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Do Conditional Cash Transfers (CCT) Raise Educational Attainment?
A Case Study of Juntos in Peru

Anja Gaentzsch

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Abstract

This paper empirically investigates the impacts of Peru's Conditional Cash Transfer (CCT) programme JUNTOS upon educational outcomes of beneficiary children. The findings associate Juntos participation with higher overall enrolment rates and grades of schooling for children aged 12 to 18 years. This effect translates into a higher probability of finishing primary school and entering secondary school for the same age group. Evidence suggests that this is linked to a faster progression through grades rather than final years of schooling. We find no impact on enrolment or school progression for younger children aged 6 to 11 years. Further, Juntos participation does not have a positive impact upon scores of receptive vocabulary and mathematics tests. Rather, children aged 7-9 years seem to make less progress over time compared to children from non-beneficiary families, while there is no impact upon older children. Evidence on the underlying reasons for this is inconclusive and merits further analysis.

JEL Classification: I24, I25, I38

Keywords: conditional cash transfer, human capital investment, social assistance, educational attainment.

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* Anja Gaentzsch (anja.gaentzsch@fu-berlin.de) is a PhD candidate at the Economics Department of Freie Universitaet Berlin.

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1 Background

Conditional cash transfers (CCT) are among the largest social assistance programmes in many Latin American countries. 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 started its CCT programme *Programa Nacional de Apoyo a los más Pobres Juntos* (National Programme to Support the Poorest Together), shortly referred to as Juntos, in 2005. This paper aims to evaluate its impact upon educational outcomes, specifically asking whether Juntos raises the educational attainment of beneficiary children. The analysis encompasses both the effect on the demand for education services in terms of participation, as well as the impact upon learning outcomes that may result from it. 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 paper 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 under-investment into human capital through a demand-side intervention. Little investment into human capital – in particular health and education – can reinforce poverty traps and foster an intergenerational transmission of poverty (Fiszbein et al., 2009). Although in the bulk of countries where CCTs operate, public primary and secondary education is free of charge, large inequalities in school enrolment and completion rates among income groups persist. Table 1 shows that this is also the case in Peru, where net enrolment at primary level is almost balanced (average net enrolment of 92.9 respectively 93.3 percent for those classified as non-poor by the Peruvian government versus those classified as extremely poor) but significant disparities exist at the secondary level (86.5 versus 66.2 percent, respectively) (MINEDU, 2014).

Table 1: Net school enrolment rates* in 2014 (by region and poverty status)

	Extremely poor	Non-poor	Mean	Rural	Urban
Primary	92,9	93,3	**92,9**	93,2	92,7
Secondary	66,2	86,5	**82,9**	74,5	86,7
Tertiary**	9,6	75,9	**64,7**	29,7	75,4

* The net enrolment rate refers to the percentage share of enrolled children of the official age group for a given level of education out of the total of this age group.

** Includes all form of post-secondary education.

Source: MINEDU (2014)

CCTs aim to tackle this by effectively subsidizing education through lowering its opportunity costs. Hence, 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 paper aims to investigate the overall impact of Juntos upon educational outcomes of beneficiary children.[1] Specifically, it addresses the following two questions:

[1]This overall impact may result from an income and/or substitution effect. Empirically, it is difficult to decompose any overall impact into an income and substitution effect unless through a randomized controlled trial that features both a conditional and an unconditional transfer, or with a structural model that estimates the parameters determining demand for schooling. Such decomposition, which would be insightful in order to assess for example the benefits of a conditional programme over an unconditional one against the costs that compliance monitoring creates, goes beyond the scope of this paper.

1) What has been the impact of Juntos upon school participation?
2) Can programme participation be linked to impacts upon cognitive skills?

Juntos began to operate in Peru in the second half of 2005. Starting on a small scale in some of the poorest regions of the country, it has since been rolled out to cover more than 750.000 households in nearly 60 percent of the country's districts. The programme targets beneficiaries in eligible districts via a proxy-means test that takes into account demographic and socio-economic criteria. The conditionalities that the household has to meet in order to receive the cash transfer of 200 Nuevo Soles bimonthly per family (amounting to approximately US$ 107 in PPP terms[2]) are outlined in Table 2. Eligible families must comprise at least one member under 19 years of age or pregnant, and live in the district of enrolment for a minimum of six months before receiving a transfer. Children under the age of 6 have to attend regular health checks and receive vaccinations, while school-aged children between 6 and 14 have to be enrolled in school and attend a minimum of 85 percent of the classes. Pregnant or lactating women need to undergo pre- and post-natal health examinations. 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 for example in Colombia) or pay an education premium to girls and for advancing to higher grades (as for example in Mexico).

Table 2: Juntos conditionalities

Target group	Conditionality	Benefit
Children under 6 years	Attendance of regular health checks (CRED), vaccinations	100 Soles per month per family (≈ US$ 35)
Children aged 6-14 years	School attendance of at least 85% of the school year	
Pregnant and lactating women	Pre- and post-natal health checks	

[2] The PPP exchange rate is based on the data provided by the International Comparison Group (ICP) 2011 of the World Bank Group (PPP Peru of 1,521 (US$=1)). http://siteresources.worldbank.org/ICPEXT/Resources/ICP_2011.html

2 Literature review

CCT programmes in Latin America have been subject to numerous empirical impact evaluations. Broadly, these can be grouped into four categories (see Fiszbein, Schady, 2009, Appendix B). 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 while attrition was low. Maluccio and Flores (2005) provide an impact evaluation of the Nicaraguan CCT *Red de Protección Social* using experimental design. The second category is also based on experimental design methods but studies larger scale programmes, thus raising fewer questions on external validity. The most prominent example is certainly Mexico's *Prospera*[3] programme which has been evaluated on many accounts. Evaluations include for example Skoufias (2005) who associates 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) has evaluated later stages of the programme in rural areas, where no control groups had been established anymore. Using a matching design combined with first-difference regression analysis, he concludes that the programme has a positive effect on schooling; this effect is largest for children in the age group of transition 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): transfer eligibility is often determined by means-testing, where households falling below a certain poverty threshold are selected into the treatment group. RDD compares outcomes for households just below this cut-off point (treatment group) with those just above the threshold (control group). Osterbeek, Ponce and Schady (2008) have used this approach to evaluate *the Bono de Desarrollo Humano (BDH)* programme in Ecuador. Since there was a significant amount of non-eligible households just above the threshold that received transfers nonetheless, the

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[3] At the start in 1997, the Mexican CCT programme was called Progresa, it then changed its name to Oportunidades in 2002 and was recently rebranded as Prospera. For simplification, this paper refers to the programme only as Prospera.

authors additionally use an instrumental variable to control for this bias. They conclude that the programme had a positive effect on school enrollment 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 of 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. This is mainly due to the lack of available data on cognitive skills or test scores of children. This paper wants to make a contribution 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 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 as Attanasio et al. (2005) do.

To my knowledge, there is only one study that investigates Juntos' impacts on cognitive skills: Andersen et. al (2015) study the impacts of Juntos upon nutritional and anthropometric scores as well as language development and grade attainment among young children aged 7-8 years, and find no effect on the latter. Impacts upon 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 regards 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. Jaramillo and Sánchez (2011) focus on nutritional outcomes among children aged 0 to 5 years and find that Juntos reduces the incidence of chronic malnutrition among beneficiary children significantly, with a positive effect again attributed to length of exposure. Escobal and Benites (2012) find positive impacts upon household welfare and consumption and a negative impact upon child work, but no significant effect upon child nutrition. Other evaluations of Juntos focus on the programme's impact upon social engagement (Camacho, 2014) and labour supply decisions (Fernandez and Saldarriaga, 2014).

3 Data

The paper 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.[4] The Peruvian sub-sample follows two cohorts of children since 2002 and covers more than 2.700 households, for which three survey waves are currently available (2002, 2006/07, 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 8 by 2009, while the older cohort children were 7-8 years old in 2002 and around 15 years in 2009. Approximately 17 percent of the sample lived in Juntos beneficiary families in the last survey round. Table 3 summarizes the basic structure of the Peruvian Young Lives panel.

Table 3: Structure of the Young Lives Panel

	Younger cohort			**Older cohort**			**Siblings**		
Round	2002	2006/07	2009	2002	2006/07	2009	2002	2006/07	2009
N	2052	1963	1943	714	685	678	3915	4792	4408
Juntos	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

[4] Young Lives is coordinated by the University of Oxford and its partner institutions in the study countries. Further details available at: www.younglives.org.uk

While the Young Lives study focuses on these selected cohort children, a vast 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, I have access to geographical data from the Juntos administration, in particular the geographic poverty score that was 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 years up to 2009. During these early years, Juntos was rolled out to prioritized districts gradually so that it is still possible to compare treated districts with similarly poor districts that were not yet incorporated into the programme. The panel survey comprises an extensive section on livelihoods, income and consumption, which features several questions on Juntos participation[5] through which I can identify treated households. In terms of impacts, 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 I do not observe children at the end of their school career, the analysis will give me 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 areas[6]. In particular, the transition from primary to secondary school thus becomes a critical point with higher risk of drop-out. Beyond the Young Lives cohort children, my sample also includes their (half-) siblings if they were born to the same mother and lived in the same household in both survey rounds.

[5] While survey wave 2009 contains a direct question on Juntos participation during the past 12 months, I have to reconstruct this for the second wave. This is possible because wave 3 contains enrolment date and information on transfer suspension and programme exit.

[6] According to UNICEF, an average of 41 percent of children aged 12-15 are in a school grade that does not correspond to their age, the figure being as high as 60 percent in rural areas (UNICEF, 2008).

In a second step, the analysis will focus on cognitive skills and learning outcomes. Young Lives administers a range of tests covering numerical and receptive vocabulary skills to the cohort child and selected siblings. For the purpose of this study, the Peabody Picture and Vocabulary Test (PPVT) and a math test will be used[7]. The PPVT is a widely used test that was originally developed in 1959 in English language but has later been adapted to Spanish for Latin America (PPVT-R, for detailed information see Cueto and León, 2012). It measures receptive vocabulary skills by presenting, in increasing order of difficulty, pictures to the child who has to choose the word that best matches them. The measures correspond to the highest item reached out of a total of 125 items for the Spanish version, hence younger children tend to score lower on average by design. The test, which is untimed and norm-referenced, has been adapted to Quechua as the most widely used indigenous language in Peru by a panel of experts. The math test slightly differed between survey wave 2006/07 and 2009 because of the age differences and the need to increase difficulty. In 2006/07, the younger cohort (aged 4-5 years) was administered a 15-item-test of basic numeric concepts[8] while the older cohort (aged 11-12 years) completed a more difficult 10-item subset of the Trends in International Mathematics and Science Study (TIMMS) of 2003, testing basic numerical operations. In 2009, both cohorts took a test comprised of a 20-item arithmetic operations section and a second section testing quantitative and number notions (9 items for the younger cohort) respectively algebra and geometry (10 items for the older cohort)[9]. The tests were timed and no aids such as calculators or books were allowed.

The siblings did not participate in the math test (by survey design), while they did take the PPVT in the third round as long as they were at least 4 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 no school tests, but

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[7]Young Lives administers several other tests, however, these were either not continued through both survey wave 2 and 3, or they were changed such that they are not comparable over time.

[8]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 4-year olds. For more information, see Cueto et. al, 2009.

[9]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.

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 A1 (Annex) summarizes the outcomes that will be analyzed, while Table 4 reports descriptive statistics for these outcomes for the post-treatment round of 2009. It shows that a high proportion of children in both groups is enrolled in school, while more than half are still in primary school, and about a third of children from non-beneficiary households compared to about one fourth from beneficiary households attend secondary school. The mean child has completed fourth grade. On average, this seems to be in line with their age. In terms of scores on the PPVT and math tests, beneficiary children tend to score significantly lower than their peers from non-Juntos families. These figures refer to the whole sample of children before matching and include families from urban areas including the province of Lima as well as more remote rural areas.

Table 4: Outcomes (child-level) by treatment status in 2009

	Non-Juntos children		**Juntos children**		**Difference**	
	Mean	N	Mean	N	Points	t-stat
Enrolled	0.93	4074	0.95	1095	-0.01	-1.27
Highest grade	4.43	4074	4.17	1095	0.26	2.32
Age-for-grade	-0.55	4074	-0.27	1095	-0.28	-7.99
Primary complete	0.40	4074	0.34	1095	0.06	3.68
In secondary	0.29	4074	0.23	1095	0.05	3.59
PPVT raw score	72.39	2102	50.08	442	22.31	19.03
Math raw score	14.74	2069	10.33	420	4.42	14.92

4 Identification strategy

The impact that Juntos participation has 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. The fundamental problem of any evaluation is that we cannot observe an individual in both states of participation and non-participation. This paper 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 of the conventional difference-in-difference 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, it matches “statistical twins”. 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:

$$E(Y_{0i}|T = 1, X_i) = E(Y_{0i}|T = 0, X_i) \tag{1}$$

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 me 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:

$$E\,[(u_{1i}\,-\,u_{0i})\,|\,T=1,\,X_i]\;=\;E\,[(u_{1i}\,-\,u_{0i})\,|\,T=0,\,X_i] \tag{2}$$

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:

$$0\;<Pr\;(T_{1i}\,|\,X_i)\;<1 \tag{3}$$

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:

$$ATT^{MDID}=\sum_i\{(y_{1i}-y_{0i})-\sum_j\left(y_{1j}-\,y_{0j}\right)\,w_{ij}\} \tag{4}$$

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 and 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[10]. Based on this score, which was calculated according to a 2005 census (renewed in 2007), 638 districts were prioritised 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 5 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 (total per capita expenditure lies on average more than 50 percent below that of non-Juntos families) and in terms of wealth[11]. They are far more likely to live in rural areas where reaching the nearest primary school takes on average 7 more minutes. Perhaps more striking is the fact that the mother in the household has completed on average less than half the years of schooling compared to those in non-Juntos households (less than 4 years as compared to over 8 years). Juntos families tend to live in districts that were ranked in the poorest two quintiles (in terms of poverty

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[10]For further details on the algorithm applied and an extensive discussion of the targeting process, see Escobal et al. (2012).

[11] The wealth index is a composite score that measures by equal weighting: (i) the housing quality in terms of size and building materials, (ii) possession of consumer durables, (iii) access to services of water, sanitation and electricity.

incidence) as of 2005 with a prevalence of malnutrition among children aged 6-9 years of a staggering 45 percent compared to just under 20 percent in non-Juntos districts in this sample. It is evident that beneficiary households systematically differ from non-beneficiary households. In the first step, I will hence apply matching to find a suitable control group by replicating the programme's targeting criteria as closely as possible with the data.

Table 5: Household characteristics in 2006/07 by treatment status

	Non-Juntos HH		**Juntos HH**		**Difference**	
	Mean	N	Mean	N	Points	t-stat
Household size	5.36	2103	6.18	320	-0.83	-6.80
Wealth index	0.53	2103	0.26	320	0.27	21.32
Total expenditure	179.45	2103	83.46	320	96.00	9.60
Ethnic: Mestizo	0.91	2103	0.97	320	-0.07	-4.03
Ethnic: White	0.06	2103	0.02	320	0.04	2.69
Mother's education (in years)	8.56	2103	3.54	320	5.02	20.14
Mother's age	33.80	2103	34.23	320	-0.43	-0.84
Rural (1 if yes)	0.19	2103	0.78	320	-0.60	-25.26
Minutes to school	12.94	2103	20.31	320	-7.36	-9.67
District poverty quintile*	2.82	2103	1.29	320	1.52	22.60
District rate of child malnutrition in %*	19.64	2103	45.72	320	-26.08	-34.44

* 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.

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 pro-actively approaches eligible households to explain and offer affiliation with Juntos[12]. Hence, the risk that eligible households are unaware of the programme and thus do not register is low. The sequential regional roll-out may reduce incentives for

[12] This was the case in the first programme years (Escobal et al, 2012). Nowadays, households are not necessarily informed individually, but lists of eligible households are posted in the municipality.

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. Furthermore, various channels exist for families to complain and demand a re-assessment of eligibility.

Even if we thus believe that the programme rules successfully target the poorest, there may be systematic unobserved differences if for example some parents value education more than others or place more trust in the local health services. Hence, in order to control for any unobserved pre-existing differences between the control and treatment groups, I will apply difference-in-difference estimation on the matched sample. Applied to the present case of 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 5, Juntos households differ from non-Juntos households in observable characteristics that may simultaneously affect the outcome variables. Hence, I apply a Kernel-matching estimator by applying an Epanechnikov Kernel with a bandwidth of 0.05[13] (respectively 0.06 and 0.07 for different subsamples, see below) to restrict my 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 (depending on the distance of the propensity score) of

[13] 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 $h = 1.06\,\frac{A}{n^{1/5}}$, $A = \min(\sqrt{Var(x)}, \frac{IQR(x)}{1.34})$ 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 o 0.04 and 0.06 have not yielded materially different results.

(nearly) all control observations[14] 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 et al, 2008). Since the treatment itself can affect matching covariates, matching is best undertaken on the basis of pre-treatment characteristics (Blundell and Dias, 2009). I 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 my sample were non-beneficiaries in the observation year 2006/07, while 16 percent benefitted from Juntos in the observation year 2009.

Table 6: 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
Child's sex (girl=1)	20.89	13.21	0.991	18.16	16.48	0.477
Indigenous language (=1)	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
Total per cap. 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-headed HH	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 site (=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

[14] Depending on whether an Epanechnikov or Gaussian function is applied, the estimation uses all control observations (Gaussian) or just those within a specified caliper of the distance to the treated propensity score. For further details, see Heckman et al (1997).

My sample includes children of both age cohorts and their siblings if these were at least six years old in 2009 and lived in the household in both survey rounds. In choosing the matching covariates, I try to replicate the actual targeting criteria outlined above as closely as possible. In a first step, I exclude all households from the department of Lima (spanning both the capital and surrounding provinces) since this densely populated area may not serve as a good control group for treated rural districts. Since the range of the geographical score is still fairly large, I include the score itself in the matching covariates to ensure balancing between the two groups. As further geographical controls I include the distance to the next primary and secondary schools and whether the child lives 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 6 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. In fact, among the treated group, more than 50 percent of mothers have only two years or less of formal education while in the control group this figure lies at only 14 percent. This unbalanced distribution is a source of concern since we would expect the educational status of the mother to affect that of her children. Since this relationship is, however, a positive one, it would likely introduce a downward bias in the estimation.

Graph A1 (Annex) shows the propensity score before and after matching, as well as the region of common support. It confirms that both groups share a rather large area of common support, although 6 out of the 816 children from the treated group have to be dropped because they lie outside of this region. Further, Graph A2 shows that the distribution of key pre-treatment characteristics, which may plausibly be related to the outcomes measured, can be balanced through the matching specification. The matched sample now includes 6.260 observations, of which 1.620 belong to the treatment group (2.320 respectively 810 children per round). They cover the age range of 6 to 18 years and have a mean age of 10.8 years in the post-treatment round of 2009. Graph A3 (Annex) shows the post-treatment age distribution with two peaks around approximately 8 years (younger cohort) and 14 years (older cohort), and fewer observations (siblings) in between. A large share of the sample is hence still of primary school age (up to grade 6). Graph A4 shows the grade distribution by enrolment status: there is a corresponding peak of children, which have finished first grade, while there is no clear peak at later grades. It further shows that the majority of children is registered in school, while the highest risk of dropout seems to be around 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 developed 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.

Graphs A5 and A6 (Annex) depict the trends in enrolment and progression through grades from 2002 to 2006/07, the years just before the families in our sample began to benefit from Juntos. The sample used here includes the same children as long as these were between the age of 6 and 18 in 2006/07 (it hence excludes the younger cohort children altogether). Graph A4 shows that, while mean enrolment rates slightly differ between the two groups, the trend over time runs parallel. In a similar way, trends in progression through grades do not differ significantly between the two groups. Table A2 (Annex) reports the difference-in-difference estimation for the same time period and confirms that trends in outcomes between the two groups do not statistically differ from each other. Unfortunately, the PPVT and math tests were not yet administered in the first Young Lives survey wave of 2002 so that the pre-treatment trend cannot be observed. It seems plausible though to argue that if trends in school participation ran parallel for the two groups, the same holds for learning progress.

5 Results

Having balanced the two groups in terms of observable characteristics before treatment, I apply difference-in difference estimation in a second step. In our estimation, 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 percent 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.

5.1 Impacts upon school participation

Table 7 reports the results for the first set of outcomes. The parameter of interest is DiD: it captures the change in outcome levels over time between children of beneficiary and non-beneficiary families.[15] 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. [16]

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 percent). This is different for secondary schooling where mean school participation is significantly lower (83 percent) and differences run both along a rural-urban divide and between income groups (see Table 1, MINEDU 2014). 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 of 12 years. 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

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[15] An additional control related to the interview date are included (but not reported in the table) 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.

[16] The results are robust to clustering standard errors at the household level instead, bootstrapping standard errors or leaving out clusters altogether.

in primary school. The same holds for trends in years of schooling and conformity with the regular age-for-grade.

Table 7: Juntos impacts upon schooling outcomes (MDID)

Outcomes	(1) **Enrolled**	(2) **Highest grade**	(3) **Age-for-grade**	(4) **Complete primary**	(5) **In secondary**
Panel A: Pooled Sample					
Baseline					
Diff (T-C)	-0.013	-0.298	0.181**	-0.030*	-0.028**
	(0.019)	(0.191)	(0.077)	(0.016)	(0.012)
Follow-up					
Diff (T-C)	0.038**	-0.234	0.015	-0.040	-0.011
	(0.018)	(0.232)	(0.097)	(0.030)	(0.034)
DiD	**0.051****	**0.064**	**-0.1666*****	**-0.010**	**0.017**
	(0.020)	(0.068)	(0.054)	(0.023)	(0.029)
Observations	6260	6260	6260	6260	6260
R-squared	0.15	0.14	0.04	0.09	0.07
Panel B: Age group primary school (under 12 years)					
Baseline					
Diff (T-C)	-0.010	0.012	-0.028		
	(0.043)	(0.064)	(0.054)		
Follow-up					
Diff (T-C)	0.012*	-0.022	-0.052		
	(0.007)	(0.134)	(0.086)		
DiD	**0.022**	**-0.010**	**-0.024**		
	(0.041)	(0.094)	(0.069)		
Observations	3346	3346	3346		
R-squared	0.42	0.35	0.21		
Panel C: Age group secondary school (12-18 years)					
Baseline					
Diff (T-C)	-0.006	-0.539**	0.496***	-0.079**	-0.089***
	(0.011)	(0.205)	(0.126)	(0.027)	(0.024)
Follow-up					
Diff (T-C)	0.067**	-0.217	0.107	-0.009	0.004
	(0.032)	(0.227)	(0.151)	(0.047)	(0.068)
DiD	**0.073****	**0.322*****	**-0.389*****	**0.070****	**0.093***
	(0.034)	(0.065)	(0.094)	(0.031)	(0.053)
Observations	1956	1956	1956	1956	1956
R-squared	0.04	0.38	0.02	0.34	0.22

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 6.

The next panel C performs the same analysis for the older age group of 12 years or above. This group contains 1.956 observations out of which 646 belong to the treated group. Here, the positive impact upon enrolment rates[17] is significant at the 5 percent 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 4 months more schooling over time than non-treated children. This is consistent with the positive impact upon enrolment that indicates a lower dropout rate among Juntos children. It may further 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 percent level and larger than that on enrolment. Hence, the impact may be a cumulative effect of less dropout after primary school and faster progression, be that a result of the minimum attendance requirement of 85 percent, better performance or other driving forces.

In a nutshell, Table 7 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 dropout, and that the transition from primary to secondary school is a critical point. If we look at simple 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

[17] 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 are only few observations).

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 percent. 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 8 looks at learning outcomes as measured by the PPVT and math 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.[18] Since the tests were administered to siblings in the post-treatment round only while the Young Lives cohort children were tested in both rounds, I need to 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[19] is included.

Column 1 and 2 report the results for the PPVT and math tests of the younger cohort children. In both cases, the coefficients are negative but only in case of the math 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. Graphs A7 and A8 show the trend in PPVT and math scores over time: it becomes apparent that both groups have improved their

[18] The PPVT test has been standardized using a z-score standardization while for the math tests, a quintile range standardization was applied. The standardization was applied in age strata of 9 months.
[19] 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.

scores over time while beneficiary children have done so by fewer points than their counterparts. In the younger cohort, treated children increased their math test score by on average 1.5 points (approximately ½ standard deviation) less than non-treated children did.

Table 8: Juntos impacts upon learning outcomes (MDID)

Outcomes	Younger cohort		Older cohort	
	PPVT	**Math**	**PPVT**	**Math**
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)
DiD	**-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

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 6 and the child's age in months, siblings rank and whether s/he attended pre-school.

A further note of caution applies to the measurement of the younger cohort's math score since the baseline CDA in the pre-treatment round only tests basic numerical concepts and hence may not be a good predictor of later math abilities. If we look at simple differences only, however, 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 math scores are statistically significant for the younger cohort, while for the older cohort only the difference in PPVT scores is weakly significant.[20] The stronger effect in the

[20] The table reports estimates based on standardized test scores and shows, estimates based on raw scores yielded the same results.

first different 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 margin if households comply with conditionalities, and if the incentive provided lowers the opportunity costs of schooling significantly for at least some families[21]. 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 hence 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 I do control for regional characteristics related to poverty levels and distance to schools, unfortunately I 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, I have 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 I cannot yet observe long-term trends.

6 Conclusion

This paper 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 paper has adopted

[21] 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 enroll that would comply with conditionalities even in the absence of the transfer, the behavioural change may be zero.

a combined matching and difference-in-difference approach to analyze whether Juntos can be associated with higher levels of schooling reached and improved learning outcomes. It has focused on a sample of over 2.300 children aged between 6 and 18 years in the period under analysis, which were first surveyed in 2006/07 (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 outset. A positive impact is observed for children of secondary school age: treated children have 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 Mexico, where CCTs significantly decreased the risk of dropout 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 I 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 math test scores of the older cohort children, and even a negative effect on math scores of the younger cohort.

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 paper 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.

In a similar fashion, this paper has not addressed heterogeneous effects that may differ between different family types, ethnic background or risk groups. As such, 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). Evidence from qualitative interviews[22] furthermore suggests that transaction costs related to the fulfilment of conditionalities (in particular waiting times at health centres) and cash withdrawal may differ substantially between sparsely populated rural areas and more densely populated ones. From a policy perspective, this analysis has also not evaluated the benefits of a conditional versus an unconditional transfer. 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). 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. In this sense, 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 or investing in school infrastructure.

In summary, this paper has offered some support to earlier findings from different countries that attest CCTs a positive impact upon school participation of secondary school aged children that may be at risk of school dropout 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 at further analysis needed to investigate potential links between CCTs and skills formation.

[22] Qualitiative interviews related to programme participation and effects have been conducted with beneficiary families, school directors and local Juntos administrators in 4 districts in 2 departments between December 2015 and January 2016.

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Annex – Tables

Table A1: Outcome variables

Outcome	Variable	Sample
Schooling outcomes		
Enrolled	Child currently attends school (yes/no)	Full sample (Young Lives cohort children and siblings)
Grade	Highest grade completed (in years)	
Age-for-grade	Age deviation from regular age for grade attended (benchmark: 6-7 years in first grade)	
Primary	Child has completed primary school (yes/no)	
In secondary	Child has entered secondary school (yes/no)	
Learning outcomes		
PPVT	Standardized z-score (age-stratified) of the PPVT raw test score	Young Lives cohort children
Math	Standardized quintile range (age-stratified) of the raw CDA (in case of younger cohort 2006/07 round) and math score	

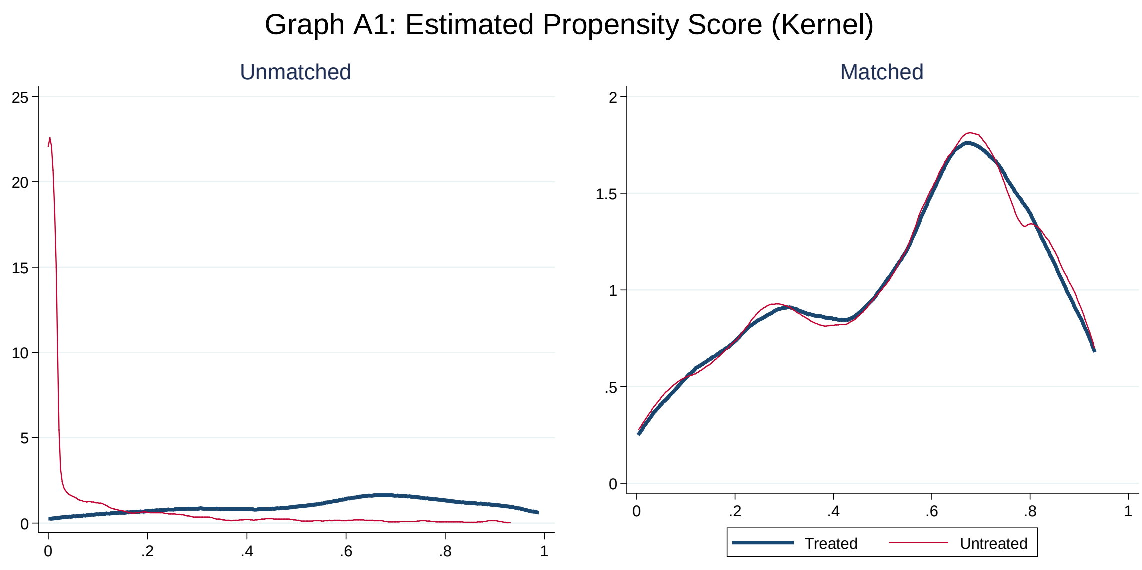
Table A2: Diff-in-diff estimation on pre-treatment trends

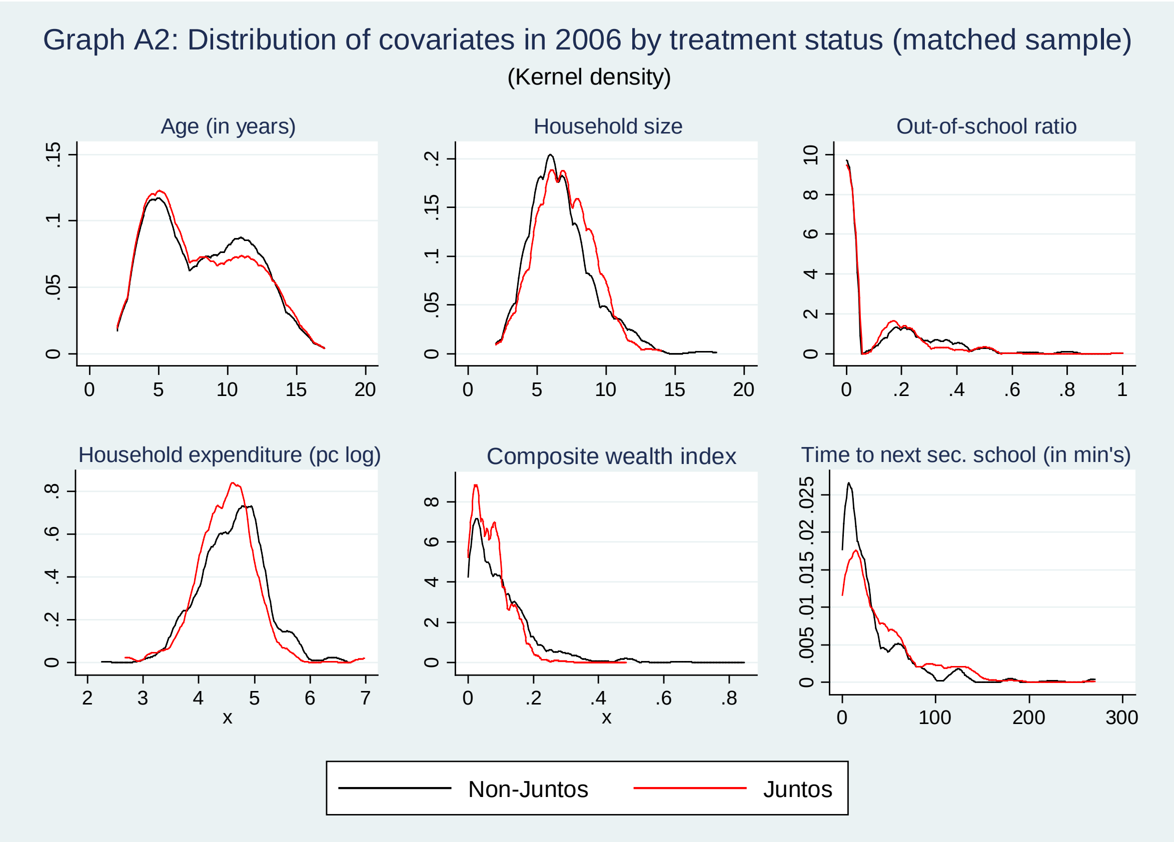
	Enrolled	**Highest grade**
post*T	**-0.000148**	**-0.122**
	(0.0430)	(0.200)
T	0.00808	0.140
	(0.0412)	(0.0884)
post	0.403***	2.286***
	(0.0347)	(0.157)
Constant	0.567***	0.686***
	(0.0333)	(0.0654)
Observations	2,952	2,940
R-squared	0.233	0.287

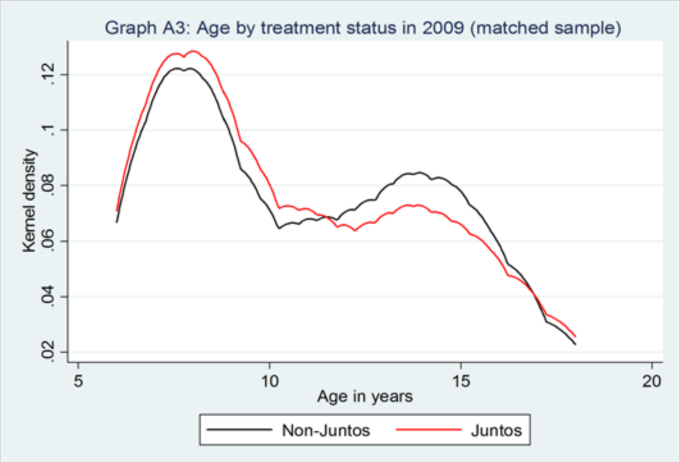
Robust standard errors in parentheses, clustered by HH

*** p<0.01, ** p<0.05, * p<0.1

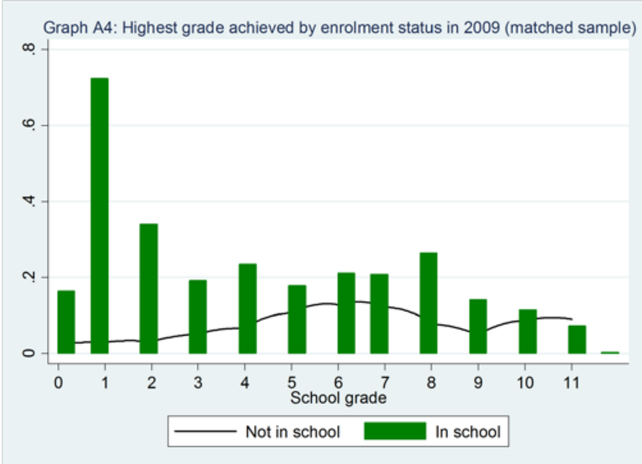
Annex - Graphs



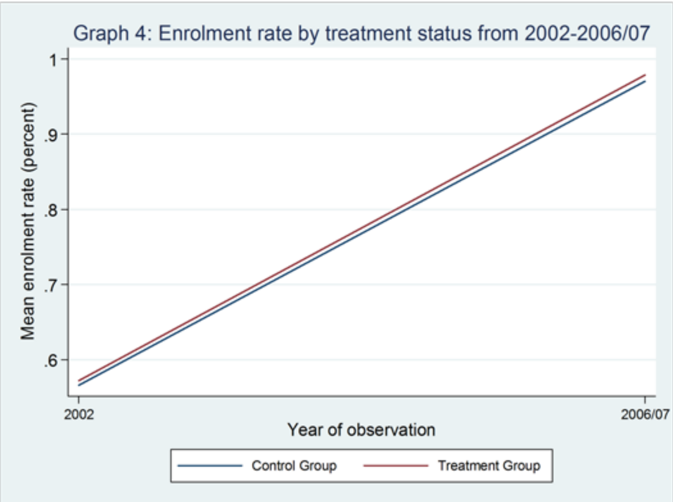




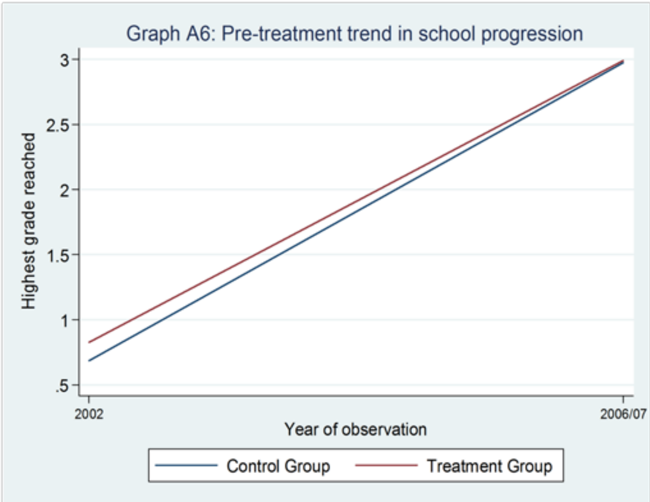
Graph A3: Age by treatment status in 2009 (matched sample)



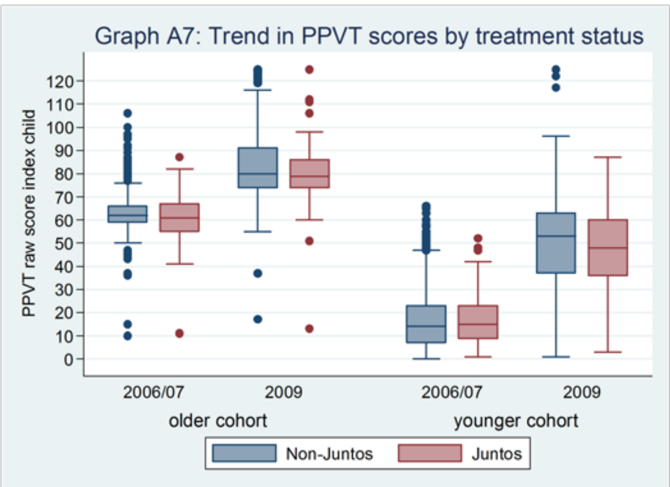
Graph A4: Highest grade achieved by enrolment status in 2009 (matched sample)



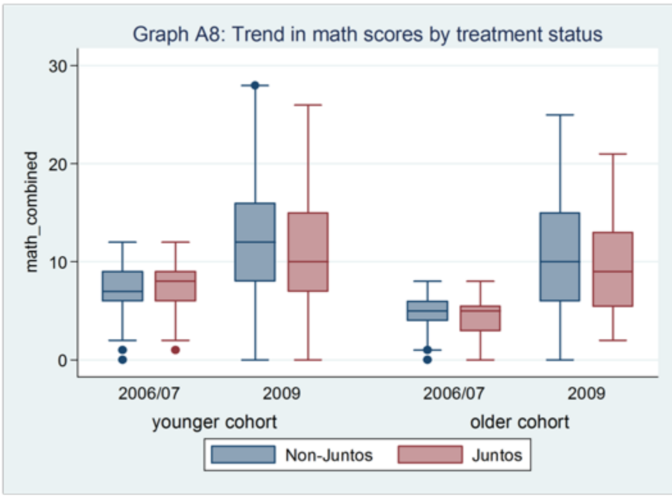
Graph 4: Enrolment rate by treatment status from 2002-2006/07



Graph A6: Pre-treatment trend in school progression



Graph A7: Trend in PPVT scores by treatment status



Graph A8: Trend in math scores by treatment status

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