Discussion Paper Series

IZA DP No. 18864

August 2026

Compulsory Education, Life Trajectories, and Intergenerational Mobility: Evidence from Mexico's 1993 Schooling Reform

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IZA@LISER NETWORK
ISSN: 2365-9793

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Abstract

Mexico's 1993 reform extended compulsory schooling from six to nine years, creating a birth-cohort discontinuity in educational exposure. Using a regression discontinuity and instrumental variable design in the 2020 Mexican Census, this paper traces the reform's effects from educational attainment through fertility, child mortality, employment, and migration, into the schooling of the next generation. The reform raised attainment across all groups, but its effects were concentrated among Indigenous communities and rural populations, where baseline attainment was lowest: Indigenous literacy rose by 4.5 percentage points and lower-secondary completion by 3.8, from baselines of 78 and 29 per cent. These gains translated into lower fertility, reduced child mortality, higher employment, and a reallocation of workers out of low-productivity agriculture into higher-productivity sectors. Linking parents to children through census household identifiers, an additional year of parental schooling raised children's secondary enrolment by 3 to 4 percentage points and upper-secondary enrolment by 5 to 8. A single compulsory schooling reform thus generated a sustained, multi-domain shift in life trajectories extending across generations.

JEL classification

I26, I28, I24, J13, J62, O15

Keywords

compulsory schooling, education reform, intergenerational mobility, fertility, labour markets outcomes, migration

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I. Introduction

The long-run consequences of education extend well beyond the classroom. A large body of causal evidence –drawn from compulsory schooling mandates, school construction programmes, and changes in school year length–[1] documents that additional schooling reshapes labour market trajectories, lowers fertility, improves health, reduces crime, and transmits advantages across generations.[2] Yet this evidence base has a pronounced geographic skew: most of what is known comes from high-income countries with near-universal baseline attainment, formalised labour markets, and strong institutional capacity. Whether the same mechanisms operate in settings characterised by high inequality, labour market informality, and large populations with historically low educational attainment remains an open and consequential question –one whose answer matters both for the external validity of the existing literature and for the design of education policy in the countries where schooling gaps remain largest.

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 largest and most unequal economies –one where inequality and limited mobility remain pervasive (Campos-Vázquez et al., 2021; Grajales & Monroy-Gómez-Franco, 2017, 2025; Hertz et al., 2008; Neidhöfer et al., 2018) and where lower-secondary completion among cohorts born in the early 1980s still fell below 60 percent. Its scale, sudden implementation, and sharp birth-cohort eligibility threshold created a natural setting for causal evaluation.

A small but growing set of studies has used this reform to identify causal impacts in specific domains, examining outcomes ranging from cognitive health and psychosocial well-being to crime and fertility (Fabregas & Navarro-Sola, 2025; Gleditsch et al., 2022; Gutierrez et al., 2024; Ma et al., 2021).[3] These contributions are valuable, but they share a common limitation: each focuses on a single outcome domain, and none examines whether the educational gains produced by the reform are transmitted across generations. A recent working paper by León Bravo (2025) is closest in scope, using a regression discontinuity design to examine employment effects and finding no significant labour market impacts in the aggregate. This paper revisits and contextualises that finding in three ways. First, it uses the 2020 Population and Housing Census rather than the ENOE labour force survey, providing full population coverage including rural and Indigenous workers who are underrepresented in the ENOE sampling frame and among whom

[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; Fagernäs et al., 2025). These citations are illustrative rather than exhaustive, situating the present study within a broad 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., 2016; Kampelmann et al., 2018; Keats, 2018; McCrary & Royer, 2011); migration (Aydemir et al., 2022; 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.

the reform's effects are largest. Second, it includes municipality fixed effects, absorbing time-invariant local heterogeneity that may attenuate estimates in specifications with coarser geographic controls. Third, and most importantly, it shows that aggregate null results mask substantial heterogeneity: employment gains are concentrated among Indigenous and rural populations for whom baseline labour market integration was most constrained and educational gains from the reform were largest. The absence of significant aggregate effects in León Bravo (2025) is therefore consistent with the findings here once heterogeneity is accounted for.

The present paper also connects to a broader body of work evaluating successive expansions of Mexico's compulsory education system. Behrman et al. (2024) evaluate the 2002 preschool mandate using a difference-in-discontinuity design and find sustained gains in cognitive test scores, noncognitive skills, and, nearly two decades later, high school completion and college enrolment. Garcez et al. (2025) evaluate a reform that made upper-secondary education compulsory, finding that municipalities with greater expansion in high school capacity experienced a 2.8 percent reduction in teenage birth rates relative to low-exposure municipalities. Together, these studies document that each successive step of Mexico's compulsory education ladder generated meaningful human capital gains and downstream behavioural changes. The present paper occupies the middle rung of this sequence (the extension of lower-secondary education) and, unlike either study, follows the affected cohorts across demographic, labour market, and migration domains and into the schooling of the next generation.

While these studies establish the broader relevance of compulsory schooling expansions in Mexico, they share a limitation common to much of the compulsory schooling literature: a focus on a single life domain and a single generation (Black & Devereux, 2011; Holmlund et al., 2011; Oreopoulos, 2006). A smaller set of papers traces a single shock across multiple life domains and generations within one causal design (Akresh et al., 2023; Bandiera et al., 2020; Bell et al., 2023), but none exploits a schooling mandate in a large middle-income country. This paper provides the first such evidence from a legal schooling mandate in one of Latin America's largest economies.

The empirical strategy exploits the birth-cohort discontinuity created by the reform in a two-stage design. A regression discontinuity estimates the reform's impact on educational attainment; the reform eligibility indicator then serves as an instrumental variable for years of schooling in estimating effects on fertility, child mortality, labour market participation, sectoral allocation, geographic mobility, and –following theories of intergenerational human capital transmission (Becker & Tomes, 1986; Black & Devereux, 2011)– the schooling outcomes of the next generation. Throughout, the identifying variation is the same –proximity to the 1981 birth-year cutoff– and both stages include municipality fixed effects and are estimated on the 2020 Mexican Census, a dataset of over one million individuals observed in adulthood with their co-residing children linked through household identifiers. The estimates identify local average treatment effects for compliers: individuals whose educational attainment increased because of the reform. They should be interpreted as net causal effects of education rather than decompositions into specific mechanisms. The design is supported by balance tests showing no discontinuities in predetermined characteristics at the cutoff, and estimates are robust to alternative bandwidth choices and supported by placebo tests that yield null effects at false cutoffs.

This paper makes three contributions. First, it traces the consequences of a single reform through the sequenced margins of the life course within one causal framework; this life-course perspective is rare in the compulsory schooling literature. Because every margin is identified from the same reform, cohorts, and data, the estimates map where along the life course the compulsory schooling expansion operated and where it did not, a comparative structure that cannot be assembled from single-outcome studies estimated across heterogeneous settings.

Second, the study documents that the reform's educational and downstream gains were largest among Mexico's most structurally disadvantaged populations (Indigenous communities and rural localities), narrowing longstanding gaps in educational attainment and the outcomes that follow from it. Third, it provides causal evidence on the long-term and intergenerational impacts of compulsory schooling in a large middle-income country, showing that the responses documented in high-income settings (fertility adjustment, occupational upgrading, intergenerational transmission) also operate in a context of high informality, institutional constraints, and deep inequality.

The remainder of the paper is organised as follows. Section II describes the institutional context and data. Section III outlines the empirical strategy. Sections IV and V present and discuss the results. Section VI concludes.

II. Institutional Background and Data

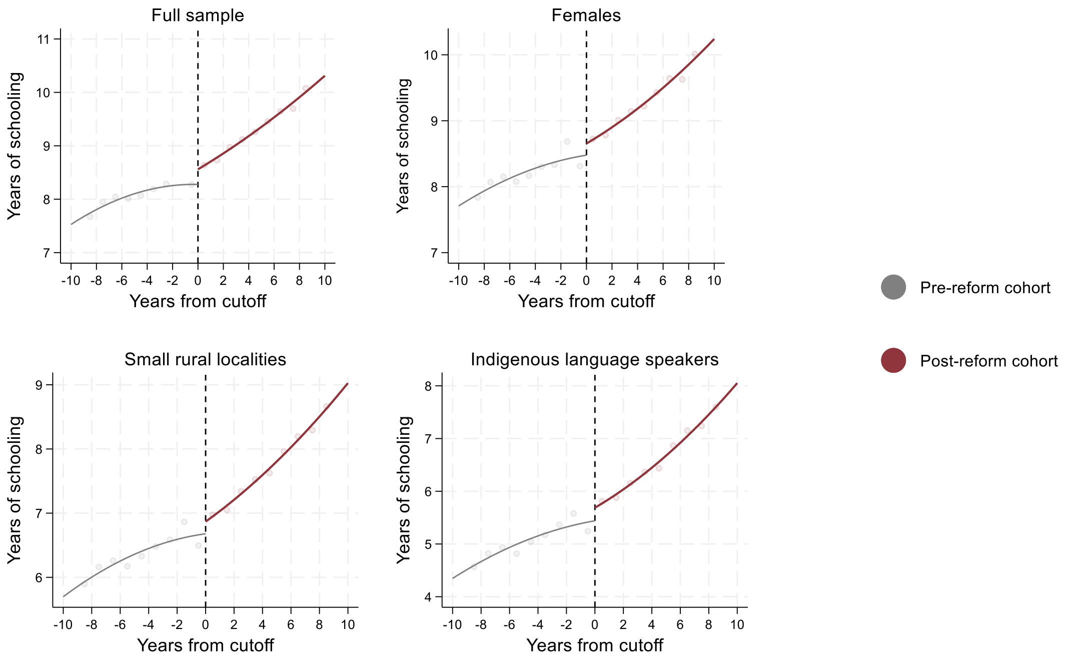
In 1993, Mexico enacted a major reform to its General Education Law, extending compulsory schooling from six to nine years and making lower secondary education (grades 7–9) mandatory nationwide. The new requirement applied to all children younger than 12 at the start of the 1993 school year, creating a 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: while only 4% of the 1930 cohort completed nine or more years of schooling, 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). Because compliance varied with geography and school infrastructure, the empirical approach identifies a local average treatment effect (LATE) for those induced to complete schooling by the reform. Imperfect compliance does not undermine the validity of the design but shapes the interpretation of the estimates: the LATE captures the effect of schooling for individuals at the margin of compliance, rather than the average effect across the full population.

The 1993 reform extended compulsory schooling within a broader policy framework that included complementary provisions relevant for interpreting the estimated effects. Lower secondary education was already provided free of charge under Article 3 of the Mexican Constitution, which guaranteed free public education at all levels; the 1993 General Law of Education reinforced this entitlement by making attendance compulsory and clarifying that states were obligated to provide basic education services, including secondary. Infrastructure investment accompanied the mandate, including an expansion of the *telesecundaria* programme –a distance-learning model using televised instruction that had operated since 1968– to reach rural and remote communities lacking conventional secondary schools (Fabregas & Navarro-Sola, 2025); Gleditsch et al., 2022). Importantly, the reform did not immediately improve teacher quality: formally trained subject teachers for the new secondary curriculum were not available until a separate teacher education reform in 1999, six years after the 1993 mandate (Santibañez et al., 2005). This suggests that the effects estimated here are driven primarily by expanded access to schooling rather than concurrent improvements in school quality, which limits the scope for quality-based alternative channels.

Figure 1. Years of Schooling Around the Reform Cutoff



Note: Each point reports the mean years of schooling for a one-year birth cohort, plotted relative to the 1981 eligibility cutoff (0). Lines show quadratic fits estimated separately for cohorts on either side of the eligibility cutoff (marked with a dashed vertical line). The groups presented are that of the full sample and those for which the reform's effects appear to be larger, which also correspond to the settings where baseline schooling levels were lowest and marginal gains from compulsory schooling reforms are expected to be highest. Appendix Figure A1 presents the full set of subgroup results, including Males, Urban, and Non-Indigenous populations, to assess heterogeneity comprehensively. For visual clarity, the cohort at –2 years, whose mean is higher than average, is omitted from the "full sample" panel in this figure. This cohort is, however, included in all baseline regressions, and in Figure A1.

The eligibility cutoff generated a large and statistically significant increase in completed schooling among eligible cohorts. Figure 1 plots mean completed schooling by birth cohort against distance to the 1981 eligibility cutoff. The figure conveys an upward shift in attainment at the cutoff, together with a steeper post-cutoff slope, consistent with a discrete increase in compulsory schooling exposure for the cohorts just eligible for the reform. This visual evidence is aligned with the identifying assumptions of a regression discontinuity design and motivates the use of a post-1980 eligibility binary indicator as the RDD treatment and IV instrument in the empirical strategy.

While the reform was implemented simultaneously across all states, the speed of school expansion varied across municipalities and later cohorts may have been exposed to higher treatment intensity than earlier ones as infrastructure expanded over several years (Fabregas & Navarro-Sola, 2025). The estimation addresses this in four ways: municipality fixed effects and controls for locality size absorb time-invariant differences in educational and economic conditions; the baseline estimation window covers cohorts within ±3 years of the cutoff; a piecewise linear specification for age allows for different pre- and post-cutoff slopes, accommodating secular trends in implementation intensity; and the model is re-estimated using a narrower bandwidth (±1 year around the cutoff), reducing potential contamination from adjacent cohorts. Estimates remain quantitatively similar across specifications.

Data

The analysis uses the 2020 Mexican Population and Housing Census, accessed through the Integrated Public Use Microdata Series (IPUMS) and collected by the National Statistics Office (INEGI), covering approximately 126 million residents.[4] The census provides detailed demographic, educational, labour market, and migration information, as well as household composition and housing characteristics.

The analysis centres on the 2020 census because it is the earliest wave in which the reform cohorts (born 1978–1983) can be observed at an age where a full range of long-term outcomes is meaningfully measurable. Observed at ages 37–42 in 2020, these individuals have completed their schooling, largely completed their fertility, established their labour market trajectories, and have children old enough for some of their educational outcomes to be observable. Each of these conditions is necessary for the life-course perspective that motivates the paper.

Earlier census rounds do not satisfy these requirements for the cohorts of interest. In the 2010 census the reform cohorts are 27–32 years old –an age at which fertility is still incomplete for a meaningful share of women, labour market trajectories are not yet settled, and the intergenerational analysis is infeasible since co-residing children would not yet have reached school age. The 2015 intercensal update improves on this, but the cohorts are still only 32–37, leaving the intergenerational outcomes only partially observable. The 2020 census is the only wave that permits a comprehensive life-course analysis of this cohort, and is therefore used as the primary data source throughout.

Beyond age-incompleteness, earlier waves introduce a systematic selection concern for several key outcomes. Observed outcomes in earlier waves are not a random sample of eventual outcomes –they reflect the subset of individuals who have already reached particular life-course transitions by a younger age. For fertility and intergenerational outcomes this is particularly acute: at ages 27–32 and 32–37, women with completed fertility or with children old enough to be observed in secondary education are selected towards earlier childbearers and earlier parents – groups that are systematically different from the full population in ways that may bias estimates. The reform's effects operate precisely through delaying and reshaping these transitions, meaning that samples selected on early transition are least representative. By ages 37–42 in 2020 these selection concerns are substantially reduced, providing a more complete and representative sample for the outcomes through which the reform's effects are expected to operate.

Specifically, the outcomes of interest are educational attainment, which 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). Fertility 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 (e.g. agriculture, manufacturing, construction, etc.). While census earnings data suffer from high non-response, and is therefore not included in the analysis, 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.

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

[4] The 2020 Population and Housing Census accessed through IPUMS is a 10 percent representative subsample. It was conducted by INEGI between 2 and 27 March 2020, largely before the onset of major COVID-19 restrictions in Mexico.

household composition changes arising from older children leaving the parental household. 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). This age restriction aims to enhance internal validity, though estimates could be influenced if leaving-home patterns vary systematically with parental education. Within the estimation bandwidth, post-reform parents are on average three years younger than pre-reform parents, so their co-residing children are correspondingly younger –a mechanical feature of the sample rather than evidence of differential retention driven by parental education.

Figure A2 provides direct evidence on co-residence selection by plotting literacy rates and lower-secondary completion rates of co-residing children by single year of age, for children of parents aged 37–42. Both outcomes evolve smoothly and predictably with child age: literacy rises steeply between ages 6 and 11 and then plateaus completely, while secondary completion rises between ages 13 and 16 and stabilises thereafter. Neither outcome shows any discontinuity nor change in gradient within the age brackets used for estimation. If co-residence selection were driving the results –for instance, if more educated parents were disproportionately retaining higher-attaining children at home at older ages– one would expect to see literacy and completion rates of co-residing children rise discontinuously at the ages where selective departure begins. The absence of any such pattern suggests that the co-residing sample is not systematically more advantaged at older ages, and that the intergenerational estimates reflect parental human capital transmission rather than compositional change in who remains in the household. This is corroborated formally in Appendix Table A1, which regresses child age, literacy, and school enrolment on the parental reform indicator conditional on child age. The reform predicts child age in both father-child and mother-child pairs, consistent with the mechanical age difference within the bandwidth. Conditional on child age, the reform indicator is statistically indistinguishable from zero for literacy and upper-secondary enrolment across all pairs, and for secondary enrolment among father-child pairs. The one exception –a small positive coefficient on secondary enrolment for mother-child pairs– is directionally consistent with the main intergenerational results and is plausibly interpreted as a signal of educational transmission rather than as evidence of co-residence selection bias.

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

Sample Birth years	Full sample (1978–83)	Males (1978–83)	Females (1978–83)	Pre reform (1978–80)	Post reform (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 (%)	.	.	6.41	.	.
Child mortality (%)	.	.	2.85	.	.
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. Fertility outcomes statistics estimated for female parents only. "Indigenous language" = % speaking an Indigenous language in addition to Spanish. Locality categories reflect population size.

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 different windows (±1, ±10, doughnut). 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; 96.8% are literate; and 74.3% 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%. Missing statistics correspond to fertility measures that are constructed only for the female sample.

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 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.

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.

The 1990 and 2000 Mexican Population and Housing Censuses, also accessed through IPUMS, are used for an auxiliary purpose: to construct pre-reform municipality-level trends in employment, migration, and education, which are used to validate the identifying assumption that reform uptake is not systematically correlated with pre-existing local trajectories (Table A1.1).

The running variable for the regression discontinuity design is year of birth. The census does not record month of birth, so treatment exposure is measured with error for individuals born near the cutoff; because this error is unrelated to potential outcomes, it attenuates the estimates toward zero, making the results conservative. All estimates are weighted using census expansion factors to ensure national representativeness.

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 extensive robustness analysis, including municipality-level fixed effects and subgroup analysis.

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. Treatment assignment is assumed to be 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:

$$\mathrm{Reform_i} = \begin{cases} 1 \text{ if age} < 12 \text{ in } 1993 \\ \quad 0 \text{ otherwise} \end{cases}$$

The baseline regression discontinuity (RDD) specification is:

$$\mathrm{Y_i} = \alpha + \tau \mathrm{Reform_i} + \beta_1 \mathrm{a_i} + \beta_2(\mathrm{Reform_i} * \mathrm{a_i}) + \Omega \mathrm{X_i} + \mu + \epsilon_\mathrm{i} \dots \text{(1)}$$

where $\mathrm{Y_i}$ denotes the outcome for individual i in municipality m, $\mathrm{Reform_i}$ is the treatment indicator, $\mathrm{a_i}$, is year of birth centred at the cut-off, and $\mathrm{X_i}$ are sex and locality size controls. The piecewise linear specification for age allows for different pre- and post-cutoff slopes, accommodating secular trends.

Municipality fixed effects[5] (μ) absorb time-invariant local heterogeneity. The model is estimated using survey weights, with standard errors clustered at the municipality level. The baseline estimation window includes individuals aged 37–42 in 2020, providing a symmetric bandwidth around the cutoff (Figure A3). Under standard assumptions τ captures the causal effect of exposure to the reform on outcomes near the cutoff. Alternative specifications of the sample are constructed to test the stability of the estimates. In one, the sample is narrowed to ±1 year around the cutoff, yielding estimates based on a temporally tighter comparison (Figure A4). In a second, the sample is expanded to ±10 years around the cutoff, with age controlled for with a linear trend, to test whether the estimate is confounding variation from the discontinuity with general trends by cohort (Figure A5). Two placebo tests are performed by shifting the reform timing three years forward and backward within the main cohort window (Figures A6 and A7). In both cases, no significant discontinuities are detected in educational attainment at either placebo cutoff,

[5] 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.

confirming that the estimated discontinuity at the true cutoff is not an artefact of spurious cohort trends in schooling.

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 A2 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. Taken together, the absence of discontinuities in predetermined characteristics provides no evidence of sorting or differential composition at the cutoff, supporting the continuity assumption underpinning the RDD. Appendix Table A3 documents the distribution of birth cohorts around the cutoff. Although census ages are self-reported and exhibit mild heaping at round ages, particularly $t-2$, this pattern likely reflects classical measurement noise rather than manipulation since heaping appears to be orthogonal to reform exposure. As a robustness check against residual noise near the cutoff, a specification excluding the closest adjacent cohorts is estimated (Figure A8).

In the second stage, the reform serves as an instrument for completed years of schooling in a two-stage least squares (2SLS) framework, following a well-established literature using schooling reforms as instruments (e.g. Begerow & Jürges, 2022; Lleras-Muney, 2005; Oreopoulos, 2006).

First stage:

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

Second stage:

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

Both stages include municipality fixed effects (μ) and controls for sex and locality size $(\mathrm{X_i})$, with standard errors clustered at the municipality level and survey weights applied throughout. The first-stage F-statistic exceeds conventional weak-instrument thresholds (see Appendix Tables A4, A4.1 and A4.2). This approach identifies the LATE: the mean effect of an additional year of schooling for individuals whose educational attainment increased due to the reform.

Table A5 examines the determinants of reform uptake by regressing the municipality-level first-stage coefficient on pre-reform baseline characteristics from the 1990 census. Uptake was systematically higher in municipalities with lower educational attainment, higher shares of Indigenous and rural population, greater agricultural employment, and lower rates of outmigration. Since individuals are more likely to be compliers if they live in high-uptake municipalities, these patterns suggest that compliers are disproportionately drawn from Indigenous and rural communities –precisely where the heterogeneity analysis documents the largest effects. Specifically, municipalities with higher baseline literacy, secondary completion, average years of schooling and large urban population shares had correspondingly lower uptake, consistent with the reform having less margin to operate where attainment was already relatively high.

Together, these patterns paint a consistent picture of the complier population, which has two implications for interpretation. First, the LATE identified here reflects returns to schooling for a population starting from low educational baselines and facing substantial barriers to labour market integration –a context where the marginal returns to crossing the lower secondary threshold may be larger. Second, the heterogeneity results for Indigenous and rural populations, which show the largest effects, are not incidental but reflect the composition of the complier population rather than differential treatment effects.

The empirical strategy is designed to answer two distinct questions. The RDD estimates the first-order effect of the reform on educational attainment among eligible cohorts. The IV then uses the reform-induced variation in schooling to estimate the causal effect of education itself on downstream outcomes: fertility, labour market participation, migration, and intergenerational transmission. While the RDD identifies what the reform did to schooling, the IV identifies what additional schooling does to life trajectories.

The IV strategy imposes an additional requirement beyond the RDD continuity assumption: the reform must affect downstream outcomes solely through its impact on educational attainment. Three threats to this exclusion restriction merit explicit discussion.

First, the reform may have operated through channels beyond individual schooling gains – including changes in school quality or cohort composition. School quality improvements and infrastructure expansion accompanied the 1993 reform, including the *telesecundaria* programme and secondary school construction in underserved areas. However, as noted in Section II, teacher training for the new secondary curriculum was not implemented until 1999, six years after the reform, limiting the scope for quality improvements to operate as an independent channel. Further, any infrastructure improvements were gradual and geographically uneven, and the narrow bandwidth around the eligibility cutoff limits differential exposure to cohort-level spillovers. The municipality fixed effects absorb time-invariant local differences in school quality and infrastructure. The robustness checks discussed below support the interpretation that the estimated effects operate through individual schooling gains rather than through broader environmental changes. Further, the placebo approach is applied to the downstream IV outcomes as well, and show that neither placebo cutoff produces systematic significant effects on fertility, labour market, or migration outcomes (Figure A9).

Second, the gradual rollout raises the concern that cohort-level variation in treatment intensity may be correlated with other cohort-level trends that independently affect adult outcomes. The piecewise linear age specification accommodates differential pre- and post-cutoff trends, and the stability of estimates across the ±1, ±3, and ±10 year bandwidth specifications (Figures A3–A5) confirms that the results are not driven by secular trends in the intensity of reform implementation.

Third, reform uptake could reflect pre-existing differences in local labour market trajectories rather than a response to the policy itself. For policy eligibility instruments of this type, Borusyak and Hull (2023) point that a relevant validity check is whether predetermined variables predict uptake; that is, whether municipalities with systematically different pre-reform trajectories were also those where the reform induced larger increases in schooling. To implement this test directly, a municipality-level measure of reform uptake is constructed using equation (1)[6] and regressed on municipality-level pre-reform changes in employment, migration, and educational attainment measured from the 1990 and 2000 censuses. These three outcomes are the most likely to reflect pre-existing differences in local labour demand trajectories and therefore provide the most direct test of the identifying assumption. None of the pre-reform trend variables is individually significant, and they are jointly insignificant predictors of uptake ($F = 0.82$, $R^2 = 0.001$, $N = 2,112$ municipalities; Table A1.1), supporting the assumption that differential uptake was not driven by pre-existing municipal trajectories. The pre-reform trend variables are measured over 1990–2000, a window that partly overlaps with the early years of the reform's implementation; to the extent that this introduces any contamination, it biases the test towards finding a relationship between uptake and pre-reform trends; the null joint F-statistic is therefore a conservative bound on the absence of pre-existing differential trends.

[6] Municipality-level uptake is estimated for municipalities with at least 100 individuals in the estimation sample.

For completeness, Appendix Tables A19–A22 report reduced-form regressions of reform eligibility on each downstream outcome, estimated with the same controls, fixed effects, and weights as the IV specifications. These intention-to-treat estimates are directionally consistent with the IV results throughout, and their magnitudes correspond to the IV estimates scaled by the first stage.

The IV framework is applied to both direct and intergenerational outcomes. In both cases the main explanatory variable, $\mathrm{Schooling_i}$, refers to parental education. In the first case, $\mathrm{Y_i}$ represents adult outcomes such as fertility, employment, and migration patterns. In the latter case, $\mathrm{Y_i}$ 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.

A potential concern is that children of post-reform parents in the upper-secondary age bracket (16–18) were themselves exposed to Mexico's 2012 reform making upper secondary education compulsory, which could independently raise their enrolment rates and confound the intergenerational estimates. The identification strategy is robust to this concern by construction: the intergenerational estimates are identified from a discontinuity at the *parental* birth-year cutoff, so any contemporaneous policy shift that affects children of the same age on both sides of that cutoff –including the 2012 upper-secondary reform– cancels out in the comparison and cannot generate bias.[7] What remains after netting out this common exposure is the variation of interest: differences in child outcomes attributable to the parental education gains induced by the 1993 reform, holding child cohort fixed.

This is corroborated empirically by Figure 8 and Tables A17–A18, discussed below, which report estimates separately across all child age groups, including literacy (ages 9–15) and lower-secondary enrolment (ages 13–15), age groups unaffected by the 2012 upper-secondary mandate. The estimates for these younger age groups are consistent with those for upper-secondary enrolment, supporting the interpretation that the intergenerational effects reflect parental human capital transmission rather than concurrent child-level policy exposure.

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 evidence that large-scale compulsory schooling reforms can alter life trajectories in ways consistent with these mechanisms. In this manner, the study 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).

A set of considerations warrant caution in interpretation and extrapolation beyond the setting. First, all estimates are local average treatment effects for compliers rather than average treatment effects for the full population. The complier characterisation (Table A5) shows that this population is drawn disproportionately from Indigenous, rural, and low-attainment contexts.

[7] All specifications additionally control for parental age at birth, which accounts for the possibility that younger post-reform parents differ from older pre-reform parents in the timing of childbearing and the resulting age composition of co-residing children.

Whether the LATE lies above or below the population-average effect is theoretically ambiguous: compliers faced previously binding constraints, suggesting high marginal returns, but were also concentrated in labour markets that may reward additional schooling less generously. The estimates are therefore best read as the causal effects of schooling for the marginal, relatively disadvantaged student reached by a compulsory schooling expansion. While this population is of direct policy relevance, it is not representative of the average Mexican student.

Second, the exclusion restriction cannot be verified directly. Interpreting the IV estimates as effects of schooling requires that cohort exposure operates only through attained schooling. The principal threat to the IV interpretation is bundling with other components of the 1993 package. The reduced-form estimates of the reform's effects, by contrast, do not rely on this assumption. Sections II and IV argue that the most salient of these, teacher quality and infrastructure, are unlikely to operate as independent channels for the cohorts studied. More fundamentally, the schooling induced by any compulsory reform is delivered within a particular institutional configuration and cannot be separated from it. The estimates are thus best interpreted as the effect of reform-induced schooling under the 1993 framework.

Third, the 2020 Census observes individuals residing in Mexico in 2020. If the reform altered population composition, for instance via selective emigration or mortality, the sample would differ across the cutoff. Because the running variable is year of birth, fixed before the reform was announced, the composition of cohorts cannot have been manipulated ex ante, and a compositional difference must instead arise from endogenous responses (that is, from selection out of the observed sample). Such selection would be expected to surface as imbalance in predetermined characteristics, which does not appear to be the case for the characteristics observed (Table A2). A parallel concern applies to the intergenerational sample, where children are observed only while co-residing. The co-residence diagnostics for the sample (Figure A2) show that outcomes of co-residing children are flat across child age within the estimation brackets, indicating that age-related selection into co-residence is unlikely to bias the intergenerational estimates. Further, while the cohort-size distribution (Table A3) exhibits mild heaping at round ages, it is consistent with classical age misreporting rather than differential selection. In any case, results are robust to excluding cohorts adjacent to the cutoff (Figure A8) and to alternative bandwidths (Figures A4 and A5). Residual compositional selection cannot be excluded entirely, but to be meaningfully present it would have to affect outcomes while leaving observed predetermined characteristics, cohort sizes, and co-residence patterns undisturbed across the cutoff, a demanding combination that bounds its likely quantitative importance.

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

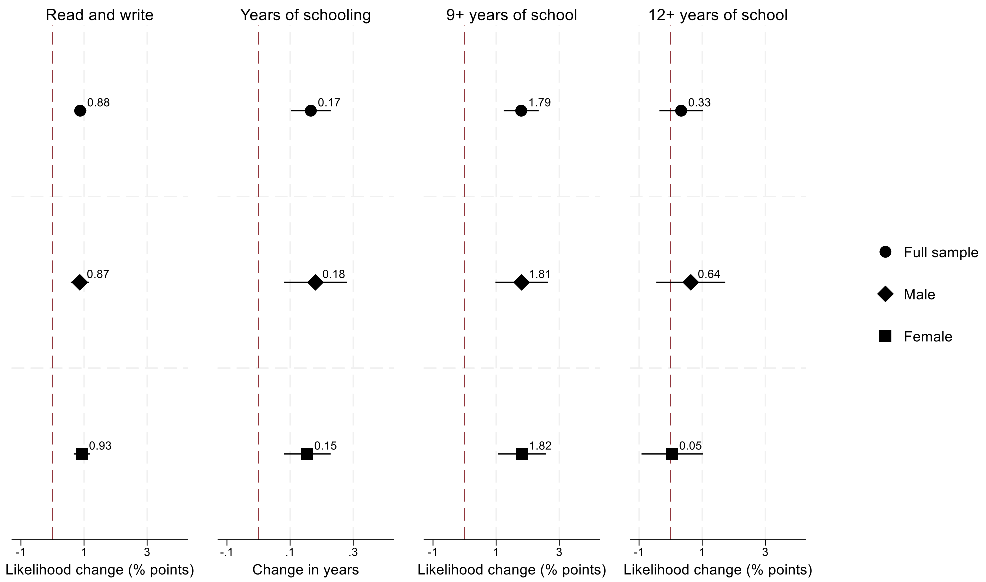
IV. Results

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.

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 aligned across locality types, with larger magnitudes in rural areas. Significant increases in upper secondary completion appear only in small rural localities and among Indigenous groups (for subgroup analysis see Appendix Table A6). The overall effects are especially pronounced among Indigenous individuals, for whom 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 percentage points (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 alternative bandwidths (see Appendix Figures A3, A4 and A5).[8]

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



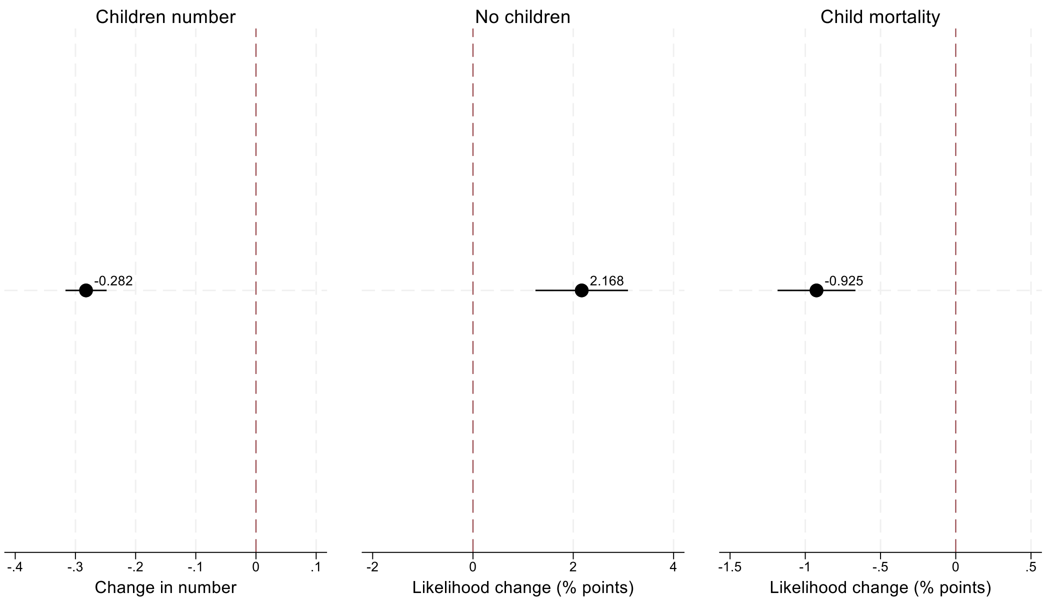
Note: Each panel plots regression discontinuity estimates of the effect of the reform on educational outcomes. Dependent variable: (a) =1 if knows how to read and write, =0 otherwise; (b) number of years of completed education (ranges 0 to 18); (c) =1 if has completed at least nine years of schooling, =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.

Beyond attainment, additional schooling reshaped fertility outcomes through both number of children 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

[8] 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

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 (Appendix Tables A7 and A8 provide subgroup analysis and OLS estimates). Jointly, the estimates demonstrate that increased schooling translated into smaller family sizes and higher child survival, with the largest gains concentrated among disadvantaged groups.

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



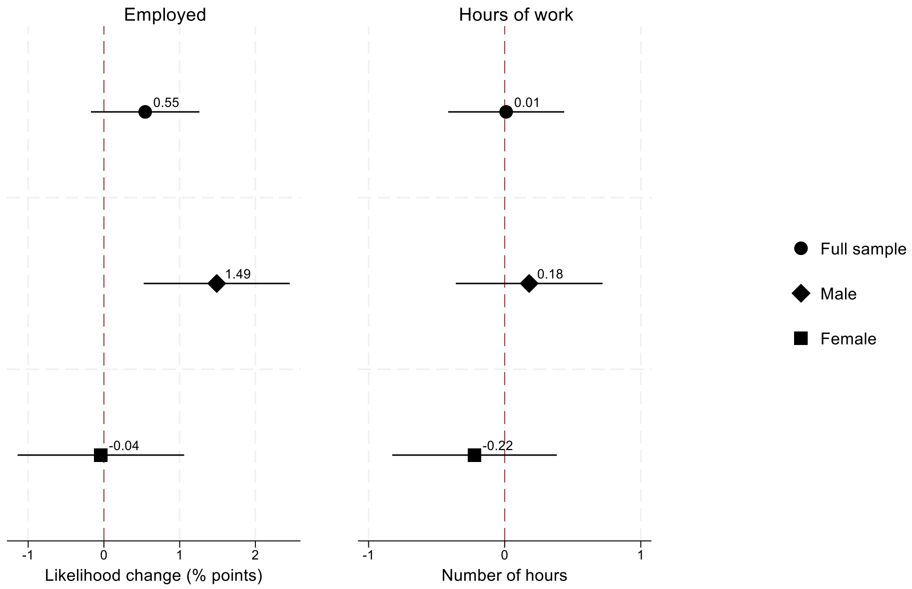
Note: Each panel presents instrumental variable estimates of the effect of completed years of education (0–18) on the (1) number of children living in household; (2) a binary indicator equal to 1 if the individual reports no children ever born, and 0 otherwise; (3) percentage of children born no longer 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 fertility estimates should be interpreted with some caution regarding right-censoring. Women in the sample are aged 37–42 in 2020, an age by which most fertility is complete, but a small share of the youngest post-reform cohort (born 1981–83, aged 37–39) may not yet have completed childbearing. Estimates for this cohort may therefore be slightly conservative. However, the child mortality and childlessness outcomes, which do not suffer from right-censoring in the same way, show consistent effects in the expected direction across all subgroups, supporting the interpretation that the estimated fertility effects reflect genuine reform-induced changes.

An early childbearing variable, such as age at first birth, cannot be constructed reliably from census data. Identifying age at first birth requires knowing which co-residing child is the first-born, but older children –those born when the mother was a teenager– are the least likely to remain co-resident at the time of the census. The resulting measurement error would be concentrated among the women most likely to have experienced early childbearing, introducing non-classical measurement error.

The link between schooling and labour market outcomes is conveyed in Figures 4 and 5. Average effects on employment are significant for males and particularly sizeable and precisely estimated for specific groups, with the largest gains observed among Indigenous individuals, for whom an additional year of schooling increased the probability of employment by 5.5 percentage points (baseline: 41%, see Appendix Tables A9 and A10). Focusing on weekly hours worked, the estimate for Indigenous individuals is 1.16 (baseline: 39.7). Across all subgroups, no group shows a reduction in employment.

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

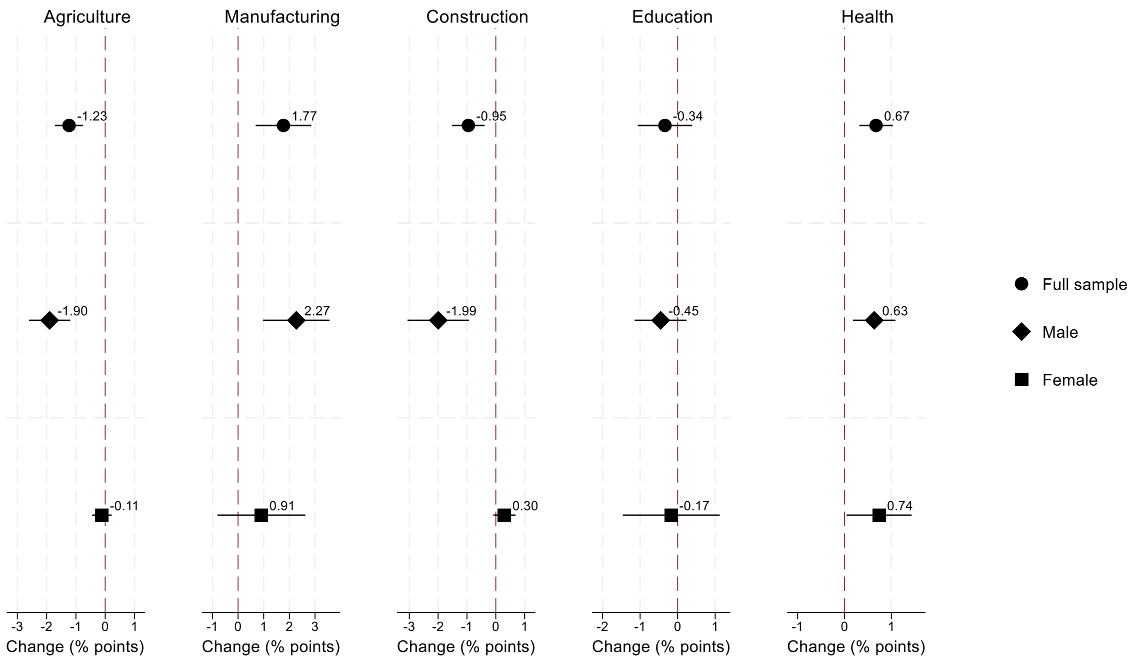


Note: Each panel reports instrumental variable estimates of the effect of completed years of education (0–18) on (a) the probability of being employed (defined as reporting any paid work), and (b) total weekly hours worked among those employed. Estimates are shown for the full sample of individuals aged 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.

Beyond employment levels, 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%). As expected, larger declines are observed among small rural communities (-2.9%, baseline: 34%) and Indigenous populations (-2.8%, baseline: 39%), where agriculture accounts for a large share of jobs.[9] 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 for detailed analysis). Overall, the results indicate that the reform facilitated a structural reallocation away from low-productivity agriculture and into more diversified, skill-intensive sectors.

[9] De la Fuente Stevens and 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 language 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 if 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 for locality size and sex. Standard errors are clustered at a municipality level. Confidence intervals at a 95% level.

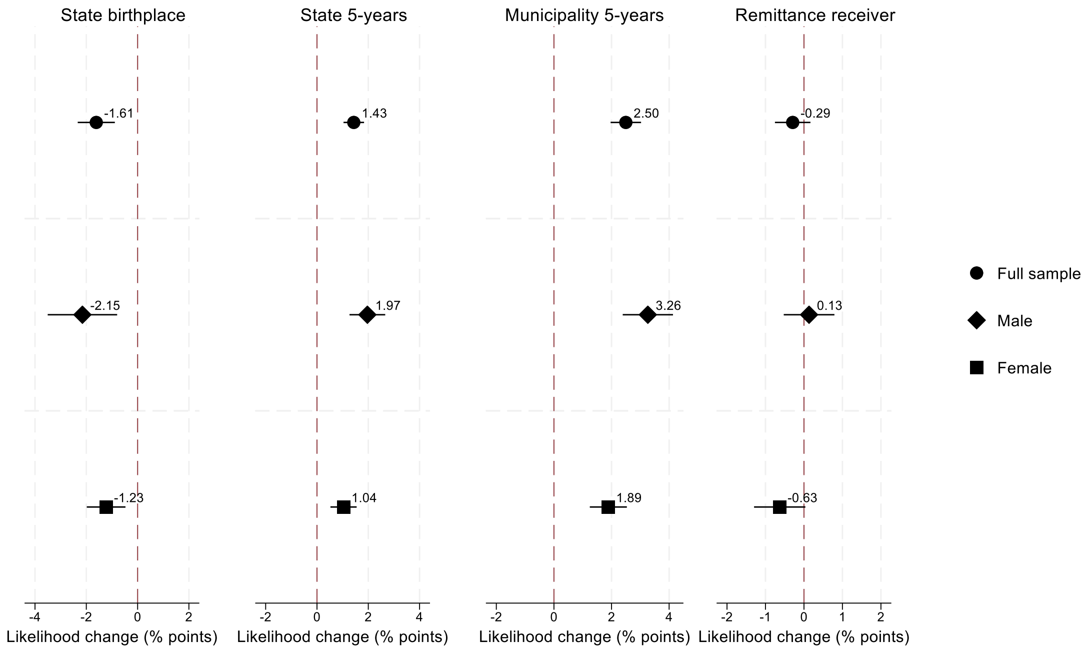
Education also reshaped mobility patterns (Figure 6). Long-term migration –living outside one's state of birth– fell by 1.61 percentage points (baseline: 13%), with larger declines for males (–2.15, baseline: 13%) and urban residents (–3.19, baseline 28%). In contrast, recent mobility increased. The 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.

The estimate for migration from birth state should be interpreted with some caution. Placebo tests reveal a negative coefficient for this outcome at both placebo cutoffs, suggesting the result is not specific to the reform cohort. Two complementary factors may explain this pattern. First, this variable measure lifetime residential mobility –whether an individual currently lives outside their state of birth– which accumulates mechanically with age: older individuals have had more years to have moved away from their birth state and not returned, a nonlinear effect that may not be fully absorbed by the linear age control. Second, this pattern is consistent with a secular decline in long-distance internal mobility across cohorts in Mexico, where successive generations show declining rates of permanent internal migration.

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.[10] 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. Yet, together, the findings indicate that rising education reduced exposure to international migration networks while fostering more adaptive patterns of internal mobility.

Figure 6. Estimated Effects of Education on Migration and Remittances



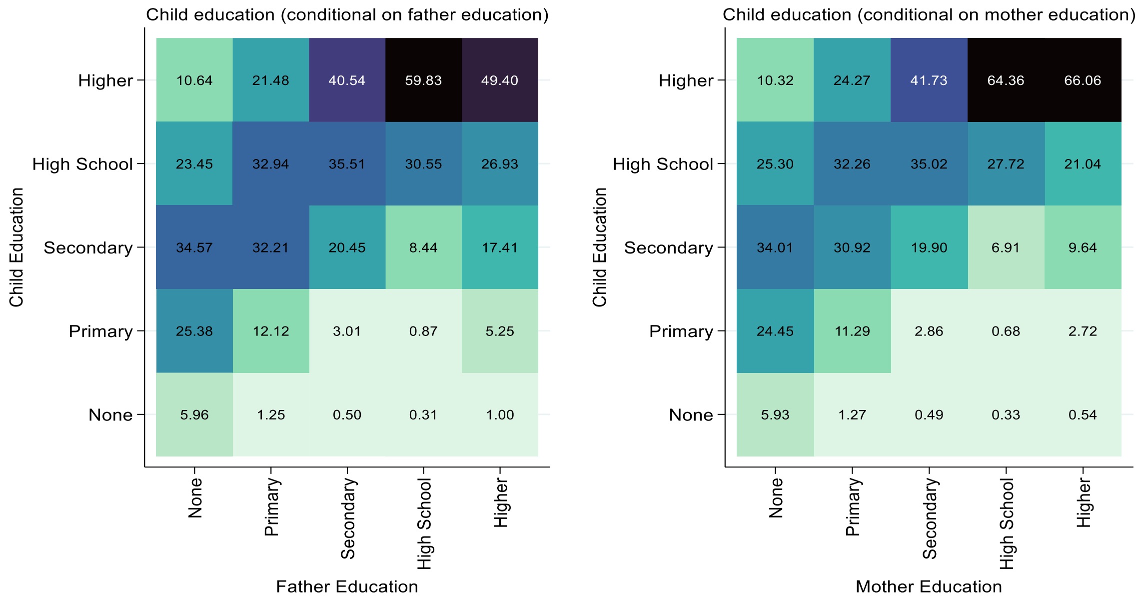
Note: Each panel reports instrumental variable estimates of the effect of completed years of education (0–18) on migration and remittance outcomes. State birthplace indicates whether an individual resides in a state different from their state of birth; State 5-years and Municipality 5-years indicate 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 children's schooling conditional on parental education, and the gradient is steep: among parents with no schooling, roughly 10% of children attain more than 12 years of education (Higher), for fathers and mothers alike, while among parents with higher education the share reaches 49% for fathers and 66% for mothers. These descriptive gaps are substantial, but they conflate causal transmission with sorting and unobserved family background; the estimates that follow use the reform-induced variation in parental schooling to isolate the causal component.

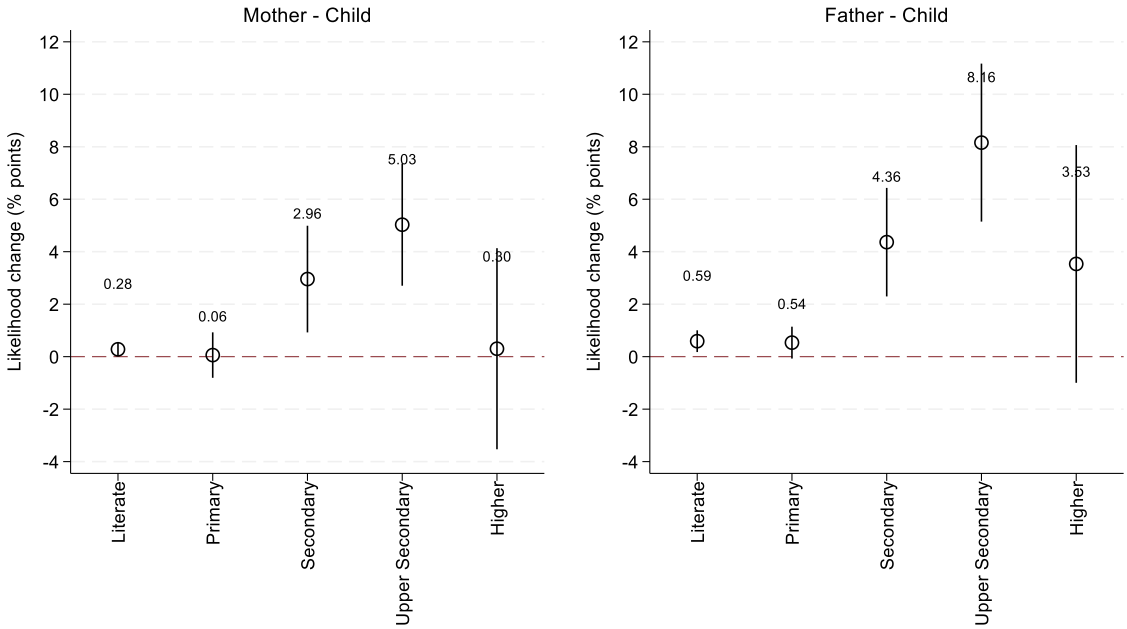
[10] This pattern aligns with broader evidence that remittance dependence is more prevalent in smaller municipalities than in larger urban centres (De la Fuente Stevens, 2026).

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.

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 percentage 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 effects are consistent in direction across mother–child and father–child pairs, and are especially pronounced for families in small rural localities (Appendix Tables A15–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 at upper levels of education are therefore best read as lower bounds on intergenerational transmission.

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.

V. Discussion

The 1993 reform extended compulsory schooling in Mexico from six to nine years and produced lasting increases in educational attainment that reverberated across adult and intergenerational domains. A single policy instrument that raised mandatory schooling by three years set in motion a sequence of changes spanning fertility, child survival, labour market participation, sectoral allocation, and the schooling of the next generation. Tracing this sequence within a unified causal framework is the central analytical contribution of the paper: rather than isolating a single outcome, the analysis follows the reform's consequences along the arc of adult life. The estimates identify local average treatment effects for individuals whose schooling rose because of the reform –primarily those from disadvantaged backgrounds where the reform had most margin to operate– and should be interpreted as net causal effects of education rather than decompositions into specific mechanisms.

The reform's most immediate downstream effects were demographic. Fertility declined (evidenced by smaller family sizes and a higher probability of remaining childless) and child survival improved across all subgroups. These patterns align with well-established mechanisms: 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 the household resources and maternal human capital available for child health (Currie & Moretti, 2003).

The fertility estimates are consistent with evidence from schooling reforms. Fort et al. (2016) find an estimate of approximately -0.3 children per year of schooling in England –strikingly close to the full-sample estimate of -0.28 here– while finding null or positive effects in Continental Europe where baseline attainment was already high and fertility-education complementarities dominate. Akresh et al. (2023) document reductions in household fertility for women exposed to school construction in Indonesia, despite finding no effect on age at first marriage, suggesting the fertility-reducing effect operates through channels beyond timing of union formation alone. Black et al. (2008) document reductions in teen birth probability of 3.5–4.7% from compulsory schooling reforms in Norway and the United States, directionally consistent with Garcez et al.'s (2025) finding of a 2.8% reduction in teenage birth rates following Mexico's upper secondary reform. The full-sample estimate of -0.28 children therefore sits within the range established by comparable studies, while the larger effect among Indigenous women (-0.40) reflects the pattern documented across this literature that fertility responses to education are largest where baseline attainment is lowest and opportunity costs of childbearing were most constrained.

On child mortality, Currie and Moretti (2003) find that an additional year of maternal education reduces low birthweight by approximately 10% and preterm birth by 6% in the United States, operating through changes in maternal behaviour and improved household resources. Breierova and Duflo (2004), examining primary school construction in Indonesia, find no significant effect on completed fertility but do find reductions in child mortality driven primarily by women's education. The estimates here extend this pattern to a middle-income Latin American setting, with child mortality declining consistently across all subgroups and effects largest among Indigenous and rural women, consistent with the maternal human capital channel identified across both studies.

Labour market outcomes moved in the direction of occupational upgrading, with sectoral reallocation out of agriculture into higher-productivity activities constituting the most consistent and precisely estimated effect. At the aggregate level, the modest employment effects estimated here align with León Bravo (2025), who finds no significant aggregate labour market impact of the same reform using the ENOE survey (a result shown here to reflect the composition of that sample rather than the absence of effects, since gains are concentrated among Indigenous and rural workers underrepresented in the ENOE sampling frame). In high-income contexts, Oreopoulos (2006) documents substantial employment and earnings gains from compulsory schooling reforms in Canada, the United Kingdom, and the United States, but the institutional distance (formalised labour markets, stronger enforcement, higher baseline attainment) limits direct magnitude comparisons. A more instructive comparator is Akresh et al. (2023), who find that men exposed to Indonesia's school construction programme are 1.2 percentage points less likely to work in agriculture and 1.1 percentage points more likely to work in the formal sector –a pattern strikingly similar to the reallocation documented here. The full-sample decline in agricultural employment of 1.23 percentage points matches Akresh et al.'s estimate almost exactly, and the larger declines among Indigenous (-2.8 percentage points) and small rural populations (-2.9 percentage points) follow the same logic: reallocation is most pronounced where agriculture accounted for the largest share of employment prior to the reform. In contrast to Akresh et al., where gains shift primarily towards services, the reallocation here is concentrated in manufacturing for men (+2.27 percentage points), with smaller but consistent gains in the health sector across both genders, reflecting differences in the sectoral composition of the Mexican and Indonesian economies. The gender asymmetry in sectoral reallocation is also consistent across both studies: employment and occupational upgrading effects are substantial for men (+1.49 percentage points) but negligible for women (+0.04 percentage points), though the mechanisms underlying this asymmetry –whether reflecting labour market informality, sectoral segregation, or social norms around female employment– lie beyond the scope of the current analysis.

The most enduring effects of the reform materialised across generations. An additional year of parental schooling raised the probability of children's secondary school enrolment by 3–4 percentage points and upper-secondary enrolment by 5–8 percentage points, with effects largest for families in small rural localities. 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 results support 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. Benchmarking these estimates requires some care given the data architecture. Unlike studies using administrative records linking parents and children across registers (Black et al., 2005; Holmlund et al., 2011), the estimates here are based on co-residing parent-child pairs observed in the census, an approach also used in comparable development contexts where administrative links are unavailable (Alesina et al., 2021). The closest contextual comparator is Akresh et al. (2023), who find that children of mothers exposed to Indonesia's school construction programme obtain an additional 0.17 years of schooling, a meaningful second-generation effect that persists despite attenuation across generations, and where maternal exposure drives intergenerational transmission more strongly than paternal exposure, a pattern that also resonates with the estimates here. More broadly, Alesina et al. (2021) document that within-country variation in intergenerational mobility across 27 African countries is substantial and concentrated among the most disadvantaged populations, echoing the finding here that intergenerational effects are largest for Indigenous and rural families.

Geographic mobility responded in a more nuanced way, and the migration results should be interpreted with caution. Education reduced long-term displacement from the place of birth yet increased short-term internal migration across both state and municipality boundaries, consistent with schooling enhancing responsiveness to labour-market opportunities rather than reducing overall mobility (Aydemir et al., 2022; Machin et al., 2012).

Across all outcomes, the reform's effects were largest among Indigenous communities and rural populations. Several factors explain this concentration. Indigenous communities in Mexico are disproportionately deprived and located in remote areas where secondary school access was historically limited (Narro et al., 2013). Language barriers and the higher opportunity costs of schooling in subsistence agricultural households further constrained secondary enrolment before the reform, meaning the mandate had substantially more margin to operate in these contexts than in urban or non-Indigenous settings. The expansion of the *telesecundaria* programme –a distance-learning model specifically designed to reach remote and rural communities– may have been instrumental in extending secondary education to these populations.

The heterogeneity findings have implications beyond Mexico: countries characterised by comparable patterns of indigenous minority populations, geographic remoteness, and persistent educational gaps (including much of Latin America, parts of Sub-Saharan Africa, and South and Southeast Asia) face structurally similar constraints on intergenerational mobility. The evidence here suggests that compulsory schooling mandates accompanied by supply-side expansion can generate large returns in these contexts, provided the infrastructure investment reaches the communities where the gap between actual and potential attainment is widest.

This paper connects with the large literature on compulsory schooling reforms and extends it to a middle-income setting characterised by high structural inequality and lower baseline attainment. The magnitudes are consistent with evidence from comparable middle-income settings and remain economically meaningful, even if smaller than estimates from high-income studies where institutional environments and baseline attainment are more favourable. Collectively, the results demonstrate that the core mechanisms linking education to long-run outcomes (reduced fertility, improved child survival, occupational upgrading, intergenerational human capital transmission) are not exclusive to high-income institutional environments. They operate in Mexico, and their social returns are largest precisely where educational investment has historically been most absent.

From a policy perspective, the results suggest that raising compulsory schooling can yield broad and lasting private and social returns. The heterogeneity patterns highlight that gains were concentrated among those starting furthest behind, underlining the importance of complementary interventions: investments in school quality, targeted support for Indigenous and rural communities, Indigenous-language instruction, and measures to expand access to post-secondary education so that gains at the secondary level translate into further educational advancement. The labour market results also point to the role of local demand conditions in shaping returns: without local demand for skilled labour, the potential of schooling reforms will be under-realised. By demonstrating multi-domain, intergenerational benefits the findings strengthen the case for education policy as a central lever for reducing structural inequalities.

VI. Conclusions

Mexico's 1993 compulsory schooling reform was, in aggregate terms, a moderate intervention. Observed at the national level, it added an average of 0.17 years of completed schooling to eligible cohorts –a small shift in a country where the majority of the population was already literate and primary education was near-universal. Yet its consequences were neither moderate nor uniform. By operating at the lower end of the educational distribution –among Indigenous communities, rural populations, and households where secondary completion was the exception rather than the rule– the reform set in motion a sequence of changes that extended across the full arc of adult life and into the schooling of the next generation. Fertility declined by approximately one fifth of a child per woman, child survival improved, and workers shifted out of low-productivity agriculture into higher-productivity sectors. These gains extended to the next generation: the probability of children completing secondary school rose by 3 to 4 percentage points, and upper-secondary enrolment by 5 to 8 percentage points. These are not isolated effects; they are a coherent trajectory, and they were largest where the gap between actual and potential attainment was widest.

The conceptual implication is straightforward but consequential. The mechanisms linking education to long-run outcomes –higher opportunity costs of early fertility, access to higher-productivity labour markets, greater household investment in children– are not exclusive to high-income settings with strong institutions and formalised economies. They operate in Mexico, a country characterised by deep inequality, high informality, and persistent intergenerational immobility. And in that context, their effects are largest where the marginal return to additional schooling was steepest: among the populations that markets and prior policy had consistently failed to reach. For Indigenous individuals alone, the reform induced gains in schooling approximately three times larger than the national average (0.50 additional years), translating into a 4.5 percentage point increase in literacy and correspondingly larger effects on fertility, labour market outcomes, and geographic mobility.

The policy implications follow directly. Compulsory schooling mandates are among the most scalable instruments available to governments seeking to raise attainment at the lower end of the educational distribution. This paper shows that such mandates can generate broad, lasting, and intergenerational returns –and that those returns are highest for the groups that start furthest behind. The heterogeneity documented here is not a caveat; it is the central finding. Complementary investments –in school quality, Indigenous-language instruction, and local labour market development– would operate precisely where the marginal return to additional schooling is highest. The gains documented here were achieved by a mandate alone; what becomes possible when that mandate is matched by investments designed to amplify it remains one of the biggest open questions in education policy.

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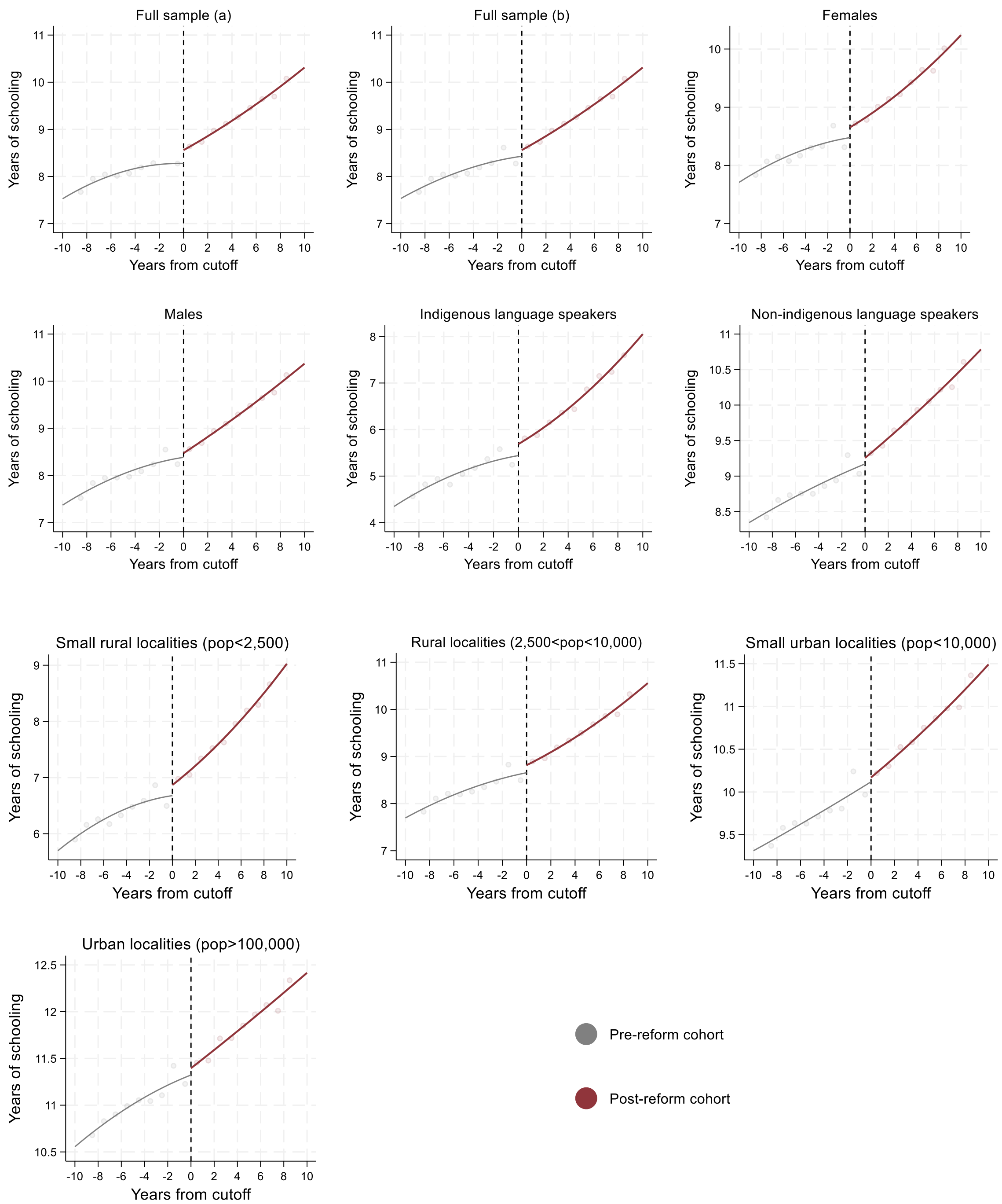
Appendix

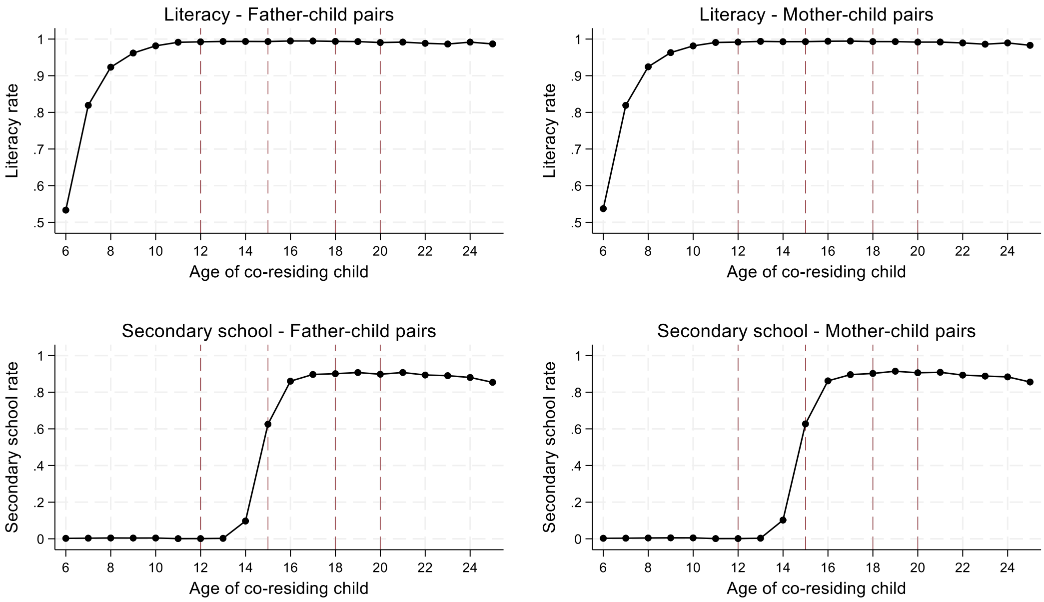
Figures and Tables

Figure A1. Years of Schooling Around the Reform Cutoff. Full subgroup analysis.



Note: *This figure reproduces the discontinuity plots for the full set of subgroups using the complete ±10-year window around the 1981 eligibility cutoff. Each point reports the mean years of schooling for a one-year birth cohort, and lines show quadratic fits estimated separately for cohorts on either side of the cutoff (marked with a dashed vertical line). All cohorts, including the cohort at –2 years, whose mean is higher than average, are included here. As shown, including this cohort in the full-sample plot ("Full sample (b)") does not alter the qualitative pattern relative to the version shown in panel "Full sample (a)" which corresponds to the first panel in the main-text figure (Figure 1). These panels complement the main-text analysis by providing a comprehensive view of heterogeneity across Males, Urban, and Non-Indigenous populations, in addition to the groups highlighted in the main figure.*

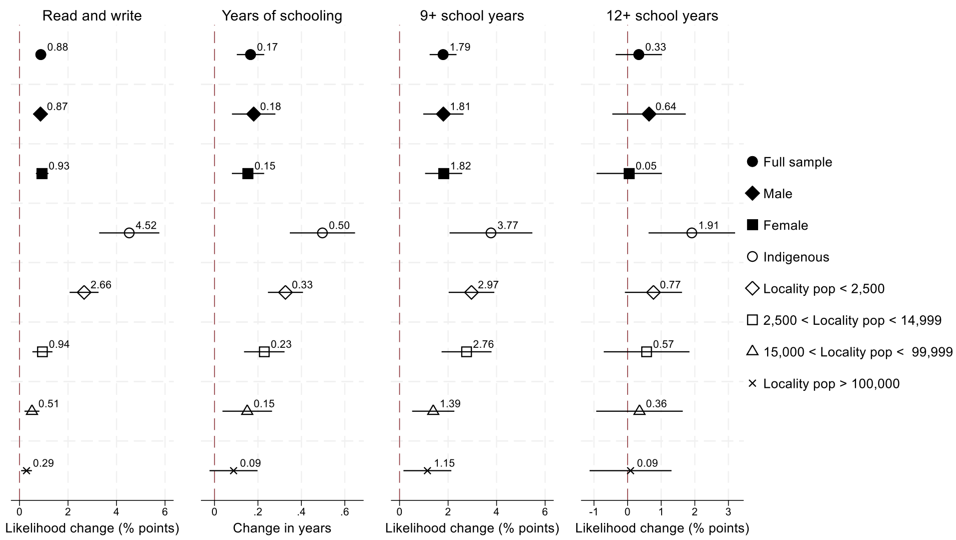
Figure A2. Co-residing Children: Literacy and Completion of Secondary School by Age



Note: sample corresponds to children of parents aged 37–42 in 2020 census. Father-mother pairs shown separately. The dashed vertical lines in Figure A2 mark the boundaries of the age brackets used in the intergenerational analysis: ages 6–12 (primary), 13–15 (lower secondary), 16–18 (upper secondary), and 19–20 (higher education).

Figure A3. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

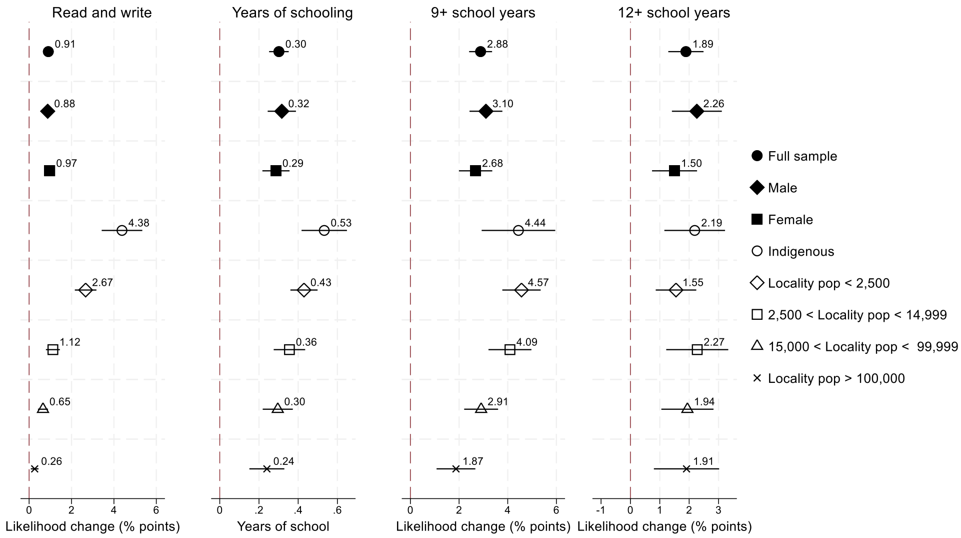
Panel A: Baseline, bandwidth ± 3 years (full subsamples)



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.

Figure A4. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

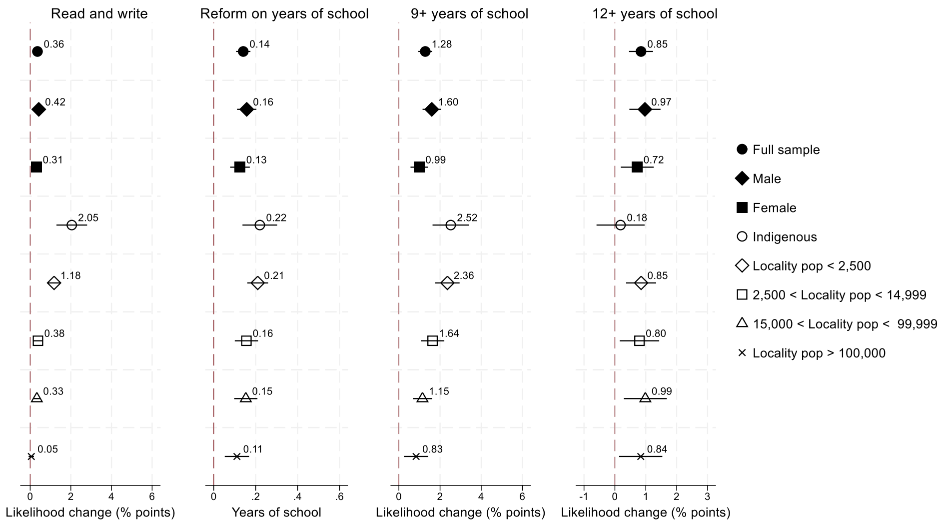
Panel B: Bandwidth ± 1 years (narrow window)



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 39 and 0 for those aged 40. 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.

Figure A5. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

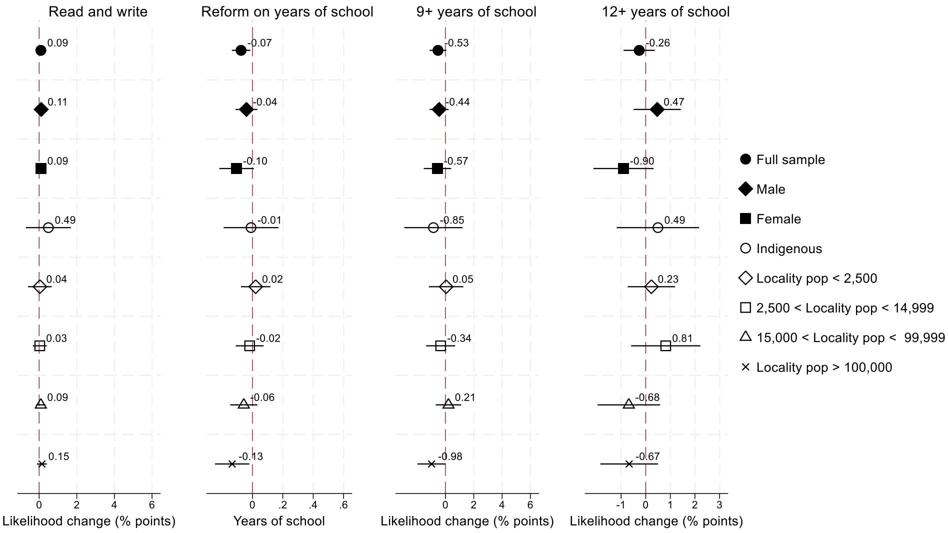
Panel C: Bandwidth: ±10 years (large window)



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 30–39 and 0 for those aged 40–49. 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.

Figure A6. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

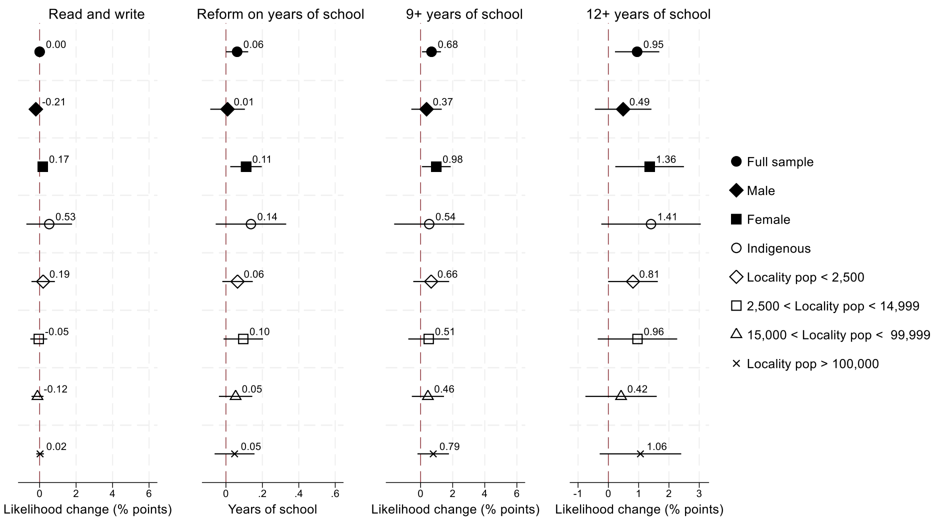
Baseline specification. Binary indicator pushed forward three years (placebo tests)



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 40–42 and 0 for those aged 43–45. 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.

Figure A7. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

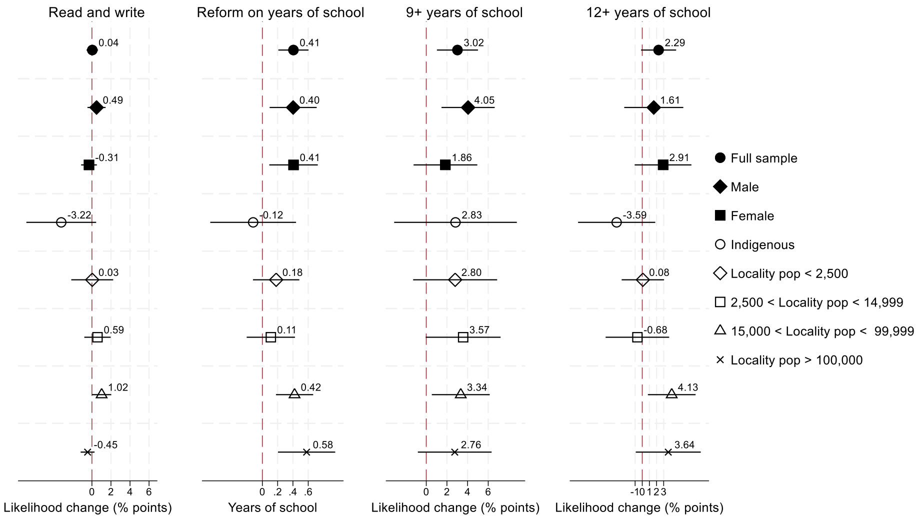
Baseline specification. Binary indicator pushed backward three years (placebo tests)



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 34–36 and 0 for those aged 37–39. 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.

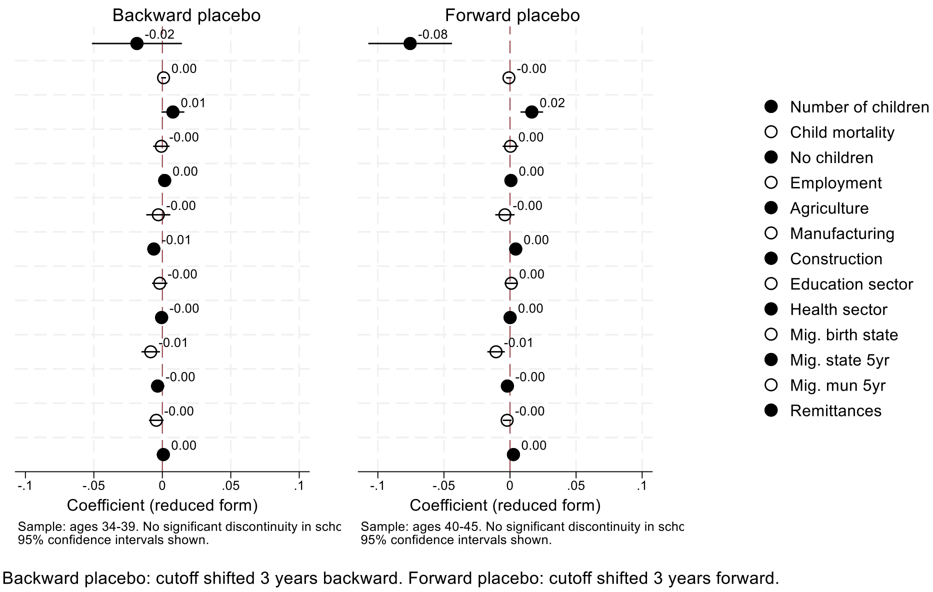
Figure A8. Estimated Impact of the 1993 Schooling Reform on Educational Attainment

Panel D: Doughnut sample (excludes two cohorts adjacent to cutoff), bandwidth: ± 2 years (heaping check)



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 36–37 and 0 for those aged 42–43. 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

Figure A9. Placebo Tests: Reduced Form Estimates for IV Outcomes



Note: Each panel reports reduced-form estimates of a placebo reform indicator on all IV outcomes. The backward placebo shifts the reform cutoff three years backward (sample: ages 34–39); the forward placebo shifts it three years forward (sample: ages 40–45). Both placebo cohorts are unaffected by the 1993 reform. Hours worked is excluded as its scale differs from the binary outcomes. The negative coefficient on migration from birth state reflects a secular decline in long-distance mobility across cohorts unrelated to the reform, which appears at both placebo cutoffs. All remaining outcomes are statistically indistinguishable from zero in at least one placebo direction, with no consistent pattern of significant results across both tests. 95% confidence intervals shown.

Table A1. Balance Test: Reform and Co-residing Child Characteristics

	(1) Age (F)	(2) Age (M)	(3) Literacy (F)	(4) Literacy (M)	(5) Secondary (F)	(6) Secondary (M)	(7) UpperSec (F)	(8) UpperSec (M)
Reform (Father)	-0.1379***		0.0030		0.0000		-0.0040	
	(0.0509)		(0.0022)		(0.0034)		(0.0032)	
Reform (Mother)		-0.1072**		-0.0004		0.0063**		0.0038
		(0.0530)		(0.0018)		(0.0026)		(0.0024)
N	791737	1003390	789945	1000949	791737	1003390	791737	1003390

*F = Father-child pairs. M = Mother-child pairs. All regressions control for parental age trend and fixed effects. Test 1 (Age, col 1–2): reform predicts child age - expected given mechanical age difference. Test 2 (Outcomes, cols 3–8): reform predicts outcomes conditional on child age - key null result. ** p<0.05, *** p<0.01*

Table A1.1. Pre-reform Trends and Municipality-level Reform Uptake

ΔEmployment (1990–2000)	0.187
	(0.217)
ΔMigration (1990–2000)	-0.489
	(0.387)
ΔYears of Schooling (1990–2000)	-0.010
	(0.026)
Constant	0.244***
	(0.048)
Observations	2112
R-squared	0.001
F-statistic	0.82

*Notes: Dependent variable is municipality-level reform uptake measured using the model described in equation (1). Independent variables are municipality-level changes in migration (living in a different state to where born), education (average years of schooling) and employment rate between 1990 and 2000. Regressions are weighted by the precision of the first-stage estimates (1/s.e.^2) . Standard errors in parentheses. * p<0.10 ** p<0.05 and *** p<0.01*

Table A2. 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 A3. 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 A4. 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 A4.1 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: 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 A4.2 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: 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. Municipality-level Characteristics and Reform Uptake: Complier Analysis

Municipality characteristic	Coefficient	Std. Error	N
Panel A. Educational characteristics:			
Average years of schooling	-0.0567***	(0.0078)	2113
Literacy rate	-0.5776***	(0.0824)	2113
Lower secondary completion	-0.6333***	(0.1050)	2113
Panel B. Demographic and geographic characteristics:			
Indigenous language speakers	0.1685***	(0.0429)	2113
Bilingual indigenous speakers	0.1369**	(0.0610)	2113
Monolingual indigenous speakers	0.5440***	(0.1060)	2113
Rural population share	0.2556***	(0.0432)	2113
Large urban share (>100,000)	-0.1845***	(0.0524)	2113
Panel C. Labour market characteristics:			
Employment rate	-1.0022***	(0.2073)	2113
Migration rate	-0.4601***	(0.0920)	2113
Agriculture share	0.2993***	(0.0537)	2113

*Note: Each row reports the coefficient from a separate OLS regression of municipality-level reform uptake on the indicated characteristic, weighted by the precision of the uptake estimate ($1/se^2$). Reform uptake is the municipality-level first-stage coefficient from equation (1), estimated for municipalities with at least 100 observations in the estimation sample. All characteristics are constructed from the 1990 Mexican Population and Housing Census –three years prior to the reform– and collapsed to the municipality level using census expansion weights. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.*

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

	(1A)	(2A)	(3A)	(4A)		(1B)	(2B)	(3B)	(4B)
	Literacy	Years Schooling	Nine +	Twelve +		Literacy	Years 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 on Fertility Outcomes 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, and (c) share of children born that are no longer living (child mortality). The sample includes women aged 37–42 observed in the 2020 Mexican Population and Housing Census. 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. Instrumental Variables: Education on Fertility Outcomes 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

*Note: The table reports instrumental variable estimates of the effect of years of schooling and (a) number of children per woman residing in the household, (b) likelihood of having no children, and (c) share of children born that are no longer living (child mortality). The sample includes women aged 37–42 observed in the 2020 Mexican Population and Housing Census. Years of schooling is instrumented by reform eligibility, defined as an indicator equal to 1 for individuals born in 1981 or later (post-reform cohorts). 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 among those employed. The sample includes individuals aged 37–42 observed in the 2020 Mexican Population and Housing Census. 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. Instrumental Variable 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	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	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	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	N	142687	109246
F	4.70	2.84	F	0.74	1.69

*Note: The table reports instrumental variable estimates of the effect of years of schooling and (a) whether the individual is doing paid work and (b) the number of hours worked among those employed. The sample includes individuals aged 37–42 observed in the 2020 Mexican Population and Housing Census. Years of schooling is instrumented by reform eligibility, defined as an indicator equal to 1 for individuals born in 1981 or later (post-reform cohorts). 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

	Agriculture	Manufacturing	Construction	Education	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 and 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

	Agriculture	Manufacturing	Construction	Education	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 and 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

	State Birthplace	State 5–years	Municipality 5–years	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 and (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)	(2)	(3)	(4)
	State	State	Municipality	Remittance
	Birthplace	5–years	5–years	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 instrumental variable estimates of the association between years of schooling and (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: Father-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.590**	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: Mother-child

	(1) Literacy	(2) Primary	(3) Secondary	(4) Upper Secondary	(5) Higher
Baseline: full sample of eligible parent-child pairs					
Years of school	0.277*	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 father mother 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 Fertility Outcomes and Child Mortality

	(1)	(2)	(3)		(1)	(2)	(3)
	Children Number	Child Mortality	No Children		Children Number	Child Mortality	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 born 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)	(2)		(1)	(2)
	Employment	Number of Hours		Employment	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	N	554473	488569
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	274325	N	223510.00	124856
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	N	317078.00	215204
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	N	143064.00	109536
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 + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.*