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Compulsory Schooling and Long-Term Outcomes: Evidence from a Nationwide Education Reform in Mexico

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Abstract: This paper examines the long-term and intergenerational impacts of Mexico's 1993 reform extending compulsory schooling from six to nine years. Exploiting the age-based discontinuity in exposure, the study implements a regression discontinuity and instrumental-variable strategy to estimate the causal effect of education. The reform increased schooling on average, with disproportionately large gains among Indigenous and rural populations. These educational improvements translated into lasting shifts in fertility, child mortality, employment, and internal migration. Intergenerationally, parental schooling gains raised secondary and upper-secondary enrolment among children. By following a single reform across demographic, labour-market, and intergenerational domains, the paper provides a life-course perspective on how expanded schooling reshapes life trajectories. The results highlight the role of compulsory schooling in reducing structural inequalities and demonstrate that, in the context of a large middle-income country, such reforms can generate sustained and intergenerational benefits beyond immediate educational attainment.

JEL codes: I25, I26, J24, J62,

Key words: Education policy, Compulsory schooling, Educational attainment, Intergenerational mobility, Fertility, Labour Markets, Migration.

I. Introduction

Education is widely recognised as a key driver of social and economic development, and understanding its long-term impacts remains a central concern for research and policy. A large body of empirical work has examined how education reforms (such as compulsory schooling mandates, school construction or changes in term length)[1] have influenced outcomes ranging from labour market performance to fertility, health, crime, and intergenerational mobility.[2] Most causal evidence, however, comes from high-income countries, and large-scale reforms in middle-income settings remain comparatively underexamined. Mexico offers a particularly important case.

In 1993, the Mexican government extended compulsory schooling from six to nine years, making lower secondary (grades 7–9) mandatory alongside primary schooling. This reform applied to cohorts born after 1980 and marked a major institutional shift in one of Latin America's most unequal economies. Its scale, sudden implementation, and sharp eligibility threshold created a natural setting for causal evaluation. Assessing whether such reforms translate into durable gains -and whether their effects extend across generations- is central to understanding the developmental role of education in middle-income contexts.

A small but growing set of studies has used this reform to identify causal impacts in specific domains. Close in scope is Leon Bravo 2025, a working paper that uses a regression discontinuity design to estimate effects on school attendance and employment, finding gains in the former but no significant impacts on labour market outcomes. Other studies have disentangled effects on cognitive health (Ma et al., 2021), psychosocial well-being (Gutierrez et al., 2024) and crime (Gleditsch et al., 2022).[3]

While these Mexico-based studies underline the reform's wider relevance, they focus on single outcomes, much like the broader compulsory schooling literature, which often centres on earnings or isolated demographic measures (Black & Devereux, 2011; Holmlund et al., 2011; Oreopoulos, 2006). In contrast, this paper follows a single reform across education, fertility, labour, migration, and intergenerational outcomes. This broad life-course perspective is less common but resonates with other studies that trace a unified shock across multiple domains and generations (Bandiera et al., 2020; Bell et al., 2023; Oreopoulos & Salvanes, 2011) and in doing

[1] The literature on education's long-run effects is extensive and diverse. Key strands include evaluations of major reforms such as compulsory schooling mandates (Cornelissen & Dang, 2022; Cygan-Rehm & Maeder, 2013; Machin et al., 2012); school construction (Akresh et al., 2023; Duflo, 2001, 2004); school year length (Agüero & Beleche, 2013; Pischke, 2007); school expansion (Dinerstein & Smith, 2021); and public education spending (Andrabi et al., 2024). These citations are illustrative rather than exhaustive, situating the present study within a broad comparative literature on education reforms.

[2] For studies on educational impacts across life domains, see: labour market outcomes (Becker & Chiswick, 1966; Braga, 2018; Psacharopoulos & Patrinos, 2018); fertility (Chen & Guo, 2022; Fort et al., 2016a; Kampelmann et al., 2018; Keats, 2018; McCrary & Royer, 2011); migration (Aydemir et al., 2022a; Bandiera et al., 2019); intergenerational mobility (Alesina et al., 2021; Arendt et al., 2021; Black et al., 2005; Black & Devereux, 2011; Holmlund et al., 2011; Restuccia & Urrutia, 2004); crime (Baron et al., 2024; Bell et al., 2022; Huttunen et al., 2023); and health (Clark & Royer, 2013). These citations are illustrative rather than exhaustive, positioning this study within the evidence on education's effects across life domains.

[3] Ma et al., 2021 examine whether children's education influences parents' cognitive health, while Gutierrez et al., 2024 analyse effects on parental psychosocial well-being. In a separate line of work, (Gleditsch et al., 2022) show that increased attendance in secondary and tertiary education reduces homicide rates, highlighting the potential for educational access to shape broader societal outcomes.

so, provides the first evidence from a large middle-income country showing how compulsory schooling reforms can reshape life trajectories across life stages and between generations.

This study examines the long-term consequences of the reform by comparing individuals born around the eligibility cutoff, observed in adulthood using 2020 census data. This design enables the analysis of a comprehensive array of long-term outcomes, including their intergenerational transmission. This intergenerational perspective is central to theories of human capital transmission (Becker & Tomes, 1979; Black & Devereux, 2011), which posit that parental schooling shapes children's outcomes through cognitive, behavioural, and resource-based channels.

In Mexico, where inequality and limited mobility remain pervasive (Campos-Vázquez et al., 2021; CEEY, 2025; Grajales & Monroy-Gómez-Franco, 2017; Hertz et al., 2008; Neidhöfer et al., 2018), these intergenerational effects are especially consequential, with the potential either to entrench persistent disadvantage or to foster upward mobility. This motivates the study's focus on both direct and intergenerational impacts.

The analysis draws on rich microdata from the 2020 Mexican Population and Housing Census, which provide detailed demographic, educational, and labour market information and allow the identification of intergenerational outcomes by linking parents to co-residing children. The census scale supports precise estimation and heterogeneity analysis by gender, Indigenous status, and locality size.

Using this data, the empirical strategy follows a two-stage approach. First, a regression discontinuity design (RDD) estimates the impact of the reform on educational attainment by exploiting the eligibility cutoff at birth year. Under standard continuity assumptions, supported by balance tests and robustness to narrower bandwidths, comparisons near the cutoff yield causal estimates of the reform's impact. Second, the reform serves as an instrumental variable (IV) for years of schooling to estimate the causal effect of education on broader outcomes -including fertility, employment, migration, and intergenerational indicators. This unified design follows a single causal shock across multiple domains, providing a comprehensive view of how expanded schooling reshapes trajectories and transmits advantages across generations. These approaches identify local average treatment effect (LATE) for compliers -individuals whose attainment increased because of the reform- and should be interpreted as net causal effects of education rather than decompositions into specific mechanisms.

The findings show that exposure to the reform led to substantial gains in educational attainment, particularly in secondary and upper-secondary completion. These gains translated into delayed fertility, improved employment, and greater geographical mobility. Intergenerationally, children of affected individuals were more likely to be enrolled in school and achieve literacy. Effects are directionally consistent across subgroups, with the largest gains among rural and Indigenous populations.

This paper makes three contributions. First, it provides new causal evidence on the long-term and intergenerational impacts of compulsory schooling in a large middle-income country, exploiting a sharp regression discontinuity and instrumental-variable design applied to census data. Second, it traces a single reform across demographic, labour-market, and intergenerational domains, offering a unified life-course perspective that complements studies focused on mechanisms of specific outcomes. Third, it documents marked heterogeneity in effects, with the greatest gains among Indigenous and rural populations, showing how education policy can reduce persistent structural inequalities.

The remainder of the paper is organised as follows. Section 2 describes the institutional context and data. Section 3 outlines the empirical strategy and results. Section 4 concludes.

II. Institutional Background and Data

In 1993, Mexico enacted a major reform to its General Education Law, extending compulsory schooling from six to nine years and making lower secondary education (grades 7–9) mandatory nationwide. The new requirement applied to all children younger than 12 at the start of the 1993 school year (September), creating a sharp birth-cohort eligibility cutoff that underpins the empirical strategy.

The reform responded to persistent gaps in educational attainment despite large gains in basic education. Throughout the 20th century, education policy in Mexico prioritised the reduction of illiteracy and the expansion of access to primary education. Literacy rates rose from 46.2% among individuals born in 1930 (measured in 2020) to 93% among those born around the reform cutoff, and 98.3% for the 2008 birth cohort. Yet lower-secondary school completion remained persistently low: only 4% of the 1930 cohort completed nine or more years of schooling, and over 40% of individuals born in the early 1980s still failed to do so.

The reform was implemented simultaneously across all Mexican states. While it did not mandate sanctions for non-compliance and uptake was imperfect, estimates suggest that nearly half of the observed increase in secondary and tertiary enrolment between 1992 and 2000 occurred within the first three years of implementation (Gleditsch et al., 2022). Compliance and uptake likely varied by geography and school infrastructure. Nonetheless, the eligibility cutoff generated a large and statistically precise increase in completed schooling among eligible cohorts. Imperfect compliance does not undermine the validity of the empirical design, which identifies a local average treatment effect (LATE) for those induced to complete schooling by the reform.

Figure 1. Discontinuity in Educational Attainment at the Reform Cutoff

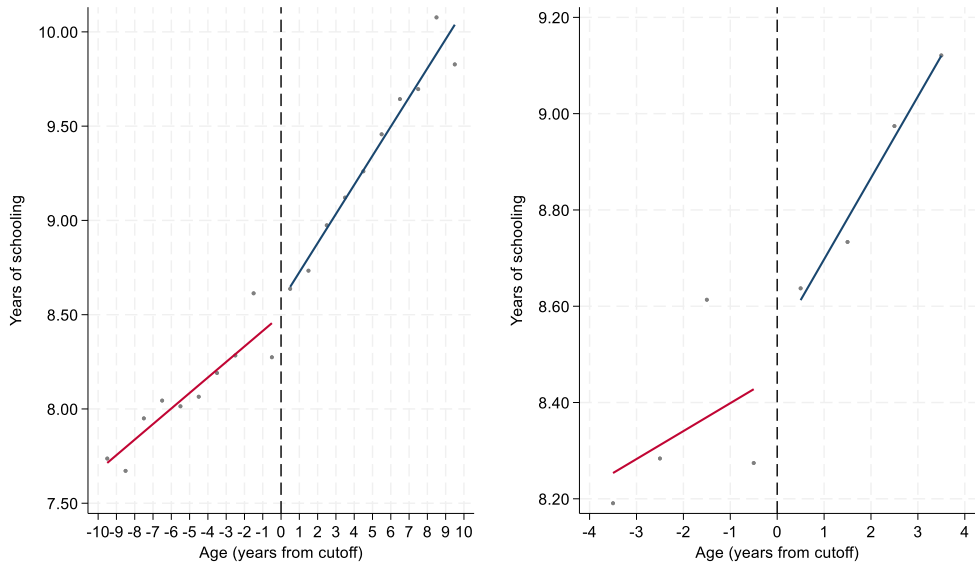


Figure 1 plots cohort averages of completed years of schooling against year of birth. Each dot is the mean for a single birth cohort; fitted lines represent linear trends on either side of the 1981 eligibility cutoff. The vertical line marks the eligibility threshold.

In the analysis, these concerns about heterogeneous implementation are addressed explicitly by incorporating municipality fixed effects and controls for locality size, absorbing time-invariant differences in educational and economic conditions. The regression discontinuity design further limits such concerns by focusing on narrow, symmetric birth cohorts around the eligibility cutoff.

Baseline estimates use a ±3-year window, with results robust to narrower bandwidths such as ±1 year.

This discontinuity is visible in Figure 1, which shows cohort averages of completed years of schooling by year of birth. The pattern reveals a sharp discontinuity at the 1981 cutoff, consistent with a level shift in attainment and a steeper post-cutoff trend, supporting the use of a binary indicator (=1 if born after 1980) as the RDD treatment and IV instrument.

Data

The analysis uses microdata from the 2020 Mexican Population and Housing Census, covering approximately 126 million residents. The census provides detailed demographic, educational, labour market, and migration information, as well as household composition and housing characteristics. All estimates are weighted using census expansion factors to ensure national representativeness.

The running variable for the regression discontinuity design is year of birth. Treatment status equals 1 for individuals younger than 12 at the start of the 1993 school year (born in 1981 or later) and 0 otherwise. Educational attainment measures include literacy (ability to read and write), completed years of schooling, and binary indicators for lower-secondary completion (≥9 years) and upper-secondary completion (≥12 years). Family formation outcomes comprise number of children ever born, an indicator for having no children, and child mortality (share of children ever born who are no longer living). Labour market outcomes include employment status (currently working for pay), weekly hours worked, and sector of employment (agriculture, industry, services). While earnings data suffer from high non-response, employment and sectoral shifts are reliably observable. Migration indicators capture whether respondents reside in the same state or municipality as their place of birth, and whether they have remained in the same location as five years prior.

Table 1. Descriptive statistics of population born adjacent to the reform cutoff

Sample	Full sample	Males	Females	Pre reform	Post reform
Birth years	(1978-83)	(1978-83)	(1978-83)	(1978-80)	(1981-83)
Sample size	1,164,785	554,473	610,312	591,528	573,257
Population	10,430,117	4,996,261	5,433,856	5,337,543	5,092,574
Literacy (%)	96.81	97	96.62	96.52	97.11
Lower Secondary completion (%)	74.28	73.94	74.6	72.66	75.98
Upper secondary completion (%)	41.1	41.56	40.68	39.7	42.57
Indigenous language (%)	6.61	6.78	6.47	6.51	6.73
Female (%)	52.1	.	.	52.13	52.07
No children (%)	.	.	12.32	.	.
Child mortality (%)	.	.	5.46	.	.
Number of children	.	.	2.61	.	.
Locality <2,500	18.72	18.93	18.54	18.54	18.92
Locality 2,500 -14,999	14.95	14.79	15.09	14.83	15.07
Locality 15,000 - 99,999	16.29	16.13	16.43	16.15	16.43
Locality ≥ 100,000	50.04	50.15	49.94	50.48	49.58

Notes: Sample includes individuals born within ±3 years of the 1981 eligibility cutoff (1978–1983) observed in the 2020 Mexican Census. "Sample size" is unweighted; "Population" applies census weights. "Literacy" = % able to read and write. "Lower secondary" = ≥9 years schooling; "Upper secondary" = ≥12 years. "No children" = % childless; "Number of children" = mean among parents; "Child mortality" = % of children ever born who are deceased. Family formation statistics estimated for female parents only. "Indigenous language" = % speaking an Indigenous language in addition to Spanish. Locality categories reflect population size.

To examine intergenerational impacts, adults are linked to co-residing children aged ≤18 using household identifiers and parent-child relationship codes, thereby reducing selection bias from household composition changes arising from older children leaving the parental household. This restriction enhances internal validity, though estimates could be influenced if leaving-home patterns vary systematically with parental education. Children's outcomes are measured as literacy and school enrolment across age brackets corresponding to primary (ages 6–12), lower secondary (13–15), upper secondary (16–18), and higher education (19–20).

The baseline adult sample includes individuals born within ±3 years of the 1981 eligibility cutoff (cohorts 1978–1983) observed in 2020, with robustness checks using narrower windows. Table 1 reports descriptive statistics for the full adult sample, shown for the pooled sample and separately for pre- and post-reform cohorts, and by gender. The sample includes 1.16 million individuals: 52.1% are female; 6.6% speak an Indigenous language in addition to Spanish; 97% are literate; and 74% have completed secondary education. On average, 6.4% are childless; among parents, the mean number of children is 2.61, and the mean child mortality rate is 2.85%.

The analysis is conducted on the full sample and then partitioned across gender, Indigenous identity, and locality type. Localities are classified into four categories: small rural (<2,500 residents), rural (2,500–14,999), small urban (15,000–99,999), and large urban (≥100,000), capturing structural differences in school access and service provision. While IV estimates identify local average treatment effects, analysing subsamples provides insight into how impacts vary across groups and helps assess whether heterogeneous implementation shaped the magnitude of estimated effects.

Table 2. Descriptive statistics of children of parents born adjacent to the reform cutoff

Parent	Panel A: Father-child pairs		Panel B: Mother-child pairs	
Birth years	1978-80 (Pre)	1981-83 (Post)	1978-80 (Pre)	1981-83 (Post)
Sample size	227,494	145,877	296,719	216,747
Population	1,728,700	1,070,960	2,358,718	1,613,922
Female (%)	47.27	47.54	47.78	48.45
Indigenous language (%)	6.22	6.77	5.04	5.92
Literacy (%)	99.11	99.17	99.05	99.16
Lower Secondary school (%)	63	69.3	61.73	68.99
Employed (%)	4.52	5.22	4.6	5.24
Works in agriculture (%)	45.23	41.53	40.76	38.15
Locality <2,500	24.48	26.09	22.05	24.78
Locality 2,500 -14,999	17.18	17.21	16.65	17.15
Locality 15,000 - 99,999	16.16	16.67	16.88	16.88
Locality ≥ 100,000	42.19	40.03	44.43	41.19

Notes: Intergenerational sample links adults born within ±3 years of the 1981 cutoff (1978–1983) to co-residing children aged 6–20 using census household identifiers (aged 13-15 for employment statistics). Panel A reports father–child pairs; Panel B reports mother–child pairs. "Sample size" is unweighted; "Population" applies census weights. "Literacy" = % able to read and write. "Lower secondary" = ≥9 years schooling. "Employed" = % of children aged 13–15 in paid work; "Works in agriculture" = % of employed children in agriculture. "Indigenous language" = % speaking an Indigenous language. Locality categories reflect population size.

Table 2 reports descriptive statistics for the intergenerational sample, distinguishing between father-child and mother-child pairs to examine differences in educational transmission. The intergenerational sample comprises 372,000 father-child pairs and 508,000 mother-child pairs. Among children, 47-48% are female and 5-7% speak an Indigenous language. Over 99% are literate, and 62-69% are enrolled in secondary school. Among those aged 13-15, 4-5% are in paid employment, with 38-45% of these working in agriculture, reflecting the rural concentration of

early labour participation. The geographical distribution mirrors that of the adult sample, with the majority residing in large urban localities.

III. Methodology: Empirical strategy

This study adopts a two-stage empirical strategy. First, it exploits the discontinuity in eligibility for the education reform to estimate the effect of compulsory schooling on educational attainment. Second, reform eligibility is used as an instrumental variable to identify the causal impact of schooling on adult and intergenerational outcomes. The large census sample allows for precise estimation and extensive robustness analysis, including fixed effects and subgroup heterogeneity.

The identification strategy rests on the 1993 reform to Mexico's General Education Law, which raised mandatory schooling from six to nine years. This policy created an age-based discontinuity: individuals born after 1980 were subject to the new compulsory schooling law, while those born before were not. Treatment assignment is thus deterministic and cannot be manipulated ex post. The main treatment variable, $Reform_i$, is defined as an indicator equal to 1 if individual i was younger than 12 in 1993:

$$\text{Reform}_\text{i} = \begin{cases} 1 \text{ if age} < 12 \text{ in } 1993 \\ 0 \text{ otherwise} \end{cases}$$

The baseline regression discontinuity (RDD) specification is:

$$\text{Y}_\text{i} = \alpha + \tau\text{Reform}_\text{i} + \beta_1 \text{a}_\text{i} + \beta_2(\text{Reform}_\text{i} * \text{a}_\text{i}) + \Omega\text{X}_\text{i} + \mu + \epsilon_\text{i} \ldots (1)$$

where Y_i denotes the outcome for individual i in municipality m, Reform_i is the treatment indicator, a_i, is year of birth centred at the cut-off, and X_i are sex and locality size controls. The piecewise linear specification allows for different pre- and post-cutoff slopes, accommodating secular trends. Municipality fixed effects[4] (μ) absorb time-invariant local heterogeneity. The model is estimated using survey weights, with standard errors clustered at the municipality level. The estimation window includes individuals aged 37–42 in 2020, providing a symmetric bandwidth around the cutoff. Under standard continuity assumptions τ captures the causal effect of exposure to the reform on outcomes near the cutoff.

The credibility of the RDD rests on two conditions: (1) absent the reform, potential outcomes would have evolved smoothly across the cutoff and (2) the running variable is not manipulated. Diagnostics in Appendix Table A1 report covariate balance tests focusing on various characteristics, showing no statistically significant discontinuities in gender, Indigenous status, locality type, household size, water access, or remittance receipt. Appendix Table A2 documents the distribution of birth cohorts around the cutoff, showing smooth population changes around the 1981 threshold. Taken together, the absence of discontinuities in predetermined characteristics and the smooth distribution of birth cohorts provide no evidence of sorting or differential composition at the cutoff, supporting the continuity assumption underpinning the RDD.

In the second stage, the reform serves as an instrument for completed years of schooling in a two-stage least squares (2SLS) framework.

[4] These account for geographic and structural confounders, including features such as local education infrastructure, labour market structure, historical migration patterns, fertility norms, long-standing economic development, language use, and differential access to services across rural and urban areas.

First stage:

$$\text{Schooling}_i = \alpha_0 + \lambda \text{Reform}_i + \Gamma X_i + \mu + \xi_i \ldots (2)$$

Second stage:

$$y_i = \alpha_1 + \rho \widehat{\text{Schooling}}_i + \Phi X_i + \mu + \eta_i \ldots (3)$$

Both stages include controls (X_i), and municipality fixed effects (μ). Standard errors are clustered at the municipality level, and survey weights are applied throughout to preserve national representativeness. The first-stage F-statistic exceeds conventional weak-instrument thresholds, ensuring strong relevance (see Appendix Tables A3, A4 and A5). This approach follows a well-established literature using schooling reforms as instruments (e.g. Begerow & Jürges, 2022; Lleras-Muney, 2005; Oreopoulos, 2006)[5], and identifies the LATE: the mean effect of an additional year of schooling for individuals whose educational attainment increased due to the reform.

For completeness, the analysis also reports reduced-form regressions of reform eligibility on outcomes, which provide the intention-to-treat effects of the policy. These reduced-form estimates (reported in Appendix Tables A19–A22 for conciseness) mirror the IV specifications in terms of controls, fixed effects, and weighting, and show effects that are directionally consistent and of magnitudes that align with the IV estimates.

Identification requires that, conditional on the controls, the reform affects the outcomes of interest solely through its impact on education, and that potential outcomes would have evolved smoothly across the cutoff had the reform not been implemented. While the exclusion restriction is likely to hold for most outcomes, potential indirect effects (such as peer spillovers, cohort dynamics, or concurrent institutional changes) are discussed.

The framework is applied to both direct and intergenerational outcomes. In both cases the main explanatory variable, Schooling_i, refers to parental education. In the first case, Y_i represents adult outcomes such as fertility, employment, and migration patterns. In the latter case, Y_i captures child literacy and school enrolment, where the coefficient, ρ, is interpreted as the change in the conditional probability of the outcome with an additional year of parental education, and therefore reflects the transmission of human capital across generations.

While the empirical strategy identifies the causal effect of additional schooling on long-term and intergenerational outcomes, it does not disentangle the specific channels through which these effects operate. Schooling is a multi-dimensional treatment: it may influence fertility, employment, migration, and intergenerational mobility through improved cognitive and non-cognitive skills (Card, 1999; Oreopoulos & Salvanes, 2011), shifts in preferences and intra-household allocations (Currie & Moretti, 2003; Cygan-Rehm & Maeder, 2013), or changes in opportunity costs and behavioural constraints (Bell et al., 2022; Machin et al., 2011). Although the design does not allow for a formal decomposition, the results provide credible evidence that large-scale compulsory schooling reforms can alter life trajectories in ways consistent with these mechanisms. The paper contributes to a broader literature on the long-run and intergenerational returns to education (e.g. Attanasio et al., 2022; Björklund & Salvanes, 2011; Black et al., 2005; Black & Devereux, 2011; Heckman & Mosso, 2014; Holmlund et al., 2011; Oreopoulos & Salvanes, 2011).

[5] The baseline model omits flexible age trends to avoid overfitting, preserve a clear interpretation of the instrument's effect and maintain comparability with prior work. Nonetheless, robustness checks introduce alternative specifications that include controls for age.

The next section presents the empirical results, beginning with impacts on educational attainment and then extending to adult and intergenerational outcomes.

IV. Results

This section presents the main empirical findings, proceeding sequentially from the most immediate effects of the reform on schooling to its downstream impacts on fertility, child mortality, labour market participation, sectoral allocation, and geographic mobility. The final part examines intergenerational spillovers, assessing whether the parental education gains induced by the reform improved children's schooling outcomes. Across all outcomes, estimates are interpreted as local average treatment effects for individuals whose schooling increased due to exposure to the reform. The section is structured to show how the reform's educational gains translate into broader life-course changes and, ultimately, intergenerational benefits.

Adult Outcomes

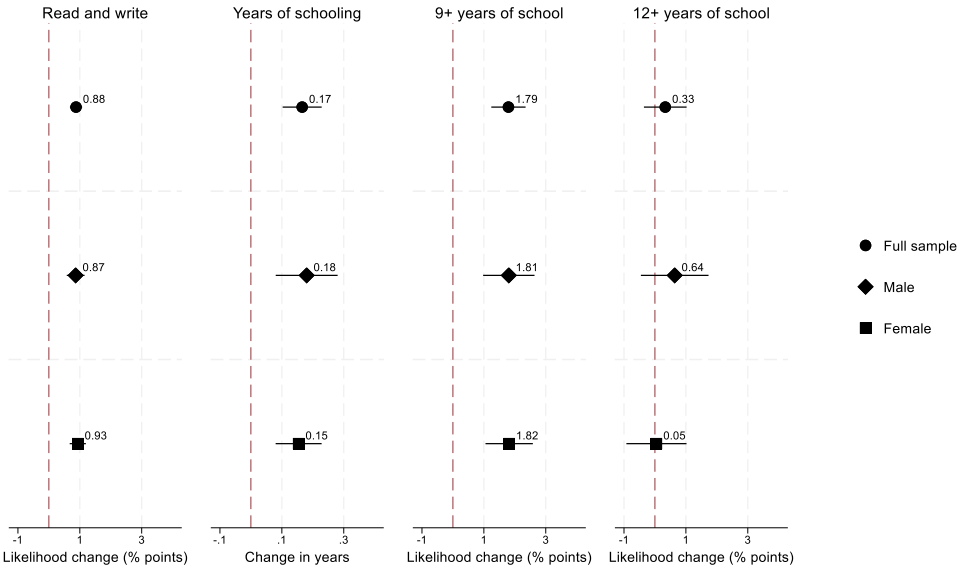
The reform produced sizeable improvements in educational attainment, the primary target of the policy (Figure 2). For the full sample, individuals born just after the cutoff were 0.88 percentage points more likely to be literate (baseline: 93%), completed 0.17 additional years of schooling (baseline: 8.57), and were 1.79 percentage points more likely to finish lower secondary school (baseline: 59%). These gains are broadly similar for males and females, and directionally consistent across locality types, with larger magnitudes in rural areas (see Appendix Table A6). Significant increases in upper secondary completion appear only in small rural localities and among Indigenous groups. The overall effects are especially pronounced among Indigenous individuals, literacy increased by 4.52 percentage points (baseline: 78%), average schooling by 0.50 years (baseline: 5.65), and completion rates for lower and upper secondary school by 3.77 (baseline: 29%) and 1.91 percentage points (baseline: 10%), respectively. Although moderate in the aggregate, the effects are substantially larger among disadvantaged subgroups, suggesting that the reform disproportionately benefited populations with lower initial attainment and thereby helped narrow educational gaps. Results are robust to narrower bandwidths (±1 year; see Appendix Figure A1).[6]

Beyond attainment, additional schooling reshaped family formation through both fertility decisions and child survival (Figure 3). Among the full sample of women, IV estimates indicate that an additional year of schooling reduced the number of children by 0.28 (baseline: 2.68) and increased the probability of having no children by 2.17 percentage points (baseline: 10%). These effects vary by subgroup: the fertility-reducing effect is strongest among Indigenous women and those in rural areas (-0.4 and -0.33 fewer children ; baseline averages 3.49 and 3.08, respectively), whereas the rise in childlessness is most pronounced in large urban localities (+4.26 percentage points, from baseline 14%). Child survival also improves: the child mortality rate -the proportion of children born who are no longer living- declines significantly across all groups, with relatively modest heterogeneity in magnitudes (see Table A7 and A8). Taken together, the estimates

[6] Reduced-form estimates of reform eligibility on outcomes are reported in Appendix Tables A19–A22. These estimates show directionally consistent and comparable magnitudes, supporting the IV interpretation

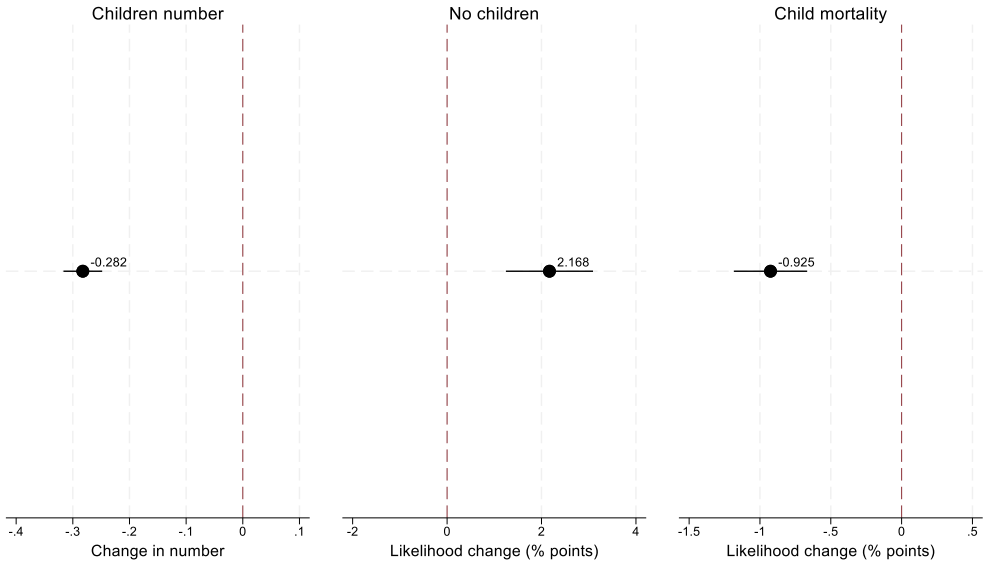
demonstrate that increased schooling translated into smaller family sizes and higher child survival, with the largest gains concentrated among disadvantaged groups.

Figure 2. Estimated Impact of the 1993 Schooling Reform on Educational Attainment



Note: Each panel plots regression discontinuity estimates of the effect of the reform on educational outcomes. Dependent variable: (a) =1 if knows how to read and write, =0 otherwise; (b) number of years of completed education (ranges 0 to 18); (c) =1 if has completed at least nine years of schooling, =0 otherwise; (d) =1 if has completed at least 12 years of schooling, =0 otherwise. Independent variable =1 if individual aged 37 to 39, =0 if aged 40 to 42. The models control for municipality fixed effects, locality size and sex. Standard errors are clustered at a municipality level. Confidence intervals at a 95% level.

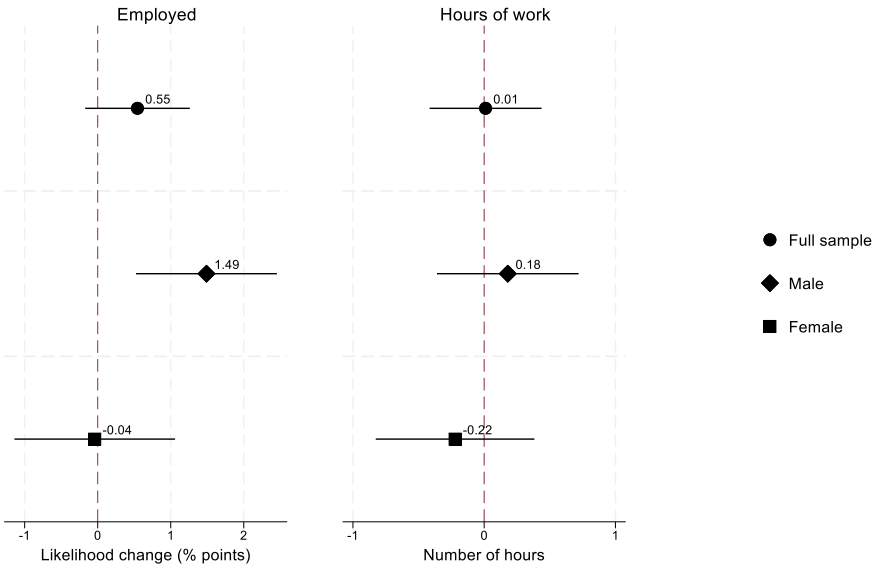
Figure 3. Estimated Effects of Education on Fertility and Child Mortality



Note: Each panel presents instrumental variable estimates of the effect of completed years of education (0–18) on the probability of employment in a specific sector: agriculture, manufacturing, construction, education, and health. The dependent variable in each case is (1) number of children living in household; (2) a binary indicator equal to 1 if the individual reports no children ever born, and 0 otherwise; (3) percentage of children born no longer living. All models include municipality fixed effects and control for sex and locality size. Standard errors are clustered at the municipality level. 95% confidence intervals shown.

The link between schooling and labour market outcomes is explored in Figures 4 and 5. Average effects on employment and hours worked are particularly sizeable and precisely estimated for specific groups. The largest gains are observed among Indigenous individuals, for whom an additional year of schooling increased the probability of employment by 5.5 percentage points (baseline: 41%) and weekly hours worked by 1.16 (baseline: 39.7). Gains also appear for males, with no subgroup showing reduced employment (see Appendix Tables A9 and A10).

Figure 4. Estimated Effects of Education on Employment and Working Hours

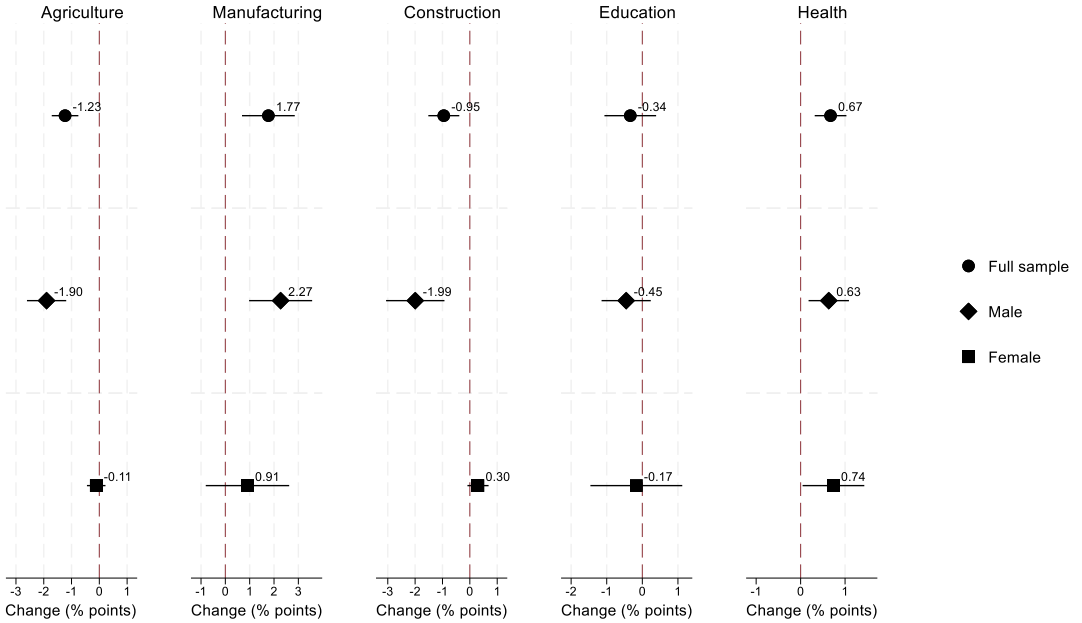


Note: Each panel reports instrumental variable estimates of the effect of completed years of education (0–18) on (a) the probability of being employed (defined as reporting any paid work), and (b) total weekly hours worked among those employed. Estimates are shown for the full sample of individuals aged 37–42, and for subgroups defined by sex. All models include municipality fixed effects and control for sex and locality size. Standard errors are clustered at the municipality level. 95% confidence intervals shown.

Schooling also reshaped sectoral allocation. The most consistent reallocation is a shift out of agriculture: the probability of working in agriculture falls by 1.2 percentage points (baseline: 17%), with larger declines among rural (-2.9%, baseline: 34%) and Indigenous populations (-2.8%, baseline: 39%), where agriculture accounts for a large share of jobs.[7] This shift was partly offset by increases in manufacturing and services. Manufacturing gains are concentrated among males and residents of smaller localities, while in services, employment expanded most clearly in health (+0.67 percentage points, baseline: 3%) and, to a lesser extent, in education. Construction exhibits mixed patterns, declining for males and urban residents but increasing for Indigenous workers (Appendix Tables A11–A12). Overall, the results indicate that the reform facilitated a structural reallocation away from low-productivity agriculture and into more diversified, skill-intensive sectors. The next section examines whether these educational gains also influenced patterns of geographical mobility.

[7] De la Fuente Stevens & Pelkonen, 2023, show that while agriculture accounts for roughly 10% of employment among working-age adults (25–64) nationwide, the share among Indigenous populations is markedly higher, varying between 22% and 82% across groups.

Figure 5. Estimated Effects of Education on Sector of Employment



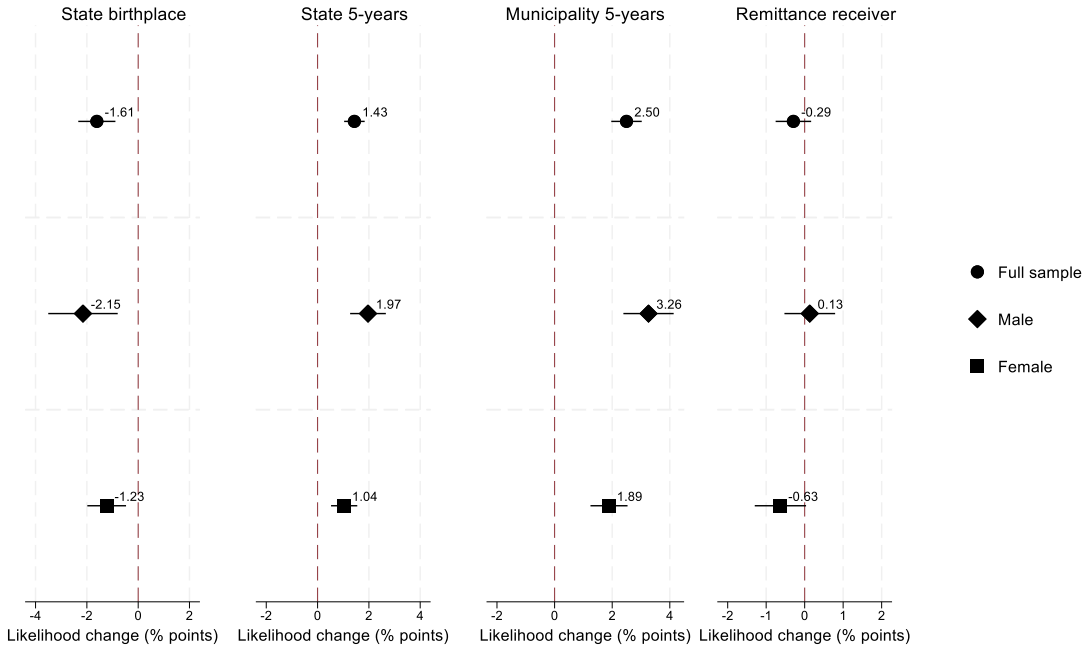
Note: Each panel reports instrumental variable estimates of the effect of completed years of education (0–18) on sectoral employment. In this model the dependent variables are equal to 1 if the observed individual has paid work in one of these sectors, and equal to zero is employed in another one. Estimates are therefore expressed as percentage point changes in the likelihood of the outcome. The model is estimated for the full sample of individuals aged 37–42, disaggregated by sex. The model is estimated with municipality fixed effects and controls locality size and sex. Standard errors are clustered at a municipality level. Confidence intervals at a 95% level.

Education also reshaped mobility patterns (Figure 6). Long-term migration -living outside one's state of birth- fell by 1.61 percentage points (baseline: 13%), with larger declines for males (–2.15, baseline: 13%) and urban residents (–3.19, baseline 28%). In contrast, recent mobility increased. The likelihood of moving across state or municipality boundaries in the preceding five years rose by 1.43 and 2.5 percentage points, respectively, in the full sample, a pattern that holds across subgroups (see Appendix Tables A13 and A14). This suggests that education facilitated more adaptive migration, in ways consistent with responsiveness to labour-market opportunities. The evidence points to a shift in the composition of mobility: reduced reliance on permanent moves and greater responsiveness to short-term opportunities.

Regarding remittances, the probability of living in a remittance-receiving household declined with schooling, with significant effects concentrated in rural areas (-0.79 percentage points, baseline:10%), where remittance reliance is highest.[8] Interpretation should be cautious since remittance receipt is measured at the household level and may reflect transfers to other household members rather than the respondent. Taken together, the findings indicate that rising education reduced exposure to international migration networks while fostering more adaptive patterns of internal mobility.

[8] This pattern aligns with broader evidence that remittance dependence is more prevalent in smaller municipalities than in larger urban centres (De la Fuente Stevens, 2024).

Figure 6. Estimated Effects of Education on Migration and Remittances



Note: Each panel reports instrumental variable estimates of the effect of completed years of education (0–18) on migration and remittance outcomes. State birthplace indicates whether an individual resides in a state different from their state of birth; State 5-years and Municipality 5-years indicate whether the individual moved across state or municipality boundaries in the five years preceding the survey; Remittance receiver equals 1 if the household receives income from abroad. All models include municipality fixed effects and controls for locality size and sex. Standard errors are clustered at the municipality level. Confidence intervals at the 95% level.

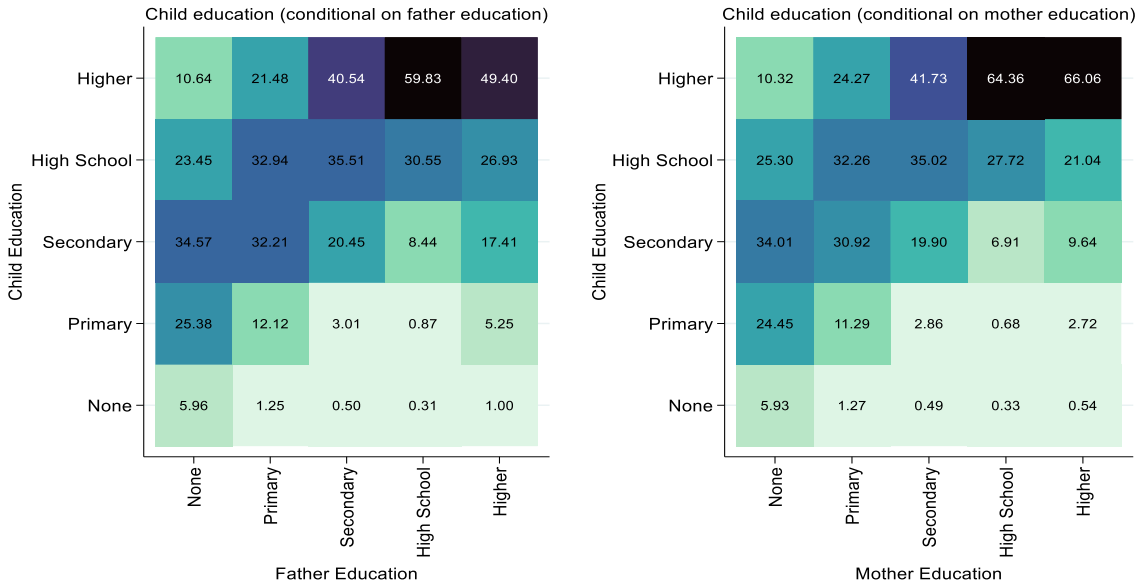
Intergenerational Outcomes

The most enduring benefits of education may materialise across generations. Figure 7 presents mobility matrices, reporting the distribution of child education conditional on parental education. This figure shows strong associations between parental and child schooling. For parents with no education, around 10% (fathers) or 10.3% (mothers) of children attain higher education (more than 12 years of education). In contrast, among parents with higher education, the share rises to 49% for fathers and 66% for mothers. These descriptive gaps are substantial, but they may reflect selection and confounding rather than causal effects.

Instrumental variable estimates (Figure 8) provide causal evidence. An additional year of parental schooling increased child secondary school enrolment by 3-4 percentage points (baseline: 81.6%) and upper secondary enrolment by 5-8 points, equivalent to a roughly 10-14% increase over baseline. Effects on literacy and primary enrolment are modest and statistically significant only for father-child pairs (0.59 percentage points, baseline 97.7%), while higher education enrolment shows no significant change. The broadly consistent direction of effects across mothers and fathers reinforces the conclusion that parental education positively influences children's schooling. Effects are especially pronounced for families in small rural localities where baseline attainment is lowest (Appendix Tables A14 -A18). These patterns align with the LATE interpretation: the reform affected parents at the lower end of the education distribution, whose children remain below the thresholds where tertiary enrolment transmission is strongest.

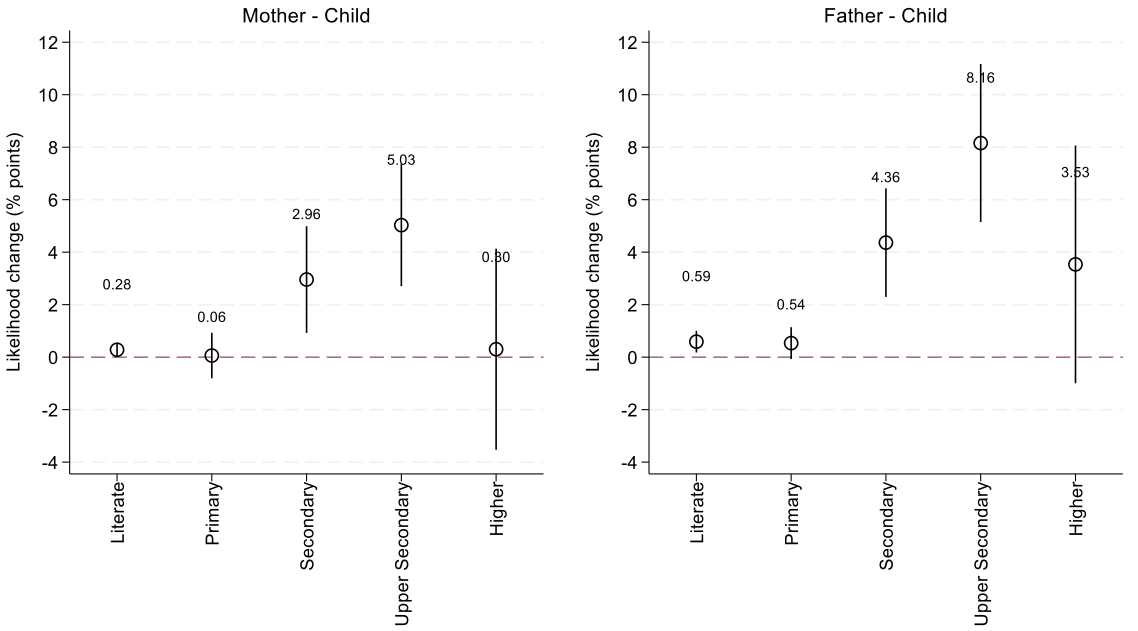
Consequently, the estimates for upper levels of education may be interpreted as conservative estimates of intergenerational transmission of education.

Figure 7. Intergenerational Mobility: Children's Education Conditional on Parental Education



Note: The sample restricts parents to those aged 45–60 who had their children after age 25. Children are required to be aged 20-25. Each column displays the distribution of educational attainment among children, conditional on the parent's education level. Percentages sum to 100% within each column.

Figure 8. Intergenerational Effects of Parental Education on Child Literacy and School Enrolment



Note: Each panel reports instrumental variable estimates of the effect of parental education (0–18 years) on child literacy and school enrolment. The left panel shows mother–child estimates; the right, father–child estimates. Outcomes include literacy (children aged 9–15), and school enrolment at the primary (ages 6–12), secondary (13–15), upper secondary (16–18), and higher education levels (19–20). All models control for child age, sex, single-parent

household status, parental age at birth, and locality size, and include municipality fixed effects. Standard errors are clustered at the municipality level. 95% confidence intervals shown.

Taken together, the results trace a coherent trajectory, demonstrating that compulsory schooling reforms not only improved adult outcomes but also enhanced children's schooling, with the largest gains concentrated among disadvantaged groups -particularly Indigenous and rural populations. This intergenerational reach emphasizes the potential of education policy to reduce persistent inequalities. By extending the evidence on compulsory schooling reforms to a highly unequal, middle-income setting, the analysis shows that even moderate gains in attainment can generate sustained and intergenerational benefits. The findings highlight the role of education policy as a central lever for reducing structural inequalities in unequal societies.

While the analysis does not isolate specific mechanisms, the results should be interpreted as net causal effects of education across multiple life outcomes. The patterns align with channels highlighted in prior work, such as higher opportunity costs of early fertility, shifts in preferences over family formation, and greater household investment in children's human capital, but the design identifies the overall impact of expanded schooling rather than decomposing individual pathways.

V. Discussion

The 1993 reform extending compulsory schooling in Mexico from six to nine years produced lasting increases in educational attainment that reverberated across adult and intergenerational domains. Exploiting the age-based discontinuity in exposure and using the reform as an instrument for years of schooling, the estimates identify the LATE for individuals whose education rose because of the reform. On average, the induced gains in schooling were moderate on average but large for disadvantaged groups, particularly among Indigenous and rural populations. This pattern points to convergence in attainment, with the reform disproportionately benefiting groups that had historically faced the steepest barriers to education. Even if moderate on average, these effects set in motion a wide set of downstream changes across demographic, labour-market, and intergenerational outcomes.

These education gains translated into clear demographic shifts. Fertility declined, with stronger effects for Indigenous and rural women, and the probability of remaining childless rose, particularly in urban areas. Child survival improved across all subgroups. Together, these patterns are consistent with mechanisms emphasised in the literature: education raises the opportunity cost of childbearing (Becker & Lewis, 1973; McCrary & Royer, 2011), shifts fertility preferences (Cygan-Rehm & Maeder, 2013; Fort et al., 2016), and expands household resources available for child health (Currie & Moretti, 2003).

Labour market outcomes moved in the direction of occupational upgrading. While aggregate employment effects were consistently estimated and larger gains were concentrated among disadvantaged groups, particularly Indigenous individuals, who experienced higher employment rates and longer working hours. Sectoral shifts were more pronounced: there was a clear reallocation out of agriculture, accompanied by gains in manufacturing, health, and education. These patterns are consistent with schooling raising access to higher-productivity sectors (Braga, 2018; Card, 1999), even if direct wage effects are not directly observable in the census data.

Geographic mobility responded in a nuanced way. Education reduced long-term displacement from the place of birth yet increased short-term internal migration. In rural areas, remittance receipt fell, consistent with reduced dependence on international migration networks. These

patterns point to a reallocation in the composition of mobility, consistent with evidence that schooling enhances responsiveness to labour-market opportunities (Aydemir et al., 2022b; Machin et al., 2012).

Intergenerational effects were sizeable for secondary and upper-secondary enrolment, especially in rural areas and for families starting from low educational baselines. Effects on tertiary enrolment were negligible, consistent with the LATE interpretation: compliers in this setting remain far from the thresholds where higher-education transmission is strongest. These patterns are consistent with canonical models of human capital transmission (Becker & Tomes, 1986; Black & Devereux, 2011), in which parental schooling raises children's attainment through cognitive, behavioural, and resource-based channels. Similar to findings in other middle-income settings (Cornelissen & Dang, 2022; Holmlund et al., 2011), the results suggest that incremental parental gains can translate into measurable improvements in children's schooling.

These findings connect to the large literature on compulsory schooling reforms but extend it to a middle-income setting characterised by high structural inequality and lower baseline attainment. The magnitudes are smaller than in many high-income studies but remain economically meaningful given weaker institutional environments and the larger share of disadvantaged populations. In this sense, the Mexican evidence complements work from both advanced economies and other emerging economies (e.g. Duflo, 2001; Banerjee et al., 2010), highlighting that the impacts of compulsory schooling reforms are shaped by initial inequalities in access to education and that such reforms can contribute to reducing long-standing educational gaps.

From a policy perspective, the results suggest that raising compulsory schooling can yield broad and lasting private and social returns -reducing fertility, improving child survival, promoting sectoral upgrading, and enhancing educational mobility across generations. However, the heterogeneity patterns highlight that gains were concentrated among those starting furthest behind, underlining the importance of complementary interventions. These could include investments in school quality, targeted support for Indigenous and rural communities, and measures to expand access to post-secondary education so that gains at the secondary level translate into further educational advancement. The mobility results also point to the role of local labour market conditions in shaping returns: without local demand for skilled labour, the potential of schooling reforms will be under-realised. Finally, by demonstrating multi-domain benefits beyond earnings, the findings strengthen the case for education policy as a central lever for advancing economic mobility and social inclusion in unequal societies.

VI. Conclusions

This paper shows that Mexico's 1993 expansion of compulsory schooling from six to nine years produced lasting gains in education, demographic outcomes, labour allocation, and intergenerational schooling. Using the reform as an instrument, the study finds the largest benefits for Indigenous and rural populations, reducing long-standing disparities. Education lowered fertility, improved child survival, and shifted workers towards higher-productivity sectors, with intergenerational spillovers raising secondary and upper-secondary enrolment among children.

Conceptually, the results highlight that in middle-income settings, education reshapes life trajectories and transmits advantages across generations. The heterogeneity patterns show that the largest gains arose among those starting from lower baselines, suggesting that schooling expansions can both raise overall attainment and reduce structural gaps.

From a policy perspective, the findings show that extending compulsory schooling can foster economic mobility and social inclusion. Future research could examine how complementary interventions -such as improvements in quality, local labour demand, or post-secondary access- may amplify these long-term and intergenerational returns.

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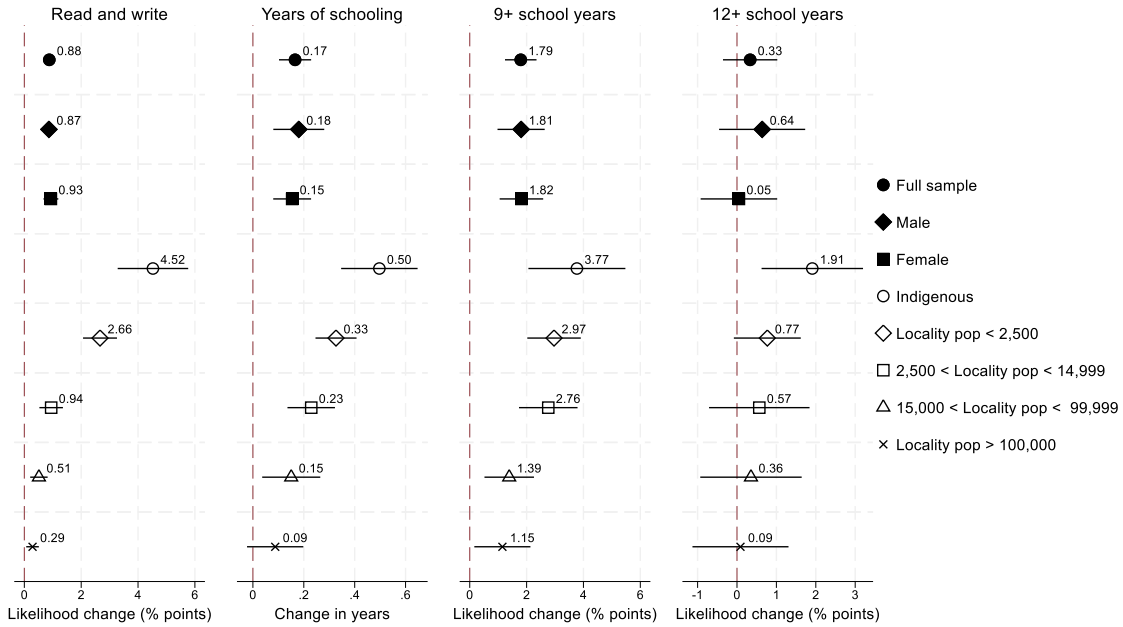
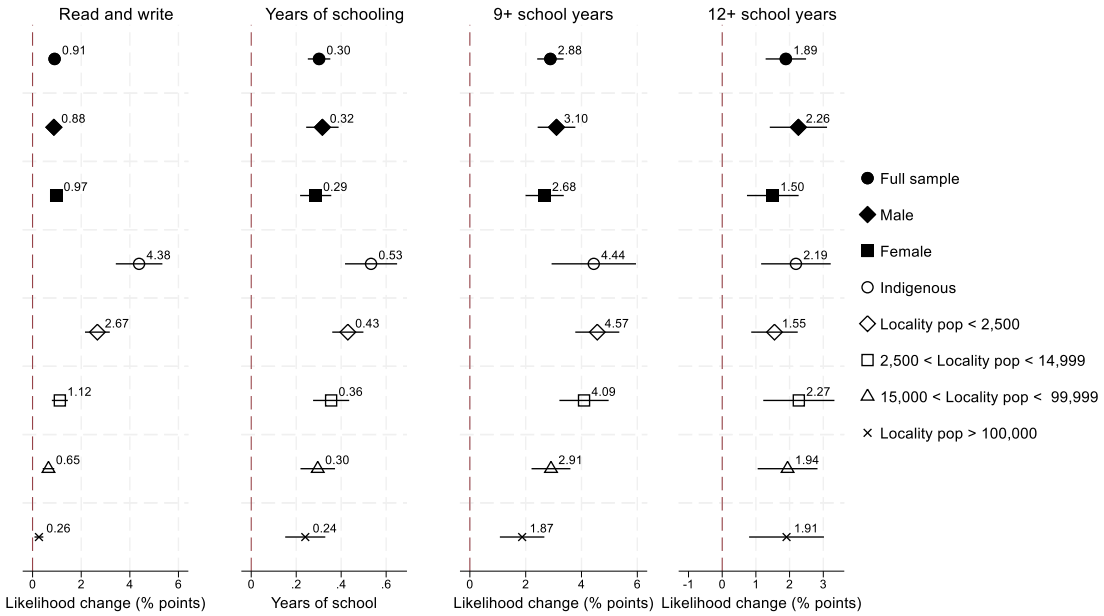
Appendix: Figures and Tables

Figure A1. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

Panel A: Bandwidth +- 3 years



Panel A: Bandwidth +- 1 years



Note: Each panel reports regression discontinuity estimates of the reform's impact on educational outcomes. Dependent variables: (a) literacy (=1 if the individual can read and write), (b) completed years of schooling (0–18), (c) completion of at least nine years of schooling, and (d) completion of at least twelve years. The treatment indicator equals 1 for individuals aged 37–39 and 0 for those aged 40–42. All regressions include municipality fixed effects, controls for locality size and sex, and cluster standard errors at the municipality level. Ninety-five percent confidence intervals shown.

Table A1: Covariate Balance around the RDD Cutoff

	Female	Indigenous	Rural	Household	Water	Remittance
Reform	-0.002	-0.000	0.000	-0.329	0.000	0.001
	0.004	0.001	0.000	0.256	0.001	0.002
Mean	0.524	0.192	0.688	44.535	0.932	0.084
N	1,164,785	1,164,602	1,164,785	1,164,785	1,164,659	1,163,718

Notes: The table tests for discontinuities in predetermined characteristics at the 1981 cutoff. "Female" is an indicator equal to 1 for women; "Indigenous" equals 1 if the respondent speaks an Indigenous language; "Rural" indicates residence in a locality with fewer than 15,000 inhabitants; "Household size" is the number of household members; "Water" equals 1 if the dwelling has access to piped or tanked water; "Remittance" equals 1 if the household reports receiving remittances. The treatment variable ("Reform") equals 1 if born after the cutoff. Standard errors are clustered at the municipality level. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$.

Table A2: Distribution of Birth Cohorts (1978–1983)

Age in 2020	Sample count	Sample share (%)	Population count	Population share (%)
37	178,216	15.30	1,580,683	15.15
38	211,363	18.15	1,889,063	18.11
39	183,678	15.77	1,622,828	15.56
40	239,543	20.57	2,146,745	20.58
41	143,419	12.31	1,305,221	12.51
42	208,566	17.91	1,885,577	18.08

Note: The table reports the distribution of individuals aged 37–42 in 2020, corresponding to birth cohorts 1978–1983. Columns compare sample counts with population totals from the 2020 Mexican Census.

Table A3 (part 1/3). First-Stage Relevance Tests for the Instrument: Adult outcomes

Dependent variable	Sample	F-statistic	Number of observations	Clusters
Theme: Fertility and Child mortality				
Number of children	Baseline (full sample)	316.11	607,983	2325
	Rural	154.49	167,406	1391
	Rural small	281.28	249,847	2278
	Urban small	89.26	74,223	121
	Urban small	283.96	116,494	449
	Indigenous	123.19	115,587	1506
Child mortality	Baseline (full sample)	212.95	544,170	2324
	Rural	153.01	150,455	1391
	Rural small	299.57	226,394	2271
	Urban small	45.23	63,550	121
	Urban small	202.84	103,756	449
	Indigenous	100.05	104,164	1483
No children	Baseline (full sample)	318.65	609,071	2325
	Rural	155.56	167,550	1391
	Rural small	285.04	250,753	2278
	Urban small	89.63	74,231	121
	Urban small	284.78	116,524	449
	Indigenous	127.02	116,414	1508
Theme: Employment and Working Hours				
Employment	Baseline (full sample)	490.66	1,162,974	2325
	Males	284.25	553,639	2325
	Females	317.08	609,335	2325
	Rural-small	477.92	483,546	2287
	Rural	276.82	316,575	1392
	Urban-small	357.01	220,156	449
	Urban	136.82	142,687	121
	Indigenous	1684.65	1,136,366	2124
Hours worked	Baseline (full sample)	369.06	761,574	2324
	Males	267.04	487,824	2324
	Females	148.97	273,743	2316
	Rural-small	310.16	275,770	2283
	Rural	209.58	214,826	1390
	Urban-small	257.72	161,721	449
	Urban	108.14	109,246	121
	Indigenous	951.18	526,893	2024
Theme: Sector of Employment				
Agriculture	Baseline (full sample)	326.84	672,386	2321
	Males	244.85	425,911	2318
	Females	120.63	246,464	2305
	Rural-small	235.04	222,312	2277
	Rural	192.57	197,907	1390
	Urban-small	235	152,016	449
	Urban	95.19	100,141	121
	Indigenous	960.45	347,449	1989
Construction	Baseline (full sample)	326.84	672,386	2321
	Males	244.85	425,911	2318
	Females	120.63	246,464	2305
	Rural-small	235.04	222,312	2277
	Rural	192.57	197,907	1390
	Urban-small	235	152,016	449
	Urban	95.19	100,141	121
	Indigenous	960.45	347,449	1989
Manufacturing	Baseline (full sample)	326.84	672,386	2321
	Males	244.85	425,911	2318
	Females	120.63	246,464	2305
	Rural-small	235.04	222,312	2277
	Rural	192.57	197,907	1390

	Urban-small	235	152,016	449
	Urban	95.19	100,141	121
	Indigenous	960.45	347,449	1989
Education	Baseline (full sample)	326.84	672,386	2321
	Males	244.85	425,911	2318
	Females	120.63	246,464	2305
	Rural-small	235.04	222,312	2277
	Rural	192.57	197,907	1390
	Urban-small	235	152,016	449
	Urban	95.19	100,141	121
	Indigenous	960.45	347,449	1989
Health	Baseline (full sample)	326.84	672,386	2321
	Males	244.85	425,911	2318
	Females	120.63	246,464	2305
	Rural-small	235.04	222,312	2277
	Rural	192.57	197,907	1390
	Urban-small	235	152,016	449
	Urban	95.19	100,141	121
	Indigenous	960.45	347,449	1989
Theme: Migration and Remittances				
Migration State of birth	Baseline (full sample)	496.05	1,162,783	2325
	Males	286.12	553,543	2325
	Females	322.23	609,240	2325
	Rural-small	479.19	483,513	2287
	Rural	275.7	316,539	1392
	Urban-small	362.15	220,110	449
	Urban	138.87	142,611	121
	Indigenous	275.37	223,280	1739
State 5-years	Baseline (full sample)	490.63	1,162,974	2325
	Males	284.25	553,639	2325
	Females	317.08	609,335	2325
	Rural-small	479.52	483,546	2287
	Rural	276.27	316,575	1392
	Urban-small	359.31	220,156	449
	Urban	136.95	142,687	121
	Indigenous	274.08	223,289	1739
Municipality 5-years	Baseline (full sample)	490.63	1,162,974	2325
	Males	284.25	553,639	2325
	Females	317.08	609,335	2325
	Rural-small	479.52	483,546	2287
	Rural	276.27	316,575	1392
	Urban-small	359.31	220,156	449
	Urban	136.95	142,687	121
	Indigenous	274.08	223,289	1739
Remittance receiver	Baseline (full sample)	498.26	1,161,948	2325
	Males	294.12	553,131	2325
	Females	305.65	608,817	2325
	Rural-small	476.62	483,290	2287
	Rural	275.43	316,419	1392
	Urban-small	361.61	219,927	449
	Urban	138.89	142,302	121
	Indigenous	280.21	223,122	1739

Note: Each panel reports the Kleibergen–Paap rk Wald F-statistic for the null of weak identification. Standard errors are clustered at the municipality level.

Table A4. First-Stage Relevance Tests for the Instrument: Adult outcomes (father-child pairs)

Dependent variable	Sample	F-statistic	Number of observations	Clusters
Literacy				
	Baseline (full sample)	158.55	415,265	2322
	Indigenous	35.58	88,248	913
	Rural small	165.53	208,012	2264
	Rural small	80.4	108,324	1390
	Urban small	38.88	64,576	448
	Urban small	23.34	34,336	121
Primary				
	Baseline (full sample)	200.57	382,328	2320
	Indigenous	49.74	79,651	867
	Rural small	209.77	192,264	2258
	Rural small	101	98,423	1391
	Urban small	67.29	59,143	448
	Urban small	31.09	32,480	121
Secondary				
	Baseline (full sample)	53.27	179,048	2305
	Indigenous	36.52	38,466	753
	Rural small	76.71	89,397	2225
	Rural small	27.2	47,202	1385
	Urban small	14.89	27,888	448
	Urban small	4.2	14,532	121
Upper secondary				
	Baseline (full sample)	90.53	143,268	2298
	Indigenous	16.99	30,409	752
	Rural small	30.32	69,030	2209
	Rural small	19.09	38,586	1381
	Urban small	1.55	23,238	448
	Urban small	30.12	12,391	121
Higher				
	Baseline (full sample)	27.25	51,595	2184
	Indigenous	9.83	10,696	575
	Rural small	13.17	23,318	1955
	Rural small	4.4	14,202	1289
	Urban small	8.73	8,850	447
	Urban small	12.79	5,052	121

Note: *Note: Each panel reports the Kleibergen–Paap rk Wald F-statistic for the null of weak identification. Standard errors are clustered at the municipality level.*

Table A5. First-Stage Relevance Tests for the Instrument: Adult outcomes (mother-child pairs)

Dependent variable	Sample	F-statistic	Number of observations	Clusters
Literacy				
	Baseline (full sample)	198.51	464,881	2324
	Indigenous	124.61	91,761	965
	Rural small	235.46	224,314	2266
	Rural small	99.35	122,541	1390
	Urban small	148.06	76,245	449
	Urban small	40.08	41,771	121
Primary				
	Baseline (full sample)	229.45	381,424	2323
	Indigenous	118.03	76,888	883
	Rural small	275.19	185,806	2265
	Rural small	68.3	98,910	1391
	Urban small	121.48	61,539	449
	Urban small	43.61	35,159	121
Secondary				
	Baseline (full sample)	93.1	218,529	2317
	Indigenous	76.5	42,379	787
	Rural small	123.87	104,797	2245
	Rural small	30.1	58,289	1387
	Urban small	63.8	36,138	449
	Urban small	16.98	19,288	121
Upper secondary				
	Baseline (full sample)	147.69	210,429	2318
	Indigenous	41.75	39,368	821
	Rural small	98.34	98,021	2243
	Rural small	63.7	57,066	1389
	Urban small	59.14	36,188	448
	Urban small	30.61	19,132	121
Higher education				
	Baseline (full sample)	51.29	98,152	2281
	Indigenous	15.45	17,983	682
	Rural small	44.65	43,929	2154
	Rural small	29.86	27,110	1379
	Urban small	26.59	17,245	449
	Urban small	8.06	9,812	121

Note: *Note: Each panel reports the Kleibergen–Paap rk Wald F-statistic for the null of weak identification. Standard errors are clustered at the municipality level.*

Table A6. Regression Discontinuity Results: All Individuals, Males, Females, Indigenous and by Locality Size.

	(1A)	(2A)	(3A)	(4A)		(1B)	(2B)	(3B)	(4B)
		Years					Years		
	Literacy	Schooling	Nine +	Twelve +		Literacy	Schooling	Nine +	Twelve +
Baseline: full sample of eligible individuals					Males				
Reform	0.0088**	0.1657**	0.0179**	0.0033	Reform	0.0087**	0.1803**	0.0181**	0.0064
SE	0.0010	0.0322	0.0028	0.0035	SE	0.0015	0.0510	0.0042	0.0056
Mean	0.93	8.57	0.59	0.27	Mean	0.94	8.62	0.59	0.28
R2	0.09	0.20	0.16	0.13	R2	0.06	0.19	0.16	0.13
N	1163803	1162974	1164785	1164785	N	554053	553639	554473	554473
F	42.32	210.87	126.98	132.82	F	26.80	135.18	95.24	58.59
Females					Indigenous speakers				
Reform	0.0093**	0.1542**	0.0182**	0.0005	Reform	0.0452**	0.4970**	0.0377**	0.0191**
SE	0.0013	0.0379	0.0039	0.0050	SE	0.0063	0.0766	0.0087	0.0066
Mean	0.92	8.52	0.60	0.27	Mean	0.78	5.65	0.29	0.10
R2	0.13	0.21	0.16	0.13	R2	0.14	0.21	0.18	0.11
N	609750	609335	610312	610312	N	223405	223289	223510	223510
F	32.76	114.72	93.60	70.40	F	50.63	105.27	96.43	16.90
Locality: population<2,500					Locality: 2500<population<15000				
Reform	0.0266**	0.3267**	0.0297**	0.0077+	Reform	0.0094**	0.2292**	0.0276**	0.0057
SE	0.0030	0.0411	0.0048	0.0043	SE	0.0021	0.0476	0.0052	0.0065
Mean	0.88	6.86	0.43	0.13	Mean	0.94	8.79	0.63	0.29
R2	0.11	0.18	0.14	0.10	R2	0.08	0.16	0.13	0.11
N	483802	483546	484057	484057	N	316812	316575	317078	317078
F	58.53	179.33	165.75	72.22	F	27.44	122.01	114.60	52.27
Locality: 15000<population<100000					Locality: population>100000				
Reform	0.0051**	0.1506**	0.0139**	0.0036	Reform	0.0029*	0.0876	0.0115*	0.0009
SE	0.0016	0.0581	0.0044	0.0066	SE	0.0011	0.0559	0.0050	0.0062
Mean	0.97	10.16	0.75	0.41	Mean	0.99	11.38	0.85	0.52
R2	0.02	0.08	0.07	0.06	R2	0.01	0.06	0.03	0.05
N	220367	220156	220576	220576	N	142812	142687	143064	143064
F	19.09	158.62	115.04	62.89	F	3.25	58.87	34.22	41.13

Note: The table reports regression discontinuity estimates of the reform's impact on literacy, years of schooling, and indicators for completing at least nine or twelve years of education. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Sample sizes correspond to the relevant subgroups. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$.

Table A7. Ordinary Least Squares of Education of Family Formation: Fertility and Child Mortality

	(1A) Children Number	(2A) Child Mortality	(3A) No Children		(1B) Children Number	(2B) Child Mortality	(3B) No Children
Baseline: full sample of eligible females				Indigenous speakers			
Years of school	-0.0997**	-0.0013**	0.0080**	Years of school	-0.0979**	-0.0014**	0.0019*
SE	0.0015	0.0001	0.0008	SE	0.0033	0.0002	0.0008
Mean	2.68	0.02	0.10	Mean	3.49	0.03	0.10
r2	0.16	0.01	0.03	r2	0.17	0.05	0.04
N	607983	544170	609071	N	115587	104164	116414
F	4663.89	436.59	89.46	F	889.01	89.74	6.11
Locality: population<2,500				Locality: 2500<population<15000			
Years of school	-0.0885**	-0.0015**	-0.0002	Years of school	-0.0893**	-0.0013**	0.0035**
SE	0.0024	0.0001	0.0004	SE	0.0020	0.0001	0.0004
Mean	3.08	0.03	0.09	Mean	2.58	0.02	0.10
r2	0.14	0.02	0.03	r2	0.12	0.02	0.02
N	249847	226394	250753	N	167406	150455	167550
F	1318.99	137.36	0.23	F	1965.05	131.26	74.17
Locality: 15000<population<100000				Locality: population>100000			
Years of school	-0.0913**	-0.0014**	0.0061**	Years of school	-0.1062**	-0.0012**	0.0124**
SE	0.0016	0.0001	0.0004	SE	0.0023	0.0001	0.0012
Mean	2.35	0.02	0.11	Mean	2.08	0.02	0.14
r2	0.11	0.01	0.02	r2	0.14	0.01	0.04
N	116494	103756	116524	N	74223	63550	74231
F	3376.37	173.89	228.16	F	2194.05	125.13	111.21

Note: The table reports ordinary least squares estimates of the association between years of schooling and (a) number of children per woman residing in the household, (b) likelihood of having no children, (c) share of children bon that are no longer living (child mortality). All regressions include municipality fixed effects, controls for locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01.

Table A8. Ordinary Least Squares of Education of Family Formation: Fertility and Child Mortality

	(1A) Children Number	(2A) Child Mortality	(3A) No Children		(1B) Children Number	(2B) Child Mortality	(3B) No Children
Baseline: full sample of eligible females				Indigenous speakers			
Years of school	-0.2824**	-0.0093**	0.0217**	Years of school	-0.3976**	-0.0113**	-0.0051
	0.0174	0.0013	0.0047		0.0472	0.0023	0.0064
Mean	2.68	0.02	0.10	Mean	3.49	0.03	0.10
N	607983	544170	609071	N	115587	104164	116414
F	262.60	49.09	21.24	F	70.82	23.79	0.62
Locality: population<2,500				Locality: 2500<population<15000			
Years of school	-0.3279**	-0.0093**	-0.0042	Years of school	-0.2789**	-0.0066**	0.0092
	0.0287	0.0015	0.0048		0.0310	0.0021	0.0057
Mean	3.08	0.03	0.09	Mean	2.58	0.02	0.10
N	249847	226394	250753	N	167406	150455	167550
F	131.00	37.96	0.76	F	81.05	10.08	2.63
Locality: 15000<population<100000				Locality: population>100000			
Years of school	-0.2344**	-0.0071**	0.0149**	Years of school	-0.2748**	-0.0112**	0.0426**
	0.0203	0.0017	0.0053		0.0303	0.0030	0.0092
Mean	2.35	0.02	0.11	Mean	2.08	0.02	0.14
N	116494	103756	116524	N	74223	63550	74231
F	132.98	18.20	7.92	F	82.38	13.60	21.46

Notes: *Note: The table reports instrumental variables estimates of the association between years of schooling and (a) number of children per woman residing in the household, (b) likelihood of having no children, (c) share of children bon that are no longer living (child mortality). All regressions include municipality fixed effects, controls for locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01*

Table A9. Ordinary Least Squares Estimates of Education on Employment and Hours Worked.

	(1) Employment	(2) Number of hours		(1) Employment	(2) Number of hours
Baseline: full sample of eligible individuals			Males		
Years of school	0.0150**	-0.1937**	Years of school	0.0062**	-0.2632**
	0.0005	0.0182		0.0005	0.0218
Mean	0.59	43.49	Mean	0.78	46.36
R2	0.19	0.08	R2	0.07	0.04
N	1162974	761574	N	553639	487824
F	1036.16	113.17	F	177.36	146.17
Females			Indigenous speakers		
Years of school	0.0233**	-0.0924**	Years of school	0.0132**	-0.0634
	0.0006	0.0234		0.0008	0.0388
Mean	0.41	38.37	Mean	0.41	39.67
R2	0.10	0.02	R2	0.32	0.15
N	609335	273743	N	223289	124708
F	1791.01	15.57	F	264.47	2.66
Locality: population<2500			Locality: 2500<population<15000		
Years of school	0.0164**	0.0977**	Years of school	0.0172**	-0.1044**
	0.0005	0.0226		0.0005	0.0226
Mean	0.46	41.20	Mean	0.63	44.22
R2	0.30	0.12	R2	0.22	0.10
N	483546	275770	N	316575	214826
F	1190.25	18.77	F	1413.40	21.36
Locality: 15000<population<100000			Locality: population>100000		
Years of school	0.0151**	-0.2713**	Years of school	0.0132**	-0.2900**
	0.0004	0.0218		0.0008	0.0270
Mean	0.70	45.36	Mean	0.72	45.03
R2	0.16	0.08	R2	0.12	0.07
N	220156	161721	N	142687	109246
F	1344.77	154.90	F	291.88	115.14

Note: The table reports ordinary least squares estimates of the association between years of schooling and (a) whether the individual is doing paid work and (b) the number of hours worked. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A10. Ordinary Least Squares Estimates of Education on Employment and Hours Worked.

	(1) Employment	(2) Number of hours		(1) Employment	(2) Number of hours
Baseline: full sample of eligible individuals			Males		
Years of school	0.0055	0.0107	Years of school	0.0149**	0.1800
	0.0037	0.2176		0.0049	0.2750
Mean	0.59	43.49	Mean	0.78	46.36
N	1162974	761574.00	N	553639	487824
F	2.23	0.00	F	9.18	0.43
Females			Indigenous speakers		
Years of school	-0.0004	-0.2218	Years of school	0.0550**	1.1628**
	0.0056	0.3083		0.0017	0.0946
Mean	0.41	38.37	Mean	0.31	37.80
N	609335	273743.00	N	1136366	526893
F	0.01	0.52	F	1091.88	151.21
Locality: population<2500			Locality: 2500<population<15000		
Years of school	0.0033	0.7308**	Years of school	0.0004	0.1225
	0.0039	0.2492		0.0054	0.3325
Mean	0.46	41.20	Mean	0.63	44.22
N	483546	275770.00	N	316575	214826
F	0.69	8.60	F	0.01	0.14
Locality: 15000<population<100000			Locality: population>100000		
Years of school	0.0120*	0.5551+	Years of school	0.0068	-0.5665
	0.0055	0.3292		0.0079	0.4356
Mean	0.70	45.36	Mean	0.72	45.03
N	220156	161721.00	N	142687	109246
F	4.70	2.84	F	0.74	1.69

Note: The table reports instrumental variable estimates of the association between years of schooling on (a) whether the individual is doing paid work and (b) the number of hours worked. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A11. Ordinary Least Squares Estimates of Education on Sector of Employment

	(1) Agriculture	(2) Manufacturing	(3) Construction	(4) Education	(5) Health
Baseline: full sample of eligible individuals					
Years of school	-0.0075**	-0.0076**	-0.0095**	0.0195**	0.0083**
SE	0.0006	0.0005	0.0004	0.0005	0.0002
Mean	0.17	0.15	0.11	0.07	0.03
R2	0.28	0.09	0.09	0.13	0.05
N	672386	672386	672386	672386	672386
F	155.20	233.81	678.58	1407.41	2223.05
Males					
Years of school	-0.0099**	-0.0033**	-0.0164**	0.0130**	0.0056**
SE	0.0008	0.0005	0.0006	0.0004	0.0002
Mean	0.24	0.14	0.16	0.04	0.01
R2	0.34	0.09	0.07	0.09	0.03
N	425911	425911	425911	425911	425911
F	163.04	46.07	736.28	1014.24	1203.17
Females					
Years of school	-0.0031**	-0.0137**	0.0005**	0.0289**	0.0124**
SE	0.0003	0.0010	0.0001	0.0008	0.0003
Mean	0.05	0.17	0.01	0.12	0.05
R2	0.14	0.12	0.01	0.16	0.05
N	246464	246464	246464	246464	246464
F	88.27	198.72	18.27	1194.20	1451.97
Indigenous speakers					
Years of school	-0.0149**	-0.0049**	-0.0087**	0.0236**	0.0037**
SE	0.0010	0.0007	0.0006	0.0011	0.0003
Mean	0.33	0.15	0.14	0.06	0.01
R2	0.39	0.15	0.15	0.26	0.06
N	90976	90976	90976	90976	90976
F	226.52	47.11	191.15	501.84	193.35
Locality: population<2500					
Years of school	-0.0232**	-0.0015**	-0.0069**	0.0196**	0.0046**
SE	0.0005	0.0004	0.0005	0.0005	0.0002
Mean	0.34	0.12	0.13	0.05	0.01
R2	0.30	0.11	0.13	0.19	0.06
N	222312	222312	222312	222312	222312
F	1863.63	12.17	216.89	1542.35	475.76
Locality: 2500<population<15000					
Years of school	-0.0131**	-0.0058**	-0.0107**	0.0245**	0.0072**
SE	0.0006	0.0005	0.0004	0.0006	0.0003
Mean	0.14	0.17	0.11	0.08	0.03
R2	0.22	0.09	0.12	0.20	0.06
N	197907	197907	197907	197907	197907
F	438.07	153.19	685.60	1553.00	584.02
Locality: 15000<population<100000					
Years of school	-0.0057**	-0.0088**	-0.0107**	0.0240**	0.0090**
SE	0.0005	0.0006	0.0003	0.0007	0.0002
Mean	0.05	0.17	0.09	0.09	0.04
R2	0.13	0.09	0.09	0.17	0.05
N	152016	152016	152016	152016	152016
F	121.49	244.37	1072.19	1336.18	1449.96
Locality: population>100000					
Years of school	-0.0006**	-0.0093**	-0.0095**	0.0166**	0.0094**
SE	0.0001	0.0008	0.0007	0.0006	0.0003
Mean	0.01	0.17	0.07	0.07	0.05
R2	0.02	0.10	0.07	0.09	0.05
N	100141	100141	100141	100141	100141
F	15.71	124.51	193.82	735.64	1291.78

Note: The table reports ordinary least squares estimates of the association between years of schooling on whether the individual doing paid work is employed in (agriculture), (b) manufacturing, (c) construction, (d) health, and (e) education. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$.

Table A12. Instrumental Variable Estimates of Education on Sector of Employment

	(1) Agriculture	(2) Manufacturing	(3) Construction	(4) Education	(5) Health
Baseline: full sample of eligible individuals					
Years of school	-0.0123**	0.0177**	-0.0095**	-0.0034	0.0067**
SE	0.0024	0.0055	0.0029	0.0037	0.0018
Mean	0.17	0.15	0.11	0.07	0.03
N	672386	672386	672386	672386	672386
F	25.47	10.24	10.87	0.84	13.69
Males					
Years of school	-0.0190**	0.0227**	-0.0199**	-0.0045	0.0063**
SE	0.0036	0.0066	0.0054	0.0035	0.0023
Mean	0.24	0.14	0.16	0.04	0.01
N	425911	425911	425911	425911	425911
F	27.82	11.82	13.44	1.66	7.47
Females					
Years of school	-0.0011	0.0091	0.0030	-0.0017	0.0074*
SE	0.0017	0.0087	0.0020	0.0066	0.0035
Mean	0.05	0.17	0.01	0.12	0.05
N	246464	246464	246464	246464	246464
F	0.43	1.08	2.30	0.07	4.35
Indigenous speakers					
Years of school	-0.0279**	0.0039	0.0128**	0.0046**	0.0010
SE	0.0018	0.0026	0.0021	0.0014	0.0006
Mean	0.39	0.15	0.11	0.05	0.01
N	347449	347449	347449	347449	347449
F	240.35	2.33	35.91	10.89	2.68
Locality: population<2500					
Years of school	-0.0297**	0.0218**	-0.0089+	0.0062*	0.0006
SE	0.0062	0.0058	0.0051	0.0029	0.0018
Mean	0.34	0.12	0.13	0.05	0.01
N	222312	222312	222312	222312	222312
F	22.87	14.18	3.01	4.52	0.10
Locality: 2500<population<15000					
Years of school	-0.0247**	0.0226**	-0.0017	-0.0097+	0.0052
SE	0.0063	0.0078	0.0064	0.0051	0.0033
Mean	0.14	0.17	0.11	0.08	0.03
N	197907	197907	197907	197907	197907
F	15.33	8.42	0.07	3.65	2.47
Locality: 15000<population<100000					
Years of school	-0.0084**	-0.0006	-0.0099*	0.0013	0.0058+
SE	0.0029	0.0118	0.0048	0.0047	0.0034
Mean	0.05	0.17	0.09	0.09	0.04
N	152016	152016	152016	152016	152016
F	8.18	0.00	4.22	0.07	3.00
Locality: population>100000					
Years of school	-0.0000	0.0191+	-0.0118*	-0.0066	0.0106**
SE	0.0018	0.0098	0.0056	0.0077	0.0037
Mean	0.01	0.17	0.07	0.07	0.05
N	100141	100141	100141	100141	100141
F	0.00	3.84	4.38	0.75	8.23

Note: The table reports instrumental variable estimates of the association between years of schooling on whether the individual doing paid work is employed in (agriculture), (b) manufacturing, (c) construction, (d) health, and (e) education. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A13. Ordinary Least Squares Estimates of Education on Migration and Remittances

	(1) State Birthplace	(2) State 5-years	(3) Municipality 5-years	(4) Remittance Receiver
Baseline: full sample of eligible individuals				
Years of school	0.0018	0.0012**	0.0025**	-0.0005**
SE	0.0011	0.0002	0.0003	0.0002
Mean	0.13	0.03	0.06	0.08
R2	0.19	0.02	0.03	0.02
N	1162785	1162785	1162785	1162785
F	2.66	27.55	85.91	8.70
Males				
Years of school	0.0031**	0.0016**	0.0033**	-0.0016**
SE	0.0010	0.0003	0.0003	0.0002
Mean	0.13	0.04	0.07	0.07
R2	0.19	0.02	0.03	0.02
N	553543	553639	553639	553131
F	8.95	42.20	109.36	62.91
Females				
Years of school	0.0006	0.0008**	0.0019**	0.0004+
SE	0.0012	0.0002	0.0003	0.0002
Mean	0.13	0.03	0.05	0.10
R2	0.19	0.02	0.03	0.02
N	609240	609335	609335	608817
F	0.23	11.12	45.70	3.29
Indigenous speakers				
Years of school	-0.0026**	0.0005	0.0013**	-0.0002
SE	0.0009	0.0003	0.0004	0.0004
Mean	0.04	0.02	0.03	0.07
R2	0.66	0.10	0.11	0.06
N	223280	223289	223289	223122
F	8.74	1.81	9.30	0.29
Locality: population<2500				
Years of school	0.0035**	0.0015**	0.0032**	-0.0007**
SE	0.0005	0.0002	0.0003	0.0003
Mean	0.07	0.03	0.05	0.10
R2	0.13	0.03	0.04	0.05
N	483513	483546	483546	483290
F	56.35	62.51	123.37	7.34
Locality: 2500<population<15000				
Years of school	0.0046**	0.0010**	0.0031**	-0.0006**
SE	0.0008	0.0003	0.0005	0.0002
Mean	0.11	0.03	0.06	0.08
R2	0.16	0.03	0.06	0.04
N	316539	316575	316575	316419
F	35.91	12.28	44.75	6.72
Locality: 15000<population<100000				
Years of school	0.0035**	0.0010**	0.0022**	-0.0007**
SE	0.0009	0.0003	0.0003	0.0003
Mean	0.17	0.04	0.07	0.07
R2	0.17	0.04	0.06	0.02
N	220110	220156	220156	219927
F	14.56	13.81	44.73	7.07
Locality: population>100000				
Years of school	-0.0001	0.0012**	0.0021**	-0.0004
SE	0.0019	0.0004	0.0004	0.0003
Mean	0.28	0.04	0.07	0.07
R2	0.17	0.01	0.01	0.01
N	142611	142687	142687	142302
F	0.00	9.57	22.58	1.68

Note: The table reports ordinary least squares estimates of the association between years of schooling on (a) migration from state of birthplace, (b) migration from municipality of residence in the last five years, (c) migration from state of residence in the last five years, and (d) whether the individual lives in a household that receives international remittances. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A14. Instrumental Variable Estimates of Education on Migration and Remittances

	(1) State Birthplace	(2) State 5-years	(3) Municipality 5-years	(4) Remittance Receiver
Baseline: full sample of eligible individuals				
Years of school	-0.0161**	0.0143**	0.0250**	-0.0029
SE	0.0037	0.0020	0.0027	0.0024
Mean	0.13	0.03	0.06	0.08
N	1162785	1162785	1162785	1162785
F	19.06	49.40	85.77	1.55
Males				
Years of school	-0.0215**	0.0197**	0.0326**	0.0013
SE	0.0069	0.0036	0.0044	0.0034
Mean	0.13	0.04	0.07	0.07
N	553543	553639	553639	553131
F	9.71	30.63	53.73	0.16
Females				
Years of school	-0.0123**	0.0104**	0.0189**	-0.0063+
SE	0.0038	0.0026	0.0033	0.0034
Mean	0.13	0.03	0.05	0.10
N	609240	609335	609335	608817
F	10.18	15.84	33.11	3.43
Indigenous speakers				
Years of school	0.0016	0.0115*	0.0130*	-0.0069+
SE	0.0051	0.0045	0.0054	0.0039
Mean	0.04	0.02	0.03	0.07
N	223280	223289	223289	223122
F	0.10	6.58	5.86	3.22
Locality: population<2500				
Years of school	0.0024	0.0077**	0.0131**	-0.0073*
SE	0.0033	0.0018	0.0023	0.0030
Mean	0.07	0.03	0.05	0.10
N	483513	483546	483546	483290
F	0.51	18.24	31.73	6.00
Locality: 2500<population<15000				
Years of school	-0.0056	0.0139**	0.0249**	-0.0046
SE	0.0062	0.0035	0.0043	0.0040
Mean	0.11	0.03	0.06	0.08
N	316539	316575	316575	316419
F	0.81	15.75	34.33	1.32
Locality: 15000<population<100000				
Years of school	-0.0199**	0.0128**	0.0261**	-0.0033
SE	0.0064	0.0038	0.0048	0.0039
Mean	0.17	0.04	0.07	0.07
N	220110	220156	220156	219927
F	9.58	11.24	29.48	0.72
Locality: population>100000				
Years of school	-0.0319**	0.0184**	0.0305**	0.0004
SE	0.0075	0.0040	0.0057	0.0043
Mean	0.28	0.04	0.07	0.07
N	142611	142687	142687	142302
F	18.26	21.11	28.61	0.01

Note: The table reports ordinary least squares estimates of the association between years of schooling on (a) migration from state of birthplace, (b) migration from municipality of residence in the last five years, (c) migration from state of residence in the last five years, and (d) whether the individual lives in a household that receives international remittances. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A15. Intergenerational Ordinary Least Squares Estimates: Father-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper Secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.203**	0.334**	1.666**	3.342**	3.667**
SE	0.017	0.030	0.054	0.092	0.127
Mean dependent variable	97.74	94.45	81.60	58.57	31.26
R2	0.04	0.02	0.11	0.17	0.17
N	415290	382311	179059	143269	51592
F	148.57	123.41	938.09	1318.75	831.05
Locality: population<2,500					
Years of school	0.406**	0.570**	2.086**	3.387**	2.840**
SE	0.044	0.060	0.093	0.202	0.270
R2	0.06	0.06	0.15	0.18	0.24
N	208018	192244	89403	69020	23315
F	84.73	89.50	506.42	280.50	110.60
Locality: 2500<population<15000					
Years of school	0.217**	0.351**	1.823**	3.173**	3.396**
SE	0.020	0.044	0.081	0.130	0.205
R2	0.04	0.05	0.13	0.17	0.23
N	108337	98411	47221	38601	14195
F	121.16	63.15	512.16	598.71	274.44
Locality: 15000<population<100000					
Years of school	0.158**	0.335**	1.572**	3.492**	3.859**
SE	0.017	0.041	0.084	0.118	0.217
R2	0.02	0.03	0.09	0.15	0.17
N	64579	59147	27878	23229	8853
F	81.73	67.05	353.48	875.32	316.54
Locality: population>100000					
Years of school	0.094**	0.194**	1.354**	3.239**	3.842**
SE	0.020	0.051	0.091	0.158	0.217
R2	0.01	0.01	0.05	0.12	0.12
N	34338	32490	14529	12395	5055
F	21.45	14.76	221.98	418.18	313.26
Indigenous speaking individuals					
Years of school	0.720**	0.767**	2.158**	2.317**	1.944**
SE	0.075	0.118	0.185	0.507	0.455
R2	0.09	0.12	0.19	0.18	0.18
N	88227	79627	38453	30416	10699
F	92.61	42.07	136.66	20.86	18.23

Note: The table reports ordinary least squares estimates of the association between father years of schooling and children schooling (a) literacy (knows to read and write, (b) enrolled in school aged 6-12 (Primary), (c) enrolled to school aged 13-15 (Secondary), (d) enrolled to school aged 16-18 (Upper secondary), (e) enrolled to school aged 19-20 (Higher).. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A16. Intergenerational Ordinary Least Squares Estimates: Mother-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper Secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.217**	0.369**	1.732**	3.388**	3.941**
SE	0.015	0.031	0.060	0.072	0.083
Mean dependent variable	97.85	94.33	81.87	59.30	33.18
R2	0.03	0.02	0.10	0.16	0.15
N	464829	381395	218504	210431	98143
F	209.25	137.44	844.55	2222.07	2247.43
Locality: population<2,500					
Years of school	0.367**	0.579**	2.141**	3.419**	2.939**
SE	0.025	0.050	0.087	0.112	0.128
R2	0.05	0.06	0.13	0.16	0.19
N	224255	185790	104763	98029	43927
F	210.67	133.02	604.49	933.83	530.45
Locality: 2500<population<15000					
Years of school	0.247**	0.407**	1.960**	3.581**	3.997**
SE	0.022	0.049	0.077	0.098	0.161
R2	0.04	0.05	0.12	0.16	0.19
N	122542	98904	58292	57066	27104
F	122.18	68.62	656.31	1322.01	619.28
Locality: 15000<population<100000					
Years of school	0.182**	0.398**	1.719**	3.327**	3.839**
SE	0.019	0.041	0.083	0.111	0.146
R2	0.02	0.03	0.09	0.13	0.14
N	76247	61533	36137	36183	17246
F	94.66	92.65	426.93	891.71	690.14
Locality: population>100000					
Years of school	0.123**	0.248**	1.460**	3.282**	4.202**
SE	0.023	0.050	0.115	0.133	0.165
R2	0.01	0.01	0.05	0.11	0.12
N	41775	35158	19295	19131	9810
F	28.37	25.00	161.79	608.96	647.90
Indigenous speaking individuals					
Years of school	0.657**	0.802**	2.309**	2.647**	2.168**
SE	0.056	0.090	0.200	0.219	0.206
R2	0.09	0.12	0.18	0.18	0.19
N	91726	76876	42363	39377	17980
F	136.61	78.86	132.67	146.65	110.68

Note: The table reports ordinary least squares estimates of the association between mother years of schooling and children schooling (a) literacy (knows to read and write, (b) enrolled in school aged 6-12 (Primary), (c) enrolled to school aged 13-15 (Secondary), (d) enrolled to school aged 16-18 (Upper secondary), (e) enrolled to school aged 19-20 (Higher).. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$

Table A17. Intergenerational Instrumental Variable Estimates: Mother-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.590**	0.578+	4.265**	8.329**	3.693+
SE	0.211	0.305	1.067	1.564	2.244
Mean dependent variable	97.74	94.45	81.60	58.57	31.26
N	415290	382311	179059	143269	51592
F	7.81	3.59	15.99	28.37	2.71
Locality: population<2,500					
Years of school	0.384	1.470**	4.565**	13.060**	3.458
SE	0.281	0.373	1.068	3.583	3.633
N	208018	192244	89403	69020	23315
F	1.87	15.56	18.28	13.29	0.91
Locality: 2500<population<15000					
Years of school	-0.370	0.348	5.740**	22.260**	9.486
SE	0.283	0.532	1.888	5.026	6.121
N	108337	98411	47221	38601	14195
F	1.71	0.43	9.24	19.62	2.40
Locality: 15000<population<100000					
Years of school	-0.880**	0.393	6.776*	46.623	4.161
SE	0.337	0.537	2.821	36.473	5.096
N	64579	59147	27878	23229	8853
F	6.82	0.53	5.77	1.63	0.67
Locality: population>100000					
Years of school	-0.640	0.121	15.257*	17.726**	7.123
SE	0.570	0.926	7.365	3.195	4.421
N	34338	32490	14529	12395	5055
F	1.26	0.02	4.29	30.77	2.60
Indigenous speaking individuals					
Years of school	0.344	1.810**	4.632**	6.790	3.189
SE	0.483	0.530	1.073	4.594	2.616
N	88227	79627	38453	30416	10699
F	0.51	11.66	18.62	2.18	1.49

Note: The table reports instrumental variable estimates of the association between father years of schooling and children schooling (a) literacy (knows to read and write, (b) enrolled in school aged 6-12 (Primary), (c) enrolled to school aged 13-15 (Secondary), (d) enrolled to school aged 16-18 (Upper secondary), (e) enrolled to school aged 19-20 (Higher).. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level p < 0.10, p < 0.05, p < 0.01

Table A18. Intergenerational Instrumental Variable Estimates: Father-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper Secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.277*	0.046	2.911**	5.065**	0.493
SE	0.131	0.440	1.042	1.189	1.904
Mean dependent variable	97.85	94.33	81.87	59.30	33.18
N	464829	381395	218504	210431	98143
F	4.45	0.01	7.81	18.15	0.07
Locality: population<2,500					
Years of school	-0.395	0.141	4.130**	8.271**	2.705
SE	0.246	0.384	1.083	1.532	1.997
N	224255	185790	104763	98029	43927
F	2.58	0.14	14.54	29.14	1.84
Locality: 2500<population<15000					
Years of school	0.083	-0.394	4.644*	10.737**	3.391
SE	0.248	0.614	1.806	1.911	2.415
N	122542	98904	58292	57066	27104
F	0.11	0.41	6.61	31.57	1.97
Locality: 15000<population<100000					
Years of school	-0.098	-0.079	1.715	7.234**	-0.373
SE	0.228	0.647	1.140	2.175	3.716
N	76247	61533	36137	36183	17246
F	0.19	0.01	2.26	11.06	0.01
Locality: population>100000					
Years of school	-0.281	0.753	6.586*	13.004**	1.888
SE	0.225	0.997	2.727	3.453	4.255
N	41775	35158	19295	19131	9810
F	1.56	0.57	5.83	14.18	0.20
Indigenous speaking individuals					
Years of school	-0.660	-0.211	2.346*	9.365**	-2.169
SE	0.607	0.763	0.937	2.456	3.254
N	91726	76876	42363	39377	17980
F	1.18	0.08	6.27	14.54	0.44

Note: The table reports instrumental variable estimates of the association between mother years of schooling and children schooling (a) literacy (knows to read and write, (b) enrolled in school aged 6-12 (Primary), (c) enrolled to school aged 13-15 (Secondary), (d) enrolled to school aged 16-18 (Upper secondary), (e) enrolled to school aged 19-20 (Higher).. All regressions include municipality fixed effects, controls for sex and locality size, and cluster standard errors at the municipality level. Coefficients reflect the marginal effect of an additional year of schooling. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$

Table A19. Reduced form equations of Reform on Family Formation: Fertility and Child Mortality

	(1) Children Number	(2) Child Mortality	(3) No Children		(1) Children Number	(2) Child Mortality	(3) No Children
Baseline: full sample of eligible females				Indigenous speakers			
Reform	-0.1172**	-0.0033**	0.0089**	Reform	-0.2326**	-0.0064**	-0.0031
	0.0068	0.0004	0.0018		0.0239	0.0012	0.0038
R2	0.10	0.01	0.02	R2	0.14	0.04	0.04
N	608936	544995	610024	N	115685	104252	116512
F	296.91	66.72	23.80	F	94.87	26.91	0.64
Locality: population<2,500				Locality: 2500<population<15000			
Reform	-0.1682**	-0.0047**	-0.0021	Reform	-0.1361**	-0.0029**	0.0045
	0.0143	0.0007	0.0025		0.0140	0.0008	0.0028
R2	0.11	0.02	0.02	R2	0.07	0.01	0.02
N	250120	226633	251026	N	167674	150691	167818
F	138.83	40.71	0.70	F	94.69	11.69	2.55
Locality: 15000<population<100000				Locality: population>100000			
Reform	-0.1054**	-0.0028**	0.0067**	Reform	-0.0985**	-0.0032**	0.0151**
	0.0087	0.0006	0.0025		0.0105	0.0007	0.0028
R2	0.04	0.01	0.01	R2	0.05	0.00	0.02
N	116705	103940	116735	N	74424	63716	74432
F	146.69	20.64	7.48	F	87.26	19.13	28.32

Notes: The table reports reduced-form regressions of reform eligibility on (a) number of children per woman residing in the household, (b) likelihood of having no children, (c) share of children bon that are no longer living (child mortality). Specifications mirror the IV models in the main text, including municipality fixed effects, controls for sex and locality size, and clustered standard errors at the municipality level. Coefficients capture the intention-to-treat effect of the reform. Significance level p < 0.10, p < 0.05, p < 0.01

Table A20. Reduced form equations of Reform on Employment and Hours Worked

	(1) Employment	(2) Number of Hours		(1) Employment	(2) Number of Hours
Baseline: full sample of eligible individuals			Males		
Reform	0.0021	0.0061	Reform	0.0053**	0.0605
	0.0014	0.0799		0.0017	0.0916
R2	0.18	0.08	R2	0.06	0.04
N	1.16e+06	762901.00	N	554473.00	488569.00
F	2.31	0.01	F	9.46	0.44
Females			Indigenous speakers		
Reform	-0.0002	-0.0887	Reform	-0.0001	-0.3304
	0.0023	0.1265		0.0036	0.2591
R2	0.07	0.02	R2	0.31	0.15
N	610312.00	274325.00	N	223510.00	124856.00
F	0.01	0.49	F	0.00	1.63
Locality: population<2500			Locality: 2500<population<15000		
Reform	0.0017	0.3728**	Reform	0.0003	0.0553
	0.0021	0.1301		0.0024	0.1386
R2	0.29	0.12	R2	0.20	0.10
N	484057.00	276112.00	N	317078.00	215204.00
F	0.69	8.21	F	0.02	0.16
Locality: 15000<population<100000			Locality: population>100000		
Reform	0.0047*	0.2097+	Reform	0.0021	-0.1744
	0.0022	0.1202		0.0025	0.1315
R2	0.15	0.08	R2	0.10	0.06
N	220576.00	162038.00	N	143064.00	109536.00
F	4.77	3.04	F	0.71	1.76

Notes: The table reports reduced-form regressions of reform eligibility on (a) whether the individual is doing paid work and (b) the number of hours worked. Specifications mirror the IV models in the main text, including municipality fixed effects, controls for sex and locality size, and clustered standard errors at the municipality level. Coefficients capture the intention-to-treat effect of the reform. Significance level $p < 0.10$, $p < 0.05$, $p < 0.01$

Table A21. Reduced form equations of Reform on Sector of Employment

	(1) Agriculture	(2) Manufacturing	(3) Construction	(4) Education	(5) Health
			Baseline: full sample of eligible individuals		
Reform	-0.0045**	0.0063**	-0.0034**	-0.0013	0.0024**
	0.0009	0.0019	0.0011	0.0013	0.0007
R2	0.27	0.08	0.07	0.04	0.02
N	673579.00	673579.00	673579.00	673579.00	673579.00
F	26.86	11.05	10.01	0.97	13.38
			Males		
Reform	-0.0064**	0.0075**	-0.0066**	-0.0015	0.0021**
	0.0012	0.0021	0.0019	0.0011	0.0008
R2	0.32	0.08	0.03	0.02	0.01
N	426578.00	426578.00	426578.00	426578.00	426578.00
F	28.07	12.77	12.63	1.74	7.49
			Females		
Reform	-0.0004	0.0035	0.0012	-0.0009	0.0029+
	0.0007	0.0032	0.0008	0.0026	0.0015
R2	0.13	0.10	0.01	0.02	0.01
N	246990.00	246990.00	246990.00	246990.00	246990.00
F	0.42	1.17	2.16	0.12	3.84
			Indigenous speakers		
Reform	-0.0094**	0.0018	0.0071	-0.0021	0.0016
	0.0033	0.0057	0.0050	0.0031	0.0015
R2	0.37	0.14	0.13	0.09	0.04
N	91102.00	91102.00	91102.00	91102.00	91102.00
F	8.29	0.10	2.04	0.46	1.13
			Locality: population<2500		
Reform	-0.0152**	0.0110**	-0.0046+	0.0033*	0.0003
	0.0031	0.0029	0.0026	0.0015	0.0009
R2	0.27	0.11	0.12	0.06	0.04
N	222614.00	222614.00	222614.00	222614.00	222614.00
F	23.44	14.82	3.06	4.53	0.10
			Locality: 2500<population<15000		
Reform	-0.0099**	0.0092**	-0.0006	-0.0041*	0.0022
	0.0026	0.0030	0.0025	0.0020	0.0014
R2	0.19	0.08	0.10	0.07	0.03
N	198257.00	198257.00	198257.00	198257.00	198257.00
F	14.76	9.49	0.06	4.26	2.50
			Locality: 15000<population<100000		
Reform	-0.0030**	-0.0002	-0.0037*	0.0004	0.0020+
	0.0011	0.0042	0.0017	0.0017	0.0012
R2	0.11	0.08	0.07	0.05	0.02
N	152304.00	152304.00	152304.00	152304.00	152304.00
F	8.02	0.00	4.45	0.05	2.75
			Locality: population>100000		
Reform	0.0000	0.0059*	-0.0036+	-0.0022	0.0034**
	0.0006	0.0029	0.0019	0.0024	0.0012
R2	0.02	0.09	0.05	0.03	0.02
N	100394.00	100394.00	100394.00	100394.00	100394.00
F	0.00	4.10	3.83	0.88	8.09

Notes: The table reports reduced-form regressions of reform eligibility on whether the individual doing paid work is employed in (agriculture), (b) manufacturing, (c) construction, (d) health, and (e) education. Specifications mirror the IV models in the main text, including municipality fixed effects, controls for sex and locality size, and clustered standard errors at the municipality level. Coefficients capture the intention-to-treat effect of the reform. Significance level p < 0.10, p < 0.05, p < 0.01

Table A22. Reduced form equations of Reform on Migration and Remittances

	(1)	(2)	(3)	(4)
	State Birthplace	State 5-years	Municipality 5-years	Remittance Receiver
	Baseline: full sample of eligible individuals			
Reform	-0.0061**	0.0055**	0.0096**	-0.0011
	0.0014	0.0007	0.0009	0.0009
R2	0.19	0.02	0.03	0.02
N	1.16e+06	1.16e+06	1.16e+06	1.16e+06
F	18.65	62.07	106.80	1.37
	Males			
Reform	-0.0074**	0.0070**	0.0114**	0.0005
	0.0023	0.0012	0.0014	0.0012
R2	0.19	0.02	0.03	0.02
N	554376.00	554473.00	554473.00	553943.00
F	10.10	36.21	69.52	0.20
	Females			
Reform	-0.0050**	0.0043**	0.0078**	-0.0026+
	0.0016	0.0010	0.0012	0.0014
R2	0.19	0.02	0.03	0.02
N	610213.00	610312.00	610312.00	609775.00
F	9.61	18.55	39.28	3.34
	Indigenous speakers			
Reform	0.0010	0.0067**	0.0075*	-0.0040+
	0.0030	0.0026	0.0031	0.0022
R2	0.65	0.10	0.11	0.06
N	223500.00	223510.00	223510.00	223342.00
F	0.12	6.76	5.99	3.18
	Locality: population<2500			
Reform	0.0013	0.0040**	0.0068**	-0.0038*
	0.0017	0.0009	0.0012	0.0015
R2	0.13	0.03	0.04	0.05
N	484022.00	484057.00	484057.00	483796.00
F	0.54	20.24	35.07	6.28
	Locality: 2500<population<15000			
Reform	-0.0024	0.0063**	0.0112**	-0.0021
	0.0028	0.0015	0.0019	0.0018
R2	0.16	0.03	0.06	0.04
N	317042.00	317078.00	317078.00	316913.00
F	0.71	18.02	35.36	1.35
	Locality: 15000<population<100000			
Reform	-0.0075**	0.0049**	0.0100**	-0.0012
	0.0024	0.0014	0.0017	0.0015
R2	0.17	0.04	0.06	0.02
N	220529.00	220576.00	220576.00	220339.00
F	9.80	11.57	33.54	0.68
	Locality: population>100000			
Reform	-0.0101**	0.0059**	0.0098**	0.0003
	0.0023	0.0011	0.0016	0.0014
R2	0.17	0.01	0.01	0.01
N	142986.00	143064.00	143064.00	142660.00
F	18.53	28.07	39.17	0.04

Notes: The table reports reduced-form regressions of reform eligibility on (a) migration from state of birthplace, (b) migration from municipality of residence in the last five years, (c) migration from state of residence in the last five years, and (d) whether the individual lives in a household that receives international remittances. Specifications mirror the IV models in the main text, including municipality fixed effects, controls for sex and locality size, and clustered standard errors at the municipality level. Coefficients capture the intention-to-treat effect of the reform. Significance level p < 0.10, p < 0.05, p < 0.01