UNIVERSITY OF TRENTO - Italy

UNIVERSITÀ DEGLI STUDI FIRENZE

Doctoral School of Social Sciences
Doctoral programme in Development Economics and Local Systems
Curriculum in Development Economics

Conditional Cash Transfers, Risk-coping Strategies and Aspirations: Impact Evaluations from Peru

a dissertation submitted in partial fulfillment of the requirements for the doctoral degree (Ph.D.) in Development Economics and Local Systems

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2017/2018

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Acknowledgments

I am grateful to my supervisor, Professor Giorgia Giovannetti, for her constant and precious academic guidance and for the academic freedom I experienced with her during these three years. I would like to thank her for always pushing me to reach my potential and go further, and for supporting me in finding the field of studies which I am passionate about. Field of studies which shaped the path of my career allowing me to realize some of my dreams.

I would also like to thank my co-supervisor, Professor Bruno Martorano, for sharing his deep knowledge and experience about social protection and impact evaluations and for his insightful comments and ideas which improved the quality of my thesis.

Thanks to the two reviewers of my thesis, Professor Andy McKay and Professor Stephen Dietrich, for having provided me interesting comments.

I am grateful to Professors Gabriella Berloffa, Gianna Giannelli, Donato Romano, Giuseppe Folloni, and Paolo Brunori for their important inputs and suggestions provided during discussions and Ph.D. presentations. I thank people who provided me comments during the visiting period at UCL, in particular Professor Orazio Attanasio, Professor Marcos Vera-Hernández, Professor Arun Advani, and Federico Tagliati.

I would like to thank also my Ph.D. colleagues and friends (Luca Bortolotti, Filippo Santi, Giulio Galdi, Amir Maghssudipour, Niccolo' Pieri and Alessia Dorigoni), for the exchange of experiences and opinions which made us better researchers and thinkers. I also thank them because: we shaped together the kinds of researchers, but overall citizens, we would like to be (or not to be); we shared inputs (and STATA commands) for each other's papers; and we made these years intellectually stimulating.

Moreover, I would like to thank all my friends for having believed in the sense of what I was putting effort on and for being always close, even when distant.

I thank Giacomo, for having been supportive, optimistic, and ironic in every step of my thesis, for being enthusiastic about every small achievement and for reminding me the beauty of research (and, overall, life).

Finally, my endless gratitude goes to my parents and my sisters, to their strength and their positive attitude, to the infinite support they assured me during these three years (and during the other 27), and to the pride they felt and transmitted me about my Ph.D. thesis.

Contents

[1] *The data used in this thesis come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India, Peru and Vietnam (www.younglives.org.uk). Young Lives is funded by UK aid from the Department for International Development (DFID). The views expressed here are those of the author(s). They are not necessarily those of Young Lives, the University of Oxford, DFID or other funders.*

Introduction

Social protection targets poor and vulnerable individuals to reduce poverty and promote equality of opportunities and social inclusion. These policies and programmes are implemented by governments, with the possible financial and technical support of international organizations. Social protection programmes are gaining an increasing role in the governments' poverty reduction strategies and, over the past decade, an "impressive extension of social protection coverage" (ILO 2014) and a strong increase in the number of social protection programmes in place (Gentilini et al. 2014) have been observed. The increasing implementation of these kinds of interventions is due to the fact that social protection programmes have proven to be essential instruments for reducing hunger and income poverty and breaking the poverty trap (ODI 2011; Bastagli et al. 2016). According to a recent review by Bastagli et al. (2016) most of the programmes positively and significantly affect children school attendance, uptake of health services, dietary diversity, small livestock ownership while less clear-cut evidence was found about the impact on children learning outcomes, anthropometric outcomes, business and enterprises, agricultural and non-agricultural productive assets ownership, employment and migration. Additionally, several studies highlighted other unintended programmes effects. Despite the rapid expansion of these interventions, governments still underinvest in social protection and only 45% of the global population is covered by at least one programme (ILO 2017).

Social protection can be provided through contributory programmes (social insurance and social security schemes) or non-contributory programmes (social assistance and labour market programmes). The focus of this thesis is on a specific type of non-contributory social assistance programme. More specifically, we analyse Juntos, a conditional cash transfer programme implemented by the Government of Peru in 2005. Conditional cash transfers aim at acting on both short-term poverty, through the injection of liquidity within the household, and inter-generational long-term poverty, through conditionalities related to education and health. Juntos succeeded in reaching its main objectives reducing poverty, increasing the utilization of health services, improving nutritional intake and increasing attendance in primary

education (Perova and Vakis 2009). However, the unintended impacts of Juntos are still unexplored in the literature.

In the line of Deveurex and Sabathees Wheeler (2004) framework, conditional cash transfers may be seen as "protective measures" aimed at providing relief from poverty. However, the empirical evidence highlighted also their unintended "preventive" effect on deprivation through risk diversification, their "promotive" positive effects on economic opportunities and, more recently, their "transformative" role for socially marginalised groups. These effects of conditional cash transfers are at the core of this dissertation. The thesis is composed of three chapters where we explore, relying on impact evaluation techniques, whether Juntos may produce unexpected preventive, promotive and transformative effects, by acting on households' investments, migration and children aspirations.

The first chapter focuses on the impact of the Juntos programme on households' investments in productive assets and activities. To this regard, conditional cash transfers have not only a "promotive role" that allows households to improve real income (Deveurex and Sabathees Wheeler, 2004), but even a deeper "production role" which allows them to invest, by relaxing liquidity constraints and providing consumption and assets security (Barrientos 2012). Additionally, assets accumulation may represent an insurance for households in case of shocks, hence in this case the "promotive" and "preventive" effects of conditional cash transfer, on productive assets, overlap (Deveurex and Sabathees Wheeler 2004).

The second chapter analyses the impact of Juntos on domestic migration. Currently, migration is at the core of the international debate and represents a huge challenge for Peru and for the entire world[2]. Migration may have a "preventive" role for the household since it is a form of risk diversification. The chapter analyses whether the presence of a social protection intervention in the place of origin affects domestic migration from rural to urban areas.

The third chapter focuses on the "transformative" role of conditional cash transfer. In particular, we analyse whether Juntos affects children aspirations related to education and work, which are proved to be important predictors of actual

[2] In 2013 at the global level there were approximately 763 million of internal migrants (Bell and Charles-Edwards 2013) and 232 million of international migrants (UN 2013)

educational outcomes. This paper contributes to the recent and scarce literature about the psychological effect of social protection intervention and the role of aspirations in poor settings.

In the three chapters we implement three different impact evaluations. In absence of a randomize control trial experiment, to remove the selection-bias due to observable characteristics we rely on quasi-experimental techniques calculating the probability of the household to be selected by the programme. However, to assess the programme impact, in each chapter we rely on different impact evaluation techniques due to the nature and availability of data on the outcome variables[3]. In the first chapter we rely on the combination between Propensity Score and difference in difference technique. In the second chapter, due to the dynamic nature of the dependent variable and the way it was built, we rely on Propensity Score Matching and then we calculated the programme impact relying on Average Treatment effect on the Treated using different matching algorithms. In the third chapter, because of data constraints and analysis objectives, we use Inverse Probability Weighting (IPW) and Ordinary Least Squares models. The choice was motivated on different grounds: first, we do not have information about the outcome variable in the pre-programme period and we cannot perform a difference-in-difference analysis; second, we prefer IPW to traditional matching algorithms since the size of the sample with available information on the outcome variable was not as large as the ones of the other papers and because IPW shows several practical advantages in assessing the heterogenous effects and studying the role of covariates which was relevant in the case of this chapter's objectives.

There are three factors that motivated the research questions developed in this thesis. First, conditional cash transfer programmes have limited duration for beneficiaries and are not meant to cover individuals for the whole life cycle. Available data do not always allow to conduct studies on the long-term effect of social policies. Therefore, studying whether conditional cash transfers may have a "promotive" and "transformative" effect can shed a light on this issue and provide evidence of the effectiveness of government expenditure in the long run.

[3] To develop the empirical analysis, we rely on the longitudinal Young Lives data and for each chapter we focus on different database sub-sections concerning households, individuals and children.

Second, assets investments, migration and aspirations have strong consequences in terms of poverty and social changes. Indeed, both investments and migration, can be considered risk-coping strategies: assets accumulation may act as an insurance for future consumption in case of bad economic conditions (Zimmerman and Carter 2003), while migration generates income sources differentiation for the household. In developing countries, households are highly exposed to risk and vulnerability and often face liquidity or credit constraints. Given the lack of means to deal with these issues, households may put in place ex-ante and ex-post risk coping strategies (such as: savings, assets accumulation, migration and income sources differentiation). There is an increasing evidence about the fact that the lack of means for coping with risk and shocks may be itself a cause of persistent poverty (Dercon, 2006) and that some risk coping strategy, as livestock and productive assets sales or school drop-out, may be highly detrimental in the long-run (FAO 2013). Other strategies, as domestic and international migration, may imply huge social changes and have implications in terms of poverty reduction (see Imai et al. 2017). At the same time, a recent strand of the economic literature showed that individual aspirations are correlated with effort, investments and real outcome (Ray 2006; Serneels and Dercon, 2014; Chiapa et al. 2012) and that low individuals' aspirations about future opportunities may lead them to remain trapped in poverty, creating a vicious cycle (Ray 2006). Since the way risk-coping strategies and aspirations are formed may determine important consequences in terms of poverty, it is relevant to analyse to what extent conditional cash transfers may affect these dimensions.

Third, to the best of our knowledge, there are no studies about the impact of Juntos on productive assets and activities, on households' migration and on children aspirations.

This thesis shows that the Juntos programme is able, not only to deter domestic migration from rural to urban areas, but also to allow beneficiary households to invest in productive assets and activities. Additionally, the programme has also a psychological effect on beneficiary children who have higher aspirations about their future studies and work.

Social protection programmes are not substitute for economic growth and structural policies, but they can support poor households in accessing available services and

break the inter-generational transmission of poverty (Giovannetti et al. 2010). From a policy perspective, the positive promotive and transformative effects of conditional cash transfers highlight the importance to support households through these interventions and show that the initial government investments may be boosted by the household capacity to make the most of the money received and by psychosocial changes which allow beneficiaries to choose freely where to live and to aspire without constraints.

Chapter 1

Do Cash Transfers Trigger Investments?

Evidence from Peru

Abstract

This chapter provides an impact evaluation of the Juntos program on households' decisions to invest in livestock and agricultural and non-agricultural assets used for income generating activities. Using Propensity Score Matching and Difference-in-Difference techniques, we show: i) that beneficiaries are significantly more likely to invest in productive assets and activities with respect to non-beneficiaries; ii) that Juntos is more likely to relax liquidity constraints rather than to be used as an insurance for risky investments; iii) that the program benefits the poor but not the poorest of the poor. Duration and transfers regularity do not produce significant differences between groups of beneficiaries. However, results show a sustained impact of the program over time.

Keywords: Conditional Cash Transfers; Impact Evaluation; Households Investments; Peru; Juntos.

1. Introduction

In the past five years, the number of social assistance programmes in developing countries has almost doubled with every country having at least one social assistance programme in place (Gentilini et al. 2014). When implemented in a sound macroeconomic environment, social assistance programmes, and, in particular, cash transfers, are important instruments for reducing hunger and income poverty and breaking the poverty trap (Hagen-Zanker et al. 2011). In the past, social assistance programmes were seen as a mere emergency relief for dealing with climatic shocks, famines and conflicts; more recently their promotive and transformative role (Deveurex and Sabates-Wheeler 2004) and their long-term impact on productivity and living standards has been increasingly recognised.

The economic rationale for cash transfers was first identified by Fizbein et al. (2009): cash transfers (hereafter CTs) may reach the poor, reduce poverty and redistribute more with respect to other forms of untargeted public expenditure. Moreover, in case of imperfect credit markets, CTs may allow also credit-constrained households to efficiently allocate the capital within the economy. Finally, CTs, in case of income fluctuation and imperfect insurance markets, may support households in smoothing consumption. The main objective of unconditional CTs is to reduce poverty and vulnerability, while conditional cash transfers (hereafter CCTs) are designed to affect also health and education outcomes. However, beside the intended impacts, both conditional and unconditional CTs may produce several unintended effects such as, for instance, changes in households' investment decisions.

Barrientos (2012) shows that cash transfers may have a productive role for beneficiaries at the microeconomic and local level through three channels: relaxing credit and liquidity constraints; providing consumption and asset security; allowing household to optimally allocate resources.

Indeed, cash transfers increase the household income relaxing liquidity constraints and, when regular and predictable, support households in investing in risky but high-return activities. The idea is that beneficiaries use the transfer for immediate consumption and to pay the transaction costs the household incurred in to get the transfer. In the case of conditional cash transfer, a fraction of the transfer may be devoted also to activities required by the programme. Then, the remaining amount

(after immediate consumption, transaction costs, and activities related to the programme, if any) can be used for saving or as a collateral to borrow. CTs are not meant to cover the individual for the whole life cycle and often they have a limited duration. Hence, households may decide to invest the saved amount in productive assets and activities. The investment in productive assets is one of the channels through which beneficiaries can maintain the living standards reached thanks to the programme also after its termination. Even though assets accumulation may come at the expenses of current consumption, it represents an intertemporal defensive strategy for poor households since assets accumulation may act as an insurance for future consumption in case of bad economic conditions (Zimmerman and Carter 2003).

We investigate whether the Peruvian programme, Juntos, has an impact on households' investment in productive assets and activities. More precisely, we analyse the programme impact on agricultural and non-agricultural assets (used by the household for income-generating activities) and on livestock. Moreover, we inquire whether specific programme design and implementation features, such as programme duration and regularity of payment, produce different effects on investments.

The objective of this paper is to produce an impact evaluation of the Juntos programme in Peru and contribute to the empirical evidence about the productive role of cash transfer programmes. To our knowledge, this is the first impact evaluation of Juntos which focuses on agricultural and non-agricultural assets used specifically for income generating activities. Also, Del Pozo and Guzmán (2011) analyse the productive role of Juntos but their focus was mainly on land and livestock ownership. Additionally, we investigate whether the main programme mechanism is to relax liquidity constraints or to provide an insurance for risky investments.

Investigating the productive role of cash transfers is relevant because in developing countries, governments still face several constraints to finance, design and implement social assistance programmes and these interventions are designed to have a limited duration for beneficiaries. Identifying the determinants that may influence impact on investments and on living standards is crucial to properly design interventions and to maximise their long-term impact on the targeted population.

The remainder of this paper is organised as follows: Section 2 reviews the existing literature about the productive impact of cash transfer programmes in developing countries; Section 3 describes the Juntos programme in Peru; Section 4 describes the data; Section 5 presents the empirical analysis and shows the descriptive statistics, the identification strategy and the methods; Section 6 shows and discuss the results; Section 7 presents robustness checks; Section 8 concludes.

2. Literature Review

The interest on the impact of social transfers on productive assets and activities is recent but several studies have already addressed the issue of under which conditions cash transfer affect households' investment decisions. According to a recent literature review (Bastagli et al. 2016), most of the studies on the impact of cash transfers on livestock and agricultural asset ownership show positive and significant effects. Conversely, the evidence on non-agricultural assets impact is still mixed.
Indeed, according to Daidone et al. (2014b), in Lesotho cash transfers have a negative effect on non-agricultural assets ownership while Asfaw et al. (2014) and Blattman et al. (2012) find the opposite for Kenya and Uganda. An explanation for these differences could be due to the programme design, since the Ugandan programme was specifically designed to invite beneficiaries to invest in income generating activities.

For what concerns agriculture, the effect of unconditional cash transfers on livestock ownership in several Sub-Saharan African countries is positive and significant (Covarrubias et al. 2012; Daidone et al. 2014a; Blattman et al. 2014; Evans et al. 2014; Asfaw et al. 2014), also in case of environmental shocks (Merttens et al. 2013), but there is not a clear-cut evidence on the impact of unconditional cash transfer on agricultural and non-agricultural assets ownership[4].

The existing empirical evidence on households' ownership of other agricultural assets is mixed. According to Berhane et al. (2011), Covarrubias et al. (2012), and Daidone et al. (2014a) in a number of Sub-Saharan African countries, agricultural assets are positively and significantly affected by unconditional cash transfers; while according

[4] It is worth noting that most of the literature about the productive role of cash transfers in Sub-Saharan Africa was developed within the FAO "From Protection to Production" project.

to Pellerano et al. (2014), Merttens et al. (2013), and Gilligan et al. (2009) in other countries this was not the case. Merttens et al. (2013) explained the absence of an impact with the lack of arable land in targeted HSNP Kenya districts, while Pellerano et al. (2014) argued that the lack on an impact in Lesotho was due to the fact that the programme explicitly suggests spending money for children even though the transfer was unconditional. However, there is not experimental evidence about the fact that conditionalities or messages associated to the transfer may produce different effect on households' investments with respect to traditional unconditional cash transfers. For instance, in Latin America most of the cash transfers are conditional on requirements related to health and education and this leads beneficiaries to allocate part of the transfer to comply with them. Nevertheless, also in these cases, programmes allow beneficiary households to invest in livestock (Todd et al. 2010; Veras Soares et al. 2010; Gertler et al. 2012; Del Pozo and Guzmán 2011). Also, in Latin America the impact of cash transfers on non- agricultural assets is mixed. Gertler et al. (2012) find a positive and significant impact on non-agricultural assets used for micro-enterprise activities. Conversely, Maluccio (2010) finds a negative impact of conditional cash transfer in Nicaragua and justify it with the fact that in rural areas, where the programme operates, there are poor infrastructure and the micro-enterprise activities produce too low marginal returns. However, Maluccio (2010) did not find programme effect for beneficiaries on other assets either and explained this finding maintaining that the marginal propensity to consumption was close to one, so that people tend to use the whole transfer for current consumption.

Several authors analyzed agricultural and non-agricultural assets specifically used to generate income. In these cases, the studies analyzed not only the mere asset ownership but also the fact that thanks to those assets households starts (or improve) new (or existing) micro-enterprise or farm activities. According to Sadoulet et al. (2001) and Tirivayi et al. (2016), cash transfers may generate multiplier effects on income to the extent that they are used to invest in existing productive activities that otherwise would not be improved because of credit constraints. In some case beneficiaries, with access to both social protection and other complementary packages of agricultural support, are not only more likely to borrow for productive purposes and to use improved agricultural technologies, but also to invest in their own business activities (Gilligan et al. 2009).

Looking at the current literature, the mixed evidence does not seem to be justified by the presence of conditionalities or messages associated to the programme: conditional cash transfers (in Latin America) show a positive impact in several cases. The issue seems to be more related to the amount of money left from current consumption that the household can save, therefore it can depend on the transfer size. As reported by FAO (2015), different outcomes across programmes in Sub-Saharan Africa can be explained by differences in the amount. Also, Haushofer and Shapiro (2013) show that beneficiaries who receive a larger transfer tends to have higher savings and livestock ownership[5].

Other programme features may affect the productive role of an intervention. The duration a beneficiary is exposed to the programme (see Gertler et al. 2012) and the presence of complementary interventions (see Blattman et al. 2014) may affect households' investments and in turn long-term living standards. Additionally, households' responses to social transfers may be differently affected by past, current or expected future transfers, thus the transfer time profile is an important factor to be considered (Bianchi and Bobba 2012; Blattman et al. 2013). Finally, according to some research there is heterogeneity in the effect of cash transfers on households' investment choices also according to gender (Covarrubias et al. 2012; Evans et al. 2014).

3. The Juntos Programme

Juntos is a Conditional Cash Transfer (CCT) programme implemented in 2005 by the Government of Peru. The general objectives of the programme are to reduce poverty both in the short and long-run respectively through the injection of liquidity to poor households (via cash transfers) and the improvement of education and health status (through the conditionalities attached to cash transfers) (Perova and Vakis 2009). The targeting of Juntos has three steps. First, a geographical targeting selects districts according to: exposure to violence; poverty level, measured as a proportion of population with unsatisfied basic needs; poverty gap; level of child malnutrition; and presence of extreme income poverty (Perova and Vakis 2009). Second, a proxy means

[5] Bastagli et al. 2016 claim that most of the studies show increases in households saving and investments. However, recently Dasso and Fernandez, 2013 showed that cash transfers may also increase current consumption in "temptation goods".

test (PMT) identifies poor households and is combined with a categorical targeting selecting only households with pregnant women or children aged less than 14[6]. Third, a community-based targeting helps refining the list produced by the PMT strategy.

In order to receive the transfer, households are required to comply with specific conditionalities such as: at least 85% school attendance during the school year for children in school age and regular health check-up for children under 5 and pregnant women (Perova and Vakis 2009; Alcázar and Espinoza 2014). According to the official data reported by the Juntos Team (2017) in the first two months of 2017, 97.3% of targeted households complied with the required conditionalities at least in one of the two months.

The transfer amount is 100 Peruvian Nuevos Soles (around 30 USD) which, in 2009, represented 13% of the total monthly household consumption (Perova and Vakis 2009). The transfer is delivered monthly through bank deposits or with armored van. Even though the transfer is addressed to the household, the person entitled to collect the benefits are women. It is worth noting that the transfer promotes also some accompanying measures, namely beneficiaries' participation in awareness seminars on nutrition, family practices, health, sanitation, literacy, and productive activities.

The geographical coverage of the programme changed over the years. In 2005 only 4 departments (out of the 24 departments in Peru) were covered, while in 2017, 21. Juntos was first implemented in the poorest areas and then extended to other areas. Due to this geographical targeting strategy based on poverty indicators, the first areas covered by the programme were the rural ones. The programme was not designed to be addressed only to the rural population but, in its initial stage, was concentrated mainly in these areas (Trivelli & Díaz 2010).

According to the official data, until April 2017 the programme covered 749,349 households in 21 departments (including 1, 304 districts)[7]. The programme was found to have an impact on poverty reduction, utilization of health services,

[6] It is worth noting that in 2012 the eligibility criteria slightly changed and define eligible also households with children aged less than 19 (Alcázar and Espinoza 2014).

[7] See http://www.juntos.gob.pe/modulos/mod_infojuntos/

improvement of nutritional intake and increase in primary education (Perova and Vakis, 2009)[8].

4. Data

We use Young Lives Data, a longitudinal households survey collected to study childhood poverty. The survey constitutes a rich set of information. In order to have a sample of comparable households (both poor and better-off), the Young Lives team implemented a multi-stage sampling strategy reported in the Young Lives Method Guide (2011) and summarised here.

Firstly, sites predominantly located in poor areas were selected to reflect heterogeneity of ethnicity and religion and were over-sampled to ensure households comparability. Then within the communities, children and their households were randomly selected. The selection of sentinel sites (which correspond to districts) was done relying on a national poverty map (developed by FONCODES, the National Fund for Development and Social Compensation in 2000) that ranks all districts according to a poverty index calculated from variables including infant mortality rates, housing, schooling, roads and access to services. The richer 5% of districts were excluded from the analysis. The coverage of rural, urban, peri-urban and Amazonian areas was ensured. Then, within each selected district, small geographical areas were randomly chosen and within them households with one child in the age of interest of the survey were randomly selected. Young Lives Database was not intended to be nationally representative, but the objective was to ensure analysis of causal relations during long periods of time. However, the Peru survey was compared with other surveys showing that households poverty rates were similar to the ones of ENAHO 2001 but slightly better-off than the ones surveyed by DHS 2000 in terms of access to health and prenatal care services (Escobal and Flores, 2008). Therefore, the survey was considered representative at the national level.

Data were collected in 4 rounds: Round 1 was conducted in 2002, when the Juntos programme was still not in place, Round 2 in 2006, but still did not contain questions about households' participation in the programme, Round 3 (conducted in 2009) and

[8] Perova and Vakis (2009) provided a non-experimental impact evaluation of the Juntos programme relying on data from ENAHO, INEI and RENAMU.

Round 4 (in 2013) contain questions on the status of beneficiaries/non-beneficiaries. In each round 2,766 households were surveyed. There are two types of households in the Young Lives questionnaire. Those having a surveyed child aged 1 year old during the first round (Younger Cohort) and those having a surveyed child aged 8 years during the first round (Older Cohort). Attrition rates are low compared to other longitudinal studies, for both the Younger Cohort (6.3%) and the Older Cohort (10.3%) (Young Lives, 2014). This database has the advantage to include information about on a wide range of productive assets and investments, that other databases (for instance, the Enaho Panel) do not include. The questionnaire was meant to follow children over time, but unfortunately Older Cohort households in Round 4 were not surveyed about all dimensions. For instance, the last round does not contain information on land ownership, transfer and remittances received by the household, and regularity of payment of the Juntos programme. Moreover, another data limitation is that for Round 1 information on consumption is not available.

5. Empirical Analysis

The survey contains two variables that allow us to identify programme beneficiaries, one asking people if they are currently Juntos beneficiaries and one asking if they received Juntos during the last 12 months. We define programme beneficiaries those who reported having received transfers for the programme in the last 12 months. We find some inconsistency between these two variables. For instance, someone answered "I am not a current beneficiary" but then reported to have received the transfer in the last 12 months. This is due to the fact that they stopped receiving benefits after few months (as clear from the control answers).

To build a reliable counterfactual, we exclude households who received Juntos in the past but are no longer beneficiaries (because these 19 households may still have some long run effect of having received Juntos) and we also exclude households who reported to be current beneficiaries but did not report having received money in the last 12 months (because we are not sure if these 20 households are beneficiaries or not)[9].

[9] We could not crosscheck this information only for the Older Cohort beneficiaries Households of Round 4, since they were no longer surveyed about the question related to having received money from

Recalling that the programme started in 2005, in Round2 of the survey, conducted in 2006, there are people benefiting from the programme but, as mentioned above, no questions about the participation. To build the beneficiary status for Round 2 we rely on retrospective questions asked in Round 3. The number of beneficiaries in Round 2 is still very small since the programme just started to operate, therefore, we do not use Round 2 and we rely on Round 3 to identify beneficiaries.

Cross-checking information about the localities where the survey took place (reported in Escobal and Flores 2008) and the departments where Juntos was implemented, we find that the survey includes also districts were the programme never operated (namely: Callao, Moquega, Tumbes, ICA). To allow a higher level of comparability between treatment and control group we exclude households living in areas not targeted by the programme since they are areas with lower levels of poverty, food insecurity and crimes. Then, we confine the analysis to households living in Sierra (the central area of Peru) because among the households selected to be surveyed by Young Lives in 2002, 99% of Juntos beneficiaries live in this area in 2009 (Round 3). Comparing beneficiaries (mainly located in Sierra) with non-beneficiaries living in other part of the country could have led to misleading results since the two groups may differ for several characteristics.

Since our study focuses only on one of the three Peruvian regions, we compare the YL data, that show a high concentration of beneficiary households in Sierra, with the administrative data. A document from the Ministerio de Desarrollo e Inclusión Social (2014) shows that, among the 14 Peruvian departments covered by Juntos in 2009, 10 were in Sierra (namely, Apurimac, Ayacucho, Cajamarca, Cusco, Huancavelica, Huanuco, Junin, La Libertad, Pasco, Puno). This amounts to around 83% of the beneficiaries living in Sierra districts. Hence, the high concentration of beneficiaries found in the YL data seems to be confirmed by the administrative data.

Due to geographical quota, the programme was not implemented in the same period in all eligible districts. To build the counterfactual, we cannot rely on differences in quotas during the programme implementation because all the Sierra departments

Juntos during the last 12 months. They were surveyed only about the fact of being "current beneficiaries".

were targeted by Juntos before 2009. Therefore, we use an approach similar to Andersen et al. (2015), who analysed the Juntos impact on nutrition outcomes relying on Young Lives data.

As mentioned above, in Round 1 (2002) the programme was not operating, hence we have all the pre-programme households' characteristics that allow us to analyse the difference before and after the treatment for beneficiaries and non-beneficiaries.

The main issue to develop an impact evaluation in absence of treatment randomization, is to identify and remove the selection bias through quasi-experiment techniques. Since the programme participation is not random, it is possible that beneficiaries and non-beneficiaries are different not only in their status but also in other observable and unobservable dimensions that determine their eligibility and affect the outcome variables. If beneficiaries and non-beneficiaries are different, not only in the programme participation but also in other dimensions, then we cannot disentangle the effect of the programme from one of these dimensions on the observed outcomes of interest. In this case the estimator can be biased and lead to misleading results.

In order to remove the selection-bias, we rely on a two steps procedure. First, we identify a control group calculating the propensity score (based on pre-programme observable characteristics) and we select for the analysis only observations in the common support. Second, we implement a difference-in-difference technique (only on the common support sample selected through the propensity score) to remove the selection bias due to unobserved and time invariant characteristics specific of the treatment and the control group.

a. Descriptive Statistics

After excluding some observations, following the criteria explained above, at baseline (Round 1, in 2002) we have 1,173 households for which we have information about their status in Round 3. Among them, 429 will become beneficiaries in Round 3 and 744 will not. Table 1 shows households characteristics at baseline. Beneficiaries are more concentrated in rural areas, they have lower wealth index (built relying on housing quality index, access to service index and consumer durable index) with respect to future non-beneficiaries.

Table 1: Beneficiaries and Non-beneficiaries at Baseline

	Non-Beneficiaries				Beneficiaries					
	N	**Mean**	**Min**	**Max**	**N**	**Mean**	**Min**	**Max**	**Diff**	**P-value**
Community Characteristics										
Rural Area=1	744	0.265	0	1	429	0.804	0	1	-0.539	0.000
Households reporting crime in the community	744	0.081	0	0.217	429	0.076	0	0.189	0.005	0.080
Stunting children in the community	744	0.304	0	0.75	429	0.486	0	0.806	-0.183	0.000
Household composition										
Household size	744	5.618	2	16	429	6.14	2	18	-0.522	0.000
Presence of children under 7 in the household=1	744	0.941	0	1	429	0.986	0	1	-0.045	0.000
Household Head Age	744	36.827	17	83	416	35.608	16	73	1.218	0.089
Househol Head Sex	744	0.862	0	1	429	0.886	0	1	-0.024	0.235
Caregiver is literate=1	744	0.774	0	1	429	0.289	0	1	0.485	0.000
Household Etnicity										
White	744	0.043	0	1	429	0.023	0	1	0.020	0.080
Mestizo	744	0.956	0	1	429	0.977	0	1	-0.021	0.065
Household First Language										
Spanish	732	0.914	0	1	423	0.404	0	1	0.510	0.000
Quechua	732	0.074	0	1	423	0.546	0	1	-0.472	0.000
Other	732	0.012	0	1	423	0.05	0	1	-0.037	0.000
Household Economic Status										
Housing Quality Index	743	0.417	0.006	1	428	0.241	0	0.785	0.177	0.000
Access to Service Index	743	0.663	0	1	428	0.305	0	1	0.358	0.000
Consumer Durables Index	744	0.29	0	0.917	429	0.108	0	0.75	0.182	0.000
Ownership of livestock	744	0.684	0	1	429	0.97	0	1	-0.286	0.000
Ownership of production animals	744	0.68	0	1	429	0.97	0	1	-0.290	0.000
Number of owned production animals	744	13.325	0	311	429	24.014	0	298	-10.689	0.000
Ownership of draft animals	744	0.257	0	1	429	0.524	0	1	-0.268	0.000
Number of owned draft animals	744	0.794	0	15	429	1.131	0	13	-0.336	0.002
Agricultural assets ownership	744	0.591	0	1	429	0.963	0	1	-0.371	0.000
Non-Agricultural assets ownership	744	0.675	0	1	429	0.235	0	1	0.439	0.000

Future beneficiaries own more livestock and agricultural assets but less non-agricultural assets. This may be explained by the fact that better-off households are likely to be less engaged in farm or pastoral activities and more involved in micro-enterprise activities. Indeed, the correlation between the wealth index and the ownership of non-agricultural assets is positive while that between the wealth index and the ownership of agricultural assets and livestock is negative. Only 29% of future beneficiary households have a children caregiver who is literate against the 77% of non-beneficiaries.

Moreover, 91% of non-beneficiaries speak Spanish against 40% of beneficiaries who speak other languages. Future beneficiaries and non-beneficiaries at baseline are significantly different in most of the community and households' characteristics.

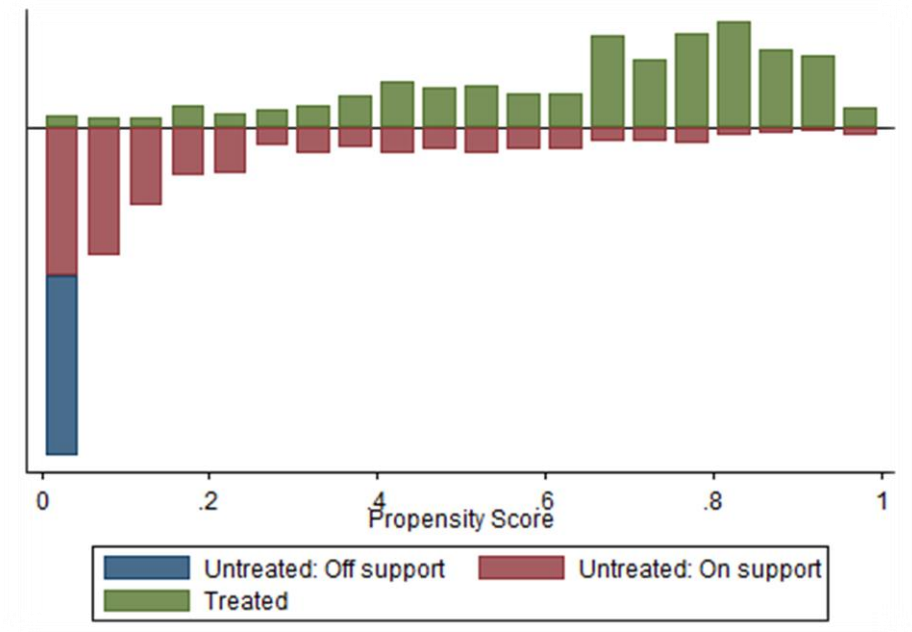
b. Methods

Since the programme eligibility was based on observable characteristics, we rely on them to identify a counterfactual for treated households. We implement a Propensity Score Matching, calculating the probability of being beneficiary through a logit model. In particular, we compute the probability of being eligible in Round 3 given the pre-programme households characteristics in Round 1[10]. To calculate the Propensity Score, we included the programme targeting criteria and some other households' demographic and educational characteristics. As reported in Section 3, the targeting strategy of Juntos includes geographical targeting, categorical targeting, proxy-means test and community-based targeting. More specifically, for the geographical targeting, and in line with the targeting of the programme, we calculate and include in the propensity score the percentage of crime and stunting children registered in each community. To account for the categorical component of the targeting strategy, we

[10] One issue of implementing the Propensity Score Matching consists in the fact that we use the characteristics of households in 2002. People who were identified by the survey as beneficiaries in Round 3, were selected by the programme in different points in time starting from 2005. However, it is possible that, between 2002 and the date households started to be beneficiaries, some of the baseline observable characteristics included in the Propensity Score Matching followed different trends for different households, differently affecting households' eligibility for the programme. One solution could be to rely on characteristics of Round 2 (collected in 2006) but we cannot do it because the programme was announced and implemented in 2005, therefore, it is possible that some households characteristic in 2006 (for example, housing quality index or consumer durables index) was already affected by the programme implementation (for households selected by the programme) or by some anticipation effects (for example, the household demographic composition for households who wanted to apply for the programme).

include a dummy equal to one when, within the household, there was at least one child aged under 7 in 2002, so that in 2009 the household could be still eligible for the programme having at least one child aged less than 14. We also rely on proxies of the household poverty level such as the wealth index (which includes the housing quality index, consumer durables index and access to services) and we include the household size and the educational level of children caregiver within the household. The final step of targeting is community-based and is not driven by formal rules, hence the only thing we cannot control are the criteria implemented by each community to validate the list of potential beneficiaries. The Propensity Score Matching allows us to identify a common support for beneficiary households (Figure 1) and to exclude households who show very different characteristics with respect to those eligible for the programme[11].

Figure 1: Common Support after Propensity Score Matching



In particular, we exclude around 20.8% of non-beneficiaries, and 0.5% of beneficiary, who are out from the common support. After these operations, we end up with a sample of 1,016 households, 427 beneficiaries and 589 non-beneficiaries. We compute the standardized mean difference[12] to check whether the two groups are

[11] Propensity Score Estimates are showed in Appendix 1.1

[12] Instead of relying on a simple t-test, which is size sensitive, we used the standardized mean difference (that is the mean difference across groups on the pooled standard deviation).

similar in observable characteristics used to define eligibility. In Table 2, we show how beneficiaries and non-beneficiaries are not significantly different in the observable eligibility criteria.

As reported by Khandker et al. 2009, propensity score matching alone cannot take into account unobserved characteristics (that might determine the treatment). Therefore, after the selection of a reliable counterfactual, we implement also a difference-in-difference, between Round 3 and Round 1, only on the sample of households in the common support (see Khandker et al. 2009). This methodology allows us to remove time-invariant unobserved differences between beneficiaries and non-beneficiaries. We estimate the difference in outcomes between beneficiaries and non-beneficiaries after and before the treatment. If the two groups are the same on average in both observable and unobservable time invariant characteristics, except that one is treated and the other is not, then we can ascribe the differences in outcomes to the impact of the treatment.

Table 2: Comparison between groups after the Propensity Score Matching

	Mean in Treated	Mean in Untreated	P-value for difference
Rural area=1	0.810	0.820	0.630
Percentage of households reporting crime in the community	0.080	0.080	0.540
Percentage of households reporting stunting children in the community	0.490	0.480	0.654
Presence of children under 7 in the household=1	0.990	0.990	0.921
Wealth Index	0.22	0.21	0.550
Household size	0.810	0.820	0.630
Caregiver is literate=1	0.080	0.080	0.540

Our dependant variables of interest are: (i) household ownership of any livestock; (ii) household ownership of production animals including both ruminant animals (such as sheep, goats, pigs, poultry, rabbits) and milk animals (traditional and modern cows); (iii) number of owned production animals; (iv) household ownership of draft

animals (including: donkey, horses, mules and oxen) (v) number of owned draft animals; (vi) household ownership of agricultural assets (for instance: shovel, rakes, cart/wheelbarrow, motorised saw, animal drawn plough, storehouse/granary, silos and harvester tractor); (vii) household ownership of non-agricultural assets (namely: construction tools, food preparation equipment, transport equipment, weaving equipment and computer).

It is worth noting that questions related to agricultural and non-agricultural assets specifically ask if these assets were used for income-generating activities. Therefore, we can interpret these variables as the fact that the household is involved in income-generating micro-enterprise or farm activities.

Firstly, we estimate the following model with no covariates:

$$\Delta \boldsymbol{Y_{it}} = \boldsymbol{\alpha} + \boldsymbol{\beta T_i} + \boldsymbol{u_{it}} \qquad \textbf{(1)}$$

Where, α is a constant term, β is the true effect of treatment (the difference in difference estimator) and u is the error term.

Then we control for a vector of covariate X, including: type site (rural or urban); wealth index; other unpredictable transfers received by the household (namely: transfers from religious organizations; transfers from charity groups; donations; other transfers); shocks that affected the households in last 12 months (environmental shocks; crime shocks; economic shocks; family shocks); and household size.

$$\Delta \boldsymbol{Y_{it}} = \boldsymbol{\alpha} + \boldsymbol{\beta T_i} + \boldsymbol{\theta \Delta X_{it}} + \boldsymbol{u_{it}} \qquad \textbf{(2)}$$

In Round 4 for the older cohort households, we do not have information about: other kind of transfers received by the households (therefore we cannot control for them); about the Juntos payment frequency; and we cannot cross-check information on household's programme participation. For this reason, we use Round 4 just for the robustness check and to analyse whether the effect of the programme is sustained over time for households that were beneficiaries in both rounds.

6. Results

We investigate whether the conditional cash transfer programme Juntos has an impact on the productive investment decision of beneficiaries. In Table 3 we present the results of Model 1, showing the difference-in-difference estimator.

Table 3: Impact on Productive Assets and Activities

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Juntos	-0.006	0.044	0.071*	-0.012	0.097**	0.140***	0.168***
	(0.0240)	(0.0258)	(0.0331)	(1.628)	(0.129)	(0.0246)	(0.0353)
N	1,016	1,016	1,016	1,016	1,016	910	910

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Table 4: Impact on Productive Assets and Activities- With Covariates

	Livestock =1	Production Animals=1	Draft Animals =1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Juntos	-0.018	0.023	0.049	-0.001	0.086*	0.119***	0.145***
	(0.0262)	(0.0281)	(0.0357)	(1.783)	(0.139)	(0.0265)	(0.0374)
Rural Area=1	0.061	0.047	0.119***	0.081*	0.107**	0.071*	-0.019
	(0.0470)	(0.0505)	(0.0642)	(3.200)	(0.250)	(0.0491)	(0.0693)
Wealth Index	-0.093**	-0.092**	-0.021	0.008	0.035	-0.070*	0.133***
	(0.0965)	(0.104)	(0.132)	(6.566)	(0.513)	(0.104)	(0.146)
Other Trasfers=1	0.017	0.012	0.084**	0.018	0.120***	-0.020	0.082*
	(0.0195)	(0.0209)	(0.0266)	(1.328)	(0.104)	(0.0201)	(0.0284)
Environmental Shocks	0.027	0.050	0.047	-0.062	0.007	0.057	0.046
	(0.0248)	(0.0267)	(0.0339)	(1.690)	(0.132)	(0.0250)	(0.0353)
Crime Shocks	0.028	0.041	-0.007	0.015	0.027	-0.016	-0.029
	(0.0349)	(0.0374)	(0.0476)	(2.373)	(0.185)	(0.0360)	(0.0508)
Economic Shocks	-0.008	-0.000	0.012	0.019	0.001	0.044	0.052
	(0.0239)	(0.0257)	(0.0326)	(1.628)	(0.127)	(0.0243)	(0.0343)
Family Shocks	-0.040	-0.037	0.015	-0.039	0.050	-0.049	-0.014
	(0.0208)	(0.0224)	(0.0284)	(1.418)	(0.111)	(0.0216)	(0.0305)
Male Headed Household=1	0.037	0.059	-0.006	0.015	-0.000	0.026	0.007
	(0.0357)	(0.0383)	(0.0487)	(2.431)	(0.190)	(0.0392)	(0.0553)
Household Size	0.081*	0.066*	0.073*	0.109***	0.099**	0.074*	0.037
	(0.00594)	(0.00638)	(0.00811)	(0.404)	(0.0316)	(0.00635	(0.00896)
N	986	986	986	986	986	881	881

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

While Table 4 shows the results for Model 2 when we control for other covariates (that can be interpreted as changes between Round 3 and Round 1).

Moreover, beneficiaries are also significantly more likely to invest in agricultural assets used for income generating farm activities and to invest in non-agricultural assets used for micro-enterprise activities.

The specification with covariates (Table 4) confirms all the results of Table 3 except for the coefficient related to the ownership of draft animals which is no longer significant.

The impact of Juntos on investments in number of draft animals, is still positive and significant at 5% while coefficients related to agricultural and non-agricultural assets are still significant at 0.1%. Our results confirm the existing evidence about the fact that an injection of liquidity may allow households to invest in productive assets and engage in income-generating activities[13].We are also interested to find out whether the conditional cash transfer is more likely to relax liquidity constraints or to be used as an insurance for risky investments thanks to the fact that the transfer (by design) is regular and predictable. Bianchi and Bobba (2012) studied these mechanisms analysing the impact of Progresa on occupational choices in Mexico. However, there are no studies that test this mechanism for investment in productive assets and activities. Ideally, we could test this hypothesis relying on the fact that households with only children aged 14 (which until 2012 was the age threshold to receive the transfer) will not be eligible in the future and check whether these households changed investment decision due to the lack of an insurance for the future. Unluckily, in Round 3, 94.4% of households have at least one child aged under 10. Therefore, the sample of households that will have to leave the programme soon (households with children aged between 10 and 14 years) is too small to test our hypothesis. However, the high percentage of households with children aged under 10 means that, *ceteris paribus,* most households of our sample know that they could still be eligible for the transfer at least for other 4 years, until all children turn 14.

[13] These results are coherent with the findings of Del Pozo and Guzmán (2011), who studied investment in livestock for Juntos beneficiaries. However, in their study is not specified whether beneficiaries use these livestock for income generating activities.

Given these data limitations, we cannot directly test the aforementioned hypothesis and, in the attempt to find a solution, we build an indicator of 'other transfers', for which we control for. The indicator is a dummy variable taking value one when the household receives at least one unpredictable transfer, defined as transfers that consist in lump sum given without predictability and frequency (such as transfers from religious organizations, from charity groups, donations, and other non-specified transfers different from predictable transfers; therefore we excluded predictable transfers, such as pensions and social security). Looking at the coefficient related to other unpredictable transfers we can observe that its impact on dependent variables related to draft animals and non-agricultural assets is positive and significant. Our findings suggest that predictability of payment does not play a central role in the case of Juntos, since also unpredictable transfers have an effect on investments. Therefore, it is possible that the main role of Juntos is to relax liquidity constraints rather to work as an insurance for risky investments.

To further check this hypothesis, we build different conditional samples first comparing beneficiaries that receive the transfer regularly with non-beneficiaries and then comparing beneficiaries that do not receive the transfer regularly with non-beneficiaries. According to Barrientos (2012), social transfers may reduce insecurity, and consequently the inefficient use of resources, when they are "regular and reliable". If the "insurance channel" would be verified, we can expect that households that receive the transfer regularly (every month, as by programme design) look at the transfer as a predictable and regular source of income and invest more in productive assets with respect to people who did not receive the transfer regularly (they did not receive the transfer every month in the last year).

Coefficients are higher and more significant for beneficiaries that receive the transfer regularly, however, looking at the p-value for the difference between coefficients we can conclude that the two groups (beneficiaries who received the transfer regularly and who did not) are not significantly different (Table 5). Transfer regularity does not play a significant role in improving investment in productive assets. This finding reinforces the hypothesis that Juntos acts on investment mainly by relaxing liquidity constraints and it is not perceived by beneficiaries as a source of insurance.

Table 5: Transfer Regularity

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets - Beneficiaries who did receive the transfers regularly							
Juntos	-0.023	0.007	0.048	-0.007	0.080*	0.103**	0.132***
	(0.0329)	(0.0353)	(0.0405)	(2.081)	(0.169)	(0.0324)	(0.0442)
N	811	811	811	811	811	709	709
Impact on Productive Assets - Beneficiaries who did not receive the transfers regularly							
Juntos	-0.000	0.037	0.034	0.019	0.075	0.087*	0.120**
	(0.0383)	(0.0406)	(0.0437)	(2.145)	(0.178)	(0.0392)	(0.0522)
N	744	744	744	744	744	645	645
P-value for difference	0.4151	0.2469	0.8258	0.6358	0.9076	0.9152	0.9632

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Another purpose of our analysis is to check whether the programme features, such as length of programme exposure, may affect investment decisions. Our results (see Table 6) suggest that beneficiaries that receive the transfer for less than two years are still significantly more likely to invest in agricultural assets and non-agricultural assets with respect to non-beneficiaries but only beneficiaries who receive the transfer for more than two years are also more likely to invest in draft animals with respect to non-beneficiaries.

Table 6: Programme Duration - With Covariates

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets - Beneficiaries who receive the transfers for less than 2 years							
Juntos	-0.036	-0.015	-0.017	-0.032	0.045	0.107**	0.114**
	(0.0370)	(0.0398)	(0.0427)	(2.003)	(0.176)	(0.0384)	(0.0503)
N	742	742	742	742	742	639	639
Impact on Productive Assets - Beneficiaries who receive the transfers for more than 2 years							
Juntos	0.005	0.049	0.083*	0.023	0.095*	0.086*	0.140***
	(0.0337)	(0.0357)	(0.0413)	(2.186)	(0.171)	(0.0328)	(0.0455)
N	818	818	818	818	818	720	720
P-value for difference	0.0983	0.0275	0.0767	0.2981	0.2616	0.2379	0.7296

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Coefficients for the two groups are not significantly different. We can conclude that the programme features do not significantly affect outcomes differently across beneficiaries. Finally, we divide the sample according to the wealth index (hereafter WI) at the baseline (Round 1). Comparing beneficiaries and non-beneficiaries with a

WI level below the median we do not find significant programme effect. Conversely, for households with a WI higher than the median, we do find that the programme impact for beneficiaries is positive and significant for both agricultural and non-agricultural assets used for income generating activities. The numerical coefficients of the two sub-samples are not significantly different, however Table 7 suggests that the programme produces a stronger impact for better-off beneficiaries. This finding is consistent with the hypothesis that wealthier people tend to pursue an asset accumulation strategy while poor agents tend to accumulate low-return buffer assets threshold (see Zimmerman and Carter 2003) and, therefore, that social protection policies may affect agents investments behaviours differently depending on the fact that they are above or below a poverty threshold. In our case, it seems that the CCT is able to positively and significantly affect beneficiaries who are relatively better-off in terms of wealth, while it is not able to do the same for beneficiaries who are far from a certain WI threshold. In sum, the programme seems to have an impact on the poor but not on the poorest of the poor[14].

Table 7: Impact on beneficiaries with different levels of Wealth Index- With Covariates

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets - WI Below the median							
Juntos	0.034	0.060	0.016	0.060	0.052	0.048	0.026
	(0.0230)	(0.0276)	(0.0571)	(2.327)	(0.218)	(0.0224)	(0.0433)
N	493	493	493	493	493	474	474
Impact on Productive Assets - WI Above the median							
Juntos	-0.073	-0.041	0.040	-0.083	0.055	0.108*	0.108*
	(0.0560)	(0.0580)	(0.0490)	(3.172)	(0.197)	(0.0588)	(0.0701)
N	493	493	493	493	493	407	407
P-value for difference	0.0236	0.0924	0.8118	0.0993	0.9239	0.0694	0.1435

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

7. Robustness Check

As mentioned above, we use Round 3 for our analysis, since we do not have all the information for Round 4. In particular, in Round 4 we do not have information about

[14] It could be interesting also to look only at the programme impact on households who did not own assets at baseline but unfortunately the sample is too small to conduct an analysis on this conditional sample.

the payment frequency and about the other transfers received by the Older Cohort households, so we cannot control for these variables. Therefore, we use Round 4 only for a robustness check. First, we calculate the Difference in Difference estimator without covariates relying on Round 1 and Round 4. We analyse only households in the common support and people who are beneficiaries in both Round 3 and Round 4, that are 301 individuals (see Table 8).

Table 8: Beneficiary status in Round 3 and Round 4

		Round 4			
		Untreated	Treated	Missing	Total
Round3	Untreated	297	100	192	589
	Treated	24	301	102	427
	Total	321	401	294	**1,016**

We find a positive and significant impact of the programme on all the dependent variables that were found to be significant relying on Round 3 (see Table 3) except for the draft animal ownership, which coefficient was found to be significant for Round 3 but not for Round 4 (see Table 9).

Table 9: Robustness Check: Using data from Round 4

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets-Without Covariates							
Juntos	0.351***	0.464***	0.040	0.131**	0.133**	0.250***	0.115*
	(0.0355)	(0.0353)	(0.0540)	(2.194)	(0.206)	(0.0361)	(0.0552)
N	512	553	553	553	512	433	433
Impact on Productive Assets- With covariates							
Juntos	0.190***	0.242***	0.057	-0.009	0.072	0.139**	0.120*
	(0.0373)	(0.0381)	(0.0605)	(2.570)	(0.235)	(0.0376)	(0.0598)
N	504	504	504	504	504	433	433

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Additionally, for households that are beneficiaries in both Round 3 and Round 4, the programme has a positive and significant impact also on livestock ownership and on production animals ownership. Controlling for the same covariates used in the rest of the analysis (except for other transfers received by the household) results are partially confirmed. With respect to Round 3, the programme impact is still positive and significant at 0.1% for agricultural assets. However, the coefficient related to non-

agricultural assets loses significance shifting from 0.1% to 5%. Also controlling for covariates, beneficiary households are significantly more likely to invest in livestock and in production animals with respect to non-beneficiaries while the coefficient related to the number of draft animals is no longer significant in Round 4. While initially (in Round 3) beneficiary households invest in draft animals, agricultural and non-agricultural assets, then they start to invest also in other kind of livestock, particularly in production animals. The fact that households start to invest also in production animals is consistent with the idea that households tend to combine different assets to achieve the preferred risk-return combination (Zimmerman and Carter 2003). Moreover, after a certain threshold of the same kind of accumulated assets, the returns may start to be decreasing and therefore investing in other assets may be more convenient. A problem for our analysis is that to build the counterfactual we could not rely on differences in quota determined by the geographical roll-out of the programme, since all the Sierra departments were targeted before 2009. However, in Round 4 there are 100 new beneficiaries in Sierra (see Table 8). We then conduct a robustness check comparing beneficiaries of Round 3 with these 100 people who are not beneficiaries in Round 3 but become beneficiaries in Round 4. Table 10 suggests that only the coefficient related to the number of owned draft animals is still significant, other coefficients maintain the same sign, but they lose significance. However, we should be cautious in interpreting the results since, in this case, the sample is small (100 non-beneficiaries).

Table 10: Robustness Check: Including 100 new beneficiaries

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets- Without Covariates							
Juntos	-0.030	-0.017	0.065	0.077	0.087*	0.012	0.039
	(0.0261)	(0.0303)	(0.0670)	(3.354)	(0.232)	(0.0263)	(0.0543)
N	527	527	527	527	527	511	511
Impact on Productive Assets- With Covariates							
Juntos	-0.031	-0.017	0.051	0.069	0.060	0.014	0.046
	(0.0272)	(0.0314)	(0.0690)	(3.489)	(0.236)	(0.0272)	(0.0551)
N	516	516	516	516	516	501	501

Standardized beta coefficients; Standard errors in parentheses, * p<0.05, ** p<0.01, *** p<0.001

8. Conclusion

The paper provides an impact evaluation of the Juntos programme on households' decisions to invest in livestock, and agricultural and non-agricultural assets used for income generating activities. The idea is that cash transfer on the one hand can relax households' liquidity constraints and, on the other hand, when regular and predictable, can work as an insurance mechanism, therefore allowing households to invest in risky but higher-return activities. The reason why households should invest the saved amount in productive assets and activities is linked to the fact that the cash transfer programmes have usually a limited duration, and the investment in productive assets and activities is one of the channels through which beneficiaries can maintain the living standards reached thanks to the programme also after its termination. Moreover, assets accumulation represents an insurance for consumption in case of bad economic conditions.

To assess the programme impact, we rely on Propensity Score Matching and Difference in Difference. We show that beneficiaries households are significantly more likely to invest in agricultural and non-agricultural assets used for income generating activities and draft animals. Comparing the programme impact with the effect of other unpredictable transfer received by the households, we argue that it seems to be more likely for Juntos to help relaxing liquidity constraints rather than working as an insurance for risky investments. This result is confirmed by the fact that transfers regularity does not produce significant differences between groups of beneficiaries and non-beneficiaries. Moreover, we analyse another programme feature, we show that the programme duration does not produce significant differences between groups of beneficiaries and non-beneficiaries. We also show that the programme has a productive role for the poor but not for the poorest of the poor. We check the robustness of our results using outcomes from another survey round and we find that results are robust and that the programme shows a sustained impact over time.

This paper shows that, beyond the mere protection, the Juntos programme has also a production role, positively affecting beneficiaries' decisions to invest in productive assets and activities. Hence, cash transfer programmes may affect poverty not only in the short-term during the programme implementation but also in the long-term,

through investments in assets and activities that may support poor households also after the programme termination.

Appendix 1.1

Table 1.1: Propensity Score Estimates

	Juntos (Beneficiary=1 in Round 3)
Area of residence (rural=1)	0.502*
	(0.199)
Percentage of households reporting crimes in the community	-0.715***
	(1.780)
Percentage of stunted children in the community	1.661***
	(0.621)
Presence of children under 14 in the household in Round 3 (under 7 at the baseline)	0.511*
	(0.527)
Wealth index	-2.295***
	(0.650)
Household size	0.153
	(0.0377)
Caregiver is literate=1	-0.849***
	(0.174)
N	1,169

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Appendix 1.2

We check the programme impact on two subsamples of people living in rural or in urban areas. We find that in both rural and urban areas the programme positively and significantly affects beneficiaries investments in non-agricultural assets used for income generating activities. Conversely, as expected, only in rural areas programme beneficiaries are more likely to invest in livestock while in urban areas the opposite happens. One interesting result is that only in urban areas the impact of the programme on agricultural assets is positive and significant, while in rural areas is positive but not significant. This result can be explained by the fact that in our sample (in Round 3), around 37% of urban households own land for agricultural purposes.

Table 1.2 Impact on Rural and Urban Areas

	Livestock=1	Production Animals=1	Draft Animals=1	Number of Production Animals	Number of Draft Animals	Agricultural Assets=1	Non-agricultural Assets=1
Impact on Productive Assets - People who live in rural area							
Juntos	0.026	0.070	0.017	0.120**	0.129**	0.033	0.095*
	(0.0157)	(0.0206)	(0.0560)	(2.632)	(0.212)	(0.0207)	(0.0442)
N	501	501	501	501	501	491	491
Impact on Productive Assets - People who live in urban areas							
Juntos	-0.038	-0.005	-0.051	-0.200***	-0.104*	0.137**	0.145**
	(0.0569)	(0.0596)	(0.0477)	(2.534)	(0.195)	(0.0576)	(0.0703)
N	515	515	515	515	515	419	419
P-value for difference	0.2178	0.4504	0.3674	0.0003	0.0010	0.0029	0.1486

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Chapter 2

Making Migration a Choice:

the Impact of Cash Transfers on Internal Migration in Peru

Abstract

This chapter analyses whether welfare interventions, such as conditional cash transfer programmes, may affect households' internal migration from rural to urban areas. Using Propensity Score Matching, we analyse the impact of the Juntos programme in Peru and find that beneficiary households are significantly less likely to migrate from rural to urban areas with respect to similar non-beneficiaries because receiving a cash transfer increases the opportunity cost to migrate. To confirm this hypothesis, we also show that Juntos does not crowd-out private transfers allowing an increase in the opportunity cost of moving. Results are robust confining the analysis to the Sierra region, where the majority of the programme beneficiaries live. We find a sustained impact of the programme over time.

Keywords: Conditional Cash Transfers; Impact Evaluation; Migration; Peru; Juntos.

1. Introduction

The existing literature on migration focused on several drivers of international and internal migration, among others: differences in origin and destination labour markets, income differential, distance, presence of social networks in destination countries, skills transferability, political stability and conflicts. The objective of this paper is to study whether welfare interventions may be also considered relevant determinants of migration.

The idea that welfare may influence migrations choices was firstly introduced by Borjas who, in 1999, developed the welfare magnet hypothesis. His theoretical model focuses on international migration and it is based on the hypothesis that migrants are attracted by countries that offer better welfare systems. In its seminal work, Borjas showed that migrants arrived in the U.S. were more attracted by states with higher levels of welfare and that immigrants who are potential welfare recipients were more clustered in states with higher levels of cash transfers benefits.

In developing countries, welfare policies targeted at specific vulnerable or poor groups are often referred to as social protection policies. In what follows we use the terms “welfare” and “social protection” interchangeably.

The existing literature defined “welfare migration” often from the perspective of the receiving countries considering welfare benefits as one of the factors that migrants may take into account in their decision about the destination country. However, as pointed out by Lee (1966), migration should be studied jointly considering pull and push factors but also intervening obstacles which may favour or discourage migration for certain population groups given economic, geographical and political conditions. Therefore, to have a comprehensive overview of the phenomena, “welfare migration” could be studied and analysed also from the perspective of the origin community or country. If people migrate toward countries (or regions) with high welfare benefits levels, it could be the case that the lack of welfare in source countries (or regions) is also one of the causes that lead them to migrate.

Our objective, therefore, is to test if welfare interventions may affect the decision to migrate, not from the usual perspective of the place of destination (i.e. attracting

migrants) but from the perspective of the place of origin in the case of internal migration.

Although the current debate on migration focuses mostly on international migration, in 2013 at the global level there were approximately 763 million of internal migrants[15] (Bell and Charles-Edwards 2013) against 232 million of international migrants (UN 2013). Internal migration from rural to urban areas was firstly explained by Harris and Todaro in 1970, who developed a theoretical model showing that the decision to migrate is based on expected income differential between rural and urban areas. Indeed, individuals migrate if the expected income in the destination country or community, net of the cost to migrate, is higher than the income in origin country or community. In 1980, Todaro developed a framework describing the economic and non-economic determinants of migration. Among the costs of migration, Todaro mentioned transport cost, cost of living and opportunity cost. The opportunity cost of moving can be described as the "income a migrant could have earned if he did not move" (Levy and Wadycki,1974).

Cash transfers may increase the opportunity cost of moving by providing to the household an additional source of income in the place of origin. In our paper, we want to analyse whether the presence of welfare interventions may affect the households' decision to migrate from rural to urban areas. In particular, we show that social transfers discourage migration by increasing the opportunity cost of moving. This research aims at contributing to the existing literature, which so far has had mixed results, through a new case study analysing the relationship between Juntos, a Peruvian social protection programme, and internal migration flows in Peru. To check the robustness of our results, we check whether Juntos may crowd-out private transfers. If this is the case, the opportunity-cost to migrate remains the same of the one registered before the start of the programme, because the increase in income due to cash transfers is offset by the loss of private transfers. In this case we could expect that the programme does not produce any impact on migration. Conversely, if cash transfers do not crowd-out private transfers the opposite happens: there is a real

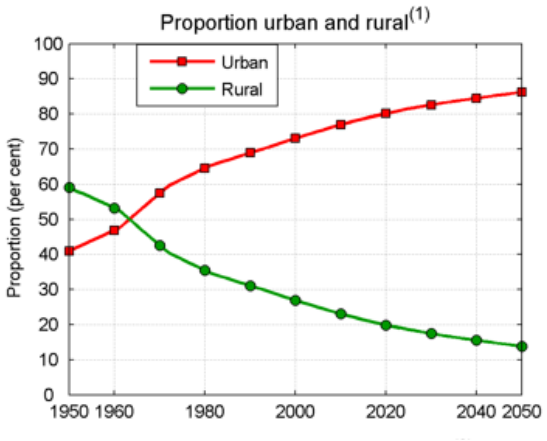
[15] Defined as those who live in a region different from the one of birth

increase in the opportunity cost of moving and, in this case, we can expect that cash transfers may deter migration.

Additionally, to confirm the opportunity-cost mechanism, to the extent allowed by the data, we analyse whether the presence of conditionalities attached to the transfer may have a role in affecting migration. Conditionalities require children and caregiver to stay in the place of origin to attend school and health check-up. This affects their migration decision and may constitute a reason to avert migration. However, the other working age members are not affected by conditionalities. To disentangle the possible effect of conditionalities on migration choices we conduct the analysis for both the households and for single households' members.

We choose to analyse Peru as a case study because there was an impressive migration from rural to urban areas and this trend is likely to continue in the future (see Figure 1). The internal Peruvian migration to urban areas (and in particular to Lima) caused the creation of the so called "pueblos jovenes", shanty towns where there are poor housing and services conditions.

Figure 1: Rural Urban Migration in Peru



Source: United Nations, Department of Economic and Social Affairs,
Population Division (2014): World Urbanization Prospects: The 2014 Revision.
(1)Proportions of urban and rural population in the current country or area in per cent of the total population, 1950 to 2050.

This paper is organised as follows: Section 2 reviews the existing literature about the relations between welfare and migration; Section 3 describes the Juntos programme in Peru; Section 4 describes the data; Section 5 presents the empirical analysis and

shows the descriptive statistics, the identification strategy and the methods; Section 6 shows the results; Section 7 presents robustness checks; Section 8 concludes.

2. Literature review

Let us briefly analyse in turn the two streams of literature about social protection as "pull" and "push" factor and the literature about crowding-out effect between public and private transfers.

a. Social Protection as "Pull Factor"

Since its postulation, the welfare magnet hypothesis, developed by Borjas, was tested by several scholars with very different methods, assumptions and results and was mainly studied for international migration flows. The theory was confirmed by some research (among others: De Giorgi and Pellizzari 2006; Warin and Svaton 2008) and was able to explain migration inflows under certain specific conditions and for certain groups: for unskilled workers but not for the skilled ones (Brucker et al. 2002; Meyer 2000); for unskilled migrants in case of free-migration regimes but not in case of restricted-migration regimes (Razin and Wahba 2015); for groups who are more likely to be eligible for welfare programmes, such as women and mothers (Enchautegui 1997; Meyer 2000); for groups who are eligible for specific programmes in certain country states (Southwick 1981). However, the welfare magnet hypothesis was also rejected by a number of studies (Levine and Zimmerman 1999; Menz 2008; Giulietti et al. 2013; Giulietti 2014) that found that migration seems to be much more driven by other factors, such as social networks rather than by the level of welfare benefits (De Giorgi and Pellizzari 2009).

The mixed evidence about this issue may be explained by the following reasons: many studies do not account for the possible reversal causality between migration and welfare spending (Giulietti and Wahba 2012); several studies lack of a clear differentiation across forced migrants and labour migrants (Schulzek 2012); often studies underestimate the linkages between the factors that lead of different groups to migrate and the choice of the destination country.

b. Social Protection as "Push Factor"

The literature about how social protection affects the probability to migrate is still very limited, mixed and mainly based on empirical evidence rather than theoretical models.

As summarized by Hagen-Zanker and Himmelstine (2012), social protection programmes may produce two different effects: on the one hand, they may deter migration by providing additional sources of income to potential migrants or by increasing the opportunity cost to migrate since potential migrants do not want to lose the benefit they obtained; on the other hand, the additional income received may be saved and used to migrate for poor households that otherwise would have not enough money to cover the migrations costs.

Hagen-Zanker and Himmelstine (2012) reviewed 22 studies of social protection programmes (unconditional and conditional cash transfers, public works programmes, health insurance, unemployment insurance, health fee waivers, school subsidies and asset transfers) and economic labour migration. They found evidence in favour of both hypotheses. Social programmes increased the beneficiaries' probability to migrate when the transfer amount was not enough generous to satisfy the households' needs in the place of origin and, therefore, the transfer was used to fund migration. Instead, social programmes were able to reduce the beneficiaries' probability to migrate when the benefits amount was generous enough to allow households to have an adequate standard of living in their own country.

Deshingkar et al. (2015), through a qualitative study conducted in Ethiopia, Tanzania, Kenya and Malawi, tested the hypothesis that cash transfers may directly create the conditions to reduce the need to migrate or instead be used to fund the cost of migration. What they found is that social protection benefits do not seem to have a direct impact on migration but, in some cases, cash transfers allow children to be enrolled at school and to achieve a higher level of education enabling them to access jobs and, in turn, to fund the migration costs when they are older.

Thus, there is no clear evidence about the impact of social protection on migration. This depends on whether social transfer crowd-out private transfers (see next section) and on: contextual factors (e.g. the possibility to access the labour market in

source countries or community); programme design and implementation features (in particular, the transfer amount); beneficiary characteristics (gender, skills and poverty levels); and on the reasons that lead households to migrate (Hagen-Zanker and Himmelstine 2012). A cross-country study on this issue was developed by Greenwood et al. (1999, 2011). The studies, relying on an *ad hoc* database and on a cross-sectional time-series analysis, focused on the influence of social programmes in origin countries on migration to the United States. The authors found that the existence of old- age programmes, unemployment insurance programmes, and to a lesser extent sickness and medical programmes in origin countries, discourages the movement to the U.S.A. and that programmes affect the skills composition of migrants. Indeed, as showed by Greenwood et al. (1999) different types of social protection programmes may produce different impacts on beneficiaries. For instance, old-age grants may be used by working age individuals within the household to migrate (Posel et al. 2006).

Our focus is on the impact of conditional cash transfer programmes on migration, to our knowledge less explored in the literature. Moreover, there are no studies able to disentangle the effect of the cash transfer from the one of the conditionalities in affecting migration. Indeed, children school attendance or regular health check-up may affect the decision to migrate for people affected by conditionalities within the household (Steklov et al., 2005). Conditionalities may have different effect in the short and in the long run. In the short run, they may influence households to stay in the origin community. However, if conditionalities positively affect schooling, the child, once he/she complete the secondary grade may decide to migrate to areas with higher returns to education (see Behrman et al. 2008).

Furthermore, the analysis of migration patterns should be made at both individual and household level because the programme design may differently affect migration choices of specific members within the household. For instance, in several countries, recipients of conditional cash transfer programmes are women and this may affect both household and women migration, but to a lesser extent the migration of the male within the household. Unfortunately, available data on these topics do not always allow to disentangle household and individual migration and no experiments are available about the specific role of conditionalities in affecting migration.

The paper focuses on the impact conditional cash transfers on internal migration and, to the extent allowed by the data, also analyses the role of conditionalities. Existing evidence about conditional cash transfers shows that in Mexico and Honduras, these programmes do not significantly affect beneficiaries domestic migration (Angelucci,2011; Stecklov et al. 2005; Winters et al 2005) while Bolsa Familia decreases the probability to migrate for Brazilian beneficiaries (Neto 2008). Conversely, in Nicaragua, conditional cash transfers increase the probability to migrate for beneficiaries (Winters et al 2005).

c. Do Social Transfers crowd-out private transfers?

As reported in Section 1, the effect of cash transfers on migration may be mediated by the fact that cash transfers may crowd-out private transfers. If this happens the increase in income (due to the cash transfers) is compensated by the loss of private transfers, there is not a real increase in the opportunity cost of moving and we may expect no effect on migration. Conversely, if cash transfers do not crowd-out private transfers, there is an increase in income sources diversification and in the opportunity cost of moving. In this case, *ceteris paribus*, we expect cash transfers deterring migration. Cox (1987) compares two motives for private transfers: altruism (Becker, 1974) and exchange (Bernheim, 1986).

These "motives" affect the impact of public transfer on privates. If "altruism" prevails, public transfers may be offset by the reduction of private transfers; while in case of an "exchange motive" this would not be the case. Cox et al. (1987) shows that the "exchange motive" is prevalent for private transfers. This result was further confirmed relying on data from Peru showing that public transfers, not only do not crowd-out the private ones but, may reinforce them (Cox et al. 1998). Oruč (2011) confirms that social transfers are able to crowd-in remittances in Bosnia Herzegovina. Also, Maluccio and Flores (2005) do not find a significant effect of conditional cash transfers on remittances in Nicaragua while Jensen (2003) found in South Africa an opposite evidence showing that private transfers decrease of around 25% in response to public transfers.

3. The Juntos Programme[16]

Juntos is a Conditional Cash Transfer (CCT) programme implemented in 2005 by the Government of Peru to reduce poverty and improve education and health status (Perova and Vakis 2009).

Juntos beneficiaries are selected through: (i) a geographical targeting (selects districts according to exposure to violence; poverty; children malnutrition); (ii) a proxy means test (which identifies poor households) combined with a categorical targeting (selecting only households with pregnant women or children aged less than 14[1]); (iii) a community-based targeting (to refine the list produced by the other targeting mechanisms) (Perova and Vakis 2009).

The conditionalities attached to the transfer require households: at least 85% of school attendance during the school year for children in school age and regular health check-up for children under 5 and pregnant women (Perova and Vakis 2009; Alcázar and Espinoza 2014). According to the official data reported by the Juntos Team (2017) in the first two months of 2017, 97.3% of targeted households complied with the required conditionalities at least in one of the two months.

The transfer amount is 100 Peruvian Nuevos Soles (around 30 USD) and is delivered monthly through bank deposits or with armored van in remote areas. Even though the transfer is addressed to the household, the person entitled to collect the benefits are women. In some community the programme promotes also some accompanying measures, such as awareness seminars on nutrition, family practices, health, sanitation, literacy, and productive activities.

Juntos was first implemented in the poorest areas and then extended to other areas. The programme was not designed only for rural population but, in its initial stage, was concentrated mainly in these areas because they were the poorest ones (Trivelli & Díaz 2010). The concentration of the programme in these areas matters for the study of migration because rural districts are also the place of origin of the internal migration in Peru (as showed in Figure 1). An impact on poverty reduction, utilization of health services, improvement of nutritional intake and increase in primary education was detected (Perova and Vakis 2009). However, to the best of our knowledge, there are no studies about Juntos and internal migration in Peru.

[16] This section, which describes the programme, is based on the analogous section of Chapter 1

4. Data

To develop the analysis, we rely on the Young Lives database (which was described in detail in Chapter 1 and briefly presented here). In order to have a sample of comparable households (both poor and better-off), the Young Lives team implemented a multi-stage sampling strategy selecting sites located in poor areas (relying on a national poverty map) and then randomly selecting small geographic areas and then households and children (Young Lives Method Guide, 2011).

The coverage of rural, urban, peri-urban and Amazonian areas was ensured. Young Lives Database was not intended to be nationally representative, however, the Peru survey was compared with other surveys showing that households poverty rates were similar to the ones of ENAHO 2001 but slightly better-off than the ones surveyed by DHS 2000 in terms of access to health and prenatal care services (Escobal and Flores, 2008). Therefore, the survey can be considered representative enough to conduct the analysis. Data were collected in 4 rounds: Round 1 was conducted in 2002, when the Juntos programme was still not in place, Round 2 in 2006, but still did not contain questions about households' participation in the programme, Round 3 (conducted in 2009) and Round 4 (in 2013) contain questions on the status of beneficiaries/non beneficiaries. In each round 2,766 households were surveyed. There are two types of households in the Young Lives questionnaire. Those having a surveyed child aged 1 year old during the first round (Younger Cohort) and those having a surveyed child aged 8 years during the first round (Older Cohort).

a. Young Lives Data about Migration

The advantage to use this data to study internal migration consists in the fact that Young Lives keeps track of all children in the cohort, even if they change location. The tracking system is based on the collection of names and address of two contacts within the community but outside the household to ask information in case that the household move to a new location. Between rounds fieldworkers verify each household location and look for the new location in case of migration (Young Lives,2011).

We built a variable for migration which is based on the change in the place of residence reported by field worker. The place of residence is at household level,

therefore we consider the household as the decision-making unit for migration. In the questionnaire there are also questions about migration at household and child level. At the household level, the questionnaire asks households if they moved between Round 2 and Round 3, however this information is not perfectly consistent with migration dummy we built basing on the change in the place code recorded by the fieldworkers who visited the household[17]. This could be due to the fact that households moved between Round 2 and Round 3 but then they came back again to the place of origin. For this reason, we consider more reliable the information directly documented by fieldworkers who visited the place where the household is actually living. Additionally, we are interested in capturing households migration between the baseline and Round 3, instead of looking only at migration between Round 2 (when the programme was already announced) and Round 3. Before starting our analysis, we show some descriptive statistics about self-reported migration and migration documented by fieldworkers (see Table 1) between Round 1 (2002) and Round 3 (2009).

Table 1: Rural Urban migration Between Round 1 and Round 3

	Migrant households (%)	Beneficiaries migrant households (%)	Non-beneficiaries migrant households (%)
Rural-Urban migration	5.05	4.81	5.32

5. Empirical analysis

The objective of our analysis is to study the impact of Juntos on households' decision to migrate. In order to study this issue, we need to identify a reliable counterfactual. In absence of experimental data where households are randomly selected into the programme, we address the issue of the selection bias relying on quasi-experimental methods. In particular, we want to ensure that the treated group is compared with a group that is similar on average to avoid that permanent differences across the two groups may influence the outcome biasing the real effect of the treatment. Before to

[17] Between Round 2 and Round 3, 19.23% of the households head reported to have migrated, while according to fieldworkers 17.33% moved. Self-reported migration alone does not allow us to disentangle rural-urban migration from other types of migration (urban-rural or rural-rural).

perform the analysis, we checked whether there could be a reverse causality between migration and Juntos participation.

Therefore, we checked whether people moved to departments targeted by the programme in order to be selected as beneficiaries. From the descriptive statistics we know that a very low percentage of households move to departments targeted by Juntos and very few migrant households were then selected by the programme (see Table 2). Moreover, the correlation between migration and Juntos participation is negative and very low, therefore we can exclude the existence of a reverse causality.

Table 2: Migration to Juntos Departments

	From 2002 to 2006	**From 2006 to 2009**
Move to a Juntos Department	17 households (0.67% of the sample)	57 households (2.2% of the sample)
Selected by Juntos in Round 3	2 households	8 households

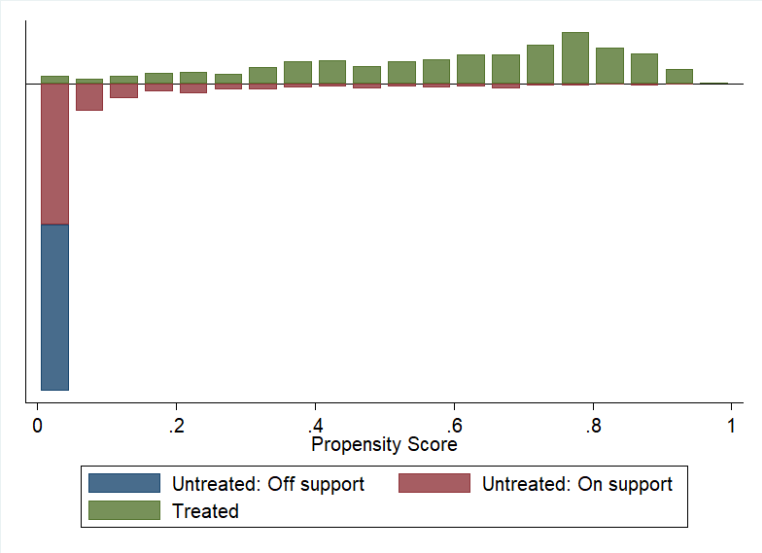
a. Methods

In order to address the issue of the selection bias we rely on a quasi-experimental technique, the Propensity Score Matching, firstly introduced by Rubin (1974), Rosenbaum and Rubin (1983, 1985). Through the propensity score, our objective is to select a control group that is similar to the treatment group in all the relevant preprogramme characteristics. To implement the Propensity Score Matching we rely on the Conditional Independence Assumption, assuming that we can observe variables that influence the selection into treatment and the outcomes of interest (Caliendo and Kopeinig, 2005). Given the structure of the data, we can rely on pre-programme characteristics to build the propensity score, since the variables included in the matching have to be measured before the programme or to be time invariant variables not affected by the treatment. Therefore, we calculate the probability of being beneficiary in Round 3 given a set of baseline characteristics required by the targeting criteria and some other households' demographic and educational characteristics. As reported in Section 3, the targeting strategy of Juntos includes geographical targeting, categorical targeting, proxy-means test and community-based targeting. To take into account the variables used for the geographical targeting, we calculate and include in the propensity score the percentage of crime and of stunting

children registered in each community. Then, to account for the categorical component of the targeting strategy, we include a dummy equal to one when, within the household, there is at least one child aged under 7 in 2002, so that in 2009 the household could be still eligible for the programme having at least one child aged less than 14. Then we also rely on proxies of the household poverty level such as the wealth index (which includes the housing quality index, consumer durables index and access to services). Finally, we include other characteristics, such as: the household size, the educational level of children caregiver, the region of origin, the age and sex of the household head (see Appendix 2.1 for propensity score estimates).

The balancing property and the common support assumptions are respected. However, through a visual analysis (see Figure 2) we found that the density in the right tail of the propensity score distribution is very thin for the control group.

Figure 2: Propensity Score distribution for Treated and Controls



We check the actual distribution of treated and control units in Table 3, showing that for high values of the propensity score (which in the last block of the distribution correspond to propensity score higher than 0.83), the common support assumption is respected (since we have 19 controls against 63 treated) but is weak. To overcome similar issues, Smith and Todd (2005) suggest using a trimming procedure to determine the common support region, by selecting only observations with a probability which exceed a given threshold by a certain amount. Hence, to allow a stronger level of comparability between the treated and the control group, we decided

to trim the common support region by excluding observations with a propensity score higher than 0.83 (corresponding to the last block of observations calculated through the propensity score).

Table 3: Common Support Assumption for high values of the propensity score

JUNTOS	P. Score<0.83	P. Score>0.83	Total
N. Controls	1255	**19**	1274
N. Treated	358	**63**	421
Total	1613	**82**	1695

After the implementation of the propensity score, we show that the two groups are not significantly different on average (Table 4), therefore we can rely on a reliable counterfactual for our treated observations.

Table 4: Comparison between Treated and Untreated after the Propensity Score Matching

	Mean in Treated	Mean in Untreated	p-value for diff.
Rural area=1	0.80	0.82	0.537
Percentage of households reporting crime in the community	0.08	0.08	0.479
Percentage of households reporting stunting children in the community	0.48	0.48	0.625
Presence of children under 7 in the household=1	0.99	0.99	0.956
Wealth Index	0.22	0.21	0.430
Household size	6.14	6.17	0.881
Caregiver is literate=1	0.30	0.30	0.831
Sierra Region	1.00	0.97	0.445
Selva Region	0.00	0.01	0.758
HH Head Sex	0.89	0.89	0.991
HH Head Age	35.53	35.98	0.615

Having addressed the issue of the selection-bias, we estimate the Average Treatment effect on Treated (ATT) for Round 3:

$$\boldsymbol{ATT = E(Y_1 - Y_0 | T = 1)}$$

where, Y is a dummy variable equal to one when the household moves from rural to urban areas and zero otherwise (e.g. when the household does not move between rounds or moves from urban to rural areas). This variable was built relying on the changes in the place of residence registered between the survey rounds. This data is reported by field worker who, thanks to a tracking system, may find and interview household also if they change locality. In our sample, 5% of the households migrate from rural to urban areas between Round 1 and Round 3. To calculate the ATT we rely on different matching algorithms: the Nearest Neighbour matching (each treated individual is compared to the closest untreated individual in terms of propensity score); the stratification matching (that compare the outcome mean of treated and untreated individuals within each block of observations); and the Kernel Matching (that rely on the weighted average of all the untreated individuals assigning more weight to comparison units similar to the treated units). The choice about the algorithm to use may be important, in particular in cases of a small sample (particularly, with few control units), when there could be a trade-off between bias and variance which may lead to different results across methods (Caliendo et al. 2005). Instead, in case of large samples, all PSM estimators should bring to similar results as they are more able to identify and compare only exact matches (Smith 2000). Indeed, in our case we implement three different methods and results are very robust across them.

6. Results

In Table 5 we show that the Average Treatment effect on the Treated coefficient is negative, meaning that the programme discourages migration from rural to urban areas and results are very robust using different matching methods (namely: Nearest Neighbor; Stratification; and Kernel Matching). As explained in Section 4, Young Lives keeps track of all children even if they change location thanks to a tracking system. Therefore, our outcome variable for migration is based on the change in the place of residence, reported by field worker, which is at household level. Therefore, in our analysis we consider the household as the decision-making unit for migration. Coefficients are negative and significant at 0.1%, indeed beneficiary households are significantly less likely to move from rural to urban areas with respect to non-beneficiaries.

Table 5: Programme impact on the decision to migrate from rural to urban areas

Method	ATT	S.E.	N. Treated	N. Controls
Nearest Neighbor Method (random draw version)	-0.156***	0.042	358	162
Stratification Method	-0.159***	0.039	358	1255
Kernel Matching Method (bootstrapped standard errors)	-0.148***	0.033	358	1255

ATT: Average Treatment effect on Treated
* p<0.05, ** p<0.01, *** p<0.001

Households migrate from rural to urban areas if the expected income in the destination place, net of the cost to migrate, is higher than the income in origin community. An increase in households' income due to a cash transfer provided by the government reduces the gap between the current household income and the expected net gain in the destination country. Indeed, conditional cash transfer programmes increases the opportunity cost to migrate providing an additional source of income for beneficiaries. The increase in the opportunity-cost to migrate is possible if cash transfers do not crowd-out private transfers. Conversely, if public transfers crowd-out private transfers the increase in income due to cash transfers is offset by the loss of private transfers and in this case, we could expect that the programme does not produce any impact on migration flows. To confirm this hypothesis, we also check whether Juntos may crowd-out private transfers. As expected, and coherently with Cox (1987), in Table 6 we show that Juntos does not seem to have a significant effect on private transfers, confirming that they reduce the probability to migrate by increasing its opportunity-cost.

Table 6: Programme impact on private transfers

Method	ATT	S.E.	N. Treated	N. Controls
Nearest Neighbor Method (random draw version)	-0.001	0.033	414	157
Stratification Method	0.016	0.023	414	570
Kernel Matching Method (bootstrapped standard errors)	0.014	0.026	414	570

ATT: Average Treatment effect on Treated
* p<0.05, ** p<0.01, *** p<0.001

Still, one could argue that conditional cash transfers may deter migration also through conditionalities, which requires children and a caregiver to stay in the place of origin to attend health check-up and school to receive the transfer. If this would be the case, Juntos should deter only migration of children and caregiver but not the one of other household members, who can use part of the transfer to fund migration cost. Conversely, if the increase in the opportunity cost of moving is the leading reason pushing households to avoid migration we can expect that the programme is able to deter not only migration of the whole household (as showed in Table 5) but also migration of single household members. To confirm our hypothesis, we perform an additional analysis on the probability that at least one household member, who was present in Round 1 and Round 2, leaves the household in Round 3. We focus our analysis only on working age members (aged between 18 and 65 in Round 3) excluding people who died and labourer, tenant or servant who were living within the household. It is worth noting that results must be read with cautions since the sample include both people who migrate and people who leaves the household for other reason (e.g. to form a new household). Unfortunately, we cannot disentangle these two motives due to data limitations.

Results, reported in Table 7, confirm our hypothesis showing that the programme negatively affects also the probability that at least one working age household member leaves the household.

Table 7: Programme impact on the probability that at least one working age member leaves the household

Method	ATT	S.E.	N. Treated	N. Controls
Nearest Neighbor Method (random draw version)	-0.143 **	0.047	414	160
Stratification Method	-0.101*	0.043	414	570
Kernel Matching Method (bootstrapped standard errors)	-0.084*	0.038	414	570

ATT: Average Treatment effect on Treated
* p<0.05, ** p<0.01, *** p<0.001

7. Robustness Check

The first robustness check consists in testing our hypothesis only on the Sierra region, since 99% of beneficiaries live there. We confirm that Juntos discourages migration from rural to urban areas (see Table 8).

Table 8: Programme impact on the decision to migrate from rural to urban areas (Sierra region only)

Method	ATT	S.E.	N. Treated	N. Controls
Nearest Neighbor Method (random draw version)	-0.162***	0.038	414	160
Stratification Method	-0.145 ***	0.042	414	570
Kernel Matching Method (bootstrapped standard errors)	-0.127***	0.032	414	570

ATT: Average Treatment effect on Treated
* p<0.05, ** p<0.01, *** p<0.001

Then, we also check whether results are confirmed using outcome variables from Round 4. In particular, we compare households in the common support who were beneficiaries in both Round 3 and Round 4 against people who were never beneficiaries (see Table 9). Relying on data from Round 4, we confirm that the Juntos programme has a negative and significant effect on the households' probability to migrate from rural to urban areas.

Table 9: Programme impact on the decision to migrate from rural to urban areas – Round 4

Method	ATT	S.E.	N. Treated	N. Controls
Nearest Neighbor Method (random draw version)	-0.181**	0.057	298	129
Stratification Method	-0.181***	0.051	298	490
Kernel Matching Method (bootstrapped standard errors)	-0.196***	0.043	298	490

ATT: Average Treatment effect on Treated
* p<0.05, ** p<0.01, *** p<0.001

8. Conclusion

In this paper we investigate whether welfare programmes affect internal migration in Peru. In particular, we analyse the impact of a conditional cash transfer programme, Juntos, on the decision to migrate from rural to urban areas. The internal migration is an important issue in Peru and at the global level, since there is a massive migration from rural to urban areas that is likely to continue also in the future.

Through Propensity Score Matching methods, we found that beneficiary households are significantly less likely to migrate with respect to similar non-beneficiaries. Indeed, the Juntos programme discourages households to leave the rural setting and to move to urban areas. One mechanism through which Juntos may negatively affect migration is the increase in the opportunity cost of moving. This mechanism is possible only if cash transfers do not crowd-out private transfers. Conversely, if public transfers crowd-out private transfers the increase in income due to cash transfers is offset by the loss of private transfers and, in this case, the programme may not produce any impact on migration flows. We show that Juntos deters migration by increasing the opportunity cost to migrate, since it does not crowd-out private transfers allowing a real increase in household income.

Conditionalities require school attendance and regular health check-up in the place of origin, hence may deter migration for children and caregiver who are the target individuals required to accomplish with conditionalities. However, they are not supposed to affect migration decisions of other household members who can decide to use part of the transfer to fund the migration cost. To confirm our hypothesis that the increase in the opportunity cost is the leading reason pushing individuals to avoid migration we show that the programme is able to deter not only migration of the whole household (which can be constrained by conditionalities) but also migration of single household members who are not constrained by conditionalities.

The main results are robust also using outcome variables from another survey round, showing also a sustained impact of the programme over time. Our study confirms the hypothesis that the introduction of welfare interventions in the place of origin produce better economic conditions, which may allow the households not to migrate but to stay in the place of origin. This finding may have concrete policy implications

only for settings and cases where the main reason to migrate can be identified in economics constraints. There are cases when migration is motivated by conflicts in the place of origin, in this case the reasons that push households to migrate are not economic and, additionally, in these settings there are several difficulties even in financing and delivering social protection benefits. In cases of migration motivated by climate changes, social transfers may support the households but in the long-run climate shocks affect, not only the economic conditions, but also working opportunities and social interactions.

The reduction of rural-urban migration *per se* cannot be judged as a positive or a negative phenomenon but its impact on poverty may have strong implications in developing countries. Recently, Imai et al. (2017) showed that the increased population in mega cities and in secondary towns seem not to be associated with poverty reduction, while policies and investments in agricultural and non-agricultural sectors for rural areas may have a stronger impact on poverty. Our research shows that welfare interventions may make migration a choice, by increasing households' income allowing them to decide freely where to leave and to stay in the place of origin with better economic conditions. Indeed, this finding has important policy implications in cases when migration is motivated by economic reasons and when all the other social, political and environmental conditions are stable.

Appendix 2.1

Table 2.1: Propensity Score Estimates

	Juntos (Beneficiary=1 in Round 3)
Area of residence (rural=1)	0.723**
	(0.189)
Percentage of households reporting crimes in each community	-0.690**
	(1.665)
Percentage of stunted children in the community	1.892***
	(0.589)
Presence of children under 7 (at baseline) within the household	0.522
	(0.516)
Wealth index	-2.749***
	(0.601)
Household size	0.386
	(0.0393)
Caregiver is literate=1	-1.009***
	(0.165)
Sierra region	5.277***
	(1.018)
Selva region	-1.617
	(1.432)
Sex of household head	-0.184
	(0.245)
Age of household head	-0.545*
	(0.00719)
N	2,550

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Chapter 3

Conditional Cash Transfers and Aspirations in Peru

Abstract

The aim of this chapter is to provide an impact evaluation of the Peruvian conditional cash transfer programme, Juntos, on children aspirations related to education and future work opportunities. Relying on Inverse Probability Weighting, we show that the programme positively and significantly affects children. We show that the programme does not increase children aspirations through an economic channel. Conversely, we identify a psychosocial effect showing that Juntos has a stronger effect for beneficiary children who feel included in the community and at school with respect to children who perceive stigma.

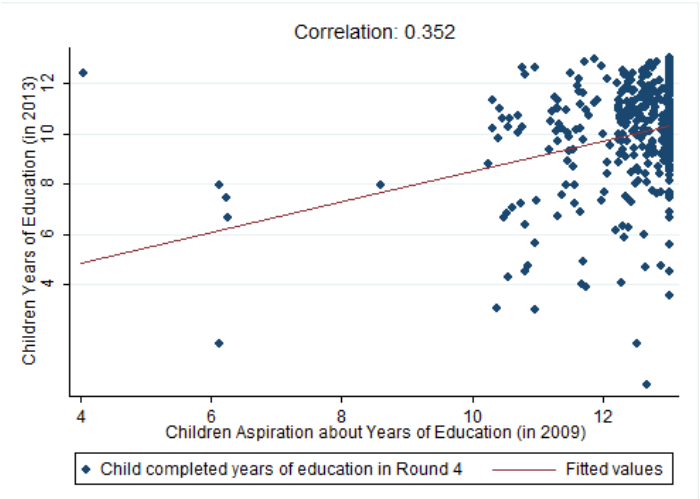
Keywords: Conditional Cash Transfers; Impact Evaluation; Aspirations; Peru; Juntos.

1. Introduction

Aspirations are commonly defined as desires, as something that people hope to achieve. In Appadurai's (2004) view, aspirations are strongly affected by the level of well-being and poor people may lack the "capacity to aspire". At the same time, low aspirations may lead individuals to remain trapped in poverty, creating a vicious cycle (Ray 2006). According to Dalton et al. (2016) "the aspiration failure is a consequence of poverty, rather than a cause". The "aspiration failure" of poor people, is due to the fact they face higher risk and costs of efforts than rich people, therefore they make less effort to realize their initial aspirations with the consequence of lower expected marginal benefits, which in turn make their initial aspirations diverging from the one of better-off people.

Why do aspirations matter? They have an intrinsic value because, in particular for children, they represent the freedom to desire. Additionally, parents and children aspirations about education and future work may concretely affect current individuals' preferences for school. There is evidence of the fact that educational aspirations positively affect years of schooling and educational attainment (Serneels and Dercon, 2014; Chiapa et al. 2012). Using the Young Lives survey in Peru, we show that children educational aspirations are positively correlated to real outcome and, in particular, to the grade (year of education) completed by children four years later (see Figure 1).

Figure 1: Correlation between educational aspirations and outcomes



Note: This Figure shows the correlation between children aspiration about years of education in 2009 and actual years of education reached four years later, in 2013. The coding for the years of education can be found in Annex 3.1. The sample of children analyzed in this figure was aged 14/15 years *in 2009.*

According to Ray (2006), aspirations affect behaviours depending on the size of the "aspiration gap", this is the difference between the current individual status and the one to which the individual aspires to. An individual may change behaviour and make some kind of investment in order to reduce the "aspiration gap", but this works only when the gap is not too small or too large (because when the gap is too small individuals would not put effort to achieve a status similar to the current one; while when the gap is too large, despite the effort, it would remain too huge to overcome). Ross (2017) provided some empirical support to the "aspiration gap" theory. Using data from India he found an inverted-U relationship between the children "gap" in work aspirations and the real education outcomes.

Aspirations are shaped by individual characteristics (such as: optimistic or pessimistic attitude, ethnicity, sense of identity) but they are likely not to be constant over time and to vary according to personal experiences and social changes. Aspirations changes may be determined by modifications in the awareness of its own self-perception (Mukherjee 2015), by inspirational experiences (Bernard et al. 2014, Wydick 2013) or by social programmes. Genicot and Ray (2017) model how social outcomes external to the individual may affect aspirations, and show that aspirations evolves jointly, not only with income, but also with its distribution.

Studying what may affect initial aspirations is relevant from a policy perspective because what people desire changes over time and has an impact on effort and achievements. The aim of this paper is to study whether conditional cash transfers may affect children and adolescents' aspirations related to future education and work. Relying on Young Lives data and implementing Inverse Probability Weighting, we show that the Peruvian conditional cash transfer programme, Juntos, positively affects children aspirations.

Conditional cash transfers are at the core of social protection policies in many developing countries and their main objective is to improve education, health, and reduce child labour. If sending children to school would be efficient, parents should decide to do it even without the enforcement of conditionalities but there are several reasons why parents may underinvest in education (imperfect information about the returns to education, low will-power in assessing the future utility derived by current choices, low altruism level due to the impossibility to enforce reciprocity when they

provide informal transfers to their children) (Fiszbein et al. 2009). These distortions may justify government intervention in the economy in view of the private optimum of each family but also of welfare maximization at the aggregate level. Unconditional cash transfers generate an "income effect" for the household which avoid the implementation of detrimental risk coping strategies (as child drop-out from school in case of negative shocks), while the attachment of a conditionality to the cash transfer may produce also a "substitution effect", because remunerating education lowers the opportunity cost of studying with respect to work (ibid.). Thus, conditional cash transfers create an economic incentive for households to invest in education. From an economic perspective, this is the main channel through which conditional cash transfers affect education and, in turn, future work opportunities. In our paper, we consider that parents may underinvest in children education, not only for the aforementioned economic reasons, but also because they may "lack the capacity to aspire" to a better future for their children.

In our main analysis we check whether conditional cash transfers affect aspirations but then we also try to shed a light on the mechanisms and the channels through which this process takes place. In particular, we focus on two channels: the "economic channel" and the programme "psychosocial channel". For what concerns the "economic channel", the injection of liquidity within the households increases income and improves the household well-being; this in turn may reduce the wealth gap between the poor and the better-off. According to Dalton et al. (2016), poor people, with respect to rich people, experience an "aspiration failure" and have lower aspirations because the expected marginal benefit of the effort to reach their aspirations is lower, due to the fact that they face higher risk and costs of efforts. Stringent initial external constraints (such as low wealth) determine low effort-low aspirations (ibid.). In our view, conditional cash transfers, through the injection of liquidity within the household and the provision of a secure source of income, may reduce the wealth gap between poor and better-off and alter the propensity to make some kind of effort (or investment)[18].

[18] See Bastagli et al. 2016, for a review of the impact of CCTs on different kinds of investments.

In turn, this affects the initial gap between poor and the rich people aspirations reducing the likelihood of an "aspiration failure"[19].

We also focus on the programme "psychosocial channel", since being part of the programme may have two opposite effects on stigma, well summarized by Roelen (2017): (i) cash transfers can reduce the stigma by improving mental health (Samuels and Stavropoulou 2016), reducing the need for private loans (Berhane et al. 2012) or by allowing children to have new clothes and to be cleaner (Adato et al 2016). This improvement in people life may make them more prone to aspire; (ii) cash transfers, on the other hand, may trigger a negative effect on stigma due to: the targeting mechanism if it requires that people publicly reveal their intent to participate to the programme (Coady et al. 2004); to the payment mechanism, if it requires people to queue in front of the payment office (Bastagli et al. 2016); to the behaviour of the programme staff and of the rest of the community (Wright et al. 2014). In this case revealing and priming the fact of being poor in front of the community may negatively affect people aspirations (Mukherjee 2015).[20] Through these mechanisms conditional cash transfers may affect people capacity to aspire. This paper contributes to the recent literature about the role of aspirations in poor settings and provide additional evidence of the fact that they are endogenous to economic, social and psychological changes. To the best of our knowledge, this is the first paper assessing the impact of Juntos on Peruvian children aspirations. Most importantly we explore the economic and the psychosocial mechanisms through which conditional cash transfers may affect aspirations. This recent strand of literature may have important policy implications, since it highlights that the impact of conditional cash transfers on schooling and human capital may be amplified also by changes in children aspirations.

The remainder of the paper is organized as follows. In section 2 we review the literature about cash transfers and aspirations; in section 3 we describe the Juntos programme; in section 4 we present the Young lives data and their value added in terms of information about children aspirations; in section 5 we explain the method

[19] It is worth noting that, Dalton et al (2016) identify one standard and one behavioural poverty trap and for some ranges of initial wealth the poverty trap can be broken by acting only on aspirations. However, for very low levels of wealth also material constraints should be addressed.

used for the analysis; section 6 shows the main findings of this paper and section 7 concludes.

2. Literature Review

Aspirations are shaped by individual characteristics but can also be affected by life experiences and by social and economic changes. The modifications in the awareness of its own self-perception, the exposure to inspirational experiences or to certain models may contribute to form people aspirations during the life-cycle. Mukherjee (2015) showed that aspirations of Indian people changed after a psychological exercise based on identity priming. Also, aspirational models or movies may produce effects on aspirations formation. Bernard et al. (2014) showed that people who watched an "aspirational" documentary, about successful stories of similar people working in agriculture or small business, experienced an increase in aspirations which was then translated in forward-looking behaviours (savings and investments in children education). This effect persisted even five years after the intervention, when in treated villages the level of aspirations, the percentage of children enrolled at school and the number of owned assets, was higher with respect to the control villages (Bernard et al. 2017). In Uganda, students who watched the aspirational "Queen of Katwe" movie, before the math test, performed better than students who watched a placebo movie and this result was stronger for girls and lower performing students (Riley 2018). Beaman et al. (2012), show that reserving leadership positions for women, in randomly selected Indian villages, increased girls' aspirations due to a role model effect. Indeed, adolescent girls, who live in villages where there is a female leader, raise their aspirations reducing the gender aspirations gap.

Besides experiences that alter the psychological self-perception, also awareness and inclusion activities may affect people aspirations. For instance, in India the provision of information about new economic sectors and the opportunities of news jobs in those sectors influenced women aspirations regarding their career and push them to study longer (Jensen 2012). Furthermore, both Wydick et al. (2013) and Glewwe et al. (2015), found that child sponsorship programmes influence the capacity to aspire, which in turn positively affect years of schooling, school completion, and the quality of future employment.

Finally, also social programmes, such as conditional cash transfer programmes may have an impact on people aspirations. This channel has been only recently highlighted and the literature is still scarce. However, most studies find a positive effect on parents and children aspirations. Chiapa et al. (2012), analyse the impact of the PROGRESA programme on parents' educational aspirations for their children in terms of years of schooling, and they found that the programme is significantly associated with an increase in educational aspirations in terms of year of schooling. They also studied one of the "conditionalities effect" on aspirations, and in particular the fact that households with very young children must attend regular health check-up, so they were exposed to highly educated professionals, which seems to positively affect the desired school years. Additionally, they showed that parents aspirations for their children to complete "college" increase thanks to the programme and these results depend, partially, on the "conditionality effect" of exposure to educated professionals. This result is in line with the one of Macours and Vakis (2014) who showed that, in Nicaragua, conditional cash transfer beneficiaries exposed to social interactions with programme female leaders increased their aspirations for the future. The authors find that this effect was driven by the fact that people were exposed to positive and successful examples.

The case of the Colombian CCT, Familias en Accion, was studied by Contreras Suarez and Cameron (2016) and Garcia et al. (2016), with contrasting results. Contreras Suarez and Cameron (2016) did not find a programme effect on aspirations while Garcia et al. (2016) find a positive programme impact to attain post-secondary education, in particular for the poorest, more pessimistic and less educated households. The difference between these results was explained on the ground that the former studied the long-term effect of the programme, while the latter analysed the short-term programme impact basing the study on a time span when the programme included also workshops for beneficiaries (that allow interactions with educated professionals), which were then changed or removed some years after the programme started (Garcia et al. 2016).

Other authors analyse the impact of unconditional cash transfers, microfinance, or cash-for-work programmes and find that also these programmes are able to produce an impact on aspirations. Gebremariam et al. (2017) analyse the Productive Safety Net Programme in Ethiopia and find that the intervention positively affects children

aspirations about the years of schooling. In Mexico, also microfinance loans for indigenous people had a positive impact on hope and aspirations (Lybbert and Wydick, 2016). Ross (2017) find that the India's National Rural Employment Guarantee Act (NREGA) increases the level of aspirations of Indian children but is not able to significantly affect the size of the "aspiration gap" in a way to affect the level of investment in human capital. Since these authors show that also unconditional cash transfers may have an effect on aspirations, in absence of experimental evidence and relying on the scarce existing literature, we could speculate that beyond the "conditionalities channel" also the economic and the programme inclusion channels may play a role in affecting aspirations.

3. The Juntos Programme[21]

Juntos is a Conditional Cash Transfer (CCT) programme first implemented in 2005 by the Government of Peru with the objective to reduce poverty in the short and in long-run, respectively by transferring cash to the households and improving education and health (through the conditionalities attached to cash transfers) (Perova and Vakis 2009). The programme was able to reach the intended effects and it has a positive impact on poverty reduction, health services, nutrition and education (ibid.). In 2018, the programme covers 765,380 households in 1,325 districts[22]. According to the qualitative evidence, some beneficiary reported that the conditionalities related to education were valued by parent as a way for their children to reach higher professional positions in the future (Juntos 2010)[23].

There are two different types of conditionalities that households are required to comply in order to receive the transfer; they apply differently according to the children's age. Children in school age should attend at least 85% of the school year, while children under 5 and pregnant women must attend regular health check-up (Perova and Vakis 2009; Alcázar and Espinoza 2014). According to the official data reported by the Juntos Team (2018) in the second quarter of 2018, 97.8% of

[21] This section, which describes the programme, is based on the analogous section of Chapter 1
[22] See http://www.juntos.gob.pe/modulos/mod_infojuntos/
[23] *"¿Qué fue lo que más le gustó de JUNTOS?* Desde el principio me gustó la educación…Para que supere más pe, para que rinde más en su estudio, que sea mañana, más tarde un profesional"(Beneficiary of Puncupata) (Juntos 2010). "sueño con que mi hija pueda ser doctora, mientras no me pase nada, yo apoyaré a mi hija para que salga adelante" (Juntos 2009).

households complied with the required conditionalities at least in one month. Households are selected by the programme through the implementation of different targeting mechanisms at both geographical and household level. First, the programme selects districts according to: exposure to violence; poverty level, measured as a proportion of population with unsatisfied basic needs; poverty gap; level of child malnutrition; and presence of extreme income poverty (Perova and Vakis 2009). Then, a proxy means test is combined with a categorical targeting to select poor and vulnerable households with pregnant women or children under 14[24]. Finally, a team in each community validated the lists produced by the other targeting mechanisms. A transfer of 100 Peruvian Nuevos Soles (around 30 USD) is delivered monthly, to the women within the households, through bank deposits or, in remote areas, with armored van. In some case, the programme promotes some accompanying measures, namely beneficiaries' participation in awareness seminars on nutrition, family practices, health, sanitation, literacy, and productive activities but, to our knowledge, these seminars are promoted only by some community on a voluntary basis.

4. Data

We use Young Lives Data (which was described in details in Chapter 1), a longitudinal households survey collected in four rounds; in each round 2,766 households were surveyed. In order to have a sample of comparable households (both poor and better-off), the Young Lives team implemented a multi-stage sampling strategy selecting sites located in poor areas and then randomly selecting small geographic areas, households and children (Young Lives Method Guide, 2011). There are two types of households in the Young Lives questionnaire: those having a surveyed child aged 1 year old during the first round (Younger Cohort) and those having a surveyed child aged 8 years during the first round (Older Cohort). Round 1 was conducted in 2002, when the Juntos programme was still not in place, Round 2 in 2006, but still did not contain questions about households' participation in the programme, Round 3 (2009) and Round 4 (2013) embody questions on the status of beneficiaries/non-beneficiaries. Young Lives Database can be considered representative enough to conduct the analysis, since household's poverty rates were similar to the ones

[24] In 2012 the eligibility criteria slightly changed and define eligible also households with children aged less than 19 (Alcázar and Espinoza 2014).

reported by ENAHO 2001 and only slightly better-off (in terms of access to health and prenatal care services) than the ones surveyed by DHS 2000 (Escobal and Flores, 2008).

a. Young Lives Data on Aspirations

Young Lives collected a lot of information about children aspirations in Round 2, Round 3 and Round 4. For the main analysis, we rely on data from Round 3. In this Round, the younger cohort children (around 8 years old) were not surveyed about educational aspirations but only about work aspirations. According to Dalton (2016), initial aspirations may be formed also by the concrete possibilities to achieve a goal, therefore the distinction between aspirations and expectation may be not trivial. However, Young Lives questions about aspirations are formulated in a way to avoid confusion between aspirations and expectations (which attain more to what people would like to achieve given what they can achieve). In particular, the questionnaire asks children *"Imagine you had no constraints and could study for as long as you liked or go back to school if you have already left. What level of formal education would you like to complete?"* and *"What do you want to be when you grow up?"*[25].

We use two variables for educational aspirations, one related to the school years and one to the completed level of education one aspires to. The coding for work aspirations is based on Pasquier-Doumer and Rissa Brandon (2015) who, relying on data from ENAHO 2006, assigned a score to each occupational aspiration which is a 'linear combination of the average education level required by the occupation and the average income it generates'[26]. Descriptive statistics about children work and education aspirations, before the matching, show that aspirations between the treated and the control group were very similar (see Appendix 3.2).

5. Methods

In absence of experimental data, we implement quasi-experimental techniques. To remove the unobservable time invariant differences, the ideal solution would be to implement a difference-in-difference technique but unfortunately, the database does

[25] See Appendix 3.1 for more details about questions and variables coding.
[26] With respect to Pasquier-Doumer and Brandon (2015) we rely on three groups of work aspirations instead of four.

not contain questions about education aspirations in Round 1 and only few observations about the work aspirations of the older cohort. We could use Round 2 as a baseline, but at that time the programme was already announced and in place in some community (however, the number of households who are already programme beneficiaries was very low, so we cannot use Round 2 for the analysis). Given the nature of the data we decided to implement a semi-parametric method based on Inverse Probability Weighting. First, we calculate the probability of being beneficiaries in Round 3, given the pre-programme characteristics of Round 1, through a probit model. We define programme beneficiaries those who reported having received transfers for the programme in the last 12 months. Respectively through retrospective questions and variables cross-checking, we excluded households who received Juntos in the past (in Round 2) but are no longer beneficiaries and households who reported contradictory information. To allow a higher level of comparability between the treatment and the control group the analysis is based only on people living in the Sierra region, because among the households surveyed by Young Lives in 2002, 99% of Juntos beneficiaries live in this area in 2009 (Round 3), and the high concentration of beneficiaries found in the YL data is confirmed by the administrative data (according to the Ministerio de Desarrollo e Inclusión Social (2014), around 83% of the beneficiaries live in Sierra districts). Unfortunately, we cannot rely on differences in quotas during the programme implementation because all the Sierra departments were targeted by Juntos before 2009. First, we include the characteristics used for the geographical targeting: the percentage of crime and of stunting children registered in each community. Second, we control for the variables used to select households through the categorical targeting and the proxy means test: a dummy equal to one when, in 2002, there is at least one child aged under 7 in the household (so that in 2009 the household could be eligible for the programme having at least one child aged less than 14); and the wealth index (which includes the housing quality index, consumer durables index and access to services). Finally, we include other characteristics, such as: household size, educational level of children caregiver, children age, sex, first language, and children cohort. After the calculation of the propensity score[27], we

[27] Propensity score estimates are showed in Appendix 3.3

compute the Inverse Probability Weighting (IPW) and through an Hotelling test we show that the treated and the control group are not significantly different (Prob>F(11,1139)=0.1433). Additionally, we show that at baseline individuals in the treated and control group (weighted through the IPW) are not significantly different in the relevant characteristics (Table 1).

Table 1: Comparison between groups

	Mean in Treated	Mean in Untreated	P-value for difference
Rural area=1	0.804	0.838	0.231
Households reporting crime in the community	0.076	0.075	0.931
Stunting children in the community	0.486	0.508	0.182
Presence of children under 7 in the household=1	0.986	0.982	0.708
Wealth Index	0.216	0.207	0.484
Household size	6.139	6.011	0.617
Caregiver is literate=1	0.289	0.259	0.466
Child sex	0.496	0.525	0.627
Child Age	1.736	2.118	0.257
Child first language	0.404	0.401	0.958
Child age cohort	0.825	0.774	0.292

Then we perform an OLS model on Round 3, weighting observations through the Inverse Probability Weighting and clustering observations at the community level. The estimated model is the following:

$$\boldsymbol{Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon_{is}}$$

where Y_i are children aspirations, T_i is a dummy for the household treatment status; X_i is a vector of covariates including: wealth index; area of residence; household head years of education; household head age; child sex; ethnicity; child's age - in months; household size; dependency ratio; child age cohort; ε_{is} is the error term and it is clustered at community level.

6. Results

We find a positive and significant programme impact of Juntos on children aspirations related to years of education and completed level of education (Table 2)[28]. The two coefficients are similar in magnitude and significant respectively at 1% and 5%.

Table 2: Impact of Juntos on Education and Work Aspirations-Round 3

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations
	Older Cohort	Older Cohort	Both Cohorts
Juntos	0.164***	0.161**	0.040
	(0.118)	(0.112)	(0.0841)
Wealth index	0.180**	0.138	0.202***
	(0.733)	(0.630)	(0.423)
Area of residence (rural=1)	-0.001	-0.024	-0.114**
	(0.118)	(0.125)	(0.105)
Household head years of education	0.017	0.048	-0.006
	(0.0208)	(0.0197)	(0.0120)
Household head age	-0.083	-0.169	-0.024
	(0.00928)	(0.0103)	(0.00388)
Child sex (Male=1)	0.093	0.130	-0.115*
	(0.178)	(0.154)	(0.107)
Ethnicity (White=1)	0.059	0.056	-0.034
	(0.339)	(0.238)	(0.255)
Child's age - in months	-0.110	-0.063	0.666*
	(0.0327)	(0.0155)	(0.00866)
Household size	-0.251**	-0.273**	-0.110**
	(0.0567)	(0.0501)	(0.0243)
Dependency ratio	0.186**	0.231**	0.010
	(0.000994)	(0.001000)	(0.000732)
Child Age Cohort (Younger Cohort=1)			0.522
			(0.723)
N	250	251	1,021

Standardized beta coefficients; Standard errors in parentheses; Errors are clustered at the community level; The propensity score is introduced as weights in the regression; * p<0.10, ** p<0.05, *** p<0.01

[28] Appendix 3.4 shows also the impact of Juntos on parents aspirations which we do not report here since the focus of the paper is on children aspirations. Moreover, adding parental aspirations as a control variable to regressions on education and job aspirations, results are confirmed.

As explained in Section 4, the variables related to educational aspirations are available for the older cohort only, while it is possible to conduct the analysis on both cohorts for what concerns work aspirations.

The impact of Juntos on work aspirations is not significant, and we investigate whether this can be due to some cohort effects. In line with Dalton et al. view (2016), this may be due to the fact that people form their aspirations step-by-step. Children focus first on education and then on work, once the first objective is fixed, they start to create new aspirations for the following step. In Table 3 we find evidence in support of this hypothesis showing that the programme affects the older cohort's work aspirations but not the younger cohort's ones. This cohort effect can be explained also with the fact that the Older Cohort children experienced a change in their life (before and after the programme) during the adolescence (14/15 years old in Round 3), while the younger cohort started to receive the transfer when they were still very young to perceive changes in the household status (7/8years old). For this reason, we decided to test programme mechanisms and heterogeneous effects for the older cohort only (see next section).

Table 3: Cohort Effect

	Children job aspirations
Older Cohort	
Juntos	0.204***
	(0.109)
N	227
Younger Cohort	
Juntos	0.024
	(0.113)
N	794
P-value for difference	0.1556

Standardized beta coefficients; Standard errors in parentheses; Errors are clustered at the community level; The propensity score is introduced as weights in the regression; * p<0.10, ** p<0.05, *** p<0.01

The two coefficients reported in Table 3 are not significantly different but the p-value for the difference between the coefficients is extremely low. Programme Mechanisms

As reported in Section 1, conditional cash transfers may affect people capacity to aspire through two different channels: the "economic channel" and the "programme psychosocial channel". To test the "economic channel", we check whether the programme has a differential impact for children who, at the baseline, have a wealth

index below or above the median. Relying on our analysis we cannot identify an economic mechanism, the programme has not a differential impact for children whose wealth was below or above the median in Round 1 (Table 4). According to Dalton et al. (2016), two poverty traps must be taken into account when studying aspirations: the standard material poverty trap and the behavioural poverty trap. The initial wealth plays a role in the formation of aspirations since there are wealth levels so low that individuals may be caught in a poverty trap. However, in our case we cannot identify a material poverty trap which generates differential programme impact on aspirations formation (see Table 4).

Table 4: Economic mechanism

	Children educational aspirations (Years)	Index-Children educational aspirations (level of education)	Children job aspirations
Children with WI below the median of the community in Round 1, with controls			
Juntos	0.133*	0.165**	0.226***
	(0.173)	(0.129)	(0.101)
N	113	113	102
Children with WI above the median of the community in Round 1, with controls			
Juntos	0.367**	0.367**	0.347*
	(0.229)	(0.229)	(0.300)
N	137	137	124
P-value for difference	0.3910	0.1925	0.3651

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

In Dalton et al. (2016) view, policies should be accompanied by measures acting, not only on the standard poverty trap but also, on the behavioural poverty trap, which can also generate the aspiration failure. Conditional cash transfer may act on the behavioural poverty trap through: (i) conditionalities (which allows social interactions and exposure to role model); (ii) by reducing the stigma associated to the fact of being poor. As mentioned above, due to data limitations we cannot test the conditionality channel for Juntos[29]. However, we can analyse the "psychosocial

[29] Conditionalities may be seen as a measure which acts on aspirations by exposing children to role models, such as teachers and doctors. Within the Juntos programme, children aged less than 5 are required to attend regular health check-up while children aged more than 6 should attend school. According to Chiapa et al. (2012) the exposure to educated professionals (e.g. doctors and nurses) positively affected aspirations of mexican beneficiaries of the conditional cash transfer programme, PROGRESA.

channel" checking whether the effect of Juntos on aspirations is stronger for children who are less affected by stigma. While shame consists in a personal evaluation, stigma occurs through social interaction with other people (Bos et al. 2013). According to Roelen (2017) cash transfers may reduce the stigma associated with the status of poor by improving mental health (Samuels and Stavropoulou 2016), reducing the need for private loans (Berhane et al. 2012) or by allowing children to have new clothes (Adato et al 2016). As a proxy of stigma we decided to rely on an Inclusion Index to check whether the programme has a stronger impact for children who feel more included at school and in the community. Following Dercon and Singh (2013), we build an index relying on the following questions addressed to children: *"The other children in my class treat me with respect"; "Pupils in my class never tease me at school"; "Adults in my community treat me as well as they treat other children at my age".* Children answers are on Likert-type scales ranging from 1 to 5 (from being "strongly disagree" to "strongly agree"). We create the index by summing the score from each single answer, then we calculate the median value of the index for all children and finally we divide the sample according to whether children have an Inclusion Index below or above the median. From Table 5, we can identify a psychological effect of the programme showing that Juntos has a stronger effect for beneficiary children who, after the start of the programme, feel included in the community and at school with respect to children who feel stigma.

Table 5: Stigma Mechanism

	Children educational aspirations (Years)	Index-Children educational aspirations (level of education)	Children job aspirations
Children with inclusion index below the median, with controls			
Juntos	-0.110	-0.110	0.002
	(0.278)	(0.278)	(0.241)
N	47	47	44
Children with inclusion index above the median, with controls			
Juntos	0.154**	0.158**	0.208**
	(0.133)	(0.114)	(0.123)
N	203	204	183
P-value for difference	0.5643	0.7296	0.8612

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

For children for which stigma persist even after the programme implementation and inclusion is low, Juntos is not able to significantly affect aspirations. Conversely, for children who do not suffer from stigma the programme significantly and positively affects aspirations. Therefore, the programme impact seems to be mediated by a psychosocial mechanism but not by the economic mechanism.

Additionally, we analyse whether there are other heterogeneous programme effects due to other children characteristics, such as sex and area of residence (See Appendix 3.5). We find that the programme has a stronger effect on girls, however coefficients are not significantly different from those of boys. We do not find that the programme differently affects children living in urban or rural areas.

a. Long Term Programme Effect

Finally, we use Round 4 to assess the long-term impact of the programme over time and the robustness of our results. In particular, using outcome variables from Round 4, we compare older cohort children who are beneficiaries in both Round 3 and Round 4 against children who were never beneficiaries. Using outcome and control variables from Round 4 we confirm results from our main estimation and show that there is a sustained impact of the programme over time. This finding is in line with Bernard et al. (2017) who show that the effect of an "aspirational documentary" persisted even five years after the intervention. Table 6 reveals that there is not only a short-term effect of being part of the programme but the psychosocial effect persists along the years and this may have concrete implications in terms of human capital investment and educational outcomes (Serneels and Dercon, 2014; Chiapa et al. 2012).

Table 6: Impact of Juntos on Education and Work Aspirations-Round 4 (with controls)

	Children educational aspirations (Years)	Index-Children educational aspirations (level of education)	Children job aspirations
Juntos	0.149	0.213***	0.200*
	(0.279)	(0.126)	(0.157)
N	196	199	157

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

7. Robustness check

To check the robustness of our results, we implement different tests. First, we apply Propensity Score Matching estimations using three different methods of matching (namely: Nearest Neighbor; Stratification; Kernel matching) to estimate the average treatment effect on the treated. Both the results of the main estimation (showed in Table 2) and the cohort effect are confirmed in Table 7.

Table 7: Average Treatment effect on the Treated

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations	Children job aspirations
	Older Cohort	Older Cohort	Both Cohorts	Older Cohort
Nearest Neighbor Matching method (random draw version)	0.468* (0.284)	0.385* (0.192)	0.025 (0.111)	0.517*** (0.193)
Stratification method	0.415*** (0.109)	0.300*** (0.092)	0.031 (0.090)	0.276* (0.153)
Kernel Matching method (Bootstrapped standard errors)	0.405** (0.198)	0.309* (0.159)	0.009 (0.089)	0.277 * (0.157)

ATT; Standard errors in parentheses; * p<0.10, ** p<0.05, *** p<0.01

As a second robustness check, we implement the OLS analysis with IPW for the older cohort sample trimmed according to the probability of being selected by the programme. We exclude from the sample people with a probability of being selected by the programme lower than 0.15% and with a probability higher than 0.95%.

Trimming the sample, we exclude beneficiaries and non-beneficiaries with very different probability of being selected by the programme, so we perform the analysis on a sample with a very similar probability of being selected by the programme. In Table 8, we show that results are robust to this test and that the programme positively and significantly affects children aspirations related to education and work. Additionally, performing the analysis on the pooled sample for both cohorts in round 3 and round 4 and clustering errors at individual level, we confirm the robustness of our results on educational aspirations (see Appendix 3.6).

Table 8: Impact of Juntos on Education and Work Aspirations-Trimmed Sample (with controls)

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations
Juntos	0.158**	0.175**	0.204**
	(0.137)	(0.123)	(0.113)
N	112	112	103

Standardized beta coefficients; Standard errors in parentheses; Errors are clustered at the community level; The propensity score is introduced as weights in the regression; * p<0.10, ** p<0.05, *** p<0.01

8. Conclusion

The aim of this paper is to test whether the Peruvian conditional cash transfer programme, Juntos, affects children aspirations related to education and future work opportunities. The literature on this topic is very scarce and recent. To the best of our knowledge, this is the first paper analysing the impact of the Peruvian programme on aspirations and trying to analyse the economic and psychosocial mechanisms behind it. Our results show that the programme positively and significantly affect children educational aspirations. Conversely, for work aspirations we identify a cohort effect, showing that the programme has a significant impact on adolescents work aspirations but not on younger children aspirations. This result is in line with the idea that people form their aspirations step-by-step (Dalton et al. 2016). Children focus first on education and then on work, once the first objective is fixed, they start to create new aspirations for the following step. We test two main mechanisms through which conditional cash transfers may affect aspirations: the economic channel and the programme inclusion channel. We do not identify an economic mechanism, since we find that the programme does not differently affect children whose wealth index at the baseline is above or below the median of the distribution. Conversely, we can identify a psychosocial effect: the programme has a significant impact on children who, after the start of the programme, are highly included at school and in the community but not on children who perceive stigma and feel excluded within their usual context. Finally, the programme has a sustained impact over time and a stronger effect on girls than on boys. There is evidence that aspirations about education and future work may concretely affect current individuals' preferences for school, years of schooling and educational attainment. Showing that conditional cash transfers may

affect aspirations confirms their well acknowledged role in affecting schooling and investments in human capital, not only through conditionalities but also by acting on people aspirations.

Appendix 3.1

Table 3.1: Variables about aspirations and coding

Indicator	Question	Coded
	CHILDREN	
Education Aspiration (Years)	Q. "Imagine you had no constraints and could study for as long as you liked, or go back to school if you have already left. What level of formal education would you like to complete?"	[Coded as years of education: years of school= (0-11); Complete Technical College=12; Complete University=13]
Education Aspiration (Level of education)	Q. "Imagine you had no constraints and could study for as long as you liked, or go back to school if you have already left. What level of formal education would you like to complete?"	[Coded as completed level of education: 0=None; 1=Primary; 2=Secondary; 3=Complete Technical College; 4=Complete University]
Work Aspiration (three categories)	Q. "What do you want to be when you grow up?"	[Coded as: 1=low aspirations; 2=Intermediate aspirations; 3=High aspirations]

Appendix 3.2

Table 3.2: Descriptive statistics on aspirations (Round 3)

	Older Cohort		**Younger Cohort**	
	Non-beneficiaries	Beneficiaries	Non-beneficiaries	Beneficiaries
Children job aspirations	2.793 (0.547)	2.845 (0.497)	2.697 (0.683)	2.352 (0.888)
Observations	**179**	**71**	**512**	**330**
Children educational aspirations (Years)	12.72 (0.984)	12.78 (0.580)		
Index-Children educational aspirations (Level of education)	3.775 (0.630)	3.784 (0.580)		
Observations	**200**	**74**		

mean coefficients; sd in parentheses
Variables coding are defined in Table 3.1

Appendix 3.3

Table 3.2: Propensity Score Estimates

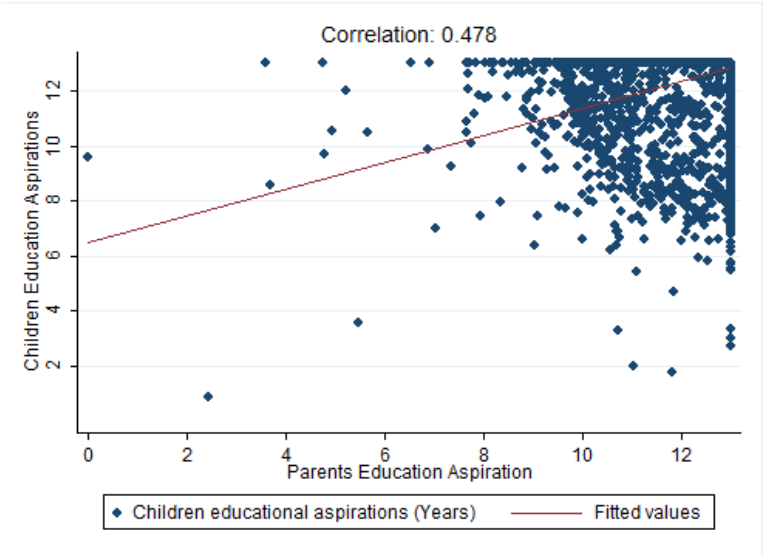
	Juntos (Beneficiary=1 in Round 3)
Area of residence (rural=1)	0.112
	(0.119)
Percentage of households reporting crimes in each community	-0.345***
	(1.003)
Percentage of stunted children in the community	0.717***
	(0.410)
Presence of children under 7 (at baseline) within the household	0.187
	(0.372)
Wealth index	-1.363***
	(0.351)
Household size	0.146
	(0.0220)
Caregiver is literate=1	-0.256*
	(0.111)
Child age	-0.489
	(0.0975)
Child sex (Male=1)	-0.011
	(0.0959)
Spoken language (Spanish)	-0.755***
	(0.129)
Child Age Cohort (Younger Cohort=1)	-0.240
	(0.674)
N	1,151

Standardized beta coefficients; Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Appendix 3.4

The focus of our paper is on children aspirations, however we also show some result about parents aspirations, since children and parents aspirations are positively correlated (Figure 2).

Figure 2: Correlation between parents and children aspirations



Parents are asked to answer to the following questions: *"Ideally what level of formal education would you like child to complete?"*; *"What job would you most like child to do in the future"*.

	PARENTS	
Education Aspiration (Years)	Q. "Ideally what level of formal education would you like child to complete?"	[Coded as years of education: years of school=(0-11); Complete Technical College=12; Complete University=13]
Education Aspiration (Level of education)	Q. "Ideally what level of formal education would you like child to complete?"	[Coded as completed level of education: 0=None; 1=Primary; 2=Secondary; 3=Complete Technical College; 4=Complete University]
Work Aspiration (three categories)	Q. "What job would you most like child to do in the future"	[Coded as: 1=low aspirations; 2=Intermediate aspirations; 3=High aspirations]

We do not find a significant programme impact on parents' aspirations (see Table 4). For parents, the ethnicity seems to be an important predictor of aspirations while

regression coefficients of Table 2 reveal that for children this is not an important component in forming aspirations. This result is in line with Pasquier-Doumer and Risso Brandon (2015) who found that indigenous children do not internalize racial scheme for their opportunities. Also, household head age is not a significant predictors of children aspirations while the opposite happens for parents.

Table 3.4: Impact of Juntos on Education and Work Parents Aspirations

	Parents educational aspirations (Years)	Index-Parents educational aspirations (Level of education)	Parents job aspirations for children
Juntos	0.053	0.024	0.066
	(0.178)	(0.107)	(0.0540)
Wealth index	0.249**	0.236***	0.196**
	(1.045)	(0.481)	(0.348)
Area of residence (rural=1)	-0.040	-0.052	-0.054
	(0.138)	(0.113)	(0.0669)
Household head years of education	0.020	0.044	0.026
	(0.0206)	(0.0117)	(0.00637)
Household head age	-0.211**	-0.165**	-0.107
	(0.0128)	(0.00503)	(0.00373)
Child sex (Male=1)	-0.086	-0.069	-0.179***
	(0.190)	(0.0883)	(0.0792)
Ethnicity (White=1)	0.054***	0.069***	0.030
	(0.175)	(0.108)	(0.162)
Child's age - in months	0.156	0.407	0.569
	(0.0241)	(0.0100)	(0.00744)
Household size	0.038	-0.043	-0.043
	(0.0601)	(0.0316)	(0.0228)
Dependency ratio	-0.145*	-0.119*	-0.065
	(0.00172)	(0.000703)	(0.000597)
Child Age Cohort (Younger Cohort=1)	0.203	0.467	0.619
	(2.136)	(0.878)	(0.632)
N	1,083	1,085	1,048

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

Appendix 3.5

Table 3.5.A: Heterogeneous Effects by Child Sex

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations
Aspirations: Girls, with controls			
Juntos	0.234**	0.256***	0.246***
	(0.223)	(0.130)	(0.124)
N	133	133	125
Aspirations: Boys, with controls			
Juntos	0.064	0.029	0.088
	(0.200)	(0.177)	(0.199)
N	117	118	102
P-value for difference	0.2162	0.1256	0.8602

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

Table 3.5.B: Heterogenous Effect by Area

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations
Aspirations: Rural sample, with controls			
Juntos	0.161**	0.152	0.182*
	(0.179)	(0.172)	(0.169)
N	94	95	84
Aspirations: Urban sample, with controls			
Juntos	-0.004	-0.004	0.276**
	(0.101)	(0.101)	(0.0327)
N	156	156	143
P-value for difference	0.2610	0.4059	0.5175

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

Appendix 3.6

Table 3.6: Impact of Juntos on Education and Work Aspirations – Pooled Sample

	Children educational aspirations (Years)	Index-Children educational aspirations (Level of education)	Children job aspirations
Juntos	0.066	0.106**	0.069
	(0.110)	(0.0804)	(0.0822)
Wealth index	0.108*	0.102*	0.083
	(0.483)	(0.325)	(0.285)
Area of residence (rural=1)	-0.033	-0.010	-0.114**
	(0.0956)	(0.0882)	(0.0756)
Household head years of education	0.160***	0.224***	0.035
	(0.0111)	(0.00960)	(0.00941)
Household head age	-0.006	-0.009	0.061
	(0.00525)	(0.00377)	(0.00371)
Child sex (Male=1)	0.070	0.049	-0.116***
	(0.0952)	(0.0693)	(0.0618)
Ethnicity (White=1)	-0.079	-0.052	-0.037
	(0.718)	(0.368)	(0.149)
Child's age - in months	-0.152	-0.071	0.539
	(0.0125)	(0.00882)	(0.00889)
Household size	0.002	-0.062	-0.021
	(0.0286)	(0.0195)	(0.0176)
Dependency ratio	-0.020	-0.065	-0.060
	(0.000722)	(0.000646)	(0.000561)
Child Age Cohort (Younger Cohort=1)	-0.037	0.214	0.434
	(1.060)	(0.765)	(0.766)
Year Dummy (Round4=1)	-0.082	-0.265	-0.243
	(0.635)	(0.467)	(0.435)
N	1,089	1,095	1,664

Standardized beta coefficients; Standard errors in parentheses
Errors are clustered at the community level; The propensity score is introduced as weights in the regression
* p<0.10, ** p<0.05, *** p<0.01

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