# Communal Violence and Educational Attainment: Evidence from the 2002 Gujarat Violence

Meghna Laha*

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**Abstract**

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This paper examines how the long-run education trajectories were impacted by the 2002 Gujarat violence, which is considered one of the most severe episodes of communal conflict in post-independence India. Using individual-level data from the 2019–21 National Family Health Survey (NFHS-5), I implement a difference-in-differences design that exploits variation in exposure to violence across regions and age cohorts. I find that individuals who were of school-going age in Gujarat at the time of the violence completed fewer years of schooling and were significantly less likely to complete higher-secondary education. These adverse effects are substantially larger for women and individuals belonging to minority religious communities. The results are statistically significant and robust to alternative specifications, including different control group definition and a synthetic control approach.

*John E. Walker Department of Economics, Clemson University. Address: 309-C Wilbur O. and Ann Powers Hall. Clemson, SC 29634. E-mail: mlaha@clemson.edu

# 1. Introduction

Educational attainment in developing countries such as India is characterized by persistent disparities across gender, religion, and socioeconomic status. Episodes of large-scale violence can further exacerbate these inequalities by disrupting schooling trajectories, affecting labor-market opportunities, and altering intra-household bargaining power. These effects are likely to be particularly pronounced for girls and for religious minorities, who already face structural disadvantages in access to education and economic opportunities.

This paper contributes to the literature examining how violence shapes socio-economic outcomes, particularly human capital accumulation. A large body of work documents that conflict and crime adversely affect political behavior, civilian well-being, and educational investments (Ang, 2021; Brown & Velásquez, 2017; Cunningham & Gillezeau, 2021; Koppensteiner & Menezes, 2021; Shemyakina, 2011; Williams, 2022). However, much less is known about the long-run consequences of sudden, localized episodes of communal violence, especially in contexts where crimes are systematically underreported.

In this paper, I study the long-run educational consequences of the 2002 Gujarat violence, one of the most severe episodes of communal conflict in post-independence India. Contemporary accounts and subsequent investigations suggest that the scale of the violence was heavily underreported (Ghose & Pandey, 2025; Human Rights Watch, 2002), a pattern that is common in the Indian context and particularly pronounced for crimes involving sexual and gender-based violence.

A substantial literature examines the nature and evolution of Hindu–Muslim riots in India (Varshney, 2002; Wilkinson, 2004), as well as the political and economic determinants of communal violence (Bohlken & Sergenti, 2010; Jha, 2014; Mitra & Ray, 2014). Related work studies the consequences of such violence for electoral outcomes and economic interactions, including voting behavior and intergroup lending (Fisman, Sarkar, Skrastins, & Vig, 2020; L. Iyer & Shrivastava, 2018), as well as marital outcomes (Ghose & Pandey, 2025). To the best of my knowledge, this paper is the first to examine the long-run educational consequences of the 2002 Gujarat violence. Given that this episode disproportionately targeted women and children, the analysis places particular emphasis on age at exposure during

childhood.

Specifically, this paper studies how exposure to the Gujarat violence affects educational attainment in adulthood, with a focus on heterogeneity by gender and religious affiliation. By explicitly exploiting variation in age at exposure, the analysis highlights how identity-targeted violence during critical schooling years can generate persistent and unequal losses in human capital. The paper complements existing research on crime, terrorism, and education in India (Diwakar, 2023, 2024; Sharma & Sunder, 2024) by focusing on long-run outcomes and cohort-based exposure.

Using individual-level data from the 2019–21 National Family Health Survey (NFHS-5), I implement a difference-in-differences design that exploits variation in exposure to violence across age cohorts and regions similar to Duflo (2001). Specifically, I compare individuals who were of school-going age at the time of the violence to older cohorts who had largely completed their schooling, within Gujarat and relative to other Indian states. This cohort-based approach allows me to isolate the causal impact of exposure to violence during critical schooling years on long-run educational outcomes.

The results reveal substantial and persistent educational losses among individuals exposed to the violence during their school-going years. Total years of schooling declined by approximately 0.7 years for girls, compared to 0.2 years for boys. Grade 12 completion rates fell by 9 percentage points for girls and 4 percentage points for boys. The effects are even larger for religious minorities: the loss in total years of schooling is nearly three times greater for Muslims than for Hindus. Disaggregated estimates show that both Hindu and Muslim women experienced the largest setbacks, with Muslim women losing approximately 1.4 years of schooling and both groups exhibiting declines of nearly 10 percentage points in Grade 12 completion. Event-study estimates indicate that these effects are strongest for individuals exposed during primary school ages (5–10 years), followed by exposure during upper-primary and secondary school ages, highlighting the importance of age at exposure.

The paper also relates to a growing literature examining how violence alters marital outcomes (Field & Ambrus, 2008; Ghose & Pandey, 2025; Sarkar, 2024). I explore potential mechanisms underlying the observed educational losses and find that cohorts exposed to violence are more likely to marry at younger ages, including below the legal minimum

age, consistent with early marriage serving as a household response to insecurity and economic distress. Additional evidence suggests a reallocation of household resources away from education and a disproportionate deprioritization of girls' schooling, consistent with prior findings in the literature (Diwakar, 2024; P. Singh & Shemyakina, 2016). While the paper does not attempt to disentangle these mechanisms fully, the evidence suggests that they are plausible channels through which violence affects long-run educational outcomes.

A series of robustness checks support the validity of the findings. Estimates remain stable when using alternative control group definitions, including restricting comparisons to neighboring states, and when employing a synthetic control approach to construct a counterfactual trajectory for Gujarat. Together, these results underscore the substantial human capital costs of large-scale violence and highlight how such shocks disproportionately burden women, children, and religious minorities.

The remainder of the paper is organized as follows. Section 2 provides background on the Gujarat violence. Sections 3 and 4 describe the data and empirical strategy. Section 5 presents the main results. Sections 6 examine potential mechanisms. Section 7 discusses robustness checks, and Section 8 concludes.

## 2. Background: Gujarat Violence, 2002

India has a long history of communal violence involving both Hindu and Muslim communities. Large-scale religious conflict accompanied the Partition of India in 1947, leaving deep social and political scars that have continued to shape intergroup relations in the post-independence period. Since then, communal violence has recurred episodically in the form of riots, often concentrated around religious festivals, electoral cycles, and disputes over places of worship (Wilkinson, 2004), (Varshney, 2002), (Bohlken & Sergenti, 2010), (S. Iyer & Shrivastava, 2018). While such episodes are typically localized and short-lived, prior research shows that they can generate persistent economic, social, and political consequences.

The Gujarat riots of 2002 represent one of the most severe episodes of communal violence in post-independence India in terms of scale, geographic spread, and brutality. The violence was triggered on February 27, 2002, when a train carrying Hindu activists was set on fire

in the town of Godhra, resulting in the deaths of 58 passengers. In the days that followed, widespread retaliatory violence was directed primarily against Muslim communities across the state. Unlike many earlier riots in India, which were largely confined to urban areas, the 2002 violence spread extensively into rural regions, significantly expanding its reach and potential long-run impacts (Jaffrelot, 2011).

Official government figures report approximately 1,000 deaths, but independent investigations and human rights organizations estimate that close to 2,000 Muslims were killed in the months following the initial incident, with many more injured (Amnesty International, 2005). In addition, more than 200,000 Muslims were displaced due to the systematic destruction of homes, businesses, and places of worship, leading to long-lasting economic dislocation and residential segregation (Srivastava, 2011). Subsequent inquiries documented serious failures in policing, investigation, and prosecution, raising concerns about widespread underreporting of crimes.

Women were disproportionately affected during the Gujarat violence. Numerous reports document the use of sexual violence, including rape and gang rape, as a tool of intimidation and collective punishment (Hameed, Sheikh, & Setalvad, 2002; Human Rights Watch, 2002). While Muslim women were the primary targets, evidence indicates that Hindu women were also victimized in some instances (Farasat & Jha, 2016). Many cases of sexual violence went unreported due to fear of retaliation, social stigma, and a lack of trust in state institutions.

Existing research emphasizes that sexual violence during communal conflict serves not only as a form of physical harm but also as a mechanism for generating fear, reinforcing ethnic boundaries, and disrupting social reproduction (Das, 2007). Exposure to such violence can have enduring consequences for women's mental health, mobility, household bargaining power, and life-course decisions. These gendered dynamics suggest that episodes of communal violence may have particularly severe and persistent effects on girls and women who are exposed during critical periods of their development.

# 3. Data

This paper primarily uses data from the fifth round of the National Family Health Survey (NFHS-5), conducted in India between 2019 and 2021. NFHS-5 is part of the Demographic and Health Surveys (DHS) program and provides nationally representative data at the district, state, and national levels. The survey covers 707 districts across 28 states and 8 union territories and collects detailed information through household, women's, and men's questionnaires. The data consist of repeated cross-sections and are designed to be representative of India's population. The primary analysis relies on data from the Household Questionnaire, which records information on all usual household members as well as visitors who stayed in the household on the night prior to the interview. This questionnaire provides detailed demographic characteristics and educational attainment for each individual, enabling the construction of long-run educational outcomes for cohorts exposed to violence at different ages.

The survey reports respondents' current age at the time of interview. Using this information, I calculate each individual's age in 2002, the year of the Gujarat violence. The main sample consists of individuals who were between 5 and 35 years old in 2002. Applying these restrictions yields a final sample of 1,134,707 observations (see Table 1). This age range allows for a comparison between cohorts plausibly exposed to violence during their school-going years and cohorts who had largely completed their education prior to the violence. Accordingly, I define two cohorts central to the identification strategy: a school-going cohort (ages 5–23 in 2002) and an older cohort (ages 24–35 in 2002), who were unlikely to have been enrolled in formal schooling at the time of the violence.

NFHS-5 is particularly well-suited for this analysis for several reasons. First, its large sample size allows for precise estimation, including for treated groups and subgroup analyses. Second, its later survey timing provides more observations of cohorts that have completed schooling, enabling the study of long-run educational outcomes. Third, its wide geographic coverage and rich information on educational attainment, marital outcomes and fertility make it possible to examine both primary outcomes and underlying mechanisms.

The primary educational outcomes of interest are (i) total years of schooling, measured

as completed years of formal education ranging from 0 to 12, and (ii) an indicator for completion of Grade 12. NFHS-5 provides direct information on both measures.

To examine potential mechanisms, I complement the main analysis with additional data sources. I use the NFHS-5 women's questionnaire (2019–21) to analyze marital and fertility outcomes for both men and women, including age at first marriage, age at first cohabitation, and age at first birth. These variables allow me to assess whether exposure to violence during school-going years altered life-course transitions in ways that plausibly contributed to observed educational losses. In addition, I use household data from earlier survey rounds—NFHS-2 (1998–99) and NFHS-3 (2005–06)—to study short-run educational outcomes closer to the period of the violence.

Table 2 presents summary statistics for the older cohort in Gujarat relative to the rest of India, while Table 3 reports analogous statistics for the school-going cohort. Table 4 compares the older and school-going cohorts within Gujarat.

# 4. Empirical Strategy

## 4.1 Identification

To identify the causal effect of communal violence on long-term educational outcomes, I exploit the 2002 Gujarat violence as an exogenous event. The three days period of intense inter-religious violence was triggered by the burning of the *Sabarmati Express* at Godhra. While episodes of religious violence have occurred intermittently across India, the specific timing, geographic concentration, and magnitude of the 2002 violence in Gujarat were largely unanticipated. (Varshney, 2020).

Although the violence was widespread, it was geographically confined to the state of Gujarat and varied substantially in intensity across locations within the state (Jha, 2014); (Varshney, 2020). For instance, many towns and cities such as that of Surat's old city were completely insulated from the nearby cities of Vadodara and Ahmedabad that experienced severe violence. (Varshney, 2020)

The unexpected and localized nature of the 2002 Gujarat Violence makes it appropriate

for analyzing the impacts of communal violence. (Ghose & Pandey, 2025).To estimate these effects, I compare educational outcomes across cohorts differentially exposed to the violence based on their age in 2002. Specifically, I compare individuals who were of school-going age at the time of the riots with older cohorts who had plausibly completed their schooling prior to 2002 similar to (Duflo, 2001).

The identifying assumption is that, in the absence of communal violence, age-specific educational trajectories in Gujarat would have evolved similarly to those in the rest of India. I assess the plausibility of this parallel trends assumption using an event-study specification, complemented by plots of raw mean outcomes over time (Figures A.1 & A.2)

The event-study specification is given by:

$$Y_{isa} = \alpha + \sum_{\substack{j=5 \\ j \neq 24}}^{35} \delta_j \cdot (\mathbf{1}[\text{Age}_i = j] \times \text{Riot}_s) + \gamma_a + \theta_s + \varepsilon_{isa} \tag{1}$$

where $Y_{isa}$ denotes the educational outcome—Grade 12 completion or total years of education—for individual $i$ in state $s$ and age $a$. $\mathbf{1}[\text{Age}_i = j]$ is an indicator for being age $j$ in 2002, with age 24 omitted as the reference cohort. $\text{Riot}_s$ is an indicator equal to one if the individual resides in Gujarat. $\gamma_a$ and $\theta_s$ denote age and state fixed effects, respectively. Standard errors are clustered at the state level.

I discuss the choice of the reference cohort and the definition of treatment and control groups in greater detail in the next section.

In the main specification, I aggregate cohorts into broader age bins to improve precision. Under this aggregation, the identifying assumptions appear even more plausible.

## 4.2 Choice of Treatment and Control Groups

I treat the 2002 Gujarat violence as an exogenous shock, defining Gujarat as the treated state and using the rest of India, as well as, in robustness checks, neighboring states or a synthetic control constructed from other states, as the control group, following the approach in (Ghose & Pandey, 2025).

As discussed in (Ghose & Pandey, 2025), a within-state analysis comparing districts that experienced higher versus lower violence intensity raises two important concerns. First, Gujarat exhibits high levels of intra-state migration: approximately 84.6% of migration occurs within the state, while only 14.6% is inter-state. Migration is primarily driven by employment for men and marriage for women. Since, the NFHS household survey does not provide information on migration histories, a district-level analysis within Gujarat would be unable to account for spillovers between treated and control districts arising from intra-state mobility.

Second, the violence was geographically widespread, affecting 16 out of 24 districts in Gujarat. This limits the availability of credible within-state control districts that were plausibly unexposed to the violence. Taken together, these considerations motivate the use of Gujarat as the treated unit and support a state-level research design as the most appropriate approach.

## 4.3 Choice of Pre-Treatment and Post-Treatment Periods

As discussed in Section 4.1, I exploit variation in individuals' age at the time of the 2002 Gujarat violence to capture differential exposure to the shock. Since, the violence occurred at a single point in time, exposure is determined entirely by whether an individual was still of school-going age in 2002.

Individuals who were young enough in 2002 for their educational trajectories to be affected by the violence are classified as belonging to the post-treatment group. In contrast, individuals who were sufficiently old to have plausibly completed their schooling by 2002 are classified as belonging to the pre-treatment group and are therefore unexposed.

The analysis focuses on two educational outcomes: (i) total years of schooling, which ranges from 0 to 12 and captures completed schooling up to the high school level, and (ii) a binary indicator for the completion of Grade 12.

In India, children typically begin school between ages 5 and 6[1] and, in the absence of

[1]Under the National Education Policy (NEP) 2020, children are required to be six years old at the time of admission to Class 1. Prior to 2020, admission ages varied across states, with children typically entering Grade 1 at age five or six depending on state-specific cut-off dates and birth timing.

delays, complete Grade 12 by age 18. However, the literature and statistical reports suggest that there are significant delays in completing higher-secondary schooling in India. To characterize the age profile of completion, Figure A.3 plots the cumulative distribution function (CDF) of age at Grade 12 completion using NFHS-5 data. The distribution rises sharply between ages 16 and 19 but continues to increase through the early twenties, with completion rates plateauing only around age 23 for both boys and girls. While most individuals complete Grade 12 by age 19, a meaningful share does so at later ages. This pattern is consistent with Figures A.4 and A.5, which show that a non-trivial fraction of individuals remain enrolled in school and in Grade 12 beyond age 19. Evidence from NFHS-2 (1998–99), shown in Figure A.6, further confirms that completion continues to rise after age 18 before flattening at older ages.

To ensure that observed outcomes reflect completed education rather than ongoing enrollment, I restrict the sample to individuals who were between 5 and 35 years old in 2002. Given the timing of the NFHS-5 survey (2019–2021), this implies that the youngest individuals in the sample are at least 22 years old at the time of observation and are therefore likely to have completed their schooling.

Under this framework, individuals who were 23 years old or younger in 2002 are classified as exposed (post-treatment), as their educational trajectories could have been affected by the 2002 violence. In contrast, individuals who were 24 years or older in 2002 are classified as unexposed (pre-treatment), as they would have plausibly completed their schooling prior to the violence.

To capture variation in exposure intensity, I further distinguish between cohorts within the exposed group. Individuals aged 5–18 in 2002 are classified as fully treated, as they were within the typical school-going ages at the time of the shock. Individuals aged 19–23 in 2002 are classified as partially treated, reflecting delayed schooling completion documented in the data. Individuals aged 24–35 in 2002 serve as the untreated comparison group.

## 4.4 Main Specification

To estimate the causal effect of the 2002 Gujarat violence, I employ a difference-in-differences framework complemented by an event-study analysis. A key challenge in estimating event-

study effects at the single-year age level is that reported ages in the NFHS(consistent with other Indian household surveys) exhibit substantial age heaping. As shown in Figures A.7b and A.8b, there are pronounced spikes at ages ending in 0 or 5, reflecting digit preference in age reporting (M. Singh, Kashyap, & Bango, 2022).

Because age in 2002 is constructed from reported age in the 2019–2021 survey, this heaping mechanically translates into uneven cohort sizes in the inferred 2002 age distribution (Figures A.7a and A.8a). For example, individuals aged 25 in 2019 correspond to age 8 in 2002, while those aged 30 correspond to age 13, leading to disproportionately large cohorts at certain ages. This uneven distribution reduces precision and generates noisy estimates in age-specific event-study specifications. To address this issue and align exposure groups with meaningful stages of schooling, the main specification aggregates individuals into age bins defined by age in 2002. Specifically, I group individuals aged 5–10 into a primary school cohort, ages 11–13 into an upper primary cohort, ages 14–16 into a secondary cohort, and ages 17–19 into a higher secondary cohort. Individuals aged 20–23 are classified as a delayed completion group and are considered partially exposed to the violence.

As the reference category, I use individuals aged 24–27 in 2002, who would have plausibly completed their schooling prior to the shock. Individuals aged 28–31 and 32–35 serve as older, unexposed cohorts and provide additional comparison groups.

I estimate the following event study specification:

$$Y_{isa} = \alpha + \sum_{\substack{j=1 \\ j\neq 6}}^{8} \delta_j \cdot \left(\mathbf{1}\{\text{AgeBin}_i = j\} \times \text{Violence}_s\right) + \lambda_b + \theta_s + \varepsilon_{isa} \tag{2}$$

where, $Y_{isa}$ is the education outcome for individual $i$ in state $s$ and age-bin $a$; $\mathbf{1}\{\text{AgeBin}_i = j\}$ is an indicator for age bin $j$ in 2002; $\text{Violence}_s$ equals one if the individual resides in Gujarat; $\lambda_b$ & $\theta_s$ are age-bin fixed effects and state fixed effects, respectively. The coefficients $\delta_j$ capture the differential effect of exposure to the Gujarat violence for individuals in age bin $j$ relative to the baseline bin. Standard errors are clustered at the state level.

To summarize the average effect of exposure, I additionally estimate the following

difference-in-differences specification:

$$Y_{isa} = \beta_0 + \beta_1 \cdot \text{SchoolAge}_a + \beta_2 \cdot \text{Riot}_s + \beta_3 \cdot (\text{SchoolAge}_a \times \text{Riot}_s) + \delta_a + \gamma_s + \varepsilon_{isa} \quad (3)$$

where $Y_{isa}$ denotes the educational outcome—Grade 12 completion, Total years of School for individual $i$ residing in state $s$ and belonging to age cohort $a$. $\text{SchoolAge}_a$ is an indicator equal to one if the individual was aged 23 or younger in 2002 and therefore potentially exposed to the violence, and zero otherwise. $\text{Riot}_s$ is an indicator equal to one if the individual resides in Gujarat. $\delta_a$ and $\gamma_s$ denote age-cohort and state fixed effects, respectively. The coefficient $\beta_3$ identifies the average effect of exposure to the 2002 Gujarat violence. Standard errors are clustered at the state level.

### 4.5 Heterogeneity

Next, I conduct a series of heterogeneity analyses to examine whether the effects of the 2002 Gujarat violence vary systematically across social groups. Specifically, I re-estimate Equations (2) and (3) separately for males and females to assess gender-specific impacts. I then extend this analysis by estimating the same specifications for Hindu and Muslim populations and further disaggregating these effects by gender. This allows me to examine differential exposure and vulnerability along both religious and gender dimensions.

## 5. Results

### 5.1 Total Years of School

The impact of the 2002 Gujarat violence on total years of School, estimated using the difference-in-differences specification in Equation (3), is reported in Column (1) of Table 5. The coefficient on the interaction term indicates that exposure to the violence reduced total years of schooling by approximately 0.5 years, on average, for individuals who were of school-going age in 2002, relative to older cohorts in Gujarat and to comparable cohorts in the rest of India. Relative to the sample mean of 6.97 years, this corresponds to a decline of roughly

7 percent, indicating a sizable reduction in total years of schooling.

Figure 1 provides complementary graphical evidence using raw means and the binned event-study specification in Equation (2). Figure 1a plots average years of schooling by age-bin for Gujarat and the rest of India. For older, untreated cohorts, the trajectories in Gujarat closely track those in the rest of the country, with Gujarat showing a higher average total years of schooling. This suggests similar pre-treatment trends despite level differences. In contrast, for younger cohorts, average educational attainment in Gujarat declines relative to the rest of India, with the divergence becoming more pronounced for cohorts exposed to the violence at earlier stages of schooling.

Figure 1b reports the corresponding event-study coefficients, normalized to the baseline age bin of 24–27. For older, untreated cohorts, the coefficients are small, statistically insignificant, and centered around zero, providing support for the parallel trends assumption. Estimates for the partially treated cohort (ages 20–23) are negative but modest in magnitude, consistent with partial exposure during the later stages of schooling. In contrast, for fully treated cohorts, the coefficients are large, negative, and statistically significant, with magnitudes increasing monotonically as age at exposure decreases. The largest effects, nearly 0.6 - 0.8 years, are observed for individuals who were of primary school age at the time of the riots.

## 5.2 Total Years of Education by Gender & Religion

Table 6 reports heterogeneous effects of the 2002 Gujarat violence on total years of education by gender. Columns (1) and (2) present estimates separately for males and females. Column (1) shows that exposed males experience a reduction of approximately 0.2 years of schooling, corresponding to a 2.5% decline relative to the male sample mean of 7.98 years. Column (2) shows a larger effect for females: exposure reduces total years of education by 0.68 years, which corresponds to an 11.3% decline relative to the female sample mean of 6.03 years. These estimates indicate that the decline in educational attainment is larger for females than for males.

Table 7 examines heterogeneity by religious affiliation. Column (1) shows that Hindu individuals experience a reduction of approximately 0.4 years of schooling, corresponding to

a 5.7% decline relative to the sample mean. In contrast, Column (2) shows a larger reduction for Muslim individuals of approximately 1.2 years, or 18.9% relative to the baseline mean. All estimates are statistically significant at the 1% level.

Figures 2a and 2b present raw mean outcomes and event-study estimates of total years of schooling by age bin, estimated separately for males and females using (2). Figure 2a shows that average years of schooling are lower for females than for males in both Gujarat and the rest of India across all age bins. For both genders, average schooling declines for cohorts exposed to the violence during their school-going years. The raw means further indicate that the gender gap in educational attainment widens for younger cohorts in Gujarat, while it narrows for comparable cohorts in the rest of India.

Figure 2b reports the corresponding event-study coefficients. For older, untreated cohorts, the estimates are close to zero for both males and females. The partially treated cohorts exhibit small negative coefficients. For fully treated cohorts, the decline in total years of schooling increases in magnitude for younger age bins, with larger negative estimates for females than for males.

Figures 3a and 3b present the event-study results from Equation (2), stratified by religious affiliation. Panel (a) displays the raw cohort means. The figure indicates that, in Gujarat, the gap in total years of education between Hindu and Muslim individuals widens for younger cohorts, whereas in the rest of India the Hindu–Muslim gap remains relatively stable over time. Figure 3b reports the corresponding event-study coefficients. The estimates show declines in total years of education for both religious groups among cohorts exposed during school-going ages; however, the magnitude of these declines is substantially larger for the Muslim population. This pattern suggests that exposure to the violence had a disproportionately adverse effect on the educational attainment of Muslims, consistent with the heterogeneous impacts documented in the baseline results.

Table 8 further disaggregates these effects by gender within each religious group. Among Hindus, Column (1) shows a reduction of 0.2 years of schooling for males, statistically significant at the 10% level, while Column (2) shows a larger decline of approximately 0.6 years for females. Columns (3) and (4) show that the effects are statistically significant at the 5% level for both Muslim males and females. Muslim males experience a reduction of approximately

0.8 years of schooling, while Muslim females experience the largest decline, at approximately 1.4 years.

Figures 4 and 5 present the raw-means and event-study coefficients from Equation (2), further disaggregated by gender for Hindu and Muslim populations. The estimates are consistent with the preceding results: effects are close to zero for older cohorts and increasingly negative for younger cohorts, with larger magnitudes for females. Within the Muslim population, the largest negative effects are observed for female cohorts exposed at younger ages.

Taken together, these results show that the reduction in educational attainment associated with exposure to the 2002 Gujarat violence varies substantially across gender and religious groups. The estimated declines are larger for females than for males and larger for Muslims than for Hindus, with the largest effects observed among Muslim women.

### 5.3 Grade 12 Completion

The second outcome variable is a binary indicator for the completion of Grade 12 (higher secondary education). In India, compulsory schooling extends only up to Grade 8; completion of Grade 12 therefore captures persistence into post-compulsory education and reflects sustained household investment in schooling. Moreover, Grade 12 completion is a prerequisite for entry into higher education and is the minimum qualification required for many clerical and entry-level government jobs (Government of India, 2020).

Column (1) of Table 10 reports the difference-in-differences estimate for Grade 12 completion. Exposure to the 2002 Gujarat violence reduces the probability of completing Grade 12 by 5.9 percentage points. Relative to a baseline completion rate of 26 percent, this corresponds to a decline of approximately 22.6 percent, and the estimate is statistically significant.

Figure 6a & 6b provides complementary graphical evidence. Figure 6a plots raw mean Grade 12 completion rates by age bin for Gujarat and the rest of India. For older, unexposed cohorts, completion rates in Gujarat closely track those in the rest of the country, indicating similar pre-treatment trends. For younger cohorts, the gap between Gujarat and the rest of India widens monotonically, with lower completion rates observed in Gujarat for cohorts exposed to the violence during school-going ages.

Figure 6b presents the corresponding event-study coefficients estimated using Equa-

tion (2). The estimates for older, untreated cohorts are close to zero. The partially treated cohorts and cohorts near the completion margin exhibit small negative coefficients. In contrast, the coefficients for younger, fully treated cohorts are larger in magnitude and statistically significant, indicating substantially lower probabilities of completing Grade 12 for individuals exposed at earlier schooling stages.

Taken together, the table and figure indicate that exposure to the 2002 Gujarat violence significantly reduced Grade 12 completion, with the largest effects concentrated among cohorts exposed at younger ages.

## 5.4 Grade 12 Completion by Gender & Religion

Next, I examine heterogeneity in the impact of the 2002 Gujarat violence on Grade 12 completion. Table 11, Columns (1) and (2), report estimates separately for males and females. Exposure to the violence reduces the probability of completing Grade 12 by 4.5 percentage points for males and by 7.2 percentage points for females. Given that females already exhibit lower baseline rates of high school completion, these estimates indicate that exposure to violence further widens pre-existing gender disparities in educational attainment.

Figures 7a and 7b provide complementary graphical evidence. Figure **??** plots raw mean Grade 12 completion rates by age bin and shows a decline for both males and females in Gujarat among treated cohorts. Figure 7b presents the corresponding event-study coefficients. Two patterns emerge. First, the estimated effects are consistently larger in magnitude for females than for males. Second, the negative effects increase for younger cohorts, indicating greater sensitivity to exposure at earlier schooling stages. For the youngest female cohorts, the decline in Grade 12 completion is approximately 12 percentage points, substantially larger than the average effect.

Table 8, Columns (3) and (4), report difference-in-differences estimates for Hindus and Muslims. Exposure to the violence reduces Grade 12 completion by approximately 5.4 percentage points for Hindus and 7.1 percentage points for Muslims, yielding comparable magnitudes but slightly more negative impact for Muslims.

Table 13 further dis-aggregates these effects by gender. Among males, Hindu individuals experience a statistically significant decline of 4 percentage points, while the estimate for

Muslim males 5 percentage points but not statistically significant. In contrast, the estimated effects for females are larger across religious groups, with Grade 12 completion declining by roughly 6.7 percentage points for Hindu and by 9 percentage points for Muslim.

Figures 8a and 8b provide graphical evidence for these patterns. Figure 8a plot raw mean Grade 12 completion rates for Hindus and Muslims. While there are clear level differences between the two groups, trends are broadly similar, with a flattening in completion rates for treated cohorts in both populations. Figure 8b presents the corresponding event-study coefficients, which indicate relatively larger declines for the Muslim population, with the largest effects for cohorts exposed during primary and upper-primary ages. The confidence intervals are wider, particularly for Muslims, reflecting smaller sample sizes for this outcome.

Finally, Figures 9 and 10 disaggregate the event-study estimates by both gender and religion. These figures show that, within both Hindu and Muslim populations, females experience larger declines in Grade 12 completion than males, with the largest effects concentrated among younger cohorts.

Taken together, these results indicate substantial gender and religious heterogeneity in the impact of the 2002 Gujarat violence on Grade 12 completion, similar to the effects on total years of schooling.

# 6. Discussion of Potential Transmission Mechanisms

This section investigates the mechanisms that may drive the persistent decline in long-run human capital accumulation and situates the observed heterogeneous effects within the broader literature. While the data do not allow for a clear disentanglement of the underlying mechanisms, the results provide suggestive evidence of their relevance.

## 6.1 Short-Run Changes in Education Outcomes

To examine the mechanisms underlying the long-run education losses observed for the 2002 school-age cohort, I analyze short-run schooling outcomes using data from the National Family Health Surveys (NFHS-2: 1998–99 and NFHS-3: 2005–06). The timing of these survey rounds enables a comparison of schooling outcomes before and after the 2002 violence,

allowing a closer focus on the immediate period surrounding the shock. I examine grade completion at key educational thresholds within the school-age population (ages 5–23). In particular, primary school progression is measured by Grade 5 completion among individuals aged 11–15, reflecting successful completion of the primary cycle, while middle school transition is captured by Grade 8 completion among individuals aged 14–17, indicating entry into lower secondary education. Finally, Grade 12 completion among individuals aged 18–23 is used to assess progression into higher secondary education. These measures allow me to assess the immediate impact of the violence on schooling trajectories.

I next examine delays in grade progression to better understand disruptions in schooling trajectories. Grade 5 delay is defined for individuals aged 13 or older who have not completed Grade 5, capturing slower progression or grade repetition during the primary school years. Grade 8 delay is defined for individuals aged 15 or older who have not completed Grade 8, reflecting disruptions around the transition to secondary education. Grade 12 delay is defined for individuals aged 19 or older who have not completed Grade 12, capturing delays in completing higher secondary education. The sample is restricted to individuals aged 23 or younger. Together, these measures allow me to assess whether exposure to violence led not only to lower completion rates but also to delays in educational progression across different stages of schooling.

To estimate the short-run effects, I implement a difference-in-differences specification comparing outcomes in Gujarat to those in other states before and after the 2002 riots. The estimating equation is given by

$$Y_{ist} = \alpha_s + \lambda_t + \beta(Post_t \times Gujarat_s) + \varepsilon_{ist} \tag{4}$$

where $Y_{ist}$ denotes the schooling outcome for individual $i$ in state $s$ and survey year $t$. The term $\alpha_s$ represents state fixed effects and $\lambda_t$ denotes survey fixed effects. The indicator $Post_t$ equals one for NFHS-3 (2005–06) and zero for NFHS-2 (1998–99), while $Gujarat_s$ identifies individuals residing in Gujarat. The coefficient of interest, $\beta$, captures the differential change in schooling outcomes in Gujarat after the riots relative to other states. Standard errors are clustered at the state level. All specifications are estimated on age-eligible samples for the

respective outcomes and are further stratified by gender and religion.

Table 15 presents difference-in-differences estimates of grade completion across key schooling thresholds and reveals a clear pattern of heterogeneous effects. For males, exposure to violence is associated with a significant increase in Grade 5 completion (4.3 percentage points), no statistically significant effect at Grade 8, and a substantial decline in Grade 12 completion (7.3 percentage points). For females, the gains at the primary level are smaller (3.8 percentage points) and are followed by significant declines at higher levels—4 percentage points at Grade 8 and 5.6 percentage points at Grade 12, highlighting gendered differences in responses. A similar pattern emerges across religious groups: Hindu individuals experience a positive effect at Grade 5 (4 percentage points) but negative effects at higher levels, particularly at Grade 12 (5.5 percentage points). In contrast, Muslim individuals experience negative and statistically significant effects across all schooling thresholds, including a 3.9 percentage point decline in Grade 5 completion, an 11.1 percentage point decline at Grade 8, and a 7.7 percentage point decline at Grade 12. Taken together, these results suggest that higher levels of schooling were more severely disrupted following the violence. While primary completion appears relatively resilient, the consistently negative effects at secondary and higher secondary levels indicate a reduction in progression beyond primary schooling, pointing to increased costs of or reduced investments in continued education in the post-violence period. Moreover, the smaller gains for females and the uniformly negative effects for Muslims, even at the primary level highlight important gender and religious disparities in how educational trajectories were affected.

Table 16 complements these findings by examining delays in grade progression and shows a consistent pattern of positive and statistically significant effects, indicating widespread disruptions in schooling trajectories. For males, delays are concentrated at higher levels, with a 4 percentage point increase at Grade 8 and an 8.9 percentage point increase at Grade 12, while effects at Grade 5 are small and not statistically significant. A similar pattern is observed for females, with insignificant effects at the primary level but significant increases of 5 percentage points at Grade 8 and 5.7 percentage points at Grade 12. Among Hindus, delays are again concentrated at higher grades, with significant increases of 3.2 percentage points at Grade 8 and 6.3 percentage points at Grade 12. In contrast, Muslim individuals

experience large and statistically significant increases in delays across all schooling stages, including a 5.4 percentage point rise at Grade 5, 13.9 percentage points at Grade 8, and 8.2 percentage points at Grade 12.

Taken together, the completion and delay results point to a coherent mechanism: while initial entry and completion at the primary level may have been relatively protected, progression through higher levels of schooling was substantially disrupted. The increase in delays, particularly at secondary and higher secondary stages suggests that students faced interruptions that slowed their educational trajectories, likely through grade repetition, temporary withdrawal, or irregular attendance. These delays can accumulate over time, increasing the probability of eventual dropout and reducing effective learning. The fact that these disruptions are more pronounced at higher grades, and among disadvantaged groups such as females and Muslims, is consistent with a framework in which the costs of continuing education rise following exposure to violence. Ultimately, these short-run disruptions in progression and completion provide a plausible pathway through which violence translates into the persistent declines in long-run human capital documented earlier.

## 6.2 Household Re-allocation of Wealth

Violent events can affect educational outcomes through multiple channels, one of which is the destruction of household assets and the resulting income shocks. In response to such shocks, households may reallocate resources across children, with educational investments in boys and girls potentially acting as substitutes (P. Singh & Shemyakina, 2016). Consistent with this mechanism, Diwakar (2024) show that in districts more exposed to Naxalite violence, household expenditure on education increases in all-boys and mixed-gender households but declines in all-girls households. These findings suggest that during periods of conflict, households may deprioritize investments in girls' education, contributing to larger educational losses for girls relative to boys. In addition, violence may reduce the perceived returns to education for girls relative to boys due to heightened insecurity, mobility constraints, early marriage considerations, and persistent patriarchal norms in the Indian context.

While directly testing this mechanism is not feasible in the present study due to the absence of household expenditure data in the NFHS and the mismatch between the timing

of observed household characteristics (2019–21) and the period of exposure (2002), I provide suggestive evidence by examining heterogeneity in educational outcomes by household composition. Using the same specification as in (4), I estimate effects on Grade 5, 8, and 12 completion separately for children in mixed-gender households (those with at least one boy and one girl aged 5–23) and single-gender households (those with only boys or only girls in the same age range). The results, presented in Table 17, reveal a consistent pattern of negative effects at higher levels of schooling across all household types, reinforcing the earlier finding that progression beyond primary education is particularly vulnerable to disruption. In mixed-gender households, both boys and girls experience gains in Grade 5 completion (4.3 and 5 percentage points, respectively), but face declines at higher levels, particularly at Grade 12 (4.8 percentage points for boys and 4.4 percentage points for girls). In single-gender households, boys in boys-only households exhibit a similar pattern of primary gains (4.3 percentage points) but substantially larger declines at Grade 12 (10.5 percentage points). Most notably, girls in girls-only households experience negative and significant effects across all schooling thresholds, including a 1.9 percentage point decline at Grade 5, a sharp 14.5 percentage point decline at Grade 8, and a 7.7 percentage point decline at Grade 12, making them the most vulnerable group.

Taken together, these results suggest that while higher-grade disruptions are widespread, the burden is not evenly distributed. The particularly severe and pervasive losses among girls in girl-only households point to the importance of intra-household resource constraints and allocation decisions, and are consistent with a setting in which adverse shocks disproportionately reduce investments in girls' education.

## 6.3 Marital Outcomes of Women & Men

A growing body of literature shows that exposure to violence and higher crime incidence increases the likelihood of early marriage, particularly for women (Ghose & Pandey, 2025; Sarkar, 2024). At the same time, early marriage is strongly associated with school dropout and lower educational attainment (Field & Ambrus, 2008; Wilson, 2022). Using the 2002 Gujarat violence, Ghose and Pandey (2025) document that women exposed to the shock were more likely to marry earlier, including before the legal minimum age, and subsequently

attained lower levels of education. Motivated by this evidence, I examine early marriage as a potential mechanism linking exposure to violence during school-going years to long-run educational losses. Using the identification strategy described in Section 5, I analyze marital outcomes for cohorts who were of school-going age in 2002, comparing Gujarat to other states. I focus on age at first marriage, age at first cohabitation, age at first sexual intercourse, age at first birth, and the likelihood of marrying before age 18. For women, I use NFHS-5 women's data and construct age at first marriage using reported year of marriage and year of birth.

The results provide suggestive evidence of shifts toward earlier marriage among exposed cohorts. In the full sample, women in Gujarat are 3.5 percentage points more likely to marry before age 18, although changes in the timing of marriage and related outcomes are small and not statistically significant. Dis-aggregating by religion reveals stronger effects for Hindu women, who experience a significant decline in age at first marriage (approximately 0.3 years) and a 5 percentage point increase in the likelihood of marrying before age 18, along with earlier cohabitation and age at first sex. For Muslim women, the estimates are generally negative but imprecisely estimated, with limited statistical significance.

I find similar patterns for men: Hindu men exposed to violence marry earlier and are 4.9 percentage points more likely to marry before age 18, while effects for Muslim men are not statistically significant.

Overall, these findings are consistent with early marriage acting as a potential channel through which violence affects educational trajectories, particularly for certain groups. However, given data limitations, these results should be interpreted as suggestive rather than causal. Supporting evidence, including event-study estimates and raw trends, is presented in the Additional Figures section. (Figures A.9–A.14).

# 7. Robustness Checks

In this section, I address concerns regarding the validity of the control group, the binning specifications and employ alternate estimation techniques to establish robustness of the results.

## 7.1 Gujarat and the Neighboring States

Gujarat differs from other parts of India along several observable dimensions: it is relatively wealthier, has a higher share of the Hindu population, and exhibits distinct social norms, as documented in Tables 2 and 3. These differences raise concerns that the baseline estimates may be driven by structural dissimilarities between Gujarat and the rest of the country rather than by exposure to violence.

To address this concern, I perform a robustness check in which I re-estimate equation (2) using an alternative control group consisting only of states that share a geographic boundary with Gujarat. Restricting the control group to neighboring states helps ensure greater comparability in economic conditions, demographics, and social norms due to geographic proximity. Similar approaches using neighboring regions as alternative control groups have been employed in prior work (e.g., Anukriti, 2018; Ghose & Pandey, 2025). Following Ghose and Pandey (2025), the neighboring-state control group includes Madhya Pradesh, Maharashtra, and Rajasthan.

Figures 11a and 11b present the event-study coefficients from equation (2) for total years of education and Grade 12 completion, respectively. Figure 11a shows that cohorts exposed to violence during school-going years experience persistent reductions in total years of education relative to cohorts in neighboring states. Figure 11b displays consistently negative point estimates for Grade 12 completion among treated cohorts; however, the estimates are imprecisely estimated, with wide confidence intervals, likely reflecting the smaller sample size in this restricted comparison.

## 7.2 Synthetic Controls Approach

The event-study results—particularly the non-binned, age specific estimates shown in Figures A.1b and A.2b, suggest the possibility of differential pre-trends between Gujarat and the rest of India, even among older cohorts that were unlikely to be directly exposed to the violence. To address this concern and assess the robustness of the baseline results, I complement the event-study analysis with a synthetic control approach following Abadie, Diamond, and Hainmueller (2010).

The synthetic control method constructs a counterfactual trajectory for Gujarat, referred to as Synthetic Gujarat, using a weighted combination of other Indian states drawn from a donor pool. The weights are chosen such that the synthetic control closely matches Gujarat on a set of predictors of educational outcomes. Post-violence differences between observed Gujarat and Synthetic Gujarat are then interpreted as the effect of the 2002 violence under the assumption that, absent the violence, educational outcomes in Gujarat would have followed the trajectory of its synthetic counterpart.

The predictor set includes educational outcomes of the unexposed & older cohorts, the proportion of individuals with zero years of education, the wealth index, the share of the population residing in rural areas, average household size, and the religious composition of the state, measured by the proportions of Hindu and Muslim populations. These variables capture key socioeconomic and demographic factors that are predictive of educational attainment and ensure a close pre-treatment fit between Gujarat and its synthetic control.

Figures 12a and 12b present the results from the synthetic control analysis for Total Years of Schooling and Grade 12 Completion, respectively. In both cases, educational outcomes in observed Gujarat and Synthetic Gujarat track each other closely for cohorts that were older than 23 years in 2002—those unlikely to have been exposed to the violence during their schooling years. For younger cohorts, however, outcomes in Gujarat diverge sharply and persistently below those of Synthetic Gujarat. This pattern mirrors the event-study estimates and provides additional support for the interpretation that the observed educational losses are attributable to exposure to the 2002 violence rather than to differential pre-existing trends.

## 7.3 Leave One Out Analysis

To examine whether the results are driven by any particular state or district within Gujarat, I conduct a leave-one-out robustness exercise. Specifically, I repeatedly re-estimate the main specification in equation (2), each time excluding one state from the sample. Figures 13a and 13b present the resulting estimates for Total Years of Schooling and Grade 12 completion, respectively, when each state is excluded in turn. Figures 16a and 16b perform a similar exercise by excluding one district at a time.

The coefficients remain stable across all iterations and are quantitatively similar to the baseline estimates, indicating that the results are not driven by any single state or district. Gujarat is numerically coded as 24 and therefore does not appear as a separate coefficient in the figures.

Next, I assess whether the results are sensitive to the inclusion of particular age cohorts. Specifically, equation (1) is re-estimated while sequentially excluding one age cohort at a time. Figures 14a and 14b show that the estimated coefficients remain stable across all leave-one-out iterations. Finally, I re-estimate the main specification in equation (2) by excluding each age cohort in turn. Figures 15a and 15b demonstrate that the estimates remain stable, further confirming that the results are not driven by any specific age cohort.

## 7.4 Dropping Spiked Ages

I re-estimate the main specification given by (2) after excluding ages that exhibit heaping. As discussed earlier and illustrated in the histograms in Figures A.7 and A.8, the age distribution shows noticeable spikes at ages ending in 0 or 5 in the survey year, reflecting age-heaping in reported ages.

To address this concern, I conduct a robustness check by re-estimating the main specification after excluding individuals whose reported ages in the survey year are 25, 30, 35, 40, 45, and 50. For example, if a state was surveyed in 2019, these exclusions correspond to ages 8, 13, 18, 23, 28, and 32 in 2002.

The results, shown in Figures A.16 and A.24, remain broadly consistent with the baseline estimates after excluding these heaped ages. However, because this restriction reduces the sample size, the estimates exhibit wider confidence intervals and some evidence of pre-trend fluctuations. Results for all key stratifications are reported in the Additional Figures section.

## 7.5 Adding Controls

I re-estimate my main specification by including additional controls such as wealth index, place of residence (urban/rural), and household size. The specification is given by:

$$Y_{isa} = \alpha + \sum_{\substack{j=1 \\ j \neq 6}}^{8} \delta_j \left( \mathbf{1}\{AgeBin_i = j\} \times Riot_s \right) + \beta' X_{isa} + \lambda_a + \theta_s + \varepsilon_{isa} \tag{5}$$

where $Y_{isa}$ is the education outcome for individual $i$ in state $s$ and age-bin $a$; $\mathbf{1}\{AgeBin_i = j\}$ is an indicator for age bin $j$ in 2002, with bin 6 serving as the omitted baseline category; $Riot_s$ equals one if the individual resides in Gujarat; and $X_{isa}$ is a vector of individual and household controls including wealth index, place of residence (urban/rural), and household size. $\lambda_a$ and $\theta_s$ denote age-bin fixed effects and state fixed effects, respectively. Standard errors are clustered at the state level.

These controls are included as a robustness check rather than in the main specification because, given my identification strategy, I do not observe these characteristics for individuals at the time of the violence. Since individuals are observed approximately 17 years after the violence occurred, these variables may themselves be influenced by the event and are therefore potentially endogenous. For example, household wealth and current place of residence may have been affected by the violence. Nevertheless, including a set of controls provides a useful sensitivity check for the baseline results.

Figures A.25 and A.30 show that the results remain robust to the inclusion of these controls, although the point estimates are somewhat smaller in magnitude compared to the baseline specification. The DiD tables report the average effects both with and without controls, and additional stratifications are presented in the Additional Figures section.

# 8. Conclusion

This paper shows that exposure to communal violence generates substantial human capital losses that persist decades after the initial shock. Individuals who were very young at the time of the violence experience the largest educational setbacks, consistent with early school disruption leading to permanent dropout. These long-run effects contribute to wider and enduring socio-economic inequalities. Evidence from short-run outcomes suggests that these losses are driven by disruptions in educational progression, reflected in lower grade com-

pletion at higher levels and substantial increases in delays in grade progression. Together, these patterns indicate that while primary schooling may remain relatively resilient, progression to secondary and higher secondary education is significantly disrupted, consistent with increased costs of, or reduced investments in, continued schooling following the shock.

The results further point to important mechanisms operating through household decision-making and social norms. I find suggestive evidence that resource constraints and intra-household allocation may differentially affect children, with girls, especially those in girl-only households, being the most vulnerable. In addition, violence appears to increase the likelihood of early marriage, particularly for women, including marriage below the legal minimum age. Early marriage plausibly reduces women's intra-household bargaining power and increases vulnerability to adverse outcomes, including intimate partner violence, as documented in related work (Field & Ambrus, 2008; Ghose & Pandey, 2025). Together, these findings highlight how violence can alter both economic incentives and social institutions in ways that reinforce gender disparities in human capital accumulation.

Overall, the findings underscore the disproportionate burden of communal violence borne by women and children, while also demonstrating that violence imposes substantial costs on all individuals residing in affected areas. These results point to an urgent need for policies that protect vulnerable populations in violence-prone regions and mitigate long-run human capital losses. Policy interventions that improve access to schooling through infrastructure investments, safety-enhancing measures, and targeted incentives, particularly for girls may play an important role in offsetting the educational consequences of violence (Muralidharan & Prakash, 2017). More broadly, efforts to create safer local environments are essential for sustaining human capital formation in settings exposed to recurrent violence.

This paper is subject to several limitations. In particular, data constraints prevent a clean disentanglement of the relative importance of the mechanisms underlying the observed effects, and the evidence presented should be interpreted as suggestive. Future research could examine these channels more directly, including household responses, marriage market dynamics, and migration decisions following episodes of communal violence. A deeper understanding of these mechanisms is crucial for designing policies that not only reduce exposure to violence but also address its long-run consequences for human capital accumulation.

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# Tables

**Table 1:** Sample of Interest: Individuals 5–35 Years Old in 2002

| **Variable** | **Rest of India** | **Gujarat** |
|---|---|---|
| Total observations | 1,134,707 | 58,989 |
| **Older cohort (24–35 years in 2002)** | 320,456 | 17,397 |
| Male (=1) | 155,955 | 8,792 |
| **School-age cohort (5–23 years in 2002)** | 814,251 | 41,592 |
| Male (=1) | 391,237 | 20,990 |

**Note:** The total number of observations corresponds to the sample of interest, which is restricted to individuals who were aged 5–35 at the time of the 2002 violence. By the NFHS-5 survey period (2019–21), these individuals are aged 22–52, ensuring that all cohorts are old enough to have potentially completed twelve years of schooling. The sample is divided between individuals residing in Gujarat (treated state) and those residing in other Indian states (control states).

**Table 2:** Older Cohort(24-35 years old as of 2002)

| | (1) Other States | (2) Gujarat | (2) - (1) |
|---|---|---|---|
| **Control Variables:** | | | |
| Hindu (=1) | 0.75 | 0.89 | 0.14*** |
| Muslim (=1) | 0.11 | 0.09 | -0.02*** |
| Male (=1) | 0.48 | 0.50 | 0.02*** |
| Urban (=1) | 0.26 | 0.31 | 0.05*** |
| Household Size | 4.85 | 5.05 | 0.19*** |
| Male Household Head | 0.88 | 0.91 | 0.03*** |
| Wealth Index (1–5) | 2.98 | 3.31 | 0.33*** |
| **Outcome Variables of Interest:** | | | |
| No Education | 0.35 | 0.31 | -0.04*** |
| Completed Grade 12 | 0.24 | 0.22 | -0.02*** |
| Total Years of School | 5.34 | 5.63 | 0.29*** |
| (Male) Total Years | 6.83 | 6.96 | 0.13*** |
| (Female) Total Years | 3.95 | 4.28 | 0.33*** |

**Note:** This table reports differences in mean characteristics between individuals who were aged 24–35 in 2002 and resided in Gujarat and those residing in other Indian states, using household survey data from the 2019–21 National Family Health Survey (NFHS-5).

* Significant at 10%; ** Significant at 5%; *** Significant at 1%

**Table 3:** School-Age Cohort (5-23 years old as of 2002)

| | (1) Other States | (2) Gujarat | (2) - (1) |
|---|---|---|---|
| **Control Variables:** | | | |
| Hindu (=1) | 0.74 | 0.88 | 0.14*** |
| Muslim (=1) | 0.12 | 0.10 | -0.02*** |
| Male (=1) | 0.47 | 0.50 | 0.03*** |
| Urban (=1) | 0.25 | 0.31 | 0.06*** |
| Household Size | 5.56 | 5.86 | 0.30*** |
| Male Household Head | 0.85 | 0.90 | 0.05*** |
| Wealth Index(1-5) | 2.94 | 3.21 | 0.27*** |
| **Outcome Variables of Interest:** | | | |
| No Education | 0.17 | 0.14 | -0.03*** |
| Completed Grade 12 | 0.38 | 0.29 | -0.08 *** |
| Total Years of Education | 7.70 | 7.52 | -0.17** |
| (Male) Total Years | 8.50 | 8.38 | -0.12*** |
| (Female) Total Years | 6.95 | 6.42 | -0.31*** |

**Note:** This table reports differences in mean characteristics between individuals who were aged 5–23 in 2002 and resided in Gujarat and those residing in other Indian states, using household survey data from the 2019–21 National Family Health Survey (NFHS-5).

* Significant at 10%; ** Significant at 5%; *** Significant at 1%

**Table 4:** Summary Statistics - Older vs School-Age Cohort of Gujarat

| | (1) Older Cohort | (2) School-Age Cohort | (2) – (1) |
|---|---|---|---|
| **Control Variables:** | | | |
| Hindu (=1) | 0.89 | 0.88 | -0.009*** |
| Muslim (=1) | 0.09 | 0.1 | -0.009*** |
| Male (=1) | 0.5 | 0.5 | 0.00 |
| Urban (=1) | 0.31 | 0.31 | 0.00 |
| Household Size | 5.24 | 5.86 | 0.62*** |
| Male Household Head | 0.909 | 0.903 | -0.006*** |
| Wealth Index(1-5) | 3.25 | 3.21 | -0.03*** |
| **Outcome Variables of Interest:** | | | |
| No Education | 0.31 | 0.14 | -0.16*** |
| Completed Grade 12 | 0.22 | 0.29 | 0.07 *** |
| Total Years of Education | 5.63 | 7.52 | 1.9*** |
| (Male) Total Years | 6.96 | 8.38 | 1.44*** |
| (Female) Total Years | 4.28 | 6.42 | 2.37*** |

**Note:** This table reports simple means in characteristics of individuals residing in Gujarat, comparing those who were aged 24–35 in 2002 (older cohort) to those who were of age 5-23 in 2002 (school-age cohort).

* Significant at 10%; ** Significant at 5%; *** Significant at 1%

**Table 5:** Effect of Violence Exposure on Total Years of Schooling

| | Total Years of School (1) | Total Years of School (2) |
|---|---|---|
| SchoolAge X Violence | −0.502*** | −0.390*** |
| | (0.099) | (0.097) |
| Sample Mean | 6.97 | 6.97 |
| Observations | 1,193,696 | 1,193,696 |
| R-Squared | 0.14 | 0.32 |
| Controls | No | **Yes** |
| Fixed Effects | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3), where the outcome variable is total years of schooling. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at the state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 6:** Effect of Violence Exposure on Total Years of Schooling by Gender

| | Total Years of School | | Total Years of School | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| SchoolAge X Violence | -0.2714*** | -0.6813*** | -0.151*** | -0.577*** |
| | (0.0947) | (0.1134) | (0.102) | (0.106) |
| Sample Mean | 7.98 | 6.03 | 7.98 | 6.03 |
| Observations | 576,974 | 616,674 | 576,974 | 616,674 |
| R-Squared | 0.099 | 0.22 | 0.27 | 0.41 |
| Controls | No | No | **Yes** | **Yes** |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3) where the outcome variable is Total Years of Education, further dis-aggregated by gender. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 7:** Effect of Violence Exposure on Total Years of Schooling by Religion

| | Total Years of School | | Total Years of School | |
|---|---|---|---|---|
| | Hindu | Muslim | Hindu | Muslim |
| | (1) | (2) | (3) | (4) |
| SchoolAge X Riot | -0.448*** | -1.206*** | -0.349*** | -1.044*** |
| | (0.110) | (0.239) | (0.108) | (0.235) |
| Sample Mean | 6.99 | 6.32 | 6.99 | 6.32 |
| Observations | 898,919 | 145,597 | 898,919 | 145,597 |
| R-Squared | 0.15 | 0.20 | 0.34 | 0.32 |
| Controls | No | No | **Yes** | **Yes** |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3) where the outcome variable is Total Years of School, further dis-aggregated by religion. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 8:** Effect of Violence Exposure on Total Years of Schooling by Gender & Religion (No Controls)

| | Hindu | | Muslim | |
|---|---|---|---|---|
| | Male (1) | Female (2) | Male (3) | Female (4) |
| SchoolAge X Riot | -0.2052* | -0.6425*** | -0.8365*** | -1.477*** |
| | (0.1094) | (0.1246) | (0.1816) | (0.3038) |
| Sample Mean | 8.08 | 5.97 | 7.25 | 5.46 |
| Observations | 434,731 | 463,666 | 70,158 | 75,349 |
| R-Squared | 0.10 | 0.22 | 0.16 | 0.27 |
| Controls | No | No | No | No |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3). The outcome variable is total years of schooling, disaggregated by religion and further by gender. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 9:** Effect of Violence Exposure on Total Years of Schooling by Gender & Religion (With Controls)

| | Hindu | | Muslim | |
|---|---|---|---|---|
| | Male (1) | Female (2) | Male (3) | Female (4) |
| SchoolAge X Riot | -0.101 | -0.55*** | -0.668*** | -1.31*** |
| | (0.11) | (0.11) | (0.19) | (0.28) |
| Sample Mean | 8.08 | 5.97 | 7.25 | 5.46 |
| Observations | 434,731 | 463,666 | 70,158 | 75,349 |
| R-Squared | 0.29 | 0.43 | 0.29 | 0.40 |
| Controls | Yes | Yes | Yes | Yes |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (5). The outcome variable is total years of schooling, disaggregated by religion and further by gender. The controls include wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 10:** Effect of Violence Exposure on Grade 12 Completion

| | Grade 12 Completion (1) | Grade 12 Completion (2) |
|---|---|---|
| SchoolAge X Violence | $-0.059^{***}$ | $-0.049^{***}$ |
| | (0.011) | (0.011) |
| Sample Mean | 0.26 | 0.26 |
| Observations | 1,193,045 | 1,193,045 |
| R-Squared | 0.08 | 0.22 |
| Controls | No | **Yes** |
| Fixed Effects | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3) where the outcome variable is Grade 12 Completion. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at the state level.

$^{*}$ Significant at 10%; $^{**}$ Significant at 5%; $^{***}$ Significant at 1%.

**Table 11:** Effect of Violence Exposure on Grade 12 Completion by Gender

| | Grade 12 Completion | | Grade 12 Completion | |
|---|---|---|---|---|
| | Male (1) | Female (2) | Male (3) | Female (4) |
| SchoolAge X Violence | $-0.045^{***}$ | $-0.072^{***}$ | $-0.033^{***}$ | $-0.065^{***}$ |
| | (0.011) | (0.012) | (0.012) | (0.012) |
| Sample Mean | 0.31 | 0.21 | 0.31 | 0.21 |
| Observations | 576,974 | 616,674 | 576,974 | 616,674 |
| R-Squared | 0.06 | 0.12 | 0.21 | 0.25 |
| Controls | No | No | **Yes** | **Yes** |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3) where the outcome variable is Grade 12 Completion, further dis-aggregated by gender. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

$^{*}$ Significant at 10%; $^{**}$ Significant at 5%; $^{***}$ Significant at 1%.

**Table 12:** Effect of Violence Exposure on Grade 12 Completion by Religion

| | Grade 12 Completion | | Grade 12 Completion | |
|---|---|---|---|---|
| | Hindu | Muslim | Hindu | Muslim |
| | (1) | (2) | (3) | (4) |
| SchoolAge X Riot | -0.054*** | -0.071*** | -0.046*** | -0.059*** |
| | (0.012) | (0.027) | (0.012) | (0.026) |
| Sample Mean | 0.27 | 0.21 | 0.27 | 0.21 |
| Observations | 898,429 | 145,513 | 898,429 | 145,513 |
| R-Squared | 0.09 | 0.10 | 0.23 | 0.19 |
| Controls | No | No | **Yes** | **Yes** |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:** This table reports coefficient estimates from Specification (3) where the outcome variable is Grade 12 Completion, further dis-aggregated by religion. The specification with controls corresponds to (5) and includes the wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 13:** Effect of Violence Exposure on Grade 12 Completion by Gender & Religion (No Controls)

| | Hindu | | Muslim | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| SchoolAge X Riot | -0.04*** | -0.067*** | -0.05 | -0.09*** |
| | (0.012) | (0.014) | (0.03) | (0.024) |
| Sample Mean | 0.32 | 0.21 | 0.26 | 0.16 |
| Observations | 434,731 | 463,666 | 70,158 | 75,349 |
| R-Squared | 0.07 | 0.12 | 0.10 | 0.12 |
| Controls | No | No | No | No |
| Fixed Effects | Yes | Yes | Yes | Yes |

**Note:**This table reports coefficient estimates from Specification (3). The outcome variable is Grade 12 Completion, disaggregated by religion and further by gender. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

Note: Standard errors are clustered at state level.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

**Table 14:** Effect of Violence Exposure on Grade 12 Completion by Gender & Religion (With Controls)

| | Hindu | | Muslim | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| | (1) | (2) | (3) | (4) |
| SchoolAge X Riot | -0.03*** | -0.061*** | -0.033 | -0.08*** |
| | (0.013) | (0.013) | (0.029) | (0.023) |
| Sample Mean | 0.32 | 0.21 | 0.26 | 0.16 |
| Observations | 434,731 | 463,666 | 70,158 | 75,349 |
| R-Squared | 0.22 | 0.26 | 0.19 | 0.20 |
| Controls | Yes | Yes | Yes | Yes |
| Fixed Effects | Yes | Yes | Yes | Yes |

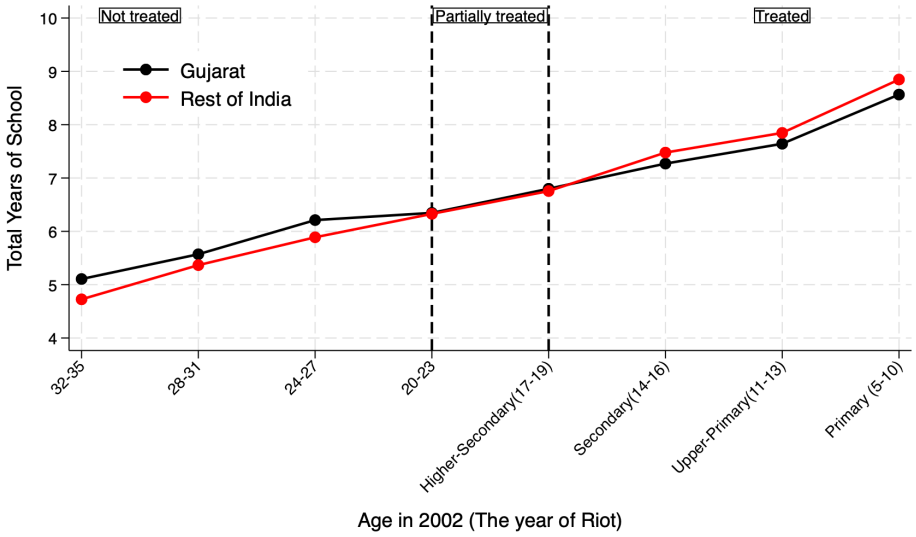
**Note:** This table reports coefficient estimates from Specification (5). The outcome variable is Grade 12 Completion, disaggregated by religion and further by gender. The controls include wealth index, place of residence (urban/rural), and household size. Standard errors are clustered at the state level. All specifications include age and state fixed effects.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

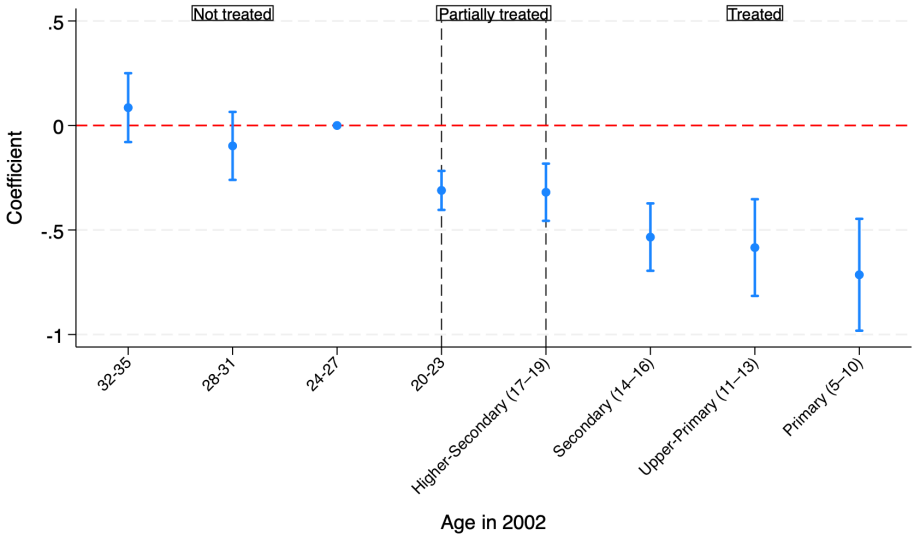
# Figures

**Figure 1:** Raw Means and Event Study Estimates - Total Years of Schooling

**(a)** Raw Means - Total Years of School



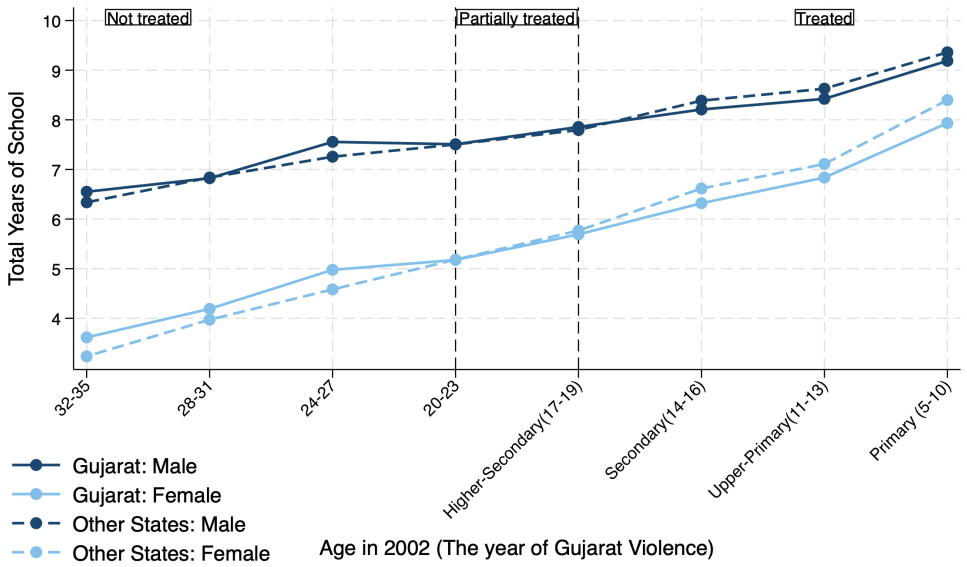

**(b)** Event Study Estimates - Total Years of School



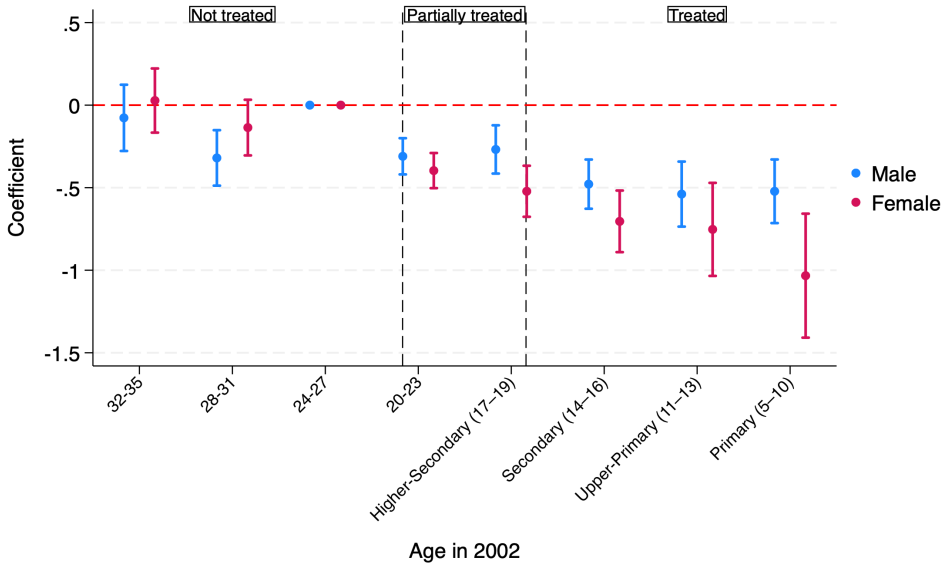

**Note:** This figure shows the outcome variable *Total Years Of Schooling* using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 2:** Raw Means and Event Study Estimates - Total Years of School by Gender

**(a)** Raw Means- Total Years of School by Gender



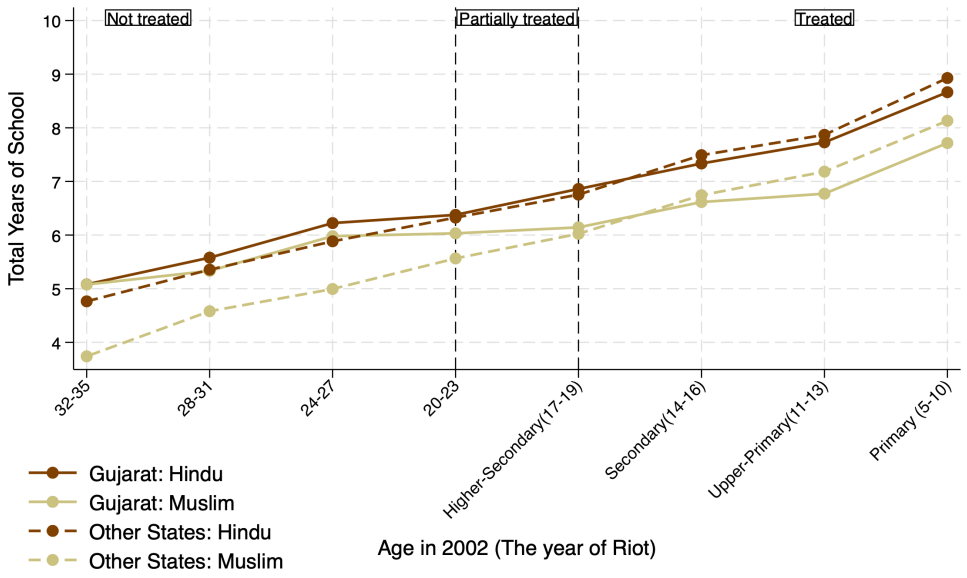

**(b)** Event Study Estimates - Total Years of School by Gender



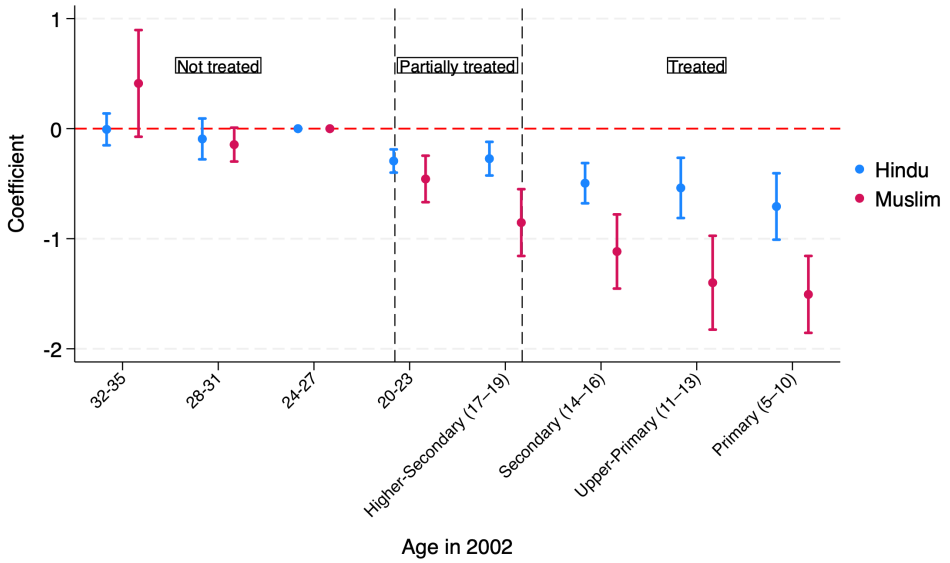

**Note:** This figure shows the outcome variable *Total Years Of Schooling* startified by gender using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 3:** Raw Means and Event Study Estimates - Total Years of School by Religion

**(a)** Raw Mean - Total Years of School by Religion



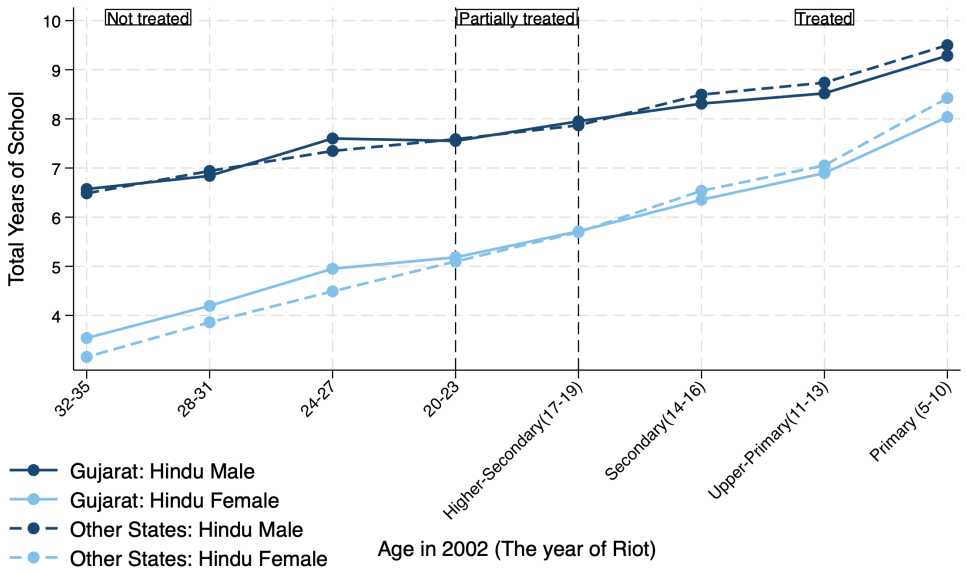

**(b)** Event Study Estimates - Total Years of School by Religion



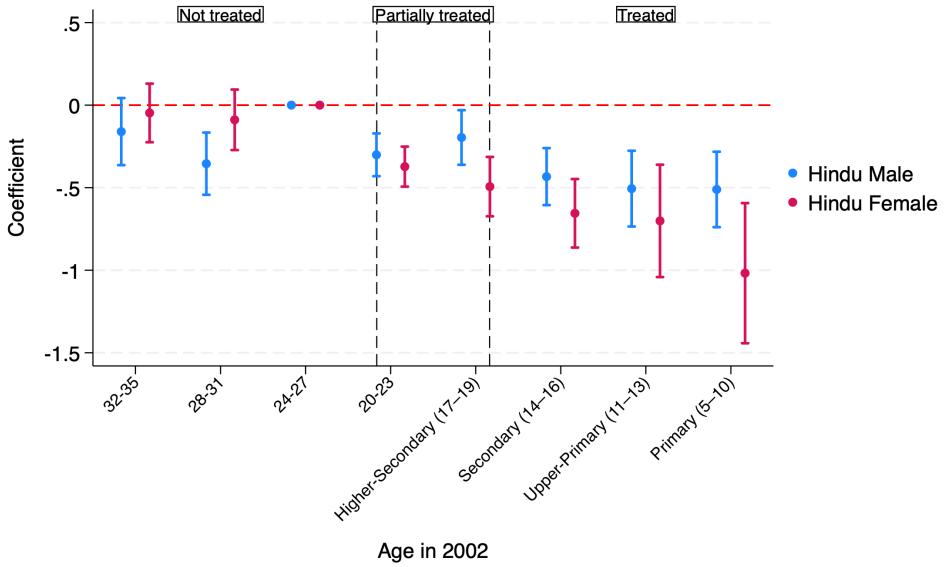

**Note:** This figure shows the outcome variable *Total Years Of Schooling* startified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 4:** Raw Means and Event Study Estimates - Total Years of School by Gender: Hindu Population

**(a)** Raw Mean - Total Years of School by Gender: Hindu Population



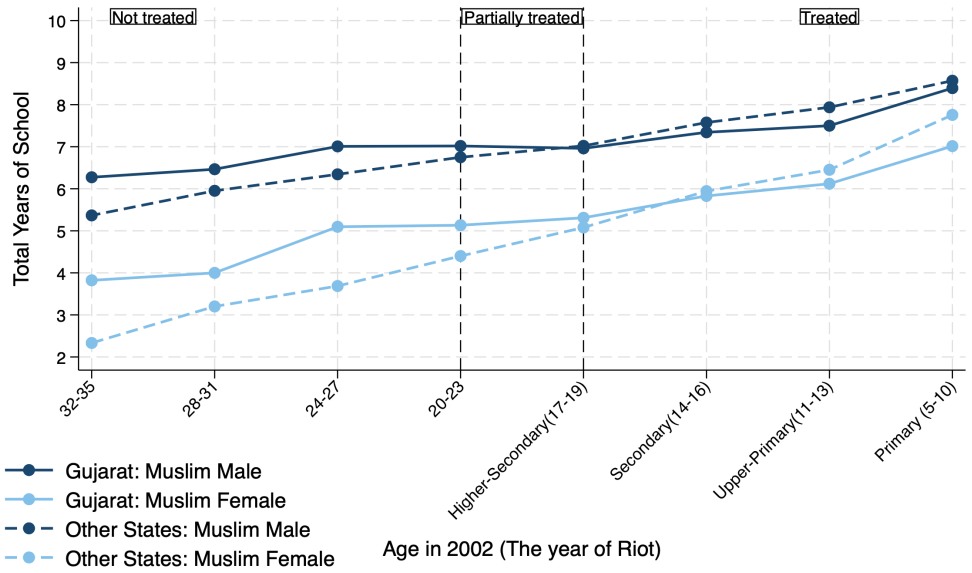

**(b)** Event Study Estimates - Total Years of School by Gender: Hindu Population



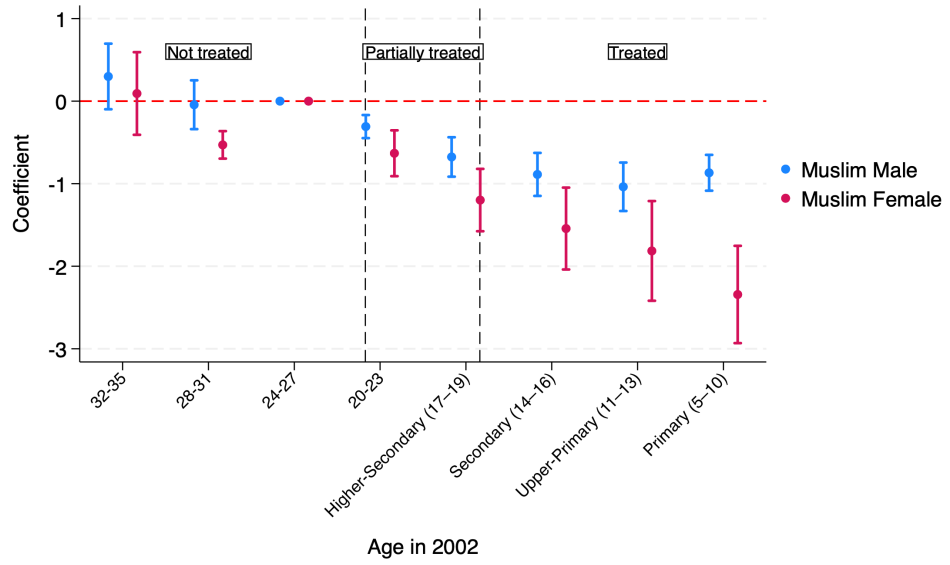

**Note:** This figure shows the outcome variable *Total Years Of Schooling* startified by gender for the Hindu Population using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 5:** Raw Means and Event Study Estimates - Total Years of School by Gender: Muslim Population

**(a)** Raw Mean - Total Years of School by Gender: Muslim Population



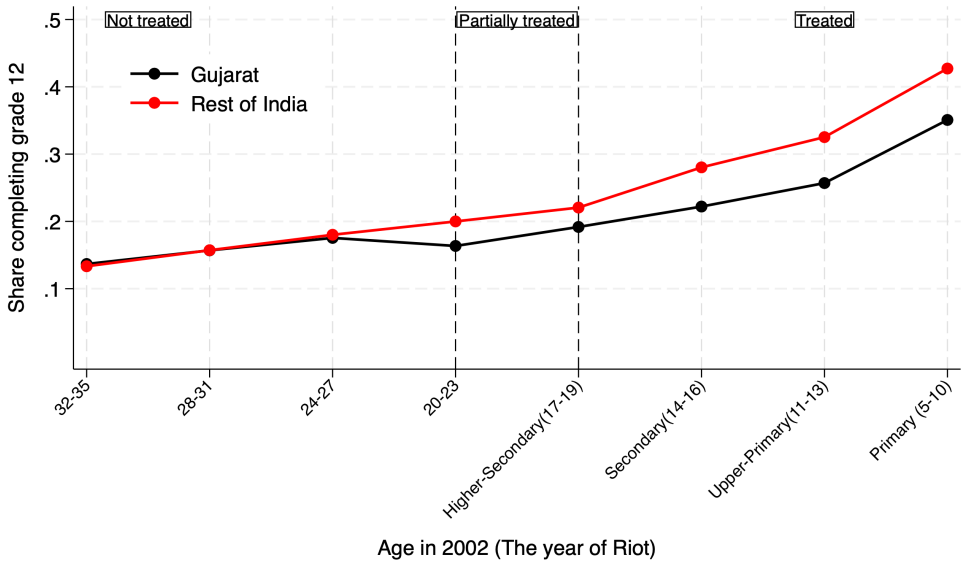

**(b)** Event Study Estimates - Total Years of School by Gender: Muslim Population



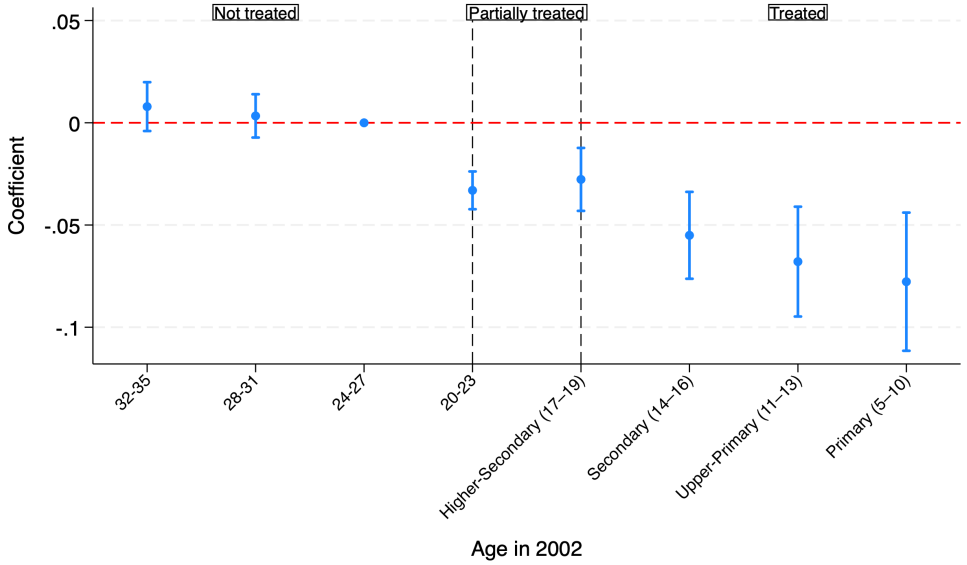

**Note:** This figure shows the outcome variable *Total Years Of Schooling* startified by gender for the Muslim Population using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 6:** Raw Means and Event Study Estimates - Grade 12 Completion

**(a)** Raw Means - Grade 12 Completion



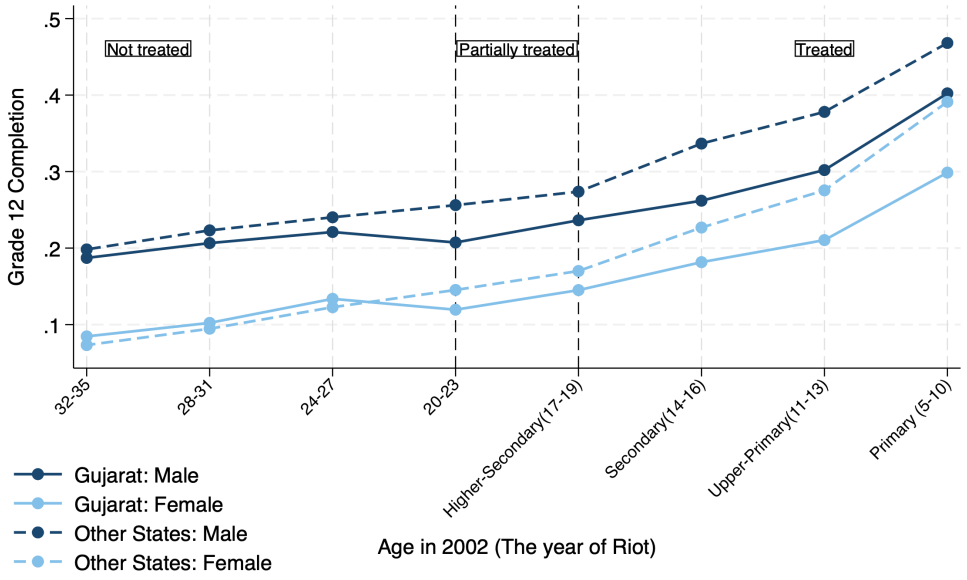

**(b)** Event-Study Estimates - Grade 12 Completion



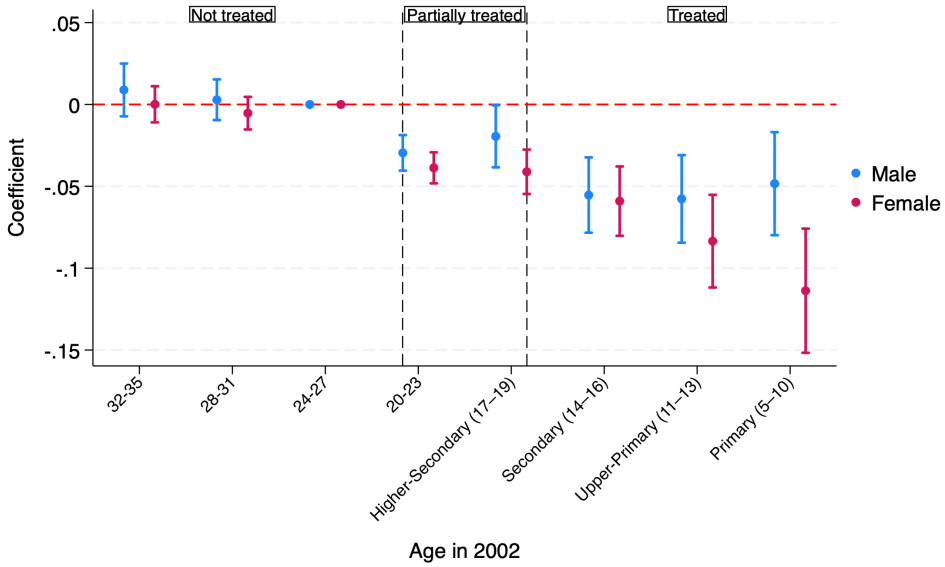

**Note:** This figure shows the outcome variable *Grade 12 Completion* startified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 7:** Raw Means and Event Study Estimates - Grade 12 Completion by Gender

**(a)** Raw Means - Grade 12 Completion by Gender



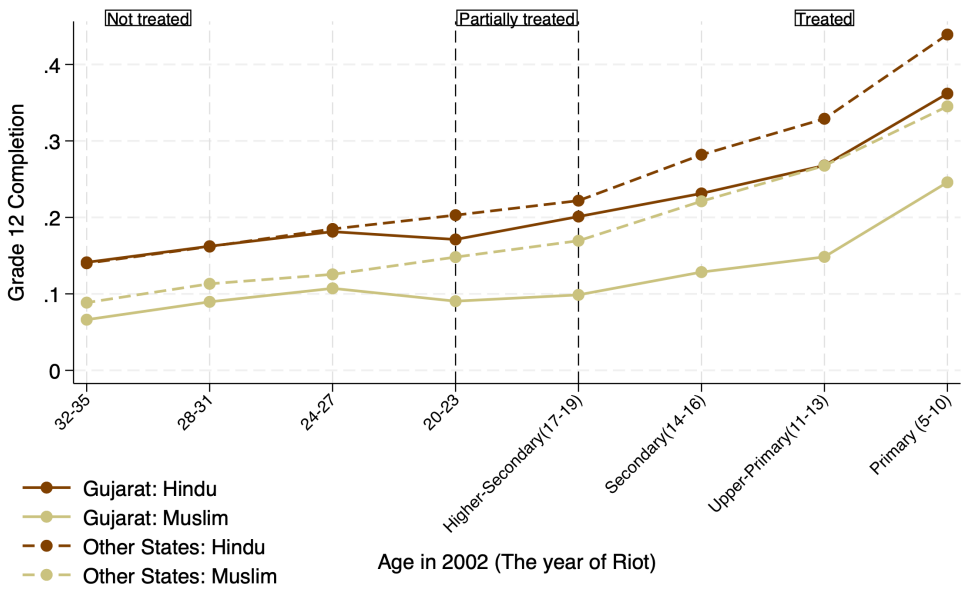

**(b)** Event-Study Estimates: Grade 12 Completion by Gender



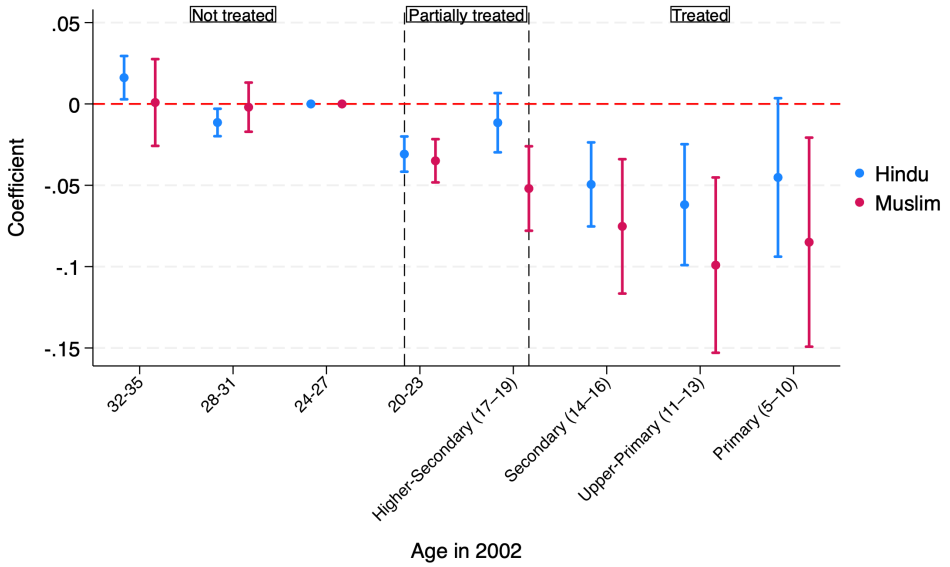

**Note:** This figure shows the outcome variable *Grade 12 Completion* startified by gender using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 8:** Raw Means and Event Study Estimates - Grade 12 Completion by Religion

**(a)** Raw Means - Grade 12 Completion by Religion



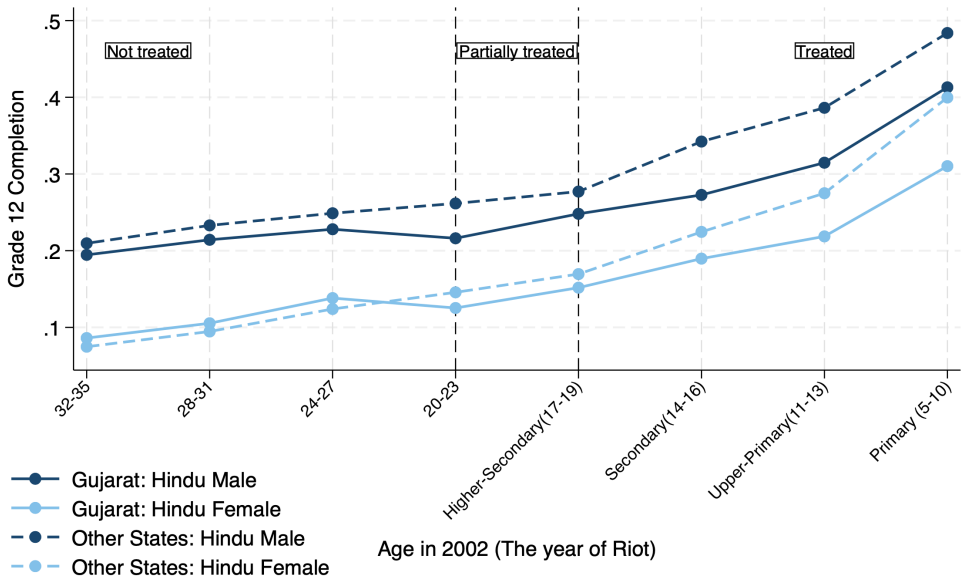

**(b)** Event-Study Estimates: Grade 12 Completion by Religion



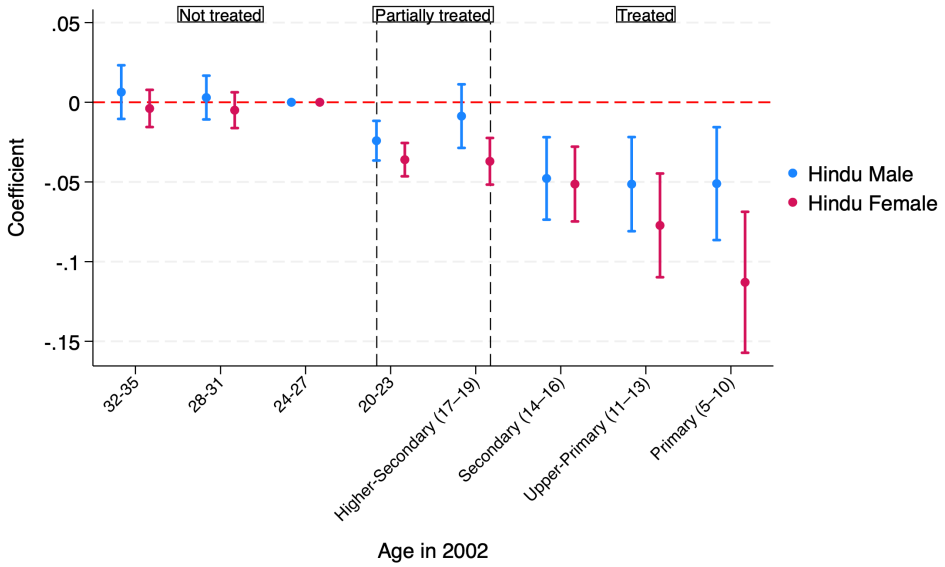

**Note:** This figure shows the outcome variable *Grade 12 Completion* startified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 9:** Raw Means and Event Study Estimates - Grade 12 Completion by Gender: Hindu Population

**(a)** Raw Means - Grade 12 Completion by Gender: Hindu Population



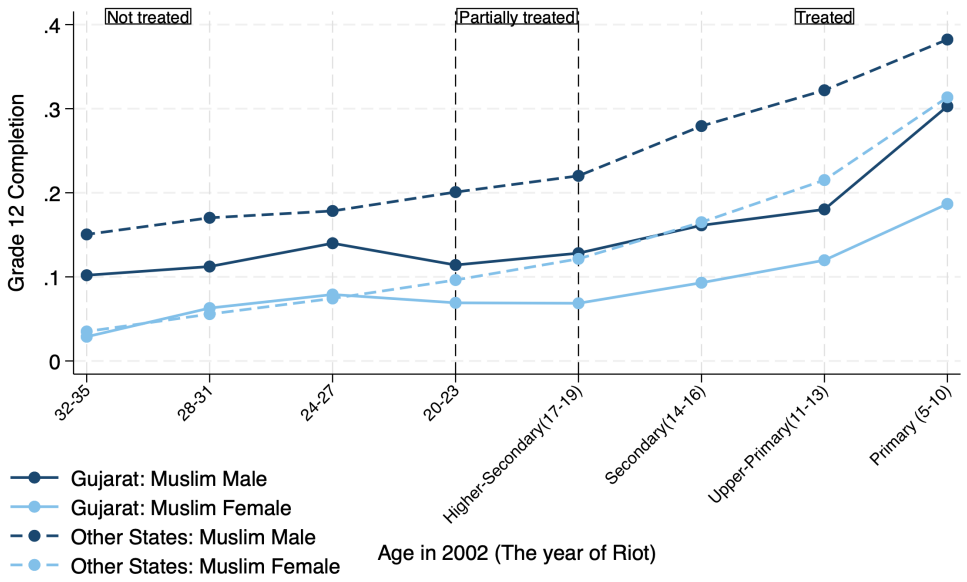

**(b)** Event-Study Estimates: Grade 12 Completion by Religion



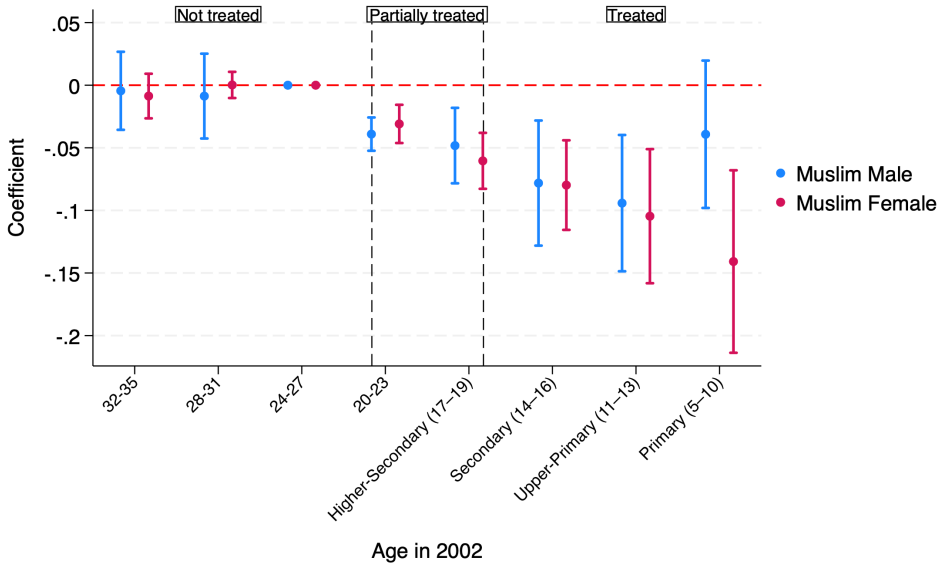

**Note:** This figure shows the outcome variable *Grade 12 Completion* startified by gender for the Hindu Population using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 10:** Raw Means and Event Study Estimates - Grade 12 Completion by Gender: Muslim Population

**(a)** Raw Means - Grade 12 Completion by Gender: Muslim Population



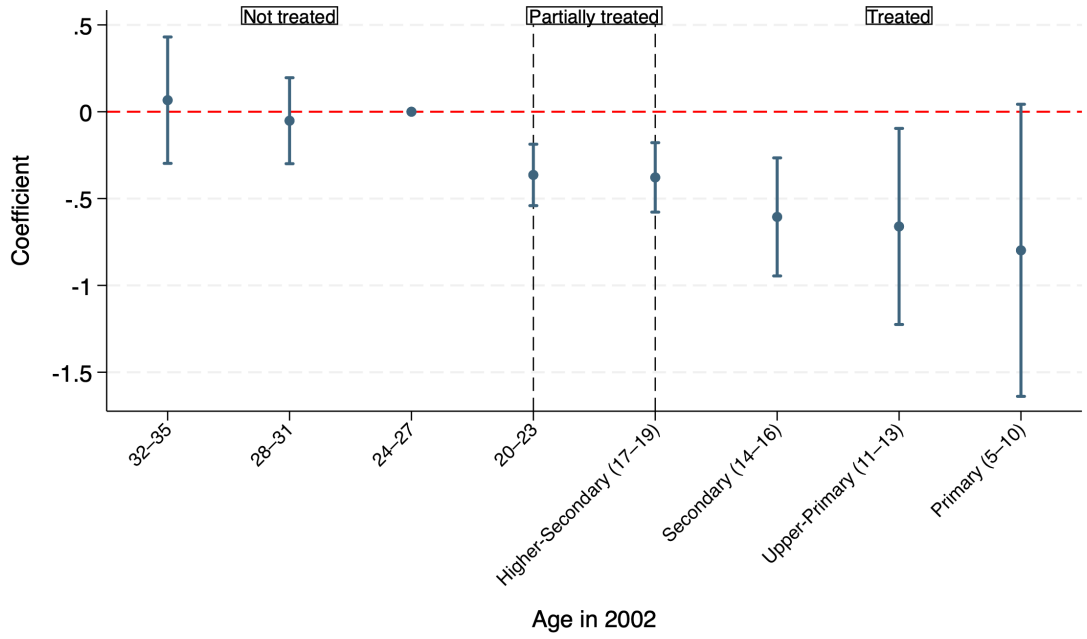

**(b)** Event-Study Estimates: Grade 12 Completion by Gender: Muslim Population



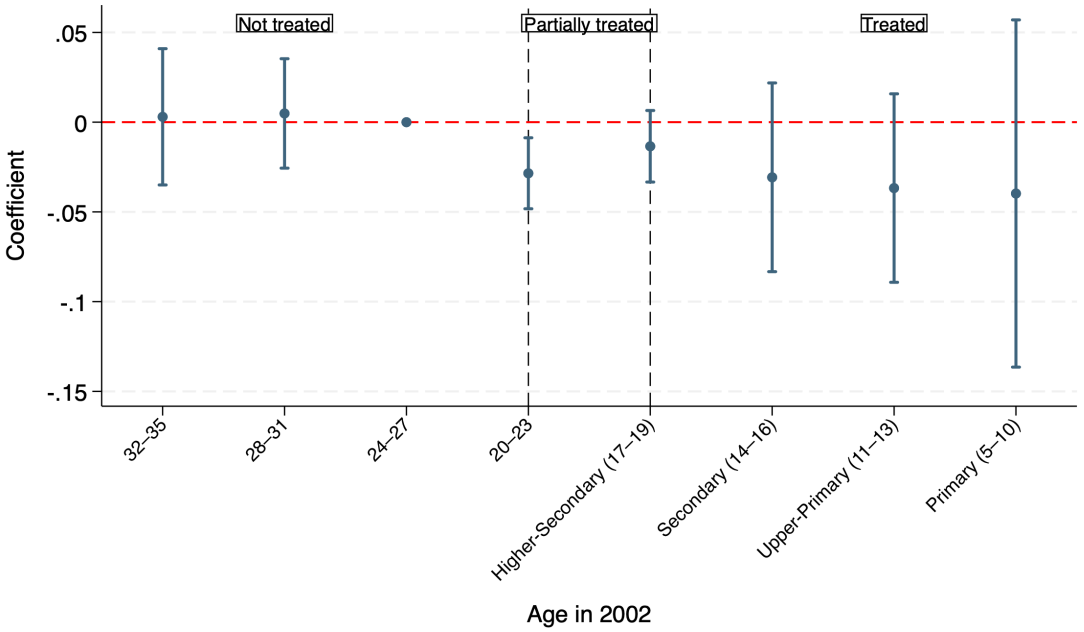

**Note:** This figure shows the outcome variable *Grade 12 Completion* startified by gender for the Muslim Population using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure 11:** Alternate Control Group

**(a)** Total Years of Education: Controls Incudes Only Neighboring States



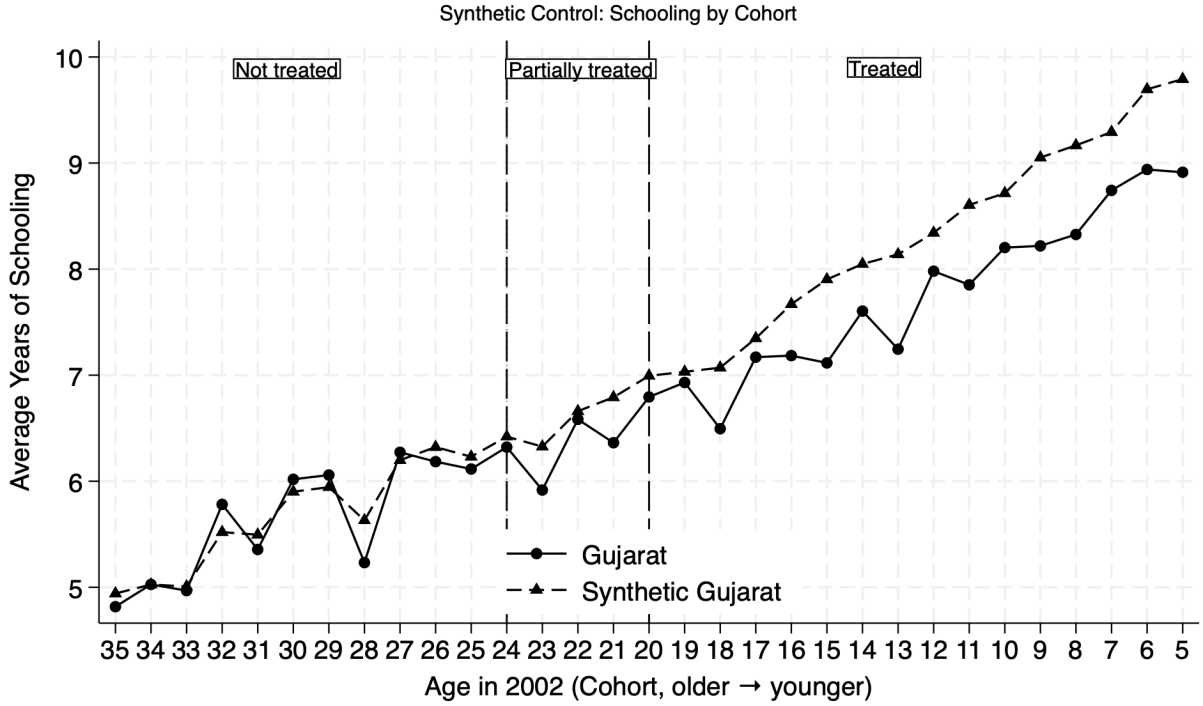

**(b)** Grade 12 Completion: Controls Incudes Only Neighboring States



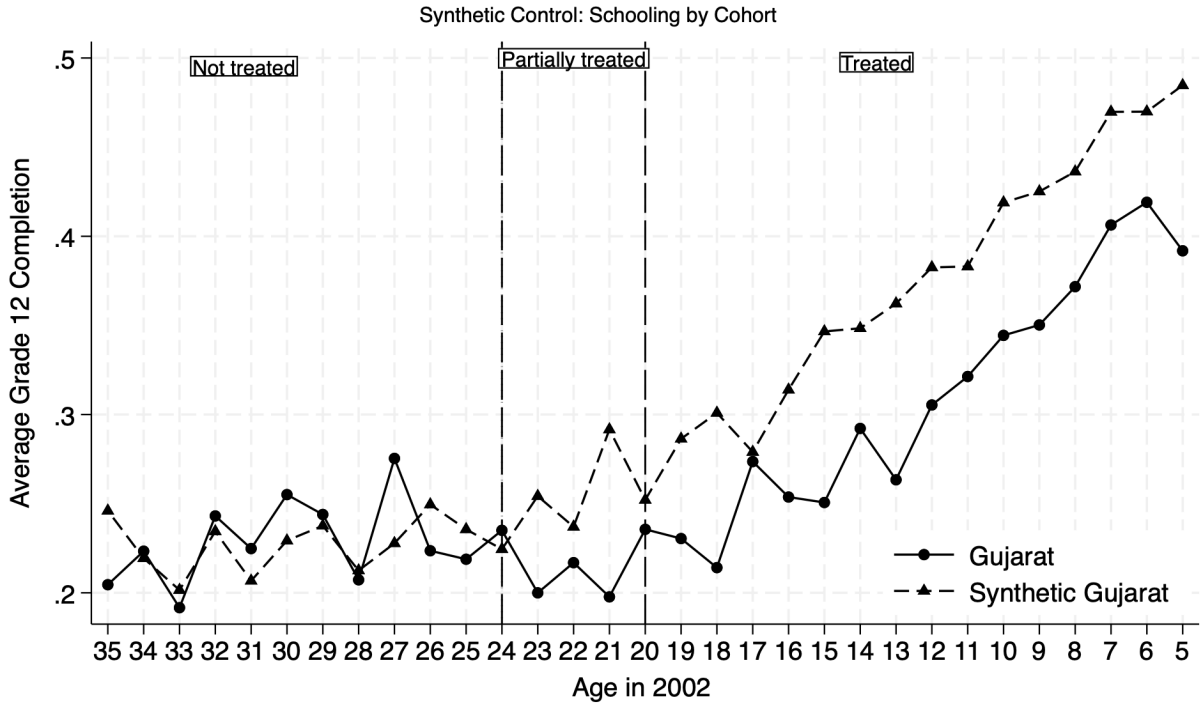

**Note:** This figure plots the difference-in-differences estimates from specification (2) using NFHS-5. The control states only include the bordering states of Gujarat, which are Maharashtra, Rajasthan and Madhya Pradesh. Standard errors were clustered at the state level.

**Figure 12:** Synthetic Control Analysis

**(a)** Total Years of School



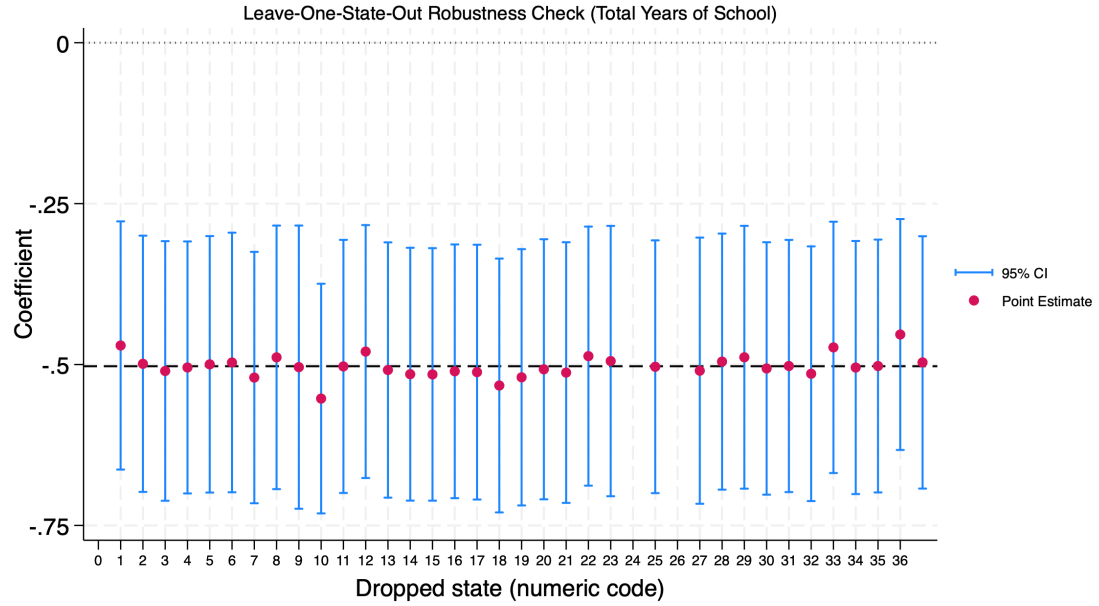

**(b)** Grade 12 Completion




**Note:** The figure plots the estimates using synthetic controls approach and using data from NFHS-5.

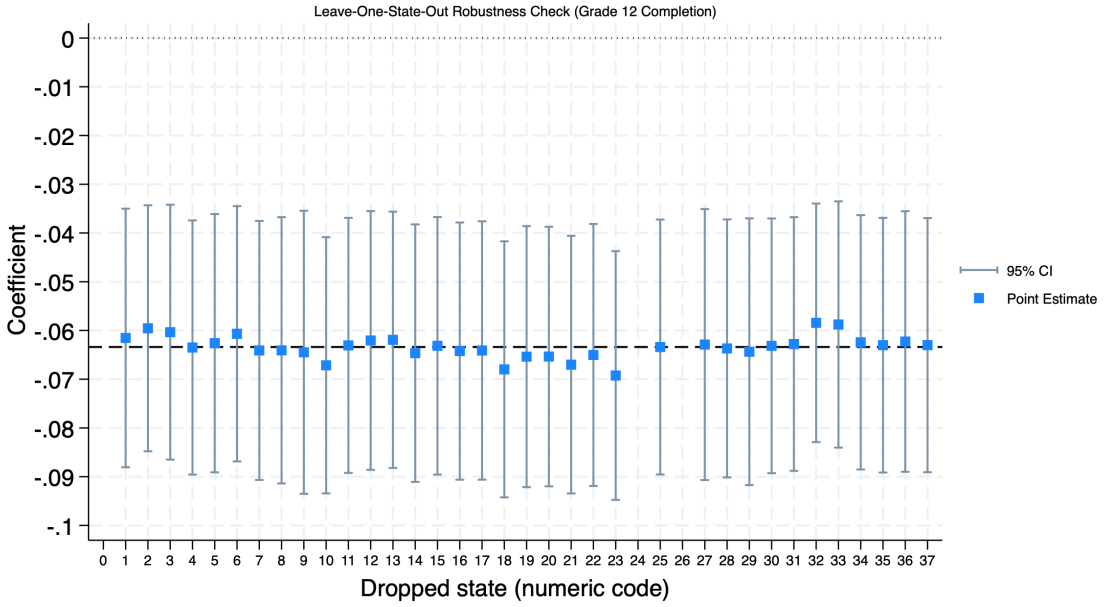
**Figure 13:** Leave One Out Analysis: Dropping one State at a Time

**(a)** Total Years of School




State 24 (Gujarat) yields no estimate because the interaction is not identified when Gujarat is excluded. State 26 does not appear in the data and is shown only as an axis label.

**(b)** Grade 12 Completion

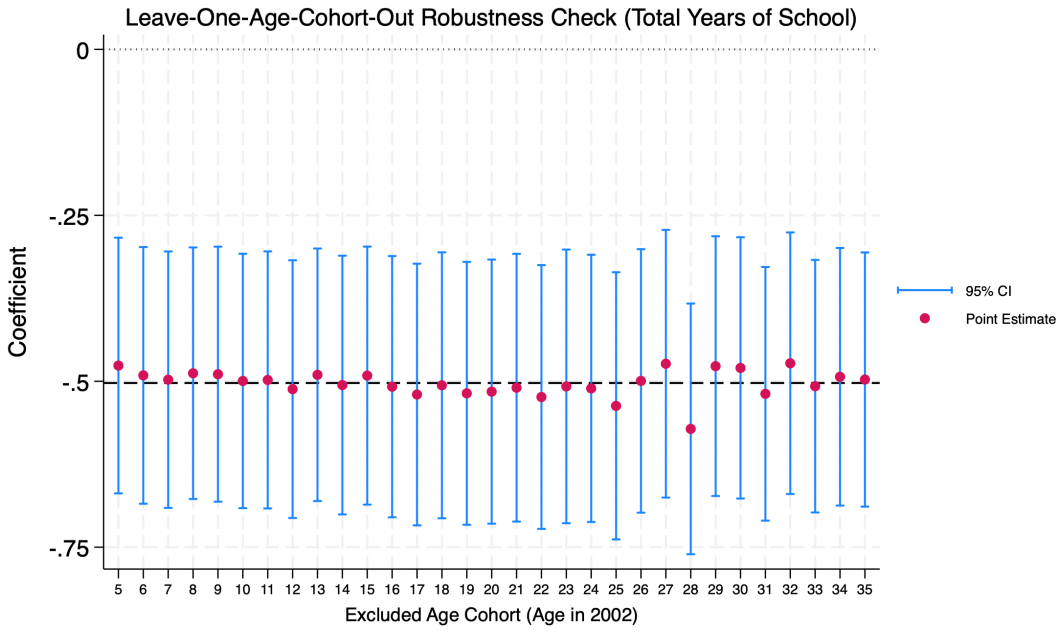



State 24 (Gujarat) yields no estimate because the interaction is not identified when Gujarat is excluded. State 26 does not appear in the data and is shown only as an axis label.

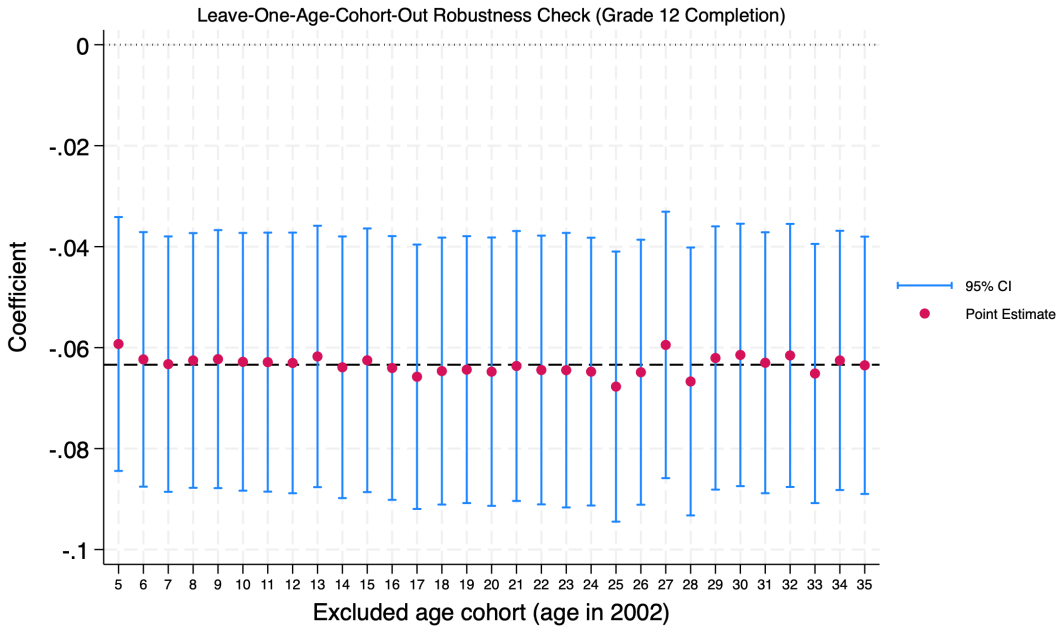
**Note:** This figure reports estimates from a leave-one-out robustness exercise in which the model specified by equation (2) is re-estimated repeatedly, each time excluding one state from the sample.

**Figure 14:** Leave One Out Analysis: Dropping one Age-Cohort at a Time

**(a)** Total Years of School



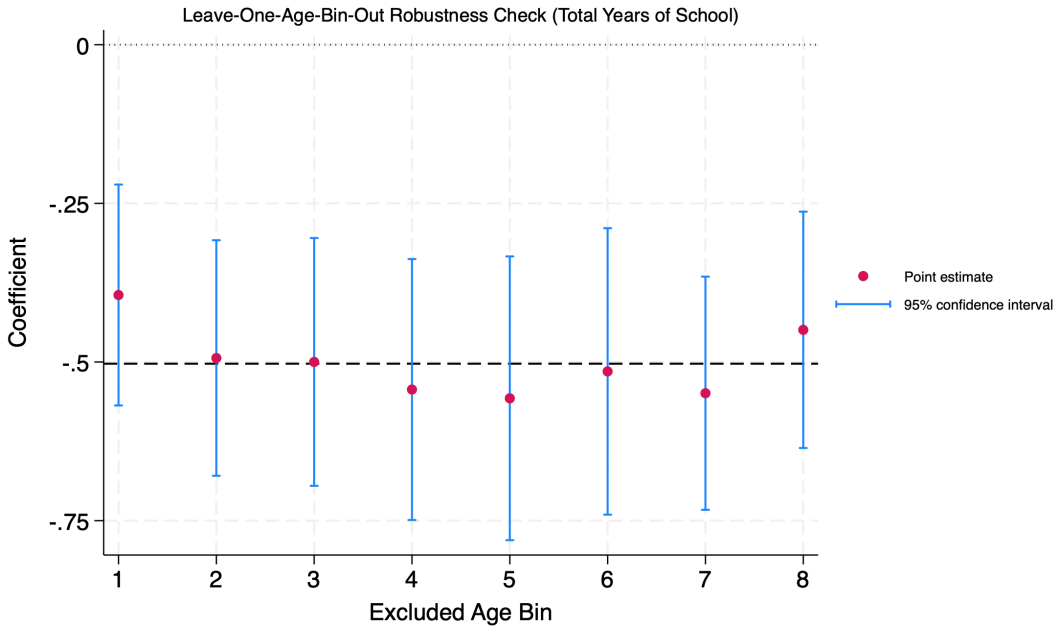

**(b)** Grade 12 Completion



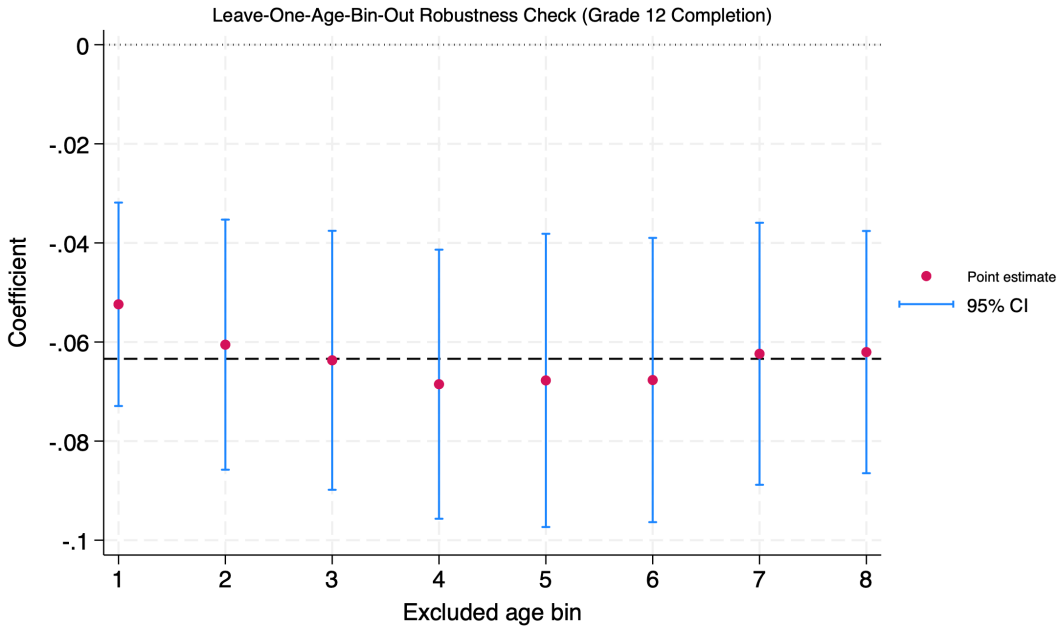

**Note:** This figure reports estimates from a leave-one-out robustness exercise in which the model specified by equation (1) is re-estimated repeatedly, each time excluding one age-cohort from the sample.

**Figure 15:** Leave One Out Analysis: Dropping one Age-Bin at a Time

**(a)** Total Years of School



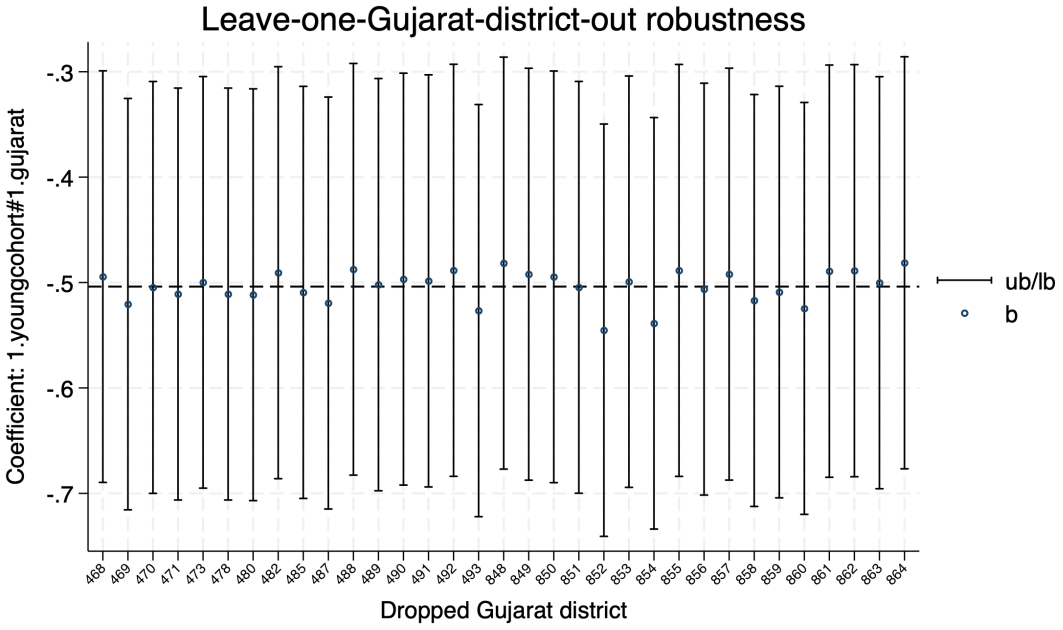

**(b)** Grade 12 Completion



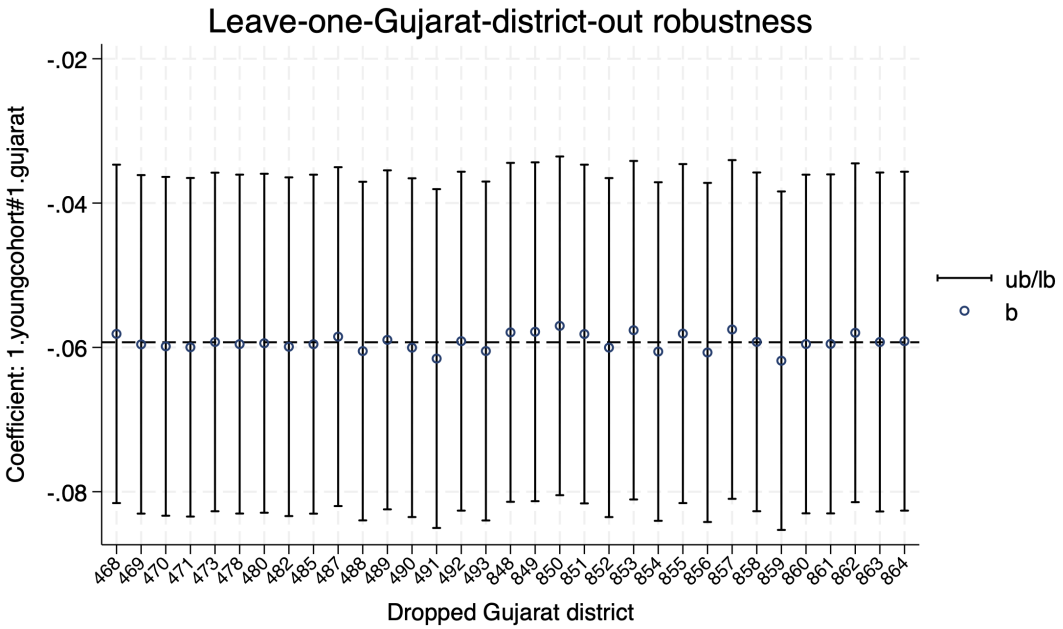

**Note:** This figure reports estimates from a leave-one-out robustness exercise in which the model specified by equation (2) is re-estimated repeatedly, each time excluding one age-bin from the sample.

**Figure 16:** Leave One Out Analysis: Dropping one District at a Time

**(a)** Total Years of School




**(b)** Grade 12 Completion




**Note:** This figure reports estimates from a leave-one-out robustness exercise in which the model specified by equation (2) is re-estimated repeatedly, each time excluding one district from the sample.

# Additional Tables

**Table 15:** Grade Completion Outcomes (DiD: NFHS-2 vs NFHS-3)

| Stratification | Grade 5 Completion | Grade 8 Completion | Grade 12 Completion |
|---|---|---|---|
| Male | 0.043*** | -0.02 | -0.073*** |
| | (0.009) | (0.013) | (0.01) |
| Observations | 59,020 | 43,288 | 60,968 |
| Sample Mean | 0.68 | 0.53 | 0.24 |
| Female | 0.038*** | -0.04** | -0.056*** |
| | (0.014) | (0.016) | (0.009) |
| Observations | 55,525 | 41,823 | 64,734 |
| Sample Mean | 0.64 | 0.48 | 0.20 |
| Hindu | 0.04*** | -0.029* | -0.055*** |
| | (0.011) | (0.015) | (0.009) |
| Observations | 82,412 | 61,040 | 91,802 |
| Sample Mean | 0.68 | 0.53 | 0.23 |
| Muslim | -0.039** | -0.111*** | -0.077*** |
| | (0.018) | (0.023) | (0.018) |
| Observations | 17,081 | 12,784 | 18,184 |
| Sample Mean | 0.56 | 0.41 | 0.14 |
| State FE | Yes | Yes | Yes |
| Survey FE | Yes | Yes | Yes |

**Note:** Standard errors clustered at the state level are reported in parentheses. Coefficients report the difference-in-differences estimate from Equation (4) using data from NFHS 2 & NFHS 3.

Grade 5 completion is defined for individuals aged 11–15, Grade 8 completion for individuals aged 14–17, and Grade 12 completion for individuals aged 18–23.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 16:** Grade Delay Outcomes (DiD: NFHS-2 vs NFHS-3)

| Stratification | Grade 5 Delay | Grade 8 Delay | Grade 12 Delay |
|---|---|---|---|
| Male | 0.007 | 0.04*** | 0.089*** |
| | (0.007) | (0.01) | (0.01) |
| Observations | 115,875 | 92,877 | 47,123 |
| Sample Mean | 0.19 | 0.36 | 0.73 |
| Female | 0.002 | 0.05*** | 0.057*** |
| | (0.013) | (0.014) | (0.01) |
| Observations | 118,341 | 96,187 | 51,181 |
| Sample Mean | 0.31 | 0.48 | 0.78 |
| Hindu | 0.005 | 0.032*** | 0.063*** |
| | (0.010) | (0.012) | (0.01) |
| Observations | 169,526 | 137,282 | 71,916 |
| Sample Mean | 0.24 | 0.42 | 0.74 |
| Muslim | 0.054*** | 0.139*** | 0.082*** |
| | (0.013) | (0.014) | (0.018) |
| Observations | 34,478 | 27,710 | 14,017 |
| Sample Mean | 0.33 | 0.53 | 0.84 |
| State FE | Yes | Yes | Yes |
| Survey FE | Yes | Yes | Yes |

**Note:** Standard errors clustered at the state level are reported in parentheses. Coefficients report the difference-in-differences estimate from Equation (4) using data from NFHS 2 & NFHS 3.

Grade 5 delay is defined for individuals aged 13 or older who have not completed Grade 5, Grade 8 delay for individuals aged 15 or older who have not completed Grade 8, and Grade 12 delay for individuals aged 19 or older who have not completed Grade 12. The sample is restricted to individuals aged 23 or younger.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 17:** Educational Completion by Household Composition

| Stratification | Grade 5 Completion | Grade 8 Completion | Grade 12 Completion |
|---|---|---|---|
| **Mixed-Gender Households** | | | |
| Boys | 0.043*** | -0.019 | -0.048*** |
| | (0.006) | (0.013) | (0.009) |
| Observations | 43,861 | 31,849 | 42,879 |
| Sample Mean | 0.67 | 0.51 | 0.21 |
| Girls | 0.05*** | -0.021 | -0.044*** |
| | (0.014) | (0.017) | (0.010) |
| Observations | 45,190 | 34,317 | 44,194 |
| Sample Mean | 0.62 | 0.47 | 0.18 |
| **Single-Gender Households** | | | |
| Boys Only Households | 0.043*** | -0.018 | -0.105*** |
| | (0.013) | (0.015) | (0.013) |
| Observations | 15,159 | 11,439 | 18,089 |
| Sample Mean | 0.71 | 0.57 | 0.31 |
| Girls Only Households | -0.019 | -0.145*** | -0.077*** |
| | (0.015) | (0.017) | (0.011) |
| Observations | 10,335 | 7,506 | 20,540 |
| Sample Mean | 0.68 | 0.55 | 0.21 |
| State FE | Yes | Yes | Yes |
| Survey FE | Yes | Yes | Yes |

**Note:** Standard errors clustered at the state level are reported in parentheses. Coefficients report the difference-in-differences estimate from Equation (4) using data from NFHS 2 & NFHS 3.

Mixed-gender households contain at least one boy and one girl aged 5–23, and single-gender households contain children of only one gender in the same age range.

Grade 5 completion is defined for individuals aged 11–15, Grade 8 completion for individuals aged 14–17, and Grade 12 completion for individuals aged 18–23.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 18:** Marital Outcomes Post the Gujarat Violence of 2002: Full Sample of Women

| | (1) Age at Marriage | (2) Married Before Age 18 | (3) Age at Cohabitation | (4) Age at First Sex | (5) Age at First Birth |
|---|---|---|---|---|---|
| SchoolAge × Violence | -0.173 | 0.035*** | -0.092 | -0.132 | 0.048 |
| | (0.13) | (0.01) | (0.11) | (0.24) | (0.10) |
| Constant | 19.28 | 0.34 | 19.01 | 17.73 | 20.88 |
| Observations | 490,829 | 490,829 | 495,058 | 531,096 | 466,495 |
| R-squared | 0.104 | 0.07 | 0.09 | 0.08 | 0.05 |
| Control Average | 19.27 | 0.35 | 19.01 | 17.73 | 20.88 |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Age Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Controls | No | No | No | No | No |

**Note:** This table reports coefficient estimates from Specification (3). Standard errors are clustered at the state level. All specifications include age and state fixed effects.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 19:** Marital Outcomes Post the Gujarat Violence of 2002: Hindu Women

| | (1) Age at Marriage | (2) Married Before Age 18 | (3) Age at Cohabitation | (4) Age at First Sex | (5) Age at First Birth |
|---|---|---|---|---|---|
| SchoolAge × Violence | -0.295*** | 0.05*** | -0.18* | -0.52*** | -0.02 |
| | (0.11) | (0.01) | (0.10) | (0.16) | (0.10) |
| Observations | 374,899 | 374,899 | 378,216 | 400,666 | 356,663 |
| R-squared | 0.09 | 0.07 | 0.09 | 0.07 | 0.05 |
| Control Average | 18.95 | 0.37 | 18.73 | 17.70 | 20.73 |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Age Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Controls | No | No | No | No | No |

**Note:** This table reports coefficient estimates from Specification (3). Standard errors are clustered at the state level. All specifications include age and state fixed effects.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 20:** Marital Outcomes Post the Gujarat Violence of 2002: Muslim Women

| | (1) Age at Marriage | (2) Married Before Age 18 | (3) Age at Cohabitation | (4) Age at First Sex | (5) Age at First Birth |
|---|---|---|---|---|---|
| SchoolAge × Violence | -0.47** | 0.004 | -0.39** | 0.43 | -0.17 |
| | (0.19) | (0.07) | (0.10) | (0.76) | (0.16) |
| Constant | 19.51*** | 0.33*** | 18.74*** | 17.32*** | 20.85*** |
| Observations | 58,033 | 58,033 | 58,360 | 64,362 | 54,971 |
| R-squared | 0.13 | 0.07 | 0.12 | 0.07 | 0.11 |
| Control Average | 19.50 | 0.33 | 19.14 | 17.34 | 20.85 |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Age Fixed Effects | Yes | Yes | Yes | Yes | Yes |
| Controls | No | No | No | No | No |

**Note:** This table reports coefficient estimates from Specification (3). Standard errors are clustered at the state level. All specifications include age and state fixed effects.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table 21:** Marital Outcomes Post the Gujarat Violence of 2002: Men

| | Hindu | | Muslim | |
|---|---|---|---|---|
| | Age at Marriage | Married Before 18 | Age at Marriage | Married Before 18 |
| SchoolAge × Violence | -0.839*** | 0.049*** | -0.209 | 0.002 |
| | (0.17) | (0.008) | (0.18) | (0.008) |
| Sample Mean | 23.38 | 0.42 | 24.18 | 0.37 |
| Observations | 51,767 | 51,767 | 8,286 | 8,286 |
| R-squared | 0.15 | 0.05 | 0.07 | 0.02 |
| State Fixed Effects | Yes | Yes | Yes | Yes |
| Age Fixed Effects | Yes | Yes | Yes | Yes |
| Controls | No | No | No | No |

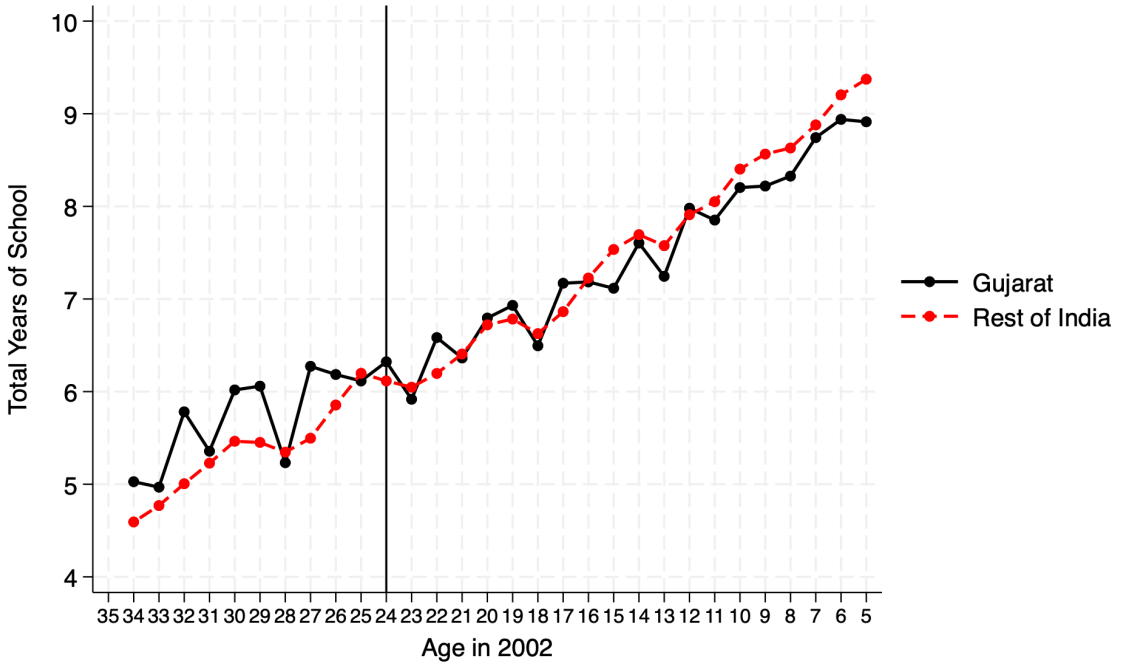
**Note:** This table reports coefficient estimates from Specification (3). Standard errors are clustered at the state level. All specifications include age and state fixed effects.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

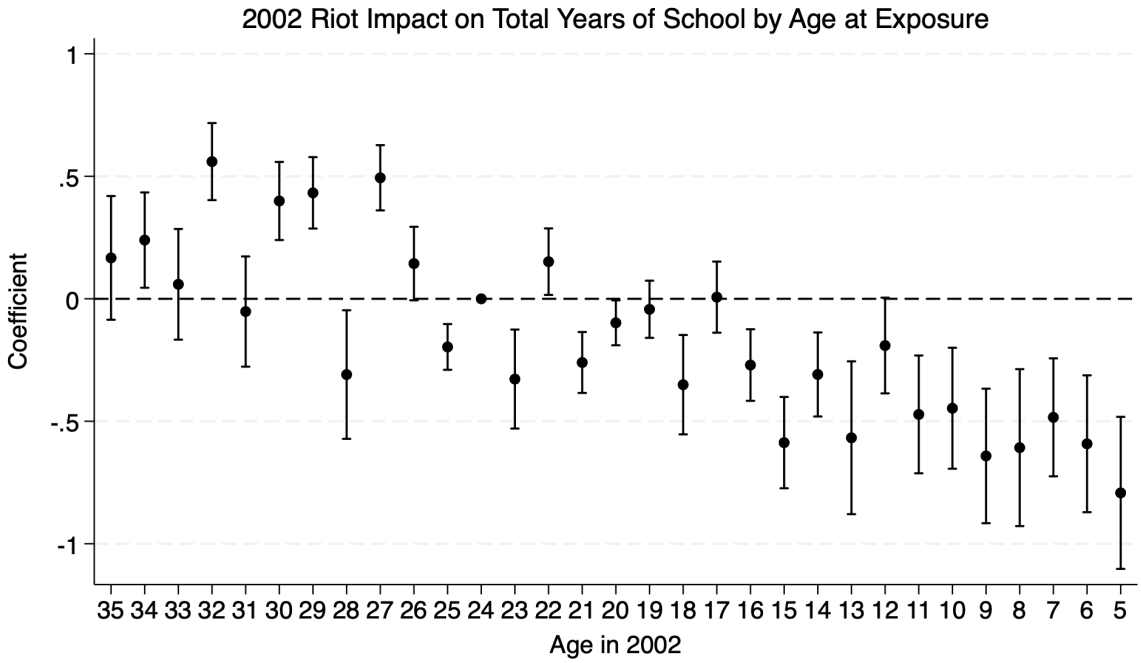
# Additional Figures

**Figure A.1:** Raw Means and Event Study Estimates - Total Years of Schooling

**(a)** Trends in Total Years of Education



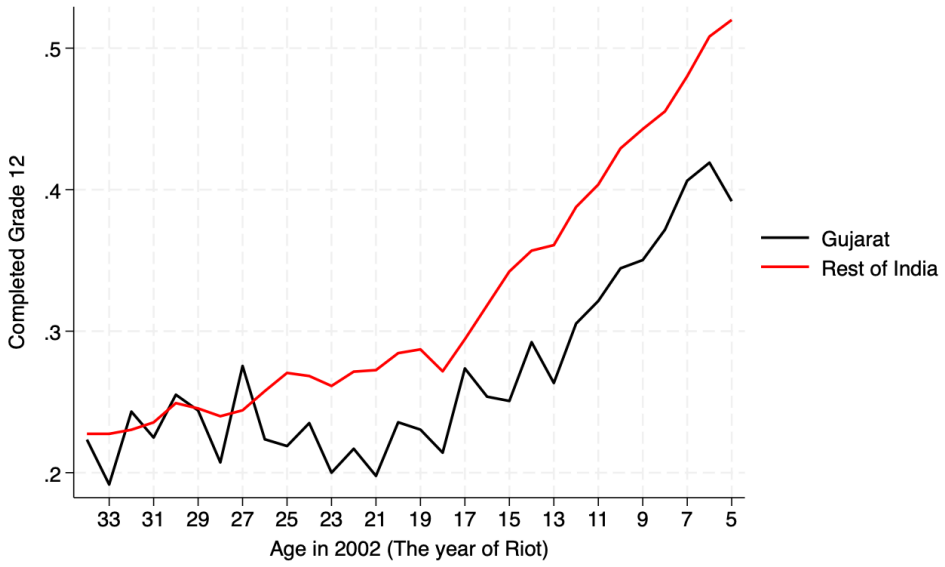

**(b)** Event-Study Estimates by Age Cohort



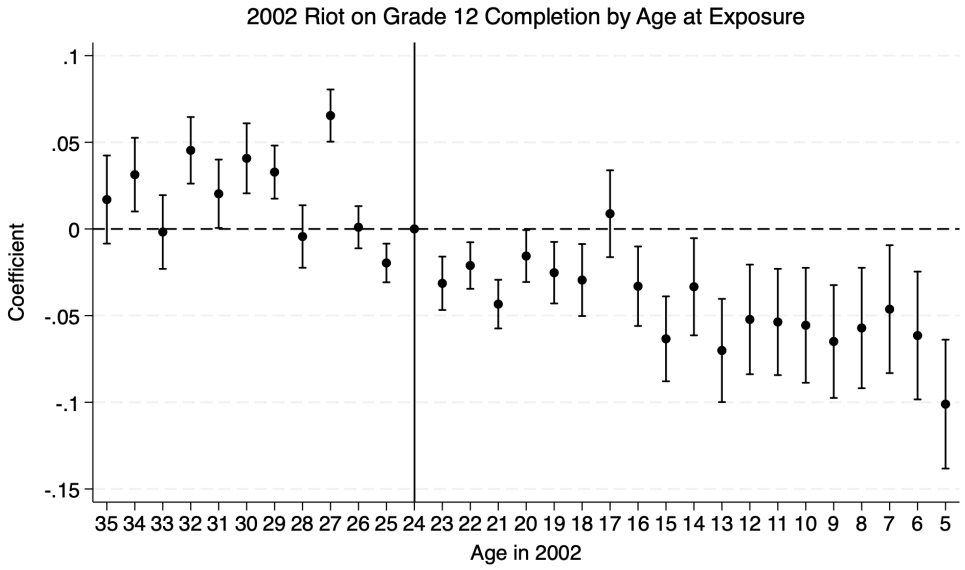

**Note:** This figure shows the outcome variable *Total Years Of Schooling*, using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (1). Standard errors are clustered at the state level.

**Figure A.2:** Raw Means and Event Study Estimates - Grade 12 Completion

**(a)** Trends in Grade 12 Completion

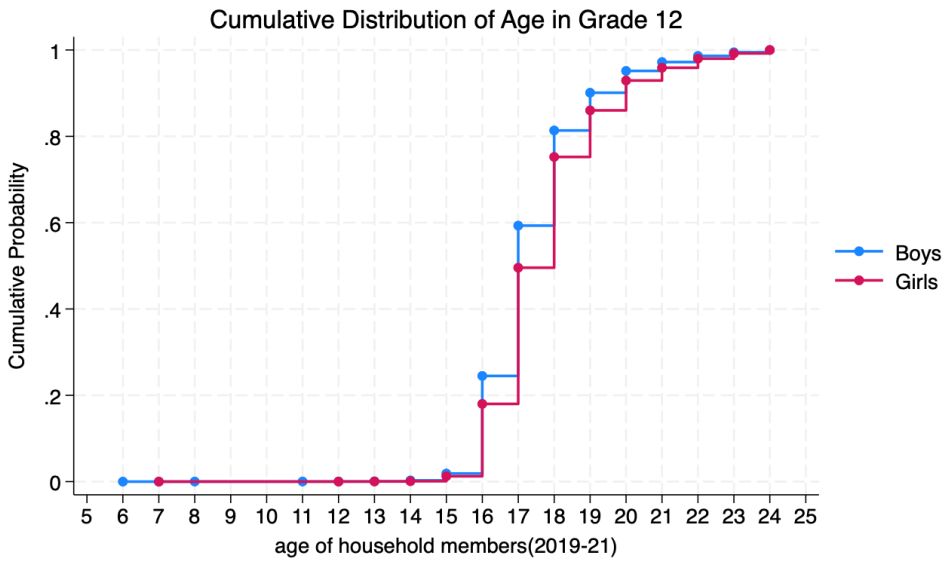



**(b)** Event-Study Estimates by Age Cohort



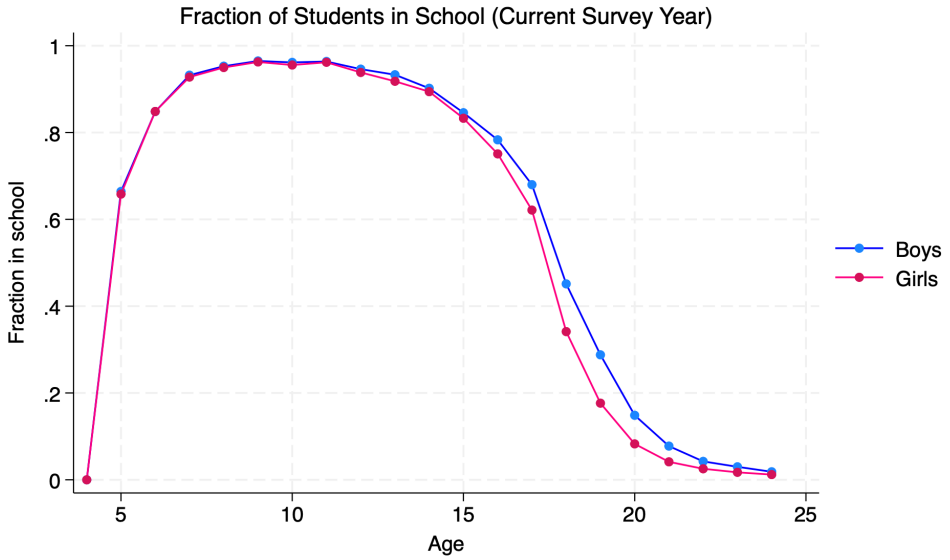

**Note:** This figure shows the outcome variable *Grade 12 Completion*, using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (1). Standard errors are clustered at the state level.

**Figure A.3:** Cumulative Distribution of Age in Grade 12



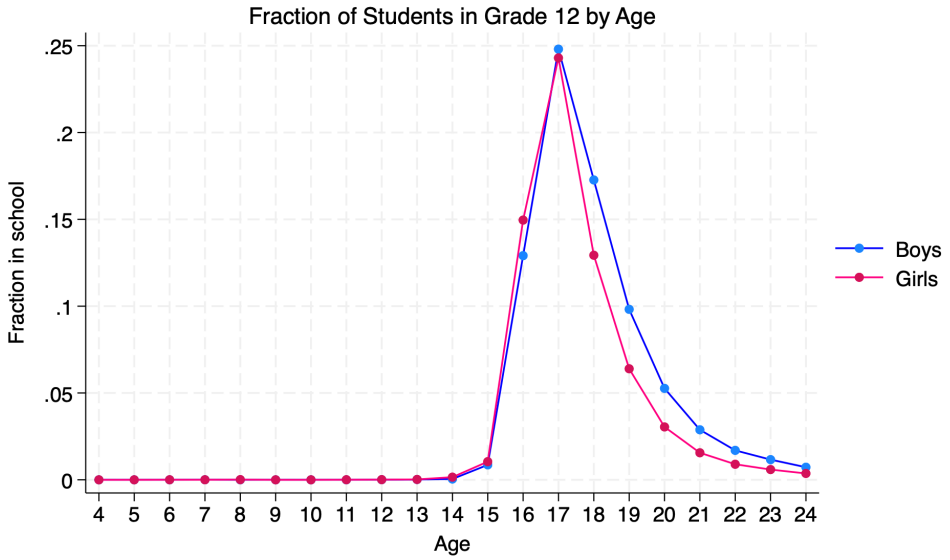

**Note:**This figure plots the cumulative distribution of age at Grade 12 completion for boys and girls using data NFHS-5. The horizontal axis shows individuals' age in the current survey year, while the vertical axis reports the cumulative probability of having completed Grade 12 by a given age.

**Figure A.4:** Fraction of Students in School by Age



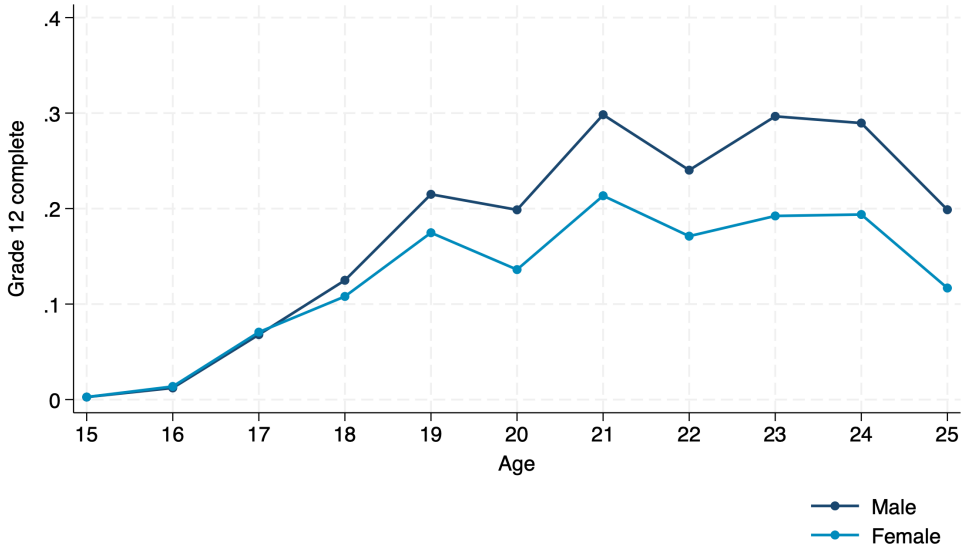

**Note:** This figure shows the fraction of individuals currently enrolled in school at each age, separately for boys and girls, using data from NFHS-5. The horizontal axis shows age at the time of the survey, while the vertical axis reports the proportion of individuals in school.

**Figure A.5:** Fraction of Students in Grade 12 by Age




**Note:** This figure plots the fraction of individuals enrolled in Grade 12 at each age, separately for boys and girls, using data from NFHS-5. The horizontal axis shows age at the time of the survey, while the vertical axis reports the proportion of individuals in Grade 12.

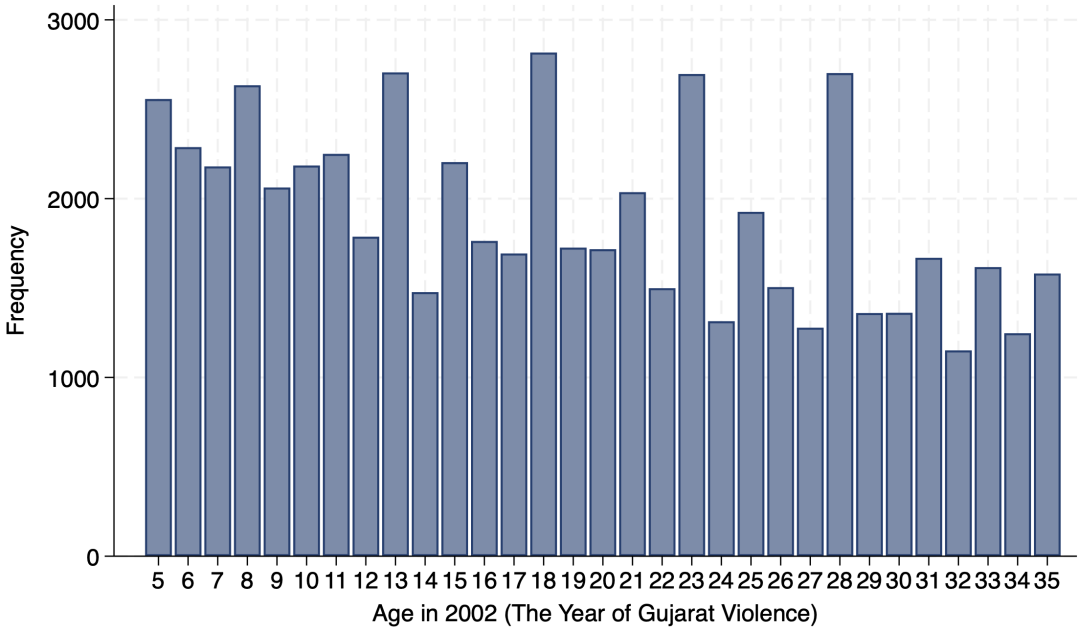
**Figure A.6:** Age Profile of Grade 12 Completion by Gender



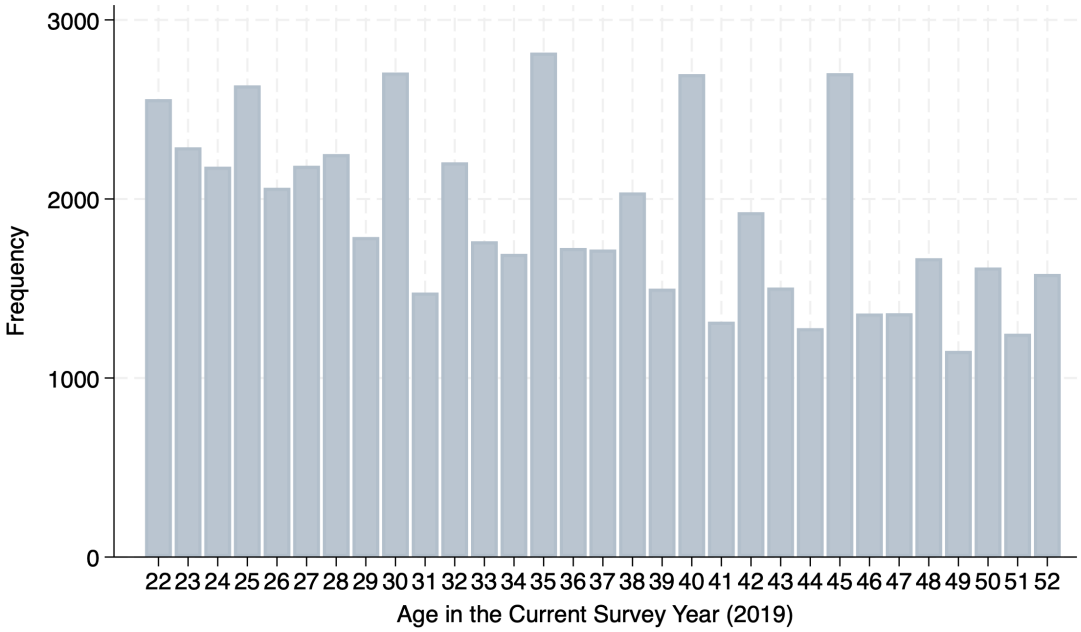

**Note:** This figure plots the fraction of individuals who have completed Grade 12 by age, separately by gender, using NFHS-2 (1998–99) data.

**Figure A.7:** Distribution of the Sample in Gujarat by Age

**(a)** Age in 2002: Gujarat



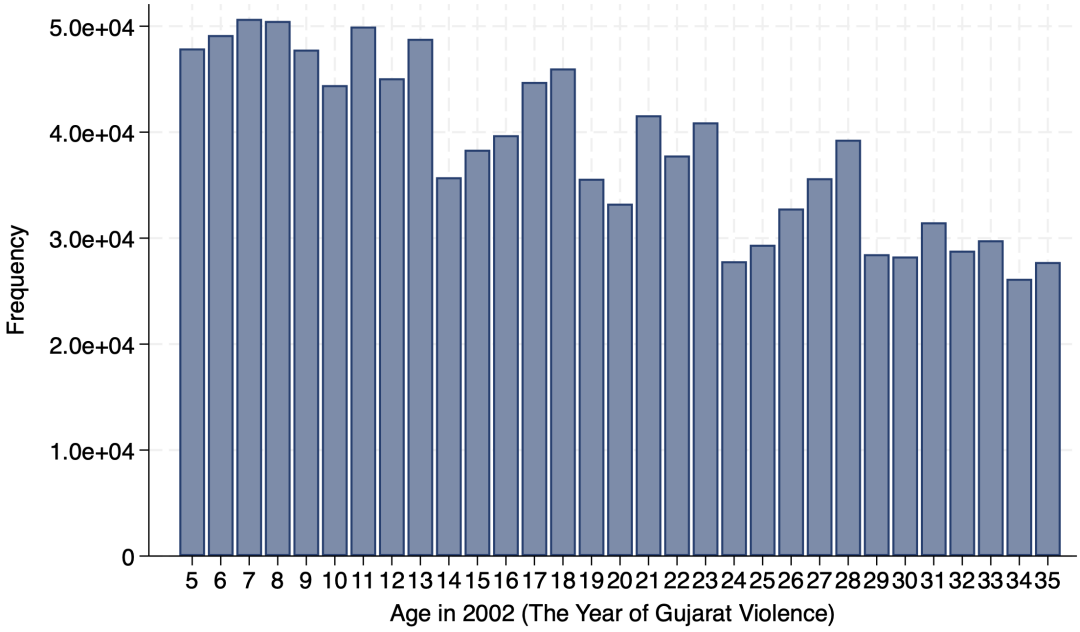

**(b)** Age in the Current Survey Year: Gujarat



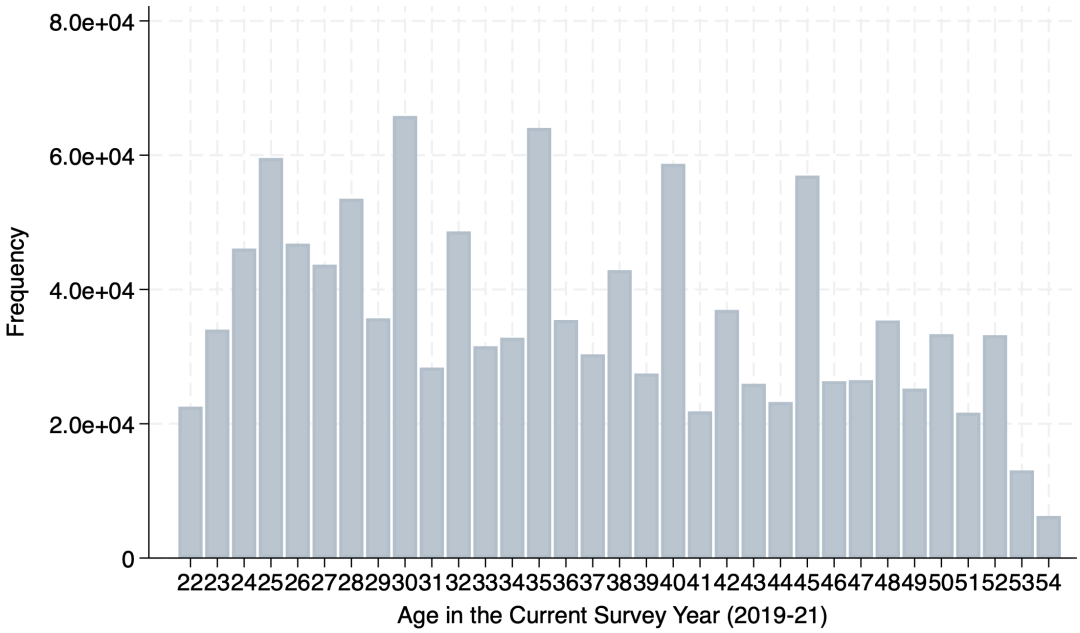

**Note:** This figure presents the distribution of individuals in the Gujarat sample across age cohorts using NFHS-5 data. Panel (a) displays the histogram of age in 2002, while Panel (b) shows the histogram of current age, which is used to calculate individuals' age in 2002.

**Figure A.8:** Distribution of the Sample in India by Age

**(a)** Age in 2002: Gujarat



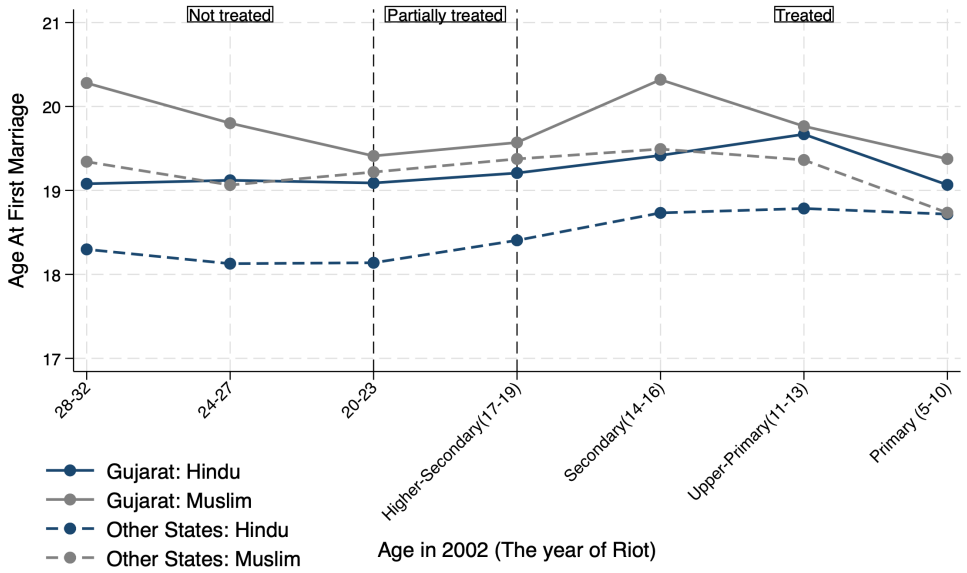

**(b)** Age in the Current Survey Year: Gujarat



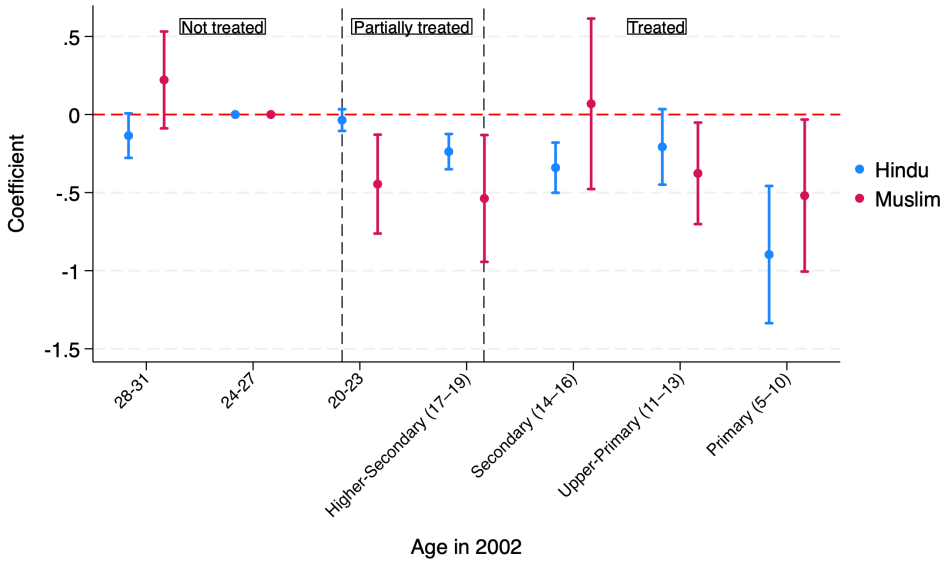

**Note:** This figure presents the distribution of individuals in India sample across age cohorts using NFHS-5 data. Panel (a) displays the histogram of age in 2002, while Panel (b) shows the histogram of current age, which is used to calculate individuals' age in 2002.

**Figure A.9:** Raw Means and Event Study Estimates - Age at Marriage for Women

**(a)** Raw Means - Age at First Marriage



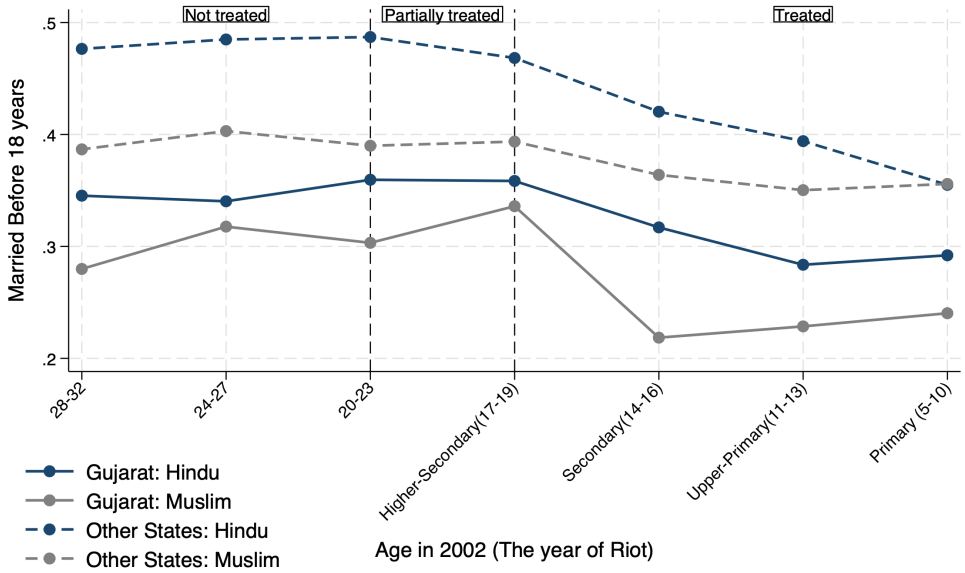

**(b)** Event-Study Estimates - Age at First Marriage



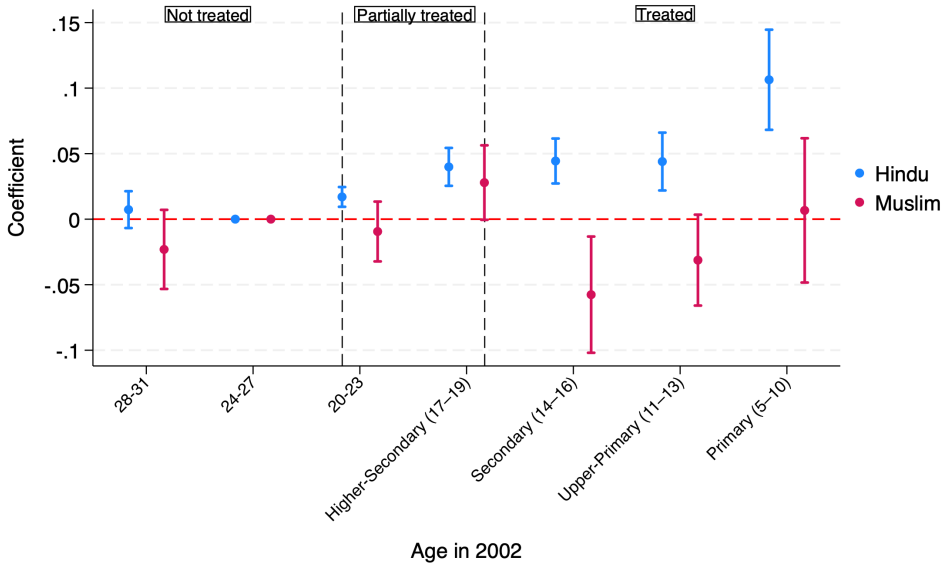

**Note:** This figure shows the outcome variable *Age at First Marriage* for women stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.10:** Raw Means and Event Study Estimates - Married before 18 years for Women

**(a)** Raw Means - Married Before 18 years



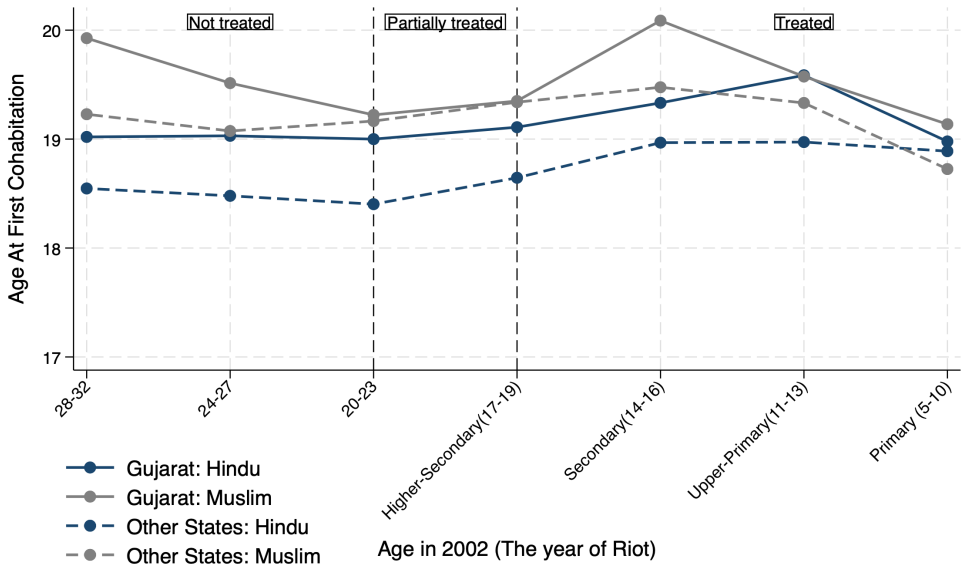

**(b)** Event-Study Estimates - Married Before 18 Years



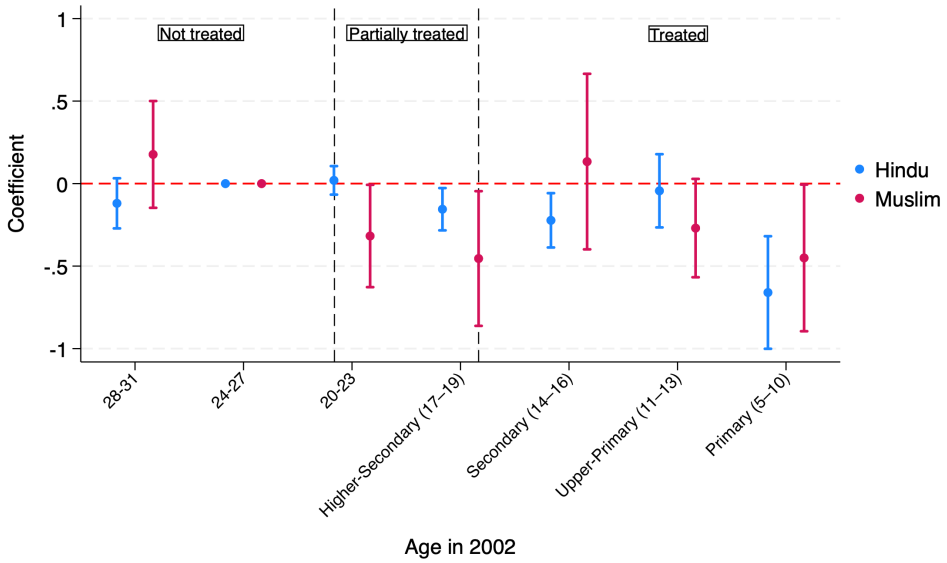

**Note:** This figure shows the outcome variable *Married before 18 years* for women stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.11:** Raw Means and Event Study Estimates - Age at First Cohabitation for Women

**(a)** Raw Means - Age at First Cohabitation



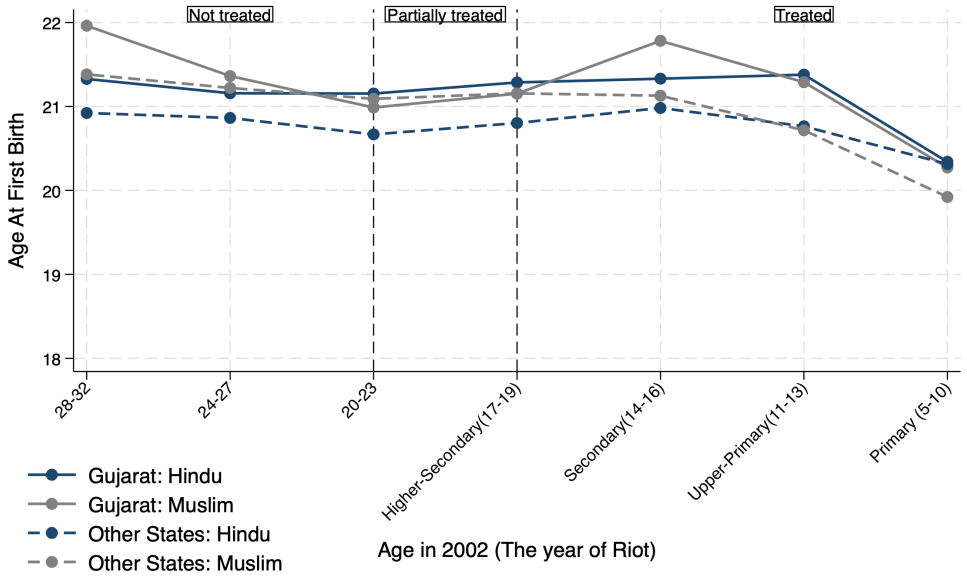

**(b)** Event-Study Estimates - Age at First Cohabitation



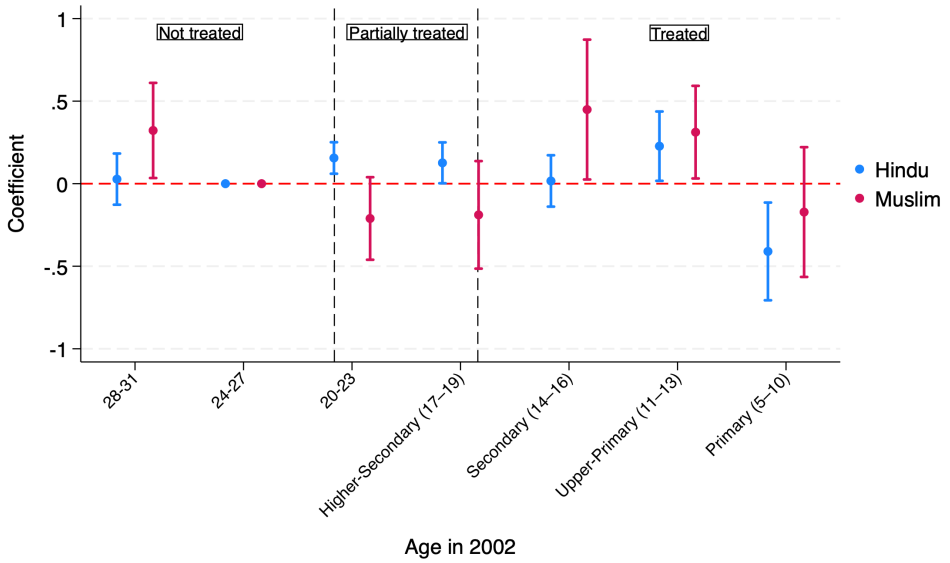

**Note:** This figure shows the outcome variable *Age at First Cohabitaion* for women stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.12:** Raw Means and Event Study Estimates - Age at First Birth for Women

**(a)** Raw Means - Age at First Birth



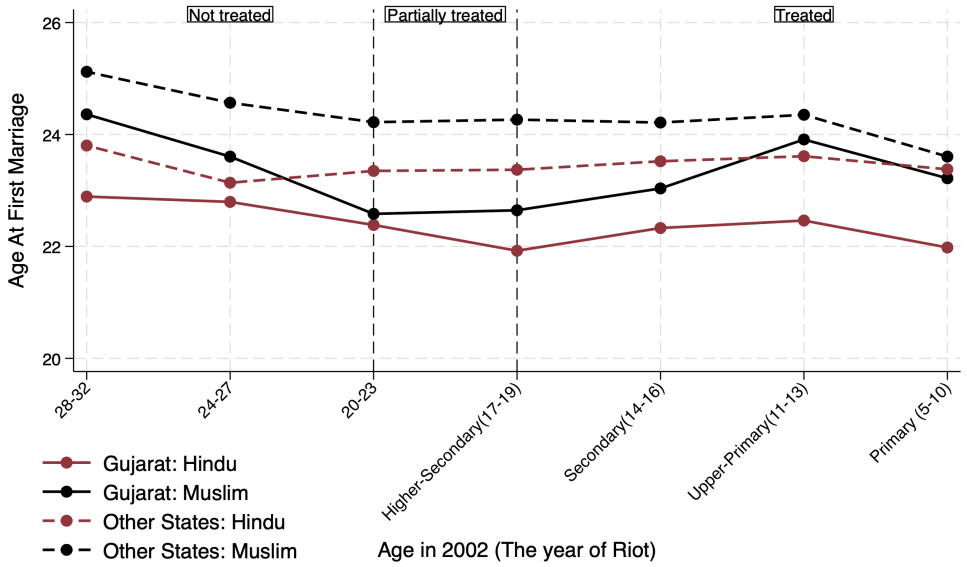

**(b)** Event-Study Estimates - Age at First Birth



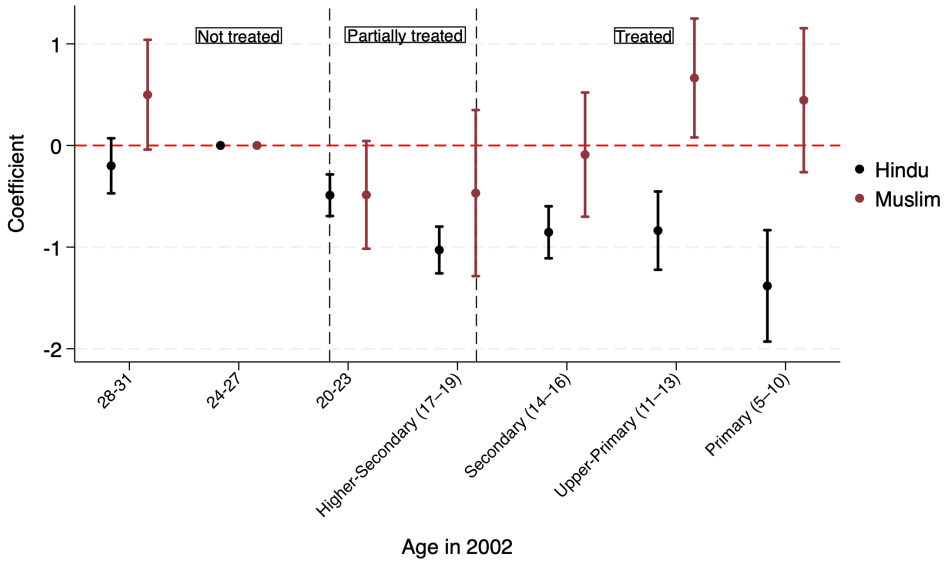

**Note:** This figure shows the outcome variable *Age at First Birth* for women stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.13:** Raw Means and Event Study Estimates - Age at First Marriage for Men

**(a)** Raw Means - Age at First Marriage



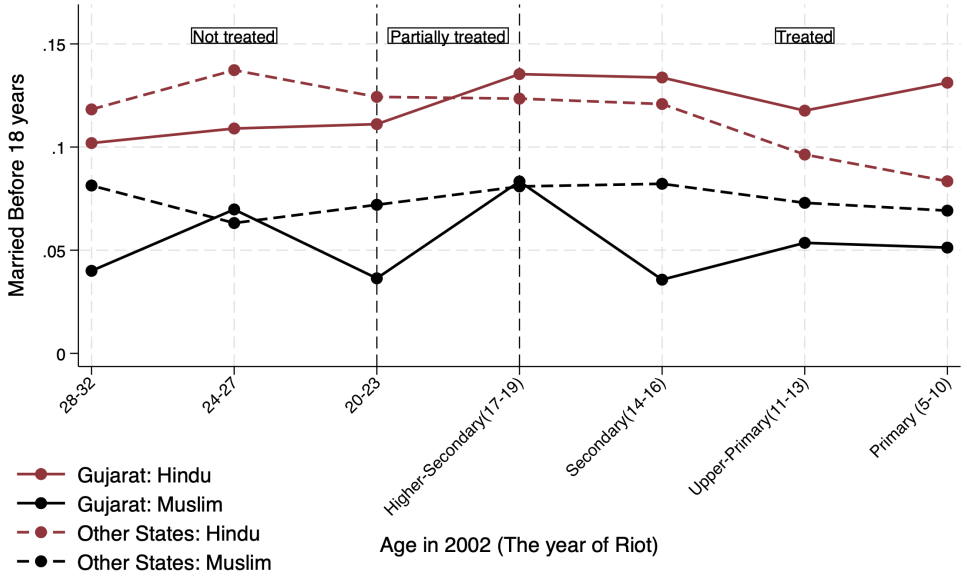

**(b)** Event-Study Estimates - Age at First Birth



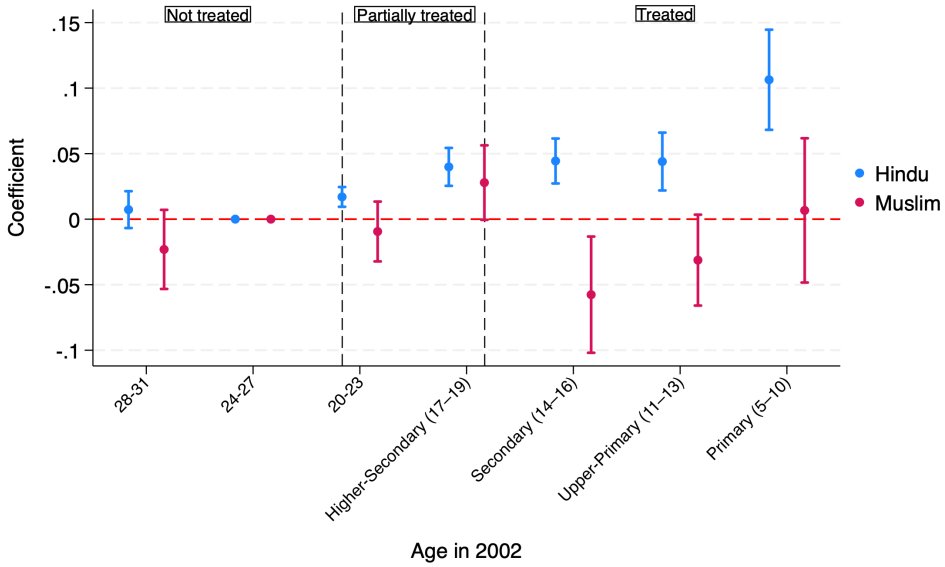

**Note:** This figure shows the outcome variable *Age at First Marriage* for Men stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.14:** Raw Means and Event Study Estimates - Married Before 18 Years for Men

**(a)** Raw Means - Married Before 18 Years

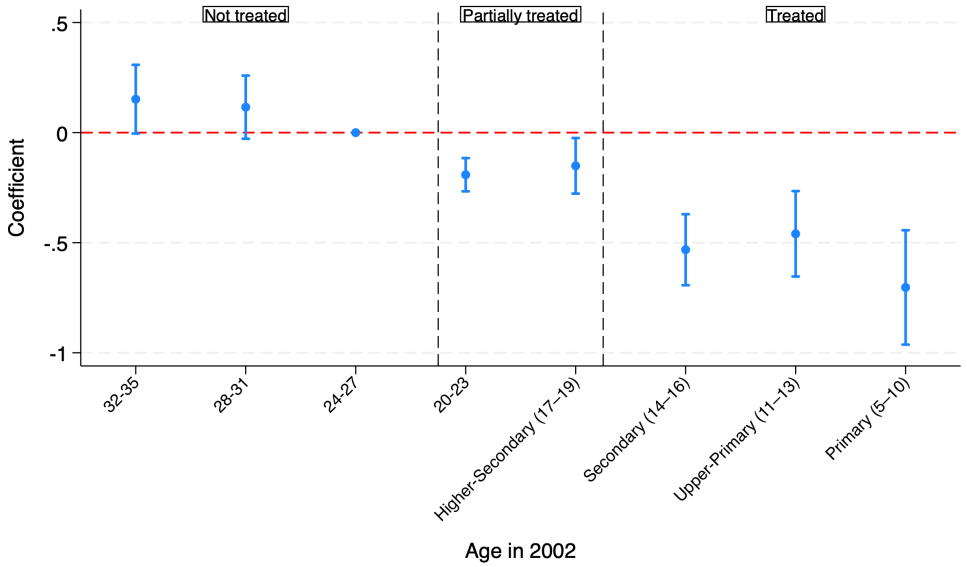



**(b)** Event-Study Estimates - Married Before 18 Years



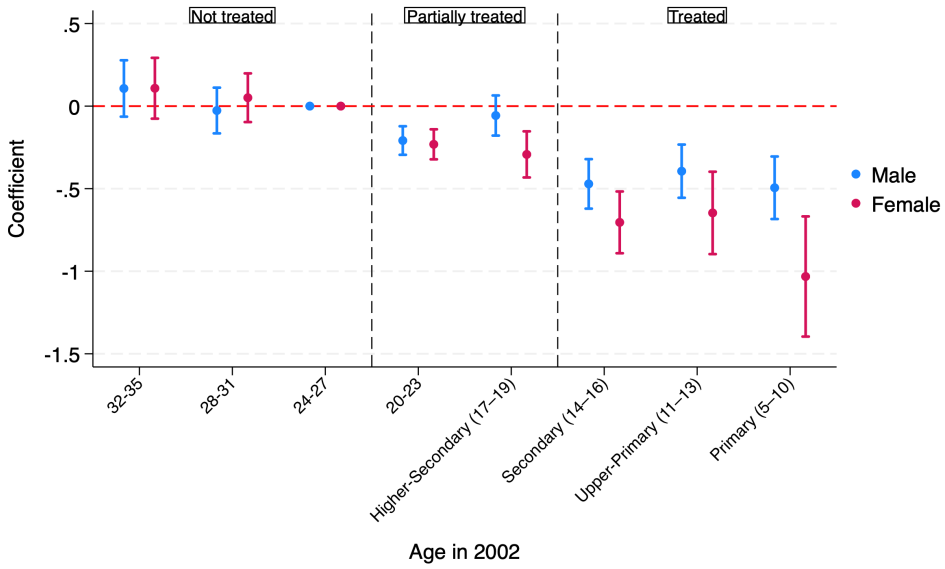

**Note:** This figure shows the outcome variable *Married Before 18 Years* for Men stratified by religion using data from NFHS-5. Subfigure (a) plots raw mean years of schooling for individuals residing in Gujarat and in other Indian states. Subfigure (b) presents event-study estimates based on Specification (2). Standard errors are clustered at the state level.

**Figure A.15:** Total Years of School: Dropping Spiked Ages



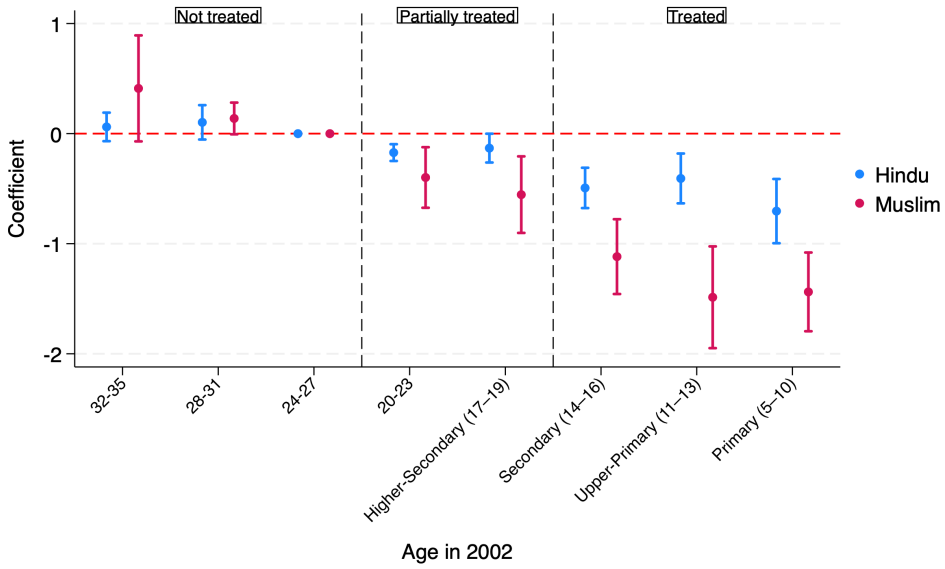

**Note:** This figure presents the outcome variable *Total Years of Schooling* using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping.

**Figure A.16:** Total Years of School by Gender: Dropping Spiked Ages



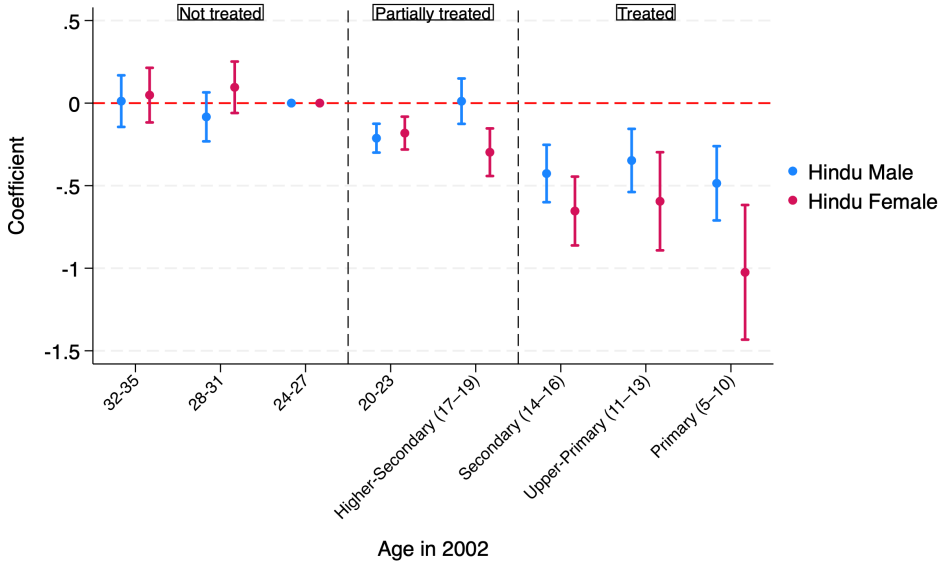

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping.

**Figure A.17:** Total Years of School by Religion: Dropping Spiked Ages



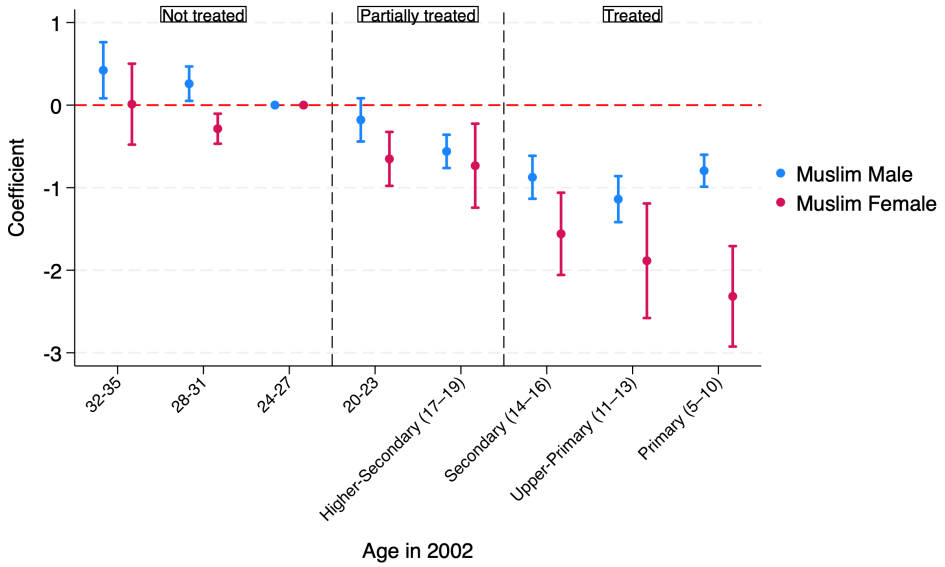

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Religion using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping.

**Figure A.18:** Total Years of School by Gender for the Hindu Population: Dropping Spiked Ages



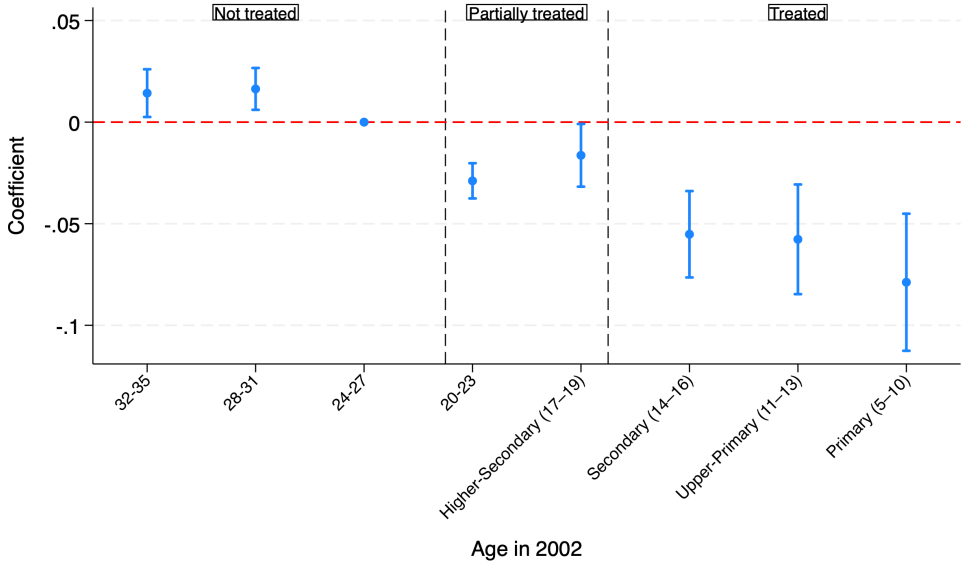

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender for the Hindu Population using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.19:** Total Years of School by Gender for the Muslim Population: Dropping Spiked Ages



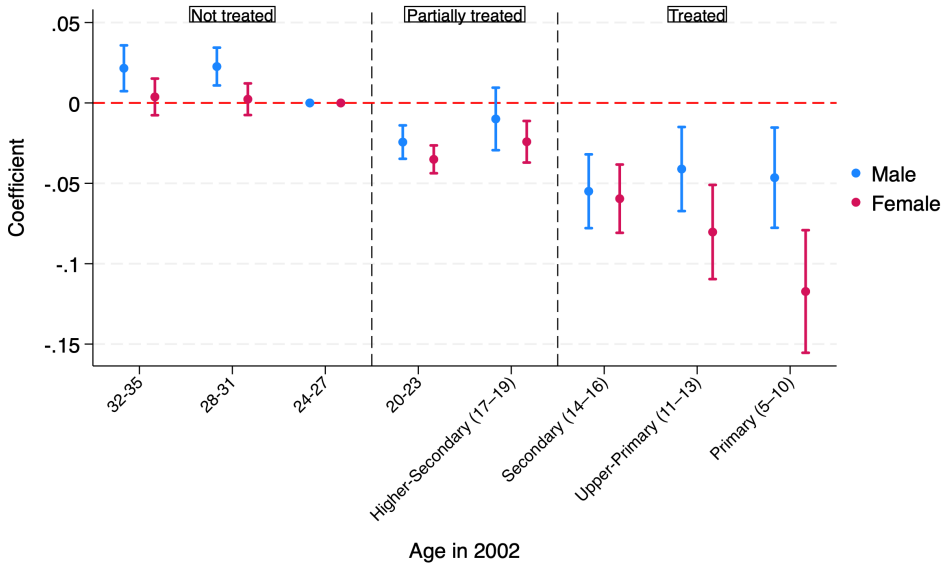

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender for the Muslim Population using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.20:** Grade 12 Completion: Dropping Spiked Ages



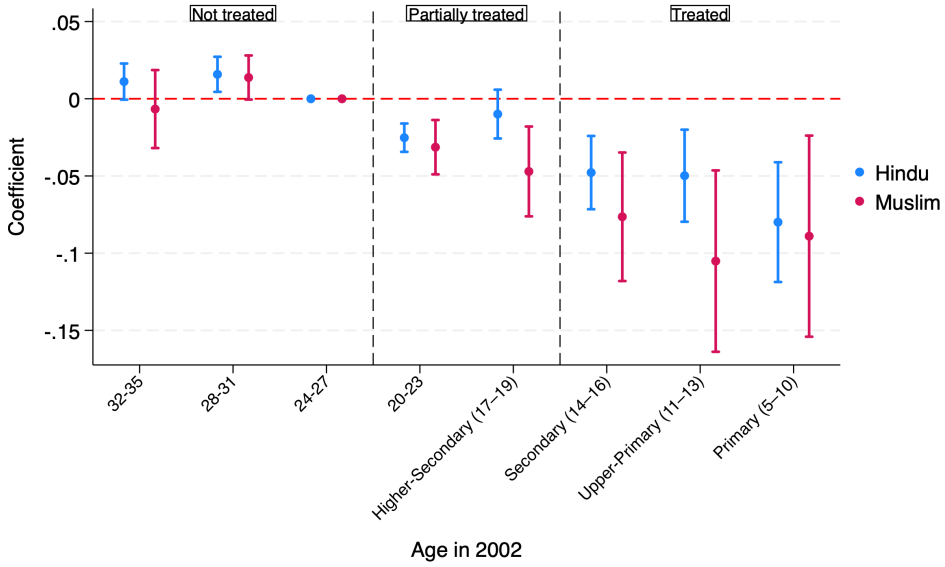

**Note:** This figure presents the outcome variable *Grade 12 Completion* using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.21:** Grade 12 Completion by Gender: Dropping Spiked Ages



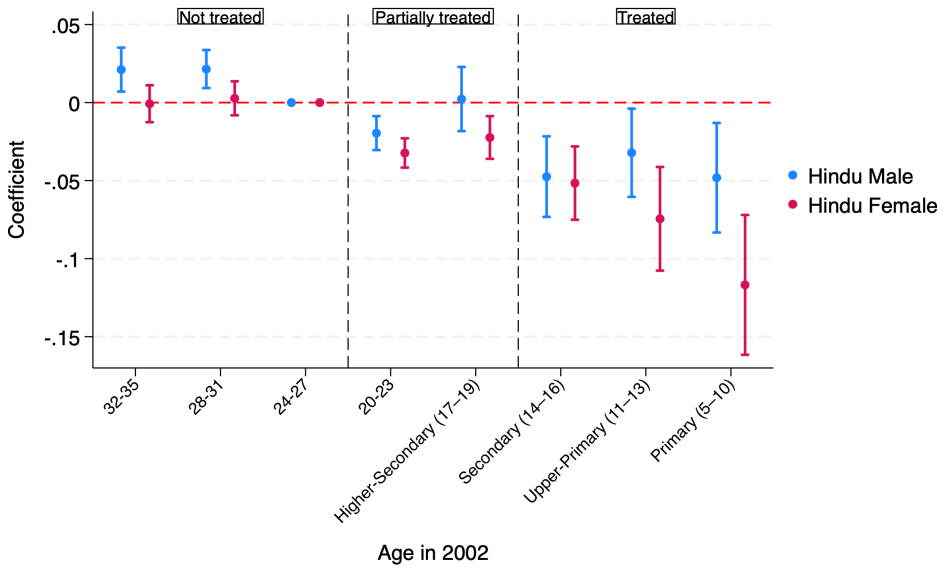

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.22:** Grade 12 Completion by Religion: Dropping Spiked Ages



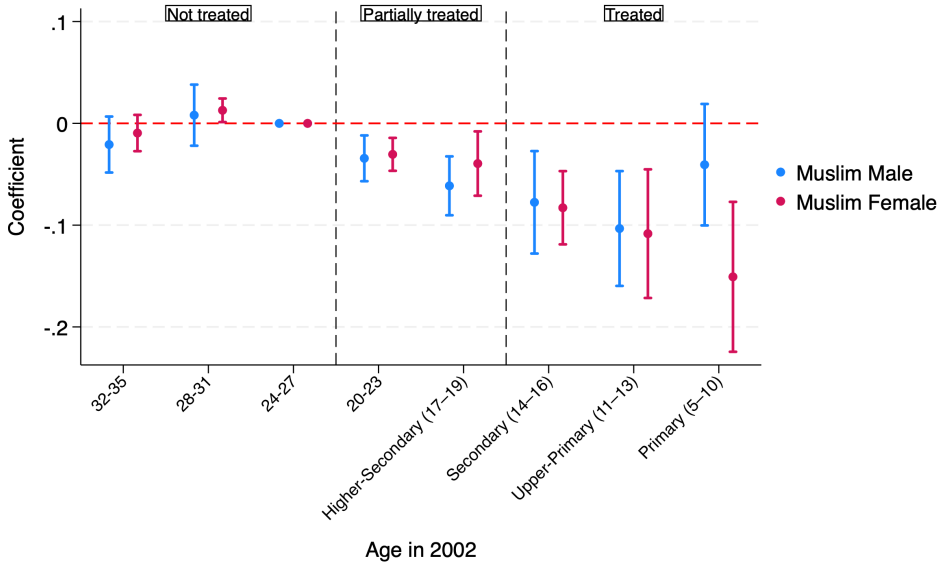

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Religion using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.23:** Grade 12 Completion by Gender for Hindu Population: Dropping Spiked Ages



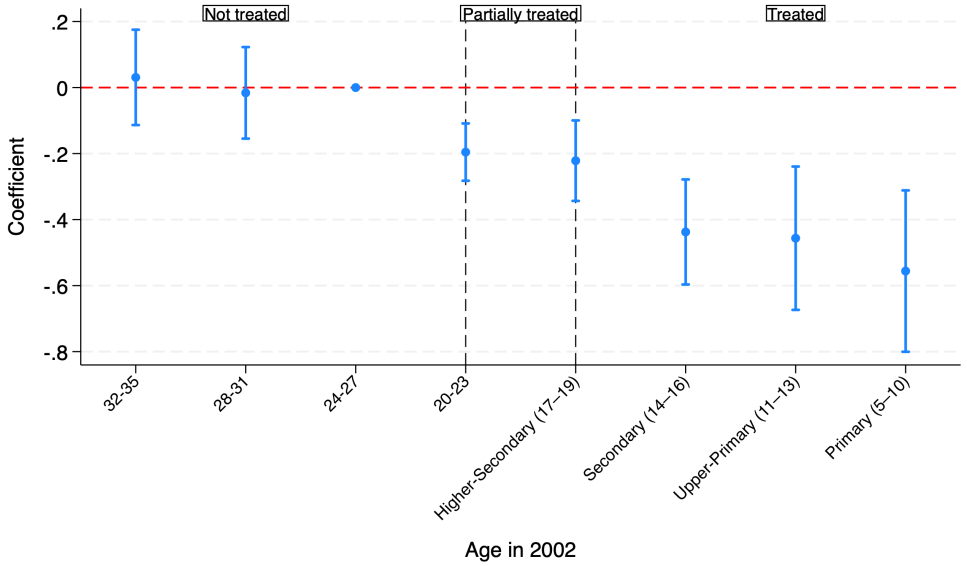

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender for Hindu Population using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.24:** Grade 12 Completion by Gender for Muslim Population: Dropping Spiked Ages



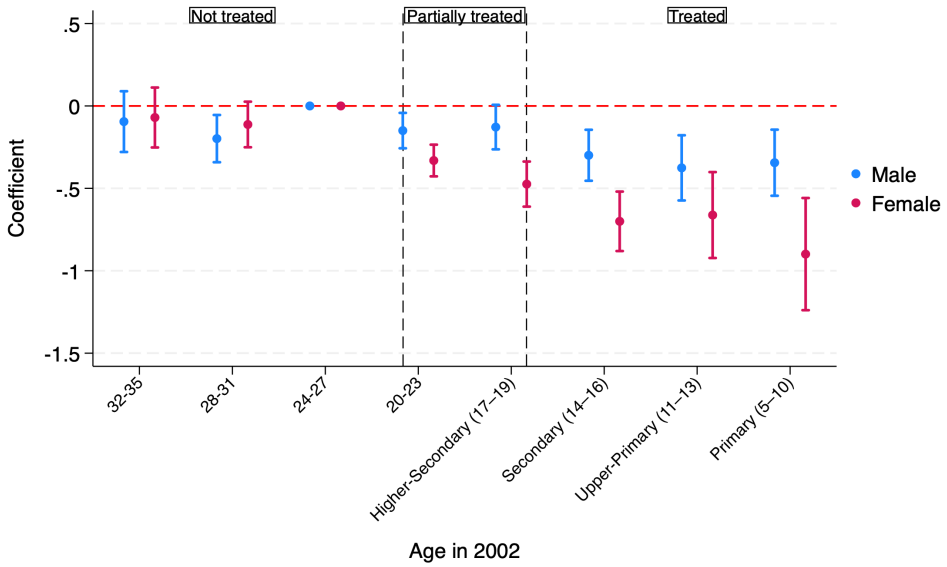

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender for Muslim Population using NFHS-5 data after excluding heaped ages (25, 30, 35, 40, 45, and 50) from the current survey year. This serves as an additional robustness check to ensure that the results are not driven by age heaping. .

**Figure A.25:** Total Years of School: Adding Controls



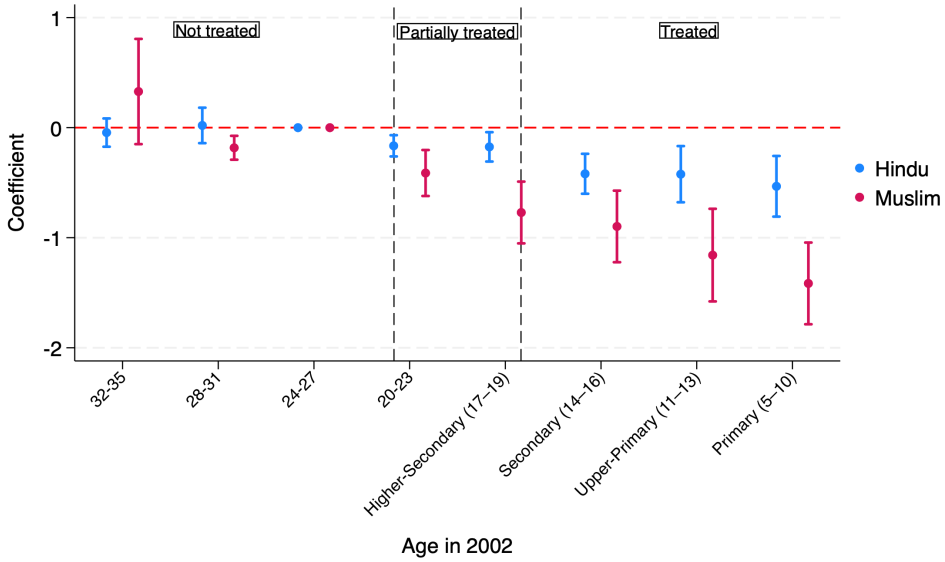

**Note:** This figure presents the outcome variable *Total Years of Schooling* using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.26:** Total Years of School by Gender: Adding Controls



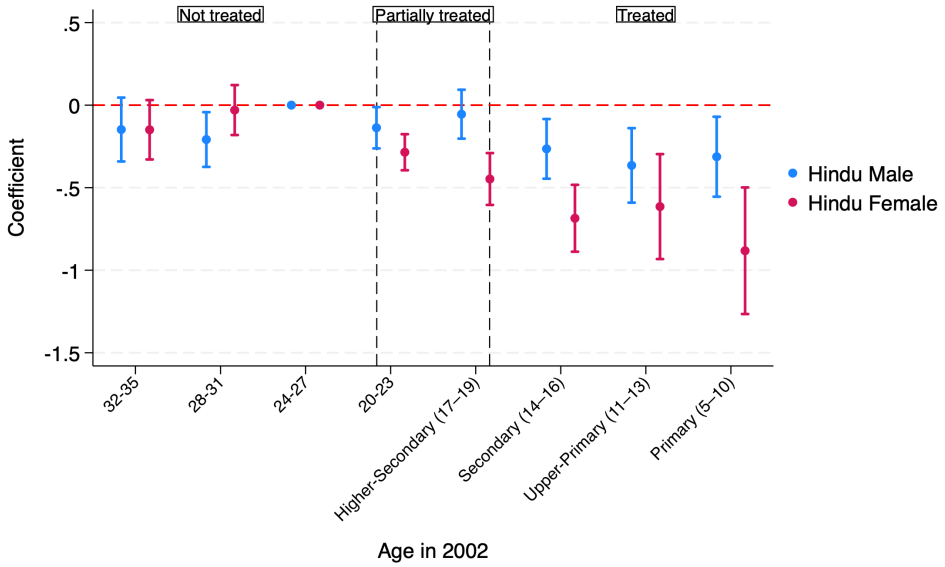

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.27:** Total Years of School by Religion: Adding Controls



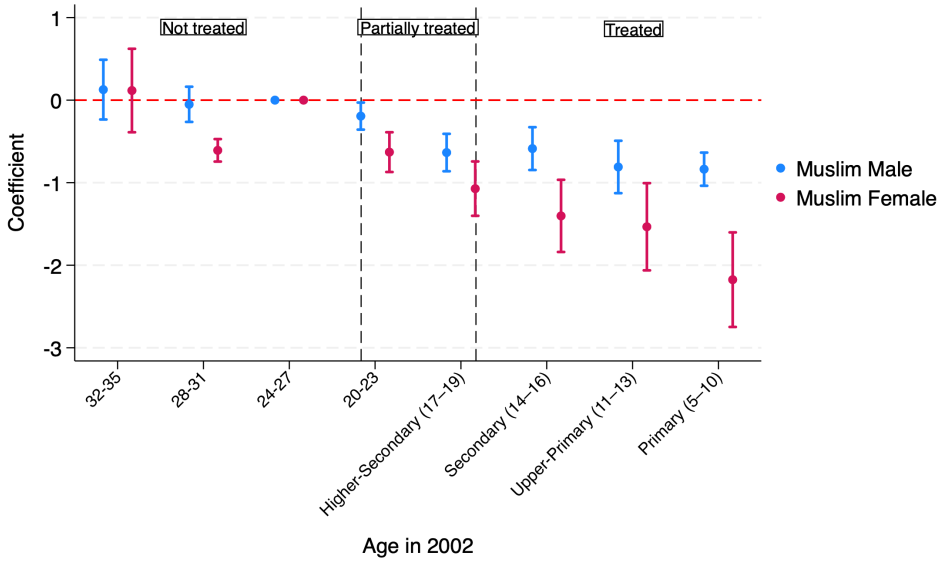

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Religion using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.28:** Total Years of School by Gender for the Hindu Population: Adding Controls



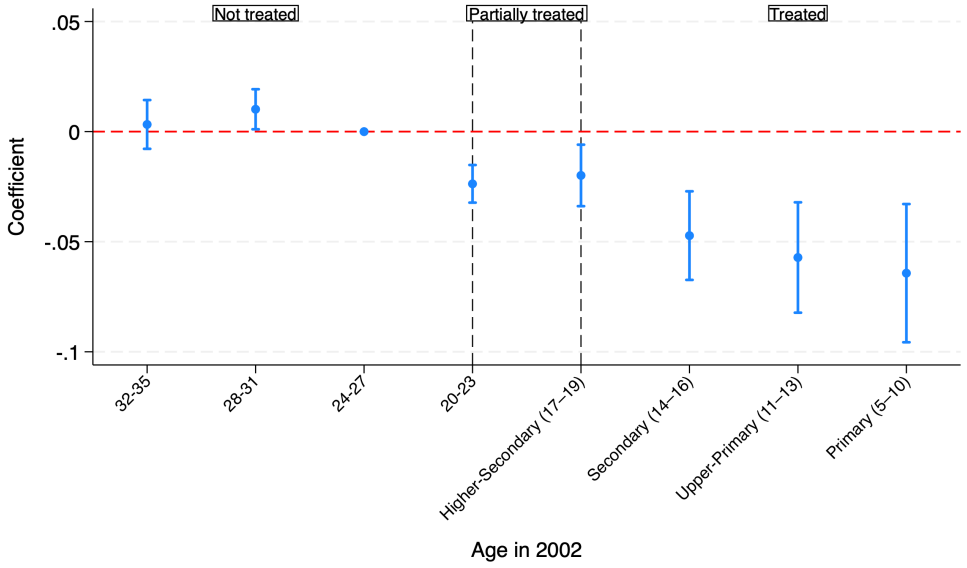

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender for the Hindu Population using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.29:** Total Years of School by Gender for the Muslim Population: Adding Controls



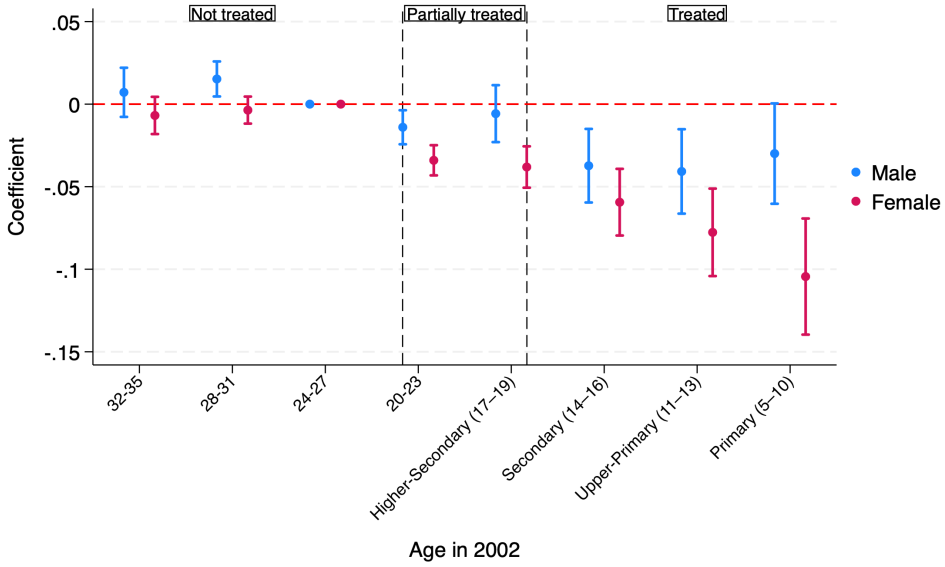

**Note:** This figure presents the outcome variable *Total Years of Schooling* by Gender for the Muslim Population using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.30:** Grade 12 Completion: Adding Controls



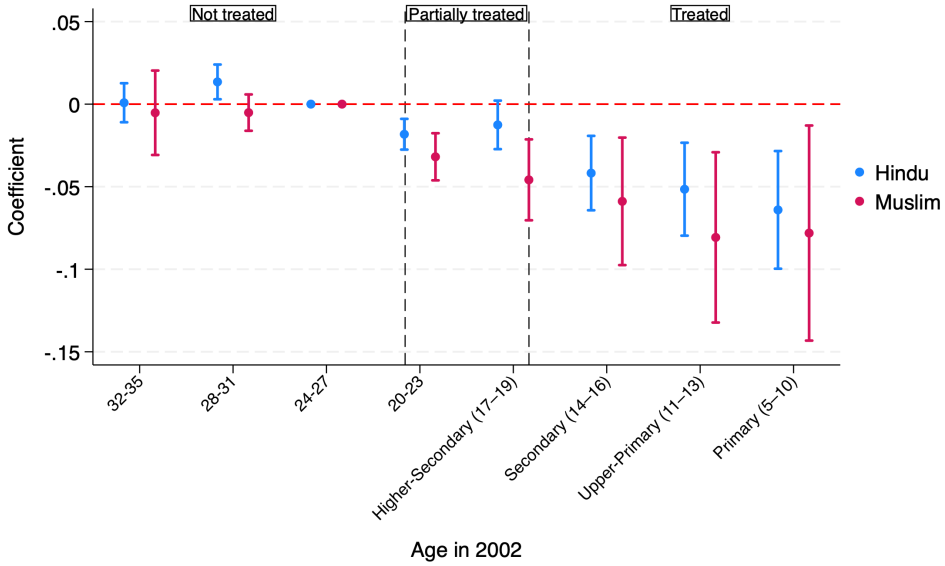

**Note:** This figure presents the outcome variable *Grade 12 Completion* NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.31:** Grade 12 Completion by Gender: Adding Controls



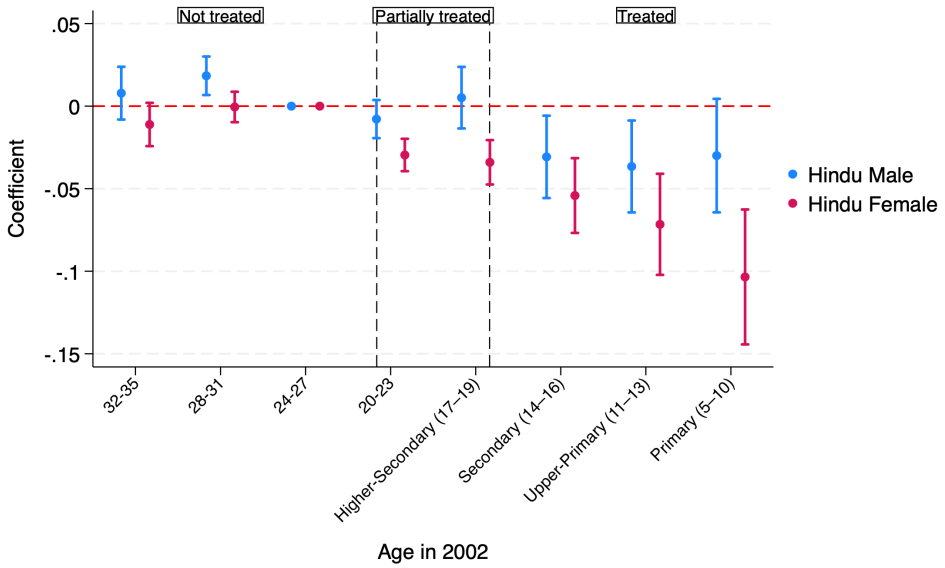

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.32:** Grade 12 Completion by Religion: Adding Controls



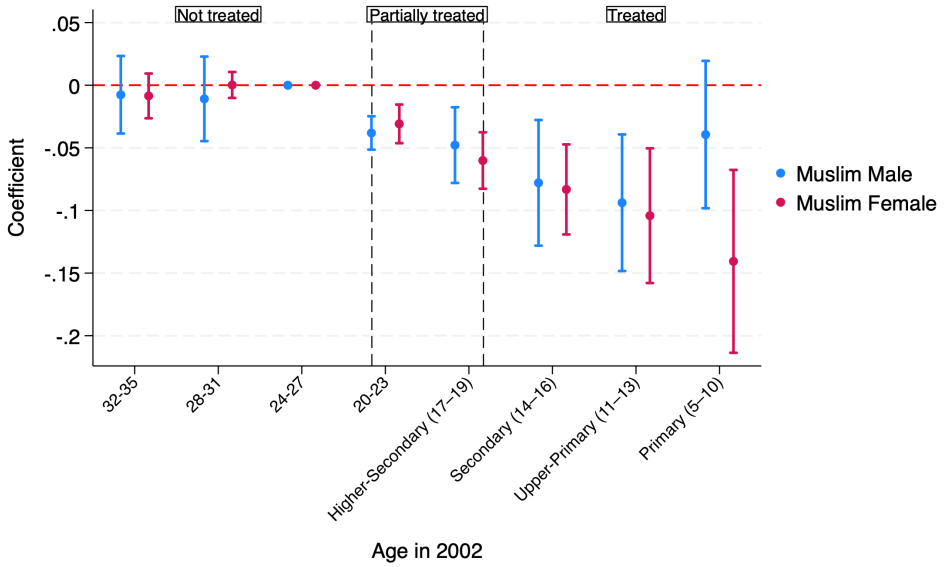

**Note:** This figure presents the outcome variable *Grade 12 Completion* by Religion using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.33:** Grade 12 Completion by Gender for the Hindu Population: Adding Controls




**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender for Hindu Population using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.

**Figure A.34:** Grade 12 Completion by Gender for the Muslim Population: Adding Controls




**Note:** This figure presents the outcome variable *Grade 12 Completion* by Gender for Muslim Population using NFHS-5 data, after controlling for wealth index (1–5), place of residence (urban/rural), and household size.