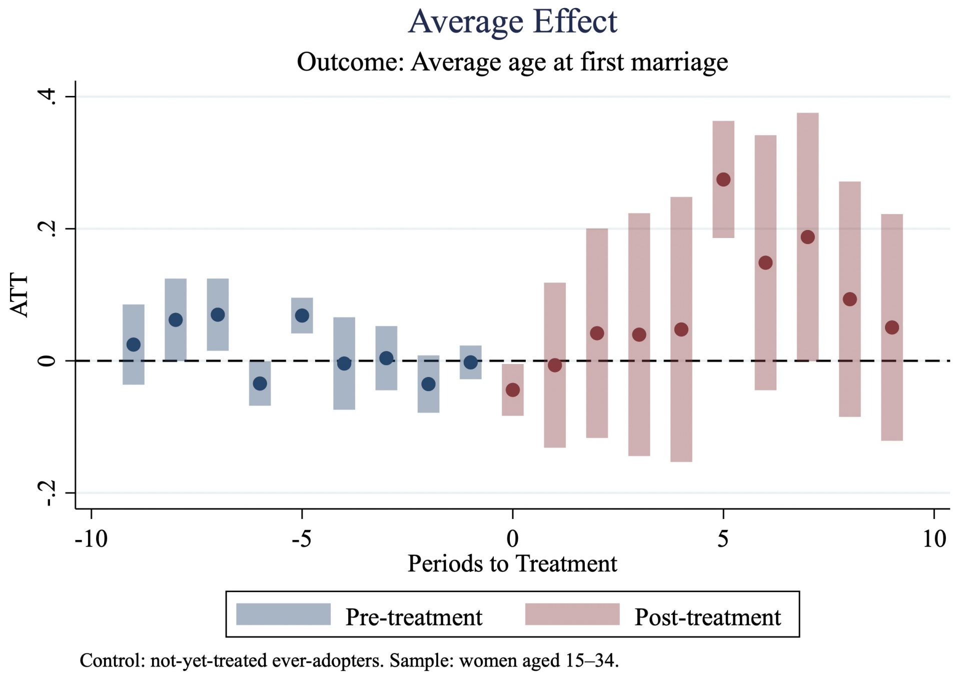
(c) % Married Under Age 17



(d) % Married Under Age 15

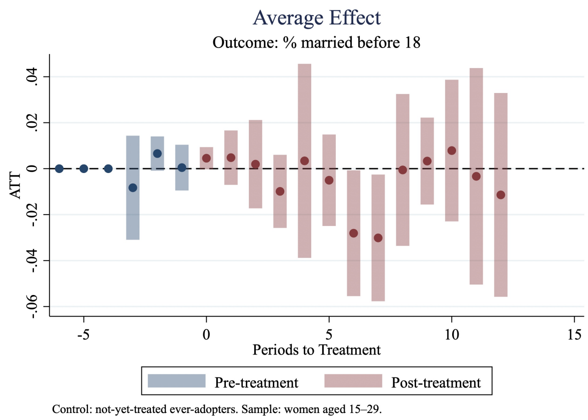
Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Figure A5: ATT of Mean Age at First Marriage Using Women Sample Aged 15-34

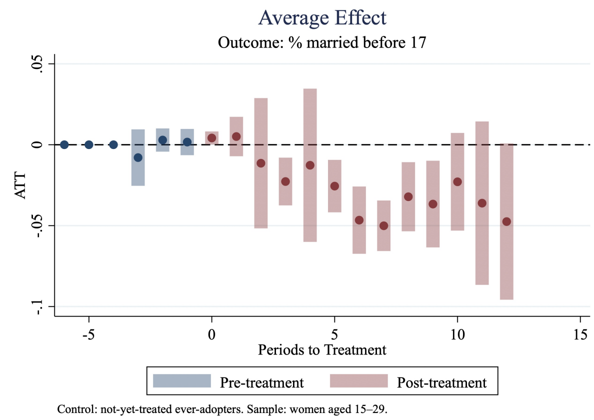


Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

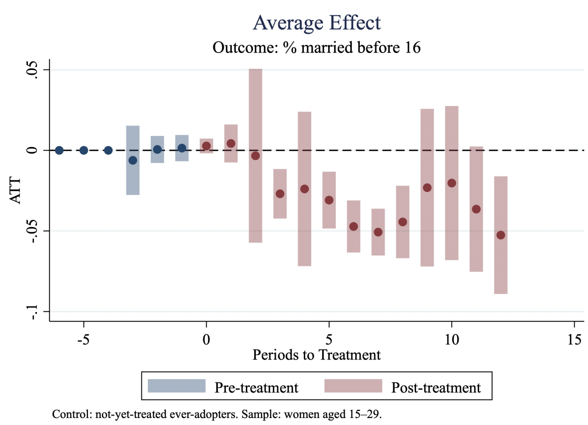
Figure A6: ATT of % Married Under Certain Age Using Actual Enactment Year (Without Re-Grouping Cohorts)



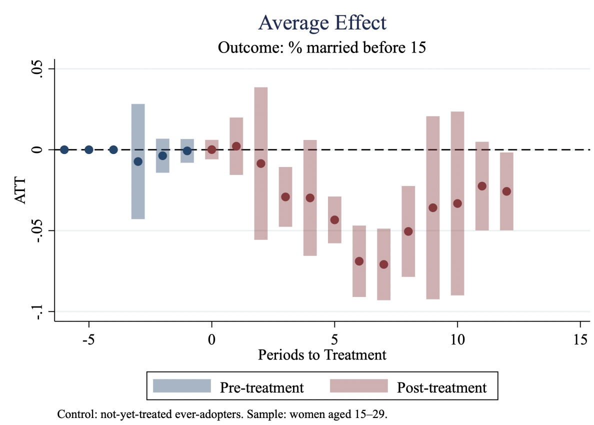
(a) % Married Under Age 18



(b) % Married Under Age 17



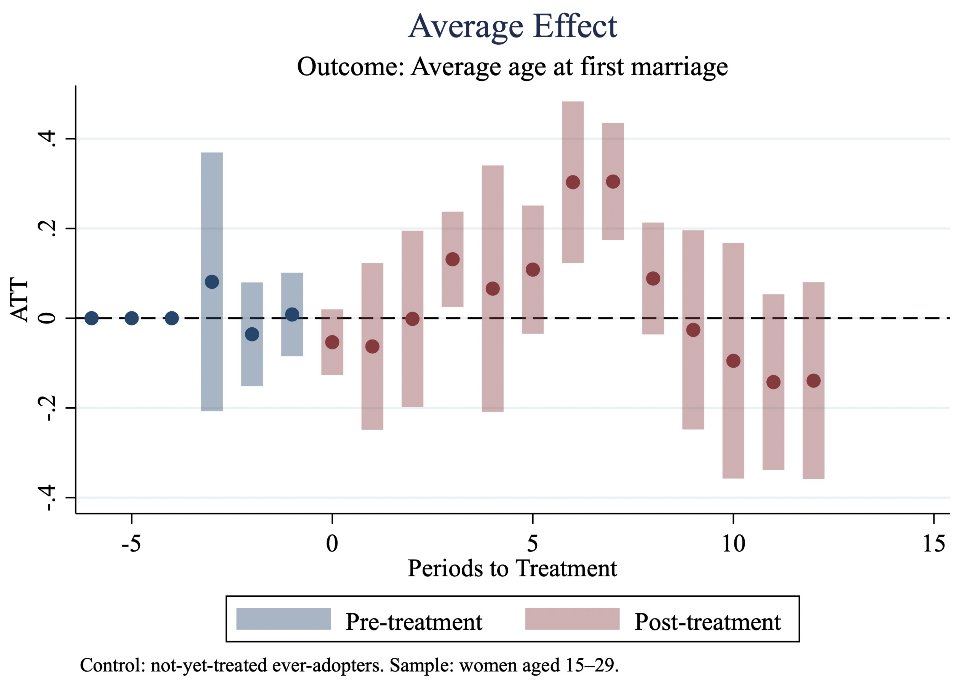
(c) % Married Under Age 17



(d) % Married Under Age 15

Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Figure A7: ATT of Mean Age at First Marriage Using Actual Enactment Year (Without Re-Grouping Cohorts)



Source: Authors' calculations from Nigeria DHS (2003, 2008, 2013, 2018).

Appendix B Tables

Table A1: List of Abbreviations

Abbreviation	Definition
CRA	Child's Right Act
DHS	Demographic and Health Survey
FCT	Federal Capital Territory
SW	South West
SE	South East
SS	South South
NC	North Central
NW	North West
NE	North East
DiD	Difference-in-Differences

Table A2: Child Rights Act Tracker

STATE	Zone	CRA Enactment Year
Fct Abuja	NC	2003
Anambra	SE	2004
Imo	SE	2004
Kwara	NC	2005
Nasarawa	NC	2005
Plateau	NC	2005
Taraba	NE	2005
Abia	SE	2006
Ekiti	SW	2006
Ogun	SW	2006
Oyo	SW	2006
Kogi	NC	2007
Edo	SS	2007
Lagos	SW	2007
Ondo	SW	2007
Osun	SW	2007
Benue	NC	2008
Akwa Ibom	SS	2008
Cross River	SS	2008
Delta	SS	2008
Rivers	SS	2009
Niger	NC	2010
Ebonyi	SE	2010
Enugu	SE	2016
Bayelsa	SS	2016
Kaduna	NW	2018
Jigawa	NW	2021
Katsina	NW	2021
Sokoto	NW	2021
Adamawa	NE	2022
Borno	NE	2022
Yobe	NE	2022
Kebbi	NW	2022
Zamfara	NW	2022
Bauchi	NE	2023
Kano	NW	2023
Gombe	NE	Not enacted as of 2025

Source: Partners West Africa Nigeria (2025).

Note: Zones include South East (SE), South South (SS), South West (SW), North Central (NC), North West (NW), and North East (NE).

Table A3: CRA Enforcement Summary

Enacted During which Year	Total Number of State	Geopolitical Zones					
		SW	SE	SS	NC	NW	NE
2003	1	0	0	1	0	0	0
(2003, 2008]	19	6	3	4	5	0	1
(2008, 2013]	3	0	1	1	1	0	0
(2013, 2018]	3	0	1	1	1	0	0
(2018, 2025]	10	0	0	0	6	4	0
Total	33	6	5	7	7	6	5

Source: Partners West Africa Nigeria (2025)

Note: There are in total 36 states + 1 federal capital territory. The only one state, Gombe, that has not enacted CRA is located in north east region as of 2025. SW, SE, SS, NC, NW, and NE stands for South West, South East, South South, North Central, North West, and North East, respectively.

Table A4: Adoption Status

State Group	DHS-grid cohort (g^*)	Enactment Year (t)
Early adopters	2008	Treated from 2004 onward
Middle adopters	2013	Treated from 2008 onward
Late adopters	2018	Treated from 2013 onward
Never adopters	0 (never enacted)	Never enact or enact after 2018

Note: The DHS-grid cohort g^* is defined as the first DHS survey wave at or after a state's CRA enactment year (2004–2008 $\rightarrow$ 2008; 2009–2013 $\rightarrow$ 2013; 2014–2018 $\rightarrow$ 2018). "Never adopters" are states that did not enact by 2018 or enacted after 2018, and therefore have $g^* = 0$. In each calendar year t, the comparison group under the ever-adopters design consists exclusively of not-yet-treated ever-adopting states, i.e., states with $g^* > t$. States that never enact by 2018 (with $g^* = 0$) are excluded from the analysis. As time progresses and more ever-adopters receive treatment, the comparison pool shrinks. By 2013, only states in the 2018 cohort remain not yet treated, and after 2018 no ever-adopters remain untreated. This regrouping improves overlap across event times and reduces noise in the dynamic estimates.

Table A5: Mapping calendar years t into event time $e = t - g_s^*$ by DHS-grid cohort

	$e = t - g_s^*$ by cohort			
Calendar year t	$g_s^* = 2003$	$g_s^* = 2008$	$g_s^* = 2013$	$g_s^* = 2018$
2003	0	−5	−10	−15
2004	1	−4	−9	−14
2005	2	−3	−8	−13
2006	3	−2	−7	−12
2007	4	−1	−6	−11
2008	5	0	−5	−10
2009	6	1	−4	−9
2010	7	2	−3	−8
2011	8	3	−2	−7
2012	9	4	−1	−6
2013	10	5	0	−5
2014	11	6	1	−4
2015	12	7	2	−3
2016	13	8	3	−2
2017	14	9	4	−1
2018	15	10	5	0

Notes: This table shows how we construct event time $e = t - g_s^*$ used in the staggered difference-in-differences design. Each row corresponds to a calendar year $t \in \{2003, \ldots, 2018\}$, and each column corresponds to a DHS-grid adoption cohort $g_s^* \in \{2003, 2008, 2013, 2018\}$, defined as the first DHS survey wave at or after a state's CRA enactment year. Negative values of e indicate pre-treatment periods (leads), positive values indicate post-treatment periods (lags), and $e = 0$ is the first treated year for that cohort on the DHS grid. For example, in 2010 a state with $g_s^* = 2008$ is at event time $e = 2$ (two years after enactment), whereas a state with $g_s^* = 2013$ is at $e = -3$ (three years before enactment). The 2003 cohort (FCT) is treated throughout our observation window and therefore does not contribute to pre-treatment periods for lead diagnostics, and its event-time values are included here only to illustrate the mapping.

Table A6: Baseline Gaps Between Ever-Adopters vs. Never-Adopters

Variable	Ever adopters (1)	Never adopters (2)	Norm. Diff (3)
Lives in rural	0.81	0.64	1.26
Household wealth index quintiles			
Poorest	0.39	0.15	1.63
Poorer	0.31	0.19	1.19
Middle	0.17	0.24	-0.96
Richer	0.10	0.22	-1.66
Richest	0.04	0.20	-1.30
Religion			
Muslim	0.95	0.40	2.14
Catholic & other religions	0.00	0.16	-1.18

Source: Nigeria DHS (2003, 2008, 2013, 2018).

Note: This table reports the averages and standardized differences of each covariate comparing CRA ever adopters vs. never adopters. All variables are in percentages. All measures are weighted.

Appendix A Child Rights Act (2003)

Figure A8: CRA Section III, 21, 22

PART III — PROTECTION OF THE RIGHTS OF A CHILD

Prohibition of child marriage.

21. No person under the age of 18 years is capable of contracting a valid marriage, and accordingly, a marriage so contracted is null and void and of no effect whatsoever.

Prohibition of child betrothal.

22.—(1) No parent, guardian or any other person shall betroth a child to any person.

Source: (Child's Rights Act, 2003)

Figure A9: CRA Section III, 23

Punishment for child marriage and betrothal.

23. A person—

(*a*) who marries a child, or

(*b*) to whom a child is betrothed, or

(*c*) who promotes the marriage of a child, or

(*d*) who betroths a child,

commits an offence and is liable on conviction to a fine of ₦500,000 ; or imprisonment for a term of five years or to both such fine and imprisonment.

Source: (Child's Rights Act, 2003)

Summary of parts included in the Child Rights Act 2003:

- Part I: Best interests of a child to be of paramount consideration in all actions
- Part II: Rights and Responsibilities of a child
- Part III: Protection of the rights of a child
  - Use of children in other criminal activities
  - Child labor
  - Unlawful sexual intercourse, etc.
  - Other forms of sexual abuse and exploitation
  - Other forms of exploitation
  - Recruitment into the armed forces

  - Harmful publication
  - Miscellaneous

- Part IV: Protection of children
- Part V: Children in need of care and protection
- Part VI: Care and supervision
- Part VII: Provisions for use of scientific tests in determining paternity of maternity, etc.
- Part VIII: Possession and custody of children
- Part IX: Guardianship
- Part X: Wardship
- Part XI: Fostering
- Part XII: Adoption
- Part XIV: Child minding and day care of young children
- Part XV: State government support for children and families
- Part XVI: Community homes
- Part XVII: Voluntary homes and voluntary organizations
- Part XVIII: Registered children's homes
- Part XIX: Supervisory functions and responsibilities of the minister
- Part XX: Child justice administration
- Part XXI: Supervision
- Part XXII: Approved institutions and post-release supervision
- Part XXIII: The national, state, and local government child rights implementation committees
- Part XXIV: Miscellaneous