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Participation in the *Juntos* Conditional Cash Transfer Program in Peru Is Associated with Changes in Child Anthropometric Status but Not Language Development or School Achievement[1–4]

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Abstract

Background: It is unclear what effects a conditional cash transfer (CCT) program would have on child anthropometry, language development, or school achievement in the context of the nutrition transition experienced by many low- and middle-income countries.

Objective: We estimated the association of participation in Peru's *Juntos* CCT with anthropometry, language development, and school achievement among children aged 7–8 y.

Methods: We used data from the Young Lives Study of a cohort born between 2001 and 2002. We estimated associations of the *Juntos* program with height-for-age *z* score (HAZ), body mass index–for–age *z* score (BAZ), stunting, and overweight at age 7–8 y separately for children participating in the program for ≥2 y (n = 169) and children participating for <2 y (n = 188). We then estimated associations with receptive vocabulary and grade achievement among children who had been assessed at age 4–6 y before enrollment in *Juntos* (n = 243). We identified control subjects using propensity score matching and conducted difference-in-differences comparisons.

Results: *Juntos* participation was associated with increases in HAZ among boys participating for ≥2 y [average effect of treatment among the treated (ATT): 0.43; 95% CI: 0.09, 0.77; P = 0.01] and for boys participating for <2 y (ATT: 0.52; 95% CI: 0.23, 0.80; P < 0.01). Among girls participating in the program for ≥2 y, BAZ declined (ATT: –0.60; 95% CI: –1.00, –0.21; P < 0.01) as did the prevalence of overweight (ATT: –22.0 percentage points; 95% CI: –42.5, –2.7 percentage points; P = 0.03). We observed no significant associations of *Juntos* participation with receptive vocabulary or grade attainment.

Conclusions: CCT program participation in Peru was associated with better linear growth among boys and decreased BAZ among girls, highlighting that a large-scale poverty-alleviation intervention may influence anthropometric outcomes in the context of the nutrition transition. *J Nutr* 2015;145:2396–405.

Keywords: *Juntos*, conditional cash transfer, height-for-age, stunting, body mass index-for-age, overweight, receptive vocabulary, children, cohort study, Peru

Introduction

Although the prevalence of stunting among children <5 y old in Peru has decreased from 28.5% in 2007 to 17.5% in 2013, growth faltering continues to be an important challenge among the poor. In 2011, for example, 43% of children in the lowest wealth quintile and >50% of children whose mothers had no formal

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[4] Supplemental Tables 1–3 are available from the ''Online Supporting Material'' link in the online posting of this article and from the same link in the online table of contents at http://jn.nutrition.org.

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schooling were stunted (1). Undernutrition contributes to greater susceptibility to infection and mortality in childhood and to chronic disease in adulthood (2–4). There are also negative consequences of undernutrition for cognitive development (5, 6), which can have long-term implications for future earnings (7, 8).

For Peruvian children in the lowest wealth quintile under the age of 5 y, the prevalence of overweight (weight-for-height *z* score >2) was ~4% in 2011 (1). By the age of 5–9 years, 8.9% of Peruvian children in the poorest quintile were overweight (BMI-for-age *z* score >1) (9). Overweight and obesity during childhood are associated with increased risk of poor concurrent health outcomes (10), in addition to increased risk of adult metabolic disorders, ischemic heart disease, and mortality (11, 12).

Conditional cash transfer (CCT)[17] programs target low-income populations for cash income supplements that are disbursed if recipients meet specified conditions. The purpose of CCT programs is to simultaneously fight current poverty through income supplementation and promote long-term human capital development through the conditions attached to the cash transfers. CCTs have the potential to influence anthropometric, cognitive, and educational outcomes among children, although the estimated effects on these outcomes differ greatly across programs and countries (13, 14). The associations of CCT program participation with child height-for-age and weight-for-age have been previously studied, and findings were mixed (15). Program effects on height-for-age have been inconsistent, and a recent meta-analysis found that overall there is no statistically significant effect (14). Few studies have found statistically significant associations between CCTs and weight-for-age (16). New evidence from individual programs can yield insights into where cash transfer programs can be successful and what programmatic, economic, or cultural characteristics of those programs or populations may be contributing to a program's successes or failures.

To our knowledge, the link between CCT programs and cognitive outcomes has only been investigated in Mexico (17, 18), Nicaragua (19–21), and Ecuador (22–24); and the results have been heterogeneous. One explanation for this heterogeneity may be that there are important differences between the programs and contexts of Mexico, Nicaragua, and Ecuador. Results from Peru may differ from existing findings due to the many ways that the countries and CCT programs vary. For example, CCTs in Latin America vary considerably by transfer magnitude (15). In Mexico, cash transfers comprised 20–30% of household income (17), with lower transfer values in Nicaragua (15%) and Ecuador (10%) (19, 22). In Peru, the transfers are equivalent to only 15% of household spending (25). Ecuador stands apart from the other countries in that the conditions for its program were not enforced, making it more similar to an unconditional cash transfer program (24). Another point of differentiation is the gross national income per capita of these countries. For example, in 2009, Nicaragua's per capita income was 76.2% less than in Peru, whereas Mexico's was 113.3% greater than in Peru. Ecuador's income was more similar along this indicator (5.5% less than in Peru) (26). Given these differences, exploring the associations of CCT programs with cognitive outcomes in additional countries with different contexts is important.

Peru's CCT program, *Juntos*, began in 2005; by 2012, it reached ~810,000 households in 1143 districts, covering ~10% of households. There have been 2 previous evaluations of the impact of *Juntos*, which reached different conclusions. The first article used a regression approach to compare children <5 y old in recipient and nonrecipient households in terms of height-for-age *z* score (HAZ) and weight-for-age *z* score data from 2006 to 2007 and found no associations of *Juntos* participation with malnutrition or anemia (25). The second article used difference-in-differences propensity score matching and found a reduction in the prevalence of severe stunting (HAZ < −3) and an increase of 0.13 in HAZ among *Juntos* participants (27). Their approach was to use posttreatment cross-sectional data from the 2008 and 2010 rounds of the Demographic Health Survey. Although they lacked a proper baseline, the authors make the argument that due to the rollout of the program, only a small proportion of children had been affected by *Juntos* by 2008 during the first 2 y of life, whereas by 2010 most of them had been affected.

Although both of these studies controlled for potential confounding by including district and household characteristics in their analyses, they lack outcome data on individual participants before their enrollment in *Juntos*. The present study adds to previous work by presenting the first analysis that includes pre-enrollment data on anthropometry, cognitive ability, and covariates, which permit control for preintervention differences between *Juntos* participants and nonparticipants. This analysis also adds to the literature by investigating height-for-age and weight-for-age up to the age of 8 y, whereas the previous analyses only examined children <5 y of age. Investigating the effects of a CCT program at a later age is important given recent evidence in Peru of increases in HAZ among children after 2 y of age (28–30). Finally, to our knowledge, this is the first investigation of associations of *Juntos* participation with child language or cognitive achievement outcomes.

Methods

The Juntos Program. Rollout of *Juntos* began with 110 districts in 2005 and expanded to 1143 districts by 2014. The implementation of *Juntos* has been described previously in detail (31). As an overview, *Juntos* eligibility is based on a 3-stage selection process: selection of eligible districts, selection of eligible households within eligible districts, and a community validation process. Although the exact criteria used to establish district eligibility were modified throughout the rollout of *Juntos*, the district eligibility criteria generally included indicators of poverty and unmet basic needs, child undernutrition, and exposure to violence due to guerrilla activity. These variables were then used to create a district poverty index. Districts were ranked according to this index and, with some exceptions, the 638 poorest districts of the country were enrolled between 2005 and 2007. Household eligibility within districts was determined on the basis of poverty as calculated by using a proxy-means formula. During the period of time covered by the data used in this analysis, eligible households had children <14 y old or a pregnant woman. Finally, community members, local authorities, and Ministry of Education and Health representatives conducted a validation process to reduce inclusion and exclusion errors. During the validation process, the community representatives went one-by-one through the households that met the first 2 eligibility criteria to exclude those that were ineligible for other reasons (e.g., because the household owned cattle) (31).

The *Juntos* conditionalities during the study period varied according to the age and eligibility of the participant. Members of households with children <5 y of age or with a pregnant or lactating woman were required to attend regular health care visits. Children aged 6–14 y who had not completed primary school were required to attend school 85% of the days. Beneficiary households received transfers of 100 *soles* (~30 US dollars) each month regardless of household composition, representing ~15% of beneficiary household spending (25). No impact evaluation was planned as part of *Juntos* (32).

[17] Abbreviations used: ATT, average effect of treatment among the treated; BAZ, BMI-for-age *z* score; CCT, conditional cash transfer; HAZ, height-for-age *z* score; pp, percentage point(s); TVIP, *Test de Vocabulario en Imagenes Peabody.*

Data source. This analysis uses a subset of data from the Young Lives Study, which aims to characterize the causes and consequences of childhood poverty and inform the development of future policies aimed at improving child welfare. Two cohorts of children in 4 countries [Ethiopia, India (Andhra Pradesh and Telangana), Peru, and Vietnam] are being followed for over 15 y. In each country, a cohort of ~2000 children aged between 6 and 18 mo and a cohort of ~1000 children aged 7 and 8 y were recruited in 2002. The Young Lives Study is coordinated by the University of Oxford's Department of International Development in association with research and policy partners in the study countries (33).

The Peruvian sample was recruited from 20 sampling sites selected to reflect diversity in region, ethnicity, and religion. The wealthiest 5% of districts were excluded in an effort to oversample poor sites. Within the study sites, children within the eligible age category were randomly sampled for participation (34). The present analysis uses data from the younger cohort. Although the Young Lives Study was not specifically designed to evaluate the *Juntos* program, the study does collect data on *Juntos* participation. The original sample recruited in 2002 consisted of 2052 children (round 1). Follow-up data were collected in 2006 when the children were 4–6 y old (round 2) and in 2009 when children were 7–8 y old (round 3).

Our analysis uses data from round 1 (before *Juntos* enrollment) and round 3 (post–*Juntos* enrollment) to measure associations of *Juntos* with anthropometric outcomes and data from round 2 (preintervention for households not enrolled in *Juntos* at round 2) and round 3 (post-intervention) to measure associations of *Juntos* with language development outcomes. We were not able to use round 1 data for language development because children were too young to be assessed at that time.

Figure 1 shows the criteria for inclusion in the propensity score estimation model used to match observations for the analysis of anthropometric outcomes. In our sample, 98% of children whose families received *Juntos* benefits lived in the mountainous region of Peru, so the analysis was restricted to those observations with full *Juntos* participation data living in this region (n = 960). For the analysis of anthropometric outcomes, children with full covariate and anthropometric data from all 3 rounds were retained (n = 914; 95.2%). The sample used to estimate the propensity score for language development and school achievement outcomes is shown in **Figure 2** (n = 755; 78.6%). The treated population consisted of children who had round 2 receptive vocabulary assessments completed before enrollment in *Juntos*, as well as full covariate data, so as to provide an untreated baseline. Controls for the language development and school achievement analysis sample required complete outcome and covariate data.

Anthropometric outcomes. Anthropometric variables included in the analysis are as follows: HAZ, BMI-for-age z scores (BAZ), stunting (HAZ < −2), and overweight (BAZ >1), computed according to the WHO growth references (35). The same cutoff points defined above for stunting and overweight were used in both rounds 1 and 3 to maintain consistent definitions. Changes in anthropometric outcomes were obtained by subtracting round 1 scores from round 3 scores.

In the Young Lives sample in general, children who were younger at round 1 tended to have higher HAZ values and children who were older at round 1 tended to have lower HAZ values (28). There was no association between child age and HAZ at round 3. Failure to correct for this age pattern at round 1 might have biased matching results. Consequently, all round 1 HAZ measurements were adjusted to their predicted value at age 12 mo by adding the difference of the child's HAZ from the mean HAZ of children within the same 2-mo age interval to the mean HAZ for children aged 11 to 13 mo. The predicted HAZ at age 12 mo was then used as the baseline measurement for further analyses of HAZ and stunting, a technique that has been used previously (28, 30).

Language development and school achievement outcomes. Language development was measured by using the Spanish or Quechua version of the Peabody Picture Vocabulary Test [*Test de Vocabulario en Imagenes Peabody* (TVIP)]. Raw scores of the TVIP in rounds 2 and 3 were used. TVIP scores were internally standardized to the control group. Data were stratified into 6-mo age categories. A standardized score was obtained by subtracting the mean score for the controls in the child's age category from the child's score and dividing by the SD of the control group in that category. School achievement was assessed by using the highest grade achieved by 2008.

Juntos exposure definitions. Household respondents were asked to report their month and year of *Juntos* initiation, as well as the month and year of discontinuation (if applicable). We consider a child exposed to *Juntos* if his or her household ever received a *Juntos* transfer and if data were available on the duration of *Juntos* transfers.

For the analyses of anthropometric outcomes, *Juntos* treatment was characterized as ≥2 y of participation or <2 y of participation. The treatment sample was divided by duration because we hypothesized that longer treatment would permit a longer period for nutritional supplementation and subsequent anthropometric effects to develop. In addition, because Young Lives sampled children of a narrow age range, Young Lives children who have been enrolled in *Juntos* longer were enrolled at a younger age. Finally, the *Juntos* program was rolled out first to the poorest districts, so children who have been enrolled longer were also poorer (31). There is also evidence that cash transfer programs tend to have greater benefits for children enrolled at younger ages or for those from poorer households (14).

For language development and school achievement outcomes, we considered participation in *Juntos* beginning after the round 2 assessments to allow for inclusion of an untreated baseline. In all cases, comparisons were made with the group who did not participate in *Juntos*.

Statistical analysis. Estimates were obtained by using difference-in-differences propensity score matching. The difference-in-differences technique estimates program impacts by taking the difference between the change in outcomes for children participating in *Juntos* and the change in outcomes for children not participating in *Juntos*. The key assumption of a difference-in-differences estimator is that the mean change in outcomes for both groups would have been the same in the absence of the intervention. To validate the plausibility of this assumption, we assessed whether the pretreatment trends were the same between the

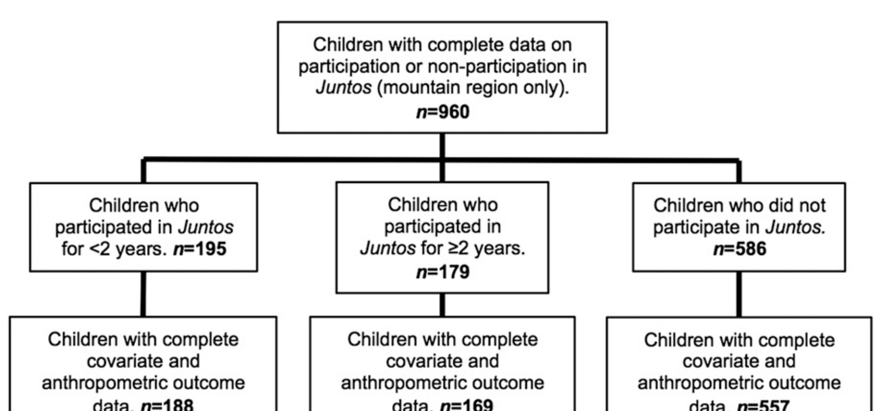

FIGURE 1 Inclusion criteria and sample size for analysis of the association between the *Juntos* conditional cash transfer program and anthropometric outcomes among children in Peru.

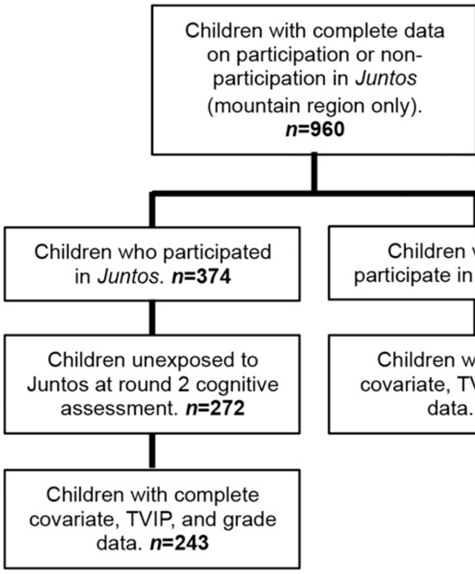

FIGURE 2 Inclusion criteria and sample size for analysis of the association between the *Juntos* conditional cash transfer program and language development among children in Peru. TVIP, *Test de Vocabulario en Imagenes Peabody.*

treatment and control groups. We did this by comparing the change in HAZ and BAZ between round 1 and round 2 among children who were enrolled in *Juntos* by round 3 but not at round 2 and children who never received *Juntos* and found no significant differences ($P > 0.1$).

Propensity score matching controls for confounding by matching observations on the basis of their predicted probability of exposure to the treatment of interest by using a set of characteristics assumed not to be affected by the treatment. This method is especially useful in situations in which few unexposed units of observation are comparable to the exposed units across all covariates, and when the units of observation can be compared across a high number of preprogram covariates (36). Matching on the propensity score reduces the overall imbalance in baseline covariates between the treatment and comparison groups.

The probability of exposure to the *Juntos* program was predicted by using a probit model based on the following round 1 characteristics: child sex, household wealth, number of household members, rural or urban household location, percentages of household members who were <6 y old and 6–14 y old, indigenous language as a first language for the child's primary caregiver, and mother's height. Caregiver completion of primary school was assessed by using round 2 data due to data-quality concerns for this variable in round 1. In predicting the propensity score for the language development and school achievement analysis, the set of covariates above was not sufficient to balance potential confounders after matching, so interaction and polynomial terms were used to achieve balance. Household wealth was measured by using an index of housing quality, consumer durables, and household services (37). Household size was grouped into 4 categories (2–4, 5, 6–7, and ≥8 members) to account for a skewed distribution. The models estimating the probability of treatment predicted by the covariates achieved a pseudo-R^2 between 0.27 and 0.55.

Each *Juntos* recipient was matched to the untreated observation with the propensity score closest to his or her own. When a single untreated observation was the closest match to multiple *Juntos* recipients, matching with replacement was performed. Observations were only matched if they were on common support (the interval of mutual overlap for the propensity score distributions of the treatment and control groups). All treatment observations were on common support, so there is no concern with selection bias due to dropping treatment observations. Student's *t* test and Pearson's chi-square test were used to compare differences between treatment groups before and after matching (38). Treatment effects were estimated by using difference-in-differences comparisons where the mean change in each outcome for the matched comparison group was subtracted from the mean change in outcome for the treatment group. SEs were calculated as described by Abadie and Imbens (39) and account for the fact that propensity scores were estimated and not observed.

Estimates were also obtained by matching within strata of sex. Stratification by sex was performed on the basis of previous research that effects of cash transfer programs on anthropometric outcomes may differ by sex (14).

The statistical analysis was conducted by using Stata version 13 (40). Values reported in the text are presented as means ± SDs and ranges or average treatment effect among the treated (ATT; 95% CI) and *P* values. Two-sided *P* values <0.05 were considered significant, but *P* values <0.1 are also reported in the text.

Ethics. The Young Lives protocol was approved by the Ethics Committee of Oxford University and the Instituto de Investigación Nutricional Ethics Committee in Lima. Written informed consent was obtained from all household heads or guardians of the children surveyed. Additional ethical approval for this analysis was not required.

Results

Sample characteristics. In both rounds 1 and 3, mean HAZ was lower among *Juntos* recipients than among nonrecipients (**Supplemental Table 1**). Mean HAZ increased between rounds 1 and 3. BAZ scores were lower among *Juntos* beneficiaries in both rounds than among nonrecipients and declined across rounds. We note that the prevalence of overweight reported at round 1 is higher than the prevalence of overweight for young children in Peru cited in the Introduction, which is a consequence of using a different definition for overweight in this analysis to maintain consistency across both rounds. When overweight is calculated by using a cutoff of BAZ >2 at round 1, then the prevalence of overweight in the analyzed sample overall is 11.3%, which is more similar to the values cited in the Introduction.

There were significant differences between *Juntos* recipients and nonrecipients for nearly all covariates at round 1, all of which indicated an increased level of vulnerability and poverty among *Juntos* participants. For example, *Juntos* participants were more likely than nonparticipants to live in rural areas, have a lower wealth index, have a caretaker who spoke an indigenous language, and have a caretaker who did not complete primary education. Children living in families who received *Juntos* transfers for ≥2 y and children who had received *Juntos* transfers for <2 y were similar with respect to rural status, wealth, and caretaker language and education. Children who participated in *Juntos* for ≥2 y ($n = 169$) enrolled in the program at a mean age of 56.5 mo (SD = 10.1 mo; range: 29.3–75.1 mo), whereas children who participated in *Juntos* for <2 y ($n = 188$) enrolled at a mean age of 77.1 mo (SD = 8.1 mo; range: 37.4–96.8 mo).

In the sample analyzed for associations with language development and school achievement ($n = 243$), *Juntos* participants scored lower than nonparticipants on the TVIP in round 2 and completed fewer grades of schooling (**Supplemental Table 2**). Similar patterns in covariates were observed. Children participating in *Juntos* included in the assessment of language development and school achievement outcomes were enrolled at a mean age of 74.1 mo (SD = 8.6 mo; range: 52.1–96.8 mo).

Across all models used to estimate the propensity score, only 4 variables were significantly predictive of *Juntos* participation at the $P < 0.1$ level of significance, namely the following: household wealth, caregiver speaking an indigenous first language, rural residence, and caregiver completion of primary school (**Supplemental Table 3**). These 4 variables were used to assess covariate balance after matching. The covariate balance within the matched data sets is reported in **Table 1**. After matching, all covariates targeted for balancing were not associated with *Juntos* participation ($P > 0.1$).

Program effects on anthropometric outcomes. Table 1 shows outcomes pre- and posttreatment for each matched sample. Balance for potentially confounding variables identified through probit models predicting *Juntos* participation is also reported. Although potentially confounding covariates could be balanced with the propensity score match, balance could not be achieved for baseline outcomes in all analyzed samples. The number of unique control observations selected to be matched, as well as the total number of control observations selected when accounting for matching with replacement, are reported.

The difference-in-differences estimates are presented in **Table 2**. In the sample as a whole, *Juntos* participation was not associated with changes in HAZ for any treatment duration. When the sample is split by sex, however, boys who received *Juntos* transfers for ≥2 y showed improvements in HAZ (ATT: 0.43; 95% CI: 0.09, 0.77; $P = 0.01$), as did boys receiving *Juntos* for <2 y (ATT: 0.52; 95% CI: 0.23, 0.80; $P < 0.01$). There was a nonsignificant tendency for a reduction in stunting when treatment occurred for 2 y or more in the sample overall [ATT: −18.3 percentage points (pp); 95% CI: −38.3, 1.6 pp; $P = 0.07$], as well as for girls (ATT: −19.0 pp; 95% CI: −38.5, 0.4 pp; $P = 0.06$). Boys participating in *Juntos* for <2 y also showed a nonsignificant tendency for reductions in stunting (ATT: −18.2 pp; 95% CI: −38.3, 2.00 pp; $P = 0.08$).

For children participating in *Juntos* for ≥2 y, BAZ decreased nonsignificantly in the whole sample (ATT: −0.36; 95% CI: −0.79, 0.06; $P = 0.09$) but significantly for girls (ATT: −0.60; 95% CI: −1.00, −0.21; $P < 0.01$). This decrease in BAZ corresponded to a significant reduction in the prevalence of overweight in girls (ATT: −22.0 pp; 95% CI: −42.5, −2.7 pp; $P = 0.03$).

Program effects on language development and school achievement. Among children who began enrollment in the *Juntos* program after the round 2 interview, there were no significant associations of *Juntos* participation with receptive vocabulary or grade achieved by 2008 (Table 2). A similar null result was observed for effects on grade in 2008 when the sample of all children who participated in the *Juntos* program, regardless of enrollment time, were used (estimates not shown).

Discussion

Our analyses suggest that *Juntos* participation was associated with increases in HAZ among boys but not among girls, whereas BAZ and prevalence of overweight declined only among girls who participated in *Juntos* for ≥2 y. There was no association of *Juntos* program participation with child receptive vocabulary scores or grade attainment. These findings are likely to be of interest to researchers and policy makers who aim to reduce the dual burden of undernutrition and overnutrition in developing countries, while also promoting language development and school achievement.

Our study demonstrates heterogeneity of associations within the same program, showing positive associations of *Juntos* participation with HAZ for boys and decreases in BAZ and the prevalence of overweight for girls enrolled in *Juntos* for ≥2 y. A previous review of cash transfer programs and height-for-age indicated that CCT programs in general have heterogeneous effects on HAZ according to type of program, targeted recipient, environmental characteristics, economic conditions, policy regimes, and political contexts (14). In the meta-analysis, associations of cash transfers with nutrition were seen to be stronger for girls and for poorer children. One possible explanation for why the association between *Juntos* and anthropometric measurements differed for boys and girls in our study was that the 2 groups had different baseline anthropometric measurements. Boys had a lower mean baseline HAZ than girls and therefore had more of a deficit from which they could catch up. It may also be the case that having a higher mean baseline BAZ for girls contributed to the reductions in BAZ observed for girls but not for boys.

Another potential explanation for why increases in HAZ were observed for boys in *Juntos* but not for girls is that boys in Peru might be more responsive to nutritional insults and improvements at the age the intervention took place, even when holding baseline outcomes constant. Previous studies of growth among children in Peru suggest that there may be differential growth patterns between boys and girls. For example, 1 study found that although rural girls caught up with urban girls in height during childhood, rural boys did not catch up with urban boys during the same time frame (41). Another study including Peruvian children <2 y of age found that boys showed greater growth in the absence of diarrheal infection than did girls (42). These studies indicate that insults may have a greater effect on growth among boys, and therefore an improvement in conditions would facilitate even greater improvements among boys. It is important to acknowledge that although associations of *Juntos* with HAZ were not observed for girls, there was a nonsignificant trend for *Juntos* to be associated with a reduction in stunting among girls participating for ≥2 y. Thus, there is some suggestion that *Juntos* might also have improved growth for girls.

The HAZ distributions in the treatment groups are centered very close to the cutoff for stunting (HAZ < −2). This distribution may in part explain the large associations we observed for stunting, because even small increases in the distribution of HAZ would move a large portion of the population across the stunting threshold. A similar argument could be made for the substantial association of *Juntos* with reduced overweight among girls participating for ≥2 y, given that the distributions of BAZ are very close to the cutoff for overweight. The association seen with HAZ for boys is larger than those observed for other cash transfer interventions, although a cash transfer program in India observed a large program effect of +0.33 HAZ in the sample overall (14). However, nutrition education programs have also seen consistently large effects in younger children. A review of nutrition education for food-secure children <2 y of age found an HAZ increase of 0.35, whereas complementary food provision interventions with or without education among food-insecure children found a mean increase in HAZ of 0.39 (43).

We found that *Juntos* participation for ≥2 y is associated with a reduction in the prevalence of overweight and in BAZ among girls. This is an encouraging finding, because it suggests that cash transfers might be an effective strategy to combat increasing levels of overweight among some children in some developing countries. The effects observed for girls are large, but unfortunately little research for comparison is available on the impact of large-scale interventions to reduce overweight in developing countries (44). Evidence from Brazil has shown that each additional month of time enrolled in the *Bolsa Alimentação* cash transfer program was associated with a 31-g reduction in weight gain (45). A study in Mexico found that a doubling of the *Oportunidades* cash transfer amount was associated with a nearly 3-pp reduction in BMI-for-age, and an 8-pp reduction in overweight among children (18).

We did not find effects of *Juntos* on language development or school achievement, which may be due to aspects of our study design that limit our ability to see these associations. Notably, we included only those individuals who were not enrolled in *Juntos* at round 2 in order to provide an untreated baseline. This sample overlaps largely

TABLE 1 Outcomes and confounding variables after matching for Peruvian children participating and not participating in the *Juntos* cash transfer program[1]

	Full sample			Girls			Boys		
	Juntos nonparticipants	*Juntos* participants	P^2	*Juntos* nonparticipants	*Juntos* participants	P^2	*Juntos* nonparticipants	*Juntos* participants	P^2
Juntos participation for ≥2 y									
n	169	169		84	84		85	85	
n_u	78	—		32	—		37	—	
Outcome variables									
HAZ									
Round 1	−2.08 ± 1.12	−2.11 ± 1.24	0.76	−2.40 ± 1.17	−1.99 ± 1.21	0.03	−1.67 ± 0.742	−2.24 ± 1.26	<0.01
Round 3	−1.95 ± 0.813	−1.85 ± 0.829	0.25	−2.07 ± 0.768	−1.85 ± 0.789	0.07	−1.71 ± 0.548	−1.85 ± 0.871	0.21
BAZ									
Round 1	0.622 ± 1.30	0.613 ± 1.23	0.94	0.624 ± 0.795	0.769 ± 1.17	0.35	0.678 ± 0.914	0.459 ± 1.28	0.20
Round 3	0.622 ± 0.773	0.248 ± 0.788	<0.01	0.737 ± 0.525	0.277 ± 0.682	<0.01	0.471 ± 0.674	0.219 ± 0.884	0.04
Stunting									
Round 1	84 (49.7)	101 (59.8)	0.06	48 (57.1)	47 (56)	0.88	25 (29.4)	54 (63.5)	<0.01
Round 3	81 (47.9)	67 (39.6)	0.12	50 (59.5)	33 (39.3)	<0.01	17 (20)	34 (40)	<0.01
Overweight									
Round 1	64 (37.9)	65 (38.5)	0.91	23 (27.4)	38 (45.2)	0.02	44 (51.8)	27 (31.8)	<0.01
Round 3	42 (24.9)	28 (16.6)	0.06	15 (17.9)	11 (13.1)	0.39	8 (9.4)	17 (20)	0.05
Confounding variables									
Wealth index	0.24 ± 0.09	0.23 ± 0.10	0.58	0.23 ± 0.085	0.23 ± 0.10	0.66	0.25 ± 0.077	0.23 ± 0.09	0.13
Caregiver speaks indigenous first language	149 (88.2)	152 (89.9)	0.60	81 (96.4)	82 (97.6)	0.65	70 (82.4)	70 (82.4)	1.0
Caregiver completed primary education	30 (17.8)	36 (21.3)	0.41	12 (14.3)	16 (19)	0.41	18 (21.2)	20 (23.5)	0.71
Rural	153 (90.5)	150 (88.8)	0.59	75 (89.3)	74 (88.1)	0.81	77 (90.6)	76 (89.4)	0.80
Juntos participation for <2 y									
n	188	188		100	100		88	88	
n_u	104	—		57	—		51	—	
Outcome variables									
HAZ									
Round 1	−1.80 ± 1.02	−1.97 ± 1.10	0.13	−1.81 ± 0.936	−1.71 ± 1.05	0.50	−1.76 ± 1.25	−2.26 ± 1.09	<0.01
Round 3	−1.71 ± 0.757	−1.76 ± 0.864	0.56	−1.80 ± 0.871	−1.78 ± 0.870	0.83	−1.76 ± 0.774	−1.75 ± 0.861	0.89
BAZ									
Round 1	0.790 ± 0.986	0.527 ± 1.15	0.02	0.769 ± 1.20	0.450 ± 1.22	0.06	0.550 ± 1.13	0.616 ± 1.06	0.69
Round 3	0.436 ± 0.739	0.145 ± 0.833	<0.01	0.281 ± 0.802	−0.0119 ± 0.839	0.01	0.523 ± 0.944	0.323 ± 0.795	0.13
Stunting									
Round 1	80 (42.6)	91 (48.4)	0.25	40 (40)	36 (36.0)	0.56	34 (38.6)	55 (62.5)	<0.01
Round 3	76 (40.4)	72 (38.3)	0.67	36 (36)	36 (36.0)	1.0	31 (35.2)	36 (40.9)	0.44
Overweight									
Round 1	81 (43.1)	65 (34.6)	0.09	43 (43)	33 (33.0)	0.15	33 (37.5)	32 (36.4)	0.88
Round 3	34 (18.1)	24 (12.8)	0.15	15 (15)	11 (11.0)	0.40	20 (22.7)	13 (14.8)	0.18

(Continued)

TABLE 1 *Continued*

	Full sample			Girls			Boys		
	Juntos nonparticipants	*Juntos* participants	P^2	*Juntos* nonparticipants	*Juntos* participants	P^2	*Juntos* nonparticipants	*Juntos* participants	P^2
Confounding variables									
Wealth index	0.27 ± 0.1	0.28 ± 0.10	0.33	0.25 ± 0.10	0.27 ± 0.10	0.18	0.31 ± 0.10	0.29 ± 0.10	0.13
Caregiver speaks indigenous first language	128 (68.1)	130 (69.1)	0.82	63 (63)	66 (66.0)	0.66	67 (76.1)	64 (72.7)	0.60
Caregiver completed primary education	78 (41.5)	73 (38.8)	0.60	39 (39)	40 (40.0)	0.88	28 (31.8)	33 (37.5)	0.43
Rural	138 (73.4)	145 (77.1)	0.40	79 (79)	81 (81.0)	0.72	66 (75.0)	64 (72.7)	0.73
Any *Juntos* participation after round 2 language score assessment									
n	243	243		117	117		126	126	
n_u	113	—		59	—		51	—	
Outcome variables									
TVIP									
Round 2	−0.531 ± 0.761	−0.538 ± 0.782	0.92	−0.511 ± 0.862	−0.618 ± 0.766	0.31	−0.259 ± 0.798	−0.463 ± 0.793	0.04
Round 3	−0.552 ± 1.03	−0.718 ± 0.959	0.07	−0.474 ± 0.972	−0.819 ± 0.971	<0.01	−0.385 ± 1.05	−0.625 ± 0.942	0.06
Grade									
Round 3	1.29 ± 0.529	1.24 ± 0.517	0.61	1.20 ± 0.545	1.21 ± 0.506	0.67	1.31 ± 0.497	1.27 ± 0.528	0.29
Confounding variables									
Wealth index	0.26 ± 0.10	0.27 ± 0.10	0.40	0.26 ± 0.10	0.27 ± 0.094	0.40	0.26 ± 0.088	0.27 ± 0.10	0.34
Caregiver speaks indigenous first language	171 (70.4)	180 (74.1)	0.36	80 (68.4)	84 (71.8)	0.57	89 (70.6)	96 (76.2)	0.32
Caregiver completed primary education	89 (36.6)	81 (33.3)	0.45	52 (44.4)	43 (36.8)	0.23	42 (33.3)	38 (30.2)	0.59
Rural	199 (81.9)	195 (80.2)	0.64	101 (86.3)	95 (81.2)	0.29	94 (74.6)	100 (79.4)	0.37

[1] Values are means ± SDs or *n* (%). BAZ, BMI-for-age *z* score; HAZ, height-for-age *z* score; *n*, total number of control observations after matching with replacement; n_u, number of unique control observations matched to treatment observations; TVIP, *Test de Vocabulario en Imagenes Peabody*.

[2] *P* values for Student's *t* test or Pearson's chi-square test between *Juntos* nonrecipients and *Juntos* recipients.

TABLE 2 Difference-in-differences treatment effect estimates by sex and length of *Juntos* cash transfer program participation among Peruvian children[1]

Outcome	Full sample[2] ATT	Full sample[2] P	Girls ATT	Girls P	Boys ATT	Boys P
Juntos participation for ≥2 y						
n	169		84		85	
HAZ	0.14 (−0.20, 0.49)	0.41	−0.19 (−0.79, 0.41)	0.54	0.43 (0.09, 0.77)	0.01
BAZ	−0.36 (−0.79, 0.06)	0.09	−0.60 (−1.0, −0.21)	<0.01	−0.034 (−0.56, 0.49)	0.90
Stunting,[3] pp	−18.3 (−38.3, 1.59)	0.07	−19.0 (−38.5, 0.410)	0.06	−14.1 (−55.6, 27.4)	0.50
Overweight,[3] pp	−8.89 (−24.7, 7.00)	0.27	−22.6 (−42.5, −2.74)	0.03	30.6 (−11.5, 72.6)	0.15
Juntos participation for <2 y						
n	188		100		88	
HAZ	0.12 (−0.10, 0.33)	0.28	−0.069 (−0.33, 0.19)	0.60	0.52 (0.23, 0.80)	<0.01
BAZ	−0.028 (−0.31, 0.25)	0.84	0.026 (−0.38, 0.44)	0.90	−0.27 (−0.72, 0.19)	0.25
Stunting,[3] pp	−7.98 (−22.3, 6.34)	0.27	4.00 (−15.0, 23.0)	0.68	−18.2 (−38.3, 1.98)	0.08
Overweight,[3] pp	3.19 (−9.93, 16.3)	0.63	6.00 (−10.9, 22.9)	0.49	−6.82 (−28.1, 14.5)	0.53
Any *Juntos* participation after round 2 language development score assessment						
n	243		117		126	
TVIP	−0.15 (−0.37, 0.066)	0.17	−0.22 (−0.52, 0.071)	0.14	−0.025 (−0.52, 0.47)	0.92
Highest grade achieved in 2008	−0.045 (−0.17, 0.083)	0.49	0.017 (−0.15, 0.19)	0.84	−0.040 (−0.19, 0.11)	0.61

[1] Values are effect estimates (95% CIs). Coefficients represent the change in the difference in outcomes between the *Juntos* participants and matched controls. ATT, average effect of treatment among the treated; BAZ, BMI-for-age *z* score; HAZ, height-for-age *z* score; *n*, number of *Juntos* participants; an equal number of matched controls were selected (with replacement); pp, percentage points; TVIP, *Test de Vocabulario en Imagenes Peabody*.

[2] The effects for the full sample and the weighted average of the effects for girls and boys are not the same because the propensity score match was conducted separately for each sample.

[3] Effects reported are changes in pp.

with the sample of children who participated in *Juntos* for <2 y. The only significant association of *Juntos* participation with anthropometry witnessed in this group was with HAZ among boys. We speculate that there may be greater potential for treatment effects on language development or school achievement among children who receive a longer duration of treatment or who started the program earlier. More research is needed to identify the associations between CCTs and cognitive and language outcomes for children who have been exposed to these interventions for ≥2 y and beginning before entry into school.

Some, but not all, previous studies of CCTs in Latin America have shown positive associations with cognitive or language development or grade attainment (e.g., 13, 18, 19, 22, 23). Previous research found that primary school attendance in Peru is high, and that *Juntos* participation is not associated with improved school attendance overall, although there were improvements in early enrollment (25). The lack of improved attendance may explain the null effect on receptive vocabulary. Furthermore, evidence from the Young Lives cohort suggests that patterns of growth recovery and faltering throughout childhood are associated with performance on tests of cognition (30, 46). Our finding that gains in language development were not observed for boys, despite the fact that *Juntos* was associated with growth in boys, indicates that the test we used to assess language development may not have been sensitive enough to detect effects, given that we would expect improved nutrition and child development to be associated (47). Given the relatively short duration of exposure to *Juntos* among those in the sample used for analysis of language development and school achievement, another explanation is that gains in growth among boys did not have enough time to translate to receptive vocabulary outcomes.

A strength of our study is that it controls for unobserved fixed effects, such as innate growth and cognitive potential, and matches on observed characteristics in a cohort study with limited attrition. Thus, conditional on the assumption that time-varying unobservable confounders are balanced, our study permits the interpretation that associations between *Juntos* and child anthropometric and language outcomes reflect program effects. Another strength of this study compared with previous analyses of *Juntos* is that we included an untreated baseline with which to compare the differences in outcomes between treatment and control groups. In addition, the use of propensity score matching increased the balance of measured confounders at baseline between treatment and control groups, thereby reducing the threat of confounding. As is the case for all nonexperimental data, there is the potential concern of bias due to unmeasured confounders. By using a difference-in-differences analysis, however, we controlled for individual-level, unobserved, time-invariant characteristics such as growth and language potential.

Time-varying unobserved confounders that differed between the treatment and the control groups were not controlled for in this analysis. Potential sources of unobserved time-varying confounding might include changes in the availability or price of food and changes in the coverage and quality of medical care and education services provided by the government in poor districts. However, we are not aware of reasons why these may have varied systematically between the treatment and control groups. Furthermore, we are unable to empirically validate the difference-in-differences assumption that the change in outcomes would be the same for the treatment and control groups in the absence of treatment. Despite this limitation, the absence of significant differences in the pretreatment 2002–2006 period is reassuring. In light of these limitations inherent to our quasi-experimental study design, treatment effects should be interpreted with some caution. An additional limitation is that we were not able to distinguish between the effects of longer treatment duration, younger age at initiation of treatment, and a focus on poorer subjects, because these characteristics were highly correlated in our data.

A common cause of selection bias in cohort studies is differential loss to follow-up, but that does not appear to be a concern in this study. In our sample from Young Lives, <3% of children were completely lost to follow-up between rounds 1 and 3. Those lost to follow-up were more likely to have a caretaker who spoke an indigenous language, but they were similar across all other covariates and baseline outcomes. Given the low level of loss to follow-up, the difference in this 1 variable is unlikely to bias the empirical estimates.

The cohort of children analyzed in this sample began receiving *Juntos* after the age of 18–24 mo, which is claimed by some to be a threshold above which recovery from growth faltering is unlikely (48). However, recent evidence suggests increases in HAZ after the age of 24 mo in the Young Lives cohort (28–30) and in other birth cohorts (49), so it is worth examining factors that might enhance these increases. Our findings provide evidence that there may be opportunities to address undernutrition among boys after the first 18–24 mo of life.

We hypothesize several pathways through which *Juntos* might produce beneficial effects on HAZ (15). First, households participating in *Juntos* have 100 *soles*/mo of additional income, and reported spending a mean of 72% of transfers on food. Growth may also be improved due to information given to caretakers at growth-control appointments on how to feed their children, resulting in improved food consumption. BAZ and overweight may also have been reduced due to nutritional recommendations given at medical visits or improved access to healthier food due to increased income. Finally, reduced illness as a result of medical visits or healthier home environments may lead to effects on HAZ.

The average treatment effects reported in this study should not be interpreted as the absolute difference between the *Juntos* beneficiaries and matched controls. Rather, the difference-in-differences estimates report the change in the difference in outcomes between *Juntos* beneficiaries and their matched controls. Although the propensity score matching reduced imbalance between potentially confounding baseline covariates, balance of baseline outcomes was not possible. For example, in the sample of boys who received *Juntos* for ≥2 y, the round 1 HAZ was −2.24 and the round 3 HAZ was −1.85. The weighted mean HAZ in the matched control group was −1.67 in round 1 and −1.71 in round 3. These values illustrate that *Juntos* beneficiaries experienced substantial improvements in HAZ compared with matched controls.

This study of the national Peruvian cash transfer program *Juntos* provides evidence to policy makers grappling with the nutritional transition in middle-income countries. The results suggest that social-protection programs, when coupled with health and educational conditions, are associated with a reduction in overweight and improvements in growth at a time when countries are attempting to address both issues. Furthermore, it provides evidence to inform the deployment of policies that may affect language outcomes through reductions in poverty and malnutrition (50). The null association of *Juntos* with language development outcomes suggests that future studies should investigate the effects of cash transfers on language development when initiated at an earlier age and for longer duration of time.

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CTA, SAR, and LCHF designed the research; CTA performed statistical analysis and had primary responsibility for writing the manuscript and final content; and CTA, SAR, JRB, BTC, KAD, JE, SM, AS, ADS, and LCHF contributed to the writing of the manuscript and the interpretation of the results. All authors read and approved the final manuscript.

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