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ABSTRACT

We assess long-term impacts of the Mexican conditional cash transfer (CCT) program on youth employment and earnings. We rely on the original random assignment into early and late treatment localities, which introduced CCTs in 1998 and 2000. We focus on children between 7 and 16 years of age in 1997, who we follow up to 17 years later. Using the household surveys between 2003 and 2015, we find that those with greater time of exposure to CCTs had greater increases in educational attainment. Moreover, we find significant and positive impacts of the program on the likelihood and quality of employment.

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1. Introduction

PROSPERA, formerly known as OPORTUNIDADES and PROGRESA, was the first nationwide Conditional Cash Transfer (CCT) program to be ever introduced in the world back in 1996.[1] PROSPERA remains today one of the largest CCT programs around the world. The program initially covered 300,000 households and, by 2016, PROSPERA provided CCTs to the poorest 6.8 million households located in 116,601 localities from all 32 Mexican states (Secretaria de Desarrollo Social, SEDESOL, 2016). The Brazilian CCT, Bolsa Familia, has grown from 400,000 households to 11 million and is the biggest in sheer size in the world. For many years the Mexican and Brazilian CCTs were the only programs of this type around the world, but CCTs spread all throughout Latin America in the 2000s and have now spread to two dozen countries in all continents around the world.

CCT's provide monetary transfers to families below a certain income level, conditional on the children in the household attending school and conditional on the children in the household receiving immunizations and health check-ups. CCT programs were originally designed with the goal of achieving two key objectives. The first objective of CCTs is to provide a safety net by giving income support to poor families when they fall below poverty levels. The second key objective of CCT programs is to increase human capital and to eventually permit those who improve their educational and health status to achieve self-sufficiency. The Mexican CCT program, PROSPERA, has typically put more emphasis on the second goal of increasing human capital, while the Brazil's Bolsa Familia has put more emphasis on the safety net role of CCT's.

This is the first assessment of the Mexican CCT program that considers information gathered after 2007, and that follows beneficiaries for up to 17 years after they initially received

[1] The first CCT introduced at a local level was Bolsa Escola introduced in the Federal District of Brasilia in 1995 and later expanded to the rest of the country as Bolsa Familia in 2003.

the CCTs. This evaluation of the Mexican CCT, thus, contributes to the still small but growing literature on impact evaluations that examine the long-term effects of government programs in developing countries. The ability to examine long-term effects is crucial because it allows discerning if effects are long-lasting or dissipate and examining spillovers and impacts on additional outcomes as a result of the primary impact of an intervention. For instance, in our analysis we focus on the long-term impacts of CCTs on employment and earnings, instead of just the immediate effects on education and health.

In this analysis, we exploit exposure to treatment at the individual level to identify the effects of CCT programs. Previous analysis of CCT's have exploited only the initial random assignment; comparisons between regions that adopt CCTs earlier or later; comparisons between those below and above the threshold index at which households qualify for CCT's, and matched comparisons to individuals not qualifying for CCTs according to observable characteristics. Our analysis, instead, focuses on those originally qualifying for PROSPERA according to their poverty status and compares those with greater and lesser exposure due to being younger or older within a given locality as well as between those, of a given age, with 2 more years of exposure due to being in localities which were randomly selected to qualify for PROSPERA in 1998 rather than in 2000. The exposure variable is, thus, the number of years that each individual was eligible to receive PROSPERA scholarships, taking into account the age, the poverty status of the household and the year when the locality started receiving the treatment. Our study focuses on children whose mothers were eligible to receive PROSPERA scholarships starting in 1998 and for as long as they fulfilled the established requirements. Thus, we focus on individuals between 7 and 16 years old in 1997 whose income was below the poverty line. We use data from the Household Evaluation Surveys (Encuestas de Evaluación de los Hogares, ENCEL) conducted in 2003 and 2007, which follow children and households who were in the original 1997 baseline survey. In addition, we use data from the Reevaluation, Recertification and Permanent Verification of Socioeconomic Conditions (Verificación Permanente de Condiciones Socioeconómicas, VPCS) Surveys, which collected information about these individuals from 2008 through 2015.

We assess the impact of the Mexican CCT program on education as well as on employment outcomes (including employment status, hours worked, type of contract, and receipt of non-wage benefits) and earnings. Data about most outcomes of interest was collected in 2003, and between 2007 and 2015. We find positive impacts of the Mexican CCT program on education, as well as on the likelihood and quality of employment. Specifically, we find positive effects of the exposure to the Mexican CCT program on years of education, on the likelihood of completing high school and on the likelihood of studying tertiary education. The average youth exposed to 7 years of the CCT program has almost 3 additional years of education compared to someone who never received CCTs. Similarly, the average person exposed to PROSPERA is almost 18 pp and 5 pp more likely to complete high school and go on to tertiary education compared to someone never exposed to the program. Thus, we find that the effects of PROSPERA are long-lasting.

Most importantly, we find that the effects of PROSPERA on education allow individuals to later gain access to more and better employment. We find a positive impact of length of exposure to PROSPERA on the probability of being employed, on hours worked per week and on various measures of the quality of employment (including having a contract, receiving non-wage benefits, and wages). The average person exposed to PROSPERA is 36.6 pp more likely to be employed, 6.6 pp more likely to have a contract and 2.3 pp more likely to have non-wage benefits compared to someone who never received CCTs. Moreover, the average person works 9 more hours a week and is paid 5 more pesos per hour compared to someone never exposed to the PROSPERA program.

In Section 2, we provide institutional background on the Mexican CCT program and also provide a literature review. In Section 3, we describe the data sources and present descriptive statistics. In Section 4, we present the identification strategy. In Section 5, we present the results of the effects of CCTs on education and employment. We conclude in Section 6.

2. Institutional Background and Literature Review

In this section, we describe some key characteristics of the Mexican CCT program relevant for this evaluation. Then, we provide an overview of the existing literature on the effects of CCT programs on education and employment, focusing on studies that examine the impacts of CCTs at least five years after their implementation.

2.1. Institutional Background

PROSPERA began its operation in 3,461 localities from nine Mexican states in 1996 (Skoufias et al., 1999). Since its establishment, the program has given subsidies for food, as well as cash transfers conditional on the household's members using health services and the children attending to school (Instituto Nacional de Salud Pública, INSP, 2006).

At the beginning, the program provided cash transfers for children between 8 and 17 years old who were enrolled between the third year of primary education through the last year of high school (Skoufias et al., 2000), conditional on their enrollment in school. In 2001, the program expanded the beneficiary group to include those up to age 21 who were enrolled in high school (INSP, 2005).

In order to conduct a rigorous impact evaluation of the program, in 1997 505 rural localities, where the program had not yet been implemented, were chosen to participate in an experiment. These localities belonged to seven states: Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí and Veracruz (INSP and SEDESOL, 2006). Out of the 505 localities, 320 were randomly assigned to the treatment group, while the rest were randomly assigned to the control group (Behrman and Todd, 1999). The program later expanded its geographic coverage, so the remaining 185 localities that constituted the original control group started receiving the treatment in 2000. In other words, the initial random assignment determined if each locality would receive the treatment in 1998 ('Treatment' or 'Early Treatment' from now on) or 2000 ('Control' or 'Late Treatment' from now on).[2]

In each locality, households were eligible for the conditional transfers according to their poverty status (see Skoufias et al., 1999). The Socioeconomic Characteristics of Households Survey (Encuesta de Características Socioeconómicas de los Hogares, ENCASEH) is the 1997 baseline study conducted for all households belonging to the 505 rural localities that were originally assigned to the treatment or control groups. With the information gathered, PROSPERA calculated per capita household incomes taking into account the total household income (without the earnings of children aged 8-18). The total income of each household was then compared to the poverty line, based on the Mexican Standard Food Basket, to classify households as poor or non-poor. Finally, the last step in PROGRESA's eligibility criteria consisted in performing discriminant analysis to incorporate other multi-dimensional household characteristics into the determination of the poverty status of households. For each region, the variables that best differentiated poor from non-poor households were identified using the discriminant analysis.

[2] See INSP and SEDESOL, 2006.

These variables were then used to generate an index that classified households as poor or non-poor.

2.2. Previous Literature

In this section, we focus on studies that assess the long-term effects of CCTs on young adults who started receiving the treatment when they were school-aged children or teenagers.[3] First, we review impact evaluations conducted in several Latin American countries. Then, we present assessments of the Mexican CCT in greater detail. This allows us to compare our findings and approach to previous evaluations of PROSPERA.

Most studies have found positive long-term effects of CCTs on education in Latin America (see Molina-Millan et al., 2016). Behrman et al. (2011) estimate a significant positive impact on years of education between 15-21 year olds originally assigned to PROSPERA and those in the control group in 2003. García et al. (2012) use quasi-experimental methods to evaluate Familias en Acción, a Colombian CCT program. The authors use difference-in-differences with the original sample and compare households in municipalities where Familias en Acción started operating in 2002 with households in municipalities that were originally not selected to participate in the program, but that started receiving the CCTs five years later. Also, they use a regression discontinuity design, employing the score produced by the Potential Beneficiaries of Social Program Identification System (SISBEN) to compare households from the original sample and a comparison group around the 1999 eligibility cutoff.[4] They find positive 10-year effects on school attainment and completion of upper secondary education, but a negative impact on enrolling in

[3] We focus on educational and employment outcomes and do not review the literature examining the impact of CCTs on health outcomes. Moreover, we do not review studies that focus on impacts on the adult household members nor papers that analyze children exposed to treatment in utero or in early childhood.
[4] The original sample consisted of SISBEN 1 households (the poorest in the country), while SISBEN 2 households make up the new sample.

tertiary education for youth aged 18 to 26 in 2011. Barrera-Osorio et al. (2017) examine the medium and long-term effects of a randomized CCT program introduced only in the capital of Colombia. They find that the CCT program increased enrollment in tertiary education 8 to 12 years after random assignment into three transfer programs.[5]

Moreover, Barham et al. (2013) use the experimental design of Red de Protección Social, a Nicaraguan CCT program, and find positive 10-year effects on years of schooling and learning for young men. They compare early- and late-treatment boys aged 9-12 at the start of the program – each of the groups being exposed to the treatment for around three years. Further, Araujo et al. (2016) estimate a positive 10-year effect of Bono de Desarrollo Humano, an Ecuadorian CCT program, on secondary school completion but only for men. Using a regression discontinuity design based on the poverty index that determined eligibility to the program, they also find no significant effects of the CCT program on employment for young adults aged between 9 and 15 at baseline.

In contrast, there is mixed evidence about the impacts of the Mexican CCT program on youth employment. Behrman et al. (2011) estimate negative and significant effects on the likelihood of employment for men, but insignificant effects for women. Similarly, Rodríguez-Oreggia and Freije (2012) do not find significant effects on the likelihood of employment for their full sample. Moreover, Gutiérrez (2008) does not find significant effects of the length of exposure to the program on the likelihood of being employed for either men or women.

[5] Barrera-Osorio et al. (2017) estimate the impact of three different CCT programs: (i) a traditional CCT for secondary school, (ii) a traditional CCT for secondary school plus requirement to save, and (iii) a traditional CCT for both secondary and tertiary education. They find increases in tertiary education from the second and third treatments and no differential effect of directly providing a CCT for tertiary education vs. forcing households to save for tertiary education.

In addition, Rodríguez-Oreggia and Freije (2012) only find significant and positive impacts on wages for men living in localities where the program operated for at least six years, and on the likelihood of moving to higher ranked occupation[6] for women in localities exposed to the program for up to three years. Similarly, Rodríguez-Oreggia (2010) finds a positive effect of the program only on women's intergenerational occupational mobility (see Campos, 2000). However, Yaschine (2014) did not find significant impacts on youth's inter-generational occupational mobility.[7]

These evaluations of the Mexican CCT use only data from the 2003 and 2007 follow-up surveys to estimate the impacts of the program on young beneficiaries up to between 5 and 9 years after they start receiving CCTs. Behrman et al. (2011) study impacts up to 5 years after implementation of PROSPERA, while others examine impacts up to 9 years after implementation. The exception is the study by Yaschine (2014), which uses both the ENCEL 2007 and the ENCELMIG 2008 to study impacts up to 10 years after the Mexican CCT is introduced. This contrasts with our study, which examines effects up to 17 years after PROSPERA was first introduced.

Our study also differs from these studies in the terms of its identification strategy. Behrman et al. (2011) relies on Differences-in-Differences to compare early and late treatment individuals, based on when their localities started receiving the treatment. Rodríguez-Oreggia (2010), instead, uses a regression discontinuity design based on the score that determines household's eligibility into the program as well as households' year of incorporation into the program. Yaschine (2014) used the program's records to calculate how long the household had received CCTs up until 2007.

[6]This study uses an occupational ranking based on a pyramidal scheme with eight levels, with agricultural activities at the bottom and professional activities at the top, rather than using standard occupation status measures.
[7]Measuring intergenerational mobility by comparing the income of children while young to the income of adults as Rodríguez-Oreggia and Freije (2012) Rodríguez-Oreggia (2010) is problematic because one is comparing parents and children at different points in their life-cycle, when children would not have lived up to their full earnings potential.

She, then, compared the group with nine or ten years of exposure with the group with less than six years of exposure, using propensity scores. Our study not only exploits differences between early/late treatment localities, but also additional differences in exposure to treatment as a result of individuals' ages at the time of implementation of the program. The fact that age is given means that the exposure variable is exogenous and cannot be manipulated. This is, thus, preferable to the strategies used by Rodríguez-Oreggia (2010) and Yaschine (2014). Rodríguez-Oreggia (2010) compares individuals who are poor or non-poor and are different in other ways, but also uses eligibility thresholds that vary over time and the index to determine eligibility could be endogenous. Yaschine (2014) instead uses actual take up, which is subject to selection biases.

Aside from the much longer follow-up and the different identification strategy followed in this paper, we also examine the impacts of the Mexican CCT program on important additional outcomes. In particular, we examine the impacts of PROSPERA not only on education and the external margin of employment, but also on changes in employment at the internal margin (i.e., hours worked per week) and on employment quality (i.e., the likelihood of having a contract, the likelihood of having non-wage benefits, and hourly wages). Examining the impact on CCTs on employment quality and not only employment levels is important, since a final goal of these programs is to help individuals and households improve their standards of living in the longer-term.

This paper contributes to the few evaluations that have examined long-term impacts of randomized trials in developing countries. Aside from the handful of evaluations that examine medium and longer term impacts of CCTs, only a handful of RCTs in developing countries have been evaluated in the very long-term. Maluccio et al. (2009) examines the impact of a randomly assigned early childhood nutrition program introduced in Guatemala on educational attainment 25 years later. They find that exposure to healthier food up until 36 months increased the years of education, speed of progression in school, and the likelihood of finishing primary school and attending secondary school. Baird et al. (2016) evaluates the impact of a de-worming experiment in Kenya 10 years after random assignment. They find that de-worming has positive impacts on the labor market outcomes 10 years after, increasing hours worked and increasing the likelihood of non-agricultural employment and of having manufacturing jobs. Kugler et al. (2015) examine the impact of vocational training program in Colombia on formal education 3 to 8 years after random assignment. They find that students exposed to vocational training were also more likely to complete high school and attend university. Angrist et al. (2006) examine the impact of educational vouchers 7 years after random assignment in Colombia, and find that the vouchers increased high school completion, in addition to the short-term impact of lowering grade repetition, increasing test scores and lowering the likelihood of children working (Angrist et al., 2002). Muralidharan (2012) examines the 5-year impact of an experiment in Andhra Pradesh, which provided individual and group performance pay incentives for teachers. Like our study, he uses exposure to treatment and he finds that students exposed to teachers who received performance pay incentives increases their test scores in math and English, but also in natural and social sciences for which the teachers did not receive performance pay. Thus, most middle- to long-term evaluations have examined impacts from 5 to 10 years after random assignment. Only the study for Guatemala by Maluccio et al. (2009) looks at impacts in the longer term.[8]

Unlike previous studies, this evaluation includes information collected from 2008 to 2015, or up to 17 years after the implementation of the program. An additional novelty is that we exploit

[8] There are also a handful of follow ups of randomized evaluations of programs in the very long-term in high-income countries. For example, Chetty et al. (2016) and Chetty et al. (2011) find that exposure to better neighborhoods through vouchers from the Moving to Opportunity project and exposure to better classrooms through the STAR project in childhood lead to higher likelihood of attending college and higher earnings.

the length of exposure to CCTs at the individual level to estimate the impact of the program. Finally, in contrast to the existing literature, we focus not only on the likelihood of employment but also on the quality of employment.

3. Data description

In this section, we describe the data used in the evaluation. First, we describe the sources of information and the baseline characteristics for the early treatment and late treatment groups. Then, we explain how we solve the problem of attrition observed in the data. Later on, we present the outcome variables of interest. Finally, we describe how we calculate the length of exposure variable.

3.1. Sources of Information

This study uses three primary sources of information. The first source is the baseline survey, ENCASEH, which collected information in 1997. We also use the ENCEL, collected in 2003 and 2007. Finally, we use data from 2008 through 2015 from the Reevaluation, the Recertification and the VPCS Surveys.

The 1997 ENCASEH collected information on 23,213 children from poor households between 7 and 16 years old in 1997. Table 1 includes baseline characteristics at the locality level in Columns (1) through (4) and at the individual level in Columns (5) through (8). Columns (1)-(4) report averages of the households living in each locality in our sample. Columns (1) through (3) of Table 1 show the means of all characteristics at the locality level for the total sample, and the early and late treatment samples, respectively. Column (4) shows the differences in locality level average characteristics in treatment and control localities.[9] Most of these differences are

[9] The locality level means only include the information of the individuals of interest.

insignificant, except for the higher proportion of women and higher log of household income in the late treatment (significant at the 5% and 1% level). By contrast, Columns (5)-(8) show the average for all individuals in the sample, the average for all the individuals in the early adoption localities, and the average of all individuals in the late adoption localities. Column (8) shows that comparing early and late localities overall for all individuals not only shows differences in the share of women, but also in the father's education and age, in households building materials and connectivity to electricity and in the level of income. To take account of this, we use the inverse probability of treatment weighting (IPTW) as described in Section 4.1.

We also use the ENCEL surveys for 2003 and 2007, which allows us to quantify the medium- and long-term impacts of the Mexican CCT program. These databases provide information for most of the relevant outcome variables. This evaluation only focuses on the households that were assigned to the original treatment and control groups – who participated in the baseline study – although information of households that were not surveyed in the ENCASEH 1997 is included in the two ENCEL datasets.

We also use information from the Reevaluation, Recertification or VPCS Surveys. The Reevaluation and VPCS Surveys provide information from 2008 to 2015, while the Recertification Survey only has information for the period from 2008 to 2012. These surveys include only households that continued as beneficiaries of the program at the time of the survey.

3.2. Dealing with Attrition

Not all of the children surveyed in 1997 are present in the databases in 2003, and later between 2007 and 2015. Table 2 shows that attrition rates are higher for the most recent periods, indicating that it is easier to re-interview a person when the survey is conducted closer to the baseline year. While the attrition rate was 12 percent in 2003 for the children aged 7-16 in 1997, it doubled to 24 percent in 2007. Then, the attrition rate significantly increased to over 70 percent in each of the following years. Column (2) of Table 2 reports attrition rates are calculated as the share of individuals, from the 1997 database, who cannot be matched using unique identifiers with individuals from the more recent databases. In Column (4) of Table 2 we also use age as an additional criteria to determine if the right person was matched. As expected, the attrition rates increased when considering individuals' ages. The observations of individuals who did not attrite and included in Column (3) of Table 2 make up the samples under study in each period.

Attrition is more problematic if it is related to early vs. late treatment. Thus, in Table 3 we report results of a probit regression where the dependent variable is equal to 1 if the individual attrited and the explanatory variables include the observable characteristics in 1997 presented in Table 1, as well as an indicator of whether the person lives in a treatment or control locality. The results show that being from an early/later treatment locality is not related to attrition in 2003 and 2015. However, attrition is higher for early treatment localities in 2008 and between 2011 and 2014. By contrast, attrition is lower in early treatment localities in 2007, 2009 and 2010.

In order to balance the data to deal with differential attrition, we use the methodology developed by Fitzgerald et al. (1999). This methodology consists in deriving the population density using the inverse probabilities of non-attrition as weights. The inverse probability weights (IPW) reduce bias by giving higher weights for those remaining units with very low probability of remaining in the sample (see Wooldridge, 2002).

3.3. Outcome Variables

Given attrition, we combine the information and keep the most recent value for each outcome of interest for each individual. Therefore, the number of attrited individuals observed in the pooled samples is less than in any of the separate samples: 16 percent for the variables whose data is available in the years 2003 and 2007, and 14 percent in the case of variables coming from the 2003-2015 period.

We show the number of observations that each year provides in the case of each outcome variable in Table 4. Around 45 percent of the available pooled data for education variables comes from data after 2003 and half of the information of employment and non-wage benefits come from data after 2003. The whether the person has a contract comes only from the 2007 ENCEL, and the hourly wage and hours worked per week variables comes from the 2003-2007 pooled database.

Table 5 shows the descriptive statistics of the outcome variables, incorporating the inverse probabilities of non-attrition as weights for all non-baseline values. In addition to presenting the outcome variables for the periods 1997, 2003, 2007 and 2015, this table includes the mean values of the pooled samples. Column (1) shows the descriptive statistics for the full sample, while Columns (2) and (3) show the descriptive statistics for the samples of those living in early and late treatment localities. Column (4) shows differences between those in early and late treatment localities. There are no significant differences in years of education and likelihood of high school completion for the pooled sample, but those in late treatment localities are more likely to do more than high school. By contrast, employment, hours worked, the likelihood of receiving non-wage benefits and wages are all higher for those in the early treatment compared to the late treatment localities when using the pooled sample. This is as expected since those in the early treatment localities would had been exposed to two more years of CCTs. However, these are simple comparisons of means that do not control for other differences between individuals in those localities, including age, which also determines the years of exposure to CCTs.

3.4. Length of Exposure to the Program

We calculate the length of exposure to the program as the number of years that each individual was eligible to receive PROSPERA's scholarships. Table 6 shows the average number of years that individuals in the treatment and control localities were exposed to the program for each year of available information.

We use three exogenous variables to construct the length of exposure: each individual's age, the poverty status of her household in 1997 and the year when her locality started receiving the treatment.[10] Taking into consideration these variables, we estimate how long each person could have been exposed to the program. Since only those aged 8-17 for scholarships between 1998 and 2000 and only those age 8-21 were eligible since 2001, we only consider those aged 7 to 17 in 1997. Also, we only consider children from a households classified as poor in 1997, to avoid using children from households who may had a manipulated their status to qualify for scholarships. Finally, we take into account that individuals living in early treatment localities could receive the scholarships since 1998, and those living in late treatment localities could get PROSPERA's scholarships starting in 2000.

In order to know how long each person qualifies for the scholarships, we jointly consider age, poverty status and locality. For example, a person from a poor household who was 10 years old in 1997 and lived in a late treatment locality was eligible to receive the scholarship from 2000 (when she was 13) until 2008 (when she turned 21). That is, this person was exposed to the program for a period of nine years.

For each person, we use the most updated information that is available for each outcome variable and its respective length of exposure to the program. Therefore, the same person could

[10] The calculations of length of exposure do not incorporate if the children were regularly going to school, as school attendance is a direct outcome of the program.

have different lengths of exposure for each outcome variable, conditional on the availability of information for various outcomes. Table 7 shows the mean, minimum and maximum number of years of exposure to the program by outcome variable. The average length of exposure is between five and eight years, depending on the variable.

4. Empirical Strategy

This section presents our empirical strategy. We, first, describe the early and late treatment groups, and explain why we need to rely on a quasi-experimental strategy using sample weights. Then, we present the specification we estimate and the identifying assumptions behind these estimations.

4.1. Propensity Scores

The original sample consists of those in the 505 rural localities that were originally assigned to the treatment or control groups. The program started operating in the first group of localities in 1998, while it started providing CCTs in the localities of the control group around 18 months later (INSP, 2005). Therefore the 320 localities assigned to the original treatment group constitute the early treatment group, while the remaining 185 localities represent the late treatment group that started receiving the program's benefits in 2000.

While the weighted means of the treatment and control localities do not differ from each other, there are differences in baseline characteristics for individuals living in the treatment and control localities (see Columns (5)-(8) in Table 1).

The randomized controlled trial of the original evaluation design was conducted at the locality level. However, due to systematic differences observed between the treatment and control individuals before the intervention took place, we require quasi-experimental methods to minimize the lack of comparability of the groups at the individual level (see Gertler et al., 2016; Khandker et al., 2010). Since the treatment and control groups are comparable when analyzed at the locality level, but not at the household or individual level (INSP and SEDESOL, 2006), we use propensity score methods to balance individuals in the two groups (Becker and Ichino, 2002).

In order to improve the comparability of the treatment and control groups at the individual level, we estimate each person's likelihood of belonging to the treatment group, based on pre-treatment characteristics, i.e., the propensity score. Only two outcomes are possible – a person being assigned to the treatment or the control group – so we use the following probit model:

$$P_i(Treatment = 1 \mid X) = \Phi\ (\alpha_0 + \alpha_i \times \mathrm{X}_i),$$

where P denotes the probability of success, that is, an individual being assigned to the treatment group. Φ represents the cumulative normal distribution function of the standard normal distribution and α_i are the parameters estimated using a probit model. X symbolizes all the baseline individual and household characteristics included in Table 1, which we use as covariates in this model. Our propensity score model uses sampling weights, the IPWs that account for the existing attrition in the data, as suggested by Ridgeway et al. (2015) for any kind of survey weight that make the sample of respondents representative of the original baseline sample. Figure 1 shows that the distribution of the propensity scores of the individuals assigned to treatment and control for the 2003-2015 pooled sample are on top of one another, indicating that there is a common support.

Since the distribution of baseline covariates is similar between treated and untreated subjects conditional on the propensity score, we can obtain unbiased average treatment effects can be calculated by using the inverse of the probability of receiving the treatment as weights to create an artificial population in which the covariates are not associated with the treatment (Joffe et al., 2004). Additionally, there are gains in efficiency when using estimated propensity scores.

Therefore, it is possible to use the inverse of the predicted values of the propensity scores to obtain unbiased and efficient estimates of the average treatment effect (Hirano et al., 2003).

We calculate the inverse probability of treatment weighting (IPTW) for each individual, according to the following formula (Emsley et al., 2008):

$$IPTW = \frac{A}{\hat{p}_X} + \frac{1 - A}{1 - \hat{p}_X}$$

where A is equal to 1 if the individual was assigned to the treatment group, and takes the value of zero if she lived in a locality originally assigned to the control group. Furthermore, $\hat{p}_x$ is the estimated propensity score, conditional on the defined set of baseline covariates.

4.2. Sample Weights

In Section 3.3., we presented the magnitude of the attrited sample and how we fix the problem it raises by using an inverse probability weight. In addition, the previous section also showed that differences in pre-treatment characteristics of individuals from the treatment and control groups make it necessary to balance the sample by using propensity scores. Consequently, we need two different sample weights – both IPW and IPTW – to obtain unbiased treatment estimates.

It is necessary to combine the IPW and IPTW in order to have a single weight that we can use to generate estimates that are representative of the original survey target population (Bryson et al., 2002). Therefore, we calculate a new hybrid weight by multiplying the propensity score weight and the survey weight for each observation (DuGoff et al., 2014). In other words, our final sample weight is the product of the IPW and the IPTW.

4.3. Identification Strategy

We estimate the impact of the length of exposure on education and employment outcomes by estimating the following regression:[11]

$$Y_i = \beta_0 + \beta_1 \times T_i + \beta_2 \times E_i + \beta_3 \times Year_i + \beta_4 \times C_i + \beta_5 \times \mathrm{U}_i + \beta_6 \times \mathrm{Z}_i + \mathrm{e}$$

where Y is the outcome variable; T is an indicator variable equal to 1 if the locality of residence in 1997 was assigned to the early treatment group; E is the continuous variable that represents the length of exposure; Year represents indicator variables that are equal to 1 for the year of most recent information available (base=2003); C is an indicator variable equal to 1 when the year of most recent information is 2008 or 2009 – the years of the economic crisis; U represents the level of unemployment of the state where the individual lives;[12] and Z is a set of baseline variables, including age and other individual characteristics as well as household characteristics in 1997.

For education, employment, and receipt of non-wage benefits, we have information for the entire period from 2003 to 2015, so we are able to include all the controls in the regression above. However, the data for hours worked per week and hourly wages are only in the 2003 and 2007 survey, so we exclude the economic crisis variable in these regressions. Finally, since the contract variable is only asked in the 2007 survey, we do not control for the indicator for the year in which the most recent information is available nor the economic crisis variable of economic crisis in this regression.

The regression includes an early treatment indicator as well as controls for age. Thus, the estimation strategy exploits differences in exposure to treatment for younger relative to older

[11] The quadratic form of the mean centered length of exposure was added in the models, but only the linear form of the mean centered length of exposure was significant for all outcomes. In addition, the length of exposure was included in the models as a categorical variable, obtaining similar results to the regressions with the variable included in its linear form.
[12] We include unemployment rates from 2015 since information from 1997 is not available.

individuals within a locality and it also exploits differences in exposure to treatment between early and late treatment localities for those within the same group. Since both age and assignment to treatment are given to the individual, there is no room to self-select into length of exposure to treatment. The first identifying assumptions is that that younger and older individuals would have had similar education and employment if living in the same locality absent CCTs. The second identifying assumption is that those in early treatment and late treatment localities would have had the same education and employment absent the treatment for those at a given age.

4.4. Controlling for False Discoveries with Multiple Hypothesis Testing

When one is conducting multiple hypotheses testing, it is possible to make discoveries (rejection of null hypothesis) that are false, by chance. Benjamini and Hochberg (1995) defined the false discovery rate (FDR) as the proportion of the rejected null hypotheses, which are erroneously rejected.

Since we are testing for multiple hypotheses by examining the impacts of CCTs on a number of outcomes for various sub-groups, we adjust the p-values to avoid false discoveries. FDR adjusted q-values are equal to the smallest level at which each hypothesis would be rejected (see Anderson, 2008). Since q-values account for false discoveries, they are higher than the traditional p-values.

We use the Stata code developed by Anderson (2008) to calculate FDR adjusted q-values for our nine families[13] of tests. Our 9 families include the full sample, and one for each subsample (younger population, older population, females, males, children of illiterate women at baseline, children of literate women at baseline, children of unemployed fathers at baseline, and children of

[13] One for the full sample, and one for each subsample (younger population, older population, females, males, children of illiterate women at baseline, children of literate women at baseline, children of unemployed fathers at baseline, and children of employed fathers at baseline).

employed fathers at baseline). Tables 8 to 13 present the effects of exposure on education and employment and include FDR adjusted q-values.

5. Long-term Impacts of CCTs on Education and Employment

In this section, we present the evaluation's results for the full sample, and sub-samples that control for the individuals' age and sex, as well as her mother's education and her father's employment status at baseline. These results show positive impacts of the Mexican CCT program on education, similarly to previous studies, as well as new results on the likelihood and quality of employment.

5.1. Education

We find that greater exposure to the Mexican CCT program increases educational attainment, high school completion and studying at least one year of tertiary education. However, the impacts are weaker for the older group of youth, and for the children of illiterate women at baseline.

Column (1) of Table 8 shows the estimates for all children aged 7-16 in 1997. The length of exposure has positive and significant impacts on years of education, the likelihood of high school completion and the likelihood of attending at least one year of tertiary education. However, the estimated coefficient for the variable that indicates if a person was assigned to the original treatment or control groups is negative for all the variables studied. This means that the CCT program might not have a positive or significant effect, given a short period of exposure. By contrast, we observe positive impacts on years of education, the likelihood of high school completion and the likelihood of studying tertiary education starting at the second or third year of exposure.

The program increases the years of education by 0.8 years for the youth receiving the scholarships for three years, compared to those without access to the CCTs. Then, each additional year of exposure to the program is associated with 0.5 additional years of education. The average individual, someone exposed seven years to the CCT program, would achieve 2.9 additional years of education, in comparison to a person never exposed to the program. Given that the average individual between 25 and 34 years old in 1997 had 4.6 years of education at baseline, the program increases the years of education of its average beneficiary by 63 percent.

In addition, the program increases the likelihood of finishing high school and the likelihood of studying tertiary education by 3.3 and 0.2 percentage points, respectively, for the those receiving the scholarships for three years, compared to those without access to the CCTs. After that, each additional year of exposure to the program increases the probability of completing high school and going to tertiary education by 2.9 and 0.9 percentage points, respectively. Thus, a person exposed eight years to PROSPERA, is 17.9 and 4.9 percentage points more likely to finish high school and study tertiary education, respectively, in comparison to a person never exposed to the program. Since the high school completion rate of individuals between 25 and 34 was 0.7 percent at baseline, the program increases 24.6 times the likelihood of finishing high school. Also, given that the proportion of individuals aged 25-34 with at least one year of tertiary education at baseline was 0.4 percent, so the program increases 11.3 times the likelihood of studying tertiary education.

We estimate differential impacts of exposure for Younger and Older individuals based on their age at baseline. Columns (2) and (3) of Table 8 present results of the ‘Younger’ sample or children between 7 and 11 years old at baseline, and for the ‘Older’ sample or those between 12 and 16 years old at baseline. The estimated impacts of the younger population are similar to those of the full sample, but we do not find significant impacts of the Mexican CCT on the educational outcomes of the older group. In the case of the younger group, we estimate that someone exposed seven years to the CCT has 3.1 more years of education than a person of the same age group with no access to the program. We also find that someone aged 7-11 at baseline and exposed eight years to PROSPERA is 5 percentage points more likely to study tertiary education than a person who is the same age but has not been exposed to the CCTs.

Column (4) and (5) of Table 8 show results for women and men. The results show bigger effects of exposure to CCTs for men than for women. For instance, after seven years of exposure to the program, the expected increase in years of education is higher for men – 3.1 years – than for women – 2.5 years, compared to men and women without access to the CCTs. In addition, after eight years of exposure to the program, the expected increase in the likelihood of finishing high school is higher for men – 17.2 percentage points – than for women – 15.0 percentage points, compared to those never exposed to the program. Nevertheless, after eight years of exposure to the program, the likelihood of studying tertiary education increases around four percentage points for both men and women, compared to those not exposed to the program.

Columns (1) and (2) of Table 9 report heterogeneous treatment effects depending on the mother's education. Interestingly, we mostly find positive impacts of the program when the mother was literate at baseline (Column (1)), but not when the mother was illiterate (Column (1)). Children of literate women at baseline who are exposed to the program for seven years have 3.6 more years of education than children of literate women without access to the CCTs. Similarly, children of literate women who are exposed to the program for eight years are 28.5 and 8.0 percentage points more likely to finish high school and study tertiary education, respectively, in comparison to the children of literate women with no exposure to the CCT. However, we find positive impacts of the length of exposure to the program on the likelihood of studying tertiary education for children illiterate mothers at baseline, though the effect is small in magnitude. Each additional year exposed to the program increases the likelihood of studying tertiary education by 0.2 percentage points, compared to children of illiterate women never exposed to the program.

Finally, we estimate differential effects for those with a father who is employed or unemployed at baseline. Column (4) of Table 9 show that the impacts for those with fathers who are employed are positive and significant and similar to those in the full sample. By contrast, Column (3) of Table 9 shows that for those whose father was unemployed at baseline we only find a positive effect on high school completion. In this case, each additional year of exposure to the program increases the likelihood of finishing high school by 3.5 percentage points, compared to children never exposed to the CCT and whose fathers were unemployed at baseline. However, it is worth noting that the effect for those with unemployed fathers at baseline are imprecise due to the small sample size for this group.

5.2. Employment and Earnings

Given the positive impacts of CCTs on educational attainment, one would expect individuals exposed to the program to have better labor market opportunities not only in terms of getting jobs but also in term so getting higher quality jobs. We find positive effects of the length of exposure on the probability of being employed and the number of hours worked per week. An increase in the length of exposure to the CCT program is also associated with an improvement in employment quality, especially hourly wages. These impacts are stronger for the younger population of the sample and for males.

Column (1) in Tables 10 and 11 include the estimates for all children aged 7-16 in 1997. We observe that the estimated coefficients of the length of treatment is significant and positive for all the outcomes studied. In the case of the variable that denotes if an individual lives in a locality assigned to the early treatment group, it is only significant – and negative – for the number of hours worked per week, the likelihood of having non-wage benefits, and the hourly wage.

A person exposed to the program for three years is 13.7 percentage points more likely to be employed, works 2.9 more hours per week, and earns 1.4 more pesos per hour than someone never exposed to CCTs. Moreover, these outcomes increase 4.6 percentage points, 3.1 hours per week and 1.2 pesos per hour, respectively, with each additional year exposed to the program. Similarly, a person exposed four years to the CCT is 0.3 percentage points more likely to have non-wage benefits than someone never exposed to the program, and this effect increases 0.5 percentage points for each additional year of exposure. Furthermore, someone exposed five years to PROSPERA is 4.2 percentage points more likely to have an employment contract than a person without access to the program, and this impact increases 0.8 percentage points with each additional year exposed to the CCTs.

The average individual was exposed to the program eight years in the case of variables collected until 2015, and between five and six years in the case of the variables whose information was collected only until 2007. Thus, the average person aged 6-17 in 1997 is 36.6 percentage points more likely to be employed, works 9 more hours per week, is 6.6 percentage points more likely to have an employment contract, is 2.3 percentage points more likely to have non-wage benefits, and earns 5.0 more pesos per hour than an individual never exposed to the CCTs. The average impact represents a 40 percent increase on the likelihood of being employed, considering that 91 percent of the heads of households were employed in 1997. Similarly, we observe a 25 percent increase in the number of hours worked per week, a 69 percent increase in the likelihood of having non-wage benefits, and a 38 percent increase in the hourly wages for the average individual, when comparing them to their heads of households at baseline.

Column (2) of Tables 10 and 11, shows the results for the younger population of the sample. The impacts are significant in the case the number of hours worked per week, non-wage benefits and hourly wages. Thus, someone aged 7-11 at baseline and exposed five years to PROSPERA works 23.2 more hours per week than people of similar age with no exposure to the program. Also, a person in this same age group that has been exposed to the CCTs for eight years is 5.6 percentage points more likely to have non-wage benefits than someone without access to PROSPERA. In addition, someone between 7 and 11 years old at baseline with six years of exposure to the program earns 3.4 more pesos per hour than another person of similar age that has not been exposed to the CCTs. Nevertheless, we do not find a statistically significant effect of the program on the likelihood of working or having an employment contract in the case of the younger group of individuals. Similarly, Column (3) of Tables 10 and 11 show no significant impacts in the case of any of the outcomes when we examine the older group of individuals.

When we split the sample by gender in Columns (4) and (5) of Tables 10 and 11, we find no statistically significant effects on the likelihood of working and on the likelihood of having an employment contract for either men or women. However, we do find that the average man exposed to the program for eight years earns 8.0 more pesos per hour than a man with zero years of exposure, while there are not significant effects on women's hourly wages. We also find a substantial difference in the impacts on weekly working hours. A man with five years of exposure to the program works 14.1 more hours per week than a man that has not been exposed to the CCTs, but this difference decreases to 3.5 hours in the case of women.

Like for educational attainment, Columns (1) and (2) of Tables 12 and 13 show stronger effects for the children of literate women at baseline than for the children of illiterate women in 1997. We find impacts that are very similar to the full sample for the children of literate women in the case of likelihood of being employed, weekly working hours, likelihood of having non-wage benefits, and hourly wages. However, we only find significant impacts on the children on illiterate women in the case of weekly working hours – and this effect is not as strong as in the case of the children of literate women. Thus, we find that a person exposed five years to the program works 7.2 and 10.5 more hours per week in the case of children of illiterate and literate women, respectively, compared to someone without access to the CCTs.

Lastly, we report differential effects on employment outcomes for children of unemployed and employed fathers at baseline in Columns (3) and (4) of Tables 12 and 13. We find positive and significant impacts of the exposure to the program in the case of those individuals whose father reported to have a job at baseline. These effects are similar to those of the full sample, as the fathers of 91 percent of the youth were employed at baseline. Accordingly, we find that a child whose father had a job in 1997 and has been exposed to the program for eight years is 38.2 percentage points more likely to work, 6.3 percentage points more likely to have an employment contract and 1.8 percentage points more likely to have non-wage benefits than a child never exposed to the CCTs and whose father reported to be unemployed at baseline. In a similar way, we find that the children of unemployed fathers at baseline work 9.7 more hours per week and earn 5 more pesos per week after five and six years of exposure to the program, respectively, compared to the children of unemployed fathers in 1997 without access to the program. On the other hand, all other effects for the children of fathers unemployed in 1997 are not statistically significant.

6. Conclusions

This is the first evaluation that follows up beneficiaries of the Mexican CCT program for up to 17 years. We study the children aged 7-16 in 1997 that live in the localities who were either randomly assigned to the early treatment and started receiving CCTs in 1998, or to the late treatment groups where the program was implemented in 2000. Unlike previous analysis of the Mexican CCT, we construct a variable that quantifies the time that each person was exposed to the program, using three exogenous variables: each individual's age, the poverty status of her household, and the year when her locality started receiving the treatment.

We examine three education variables and five employment variables, and we find significant and positive impacts of the CCT program in all cases after controlling for multiple hypotheses testing. We find positive effects of the exposure to the Mexican CCT program on educational attainment, high school completion and studying at least one year of tertiary education. However, the impacts are weaker for women and for the children of illiterate women at baseline, implying that educational policies should focus on these vulnerable groups.

In contrast to previous studies, we also find positive impacts of the program on the likelihood and quality of employment. We find positive effects of the length of exposure on the probability of being employed and on the number of hours worked per week. Moreover, an increase in the length of exposure to the CCT program is also associated with an improvement in employment quality, including the likelihood of having an employment contract, the likelihood of having non-wage benefits, and hourly wages.

Once again, the impacts on employment and quality of employment are stronger for men and for the children of literate women, showing that it is more difficult to women and for children of illiterate women to continue studying after high school and/or to transition to the labor market.

These results show that the effects of CCTs are long-lasting and go well-beyond the initial impact on education that have been so well documented. Our study shows that the human capital investments induced by CCTs allow individuals to improve their standards of living by gaining access to better quality employment and higher paying jobs. Thus, the initial intent of CCTs of helping individuals in poor rural communities to become self-sufficient appears to have been achieved.

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Table 1: Descriptive Statistics of Baseline Characteristics in 1997

Variable	Locality				Individual			
	Total	Early treatment	Late treatment	Early treatment – Late treatment	Total	Early treatment	Late treatment	Early treatment – Late treatment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age in 1997	11.0941	11.1201	11.049	0.0711	11.1071	11.1049	11.1105	-0.0056
	(0.5962)	(0.6033)	(0.5825)	(0.0551)	(2.7885)	(2.7905)	(2.7854)	(0.0377)
HH classified as poor (2003 methodology)	0.9968	0.9971	0.9962	0.001	0.9973	0.9976	0.9968	0.0008
	(0.0194)	(0.02)	(0.0185)	(0.0018)	(0.052)	(0.0492)	(0.0564)	(0.0007)
Female	0.4806	0.473	0.4938	-0.0208**	0.4852	0.4802	0.4934	-0.0132*
	(0.1103)	(0.1147)	(0.101)	(0.0102)	(0.4998)	(0.4996)	(0.5)	(0.0068)
Indigenous language	0.2464	0.2362	0.2642	-0.028	0.3502	0.3394	0.368	-0.0286***
	(0.3981)	(0.3935)	(0.4063)	(0.0368)	(0.4771)	(0.4735)	(0.4823)	(0.0065)
Working father (1997)	0.9063	0.9106	0.8988	0.0118	0.9152	0.9231	0.9021	0.0210***
	(0.1227)	(0.1302)	(0.1084)	(0.0113)	(0.2786)	(0.2664)	(0.2972)	(0.0038)
Working mother (1997)	0.0973	0.1058	0.0825	0.0233	0.0994	0.1121	0.0786	0.0334***
	(0.1633)	(0.1702)	(0.1499)	(0.0151)	(0.2992)	(0.3155)	(0.2692)	(0.004)
Father's age (1997)	43.5298	43.4417	43.6831	-0.241	43.2505	43.0053	43.6534	-0.648***
	(4.3552)	(4.3572)	(4.3594)	(0.403)	(10.9914)	(10.712)	(11.4248)	(0.149)
Mother's age (1997)	38.1819	38.284	38.0044	0.28	38.0387	37.9983	38.1056	-0.107
	(3.6134)	(3.6031)	(3.6342)	(0.334)	(9.0884)	(9.0207)	(9.1996)	(0.129)
Father's years of education (1997)	2.5472	2.5674	2.5119	0.0555	2.5533	2.5869	2.4979	0.0889***
	(1.1792)	(1.2059)	(1.1335)	(0.109)	(2.4621)	(2.4882)	(2.4178)	(0.0333)
Mother's years of education (1997)	2.2366	2.2293	2.2493	-0.0199	2.1078	2.1314	2.0691	0.0623*
	(1.1443)	(1.1307)	(1.1706)	(0.106)	(2.3768)	(2.3569)	(2.4089)	(0.0322)
Home with concrete, adobe, partition, brick, stone or cement walls (1997)	0.5332	0.5421	0.5178	0.0243	0.5165	0.5402	0.4781	0.0621***
	(0.3808)	(0.376)	(0.3894)	(0.0353)	(0.4997)	(0.4984)	(0.4996)	(0.0069)
Number of rooms in dwelling w/o bathroom and kitchen (1997)	1.7317	1.7386	1.7196	0.019	1.7022	1.7074	1.6935	0.0139
	(0.4298)	(0.4237)	(0.441)	(0.0398)	(0.9794)	(0.9553)	(1.0177)	(0.0133)
HH with electrical connection (1997)	0.6658	0.652	0.6896	-0.0376	0.6639	0.6527	0.6824	-0.0296***

Variable	Locality				Individual			
	Total	Early treatment	Late treatment	Early treatment – Late treatment	Total	Early treatment	Late treatment	Early treatment – Late treatment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0.3844)	(0.3964)	(0.3624)	(0.0356)	(0.4724)	(0.4761)	(0.4656)	(0.0064)
Head of household's number of male children (1997)	2.4758	2.4979	2.4372	0.0607	2.5166	2.5469	2.4668	0.0800***
	(0.6187)	(0.6307)	(0.597)	(0.0572)	(1.583)	(1.589)	(1.5718)	(0.0214)
Head of household's number of female children (1997)	2.3221	2.3272	2.3131	0.0141	2.3108	2.315	2.3039	0.0111
	(0.7078)	(0.7226)	(0.6832)	(0.0655)	(1.5546)	(1.5567)	(1.5513)	(0.021)
Natural log of household income (1997)	9.5376	9.4783	9.6408	-0.163*	9.5112	9.4334	9.6392	-0.206***
	(1.0615)	(1.1656)	(0.8438)	(0.098)	(1.8959)	(2.031)	(1.642)	(0.0256)
Maximum Number of Observations	504	320	184	504	23,213	14,437	8,776	23,213

Notes: The sample consists of individuals from households classified as poor and aged 7-16 in 1997. Columns (1)-(4) include simple averages of individuals at each locality. Columns (5)-(8) include averages of all individuals in the early localities and of all the individuals in the late localities. Total, Early treatment and Late treatment: Mean, and standard deviation in parentheses. Columns (4) and (8) include Early treatment - Late treatment difference and standard errors in parentheses. Significance of difference between groups: *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Attrition of Original Sample of Individuals Aged 7-16 in 1997 over 2003-2015

Database	Status	Not considering age		Considering age	
		Frequency (1)	**Percentage (2)**	**Frequency (3)**	**Percentage (4)**
2003	Non-attrited	20,413	87.94	19,319	83.22
	Attrited	2,800	12.06	3,894	16.78
2007	Non-attrited	17,503	75.40	16,631	71.65
	Attrited	5,710	24.60	6,582	28.35
2008	Non-attrited	4,281	18.44	2,393	10.31
	Attrited	18,932	81.56	20,820	89.69
2009	Non-attrited	6,918	29.80	4,109	17.70
	Attrited	16,295	70.20	19,104	82.30
2010	Non-attrited	48	0.21	14	0.06
	Attrited	23,165	99.79	23,199	99.94
2011	Non-attrited	245	1.06	34	0.15
	Attrited	22,968	98.94	23,179	99.85
2012	Non-attrited	1,072	4.62	104	0.45
	Attrited	22,141	95.38	23,109	99.55
2013	Non-attrited	159	0.68	25	0.11
	Attrited	23,054	99.32	23,188	99.89
2014	Non-attrited	123	0.53	13	0.06
	Attrited	23,090	99.47	23,200	99.94
2015	Non-attrited	142	0.61	13	0.06
	Attrited	23,071	99.39	23,200	99.94
Total		23,213	100	23,213	100

Notes: The sample consists of individuals from households classified as poor and aged 7-16 in 1997. In Columns (3) and (4), we drop out of the sample any individual with more than two years of difference between the age reported in each survey and the age calculated using data from the baseline study.

Table 3: Attrition by Year

Year:	2003		2007		2008		2009		2010	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Early Treatment Group	0.0331 (0.0551)	0.0151 (0.128)	0.0956 (0.0636)	-0.197* (0.119)	2.273*** (0.136)	4.712*** (0.277)	-1.752*** (0.0939)	-1.965*** (0.136)	-0.361 (0.231)	-6.294*** (1.161)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Observations	23,213	18,232	23,213	18,270	23,213	6,155	23,213	10,689	23,213	410

Year:	2011		2012		2013		2014		2015	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Early Treatment Group	0.153 (0.149)	3.744*** (0.423)	-0.0913 (0.138)	4.173*** (0.274)	0.173 (0.179)	4.901*** (0.676)	-0.198 (0.238)	5.534*** (1.347)	0.0977 (0.171)	0.129 (1.148)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Observations	23,213	1,428	23,213	2,167	23,213	851	23,213	427	23,213	650

Notes: The sample consists of individuals from households classified as poor and aged 7-16 in 1997. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Robust standard errors clustered at the locality level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Available Data of Outcome Variables by Survey Year

Outcome variables	Number of observations	Year of information (%)									
		2003	2007	2008	2009	2010	2011	2012	2013	2014	2015
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Education											
Years of schooling	14,437	62.22	19.30	6.48	10.79	0.08	0.18	0.62	0.16	0.08	0.08
Likelihood of finishing high school	14,491	54.89	17.20	9.76	16.93	0.08	0.19	0.62	0.16	0.08	0.08
Likelihood of studying tertiary education	14,495	54.27	16.90	10.00	17.61	0.08	0.19	0.62	0.16	0.08	0.08
B. Employment											
Likelihood of being employed	11,830	44.52	29.39	9.26	15.33	0.11	0.23	0.76	0.19	0.10	0.10
Hours worked per week	14,431	79.72	20.28								
Likelihood of having a contract	4,398		100.00								
Likelihood of having non-wage benefits	11,586	49.27	26.72	11.46	11.66	0.12	0.10	0.40	0.13	0.07	0.06
Hourly wage	11,362	56.58	43.42								

Notes: Data refer to most updated available value between 2003 and 2015. Sample: individuals from households classified as poor and aged 7-16 in 1997.

Table 5: Descriptive Statistics of Outcome Variables

Variable	Total (1)	Early treatment (2)	Late treatment (3)	Early treatment - Late treatment (4)
Years of education in 1997	3.8786	3.8942	3.8527	0.0415
	(2.2617)	(2.2607)	(2.2633)	(0.0314)
Years of education in 2003	7.0513	7.1267	6.9311	0.196***
	(2.4415)	(2.4232)	(2.4658)	(0.0414)
Years of education in 2007	8.3447	8.4189	8.2313	0.188**
	(2.6632)	(2.6351)	(2.7025)	(0.0858)
Years of education in 2015	6.0719	5.0351	9.0728	-4.038
	(4.0256)	(4.1137)	(2.2332)	(2.376)
Years of education (2003-2015)	7.8998	7.8808	7.9265	-0.0456
	(2.8694)	(2.7522)	(3.0262)	(0.0471)
Completed high school in 1997	0.0002	0.0003	0.0000	0.0003
	(0.0131)	(0.0166)	(0.0000)	(0.0002)
Completed high school in 2003	0.0370	0.0369	0.0371	-0.0003
	(0.1887)	(0.1884)	(0.1891)	(0.0032)
Completed high school in 2007	0.1373	0.1416	0.1308	0.0108
	(0.3442)	(0.3487)	(0.3373)	(0.0111)
Completed high school in 2015	0.0971	0.0342	0.2791	-0.245
	(0.3081)	(0.1962)	(0.4914)	(0.191)
Completed high school (2003-2015)	0.0881	0.0898	0.0859	0.0040
	(0.2834)	(0.2860)	(0.2802)	(0.0046)
More than high school in 1997	0.0001	0.0001	0.0000	0.0001
	(0.0093)	(0.0118)	(0.0000)	(0.0001)
More than high school in 2003	0.0096	0.0110	0.0075	0.0035**
	(0.0977)	(0.1043)	(0.0862)	(0.0017)
More than high school in 2007	0.0273	0.0250	0.0309	-0.0060
	(0.1630)	(0.1560)	(0.1731)	(0.0053)
More than high school in 2015	0.0000	0.0000	0.0000	0
	(0.0000)	(0.0000)	(0.0000)	(0)
More than high school (2003-2015)	0.0230	0.0204	0.0263	-0.0059**

Variable	Total (1)	Early treatment (2)	Late treatment (3)	Early treatment - Late treatment (4)
	(0.1498)	(0.1413)	(0.1601)	(0.0024)
Employment in 1997	0.1196	0.1270	0.1076	0.0194***
	(0.3245)	(0.3329)	(0.3098)	(0.0044)
Employment in 2003	0.4132	0.4157	0.4090	0.0067
	(0.4924)	(0.4929)	(0.4917)	(0.0095)
Employment in 2007	0.5402	0.5648	0.5016	0.0633***
	(0.4984)	(0.4959)	(0.5001)	(0.0141)
Employment in 2015	0.4529	0.3662	0.7040	-0.338
	(0.5181)	(0.5204)	(0.5000)	(0.328)
Employment (2003-2015)	0.4162	0.4760	0.3448	0.131***
	(0.4929)	(0.4995)	(0.4754)	(0.0088)
Hours worked per week in 1997	4.1788	4.3562	3.8874	0.469***
	(13.0945)	(13.2541)	(12.8233)	(0.178)
Hours worked per week in 2003	12.1681	12.4658	11.6926	0.773**
	(23.2506)	(23.6648)	(22.5670)	(0.388)
Hours worked per week in 2007	43.0183	42.8612	43.2849	-0.424
	(21.6623)	(21.8548)	(21.3387)	(0.817)
Hours worked per week (2003-2007)	17.7830	18.2059	17.1012	1.105**
	(25.8372)	(26.2020)	(25.2251)	(0.429)
Contract in 2007	0.0224	0.0230	0.0215	0.0014
	(0.1480)	(0.1498)	(0.1452)	(0.0045)
Non-wage benefits in 1997	0.0013	0.0012	0.0014	-0.0001
	(0.0359)	(0.0353)	(0.0370)	(0.0005)
Non-wage benefits in 2003	0.0046	0.0052	0.0035	0.0017
	(0.0673)	(0.0719)	(0.0592)	(0.0013)
Non-wage benefits in 2007	0.0342	0.0340	0.0344	-0.0004
	(0.1817)	(0.1813)	(0.1824)	(0.0056)
Non-wage benefits in 2015	0.0000	0.0000	0.0000	0
	(0.0000)	(0.0000)	(0.0000)	(0)
Non-wage benefits (2003-2015)	0.0179	0.0212	0.0140	0.0072***
	(0.1325)	(0.1442)	(0.1177)	(0.0024)
Hourly wage in 1997	0.6801	0.5103	0.9534	-0.443

Variable	Total	Early treatment	Late treatment	Early treatment - Late treatment
	(1)	(2)	(3)	(4)
	(27.5994)	(2.7792)	(44.4429)	(0.383)
Hourly wage in 2003	2.6741	2.6930	2.6436	0.0494
	(6.2247)	(6.2147)	(6.2415)	(0.121)
Hourly wage in 2007	4.8891	5.1380	4.5005	0.638**
	(8.6822)	(8.8224)	(8.4462)	(0.249)
Hourly wage (2003-2007)	3.9570	4.0883	3.7441	0.344**
	(7.7443)	(7.8496)	(7.5664)	(0.147)
Maximum Number of Observations	23,213	14,437	8,776	23,213

Notes: Pool data refer to most updated available value between 2003 and 2007, or between 2003 and 2015. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. Total, Early treatment and Late treatment: Mean, and standard deviation in parentheses. Early treatment – Late treatment: Standard errors in parentheses. Significance of difference between groups: *** p<0.01, ** p<0.05, * p<0.1. Inverse probability of non-attrition in 2003, 2007, 2015 and pool data used as individual weights.

Table 6: Length of Exposure to the CCT Program by Year

Year	Total (1)	Early treatment (2)	Late treatment (3)	Early treatment – Late treatment (4)
2003	4.2193	4.8684	3.0000	1.868***
	(0.9757)	(0.4959)	(0.0000)	(0.0070)
2007	7.9165	8.5983	6.6696	1.929***
	(1.1661)	(0.7405)	(0.6624)	(0.0131)
2008	7.3073	8.6155	7.2159	1.400***
	(0.9752)	(1.3723)	(0.8737)	(0.0867)
2009	9.3903	9.5536	8.3409	1.213***
	(1.5499)	(1.5410)	(1.1506)	(0.0730)
2010	10.5726	10.8309	10.0000	0.831
	(1.6089)	(1.8897)	(0.0000)	(0.937)
2011	9.6060	9.4193	9.7722	-0.353
	(1.7634)	(1.9443)	(1.6377)	(0.690)
2012	9.8865	10.7133	8.4517	2.262***
	(2.6254)	(2.2924)	(2.5831)	(0.514)
2013	8.1923	10.4802	7.7571	2.723**
	(2.0206)	(2.3909)	(1.6430)	(1.016)
2014	9.8499	9.8302	11.0061	-1.176
	(1.9893)	(2.0177)	(1.4142)	(4.913)
2015	10.3286	10.9994	8.2611	2.738
	(2.7148)	(2.2354)	(3.3009)	(1.704)
Maximum Number of Observations	14,282	9,227	5,055	14,282

Notes: The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. Total, Early treatment and Late treatment: Mean, and standard deviation in parentheses. Early treatment – Late treatment: Standard errors in parentheses. Significance of difference between groups: *** p<0.01, ** p<0.05, * p<0.1. Yearly inverse probabilities of non-attrition used as individual weights.

Table 7: Length of Exposure to the CCT Program by Outcome Variable

Outcome variables	Mean (1)	Minimum (2)	Maximum (3)
A. Education			
Years of schooling	6.88	3	14
Likelihood of finishing high school	7.51	3	14
Likelihood of studying tertiary education	7.53	3	14
B. Employment			
Likelihood of being employed	7.77	3	14
Hours worked per week	4.85	3	9
Likelihood of having a contract	7.84	5	9
Likelihood of having non-wage benefits	7.58	3	14
Hourly wage	5.77	3	9

Notes: Data refer to most updated available value between 2003 and 2015. The sample consists of individuals from households classified as poor and aged 7-16 in 1997.

Table 8: Long-term Impacts of Mexican CCT on Education

	All	Age		Sex	
		Younger	Older	Women	Men
	(1)	(2)	(3)	(4)	(5)
		A. Years of education			
Years of Exposure to Treatment	0.531***	0.445**	-0.121	0.364***	0.587***
	(0.104)	(0.216)	(0.162)	(0.0933)	(0.111)
	[0.001]	[0.072]	[0.811]	[0.001]	[0.001]
Early Treatment Group	-0.832***	-0.790*	0.872**	-0.456	-1.018***
	(0.284)	(0.426)	(0.412)	(0.330)	(0.320)
	[0.008]	[0.104]	[0.32]	[0.268]	[0.004]
Number of Observations	14,437	9,395	5,042	6,975	7,462
		B. High school completion			
Years of Exposure to Treatment	0.0292***	0.0141	-0.0147	0.0188***	0.0319***
	(0.0059)	(0.0117)	(0.0112)	(0.0069)	(0.0075)
	[0.001]	[0.291]	[0.685]	[0.036]	[0.001]
Early Treatment Group	-0.0542***	-0.0307	0.0577**	-0.0253	-0.0833***
	(0.0204)	(0.0271)	(0.0284)	(0.0235)	(0.0273)
	[0.01]	[0.302]	[0.32]	[0.346]	[0.002]
Number of Observations	14,491	9,417	5,059	6,993	7,491
		C. Tertiary education			
Years of Exposure to Treatment	0.0093***	0.0101***	0.0026	0.008***	0.0077***
	(0.0015)	(0.0031)	(0.0029)	(0.0017)	(0.0020)
	[0.001]	[0.001]	[0.803]	[0.001]	[0.001]
Early Treatment Group	-0.0257***	-0.0309***	0.0026	-0.0275***	-0.0193**
	(0.0084)	(0.0142)	(0.0066)	(0.0108)	(0.0111)
	[0.001]	[0.007]	[0.868]	[0.006]	[0.045]
Number of Observations	14,483	9,414	5,050	6,958	7,487

Notes: Data refer to most updated available value between 2003 and 2015. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: OLS for years of education, and Dprobit for all other dependent variables. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Older defined as those between 12 and 16 in 1997, whereas those between 7 and 11 in 1997 are considered Younger.

Table 9: Long-term Impacts of Mexican CCT on Education by Parents' Characteristics

	Mother's literacy		Father's employment status	
	Illiterate	Literate	Unemployed	Employed
	(1)	(2)	(3)	(4)
	A. Years of education			
Years of Exposure to Treatment	0.180	0.723***	0.268	0.547***
	(0.129)	(0.137)	(0.185)	(0.107)
	[0.292]	[0.001]	[0.32]	[0.001]
Early Treatment Group	0.330	-1.415***	-0.174	-0.850***
	(0.302)	(0.327)	(0.482)	(0.288)
	[0.368]	[0.001]	[0.839]	[0.006]
Number of Observations	5,644	8,793	693	13,744
	B. Completed high school			
Years of Exposure to Treatment	0.0051	0.0488***	0.0347***	0.0297***
	(0.0077)	(0.0077)	(0.0112)	(0.0061)
	[0.554]	[0.001]	[0.014]	[0.001]
Early Treatment Group	0.0094	-0.105***	-0.0484	-0.0550***
	(0.0229)	(0.0250)	(0.0378)	(0.0209)
	[0.682]	[0.001]	[0.32]	[0.012]
Number of Observations	5,657	8,831	691	13,798
	C. More than high school			
Years of Exposure to Treatment	0.0018***	0.0147***	7.61e-05	0.0096***
	(0.0006)	(0.0028)	(0.0003)	(0.0016)
	[0.001]	[0.001]	[0.664]	[0.001]
Early Treatment Group	-0.0009	-0.0381***	0.0002	-0.0271***
	(0.0015)	(0.0117)	(0.0005)	(0.0087)
	[0.554]	[0.001]	[0.664]	[0.001]
Number of Observations	5,646	8,825	530	13,790

Notes: Data refer to most updated available value between 2003 and 2015. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; Natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: OLS for years of education, and Dprobit for all other dependent variables. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Mother's education and father's employment status, according to baseline (1997 data).

Table 10: Long-term Impacts of Mexican CCT on Employment

	All	Age		Sex	
		Younger	Older	Women	Men
	(1)	(2)	(3)	(4)	(5)
		A. Employment			
Years of Exposure to Treatment	0.0458**	0.0948*	0.0122	0.0370*	0.0306
	(0.0187)	(0.0552)	(0.0398)	(0.0206)	(0.0208)
	[0.02]	[0.133]	[0.868]	[0.144]	[0.163]
Early Treatment Group	-0.0376	-0.127	0.0032	0.0109	-0.0732
	(0.0478)	(0.102)	(0.0974)	(0.0536)	(0.0540)
	[0.431]	[0.291]	[0.974]	[0.894]	[0.19]
Number of Observations	11,830	8,533	3,293	5,506	6,317
		B. Hours worked per week			
Years of Exposure to Treatment	3.048***	7.916***	-0.222	1.257**	4.721***
	(0.457)	(0.192)	(0.57)	(0.559)	(0.675)
	[0.001]	[0.001]	[0.868]	[0.058]	[0.001]
Early Treatment Group	-6.290***	-16.34***	2.624*	-2.778**	-9.530***
	(1.110)	(0.837)	(1.455)	(1.209)	(1.612)
	[0.001]	[0.001]	[0.384]	[0.058]	[0.001]
Number of Observations	14,431	8,739	5,692	6,953	7,478

Notes: Data refer to most updated available value between 2003 and 2015 for employment, and between 2003 and 2007 for hours worked per week. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: Dprobit for employment, and OLS for hours worked per week. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Older defined as those between 12 and 16 in 1997, whereas those between 7 and 11 in 1997 are considered Younger.

Table 11: Long-term Impacts of Mexican CCT on Quality of Employment

	All	Age		Sex	
		Younger	Older	Women	Men
	(1)	(2)	(3)	(4)	(5)
			A. Contract		
Years of Exposure to Treatment	0.0083*	0.0014	-0.0079	0.0064	0.0090
	(0.0046)	(0.0031)	(0.0076)	(0.0061)	(0.0060)
	[0.07]	[0.691]	[0.774]	[0.346]	[0.138]
Early Treatment Group	-0.0175		0.0098	-0.0179	-0.0156
	(0.0126)		(0.0179)	(0.0167)	(0.0161)
	[0.147]		[0.868]	[0.346]	[0.295]
Number of Observations	4,379	3,483	857	2,087	2,217
			B. Non-wage benefits		
Years of Exposure to Treatment	0.005**	0.0110**	-0.0036	0.0041*	0.0074**
	(0.0022)	(0.0047)	(0.0031)	(0.0023)	(0.0037)
	[0.02]	[0.044]	[0.685]	[0.144]	[0.056]
Early Treatment Group	-0.0174**	-0.0322***	-0.0009	-0.0173**	-0.0202*
	(0.0095)	(0.0145)	(0.0099)	(0.0095)	(0.0125)
	[0.031]	[0.006]	[0.974]	[0.058]	[0.079]
Number of Observations	11,483	8,374	3,104	5,375	6,098
			C. Hourly wage		
Years of Exposure to Treatment	1.181***	0.756***	0.227	0.164	1.988***
	(0.243)	(0.0507)	(0.304)	(0.326)	(0.328)
	[0.001]	[0.001]	[0.811]	[0.702]	[0.001]
Early Treatment Group	-2.131***	-1.185***	-0.243	0.0841	-3.927***
	(0.533)	(0.235)	(0.728)	(0.679)	(0.739)
	[0.001]	[0.001]	[0.868]	[0.901]	[0.001]
Number of Observations	11,362	7,958	3,404	5,297	6,065

Notes: Information comes from 2007 for employment contract; and data refer to most updated available value between 2003 and 2015 for non-wage benefits, and between 2003 and 2007 for hourly wage. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: OLS for hourly wage, and Dprobit for all other dependent variables. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Older defined as those between 12 and 16 in 1997, whereas those between 7 and 11 in 1997 are considered Younger.

Table 12: Long-term Impacts of Mexican CCT on Employment by Parents' Characteristics

	Mother's literacy		**Father's employment status**	
	Illiterate	**Literate**	**Unemployed**	**Employed**
	(1)	**(2)**	**(3)**	**(4)**
A. Employment				
Years of Exposure to Treatment	0.0413	0.0501***	0.0465	0.0478**
	(0.0342)	(0.0172)	(0.0565)	(0.0193)
	[0.331]	[0.006]	[0.654]	[0.021]
Early Treatment Group	-0.0928	-0.0548	-0.0939	-0.0413
	(0.0901)	(0.0510)	(0.116)	(0.0496)
	[0.376]	[0.29]	[0.654]	[0.405]
Number of Observations	4,542	7,286	542	11,282
B. Hours worked per week				
Years of Exposure to Treatment	2.796***	3.295***	-0.388	3.260***
	(0.700)	(0.572)	(2.016)	(0.464)
	[0.001]	[0.001]	[0.869]	[0.001]
Early Treatment Group	-6.829***	-6.011***	-0.771	-6.575***
	(1.679)	(1.343)	(4.684)	(1.125)
	[0.001]	[0.001]	[0.869]	[0.001]
Number of Observations	5,784	8,647	692	13,739

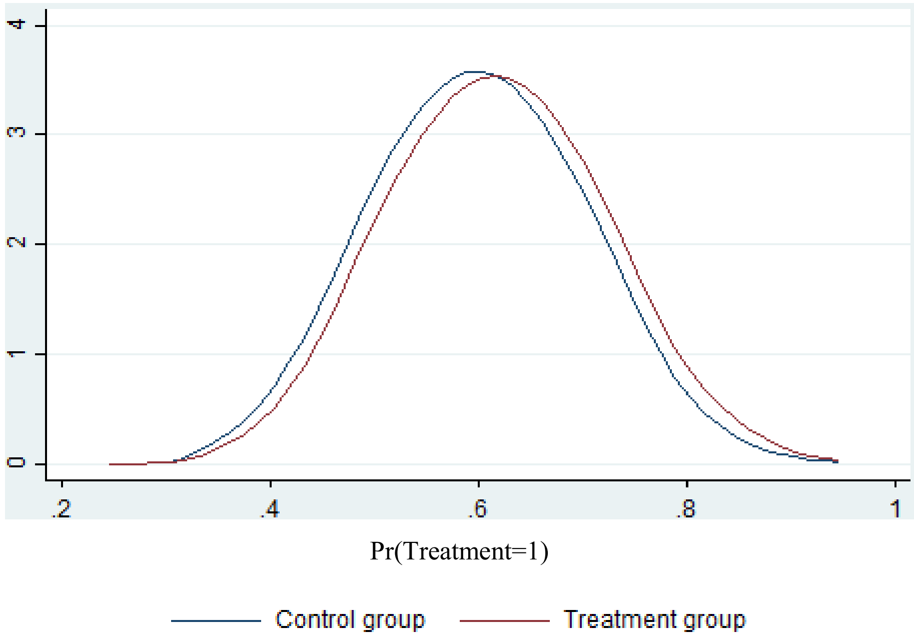
Notes: Data refer to most updated available value between 2003 and 2015 for employment, and between 2003 and 2007 for hours worked per week. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: Dprobit for employment, and OLS for hours worked per week. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Mother's education and father's employment status, according to baseline (1997 data).

Table 13: Long-term Impacts of Mexican CCT on Quality of Employment by Parents' Characteristics

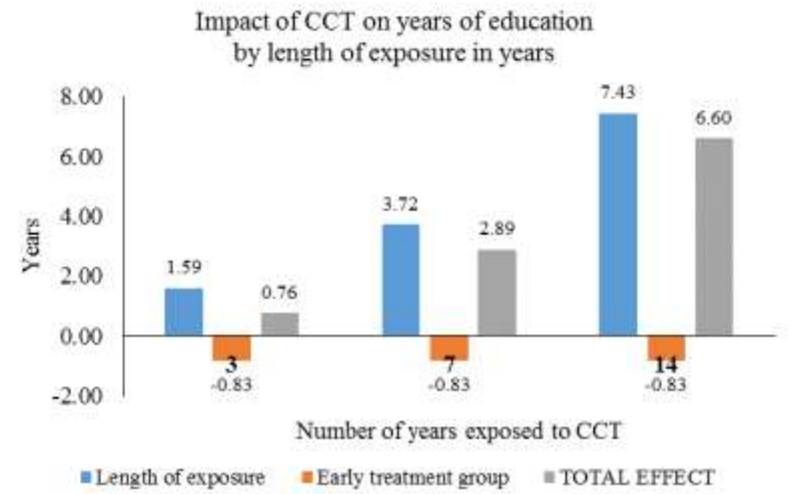
	Mother's literacy		Father's employment status	
	Illiterate	Literate	Unemployed	Employed
	(1)	(2)	(3)	(4)
	A. Contract			
Years of Exposure to Treatment	0.0046*	0.0091	0	0.0079*
	(0.0031)	(0.0067)	(0)	(0.0046)
	[0.166]	[0.197]		[0.093]
Early Treatment Group	-0.0141*	-0.0180	0	-0.0170
	(0.0113)	(0.0178)	(0)	(0.0128)
	[0.166]	[0.29]		[0.17]
Number of Observations	1,508	2,785	43	4,188
	B. Non-wage benefits			
Years of Exposure to Treatment	0.0033*	0.0061**	0.0197**	0.0044**
	(0.0014)	(0.0026)	(0.0109)	(0.0021)
	[0.166]	[0.014]	[0.175]	[0.032]
Early Treatment Group	-0.0054	-0.0248**	-0.0953*	-0.0175**
	(0.0042)	(0.0142)	(0.100)	(0.0095)
	[0.303]	[0.032]	[0.29]	[0.03]
Number of Observations	4,420	7,061	477	10,953
	C. Hourly wage			
Years of Exposure to Treatment	0.711*	1.475***	1.240	1.174***
	(0.374)	(0.290)	(0.871)	(0.245)
	[0.166]	[0.001]	[0.32]	[0.001]
Early Treatment Group	-1.344*	-2.634***	-2.833	-2.088***
	(0.762)	(0.652)	(1.881)	(0.533)
	[0.166]	[0.001]	[0.32]	[0.001]
Number of Observations	4,357	7,005	523	10,839

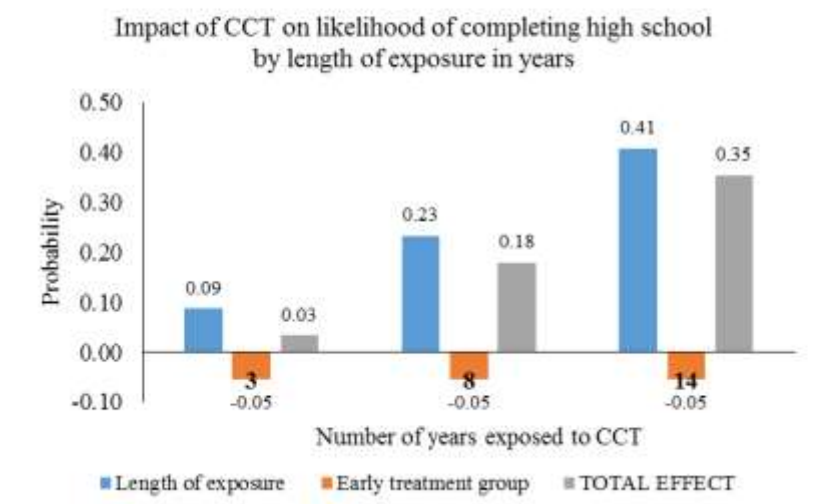
Notes: Information comes from 2007 for employment contract; and data refer to most updated available value between 2003 and 2015 for non-wage benefits, and between 2003 and 2007 for hourly wage. The sample consists of individuals from households classified as poor and aged 7-16 in 1997. The length of exposure variable is calculated based on when the locality started receiving the treatment and the age of the individuals. Early treatment group refers to localities that started receiving the treatment in 1998, as opposed to 2000. Additional independent variables include: (1997 values): age; whether household is classified as poor according to 2003 methodology; female indicator; indigenous language indicator; working father indicator; working mother indicator, father's age; mother's age; father's years of education; mother's years of education; indicators of whether home is made out of concrete, adobe, partition, brick, stone or cement walls; number of rooms in dwelling w/o bathroom and kitchen; whether household has an electrical connection; number of male children; number of female children; the natural log of household income. Other independent variables include: indicator variables for most recent year of available information of outcome under evaluation (base=2003), and indicator variable for economic crisis (equal to one when year of information is 2008 or 2009). Estimation method: OLS for hourly wage, and Dprobit for all other dependent variables. Individual weights: Inverse probability of non-attrition times inverse probability of belonging to early treatment or late treatment group. Robust standard errors clustered at the locality level in parentheses, and false discovery rate adjusted q-values in brackets. ***P-value<0.01, **P-value<0.05, *P-value<0.1. Mother's education and father's employment status, according to baseline (1997 data).

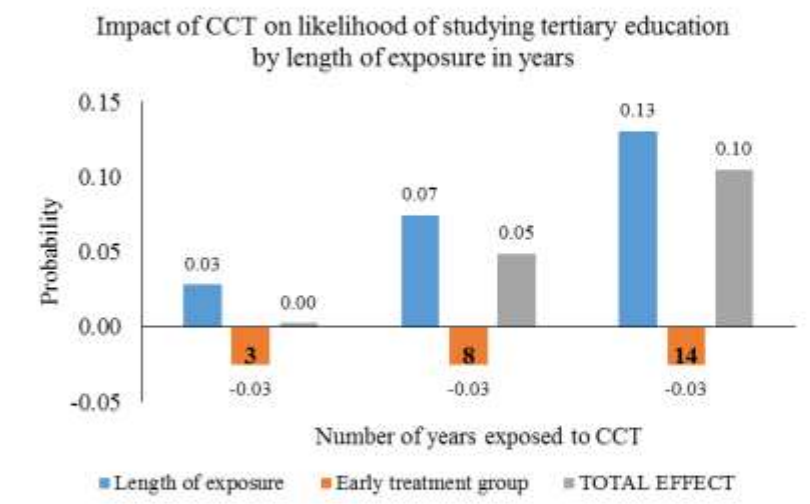
Figure 1: Kernel Density of Propensity Scores by Treatment Status

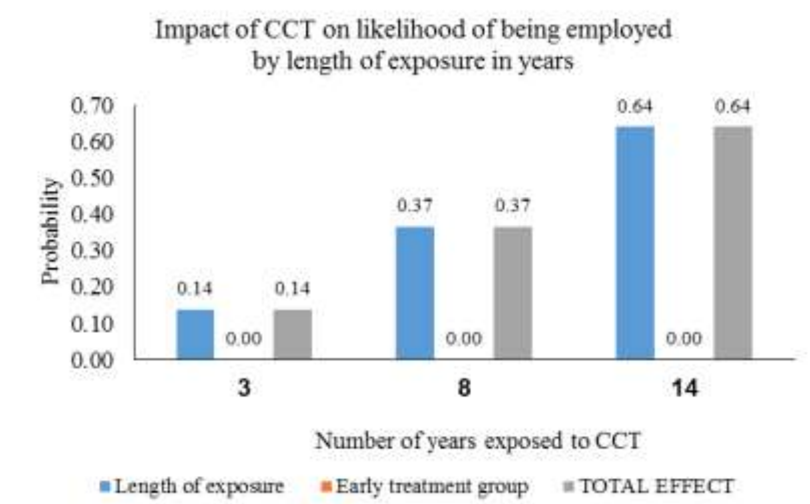


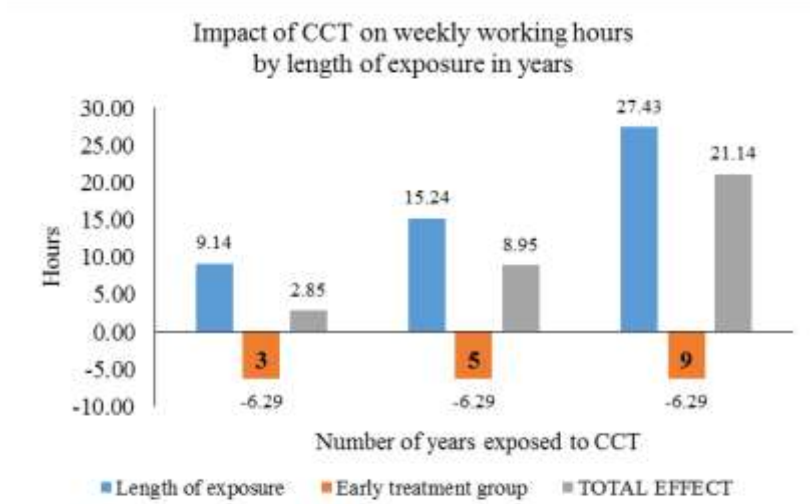
Appendix Figures: Long-term Impacts of Mexican CCT by Length of Exposure (Minimum, Mean and Maximum Years Exposed to the Program for Each Dependent Variable)

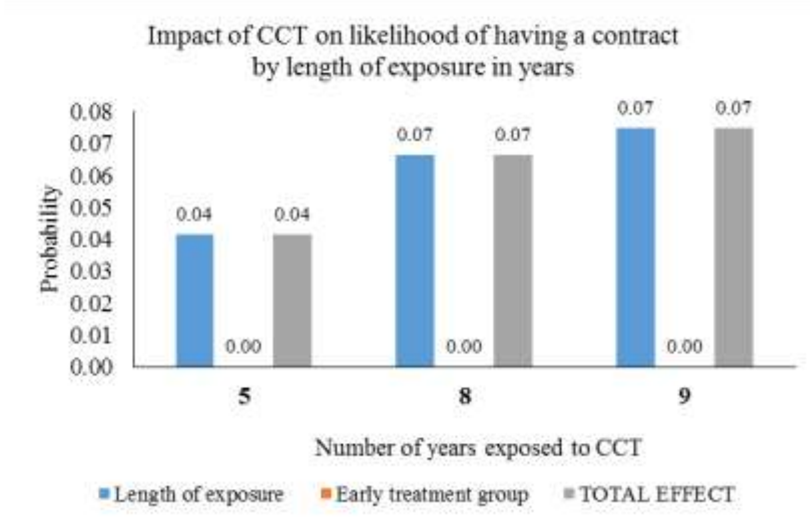


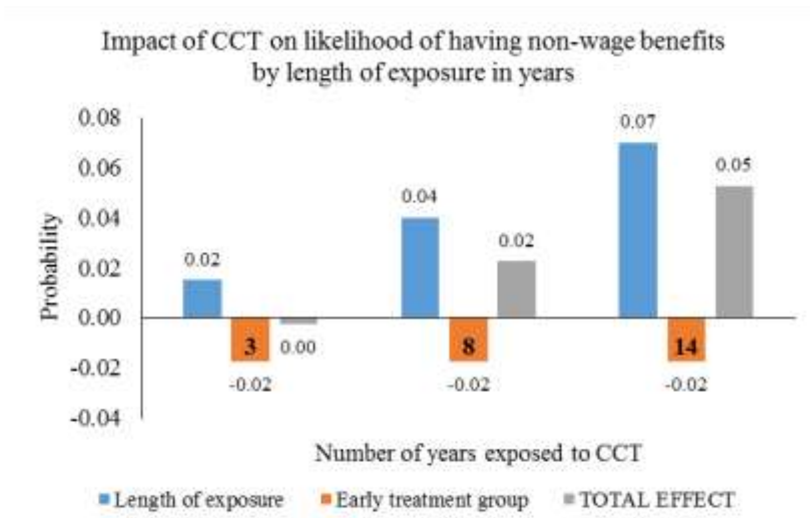


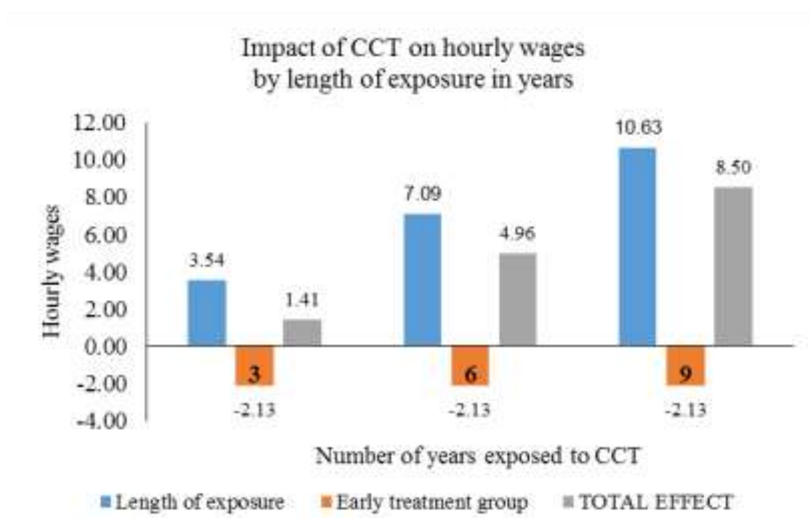

Impact of CCT on likelihood of studying tertiary education
by length of exposure in years
Probability
0.15
0.10
0.05
0.00
-0.05
0.03
0.00
0.07
0.05
0.13
0.10
3
8
14
-0.03
-0.03
-0.03
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT


Impact of CCT on likelihood of being employed
by length of exposure in years
Probability
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
0.14
0.00
0.14
0.37
0.00
0.37
0.64
0.00
0.64
3
8
14
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT


Impact of CCT on weekly working hours
by length of exposure in years
Hours
30.00
25.00
20.00
15.00
10.00
5.00
0.00
-5.00
-10.00
9.14
2.85
15.24
8.95
27.43
21.14
3
5
9
-6.29
-6.29
-6.29
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT


Impact of CCT on likelihood of having a contract by length of exposure in years
Probability
0.08
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0.00
0.04
0.00
0.04
0.07
0.00
0.07
0.07
0.00
0.07
5
8
9
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT


Impact of CCT on likelihood of having non-wage benefits by length of exposure in years
Probability
0.08
0.06
0.04
0.02
0.00
-0.02
-0.04
0.02
3
0.00
-0.02
0.04
8
0.02
-0.02
0.07
14
0.05
-0.02
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT


Impact of CCT on hourly wages by length of exposure in years
Hourly wages
12.00
10.00
8.00
6.00
4.00
2.00
0.00
-2.00
-4.00
3.54
3
-2.13
1.41
7.09
6
-2.13
4.96
10.63
9
-2.13
8.50
Number of years exposed to CCT
Length of exposure
Early treatment group
TOTAL EFFECT