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Do benefit recipients change their labor supply after receiving the cash transfer? Evidence from the Peruvian *Juntos* program

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Abstract

We investigate the short-term labor supply responses to a Conditional Cash Transfers program in Peru. Rather than comparing treated and non-treated households, we examine how benefit recipients change their labor supply after receiving the cash transfer. Our empirical strategy exploits exogenous variation in the distance between the program's payment schedule and interview dates from the Peruvian National Household Survey. Results suggest that cash recipients reduce their labor supply by 6–10 hours in the week following the payment date. This reduction in hours of work is larger for married women and for mothers with children aged 5 or less. In addition, results are robust to different specifications, changes in the sample and a placebo test.

JEL codes: I38, J22
Keywords: Conditional cash transfers; Labor supply; Juntos; Peru

1 Introduction

Around the world, Conditional Cash Transfer programs (henceforth, CCTs) are considered powerful means to reduce poverty. By providing monetary transfers to families conditional on a set of fulfillments, such as school attendance and health care of children, the objective of CCTs is twofold. The first is alleviation of current poverty through periodical stipends; allowing families to increase overall consumption. The second goal is to reduce future poverty by increasing human capital of children, which is achieved by means of program conditionalities.

During recent years, CCTs have received a great attention from policymakers and academics, since significant reductions in poverty levels have been observed after their implementation. Furthermore, these programs have been catalogued as one of the main models of safety-nets in developing economies. After the success of programs such as *Bolsa Escola* in Brazil and *PROGRESA* in Mexico "virtually every country in Latin America has such a program" (Fiszbein and Schady 2009).

Most of the existing literature on the effects of CCTs has focused on scholastic achievement, health and nutritional outcomes of children. However, less attention has been paid to the indirect effects that cash transfers could have on adults' behavior. More specifically, little is known about the effects of CCTs on adult labor supply. While cash transfers are necessary to accomplish improvements in consumption, education and health, they can

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also generate incentives to reduce work intensity among adults, since this payment can be thought of as a pure *income* effect.

Recent experimental evidence has shown small effects of CCTs on labor supply of adults from beneficiary households (Parker and Skoufias 2000; Maluccio and Flores 2005; Skoufias and Di Maro 2008; Galasso 2006; Foguel and Paes de Barros 2010). This literature relies on comparisons between beneficiaries and non-beneficiaries to estimate the so-called average treatment effect of CCTs on labor supply. Nevertheless, there is no evidence on the immediate labor supply response to cash benefits.

This article deviates from the previous literature in two subtle but important ways. On the one hand, this study represents the first attempt to analyze the transitory effects of welfare programs, namely CCTs, on labor supply. That is, we do *not* aim to estimate the average treatment effect of CCTs on labor supply. Instead, we are interested in exploring whether benefit recipients change their labor supply after they receive the cash transfer. On the other hand, we adopt a novel empirical strategy which exploits exogenous variation in the difference between the program's pay dates and interview dates of a household survey. The combination of pay dates and interview dates allows us to compare beneficiaries' labor supply before and after receiving the cash transfer. We think of these deviations as representing our contribution to the literature on the labor supply responses to cash transfers.

There are several reasons why analyzing immediate labor supply responses to cash transfers can be of particular interest. First, cash recipients are independent workers and the available evidence suggests that such workers do not behave according to life-cycle models of labor supply but instead they work "one day at a time" (Camerer et al. 1997; Fehr and Goette 2007; Goette et al. 2004). Moreover, these studies argue that independent workers (who are free to choose when and how much to work) are better described as having income targets: once they reach their income target they stop working. Second, beneficiaries of CCTs are, by construction, credit constrained. These restrictions may prevent households to smooth consumption and leisure and, therefore, both variables may react to the timing of cash transfers. Indeed, empirical studies have shown that consumption of welfare recipients jumps up after the pay date and then declines (Shapiro 2005; Mastrobuoni and Weinberg 2009). Third, benefit recipients of CCTs live in rural areas where access to markets is quite limited. In such locations, every time beneficiaries are paid, they must incur in transportation costs (money but also time). Therefore, these short-term responses are relevant for the design of CCTs. In particular, the time that beneficiaries spend picking up the money is an opportunity cost that policy makers should take into account when choosing among alternative payment methods (bank deposits versus cash-in-hand) and frequencies (monthly versus bimonthly).

We find that cash recipients (female household heads) hours of work are reduced by 6 hours in the week following the pay date. This reduction is rather large, since it implies a decline of roughly 20% of their weekly hours of work. Moreover, this decrease in hours of work is larger for married women and mothers with children aged 5 or less. However, no significant effects are found for labor force participation, nor for the probability of working for paid activities. We do not find significant effects of cash transfers on the labor supply of recipients' partners (when we restrict the sample to married recipients).

The document is structured as follows. Related literature is reviewed in Section 2. In Section 3, we describe the program, named *Juntos* and its mechanics. Section 4 presents

the econometric set-up and describes the data. Section 5 presents the results and additional robustness checks. In Section 6 we discuss our results and make comparisons with respect to previous empirical findings. Section 7 concludes.

2 Literature review

2.1 Theoretical considerations

Research on labor supply responses to welfare programs has long been a subject of interest for economists, especially in developed economies where the expansion of benefit transfer programs to low-income population was initiated during the 1960s. Since then, researchers and policy-makers have been concerned on how welfare programs affect working incentives of beneficiaries as well as the indirect (unintended) effects these transfers may generate on non-targeted population living in localities covered by the program.

For instance, the effect of welfare programs on labor supply has been widely studied. The most prominent programs are Aid to Families with Dependent Children (AFDC), the Earned Income Tax Credit (EITC), and more recently the Food Stamp Program in the U.S. along with the Working Families Tax Credit in the U.K. (see Moffitt 2002 for an extended review and discussion). The discussion of how welfare participation affects labor supply of adults can be divided according to (i) the predicted effects of the canonical model of labor supply, (ii) program conditions, and (iii) models of household labor supply.

The potential effects of benefit transfers can be explained based on the basic static model of labor supply. In this model, individuals maximize between consumption and leisure facing a budget constraint, which is composed by labor (wage) and non-labor (initial wealth and monetary or in-kind transfers) income. In this study, we focus on the particular role CCTs can play in determining working incentives[1].

As pointed out by Alzúa et al. (2013), CCTs have four potential channels through which adult labor supply could be affected. First, cash transfers represent an increment in non-labor income. Given that no conditions are imposed with regard to labor effort of beneficiaries, this lump-sum transfer is a pure income effect, and therefore, both employment and working hours are expected to decline. Second, program conditions can also alter working behavior of adults. For instance, most of the conditions attached to cash transfer programs imply school enrolment and a maximum number of days accepted for children to be absent from school. This increase in school attendance of children allows parents to augment labor participation and working hours as well, for they avoid allocating time in childcare. Third, if child labor is crucial in determining households' budget constraint, increasing school attendance would also affect adult labor supply. Fourth, cash transfers can also affect local markets, and thereby, have an indirect impact on non-beneficiaries. Using a sample from the Mexican *PROGRESA* program, Angelucci and De Giorgi (2009) find that consumption of ineligible households increases in villages where the program was randomly implemented. Alternatively, qualitative studies (Segovia 2001, for example) have described the appearance of fairs ever since CCTs arrived to different localities[2].

Another important consideration is whether welfare programs impose arbitrary restrictions on adult labor supply in order to circumvent working disincentives. Despite the initial unconditional intent related to working effort, some developed countries have indexed program benefits according to the labor supply behavior of beneficiaries. For

instance, the Temporary Assistance for Needy Families (TANF) program in the U.S. (formerly known as the AFDC) initially imposes that at least 20% of TANF recipients in each State participate in work or work-related activities for a minimum of 20 hours per week. These activities include regular employment, subsidized employment, commuting, on the job training, and 12 months of vocational training for young beneficiaries aiming to participate in the labor force. Alternatively, the EITC program, also in the U.S., consists in a refundable tax credit for low- and medium-income families which increases according to a standard range of annually labor income and the number of qualifying children in the family[3]. These types of cash transfers, both conditional on minimum working hours or increasing with earned income, act like a contract rigidity, not allowing individuals to make optimal allocation of working hours. Thus, especially in the case of the EITC where the benefit is attached to labor income, the response on individual working effort would depend on which of the two possible effects - substitution or income - prevail. Empirical findings suggest that it is participation (entry) rather than hours of work which responds to the EITC[4].

Unlike these "tied welfare benefits", CCTs in Latin American do not restrict eligibility on labor force participation[5]. This lack of restrictions implies that the *looseness* of the budget constraint due to the welfare benefit introduces a pure income effect, hence, encouraging beneficiaries to demand more leisure. Further, if those individuals barely ineligibles (say because of being just above the poverty line) reduce their working effort in order to narrow down total income and "cheat" the system to become eligibles, then the net effect of CCTs on labor supply would depend not only on the amount of reduced working hours of the ever-eligibles and the formerly ineligibles, but also on the behavior of the latter group once they have been selected as program beneficiaries and the transfer has been received (e.g., they can return to their initial - optimal - working effort)[6].

An open question is who in the family actually reduces his working effort. Since cash is usually transferred to a particular household member (i.e., housewives), it is worth taking into consideration how welfare is distributed among family members. For this reason, theoretical considerations of models of household labor supply can also add useful insights. In this line, aside from the potential effects of CCTs on individual adult labor supply, there exists an open debate on whether families pool their welfare resources. According to this hypothesis, family members act as if they are maximizing a single utility function. Two separate models have been developed associated to this "unitary" behavior: the "agreement" (Samuelson 1956) and the "dominant family member" frameworks (Becker 1981).

Maximizing a single utility function implies that, regardless of who receives the welfare income, each of the family members would benefit from the monetary transfer through an intra-family allocation process. In contrast to this "common will" frame, individual cooperative utility models of intra-family bargaining processes (Manser and Brown 1980; McElroy and Horney 1981; and Lundberg and Pollak 1993) as well as non cooperative bargaining models (Lundberg and Pollak 1994) have also been postulated. In these models, income is administered by a single agent within the family (for example, the mother) and thus allocation of resources on consumption and leisure could differ across household members.

Recent empirical evidence suggests that single cooperative utility functions prevail in the family bargaining process. Regarding welfare benefits, Lundberg et al. (1997) test the hypothesis of whether families pool their resources exploiting a U.K. policy change which

dictated that child allowances were to be transferred exclusively to wives (mothers). The authors find evidence that this policy change induced women to spend more resources on women's and children's clothing relative to men's clothing. In spite of labor supply, Bertrand et al. (2003) suggest that drops in prime-age men's labor supply are stronger than that of prime-age women when the South African pension benefits are received by women. In a recent study, Ardington et al. (2009) discuss that pension benefits could, in the case of perfect resource sharing within the family, reduce hours of work and participation of adults, or in the case of imperfect credit markets, social pensions can be used as a credit support for job seekers.

2.2 Empirical evidence from Latin American countries

To the best of our knowledge, seven empirical studies have been carried out addressing the potential effects of CCTs on adult labor supply in Latin American countries. Identification strategies of most of these studies are based on the fact of random treatment (most of them at the village level) of the CCTs across the targeted population.

Parker and Skoufias (2000) exploit the experimental design of the Mexican *PROGRESA* program (currently known as *Oportunidades*), which randomly assigned treated and control villages, to address the question of whether CCTs alter labor participation and overall leisure time of adults. The authors find no significant effects of program participation on participation rates in the labor force. Instead, they do find that women are more likely to reduce hours allocated to leisure mainly because of program commitments such as taking children to schools, health centers and participating in community work.

In a later study, Skoufias and Di Maro (2008) evaluate the effects of *PROGRESA* on outcomes measuring adult labor supply. Alike Parker and Skoufias (2000) their identification strategy relies on a difference-in-differences estimation procedure comparing eligible adults living in treated villages (beneficiaries) versus eligible adults living in non treated low-income Mexican villages. The authors do not find statistically significant effects of program participation on the probability of being employed. Moreover, given random assignment of program deployment across villages, the authors find that cross-sectional estimates of CCTs on working hours of adults living in treated villages are not statistically different from working hours of adults living in (randomly) untreated villages.

Using a similar estimation methodology for the Nicaraguan *Red de Protección Social (RPS)* program, Maluccio and Flores (2005) find that program participation reduces men's (but not women's) working effort by 5.5 hours. Maluccio (2010) analyzes the effect of *RPS* on the overall household labor supply; that is, the sum of each member's labor intensity. The author finds a negative small but statistically significant effect of the program on household hours of work, especially in agricultural activities, and argues that this reduction can be explained based on the fact that these activities are perhaps associated to lower marginal rates of return. In contrast, Foguel and Paes de Barros (2010) find no statistically significant effects of six Brazilian programs (*Bolsa Escola*, *Bolsa Alimentação*, *Bolsa Família*, among others) on adult labor supply, neither on the extensive nor the intensive margins.

Galasso (2006) uses propensity score matching and regression discontinuity methods for evaluating the impact of *Chile Solidario* on adult labor supply. Although positive impacts are found for the take-up of labor market programs, such as re-insertion and training programs, the author finds no increments on the share of beneficiaries who are

employed, nor on the share of beneficiaries who have a stable employment. However, increases in participation rates in the labor force are observed only for rural areas.

Finally, Alzúa et al. (2013) find negative but small - if not inexistent - effects of three different programs from Latin American countries (*RPS* in Nicaragua, *PROGRESA* in Mexico, and *Programa de Asignación Familiar* in Honduras) on adult labor force participation and the probability of migrating from agricultural to other working activities. However, they do find a reduction of about 4.7 to 6.3 weekly hours worked in the case of Nicaraguan *RPS* and a positive and significant effect of Mexican *PROGRESA* program on male wages.

Most of the aforementioned studies rely on the experimental design of the different programs evaluated, and most of them (with the exception of Galasso 2006 and Skoufias and Di Maro 2008) fail to control for the possibility of reallocation of working effort of ineligibles in communities or villages regarding program deployment, as pointed out by Angelucci and De Giorgi (2009). Not taking into account this potential effect may introduce negative bias (in absolute terms) to the parameters of interest assuming that ineligibles are more prone to increase their labor intensity given the increase in the demand for consumable goods and agricultural productive assets in days nearby the transfer schedules. Because this potential increase in the demand of a particular set of goods may increase real wages of ineligibles (introducing a substitution effect), previous empirical findings based on double-difference comparisons are likely to understate the labor supply responses to CCTs.

Unlike previous studies we adopt a different approach to measure labor force variations as a response to welfare income. In particular, we are interested in exploring whether working behavior changes in days near *Juntos* pay dates. Although this analysis does not allow us to identify average treatment effects of program participation on working effort of adults, it is useful for reconciling theoretical aspects of the canonical labor supply model with empirical evidence. The advantage of examining short-term effects of cash transfers on labor supply of adults is that: (i) it is possible to disentangle *income* effects from general equilibrium effects often observed in the long run, and (ii) capture effects of transfers itself and no other effects such as labor supply responses of parents to a reduction in child labor introduced by the program.

3 The program and its mechanics

3.1 Background

The Peruvian *Juntos* program was implemented on April 2005 after a period of political upheaval and relatively economic stagnation experimented at the beginning of the new century during government transition. By 2002, with the new economic reforms brought up by the former president Alejandro Toledo, the country's economy began to recover reaching growth rates above 5% by the mid-2000s. Together, the economic expansion and the implementation of welfare programs focusing on poverty alleviation and job creation lead to a significant increase of mean per capita income (from US$ 2,450 by 2005 to US$ 4,050 PPP by the end of year 2010) and, more strikingly, a sharp reduction of roughly 50% of the overall poverty rate which went from 54% in the mid-2000s to 27% by the end of the last decade.

Particularly, *Juntos* periodically transfers a stipend to families living in poverty and extreme poverty conditions, and in return, families must meet certain requirements

including schooling and health care. The program was created with the aim of strengthening government presence in remote areas of the country and providing eligible families with a set of health, nutritional, educational, and identity services, for enhancing health and nutritional status of pregnant women and their babies, nursing women, and infants, as well as fostering human capital accumulation of children under age 14.

Juntos is still the most remarkable amid the social welfare programs in Peru, which has generated the greatest impacts on poverty alleviation and human capital accumulation of children. This can be noticed through the great expansion of the program since it began to operate. By 2005, almost 22,500 households living in 26 municipalities benefited from this program, whereas in 2012 *Juntos* was deployed in almost 1,011 municipalities, representing roughly 55.2% of the national territory and benefiting 649,553 households living in poverty and extreme poverty conditions. Recently, Perova and Vakis (2012) found that *Juntos* increased overall household income by 43% and was responsible of a 16% and 30% decrease in poverty and extreme poverty rates, respectively, in municipalities where *Juntos* was initially deployed[7].

In terms of investments, public expenditures generated by *Juntos* went from US$ 45 million in 2005 to US$ 177 million in 2007. In this latter year, there was a noticeable expansion of the program along the Peruvian territory, covering almost 612 more municipalities and more than 400,000 households relative to the 2005 wave. By the end of 2012, public expenditures associated to *Juntos* were calculated to be almost US$ 225 million. This figure represents roughly 18% of the Peruvian expenditures in safety-net programs.

3.2 Eligibility

Juntos is a means tested program. As Perova and Vakis (2012) clearly describe, selection of the beneficiary households consists in three steps. The first one is related to selection of eligible municipalities. This selection is based on five criteria: (i) exposure to violence during the late 1980s and early 1990s terrorism era; (ii) poverty level, measured as the proportion of population with unsatisfied needs; (iii) poverty gap; (iv) level of under five chronic malnutrition; and (v) presence of extreme income poverty.

The second step consists in a census of all households in eligible districts collected by the Instituto Nacional de Estadística e Informática (INEI). A proxy means formula was used to determine household eligibility, based on poverty. Only households with the presence of children under age 14 or pregnant women were selected. The algorithm for defining eligibility of households is based on a Logit model, which estimates the probability of a household living in poverty conditional on a set of observable characteristics.

Finally, the third stage consists in community validation. This was done in community assemblies, carried out by local authorities and representatives of the Ministry of Education and Ministry of Health with the aim of minimizing inclusion and exclusion errors. In general, final selection depends on community validation, and once the household is selected, the housewife (household recipient) must sign a letter in which the household is committed to meet the co-responsibilities, and a health center or post is selected in order for the beneficiaries to make their periodical medical checkups.

Once the household is enrolled in the program, transfers are given to the female head of the household according to the payment schedule defined by the program's administration. According to the Peruvian National Household Survey (ENAHO, for its Spanish

acronym), almost 99% of female heads reported to be receiving the transfer on a monthly basis.

3.3 Components

Initially, the monthly amount was 100 Nuevos Soles (Peruvian local currency). This amount is roughly equivalent to US$ 37 (in current dollars). Since 2010, however, the amount was doubled (200 Nuevos Soles) but beneficiaries would receive the cash transfer every two months so that the level of the annual amount remained unchanged (1200 Nuevos Soles). This change was introduced because of the low rate of money withdrawal from bank accounts given the long distances beneficiaries must travel in order to pick up the money. In our context, the monthly transfer was quite generous, representing over 50% of beneficiaries' monthly per capita household expenditures.

Pay dates are defined at the village level which implies that some municipalities have more than one payment date. *Juntos* sets a particular day in every village so we have some within-municipality variation in payment dates. However, within a district, all payments occur on the same week. This feature of the program does not represent a major problem to our strategy as it will be shown in Section 5.

Once they receive the cash, beneficiaries are free to choose how they spend the money. However, all beneficiaries must meet the following conditions: (i) children of ages 6–14 years attend at least 85% school classes; (ii) children of ages 0–59 months get fully immunized and visit health centers where their growth is measured and vitamins are provided; (iii) children of ages 3–36 months get nutrition supplements; (iv) pregnant women visit health clinics for prenatal care; (v) nursing women visit health centers for postnatal care; (vi) parents attend health clinics to receive information about nutrition, health and hygiene; (vii) parents without ID (identification) attend the program Mi Nombre (*My Name*). *Juntos* was initially intended as a program of temporary assistance to families, with a duration of 4 years, conditional on households escaping from poverty. Yet, impoverished households can renew their participation for 4 more years with a benefit reduction of 20%.

In 2009 there were two payment methods through which beneficiaries can receive the cash transfer. The main way to receive the cash was to go to the local branch of the Peruvian National Bank and withdraw the money (54% of the beneficiaries in our sample). The second way was to go to the main square of the village on the day of payment and wait for an armored van which contained the money. The difference between these methods is that the former allows the beneficiary to go to the bank at some other day while the latter does not. Moreover, the armored van constitutes a deliver mechanism in which beneficiaries do not spend much time in going to withdraw the money. Finally, both systems are mutually exclusive at the village level so beneficiaries do not choose the way they get the money.

4 Methodology

4.1 Econometric model

Differences between Juntos pay dates and ENAHO's interview dates within a given municipality constitute the basis of our empirical strategy. In particular, we will explore whether labor supply is reduced in the days near the pay dates. What we do in practice is to compare beneficiaries, within the same municipality, who are interviewed just after the

payment to those who are not. Given that most households members are engaged in agricultural and highly-flexible occupations (i.e., independent workers), it is likely to observe that individuals reduce their working effort in days following the cash receipt.

Though we exploit within-municipality variation in interview dates of ENAHO, our measure of temporal distance is constructed as the difference between pay dates and the week previous to the survey. This week prior to the interview day is called the "reference week". When interviewers survey households, they usually ask household members whether they have done specific activities during the last seven days. For instance, when asking about labor force participation, interviewers ask the following question: "during the last week, from [day 1] to [interview day], did you have any job?". Given the way the survey is conducted, all outcomes related to labor supply of surveyed members correspond to the seven days prior to the interview day (i.e., the reference week).

For the empirical analysis, we construct four dummy variables according to the distance between the pay day and the reference week. Specifically, the first dummy variable is equal to one if the pay date takes place, at least, two weeks before the reference week, and zero otherwise. Similarly, the second dummy takes the value of one when the pay date occurs one week before the reference week, and zero otherwise. The third dummy variable is equal to one if the pay date happens some day during the reference week, and zero otherwise. Finally, the fourth dummy variable takes the value of one when the pay date takes place after the reference week, and zero if not[8].

We divide the temporal distance between pay dates and interview dates in terms of weeks for two reasons. First, as pointed earlier, within a given municipality there exists a probability to observe more than one pay date. This is because administrative records on pay dates are available at the village level, which is the smallest geopolitical unit in Peru. Yet, ENAHO dataset contains geographical identifiers only at the municipality level (which may contain more than one village). This data limitation forces us to collapse administrative records containing payment dates by village at the municipality level in order to merge them with the ENAHO dataset. When doing so, almost all villages (99.8% in our sample) within a given municipality are observed to have pay dates during the same week. Second, it has been observed that not all beneficiaries withdraw the money on the very same pay date. For this reason, we assume a rather more parsimonious definition of temporal distance which allows for some delay in order for beneficiaries to have the money.

When observing a municipality with more than one pay date, we use the earliest pay date to define the distance between payments and interviews. Though this criterion may introduce measurement bias, we also perform additional regressions using the last pay date within the municipalities to re-define temporal distance between payments and interviews. In Section 5.3, however, we perform an additional sensitivity analysis to check whether results hold when using exact payment dates.

Figure 1 depicts hours worked in the reference week for distinct groups of beneficiaries according to the distance (in weeks) between pay dates and the reference week. The first group is composed of individuals who received the cash transfer two or more weeks before they were interviewed (i.e., received the transfer at least two weeks before the reference week). Similarly, the second, third, and fourth groups are composed of individuals for whom the cash transfer occurred one week before, during, and one week after the reference week, respectively. The decline of hours worked during the reference week is

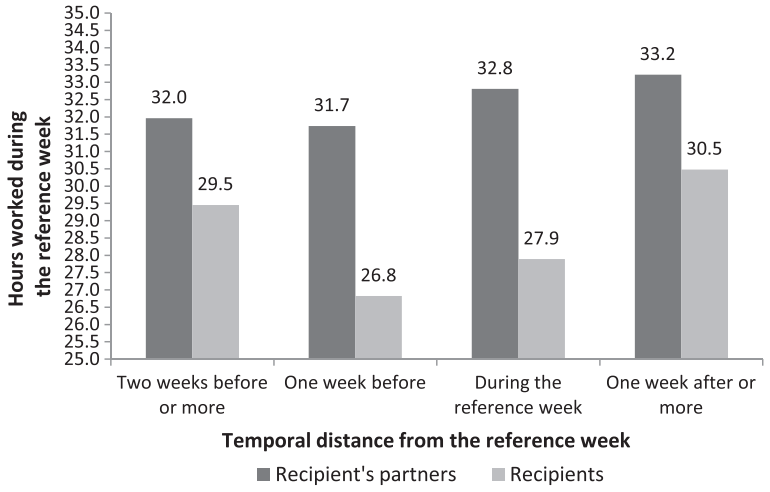

Figure 1 Hours of work according to the distance between pay and reference week. *Notes*: Dark grey bars correspond to hours of work during the reference week of recipient's partners. Light grey bars correspond to hours of work during the reference week of benefit recipients (housewives). Each pair of bars represents weekly hours of work according to the temporal distance (in weeks) between the pay and interview dates. "Two weeks before or more" implies that the payment was observed to occur at least two weeks before the reference week. "One week before" implies that the payment occurred one week before the reference week. "During the reference week" denotes that the payment occurred in some day corresponding to the reference week. "One week after or more" denotes that the payment will occur one week after the individual is surveyed.

linked to the week in which the transfer is received for all individuals included in our sample, and this decline is largest among those who receive the cash transfer one week before the reference week. Furthermore, this decline is larger for cash recipients than for their partners. This greater decline in weekly hours worked can be interpreted as containing effects of (i) time spent in going to withdraw the money, (ii) time taken to spent the money once it is received (i.e., purchasing consumption goods), and (iii) income effects. The first two effects are related to the program's features, and do not represent disincentives to work. Thus, we are particularly interested in isolating the third effect from the overall labor supply response to welfare transfers.

Based on the graphical evidence and exploiting within-municipality differences between pay dates and interview dates (reference week), we estimate the following model:

$$y_{ij} = \alpha + \sum_{k} \delta_k d_{ij} + X_i'\beta + \lambda_j + \mu_{ij}, \tag{1}$$

where y_{ij} is the outcome variable (labor force participation, hours of work, etc.) of individual i living in municipality j, d_{ij} denotes the distance (in weeks) between the payment and the reference week, X_i is a vector of individual characteristics, λ_j is a vector of municipality fixed effects, and μ_{ij} is an error term capturing all other omitted factors. Our parameters of interest are denoted by δ_k, which measures the effect of the distance between pay dates and interview dates. Therefore, these parameters are recovered using across-municipalities variation in pay dates and within-municipality variation in interview dates. In what follows, the omitted category is that the payment takes place at least two weeks before the reference week (i.e., the first dummy variable).

In this specification, each dummy may capture a specific effect related to the distance between pay dates and the reference week. For instance, the second dummy variable, "payment occurs one week before the reference week", might capture effects related to purchasing consumable goods with the cash received, whereas the third dummy variable, "payment occurs during the reference week", could capture effects related to the time spent in going to the bank and picking up the money. The fourth dummy variable, "payment occurs one week after the reference week", may capture additional changes in working effort of individuals in days prior to the cash transfer.

There are some caveats in our empirical strategy which are worth describing with further detail. First, we only have information on pay dates established by *Juntos* but we fail to observe the actual date the beneficiary went to the bank to withdraw the money. For this reason, we assume that recipients withdraw the money within the week in which the cash transfer was made available[9]. Second, it may be possible that when interviewers arrive to a given municipality, they begin to survey families who work less (i.e., those who are almost always present at home) and then survey families who work harder. If this were the case, our estimates should be seen as a lower bound (in absolute terms), since those who were surveyed earlier are more likely to be captured in the omitted category of distance between pay and interview dates, and by construction, all our parameters of interest are interpreted as a function of the omitted category. Thus, this omitted category captures the average working intensity of individuals who were surveyed earlier, and are presumed to have a lower labor intensity[10]. Finally, our indicators of distance between pay and interview dates may capture other effects not related to the transfer but correlated with other unobservable variables. For instance, it could be the case that pay days are established on days when labor supply is low for a different reason than the transfer (e.g., holidays). To check this is not the case, we perform falsification tests in Section 5.3.

Variation in pay and interview dates is crucial to our strategy. In Table 1, we present the distribution of payment dates associated with the cash transfer from *Juntos*. Regarding the day of the month, we do not find any special pattern. If anything, we could say that there is a slight concentration around the third week of the month, between the 16th and the 20th day. Regarding the day of the week, it seems that Mondays are the most common day of payment while Sundays are the least frequent. The distribution of interview dates is presented in the bottom half of the table. The frequency of dates looks pretty balanced throughout the month. It is also worth noticing that almost all interviews are conducted on Sundays, when most of the family members stay at home. Finally, the survey process within a municipality has an average duration of 30 days.

4.2 Data

Our primary source of information is the ENAHO conducted in 2009 by INEI. The ENAHO collects individual level information and is a nationwide representative survey, both in urban and rural areas. We use information from the employment and income registry, which restricts the sample only for individuals aged 14 or older. The ENAHO has three important features. First, it includes several questions which allow us to accurately identify households receiving monetary transfers from *Juntos*. This is particularly important since the program design refers to women as the benefit recipients. Second, this survey includes questions regarding relationship with the family head, enabling us to distinguish the potential impact for different household members, say male heads and

Table 1 Distribution of payment and interview dates

Panel A: Payment dates	**Frequency**	**Percentage**
Day of the month		
1-5	323	8,5
6-10	485	12,8
11-15	726	19,2
16-20	1006	26,6
21-25	712	18,8
26-31	529	14,0
Day of the week		
Sunday	178	4,7
Monday	1,099	29,1
Tuesday	512	13,5
Wednesday	490	13,0
Thursday	648	17,1
Friday	449	11,9
Saturday	405	10,7
Panel B: Interview dates	**Frequency**	**Percentage**
Day of the month		
1-5	664	17,0
6-10	579	14,8
11-15	892	22,9
16-20	556	14,3
21-25	624	16,0
26-31	584	15,0
Day of the week		
Sunday	3,780	99,97
Monday	1	0,03

Sources: Juntos administrative data (payment dates) and ENAHO surveys (interview dates).

female spouses (cash recipients). Finally, this dataset provides a rich set of variables that allows us to construct different labor supply outcomes and include a wide set of controls in our regressions.

To precisely estimate the impact of the proximity to the payment date on labor supply outcomes we need a representative sample of all municipalities which are beneficiaries from *Juntos*. By 2009, 638 municipalities were part of the program. Given that the ENAHO follows a stratified sampling procedure, this survey collected information in 260 municipalities enrolled in *Juntos* in this particular year. This represents roughly 40.8% of the municipalities in which the *Juntos* program was present in 2009. Nevertheless, when expanding the sample using the survey weights from the sampling design, Perova and Vakis (2012) find that the number of households which report receiving cash transfers from *Juntos* surveyed in the ENAHO 2009 is very close to the actual number of beneficiary households listed in the program's official records. We therefore use sample weights in all of our regressions and correct standard errors taking into account ENAHO's sampling design.

As an additional concern we check whether the transfer conditions are consistently reproduced in each of the surveyed households. In other words, we check that (i) the benefit recipient is the mother (female household head or the head's spouse), (ii) women report to receive 100 Peruvian Nuevos Soles (around 37 U.S. current dollars), and (iii) the

frequency of transfers is monthly. Around 98% of the cash recipients in our sample are women satisfying the mentioned conditions.

We also check that surveyed households who are receiving monetary transfers from *Juntos* satisfy the eligibility conditions. Despite the fact that eligible households should be below the poverty line in order to receive the transfer, our sample suggests that around 19% of the households are above the poverty line defined by INEI. We exclude non-poor households from our empirical analysis below and discuss in Section 5.3 how including these households can affect our results[11].

Information on pay dates comes from administrative records provided by *Juntos* managers[12]. As mentioned in Section 4.1, payment schedules are reported at the village level. Unique municipality identifiers are used to match the information of payment dates from the administrative dataset, previously collapsed at the municipality level, to the beneficiaries sample built up from the ENAHO 2009. Our final sample contains information of 1,995 individuals living in 1,087 households enrolled in *Juntos*. Of these individuals, 1,615 live in poor conditions, whereas 380 are not poor according to the standard per-capita daily expenditure measure. Variable averages and standard errors (reported in parentheses) are shown in Table 2. Each column reports summary statistics of all individuals included in each of our four dummy variables defining temporal distance between pay and interview dates.

4.3 Outcome variables

We focus on three different measures of labor supply: participation (extensive margin) weekly hours worked (intensive margin) and working for paid activities. As described above, each of the outcome variables are measured for the week before (reference week) the interview (which usually takes place on Sundays).

Labor participation is a dummy variable which is equal to one when the individual reported having worked or searching for a job any time during the reference week. To measure labor intensity, we take the total number of hours worked during the same reference week. Lastly, the indicator for working for paid activities is relevant for evidencing changes in labor supply alternative margins once the payment has already been done or is about to occur (for instance, household members could reallocate time to family or home production related unpaid activities once the cash has been transferred). The last two outcome variables are defined only for those who reported having been employed during the reference week.

Given that we have information of the number of hours worked on each day of the reference week, we are also able to test whether individuals change their labor supply behavior in a given day or whether they balance their labor intensity throughout the whole week. This insight will be helpful when interpreting our main results.

5 Results

5.1 Main results

Table 3 reports the results for the equation of labor force participation. Each row indicates the distance between the cash transfer receipt and the reference week. Columns (3), (6), and (9) are our preferred specifications since they control for municipality fixed effects as well as individual covariates (sex, age, educational level, marital status, indicators for mother tongue, indicators for poverty status, an indicator for rural residence, and

Table 2 Descriptive Statistics (according to temporal distance between pay and reference week)

Variable	Temporal distance between pay and reference week			
	At least two weeks before	One week before	During the reference week	At least one week after
Age	43.53	41.70	42.53	42.27
	(12.16)	(10.60)	(12.29)	(11.68)
Male	0.47	0.48	0.46	0.46
	(0.50)	(0.50)	(0.50)	(0.50)
Education level: No education	0.19	0.18	0.17	0.19
	(0.39)	(0.39)	(0.38)	(0.39)
Education level: Primary	0.60	0.67	0.64	0.63
	(0.49)	(0.47)	(0.48)	(0.48)
Education level: Secondary	0.21	0.14	0.18	0.17
	(0.40)	(0.34)	(0.38)	(0.37)
Education level: Tertiary	0.01	0.01	0.01	0.01
	(0.11)	(0.12)	(0.11)	(0.11)
Mother tongue: Spanish	0.30	0.28	0.33	0.27
	(0.46)	(0.45)	(0.47)	(0.44)
Mother tongue: Quechua	0.61	0.71	0.62	0.68
	(0.49)	(0.46)	(0.49)	(0.46)
Mother tongue: Other	0.09	0.01	0.05	0.05
	(0.28)	(0.09)	(0.22)	(0.22)
Poverty condition: Extremely poor	0.40	0.47	0.43	0.47
	(0.49)	(0.50)	(0.50)	(0.50)
Poverty condition: Poor	0.37	0.39	0.36	0.35
	(0.48)	(0.49)	(0.48)	(0.48)
Poverty condition: Non poor	0.22	0.14	0.21	0.18
	(0.42)	(0.35)	(0.41)	(0.39)
Lives in rural area	0.90	0.77	0.83	0.86
	(0.30)	(0.42)	(0.37)	(0.35)
In labor force	0.95	0.93	0.93	0.95
	(0.21)	(0.25)	(0.26)	(0.22)
Weekly hours worked	30.66	29.25	30.19	31.77
	(15.68)	(15.91)	(17.97)	(16.22)
Worked for paid activities	0.55	0.58	0.53	0.55
	(0.50)	(0.49)	(0.50)	(0.50)
Observations	563	425	442	565

interactions between indicators of distance between the payment date and the reference week and the dummy variable determining whether or not the individual is the household head). Results from these columns suggest that there are no effects of the cash transfer receipt on labor force participation, even when splitting the sample by recipients and recipients' partners.

Table 4 shows results for the equation of hours worked in the reference week. For the sample as a whole, there are no significant effects on the intensive margin. However, we find that having received the cash transfer within the seven days before the reference week reduces about 5.7 hours of work in the reference week for recipients only - see column (6). Recall that the effect of the transfer among recipients may be driven by two possible confounding factors: time spent in transportation from the

Table 3 Effects of temporal distance between pay and reference week on labor force participation

	All individuals			Recipients			Recipients' partners		
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
One week before the reference week	-0.028*	-0.009	-0.009	-0.072**	-0.044	-0.044	0.015*	0.006	0.148
	(0.016)	(0.030)	(0.030)	(0.029)	(0.060)	(0.060)	(0.009)	(0.007)	(0.203)
During the reference week	-0.027	-0.011	-0.011	-0.054*	-0.046	-0.046	0.002	0.019	0.162
	(0.017)	(0.032)	(0.032)	(0.029)	(0.059)	(0.059)	(0.012)	(0.016)	(0.206)
At least one week after the reference week	-0.011	-0.007	-0.007	-0.029	-0.024	-0.024	0.010	-0.001	-0.319
	(0.014)	(0.024)	(0.024)	(0.024)	(0.047)	(0.047)	(0.010)	(0.009)	(0.336)
Municipality fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,615	1,615	1,615	859	859	859	756	756	756
R-squared	0.003	0.144	0.144	0.008	0.284	0.284	0.005	0.280	0.280

Notes: Robust standard errors in parentheses. Additional controls include: an indicator for sex, an indicator for marital status (married or living together), age, indicators for educational level (primary, secondary, tertiary), an indicator for Spanish mother tongue, an indicator for poverty status, an indicator for living in rural areas, and interactions terms between an indicator for household head and temporal distance between pay and reference week.
***p < 0.01, **p < 0.05, *p < 0.1.

location of residence to the bank and time spent in purchasing the goods or consuming the money once it has been withdrawn from the bank. If those who were paid during the reference week have also anticipated the transfer date (and, therefore, have reduced their working hours) and have spent some time in receiving the transfer, then the resulting point estimate for those who were paid one week before

Table 4 Effects of temporal distance between pay and reference week on weekly hours of work

	All individuals			Recipients			Recipients' partners		
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
One week before the reference week	-0.558	-1.899	-1.756	-1.640	-4.192*	-5. 618**	0.241	-0.425	-0.726
	(1.131)	(1.840)	(1.832)	(1.398)	(2.284)	(2.397)	(1.789)	(2.903)	(2.916)
During the reference week	-0.294	-1.234	-0.972	-0.790	-0.594	-1.836	0.196	-1.537	-1.155
	(1.164)	(1.849)	(1.835)	(1.414)	(2.213)	(2. 367)	(1.872)	(3.017)	(3.012)
At least one week after the reference week	1.308	0.678	0.996	0.992	3.802	2.573	1.614	-3.312	-2.332
	(1.073)	(1.825)	(1.806)	(1.300)	(2.206)	(2.295)	(1.729)	(2.918)	(2.919)
Municipality fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Additional controls	No	NoZ	Yes	No	No	Yes	No	No	Yes
Observations	1,577	1,577	1,577	827	827	827	750	750	750
R-squared	0.002	0.259	0.259	0.005	0.395	0.395	0.001	0.419	0.436

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

the reference week is not driven by these particular confounding effects. Nonetheless, time spent in purchasing goods with the received money could also be affecting our estimates[13].

Table 5 reports the results for the equation of "working for paid activities". Results do not show a clear pattern regarding the effects of the distance between pay and interview dates on working in a paid-job, even for different household members. Moreover, these effects are statistically insignificant. One possible interpretation for the lack of effects of the cash transfer receipt on the probability of working for paid activities is that there may exist some rigidities in switching from unpaid to paid jobs in the very short run.

Given the results, all the remaining analysis is based on the short run effects of cash transfers on hours of work. In the following lines we try to disentangle the potential *income* effect generated by the welfare transfer from the aforementioned confounding effects. To do so, we begin by exploring whether the reduction in working hours brought up by the welfare transfer is evenly distributed along the whole week or if it is concentrated in a particular day or days of the reference week. Under the hypothesis that the reduction in hours of work is being driven by time spent in purchasing goods (once we control for the potential anticipation and transportation effects), one should expect that the effect of the transfer is grouped in a particular day of the week (say, the day which is closer to the payment date).

In Table 6 we report the resulting coefficients for every day of the reference week. Consistent with the estimates shown in Table 4, we find negative and significant effects for those who are paid within the seven days before the reference week. Specifically, we find that working hours are reduced by roughly 1.3 hours in every day except for Sundays. In addition, we find that hours of work on Thursday are reduced by 1 hour if payment occurs during the reference week.

Overall, results show a decrease in hours worked when payment occurs in the reference week. This reduction is most likely to be driven by time spent on going to the bank.

Table 5 Effects of temporal distance between pay and reference week on working for paid activities

	All individuals			Recipients			Recipients' partners		
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
One week before the reference week	0.027	0.070	0.070	-0.019	-0.015	-0.015	0.004	0.028	0.027
	(0.035)	(0.065)	(0.065)	(0.037)	(0.062)	(0.062)	(0.016)	(0.024)	(0.024)
During the reference week	-0.020	-0.062	-0.062	-0.030	-0.086	-0.086	-0.026	-0.003	-0.005
	(0.037)	(0.065)	(0.065)	(0.037)	(0.064)	(0.064)	(0.021)	(0.026)	(0.026)
At least one week after the reference week	0.005	-0.029	-0.029	0.001	-0.081	-0.081	-0.003	0.001	0.010
	(0.034)	(0.065)	(0.065)	(0.035)	(0.065)	(0.065)	(0.016)	(0.019)	(0.021)
Municipality fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,577	1,577	1,577	827	827	827	750	750	750
R-squared	0.001	0.047	0.047	0.001	0.358	0.358	0.004	0.402	0.436

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

Table 6 Effects of temporal distance between pay and reference week on daily hours of work (recipients only)

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
One week before the	0.828*	-0.732	-0.810*	-1.269**	-1.319***	-1.334***	-0.982*
reference week	(0.452)	(0.494)	(0.491)	(0.498)	(0.497)	(0.514)	(0.535)
During the reference	1.231**	-0.183	-0.760	-0.783	-0.994*	-0.644	0.297
week	(0.500)	(0.531)	(0.527)	(0.520)	(0.534)	(0.548)	(0.552)
At least one week after	1.341***	0.342	0.225	0.019	-0.084	0.074	0.656
the reference week	(0.487)	(0.453)	(0.458)	(0.470)	(0.493)	(0.459)	(0.533)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	827	827	827	827	827	827	827
R-squared	0.398	0.366	0.395	0.373	0.366	0.379	0.345

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

However, when payment takes place one week before the reference week, the reduction in labor intensity is evenly distributed along the reference week which is inconsistent with the hypothesis that our results are mainly driven by transportation from the household to the bank. We interpret these results as if the dummy variable representing that the payment was made "one week before the reference week" reflects the immediate disincentives to work generated by the having received the cash transfer.

5.2 Heterogeneous effects

We next explore whether the decline in hours worked during the reference week is homogeneous across all recipients or if they differ according to their observable characteristics. First, in Table 7 we divide the sample of recipients according to their marital status (married and non-married). Interestingly, we find that the reduction in working hours of recipients when the transfer was observed to happen one week before the reference week is driven by the sub-sample of married women. The point estimate for married women is -11.3 and is statistically significant at the 1% level, whereas the coefficient of the sub-sample of non-married women is not statistically significant. One plausible explanation for this difference is that married women also rely on their husbands' income and this allow them to reduce their working intensity more than non-married women.

Second, we analyze if there exists heterogeneity between old and young recipients. In order to keep a balanced sample in both groups, we say that a recipient is young if she is 40 years old or younger and she is old, otherwise. Table 8 presents results from this specification. Results suggest that younger women (i.e., recipients aged 40 or less) reduce more their working hours relative to older women. In fact, working behavior of older women seems to be unaffected by the cash transfer receipt, since all the coefficients are statistically insignificant. On the other hand, younger women reduce working intensity by around 12 hours when the payment occurs one week before the reference week.

Third, we distinguish between recipients who have children aged 5 or less and those who do not. This distinction is important because the presence of young children at home is a major determinant in female labor supply. Results from splitting the sample according to children's age are presented in Table 9. As expected, recipients with children aged 5 or less reduce their labor supply more than recipients with older children. The point estimate

Table 7 Effects of temporal distance between pay and reference week on weekly hours of work by marital status (recipients only)

	Married		Non-married	
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**
One week before the reference week	-2.318	-11.293***	-2.825	-2.620
	(1.875)	(3.667)	(2.068)	(4.647)
During the reference week	-2.165	-6.788	0.088	3.899
	(2.058)	(4.599)	(2.325)	(4.557)
At least one week after the reference week	-1.310	-1.808	3.246	5.252
	(1.745)	(3.823)	(2.023)	(4.344)
Municipality fixed effects	No	Yes	No	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	446	446	381	381
R-squared	0.072	0.540	0.063	0.540

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

of having received the cash transfer one week before the reference week is -9.96 hours for recipients with young children. This could suggest that recipients reduce their hours of work in order to spend this additional time in activities related to child rearing.

5.3 Robustness analysis

5.3.1 Changes in the sample and different specifications

Though distance between program's pay dates and ENAHO's interview dates is presumably exogenous, the estimates in the previous section may be capturing some confounding effects. First, the way we construct the temporal distance when observing more than one pay date in a given municipality could introduce measurement bias, attenuating our estimated effects. Second, there still some additional effects which have not been discarded in previous estimates, downwardly biasing our results. In particular, the way in which time spent in going to withdraw the money can affect the results has not been discussed, and can erroneously be interpreted as a disincentive effect to work. Third, results can be sensitive to the inclusion of non-poor beneficiaries in the sample. Finally, our measures

Table 8 Effects of temporal distance between pay and reference week on weekly hours of work by age group (recipients only)

	Young (under 40)		Old	
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**
One week before the reference week	-4.224**	-11.985***	-0.502	-2.088
	(2.035)	(4.350)	(1.919)	(4.887)
During the reference week	-3.163	-8.396*	0.841	4.143
	(2.211)	(4.671)	(2.186)	(4.883)
At least one week after the reference week	-0.897	-2.033	1.881	7.796
	(1.845)	(3.750)	(1.893)	(4.814)
Municipality fixed effects	No	Yes	No	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	411	411	416	416
R-squared	0.061	0.544	0.058	0.553

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

Table 9 Effects of temporal distance between pay and reference week on weekly hours of work by children's age (recipients only)

	With children aged 5 or less		With children aged 6 or more	
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**
One week before the reference week	-4.346**	-9.962**	-0.637	-6.152
	(1.822)	(3.878)	(2.209)	(4.867)
During the reference week	-0.336	-3.830	-2.747	-2.149
	(1.903)	(4.900)	(2.664)	(5.689)
At least one week after the reference week	0.686	1.870	-0.624	4.646
	(1.618)	(3.548)	(2.256)	(5.791)
Municipality fixed effects	No	Yes	No	Yes
Additional controls	Yes	Yes	Yes	Yes
Observations	447	447	354	354
R-squared	0.080	0.511	0.042	0.563

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

of distance between pay and interview dates can be capturing effects other than *income* effects, affecting the interpretation of our results.

We begin our robustness checks by exploring whether the inclusion of non-poor families in our sample could yield different results. To the extent that poor beneficiaries can live in more remote areas and have less access to transportation, results shown in the previous section can represent lower-bound estimates. In Table 10 we report the results for weekly hours of work after including non-poor beneficiaries in our sample. Results correspond to recipients only. All the estimated coefficients are negative and statistically significant at the 1% level. In our most preferred specification (i.e., including municipality fixed effects and controlling for individual characteristics), we find that having been paid one week before the reference week reduces working intensity by around 6 hours.

The estimated effects are also larger than those presented in Table 4, where we exclude non-poor families. This additional evidence suggests that the decline in hours of work is not driven by the time needed to withdraw the money, since non-poor beneficiaries are more likely to spend *less* time going from home to the bank. Moreover, this difference

Table 10 Effects of temporal distance between pay and reference week on weekly hours of work, including non-poor beneficiaries (recipients only)

Transfer occurred:	**(1)**	**(2)**	**(3)**
One week before the reference week	-2.632	-4.881**	-5.998**
	(1.329)	(2.466)	(2.408)
During the reference week	-1.564	-0.685	-1.948
	(1.319)	(2.583)	(2.597)
At least one week after the reference week	1.024	3.071	2.251
	(1.225)	(2.251)	(2.275)
Municipality fixed effects	No	Yes	Yes
Additional controls	No	No	Yes
Observations	1,015	1,015	1,015
R-squared	0.009	0.340	0.354

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

may suggest that non-poor beneficiaries' labor supply is more elastic (with respect to cash transfers) than that of poor beneficiaries.

As an attempt to dissipate concerns related to measurement errors, we next construct a different measure of distance between pay and interview dates. Recall that our measure of "recentness" of the cash transfer is defined as the distance in weeks between the pay date and households' interview dates within a given municipality. Nevertheless, a municipality could have more than one pay date, since *Juntos* payment schedule is defined at the village level. Up until now, we have used the earliest date to construct our measures of distance between pay dates and the reference week.

In Table 11 we present results when constructing the indicators of distance using the last date of payment within the municipality. We only include recipients in the regressions. In columns (1) to (3) we exclude non-poor recipients while in columns (4) to (6) we include them in the sample. When controlling for recipients characteristics, results suggest that poor beneficiaries reduce their weekly hours of work by 4.85 (statistically significant at the 5% level) when the cash transfer occurred one week before the reference week. This reduction in weekly hours of work is larger (-5.97 hours) when including non-poor households in the sample (column 6) and is statistically significant at the 5% level. Coefficients reported in columns (3) and (6) of Table 11 are similar to those reported in column (6) of Table 4 and column (3) of Table 10, respectively, where we use the same samples and the first date of payment within the municipality to define temporal distance. The evidence presented on this table suggests that the impact of having received the cash transfer one week before the reference week on hours of work does not significantly change when we modify the definition of the municipality-payment date.

A major threat to our identification strategy is that the dummy variables denoting the distance between payment and interview dates may be capturing other factors not related to the cash transfer, but to the specific date of the payment. For instance, it could be the case that payment dates are established on days when the labor supply is low for a different reason than the transfer (e.g., holidays). This potential correlation between dates and unobservable variables that affect hours of work would invalidate our strategy. To check that this is not the case, we perform a falsification test using available data from non-beneficiaries. If the dummy variables representing the distance between payment

Table 11 Effects of temporal distance between pay and reference week on weekly hours of work using the last pay date within municipalities (recipients only)

	Poors			Poors and non-poors		
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
One week before the reference week	-1.229	-3.781	-4.847**	-2.118	-5.194*	-5.970**
	(1.646)	(3.171)	(2.461)	(1.570)	(2.983)	(2.905)
During the reference week	-1.049	1.595	0.817	-1.503	0.803	-0.094
	(1.612)	(3.344)	(3.437)	(1.506)	(2.957)	(3.016)
At least one week after the reference week	-0.951	2.238	1.550	-0.450	1.538	1.044
	(1.346)	(2.718)	(2.187)	(1.287)	(2.536)	(2.520)
Municipality fixed effects	No	Yes	Yes	No	Yes	Yes
Additional controls	No	No	Yes	No	No	Yes
Observations	827	827	827	1,015	1,015	1,015
R-squared	0.001	0.392	0.432	0.003	0.339	0.384

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

and interview dates were correlated with other variables that affect labor supply, they should also have an impact on the hours of work of non-beneficiaries.

We perform regressions for weekly hours of work including in the sample spouses of household heads who are not beneficiaries from *Juntos* but who live in treated municipalities. Table 12 presents the results from these regressions. Results show that none of the dummies measuring the distance between pay and interview dates are significant at any conventional level. We interpret these results as if our indicators of distance are not correlated with omitted variables that may affect labor supply by alternative channels.

Finally, we perform an additional specification that allows us to rule out the hypothesis that our results may be driven by the time spent picking up the money. In particular, we perform two separate regressions according to the program's payment method in order to rule out this possibility. Recall that there exist two payment methods: deposits to bank accounts and delivering cash in armored van. The former implies that beneficiaries go to the bank and withdraw the money through ATM, whereas armored vans distribute the money to beneficiaries in the main square of their village. This second payment method was introduced to reach the most remote places of the Peruvian territory and does not require beneficiaries to move long distances in order to pick up the money. Thus, the effect of temporal distance between interview and payment dates on labor supply for beneficiaries who received the cash transfer via armored vans does not contain confounding factors such as time spent in withdrawing the money.

Table 13 reports the results for weekly hours of work divided according to the payment mechanism. Columns (1) and (2) correspond to the bank account mechanism, and columns (3) and (4) correspond to payment through armored vans. All the regressions are performed for recipients only. Coefficients for "bank account" are not statistically significant at any conventional level. In contrast, coefficients for "armored van" are statistically significant at the 1% level when including the full set of controls. In particular, work intensity of individuals for whom the cash transfer was made by armored vans is reduced by 8.8 hours when the transfer occurred 1 week before the reference week. We interpret this result as if the reduction in hours of work is purely due to an income effect and is not related to the time spent going to the bank.

Table 12 Effects of temporal distance between pay and reference week on weekly hours of work (non-beneficiary housewives)

Transfer occurred:	**(1)**	**(2)**	**(3)**
One week before the reference week	-0.054	-0.799	-0.210
	(1.542)	(3.021)	(3.024)
During the reference week	-1.603	-2.155	-1.997
	(1.557)	(2.831)	-(2.793)
At least one week after the reference week	3.352	2.211	2.099
	(1.663)	(2.848)	(2.790)
Municipality fixed effects	No	Yes	Yes
Additional controls	No	No	Yes
Observations	927	927	927
R-squared	0.010	0.333	0.348

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

Table 13 Effects of temporal distance between pay and reference week on weekly hours of work by payment mechanism (recipients only)

	Bank		Armored van	
Transfer occurred:	**(1)**	**(2)**	**(3)**	**(4)**
One week before the reference week	-3.195*	-0.913	-2.335	-8.807**
	(1.839)	(4.353)	(2.087)	(3.056)
During the reference week	-0.670	7.421	-2.835	-6.207
	(2.275)	(4.529)	(2.119)	(3.864)
At least one week after the reference week	0.201	11.202*	-0.418	-1.006
	(2.082)	(6.448)	(1.763)	(2.667)
Municipality fixed effects	No	Yes	No	Yes
Additional controls	No	Yes	Yes	Yes
Observations	369	369	458	458
R-squared	0.086	0.478	0.060	0.400

Notes: Robust standard errors in parentheses. See additional notes on Table 3.
***p < 0.01, **p < 0.05, *p < 0.1.

5.3.2 An alternative measure of temporal distance

Additional concerns about the econometric approach can be related to the way temporal distance is defined. In this section, we construct an alternative measure of temporal distance based on the exact date of payment and interview dates. More specifically, since we observe hours of work for every day of the reference week and the municipality payment date, we can calculate the exact number of days between the pay day and each of the days in the reference week. Moreover, since we have exactly 7 different observations for each individual (associated to the seven days of the reference week), we can perform fixed effect regressions at the individual level using temporal distance in days as the independent variable.

Panel A of Table 14 reports the results from the linear specification. Columns with even numbers include only poor recipients, whereas columns with odd numbers allow for the inclusion of non-poor recipients in the sample. The first two columns use the earliest pay date and the last two columns use the last payment date within a given municipality to define the number of days between the cash transfer and the "reference day" (the day for which hours of work are reported). That is, each individual's hours of work are measured in 7 different reference days. The point estimate for poor recipients is 0.127 (statistically significant at the 1% level), suggesting that hours of work go up by 0.127 hours per day when the cash transfer occurs after the reference day. When including non-poor beneficiaries in the sample, the point estimate rises to 0.131, and is also statistically significant at the 1% level. A similar pattern is observed when using the last payment date within the municipality.

In Panel B of Table 14 we present the estimated coefficients associated to dummy variables indicating the number of days between the pay day and the reference day. For the sake of simplicity, we construct 10 such variables, each representing the number of days between the cash transfer and the reference day. In particular, we include in the regressions indicators for cash transfer occurring 1 day, 2 days, 3 or 4 days, 5 or 6 days, and 7 or more days after (or before) the reference day. The omitted category is when the pay day and the day of report are the same (i.e., temporal distance between pay and the reference day is equal to zero). Results show that working hours are reduced when the cash transfer occurs before the day in which individuals report their working behavior. The largest

Table 14 Effect of temporal distance between pay and reference day on daily hours of work (recipients only)

	Payment date within municipalities			
	Earliest pay date		Latest pay date	
	(1)	(2)	(3)	(4)
Panel A				
Coefficient on linear specification	0.127***	0.131***	0.119***	0.123***
	(0.014)	(0.013)	(0.025)	(0.039)
	[0.523]	[0.546]	[0.489]	[0.557]
Panel B				
Transfer occurred (with respect to the reference day):				
At least 7 days before	0.157***	0.109***	0.123**	0.173***
	(0.044)	(0.034)	(0.041)	(0.053)
5or 6 days before	-0.288**	-0.268*	-0.230*	-0.196*
	(0.116)	(0.156)	(0.101)	(0.091)
3 or 4 days before	-0.434**	-0.489**	-0.339	-0.432**
	(0.228)	(0.204)	(0.243)	(0.213)
2 days before	-0.251	-0.266	-0.252	-0.209
	(0.237)	(0.212)	(0.212)	(0.221)
1 day before	0.053	-0.067	-0.039	0.051
	(0.234)	(0.209)	(0.249)	(0.219)
1 day after	0.180	0.174	0.178	0.263
	(0.244)	(0.219)	(0.256)	(0.227)
2 days after	0.343	0.313	0.442*	0.448**
	(0.247)	(0.223)	(0.257)	(0.230)
3 or 4 days after	0.243	0.203	0.326	0.380*
	(0.231)	(0.209)	(0.242)	(0.217)
5 or 6 days after	0.829***	0.818***	0.962***	1.023***
	(0.263)	(0.241)	(0.278)	(0.252)
At least 7 days after	1.132***	1.203***	1.267***	1.134***
	(0.276)	(0.253)	(0.294)	(0.277)
	[0.520]	[0.610]	[0.519]	[0.537]
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	5,789	7,105	5,789	7,105
Number of individuals	827	1,015	827	1,015

Notes: Robust standard errors in parentheses. R-squared in brackets.
***p < 0.01, **p < 0.05, *p < 0.1.

decline in hours of work occurs when the payment was observed to happen 3 or 4 days before the day of report, and becomes negligible when the pay day gets closer to the day of report of working behavior.

In Figure 2 we plot the estimated coefficients along with their 95% confidence intervals according to the distance (in days) between the pay day and the reference day. We use day 0 (i.e., cash transfer occurred the reference day) as the benchmark dummy variable. The figure clearly describes a U-shaped pattern, implying that working intensity is reduced when the cash transfer was made within 5 days before the reference day and remains at its *normal* level when cash transfers occur after the reference day.

6 Discussion

Along the document, we have argued that, although CCTs may not have long-term or permanent effects on labor supply of adults, it may be the case that individuals reduce

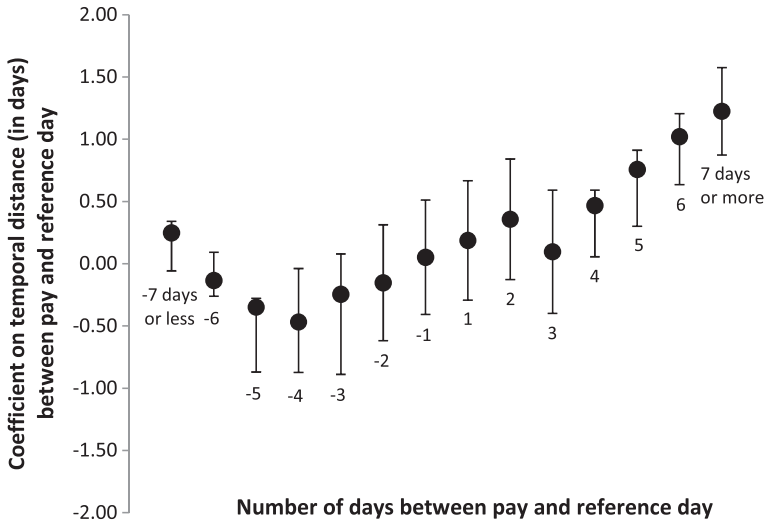

Figure 2 Coefficients on temporal distance (in days) between pay and reference day. *Notes*: Each point in the graph corresponds to a coefficient associated to a dummy variable measuring the temporal distance between pay and reference day (day in which the hours of work are reported). The omitted category is "distance between pay and reference day equals 0", which implies that the pay and the reference day are the same.

their working intensity in the short-term as a response to welfare transfers. Our empirical analysis is based on the plausible assumption that payment schedules of *Juntos* are orthogonal to interview dates of ENAHO survey, and, using this identifying assumption, we are able to construct measures of temporal distance to test whether the recentness of cash transfers affects benefit recipients' labor supply. In particular, we find that working hours of recipients are reduced when the cash transfer was observed to happen within one week before the interview took place. In this section we discuss some possible paths through which it is possible to observe short- but not long-term effects of welfare transfers on labor supply of adults.

Results shown in Section 5.3 suggest that declines in working intensity are mostly driven by married women in their childrearing ages. As discussed by Moffitt (1992), there is a voluminous empirical literature supporting the idea that divorced or separated women have a more inelastic labor supply relative to women living in husband-wife (or maritally stable) families. There exist several explanations for these observed differences. The absence of a male with income to help support the children is the most common explanation. Two-parent families not only have the capability of generating more income since both parents can work for paid activities, but can also allocate more efficiently the time devoted to parenting practices. In contrast, child bearing and work in female-headed houses are both mutually exclusive activities. Therefore, the *income* effect generated by the welfare transfer is more likely to affect women living in husband-wife families.

Time not devoted to work can be re-allocated to parenting activities or, more broadly, home production. However, this implies that activities in which beneficiaries are involved might be such that they can *freely* switch between home production and paid job at any point of time (highly flexible activities). In the specific case of the Peruvian *Juntos* program, del Pozo and Guzmán (2011) find that the welfare transfers increase the likelihood of owning small animals, such as poultry and guinea pigs, as well as the use of land for

growing natural grasses (which are used to feed guinea pigs). Moreover, authors find that income generated by selling small animals as a fraction of household total income is greater for program beneficiaries relative to non-treated households. Since raising small animals has become a profitable business that might be carried out as part of household activities, it is likely that program beneficiaries - in particular housewives - have turned to this activity as a compliment of their home-related duties, and which allow them to earn labor income while taking care of their children.

Aside from the working incentives welfare transfers may generate and the way in which time is allocated across home production and work effort, it is important to emphasize that results shown in the empirical analysis correspond to short-term responses of working behavior to welfare transfers and might not be taken as program's average treatment effects. As discussed in the literature review, most of the studies evaluating the effect of CCTs on labor supply (in Latin America) do not find significant changes in working behavior of adults. However, "disincentive effects on adult labor supply are found only for the program that made the most generous transfers, the *RPS* in Nicaragua" (Fiszbein and Schady 2009). This piece of evidence suggests that *income* effects generated by welfare transfers may depend on the amount of the transfer relative to individual's earned income[14].

The observed decrease in working effort of recipients in the short run can be explained from consumption patterns over time. If the welfare transfer allows households to achieve a higher consumption bundle over a certain period of time (say, during periods in which the cash transfer has recently been made) but not permanently, it is likely that the *income* effect would be visible only in the short run. However, when the cash transfer has been totally spent, labor supply must increase in order to afford the initial (i.e., out-of-welfare) consumption bundle. Put it differently, welfare transfers can "buy" leisure as long as a certain level of consumption has been achieved. Below this threshold, labor supply increases and returns to its initial level. Thus, if the cash transfer is sufficiently large to afford higher levels of consumption during a long period of time, then this would generate a permanent decrease of labor supply of individuals. This insight can explain why CCTs giving more generous stipends are observed to have disincentive effects on labor supply of adult beneficiaries.

7 Conclusion

It is well-known that welfare programs in developed countries may have unintended effects on labor supply (Ashenfelter 1983; Moffitt 2002). Conditional Cash Transfer programs are not the exception. However, empirical evidence from Latin American countries does not seem to be consistent with the *income* effect predicted by the standard model of labor supply. Evidence drawn from experimental and quasi-experimental evaluation methods is associated with the long term effects of program participation on adult labor supply. Although these empirical approaches allow researchers to accurately identify average treatment effects of welfare programs on different set of outcomes, it does not come without its drawbacks. In the specific case of adult labor supply, particular concerns should be given to general equilibrium effects potentially introduced by program's benefits, and indirect effects generated by the reduction in child labor.

In this paper we adopt a novel empirical approach which allow us to estimate short term effects of cash transfers on adults' working effort. In particular, we exploit within

municipality exogenous variation introduced by the temporal distance between payment schedules of the Peruvian *Juntos* program and interview dates of the Peruvian National Household Survey to explore whether the monthly receipt of cash transfers affect working behavior. Despite the fact that we cannot estimate average treatment effects, this empirical approach is useful for isolating immediate effects on labor supply generated by program stipends from other potentially confounding factors.

We find that women's (but not men's) weekly hours of work are reduced by almost 6 hours in the week following the payment date. This effect is mainly driven by married (maritally stable) women and by mothers with children aged 5 or less. However, no effects are found for labor force participation, nor for working for paid activities. These results are robust to changes in the sample, different specifications, and alternative measures of temporal distance.

Some interesting implications arise from our findings. From an academic perspective, we reconcile empirical evidence with the theoretical predictions of the standard model of labor supply. In this regard, although *income* effects are not observed in previous studies analyzing the impact of CCTs on adult labor supply in the sense of working disincentives, we do find that, in the short run, these effects are likely to appear. The absence of long term effects of cash transfers on working effort can be attributed to transfer's generosity and consumption patterns over time. The latter implies the way households maximize consumption in days near pay dates relative to more distant days.

For evaluation purposes, special attention should be paid to general equilibrium effects and changes in the labor market composition potentially introduced by CCTs. With respect to the former, it is possible that, within targeted communities, reductions in labor force participation generated by the introduction of welfare transfers can increase wages, thereby mitigating the predicted *income* effects. In fact, Alzúa et al. (2013) find that household earned income increase as a result of increases in wages among male beneficiaries when analyzing the Mexican *PROGRESA* program. Regarding labor market composition, there is some evidence on changes in working activities. For instance, del Pozo and Guzmán (2011) find that women are more likely to work closer to home. Likewise, Adato and Roopnaraine (2004) find that adult males work longer in their own parcels relative to their counterparts when assessing the Nicaraguan *RPS*.

From a policy perspective, different issues arise. First, changing the amount and frequency of the payments could, as suggested by Fiszbein and Schady (2009), amplify the immediate labor supply response to cash transfers. Second, as an attempt to minimize the decrease in hours of work, pay dates could be defined on days when labor supply is expected to be low (e.g., weekends). Third, policy makers should take into account the trade-off they face in terms of payment methods: bank deposits can be operationally cheaper but they impose larger transportation costs (not only money but also time) than delivering cash in armored vans.

Finally, we believe that our empirical approach could be useful to answer other related outcomes that might respond to the receipt of the cash transfer. For instance, we could examine if households change their regular consumption pattern during the week after the payment (e.g., going to restaurants instead of eating at home). Also, it would be relevant to check whether cash recipients do not lose control over the money once they arrive home. If potential disputes within the household arise after the payment, we could test whether there is an increase in domestic violence during these

days. It would also be relevant to examine whether children's school attendance (or education-related expenses) increase after the pay date. These are promising avenues for future research that may expand the discussion about the benefits and limitations of CCTs.

Endnotes

[1]In general, welfare programs can be divided according to the type of benefits: cash and in-kind transfers. Ashenfelter (1983), Moffitt (1992), and Moffitt (2002) provide outstanding reviews of cash transfer programs. For a review of the relationship between in-kind benefits and labor supply, see Currie (1994), Yelowitz (1995), Blundell and MaCurdy (1999), Moffitt (2002), and Hoynes and Schanzenbach (2009).

[2]Other studies suggest that individuals are likely to invest in agricultural related productive assets. In a recent article, Duflo et al. (2011) document that demand for agricultural tools tend to increase on days close to pay dates

[3]In order to qualify, children must be 18 years old or under (with few exceptions accepting families with children "permanently and totally disabled" aged 19 or above), must be somehow related to the claimant (blood, marriage or law), and must be resident of the United States.

[4]Other See Eissa and Hoynes (2006) and references therein.

[5]This is not the case for the Chilean *Chile Solidario* program, since one of the conditions for eligibility is that unemployed household members should be enrolled in local employment offices.

[6]See Moffitt (2002) for a further examination of this particular scenario.

[7]There does not exist an extensive literature evaluating *Juntos* impacts on program beneficiaries. Most of the studies evaluating *Juntos* are based on qualitative analysis of program's impacts (Escobal and Benites 2012, Alcázar 2010). Until this date, Perova and Vakis (2012), Sánchez and Jaramillo (2012), and del Pozo and Guzmán (2011) constitute the only quantitative studies of this program. The latter two studies evaluate program's impacts on nutritional status of infants and household's productive investments and agricultural production, respectively.

[8]See the Appendix for further detail on variables definition.

[9]It is worth noting, however, that this would be true only in villages where the payment method is through bank deposits, but not in villages where beneficiaries go to the main square on the pay day and wait for the armored van. We use this feature of the program to perform additional sensitivity tests in Section 5.3.

[10]Cash transfers were usually made available during the third week of the month, and interviews in a given district usually began during the first days of the calendar month and lasted almost 30 days. Based on this information, those who were interviewed at the end of the month, are more likely to be captured in the dummy variables encompassing individuals who received the transfer "during the reference week" or "one week after the reference week".

[11]The reason underlying the filters of non poor households as part of the Juntos beneficiaries can be explained based on poverty transitions (households being initially poor and then escaping from poverty once they had already been selected as beneficiaries) and program administrative failures (non poor households selected as beneficiaries even when the program was initially targeted to households below the poverty line).

[12]This information is also available at: http://www.juntos.gob.pe/cronograma_transportadora.php.

[13]It is worth noting, however, that the reduction in working hours occurs in the reference week. This implies that, if there exists an effect encompassing time spent in consumption of goods, then it is likely that this effect should appear just after the transfer has been done, but not in the reference week (seven days after the payment date).

[14]One important consideration regarding the lack of evidence of negative effects of welfare transfers on labor supply is the timing in which program evaluations are made. As discussed by Fiszbein and Schady (2009), most of the existing studies use data collected shortly after the households have become eligibles to participate in the program for the first time. However, it may take some time before households adjust their working behavior to welfare benefits.

Appendix

Program's features

Eligibility Consists of three stages: (i) selection of eligible municipalities (based on poverty level, under age 5 chronic malnutrition, and exposure to violence during 1980s and 1990s); (ii) selection of eligible households (based on Logit model measuring the probability of a household living in poverty); (iii) community validation (community assemblies carried out by local authorities and government representatives)

Cash transfer 100 Nuevos Soles (US$ 37) per month

Conditions (i) children ages 6–14 attend at least 85% of yearly school classes; (ii) children ages 0–59 months old get fully immunized and attend periodically to growth controls; (iii) children ages 3–36 months get nutrition supplements; (iv) pregnant women visit health clinics for prenatal care; (v) nursing women visit health clinics for postnatal care; (vi) parents attend health clinics to receive information about nutrition, health and hygiene; (vii) parents without ID (identification) attend to program Mi nombre (*My name*).

Payment methods (i) bank deposit and (ii) cash delivery with armored van

Variables definition

Reference week Seven days before the interview day

Reference day The day of the reference week for which hours of work is reported

First pay date The first (earliest) pay date within a municipality

Last pay date The last (latest) pay date within a municipality

Outcomes (i) participation: a dummy variable which is equal to one if the individual had a job or looked for one in the reference week, and zero otherwise; (ii) hours of work: hours worked in the reference week (or in the reference day); (iii) working for paid activities: a dummy variable which is equal to one if the individual had a paid-job in the reference week, and zero otherwise

Variables of temporal distance between pay dates and the reference week (i) payment takes place, at least, two weeks before the reference week; (ii) payment occurs one week before the reference week; (iii) payment takes place during the reference week; (iv) payment occurs after the reference week. Omitted category: payment takes place, at least, two weeks before the reference week

Additional samples

Placebo sample Non-treated housewives (female heads who are not enrolled in the program) who live in municipalities covered by *Juntos*

Abbreviations
CCTs: Conditional Cash Transfer programs; Juntos: Conditional Cash Transfer program in Peru; ENAHO: Encuesta Nacional de Hogares (Peruvian National Household Survey); INEI: Instituto Nacional de Estadistica (Peruvian National Institute of Statistics).

Competing interests
The IZA Journal of Labor & Development is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles.

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