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5 Years in *Juntos*: New Evidence on the Program's Short and Long-Term Impacts

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ABSTRACT

This study presents results from a quantitative impact evaluation of the Conditional Cash Transfer (CCT) program, *Juntos*, in Peru. Using instrumental variable techniques, it estimates the overall impact of *Juntos* five years after its initial rollout and explores the differential impacts among beneficiaries according to the length of time they spent in the program. In so doing, the analysis explores whether it takes time for the program to make significant and sizable impacts; and whether some impacts change in magnitude the longer the beneficiaries spend in the program. The results seem to confirm both hypotheses: almost all indicators of interest are significantly higher among beneficiaries with longer treatment spells. However, in many cases these improvements are too small to be picked up in the analysis of overall effects, when beneficiaries are compared to non-beneficiaries. These findings suggest that while the program has a non negligible impact on welfare, there is room for improvement.

Keywords: poverty, transfers, Peru.

JEL Classification: H53

Cinco años del programa *Juntos*: Nuevas evidencias sobre sus impactos de corto y largo plazo

RESUMEN

Este estudio presenta resultados de la evaluación de impacto cuantitativo del programa de transferencias de dinero condicionales *Juntos*, aplicado en el Perú. Usando técnicas de variables instrumentales, se estima el impacto promedio del programa cinco años, desde su inicio, y explora los impactos diferenciados entre los beneficiarios, dependiendo de su tiempo de permanencia en el programa. Al hacer esto, el análisis explora si se necesita de tiempo para que el programa tenga impacto significativo, y si la magnitud del impacto depende de la estancia de los beneficiarios en el programa. Los resultados parecen confirmar ambas hipótesis: casi todos los indicadores de interés son significativamente más altos entre los beneficiarios con mayor tiempo en el programa.

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Sin embargo, en muchos casos estas mejoras son muy pequeñas como para ser tomadas en cuenta en el análisis de los efectos promedio, cuando los beneficiarios son comparados con los no beneficiarios. Estos descubrimientos sugieren que mientras el programa tenga un impacto poco significativo en el bienestar, hay espacio para mejorar.

Palabras clave: pobreza, transferencias, Perú.

1. INTRODUCTION

The strong potential for CCT programs to alleviate poverty and increase investments in human capital has been evidenced by the extensive literature on the subject, ranging from experimental studies to non-experimental evaluation of impacts on a broad range of indicators[1]. However, despite widespread adoption of CCT programs around the world (the number of countries with a CCT program increased from 3 in 1997 to 40 in 2010), the introduction of *Juntos* as one of Peru's flagship social programs has been met with mixed reactions.

The *Juntos* program commenced in 2005, and by 2011 had grown from operating in 110 districts and covering approximately 32,000 households[2], to span some 500,000 households in 638 districts. However, the debate about the merits of the program is still ongoing, but is rarely centered on the program's actual impacts. Unfortunately, *Juntos* did not include a systematic impact evaluation in its initial design and consequently, little quantitative information has been gathered with regard to its impacts and ability to achieve the key objectives of reducing poverty and building human capital.

The absence of an evaluation framework, developed contemporaneously with the program's deployment, significantly impeded evaluation efforts. Indeed, rigorous evidence on the program's impacts has been limited to the work by Perova and Vakis (2009) and Jaramillo and Sanchez (2011). Both studies take advantage of the existing data, where identification of the program beneficiaries is possible.

Perova and Vakis (2009) combine several data sources: the National Household Survey (*Encuesta Nacional de Hogares*, ENAHO), the National Register of Municipalities (*Registro Nacional de Municipalidades*, RENAMU), and administrative data from *Juntos* itself; their research spanned the period from 2006 to 2007, and aimed to identify the impact of the program using matching techniques. Their evaluation concludes that the *Juntos* program has had a moderate impact in reducing poverty and increasing monetary measures of both income and consumption. The results also suggest that the program increased the utilization of health services for both children and women, and improved the nutritional intake of participating households. The analysis detected significant impacts on school registration and attendance mainly at the transition points, similarly

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[1] For a detailed review of quantitative evidence on the impacts of the CCT programs around the world, see Fiszbein and Schady (2009).

[2] Juntos web-site: http://juntos.gob.pe/ctrabajamos_fase.php

to other CCT programs in the contexts where primary school attendance is high. Jaramillo and Sanchez (2011) focus on the impacts of the *Juntos* program on nutritional status. Taking advantage of the Demographic and Family Health Survey (*Encuesta Demográfica y de Salud Familiar*, ENDES) data from 2008-2010, they estimated the impact of the program on chronic malnutrition. The authors used a combination of difference-in-difference and matching, and conclude that the program did in fact contribute to the reduction of early chronic malnutrition. However, the impact on overall chronic malnutrition was found to be negligible.

This paper contributes to the existing non-experimental impact evaluations of the *Juntos* program in a number of important dimensions.

First, it addresses an important question as to the sustainability of the program's impacts, as well as their dynamics over time. One can imagine several plausible scenarios: the impacts may become weaker if monitoring is not strictly enforced and beneficiaries learn to «play the system.» Alternatively, as time passes, participation may trigger a change in attitudes and perceptions. The resulting behavioral change may lead to even stronger impacts, compared to mechanical compliance with the conditions during the initial stages of the program. It is also possible that the impacts remain mechanically driven and do not change overtime. Consequently, the question of whether the impacts in 2009 are the same, lower, or higher than at the beginning of the program can only be ascertained empirically. The present evaluation addresses this question by taking advantage of the new rounds of ENAHO data which allow the analysis to span 5 years of *Juntos*, from late 2005 through 2009.

Secondly, this work examines whether and how the impacts change depending on treatment spell duration, or simply the amount of time an individual spent in the program. In this exercise, the identification strategy relies on the fact that ENAHO interview dates are not correlated with the *Juntos* rollout schedule. Consequently, the length of time during which an individual received benefits can be considered randomly assigned. Taking advantage of this quasi-random assignment of treatment spells, one can cleanly identify the marginal impact of an additional year (or month) in the program.

The present evaluation also contributes to the existing literature by using an alternative estimation strategy: instrumental variables as opposed to/in addition to matching, as in Perova and Vakis (2009). Finally, it reaps the benefits of having two additional years of data (2008 and 2009) following on from the start of the program; the resultant higher number of *Juntos* beneficiaries featured in the data may improve the precision of the estimates.

The paper is organized as follows. Section 2 provides a description of the *Juntos* program and its main components; Section 3 develops the econometric methodology used to make causal inferences; Section 4 presents the results, and section 5 concludes.

2. PROGRAM DESCRIPTION

2.1. Program objectives and conditions

As with other CCT programs, *Juntos* is based on two broad objectives: (i) in the short term, to reduce poverty by providing households with cash transfers; and (ii) in the long term, to break the intergenerational transmission of poverty through promotion of human capital via improved access to education and health services. The program aims to achieve these objectives through the provision of eligible households with a monthly cash transfer of S/. 100 (Peruvian Nuevos Soles). Unlike other CCT programs, this is a lump-sum payment and does not differ across households depending on the number of children. In order to receive this payment, households need to comply with a number of requirements. These «conditionalities»[3] vary depending on the age and gender of the beneficiaries, and are listed in Box 1.

Box 1. Program Conditionalities

For children under 6 years, pregnant and lactating women:	Attend regular health checks: CRED for children, pre-natal and post-natal checks for women
For children between 6-14 years who did not complete primary school:	School attendance for at least 85% of the school year

2.2. Program eligibility

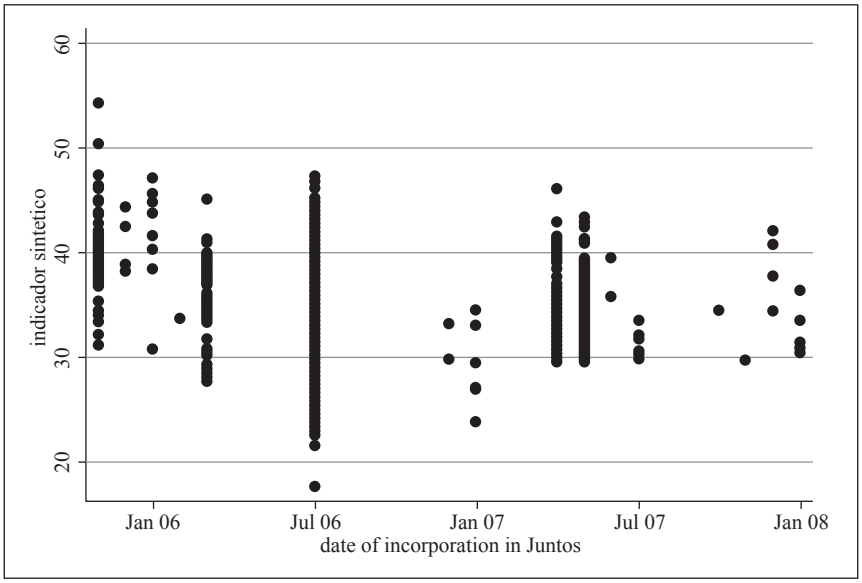
The selection of beneficiary households is comprised of three stages: selection of eligible districts, selection of eligible households within those districts, and, finally, a community level validation, which finalizes the actual beneficiary list. At the first stage, participating districts were selected on the basis of the five criterions: (i) exposure to violence during Shining Path (*Sendero Luminoso*) guerilla activity; (ii) poverty level, measured as a proportion of population with unsatisfied basic needs; (iii) poverty gap; (iv) level of child malnutrition; and (v) presence of extreme income poverty. These variables were combined to create a «summary indicator» (*indicator sintético*) —a score calculating each district's needs for governmental assistance. 638 districts were selected on the basis of this score. Although the *Juntos* management initially considered rolling out the program in descending order as determined by the summary indicator, i.e. from the districts in most dire need to less disadvantaged districts, this order was not followed (as Figure 1 shows). The *Juntos* management cited random events – such as adverse weather conditions— as the reasons behind their failure to adhere to the planned order.

In the second stage, the National Institute of Statistics and Information (*Instituto Nacional de Estadística e Informática*, INEI) took a census of all households in each of the

[3] These are also referred to as «co-responsibilities» (*corresponsabilidades* in Spanish).

eligible districts. Using this data, a proxy means test formula was applied to determine household eligibility based on poverty. In addition, given that the primary focus of the program is on young children and pregnant mothers, only households with children under 14 years old or at least one pregnant woman were selected. Finally, a community validation exercise took place to complete the list of eligible households. This task was performed by community members, local authorities and representatives of the Ministries of Education and Health with the aim of minimizing both inclusion and exclusion errors.

Figure 1. Initial rollout of *Juntos*



3. IMPACT EVALUATION METHODOLOGY

Unfortunately, an experimental impact evaluation framework was not incorporated into the design of the *Juntos* program, and randomized rollout did not take place. In its absence, the feasibility of an impact evaluation depends on the existence of data on *Juntos* beneficiaries, as well as the possibility of credibly constructing counterfactual control groups through the use of econometric techniques.

3.1. The data

A number of data sources were combined to carry out the present non-experimental impact evaluation. The principal source of data is the ENAHO household survey; ENAHO is a continuous (annual) survey and contains rich data on household consumption and spending patterns, assets, education, health and utilization of medical

services. It also features questions and answers, which allow one to identify those respondents who received *Juntos* benefits during the year when the interview took place.

The present analysis is based on the data from 2006 to 2009. Up to 2007, identification of *Juntos* beneficiaries in the ENAHO data is based on the question: «In the last 6 months, did you receive any public or private transfers?» The question allows one to specify the nature of the transfer. Those who responded positively to this question and specified *Juntos* were included in the group of beneficiary households. However, it is plausible that such a method of identifying *Juntos* beneficiaries may cause a bias.

Indeed, respondents are not explicitly asked about the transfer. If some beneficiaries fail to recall that they received a transfer from *Juntos*, they may differ from those who remember it in some important dimensions, for example, in how diligently they complied with program conditions. Consequently, the impact of the program may be lower for this group. Its exclusion from the sample of beneficiaries is likely to result in an overestimation of the impact.

However, a change in the phrasing of the question used for identification of beneficiaries was introduced in ENAHO 2007, and this renders such a scenario implausible. Starting 2007, the question explicitly mentions *Juntos*. More specifically, it asks: «In the last 6 months, did you receive any public or private transfers, for example, *Juntos* program transfers.» Where the question is answered positively, a respondent specifies whether the received transfers were from the *Juntos* program or from any other source.

Furthermore, it is possible to test the quality of identification relating to *Juntos* beneficiaries in ENAHO by comparing the numbers herein to the actual quantity of J*untos* beneficiaries based on the Register of Beneficiaries (*Padrón de Beneficiarios*). Table 1 presents estimates of the numbers of beneficiaries based on the ENAHO survey, actual numbers from *Juntos* administrative data, and the percentage difference between the two numbers.

Table 1. Differences between the actual number of beneficiaries and the ENAHO-based estimate

Year	Number of beneficiary households based on ENAHO	Number of beneficiary households based on *Juntos* administrative data	Percentage difference
2008	440,755	476,525	8%
2009	497,768	501,885	1%

The actual number of beneficiaries on December 2008 and 2009 is reported[1]

Table 1 clearly shows that the discrepancy between the ENAHO estimate and the actual number of beneficiaries is negligible: 8 percent in 2008 and 1 percent in 2009. Given this practically undistinguishable difference between the actual number of beneficiaries and the ENAHO estimate, the possibility of selection bias due to the exclusion of forgetful beneficiaries can be ruled out for the 2008 and 2009 data.

The study complements ENAHO with other data sources, including the 2005 census of *Juntos* districts, the RENAMU, and administrative data.

The census of *Juntos* districts was taken by the INEI in 2005 in order to identify eligible households. As eligibility is determined based on a proxy means test index (discussed above), the census includes detailed information on household assets, characteristics of the dwelling, demographic characteristics and household member education level: all variables required for calculation of the proxy means test index. The RENAMU database contains rich information on infrastructure, public services, economic activity and further information relating to the districts and can be used to take into account district-level heterogeneity. Administrative data used in the study include the dates of incorporation in the program by district, the registry of program beneficiaries, and district-level information used in CRECER selection.

3.2. Identification Strategy

The major challenge for identification of the causal impacts of social programs in the absence of randomized rollout stems from the likelihood of unobserved heterogeneity among program beneficiaries and non-beneficiaries.

Given the initial differences in characteristics that may affect consumption, human capital investments and other outcomes of interest, it would only be possible to attribute simple differences in these outcomes among beneficiaries and non-beneficiaries on the effect of the program if we could exhaustively control all the differences between the two groups. However, the means of controlling all the characteristics that could potentially affect outcomes of interest belongs to the realm of econometric utopia.

There are a number of econometric techniques that allow one to circumvent the problems that stem from unobserved heterogeneity. This study relies on the instrumental variables method and uses matching as a robustness check. The principal advantage of using the combination of the two methods is that they identify impacts through different sources of variation. More specifically, the instrumental variables method essentially compares *Juntos* beneficiaries to non-beneficiaries in the same districts, while matching relies on the comparison of *Juntos* beneficiaries with non-beneficiaries in the districts that have never received the program, but which are included in its expansion plans. The following sections will describe the sources of variation on the basis of the two methods in greater detail, and will outline the specifics of the corresponding identification strategies.

3.2.1. Instrumental variables

The process of selecting *Juntos* beneficiaries makes the instrumental variables method a strong candidate for identifying program impacts. Eligibility based on proxy means that the score, combined with interplay between the *Juntos* roll-out schedule and

the ENAHO interview date, give sufficient information to create a variable which satisfies the requirements for a valid instrument.

For a variable to be considered a valid instrument, it should be strongly correlated with the instrumented regressor, and be correlated only with the outcome of interest through this regressor. This study uses an interaction between a dummy equal to 1 if a respondent is eligible based on his/her proxy means score[4], and the number of months over which the district had been receiving *Juntos* by the time of the interview. Their interaction is likely to satisfy the afore-mentioned conditions, conditional on eligibility score and the time elapsed since a given district was incorporated into the program (the two components of the instrument).

Let D_{ij} be the number of months that passed between district *j*'s incorporation into the program and the time when individual *i* was interviewed, and E_{ij} be equal to 1 if an individual *i* from district *j* is eligible for the program according to proxy means score. The vector X_{ij} is a vector of individual, households and district level characteristics potentially correlated with the outcome. T_{ij} is equal to 1 if an individual is receiving *Juntos*. Thereafter, the following second stage regression will estimate the impact of *Juntos*:

$$Y_{ij} = \beta_0 + \beta_1 D_{ij} + \beta_2 E_{ij} + \beta_3 T_{ij} + \beta_4 X_{ij} + \psi_{ij}. \quad (1)$$

The corresponding first stage is:

$$T_{ij} = \delta_0 + \delta_1 D_{ij} + \delta_2 E_{ij} + \delta_3 D_{ij} E_{ij} + \delta_4 X_{ij} + \psi_{ij}, \qquad (2)$$

Vector X_{ij} includes geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, types of materials used in the construction of the dwelling. Indicators equal to 1 indicate that the household head works in agriculture; district level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, number of organizations and beneficiaries of *Club de Madres*, *Vaso de Leche* and *Comedor Popular*[5], as well as gender and age. In the household level regressions indicator, *i* denotes a household and vector *X* does not include individual characteristics.

The impact of *Juntos* is captured in the coefficient β_3. As the estimating equation includes a control for receiving *Juntos* earlier or later (D_{ij}), as well as for the household's eligibility (E_{ij}); their interaction is likely to affect the outcomes of interest only through the higher likelihood of participation in the *Juntos* program. Thus, it is highly plausible

[4] The proxy means test score is created based on the formula made available to the authors by the Juntos administration. The recreation of this score is possible due to the fact that all the questions from the 2005 INEI census in Juntos districts appear in the ENAHO questionnaires, with nearly identical phrasing.

[5] «Mother's Club,» «Glass of Milk,» and «Community Kitchen» respectively: three social programs run by the Peruvian government.

that the exclusion restriction holds. Eligibility score and district program incorporation dates may be correlated not only with the treatment variables, but also with the outcomes. However, once these variables are controlled, it is likely that their interaction will only affect outcomes through increasing the chances of receiving the *Juntos* transfer. Moreover, the value of D_{ij} is determined not only by *Juntos* rollout (which may be correlated with the outcomes) but also by INEI fieldwork, which is completely orthogonal to the former. In other words, even if two districts received the *Juntos* program in the same year, the similarly eligible respondents in these two districts may have different values of D_{ij} if the ENAHO survey was administered in January in one district, in August in another, while the *Juntos* program materialized some time in February. This element of quasi-randomness further strengthens the proposed approach.

The first condition can be easily tested in the first regression stage. Table A1 shows that the instrument is significant in all samples (household, children under 5, children aged 6 to 14, and women of childbearing age). The corresponding F-statistics exceed the required threshold of 10 in all cases. The magnitude of the coefficient on the instrument is low: it ranges between 0.004 and 0.01, depending on the sample. It is important to remember that the coefficient captures the differential impact of an additional month of enrollment in *Juntos* for eligible cases, compared to non-eligible. The small difference in the likelihood of enrollment between eligible and non-eligible may partially reflect the role of community validation procedures, which «overwrote» eligibility based on proxy means. However, it may also stem from the fact that eligibility is determined based on contemporaneous values of the components of the proxy means test score, which may have changed after their collection by INEI in 2005.

3.2.2. Matching

As an alternative identification strategy, the study also uses matching. Matching techniques allow one to construct an artificial counterfactual: a control group, based on households, which are similar to the beneficiary households except for the fact that they did not receive the transfer. These techniques provide a credible empirical framework for impact evaluation in the absence of random assignment (Abadie and Imbens [2006], Imbens [2004], Rosenbaum and Rubin [1983]).

Matching techniques have been widely used in empirical work, and a variety of matching estimators have been developed (Imbens [2004]). This study will evaluate the impact of *Juntos* by matching observationally similar households to beneficiary households by using an algorithm developed by Abadie and Imbens (2006), and conducting regression analysis on the matched sample. Intuitively, the method hinges on choosing a set of matching characteristics, and selecting for each beneficiary a match that has the closest values in all these characteristics. A more formal explanation of the selection of the matches can be found in Abadie *et al.* (2004).

Matching characteristics used in this study include the components of the district poverty score, such as district poverty level, childhood malnutrition level, the fraction of households exposed to violence during Shining Path activity, as well as proxy means score used to determine household eligibility. Additionally, the pool of potential matches is limited to 243 CRECER districts only. Non-beneficiaries in *Juntos* districts are not included. These steps effectively ensure that the matches are drawn according to the same selection criterion that was used by the *Juntos* program to select the current beneficiaries from the districts included in the program expansion plans. Intuitively, beneficiaries are matched with the individuals that would have become beneficiaries, had the program already arrived to their districts.

The matched sample is subsequently used in regression analysis to yield impact estimates. Regression framework allows for the control of variables not included in the matching algorithm, but which can nevertheless affect the outcome, such as pre-treatment outcome levels or survey dates, to capture seasonality effects.

The average effect of *Juntos* on individual or household-level outcomes is estimated in the following regression:

$$Y_i = \alpha_1 + \alpha_2 T_i + \alpha_3 X_i + \alpha_4 Z_i + \xi_I, \qquad (3)$$

where Y_i denotes an outcome of interest, T_i is a dummy equal to one if an individual/household i receives program benefits, X_i is a vector of matching covariates, and Z_i is a vector of additional individual, household, and district level characteristics potentially correlated with the outcome. The vector X_i includes pre-program district levels of poverty, childhood malnutrition, violence, monthly per capita household spending, and the household level proxy means test indicator. In addition, the vector Z_i includes 2005 (pre-program) district averages of the outcome variable, household size and the proportion of children per household, geographical controls, indicators for access to drinkable water, electricity and sanitation, and materials used for dwelling construction. indicators equal to 1 if the household head works in agriculture, number of hospitals, healthcare centers, pharmacies, number of organizations, and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age. The impact of *Juntos* is captured in the coefficient α_2.

It is important to note that the context of this study differs from the ideal setting in which matching methods are likely to yield unbiased estimates. First, treated and control units are matched on contemporaneous, rather than pre-treatment, variables. Data limitations make matching on pre-treatment covariates impossible: ENAHO's panel component is discontinued every 4 years. Consequently, the estimation can be carried out only with a cross-sectional sample, where households are matched on contemporaneous co-variates. Second, the community validation stage in the selection of beneficiaries guarantees that participation in the program depends not only on the basis

of observed variables included in the proxy means test score, but also on unobservable characteristics.

The first limitation is likely to lead to a downward bias, as the program may have already affected matching covariates in the treated group. In this case, *Juntos* beneficiaries will be compared not to similar matches, but to somewhat wealthier control households. This problem is more pronounced in the study of longer-term effects. The matching covariates may not be easy to change, which may justify the use of the method in the short run, but as the amount of time during which the families were exposed to the program increases, so does the likelihood that the transfer affects the matching characteristics.

The direction of the bias due to the presence of unobserved variables is ambiguous. If community validation allows local elites to smuggle relatively affluent relatives who would not qualify into the beneficiary lists, those treated may be better off than the selected controls on the basis of the proxy means test score alone. If, on the other hand, community validation improves targeting, beneficiaries are likely to be poorer than controls.

Given the likelihood of bias due to matching on contemporaneous covariates and due to the presence of unobserved selection characteristics, instrumental variables remain the preferred method for this study. The results based on matching technique are reported in the Appendix, mainly for greater transparency and continuity with the previous non-experimental impact evaluation of the *Juntos* program (Perova and Vakis, 2009).

3.2.3. Intensity (dose) analysis

Both proposed methods are not experimental and use pre-existing data. Consequently, potential selection problems – of beneficiaries or beneficiary districts – will always cast a shadow on the estimation results. Ironically, yet another impediment to rigorous evaluation – the absence of purpose-collected data for the evaluation – generated some random variation in the length of observed treatment spells, thus making it feasible to carry out a rigorous estimation of marginal effects for an additional year (or month) in the program.

The ENAHO fieldwork schedule is completely orthogonal to the program's rollout. Consequently, if *Juntos* beneficiaries alone are considered, the length of the observed treatment spell conditional on the enrollment year, i.e. the number of months between the date when the respondent started receiving the program and the date of the interview, can be considered «randomly assigned.» Consider a hypothetical example: in two districts, which were incorporated into the *Juntos* program on the same date, say, January 2006, ENAHO interviewers arrive on different dates. The beneficiaries in districts A are interviewed in April and beneficiaries in district B are interviewed in September. Although the actual length of treatment is the same on any given date,

the observed treatment spells – 4 months in district A and 9 months in district B – differ, and can be considered randomly assigned.

There is an important limitation to this argument: the dates when ENAHO respondents start receiving *Juntos* transfers are not available in the survey. Instead, this study uses district enrollment dates as the proxies for the dates when individuals joined the program, or in other words, it assumes that all district beneficiaries take up the program in the first month of its arrival in the district.

This assumption is quite realistic, given the program's operational guidelines. *Juntos*'s policies require that all beneficiaries in the district enroll in the program at approximately the same time. Moreover, it is possible to test this assumption using the Register of Beneficiaries: a record of all payments to all beneficiaries. According to the Register, the majority of beneficiaries join the program within the first three months of its arrival in the district.

Constructing individual treatment spells as a difference between the month of the program's arrival to the district and the date of the interview; one can estimate the differential impact of treatment spells of different lengths. Let $L1_{ic}$ be a dummy equal to 1 if a respondent i from enrollment cohort c participated in the program from 12 to 23 months; $L2_{ic}$ is equal to 1 if a respondent i participated in Juntos from 24 to 35 months and $L3_{ic}$ be equal to 1 if a respondent i participated in the program for 36 months or longer. To capture heterogeneity in the effects depending on the length of the treatment, one can estimate the following equation:

$$Y_{ic} = \gamma_0 + \gamma_1 L_{1ic} + \gamma_2 L_{2ic} + \gamma_3 L_{3ic} + \gamma_4 X_{ic} + \varphi_c + \xi_i, \qquad (4)$$

where Y_{ic} captures the outcome of interest, φ_c is a cohort effect and X_{ic} includes individual and household characteristics as in (1). Additionally, vector X_i includes year and month dummies to purge seasonality effects and to account for overtime changes in economy. Coefficients γ_1, γ_2 and γ_3 capture the effect of the program for beneficiaries who have been receiving the transfer between 1 and 2 years, 2 and 3 years, and over 3 years, respectively. The omitted category includes beneficiaries who have been receiving the program for 11 months or less.

The equation in (4) provides an opportunity to test two hypotheses concerning the program impacts. First, it may take time for some impacts to materialize. The significance of any of the coefficients on the treatment spells combined with the absence of overall impact would attest to this supposition. Second, a test of the significance of the differences in the two coefficients can show whether the impacts accumulate over time. Finding significantly higher γ_3 compared to γ_2 suggests that an additional year in the program not only maintains the program's impact, but increases it.

4. RESULTS

4.1. Overall impacts

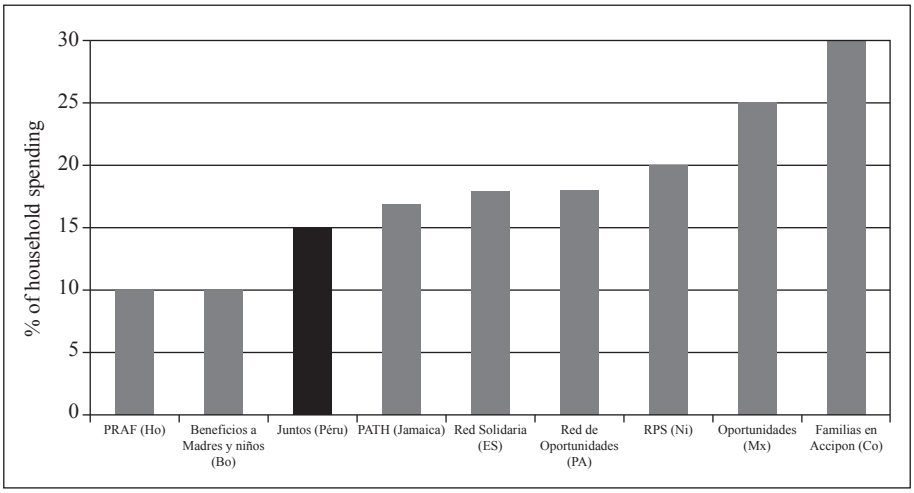
As the discussion above indicates, in the context of this study the instrumental variables method has a number of advantages over matching. Due to the limitations of the data, matching on pre-treatment variables is not feasible. Furthermore, the presence of unobservable characteristics based on which eligibility is determined is not unlikely, due to the community validation stage of beneficiary selection. Therefore, the following section focuses on the IV estimation based results. Those from matching are presented in Appendix 2.

4.1.1. Impacts on consumption, income and poverty

The *Juntos* transfer constitutes approximately 15 percent of average household spending among beneficiaries, which places it towards the lower end of the ranking of CCT programs in Latin America (Figure 2).

Despite the relatively moderate figure transferred, estimation results suggest that participation in *Juntos* triggered significant increases in consumption, income, and poverty indicators among beneficiaries. Overall consumption went up by 33 percent (Table 2). This increase in overall consumption comprises a positive change in the consumption of food as well as non-food items, though consumption of the latter went up by a higher margin: 65 percent, compared to a 15 percent increase in food consumption. Overall income also increased by 43 percent.

Figure 2. CCT as a fraction of average household spending by country



The increase in consumption was sufficiently high to be reflected in the decrease in poverty numbers. The poverty headcount went down by 14 percentage points, while the fraction of extreme poor decreased by 19 percentage points (Table 2). It is important to keep in mind that the estimation was taken from the sample of *Juntos* districts, where the poverty and extreme poverty rates are higher than the national poverty rates (70 and 36 percent of households respectively in 2009, compared to 34 and 11 percent nationally[6]). An increase in consumption is also reflected in the changes in poverty gaps: moderate and extreme poverty gaps among *Juntos* beneficiaries narrowed by 14 and 7 soles, respectively.

Table 2. *Juntos* impacts on consumption and income

	Pre-treatment level (average among eligible in Juntos districts in 2005)	coefficient and standard error	number of observations
overall consumption	108.19	0.33***	14,670
		(0.06)	
consumption of food	54.34	0.15**	14,561
		(0.07)	
consumption of non-food	41.96	0.65***	14,670
		(0.09)	
overall income	90.27	0.43***	14,670
		(0.07)	
poor	0.85	-0.14***	14,670
		(0.05)	
extreme poor	0.61	-0.19***	14,670
		(0.05)	
poverty gap	75.08	-14.52***	14,670
		(5.24)	
extreme poverty gap	23.45	-7.23***	14,670
		(2.55)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular.*

*** denotes 99% significance; ** 95% significance and * 90% significance

6 Authors' calculations based on ENAHO 2009.

Nevertheless, it is important to note that the impact of *Juntos* is lower compared to that of comparable CCT programs, which provide beneficiaries with higher transfers. While participation in *Juntos* has been shown to increase consumption by 15 percent, participation in similar programs in Colombia (*Familias en Acción*) and Nicaragua (*Atención a Crisis*) triggered higher increases in food consumption: 20 and 27 percent respectively (Fiszbein and Schady, 2009). Though these comparisons should be interpreted with great caution due to contextual differences, they suggest that the *Juntos* program may not be realizing its full potential.

4.1.2. Impacts on the utilization of healthcare services

The estimation suggests that the *Juntos* program has increased utilization of health services among children under the age of 6 and women of childbearing age (12 to 49 years old). Children from beneficiary households were 69 percentage points more likely to have received health checks during the three months prior to being interviewed and were 55 percentage points more likely to have sought medical attention in the event of illness (Table 3).

Table 3. *Juntos* impacts on the utilization of medical and healthcare services (for children under 5)

	Pre-treatment level	coefficient and standard error	number of observations
received health checks in the last three months	0.35	0.69*** (0.18)	8,992
received vaccinations in the last three months	0.54	0.05 (0.18)	8,992
did not experience any illness	0.51	-0.18 (0.21)	8,992
sought medical attention in the event of illness	0.31	0.55* (0.32)	4,466

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

Among women of childbearing age from beneficiary households. only the likelihood of doctor-assisted delivery and contraceptive usage significantly increased: by 91 and 12 percentage points, respectively (Table 4).

Table 4. *Juntos* impacts on utilization of medical and healthcare services (for women of childbearing age)

	Pre-treatment average	coefficient and standard error	number of observations
delivery was assisted by the doctor	0.23	0.91***	2,309
		(0.30)	
did not experience any illness	0.40	-0.07	11,369
		(0.10)	
sought medical attention in case of illness	0.27	0.13	6,922
		(0.11)	
participated in the family planning activities	0.18	0.10	11,369
		(0.07)	
participated in health campaigns	-0.00	0.01	11,369
		(0.02)	
used contraceptives	0.16	0.12*	11,369
		(0.07)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular* ;; individual level characteristics include age, years of schooling, marital status, number of hours worked the previous week, type of work (agricultural or otherwise, salaried or otherwise).

*** denotes 99% significance; ** 95% significance and * 90% significance

Although the program has indisputably made some positive impacts, it is important to keep in mind that the program did not significantly affect certain indicators of interest, such as vaccinations, or the only available indicator of final outcome – the likelihood of getting sick (Table 3). It is plausible that the analysis does not detect a significant increase in the likelihood of being vaccinated among beneficiary children due to the phrasing of the ENAHO question: only vaccinations received in the last three months were reported. Consequently, if beneficiaries try to vaccinate their children early in an effort to comply with the requirements of the program, these vaccinations may not be recorded during data collection.

Nevertheless, the absence of significant impacts in other indicators of interest shows that the program falls short of triggering changes in all targeted areas. Neither children

nor women of childbearing age from beneficiary households are less likely to fall sick. Beneficiary women are not more likely to seek medical attention in the event of illness, or to participate in family planning events or health campaigns.

4.1.3. Average impacts on education

The present study focuses on three educational outcomes: school registration, school attendance, and the probability that a child worked the previous week.[7] While no significant impacts on registration or child labor have been recorded, the likelihood of beneficiary children attending school, conditional on registration, increases by a considerable 25 percentage points (Table 5).

Table 5. *Juntos* impacts on education (for children aged 6 to 14)

	Pre-treatment level: average among the eligible in *Juntos* districts	coefficient and standard error	number of observations
Registered at school	0.77	0.06 (0.09)	17,473
Currently attending school	0.86	0.25*** (0.07)	13,973
Worked the previous week	0.43	0.17 (0.13)	17,771

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

It is plausible that these results are explained by an underlying heterogeneity among beneficiaries. Suppose that beneficiaries differ in their pre-program willingness to go to school: some children keener on studying than others, some parents with a greater appreciation for education. The impact of the transfer is not sufficiently strong to alter the decision to register, which in part reflects a prior willingness to study. However, for the willing families, in which absence from school most frequently occurs due to some constraint, such as nutrition level or disease prevalence,[8] the impact of the program is sufficiently high to manifest itself in significant estimates. Of course, this is merely

[7] The variable, which captures child labor, was created based on the household questionnaire; thus working the previous week is not necessarily self-reported – this information may be given by one of the household members.

[8] For example, Kremer and Miguel (2004) show that having intestinal parasites increases absenteeism.

a description of one of the potentially numerous explanations behind the results presented in Table 5, and is by no means causal.

However, it is important to note that the levels of registration among non-beneficiaries are not particularly high. The predicted counterfactual is only 77 percent (Table 5), while actual registration among the eligible in *Juntos* districts in 2009 was only slightly higher: 80 percent. These numbers suggest that the potential for improvement is definitely there, and the absence of impacts cannot be explained by near-universal attendance prior to the program.[9]

4.2. Intensity (dose) analysis

The independence of both *Juntos*'s rollout and the ENAHO fieldwork schedule makes it possible to rigorously estimate the marginal impact on beneficiaries of receiving *Juntos* for a longer period of time. Though the quasi-random assignment of treatment spells offers a possibility to cleanly estimate the marginal impacts, the downside of the method is that it precludes benchmarking of effects. There is no non-treated counterfactual: *Juntos* beneficiaries are compared to *Juntos* beneficiaries, the only difference being the amount of time across which transfers were received. This is a double edged sword: on the one hand, the estimation strategy clearly precludes the problem of bias, though, on the other, it deprives one of an intuitive translation of the estimate of the fraction of children from beneficiary families who will register for school or get vaccinated. One can only state that the impact of two years of participation in *Juntos* is significantly higher than that of one year.

Nevertheless, the exercise unquestionably provides valuable information for policy makers. As mentioned above, the analysis of differences in impacts depending on the length of time an individual has been receiving *Juntos* transfers provides answers to two important questions with immediate policy ramifications. Firstly, it shows whether it takes time for program impacts to manifest themselves in observable changes (hereafter referred to as «duration effect»). Secondly, it demonstrates whether the impacts intensify over time («accumulation effect»). This section presents the results of the dose analysis, or the investigation into the differential effects of receiving the program for shorter or longer periods of time.

4.2.1. Treatment intensity for consumption, poverty and income indicators

Table 6 shows that the impact of the program in overall and food consumption is stronger among beneficiaries who spent one year or longer in the program compared to those who have been receiving benefits for less than one year. Similarly, the fractions of the poor and extremely poor reduce at higher rates among the group with longer treatment spells.

[9] In some contexts the absence of impacts can be attributed to near-universal enrolment. Thus, the impact of *Oportunidades* on enrolment in grades 0 through 5 was not significant, akthough baseline enrolment in that case reached 94 percent (Fiszbein and Schady, 2009).

Table 6. Overtime impacts of *Juntos* on consumption and income

	12 to 23 months in *Juntos*	24 to 36 months in *Juntos*	over 36 months in *Juntos*	F-test between (I) and(II)	F-test between (II) and(III)	F-test between (I) and(III)	Number of observations
	(I)	(II)	(III)				
overall consumption	0.09***	0.11***	0.15**	0.59	2.37	1.94	4,250
	(0.03)	(0.04)	(0.06)	0.44	0.12	0.16	
consumption of food	0.09***	0.10**	0.13*	0.25	0.85	0.75	4,245
	(0.03)	(0.05)	(0.07)	0.61	0.36	0.39	
consumption of non-food	0.11***	0.14**	0.20**	0.46	2.48	1.86	4,250
	(0.04)	(0.06)	(0.09)	0.50	0.12	0.17	
overall income	0.05	0.08	0.04	1.19	1.26	0.01	4,250
	(0.03)	(0.05)	(0.07)	0.27	0.26	0.93	
poor	-0.08***	-0.10***	-0.10*	0.97	0.00	0.27	4,250
	(0.02)	(0.04)	(0.05)	0.33	0.95	0.60	
extremely poor	-0.07**	-0.09*	-0.09	0.70	0.01	0.16	4,250
	(0.03)	(0.05)	(0.07)	0.40	0.91	0.69	
poverty gap	-4.80*	-3.80	-0.61	0.13	1.05	0.72	4,250
	(2.75)	(4.54)	(6.74)	0.72	0.30	0.40	
extreme poverty gap	-2.98**	-2.66	-4.25	0.05	0.94	0.25	4,250
	(1.46)	(2.35)	(3.49)	0.82	0.33	0.62	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*.

*** denotes 99% significance; ** 95% significance and * 90% significance

It is reassuring to find no differential impacts on income – the figure is mechanically fixed at S/.100 per month, and significant differences in impacts depending on the amount of time in the program would raise some doubts as to the validity of the estimation technique.

It is worth noting that in the case of some indicators, the impacts significantly increase at first, before returning to levels that are indistinguishable from the impacts in the initial eleven months of participation. Such impact dynamics may be driven by unequal distribution of the sample across groups with differential participation times. Those who spent the longest time in the program are indisputably the smallest group, so it is plausible that the significance is not picked up due to the low sample size.

While duration effects in consumption and poverty indicators are indisputable, there is no evidence that the impacts on consumption or poverty accumulate, or become stronger over time. Columns IV through VI of Table 6 show that F-statistics are not sufficiently high to reject the hypothesis of coefficient equality on treatments of differing durations.

Table 7. Overtime impacts of *Juntos* on health (for children under 5)

	12 to 23 months in *Juntos*	24 to 36 months in *Juntos*	over 36 months in *Juntos*	F-test between (I) and(II)	F-test between (II) and(III)	F-test between (I) and(III)	Number of observations
	(I)	(II)	(III)				
received health checks in the last three months	0.08***	0.11***	0.13**	2.66	0.37	1.78	3,972
	(0.02)	(0.04)	(0.05)	(0.10)	(0.55)	(0.18)	
received vaccinations in the last three months	0.10***	0.19***	0.14*	7.41	2.32	0.36	3,972
	(0.03)	(0.05)	(0.07)	(0.01)	(0.13)	(0.55)	
did not experience any illness	0.01	0.03	0.10	0.20	4.19	2.38	3,972
	(0.03)	(0.05)	(0.08)	(0.65)	(0.04)	(0.12)	
sought medical attention in the event of illness	0.10**	0.18**	0.21*	2.74	0.40	1.75	1,951
	(0.05)	(0.08)	(0.11)	(0.10)	(0.53)	(0.19)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

4.2.2. Differential effects depending on treatment duration: health

The results presented in Tables 7 and 8 provide additional insights into the nature of the program's impact on child and maternal health. Earlier analyses did not detect any impact in such outcomes as vaccination among children under the age of 5, or participation in family planning activities among women. However, the present analysis shows that children who participated in the program for at least one year are ten percentage points more likely to remain healthy compared to their peers who have been in the program for less than twelve months (Table 7). Similarly, the likelihood of participation in family planning activities is significantly higher among women who have been receiving *Juntos* transfers for at least twelve months, compared to women who participated in the program for less than one year.

Table 8 also presents some evidence of the intensity effect. F-statistics in columns (IV) through (VI) are sufficiently high to reject the hypothesis of coefficient equality on different treatment spells for the use of contraceptives and having a doctor-assisted delivery. While receiving *Juntos* from 24 months to 36 months increases the likelihood of a doctor-assisted delivery by 17 percentage points compared to the treatment spell of 11 months or less, an additional 12 months increases this probability by another 17 percentage points. It would not be correct to interpret insignificant coefficients on the shorter treatment spells as evidence to confirm that the program impacts materialize only with a lag of several months; our control group in the present specification comprises the beneficiaries with the shortest treatment spell, which may already be affected. However, Table 8 clearly demonstrates that the effects accumulate over time.

Table 8. Overtime impacts of *Juntos* on health (women of childbearing age)

	12-23 months in *Juntos*	24 to 36 months in *Juntos*	over 36 months in *Juntos*	F-test between (I) and(II)	F-test between (II) and(III)	F-test between (I) and(III)	Number of observations
	(I)	(II)	(III)				
received pre-natal checks in the last 12 months	0.00	0.01	0.01	0.28	0.10	0.01	4,401
	(0.02)	(0.03)	(0.04)	(0.60)	(0.76)	(0.92)	
delivery was assisted by the doctor	0.08	0.17*	0.34***	2.92	9.26	7.99	1,034
	(0.06)	(0.09)	(0.13)	(0.09)	(0.00)	(0.00)	
did not experience any illness	-0.02	-0.02	-0.03	0.00	0.08	0.03	4,401
	(0.03)	(0.05)	(0.07)	(1.00)	(0.78)	(0.86)	
sought medical attention the event of illness	0.06	0.10	0.12	1.31	0.16	0.79	2,687
	(0.04)	(0.07)	(0.10)	(0.25)	(0.69)	(0.37)	
participated in the family planning activities	0.06**	0.07*	0.10	0.21	0.81	0.66	4,401
	(0.02)	(0.04)	(0.06)	(0.65)	(0.37)	(0.42)	
participated in health campaigns	0.00	0.01	0.02	2.36	0.52	1.68	4,401
	(0.01)	(0.01)	(0.02)	(0.12)	(0.47)	(0.20)	
used contraceptives	0.08***	0.12***	0.18***	3.00	5.90	6.17	4,401
	(0.02)	(0.04)	(0.06)	(0.08)	(0.02)	(0.01)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of Club de Madres, Vaso de Leche and Comedor Popular; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

4.2.3. Differential effects depending on treatment duration: education

As in the case with health outcomes, the present analysis detects significant impacts on the outcomes where no average impacts were previously identified. Table 9 shows that registration rates are significantly higher among children who have been receiving benefits for two years or longer compared to their peers who participated in the program for less than one year. On the other hand, in the case of attendance, while we find a significant overall effect, there is no strong evidence of differences in the impacts depending on treatment spell duration.

The combination of the two types of analysis offers greater insights into the workings of the program. While the analysis of treatment spells sheds light on the dynamics of the effects among beneficiaries, the analysis of the average effects shows the result of this process, i.e. post-treatment level compared to the level among non-beneficiaries. Putting together these two pieces of the jigsaw, one can conclude that the mechanisms through which the program works vary depending on the outcome. In the case of attendance, the program appears to affect the beneficiaries immediately – the impacts barely vary with treatment spell duration. In the case of registration, the impacts gradually build up, as evidenced by duration analysis. However, they are not sufficiently high to be detected in the comparisons with non-beneficiaries.

Sadly, along with the positive effects of the program, the intensity analysis confirms earlier findings that beneficiary children are more likely to be engaged in labor. This unintended and unexpected impact warrants further research on the mechanisms and driving forces behind it, which unfortunately are beyond the scope of this study.

Table 9. Overtime impacts of *Juntos* on education

	12 to 23 months in *Juntos* (I)	24 to 36 months in *Juntos* (II)	over 36 months in *Juntos* (III)	F-test between (I) and(II)	F-test between (II) and(III)	F-test between (I) and(III)	Number of observations
Registered at school	0.01	0.08***	0.08***	50.66	0.08	30.87	7,749
	(0.01)	(0.01)	(0.01)	(0.00)	(0.78)	(0.00)	
Currently attending school	0.01	0.03***	0.01	13.41	7.35	0.22	6,478
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.64)	
Worked last week	-0.00	0.03*	0.13***	5.38	36.52	43.26	7,820
	(0.01)	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

5. UNDERSTANDING THE RESULTS

5.1. Opening the black box: three mechanisms behind the impacts

Up to this point, this study has focused on two types of effects of the *Juntos* program: average effects estimated for all *Juntos* beneficiaries in comparison with non-beneficiaries, and treatment intensity effects, estimated for groups of beneficiaries exposed to the program for a differing number of months, in comparison to beneficiaries with (arbitrarily chosen) minimal participation in the program: less than one year.

Combining the results based on these two types of analysis, one can easily distinguish three mechanisms through which *Juntos* appears to affect the outcomes of interest. This section presents a stylized topology of the effects mediated through such mechanisms (summarized in Table 10).

Firstly, there are immediate effects: in some indicators the program triggers changes shortly after the beneficiaries join; however, the effects remain at the same level and do not intensify over time. This dynamic is manifested in significant overall effects along with the absence of significant differences in outcomes among beneficiaries with differing treatment spell duration. This group of outcome indicators includes income and school attendance.[10]

Table 10. Overall, longer-term and cumulative effects of *Juntos*

	Overall effects	Immediate effects	Longer-term effects	Cumulative effects
Consumption and poverty	(I)	(II)	(III)	(IV)
overall consumption	yes		yes	
food consumption	yes		yes	
non-food consumption	yes		yes	
income	yes	yes		
poverty	yes		yes	
extreme poverty	yes		yes	
poverty gap	yes		yes	
extreme poverty gap	yes		yes	
Education and child labor				
registration			yes	
attendance	yes	yes		
child labor			yes	

[10] The coefficient on the treatment spell of 24 to 36 months (Table 9) is marginally significant, therefore it was not considered indicative of the differences in impacts depending on the treatment spells.

	Overall effects	Immediate effects	Longer-term effects	Cumulative effects
Health (children under 5)				
health checks	yes		yes	
vaccinations			yes	
medical attention in the event of illness	yes			
did not get sick			yes	
Health (mothers)				
doctor-assisted deliveries	yes		yes	yes
use of contraceptives	yes		yes	yes
participation in family planning activities			yes	
participation in health campaigns				
medical attention in case of illness				

Secondly, there are lagged effects, which are stronger among the beneficiaries who have been in the program for a sufficient length of time. The estimates of these effects are significantly higher among the groups of beneficiaries who have spent over one year in the program compared to the group with minimal treatment durations (under eleven months). The amount of time needed for the impacts to increase compared to the baseline treatment varies: in the case of doctor-assisted deliveries, only after spending at least two years in the program do beneficiaries become more likely to seek medical help during the delivery, compared to their counterparts who have been receiving *Juntos* transfers for a shorter time. In the case of consumption, poverty, and health outcomes among children, the increase in impact compared to that for short treatment spells begins to manifest itself after twelve months of involvement in the program.

Depending on their strength, lagged effects may or may not be reflected in the average effects captured in the comparison of beneficiaries to non-beneficiaries. They should be interpreted as evidence that the program is working, while the absence of significant average effects indicates that the triggered changes are not sufficiently high to be detected in comparison with non-beneficiaries.

Finally, one can distinguish cumulative effects, which significantly increase over time. These effects are registered in doctor-assisted deliveries and the use of contraceptives by beneficiary mothers. It is important to remember that the suggested topology of effects is highly stylized and by no means rigorous. The distinction between lagged effect and cumulative effects is somewhat vague: the impacts of longer treatment spells are compared to the minimal treatment spell, which may have already triggered a change in outcome compared to non-treatment. Unfortunately, the available data does not allow one to cleanly assess whether there was any impact on the beneficiaries with the shortest

treatment spell. If the minimal treatment already had some effect, then significant impacts of longer treatment spells qualify as cumulative effects. However, it is beyond the scope of this study to identify such cases.

5.2. Ways forward

It is worth noting that of all the outcome indicators analyzed in this study, the program has had no impact at all on two indicators only: participation in health campaigns and the likelihood to seek medical treatment in the event of illness among women of childbearing age (Table 10). It is true that some of these effects are negligible: they can only be picked up when the comparisons are made among different groups of beneficiaries. Lamentably, only a small subset of the indicators considered is affected strongly enough to capture the effects in comparisons with non-beneficiaries: overall effects are significant only in consumption and poverty indicators, school attendance, health checks and likelihood to seek medical help among children under the age of 6, doctor-assisted deliveries, and the use of contraceptives among women of childbearing age. There is no evidence of overall program effects on school registration and vaccinations, likelihood to remain healthy, participation in family planning activities, health campaigns, or likelihood to seek medical attention among women.

This combination of significant results in nearly every category of interest in the analysis of treatment intensity, and a much less impressive record of significance when overall impacts are considered, suggests that *Juntos* definitely works, but the pace at which it affects the beneficiaries leaves considerable room for improvement. What might accelerate this pace? A few options are worthy of consideration.

First, an increase in the size of the transfer could work as such an accelerator. *Juntos* transfers as a percentage of beneficiaries' average monthly consumption ranks relatively low alongside CCTs in other Latin American countries (Figure 2). Moreover, unlike other CCTs, the transfer is the same regardless of the number of children in the beneficiary family. This rule implicitly penalizes families with higher numbers of children, thus making the program less attractive to them. Introduction of a differentiated payment scheme depending on number of children could address this problem.

Second, the problem of slow impacts may be addressed via improvements in program administration. Surprisingly, although school attendance is one of the conditionalities, only 80 percent of school-aged children in beneficiary families were registered in 2009. Moreover, there is no evidence of an overall impact on school registration. These facts clearly point to the potential for improvement in the system of verifying compliance with conditionalities until 2009.[11]

[11] Program restructuring, which took place in 2010, may in time prove to be addressing these problems.

The third means of addressing the program is through information and communication. Misunderstanding of conditions triggers the same effect as deliberate non-compliance. It may be worthwhile for program management to explore options for improving information campaigns and communication strategies.

Finally, provision of medical and educational services in the districts where the program operates may be an important prerequisite for success. Even if beneficiaries are willing and eager to comply with the conditions to get their children vaccinated, the local health centers may not necessarily have adequate supplies, and the vaccination rate will not increase as a result. Unfortunately, there is no data available to test whether this hypothetical example is borne out in reality. While it is highly plausible that the impacts of *Juntos* are reinforced by access to high quality education and medical services, data on the provision of these services is lacking. The hypotheses on the effect of the extent of service provision on program effect heterogeneity remain to be explored in future work.

6. CONCLUSION

This study presents a quantitative impact evaluation of the CCT program *Juntos* in Peru. Taking advantage of a larger sample of data and improved identification strategy than was available in previous analyses, the study addresses two issues. Firstly, it estimates the overall impact of *Juntos* five years after its rollout. Secondly, it explores differential impacts among beneficiaries, depending on program involvement duration. This analysis seeks to answer two questions: (i) whether it takes time for the program to make significant and sizable impacts; (ii) whether some impacts change in magnitude the longer the beneficiaries spend in the program.

The results point out two compelling effects of the program. Almost all indicators of interest are significantly higher among beneficiaries with longer treatment spells. However, in many cases these improvements are too small to be picked up in the analysis of overall effects, when beneficiaries are compared to non-beneficiaries. These findings suggest that while the program indisputably works, there remains significant room for improvement.

APPENDIX 1. ADDITIONAL TABLES

Table A1. First stage regression results

Dependent variable in second stage regression	sample	coefficient on the instrument	number of observations	F-statistic
overall consumption	households	0.010***	14,670	543
		(0.000)		
registration	children aged 6 to 14	0.004***	17,473	83
		(0.000)		
attendance		0.004***	13,973	59
		(0.000)		
worked the previous week		0.004***	17,771	85
		(0.000)		
health checks	children under 5	0.005***	8,992	43
		(0.001)		
vaccinations		0.005***	8,992	43
		(0.001)		
did not experience any illness		0.004***	8,992	41
		(0.001)		
sought medical help in the event of illness		0.004***	4,466	19
		(0.001)		
pre-natal checks	women of childbearing age	0.007***	11,369	169
		(0.001)		
Doctor-assisted birth		0.006***	2,309	22
		(0.001)		
did not experience any illness		0.007***	11,369	171
		(0.001)		
sought medical help in the event of illness		0.008***	6,922	124
		(0.001)		
participated in family planning events		0.007***	11,369	169
		(0.001)		
participated in health campaigns		0.007***	11,369	172
		(0.001)		
used contraceptives		0.007***	11,369	168
		(0.001)		

*** denotes 99% significance; ** 95% significance and * 90% significance

APPENDIX 2. ROBUSTNESS CHECKS: MATCHING RESULTS

Matching analysis is intuitively based on comparison of beneficiaries to would-be beneficiaries in the districts that have not been yet incorporated into the program, but which are included in program expansion plans. The districts that were enrolled in *Juntos* earlier may differ in important dimensions from the ones that are still awaiting incorporation into the program. The government of Peru originally planned to rollout the program in descending order of the summary indicator, or from poorer to less poor districts. Although this order was not adhered to, t-tests of differences in means tests show that poverty rate, prevalence of malnutrition, and exposure to violence are higher in the treated districts. Such district-level differences are likely to result in the underestimation of the program's effects: the base level of outcome indicators is likely to be higher in the control districts, while compliance with conditionality may be more difficult in the treatment districts (for example, due to higher supply of medical services). Such a possibility for bias makes IV regressions the preferred technique of this study. However, it is encouraging that many of the outcome indicators have the same sign and similar magnitude in both specifications. All results based on matching specification are presented below.

Table A2. *Juntos* impacts on consumption and income (matching)

	coefficient and standard error	number of observations
overall consumption	0.01 (0.01)	21,256
consumption of food	-0.10*** (0.01)	21,117
consumption of non-food	0.21*** (0.02)	21,256
overall income	0.09*** (0.01)	21,256
poor	-0.00 (0.01)	21,256
extreme poor	0.07*** (0.01)	21,256
poverty gap	8.99*** (1.11)	21,256
extreme poverty gap	2.49*** (0.54)	21,256

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*.

*** denotes 99% significance; ** 95% significance and * 90% significance

Table A3. *Juntos* impacts on utilization of medical services and health (for children under 5)

	coefficient and standard error	number of observations
received health checks in the last three months	0.38***	9,853
	(0.01)	
received vaccinations in the last three months	0.03*	9,853
	(0.01)	
did not experience any illness	-0.02	9,853
	(0.02)	
sought medical attention in case of illness	-0.12***	5,005
	(0.02)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

Table A4. *Juntos* impacts on utilization of medical services and health (for women of childbearing age): matching

	coefficient and standard error	number of observations
delivery was assisted by a doctor	0.16***	1,991
	(0.03)	
did not experience any illness	-0.04***	9,209
	(0.01)	
sought medical attention in the event of illness	0.03	5,433
	(0.02)	
participated in family planning activities	0.07***	9,209
	(0.01)	
participated in health campaigns	0.01***	9,209
	(0.00)	
used contraceptives	0.08***	9,209
	(0.01)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include age, years of schooling, marital status, number of hours worked the previous week, type of work (agricultural or otherwise, salaried or otherwise).

*** denotes 99% significance; ** 95% significance and * 90% significance

Table A5. *Juntos* impacts on education (for children aged 6 to 14): matching

	coefficient and standard error	number of observations
Registered at school	0.04***	18,555
	(0.01)	
Currently attending school	0.00	14,796
	(0.00)	
Worked last week	0.15***	18,555
	(0.01)	

Control variables include: geographical controls, proxy means test index, indicators for access to drinkable water, electricity and sanitation, materials used in dwelling construction, indicators equal to 1 if the household head works in agriculture. District level variables include: monetary poverty, severity of poverty, poverty measured as unsatisfied basic needs, proportion of urban centers affected by violence, number of hospitals, healthcare centers, pharmacies, and number of organizations and beneficiaries of *Club de Madres, Vaso de Leche* and *Comedor Popular*; individual level characteristics include gender and age.

*** denotes 99% significance; ** 95% significance and * 90% significance

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