Do Conditional Cash Transfers Improve Economic Outcomes in the Next Generation? Evidence from Mexico*

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Abstract

Conditional cash transfer programs have spread to over 80 countries in the past two decades, but little is known about their long-term effects. We estimate the impact of childhood exposure to the Mexican program Progresa by leveraging the age structure of benefits and geographic variation in early program penetration nationwide. Childhood exposure improves educational attainment, geographic mobility, labor market performance, and household economic outcomes in early adulthood. Schooling increases by 1.3 grades for both sexes, while labor market impacts are more pronounced for women, amounting to one-quarter of their labor force participation rate and half of their average labor income.

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1 Introduction

Conditional cash transfer (CCT) programs were first introduced two decades ago and have since spread around the world, now operating in more than 80 countries, in many cases representing a key government strategy for reducing poverty. By linking monetary transfers to children's human capital investment, the programs 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 programs was Mexico's program Progresa, which began in 1997 and is well known due to its initial randomized evaluation, the basis for numerous published studies (Parker and Todd, 2017).[1] The program's novelty and positive evaluation findings contributed to both a large scaleup within Mexico and the spread of its key features to new programs around the world. CCT programs now operate throughout Latin America, in a number of poor countries in Africa and Asia, and even in a few developed countries, including the United States. This paper investigates whether these programs are achieving the second of the dual goals: to improve economic wellbeing in the next generation.

A number of previous studies of Progresa and other CCT programs have shown positive program impacts on the education levels of poor youth (Parker and Todd, 2017; Fizbein and Schady, 2009; Baird et al., 2013). Nevertheless, evidence on whether these increases in education translate to better economic outcomes in the next generation is limited, creating a stumbling block for cost-benefit analyses. Because these programs explicitly seek to improve the economic outcomes of exposed youth when they grow up, information on 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 organizationally difficult, especially in contexts with high rates of migration.

This paper estimates the long-term impacts of Progresa in a quasi-experimental design using census data linked to administrative data on program enrollment. We study the educational, labor market, household, and demographic outcomes of the program's earliest beneficiaries, who were of primary school age when the program began in 1997 and are now young adults. Following up this group in the early stages of adulthood, we estimate impacts in a difference-in-difference de-

[1] The program began in 1997 as Progresa (Programa de Educación, Salúd y Alimentación), was renamed Oportunidades in 2001 at the start of the Fox presidency, and was renamed Prospera in 2013 during the Peña Nieto presidency.

sign akin to Duflo's (2001) analysis of a school construction program in Indonesia, comparing early beneficiaries to a slightly older group that was too old at rollout to reap the program's educational benefits, across municipalities with varying exposure to early program rollout. Our research design allows us to consider a more sustained intervention than Progresa's original randomized experiment, in which treated communities were exposed to the program for only 1.5 years longer than control communities. It also allows us to study a nationwide progam at scale, which is likely to improve external validity and relevance to policymakers (Muralidharan and Niehaus, 2017).

A few studies have attempted to estimate the effects of childhood CCT exposure on later outcomes, but typically with small samples, limited numbers of outcomes, or short follow-up periods. In the contribution closest to ours, Barham, Macours, and Maluccio (2017) study a CCT experiment in 42 Nicaraguan communities, in which households were randomized to early (2000-2003) or late (2003-2005) receipt of transfers. At ten-year follow-up, results showed some positive effects on education (0.3 grades), off-farm work (6 percentage points), and income (15 percent) for boys aged 9 to 12 at baseline, but the authors do not analyze data on girls. In another related study, Barrera-Osorio, Linden, and Saavedra (2017) examine 8-12 year education effects of a Colombian program that compared traditional bimonthly CCT payments with variants that delay a portion of payments until late in the year when enrollment decisions are made, finding larger schooling effects of the 'savings' variant. They do not analyze work outcomes. Ham and Michelson (2018) study a Honduran pilot in 70 municipalities with both demand-side transfers and transfers combined with supply side subsidies between 2000 and 2005. Eight years later, they find no effects of the transfers-only variant, whereas transfers with supply subsidies show positive effects on education (0.3 grades) and female work (3 percentage points). However, all groups (including control) received the country's flagship Poverty Reduction Strategy program beginning in 2002, complicating interpretation of the CCT's effects. Using a much shorter horizon, Baird, McIntosh and Ozler (2018) study the effects of a 2-year CCT program on adolescent females in Malawi 2-4 years after the program ended. While the CCT increased schooling in certain sub-samples, labor market outcomes were unaffected. For Progresa specifically, efforts to analyze longer-run outcomes in the experimental cohort have been hampered by extremely high rates of attrition, with at least 25 percent of the cohort attriting within 10 years and over 99 percent lost within our follow-up period (Rodríguez-Oreggia and Freije, 2012;

Kugler and Rojas, 2018).[2]

Relative to these studies, our paper provides the first estimates of the long-term impacts of an influential and nationwide CCT program on a generation of youth who have effectively grown up with it. Our difference-in-difference strategy is motivated by earlier studies finding few schooling impacts on youth who were offered the program 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. Using this difference-in-difference strategy, we find that exposure to greater program rollout before age 12 improves accumulated education, labor market outcomes, housing characteristics, durable goods ownership, and geographic mobility. Compared with those offered the program 'too late,' early beneficiary men and women in a fully treated municipality complete $1\frac{1}{3}$ additional grades of schooling, 15-20 percent of mean educational attainment. Likely reflecting the benefits of this additional education, we also find large effects on labor market outcomes. For women, childhood program exposure increases labor market participation 6-11 percentage points, more than one quarter of mean female participation, while labor earnings rise US$30-40 per month, roughly half of mean earnings. Effects on male labor outcomes are large but less statistically significant. Hours worked for men increase by more than three hours per week, accompanied by shifts from agriculture to non-agriculture and from the informal to formal sectors, as well as a positive but insignificant increase in labor earnings. Conditional on being in the labor force, however, men who benefited from the program work in significantly higher paying industries and oocupations. Because beneficiaries tend to have more educated spouses, the effects on household labor income are larger than the effects on individual labor income. Both men and women also display positive effects on housing conditions and on ownership of durable goods, although this result may reflect either the effect of human capital accumulation or the direct benefits of greater parental wealth.

The estimated effects are large by any measure, particularly for women, who historically have had low status and labor force participation in program areas. Our results are robust to controlling for economic convergence, political clientilism, school construction, and violence, as well as to various strategies for assigning program exposure to migrants, given the census's limitations on location of childhood residence. We incorporate our results in a cost-benefit analysis and find ra-

[2]These longer-run studies using Progresa's original evaluation sample find both positive and null results, highlighting how high and selective attrition rates make results sensitive to methodological choices.

tios of benefits to costs substantially above one even under lower bound assumptions on earnings impacts and earnings growth. Two decades after CCT programs began, these results suggest that CCTs and their accompanying educational gains have important economic consequences for the next generation.

Beyond the literature on CCTs specifically, these 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; Akresh et al., 2015), 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 developing country context, Araujo et al. (2017) carry out a ten-year follow-up study of Ecuador's cash transfer program—in which transfers were unconditional, although some beneficiaries mistakenly thought they were conditional—finding mixed results on education and no significant effects on labor market outcomes. Baird, McIntosh, and Ozler (2018) also provide results for an unconditional treatment arm, finding no effect on schooling and a short-term effect on early marriage that disappears within two years of the program. Some evidence is also available from the historical United States, where welfare programs for mothers (Aizer, Eli, Ferrie, and Lleras-Muney, 2016) and food stamp programs (Hoynes, Schanzenbach, and Almond, 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-run effects of Progresa and other CCT programs provide much guidance to policymakers, long-term follow-up on the next generation is crucial to assessing whether the programs are achieving the second of their dual goals of reducing poverty in future generations. A few recent studies suggesting smaller-than-expected effects of cash transfers in the longer term (e.g., Baird, McIntosh, and Oxler, 2018; Haushofer and Shapiro, 2018) has led to debate over their promise, but this debate concerns whether the effects of short-lived experimental programs (on the current generation) 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-standing government program on the next generation. The results presented here are very encouraging with respect to the potential of CCTs to reduce poverty of the next generation. While studies of other programs

are clearly needed, the experience of the earliest beneficiaries of Mexico's pioneering program bode well for the millions of other children benefiting from CCTs around the world.

2 Program Background

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 programs and eliminating general food subsidies. It quickly grew over time and currently covers six million families, or about one quarter of all families in Mexico. While the program has expanded into urban areas, it remains largely rural, with about two thirds of its household beneficiaries deriving from communities with less than 2500 inhabitants. Figure 1 shows the aggregate numbers of households who became beneficiaries per year since the program began. New enrollment activity was most intense during the first decade of the program, under the presidencies of Ernesto Zedillo and Vicente Fox, with clear troughs in presidential (2000, 2006, 2012) and midterm (2003, 2009, 2012) election years. These troughs, which reflect an anti-vote buying policy that prohibits social program expansion in the leadup to national elections, provides a rhythm to the rollout that will be useful for our research design.

The program conditions cash payments to families on children regularly attending schools and on family members visiting health clinics for checkups. Program take up was exceedingly high when the program began, with 97 percent of families who were offered the program participating (Skoufias and Parker, 2001). Program rules allow students to fail each grade once, but if a student repeats a grade twice, the schooling benefits are discontinued permanently. The program also provides some additional subsidies for school supplies and a transfer linked to regular visits to health clinics.[3] Children and youth age 21 and younger are eligible to receive the school subsidies.

To illustrate the evolution of the benefit structure during the rollout period, Table 1 shows the monthly grant levels for children between the third grade and the twelfth grade in the second semesters of 1997 and 2003 (when the exchange rate was about 8 and 11 pesos per U.S. dollar, respectively).[4] Originally, the program provided grants only for children between the third and

[3]Since 2006, program benefits have been extended in several ways, including fixed benefits for households with more elderly individuals (in 2006), for households with low energy consumption (in 2007), and for households with more children under 9 (in 2010).

[4]The Mexican education system defines grades 1 through 6 as primary school (primaria), 7 through 9 as junior high

ninth grades, but in 2001, the grants were extended to grades 10-12. At grades seven and above, the grants are slightly higher (by about 13 percent) for girls than boys, a response to historically higher dropout rates among girls than among boys after primary school.

Specific grant amounts range in 2003 from $US9.50 (105 pesos) in the third grade of primary to about $US53 (580 pesos) for boys and $US60 (660 pesos) for girls in the third year of senior high school (grades 10-12). By the senior year of high school, the grant amount represents about two thirds of Mexico's minimum wage. All monetary grants are given to the mother of the family, with the exception of scholarships for upper-secondary school, which the youth can receive themselves subject to the mother's authorization. The program is means tested, with both geographic and household-level targeting. The geographic targeting uses aggregate census indicators to select poor rural communities based on a marginality index described in Section 3.[5] Progresa carries out a socio-economic survey for all households in eligible communities and then uses discriminant analysis to distinguish eligible from ineligible households using characteristics including dwelling conditions, dependency ratios, ownership of durable goods, animals and land, and the presence of disabled individuals. Skoufias, Davis, and de la Vega (2001) compare the targeting algorithm with consumption- and geography-based alternatives and conclude that the program performs well in targeting the poorest.

The original, well-known evaluation of Progresa was based on an experimental program design that randomly assigned 506 communities from 7 states to a treatment group (320 communities) and control group (186 communities). Eligible households in treatment communities began to receive benefits in 1998, while eligible households in control communities began in 2000. This experimental evaluation generated a large number of studies estimating short-term impacts of Progresa during the length of the experiment (Parker and Todd, 2017). Longer-term follow-up evaluations based on the original experimental design are complicated by the small, 1.5-year difference in the duration of program exposure between treatment and control communities.[6]

Studies of education impacts during the 18-month experiment show large enrollment effects at

school (secundaria), and 10 to 12 as senior high school (media superior or prepatoria).

[5]Before 2002, the community was required to have a primary/secondary school within 5/10km when if close to a federal highway and 4/6km if close to a state highway; beginning in 2002, sufficient school access was still required, but program rules no longerspecified explicit critera. Internal program documents indicate that of about 75,000 eligible communities identified in 1997, about 13 percent did not meet the school access criteria.

[6]Two additional follow-up surveys were conducted in treatment and control communities in 2003 and 2007, and a matched comparison group was added in 2003. The 2007 follow-up suffers from high levels of attrition.

the transition between primary and secondary school (6th to 7th grade) (Schultz, 2004; Behrman, Sengupta, and Todd, 2005) and reductions in grade repetition in primary school (Behrman, Sengupta, and Todd, 2005). However, few significant effects were observed for youth who had 6 or more years of schooling or were older than 15 at the program's start. In a non-experimental study with longer follow-up, Behrman, Parker, and Todd (2011) find that beneficiary children aged 9-12 at the program'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 the start of receiving benefits. However, their older counterparts saw no such gains, suggesting that this group—although technically eligible for grants—was effectively too old; the offer of the program came too late to undo dropout. This group thus offers a convenient comparison group for those offered the program at an earlier age.

Progresa's school subsidies reduce the shadow wage (or relative value) of children's time in activities other than school but also raise income, so they have a priori ambiguous effects on work and leisure. While the income effect would increase time spent in leisure, the net effects on time spent at school and working would be respectively positive and negative if the substitution effect dominates. Previous studies find child labor reductions in similar age groups to those showing improvements in schooling (Skoufias and Parker, 2001). As children age, we expect the substitution effect of the program to diminish and thus alter the overall impact of the program on work. As children and youth accumulate schooling, they receive higher wage offers. Assuming diminishing marginal returns to schooling, the marginal benefit of schooling (higher future wages) will eventually no longer exceed the marginal cost (foregone wages and leisure time). These considerations lead us to expect that over the longer run, our focus, the program should increase work and wages.

School quality is lacking in the isolated, rural areas of Mexico we study (OECD, 2013), and program critics have long pointed to the possibility that increasing education levels may not increase incomes. Previous studies of program effects on standardized achievement tests have suggested limited impacts on learning in spite of increased enrollment (Behrman, Parker and Todd, 2009; Baird et al, 2018), consistent with low returns to education. Additionally, much of the population in rural areas is engaged in agriculture, where returns to schooling may be lower. Nevertheless, research on school construction in similarly poor, rural areas suggests large long-run benefits (Duflo 2001).

3 Data and Method

3.1 Data

We use the 10 percent sample of households from the Mexican Population Census of 2010, at which time early program beneficiaries were generally old enough to be out of school and in the adult labor force.[7] The census applied an extended questionnaire to all household members, providing information on schooling, labor market outcomes, household structure, geographic mobility, housing conditions, and durable goods ownership. Our starting sample includes twenty cohorts of individuals, aged 20-39, although our main empirical work restricts attention to a subset of these cohorts based on supplementary analyses in the remainder of this section. To this micro-dataset, we merge administrative program information on the cumulative number of households enrolled in Progresa by year and by municipality, data supplied by Progresa administrative personnel.

The census offers a range of useful outcome variables. For schooling, we analyze schooling level indicators as well as grades completed. For labor market outcomes, we consider indicators for labor force participation, wage work, and agricultural work, as well as weekly labor hours and monthly labor income. Due to high rates of non-attachment to the labor force among women, we measure income in levels rather than logs in our main analysis and do not condition on participation.[8] At the household level, we estimate effects on total household monthly labor income, indices of housing conditions and durable goods ownership, and household composition. Each index is defined as the first principal component of a vector of indicators relating to housing or durables ownership, standardized to have mean 0 and standard deviation 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. Household composition outcomes include household size and indicators for parental coresidence and marital status: all important for interpreting the household economy results. Finally, to assess the role of geographic mobility, we consider indicators for urban residence in 2010 and for moving to a new municipality between 2005 and 2010.

[7] We use the 2010 census rather than the 2015 intercensal survey because the latter poses more risk for selection bias due to migration. The 2015 survey only provides municipal migration history back to 2010, and we find large program effects on cross-municipal migration between 2005 and 2010.

[8] We provide a supplementary analysis of compensation conditional on working among men, who have lower rates of non-attachment.

Progresa primarily operates in municipalities with a high or very high level of marginality (or poverty) as defined by CONAPO (the Mexican Population Council), which classifies municipalities using a marginality index based on nine municipal-level socioeconomic variables from the census.[9] CONAPO assigns each municipality to one of five categories, ranging from very low to very high marginality. Accordingly, our analysis focuses only on municipalities with high or very high marginality in 1990, the year of the last pre-program 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 1143 'master' municipalities with high or very high marginality, of a total of 2382 'master' municipalities nationwide.

The program was operating in all high and very high marginality municipalities by the year 2000, so we measure the intensity of program penetration rather than an indicator for any penetration.[10] To measure enrollment intensity over any given period, we divide new household enrollment during that period by the estimated number of households in the municipality in 1997 (interpolated between the 1990 and 2000 censuses). We call this measure the 'enrollment ratio.'

3.2 Research Design

Our identification strategy relies on two sources of variation: spatiotemporal variation in program rollout at the municipal level and cohort variation in the age at which children in eligible households were offered the program. Our difference-in-difference estimations interact age eligibility with administrative information on the proportion of households receiving benefits in the municipality of residence.

The early beneficiaries we study were the first cohort to grow up with the program: those between ages 7 and 11 when the program began. Because this group was eligible for the entire set of education grants beginning in the third grade of primary school, we term this group 'fully exposed' or 'post-program.' Our comparison group, which we term 'not exposed' or 'pre-program,' includes individuals who were older than 15 when the program began, mostly past the primary-to-

[9] The index is the normalized first principal component of nine municipal population shares: the share illiterate, the share with less than primary school education, the share without a toilet, the share without electricity, the share without running water, the share with crowding (few rooms per capita), the share with a dirt floor, the share living in communities with less than 5000 inhabitants, and the share earning less than twice the minimum wage.

[10] Variation in the proportion of beneficiaries enrolled over time across municipalities may be due to differences in the timing of the rollout of the program at the community level or to differences in the proportion of households in selected communities who receive the program.

secondary transition. The group aged 12-14 at the program's start may or may not have missed the opportunity to avoid dropout at the primary-to-secondary transition, so it is effectively 'partially exposed.' Because of the partially exposed group's ambiguous treatment status, we omit it from our main estimations, but we include it in graphical event study representations of our results.

The ambiguity of treatment status is in fact broader than just the 12-14 cohort, owing to high rates of grade repetition, especially among boys. The prevalence of grade repetition is apparent in Appendix Figure A1, which uses data on sample municipalities in the 2000 census to plot enrollment rates in each education level by age. At age 12, primary school enrollment begins a slow decline, while secondary school enrollment begins a slow rise. Even at age 15, an age we treat as past the primary-to-secondary transition, 16 percent of boys are enrolled in primary school, compared to 40 percent in secondary. As such, our cohort exposure classifications should be seen as approximate.

Nevertheless, we expect the younger, fully exposed cohort to show greater program impacts on education and thus also work, earnings, and other economic outcomes by early adulthood. While education is likely to be an important mechanism, we do not attempt to distinguish it from other potential mechanisms, such as greater parental income during childhood. In that sense, our estimates represent the overall effect of childhood exposure to conditional cash transfers.

We focus on variation in enrollment during the two main phases of rollout—1997 to 1999 and 2001 to 2005—which are separated by the election-related lull in enrollment activity in the year 2000. Figure 2 plots the enrollment ratio against the 1990 marginality index for the two rollout phases and shows, as expected, that enrollment intensity rises with the level of marginality. In both phases, enrollment is most intense in high and very-high marginality municipalities, which are to the right of the vertical line. The program is slightly more targeted to high and very-high marginality municipalities in the first phase, but the patterns are broadly similar. In the post-2006 era, the nature of program expansion changed. Progresa moved into cities and other areas where poor communities were less densely distributed. Indeed, Appendix Figure A2 shows that that the relationship between the marginality index and new enrollment is more similar in the first and second phases of rollout than after. To demonstrate changes in the geography of rollout more broadly, Appendix Figure A3 maps new enrollment ratios across municipalities in 1997-99, 2001-05, and 2006-11. Here again, patterns are broadly similar in the first two periods, with more intense enrollment in the poorer western and southern regions of Mexico. After 2005, enrollment spreads into the wealthier

northern region. The comparability of the first and second phases supports our use of municipalities with more intense enrollment activity during 2001-05 as a control group for municipalities with more intense enrollment activity during 1997-99.

Our estimation sample includes only high and very-high marginality municipalities, due to their much higher rates of program penetration. Table 2 details program rollout and economic conditions in these sample municipalities and their lower marginality counterparts. On average, 34 percent of households in sample municipalities were enrolled in Progresa by 1999, and 63 percent were enrolled by 2005. In contrast, average enrollment rates in the lower marginality municipalities omitted from the estimation sample were 6 percent in 1999 and 19 percent in 2005. By construction, the marginality index is higher in sample municipalities, as are the nine components of the index. For example, in the average sample municipality, 34 percent of the adult population was illiterate in 1990, and 60 percent of households did not have a toilet; in the lower-marginality municipalities omitted from the estimation sample, these rates were 13 percent and 26 percent, respectively.

3.3 Estimation

Our identification strategy is a cohort difference-in-difference design, leveraging variation in program exposure across cohorts and space. In the typical approach to this design (e.g., Dulfo 2001), a researcher would regress an outcome on municipality fixed effects, cohort fixed effects, and the interaction of early program intensity with a cohort exposure indicator. To yield unbiased estimates of program impacts, this specification requires the assumption that in the absence of the program, cross-cohort trends in municipalities more intensively treated at the start of the program would be parallel to those in all other municipalities. Because initial poverty so strongly predicts enrollment intensity, this assumption would be violated if, for example, initially poor municipalities tended to converge toward less poor municipalities across successive cohorts. Said another way, early program intensity may be correlated with total program intensity. As such, we modify the standard specification to ask whether, among municipalities with a given level of cumulative enrollment in 2005, those that saw more of that enrollment before 2000 experienced larger gains in early beneficiary cohorts.

In practice, this approach only requires us to include one additional covariate, the interaction of later cumulative program intensity with a cohort exposure indicator. For individual i from munici-

pality m and birth cohort t, our main regression specification is:

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

where y_{imt} is an outcome; $enroll_m^{\tau}$ measures the cumulative enrollment ratio by the end of year τ (1999 or 2005); $post_t$ is an indicator for being younger than 12 in 1997; and δ_m and η_t are municipality and cohort fixed effects, respectively. γ absorbs any cross-cohort changes that differentially affect municipalities with more eligible households, while β captures the effects of having greater enrollment intensity in the first rather than second phase of rollout. We provide evidence on the importance of γ in the Appendix and discuss it in Section 5, alongside robustness and falsification checks.

Coupled with our focus on high and very-high marginality municipalities, our inclusion of $enroll_m^{2005}$ goes a long way in addressing concerns about cross-cohort convergence across poorer and richer municipalities. However, Figure 2 did show slightly different relationships between the marginality index and enrollment ratios in the two rollout phases, suggesting that our research design may not fully eliminate concerns about convergence. For further insight into this issue, Appendix Table A1 regresses the new enrollment ratio in each rollout phase on the marginality index and its components in sample municipalities. A standard deviation increase in the marginality index is associated with a 16 percentage point increase in new enrollment during the first phase and a 10 percentage point increase in new enrollment during the second phase, a moderate weakening of the relationship. This moderate weakening occurs for most of the components of the index, so as a robustness check, we also estimate a specification that controls for interactions of $post_t$ with each component:

$$y_{imt} = \beta(enroll_m^{1999} \times post_t) + \gamma(enroll_m^{2005} \times post_t) + (post_t X_m^{1990})'\Psi + \delta_m + \eta_t + \varepsilon_{imt} \quad (2)$$

where X_m^{1990} is a vector of the nine components of the marginality index. In this regression, Ψ absorbs any differential cross-cohort changes driven by variation in measured initial conditions. All regression tables report estimates of β using equations (1) and (2), leaving out the partially exposed group aged 12-14 in 1997. As an alternative to this strategy, we also match on percentiles of the overall marginality index by redefining X_m^{1990} to include a full set of indicators for percentile bins of

the index, reporting the results in the Appendix and Section 5.

If all program effects were private to enrollees, then one could interpret β as quantifying the individual-level effect of childhood enrollment on later life outcomes. However, because our identification strategy leverages variation in program exposure across areas that contain many individuals, β may also reflect spillovers. Indeed, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) find in the original experimental evaluation of Progresa that school enrollment and attendance rose in treatment communities even among children ineligible for the program, implying social spillovers. As such, β is best interpreted as the effect of local program expansion on all children, eligible and ineligible, with its magnitude reflecting a move from having no households enrolled to having all households enrolled.

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

$$y_{imt} = \beta_t enroll_m^{1999} + \gamma_t enroll_m^{2005} + \delta_m + \eta_t + \varepsilon_{imt} \tag{3}$$

We report estimates of β_t in a series of event study diagrams, normalizing β_t to zero for middle cohort of the partially exposed group, aged 13 in 1997. To be consistent with our hypothesized age pattern of program impacts, the event studies should show limited trends in β_t across unexposed cohorts older than 15 in 1997 and positive changes in β_t for the fully exposed cohorts aged 7-11 in 1997.

All estimations are stratified by gender and make conservative assumptions for inference. Conventionally, researchers cluster standard errors at the municipality level in a municipal panel analysis like ours. However, the maps in Appendix Figure A2 show that rollout across municipalities in highly spatially correlated, raising concerns that the conventional approach would overstate the precision of our estimates. To be conservative, we cluster at the state level throughout the analysis.

3.4 Threats to Identification

In leveraging both the timing and intensity of rollout across municipalities, our empirical strategy seeks to ensure unbiased estimates of program effects. While the strategy goes a long way in achieving this goal, threats to identification arise in at least two areas: endogenous enrollment intensity and endogenous migration.

Progresa targets the poor, so municipal poverty dynamics may partially drive the timing of enrollment intensity. In this case, differential cross-cohort changes between municipalities with earlier and later enrollment intensity may reflect childhood exposure to poverty rather than Progresa. We focus on broad implementation phases rather than higher-frequency intervals partly to address this concern. Anecdotal evidence suggests that organizational idiosyncrasies unrelated to underlying poverty were important drivers of the timing of enrollment intensity across these broad phases. Furthermore, by including interactions of $post_t$ with the components of the marginality index, equation (2) eliminates bias stemming from poverty dynamics that are related to measured initial conditions. In any case, if residual poverty dynamics do play a role of the timing of enrollment intensity, they likely bias us against finding positive program impacts. If Progresa tends to enter communities during periods of increased poverty, then childhood exposure to the program is associated with childhood exposure to adverse economic conditions. This reasoning would suggest that our strategy delivers a lower bound on the long-term benefits of the program.[11]

A separate threat to identification is migration. To avoid bias from endogenous internal migration, our strategy requires that we assign program exposure to individuals according to their pre-program municipality of residence, not the current municipality of residence at the time of the 2010 census. The census includes questions on the municipality of residence in 2005 as well as the state of birth, allowing us to 'return' migrants to their places of origin. Our analysis sample shows limited but nontrivial internal migration, with 7 percent of individuals living in a different municipality in 2005 and 8 percent born in a different state, with 11 percent meeting at least one of these conditions. As our main approach, we use the municipality of residence in 2005 as a proxy for the municipality of residence in 1997, before the program.

This approach goes a long way to reduce concern about endogenous adult migration, although it still leaves some room for concern about migration before 2005, when fully exposed cohorts were teenagers, and unexposed cohorts were in their early to mid twenties. An endogenous migration response is likely only for exposed cohorts after schooling age, suggesting that pre-2005 migration does not pose a major problem for our analysis. But some concern remains, especially for individuals who were born in a different state from where they resided in 2005. In the Appendix, we find similar results in alternative estimations assigning birth state (rather than municipal) average

[11] A counterargument is that the same forces would lead older control cohorts to experience adverse economic conditions at labor market entry, potentially biasing our estimates upward.

program exposure to these individuals.

International migration poses a separate set of issues. If Progresa affected emigration from Mexico, then individuals remaining in Mexico may be non-randomly selected, potentially biasing our estimates. While no research has investigated the program's long-term effects on international migration, two studies on the initial years of the program provide conflicting evidence on short-term effects, one suggesting more migration (Angelucci, 2013) and one suggesting less (Stecklov et al., 2005). However, both studies find small impacts (in absolute terms), corresponding to less than a 0.5 percentage point change in the probability of migrating to the United States (on a base of 1 percentage point).

To assess potential selection from international migration, we test for differential changes in municipal cohort size between municipalities more intensively treated in the first and second phases of rollout. For all twenty cohorts in our starting sample, aged 20-39 in 2010, we estimate three municipal cohort sizes by summing the sampling weights within each municipal cohort cell: one count of non-migrants (2005 residents who did not move by 2010), another that includes internal migrants (2005 residents who moved elsewhere in Mexico by 2010), and a third that also includes external migrants (2005 residents who left Mexico by 2010), reported by household member who remained in Mexico at the time of the census. We then use the logarithms of these estimates as outcomes in a cell-level version of equation (3). Appendix Figure A4 presents the resulting event study, revealing that from ages 20 and up in 1997, earlier rollout municipalities experienced differential growth in cohort size. In contrast, the cohort size differential between earlier and later rollout municipalities is more stable across younger cohorts. Consistent with differential migration, the differential growth is strongest for the measure that omits all migrants. In principle, if we perfectly measured the number of migrants, the most inclusive measure of cohort size would show no differential growth, but our approach omits pre-2005 migrants as well as external migrants who did not remain members of resident households.[12] This result suggests that Progresa contemporaneously discouraged young adults from leaving program areas, perhaps because these areas became more appealing places to start a family. Because of these potential migration effects among individuals in their twenties at the start of rollout, our analysis sample only includes individuals who were younger than 20 in 1997.

[12]If we estimate a cell-level version of equation (1) replacing $post_t$ with an indicator for being 20 or older in 1997, we find that a unit increase in our measure of earlier rollout is associated with differential increases in cohort size of 17 percent with migrants, 14 percent with only internal migrants, and 12 percent with both internal and external migrants.

Data from the Census household roster of external migrants also indicate that, in our age groups, international migrants comprised 2 percent of the total population for women and 7 percent for men. These figures do not include migrants who left between 1997 and 2005, but they make clear that our impact estimates are based on the overwhelming majority of young adults who grew up in beneficiary areas. They also make clear that international migration is much more prevalent among men than among women, perhaps implying that our female results may be less subject to selection concerns than our male results. Reinforcing these concerns about selectivity among men, our male sample is approximately 15 percent smaller than our female sample. Mexico's low sex ratio among young adults is a well-known phenomenon, and it is not specific to our dataset (Conover et al., 2015). One underlying cause is migration, but mortality may also play a role in the 2010 census; male life expectancy in Mexico declined in Mexico between 2005 and 2010, driven in large part by a rise in homicide mortality in the age group we study (Aburto et al., 2016). One implication of this phenomenon is that our analyses of women have lower risk of bias than our analyses of men.

Based on the cohort size results, we restrict our sample to individuals who were less than 20 years old in 1997, leaving a sample for the analysis of 20-32 years olds in 2010. To clarify our sample and study design, Figure 3 summarizes sample inclusion and program exposure across cohorts. We omit individuals under 20 in 2010 (7 in 1997) and over 20 in 1997 (33 in 2010). The former group is not old enough for the study of adult economic outcomes, while the latter group shows evidence of a (negative) migration response to early program exposure. Individuals 11 and under in 1997 are fully exposed, while those 15 and over are unexposed, and those aged 12-14 are partially exposed. As described in Section 3.2, estimations of equations (1) and (2) omit the partially exposed group, while event study estimations of equation (3) use all 13 birth cohorts.

Beyond these two main threats to identification, one may be concerned about issues beyond differential trends by the baseline characteristics included in X_m^{1990}. In the Appendix, we find that our main results are robust to controlling for political cleintelism, school construction, and violence.

4 Results

We separate our results into three sets of outcomes: education, labor market, and household/demographic. For each set of outcomes, we provide numerical and graphical representations of the results. For

the former, Tables 3-6 report difference-in-difference estimates based on equations (1) and (2), which pool cohorts into two exposure groups. In each table we present four results: two specifications, one with marginality component interactions and one without, separately for men and women. For brevity, we report only the impact coefficients, β in equations (1) and (2), with the means of the dependent variables. For a graphical representation of our estimation, Figures 4-6 report event study estimates based on equation (3). The figures plot the estimated coefficients on interactions between cohort indicators and the proportion of households enrolled between 1997 and 1999, along with 95 percent 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.

4.1 Educational Outcomes

We study four separate measures of education: completed grades, the proportion with some secondary school completed, the proportion with some high school completed, and the proportion with some college completed. Table 3 reveals that program rollout before the critical primary-to-secondary transition has large, positive effects on completed years of education. The estimated coefficients indicate that full rollout in a municipality raises schooling in exposed cohorts by 1.3-1.4 years for both men and women. Compared to average education, 7.9 years for men and 7.7 for women, this effect corresponds to a 17-18 percent increase.

Which schooling levels account for these increases? For both men and women, Table 3 shows significant effects on the probabilities of accumulating at least one year of secondary school and high school. The secondary school impacts are 29 percentage points for women and 17-18 percentage points for men: enormous when compared to sample-wide secondary schooling rates (58 percent among men, 54 percent among women). For high school, the effects are no less impressive. Childhood exposure to Progresa raises the probability of completing some high school 10-15 percentage points for both sexes, relative to a mean of one-quarter. The secondary and high school impacts thus range from approximately one-third to two-thirds of sample-wide schooling rates. All of the aforementioned effects are significant at the 1 percent level. In contrast, we find no significant effects on college enrollment, although the coefficients for men are positive. To visualize these effects grade by grade, Appendix Figure A5 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 enroll in the next level of schooling, they stay on until its conclusion.

Figure 4 shows event study graphs for education, providing additional clear evidence of Progresa-related gains. Beginning with grades of completed schooling, the estimated coefficients rise with program exposure for both men and women aged 7-11 in 1997, consistent with positive program impacts on education. The partially exposed group, aged 12-14 in 1997, displays smaller but still positive gains over the unexposed group, aged 15 and over in 1997. Additionally, the coefficients are relatively flat across unexposed cohorts, supporting a lack of differential pre-program trends between municipalities with earlier and later enrollment intensity. For both genders, the event study graphs for the proportion with some secondary school and some high school show similar patterns, with the largest impacts for the fully exposed group and smaller impacts for the partially exposed group, as well as little evidence of pre-program trends in the unexposed group. In the final panel of Figure 4, the event study coefficients for some college are completely flat across all cohorts, suggesting that childhood exposure to Progresa does not raise college attendance. Overall, the estimated education impacts represent remarkable increases in educational attainment among children who grow up with Progresa.

4.2 Labor Market Outcomes

We now turn to the long-run impacts of Progresa on labor market outcomes, studying labor market participation, participation in paid and agricultural work, hours, job-related health insurance, and labor earnings. The large increases in education documented in Table 3 may translate to improved outcomes in the labor 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 labor market effects, other mechanisms may also be at work. For example, due to program 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-rollout municipality.

Consistent with the large impact on female education relative to baseline, Table 4 reveals that Progresa had equally impressive effects on female labor market outcomes. Against a participation

rate of 26 percent, program impacts of full municipal rollout on labor force participation range from 6 to 11 percentage points. Virtually all of this impact is driven by increases in wage work, which also increases 7-11 percentage points compared with a mean of 17 percent. Progresa also raises hours worked and labor earnings. Unconditional labor supply increases by 3 to 6 hours per week on average, relative to a mean of 10 hours. Affiliation with the social security health insurance system, a measure of formal sector attachment, increases by a few percentage points, but insignificantly. Program impacts on monthly labor market earnings range from 335 to 438 pesos, relative to a mean of 670 pesos. For labor force participation, wage work, hours, and earnings, estimates based on equation (1)—without the marginality component interactions—are all significant at the 8 percent level and sometimes significant at the 5 percent level, while estimates based on equation (2)—with the interactions—are all significant at the 5 percent level. The corresponding event studies in Figure 5 show gains for post-program cohorts in earlier rollout municipalities for all labor market outcomes except agricultural work, although the individual 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. An exception to this pattern is the event study for labor earnings, which shows some evidence of a pre-trend for women. However, the event study for wage work is flat for unexposed cohorts, and we find below that most of the earnings effect can be attributed to a shift on the extensive margin from zero to positive pay.

Table 4 also demonstrates some labor market effects for men. While the probability of working or receiving a wage for men does not change significantly (81 percent of the the sample works), we observe large reductions in agricultural work, increases in hours worked, and increases in formal sector attachment. Hours worked rise on the order of 3 per week, compared to an average of 36, while agricultural employment declines 9-11 percentage points against a base of 39 percent, and job-related health insurance rises 8-9 percentage points on a base of 14 percent. In the specification with marginality component interactions, all three results are significant at the 5 percent level; without the interactions, the labor supply result is significant at 5 percent, while the others are significant at 7-8 percent. Effects on labor earnings are insignificant, although the coefficients are positive and roughly as large as those for women. The larger standard errors for men reflect their higher income dispersion.

For men, the event studies for labor market outcomes in Figure 5 are noisier for participation

in any work and paid work, showing no clear evidence of effects on these outcomes. Other labor market outcomes—including hours, agricultural work, and income—show similar (but still less precise) patterns to education. Agricultural work declines, hours increase, and income increases.[13] Overall, Figure 5 suggests meaningful impacts on labor market outcomes, but estimates are imprecise for individual birth cohorts.

To what extent can the earnings results be attributed to changes in labor force participation and the prevalence of paid work? Figure 7 plots effects on the complementary cumulative distribution of earnings for each sex separately. For a series of thresholds from 0 to 5000 pesos per month (in increments of 100), we estimate versions of equations (1) and (2) in which the dependent variable is an indicator for earnings exceeding the threshold. Consistent with an important role for female labor 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.

An increase in labor supply—on the extensive margin, for women—plays a clear role in the labor market effects documented in Table 4. But to what extent does compensation improve conditional on participation? An analysis of this issue is fraught with selection concerns for women, so we answer this question in Table 5 only for men. Conditional on participation, wage work and labor hours both increase significantly (cols. [1]-[2]), and the point estimates for labor earnings (col. [3]) are larger than in Table 4, albeit still insignificantly different from zero. A common approach to analyzing compensation among workers is to use the logarithm of earnings or wages as the dependent variable, but with only half of workers receiving a wage, the inability of this approach to accommodate zeros may still introduce a selection problem. Indeed, when we use these variables as outcomes in columns (4) and (7), the sample size falls by 40 percent, and the estimated effects, though positive, are imprecisely estimated and unstable across specifications.

Workers in our estimation sample are in their twenties and early thirties, a phase of the lifecycle in which unsalaried work is prevalent, and individual earnings trajectories are steep and heterogeneous. As an alternative approach that gets around the complications these patterns pose for studying compensation, we assign each worker a sectoral earnings score and a sectoral wage score based on the compensation of prime-age men in his sector nationwide. To generate the scores, we

[13]Due to large and significant coefficients for the cohort aged 18 in 1997 in the agriculture and health insurance event studies for men, it is difficult to distinguish a program effect on these outcomes from pre-existing trends.

obtain predicted values from regressions of earnings and wages on occupation and industry indicators in a sample of all male workers ages 35-44.[14] Ages 35 to 44 were chosen because the age profiles of earnings on the intensive and extensive margins are relatively flat over this interval, contrasting the 20-32 age range in our main estimation sample (see Appendix Figure A6). We view the scores as a way to learn about the earnings potentials of the young men in our sample based on their early sectoral affiliations, regardless of their current pay. As revealed in columns (5)-(6) and (8)-(9) of Table 5, we find significant program impacts on both sectoral compensation scores, whether measured in levels or logarithms. In the specification that controls for baseline marginality components interacted with the post-program indicator, estimated effects on all four score outcomes (earnings and wages, in levels and logs) are significant at the 5 percent level. In fact, the point estimates for the earnings score and the log earnings score are very similar to those for individual earnings and log earnings; column (5), Panel B, shows a sectoral earnings impact of 498 pesos per month. Without the marginality interactions, the standard errors for all four estimates are slightly larger, resulting in p-values from 0.037 to 0.095. Progresa exposure shifts male workers into higher-paying sectors.

To summarize this section and the last, in a context of low female labor force participation and education, Progresa has led to striking growth in both areas for young adult women. Male education has increased by similar amounts, accompanied by significant but proportionately more modest effects on labor supply and sectoral affiliation. Estimated effects on reported labor earnings are positive for both sexes but statistically significant only for women, although we find significant effects on sectoral average earnings among male workers. Both the mean and the dispersion of earnings rise steeply with age and experience early in the lifecycle, so these effects will likely grow as Progresa's early beneficiaries approach middle age. Additionally, productivity losses from lost experience due to schooling may fade over time. In fact, about 10 percent of our sample remains in school, so further work and education impacts may become apparent as these youth leave school.

4.3 Household and Demographic Outcomes

A key question is whether Progresa's apparent labor market benefits translate to higher household income and consumption, which bear a more direct link to welfare. Although the census does

[14] We use 1-digit codes from the International Standard Classification of Occupations and the International Standard Industrial Classification.

not directly measure consumption, the housing quality and durable goods indices offer proxies based on a subset of the consumption basket. Table 6 shows positive impacts on household housing quality and durable goods ownership for both sexes—significant at the 5 percent level in all specifications—as well as positive impacts on total household labor income, which are significant in all specifications for women but only suggestive for men. Housing conditions improve by 0.2-0.3 standard deviations, durable goods ownership increases by 0.1-0.2 standard deviations, and total household labor income rises on the order of 1000 pesos. The event studies in Figure 6 show flat trends across partially exposed and unexposed cohorts, followed by a sharp increase in all three outcomes in exposed cohorts. Importantly, parents who receiving program benefits for a longer time may have transfered some of their accumulated wealth to their children, so the increased consumption proxies do not necessarily directly reflect the labor 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.

More generally, impacts on these household-level economic outcomes are inter-related with how the program affects household size, parental coresidence, marriage, and fertility, so we also analyze these outcomes in Appendix Table A2. We find no effects on any of these outcomes among women but suggestive evidence of small increases in household size and decreases in parental coresidence among men. The decline in parental coresidence 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 analyze spousal characteristics among married individuals in Appendix Table A3, finding that greater early program exposure is associated with significantly higher spousal education for both sexes; marginally significantly higher spousal age for men and lower spousal age for women; and marginally significantly higher spousal labor earnings for women. These results require cautious interpretation because our young sample's low marriage rate (less than 50 percent in the fully exposed group) raises concerns about selectivity. Nevertheless, they suggest that Progresa's effects on household economic outcomes—particularly total household labor income, for which effects are twice size of those for individual labor income—are partly mediated by the marriage market.

Geographic mobility may be an important channel for program impacts on labor market out-

comes. The final two rows of Table 5 show 6-12 percentage point increase in cross-municipal migration (over the previous five years) and urban residence, which are significant for women but not men. Estimated with the same specification for the same gender, the effects on the two outcomes have similar magnitudes, suggesting that they reflect the same phenomenon. Estimated effects on migration are uniformly greater than the average propensity to migrate, 7 percentage points. The event studies in Figure 6 again show flat trends across partially exposed and unexposed cohorts, followed by a sharp increase in migration by exposed cohorts.

Taken together, these results point to positive effects on household economic status for both men and women, 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, most notably by increasing the average education among both husbands and wives. 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.

5 Sensitivity Analyses and Falsification Checks

Throughout the analysis, we have reported estimations with and without marginality component interactions to give a sense of robustness to controlling for differential trends by key determinants of program rollout. Overall, the results are similar with and without these additional covariates, reducing concerns on our difference-in-difference methodology. For added evidence , this section performs additional checks on our research design. In sensitivity analyses, we change the rule for assigning program exposure to out-of-state migrants; we explore alternative difference-in-difference designs; and we control for the potentially confounding roles of politics, school construction, and violence. Finally, in a falsification check, we repeat our analysis using the 1990 census, years before program rollout. We focus on seven key outcomes from our main analysis that are also available in the 1990 census: years of education, working, working for pay, weekly labor hours, monthly earnings, housing conditions, and urban residence.

The first sensitivity analysis involves the way we assign program exposure to migrants. So far, our strategy has been to use the municipality of residence in 2005. However, some individuals in

our sample were born in a different state from where they lived in 2005, and where these migrants resided in the late 1990s is unclear. As an alternative to our main strategy, we assign these migrants state-level enrollment ratios based on their states of birth. Additionally, 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 in which the average marginality index of the state exceeds the municipal 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 enrollment in 1999 and 2005. As reported in Appendix Table A4, this alternative approach leads to small variations in the estimated effects, but significance levels are similar, and the overall conclusions are unchanged.

In a second sensitivity analysis, Appendix Table A5 assesses the importance of $enroll_m^{2005} \times post_t$, which accounted for cross-cohort trends differentially affecting municipalities with more eligible households. If we leave $enroll_m^{2005} \times post_t$ out of equation (1), corresponding to a simpler difference-in-difference design, we ask whether outcomes improved in early rollout municipalities relative to all other municipalities, rather than municipalities with the same 2005 penetration but later roll-out. In this approach, our results are broadly similar, except that earnings impacts are large and statistically significant for both men (1710 pesos per month) and women (511 pesos per month). If we estimate equation (2) without $enroll_m^{2005} \times post_t$, earnings impacts are 511 for men and 354 for women, both statistically significant and similar in magnitude to our main results. These two sets of estimations suggest that $enroll_m^{2005} \times post_t$ and the marginality interactions absorb much of the same variation, which is unsurprising because the marginality index is highly predictive of cumulative enrollment in 2005.[15] We treat this variation as confounding but in fact cannot distinguish differential trends from accumulated impacts to longer-term program exposure, both of which are differenced out by the inclusion of $enroll_m^{2005} \times post_t$. To see this point more clearly, Appendix Figure A7 plots education and earnings event studies that do and do not control for the interaction of $enroll_m^{2005}$ with cohort indicators. Both with and without $enroll_m^{2005}$, the event studies for men and women are flat across pre-program cohorts, but the take-off in post-program cohorts is larger and more significant without $enroll_m^{2005}$. These new event studies highlight the fact that the youngest co-

[15] A regression of $enroll_m^{2005}$ on the components of the marginality index has an R^2 of 0.81.

horts may have started secondary school early in the control period. Our research design is unable to distinguish the effects of accumulated program exposure after 2000 from the onset of confounding trends in exposed cohorts, so we emphasize the more conservative estimations that difference out post-2000 exposure.

A third sensitivity analysis considers whether the marginality component interactions in equation (2) are sufficiently flexible. As an alternative approach, Appendix Table A6 splits the overall marginality index into 100 bins of equal size and interacts a full set of bin indicators with $post_t$: equivalent to matching on percentiles of the marginality index. The estimated coefficients are broadly similar to our main results, although they are slightly larger and more significant for earnings and slightly smaller for educational attainment.

The remaining robustness checks return to the original specifications and assignment rules but add control variables to deal with concerns about confounding from politics, school construction, and violence. In Appendix Table A7, we address politics: in particular, the concern that municipalities with a history of supporting Institutional Revolutionary Party (PRI, which was in power until 2000) might have received beneficial treatment during the early phase of rollout. To address the possibility that these PRI municipalities were on differential cross-cohort trends, we interact the post-cohort indicator with the municipality's PRI vote share in the 1994 presidential election and add this interaction to specification (2). In Appendix Table A8, we consider whether a school construction program that began in the mid-1990s can explain our estimated effects of Progresa. We interact the post-cohort indicator with the numbers of new schools per capita in 1995-2000 and 2000-2005 and add both interactions to specification (2). Finally, in Appendix Table A9, we assess whether the surge in drug-related violence after 2006 biases our results. Here, we interact the post-cohort indicator with the change in the municipal homicide rate from 2006 to 2010. In all three cases, the additional covariates do not appreciably change the estimated effects or their significance levels.

As a falsification exercise, we apply our research design in a dataset that should show no program effects. We do so using the 10 percent sample of the 1990 census, which was conducted 20 years before our main dataset was collected and 7 years before the start of Progresa. In Appendix Table A10, we assign program exposure based on municipality of residence and age in 1977, which preceded our main cohort classification year by 20 years. Carrying out this exercise for 10 outcomes for each gender, we find 1 significant coefficient out of 14 estimated, and it is of the wrong sign: a

decrease in urban residence for men. The 1990 results suggest that our 2010 findings do not reflect longstanding differential trends between earlier and later rollout municipalities.

6 A Lower Bound on Benefit/Cost Ratios

Do the benefits from increased lifetime earnings exceed the costs of the program? While several previous studies of Progresa have simulated benefit/cost estimations based on initial education impacts and under varying assumptions of the likely returns to schooling (Behrman, Sengupta and Todd, 2005; Behrman, Parker and Todd, 2011) our long-term impacts allow us to calculate benefits based on actual earnings and completed education impacts. We consider our estimates to be a lower bound because we (1) assume no impacts on outcomes that are not captured by earnings, and (2) use a conservative estimate of the earnings impact. We also use two conservative scenarios for growth in earnings, one with zero growth and one with 2 percent annual growth. For completeness, we include both men and women in our calculation of this lower bound. We acknowledge that the case for including women is more clear-cut than that for including men, given that we find a statistically significant program impact on the level of earnings only for women. However, the combined evidence in Tables 4-6 suggests that male earnings impacts are similar in magnitude to those for women.[16] First, the point estimates for men's earnings in the full sample are similar to (and statistically indistinguishable from) those for women's earnings, even if they have larger standard errors. Second, we find a significant impact on sectoral earnings among male workers. In the more demanding specification with 1990 marginality interactions, exposure to full rollout of Progresa leads to a 498 increase in sectoral average earnings, which multiplied with the male labor force participation rate of 81 percent implies a sectoral earnings impact 403 for all men: again similar to the female earnings impact. If we pool men and women, we estimate individual earnings impacts of 524 (std. err. = 296) and 381 (std. err. = 247), respectively, without and with interactions of the post cohort indicator and the 1990 marginality components. We rely on the smaller of these two estimates (from the specification with marginality interactions) for our full-sample cost-benefit calculations.[17]

[16]In fact, Appendix Table A5 and Appendix Figure A7, which leave out the $enroll_m^{2005}$ interactions, suggest that these estimates are lower bounds on the male earnings impact.

[17]Results based on pooled estimates are similar to what we would obtain if we allowed costs and benefits to differ by gender.

We assume that an additional year spent in school leads to a corresponding year-long delay of labor market entry. Pooled estimates with marginality interactions indicate that exposure to full rollout raises secondary school enrollment by 23.8 percentage points, high school enrollment by 10.3 percentage points, and completed grades by 16.3 months (1.36 years). If we attribute the increase in overall attainment entirely to secondary and high school and assume minimal dropout within schooling level (consistent with Appendix Figure A5), then 70 percent of the increase is due to time in secondary school and 30 percent is due to time in high school. Using data from sample municipalities in the 2000 census (around the time that exposed cohorts were making enrollment decisions), we find average earnings of 291 pesos per month among dropouts of secondary school age (13-15) and 534 pesos per month among dropouts of high school age (16-18). Therefore, we calculate monthly lost earnings as $0.7 \times 291 + 0.3 \times 534 = 364$ pesos, for an overall opportunity cost of 5932 pesos over the 16 months of the program.

The costs of the program include the administrative costs (costs of transferring benefits, conditionality, and targeting), private costs, and the deadweight loss from raising taxes to pay for the program. The administrative and private costs of participation in the program were calculated in Coady (2000) at 0.113 pesos per peso transferred; we use his estimates here. For each peso of governmental expenditures, as in Duflo (2001), we present two scenarios, 0.2 and 0.6 for deadweight loss.[18] Transfer amounts are calculated as the sum of average yearly transfers per family between the years of 1998 and 2003, before the late treatment group is offered the program. We assume that the value of the transfers to taxpayers is equal to their value to recipients (a conservative assumption, given lower consumption among recipients), so that only the deadweight loss of the transfer accrues to program costs (Behrman, Parker, and Todd, 2011; Dhaliwal et al., 2012). We assume a retirement age of 63, implying 45 years of potential years of work. All costs and benefits are discounted to the beginning of program implementation using a discount rate of 5 percent (Duflo 2001).

As shown in Table 7, our lower bound estimate of the ratio of benefits to costs is well above one in all scenarios. Even the least favorable scenario with no growth in real earnings and high deadweight loss implies a reasonable benefit/cost ratio of 2.8. Our results suggest that Progresa's benefits are likely to far outway its costs even in a context of low (or nonexistent) economic growth.

[18] We do not have estimates of deadweight loss specifically for Mexico.

7 Conclusions

Conditional cash transfer programs 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 as large as or larger than the current poverty effects. We estimate the long-term effects of the Mexican program Progresa on the educational, labor market, household, and demographic outcomes of young adults who effectively grew up with the program. 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 labor 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-program cohorts, the estimated effects on female labor force participation exceed one-quarter, and the estimated effects on female labor earnings exceed one-half. 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 similar education effects but more nuanced labor market effects. While earnings impacts are insignificant (albeit positive) for men, we find significant increases in labor supply, significant decreases in agricultural work, and significant increases in sectoral earnings measures. For both sexes, total household labor earnings, housing conditions, and durable goods ownership rise, although it is unclear whether the last two results reflect greater human capital or greater parental wealth.

These results are highly encouraging for the long-term prognosis of children from households receiving CCTs. The earnings impacts on young adults, many of whom no longer live with their parents, are not likely to disappear when their parents stop receiving cash transfers. As a consequence, these impacts are likely to represent permanent increases in wellbeing due to Progresa.

Nevertheless, further work is needed, including long-term follow-up studies on other countries' CCT programs. For Progresa, further studies following youth beneficiaries past their early twenties

would be useful, although the difficulty of tracking and the small size of the affected group in the original randomized evaluation seem likely to complicate such efforts. Incorporating migrants to the United States would also be an important area for future research.

Our analysis does not directly speak to the debate over whether cash transfers to poor families should be conditional (Baird et al. 2013; Akresh et al., 2015). At the same time, for conditional transfers to be preferable to unconditional transfers, they must at a minimum improve the lives of children, the sole targets of conditionality. Short-run benefits like increases in school enrollment do not on their own meet this standard, unless one views enrollment as having intrinsic rather than instrumental value. As a result, our evidence of long-run benefits to childhood beneficiaries provide a necessary, though not sufficient, input to the design of anti-poverty programs.

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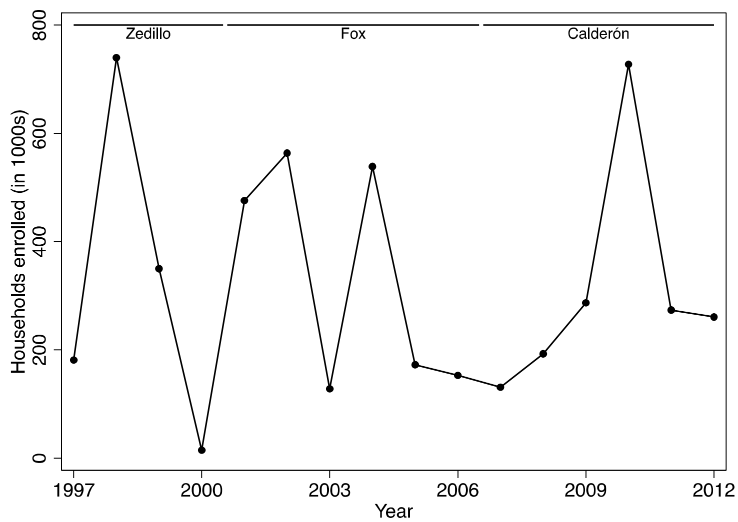
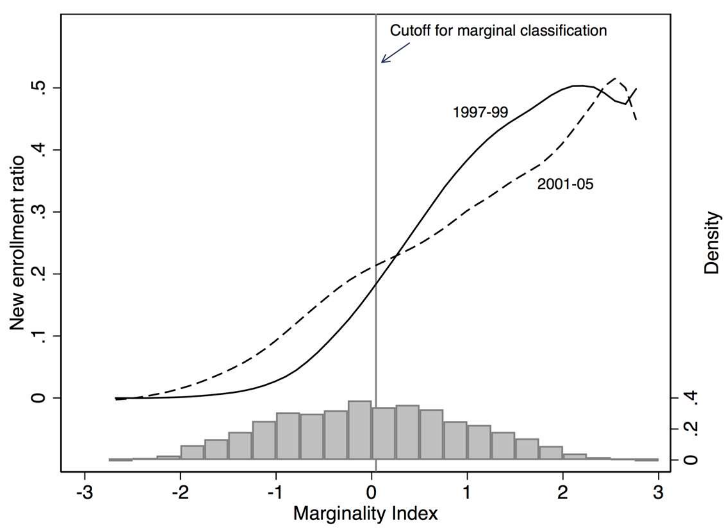
Figure 1: Number of New Households Enrolled Per Year, Nationwide



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

Figure 2: Municipal Economic Conditions and New Enrollment Intensity, 1997-99 vs. 2001-05



Note: Curves are local linear regressions with bandwidths of 0.25; bars are a histogram of the marginality index. The new enrollment ratio is the count of new households enrolled divided by the estimated number of households in the municipality in 1997. The marginalization index is defined (using 1990 census data) by the Mexican government as the normalized first principal component of nine municipal population shares: the share living in communities with less than 5000 inhabitants, the share earning less than twice the minimum wage, the share illiterate, and the shares with less than primary school, without a toilet, without electricity, without running water, with crowding, and with a dirt floor.

Figure 3: Cohort Exposure Timeline

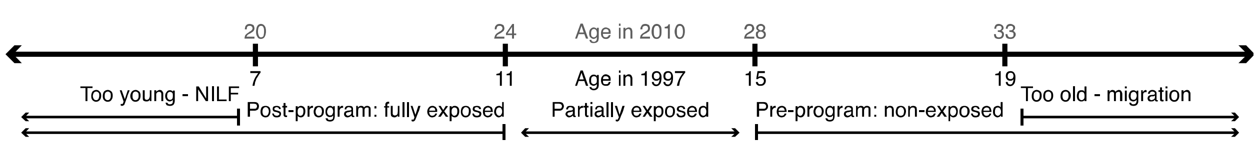
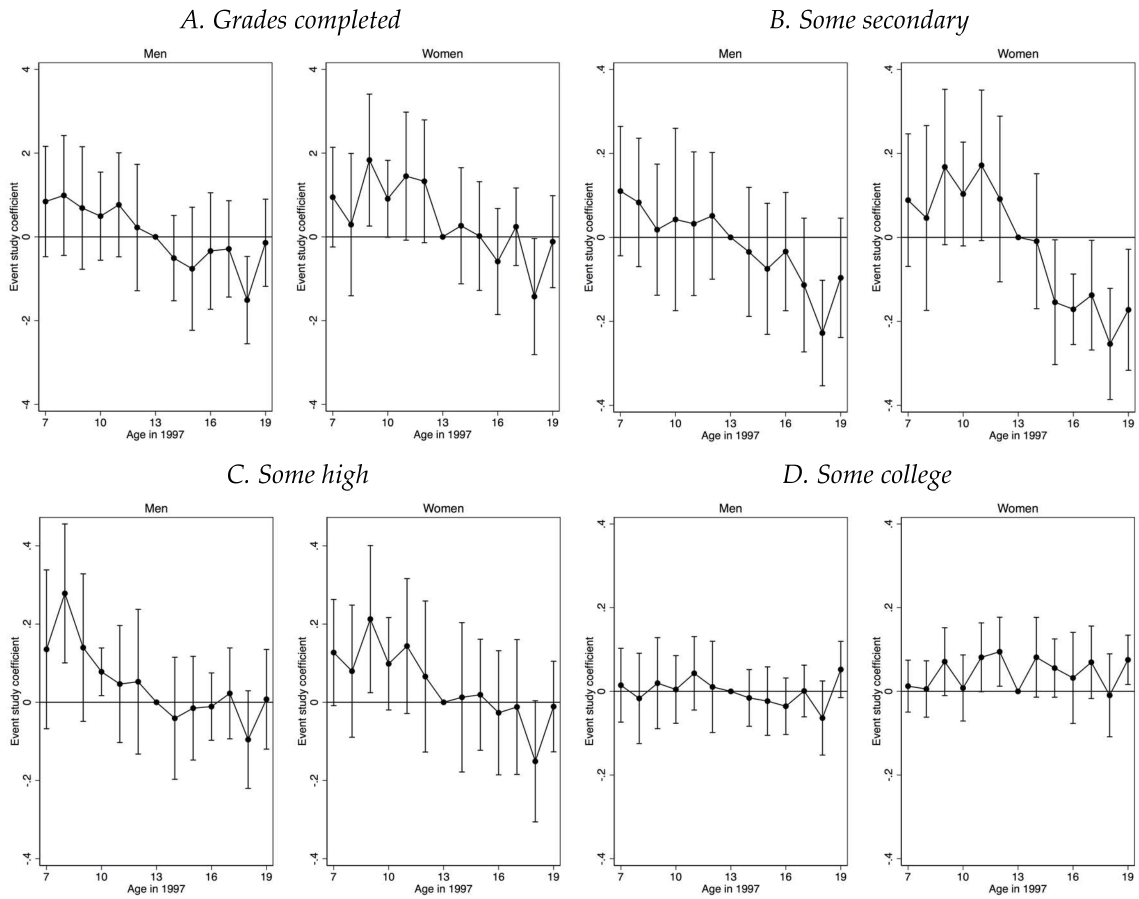
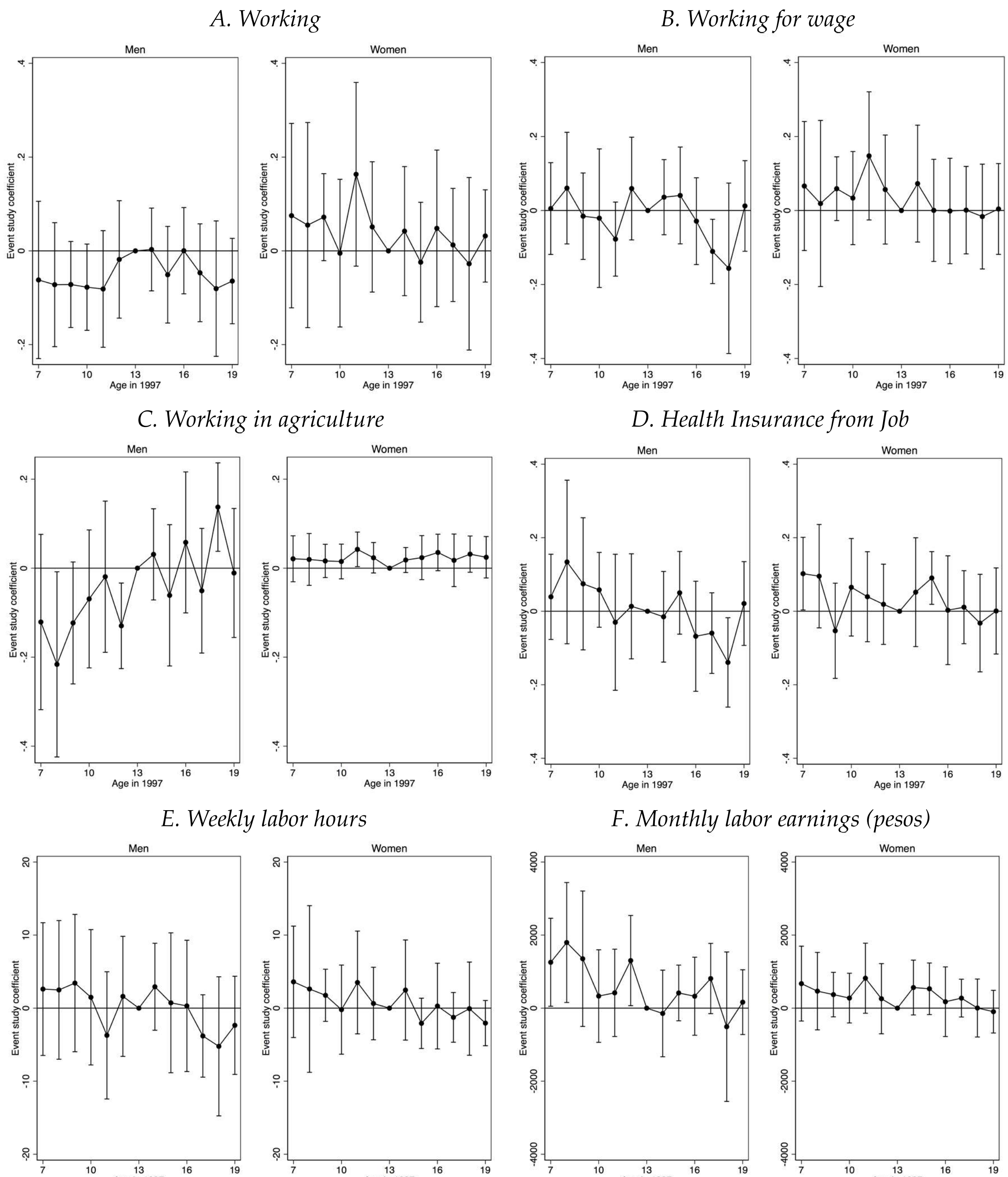


Figure 4: Event Study Graphs, Education



Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. The interaction for the cohort aged 13 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. All regressions include cohort fixed effects, municipality fixed effects, and interactions of cohort indicators with the cumulative enrollment ratio in 2005.

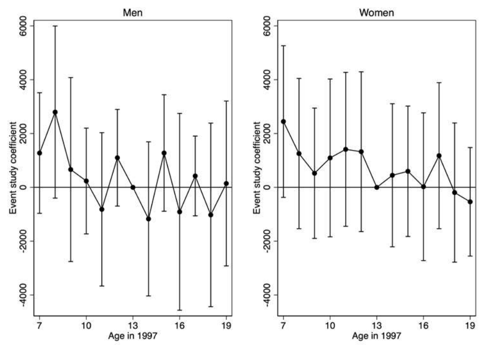
Figure 5: Event Study Graphs, Labor Market Outcomes



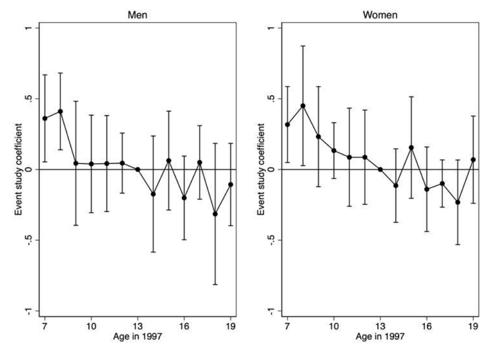
Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. The interaction for the cohort aged 13 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. All regressions include cohort fixed effects, municipality fixed effects, and interactions of cohort indicators with the cumulative enrollment ratio in 2005.

Figure 6: Event Study Graphs, Household and Demographic Outcomes

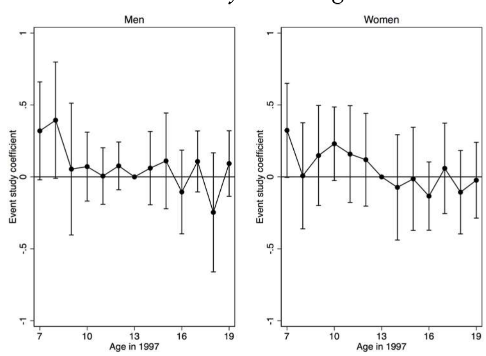
A. Household monthly labor income



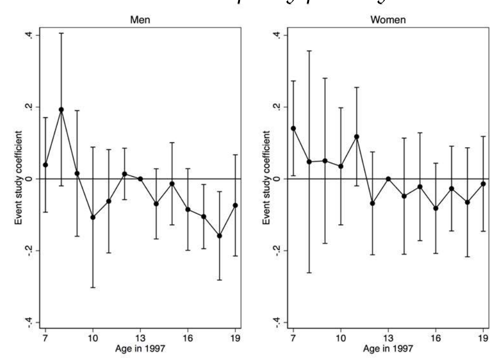
B. Index of housing conditions



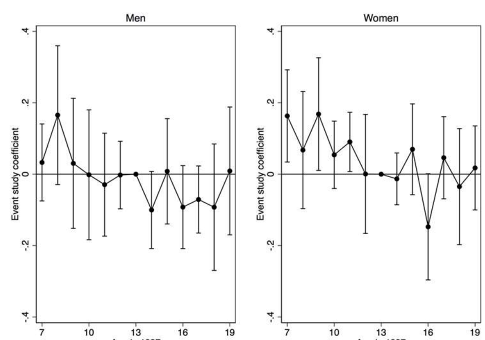
C. Index of durable goods



D. New municipality past 5 years

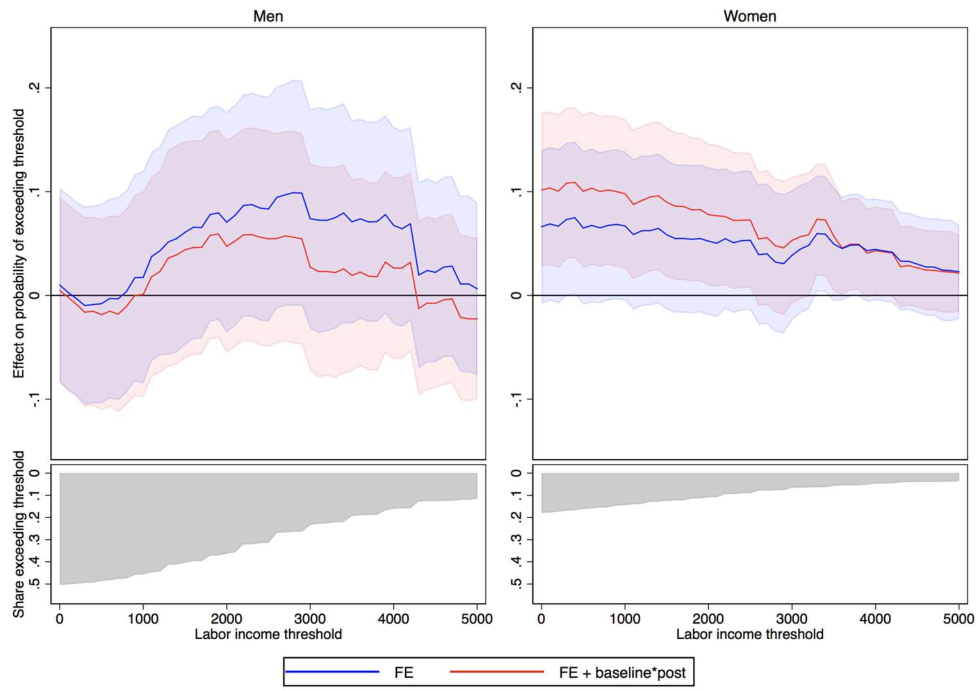


E. Urban residence



Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. The interaction for the cohort aged 13 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. Regressions include cohort and municipality fixed effects, plus interactions of cohort indicators with cumulative enrollment in 2005.

Figure 7: Program Impacts on the Complementary Cumulative Distribution of Earnings



Note: Coefficients on interaction of the post indicator with the cumulative enrollment ratio in 1999. Dependent variable is an indicator for labor income exceeding the specified threshold, which increases in increments of 100. Shaded areas represent 95% confidence intervals based on standard errors clustered at the state level. Regressions include cohort and municipality fixed effects, plus interaction of the post indicator with cumulative enrollment in 2005.

Table 1: Monthly Amount of Schooling Grants, 1997 and 2003

	2nd semester 1997		2nd semester 2003	
	Boys	Girls	Boys	Girls
Primary school				
3rd year	60	60	105	105
4th year	70	70	120	120
5th year	90	90	155	155
6th year	120	120	210	210
Secondary school				
1st year	175	185	305	320
2nd year	185	205	320	355
3rd year	195	225	335	390
High school				
1st year			510	585
2nd year			545	625
3rd year			580	660
Max HH amount without high-schooler	550		950	
Max HH amount with high-schooler			1635	

Note: Amounts in nominal pesos. The peso-to-dollar exchange rate was exchange rate was roughly 8 in 1997 and 11 in 2003. Source: www.prospera.gob.mx.

Table 2: Municipal Marginality and Program Rollout

	Very low/low/medium marginality	High/very high marginality
	(1)	(2)
A. Program rollout		
Cumulative enrollment ratio, 1999	0.06	0.34
	(0.07)	(0.16)
Cumulative enrollment ratio, 2005	0.19	0.63
	(0.15)	(0.22)
B. Marginality index and components		
Index (1st principal component)	-0.79	0.86
	(0.56)	(0.57)
Share illiterate	0.13	0.34
	(0.07)	(0.13)
Share without toilet	0.26	0.60
	(0.17)	(0.19)
Share without electricity	0.12	0.37
	(0.10)	(0.25)
Share without running water	0.19	0.51
	(0.15)	(0.24)
Share with dirt floor	0.22	0.62
	(0.14)	(0.22)
Share earning <2x minimum wage	0.69	0.86
	(0.12)	(0.08)
Share with primary education or less	0.46	0.70
	(0.12)	(0.10)
Share with crowding	0.60	0.74
	(0.10)	(0.08)
Share in localities with pop. < 5000	0.60	0.95
	(0.36)	(0.14)
Number of municipalities	1239	1143
Number of states	32	24

Note: The cumulative enrollment ratio in year *t* is the number of households enrolled in Progresa up to and including *t* divided by the estimated number of households in 1997. The remainder of the analysis focuses on the high or very high marginality municipalities in column (2), where program activities were concentrated.

Table 3: Program Impacts on Educational Attainment

	Men		Women	
	(1)	(2)	(3)	(4)
A. Grades completed				
Enrollment ratio, 1999	1.310	1.373	1.370	1.339
× post cohort	[0.405]	[0.225]	[0.392]	[0.441]
Mean (SD)	7.90 (3.96)		7.65 (4.07)	
Observations	319,714		375,892	
B. Some secondary				
Enrollment ratio, 1999	0.165	0.176	0.288	0.290
× post cohort	[0.054]	[0.045]	[0.057]	[0.081]
Mean (SD)	0.58 (0.49)		0.54 (0.50)	
Observations	320,423		376,753	
C. Some high				
Enrollment ratio, 1999	0.145	0.098	0.161	0.105
× post cohort	[0.058]	[0.032]	[0.042]	[0.036]
Mean (SD)	0.26 (0.44)		0.25 (0.43)	
Observations	320,423		376,753	
D. Some college				
Enrollment ratio, 1999	0.023	0.025	-0.015	-0.014
× post cohort	[0.017]	[0.018]	[0.023]	[0.019]
Mean (SD)	0.09 (0.28)		0.08 (0.28)	
Observations	320,423		376,753	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Sample includes individuals from high and very high marginality municipalities who were aged 7-11 and 15-19 in 1997.

Table 4: Program Impacts on Labor Market Outcomes

	Men		Women	
	(1)	(2)	(4)	(5)
A. Working				
Enrollment ratio, 1999	-0.025	-0.020	0.063	0.105
× post cohort	[0.037]	[0.030]	[0.036]	[0.032]
Mean (SD)	0.81 (0.40)		0.26 (0.44)	
Observations	320,133		377,236	
B. Working for wage				
Enrollment ratio, 1999	0.037	0.039	0.067	0.107
× post cohort	[0.043]	[0.037]	[0.032]	[0.034]
Mean (SD)	0.48 (0.50)		0.17 (0.36)	
Observations	313,459		374,600	
C. Working in agriculture				
Enrollment ratio, 1999	-0.114	-0.087	-0.003	0.004
× post cohort	[0.065]	[0.038]	[0.009]	[0.010]
Mean (SD)	0.39 (0.49)		0.03 (0.17)	
Observations	317,865		376,067	
D. Health insurance from job				
Enrollment ratio, 1999	0.089	0.078	0.031	0.023
× post cohort	[0.049]	[0.034]	[0.028]	[0.018]
Mean (SD)	0.14 (0.35)		0.13 (0.34)	
Observations	319,695		376,409	
E. Weekly labor hours				
Enrollment ratio, 1999	3.448	3.032	3.459	5.547
× post cohort	[1.681]	[1.457]	[1.753]	[1.675]
Mean (SD)	36.18 (24.24)		10.30 (20.54)	
Observations	316,879		376,081	
F. Monthly labor earnings				
Enrollment ratio, 1999	725	281	335	438
× post cohort	[541]	[395]	[185]	[172]
Mean (SD)	2081 (4839)		670 (2168)	
Observations	307,326		373,088	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. All labor market outcomes are unconditional on labor force participation. Sample includes individuals from high and very high marginality municipalities who were aged 7-11 and 15-19 in 1997.

Table 5: Program Impacts on Compensation, Conditional on Participation, Men

	Working for wage	Weekly labor hours	Monthly labor earnings	Log earnings	Sectoral earnings score	Log earnings score	Log wage	Sectoral wage score	Log wage score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Without 1990 marginality interactions									
1999 ratio	0.105	5.550	972	0.249	646	0.126	0.120	11.474	0.110
× post	[0.048]	[1.926]	[677]	[0.176]	[309]	[0.069]	[0.154]	[6.417]	[0.066]
B. With 1990 marginality interactions									
1999 ratio	0.110	4.703	448	0.115	498	0.098	0.026	8.248	0.083
× post	[0.047]	[1.865]	[510]	[0.132]	[197]	[0.044]	[0.102]	[4.100]	[0.042]
Mean	0.49	43	2006	7.81	40,711	8.15	2.60	88	4.32
(SD)	(0.50)	(18)	(5091)	(0.79)	(2717)	(0.56)	(0.79)	(58)	(0.54)
Observations	248,428	251,848	242,295	147,674	254,358	252,581	145,930	252,581	252,581

Note: Estimates are conditional on working. Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Sample includes male workers from high and very high marginality municipalities who were aged 7-11 and 15-19 in 1997. The wage is calculated as monthly earnings divided by monthly hours. The sectoral earnings and wage scores are obtained by regressing the earnings and wages on indicators for 1-digit occupation and industry codes (from the International Standard Classification of Occupations and the International Standard Industrial Classification) in the nationwide sample of prime-age male workers, ages 35-44.

Table 6: Program Impacts on Household and Demographic Outcomes

	Men		Women	
	(1)	(2)	(4)	(5)
A. HH monthly labor earnings				
Enrollment ratio, 1999	735	1211	1092	1284
× post cohort	[701]	[650]	[502]	[461]
Mean (SD)	3856 (8146)		1863 (7429)	
Observations	312,730		376,384	
B. Index of housing conditions				
Enrollment ratio, 1999	0.263	0.177	0.278	0.179
× post cohort	[0.116]	[0.070]	[0.098]	[0.081]
Mean (SD)	-0.02 (1.00)		0.02 (0.99)	
Observations	315,190		370,997	
C. Index of durable goods				
Enrollment ratio, 1999	0.162	0.121	0.216	0.137
× post cohort	[0.082]	[0.063]	[0.070]	[0.065]
Mean (SD)	-0.02 (1.00)		0.02 (1.01)	
Observations	316,412		372,371	
D. New municipality last 5 years				
Enrollment ratio, 1999	0.099	0.074	0.119	0.090
× post cohort	[0.079]	[0.049]	[0.054]	[0.043]
Mean (SD)	0.07 (0.25)		0.07 (0.26)	
Observations	321,910		378,674	
E. Urban residence				
Enrollment ratio, 1999	0.083	0.061	0.117	0.061
× post cohort	[0.061]	[0.037]	[0.050]	[0.031]
Mean (SD)	0.35 (0.48)		0.36 (0.48)	
Observations	321,910		378,674	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Sample includes individuals from high and very high marginality municipalities who were aged 7-11 and 15-19 in 1997. The housing index is the standardized first principal component of indicators for having a dirt floor, a modern roof, sewage, a flush toilet, piped water, and electricity; the durables index is the same for indicators for having a car, a mobile phone, a computer, a washer, a refrigerator, a television, and a hot water heater.

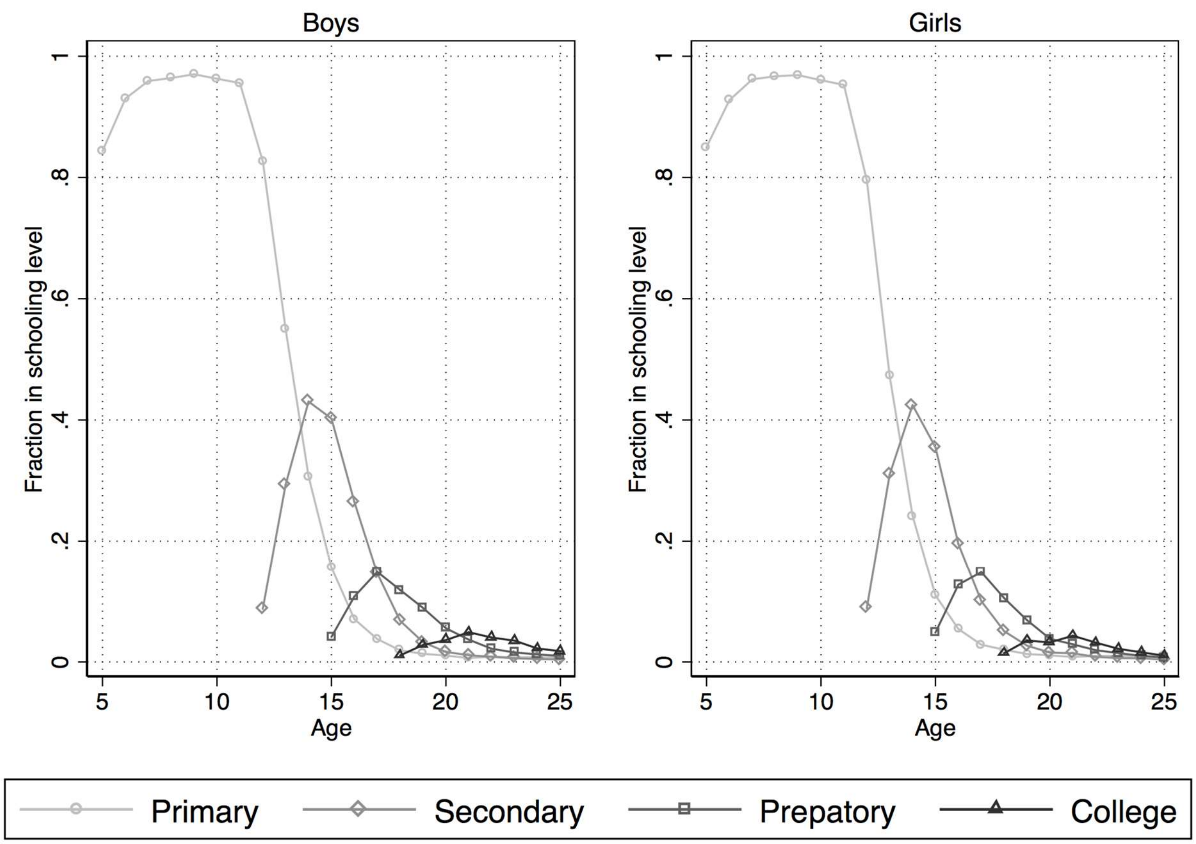
Table 7: Evaluation of Program Benefits and Costs

	No ann. earnings growth		2% ann. earnings growth	
	DWL = 0.2	DWL = 0.6	DWL = 0.2	DWL = 0.6
	(1)	(2)	(3)	(4)
Lower bound on benefits, 2010 pesos	47,037	47,037	81,915	81,915
Costs, 2010 pesos	7,456	16,985	7,456	16,985
Lower bound on net benefits as % of 2010 GDP p.c.	38%	29%	71%	62%
Lower bound on B/C Ratio	6.31	2.77	10.99	4.82

Note: Costs include administrative and private costs = 0.113 per peso of transfers (Coady, 2000), plus the deadweight loss of taxation to finance direct costs and transfers. The discount rate is assumed to be 0.05.

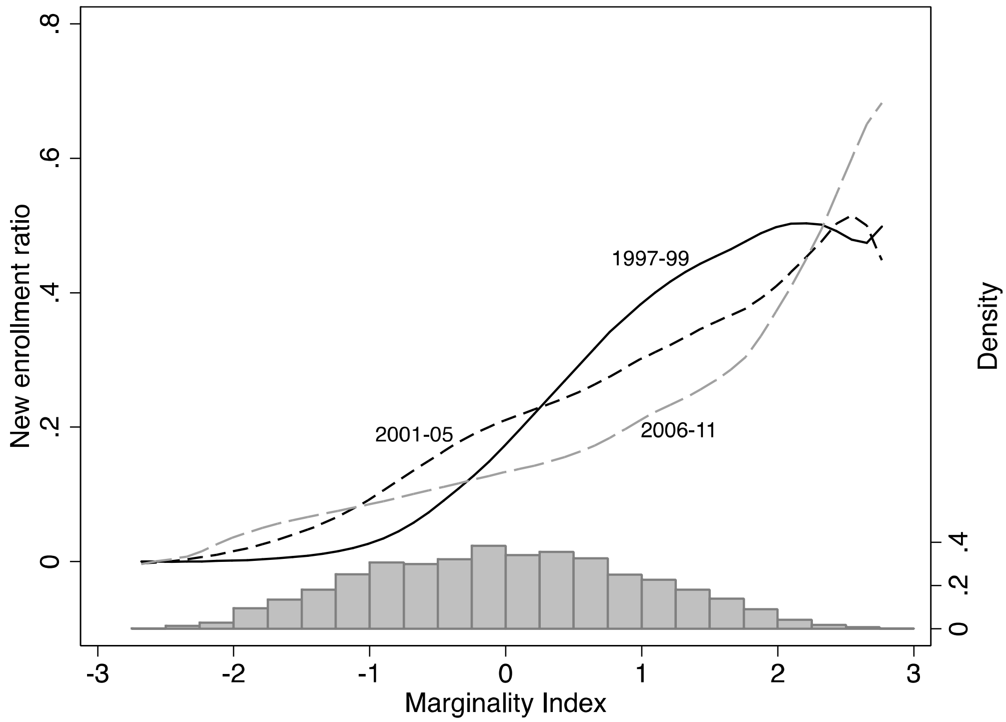
FOR ONLINE APPENDIX

Figure A1: School Enrollment by Age in 2000



Note: School enrollment in sample municipalities in the 2000 Census. Cash transfer conditionality was limited to primary and secondary school in the first program wave (1997-99).

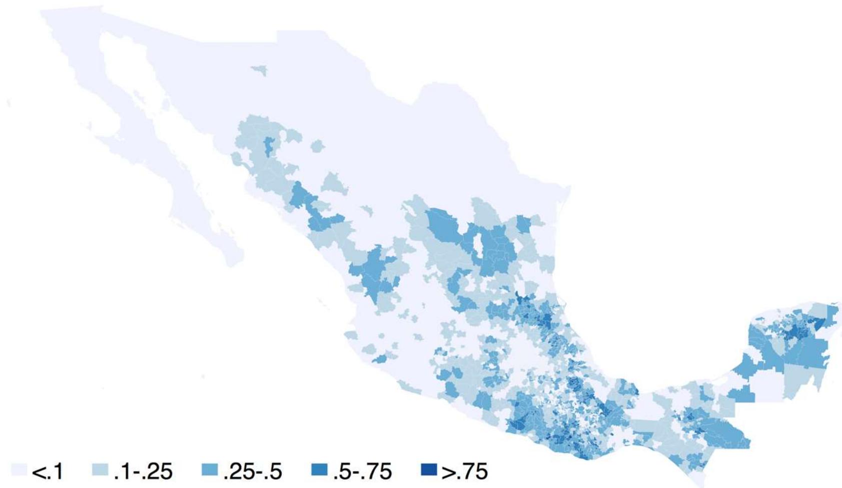
Figure A2: Economic Conditions and Enrollment Intensity, 1997-99, 2001-05, and 2006-11



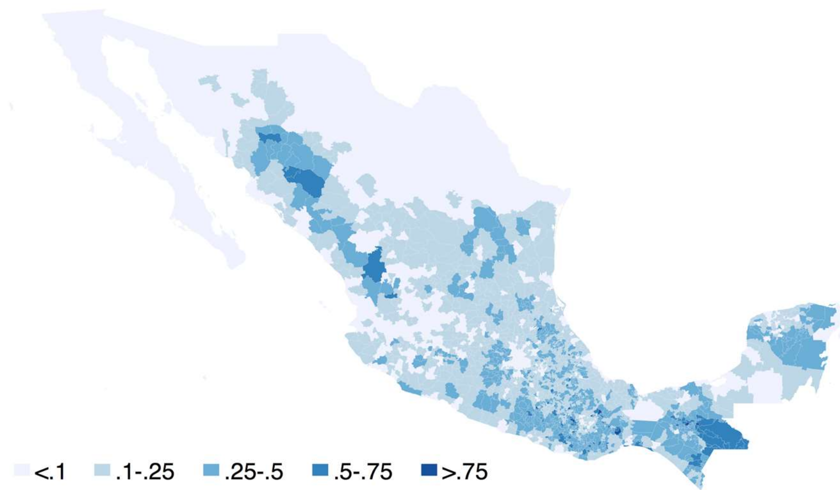
Note: The new enrollment ratio is the count of new households enrolled divided by the estimated number of households in 1997. The marginality index is defined (using 1990 census data) by the Mexican government as the normalized first principal component of nine municipal population shares: the share illiterate, the share with less than primary school education, the share without a toilet, the share without electricity, the share without running water, the share with crowding as measured by number of rooms divided by household size, the share with a dirt floor, the share living in communities with less than 5000 inhabitants, and the share earning less than twice the minimum wage. Curves are local linear regressions with bandwidths of 0.25.

Figure A3: Maps of Enrollment Intensity by Implementation Phase

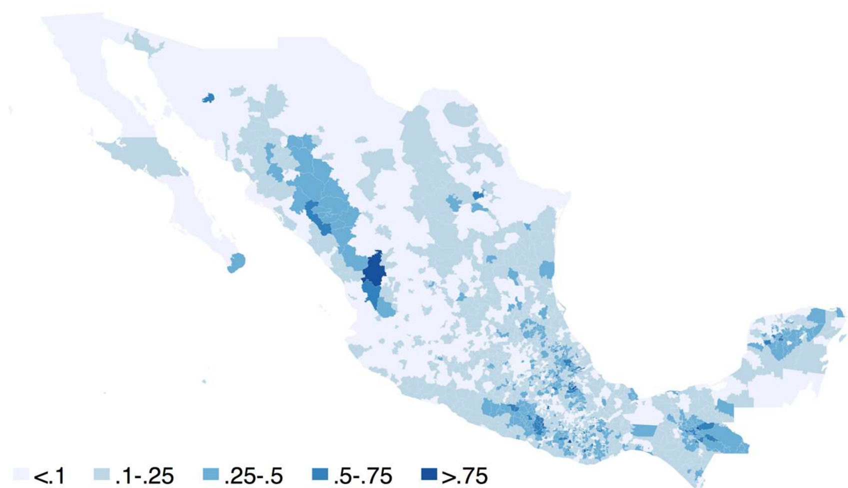
A. 1997-1999



B. 2001-2005

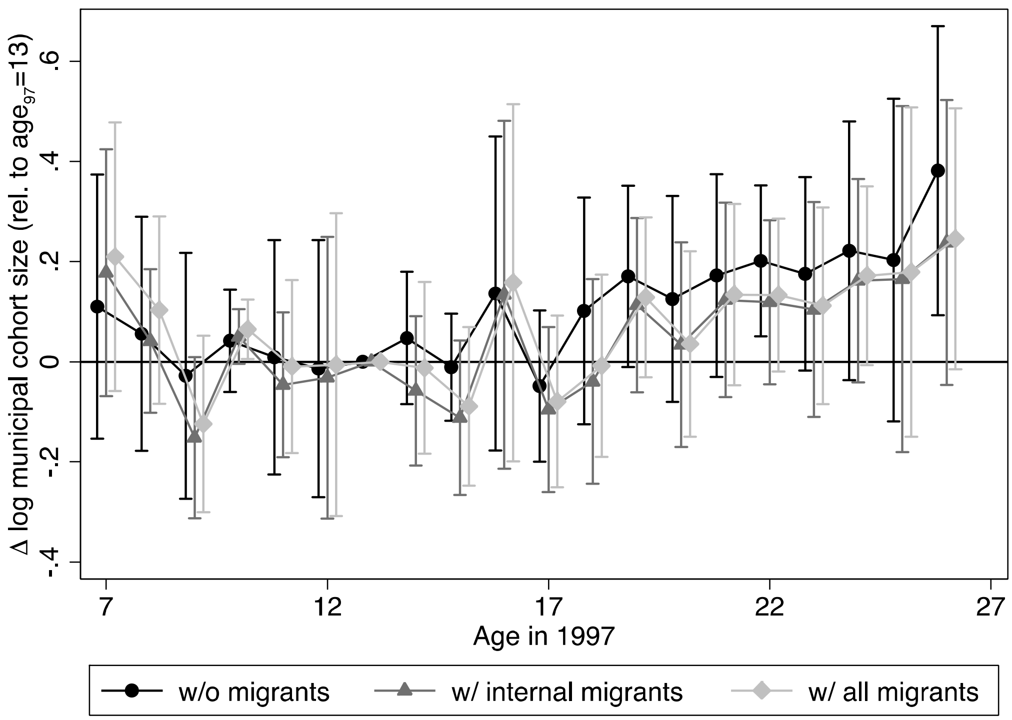


B. 2006-2011



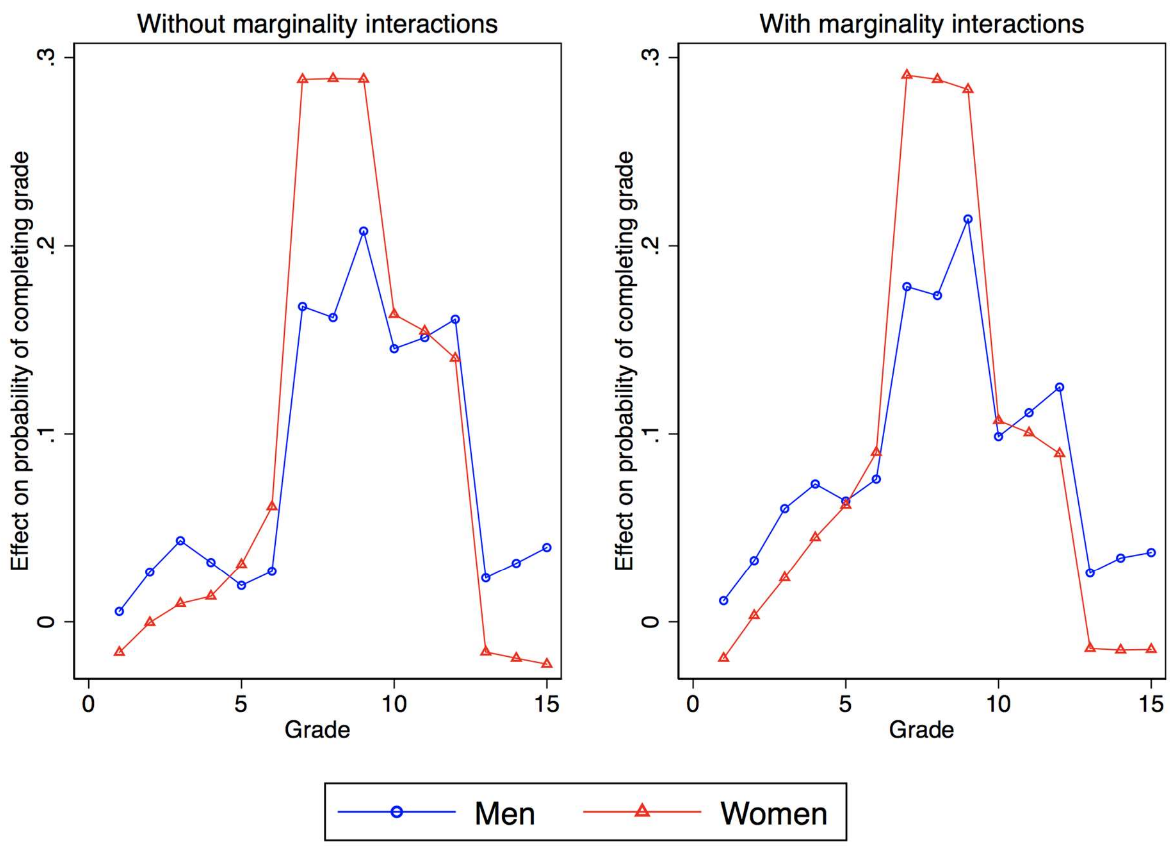
Note: New enrollment ratio is the count of new households enrolled divided by the estimated number of households 1997.

Figure A4: Early Program Exposure and Log Municipal Cohort Size



Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. Regressions include cohort fixed effects, municipality fixed effects, and interactions of cohort indicators with the cumulative enrollment ratio in 2005. For consistency with our main event study graphs, the interaction for the cohort aged 13 in 1997 is omitted. Sample includes individuals aged 20-39 in 2010. Internal migrants are identified by having a different municipality of residence in 2010 than in 2005. International migrants are counted from the household roster of all individuals who left the household for another country since 2005.

Figure A5: Effects on Educational Attainment by Grade

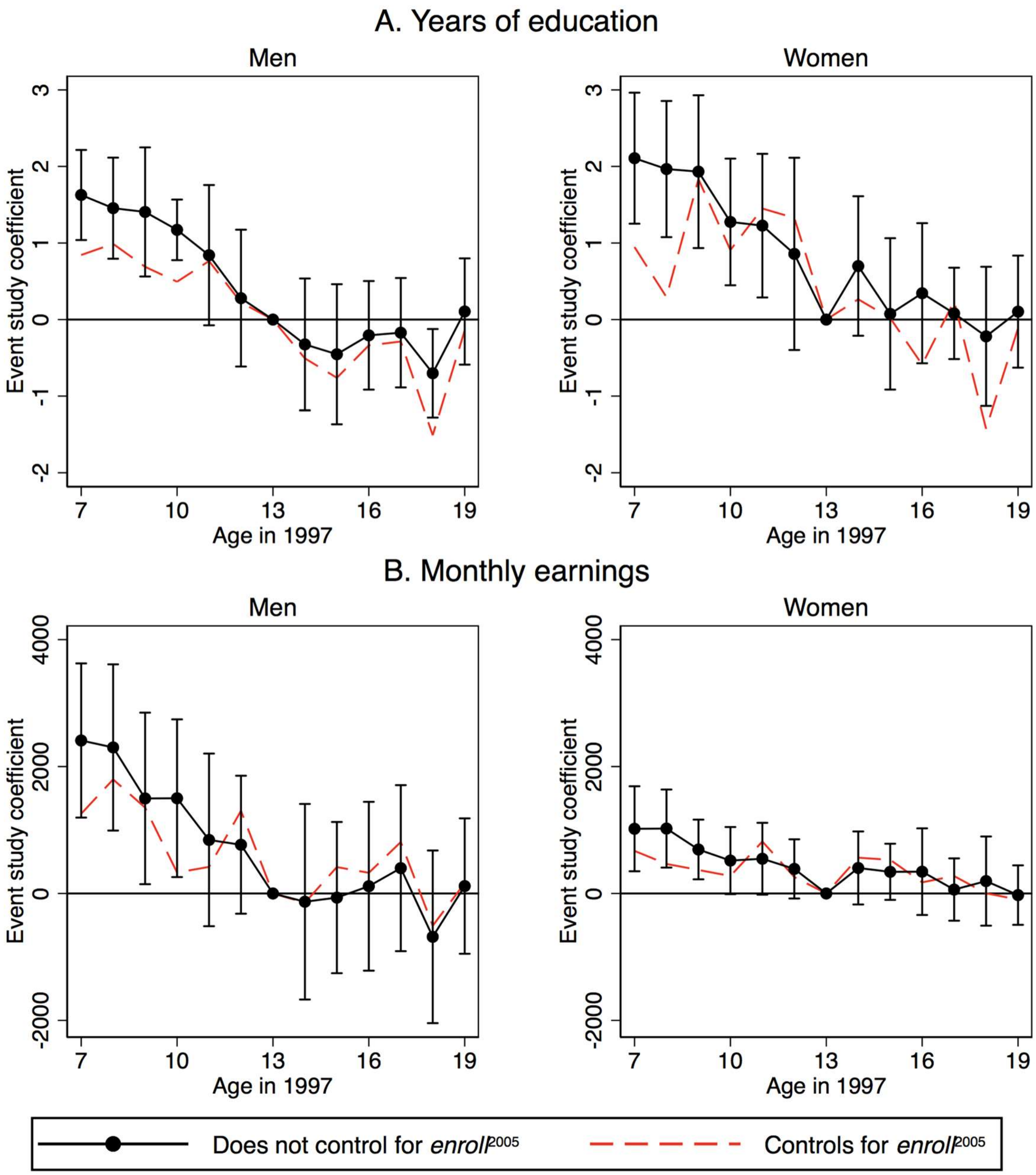


Note: Coefficients on the interaction of the post-cohort indicator with the cumulative enrollment ratio in 1999. Each point is from a different regression in which the dependent variable is an indicator for completing at least *x* years of schooling. All regressions include cohort fixed effects, municipality fixed effects, and interactions of cohort indicators with the cumulative enrollment ratio in 2005.

Figure A6: Lifecycle Profiles of Labor Earnings, Full Population

Note: Means from all men and women in the 10 percent sample of the 2010 census, nationwide. Vertical bars at 35 and 44 indicate the age span for the estimation of the sectoral compensation scores in Table 5. The age span was chosen because all four profiles are relatively flat over the 35-44 interval.

Figure A7: Event Studies with and without $enroll_t^{2005}$



Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. The interaction for the cohort aged 13 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. All regressions include cohort fixed effects and municipality fixed effects; dashed red results include interactions of cohort indicators with the cumulative enrollment ratio in 2005, while solid black results do not.

Table A1: Marginality and Enrollment Intensity, Sample Municipalities, 1997-99 vs. 2001-05

	1997-1999		2001-2005	
	Uni-variate	Multi-variate	Uni-variate	Multi-variate
	(1)	(2)	(3)	(4)
A. Overall marginality index				
Standardized index	0.163		0.102	
	[0.019]		[0.007]	
B. Index components: share of population…				
Illiterate	0.536	0.004	0.335	-0.019
	[0.127]	[0.069]	[0.061]	[0.067]
Without toilet	0.147	-0.005	0.120	0.034
	[0.064]	[0.039]	[0.035]	[0.041]
Without electricity	0.195	0.029	0.193	0.121
	[0.044]	[0.023]	[0.041]	[0.034]
Without running water	0.106	0.010	0.068	-0.029
	[0.026]	[0.018]	[0.036]	[0.035]
With dirt floor	0.386	0.199	0.222	0.077
	[0.053]	[0.036]	[0.028]	[0.034]
Earning <2x minimum wage	0.527	0.224	0.145	-0.005
	[0.136]	[0.048]	[0.070]	[0.067]
With primary education or less	0.761	0.263	0.481	0.207
	[0.150]	[0.074]	[0.084]	[0.082]
With crowding	0.996	0.639	0.573	0.343
	[0.136]	[0.085]	[0.124]	[0.101]
In localities with pop. < 5000	0.141	0.052	0.053	-0.007
	[0.031]	[0.024]	[0.033]	[0.015]
Number of municipalities	1,143	1,143	1,143	1,143

Note: Brackets contain standard errors clustered at the state level. In odd columns, each regression coefficient is from a separate regression; in even columns, all index components are included in the same regression. The dependent variable is the new enrollment ratio in each period: the count of new households enrolled divided by the estimated number of households in 1997. In Panel A, the index is the standardized first principal component of the covariates in Panel B. Sample includes municipalities exceeding the Mexican government's marginality index threshold for classification as high or very high marginality.

Table A2: Program Impacts on Household and Family Structure

	Men		Women	
	(1)	(2)	(4)	(5)
A. Household size				
Enrollment ratio, 1999	0.152	0.530	-0.055	0.198
× post cohort	[0.375]	[0.234]	[0.357]	[0.205]
Mean (SD)	5.44 (2.63)		5.50 (2.55)	
Observations	321,910		378,674	
B. Living with parent				
Enrollment ratio, 1999	-0.154	-0.098	-0.040	0.003
× post cohort	[0.081]	[0.053]	[0.041]	[0.033]
Mean (SD)	0.51 (0.50)		0.35 (0.48)	
Observations	321,910		378,674	
C. Married				
Enrollment ratio, 1999	-0.002	-0.022	-0.030	-0.046
× post cohort	[0.031]	[0.033]	[0.037]	[0.046]
Mean (SD)	0.57 (0.49)		0.64 (0.48)	
Observations	321,368		378,009	
D. Number of coresident children born before age 20				
Enrollment ratio, 1999			-0.062	-0.035
× post cohort			[0.077]	[0.078]
Mean (SD)			0.45 (0.72)	
Observations			378,674	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. We study teenage fertility rather than children ever born to ensure that the exposure period is the same for all women.

Table A3: Program Impacts on Spousal Characteristics, Conditional on Marriage

	Men		Women	
	(1)	(2)	(4)	(5)
A. Spouse's education				
Enrollment ratio, 1999	0.920	0.838	0.996	0.944
× post cohort	[0.434]	[0.270]	[0.273]	[0.292]
Mean (SD)	6.46 (3.67)		6.44 (3.73)	
Observations	179,778		221,463	
B. Spouse's age				
Enrollment ratio, 1999	0.926	0.902	-0.701	-0.235
× post cohort	[0.457]	[0.516]	[0.398]	[0.460]
Mean (SD)	25.03 (5.52)		30.32 (6.90)	
Observations	180,542		222,496	
C. Spouse works				
Enrollment ratio, 1999	0.025	0.026	0.027	0.015
× post cohort	[0.036]	[0.027]	[0.021]	[0.024]
Mean (SD)	0.13 (0.33)		0.87 (0.34)	
Observations	180,051		221,735	
D. Spouse's monthly labor earnings				
Enrollment ratio, 1999	24	50	922	669
× post cohort	[190]	[112]	[444]	[618]
Mean (SD)	256 (1578)		1892 (6013)	
Observations	178,965		212,535	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005.

Table A4: Robustness Check for Key Outcomes: Assigning Exposure to Out-of-State Migrants

	Assignment using municipality of residence 5 years ago		Assignment using state of birth	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
A. Years of education				
Enrollment ratio, 1999	1.603	1.525	1.98	2.166
× post cohort	[0.534]	[0.428]	[0.554]	[0.490]
Observations	319,714	375,892	322,208	376,839
B. Working				
Enrollment ratio, 1999	-0.021	0.064	-0.029	0.090
× post cohort	[0.035]	[0.036]	[0.043]	[0.059]
Observations	320,133	377,236	322,611	378,139
C. Working for wage				
Enrollment ratio, 1999	0.055	0.064	0.093	0.095
× post cohort	[0.047]	[0.035]	[0.050]	[0.038]
Observations	313,459	374,600	315,913	375,478
D. Weekly labor hours				
Enrollment ratio, 1999	4.194	3.551	3.943	5.128
× post cohort	[1.554]	[1.773]	[1.430]	[2.913]
Observations	316,879	376,081	319,343	376,960
E. Monthly labor earnings				
Enrollment ratio, 1999	944	327	1382	449
× post cohort	[577]	[194]	[483]	[329]
Observations	307,326	373,088	309,573	373,842
F. Index of housing conditions				
Enrollment ratio, 1999	0.298	0.334	0.180	0.230
× post cohort	[0.119]	[0.112]	[0.098]	[0.083]
Observations	315,190	370,997	317,573	371,840
G. Urban residence				
Enrollment ratio, 1999	0.105	0.114	0.068	0.057
× post cohort	[0.076]	[0.064]	[0.042]	[0.075]
Observations	321,910	378,674	324,383	379,586

Note: Coefficients on the 1999 enrollment ratio interacted with the post indicator. Brackets contain standard errors clustered at the state level. All regressions control for the interaction of the post indicator with the cumulative enrollment ratio in 2005, as well as cohort indicators and the main effects of the 1999 and 2005 enrollment ratios. The main effects of these variables are included instead of municipality fixed effects because no municipality is assigned to out-of-state migrants in columns (3)-(4). Columns (1)-(2) apply this regression specification to the original sample, assigning program exposure to out-of-state migrants based on municipality of residence in 2005. Columns (3)-(4) apply this regression specification to a larger sample that adds out-of-state migrants whose birth state average marginality index exceeds the municipal threshold for high or very high marginality, regardless of current residence, assigning program exposure to out-of-state migrants based on state of birth. An out-of-state migrant is defined as an individual whose birth state differs from her state of residence in 2005.

Table A5: Difference-in-Difference Omitting $enroll_m^{2005} \times post_t$

	Men		Women	
	(1)	(2)	(4)	(5)
A. Years of education				
Enrollment ratio, 1999	1.583	0.895	1.634	0.673
× post cohort	[0.234]	[0.269]	[0.151]	[0.226]
Observations	319,714		375,892	
B. Working				
Enrollment ratio, 1999	0.056	-0.039	0.066	0.073
× post cohort	[0.019]	[0.024]	[0.018]	[0.024]
Observations	320,133		377,236	
C. Working for wage				
Enrollment ratio, 1999	0.101	0.022	0.026	0.050
× post cohort	[0.024]	[0.034]	[0.020]	[0.022]
Observations	313,459		374,600	
D. Weekly labor hours				
Enrollment ratio, 1999	7.353	2.539	2.356	4.208
× post cohort	[1.288]	[0.791]	[1.060]	[1.586]
Observations	316,879		376,081	
E. Monthly labor earnings				
Enrollment ratio, 1999	1710	511	592	354
× post cohort	[163]	[222]	[123]	[127]
Observations	307,326		373,088	
F. Index of housing conditions				
Enrollment ratio, 1999	0.110	0.135	0.071	0.050
× post cohort	[0.056]	[0.059]	[0.047]	[0.045]
Observations	315,190		370,997	
G. Urban residence				
Enrollment ratio, 1999	-0.002	0.012	0.049	0.018
× post cohort	[0.034]	[0.030]	[0.021]	[0.020]
Observations	321,910		378,674	
Municipality and cohort FE	X	X	X	X
1990 marginality interactions		X		X

Note: Brackets contain standard errors clustered at the state level. All regressions **do not** control for the interaction of the post indicator with the cumulative enrollment ratio in 2005.

Table A6: Matching on Percentiles of the Marginality Index

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Years of education						
Enrollment ratio, 1999	1.310	1.373	1.200	1.370	1.339	1.053
× post cohort	[0.405]	[0.225]	[0.247]	[0.392]	[0.441]	[0.389]
Observations		319,714			375,892	
B. Working						
Enrollment ratio, 1999	-0.025	-0.020	-0.041	0.063	0.105	0.099
× post cohort	[0.037]	[0.030]	[0.024]	[0.036]	[0.032]	[0.036]
Observations		320,133			377,236	
C. Working for wage						
Enrollment ratio, 1999	0.037	0.039	0.045	0.067	0.107	0.103
× post cohort	[0.043]	[0.037]	[0.041]	[0.032]	[0.034]	[0.038]
Observations		313,459			374,600	
D. Weekly labor hours						
Enrollment ratio, 1999	3.448	3.032	1.684	3.459	5.547	5.487
× post cohort	[1.681]	[1.457]	[1.651]	[1.753]	[1.675]	[1.863]
Observations		316,879			376,081	
E. Monthly labor earnings						
Enrollment ratio, 1999	725	281	453	335	438	506
× post cohort	[541]	[395]	[434]	[185]	[172]	[237]
Observations		307,326			373,088	
F. Index of housing conditions						
Enrollment ratio, 1999	0.263	0.177	0.250	0.278	0.179	0.287
× post cohort	[0.116]	[0.070]	[0.092]	[0.098]	[0.081]	[0.099]
Observations		315,190			370,997	
G. Urban residence						
Enrollment ratio, 1999	0.083	0.061	0.074	0.117	0.061	0.104
× post cohort	[0.061]	[0.037]	[0.043]	[0.050]	[0.031]	[0.041]
Observations		321,910			378,674	
Municipality and cohort FE	X	X	X	X	X	X
1990 marginality interactions						
Continuous components		X			X	
Percentile bin indicators			X			X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Column (3) and (6) include interactions of $post_t$ with indicators for percentile bins of the marginality index.

Table A7: Robustness Check for Key Outcomes: Controlling for Politics

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Years of education						
Enrollment ratio, 1999	1.310	1.373	1.385	1.367	1.338	1.472
× post cohort	[0.405]	[0.225]	[0.219]	[0.391]	[0.442]	[0.447]
Observations		319,685			375,840	
B. Working						
Enrollment ratio, 1999	-0.025	-0.02	-0.016	0.063	0.106	0.102
× post cohort	[0.037]	[0.030]	[0.029]	[0.036]	[0.032]	[0.038]
Observations		320,104			377,184	
C. Working for wage						
Enrollment ratio, 1999	0.037	0.039	0.050	0.067	0.107	0.108
× post cohort	[0.043]	[0.037]	[0.039]	[0.032]	[0.034]	[0.039]
Observations		313,430			374,548	
D. Weekly labor hours						
Enrollment ratio, 1999	3.438	3.029	3.159	3.465	5.551	5.421
× post cohort	[1.679]	[1.457]	[1.537]	[1.753]	[1.674]	[1.949]
Observations		316,850			376,029	
E. Monthly labor earnings						
Enrollment ratio, 1999	724	281	315	335	438	456
× post cohort	[541]	[395]	[436]	[185]	[172]	[206]
Observations		307,297			373,036	
F. Index of housing conditions						
Enrollment ratio, 1999	0.263	0.177	0.156	0.278	0.179	0.180
× post cohort	[0.116]	[0.070]	[0.076]	[0.098]	[0.081]	[0.094]
Observations		315,161			370,945	
G. Urban residence						
Enrollment ratio, 1999	0.083	0.061	0.060	0.117	0.061	0.054
× post cohort	[0.061]	[0.037]	[0.035]	[0.050]	[0.031]	[0.036]
Observations		321,881			378,622	
Municipality and cohort FE	X	X	X	X	X	X
1990 marginality interactions		X	X		X	X
1994 PRI vote share * post cohort			X			X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005.

Table A8: Robustness Check for Key Outcomes: Controlling for School Construction

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Years of education						
Enrollment ratio, 1999	1.310	1.373	1.366	1.370	1.339	1.329
× post cohort	[0.405]	[0.225]	[0.233]	[0.392]	[0.441]	[0.430]
Observations		319,714			375,892	
B. Working						
Enrollment ratio, 1999	-0.025	-0.020	-0.020	0.063	0.105	0.107
× post cohort	[0.037]	[0.030]	[0.030]	[0.036]	[0.032]	[0.033]
Observations		320,133			377,236	
C. Working for wage						
Enrollment ratio, 1999	0.037	0.039	0.039	0.067	0.107	0.108
× post cohort	[0.043]	[0.037]	[0.037]	[0.032]	[0.034]	[0.033]
Observations		313,459			374,600	
D. Weekly labor hours						
Enrollment ratio, 1999	3.448	3.032	2.985	3.459	5.547	5.611
× post cohort	[1.681]	[1.457]	[1.418]	[1.753]	[1.675]	[1.680]
Observations		316,879			376,081	
E. Monthly labor earnings						
Enrollment ratio, 1999	725	281	281	335	438	440
× post cohort	[541]	[395]	[393]	[185]	[172]	[175]
Observations		307,326			373,088	
F. Index of housing conditions						
Enrollment ratio, 1999	0.263	0.177	0.178	0.278	0.179	0.178
× post cohort	[0.116]	[0.070]	[0.070]	[0.098]	[0.081]	[0.081]
Observations		315,190			370,997	
G. Urban residence						
Enrollment ratio, 1999	0.083	0.061	0.061	0.117	0.061	0.060
× post cohort	[0.061]	[0.037]	[0.037]	[0.050]	[0.031]	[0.030]
Observations		321,910			378,674	
Municipality and cohort FE	X	X	X	X	X	X
1990 marginality interactions		X	X		X	X
Δ schools per capita, 1995-2000 × post cohort			X			X
Δ schools per capita, 2000-2005 × post cohort			X			X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Schools construction is normalized by the population of children aged 15-17.

Table A9: Robustness Check for Key Outcomes: Controlling for Violence

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Years of education						
Enrollment ratio, 1999	1.31	1.373	1.339	1.370	1.339	1.225
× post cohort	[0.405]	[0.225]	[0.193]	[0.392]	[0.441]	[0.432]
Observations		319,714			375,892	
B. Working						
Enrollment ratio, 1999	-0.025	-0.02	-0.027	0.063	0.105	0.102
× post cohort	[0.037]	[0.030]	[0.033]	[0.036]	[0.032]	[0.028]
Observations		320,133			377,236	
C. Working for wage						
Enrollment ratio, 1999	0.037	0.039	0.032	0.067	0.107	0.102
× post cohort	[0.043]	[0.037]	[0.035]	[0.032]	[0.034]	[0.030]
Observations		313,459			374,600	
D. Weekly labor hours						
Enrollment ratio, 1999	3.448	3.032	2.69	3.459	5.547	5.144
× post cohort	[1.681]	[1.457]	[1.515]	[1.753]	[1.675]	[1.413]
Observations		316,879			376,081	
E. Monthly labor earnings						
Enrollment ratio, 1999	725	281	257	335	438	433
× post cohort	[541]	[395]	[399]	[185]	[172]	[163]
Observations		307,326			373,088	
F. Index of housing conditions						
Enrollment ratio, 1999	0.263	0.177	0.175	0.278	0.179	0.170
× post cohort	[0.116]	[0.070]	[0.071]	[0.098]	[0.081]	[0.083]
Observations		315,190			370,997	
G. Urban residence						
Enrollment ratio, 1999	0.083	0.061	0.058	0.117	0.061	0.058
× post cohort	[0.061]	[0.037]	[0.035]	[0.050]	[0.031]	[0.031]
Observations		321,910			378,674	
Municipality and cohort FE	X	X	X	X	X	X
1990 marginality interactions		X	X		X	X
Δ homicide rate, 2006-10 × post cohort			X			X

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005. Homicide rates are from INEGI.

Table A10: Falsification Check for Key Outcomes: 1990 Census

	Men (1)	Women (2)
A. Years of education		
Enrollment ratio, 1999	0.742	0.529
× post cohort	[0.515]	[0.450]
Observations	91,167	97,977
B. Working		
Enrollment ratio, 1999	0.022	0.002
× post cohort	[0.048]	[0.030]
Observations	90,439	96,908
C. Working for wage		
Enrollment ratio, 1999	0.024	-0.017
× post cohort	[0.034]	[0.028]
Observations	86,018	95,702
D. Weekly labor hours		
Enrollment ratio, 1999	-1.467	0.34
× post cohort	[2.970]	[1.480]
Observations	87,427	95,964
E. Monthly labor earnings (in 2010 pesos)		
Enrollment ratio, 1999	258	-39
× post cohort	[265]	[124]
Observations	85,107	96,794
F. Index of housing conditions		
Enrollment ratio, 1999	-0.091	0.046
× post cohort	[0.088]	[0.107]
Observations	87,301	93,883
G. Urban residence		
Enrollment ratio, 1999	-0.072	0.004
× post cohort	[0.028]	[0.038]
Observations	91,167	97,977

Note: Brackets contain standard errors clustered at the state level. All regressions additionally control for the interaction of the placebo indicator with the cumulative enrollment ratio in 2005. Placebo cohorts were born 20 years before the true post-program cohorts. Sample sizes are smaller than in the 2010 Census because of differences in sampling methods for the public use sample.