

DO CONDITIONAL CASH TRANSFERS IMPROVE ECONOMIC OUTCOMES IN THE NEXT GENERATION? EVIDENCE FROM MEXICO*

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Conditional cash transfer programmes have spread to over 60 countries in the past two decades, but little is known about their long-term effects. We estimate the lasting impact of childhood exposure to the Mexico’s flagship programme Progresa by leveraging the age structure of benefits and geographic variation in early programme penetration nationwide. Childhood exposure improves women’s outcomes in early adulthood with increases in educational attainment, geographic mobility, labour market performance and household living standards. For men, effects are smaller and more difficult to distinguish from spatial convergence.

Conditional cash transfer (CCT) programmes were first introduced two decades ago and have since spread around the world, now operating in more than 60 countries, in many cases representing a key government strategy for reducing poverty. By linking monetary transfers to children’s human capital investment, the programmes aim to both alleviate current poverty and reduce future poverty by increasing the human capital levels of children and thus their lifetime earnings potential. One of the earliest of these programmes was Mexico’s Progresa, which began in 1997 and is well known due to its 18-month randomised evaluation, the basis for numerous published studies (Parker and Todd, 2017).¹ The programme’s novelty and positive evaluation findings contributed to both a large scale-up within Mexico and the spread of its key features to new programmes around the world. CCT programmes now operate throughout Latin America, in a number of poor countries in Africa and Asia, and even in a few high-income countries, including the United States. Using data on the long-term outcomes of Progresa’s earliest beneficiaries, this paper investigates whether this landmark programme achieved the second of the dual goals: to improve economic well-being in the next generation.

Several studies of Progresa and other CCT programmes have shown positive programme impacts on the education levels of poor youth (Fiszbein and Schady, 2009; Baird *et al.*, 2013; Parker and Todd, 2017). Nevertheless, evidence on whether these increases in education translate to better economic outcomes in the next generation is limited. Because these programmes explicitly seek to improve the economic outcomes of exposed youth when they grow up, information on

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¹ The programme began as Progresa (Programa de Educación, Salud y Alimentación) in 1997, but was renamed Oportunidades in 2001 and Prospera in 2013. It ceased operation in 2019.

long-run outcomes is crucial for assessing their benefits. In part, the lack of evidence reflects the necessity of a long follow-up for measuring impacts on economic outcomes for youth beneficiaries, and long follow-ups of experimental evaluations are costly and organisationally difficult, especially in contexts with high rates of migration. Furthermore, the childhood beneficiaries of these programmes are only now reaching adulthood. A recent literature has begun to follow up the original study cohorts from experimental evaluations of CCTs, although often with limited sampling frames, small samples, or short follow-up periods. We build on these efforts by leveraging Progresa's nationwide roll-out, comparing early beneficiaries to a slightly older group that was too old to benefit much from education transfers, across municipalities with varying early programme penetration. This approach allows us to consider a more sustained intervention than most experimental evaluations of CCT programmes and also to evaluate a nationwide programme at scale, improving external validity and relevance to policymakers (Muralidharan and Niehaus, 2017).

The experimental follow-ups, mostly of CCT pilot programmes, find modest effects on schooling and work.² Barham *et al.* (2017, 2018) study a CCT experiment in 42 Nicaraguan communities, in which households were randomised to early (2000–3) or late (2003–5) receipt of transfers. At ten-year follow-up, men aged 9 to 12 at baseline showed positive effects of early receipt on educational attainment, test scores, off-farm work, labour migration, and income. Women in the same cohort showed positive effects on non-educational outcomes, but weak to null effects on attainment and test scores. The crossover design—with transfers ending for early communities as they started for late communities—complicates interpretation because transfers flowed only to the control group during the late period. A Honduran pilot programme avoids this complication because the programme never expanded to control areas. Ham and Michelson (2018) use municipal level data from the Honduran 2013 census to follow up the 70 study municipalities a decade after they were randomised to demand-side transfers (a conventional CCT), transfers combined with supply-side subsidies, or control, finding positive effects only of the combined intervention on education for both genders and on female work. However, Molina Millán *et al.* (2020) revisit the programme using individual level data from the census and find positive effects of the conventional CCT on education for both genders, international migration for men, and mostly insignificant labour market effects for both genders.³

For Progresa specifically, efforts to track the longer-run outcomes of children in the experimental cohort have proved challenging because of migration. Analyses using programme data face extremely high rates of attrition, losing at least 25% of the cohort within 10 years and over 99% within the overall follow-up period (Rodríguez-Oreggia and Freije, 2012; Kugler and Rojas, 2018). A recent study improves on the programme data by carrying out a 20-year tracking survey of youth from the 506 evaluation localities (Araujo and Macours, 2021).⁴ The survey obtained follow-up data on 94% of individuals who were of school age in 1997, including US migrants: 40% by face-to-face interview, 42% by phone interview, and 12% by proxy.⁵ Results

² A separate literature studies whether early exposure to CCTs as infants leads to better school outcomes when children begin school (Behrman *et al.*, 2009; Sanchez Chico *et al.*, 2020).

³ Other CCT studies showing positive longer term effects on education attainment include Barrera-Osorio *et al.* (2019) on Colombia and Cahyadi *et al.* (2020) on Indonesia. With a shorter horizon, Baird *et al.* (2019) estimate the effects of a two-year CCT programme on adolescent females in Malawi two–four years after the programme ended. While the CCT increased schooling in certain sub-samples, labour market outcomes were unaffected.

⁴ The study also tracked individuals who were in gestation or infancy during the experiment.

⁵ Araujo and Macours (2021) report tracking rates by treatment arm. We take their weighted average assuming that treatment shares among individuals matched those among localities.

show that an additional 18 months of programme exposure at the end of primary school raised subsequent educational attainment, geographic mobility and income, especially for women. The two studies complement each other well. Relative to ours, the other's advantages include its prospective randomised design and its tracking of international migrants; its disadvantages include the short duration of differential programme exposure and its heavy reliance on phone and proxy interviews.⁶

We estimate the long-term impacts of Progresa in a quasi-experimental design using census data linked to administrative data on programme enrolment. We study the educational, labour market, household, and demographic outcomes of the programme's earliest beneficiaries, who were of primary school age when the programme began in 1997 and are now young adults. Following up this group in the early stages of adulthood, we rely on a difference-in-differences design akin to Duflo's (2001) analysis of a school construction programme in Indonesia. Our design is motivated by earlier studies finding few schooling impacts on youth who were offered the programme at age 15 or later, past the critical transition between primary and secondary school (Parker and Todd, 2017). We confirm these findings in our data and thus use these older cohorts as a comparison group. We interact this cohort variation in programme incidence with geographic variation in municipality programme penetration during the Progresa's first phase of roll-out across Mexico.

Using this difference-in-differences strategy, we find that exposure to greater programme penetration before reaching the primary-to-secondary transition improves accumulated education, labour market outcomes, housing characteristics, durable goods ownership and geographic mobility, primarily for women. Compared with those offered the programme too late, women from early beneficiary cohorts in fully treated municipalities complete 1–1.6 additional grades of schooling, 15–20% of mean educational attainment. Women's labour market outcomes also improve, with exposure to full roll-out increasing participation by 5–9 percentage points (21–38% of the mean), work for pay by 6–8 percentage points (39–48% of the mean), and labour earnings by roughly 40% of the mean. We interpret these labour market impacts as reflecting the benefits of additional education, possibly mediated by migration, which also rises with childhood exposure. Moving to household outcomes, we find positive effects on housing conditions and durable goods ownership, although these results may reflect either human capital or the direct benefits of greater parental wealth. For men, we find modest effects on a few outcomes—educational attainment, migration, and housing conditions—but most outcomes fail to show robust evidence of programme impacts.

Because Progresa targeted more disadvantaged areas, our difference-in-differences design risks confounding from differential cohort trends between more and less disadvantaged areas. We address this concern in four ways. First, we restrict our sample to municipalities in the highest categories of the Mexican government's marginality classification where roll-out was most intense. Second, all of our models allow outcome differences between pre- and post-programme cohorts to vary with cumulative municipal enrolment at the end of a second phase of roll-out. As a consequence, identification requires parallel trends only between early and late roll-out municipalities, rather than between early roll-out municipalities and all others. Third, we estimate models that allow cohort trends to vary flexibly with initial marginality, measured at both the municipality and locality levels. Fourth, we assess pre-programme trends by estimating event study models and running falsification checks using a census that took

⁶ The proxy interviews omitted questions about some primary outcomes, including income.

place seven years before roll-out began. For women, we find that the estimates are stable across regression specifications and cannot be attributed to pre-existing trends.⁷ Men's results are less stable and less distinguishable from pre-existing trends.

Our findings of stronger results for women than men match the experimental follow-up by Araujo and Macours (2021). Although these differences may point to Progresa as a driver of gender equality, they may also partially reflect sample selectivity. Our main regression sample contains 16% fewer men than women. This gender imbalance raises questions about selectivity among young men, who emigrate (INEGI, 2009) and risk homicide (INEGI, 2019) at higher rates than young women. The gender imbalance is not associated with programme exposure in our study cohorts, nor is cohort size, allaying concerns about selection directly on treatment.⁸ However, international migration and mortality may induce selection on treatment effects in which the men with the largest potential gains from programme exposure select out of the sample. As a consequence, we interpret the gender differences with caution.

Nevertheless, the gender differences are noteworthy in light of women's historically low status, education and labour force participation in programme areas. In Mexico's poorest states, we find that the closing of the gender gap in secondary school attainment coincided with the onset of Progresa. We estimate that the programme can account for one-third to one-half of this convergence. Gender parity in education was a secondary goal of Progresa: grant amounts were larger for girls than for boys, and mothers rather than fathers received the transfers. Our results suggest that Progresa achieved this goal.

Beyond the literature on CCTs specifically, our results relate to a growing body of work on cash transfers more generally, including those that do not condition on child investment. Debates persist on the pros and cons of conditionality (Baird *et al.*, 2013), but both forms of cash transfers are growing in popularity. Noting the limited evidence on their long-term consequences, Blattman *et al.* (2017) call for a redoubling of efforts to learn about the long run. In the case of unconditional transfers, long-run evidence for the next generation is also thin. In a middle-income country context, Araujo *et al.* (2017) carry out a ten-year follow-up study of Ecuador's cash transfer programme—in which transfers were unconditional, although some beneficiaries mistakenly thought they were conditional—finding mixed results on education and no significant effects on labour market outcomes.⁹ Some evidence is also available from the historical United States, where welfare programmes for mothers (Aizer *et al.*, 2016) and food stamp programmes (Hoynes *et al.*, 2016) had a range of long-term benefits for individuals exposed as children.

Our findings add much-needed evidence on the long-run impacts of a popular anti-poverty policy. While the many studies on the short- and medium-run effects of Progresa and other CCT programmes provide much guidance to policymakers, long-term follow-up on the next generation is crucial to assessing whether the programmes are achieving the second of their dual goals: reducing poverty in future generations. A few recent studies finding smaller-than-expected effects of cash transfers in the longer term (e.g., Haushofer and Shapiro, 2018; Baird *et al.*, 2019) have led to debate over their promise, but this debate concerns whether the effects of short-lived experimental programmes are sustained in the few years after the end of operations. That question is conceptually distinct from ours, which deals with the effects of a long-lasting government programme on the next generation. While studies of other programmes are clearly

⁷ Results are also robust to controlling for political clientelism, school construction, and violence.

⁸ We also take several approaches to ensure that our results are not biased by internal migration.

⁹ Baird *et al.* (2019) also provide results for an unconditional treatment arm, finding no effect on schooling and an effect on early marriage that disappears within two years.

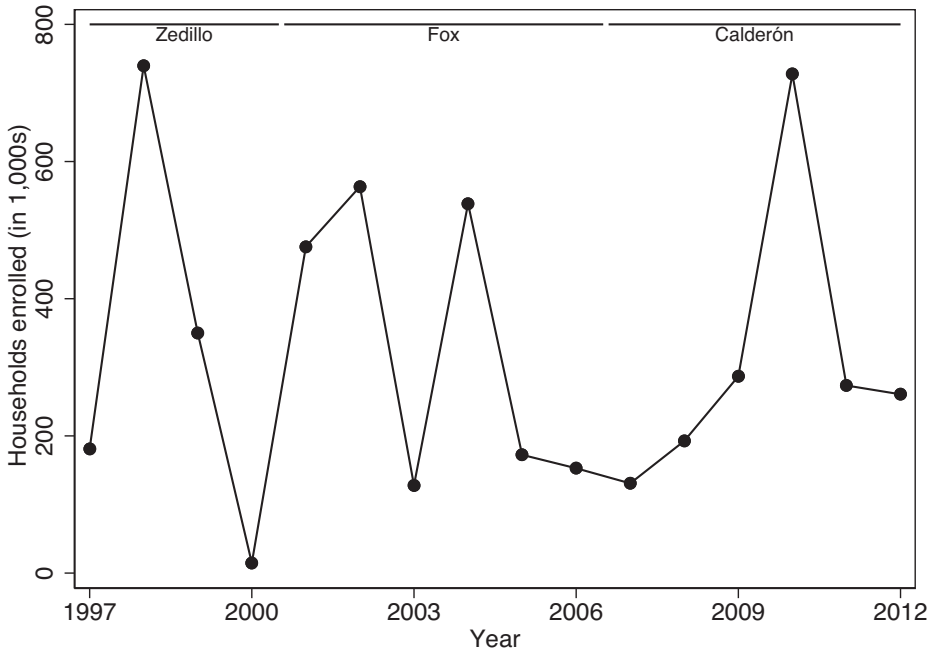


Fig. 1. Number of New Households Enrolled per Year, Nationwide.

Note: Presidential elections occurred in 2000, 2006 and 2012; midterms occurred in 1997, 2003 and 2009.

needed, the results on Mexico’s pioneering programme are encouraging with respect to the potential of CCTs to reduce poverty in the next generation, particularly for women.

1. Programme Background

1.1. Roll-out Patterns

Progresa began operating in small rural communities in 1997, following a macroeconomic crisis in Mexico in 1995, and was part of a transition towards implementing targeted anti-poverty programmes and eliminating general food subsidies. It quickly grew over time and at its peak covered 7 million families, about one-quarter of all families in Mexico. While the programme eventually expanded into urban areas, it remained largely rural throughout, with about two-thirds of its household beneficiaries deriving from communities with less than 2,500 inhabitants. Figure 1 shows the aggregate numbers of households who became beneficiaries in each year through 2012. New enrolment activity was most intense during the first decade of the programme, under the presidencies of Ernesto Zedillo and Vicente Fox, with clear drops in presidential (2000, 2006, 2012) and midterm (2003, 2009, 2012) election years. These drops, which reflect an anti-vote buying policy that prohibits social programme expansion in the lead-up to national elections, provide a rhythm to the roll-out that is useful for our research design. The geographic component of our difference-in-differences design compares areas that enrolled households more intensively under Zedillo (1997–9) to those that enrolled households more intensively under Fox (2000–5).

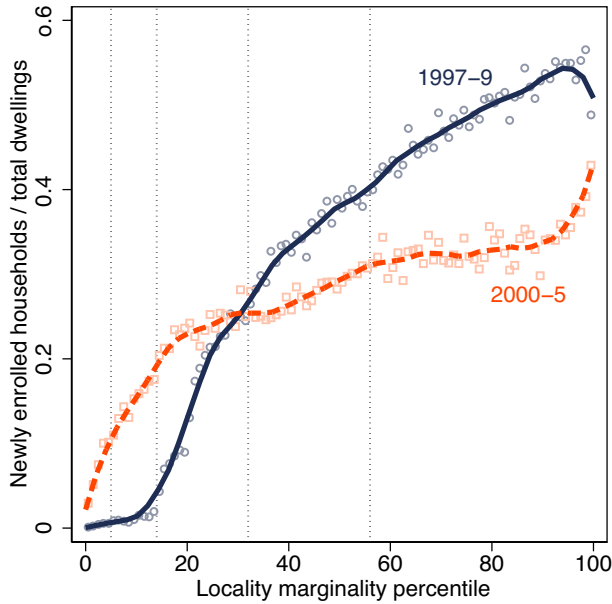


Fig. 2. *Locality Enrolment Ratio Versus Locality Marginality Percentile.*

Note: Curves are local linear regressions with bandwidths of 2.5; each scatter point represents a 1-point bin. Sample includes 58,221 localities with at least 10 dwellings in 1995. The enrolment ratio is the count of new households enrolled divided by the number of dwellings in 1995. Vertical lines represent cut-offs between marginality categories.

The programme was means tested, with both geographic and household-level targeting. The geographic targeting selected poor rural localities in part by using a locality-level marginality index, formed by taking the first principal component of socio-economic aggregates from census data. Within selected localities, Progresa surveyed all households and then used discriminant analysis to distinguish eligible from ineligible households based on characteristics such as dwelling conditions, dependency ratios, ownership of durable goods, animals and land, and the presence of disabled individuals. Skoufias *et al.* (2001) compare the targeting algorithm with consumption- and geography-based alternatives and conclude that the programme performed well in targeting the poorest.

Figure 2 documents how the marginality index related to the selection of localities during the two roll-out phases that are key to our research design. On the horizontal axis is a locality's percentile rank in the distribution of the 1995 locality marginality index, which directly guided geographic targeting. On the vertical axis is the number of households in the locality that were newly enrolled during each phase, divided by the number of dwellings in the locality in 1995.¹⁰

¹⁰ The marginality index and dwelling counts are constructed by the Mexican Population Council (CONAPO) based on the 1995 intercensal survey, which reported dwelling, but not household counts at the locality level. The merge to Progresa's enrolment database is imperfect due to poorly documented changes in locality identifiers and the suppression of CONAPO data for very small localities. After accounting for documented splits and merges, the enrolment database shows 82,648 localities with at least one household enrolled by the end of 2005. We successfully merge 90% of these localities to the CONAPO data. Of the localities which do not merge, 78% have fewer than 10 households enrolled and all have fewer than 250. To construct Figure 2, we assume zero enrolment for unmerged localities and restrict the sample to localities with at least 10 dwellings.

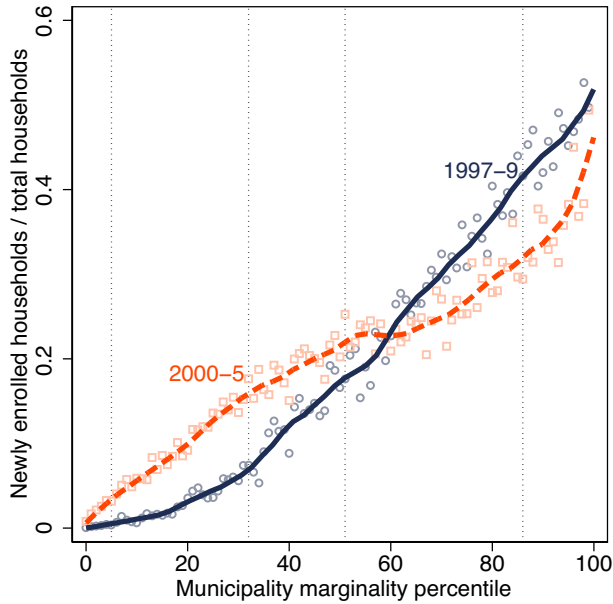


Fig. 3. *Municipality Enrolment Ratio Versus Municipality Marginality Percentile.*

Note: Curves are local linear regressions with bandwidths of 2.5; each scatter point represents a 1-point bin. The enrolment ratio is the count of new households enrolled divided by the estimated number of households in 1997. Vertical lines represent cut-offs between marginality categories.

The scatterplots represent the mean of the new enrolment ratio within each percentile bin, while the curves are local linear regressions. The vertical lines demarcate the five marginality categories designated by CONAPO (the Mexican Population Council), from ‘very low’ to ‘very high’ marginality.

During both roll-out phases, new enrolment activity rose with the marginality index. Consistent with programme protocol, Progresa targeted the top two marginality categories during the first phase of roll-out, with percentile mean enrolment ratios ranging from 0.26 to 0.56. By comparison, percentile means in the bottom two marginality categories were all below 0.02 during the first phase of roll-out. The programme became less geographically targeted in the second phase of roll-out, but new enrolment ratios remained far higher in more marginalised localities.¹¹

Data constraints force us to use municipality-level rather than locality-level variation in our impact analysis. To visualise how the relationship in Figure 2 aggregates to this less granular geography, Figure 3 plots the new enrolment ratio against percentiles of the marginality index at the municipality level.¹² The municipality-level enrolment ratio divides by the estimated number of households in 1997 (interpolated between the 1990 and 2000 censuses), and the municipality marginality index uses socio-economic aggregates from the 1990 census.¹³ As in Figure 2, we

¹¹ Programme documents describing the selection of localities in the first years are vague, but emphasise ‘marginality, population and services’ as well as adjacency to localities already enrolled. Communities with less than 10 households were initially excluded. By the end of 1998, the programme had enrolled families in 30 of 31 states, excluding Mexico City (Progresa, 2000).

¹² Online Appendix Figure 10 maps new enrolment ratios across municipalities in 1997–9 and 2000–5. Geographic patterns are broadly similar, with more enrolment in the poorer west and south.

¹³ We use the 1990 rather than 1995 marginality index because it incorporates more data.

plot percentile means and local linear regressions for each roll-out phase, with vertical lines demarcating CONAPO's five marginality categories.

Similar to the locality-level patterns in Figure 2, the municipality-level patterns for both phases in Figure 3 reveal more intense roll-out in more marginalised places. Two differences emerge, however. First, the municipality-level patterns are more gradual. Steep changes at the boundaries between marginality categories disappear due to aggregation. Second, differences between roll-out phases are less stark in the municipality-level data. In Figure 2, 56 of the locality percentile bins have gaps between roll-out phases of more than 10 percentage points; in Figure 3, only 15 of the municipality percentile bins have gaps exceeding this threshold. The greater overlap between municipalities with similar socio-economic conditions but different roll-out timing is useful for our difference-in-differences design, because municipalities with similar socio-economic conditions may be more likely to follow parallel trends in potential outcomes.

1.2. Age Structure of Benefits

The programme conditioned cash payments to families on children regularly attending school and on family members visiting health clinics for checkups. Programme rules allowed students to fail each grade once, but if a student repeated a grade twice, the schooling benefits were discontinued permanently. The programme also provided some additional subsidies for school supplies and a transfer linked to regular visits to health clinics.¹⁴ Children and youth aged 21 and younger were eligible to receive the school subsidies. Originally, the programme provided grants only for children in grades 3–9, but in 2001, the grants were extended to grades 10–12. As shown in [Online Appendix Table 6](#), grant sizes rose with grade, with discontinuous jumps between schooling levels. By the final year of high school, the grant amounted to roughly two-thirds of Mexico's minimum wage. Beginning with 7th grade (the first year of middle school), the grants are about 15% higher for girls than boys, a feature aimed at reducing attendance gaps between boys and girls in rural areas.

Existing evidence suggests that the programme was particularly important for preventing dropout during the transition from primary to secondary school, between the 6th and 7th grades. The original evaluation of Progresa randomly assigned 506 communities to treatment and control groups. Eligible households in treatment communities began to receive benefits in 1998, while eligible households in control communities began in 2000. Studies of education impacts during the 18-month experiment show large enrolment effects at the transition between primary and secondary school (Schultz, 2004; Behrman *et al.*, 2005) and reductions in grade repetition in primary school (Behrman *et al.*, 2005). Few significant effects were observed for youth who had six or more years of schooling or were older than 15 at the programme's start. In a non-experimental study with a longer follow-up, Behrman *et al.* (2011) find that beneficiary children aged 9–12 at the programme's start accumulated nearly a grade of additional schooling relative to a matched comparison group not receiving benefits, while older cohorts experienced much smaller effects.

In short, previous studies of Progresa indicate large education gains for youth who had not yet reached the primary-to-secondary transition at roll-out. Older youths saw no such gains, suggesting that this group—although technically eligible—was effectively too old; the programme

¹⁴ The programme added a fixed monthly transfer to each individual over 70 in 2006, an additional fixed monthly benefit linked to (but not conditioned to) energy consumption in 2007 and a fixed monthly benefit per child aged 0 to 9 in 2008.

came too late to undo dropout. This cohort can serve as a comparison group for cohorts exposed at an earlier age.

2. Data and Methods

2.1. Data

Our main outcomes dataset is the 10% sample from the Mexican Population Census of 2010, at which time early programme beneficiaries were generally old enough to be out of school and in the adult labour force.¹⁵ We also draw on earlier 10% samples for supplementary analyses: from the 1990 census for falsification checks and from the 2000 census and 2005 intercensal survey for information on school enrolment and migration.

The 2010 census applied an extended questionnaire to all household members, providing information on schooling, labour market outcomes, household structure, geographic mobility, housing conditions and durable goods ownership. For schooling, we analyse schooling level indicators as well as grades completed. For labour market outcomes, we consider indicators for labour force participation, wage work and agricultural work, as well as monthly labour income. Due to lower labour force participation rates among women, we measure income in levels rather than logs and do not condition on participation.¹⁶ At the household level, we estimate effects on a housing conditions index, a durable goods ownership index and total household monthly labour income per capita. Each index is defined as the first principal component of a vector of indicators relating to housing or durables ownership, standardised to have mean 0 and SD 1. For the housing index, we use indicators for having a dirt floor, modern roof, flush toilet, sewage, piped water and electricity; for the durables index, we use indicators for having a car, mobile phone, computer, washer, refrigerator, TV and hot water heater. To assess the role of geographic mobility, we consider indicators for urban residence in 2010 and for moving to a new state or municipality between 2005 and 2010. Most of these variables are available in the 1990 census, which we use for falsification checks.

Although Progresa relied on locality-based geographic targeting, the 2010 census allows us to track only municipality backward through an individual's residence history. As such, we merge the census data to Progresa enrolment data by municipality. To minimise concerns about endogenous migration, we would ideally rely on municipality of residence before the programme. However, the 2010 census only tracks municipality backward to 2005, so we assign programme exposure based on municipality of residence in that year. We discuss bias from pre-2005 migration in Section 3.

As a first step toward reducing concerns about differential trends between richer and poorer parts of Mexico, we only include municipalities classified as high or very high marginality in 1990, the year of the last pre-programme census. To accommodate the formation of 41 new municipalities over our sample period, we aggregate municipalities into the smallest units with contiguous borders from 1990 to 2010, arriving at 1,143 'master' municipalities with high or very high marginality, of a total of 2,382 'master' municipalities nationwide. The programme was

¹⁵ We use the 2010 census because the 2015 intercensal survey and 2020 census risk greater bias from migration. Each can backdate place of residence by five years; we find migration impacts in 2010. A post-enumeration survey found that the 2010 census interviewed 99% of the Mexican population in Mexico (INEGI, 2012). Coverage exceeded 98% for both genders and all age groups.

¹⁶ We measure incomes in 2010 pesos (worth US\$0.13 purchasing power parity (PPP) in 2010 according to the Penn World Table v9.1).

operating in all high and very high marginality municipalities by the year 2000, so we measure the intensity of programme penetration rather than an indicator for any penetration. To measure enrolment intensity over any given period, we divide new household enrolment during that period by the estimated number of households in the municipality in 1997 (interpolated between the 1990 and 2000 censuses).

Apart from municipality, birth cohort is the other basis for assigning programme exposure. We form multi-year birth cohorts to avoid bias from differential age heaping. [Online Appendix Figure 11](#) demonstrates the problem and our solution by plotting age histograms for ages 19–51 in sample municipalities. The single-year histogram reveals extensive age heaping, with disproportionate mass at ages ending in 0 and 5. If this age heaping were endogenous to the programme, then single-year cohort designations would be problematic for evaluating the programme. Indeed, [Online Appendix Figure 12](#) suggests that the tendency to misreport age declines with human capital: average education is lower among individuals reporting an age ending in 0 than among individuals reporting either of the neighbouring ages. Consequently, we form three-year bins surrounding each multiple of 5 and form two-year bins between them, which leads to smoother density and conditional mean functions, as shown in [Online Appendix Figures 11 and 12](#). In the final sample, we only include bins with less than 10% still in school, leading us to drop 19–21-year-olds (18% were still in school).

2.2. Design and Estimation

Our identification strategy relies on two sources of variation: spatio-temporal variation in programme roll-out at the municipality level and cohort variation in the age at which children in eligible households were offered the programme. We interact age eligibility with administrative information on the proportion of households receiving benefits in the municipality of residence.

Our definition of age eligibility must contend with the prevalence of grade repetition in Mexico. Mexican children can start secondary school as early as age 12, but due to grade repetition, the primary-to-secondary transition spans many ages. [Online Appendix Figure 13](#) plots enrolment rates in each education level by age in sample municipalities in the 2000 census. Secondary education in Mexico is split into two levels, middle (grades 7–9) and high (grades 10–12). From ages 9–11 to 12–13 to 14–16 to 17–18, primary school enrolment drops from 96% to 67% to 17% to 3%; middle school enrolment peaks at 36% at ages 14–16. Based on these patterns, we treat cohorts aged 13 and younger in 1997 as ‘fully exposed’, cohorts aged 16 and older in 1997 as ‘not exposed’ and the cohort aged 14–15 in 1997 as ‘partially exposed’. We omit the partially exposed group from our main estimations, but we include it in graphical event studies.

A typical approach to difference-in-differences estimation involves regressing an outcome on municipality fixed effects, cohort fixed effects and the interaction of early programme intensity with a cohort exposure indicator. This strategy requires that in the absence of roll-out, cross-cohort trends would be parallel in municipalities more and less intensively treated at the start of the programme. Because initial poverty predicts enrolment intensity, this assumption would be violated if, for example, initially poor municipalities tended to converge toward less poor municipalities across successive cohorts. As such, we modify the standard specification to ask whether, among municipalities with the same cumulative enrolment intensity at the end of the Fox administration (2005), those that saw more intensity during the Zedillo administration (1997–9) experienced larger gains in early beneficiary cohorts. Thus, the spatial component of our design

focuses on an early-versus-late comparison, rather than ever-versus-never. We reason that early and late municipalities with the same overall enrolment intensity are more likely to share trends than municipalities with different overall enrolment intensities.

This approach only requires us to include one additional covariate, the interaction of later cumulative programme intensity with a cohort exposure indicator. For individual i from municipality m and birth cohort t , our baseline regression specification is:

$$y_{imt} = \beta (enrol_m^{1999} \times post_t) + \gamma (enrol_m^{2005} \times post_t) + \delta_m + \eta_t + \varepsilon_{imt}, \quad (1)$$

where y_{imt} is an outcome; $enrol_m^\tau$ measures the cumulative enrolment ratio by the end of year τ (1999 or 2005); $post_t$ is an indicator for being younger than 14 in 1997; and δ_m and η_t are municipality and cohort fixed effects, respectively.¹⁷ Cross-cohort trends that differentially affect municipalities with greater shares of eligible households load onto γ , while β captures the effect of having greater enrolment intensity in the first rather than second phase of roll-out.

To represent our results graphically, we also estimate an event study specification:

$$y_{imt} = \beta_t enrol_m^{1999} + \gamma_t enrol_m^{2005} + \delta_m + \eta_t + \varepsilon_{imt}. \quad (2)$$

We report estimates of β_t in a series of event study diagrams, normalising β_t to zero for the last unexposed cohort, aged 16–18 in 1997. To be consistent with the hypothesised age pattern of programme impacts, the event studies should show limited trends in β_t across the unexposed cohorts and positive changes in β_t for the fully exposed cohorts. However, due to the small number of study cohorts, the event study model does not provide a strong test of parallel trends (Roth, 2022). We therefore use the 1990 falsification check as an additional check on parallel trends.

We also augment (1) to further address concerns about parallel trend violations. Coupled with our focus on high and very high marginality municipalities, our inclusion of $enrol_m^{2005}$ goes a long way in addressing concerns about differential cohort trends across poorer and richer municipalities. However, Figure 3 shows that the relationship between the marginality index and enrolment ratios flattens slightly between the two roll-out phases, suggesting that $enrol_m^{2005}$ may not fully capture the unobserved heterogeneity that may be correlated with trends. As a robustness check to address this concern, we allow cohort effects to vary by a place’s initial marginality. The discordance between our municipality-level research design and Progres’s locality-level geographic targeting strategy complicates the measurement of initial marginality. We take two approaches.

First, we include indicators for the municipality’s percentile in the municipality marginality distribution, allowing the coefficients to vary by cohort:

$$y_{imt} = \beta (enrol_m^{1999} \times post_t) + \gamma (enrol_m^{2005} \times post_t) + \sum_p \pi_t^p \mathcal{M}_m^p + \delta_m + \eta_t + \varepsilon_{imt}, \quad (3)$$

where \mathcal{M}_m^p is an indicator for whether municipality m falls in the p -th percentile of the municipality marginality distribution. The coefficients π_t^p vary by cohort t , so terms in the summation amount to interacting the percentile indicators with year indicators. This approach effectively

¹⁷ The denominator of the enrolment ratio is the 1997 household count, interpolated between 1990 and 2000. Use of the 2000 data raises concerns about post-roll-out migration. However, the interpolated denominator rescales estimates in proportion to average population growth. We estimate that the average municipality had 17% more households in 1997 than in 1990. Using the 1990 count instead of the 1997 estimate shrinks estimated impacts by roughly 17%.

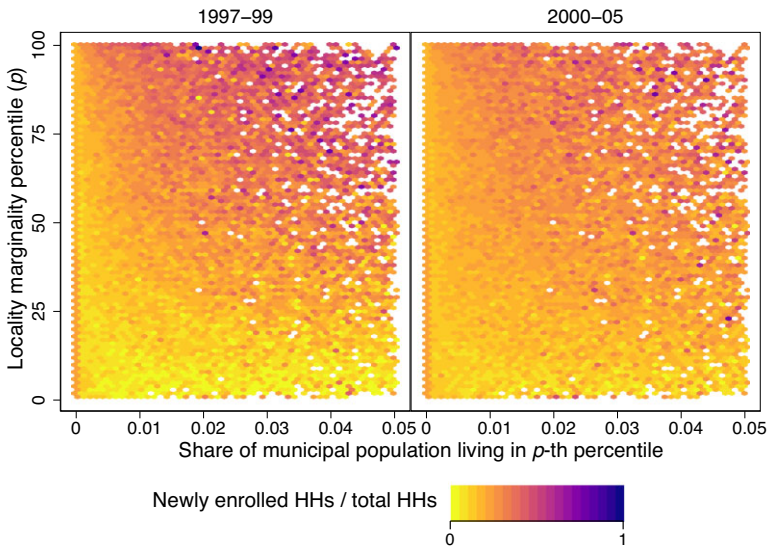


Fig. 4. *Municipality Enrolment Ratio Versus Locality Marginality Share.*

Note: The unit of observation is the municipality. Each hexagon is one-tenth percentage points wide and 1 percentile point tall. White hexagons have no data. The enrolment ratio is the count of new households enrolled divided by the estimated number of households in 1997. For each percentile bin of the locality marginality index, the heat plot displays how the enrolment ratio varies with the share of the municipal population living in a locality in that percentile bin.

absorbs the variation displayed in Figure 3, controlling for differential trends between more and less disadvantaged *municipalities*.

Second, we include shares of the municipality's population residing in localities at each percentile of the locality marginality distribution, again allowing the coefficients to vary by cohort. This approach partially captures the variation displayed in Figure 2, controlling for differential trends between municipalities with varying shares living in disadvantaged *localities*. To represent this variation visually, Figure 4 draws a heat map in which the vertical axis is the locality marginality percentile bin, the horizontal axis is the share of the municipality's population living in a locality at that percentile, and the shading represents the new enrolment ratio. Consistent with Figure 2, enrolment ratios are higher in municipalities with greater shares of people living in marginalised localities. The pattern is stronger in the first phase of roll-out. Adding the locality marginality shares to (4), we obtain:

$$y_{imt} = \beta (enrol_m^{1999} \times post_t) + \gamma (enrol_m^{2005} \times post_t) + \sum_p \pi_t^p \mathcal{M}_m^p + \sum_p \varphi_t^p \mathcal{L}_m^p + \delta_m + \eta_t + \varepsilon_{imt}, \quad (4)$$

where \mathcal{L}_m^p is the share of municipality m 's population residing in localities at the p -th percentile of the locality marginality distribution. Here, β captures the effect of the programme on fully exposed individuals in early roll-out municipalities relative to individuals in late roll-out municipalities

that had the same marginality percentile, the same shares of individuals living in disadvantaged localities and the same cumulative enrolment in 2005.¹⁸

Progresa targets the poor, so municipal poverty dynamics may partially drive the timing of enrolment intensity. In this case, differential changes in cohort outcomes between municipalities with differing enrolment intensities may reflect childhood exposure to poverty rather than Progresa. Equation (4) eliminates bias stemming from poverty dynamics that are correlated with initial conditions, but not from shocks. We focus on broad implementation phases rather than higher-frequency intervals partly to address this concern; the intensity of roll-out over the course of each phase reflects idiosyncratic bureaucratic and programmatic considerations. Further, if residual poverty dynamics do play a role in the timing of enrolment intensity, they likely bias against finding positive programme impacts. If Progresa tended to enter communities during shocks that increased poverty, then childhood exposure to the programme would be associated with childhood exposure to adverse economic conditions. This reasoning would suggest that our estimator produces a lower bound on the long-term benefits of the programme.¹⁹

We interpret β as a dose response to roll-out at the municipality level, rather than an average effect of childhood enrolment at the individual level, because programme effects are unlikely to be private to enrollees. Using data from the original experimental evaluation of Progresa, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) find that school enrolment and attendance rose in treatment communities even among children ineligible for the programme, implying social spillovers. As such, β is best interpreted as the slope of the dose response function linking programme penetration to the outcomes of all children, eligible and ineligible, scaled to reflect a move from no households enrolled to all households enrolled.²⁰ If the dose response function is heterogeneous, then identification of an average slope requires a strong version of the parallel trends assumption: the average change in outcomes for municipalities at a given dose is the same as all municipalities would experience, on average, if all experienced that dose. Under this assumption, a two-way fixed effects specification like ours recovers a weighted average of slopes with positive weights; if the dose is normally distributed, then this weighted average approximates the average causal response (Callaway *et al.*, 2021).²¹ Conditional on $enrol_m^{2005}$, the distribution of $enrol_m^{1999}$ displays slightly more peakedness than the normal distribution, but is otherwise similar (Online Appendix Figure 14).

2.3. Migration

Migration, both internal and external, poses a threat to identification. If Progresa affected emigration from sample municipalities, then remaining individuals may be non-randomly selected, potentially biasing our estimates. We are aware of three studies of programme impacts on migration, all based on the 506 communities in the original randomised evaluation. Two studies focus

¹⁸ To shed light on the identifying variation, Online Appendix Table 7 reports the R^2 from regressions of $enrol_m^{1999}$ on the other covariates. In sample municipalities, $enrol_m^{2005}$ accounts for 65% of the variance of $enrol_m^{1999}$. Adding \mathcal{M}_m^p increases the R^2 to 0.67. Further adding \mathcal{L}_m^p increases it to 0.75.

¹⁹ A counterargument is that the same forces might lead older control cohorts to experience adverse economic conditions at labour market entry, potentially biasing our estimates upward. However, data from the 1990 census and 1995 intercensal survey indicate that pre-programme trends in teenage rates of working and working for pay were not associated with early programme intensity.

²⁰ This interpretation also avoids the pitfalls of interpreting a ‘fuzzy differences-in-differences’ design (De Chaisemartin and d’Haultfoeuille, 2018) with an unstable share of treated individuals in the control group.

²¹ Other recent developments in the interpretation of two-way fixed effects estimators deal with variable treatment timing (De Chaisemartin and d’Haultfoeuille, 2020; Callaway and Sant’Anna, 2021) so do not apply to our design.

on the short-term effects of the programme, with one suggesting more international migration (Angelucci, 2015) and one suggesting less (Stecklov *et al.*, 2005). While conflicting on the sign of the effect, both studies find small impacts in absolute terms, with less than a 0.5 percentage point change in the probability of migrating to the United States. In their 20-year follow-up, Araujo and Macours (2021) study migration effects on individuals who were enrolled in 6th grade at the beginning of the programme, finding mixed results. Receiving the programme for 20 years (versus 18.5 for the original control group) made women 9 percentage points more likely to migrate out of their origin municipality by 2017 and 3 percentage points more likely to live in the United States in 2017; results for men are somewhat smaller and insignificantly different from zero.

To avoid bias from a migration response to childhood programme exposure, our strategy requires that we assign programme exposure to individuals according to their pre-programme municipality of residence, not the current municipality of residence at the time of the 2010 census. As mentioned in Section 3, the census only allows us to ‘return’ migrants to their places of residence in 2005. Fully exposed cohorts may have had opportunities to migrate before 2005, principally during 2000–5. However, data from the 2005 intercensal survey reveal low rates of interstate migration during 2000–5, lower than during either adjoining 5-year interval. Nationwide, only 3.5% of fully exposed cohorts reported different states of residence in 2000 and 2005. The intercensal survey did not collect data on intrastate migration, but our results for the post-2005 period show effects only on interstate migration. To assuage any remaining concerns about endogenous migration, the [Online Appendix](#) reports two robustness checks. The first assigns birth state average programme exposure to individuals whose 2005 state of residence differs from their birth state. The second controls for interactions of birth year indicators with each municipality’s proportional change in the residents from study cohorts (born 1977–88) between 1990 and 2005.

Some risk of bias remains from international migration as well as intranational migration in older cohorts. To assess potential selection from these channels, we test for differential changes in cohort size between municipalities more intensively treated in the first and second phases of roll-out. We estimate municipality cohort sizes in 2005 based on the migration histories in the 2010 census, and we then use the logarithms of the estimate as the outcome in a cell-level version of (2).

[Online Appendix Figure 15](#) presents the resulting event study, revealing that cohorts aged 21–23 and up in 1997 experienced significantly higher growth in earlier roll-out municipalities. In contrast, the cohort size differential between earlier and later roll-out municipalities is stable across younger cohorts. Meanwhile, when we repeat the exercise for 1990 municipality of residence in the 1990 census, we find no differential changes in municipality cohort sizes. Because differential cohort growth is evident only after Progresá, these estimates are consistent with a migration response to the programme. Progresá discouraged young adults from leaving, perhaps because programme areas became more appealing places to start a family. Due to these apparent migration effects among individuals in their twenties, our analysis sample only includes individuals who were 20 or younger in 1997.

2.4. Summary of Final Sample

Our final sample consists of individuals who were aged 9–20 in 1997 and resided in high or very high marginality municipalities in 2005. Younger individuals are omitted due to high rates

Table 1. *Summary Statistics for the 2010 Regression Sample.*

	Men (1)	Women (2)
A. Programme roll-out		
Cumulative enrolment ratio, 1999	0.34 (0.14)	0.34 (0.14)
Cumulative enrolment ratio, 2005	0.64 (0.22)	0.63 (0.22)
B. Education		
Grades completed	7.62 (4.08)	7.26 (4.14)
At least some middle	0.53 (0.50)	0.48 (0.50)
At least some high	0.23 (0.42)	0.21 (0.41)
At least some university	0.08 (0.28)	0.08 (0.27)
C. Labour market outcomes		
Working	0.83 (0.38)	0.24 (0.43)
Working for a wage	0.48 (0.50)	0.16 (0.36)
Working in agriculture	0.41 (0.49)	0.03 (0.17)
Monthly earnings	2,101 (2,558)	651 (1,681)
D. Household economic well-being		
Housing index	-0.03 (1.01)	0.02 (0.99)
Durable goods index	-0.03 (0.99)	0.02 (1.00)
Household monthly earnings per capita	902 (1,241)	768 (1,128)
E. Migration		
Cross-municipal migration	0.06 (0.24)	0.06 (0.24)
Cross-state migration	0.04 (0.19)	0.04 (0.18)
Cross-municipal, intrastate migration	0.02 (0.14)	0.03 (0.16)
Urban residence	0.35 (0.48)	0.35 (0.48)
Number of observations	301,140	358,339
Number of municipalities	1,143	1,143

Notes: Parentheses contain standard errors. The cumulative enrolment ratio in year t is the number of households enrolled in Progreso up to and including t divided by the estimated number of households in 1997. All labour market outcomes are unconditional on labour force participation. Earnings are denominated in 2010 Mexican pesos. The housing index is the standardised first principal component of indicators for a dirt floor, a modern roof, sewage, a flush toilet, piped water and electricity. The durable goods index is the standardised first principal component of indicators for a car, a mobile phone, a computer, a washing machine, a refrigerator, a television and a hot water heater. Migration is measured between 2005 and 2010.

of continued school attendance, while older cohorts are omitted due to differential migration. Binning leads to five distinct cohorts, aged 9–10, 11–13, 14–15, 16–18 and 19–20 in 1997. Event studies use all five cohorts, while our main regressions omit the partially exposed cohort aged 14–15 in 1997.

Table 1 reports summary statistics by gender for the 2010 regression sample, which omits the partially exposed cohort. Most averages are similar for men and women, except in the domain of the labour market. For both genders, cumulative municipal enrolment averaged 34% at the end of the first phase of roll-out and 63–64% at the end of the second. Schooling outcomes slightly favour men, but both men and women average between seven and eight grades completed. In contrast, men exceed women in earnings, work participation, and wage work participation by factors of more than 2.5 on average. At the household level, however, men and women exhibit similar average economic well-being. They also migrate internally at similar rates, with 6% changing municipalities over the previous five years.

A notable difference between the male and female samples is the number of observations, with 16% more women than men. This sample size difference has two likely causes. First, men are more likely to migrate internationally (INEGI, 2009). The 2010 census asks respondents to list all household members who had departed for another country since 2005. In sample municipalities, men comprise 82% of household members from sample cohorts who departed for

Table 2. *Programme Impacts on Educational Attainment.*

	Men				Women			
	2010		1990		2010		1990	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Grades completed								
Enrolment ratio, 1999	1.026	0.866	0.596	0.242	1.570	1.374	1.032	0.755
× post cohort	[0.360]***	[0.327]***	[0.315]*	[0.386]	[0.307]***	[0.311]***	[0.309]***	[0.364]**
<i>N</i>	299,237	299,237	299,237	83,925	355,986	355,986	355,986	89,929
B. At least some middle								
Enrolment ratio, 1999	0.156	0.163	0.130	0.041	0.293	0.302	0.225	0.018
× post cohort	[0.050]***	[0.043]***	[0.043]***	[0.042]^	[0.038]***	[0.039]***	[0.039]***	[0.032]‡
<i>N</i>	299,906	299,906	299,906	84,489	356,801	356,801	356,801	90,433
C. At least some high								
Enrolment ratio, 1999	0.088	0.088	0.034	0.008	0.161	0.170	0.169	0.008
× post cohort	[0.039]**	[0.037]**	[0.038]	[0.026]^	[0.036]***	[0.032]***	[0.032]***	[0.021]‡
<i>N</i>	299,906	299,906	299,906	84,489	356,801	356,801	356,801	90,433
D. At least some university								
Enrolment ratio, 1999	0.023	0.029	0.016	0.007	0.009	0.016	0.017	−0.012
× post cohort	[0.028]	[0.023]	[0.024]	[0.021]	[0.021]	[0.020]	[0.020]	[0.015]
<i>N</i>	299,906	299,906	299,906	84,489	356,801	356,801	356,801	90,433
Municipality FE, cohort FE	X	X	X	X	X	X	X	X
Cohort dummies ×								
Muni. marg. %-ile dummies	X	X			X	X		
Locality marg. %-ile shares			X				X	

Notes: Brackets contain standard errors clustered at the municipality level. All regressions additionally control for the interaction of the post indicator with the cumulative enrolment ratio in 2005. Test versus 0: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Test versus 2010 coefficient: ^ $p < 0.1$, † $p < 0.01$.

the United States (and remain there). Second, homicide mortality surged among young Mexican men from 2006 onwards (INEGI, 2019). Either explanation suggests lower risk of bias from sample selectivity in our analyses of women than our analyses of men.

3. Results

We separate our main results into four sets of outcomes: education, labour market, household economic well-being and migration.²² For each set of outcomes, we provide numerical and graphical representations of the results. For the former, Tables 2–5 report difference-in-differences estimates pooling cohorts into two exposure groups. For each outcome, we present four estimates for men and four for women: one each of (1), (3) and (4) in the 2010 census, and one of (1) in the 1990 census as a falsification check.

The 1990 falsification checks rely on (1), so they are only directly comparable with the 2010 estimates based on the same regression specification. We do not estimate (3) and (4) in the 1990 census because the marginality percentiles were determined in or after 1990. Each table reports the 1990 falsification checks in column (4) for men and column (8) for women. These magnitudes can be compared with the 2010 estimations of (1), which appear in columns (1) and

²² For our main outcomes, we chose the principal variables available in the Mexican Population Census that are plausibly impacted in the long run. The Online Appendix reports some additional outcomes that relate to mechanisms or raise methodological concerns. The Online Appendix outcomes include household and family structure and spousal attributes; the latter are only available for married individuals. We do include one mechanism outcome in the main results—migration—because it is illuminating and directly related to the identification strategy. We do not study job quality due to the lack of detailed characteristics on employment.

(5), respectively. Unfortunately, the 1990 sample has less than one-third the observations as the main 2010 sample. As a consequence, the 1990 estimates tend to have large standard errors, so formal tests of the 1990 versus 2010 comparison are weak. As such, we emphasise the individual magnitude and significance of the 1990 coefficient as a standalone, albeit suggestive, assessment of pre-existing differential trends. For completeness, however, we report significance levels ($\hat{p} < 0.1$, $\dagger p < 0.05$, $\ddagger p < 0.01$) from tests of equality between the 1990 and 2010 coefficients. These markings complement the standard asterisks ($* p < 0.1$, $** p < 0.05$, $*** p < 0.01$) for tests of individual regression coefficients against the null of 0.

For graphical representation of our results, Figures 5, 6, 8 and 9 report event study estimates based on (2). The figures plot the estimated coefficients on interactions between cohort indicators and the proportion of households enrolled between 1997 and 1999, along with 95% confidence intervals. These event study graphs complement our main results by presenting our identifying variation most transparently, at the cost of wider confidence intervals on the cohort-specific coefficients.

3.1. Educational Outcomes

We study four separate measures of education: completed grades, the proportion with some middle school completed, the proportion with some high school completed and the proportion with some college completed. Table 2 reveals that programme exposure before the critical primary-to-secondary transition has large, positive effects on completed years of education. The estimated coefficients indicate that full roll-out in a municipality raises schooling in exposed cohorts by 1.0–1.6 years for women and 0.6–1 year for men. Compared to average education, 7.6 years for men and 7.3 for women, these effects correspond to 8–13% and 14–22% increases for men and women, respectively. The coefficients shrink as we model cohort trends more flexibly, suggesting that some part of the baseline difference-in-differences reflects convergence between high- and low-marginality areas. Supporting this interpretation, the falsification exercise yields positive, but imprecise coefficients on grades completed for both men and women (0.24 and 0.76), suggesting pre-existing differential trends between earlier and later roll-out municipalities. However, the event study graphs discussed below do not show differential trends.

Which schooling levels account for these increases? For both men and women, Table 2 shows significant effects on the probabilities of accumulating at least one year of middle school and high school. The middle school impacts range from 23 to 30 percentage points for women and 13 to 16 percentage points for men: enormous when compared to sample-wide middle schooling rates (53% among men, 48% among women). For high school, the effects are large for women (16–17 percentage points), but small for men (3–9 percentage points and insignificant in the most demanding specification). In contrast, we find no significant effects on college enrolment. To visualise these effects grade by grade, Online Appendix Figure 16 estimates Progresa's impact on the unconditional probability of completing at least x grades of schooling, plotting the coefficients against x from 1 to 15. The coefficients are stable for grades within schooling levels, suggesting that once students are induced to enrol in the next level of schooling, they stay on until its conclusion. The falsification exercises for schooling levels based on the 1990 census are statistically insignificant, the coefficients are small relative to the 2010 effects, and they are significantly different from the 2010 effects for middle school and high school.

Figure 5 shows event study graphs for education, supporting our interpretation of Progresa-related gains. Beginning with grades of completed schooling, the estimated coefficients rise

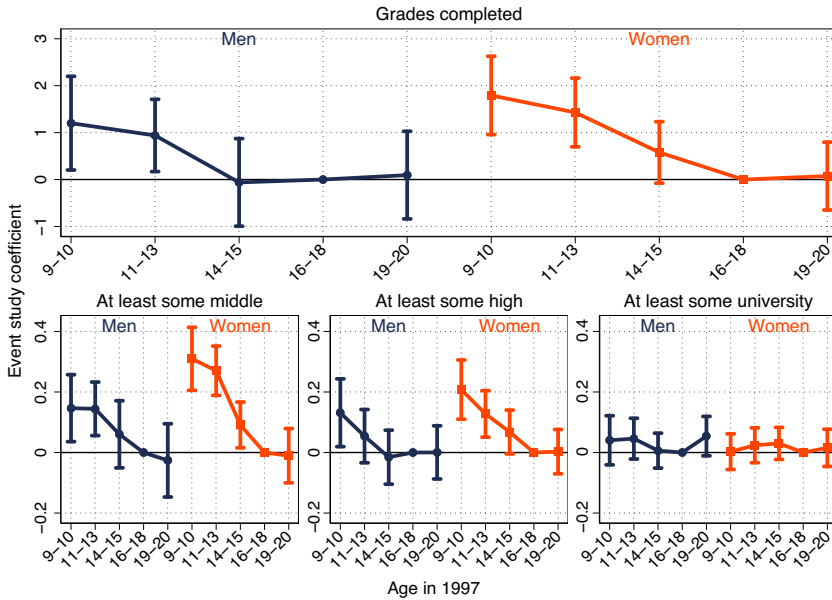


Fig. 5. *Event Studies for Education.*

Note: Coefficients on interactions of cohort indicators with the cumulative enrolment ratio in 1999. The interaction for the cohort aged 16–18 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the municipality level. All regressions include cohort fixed effects, municipality fixed effects and interactions of cohort indicators with the 2005 cumulative enrolment ratio.

with programme exposure for both men and women aged 9–13 in 1997, consistent with positive programme impacts on education. The coefficients are flat for the unexposed cohorts, suggesting a lack of differential pre-programme trends between municipalities with earlier and later enrolment intensity, although we reiterate the weakness of this test with just two pre-programme cohorts. The partially exposed group, aged 14–15 in 1997, displays smaller but still positive gains for women and no differences for men relative to the unexposed group, aged 16 and over in 1997. The event study graphs for the proportion with some middle and high school show similar patterns, with the largest impacts for the fully exposed group, as well as little evidence of differential pre-programme trends in the unexposed group. In the final panel of Figure 5, the event study coefficients for some college are completely flat across all cohorts, suggesting that childhood exposure to Progresca does not raise college attendance. Overall, the estimated education impacts represent substantial increases in attainment among individuals who grew up with Progresca, particularly women.

Our estimates are in the upper range of previous estimates. In early evaluation studies, Schultz (2004) and Behrman *et al.* (2005) simulated total schooling impacts, suggesting the long-run effect of the programme to be 0.6–0.7 years of schooling, significantly lower than our estimate of 1.3 years. However, both studies assume no education effects after 9th grade, whereas ours finds that the effects continue into high school. They also rely on impacts only in the first year of the programme, potentially ignoring growth in impacts over time. Based on 5-year follow-up data on the original evaluation sample, Behrman *et al.* (2011) estimate medium-term education

Table 3. *Programme Impacts on Labour Market Outcomes.*

	Men				Women			
	2010			1990	2010			1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Working								
Enrolment ratio, 1999	-0.015	-0.008	0.001	0.016	0.053	0.062	0.093	0.007
× post cohort	[0.030]	[0.028]	[0.030]	[0.033]	[0.032]*	[0.031]**	[0.031]***	[0.026]
N	299,515	299,515	299,515	83,938	357,018	357,018	357,018	89,583
B. Working for a wage								
Enrolment ratio, 1999	0.032	0.072	0.059	0.043	0.063	0.077	0.073	-0.011
× post cohort	[0.040]	[0.039]*	[0.042]	[0.047]	[0.029]**	[0.027]***	[0.027]***	[0.018] [†]
N	293,165	293,165	293,165	79,896	354,440	354,440	354,440	88,522
C. Working in agriculture								
Enrolment ratio, 1999	-0.090	-0.107	-0.050	0.060	-0.002	0.002	0.009	0.014
× post cohort	[0.046]*	[0.035]***	[0.035]	[0.046] [†]	[0.008]	[0.008]	[0.009]	[0.011]
N	297,315	297,315	297,315	82,402	355,898	355,898	355,898	88,518
D. Monthly earnings								
Enrolment ratio, 1999	494	729	268	599	229	261	255	-11
× post cohort	[383]	[256]***	[261]	[352]*	[135]*	[134]*	[139]*	[142]
N	288,431	288,431	288,431	81,053	354,156	354,156	354,156	89,657
Municipality FE, cohort FE	X	X	X	X	X	X	X	X
Cohort dummies ×								
Muni. marg. %-ile dummies		X	X			X	X	
Locality marg. %-ile shares			X				X	

Notes: Brackets contain standard errors clustered at the municipality level. All regressions additionally control for the interaction of the post indicator with the cumulative enrolment ratio in 2005. All labour market outcomes are unconditional on labour force participation. Earnings are denominated in 2010 Mexican pesos. Test versus 0: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Test versus 2010 coefficient: [†] $p < 0.05$.

effects of about one full year of schooling, more comparable to the long-term results obtained here.

3.2. Labour Market Outcomes

We now turn to the long-run impacts of Progresa on labour market outcomes, studying labour market participation, participation in paid and agricultural work and labour earnings. The large increases in education documented in Table 2 may translate to improved outcomes in the labour market as the young adults in our sample enter the workforce. As discussed previously, while increased education is likely to be the principal vehicle for labour market effects, other mechanisms may also be at work. For example, due to programme transfers, early beneficiaries may have consumed higher quality diets in childhood. Our research design cannot isolate these mechanisms, allowing us only to observe the overall effects of being in the exposed group in an earlier roll-out municipality.

Consistent with the large impact on female education relative to baseline, Table 3 reveals that Progresa also had large effects on female labour market outcomes. Against a participation rate of 24%, programme impacts of full municipality roll-out on labour force participation range from 5 to 9 percentage points, with larger estimates from the more demanding regression specifications. Almost all this impact is driven by increases in wage work, which increases 6 to 8 percentage points compared with a mean of 16%. Progresa also raises monthly labour earnings, with impacts ranging from 229 to 261 pesos (US\$31–US\$35 PPP in the same year), relative

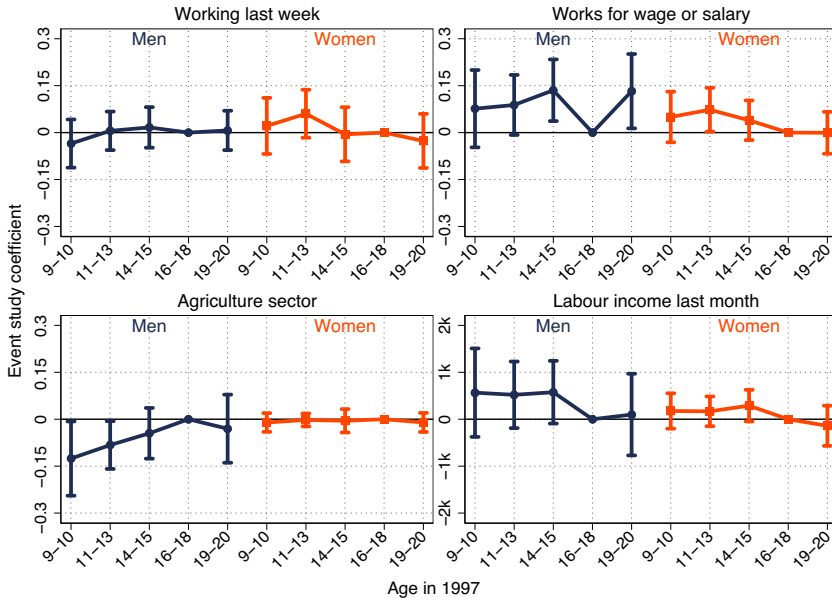


Fig. 6. Event Studies for Labour Market Outcomes.

Note: Coefficients on interactions of cohort indicators with the cumulative enrolment ratio in 1999. The interaction for the cohort aged 16–18 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the municipality level. All regressions include cohort fixed effects, municipality fixed effects and interactions of cohort indicators with the 2005 cumulative enrolment ratio.

to a mean of 651 pesos (US\$85). For labour force participation, wage work, and earnings, all estimates are significant at the 10% level, and all but three are significant at the 5% level. We observe no statistically significant effects of Progresa on participation in agricultural work for women.

The falsification checks and event study estimates for women's labour market outcomes suggest little role for differential cohort trends. In Table 3, estimates from the 1990 census are all insignificant and close to zero. In Figure 6, the event studies show post-programme gains in earlier roll-out municipalities for all labour market outcomes except agricultural work, although the coefficients are imprecisely estimated. Coefficients are positive for the fully exposed group, generally decline for the partially exposed group, and stay small for the unexposed group.

Table 3 demonstrates fewer labour market effects for men. The 2010 estimates are unstable across regression specifications, and for none of the outcomes are they significant across all three. The probability of working or receiving a wage does not change significantly. We do observe reductions in agricultural work as large as 11 percentage points, but we cannot reject that they are attributable by differential trends. Effects on labour earnings are positive, but variable across the three specifications and statistically significant only in (3) without locality marginality percentile shares.²³ The falsification exercise also yields a significant and positive coefficient on

²³ The larger standard errors for men reflect their greater earnings dispersion.

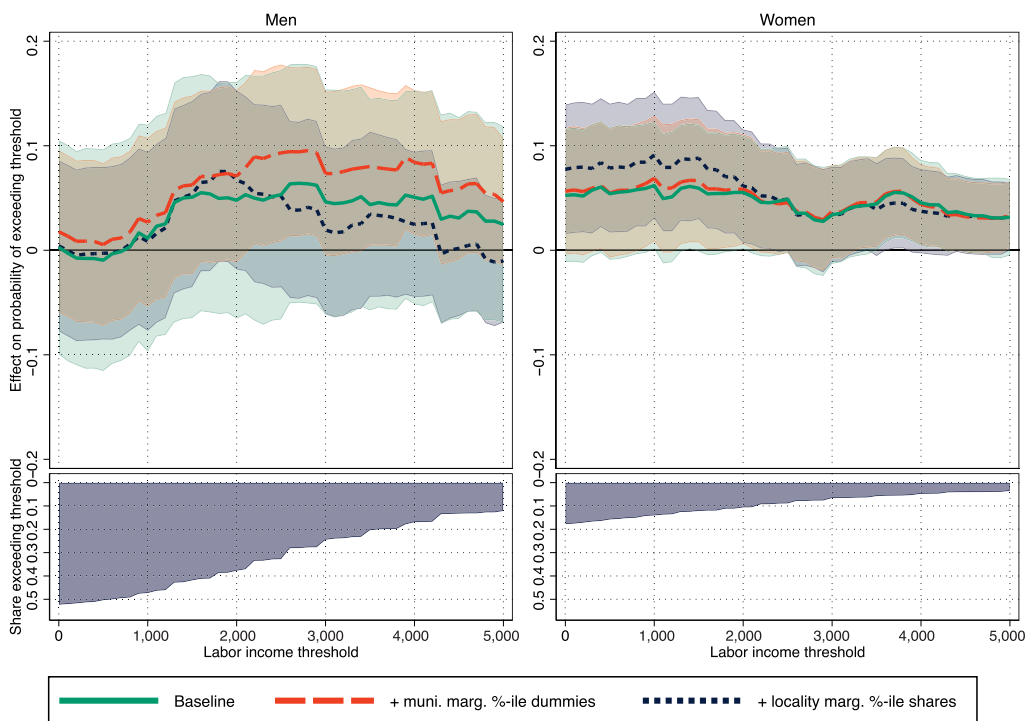


Fig. 7. Programme Impacts on the Distribution of Earnings.

Note: Coefficients on interaction of the post indicator with the cumulative enrolment ratio in 1999. Dependent variable is an indicator for monthly earnings exceeding the specified threshold, which increases in increments of 100. Earnings are denominated in 2010 Mexican pesos. Shaded areas represent 95% confidence intervals based on standard errors clustered at the municipality level. All regressions include cohort and municipality fixed effects, plus the interaction of the post indicator with cumulative enrolment in 2005.

income, similar in size to our coefficient estimates, raising the possibility that gains for men may be driven by convergence between poor and less poor areas. In Figure 6, the event studies for men’s participation in any work and paid work are noisy, consistent with no clear evidence of effects on these outcomes. Those for agricultural work and labour earnings are more consistent with impacts, but the results from Table 3 push back against this interpretation.

To what extent can the earnings results be attributed to changes in labour force participation and the prevalence of paid work? Figure 7 plots effects on the complementary cumulative distribution of earnings for each gender separately. For a series of thresholds from 0 to 5,000 pesos per month (in increments of 100), we estimate versions of (1) and (4) in which the dependent variable is an indicator for earnings exceeding the threshold. Consistent with an important role for female labour force participation, the results reveal that the largest change in the female earnings distribution is at the bottom: a shift from zero to positive earnings. For men, the largest shift in mass occurs more centrally in the distribution, although none of the estimated effects are statistically significant. Placebo estimates using 1990 census data appear in Online Appendix Figure 17, finding no evidence of differential pre-programme trends.

Table 4. *Programme Impacts on Household Economic Well-being.*

	Men				Women			
	2010		1990		2010		1990	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Housing index								
Enrolment ratio, 1999	0.209	0.239	0.199	-0.019	0.243	0.253	0.187	0.019
× post cohort	[0.114]*	[0.099]**	[0.103]*	[0.087]	[0.065]***	[0.070]***	[0.072]***	[0.082] [†]
<i>N</i>	294,969	294,969	294,969	80,947	351,077	351,077	351,077	86,673
B. Durable goods index								
Enrolment ratio, 1999	0.105	0.098	0.050	—	0.229	0.194	0.146	—
× post cohort	[0.110]	[0.094]	[0.097]		[0.073]***	[0.062]***	[0.062]**	
<i>N</i>	295,927	295,927	295,927		352,337	352,337	352,337	
C. HH monthly earnings p.c.								
Enrolment ratio, 1999	-75	74	34	-26	97	90	71	28
× post cohort	[177]	[138]	[148]	[178]	[92]	[89]	[94]	[123]
<i>N</i>	292,360	292,360	292,360	84,405	356,100	356,100	356,100	90,334
Municipality FE, cohort FE	X	X	X	X	X	X	X	X
Cohort dummies ×								
Muni. marg. %-ile dummies		X	X			X	X	
Locality marg. %-ile shares			X				X	

Notes: Brackets contain standard errors clustered at the municipality level. All regressions additionally control for the interaction of the post indicator with the cumulative enrolment ratio in 2005. Earnings are denominated in 2010 Mexican pesos. The 1990 census did not collect data on durable goods ownership. Test versus 0: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Test versus 2010 coefficient: [†] $p < 0.05$.

To summarise this section and the last, in a context of low female labour force participation and education, Progresas has led to striking growth in both areas for young adult women, as well as a significant increase in labour earnings. Male education increased by a lesser amount and men show fewer labour market effects, with no effects on participation in overall or wage work. Estimated effects on male labour earnings show an inconsistent pattern, but our falsification exercise was suggestive that any positive effects may be driven by convergence between poor and less poor areas.

Both the mean and the dispersion of earnings rise with age and experience early in the life cycle, so earning effects may grow as Progresas's early beneficiaries approach middle age. Additionally, productivity losses from lost experience due to schooling may fade over time. In fact, about 10% of our sample is in school, so work impacts may grow as these young adults enter the labour force.

3.3. Household and Demographic Outcomes

A key question is whether Progresas's labour market benefits translate to higher household income and consumption, which bear a more direct link to welfare. Although the census does not directly measure consumption, housing quality and durable goods indices offer proxies based on a subset of the consumption basket. Table 4 finds clear evidence of positive impacts on household housing and durable goods ownership for women. Effects range from 0.19 to 0.25 SDs for housing quality and from 0.15 to 0.23 SDs for durable goods ownership, significant at the 1% level in all specifications. Effects on total household labour earnings are small and statistically insignificant, perhaps reflecting intra-household substitution of labour. For men, we find a significant effect only for housing quality, with significance levels varying between 5% and 10%. The results for housing quality are especially likely to reflect, at least partially, parents' accumulation of a longer

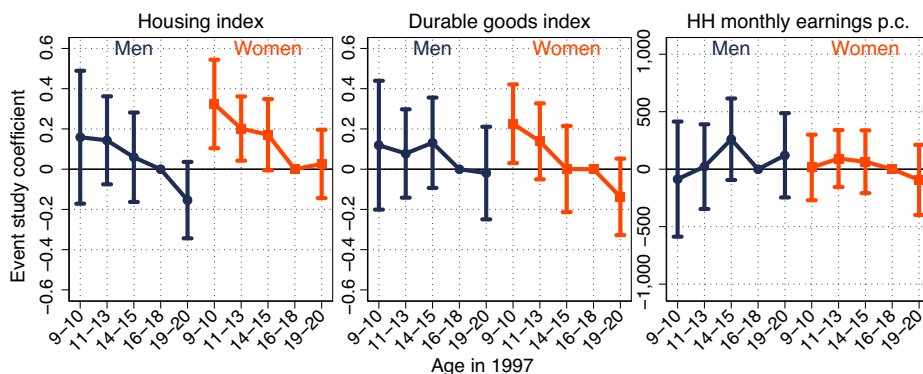


Fig. 8. *Event Studies for Household Economic Well-being.*

Note: Coefficients on interactions of cohort indicators with the cumulative enrolment ratio in 1999. The interaction for the cohort aged 16–18 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the municipality level. All regressions include cohort fixed effects, municipality fixed effects and interactions of cohort indicators with the 2005 cumulative enrolment ratio.

stream of transfers, rather than the labour market improvements we observe in our sample. This caveat is especially germane because over half of the fully exposed group continues to live with at least one parent.

Together with the event studies in Figure 8, the evidence in Table 4 suggests that differential cohort trends are unlikely to explain the positive results for household economic well-being, at least for women. First, placebo coefficients using the 1990 census are small and insignificant for housing quality and household earnings; the 1990 census did not collect data on durables ownership. Second, the 2010 results are robust across specifications. Third, the event studies for women reveal no significant difference in pre-programme trends between early and late municipalities, and a stark, significant post-programme gap. The event study for the men’s housing index is difficult to distinguish from differential trends, although none of the coefficients are significantly different from zero.

Impacts on these household-level economic outcomes are inter-related with how the programme affects household size, parental co-residence, marriage, and fertility, so we also analyse these outcomes in Online Appendix Table 8. We find no effects on any of these outcomes among women, but do find decreases in parental co-residence among men, ranging from 11 to 13 percentage points. The decline in parental co-residence presumably reduces household wealth, so the effects on housing conditions and durable goods may be a lower bound for men.

Although we find no effects on the probability of being married, marriage markets may still mediate the household results through changes in matching outcomes. Thus, we analyse spousal characteristics among married individuals in Online Appendix Table 9, finding that greater early programme exposure is associated with significantly higher spousal work and education for men and higher spousal earnings for women. These results require cautious interpretation because our young sample’s low marriage rate (less than 50% in the fully exposed group) raises concerns about sample composition. Nevertheless, they suggest that Progresa’s effects on household economic outcomes may be partly mediated by the marriage market.

Table 5. *Programme Impacts on Domestic Migration.*

	Men				Women			
	2010		1990		2010		1990	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Cross-muni. migration								
Enrolment ratio, 1999	0.067	0.104	0.072	—	0.073	0.076	0.062	—
× post cohort	[0.052]	[0.039]***	[0.041]*		[0.031]**	[0.029]***	[0.029]**	
<i>N</i>	301,140	301,140	301,140		358,339	358,339	358,339	
B. Cross-state migration								
Enrolment ratio, 1999	0.073	0.101	0.074	0.014	0.062	0.068	0.063	−0.005
× post cohort	[0.046]	[0.035]***	[0.036]**	[0.014]	[0.027]**	[0.026]***	[0.026]**	[0.013] [†]
<i>N</i>	301,140	301,140	301,140	84,062	358,339	358,339	358,339	89,977
C. Intrastate migration								
Enrolment ratio, 1999	−0.006	0.003	−0.002	—	0.012	0.008	−0.001	—
× post cohort	[0.022]	[0.018]	[0.018]		[0.016]	[0.016]	[0.016]	
<i>N</i>	301,140	301,140	301,140		358,339	358,339	358,339	
D. Urban residence								
Enrolment ratio, 1999	0.066	0.081	0.077	−0.078	0.110	0.086	0.044	0.041
× post cohort	[0.050]	[0.043]*	[0.043]*	[0.034]**,†	[0.034]***	[0.032]***	[0.033]	[0.033]
<i>N</i>	301,140	301,140	301,140	84,489	358,339	358,339	358,339	90,433
Municipality FE, cohort FE	X	X	X	X	X	X	X	X
Cohort dummies ×								
Muni. marg. %-ile dummies	X	X			X	X		
Locality marg. %-ile shares		X				X		

Notes: Brackets contain standard errors clustered at the municipality level. All regressions additionally control for the interaction of the post indicator with the cumulative enrolment ratio in 2005. Migration is measured over the five years prior to the census. The 1990 census did not collect data on cross-municipal migration. Test versus 0: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Test versus 2010 coefficient: [†] $p < 0.05$.

Geographic mobility may be an important channel for programme impacts on labour market outcomes. Table 5 studies Progresa's effects on domestic migration and shows significant 6–10 percentage point increases in cross-municipal and cross-state migration (over the previous five years) and urban residence. Effects on intrastate migration are small and insignificant, however, suggesting that the cross-municipal effects primarily derive from cross-state migration. The migration effects are larger than the average propensity to migrate, which is 6% for cross-municipal migration and 4% for cross-state migration. Both the 1990 falsification checks and the event studies in Figure 9 suggest that differential trends do not explain the positive results.²⁴

Taken together, these results point to positive effects on household economic status for women and to a lesser extent for men, which cannot be explained by changes in living arrangements, but may be partly explained by changes in spousal characteristics. While Progresa did not affect the probability of being married, it does appear to have altered spousal characteristics in some dimensions. The results on geographic mobility suggest that Progresa's effects may also be partly mediated by migration from depressed rural areas to more economically vibrant urban areas.

²⁴ The 1990 census did not collect data on intrastate migration, so falsification checks are only possible for cross-state migration and urban residence. We find a significant placebo effect on male urban residence, but it has the opposite sign.

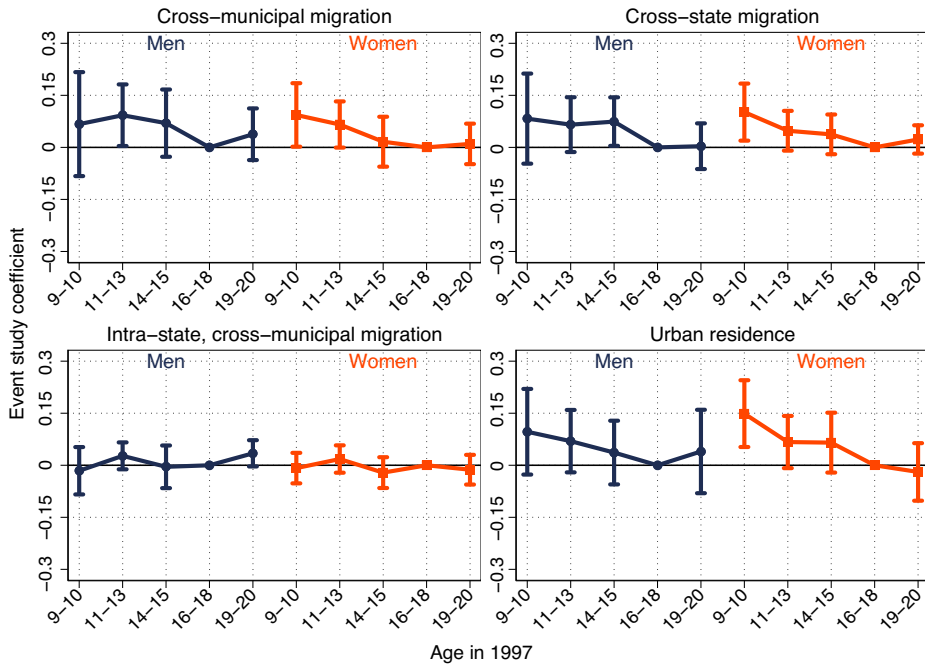


Fig. 9. Event Studies for Migration.

Note: Coefficients on interactions of cohort indicators with the cumulative enrolment ratio in 1999. The interaction for the cohort aged 16–18 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the municipality level. All regressions include cohort fixed effects, municipality fixed effects and interactions of cohort indicators with the 2005 cumulative enrolment ratio.

4. Robustness

4.1. Adjustments for Multiple Inference

We have 15 main outcomes, which may raise questions about multiple hypothesis testing. We confront this issue in two ways: by forming summary indices and by controlling the false discovery rate. Our results withstand both corrections, continuing to reject null impacts on a range of outcomes for women, but not men.

Online Appendix Table 10 runs regressions (1), (3), and (4) using an index for each family of outcomes: education, labour market, household economic well-being, and domestic migration. Following Kling *et al.* (2007), we first generate a gender-specific z-score for each variable by subtracting its mean and dividing by its standard deviation, and we then average z-scores within each family.²⁵ For women, all but one of 12 index impact estimates are statistically significant at the 10% level, and all but two are significant at the 5% level. For men, only education and migration consistently show evidence of impacts.

If we control the false discovery rate across all main outcomes using the approach of Benjamini and Hochberg (1995), we continue to find robust results for women and tenuous results for men. Online Appendix Table 11 reports unadjusted and adjusted *p*-values. For women, the most

²⁵ We reverse the coding ‘working in agriculture’ so that better outcomes are assigned higher values.

exacting regression specification, in (4), found nine unadjusted p -values below 0.05 and one between 0.05 and 0.10. After controlling the false discovery rate, we find eight adjusted p -values below 0.05 and two between 0.05 and 0.10. For men, the same regression specification found two unadjusted p -values below 0.05 and four between 0.05 and 0.10. We find one adjusted p -value below 0.05 and none between 0.05 and 0.10.

4.2. *Alternative Assignment Rules for Migrants*

So far, we have assigned programme exposure using the municipality of residence in 2005. However, some individuals in our sample were born in a different state from where they lived in 2005; where these migrants resided in the late 1990s is unclear. As an alternative to our main strategy, we assign these migrants enrolment ratios based on state of birth. Because it is not possible to determine whether these individuals were born in high or very high marginality municipalities (an inclusion criterion for our sample), we add to the sample those migrants who were not living in high or very high marginality municipalities in 2005, but were born in states where the average marginality index of the state exceeded the municipality threshold for high/very high status. The state-level assignment rule for out-of-state migrants makes municipality fixed effects impossible, so we instead include the main effects of cumulative enrolment in 1999 and 2005. As reported in [Online Appendix Table 12](#), this alternative approach leads to small variations in the estimated effects, but significance levels are similar and the overall conclusions are unchanged.

4.3. *Specification Checks*

The remaining robustness checks return to the original assignment rule, but add covariates to deal with concerns about confounding from poverty, politics, school construction, violence and migration. In [Online Appendix Tables 13](#) (for men) and [14](#) (for women), we reproduce the main results in the first three columns and then address each concern in an additional specification in columns (4)–(8). In column (4), we interact $post_t$ with each of the nine components of the municipality marginality index to absorb any remaining differential cross-cohort changes driven by variation in measured initial poverty conditions. In column (5), we address the concern that municipalities with a history of supporting the Institutional Revolutionary Party (PRI, which was in power until 2000) might have received beneficial treatment during the early phase of roll-out. To do so, we interact $post_t$ with the municipality's PRI vote share in the 1994 presidential election. In column (6), we consider whether a school construction programme that began in the mid-1990s can explain our estimated effects of Progresá, interacting $post_t$ with the number of new schools per capita in 1995–2000 and 2000–5. In column (7), we assess whether the surge in drug-related violence after 2006 biases our results, interacting $post_t$ with the change in the municipality homicide rate from 2006 to 2010. In column (8), we report an additional check on the role of endogenous migration before 2005, interacting $post_t$ with the growth rate of the study population (born 1977–98) from 1990 to 2005.²⁶ The new covariates do not appreciably change the point estimates or significance levels.

²⁶ We estimate these municipality-specific growth rates using microdata from the 1990 census and the 2005 intercensal survey.

5. Discussion

We find clear evidence that childhood exposure to Progresa had lasting economic benefits for women, but the evidence on men is weaker and less conclusive. These findings beg two follow-up questions. First, do the benefits to women by themselves exceed the costs of the programme? Second, how have programme impacts influenced gender inequality in the poorest parts of Mexico?

5.1. Implications for Benefit–Cost Ratios

While several previous studies of Progresa have simulated benefit–cost analyses based on initial education impacts under varying assumptions regarding the returns to schooling (Behrman *et al.*, 2005, 2011), our results allow us to use actual earnings impacts. Given our inconclusive results for men, we calculate a lower bound for the benefit–cost ratio, asking whether the female earnings impacts, if sustained over the life cycle, are large enough to outweigh the cost of rolling out the programme in the early period, 1997–2000. In the spirit of finding a lower bound, we rely on our smallest estimated earnings impacts and assume no earnings growth over the life cycle. Also in the spirit of a lower bound, we include programme costs for both genders, despite restricting programme benefits to one gender. We express all monetary values in 2010 Mexican pesos (worth US\$0.13 PPP in 2010 according to the Penn World Table v9.1) and discount all streams to the start of the programme at a 2% rate.²⁷

For benefits, we compute the aggregate effect of early (pre-2000) roll-out on aggregate lifetime earnings for women in the early beneficiary cohort, aged 9–13 in 1997. The smallest estimated effect of full roll-out on female earnings is 236 pesos per month, or 2,832 pesos per year. Assuming that cohort members exit school age 7 years after the start of the programme and may subsequently work for 45 years, this impact translates to a discounted lifetime impact of 72,706 pesos per woman. Multiplying by early programme exposure in the 2010 census, we calculate that Progresa raised aggregate lifetime earnings by 26.4 billion pesos among women in the early beneficiary cohort.²⁸

We account for two types of programme costs: opportunity costs of lost work and direct costs. For the former, we assume that an additional year spent in school implies 10 months of lost earnings. For a lower bound on the benefit–cost ratio, we rely on upper bound estimates of this opportunity cost. Our largest estimated impacts of full roll-out on the probability of completing any middle school are 16.3 percentage points for men and 30.2 percentage points for women. For high school, the corresponding impacts are 8.8 and 17.0 percentage points, respectively. We estimate potential earnings during school age using the 2000 census. Weighted by early programme exposure, male and female dropouts of middle school age (13–15) earned 739 and 170 pesos per month on average; dropouts of high school age (16–18) averaged 1,159 and 346 pesos. Assuming minimal dropout within schooling level (consistent with [Online Appendix Figure 16](#)) and multiplying by early programme exposure in the 2010 census, we calculate 5.4 billion pesos in discounted lost earnings for early beneficiary cohorts. Note that the lost earnings are mostly men's, while the benefits in our calculations accrue only to women.

²⁷ [Online Appendix Table 15](#) reports the parameters and results of the benefit–cost analysis.

²⁸ Because our programme expenditure data do not disaggregate by municipality, we estimate nationwide benefits and costs. We assume that our impact estimates (scaled by enrolment) for high and very high marginality municipalities also apply to lower-marginality municipalities.

For the direct costs of the programme, we have access to annual programme spending by category. We begin with the discounted sum of transfers in the early period, from 1997 to 2000. Data from Coady (2000) indicate 11.6 billion pesos of education transfers and 11.6 billion pesos of health transfers during this period. Our research design evaluates a component of Progresa most closely linked with secondary school transfers, which directly disincentivise dropout at the primary-to-secondary transition. However, to be conservative, we also include primary school transfers and health transfers, which may have contributed to our results by raising household incomes.²⁹ Following Behrman *et al.* (2011) and Dhaliwal *et al.* (2013), we do not include the transfers as direct costs because they are likely to be at least as valuable to beneficiary families (who are poor) as they are to taxpayers. Instead, we use them to estimate administrative costs (of transferring benefits, conditionality and targeting), private costs and the deadweight loss of raising taxes to pay for the programme. For administrative and private costs, we rely on Coady's estimates of 0.089 and 0.024 pesos per peso transferred, respectively. For deadweight loss, we do not have estimates of the marginal cost of public funds (MCPF) in Mexico, so we consider a range of values.³⁰

Despite our conservative assumptions, the earnings benefits to women far exceed the overall costs of early programme roll-out for any of the MCPF values we consider. Auriol and Warlters (2012) estimate the average MCPF in 38 African countries to be 1.2. At this value, the lower bound on the benefit–cost ratio is 2.0. If we instead rely on the benchmark of 1.4 that Olken (2007) applies to Indonesia, the lower bound is 1.5.

5.2. Implications for Gender Equality

Early programme documents stated: 'Progresa seeks to improve the condition of women and empower the decisive role they play in family and community development' (Adato and Roopnaraine, 2010). What was Progresa's role in closing Mexico's gender gaps across successive birth cohorts? In our cross-sectional dataset, age effects limit our ability to answer this question for economic outcomes, so we focus on educational attainment, which changes little in adulthood.

Until recently, women lagged behind men in secondary school attainment, particularly in poor, rural areas. To illustrate this pattern, [Online Appendix Figure 18](#) plots long-term cohort trends in the rate of completing any secondary school by gender, separately for individuals born in higher- and lower-marginality states.³¹ For the cohort born around 1960 (age 36–8 in 1997), men's secondary attainment exceeded women's by 9.0 percentage points in higher-marginality states and by 7.3 percentage points in lower-marginality states. The gender gap then declined as men's educational progress slowed more than women's amidst economic crisis and budget cuts to education in the 1980s, so that the cohort born around 1975 (age 21–3 in 1997), the gender gap had shrunk to 4.5 percentage points in higher-marginality states and disappeared altogether in lower-marginality states. After this reduction, the gap in higher-marginality states held steady across a decade and a half of birth cohorts, only to finally close in the Progresa generation. Between the last unexposed cohort (age 16–18 in 1997) and the youngest exposed cohort (age 9–10 in 1997) in our sample, the gender gap fell from 5.5 to 1.2 percentage points.

²⁹ We exclude direct expenditures on nutritional supplements and health consultations, which do not plausibly play a role in our results. We also exclude post-2000 continuing transfers to students still enrolled because of pre-2001 exposure, since we cannot distinguish among post-2000 transfer recipients.

³⁰ Due to the ambiguity of the marginal *cost* of public funds, Hendren (2016) suggests the marginal *value* of public funds as an alternative for welfare analysis. We lack the data to estimate fiscal externalities so cannot compute it.

³¹ We plot trends by state of birth instead of municipality of residence to avoid migration concerns.

To what extent can we attribute this cross-cohort gender convergence to Progresa? Programme data indicate that Progresa enrolled 15.3% of households in higher-marginality states in the first phase of roll-out. Multiplying this enrolment ratio by the gender difference in programme impacts, we calculate that programme exposure shrank the gender gap in secondary school attainment by 1.5–2.1 percentage points.³² We conclude that Progresa can account for one-third to one-half of the closing of the gender gap in the poorer parts of Mexico. Recall that the education grants for girls exceeded those for boys by about 15% in middle and high school, which may help explain the larger impacts on girls than boys.

6. Conclusions

Conditional cash transfer programmes began two decades ago, transforming anti-poverty policy around the world. Their linkage of payments to human capital investment had the dual goals of alleviating current poverty and reducing poverty in the next generation. While previous studies have found contemporaneous education and health benefits for children from beneficiary households, little research has addressed whether these changes improve the lives of these children when they reach adulthood. Whether the benefits of CCTs flow intergenerationally has remained an open question.

This paper provides new evidence that the intergenerational benefits of CCTs may be substantial. We estimate the long-term effects of Mexico's flagship programme Progresa on the educational, labour market, household and demographic outcomes of young adults who effectively grew up with the programme. The results show large effects on the next generation's completed education, work, earnings and household economic status, particularly for women. Before Progresa, women's labour force participation was extremely low in its original communities, as was their status in the household (Attanasio and Lechene, 2002). Relative to average outcomes in pre-programme cohorts, the estimated effects of full roll-out on female labour force participation range from 21% to 38%, and the estimated effects on female labour earnings are about 40%. Women also experience large increases in urban residence and cross-municipal migration, suggesting that geographic mobility may play a role in these impacts.

Men exhibit more nuanced results, with smaller education effects and fewer labour market effects. While we cannot reject that any observed male income gains reflect convergence between poor and less poor areas, we do find significant decreases in agricultural work consistent with some improvement in labour market conditions. For both genders, we observe improvements in housing conditions and durable goods ownership, although these results may reflect greater human capital, greater parental wealth, or both.

Our findings have much in common with the experimental follow-up by Araujo and Macours (2021). Like us, they find that exposure to Progresa at the primary-to-secondary transition increased completed education and income, with somewhat stronger results for women than men. However, their point estimates for education are smaller than ours—by 43% for men and 79% for women—while those for income are larger—by 42% for men and 60% for women. The difference for education may reflect the geography of our research design, which captures broader spillover effects. The difference for income may be due to our measurement of income among the exposed at an earlier life cycle stage, when the variance is smaller.

³² We reject equality of effects on male and female secondary school in the baseline specification ($p < 0.01$), after adding municipality marginality dummies ($p < 0.01$), and after adding locality marginality shares ($p = 0.05$).

Nevertheless, both sets of results are encouraging for the long-term prognosis of children from households receiving CCTs, particularly girls. Progresa significantly contributed to the closing of the education gender gap in Mexico's poorer states. Furthermore, the earnings impacts on young adults, many of whom no longer live with their parents, are unlikely to disappear when their parents stop receiving cash transfers. Similar reasoning applies for housing conditions and durable goods. As a consequence, these impacts are likely to represent permanent increases in well-being due to Progresa. In fact, our benefit–cost analysis finds that the female earnings gains, if sustained over the life cycle, can by themselves pay for programme costs.

General equilibrium effects complicate the interpretation of a cohort difference-in-differences design like ours, but we argue that they are unlikely to bias us toward overstating the benefits of programme exposure. Duflo (2004) and Khanna (2023) point out that in general equilibrium, a programme like Progresa may affect unexposed cohorts by changing wage rates paid to workers with a range of attributes: skilled and unskilled, young and old. One might worry that our positive estimates reflect deteriorating outcomes among unexposed cohorts living in early roll-out municipalities, due to an increased supply of young, skilled workers.³³ However, migration is common in Mexico, and both Duflo and Khanna emphasise that migration undoes the local general equilibrium effects that they study. Indeed, full municipality roll-out is associated with a relative increase in interstate migration for exposed cohorts, substantially larger than the sample mean. This relative change is best interpreted as moving to opportunity among exposed cohorts, which mitigates local general equilibrium effects. For it to reflect reduced migration by unexposed cohorts, one would need to assume that income reductions from general equilibrium effects were severe enough to cause credit constraints to bind.

Our analysis does not directly speak to the debate over whether cash transfers to poor families should be conditional (Baird *et al.*, 2013). At the same time, for conditional transfers to be preferable to unconditional transfers, they must at a minimum improve the lives of children, who are the sole targets of conditionality. Short-run benefits like increases in school enrolment do not meet this standard on their own, unless one views enrolment as having intrinsic rather than instrumental value. As a result, our evidence of long-run benefits to childhood beneficiaries provides a necessary, though not sufficient, input to the design of anti-poverty programmes.

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Additional supporting information may be found in the online version of this article:

Online Appendix Replication Package

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³³ Such a deterioration arises in Duflo (2004), but not Khanna (2023).

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