IDB WORKING PAPER SERIES Nº IDB-WP-908

Six Years of Comunidades Solidarias Rurales

Impacts on School Entry of an Ongoing Conditional Cash Transfer Program in El Salvador

Ana Sanchez Chico
Karen Macours
John A. Maluccio
Marco Stampini

Inter-American Development Bank
Social Protection and Health Division

June 2018

Inter-American Development Bank

Six Years of Comunidades Solidarias Rurales

Impacts on School Entry of an Ongoing Conditional Cash Transfer Program in El Salvador

Ana Sanchez Chico
Karen Macours
John A. Maluccio
Marco Stampini

June 2018

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library
Six years of Comunidades Solidarias Rurales: impacts on school entry of an ongoing conditional cash transfer program in El Salvador / Ana Sanchez Chico, Karen Macours, John A. Maluccio, Marco Stampini.
p. cm. — (IDB Working Paper Series ; 908)
Includes bibliographic references.
1. Transfer payments-El Salvador. 2. Income maintenance programs-El Salvador. 3. Economic assistance, Domestic-El Salvador. 4. School enrollment-El Salvador. 5. Rural development-Government policy-El Salvador. 6. Poverty-Government policy-El Salvador. I. Sanchez Chico, Ana. II. Macours, Karen. III. Maluccio, John A. IV. Stampini, Marco. V. Inter-American Development Bank. Social Protection and Health Division. VI. Series.
IDB-WP-908

http://www.iadb.org

scl-sph@iadb.org

www.iadb.org/SocialProtection

Six Years of *Comunidades Solidarias Rurales*

Impacts on School Entry of an Ongoing Conditional Cash Transfer Program in El Salvador

Ana Sanchez Chico, Karen Macours, John A. Maluccio, Marco Stampini[1]

June 2018

Abstract

Conditional Cash Transfer (CCT) programs are important anti-poverty programs in Latin America and the Caribbean. There is little evidence, however, of the effectiveness of ongoing CCT programs several years after they have begun. Such evidence is particularly relevant for policymakers because program effects may become larger, as with the operational cycle, or smaller, if enthusiasm on the part of the beneficiaries or the program team wanes. We analyze whether children exposed since birth to a CCT in El Salvador have better outcomes at initial school ages. As such, we capture the cumulative effects of the CCT during early childhood, combined with the current effects of the CCT transfers and conditionalities. Our results show exposure significantly increased school enrollment and early attainment for five-year-olds, with smaller effects for six-year-olds. Families of the latter experienced a significant improvement as measured by a wealth index. The pattern of impacts suggests continued program exposure might be improving school readiness or shifting norms around child investment.

JEL Classification: I25, I28, I38

Keywords: conditional cash transfers (CCTs), education, Latin America, El Salvador.

[1] Sanchez Chico is at Middlebury College (asanchezchico@middlebury.edu), Macours is Associate Professor at the Paris School of Economics and INRA Researcher (karen.macours@psemail.edu), Maluccio is Professor of Economics at Middlebury College (maluccio@middlebury.edu), and Stampini is Social Protection Lead Specialist in the Social Protection and Health division of the Inter-American Development Bank (IDB) (mstampini@iadb.org). This research was supported by funds from the IDB Economic and Sector Work "CCT Operational Cycles and Long-Term Impacts" (RG-K1422). We thank the Government of El Salvador for providing the data used for the analysis and María Deni Sanchez for her support across the whole research project. We also thank Teresa Molina Millán, Tania Barham, Pablo Ibarrarán, Norbert Schady, Diether Beuermann Mendoza, Ana Sofia Martinez, and participants in presentations at the IDB for useful comments and suggestions. This paper was professionally edited by Sarah Dotson. All remaining errors are our own. The content and findings of this paper reflect the opinions of the authors and not those of the IDB, its Board of Directors or the countries they represent.

1. Introduction

Conditional Cash Transfer (CCT) programs are among the most important anti-poverty programs in Latin America and the Caribbean. Two decades after their introduction, they operate in 18 countries and benefit approximately 137 million people, covering a quarter of the region's population.[2] Transfers are targeted to the poor and are generally paid to women. The conditionalities typically include scheduled visits to healthcare providers for young children under five years old and school enrollment and regular attendance for school-aged children. Often, the conditionalities are complemented by educational workshops and social marketing to encourage investment in nutrition, health and education. In addition to alleviating short-term poverty, CCTs aim to reduce poverty in the long term through enhanced investment in human capital.

Numerous evaluations of CCTs, many of which are based on rigorous designs, demonstrate positive short-term effects. These include increased consumption (and corresponding poverty alleviation), improved nutrition and health (for young children), and increased school enrollment and attainment (Fiszbein and Schady, 2009; Bastagli et al., 2016). Most available research examines impacts during the initial years of program implementation, while relatively little considers the effectiveness of ongoing programs several years after they have begun.[3] This imbalance reflects the importance of early evaluation for ensuring that new programs are effective, which is often a condition for continuation of program funding. Additionally, it reflects the difficulty of identifying program impacts once coverage has expanded and control groups used in early evaluations have been incorporated. It does not mean programs are no longer monitored, but the typical program monitoring and evaluation systems mainly track processes and service delivery without assessing impact.

Rigorously assessing program impacts after several years of implementation is particularly relevant for policymakers, as impacts may persist or change in magnitude. Different scenarios with increasing or decreasing impacts are possible. On one hand, it is possible that valuable program learning occurs in the early years and effects become larger over time as operations improve or become more efficient. On the other hand, if program activities or benefits are captured by rent-seeking or corrupt agents, effects may become smaller. Early positive results may also be the product of enthusiastic program kickoffs and evaluations that are highly

[2] Statistics based on IDB Sociometro, available at http://www.iadb.org/en/research-and-data//social-transfers,7531.html, combined with information on the programs in Jamaica and the Bahamas. For discussion of 1) the expansion of CCT programs in the region, see Stampini and Tornarolli (2012); 2) CCT coverage of the region's poor, see Robles, Rubio and Stampini (forthcoming); and CCT program functioning, see Ibarrarán et al. (2017).
[3] Molina Millán et al. (2016) review the impacts of CCTs on longer-term outcomes.

visible to both beneficiaries and program operators; enthusiasm on the part of the beneficiaries or the program implementation team could wane over time, leading to decreased effectiveness. In addition to these operations-related aspects, observed impacts for individuals also may change as a direct result of continued program exposure. Outcomes after six years, for example, may be enhanced by earlier program effects, such as dynamic complementarities between investments earlier and somewhat later in life (Cunha and Heckman, 2007) or changing norms on child investment (Macours, Schady and Vakis, 2012; Macours and Vakis, 2017).

In this paper, we assess the ongoing effectiveness of the Salvadoran CCT, *Comunidades Solidarias Rurales*, after six years of operation. We analyze whether children exposed since birth to the CCT have better outcomes at initial school ages. As such, we capture the cumulative effects of the CCT during early childhood (which may include improved early life nutrition and health that contribute to earlier school readiness or changes in norms), combined with the current (i.e., contemporaneous) effects of the CCT transfers and related conditionalities at early school ages. Early or on-time school-entry can positively influence later schooling outcomes and is an important way in which CCTs can increase schooling. To date, however, there is only minimal evidence on how CCTs influence school entry. We also explore whether families with eligible children live in households that have higher asset holdings, consistent with the possibility that the CCT is helping build wealth. This represents a related mechanism through which the CCT could improve schooling since better off households are more likely to send their children to school.

We exploit the program eligibility rules to assess the CCT. At the start of the program in 2005, all families with children or a pregnant woman were eligible. Families with a woman who conceived her first child *after* the program began in their municipality, in contrast, were not eligible until recertification several years later. The identification strategy takes advantage of these eligibility rules and the program rollout, including different start dates in different municipalities, to estimate intent-to-treat (ITT) effects for firstborn children using an indicator of eligibility. An administrative program census (starting in late 2012) provides the outcome measures for the study. As in most longer-term evaluations, an important threat to the identification strategy, however, is selective migration out of or into the program municipalities, and we assess the robustness of the findings to that threat as well as others.

We find that program exposure six years after the start of the program significantly increased school enrollment and early attainment at initial school ages. Five-year-old eligible children were approximately 9 percentage points more likely to have completed at least one year of schooling, including preschool, an increase of approximately 25 percent. They were also 12 percentage points more likely to be currently attending school, a 30 percent increase. There were positive but smaller and less significant effects for school outcomes of six-year-olds, possibly reflecting that by that age, most children were in school. For this older group, though, there is a significant improvement in a wealth index of household assets and dwelling characteristics of more than a third of a standard deviation (SD).

Our findings add to the relatively small literature on the effects of benefitting from cash transfers during early childhood but after several years of program operation, much of which also is nonexperimental (Behrman, Parker and Todd, 2009, 2011; García et al., 2012), with some notable exceptions (Araujo, Bosch and Schady, forthcoming). Our paper is also novel in demonstrating sizable effects on early school entry. Last, while not possible to identify relative contributions from the cumulative versus current program effects, this paper underscores the potential importance of these considerations in longer-term assessments of CCTs, which are becoming increasingly possible in Latin America and elsewhere.

The remainder of the paper is organized as follows. Section 2 describes El Salvador's CCT program and summarizes the findings of its previous short-term impact evaluations. Section 3 describes data and methodology. Section 4.1 presents the estimated program effects, Section 4.2 provides related sensitivity analyses and section 5 concludes.

2. The Salvadoran Conditional Cash Transfer Program[4]

El Salvador's CCT program began in late 2005 and covered the poorest 100 of the country's 262 municipalities by 2010, benefiting over 100,000 families (Beneke de Sanfeliú, Angel and Shi, 2015).[5] Health conditionalities included pre- and postnatal checkups and growth monitoring and vaccinations for children under five years old. Education conditionalities included enrollment and regular attendance through sixth grade for children 5–15 years old.[6] By law, primary schooling begins at age seven, but younger children deemed prepared can start earlier. For children under seven years old but not yet in primary school, program conditions were somewhat flexible. Specifically, in communities with a preschool, the conditions were binding for children ages five or older at the start of the school year but were not enforced if there was no

[4] Program design details in this section are described in further detail in Government of El Salvador (2005), Britto (2007) and Beneke de Sanfeliú, Angel, and Shi (2015).
[5] Originally called *Red Solidaria*, the program was renamed *Comunidades Solidarias Rurales* in 2009.
[6] The education transfer eligibility cutoff was raised to age 18 for municipalities entering the program in 2008 and 2009 (de Brauw and Peterman, 2011); this does not affect our approach for determining eligibility.

preschool. The school year runs from February to November so that children with birthdays in December, January or February, are those most likely to face program conditionalities. To our knowledge, this is the only CCT program in the region with explicit conditionality on preschool enrollment; unfortunately, there is no evidence available on the extent to which it was enforced.

The amount of the transfer did not vary with the size of the recipient family but did vary with family composition. Specifically, there was a health component (for families with a pregnant woman or children under five years old) and an education component (for families with school-age children 5–15 years old). Families eligible for only one of the two components received USD 15 per month, and families eligible for both, USD 20.[7] The value of the monthly transfers was less than 10 percent of average household expenditures, relatively small in comparison to other CCT programs in the region (Beneke de Sanfeliú, Angel and Shi, 2015). This rose to a third, however, for those in the poorest two quintiles of the income distribution (IFPRI and FUSADES, 2010). Transfers were targeted to women, but when possible a second adult in the family was identified as an alternate. The program was implemented by the Social Investment Fund for Local Development (FISDL for its acronym in Spanish), which contracted NGOs for local-level monitoring and implementation.

In addition to the demand-side components common to most CCTs, the program also made other investments in the targeted municipalities. These strengthened existing health and education services, the essential supply-side components necessary for the CCT. They also improved basic water, sanitation and electric services, as well as rural roads. These other investments targeted entire communities rather than individual households—therefore they likely benefited all households in the program areas and not just the CCT beneficiaries.

The program was rolled out across municipalities following the 2005 national poverty map. Cluster analysis, using municipal-level extreme poverty and first-grader severe stunting rates, partitioned the 100 poorest municipalities into a "severe extreme poverty group" (32 municipalities), where the program began in late 2005 and 2006, and a "high extreme poverty group" (68 municipalities), where it began in 2007. Further, within each group, municipalities were ordered by a marginality index based on poverty, education levels and housing characteristics; program rollout roughly followed that order (FISDL, 2005; Britto, 2007). This phased rollout has important consequences for interpretations of the analysis, since areas that have been in the program longer are by definition poorer.

---

[7] The transfer amounts doubled in 2015, after the period covered in this study (Beneke de Sanfeliú, Angel and Shi, 2015).

In the rural areas within these municipalities, all families with a pregnant woman or a child up to 15 years old who had not yet completed 6th grade were eligible.[8] Initial eligibility was determined via the (first) administrative program census carried out at the time each municipality was being incorporated into the program. After program start in each municipality, new families were not eligible and could not be enrolled in the program until 2013 or later.[9] Beneficiary families lost eligibility if they did not comply with the conditions, graduated out of the eligibility criteria (e.g., because their youngest child had become too old) or moved out of the program municipality. Administrative data indicate that compliance with conditions was high—above 90 percent in 2007 (Government of El Salvador, 2008)—and most families exiting did so because they graduated out of eligibility (IFPRI and FUSADES, 2010).

In the short term, the CCT was evaluated exploiting the nonexperimental rollout that incorporated municipalities in the severe poverty group first and the high poverty group afterward. Outcomes in the first group were compared with the second, before the latter was exposed, and for some, retrospective information was used to construct double-difference (DD) estimates. This comparison was strengthened using a regression discontinuity design (RDD) in which the running variable was derived from the same factors (extreme poverty rate and severe stunting among first graders) used for the original partitioned cluster analysis underlying the national poverty map (de Brauw and Gilligan, 2011).[10]

Examining the nutrition and health components of the program after one year, de Brauw and Peterman (2011) find large improvements in maternal health-seeking behavior at the time of birth (giving birth in a government or private hospital and, relatedly, attendance of a doctor and nurse at birth) but no effects on pre- or postnatal care. For under-three-year-olds, there was also some evidence of reductions in the prevalence of diarrhea in the last 15 days and in the percent stunted (height-for-age z-score below -2.0), although these findings may have been in part due to preprogram differences between the severe and high poverty groups (IFPRI and FUSADES, 2010).

---

[8] In urban areas of the selected municipalities starting from the high poverty group, only households below a fixed cutoff of a proxy means test (PMT) were eligible (IFPRI and FUSADES, 2010). Information required to estimate the PMT score for families at program start is not available. Moreover, sample sizes for urban residents in the 32 mainly rural municipalities of the severe extreme poverty group are too small for reliable estimation. Therefore, in this study we focus on rural areas only.

[9] Local program promoters could, however, request that a household erroneously excluded at the time of the first administrative program census be incorporated. Program documentation and evaluation reports make clear that the rule excluding families fulfilling eligibility criteria after the cutoff was generally enforced. Evaluation reports provide evidence of one consequence of this: increased under-coverage of the poor (IFPRI and FUSADES, 2010).

[10] The International Food Policy Research Institute (IFPRI) and the *Fundación Salvadoreña para el Desarrollo Económico y Social* (FUSADES) carried out the evaluation (IFPRI and FUSADES, 2008, 2009, 2010). The identification strategy used for the short-term evaluation is not feasible for assessing the accumulated impacts of the program after several years of operation. First, all of the 100 poorest municipalities of the country had been included in the program by 2010, leaving no comparison group without the program in that set. Second, there are no later follow-up evaluation surveys.

For children exposed to the CCT, de Brauw and Gilligan (2011) find that the program significantly increased preschool and primary school enrollment after one year. There were particularly large improvements in enrollment of six-year-olds (approximately 15 percentage points on a base of 65 percent) along with more modest improvements in enrollment for children between 7 and 12 years old (approximately 4–5 percentage points on a base of 90 percent).[11]

Overall, the research on the early years of program operation suggests modest effects in early childhood with more substantial effects at early school ages. Because the transfer sizes were relatively small, these results may reflect a shift in parental behavior beyond the program's income or price effects, increasing the potential for enduring effectiveness. Changes made to the program also may have influenced its ongoing effectiveness. In 2010 after various evaluations, program modifications were made to improve performance of the implementing NGOs, for example, clearer compliance rules. In addition, NGO responsibilities were expanded to include administration of the newly developed universal pension available for all individuals over 70 years old (regardless of their family's eligibility for the CCT), which began in the severe poverty group in 2009 and elsewhere in 2011 (IFPRI and FUSADES, 2010). In this paper, we examine whether the short-term improvements in school attendance for young children at the start of the program in 2007 persist for similarly aged children in 2013, after they had been exposed to the program since birth.

3. Data and Methodology

Starting in 2012, the second administrative program census was conducted to confirm the ongoing eligibility of continuing beneficiary families as well as identify new beneficiary families in the poorest 56 of the original 100 municipalities. In these 56 municipalities, the first administrative program census had been administered (and the CCT began) between September 2005 and March 2008; the second census was conducted between October 2012 and December 2013. Our identification strategy compares—after approximately six years of program operation—children (and families) who were just eligible versus those just ineligible, as determined at the time of the first census.

[11] A qualitative evaluation of the program in 2009 and 2010 demonstrated a positive shift in women's empowerment, stemming in part from attending the program's information sessions on education, health and nutrition (Adato, Morales Barahona and Roopnaraine, 2016). A separate study further confirms the importance of increased social capital for women and also finds greater financial inclusion in 2014 (Beneke de Sanfeliú, Angel and Shi, 2015). Finally, as found in most such programs, household expenditures increased (IFPRI and FUSADES, 2010).

The second census collected information for all individuals, including date of birth, family relationships and schooling, and for all households, including ownership of household assets and dwelling characteristics. Using the maternal relationship codes within each household in the 2012/13 census, we identify all resident children of each woman and define children with the same mother to be in a single nuclear family. Households in the census can contain more than one such nuclear family. The CCT targeted the nuclear family unit, so it was possible for more than one family in a household to receive its own benefits. For each such maternally defined nuclear family, we keep five- and six-year-olds who are the oldest resident child in the household and assume they are the firstborn. This is the sample used in our main analyses.[12]

We then compare the date of birth of each firstborn child with the date of the first program census in his or her (current) municipality of residence to determine whether the child (and family) was likely to have been eligible.[13] Assuming that a woman would have known about being pregnant with her first child at the end of her first trimester, and therefore would have been able to report the pregnancy during the initial program census, families of all firstborn children born within six months of the initial census interview date in their municipality are considered eligible (and comprise the treatment group). Families with firstborn children born more than six months after the initial census are not considered eligible, since the mother had no other (older) children and likely learned of her pregnancy or became pregnant after the first program census; they form the ineligible comparison group.

Figure 1 presents a timeline explaining how we use date of birth of the firstborn child, the first administrative program census date and a six-month lag to demarcate eligibility. We begin with a 12-month window of birthdates (six months on either side of the eligibility cutoff) and for increased power expand it to consider 24- and 36-month windows. All children born prior to the cutoff are treated as eligible and all those born after, ineligible. Within the 24-month window sample, for example, eligible children were born between six months before and six months

[12] By the second census in 2012/13, of course, the composition of families may have changed relative to the first census. In particular, because some children may have migrated or died, we cannot identify with certainty the firstborn child for each mother. For those children who could not be linked to their mother via the relationship codes (because the mother was not resident), it is possible to identify, though with less confidence, whether they are firstborn children from other nuclear families based on: 1) paternal relationships, and 2) relationship with the household head or the grandparents (i.e., for those who have neither mother nor father identified). Children assigned to a nuclear family through the maternal relationship comprise 88 percent of the likely firstborn five- and six-year-olds (with paternal relationship yielding an additional 2 percent and household heads or grandparents the remaining 10 percent). Expanding the sample to include the firstborns identified through paternal or other relationships leads to similar findings.

[13] Apart from the dates of interviews in each municipality, we do not use data from the first census because it does not contain information on children yet to be born who form the ineligible comparison group. In addition, it is not possible to link individuals across the two censuses. For the date of the first census, we use the first day of interviews within the municipality. On average, the first census fieldwork within a municipality lasted 45 days. Results change little when we set the cutoff based on the last day of interviews of the first census in each municipality.

after the first census interview date, and the ineligible between 6 and 18 months after the first census interview date. The sample is restricted to firstborn children rather than including all children with birthdates in this range because any child with older siblings would be in an eligible family regardless of where their own birthdate falls relative to the cutoff.

By definition, eligible children will on average be older than ineligible children when measured in 2012/13. This makes flexible controls for age essential for analyzing outcomes related to age, especially for the wider windows. If the second census measurement had been completed in a single day in each municipality, every eligible child within the municipality would be older than any ineligible children. However, different interview dates *within* each municipality in the second census have the beneficial implication that despite being born later, some ineligible children are, at the time of measurement, older than some eligible children in the same municipality.[14] This relationship between eligibility, birthdates and the second census date enables the use of municipality-level or finer canton-level geographic fixed effects in the analysis, strengthening the identification strategy. For example, canton-level fixed effects control for any additive effects due to other programs or policies implemented in some but not all of the 56 municipalities (Beneke de Sanfeliú, Angel and Shi, 2015) as well as in some instances possible school-level effects in cantons with a single school.

A key identification assumption is that eligible and ineligible firstborn children conceived in a short interval around the date of the first census are comparable in observable as well as unobservable aspects, except for their eligibility to receive the program. The setup is similar to a RDD in which the running variable is date of birth relative to the cutoff. Because of the small sample sizes in narrow intervals (yielding relatively low density of observations near the cutoff) and imprecision regarding the exact cutoff date that was binding for each individual, however, we do not use the standard RDD specification as our main analysis approach but consider it in sensitivity analyses.

Instead, we estimate the following single-difference ITT equation using ordinary least squares (OLS):

$$Y_i = \beta_o + \beta_1 eligible_i + \boldsymbol{X_i \beta_2} + \alpha_c + \varepsilon_i \quad (1)$$

where: Y_i is the outcome of interest for firstborn child *i*; $eligible_i$ indicates whether the family of this firstborn child was determined eligible for the program (at the time of the first census); $\boldsymbol{X_i}$ is a vector of variables including the whether the child is female, monthly age indicator variables

[14] This pattern also holds at the lower administrative level within municipalities, the canton.

and the age and education of the household head; α_c are the geographic-level fixed effects; and ε_i represents an idiosyncratic error term. β_1 is the ITT estimate of the program effect. In the main analysis, we control for canton-level fixed effects (the administrative level below the municipality) and standard errors are adjusted for clustering at that level.

The timing of the two censuses and our identification strategy enable analysis of both five- and six-year-olds, critical ages for school-entry decisions. Because the CCT first began in poorer municipalities, we estimate the impacts at five and six years old separately. Direct comparison of results for five- versus six-year-olds is complicated by the fact that differences may not only reflect variation in length of exposure but also possible differences in program effects by initial poverty, which differs for the two age groups. In other words, we caution against treating the six-year-old cohort as necessarily similar to the five-year-olds, but just one year older. The available data in the second census permit examination of basic schooling outcomes as well as household assets and characteristics of the dwelling in which the family of each child resides. Specific outcomes at the individual level include the following: 1) whether the child is currently attending school, at any level; 2) whether the child has completed at least one year of schooling, including preschool; and 3) whether the child has completed at least one year of primary school (for six-year-olds only). Separately, for the family of each five- and six-year-old, we use principal components to construct a wealth index of household assets and dwelling characteristics described in detail below. For the wealth index, in addition to models estimated using OLS, we present median regression models. For individual-level outcomes, there is one observation per family (except for cases of twins, in which we retain both children, comprising about 1 percent of the sample). We use the same sample (for all observations with complete information) for the family-level wealth index; about 2 percent of the sample has multiple nuclear families in the same household.

4. Results

4.1 Intent-to-Treat Effects of the Salvadoran Conditional Cash Transfer Program

Tables 1 and 2 present summary statistics for the sample of firstborn children and their families who fall within the 24-month window (12 months on either side of the cutoff).[15] Nearly half of the five-year-olds currently attend school and have completed at least some preschool. Over 80 percent of six-year-olds are currently attending school, 15 percentage points higher

---

[15] Statistics are similar for the 12- and 36-month window samples and differ minimally between boys and girls (not shown).

than reported in the 2007 baseline survey (de Brauw and Gilligan, 2011), reflecting possible program effects as well as secular increases.[16] On average, eligible children have better outcomes than ineligible, which may be due in part to the fact that they are two to three months older. Within municipalities, however, average age differences are less than one month (not shown). Examination of parental and household characteristics, on the other hand, show few differences between the two groups for each age, though (as expected) eligible households have slightly older mothers and more children (Table 2). Approximately 60 percent of households have electricity and nearly half have good-quality water, defined as a tap in the house or compound. While owning a vehicle is uncommon, most households have a cell phone, more than half have a television and radio, and about a third have a refrigerator.

Estimation of equation (1) permits analysis of the effect of up to six years of exposure to the CCT. If eligible families remain in the program throughout (which requires complying with the conditionalities), estimated impacts reflect a combination of long- and short-term effects. The former arise from benefiting from the transfers, conditionalities and services since birth; the latter are more directly related to the current transfers and current conditionalities. For each outcome and age group, we present the 12-, 24- and 36-month windows but focus on the 24-month interval in the discussion. All reported results are from the estimation of equation (1) controlling for canton-level fixed effects.[17]

After approximately six years of exposure, CCT program eligibility increased the probability that a five-year-old was attending school by 12.3 percentage points, a 30 percent increase over the 42 percent attendance rate for ineligible five-year-olds (Table 3a). Relatedly, the program increased the probability that the child had completed some schooling (including preschool) by 8.7 percentage points, a 20 percent increase. Effect sizes are similar for the other windows, including the shorter 12-month window where age and other differences between eligible and ineligible children and their families are the least, though the estimates on this smaller sample are less precise. The effects are consistent with the presence of long-term impacts and/or the current conditionalities of the CCT program, which include at least some of the five-year-olds attending preschool. Consequently, the results suggest that the CCT is effective at promoting school during the important early school-age years in the less poor municipalities.

---

[16] Indeed, current attendance is also above 80 percent for six-year-olds living in rural areas measured in the 2013 Salvadoran national household survey (*Encuesta de Hogares de Próposítos Múltiples*).

[17] Results controlling for the more aggregate-level municipality fixed effects are similar and shown in the Appendix (in Tables 2a, 2b and 3).

While positive, the estimated effects for attendance for six-year-olds are small and insignificant. There is also no apparent effect on having completed some schooling. The probability of having completed at least one year of primary school for the 24- and 36-month intervals, however, are significant at 7.4-7.5 percentage points, which is large relative to the average for ineligible children (10 percent). This is consistent with the possibility that the early start for five-year-olds translates into being more likely to have completed first grade by age six, but because by then the vast majority of children are attending, there is no differential in current attendance. The smaller effect for older children may be partly explained by the fact that enrollment increases with age and therefore has less potential for a differential at older ages. It might also reflect differences in impact due to the poverty differences between the severe and high poverty groups. Regardless, compared with the initial effects one year after the program started (de Brauw and Gilligan, 2011), it is notable that while initially the program had large effects on six-year-olds, six years later it has similarly large effects on children a year younger.

To explore further whether cumulative or current effects underlie the findings, we consider even narrower age ranges and exploit the conditionality rules that children who have not turned five by the start of the school year, or who live in a community without a preschool, were not subject to the conditions. In Table 4, we first present results for six-month age groups (61–66, 67–72, 73–78 and 79–84 months) covering both five- and six-year-olds. Then, as an alternative, we examine five-year-olds, splitting them into those with birth months between March and August (inclusive) versus September–February. The youngest age group (61–66 months) and those born March–August are the two groups of five-year-olds who were less likely to be five at the start of the school year and therefore not subject to preschool conditionality requirements. Results for having completed at least one year of school are shown in the top panel of Table 4 for the 12- and 24-month windows. Despite the reduced precision for these narrow age groups, point estimates indicate larger effects for the youngest group (61–66 months of age) and for five-year-olds born March to August, groups for whom the conditions were not binding. This is suggestive evidence that for these children, improvements in schooling were not primarily due to current conditionality but rather might be linked to either earlier benefits from the program (making them better prepared for school) or shifts in norms around investment in children associated with six years of program exposure. We indirectly explore whether the six-year-olds might have similarly benefited when younger by examining effects on the *number* of years of preschool they had attended but find no evidence of an effect (not shown). This may be due to limited access to preschools in poorer municipalities.

While all models presented flexibly control for age (using dummy indicator variables for age in months), since the identification strategy on average yields older children in the eligible versus ineligible group, there may still be upward bias because the outcomes examined tend to increase with age. We examine this concern by estimating two placebo models. The first uses the same age groups and cutoff dates to assign placebo eligible and ineligible status but includes only children who were *not* firstborn in their families and had an older sibling under 15 years of age so that, in fact, all were eligible by virtue of being in a family with an older eligible sibling. The second placebo model compares *only* firstborn children in cases where neither child was eligible for the program or both whom were eligible, as depicted in Figure 2. More specifically, for five-year-olds in the 24-month window, we drop the eligible children (born before the cutoff), relabel the original ineligible group as eligible (those born in the 12 months after the cutoff) and construct a second group of those born between 12 and 24 months after the cutoff. By construction, *all* children in both of these groups were ineligible for the CCT. In a similar fashion, for six-year-olds in the 24-month window, we drop the ineligible children, relabel the original eligible group as ineligible (those born in the 12 months prior to the cutoff) and construct a second group of those born between 12 and 24 months prior to the cutoff. By construction, *all* children in this group were eligible. (A parallel process defines the 12-month window for this firstborn placebo group.) There is little evidence of such false treatment effects from the two placebo models, with insignificant and relatively small point estimates for both ages and outcomes (Table 3b). This suggests that we adequately control for age differences across the eligible and ineligible groups in the main analyses.

Following Filmer and Pritchett (2001), we use principal components analysis to aggregate ownership of household assets and dwelling characteristics into a wealth index. We exclude water and electricity indicators from the index because of the complementary programs implemented to increase their access and availability to all households in the program municipalities. The principal components are estimated using the full census sample (about 75,000) of all rural households in the 56 municipalities, irrespective of family eligibility, based on ownership of six assets (cell phone, cable internet, radio, television, refrigerator and vehicle) and dwelling characteristics including good-quality floor, walls, roof and number of rooms. We retain the first principal component, which explains 28 percent of the total variation. All but one of the included variables have positive loadings greater than 0.2 (Appendix Table 1).

The main ITT effects of the CCT on the household wealth index are shown in the top panel in Table 5 for the sample of families with five- and six-year-olds. Although wealth is a household-level outcome, we examine the age groups separately for two reasons. First, families of six-year-olds have been exposed one year longer. Because 70 percent of eligible families in the sample had more than one child at the time of the second census, for the past two years those families would have had the potential to receive the larger transfer with a child under five and another five or older. Second, because of the program rollout, families of six-year-olds all come from the worst-off severe poverty group municipalities.

There is no evidence of increased wealth as measured by the index for families of five-year-olds, but there are substantial improvements for the families of six-year-olds. For the 24-month window, the OLS estimates for six-year-olds indicate an increase of 0.35 SD and median regression estimates an effect over 0.5 SD (Table 5). While estimated magnitudes are large in SD terms (and even larger for the 12-month window), they may reflect relatively low absolute differences in wealth within these poor, rural communities.[18] For example, ownership of a refrigerator is associated with a 0.4 increase in the wealth index (Appendix Table 1). Placebo tests for those not firstborn show little evidence against the validity of our approach (Table 5).[19]

Considering in more detail the timing of these effects, we also estimated the ITT effect on the wealth index for the six-month age groups (Table 4). Strikingly, most of the impact for the six-year-olds is concentrated in the oldest group (79–84 months). Along with the evidence of no effects for the five-year-olds, this suggests that the observed wealth effects do not reflect a long-term accumulation from ongoing transfers. Rather, they are consistent with families being able to allocate some of the windfall resulting from the increased transfer size (USD 15 to USD 20) to asset accumulation, possibly because for six-year-olds the school conditionalities were de facto not binding (with most of them already in in school).

4.2 Limitations and Sensitivity Analyses

In subsection 4.1, we demonstrated that the main results, all of which control for canton-level fixed effects, are broadly similar for the different windows of inclusion, from 12 to 36 months. In this subsection, we explore three important limitations of the analysis and examine the sensitivity of the main results to the extent possible. First, given the timing of the available post-intervention information in 2012/13, and the possibility of selective in- or out-migration for young families and their children since the start of the CCT, we explore the likely extent of migration using national census data. Second, we consider an alternative RDD estimation strategy, using

[18] Results controlling for the more aggregate-level municipality fixed effects are similar and shown in Appendix Table 3.
[19] Firstborn placebo tests as in Table 3 are not estimated for wealth in Table 5. They are poor placebos for the wealth index, as longer exposure to transfers may enable asset accumulation for the six-year-olds, all of whom were eligible.

the distance between birthdate and the cutoff date as the running variable. Third, we examine whether results are sensitive to the timing of birth during the calendar year in relation to official guidelines regarding ages for starting school.

Migration Selectivity

Given high levels of domestic and international migration for El Salvador, an important concern for the validity of our analyses is potential (selective) migration of families with young children during the period between the first and second administrative program censuses. Such migration may have involved entire families, women of childbearing ages or just the firstborn child. If the families that migrated out are selected, ITT estimates based on the remaining families observed in the second census are biased. Complicating the potential bias, families that migrated into the areas after the first census, which are therefore not eligible for the CCT, may be misclassified in our analysis as eligible based on the age of their firstborn child. A further potential source of bias is that the CCT itself may have influenced migration. Arguably, the focus on young children, a less mobile population than teenagers or young adults, reduces these concerns at least in part, but not entirely.

We used the 2007 Salvadoran national census to estimate historical absolute in- and out-migration rates in the 56 municipalities prior to 2007. The census includes municipality of birth, current municipality and number of years in the current location. In-migration increases with age to 6 percent for five- and six-year-olds while out-migration, also increasing with age, is half that. Therefore, unless patterns changed substantially, it is plausible that such migration may not lead to highly selective samples for our analyses. For example, the estimated effect on attendance for five-year-olds would reduce by half (from 0.12 to 0.06) in an extreme bounds case (i.e., assuming in-migration and out-migration were both 3 percent and *every* child—whether migrating in or out, eligible or ineligible—behaved in a fashion to reduce the estimated impact. That is, when every age-eligible child moving in, and therefore misclassified as eligible, was not attending school in 2013, whereas every age-ineligible child moving in was attending school in 2013—and vice versa for those moving out. With this bounds calculation we conclude that selective in or out-migration for these young families is not likely to have changed the substantive conclusions of our analysis.[20]

[20] In the appendix we also summarize findings using the national census to estimate short-term effects of the program on these migration rates.

Regression Discontinuity Design

As described in the methodology section, the research design is similar to a RDD in which the running variable is defined as the date of birth relative to the cutoff. After partialling out all controls used in the main models (including canton-level fixed effects), we implement a standard RDD estimation on the predicted residuals. Using a linear smoothing function, apart from six-year-olds completing primary school, the RDD estimates are similar or larger in magnitude to the main specifications and have the same pattern of significance (Appendix Table 4). While not our preferred approach because of the low density of observations near the cutoff dates as well as imprecision regarding the exact cutoff date that was binding for each individual, these findings provide further evidence that results are not merely being driven by age differences.

Timing of Birth and the School Year

The legal age for starting primary school in El Salvador is seven years old, although six-year-olds can attend if deemed prepared. For example, in the 2013 Salvadoran national household survey, 20 percent of rural six-year-olds are in first grade of primary school. Nevertheless, a potential concern in the analysis is that rather than the effect of the CCT, our identification strategy instead picks up the effect of official age-eligibility rules for attending school (Berlinski, Galiani and McEwan, 2011). If the CCT eligibility cutoff date coincides with the period just before the start of the school year, children whose birthdays are also just before the cutoff might be more likely to attend in that year but not necessarily because of the program.

The school year runs from February to November. Children with birthdays in December, January or February, then, are those most likely to face binding constraints on enrollment if official rules are enforced. To assess sensitivity to this concern (in addition to the similar concerns addressed in Table 4), we rerun the schooling analyses excluding all municipalities for which the eligibility cutoff date based on the first administrative program census falls in December, January or February. Estimated results are similar (though less significant), suggesting the main analyses do not suffer from bias related to official ages for starting school (Appendix Table 5).

5. Conclusions

Rigorous evaluations of ongoing CCT programs are uncommon. Yet, they are highly relevant for governments as well as researchers trying to understand whether CCTs remain effective and lead to longer-term impacts. Moreover, these evaluations can provide clues as to whether later program effects result from the cumulative effect of the program, reflecting potential synergies—for example across nutrition, health and education—often used to justify these programs or the possibility that they affect norms.

For the CCT program in El Salvador, operating for more than a decade now, we use administrative program census data and eligibility rules to assess effects of six years of exposure. Although the program census does not provide the full range of measures for a comprehensive assessment, it does provide information to examine important early schooling outcomes and a wealth index that may itself influence those outcomes. We find that the CCT improves (early) enrollment of five-year-olds as well as an index of household assets and dwelling characteristics for families of six-year-olds. Results are estimated on an ITT basis and, although it appears take-up was high, in that sense are conservative. Moreover, results are robust to different specifications and estimation approaches and to the threat of selective migration out of or into the program municipalities.

The Salvadoran CCT, therefore, continues to be effective. The pattern of impacts, somewhat higher for groups that were eligible but not yet under binding conditionality compared to others, suggests continued program exposure might be improving school readiness or shifting norms favoring child investment. The latter is congruent with earlier evidence showing improvements in women's empowerment. In comparison with the short-term evaluation results for the same program, the largest effects are no longer for six-year-olds; instead, they are for children one year younger. Hence, the CCT appears to be getting children into school at even younger ages and at the same time improving economic well-being. Additional assessments using other similar data sources are possible in the future. One obvious possibility is the upcoming 2020 Salvadoran national census, for which the identification strategy outlined here could be used on the same cohort once they are 14 and 15 years old.

6. References

Adato, M., O. Morales Barahona, and T. Roopnaraine. 2016. "Programming for Citizenship: The Conditional Cash Transfer Programme in El Salvador." *Journal of Development Studies* 52(8): 1177–91.

Araujo, M.C, M. Bosch and N. Schady. Forthcoming. "Can Cash Transfers Help Households Escape an Inter-Generational Poverty Trap?" In: C. Barrett, M.R. Carter and J.-P. Chavas, editors. *The Economics of Poverty Traps*. Chicago, IL, United States: University of Chicago Press.

Bastagli, F., J. Hagen-Zanker, L. Harman, G. Sturge, V. Barca, T. Schmidt, and L. Pellerano. 2016. *Cash Transfers: What Does the Evidence Say? A Rigorous Review of Impacts and the Role of Design and Implementation Features*. London, United Kingdom: Overseas Development Institute. Available at: https://www.odi.org/publications/10505-cash-transfers-what-does-evidence-say-rigorous-review-impacts-and-role-design-and-implementation.

Beneke de Sanfeliú, M., M.A. Angel and M.A. Shi, 2015. "Estudios de caso por países: experiencias emergentes – El Salvador." In: Maldonado, Moreno-Sanchez, Jomez, and Jurado, editors. *Proteccion, produccion, promocion: Explorando Sinergias entre Politicas de Proteccion Social y Desarrollo Rural en Latinoamerica,* Mimeographed document.

Behrman, J.R., S.W. Parker and P.E. Todd. 2009. "Schooling Impacts of Conditional Cash Transfers on Young Children: Evidence from Mexico." *Economic Development and Cultural Change* 57(3): 439–477.

Behrman, J.R., S.W. Parker and P.E. Todd. 2011. "Do Conditional Cash Transfers for Schooling Generate Lasting Benefits? Five-Year Follow-Up of *PROGRESA*/*Oportunidades*." *Journal of Human Resources* 46(1): 93–122.

Berlinski, S., S. Galiani and P.J. McEwan. 2011. "Preschool and Maternal Labor Market Outcomes: Evidence from a Regression Discontinuity Design." *Economic Development and Cultural Change* 59: 313–334.

Britto, T.F. 2007. *The Challenges of El Salvador's Conditional Cash Transfer Programme, Red Solicaria*. Country Study Number 9. Brasilia, Brazil: International Poverty Centre.

Cunha, F., and J. Heckman. 2007. “The Technology of Skill Formation.” *American Economic Review Papers and Proceedings* 97(2): 31–47.

de Brauw, A., and A. Peterman. 2011. “Can Conditional Cash Transfers Improve Maternal Health and Birth Outcomes? Evidence from El Salvador’s Comunidades Solidarias Rurales.” IFPRI Discussion Paper No. 01080. Washington, DC, United States: International Food Policy Research Institute.

de Brauw, A., and D. Gilligan. 2011. “Using the Regression Discontinuity Design with Implicit Partitions: The Impacts of *Comunidades Solidarias Rurales* on Schooling in El Salvador,” IFPRI Discussion Paper No. 01116. Washington, DC, United States: International Food Policy Research Institute.

Filmer D., and L. Pritchett. 2001. “Estimating Wealth Effects without Expenditure Data – or Tears: An Application to Educational Enrollments in States of India.” *Demography* 38(1): 115–32.

FISDL (*Fondo de Inversión Social para el Desarrollo Local de El Salvador*). 2005. *Mapa de Probreza*. San Salvador, El Salvador: Government of El Salvador.

Fiszbein, A., and N. Schady. 2009. “Conditional Cash Transfers: Reducing Present and Future Poverty.” *World Bank Policy Research Report*. Washington, DC, United States: World Bank.

García, A., O.L. Romero, O. Attanasio, O. and Pellerano, L. (2012). “Impactos de Largo Plazo del Programa Familias en Acción en Municipios de Menos de 100 mil Habitantes en los Aspectos Claves del Desarrollo del Capital Humano.” Technical Report. Bogotá, Colombia: Union Temporal Econometria S.A. Sistemas Especializadas de Información, con la asesoria del Institute for Fiscal Studies.

Government of El Salvador. 2005. “Programa Social de Atención a las Familias en Extrema Pobreza de El Salvador: Red Solidaria,” Secretaría Técnica de la Presidencia (STP) Coordinación Nacional del Á *Red en Acción.* San Salvador, El Salvador: Government of El Salvador.

Government of El Salvador. 2008. “El Salvador: Red Solidaria-Taller de Análisis y Reflexión de Programas TMC.” 16 de enero 2008, Cuernavaca, Mexico.

Ibarrarán, P., N. Medellín, F. Regalia and M. Stampini, editors. 2017. *How Conditional Cash Transfers Work: Good Practices after Twenty Years of Implementation*. Washington, DC, United States: Inter-American Development Bank. Available at: https://publications.iadb.org/handle/11319/8159

IFPRI (International Food Policy Research Institute) and FUSADES (Fundación Salvadoreña para el Desarrollo Económico y Social). 2008. *Evaluación de impacto externa de la Red Solidaria: Informe de línea basal.* Report submitted to the Fondo de Inversión Social para el Desarrollo Local, El Salvador. San Salvador, El Salvador.

IFPRI and FUSADES. 2009. *Evaluación de impacto externa de la Red Solidaria: Informe de impactos al año de implementación.* Report submitted to the Fondo de Inversión Social para el Desarrollo Local, El Salvador. San Salvador, El Salvador.

IFPRI and FUSADES. 2010. *Evaluación de impacto externa de la Red Solidaria: Informe de sostenibilidad del programa.* Report submitted to the Fondo de Inversión Social para el Desarrollo Local, El Salvador. San Salvador, El Salvador.

Macours, K., N. Schady and R. Vakis. 2012. "Cash Transfers, Behavioral Changes, and Cognitive Development in Early Childhood: Evidence from a Randomized Experiment." *American Economic Journal: Applied Economics* 4(2): 247–273.

Macours, K., and R. Vakis. Forthcoming. "Sustaining Impacts When Transfers End: Women Leaders, Aspirations, and Investment in Children." In: C. Barrett, M.R. Carter and J.-P. Chavas, editors. *The Economics of Poverty Traps*. Chicago, IL, United States: University of Chicago Press.

Molina Millán, T., T. Barham, K. Macours, J.A. Maluccio, and M. Stampini. Forthcoming. "Long-Term Impacts of Conditional Cash Transfers: Review of the Evidence." *World Bank Research Observer*.

Robles, M., M.G. Rubio and M. Stampini. Forthcoming. "Have Cash Transfers Succeeded in Reaching the Poor in Latin America and the Caribbean?" *Development Policy Review*, forthcoming. doi: https://doi.org/10.1111/dpr.12365.

Stampini, M., and L. Tornarolli. 2012. "The Growth of Conditional Cash Transfers in Latin America and the Caribbean: Did They Go Too Far?" IDB Policy Brief No. IDB-PB-185.

Washington, DC, United States: Inter-American Development Bank. Available at: https://publications.iadb.org/handle/11319/1448.

7. Figures and Tables

Table 1. Individual Characteristics of Children in 24-month Window

	(1)	(2)	(3)	(4)
	Five-year-olds		Six-year-olds	
Variable	Ineligible	Eligible	Ineligible	Eligible
Currently attending school	0.42	0.54	0.81	0.86
Completed at least one year of school (including preschool)	0.38	0.50	0.79	0.83
Completed at least one year of primary school	0.03	0.02	0.10	0.10
Age in months	66.39	68.56	75.67	78.28
	(3.50)	(2.81)	(2.61)	(3.53)
N	1,191	307	594	1,011

Notes: Samples include identified mother's oldest child with birthdate within +/- 12 months of the cutoff. Standard deviations shown in parentheses. The timing of program entry and later measurement leads to relatively more eligible versus ineligible six-year-olds (and relatively fewer eligible versus ineligible five-year-olds) measured in 2012/13.

Table 2. Household Characteristics of Families with Children Ages Five and Six in 24-Month Window, by Age Group

		(1)	(2)	(3)	(4)
		Five-year-olds		Six-year-olds	
		Ineligible	Eligible	Ineligible	Eligible
	Mother	25.88	26.44*	26.80	27.24
		(4.70)	(5.60)	(5.41)	(5.26)
Age	Father [N=2093]	30.30	29.91	31.75	31.50
		(7.33)	(6.30)	(7.92)	(7.25)
	Household head	37.16	37.66	38.15	37.39
		(15.28)	(15.44)	(15.52)	(14.82)
	Mother	5.61	5.75	5.59	5.32
		(3.53)	(3.57)	(3.76)	(3.65)
Education	Father [N=2100]	5.81	6.14	5.40	5.20
		(3.90)	(3.61)	(3.79)	(3.85)
	Household head	4.59	4.91	4.39	4.21
		(3.97)	(3.77)	(3.87)	(3.85)
Miscellaneous household	Household size	4.60	4.48	4.46	4.62
		(2.14)	(2.02)	(1.85)	(2.05)
	Number of children (age	1.64	1.66	1.69	1.77**
	≤7) in nuclear family	(0.66)	(0.58)	(0.67)	(0.70)
	Electricity	0.56	0.50**	0.65	0.56*
	Good-quality water	0.45	0.44	0.47	0.44
Wealth index variables	Cell phone	0.85	0.83	0.82	0.84
	Cable internet	0.09	0.06*	0.08	0.09
	Radio	0.54	0.52	0.60	0.53*
	Television	0.60	0.61	0.61	0.59
	Refrigerator	0.29	0.30	0.32	0.31
	Vehicle	0.03	0.04	0.04	0.04
	Good-quality roof	0.70	0.65	0.78	0.72
	Good-quality wall	0.79	0.74	0.82	0.80
	Good-quality floor	0.44	0.46	0.42	0.41
	Latrine	0.76	0.72	0.79	0.76
	Number of rooms	1.79	1.72	1.76	1.78
		(1.00)	(0.97)	(1.10)	(1.00)

Notes: N=3,103 (or within 10 observations, 1,498 for five-year-olds and 1,605 for seven-year-olds) for all unless otherwise indicated. Standard deviations in parentheses. Results from statistical tests of the equality of means (comparing ineligible versus eligible clustering standard errors at the municipality level) are shown in the final column. *** p<0.01; ** p<0.05; * p<0.10.

Table 3a. Effect on Individual-Level School Outcomes for Five- and Six-Year-Olds

	(1)	(2)	(3)	(4)	(5)
	Five-year-olds		Six-year-olds		
Variable	Currently attending school	Completed at least one year of school (including preschool)	Currently attending school	Completed at least one year of school (including preschool)	Completed at least one year of primary school
12-month interval	0.120*	0.101	0.015	-0.061	0.051
	(0.069)	(0.075)	(0.054)	(0.064)	(0.047)
N	796	796	1,002	1,002	1,002
Number of cantons	135	135	229	229	229
Control mean	0.411	0.378	0.818	0.801	0.102
24-month interval	0.123***	0.087**	0.014	-0.023	0.074**
	(0.042)	(0.043)	(0.040)	(0.049)	(0.036)
N	1,492	1,492	1,598	1,598	1,598
Number of cantons	244	244	267	267	267
Control mean	0.420	0.378	0.807	0.791	0.096
36-month interval	0.111***	0.087**	0.027	0.001	0.075**
	(0.038)	(0.038)	(0.037)	(0.043)	(0.032)
N	1,963	1,963	1,818	1,818	1,818
Number of cantons	280	280	279	279	279
Control mean	0.432	0.388	0.807	0.791	0.096

Notes: Standard errors allowing for clustering at the canton level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6, 12 or 18 months of the cutoff. All models include canton-level fixed effects, dummy variables for age in months, female, and age and education of the household head. *** p<0.01; ** p<0.05; * p<0.10.

Table 3b. Placebo on Individual-Level School Outcomes for Five- and Six-Year-Olds

Variable	(1) Five-year-olds: Currently attending school	(2) Five-year-olds: Completed at least one year of school (including preschool)	(3) Six-year-olds: Currently attending school	(4) Six-year-olds: Completed at least one year of school (including preschool)	(5) Six-year-olds: Completed at least one year of primary school
Placebo: Not firstborn	0.040	0.036	-0.008	-0.071	0.038
(12-month interval)	(0.070)	(0.085)	(0.037)	(0.045)	(0.040)
N	744	744	1,208	1,208	1,208
Number of cantons	131	131	237	237	237
Placebo control mean	0.489	0.432	0.811	0.784	0.106
Placebo: Not firstborn	0.033	0.013	0.041	-0.010	0.007
(24-month interval)	(0.049)	(0.053)	(0.026)	(0.030)	(0.029)
N	1,476	1,476	1,947	1,947	1,947
Number of cantons	246	246	277	277	277
Placebo control mean	0.454	0.403	0.789	0.758	0.100
Placebo: Firstborn	-0.002	0.037	0.008	0.007	0.006
(12-month interval)	(0.064)	(0.064)	(0.049)	(0.052)	(0.046)
N	1,187	1,187	1,006	1,006	1,006
Number of cantons	232	232	213	213	213
Placebo control mean	0.427	0.378	0.852	0.817	0.128
Placebo: Firstborn	-0.021	-0.047	-0.021	-0.025	0.045
(24-month interval)	(0.044)	(0.043)	(0.037)	(0.044)	(0.039)
N	1,827	1,827	1,316	1,316	1,316
Number of cantons	271	271	226	226	226
Placebo control mean	0.458	0.402	0.866	0.835	0.0954

Notes: Standard errors allowing for clustering at the canton level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6, 12 or 18 months of the cutoff. All models include canton-level fixed effects, dummy variables for age in months, female, and age and education of the household head. The placebos are described in the text Section 4.1. *** p<0.01; ** p<0.05; * p<0.10.

Table 4. Effect on Completed Schooling and Wealth Index, by Narrow Age Group

					Five-year-olds	
Variable	Age 61–66 months	Age 67–72 months	Age 73–78 months	Age 79–84 months	Born March–August	Born September–February
Completed at least one year						
12-month interval	0.274*	0.042	-0.211*	0.064	0.243	0.038
	(0.151)	(0.096)	(0.107)	(0.071)	(0.173)	(0.103)
N	315	481	644	358	358	438
Control mean	0.262	0.464	0.784	0.860	0.321	0.421
24-month interval	0.144*	0.069	-0.173	0.058	0.275**	0.015
	(0.074)	(0.075)	(0.105)	(0.055)	(0.128)	(0.069)
N	667	825	988	610	691	801
Control mean	0.264	0.489	0.776	0.860	0.310	0.440
Wealth index						
12-month interval	0.172	-0.023	0.362	1.108***	0.028	0.089
	(0.431)	(0.305)	(0.379)	(0.362)	(0.680)	(0.346)
N	314	473	634	355	356	989
Control mean	-0.485	-0.223	-0.084	-0.320	-0.486	-0.222
24-month interval	0.152	-0.071	0.214	0.810***	-0.066	0.262
	(0.302)	(0.241)	(0.358)	(0.309)	(0.488)	(0.258)
N	661	816	974	605	685	792
Control mean	-0.304	-0.136	-0.066	-0.320	-0.276	-0.166

Notes: Standard errors allowing for clustering at the canton level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6 or 12 months of the cutoff. All models include canton-level fixed effects, dummy variables for age in months, female, and age and education of the household head. *** p<0.01; ** p<0.05; * p<0.10.

Table 5. Effects on and Placebo Tests for Household-Level Wealth Index for Five- and Six-Year-Olds

	(1)	(2)	(3)	(4)
	Five-year-olds		Six-year-olds	
Regression method	OLS regression	Median regression	OLS regression	Median regression
Effects on wealth				
12-month interval	0.101	0.034	0.647***	0.941***
	(0.254)	(0.328)	(0.223)	(0.239)
N	787	787	989	989
Number of cantons	134	134	228	228
24-month interval	0.167	0.081	0.298*	0.361*
	(0.173)	(0.214)	(0.174)	(0.216)
N	1,477	1,477	1,579	1,579
Number of cantons	244	244	266	266
36-month interval	0.169	0.069	0.111	0.146
	(0.157)	(0.187)	(0.159)	(0.199)
N	1,946	1,946	1,796	1,796
Number of cantons	280	280	278	278
Placebo tests				
Placebo: Not firstborn	0.115	0.050	-0.123	-0.137
(12-month interval)	(0.165)	(0.262)	(0.153)	(0.185)
N	1,297	1,297	2,013	2,013
Number of cantons	137	137	251	251
Placebo: Not firstborn	0.157	0.156	-0.011	-0.047
(24-month interval)	(0.130)	(0.177)	(0.101)	(0.143)
N	2,506	2,506	3,211	3,211
Number of cantons	263	263	286	286

Notes: Standard errors allowing for clustering at the canton level are shown in parentheses. All models include canton-level fixed effects, dummy variables for age in months, female, and age and education of the household head. The placebo is described in the text. OLS=ordinary least squares. *** p<0.01; ** p<0.05; * p<0.10.

Figure 1: Date of Birth of Firstborn Child Relative to First Census Interview

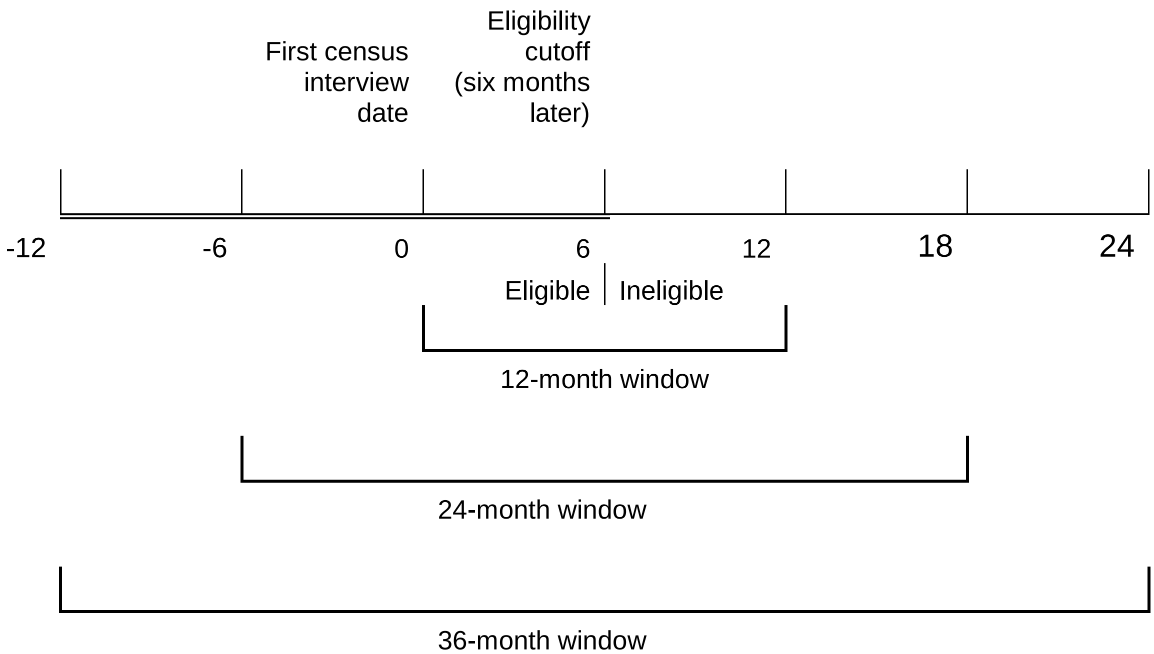
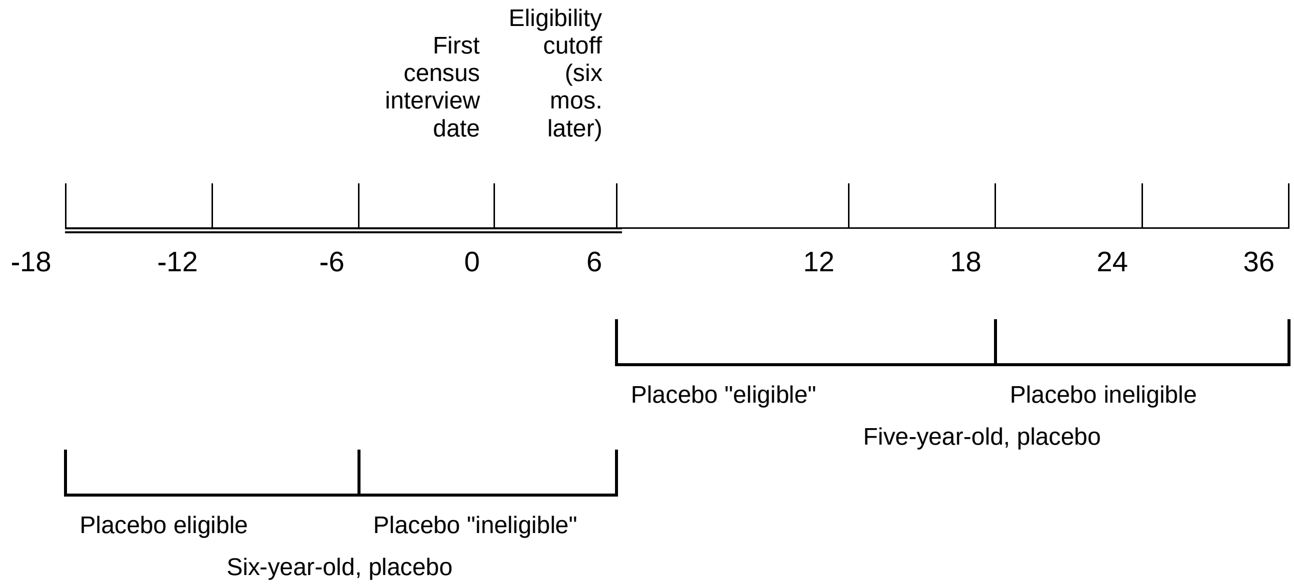


Figure 2: Date of Birth of Firstborn Child Relative to First Census Interview in 24-Month Window (placebo)



Appendix Table 1. Principal Components Analysis Loadings

Variable	PCA loading
Cell phone	0.217
Cable Internet	0.257
Radio	0.271
Television	0.389
Refrigerator	0.418
Vehicle	0.221
Good-quality roof	0.146
Good-quality wall	0.283
Good-quality floor	0.385
Latrine	0.244
Number of rooms	0.361

Note: PCA is principal components analysis.

Appendix Table 2a. Effect on Individual-Level School Outcomes for Five- and Six-Year-Olds, Municipality-Level Fixed Effects

	(1)	(2)	(3)	(4)	(5)
Age	Five-year-olds		Six-year-olds		
Variable	Currently attending school	Completed at least one year of school (including preschool)	Currently attending school	Completed at least one year of school (including preschool)	Completed at least one year of primary school
12-month interval	0.155**	0.105	0.005	-0.062	0.040
	(0.064)	(0.071)	(0.039)	(0.043)	(0.041)
N	796	796	1,002	1,002	1,002
Number of municipalities	25	25	47	47	47
Control mean	0.411	0.378	0.818	0.801	0.102
24-month interval	0.114***	0.069*	0.004	-0.036	0.057
	(0.033)	(0.035)	(0.029)	(0.035)	(0.036)
N	1,492	1,492	1,598	1,598	1,598
Number of municipalities	54	54	56	56	56
Control mean	0.420	0.378	0.807	0.791	0.096
36-month interval	0.100***	0.069**	0.011	-0.019	0.056
	(0.030)	(0.031)	(0.028)	(0.033)	(0.034)
N	1,963	1,963	1,818	1,818	1,818
Number of municipalities	56	56	56	56	56
Control mean	0.432	0.388	0.807	0.791	0.096

Notes: Standard errors allowing for clustering at the municipality level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6, 12 or 18 months of the cutoff. All models include municipality-level fixed effects (instead of finer canton-level), dummy variables for age in months, female, and age and education of the household head. *** p<0.01; ** p<0.05; * p<0.10.

Appendix Table 2b. Placebo on Individual-Level School Outcomes for Five- and Six-Year-Olds, Municipality-Level Fixed Effects

	(1)	(2)	(3)	(4)	(5)
Age	Five-year-olds		Six-year-olds		
Variable	Currently attending school	Completed at least one year of school (including preschool)	Currently attending school	Completed at least one year of school (including preschool)	Completed at least one year of primary school
Placebo: Not firstborn	0.034	0.017	0.028	-0.021	0.016
(12-month interval)	(0.062)	(0.071)	(0.024)	(0.029)	(0.031)
N	744	744	1,208	1,208	1,208
Number of municipalities	26	26	47	47	47
Placebo control mean	0.489	0.432	0.811	0.784	0.106
Placebo: Not firstborn	0.036	0.009	0.060***	0.011	-0.009
(24-month interval)	(0.056)	(0.070)	(0.017)	(0.020)	(0.022)
N	1,476	1,476	1,947	1,947	1,947
Number of municipalities	54	54	56	56	56
Placebo control mean	0.454	0.403	0.789	0.758	0.100
Placebo: Firstborn	0.003	0.027	0.027	0.030	-0.005
(12-month interval)	(0.055)	(0.049)	(0.047)	(0.058)	(0.033)
N	1,187	1,187	1,006	1,006	1,006
Number of municipalities	54	54	53	53	53
Placebo control mean	0.427	0.378	0.852	0.817	0.128
Placebo: Firstborn	0.003	-0.028	-0.010	-0.015	0.042*
(24-month interval)	(0.046)	(0.042)	(0.038)	(0.044)	(0.025)
N	1,827	1,827	1,316	1,316	1,316
Number of municipalities	56	56	53	53	53
Placebo control mean	0.458	0.378	0.866	0.835	0.0954

Notes: Standard errors allowing for clustering at the municipality level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6, 12 or 18 months of the cutoff. All models include municipality-level fixed effects (instead of finer canton-level), dummy variables for age in months, female, and age and education of the household head. The placebos are described in the text Section 4.1. *** p<0.01; ** p<0.05; * p<0.10.

Appendix Table 3. Effects on and Placebo Tests for Household-Level Wealth Index for Five- and Six-Year-Olds, Municipality-Level Fixed Effects

	(1)	(2)	(3)	(4)
Age	Five-year-olds		Six-year-olds	
Regression method	OLS regression	Median regression	OLS regression	Median regression
Effects on wealth				
12-month interval	0.001	-0.060	0.617**	0.765***
	(0.305)	(0.318)	(0.259)	(0.285)
N	787	787	989	989
Number of municipalities	25	25	47	47
24-month interval	0.095	-0.030	0.331*	0.482**
	(0.215)	(0.233)	(0.177)	(0.226)
N	1,477	1,477	1,579	1,579
Number of municipalities	54	54	56	56
36-month interval	0.090	-0.039	0.203	0.301
	(0.176)	(0.205)	(0.157)	(0.197)
N	1,946	1,946	1,796	1,796
Number of municipalities	56	56	56	56
Placebo tests				
Placebo: Not firstborn	0.357	0.052	-0.010	0.122
(12-month interval)	(0.296)	(0.333)	(0.206)	(0.257)
N	721	721	1,193	1,193
Number of municipalities	26	26	47	47
Placebo: Not firstborn	0.251	0.193	-0.026	-0.047
(24-month interval)	(0.225)	(0.252)	(0.155)	(0.187)
N	1,442	1,442	1,922	1,922
Number of municipalities	54	54	56	56

Notes: Standard errors allowing for clustering at the municipality level are shown in parentheses. All models include municipality-level fixed effects (instead of the finer canton-level), dummy variables for age in months, female and age and education of the household head. The placebo is described in the text. OLS=ordinary least squares. *** p<0.01; ** p<0.05; * p<0.10.

Appendix Table 4. Regression Discontinuity Design Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Age	Five-year-olds			Six-year-olds		
Variable	Currently attending school	Completed at least one year of school (including preschool)	Wealth index	Currently attending school	Completed at least one year of primary school	Wealth index
	0.178**	0.128*	0.211	0.030	0.024	0.849***
	(0.075)	(0.074)	(0.277)	(0.063)	(0.052)	(0.286)
N	522	588	495	476	719	493

Notes: Robust standard errors are shown in parentheses. Samples include identified mother's oldest child with birthdate within the optimal bandwidth of the cutoff. All models control for canton-level fixed-effects, dummy variables for age in months, female, and age and education of the household head. *** p<0.01; ** p<0.05; * p<0.10.

Appendix Table 5. Effect on Individual-Level School Outcomes for Five- and Six-Year-Olds excluding Municipalities with Cutoff Dates in December, January or February

	(1)	(2)	(3)	(4)
Age	Five-year-olds		Six-year-olds	
Variable	Currently attending school	Completed at least one year of school (including preschool)	Currently attending school	Completed at least one year of primary school
12-Month Interval	0.187**	0.137	0.009	0.018
	(0.086)	(0.096)	(0.057)	(0.051)
N	556	556	826	826
Number of cantons	101	101	177	177
Control mean	0.434	0.400	0.826	0.105
24-Month Interval	0.131**	0.088	0.008	0.060
	(0.061)	(0.059)	(0.043)	(0.039)
N	1,126	1,126	1,256	1,256
Number of cantons	197	197	213	213
Control mean	0.447	0.400	0.812	0.101
36-Month Interval	0.098*	0.075	0.031	0.052
	(0.052)	(0.049)	(0.040)	(0.035)
N	1,528	1,528	1,420	1,420
Number of cantons	221	221	222	222
Control mean	0.445	0.398	0.812	0.101

Notes: Standard errors allowing for clustering at the canton level are shown in parentheses. Samples include identified mother's oldest child with birthdate within +/- 6, 12 or 18 months of the cutoff. All models include canton-level fixed effects, dummy variables for age in months, female, and age and education of the household head. *** p<0.01; ** p<0.05; * p<0.10.

Evidence on Migration Selectivity

To explore further the extent of potential concerns regarding migration selectivity, we carried out additional analyses using the full sample 2007 Salvadoran National Census, which includes municipality of birth, current municipality and number of years in the current location. In particular, we focused on all individuals born or currently living in any one of the 56 municipalities. By the end of 2006, the CCT had started in the 32 municipalities in the extreme poverty group. Using the retrospective information on when individuals moved, we construct an individual-level panel data set with up to four annual observations (depending on the individual's age)—2004 through 2007—for all individuals born in the municipality. We then estimated a linear probability model for out-migration (including domestic and international). In addition to municipality-level fixed effects, we control for gender, calendar year and age. The covariate of interest is an indicator variable for whether the municipality of birth had the CCT program in that year, yielding the DD estimate of the program on out-migration in the short term. Although generally negative, for a wide range of estimates for single-year or combined age groups (1–6, 6–10, 11–15, 16–20, 21–25, 26–30) for men and women, estimated DD magnitudes are small in absolute value (always smaller than 0.003), and most are insignificant despite the power of having the full national census data for the analysis. We conclude that after 1–2 years of program operation, at least, the CCT did not appear to influence out-migration rates.

A parallel analysis estimating the CCT effect on migration into program municipalities, however, does point to a possible program effect after just a few years. Municipalities with the program had in-migration rates 1–2 percentage points lower than the others. As indicated above, if true this likely leads to greater misclassification error and possible downward bias on the program estimates, although the exact sign of bias would depend on outcomes for all the in-migrants and not just those misclassified.

The validity of the DD results, of course, relies on the common trends assumption, which is potentially problematic in this context because municipalities receiving treatment prior to 2007 are in the severe poverty group, and thus poorer than those incorporated later. To explore common trends, we set up a false treatment similar to the one above by comparing moves made from 1993 to 1997 with those from 1998 to 2002 and the DD indicator for treatment equal to 1 in the 32 municipalities in the later period. Results show that even *before* the program, trends in out-migration and in-migration in the poorest municipalities were below the other municipalities—with broadly similar-sized point estimates to above. This suggests the DD estimates for the more recent years 2004–2007 may exaggerate potential program impacts on migration somewhat. Even if we cautiously treat them as valid, however, the findings suggest relatively small effects of the program on migration.[21]

[21] Also potentially relevant is whether the CCT influenced fertility decisions, for example, across eligible and ineligible families. Estimations comparing the characteristics of mothers (age, education, civil status, parity and whether they have electricity in the house) show no differences between those having a child just before the eligibility cutoff and those after. Number of children born to residents in program municipalities the year before or after the initial census interviews show no differences (prior to the 2007 national census). Additionally, rate of women having their first pregnancy was also virtually identical. Given its focus on the pre- and postnatal behaviors, another possibility is that the CCT influenced infant mortality, but we are unable to assess this. According to the World Bank, infant mortality was relatively low in El Salvador, 19.0 deaths per 1000 live births in 2007 declining to 14.4 by 2013.