Counterintuitive Cash Transfers: Incentivizing Child Marriages in West Bengal, India

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November 21, 2024

Abstract

This paper examines the Kanyashree Prakalpa (KP), a Conditional Cash Transfer (CCT) program launched in West Bengal, India, in 2013, which ties benefits directly to marital age rather than relying solely on improving education. In West Bengal, a state with the highest rates of child marriage and early motherhood in India, the policy increased marriage likelihood by approximately 4-6 percentage points (10-18%) for girls aged 16 to 17 after four to six years of exposure. Similarly, early motherhood likelihood rose by around 4 percentage points (10-12%) for ages 16 to 18, particularly with extended policy exposure. Meanwhile, the program had limited impact on educational outcomes, achieving modest gains in middle grades but minimal changes in higher grades. This paper highlights how financial incentives targeting marital age, a novel approach distinct from education-focused CCTs, can be influenced by local practices like dowry. These findings underscore the limited role of education in delaying marriage in this context and demonstrate how deeply rooted social norms can lead to unintended consequences, even with well-intentioned policies.

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1 Introduction

Over the past decade, India has made substantial progress in reducing child marriages, with the proportion of girls married as children decreasing from one in four to one in five. Despite this national trend and various state-level interventions, West Bengal stands as a notable outlier, where child marriage rates have not only persisted but increased.[1][2]

Child marriage is more than a violation of human rights; it is associated with a range of adverse outcomes. Girls who marry young often experience reduced educational attainment, early childbearing, higher fertility rates, diminished autonomy, increased risk of domestic violence, lower human capital investment in future generations, and serious consequences for their reproductive and sexual health. These impacts extend beyond the individual, affecting the well-being and future prospects of entire families and communities.[3]

This paper investigates the impact of the Kanyashree Prakalpa (KP) Conditional Cash Transfer (CCT) program, one of India's largest social initiatives, launched in West Bengal in October 2013. KP provides an annual benefit of Rs 1,000 (US$17.07 at the 2013 exchange rate) to girls aged 13-18 who remain in school, alongside a lump sum of Rs 25,000 (US$427) if they stay unmarried until they reach 18.

Using both cohort and state variation, this study applies a difference-in-differences and event study framework based on the most recent Demographic and Health Survey (DHS) (2019–21).[4] The first difference exploits variation across birth cohorts: girls aged 13–18 in 2013 or later, who received one to six years of exposure to KP, are compared with girls who were too old to qualify for the program. The second difference compares girls residing in West Bengal (the treated state) with those in neighboring states, which serve as control Figure 1. Neighboring states are selected based on their socio-economic similarities to West Bengal, providing a counterfactual to isolate

[1] UNICEF, 2018, *25 million child marriages prevented in the last decade due to accelerated progress*, UNICEF Press Release, accessed 2024-09-30.

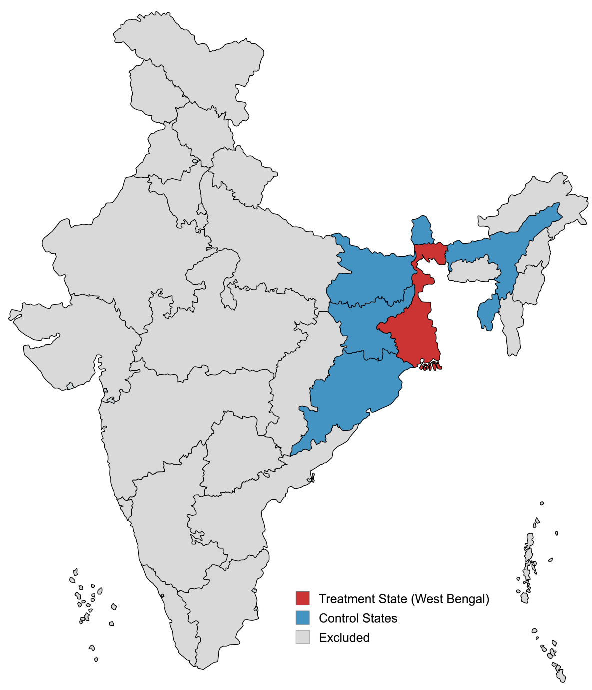
[2] UNICEF, 2019, *Child marriage: Latest trends and future prospects*, UNICEF India Profile, accessed 2024-09-30.

[3] Jensen and Thornton (2003), Field and Ambrus (2008), Breierova and Duflo (2004), Amin et al. (2016), Clark et al. (2006), Duflo and Topalova (2015), Chari et al. (2017), Asadullah and Wahhaj (2019), Goldin and Katz (2002)

[4] Also referred to as the National Family Health Survey (NFHS-5) 2019–21.

the policy's effects. This approach assumes that, in the absence of KP, education and marriage trends would have followed similar trajectories across these regions. Year fixed effects control for time-specific unobservables common across all states, while district fixed effects account for time-invariant characteristics that could influence education, marriage, and childbirth outcomes. The event study framework examines within-state changes across different years of policy exposure, testing for pre-existing trends and capturing the longer-term effects of KP after seven years of implementation.

Figure 1: Treated and Control States in India.



The map highlights West Bengal as the treated state (in red), and the control states (in blue) are Sikkim, Assam, Tripura, Bihar, Jharkhand, and Odisha.

The dependent variables in this study include total years of education, age at marriage, and age at first childbirth, alongside binary variables. The analysis focuses on middle school grades (7 to 9) and high school grades (10 to 12) for education, and the corresponding ages (13 to 18 years) to examine the impact of a CCT on educational progression, marital timing, and childbearing ages. This approach allows for identifying the specific stages at which the policy influences these outcomes.

A key identification challenge arises due to concurrent educational infrastructure improvements in West Bengal, which may be correlated with unobserved determinants of education and marriage outcomes, potentially confounding the policy's effects. Additionally, potential spillover effects are considered, as boys living in the same households and marriage markets may also be influenced by the policy. To assess this, a similar difference-in-differences analysis is conducted on a sample of boys, revealing no significant changes in their educational outcomes.[5]

I present three key results from the event study analysis. First, the implementation of KP increased the likelihood of passing middle school grades after three to five years of exposure to the policy, with gains ranging from 5% to 10%. However, these positive effects do not extend to higher grades, where the likelihood of passing remains mostly unchanged, with effects hovering around zero across all years of exposure. Importantly, the event study analysis confirms that the pre-policy parallel trends assumption holds, as pre-policy coefficients are close to zero and statistically insignificant across all grades.

Second, the likelihood of marriage increased significantly at ages 16 to 18 after four to six years of policy exposure. For example, marriage likelihood rose by 12% to 18% at age 16 and by 10% to 12% at age 17, while at age 18, there was an 8.8% increase after three-four years of exposure. These results suggest that the policy inadvertently accelerated marriage rates as girls approached the legal minimum age.Third, the likelihood of early motherhood also rose at ages 16 to 18, particularly after four to six years of exposure. At age 16, early motherhood likelihood increased by approximately 10%, while at age 17, it rose by 12%. By age 18, the increase was around 12% to 14%, reflecting a consistent trend with the marriage results.

Overall, these findings suggest that while KP modestly increased educational attainment up to middle school grades, these effects did not persist through high school. Consequently, as girls

[5] I conducted a difference-in-differences analysis for boys (see Appendix Figure A.16, Figure A.17, and Figure A.18) and found no significant changes in their educational outcomes.

transitioned out of school, they were more likely to marry and have children at younger ages. This pattern aligns with the timing of educational progression in this context, where grade 9 typically corresponds to age 15, highlighting a turning point where girls often left school to marry.

The rest of the paper is organized as follows. Section 2 presents the literature review. Section 3 discusses the context and details of the policy. Section 4 describes the data used in the paper. Section 5 explains the empirical strategy. Section 6 reports the main results. Section 7 presents the discussion and conclusion.

2 Literature Review

This paper contributes to the literature on CCTs by analyzing a program that uniquely ties benefits directly to marital age rather than solely through education, addressing the socio-economic nuances of dowry-practicing contexts in India.[6] CCTs, first popularized in Latin America through programs like Mexico's Prospera and Brazil's Bolsa Família, were primarily designed to improve education and health outcomes (Fiszbein and Schady, 2009; Biswas and Das, 2021; Baird et al., 2011, 2014; Dey and Ghoshal, 2021). These programs generally delayed marriage in non-dowry contexts (Ashraf, 2016). For instance, in Malawi, CCTs reduced school dropouts and successfully delayed marriage (Baird et al., 2011). However, in Mexico, Prospera increased both education and marriage rates, as rising household incomes enabled families to view marriage as a "normal good" (Figueiredo, 2024). In South Asia, where dowry norms often incentivize early marriage, the impact of CCTs has been mixed. Bangladesh's secondary school-focused CCTs delayed marriage by encouraging families to prioritize education (Hahn et al., 2015), while Pakistan's Female School Stipend Program produced similar results (Alam et al., 2011). In contrast, India's Apni Beti Apna Dhan program incentivized families to marry daughters soon after they turned 18 to secure the payout (Biswas and Das, 2021; Fors et al., 2023), with little effect on shifting attitudes toward child marriage (Nanda et al., 2016).

[6] Dowry refers to a transfer of goods or money from the bride's family to the groom's family, a practice common in South Asia. In contrast, bride price, a transfer from the groom's family to the bride's family, is more typical in African cultures.

This paper examines the relationship between education and marital timing, specifically exploring how a CCT program impacts both. While education is often assumed to delay marriage by increasing knowledge, shifting preferences, and raising wages (Becker, 1973; Chiappori et al., 2009; Heath and Mobarak, 2015; Jensen, 2012), this relationship is not always straightforward. Under conditions such as marriage market imperfections, rigid social norms, and low labor market returns for women, the positive correlation between education and delayed marriage can break down (Biswas and Das, 2021; Buchmann et al., 2018; Lavy and Zablotsky, 2011). In such cases, education can paradoxically increase marriageability, as shown by Agarwal et al. (2022) in dowry-practicing contexts and by Figueiredo (2024) in Mexico, where rising incomes through education-focused programs made marriage more accessible. My findings align with these results. Other interventions aimed at delaying marriage, including education, legal measures, and financial incentives, reveal similar complexities. Vocational training with sex education in Uganda delayed marriage and first pregnancies (Bandiera et al., 2020), while education subsidies and vouchers in Colombia and Zimbabwe increased school attendance and reduced marriage rates (Angrist et al., 2002; Hallfors et al., 2015). Legal measures, such as raising the minimum marriage age, have often failed due to weak enforcement (Bharadwaj and Lakdawala, 2015; McGavock, 2021). Cash transfers show varied outcomes: UCTs in Malawi delayed marriage (Baird et al., 2011), but had no measurable impact in Kenya (Handa, 2015). These examples underscore the need for tailored, context-specific policies that recognize how social norms and economic incentives mediate the relationship between education and marital timing—a relationship this study examines in depth.

This study offers two additional insights into the long-term impacts of KP. First, by employing grade-specific and age-specific analyses, it reveals nuanced program effects across different life stages, unbundling impacts often hidden in aggregated evaluations. Second, it examines KP's sustained impacts over seven years, specifically capturing two cohorts fully exposed to the policy from ages 13 to 18. Unlike prior evaluations of KP, which focused only on short-term effects within two years of implementation (Das and Sarkhel, 2020; Dutta and Sen, 2020; Dey and Ghoshal, 2021), this paper provides a deeper understanding of the program's effects on education, marriage, and motherhood timing.

3 Context and Policy

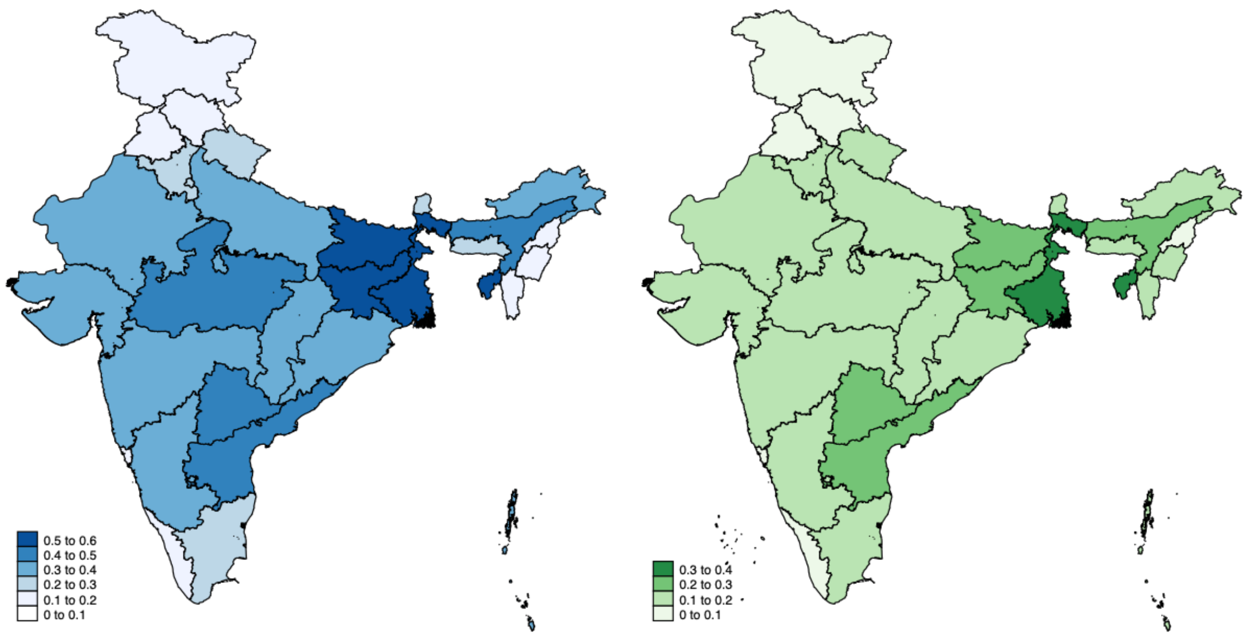
3.1 Context

West Bengal has the highest rate of child marriage and early motherhood in India. According to DHS-4, 55% of women aged 19-34 in West Bengal were married before turning 18 Figure 2). However, West Bengal presents a paradox: the state lacks many of the negative social indicators typically associated with high child marriage rates (Census of India, 2011; Kohli, 2012). In most of India, child marriage is associated with skewed sex ratios, low female literacy, and gender disparities, often driven by traditional beliefs favoring sons and supporting dowry systems. However, West Bengal does not follow this pattern. The state performs better than the national average in development indicators like sex ratio and female literacy, with girls averaging 7.72 years of schooling compared to 6.5 years in neighboring states (DHS-5 2019-21).

Despite these encouraging social indicators, child marriage and early motherhood remain prevalent. DHS-5 shows that the average age at marriage in West Bengal is 17.7 years, while the average age at first birth is 19.38 years. Both figures are lower than neighboring states' averages of 19.05 years for marriage and 20.77 years for first birth. Although trends of underage marriage and early motherhood have declined in both West Bengal and its neighboring states like Bihar, Jharkhand, and Odisha, where the practice also remains a persistent issue, the decline has been slower in West Bengal, with child marriage rates rising from 44% to 47% between DHS-4 (2015-16) and DHS-5, making it the highest in India.

A key feature of marriages in India is the dowry system, where payments are made from the bride's family to the groom's family. Although dowry is illegal, the practice remains common, especially in rural areas (Anderson, 2007; Gaurav and Weaver, 2023). In 2008, the national average net dowry payment was ₹ 35000 ($598), which represented around 14% of annual household

Figure 2: State-wise distribution of early marriage and motherhood in India.



The first panel shows the proportion of women aged 19–34 who were married by age 18, while the second panel shows the proportion of women aged 19–34 who had their first baby by age 18 using DHS 2019-21.

income (Anukriti, 2018; Rural Economic & Demographic Survey, 2006). In contrast, West Bengal had significantly lower dowry payments, averaging ₹ 14,000 ($239) by 2008. Additionally, there is a positive correlation between a bride's years of schooling and dowry payments, which could be due to educational mismatches between brides and grooms or societal perceptions of older brides being less desirable in the marriage market (Anukriti, 2018).

According to the Child Marriage Restraint Law (1929), later revised as the Prohibition of Child Marriage Act 2006, the minimum legal age for marriage for females is 18 years and for male is 21 years. [7] However, enforcement of these laws has been inconsistent, especially in West Bengal. According to a founder of a non-governmental organization that works to prevent and annul child marriages, [8] families often conduct child marriages in secret, holding ceremonies late at night with only a few attendees to avoid detection. They note that after the marriage, underage brides remain

[7] Parents who are caught marrying their underage children will be charged a fine of 100,000 Indian rupees ($1975 approx.) and two years in prison.

[8] Taken from the NPR article, *UNICEF's Good News About Child Marriage Isn't Quite As Good As It Sounds*: https://www.npr.org/sections/goatsandsoda/2018/03/14/593155781/unicefs-good-news-about-child-marriage-isnt-quite-as-good-as-it-sounds

with their parents until puberty, allowing families to claim no marriage has taken place if authorities investigate. Since there is often no photographic evidence or formal registration, prosecutions are rare. In states with high child marriage rates, like Rajasthan, they have found that families have become adept at evading the law. As long as the marriage is kept quiet, authorities rarely intervene, especially where child marriage is cultural norm.

3.2 The Kanyashree Prakalpa Policy

In 2011, a significant program for adolescent girls, the Kanyashree Prakalpa (KP) scheme, was announced shortly after the election of the state's first female Chief Minister, Mamata Banerjee. The first school cohort exposed to the KP was in 2013-14. Since its inception, approximately 7.6 million girls have benefited from the scheme, which also received international recognition, winning a United Nations Public Service Award, with projects from 52 countries nominated.

KP targets girls aged 13-18 years from families with an annual household income of less than Rs 120,000. [9] The CCT includes two components: firstly, an annual cash transfer of Rs 1000 (US$ 17.07 at the 2013 exchange rate) to cover school expenses for girls in eighth grade or above. Secondly, a lump sum of Rs 25,000 (US$ 427) is awarded when the girl turns 18, provided she is unmarried. All benefits are transferred directly to bank accounts in the names of the enrolled students. Unlike other state schemes such as Apni Beti Apna Dhan in Haryana, Ladli Lakshmi Yojana in Madhya Pradesh, and a child protection scheme in Uttarakhand, which transfer money into saving bonds redeemable only when the daughter turns 18 and remains unmarried, KP allows beneficiaries to access the annual scholarship immediately rather than freezing it until they turn 18. Additionally, while CCTs in other states are often initiated at birth to address the skewed child sex ratio, KP specifically focuses on educational and marital outcomes.The program's design seemed well-targeted at girls in the right age group, as the mean years of schooling for girls before the program was 7.72 (DHS, 2019). This suggests that many girls were dropping out in middle

[9] Annual income of ₹ 120,000 is $4.03 per day. According to both Census 2011 and National sample survey (NSS) Household Consumption Survey 68th round 2011-12, about 93% of household satisfied this condition this income ceiling condition was removed in 2018.

school. Therefore, KP had the potential to encourage girls to remain in school longer. It should be noted that in 2011, the new Chief Minister also significantly increased the education budget from approximately Rs 14,000 crore to Rs 37,000 crore (US$ 1700 million to US$ 4500 million), which was invested in enhancing school infrastructure alongside the KP program.

The verification process for accessing KP benefits differed between the annual scholarship and the lump sum transfer. For the annual scholarship, girls had to provide a set of documents, including a birth certificate, a parent's voter ID, and bank account details. These documents were then approved by their school. In contrast, accessing the lump sum at age 18 required only a declaration of marital status made by the parent or guardian. While the program recommended a sample physical verification for 5% of applicants by district authorities, there was no robust system to ensure that girls receiving the lump sum were genuinely unmarried. This discrepancy in verification processes may leave room for exploitation, as families could falsely declare a girl's marital status and claim the lump sum while having already arranged her marriage.

4 Data and Descriptive Statistics

This study utilizes data from the Demographic and Health Survey (DHS) or National Family Health Survey (NFHS-5), conducted from 2019 to 2021 by the Ministry of Health and Family Welfare.[10] DHS-5 is one of India's largest demographic and health surveys, offering district-level representation and comprehensive socio-economic and health data. Two key DHS-5 schedules provide the foundation for this analysis. The household schedule collects information on all usual members of the household, including socio-economic characteristics, educational attainment, marital status, water and sanitation access, and health insurance coverage. This data is used to analyze educational outcomes, including grade completion and total years of schooling. The women's schedule focuses on women aged 15–49, gathering detailed data on marriage, fertility, contraception, reproductive health, children's immunization, and healthcare access. It includes

[10]The data collection spanned over three years, with different states surveyed at different times. West Bengal, Assam, Sikkim, and Tripura were interviewed in 2019, Bihar in 2019–20, and Jharkhand and Odisha in 2020–21.

timing of marriage and age at first childbirth, which are central to this study. While the household schedule provides a broader sample, the women's schedule enables a focused analysis of marital and childbirth outcomes.

The sample focuses on females aged 19–34 at the time of the survey, capturing cohorts with varying degrees of exposure to the Kanyashree Prakalpa (KP) policy. This includes six post-policy cohorts in West Bengal, with full exposure experienced by girls aged 12 or 13 at the time of policy implementation and 18 or 19 during the survey. These cohorts allow the study to examine how prolonged exposure to KP affected educational attainment and early marriage and childbirth outcomes.

To serve as control states, the study includes Bihar, Jharkhand, Odisha, Assam, Sikkim, and Tripura. These neighboring states are chosen for their socio-economic similarities to West Bengal, following a precedent set by Nanda et al. (2016). However, this approach has potential drawbacks, as families from bordering states might relocate to West Bengal to benefit from the program, raising concerns about spillover effects. [11] Despite these limitations, the inclusion of neighboring states as controls provides an important counterfactual to isolate the policy's effects.

The primary outcomes analyzed include educational attainment, timing of marriage, and age at first childbirth. Binary variables are constructed to capture grade completion (grades 7 through 12) and demographic milestones for marriage and first childbirth (ages 13–18). Controls include socio-demographic variables such as religion, caste, wealth index, number of household members, the sex of the household head, rural residence, and language indicators for Hindi and Bengali speakers. By leveraging the DHS-5 data, this study evaluates policy effects on key educational and demographic outcomes, offering detailed insights into variations across cohorts and regions.

The summary statistics (Table A.1) reveal modest pre-policy educational advantages in the treated state, with an average of 8 years of schooling compared to 7 years in control states. Post-

[11] (Bell et al., 2015) estimated that India's interstate migration rate is slightly above 1%, based on the 2011 Census data. In comparison, Mexico's interstate migration rate is approximately 3.6%, China's is 4.7%, and the United States' is nearly 10%. Additionally, the Economic Survey of India (2017) reports that the interstate migrant population was 60 million, with an average annual flow of 9 million between 2011 and 2016. These figures suggest that migration is more employment-driven than policy-driven, minimizing spillover effects in this study.

policy, the cumulative distribution function (CDF) of schooling (Figure A.10) shows improvements in educational attainment for both groups, with treated states experiencing larger gains in middle grades. For example, the proportion of girls completing Grade 8 increased by 20% in treated states versus 10% in control states, and Grade 10 completion rose by 15% compared to 5%. However, higher-grade progression (Grades 11 and 12) showed minimal differences, suggesting that educational gains were concentrated in middle grades. These trends provide initial indications of limited policy impacts on sustained higher education.

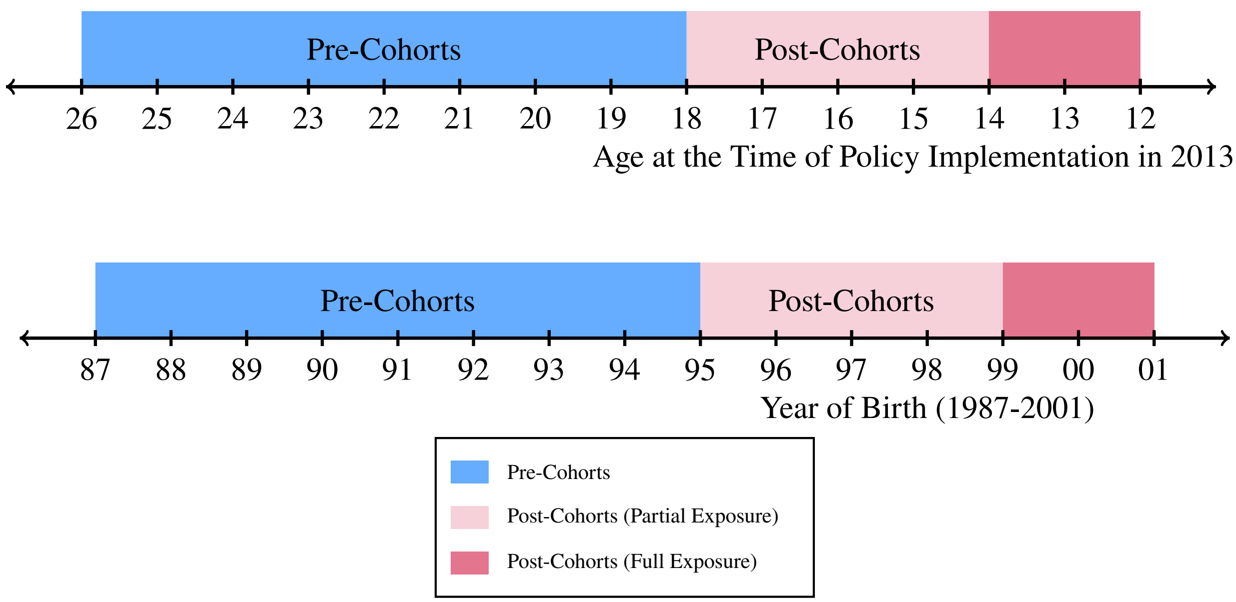
Early marriage trends highlight persistent challenges, with West Bengal showing higher rates than control states pre- and post-policy (Table A.4). While early marriage rates dropped from 59% to 49% in West Bengal and from 42% to 28% in control states, the reductions were smaller in the treated state. The CDF plots of marriage timing (Figure A.11) reveal that post-policy cohorts delayed marriage slightly but caught up with pre-policy cohorts by age 18, surpassing them by age 19. Similarly, first childbirth trends (Table A.7) show narrower reductions in West Bengal, with early childbirth rates decreasing by only 10 percentage points compared to 9 points in control states. The CDF of age at first childbirth (Figure A.12) reflects similar patterns, where post-policy cohorts delayed early childbirth slightly but caught up with and surpassed pre-policy cohorts just before age 18, indicating smaller policy impacts in the treated state compared to the control states.

5 Empirical Strategy

The empirical strategy leverages district-by-cohort variation to assess the impact of the KP on educational attainment, marriage timing, and motherhood. I exploit the exogenous variation introduced by the policy's implementation, which conditioned benefits on two dimensions: spatial variation between treated and control districts and cohort-based variation between eligible and ineligible girls defined by their age at the time of policy rollout. Treated districts are located within the state of West Bengal, where the policy was implemented. Girls residing in these districts were 18 years old or younger in 2013 when the policy began. Control districts are located in neighboring states where the policy was not implemented, and the control group includes (a) older girls in treated districts of West Bengal and (b) similarly aged girls in control districts of neighboring states.

The policy was introduced during the 2013–14 school year, making girls aged 18 or younger at the time eligible for its benefits. As shown in Figure 3, cohorts are categorized based on their age at implementation: Pre-Cohorts, born before 1995, were too old to be eligible for KP and serve as a natural control group; Post-Cohorts with Partial Exposure, born between 1995 and 1999, experienced varying degrees of exposure depending on their specific birth year; and Post-Cohorts with Full Exposure, born in 2000–01, were ideally positioned at ages 12–13 to benefit fully from the policy. This classification, aligned with the policy's timing, forms the basis of my identification strategy.

Figure 3: Policy Timing and Cohort Eligibility: Birth Years and Age at Implementation



The blue region represents the pre-cohort (born before 1996), the red region represents the post-cohort (born after 1996), and the darker red region indicates full exposure to the policy. Cohorts born between 1987-2001 are shown in the timeline, representing girls aged 19-34 during the survey year.

For outcomes such as total years of schooling, age at marriage, and age at first birth, I use a difference-in-differences framework to estimate the average treatment effects of the policy. The model is specified as:

$$y_{idt} = \gamma_1 \cdot Treated_d + \gamma_2 \cdot Post_t + \beta \cdot (Treated_d \times Post_t) + X'_{idt}\theta + B_d + C_t + \epsilon_{idt}, \quad (1)$$

where y_{idt} represents the continuous outcome for individual i residing in district d, born in year t. $Treated_d$ is an indicator equal to 1 for individuals residing in treated districts of West Bengal, and $Post_t$ is an indicator equal to 1 for birth years after 1995, marking eligibility for the policy. The interaction term $Treated_d \times Post_t$ captures the intent-to-treat effect of the policy, β. Covariates (X'_{idt}) include caste, religion of the household head, wealth index, language spoken, and place of residence. District fixed effects (B_d) account for time-invariant differences at the district level, while birth-year cohort fixed effects (C_t) control for birth-year-specific unobservables. Standard errors are clustered at the district level.

To assess the dynamics of policy effects on above outcome variables and to validate the parallel trends assumption, I employ an event study framework. The event study specification is:

$$y_{idt} = \sum_{k=-7}^{-1} \beta_k \cdot (Prior_t^{(k)} \times Treated_d) + \sum_{k=0}^{6} \gamma_k \cdot (Post_t^{(k)} \times Treated_d) + X'_{idt}\theta + B_d + C_t + \epsilon_{idt}, \quad (2)$$

where $Prior_t^{(k)}$ is an indicator for being k years before the policy's implementation ($k = -7$ to -1), and $Post_t^{(k)}$ is an indicator for being k years of exposure to the policy ($k = 0$ to 6). The base category is defined as $T = -1$, representing the cohort just before the policy became effective. The coefficients β_k capture pre-treatment trends, while γ_k capture the dynamic policy effects for varying lengths of exposure. Insignificant pre-treatment coefficients (β_k) provide evidence supporting the parallel trends assumption.

While continuous variables like age at marriage and age at first birth are useful, they are only defined for women who are married or have had a child, respectively, excluding information about those who remain unmarried or childless. Binary variables indicating whether a woman was married or had a child by specific ages allow me to include both groups, ensuring a comprehensive analysis. Similarly, for education, binary outcomes like completed grade 8 or higher or completed grade 10 capture critical thresholds, addressing potential biases from censored data.

For binary outcomes, such as married by age 13 to 18, gave birth by age 13 to 18, and completed specific grade thresholds (e.g., grade 8 or grade 10), I use a probit model to estimate the policy's effect. The equation is:

$$P(y_{idt} = 1) = \Phi\left(\gamma_1 \cdot Treated_d + \gamma_2 \cdot Post_t + \beta \cdot (Treated_d \times Post_t) + X'_{idt}\theta + B_d + C_t\right), \quad (3)$$

where $P(y_{idt} = 1)$ represents the probability of the binary outcome occurring, and $\Phi(\cdot)$ is the cumulative distribution function of the standard normal distribution. Marginal effects are calculated to interpret the changes in probabilities attributable to the policy. To analyze the dynamic effects of the policy on binary outcomes, I extend the event study framework to a probit model:

$$P(y_{idt} = 1) = \Phi\left(\sum_{k=-7}^{-1} \beta_k \cdot (Prior_t^{(k)} \times Treated_d) + \sum_{k=0}^{6} \gamma_k \cdot (Post_t^{(k)} \times Treated_d) + X'_{idt}\theta + B_d + C_t\right). \quad (4)$$

Here, pre-treatment coefficients (β_k) provide insight into the validity of the parallel trends assumption, while post-treatment coefficients (γ_k) capture the timing and persistence of policy effects. This model evaluates binary thresholds derived from continuous variables, such as whether an individual has completed a certain grade or reached specific marriage or motherhood milestones.

There is a possibility of spillover effects, where families in neighboring states migrate to West Bengal to benefit from the policy. However, such effects are unlikely given India's low interstate migration rates.

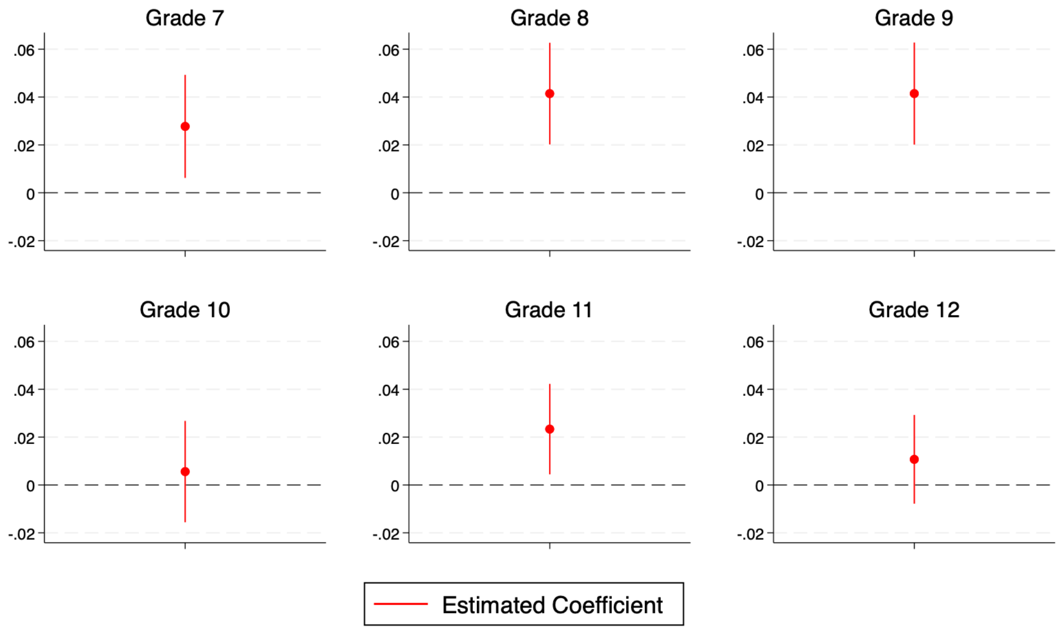
6 Results

6.1 Educational Attainment

Figure A.13 (Appendix) examines the effect of the policy on total years of schooling among students who reached at least Grade 7. This aligns with the policy's focus on supporting girls aged 13-18, or those generally in Grades 7 to 12. The results indicate a modest, statistically insignificant effect on total years of schooling with increased exposure to the policy. Specifically, for cohorts with four to six years of exposure, the average increase in schooling is between 1.73 to 2.16 years, translating to an approximate increase of 16% to 20% above the pre-policy mean of around 8 years. Despite this slight positive impact observed in later years of exposure, the overall effect on total years of schooling remains small.

Given these modest findings, a grade-wise analysis provides a more detailed understanding of how the policy might influence specific educational milestones. Figure 4 and Figure 5 display the likelihood of passing each grade from 7 through 12, with binary indicators set to '1' if a student passed and "0" if they dropped out before completing the grade. Figure 4 presents the overall point estimates.For Grades 7 to 9, the likelihood of passing shows a statistically significant increase, with gains between 2.7 to 4.1 pp (or 4 to 7%). These middle grades exhibit the most substantial positive impact from the policy, suggesting it was most effective in supporting students at this stage. Passing rates for Grade 11 also see a modest increase, around 4% over the pre-policy mean, although the effect is less consistent than in Grades 7 to 9. Conversely, Grades 10 and 12 show minimal and statistically insignificant changes, with increases of only 0.5 to 1.07 percentage points, or about 1% to 4% over their respective pre-policy means of 0.27 and 0.25.

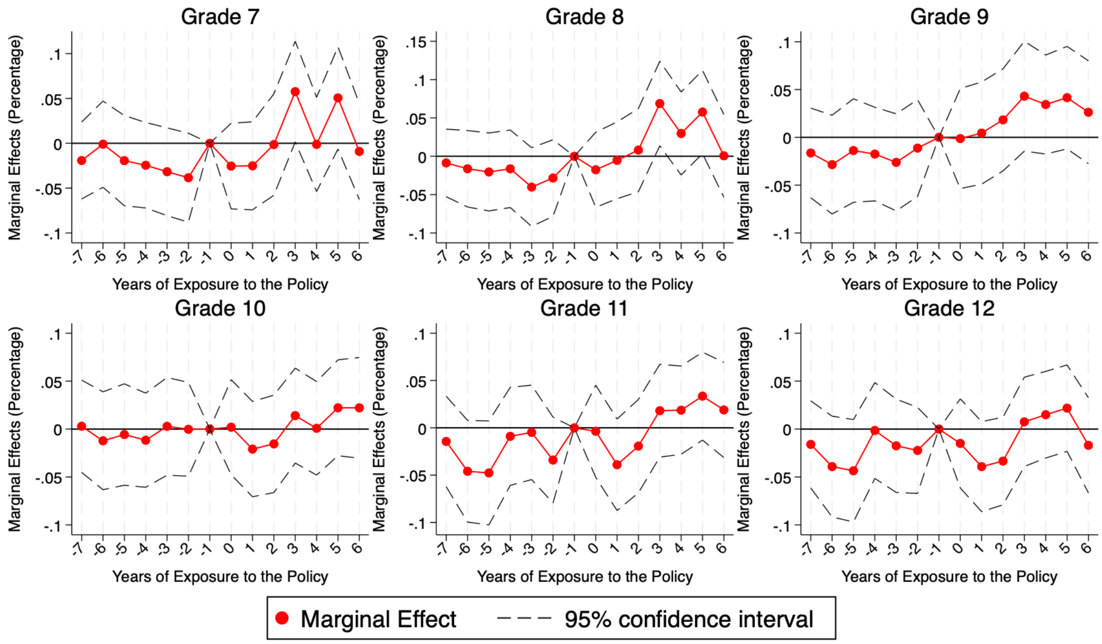
Figure 4: Impact on Grade Completion among Girls



This figure presents the difference-in-difference estimates on grade completion for girls across Grades 7 to 12. For instance, the Grade 7 variable is coded as 1 if a student has completed at least Grade 7 (including Grades 8-12) and 0 otherwise. The sample includes females aged 19-34, born between 1987-2001, drawn from six control states and the treated state of West Bengal, using data from the NFHS-5 (2019-21) household member schedule. The regression includes state and birth-year fixed effects, with standard errors clustered at the state level, and results are weighted. The vertical bars represent 95% confidence intervals for each grade level. The results are based on the Equation 3.

The event-study analysis in Figure 5 examines changes in passing rates across cohorts with varying levels of policy exposure, confirming that pre-policy coefficients remain close to zero or statistically insignificant across all grades. For Grades 7, 8, and 9, the strongest positive effects appear in the 3 to 5-year exposure range, with passing rates increasing by 5% to 10%. However, these estimates come with broad confidence intervals. For Grades 10 to 12, the effects mostly hover around zero.

Figure 5: Event Study: Impact on Grade Completion among Girls



This figure presents DID event study estimates on grade completion for girls across Grades 7 to 12. For instance, the Grade 7 variable is coded as 1 if a student has completed at least Grade 7 (including Grades 8-12) and 0 otherwise. The x-axis represents the cohort's exposure to the policy, where T=0 indicates the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years. T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. The sample includes females aged 18–34, born between 1987–2001, from 7 control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) household member schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure displays 95% confidence intervals.The results are based on the Equation 4

6.2 Age at Marriage

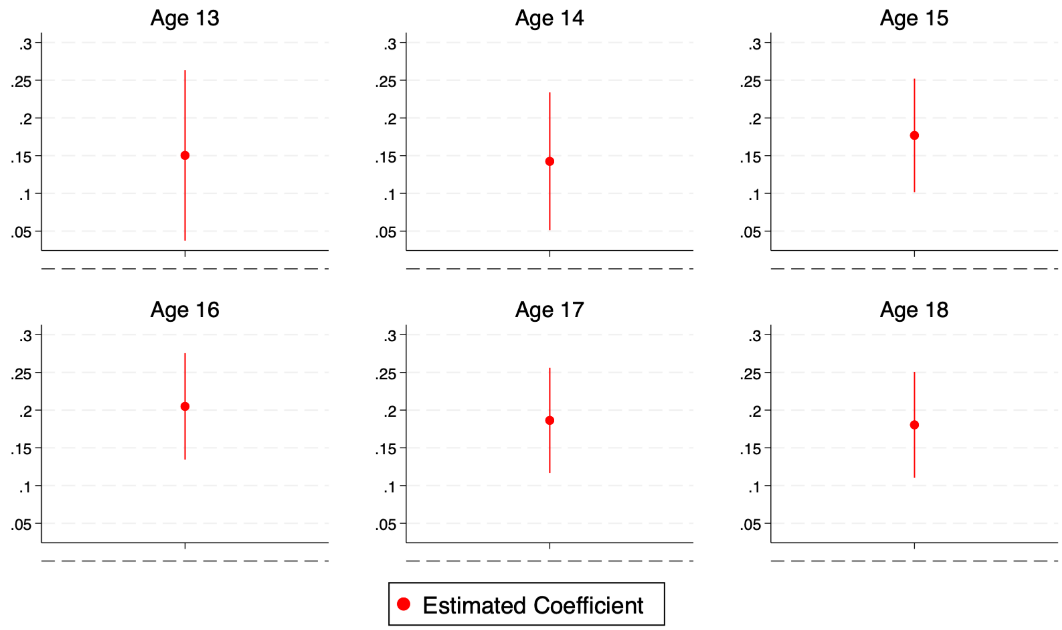
The estimated effects of the policy on age at marriage, looking only at women who are already married is presented in Figure A.14 (Appendix).[12] The results show little sign that the policy delayed marriage age, with most point estimates close to zero. One limitation of this approach is that it only includes women who are already married, leaving us unsure about the policy's impact on those who remain unmarried and may have delayed marriage. Also, as policy exposure increases, we are looking at progressively younger women, with maximum exposure at age 19. This structure makes it harder to see if the policy led some women to delay marriage. An age-wise analysis is more informative to better understand the policy's effect on marriage timing, as it breaks down marriage likelihood by each specific age, similar to the grade-wise analysis used in the education results.

Figure 6 and Figure 7 offer a closer examination of marriage likelihood by specific ages, from 13 to 18, where each age variable is coded as 1 if a woman is married by that age, and 0 if she marries later or remains unmarried. This age-specific analysis, based on marginal effects from logit regressions, helps illustrate the policy's impact on marriage timing. Figure 6, the overall point estimates for each age group reveal that for ages 13 to 15, the policy effect is statistically insignificant, indicating minimal impact on delaying marriage at these younger ages. For ages 16 to 18, there is a modest increase in marriage likelihood, with point estimates showing a rise of approximately 4-6 percentage points. Specifically, marriage likelihood increases by 9.93% for age 16, 11.21% for age 17, and 13.33% for age 18 over pre-policy levels.

Expanding on the previous analysis, Figure 7 provides an event study view, illustrating marriage likelihood across different years of policy exposure. For ages 13 to 15, the effect remains

[12]To control for age differences across cohorts, I focus only on women who married between the ages of 13 and 18. Women who married before age 13 or after age 18 are excluded from the sample. Marriages occurring after age 18 are excluded because the youngest cohort in the dataset, aged 19, would not have had enough time to exhibit trends in later-age marriages, making such data less informative for this analysis. Similarly, women married before age 13 are excluded because child marriages at such early ages may not align with the policy's intended effects or primary target group.

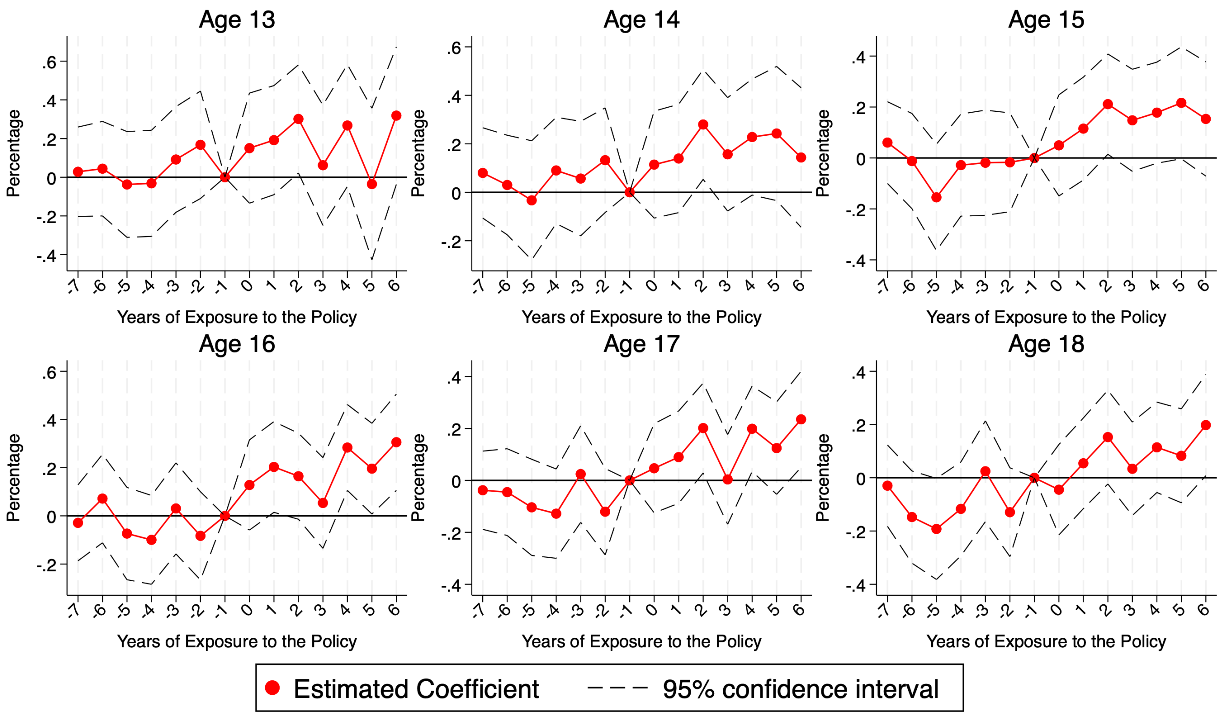
Figure 6: DID: Impact on Girls' Age at Marriage



This figure presents DID estimates on girls' age at marriage across Ages 13 to 18. For instance, the Age 15 variable is coded as 1 if a girl is married by age 15 (including Ages 13–15) and 0 otherwise. The sample includes females aged 18–34, born between 1987–2001, from 7 control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure presents 95% confidence intervals. This figure provides an alternative perspective on the conditional analysis results shown in Figure 14.

close to zero, suggesting that the policy had little to no impact on delaying marriage at these younger ages. However, for ages 16 to 18, marriage likelihood begins to rise steadily as policy exposure increases. After four years of exposure, marriage likelihood increases significantly for ages 16 and 17, with gains of approximately 4.4 to 6.5 percentage points for age 16 (equating to around 12% to 18%) and 5 to 6 percentage points for age 17 (about 10% to 12%). At age 18, although there's an upward trend, most increases are statistically insignificant, except for a notable 5.2 percentage point rise after six years (an 8.8% increase). This pattern, particularly within the 4 to 6-year exposure range, suggests that the policy may unintentionally raise marriage likelihood as girls approach the legal minimum age.

Figure 7: Event Study: Impact on Girls' Age at Marriage



This figure presents DID event study estimates on girls' age at marriage across Ages 13 to 18. For instance, the Age 15 variable is coded as 1 if a girl is married by age 15 (including Ages 13–15) and 0 otherwise. The x-axis represents the cohort's exposure to the policy, where T=0 indicates the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years. T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. The sample includes females aged 18–34, born between 1987–2001, from 7 control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure displays 95% confidence intervals.

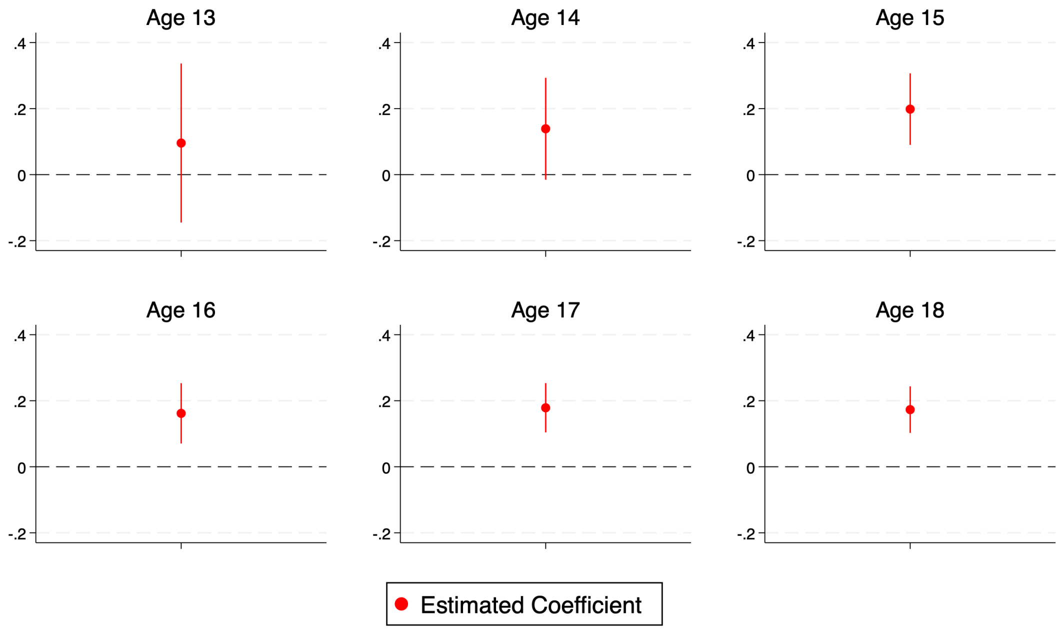
6.3 Age at First Childbirth

Figure A.15 (Appendix) evaluates the policy's effect on the timing of first childbirth among women who have already become mothers.[13] The results show limited evidence that the policy delayed the age of first childbirth, as most estimates remain close to zero. This approach, however, only includes women who have already had children, potentially overlooking the policy's effect on those who may have delayed childbirth or remained childless. To provide a broader perspective on the policy's influence on early motherhood, Figure 8 and Figure 9 offer an age-specific breakdown analogous to age at marriage.

The age-specific analysis reveals the policy's modest impact on early motherhood likelihood at different ages. It explores childbirth likelihood by ages 13 through 18, using marginal effects from logit regressions to capture the policy's influence on childbirth timing. There is no significant overall effect for ages 13 and 14, with only minor changes in early childbirth likelihood. For ages 15 to 18, however, a slight increase in the probability of having a first child emerges, with estimates showing a rise between 1.5 and 4 percentage points, translating to roughly a 10% to 14% increase. These findings suggest that, rather than delaying childbirth, the policy may have a subtle positive effect on early motherhood at these ages.

[13] To focus on early motherhood, cases of first childbirth after age 19 were excluded from the analysis. This approach mirrors the methodology used in the age at marriage section.

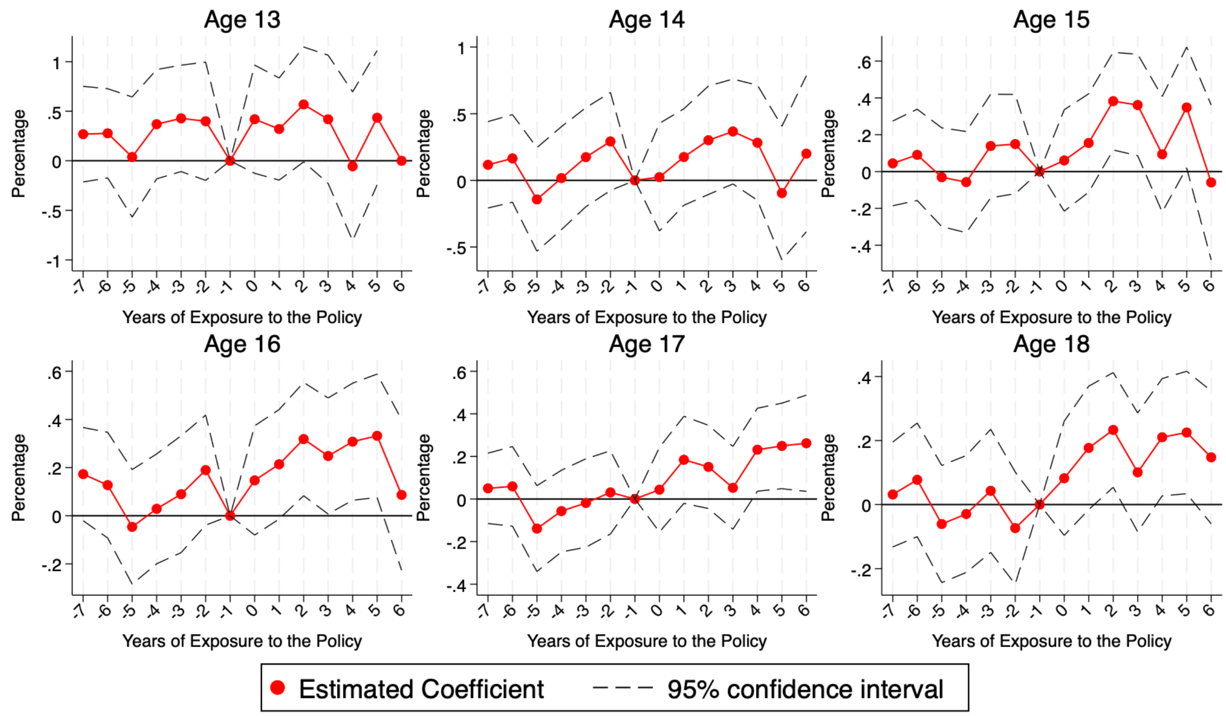
Figure 8: DID: Impact on Girls' Age at First Birth



This figure presents DID estimates on girls' age at first birth across ages 13 to 18. For instance, the Age 15 variable is coded as 1 if a girl has given birth to her first baby by age 15 (including ages 13–15) and 0 otherwise. The sample includes females aged 18–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the DHS-5 (2019–21) women's schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure presents 95% confidence intervals.

As policy exposure extends, particularly within the 4 to 6-year range, there is a gradual increase in the likelihood of early childbirth for ages 16 to 18. By age 16, this likelihood reaches around 3 percentage points, or approximately a 10% increase, after two years of exposure. At age 17, the likelihood rises to about 4 percentage points, or a 12% boost, becoming especially noticeable after four years of exposure. By age 18, the trend continues with an increase of roughly 4 to 5 percentage points, though confidence intervals remain wide. This trend aligns with the marriage timing results, where extended exposure similarly correlates with an increased likelihood of marriage as girls approach age 18.

Figure 9: Event Study: Impact on Girls' Age at First Birth



This figure presents DID event study estimates on girls' age at first birth across ages 13 to 18. For instance, the Age 15 variable is coded as 1 if a girl has given birth to her first baby by age 15 (including ages 13–15) and 0 otherwise. The x-axis represents the cohort's exposure to the policy, where T=0 indicates the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years. T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. The sample includes females aged 18–34, born between 1987–2001, from 7 control states and the treated state of West Bengal, using data from the DHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure displays 95% confidence intervals.

7 Discussion

The results indicate that KP had mixed outcomes, with modest educational improvements overshadowed by unintended increases in early marriage and motherhood. The policy's focus on girls aged 13 to 18 resulted in slight gains in middle-grade completion, particularly in grades 8 and 9, while higher grades saw minimal changes. However, as exposure to the policy increased, marriage and early motherhood likelihood rose significantly for girls aged 16 to 18. This pattern suggests that while the policy may have encouraged families to invest in education initially, cultural and economic factors, including the perception of the lump sum as a dowry substitute, may have undermined its broader goals. The observed trends highlight how KP's design interacted with entrenched social norms, revealing both the potential and limitations of financial incentives in influencing deeply rooted behaviors.

KP may have faced significant challenges in achieving its intended outcomes due to several structural and contextual factors. Weak verification and oversight mechanisms likely played a central role in undermining the program's effectiveness. While the annual scholarship required school verification to confirm eligibility, the lump sum payment at age 18 relied solely on self-attestation of marital status, without any requirement for proof of educational attainment. This lack of monitoring may have allowed families to exploit the system, particularly in cases where underage marriages were not formally registered. Reports of bribery further weakened the program's integrity, with some eligible girls being denied benefits unless they paid bribes.[14] Stricter verification measures, such as mandatory school attestation or regular audits, might have improved adherence to program goals and reduced misuse.

KP's annual scholarship benefit may have been insufficient to incentivize long-term educational engagement for many families. At Rs. 1,000 (approximately $17.07 in 2013) per year, the amount was likely too small to cover indirect schooling costs, such as transportation, uniforms, and supplies, particularly for economically disadvantaged households. While the policy resulted in

[14] For example, a girl in a district of West Bengal was denied the KP benefit after refusing to pay a bribe to a panchayat employee, despite being unmarried and eligible. For details, see: The Telegraph, March 2023.

modest increases in middle-grade completion, such as grades 8 and 9, these gains did not extend to higher grades and failed to delay marriage. This stands in contrast to the widely accepted notion that female education generally postpones marriage by shifting preferences, increasing wages, or enhancing decision-making power (Becker, 1973; Chiappori et al., 2009; Heath and Mobarak, 2015). Instead, findings align with literature suggesting that modest educational gains in dowry-practicing societies may enhance marriageability and lead to earlier unions (Agarwal et al., 2022). Other successful CCT programs, like Colombia's education voucher initiative and Mexico's Prospera program, provided consistent financial incentives tied directly to school attendance and performance (Angrist et al., 2002; Fiszbein and Schady, 2009). KP's design, however, missed the opportunity to link its lump sum to sustained educational milestones, which may have undermined its capacity to encourage longer-term educational investment.

The lump sum transfer under KP may have been perceived as a form of dowry, leading to unintended consequences. Families might have married their daughters early but still claimed the transfer at 18, as the girl would remain legally unmarried. Similar trends were observed in Haryana, where a comparable CCT program had minimal impact on altering marriage norms, with families frequently using the lump sum for wedding costs and dowry expenses (Nanda et al., 2016). Research further highlights how financial incentives and rural credit expansion in India have facilitated earlier marriages by making dowries more affordable (Corno et al., 2017; Saha, 2017). Reports and studies[15] [16] suggest that such transfers are often interpreted as government support for marriage expenses, inadvertently reinforcing dowry practices.

Cultural norms and adherence to traditional practices may further explain the policy's counterintuitive outcomes. Fors et al. (2023) emphasize the persistence of these norms, which are challenging to shift through financial incentives alone. In dowry-practicing contexts, delaying marriage may even increase dowry demands, as older and more educated brides are often perceived to command higher dowries. The average dowry amount in India, cited at ₹ 35,000 (approximately $598 in 2008), represented 14% of annual household income, a significant financial burden for

[15] Apolitical: Indian scheme shows cash transfers are not enough to curb child marriage

[16] Quartz: A scheme to end child marriage in Haryana has backfired

families. This dynamic might have incentivized families to marry daughters earlier while utilizing the lump sum as a delayed dowry payment. Buchmann et al. (2018) found that entrenched marriage norms often undermine the effectiveness of financial transfers aimed at reducing child marriage in Bangladesh, with families prioritizing marriage arrangements over long-term investments in education. Similarly, Fiszbein and Schady (2009) highlight the importance of designing cash transfer programs that align with local cultural dynamics to avoid unintended consequences. In India, Corno et al. (2017) and Saha (2017) emphasize how financial liquidity and dowry affordability in rural regions often reinforce early marriage norms rather than reduce them. KP's inability to distinguish between genuinely unmarried girls and those married before 18 but legally classified as unmarried may have further complicated its implementation, weakening its ability to achieve its goals.

These insights suggest that KP's challenges may have stemmed from insufficient incentives, entrenched marriage norms, and weak verification mechanisms, which may have limited its ability to delay marriage or promote long-term educational gains. This underscores the importance of context-sensitive policy designs.

8 Conclusion

This paper evaluates the impact of KP, a CCT program in West Bengal, India, designed to reduce child marriage and promote education. While KP successfully increased middle-grade completion, its effects on higher education were negligible. More notably, the policy unintentionally increased the likelihood of early marriage and motherhood among girls aged 16 to 18, particularly after extended exposure. These findings highlight the challenges of designing financial incentives in contexts where entrenched social norms and weak oversight mechanisms shape behavioral responses.

These findings indicate that the policy's mixed outcomes may be attributed to inadequate financial incentives, deeply rooted cultural norms, and weak verification processes. Addressing these challenges could involve linking the lump sum payment to both marital status and educational milestones, verified through secondary or tertiary institutions. Implementing stricter oversight measures, such as regular audits and independent monitoring, could further minimize corruption.

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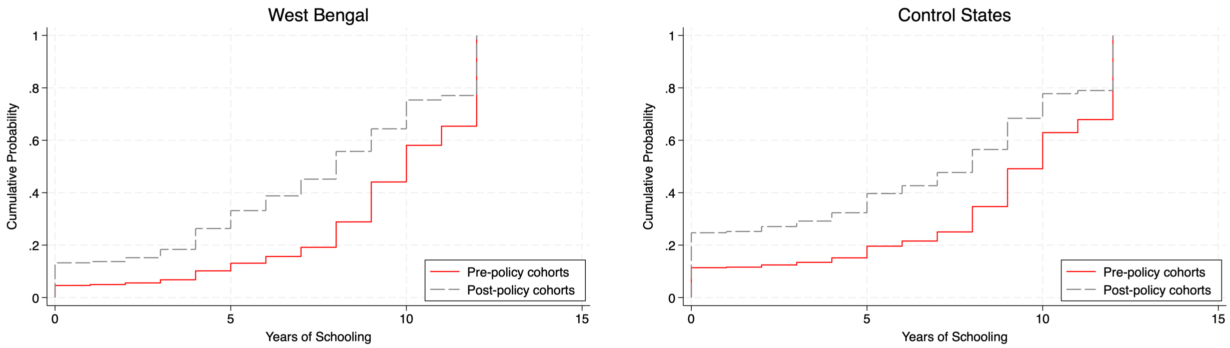
Appendix

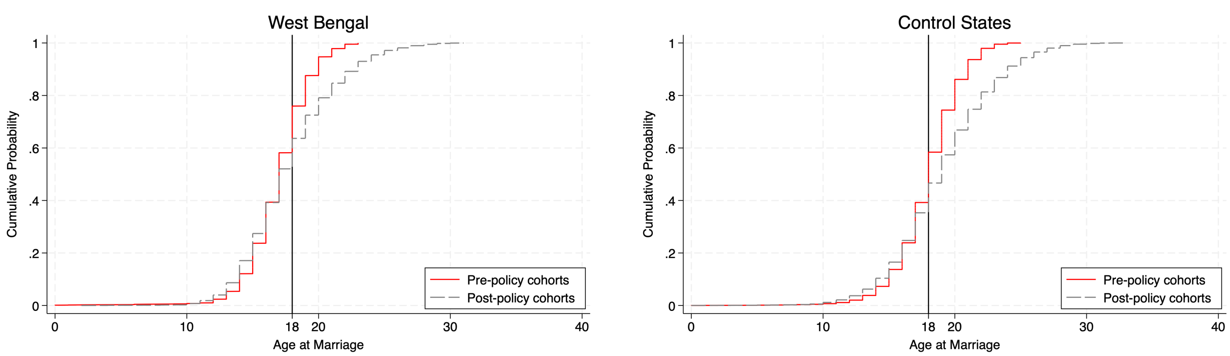
A.1 Figures

Figure A.10: CDF of Years of Schooling in West Bengal and Control States



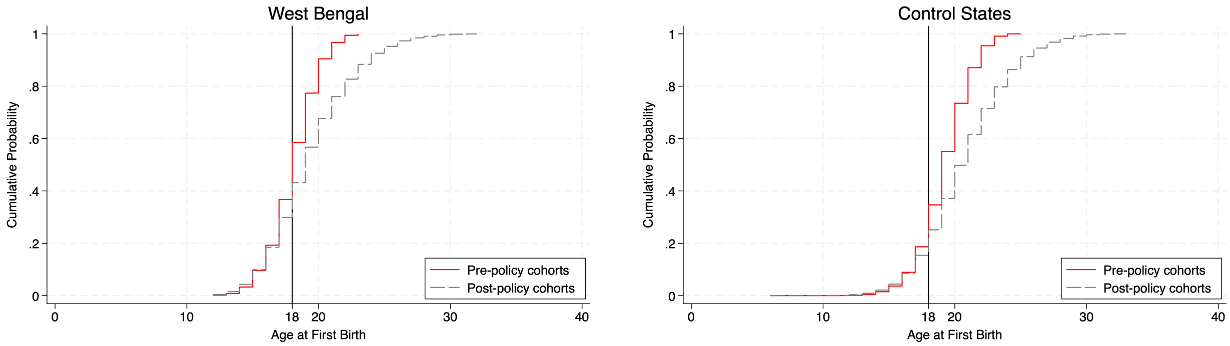
The figure shows the cumulative distribution function (CDF) of years of schooling for girls aged 19–34 in West Bengal (treated state) and the control states, pre- and post-policy, using household schedule of DHS 2019–21. Control states include Bihar, Jharkhand, Odisha, Assam, Sikkim, and Tripura. The CDF illustrates the proportion of individuals achieving specific years of schooling before and after the policy's implementation.

Figure A.11: CDF of Age at Marriage in West Bengal and Control States



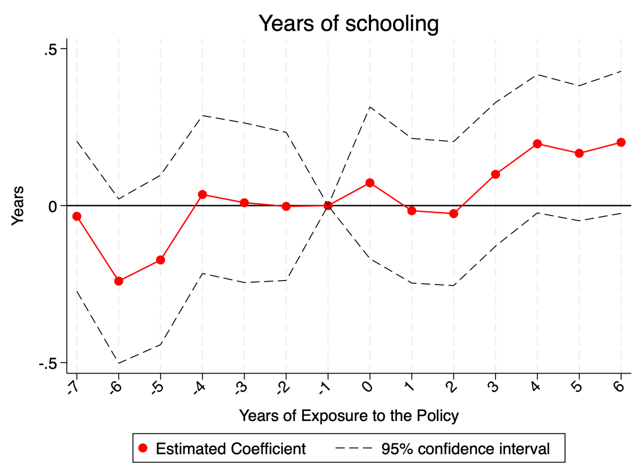
The figure shows the cumulative distribution function (CDF) of age at marriage for girls aged 19–34 in West Bengal (treated state) and the control states, pre- and post-policy, using NFHS 2019–21. Control states include Bihar, Jharkhand, Odisha, Assam, Sikkim, and Tripura. The CDF illustrates the proportion of individuals married at specific ages, comparing pre-policy and post-policy cohorts.

Figure A.12: CDF of Age at First Birth in West Bengal and Control States



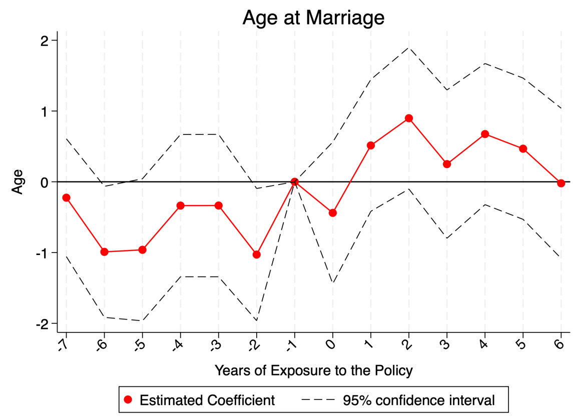
The figure shows the cumulative distribution function (CDF) of age at first birth for women aged 19–34 in West Bengal (treated state) and the control states, pre- and post-policy, using NFHS 2019–21. Control states include Bihar, Jharkhand, Odisha, Assam, Sikkim, and Tripura. The CDF illustrates the proportion of women experiencing their first childbirth at specific ages, comparing pre-policy and post-policy cohorts.

Figure A.13: Impact on Total Years of Schooling among Girls



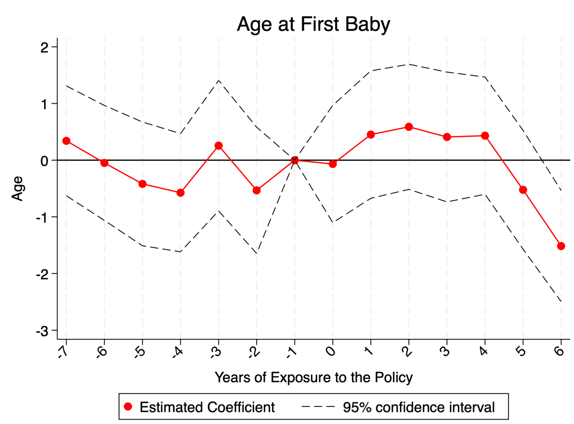
This figure presents the total years of schooling among girls in treated and control states, focusing on grades 7–12. The analysis restricts the schooling variable to this range to align with the policy's primary target group: girls aged 13–18, typically enrolled in grades 7–12. Girls who dropped out before grade 7 are excluded, and schooling years are capped at grade 12. The sample includes girls aged 19–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from DHS-5 (2019–21). The results are based on the continuous event study specification in Equation 2. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and results are weighted. The figure displays 95% confidence intervals.

Figure A.14: Impact on Age at Marriage among Girls



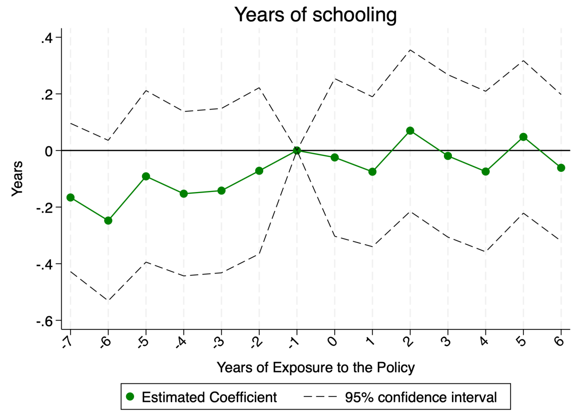
This figure presents the estimated effects of the policy on age at marriage for females aged 19–34 (DHS-5 2019–21), focusing on marriages occurring between ages 13 and 18. Ages outside this range are excluded to minimize cohort age differences and emphasize early marriage dynamics most relevant to the policy. The results are based on the continuous event study specification in Equation 2. The regression includes district and birth-year fixed effects, with standard errors clustered at the state level, and results are weighted. The figure displays 95% confidence intervals.

Figure A.15: Impact on Age at First Birth among Girls



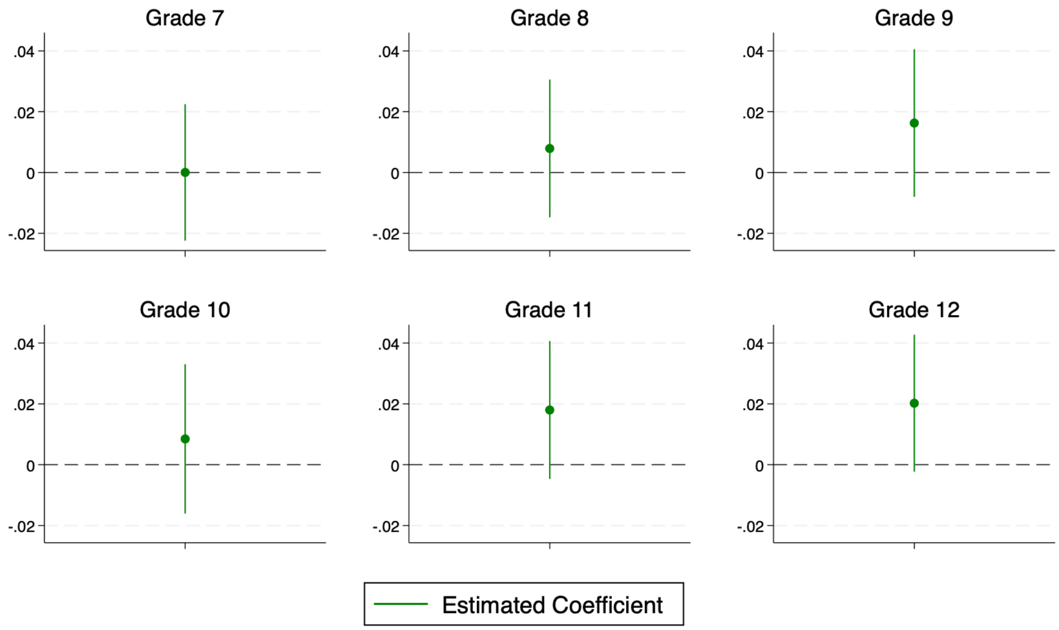
This figure presents the estimated effects of the policy on age at first birth for females aged 19–34 (DHS-5 2019–21), focusing on marriages occurring between ages 13 and 18. Ages outside this range are excluded to minimize cohort age differences and emphasize early marriage dynamics most relevant to the policy. The results are based on the continuous event study specification in Equation 2. The regression includes district and birth-year fixed effects, with standard errors clustered at the state level, and results are weighted. The figure displays 95% confidence intervals.

Figure A.16: Impact on Total Years of Schooling among Boys



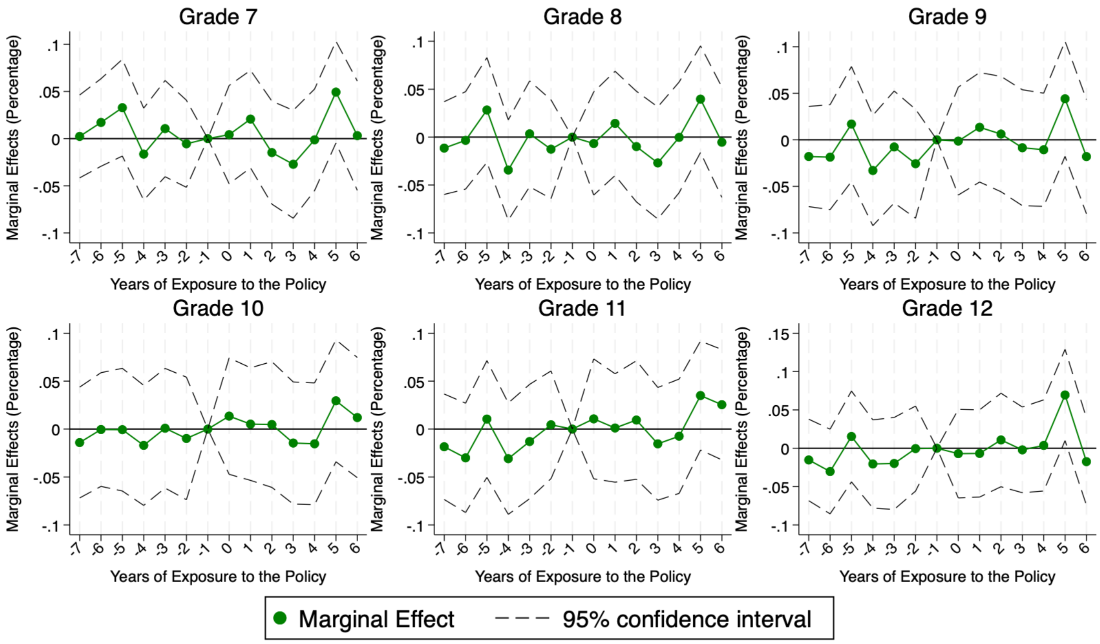
This figure presents the total years of schooling among boys in treated and control states. The analysis uses males aged 18–34, born between 1987–2001, drawn from six control states and the treated state of West Bengal, using NFHS-5 (2019–21) household member schedule. The regression includes state and birth-year fixed effects, with standard errors clustered at the state level, and results are weighted. The figure displays 95% confidence intervals.

Figure A.17: Impact on Grade Completion among Boys



This figure presents the difference-in-difference estimates on grade completion for boys across Grades 7 to 12. For instance, the Grade 7 variable is coded as 1 if a student has completed at least Grade 7 (including Grades 8–12) and 0 otherwise. The sample includes males aged 18–34, born between 1987–2001, drawn from six control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) household member schedule. The regression includes state and birth-year fixed effects, with standard errors clustered at the state level, and results are weighted.The figure displays 95% confidence intervals for each grade level.

Figure A.18: Event Study: Impact on Grade Completion among Boys



This figure presents DID event study estimates on grade completion for boys across Grades 7 to 12. For instance, the Grade 7 variable is coded as 1 if a student has completed at least Grade 7 (including Grades 8–12) and 0 otherwise. The x-axis represents the cohort's exposure to the policy, where T=0 indicates the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years. T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. The sample includes males aged 18–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) household member schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. The figure displays 95% confidence intervals.

A.2 Tables

Table A.1: Summary Statistics for Grade Completion

	Treatment			Control		
	All	Pre-Policy	Post-Policy	All	Pre-Policy	Post-Policy
Years of Schooling	8.47	7.73	9.55	7.75	6.89	8.92
	(4.37)	(4.73)	(3.53)	(4.87)	(5.10)	(4.26)
Grade 7	0.71	0.61	0.84	0.66	0.57	0.78
	(0.46)	(0.49)	(0.36)	(0.47)	(0.49)	(0.41)
Grade 8	0.65	0.55	0.81	0.62	0.52	0.75
	(0.48)	(0.50)	(0.39)	(0.49)	(0.50)	(0.43)
Grade 9	0.55	0.44	0.71	0.53	0.44	0.65
	(0.50)	(0.50)	(0.45)	(0.50)	(0.50)	(0.48)
Grade 10	0.44	0.36	0.56	0.40	0.32	0.51
	(0.50)	(0.48)	(0.50)	(0.49)	(0.47)	(0.50)
Grade 11	0.32	0.25	0.42	0.29	0.22	0.37
	(0.47)	(0.43)	(0.49)	(0.45)	(0.42)	(0.48)
Grade 12	0.28	0.23	0.35	0.26	0.21	0.32
	(0.45)	(0.42)	(0.48)	(0.44)	(0.41)	(0.47)
Observations	10,637	6,316	4,321	89,463	51,592	37,871

Table reports summary statistics for female cohorts (ages 19–34) from the household schedule (DHS-5). The table presents the mean and standard deviation (in parentheses) for years of schooling and the probability of passing grades 7–12. For example, the Grade 7 variable is coded as 1 if the individual has completed at least Grade 7 (including Grades 8–12) and 0 otherwise. The sample includes individuals born between 1987 and 2001 from six control states and the treated state of West Bengal.

Table A.2: Impact on Grade Completion

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 7	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Treat x Post	2.10	2.89	3.16	0.11	1.75	0.82
	(0.04)	(0.00)	(0.00)	(0.91)	(0.08)	(0.41)
Observations	96,829	96,829	96,829	96,829	96,829	96,829

The table presents the DID estimates on grade completion for girls using binary outcome variables. For instance, the Grade 7 variable is coded as 1 if the individual has completed at least Grade 7 (including Grades 8–12) and 0 otherwise. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes individuals aged 19–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the DHS-5 (2019-21) household schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.

Table A.3: Event Study: Impact on Grade Completion among Girls

	(1) Grade 7 β/SE	(2) Grade 8 β/SE	(3) Grade 9 β/SE	(4) Grade 10 β/SE	(5) Grade 11 β/SE	(6) Grade 12 β/SE
T=-7	-0.44	-0.05	-0.40	0.33	-0.43	-0.51
	(0.66)	(0.96)	(0.69)	(0.74)	(0.67)	(0.61)
T=-6	0.21	-0.56	-1.10	-0.38	-1.55	-1.49
	(0.83)	(0.58)	(0.27)	(0.71)	(0.12)	(0.14)
T=-5	-0.58	-0.60	-0.34	-0.19	-2.39	-2.02
	(0.56)	(0.55)	(0.74)	(0.85)	(0.02)	(0.04)
T=-4	-1.02	-0.68	-0.64	-0.41	-0.31	-0.05
	(0.31)	(0.50)	(0.52)	(0.68)	(0.76)	(0.96)
T=-3	-1.20	-1.24	-0.86	0.07	-0.37	-1.08
	(0.23)	(0.21)	(0.39)	(0.95)	(0.71)	(0.28)
T=-2	-1.59	-1.13	-0.58	-0.08	-1.44	-0.92
	(0.11)	(0.26)	(0.56)	(0.94)	(0.15)	(0.36)
T=0	-1.37	-0.87	-0.14	-0.01	-0.30	-1.02
	(0.17)	(0.38)	(0.89)	(0.99)	(0.76)	(0.31)
T=1	-1.29	-0.34	0.15	-1.14	-1.87	-1.72
	(0.20)	(0.74)	(0.88)	(0.25)	(0.06)	(0.09)
T=2	-0.25	0.18	0.61	-0.81	-0.79	-1.55
	(0.80)	(0.85)	(0.54)	(0.42)	(0.43)	(0.12)
T=3	2.02	3.08	1.84	0.60	0.71	0.34
	(0.04)	(0.00)	(0.07)	(0.55)	(0.48)	(0.73)
T=4	-0.14	1.07	1.40	-0.15	0.70	0.55
	(0.89)	(0.28)	(0.16)	(0.88)	(0.49)	(0.58)
T=5	1.53	1.69	1.50	0.69	1.15	0.85
	(0.12)	(0.09)	(0.13)	(0.49)	(0.25)	(0.40)
T=6	-0.36	-0.14	0.81	0.64	0.54	-0.90
	(0.72)	(0.89)	(0.42)	(0.52)	(0.59)	(0.37)
Observations	96,829	96,829	96,829	96,829	96,829	96,829

The table presents event study estimates on grade completion for girls across Grades 7 to 12. Each grade variable coded as 1 if an individual has completed at least that grade level (including higher grades) and 0 otherwise. The terms T=-7 to T=6 represent the cohort's exposure to the policy, with T=0 indicating the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years, while T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes females aged 19–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the DHS-5 (2019–21) household schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.

Table A.4: Summary Statistics for Age at Marriage

	Treatment			Control		
	All	Pre-Policy	Post-Policy	All	Pre-Policy	Post-Policy
Married by 13	0.06	0.08	0.03	0.04	0.06	0.02
	(0.24)	(0.27)	(0.18)	(0.20)	(0.23)	(0.13)
Married by 14	0.13	0.16	0.08	0.07	0.09	0.04
	(0.33)	(0.36)	(0.27)	(0.25)	(0.29)	(0.18)
Married by 15	0.21	0.25	0.15	0.11	0.15	0.07
	(0.41)	(0.43)	(0.36)	(0.32)	(0.35)	(0.25)
Married by 16	0.32	0.36	0.25	0.18	0.22	0.12
	(0.47)	(0.48)	(0.44)	(0.38)	(0.41)	(0.32)
Married by 17	0.44	0.48	0.38	0.26	0.31	0.19
	(0.50)	(0.50)	(0.48)	(0.44)	(0.46)	(0.39)
Married by 18	0.55	0.59	0.49	0.36	0.42	0.28
	(0.50)	(0.49)	(0.50)	(0.48)	(0.49)	(0.45)
Observations	10,161	6,062	4,099	335,053	197,662	137,391

Table reports summary statistics for age at marriage for females (ages 19–34) from the individual schedule (DHS-5). It presents the mean and standard deviation (in parentheses) for the probability of being married by ages 13–18. For instance, the Married by 15 variable is coded as 1 if a girl is married by age 15 (including ages 13–15) and 0 otherwise. The sample includes females born between 1987 and 2001 from six control states and the treated state of West Bengal.

Table A.5: Impact on Girls' Age at Marriage

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 13	Age 14	Age 15	Age 16	Age 17	Age 18
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Treat x Post	2.61	3.06	4.61	5.70	5.24	5.05
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	319,267	324,506	325,669	326,344	326,348	326,348

The table presents the DID estimates for the impact of the policy on girls' age at marriage across ages 13 to 18. The age variable is coded as 1 if a girl was married by that age (including younger ages) and 0 otherwise. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes females aged 19–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the DHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.

Table A.6: Event Study: Impact on Girls' Age at Marriage

	(1) Age 13 β/SE	(2) Age 14 β/SE	(3) Age 15 β/SE	(4) Age 16 β/SE	(5) Age 17 β/SE	(6) Age 18 β/SE
T=-7	0.24	0.84	0.74	-0.36	-0.49	-0.38
	(0.81)	(0.40)	(0.46)	(0.72)	(0.62)	(0.70)
T=-6	0.36	0.29	-0.13	0.77	-0.53	-1.67
	(0.72)	(0.77)	(0.90)	(0.44)	(0.60)	(0.10)
T=-5	-0.27	-0.26	-1.45	-0.75	-1.10	-1.99
	(0.79)	(0.79)	(0.15)	(0.45)	(0.27)	(0.05)
T=-4	-0.22	0.81	-0.27	-1.06	-1.46	-1.30
	(0.82)	(0.42)	(0.78)	(0.29)	(0.14)	(0.19)
T=-3	0.66	0.47	-0.18	0.32	0.26	0.25
	(0.51)	(0.64)	(0.86)	(0.75)	(0.80)	(0.80)
T=-2	1.18	1.20	-0.17	-0.89	-1.42	-1.52
	(0.24)	(0.23)	(0.86)	(0.37)	(0.16)	(0.13)
T=0	1.04	1.01	0.49	1.34	0.53	-0.52
	(0.30)	(0.31)	(0.62)	(0.18)	(0.59)	(0.61)
T=1	1.33	1.22	1.12	2.11	0.99	0.63
	(0.18)	(0.22)	(0.26)	(0.03)	(0.32)	(0.53)
T=2	2.11	2.42	2.10	1.81	2.28	1.70
	(0.03)	(0.02)	(0.04)	(0.07)	(0.02)	(0.09)
T=3	0.39	1.31	1.45	0.56	0.04	0.38
	(0.70)	(0.19)	(0.15)	(0.58)	(0.97)	(0.71)
T=4	1.66	1.87	1.76	3.12	2.37	1.32
	(0.10)	(0.06)	(0.08)	(0.00)	(0.02)	(0.19)
T=5	-0.18	1.72	1.93	2.04	1.37	0.92
	(0.86)	(0.09)	(0.05)	(0.04)	(0.17)	(0.36)
T=6	1.76	0.98	1.34	3.00	2.49	2.04
	(0.08)	(0.33)	(0.18)	(0.00)	(0.01)	(0.04)
Observations	319,267	324,506	325,669	326,344	326,348	326,348

The table presents event study estimates on girls' age at marriage across Ages 13 to 18. The terms T=-7 to T=6 represent the cohort's exposure to the policy, with T=0 indicating the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years, while T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes females aged 19–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.

Table A.7: Summary Statistics for Age at First Birth

	Treatment			Control		
	All	Pre-Policy	Post-Policy	All	Pre-Policy	Post-Policy
Firstborn by 13	0.01	0.01	0.00	0.01	0.01	0.00
	(0.10)	(0.11)	(0.06)	(0.07)	(0.09)	(0.04)
Firstborn by 14	0.03	0.04	0.02	0.01	0.02	0.00
	(0.17)	(0.19)	(0.12)	(0.11)	(0.13)	(0.07)
Firstborn by 15	0.07	0.08	0.04	0.03	0.04	0.01
	(0.25)	(0.28)	(0.21)	(0.16)	(0.19)	(0.11)
Firstborn by 16	0.13	0.16	0.09	0.05	0.07	0.03
	(0.34)	(0.36)	(0.28)	(0.22)	(0.25)	(0.17)
Firstborn by 17	0.22	0.25	0.17	0.10	0.13	0.06
	(0.41)	(0.44)	(0.37)	(0.30)	(0.33)	(0.24)
Firstborn by 18	0.33	0.37	0.27	0.17	0.20	0.11
	(0.47)	(0.48)	(0.44)	(0.37)	(0.40)	(0.31)
Observations	10,161	6,062	4,099	335,053	197,662	137,391

Table reports summary statistics for age at first birth for females (ages 19–34) from the individual schedule (DHS-5). It presents the mean and standard deviation (in parentheses) for the probability of having the first birth by ages 13–18. For instance, the Firstborn by 15 variable is coded as 1 if a girl is had a baby by age 15 (including ages 13–15) and 0 otherwise. The sample includes females born between 1987 and 2001 from six control states and the treated state of West Bengal.

Table A.8: DID: Impact on Age at First Birth

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 13	Age 14	Age 15	Age 16	Age 17	Age 18
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
Treat x Post	0.78	1.76	3.59	3.47	4.69	4.81
	(0.44)	(0.08)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	259,271	307,201	321,913	325,398	325,911	326,348

The table presents the DID estimates on the age at first birth by ages 13 to 18. The age variable is coded as 1 if a girl had her first baby by that age (including younger ages) and 0 otherwise. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes females aged 18–34, born between 1987 and 2001, from six control states and the treated state of West Bengal, using data from the DHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.

Table A.9: Event Study: Impact on Age at First Birth

	(1) Age 13 β/SE	(2) Age 14 β/SE	(3) Age 15 β/SE	(4) Age 16 β/SE	(5) Age 17 β/SE	(6) Age 18 β/SE
T=-7	1.09	0.71	0.38	1.75	0.60	0.38
	(0.27)	(0.48)	(0.70)	(0.08)	(0.55)	(0.71)
T=-6	1.21	0.98	0.72	1.14	0.62	0.86
	(0.23)	(0.33)	(0.47)	(0.26)	(0.53)	(0.39)
T=-5	0.12	-0.72	-0.22	-0.38	-1.35	-0.65
	(0.90)	(0.47)	(0.82)	(0.70)	(0.18)	(0.52)
T=-4	1.31	0.08	-0.41	0.25	-0.58	-0.31
	(0.19)	(0.93)	(0.68)	(0.80)	(0.56)	(0.75)
T=-3	1.56	0.92	0.97	0.72	-0.17	0.44
	(0.12)	(0.36)	(0.33)	(0.47)	(0.86)	(0.66)
T=-2	1.31	1.56	1.09	1.63	0.31	-0.82
	(0.19)	(0.12)	(0.28)	(0.10)	(0.76)	(0.41)
T=0	1.51	0.12	0.43	1.27	0.43	0.90
	(0.13)	(0.91)	(0.67)	(0.20)	(0.67)	(0.37)
T=1	1.22	0.95	1.14	1.84	1.77	1.80
	(0.22)	(0.34)	(0.25)	(0.07)	(0.08)	(0.07)
T=2	1.92	1.45	2.83	2.65	1.51	2.55
	(0.05)	(0.15)	(0.00)	(0.01)	(0.13)	(0.01)
T=3	1.27	1.83	2.57	2.02	0.53	1.06
	(0.20)	(0.07)	(0.01)	(0.04)	(0.60)	(0.29)
T=4	-0.14	1.28	0.59	2.48	2.33	2.25
	(0.88)	(0.20)	(0.56)	(0.01)	(0.02)	(0.02)
T=5	1.26	-0.37	2.08	2.54	2.43	2.31
	(0.21)	(0.71)	(0.04)	(0.01)	(0.01)	(0.02)
T=6	0	0.67	-0.28	0.54	2.27	1.39
	(.)	(0.50)	(0.78)	(0.59)	(0.02)	(0.16)
Observations	258,859	307,201	321,913	325,398	325,911	326,348

The table presents event study estimates on age at first birth by ages 13 to 18. Each age variable is coded as 1 if an individual had a first baby by that age (including younger ages) and 0 otherwise. The terms T=-7 to T=6 represent the cohort's exposure to the policy, with T=0 indicating the oldest cohort exposed for just one year. These are average treatment effects on younger cohorts eligible for the policy relative to older cohorts who were not. For example, T=-2 refers to the cohort that was 20 at the time of the policy implementation, missing eligibility by two years, while T=6 shows the effect on the youngest cohort, who were exposed to the policy for the maximum duration of six years. Controls include socio-demographics such as religion, caste, sex of household head, number of household members, wealth index, rural, and Hindi and Bengali language dummy variables. The sample includes females aged 18–34, born between 1987–2001, from six control states and the treated state of West Bengal, using data from the NFHS-5 (2019–21) individual schedule. The regression includes district and birth-year fixed effects, with standard errors clustered at the district level, and the results are weighted. P-values are reported in parentheses.