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This version: June 2019

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 the 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. For the non-indigenous, we find positive and robust impacts on educational outcomes for cohorts of a very wide age range. 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. They also include substantive gains for grades attained and current enrollment for others exposed during early childhood, raising the possibility of further gains going forward. Educational gains are, however, more limited for the indigenous. Finally, exposure to the CCT increased the probability of international migration for young men, from 3 to 7 percentage points, also stronger for the non-indigenous. 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.

JEL Classification: I25, I28, I38, O15

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

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1. Introduction

Conditional cash transfer (CCT) programs are among the most popular social programs in the developing world. They have been operating in Latin America for two decades, reaching 25 percent of the region's population (Robles, Rubio and Stampini, 2017), and increasingly in other regions. CCTs aim to alleviate short-run poverty while inducing investment in the nutrition, health, and education of the next generation. Evidence from various contexts demonstrates their effectiveness in the short run (Fiszbein and Schady, 2009).[2] There is greater uncertainty, however, as to whether these translate into longer-term gains (Molina Millán et al., 2019). More generally, little is known about whether and how CCTs affect the trajectories of children benefitting directly or indirectly from different program components at different points during their childhood.

This paper provides experimental evidence on long-term impacts for children exposed at different stages of early childhood and school-ages 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 and, unlike most other randomized CCT evaluations such as Mexico's *PROGRESA*, 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 thus eight years after it ended), to analyze impacts of the CCT on individuals in cohorts spanning nearly 25 years. Assigning each individual to the municipality where they were born—a good proxy for their preprogram location—we circumvent the typical selection and attrition concerns that affect the study of long-term impacts of highly mobile cohorts of individuals. We thus estimate intent-to-treat (ITT) impacts that account for migration within the national territory, as high as 30 percent for some cohorts of interest. The census also includes information on current international migration of former household members, allowing direct study of international migration as an outcome and assessment of such migration as another potentially important selection concern.[3]

Beyond these key advantages, the census data provide sufficient statistical power for the estimation of long-term impacts on several different cohorts of interest, and we can separately

[2] Recent evidence on short-term effects of CCTs on education includes reviews (Murnane and Ganimian, 2014; Glewwe and Muralidharan 2015) and meta-analyses (Saavedra and García, 2012; Baird et al. 2014; McEwan 2015).

[3] An important limitation of using census data, however, is that we are unable to capture measures of nutrition or health improvements.

estimate the impacts on children exposed to the nutrition and health components of the program during early childhood and to 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 spillover or indirect effects on others 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 program ended.

The wide age range examined in a single setting constitutes an important contribution of this paper, providing a better understanding on whether exposure to CCTs at different ages can impact human capital and subsequent outcomes. This is particularly relevant as some transfer programs narrowly target specific ages. 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 nutrition and health in very early childhood; only educational outcomes at critical ages in primary or secondary school). Beyond the cash transfer literature, examination of impacts at different ages of exposure is relevant for the literature on human capital formation. Indeed, economists often motivate focusing on early childhood based on Cunha and Heckman's (2007) multistage model of skill formation that predicts "skill begets skill" with investments made in early life being 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 that may differ by cohort, 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 thus provide insights on the potential importance of other constraints. Specifically, we analyze impacts by sex and ethnic background (non-indigenous versus indigenous), resulting in four distinct 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, poorer access to markets, and labor market discrimination, which together with a strong emphasis on community ties and attachment to the land may 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 remains relatively scarce. There is a body of ethnographic work that points to specific challenges related to CCT and related programming for indigenous populations (Correa Aste and Roopnaraine, 2014). For many programs in Latin America, there have been efforts to improve targeting to indigenous populations, but less has been done to adapt programs to better accommodate indigenous cultures or the particular challenges they face. Programs targeting the nuclear family, for example, may not adequately reach the person or persons 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).

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 for understanding the potential long-term impacts of CCTs. And 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 (Stecklov et al., 2005; Angelucci, 2015). Given the wide reach of CCT programs, evidence on migration impacts can not only help understand potential selection biases but also inform the more general international migration policy debate.

We find 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 grades directly targeted by the program. They also include substantive gains for grades attained and current enrollment for children exposed during early childhood, raising the possibility of further gains going forward. Effects for indigenous beneficiaries, however, are much more limited than those for the non-indigenous. We also find 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.

As such, this paper complements other recent evidence on long-term impacts of CCTs (see Molina Millán et al. (2019) 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, 2019) 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 *absolute* long-term impacts. Other studies estimating absolute long-term impacts include: 1) Barrera-Osorio, Linden, and Saavedra (2019), 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; 2) Baird, McIntosh, and Özler (2018), who also study impacts of an educational CCT in Malawi two years after it ended; and 3) 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.[4]

For children exposed during school-going ages, the existing evidence from several of the above studies generally indicates that CCTs help them obtain higher education, but is less conclusive for subsequent outcomes. 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

[4] More broadly, this paper also relates to the mixed emerging longer-term evidence for unconditional cash transfers (Bandiera et al., 2017; Banerjee et al., 2016; Handa et al., 2018; Haushofer and Shapiro, 2018).

CCT literature by providing experimental evidence on the absolute long-term impacts of program exposure during a wide range of ages in early childhood and adolescence, and for a program that ended eight years earlier.

2. The Honduran CCT Program and Prior Evidence

We study the long-term impacts of the second phase of PRAF, a CCT implemented from 2000 to 2005. PRAF-II aimed to increase investment in human capital, including nutrition and health during early childhood and education during primary-school ages. Modeled after the *PROGRESA* program in Mexico, PRAF-II provided cash transfers (in the form of readily exchangeable vouchers) to: 1) households with pregnant women and (initially) children ages 0–3 (extended to age five in 2003), conditional on attendance at prenatal and child health and growth monitoring appointments and health education workshops; and 2) households with children ages 6–12 who had not yet completed fourth grade, conditional on school enrollment and attendance. Transfers averaged 4 percent of total preprogram household income, relatively little compared to other CCTs in the region, and were delivered twice annually. In randomly selected areas, the program also aimed to strengthen the supply-side through investments in the quality of both health and education services (IDB, 1998, 2006; IFPRI, 2003; Moore, 2008).[5]

The CCT targeted 70 rural municipalities in western Honduras with among the highest malnutrition rates in the country, and a municipality-level randomized assignment was used to determine treatment and control municipalities for evaluation. Randomization was stratified into five blocks of 14 municipalities each, after ordering them 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 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 program impacts by comparing outcomes of children born in experimental treatment versus control municipalities (IDB, 2006).

The evaluation design included three different benefit packages and a control group:

1. G_1: Households received cash transfers conditional on nutrition, health, and education behaviors (20 municipalities). We refer to this as the "basic CCT."

[5] Appendix B provides further information about the CCT, as well as other related interventions implemented in program municipalities in later years. We treat all other interventions post-randomization as potentially endogenous and therefore do not control for them in the main analyses.

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 control group (20 municipalities).

Program monitoring documents and 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 not fully operational until after 2002. The reports do not allow a full characterization of these delays, which may have disrupted health and education services and/or affected in unknown ways individual perceptions and expectations in G_2 and G_3. Therefore, while we adhere to the experimental design accounting for all three treatment arms, we focus our discussion on the impacts of the basic CCT components (G_1).[6] Emphasis on the basic CCT has the additional advantage of making the analysis more comparable to most existing research on the long-term impacts of CCTs.

Prior evidence from short-term evaluations shows impacts on early-life health indicators and schooling that are qualitatively similar to those found for other CCTs in the region. Morris et al. (2004) examine the short-term program evaluation data and find that after two years the basic CCT increased the uptake of prenatal care (five or more visits) for pregnant women by nearly 20 percentage points (on a base of about 50 percent). They find similarly large increases for routine checkups (including growth monitoring) for children under 3 years, which they suggest could be important for administering vaccinations more opportunely, though there was little evidence of improved vaccination at that stage. Effects on schooling were more modest than observed in other contexts, however, possibly reflecting the relatively small transfer size in comparison to other programs in the region. Galiani and McEwan (2013) use the 2001 national census, administered after eight months of transfers, and find an increase in enrollment rates of about 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 larger effects in the two strata with the highest levels of malnutrition at baseline. Glewwe and Olinto (2004) use the short-term program evaluation data after two years and show significant but smaller increases in primary school enrollment rates of around 3 percentage points for children 6–12 years old at

[6] Estimates for G_2 are presented in Appendix D.

baseline as well as modest improvements in attendance and grade promotion, and a slight reduction in hours worked.

In other prior work, Stecklov et al. (2007) demonstrate that the program led to an increase in fertility of 2–4 percentage points by 2002. These changes in fertility point to the possibility that cohorts born after the start of the program in 2000, the younger cohorts we analyze, could be selective.[7] In addition, if such changes in fertility in turn led to a shift in fertility norms, they also may have had indirect effects on the older cohorts reaching reproductive ages.

Lastly, examining the same period as we do, Ham and Michelson (2018) use municipality-level averages from both the 2001 and 2013 Honduran censuses to analyze the impact of PRAF-II for children ages 6–12 in 2001 (and thus 18-24 in 2013). They exploit the randomized design and estimate municipal level differences-in-differences for this age cohort, showing increases in grades attained, secondary school completion, and labor force participation, especially for females in G_2, after controlling for municipality-level fixed effects and a number of time-variant (and hence possibly endogenous) controls. As the analysis uses average outcomes based on place of residence in 2001 and 2013, it makes the strong assumption that migration between 2001 and 2013 (over 25 percent for thus age cohort) does not affect the internal validity and the estimates do not account for any returns that materialize through migration.

3. Data and Methodology

The principal data source is the XVII Honduran National Population and Housing Census of 2013.[8] For complementary analyses, we also use the 1988 and 2001 national censuses, and the short-term program evaluation data collected for PRAF-II. For the main analyses in 2013, we limit the census sample to all individuals born in the 70 targeted rural municipalities regardless of their current residential location. For the age cohorts we study, municipality of birth together with the municipal-level randomized program assignment provides an exogenous indicator of program exposure not influenced by subsequent domestic migration or geographical sorting that may have occurred during or after the program.[9] In addition to including other typical

[7] Program rules were 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. those ages 9–12 in 2013.

[8] Below we consider in more detail potential problems with national census data; for a general description, see Cleland (1996).

[9] We separately examine the likelihood that different age cohorts born before the program were still living in their municipality of birth at the time the program started (section 4.4); in general such preprogram migration would attenuate estimates relative to ones for which residential location at the moment of program assignment was used.

information (sex, age, ethnicity, education, migration, civil status, and fertility) for all current residents, the census includes basic information on former household members who left Honduras at any point over the prior decade. Information available on these international migrants includes sex, age, year of migration, and current country of residence—but not schooling. To estimate program impacts on international migration itself, we incorporate the migrants into the individual-level census sample by assuming they were born in the municipality where the household from which they migrated is located in 2013.[10]

An implication of targeting areas with the highest malnutrition rates in the country was that PRAF-II operated in regions with a high share of indigenous people. While the indigenous in Honduras comprise only 6.5 percent of the national population, they make up 39 percent of the main analysis sample (individuals ages 6–29 in 2013 and born in one of the 70 program municipalities). We classify as indigenous all individuals who identify themselves in the census[11] as indigenous, Afro-Honduran, or black—95 percent of whom in the sample are Lenca.[12]

Given randomized assignment and results in appendix C that provide evidence of balance on observables across treatment arms using data from the 1988 and 2001 population censuses, our main methodological approach is to estimate a single-difference ITT model

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

where Y_{ij} is the outcome 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 yields the estimate of the long-term ITT absolute impact of past program exposure. To adhere to the experimental design, we control for the other treatment arms with indicator variables for whether municipality *j* benefited from both the CCT

[10] While the census data contain urban or rural designation for current residential location, they do not include it for location at birth, so our original analysis plan to examine the effects of the CCT separately for urban versus rural origin locations is not possible. Instead, we distinguish between the indigenous and non-indigenous populations as described below. 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. The analysis in this paper follows the research strategy outlined in that proposal, although we did not pursue analysis of Demographic and Health Survey data since it does not contain location of birth. In addition, we added analysis of spillovers to other age cohorts and added a control for the baseline outcome variable in each main specification, which in general results in more conservative estimates (see appendix A).

[11] A potential concern is that the CCT 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, Lenca, do not speak a different language from the rest of the population as is common for several indigenous populations in other countries in Central America. For females and males, we fail to reject the null that the probability of reporting as indigenous is unrelated to treatment status (*p*-values of 0.640 and 0.622, respectively). All 70 municipalities have both indigenous and nonindigenous populations.

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

and supply-side interventions simultaneously (G_{2j}) or the supply-side interventions only (G_{3j}). Following Athey and Imbens' (2017) recommendation to use limited and binary controls when analyzing randomized experiments, X_{ij} includes indicator variables for the five strata used in randomization, single-year age fixed effects, and when available an indicator for whether the average value of outcome Y in municipality j for individuals born in the municipality and ages 20–25 in 2001 is above the median of the municipality-level averages for all 70 municipalities.[13]

Robust 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 G_4, the control), we also replicate all hypothesis tests using randomization-based inference tests (Athey and Imbens, 2017; Young, 2019). 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 all treatment effects are zero 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-based inference provides exact finite sample test statistics without appealing to asymptotic results and as such allows testing for the influence of potential outliers and protects against accidental imbalance affecting the results.

For the main analyses, we estimate ITT effects for several different age cohorts, whose selection is informed by the design and timing of the CCT. The program operated for five years (2000–2005) targeting households with pregnant women and (initially) children under three (extended to under five starting in 2003) and school-age children ages 6–12. Consequently, individuals in treatment municipalities were potentially exposed to different program components in part or in full depending on when they were born. For example, only a child born in 2000 could have directly benefitted from the nutrition and health component for the full five years. Children born a few years earlier or later, however, would have had less exposure to that

[13] While the program had been in place for eight months by the time of the 2001 census, grades attained for the cohort of individuals ages 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, see appendix C). At the same time, this cohort is young enough to reflect recent general secular differences in schooling in the program municipalities. Figures A.1 to A.4 replicate estimates for highest grade attained without controlling for 2001 municipality-level education and demonstrates that, if anything, the control leads to more conservative estimates. We deviate slightly from Athey and Imbens' (2017) recommendation by not including an interaction effect of each of the binary control variables with the treatment indicators because of the limited number of municipalities assigned to each treatment arm and the large number of interactions effects this would introduce.

component, such as a child born in 2003 who could have directly benefited from the nutrition and health component for only two years postnatal before the program ended.[14]

Turning to the education component, any child 6–9 years old in 2000 could directly and fully benefit from first through fourth grade. Children older than that in 2000 also potentially benefitted, and possibly even more so if the program affected them at ages at which they otherwise might have started to drop out of school. In Figure 1, we use the short-term program evaluation data to characterize average preprogram enrollment rates for girls and boys at baseline. Patterns by age are broadly similar across the municipalities subsequently exposed to the CCT and those in the control, providing further evidence of strong balance. For both sexes, enrollment rates are above 90 percent until about age 11 after which they decline considerably. Consequently, individuals 11–13 years old in 2000 were at higher risk of dropping out when the program started; similarly, those 6–10 years old were at risk of dropping out at some point during the five years the program was in operation. Finally, individuals 14–16 years old in 2000 would not themselves have been eligible for any transfers, but nevertheless may have benefitted from transfers received by their households at ages in which their risk of dropout was high, or through peer effects.

A re-analysis of the short-term program evaluation data confirms that after two years of exposure, not only cohorts directly exposed to the education conditionalities but also the older 14–16 year-old cohort (in 2000) experienced significant educational gains. Table 1 presents the ITT estimates of G_1 program impacts on highest grades attained after two years. It uses the 2002 follow-up data for all children living in baseline households that by 2000 had reached primary-school age. While children with uninterrupted annual grade progression would have finished primary by ages 14–16 years, in practice more than half of the approximately 30 percent of children in this age cohort enrolled in 2000 were still enrolled in primary school (and a quarter still in the first four grades), illustrative of the accumulated schooling delays common in poor rural Honduras (Glewwe and Olinto, 2004). This also means that those children potentially would have been in the same classrooms as much younger children directly affected by the education component and conditionalities. In addition, exploration of whether effects for this ineligible cohort are concentrated in households with younger eligible siblings, via an interaction with the G_1 indicator, reveal they do not seem to be. Thus, the impact on this older cohort is in line with evidence of peer effects on ineligible children in *PROGRESA* (Bobonis and Finan,

[14] Of course a child born in 2003 also may have benefited (indirectly) in utero.

2009; Lalive and Cattaneo, 2009) and indicates important short-term spillovers existed for this older non-targeted age cohort.

Within this context, we use the patterns of partial or full exposure to define a set of age cohorts for analysis 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 outcome measurement, and potential number of years of exposure. In addition to the age cohorts described above, with the census we can extend the window and include younger cohorts. Since outcomes are measured in 2013 and we include estimates for children born after the start of the program in 2000, going forward we report ages in 2013 as shown in the bottom row of Figure 2. In the main analyses, we estimate the impact of the basic CCT (β_1) separately for each age cohort. To verify that the significance of results is 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 (2019) omnibus randomization-based inference test.

Given that the experiment included three treatment arms and we estimate effects for as many as eight different age cohorts for a single outcome, for each demographic group we alternatively estimate a model combining individuals from all age cohorts relevant to the outcome being examined and directly test 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 *C* included cohorts. The age cohort indicator variables are also each interacted with assignment to treatment (and all other controls) yielding:

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

For each demographic group we then implement a parametric F-test for the null hypothesis that there are no cohort-specific treatment effects in the pooled specification (Chetty, Hendren, and Katz, 2016). 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 all of the estimated G_1 treatment effects. For outcomes such as grades attained, this corresponds to the eight age cohorts shown in Figure 2. This single test on the pooled sample is not vulnerable to over-rejection rates that occur when analyzing the individual age cohorts separately. We also use Equation 2 to directly test whether impacts on grades attained differ between the cohorts with full exposure to the education transfers (19–23 years old) and with full exposure to the nutrition and health components (11–12 years old).

Finally, before presenting the results, we assess three selection concerns for individuals observed in the 2013 national census that might affect internal validity related to fertility, mortality, or possible undercoverage. First, as described above unintended incentives created by program eligibility rules have been linked to short-term increases in fertility in treatment areas. This is most likely for those born in the first few years of the program (i.e., ages 9–12 in 2013), after which the rules were modified. Second, if exposure to the nutrition and health components of the program reduced infant mortality, differential mortality rates are possible. Plausibly, both these forms of selectivity would increase relative cohort sizes (e.g., the ratio of children under five born to women of childbearing age) in treatment areas. We examine this possibility and find that the differences between relative cohort sizes in G_1 and the control are small and insignificant for all four demographic groups (appendix Table C.5) suggesting that ITT estimates on other outcomes are unlikely to be strongly affected by fertility or mortality selection.

A further potential concern is that census coverage is incomplete, with selected individuals omitted from the census for unknown reasons and possibly differentially across treatment arms. To analyze this possibility, we compare cohort sizes for those born in the 70 municipalities in the 2001 and 2013 censuses. For this exercise, we use 5-year cohorts to help neutralize differences due to age-heaping (West and Fein, 1990). Appendix Figures C.1–C.4 present the data, which include the reported international migrants. The first finding for both censuses is that within each census younger cohorts are for the most part larger than older ones, consistent with the growing Honduran population (and manifested in the empirical work below by larger sample sizes for younger cohorts), and this pattern is stronger for the indigenous populations. The figures also track cohort sizes over time, comparing the size of each cohort in 2001 with the same cohort of individuals reported 12 years later in 2013. For the 16 to 35 year olds, there are no large differences in cohort sizes between 2001 and 2013 for the four demographic groups (comparison of dark grey with blue bars).[15] Differences are minimal and not systematically positive or negative for indigenous and non-indigenous women, or for indigenous men. They are a bit larger for non-indigenous men, however, possibly because of higher mortality rates (consistent with high levels of violence in rural Honduras), failure of the 2013 census to capture

[15] For those below 16, 5-year old cohort sizes are not comparable between 2001 and 2013 as they were mostly born after 2001. We therefore also show the 12-15 year cohort separately. For the indigenous, the reported population size in 2001 using 2013 age is lower than in 2013, possibly due to underreporting of newborns in 2001.

a subset of young male adults living in Honduras[16], or underestimation of international migration for this demographic group. Crucially for our analyses, however, none of the 2001–2013 differences are significantly correlated with treatment status, further alleviating potential selection concerns.

4. Results

In the presentation of the results, we focus discussion on the long-term impacts of the basic CCT (G_1), captured by β_1. Appendix D presents impacts for G_2 and compares them with those of G_1. All ages are in 2013. All significant effects reported are robust to randomization-based inference. Specifically, randomization-based inference tests yield *p*-values and significance levels that are similar to the results obtained using regression-based inference tests accounting for clustering at the municipality level and all of the statistically significant point estimates reported in the figures and discussed in the text are also significantly different from zero under both methods of randomization-based inference suggested in Young (2019) (see appendix Tables A.3–A.6 for results not reported in the main tables). Appendix Table A.7 reports the *p*-values from omnibus tests that combine estimates for all cohorts and outcomes examined for each demographic group, confirming the overall significance of the findings for each group.[17]

4.1 Education

Table 2 presents the long-term impacts of the CCT on highest grade attained (defined as grades of completed schooling) by cohort and separately for females and males. For both sexes, there is clear evidence of an impact on the older cohorts fully or partially exposed to education transfers as children. Individuals 19–26 years of age in 2013 have between 0.31 and 0.43 more grades attained (compared with control group averages of about six grades or lower). While positive, effects for the younger cohorts exposed to the nutrition and health components or born after the program ended are mostly insignificant.

At face value, these results suggest only minimal long-term effects on grades attained from early life exposure to the CCT. This would be a somewhat surprising conclusion, however, given the evidence on short-term effectiveness of this program for young children (Morris et al., 2004)

[16] Undercounting, in part due to under-enumeration of mobile young men living in single-person households, has been documented in censuses across the world (Ewbank, 1981; Philips, Anderson, and Tsebe, 2003).

[17] 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, confirming significance for each of the families.

and recent growing evidence in other contexts of the importance of investment during this early period of life. It may be that other constraints inhibit translation of the short-term effects into later improvements in education for part or all of the population. To explore this further, we consider a second potentially important dimension of heterogeneity—ethnicity. In addition to being predetermined, indigenous identity in this context may be a proxy measure for a combination of additional constraints faced by individuals during or after the program.

Figures 3–12 present impacts on educational outcomes for the relevant outcomes and the four demographic groups of interest: females and males with and without indigenous backgrounds. Each figure shows the ITT effects (equation 1) of the basic CCT for individuals born in G_1 municipalities, on the outcome of interest by age cohort. Point estimates are represented by blue dots with corresponding 90 (blue dash) and 95 (red square) percent regression-based confidence intervals. Figures show ages at the time of measurement in 2013, 13 years after program began, as illustrated in Figure 2. Each figure also shows the average value of the outcome in the control group (G_4 municipalities) in parentheses on the x-axis label below each age cohort.

Non-indigenous Females

Figure 3 presents the estimates of the ITT long-term impact on grades attained for non-indigenous females. The cohort that benefited the most based on the point estimates was 19–23 years old in 2013, i.e., those with the longest potential exposure to the G_1 transfers during school-going ages. Their highest grade attained increased by more than 0.5 grades (a significant increase of nearly 10 percent). 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 cohorts. The latter include those not yet born during the program and those too old to have benefited directly from the education component. They also include girls (ages 16–18) too old at the start of the program for the nutrition and health package and too young to have directly received the full educational transfers, who in a sense fell into a gap of program coverage in the initial design.

These gains in grades attained for non-indigenous females are reflected in much higher completion rates for different levels of schooling. Figure 4a presents the estimates on the probability of: (1) completing fourth grade (top-left); (2) completing primary school (i.e., sixth grade, top-right); (3) completing secondary school (i.e., 12^{th} grade, 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 room for improvement as revealed by the control group. For the two youngest cohorts, the CCT also increased the probability of being enrolled in 2013 by 4.7 percentage points or more (Figure 5). Non-indigenous females in G_1 municipalities were starting school earlier eight years after the CCT had ended and after households had stopped receiving transfers. This pattern is plausibly due to improved nutrition and health earlier in life but may also reflect changes in norms of schooling in these municipalities.[18]

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 (nearly 30 percent) and 6.7 percentage points (about 10 percent), respectively. Smaller positive but insignificant effects on completing primary education are observed in the older cohorts. Those 16 years or older are more likely to have completed 12 years of schooling, with an effect of 1.3 percentage points in the 16–18 year-old cohort and approximately 3.5 percentage points in the older cohorts (roughly 30 percent). Finally, among non-indigenous women old enough to have begun university, those in the 19–23 and 24–26 year-old cohorts, both of whom were 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 of 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. We also find significant spillover effects on current enrollment for the cohort born after the program ended, as well as some spillover effects on completing four or 12 years of schooling for the oldest cohort who were too old at the time of the program to be eligible themselves, indicating that gains seen in the short-term evaluation for this cohort

[18] These results also raise the possibility of spillovers on even younger cohorts. As these cohorts would not yet have reached primary school, we cannot analyze impacts for them on the same outcomes. Assessment of impacts on enrollment in preschool for the 4–5 year-olds in 2013 indicate, however, that there are positive and marginally significant effects for non-indigenous and indigenous boys (not shown), in line with the CCT having lasting effects on future generations.

persisted. We reject (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 from equation 2). With the exception of the youngest cohorts, however, Figure 5a shows no other enrollment effects in 2013.

Indigenous Females

In contrast to the non-indigenous, there are few long-term ITT impacts of the CCT on grades attained for indigenous women (Figure 6). The exceptions are for the two oldest cohorts, where there is an effect of approximately one-half a grade. Turning to 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, there are positive and significant effects on the probability of completing fourth grade, ranging from 3.3 to 9.3 percentage points. Moreover, there are large spillover effects (9.0 percentage points) for the oldest cohort.[19] Finally, we estimate positive and statistically significant effects on the probability of completing secondary school and having reached university for the 24–26 year-olds. Figure 5b demonstrates that indigenous women between 16–23 years old are more likely to still be enrolled in school in 2013, suggesting grade differentials for them may increase further. In contrast to the evidence for non-indigenous girls, however, there are no significant enrollment effects for the youngest cohorts.

Overall, indigenous women exposed to education transfers at ages when they were at higher risk of dropping out of school benefited the most, followed by younger cohorts also exposed to education transfers. Indigenous women 24–26 years old 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. We reject (p-value < 0.001) the hypothesis that the estimated G_1 treatment effects for the eight age cohorts on grades attained are all equal to zero.

Non-indigenous Males

Results for males are broadly similar to those for females, with larger and more significant impacts for the non-indigenous. ITT 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 the nutrition and health components

[19] 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.

had higher grades attained (Figure 7a). The largest relative impacts are among the cohorts eligible for the education component, for whom we observe more than a half grade increase.

All cohorts except the youngest have significant differences in at least one of the specific schooling levels (Figure 8a). Yet, 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 (Figure 9a). There are positive and significant increases of 4.9–8.6 percentage points (Figure 8a) on the probability of completing fourth grade for cohorts that were eligible for the nutrition and health component and cohorts exposed to the education component. 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. The effect size ranges from a 4.7 percentage point increase for the youngest cohort to a 7.0 percentage point increase for the cohorts exposed to the education component. For the oldest cohorts, there is an increase of about 4 percentage points for completing secondary school (an increase of over a third). The probability of reaching university almost doubled, with men ages 19–26 0.9 percentage points more likely to have university studies. Moreover, the CCT also increases the probability of still being enrolled in school by about 1.5–2.5 percentage points for the 19–23 year-old cohort (Figure 9a).

Across the different outcomes, there are large spillover effects on the oldest males. The oldest cohort had nearly 0.5 higher grades attained (Figure 7a) and achieved higher levels of secondary school completion and starting university (Figure 8a), and is even more likely to be still enrolled (Figure 9a), with the size of the treatment effects similar to those for the younger 24–26 year-old 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).

We reject (p-value = 0.003) the hypothesis that the estimated G_1 treatment effects for the eight age cohorts on grades attained are all equal to zero.

Indigenous Males

In contrast to the non-indigenous males, ITT estimates on grades attained for indigenous males are small and insignificant (Figure 7b). This lack of impact is largely mirrored in the completion of different schooling levels for the various cohorts (Figure 8b), even without controlling for 2001 municipality-level average education (Figure A.4). In contrast to the results for indigenous women in the oldest cohorts, there are minimal long-term effects for indigenous men with the exception of starting university, where point estimates are significant for those ages 19–29 years. As with the other demographic groups, while the point estimates show that the probability of having university studies increased by only 1 percentage point, the relative size of the impact on starting university studies is large. Indeed, we reject the joint null of no G_1 treatment effects for all three age cohorts relevant for university studies (p-value = 0.006).

Putting the Education Results Together

Overall, the results for educational outcomes indicate positive and significant ITT effects of the CCT for non-indigenous females and males across different age cohorts, and for all levels of education. Exposure to the nutrition and health component, or to the education component, both lead to improvements. In addition, there are important spillover effects on those too young or too old to have been directly affected directly by the education conditionalities. In contrast, for the indigenous population, there are significant positive effects only for the subsample of women directly exposed to the educational component (although that effect is not statistically different from the estimated effect on the 11–12 year-old cohort exposed to nutrition and health), and gains for the males were limited to a specific group of older men who had reached university studies and comprise less than 1 percent of the population.

To interpret the differences between the indigenous and non-indigenous, we examine their respective educational distributions in the absence of the program. As can be inferred from the control means in Figures 1 to Figures 9a, the indigenous population, in control municipalities, is more likely to complete primary but not more likely to advance to secondary or university level.[20] It is therefore possible that the potential for increases was smaller for the primary schooling outcomes of the indigenous. That said, as the means in Figures 4 and 8 demonstrate, even for the indigenous there was substantial room for improvement in primary education outcomes, as (for instance) only 64 percent of the 19-23 year olds women (66 percent of men) had completed primary. Together with the more limited outcomes at higher education levels this suggests that other external factors may have limited the educational impacts of the program for the indigenous populations.

Finally, to provide additional evidence in support of the internal validity of the results, we examine the effects for two plausible placebo (older) age cohorts: 30–32 and 33–35 year-olds in

[20] While at first glance this relative advantage for the indigenous at lower education levels may seem surprising, this likely reflects much stronger selective outmigration by non-indigenous parents (including previous generations) than indigenous parents from these poor rural areas. Indeed while on average the non-indigenous in Honduras are more likely to complete primary than the indigenous (78 versus 74 percent in the 2013 census), the pattern is reversed for the rural population (72 versus 73 percent). This is in line with the overall lower geographic mobility of the indigenous population. The lower mobility could also help explain why the upper tail of the education distribution is thicker for the non-indigenous (as even among the remaining households, expectations of outmigration may still be higher).

2013. Education for individuals in these cohorts, who were 17–19 and 20–22 years old in 2000, was unlikely to have been influenced by the program.[21] We find no significant effects for the two cohorts, with one exception (Table 3). The probability of women (but not men) having completed four years of primary is slightly higher. As women in these age cohorts were more likely to have had children in 2000 and therefore be beneficiaries of the program themselves when they were younger, this may well point to reporting bias. Former beneficiaries could possibly use the conditionality related to the first four grades as a benchmark for reporting on their own education.[22]

4.2 Migration

Domestic Migration

As previously described, the ITT estimates above are not subject to selection bias 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 domestic migration (whether in 2013 the individual is living in a different municipality than the municipality of birth) and, separately, urban domestic migration (whether in 2013 the individual is living in an urban area in a municipality other than the municipality of birth). While these available outcomes do not capture all domestic migration (for example, to urban centers within the municipality of birth), as the 70 municipalities are predominantly rural they likely capture most substantive migration. Estimation of the long-term impacts for the pooled samples suggests the CCT reduced domestic migration by 4 percentage points for men ages 19–23 and 27–29 (Table 4).[23] The point estimate for women in the 19–23 age cohort is similar in size but not significant.

Figure 10 presents the CCT impacts on any domestic migration for the four demographic groups, and Figure 11 on migration to urban locations (outside the municipality of birth).

[21] This is in contrast to its more likely influence on migration or fertility, for example, which is why we do not consider tests for those outcomes as placebos.

[22] These two age cohorts were also less likely to still have been in their municipality of birth by the start of the program, having reached by 2000 ages with high mobility. This makes the ITT effects for them more difficult to interpret.

[23] 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 insignificant average effects may well mask that different types of individuals decide to leave or stay, and that decision could be affected by past treatment.

Domestic migration is common for the non-indigenous, with rates over 30 percent for men and about 40 percent for women in the oldest age cohort. Among the indigenous population, domestic migration is an order of magnitude lower. For example, only 2 percent of the indigenous sample of girls and boys under the age of 15 years were no longer living in their municipality of birth, and this rate increases to at most 8 percent in the oldest cohorts (ages 27–29). When considering effects by ethnic group, the only significant effects are that indigenous men ages 11-12 and 19–23 years are less likely to migrate to urban areas.

International Migration

While domestic migration is common, international migration is relatively rare. Understanding CCT impacts on international migration is nevertheless important, both to understand any remaining selection using the national census and because it is itself an important outcome. Table 4 shows that for the pooled older cohorts, men in the control are approximately 3 percentage points more likely to migrate abroad than women.[24] In these same age cohorts, there are also large positive effects on migration for men, but no significant effects for women. Figure 12 presents the impact on international migration for the four demographic groups. For non-indigenous men in the two oldest cohorts, exposure to the CCT doubles the probability of international migration (from 3 to 7 percentage points). F-tests for all cohorts indicate that the G_1 treatment effects are jointly significant for them (p-value = 0.052). Point estimates for indigenous men are positive and similar in magnitude, but insignificant. Taken together, the results indicate 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 findings on international migration raise the possibility that the long-term effects on education estimated in section 4.1 suffer from sample selection bias. If the CCT increases education and induces disproportionally more migration among the better educated, for example, the true effect on educational attainment may be underestimated. 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. Rigorous research on the selectivity of migrants from the region is rare, but recent work suggests migrants from Honduras are likely positively selected—with higher education levels (Del Carmen and Sousa, 2018).

[24] Migration is not captured in 2001 census, so 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.

The census does not provide information about the education levels of the international migrants that would allow more direct assessment of these potential biases, but we explore these possibilities indirectly. 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. 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 (or fourth grade). This result, together with the low overall levels, suggests it is unlikely that international migration leads to strong selection concerns in section 4.1. Nevertheless, it is a potential caveat for the educational outcomes for men in the oldest two cohorts.

Domestic versus International Migration

The relatively strong effects on international migration contrast with the lack of influence (and indeed negative point estimates) for domestic migration. In part this could reflect a substitution between domestic and international migrants (individuals induced by the program to migrate internationally may be those who in the absence of the program would have migrated domestically). More broadly, the results are consistent with a context in which returns to migration (possibly through education) are lower for domestic migration within Honduras than for international migration to the U.S. This is consistent with the literature suggesting positive selection of international migrants (Chiquiar and Hanson, 2005; Caponi, 2011; Del Carmen and Sousa, 2018), and also with the literature pointing to related non-educational factors driving domestic migration, such as marriage and fertility decisions (particularly for young women) (Thomas and Smith, 1998; McKenzie 2008) and risk diversification (Stark and Bloom, 1985).

Finally, note that the omnibus test for migration indicates there are overall treatment effects for both men and women; the latter appear to be driven in part by effects in G_2 (appendix D).

4.3 Marriage and Fertility for Women

In spite of the various impacts on education, there are no significant long-term effects of the CCT on marriage for women (Table 5). Exploration of the effects on fertility, however, yields mixed evidence. There is a significant increase in the probability of having a child during early teenage years (ages 13–15) for all women. For the non-indigenous, there is also an increase among those ages 16–18 and 24–26 years, with point estimates indicating about a 2 percentage point increase. In contrast, indigenous women from the oldest cohorts (ages 24–29) are about 2–4 percentage points less likely to have begun childbearing. The joint test of G_1 treatment effects on fertility for all five age cohorts in Table 5 indicates joint significance for both the non-indigenous (p-value = 0.043) and the indigenous (p-value = 0.010).[25]

While the reduction in fertility for older indigenous women is consistent with the findings of higher schooling for that group, results for non-indigenous women pointing to an increase in early teenage pregnancy are less readily understood. The data are not well suited to disentangling the underlying mechanisms, but other results in the literature offer possible explanations. Stecklov et al. (2007) found a short-term increase on fertility in PRAF-II, and if this change led to a change in social norms regarding fertility, it may have had longer-term consequences. Barham, Macours, and Maluccio (2019) show that CCT nutrition shocks related to greater food availability 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 results on early teenage fertility are driven by a group of girls who completed schooling earlier because of the program and therefore made earlier transitions to the next phase in their life cycle. More detailed micro-level analysis with targeted household surveys would likely be needed to better understand these results and adjudicate between the possible explanations.

4.4 Discussion and Interpretation

Municipality of birth versus 2000 location

Municipality of birth is the key information available in the Honduran census allowing estimation of the long-term effects in this paper. An important consideration, however, is the extent to which it accurately reflects actual residential location at program assignment in 2000. To approximate the extent of migration between birth and 2000 we use the 2001 census, collected eight months after program start, and estimate rates of domestic migration between birth and 2001. Rates are 1–17 percent, with higher rates for the non-indigenous, for older cohorts, and for women (Table C.4). This is consistent with labor market and marriage patterns (with women notably more likely than men to relocate upon marriage). Migration for the cohorts with early childhood exposure is at most 5 percent for all groups. And, while domestic migration between birth and 2001 appears slightly lower in treatment than in the control areas (consistent with the CCT having begun by then and possibly having induced some individuals and

[25] For men, we find a negative treatment effect for non-indigenous males ages 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). Male fertility is not available in the census.

households to remain in treatment municipalities), none of the differences are statistically significant.

We also examine the extent of movement between treatment and control groups (again using the 2001 census), a form of movement between birth and program assignment particularly relevant to the analyses and likely to attenuate ITT estimates. Movement from the control to G_1 is less than 1 percent for all but the oldest cohort of women, for whom it is approximately 1.4 percent. Movement from G_1 to the control is about half as large. These minimal percentages suggest relatively few subjects misclassified across treatment arms.

We conclude that the evidence on preprogram migration patterns suggests our ITT estimates might modestly underestimate program impacts, especially for the older non-indigenous cohorts.

Comparison of treatment effects between cohorts

In theory, estimated treatment effects for a wide range of cohorts allow comparison of impacts between cohorts. Many of the outcomes studied across the nearly 25-year age range are relatively strongly age-dependent, with some such as migration, university studies, or fertility more relevant (and with more scope to be affected) for older cohorts, and others such as current enrollment more malleable for younger cohorts. Grade attainment, however, is arguably a bit more comparable between cohorts and we therefore tested whether impacts differ significantly between the cohort fully exposed to the education component (19–23 year olds) and those directly targeted by the nutrition and health components (11–12 year olds). This test shows no significantly differences for any of the four demographic groups. At face value, this suggests that the nutrition and health component of the CCT program in Honduras was not less effective in promoting educational gains than the educational component.

Caution is warranted for this interpretation, however, as differences in treatment effects between cohorts (or lack thereof) may reflect differences in the trajectories of cohorts born at different times, as well as differences in the effects of the intervention on cohorts that otherwise experience the same environment. The mean outcomes of the control indicate, unsurprisingly, that in absence of the program there are differences in average grades attained for cohorts born at different times (11–12 year-olds in the control have on average 4 grades attained while 19–23 year-olds have 6 grades attained). Similar considerations affect other possible comparisons with grades attained, for instance, higher on average for the 16–23 than for the 24–29 year-olds). It is possible that such secular differences are the drivers of the size of treatment effects for different cohorts, rather than the differences in the effectiveness of the CCT program components for different age cohorts.

One obvious potential reason for secular differences would be the presence of subsequent programs affecting the cost or returns to education that affect distinct age cohorts differently. One of the most important candidate programs to consider in this context is the Integrated Social Protection Program (*Programa Integral de Protección Social* or PIPS) a follow-up CCT begun in 2006 in parts of the same region. PIPS could have affected the costs of education of the younger cohorts differently than that of the older ones. Incorporating controls for the presence of PIPS in the municipality in 2007, Appendix Figures A.5–A.8 show that the overall pattern of results are robust to controlling for PIPS, suggesting that at least this closely related program is not a main driver of PRAF-II treatment differences between cohorts.[26]

Further speaking to possible complementarities with other interventions are the results for the G_2 treatment. As described at the outset, while we focus discussion in this paper on the "basic" CCT treatment for PRAF-II in G_1, the randomized experiment also included other treatment arms, including one with demand- and supply-side incentives. Overall, the results for the G_2 treatment (appendix D) are qualitatively similar to those observed for G_1 though 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 program. That said, since few of the differences between G_1 and G_2 are significant, we do not put too much weight on the differences between them.

Labor market returns

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 benefits including increased lifetime earnings. The overall positive and statistically significant long-term impacts on education and international migration stemming from the Honduran CCT naturally lead to questions about whether there are any 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 are almost certainly related to labor force participation decisions,

[26] Because targeting of PIPS, as well as other later programs described in the appendix may well have been affected by the prior presence of PRAF-II, we do not use information on them in our main specification.

again particularly for women. An accompanying empirical challenge for exploring labor market outcomes is the complete lack of information on earnings and sparse information on labor force participation available in the census data, making it ill-suited to understanding labor market impacts.[27] Earnings data is available in the national annual labor survey in Honduras, collected in both rural and urban areas covering all of Honduras, which importantly also includes information on the municipality of birth. Acknowledging various caveats for analyses using such data (including that it is not representative at the municipality level), in appendix E we pool data from multiple rounds (2010–2016) to analyze impacts on labor market activities and earnings. We find no strong domestic labor market returns to the increased human capital engendered by the CCT for the older cohorts ages 19–26 years.

5. Conclusions

Since CCT programs began 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 individuals across many cohorts.

The experimental results indicate long-term gains in schooling among females and males of non-indigenous background who benefited at different ages from different components of the CCT. There are positive and significant impacts on completing primary and secondary education and reaching tertiary studies. More modest effects are seen for indigenous populations, although 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 reflect in part their different educational completion patterns, though also may lend some support to the notion that to achieve their intended objectives for indigenous populations, the design of CCTs needs to be culturally adapted and/or complemented with interventions targeting remaining constraints.

[27] The census captures economic activities only for the previous seven days. Given the highly seasonal nature of those activities in rural Honduras, this is unlikely to fully reflect labor market activities for the target population.

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. Such migration likely implied a substantial return for these individuals. Since international migration is relatively rare, however, the absolute effect in the overall population is not large. Nevertheless, the migration results from this first-generation CCT point to the possible 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.

The evidence in this paper stands out by demonstrating positive and robust impacts on educational outcomes for individuals across a 25-year 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 important spillover effects that need to be considered when analyzing the return on investment of a CCT. It also suggests that programs targeting only narrow age groups may miss important opportunities to improve human capital.

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. Of particular note is that they stem from a CCT with relatively modest transfers in comparison to most other programs for which there is evidence. 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. Short-Term Impacts of CCT (G_1) on Grades Attained

			G_1		G_1		G_1 x Sibling´s Voucher	
Age in 2000	**Obs**	**Mean G_4**	**Coef. (s.e.)**	**Exact P-value**	**Coef. (s.e.)**	**Exact P-value**	**Coef. (s.e.)**	**Exact P-value**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Females				
6-10 years old	1714	2.58	0.648***	0.001	0.624***	0.001	0.024	0.592
			(0.078)		(0.116)		(0.100)	
11-13 years old	1017	4.41	0.287***	0.026	0.585***	0.004	-0.424**	0.017
			(0.104)		(0.205)		(0.209)	
14-16 years old	729	4.60	0.253**	0.061	0.429*	0.504	-0.256	0.941
			(0.123)		(0.255)		(0.300)	
				Males				
6-10 years old	1696	2.34	0.577***	0.001	0.563***	0.001	0.020	0.635
			(0.111)		(0.142)		(0.124)	
11-13 years old	1102	4.12	0.533***	0.001	0.769***	0.001	-0.304	0.221
			(0.120)		(0.211)		(0.235)	
14-16 years old	899	4.19	0.439***	0.002	0.979***	0.003	-0.666**	0.092
			(0.128)		(0.292)		(0.289)	

Notes: Authors calculations using PRAF-II short-term evaluation data. Estimates in column 3 show the ITT coefficients of two-year exposure to G_1 (compared to the control). Columns 5–8 present results for a model including interactions between the G_1 treatment indicator and a binary indicator for whether there was at least one other eligible individual in the household. Robust standard errors clustered at the municipality level in parentheses. Exact p-values are randomization-t p-values following Young (2019). *** p < 0.01, ** p < 0.05, * p < 0.1.

Table 2. Long-Term Impacts of CCT (G_1) on Grades Attained

Age in 2013	Females				Males			
	N	**Mean G_4**	**Coef. (s.e.)**	**Exact *p*-value**	**N**	**Mean G_4**	**Coef. (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

Notes: Estimates show the ITT coefficient of five-year exposure to G_1 (defined as being born in a G_1 municipality compared to in a control municipality). Robust standard errors clustered at the municipality level in parentheses. Exact p-values are randomization-t p-values following Young (2019). Randomization-c p-values (not reported) are lower than the randomization-t p-values for all estimations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3. Placebo Tests. Long-Term Impacts of CCT (G_1) on Education Outcomes, Older cohorts

	Females						Males					
	Non-Indigenous			Indigenous			Non-Indigenous			Indigenous		
Age in 2013	**Mean G4**	**Coeff. (s.e.)**	**Exact P-value**	**Mean G4**	**Coeff. (s.e.)**	**Exact P-value**	**Mean G4**	**Coeff. (s.e.)**	**Exact P-value**	**Mean G4**	**Coeff. (s.e.)**	**Exact P-value**
30-32 years old		N=14538			N=7203			N=12611			N=6788	
Grades attained	5.13	0.077	0.740	4.17	0.243	0.238	4.45	0.283	0.155	4.23	0.224	0.432
		(0.219)			(0.204)			(0.199)			(0.267)	
Currently enrolled (=1)	0.05	0.004	0.442	0.04	-0.010*	0.112	0.03	0.007	0.272	0.03	-0.002	0.817
		(0.006)			(0.006)			(0.006)			(0.009)	
Four or more years (=1)	0.61	0.032*	0.111	0.52	0.073**	0.029	0.55	0.019	0.581	0.55	0.038	0.274
		(0.019)			(0.031)			(0.033)			(0.035)	
Completed primary (=1)	0.49	-0.005	0.830	0.36	0.019	0.516	0.45	0.021	0.423	0.41	0.060	0.143
		(0.025)			(0.026)			(0.028)			(0.040)	
Completed secondary (=1)	0.11	0.016	0.264	0.06	0.019	0.105	0.06	0.022	0.157	0.05	0.009	0.413
		(0.013)			(0.012)			(0.015)			(0.011)	
University studies (=1)	0.03	0.008	0.142	0.02	0.001	0.741	0.02	0.002	0.650	0.01	0.006	0.170
		(0.005)			(0.004)			(0.005)			(0.004)	
33-35 years old		N=13427			N=6668			N=11902			N=6341	
Grades attained	4.58	0.189	0.432	3.58	0.364	0.143	4.26	0.344	0.093	4.06	0.164	0.492
		(0.232)			(0.238)			(0.211)			(0.238)	
Currently enrolled (=1)	0.04	0.006	0.331	0.03	-0.008	0.362	0.03	0.008	0.247	0.03	-0.000	0.948
		(0.006)			(0.008)			(0.007)			(0.006)	
Four or more years (=1)	0.55	0.059***	0.010	0.44	0.079**	0.014	0.53	0.013	0.547	0.53	0.025	0.470
		(0.022)			(0.030)			(0.024)			(0.033)	
Completed primary (=1)	0.43	0.019	0.456	0.31	0.015	0.671	0.43	0.032	0.161	0.39	0.038	0.283
		(0.027)			(0.033)			(0.023)			(0.034)	
Completed secondary (=1)	0.08	0.017	0.135	0.04	0.019	0.114	0.06	0.019	0.202	0.04	0.009	0.337
		(0.012)			(0.012)			(0.015)			(0.008)	
University studies (=1)	0.02	0.003	0.651	0.01	0.010**	0.025	0.01	0.007	0.134	0.01	0.006*	0.100
		(0.005)			(0.004)			(0.005)			(0.004)	

Notes: Estimates show the ITT coefficient of five-year exposure to G_1 (defined as being born in a G_1 municipality compared to in a control municipality). Robust standard errors clustered at the municipality level in parentheses. Exact p-values are randomization-t p-values following Young (2019). Randomization-c p-values (not reported) are lower than the randomization-t p-values for all estimations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4. Long-Term Impacts of CCT (G_1) on Migration Outcomes

Age in 2013	Females				Males			
	N	Mean G_4	Coef. (s.e.)	Exact *p*-value	N	Mean G_4	Coef. (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 (0.027)	0.410	21,619	0.12	-0.022 (0.022)	0.356
International migrant (=1)	24,224	0.01	0.005 (0.005)	0.305	22,936	0.04	0.034** (0.014)	0.013
27–29 years old								
Domestic migrant (=1)	20,769	0.29	-0.021 (0.035)	0.552	18,263	0.23	-0.045* (0.026)	0.095
Urban migrant (=1)	20,769	0.18	-0.027 (0.028)	0.342	18,263	0.14	-0.018 (0.023)	0.446
International migrant (=1)	21,111	0.01	0.005 (0.004)	0.307	19,430	0.04	0.040*** (0.014)	0.004

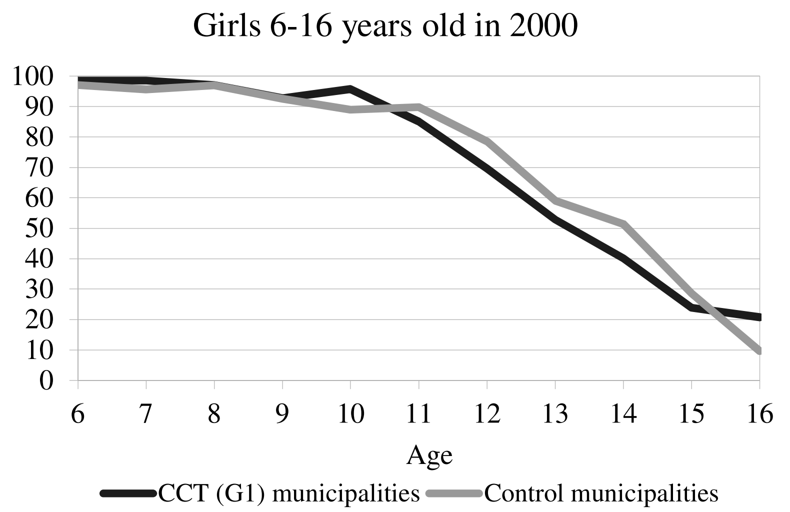
Notes: Estimates show the ITT coefficient of five-year exposure to G_1 (defined as being born in a G_1 municipality compared to in a control municipality). Robust standard errors clustered at the municipality level in parentheses. Exact p-values are randomization-t p-values following Young (2019). Randomization-c p-values (not reported) are lower than the randomization-t p-values for all estimations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

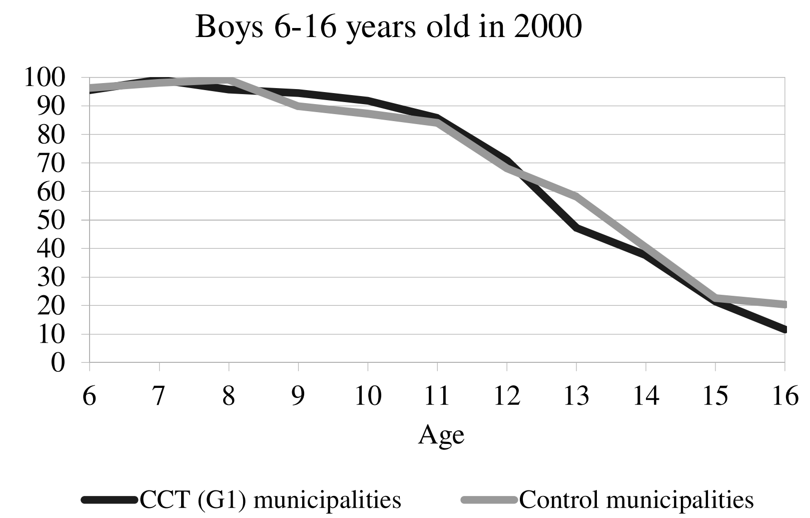
Table 5. Long-Term Impacts of CCT (G_1) on Fertility and Marriage Outcomes for Women

	All			Non-indigenous			Indigenous		
Age in 2013	**Mean G4**	**Coef. (s.e.)**	**Exact *p*-value**	**Mean G_4**	**Coef. (s.e.)**	**Exact *p*-value**	**Mean G_4**	**Coef. (s.e.)**	**Exact *p*-value**
13–15 years old									
Ever married (=1)	0.04	0.006	0.283	0.05	0.004	0.528	0.02	0.008	0.116
		(0.006)			(0.007)			(0.005)	
Child born alive (=1)	0.01	0.005**	0.035	0.01	0.005*	0.114	0.01	0.003*	0.109
		(0.002)			(0.003)			(0.002)	
16–18 years old									
Ever married (=1)	0.25	0.016	0.424	0.28	0.015	0.511	0.20	0.021	0.343
		(0.020)			(0.022)			(0.021)	
Child born alive (=1)	0.19	0.017	0.165	0.20	0.021*	0.055	0.17	0.011	0.589
		(0.012)			(0.011)			(0.019)	
19–23 years old									
Ever married (=1)	0.55	0.001	0.973	0.58	-0.005	0.835	0.49	0.007	0.776
		(0.020)			(0.022)			(0.023)	
Child born alive (=1)	0.58	-0.004	0.777	0.58	0.006	0.584	0.59	-0.022	0.414
		(0.014)			(0.011)			(0.025)	
24–26 years old									
Ever married (=1)	0.70	0.007	0.742	0.72	0.002	0.940	0.66	0.011	0.750
		(0.020)			(0.020)			(0.030)	
Child born alive (=1)	0.78	0.004	0.714	0.77	0.019*	0.103	0.81	-0.040**	0.039
		(0.012)			(0.012)			(0.018)	
27–29 years old									
Ever married (=1)	0.78	-0.006	0.764	0.80	-0.006	0.729	0.74	-0.001	0.980
		(0.019)			(0.017)			(0.027)	
Child born alive (=1)	0.86	0.001	0.907	0.86	0.008	0.364	0.87	-0.017	0.134
		(0.009)			(0.008)			(0.011)	

Notes: Estimates show the ITT coefficient of five-year exposure to G_1 (defined as being born in a G_1 municipality compared to in a control municipality). Robust standard errors clustered at the municipality level in parentheses. Exact p-values are randomization-t p-values following Young (2019). Randomization-c p-values (not reported) are lower than the randomization-t p-values for all estimations. *** p < 0.01, ** p < 0.05, * p < 0.1. Total N=212,785. See Table 2 for N by cohort and Table A.3 for N by demographic group.

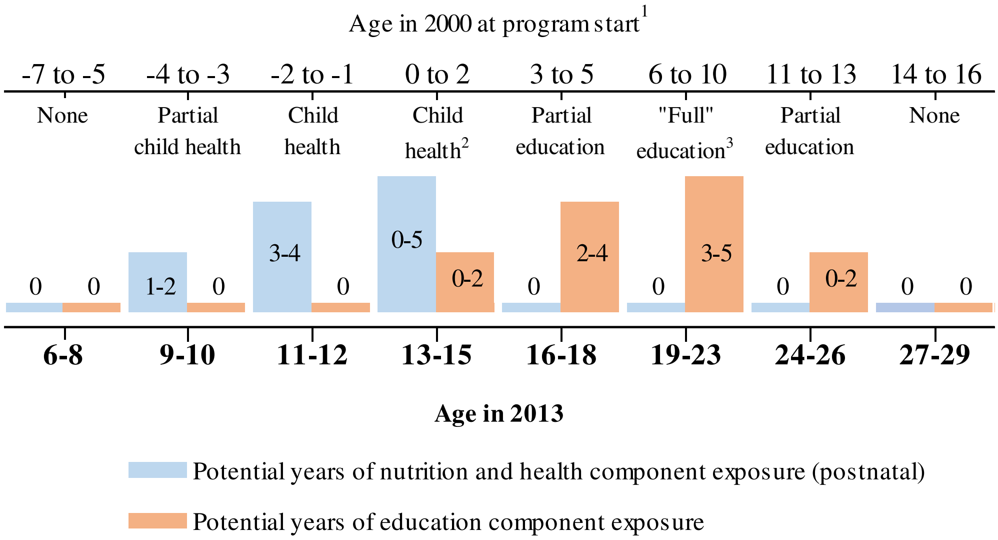
Figure 1. Preprogram Enrollment Rates by Age





Source: Baseline Data Short-Term Evaluation.

Figure 2. Age Cohorts and Exposure



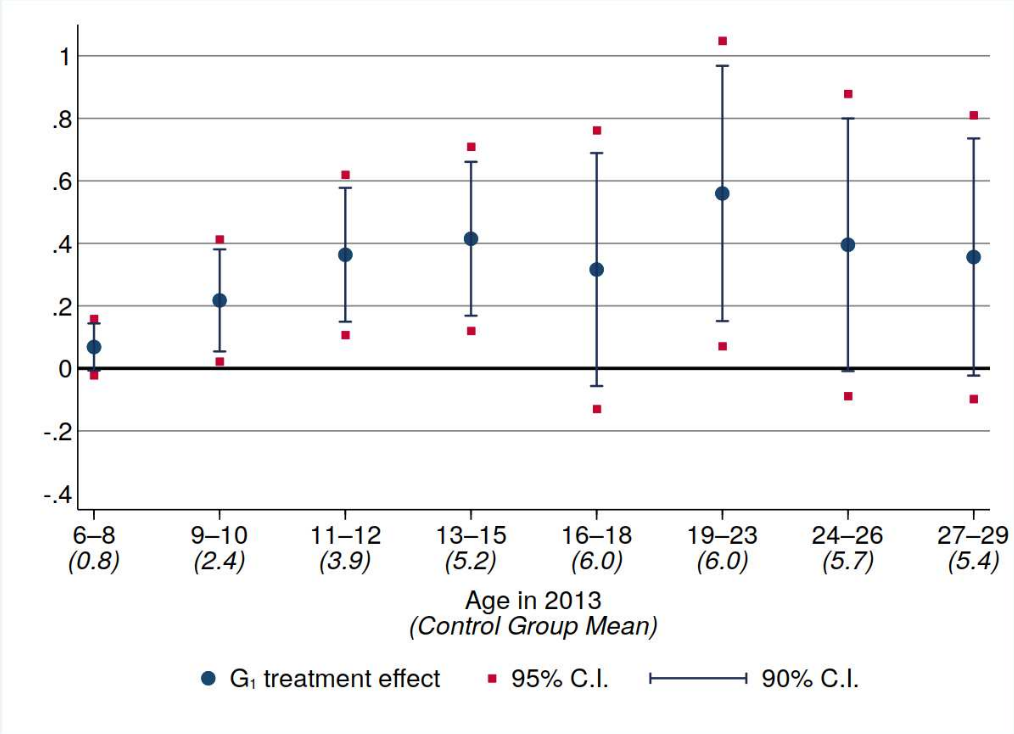
Notes: Exposures are approximate as they depend on birthdates (unavailable in the census) and age when the program started in late 2000. No eligibility criteria applied to the cohorts ages 6–8 and 27–29 years, but their households could have received transfers through eligibility of other members.

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. Children born at the start of the program or later were eligible for five years of the nutrition and health component, while children born before the start of the program were eligible for at most three years.

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



Notes: The figure shows the ITT effects of five-year exposure to G_1 (defined as being born in a G_1 municipality compared to in a control municipality) by age cohort, measured in 2013. Each regression includes strata fixed effects, single-year age fixed effects and a baseline proxy for the outcome measure calculated for 20–25 year-olds using the 2001 census. Robust standard errors are clustered at the municipality. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table A.3 shows sample size by cohort. Total N=143,007.

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

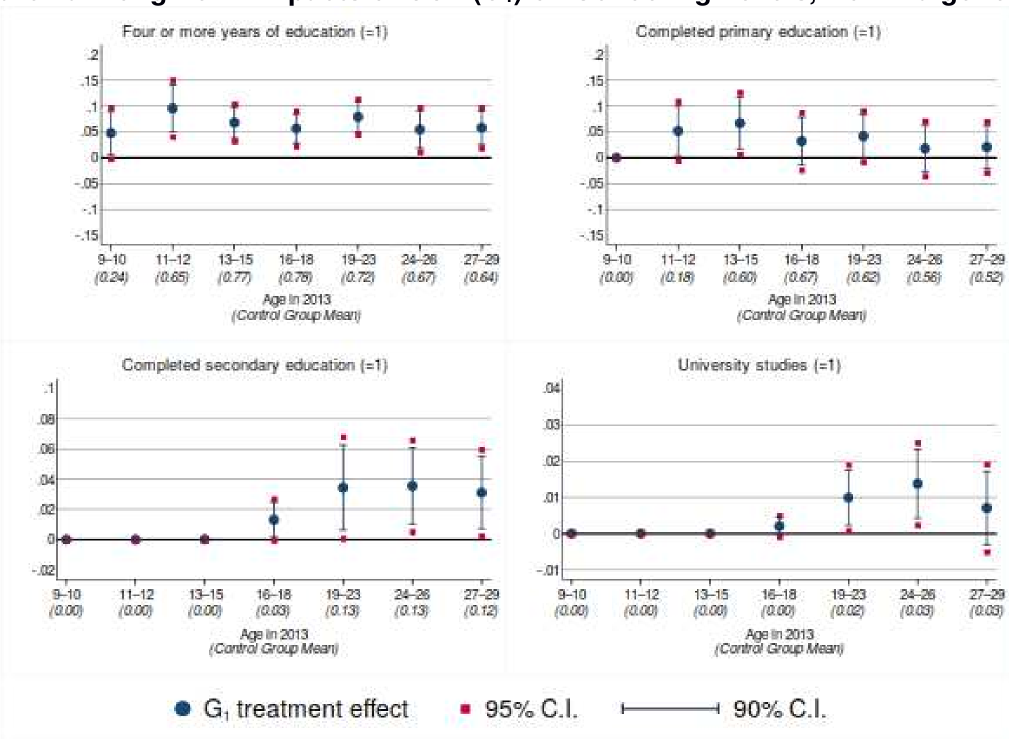
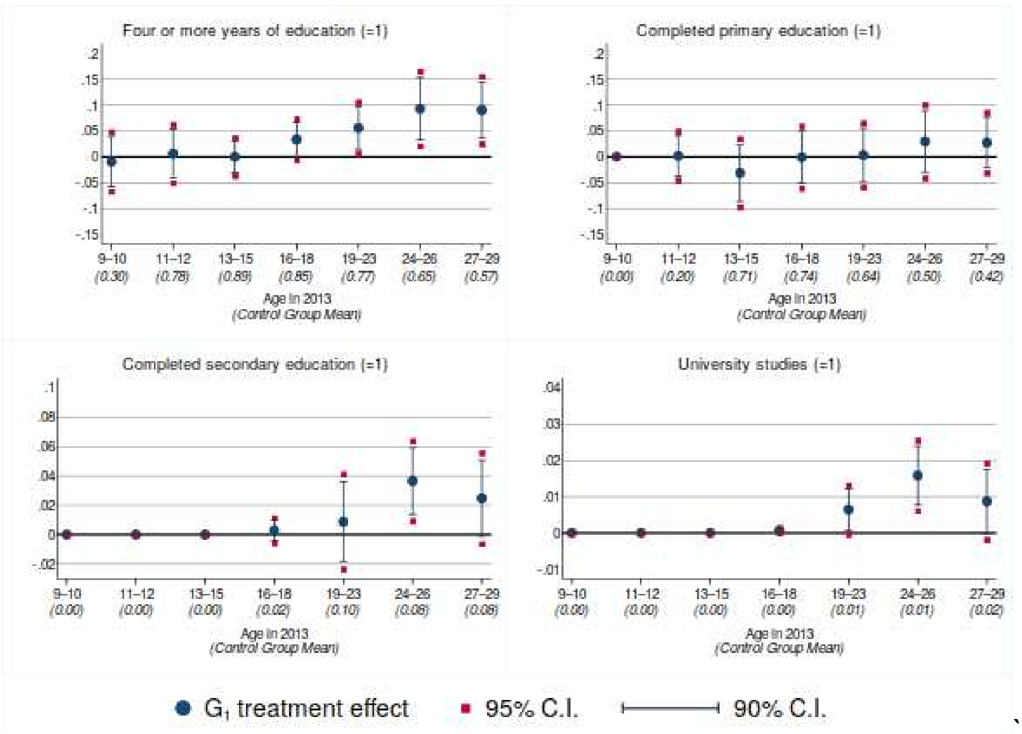


Figure 4b. Long-Term Impacts of CCT (G_1) on Schooling Levels, Indigenous Females



Notes: See Figure 3. Graphs do not include 6–8 year-olds as they are too young to have completed any of these 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

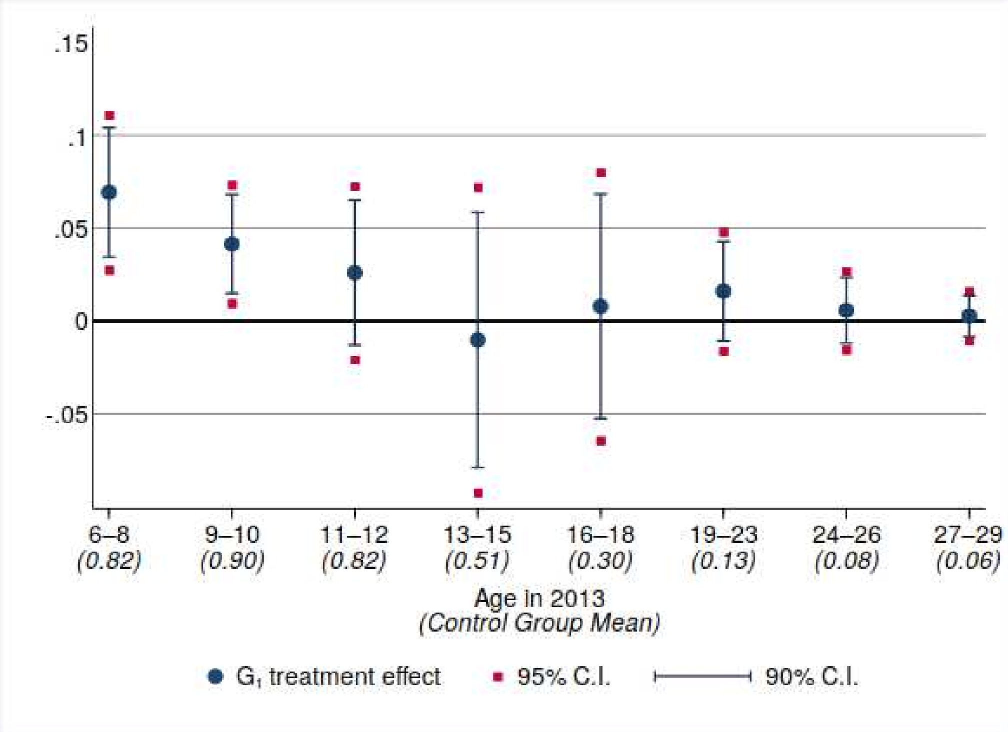
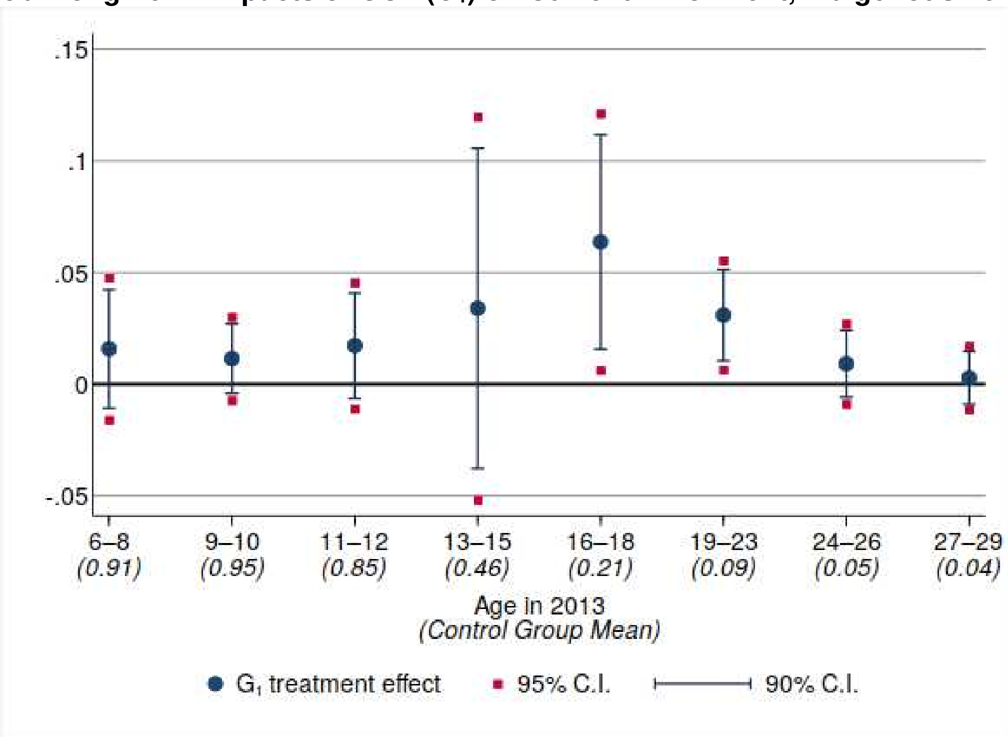
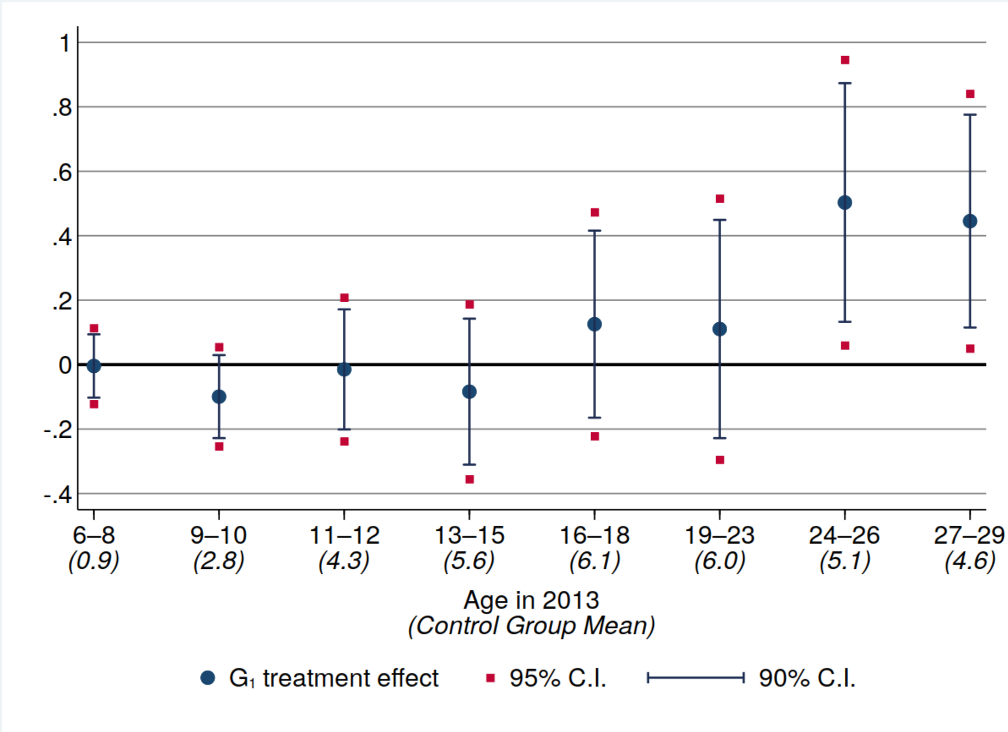


Figure 5b. Long-Term Impacts of CCT (G_1) on Current Enrollment, Indigenous Females



Notes: See Figure 3. N= 143,007 for Figure 5a and N=90,547 for Figure 5b.

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



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

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

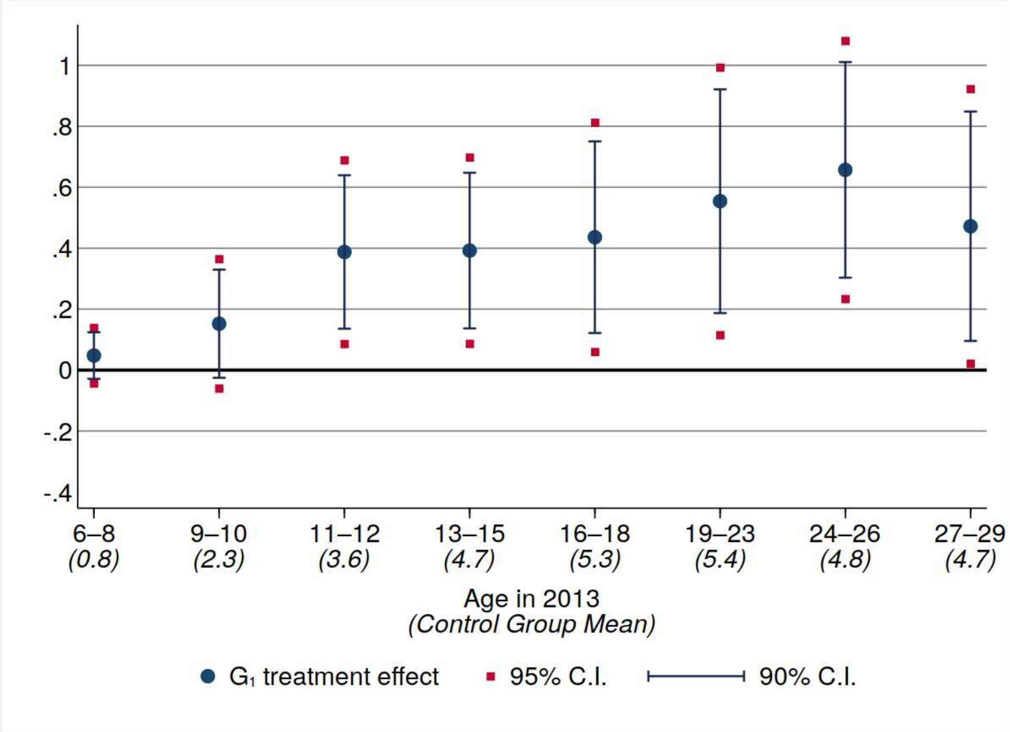
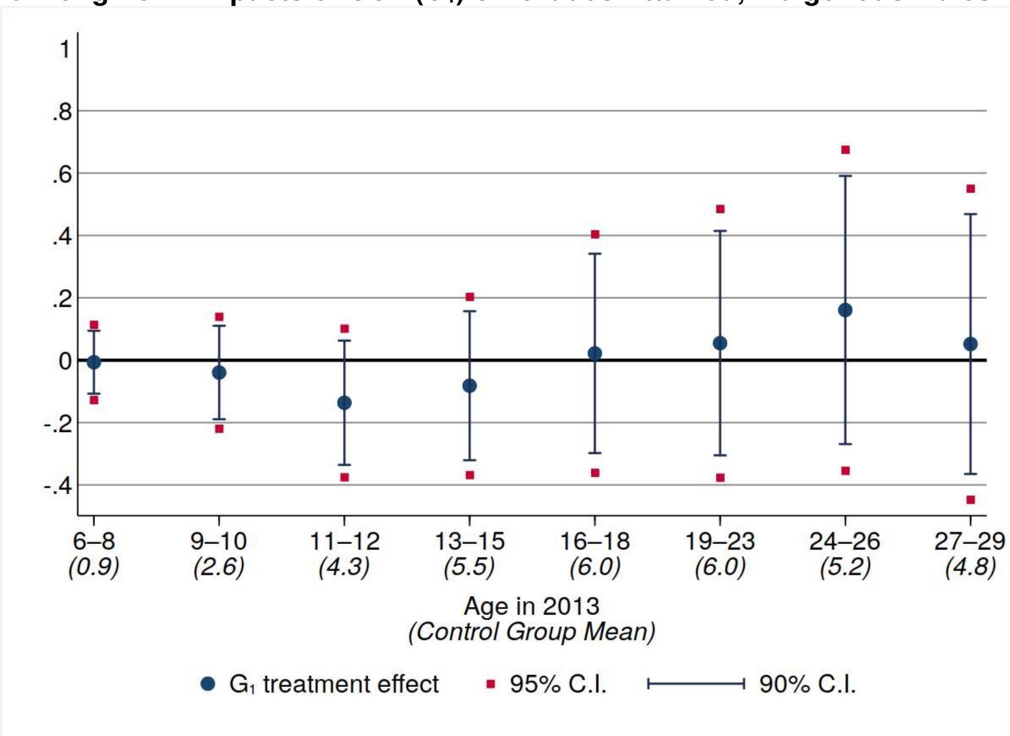


Figure 7b. Long-Term Impacts of CCT (G_1) on Grades Attained, Indigenous Males



Notes: See Figure 3. N=139,093 for Figure 7a and N=93,479 for Figure 7b.

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

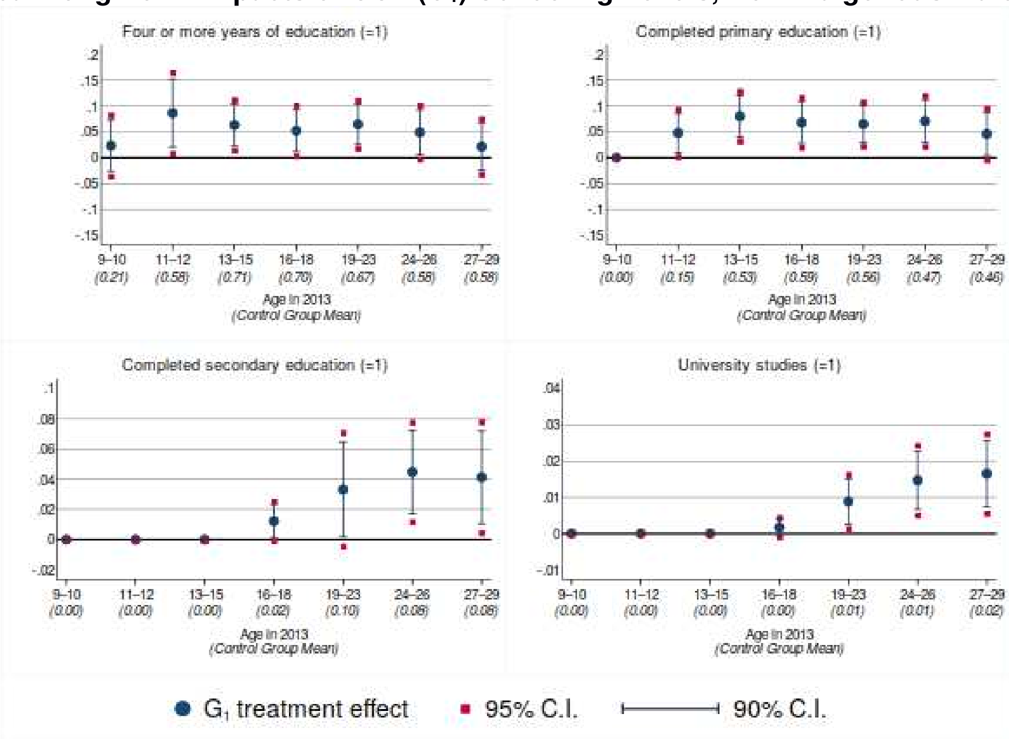
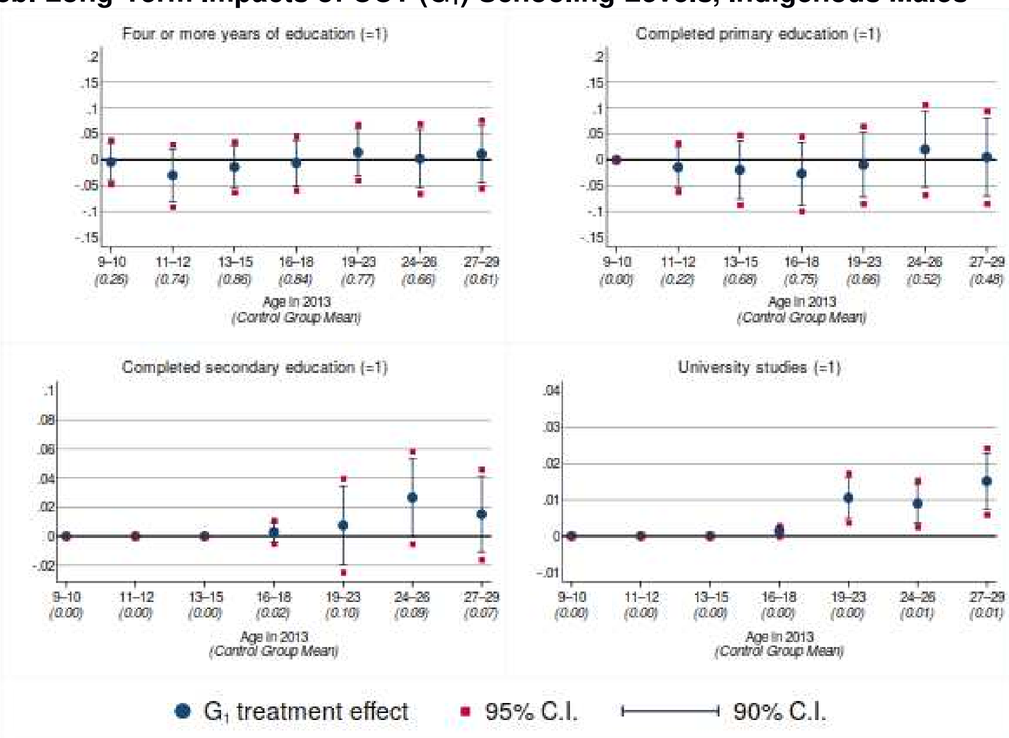


Figure 8b. Long-Term Impacts of CCT (G_1) Schooling Levels, Indigenous Males



Notes: See Figure 3. Graphs do not include 6–8 year-olds as they are too young to have completed any of these education levels. N=120,264 for Figure 8a and N=79,474 for Figure 8b.

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

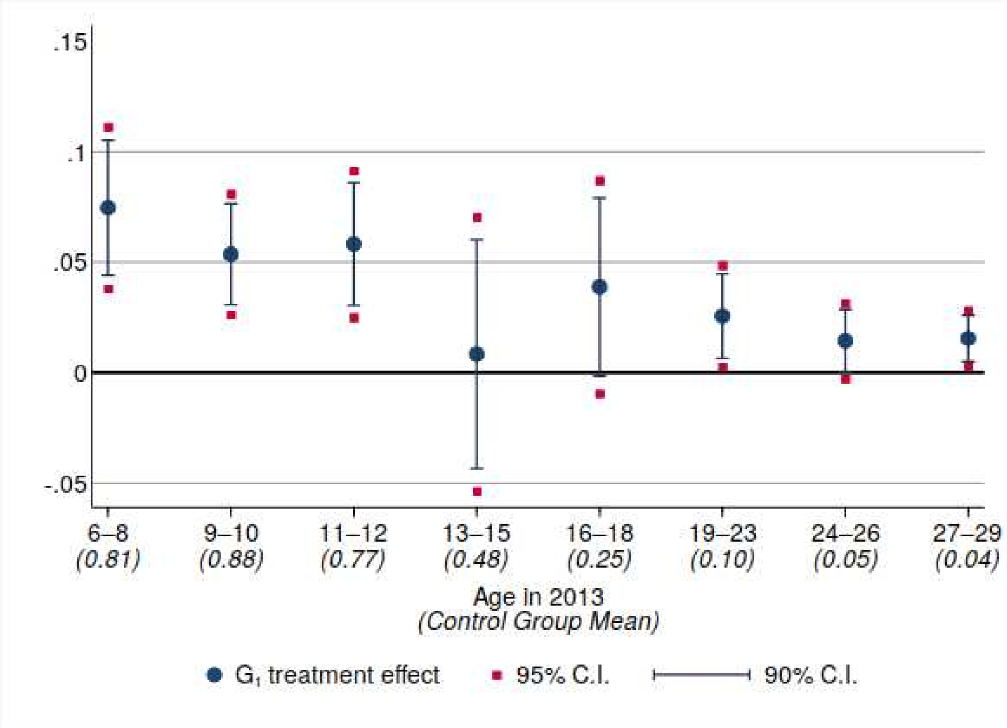
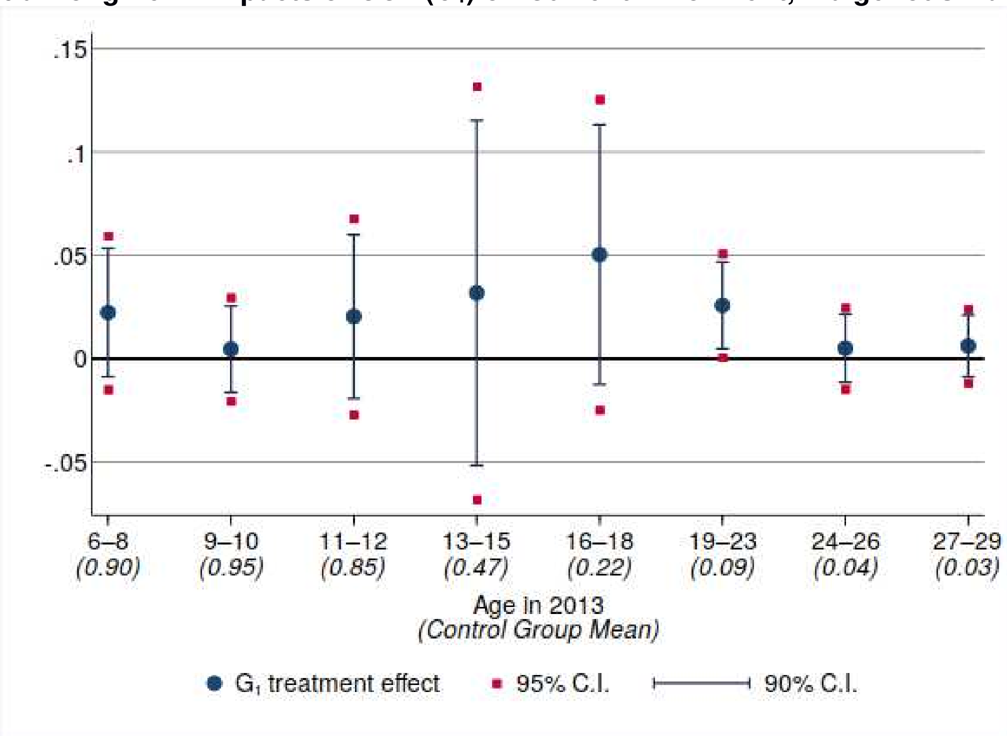
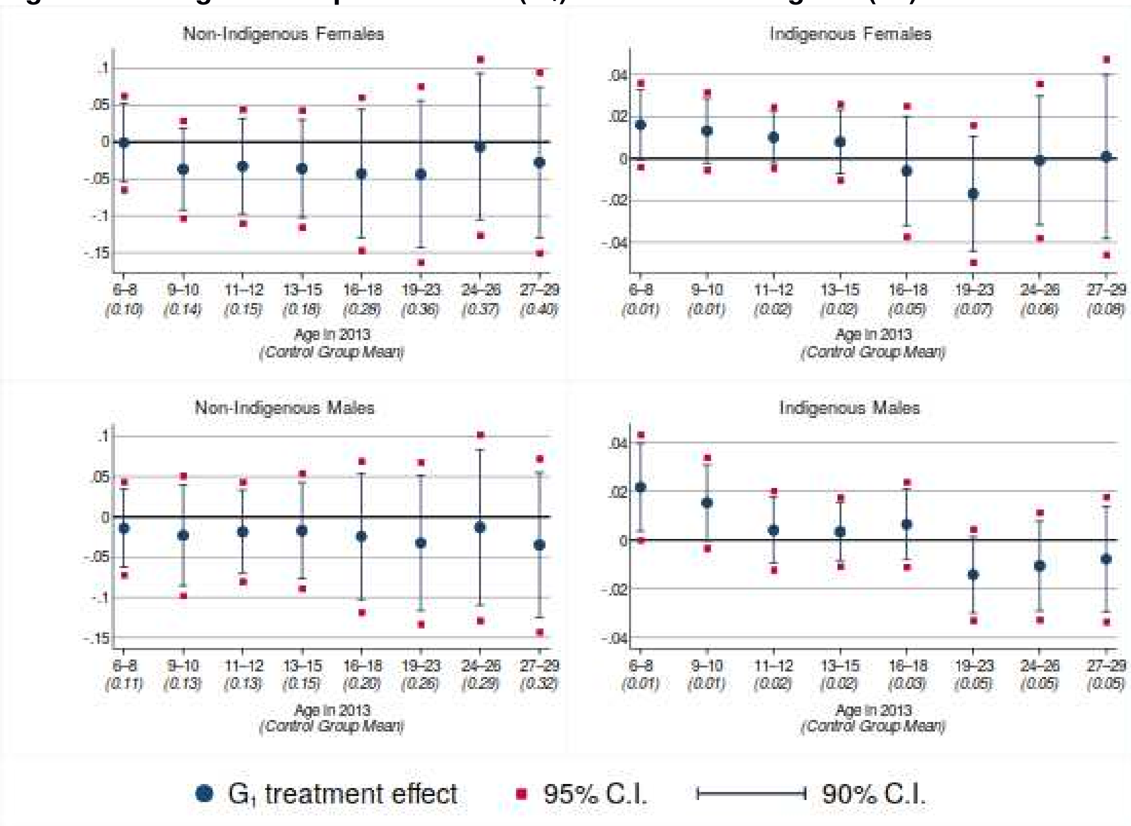


Figure 9b. Long-Term Impacts of CCT (G_1) on Current Enrollment, Indigenous Males



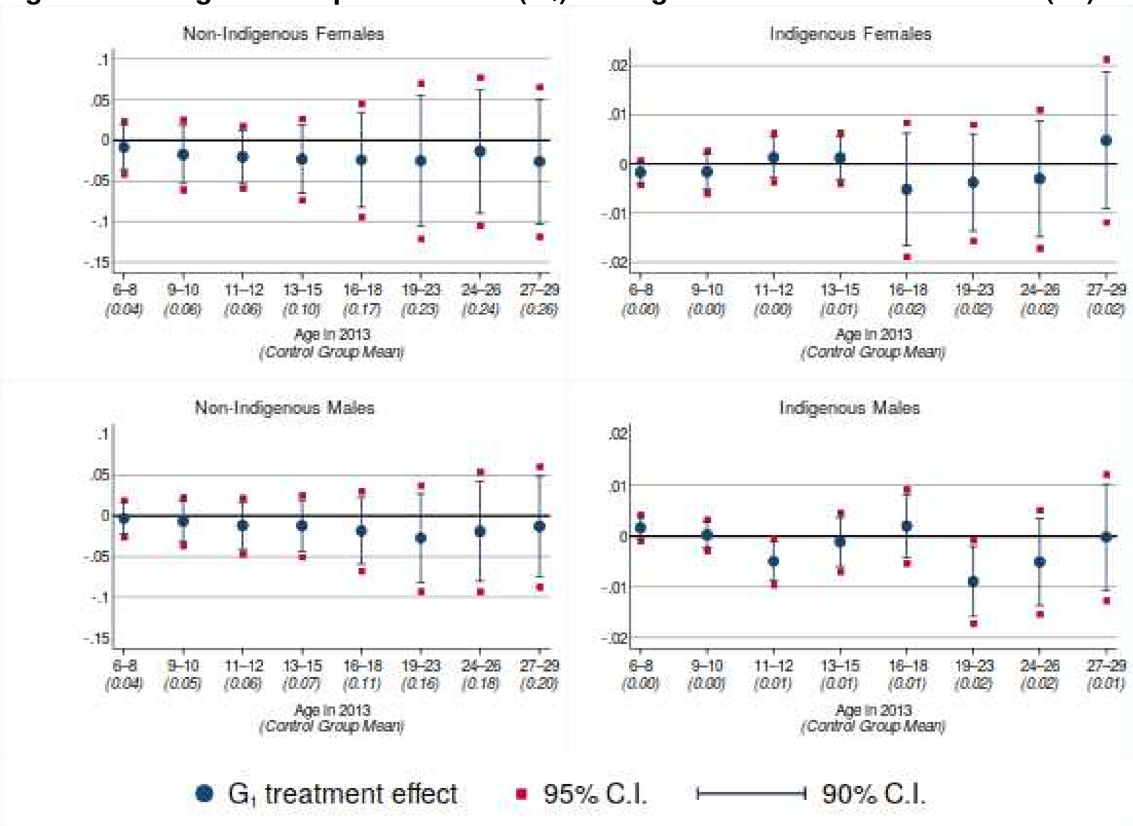
Notes: See Figure 3. N=139,093 for Figure 9a and N=93,479 for Figure 9b.

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



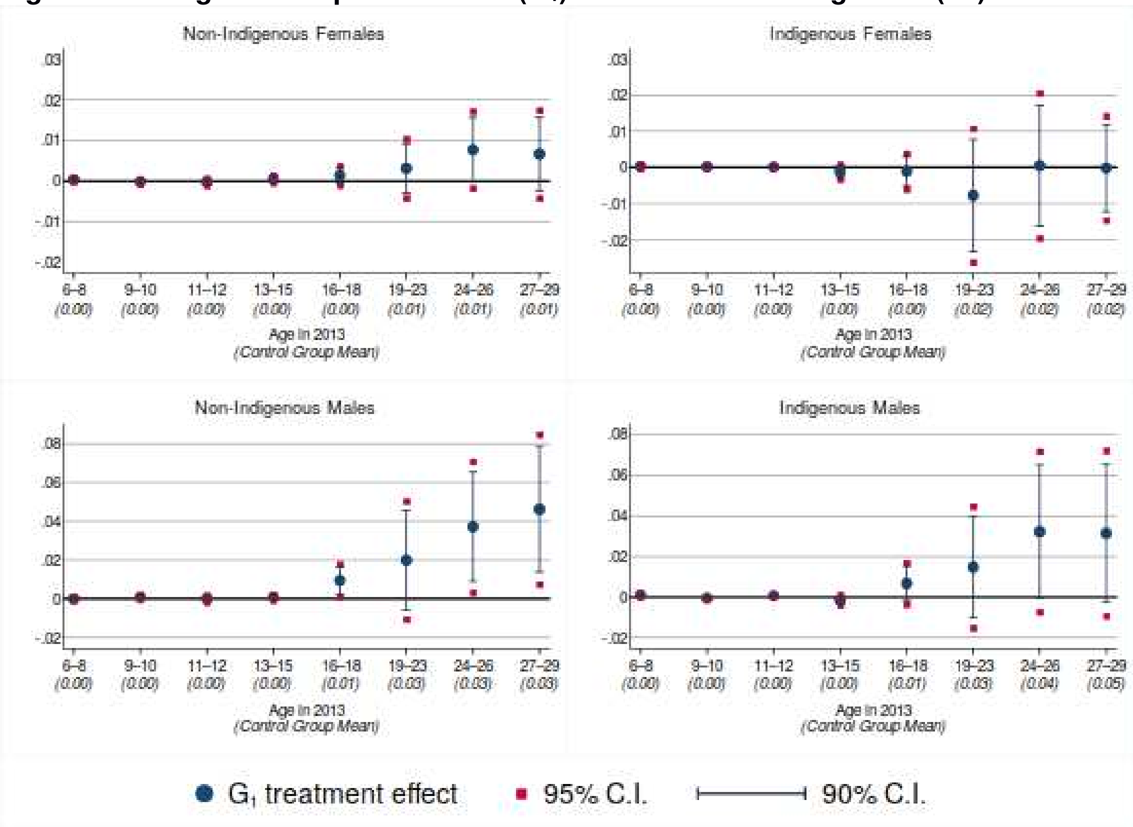
Notes: See Figure 3. For non-indigenous females N=143,007; for indigenous females N=90,547; for non-indigenous males N=139,093; and for indigenous males N=93,479.

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



Notes: See Figure 3. *Notes:* See Figure 3. For non-indigenous females N=143,007; for indigenous females N=90,547; for non-indigenous males N=139,093; and for indigenous males N=93,479.

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



Notes: See Figure 3. For non-indigenous females N= 143,833; for indigenous females N= 91,060; for non-indigenous males N= 142,222; and for indigenous males N= 95,137.