School Construction, Household Formation, and Intergenerational Human Capital 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 fertility behavior and the educational and health outcomes of the next generation. An additional school per 1,000 school-age children, equivalent to roughly an 11 percent increase relative to the sample mean exposure, delays age at first birth for women by approximately 0.03 years and reduces completed fertility by nearly 6 percent over the life cycle. Increased school access also generated significant intergenerational improvements in children's educational attainment, increasing years of schooling by 8.6 percent and reducing the probability of falling behind grade level by nearly 19 percent. School construction additionally improves child height-for-age and increases the likelihood of institutional delivery. Overall, the findings suggest that large-scale investments in schooling generated persistent intergenerational benefits extending beyond directly exposed cohorts. School construction increased parental educational attainment and altered the distribution of education across households, but I find limited evidence that educational expansion substantially strengthened complementarities between parents in the production of child human capital.

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1 Introduction

Large-scale investments in schooling are typically evaluated by their effects on the directly exposed generation, including educational attainment, labor market outcomes, and demographic behavior. Yet educational expansion may also have consequences that extend across generations. By changing who becomes educated, patterns of marital sorting, and the timing and quantity of fertility, schooling policies may reshape the distribution of human capital across households and thereby affect the conditions into which children are born and the investments they receive (Becker, 1981; Fernández, Guner and Knowles, 2005; Greenwood et al., 2014; Eika, Mogstad and Zafar, 2019). These intergenerational effects are central to understanding the long-run returns to education policy, particularly in settings where marriage and fertility remain closely linked to household formation 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 access alongside substantial declines in fertility, delays in marriage, and large increases in women's educational attainment. At the same time, marriage remains nearly universal and continues to structure household formation, fertility, and the transmission of socioeconomic advantage across generations (Becker and Tomes, 1979; Black and Devereux, 2011). Moreover, because marriages in India remain heavily family-mediated[1], educational attainment functions as an especially salient matching characteristic within marriage markets. Educational assortative matching has also strengthened over time, suggesting that educational expansion may have altered not only average human capital accumulation, but also the distribution of parental human capital across households. As educational attainment rises, changes in assortative matching and fertility behavior may concentrate parental human capital within some households more than others, potentially reshaping the transmission of advantage across generations.

This paper studies the long-run intergenerational effects of educational expansion in India. I combine administrative data on school construction with nationally representative Demographic and Health Survey (DHS) data to examine how women's childhood exposure to school construction

[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.

affects fertility behavior and the educational and health outcomes of their children. The empirical strategy exploits variation across districts and birth cohorts in cumulative exposure to school construction during ages 6–15. This design allows me to study cumulative educational expansion over multiple decades rather than the effects of a single policy episode or short-run schooling reform.

The paper contributes to several literatures. First, it contributes to work on educational expansion and human capital accumulation. A large literature shows that increased access to schooling through school construction and educational expansion policies raises educational attainment and affects later-life demographic and labor market outcomes (Duflo, 2001; Breierova and Duflo, 2004; Osili and Long, 2008; Abanokova, Campos and Maitra, 2020; Khanna, 2023). In India, much of the existing evidence focuses on specific schooling reforms, particularly the District Primary Education Programme (DPEP), or on relatively shorter-run effects of educational expansion (Azam and Saing, 2016; Khanna, 2023; Sunder, 2019). I instead construct a long-run district-cohort panel of school construction exposure, allowing me to trace how cumulative educational expansion affected fertility behavior and the next generation's outcomes over multiple decades.

Second, the paper connects educational expansion to fertility and household formation. Education may affect the next generation not only by increasing parental schooling directly, but also by changing marriage timing, fertility behavior, and the allocation of human capital across households through assortative matching. This link is especially important in India, where marriage and childbearing remain closely connected and where educational sorting within marriage markets has strengthened over time. The paper therefore relates to work on fertility, marriage markets, and household sorting (Becker, 1981; Desai and Andrist, 2010; Vogl, 2013; Fernández, Guner and Knowles, 2005; Greenwood et al., 2014; Eika, Mogstad and Zafar, 2019; Schwartz, 2013). I show that school construction delayed fertility and reduced completed fertility, suggesting that educational expansion altered both the timing and quantity of childbearing. Together with the child education results, these patterns are consistent with a quantity–quality framework in which lower fertility is accompanied by greater investments in children's human capital (Becker, 1960; Becker and Lewis, 1973; Becker and Tomes, 1976; Doepke, 2015).

Third, the paper contributes to the literature on intergenerational transmission of human capital. Prior work shows that parental education is strongly associated with children's schooling and health, though identifying causal effects is difficult because parental education is correlated with family background, preferences, and resources (Behrman et al., 1999; Behrman and Rosenzweig, 2002; Currie and Moretti, 2003; Black, Devereux and Salvanes, 2005; Oreopoulos, Page and Stevens, 2006; Azam, 2015; Sunder, 2019). I use school construction as a source of variation in parental schooling and household formation to examine whether educational expansion generated persistent intergenerational improvements in children's outcomes. The estimates indicate significant positive effects on children's educational attainment and grade progression, with children of exposed women attaining more schooling and exhibiting smaller schooling deficits relative to age-appropriate progression. Effects on child health are more selective. School construction increases height-for-age and institutional delivery, but has limited effects on stunting and vaccination, suggesting that improvements in child health may depend more heavily on complementary health infrastructure and service provision.

I also examine how parental education and household educational composition shape these intergenerational effects. School construction increased women's educational attainment and altered the joint distribution of parental education through changes in marital sorting. The evidence suggests that improvements in children's schooling are closely linked to parental human capital and household educational composition. However, I find little evidence that school expansion strengthened complementarities between mothers' and fathers' education in producing child human capital. Instead, the relationship between maternal and paternal education exhibits diminishing marginal returns within households.

Overall, the paper shows that educational expansion generated persistent benefits extending beyond the directly exposed generation through changes in fertility behavior, parental human capital, and household formation. These findings broaden the interpretation of schooling investments: their effects are not limited to the individuals directly exposed to new schools, but extend to the next generation through the structure and resources of the households they form.

2 Institutional Context on Educational Expansion and Intergenerational Outcomes 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 barriers to schooling. Initiatives such as the Minimum Needs Programme in the 1970s, the District Primary Education Programme (DPEP) in the 1990s, and the Sarva Shiksha Abhiyan (SSA) in the 2000s substantially increased school construction, teacher recruitment, and educational funding across the country. These expansions generated large increases in the availability of primary and middle schools over time, particularly in rural areas where initial access had been limited.

As a result of these policies, younger cohorts were exposed to substantially greater school availability during their childhood years. Figure 1 shows that cumulative school exposure rose sharply across birth cohorts between the 1960s and 1990s. Average exposure more than doubled over this period, generating substantial variation across districts and cohorts in access to schooling during childhood. Census statistics similarly document major increases in educational infrastructure during this period. The share of villages with a primary school increased substantially between the 1970s and 1990s, while access to middle and secondary schools also expanded rapidly. By the 2010s, most rural households had access to a primary school within walking distance.

These educational expansions coincided with major changes in women's educational attainment, marriage timing, fertility behavior, and household formation. Figure 2 shows substantial increases in educational attainment across cohorts for both women and their husbands, implying large changes in the distribution of parental human capital across households over time. Women's average years of schooling rose sharply across cohorts, while the gender gap in education narrowed substantially among younger cohorts. At the same time, child marriage declined and fertility rates fell sharply over the same period. Educational assortative matching within marriage markets also strengthened, increasing the concentration of human capital within households. These patterns suggest that educational expansion may have affected children not only through increases in maternal schooling, but also through broader changes in household educational composition and parental matching patterns.

Several mechanisms may link parental educational expansion to children's outcomes. More educated parents may invest more in children's schooling, possess greater educational and health knowledge, delay fertility, or allocate household resources differently. Educational expansion may also alter the types of households children are born into by changing marriage market matching patterns and the joint distribution of parental human capital within families. These channels imply that large-scale schooling expansions may generate substantial intergenerational effects extending beyond the directly exposed cohorts themselves.

India provides a particularly useful setting for studying these intergenerational effects because educational expansion occurred over multiple decades and varied substantially across cohorts and districts. This allows the construction of cohort-specific measures of childhood exposure to school construction that can be linked to later fertility behavior and children's educational outcomes observed in nationally representative household surveys.

3 Conceptual Framework

Educational expansion may affect children's outcomes through several interconnected household channels. First, increased access to schooling raises parental educational attainment. More educated parents may invest more in children's human capital, possess greater educational and health knowledge, or allocate household resources differently. These effects may improve both children's educational attainment and health outcomes. Second, educational expansion may alter fertility and household formation decisions. Related work using the same school construction setting finds that increased school access delayed marriage, while the empirical results presented later in this paper show corresponding changes in fertility timing. Lower fertility or delayed childbearing may increase investments per child through quantity–quality tradeoffs and changes in parental resource allocation over the life cycle. Third, educational expansion may reshape marriage market matching patterns and the joint distribution of parental education within households. If school expansion increases educational assortative matching, children may increasingly be raised in households where both parents have relatively high levels of human capital. Changes in household educational composition may therefore affect intergenerational outcomes beyond the direct effect of maternal education alone.

These channels are jointly determined and difficult to separately identify empirically. Parental education, fertility behavior, and marital sorting are all endogenous responses to educational expansion and may affect one another simultaneously. The empirical analysis therefore focuses primarily on reduced-form intergenerational effects of school construction exposure. The parental education and household composition specifications should be interpreted as evidence on potential mechanisms and household correlates of intergenerational transmission rather than as a formal causal mediation decomposition.

4 Data

This paper combines administrative data on school construction with nationally representative household survey data and district-level population statistics to construct measures of educational exposure across districts and cohorts in India.

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 nationwide. The database contains detailed school-level information including school location, grade levels offered, management type, enrollment, infrastructure, teacher characteristics, and year of establishment. I use information on school location and establishment year to construct district-level measures of cumulative school availability over time. Although the UDISE data were collected in 2018, the database records establishment years for all schools operating at the time of the survey, allowing the construction of a long-run historical panel of school expansion. School establishment information is aggregated to the district-year level and merged with district-level population estimates to construct measures of schools per 1,000 school-age children. The main treatment variable is the cumulative number of schools constructed per 1,000 school-age children in a woman's district of residence during the years in which she was between ages 6 and 15. The treatment therefore captures exposure to the stock of schools available during childhood rather than contemporaneous construction flows. One limitation of the UDISE data is that it only includes schools operating at the time of the 2018 survey. Schools constructed historically but closed prior to the survey are not observed, potentially generating measurement error in historical exposure measures, particularly for earlier cohorts. If school closures are unrelated to later outcomes conditional on district and cohort fixed effects, this measurement error would tend to attenuate estimates toward zero. However, because older schools are mechanically more likely to be missing, some non-classical measurement error remains possible.

Information on education, fertility, and child outcomes comes from the Indian Demographic and Health Survey (DHS), also known as the National Family Health Survey (NFHS). The DHS is a nationally representative household survey collecting detailed information on demographic behavior, educational attainment, fertility histories, health outcomes, and household characteristics. The primary analysis uses data from the women's individual recode (IR), the household roster (PR), and the child recode (KR). The women's survey contains information on women aged 15–49, including years of schooling, fertility histories, age at first birth, marriage timing, and household characteristics. The household roster is used to construct education outcomes for school-age children and to recover husband characteristics using spouse line numbers reported in the women's questionnaire. The child recode contains detailed information on children under age five, including anthropometric measures, vaccination histories, and birth characteristics. The main fertility outcomes include age at first birth, children ever born, age at last birth, and the interval between marriage and first birth. Child education outcomes include years of schooling, a schooling gap measure defined relative to age-appropriate grade progression, and an indicator for being behind grade level. Child health outcomes include height-for-age z-scores (HAZ), stunting (an indicator for HAZ below -2), a vaccination index based on receipt of basic childhood vaccines, and an indicator for institutional delivery, defined as birth occurring in a public or private health facility rather than at home. District identifiers are publicly available in the Indian DHS beginning with the 2015 round. The analysis therefore relies primarily on the 2015 and 2019 DHS rounds, which provide nationally representative data with district-level geographic identifiers. The analysis links individuals to school construction exposure based on district of current residence. While the DHS does not directly observe childhood district of residence, migration during school-age years is relatively limited in India. Moreover, Census evidence suggests that most marriage migration occurs over relatively short geographic distances, implying that district of residence is a reasonable proxy for childhood exposure for much of the population.

Population data are drawn from decadal Census of India population tables and district variation tables. These data are used to construct annual estimates of district-level school-age population through linear interpolation across census years. Combining these population estimates with school establishment information allows the construction of cohort-specific measures of school availability. A major challenge in long-run district-level analysis in India is the large number of administrative boundary changes since the first post-Independence census. Districts have been repeatedly split, merged, and reorganized, while several new states have also been created. To address this issue, I construct a harmonized district panel that maps historical school construction data, Census population estimates, and DHS district identifiers into a common geography based on harmonized 2011 district boundaries.[2] This harmonization allows consistent measurement of school exposure across districts and cohorts over multiple decades.

Table 1 reports summary statistics for the main analysis samples. Women in the mother-level sample have an average of 4.6 years of schooling and approximately 3 children ever born. Among children aged 6–14, average schooling is 3.6 years, while 35 percent are behind age-appropriate grade level. Parents in the child education sample have substantially higher average schooling than women in the overall mother-level sample, reflecting selection into the sample of households with school-age children observed in the household roster. Child health outcomes indicate substantial deprivation: average height-for-age is more than one standard deviation below the WHO reference population, and roughly 35 percent of children are stunted.

5 Empirical Strategy

The empirical analysis exploits variation across districts and birth cohorts in exposure to school construction during childhood. School expansion in India occurred gradually over several decades through large-scale public investments in educational infrastructure under successive Five-Year Plans and later programs such as the District Primary Education Programme (DPEP) and Sarva Shiksha Abhiyan (SSA). These programs substantially expanded school availability across the country, generating variation across districts and cohorts in exposure to schooling during the school-age years.

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

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

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

where Y_{ijdt} denotes either an outcome for woman i or an outcome for child j born to woman i, where the mother belongs to birth cohort t and resides in district d. The treatment variable, SchoolExposure$_{dt}$, measures the cumulative number of schools constructed per 1,000 school-age children in district d during the years in which woman i was between ages 6 and 15.

The identifying assumption is that, absent school construction, outcomes across districts would have evolved similarly across 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 childhood.

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 exposure. Following Duflo (2001), I additionally present specifications using a binary treatment measure equal to one if exposure is above the cohort-specific mean and zero otherwise. The results are qualitatively similar across specifications.

The analysis examines several categories of outcomes. First, I estimate the effect of school construction exposure on fertility and household formation outcomes, including age at first birth, completed fertility, and fertility timing over the life cycle. Second, I examine children's educational outcomes, including years of schooling, schooling gaps relative to age-appropriate progression, and the probability of falling behind grade level. Third, I examine child health outcomes including height-for-age, stunting, vaccination, and institutional delivery. Finally, I explore the role of parental educational composition and assortative matching in shaping intergenerational transmission.

One limitation of the empirical design is that the DHS does not directly observe childhood district of residence. Individuals 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 fertility or child 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 underlying the empirical strategy is that, absent school construction, outcomes across districts would have evolved similarly over time. Standard event-study approaches are less well suited to this setting because school expansion occurred gradually over several decades rather than through a discrete policy shock, and treatment intensity varies continuously across cohorts within districts. The relevant concern is therefore whether districts that ultimately experienced larger school expansion were already on different outcome trajectories prior to substantial exposure. To assess this, I implement cohort-based pre-trend tests using early maternal cohorts only. I first construct a measure of eventual school exposure, defined as the district-level average cumulative school construction exposure among later cohorts. I then interact this measure with indicators for 5-year maternal birth cohort bins while including district and cohort-bin fixed effects. Under the identifying assumption, cohorts born prior to major school expansion should not exhibit systematically different trends across districts with higher versus lower eventual exposure.

Appendix Table B1 reports the results for age at first birth, completed fertility, and child years of schooling. Across all outcomes, the interaction coefficients between cohort indicators and eventual exposure are small in magnitude and statistically insignificant. For age at first birth, the coefficients for the 1970 and 1975 cohorts are -0.125 and -0.145, respectively, with large standard errors relative to the point estimates. Similarly, for completed fertility, the estimated interactions are close to zero and imprecisely estimated. Joint tests fail to reject the null hypothesis that the interaction terms are jointly equal to zero, with p-values of 0.867 and 0.423 for age at first birth and completed fertility, respectively. The final column presents an analogous exercise for children's years of schooling. The interaction coefficients are again statistically indistinguishable from zero, and the joint test yields a p-value of 0.810. This specification should be interpreted more cautiously because children observed among early maternal cohorts constitute a selected sample. Nonetheless, the absence of differential trends is broadly consistent with the identifying assumptions of the empirical design.

Overall, the evidence from these cohort-based pre-trend tests suggests that districts with greater eventual school expansion were not already experiencing systematically different trends in fertility or intergenerational outcomes prior to substantial exposure, supporting the interpretation of the main estimates as reflecting the causal effects of school construction.

7 Results

For the main outcome tables in the paper, I use two measures of school construction exposure. The first is a continuous measure capturing the cumulative number of schools constructed per 1,000 school-age children in a woman's district during ages 6–15. The second is a binary indicator equal to one if exposure is above the cohort-specific mean. For the continuous measure, one additional school per 1,000 children corresponds to roughly an 11 percent increase relative to the sample mean exposure.

7.1 Effects on Women's Fertility Outcomes

Educational expansion may affect fertility behavior through several channels. Increased access to schooling may delay marriage formation, while additional education may also alter fertility preferences, bargaining power within households, and investments in child quality. In settings such as India, where marriage and childbearing remain closely linked, changes in marriage timing may translate directly into changes in fertility behavior over the life cycle. Earlier results in the paper show that school construction increased women's educational attainment and delayed marriage formation. This raises the question of whether educational expansion also affected subsequent fertility timing and completed fertility.

Figure 3 provides a descriptive view of fertility patterns across maternal birth cohorts by plotting age-specific fertility rates over the life cycle. Later cohorts exhibit systematically lower fertility throughout the prime childbearing ages, particularly during the early twenties when fertility is most concentrated among older cohorts. Fertility profiles also become somewhat flatter across cohorts, with fertility shifting modestly away from the youngest childbearing ages. At older maternal ages, fertility rates for younger cohorts do not fully converge to those of older cohorts, suggesting that delayed fertility does not completely offset reductions in total fertility. These descriptive patterns are consistent with both declining fertility levels and modest delays in fertility timing across cohorts.

Table 2 reports the estimated effects of school construction exposure on fertility outcomes. Increased school access significantly delays the timing of childbearing. A one-unit increase in school construction exposure increases age at first birth by 0.034 years. At the same time, school construction substantially reduces fertility levels. A one-unit increase in exposure reduces the number of children ever born by 0.068 children, corresponding to roughly a 19 percent decline relative to the sample mean. The results further suggest that these effects reflect persistent changes in lifetime fertility rather than temporary delays alone. Completed fertility among women aged 40 and above also declines significantly with school exposure, with an estimated reduction of approximately 6 percent relative to the sample mean.[3] The estimates also indicate changes in fertility timing over the life cycle. Although school exposure increases age at first birth, age at last birth among women aged 40 and above declines modestly. In contrast, the interval between marriage and first birth remains small and statistically insignificant. Together, these patterns suggest that delayed fertility operates primarily through later marriage formation rather than through postponement of births within marriage. The decline in age at last birth is similarly consistent with lower completed fertility among exposed women.

Overall, the fertility results indicate that educational expansion affected not only women's schooling and marriage outcomes, but also subsequent household formation and demographic behavior over the life cycle. These fertility responses may themselves contribute to intergenerational effects if lower fertility and delayed childbearing increase investments in children's human capital.

[3] Appendix Table B2 shows that the negative effects on completed fertility are broadly similar when restricting the sample to women aged 35 and above or 45 and above, although estimates become less precise at older age thresholds due to smaller sample sizes.

7.2 Intergenerational Effects on Children's Education

The effects of educational expansion may extend beyond women's own schooling, marriage, and fertility outcomes to influence investments in the next generation. Increased maternal education may improve children's educational attainment through several channels, including higher household resources, greater parental investments in child quality, changes in fertility behavior, and improved educational matching within households. The fertility results presented earlier are particularly suggestive of a quantity–quality trade-off, as school construction exposure reduced completed fertility while simultaneously delaying childbearing. This raises the question of whether these demographic changes translated into improved educational outcomes for children.

To examine these intergenerational effects, I estimate variants of equation (1), where exposure is defined based on the mother's childhood district and cohort. Table 3 reports the effects of school construction exposure on children's education outcomes among children aged 6–14. Outcome variables include years of schooling, a schooling gap measure defined as completed schooling minus age-appropriate grade-for-age, and an indicator for being behind the age-appropriate grade level. All specifications include district and maternal birth cohort fixed effects. Regressions for years of schooling additionally include child age fixed effects, while all specifications include a gender indicator for the child. The results indicate significant improvements in children's educational attainment. A one-unit increase in school construction exposure increases children's years of schooling by 0.034 years. Exposure also improves grade progression outcomes. Children of exposed mothers are 0.7 percentage points less likely to fall behind the age-appropriate grade level, corresponding to an elasticity of roughly -0.19.

Taken together with the fertility results, the estimates suggest that educational expansion affected not only women's own human capital accumulation, but also investments in the next generation. The combination of lower fertility and improved child educational outcomes is consistent with a quantity–quality trade-off in which reductions in fertility are accompanied by greater investments in children's schooling.[4]

[4] Appendix Table B3 shows that the results are qualitatively similar when expanding the sample to include children aged 6–18, although estimates become modestly larger in magnitude for some outcomes.

7.3 Heterogeneity by Child Gender

While the baseline results show positive intergenerational effects on children's education, an important question is whether these gains are distributed similarly across boys and girls. Gender disparities in schooling remain substantial in many developing country contexts, including India, and educational investments within households may differ systematically by child gender. Examining heterogeneity by gender therefore provides insight into whether the intergenerational effects of educational expansion reinforce or reduce existing gender inequalities in child human capital accumulation.

Table 4 examines heterogeneity by child gender by interacting school construction exposure with an indicator for female children. The coefficient on school exposure captures the effect for boys, while the interaction term measures the differential effect for girls relative to boys. The estimates indicate that school construction exposure improves educational outcomes for boys across all measures. Boys experience increases in years of schooling, reductions in schooling gaps, and lower probabilities of being behind the age-appropriate grade level. The interaction terms are statistically significant but small in magnitude. Relative to boys, girls experience slightly smaller gains in years of schooling and schooling progression outcomes. Nevertheless, the overall effects for girls remain positive across outcomes.

These results suggest that educational expansion generated broad intergenerational improvements in children's education for both boys and girls, although gender gaps in educational attainment and progression persist. The findings are therefore more consistent with unequal gains across gender rather than complete convergence in educational outcomes.

7.4 Parental Educational Composition and Children's Education

The baseline results in Table 3 show that school construction exposure improves children's educational outcomes, increasing years of schooling and reducing both schooling gaps and the probability of being behind grade level. The most direct mechanism through which these intergenerational effects are likely to operate is parental human capital. School construction increased women's educational attainment and altered marriage market matching patterns, changing the educational composition of households. More educated parents may invest more in children's schooling, possess greater educational and health knowledge, or provide home environments more conducive to learning. At the same time, changes in assortative matching may alter the joint distribution of parental education within households, potentially affecting child outcomes through household educational composition rather than maternal education alone. In practice, however, these channels are difficult to cleanly separate because parental education and marital sorting are themselves endogenous outcomes of school construction exposure. The analysis in this section therefore does not attempt a formal causal decomposition of mechanisms. Instead, it examines how the estimated relationship between school construction and child schooling changes once parental education and household educational composition are incorporated into the specification. This provides evidence on the extent to which the reduced-form intergenerational effects are closely associated with parental human capital and sorting patterns within households.

All specifications in Table 5 use children's years of schooling as the outcome and include district and maternal birth cohort fixed effects, along with child age fixed effects and an indicator for child gender. Mother's and father's years of education are standardized to have mean zero and standard deviation one. Standardization allows the interaction coefficients to be interpreted in comparable units and aligns naturally with the assortative matching framework, since relative educational position within the maternal and paternal education distributions may matter more for household formation and child outcomes than absolute years of schooling alone. In the estimation sample, one standard deviation corresponds to approximately five years of schooling for both mothers and fathers.

Column (1) includes additive measures of maternal and paternal education. Both are strongly positively associated with child schooling. A one standard deviation increase in maternal education is associated with approximately 0.04 additional years of child schooling, while the corresponding association for paternal education is substantially larger at approximately 0.13 years. Relative to the baseline specification in Table 3, the coefficient on school construction exposure declines meaningfully once parental education is included, although it remains positive and statistically significant. This pattern suggests that an important share of the reduced-form intergenerational relationship operates through parental educational attainment and household composition. Column (2) additionally includes an interaction between maternal and paternal education. The interaction term is negative and statistically significant, indicating that the marginal association between one parent's education and child schooling is smaller when the other parent is already highly educated. This pattern is inconsistent with strong complementarities or supermodularity in parental education. Instead, it suggests diminishing marginal returns to parental human capital within households. Put differently, increases in maternal education are most strongly associated with child schooling in households where paternal education is relatively low, while the marginal association becomes weaker when fathers are already highly educated. Column (3) further allows these parental education interactions to vary with school construction exposure through a three-way interaction. The estimated triple interaction is positive and statistically significant, but economically small in magnitude. Similarly, the interactions between parental education and school construction exposure are quantitatively modest. Taken together, these estimates provide little evidence that school expansion substantially changes the relationship between maternal and paternal education in the production of child human capital. Because parental education is itself affected by school construction, these specifications should not be interpreted as a formal causal mediation analysis. Rather, the results indicate that much of the intergenerational relationship between school construction and child schooling operates through parental educational attainment and household educational composition, with limited evidence of additional changes in complementarities between parents.

Figure 4 provides a complementary visualization of these patterns by plotting predicted child years of schooling across different levels of maternal education separately for low, average, and high levels of paternal education. The figure illustrates the negative interaction estimated in Table 5. When paternal education is relatively low, increases in maternal education are strongly associated with higher levels of child schooling. In contrast, when paternal education is already high, the relationship between maternal education and child outcomes becomes substantially flatter and may even turn slightly negative at the upper end of the distribution. This pattern is more consistent with diminishing marginal returns to parental human capital within households than with strong complementarities between parents. At the same time, households with highly educated fathers maintain consistently higher predicted levels of child schooling overall, indicating persistent advantages associated with higher parental human capital.

7.5 Intergenerational Effects on Children's Health

Educational expansion may affect not only children's educational attainment, but also broader dimensions of child well-being. Increased parental schooling can improve health knowledge, healthcare utilization, nutrition practices, and investments in children, potentially generating intergenerational improvements in child health. In addition, the fertility results presented earlier suggest that school construction reduced fertility and delayed childbearing, both of which may alter parental investments per child. Examining child health outcomes therefore provides a complementary perspective on the broader intergenerational consequences of educational expansion.

Table 6 reports the effect of school construction exposure on child health outcomes using the same empirical specification as equation (1), applied to a sample of children under age five from the DHS KR data. The outcomes examined include height-for-age z-scores (HAZ), an indicator for stunting, a vaccination index, and institutional delivery. Height-for-age is a widely used measure of long-run nutritional status and chronic childhood health because it captures cumulative health and nutritional deficits experienced during early childhood. Unlike short-run measures such as weight-for-age, height-for-age reflects persistent investments in child health and is therefore particularly useful for studying longer-term intergenerational effects. Stunting is defined as an indicator equal to one if height-for-age falls below -2 standard deviations relative to WHO reference standards, capturing moderate or severe chronic undernutrition. The remaining outcomes capture healthcare utilization and access to formal health services. The vaccination index is constructed as the average of indicators for receipt of basic childhood vaccines, including BCG, DPT, polio, and measles vaccines. Institutional delivery is an indicator equal to one if the child was delivered in a public or private health facility rather than at home or in a non-institutional setting. Relative to anthropometric outcomes, these measures are likely to depend more directly on access to healthcare infrastructure and public health service provision. Specifications for vaccination outcomes include child age fixed effects to account for the age-specific timing of immunization schedules, while gender indicators are included throughout.

The results suggest that school construction had modest but meaningful effects on some dimensions of child health. Exposure to school construction significantly increases children's height-for-age scores, indicating improvements in long-run nutritional status and early-life health investments. In contrast, the estimated effects on stunting are negative but small and statistically insignificant. This pattern suggests that educational expansion may have improved average nutritional outcomes without substantially reducing the prevalence of severe chronic undernutrition at the lower tail of the distribution. The results also indicate positive effects on institutional delivery. A one-unit increase in school construction exposure increases the probability of institutional delivery by approximately 0.009, corresponding to an elasticity of roughly 0.10 relative to the sample mean. This finding is consistent with increased healthcare utilization and greater engagement with formal medical services among exposed cohorts. By contrast, the estimated effects on vaccination outcomes are close to zero and statistically insignificant. One possible explanation is that vaccination coverage depends more heavily on the local supply and outreach of public health systems, limiting the extent to which increased maternal education alone translates into higher vaccination rates.

Taken together, the results suggest that the intergenerational effects of educational expansion extend beyond schooling outcomes to include some improvements in child health and healthcare utilization. However, the health effects appear more modest and selective than the corresponding effects on children's educational attainment, indicating that educational expansion alone may be insufficient to substantially improve all dimensions of child health in the absence of complementary health infrastructure and service provision.

8 Conclusion

The evidence presented in this paper suggests that large-scale educational expansion in India generated significant intergenerational effects on education and health outcomes through changes in fertility behavior, parental human capital, and household formation. An additional school per 1,000 school-age children delayed fertility and reduced completed fertility over the life cycle, while also improving children's educational attainment and grade progression. In contrast, the effects on child health were more modest, with positive effects on height-for-age and institutional delivery but weaker effects on vaccination and stunting. School construction increased women's educational attainment and altered patterns of marital sorting, changing the distribution of parental human capital across households. At the same time, I find little evidence that educational expansion strengthened complementarities between mothers' and fathers' education in the production of child human capital. Instead, the relationship between maternal and paternal education exhibits diminishing marginal returns within households.

More broadly, the paper highlights the importance of studying educational expansion in general equilibrium household settings rather than focusing solely on individual-level returns to schooling. Changes in who becomes educated, who marries whom, and when households form may all influence the long-run distribution of human capital across generations. In future work, I hope to further examine how educational expansion interacts with labor markets and household formation to shape intergenerational mobility and household stratification over time.

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9 Figures and tables

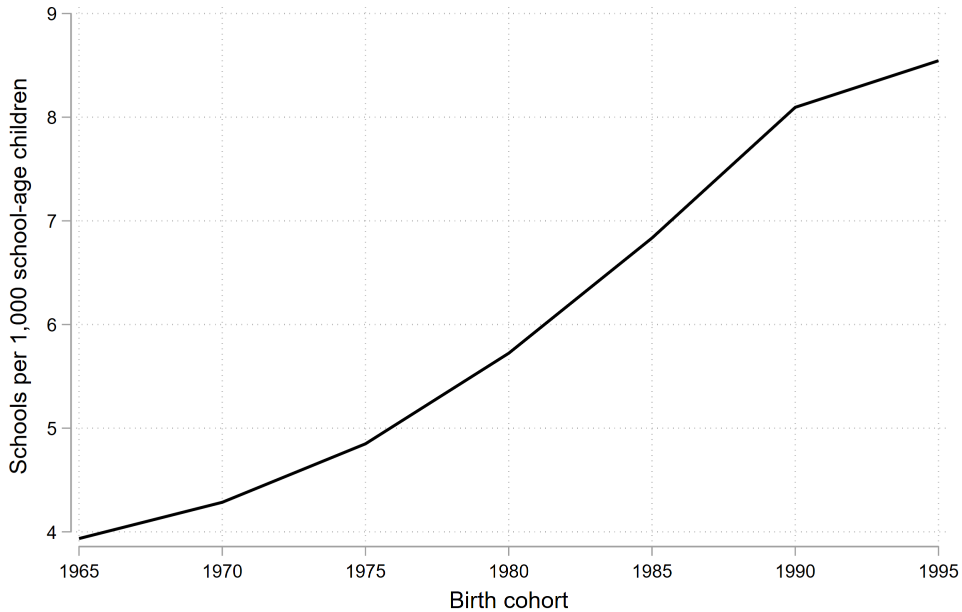

Figure 1: School exposure by women's birth cohort

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

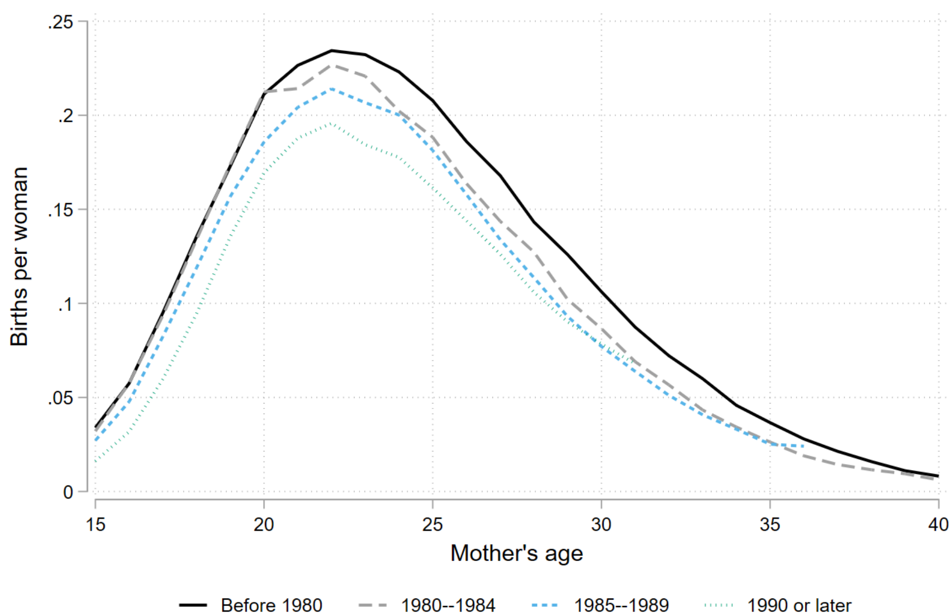

Figure 3: Age-specific fertility by mother's birth cohort
Notes: Fertility rates are calculated as the weighted number of births occurring at each maternal age divided by the weighted number of women observed at that age. Cohorts are grouped by mother's year of birth. The denominator includes all women observed at a given age, including women with no births. Note that observations at later ages for younger cohorts are based on progressively smaller samples.

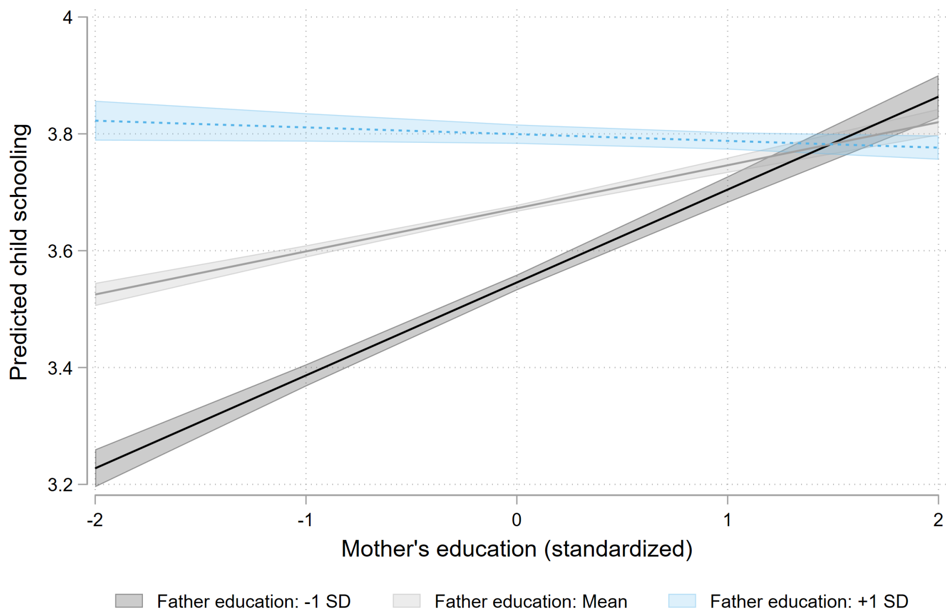

Figure 4: Predicted Child Schooling by Maternal and Paternal Education

Notes: The figure plots predicted years of child schooling from the specification including interactions between maternal and paternal education reported in Column 2 of Table 5. Mother's and father's education are standardized to have mean zero and standard deviation one. In the estimation sample, one standard deviation corresponds to approximately 5 years of schooling for both mothers and fathers. The horizontal axis plots maternal education continuously from -2 to $+2$ standard deviations, while separate lines correspond to paternal education evaluated at -1 standard deviation, the mean, and $+1$ standard deviation. Shaded areas represent 95 percent confidence intervals. Predictions are evaluated holding other covariates at their observed values.

Table 1: Summary Statistics

Variable	Mean	SD	N
Panel A: Mother fertility outcomes sample			
School exposure	6.638	5.611	146,388
Years of schooling	4.565	4.993	146,388
Age at first birth	21.113	4.341	141,074
Children ever born	3.073	1.657	146,388
Age at last birth	27.922	5.359	141,074
Panel B: Child education outcomes sample			
Child years of schooling	3.556	2.628	372,586
Schooling gap	-0.344	1.328	372,586
Behind grade level	0.354	0.478	372,586
Mother years of schooling	5.438	5.002	372,671
Father years of schooling	7.345	4.813	322,053
Panel C: Child health outcomes sample			
Height-for-age z-score	-1.312	1.816	151,541
Stunted	0.353	0.478	151,541
Vaccination index	0.817	0.312	89,367
Institutional delivery	0.861	0.346	170,241

Notes: Panel A reports summary statistics for the mother-level fertility sample restricted to women over 40 with completed fertility. Panel B reports summary statistics for the child education sample of children aged 6–14. Panel C reports summary statistics for the child health sample of children under age five. School exposure is the cumulative number of schools constructed per 1,000 school-age children in the mother's district of residence during ages 6–15.

Table 2: Effect of School Construction on Fertility Outcomes

	(1) Marriage to first birth	(2) Age at first birth	(3) Age at last birth	(4) Children ever born	(5) Completed fertility
Panel A: Continuous treatment					
Schools built	-0.010	0.034**	-0.276***	-0.068***	-0.037**
	(0.007)	(0.014)	(0.071)	(0.008)	(0.017)
Mean of treatment	6.258	6.255	4.626	6.383	4.627
Elasticity of outcome	-0.027	0.010	-0.047	-0.185	-0.058
Panel B: Binary treatment					
Schools built > mean	-0.021	0.248***	0.066	-0.176***	-0.069
	(0.032)	(0.066)	(0.124)	(0.031)	(0.051)
Mean of treatment	0.164	0.164	0.148	0.166	0.148
Elasticity of outcome	-0.001	0.002	0.000	-0.012	-0.003
Mean of outcome	2.327	20.717	27.378	2.347	2.993
N	397,202	407,881	141,074	462,963	146,388

Notes: The table reports estimates of the effect of school construction exposure on fertility outcomes. The treatment variable in Panel A is the cumulative number of schools built per 1,000 school-age children in a woman's district of residence during ages 6–15. Panel B uses a binary indicator equal to one if exposure is above the sample mean. Age at first birth, the interval between marriage and first birth, and age at last birth are measured in years, while children ever born and completed fertility are measured as counts. Age at last birth and completed fertility are measured among women aged 40 and above. Regressions for children ever born additionally include mother age fixed effects to account for mechanical lifecycle variation in fertility. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment to the mean of the outcome. Standard errors (in parenthesis) are clustered at the district level.

Table 3: Intergenerational Effect of School Construction on Child Education Outcomes

	(1) Years of schooling	(2) Schooling gap	(3) Behind grade level
Panel A: Continuous treatment			
Schools built	0.034***	0.030***	-0.007***
	(0.006)	(0.006)	(0.002)
Mean of treatment	8.993	8.993	8.993
Elasticity of outcome	0.086	-0.791	-0.188
Panel B: Binary treatment			
Schools built > mean	0.011	0.009	-0.000
	(0.023)	(0.023)	(0.010)
Mean of treatment	0.336	0.336	0.336
Elasticity of outcome	0.001	-0.009	-0.000
Mean of outcome	3.556	-0.344	0.354
N	372,586	372,586	372,586
Child age FE	Yes	No	No
Child gender FE	Yes	Yes	Yes

Notes: The table reports estimates of the intergenerational effect of school construction exposure on children's education outcomes. The treatment variable in Panel A is the cumulative number of schools built per 1,000 school-age children in the mother's district of residence during ages 6–15. Panel B uses a binary indicator equal to one if exposure is above the cohort-specific mean. The sample includes children aged 6–14 at the time of the survey. Outcome variables include years of schooling, schooling gap (defined as years of schooling minus expected grade-for-age), and an indicator for being behind the age-appropriate grade level. Regressions for years of schooling additionally include child age fixed effects, while all specifications include a gender indicator for the child. All specifications include district and maternal birth cohort fixed effects. Standard errors (in parentheses) are clustered at the district level. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment to the mean of the outcome.

Table 4: Intergenerational Effect of School Construction on Child Education: Heterogeneity by Gender

	(1) Years of schooling	(2) Schooling gap	(3) Behind grade level
Schools built	0.035***	0.032***	-0.008***
	(0.006)	(0.006)	(0.002)
Female child	0.119***	0.119***	-0.042***
	(0.012)	(0.012)	(0.004)
Schools $\times$ Female child	-0.003**	-0.003**	0.001***
	(0.001)	(0.001)	(0.000)
Mean of outcome	3.556	-0.344	0.354
N	372,586	372,586	372,586
Child age FE	Yes	No	No

Notes: The table reports heterogeneous effects of school construction exposure on children's education outcomes by child gender. The treatment variable is the cumulative number of schools built per 1,000 school-age children in the mother's district of residence during ages 6–15. The sample includes children aged 6–14. Regressions include an interaction between school construction exposure and an indicator for female child. The coefficient on "Schools built" represents the effect for boys, while the interaction term captures the differential effect for girls. Child age fixed effects are included only for the years of schooling outcome. For schooling gap and behind-grade indicators, age is accounted for in the construction of the dependent variable and is therefore not included as a control. All specifications include district and maternal birth cohort fixed effects. Standard errors are clustered at the district level.

Table 5: Intergenerational Effect of School Construction on Child's Years of Schooling:
Effect of Parental Education Channel

	Child years of schooling		
	(1) Additive parental education	(2) Parental education interaction	(3) Interaction with school exposure
Schools built	0.026***	0.026***	0.026***
	(0.006)	(0.005)	(0.005)
Mother education	0.036***	0.074***	0.076***
	(0.005)	(0.005)	(0.010)
Father education	0.134***	0.127***	0.156***
	(0.008)	(0.007)	(0.012)
Mother education × Father education		-0.085***	-0.101***
		(0.005)	(0.009)
Mother education × Schools built			-0.000
			(0.001)
Father education × Schools built			-0.004***
			(0.001)
Mother × Father × Schools built			0.002**
			(0.001)
Mean of outcome	3.581	3.581	3.581
N	321,998	321,998	321,998
Child age FE	Yes	Yes	Yes
Child gender FE	Yes	Yes	Yes

Notes: The table reports estimates of the intergenerational effect of school construction exposure on children's years of schooling. The treatment variable is the cumulative number of schools built per 1,000 school-age children in the mother's district of residence during ages 6–15. The sample includes children aged 6–14 at the time of the survey. Mother's and father's education are standardized to have mean zero and standard deviation one. Column (1) includes additive measures of parental education. Column (2) additionally includes an interaction between mother's and father's education. Column (3) further allows the parental education interaction to vary with school construction exposure. All specifications include district and maternal birth cohort fixed effects, child age fixed effects, and an indicator for child gender. Standard errors (in parentheses) are clustered at the district level.

Table 6: Intergenerational Effect of School Construction on Child Health Outcomes

	(1) Height-for-age (z-score)	(2) Stunted (z-score < −2)	(3) Vaccination index	(4) Institutional delivery
Panel A: Continuous treatment				
Schools built	0.018**	-0.002	-0.001	0.009***
	(0.009)	(0.002)	(0.002)	(0.002)
Mean of treatment	10.556	10.556	10.568	10.479
Elasticity of outcome	-0.147	-0.057	-0.007	0.104
Panel B: Binary treatment				
Schools built > mean	0.058	-0.014	-0.001	0.015*
	(0.043)	(0.011)	(0.009)	(0.009)
Mean of treatment	0.356	0.356	0.353	0.352
Elasticity of outcome	-0.016	-0.014	-0.000	0.006
Mean of outcome	-1.312	0.353	0.817	0.863
N	151,540	151,540	89,367	164,195
Child age FE	No	No	Yes	No
Child gender FE	Yes	Yes	Yes	Yes

Notes: The table reports estimates of the intergenerational effect of school construction exposure on child health outcomes. The treatment variable in Panel A is the cumulative number of schools built per 1,000 school-age children in the mother's district of residence during ages 6–15. Panel B uses a binary indicator equal to one if exposure is above the sample mean. The sample consists of children under age five. Height-for-age (HAZ) is constructed using WHO-standardized z-scores, rescaled by dividing by 100. Stunting is an indicator equal to one if HAZ is below −2, capturing moderate or severe chronic undernutrition. The vaccination index is defined as the average of indicators for receipt of basic vaccines (BCG, DPT, polio, and measles), where each vaccine is coded as one if recorded on the vaccination card or reported by the mother. Institutional delivery is an indicator equal to one if the child was delivered in a public or private health facility, and zero if delivered at home or in a non-institutional setting. Child age fixed effects are included for vaccination outcomes to account for the age-specific timing of immunization schedules. For height-for-age and stunting, age is accounted for in the construction of standardized z-scores, and for institutional delivery the outcome is determined at birth; therefore, age fixed effects are not included for these outcomes. A gender indicator is included in all specifications. All regressions include district and maternal birth cohort fixed effects. Standard errors are clustered at the district level. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment to the mean of the outcome.

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 B Additional Tables

Table B1: Pre-trend tests for fertility and intergenerational outcomes

	(1) Age at first birth	(2) Completed fertility	(3) Child years schooling
1970 cohort × eventual exposure	-0.125	-0.019	0.154
	(0.276)	(0.112)	(0.265)
1975 cohort × eventual exposure	-0.145	-0.050	0.143
	(0.285)	(0.114)	(0.269)
Mean of outcome	21.261	3.156	4.716
N	77,598	80,409	25,131
Joint F-statistic	0.142	0.861	0.211
Joint p-value	0.867	0.423	0.810

Notes: The table reports cohort-specific pre-trend tests using early maternal cohorts only. Eventual school exposure is defined as district-level average cumulative school construction exposure among later cohorts and standardized to have mean zero and standard deviation one. All regressions include district and cohort-bin fixed effects. The child years of schooling specification additionally includes child age fixed effects and a child gender indicator. Standard errors are clustered at the district level.

Table B2: Effect of School Construction on Completed Fertility: Robustness to Age Restrictions

	(1) Age 35+	(2) Age 40+	(3) Age 45+
Schools built	-0.047***	-0.037**	-0.037
	(0.009)	(0.017)	(0.047)
Mean of treatment	5.087	4.627	4.333
Elasticity of outcome	-0.083	-0.058	-0.052
Mean of outcome	2.861	2.993	3.099
N	233,302	146,388	74,421

Notes: The table reports estimates of the effect of school construction exposure on completed fertility under alternative age restrictions. The treatment variable is the cumulative number of schools built per 1,000 school-age children in a woman's district of residence during ages 6–15. Each column restricts the sample to women above the indicated age threshold. All specifications include district and birth cohort fixed effects. Standard errors are clustered at the district level. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment to the mean of the outcome.

Table B3: Intergenerational Effect of School Construction on Child Education: Robustness to Age Window (6-18 Years)

	(1) Years of schooling	(2) Schooling gap	(3) Behind grade level
Schools built	0.046***	0.044***	-0.010***
	(0.008)	(0.007)	(0.002)
Mean of treatment	8.638	8.638	8.638
Elasticity of outcome	0.084	-0.733	-0.227
Mean of outcome	4.744	-0.514	0.392
N	480,400	480,400	480,400
Child age FE	Yes	No	No
Child gender FE	Yes	Yes	Yes

Notes: The table reports robustness estimates of the intergenerational effect of school construction exposure on children's education outcomes using a broader sample of children aged 6–18. The treatment variable is the cumulative number of schools built per 1,000 school-age children in the mother's district of residence during ages 6–15. Outcome variables include years of schooling, schooling gap, and an indicator for being behind the age-appropriate grade level. Child age fixed effects are included only for the years of schooling regression, while a gender indicator is included in all specifications. For schooling gap and behind-grade indicators, age is accounted for in the construction of the dependent variable and is therefore not included as a control. All specifications include district and maternal birth cohort fixed effects. Standard errors are clustered at the district level. Elasticities are calculated as the coefficient multiplied by the ratio of the mean of the treatment to the mean of the outcome.