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Higher Education and International Migration

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Experimental Long-Term Effects of Early-Childhood and School-Age Exposure to a Conditional Cash Transfer Program

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Abstract

Numerous evaluations of conditional cash transfer (CCT) programs show positive short-term impacts, but there is only limited evidence on whether these benefits translate into sustained longer-term gains. This paper uses the municipal-level randomized assignment of a CCT program implemented for five years in Honduras to estimate long-term effects 13 years after program began. We estimate intent-to-treat effects using individual-level data from the population census, which allows assignment of individuals to their municipality of birth, thereby circumventing migration selection concerns. We find positive and robust impacts on educational outcomes for cohorts of a very wide age range, demonstrating that both early childhood exposure to the nutrition and health components of the CCT as well as exposure during school-going ages to the educational components led to sustained increases in human capital. These include increases of more than 50 percent for secondary school completion rates and the probability of reaching university studies for those exposed at school-going ages. Educational gains are, however, much more limited for indigenous children. Finally, exposure to the CCT increased the probability of international migration for young men, from 3 to 7 percentage points, while other labor market results are inconclusive, which highlights the need to better coordinate the program with labor market opportunities for this group.

JEL Classification: I25, I28, I38

Keywords: conditional cash transfers (CCTs), early childhood, education, migration

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1. Introduction

Conditional cash transfer (CCT) programs, which have been operating in Latin America for two decades, currently reach 25 percent of the region's population (Robles, Rubio and Stampini, 2017), and new programs have recently expanded to other regions. CCTs aim to alleviate poverty in the short run while at the same time inducing households to invest in the nutrition, health, and education of the next generation. A large body of evidence demonstrates their success in fulfilling both of these objectives in various contexts in the short run (Fiszbein and Schady, 2009).[2] There is greater uncertainty, however, as to whether CCTs also manage to break the intergenerational transmission of poverty, the longer-term goal for which they were designed (Molina Millán et al., 2018). More generally, relatively little is known about how CCTs affect the trajectories of children who benefitted directly or indirectly from different program components at different points in their childhood.

This paper provides experimental evidence on long-term impacts for children exposed during different stages of their childhood to five years (2000–2005) of a Honduran CCT program, the *Programa de Asignación Familiar* (PRAF-II). This CCT, similar in design to other programs in the region, provides a unique opportunity to study long-term impacts because it was randomized across 70 municipalities for its evaluation but, unlike most other randomized CCT evaluations such as Mexico's *PROGRESA*, the control municipalities were never phased into the program. Exploiting the municipality-level randomized assignment, we use individual-level data from the national census, collected 13 years after the program began (and eight years after the program ended), to analyze impacts of the CCT on cohorts spanning 24 years. We circumvent many of the selection and attrition concerns that typically affect the study of long-term impacts of highly mobile cohorts of individuals, as we can assign each individual to the municipality where he or she was born—a good proxy for their preprogram location—and hence estimate intent-to-treat (ITT) impacts unaffected by any subsequent domestic migration.

The availability of individual-level national census data allows us to account directly for migration within the national territory, which is as high as 30 percent for some of the cohorts of interest. The national census also includes information on current international migration of former household members as well as past international migration of current household members, allowing direct study of international migration as an outcome, which is an important potential concern for attrition bias.

[2] More recent literature examining short-term educational outcomes of CCTs includes reviews by Murnane and Ganimian (2014) and Glewwe and Muralidharan (2015) and meta-analyses by Saavedra and García (2012), Baird et al. (2014), and McEwan (2015).

Beyond these key advantages, the census data provide sufficient statistical power for the estimation of long-term impacts on many different cohorts of interest, and we can separately estimate the impacts on children exposed to the nutrition and health components of the program during early childhood and the education components of the program at older school-going ages, as well as estimate the impacts on those who benefitted (partially) from both. We also analyze whether there are spillovers or indirect effects on other children in the household by examining cohorts of children who were too old to have been directly affected by the education conditionalities when the program started in 2000 as well as children born after the end of the program.

The wide age range that can be examined in a single context offers an important advantage over other studies, as a better understanding on whether CCTs have a greater impact on human capital and subsequent outcomes at some ages versus others can be important for targeting. This is particularly relevant as some transfer programs target narrow age ranges. While the first generation of CCT programs in Latin America typically covered a wide age range, as was the case in Honduras, more recent programs in Asia (Filmer and Schady, 2014; Levere, Acharhya and Bharadwaj, 2016) and Africa (Baird, McIntosh and Özler, 2011; Benhassine et al, 2015) often target narrower populations and objectives (e.g., only health and nutrition in very early childhood; only educational outcomes at critical ages in primary or secondary school). Beyond the cash transfer literature, the comparison between ages is relevant for the broader literature on human capital formation. Indeed, economists often motivate focusing attention on early childhood based on Cunha and Heckman's (2007) multistage model of skill formation that predicts "skill begets skill" and therefore investments made in early life are favored over those made later in childhood.

Whether investments in early life translate into outcomes during a later phase in childhood of course also depends on how well each component of the CCT was implemented, exposure to other programs, and any remaining constraints that beneficiaries may face as they grow up. As no identifiable exogenous variation in program implementation or later program exposure exists, we abstract from such effects in this paper, as is done in most studies analyzing long-term impacts. However, the census data do provide sufficient power to study program impacts separately across groups likely to face different constraints in both the short- and long-term in the Honduran context, and as such provide insights on the potential importance of other constraints. Specifically, we analyze impacts by gender and ethnic background (non-indigenous versus indigenous), resulting in four demographic groups. Because no other variables in the census can reasonably be considered exogenous 13 years after the start of the program, these are the only four groups for which we can examine heterogeneity at the individual level without introducing the typical concerns regarding the use of specification searches in heterogeneity analysis. The groups provide important variation in possible constraints. As in many low- and middle-income countries, education and labor market decisions for men and women are quite different, with women experiencing much lower labor market participation and stronger interactions between labor market and fertility outcomes than men. And similar to those in many other Latin American countries, the indigenous population in Honduras has long suffered from higher poverty, lower human capital, poorer access to markets, and labor market discrimination, which together with strong emphasis on community ties and attachment to land and natural resource endowments may also make them less mobile (World Bank, 2006; UNSR, 2016).

Despite the vast literature on CCT programs, quantitative work specifically examining program impacts on indigenous populations is relatively scarce, although there is a body of ethnographic work that points to specific challenges related to CCT programming (Correa Aste and Roopnaraine, 2014). For many programs in Latin America, substantial effort was put into improving targeting to indigenous populations, but less was done in adapting rules to better fit indigenous cultures. Programs targeting the nuclear family, for example, may not adequately reach the person or persons in the community in charge of making decisions about education and health spending. PRAF-II, to our knowledge, took no explicit measures specifically related to indigenous beneficiaries (Hernandez Ávila, 2011). For these reasons, it is important to separate non-indigenous and indigenous beneficiaries to explore heterogeneity but important to do so.

Finally, another key advantage of using individual census data is that it allows reliable estimation of impacts on rare outcomes. This is relevant for international migration, which is only 1–3 percent for the oldest cohorts in the control group. It is even more salient for the highest levels of education. Less than 1 percent of the older cohorts, for example, have some university-level education. Very early teenage pregnancy is another key outcome variable that can be analyzed for the same reason. All of these are important outcomes for better understanding the potential long-term impacts of CCTs. Indeed, for some such outcomes, even the short-term evidence is relatively scarce and inconclusive for similar reasons. For international migration, in particular, two studies with experimental estimates of the short-term impacts of the Mexican CCT program show opposite results, with Angelucci (2015) finding modestly higher international migration and Stecklov et al. (2005) finding lower migration. Given the wide reach of CCT programs, more evidence on their impact on such migration is important not only for better understanding potential selection biases but also for the more general international migration policy debate.

Using census data, however, does constrain our analysis to the relatively limited set of measures available in the census form. As such, the data do not allow the disentangling of the exact mechanisms underlying some of the long-term impacts, nor do they allow thorough examination of returns to higher educational achievement in the labor market. The census does not contain earnings information and has only incomplete data on labor market participation and occupation. For these reasons, we complement the analysis of the national census data with analysis of a much smaller, but more comprehensive, annual household survey (the Permanent Multiple Purpose Household Survey, EPHPM for its Spanish acronym). We pool data from multiple survey rounds (2010–2016) to study impacts on the incomes of the cohorts old enough to have begun their transition into the labor market.

Results using the census data show that the Honduran CCT led to long-term significant increases in schooling for both women and men, including at the university level, well beyond the primary-school level directly targeted by the program. Effects for the indigenous beneficiaries, however, are much more limited than those for the non-indigenous. We also find statistically significant effects on international migration (though from a small base), a result that demonstrates how program exposure can set children on different pathways and have potentially important public policy implications. Findings on labor outcomes are less definitive and underscore the difficulty of estimating labor market returns when young adults are still relatively early in their transition into the labor market. Nevertheless, the findings suggest that women born in CCT municipalities work less but do not earn less per hour worked. Results for men are inconclusive.

As such, this paper complements other recent evidence on long-term impacts of CCTs (see Molina Millán et al. [2018] for a review). It is closely related to long-term impact studies exploiting the randomized phase-in of cash transfer interventions in Mexico (Behrman, Parker and Todd, 2009, 2011; Fernald, Gertler and Neufeld, 2009), Nicaragua (Barham, Macours and Maluccio, 2013, 2018a, 2018b) and Ecuador (Araujo, Bosch and Schady, 2018). There are also clear parallels with Parker and Vogl (2018), who use Mexican census data and the non-experimental national rollout of *PROGRESA* to analyze differential long-term impacts. This paper differs from those studies in its ability to experimentally estimate the *absolute* long-term impacts, as the randomized control group was never phased in. Other studies with absolute long-term impacts are: Barrera-Osorio, Linden, and Saavedra (2017), who study impacts 13 years after an individually randomized educational CCT in urban Colombia using administrative data for a specific cohort targeted by the intervention; Baird, McIntosh, and Özler (2018), who also study impacts of an educational CCT in Malawi two years after it ended; and Cahyadi et al. (2018), who study the six-year absolute impacts of an ongoing Indonesian CCT program on ages ranging from 0 to 15 at the start of the program.[3]

For children exposed during school-going ages, the existing evidence generally indicates that CCTs consistently help them obtain higher levels of education. Less conclusive is the extent to which these investments improve labor-market and family-related outcomes or lead to higher lifetime earnings. Evidence on relatively rare events, such as international migration or university studies, is also limited. The evidence base is even narrower for children exposed to the nutrition and health components of CCTs during early childhood, with several experimental differential studies suggesting fadeout of impacts or catch-up of original control groups that received similar benefits a little bit later in life, while other studies point to positive long-term effects on cognition and education. Most estimates are, however, for programs that are ongoing, and it is often not possible to disentangle whether the estimated impacts are driven by the cumulative exposure to the CCT since early childhood or are instead capturing short-term impacts of the start of the schooling conditionality and transfers when children reach school age. Given that the program we study had ended prior to the census data collection, this paper can isolate the long-term impacts of early childhood exposure alone. Overall, we contribute to the CCT literature by providing experimental evidence on the absolute long-term impacts of program exposure during a wide range of ages in early and later childhood and for a program that ended eight years earlier.

2. The Honduran CCT Program and the Short-Term Evaluations

We evaluate the long-term impacts of the second phase of PRAF-II, the Honduran CCT implemented between 2000 and 2005 in some of the poorest regions of Honduras. PRAF-II aimed to increase investment in human capital, including nutrition and health, during early childhood and education for children of primary-school age. The intervention, modeled after the *PROGRESA* program in Mexico, provided cash transfers (in the form of readily exchangeable vouchers) to: (i) households with pregnant women and children ages 0–3 (extended to age five in 2003), conditional on their attendance at child health and growth monitoring visits and the mother's attendance at health education workshops; and (ii) households with children ages 6–12 who had not yet completed fourth grade, conditional on school enrollment and attendance. Transfers averaged approximately 4 percent of total preprogram household income, relatively small

[3] More broadly, this paper also relates to ongoing debates on the longer-term evidence of unconditional cash transfers (Bandiera et al., 2017; Banerjee et al., 2016; Handa et al., 2018; Haushofer and Shapiro, 2018).

compared with other programs in the region at the time, and were scheduled twice annually. In some randomly selected areas, the program also aimed to strengthen supply-side constraints through improvements in the quality of both health and education services (IDB, 1998, 2006; IFPRI, 2003).[4]

The CCT targeted 70 of the rural municipalities in western Honduras with the highest malnutrition rates in the country, and a municipality-level randomized assignment was used to determine treatment and control municipalities. Randomization was stratified into five blocks of 14 municipalities each, after ordering by malnutrition levels (Morris et al., 2004). In the randomly selected treatment municipalities, all households with children in the specified age groups were eligible to receive program benefits for up to five years, from the start of the program in 2000 until 2005, after which the program ended. The control municipalities never received the program, an essential feature that allows estimation of the absolute long-term impacts by comparing outcomes of children born in treatment versus control municipalities.

The evaluation design included three benefit packages (or treatment arms) and a control group randomly allocated at the municipality level:

1. G_1: Households received cash transfers conditional on nutrition, health, and education behaviors (20 municipalities).

2. G_2: Households received cash transfers conditional on nutrition, health, and education behaviors, *and* schools and health centers received direct investments and support (20 municipalities).

3. G_3: Schools and health centers received direct investments and support, but households did not receive any direct benefits (10 municipalities).

4. G_4: The remaining municipalities were assigned to the control (20 municipalities).

Program monitoring documents and the short-term evaluation reports indicate that the health and schooling supply-side interventions in G_2 and G_3 were implemented with considerable delays and were not fully operational until after 2002. As the program documents do not allow us to fully characterize these delays, which may have disrupted health and education services and/or affected perceptions and expectations in G_2 and G_3 in a variety of ways, we focus on the impacts of the basic CCT components (G_1) but account for the other benefit packages in all estimations

[4] Appendix B provides further information about the CCT and its components as well as other related interventions implemented in the program municipalities in subsequent years. We consider all other interventions post-randomization as potentially endogenous and therefore do not account for them in the estimations.

to adhere to the experimental design. Our emphasis on the basic CCT component has the additional advantage of making the analysis more comparable to other research on the long-term impacts of CCTs.

Evidence from the short-term evaluations after two years shows impacts on early-life health outcomes, as well as on schooling, that are qualitatively similar to those found for other CCTs in the region, though modest in size (possibly reflecting the relatively small transfer size in comparison to other programs in the region). Morris et al. (2004) found large increases of 18–20 percentage points (on a base of approximately 50 percent) on the uptake of prenatal care and routine child checkups and 15 percentage points (on a base of approximately 10 percent) on growth monitoring among women and children with the CCT treatment. Glewwe and Olinto (2004) show increased enrollment rates of around 2.6 percentage points and reduced absenteeism. Similarly, Galiani and McEwan (2013) use the 2001 national census, which was administered after eight months of transfers, and find an increase in enrollment rates of approximately 8 percentage points among children eligible for the educational transfer and a decrease of 3 percentage points in the probability of having worked in the last week, with effects being larger in the two strata with the highest level of malnutrition at the baseline.

Also, potentially relevant for our analysis is a study by Stecklov et al. (2007) showing that PRAF-II led to an increase in fertility of 2 to 4 percentage points by 2002. These changes in fertility could possibly affect our results for the youngest cohorts.[5] In addition, if such changes in fertility in turn led to a shift of fertility norms, they may also have had indirect effects on the older cohorts once they reached reproductive age.

Ham and Michelson (2018) use municipality-level averages from the 2001 and 2013 Honduran censuses to analyze the impact of PRAF-II for children ages 6–12 in 2001. They exploit the randomized design and show increases in years of schooling, secondary school completion, and labor force participation, especially for females in G_2, and after controlling for a large number of time-variant (and hence possibly endogenous) controls. As the paper uses average outcomes based on place of residence in 2013, it makes the strong assumption that migration between 2001 and 2013 (over 25 percent for this age cohort with less than 5 percent internal to the program municipalities) does not affect the internal validity and cannot account for any returns that materialize through migration.

[5] The program design was altered in 2003, removing this possible fertility incentive, so that the short-term fertility increase most likely affected only those born in the first years after the start of the program (i.e., ages 9–12 in 2013).

3. Data and Methodology

The principal data source is the XVII Honduran National Population and Housing Census of 2013. For the main analyses, we limit the sample to all individuals born in the targeted 70 rural municipalities regardless of their current residential location. For the age groups we study, municipality of birth together with the municipal-level randomized program assignment provides an exogenous indicator of program exposure and allows the factoring out of any subsequent domestic migration or geographical sorting that may have occurred during or after the program.[6] In addition to capturing all residents, the census includes basic information on former household members who left the country at any point over the prior decade. Information available on these international migrants includes gender, age, ethnicity, year of migration, and current country of residence—but no information on schooling. To estimate program impacts on international migration, we incorporate the migrants into the individual-level census sample by assuming they were born in the same municipality that the household from which they migrated is located in 2013.

As a consequence of targeting the program to areas with the highest malnutrition rates in the country, PRAF-II operated in regions with a high share of indigenous people. While the indigenous population in Honduras comprises only 6.5 percent of the national population, 39 percent of the analysis sample (individuals ages 6–29 in 2013 and born in the 70 program municipalities) is indigenous. We classify as indigenous all individuals who identify[7] as indigenous, Afro-Honduran, or black—95 percent of whom in our sample are Lenca.[8]

Given the randomized assignment and the results of the balance tests in Appendix C that show a balance on observables using the 2001 and 1988 population censuses, our main methodological approach for determining the long-term impacts of the CCT is to estimate a single difference ITT model, as in equation (1),

[6] While the census data contain rural or urban designation for current residential location, they do not include it for location at birth, so our original plan to examine the effects of the CCT for urban versus rural origin locations is not possible. Instead, we distinguish between the indigenous and non-indigenous population. For other details on the proposed research design prior to data access, see Molina Millán et al. (2015), available at CCT Long-Term Impacts in Latin America: Research Proposal for Honduras.

[7] Because we use self-reported ethnicity, a potential concern is that the program might influence how people report their ethnicity. In other contexts, economic status has been shown to be associated with reported ethnicity, though this may be less salient in Honduras since the dominant indigenous group in the sample, the Lenca, does not speak a different language from the rest of the population (as opposed to the indigenous people in other countries in Central America). For males and females, we fail to reject the null that the probability of reporting as indigenous is unrelated to treatment status (*p*-values of 0.622 and 0.640, respectively). If anything, the point estimates suggest slightly more people reporting to be indigenous in G_1. All 70 municipalities have both indigenous and nonindigenous populations.

[8] Galiani, McEwan, and Quistorff (2017) provide a map of the concentration of the Lenca population in 2001, the largest indigenous group in both Honduras and the program area.

$$Y_{ij} = \alpha + \beta_1 G_{1j} + \beta_2 G_{2j} + \beta_3 G_{3j} + \gamma X_{ij} + \epsilon_{ij} \quad (1)$$

where Y_{ij} is the outcome variable of interest measured in the 2013 census for individual *i*, born in municipality *j,* and G_{1j} takes the value 1 if municipality *j* benefited from the (basic) CCT and 0 otherwise. β_1 is the parameter of interest and provides the estimate of the absolute long-term effect of past program exposure. To adhere to the experimental design, we control for the other treatment groups with indicator variables for whether municipality *j* benefited from both the CCT and supply-side interventions simultaneously (G_{2j}) or the supply-side interventions only (G_{3j}). Following Athey and Imbens' (2017) recommendation of using limited and binary controls given the randomized design, the vector X_{ij} includes indicator variables for four of the five strata used in randomization, single-year age fixed effects, and, when available, a binary indicator for whether the average value of outcome *Y* in municipality *j* for individuals aged 20–25 in 2001 (and born in the municipality) is above the median of the municipality-level averages across all 70 municipalities.[9]

Standard errors are calculated allowing for clustering at the municipality level. Because of the relatively small number of municipalities (40 for our principal comparison of G_1 versus the control), we also replicate all hypothesis tests using randomization-based inference tests, as suggested by Athey and Imbens (2017) and Young (2017). In randomization-based inference, uncertainty in the estimates arises from the random assignment of the treatments rather than from sampling. This method allows estimating the exact *p*-value under the sharp null hypothesis that the treatment effect is null by calculating all possible realizations of a test statistic and rejecting if the observed realization in the experiment itself is above the significance level cutoff for the generated distribution of test statistics. Randomization inference provides exact finite sample test statistics without appealing to asymptotics and as such allows testing for the influence of potential outliers and protects against accidental imbalance affecting the results.

We estimate ITT effects for several different age cohorts (over a wide range from 6 to 29 years old in 2013), whose selection is informed by the design and timing of the CCT. All outcomes from the census are measured in 2013. As described above, the program ran five years (2000–2005), targeting households with pregnant women and, initially, children under three (extended to under five starting in 2003) and school-aged children, ages 6–12. Consequently, children in

[9] While the program had been in place for eight months by the time of the 2001 census, the schooling of the cohort of individuals aged 20–25 years should not have been directly affected given the program rules (and was likely to have only been minimally indirectly affected, if at all). At the same time, this cohort is young enough to be reflective of general secular differences in schooling in the program municipalities. See also Appendix C. Figures A.1 to A.4 repeat the main estimates of highest grade attained without controlling for 2001 municipality educational level and demonstrate that, if anything, the controls lead to conservative estimates.

treatment areas were potentially exposed to the different program components in full or in part, depending on when they were born. For example, only a child born in 2000 could have directly benefitted from the health component for the full five years (and he or she would be 13 in 2013). A child born in 2003 (i.e., 10 in 2013) could have directly benefited from only the nutrition and health component for two years postnatal before the program ended.[10] In contrast, a six-year-old child in 2000 (i.e., 19 in 2013) could directly and fully benefit from the education components from first through fourth grade.

Children older than six years of age in 2000 also benefitted, and possibly even more if the program affected them at ages at which they might otherwise have started to drop out. In Figure 1, we use the short-term program evaluation baseline data to show average preprogram enrollment rates for boys and girls by age in 2000. Trends are broadly similar across the municipalities subsequently exposed to the CCT and the control, providing further evidence of balance. For both boys and girls, enrollment rates are above 90 percent until about age 11, after which they decline considerably. Consequently, individuals 24–26 years of age in 2013 were at highest risk of dropping out when the program started; similarly, those 19–23 years of age were at risk of dropping out during the five years when the program was ongoing. Finally, individuals 27–29 years of age would not themselves have been eligible for any transfers, but they nevertheless may have benefitted from transfers received by their households (for younger siblings) at ages in which their risk of dropout was high.

We use the patterns of full or partial exposure to define a set of age cohorts as shown in Figure 2, where for each cohort we indicate ages at the start of the program in 2000, ages in 2013 at the time of measurement, and approximate potential number of years of exposure. In the main analysis, we estimate the impact of the basic CCT (β_1) separately for each age cohort. To verify that the results are not driven by multiple hypotheses testing, we compute the joint significance test of the estimated coefficients (β_g) for all age cohorts using Young's (2017) omnibus randomization test. Appendix Table A.7 shows *p*-values from omnibus tests that combine estimates for all cohorts and outcomes obtained from the census data for each demographic group, confirming the overall significance of the findings for each group. Table A.7 also reports omnibus joint-significance tests for all cohorts by family of outcomes (education, migration, and marriage and fertility) separately for each demographic group.

[10] The child also may have benefited (indirectly) in utero.

Given that the experiment included three treatment groups and we estimate treatment effects for eight different age cohorts, we alternatively estimate for each demographic group a model combining individuals from all eight age cohorts shown in Figure 2 and directly test for differences in program effects by age cohort as well as for the overall program impact across all age cohorts. Specifically, we extend equation (1) to include indicator variables for the age cohorts ($COHORT_c$), taking the value 1 if individual *i* belongs to age cohort *c*, where *c* represents all except one of the eight defined cohorts. The age cohort indicator variables are also each interacted with assignment to treatment (and all other controls) yielding equation (2):

$$Y_{icj} = \alpha + \widetilde{\beta_1} G_{1j} + \widetilde{\beta_2} G_{2j} + \widetilde{\beta_3} G_{3j} + \sum_{c=1}^{7} \theta_c COHORT_c + \sum_{g=1}^{3} \sum_{c=1}^{7} \delta_{cg} \left(G_{gj} \times COHORT_c \right) + \gamma X_{icj} + \epsilon_{icj} \qquad (2)$$

Following Chetty, Hendren, and Katz (2016), we then implement a set of parametric F-tests for the null hypotheses that there are no cohort-specific treatment effects in the pooled data. We compute the joint significance test of the estimated coefficients $\tilde{\beta}_1$, and each of the ($\tilde{\beta}_1$+δ_{c1}). That is, we test the joint significance of the eight estimated G_1 treatment effects (corresponding to the eight age cohorts for each demographic group). This single test on the pooled sample is not subject to the over-rejection rate that occurs when analyzing the individual subgroups separately.

Finally, before presenting the results, we note two potential selection concerns. First, remaining attrition selection could be related to differential mortality rates, in particular if exposure to the health and nutrition components reduced infant mortality in the treatment group. Second, as noted above, there is also a potential selection concern due to the short-term fertility increase in the treatment area related to unintended incentives for increased fertility following from the program rules. This could particularly be a concern for those born in the first few years after the start of the program (i.e., age 9–12 in 2013), after which the program rules were changed. That said, differences in both fertility and mortality should affect relative cohort sizes, and we do not find any significant differences in cohort sizes for the four demographic groups analyzed. The differences between G_1 and the control are small (Appendix Table C.4) suggesting that ITT estimates on other outcomes are unlikely to be strongly affected by any fertility and/or mortality selection.

4. Results

In the discussion of the results, we focus on the long-term impacts of the basic CCT, that is G_1, captured by β_1. (Appendix D presents impacts for G_2 and compares them with those of G_1.)

4.1 Education

Table 1 presents the long-term impacts of the CCT on highest grade attained (defined as grades of completed schooling) for the eight cohorts and separately for females and males. For both sexes, there is clear evidence of an impact on the older cohorts that were fully or partially exposed to education transfers as children. Individuals 19–26 years of age have attained between 0.31 and 0.43 more grades (compared with control group averages of about six grades or lower). The significance of the effects is robust to randomization inference. While positive, effects for the younger cohorts exposed to the nutrition and health components or born after the program ended, however, are mostly not significant.

Taken on their own, these results suggest only minimal long-term effects on education from early life exposure to the CCT for both females and males. This is a somewhat surprising first result, however, given the at-least-modest evidence of short-term effectiveness of the program on young children (Morris et al., 2004) and the recent emphasis on the importance of investment during this period of life. It may be that other constraints that inhibit translation of the short-term results into later improvements in education are affecting all or part of the population. To explore this further, we consider a second potentially important dimension of heterogeneity—ethnicity. Apart from being predetermined, ethnicity in this context likely proxies for a combination of additional constraints during or after program implementation.

The graphical presentation in Figures 3 to 12 shows impacts for each outcome and age cohort for the four groups of interest: females and males with and without indigenous backgrounds. Each figure shows the ITT effects (equation 1) of the CCT for children born in G_1 municipalities on the outcome of interest by age cohort. Point estimates are represented by dark blue dots (left vertical-axis scale) with their corresponding 90 (blue dash) and 95 (red square) percent confidence intervals. Each figure also shows the average value of the outcome in the control group (G_4 municipalities) (blue line, right vertical-axis scale). Figures show ages at the time of measurement in 2013, 13 years after program began, as illustrated in Figure 2, showing the variation in the exposure to different program components across ages.

Non-indigenous Females

Figure 3 presents the estimates of the long-term impact on grades attained for non-indigenous females. The cohort that benefited the most based on the point estimates was 19–23, i.e., those exposed to the G_1 transfers during school-going ages (6–10 at the start of the program) for potentially the longest period. Their highest grade attained increased by more than 0.5 grades (a significant increase of about 10 percent). But importantly, the effects are also positive and significant (about 0.4 grades) for those exposed to the nutrition and health package in early childhood, ages 11–12 and 13–15. Estimates are significant but smaller (0.2 grades) for girls 9–10 years old who were born during the program and positive but not significant for the other age groups. These include those not yet born during the program and those too old to have received the education transfers directly. They also include girls (ages 16–18) who were too old at the start of the program for the health and nutrition package and too young to have directly received the full educational transfers, in a sense falling in a gap of program coverage in the initial design.

The gains in grades attained for non-indigenous females are reflected in much higher completion rates for different levels of schooling. Figure 4a shows ITT estimates on the probability of: (1) completing fourth grade (top-left); (2) completing primary school (i.e., sixth grade, top-right); (3) completing 12 years of school—i.e., secondary school studies (bottom-left); and (4) having started university studies (bottom-right). The impacts follow clear age patterns and show relatively large gains for the school level most relevant to each age cohort.

The CCT impact on completing at least fourth grade (beyond which the conditionality ended), for instance, is significant and relatively large for all age cohorts shown (4.7–9.5 percentage points); in relative terms, the largest impact is observed for the youngest cohort (ages 9–10), the age group for which there was more potential for improvement as measured by the control group. Among females in the two youngest cohorts, the CCT also increased the probability of being enrolled in 2013 by 4.7 percentage points or more (Figure 5a). These results suggest that non-indigenous females in G_1 municipalities were still starting school earlier *several years after the CCT had ended* and households had stopped receiving transfers, plausibly due to improved health and nutrition earlier in life.

For the next older cohorts, 11–12 and 13–15 years old, we find an increase (Figure 4a) in the probability of completing primary school of 5.1 percentage points (about 28 percent) and 6.7 percentage points (about 11 percent), respectively. Smaller positive but insignificant effects on completing primary education are observed in the older cohorts. Among females old enough to have completed 12 years of schooling, we observe an effect of 1.3 percentage points in the youngest cohort (16–18) and approximately 3.5 percentage points in the other cohorts. Finally, among those old enough to have begun university, non-indigenous females in the 19–23 and 24–26 cohorts, both at least partially exposed to the education components of the CCT, were at least

1.0 percentage point more likely to have reached university, an approximately 50 percent increase.

Overall, the results show robust improvement on educational outcomes for non-indigenous females in age cohorts directly affected by the CCT at an earlier stage of their lives, and this holds both for those directly affected by the education as well as those affected by the nutrition and health components of the program. We also observe significant spillover effects on current enrollment for the youngest cohort (ages 6–8) born after the program ended, as well as some spillover effects on completing four or 12 years of schooling for the oldest cohort (ages 27–29) that was too old at the time of the program to be eligible themselves. With the exception of the youngest cohorts, however, Figure 5a shows no contemporaneous enrollment effects.

Randomization-based inference tests yield *p*-values and significance levels consistent with the results obtained using regression-based inference tests accounting for clustering at the municipality level. All of the statistically significant point estimates reported in the figures are also significantly different from zero under both methods of randomization inference suggested in Young (2017) (see Appendix Table A.3). We further reject the null of no treatment effects across all regressions and treatments on education outcomes at the 1 percent level (Appendix Table A.7). We also reject at the 1 percent level (p-value = 0.002) the hypothesis that the estimated G_1 treatment effects for the eight age cohorts on grades attained are all equal to zero (linear joint test of G_1 treatment effects for all eight cohorts from equation 2).

Indigenous Females

Figure 6 presents the results for the highest grade attained for indigenous females. In contrast to non-indigenous females, there are few long-term impacts of the CCT, suggesting the program did not manage to overcome constraints facing educational investments for most girls from this more marginalized population. The notable exceptions are for the two oldest cohorts, where there is an ITT effect of approximately one-half a grade for those 24–29 years of age. Focusing on specific education levels, Figure 4b shows that impacts on school-level completion are negligible in size and not significant for the cohorts of indigenous females who would have been eligible for nutrition and health transfers. For those eligible for the education transfers, however, we observe positive and significant effects on the probability of completing fourth grade, from 3.3 to 9.3 percentage points. Moreover, we find large spillover effects for the oldest cohort, who were too old to receive the transfers at the start of the program, a 9.0 percentage point increase.[11] Finally, we estimate positive and statistically significant effects on the probability of completing secondary school and having reached university for the age 24–26 cohort. Estimates are also positive but smaller for the next younger and older cohorts. Figure 5b demonstrates that indigenous females between 16 and 23 years old exposed to the CCT in the past are more likely to still be enrolled in school in 2013, suggesting grade differentials for them may further increase. In contrast to the evidence for non-indigenous females, however, there are no significant enrollment effects for the youngest cohorts.

Overall, indigenous females exposed to education transfers at ages when they were at higher risk of dropping out of school benefited the most in terms of educational outcomes, followed by younger females also exposed to education transfers. Indigenous females 24–26 years old who were born in G_1 municipalities have on average a half grade more schooling and are 3.7 percentage points more likely to have completed secondary school. The significance of these findings is supported by the randomization-based inference tests on significance for single point estimates (Appendix Table A.3) and all educational outcomes and treatments together (Appendix Table A.7). The linear joint test of G_1 treatment effects for all eight cohorts from equation 2 further indicates the G_1 effects on highest grade attained to be jointly significant ($p < 0.001$).

Non-indigenous Males

Results for males are broadly similar to those for females, with larger estimated and more significant impacts for non-indigenous than for indigenous males. Treatment effects for non-indigenous males are significant for at least some outcomes across all of the age cohorts. Both the cohorts exposed to the educational components of the CCT and those exposed to nutrition and health components had higher grades attained (Figure 7a). The largest impacts in relative terms are found among cohorts eligible for the education transfers, for whom we observe more than a half grade increase (over 10 percent among males in the age 19–23 and 24–26 cohorts).

Figure 8a further shows G_1 impacts on the likelihood of completing various schooling levels for non-indigenous males, with all cohorts except the youngest (ages 9–10) showing significant differences in at least one of the levels examined. Yet, as shown in Figure 9a, the youngest cohort may still be on track to higher levels given that they are currently 5.4 percentage points more likely to be enrolled. We also find positive and significant increases of 4.9–8.6 percentage points (Figure

[11] Figure A.2 shows that estimates for grades attained are positive and significant for all age groups exposed to the educational components (ages 16–27) when not controlling for baseline education. This further suggests that the health and nutrition components in particular were less effective in increasing educational components for the indigenous compared to the non-indigenous.

8a) on the probability of completing fourth grade for cohorts that were eligible for the nutrition and health transfers (ages 11–15) and cohorts exposed to the education transfer (ages 16–26). In contrast with the findings for non-indigenous females, the probability of completing primary school also significantly increases for cohorts old enough to have reached sixth grade (age 12 or older). The ITT effect goes from a 4.7 percentage point (22 percent) increase for the youngest cohort (ages 11–12) to a 7.0 percentage point increase for the cohorts exposed to the education transfer (10 percent). The largest relative effects for the oldest cohorts are for completing secondary school and starting university. The results show an increase of about 4 percentage points for completing secondary school (an increase of 34 to 56 percent). Even larger relative effects are observed on the probability of reaching university. Non-indigenous men ages 19–26 are 0.9 percentage points more likely to have university studies, indicating that university enrollment almost doubled compared with the control. Moreover, Figure 9a shows that the CCT also increases the probability of still being enrolled in school by about 1.5–2.5 percentage points for those ages 19–23.

Across the different outcomes, we also find large spillover effects on the oldest males. The age 27–29 cohort had nearly 0.5 higher grades attained (Figure 7a) and achieved higher levels of secondary school completion and starting university (Figure 8a), and they are still more likely to be enrolled (Figure 9a), with the size of the treatment effects similar to those for the younger 24–26 cohort. There were also spillovers to the youngest cohort born after the end of the program, who are 7.5 percentage points more likely to be enrolled (Figure 9a).

Results for non-indigenous males are robust to running randomization-based inference significance tests for all of the educational outcomes separately (Appendix Table A.4) and pooled together (Appendix Table A.7). We also reject the hypothesis that all estimated G_1 treatment effects on grades attained are equal to zero (linear joint test of G_1 treatment effects for all eight cohorts from equation 2) with *p*-value = 0.003.

Indigenous Males

In contrast, Figure 7b shows that among indigenous males, ITT estimates on grades attained are small and insignificant. This result largely holds when considering completion of different schooling levels for the various cohorts (Figure 8b) and when not controlling for 2001 education levels (Figure A.4). In contrast to the results for indigenous females in the oldest cohorts, there are minimal long-term effects for indigenous males with the exception of starting university, where point estimates are significant for the cohorts from ages 19–29 (and robust to randomization-inference significance tests, Appendix Table A.7). As for the other cohorts considered, while the point estimates show that the probability of having some university studies increased by only 1 percentage point, the relative size of the impact on starting university studies is large, as it implies (approximately) a doubling compared with the low rate in the control group. Indeed, we reject the joint null of no G_1 treatment effects for all eight age cohorts for university studies at the 1 percent level (*p*-value = 0.006).

Putting the Education Results Together

Overall, the results on educational outcomes show positive and significant ITT effects of the CCT for non-indigenous females and males, across different age cohorts and for all levels of education. In contrast, for the indigenous population, positive ITT effects were observed for only the subsample of females eligible for the educational transfer, while gains for the males were limited to a specific group of older males who had reached university studies and represent less than 1 percent of the population.

4.2 Migration

Domestic Migration

As previously described, the ITT estimates above are not subject to selection from domestic migration because we assign treatment eligibility status based on the municipality of birth. Domestic migration is a potentially important outcome in its own right, however, especially in settings in which migration to urban areas often improves access to economic opportunities. We examine municipal-level domestic migration (whether at the time of the 2013 census the individual was living in a different municipality than the municipality of birth) and, separately, urban domestic migration (whether at the time of the 2013 census the individual was living in an urban area in a different municipality than the municipality of birth). While this does not capture all domestic migration (for example, migration to urban centers within the municipality of birth), as the 70 municipalities are predominantly rural, it is likely to capture most substantive migration. Estimation of the long-term impacts in Table 2 suggests the CCT reduced domestic migration by 4 percentage points (significant at 10 percent with randomization inference) of males ages 19–23 and 27–29 who had been eligible for the educational components.[12] The point estimates for girls

[12] The large incidence of domestic migration in the non-indigenous population, especially in the groups exposed to the education transfers, and the causal negative effect of the program in domestic migration of males imply that any ITT estimates based on current municipality could be substantially biased. Further, the lack of a treatment effect on domestic migration for other age groups and females does not mean that estimates of program effects based on current rather than birth municipality would not be affected by selection, as nonsignificant average effects may well mask that different types of individuals decide to leave or stay, and that decision could be affected by past treatment.

in the 19–23 age cohort is similar in size but not significant, but the omnibus indicates that overall there are also treatment effects for girls (driven in part by G_2 as shown in Appendix D).

Figure 10 presents the CCT impacts and incidence of any domestic migration for the four demographic groups and by age cohort, while Figure 11 focuses on migration to urban destinations only. Domestic migration is very common for the non-indigenous, with rates over 30 percent for males and about 40 percent for females in the oldest age cohort. Among the indigenous population, regardless of gender, domestic migration is an order of magnitude smaller. For example, only 2 percent of the indigenous sample of females and males under the age of 15 were no longer living in their municipality of birth, and this rate does not exceed 8 percent in the oldest cohort (ages 27–29). For indigenous males 19–23 years of age, there is also a significant negative treatment effect for migration to urban areas.

International Migration

While domestic migration is common, international migration in the sample is relatively rare. Understanding the program's impacts on international migration is nevertheless important, both to understand any remaining selection and because it is an important outcome in its own right. Table 2 shows that international migration in the control group is uncommon, but in the older cohorts, men are approximately 3 percentage points more likely to migrate abroad than women.[13] In these same cohorts, there are also large positive impacts of G_1 on migration for men, but no significant effects for females. The two top panels of Figure 12 present the impact on international migration for females for the non-indigenous subsample (left panel) and for the indigenous subsample (right panel) and the bottom two for their male counterparts. For non-indigenous males in the two oldest cohorts, exposure to the CCT doubles the probability of international migration (from 3 to 7 percentage points). The joint tests for all cohorts (based on equation 2) indicate that the G_1 treatment effects are jointly significant (p-value = 0.052). Point estimates for indigenous males are positive and of similar magnitude, though not significantly different from zero.

Taken together, these results suggest a statistically significant impact on international migration, albeit from very low initial levels. They also illustrate the advantages of using the population census, as it provides sufficient power to identify impacts on relatively rare, but potentially important, outcomes. The p-values on hypothesis tests for all migration outcomes

[13] Because migration is not observed in 2001, we do not control for the outcome measure from 2001 for the 20–25 age cohort but instead control for grades attained by that group.

using randomization-based inference are consistent with the confidence intervals reported in Figures 10–12 (and shown in Appendix Tables A.5, A.6, and A.7).

The findings on international migration raise the possibility that the long-term effects on education estimated in the previous section suffer from sample selection bias. If the CCT treatment increases education and induces disproportionally more migration among the educated, the impact on educational attainment may be underestimated. Rigorous research on the selectivity of migrants from the region is rare, although recent work suggests that, if anything, migrants from Honduras are likely positively selected in this fashion—that is, with higher education levels (Del Carmen and Sousa, 2018). On the other hand, if those who migrated in response to the CCT tend to be less well educated, treatment effects on education may be overestimated. Unfortunately, the census does not include information about the education levels of the international migrants that would allow direct assessment of these potential biases, but we can explore these possibilities indirectly.

Using the census data for all households in the 70 municipalities, we examine the relationship between the education of the household head and the probability of having an international migrant from the household to shed some light on potential selection. Empirically, the relationship is weak and non-monotonic, with international migration increasing with household head education at low levels and then decreasing at about the 60th percentile (fourth grade). This result together with the low overall levels of international migration makes it seem unlikely that international migration leads to strong selection concerns in the previous section. Nevertheless, they are a potential caveat for the educational outcomes for boys in the oldest two cohorts.

4.3 Marriage and Fertility for Women

Exploration of the effects of the Honduran CCT on marriage and fertility yield rather mixed evidence and suggest that effects may not have been homogenous across age cohorts (Table 3). There is a significant increase in fertility during early teenage years (ages 13–15) for all women. For non-indigenous females, there is also an increase among those aged 16–18 and 24–26, with point estimates indicating about a 2 percentage point increase. In contrast, indigenous girls from the oldest cohorts (ages 24–29) are around 2–4 percentage points less likely to have started childbearing. In none of the women's subsamples do we observe a significant effect on marriage outcomes. These results are robust to randomization inference and supported by the omnibus test (Appendix Table A.7). The linear joint test of G_1 treatment effects on fertility for all age cohorts (based on equation 2) indicates joint significance for both the non-indigenous (p-value = 0.043) and the indigenous (p-value = 0.010).[14]

While the results for the older indigenous women are consistent with the findings of higher schooling for this group, the results for the non-indigenous are less readily understood, as they point to an increase in very early teenage pregnancy in those groups. Our data are not well suited to disentangle the underlying mechanisms, but other results in the literature offer possible explanations. Stecklov et al. (2007) found a short-term increase on fertility, and if this change led to a change in the social norm regarding fertility, it may have had longer-term consequences. Barham, Macours, and Maluccio (2018b) show that CCT nutrition shocks can affect the age-of-menarche, leading to earlier sexual maturity. And Baird, McIntosh, and Özler (2018) show that delays in fertility during a CCT in Malawi were offset once the program ended. It is also possible that the early fertility results in Honduras are driven by a group of girls that completed schooling earlier because of the program and therefore made earlier transitions to the next phase in their life cycle. Overall, more detailed micro-level analysis with targeted household surveys would likely be needed to better understand these patterns.

4.4 Labor Market Outcomes

The primary objectives of CCT programs are to alleviate poverty in the short run and foster investment in human capital with the expectation that those investments will lead to long-run improvements, including increased lifetime earnings. The overall positive and statistically significant long-term impacts that we find on education and international migration stemming from the Honduran CCT naturally lead to questions about whether there are any corresponding effects on earnings. A conceptual challenge in exploring questions for these age groups, however, is that many are only just transitioning to the labor market, and for women, labor force participation rates are low. Additionally, fertility decisions, which show less conclusive results for women old enough to be working, are almost certainly related to their labor force participation decisions.

An accompanying empirical challenge for exploring labor market outcomes is the sparse information on labor force participation and lack of information on earnings outcomes in the census data, making it ill-suited to understanding labor market impacts.[15] Therefore, we

[14] Among men, we estimate a negative treatment effect for non-indigenous males aged 13–15 on the probability of being married but positive and significant treatment effects of about 1.5–4.5 percentage points for ages 16–23. Results for indigenous males are not significant (Appendix Table A.2).

[15] The census collects data on economic activities only for the previous seven days. Given the highly seasonal nature of economic activities in rural Honduras, this is unlikely to accurately capture labor market activities for the target population.

complement the analysis of the census data with analysis of data from the more comprehensive annual labor survey, the EPHPM, collected one or two times per year with samples in both rural and urban areas covering all 18 departments of Honduras. Similar to the national census, the EPHPM crucially includes information on location of birth, which allows assignment of treatment status in the same manner and circumvents selectivity from domestic migration that could affect internal or external validity.

Compared with the census, however, the EPHPM has three main caveats: first, sample sizes are relatively small; second, it is not representative of the population at the municipality level, potentially introducing sample selection bias; and third, it does not provide information on ethnicity. We address the first caveat by pooling seven years of data (2010–2016) but recognize that even after pooling data across multiple rounds, the relatively small sample size may affect the precision of the estimates.[16] Each EPHPM survey round we use has sample observations from about 80 percent of the 70 PRAF-II municipalities. We address the second caveat by constructing population weights and then benchmarking them against the census data via a comparison of estimated means and CCT program impacts on the educational outcomes. We address the third caveat by presenting results for individuals from predominantly non-indigenous localities, as determined by the census alongside the results for all individuals.[17]

We focus the analysis on young adults who were exposed to the educational transfers and are old enough to have started their transition into the labor market, that is, individuals aged 19–26 in 2013. This combines the two cohorts for which there is strong evidence of impacts on education. We estimate equation (3),

$$Y_{ij} = \alpha + \beta_1 G_{1j} + \beta_2 G_{2j} + \beta_3 G_{3j} + \sum_{t=2010}^{2016} \theta T_t + \gamma X_{ij} + \epsilon_{ij} \qquad (3)$$

by both ordinary least squares (OLS) and weighted least squares (WLS), where T_t equals 1 if the individual was interviewed in year t and 0 otherwise. Other variables are as in equation (1). Using equation 3, we first replicate the analysis done using census data on educational as well as selected demographic outcomes and then extend it to labor outcomes. Appendix E compares the results on educational and demographic outcomes using census data to estimate equation (1) and using EPHPM data to estimate equation (3) by OLS and WLS. This exercise confirms that the sampling frame for the surveys does not appear to be fully representative of those born in the

[16] This approach is similar to Rackstraw (2014), who uses a somewhat different empirical specification and time period, so results are not directly comparable. Another survey available for Honduras in 2005 and 2011, the Demographic and Health Survey, does not collect information on municipality of birth so we do not use it.

[17] Details of pooling surveys, constructing and benchmarking sample weights, and determining non-indigenous localities are provided in Appendix E.

program areas. While point estimates of the ITT effects for most variables for women are relatively consistent across data sources (see below), this is not the case for the estimates for men (Appendix Table E.4). Consequently, we have less confidence in the results for men and only present them in the appendices, focusing the analysis here on the plausibly more reliable results for women (see appendix E for details).

The first row in Table 4 shows that, on average, women aged 19–26 born in control municipalities in the household survey have higher levels of education (about half a grade attained) than the same age cohort in the census. The ITT estimate on grades attained estimated using WLS or OLS are similar in magnitude to the ITT estimates with the census, although it is not significant. The survey data further suggest that females exposed to the CCT are about 4 percentage points more likely to be full-time students, which does not match the census result. Applying the population weights, we partially correct for the differences with respect to the census (see Appendix Table E.1).

In a context in which less than one-third of women of the relevant age cohort are working, the ITT estimates in Table 4 further show lower participation rates among females born in G_1 municipalities at both the extensive and intensive margins (measured in hours worked per week) compared with the control. The differences are particularly large for those living in non-indigenous villages, with a 12 percentage point decline in the probability of working in the WLS estimates. There are no clear shifts between sectors, but women in G_1 municipalities are 4 to 9 percentage points less likely to work as domestic workers, compared with the mean of 7–10 percent in the control, and women born in G_1 also work significantly fewer hours compared with those from the control communities (where the average number of hours worked for those working is 48 hours).

As the young women from G_1 municipalities in the sample work substantially less, we would expect them to have lower earnings unless those working obtain much higher wages. In Table 4 we also show ITT estimates on monthly and hourly labor earnings (winsorized at the top 1 percent of values) in 2013 lempiras.[18] We first consider unconditional results, as working is an endogenous outcome, and those that do not work are attributed zero earnings in these estimates. Table 4 shows significant and large negative effects on earnings.[19] This result of course partially reflects the lower labor participation rate among females in this group. To better understand the mechanism, we therefore also report conditional results, restricting the analysis to the sample of

[18] Monthly labor income includes monetary and nonmonetary earnings from up to two activities.

[19] Results are robust to two transformations of the outcome variables made to reduce the sensitivity to outliers: the inverse hyperbolic sine transformation for the unconditional earnings to account for the many 0 values, and the use of the rank of income for the conditional earnings (following Athey and Imbens, 2017).

females who are active in the labor market. Point estimates on conditional monthly earnings are still negative but no longer significant, and estimates on hourly income are positive but insignificant.

The results could reflect that many females born in the CCT municipalities have not (yet) transitioned into the labor market, and hence it may be too early to determine long-term program effects on returns to education for them. To gauge the potential for gains, however, we provide an alternative and more speculative assessment of the long-term effects by incorporating into the analysis the subsample of full-time students. We assume that current full-time students will participate in the labor market and earn the median labor income of a full-time worker in the same age cohort with the same level of education. The bottom panel of Table 4 shows the results on earnings, including the subsample of full-time students with imputed monthly and hourly earnings. While point estimates on monthly earnings are not significantly different from zero, point estimates on hourly income suggest that females exposed to the CCT would be earning approximately 22 percent more per hour worked than those in the control. Restricting the sample to females born in predominantly non-indigenous villages increases the point estimates further. Finally, also note that apart from the non-negligible share of full-time students in the sample, a relatively large share are part-time students (10 percent among all women and 19 percent among non-indigenous women), and the overall increase in educational level also means that those that are already working may have accumulated fewer relevant years of relevant experience compared to those in the control.

Overall, then, the income results for young women present a mixed picture regarding potential labor market returns. Results suggest that constraints on young women's labor force participation likely remain important in this context. Total incomes do not increase even when accounting for sample selection, although the evidence suggests the women may be earning more per hour worked, consistent with the shift out of domestic work toward possibly higher-quality jobs. With slightly higher levels of education, women may also be able to afford to refuse the lowest-paying jobs and possibly wait for better opportunities to arise. Nevertheless, taken at face value, these results appear to suggest that there are no strong labor market returns to the increased human capital engendered by the CCT. At the same time, the analysis also underlines the difficulties in estimating labor market returns for young women who are still transitioning (or have just transitioned) into the labor market.

5. Conclusions

Since the start of CCT programs in the late 1990s, several evaluations have rigorously shown their short-term impacts in different settings. Impacts include poverty alleviation, health improvements, and increases in educational outcomes. But only a few studies have investigated whether short-term gains have translated into long-term benefits, as well. This paper presents new evidence on the long-term impact of the PRAF-II CCT program in Honduras using individual census data collected 13 years after the start of a program, which is also eight years after it ended. We exploit the randomized design of PRAF-II and show statistically significant long-term impacts on education and international migration outcomes for many cohorts.

The experimental results indicate long-term gains in schooling among females and males of non-indigenous background who benefited at different ages of different components of the CCT. We find positive and significant impacts on completing primary and secondary education and reaching tertiary studies. Results show more modest effects for indigenous populations, even though indigenous females in ages at higher risk of dropping out of school at the start of the program also benefited in terms of schooling. This may lend some strength to the notion that the design of CCTs for indigenous populations needs to be culturally adapted and/or complemented with interventions targeting remaining constraints to education to achieve their intended objectives. Results further show statistically significant positive CCT effects on international migration among non-indigenous males, and to a lesser extent among non-indigenous females and indigenous males. Since international migration is relatively rare, the absolute effect in the overall population is not large. Nevertheless, the migration results from this first-generation CCT point to the need for complementary policy initiatives to support the transition from the CCT to the domestic labor market (such as training and labor market insertion programs currently implemented in Honduras), which may serve to reduce this effect. Analysis of these more recent next-generation CCT programs in Honduras and elsewhere is needed to understand whether they, too, influence migration. Results for labor market outcomes were inconclusive.

The evidence in this paper stands out by demonstrating positive and robust impacts on educational outcomes for cohorts of a very wide age range, showing that the CCT program sustainably affected human capital both through early childhood exposure to the nutrition and health components and through exposure during school-going ages to the educational components. Overall, the five-year intervention appears to have changed the educational profile of a generation from the beneficiary municipalities, and the results suggest that some of the increased investments in education occurred years after the end of the intervention, including on those not directly targeted by the program eligibility rules. This result highlights spillover effects that need to be considered when analyzing the return on investment of a CCT.

The estimated impacts are not only significant but also substantial, with an increase of 0.6 grades attained and increases in secondary school completion and the starting of university studies of more than 50 percent for those exposed at school-going ages. These large gains in part reflect the low educational levels at the baseline but also suggest that average gains in education can mask very important gains obtained by a subset of the population. The results on international migration further highlight the potential important heterogeneity in outcomes. Taken together, the results of this study suggest the presence of many remaining constraints that may be preventing a large share of the target population from getting higher long-term returns from the CCT intervention. Even so, they also show the potential of CCTs to lead to sustained long-term effects.

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Tables and Figures

Table 1. Long-Term Impacts of CCT (G_1) on Grade Attained

	Females				Males			
	N	**Mean G_4**	**Coeff. (s.e.)**	**Exact *p*-value**	**N**	**Mean G_4**	**Coeff. (s.e.)**	**Exact *p*-value**
6–8 years old	31,665	0.83	0.044 (0.040)	0.281	32,834	0.81	0.013 (0.041)	0.769
9–10 years old	20,838	2.59	0.097 (0.068)	0.168	22,080	2.47	0.047 (0.084)	0.564
11–12 years old	22,299	4.09	0.199* (0.104)	0.058	23,984	3.89	0.109 (0.130)	0.420
13–15 years old	35,638	5.37	0.182 (0.120)	0.130	36,872	5.05	0.155 (0.141)	0.277
16–18 years old	32,823	6.02	0.229 (0.161)	0.163	33,876	5.60	0.225 (0.174)	0.194
19–23 years old	45,655	6.00	0.336** (0.168)	0.057	43,044	5.63	0.312* (0.177)	0.075
24–26 years old	23,867	5.49	0.404** (0.179)	0.033	21,619	4.90	0.427** (0.182)	0.025
27–29 years old	20,769	5.08	0.322** (0.158)	0.047	18,263	4.75	0.284 (0.181)	0.129

Note: All estimates show the ITT coefficient of five-year exposure to G_1 (measured by being born in a G_1 municipality compared to in a control municipality). Cluster robust standard errors at the municipality level from regression inference are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Exact *p*-values are randomization-t values following Young (2017). Randomization c-values (not reported) are lower than the randomization-t values for all estimations.

Table 2. Long-Term Impacts of CCT (G_1) on Migration Outcomes

	Females				Males			
	N	Mean G_4	Coeff. (s.e.)	Exact *p*-value	N	Mean G_4	Coeff. (s.e.)	Exact *p*-value
6–8 years old								
Domestic migrant (=1)	31,665	0.06	0.004 (0.018)	0.860	32,834	0.07	-0.004 (0.016)	0.822
Urban migrant (=1)	31,665	0.02	-0.009 (0.008)	0.313	32,834	0.02	-0.003 (0.006)	0.685
International migrant (=1)	31,670	0.00	0.000 (0.000)	0.143	32,845	0.00	0.000* (0.000)	0.062
9–10 years old								
Domestic migrant (=1)	20,838	0.08	-0.014 (0.017)	0.437	22,080	0.08	-0.010 (0.020)	0.622
Urban migrant (=1)	20,838	0.03	-0.013 (0.012)	0.296	22,080	0.03	-0.005 (0.008)	0.559
International migrant (=1)	20,844	0.00	0.000 (0.000)	0.952	22,093	0.00	0.000 (0.000)	0.474
11–12 years old								
Domestic migrant (=1)	22,299	0.09	-0.016 (0.019)	0.392	23,984	0.08	-0.013 (0.015)	0.405
Urban migrant (=1)	22,299	0.04	-0.014 (0.011)	0.184	23,984	0.03	-0.010 (0.010)	0.316
International migrant (=1)	22,311	0.00	-0.000 (0.000)	0.571	23,996	0.00	0.000 (0.001)	0.469
13–15 years old								
Domestic migrant (=1)	35,638	0.12	-0.019 (0.019)	0.333	36,872	0.09	-0.014 (0.017)	0.418
Urban migrant (=1)	35,638	0.06	-0.016 (0.013)	0.249	36,872	0.04	-0.009 (0.010)	0.401
International migrant (=1)	35,678	0.00	-0.000 (0.001)	0.745	36,919	0.00	-0.000 (0.001)	0.826
16–18 years old								
Domestic migrant (=1)	32,823	0.19	-0.034 (0.027)	0.211	33,876	0.13	-0.019 (0.021)	0.380
Urban migrant (=1)	32,823	0.11	-0.025 (0.017)	0.157	33,876	0.07	-0.013 (0.013)	0.314
International migrant (=1)	32,912	0.00	0.000 (0.001)	0.761	34,311	0.01	0.008** (0.004)	0.031
19–23 years old								
Domestic migrant (=1)	45,655	0.26	-0.044 (0.032)	0.195	43,044	0.18	-0.040* (0.024)	0.094
Urban migrant (=1)	45,655	0.15	-0.030 (0.027)	0.279	43,044	0.10	-0.025 (0.018)	0.156
International migrant (=1)	46,144	0.01	-0.001 (0.004)	0.870	44,830	0.03	0.018 (0.012)	0.138
24–26 years old								
Domestic migrant (=1)	23,867	0.26	-0.012 (0.033)	0.702	21,619	0.21	-0.032 (0.028)	0.252
Urban migrant (=1)	23,867	0.16	-0.023	0.410	21,619	0.12	-0.022	0.356

			(0.027)				(0.022)	
International migrant (=1)	24,224	0.01	0.005	0.305	22,936	0.04	0.034**	0.013
			(0.005)				(0.014)	
27–29 years old								
Domestic migrant (=1)	20,769	0.29	-0.021	0.552	18,263	0.23	-0.045*	0.095
			(0.035)				(0.026)	
Urban migrant (=1)	20,769	0.18	-0.027	0.342	18,263	0.14	-0.018	0.446
			(0.028)				(0.023)	
International migrant (=1)	21,111	0.01	0.005	0.307	19,430	0.04	0.040***	0.004
			(0.004)				(0.014)	

Note: All estimates show the ITT coefficient of five-year exposure to G_1 (measured by being born in a G_1 municipality compared to in a control municipality). Cluster robust standard errors at the municipality level from regression inference are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Exact p-values are randomization-t values following Young (2017). Randomization c-values (not reported) are lower than the randomization-t values for all estimations.

Table 3. Long-Term Impacts of CCT (G1) on Fertility and Marriage Outcomes of Young Women

	All			Non-indigenous			Indigenous		
	Mean G4	Coeff. (s.e.)	Exact *p*-value	Mean G_4	Coeff. (s.e.)	Exact *p*-value	Mean G_4	Coeff. (s.e.)	Exact *p*-value
13–15 years old									
Ever married (=1)	0.04	0.006 (0.006)	0.283	0.05	0.004 (0.007)	0.528	0.02	0.008 (0.005)	0.116
Has a child born alive (=1)	0.01	0.005** (0.002)	0.035	0.01	0.005* (0.003)	0.114	0.01	0.003* (0.002)	0.109
16–18 years old									
Ever married (=1)	0.25	0.016 (0.020)	0.424	0.28	0.015 (0.022)	0.511	0.20	0.021 (0.021)	0.343
Has a child born alive (=1)	0.19	0.017 (0.012)	0.165	0.20	0.021* (0.011)	0.055	0.17	0.011 (0.019)	0.589
19–23 years old									
Ever married (=1)	0.55	0.001 (0.020)	0.973	0.58	-0.005 (0.022)	0.835	0.49	0.007 (0.023)	0.776
Has a child born alive (=1)	0.58	-0.004 (0.014)	0.777	0.58	0.006 (0.011)	0.584	0.59	-0.022 (0.025)	0.414
24–26 years old									
Ever married (=1)	0.70	0.007 (0.020)	0.742	0.72	0.002 (0.020)	0.940	0.66	0.011 (0.030)	0.750
Has a child born alive (=1)	0.78	0.004 (0.012)	0.714	0.77	0.019* (0.012)	0.103	0.81	-0.040** (0.018)	0.039
27–29 years old									
Ever married (=1)	0.78	-0.006 (0.019)	0.764	0.80	-0.006 (0.017)	0.729	0.74	-0.001 (0.027)	0.980
Has a child born alive (=1)	0.86	0.001 (0.009)	0.907	0.86	0.008 (0.008)	0.364	0.87	-0.017 (0.011)	0.134

Note: All estimates show the ITT coefficient of five-year exposure to G_1 (measured by being born in a G_1 municipality compared to in a control municipality). Cluster robust standard errors at the municipality level from regression inference are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Exact *p*-values are randomization-t values following Young (2017). Randomization c-values (not reported) are lower than the randomization-t values for all estimations. N=212,785. See Table 1 for number of observations by cohort, and Table A.3 in the Online Appendix for the number of observations on non-indigenous and indigenous females.

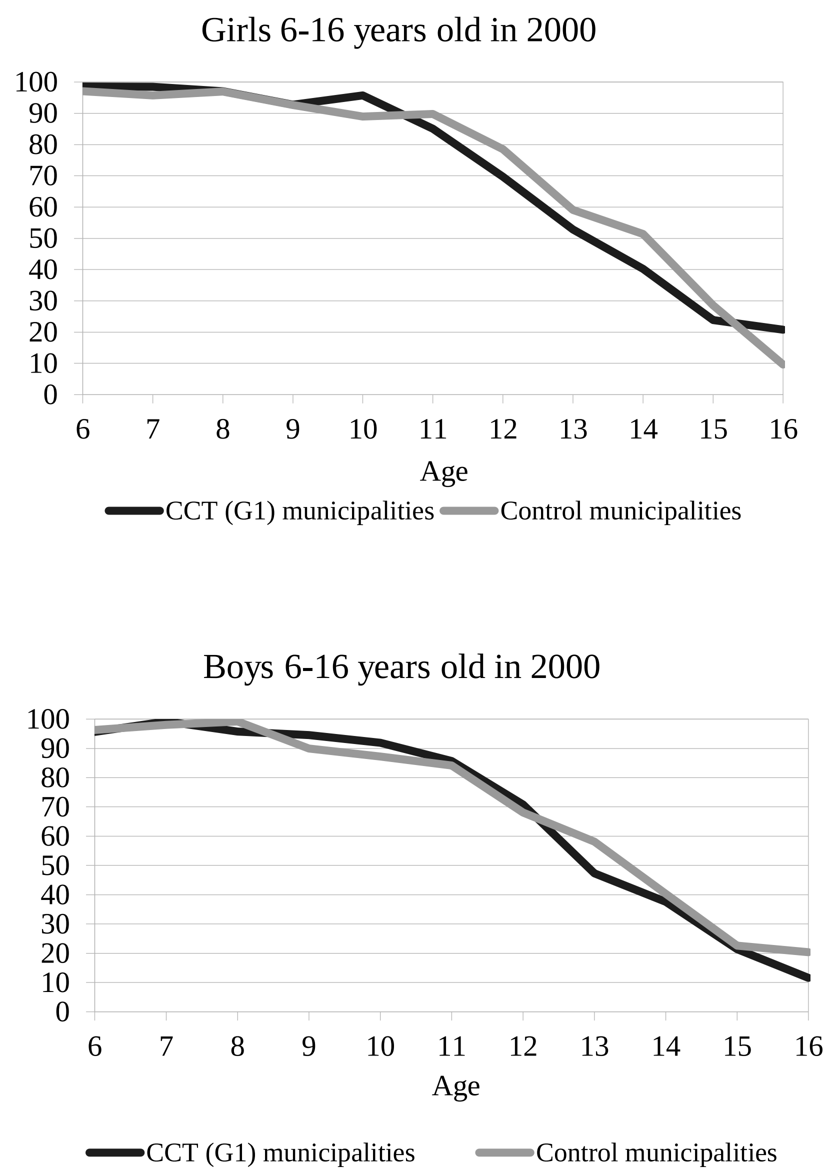
Table 4. Long-Term Impacts of CCT (G_1) on Education, Labor Participation, and Earnings for Women, Ages 19–26 Years, (EPHPM 2010–2016)

	Full sample			Living in non-indigenous villages		
	WLS		OLS	WLS		OLS
	Mean (1)	G_1 (2)	G_1 (3)	Mean (4)	G_1 (5)	G_1 (6)
Highest grade attained	6.20	0.311	0.409	6.67	0.462	0.577
		(0.273)	(0.285)		(0.412)	(0.397)
Full time student	0.07	0.015	0.035*	0.08	0.028	0.052*
		(0.021)	(0.020)		(0.030)	(0.027)
Labor market participation	0.31	-0.054	-0.058	0.38	-0.120**	-0.112***
		(0.040)	(0.038)		(0.045)	(0.042)
Number of hours worked weekly	13.10	-4.369**	-4.693***	16.36	-6.919***	-7.025***
		(1.762)	(1.662)		(2.381)	(2.282)
# of hours worked weekly (conditional on working)	46.77	-7.657**	-6.960**	48.09	-7.481*	-6.580*
		(3.120)	(2.932)		(3.862)	(3.494)
Formal worker	0.01	-0.004	-0.002	0.02	-0.007	-0.003
		(0.008)	(0.012)		(0.013)	(0.019)
Agriculture	0.08	-0.013	-0.011	0.05	-0.010	-0.006
		(0.030)	(0.025)		(0.026)	(0.020)
Non-agriculture	0.24	-0.040	-0.044	0.33	-0.105**	-0.100**
		(0.033)	(0.031)		(0.043)	(0.040)
Domestic worker	0.07	-0.041***	-0.053***	0.10	-0.080***	-0.090***
		(0.015)	(0.017)		(0.021)	(0.023)
INCOME (in 2013 lempiras)						
Monthly income	812.24	-320.472***	-346.004***	1204.59	-558.737***	-557.180***
		(116.659)	(124.549)		(178.607)	(197.186)
Hourly income	4.92	-1.386	-1.661	6.95	-2.635**	-2.849*
		(0.997)	(1.056)		(1.311)	(1.480)
Monthly income, conditional on working	2808.69	-620.508	-557.715	3430.64	-789.314	-656.874
		(386.372)	(388.109)		(500.875)	(492.384)
Hourly income, conditional on working	22.77	1.775	1.137	24.89	2.304	1.825
		(3.477)	(3.197)		(4.364)	(3.877)
INCOME APPROXIMATION with imputed values for full-time students						
Approximate monthly income°	1005.41	-134.501	-44.265	1407.28	-234.097	-108.167
		(131.562)	(138.421)		(180.515)	(194.543)
Approximate hourly income°	7.03	0.258	1.004	9.05	0.116	1.088
		(1.464)	(1.507)		(1.816)	(1.875)
Approximate monthly income, conditional on working°	2659.84	-48.770	69.434	3270.89	-44.115	63.238
		(305.098)	(306.648)		(390.598)	(381.719)
Approximate hourly income, conditional on working°	23.04	5.246	5.478*	25.26	8.101**	7.622**
		(3.321)	(3.037)		(3.656)	(3.349)

Observations	1,575	1,169

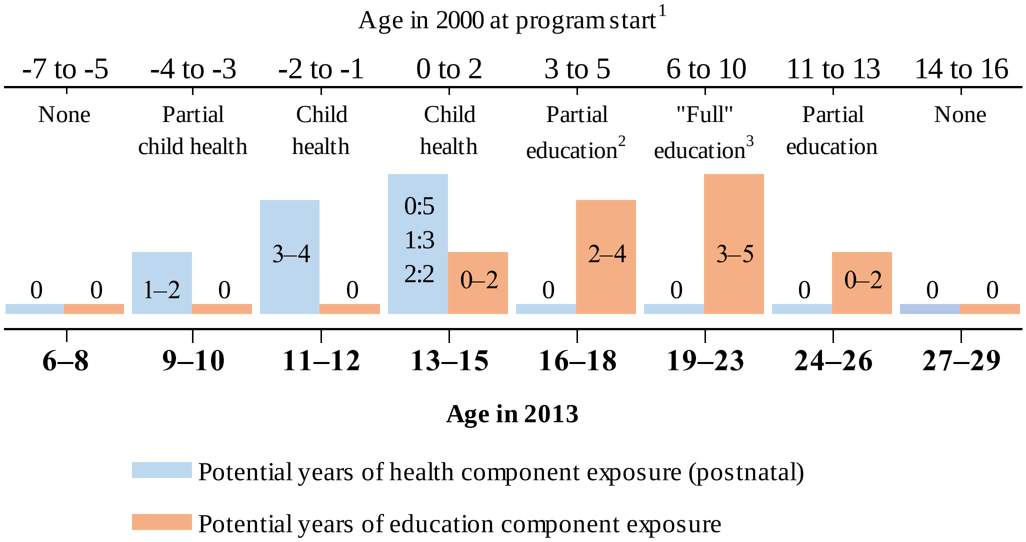
Note: For the sample of full-time students we impute monthly and hourly median earnings by gender and education level. Therefore, the conditional values are conditional on working or on being a full-time student. Results are robust to the inverse hyperbolic sine transformation of income and the use of the rank of conditional income. Cluster robust standard errors at the municipality level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 1. Age Pattern in Pre-program Enrollment Rates



Source: Baseline Data Short-Term Evaluation.

Figure 2. Age Cohorts and Exposure



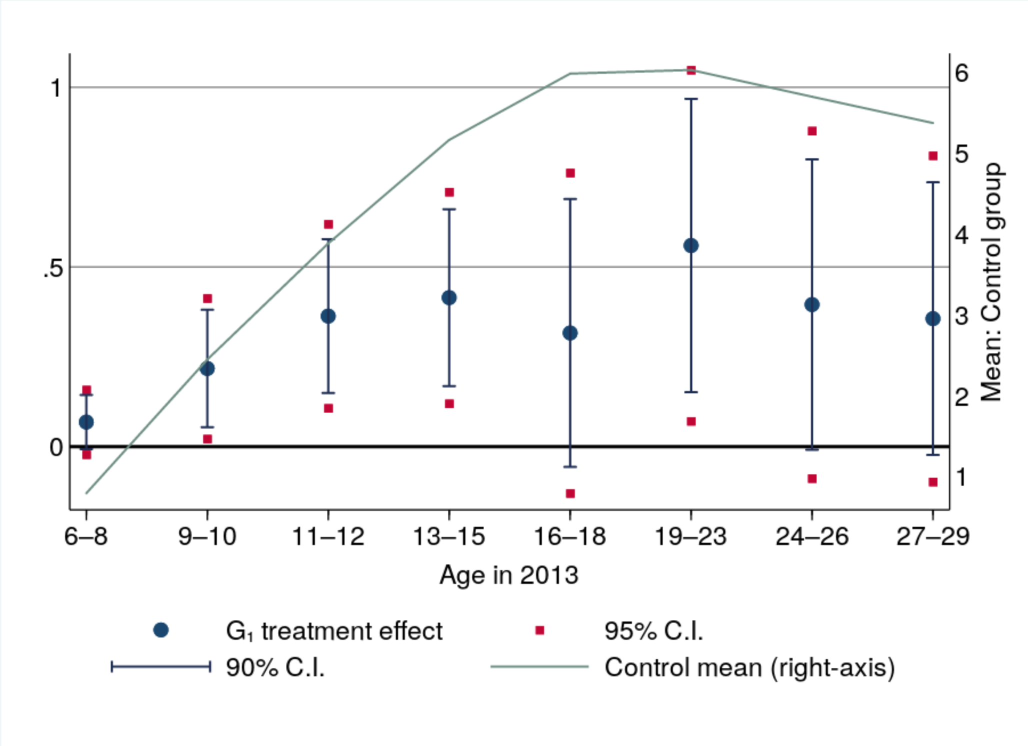
Note: Exposures are approximate as they depend on birthdates (unavailable in the census) and age when the program started in late 2000. No eligibility criteria applied to the ages 6–8 and 27–29 cohorts, but many of their households would have received transfers through eligibility of siblings.

1. Negative age indicates not yet born in 2000.

2. At the start of the program in 2000, the nutrition and health component of the CCT targeted households with children under three but in 2003 this was extended to children under five.

3. Potential years of exposure for education abstracts from the requirement of not yet having completed fourth grade.

Figure 3. Long-Term Impacts of CCT (G_1) on Grades Attained, Non-indigenous Females



Note: The figure shows the ITT effects (equation 1) of the CCT for children born in G_1 municipalities (compared to being born in the control G_4) on the outcome of interest by age cohort. Each regression includes strata fixed effects, single-year age fixed effects and a baseline proxy for the outcome calculated for 20–25 years old from the 2001 census. Standard errors are clustered at the municipality level. Point estimates are represented by dark blue dots (left vertical-axis scale) with their corresponding 90 (blue dash) and 95 (red square) percent confidence intervals. Each figure also shows the average value of the outcome in the control group (G_4 municipalities) (blue line, right vertical-axis scale). Figures show ages at the time of measurement in 2013, 13 years after the program began as illustrated in Figure 2. N=143,007.

Figure 4a. Long-Term Impacts of CCT (G_1) on Schooling Levels, Non-indigenous Females

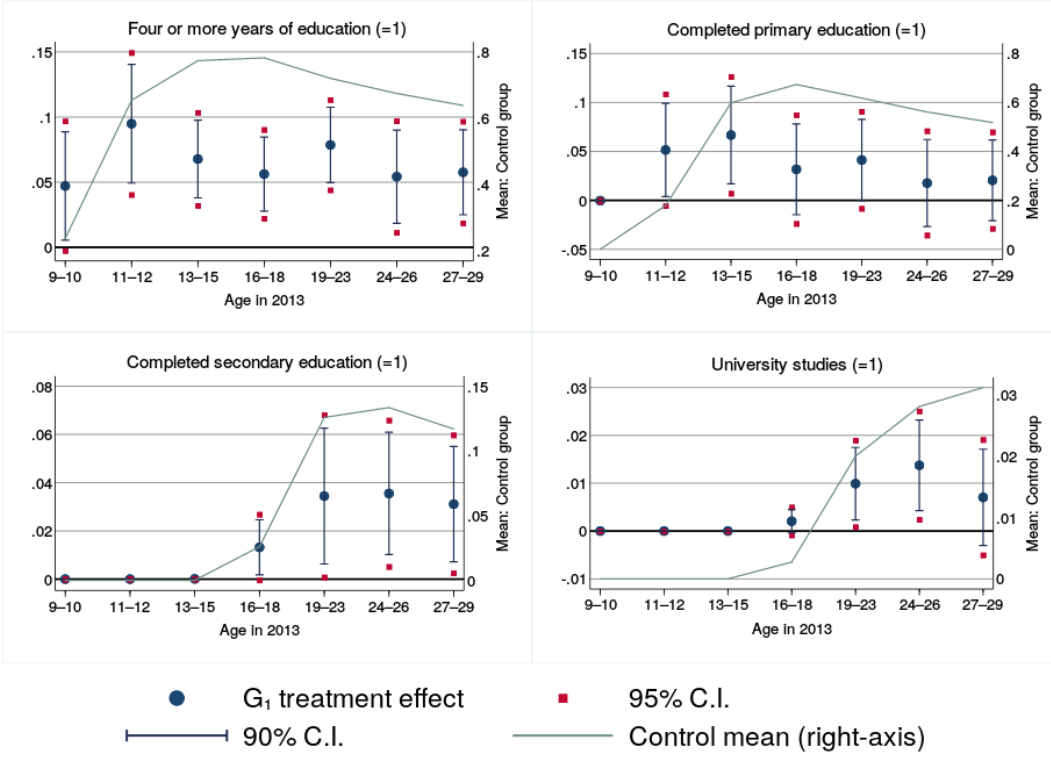
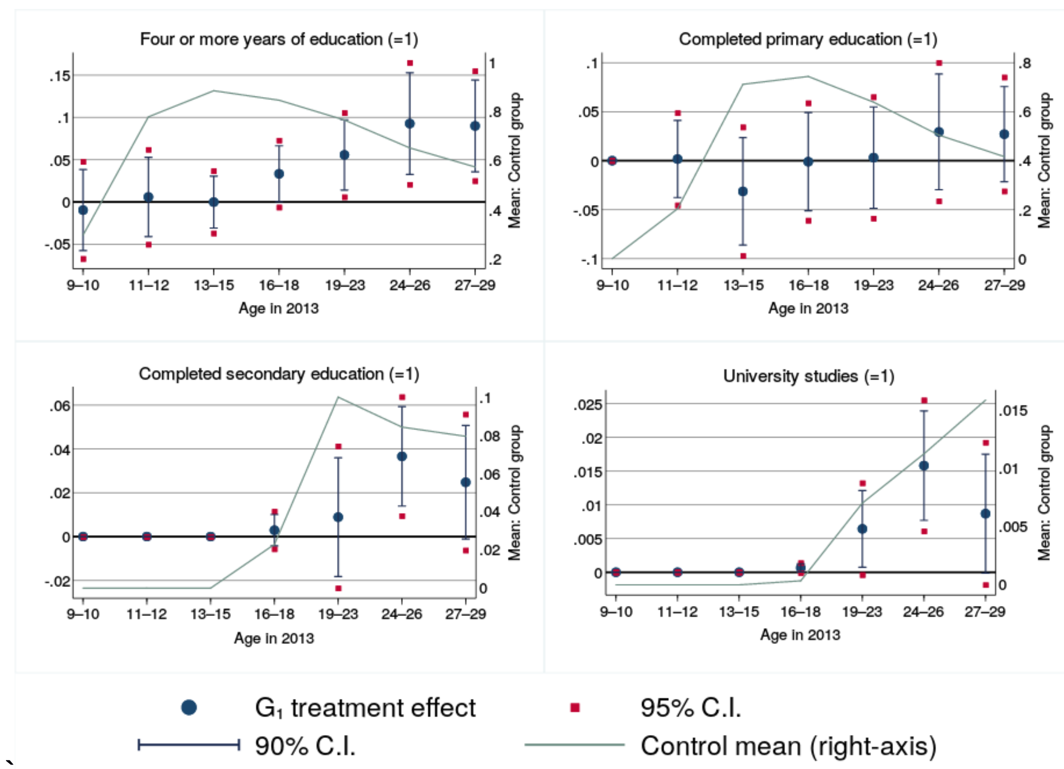


Figure 4b. Long-Term Impacts of CCT (G_1) on Schooling Levels, Indigenous Females



Note: See Figure 3. Graphs do not include 6–8 years old as they are too young to have completed any of the education levels. N= 124,899 for Figure 4a and N=76,990 for Figure 4b.

Figure 5a. Long-Term Impacts of CCT (G_1) on Current Enrollment, Non-indigenous Females

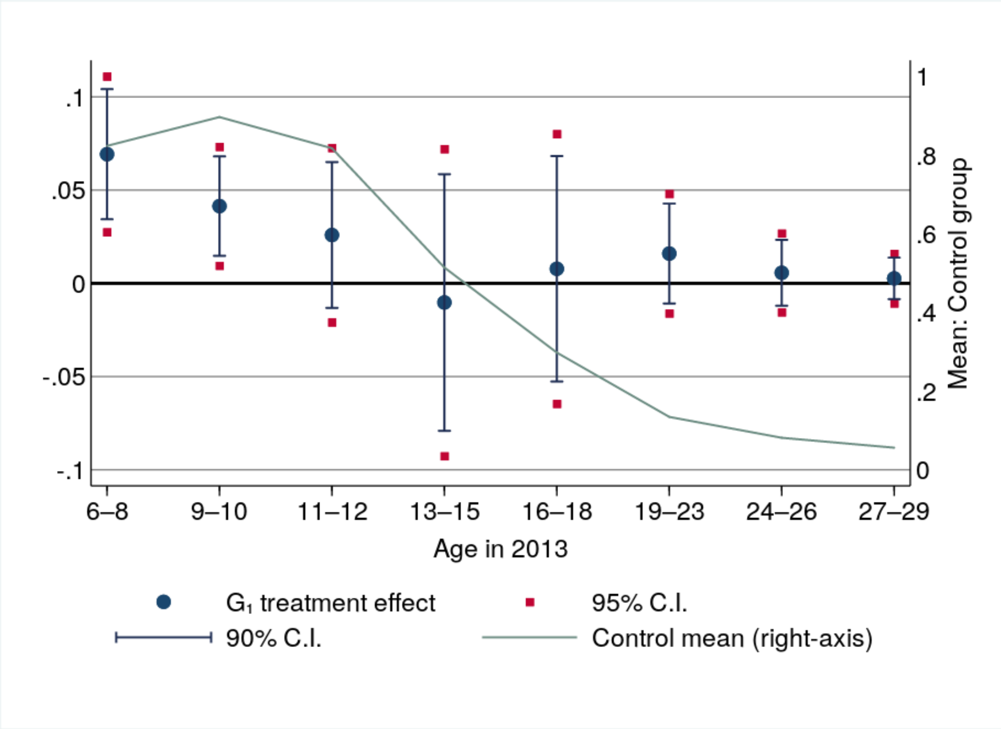
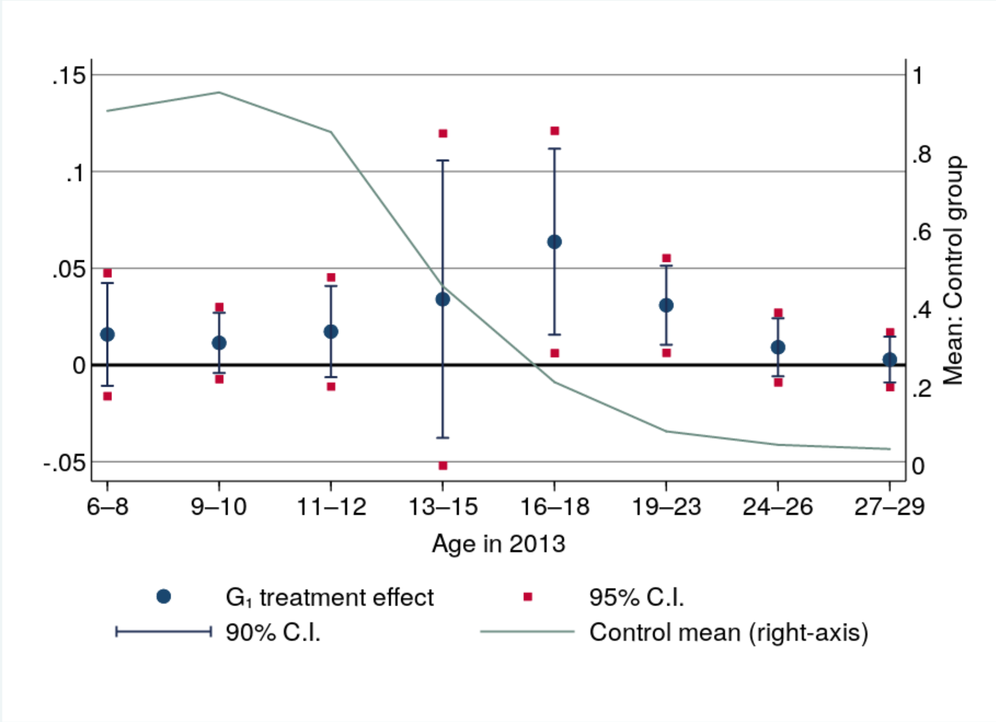
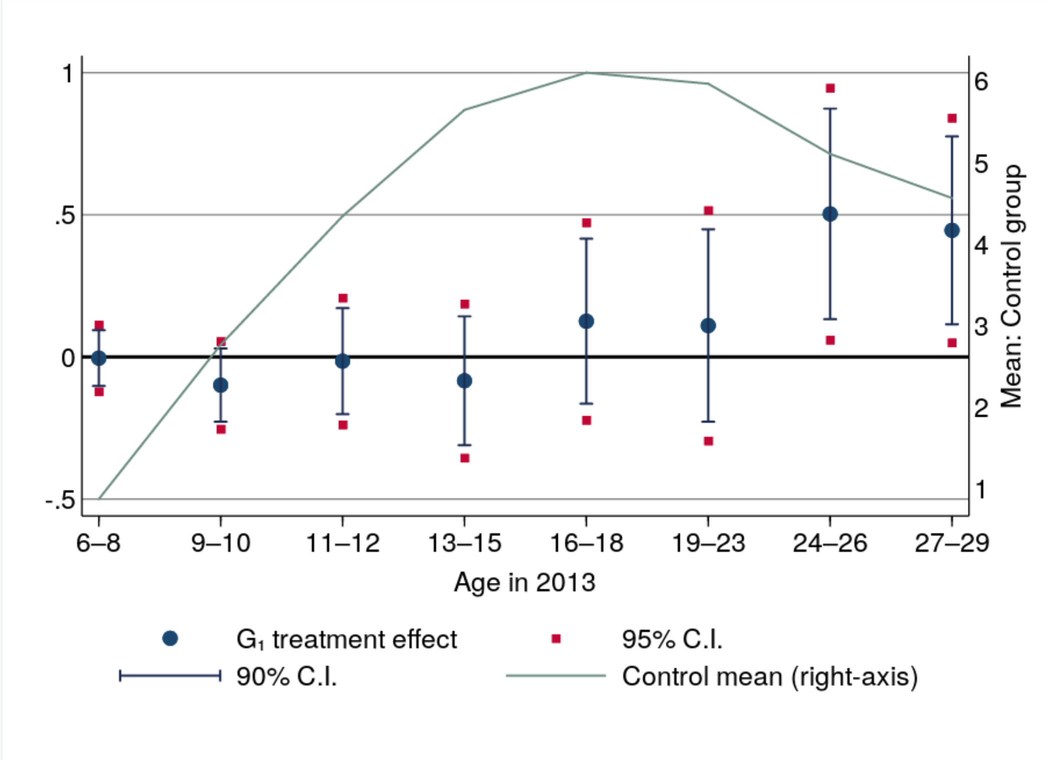


Figure 5b. Long-Term Impacts of CCT (G_1) on Current Enrollment, Indigenous Females



Note: See Figure 3. N in Figure 5a is 143,007 and in Figure 5b is 90,547.

Figure 6. Long-Term Impacts of CCT (G_1) on Grades Attained, Indigenous Females



Note: See Figure 3. N= 90,547.

Figure 7a. Long-Term Impacts of CCT (G_1) on Grades Attained, Non-indigenous Males

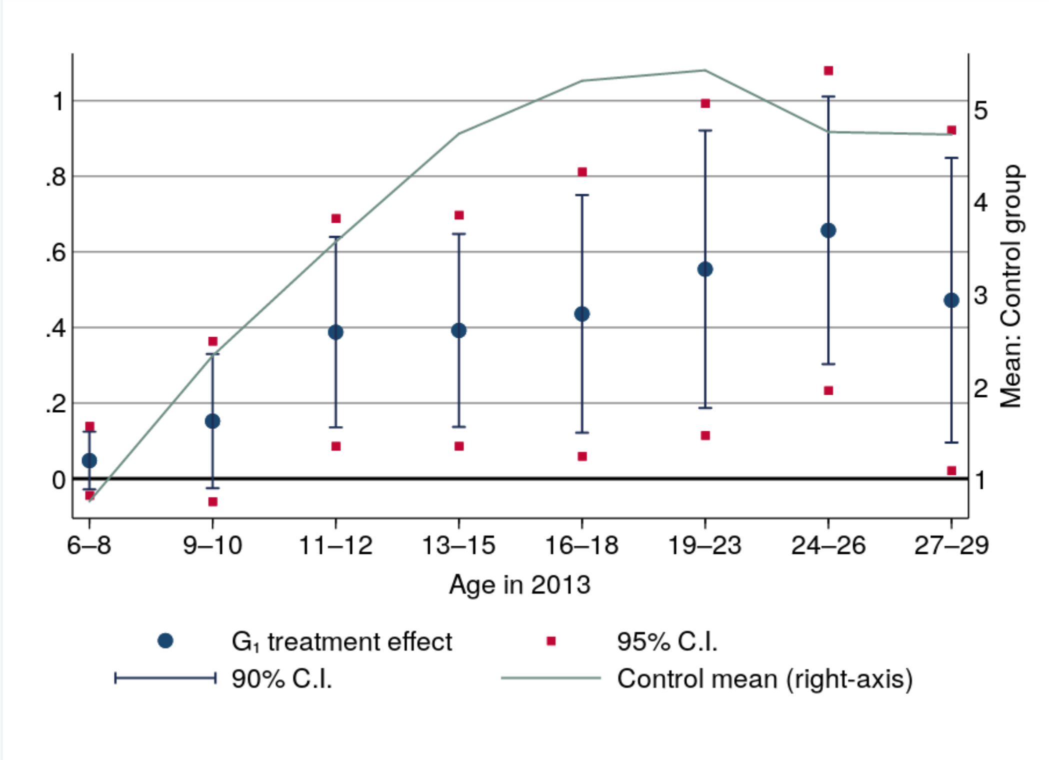
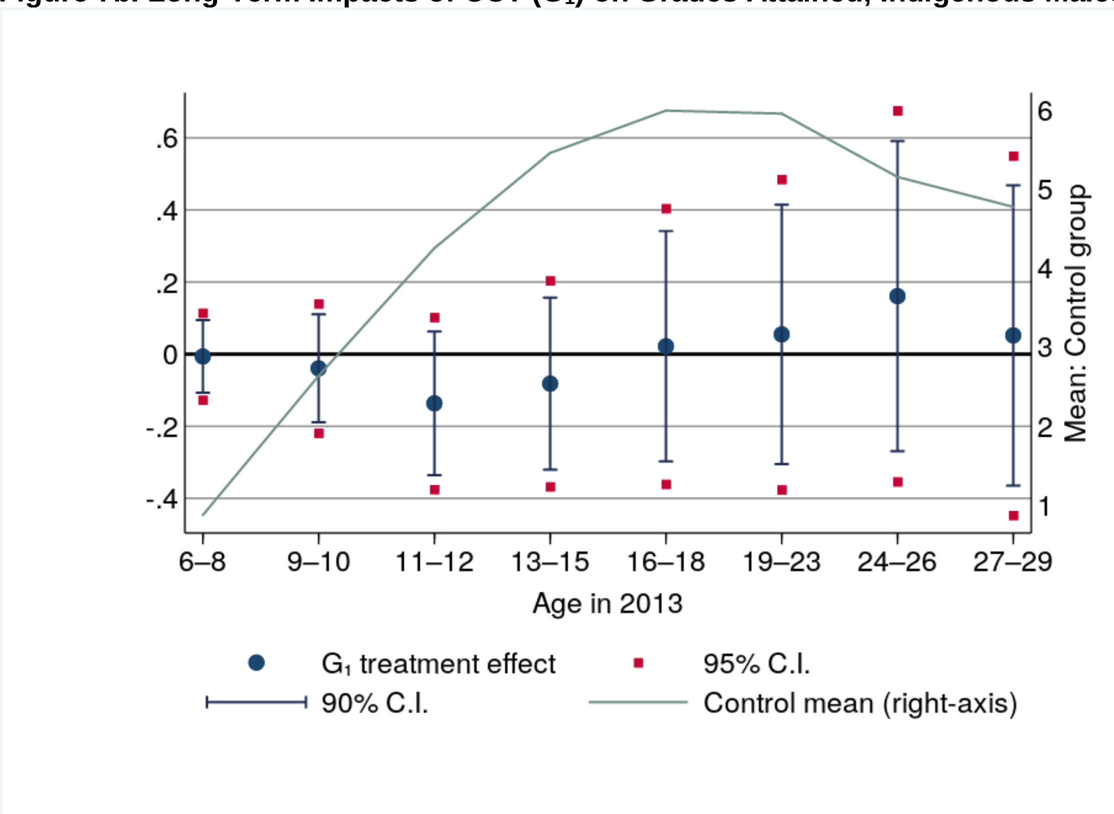


Figure 7b. Long-Term Impacts of CCT (G_1) on Grades Attained, Indigenous Males



Note: See Figure 3. N in Figure 7a is 139,093 and in N in Figure 7b is 93,479.

Figure 8a. Long-Term Impacts of CCT (G_1) Schooling Levels, Non-indigenous Males

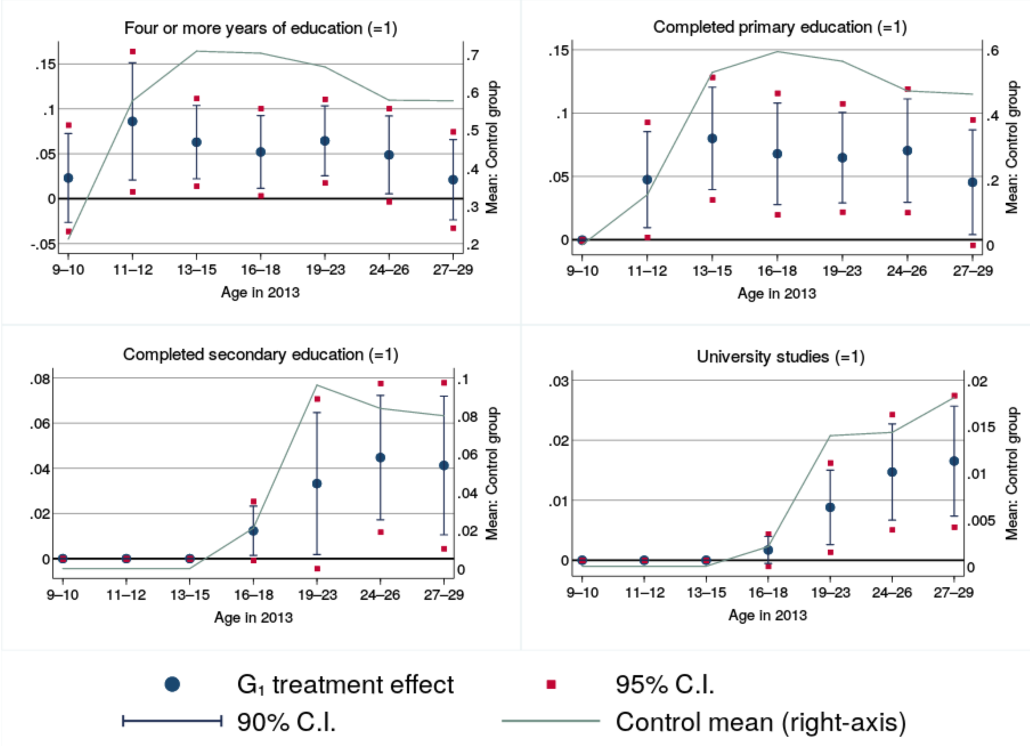
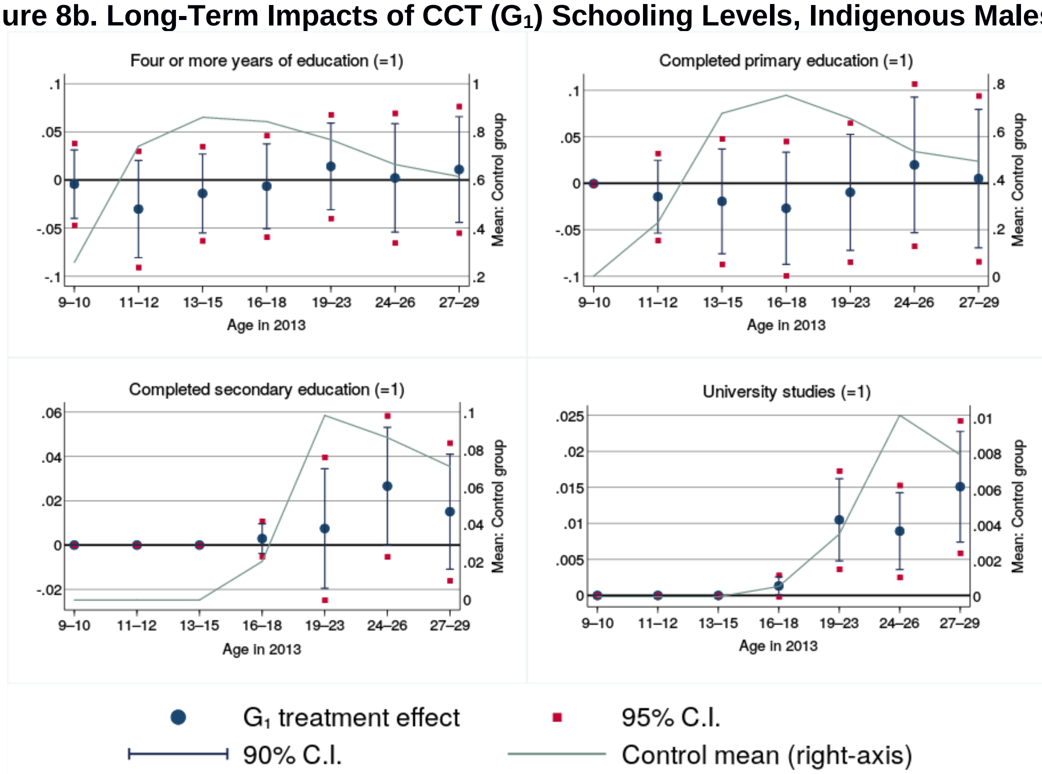


Figure 8b. Long-Term Impacts of CCT (G_1) Schooling Levels, Indigenous Males



Note: See Figure 3. Graphs do not include 6–8 years old as they are too young to have completed any of the education levels. N in Figure 8a is 120,264 and in N in Figure 8b is 79,474.

Figure 9a. Long-Term Impacts of CCT (G_1) on Current Enrollment, Non-indigenous Males

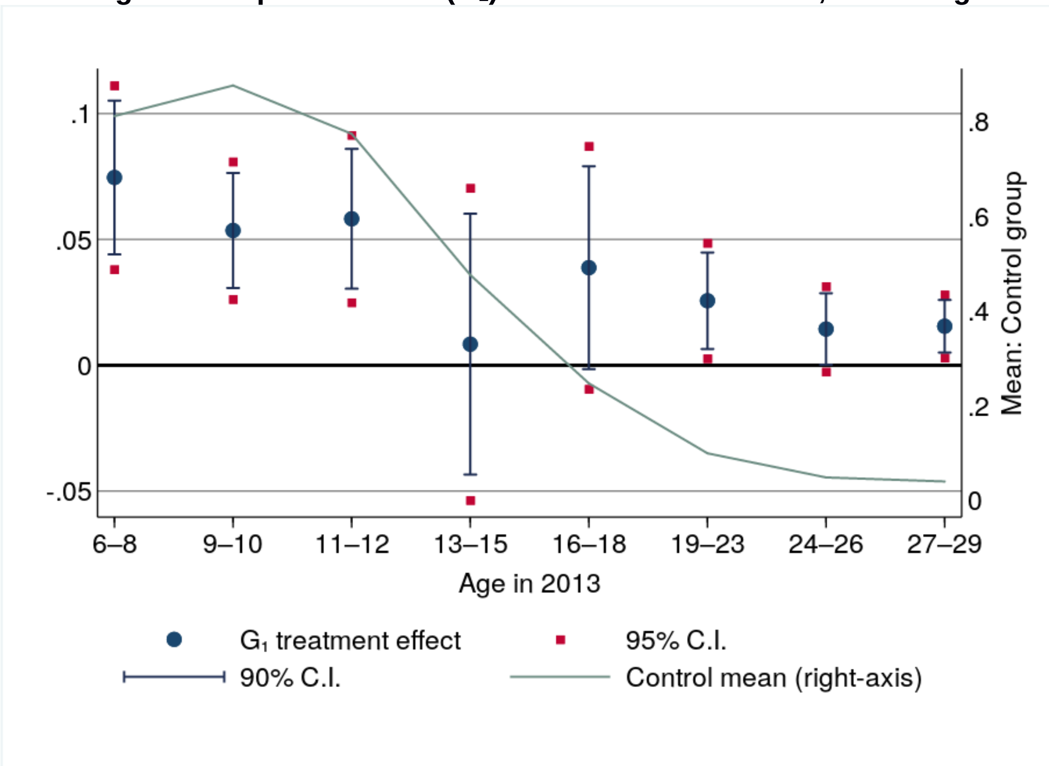
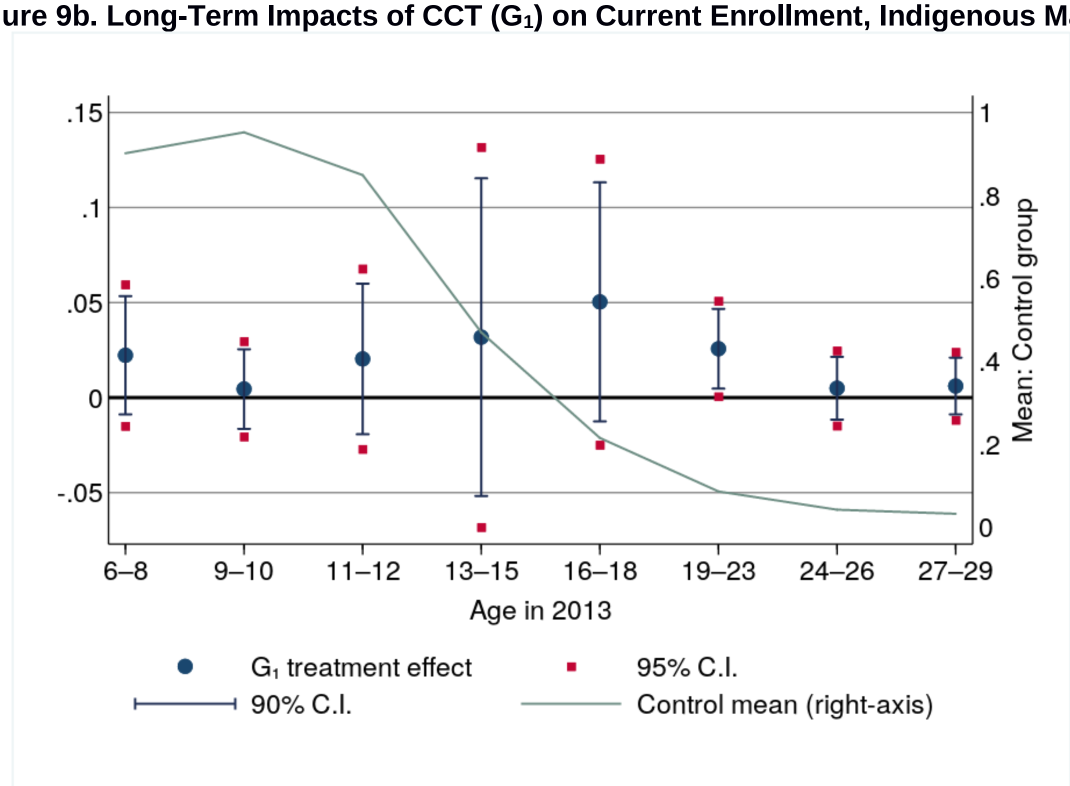
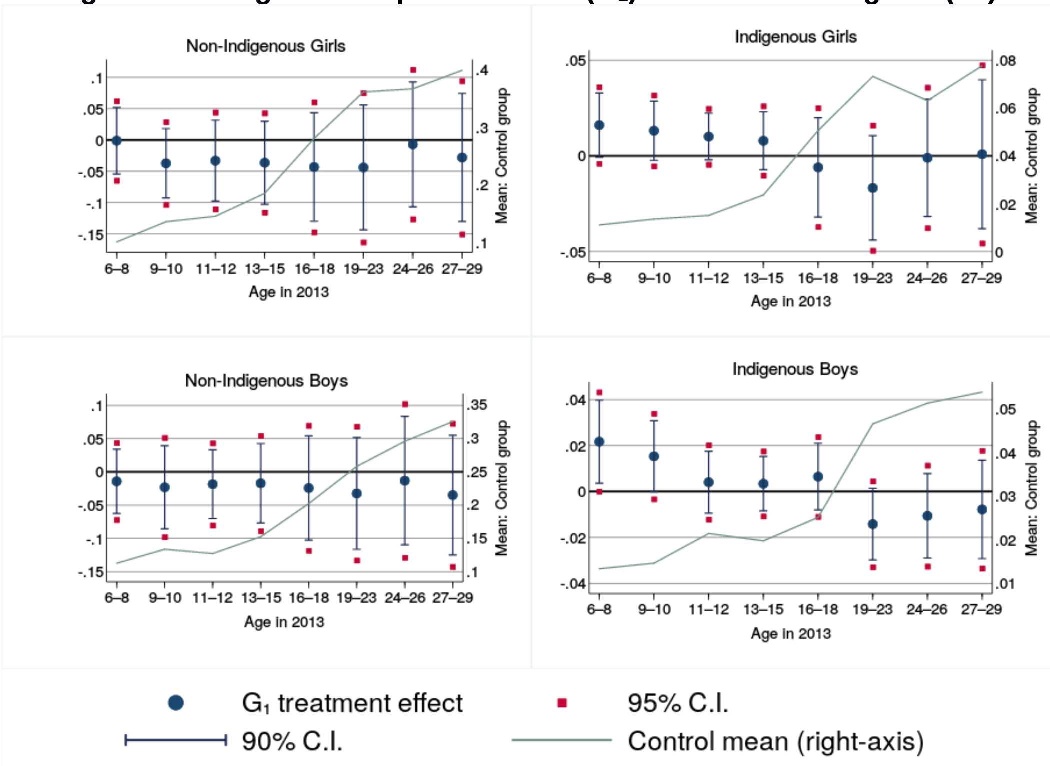


Figure 9b. Long-Term Impacts of CCT (G_1) on Current Enrollment, Indigenous Males



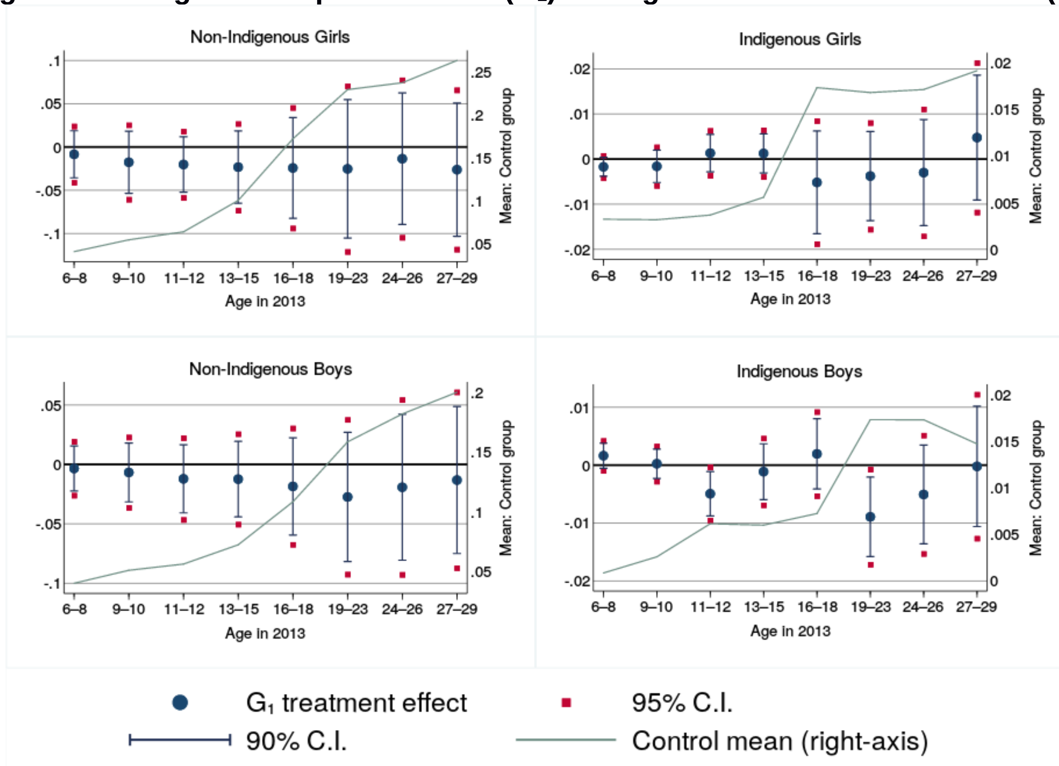
Note: See Figure 3. N in Figure 9a is 139,093 and in N in Figure 9b is 93,479.

Figure 10. Long-Term Impacts of CCT (G_1) on Domestic Migrant (=1)



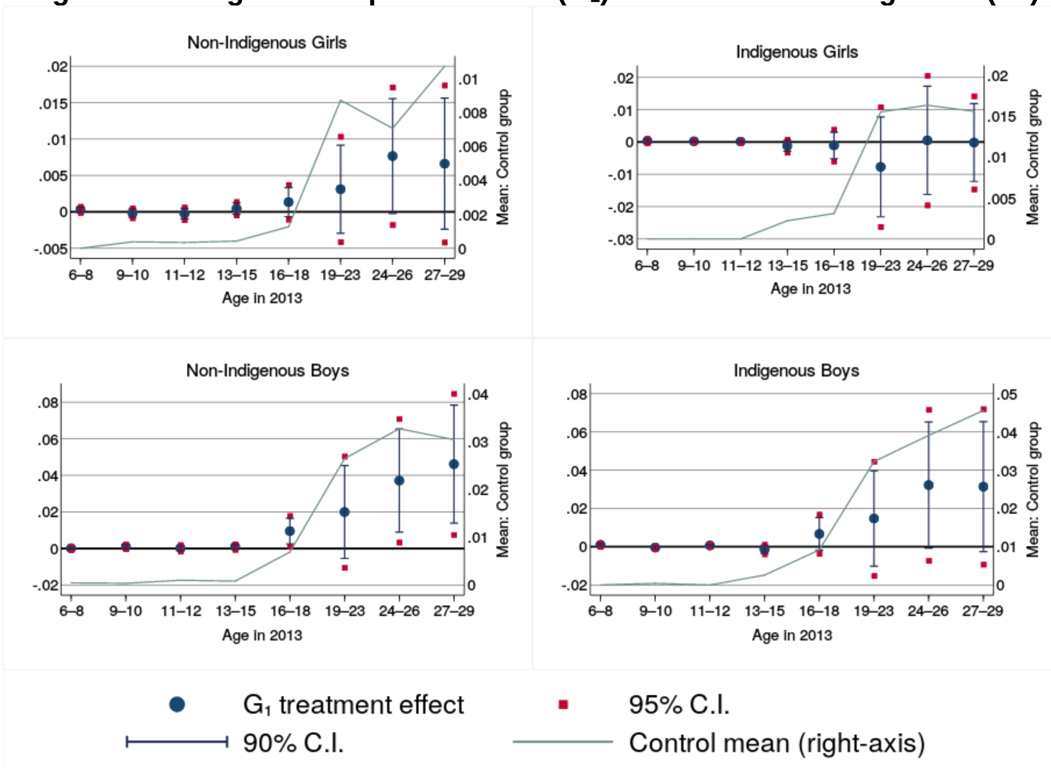
Note: See Figure 3. For the two top figures see number of observation in Figures 5a and 5b, for the two bottom figures see number of observations in Figures 7a and 7b.

Figure 11. Long-Term Impacts of CCT (G_1) on Migration to Urban Destination (=1)



Note: See Figure 3. For the two top figures see number of observation in Figures 5a and 5b, for the two bottom figures see number of observations in Figures 7a and 7b.

Figure 12. Long-Term Impacts of CCT (G_1) on International Migration (=1)



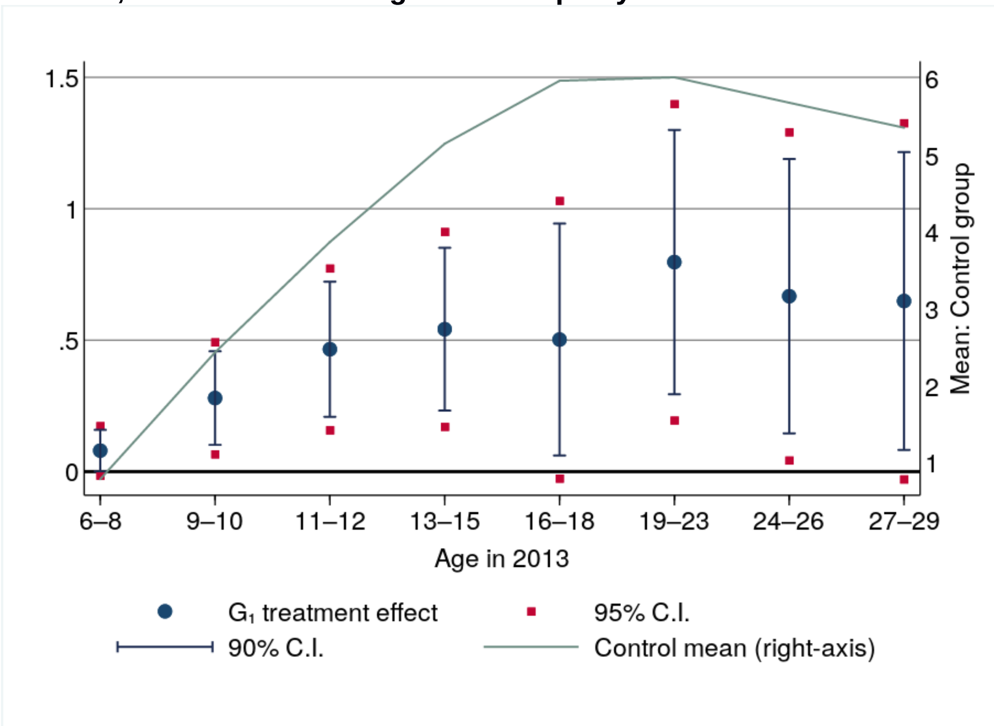
Note: See Figure 3. The number of observations in the top-left figure is 143,833; in the top-right figure is 91,060; in the bottom-left figure is 142,222 and in the bottom-right figure is 95,137.

Online Appendices for "Experimental Long-Term Effects of Early Childhood and School-Age Exposure to a Conditional Cash Transfer Program"

Teresa Molina Millán, Karen Macours, John A. Maluccio and Luis Tejerina

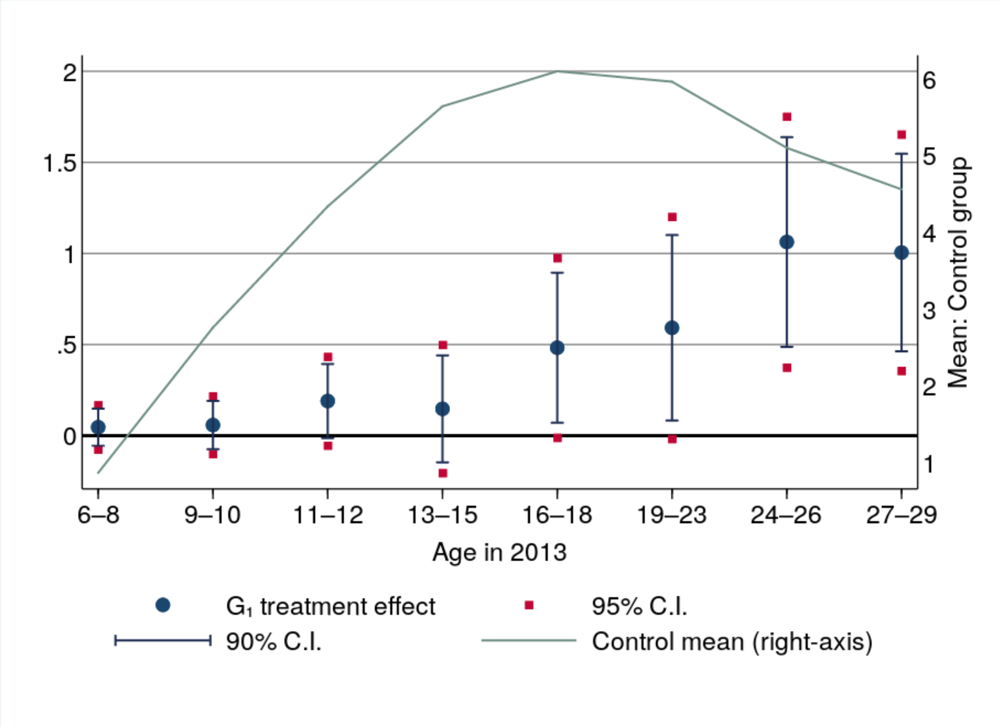
Appendix A: Additional Tables and Figures

Figure A.1. Long-Term Impacts of CCT (G_1) on Grades Attained, Non-indigenous Females, Without Controlling for Municipality Level of Education in 2001



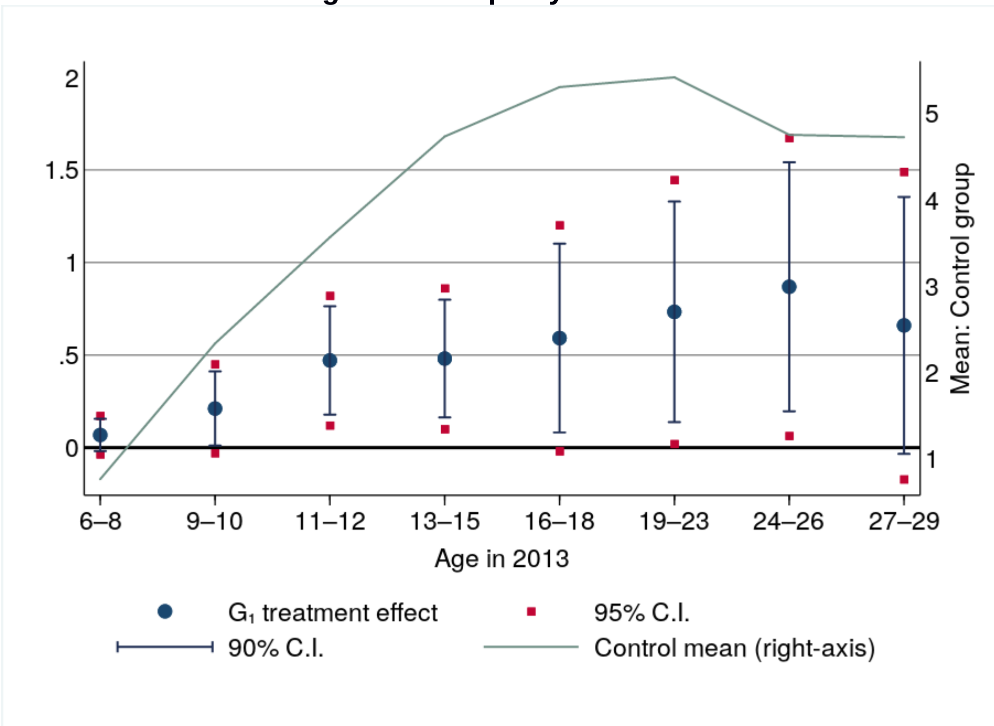
Note: The figure shows the ITT effects (equation 1) of the CCT for children born in G_1 municipalities (compared to being born in the control G_4) on the outcome of interest by age cohort. Each regression includes strata fixed effects, and single-year age fixed effects. Standard errors are clustered at the municipality level. Figures show ages at the time of measurement in 2013. N=143,007.

Figure A.2. Long-Term Impacts of CCT (G_1) on Grades Attained, Indigenous Females, Without Controlling for Municipality Level of Education in 2001



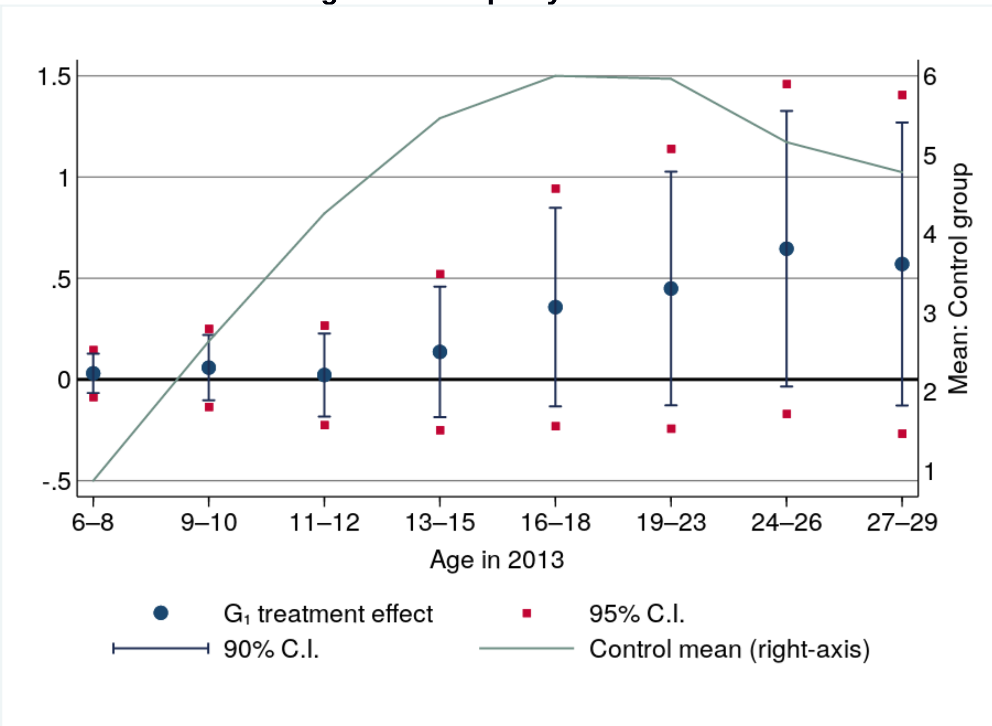
Note: See Figure A.1. N= N= 90,547.

Figure A.3. Long-Term Impacts of CCT (G_1) on Grades Attained, Non-indigenous Males, Without Controlling for Municipality Level of Education in 2001



Note: See Figure A.1. Number of observations reported in Figure 7a. N is 139,093.

Figure A.4. Long-Term Impacts of CCT (G_1) on Grades Attained, Indigenous Males, Without Controlling for Municipality Level of Education in 2001



Note: See Figure A.1. N is 93,479.

Table A.1. Long-Term Impacts of CCT (G_1) on Education Outcomes

	Females			Males		
	Mean G_4	Coeff. (s.e.)	Exact *p*-value	Mean G_4	Coeff. (s.e.)	Exact *p*-value
6–8 years old						
Grades attained	0.83	0.044	0.281	0.81	0.013	0.769
		(0.040)			(0.041)	
Currently enrolled (=1)	0.86	0.039***	0.006	0.85	0.057***	0.001
		(0.014)			(0.016)	
9–10 years old						
Grades attained	2.59	0.097	0.168	2.47	0.047	0.564
		(0.068)			(0.084)	
Currently enrolled (=1)	0.92	0.022**	0.036	0.91	0.037***	0.011
		(0.010)			(0.014)	
Four or more years of education (=1)	0.26	0.013	0.509	0.23	0.009	0.637
		(0.020)			(0.019)	
11–12 years old						
Grades attained	4.09	0.199*	0.058	3.89	0.109	0.420
		(0.104)			(0.130)	
Currently enrolled (=1)	0.83	0.016	0.316	0.81	0.048**	0.013
		(0.016)			(0.018)	
Four or more years of education (=1)	0.71	0.043	0.132	0.65	0.026	0.417
		(0.027)			(0.031)	
Completed primary education (=1)	0.19	0.039*	0.084	0.19	0.008	0.670
		(0.020)			(0.019)	
13–15 years old						
Grades attained	5.37	0.182	0.130	5.05	0.155	0.277
		(0.120)			(0.141)	
Currently enrolled (=1)	0.49	-0.007	0.848	0.47	0.031	0.334
		(0.035)			(0.033)	
Four or more years of education (=1)	0.82	0.026	0.158	0.78	0.024	0.313
		(0.018)			(0.024)	
Completed primary education (=1)	0.65	0.029	0.316	0.59	0.021	0.427
		(0.027)			(0.027)	
16–18 years old						
Grades attained	6.02	0.229	0.163	5.6	0.225	0.194
		(0.161)			(0.174)	
Currently enrolled (=1)	0.27	0.017	0.492	0.23	0.052**	0.064
		(0.025)			(0.026)	
Four or more years of education (=1)	0.81	0.038**	0.016	0.76	0.027	0.279
		(0.016)			(0.024)	
Completed primary education (=1)	0.7	0.025	0.307	0.66	0.007	0.788
		(0.025)			(0.028)	

Completed secondary education (=1)	0.02	0.007	0.174	0.02	0.008*	0.070
		(0.005)			(0.004)	
19–23 years old						
Grades attained	6.00	0.336**	0.057	5.63	0.312*	0.075
		(0.168)			(0.177)	
Currently enrolled (=1)	0.12	0.013	0.214	0.09	0.029***	0.005
		(0.010)			(0.010)	
Four or more years of education (=1)	0.74	0.057***	0.001	0.71	0.047**	0.033
		(0.017)			(0.022)	
Completed primary education (=1)	0.63	0.034	0.143	0.6	0.014	0.569
		(0.022)			(0.024)	
Completed secondary education (=1)	0.12	0.018	0.136	0.1	0.021*	0.095
		(0.012)			(0.012)	
University studies (=1)	0.02	0.007**	0.051	0.01	0.010***	0.001
		(0.003)			(0.003)	
24–26 years old						
Grades attained	5.49	0.404**	0.033	4.9	0.427**	0.025
		(0.179)			(0.182)	
Currently enrolled (=1)	0.07	0.003	0.691	0.05	0.014*	0.069
		(0.007)			(0.007)	
Four or more years of education (=1)	0.67	0.050**	0.020	0.61	0.036	0.136
		(0.021)			(0.024)	
Completed primary education (=1)	0.54	0.037	0.185	0.49	0.030	0.218
		(0.027)			(0.024)	
Completed secondary education (=1)	0.12	0.030***	0.007	0.08	0.035***	0.006
		(0.011)			(0.012)	
University studies (=1)	0.02	0.012***	0.012	0.01	0.014***	0.000
		(0.004)			(0.003)	
27–29 years old						
Grades attained	5.08	0.322**	0.047	4.75	0.284	0.129
		(0.158)			(0.181)	
Currently enrolled (=1)	0.05	0.000	0.949	0.04	0.011**	0.062
		(0.006)			(0.006)	
Four or more years of education (=1)	0.62	0.046***	0.005	0.59	0.028	0.238
		(0.016)			(0.023)	
Completed primary education (=1)	0.48	0.036	0.107	0.47	0.010	0.659
		(0.022)			(0.024)	
Completed secondary education (=1)	0.1	0.023**	0.039	0.08	0.028**	0.036
		(0.011)			(0.012)	
University studies (=1)	0.03	0.006	0.184	0.01	0.016***	0.001

	(0.005)	(0.004)

Note: All estimates show the ITT coefficient of five-year exposure to G_1 (measured by being born in a G_1 municipality compared to in a control municipality). Cluster robust standard errors at the municipality level from regression inference are reported in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. Exact p-values are randomization-t values following Young (2017). Randomization c-values (not reported) are lower than the randomization-t values for all estimations. See number of observations in Table 1.

Table A.2. Long-Term Impacts of CCT (G_1) on Marriage Outcomes of Young Men

	All			Non-indigenous			Indigenous		
	Mean G_4	**Coeff. (s.e.)**	**Exact *p*-value**	**Mean G_4**	**Coeff. (s.e.)**	**Exact *p*-value**	**Mean G_4**	**Coeff. (s.e.)**	**Exact *p*-value**
13–15 years old	0.009	-0.003**	0.015	0.012	-0.007***	0.001	0.004	0.001	0.505
		(0.001)			(0.002)			(0.001)	
16–18 years old	0.044	0.010*	0.035	0.051	0.012*	0.067	0.034	0.006	0.236
		(0.005)			(0.006)			(0.005)	
19–23 years old	0.292	0.035**	0.035	0.304	0.045**	0.015	0.274	0.008	0.634
		(0.016)			(0.018)			(0.018)	
24–26 years old	0.575	0.020	0.369	0.591	0.032	0.211	0.547	-0.004	0.889
		(0.023)			(0.025)			(0.026)	
27–29 years old	0.702	0.009	0.618	0.716	0.022	0.277	0.677	-0.020	0.337
		(0.019)			(0.019)			(0.020)	

Note: All estimates show the ITT coefficient of five-year exposure to G_1 (measured by being born in a G_1 municipality compared to in a control municipality). Cluster robust standard errors at the municipality level from regression inference are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Exact p-values are randomization-t values following Young (2017). Randomization c-values (not reported) are lower than the randomization-t values for all estimations. See number of observations in Table 1 and Table A.4.

Table A.3. Randomization Inference Tests: *p*-values, Education Outcomes, Females

	Non-indigenous			Indigenous		
	Conv. *p*-value	Exact *p*-value		Conv. *p*-value	Exact *p*-value	
		Rand-c	Rand-t		Rand-c	Rand-t
	(1)	(2)	(3)	(4)	(5)	(6)
6–8 years old		**N=18,108**			**N=13,557**	
Grades attained	0.135	0.000	0.147	0.942	0.793	0.946
Currently enrolled (=1)	0.001	0.000	0.005	0.324	0.016	0.333
9–10 years old		**N=11,932**			**N=8,906**	
Grades attained	0.030	0.001	0.036	0.203	0.002	0.213
Currently enrolled (=1)	0.012	0.001	0.014	0.224	0.029	0.220
Four or more years of education (=1)	0.063	0.001	0.066	0.739	0.413	0.723
Completed primary education (=1)	0.260	0.562	0.001	0.805	0.865	0.965
11–12 years old		**N=12,863**			**N=9,436**	
Grades attained	0.006	0.001	0.019	0.894	0.702	0.892
Currently enrolled (=1)	0.273	0.010	0.304	0.226	0.063	0.250
Four or more years of education (=1)	0.001	0.001	0.003	0.835	0.598	0.832
Completed primary education (=1)	0.076	0.001	0.094	0.943	0.865	0.947
13–15 years old		**N=21,247**			**N=14,391**	
Grades attained	0.006	0.000	0.010	0.538	0.054	0.558
Currently enrolled (=1)	0.804	0.285	0.791	0.432	0.002	0.419
Four or more years of education (=1)	0.000	0.000	0.001	0.992	0.974	0.989
Completed primary education (=1)	0.029	0.000	0.040	0.344	0.002	0.365
16–18 years old		**N=20,537**			**N=12,286**	
Grades attained	0.162	0.001	0.177	0.473	0.053	0.464
Currently enrolled (=1)	0.830	0.401	0.826	0.030	0.000	0.040
Four or more years of education (=1)	0.002	0.001	0.004	0.098	0.000	0.099
Completed primary education (=1)	0.258	0.002	0.274	0.973	0.923	0.966
Completed secondary education (=1)	0.058	0.001	0.061	0.498	0.396	0.490
19–23 years old		**N=29,111**			**N=16,544**	
Grades attained	0.025	0.000	0.022	0.588	0.120	0.623
Currently enrolled (=1)	0.323	0.007	0.342	0.014	0.001	0.029
Four or more years of education (=1)	0.000	0.000	0.000	0.029	0.001	0.051
Completed primary education (=1)	0.102	0.000	0.116	0.919	0.729	0.915
Completed secondary education (=1)	0.045	0.000	0.072	0.586	0.182	0.573
University studies (=1)	0.032	0.000	0.040	0.064	0.009	0.084
24–26 years old		**N=15,637**			**N=8,230**	

Grades attained	0.107	0.000	0.111	0.027	0.001	0.039
Currently enrolled (=1)	0.594	0.383	0.601	0.312	0.159	0.321
Four or more years of education (=1)	0.014	0.000	0.016	0.012	0.001	0.015
Completed primary education (=1)	0.510	0.102	0.522	0.409	0.034	0.425
Completed secondary education (=1)	0.022	0.000	0.029	0.009	0.001	0.011
University studies (=1)	0.018	0.000	0.023	0.002	0.001	0.006
27–29 years old		**N=13,572**			**N=7,197**	
Grades attained	0.122	0.000	0.136	0.028	0.001	0.038
Currently enrolled (=1)	0.691	0.660	0.712	0.692	0.629	0.677
Four or more years of education (=1)	0.004	0.000	0.006	0.007	0.001	0.009
Completed primary education (=1)	0.410	0.098	0.419	0.356	0.091	0.387
Completed secondary education (=1)	0.034	0.001	0.037	0.115	0.004	0.130
University studies (=1)	0.248	0.117	0.267	0.105	0.043	0.130

Note: Columns 1 and 4 report the p–values from testing the null hypothesis that β_1=0 in equation 1. Columns 2–3 and 5–6 report *p*-values from testing the sharp null hypothesis that the treatment effect of the CCT is null. Columns 2 and 4 are based on the comparison of the relative value of the squared coefficients (Rand-c). Columns 3 and 5 are based on the comparison of the Wald statistic of a two-sided test of the null hypothesis of no treatment effects (Rand-t) following Young (2017).

Table A.4. Randomization Inference Tests: *p*-values, Education Outcomes, Males

	Non-indigenous			Indigenous		
	Conv. *p*-value	Exact *p*-value		Conv. *p*-value	Exact *p*-value	
		Rand-c	Rand-t		Rand-c	Rand-t
	(1)	(2)	(3)	(4)	(5)	(6)
6–8 years old		**N=18,829**			**N=14,005**	
Grades attained	0.300	0.003	0.316	0.915	0.694	0.934
Currently enrolled (=1)	0.000	0.001	0.001	0.237	0.002	0.263
9–10 years old		**N=12,668**			**N=9,412**	
Grades attained	0.157	0.001	0.159	0.662	0.220	0.665
Currently enrolled (=1)	0.000	0.001	0.002	0.720	0.468	0.737
Four or more years of education (=1)	0.440	0.027	0.458	0.840	0.695	0.851
Completed primary education (=1)	0.443	0.673	0.666	0.205	0.226	0.000
11–12 years old		**N=13,797**			**N=10,187**	
Grades attained	0.012	0.001	0.020	0.257	0.002	0.256
Currently enrolled (=1)	0.001	0.001	0.001	0.395	0.021	0.382
Four or more years of education (=1)	0.031	0.001	0.042	0.322	0.009	0.305
Completed primary education (=1)	0.040	0.001	0.047	0.539	0.133	0.552
13–15 years old		**N=21,709**			**N=15,163**	
Grades attained	0.013	0.000	0.014	0.570	0.050	0.567
Currently enrolled (=1)	0.788	0.383	0.798	0.528	0.000	0.532
Four or more years of education (=1)	0.012	0.000	0.013	0.571	0.057	0.567
Completed primary education (=1)	0.002	0.000	0.000	0.566	0.046	0.570
16–18 years old		**N=20,265**			**N=13,611**	
Grades attained	0.024	0.001	0.028	0.909	0.714	0.900
Currently enrolled (=1)	0.113	0.001	0.108	0.186	0.001	0.199
Four or more years of education (=1)	0.036	0.001	0.041	0.808	0.427	0.822
Completed primary education (=1)	0.006	0.001	0.005	0.459	0.007	0.483
Completed secondary education (=1)	0.064	0.001	0.066	0.470	0.365	0.478
19–23 years old		**N=26,607**			**N=16,437**	
Grades attained	0.014	0.001	0.016	0.800	0.413	0.807
Currently enrolled (=1)	0.029	0.001	0.039	0.045	0.000	0.063
Four or more years of education (=1)	0.008	0.001	0.010	0.601	0.099	0.582
Completed primary education (=1)	0.004	0.001	0.007	0.795	0.313	0.792
Completed secondary education (=1)	0.082	0.001	0.084	0.646	0.239	0.651
University studies (=1)	0.021	0.002	0.020	0.003	0.000	0.003
24–26 years old		**N=13,590**			**N=8,029**	

Grades attained	0.003	0.001	0.009	0.534	0.104	0.541
Currently enrolled (=1)	0.096	0.011	0.121	0.619	0.388	0.637
Four or more years of education (=1)	0.065	0.001	0.068	0.949	0.871	0.955
Completed primary education (=1)	0.005	0.001	0.010	0.652	0.145	0.625
Completed secondary education (=1)	0.008	0.001	0.009	0.100	0.002	0.104
University studies (=1)	0.003	0.001	0.007	0.007	0.013	0.011
27–29 years old		**N=11,628**			**N=6,635**	
Grades attained	0.040	0.000	0.047	0.836	0.671	0.863
Currently enrolled (=1)	0.016	0.003	0.023	0.499	0.295	0.494
Four or more years of education (=1)	0.434	0.110	0.463	0.743	0.469	0.746
Completed primary education (=1)	0.072	0.000	0.094	0.911	0.757	0.914
Completed secondary education (=1)	0.028	0.000	0.036	0.336	0.076	0.396
University studies (=1)	0.004	0.000	0.005	0.002	0.001	0.003

Note: Columns 1 and 4 report the p–values from testing the null hypothesis that β_1=0 in equation 1. Columns 2–3 and 5–6 report *p*-values from testing the sharp null hypothesis that the treatment effect of the CCT is null. Columns 2 and 4 are based on the comparison of the relative value of the squared coefficients (Rand-c). Columns 3 and 5 are based on the comparison of the Wald statistic of a two-sided test of the null hypothesis of no treatment effects (Rand-t) following Young (2017).

Table A.5. Randomization Inference Tests: *p*-values, Migration Outcomes, Females

	Non-indigenous				Indigenous			
		Conv.	Exact *p*-value			Conv.	Exact *p*-value	
	N	*p*-value	Rand-c	Rand-t	N	*p*-value	Rand-c	Rand-t
		(1)	(2)	(3)		(4)	(5)	(6)
6–8 years old								
Domestic migrant (=1)	18,108	0.971	0.862	0.973	13,557	0.114	0.000	0.115
Urban migrant (=1)	18,108	0.604	0.030	0.611	13,557	0.176	0.127	0.175
International migrant (=1)	18,111	0.152	0.185	0.132	13,559	0.320	0.106	0.469
9–10 years old								
Domestic migrant (=1)	11,932	0.266	0.001	0.278	8,906	0.160	0.005	0.172
Urban migrant (=1)	11,932	0.417	0.003	0.426	8,906	0.453	0.296	0.492
International migrant (=1)	11,937	0.635	0.805	0.680	8,907	0.070	0.626	0.001
11–12 years old								
Domestic migrant (=1)	12,863	0.397	0.001	0.420	9,436	0.170	0.017	0.186
Urban migrant (=1)	12,863	0.296	0.001	0.318	9,436	0.596	0.465	0.604
International migrant (=1)	12,874	0.593	0.817	0.658	9,437	0.387	0.595	0.602
13–15 years old								
Domestic migrant (=1)	21,247	0.364	0.001	0.347	14,391	0.386	0.033	0.379
Urban migrant (=1)	21,247	0.358	0.001	0.364	14,391	0.633	0.494	0.636
International migrant (=1)	21,268	0.340	0.521	0.375	14,410	0.250	0.099	0.282
16–18 years old								
Domestic migrant (=1)	20,537	0.410	0.001	0.435	12,286	0.702	0.308	0.724
Urban migrant (=1)	20,537	0.490	0.000	0.492	12,286	0.451	0.094	0.483
International migrant (=1)	20,594	0.264	0.212	0.305	12,317	0.669	0.341	0.665
19–23 years old								
Domestic migrant (=1)	29,111	0.466	0.000	0.469	16,544	0.311	0.003	0.311
Urban migrant (=1)	29,111	0.600	0.001	0.628	16,544	0.526	0.175	0.521
International migrant (=1)	29,414	0.395	0.065	0.418	16,730	0.409	0.001	0.445
24–26 years old								
Domestic migrant (=1)	15,637	0.908	0.493	0.904	8,230	0.958	0.898	0.954
Urban migrant (=1)	15,637	0.768	0.136	0.783	8,230	0.672	0.419	0.687
International migrant (=1)	15,845	0.110	0.002	0.118	8,379	0.959	0.871	0.950
27–29 years old								
Domestic migrant (=1)	13,572	0.650	0.017	0.649	7,197	0.970	0.902	0.972
Urban migrant (=1)	13,572	0.572	0.009	0.586	7,197	0.568	0.255	0.553
International migrant (=1)	13,790	0.225	0.026	0.247	7,321	0.982	0.964	0.983

Note: Columns 1 and 4 report the *p*-values from testing the null hypothesis that β_1=0 in equation 1. Columns 2–3 and 5–6 report *p*-values from testing the sharp null hypothesis that the treatment effect of the CCT is null. Columns 2 and 4 are based on the comparison of the relative value of the squared coefficients (Rand-c). Columns 3 and 5 are based on the comparison of the Wald statistic of a two-sided test of the null hypothesis of no treatment effects (Rand-t) following Young (2017)

Table A.6. Randomization Inference Tests: *p*-values, Migration Outcomes, Males

	Non-indigenous				Indigenous			
		Conv.	Exact *p*-value			Conv.	Exact *p*-value	
	N	*p*-value	Rand-c	Rand-t	N	*p*-value	Rand-c	Rand-t
		(1)	(2)	(3)		(4)	(5)	(6)
6–8 years old								
Domestic migrant (=1)	18,829	0.627	0.030	0.626	14,005	0.049	0.000	0.062
Urban migrant (=1)	18,829	0.764	0.381	0.781	14,005	0.215	0.156	0.229
International migrant (=1)	18,835	0.794	0.870	0.871	14,010	0.010	0.017	0.000
9–10 years old								
Domestic migrant (=1)	12,668	0.539	0.008	0.566	9,412	0.105	0.000	0.101
Urban migrant (=1)	12,668	0.650	0.212	0.644	9,412	0.875	0.894	0.889
International migrant (=1)	12,678	0.145	0.252	0.194	9,415	0.301	0.262	0.520
11–12 years old								
Domestic migrant (=1)	13,797	0.554	0.030	0.589	10,187	0.618	0.310	0.636
Urban migrant (=1)	13,797	0.483	0.024	0.514	10,187	0.035	0.001	0.035
International migrant (=1)	13,806	0.865	0.820	0.933	10,190	0.023	0.097	0.001
13–15 years old								
Domestic migrant (=1)	21,709	0.635	0.013	0.599	15,163	0.633	0.292	0.623
Urban migrant (=1)	21,709	0.517	0.007	0.512	15,163	0.697	0.475	0.708
International migrant (=1)	21,738	0.240	0.208	0.259	15,181	0.289	0.067	0.326
16–18 years old								
Domestic migrant (=1)	20,265	0.607	0.002	0.621	13,611	0.460	0.087	0.439
Urban migrant (=1)	20,265	0.453	0.002	0.442	13,611	0.595	0.378	0.622
International migrant (=1)	20,563	0.027	0.001	0.036	13,748	0.195	0.006	0.218
19–23 years old								
Domestic migrant (=1)	26,607	0.523	0.001	0.565	16,437	0.134	0.001	0.129
Urban migrant (=1)	26,607	0.403	0.001	0.429	16,437	0.033	0.000	0.028
International migrant (=1)	27,766	0.196	0.001	0.214	17,064	0.328	0.000	0.333
24–26 years old								
Domestic migrant (=1)	13,590	0.821	0.218	0.815	8,029	0.339	0.088	0.334
Urban migrant (=1)	13,590	0.604	0.030	0.601	8,029	0.326	0.174	0.346
International migrant (=1)	14,460	0.031	0.001	0.034	8,476	0.108	0.001	0.108
27–29 years old								
Domestic migrant (=1)	11,628	0.520	0.004	0.497	6,635	0.546	0.266	0.547
Urban migrant (=1)	11,628	0.724	0.188	0.731	6,635	0.972	0.951	0.973
International migrant (=1)	12,376	0.020	0.000	0.021	7,053	0.128	0.001	0.162

Note: Columns 1 and 4 report the *p*-values from testing the null hypothesis that β_1=0 in equation 1. Columns 2–3 and 5–6 report *p*-values from testing the sharp null hypothesis that the treatment effect of the CCT is null. Columns 2 and 4 are based on the comparison of the relative value of the squared coefficients (Rand-c). Columns 3 and 5 are based on the comparison of the Wald statistic of a two-sided test of the null hypothesis of no treatment effects (Rand-t) following Young (2017)

Table A.7. Randomization Inference Tests: *p*-values, Omnibus Test for Joint Significance across Outcomes and Cohorts

	All outcomes	**Education**	**Migration**	**Marriage and fertility**
	(1)	(2)	(3)	(4)
Females	0.001	0.001	0.001	0.001
Non-indigenous	0.001	0.000	0.000	0.001
Indigenous	0.001	0.001	0.001	0.000
Males	0.001	0.000	0.000	0.000
Non-indigenous	0.001	0.000	0.001	0.000
Indigenous	0.001	0.001	0.000	0.056

Note: The *p*-values are from Young (2017) omnibus tests based on the comparison of the relative value of the squared coefficients (Rand-c). Column 1 reports *p*-values for an omnibus joint-test of overall treatment significance across all regressions and outcomes (as reported in Tables A1, 2 and 3). Column 2–4 report *p*-values for the omnibus joint-test of overall treatment significance across all regressions on education outcomes, migration outcomes and marriage and fertility outcomes, respectively.

Appendix B: Background Information on the Honduran CCT and Related Subsequent Interventions[20]

This paper focuses on the second phase of the Honduran CCT, *Programa de Asignación Familiar* (PRAF-II), implemented between 2000 and 2005 and targeted to 70 municipalities with the highest childhood stunting rates in Honduras.[21] The program had three elements: 1) a maternal and child nutrition and health component; 2) an education component; and 3) an institutional strengthening component. Different municipalities received different combinations of two benefits packages.

The first package was modeled after the standard CCT programs in the region, and similar to *PROGRESA* in Mexico and the *Red de Protección Social* in Nicaragua (Fiszbein and Schady, 2009). It was referred to as the demand-side incentive package and consisted of cash transfers in the form of exchangeable vouchers to households with pregnant women, children under three (extended to five in 2003) years old (child nutrition and maternal health component), and/or with children ages 6–12 who had not yet completed grade four of primary school at the start of the program (education component). In exchange for receiving the vouchers, beneficiaries had to fulfill a number of conditions related to the use of health and education services. These included pregnant women attending pre- and postnatal checkups, mothers taking children under three (later five) to health controls, and mothers attending education sessions on nutrition and preventive healthcare. The child and maternal nutrition and health voucher was approximately $48 per individual per year (up to a maximum of two per household) and the education voucher consisted of transfers of $38 per child per year (up to a maximum of three children per household). In 2003, an additional transfer was added for giving birth in a formal health facility equivalent to the cost of the hospital birth.

The second package consisted of support and strengthening of the supply side of health and education services through training and cash transfers to Health Services Provision Units, Parent Teacher Associations and school managers at the departmental level, aimed at improving the quality of service provision.

[20] The appendix draws from several sources documenting the program design, implementation and evaluation. These include reports to the IDB by IFPRI (IFPRI, 2000a, 200b), reports by the IDB (IDB, 2006, 2012) as well as articles about the program and its impacts (Glewwe and Olinto, 2004; Morris et al., 2004; Moore, 2008).

[21] To identify the poorest municipalities the program used municipality-level averages of height-for-age z-score (HAZ) for first-graders, obtained from the 1997 Height Census of First-Graders. From the 298 municipalities in Honduras, the 73 with the lowest HAZ were identified, three of which were excluded as they were located far away from the main cluster of municipalities and their inclusion would have entailed much higher cost and greater logistical complexity (IFPRI, 2000a; Moore et al., 2008).

Based on randomization carried out in a public event in late October 1999, one group of municipalities received only the CCT (G_1, 20 municipalities), another benefited only from the supply-side incentive package (G_3, 10 municipalities) and a third (G_2, 20 municipalities) benefited from both packages simultaneously. Finally, a group of control municipalities (G_4, 20 municipalities) never received any of the components. The different treatments were assigned randomly through a stratified municipality-level randomization with municipalities ordered by child malnutrition and divided into five equally sized blocks. Nearly 50,000 households were beneficiaries of G_1 or G_2 so that while it was a substantial regional program, it is reasonable to assume that any general equilibrium effects on broader labor market opportunities or marriage markets are limited.

The program was financed through a loan from the Inter-American Development Bank (IDB) and several IDB and evaluation program reports document the implementation of the program and confirm that the experimental design of the evaluation was respected (IDB, 2006, 2012). These reports also document that the implementation of the supply-side package was substantially delayed due to a variety of legal, institutional, logistical and financial constraints.

As the program was targeted to some of the poorest municipalities of the country, it is unsurprising that other social programs took place in the same municipalities subsequent to PRAF-II, including within the experimental control group municipalities.

Most directly related was the Integrated Social Protection Program (*Programa Integral de Protección Social* or *PIPS*) begun in 2006, which included incentives for supply and demand through cash transfers, and operated in parts of the same region. Unlike PRAF-II, *PIPS* used geographical targeting at the village (rather than the municipality level). In 48 of the 70 municipalities included in the PRAF-II evaluation, at least one village received *PIPS*; this included 9 of the 20 municipalities in the experimental control group (IDB, 2012). In 2010, *PIPS* was replaced with a new conditional cash transfer program, *Bono* 10,000, continuing to operate in the same villages but also expanding to other localities and municipalities.

In addition to these conditional transfer programs, the 70 municipalities also benefited to varying degrees from other demand and supply-side interventions related to the national Poverty Reduction Strategy, such as school grants, a “free enrollment” program and health supply support, all implemented after 2002 (IDB, 2006).

The presence of these different interventions implies that the long-term differences we estimate may reflect, to a certain extent, any substitution or complementary effects between the different program components and other later interventions. However, none of these other programs had the same targeting mechanisms as PRAF-II. *PIPS* and *Bono* 2010 were targeted at the village level with substantially more limited coverage than PRAF-II and did not benefit all households with children within the targeted villages (Benedetti et al., 2016). Moreover, differences in designs imply they did not target children in the wide age ranges considered in the present analyses. Last, and most importantly, as these other programs began after the randomized assignment of PRAF-II, their program placement is appropriately treated as endogenous and therefore not controlled for in the analyses in this paper.

Appendix C: Baseline Balance Tests

To examine baseline balance across the randomized treatment groups, we use the two most recent previous national censuses (1988 and 2001). We assess balance at the municipality level and focus on schooling outcomes, given their primary importance in our study. The 2001 census was implemented eight months after the start of the CCT (in late 2000) and nearly two years after the randomization done in October 1999. It therefore may not reflect preprogram conditions for all measures.[22] Indeed Galiani and McEwan (2013) use it to examine the short-term effects of the program on child schooling and labor. Related, Galiani, McEwan, and Quistorff (2017) compare the experimental estimates with estimates from a geographical regression discontinuity estimation approach. Their analysis suggests that at least by 2001 households close to the municipal border had not relocated from control to treatment municipalities (only 4 percent lived in a different municipality than in 1996 and the percent did not differ between treatment and control), indicating compliance with treatment assignment. The first balance tests therefore use the cohort aged 20–25 in 2001. This cohort's schooling should not have been directly affected by the program (and was likely only to have been minimally indirectly affected), and at the same time this cohort is young enough to capture recent trends and to be reflective of any secular differences in schooling in the program municipalities.

We construct municipality-level averages for educational outcomes of all individuals 20–25 years old born in the 70 municipalities (regardless of current residential location in Honduras) for each of the four subgroups: non-indigenous females, indigenous females, non-indigenous males and indigenous males (Appendix Table C.1). We fail to reject the null hypothesis that means are jointly equal across the four groups for all but one variable, university studies for indigenous men (which is rare with an average less than 0.002). That said, a few of the differences observed in 2001 are relatively large, especially for grades attained (ranging between about 0.10–0.50 grades). Therefore, in all models we control for the 2001 outcome measure of interest or a relevant proxy and test the sensitivity of all findings with randomization inference. Appendix Tables C.2a and C.2b show descriptive statistics and mean tests for an additional set of individual- and household-level characteristics using the 2001 census. In all but a few cases, we fail to reject the null hypothesis that means are jointly equal across the four groups, and differences are small.

A remaining concern with the use of the 2001 census for balancing would be any possible geographical sorting in response to the program announcement or introduction. No evidence was

[22] The baseline report by IFPRI also provides evidence of balance across arms using a household baseline survey implemented starting in July 2000 (prior to the start of the program) as well as school and health clinic surveys (IFPRI, 2001).

found of such sorting by Galiani, McEwan, and Quistorff (2017), but it may be hard to rule out entirely the possibility that the program induced certain types of households or individuals to remain in, or move into, treatment municipalities (Molina Millán and Macours, 2017). This motivates the use of the 1988 census to explore balance further. Of course, 1988 has the offsetting disadvantage of having been collected 12 years prior to the start of the program and therefore does not capture differences across treatment groups that may have arisen between 1988 and 2000. In addition, the available data is not disaggregated by indigenous status. We hence construct municipality-level averages for the same age cohort as well as two younger ones by gender (Table C.3). We fail to reject the null hypothesis that the means between G_1 and the control group are equal, for all but one variable, further confirming that the randomization led to balance on preprogram observables.

Table C.1. Descriptive Statistics Census 2001: Education Outcomes for Ages 20–25 by Gender and Ethnicity, Municipality-Level Means (N=70)

	Mean			*p*-value	Diff	*p*-value
	G_1	G_2	G_3	G_1=G_2=G_3=0	(G_1-G_4)	G_1=0
Grades attained						
Non-indigenous women	3.953	3.935	4.173	0.441	0.280	0.275
	(0.843)	(1.111)	(1.186)		(0.255)	
Non-indigenous men	3.694	3.605	3.752	0.968	0.090	0.773
	(0.986)	(0.981)	(0.943)		(0.312)	
Indigenous women	4.091	3.852	3.484	0.372	0.559	0.201
	(1.149)	(1.364)	(0.892)		(0.433)	
Indigenous men	3.563	3.570	3.248	0.764	-0.442	0.520
	(1.230)	(1.381)	(1.071)		(0.682)	
Currently enrolled (=1)						
Non-indigenous women	0.070	0.083	0.093	0.229	0.000	0.980
	(0.033)	(0.033)	(0.056)		(0.009)	
Non-indigenous men	0.060	0.067	0.055	0.685	-0.009	0.332
	(0.028)	(0.037)	(0.043)		(0.009)	
Indigenous women	0.043	0.069	0.070	0.304	-0.013	0.465
	(0.040)	(0.051)	(0.080)		(0.018)	
Indigenous men	0.036	0.074	0.049	0.115	-0.014	0.202
	(0.032)	(0.059)	(0.033)		(0.011)	
Four or more years of education (=1)						
Non-indigenous women	0.546	0.538	0.546	0.577	0.047	0.202
	(0.111)	(0.153)	(0.137)		(0.037)	
Non-indigenous men	0.515	0.502	0.515	0.936	0.027	0.575
	(0.152)	(0.146)	(0.129)		(0.047)	
Indigenous women	0.593	0.532	0.477	0.208	0.116	0.112
	(0.175)	(0.226)	(0.143)		(0.072)	
Indigenous men	0.488	0.500	0.435	0.853	-0.012	0.878
	(0.240)	(0.231)	(0.192)		(0.080)	
Completed primary education (=1)						
Non-indigenous women	0.367	0.385	0.404	0.474	0.024	0.493
	(0.113)	(0.156)	(0.138)		(0.034)	
Non-indigenous men	0.357	0.338	0.380	0.869	0.007	0.873
	(0.143)	(0.155)	(0.141)		(0.045)	
Indigenous women	0.375	0.342	0.309	0.764	0.048	0.476
	(0.191)	(0.158)	(0.150)		(0.066)	
Indigenous men	0.347	0.298	0.301	0.650	-0.033	0.686
	(0.238)	(0.185)	(0.173)		(0.081)	
Completed secondary education (=1)						
Non-indigenous women	0.037	0.032	0.048	0.457	0.008	0.345
	(0.032)	(0.029)	(0.046)		(0.009)	
Non-indigenous men	0.027	0.023	0.028	0.827	0.005	0.495
	(0.026)	(0.027)	(0.024)		(0.008)	
Indigenous women	0.024	0.026	0.023	0.839	0.006	0.475
	(0.028)	(0.039)	(0.015)		(0.009)	
Indigenous men	0.017	0.032	0.009	0.558	-0.055	0.378
	(0.024)	(0.061)	(0.011)		(0.062)	
University studies (=1)						

Non-indigenous women	0.006	0.006	0.007	0.288	0.003	0.111
	(0.007)	(0.009)	(0.010)		(0.002)	
Non-indigenous men	0.005	0.003	0.004	0.810	0.001	0.506
	(0.007)	(0.007)	(0.006)		(0.002)	
Indigenous women	0.002	0.009	0.002	0.744	0.001	0.620
	(0.005)	(0.032)	(0.004)		(0.002)	
Indigenous men	0.002	0.001	0.000	0.006***	-0.002	0.255
	(0.004)	(0.003)	(0.000)		(0.002)	

Note: Municipality level means calculated from all individuals born in municipality and 20–25 years old in 2001. SD of the means and robust S.E. for the differences in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.2a. Descriptive Statistics Census 2001, Individual Characteristics, Municipality-Level Means (N=70)

	Mean/(s.d.)			*p*-value	Diff/ (s.e.)	*p*-value
	G_1	G_2	G_3	G_1=G_2=G_3=0	(G_1-G_4)	G_1=0
Female (=1)	0.500	0.503	0.502	0.907	0.001	0.908
	(0.019)	(0.017)	(0.019)		(0.006)	
Indigenous (=1)	0.335	0.339	0.398	0.907	0.015	0.827
	(0.201)	(0.291)	(0.256)		(0.070)	
Literate (=1)	0.578	0.580	0.568	0.959	0.011	0.723
	(0.098)	(0.094)	(0.100)		(0.030)	
Currently enrolled (=1)	0.031	0.035	0.031	0.542	0.002	0.541
	(0.012)	(0.017)	(0.012)		(0.004)	
Grades attained	2.431	2.423	2.409	0.778	0.163	0.406
	(0.657)	(0.722)	(0.637)		(0.195)	
Four or more years of education (=1)	0.280	0.284	0.284	0.809	0.019	0.491
	(0.085)	(0.093)	(0.077)		(0.027)	
Completed primary education (=1)	0.173	0.173	0.179	0.891	0.010	0.629
	(0.071)	(0.083)	(0.071)		(0.020)	
Completed secondary education (=1)	0.027	0.025	0.027	0.580	0.006	0.333
	(0.024)	(0.024)	(0.021)		(0.006)	
University studies (=1)	0.004	0.005	0.004	0.723	0.001	0.369
	(0.005)	(0.006)	(0.004)		(0.001)	
Worked last week (=1)	0.353	0.404	0.370	0.089*	-0.086**	0.019
	(0.139)	(0.111)	(0.179)		(0.036)	
Hours worked last week	0.446	0.416	0.425	0.231	0.029**	0.050
	(0.054)	(0.071)	(0.044)		(0.014)	
Wage employed (=1)	0.295	0.212	0.298	0.244	-0.003	0.969
	(0.190)	(0.134)	(0.172)		(0.065)	
Self-employed (=1)	0.506	0.552	0.471	0.664	0.006	0.915
	(0.197)	(0.167)	(0.204)		(0.059)	
Agricultural sector (=1)	0.389	0.403	0.391	0.716	-0.015	0.302
	(0.047)	(0.060)	(0.079)		(0.014)	
Non-agricultural sector (=1)	0.097	0.113	0.113	0.628	-0.017	0.224
	(0.041)	(0.084)	(0.092)		(0.014)	
Born same municipality (=1)	0.830	0.754	0.808	0.335	-0.010	0.685
	(0.075)	(0.208)	(0.095)		(0.025)	

Note: Municipality-level means calculated from all individuals born in municipality and 25–75 years old in 2001. Robust standard errors reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.2b. Descriptive Statistics Census 2001, Households Characteristics, Municipality-Level Means (N=70), Continued

	Mean/(s.d.)			p-value	Diff/ (s.e.)	p-value
	G_1	G_2	G_3	G_1=G_2=G_3=0	(G_1-G_4)	G_1=0
Own a car (=1)	0.043	0.040	0.067	0.175	-0.004	0.730
	(0.031)	(0.026)	(0.042)		(0.010)	
Own a fridge (=1)	0.066	0.067	0.078	0.905	-0.010	0.525
	(0.056)	(0.061)	(0.098)		(0.016)	
Own a washing machine (=1)	0.006	0.006	0.006	0.505	-0.004	0.186
	(0.007)	(0.006)	(0.007)		(0.003)	
Own a radio (=1)	0.720	0.718	0.703	0.915	-0.007	0.807
	(0.100)	(0.092)	(0.094)		(0.030)	
Own a sewing machine (=1)	0.105	0.137	0.115	0.835	-0.013	0.585
	(0.072)	(0.138)	(0.068)		(0.023)	
Own a TV (=1)	0.097	0.098	0.115	0.946	-0.011	0.634
	(0.079)	(0.092)	(0.145)		(0.023)	
Own A.C. (=1)	0.003	0.003	0.003	0.283	-0.003*	0.082
	(0.003)	(0.003)	(0.004)		(0.001)	
Own a computer (=1)	0.004	0.004	0.003	0.642	-0.000	0.770
	(0.003)	(0.006)	(0.003)		(0.001)	
Own a telephone (=1)	0.021	0.029	0.027	0.585	0.005	0.470
	(0.025)	(0.048)	(0.055)		(0.007)	
Dwelling with a kitchen (=1)	0.818	0.795	0.862	0.039**	0.007	0.808
	(0.075)	(0.115)	(0.048)		(0.028)	
Use wood to cook (=1)	0.967	0.962	0.967	0.802	0.006	0.404
	(0.024)	(0.034)	(0.031)		(0.007)	
Toilet with sewerage (=1)	0.048	0.058	0.056	0.911	-0.006	0.651
	(0.051)	(0.052)	(0.075)		(0.014)	
No toilet (=1)	0.435	0.430	0.415	0.770	0.041	0.333
	(0.111)	(0.112)	(0.122)		(0.042)	
Own house property (=1)	0.909	0.911	0.912	0.723	0.010	0.400
	(0.039)	(0.050)	(0.050)		(0.012)	
Good wall material (=1)	0.102	0.113	0.109	0.953	-0.013	0.595
	(0.083)	(0.066)	(0.082)		(0.023)	
Water from private or public system (=1)	0.668	0.644	0.671	0.876	0.007	0.881
	(0.119)	(0.091)	(0.143)		(0.049)	
Electricity from private or public system (=1)	0.157	0.172	0.172	0.905	-0.030	0.457
	(0.129)	(0.164)	(0.200)		(0.040)	
Number of household members	6.785	6.668	6.569	0.141	0.271**	0.024
	(0.359)	(0.419)	(0.513)		(0.118)	
Number of male members	3.476	3.406	3.371	0.153	0.145**	0.025
	(0.189)	(0.209)	(0.256)		(0.063)	
Number of female members	3.309	3.262	3.198	0.267	0.126*	0.060
	(0.217)	(0.229)	(0.277)		(0.066)	

Note: Municipality-level means calculated from all individuals born in municipality and 25–75 years old in 2001. Household means are calculated using one observation per household for all households with an individual born in the municipality. Robust standard errors reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.3. Descriptive Statistics Census 1988, Education Outcomes by Age and Gender, Municipality-Level Means (N=70)

	Mean			*p*-value	Diff	*p*-value
	G_1	G_2	G_3	G_1=G_2=G_3=0	(G_1-G_4)	G_1=0
Females: 5–13 years old						
No grades attained (=1)	0.561	0.539	0.566	0.495	-0.028	0.447
	(0.116)	(0.120)	(0.167)		(0.037)	
Primary level education (=1)	0.403	0.419	0.380	0.345	0.035	0.280
	(0.094)	(0.098)	(0.143)		(0.032)	
Females: 14–19 years old						
No grades attained (=1)	0.351	0.341	0.418	0.225	-0.046	0.281
	(0.131)	(0.128)	(0.161)		(0.043)	
Primary level education (=1)	0.594	0.605	0.541	0.366	0.036	0.381
	(0.118)	(0.111)	(0.151)		(0.041)	
Secondary level education (=1)	0.050	0.044	0.023	0.437	0.016	0.357
	(0.063)	(0.062)	(0.043)		(0.018)	
Females: 20–25 years old						
No grades attained (=1)	0.436	0.425	0.462	0.457	-0.039	0.370
	(0.130)	(0.126)	(0.152)		(0.043)	
Primary level education (=1)	0.499	0.516	0.465	0.442	0.031	0.446
	(0.109)	(0.111)	(0.144)		(0.040)	
Secondary level education (=1)	0.057	0.046	0.045	0.834	0.013	0.367
	(0.053)	(0.056)	(0.050)		(0.014)	
University studies (=1)	0.001	0.001	0.002	0.789	0.000	0.694
	(0.002)	(0.002)	(0.004)		(0.001)	
Males: 5–13 years old						
No grades attained (=1)	0.578	0.552	0.599	0.584	-0.015	0.706
	(0.128)	(0.108)	(0.156)		(0.040)	
Primary level education (=1)	0.389	0.410	0.347	0.334	0.022	0.547
	(0.111)	(0.092)	(0.137)		(0.037)	
Males: 14–19 years old						
No grades attained (=1)	0.365	0.359	0.430	0.459	-0.035	0.485
	(0.166)	(0.112)	(0.152)		(0.050)	
Primary level education (=1)	0.587	0.596	0.524	0.496	0.023	0.643
	(0.161)	(0.096)	(0.152)		(0.049)	
Secondary level education (=1)	0.044	0.037	0.026	0.631	0.017	0.263
	(0.057)	(0.049)	(0.046)		(0.015)	
Males: 20–25 years old						
No grades attained (=1)	0.387	0.376	0.460	0.324	-0.037	0.483
	(0.167)	(0.107)	(0.156)		(0.053)	
Primary level education (=1)	0.548	0.556	0.471	0.368	0.028	0.573

	(0.153)	(0.089)	(0.158)		(0.049)	
Secondary level education (=1)	0.050	0.051	0.040	0.742	0.011	0.465
	(0.044)	(0.048)	(0.039)		(0.015)	
University studies (=1)	0.002	0.001	0.002	0.188	0.002*	0.079
	(0.003)	(0.001)	(0.004)		(0.001)	

Note: Municipality level means calculated from all individuals residing in municipality in 1988. Categories indicate highest level attained and therefore are not directly comparable to measures in Table 1. Standard deviations of the means and robust standard errors for the differences reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.4. Total Infant and Adolescent Population as a Ratio of Women, Ages 15–45 in 2013, Municipality-Level Means

	All		Non-indigenous		Indigenous	
	Mean G4	G1	Mean G4	G1	Mean G4	G1
0–5 years old	0.559	0.008	0.446	0.005	0.557	0.063
		(0.043)		(0.052)		(0.065)
6–8 years old	0.289	-0.001	0.233	-0.004	0.281	0.002
		(0.017)		(0.025)		(0.033)
9–10 years old	0.185	0.012	0.141	0.011	0.184	0.007
		(0.010)		(0.013)		(0.027)
11–12 years old	0.206	0.003	0.156	0.003	0.213	-0.004
		(0.010)		(0.015)		(0.024)
13–15 years old	0.319	0.011	0.254	0.012	0.313	0.034
		(0.012)		(0.020)		(0.031)

Note: Municipality level means calculated based on population born in municipality. Age groups correspond to individuals´ age in 2013. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix D: Long-Term Impact of the Combined CCT and Supply-Side Interventions (G_2)

The original randomized evaluation of PRAF-II was designed to test for differential program effects across municipalities targeted with the different benefit packages (section 2). In this appendix, we present the long-term effect of the package in the 20 municipalities in which households received cash transfers conditional on nutrition, health and education behaviors *and* schools and health centers received direct investments and support (G_2). Program monitoring documents and the short-term evaluation reports indicate that the health and schooling supply-side interventions in G_2 were implemented with considerable delays and not fully operational until about 2002. As these delays are impossible to fully characterize and may have disrupted health and education services and/or affected perceptions and expectations in G_2, we note that they complicate interpretation of impacts in G_2 as well as differential program effects between G_1 and G_2.

Table D.1 presents the ITT estimates and associated *p*-values of the long-term impact of the program on the set of educational outcomes for non-indigenous and indigenous females born in G_1 (columns 1–2 and columns 7–8, respectively) and G_2 municipalities (columns 3–4 and columns 9–10). Columns 5 and 11 present the *p*-values from a test of whether program effects are equal across treatment arms G_1 and G_2 and columns 6 and 12 the *p*-values for joint significance test of the two treatment indicators. Results are based on equation (1) and therefore for G_1 are the same as those presented in the figures (for example, the first estimated coefficient for non-indigenous girls 6–8 years old in the first row and column corresponds to the point estimate shown in Figure 3 in the paper).

With the exception of the 19–23 age cohort, non-indigenous females born in G_2 municipalities (columns 1–6) are for the most part no better off in terms of educational outcomes than those born in the control municipalities. In that cohort, estimates indicate an ITT effect of approximately half a grade (*p*-value = 0.087)—not significantly different from the ITT effect estimated for non-indigenous females born in G_1 municipalities. There is also some evidence among the youngest cohorts in G_2 of impacts on enrollment, with increases of between 3 and 4 percentage points Nevertheless, column 5 makes clear that in general program effects on non-indigenous females born in G_2 municipalities are not significantly different from program effects in G_1 municipalities. On the whole, point estimates for G_2 are smaller and less precise than for G_1 but there is almost no evidence of statistically significant different treatment effects between G_1 and G_2. When pooling the two treatment arms, results are generally in line with G_1 although with the significance of the treatment effects lower for a number of variables measuring different levels of education (column 6).

Among indigenous females (columns 7–12), point estimates on the ITT impacts of G_2 on educational outcomes are statistically significant in only two instances. When compared to indigenous girls born in G_1 municipalities, there are a number of significant differences among the oldest cohorts (19–29 years old). Nevertheless, as with non-indigenous females, pooling the two treatment arms yield similarly significant effects to those for G_1 in all but few cases.

Table D.2 presents parallel results for males. The ITT impact estimates for non-indigenous males (columns 1–6) in G_2 are again similar, if a little smaller, to those for G_1, although point estimates are less precisely estimated and the only age cohort in which estimates are consistently significant are the individuals aged 24–26 (similar to the non-indigenous females in G_2). The vast majority of pooled estimates yield similarly significant treatment effects as for G_1 alone. Finally, among indigenous males (columns 7–12) for whom there were few statistically significant impacts in G_1, results for G_2 are similar with only a handful of statistically significant impacts (some of which are negative) and few statistical differences between the two treatment arms.

Overall, the results for G_2 are hence qualitatively similar to those observed for G_1 but ITT effects in G_2 are often smaller and less precise. One potential interpretation of this finding is that the well-documented disruptions and delays during implementation of the supply side in G_2 municipalities decreased the overall effectiveness of the benefit package. That said, as few of the differences between G_1 and G_2 are significant, we are careful not put too much weight on these results.

In terms of domestic, urban and international migration, results point in the same direction (Tables D.3 and D.4) as for education. Estimated ITT effects in G_2 are similar in sign and magnitude to those born in G_1 municipalities, and indicate no statistically significant differential treatment effects on domestic and urban migration and only one negligible but significant difference between G_1 and G_2 for international migration (indigenous males 16–18). That said, impacts on international migration are, if anything, stronger in G_2 than in G_1, in particular for women. Pooled, the findings appear to confirm the results discussed in the text for G_1, that exposure to the CCT did not significantly impact domestic migration but increase international migration.

The findings in Table D.1 and D.2 may appear at odds with Ham and Michelson (2018), who employ a difference-in-difference strategy using municipal-level averages constructed from 2001 and 2013 census data, without accounting for differences in population size between municipalities or migration since the start of the program. Their estimations also control for a large number of time varying and time-invariant covariates. Their results show significant positive impacts of G_2 on municipal-level averages of education and labor market outcomes (in particular for women), but no significant impacts for G_1. The differences between G_1 and G_2 are found to be statistically significantly different from each other for some outcomes and specifications. However, the analysis in Ham and Michelson (2018) does not allow deriving conclusions regarding individuals' returns to different types of benefit packages, as it analyzes differences in average municipal-level educational and labor market outcomes, based on the population still living in those municipalities in 2013.

Table D.1. Impact of G1 versus G2 on Education Outcomes, Females

	Non-indigenous						Indigenous					
	G_1		G_2		*p*-values		G1		G_2		*p*-values	
	Coeff.	***p*-value**	**Coeff.**	***p*-value**	β_1=β_2	β_1=β_2=0	**Coeff.**	***p*-value**	**Coeff.**	***p*-value**	β_1=β_2	β_1=β_2=0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
6–8 years old												
Grades attained	0.068	0.135	0.009	0.861	0.242	0.277	-0.004	0.942	-0.036	0.541	0.496	0.736
Currently enrolled (=1)	0.069	0.001***	0.044	0.054*	0.193	0.006***	0.016	0.324	0.029	0.130	0.499	0.292
9–10 years old												
Grades attained	0.218	0.030**	0.122	0.228	0.351	0.091*	-0.099	0.203	-0.042	0.632	0.490	0.434
Currently enrolled (=1)	0.041	0.012**	0.030	0.050*	0.429	0.033**	0.011	0.224	0.011	0.386	0.986	0.472
Four or more years of education (=1)	0.047	0.063*	0.016	0.574	0.199	0.133	-0.010	0.739	-0.016	0.653	0.851	0.896
11–12 years old												
Grades attained	0.363	0.006***	0.172	0.336	0.222	0.019**	-0.015	0.894	-0.009	0.936	0.953	0.991
Currently enrolled (=1)	0.026	0.273	0.012	0.619	0.501	0.528	0.017	0.226	0.016	0.232	0.900	0.372
Four or more years of education (=1)	0.095	0.001***	0.029	0.486	0.041**	0.001***	0.006	0.835	-0.023	0.460	0.370	0.649
Completed primary education (=1)	0.051	0.076*	0.033	0.269	0.539	0.198	0.002	0.943	0.014	0.567	0.611	0.822
13–15 years old												
Grades attained	0.415	0.006***	0.212	0.260	0.266	0.024**	-0.084	0.538	-0.012	0.917	0.572	0.800
Currently enrolled (=1)	-0.010	0.804	-0.027	0.440	0.652	0.727	0.034	0.432	0.007	0.852	0.405	0.652
Four or more years of education (=1)	0.068	0.000***	0.039	0.101	0.155	0.001***	-0.000	0.992	0.001	0.938	0.930	0.995
Completed primary education (=1)	0.067	0.029**	0.036	0.236	0.335	0.089*	-0.031	0.344	-0.022	0.444	0.731	0.615
16–18 years old												
Grades attained	0.316	0.162	0.191	0.463	0.634	0.368	0.125	0.473	0.133	0.458	0.967	0.697
Currently enrolled (=1)	0.008	0.830	-0.010	0.787	0.621	0.883	0.064	0.030**	0.014	0.561	0.072*	0.080*
Four or more years of education (=1)	0.056	0.002***	0.036	0.082*	0.251	0.006***	0.033	0.098*	0.033	0.079*	0.978	0.172
Completed primary education (=1)	0.032	0.258	0.028	0.296	0.910	0.440	-0.001	0.973	0.005	0.849	0.839	0.971
Completed secondary education (=1)	0.013	0.058*	0.002	0.720	0.093*	0.150	0.003	0.498	-0.005	0.356	0.101	0.245

19–23 years old												
Grades attained	0.560	0.025**	0.476	0.087*	0.778	0.051*	0.110	0.588	0.219	0.329	0.685	0.584
Currently enrolled (=1)	0.016	0.323	0.005	0.756	0.586	0.611	0.031	0.014**	0.018	0.230	0.420	0.040**
Four or more years of education (=1)	0.079	0.000***	0.070	0.001***	0.653	0.000***	0.056	0.029**	0.045	0.036**	0.630	0.050**
Completed primary education (=1)	0.041	0.102	0.056	0.014**	0.592	0.032**	0.003	0.919	0.009	0.709	0.840	0.930
Completed secondary education (=1)	0.034	0.045**	0.021	0.216	0.477	0.119	0.009	0.586	0.008	0.661	0.951	0.839
University studies (=1)	0.010	0.032**	0.004	0.573	0.413	0.097*	0.006	0.064*	0.003	0.335	0.389	0.142
24–26 years old												
Grades attained	0.395	0.107	0.177	0.519	0.481	0.267	0.503	0.027**	0.166	0.476	0.156	0.079*
Currently enrolled (=1)	0.006	0.594	0.003	0.816	0.821	0.860	0.009	0.312	-0.008	0.315	0.034**	0.104
Four or more years of education (=1)	0.054	0.014**	0.025	0.270	0.227	0.047**	0.093	0.012**	0.042	0.180	0.093*	0.041**
Completed primary education (=1)	0.018	0.510	0.017	0.488	0.987	0.700	0.029	0.409	-0.008	0.804	0.205	0.442
Completed secondary education (=1)	0.036	0.022**	0.008	0.624	0.155	0.071*	0.037	0.009***	0.012	0.451	0.179	0.032**
University studies (=1)	0.014	0.018**	0.000	0.953	0.067*	0.051*	0.016	0.002***	0.006	0.263	0.090*	0.007***
27–29 years old												
Grades attained	0.356	0.122	0.168	0.558	0.565	0.289	0.445	0.028**	0.207	0.395	0.314	0.086*
Currently enrolled (=1)	0.003	0.691	0.001	0.901	0.892	0.921	0.003	0.692	-0.007	0.420	0.176	0.397
Four or more years of education (=1)	0.058	0.004***	0.022	0.408	0.189	0.016**	0.090	0.007***	0.055	0.126	0.320	0.026**
Completed primary education (=1)	0.020	0.410	0.025	0.337	0.890	0.515	0.027	0.356	0.006	0.834	0.480	0.622
Completed secondary education (=1)	0.031	0.034**	0.009	0.612	0.241	0.101	0.025	0.115	-0.001	0.923	0.034**	0.086*
University studies (=1)	0.007	0.248	-0.003	0.731	0.235	0.385	0.009	0.105	-0.002	0.687	0.047**	0.121

Note: Columns 1 and 7 and columns 3 and 9 report the point estimates on β_1 and β_2 in equation 1. Columns 2 and 8, and columns 4 and 10 report *p*-values from testing the null hypothesis that $\beta_1=0$ and $\beta_2=0$ in equation 1. Columns 5 and 11 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2$ in equation 1. Columns 6 and 12 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2=0$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See Online Appendix Table A.3 for number of observations.

Table D.2. Impact of G1 versus G2 on Education Outcomes, Males

	Non-indigenous						Indigenous					
	G1		G2		*p*-values		G1		G2		*p*-values	
	Coeff.	*p*-value	Coeff.	*p*-value	$\beta_1=\beta_2$	$\beta_1=\beta_2=0$	Coeff.	*p*-value	Coeff.	*p*-value	$\beta_1=\beta_2$	$\beta_1=\beta_2=0$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
6–8 years old												
Grades attained	0.048	0.300	-0.006	0.900	0.188	0.368	-0.006	0.915	-0.060	0.319	0.237	0.412
Currently enrolled (=1)	0.075	0.000***	0.053	0.023**	0.197	0.000***	0.022	0.237	0.020	0.286	0.911	0.405
9–10 years old												
Grades attained	0.152	0.157	0.047	0.682	0.268	0.299	-0.039	0.662	-0.170	0.088*	0.107	0.161
Currently enrolled (=1)	0.054	0.000***	0.037	0.021**	0.304	0.001***	0.005	0.720	0.005	0.710	0.953	0.919
Four or more years of education (=1)	0.023	0.440	0.002	0.957	0.409	0.635	-0.004	0.840	-0.053	0.062*	0.037**	0.093*
11–12 years old												
Grades attained	0.388	0.012**	0.145	0.373	0.083*	0.031**	-0.136	0.257	-0.279	0.039**	0.210	0.116
Currently enrolled (=1)	0.058	0.001***	0.048	0.013**	0.542	0.003***	0.020	0.395	0.022	0.397	0.938	0.657
Four or more years of education (=1)	0.086	0.031**	0.014	0.739	0.041**	0.039**	-0.030	0.322	-0.068	0.077*	0.277	0.206
Completed primary education (=1)	0.047	0.040**	0.009	0.732	0.068*	0.053*	-0.014	0.539	-0.052	0.042**	0.031**	0.045**
13–15 years old												
Grades attained	0.392	0.013**	0.221	0.260	0.247	0.033**	-0.082	0.570	-0.199	0.194	0.383	0.414
Currently enrolled (=1)	0.008	0.788	0.017	0.586	0.759	0.860	0.032	0.528	0.007	0.882	0.484	0.722
Four or more years of education (=1)	0.063	0.012**	0.033	0.243	0.208	0.037**	-0.014	0.571	-0.018	0.513	0.857	0.788
Completed primary education (=1)	0.080	0.002***	0.023	0.468	0.028**	0.002***	-0.019	0.566	-0.051	0.155	0.341	0.348
16–18 years old												
Grades attained	0.436	0.024**	0.351	0.190	0.698	0.076*	0.022	0.909	-0.217	0.234	0.217	0.367
Currently enrolled (=1)	0.039	0.113	0.017	0.548	0.432	0.282	0.050	0.186	0.006	0.885	0.121	0.216
Four or more years of education (=1)	0.052	0.036**	0.042	0.149	0.675	0.108	-0.006	0.808	-0.024	0.379	0.476	0.644
Completed primary education (=1)	0.068	0.006***	0.048	0.134	0.429	0.022**	-0.027	0.459	-0.050	0.152	0.491	0.356
Completed secondary education (=1)	0.012	0.064*	0.003	0.614	0.207	0.176	0.003	0.470	0.000	0.962	0.628	0.758

19–23 years old												
Grades attained	0.554	0.014**	0.436	0.142	0.649	0.047**	0.055	0.800	-0.340	0.141	0.099*	0.207
Currently enrolled (=1)	0.026	0.029**	0.013	0.394	0.469	0.088*	0.026	0.045**	-0.003	0.843	0.009***	0.015**
Four or more years of education (=1)	0.064	0.008***	0.053	0.074*	0.616	0.027**	0.014	0.601	-0.013	0.659	0.366	0.658
Completed primary education (=1)	0.065	0.004***	0.049	0.120	0.533	0.013**	-0.010	0.795	-0.049	0.172	0.274	0.338
Completed secondary education (=1)	0.033	0.082*	0.014	0.472	0.379	0.214	0.007	0.646	-0.012	0.445	0.236	0.480
University studies (=1)	0.009	0.021**	0.004	0.400	0.446	0.045**	0.010	0.003***	0.006	0.049**	0.141	0.012**
24–26 years old												
Grades attained	0.657	0.003***	0.634	0.024**	0.936	0.005***	0.161	0.534	-0.223	0.444	0.139	0.331
Currently enrolled (=1)	0.014	0.096*	0.012	0.097*	0.757	0.155	0.005	0.619	-0.000	0.988	0.466	0.751
Four or more years of education (=1)	0.049	0.065*	0.056	0.087*	0.810	0.126	0.002	0.949	-0.031	0.515	0.400	0.698
Completed primary education (=1)	0.070	0.005***	0.062	0.058*	0.765	0.017**	0.020	0.652	-0.013	0.760	0.397	0.695
Completed secondary education (=1)	0.045	0.008***	0.030	0.073*	0.453	0.016**	0.027	0.100*	0.001	0.964	0.112	0.160
University studies (=1)	0.015	0.003***	0.014	0.029**	0.885	0.001***	0.009	0.007***	0.002	0.482	0.105	0.025**
27–29 years old												
Grades attained	0.472	0.040**	0.276	0.370	0.545	0.116	0.052	0.836	-0.245	0.388	0.260	0.511
Currently enrolled (=1)	0.016	0.016**	0.007	0.283	0.328	0.045**	0.006	0.499	0.006	0.511	0.969	0.770
Four or more years of education (=1)	0.021	0.434	0.014	0.690	0.822	0.733	0.011	0.743	-0.032	0.446	0.209	0.451
Completed primary education (=1)	0.045	0.072*	0.029	0.406	0.601	0.195	0.005	0.911	-0.033	0.455	0.317	0.564
Completed secondary education (=1)	0.041	0.028**	0.015	0.414	0.271	0.079*	0.015	0.336	0.005	0.761	0.498	0.614
University studies (=1)	0.017	0.004***	0.009	0.255	0.429	0.011**	0.015	0.002***	0.012	0.015**	0.606	0.002***

Note: Columns 1 and 7 and columns 3 and 9 report the point estimates on β_1 and β_2 in equation 1. Columns 2 and 8, and columns 4 and 10 report *p*-values from testing the null hypothesis that $\beta_1=0$ and $\beta_2=0$ in equation 1. Columns 5 and 11 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2$ in equation 1. Columns 6 and 12 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2=0$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See Online Appendix Table A.4 for number of observations.

Table D.3. Impact of G1 versus G2 on Migration Outcomes, Females

	Non-indigenous						Indigenous					
	G_1		G_2		*p*-values		G1		G_2		*p*-values	
	Coeff.	***p*-value**	**Coeff.**	***p*-value**	β_1=β_2	β_1=β_2=0	**Coeff.**	***p*-value**	**Coeff.**	***p*-value**	β_1=β_2	β_1=β_2=0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
6–8 years old												
Domestic migrant (=1)	-0.001	0.971	-0.008	0.741	0.772	0.918	0.016	0.114	0.016	0.160	0.987	0.100
Urban migrant (=1)	-0.008	0.604	-0.013	0.341	0.667	0.589	-0.002	0.176	-0.002	0.254	0.934	0.325
International migrant (=1)	0.000	0.152	0.000	0.370	0.608	0.253	0.000	0.320	0.000	0.144	0.673	0.340
9–10 years old												
Domestic migrant (=1)	-0.037	0.266	-0.028	0.349	0.691	0.525	0.013	0.160	0.014	0.193	0.938	0.205
Urban migrant (=1)	-0.018	0.417	-0.011	0.577	0.579	0.698	-0.002	0.453	-0.001	0.589	0.797	0.743
International migrant (=1)	-0.000	0.635	0.000	0.458	0.295	0.566	0.000	0.070*	-0.000	0.839	0.102	0.187
11–12 years old												
Domestic migrant (=1)	-0.033	0.397	-0.034	0.269	0.964	0.539	0.010	0.170	0.015	0.134	0.668	0.160
Urban migrant (=1)	-0.020	0.296	-0.016	0.353	0.691	0.574	0.001	0.596	0.001	0.741	0.896	0.853
International migrant (=1)	-0.000	0.593	0.002	0.115	0.078*	0.208	0.000	0.387	0.000	0.307	0.330	0.590
13–15 years old												
Domestic migrant (=1)	-0.036	0.364	-0.025	0.445	0.714	0.649	0.008	0.386	0.013	0.213	0.650	0.402
Urban migrant (=1)	-0.023	0.358	-0.011	0.663	0.478	0.609	0.001	0.633	0.004	0.256	0.475	0.506
International migrant (=1)	0.000	0.340	0.001	0.013**	0.137	0.043**	-0.001	0.250	-0.002	0.039**	0.433	0.116
16–18 years old												
Domestic migrant (=1)	-0.043	0.410	-0.037	0.340	0.886	0.605	-0.006	0.702	0.023	0.296	0.169	0.385
Urban migrant (=1)	-0.024	0.490	-0.019	0.550	0.873	0.768	-0.005	0.451	0.002	0.838	0.412	0.650
International migrant (=1)	0.001	0.264	0.003	0.004***	0.152	0.016**	-0.001	0.669	-0.002	0.412	0.404	0.583
19–23 years old												

Domestic migrant (=1)	-0.044	0.466	-0.045	0.340	0.986	0.624	-0.017	0.311	0.021	0.408	0.127	0.262
Urban migrant (=1)	-0.025	0.600	-0.037	0.379	0.737	0.672	-0.004	0.526	0.003	0.678	0.393	0.666
International migrant (=1)	0.003	0.395	0.004	0.118	0.788	0.275	-0.008	0.409	-0.005	0.581	0.557	0.666
24–26 years old												
Domestic migrant (=1)	-0.007	0.908	-0.023	0.602	0.754	0.855	-0.001	0.958	0.036	0.209	0.170	0.370
Urban migrant (=1)	-0.014	0.768	-0.032	0.417	0.612	0.684	-0.003	0.672	0.003	0.736	0.446	0.734
International migrant (=1)	0.008	0.110	0.011	0.002***	0.516	0.003***	0.001	0.959	0.002	0.857	0.805	0.963
27–29 years old												
Domestic migrant (=1)	-0.028	0.650	-0.049	0.285	0.695	0.558	0.001	0.970	0.033	0.260	0.282	0.483
Urban migrant (=1)	-0.026	0.572	-0.050	0.226	0.523	0.455	0.005	0.568	0.006	0.473	0.841	0.752
International migrant (=1)	0.007	0.225	0.008	0.043**	0.762	0.096*	-0.000	0.982	0.002	0.778	0.618	0.872

Note: Columns 1 and 7 and columns 3 and 9 report the point estimates on β_1 and β_2 in equation 1. Columns 2 and 8, and columns 4 and 10 report *p*-values from testing the null hypothesis that β_1=0 and β_2=0 in equation 1. Columns 5 and 11 report *p*-values from testing the null hypothesis that $\beta_1 = \beta_2$ in equation 1. Columns 6 and 12 report *p*-values from testing the null hypothesis that $\beta_1 = \beta_2 = 0$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See Online Appendix Table A.5 for number of observations.

Table D.4. Impact of G_1 versus G_2 on Migration Outcomes, Males

	Non-indigenous						Indigenous					
	G_1		G_2		*p*-values		G_1		G_2		*p*-values	
	Coeff.	*p*-value	Coeff.	*p*-value	$\beta_1=\beta_2$	$\beta_1=\beta_2=0$	Coeff.	*p*-value	Coeff.	*p*-value	$\beta_1=\beta_2$	$\beta_1=\beta_2=0$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
6–8 years old												
Domestic migrant (=1)	-0.014	0.627	-0.034	0.216	0.416	0.426	0.022	0.049**	0.018	0.101	0.770	0.053*
Urban migrant (=1)	-0.003	0.764	-0.005	0.583	0.809	0.848	0.002	0.215	0.001	0.574	0.655	0.435
International Migrant (=1)	0.000	0.794	-0.000	0.468	0.326	0.580	0.001	0.010**	0.000	0.121	0.059*	0.036**
9–10 years old												
Domestic migrant (=1)	-0.023	0.539	-0.056	0.108	0.206	0.184	0.015	0.105	0.013	0.161	0.867	0.152
Urban migrant (=1)	-0.007	0.650	-0.013	0.300	0.567	0.534	0.000	0.875	0.000	0.898	0.982	0.985
International migrant (=1)	0.001	0.145	0.001	0.346	0.641	0.275	-0.000	0.301	-0.000	0.716	0.505	0.576
11–12 years old												
Domestic migrant (=1)	-0.018	0.554	-0.032	0.307	0.613	0.590	0.004	0.618	0.006	0.563	0.854	0.799
Urban migrant (=1)	-0.012	0.483	-0.010	0.523	0.892	0.766	-0.005	0.035**	-0.003	0.294	0.357	0.090*
International migrant (=1)	0.000	0.865	-0.001	0.426	0.220	0.297	0.001	0.023**	-0.000	0.189	0.031**	0.070*
13–15 years old												
Domestic migrant (=1)	-0.017	0.635	-0.031	0.349	0.588	0.614	0.003	0.633	0.016	0.164	0.272	0.376
Urban migrant (=1)	-0.012	0.517	-0.013	0.417	0.973	0.716	-0.001	0.697	0.000	0.929	0.652	0.862
International migrant (=1)	0.001	0.240	0.001	0.235	0.792	0.401	-0.001	0.289	-0.002	0.125	0.409	0.269
16–18 years old												
Domestic migrant (=1)	-0.024	0.607	-0.040	0.322	0.691	0.605	0.006	0.460	0.016	0.259	0.522	0.473
Urban migrant (=1)	-0.019	0.453	-0.019	0.386	0.996	0.668	0.002	0.595	0.006	0.222	0.390	0.470
International migrant (=1)	0.010	0.027**	0.011	0.003***	0.750	0.008***	0.007	0.195	-0.000	0.970	0.051*	0.145

19–23 years old												
Domestic migrant (=1)	-0.032	0.523	-0.050	0.274	0.673	0.547	-0.014	0.134	0.010	0.533	0.116	0.154
Urban migrant (=1)	-0.027	0.403	-0.022	0.472	0.830	0.685	-0.009	0.033**	-0.002	0.715	0.230	0.072*
International migrant (=1)	0.020	0.196	0.023	0.032**	0.855	0.077*	0.015	0.328	0.003	0.847	0.214	0.423
24–26 years old												
Domestic migrant (=1)	-0.013	0.821	-0.043	0.399	0.539	0.655	-0.011	0.339	0.016	0.397	0.151	0.296
Urban migrant (=1)	-0.019	0.604	-0.020	0.525	0.989	0.806	-0.005	0.326	-0.000	0.944	0.547	0.606
International migrant (=1)	0.037	0.031**	0.042	0.000***	0.792	0.001***	0.032	0.108	0.017	0.441	0.391	0.246
27–29 years old												
Domestic migrant (=1)	-0.035	0.520	-0.068	0.143	0.495	0.333	-0.008	0.546	0.010	0.650	0.384	0.625
Urban migrant (=1)	-0.013	0.724	-0.036	0.249	0.495	0.485	-0.000	0.972	0.004	0.571	0.569	0.822
International migrant (=1)	0.046	0.020**	0.044	0.000***	0.910	0.000***	0.031	0.128	0.012	0.553	0.201	0.231

Note: Columns 1 and 7 and columns 3 and 9 report the point estimates on β_1 and β_2 in equation 1. Columns 2 and 8, and columns 4 and 10 report *p*-values from testing the null hypothesis that $\beta_1=0$ and $\beta_2=0$ in equation 1. Columns 5 and 11 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2$ in equation 1. Columns 6 and 12 report *p*-values from testing the null hypothesis that $\beta_1=\beta_2=0$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See Online Appendix Table A.6 for number of observations.

Table D.5. Impact of G_1 versus G_2 on Fertility Outcomes, Females

	Non-indigenous						Indigenous					
	G1		G2		*p*-values		G1		G2		*p*-values	
	Coeff.	*p*-value	Coeff.	*p*-value	β_1=β_2	β_1=β_2=0	Coeff.	*p*-value	Coeff.	*p*-value	β_1=β_2	β_1=β_2=0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
13–15 years old												
Ever married (=1)	0.004	0.532	0.004	0.553	0.981	0.737	0.008	0.101	0.009	0.200	0.883	0.196
Has a child born alive (=1)	0.005	0.098*	0.004	0.240	0.674	0.239	0.003	0.096*	0.006	0.053*	0.288	0.097*
16–18 years old												
Ever married (=1)	0.015	0.502	-0.010	0.708	0.361	0.630	0.021	0.318	0.029	0.194	0.710	0.413
Has a child born alive (=1)	0.021	0.051*	0.010	0.441	0.466	0.143	0.011	0.573	0.041	0.014**	0.063*	0.027**
19–23 years old												
Ever married (=1)	-0.005	0.828	-0.010	0.719	0.851	0.934	0.007	0.761	0.020	0.488	0.660	0.785
Has a child born alive (=1)	0.006	0.560	0.008	0.570	0.904	0.763	-0.022	0.392	0.023	0.332	0.068*	0.185
24–26 years old												
Ever married (=1)	0.002	0.926	-0.005	0.846	0.787	0.964	0.011	0.718	0.014	0.616	0.900	0.881
Has a child born alive (=1)	0.019	0.097*	0.016	0.238	0.820	0.206	-0.040	0.028**	0.003	0.862	0.004***	0.008***
27–29 years old												
Ever married (=1)	-0.006	0.707	-0.009	0.652	0.923	0.866	-0.001	0.982	0.011	0.690	0.654	0.884
Has a child born alive (=1)	0.008	0.363	0.004	0.606	0.732	0.649	-0.017	0.120	0.009	0.356	0.012**	0.042**

Note: Columns 1 and 7 and columns 3 and 9 report the point estimates on β_1 and β_2 in equation 1. Columns 2 and 8, and columns 4 and 10 report *p*-values from testing the null hypothesis that β_1=0 and β_2=0 in equation 1. Columns 5 and 11 report *p*-values from testing the null hypothesis that β_1 =β_2 in equation 1. Columns 6 and 12 report *p*-values from testing the null hypothesis that β_1 =β_2 =0. *** p < 0.01, ** p < 0.05, * p < 0.1. See Online Appendix Table A.3 for number of observations.

Appendix E: Labor Market Participation and Labor Income: Construction of Weights and Additional Analyses

The Permanent Multiple Purpose Household Survey (EPHPM) data used for the labor market analyses come from repeated cross-sections of the household survey, collected between 2010 and 2016. The EPHPM sample is not representative at the level of the program area and, in particular, its sampling strategy generally leads to an overrepresentation of the urban population. We therefore use information from the census, and the details of the EPHPM sampling strategy, to calculate relevant sampling weights for the 70 PRAF-II municipalities. We then informally assess their validity by comparing census and weighted EPHPM sample means and point estimates for a subset of key schooling and demographic outcomes. We also present and discuss further results on the long-term impact of the CCT on labor market outcomes not covered in the paper, particularly for the men.

Construction of Sample Weights for EPHPM

The EPHPM sampling framework over-samples urban areas and under-samples small localities in both rural and urban areas. At the national level, the EPHPM sample framework is divided into four zones: Central District-Tegucigalpa, San Pedro Sula, other urban and rural. The first three zones comprise the country's urban population. The sample is selected to be representative at the national urban and rural levels, and also at the departmental level, but not at the municipality level. As a result, among people born in the 70 PRAF-II municipalities (which do not completely cover the included departments), those living in urban areas are over sampled compared to those living in rural areas in the survey years 2010–2016. Using the EPHPM data without taking this—for our purposes endogenous—sampling, could lead to biased ITT estimates, for example if there is treatment heterogeneity on migration to urban areas overrepresented in the surveys.

To address this concern, we construct sampling weights taking into account the urban/rural designation and the population size of localities[23] from the 2013 census. First we divide localities into urban and rural, within those two categories sort all localities by size, and create two weight categories in each: localities above and below the median size. This procedure allows constructing weights that correct for the oversampling of urban areas but also for the over–representation of larger localities in both rural and urban areas. Doing so yields four categories of weights: small localities in rural areas, the remaining rural areas, urban areas and large urban areas (i.e., Central District-Tegucigalpa and San Pedro Sula). For each of these categories, we calculate from the Honduran Population Census in 2013 the total number of women and men ages 19-26 who were born in one of the 70 PRAF-II

[23] The Spanish name in the Honduran Population Census for the smaller geographic areas that we refer to as localities is *caserío* in rural areas and *barrio* in urban areas.

municipalities. Then, we match the list of localities in each of the four categories in the national census with the list of localities from the household surveys. We combine seven survey rounds and for each category calculate the number of women and men ages 19–26 (calculated in 2013 irrespective of survey year) who were born in one of the 70 PRAF-II municipalities, regardless of where they live at the time of the survey. The final weight is the inverse of the probability of having been surveyed in one of the four categories with respect to the census-population size of each category by gender and age group.

More formally, within each category we construct population weights for individuals born in the 70 targeted municipalities by gender and age cohort.

$$w_j^g = \frac{TOTPOP_{born \in 70} Census2013_j^g}{\sum_{t=2010}^{2016} TOTPOP_{born \in 70}\, EPHPM_{j,t}^g}$$

where g accounts for being male or female, j takes a value 1 to 4 and captures the sampling category, $TOTPOP_{born70}Census2013^g_j$ is total population in the census 2013 of gender g born in one of the 70 PRAF-II municipalities and living in category j, t captures the year in which the household survey was conducted and $TOTPOP_{born70}EPHPM^g_{j,t}$ is the sample of individuals in the EPHPM of gender g born in one of the 70 PRAF-II municipalities and living at time t in category j.

The EPHPM survey and sample frame present two additional problems. First, there is no information on ethnicity in the survey. Second, dwelling lists used for the household sampling were updated between the 2013 and 2014 surveys.

To address the lack of information on ethnicity in the EPHPM, we match individuals' locality with the locality in the 2013 census and calculate the rate of indigenous population living in that location in 2013. Using that information, we can restrict analyses to respondents who were living in localities in which the non-indigenous population in 2013 represented at least 25 percent of the population, which allows retention of approximately 80 percent of the total sample.[24] We report estimates for the subsample of females and males born in predominantly non-indigenous villages and for the whole sample of females and males.

The second additional concern is that household surveys conducted before 2014 used the list of registered dwellings developed in 1999 for the 2001 national census, and therefore exclude those living in dwellings constructed after 1999. Starting in 2014, however, household sampling was carried out using the 2011 pre-census list of registered dwellings developed for the 2013 census. As a result, the sample frame of dwellings for surveys conducted before 2014 did not include all new households formed after 1999. Included among such new

[24] Results differ little if we instead restrict the sample to localities in which at least 50 percent of the population is non-indigenous.

dwellings could be those constructed by individuals who directly benefited from the CCT and subsequently formed independent households. This may be especially relevant for men, for whom the likelihood of living in a single member household is higher than for women. Below we consider estimates that separately consider only the 2014-16 surveys that use the more current underlying household sampling frame, and therefore potentially provide more valid estimates.[25] This approach, however, comes at the obvious cost of smaller sample sizes.

Results for Women

We compare educational and demographic outcomes common to both the census and household survey for all women ages 19–26 in 2013 in Table E.1 and for the subsample of women without an indigenous background or from primarily non-indigenous localities in Table E.2. The first two columns in each of the tables present the sample means in the control (G_4) and the treatment effects for those women born in G_1 municipalities using the census data and estimating equation (1). Columns 3–4 replicate those results but restrict the census sample to the set of localities included in the household survey at some point from 2010–2016. Columns 5-8 present the results using data from the household survey, and estimating equation (3) including both weighted least squares (WLS) and ordinary least squares (OLS).

In general, the control-group means for highest grades attained and the probability of completing different schooling levels are higher in the household survey than in the census. Results in columns 3-4 show that the difference between samples is in part explained by the selection of localities included in the household survey. Once we restrict the census to those localities also in the household survey, the sample mean on highest grade attained in the control group increases by more than one grade and the probability of having completed any schooling level is even higher than the un-weighted sample mean in the household survey (column 7). The difference in schooling outcomes across surveys is also reflected in some of the demographic characteristics. Women in the household survey are less likely to be married and more likely to be the daughter of the household head. We also find important differences on the incidence of domestic migration. In the household survey, domestic migration among women born in control municipalities accounts for 34 percent of women in the cohort of interest; this value falls to 26 percent in the census data. However, it is similar to the share of domestic migrants reported in the restricted census sample. This suggests that the household survey is over-sampling women who were born in PRAF-II municipalities and have migrated to other municipalities. Columns 5–6 show that our sampling weights correct in part for the differences between data sources. On average, we end up with a sample in which the level of education and the incidence of domestic migration in the control group, as well, as the size of

[25] Because ages are calculated in 2013, excluding the earlier survey rounds has the additional effect of excluding those who were especially young when the survey was conducted, for example 16-year-olds surveyed in 2010.

the treatment effect on the set of outcomes shown are more similar to those observed in the comparable census data. Table E.2 shows the same exercise for the subsample of girls living in localities with a majority of non-indigenous population. Applying sampling weights, we again correct for some of the differences across surveys. Based on these results we argue it is plausible that the sampling weights help us overcome much of the sampling selection bias inherent in the EPHPM and therefore present the results on labor market participation and labor income for women between ages 19 and 26 in the main text.

Results for Men

Tables E.3–E.5 show the results for the sample of young men. As for the women, Table E.3 shows large differences for educational and demographic outcomes between the census and the surveys. Part of these differences are explained by the fact that the household survey does not include a sample of representative localities. On average, men from localities included in the EPHPM have more years of education, are more likely to finish different schooling levels and more likely to still be studying. In addition, when we estimate equation (3) by WLS or OLS on highest grade attained we no longer find a long-term impact of exposure to the CCT. Men from both samples also differ in terms of their demographic characteristics: those surveyed in the household survey are more likely to be the child of the household head and less likely to be married or to be living in a single person household. Furthermore, we observe that men from G_1 municipalities in the household survey live in larger households. Applying sampling weights to correct for the oversampling of urban and larger localities does not correct for these differences.

Part of the differences observed may be explained by the fact that the household surveys from 2010–2013 use the outdated list of registered dwellings as described earlier. Table E.4 compares the results between the census and two alternative and restricted subsamples of the EPHPM survey. Columns 1–6 in Table E.4 show sample means and CCT long term effects using the census data: for the complete census (columns 1–2), for the census restricted to localities represented in the household surveys collected between 2014 and 2016 (columns 3–4) and for the census data restricted to localities included in any household survey from 2010 to 2013 (column 5–6). Columns 7–14 show results using household survey data from 2014–2016 only (columns 7–10) and results using all the household survey rounds but restricted to men ages 19–26 at the time of the survey in the survey rounds before 2013 or ages 19–23 in 2013 and surveyed in 2013 or later (columns 11–14). Restricting the analysis to surveys between 2014–2016 leads to sample means and CCT effects for the set of schooling variables and for domestic migration that are more in line with the census results, especially after applying the sampling weights. The estimates are also more aligned for demographics. On the other hand, restricting the sample to the oldest cohorts in the first three years of surveys (2010–2012) also improve the estimates on demographic characteristics, but we cannot correct completely for the differences in terms of educational outcomes and treatment effects. Summarizing, while in the case of young women we are able to obtain from the pooled EPHPM estimates similar to the census-based population estimates in terms of schooling outcomes, demographics and domestic migration, we cannot find a single sample of men in the household survey satisfying these conditions without substantially restricting the sample size.

Nevertheless, for completeness Table E.5 presents the results on labor outcomes for men for the restricted sample of the EPHPM data. These arguably come with stronger caveats than the results for women, as in contrast to the women we cannot replicate the education treatment effects found with the census using the household survey data as just described. The rate of labor market participation among men in this context is much higher than among women, around 93 percent of young men worked, and there are no significant differences in labor market participation between men born in G_1 and those born in control municipalities. Results show that formality in this context is quite low, only 5 percent of the men working in control municipalities have a formal job and men born in G_1 municipalities are between 3 and 7 percentage points less likely to have a formal job. This result is consistent with the slightly higher number of part-time students from in G_1 municipalities. Results on income show that if anything men from G_1 municipalities earn less monthly and per hour worked. Adding full-time students does not change the results much, as contrary to the case of women, the share of full-time students from both G_1 and control municipalities is negligible.

For men, estimates could also, of course, be affected by the higher probability of international migration from CCT municipalities. To gauge the potential importance of selection into international migration we therefore use the estimated number of international migrants (based on the census) by age, gender and municipality of birth to expand the household survey and approximate labor income for these international migrants. Specifically, we estimate monthly income for the sample of international migrants using annual data from the 2013 American Community Survey (ACS).[26] For each international migrant in the Population Census we impute median earnings for full-time and year-round male workers with Honduran origin from the ACS. We add the subsample of international migrants to the household survey and give them a sampling weight of one when estimating WLS. The bottom panel of Table E.5 reports the result on monthly income after including the sample of international migrants. Point estimates on the CCT effect are positive but not statistically significant different from zero. This exercise suggests there are no strong positive long-term

[26] In 2013, around 90 percent of male international migrants in Honduras in between ages 19 and 26 were living in the United States of America.

labor market returns for the sample of young men. We emphasize, however, that because we could not replicate the census findings for education using the weighted EPHPM, confidence in these results is low– they may be driven by the peculiarities of the survey sample.

Table E.1. Education, Demographics and Migration. Comparison of Census 2013 and EPHPM 2010–2016 for Females 19–26 Years Old

	CENSUS 2013				EPHPM 2010-2016			
	All census		Restricted to EPHPM villages		WLS		OLS	
	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Highest grades attained	5.82	0.359**	6.91	0.319	6.20	0.311	6.48	0.409
		(0.163)		(0.252)		(0.273)		(0.285)
Four or more years of educ. (=1)	0.71	0.054***	0.79	0.037**	0.76	0.038	0.79	0.032
		(0.017)		(0.016)		(0.033)		(0.031)
Completed primary educ. (=1)	0.60	0.035	0.70	0.018	0.65	0.013	0.69	0.001
		(0.023)		(0.025)		(0.041)		(0.041)
Completed secondary educ. (=1)	0.12	0.022**	0.19	0.028	0.10	0.037	0.12	0.054*
		(0.011)		(0.019)		(0.028)		(0.032)
University studies (=1)	0.02	0.009**	0.03	0.019**	0.02	0.018*	0.02	0.030**
		(0.004)		(0.007)		(0.011)		(0.013)
Currently enrolled (=1)	0.10	0.009	0.15	0.013	0.13	-0.007	0.13	0.008
		(0.008)		(0.014)		(0.025)		(0.024)
Full time student (=1)	0.05	0.005	0.08	0.012	0.07	0.015	0.07	0.035*
		(0.005)		(0.009)		(0.021)		(0.020)
Ever married (=1)	0.60	0.003	0.56	0.000	0.46	0.022	0.47	-0.005
		(0.019)		(0.020)		(0.047)		(0.049)
Household head or spouse (=1)	0.48	0.001	0.45	-0.008	0.33	0.002	0.36	-0.010
		(0.020)		(0.023)		(0.034)		(0.037)
Single person household (=1)	0.01	-0.001	0.01	0.001	0.00	0.003	0.00	0.004
		(0.001)		(0.002)		(0.002)		(0.002)
Household size	5.38	0.084	5.12	0.119	5.70	0.370	5.58	0.345
		(0.146)		(0.116)		(0.278)		(0.262)
Child of the household head (=1)	0.36	-0.004	0.33	-0.008	0.44	0.010	0.41	0.025
		(0.018)		(0.023)		(0.043)		(0.044)
Child in law of the household head (=1)	0.06	0.003	0.06	0.007	0.08	0.024	0.08	0.018
		(0.008)		(0.006)		(0.023)		(0.020)
Domestic migrant (=1)	0.26	-0.033	0.39	-0.052	0.25	-0.021	0.34	-0.040
		(0.032)		(0.058)		(0.048)		(0.059)
Observations	69,522		27,350		69,680	1,575		

Note: The last row in column 6 shows estimated population size. Cluster robust standard errors at the municipality level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.2. Education, Demographics and Migration Comparison of Census 2013 and EPHPM 2010–2016 for Non-indigenous Females, 19–26 Years Old

	Non-indigenous women CENSUS 2013				Women in villages predominantly non-indigenous EPHPM 2010-2016			
	All census		Restricted to EPHPM villages		WLS		OLS	
	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Highest grades attained	5.90	0.501**	7.12	0.262	6.67	0.462	6.93	0.577
		(0.238)		(0.300)		(0.412)		(0.397)
Four or more years of educ. (=1)	0.70	0.070***	0.80	0.036**	0.76	0.092**	0.79	0.070*
		(0.018)		(0.017)		(0.043)		(0.039)
Completed primary educ. (=1)	0.60	0.033	0.71	-0.001	0.70	0.006	0.74	-0.008
		(0.025)		(0.027)		(0.048)		(0.046)
Completed secondary educ. (=1)	0.13	0.035**	0.21	0.025	0.15	0.053	0.16	0.073*
		(0.015)		(0.022)		(0.041)		(0.041)
University studies (=1)	0.02	0.011**	0.04	0.015*	0.02	0.038**	0.02	0.052***
		(0.005)		(0.008)		(0.015)		(0.017)
Currently enrolled (=1)	0.12	0.012	0.16	0.017	0.13	-0.004	0.13	0.014
		(0.014)		(0.018)		(0.034)		(0.030)
Full time student (=1)	0.06	0.006	0.08	0.012	0.08	0.028	0.07	0.052*
		(0.008)		(0.012)		(0.030)		(0.027)
Ever married (=1)	0.63	-0.002	0.57	-0.001	0.51	-0.036	0.53	-0.067
		(0.020)		(0.023)		(0.041)		(0.043)
Household head or spouse (=1)	0.52	-0.011	0.47	-0.017	0.39	-0.070*	0.41	-0.073
		(0.019)		(0.026)		(0.041)		(0.045)
Single person household (=1)	0.01	0.000	0.01	0.001	0.00	0.004	0.00	0.006
		(0.001)		(0.003)		(0.003)		(0.004)
Household size	5.07	0.133	4.91	0.181*	5.35	0.461	5.22	0.420
		(0.098)		(0.093)		(0.302)		(0.288)
Child of the household head (=1)	0.31	-0.004	0.30	-0.001	0.37	0.064	0.32	0.077
		(0.019)		(0.025)		(0.045)		(0.049)
Child in law of the household head (=1)	0.05	0.010*	0.05	0.014**	0.08	0.044	0.08	0.031
		(0.005)		(0.005)		(0.030)		(0.028)
Domestic migrant (=1)	0.36	-0.031	0.49	-0.078	0.41	-0.156*	0.50	-0.182*
		(0.060)		(0.076)		(0.085)		(0.092)
Observations	44,748		20,419			48,846		1,169

Note: The last row in column 6 shows estimated population size. Cluster robust standard errors at the municipality level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Columns 1–4 show estimates for women with non-indigenous background using National Population

Census 2013, columns 5–8 show estimates for women who were born in villages in which the non-indigenous population in 2001 represented at least 75 percent of the village population.

Table E.3. Education, Demographics and Migration. Comparison of Census 2013 and EPHPM 2010–2016 for Males, 19–26 Years Old

	CENSUS 2013				EPHPM 2010-2016			
	All census		Restricted to EPHPM villages		WLS		OLS	
	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grades attained	5.38	0.351**	6.42	0.497**	6.23	-0.002	6.43	-0.056
		(0.172)		(0.249)		(0.235)		(0.227)
Four or more years of educ. (=1)	0.67	0.043*	0.76	0.037*	0.78	0.003	0.81	-0.022
		(0.022)		(0.021)		(0.035)		(0.032)
Completed primary educ. (=1)	0.56	0.019	0.66	0.028	0.65	-0.012	0.67	-0.031
		(0.023)		(0.023)		(0.041)		(0.038)
Completed secondary educ. (=1)	0.09	0.025**	0.15	0.039**	0.12	0.014	0.13	0.016
		(0.011)		(0.018)		(0.025)		(0.025)
University studies (=1)	0.01	0.011***	0.02	0.020***	0.02	0.016	0.02	0.016
		(0.003)		(0.005)		(0.010)		(0.012)
Currently enrolled (=1)	0.08	0.024***	0.12	0.045***	0.11	0.042	0.12	0.039
		(0.009)		(0.014)		(0.028)		(0.028)
Full time student (=1)	0.04	0.010**	0.07	0.022**	0.05	0.015	0.05	0.020
		(0.005)		(0.011)		(0.016)		(0.016)
Ever married (=1)	0.39	0.030*	0.38	0.020	0.25	0.042	0.26	0.033
		(0.018)		(0.018)		(0.029)		(0.028)
Household head or spouse (=1)	0.31	0.017	0.31	0.005	0.19	0.000	0.20	-0.009
		(0.018)		(0.018)		(0.025)		(0.026)
Single person household (=1)	0.02	-0.005**	0.03	-0.000	0.00	0.012**	0.01	0.013*
		(0.002)		(0.004)		(0.006)		(0.007)
Household size	5.62	0.069	5.42	0.055	6.00	0.416*	5.94	0.453**
		(0.154)		(0.185)		(0.225)		(0.214)
Child of the household head (=1)	0.56	-0.007	0.51	0.008	0.71	0.000	0.68	0.006
		(0.016)		(0.020)		(0.030)		(0.035)
Child in law of the household head (=1)	0.02	-0.000	0.02	0.002	0.01	0.022**	0.01	0.020**
		(0.002)		(0.003)		(0.009)		(0.008)
Domestic migrant (=1)	0.19	-0.037	0.31	-0.055	0.16	-0.002	0.22	-0.024
		(0.025)		(0.053)		(0.041)		(0.050)
Observations		64,663		23,239		64,543		1,448

Note: The last row in column 6 shows estimated population size. Cluster robust standard errors at the municipality level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.4. Education, Demographics, and Migration, Comparison between Census 2013 and EPHPM 2014–16 (2010–2016) for Males, 19–26 Years Old

	CENSUS 2013						EPHPM 2014–2016				EPHPM 2010–2016 (Restricted)			
	All census		Restricted to EPHPM villages in 2014–2016		Restricted to EPHPM villages		WLS		OLS		WLS		OLS	
	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1	Mean G_4	G_1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Grades attained	5.38	0.351**	6.83	0.412*	6.42	0.497**	5.12	0.526	5.48	0.444	6.01	0.152	6.30	0.130
		(0.172)		(0.228)		(0.249)		(0.595)		(0.595)		(0.265)		(0.253)
Four or more years of education (=1)	0.67	0.043*	0.79	0.017	0.76	0.037*	0.69	-0.010	0.73	-0.048	0.74	0.017	0.77	-0.007
		(0.022)		(0.020)		(0.021)		(0.074)		(0.061)		(0.037)		(0.032)
Completed primary education (=1)	0.56	0.019	0.71	0.014	0.66	0.028	0.50	0.057	0.55	0.022	0.60	0.010	0.64	-0.012
		(0.023)		(0.024)		(0.023)		(0.091)		(0.077)		(0.042)		(0.038)
Completed secondary education (=1)	0.09	0.025**	0.18	0.044**	0.15	0.039**	0.04	0.129**	0.06	0.122**	0.12	0.040	0.14	0.046
		(0.011)		(0.019)		(0.018)		(0.051)		(0.058)		(0.028)		(0.027)
University studies (=1)	0.01	0.011***	0.03	0.019***	0.02	0.020***	0.01	0.015	0.02	0.011	0.02	0.019*	0.03	0.021
		(0.003)		(0.006)		(0.005)		(0.017)		(0.036)		(0.012)		(0.014)
Currently enrolled (=1)	0.08	0.024***	0.14	0.054***	0.12	0.045***	0.04	0.012	0.06	-0.006	0.08	0.018	0.09	0.015
		(0.009)		(0.013)		(0.014)		(0.027)		(0.039)		(0.024)		(0.022)
Full time student (=1)	0.04	0.010**	0.07	0.030**	0.07	0.022**	0.00	0.004	0.01	-0.002	0.03	-0.000	0.03	0.004
		(0.005)		(0.012)		(0.011)		(0.012)		(0.024)		(0.010)		(0.011)
Ever married (=1)	0.39	0.030*	0.38	0.008	0.38	0.020	0.48	0.019	0.48	-0.003	0.33	0.048	0.35	0.032
		(0.018)		(0.021)		(0.018)		(0.055)		(0.056)		(0.035)		(0.035)
Household head or spouse (=1)	0.31	0.017	0.32	-0.007	0.31	0.005	0.39	0.002	0.39	-0.004	0.27	-0.033	0.29	-0.049
		(0.018)		(0.023)		(0.018)		(0.068)		(0.066)		(0.031)		(0.032)

Single person household (=1)	0.02	-0.005**	0.03	-0.004	0.03	-0.000	0.00	0.025**	0.00	0.025**	0.02	-0.003	0.02	0.002
		(0.002)		(0.006)		(0.004)		(0.011)		(0.011)		(0.009)		(0.009)
Household size	5.62	0.069	5.42	-0.012	5.42	0.055	5.04	0.610	5.22	0.432	5.56	0.803***	5.48	0.844***
		(0.154)		(0.244)		(0.185)		(0.386)		(0.422)		(0.208)		(0.211)
Child of the household head (=1)	0.56	-0.007	0.44	0.039	0.51	0.008	0.50	0.017	0.49	-0.011	0.63	0.027	0.59	0.047
		(0.016)		(0.036)		(0.020)		(0.075)		(0.078)		(0.034)		(0.041)
Child in law of the household head (=1)	0.02	-0.000	0.02	0.001	0.02	0.002	0.00	0.028**	0.01	0.019	0.01	0.030***	0.01	0.028***
		(0.002)		(0.004)		(0.003)		(0.013)		(0.020)		(0.011)		(0.010)
Domestic migrant (=1)	0.19	-0.037	0.48	-0.195**	0.31	-0.055	0.20	0.006	0.34	-0.060	0.19	-0.009	0.28	-0.050
		(0.025)		(0.086)		(0.053)		(0.063)		(0.081)		(0.047)		(0.057)
Observations		64,663		14,284		23,239		64,726		406		64,522		1,324

Note: The last row in columns 8 and 12 shows estimated population size. Cluster robust standard errors at the municipality level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table E.5. Long-Term Impacts on Education, Labor Participation, and Earnings for Males, Ages 19–26 Years

	EPHPM 2014–2016			EPHPM 2010–2016		
	WLS		OLS	WLS		OLS
	Mean G_4	G_1	G_1	Mean G_4	G_1	G_1
	(1)	(2)	(3)	(4)	(5)	(6)
Grades attained	5.12	0.526	0.444	6.01	0.152	0.130
		(0.595)	(0.595)		(0.265)	(0.253)
Full time student (=1)	0.00	0.004	-0.002	0.03	-0.000	0.004
		(0.012)	(0.024)		(0.010)	(0.011)
Labor market participation (=1)	0.93	-0.025	-0.036	0.93	-0.021	-0.036
		(0.042)	(0.045)		(0.023)	(0.023)
Number of hours worked weekly	37.78	0.859	-0.100	46.49	-0.911	-1.762
		(2.467)	(2.510)		(1.548)	(1.459)
Number of hours worked weekly, conditional on working	41.21	2.303	1.786	50.09	1.133	0.667
		(2.507)	(2.595)		(1.311)	(1.228)
Formal worker (=1)	0.05	-0.050*	-0.071*	0.05	-0.031**	-0.047**
		(0.027)	(0.040)		(0.015)	(0.021)
Agricultural sector (=1)	0.76	-0.022	0.008	0.68	0.039	0.054
		(0.074)	(0.075)		(0.047)	(0.047)
Non-agricultural sector (=1)	0.28	-0.021	-0.049	0.29	-0.038	-0.064
		(0.054)	(0.059)		(0.041)	(0.043)
Construction worker (=1)	0.10	0.001	0.030	0.05	0.023	0.031
		(0.044)	(0.041)		(0.019)	(0.019)
INCOME (in 2013 lempiras)						
Monthly income	2174.61	190.221	-134.275	2434.19	-191.738	-457.845*
		(433.469)	(438.500)		(244.980)	(271.918)
Hourly income	24.69	-6.626*	-7.979**	18.53	-4.447**	-5.176**
		(3.719)	(3.733)		(1.908)	(1.987)
Monthly income, conditional on working	2446.48	199.193	-109.804	2661.93	-165.024	-409.234
		(494.008)	(501.660)		(270.129)	(292.326)
Hourly income, conditional on working	34.96	-10.113**	-10.450***	25.80	-5.908***	-6.223***
		(4.117)	(3.812)		(2.170)	(2.084)
INCOME APPROXIMATION with imputed values for full-time students						
Approximate monthly income°	2235.19	126.107	-321.801	2526.11	-171.550	-415.761
		(462.789)	(580.910)		(264.111)	(317.359)
Approximate hourly income°	25.15	-7.002*	-9.133*	19.48	-4.231**	-4.725**
		(3.918)	(4.601)		(1.993)	(2.174)
	2502.26	114.968	-311.004	2685.47	-144.359	-371.773

Approximate monthly income, conditional on working°		(512.466)	(615.170)		(281.703)	(323.878)
Approximate hourly income, conditional on working°	35.40	-10.627**	-11.754***	26.18	-5.492**	-5.573**
		(4.096)	(4.232)		(2.200)	(2.257)
INCOME APPROXIMATION with imputed values for full-time students & international migrants						
Approximate monthly income°	3793.22	339.041		4084.82	120.745	
		(474.978)			(597.765)	
Approximate monthly income, conditional on working°	4228.60	356.273		4339.67	141.228	
		(520.649)			(633.165)	
Observations		64,726	406		64,522	1,324

Note: For the sample of full-time students we impute monthly and hourly median earnings by gender and education level. For the sample of international migrants, we impute in addition estimated earnings in the US (see text for details). Results are robust to the inverse hyperbolic sine transformation of income and the use of the rank of conditional income. Cluster robust standard errors at the municipality level are reported in parentheses. The last row in columns 2 and 4 shows estimated population sizes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$