Educational Expansion and Assortative Matching: Evidence from the Marriage Market in India

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Abstract

Using staggered school construction across Indian districts, I estimate the effects of increased school access on women's educational attainment and marriage outcomes. An additional school per 1,000 school-age children, equivalent to about an 11 percent increase over the sample mean, raises women's years of schooling by nearly 2 percent and increases primary school completion by about 3 percent, with no significant effect on secondary education. Improved access to schooling also delays marriage, narrows the spousal age gap, and reduces child marriage: a comparable expansion raises the age at marriage by roughly 0.2 percent, decreases the spousal age gap by about 0.5 percent, and increases the probability of remaining unmarried by age 18 by approximately 0.5 percent. Beyond individual outcomes, school expansion reshaped marriage markets by strengthening educational sorting between spouses. Assortative matching increased among the least educated groups, while remaining comparatively stable or declining modestly at higher education levels, suggesting increased stratification at the lower end of the education distribution. Each additional school raises the assortative mating index by about 1.6 percent, and back-of-the-envelope calculations suggest that school expansion accounts for roughly 43 percent of the observed rise in assortative mating across marriage cohorts. However, I find little evidence that increased schooling improves women's intra-household outcomes: decision-making power remains unchanged, domestic violence exhibits strong intergenerational persistence, and there is no evidence of backlash driven by changes in relative spousal education. These findings indicate that large-scale investments in schooling improved women's human capital and delayed marriage while reshaping patterns of marital sorting, without substantially altering intra-household dynamics.

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1 Introduction

Investments in education influence not only individual human capital, but also patterns of marital matching. By changing the distribution of educated men and women, educational expansion reshapes marriage markets, altering who marries whom and how inequality is transmitted across generations. These effects may be particularly important in settings where marriage is nearly universal and women's labor market opportunities remain limited. In such contexts, education generates returns not only through wages, but also through the marriage market. Understanding how educational expansion affects marital matching is therefore important not only for evaluating the private returns to schooling, but also for understanding broader processes of stratification and intergenerational mobility.

India provides a particularly useful setting in which to study these dynamics. Over the past several decades, India has experienced a dramatic expansion in schooling, especially for women, alongside persistently low female labor force participation. Figure 1 shows that women's educational attainment increased substantially across birth cohorts. The gender gap in schooling narrows sharply across cohorts, with women's average years of education converging toward those of their husbands among younger cohorts. At the same time, marriage remains nearly universal and continues to structure household formation, fertility, and intergenerational mobility. Moreover, because marriages in India remain heavily family-mediated[1], educational attainment functions as an especially salient matching characteristic within marriage markets. Figure 2 shows corresponding differences in marriage timing across education groups, including later marriage and lower rates of child marriage among more educated women. These simultaneous changes raise the possibility that educational expansion affected not only schooling outcomes directly, but also the equilibrium structure of marriage markets. As the distribution of education changes, educational sorting between spouses may strengthen or weaken, marriage timing may shift, and the distribution of household advantage may become more or less stratified. These changes raise a broader question of whether educational expansion reshapes marriage markets primarily through changes in the distribution of human capital, or whether it also alters patterns of matching and household formation more

[1]Based on data from the India Human Development Survey, approximately 66 percent of ever-married women reported that their marriages were arranged primarily by parents or relatives, while an additional 29 percent reported that the match was chosen jointly by themselves and their families. Only about 5 percent reported choosing their spouse independently.

fundamentally.

A large literature studies the returns to education through labor market outcomes. However, education may also generate substantial returns through marriage markets. In particular, education may affect the probability and timing of marriage, partner quality, assortative matching, and intra-household allocations (Behrman et al., 1999; Peters, 2002; Desai and Andrist, 2010; Lafortune, 2013; Royer and Geruso, 2014; Ashraf et al., 2020; Anderberg et al., 2022; Agarwal, Bahure and Javadekar, 2023). Theoretical work in economics has long emphasized that educational sorting may arise through multiple channels. Individuals with similar educational attainment may derive gains from shared preferences and consumption complementarities (Becker, 1981; Lundberg and Pollak, 2012). Education may also increase household productivity through complementarities in investments in children and household production (Behrman and Rosenzweig, 2002; Chiappori, Dias and Meghir, 2018). In addition, education may shape bargaining power and the distribution of surplus within households (Choo and Siow, 2006; Mourifié and Siow, 2017; Doorley, Dupuy and Weber, 2019). At the same time, rising educational attainment may mechanically increase educational homogamy even in the absence of stronger assortative preferences. Distinguishing between these forces is important because assortative matching has broader equilibrium implications for inequality and intergenerational persistence (Fernández, Guner and Knowles, 2005; Eika, Mogstad and Zafar, 2019; Greenwood et al., 2014).

The paper also relates to recent work on school construction and marriage outcomes in India. Khanna (2023) studies the general equilibrium labor market effects of India's District Primary Education Program (DPEP), while Agarwal, Bahure and Javadekar (2023) examine the effects of school expansion on women's marriage outcomes using a regression discontinuity design. In contrast, I exploit continuous variation in school construction intensity across districts and cohorts over a substantially longer horizon and study not only marriage timing but also equilibrium changes in assortative matching and marriage-market stratification.

Building on this literature, this paper studies how large-scale school construction in India reshaped educational attainment, marriage timing, assortative matching, and intra-household outcomes. I combine administrative data on school construction with nationally representative Demographic and Health Survey (DHS) data and exploit variation across districts and birth cohorts in exposure to school construction during ages 6–15. The empirical design follows Duflo (2001), using district and cohort variation in school construction intensity over a substantially longer time horizon. The analysis examines both individual-level outcomes, such as schooling and age at marriage, and equilibrium marriage-market outcomes, including educational sorting and assortative mating.

I find that school construction substantially increased women's educational attainment, particularly at lower levels of schooling. An additional school per 1,000 school-age children, approximately an 11 percent increase relative to the sample mean, raises women's years of education by nearly 2 percent and significantly increases primary school completion, with little effect on secondary completion. Educational expansion also delayed marriage, reduced child marriage, and narrowed the age gap between spouses. Beyond these individual-level effects, I show that school construction reshaped the equilibrium structure of marriage markets. Educational sorting strengthened as school access expanded, with each additional school increasing the assortative mating index by approximately 1.6 percent. Homogamy among the least educated increased substantially across marriage cohorts, even as average educational attainment rose. Back-of-the-envelope calculations suggest that school construction explains nearly 43 percent of the observed rise in educational assortative mating across marriage cohorts. These patterns imply that educational expansion may simultaneously increase average human capital accumulation while increasing educational segmentation among the least educated.

At the same time, I find little evidence that educational expansion substantially altered intra-household gender dynamics. Increased schooling does not meaningfully improve women's decision-making power or reduce domestic violence, and domestic violence remains strongly persistent across generations. Nor do I find evidence that increases in women's relative education generate backlash within marriage. Taken together, the results suggest that educational expansion reshaped who marries whom and when households form, while leaving deeply entrenched intra-household norms largely unchanged.

This paper contributes to several literatures. First, it contributes to work on the returns to education by showing that large-scale educational expansion affects not only labor market outcomes, but also marriage markets and household formation. While Duflo (2001) studies the labor market consequences of school construction in Indonesia, I examine how educational expansion reshapes marriage markets in a setting where women's labor force participation remains limited. Second, the paper contributes to the literature on assortative mating by providing new evidence from a developing-country setting undergoing rapid educational expansion. While prior work has documented rising assortative mating in high-income countries (Fernández, Guner and Knowles, 2005; Eika, Mogstad and Zafar, 2019), much less is known about how educational expansion reshapes sorting in marriage markets in low- and middle-income countries. Third, the paper contributes to a growing literature on educational expansion and inequality by showing that schooling investments may increase stratification within marriage markets even while expanding access to education overall.

2 Institutional Context on School Expansion and Marriage Markets in India

School access expanded dramatically in India after Independence through a series of large-scale public investments in educational infrastructure. Beginning in the 1970s, the Indian government implemented successive programs aimed at universalizing elementary education and reducing geographic disparities in school access. The Minimum Needs Programme launched in the mid-1970s sought to ensure that every village had access to a primary school within a specified distance. Subsequent initiatives, including the District Primary Education Programme (DPEP) in the 1990s and the Sarva Shiksha Abhiyan (SSA) in the 2000s, substantially expanded school construction, teacher recruitment, and educational funding across districts. As a result of these policies, school availability increased sharply over time. Figure 3 shows that younger birth cohorts were exposed to substantially higher levels of school availability during their school-age years. Average exposure more than doubled between the 1965 and 1995 cohorts, generating substantial variation across cohorts in access to schooling. According to Census data, the share of villages with a primary school increased from approximately 53 percent in 1971 to over 70 percent by 1991, while access to middle and high schools also expanded substantially. By the 2010s, most rural households had access to a primary school within walking distance. In addition to public investments, India also experienced a large increase in low-cost private schooling during this period.

These educational expansions coincided with major changes in women's educational attainment and marriage patterns. Figure 4 illustrates how the educational composition of women changed across cohorts over the same period. The share of women with no schooling declines sharply between the 1965 and 1995 birth cohorts, while the shares completing secondary and college education increase steadily over time. Average years of schooling also rose substantially across cohorts, accompanied by a narrowing gender gap in educational attainment. These descriptive patterns are consistent with educational expansion substantially altering the distribution of education within local marriage markets, potentially changing both the availability of similarly educated partners and the structure of marital matching.

Marriage continues to play a central role in social and economic life in India. Marriage is nearly universal, and most marriages remain arranged or family-mediated, with partner search often occurring within relatively narrow caste, linguistic, religious, and regional networks (Banerjee et al., 2013). Educational attainment has increasingly become an important dimension along which matches are formed. Unlike labor market outcomes—which remain highly gendered and are frequently observed only after marriage—education is observable prior to marriage, widely understood by families, and directly shaped by policy-driven investments in human capital. These features make education a particularly salient characteristic in the marriage market and a natural dimension along which assortative matching may occur.

To provide supporting evidence that education is an important attribute in partner selection, I construct a novel dataset of approximately 15,000 matrimonial advertisements published in a major Hindi-language newspaper between 2022 and 2025.[2] The advertisements reveal that education is among the most universally valued characteristics in partner search for both men and women. Approximately three-quarters of both male and female profiles explicitly mention education, indicating that schooling has become a near-universal characteristic in self-presentation within marriage markets. In contrast, labor market characteristics remain substantially more gendered. Nearly half of female profiles seeking grooms mention employment or occupation requirements, compared to fewer than one-fifth of male profiles seeking brides. These descriptive patterns suggest that education functions as a common dimension of matching for both genders, while labor market characteristics remain disproportionately important for men. This asymmetry is consistent with a setting in which educational expansion may reshape marital matching even in

[2]Appendix Table B1 summarizes these patterns.

the presence of persistently low female labor force participation.

3 Conceptual Framework

Educational expansion may affect marriage markets through multiple channels. First, increased access to schooling raises educational attainment directly, altering the distribution of educated men and women within marriage markets. As the supply of educated individuals changes, patterns of matching may also change. If individuals prefer spouses with similar educational attainment, increases in schooling may strengthen educational assortative mating.

Positive assortative matching may arise for several reasons. Individuals with similar educational backgrounds may share preferences, lifestyles, or social environments, generating consumption complementarities within marriage (Becker, 1981; Lundberg and Pollak, 2012). Education may also increase household productivity through complementarities in investments in children, household decision-making, and specialization (Behrman and Rosenzweig, 2002; Chiappori, Dias and Meghir, 2018). Alternatively, increases in educational homogamy may emerge mechanically as educational attainment rises across the population, even in the absence of stronger assortative preferences. Distinguishing between these mechanisms is important because they imply different consequences for inequality and household formation (Almar et al., 2025).

Assortative matching has broader equilibrium implications beyond individual marriage outcomes. Stronger educational sorting can concentrate human capital within households and increase intergenerational persistence in socioeconomic status (Fernández, Guner and Knowles, 2005; Greenwood et al., 2014; Eika, Mogstad and Zafar, 2019). Conversely, educational expansion could weaken stratification if increased access broadens the set of feasible matches across educational groups. The distributional consequences therefore depend not only on whether schooling increases average attainment, but also on how it changes the structure of matching across households. Figure 5 illustrates these patterns by showing substantial increases in assortative matching among the least educated groups across marriage cohorts.

A key empirical challenge is distinguishing changes in assortative mating from changes in the marginal distribution of education. As educational attainment rises over time, educational homogamy may increase mechanically simply because more individuals attain similar levels of schooling. Following Eika, Mogstad and Zafar (2019), I therefore use a measure of assortative mating that accounts for changes in the marginal distribution of education and captures whether individuals with similar educational attainment marry each other more frequently than would occur under random matching.

Educational expansion may also affect the timing of marriage and intra-household dynamics. Increased schooling may delay marriage directly through enrollment or incapacitation effects if women remain in school longer. Education may additionally alter reservation quality in the marriage market, leading households to delay marriage in search of better matches. At the same time, increased education could improve women's bargaining power within marriage by increasing outside options or productivity. However, if labor market opportunities remain constrained or gender norms remain persistent, educational expansion may reshape marriage markets without substantially changing intra-household power relations. The empirical analysis examines each of these channels in turn.

4 Data

This paper combines administrative data on school construction with nationally representative household survey data and district-level population statistics to construct a long-run panel of educational exposure across Indian districts and cohorts.

Data on school construction and school locations come from the Unified District Information System for Education (UDISE), compiled by the Department of Education, Government of India. UDISE is the most comprehensive administrative database on schools in India and covers approximately 1.5 million public and private schools across the country. The database contains detailed school-level information including school location, management type, grade levels offered, infrastructure, enrollment, teacher characteristics, and year of establishment. For the purposes of this paper, I primarily use information on school location and year of establishment to construct district-level measures of cumulative school exposure over time. Although the UDISE data was collected in 2018 through school-level surveys, the database records establishment years for all schools that existed at the time of the survey, allowing the construction of a long-run historical panel of school expansion. I aggregate school establishment information to the district-year level and construct cumulative measures of schools per 1,000 school-age children. The treatment variable therefore captures the stock of schools available in a district during an individual's school-age years rather than contemporaneous school construction flows. One limitation of the UDISE data is that it only includes schools operating at the time of the 2018 survey. Schools that were constructed historically but shut down before the survey are not observed. This may generate measurement error in historical school exposure, particularly for earlier cohorts whose exposure occurred further in the past. If school closures are unrelated to later marriage outcomes conditional on district and cohort fixed effects, the resulting measurement error would tend to attenuate estimated effects toward zero. However, because older schools are mechanically more likely to be missing, some non-classical measurement error remains possible. At the same time, available evidence suggests that the period under study was characterized primarily by rapid educational expansion rather than widespread school closures, particularly during earlier decades.

Information on education, marriage, and intra-household outcomes comes from the Indian Demographic and Health Survey (DHS), also known as the National Family Health Survey (NFHS) in India. The DHS is a nationally representative household survey that collects detailed demographic, educational, fertility, health, and domestic violence information. The primary analysis uses data from the women's individual recode, which surveys women aged 15–49 and contains detailed information on educational attainment, age at marriage, fertility histories, household characteristics, and measures of women's autonomy and domestic violence. A key advantage of the DHS is the availability of detailed retrospective marriage and fertility information, which allows outcomes to be linked to childhood exposure to school construction. Because spouse education variables in the women's survey are available only for a restricted subsample of respondents, I supplement this data with information from the household roster. Husband characteristics, including years of schooling and age, are linked using the spouse line number reported in the women's questionnaire, allowing the construction of measures of educational assortative matching and relative spousal education for co-resident couples. To examine intra-household outcomes, I additionally use the domestic violence module, which contains information on women's participation in household decision-making, controlling behavior by husbands, and emotional, physical, and sexual violence. District identifiers are only publicly available in the Indian DHS beginning with the 2015 round. As a result, the main analysis relies primarily on the 2019 survey round, which provides the most recent nationally representative data with district-level geographic identifiers. The analysis links women to district-level school construction exposure based on district of current residence. While the DHS does not observe district of childhood residence directly, migration during school-age years is relatively limited in India. Moreover, Census evidence suggests that most marriage migration occurs within relatively local geographic areas, implying that district of residence remains a reasonable proxy for childhood exposure for much of the population.

Population data are drawn from decadal Census of India population tables. Specifically, I use district-level population counts from the Census and decadal variation tables to construct measures of school-age population over time. Population figures are linearly interpolated across years within decades in order to construct annual district-level estimates. Importantly, the Census tables account for administrative boundary changes when estimating historical district populations, which is critical for constructing consistent exposure measures over time.[3] A major challenge in conducting long-run district-level analysis in India is the large number of changes in district and state boundaries since the first post-Independence census in 1961. Over time, districts have been repeatedly split, merged, or reorganized, while new states have also been created. These changes complicate efforts to combine historical administrative data with modern survey datasets. A key contribution of this paper is the construction of a harmonized long-run district-level panel that reconciles these boundary changes across datasets and over time. Historical school construction data, population data, and DHS district identifiers are mapped into a common district framework based on harmonized 2011 district boundaries, allowing consistent measurement of school exposure across cohorts and regions over multiple decades.

5 Empirical Strategy

The empirical analysis exploits variation across districts and birth cohorts in exposure to school construction during school-age years. School construction in India was governed largely by centralized population-and-distance norms under successive Five-Year Plans and later the Sarva Shiksha Abhiyan (SSA), with funding tied to predetermined infrastructure targets rather

[3] Additional details on the construction of the harmonized district panel are provided in Appendix A.1

than contemporaneous local marriage-market conditions. Once constructed, schools represent long-lived infrastructure investments, making reverse causality from cohort-specific marriage outcomes implausible. In addition, treatment exposure is intended to capture school construction exposure during childhood, implying that variation in exposure is predetermined with respect to adulthood marriage outcomes.

The empirical design follows Duflo (2001) and exploits both spatial and temporal variation in school construction intensity. Specifically, I estimate regressions of the form:

$$Y_{idt} = \beta \text{SchoolExposure}_{dt} + \gamma_t + \delta_d + \varepsilon_{idt} \tag{1}$$

where Y_{idt} denotes the outcome of individual i born in cohort t and residing in district d, γ_t are cohort fixed effects, and δ_d are district fixed effects. The treatment variable, $\text{SchoolExposure}_{dt}$, measures cumulative schools constructed per 1,000 school-age children in district d during the years in which cohort t was between ages 6 and 15. Standard errors are clustered at the district level throughout.

The identifying assumption is that, absent school construction, outcomes across districts would have evolved similarly over cohorts. District fixed effects absorb time-invariant differences across districts, including baseline educational infrastructure and long-run socioeconomic characteristics, while cohort fixed effects absorb aggregate national trends affecting all districts similarly. Identification therefore comes from differences in the intensity of school expansion across districts experienced by different cohorts during school-age years.

I focus on several sets of outcomes. First, I estimate the effect of school construction on women's educational attainment, including years of schooling and completion of primary and secondary education. Second, I examine marriage-market outcomes, including age at marriage, the probability of remaining unmarried by age 18, and the spousal age gap. Third, I study equilibrium changes in matching patterns through measures of educational homogamy and assortative mating. Finally, I examine whether educational expansion altered intra-household outcomes such as women's decision-making power and domestic violence.

The primary treatment measure is continuous and captures cumulative school exposure during school-age years. I estimate the effect of adding one additional school per 1,000 school-age children to a district, which corresponds to approximately an 11 percent increase relative to the sample mean of 8.885 schools per thousand children. Following Duflo (2001), I additionally present specifications using a binary treatment measure equal to one if school exposure is above the cohort-specific mean and zero otherwise. The results are qualitatively similar across specifications.

A central outcome of interest is educational assortative mating. A key challenge in measuring assortative matching is distinguishing changes in matching behavior from changes in the marginal distribution of education. As educational attainment rises over time, educational homogamy may increase mechanically even in the absence of stronger assortative preferences. To address this issue, I follow Eika, Mogstad and Zafar (2019) and construct an assortative mating index that measures whether individuals with a given education level marry each other more frequently than would occur under random matching.

Marital sorting between women with education level e_f and men with education level e_m is defined as:

$$s(e_f, e_m) = \frac{Pr(E_f = e_f, E_m = e_m)}{Pr(E_f = e_f)Pr(E_m = e_m)} \tag{2}$$

The index equals one under random matching, while values greater than one indicate positive assortative matching. I focus primarily on same-education matches and construct district-by-marriage-cohort measures of assortative mating using weighted joint and marginal education distributions from the DHS.[4]

Because assortative matching is measured at the district-by-marriage-cohort level rather than the individual level, I estimate separate specifications for the assortative mating analysis. Specifically, I estimate regressions of the form:

$$S^{\text{agg}}_{dm} = \beta\, \overline{\text{SchoolExposure}}_{dm} + \delta_d + \lambda_m + \varepsilon_{dm} \tag{3}$$

where S^{agg}_{dm} denotes the aggregate assortative mating index in district d and marriage cohort m, δ_d are district fixed effects, and λ_m are marriage-cohort fixed effects. The variable $\overline{\text{SchoolExposure}}_{dm}$ measures the average school construction exposure during ages 6–15 among women in district d and marriage cohort m. Regressions are weighted by the number of couples observed within each

[4] Additional details on the construction of the index are provided in Appendix A.2.

district–marriage-cohort cell, and standard errors are clustered at the district level.

One limitation of the empirical design is that the DHS does not observe childhood district directly. Women are therefore linked to school construction exposure using district of current residence. Migration during school-age years is relatively limited in India, and Census evidence suggests that most marriage migration occurs within relatively local geographic areas. Nevertheless, some measurement error in exposure remains possible. In addition, because the UDISE school census was collected in 2018, schools that shut down before the survey are not observed. To the extent that missing historical schools are unrelated to later marriage outcomes conditional on district and cohort fixed effects, this measurement error would tend to attenuate estimated effects toward zero.

6 Pre-trends analysis

A key identifying assumption in this paper is that, absent school construction, outcomes across districts would have evolved similarly over time. In standard difference-in-differences settings with discrete policy adoption, this assumption is often assessed using event-study specifications that test for flat pre-treatment trends. However, this approach is not well-suited to the present context. School construction in India expanded gradually over several decades rather than through a sharp, discrete policy change, and treatment intensity varies continuously across cohorts within districts. As a result, defining a single "event year" for each district is inherently imprecise, and event-study estimates may capture smooth development trends correlated with school expansion rather than causal effects.

To address this, I implement a cohort-based pre-trend test that aligns directly with the identification strategy. The empirical design exploits variation in cumulative school construction exposure across cohorts within districts. The relevant concern for identification is therefore whether districts that ultimately experience greater school expansion were already on different outcome trajectories prior to treatment. To test this, I construct a measure of eventual school exposure, defined as the average cumulative school construction exposure among later cohorts within a district. I then estimate regressions on early cohorts only, interacting indicators for 5-year birth cohort bins with this measure of eventual exposure, while including district and cohort-bin fixed effects. Formally, the test examines whether the evolution of outcomes across early cohorts differs systematically across districts with higher versus lower eventual exposure.

Under the identifying assumption, these interaction terms should be jointly indistinguishable from zero: prior to meaningful exposure, cohorts in high-expansion and low-expansion districts should exhibit similar trends. A rejection would indicate that districts that expand more are already on different trajectories, undermining the causal interpretation of the main estimates.

For women's educational attainment (Table 1), the interaction coefficients between cohort bins and eventual school exposure are small in magnitude and statistically insignificant. Relative to the 1965 cohort, the coefficients for the 1970 and 1975 cohorts are -0.147 and -0.244, with standard errors of 0.291 and 0.299, respectively. These estimates are economically small and imprecisely estimated, and there is no systematic pattern across cohorts. The joint test of the interaction terms yields an F-statistic of 0.65 with a p-value of 0.524, failing to reject the null hypothesis that the interactions are jointly zero. This indicates that districts that eventually experience higher school construction exposure do not exhibit differential pre-treatment trends in women's educational attainment.

A similar pattern emerges for age at marriage (Table 2). The interaction coefficients for the 1970 and 1975 cohorts are 0.192 and 0.191, with relatively large standard errors (0.346 and 0.357), and are statistically indistinguishable from zero. The joint test yields an F-statistic of 0.16 ($p = 0.856$), again failing to reject the null of no differential trends. This suggests that, prior to school expansion, marriage timing did not evolve differently in districts that would later receive more schools.

For the probability of remaining unmarried by age 18 (Table 3), the interaction coefficients are extremely small, 0.005 and 0.003 for the 1970 and 1975 cohorts, and precisely estimated around zero. The joint test produces an F-statistic of 0.04 with a p-value of 0.963, providing strong evidence against differential pre-trends. This further supports the conclusion that early cohort trends are similar across districts with different eventual exposure levels.

Overall, the evidence from these pre-trend tests supports the assumption that, in the absence of school construction, outcomes across districts would have followed similar trajectories, lending credibility to the causal interpretation of the estimated effects of school access on education and marriage outcomes.

7 Results

This section examines how school construction affected women's educational attainment, marriage-market outcomes, assortative matching, and intra-household dynamics. Tables 4 through 11 present estimates of the effects of school exposure on education, marriage timing, educational sorting, and domestic violence. I first show that school construction substantially increased women's educational attainment, particularly at lower levels of schooling. I then examine how these educational gains reshaped marriage markets, affecting marriage timing, spousal matching, and educational assortative mating. Finally, I examine whether these changes translated into improvements in women's intra-household outcomes, including decision-making power and domestic violence. Across specifications, Panel A reports estimates using the continuous measure of cumulative schools per 1,000 school-age children, while Panel B uses a binary indicator for above-average exposure. All regressions include district and cohort fixed effects, with standard errors clustered at the district level.

7.1 Educational Attainment

If increased local availability of schools through school construction reduces the distance and cost of attending school, constraints that may be particularly salient for girls, then educational expansion may lead to higher levels of completed educational attainment among women later in life. Changes in women's educational attainment may in turn alter the composition of local marriage markets and affect subsequent marriage outcomes and assortative matching. Understanding the effect of school construction on educational attainment is therefore important for interpreting the broader marriage-market effects of educational expansion.

Across specifications, increased school construction leads to meaningful improvements in women's education, particularly at lower levels of schooling. As shown in Table 4, Panel A, an additional school per 1,000 school-age children increases women's schooling by 0.123 years ($p < 0.01$). This corresponds to an elasticity of 0.164, implying that a 10 percent increase in school exposure raises educational attainment by approximately 1.6 percent. School construction also increases the probability of completing primary education by 1.9 percentage points ($p < 0.01$), with a larger elasticity of 0.263, indicating substantial gains at the extensive margin of basic schooling. In contrast, there is no detectable effect on secondary school completion. The estimated coefficient is close to zero and statistically insignificant, with a slightly negative elasticity (-0.015), suggesting that school expansion during this period primarily affected access to and completion of lower levels of education rather than progression to higher levels. Panel B shows consistent patterns using the binary exposure measure. Women exposed to above-average school construction attain 0.428 additional years of schooling ($p < 0.01$) and are 4.5 percentage points more likely to complete primary school ($p < 0.01$). The implied elasticities are smaller in magnitude due to the discrete nature of the treatment, but the direction and relative strength of effects mirror those in Panel A. As before, there is no statistically significant effect on secondary completion. These estimates are consistent with the broader descriptive expansion in women's schooling documented in Figure 4, though the regression results isolate variation in school exposure across districts and cohorts.

Taken together, these results indicate that school construction substantially expanded access to basic education for women, with the strongest impacts at the primary level. The absence of effects on secondary completion suggests that additional constraints—such as supply limitations at higher levels or demand-side factors—may continue to restrict progression beyond primary schooling.

7.2 Marriage Timing

Educational expansion may affect marriage timing through several channels. Increased access to schooling may delay marriage directly if women remain enrolled in school longer, reducing the likelihood of marriage during adolescence. At the same time, changes in educational attainment may alter reservation quality and matching opportunities within marriage markets, leading households to delay marriage in search of more desirable matches. Because marriage remains nearly universal in India, even modest shifts in marriage timing may have important implications for household formation and women's life trajectories.

Figure 2 shows that women with higher educational attainment consistently marry later and are substantially less likely to marry before age 18 across all cohorts. The gap between secondary-educated women and women with no schooling remains large throughout the period, suggesting that educational attainment is strongly associated with delayed marriage. Appendix

Figure B3 further shows that educational differences in marriage timing persist at later ages, particularly among women with secondary education, although marriage remains nearly universal by age 25 for lower education groups.

Table 5 examines the effects of school construction on women's age at marriage, the spousal age gap, and the probability of remaining unmarried by age 18. The results indicate modest but statistically significant effects across all outcomes. Panel A shows that an additional school per 1,000 school-age children increases the age at marriage by 0.036 years relative to a mean of 19.05 years. This corresponds to an elasticity of 0.016. At the same time, school construction reduces the age gap between husbands and wives by 0.026 years, implying an elasticity of −0.048 relative to the mean age gap of 4.84 years.[5] Finally, the probability of remaining unmarried by age 18 increases by 0.3 percentage points, corresponding to an elasticity of 0.046. These findings indicate that improved access to schooling delayed marriage and modestly narrowed spousal age differences. Panel B presents results using the binary treatment. Moving from below-average to above-average school construction increases the age at marriage by 0.303 years, reduces the spousal age gap by 0.117 years, and raises the probability of remaining unmarried by age 18 by 2.4 percentage points. The corresponding elasticities are 0.005, −0.008, and 0.013, respectively. Appendix Table B2 shows that the estimated effects on remaining unmarried attenuate substantially by ages 21 and 25, suggesting that educational expansion primarily delayed the timing of marriage rather than generating persistent effects at later ages.

7.3 Educational Homogamy & Assortative Matching

The central question of this paper is whether educational expansion reshaped equilibrium patterns of marital sorting. While the previous section shows that school construction delayed marriage and increased women's educational attainment, these changes may also alter who marries whom within local marriage markets. I therefore next examine whether educational expansion strengthened educational homogamy and assortative mating beyond what would be expected from mechanical

[5] Husband's age is constructed using information from both the women's survey and the household roster of the India DHS. I use the husband's age reported in the women's survey when available and supplement missing values using the household roster for co-residing spouses. Household roster information is unavailable for husbands not residing in the household at the time of interview. For observations where husband's age is reported in both sources, the measures are nearly perfectly correlated.

changes in the distribution of education alone.

Table 6 presents reduced-form evidence on educational homogamy by examining whether women exposed to greater school construction during childhood marry more educated husbands.[6] Across specifications, school construction exposure is associated with higher levels of husband educational attainment. In the continuous specification, an additional school per 1,000 school-age children increases husbands' years of schooling by 0.081 years ($p < 0.01$), raises the probability of primary completion by 0.5 percentage points ($p < 0.01$), and increases the probability of secondary completion by 0.4 percentage points ($p < 0.05$). Relative to sample means, these correspond to elasticities of approximately 0.09, 0.06, and 0.09 respectively. The binary specification yields similar patterns. Exposure to above-average school construction increases husbands' years of schooling by 0.268 years ($p < 0.01$) and raises the probability of secondary school completion by 2.4 percentage points ($p < 0.01$), while effects on primary completion remain small and statistically insignificant. Taken together, these results suggest that educational expansion altered the composition of local marriage markets and increased matching among more educated spouses.[7]

These regressions provide suggestive evidence of increased educational homogamy, but they do not by themselves distinguish between stronger assortative matching and mechanical changes in the marginal distribution of education. In particular, if school construction increases educational attainment for both men and women within local marriage markets, then observed increases in spousal education may partly reflect compositional changes rather than stronger sorting preferences. In addition, husbands' and wives' exposure to school construction may be correlated because couples often reside in the same district and age gaps between spouses have narrowed over time, implying that men and women in more recent cohorts may have experienced educational expansion simultaneously. As a result, the estimates in Table 6 should not be interpreted as causal effects of women's education on husbands' schooling. Instead, they capture equilibrium outcomes in

[6] Husband's education is constructed using the household roster and linked to women through the spouse line number reported in the women's questionnaire. As a result, husband characteristics are unavailable for spouses not residing in the household at the time of interview. In the analysis sample, approximately 92% of currently married women report co-residing with their husbands, suggesting that non-co-residence affects a relatively small share of observations. For observations where husband's education is available both in the household roster and in the women's survey, the two measures are highly correlated.

[7] Descriptively, marriages in which wives are more educated become more common among younger cohorts, while hypergamous marriages decline (Appendix Figure B4). However, I find no evidence that school construction causally affects hypergamy or hypogamy patterns (Appendix Table B3).

local marriage markets undergoing educational expansion. To account explicitly for changes in the marginal distribution of education, I therefore turn to the assortative mating index, which measures sorting relative to random matching.[8]

Table 7 shows the assortative mating index and average school construction exposure for 5-year binned marriage cohorts. Figure 5 plots assortative mating separately by educational category across marriage cohorts. The figure reveals substantial increases in educational sorting at the lower end of the education distribution. Individuals with no schooling increasingly marry others with no schooling relative to what would be expected under random matching given the changing marginal distribution of education. The assortative mating index for the no-schooling group rises steadily across cohorts, increasing from roughly 1.4 in the earliest marriage cohorts to more than 7 in the most recent cohorts. This pattern is structurally similar to the evolution of assortative mating documented by Eika, Mogstad and Zafar (2019) for the United States and several European countries, though the increase in assortative mating among the least educated is substantially larger in the Indian context. Individuals with primary education also exhibit positive assortative matching, though the degree of stratification is less pronounced than among individuals with no schooling. By contrast, assortative mating among individuals with upper primary education remains relatively stable across cohorts, while assortative matching among those with secondary education declines modestly over time. As educational attainment expands and secondary schooling becomes increasingly common, high school education may be becoming less distinctive within the marriage market, leading to weaker educational homogamy at higher education levels. Overall, these results suggest that educational expansion increased segmentation at the lower end of the education distribution even as average educational attainment rose substantially.

I next estimate the causal effect of school construction on the educational assortative mating index. Table 8 reports two-way fixed-effects estimates of the effect of district-level school construction on the assortative mating index by marriage cohort. The coefficient on school exposure is positive and statistically significant at the 1 percent level. Quantitatively, an additional school per 1,000 school-age children increases the assortative mating index by 0.033, corresponding to an increase of approximately 1.6 percent relative to the mean index value of 2.024. These estimates indicate that educational expansion modestly but significantly strengthened educational sorting

[8]Construction of the index is described in Appendix A.2.

within marriage markets. At the same time, the baseline level of the index indicates that India's marriage market is already strongly positively assortative even in the absence of additional school construction, with same-education matches occurring roughly twice as frequently as under random matching.

Figure 6 compares the observed rise in assortative mating across marriage cohorts with the increase predicted by school construction exposure using the estimated treatment effects. The predicted series tracks a substantial share of the upward trend in assortative mating, though observed assortative mating rises more sharply during later cohorts. This suggests that educational expansion played an important role in strengthening educational sorting, while additional social and economic changes beyond school construction also contributed to the increase in assortative mating over time.

To quantify the contribution of school construction to the observed rise in assortative mating, I compare changes in the assortative mating index across marriage cohorts with the changes predicted by the estimated treatment effects. Between the early marriage cohorts (1980–1985) and the late cohorts (2010–2015), the assortative mating index increases from approximately 1.65 to 2.11, corresponding to a rise of roughly 0.46. Over the same period, average school construction exposure increases from approximately 5.09 to 11.05 schools per 1,000 children, a change of nearly 5.96. Using the estimated coefficient from Table 8, this implies a predicted increase in the assortative mating index of approximately 0.19. Back-of-the-envelope calculations therefore suggest that school construction can account for roughly 43 percent of the observed increase in assortative mating across marriage cohorts. Overall, these findings indicate that educational expansion reshaped not only individual educational attainment, but also the structure of marital matching within local marriage markets.

7.4 Decision-making & Domestic Violence

An expansion in access to schooling can plausibly affect intra-household outcomes such as women's decision-making power and the incidence of domestic violence through several channels. On the one hand, increased education may enhance women's bargaining power within marriage by improving their outside options, increasing their productivity, and shifting preferences toward greater autonomy. This would predict improvements in decision-making and reductions in domestic violence. On the other hand, if labor market opportunities remain limited—as is the case in many parts of India—education may not translate into meaningful outside options. In such settings, increases in women's education may instead alter expectations within marriage without changing underlying gender norms, potentially leading to conflict or "backlash." Finally, domestic violence may be driven by deeply entrenched norms and intergenerational transmission, in which case improvements in schooling alone may have limited effects. The empirical analysis therefore examines both average effects and potential mechanisms through which schooling could influence domestic violence outcomes.

Table 9 examines whether exposure to school construction affects women's decision-making power and the incidence of domestic violence.[9] The sample includes only women selected for the domestic violence module in the DHS data, and regressions use module weights accordingly. The results provide little evidence that school construction improved women's intra-household outcomes. Across specifications, the coefficients on school construction are small in magnitude for decision-making and most measures of domestic violence. There is weak evidence of an increase in controlling behavior and emotional abuse under the continuous specification: an additional school per 1,000 children increases controlling behavior by 0.005 ($p < 0.10$) and emotional abuse by 0.004 ($p < 0.05$). However, these effects are not robust to the binary specification, where estimates are close to zero and statistically insignificant. There is no evidence of meaningful effects on physical or sexual violence. Overall, the estimates are small and imprecisely estimated, and do not suggest economically meaningful improvements in domestic violence or decision-making. These findings suggest that, despite increasing educational attainment, school construction did not translate into substantial changes in intra-household power or relationship quality.

Table 10 examines whether domestic violence is transmitted across generations and whether increased schooling attenuates this transmission. Panel A estimates the baseline relationship between exposure to domestic violence in the parental household (measured by whether the respondent's father beat her mother) and her own experience of domestic violence in adulthood. Panel B augments the baseline specification by interacting parental exposure with school construction. Panel A reveals strong and highly significant intergenerational transmission of

[9] Additional details on the construction of domestic violence and decision-making measures are provided in Appendix A.3

domestic violence. Women whose fathers beat their mothers are substantially more likely to experience emotional, physical, and sexual violence themselves. The magnitudes are large: emotional violence increases by 13.4 percentage points, physical violence by 26.8 percentage points, and sexual violence by 6.8 percentage points relative to mean outcomes of 0.12, 0.26, and 0.05, respectively. Panel B shows that increased access to schooling does not attenuate this intergenerational persistence. The interaction between school construction and parental exposure is close to zero and statistically insignificant for emotional and physical violence, and small and positive for sexual violence (0.002, $p < 0.05$). These results indicate that, while schooling expanded educational attainment and altered marriage outcomes, it did not weaken the transmission of domestic violence across generations. This points to the persistence of deeply rooted norms that are not easily shifted by improvements in schooling alone.

Table 11 tests whether increases in women's relative education generate "backlash" within marriage. Panel A estimates the relationship between relative spousal education and domestic violence, comparing couples in which the wife has more education than her husband or vice versa, with equal-education couples as the omitted category. Relative spousal education is constructed using husband education linked from the household roster through the spouse line number reported in the women's questionnaire, so the sample is restricted to women in the domestic violence module with successfully linked co-resident spouses. Panel B interacts relative education with school construction exposure to assess whether schooling-induced changes in women's relative education affect domestic violence outcomes. Panel A provides little evidence that women who are more educated than their husbands experience higher levels of domestic violence. The coefficients on the "wife more educated" indicator are small in magnitude and statistically insignificant across emotional, physical, and sexual violence outcomes. Women whose husbands are more educated likewise do not experience systematically higher levels of violence. If anything, the estimates suggest modest reductions in violence relative to equal-education couples, though only the coefficient for sexual violence is statistically significant (-0.012, $p < 0.05$). Panel B similarly provides little support for backlash effects arising from educational expansion. The interaction between school exposure and the "wife more educated" indicator is small and negative across specifications and statistically significant for physical and sexual violence. Rather than increasing violence, greater school exposure is associated with modest declines in these outcomes among couples in which wives are more educated than their husbands. Overall, the results provide little evidence that increases in women's relative education induced by school construction generated backlash within marriage.

Taken together, the results in this section suggest that educational expansion substantially altered educational attainment and marriage-market outcomes while leaving intra-household gender dynamics largely unchanged. School construction does not meaningfully improve women's decision-making power or reduce domestic violence, and domestic violence exhibits strong intergenerational persistence that is not attenuated by increased schooling. At the same time, there is no evidence that increases in women's relative education generate backlash effects within marriage. These findings suggest that educational expansion reshaped who marries whom and when households form, while deeply entrenched norms governing intra-household dynamics remained persistent.

8 Conclusion

To summarize, I find that school construction increases women's educational attainment and generates systematic changes in marriage timing and matching patterns—women marry later, are less likely to enter child marriage, and match with husbands closer to their own age. These shifts are consistent with education raising women's value in the marriage market, thereby improving their outside options and allowing families to delay marriage in search of higher-quality matches. Taken together, these findings imply that increased access to schooling generates meaningful marital returns to education in equilibrium, even in settings where labor market returns to female education may be limited.

At the same time, the results show that these gains in the marriage market do not translate into corresponding improvements in intra-household outcomes. I find little evidence that school construction increases women's decision-making power or reduces domestic violence. Domestic violence remains strongly persistent across generations, and increased schooling does not attenuate this transmission. Moreover, there is no evidence that schooling-induced changes in women's relative education generate backlash within marriage. These findings suggest that while education reshapes who marries whom and when, it does not substantially alter the underlying norms governing intra-household dynamics.

The observed changes in assortative mating further indicate that schooling expansions reshape the equilibrium structure of marriage markets by altering the distribution of educated women. Educational sorting strengthens at the bottom of the distribution—where access remains limited—while weakening at higher levels where schooling becomes more widespread. This pattern points to a potential unintended consequence of educational expansion: increased stratification among the least educated, even as average attainment rises.

Overall, the results highlight a divergence between returns to education in the marriage market and changes in intra-household bargaining power. Schooling expansions improve matching outcomes and delay marriage, but do not, on their own, shift deeply entrenched gender norms or reduce domestic violence. In future work, I plan to examine whether these changes in marriage market structure translate into intergenerational persistence in human capital, particularly through children's educational outcomes.

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9 Figures and Tables

Figure 1: Average years of schooling among married couples by birth cohort
Notes: The figure plots average years of schooling for women and their co-resident husbands by women's birth cohort using DHS data. Women's educational attainment rises substantially across cohorts, narrowing the gender gap in schooling over time.

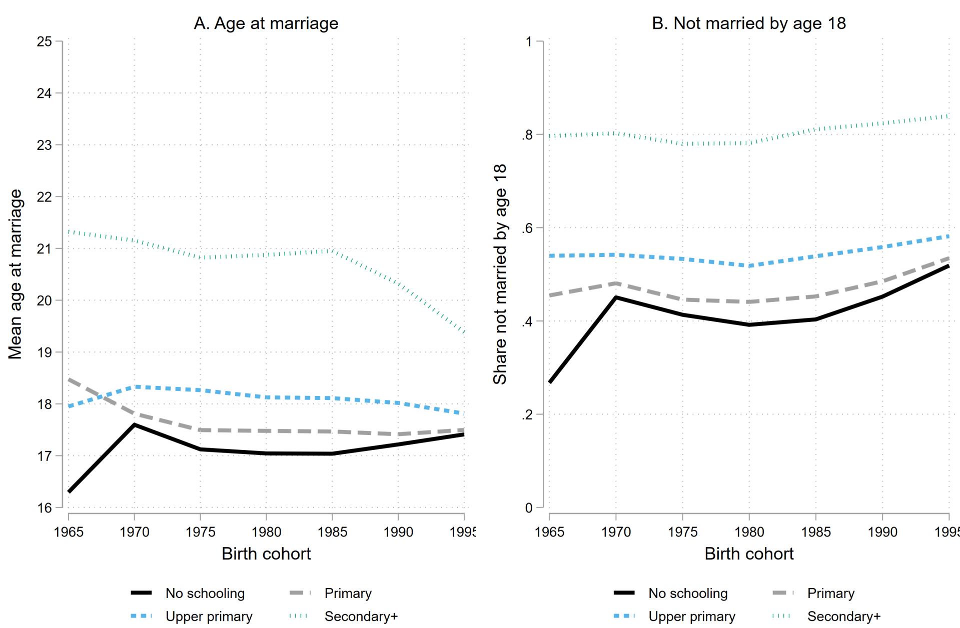

Figure 2: Marriage timing by education level
Notes: Panel A plots mean age at marriage by women's birth cohort and education category. Panel B plots the share of women not married by age 18. Educational categories are defined as no schooling, primary (1–5 years), upper primary (6–8 years), and secondary or higher (9+ years).

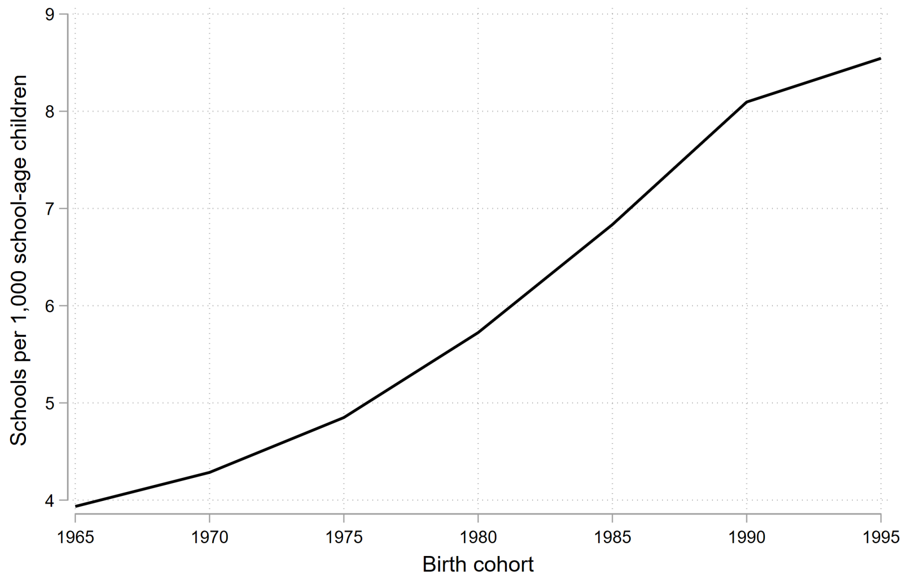

Figure 3: School exposure by birth cohort

Notes: The figure plots average cumulative school exposure during ages 6–15 by women's birth cohort. School exposure is measured as the cumulative number of schools constructed per 1,000 school-age children in a woman's district of residence during her school-age years.

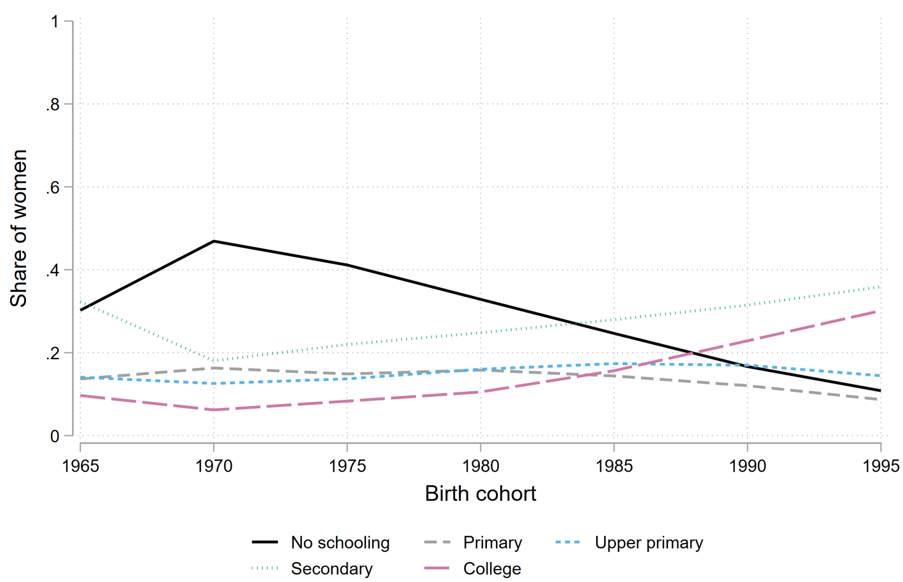

Figure 4: Distribution of women's education by birth cohort

Notes: The figure plots the share of women in each education category by five-year birth cohort. Education categories are defined as no schooling, primary, upper primary, secondary, and college.

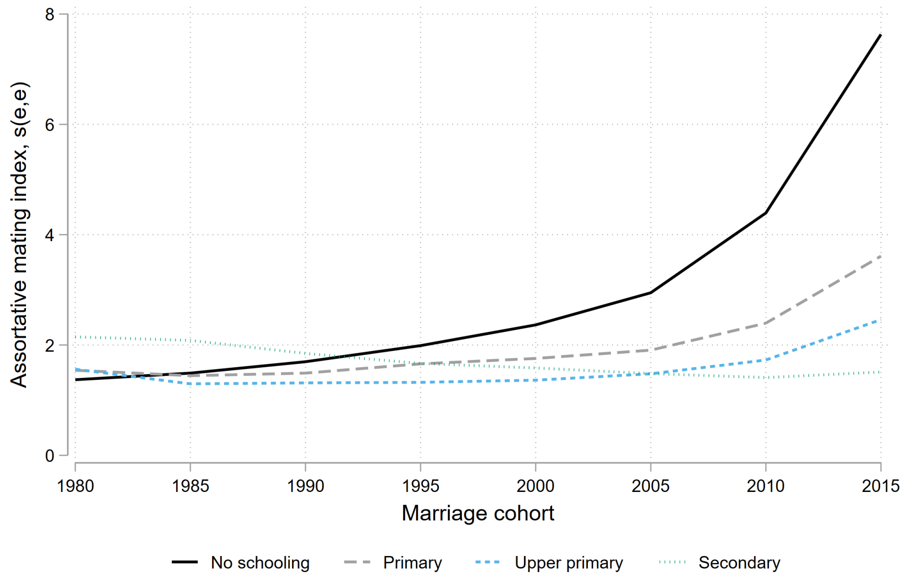

Figure 5: Educational assortative mating by marriage cohort

Notes: The figure plots the assortative mating index $s(e,e)$ for each education group, defined as the ratio of the observed share of same-education couples to the share expected under random matching. Cohorts are grouped into five-year marriage cohorts. The secondary category includes women with secondary or higher education. College is excluded due to small cell sizes.

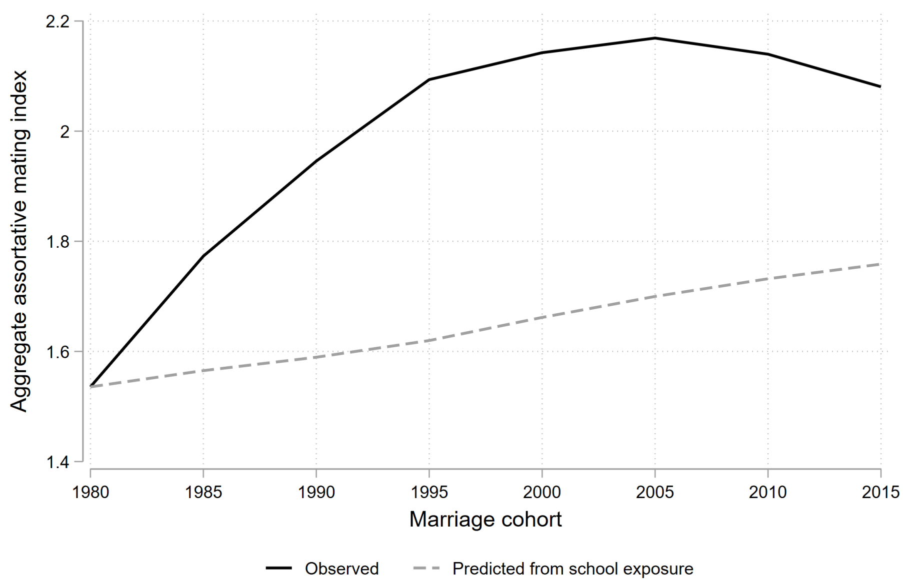

Figure 6: Observed and predicted assortative mating

Notes: The figure compares the observed aggregate assortative mating index across marriage cohorts with the level predicted by changes in school exposure using the estimated regression coefficients. The predicted series is constructed by applying the estimated effect of school exposure on assortative mating to changes in average cohort-level school exposure over time.

Table 1: Pre-Trend Test for Women's Years of Education

	(1) Years of education
5-year birth cohort=1965 × Future school exposure	0.000
	(.)
5-year birth cohort=1970 × Future school exposure	-0.147
	(0.291)
5-year birth cohort=1975 × Future school exposure	-0.244
	(0.299)
Mean of dependent variable	4.227
N	80,409

Notes: The table reports cohort-specific pre-trend tests using early cohorts only. The treatment variable is district-level eventual school construction exposure, measured as average cumulative school exposure among later cohorts and standardized to have mean zero and standard deviation one. The regression includes district and cohort-bin fixed effects. Standard errors are clustered at the district level. A joint test fails to reject that the cohort-bin interactions are zero if the p-value is large. Joint test: F = 0.65, p = 0.524.

Table 2: Pre-Trend Test for Age at Marriage

	(1) Age at marriage
5-year birth cohort=1965 × Future school exposure	0.000
	(.)
5-year birth cohort=1970 × Future school exposure	0.192
	(0.346)
5-year birth cohort=1975 × Future school exposure	0.191
	(0.357)
Mean of dependent variable	19.003
N	79,505

Notes: Sample restricted to ever-married women. The table reports cohort-specific pre-trend tests using early cohorts only. The treatment variable is district-level eventual school construction exposure, measured as average cumulative school exposure among later cohorts and standardized to have mean zero and standard deviation one. The regression includes district and cohort-bin fixed effects. Standard errors are clustered at the district level. Joint test: F = 0.16, p = 0.856.

Table 3: Pre-Trend Test for Child Marriage

	(1) Not married by 18
5-year birth cohort=1965 × Future school exposure	0.000
	(.)
5-year birth cohort=1970 × Future school exposure	0.005
	(0.026)
5-year birth cohort=1975 × Future school exposure	0.003
	(0.027)
Mean of dependent variable	0.593
N	80,409

Notes: The table reports cohort-specific pre-trend tests using early cohorts only. The treatment variable is district-level eventual school construction exposure, measured as average cumulative school exposure among later cohorts and standardized to have mean zero and standard deviation one. The regression includes district and cohort-bin fixed effects. Standard errors are clustered at the district level. Joint test: F = 0.04, p = 0.963.

Table 4: Effect of School Construction on Women's Education Outcomes

	(1) Years of education	(2) Primary completed	(3) Secondary completed
Panel A: Continuous treatment			
Schools built	0.123***	0.019***	-0.001
	(0.021)	(0.002)	(0.002)
Mean of treatment	8.904	8.904	8.904
Elasticity of outcome	0.164	0.263	-0.015
Panel B: Binary treatment			
Schools built > mean	0.428***	0.045***	0.005
	(0.110)	(0.008)	(0.011)
Mean of treatment	0.342	0.342	0.342
Elasticity of outcome	0.022	0.024	0.005
Mean of outcome	6.704	0.642	0.338
N	462,963	462,963	462,963

Notes: Each column reports estimates from regressions of the dependent variable on school construction exposure, with district and cohort fixed effects. Panel A uses cumulative schools constructed per 1,000 population aged 6–15 years. Panel B uses an indicator for whether exposure is above the cohort-specific mean. Mean of treatment reports the estimation-sample mean of the treatment variable used in that panel. Elasticities are computed as $\beta \times \bar{X}/\bar{Y}$. Standard errors (in parentheses) are clustered at the district level.

Table 5: Effect of School Construction on Women's Marriage Outcomes

	(1) Age at marriage	(2) Age gap	(3) Not married by age 18
Panel A: Continuous treatment			
Schools built	0.036**	-0.026***	0.003**
	(0.015)	(0.009)	(0.002)
Mean of treatment	8.625	8.748	8.904
Elasticity of outcome	0.016	-0.048	0.046
Panel B: Binary treatment			
Schools built > mean	0.303***	-0.117***	0.024**
	(0.082)	(0.043)	(0.010)
Mean of treatment	0.333	0.343	0.342
Elasticity of outcome	0.005	-0.008	0.013
Mean of outcome	19.052	4.842	0.642
N	432,094	368,201	462,963

Notes: Each column reports estimates from regressions of the dependent variable on school construction exposure, with district and cohort fixed effects. In columns (1) and (2), the sample is restricted to ever-married women. Column (3) includes all women. Panel A uses cumulative schools constructed per 1,000 population aged 6–15 years. Panel B uses an indicator for whether exposure is above the cohort-specific mean. Mean of treatment reports the estimation-sample mean of the treatment variable used in that panel. Elasticities are computed as $\beta \times \bar{X}/\bar{Y}$. Standard errors (in parentheses) are clustered at the district level.

Table 6: Effect of School Construction in Wife's District on Husband's Education

	(1) Years of education	(2) Primary completed	(3) Secondary completed
Panel A: Continuous treatment			
Schools built	0.081***	0.005***	0.004**
	(0.017)	(0.001)	(0.002)
Mean of treatment	8.759	8.759	8.759
Elasticity of outcome	0.089	0.058	0.089
Panel B: Binary treatment			
Schools built > mean	0.268***	0.003	0.024***
	(0.087)	(0.007)	(0.008)
Mean of treatment	0.345	0.345	0.345
Elasticity of outcome	0.012	0.002	0.020
Mean of outcome	7.953	0.765	0.411
N	360,548	360,548	360,548

Notes: Each column reports estimates from regressions of the dependent variable on school construction exposure in the woman's district when young, with district and cohort fixed effects. Panel A uses cumulative schools constructed per 1,000 population aged 6–15 years. Panel B uses an indicator for whether exposure is above the cohort-specific mean. Mean of treatment reports the estimation-sample mean of the treatment variable used in that panel. Elasticities are computed as $\beta \times \bar{X}/\bar{Y}$. Standard errors (in parentheses) are clustered at the district level.

Table 7: Trends in Assortative Mating and School Construction Exposure over Marriage Cohorts

Binned marriage cohorts	Assortative mating index	Mean school construction exposure
1980	1.536	4.642
1985	1.773	5.544
1990	1.946	6.287
1995	2.094	7.213
2000	2.143	8.498
2005	2.169	9.663
2010	2.140	10.643
2015	2.081	11.456

Notes: Table reports the aggregate assortative mating index S^{agg} and mean school construction exposure for each 5-year marriage cohort.

Table 8: Effect of School Construction on Educational Assortative Mating Index

	Assortative mating index
Schools built	0.033***
	(0.007)
Mean of outcome	2.024
N	5200

Notes: District-by-marriage-cohort regression with district and cohort fixed effects. Cells weighted by number of couples. Mean of cumulative school construction exposure in estimation sample: 7.686.

Table 9: Effect of School Construction on Autonomy and Domestic Violence

	(1) Decision making	(2) Controlling partner	(3) Emotional abuse	(4) Physical violence	(5) Sexual violence
Panel A: Continuous treatment					
Schools built	0.002	0.005*	0.004*	0.001	-0.001
	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)
Mean of treatment	9.071	9.071	9.071	9.071	9.071
Elasticity of outcome	0.024	0.115	0.265	0.045	-0.114
Panel B: Binary treatment					
Schools built > mean	0.007	-0.010	0.001	-0.001	0.005
	(0.016)	(0.021)	(0.012)	(0.017)	(0.007)
Mean of treatment	0.348	0.348	0.348	0.348	0.348
Elasticity of outcome	0.004	-0.008	0.002	-0.002	0.035
Mean of outcome	0.731	0.425	0.120	0.261	0.053
N	54,332	54,332	54,332	54,332	54,332

Notes: Each column reports estimates from regressions of the indicated outcome on school construction exposure, with district and cohort fixed effects. Panel A uses cumulative schools constructed per 1,000 population aged 6–15; Panel B uses an indicator for whether exposure is above the cohort-specific mean. Outcomes are measured for women in the DHS domestic violence module using DV weights. Decision-making is the average of indicators for participation in key household decisions; other outcomes are binary indicators of controlling behavior, emotional abuse, physical violence, and sexual violence. Mean of treatment reports the estimation-sample mean of the treatment variable used in that panel. Elasticities are computed as $\beta \times \bar{X}/\bar{Y}$. Standard errors (in parentheses) are clustered at the district level.

Table 10: Effect of School Construction on Intergenerational Transmission of Domestic Violence

	(1) Emotional	(2) Physical	(3) Sexual
Panel A: Intergenerational transmission			
Father beat mother	0.134***	0.268***	0.068***
	(0.015)	(0.014)	(0.010)
Panel B: Intergenerational transmission × school exposure			
Schools built	0.003*	0.000	-0.001
	(0.002)	(0.002)	(0.001)
Father beat mother	0.133***	0.259***	0.056***
	(0.022)	(0.023)	(0.015)
Schools × Father beat mother	0.000	0.002	0.002*
	(0.002)	(0.002)	(0.001)
Mean of outcome	0.120	0.261	0.053
N	54,332	54,332	54,332

Notes: Each column reports estimates from regressions of the indicated domestic violence outcome on indicators for exposure to domestic violence in the parental household and school construction exposure, with district and birth-cohort fixed effects. Panel A estimates the baseline relationship between a respondent's exposure to parental domestic violence (measured by an indicator for whether her father ever beat her mother) and her own experience of domestic violence in adulthood. Panel B interacts this indicator with school construction exposure in the woman's district during her school-age years. Domestic violence outcomes are binary indicators for whether the respondent has ever experienced emotional, physical, or sexual violence by her husband. The sample is restricted to women selected into the DHS domestic violence module; regressions use domestic violence weights. Standard errors (in parentheses) are clustered at the district level.

Table 11: Testing for Backlash: Relative Spousal Education, School Construction, and Domestic Violence

	(1) Emotional	(2) Physical	(3) Sexual
Panel A: Relative education			
Wife more educated	-0.010	-0.009	-0.005
	(0.008)	(0.011)	(0.006)
Husband more educated	-0.011	-0.009	-0.012**
	(0.007)	(0.010)	(0.005)
Panel B: Relative education × school exposure			
Schools built	0.003	0.001	0.000
	(0.002)	(0.002)	(0.001)
Wife more educated	-0.007	0.005	0.003
	(0.012)	(0.014)	(0.008)
Schools × Wife more	-0.001	-0.002**	-0.001**
	(0.001)	(0.001)	(0.001)
Mean of outcome	0.119	0.269	0.052
N	43,320	43,320	43,320

Notes: Each column reports estimates from regressions of the indicated domestic violence outcome on measures of relative spousal education, with district and birth-cohort fixed effects. Panel A includes indicators for whether the wife has more years of education than her husband and whether the husband has more education than the wife, with couples in which both spouses have equal education serving as the omitted category. Panel B interacts the "Wife more educated" indicator with school construction exposure in the woman's district during her school-age years. Domestic violence outcomes are binary indicators for whether the respondent has ever experienced emotional, physical, or sexual violence by her husband. The sample is restricted to women selected into the DHS domestic violence module with non-missing information on both spouses' education; regressions use domestic violence weights. Standard errors (in parentheses) are clustered at the district level.

Appendix A Data and Variable Construction

Appendix A.1 District Panel Construction

Few studies in India undertake a long time series analysis at the district level due to changes in district boundaries over time. Some studies use only districts with unchanged boundaries, which limits the sample, whereas other studies assume that district splits are subsets of the original parent district. However, in reality, new district boundaries can cut across multiple parent districts. This complicates the construction of a longer district-level panel. One contribution of this study is the construction of a consistent district panel over the period 1961–2011. I use the 640 districts in the 2011 Census, which also gives me a larger district-level sample than studies that only use unchanged districts.

If district boundary changes are not carefully accounted for, population data may contain large measurement error. To address this issue, I use harmonized population counts across districts drawn from the "Decadal Variation in Population" tables provided by the Census of India, which account for district boundary changes over time. I then interpolate these population estimates across intercensal years.

For outcome data, I use the 2019 Demographic and Health Survey (DHS). I map the shapefile for the 2019 DHS cluster geocodes to 2011 Census district administrative boundaries (available via SHRUG) using QGIS. My treatment variable comes from the UDISE school database. I scraped geocodes for all 1.5 million schools and mapped them to DHS clusters using QGIS. I then map DHS households to DHS clusters, which are now linked to Census district boundaries.

Appendix A.2 Construction of the Assortative Mating Index

This appendix describes the construction of the educational assortative mating index used in the main analysis. The measure follows Eika, Mogstad and Zafar (2019) and captures the extent to which individuals with a given education level marry each other more frequently than would occur under random matching, conditional on the marginal distributions of education.

The analysis uses data on currently married couples from the DHS Individual Recode files. For each couple, I observe the wife's and husband's educational attainment, district of residence, year of cohabitation, and DHS sampling weights. Educational attainment is grouped into five categories: no schooling, primary (1–5 years), upper primary (6–8 years), secondary (9–12 years), and college education (more than 12 years). College-educated individuals are excluded from some disaggregated trend figures because small cell sizes generate volatile series in earlier marriage cohorts. Marriage cohorts are grouped into five-year bins.

For each district d and marriage cohort t, I first compute the weighted joint distribution of spouses' educational attainment:

$$P_{dt}(e_f, e_m) = \frac{\sum_{i\in(d,t)} w_i \cdot \mathbb{1}\{E_i^f = e_f,\ E_i^m = e_m\}}{\sum_{i\in(d,t)} w_i},$$

where E_i^f and E_i^m denote the education categories of the wife and husband in couple i, and w_i denotes the DHS sampling weight. From the joint distribution, I recover the marginal distributions of women's and men's education within each district–cohort cell:

$$P_{dt}^f(e_f) = \sum_{e_m} P_{dt}(e_f, e_m), \qquad P_{dt}^m(e_m) = \sum_{e_f} P_{dt}(e_f, e_m).$$

The assortative mating index for education pair (e_f, e_m) is then defined as:

$$s_{dt}(e_f, e_m) = \frac{P_{dt}(e_f, e_m)}{P_{dt}^f(e_f) \cdot P_{dt}^m(e_m)}.$$

Under random matching with respect to education, the joint probability equals the product of the marginals and the index equals one. Values greater than one indicate positive assortative matching, meaning that individuals with those education levels marry each other more frequently than would occur under random matching.

The main analysis focuses on same-education matches, corresponding to the diagonal elements $s_{dt}(e, e)$. I additionally construct an aggregate assortative mating index for each district and marriage cohort as a weighted average of the diagonal elements, where weights are proportional to the observed share of homogamous matches in each education category:

$$S_{dt}^{\text{agg}} = \sum_{e} \omega_{dt,e}\, s_{dt}(e, e), \qquad \omega_{dt,e} = \frac{P_{dt}(e, e)}{\sum_k P_{dt}(k, k)}.$$

This aggregate measure summarizes the overall intensity of educational assortative matching within district d and cohort t, net of changes in the marginal distributions of education across cohorts.

Appendix A.3 Construction of Domestic Violence Outcomes

Outcome	Construction
Decision-making	Indicator equal to the average of four binary measures capturing whether the respondent has any say in major household decisions. For each domain, an indicator is coded as 1 if the decision is made by the respondent alone or jointly with her partner, and 0 otherwise: (1) respondent's healthcare; (2) large household purchases; (3) visits to relatives or family; (4) decisions over husband's earnings.
Controlling partner	Binary indicator equal to 1 if the respondent reports at least one controlling behavior by her husband, and 0 otherwise: (1) husband/partner is jealous if respondent talks with other men; (2) accuses respondent of unfaithfulness; (3) does not permit respondent to meet female friends; (4) tries to limit respondent's contact with family; (5) insists on knowing where respondent is at all times; (6) does not trust respondent with money.
Emotional abuse	Binary indicator equal to 1 if the respondent has ever experienced emotional violence by her husband, and 0 otherwise: (1) ever been humiliated; (2) threatened with harm; (3) insulted or made to feel bad.
Physical violence	Binary indicator equal to 1 if the respondent has ever experienced physical violence by her husband, and 0 otherwise: (1) moderate physical violence (pushed, shook, had something thrown, slapped, punched, had arm twisted, or hair pulled); (2) severe physical violence (kicked or dragged, strangled or burnt, threatened with a weapon).
Sexual violence	Binary indicator equal to 1 if the respondent has ever experienced sexual violence by her husband, and 0 otherwise: (1) ever physically forced into unwanted sex; (2) forced into other unwanted sexual acts; (3) physically forced to perform sexual acts she did not want to perform.

Notes: All domestic violence variables are measured for women selected into the DHS domestic violence module. The decision-making outcome is coded so that higher values indicate greater participation in household decision-making; the domestic violence outcomes are coded so that higher values indicate worse relationship quality.

Appendix B For Online Publication

Appendix B.1 Additional Figures

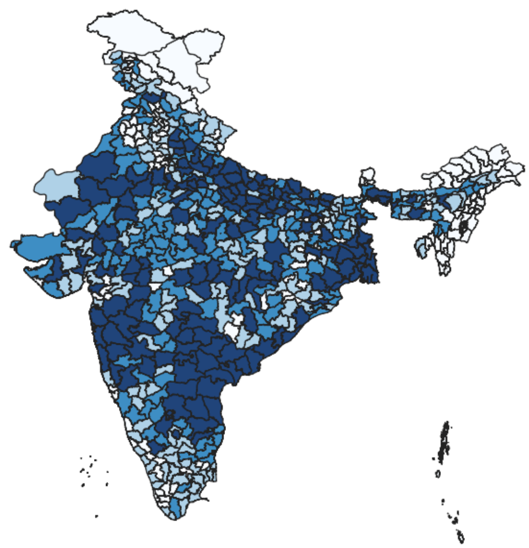

Figure B1: Spatial distribution of schools across districts

Notes: The figure maps the number of schools observed in the UDISE 2018 school census by district. Districts are grouped into quartiles of the distribution, with darker shading indicating a larger number of schools. Because UDISE records schools operating at the time of the 2018 survey, the map reflects the spatial distribution of observed schools rather than historical cohort-specific school exposure.

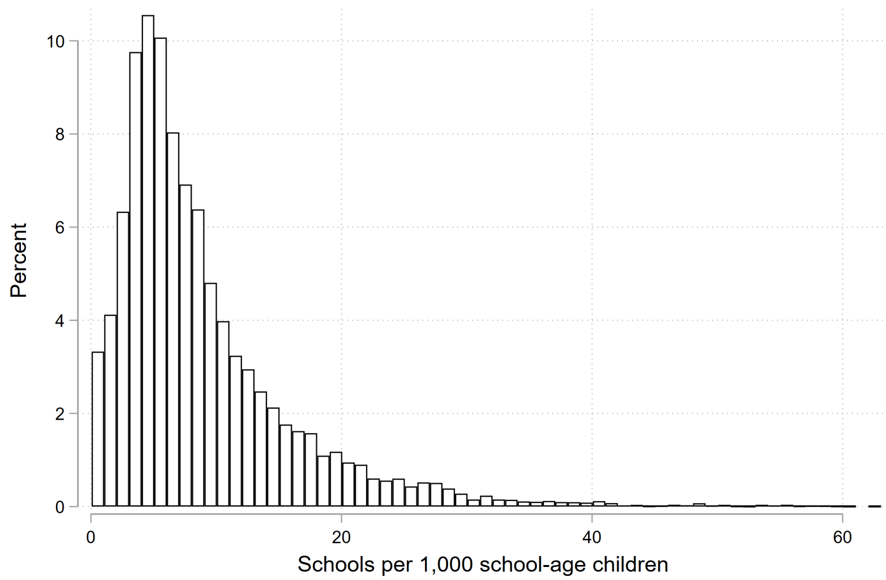

Figure B2: Distribution of school construction exposure
Notes: The figure plots the distribution of cumulative school construction exposure during ages 6–15, measured as schools per 1,000 school-age children in the respondent's district.

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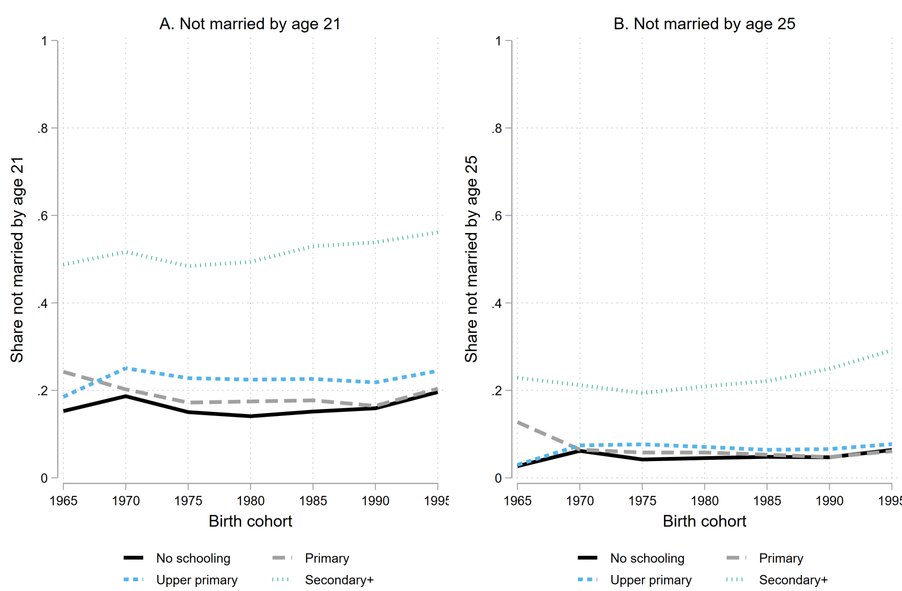

Figure B3: Marriage timing by education level
Notes: The figure plots the share of women not yet married by ages 21 and 25 across 5-year birth cohorts separately by educational attainment category. Educational categories are defined as no schooling, primary (1–5 years), upper primary (6–8 years), and secondary or higher (9+ years). Women younger than the relevant age threshold at the time of interview are excluded from the corresponding calculation.

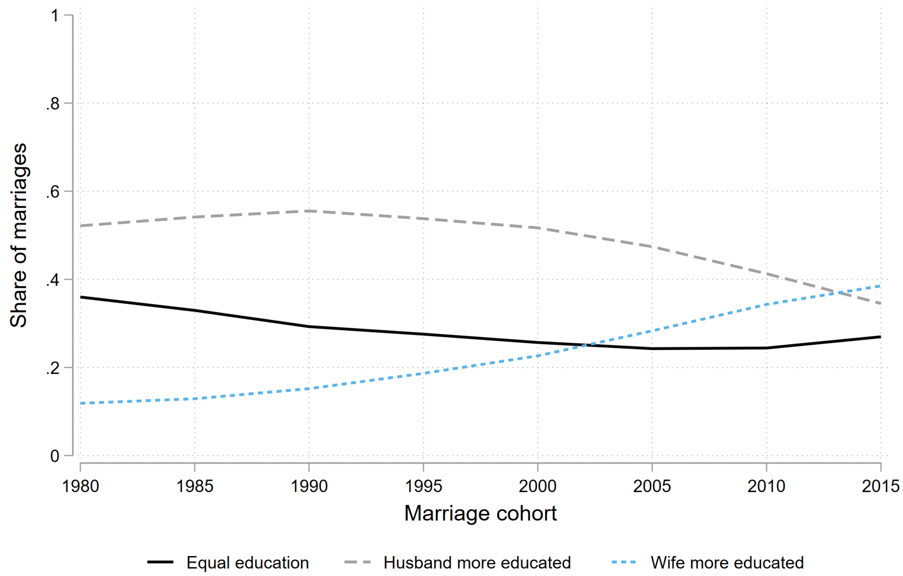

Figure B4: Direction of educational matching by marriage cohort

Notes: The figure plots the share of marriages in which spouses have equal education, the husband has more education, or the wife has more education. Marriage cohorts are grouped into five-year bins.

Appendix B.2 Additional Tables

Table B1: Education and Work in Matrimonial Advertisements

	Men (seeking brides)	Women (seeking grooms)
Number of profiles	8,355	6,430
Attributes signaled (%)		
Education signaled	76.10	75.85
Work signaled	46.99	21.12
Attributes demanded (%)		
Education demanded	53.56	47.57
Work demanded	18.77	47.57
Education or work demanded	59.34	56.25

Notes: This table summarizes characteristics of matrimonial advertisements from an Indian newspaper. "Signaled" refers to attributes mentioned in the profile description, while "demanded" refers to attributes specified for the prospective bride or groom. Education includes both explicit degrees and generic descriptors (e.g., "well-educated"). Work includes mentions of employment, occupation, or preference for employed partners. Percentages are calculated relative to the total number of profiles in each category.

Table B2: Effect of School Construction on Marriage Timing

	(1) Not married by 18	(2) Not married by 21	(3) Not married by 25
Panel A: Continuous treatment			
Schools built	0.003**	0.000	-0.002
	(0.002)	(0.002)	(0.001)
Mean of treatment	8.904	8.904	8.722
Elasticity of outcome	0.046	0.004	-0.099
Panel B: Binary treatment			
Schools built > mean	0.024**	0.011	-0.001
	(0.010)	(0.008)	(0.005)
Mean of treatment	0.342	0.342	0.340
Elasticity of outcome	0.013	0.011	-0.002
Mean of outcome	0.642	0.359	0.147
N	462,963	462,963	427,195

Notes: Each column reports estimates from regressions of the indicated marriage-timing outcome on school construction exposure in the woman's district during her school-age years, with district and birth-cohort fixed effects. Panel A uses cumulative schools constructed per 1,000 population aged 6–15 years. Panel B uses an indicator for whether exposure is above the cohort-specific mean. Elasticities are computed as $\beta \times \bar{X}/\bar{Y}$. Outcomes are indicators for whether a woman remained unmarried by the specified age. Standard errors (in parentheses) are clustered at the district level.

Table B3: Effect of School Construction on Direction of Educational Matching in Marriage

	(1) Hypergamy	(2) Hypogamy
Panel A: Continuous treatment		
Schools built	0.003	-0.000
	(0.002)	(0.001)
Mean of treatment	8.759	8.759
Elasticity of outcome	0.046	-0.016
Panel B: Binary treatment		
Schools built > mean	0.003	-0.009
	(0.009)	(0.006)
Mean of treatment	0.345	0.345
Elasticity of outcome	0.002	-0.013
Mean of outcome	0.504	0.231
N	360,548	360,548

Notes: Each column reports estimates from separate regressions where the dependent variable is an indicator for the type of educational match within marriage. In column (1), the outcome equals one if the husband has more years of schooling than the wife (hypergamy), and zero otherwise. In column (2), the outcome equals one if the wife has more years of schooling than the husband (hypogamy), and zero otherwise. Marriages in which spouses have equal education (homogamy) are included in the omitted category in both specifications. The key explanatory variable is district-level cumulative school construction exposure, measured as the number of schools per 1,000 school-age children (ages 6–15) in a woman's district during her school-age years. Panel A uses the continuous measure of exposure, while Panel B uses an indicator equal to one if exposure is above the cohort-specific mean. All regressions include district and birth-cohort fixed effects and are estimated using DHS sample weights. Standard errors, reported in parentheses, are clustered at the district level. The sample is restricted to ever-married women with non-missing information on both spouses' education. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment variable to the mean of the outcome.