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Young Lives

The Impact of the *Juntos* Conditional Cash Transfer Programme in Peru on Nutritional and Cognitive Outcomes:

Does the Age of Exposure Matter?

Alan Sanchez, Guido Melendez and Jere Behrman

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About Young Lives

Young Lives is an international study of childhood poverty, following the lives of 12,000 children in 4 countries (Ethiopia, India, Peru and Vietnam) over 15 years. **www.younglives.org.uk**

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Summary

In this study we revisit the impact of conditional cash transfers (CCTs) on child development, with an emphasis on the role of the age of exposure. We use longitudinal data from a unique paired-siblings sample of Peruvian children (the Young Lives study) to evaluate whether Juntos, a large-scale CCT implemented in Peru since 2005, has a greater effect on children who benefited from the programme during the first three years of life compared with its impact on those children who benefited between the ages of 5 and 7. To deal with programme selection we apply child fixed-effects methods. We find that exposure to the programme leads to a reduction in severe stunting and an improvement in height-for-age – but only for those exposed during the first three years of life. This result suggests that timing of exposure matters. However, no cognitive impact (as measured by a vocabulary-development test) is detected for either group.

JEL Classifications: I12, J13, O15

1. Introduction

Children in developing countries are exposed to multiple risk factors, including undernutrition and poverty, which reduce their chances of achieving their developmental potential (Grantham-McGregor et al. 2007), with consequences in terms of schooling attainment and labour productivity (Glewwe et al. 2001; Alderman et al. 2006; Hoddinott et al. 2008, 2013; Maluccio et al. 2009). Evidence suggests that investing in human capital during early 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) programmes which transfer cash to poor families on the condition that pre-specified investments in health and education are made. Despite heterogeneity in the characteristics of the CCTs implemented across countries, evidence shows that this type of programme can be effective in improving nutritional status (Fiszbein and Schady 2009; Manley and Gitter 2013). The evidence for the cognitive impact of these programmes is much more limited. Macours et al. (2012) find that the *Atención a Crisis* programme in Nicaragua had an impact on cognitive development after two years of implementation, whereas Barham et al. (2015) find that *Red de Protección Social* (also in Nicaragua) had a cognitive impact detectable seven years after the programme ended.[1] These results are encouraging. Nevertheless, similar evidence has not been found in other emblematic cash transfer programmes such as *PROGRESA*/*Oportunidades* in Mexico (Behrman et al. 2008), even though the programme has an impact on educational attainment (Behrman et al. 2005; Schultz 2004).

The age of exposure may play a central role in the potential impact of CCTs on human capital. Mothers with pre-school-age children have to visit the health centre regularly (where procedures often include a growth-monitoring component), whereas school-age children must be enrolled at school and attend for most of the year. In addition, some programmes include a nutritional supplement for infants and an information component for mothers about best childcare and feeding practices. The existence of sensitive periods of investments in human capital, combined with the way in which CCTs are designed, implies that nutritional gains are likely to be greater for those exposed during early childhood. The same is the case for cognitive gains. While school attendance alone might lead to cognitive improvement, the effect can be reinforced if the child was exposed from early childhood, due to the nutrition–cognition nexus. However, the evidence is still scarce regarding the role of age of exposure, particularly the effect of having a first exposure to a CCT programme during early childhood versus first exposure when children are a few years older and entering school. One exception is Barham et al. (2015). They find that the impact of *Red de Proteccion Social* on cognitive outcomes is apparent only for those that benefited during the first 1,000 days (which includes the *in utero* period and the first two years of life), and not for those that benefited between the ages of 2 and 5 years. Similarly, Behrman and Hoddinott (2005) find that the provision of nutritional supplements by PROGRESA in Mexico had significant effects on height-for-age z scores for children aged 12–36 months, but not for children aged 36–48 months.

1 Paxson and Schady (2010) show similar evidence for an unconditional transfer programme, *Bono de Desarrollo Humano*, in Ecuador.

In this study we revisit the impact of CCTs on child development, paying special attention to the role played by the age of exposure. We focus on the *Juntos* programme. *Juntos* is a large-scale CCT implemented in Peru since 2005. 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). The study by Andersen et al. 2015 focuses on a cohort of children exposed to *Juntos* after the age of 5 years, whereas Sánchez and Jaramillo (2012) use data from the Demographic Health Survey (DHS) to examine the nutritional impact due to exposure during the early childhood period only. This paper uses new data to expand the evidence on the impact of *Juntos*. The main goal is to evaluate whether *Juntos* has a larger effect on children who benefited early in life. In particular, we test the impact of *Juntos* on a sub-sample of paired siblings from the Young Lives study in Peru, for which outcomes were collected at two points in time. The younger siblings were exposed to the programme during the first three years of life (onwards), whereas the older siblings were exposed between ages 5 and 7 (onwards). In order to control for selection into the programme, the analysis is restricted to households from poor districts located above a certain altitude threshold. Furthermore, we apply child fixed-effects methods to deal with pre-treatment differences across children that are constant over time.

As a result of our analysis we find that *Juntos* had a positive impact on nutrition only for those exposed during the first three years of life. This impact is limited to severe stunting, which falls by around 15 percentage points at age 7–9, due to early exposure. The evidence also suggests an improvement in height-for-age at age 7–9 for those exposed early, but this effect is imprecisely estimated. In addition, no cognitive impact at age 7–9 is found. This suggests that the nutritional gains observed did not translate into cognitive gains.

The paper is structured as follows. In Section 2 we describe the *Juntos* programme. In Section 3 we describe the Young Lives dataset and how it can be used to evaluate the importance of the age of exposure to *Juntos*. In Sections 4, 5, and 6, we present our empirical strategy, descriptive statistics, and main results. Section 7 concludes.

2. Description of the *Juntos* programme

Juntos targets poor families mainly in rural areas in Peru. The programme was established in 2005, initially serving 70 districts in the southern highlands. Its geographical coverage has increased gradually over time to include other areas of the highlands and the Amazonian jungle. To date, it has covered 834,000 families in 1,142 districts (out of 1,943 districts in the country). It is estimated that 72 per cent of all potential household beneficiaries are already covered by the programme (MIDIS 2015). Up to 2009, the programme made a monthly fixed transfer of 100 Nuevos Soles (approximate US$30, or around 10 per cent of poor households' monthly consumption). In 2010, this became a bi-monthly transfer of 200 Nuevos Soles.

In order to receive the transfer, the following conditionalities were defined (see PCM 2009):

(i) children aged 5 years or less must have their growth monitored, following the protocols of the Ministry of Health;

(ii) pregnant women must have ante-natal check-ups;

(iii) children aged between 6 and 14 years must be enrolled in school, and attendance is compulsory;

(iv) children must have a national ID.

In the case of (i) and (ii), although the characteristics and frequency of the check-ups are not directly specified, according to the Ministry of Health children who attend public-health facilities were meant to have their growth and development monitored (*Control de Crecimiento y Desarrollo*, CRED) as follows: twice during the first month; six times during the first 12 months; ten times between ages 1 and 4 years; and annually thereafter. According to the Ministry of Health, CRED check-ups include nutritional monitoring, physical evaluation, immunisation, Vitamin A and Iron (sulfato ferroso) supplementation, and advice for mothers, among other factors.[2]

The fulfilment of the conditionalities (attendance at health clinics for check-ups and attendance at school) is monitored bi-monthly by *Juntos* fieldworkers, who are able to access information from schools and health centres for this purpose. Conditionalities were slightly updated in late 2010 and in 2013. In 2010, attendance at pre-school was declared compulsory, whereas in 2013 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 the age of 6 by 2011.

The selection of *Juntos* beneficiaries followed a two-stage procedure: at the district level and at the household level. In 2005, participating districts were selected according to four criteria: (a) unsatisfied basic needs, (b) levels of child malnutrition, (c) poverty gaps, and (d) percentages of households affected by terrorism in Peru's internal conflict in the 1980s and 90s. These characteristics were equally weighted to create a selection ranking. Following

2 Ministerio de Salud, Resolución Ministerial 292-2006/MINSA.

these criteria, 70 districts were selected. In 2006, a fifth criterion was added: (e) presence of extreme income poverty (which implied that the weights for each component changed from 0.25 to 0.20). In that year, 251 districts were included. In 2007, the five criteria were retained, but the information used to calculate them was updated. During that year, 317 districts were introduced. In total, between 2005 and 2007 – what we call the 'first *Juntos* expansion' – 638 districts were selected. One result of the selection procedure used is that only districts located in the lowest two quintiles of the official poverty ranking (Poverty Map from 2005) were introduced into the programme during this period.

Within each selected district, a household-eligibility criterion was applied (second stage). 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;[3]

(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 type of material 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 were eligible for the programme.

A second expansion period began in 2010. Between 2010 and 2014, 509 new districts were gradually introduced. For the 'second *Juntos* expansion' some of the definitions mentioned above were modified, in part because poverty had fallen considerably in recent years, and also to introduce regions of the country that had not benefited during the first expansion. Still, the large majority of districts introduced were located in the lowest two quintiles of poverty.

In this study we deal with the first expansion period. It is noteworthy that although this first period concluded in 2007, many households within the districts selected were not incorporated until 2008 or 2009. These households are also considered in our analysis.

3 In 2010, *Juntos* extended the age range for eligibility to 19 years.

3. Data

Young Lives in Peru is tracking a cohort of about 2,000 children born between 2001 and 2002.[4] The baseline survey was administered in 2002. Three additional waves took place in 2006, 2009, and 2013. Data collected include anthropometry, cognitive test scores (Peabody Picture Vocabulary Test, mathematics test, and reading comprehension), and socio-emotional indicators. Although Young Lives was originally designed to track only one child per household (the index child), during the last two survey waves (2009 and 2013) additional information was collected for the next younger sibling of the index child – provided that 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 them anthropometry was collected and, for those aged at least 4 years at the time of the interview, the Peabody Picture Vocabulary Test (PPVT) was administered. For this analysis we focus on the paired-siblings sample (households with data on the index children and their younger siblings). This sample comes from 779 households (45 per cent of the total number of households).[5]

In order to test the impact of *Juntos* with observational data, it is important to choose a suitable counter-factual with care. Although we have information available for 779 paired-siblings households, many are located in urban areas and/or on the coast. However, the *Juntos* programme selects poor households located in rural districts. Typically, this type of household is located in the highlands, and to a lesser extent in the Amazonian jungle. For this reason, to ensure comparability of *Juntos* and non-*Juntos* families we further restrict the study sample. In particular, we focus on 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). Likewise, we only consider households in districts located 500 metres above sea level; due to the characteristics of the Young Lives districts, this means that only areas located either in the highlands or in the Amazonian jungle are included – and districts in the coast are excluded. After applying this filter, we identify 397 households that fulfil these criteria, among which 263 have benefited from *Juntos* at some point between 2005 and 2013, whereas 134 have not.

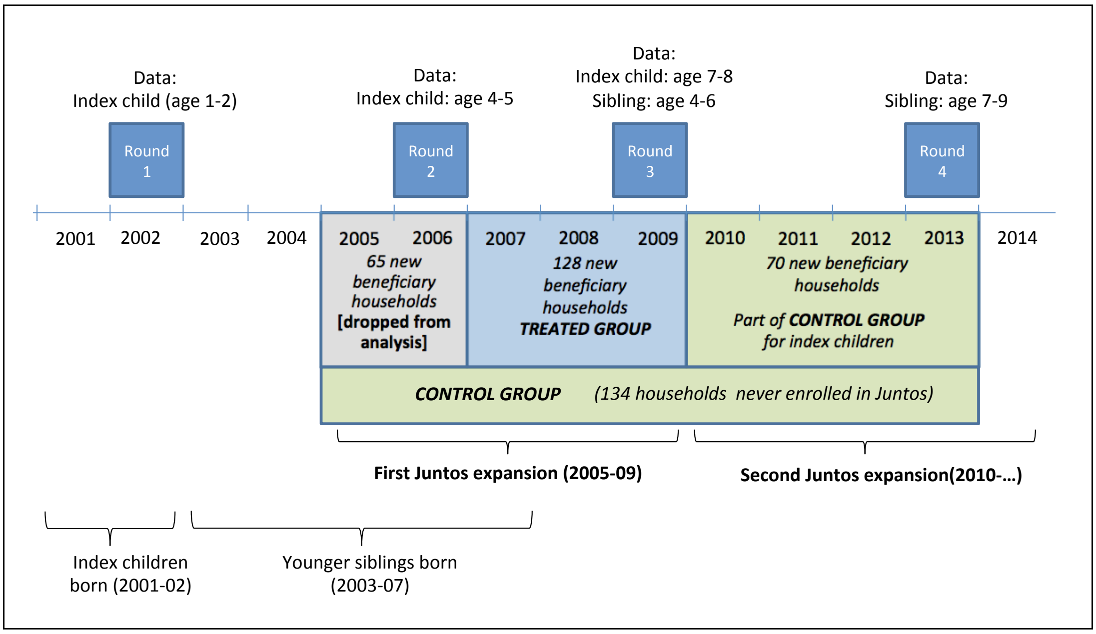
In order to test the importance of the age of exposure, we concentrate on the 'first *Juntos* expansion' (2005–2007), since in this case we can observe outcomes for all children at the same age. Due to the timing of the roll-out of *Juntos*, the first expansion affected the index children when most of them were older than 60 months, whereas the majority of the younger siblings were still below 36 months at the time of initial exposure.

Figure 1 illustrates the relationship between the data available, the roll-out of the programme, and the ages of the children in Rounds 1, 2, 3, and 4. Out of the 263 households with paired siblings who ever participated in *Juntos* over the period 2002–2013, 65 began receiving the transfer between 2005 and 2006, 128 between 2007 and 2009 (197 in total benefited from the 'first expansion'), and 70 between 2010 and 2013.

4 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).

5 The total (balanced panel) sample of Young Lives until Round 4 is 1,864 households.

Figure 1: *Roll-out of Juntos in the Young Lives households*



We evaluate the impact of the 'first *Juntos* expansion' by looking at outcomes at ages 7 to 9 years (Round 3 for the index children and Round 4 for the younger siblings). We consider the 128 households that benefited from the programme between 2007 and 2009 as the treated group. In order to use information from Round 2 as a baseline, we exclude from the analysis those households that benefited between 2005 and 2006. In addition, those treated between 2010 and 2013 become part of the control group for the index children.

As a result of the way the treated group is defined, all the index children were aged 5 to 7 years when first exposed to *Juntos*, whereas the younger siblings were aged 0 to 3 years at their first exposure.[6] After deleting observations with missing values for critical variables, the treated group is composed of 102 households, with 204 children observed in two time periods. The baseline information comes from Round 2 and Round 3 (for the index children and younger siblings, respectively). We note that the baseline information corresponding to the younger siblings is observed when *Juntos* was already operational. We discuss this aspect in Sections 4 and 5.

6 There were 10 households in which the younger siblings were first exposed after the third year of life. We exclude these households from the analysis.

4. Empirical strategy

The main purpose of the analysis is to evaluate the importance of the age of exposure on nutritional and cognitive outcomes. Outcomes are observed twice for the index children (Rounds 2 and 3) and the younger siblings (Rounds 3 and 4). We define 'initial exposure when older' as being exposed to the *Juntos* programme for the first time between ages 5 and 7 years, and 'initial exposure when younger' as being first exposed during the first three years of life.

Given the programme design, a simple comparison of treated and non-treated children would deliver biased estimates of the impact of the programme even after filtering the sample as proposed in the previous section. To estimate programme effects while dealing with programme selection, we propose the following regression specification:

$$Y_{ifs,r} = \alpha_0 + \alpha_1 D_r + \beta Beneficiary_{if,r} + X_{ifs,r} + \sigma_i + \sigma_f + \pi_s + \mu_{ifs,r} \quad (1)$$

where $Y_{ifs,r}$ is a generic outcome of child i from family f living in district s observed in Round r; D_r is a dummy which takes the value of 1 if the dependent variable is observed after *Juntos* was implemented and 0 otherwise (it is akin to a time-period dummy); $Beneficiary_{ifs,r}$ is a binary variable which takes the value of 1 if the child i received *Juntos* benefits at the time of the interview and 0 otherwise; vector $X_{ifs,r}$ accounts for observable child, family, and district characteristics that are fixed over time; σ_i, σ_f and π_s are child, family, and district unobserved characteristics that are fixed over time; and, $\mu_{is,r}$ is a random error term. Within this structure, the impact of exposure to *Juntos* is given by β. The estimation of equation (1) by OLS is likely to suffer from omitted-variable bias unless all household and district characteristics that lead to selection into *Juntos* are included in vector $X_{ifs,r}$. It is difficult to be fully convinced that all the relevant characteristics are included in this vector, particularly because we do not observe all the characteristics considered for selection at the household and district levels. Therefore, our strategy consists of obtaining child fixed-effects estimates of equation (1). In doing so, we are able to eliminate any bias due to district and household characteristics that lead to selection into *Juntos*. To obtain estimates of 'initial exposure when older' and 'initial exposure when younger', equation (1) is estimated separately for the index children and the younger siblings. When the equation is estimated for the index children, data from Rounds 2 and 3 are used, whereas data from Rounds 3 and 4 are used for the younger siblings.

Equation (1) is estimated for nutritional and cognitive outcomes. For nutritional outcomes we consider height-for-age Z-scores, stunting, and severe stunting. For cognitive outcomes we use child scores on the Peabody Picture Vocabulary Test.[7] Outcomes are observed at ages 4–5 and 7–8 for the index children ($r = 2$and $r = 3$, respectively) and at ages 4–6 and 7–9 for the younger siblings ($r = 3$and $r = 4$). Given the evidence of catch-up growth in the Peruvian children from the Young Lives study (Lundeen et al. 2013; Outes and Porter 2013; Singh et al. 2014), we expect α_1 to have a positive sign when the outcome is height-for-age, and a negative sign when the outcome is stunting or severe stunting, reflecting improvements in these outcomes as children age.

7 Young Lives collects other cognitive outcomes for the index children. However, this is the only cognitive outcome collected for paired siblings.

Since the objective is to obtain estimates of the impact of the 'first *Juntos* expansion', children from families benefited by this expansion are considered as the treated group. For the index children, the control group comes from two types of households (all of which were untreated at the time of the second measurement): children from non-treated families (either 'eligible' families in non-eligible districts; or non-eligible families living in districts selected by *Juntos*); and children from treated families that were affiliated to *Juntos* after Round 3, during the second expansion of the programme. For our analysis we decided to merge these two groups after statistical tests indicated that such merging was appropriate.[8] In the case of the younger siblings, outcomes are observed in Round 4 and, thus, only non-treated families can be considered as part of the control group.

For the index children, Round 3 (age 7–8) is used as the post-treatment-initiation period, whereas Round 2 (age 4–5) is used as the pre-treatment period. In turn, for the younger siblings, the post-treatment-initiation period is Round 4 (age 7–9), and data from Round 3 (age 4–6) are used to approximate the pre-treatment period. By Round 3 the treated younger siblings were already receiving *Juntos*. Although the lack of a pure baseline for the younger siblings is a limitation, controlling for outcomes from a previous age-period is important in order to control both for selection into the programme and for child-level heterogeneity. On the other hand, using Round 3 as baseline might lead to a bias in the estimation of the impact of *Juntos* for this cohort. Assuming that the impact of *Juntos* is positive (i.e., if the pre-treatment observation of the younger sibling was positively affected by the programme), this would reduce the difference between pre-treatment and post-treatment outcomes in the treated group, leading to a negative bias in the estimation of the programme effects. Since the sign of the expected bias is negative, this suggests that we can treat results obtained for the younger siblings as a lower bound of the true impact of *Juntos* on this cohort. Additional robustness checks are performed in Section 6.2.

4.1 Pooled OLS

The child fixed-effects model yields our preferred estimates. For comparison, we also present pooled OLS results in the Appendix. In this case, the estimation controls for child characteristics (age and gender), household characteristics (maternal education, maternal native tongue, and household´s wealth index in Round 1),[9] and the poverty level of the child's district of birth. All of these characteristics are either time-invariant or observed prior to the *Juntos* intervention. We can also use this model to formally test the importance of the age of exposure. In particular, having pooled the data from index children and younger siblings, we estimate the following model:

$$\begin{aligned} Y_{(i=j,k)fs,r} = {} & \alpha_0 + \alpha_1 D_{jf,3} + +\alpha_2 D_{kf,4} + \alpha_3 Beneficiary_{if,r} \\ & +\beta_1\left(D_{jf,3} * Beneficiary_{jf,r}\right) + \beta_2\left(D_{kf,4} * Beneficiary_{kf,r}\right) \\ & +X_{ifs,1}\Gamma + \mu_{ifs,r} \end{aligned} \quad (2)$$

8 We tested whether the impact of the programme for the index children was sensitive to the selection of the control group and rejected the possibility. Results are available upon request.

9 The wealth index is a composite measure of living standards. The variable is defined in the [0,1] interval such that a larger value reflects a wealthier household. The wealth index is calculated as the simple average of three sub-indexes also defined in the [0,1] interval: a housing-quality index (quality of floor, wall, roof, and number of rooms per capita), an access-to-services index (access to drinking water, electricity, sewage facilities, and type of fuel used for cooking), and a consumer-durables index (TV, radio, refridgerator, microwave oven, computer, etc.).

where subscripts j and k stand for the index child and the younger sibling of family f, respectively. In this case α_1 captures time trends between Rounds 2 and 3, whereas α_2 captures differences between Rounds 3 and 4. The coefficients β_1 and β_2 denote exposure 'after early childhood' and 'during early childhood', respectively. We test the null hypothesis that both coefficients are equal.

5. Descriptive statistics

We focus on the paired-siblings households. By definition, these households are larger in size. It is known that household size is positively associated with rural areas and with poverty status. Consistently with this, a higher proportion of the paired-siblings households were located in rural areas in Round 1 (40 per cent versus 25 per cent) and were poorer on average in terms of access to services and average education. Within this sub-group, we further filtered those households located in districts that are ranked in the lowest two quintiles of the Peru Poverty Map and that are at least 500 metres above sea level. In this final sub-group, average schooling attainment of the caregiver is five grades, and the proportions of children born in rural areas and whose parents' main occupation is agriculture in Round 1 (2002) are 66 per cent and 86 per cent (compared with 40 per cent and 81 per cent in the full paired-siblings sample). The high proportion of rural households engaged in agricultural activities is expected, given that this is the household profile targeted by the *Juntos* programme. These and other characteristics are reported in Table A.1 (Appendix).

Main characteristics of the children and families finally considered for the analysis are reported in Table 1. The anthropometric data are available for 490 children in total, whereas the cognitive-achievement data (PPVT scores) are available for 347 children. The reduction of sample size for PPVT is due to the fact that there were many younger siblings too young to be administered the PPVT in Round 3, and we consider only those children who took this test in Spanish in both rounds.[10, 11] Table 1 is split to distinguish between *Juntos* and non-*Juntos* children.

10 We restrict the sample in this way to avoid situations in which we compare 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.

11 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. 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 some chose to take it in Spanish.

Table 1. *Descriptive statistics*

	Index children		Younger siblings	
	Treated before 2009	Non-treated & treated after 2009	Treated before 2009	Non-treated
	(A)	(B)	(A)	(B)
Anthropometrics				
N	102	172	102	114
Round 2				
Age (in years)	4.7	4.6	NA	NA
Height-for-age	-2.44***	-1.98	NA	NA
Stunting	0.65*	0.53	NA	NA
Severe stunting	0.26***	0.12	NA	NA
Round 3				
Age (in years)	7.6	7.5	4.3	4.4
Height-for-age	-1.97***	-1.5	-2.14***	-1.54
Stunting	0.46***	0.28	0.57***	0.33
Severe stunting	0.15***	0.02	0.19***	0.04
Round 4				
Age (in years)	NU	NU	8.2	8.3
Height-for-age	NU	NU	-1.68***	-1.31
Stunting	NU	NU	0.42***	0.22
Severe stunting	NU	NU	0.03	0.02
Cognitive achievement				
N	71	177	34	65
PPVT raw score, Round 2	14.86	15.36	NA	NA
PPVT raw score, Round 3	47.51***	51.31	16.97***	27.35
PPVT raw score, Round 4	NU	NU	62.44***	70.44
Demographics & others				
Mother's education	3.1***	6.1	3.1***	6.4
School enrolment, Index Child, Round 2	0.0%	0.0%	-	-
School enrolment, Index Child, Round 2	98%	99%	-	-
% of HH members aged 6–12 enrolled at school, Round 3	98%	97%	98%	97%

Note: NA means 'does not apply' (data were not collected); NU means that data exist but were not used; a t-test was performed to compare columns (A) and (B). The aseterisks represent the p-value of the null hypothesis that both groups are equal, as follows: *** p<0.01, ** p<0.05, * p<0.1.

Four aspects of the data are worth highlighting. First, the younger siblings are overall better off than the index children: they demonstrate higher levels of height-for-age, lower levels of stunting and severe stunting, and higher scores in the PPVT than the index children at comparable ages. For anthropometric outcomes this is consistent with a trend observed in Peru during the previous decade (according to official statistics, stunting declined from 31 per cent to 13 per cent between 2000 and 2013), in turn associated with a reduction in poverty rates (from 59 per cent to 24 per cent between 2002 and 2014, according to official statistics).[12] Despite this, stunting rates among *Juntos* and non-*Juntos* children are still high.

Second, there is evidence of catch-up growth among treated and untreated children. This is consistent with evidence from other studies which used Young Lives data (Crookston et al. 2010 and 2013). Among the treated index children, stunting fell from 65 per cent (ages 4–5) to 46 per cent (ages 7–8), whereas among the treated younger siblings the reduction ranged from 57 per cent (ages 4–5) to 42 per cent (ages 7–9).

Third, even within this restricted sample, children from the treated group fare worse than children in the control group in the pre-treatment period in both nutritional and cognitive terms at comparable ages. This highlights the importance of controlling for selection into the programme. 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 that this effect was mediated by school enrolment.

6. Results

6.1 Main results

Table 2 reports results of the impact of the ‘first *Juntos* expansion’ separately for the index children and their younger siblings. This corresponds to equation (1). In terms of the impact of the programme on nutritional status, for those exposed between the ages of 0 to 3 no impact is detected on height-for-age and stunting. However, the programme has an effect on severe stunting. In particular, exposure to *Juntos* during the first three years of life reduces the probability of being severely stunted at age 7–9 by 13.4 percentage points. On the other hand, for those exposed between ages 5 and 7 we do not observe any effect on nutritional status measured at age 7–8.

Juntos might also have had an impact on cognitive achievement as measured by the Peabody vocabulary test. This is specially the case for those who benefited from the programme early in life, due the existence of links between early nutrition investments and subsequent cognitive development. However, we do not find evidence of effects on cognitive development for either of the two groups.

In Table A.2 in the Appendix we report results using pooled OLS instead of child fixed effects. In this case, selection into the programme is taken into account by controlling for household and district characteristics that are observed in the data and that are part of the programme-eligibility rule. Results remain very similar. To directly test for differences according to age of exposure, in Table A.3 all siblings are pooled, and the empirical specification corresponding to equation (2) is used. Results from the F-test show that the impact detected for severe stunting for those exposed earlier, compared with those exposed later, is statistically different. A difference in the impact of the programme on height-for-age in favour of those exposed early also emerges. The results also confirm that no other effect is found.

12 Data are from the Peru National Household Survey (ENAHO) and the Peru Demographic and Family Health Survey (ENDES), both surveys conducted by the National Institute of Statistics (INEI).

Table 2. *Impact of the 'first Juntos expansion'*

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
First exposed when older (index children)				
$Beneficiary_{i,r}$				
$D_{i,r}$	-0.211***	-0.071***	0.380***	1.915***
	(0.042)	(0.026)	(0.051)	
$(D_{i,r} * Beneficiary_{i,r})$	0.063	-0.028	0.017	-0.165
	(0.056)	(0.044)	(0.074)	(0.133)
Number of observations	548	548	548	496
R-squared	0.213	0.095	0.410	0.058
Number of children	274	274	274	248
First exposed when younger (younger sibling)				
$Beneficiary_{i,r}$				
$D_{i,r}$	-1.260	-0.206	2.457*	0.259
	(0.315)	(0.118)	(0.461)	(0.338)
$(D_{i,r} * Beneficiary_{i,r})$	-0.028	-0.134***	0.221	0.042**
	(0.067)	(0.042)	(0.147)	(0.191)
Number of observations	432	432	432	198.000
R-squared	0.190	0.144	0.207	0.173
Number of children	216	216	216	99

Notes: *** p<0.01, ** p<0.05, * p<0.1. Child fixed effects estimates.

6.2 Robustness checks

One of the aspects that is sensitive in our empirical strategy is the lack of a pure baseline for the younger siblings. To check the robustness of our previous results, in the Appendix we propose a number of robustness checks. First, we exploit the fact that paired siblings are observed to use the observation of the index child at age 4–5 (prior to *Juntos*) as a baseline for the sibling at age 4–6 (Table A.4). In other words, we used the observation from the older sibling at age 4–5 as a representation of what would have been the baseline for the younger siblings in the absence of the programme. Second, we estimate a model to predict the baseline for the sibling, using the observation of the index child at the same age, as well as controlling for other child and household characteristics (Table A.5). This strategy is explained in detail in Appendix B. Third, we estimate the pooled OLS model, including as a control the number of months in which the younger siblings were exposed to *Juntos* at age 4–5 (Table A.6). In all cases, we obtain very similar point estimates and reach the same conclusions.

7. Conclusions

Our findings suggest that the nutritional impact of *Juntos* is limited to those at the very bottom of the height distribution, as the impact on height-for-age is reflected in a reduction in severe stunting but not in stunting. Importantly, the effect is observed for those exposed between ages 0 and 3 and not for those exposed between ages 5 and 7. In terms of the related literature, a recent study which used Young Lives data for index children exposed between ages 5 and 7 (Andersen et al. 2015) detected an impact of *Juntos* on height-for-age (they did not explore the impact on severe stunting). Compared with Andersen et al., our results correspond to a more selective sample of the Young Lives children, with all children in the control group coming from very poor districts. The fact that exposure led to a reduction in severe stunting but not in stunting for those exposed during early childhood resembles findings from Sánchez and Jaramillo (2012), which used a larger sample from Peru DHS to test the impact of the programme during the first three years.

The lack of a cognitive impact of *Juntos* is not very surprising. In fact, few studies find cognitive impacts of conditional cash transfer programmes (Fiszbein and Schady 2009). There are at least three possible explanations for this result, and they probably complement each other. First, school enrolment at the primary-school level in Peru is close to universal (98 per cent of children aged 6 to 12 in the Young Lives study are attending school full-time). As a consequence of this, *Juntos* does not have 'margin' to have an impact on school attendance at this level of education, although it can probably play a more important role in the transition from primary to secondary school. Second, it might be that the nutritional impact of *Juntos* was not great enough to translate into cognitive gains. In addition to these alternatives, third, it is important to acknowledge that the sample for which cognitive outcomes are observed is small, and this reduces the power of our statistical tests.

A final aspect that should be considered in future studies of the Peruvian CCT is whether the conditionality related to pre-school attendance, which was introduced after the vast majority of Young Lives children were at least 6 years old, might have had an impact on educational achievement. Given that pre-school attendance is still relatively low in Peru, this is an area where *Juntos* could play a more significant role, compared with primary- school enrolment, which is almost universal. This could, perhaps, also create a vehicle through which the programme might have cognitive impacts on children.

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Appendix A. Descriptive statistics and main results

Table A.1. *Paired-siblings sample compared with rest of Young Lives households*

	Rest of YL Households (1)	Two-sibling Households (2)	Household sample (3)	(2)-(1)	(3)-(2)	(3)-(1)
Index children						
Height-for-age 1 to 2	-1.21	-1.38	-1.83	-0.17***	-0.45***	-0.62***
Height-for-age 4 to 5	-1.43	-1.67	-2.14	-0.24***	-0.47***	-0.71***
Height-for-age 7 to 8	-1.05	-1.29	-1.68	-0.25***	-0.38***	-0.63***
PPVT's raw score 4 to 5	32.48	26.02	16.63	-6.46***	-9.39***	-15.85***
PPVT's raw score 7 to 8	62.15	55.26	49.14	-6.89***	-6.11***	-13.00***
% of female	50.63	50.06	47.90	-0.57	-2.15	-2.72
Sibling						
Height-for-age 4 to 5		-1.47	-1.82		-0.35***	
Height-for-age 7 to 8		-1.09	-1.48		-0.39***	
PPVT's raw score 4 to 5		27.63	22.27		-5.36***	
PPVT's raw score 7 to 8		64.73	60.79		-3.94***	
% of female		46.03	48.29		2.25	
Household						
Housing-quality index in Round 1	0.44	0.37	0.26	-0.07***	-0.10***	-0.18***
Number of public services in Round 1	2.02	1.66	1.25	-0.36***	-0.40***	-0.77***
Number of missing assets in Round 1	3.99	4.36	4.72	0.37***	0.36***	0.73***
% of hh cooking with gas/ electricity R1	52.57	34.90	4.94	-17.67***	-29.95***	-47.62***
Wealth index in Round 1	0.47	0.37	0.25	-0.10***	-0.12***	-0.22***
Real monthly consumption per capita in Round 2	213	155	121	-58***	-34***	-92***
% of indigenous caregivers	23.62	38.27	47.91	14.65***	9.64***	24.29***
Caregiver's age in Round 1	28.33	25.49	26.29	-2.83***	0.80***	-2.04***
Caregiver's years of education	8.36	6.93	5.01	-1.43***	-1.92***	-3.35***
% of household head in agriculture in Round 1	76.48	80.51	85.93	4.03**	5.43***	8.48***
Household size in Round 1	5.80	5.60	5.58	-0.19*	-0.02	-0.22
% Rural in Round 1	24.69	40.31	65.78	-15.63***	25.47***	41.09***
Altitude in Round 1	1,493	1,909	2,525	416***	617***	1033***
% enrolled in *Juntos* 2005 to 2007	13.86	29.00	37.26	15.14***	8.26***	23.41***
% enrolled in *Juntos* 2010 to 2013	5.42	9.75	21.29	4.33***	11.55***	15.87***
District						
% of Poverty	54.67	62.46	75.75	7.79***	13.29***	21.07***
% of Severe poverty	6.84	8.87	12.07	2.03***	3.20***	5.23***
% of chronic malnutrition	21.82	28.17	39.35	6.34***	11.19***	17.53***
% of Unsatisfied basic needs	31.64	38.33	52.38	6.69***	14.04***	20.74***
% of Political violence (terrorism)	1.27	2.05	2.94	0.78***	0.88***	1.67***
Number of households	1,033	831	274			

Notes: *** p<0.01, ** p<0.05, * p<0.1.

Table A.2. *Impact of the 'first* Juntos *expansion' using pooled OLS*

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
First exposed when older (index children)				
$Beneficiary_{i,r}$	-0.006	0.124**	-0.233*	0.116
	(0.069)	(0.052)	(0.126)	(0.105)
$D_{i,r}$	-0.314***	-0.132***	0.591***	1.785***
	(0.055)	(0.045)	(0.083)	(0.137)
($D_{i,r}$ * $Beneficiary_{i,r}$)	0.065	-0.028	0.012	-0.157
	(0.057)	(0.044)	(0.075)	(0.134)
Number of observations	548	548	548	496
R-squared	0.130	0.092	0.217	0.208
Number of children	274	274	274	248
First exposed when younger (younger sibling)				
$Beneficiary_{i,r}$	0.187**	0.143***	-0.411***	-0.077
	(0.078)	(0.049)	(0.177)	(0.246)
$D_{i,r}$	-0.180	-0.001	0.301*	0.413**
	(0.117)	(0.033)	(0.224)	(0.307)
($D_{i,r}$ * $Beneficiary_{i,r}$)	-0.046	-0.137***	0.250	0.018
	(0.069)	(0.042)	(0.154)	(0.187)
Number of observations	432	432	432	198
R-squared	0.152	0.097	0.160	0.227
Number of children	216	216	216	99

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results control for child´s age and sex, maternal level of education and native tongue, wealth index in Round 1, and a district poverty index.

Table A.3. *Impact of the 'first* Juntos *expansion' using pooled OLS: first exposed when younger (younger siblings) versus first exposed when older (index children)*

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
$D_{i,3}$, *index* (α_1)	-0.184***	-0.041	0.298***	0.275***
	(0.047)	(0.033)	(0.087)	(0.088)
$D_{i,4}$, *sibling* (α_2)	-0.290***	-0.044	0.594***	0.658***
	(0.068)	(0.038)	(0.138)	(0.159)
$Beneficiary_{i,r}$, (α_3)	0.017	0.133**	-0.276**	0.099
	(0.066)	(0.053)	(0.126)	(0.103)
$D_{i,3}$, *index* * $Beneficiary_{i,r}$ (β_2)	0.077	-0.031	-0.006	-0.151
	(0.054)	(0.043)	(0.097)	(0.119)
$D_{i,4}$, *sibling* * $Beneficiary_{i,r}$ (β_2)	0.052	-0.162***	0.231	-0.158
	(0.071)	(0.052)	(0.146)	(0.166)
$\beta_2 = \beta_1$ (p value)	0.660	0.000	0.052	0.965
Number of observations	1,096	1,096	1,096	772
R-squared	0.131	0.084	0.171	0.233
Number of children	274	274	274	386

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results control for child´s age and sex, maternal level of education and native tongue, wealth index in Round 1, and a district poverty index.

Table A.4. *Impact of the 'first* Juntos *expansion' – using the index child as baseline for the younger sibling*

Dependent variable	Stunting		Severe stunting		Height-for-age		PPVT score	
	(1)	(1a)	(2)	(2a)	(3)	(3a)	(4)	(4a)
First exposed when older (index children)								
$Beneficiary_{i,r}$	-0.006		0.124**		-0.233*		0.116	
	(0.069)		(0.052)		(0.126)		(0.105)	
$D_{i,r}$	-0.314***	-0.211***	-0.132***	-0.071***	0.591***	0.380***	1.785***	1.915
	(0.055)	(0.042)	(0.045)	(0.026)	(0.083)	(0.051)	(0.137)	
$(D_{i,r} * Beneficiary_{i,r})$	0.065	0.063	-0.028	-0.028	0.012	0.017	-0.157	-0.165
	(0.057)	(0.056)	(0.044)	(0.044)	(0.075)	(0.074)	(0.134)	(0.133)
Number of observations	548	548	548	548	548	548	496	496
R-squared	0.130	0.213	0.092	0.095	0.217	0.410	0.208	0.058
Number of children	274	274	274	274	274	274	248	248
First exposed when younger (younger sibling)								
$Beneficiary_{i,r}$	0.092**		0.146***		-0.378***		-0.058	
	(0.081)		(0.057)		(0.146)		(0.166)	
$D_{i,r}$	-0.416	-0.310***	-0.138	-0.043*	0.695*	1.690***	0.461**	1.889
	(0.126)	(0.384)	(0.071)	(0.250)	(0.254)	(0.640)	(0.207)	(0.539)
$(D_{i,r} * Beneficiary_{i,r})$	0.026	0.020	-0.157***	-0.157***	0.213	0.202	-0.018	-0.048
	(0.080)	(0.080)	(0.054)	(0.053)	(0.154)	(0.152)	(0.216)	(0.219)
Number of observations	432	432	432	432	432	432	194	194
R-squared	0.167	0.188	0.121	0.178	0.238	0.299	0.266	0.284
Number of children	216	216	216	216	216	216	97	97

Notes: *** p<0.01, ** p<0.05, * p<0.1. Pooled OLS results are presented in columns (1), (2), (3), and (4). Child fixed effects results are presented in columns (1a), (2a), (3a), and (4a). For the younger sibling, the baseline observation is replaced by the observation of the index child at a comparable age.

Table A.5. *Impact of the 'first* Juntos *expansion' – using a predicted baseline for the younger sibling*

Dependent variable	Stunting		Severe stunting		Height-for-age		PPVT score	
	(1)	(1a)	(2)	(2a)	(3)	(3a)	(4)	(4a)
First exposed when older (index children)								
$Beneficiary_{i,r}$	-0.006		0.124**		-0.233*		0.116	
	(0.069)		(0.052)		(0.126)		(0.105)	
$D_{i,r}$	-0.314***	-0.211***	-0.132***	-0.071***	0.591***	0.380***	1.785***	1.915
	(0.055)	(0.042)	(0.045)	(0.026)	(0.083)	(0.051)	(0.137)	
$(D_{i,r} * Beneficiary_{i,r})$	0.065	0.063	-0.028	-0.028	0.012	0.017	-0.157	-0.165
	(0.057)	(0.056)	(0.044)	(0.044)	(0.075)	(0.074)	(0.134)	(0.133)
Number of observations	548	548	548	548	548	548	496	496
R-squared	0.130	0.213	0.092	0.095	0.217	0.410	0.208	0.058
Number of children	274	274	274	274	274	274	248	248
First exposed when younger (younger sibling)								
$Beneficiary_{i,r}$	0.179**		0.143***		-0.369***		0.004	
	(0.078)		(0.049)		(0.135)		(0.093)	
$D_{i,r}$	-0.184	-1.261***	-0.001	-0.206*	0.412*	2.718***	0.345**	0.667
	(0.117)	(0.316)	(0.033)	(0.118)	(0.213)	(0.384)	(0.141)	(0.445)
$(D_{i,r} * Beneficiary_{i,r})$	-0.038	-0.020	-0.137***	-0.134***	0.179	0.132	-0.039	-0.011
	(0.070)	(0.067)	(0.042)	(0.042)	(0.125)	(0.120)	(0.189)	(0.198)
Number of observations	432	432	432	432	432	432	198	198
R-squared	0.147	0.184	0.097	0.144	0.188	0.263	0.329	0.141
Number of children	216	216	216	216	216	216	99	99

Notes: *** p<0.01, ** p<0.05, * p<0.1. Pooled OLS results are presented in columns (1), (2), (3), and (4). Child fixed effects results are presented in columns (1a), (2a), (3a), and (4a). For the younger sibling, the baseline is predicted using the procedure explained in Appendix B.

Table A.6. *Impact of the 'first* Juntos *expansion' using pooled OLS – controlling for duration of exposure of the younger sibling at the baseline*

Dependent variable	Stunting	Severe stunting	Height-for-age	PPVT score
	(1)	(2)	(3)	(4)
First exposed when older (index children)				
$Beneficiary_{i,r}$	-0.006	0.124**	-0.233*	0.116
	(0.069)	(0.052)	(0.126)	(0.105)
$D_{i,r}$	-0.314***	-0.132***	0.591***	1.785***
	(0.055)	(0.045)	(0.083)	(0.137)
$(D_{i,r} * Beneficiary_{i,r})$	0.065	-0.028	0.012	-0.157
	(0.057)	(0.044)	(0.075)	(0.134)
Number of observations	548	548	548	496
R-squared	0.130	0.092	0.217	0.208
Number of children	274	274	274	248
First exposed when younger (younger sibling)				
$Beneficiary_{i,r}$	0.282**	0.122***	-0.727***	0.177
	(0.164)	(0.093)	(0.391)	(0.422)
$D_{i,r}$	-0.177	-0.000	0.293*	0.102**
	(0.118)	(0.033)	(0.229)	(0.260)
Months of exposure to Juntos prior to $baseline_{i,r}$	(-0.004)	(0.001)	(0.014)	(-0.007)
	(0.007)	(0.003)	(0.015)	(0.016)
$(D_{i,r} * Beneficiary_{i,r})$	-0.048	-0.140***	0.252	-0.016
	(0.070)	(0.043)	(0.156)	(0.189)
Number of observations	428	428	428	194
R-squared	0.156	0.100	0.168	0.230
Number of children	214	214	214	97

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results control for child´s age and sex, maternal level of education and native tongue, wealth index in Round 1, and a district poverty index

Appendix B

In this section we explain the procedure followed to predict a baseline for the younger siblings. The following model is considered for the prediction of height-for-age:

$$Y_{kfs,3} = \gamma_0 + \gamma_1 Y_{jfs,2} + \gamma_2 D_{(k-j)} + \gamma_3 Juntos\text{–}Months_{kfs,3} + X_{kfs,1}\Gamma + \gamma_4 D(Y_{kfs,3} < -2) + \gamma_5 D(Y_{kfs,3} < -3) + \varepsilon_{kfs,3} \quad \text{(i)}$$

where $Y_{kfs,3}$ is the outcome of the younger sibling at age 4–6 (Round 3); $Y_{jfs,2}$ is the outcome of the index child at age 4–5 (Round 2); $D_{(k-j)}$ stands for the difference in the age in years between the younger sibling in Round 3 and the index child in Round 2; $Juntos\_Months_{kfs,3}$ stands for the number of months during which the younger sibling was exposed to *Juntos* as of Round 3; vector $X_{kfs,1}$ controls for child´s sex, age in months, maternal schooling and native tongue, the wealth index of the household, and a poverty index of the district of birth; $\varepsilon_{kfs,3}$ is the error term. Finally, $D(Y_{kfs,3} < -2)$ and $D(Y_{kfs,3} < -3)$ are dummies which activate when the height-for-age Z-score of the younger siblings at age 4–6 was below -2 and -3, respectively. We include these dummy variables to better model the lower end of the height-for-age distribution For PPVT we estimate an analogous equation, including a dummy which activates if the child’s PPVT score is in the lowest quintile of the PPVT distribution. We estimate equation (i), recover the coefficients, and use them to obtain a prediction for the siblings, $\hat{Y}_{kfs,r}$. For the prediction $Juntos\_Months_{kfs,3}$ is assumed to be zero. This strategy provides a reliable prediction of the outcome of the younger sibling in the absence of *Juntos*, capturing all the household characteristics that are common across siblings.

A final important observation is that, when we follow this strategy, in order to be consistent the baseline is predicted for both treated and non-treated younger siblings. The results obtained by this strategy are reported in Table A.5 in Appendix A.

The Impact of the *Juntos* Conditional Cash Transfer Programme in Peru on Nutritional and Cognitive Outcomes: Does the Age of Exposure Matter?

Juntos is a large-scale conditional cash transfer (CCT) programme implemented in Peru since 2005. This programme transfers the equivalent of about US$30 monthly (around 10 per cent of monthly consumption) to poor families located, mainly, in the rural highlands, on condition that all children under the age of 5 are subject to growth monitoring controls, all children and adolescents attend school, and all pregnant women attend regular check-ups, among other aspects (conditions have evolved over time). We use data from the Young Lives study in Peru to analyse the impact of this programme on nutritional and cognitive outcomes between the ages of 7 and 9, paying special attention to the role played by the age of first exposure.

The main findings are as follows:

- Exposure to *Juntos* leads to a reduction in severe stunting and an improvement in height-for-age, but only for those exposed during the first three years of life.
- This suggests that the age of exposure to the programme matters.
- While no measurable impact on stunting is detected, a reduction of severe stunting is observed.
- No impact on cognitive development (as measured by a vocabulary-development test) is detected, either for those children exposed during the first three years of life or for those exposed later.

Peru has made substantial progress in reducing child stunting. The Peruvian CCT is likely to have contributed to this trend, but it is interesting to observe that to a large extent it helped mainly those children who were worse off in nutritional terms. Our results also show that the age of the child when first exposed to the programme is crucial. While early exposure to a CCT might lead to cognitive gains, due to the combined effects of attending school and being better nourished, we are not able to detect such effects in the Peruvian case. Future innovations in the *Juntos* programme should take account of these results when ways of maximising its potential are considered.

Young Lives

An International Study of Childhood Poverty

About Young Lives

Young Lives is an international study of childhood poverty, involving 12,000 children in 4 countries over 15 years. It is led by a team in the Department of International Development at the University of Oxford in association with research and policy partners in the 4 study countries: Ethiopia, India, Peru and Vietnam.

Through researching different aspects of children's lives, we seek to improve policies and programmes for children.

Young Lives Partners

Young Lives is coordinated by a small team based at the University of Oxford, led by Professor Jo Boyden.

- *Ethiopian Development Research Institute, Ethiopia*
- *Pankhurst Development Research and Consulting plc, Ethiopia*
- *Centre for Economic and Social Studies, Hyderabad, India*
- *Save the Children India*
- *Sri Padmavathi Mahila Visvavidyalayam (Women's University), Andhra Pradesh, India*
- *Grupo de Análisis para el Desarollo (GRADE), Peru*
- *Instituto de Investigación Nutricional, Peru*
- *Centre for Analysis and Forecasting, Vietnamese Academy of Social Sciences, Vietnam*
- *General Statistics Office, Vietnam*
- *Oxford Department of International Development, University of Oxford, UK*

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