Impact of *Juntos* conditional cash transfer program on nutritional and cognitive outcomes in Peru: Comparison between younger and older initial exposure*

Running title: Juntos CCT: comparison between younger and older initial exposure

Alan Sánchez

Grupo de Análisis para el Desarrollo

Guido Meléndez

Universidad de Piura

Jere R. Behrman

University of Pennsylvania

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Abstract

We evaluate whether the *Juntos* CCT program in Peru has a larger effect on children who benefited initially from the program during the first 4 years of life, compared to those children who benefited initially between ages 5 and 8. The former group was exposed during early-life sensitive periods, received the program for a longer period, and received more growth monitoring sessions and vaccinations. We find that exposure to *Juntos* led to an improvement in nutritional status and in cognitive achievement, both of which were larger (the latter only significant for) those initially exposed during the first 4 years of life.

JEL Classifications: I12, J13, O15

1. Introduction

Children in developing countries are exposed to multiple risk factors, including undernutrition and poverty, that reduce their chances to achieve their developmental potential (Grantham-McGregor et al., 2007; Black et al., 2017), with consequences in terms of education, labour productivity, poverty, marriage market outcomes, fertility and the next generation (Glewwe et al., 2001; Alderman et al., 2006; Hoddinott et al., 2008; Behrman et al,, 2009; Maluccio et al., 2009; Hoddinott et al., 2013). Evidence suggests that investing in human capital during childhood is one of the most effective ways to improve opportunities for poor children (Cunha and Heckman, 2006). Accordingly, many governments in developing countries have established conditional cash transfer (CCT) programs that transfer cash to poor families on the condition that pre-specified investments in children's health and education are made.

Despite heterogeneity in the characteristics of the CCTs implemented across countries, evidence shows that this type of program can be effective in improving the use of preventive health checkups for children aged 0 to 5 (Fiszbein and Schady, 2009), and school enrolment and school attendance – see Behrman et al. (2005) and Behrman et al. (2011); for a recent survey of the literature, see Glewwe and Muralidharan (2015). In contrast, the evidence on the impact on nutritional status and cognitive skills is not very clear.

Several studies suggest that CCTs can have a positive impact on nutritional status as measured by height-for-age, however many studies find no impact. See Manley et al. (2013) and de Groot et al. (2017) for recent reviews from the evidence in developing countries. While at first glance the evidence seems inconclusive, it is important noting that when focusing exclusively on experimental studies of CCTs that have attached conditionalities on health, the evidence does suggest a positive impact on height-for-age (see Manley et al., 2017). More generally, the nutritional impact of these programs depends on program characteristics (including the existence of conditionalities on health), child characteristics (the youngest age cohorts tend to benefit more), and population characteristics (largest impacts are found among the poorest families).

Evidence on the cognitive impact of these programs is also mixed. The number of available studies is more limited in this case. In the set of studies we were able to identify (9 in total) exposure to CCTs is evaluated during different age periods, including early childhood (Paxson and Schady, 2010; Macours et al., 2012), mid/late childhood (Barham et al., 2013a; Anderson et al., 2015), and adolescence (Baird et al., 2011 and 2013; Mo et al., 2013), while in 1 study initial exposure occurred between mid childhood and adolescence (Barrera-Osorio and Filmer, 2013). Evidence from a CCT pilot in Malawi (both studies from Baird et al.) and from a CCT pilot and a large-scale governmental program in Nicaragua (Macours et al.; Barham et al.) show that exposure to CCTs can enhance cognitive outcomes, both in the short-term and in the long-term.[1] At the same time, evidence from short/medium-term exposure to two CCT pilots in Cambodia and China (Mo et al.; Barrera-Osorio and Filmer)[2] and from short/long-term exposure to two large-scale governmental programs in Colombia and Peru (Baez and Camacho; Anderson et al.)[3] found no such impacts. A study from Ecuador detected a short-term impact only for the bottom quartile of the sample (Paxson and Schady).[4] Overall, evidence suggests that exposure to CCTs may improve cognitive outcomes at all age periods. The evidence is mixed for exposure during mid/late childhood and adolescence, whereas during early childhood the evidence consistently supports positive effects, but the number of studies is small.

The age of initial exposure may play a central role in the potential impact of CCTs on human capital. In particular, children exposed since early childhood are likely to benefit more than children benefited at later ages. There are several reasons for this hypothesis. First, the young children in the household are likely to benefit more from an increase in household expenditure due to the existence of sensitive periods of investment in human capital early in life. Second, by design the conditionalities linked to a CCT are likely to have larger and more persistent impacts on those benefited during early childhood. While children benefited by a CCT after the first 5 years of life are – typically – only required to be enrolled at school and attend classes on a regular basis, children benefited by a CCT who are less than 5 years of age must also attend health centers regularly, and these sessions often include growth monitoring. In addition, some CCT programs include vaccinations, nutritional supplements for infants and information components for mothers about best child care and feeding practices. Note that children that do not benefit from the CCT during the early childhood might also receive growth monitoring sessions and other health services during this period, but demand is likely to be lower without the CCT due to the direct and indirect costs families have to incur to access these services. Third, at a given subsequent age, those first exposed to an ongoing CCT when younger have been exposed longer than those exposed first when older.

The evidence regarding the role of age of initial exposure, particularly the effect of having a first exposure to a CCT program during early childhood (younger initiation of treatment) versus when children are a few years older and entering school (older initiation of treatment) is scarce. Barham et al. (2013b) evaluated the difference between younger initiation versus older initiation of treatment for the case of *Red de Proteccion Social* in Nicaragua, up to 7 years after the program ended. They found that those benefited during the first 1,000 days (which includes the *in utero* period and the first two years of life) – and exposed for a three-year period – had a larger cognitive impact than those exposed for the first time between ages 2 and 5 years – and who also received the program for 3 years. On the other hand, experimental evidence from the emblematic cash transfer program *Prospera* (aka *PROGRESA*, *Oportunidades*) in Mexico show that for a range of cognitive outcomes there was no difference between two groups of children benefited by the program during the first three years of life, with one of them exposed for an additional 18 months during this age period (see also Behrman et al., 2009).

In this study we revisit the impact of CCTs on child development paying special attention to the role played by the initial age of exposure. We focus on the *Juntos* program, a large-scale CCT implemented in Peru since 2005. Previous studies show that *Juntos* led to an increase in the demand for health checks for children between the ages of 0 and 5 years, an increase in household consumption and a reduction in poverty levels (Perova y Vakis, 2012). The evidence also shows that *Juntos* had an impact on nutritional outcomes but not on cognitive outcomes (Sánchez and Jaramillo, 2012; Andersen et al., 2015). The main goal and contribution of this paper is to evaluate whether *Juntos* had a larger effect on a cohort of children that benefited for the first time during the first four years of life compared to another cohort of children that benefited for the first time between ages 5 and 8 years. Conceptually, the most related study is Barham et al. (2013b); however our setup is different because in that study the period of treatment was kept the same for both cohorts. We apply non-experimental methods to data recently released from the Young Lives study for a paired-siblings sample in Peru. To control for fixed household and child characteristics related *inter alia* to selection into the program, we apply child fixed-effects and household fixed-effects methods. These strategies allow us to obtain estimates of the pure (absolute) impact of the program in comparison with a pure control group as well as its possible additional impact for younger children (younger initiation versus older initiation). We find that the nutritional benefits are larger for those first exposed during the first four years of life. In addition, cognitive gains are observed exclusively among those treated during the first four years. These investments earlier in life may have advantages for any or all of the three reasons noted above: early-life sensitive periods, additional programmatic investments directed towards early-life, and longer program exposure. One limitation for our analysis is that we lack a pure baseline for some of the younger siblings in the sample. To deal with this, we present alternatives estimates where the baseline observation for the younger sibling is replaced by that of the older sibling at a similar age, and also estimates that control for number of months of exposure of the younger siblings. Conclusions remain unchanged.

The paper is structured as follows. In Section 2 we describe the *Juntos* program. In Section 3 we describe our empirical strategy. In Section 4 we present the Young Lives dataset. In Sections 5 and 6, we present and discuss our main results. Section 7 concludes.

2. Description of the *Juntos* program

Juntos targets poor families mainly in rural areas in Peru. The program was established in 2005, initially serving 70 districts in the southern highlands. Its geographical coverage has increased gradually over time to other areas of the highlands and the Amazonian jungle. To date, it has covered 736 thousand families in 1304 districts (out of 1943 districts in the country). It is estimated that 72% of all potential household beneficiaries were already covered by the program by 2015 (MIDIS, 2015). Up to 2009, the program made a monthly fixed transfer of 100 Nuevos Soles ($30 or around 10% of monthly consumption of poor households). Since 2010, this changed to a bimonthly transfer of 200 Nuevos Soles.

In order to receive the transfer, the following conditionalities were relevant to our study period, see PCM (2008) and MEF (2008): (i) children aged up to 59 months must receive comprehensive health and nutrition care (including growth monitoring and complete vaccinations[5]); (ii) pregnant women must receive comprehensive health care (including ante-natal checkups and complete vaccinations); (iii) children aged between 6 and 14 years must be enrolled in school and have an attendance rate of at least 85%; (iv) children must have a national ID. Although the frequency of the ante-natal checkups and growth monitoring is not directly specified by the program, the Ministry of Health has standards about the services provided by public health posts. According to the 2008 standard[6,7] pregnant women must have at least 6 ante-natal checkups, whereas the number of growth monitoring controls (*Control de Crecimiento y Desarrollo*, CRED) was as follows: 12 measurements from months 0 to 11 (monthly frequency); 6 measurements from months 12 to 23 (bi-monthly frequency); and, 12 measurements from months 24 to 59 (quarterly frequency).[8] The nature of the growth monitoring measurements has also evolved over time. Up to 2007, it consisted essentially of the following components: (i) height and weight surveillance, (iii) assessment of psychomotor skills, (iii) anemia screening, (iv) assessment of visual and auditory acuity, (v) evaluation of vaccination schedule, (vi) delivery of vitamin A and ferrous sulfate supplementation (to be consumed at home), (vii) deworming, among others. In 2008, counseling about nutrition and hygiene for the mother was added (Cruzado, 2012). In addition, since 2008 the Ministry of Health introduced vaccinations against diarrhea and respiratory diseases in the area of influence of the *Juntos* program, though these vaccinations are independent of the CRED sessions.[9] The fulfilment of the responsibilities is monitored monthly by *Juntos* fieldworkers, who are able to access information from schools and health centres for this purpose. It is important to note that although fieldworkers monitor school attendance, attendance to CRED sessions and vaccinations, the content of the CRED sessions is not monitored. In addition, evidence suggests the number of CRED sessions the child and mother were required to attend by *Juntos* used to be below the standards required by the

Ministry of Health (MEF, 2008). Nonetheless, evidence suggest that *Juntos* fieldworkers give advice to beneficiary mothers to encourage them to comply with as many requirements as possible (Correa and Roopnaraine, 2014).

In terms of the eligibility rule, the *Juntos* program has a two-stage selection process (at the district level and at the household level). Between 2005 and 2007—what we call the "first *Juntos* expansion"—638 districts were enrolled. These were essentially the poorest 638 districts in the country according to the official poverty ranking (Poverty Map from 2005), all of them located in the two lowest quintiles, the majority in the Highlands. Within each selected district, a household eligibility criterion was applied (second stage), and only households classified as poor under this criterion and located in eligible districts were eligible for the program.[10] A second expansion period started in 2010. In this study we deal with the first expansion period.[11]

3. Conceptual framework

To set up ideas about the impact of the *Juntos* CCT, we follow the human capital model proposed by Glewwe and Miguel (2008) – see also Behrman and Lavy (1994) – with only a slight adaptation in the age periods considered by the authors: from ages 0 to 4 is period 1, and from ages 5 to 8 is period 2. In this model, the production function of cognitive skills in period 2 (denoted as T_2) is a function of nutritional status in periods 1 and 2 (H_1 and H_2, respectively), a relation that has been shown in several studies (Glewwe et al., 2001; Alderman et al., 2006; Maluccio et al., 2009). In addition, T_2 is a function of educational inputs in periods 1 and 2 (EI_1 and EI_2, respectively), years of schooling (YS), and other factors – school characteristics and innate ability (denoted as SC and μ, respectively) – as follows:

$$T_2 = T_2(H_1, H_2, EI_1, EI_2, YS, SC, \mu) \qquad \text{(i)}$$

In turn, nutritional status in period 1 (H_1) is determined by child consumption in period 1 (C_1^c), health inputs in period 1 (M_1), and other factors – the local health environment in period 1, and innate health (denoted as HE_1 and ε, respectively). Analogously, nutritional status in period 2 (H_2) is a function of investments in period 2 and of health status in period 1:

$$H_1 = H_1(C_1^c, M_1, HE_1, \varepsilon) \qquad \text{(ii)}$$

$$H_2 = H_2(H_1, C_2^c, M_2, HE_2, \varepsilon) \qquad \text{(iii)}$$

Although inputs from periods 1 and 2 matter in the production of cognitive skills and nutritional status in period 2, evidence from different disciplines (summarized in Cunha and Heckman, 2006, see also Grantham-McGregor et al., 2007) suggests that investments in the first few years of life (i.e., period 1) are more productive at the margin than investments in other stages of the life cycle (period 2 in this case).

Assuming that parents care about H_1, H_2, T_2 and own consumption in the utility function, that they maximize utility subject to (i), (ii), (iii) and budget constraints, and under the usual regularity conditions, parents will choose an optimal level of investments in their children's nutritional status and cognitive skills. However, investments in child development are likely to be sub-optimal (compared to first best) if parents are income-constrained.

Based on this model, children whose families are *Juntos* recipients are likely to improve their nutritional status through: (a) an increase in health inputs in period 1 (growth monitoring controls and vaccinations) due to the existence of health conditionalities; (b) an increase in child consumption (e.g., food) in both periods due to the cash transfers; (c) an increase in health inputs in both periods due to the cash transfers (e.g., access to clean water); and, (d) self-productivity of nutritional status. Children from *Juntos* families are also likely improve cognitive skills through: (a) an increase in years of schooling in period 2 due to the school enrolment conditionality; (b) an increase in educational inputs (e.g., educational toys) in both periods due to the cash transfers; and, (c) cross-productivity between nutritional status and cognitive skills.

In our data we observe two cohorts of children, one benefited in periods 1 and 2 of our model, and another one benefited only during period 2. Following the Glewwe and Miguel model, children whose families were recipients of *Juntos* in periods 1 and 2 are likely to benefit more from the program compared to children who were recipients only in period 2. First, they benefited from more growth monitoring and vaccinations (while these services are available for both cohorts, by design of CCTs health conditionalities are only included in period 1). Second, they benefited from the cash transfers (which might increase child consumption, health and education inputs) during a sensitive period. Third, they received a higher dose, i.e., they benefited from improvements in child consumption, health inputs, and educational inputs in two periods instead of one.

Based on the above, our hypothesis is that both groups of children – those that were program recipients in periods 1 and 2, and those that were recipients only in period 2 – can benefit in terms of nutritional status and cognitive skills, however the former group is likely to benefit more in both instances.

4. Data

4.1 The Young Lives Study

The Young Lives Study (Younger Cohort) in Peru tracks a cohort of 1,952 children born in 2001 or 2002.[12] The baseline survey was administered in 2002. Three additional waves took place in 2006, 2009 and 2013. Attrition rates in the second, third and fourth waves are 3.5%, 4.4% and 6.3% (respectively). According to Sánchez and Melendez (2015) the main reason for attrition is interview refusals. Compared to other longitudinal studies in developing countries, attrition rates are relatively low (Dercon and Outes-Leon, 2008).

Although the Young Lives Study was originally designed to track only one child per household (the index child), during the last two survey waves (2009 and 2013), information was collected for the next younger siblings of the index children—provided he/she was at least 24 months old, was able to stand and was in the community at the time of the survey. These younger siblings were born between 2003 and 2007. For this analysis we focus on the paired-siblings sample (households with data on the index children and their younger siblings). From here onwards, we use the term "older siblings" to refer to the index children. Anthropometry (height and weight) was measured for older siblings at ages 4 to 5 (round 2) and 7 to 8 (round 3), and for younger siblings at ages 2 to 6 (round 3) and 6 to 10 (round 4).[13] Similarly, the Peabody Picture Vocabulary Test – a test designed to measure the acquisition of vocabulary – was administered during each of these visits.[14]

The full paired-siblings sample comes from 831 households.[15] Among them, 322 have benefited from *Juntos* at some point between 2005 and 2013, whereas 509 have not. Figure 1 illustrates the relationship between the data available, the roll-out of the program and the ages of the children in rounds 1, 2, 3 and 4.

We concentrate on the "first *Juntos* expansion" (2005-2007) and focus the analysis on older siblings aged 5 to 8 years when first exposed to *Juntos* and on younger siblings aged 0 to 4 years at their first exposure. We consider the 174 households that benefited from the program between 2007 and 2009 as the treated group, whereas the 509 households not treated in this period as the control group. Households that started receiving the transfer between 2005 and 2007 or between 2010 and 2013 are excluded from the analysis.

INSERT FIGURE 1 HERE

4.2 Descriptive statistics

Main characteristics of the children and families considered for the analysis (the paired-siblings sample) are reported in Table 1. In relation to how these households compare to Young Lives households with one sibling or households where the other sibling did not fulfill the characteristics required for enrolment (less than 24 months old, too young to stand or older than the index child), this information is reported in Table A.1 in the Appendix A.[16]. The table is split to distinguish between *Juntos* and non-*Juntos* children. Anthropometric outcomes (height-for-age Z-scores, stunting and severe stunting) and PPVT scores (standardized by age in years) are observed at ages 4-5 and 7-8 for older siblings and at ages 2-6 and 6-10 for younger siblings. The anthropometric data are available for 1,144 children in the balanced panel of paired-siblings, whereas the cognitive achievement data (PPVT scores) are available for 838 children.[17] Among treated households, younger siblings were first exposed to the program at ages 0 to 4, whereas older siblings were first exposed at ages 5 to 8. The distribution of the age of the child at the first month of exposure to *Juntos* is reported in Figures A.1 (for the paired-siblings sample with anthropometric data) and A.2 (for the paired-siblings sample with cognitive data). In order to compare outcomes at a similar age, we compare younger siblings observed in 2013 (Round 4) at ages 6-10 with older siblings observed in 2009 (Round 3) at ages 7-8. Since we look at households enrolled by *Juntos* between 2007 and 2009, younger siblings from recipient households were exposed to the program for a longer time (5.9 years on average, versus 1.9 years on average among the older siblings).

INSERT TABLE 1 HERE

Some aspects of the data are worth highlighting. First, the younger siblings are on average better-off than the older siblings: they report higher levels of height-for-age, lower levels of stunting and severe stunting, and higher standardized scores in the PPVT than the older siblings at comparable ages. For anthropometric outcomes this is consistent with a trend observed in Peru during the last decade, in turn associated with economic growth, reduction in poverty rates and increased animal-source foods in diets (Humphries et al, 2014). Despite this, stunting rates among *Juntos* and non-*Juntos* children are still high. Second, there is evidence of substantial catch-up growth among treated and untreated. This is consistent with evidence from other studies that used Young Lives data (Crookston et al., 2010 and 2013). Among the treated older siblings, stunting fell from 63% to 44%, whereas among the treated younger siblings the reduction was from 57% to 40%.

Third, children from the treated group fare worse than children in the control group in the pre-treatment period in both nutritional and cognitive areas at comparable ages. This highlights the importance of controlling for selection into the program. Finally, it is important to note that school enrolment among primary school aged children was close to universal for both treated and non-treated households. This suggests that, if *Juntos* were to have an impact on cognitive achievement, it is unlikely this effect is mediated by school enrolment.

5. Empirical strategy

We evaluate the importance of the age of initial exposure on nutritional and cognitive outcomes. For the analysis we focus on two groups: children first exposed to the program between ages 0 and 4 years and children exposed to the *Juntos* program for the first time between ages 5 and 8 years. For simplicity, the former is labeled as the younger initiation-treatment group and the latter as the older initiation-treatment group. We look at final outcomes at approximately the same age, regardless of the age of first exposure. For this reason, the length of exposure is longer (i.e., the dose is higher) for the former group – once a household is affiliated, it remains affiliated as long as conditionalities are fulfilled and the family does not move to another district.

Given the program design, a simple comparison of treated and non-treated children would deliver biased estimates of the impact of the program. To estimate program effects while dealing with program selection, we use the following child fixed-effects specification:

$$Y_{i,r} = \alpha_0 + \alpha_1 After_{i,r} + \alpha_2 Benef_i + \beta\left(After_{i,r} * Benef_i\right) + Age_{i,r}\Gamma + \sigma_i + \mu_{i,r} \qquad (1)$$

where $Y_{i,r}$ is a generic outcome of child i observed in period $r = 0{,}1$; $After_{i,r}$ is a dummy that takes the value of 1 if the dependent variable is observed after *Juntos* was implemented and 0 otherwise; $Benef_i$ is a binary variable that takes the value of 1 if the child's i family has been selected as a *Juntos* recipient and 0 otherwise; vector $Age_{i,r}$ contains a set of dummies that control for child's age in years at the moment $Y_{i,r}$ is measured; σ_i are child unobserved characteristics that are fixed over time; and, $\mu_{i,r}$ is a random error term. Within this structure, the absolute impact of exposure to *Juntos* is given by β. This is a double-difference strategy. Since the variable $Benef_i$ does not vary over time, the coefficient α_2 is not identified. Note that in this model specification it is not necessary to control for child and household characteristics that are time-invariant, as they are absorbed by the individual fixed effects. We estimate equation (1) separately for the younger initiated-treatment group and the older initiated-treatment group.

Next, in order to formally test the importance of the age of initial exposure, we pool the data of both groups of children and estimate the following regression:

$$Y_{(i=o,y),r} = \alpha_0 + \alpha_1 After_{o,r} + \alpha_2 After_{y,r} + \alpha_3 Benef_f + \beta_1\left(After_{o,r} * Benef_f\right) + \beta_2\left(After_{y,r} * Benef_f\right) + Age_{i,r}\Gamma + \sigma_i + \mu_{i,r} \qquad (2)$$

where subscripts o and y stand for the older sibling and the younger sibling of family f (respectively). Accordingly, the coefficients β_1 and β_2 measure the impact of exposure for the older sibling and the younger sibling. The difference between these two coefficients can be interpreted as the difference between treatment initiated when younger (higher dose, exposure during early-life critical periods, and with growth monitoring sessions and vaccinations) and treatment initiated when older (lower dose, not exposed during early-life critical periods, and without growth monitoring sessions and vaccinations). This difference is of key interest for our analysis. We test the null hypothesis that both coefficients are equal. Finally, note that α_1 captures secular changes in the outcome for older siblings (between periods 1 and 2), whereas α_2 captures the analogous effect for younger siblings (between periods 2 and 3).

Throughout specifications (1) and (2), it is implicitly assumed that children were not exposed to *Juntos* at baseline ($\mathrm{After}_{\mathrm{i},0} = 0$). This is true for the older siblings but it is an assumption for the younger siblings given that the first time they were observed *Juntos* had already been operating for between 1 and 2 years. The lack of a pure baseline for the younger siblings is further discussed in sub-section 5.2.

We complete the analysis by incorporating an alternative strategy to measure the additional impact of *Juntos* for the younger siblings. We compute a model that compares outcomes of younger and older siblings from treated households with outcomes of younger and older siblings from non-treated household, as follows:

$$Y_{if} = \theta_0 + \theta_1 YSib_i + \theta_2 Benef_f + \beta\left(YSib_i * Benef_f\right) + Age_i\Omega + \theta_3 Female_i + Yob_i + \pi_\mathrm{f} + \mu_{if} \qquad (3)$$

where Y_{if} is the outcome of child i from household f observed in the final round (after *Juntos* started operating); $YSib_i$ takes the value of 1 if child i is the younger sibling, 0 otherwise;

$Benef_f$ takes the value of 1 if household f is a *Juntos* recipient, 0 otherwise; Age_i is a control for age in years; $Female_i$ takes the value of 1 if child is a female, 0 otherwise; Yob_i are year of birth fixed effects; and, π_f is a household fixed effect. Note that this specification does not make use of changes in outcomes over time. Similar to Equation (1), θ_2 is not identified. The coefficient of interest is β, which measures the additional impact of the program for the younger sibling. Specifically, this coefficient measures the marginal difference between the outcomes observed for the younger sibling and the same outcome observed for the older sibling when both children come from a household that is a *Juntos* recipient, compared with younger siblings and older siblings in non-recipient households. This also is a double-difference strategy. Note that the key difference between Equation (2) and Equation (3) is that the former exploits within-child variations (over time) while the latter exploits within-siblings variations (in the same period of time). Equation (3) has the additional advantage that it does not require baseline information.

6. Results

6.1 Main results

Table 2 reports the absolute impact of the "first *Juntos* expansion" separately for older siblings (Panel A) and younger siblings (Panel B). This corresponds to Equation (1). In these results and in all subsequent results, standard errors are clustered at the finest level (child level for estimations in which siblings are treated separately, and household level when data from both siblings are pooled). In terms of the absolute impact of the program on nutritional status, for the younger siblings we observe an improvement in height-for-age by 0.19 standard deviations and a reduction in stunting and severe stunting by 8.9 and 11.6 percentage points (respectively). This specification also shows evidence that exposure to *Juntos* for the younger group led to an improvement on Peabody test scores by 0.2 standard deviations.

In contrast, no impact is detected for the older siblings, who started receiving the treatment when older. In particular, the coefficient associated with PPVT scores is close to zero for this group. It is also worth noting that in all cases the coefficients corresponding to the younger siblings are larger (in absolute values) than those corresponding to the older siblings.

INSERT TABLE 2 HERE

To directly test for differences in the impact of the program between these two groups of children, in Table 3 we report results for the empirical specification corresponding to Equation (2). Pooling the sample of younger siblings and older siblings has the additional advantage of increasing sample size. Results from Table 3 show evidence of the additional impact of the Peruvian CCT program. The difference between coefficients β_1 and β_2 is informative of the differential impact of initial treatment when younger (higher dose, during early-life critical windows, with additional preschool nutritional monitoring and vaccination components) versus initial treatment when older (lower dose, not during early-life critical windows, and without the preschool nutritional monitoring and vaccination components). As in Table 2, in all cases results suggest larger impacts for the former group. In order to test this hypothesis formally, we run an F-test with the null hypothesis that the difference between the two coefficients is zero. This hypothesis is rejected in all cases. This shows that being in the group first exposed at a younger age rather than being in the group first exposed at an older age made a difference for both nutritional and cognitive outcomes. In addition, this model specification also shows evidence of a positive absolute impact on height-for-age and a reduction on severe stunting for the group that was older when treatment was initiated (compared to the control group). Similar to the estimates for equation (2), the estimated coefficient associated with PPVT is close to zero for this sub-group.

INSERT TABLE 3 HERE

In Table 4 we report results for Equation (3), which is an alternative way to estimate the additional impact of the program for children with younger ages at initiation. Each of the point estimates obtained in this model suggest larger impacts for the group for which treatment was initiated when younger compared to the group for which treatment was initiated when older. However, only the coefficients associated with severe stunting and PPVT are statistically significant in this case (implying a reduction by 9.6 percentage points, and an increase by 0.5 standard deviations, respectively). The coefficient that measures the additional impact on height-for-age and stunting are smaller than those obtained when estimating Equation (2), but have the same sign.

For nutritional outcomes, the estimates of the additional impact of the program obtained from Equations (3) are smaller (in absolute values) than those obtained from Equation (2), whereas the opposite is the case for cognitive achievement. This is likely to stem from differences in the sources of variation used to identify the impact of the program: while Equation (2) exploits within-child variation, Equation (3) exploits between-sibling variation. Given that both types of fixed-effects deal with program selection, it is not straightforward to conclude that one specification is superior to the other. Rather, both models should be seen as alternative estimates of the program impact. Despite these differences, one common feature across the three specifications is that the impacts of *Juntos* on severe stunting and cognitive achievement are larger for the younger siblings.

INSERT TABLE 4 HERE

To test for differential effects by gender, we re-estimated Equations (1), (2) and (3) adding interaction terms between gender and all the right-side variables. Evidence from the interacted versions of Equations (1) and (2) suggest that *Juntos* had an impact on severe stunting only for girls. However, this evidence does not show up in the interacted version of Equation (3). For this reason, we cannot a claim a differential effect according to gender on nutritional status. In addition, none of these specifications suggest a differential impact on PPVT scores according to the gender of the child (results available upon request).

6.2 Robustness checks

(a) Refining the control group

One aspect that might bias our results is the fact that some of the households in the control group are not eligible for *Juntos*. To increase the comparability of *Juntos* and non-*Juntos* families, as a robustness check we further restrict the study sample to households located in districts ranked in the lowest two quintiles of the 2005 Peru Poverty Map (during the "first *Juntos* expansion", only districts located in these quintiles were considered). When restricting the sample in this way, differences between the treated group and the control group at baseline are reduced considerably (see Table B.1 in Appendix B). A trade-off for this strategy is that sample size also falls considerably: from 1,144 to 590 children with anthropometric data, and from 838 to 372 children with cognitive data. The reduction in sample size means that results need to be interpreted with caution. Results of this robustness check are reported in Appendix B for Equations (1), (2) and (3) –Tables B.2, B.3 and B.4, respectively.

For Equations (1) and (2), estimates of the program impact on nutritional status are similar to those obtained in our main results. In this case, the impact on PPVT becomes statistically insignificant for the younger siblings and also when comparing the additional impact of the program (younger siblings versus older siblings). In the case of Equation (3), point estimates are substantially larger in absolute values than those reported in our main results. While only the additional impact of the program on severe stunting stands as statistically significant, point estimates for the additional impact on height-for-age and PPVT scores are still suggestive of a larger impact for the younger siblings.

Overall, results from this robustness check reinforce the conclusion that the younger siblings benefited more from the program than the older siblings in terms of nutritional status. A similar conclusion cannot be reached about improvements in PPVT scores; however it is important to note that the sub-sample of siblings with cognitive data available for this robustness exercise is very small.

(b) Using an alternative baseline

In the case of Equations (1) and (2), the lack of a pure baseline for the younger siblings is a limitation for our analysis. However, controlling for outcomes from a previous age-period is important in order to control both for selection into the program and for child-level heterogeneity. We analyze the consequences of following this strategy and report alternative estimates. Younger siblings in the treated group had been exposed to *Juntos* for between 0 and 3 years in the first observation available for them (1.9 years on average). Assuming the true impact of *Juntos* is positive (i.e., the pre-treatment observation of the younger sibling was positively affected by the program), this would reduce the difference between pre-treatment and post-treatment outcomes in the treated group, leading to a bias toward zero in the estimation of the program effects. Since the sign of the expected bias is negative, this suggests we can treat results obtained for the younger siblings as a lower bound of the true impact of *Juntos* on this cohort. To check the robustness of our results, we exploit the fact that paired-siblings are observed to use the observation of the older sibling at ages 4 to 5 years (prior to *Juntos*) as a baseline for the sibling at ages 2 to 6 years. In other words, we use the observation from the older sibling at baseline as a representation of what would have been the baseline for the young sibling in absence of the program. These results are reported in Appendix B (Tables B.5, B.6). None of the original conclusions change. In fact, coefficients that measure both the absolute impact of the program (for the younger siblings) and the additional impact (younger siblings versus older siblings) are in all cases larger (for nutritional status and cognitive outcomes), suggesting that our main results might in fact be downward biased.

We also report alternative estimates of Equations (1) and (2) that control for the duration of the exposure of the younger siblings in the first round. In order to introduce such an indicator in the fixed-effects estimation, a variable is defined that takes the value of 0 for the first observation for the younger siblings, and takes the value of the number of years of exposure during the first round for the second observation. *A priori* we expect lower estimates of the impact of Juntos for the younger siblings in this case, given that the duration of exposure at baseline is likely to capture part of the program impact. Estimates are reported in Appendix B (Tables B.7 and B.8). Focusing on Table B.7, the coefficients associated with the program impact for the younger siblings lose statistical significance for severe stunting and height-for-age, and remain statistically significant for stunting and PPVT. However, it is important to note that the coefficients associated with the duration of exposure and program impact tend to have opposite signs, which suggests a high degree of collinearity between the two variables (the correlation between these two variables is 0.91). In other words, it seems difficult to disentangle the impact of the duration of exposure up to the first round from the impact of the program between rounds. Despite this limitation, estimates of the additional impact of the program (Table B.8) also suggest that the impact of the program was larger among those treated when younger.

(c) Falsification test

As a final way to check the robustness of our results, we used data from before *Juntos* started operating to perform a falsification test. Specifically, we used data for the older siblings from 2002 (Round 1) and 2006 (Round 2) – both periods prior to the launching of Juntos in our sample – and labeled these rounds as baseline and follow-up periods (respectively). These results are reported in Table B.9 in the Appendix for the anthropometric indicators (PPVT was not administered in Round 1). Though in our main estimates no program impact was detected for the older siblings, it is still reassuring that no program impact is detected in this falsified scenario either.

7. Discussion

Evidence to date shows that *Juntos* has an impact on nutritional status outcomes (Sánchez and Jaramillo, 2012; Andersen et al., 2015) but not on cognitive outcomes (Andersen et al., 2015). Using a different sample of children and a different estimation strategy, our findings confirm the nutritional impact of *Juntos*. We detect an improvement in height-for-age and reductions in stunting and in severe stunting due to exposure to the program among the children who were younger than five years of age when first exposed to the program.

Our results also show that the reduction in severe stunting was larger for those first exposed during the first four years of life, compared to those first treated when older than four years. Most of our results also lead us to conclude that the improvement in height-for-age was also larger for those exposed during the first four years of life. These results are likely a result of the *Juntos* design, which delivers cash transfers to households only if all children aged 0 to 59 months had attended their growth monitoring sessions and received complete vaccinations for their age. It is well recognized that access to both health services could prevent malnutrition and promote child´s health (Kapil et al, 1994; Griffiths and Del Rosso, 2007). Although the demand for growth monitoring sessions is not observed in the Young Lives data, evidence from Perova and Vakis (2012) that use data from the Peru National Household Survey show that *Juntos* led to an increase in the demand for health checks (during the last 3 months) for children under the age of 5. Note that children in the older group when treatment was initiated might have also received growth monitoring sessions and vaccinations when younger because these services were also available for them, but their families were not incentivized by the cash transfer in that period. In addition, the possibility of sensitive early-life periods for investments in human capital suggests that children treated early in life might have benefited more from the increase in household financial resources due to the transfers (e.g., more resources available to improve access to food, medicines, and clean water). This hypothesis is also consistent with findings from Perova and Vakis (2012) and Escobal and Flores (2012). Both studies find evidence that *Juntos* led an increase in household total consumption, food consumption and non-food consumption. Finally, at any given age, those who initially were exposed to the ongoing *Juntos* program when younger had been exposed longer. With the data that we use we cannot identify the relative importance of these three possible advantages of initial program exposure when younger.

We also find evidence of cognitive gains for those in the younger initiation treatment group, but not for those in the older initiation treatment group. To a certain extent this result was expected given the larger nutritional impact observed for the younger initiation treatment group and the existence of a biological nutrition-cognition nexus (Glewwe et al., 2001; Alderman et al., 2006; Maluccio et al., 2009). However, note that the younger initiation treatment group might also have benefited more from a cognitive point of view because they received educational inputs for a longer period due to the transfers (e.g., the family had more resources available to invest in education), and because they benefited by these investments during a sensitive period.

In addition to the channels previously mentioned and that are consistent with our conceptual framework, our results could also have been reinforced by behavioral changes at the household level, possibly interacting with the influence of *Juntos* workers among the beneficiaries – evidence shows that *Juntos* workers tend to ask families to invest in their children beyond the conditionalities, giving them suggestions about how to use their money (Correa and Roopnaraine, 2014).

Given that school enrolment at the primary school level in Peru is close to universal (98 percent of children aged 6 to 12 in the Young Lives study are attending school full-time), it is not surprising that *Juntos* neither had an impact on school enrolment nor the age of treatment initiation induced a differential increase in school enrolment among the children treated at a younger age (estimates available from lead author). However, the fact that treatment initiation when younger led to better nutrition might have led some parents to invest more in the pre-school education of their younger children, either by increasing enrolments and/or by allowing them to start pre-school at younger ages. While a differential increase in pre-school investments cannot be ruled out, it is worth noticing that a recent study that uses data from Young Lives Peru (Cueto et al., 2015) shows that age of pre-school enrolment does not predict PPVT scores in rural communities, possibly due to the low-quality of the educational services offered in these locations. Therefore, it is not clear that this channel was operational.

Finally, given that the older child is initially the target child for the CCT payments, the parents, through observing the benefits of the program in terms of improvements in their older child's development, may have been induced to invest more in the younger child than they would have in the absence of the program.

There are two important caveats to consider when interpreting our results. First, children in the younger initiation treatment group not only were exposed earlier in life when they may have been in sensitive growth stages, but compared to the older initiation treatment group they were also exposed to comprehensive health and nutrition care services and were exposed for a longer time. It is possible a combination of all these aspects drives our results, but we cannot identify the separate effects with our data,

Second, one aspect that might have reinforced the impact of *Juntos* both on nutritional status and on vocabulary acquisition among children initially treated at younger ages is that this was also a younger generation. Although our model specifications control for the fact that younger siblings might have benefited more from economic growth and from improvements in public services than their older siblings, it cannot be completely ruled out that over time health and education services might have improved more in *Juntos* districts compared to non-*Juntos* districts, thereby promoting larger gains for the younger siblings in treated areas. If that were the case, one limitation of our results is that we would not be able to disentangle the importance of enrolling early in *Juntos* from the importance of having higher-quality health and education services in the *Juntos* areas.

Finally, there are some challenges to our analysis that are in turn related to the nature of the available data. First, even when focusing on the sub-sample of households located in the two lowest quintiles, differences between households in the treatment and control groups remain. Second, we lack a pure baseline for the younger siblings because the program had been operating for up to two years when they were first observed. While it is important to be aware of these limitations, it is reassuring that our results remain unaltered through the different robustness checks performed.

The potential of our data to analyze heterogeneous impacts is limited by the sample size available. Despite this limitation, it is important to fully understand the importance of the age of initial exposure to the program. For this reason, in Table B.10 (Appendix B) we split the sample of younger siblings into those first exposed during ages 0 and 2 years, and those first exposed during ages 2 and 4 years. Point estimates suggest the impact of the program on height-for-age and PPVT scores was larger for those who first benefited during the first 2 years of life. However, the hypothesis that the impact was the same for both sub-groups in both indicators cannot be rejected.

8. Conclusions

Despite the popularity of CCT programs around the world and the substantial evidence that has been collected about their impact on household poverty and on the use of health and educational services, to our knowledge there is very limited evidence (Barham et al, 2012b) showing that children who first benefited when younger from CCT programs are also likely to benefit more from them. In Barham et al., the authors used an ex-ante design in which the dose of the treatment was kept the same for the early and late treatment groups. In our case, we cannot keep the dose of the treatment constant across groups. While this might be seen as a limitation, the question remains of interest from a policy perspective, especially in a context where doubt is cast on the potential of CCT programs to improve cognitive outcomes.

As a result of our analysis we find that *Juntos* beneficiaries improved their nutritional status and cognitive achievement. We also find that the nutritional impact was larger for those exposed during the first 4 years of life. In addition, cognitive gains were observed exclusively among those treated since the early childhood period. The second and third findings are consistent with the notion that those benefited by cash transfer programs earlier in life are likely to benefit the most. While we are unable to identify the specific mechanism by which children initially exposed during the first 4 years of life accumulated more human capital, our results are informative of the substantial benefits that *Juntos* is bringing to Peruvian children benefited by this program since birth, and reinforce the notion that the earlier children are targeted by social protection schemes such as CCTs, the larger are likely to be the benefits in the long-term.

Appendix A

Table A.1

Characteristics of paired-siblings households sample versus rest of households

	Two-sibling households (1)	Rest of YL households (2)	(1)-(2)
Child characteristics:			
Older Siblings			
Height-for-age 1 to 2	-1.39	-1.22	-0.17***
Height-for-age 4 to 5	-1.67	-1.44	-0.23***
Height-for-age 7 to 8	-1.30	-1.05	-0.26***
PPVT's raw score 4 to 5	26.02	32.36	-6.34***
PPVT's raw score 7 to 8	55.23	62.06	-6.83***
% of female	49.94	50.61	-0.67
Younger Siblings			
Height-for-age 4 to 5	-1.48		
Height-for-age 7 to 8	-1.11		
PPVT's raw score 4 to 5	27.33		
PPVT's raw score 7 to 8	64.52		
% of female	46.41		
Household characteristics (2002, round 1)			
Housing quality index	0.37	0.44	-0.07***
Number of public services	1.66	2.02	-0.36***
Number of missing assets	4.37	3.99	0.37***
% of households cooking with gas/ electricity	0.35	0.52	-17.41***
Wealth Index	0.37	0.47	-0.10***
% of indigenous caregivers	38.27	24.05	14.22***
Caregiver's age	25.46	28.22	-2.76***
Caregiver's years of education	6.93	8.35	-1.41***
% of hh head in agriculture	0.81	0.77	3.92**
Household size	5.60	5.79	-0.19*
% of rural	40.09	24.65	-15.45***
Altitude	1927.17	1512.22	414.95***
District characteristics (in 2005)			
% of poverty	62.46	54.69	7.77***
% of chronic malnutrition	28.17	21.86	6.31***
Number of households	843	1,059	

Note: columns (1) reports descriptive statistics for households that are part of the paired-siblings sample. Column (2) reports the same information for the rests of household in the Young Lives sample (households with only one child, or where the additional siblings was less than 24 months old, too young to stand or older than the index child). Asterisks denote if the difference between columns (1) and (2) is statistically significant. *** p<0.01, ** p<0.05, * p<0.1.

Figure A.1

Age in months during first exposure to Juntos CCT (sample with anthropometric data available)

Note: sample composed by children from the 572 households with anthropometric data available for both siblings in two rounds (1,144 children in total).

Figure A.2

Age in months during first exposure to Juntos CCT (sample with cognitive data available)

Note: sample composed by children from the 308 households with cognitive data available for both siblings in two rounds (616 children in total).

Appendix B

Table B.1

Descriptive statistics (children from poorest quintiles)

	Older Siblings		Younger siblings	
	Treated	Never treated	Treated	Never treated
	(A)	(B)	(A)	(B)
Child characteristics:	Round 2 (2006)		Round 3 (2009)	
Age (in years)	4.7*	4.8	4.5*	4.3
Height-for-age	-2.41***	-1.70	-2.11***	-1.38
Stunting	0.64***	0.39	0.57***	0.30
Severe stunting	0.25***	0.09	0.17***	0.04
PPVT raw score	-0.54**	-0.29	-0.46**	-0.05
	Round 3 (2009)		Round 4 (2013)	
Age (in years)	7.6	7.6	8.4*	8.2
Height-for-age	-1.93***	-1.24	-1.68***	-1.18
Stunting	0.45***	0.20	0.41***	0.19
Severe stunting	0.14***	0.01	0.03	0.01
PPVT raw score	-0.51***	-0.12	-0.11*	-0.16
Number of observations:				
Anthropometric data	133	162	133	162
PPVT scores	75	156	50	94

Note: Sample is restricted to children from households located in districts from the two poorest quintiles. Treated children come from families that were enrolled into Juntos between 2007 and 2009. A t-test was performed to compare columns (A) and (B).

*** p<0.01.

* p<0.1.

Table B.2

Robustness check – Children from poorest quintiles

Impact of the Juntos CCT (split by cohort)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
Panel A: Older siblings				
$\text{After}_{i,r}$	-0.168***	-0.019	0.412***	1.915
	(0.058)	(0.029)	(0.074)	(0.000)
$(\text{After}_{i,r} * \text{Benef}_{i})$	0.015	-0.020	-0.016	-0.123
	(0.052)	(0.037)	(0.068)	(0.129)
Number of obs.	590	590	590	456
Adjusted R-squared	0.189	0.092	0.408	0.040
Number of children	295	295	295	228
Panel B: Younger siblings				
$\text{After}_{i,r}$	-1.431***	-0.181*	2.302***	0.340
	(0.261)	(0.100)	(0.314)	(0.267)
$(\text{After}_{i,r} * \text{Benef}_{i})$	-0.084	-0.106***	0.267**	0.096
	(0.055)	(0.035)	(0.126)	(0.147)
Number of obs.	590	590	590	288
Adjusted R-squared	0.192	0.107	0.190	0.111
Number of children	295	295	295	144

Note: Sample is restricted to children from households located in districts from the two poorest quintiles. Each column in each panel corresponds to a different child fixed-effects regression. When the equation is estimated for the older siblings (Panel A), data from rounds 2 ($r = 0$) and 3 ($r = 1$) are used, whereas for the younger siblings (Panel B) data from rounds 3 ($r = 0$) and 4 ($r = 1$) are used. Robust standard errors clustered at the child level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table B.3

Robustness check – Children from poorest quintiles

Impact of the Juntos CCT (pooled sample)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{j,r}$	-1.018***	-0.082	1.460***	0.043
	(0.192)	(0.052)	(0.328)	(0.114)
$\text{After}_{k,r}$	-1.974***	-0.085	2.900***	-
	(0.386)	(0.107)	(0.640)	
$\text{After}_{j,r} * \text{Benef}_j (\beta_1)$	0.005	-0.025	0.004	-0.130
	(0.052)	(0.037)	(0.067)	(0.130)
$\text{After}_{k,r} * \text{Benef}_k (\beta_2)$	-0.096	-0.137**	0.302**	-0.008
	(0.074)	(0.056)	(0.137)	(0.191)
$(\beta_2 - \beta_1)$	-0.101	-0.112	0.298	0.122
$H_0: \beta_2 = \beta_1$ (p value)	0.071	0.001	0.019	0.399
Number of observations	1,180	1,180	1,180	744
Adjusted R-squared	0.170	0.099	0.249	0.060
Number of children	590	590	590	372

Note: Sample is restricted to children from households located in districts from the two poorest quintiles. Each column corresponds to a different child-fixed regression for the pooled sample (younger siblings and older siblings). Data from rounds 2 ($r = 0$) and 3 ($r = 1$) are used for the older siblings, whereas for the younger siblings data from rounds 3 ($r = 0$) and 4 ($r = 1$) are used. Robust standard errors clustered at the household level are in brackets.

*** p<0.01.

** p<0.05.

Note: Each column corresponds to a different child-fixed regression

Table B.4

Robustness check – Children from poorest quintiles

Additional impact of the Juntos CCT on the younger siblings

Dependent variable:	Change in Stunting	Change in Severe stunting	Change in Height-for-age	Change in PPVT score
	(1)	(2)	(3)	(4)
$\mathrm{YSib_i}$	-0.530*	-0.107	1.663**	0.239
	(0.273)	(0.185)	(0.702)	(0.912)
$(\mathrm{YSib_{i,}} * \mathrm{Benef_f})$	-0.043	-0.119**	0.196	0.400
	(0.096)	(0.048)	(0.192)	(0.296)
Number of obs.	590	590	590	372
Adjusted R-squared	0.306	0.155	0.452	0.567
Number of children	590	590	590	372

Note: Sample is restricted to children from households located in districts from the two poorest quintiles. Each column corresponds to a different household fixed-effects regression for the pooled sample (younger siblings and older siblings). Robust standard errors clustered at the household level are in brackets.

** p<0.05.

* p<0.1.

Table B.5

Robustness check – Change in baseline

Impact of the Juntos CCT on the younger siblings

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{i,r}$	-0.304	0.113	1.121**	0.744
	(0.217)	(0.139)	(0.521)	(0.460)
$(\text{After}_{i,r} * \text{Benef}_i)$	-0.092*	-0.169***	0.232**	0.251*
	(0.055)	(0.040)	(0.117)	(0.147)
Number of obs.	1,144	1,144	1,144	616
Adjusted R-squared	0.107	0.106	0.186	0.080
Number of children	572	572	572	308

Note: Sample corresponds to the younger siblings. Each column corresponds to a different child fixed-effects regression. The baseline observation (round 3, $r = 0$) has been replaced by the baseline observation of the older sibling at approximately the same age (round 2, $r = 0$). Robust standard errors clustered at the child level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table B.6

Robustness check – Change in baseline

Impact of the Juntos CCT (pooled sample)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{j,r}$	-0.029	0.055	0.434	0.027
	(0.148)	(0.072)	(0.357)	(0.076)
$\text{After}_{k,r}$	-0.000	0.205	0.907	
	(0.303)	(0.147)	(0.727)	
$\text{After}_{j,r} * \text{Benef}_j (\beta_1)$	-0.067	-0.054*	0.095	-0.011
	(0.043)	(0.032)	(0.059)	(0.115)
$\text{After}_{k,r} * \text{Benef}_k (\beta_2)$	-0.160**	-0.221***	0.326**	0.272
	(0.078)	(0.066)	(0.140)	(0.225)
$(\beta_2 - \beta_1)$	-0.093	-0.167	0.231	0.283
$\beta_2 = \beta_1$ (p value)	0.088	0.000	0.048	0.067
Number of observations	2,288	2,288	2,288	1,672
Adjusted R-squared	0.105	0.091	0.206	0.037
Number of children	1,144	1,144	1,144	836

Note: Each column corresponds to a different child fixed-effects regression for the pooled sample (younger siblings and older siblings). The baseline observation of the younger sibling (round 3, $r = 0$) has been replaced by the baseline observation of the older sibling at approximately the same age (round 2, $r = 0$). Robust standard errors clustered at the household level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table B.7

Robustness check – Controlling for duration of exposure

Impact of the Juntos CCT on the younger siblings

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{i,r}$	-0.817***	-0.107*	2.938***	0.136
	(0.184)	(0.059)	(0.377)	(0.090)
$(\text{After}_{i,r} * \text{Benef}_i)$	-0.132*	-0.085	-0.032	0.492**
	(0.077)	(0.081)	(0.164)	(0.230)
$\text{Initial\_Exposure}_{i,r}$	0.024	-0.017	0.122	-0.173
	(0.038)	(0.042)	(0.089)	(0.124)
Number of obs.	1,144	1,144	1,144	620
Adjusted R-squared	0.133	0.079	0.209	0.073
Number of children	572	572	572	310

Note: Sample corresponds to the younger siblings. Each column corresponds to a different child fixed-effects regression. The specification is similar to Equation (1), except that in this case a control is added ($\text{Initial\_Exposure}_{i,r}$) that takes the value of zero in the first observation, and the number of years of exposure in the second round in the second observation. Robust standard errors clustered at the child level are in brackets.

*** $p<0.01$.

** $p<0.05$.

* $p<0.1$.

Table B.8

Robustness check – Controlling for duration of exposure

Impact of the Juntos CCT (pooled sample)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{j,r}$	-0.411***	-0.028	1.417***	0.099
	(0.112)	(0.046)	(0.323)	(0.081)
$\text{After}_{k,r}$	-0.807***	0.008	2.945***	
	(0.229)	(0.095)	(0.648)	
$\text{After}_{j,r} * \text{Benef}_j (\beta_1)$	-0.070*	-0.060*	0.135**	0.001
	(0.043)	(0.031)	(0.059)	(0.115)
$\text{After}_{k,r} * \text{Benef}_k (\beta_2)$	-0.211**	-0.153*	0.142	0.608**
	(0.083)	(0.088)	(0.179)	(0.249)
$\text{Initial\_Exposure}_{k,r}$	0.029	-0.015	0.104	-0.212*
	(0.039)	(0.043)	(0.093)	(0.117)
$(\beta_2 - \beta_1)$	-0.141	-0.093	0.007	0.607
$H_0: \beta_2 = \beta_1$ (p value)	0.077	0.261	0.969	0.005
Number of observations	2,288	2,288	2,288	1,676
Adjusted R-squared	0.111	0.066	0.200	0.028
Number of children	1,144	1,144	1,144	838

Note: Each column corresponds to a different child fixed-effects regression for the pooled sample (younger siblings and older siblings). The specification is similar to Equation (2), except that in this case a control is added ($\text{Initial\_Exposure}_{k,r}$) that takes the value of zero in the first observation, and the number of years of exposure in the second round in the second observation. Robust standard errors clustered at the household level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table B.9

Falsification test of the impact of the Juntos CCT

Dependent variable:	Stunting	Severe stunting	Height-for-age
	(1)	(2)	(3)
$\text{After}_{i,r}$	0.082	0.074	-0.221
	(0.068)	(0.049)	(0.134)
$(\text{After}_{i,r} * \text{Benef}_i)$	-0.009	-0.030	0.075
	(0.047)	(0.039)	(0.085)
Number of obs.	1,138	1,138	1,138
Adjusted R-squared	0.058	0.027	0.128
Number of children	569	569	569

Note: Sample corresponds to the older siblings. Each column in each panel corresponds to a different child fixed-effects regression. Data from rounds 1 ($r = 0$) and 2 ($r = 1$) are used (during those rounds, implemented in 2002 and 2006, respectively, Juntos was not operating). Robust standard errors clustered at the child level are in brackets.

Table B.10

Impact of the Juntos CCT on the younger siblings

Initial exposure at ages 0-2 versus ages 3-4

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
Enrolled ages 0 and 2 $*$ Benef_{f} (β_{0-2})	-0.027	-0.180***	0.338	0.349
	(0.075)	(0.057)	(0.215)	(0.232)
Enrolled ages 3 and 4 $*$ Benef_{f} (β_{3-4})	-0.154***	-0.077**	0.148	0.073
	(0.052)	(0.035)	(0.092)	(0.124)
H_0: $\beta_{0-2} - \beta_{3-4}$=0 (p value)	0.134	0.130	0.380	0.273
Number of observations	1,144	1,144	1,144	620
Adjusted R-squared	0.106	0.097	0.127	0.079
Number of children	572	572	572	310

Note: Sample corresponds to the younger siblings. Each column corresponds to a different child fixed-effects regression. The specification is analogous to Equation (1), but distinguishing between initial exposure at ages 0 to 2 and initial exposure at ages 3 to 4. Robust standard errors clustered at the household level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

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Tables

Table 1

Descriptive statistics

	Older siblings		Younger siblings	
	Treated	Never treated	Treated	Never treated
	(A)	(B)	(A)	(B)
Main child characteristics:	Round 2 (2006)		Round 3 (2009)	
Age (in years)	4.7***	4.9	4.5**	4.2
Height-for-age	-2.39***	-1.27	-2.11***	-1.10
Stunting	0.63***	0.25	0.57***	0.22
Severe stunting	0.24***	0.05	0.17***	0.03
PPVT raw score	-0.55***	0.26	-0.43***	0.42
Years exposed to Juntos	0	-	1.9	-
	Round 3 (2009)		Round 4 (2013)	
Age (in years)	7.6	7.5	8.4**	8.1
Height-for-age	-1.91***	-0.93	-1.68***	-0.80
Stunting	0.44***	0.14	0.40***	0.11
Severe stunting	0.14***	0.02	0.01*	0.04
PPVT raw score	-0.49***	0.34	-0.09***	0.47
Years exposed to Juntos	1.9	-	5.9	-
Number of observations				
Anthropometric data	136	436	136	436
PPVT scores	75	453	52	258
Other characteristics:				
Mother’s schooling grades attained	3.1***	6.1	3.1***	6.4
School enrolment, Round 2	0.0%	0.0%	-	-
School enrolment, Round 3	98%	99%	-	-

Note: Treated children come from families that were enrolled into Juntos between 2007 and 2009. Mother`s schooling grades attained is defined in a range from 0 to 17: 1 to 11 for grades of basic education –where 6 is primary complete –;13 to 16 for imputed higher education –where 16 represents university education complete –, and 17 for a master degree. A t-test was performed to compare columns (A) and (B) separately for younger siblings and older siblings. The stars reported represent the p-value of the null hypothesis that both groups are equal.

*** p<0.01.

** p<0.05.

* p<0.1.

Table 2

Impact of the Juntos CCT (split by cohort)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
Panel A: Older siblings				
$\text{After}_{i,r}$	0.000	0.000	0.071	0.168
	(0.106)	(0.075)	(0.167)	(0.119)
$(\text{After}_{i,r} * \text{Benef}_i)$	-0.047	-0.052	0.055	-0.022
	(0.043)	(0.032)	(0.060)	(0.115)
Number of obs.	1,144	1,144	1,144	1,056
Adjusted R-squared	0.116	0.064	0.278	0.017
Number of children	572	572	572	528
Panel B: Younger sibling				
$\text{After}_{i,r}$	-0.818***	-0.106*	2.934***	0.131
	(0.184)	(0.058)	(0.377)	(0.090)
$(\text{After}_{i,r} * \text{Benef}_i)$	-0.089**	-0.116***	0.190*	0.207*
	(0.045)	(0.030)	(0.107)	(0.116)
Number of obs.	1,144	1,144	1,144	620
Adjusted R-squared	0.133	0.079	0.208	0.071
Number of children	572	572	572	310

Note: Each column in each panel corresponds to a different child fixed-effects regression. When the equation is estimated for the older siblings (Panel A), data from rounds 2 ($r = 0$) and 3 ($r = 1$) are used, whereas for the younger siblings (Panel B) data from rounds 3 ($r = 0$) and 4 ($r = 1$) are used. Robust standard errors clustered at the child level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table 3

Impact of the Juntos CCT (pooled sample)

Dependent variable:	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$\text{After}_{j,r}$	-0.409***	-0.029	1.424***	0.096
	(0.112)	(0.046)	(0.323)	(0.080)
$\text{After}_{k,r}$	-0.805***	0.007	2.954***	-
	(0.229)	(0.095)	(0.648)	
$\text{After}_{j,r} * \text{Benef}_f\ (\beta_1)$	-0.070*	-0.060*	0.134**	0.002
	(0.043)	(0.031)	(0.059)	(0.115)
$\text{After}_{k,r} * \text{Benef}_f\ (\beta_2)$	-0.159***	-0.179***	0.329**	0.261*
	(0.060)	(0.049)	(0.117)	(0.157)
$(\beta_2 - \beta_1)$	-0.089	-0.119	0.195	0.258
$H_0: \beta_2 = \beta_1$ (p-value)	0.051	0.000	0.070	0.023
Number of observations	2,288	2,288	2,288	1,676
Adjusted R-squared	0.111	0.066	0.199	0.027
Number of children	1,144	1,144	1,144	838

Note: Each column corresponds to a different child-fixed regression for the pooled sample (younger siblings and older siblings). Data from rounds 2 ($r = 0$) and 3 ($r = 1$) are used for the older siblings, whereas for the younger siblings data from rounds 3 ($r = 0$) and 4 ($r = 1$) are used. Robust standard errors clustered at the household level are in brackets.

*** p<0.01.

** p<0.05.

* p<0.1.

Table 4

Additional impact of the Juntos CCT on the younger siblings

Dependent variable:	Change in Stunting	Change in Severe stunting	Change in Height-for-age	Change in PPVT score
	(1)	(2)	(3)	(4)
$\mathrm{YSib_i}$	-0.342*	-0.060	0.920*	0.188
	(0.184)	(0.122)	(0.517)	(0.706)
$(\mathrm{YSib_{i,}} * \mathrm{Benef_f})$	-0.017	-0.095**	0.113	0.516**
	(0.081)	(0.048)	(0.170)	(0.241)
Number of obs.	1,144	1,144	1,144	838
Adjusted R-squared	0.324	0.102	0.544	0.537
Number of children	1,144	1,144	1,144	838

Note: Each column corresponds to a different household fixed-effects regression for the pooled sample (younger siblings and older siblings). Robust standard errors clustered at the household level are in brackets.

** p<0.05.

* p<0.1.

Figures

Figure 1

Roll-out of Juntos in the Young Lives households

Notes

[1] Baird et al. (2011) analyzed the impact of a CCT pilot program among adolescent girls in Malawi. After 2 years of exposure, they found improvements in math and reading comprehension scores (0.12 and 0.14 sd) for girls who were attending school at baseline. Similar improvements in math and reading comprehension (0.16 and 0.13 sd) were observed among girls who were school dropouts at baseline (Baird et al., 2013). Macours et al. (2012) found that *Atencion a Crisis* – a CCT pilot program that lasted 1 year, aimed at helping families deal with a climatic shock in Nicaragua – had a positive impact on vocabulary, language, and short-term memory scores (of 0.23, 0.14 and 0.16 sd, respectively) on children aged 0 to 5 years at the beginning of the program. Barham et al, (2013a) found that *Red de Protección Social* (also in Nicaragua) had an impact on math and vocabulary test scores (of 0.17 and 0.23 sd) detectable 7 years after the program ended on children aged 9-12 years at the beginning of the program.

[2] Mo et al. (2013) evaluated the impact of a CCT pilot program in rural China on math test scores of adolescents after 1 year of exposure. Barrera-Osorio and Filmer (2013) evaluated the impact of a CCT pilot program in Cambodia on math and short-term memory test scores after three years of operation, on children who were attending primary-school at the beginning of the program. Neither detected an impact on test scores.

[3] Baez and Camacho (2011) and Anderson et al. (2015) used non-experimental impact evaluation techniques to evaluate the impact of *Familias en Acción* in Colombia and *Juntos* in Peru, respectively. Baez and Camacho looked at impacts on math and language test scores after 9 years of exposure on individuals who were school-age at the beginning of the program. Anderson et al. looked at short-term impacts (1-2 years of exposure) on vocabulary test scores on children age 5-7 years at baseline. No impacts were detected.

[4] Paxson and Schady (2010) evaluated the impact of *Bono de Desarrollo Humano* in Ecuador on a number of cognitive outcomes (vocabulary, short-term memory, long-term memory and visual integration). Children were 0 to 5 years old at the beginning of the program, and were exposed for 1.5 years. No impact was detected on average, but an impact was detected on long-term memory (0.17 sd) for children in the bottom quartile.

[5] Against tuberculosis, diphtheria, whooping cough, tetanus, polio and measles.

[6] *Ministerio de Salud*, *Resolución Ministerial* 193-2008/MINSA.

[7] This replaced the 2006 standard, see *Ministerio de Salud*, *Resolución Ministerial* 292-2006/MINSA.

[8] The 2006 standard required 6 measurements from months 0 to 11, 5 measurements from months 12 to 24 (quarterly frequency), and 5 measurements from months 25 to 59 (bi-annual frequency).

[9] Conditionalities were updated over time. From 2013 onwards, pre-school attendance was declared compulsory, and school attendance became compulsory up to the age of 19 or when the child completes school. It is unlikely that the new conditionality related to pre-school attendance affected the Young Lives children as they were all above age 6 by 2013.

[10] The following set of household characteristics was considered: (a) percentage of illiterate women in the household, (b) percentage of children between ages 6 and 14 years attending school, (c) access to industrial sources of fuel (gas, oil, kerosene), (d) number of appliances, (e) access to public services (drinking water, electricity and sanitation), and (f) types of materials used in floors, walls and ceiling. Each of these characteristics was given an implicit weight and those households above a certain threshold were considered poor. Only households classified as poor under this criterion and located in eligible districts could enroll in the program.

[11] It is noteworthy that although this first period concluded in 2007, many households within the districts selected were only incorporated in 2008 or 2009. These households are also considered in our analysis.

[12] Specifically, 20 clusters were selected at random across the country and around 100 households with at least one child aged 6 to 18 months were selected in each cluster. For a more complete explanation of the sampling procedure, see Escobal and Flores (2008).

[13] A small proportion of the older siblings (less than 5%) were 6 years of age in Round 2. Similarly, a very small proportion of the younger siblings (less than 1%) were 1 year of age in Round 3.

[14] The Young Lives study collects additional outcomes for the older siblings. However, these are the only data available for paired-siblings in two points in time.

[15] The total (balanced panel) sample of Young Lives until round 4 is 1864 households.

[16] Households from the paired-siblings sample are larger than the rest of households. It is known that household size is positively associated with living in rural areas and with poverty status. Consistent with this, a higher proportion of the paired-siblings households were located in rural areas in round 1 (40% versus 25%) and were poorer on average in terms of access to services and average education. In addition, caregivers are younger in the paired-siblings households.

[17] The smaller sample size for PPVT is because some younger siblings were too young to be administered the PPVT in Spanish in round 3 (the test was only administered to children aged 4 years old onwards), and we only consider those children who took this test in Spanish in both rounds. All children were able to choose whether to take the test in Spanish, Quechua or Aymara. At the age of 7 to 9, virtually all children took the test in Spanish as this is the language used at school in Peru. However, at the age of 4 to 6, some children whose native tongue was

Quechua (Aymara) chose to take the test in Quechua (Aymara) whereas others chose to take it in Spanish. We restrict the sample to children who took this test in Spanish in both rounds to avoid comparisons of changes in PPVT scores among children who have taken the test in two different languages over time. Results might not be fully comparable in this case as some words in Spanish cannot be directly translated into Quechua or Aymara.