YOUNG LIVES STUDENT PAPER

An Analysis of the Juntos Cash Transfer Programme in Peru, with Special Emphasis on Child Outcomes

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The data used come from Young Lives, a longitudinal study of childhood poverty that is tracking the lives of 12,000 children in Ethiopia, India (Andhra Pradesh), Peru and Vietnam over a 15-year period. **www.younglives.org.uk**

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I acknowledge that this is my own work, and I have read and understood Section 4 on plagiarism in the Red Book.

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The data used in this thesis comes from Young Lives, a 15 year survey investigating the changing nature of childhood poverty in Ethiopia, India (Andhra Pradesh), Peru, and Vietnam (www.younglives.org.uk). Young Lives is core-funded by UK aid from the Department for International Development (DfID). The views expressed here are those of the author. They are not necessarily those of Young Lives, the University of Oxford, DfID, or other funders.

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All errors are my own.

Abstract

This paper studies the impact of a large-scale conditional cash-transfer programme (Juntos) in Peru. Given that the programme was implemented in 2005, there are only a few existing evaluations of the programme. Using the Young Lives dataset, we analyse the targeting efficiency of the programme, as well as its impact across a range of indicators reflecting child well-being. We find that the targeting mechanism seems to be working as intended, with poorer households getting preference. Also, the programme seems to have a significant impact on reducing the incidence of child labour. Although we do not find evidence in favour of programme participation increasing school enrolment by itself, this is most likely due to a large number of children from the participating households already being enrolled in schools before the intervention. Finally, we do not find a significant impact of the programme on child health indicators such as bmi-for-age and height-for-age z-scores. We believe this is most likely due to the age of the children in the available dataset. We might find different results if the same analysis is conducted on a set of younger children; a dataset that is unfortunately currently not available.

Contents

List of Tables

1. Introduction

In recent years, individual and community level Conditional Cash Transfers (henceforth, CCTs) have been utilized as a form of social assistance across various countries in the world. Social Protection, as defined by Harvey (2005) and DfID (2005), encompasses a subset of public actions that address risk, vulnerability, and chronic poverty. Conditional Cash Transfers are usually understood as cash grants, cash for work, and voucher programmes, and distinct from others forms of social assistance such as microfinance, loan waivers, and budget support schemes. These transfers seek to disburse a set amount of money to poor households contingent upon certain actions, such as ensuring that the children in the household are sent to school, the adults contribute their labour to public works projects, regular visits to health centres etc.

The importance of CCTs lies in their acting not only as income support programmes, but also as a factor in promoting long-term human development. They act as a demand-side complement to the supply of schooling and healthcare facilities. Evidence from various countries across Latin America and the rest of the world shows that CCTs can contribute significantly towards increasing school enrolment rates for children, improving uptake of preventive healthcare, and raising household consumption. However, CCTs have also been criticized for poor targeting, creating disincentive effects towards work, and limited long-term welfare impacts. The most important challenge that policymakers face lies in identifying successful practises, and appropriately modifying these practises before adopting them in a new context.

CCTs have been a part of the development agenda in Latin America from the mid-1990s onwards. Following in the same tradition, the government of Peru commenced its conditional cash transfer programme, Juntos, in 2005. By 2009, the programme was operational in 638 districts, covering nearly 454,000 households. While Juntos has largely been modelled on the successful[1] *Oportunidades* programme in Mexico, the introduction of Juntos by the Peruvian government received mixed reactions. This was in part due to the debates being politically motivated, instead of focusing on the programme outcomes and its merits. Also, the initial programme design of Juntos did not include a systematic impact evaluation; as a result, quantitative information on programme participants, and its impacts on a range of indicators has not been extensively studied. In recent years, however, there has been a range of data collected by various organisations, and some studies are now available.

[1] Various studies exist that demonstrate the success of Oportunidades; for instance, see Holmes and Slater (2007), and Winters and Davis (2009).

The existing research is by no means definitive, mostly due to lack of large-scale data looking at programme participants. This paper aims at complementing the existing research using a newly made available dataset from the Young Lives organisation (more details about the data follow in a subsequent section). The objective of this research is to try and answer two specific questions about the Juntos programme; namely, targeting efficiency of the programme, and its impact on child outcomes. We try and evaluate the characteristics of people who are selected into the programme, and look at whether the programme selects those most in need. Secondly, we try and evaluate the impact of Juntos on children in participating households across a range of indicators.

Given that CCTs are intended to reach a certain underprivileged section of the population, any evaluation of a social-security programme must start by analysing whether the programme benefits are reaching the intended population. Even the most well-intentioned schemes might result in imperfect coverage of the poor, and leakage to the non-poor. As argued in Timothy and Kanbur (1990), the 'ideal solution' is for the transfers to go *only* to the poor; however most large-scale transfer programmes fall short of this ideal due to three aspects of the real world- administrative and informational costs, individual responses and incentive effects, and political economy of the problem. Also, Lanjouw and Ravallion (1998) outline past cases of issues like elite capture and corruption in such transfers. In light of this, we start our analysis by outlining some key characteristics of households that receive support from the Juntos programme.

In trying to analyse the impact of Juntos on children in participating households, we look specifically at health outcomes and also at child labour. Although there are studies that show significant improvement in health outcomes as a result of various transfer programmes, the evidence is far from undoubtable. For instance, Morris SS et al (2004), among others, find no effect on height-for-age and weight-for-age indicators, and even a negative impact for children under 7 years old for participating households in Brazil. Similarly, models laid out by Brown et al (1994) demonstrate the same possibility theoretically, albeit in case of cash-for-work programmes. Given that the programme design of Juntos focuses heavily on children and on long-term human development as the eventual goal, our research tries to analyse whether there are any palpable difference induced by programme participation.

The data used in this paper is the Young Lives data set, an extensive longitudinal study across four countries, including Peru. It offers a baseline and also a post-intervention survey with a rich range of household and child level details. The data was collected in 2002, 2006, and 2009. Given that Juntos was implemented in 2005, we use the data from the 2009 round to study the characteristics of those who self-select into the programme and for impacts on child labour, while also utilising the data from previous rounds in order to study the impact on health indicators.

In section 2, we outline the basic characteristics of the Juntos programme, and also provide the background in which the programme was designed and implemented. In section 3, we provide the relevant literature review, drawing from a wide range of papers that study specific impacts of cash transfers, and also from other disciplines that study political economy of programme implementation. Section 4 explains the available data in detail, and provides some descriptive statistics that can be used to understand some key factors for the programme. Section 5 sets out the econometric issues we need to tackle, while sections 6, 7, and 8 respectively study the characteristics of participating households, impact on child labour, and impact on health indicators. This analysis is followed by a short discussion on our findings and the conclusion in section 9.

2. Programme Background and Description

Background

Juntos ('Together' in Spanish) is Peru's flagship social protection programme. Following the lead from successful interventions in neighbouring Brazil, Chile, Mexico, and Honduras, the Peruvian government launched Juntos in 2005. Given the country's severe wealth inequalities, a need for greater public investment to provide social security to the underprivileged was widely recognised. However, given that national elections were scheduled for April 2006, this was seen as a politically motivated move by the incumbent president Alejandro Toledo, and faced opposition from various stakeholders. It was argued that targeting mechanism of the government was flawed, as the idea was to initiate the programme in urban and rural areas simultaneously, without a clear-cut mechanism for targeting the poor (however, the targeting mechanism was improved subsequently). It was also argued that the prudent thing for the government would be to restructure the existing social security programmes in Peru, rather than introduce a new programme.

To address these concerns, and to ensure political neutrality of the programme, Juntos was placed under an independent directorate named by the National Roundtable for Poverty Alleviation. Since then, the programme has garnered widespread praise and recognition[2].

Aims

The broad aims of Juntos, like other social security programmes, can be divided into two main categories:

[2] For an extended discussion on programme background, see 'Cash transfers to tackle childhood poverty and vulnerability: an analysis of Peru's Juntos Programme' by Jones, Vargas, and Villar (2008).

- Short-term objectives: Targeting household-level poverty by providing households with cash transfers.

- Long-term objectives: Promote human capital and break the vicious cycle of intergenerational poverty by improving school enrolment and access to health services (while the former aims at increasing school attendance, decreasing drop-outs and incidence of child labour, the latter aims at decreasing chronic child malnutrition, and infant and mother mortality).

Eligibility

Given that the programme design was initially criticized for having an ineffective targeting mechanism, and since the conditionality in Juntos is not work-dependent, self-selection of *only* the intended beneficiaries (the poor) into the programme was not guaranteed by design. The mechanism was subsequently improved to ensure effective targeting, and the selection of beneficiary households was done in three stages:

- Selection of eligible districts: Participating districts were selected on the basis of five criteria- exposure to violence, poverty level, poverty gap, level of child malnutrition, and presence of extreme income poverty.

- Selection of eligible households within the eligible districts: The national level statistical organisation, *Instituto de Estadistica e Informatica* (INEI), conducted a census of all households in the districts outlined during stage 1. Following this, household eligibility was determined on the basis of poverty, and from this subset, only households with children under 14 years or pregnant women were selected.

- Community validation exercise: Following stages 1 and 2, inputs were taken from community members, local authorities and representatives of the Ministry of Education and Health with the aim of minimising both inclusion and exclusion errors[3].

Payment and Conditions

After the three stage process that determined eligibility, suitable households were informed about the conditionalities that accompanied uptake of the cash transfer by the household. The programme provides eligible and participating households with a

[3] 'Welfare impacts of the "Juntos" Program in Peru: Evidence from a non-experimental evaluation' by Perova and Vakis (2009) contains an excellent description of the targeting mechanism of Juntos.

monthly cash transfer of 100 *soles* (approximately USD 30), which is a lump-sum payment and is uniform across households (i.e., does not depend on any household characteristics, such as the number of children, occupation etc.)

In order to receive this monthly transfer, the households have to conform to certain conditions. These conditions are not uniform across households, but rather depended on the age of children in the household. These are detailed as follows[4]:

- Households with pregnant and breastfeeding mothers: Mothers are expected to attend both pre- and post-natal care. The focus is to provide the mothers with appropriate vaccinations such as tetanus, anti-parasitic checks, and also provide them health supplements such as folic acid and iron.

- Households with children under 5 years of age: To prevent malnutrition, the children are required to attend regular nutrition and health controls that would provide them a complete series of vaccinations, iron and Vitamin A supplements, and anti-parasitic checks. These nutrition and health controls also involved periodic monitoring of height and weight of the children.

- Households with children between the ages of 6 and 14 years, but with incomplete primary schooling: Children from these households are required to attend school at least 85% of the academic calendar. Given that children belonging to this age-group were most likely to get caught in child labour due to poor household circumstances, the idea was to provide the households with enough monetary inflow so as to enable the children to attend school instead of being involved in paid-for work.

In addition to these age-specific conditionalities, women from participating households are also required to attend awareness-raising programmes on methods to aid the physical and social development of their children. The aim here is to ensure proper dissemination of information about other social security initiatives by the government, which, in addition to Juntos, could also help achieve the goals outlined in the programme description.

Even though the targeting mechanism is fairly detailed, issues like network effects leading to elite capture are not ruled out by design. Similarly, there are no disincentives - such as work requirements- that make it unappealing for non-eligible households to try and gain access to the programme (this is, to some extent, countered by stage 3 of the selection process which is a community validation exercise). Thus, we try and analyse

[4] See Perova and Vakis (2009)

the characteristics of participating households as a component of this paper, and try and determine whether the targeting has been effective.

3. Literature Review

Targeting efficiency

The first thing we evaluate is targeting efficiency of the programme, and characteristics of the households who are selected. As Nichols and Zeckhauser (1982) point out through a tax-incidence model, eligibility requirements that restrict the behaviour of intended beneficiaries usually promote target efficiency. These restrictions can be in the form of income, components of the consumption bundle, or time allocation. They further argue that while providing benefits to intended participants, these transfers should also, by design, impose substantial costs on imposters to deter them from posing as deserving beneficiaries. The Juntos programme satisfies the first condition; we saw that the programme requirements restrict the behaviour of intended beneficiaries by imposing constraints on what the participating households do with a fraction of their time. However, there are no penalties on imposters. Thus, if the targeting mechanism is imperfect, the non-poor are not, in any way, deterred from posing as intended beneficiaries in case they wish to.

Recent studies (World Bank 2008b) demonstrate that the Juntos programme has overestimated the level to which participating households comply with the attached conditions. This translates into Juntos tending, to some extent, towards an *unconditional* cash transfer, and thus reducing its intended welfare impact. The non-compliance with conditionalities arises because the duty of monitoring the participating households is usually intertwined with the responsibility of programme operation. This creates perverse incentives for local Juntos officials to overestimate the level of conditionalities that are actually met. One obvious way to get around this difficulty would be to segregate the operational and monitoring duties, but this would require an increase in administrative costs and might not be politically feasible.

An interesting study by Tercelli argues that since people at greatest risk, such as single mothers, orphans, and street children are not intended beneficiaries of the Juntos programme, the design by itself leads to targeting errors and potentially can only lead to minimal gains nationally across a range of indicators.

Lanjouw and Ravallion (1998) have demonstrated that social and political processes influence who gets access to large-scale transfer programmes. This might translate into better-connected households benefiting at the cost of more-deserving households. A

more recent study by Caeyers and Dercon (2008) also reveals this, paving way for the conclusion that expansion of these schemes would often more than proportionately benefit the poor.

Child Labour

The next issue that we analyse in this paper is that of impact of the programme on incidence of child labour. As argued by Lieten (2006), child labour is an issue widespread across the developing world, and has the potential to cause intergenerational transmission of poverty by ensuring that a child is caught in a situation where lack of education from an early age causes future poverty, thus increasing the probability that future generations will also be caught in the same vicious cycle.

As Amartya Sen (1999) points out, children from poor families find themselves at a disadvantage from the time they are born. Such children often tend to be in poor health, have lower education levels, and thus enter adulthood in a situation where they find themselves unable to break into more 'lucrative' sectors of the labour market that might give them a chance of enjoying an acceptable quality of life.
Poverty reduction programmes that aim at supplementing household income can thus go a long way if they can find a way to ensure more schooling and better health for children from the participating households.

The Peruvian constitution mandates the legal minimum age for child workers to be 12 years; making it one of the youngest level across the world, and specifically in Latin America. Unofficial estimates put the number of child labourers at around 2 million. Child labourers in rural areas are usually employed in agriculture, and as vendors in urban areas. A U.S. Department of State estimate from 2006 puts the numbers of sexually exploited children at more than 500,000, while an estimate from the International Occupational Safety and Health Information Centre (2004) shows that a similar number is employed as workers in gold mines. In face of this, large-scale social security programmes such as Juntos could have a huge role to play in addressing the situation.

However, the evidence in favour of cash transfer mechanisms leading to a reduction in the incidence of child labour is unclear. Ravallion and Wodon (2000) argue on theoretical grounds that a cash transfer leading to a reduction in the price of schooling might not result in reduction of child labour. The extra time spent at school might come out of the child's leisure time. Their empirical study of a targeted enrolment subsidy in Bangladesh reasserts this, where a small cash transfer was enough to ensure nearly full school attendance. However, no substantial reduction in incidence of child labour was discovered. At least in that instance, the parents were substituting other uses of the

child's time towards labour, so as to supplement the household income while also gaining access to the cash transfers from the directed subsidy.

The issue of impact of cash transfers on child labour has been widely studied for various programmes across the world; however, the research is sporadic in case of Juntos. The only major study outlining effects on child labour (among a host of other things) is by Perova and Vakis (2009). They demonstrate that children from Juntos households are more likely to have 'worked in the last week'. However, this result should be interpreted cautiously as the survey questionnaire simply enquired whether the child had worked during the last week, without making any distinctions between paid labour and household chores. Skoufias and Parker (2001), however, show a decrease in the number of hours worked as a result of the household being given assistance under the PROGRESA programme in Mexico. Even still, they do not argue that programme participation leads to a complete withdrawal from work.

In light of lack of substantial research on the impact of Juntos on child labour, this paper fills an important gap in the existing literature. We not only study the impact of Juntos on child labour, but also try and estimate whether or not the programme has any effect on school enrolment. To further strengthen the analysis, we study the impact of the programme on a combined indicator of schooling and labour.

Health Outcomes

One of the aims of most cash-transfer programmes around the world is to ensure human development, and access to a better quality of life for the households involved. Thus, studying the impact of cash-transfers on health outcomes of the involved households is an imperative task, and there is a wide range of literature covering the same.
Given that most of the cash-transfer programmes across Latin America (including Juntos) are *not* cash-for-work type programmes, we do not need to be worried about possible negative health effects of programme participation as parents substitute their time towards work and away from the child. Also, given the heavy focus on promoting better awareness about health and regular check-ups in programme conditionality, one would expect a positive impact of programme participation on a range of health indicators.

In a recent study by Fernald et al (2008), it was found that a doubling of cash-transfers in Mexico's *Oportunidades* programme was associated with reduction in stunting of children from the participating households, increase in height-for-age anthropometric z-scores, and a lower prevalence of being overweight. Improvements were also established in participating children's cognitive and motor development. Thus, this study showed that *Oportunidades* cash-transfers were associated with better outcomes in child health and development. Given that Juntos is based on *Oportunidades* and retains many of its working features, conditions, and operational techniques, we have

reason to be optimistic about the possible favourable impact of Juntos on child health outcomes.

Another programme modelled on *Oportunidades* is Nicaragua's *Red de Proteccion Social* (RPS). In an evaluation of RPS by Maluccio and Flores (2005), they find that households supported by RPS tended to spend most of their additional income on food. Also, there was an average net increase of 16 percentage points in the participation of children aged under 3 in the heath care programme VPCD. This combination of a more varied diet and use of healthcare programmes led to a marked improvement in the status of beneficiary children aged under 5; the number of stunted children declined by 5.5 percentage points.

In the only relevant study done analysing the impacts of Juntos, Perova and Vakis (2009) find that Juntos increases the utilization of health services for both children and women, and it improves nutritional intake of program households.

Notwithstanding the large number of studies that have been conducted on the possible impacts of cash-transfer programmes on health of children, there remains a gap in the existing literature in that the assessment of health outcomes seems limited only to infants and very young children. However, as Bailey and Ferro-Luzzi (1995) point out, adult and adolescent malnutrition is much more widespread than is commonly recognized, and deserves our attention. Given that children in our sample are 14-15 years of age during round 3 of data collection, our analysis focuses on health indicators appropriate for adolescents, rather than for very young children. While attempting this, we need to keep in mind that appropriate health indicators utilised for young children vary considerably from those used for adolescents**.**

4. Data and Descriptive Statistics:

Data

The data used in this paper comes from The Chidren of the Millenium Study, internationally known as Young Lives. Peru is one of the countries that the study is conducted in, and it follows the development of two groups of children- the first group (younger cohort) consists of about 2,000 children who were between 6 and 17 months old in 2002, the year the study was initiated and Peru and first data collection conducted. The second group (older cohort) consists of about 750 children who were between 7 and 8 years old in 2002. For both cohorts, the study collects a vast range of indicative data on children, the households they grow up in, and key members of their immediate communities. Since the first round of data collection in 2002, two more subsequent rounds have been conducted in 2006 and 2009.

The data from the first two rounds is currently in the public domain, while the data from round 3 (2009) is confidential. The Young Lives organisation has graciously agreed to

provide the author of this paper access to data for the older cohort (children between the ages of 7 and 8 in 2002, and thus aged 14-15 years in 2009) for round 3, and the empirical analysis in this paper focuses on the same dataset. Of the 750 children interviewed in 2002, 714 could still be tracked and interviewed in 2006, and 678 in 2009. This implies a low rate of attrition. A recent study by Outes et al (2008) shows that although the attrition in the Young Lives sample is non-random to some extent, it is unlikely to generate a noticeable attrition bias when used in empirical models.

In all three rounds, the survey design involved a broad household questionnaire that aimed at studying various characteristics of the households that Young Lives children grow up in. Data regarding Juntos is available in round 3, and a number of enquiries are made regarding the same. Some of the more interesting questions relate to programme registration, amount received (if enrolled), reasons for discontinuing enrolment (if applicable), distance of Juntos centre from household, expenses incurred while travelling to the Juntos centre, and whether the payment has been delayed on occasion. Other questions include the spending mix of participating households, that tells us how much of the Juntos money parents spend on food, clothes, education, health and business, and a scale question that asks households to rate the Juntos programme.

Although the data was not collected solely for the purpose of studying the Juntos programme, it has several strengths for our desired research questions. Given that Juntos was implemented in 2005, and we have data from 2002, 2006, and 2009, we can use the data to study programme impacts, duration effects, and try and answer 'before-after' and 'with-without' type questions. The extensive breadth of the data allows us to make adequate controls in estimation, and to our knowledge, no comparable dataset exists in the public domain that can be used to answer our precise research questions. Also, given that Juntos aims directly at improving child welfare, this dataset that provides wide-ranging information regarding the educational, social, health, and other such characteristics of the children can be used to assess how far the programme is being successful in its stated goals.

Representativeness of the Data

Given that the Young Lives data studies only a small cross-section of the population of Peru, we need to be cautious while interpreting the results from our study and generalising them to the whole population. Sample selection bias may be an issue if households with 12 year old children in 2006 differed systematically from those that did not. This could be a potential issue, as the parents of children of a particular age would most likely belong to a comparable age-range themselves, which is likely to affect labour decisions and economic situation, and thereby eligibility for selection into Juntos.

Thus, although we do not recommend that our results be generalised to the whole population, these results still remain valid for the general range of ages that we observe for households in this dataset, and especially for the cohort that we study.

Descriptive Statistics

From our sample of 678 children, we compute the following descriptive statistics:

Table 01: Descriptive Statistics

Household characteristics

	Total Sample	**Non-Juntos beneficiaries**	**Juntos beneficiaries**
Household size (avg.)	5.37	5.29	6.05
Wealth index (avg.)	0.58	0.61	0.38
Rural residence	23.5%	18.27%	64.47%
Principal household language Spanish	93.9%	98.33%	59.21%
Household head's education (avg.)	7.17 years	7.48 years	4.63 years
Major income generated via secondary activities	32.7%	34.38%	19.74%
Total observations	678	602	76

Child Labour

	Total Sample	**Non-Juntos beneficiaries**	**Juntos beneficiaries**
Male children	53.4%	54.15%	46.67%
Child labour	40.4%	37.40%	64%
Total observations	678	602	76

Health Outcomes

	Total Sample	**Non-Juntos beneficiaries**	**Juntos beneficiaries**
∆BMI for age (avg.)	-0.039	-0.069	0.200
∆Height for age (avg.)	0.060	0.012	0.442
Total observations	678	602	76

The statistics above can be used to see some of the features of the programme (however, we need to be wary of reading too much into these statistics without some rigorous analysis, which is done in the subsequent sections). We see that across most factors, the targeting of the scheme seems to be working in the intended direction. Some of the more interesting factors seem to be average of the wealth index, which is substantially lower for Juntos beneficiaries. Also, more than 60% of Juntos beneficiaries in our subsample reside in rural areas, as against about 18% of non-Juntos beneficiaries from the same subsample. In face of lack of data on ethnicity, we use the main language spoken in the household as an indicator of the same (all the households in our sample speak Spanish, Quechua, or a mixture of both. Given that Quechua is the primary language of the indigenous people, who are usually less well-off than their Spanish speaking counterparts, we have reason to believe that primary language spoken in the household might be a good proxy for socio-economic status). We see that a staggering 98.33% of the non-beneficiaries speak Spanish as the main language, while only 59% of the beneficiaries do. A comparatively larger fraction of Juntos beneficiary households also seem to rely on primary activities as their major source of income. Thus, at first glance, the targeting mechanism of the scheme seems to be working in the intended

direction, and the beneficiaries seem to be worse-off as compared to non-beneficiaries across a range of indicators.

We see that there is a wide gap in the incidence of child labour between the two groups and, we also find some evidence of programme participation having a positive impact on bmi-for-age and health-for-age z-scores in participating households.

5. Econometric Issues

Self-selection

Given that being a Juntos-beneficiary household is a far from random occurrence, we need to account for this potential source of bias. The targeted nature of the rollout of the scheme sets up a stage for potential selection bias (as mentioned previously, the scheme follows a three-stage selection process, eliminating 'non-eligible' portions of the population).

Due to data restrictions and the programme design, we cannot use an instrument for participation. This is because the programme was initiated and made available on a totally non-random basis, and any variable correlated with programme participation is also likely to be correlated with other variables that we're interested in. Heckman's correction, a two-step statistical process, offers another means of correcting for non-randomly selected samples. However, to get convincing estimates, we generally need a credible exclusion restriction. This restriction is that there must be at least one variable that does not appear in the equation of interest, but appears with a non-zero coefficient in the first-stage selection equation. This essentially amounts to instrumenting, and given that we've already discussed the infeasibility of instruments for our purposes, Heckman's correction is ruled out. Employing the method of GMM is also not an option, given that we intend to work with two-period panel data.

Thus, to get credible estimates, we need to control sufficiently for observable characteristics and cluster effects. This should not pose a problem for our analysis, as the dataset used holds a wide array of information not only about the children, but also about their households and communities.

Unobserved effects and Endogeneity

The issue of unobserved effects is particularly important while analysing health outcomes, as these are studied as a two-period panel (we analyse *change* in various health indicators from 2006 to 2009)[5]. To control for unobserved fixed effects, we employ a first-differencing strategy and estimate a difference-in-differences equation. Given that we work with a two period panel, this is operationally equivalent to controlling for individual and household level fixed effects. Thus, by this strategy, we

[5] Although the programme was implemented in 2005, we still use 2006 as the baseline against which we compare health outcomes. This is because in 2005, the programme was available to a very small number of households, and also because any possible health benefits are unlikely to have been realised in a short time-frame of one year.

remove the bias potentially resulting from time-invariant effects. We expect the health-related estimates that we derive to be unbiased and consistent, as long as any time-varying unobservable variables are uncorrelated to the independent variables used as regressors.

Also, while studying health impacts of the programme, if we leave out the lagged health status for the relevant variable (bmi-for-age, weight-for-age etc.), we implicitly assume that this lagged variable has a zero-coefficient. This, in turn, implies that the child's health status is completely independent from his past health, and starts afresh every period. As shown in studies like Kirchberger (2008), this is not true, and leads to dynamic misspecification. However, using the lagged health status variable (say, bmi in last round as a regressor for bmi in this round) is also not an option, as it leads to the problem of endogeneity. Thus, we use the method of instrumental variable instead. We instrument the lagged variable by the caregiver's perception of birth size that should exhibit strong first-stage relationship with our relevant health indicators. Also, this satisfies the exclusion restriction, as we can argue that anthropometric scores when the child is 14-15 years old are affected by birth size only *through* the initial health status.

Heterogeneity in Impacts

There is a possibility that the programme affects different household differently, depending on idiosyncratic household characteristics (heterogeneity in impacts). As an example, a household affected by crop failure or the death of household head might be affected differently from programme participation as compared to a household that has not been affected by these events. To control for this potential source of bias, we use various interaction terms in our analysis. However, these are found to be insignificant, and consequently dropped from the estimates. We can safely believe that heterogeneity in impacts is not a source of bias; this is most likely due to the extensive targeting strategy of the programme.

6. Analysis of participating households (Targeting Efficiency):

Framework and Econometric Specification:

To study the characteristics of households that determine participation in Juntos, we estimate a conditional correlation equation. The dependent variable is the probability of participation, estimated as a function of household characteristics (Household size, parental education, lagged wealth index, area of residence, language spoken), shock variables (drought, crop failure), network variable (number of influential relatives), and community-level controls.

Table 02: Description of variables

Variable	***Description***
Junreg	Dummy variable for programme registration
Hhsize	Size of the household
Wilag	Wealth index from last round of data collection.
Parented	Level of education of the household head
Rural	Dummy for residence area (1 if rural)
Headlang	Dummy for main language of the household (1 if Spanish)
Drought	Dummy for whether the household has been affected by a drought since the last round of data collection
Cropfail	Dummy for whether the household has been affected by crop failure since the last round of data collection
Infrel	Scale variable for number of influential relatives

Apart from these, to allow for within-cluster variation, we also use community-level dummies such as responses based on whether the local police does its work well, whether most people in this community are basically honest, whether the respondents believe that the government does what is right for them, whether people in this community can affect decisions that matter the most etc. We intend to use these community-level controls only to refine our analysis; we are not interested per-se in the coefficients or marginal effects of these variables, and hence coefficients on these controls are not reported to conserve space.

Thus, the equation being estimated is:

$$Junreg = \alpha + \beta_1 Hhchr + \beta_2 Shock + \beta_3 infrel + \beta_4 commcontrol + \mu$$

Results and Discussion

Estimating the model set out above, we get some interesting results. To develop our intuition, we first run a Linear Probability Model. The results are in the expected direction, but for the effect of area of residence (we get a negative coefficient on the rural dummy, implying that belonging to a rural area *reduces* the probability of being enrolled in the programme). Otherwise, bigger households, having influential relatives, having suffered from drought and/or crop failure have positive effects; while lagged wealth index, main household language being Spanish, and parental education have negative effects. However, given the well-known criticisms of the LPM, we present the more appropriate Probit model here, and the direction of results mirrors those obtained via the LPM:

Table 03: Probit on Programme Participation (coefficients and marginal effects)

Variable	Coefficient	Marginal Effect
Wilag	-0.297***	-0.118
	(0.073)	
Headlang	-0.177***	-0.071
	(0.028)	
Parenteduc	-0.018	-0.008
	(0.028)	
Infrel	0.397***	0.158
	(0.152)	
Rural	-0.168	-0.067
	(0.235)	
Drought	0.481*	0.192
	(0.263)	
Cropfail	0.575**	0.229
	(0.296)	
Hhsize	0.054	0.022
	(0.050)	
Observations	**678**	
LR statistic	**273.94**	

Note: ***p<0.01, **p<0.05, *p<0.10. Standard errors are provided in parentheses. Relevant base categories: non-Spanish speakers, urban dwellers, not drought affected, not affected by crop failure. Coefficients on the constant and community level controls not reported due to space constraints.

We notice that the coefficient on lagged wealth index from last round is highly significant and has a negative coefficient. This implies that the targeting mechanism of the scheme correctly selects households that were less wealthy during the last round of data collection. As discussed before, Spanish speakers tend to be, on an average, better-off as compared to Quechua speakers, and we see that Spanish-speaking households have a 7.1% less chance of being selected as against Quechua speakers. Similarly, an additional year of schooling for the household head reduces the chance of being enrolled by 0.8%; this can be justified as households where heads are better educated are more likely to be better off than others.

A variable that deserves attention is *infrel*, a scale variable denoting the number of influential relatives that the household has (the scale is as follows: no influential relatives: 0, 1-5 influential relatives: 1, 5-10 relatives: 2, and so on). This is a network variable, and is strongly significant and positive. We find that having five additional influential relatives increases the probability of being a beneficiary by 15.8%. This can be interpreted in two contrasting ways: this can be seen as evidence in favour of elite capture of the programme, wherein the process is biased towards the more powerful people in the community. Alternatively, we might ascribe this positive coefficient to greater awareness arising from better network connections in the community.

Another noteworthy set of variables is the two shock variables. Households that have suffered from a drought since the last round of data collection have a 19.2% greater chance of registration, while those that have been affected by crop failure have a 22.9% greater chance. Both of these variables are strongly significant, again reflecting that the targeting mechanism does seem to be reaching out to the sections of the population that are most in need.

Apart from these variables we also tried including other relevant variables. Surprisingly, some variables that one would expect to be significant turned out to be highly insignificant; these included expenses incurred while travelling to the Juntos centre, and the distance of the same. Similarly, the role of local credit institutions does not seem to be important when it comes to programme registration, as a variable that enquires 'if you approached a formal source of credit, what do you think would be the maximum amount they would lend you for 6 months?' turns out to be highly insignificant, and so does another variable that represents the number of acquaintances a household can approach when in need of a loan.

Thus, we see that programme registration does not seem to depend on proximity of the Juntos centres to the households' residence, and neither on the availability of formal or informal credit.

7. **Child Labour**

Framework and Econometric Specification:

As discussed in the literature review, the impact of cash-transfers on the incidence of child labour is not clear-cut. There are opposing forces at play, which might be summarised as follows[6]:

We assume that children's time-use is determined solely by their parents, who decide how much schooling, goods, and leisure the child consumes. The optimal consumption vector is arrived at after a constrained maximisation exercise. We assume that in making the decision of allocating their children's time use, the parents' utility is a function of the child's school attendance (S), leisure (H), and household's current consumption (C):

$$U = u(C, S, H; Z) \qquad (1)$$

Z above represents a vector of exogenous factors, such as household characteristics and geographic variables. The function u is assumed to be strictly quasiconcave in the 3 arguments we're interested in: C, S, and H. Given this utility function, parents aim to maximise their utility subject to the following two constraints:

- Child's time constraint: The total time available to the child (T) must be divided between schooling (S), leisure (H), and wage labour (L)[7]:

[6]This discussion closely follows Ravallion and Wodon (2009)

$$S + H + L = T \qquad (2)$$

- Budget Constraint: Total household consumption (C) depends on household income obtained via the earnings of the child (wL), income through other sources (Y, assumed exogenous, and determined by the factors included in Z):

$$C = wL + b + Y(Z) \qquad (3)$$

(w is the wage rate offered for child labour, b is a constant that represents the household's assets)

Thus, the household aims to maximise its utility (U), subject to the two constraints. If we now assume that Y (household income from sources other than child's earnings) increases due to an exogenous shock (cash transfer), we see that the programme impact could work towards increasing *or* decreasing child labour. This is because although Y rises unambiguously, one of the conditions of the transfer is that the children regularly attend school. This means that from the time-constraint, S definitely goes up (or remains the same). In case S goes up, the decline in child's other activities might as well come from H (the leisure time), and not necessarily from L (labour time). Thus, to answer whether participation in Juntos reduces child labour, we need to analyse the programme impact empirically.

Following the empirical strategy that we adopted while studying the characteristics of the participating households, we estimate three Probit models:

The first specification is with the dependent binary variable 'Child Labour' that takes the value 1 if the child works for wages outside the household[8]. Most of the variables used have been described in the previous section; apart from those, we also use a dummy for gender ('male'), a variable that captures parents' expectations of their children by incorporating a variable that specifies the level of education that the parents would ideally like their child to achieve ('gradelike'), and a dummy for whether the child is enrolled in school. The model being estimated is:

$$childlab = \alpha + \beta_1 junreg + \beta_2 hh + \beta_3 shock + \beta_4 gradelike + \beta_5 school + \mu$$

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[7] For this analysis, we assume that any household work, sleep etc. are included in leisure time (H), and only work for wage outside the household is counted as wage labour (L).

[8] Another possible dependent variable was a dummy that equals one if the child works outside the household for wages, and/or *within* the household on domestic chores. For the purpose of this research we focus only on work-for-wages outside the household.

The second specification studies school enrolment, and whether Juntos has any impact on the same. The dependent variable in this case is 'school', taking the value 1 if the child reports being enrolled in a school. The model is:

$$school = \alpha + \beta_1 junreg + \beta_2 hh + \beta_3 shock + \beta_4 gradelike + \mu$$

Finally, the third specification tends to look at whether programme participation leads to children substituting their time from labour and into school; for this, we construct a dummy dependent variable 'labsch' that assumes the value 1 if the child reports working for wages outside the household, and *also* being enrolled in a school:

$$labsch = \alpha + \beta_1 junreg + \beta_2 hh + \beta_3 shock + \beta_4 gradelike + \mu$$

Results and Discussion

Table 04: Probits on child labour, school enrolment, and combined child labour and school dummy

Dependent variable →	**Child labour**		**School**		**Child labour and school**	
Variable ↓	**Coefficient**	**Marginal Effect**	**Coefficient**	**Marginal Effect**	**Coefficient**	**Marginal Effect**
Male	0.231*	0.091	0.166	0.066	0.283**	0.110
	(0.138)		(0.151)		(0.141)	
Junreg	-0.906**	-0.355	0.341	0.135	-0.782**	-0.310
	(0.368)		(0.320)		(0.365)	
Wilag	-0.595*	-0.236	0.421	0.167	-0.672*	-0.266
	(0.341)		(0.390)		(0.348)	
Parented	-0.028*	-0.011	-0.012	-0.005	-0.027*	-0.011
	(0.015)		(0.018)		(0.015)	
Infrel	-0.127	-0.049	-0.147	-0.0587	-0.322	-0.128
	(0.164)		(0.146)		(0.201)	
Gradelike	-0.088***	-0.034	-0.027**	-0.011	-0.064*	-0.025
	(0.033)		(0.011)		(0.034)	
School	-0.334^	-0.133				
	(0.235)					
Headlang	0.058	0.001	-0.004	-0.001	0.058	0.021
	(0.046)		(0.037)		(0.046)	
Observations	**678**					
LR statistic	**37.28**		**13.41**		**28.51**	

Note: ***p<0.01, **p<0.05, *p<0.10, ^p<0.15. Standard errors are provided in parentheses. Relevant base categories: females, non- Juntos beneficiaries, not enrolled in school, non-Spanish speakers. Coefficients on the constant term and household size not reported due to space constraints.

Child Labour

The empirical results are noteworthy. Although we are theoretically unable to justify that a cash-transfer scheme such as Juntos will definitely lead to reduction in incidence of child labour, the empirical results do tend to suggest the same. We notice that children from Juntos beneficiary households are 35.5% less likely to be working for wages outside the household.

Another interesting variable to note is the gender dummy. We see that males have a 9.1% higher chance of being engaged in labour, as compared to females. The other variables of note have expected signs, with parental education, wealth index from the last round of data collection, influential relatives, and parental ambitions of making the child study all contributing towards reducing the probability that the child is engaged in labour.

Interestingly, we note that unlike while studying access to the programme, shock variables such as drought and crop failure turn out to be highly insignificant, and are consequently dropped from the estimated model. This might be interpreted as child labour being a structural problem, reflecting chronic rather than incidental poverty induced by one-off occurrences. Also, formal and informal sources of credit still do not seem to play any part in determining the incidence of child labour.

One variable that deserves attention is 'gradelike', that holds information about the level of education that parents would ideally like their child to achieve. The variable is negative and strongly significant. With an additional year of desired schooling by the parents, the child is 3.4% less likely to be a labourer. Thus, if the households can be educated and the importance of schooling made obvious, more households might substitute schooling in favour of child labour. In this regard programmes such as Juntos can go a long way, as they not only supplement the household income but do so *conditional* on the child attending school.

School enrolment and substitution away from labour

We notice that programme participation dummy seems to have a statistically *insignificant* effect on school enrolment, although the direction of the effect is still positive. Thus, we cannot attribute any significant increase in school enrolment to programme participation. However, the next set of results that deal with children who report both being enrolled in school *and* being involved in labour are quite interesting. We find that programme participation reduces the chance of a child being enrolled while also a labourer by 31%, and the result is statistically significant. The signs of other variables of note, such as infrel and gradelike, are also significant and in the expected direction and can be interpreted in a way similar to above. Thus, from the

above analysis, we establish that although Juntos does not seem to promote school enrolment by itself, we have evidence to believe that it leads to a reduction in incidence of child labour, and also in substitution of time away from labour and into school.

8. Health outcomes:

Framework and Econometric Specification

As set out in papers by Senauer and Garcia (1991), Behrman and Hoddinott (2005), and Singh (2008), we assume household welfare to be positively related to children's health status. We visualise health being determined through a health production function of the form:

$$\boldsymbol{H_i = H\ (F_i, C_i, D_i, G_i, U_i)}$$

Where *i* is the subscript for any child *i*, H is the health as determined by various indicators (body mass index (bmi), anthropometric z-scores), C and D are vectors respectively of the child's observable characteristics in the previous period and those of the parents, G is a similar vector of household characteristics, and U is a vector of the unobservable characteristics such as genetics, parental preferences, environmental factors etc. that are likely to affect child's health. We assume that the money given to households under the Juntos programme is partially spent on households' food consumption, thereby also resulting in a net increase in the child's food intake. Then, the change in child's health, ΔH, depends on changes in food intake, in household characteristics (such as economic shocks), and the vector of child attributes. Thus:

$$\boldsymbol{\Delta H_i = f\ (\Delta F_i, \Delta G_i, C_i)}$$

The vector C_i allows us to control for heterogeneous programme impacts based on the child's characteristics. We model the evolution of health as a dynamic process, where the child's health in the current period is influenced by his/her health in the previous periods. Thus, we include lagged health status of the child in the vector C_i.

Following this, we model health status as under:

$$Y = \alpha + \beta_{1.} junreg + \beta_2 X_i.t + \beta_3.Z_{it} + \gamma.t + \mu_i + \varepsilon_{it}$$

Where Y is the relevant anthropometric score[9], X is a vector of relevant time-invariant attributes, Z is a vector of child specific time-varying attributes such as shocks, t is time dummy assuming the value 1 for Round 3 of data collection (base period is Round 2) and μ is the unobserved fixed effect. We begin by studying this static specification, and later move onto the dynamic specification by adding the lagged health status as a dependent variable.

To get rid of the unobserved fixed effects and to account for trend effects of time-invariant characteristics, we difference the equation above and get:

$$\Delta Y = \beta_{1.} \Delta junreg + \beta_2 X_i + \beta_{3.} \Delta Z_{it} + \gamma + \Delta\, \varepsilon_{it}$$

Given that we're working with a two-period panel, $\Delta junreg$ is simply the programme registration indicator. This specification allows for time-varying heterogeneity, by incorporating time-trends for some of the regressors. This heterogeneity might exist as the effect of various factors on health might vary by the age of the child (the ages of children, although close together, and not completely uniform).

The more credible dynamic specification is then obtained by including the lagged health status as a regressor. This is likely to result in endogeneity, and we use the method of instrumental variables to counter the same[10]. The dynamic specification is:

$$\Delta Y = \beta_{1.} \Delta junreg + \beta_2 X_i + \beta_{3.} \Delta Z_{it} + \beta_{4.} Y_{t-1} + \gamma + \Delta\, \varepsilon_{it}$$

[9] Following Bailey and Ferro-Luzzi (1995), we use bmi as an indicator of health in adolescents. However, most of the literature on nutritional impacts prefers to focus on z-scores; thus, we also run one more specification- with dependent variable as the height-for-weight z-score.

[10] To deal with endogeneity that may be caused by a lagged dependent variable, we instrument the lagged variable with caregiver's perception of birth size. As explained in section 5, this instrument satisfies the exclusion restriction, as we can argue that anthropometric scores when the child is 14-15 years old is related to the said instrument only *through* the initial round 2 health status.

Results and Discussion

Variable↓	OLS	IV	OLS	IV
Junreg	0.108	0.084	0.329***	0.288*
	(0.111)	(0.127)	(0.108)	(0.169)
Drought	0.192	0.191	-0.209	-0.147
	(0.143)	(0.138)	(0.139)	(0.246)
Crop failure	-0.224	-0.210	0.198	0.093
	(0.144)	(0.145)	(0.140)	(0.384)
Male	-0.348***	-0.347***	0.291***	0.277***
	(0.145)	(0.053)	(0.054)	(0.067)
Wilag	-0.331**	-0.275	-0.649***	-0.357
	(0.145)	(0.213)	(0.142)	(1.02)
Headlang	-0.009	-0.008	0.001	0.005
	(0.012)	(0.012)	(0.012)	(0.019)
Parented	-0.002	-0.004	0.011*	0.010*
	(0.006)	(0.007)	(0.006)	(0.006)
Hh size	0.003	0.001	0.008	0.005
	(0.014)	(0.015)	(0.014)	(0.018)
Mother's weight	-6.53E-05	-1.52E-07	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Lagged bmi-for-age z score		-0.068		
		(0.194)		
Lagged height-for-age z-score				-0.148
				(0.513)
Observations	678			
R-squared	0.08	0.14	0.11	0.30
F-Stat	6.92	6.68	9.38	10.73
Dependent Variable →	**BMI-for-age z-score**		**Height-for-age z-score**	

Note: ***p<0.01, **p<0.05, *p<0.10. Standard errors are provided in parentheses. Coefficients on the constants not reported.

From the above results, we see that both in the static OLS and dynamic IV specifications, we do not get convincing results either on the bmi-for-age z-score, or the height-for-age z-score. In case of bmi-for-age, programme registration turns out highly insignificant, although in the expected direction. In case of height-for-age z-score, programme registration *does* turn out to be statistically significant and in the expected direction; however, most of the supporting regressors (apart from the gender dummy and wealth index in the last round) turn out to be statistically insignificant. Thus, although we see that programme registration seems to have a positive sign, we do not have enough evidence to be able to conclude that Juntos has a significant impact on health of the participating children.

To assess whether the results are not noteworthy due to our instrumenting strategy, we try and use another set of instruments for the lagged health status. In another specification, we instrument the lagged health status by shocks that the children suffered from shortly *before* the second round of data collection. We analyse various shocks, such as drought, frost/ hailstorm, crop failure, large increase in input prices for agricultural families and so on. The most important shock affecting households in our dataset from round 2 seems to be frost/ hailstorm, with close to 9% of the sample reporting being affected by the same. Thus, we instrument the lagged health status using incidence of frost/ hailstorm. This is likely to satisfy the exclusion restriction as well, given that the current health of the child is likely to be affected by these shocks only *through* the round 2 health status, which the shocks directly affect. However, even after this new instrument, the results we obtain do not provide strong evidence in favour of the programme having any noticeable impacts on child health indicators that we assess.

An issue of note here is the age of the children in the dataset used. Given that we only have access to the older cohort, where children are 14-15 years of age, the impact of an intervention might not be obvious. As stated by Boersma and Wit (1997), while there is great scope for catch-up growth in younger children, in later childhood outcomes are far less vulnerable to shocks and correspondingly we should expect a decrease in the responsiveness of child health outcomes to interventions such as the one that we study in this paper. Thus, a better indicator of the programme impact on child health outcomes can be derived by studying the younger cohort in Peru. Unfortunately, the data for the younger cohort has not yet been made available in the public domain by the Young Lives organisation. An extension of this research is thus possible once the more appropriate dataset is made available in late September 2011.

9. Discussion and Conclusion

In the analysis above, we try and study the impacts Juntos has on a range of indicators. In this section, we attempt at setting out the implications of these results for the programme. From the results on targeting efficiency, we see that the targeting mechanism of the programme seems to be working fairly well, with poorer households getting preference in aid. Similarly, households that reported being affected by crop-failure and drought in the near past also seem to be preferred in the programme. However, we see that having influential relatives also seems to be a significant component in explaining programme participation, and this might be interpreted as some evidence of elite capture of the programme, with better-connected households being preferred. This is most likely a result of the last-leg of targeting programme, which is the community-validation exercise. However, we must be vary of recommending rooting out this validation exercise, as this also seems to be a way of ensuring that grassroot level organisations, and the communities themselves, are a part

of the selection process. Also, despite using a variety of variables and specifications, we cannot claim to have completely overcome the potential problem arising from omitted/ unobservable variables that might be correlated with the included regressors, and this might be a potential cause of bias.

From our results on child labour and schooling, we found that programme participation reduces the incidence of child labour, but does not seem to promote school enrolment by itself. However, while studying the children who reported *both* being enrolled in school as well as being involved in child labour, we noticed that programme participation had a negative effect. This reflects that supplementing the household income and imposing programme conditionalities that involve putting the children in school does work at some level in reducing the malaise of child labour. The low impact on enrolment might be due to the fact that children from most of the participating households already seem to have been attending school even *before* the programme was introduced (87.6% of the children in the sample reported being enrolled in school). Thus, we can conclude that households that might have been economically unable to pull their children out of work-for-wages outside the household are now more able to do so, and this should translate into children spending more time at school (an indicator that has not been analysed in this paper due to data constraints). The programme thus benefits the children by enabling them to focus on their education, rather than on earning.

The last question that we assess is whether the Juntos programme has any noticeable impacts on child health outcomes such as bmi-for-age z-scores and height-for-age z-scores. We do not find any significant impacts of programme participation on these set of indicators, and we establish the same by use of various different instruments for the lagged health status. However, we believe this might be due to the age of the children in our dataset, who might be well over the age group that can show fast catch-up growth in response to interventions such as the one that we study here. A more appropriate indicator of programme impact on child health outcomes can be arrived at by following a similar evaluation strategy on the younger cohort of children in Peru, once the dataset is made available.

To conclude, we note that Juntos seems to be a praiseworthy conditional cash-transfer programme. Following the lead of various similar interventions across Latin America, Juntos seems to be well on track to achieve its stated objectives, and we look forward to its planned expansion to include more households in Peru.

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