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**ARTICLE**

# Do conditional cash transfers (CCTs) raise educational attainment? An impact evaluation of *Juntos* in Peru

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**Abstract**

**Motivation:** Many evaluations show that conditional cash transfer (CCT) programmes raise school enrolment and attendance of children from low-income households. Less evidence exists on the impact on learning and cognitive skills formation. This article contributes by evaluating the impacts of the CCT *Juntos* in Peru on school progression and performance in standardized test scores.

**Purpose:** We investigate the overall impact of *Juntos* upon educational outcomes of beneficiary children. Specifically, we address two questions: does *Juntos* have an impact on school participation? Can programme participation be linked to impacts upon cognitive skills?

**Approach and methods:** We use a quasi-experimental design that combines kernel matching with difference-in-difference estimation. We rely on data from the Young Lives panel survey and focus on a sample of 3,130 children aged 6–18 years old.

**Findings:** The article reports three main findings: (a) *Juntos* increases the chances of school enrolment and finishing primary school among children aged 12 to 18 years; (b) there is a positive effect on the transition to secondary school among the same age group, which is, however, only weakly significant at the 10% level; and (c) there is no positive impact on test scores of maths and language development among primary or secondary school-aged children.

**Policy implications:** The traditional demand-side focus of CCTs risks neglecting supply factors related to service infrastructure and quality. The article suggests that policy-makers need to study the structural mechanisms behind educational inequalities and integrate CCTs with measures to enhance skills formation for targeted households.

**KEY WORDS**

conditional cash transfer, educational attainment, Peru, propensity score matching with difference-in-difference

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## 1 | BACKGROUND

Conditional cash transfers (CCT) are among the largest social assistance programmes across Latin America. CCTs are targeted transfers to poor households that are conditional upon beneficiary families making pre-specified investments into the education and health care of their children. Typical CCTs require that school-aged children of beneficiary households are registered in school and attend classes while younger children and pregnant or lactating women need to attend regular health checks. As such, these programmes combine an immediate objective of poverty alleviation with a long-term one of enhancing intergenerational social mobility through promoting human capital investment.

Peru began its CCT programme *Programa Nacional de Apoyo Directo a los más Pobres – Juntos* (National Programme to Support the Poorest Together), referred to as *Juntos*, in 2005. This article aims to evaluate its impact upon educational outcomes, specifically asking whether *Juntos* raises the educational attainment of beneficiary children. While better learning outcomes are not an explicit objective of the programme itself, CCTs implicitly build on the assumption that more schooling for children from poor families enhances social mobility in later life. Arguably, in order to reach this long-term objective, skills acquisition and enhanced learning are crucial determinants alongside mere school participation.

The article is structured as follows: this first section gives a brief introduction to theoretical considerations behind CCT programmes and the specific set-up of *Juntos* in Peru. The second section provides a literature review before introducing the data in the third section. The fourth section explains the identification strategy, while the fifth section outlines the empirical estimation results. The last section concludes.

### 1.1 | The rationale behind CCTs

In the development policy debate, CCTs have been hailed as a promising lever to tackle underinvestment into human capital through a demand-side intervention. Little investment into human capital—particularly health and education—can reinforce poverty traps and foster an intergenerational transmission of poverty (Fiszbein et al., 2009). Although public primary and secondary education is free of charge in most countries in which CCTs operate, large inequalities in school enrolment and completion rates among income groups persist.

CCTs aim to tackle this by effectively subsidizing education through lowering its opportunity costs. A conditional transfer works through two channels: the transfer provides additional income to the household and thus relaxes a budget constraint, while the conditionality lowers the price of schooling relative to alternative time uses of children.

This article aims to investigate the overall impact of *Juntos* upon educational outcomes of beneficiary children. It addresses the following two questions:

1. What has been the impact of *Juntos* upon educational attainment?
2. Can programme participation be linked to impacts upon cognitive skills?

*Juntos* in Peru started on a small scale in some of the poorest regions of the country in 2005 and has since been rolled out to more than 750,000 households in nearly 60% of the country's districts. The programme targets beneficiaries in eligible districts via a proxy means test that accounts for demographic and socioeconomic criteria. Eligible families must comprise at least one member under 18 years of age or pregnant, and have lived in the district of enrolment for a minimum of six months

before receiving a transfer. To receive the cash transfer of PEN 200 bimonthly—approximately USD 152 at purchasing power parity (PPP)—per family, children under six years old must attend regular health checks and receive vaccinations, while those aged 6–14 need to attend at least 85% of their classes at school. Pregnant women and young mothers are obliged to attend pre- and post-natal health checks. The uniform scheme as such is rather simple when compared to other CCTs in the region that differentiate transfer amounts for example by the number of children in the household (as in Colombia) or pay an education premium to girls and for advancing to higher grades (as in Mexico).

## 2 | LITERATURE REVIEW

CCT programmes in Latin America have been subject to numerous empirical impact evaluations. Broadly, these can be grouped into four categories (Fiszbein et al., 2009). The first one comprises evaluations of smaller scale pilot programmes that are based on random assignment. Examples are CCT programmes in Nicaragua and Honduras, where random assignment to treatment and control groups has worked well and attrition was low (Maluccio & Flores, 2005). The second category is also based on experimental design methods but studies larger-scale programmes, thus raising fewer questions on external validity. Mexico's *Oportunidades* programme has been evaluated in many studies. Skoufias (2005) associate the CCT with more years of schooling and improved nutrition for poor children as well as better health outcomes for children and adults. Schultz (2004) concludes that the positive effect on schooling is largest for children in the age group transitioning from primary to secondary school.

The third category draws on studies where randomization was not possible or the control group was biased for various reasons. These studies use a regression discontinuity design (RDD) or instrumental variable estimation. Oosterbeek et al. (2008) evaluate the *Bono de Desarrollo Humano* (BDH) programme in Ecuador and find a positive effect on school enrolment for very poor households.

The fourth category uses a quasi-experimental design with difference-in-difference estimation, sometimes combining it with matching. For Colombia's CCT *Familias en Acción*, Attanasio et al. (2005) find that the programme has increased household consumption as well as school attendance by secondary school children for eligible children within the household. However, it has had no effect on ineligible siblings living in the same household.

The objective of CCTs is to promote long-term investment into the human capital of children from impoverished households. To date, there are few studies that focus on learning outcomes rather than enrolment or school attendance rates. Baez and Camacho (2011) find no significant impact on test scores in Colombia, and Behrman et al. (2011) reach a similar conclusion in Mexico when comparing long-term beneficiaries with short-term ones. The relative scarcity of evaluations of learning outcomes is mainly due to the lack of available data on cognitive skills or test scores of children. Andersen et al. (2015) study the impacts of *Juntos* upon nutritional and anthropometric scores as well as language development and grade attainment among 7–8 year-old children and find no effect on the latter. Impacts on anthropometric scores varied by gender and programme exposure.

Perova and Vakis (2012) evaluate the welfare and schooling effects of *Juntos* using instrumental variable estimation and find that the programme has weak but positive effects on consumption, poverty reduction and the use of health services. With regard to educational outcomes, the authors find that *Juntos* has no effect on enrolment while it does raise school attendance. Effects increase with the length of programme exposure.

This article contributes by evaluating the impact of Peru's *Juntos* on the educational attainment of beneficiary children as measured by children's progression through grades, the likelihood of passing

**TABLE 1** Structure of the Young Lives panel

Round	Younger cohort			Older cohort			Siblings		
	2002	2006/07	2009	2002	2006/07	2009	2002	2006/07	2009
N	2052	1963	1943	714	685	678	3915	4792	4408
<i>Juntos</i>	0	90	360	0	23	76	0	470	1565
Mean age	1.00	5.33	7.91	7.98	12.35	14.93	8.32	9.41	9.29
Boys	1027	990	980	386	368	362	2004	2412	2238
Girls	1025	973	963	328	317	316	1911	2380	2170

Source: Own calculations from Young Lives Peru rounds 1–3.

critical transition points and their performance in standardized tests. It falls into the fourth category and, while relying on survey data, uses a similar empirical approach to Attanasio et al. (2005).

### 3 | DATA

The article draws upon panel data from Young Lives, an international study of childhood poverty in four countries that tracks 12,000 children over a 15-year period. The Peruvian sub-sample follows two cohorts of children since 2002 and covers more than 2,700 households, for which three survey waves are used (2002, 2006/2007, 2009). Since the survey's objective is to provide information on childhood poverty and wellbeing, the sampling strategy is not fully random but rather oversamples poor areas. Within the chosen sentinel sites, the selection of households was at random (for a detailed overview of the sampling methodology, see Escobal and Flores, 2008). The younger cohort children were aged 6–18 months at the beginning of the study in 2002 and had reached a mean age of eight by 2009, while the older cohort children were 7–8 years old in 2002 and around 15 years old in 2009. Approximately 17% of the sample lived in *Juntos* beneficiary families in the last survey round. Table 1 summarizes the basic structure of the Peruvian Young Lives panel.

While the Young Lives study focuses on these selected cohort children, a considerable amount of data is also collected for siblings and other household members. It includes information on the socio-economic living conditions of the household, food and non-food expenditure, parental background and social capital, child health and anthropometry as well as children's school attendance, test outcomes and time use. In addition, we have access to geographical data from the *Juntos* administration, in particular the geographic poverty score used to select eligible districts in 2005 and to determine the timing of further roll-out.

This study will focus on an early expansion phase of *Juntos*, namely the period up to 2009. During these early years, *Juntos* was gradually rolled out to prioritized districts so that it is still possible to compare treated districts with similarly poor districts that had not yet been incorporated into the programme. The panel survey comprises an extensive section on livelihoods, income and consumption, which features several questions on *Juntos* participation through which we can identify treated households. In terms of impact, the analysis will look at school enrolment and progression through grades in a first step. Young Lives records for each year and each child within the household whether s/he was enrolled, in which type of school and the last grade completed. Since we do not observe children at the end of their school career, the analysis will give an indication of progress through school and compliance with the regular age-for-grade rather than final years of schooling. This is a relevant question for Peru, because late enrolment and temporary school suspension are a widespread phenomenon in rural

**TABLE 2** Outcomes (child-level) by treatment status in 2009

	<i>Non-Juntos</i>		<i>Juntos</i>		Difference	
	Mean	N	Mean	N	Points	p-value
Enrolled	0.93	4074	0.95	1095	-0.01	0.20
Highest grade	4.43	4074	4.17	1095	0.26	0.02
Age-for-grade	-0.55	4074	-0.27	1095	-0.28	0.00
Primary complete	0.40	4074	0.34	1095	0.06	0.00
In secondary	0.29	4074	0.23	1095	0.05	0.00
PPVT raw score	72.39	2102	50.08	442	22.31	0.00
Maths raw score	14.74	2069	10.33	420	4.42	0.00

Source: Own calculations from Young Lives Peru round 3.

areas.<sup>1</sup> Net enrolment at primary level is almost balanced but significant disparities exist at the secondary level (Ministry of Education Peru, 2014). The transition from primary to secondary school becomes a critical point with higher risk of drop-out. Beyond the Young Lives cohort children, our sample also includes their (half-)siblings if they were born to the same mother and lived in the same household in both survey rounds.

In a second step, the analysis will focus on cognitive skills and learning outcomes. For the purposes of this study, the Peabody Picture and Vocabulary Test (PPVT) and a maths test will be used. The PPVT measures receptive vocabulary skills by presenting, in increasing order of difficulty, pictures to the child who had to choose the word that best matches them. Hence younger children tend to score lower on average by design. The test is available in Spanish and Quechua, the most widely used indigenous language in Peru (Cueto & León, 2012). The maths test covered basic numeric concepts<sup>2</sup> for the younger cohort (aged 4–5 years old) in 2006/2007, while the older cohort (11–12 years old) completed a more difficult subset of the Trends in International Mathematics and Science Study (TIMMS) of 2003, testing basic numerical operations. In 2009, both cohorts took a test comprising an arithmetic section and a second section testing quantitative and number notions (younger cohort) or algebra and geometry (older cohort).<sup>3</sup>

The siblings did not participate in the maths test (by survey design), while they did take the PPVT in the third round as long as they were at least four years old. For this reason, the analysis of learning outcomes will focus on the smaller sample of Young Lives cohort children only. Further, it is important to note that these are not school tests, but were administered as part of the Young Lives survey. This means that children were tested regardless of their school enrolment status, and test conditions were comparable across regions. Table 2 reports descriptive statistics for the outcomes under analysis in the post-treatment round of 2009.

<sup>1</sup>According to Cueto et al. (2016), around 30% of children aged 12 were older than the normative age corresponding to their grade in 2013 for reasons of late entry, grade repetition or temporarily abandoning school. Overage is higher at age 15, and only 42% of children finish school on age.

<sup>2</sup>This refers to the quantitative subtest of the Cognitive Developmental Assessment (CDA) developed by the International Evaluation Association (IEA) to assess the cognitive development of four-year-olds (for more information, see Cueto et al., 2009).

<sup>3</sup>The tests used were a combination of the TIMMS study 2003 referred to above and selected items from national testing programmes. For more details, see Cueto et al. (2009).

## 4 | IDENTIFICATION STRATEGY

The impact of *Juntos* participation on educational outcomes of beneficiary children can be expressed as the additional benefit that an individual gains from participating in *Juntos* compared to the outcome in case of his or her non-participation. This article applies a combined matching and difference-in-difference (MDID) approach as outlined in Heckman et al. (1997) to identify the average treatment effect on the treated (ATT). MDID combines the advantages of both matching and difference-in-difference estimation while also relying on the assumptions of the two methods. According to Abadie (2005), such two-step semi-parametric estimation has advantages over a multivariate difference-in-difference estimation when pre-treatment characteristics that may be associated with the dynamics of the outcome variables are unbalanced. Kernel matching, which amounts to a weighting scheme based on the propensity score, imposes on average the same distribution of covariates for treated and control observations. The propensity score is the only function that needs to be estimated in the first step, it models the selection process. The second step estimates the differences in outcomes, where the common trend assumption can then be relaxed to holding conditional on a balanced (weighted) distribution of the specified covariates.

Matching identifies control observations that resemble the treated ones as closely as possible in observable characteristics. Identification relies on the assumption that selection into treatment is determined by observable characteristics and not confounded by unobservable characteristics that affect outcomes at the same time (conditional independence assumption, CIA). In other words, expected outcomes, given non-participation in treatment  $T$  and conditional on observable characteristics  $X$ , should be the same for participants and non-participants:

$$E(Y_{0i}|T_i = 1, X_i) = E(Y_{0i}|T_i = 0, X_i) \quad (1)$$

This is a strong assumption that may not hold if unobserved factors such as motivation or ability systematically differ by treatment status. The ATT can be estimated under arguably less restrictive assumptions if panel data are available and matching can be combined with difference-in-difference. The latter controls for selection on unobservables, but rests on the assumption that both groups would have experienced the same trends over time in the absence of treatment (common time trend). It measures the treatment effect as the difference in outcomes between treated and non-treated net of their pre-existing difference before treatment. Combining matching with difference-in-difference allows us to control both for observable and unobservable characteristics that are constant over time.

MDID rests upon two key identifying assumptions. First, conditional on observables  $X$ , the evolution of unobservables (captured by the error term  $u$ ) over time  $t$  is independent of treatment status  $T$ :

$$E[(u_{1i} - u_{0i}) | T_i = 1, X_i] = E[(u_{1i} - u_{0i}) | T_i = 0, X_i] \quad (2)$$

In other words, identification rests on the assumption that, in the absence of treatment, both groups would have experienced the same time trends. Secondly, there must be common support:

$$0 < Pr(T_i|X_i) < 1 \quad (3)$$

This requires that the probability  $Pr$  of selection into treatment  $T$  cannot be fully explained by observables  $X$ ; instead, there must be control observations with a probability of treatment in the same range as that of treated observations. MDID hence estimates the treatment effect as:

$$ATT^{MDID} = i1 = \sum_i \{(y_{1i} - y_{0i}) - ij \neq \sum (y_{1j} - y_{0j})\} w_{ij} \quad (4)$$



where  $y$  is the outcome of interest, subscripts 0 and 1 indicate the time period before and after treatment respectively, subscripts  $i$  and  $j$  indicate that the individual belongs to the treatment or control group respectively, and  $w$  is a weighting factor. The weight  $w$  is defined by the matching method chosen (in the present case a Kernel-based estimator) and represents the weight of the statistical twin  $j$  for treated person  $i$ .

#### 4.1 | Targeting and selection into Juntos

*Juntos* did not include an evaluation design from the start and, naturally, programme participation is not assigned randomly. Rather, the targeting process is a three-step procedure: at the first level (geographic targeting), eligible districts are selected according to a composite geographic score that takes into account various poverty measures, child malnutrition levels, the prevalence of unsatisfied basic needs and the extent of exposure to political violence in the previous decade. Based on this score, which was calculated according to a 2005 census (renewed in 2007), 638 districts were prioritized for roll-out during the first programme years; further districts were included from 2009 onwards. In the second step, the individual targeting, eligible households are selected according to a proxy means score that takes into account the following criteria: the ratio of illiterate women residing in the household, the ratio of minors that do not attend school, access to industrial sources of fuel for cooking, dwelling characteristics and access to basic services. Most of these targeting indicators are long-term and not easily changeable in response to expectations about the programme's inception (Ashenfelter's dip). Even for those that may easily be adjusted, such as school participation, it is unlikely that this would have been the case here because the information was recorded as part of the regular census and detailed criteria on eligibility for benefits were not disclosed beforehand. In a final step (community validation), the list of eligible households is verified by a commission of community members and local and national representatives of the *Juntos* administration in order to minimize both inclusion and exclusion errors.

Looking at our sample, Table 3 compares families that have never been *Juntos* beneficiaries in the period under analysis and those that have become *Juntos* beneficiaries at some point between programme start in 2005 and 2009. It shows that, on average, *Juntos* beneficiary families live in larger households, they are less well off in terms of expenditure and wealth.<sup>4</sup> They are far more likely to live in rural areas where reaching the nearest primary school takes on average seven more minutes. The mother in the household has completed on average less than half the years of schooling compared to those in non-*Juntos* households. *Juntos* families tend to live in districts that were ranked in the poorest two quintiles as of 2005 with a prevalence of malnutrition among children aged 6-9 years old of a staggering 45% compared to just under 20% in non-*Juntos* districts in this sample. It is evident that beneficiary households systematically differ from non-beneficiary households. Hence, in the first step, we will apply matching to find a suitable control group by replicating the programme's targeting criteria as closely as possible.

Nonetheless, a biased selection may occur if only the best informed or most mobile from the population of eligible households actually participate. The programme design reduces such risk in several ways: once a district is selected, a survey of each household is conducted in order to determine eligibility. The programme administration then proactively approaches eligible households to offer affiliation with *Juntos*.<sup>5</sup> Hence, the risk of eligible households being unaware of the programme is low. The

<sup>4</sup>The wealth index is a composite score that measures by equal weighting: (a) the housing quality in terms of size and building materials; (b) possession of consumer durables; and (c) access to services of water, sanitation and electricity.

<sup>5</sup>This was the case in the first programme years (Escobal & Benites, 2012). Nowadays, households are not necessarily informed individually, but lists of eligible households are posted in the municipality.



**TABLE 3** Household characteristics in 2006/2007 by treatment status

	Non- <i>Juntos</i> HH		<i>Juntos</i> HH		Difference	
	Mean	N	Mean	N	Points	p-value
Household size	5.36	2103	6.18	320	-0.83	0.00
Wealth index	0.53	2103	0.26	320	0.27	0.00
Total expenditure	179.45	2103	83.46	320	96.00	0.00
Ethnic: Mestizo	0.91	2103	0.97	320	-0.07	0.00
Ethnic: White	0.06	2103	0.02	320	0.04	0.00
Mother's education (years)	8.56	2103	3.54	320	5.02	0.00
Mother's age (years)	33.80	2103	34.23	320	-0.43	0.20
Rural (=1)	0.19	2103	0.78	320	-0.60	0.00
District poverty quintile	2.82	2103	1.29	320	1.52	0.00
District child malnutrition	19.64	2103	45.72	320	-26.08	0.00

*Note:* Total expenditure refers to biweekly household expenditure in PEN. The district poverty quintile and the district malnutrition rate are drawn from the 2005 census and were used by the *Juntos* administration in the geographical targeting. The district poverty quintile ranks from 1 (poorest) to 5 (least poor) and draws upon a multidimensional poverty index. The malnutrition rate refers to the age group 6–9 years.

*Source:* Own calculations from Young Lives Peru round 2.

sequential regional roll-out may reduce incentives for moving into a (poorer) programme district if a later incorporation of the home district may be expected while moving is costly. Also, a household has to live in the district for at least six months before qualifying for the transfer. Finally, the community validation aims to minimize discretionary powers of local officials or community representatives by ensuring a mixed composition of members. Various channels exist for families to complain and demand a reassessment of their eligibility.

Even if we believe that the programme rules successfully target the poorest, there may be systematic unobserved differences if some parents value education more than others or place more trust in the local health services. In order to control for any unobserved pre-existing differences between the control and treatment groups, we apply difference-in-difference estimation on the matched sample. Applied to *Juntos*, MDID compares the difference in outcomes between children of families that are similar in observable characteristics except for the fact that some benefitted from *Juntos* while others did not, taking into account the differences that existed already before treatment. The core identifying assumptions as outlined above will now be discussed further.

## 4.2 | Matching and the common support assumption

As described in Table 3, *Juntos* households differ from non-*Juntos* households in observable characteristics that may simultaneously affect the outcome variables. We apply a kernel-matching estimator with a bandwidth of 0.05<sup>6</sup> (respectively 0.06 and 0.07 for different subsamples, see below) to restrict our

<sup>6</sup>The bandwidth essentially functions as a smoothing parameter of the kernel density function that has to be chosen carefully to balance between bias and efficiency of the estimator. The bandwidth of 0.05 has been calculated using the following formula:  $h = 1.06 \frac{A}{n^{1/5}}$ ,  $A = \min(\sqrt{\text{Var}(x)}, \frac{IQR(x)}{1.34})$  according to Wilcox (2012) and Silverman (1986), with  $n$  referring to the sample size of those observations in the common support,  $IQR$  referring to the interquartile range and  $x$  referring to the estimated propensity score. Alternative bandwidths of 0.04 and 0.06 have not yielded materially different results.

control group to those observations that best resemble the former group in terms of observable characteristics. A kernel estimator has the advantage that it uses weighted averages of (nearly) all control observations and thus makes use of more information, thereby reducing the variance. This may be advisable when the number of control observations is large, as in the present case (Caliendo & Kopeinig, 2008). Since the treatment itself can affect matching covariates, matching is best undertaken on the basis of pre-treatment characteristics (Blundell & Dias, 2009). We therefore restrict the treatment group to children whose families joined *Juntos* at some point between 2007 and 2009 in order to compare outcomes before and after treatment. This way, all children in our sample were non-beneficiaries in the observation year 2006/2007, while 16% benefitted from *Juntos* in the observation year 2009.

Our sample includes children of both age cohorts and their siblings if they were at least six years old in 2009 and lived in the household in both survey rounds. In choosing the matching covariates, we replicate the actual targeting criteria outlined above as closely as possible. First, we excluded all households from the department of Lima (spanning the capital and surrounding provinces) since this densely populated area may not serve as a good control group for treated rural districts. We included the geographical targeting score in the matching covariates to ensure balancing between the two groups. As further geographical controls we included the distance to the next primary and secondary schools and whether the child lived in a rural or urban district. Household characteristics include the family's wealth and expenditure situation, the family size and composition, the ratio of minors in the household that do not attend school, as well as the mother's years of schooling. Individual characteristics include age, sex and ethnic background of the child.

Table 4 reports the balancing of these covariates before and after matching: it shows that matching achieves a balanced distribution with respect to all but one variable, namely the mother's years of education. This unbalanced distribution may be a concern since we would expect the educational status of the mother to affect that of her children. Since this relationship is a positive one, it would likely introduce a downward bias in the estimation. Overall, the propensity score of the treatment and control group share a large area of common support.

The matched sample now includes 6,260 observations, of which 1,620 belong to the treatment group (this corresponds to 2,320 children in the control group and 810 children in the treatment group per round). They cover the age range of six to 18 years and have a mean age of 10.8 years in the post-treatment round of 2009.

Given the Young Lives design, a large share of the sample constituted by the younger cohort is still of primary school age (up to grade 6).

### 4.3 | Common trend assumption

The common trend assumption essentially stipulates that, in the absence of *Juntos*, the trend in enrolment rates, progression through grades and in learning outcomes would have been the same for the treatment and control groups. In other words, the change over time in outcomes observed for the control group represents a good counterfactual of the changes beneficiaries would have experienced had they not benefitted from treatment. Naturally, we cannot test this assumption; nonetheless, trends observed in the period just before *Juntos* began to operate provide some support for it. Table 5 reports the difference-in-difference estimation among a subset of the older children in our sample in the years just before the families in the treatment group began to benefit from *Juntos*. It shows that the trends in enrolment rates and school progression did not differ significantly between the two groups. Unfortunately, the PPVT and maths tests were not yet administered in the first Young Lives survey wave of 2002 so that the pre-treatment trend cannot be observed.

**TABLE 4** Logit estimation on treatment status

Variable	Unmatched			Matched		
	Treated	Control	p-value	Treated	Control	p-value
Child's age	8.13	8.14	0.896	8.12	8.23	0.518
Girl (=1)	20.89	13.21	0.991	18.16	16.48	0.477
Indigenous language	1.82	1.22	0.000	1.81	1.81	0.939
Wealth index	0.26	0.50	0.000	0.27	0.28	0.208
Expenditure	78.71	165.54	0.000	79.73	84.61	0.361
Household size	0.09	0.09	0.000	0.08	0.08	0.632
Children aged 6–18	0.07	0.10	0.000	0.07	0.05	0.253
Generations in HH	3.09	8.00	0.448	3.21	3.55	0.858
Out-of-school ratio	1.64	1.18	0.224	1.61	1.62	0.585
Female HH-head	6.83	5.73	0.079	6.76	6.89	0.373
Mother's education	2.24	2.27	0.000	2.21	2.24	0.016
Rural (=1)	8.00	8.05	0.000	7.98	7.95	0.770
Time to school	0.49	0.48	0.000	0.49	0.50	0.581
District index	0.54	0.06	0.000	0.51	0.52	0.986

*Note:* Expenditure refers to biweekly per capita household expenditure in PEN. Indigenous language is a binary indicator that equals 1 if the child's mother tongue is Aymara or Quechua.

*Source:* Own calculations from Young Lives Peru rounds 2–3.

## 5 | RESULTS

Having balanced the two groups in terms of observable characteristics before treatment, we apply difference-in difference estimation in a second step. The first set of outcomes relates to school participation as measured by enrolment status, years of schooling, transition from primary to secondary school and age-for-grade. Intuitively, the mere compliance with conditionalities should have a positive effect on enrolment, while the effect on years of schooling is ambiguous: it may be positive if beneficiaries are induced to stay in school and advance through grades, while it may be zero (or even negative) if the incentive is only to comply with attendance requirements. The same reasoning applies to the child's grade relative to his or her age, and the transition from primary to secondary school: stringent attendance requirements should lower the risk of drop-out at this transition point. However, it may not if children repeat grades or if opportunity costs of schooling increase exponentially with age and outweigh the financial incentive. *Juntos* requires a minimum attendance of 85% of schooling hours, on which schools report to the *Juntos* office every two months. In case of non-compliance with conditionalities, a family will be suspended from the programme temporarily, but qualifies again for the payment once conditionalities are fulfilled.

The second set relates to learning outcomes. The anticipated effect is not clear-cut: regular attendance may facilitate better learning outcomes and test scores. However, mere presence in school may not be enough to facilitate an actual transfer of information into enhanced cognitive skills. While the intention of CCTs is to get children into school, prevent early drop-out and hence foster learning, these gains may not materialize if schooling quality is low or further support mechanisms for disadvantaged children are not available.

**TABLE 5** Difference-in-difference estimation on pre-treatment trends

	Enrolled	Highest grade
<b>R2*Treated</b>	−0.000148 (0.0385)	−0.136 (0.118)
Round 2	0.403*** (0.0309)	2.152*** (0.0910)
Treated	0.00808 (0.0368)	0.154 (0.0937)
Constant	0.567*** (0.0295)	0.821*** (0.0702)
Observations	2,952	2,756
R-squared	0.233	0.247

Note: Robust standard errors in parentheses; clustered at the district level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

R2 equals 1 in round 2 of the Young Lives survey (2006/2007) and 0 in round 1 (2002). Treated is a binary variable that equals 1 for all observations that received treatment in round 3 (2009) and 0 otherwise.

Source: Own calculations from Young Lives Peru rounds 1-2.

## 5.1 | Impacts upon school participation

Table 6 reports the results for the first set of outcomes. The parameter of interest is Diff-in-Diff: it captures the change in outcome levels over time between children of beneficiary and non-beneficiary families.<sup>7</sup> The simple differences between the treated group (T) and the control group (C) are reported for the baseline and follow-up period respectively. Standard errors are clustered at the district level.<sup>8</sup>

Panel A reports the outcomes for the pooled sample. The point estimates suggest that children from *Juntos* families are about 5 percentage points more likely to be enrolled in school, while the point estimates on years of schooling, albeit positive, are rather imprecisely estimated by the difference-in-difference method and thus statistically not significant. The same holds for the probability of finishing primary school and transiting to secondary school. Highly statistically significant is the difference in age-for-grade, which suggests that *Juntos* children are catching up with their regular age-for-grade: while they were are on average older than their peers of the same grade before programme start, this difference fades. While overall these results may be sobering at first sight, descriptive statistics show that school participation and enrolment rates are rather high in primary school from the outset (mean net enrolment rate of 93%). This is different for secondary schooling where mean school participation is significantly lower (83%) and differences run both along a rural-urban divide and between income groups. In this sense, the pooled sample may hide heterogeneous effects that differ between age groups.

Hence, we perform a separate analysis for children in the post-treatment age groups of primary (up to grade 6) and secondary (grade 7 to 11) school respectively. Panel B reports the MDID outcomes for the younger group below the age of 12 years. For this group, the outcomes concerning the transition from primary to secondary school are not yet relevant since this transition only happens around the age

<sup>7</sup>We include an additional control related to the interview date to control for any variation in time passed between the two survey rounds, since each was carried out over a time span of several months.

<sup>8</sup>The results are robust to clustering standard errors at the household level instead, bootstrapping standard errors or leaving out clusters altogether.

TABLE 6 *Juntos* impacts upon schooling outcomes (MDID)

Outcomes	Enrolled	Highest grade	Age-for-grade	Complete primary	In secondary
<i>Panel A: Pooled sample</i>					
Baseline					
Control	0.634	2.209	0.002	0.131	0.071
Treated	0.621	1.911	0.183	0.101	0.043
Diff (T-C)	-0.013 (0.019)	-0.298 (0.191)	0.181** (0.077)	-0.030* (0.016)	-0.028** (0.012)
Follow-up					
Control	0.945	3.999	-0.273	0.344	0.208
Treated	0.983	3.765	-0.258	0.304	0.197
Diff (T-C)	0.038** (0.018)	-0.234 (0.232)	0.015 (0.097)	-0.040 (0.030)	-0.011 (0.034)
<b>Diff-in-Diff</b>	<b>0.051**</b> (0.020)	<b>0.064</b> (0.068)	<b>-0.167***</b> (0.054)	<b>-0.010</b> (0.023)	<b>0.017</b> (0.029)
Observations	6260	6260	6260	6260	6260
R-squared	0.15	0.14	0.04	0.09	0.07
<i>Panel B: Age group primary school (under 12 years)</i>					
Baseline					
Control	0.386	0.315	-0.083		
Treated	0.376	0.303	-0.111		
Diff (T-C)	-0.010 (0.043)	-0.012 (0.064)	-0.028 (0.054)		
Follow-up					
Control	0.985	1.776	-0.546		
Treated	0.997	1.754	-0.598		
Diff (T-C)	0.012* (0.007)	-0.022 (0.134)	-0.052 (0.086)		
<b>Diff-in-Diff</b>	<b>0.022</b> (0.041)	<b>-0.010</b> (0.094)	<b>-0.024</b> (0.069)		
Observations	3346	3346	3346		
R-squared	0.42	0.35	0.21		
<i>Panel C: Age group secondary school (12-18 years)</i>					
Baseline					
Control	0.978	4.775	0.077	0.326	0.194
Treated	0.972	4.236	0.573	0.247	0.105
Diff (T-C)	-0.006 (0.011)	-0.539** (0.205)	0.496*** (0.126)	-0.079** (0.027)	-0.089*** (0.024)
Follow-up					
Control	0.882	7.390	0.164	0.861	0.529

(Continues)

TABLE 6 (Continued)

Outcomes	Enrolled	Highest grade	Age-for-grade	Complete primary	In secondary
Treated	0.949	7.173	0.271	0.852	0.533
Diff (T-C)	0.067** (0.032)	-0.217 (0.227)	0.107 (0.151)	-0.009 (0.047)	0.004 (0.068)
<b>Diff-in-Diff</b>	<b>0.073**</b> (0.034)	<b>0.322***</b> (0.065)	<b>-0.389***</b> (0.094)	<b>0.070**</b> (0.031)	<b>0.093*</b> (0.053)
Observations	1956	1956	1956	1956	1956
R-squared	0.04	0.38	0.02	0.34	0.22

Note: Robust standard errors in parentheses; clustered at the district level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Kernel bandwidth: 0.05 (Panel A), 0.06 (Panel B), 0.07 (Panel C). Matching covariates include those listed in Table 4.

of 12. The results for the relevant outcomes show no significant difference between the groups: while children participating in *Juntos* have a higher point estimate compared to their non-treated peers in terms of probability of enrolment, the difference is statistically not significant. As argued above, this is not surprising given the generally high participation in primary school. The same holds for trends in years of schooling and conformity with the regular age-for-grade.

The next panel C performs the same analysis for the older age group of 12 years or above. This group contains 1,956 observations of which 646 belong to the treated group. Here, the positive impact upon enrolment rates<sup>9</sup> is significant at the 5% level and suggests a difference of 7.3 percentage points. A significant positive impact appears for years of schooling, which suggests that children from *Juntos* families accumulate on average just over four months more schooling over time than non-treated children. This is consistent with the positive impact upon enrolment that indicates a lower drop-out rate among *Juntos* children. It may also be due to less repetition: column 3 shows that *Juntos* children progress on average faster through grades. While they are on average almost half a year older than their peers of the same grade before treatment, they close this gap over time and move closer to a regular age-for-grade. The impact is approximately of the same magnitude as that on years of schooling.

Column 4 tests whether treatment is associated with a higher likelihood of completing primary school. The effect is positive, albeit only weakly significant, and driven by a closing of the pre-treatment gap. Similarly for the probability of making the transition from primary to secondary school. The impact of 9 percentage points is weakly significant at the 10% level and larger than that on enrolment. Hence, the impact may be a cumulative effect of less drop-out after primary school and faster progression, be that a result of the minimum attendance requirement of 85%, better performance or other driving forces.

In a nutshell, Table 6 suggests that, on average *Juntos* participation has no statistically significant impact upon schooling outcomes of primary school-aged children in terms of their enrolment probability or progress through school grades. We detect a positive impact, however, upon enrolment, years of schooling and the probability of transiting from primary to secondary school among children aged 12 years and above. Descriptive statistics indicate that this age group is at higher risk of school drop-out, and that the transition from primary to secondary school is a critical point. If we look at simple

<sup>9</sup>Note that this variable actually refers to being in school or having completed secondary school; as such, the outcome is not coded zero for children that are not enrolled because they completed secondary school (which accounts for very few observations).

differences between the groups in the two time periods, it becomes apparent that positive impacts are often due to beneficiary children catching up with their peers over time. While for most outcomes, beneficiary children started at a lower level (except for enrolment), they catch up by the post-treatment period. This can plausibly be related to programme conditionalities, which not only require enrolment of children aged 6 years and above, but also a minimum and regular attendance requirement of 85%. This observation further supports the MDID strategy since it becomes apparent that, even after matching, *Juntos* children systematically start out with lower outcome levels than their non-treated peers. The difference-in-difference estimation accounts for this pre-treatment difference in outcomes and measures the change experienced over time.

## 5.2 | Impacts upon learning outcomes

Table 7 looks at learning outcomes as measured by the PPVT and maths tests. Scores are standardized by age strata in order to make them comparable over time and age groups in a linear difference-in-difference model.<sup>10</sup> Since the tests were administered to siblings in the post-treatment round only while the Young Lives cohort children were tested in both rounds, we reduce the sample to the cohort children only. An additional control dummy to capture whether a child took the PPVT test in a language other than his or her mother tongue<sup>11</sup> is included.

Columns 1 and 2 report the results for the PPVT and maths tests of the younger cohort children. In both cases, the coefficients are negative but only in case of the maths score is the difference statistically significant. For the older cohort children, aged between 14 and 15 years in the post-treatment round, the coefficients also appear negative but insignificant. The results for the older cohort need to be treated with caution since the number of treated observations only reaches 94, hence the relatively large standard errors. The negative sign of the coefficients seems counter-intuitive at first since there appears no straightforward reason to believe that *Juntos* participation would have a negative effect on learning outcomes. In fact, the trend in PPVT and maths scores over time shows that both groups have improved their scores over time while beneficiary children have done so by fewer points than their counterparts. In the younger cohort, treated children increased their maths test score by on average 1.5 points (approximately half standard deviation) less than non-treated children did.

If we look at simple differences only, it becomes apparent that the negative impact is driven by post-treatment differences: while pre-treatment scores do not statistically differ between the groups, they are significantly lower in the post-treatment round for both tests (younger cohort) respectively PPVT (older cohort). In fact, the negative effect appears even stronger in the first difference estimation: the differences in PPVT and maths scores are statistically significant for the younger cohort, while for the older cohort only the difference in PPVT scores is weakly significant.<sup>12</sup> The stronger effect in the first difference estimation is consistent with the fact that *Juntos* children already had lower mean test scores in the pre-treatment round.

When interpreting these results, one needs to examine carefully what the counterfactual of no treatment may be. *Juntos* should increase school participation both at the extensive and intensive

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<sup>10</sup>The PPVT test has been standardized using a z-score standardization while, for the maths tests, a quintile range standardization was applied. The standardization was applied in age strata of 9 months.

<sup>11</sup>Children were free to choose their preferred language and a number of children chose to take the test in their native language Quechua in the pre-treatment round, but opted for Spanish in the post-treatment round.

<sup>12</sup>The table reports estimates based on standardized test scores, estimates based on raw scores yield the same results.



**TABLE 7** *Juntos* impacts upon test scores (MDID)

Outcomes	Younger cohort		Older cohort	
	PPVT	Maths	PPVT	Maths
Baseline Diff (T-C)	0.003 (0.123)	0.256 (0.218)	-0.111 (0.100)	-0.181 (0.214)
Follow-up Diff (T-C)	-0.229* (0.118)	-0.355*** (0.035)	-0.338** (0.153)	-0.283 (0.195)
<b>Diff-in-Diff</b>	-0.232 (0.178)	-0.611** (0.231)	-0.227 (0.148)	-0.101 (0.227)
Observations	1491	1571	496	438
R-squared	0.01	0.02	0.06	0.02

Note: Robust standard errors in parentheses; clustered at the district level. Significance level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Kernel bandwidth: 0.05 (younger cohort), 0.04 (older cohort). Matching covariates include those listed in Table 4 and the child's age in months, siblings rank and whether s/he attended pre-school.

margin if households comply with conditionalities, and if the incentive provided lowers the opportunity costs of schooling significantly for at least some families.<sup>13</sup> On an individual level, the counterfactual may hence be to attend fewer school hours or to drop out of school altogether. On an aggregate district level, the increased demand for schooling may lead to overcrowding or less stringent criteria for passing school in order to prevent needy children from dropping out and so losing the transfer. Thus, treatment may have no positive impact on learning outcomes if school quality and infrastructure are not enhanced in parallel, or worse the treatment effect may even be negative if classrooms become overcrowded. Although we control for regional characteristics related to poverty levels and distance to schools, we cannot control for factors related to school infrastructure due to a lack of available data. In this sense, the quality of school infrastructure may be one channel to explain any potential relation between the presence of *Juntos* and individual learning progress, and is most certainly one that merits further investigation. Finally, we tested for the length of exposure to treatment. This did not change results significantly nor did it give evidence for positive marginal effects of an extra year of treatment, which may be due to the fact that we cannot yet observe long-term trends.

## 6 | CONCLUSION

This article has evaluated the effects of *Juntos* participation on educational attainment as measured by school participation and learning outcomes. *Juntos* constitutes a typical CCT programme that provides incentives to poor families to invest in their children's education by ensuring regular school participation. The article has adopted a combined propensity score MDID approach to analyse whether *Juntos* can be associated with higher levels of schooling reached and improved learning outcomes. It has focused on a sample of 3,130 children aged between six and 18 years in the period under analysis, which were first surveyed in 2006/2007 (pre-treatment) and a second time in 2009 (post-treatment).

The estimated results are mixed: they show no effect on school participation of primary school-aged children, which is not surprising given the high primary school enrolment rates in Peru from the

<sup>13</sup>Recall that previous absence or presence in school is no eligibility criteria, families can claim the benefit regardless of whether their children complied with the conditionalities before programme start already. Hence, if only those families enrol that would comply with conditionalities even in the absence of the transfer, the behavioural change may be zero.

outset. This is consistent with Perova and Vakis (2012) who find no impact of *Juntos* on enrolment of children aged 6–14, and with Bastagli et al. (2016) who suggest that marginal effects are highest where there is most room for improvement. A positive impact is observed for children of secondary school age: treated children have a higher enrolment probability, seem to progress faster through grades and are more likely to finish primary school and enter secondary school holding age constant. This is consistent with evidence from other countries such as Colombia (Attanasio et al., 2005) and Mexico, where CCTs significantly decreased the risk of drop-out at the transition from primary to secondary school (Schultz, 2004). It is, however, too early to assess whether any positive effect on years of schooling persists through and up to completion of secondary school, given that *Juntos* had not been around yet long enough in the post-treatment round of 2009, and given that we do not observe final years of schooling.

The findings for learning outcomes are less encouraging: programme participation has no effect on learning outcomes as measured by PPVT and maths test scores of the older cohort children, and even a negative effect on maths scores of the younger cohort. While few studies evaluate CCT impacts upon test scores, the ones that do find similar results: Baez and Camacho (2011) find non-significant negative effects on maths scores and weakly significant negative effects on language test scores of recipients of *Familias en Acción* in Colombia. Akresh et al. (2013) find non-significant positive results on both maths and French language test scores of a CCT in Burkina Faso. Reasons for the inconclusive evidence base of effects on learning outcomes may be down to the fact that learning outcomes are influenced by a range of factors that are outside the scope of CCT interventions. Bastagli et al. (2016) suggest that due to the diverse range of mediating factors, including children's nutrition, parental education and quality of service delivery, it is hard to empirically identify linear effects.

In this sense, the links between *Juntos* participation and learning outcomes are not clear-cut: the programme may have a positive impact that is transmitted via the attendance requirement and the increased awareness of the value of education that the programme promotes. There are, however, no incentives attached to learning outcomes or performance measures nor have explicit supply side interventions been linked to the programme. A negative relationship as observed for the younger cohort seems worrisome and may point to a potential mismatch between increased demand for schooling services in treatment areas and their supply in terms of quality and infrastructure. CCTs have often been criticized for focusing on the demand side of human capital investment only, neglecting supply factors that may influence schooling decisions and outcomes. While the evidence of this article is insufficient to draw such conclusion, the link between CCTs and learning progress as well as the role of school quality and infrastructure certainly merit further analysis.

Equally, if not more, important are the implications of research on skill formation that point to the important role of early childhood years for cognitive development. It is well known that abilities are not only transmitted from families to children through genes, but that parental investment and the family environment play a huge role. Cunha and Heckman (2007) find that substantial differences in abilities are evident before children start school, and that these differences are related to socio-economic background. They propose a model in which early childhood investment leads to different returns from late childhood investments, such that no equity-efficiency trade-off exists for the former. In this sense, an intervention such as a CCT may come rather late for the purpose of cognitive skill development if children are already disadvantaged when they start school. By that time, comparatively more investment is needed to close the gap in cognitive skills to their advantaged peers. This is not to suggest that there is no role for CCTs to play or to disregard other objectives they pursue. It may rather point to the argument that early and late interventions are complementarities.

This article has not addressed heterogeneous effects that may differ between different family types, ethnic background or risk groups. Larger families may find it more difficult to comply with conditionalities since more children have to fulfil them while the transfer itself stays flat (effectively decreasing in relative importance if younger siblings reach schooling age). There is evidence that CCTs can have differential impacts upon girls if their school attendance is linked to differential or higher transfers (Baez & Camacho, 2011; Baird et al., 2011). Regarding the overall effect of varying transfer size, Bastagli et al. (2016), however, do not find conclusive evidence in a review of four studies that this has an impact on educational outcomes. From a policy perspective, the benefits of a conditional versus an unconditional transfer would be a further insightful analysis. As such, we cannot determine whether any positive effects observed are primarily due to a shift in the budget constraint (i.e. the transfer) or to a decrease in the opportunity cost of schooling (i.e. the conditionality). Evidence from Burkina Faso (Akresh et al., 2013) and Malawi (Baird et al., 2011) found that attaching conditionalities related to school participation has positive impacts upon attendance rates compared to unconditional transfers. However, an unconditional cash transfer that strongly labelled the transfer as being for educational purposes had similar effects to a conditioned one in Morocco (Benhassine et al., 2015). While this may not be a relevant question when the main concern is the evaluation of impacts upon human capital formation, it would be a core question when weighing the costs of different programme alternatives against their benefits. Administrative costs related to the monitoring of compliance with conditionalities would have to be weighed against alternative uses such as increasing the transfer, covering a larger target population, investing in school infrastructure or into early childhood interventions as proposed by Cunha and Heckman (2007).

In summary, this article has offered some support to earlier findings from different countries that attest that CCTs have a positive impact upon school participation of secondary school-aged children that may be at risk of dropping out at or after the transition to secondary school. It has not found any evidence for improved learning outcomes that may result from higher school participation, but rather points to further analysis being needed to investigate potential links between CCTs and skills formation.

## DISCLAIMER

The data used in this publication come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India (Andhra Pradesh and Telangana), Peru and Vietnam ([www.younglives.org.uk](http://www.younglives.org.uk)). Young Lives is funded by UK aid from the Department for International Development (DFID), with co-funding from 2010 to 2014 by the Netherlands Ministry of Foreign Affairs, and from 2014 to 2015 by Irish Aid. The views expressed here are those of the author(s). They are not necessarily those of Young Lives, the University of Oxford, DFID or other funders.

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