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 on the youth they target. This paper estimates the impact of childhood exposure to the Mexican program Progresa on economic outcomes in early adulthood by leveraging the age structure of program benefits and geographic variation in early program penetration. Childhood exposure to Progresa improves educational attainment, geographic mobility, labor market outcomes, and household economic outcomes in early adulthood. Schooling impacts are similar for men and women, at roughly 1.5 years, while labor market impacts are more pronounced for women, amounting to 30-40% of mean labor force participation and 50% of mean labor income in pre-program cohorts. Indexes capturing household economic impacts increase on the order of 0.2 standard deviations.

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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.

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 is notably lacking on whether these increases in education translate to better economic outcomes in the next generation. In part, the lack of evidence reflects the necessity of a relatively long follow-up for measuring impacts on earnings for youth beneficiaries, and long term follow-ups of experimental evaluations remain rare (Behrman, Parker, and Todd, 2011).

This paper studies the long-term impacts of Progresa on the educational, labor market, household, and demographic outcomes of its 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 the impacts in a difference-in-difference design, comparing the early beneficiaries to a slightly older group that was too

[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.

old at rollout to reap the program's educational benefits, across municipalities with varying exposure to early program rollout. Our main data source is the 10% sample of the 2010 Mexican Population Census, linked to administrative information on program enrollment by municipality over time.

Despite the popularity of CCTs, little evidence exists on their long-term or intergenerational effects. Barham, Macours, and Maluccio et al. (2017) study a small CCT experiment in Honduras, in which households were randomized to early (2000-2003) or late (2003-2005) receipt of transfers. Ten years after the start of the experiment, 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. The authors do not analyze data on girls, reporting that the same identification strategy "does not provide easily interpretable results" for girls due to later ages of dropout. In another related study, Barrera-Osorio, Linden, and Saavedra (2017) examine longer-term 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.

Relative to these studies, our evidence derives from Mexico's larger and more influential program and provides the first estimates of the long-term impacts of CCTs on a generation of youth who have effectively grown up with the program. 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 comparison group. We rely on a difference-in-difference design because the original randomized experiment induced only a 1.5-year difference in program exposure, catching only a small share of the evaluation sample at the primary-to-secondary transition. Currently, no long-term followup data exist; the last followup of the experimental sample was carried out in 2007 but characterized by high rates of attrition.

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 complete 1.4 additional grades of schooling, 20 percent of mean educational attainment in pre-program cohorts. 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 7-11 percentage points, more than one quarter of mean participation in pre-program cohorts, while labor earnings rise US$30-40 per month, roughly half of mean earnings in pre-program cohorts. Effects on male labor outcomes are less pronounced but still important. Hours worked increase by more than three hours per week, accompanied by a shift from the agricultural to the non-agricultural sector and a positive but insignificant increase in labor earnings. Both sexes also display positive effects on housing conditions and on ownership of durable goods, although it is not clear whether this result reflects human capital accumulation or the direct benefits of greater parental wealth. The estimated effects are large by any measure, particularly for women, who have historically low status and labor force participation in program areas. 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), 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 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. 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 followup 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. 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. 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.[2] 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).[3] Originally, the program provided grants only for children between the third and 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 below. Once eligible communities are identified, Progresa

[2] 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).

[3] The Mexican education system defines grades 1 through 6 as primary school (primaria), 7 through 9 as junior high school (secundaria), and 10 to 12 as senior high school (media superior or prepatoria).

officials carry out a socio-economic survey for all households in the selected communities and then use discriminant analysis to distinguish eligible from ineligible households using characteristics such as dwelling conditions, dependency ratios, ownership of durable goods, animals and land, and the presence of disabled individuals. Nearly all selected families enrolled in the program in rural areas (Skoufias and Parker, 2001). Skoufias, Davis, and de la Vega (2001) compare the actual targeting algorithm to consumption- and geography-based alternatives and conclude that the program performs well in targeting the poorest households.

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.[4]

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

[4]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 unfortunately suffers from high levels of attrition.

ous 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 tends to 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 change 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 rural and isolated areas of Mexico where our study takes places (OECD, 2013), and program critics have long pointed to the possibility that increasing education levels in these areas may not raise incomes. Previous studies of program effects on standardized achievement tests have suggested limited impacts on learning in spite of increased schooling levels (Behrman, Parker and Todd, 2009), consistent with possible low returns to additional education. Additionally, a large proportion of the population in rural areas is engaged in agriculture, where returns to schooling may be lower. Nevertheless, research on school construction programs in similarly poor and rural areas suggests large long-run benefits (Duflo 2001).

3 Data and Method

3.1 Data

We use the 10% 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. The census applied an extended questionnaire to all household members, providing detailed 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, especially among women, we measure income in levels rather than logs and do not condition on participation. 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.[5] 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 possible role of geographic

[5]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 same for having a car, mobile phone, computer, washer, refrigerator, TV, and hot water heater.

mobility in explaining our results, we consider indicators for urban residence in 2010 and for moving to a new municipality between 2005 and 2010.

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.[6] 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 1144 'master' municipalities with high or very high marginality, of a total of 2383 '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.[7] To measure enrollment intensity over any given period, we use divide new household enrollment during that period by the number of households in the municipality in the 2000 census.[8] 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

[6] 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.

[7] 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.

[8] Results are very similar if we calculate the denominator by interpolating between the 1990 and 2000 census counts to construct the number of households in 1997, at the start of rollout.

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-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 for transparency, 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 Figure A3, 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.

As shown in the Appendix, enrollment patterns in the first phase are more similar to those in the second phase than to those after the second phase. Figure A1 and Table A1 show that, compared with new enrollment in the first two phases, new enrollment during 2006-11 is less associated with the marginality index. When we break up the marginality index into its nine components, one striking results is that the rural population share becomes much less predictive of new enrollment intensity after 2005; its association with new enrollment intensity even turns negative after conditioning on the other components of the marginality index.[9] To demonstrate changes in the geography of rollout more broadly, Figure A2 maps new enrollment ratios across municipalities in 1997-99, 2000-05, 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.

Overall, the greater comparability of the first and second phases of rollout 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.

3.3 Estimation

Our identification strategy is a cohort difference-in-difference design, leveraging variation in program exposure across cohorts and space. In the standard approach to this design,

[9]The post-2005 move away from rural areas also holds in our analysis sample of high and very high marginality municipalities, as shown in Table A2.

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 rich municipalities across successive cohorts. 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 municipality 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.

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. As an additional robustness check, we also estimate a specification that controls for interactions of $post_t$ with the components of the marginality index:

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

where X_m^{1990} is a vector of 9 municipal socioeconomic characteristics (see footnote 5) in the 1990 census. 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.

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 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. Based on this reasoning, our strategy delivers a lower bound on the long-term benefits of the Program.

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 or 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 in reducing 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 twenties. These residual concerns are especially important for individuals who were born in a different state from where they resided in 2005. In the Appendix, we report alternative estimations assigning state-level (rather than municipal) average program exposure to these individuals, finding broadly similar results.

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 municipal cohort size by summing the sampling weights within each municipal cohort cell. We then use the logarithm 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. 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 a 15 percent larger cohort size for the older cohorts. 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.[10] 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.

[10]Consistent with the migration interpretation of the result, the differential increase in cohort size is even larger, 17 percent, if we omit internal migrants from the municipal cohort size estimates.

Additional insight on the migration issue can be gleaned from the census's questions on international migration, which asked household heads to list all former household members who left for the United States during the past five years but who have not yet returned to Mexico. These data 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 out 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.

4 Results

4.1 Descriptive Statistics

Based on the cohort size results in the previous section, we restrict our sample to individuals who were less than 20 years old in 1997, leaving an analysis sample 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.

Table 2 reports descriptive statistics for the unexposed (pre-program) and fully exposed (post-program) groups. The outcomes of the unexposed group provide a base from which to measure the impact estimates below. Such a comparison works especially well for education, which is effectively fixed over the age range we study, but is slightly more complex for outcomes with lifecycle profiles, like earnings. Nonetheless, we use unexposed group averages as a basis for comparison throughout our discussion of the impact estimates.

As shown in Table 2, the average individual in our sample is from a municipality where Progresa had enrolled roughly 3 in 10 households by 1999 and 6 in 10 households by 2005. Average educational attainment ranges from 6 to 9 grades, with younger cohorts outperforming older cohorts, a pattern more pronounced for women than for men. Geographic mobility does not appear high. 5% of women and 10% of men migrated across municipal borders in the previous 5 years, and parental coresidence is not uncommon. Coresidence is higher in younger cohorts, which may partly explain the same pattern in household monthly labor income. But surprisingly, housing conditions and durable goods ownership do not vary substantially across cohorts.

Labor market outcomes differ substantially by both cohort and gender. Among men, labor force participation is high and rises with age, from 0.76 for the younger cohorts to 0.87 for the older cohorts, with concomitant gains in monthly labor income, from 1822 pesos to 2429 pesos. In contrast, women's participation and income are lower and show less change across cohorts. Approximately one quarter of women work, and they earn between 600 and 800 pesos per month on average.[11] About 40 percent of men (unconditional) work in agriculture jobs, compared with 3 percent of women.

Sample size is another important source of difference between men and women. The male sample is approximately 15 percent smaller than the female sample, likely reflecting two phenomena. One of them is migration; as discussed in Section 3.4, men are more likely than women to emigrate from Mexico. The other is mortality; 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). Either of these margins may respond to Progresa exposure, so the difference in sample sizes reinforces the notion that concerns about selection bias are more applicable to males than females.

[11] In US dollars, average monthly labor income was about $200 for men and about $55 for women.

4.2 Event Studies

To present our identifying variation most transparently, we begin by reporting event study estimates based on equation (3) in Figures 4-6. 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. The coefficient is normalized to zero for the middle cohort of the partially exposed group, aged 13 in 1997.

Figure 4 shows event study graphs for education, showing clear evidence of Progresa-related gains. We include 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. 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 fully exposed group, aged 15 and over in 1997. Additionally, the coefficients are relatively flat across fully exposed 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.

Figure 5 presents similar event study graphs for five (unconditional) labor market outcomes, including labor market participation, participation in paid and agricultural work, hours, and labor earnings. Turning first to women, graphs for all labor market outcomes except agricultural work show gains for post-program cohorts in earlier rollout municipalities, 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. Coefficients for agricultural work are essentially constant across cohorts, suggesting little effect of Progresa on participation in agricultural work for women: unsurprising, given that only 3 percent of women participate in that sector. For men, the event studies for labor market outcomes 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 as education. Agricultural work declines, hours increase, and income increases.[12] Overall, Figure 5 suggests meaningful impacts on labor market outcomes, but estimates are imprecise for individual birth cohorts.

Do these suggestive impacts translate to changes in living standards and living arrangements? Figure 6 provides event study graphs for household and demographic outcomes, including total household labor income, housing conditions, durable goods, parental coresidence, marital status, household size, cross-municipal migration, and urban residence. For the household economy, estimates are noisy for household labor income but show improvements in housing conditions and durable goods for fully exposed cohorts in earlier rollout municipalities. This greater household wealth could reflect the greater earnings potential of early beneficiaries, the greater wealth of their parents (either from coresidence or inter vivos transfers), or changes in living arrangements that raise household resources. Thus, the analyses of living arrangements help shed light on the household economy results. Marriage and household size show no clear effects for either sex, while parental coresidence if anything declines with childhood program exposure for men, suggesting that changes in living arrangements do not explain the improvement in household economic outcomes.

Geographic mobility may be an important channel for program impacts on labor market outcomes, so the final two panels of Figure 6 report event studies for cross-municipal

[12]Due to the positive coefficient for the cohort aged 18 in 1997, it is difficult to distinguish a program effect on agricultural work from pre-existing trends.

migration and urban residence. Though imprecise, the graphs suggest positive effects on both outcomes, implying a possible role for migration from depressed rural areas to more economically vibrant urban areas.

4.3 Impact Estimates

Wide confidence intervals on the cohort-specific coefficients hinder the conclusions we can draw from the event studies. By pooling cohorts into two groups, estimates of equations (1) and (2) can help shed light on the overall significance of the labor market and household economy impacts. Tables 3-5 present these estimates, the main difference-in-difference results of the paper. For each outcome 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).

4.3.1 Educational Outcomes

Starting with education, Table 3 reveals that receiving the offer of the program before the critical primary-to-secondary transition has large, positive effects on completed years of education. The estimated coefficients indicate about 1.4 years of additional schooling for both men and women. Compared to the average education of the unexposed group, 7 years for men and 6.6 for women, this effect corresponds to 20 percent of baseline schooling for men and 21 percent for women.

Which schooling levels account for these increases? For both men and women, Table 3 shows significant effects on the probability of accumulating some secondary school and some high school. The secondary school impacts are 30 percentage points for women and 18 percentage points for men: enormous when compared to secondary schooling rates in the unexposed group (45 percent among men, 39 percent among women). For high school, the effects are no less impressive. From a baseline of 18 percent among unexposed men and 16 percent among unexposed women, childhood exposure to Progresa raises the probability of completing some high school 10-15 percentage points for both sexes. The secondary and high school impacts range from 40 to 70 percent of baseline 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. Overall, the estimated education impacts represent remarkable increases in educational attainment among children who grow up with Progresa.

4.3.2 Labor Market Outcomes

We now turn to the long-run impacts of Progresa on labor market outcomes. 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 a higher quality diet 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 baseline participation rate of 26 percent for the unexposed cohort, program impacts on labor force participation range from 7 to 11 percentage points. Virtually all of this impact is driven by increases in paid work, which also increases 7-11 percentage points compared with a baseline rate of 15 percent. Progresa also raises hours worked and labor earnings. Unconditional labor supply increases by 4 to 6 hours per week on average, from a base of 10 hours for unexposed women. Program impacts on labor market earnings monthly range from 360 to 420 pesos, from a base of 750 pesos. For these outcomes, estimates based on equation (1)—without the marginality component interactions—are all significant at the 10 percent level and sometimes significant at the 5 percent level, while estimates based on equation (2)—with the interactions—are significant at the 5 percent level.

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 (87 percent of the unexposed cohort works), we observe large reductions in agricultural work and increases in hours worked. Hours worked rise on the order of 3-4 per week, compared to an average of 39 in the unexposed group, while agricultural employment declines 10-12 percentage points against a base of 43 percent.[13] In the specification with marginality component interactions, both results are significant at the 5 percent level; without the interactions, the agriculture result is significant at 5 percent, while the labor supply result is significant at 10 percent. Effects on labor income are insignificant, although the coefficients are positive and larger than those for women. The larger standard errors for men reflect their higher dispersion of incomes.

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 do labor market outcomes improve conditional on participation? An analysis of this issue is fraught with selection concerns, especially for women, but Table A3 repeats the analysis in Table 4 restricting the sample to workers. Results point to effects of the same sign as the unconditional effects, although significance levels are mixed. Conditional on participation, men see significant increases in paid work, decreases in agricultural work, and increases in weekly hours; the labor income coefficients are positive and of a similar magnitude to those in Table 4, but they are insignificant. Women see significant increases in paid work, marginally significant increases in labor income, and insignificant increases in hours. Overall, the results in Table A3 suggest that program effects operate on both the extensive and intensive margins.

To summarize this section and the last, in a context of low female labor force participation and lagging female education, Progresa has led to striking growth in both areas for young adult women. Male education has increased by similar amounts, accompanied

[13] Recall, however, that the event study for agricultural employment showed some ambiguity on the extent of pre-existing trends.

by significant but proportionately more modest effects on labor supply and sectoral affiliation. Estimated effects on labor income are positive for both sexes but statistically significant only for women. 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 labor market experience due to obtaining more education will likely fade over time. In fact, about 10% of our sample remains in school, so further work and education impacts may become apparent once these remaining youth finish school.

4.3.3 Household and Demographic Outcomes

A key question is whether Progresa's apparent labor market benefits translate to higher consumption, which bears a more direct link to welfare. Although the census does not directly measure consumption, the housing quality and durable goods indices offer proxies based on a subset of the consumption basket. However, any impacts on these household-level economic outcomes are inter-related with how the program affects marriage, living arrangements, and total resources available to the household, so we also analyze total household labor income, parental coresidence, household size, and marital status. Estimated impacts on these outcomes appear in Table 5.

Table 5 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 but insignificant coefficients for total household labor income. Housing conditions improve by 0.2-0.3 standard deviations, while durable goods ownership increases by 0.1-0.2 standard deviations. As the event studies in Figure 6 suggested, the data show little effect on marriage or household size but a marginally significant decline in parental coresidence among men.[14] The decline in parental coresidence presumably reduces household wealth, so the effects on housing conditions and durable goods may be a lower bound

[14]One of the coefficients for household size is positive and significant for men, however.

for men. Importantly, however, parents who received program benefits for a longer time may transfer some of their accumulated wealth to their children, so the increased household economic status 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.

A related question is whether childhood program exposure alters spousal characteristics. Marriage rates are still low in our younger cohorts, around one-half, so any analysis of spousal characteristics is subject to some concern about selection into marriage, even if the program did not affect the risk of marriage on average. Nevertheless, Appendix Table A4 uses data on married individuals to assess effects on spousal age, education, labor force participation, and labor income. Average spousal education increases by roughly 1 grade for both sexes. Spousal age effects diverge by sex, however, with suggestive (marginally significant) evidence that the average age of wives rises by 1 year and the average age of husbands declines by 0.25-0.75 years. We observe no significant effects on labor force participation or labor income.

The final two rows of Table 5 examine a separate demographic phenomenon that may be relevant to understanding Progresa's labor market effects: migration. Women show consistently significant increases in cross-municipal migration—on the order of 10 percentage points in the previous 5 years, twice the migration rate in the unexposed group—and in urban residence—on the order of 7-13 percentage points, more than one-fifth of the urban share in the unexposed group. These results suggest that part of the benefit women enjoy from childhood program exposure derives from moving to opportunity. As elsewhere in the analysis, coefficients for the migration outcomes are positive but insignificant for men.

Taken together, Table 6 points to positive effects on household economic status for both men and women, which cannot be explained by changes in living arrangements. While Progresa did not affect the probability of being married, is does appear to have altered spousal characteristics in some dimensions, most notably by increasing the average education among both husbands and wives. Part of the program's effect on economic outcomes may be driven by migration, especially for women.

4.4 Robustness and Falsification Checks

Throughout the analysis, we have reported estimations with and without marginality component interactions to give a sense of their robustness. Overall, the results are similar with and without these additional covariates, reducing concerns that our difference-in-difference results are spurious. For added evidence on this issue, this section performs two additional checks on our research design.

The first check 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. Where these migrants resided in the late 1990s is unclear, but as an alternative to our main strategy, we assign them state-level enrollment ratios based on their states of birth. It is not possible to determine whether these individuals were born in high or very high marginality municipalities (an inclusion criterion for our sample); as an alternative, we restrict to states of birth with higher marginality.[15] As reported in Appendix Table A5, the alternative approach to dealing with migrants if anything increases the magnitude and significance of our results.

The second check is a falsification exercise, applying our research design in a dataset that should show no program effects. We do so using the 1990 census, 20 years before our main dataset was collected and 7 years before the start of Progresa. 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 20 estimated, and it is of the wrong sign: an increase in agricultural work for men. The 1990 results suggest that our 2010 findings do

[15] We define higher marginality states as either (1) having at least 10 percent of the state's population living in high or very high marginality municipalities or (2) having an average marginality index that exceeds the municipal threshold for high/very-high status. These definitions lead to similar results.

not reflect longstanding differential trends between earlier and later rollout municipalities.

5 Conclusions

Conditional cash transfer programs began two decades ago, transforming anti-poverty policy around the world. These transfer programs innovate by linking payments to investment in children's human capital, with 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 to no 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 income 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 labor income impacts are insignificant (albeit positive) for men, we find significant increases in labor supply and significant decreases in agricultural work. For both sexes, housing conditions and durable goods ownership rise, although it is unclear whether these 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. Nevertheless, further work is needed on the issue, including long-term followup 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). 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 policymakers involved in the design of anti-poverty programs.

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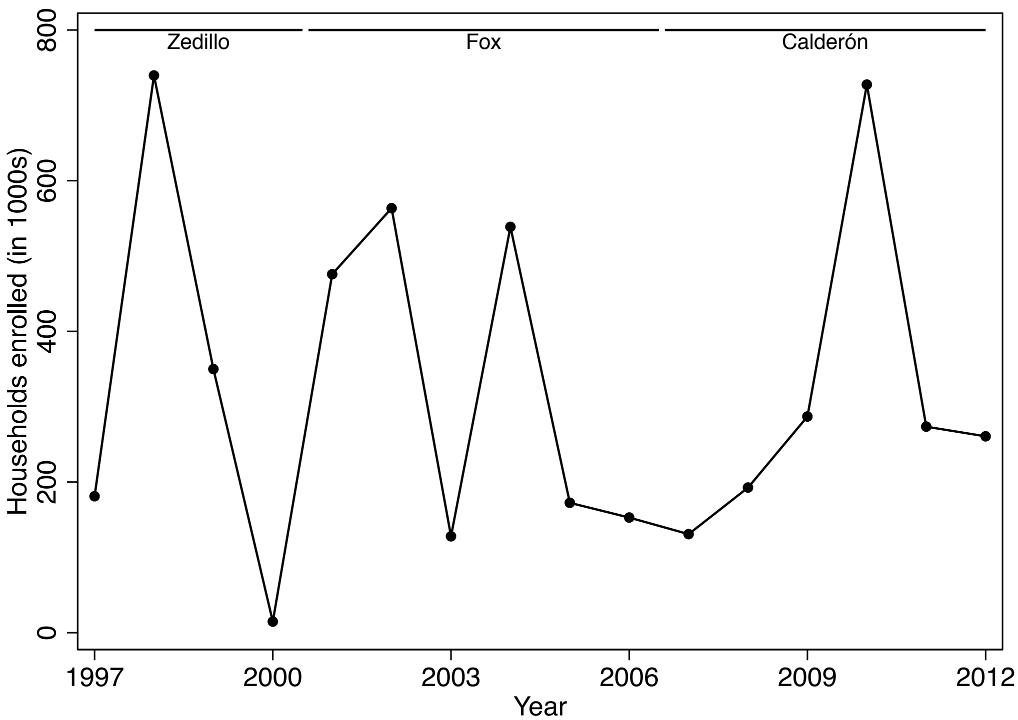
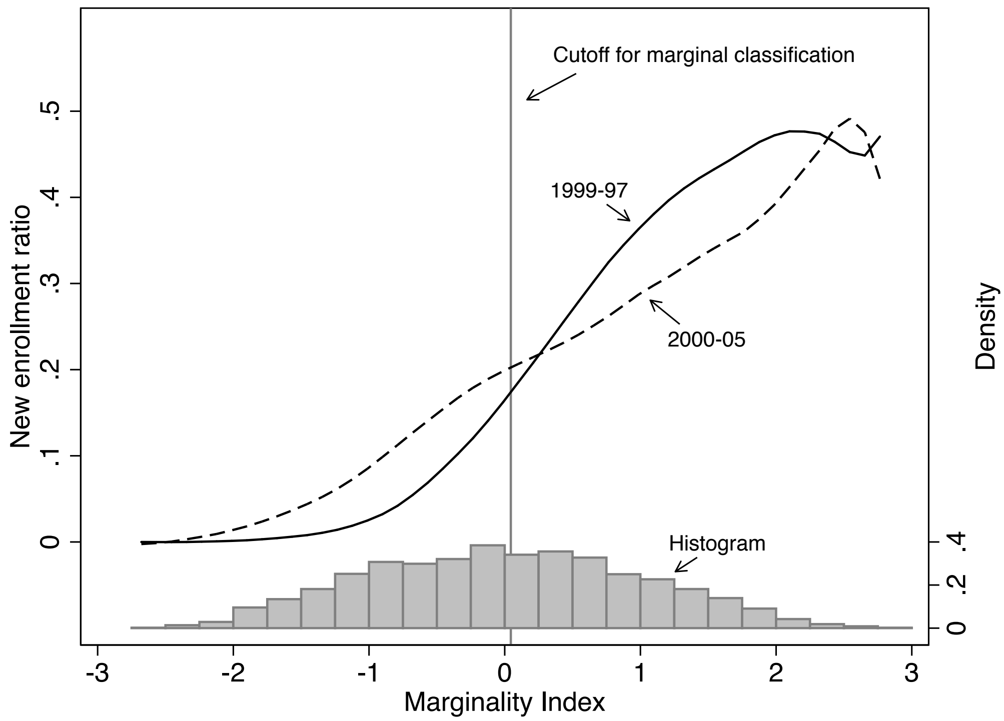
Figure 1: Number of New Households Enrolled Per Year, Nationwide



Note: Presidential elections took place in 2000, 2006, and 2012; midterm elections took place in 1997, 2003, and 2009.

Figure 2: Municipal Economic Conditions and New Enrollment Intensity, 1997-99 vs. 2000-05



Note: The new enrollment ratio is the count of new households enrolled divided by the total number of households in the municipality in the 2000 census. 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 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.

Figure 3: Cohort Exposure Timeline

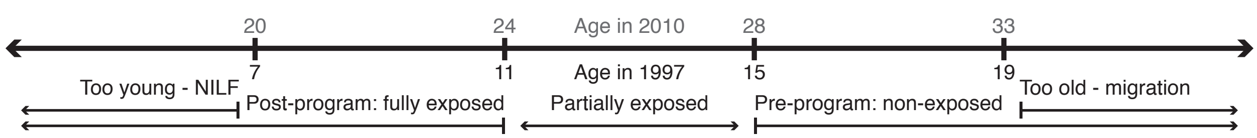
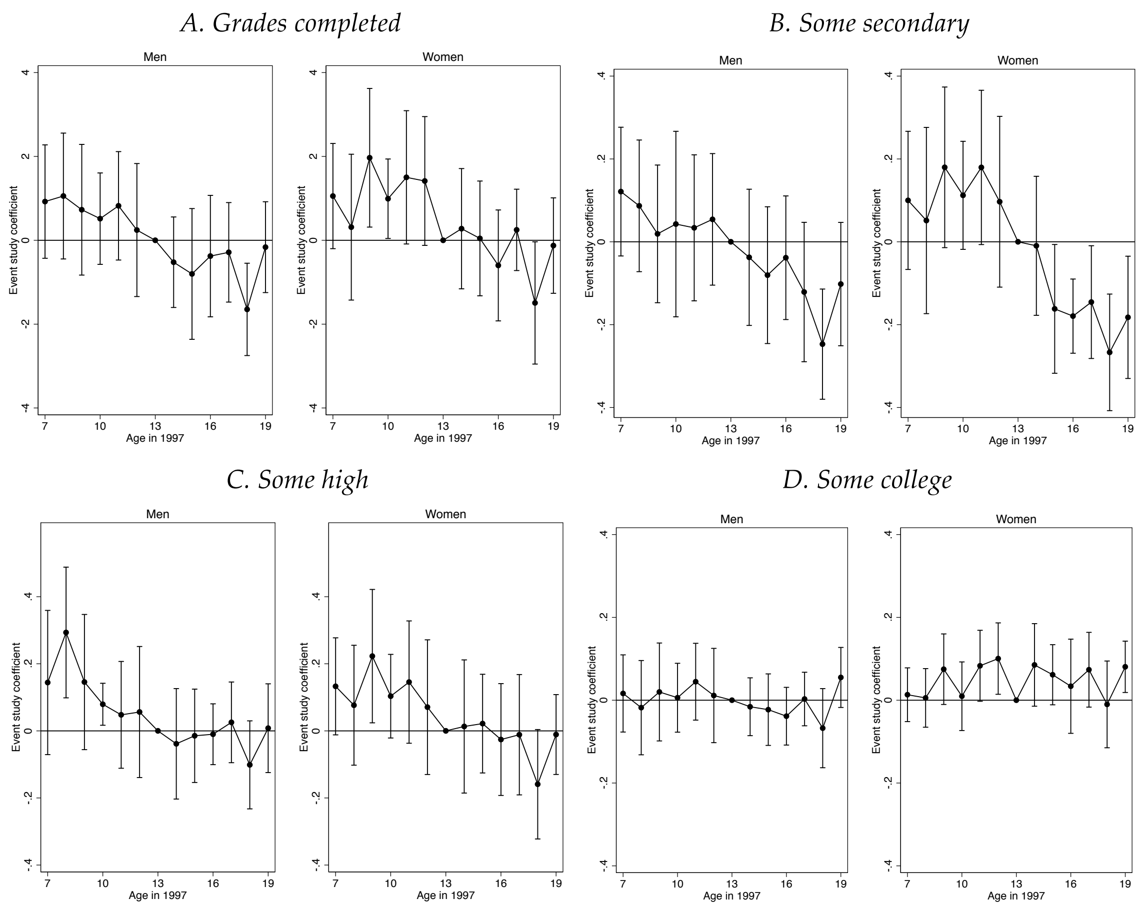
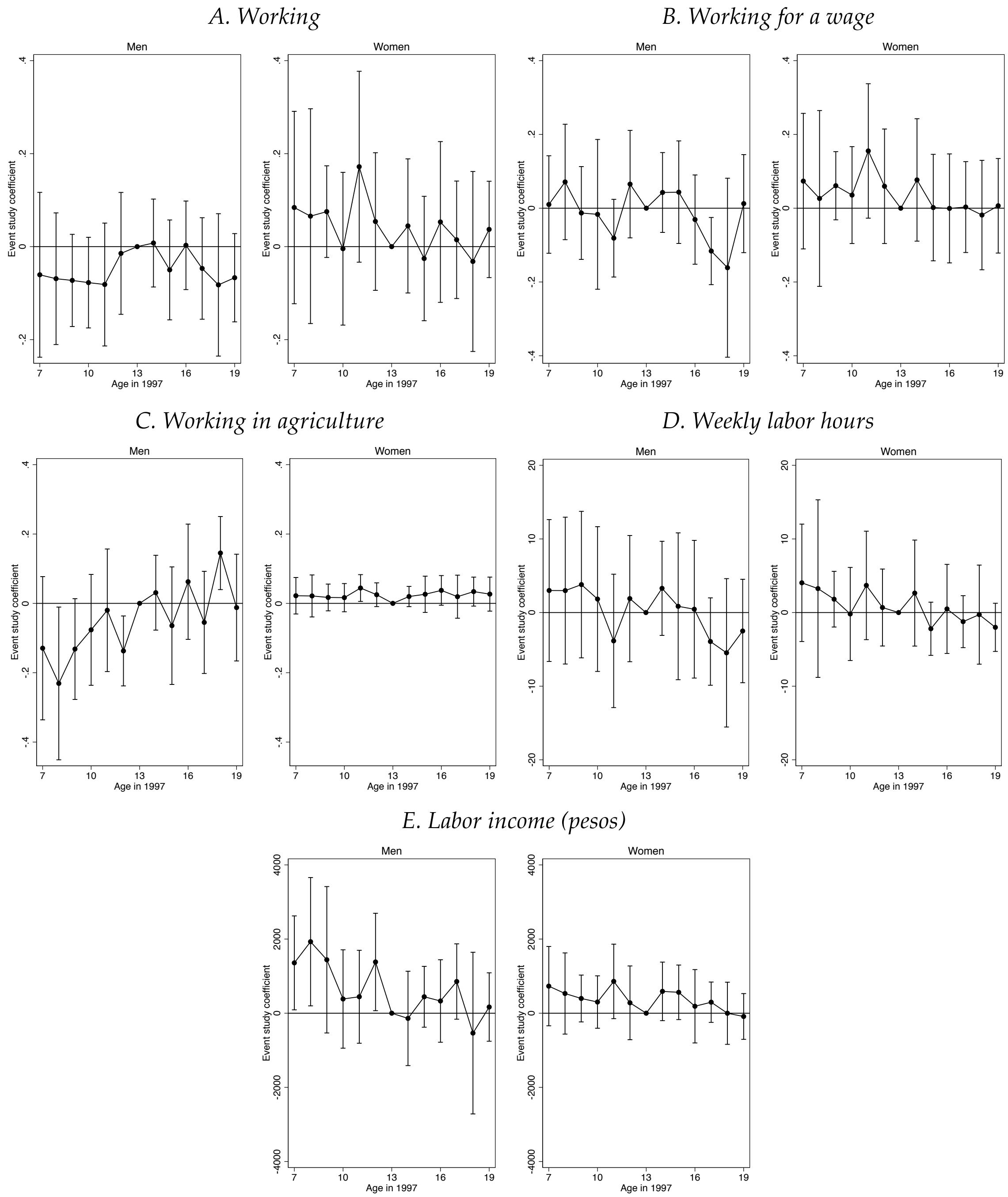


Figure 4: Event Study Graphs, Education



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.

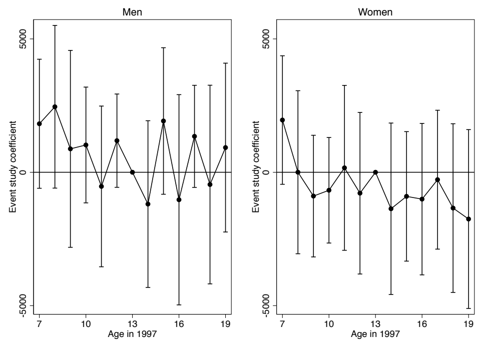
Figure 5: Event Study Graphs, Labor Market Outcomes



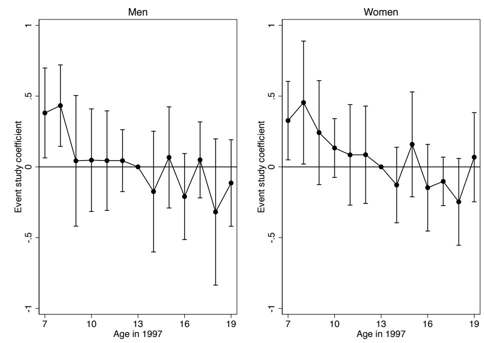
Note: Coefficients on interactions of cohort indicators with the cumulative enrollment ratio in 1999. The interaction for the cohort aged 13 in 1997 is omitted. Capped spikes represent 95% confidence intervals based on standard errors clustered at the state level. All regressions include cohort fixed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

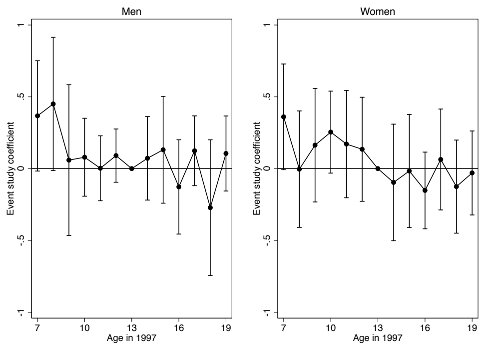
A. Household monthly labor income



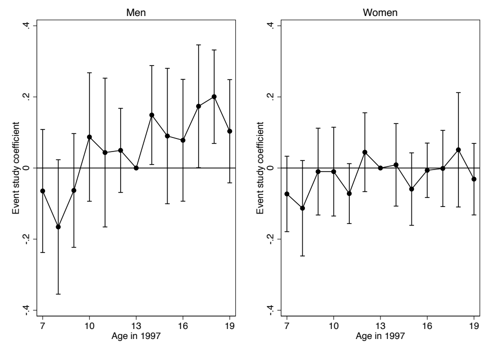
B. Index of housing conditions



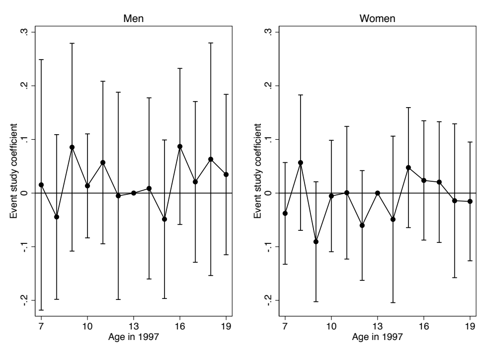
C. Index of durable goods



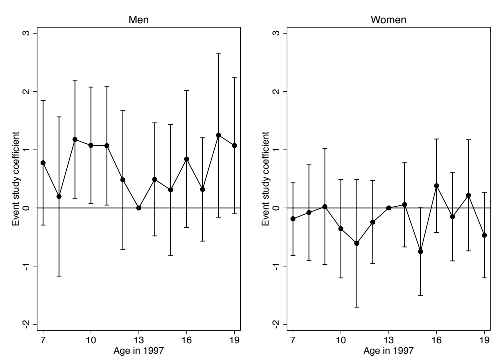
D. Living with parents



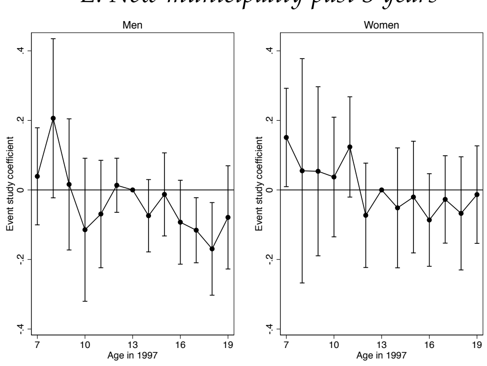
E. Married



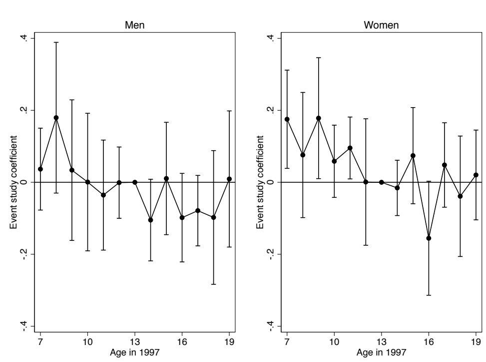
F. Household size



E. New municipality past 5 years



F. 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.

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

	2nd semester 1997		2nd semester 2003	
Grade	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 HS beneficiaries	550		950	
Max HH amount with HS beneficiaries			1635	

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

Table 2: Descriptive Statistics

	Men		Women	
	Pre-program	Post-program	Pre-program	Post-program
	(1)	(2)	(3)	(4)
A. Program data				
Cumul. enrollment ratio, 1999	0.32	0.32	0.31	0.32
	(0.14)	(0.13)	(0.13)	(0.13)
Cumul. enrollment ratio, 2005	0.60	0.60	0.59	0.60
	(0.21)	(0.21)	(0.21)	(0.21)
B. Education outcomes				
Some secondary	0.45	0.68	0.39	0.65
	(0.50)	(0.47)	(0.49)	(0.48)
Some high	0.18	0.32	0.16	0.32
	(0.38)	(0.47)	(0.36)	(0.47)
Some college	0.07	0.09	0.07	0.10
	(0.26)	(0.29)	(0.26)	(0.29)
Grades completed	7.0	8.6	6.6	8.5
	(4.2)	(3.7)	(4.2)	(3.8)
C. Labor market outcomes				
Working	0.87	0.76	0.26	0.25
	(0.34)	(0.43)	(0.44)	(0.43)
Working for a wage	0.49	0.47	0.15	0.18
	(0.50)	(0.50)	(0.36)	(0.38)
Working in agriculture	0.43	0.36	0.03	0.03
	(0.50)	(0.48)	(0.18)	(0.16)
Weekly labor hours	38.9	34.2	9.9	10.6
	(23.1)	(24.9)	(19.9)	(21.0)
Monthly labor income (pesos)	2429	1822	750	609
	(5349)	(4405)	(2447)	(1925)
D. Household and demographic outcomes				
HH monthly labor income	4478	5545	4373	5198
	(8853)	(9851)	(9427)	(10,637)
Index of housing conditions	-0.04	0.00	0.02	0.03
	(1.00)	(1.00)	(0.99)	(0.99)
Index of durable goods	-0.05	0.00	0.02	0.04
	(0.98)	(1.00)	(1.00)	(1.02)
Living with parent	0.30	0.67	0.22	0.45
	(0.45)	(0.47)	(0.41)	(0.50)
Married	0.79	0.41	0.77	0.53
	(0.41)	(0.49)	(0.42)	(0.50)
Household size	5.68	5.61	5.12	4.35
	(2.81)	(2.75)	(2.34)	(2.27)
New municipality, last 5 years	0.08	0.09	0.05	0.05
	(0.27)	(0.29)	(0.21)	(0.21)
Urban residence	0.36	0.37	0.34	0.35
	(0.48)	(0.48)	(0.47)	(0.48)
E. Sample sizes				
Observations	139,446	182,464	166,787	211,887
Municipalities	1,144	1,144	1,144	1,144
States	24	24	24	24

Note: Sample sizes correspond to Panel A; they vary slightly in Panels B and C. Enrollment ratios equal cumulative enrollment divided by the 2000 Census household count. Housing index is the standardized first principal component of indicators for having a dirt floor, modern roof, flush toilet, sewage, piped water, and electricity; durables index is the same for having a car, mobile phone, computer, washer, refrigerator, TV, and hot water heater.

Table 3: Program Impacts on Educational Attainment

	Men		Women	
	(1)	(2)	(3)	(4)
A. Grades completed				
Enrollment ratio, 1999	1.405	1.453	1.458	1.398
× post cohort	[0.419]	[0.244]	[0.408]	[0.455]
Observations	320,423		375,892	
B. Some secondary				
Enrollment ratio, 1999	0.177	0.190	0.306	0.309
× post cohort	[0.055]	[0.047]	[0.060]	[0.085]
Observations	320,423		376,753	
C. Some preparatory				
Enrollment ratio, 1999	0.151	0.103	0.165	0.105
× post cohort	[0.061]	[0.034]	[0.045]	[0.037]
Observations	320,423		376,753	
D. Some college				
Enrollment ratio, 1999	0.024	0.027	-0.018	-0.016
× post cohort	[0.018]	[0.019]	[0.024]	[0.020]
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.

Table 4: Program Impacts on Labor Market Outcomes

	Men		Women	
	(1)	(2)	(4)	(5)
A. Working				
Enrollment ratio, 1999	-0.024	-0.022	0.068	0.113
× post cohort	[0.039]	[0.031]	[0.038]	[0.034]
Observations	320,133		377,236	
B. Working for pay				
Enrollment ratio, 1999	0.042	0.043	0.071	0.113
× post cohort	[0.045]	[0.040]	[0.033]	[0.036]
Observations	313,459		374,600	
C. Working in agriculture				
Enrollment ratio, 1999	-0.122	-0.095	-0.004	0.003
× post cohort	[0.067]	[0.041]	[0.010]	[0.011]
Observations	317,865		376,067	
D. Weekly labor hours				
Enrollment ratio, 1999	3.808	3.266	3.719	5.961
× post cohort	[1.821]	[1.572]	[1.826]	[1.799]
Observations	316,879		376,081	
E. Monthly labor income				
Enrollment ratio, 1999	791	331	362	471
× post cohort	[550]	[409]	[196]	[187]
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.

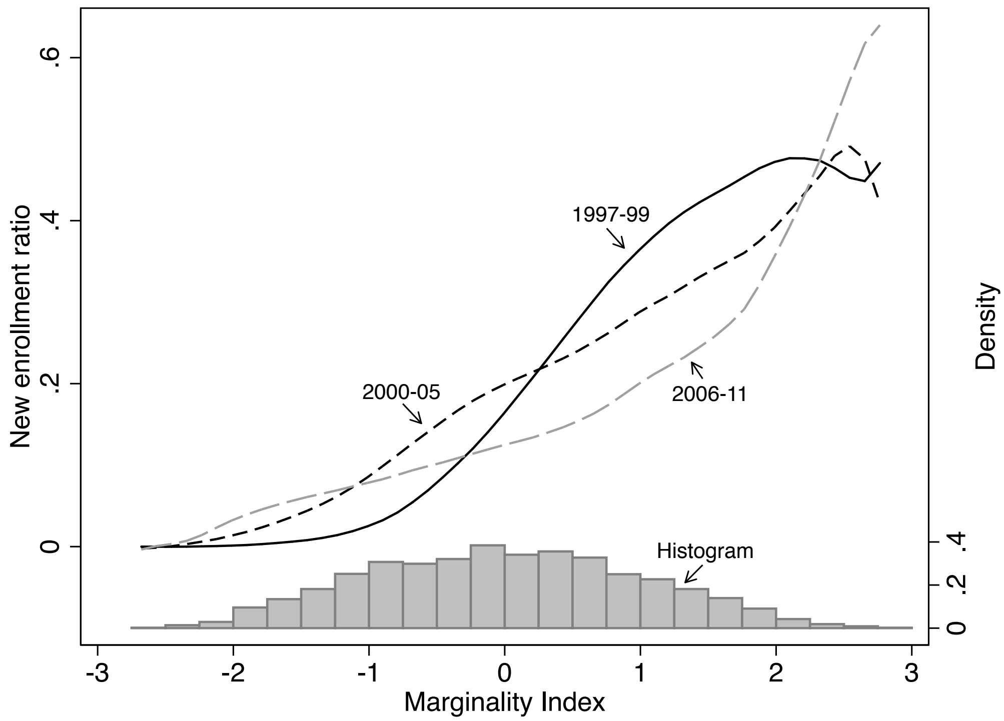
Table 5: Program Impacts on Household and Demographic Outcomes

	Men		Women	
	(1)	(2)	(4)	(5)
A. HH monthly labor income				
Enrollment ratio, 1999	450	1320	1162	1127
× post cohort	[782]	[932]	[844]	[801]
Observations	279,791		315,548	
B. Index of housing conditions				
Enrollment ratio, 1999	0.281	0.192	0.291	0.187
× post cohort	[0.124]	[0.077]	[0.103]	[0.086]
Observations	315,190		370,997	
C. Index of durable goods				
Enrollment ratio, 1999	0.170	0.126	0.224	0.139
× post cohort	[0.085]	[0.064]	[0.073]	[0.070]
Observations	316,412		372,371	
D. Living with parent				
Enrollment ratio, 1999	-0.159	-0.100	-0.042	0.004
× post cohort	[0.087]	[0.057]	[0.042]	[0.035]
Observations	321,910		378,674	
E. Married				
Enrollment ratio, 1999	-0.004	-0.026	-0.032	-0.051
× post cohort	[0.032]	[0.035]	[0.039]	[0.050]
Observations	321,368		378,009	
F. Household size				
Enrollment ratio, 1999	0.176	0.582	-0.056	0.221
× post cohort	[0.387]	[0.248]	[0.358]	[0.211]
Observations	321,910		378,674	
G. New municipality last 5 years				
Enrollment ratio, 1999	0.105	0.080	0.126	0.097
× post cohort	[0.084]	[0.055]	[0.058]	[0.046]
Observations	321,910		378,674	
H. Urban				
Enrollment ratio, 1999	0.089	0.066	0.125	0.066
× post cohort	[0.065]	[0.040]	[0.053]	[0.034]
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. 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.

APPENDIX

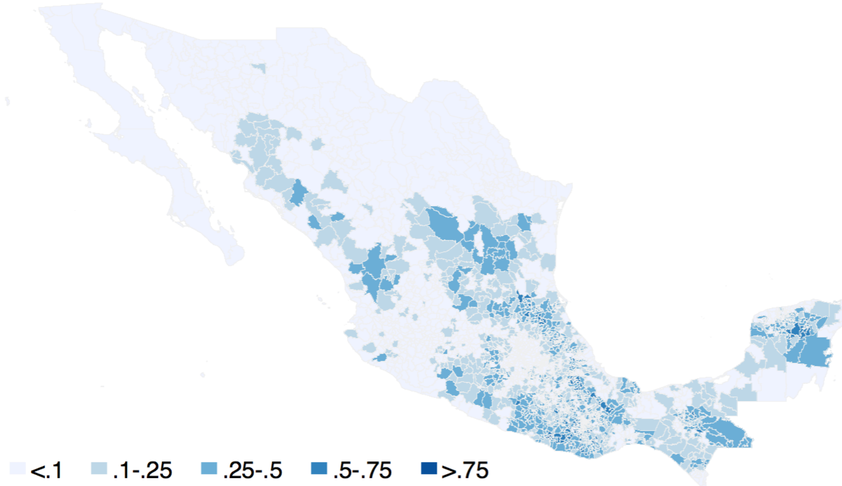
Figure A1: Economic Conditions and Enrollment Intensity, 1997-99, 2000-05, and 2006-11



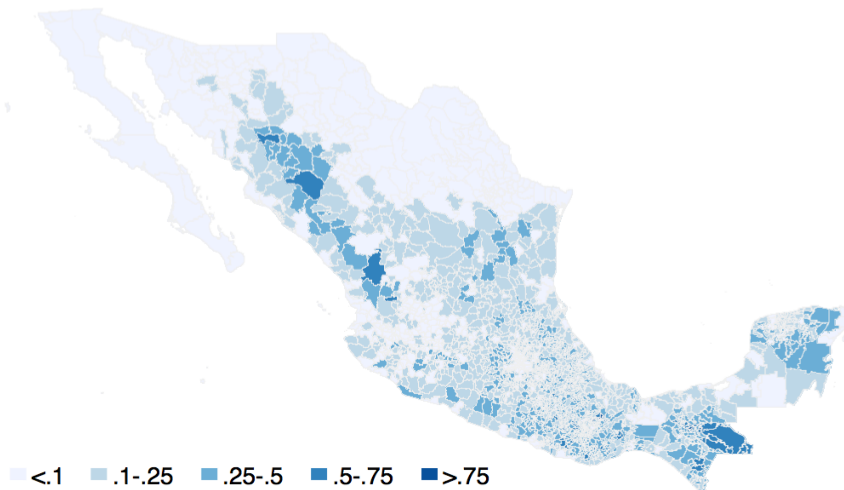
Note: The new enrollment ratio is the count of new households enrolled divided by the total number of households in the municipality in the 2000 census. The marginality index is defined (using 1990 census data) by the Mexican government as the normalized first principal component of nine municipal population 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.

Figure A2: Maps of Enrollment Intensity, 1997-99, 2000-05, and 2006-11

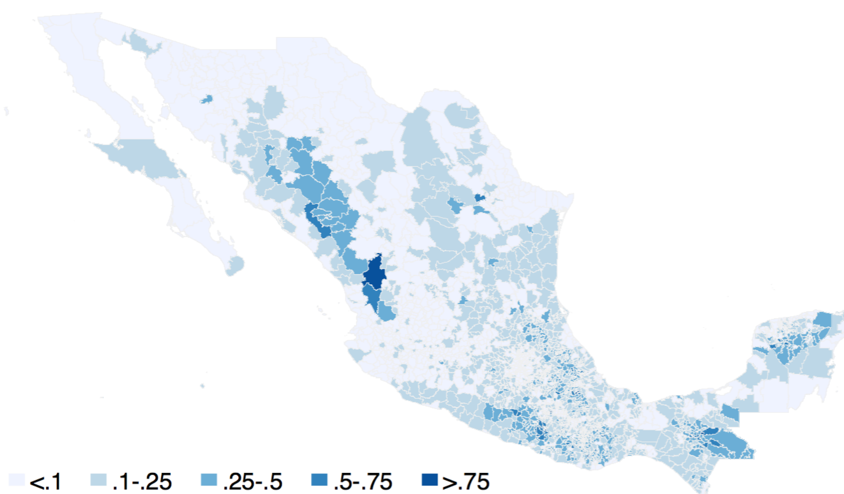
A. 1997-1999



B. 2000-2005

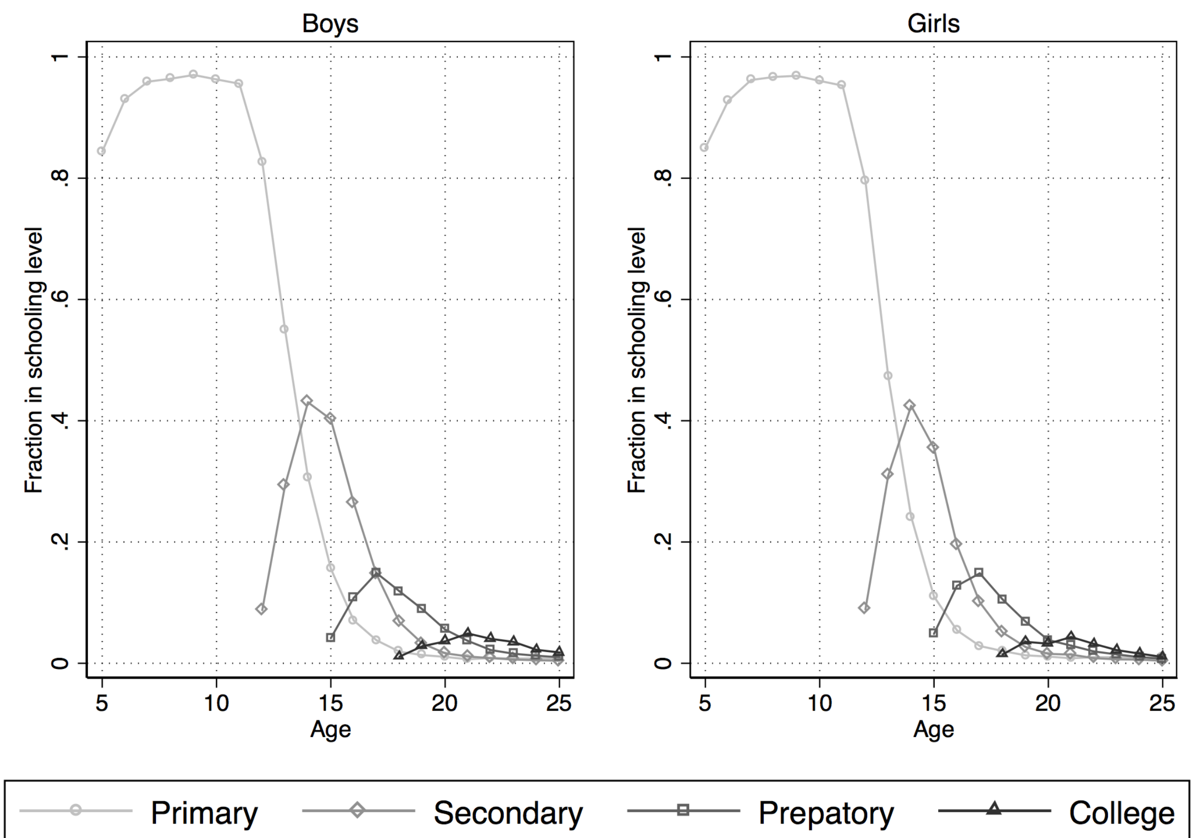


C. 2006-2011



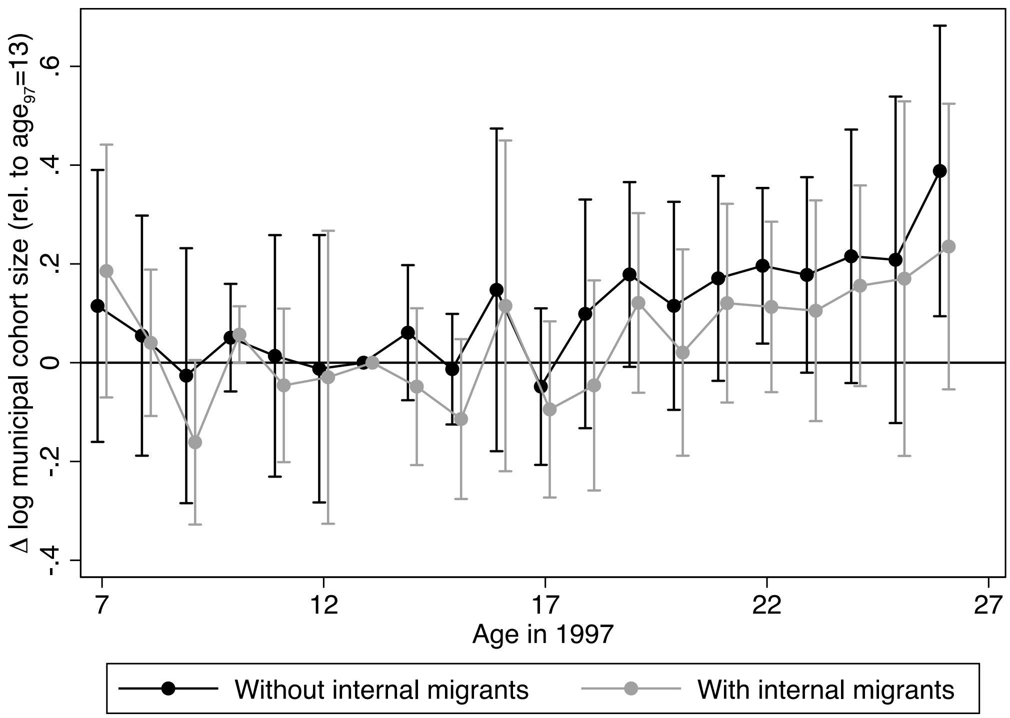
Note: New enrollment ratio is the count of new households enrolled divided by the total number of households in the municipality in the 2000 census.

Figure A3: 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 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.

Table A1: Economic Conditions and Enrollment Intensity, 1997-99, 2000-05, and 2006-11

	1997-1999		2000-2005		2006-2011	
	Uni-variate	Multi-variate	Uni-variate	Multi-variate	Uni-variate	Multi-variate
	(1)	(2)	(3)	(4)	(5)	(6)
A. Overall marginality index						
Standardized index	0.147		0.098		0.069	
(mean = 0, s.d. = 1.0)	[0.004]		[0.007]		[0.005]	
B. Index components: share of population…						
Illiterate	0.909	0.146	0.587	0.063	0.515	0.385
(mean = 0.23, s.d. = 0.15)	[0.079]	[0.124]	[0.061]	[0.045]	[0.037]	[0.073]
Without toilet	0.455	0.003	0.316	0.043	0.21	0.000
(mean = 0.42, s.d. = 0.25)	[0.025]	[0.023]	[0.028]	[0.027]	[0.022]	[0.018]
Without electricity	0.457	0.052	0.319	0.097	0.221	0.055
(mean = 0.24, s.d. = 0.23)	[0.045]	[0.022]	[0.037]	[0.035]	[0.024]	[0.024]
Without running water	0.397	0.031	0.246	-0.032	0.169	-0.029
(mean = 0.34, s.d. = 0.25)	[0.046]	[0.020]	[0.032]	[0.022]	[0.022]	[0.023]
With dirt floor	0.521	0.223	0.331	0.09	0.233	0.008
(mean = 0.41, s.d. = 0.27)	[0.028]	[0.035]	[0.030]	[0.026]	[0.025]	[0.018]
Earning <2x minimum wage	0.85	0.145	0.571	0.106	0.341	0.006
(mean = 0.77, s.d. = 0.13)	[0.054]	[0.027]	[0.051]	[0.034]	[0.054]	[0.062]
With primary education	0.807	0.146	0.533	0.082	0.378	-0.028
(mean = 0.57, s.d. = 0.16)	[0.031]	[0.065]	[0.046]	[0.033]	[0.032]	[0.053]
With crowding	1.042	0.265	0.709	0.255	0.576	0.277
(mean = 0.67, s.d. = 0.12)	[0.055]	[0.077]	[0.061]	[0.054]	[0.037]	[0.046]
In localities with pop. < 5000	0.261	0.009	0.184	0.023	0.079	-0.026
(mean = 0.77, s.d. = 0.33)	[0.026]	[0.011]	[0.024]	[0.006]	[0.012]	[0.008]
Number of municipalities	2,383	2,383	2,383	2,383	2,383	2,383

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 total number of households in the municipality in the 2000 census**.** In Panel A, the index is the standardized first principal component of the covariates in Panel B.

Table A2: Economic Conditions and Enrollment Intensity, 1997-99, 2000-05, and 2006-11 in High and Very High Marginality Municipalities

	1997-1999		2000-2005		2006-2011	
	Uni-variate	Multi-variate	Uni-variate	Multi-variate	Uni-variate	Multi-variate
	(1)	(2)	(3)	(4)	(5)	(6)
A. Overall marginality index						
Standardized index	0.155		0.100		0.116	
(mean = 0, s.d. = 1.0)	[0.018]		[0.015]		[0.013]	
B. Index components: share of population…						
Illiterate	0.501	0.001	0.317	-0.019	0.544	0.372
(mean = 0.34, s.d. = 0.13)	[0.120]	[0.065]	[0.061]	[0.068]	[0.059]	[0.082]
Without toilet	0.144	0.000	0.122	0.04	0.139	0.018
(mean = 0.60, s.d. = 0.19)	[0.061]	[0.038]	[0.033]	[0.040]	[0.042]	[0.020]
Without electricity	0.187	0.028	0.185	0.115	0.137	0.028
(mean = 0.37, s.d. = 0.25)	[0.044]	[0.022]	[0.039]	[0.033]	[0.029]	[0.022]
Without running water	0.102	0.013	0.067	-0.025	0.057	-0.023
(mean = 0.51, s.d. = 0.24)	[0.027]	[0.017]	[0.030]	[0.030]	[0.022]	[0.021]
With dirt floor	0.376	0.205	0.221	0.086	0.226	0.029
(mean = 0.62, s.d. = 0.22)	[0.049]	[0.035]	[0.028]	[0.031]	[0.037]	[0.020]
Earning <2x minimum wage	0.536	0.25	0.172	0.029	0.139	-0.038
(mean = 0.86, s.d. = 0.08)	[0.119]	[0.044]	[0.067]	[0.064]	[0.135]	[0.066]
With primary education	0.708	0.231	0.453	0.188	0.612	0.064
(mean = 0.70, s.d. = 0.10)	[0.137]	[0.067]	[0.077]	[0.077]	[0.063]	[0.074]
With crowding	0.892	0.542	0.499	0.266	0.695	0.371
(mean = 0.74, s.d. = 0.08)	[0.125]	[0.076]	[0.115]	[0.089]	[0.075]	[0.033]
In localities with pop. < 5000	0.151	0.057	0.067	0.000	-0.001	-0.03
(mean = 0.95, s.d. = 0.14)	[0.031]	[0.020]	[0.036]	[0.016]	[0.019]	[0.013]
Number of municipalities	1,144	1,144	1,144	1,144	1,144	1,144

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 total number of households in the municipality in the 2000 census**.** 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 a marginalized municipality.

Table A3: Program Impacts on Labor Market Outcomes, Conditional on Participation

	Men		Women	
	(1)	(2)	(4)	(5)
A. Works for pay				
Enrollment ratio, 1999	0.111	0.116	0.151	0.184
× post cohort	[0.052]	[0.052]	[0.096]	[0.078]
Observations	248,428		80,663	
B. Works in agriculture				
Enrollment ratio, 1999	-0.137	-0.121	-0.024	-0.010
× post cohort	[0.071]	[0.047]	[0.048]	[0.050]
Observations	252,834		82,130	
C. Hours				
Enrollment ratio, 1999	5.904	5.014	4.673	5.438
× post cohort	[2.052]	[2.022]	[3.237]	[3.371]
Observations	251,848		82,144	
D. Monthly labor income				
Enrollment ratio, 1999	1,041	490	1007	1022
× post cohort	[695]	[532]	[619]	[502]
Observations	242,295		79,151	
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: Program Impacts on Spousal Characteristics, Conditional on Marriage

	Men		Women	
	(1)	(2)	(4)	(5)
A. Spouse's education				
Enrollment ratio, 1999	1.000	0.877	1.054	0.984
× post cohort	[0.448]	[0.285]	[0.285]	[0.311]
Observations	179,778		221,463	
B. Spouse's age				
Enrollment ratio, 1999	0.938	0.909	-0.753	-0.252
× post cohort	[0.488]	[0.564]	[0.418]	[0.486]
Observations	180,542		222,496	
C. Spouse works				
Enrollment ratio, 1999	0.027	0.028	0.030	0.016
× post cohort	[0.038]	[0.029]	[0.023]	[0.026]
Observations	180,051		221,735	
D. Spouse's monthly labor income				
Enrollment ratio, 1999	35	66	981	696
× post cohort	[197]	[115]	[461]	[656]
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 A5: Robustness Check: Assigning Birth State Averages to Out-of-State Migrants

Out-of-state migrants included if…	At least 10% of birth state living in marginalized municipalities		Birth state avg. index exceeds municipal marginality threshold	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
Years of education	1.838	1.917	1.819	1.884
	[0.358]	[0.487]	[0.346]	[0.450]
Observations	352,607	412,153	322,255	376,859
Some secondary	0.222	0.376	0.223	0.370
	[0.041]	[0.077]	[0.041]	[0.069]
Observations	353,467	413,173	322,975	377,730
Some high	0.196	0.207	0.196	0.202
	[0.060]	[0.055]	[0.062]	[0.052]
Observations	353,467	413,173	322,975	377,730
Some college	0.035	-0.007	0.034	-0.008
	[0.019]	[0.023]	[0.019]	[0.023]
Observations	353,467	413,173	322,975	377,730
Working	-0.022	0.109	-0.023	0.106
	[0.046]	[0.047]	[0.043]	[0.043]
Observations	353,227	413,658	322,658	378,160
Working for a wage	0.049	0.105	0.050	0.105
	[0.049]	[0.034]	[0.044]	[0.032]
Observations	346,040	410,680	315,960	375,499
Working in agriculture	-0.154	-0.002	-0.148	-0.003
	[0.067]	[0.009]	[0.063]	[0.009]
Observations	281,402	412,306	320,384	376,974
Hours	3.521	6.122	3.414	6.065
	[1.952]	[2.434]	[1.685]	[2.206]
Observations	349,655	412,300	319,389	376,981
Monthly labor income	1,299.99	641.512	1,316.44	661.856
	[650.531]	[279.715]	[549.033]	[229.653]
Observations	338,588	408,320	309,619	373,862
Index of housing conditions	0.297	0.285	0.300	0.286
	[0.136]	[0.102]	[0.136]	[0.102]
Observations	347,684	406,851	317,619	371,861
Index of durable goods	0.219	0.244	0.214	0.255
	[0.075]	[0.087]	[0.073]	[0.088]
Observations	348,864	408,205	318,856	373,242
Municipality and cohort FE	X	X	X	X

Note: Coefficients on the 1999 enrollment ratio interacted with the post indicator. Brackets contain standard errors clustered at the state of birth level. All regressions additionally control for the interaction of the post indicator with the cumulative enrollment ratio in 2005.

Table A6: Falsification Check: 1990 Census

	Men	Women
	(1)	(2)
Years of education	0.464	0.464
	[0.422]	[0.374]
Observations	91,184	98,002
Some secondary	0.069	0.040
	[0.043]	[0.031]
Observations	91,184	98,002
Some high	0.014	0.021
	[0.028]	[0.023]
Observations	91,184	98,002
Some college	-0.005	-0.012
	[0.017]	[0.009]
Observations	91,184	98,002
Working	0.017	-0.005
	[0.039]	[0.027]
Observations	90,456	96,933
Working for a wage	0.018	-0.017
	[0.031]	[0.026]
Observations	86,035	95,727
Working in agriculture	0.076	0.013
	[0.037]	[0.008]
Observations	88,823	95,736
Hours	-1.297	0.115
	[2.417]	[1.375]
Observations	87,443	95,988
Monthly labor income	235.287	-34.984
	[236.211]	[107.290]
Observations	85,123	96,817
Index of housing conditions	-0.083	0.016
	[0.076]	[0.043]
Observations	87,312	97,125
Municipality and cohort FE	X	X

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. The 1990 Census does not include questions on durable goods; sample sizes are smaller than in the 2010 Census because of differences in sampling methods for the public use sample. For consistency with earlier tables, monthly labor income is measured in 2010 pesos.