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Conditional Cash Transfers, Payment Dates and Labor Supply: Evidence from Peru

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Conditional Cash Transfers, Payment Dates and Labor Supply: Evidence from Peru[*]

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Abstract

We assess the effects of a Conditional Cash Transfer program on adult labor supply in Peru. The program, named Juntos, lacks an experimental design so we rely on a sort of "natural experiment". Instead of comparing treated and non-treated households, our strategy exploits within-municipality variation in the distance between payment dates of Juntos and interview dates of the Peruvian National Household Survey. We find that having received the cash transfer two weeks before the interview causes a reduction of 6 hours of work of recipients during the week prior to the survey. These effects are larger for married women and for mothers with children aged 5 or less. In addition, results are robust to different specifications and changes in the sample.

Keywords: Conditional Cash Transfers, Labor supply, Juntos, Peru
JEL Codes: I38, J22

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1 Introduction

Around the world, Conditional Cash Transfers (henceforth, CCTs) are considered powerful means to reduce current and future poverty. After the success of programs such as *Progresa* in Mexico and *Bolsa Escola* in Brazil, almost every country in Latin America has implemented its own program. There are many rigorous impact evaluations of CCTs but most of these studies have paid little attention to the effects of cash transfers on the labor supply of adults in treated households.

This paper analyzes the impact of a CCT program in Peru, known as *Juntos*, on adult labor supply. Juntos lacks an experimental design so we require a credible identification strategy. In particular, we exploit differences in the interview dates of the National Household Survey and the payment dates of the program within a given municipality. The timing of the interview combined with the payment schedule of Juntos generates a sort of "natural experiment" in which some households are interviewed just after the payment while others are surveyed weeks later or before. We find that if the cash transfer occurs two weeks before the interview, recipients' hours of work are reduced by 6 hours during the week prior to the survey. This reduction is rather large since it implies a fall of roughly 20% of weekly hours of work. However, we do not find significant effects on the labor supply of recipients' partners.

Most of the large literature related to CCTs focuses on impacts on education and health (see Fiszbein and Schady 2009 for an extensive review). Few studies examine the effects of CCTs on labor supply (Skoufias and Di Maro 2006, Maluccio 2008, Foguel and Paes de Barros 2010, Alzúa, Cruces and Ripani 2010). A common feature of these studies is that they exploit the experimental design of the programs to estimate the causal effect of cash transfers on labor supply. Thus, our investigation adds to the growing literature of CCTs because, as far as we know, this is the first paper that identifies the causal effect on labor supply of a CCT program which lacks an experimental design.

The rest of the paper proceeds as follows. We review the related literature in section 2. In section 3, we describe the characteristics of Juntos. We discuss our identification strategy in section 4. In section 5, we describe the data. Results are presented in section 6 and then we present robustness checks in section 7. Concluding remarks follow.

2 Literature Review

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 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 populations living in localities associated with program deployment. For instance, the effect of welfare programs such as Aid to Families with Dependent Children (AFDC), the Earned Income Tax Credit (EITC), and more recently the Food Stamp Program in the US along with the Working Families Tax Credit in the UK has intensively been evaluated (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 static and dynamic theoretical models of individual labor supply, (ii) program conditions, and (iii) models of household labor supply.

2.1 Theoretical Considerations

The potential effects of benefit transfers can be explained based on the basic static model of labor supply, which claims that individuals maximize between consumption and leisure (assuming that leisure is a normal good) facing a budget constraint, which is merely composed by labor (wage) and non labor (initial wealth and monetary or in-kind transfers) income.

In this study, for simplicity, we focus on the role CCTs can play in determining working incentives[1]. As pointed out by Alzúa, Cruces and Ripani (2010), CCTs have four potential channel 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 fall. Second, program conditions can also alter working schedule 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 to childcare.

However, school attendance of children might also reduce household's labor income if child labor is crucial in determining total family income. This constitutes the third channel through which adult labor supply could be affected. Finally, the fourth path is associated with indirect effects in the local economies regarding the program deployment. Using a sample from Mexican Progresa welfare program, Angelucci and De Giorgi (2009) find positive effects of cash transfers

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[1]In-kind transfers can affect working behaviour in a very different way than monetary transfers do. Contrary to direct cash transfers, in-kind transfers are supposed to increase consumption, and therefore affect labor supply through, at least, three possible paths (holding wages and prices unchanged). First, since in-kind transfers are usually attached to a single or a reduced number of goods (say, food or clothing), this reduces the out-of-pocket expenditures on such consumables. The reduction of these expenditures is tantamount an increase in non labor income, and given that this is a pure income effect, this would predict a decrease in working effort. Nonetheless, it would depend on the intended beneficiary. For example, if children are benefited from nutritional programs in their schools, then parents are encouraged to send their children to school and therefore can increase their labor participation or working hours. Second, in-kind transfer programs are usually tied to working effort. In this regard, opposite to cash transfers, families with zero working hours or reduced employment are those commonly eligible for being in-kind transfer beneficiaries and benefits are reduced for each earned monetary unit. Thus, families may have incentives to reduce their labor force participation in order to become eligibles or to maintain benefits invariant, which is associated with a substitution effect. Third, labor force responses to in-kind transfers would also depend on the weight families allocate to the particular good or goods in the spectrum of consumables, and henceforth, in the budget constraint. To illustrate, assuming that health expenditures represent a considerable share of the household budget constraint, programs aiming to reduce health expenditures (e.g., Medicaid) might reduce labor force participation of adults who otherwise would have had to work intensively in order to meet those expenses. Combined with attention required by injured household members (children, for instance), these programs would reduce labor force participation. Similarly, if the particular in-kind benefit represents a minor share of the budget constraint, the predicted effect is supposed to be negligible. For a further discussion of the relationship between in-kind benefits and labor supply, see Currie 1993, Yelowitz 1995, Blundell and MaCurdy 1999, Moffit 2002, and Hoynes and Schanzenbach 2009.

to beneficiaries on consumption of non beneficiaries in villages where the program was randomly implemented. Alternatively, qualitative studies (Segovia 2011, for example) have described the appearance of fairs ever since CCTs appeared in localities[2].

Dynamic models of labor supply can also add insights to the predicted working effort responses to cash transfers of beneficiaries. According to these models, individuals can replace current for future working hours (and so does consumption) if they perfectly anticipate cash transfer dates and their inter-temporal discount rate between adjacent periods is near one (that is, individuals worth current and future working hours the same so that costs for time allocation across periods tend to zero).

This would imply that (all else equal) individuals can smooth labor intensity in days prior to the payment dates if they act in a forward-looking behavior (see Card, Chetty and Weber 2007). However, individuals can also have preferences for cash-on-hand or disposable income in order to alter labor supply behavior. Through this consideration, current non labor income is not the only path whereby labor supply can be affected by CCTs, but also inter-temporal preferences and time allocation in a dynamic framework.

Another important consideration is whether welfare programs impose arbitrary restrictions on adult labor supply in order to circumvent working disincentive. Despite the initial unconditional intent related to working effort, some developed countries have indexed program benefits according to the labor supply behaviour of eligibles. For instance, the Temporary Assistance for Needy Families (TANF) program in the US (formerly known as the AFDC) initially imposes that at least 20 percent 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 US, consists of 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 (see Eissa and Hoynes 2006 and references therein).

Contrary to these "tied welfare benefits", CCTs in Latin American do not restrict eligibility on labor force participation. This implies that the convexification or 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 that are not eligibles, say because of being just above the poverty line, reduce their working effort in order to diminish total income

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[2]Other studies suggest that individuals are likely to invest in agricultural related productive assets. In a recent article, Duflo, Kremer and Robinson (2011) document that demand for agricultural tools tend to increase in dates nearby payment days or seasons of harvest.

[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.

and "cheat" the system to become eligibles, then the net effect of the CCTs on labor supply of beneficiaries would depend not only on the amount of reduced working hours of the ever-eligibles and the formerly ineligibles, but also in the behavior of the latter group once they have been selected as program beneficiaries and the transfer have been received (e.g., they can return to their initial - optimal - working intensity)[4].

Lastly, since cash is usually transferred to a particular household member (i.e., the mother), 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 incorporate useful ideas. 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. Put it differently, there exists consensus about the redistribution of household resources among family members which are supposed to behave altruistically to each other. Two separate models have been developed associated with 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 and the program targeted beneficiaries within the family, each of the family members would benefit from the monetary transfer because of the intrafamily allocation process. In contrast to this "common will" frame, individual cooperative utility models of intrafamily 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 based on reduced form estimates instead of structural models, however, indicates that single cooperative utility functions prevail in the family bargaining process. Regarding welfare benefits, Lundberg, Pollak and Wales (1997) test the hypothesis of whether families pool their resources exploiting a UK 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. Likewise, Duflo (2003) finds that when pensions to the elderly in South Africa are received by women (grandmothers) instead of men, the physical health of girls (granddaughters) tend to improve relative to that of boys living in the same household, which implies that resources are reallocated favoring human capital formation of girls.

Regarding labor supply, Bertrand, Mullainathan and Miller (2003) suggest that drops in prime-age men's labor supply are stronger than that of prime-age women when the South African pension benefit is received by women[5]. In a recent study, Ardington, Case and Hosegood (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.

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

[5]These results, nevertheless, have been questioned by Posel, Fairbun and Lund (2006), arguing that it is household resident members who reduce labor force participation. Instead, household receiving social pensions (i.e., those who have at least one men aged 65 or over or one women aged 60 or over) are more likely to have members who have migrated to work or looking for work outside the locality.

2.2   Previous Empirical Findings in Latin American Countries

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

Parker and Skoufias (2000) exploit the experimental design of 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, finding no significant effects of Mexican Progresa program on participation rates in the labor force. Instead, they find that women are more likely to reduce hours allocated to leisure mainly because of program commitments such as taking children to schools, clinics and participating in community work.

In a later study, Skoufias and di Maro (2006) evaluate the effects of Progresa on outcomes measuring adult labor supply. Alike Parker and Skoufias (2000) their identification strategy relies on a difference-in-difference 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 a statistically significant effect of CCTs on the probability of being occupied. Moreover, based on the fact of random assignment of the program 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 residing in (randomly) untreated villages. Using a similar estimation methodology for Nicaraguan Red de Protección Social (RPS) program but analyzing the overall household labor supply, that is, the sum of each member's labor intensity, Maluccio (2008) finds a negative small but statistically significant effect of the program on household hours of work, especially in agricultural activities. The author argues that this reduction is explained based on the fact that these activities are perhaps associated with 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 Alimentacão, Bolsa Familia, among others) on adult labor supply, neither on the extensive nor the intensive margin.

Finally, Alzúa, Cruces and Ripani (2010) 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 -PRAF- in Honduras) on 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.

These studies rely on the experimental design of the different programs evaluated, and most of them (with the exception of Skoufias and di Maro 2006) fail to control for the possibility of reallocation of working effort of ineligibles in communities or villages regarding program deployment, as pinpointed 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 (hence introducing a substitution effect), previous empirical findings based on double-difference comparisons are likely to understate the labor supply responses to CCTs.

In the following lines, we attempt to add empirical evidence of the effect of CCTs on adult labor supply based on the Peruvian Juntos program. In contrast with the previous empirical studies, Juntos was not originally experimentally designed, so the identification strategy is based on a sort of "natural experiment" taking advantage of the difference between transfer and interview dates. Albeit this variation seems to be exogenous, plausible and testable assumptions are needed in order to consistently estimate adult labor supply responses to welfare transfers.

3   The Program and Its Mechanics

Following its Latin American counterparts, the Peruvian government launched a nation-wide CCT program in 2005. The program, named Juntos, seeks to reduce current and future poverty through cash transfers and investments in the human capital of children. Initially, Juntos was implemented in 70 municipalities with a budget of US$ 45 million. In 2009, 409,000 families were direct beneficiaries in 638 municipalities and the budget was raised to US$ 260 million. The amount of the transfer is 100 Peruvian *Nuevos Soles* (local currency) every month, which is equivalent to 12% of the monthly household expenditure in our sample [6]. Once the family is enrolled in the program, transfers are given to the female head of the household according to a payment schedule defined by Juntos[7].

It is worth noting that payment 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 the maximum difference between the earliest and the latest payment date is smaller than 7 days. This feature of the program does not represent a major problem to our strategy as it will be shown in section 7.

How do beneficiaries receive the cash transfer? In 2009 there were two mechanisms. 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. One 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. Both systems are mutually exclusive at the village level so beneficiaries do not choose the way they get the money. We discuss the implications of these mechanisms in the next section.

The program does not impose any constraint on the use of the money, however, all beneficiaries must meet the following conditions: i) children of age 6-14 years attend at least 85% school classes; ii) children of age 0-60 months get fully immunized and visit health centres where their growth is measured and vitamins are provided; iii) children of age 3-36 months get nutrition supplements; iv) pregnant women visit health clinics for prenatal care; v) lactating women visit health centres for post-natal care; vi) parents attend health clinics to receive information about

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[6]Alternatively, the payment is equivalent to 63% of monthly per capita expenditure.

[7]Since 2010, the cash transfer is made every two months.

nutrition, health and hygiene; vii) parents without ID (identification) attend the program *Mi nombre* (My Name).

The conditions outlined above are very similar to those of other programs such as *Oportunidades* in Mexico. Given that conditions are related to investments in education and health, these kind of outcomes have received more attention than others. More specifically, Perova and Vakis (2011), using IV and matching methods, find that Juntos has increased consumption and school enrolment. Sanchez and Jaramillo (2012) show that the program has reduced early malnutrition among children in treated households. However, it is still relevant for policy-makers to assess whether Juntos has impacts on the labor supply of its beneficiaries. Now, we turn to discuss our identification strategy.

4   Identification Strategy

Previous studies (Skoufias and di Maro, 2006; Maluccio, 2008; Alzúa et al., 2010) have relied on comparisons between beneficiaries and non-beneficiaries to identify the impact of CCTs on labor supply. Those estimates may be unbiased when randomization is possible but many programs lack an experimental design. In this paper, we propose an alternative strategy, which exploits differences between interview dates of the National Household Survey (Encuesta Nacional de Hogares - ENAHO) and payment dates of Juntos.

In particular, we will explore whether labor supply is reduced in the days near the payment date. To do so, we compare beneficiaries, within the same municipality, who were interviewed just after the payment to those who were not. Given that most households members are engaged in agricultural and highly-flexible occupations (i.e. self-employed), individuals may decide to work less in the week following the payment date.

Though we exploit within-municipality variation in interview dates of ENAHO, our measure of distance is constructed as the difference between the payment date and the week previous to the survey. These seven days prior to the interview day are called the "reference week". When interviewers survey households, they usually ask household members whether they have done specific activities during the previous seven days. For example, when asking about labor force participation, interviewers ask the following question: "during the last week, from [day 1] to [day 7], did you have any job?". Thus, our dependent variable is the hours of work during the "reference week".

To illustrate, Figure 1 plots hours worked in the reference week for distinct groups of beneficiaries according to the distance (in weeks) between the payment date and the reference week. The decline of hours worked during the reference week is linked to the week in which the transfer is received for all individuals included in our sample. Nonetheless, this decline is larger for recipients of cash compared to their partners. The largest decrease in working hours happens when the payment occurs one week before the reference week and it returns to its original level when the payment has not been done yet (the transfer would occur at least one week after the reference week).

Variation in payment 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. If we look at the day of the month, the frequency of dates looks pretty balanced. We also note that almost all interviews are conducted on Sundays, when most of the family members stay at home.

For the empirical analysis, we construct four dummies according to the distance between the payment date and the reference week. Specifically, the first is equal to one if the payment date takes place at least two weeks before the reference week. Similarly, the second dummy is equal to one if the payment is made within the week prior to the reference week. The third variable takes the value of one when the payment from Juntos occurs at some point during the reference week. The last dummy denotes that payment takes place after the reference week. Each dummy may capture a specific effect related to the distance between the date of payment and the reference week.

For instance, the second dummy could capture the time spent (during the reference week) on purchasing goods with the cash received. Similarly, the third dummy may capture the reduction in hours of work related to the time that the cash's recipient needs to go to the bank and withdraw the money. Also, the fourth may capture a "anticipation" bias from beneficiaries.

Given that the distance between date of payment and date of interview (reference week) is exogenous, our empirical equation is :

$$y_{ij} = \lambda_j + \sum_k \delta_k d_{ij} + X_i'\beta + \mu_{ij} \tag{1}$$

where y_{ij} is the outcome variable (participation, hours of work), λ_j is a municipality fixed effect, d_{ij} denotes a specific distance (in days) between date of payment and date of interview, X_i is a vector of covariates such as age, education, native language and so on, and μ is the error term. In the following analysis, the omitted category is that the payment was done at least two weeks before the reference week.

There are two potential threats to the validity of our strategy. On the one hand, it may be possible that when the interviewers of the ENAHO arrive at a given municipality, they go first to families who work less and later to families who work harder. If this were the case, our estimates should be seen as a lower bound (in absolute terms)[8]. On the other hand, our indicator variables may capture other effects not related to the transfer but correlated with other unobservable variables. To check that this is not the case, we conduct a falsification test only including non-beneficiaries in the sample. The details of this procedure would be presented in section 7.

Finally, two limitations of the data may affect our four dummies of interest. First, we only have information about payment dates established by Juntos but we fail to observe the actual date the beneficiary went to the bank and withdrew the money[9]. Second, in some municipalities, there may be two or more payment dates. For example, in a given municipality, there could

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[8]This is because our coefficients are calculated as a function of the omitted category, which is, those who were paid more than one week before the cash transfer (i.e., those who were interviewed first).

[9]This would be true only in the villages where the payment method is through the bank but not in the villages where beneficiaries go to the main square on the payment day to wait for the armored van.

be two villages and each of them may have a different payment date. However, the ENAHO only provides information up to the municipality level. Thus, we are unable to identify which households live in, say, village 1 and who lives in village 2. In these cases, we define the municipality payment date as the first date (the earliest) of payments.[10]

5 Data, Variables and Descriptive Statistics

5.1 Data

Our primary source of information is the ENAHO conducted in 2009 by the Instituto Nacional de Estadística e Informática (INEI). The ENAHO 2009 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 indentify households receiving monetary transfers from Juntos. This is particularly important since the program design refers to women as the only household's transfer 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 female spouses (or, equivalently, cash's 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 (2011) find that the number of households which report receiving cash transfers from Juntos surveyed in the ENAHO 2009 is very close to the number of beneficiary households listed in the official registries. We therefore use sample weights in all of our regressions and correct standard errors based on sampling design. This procedure should guarantee that estimates arising from our sample are representative average effects of the proximity to the payment date on labor supply incentives for all the program beneficiaries.

As an additional concern we check whether the transfer conditions were consistently reproduced in each of the surveyed households. In other words, we check that (i) the household transfer receptor is the mother (female head or household head's spouse), (ii) the monetary transfer reported by the woman is equivalent to 100 Peruvian *Nuevos Soles* (about 37 US current dollars), and (iii) the frequency of transfers is monthly. Around 98% of the transfer recipients in our sample were women satisfying the mentioned conditions.

Further, we check that the surveyed households which reported having received monetary transfers from Juntos satisfy the eligibility conditions. Regardless of the fact that eligible house-

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[10]In section 7, we use the last date of payment and our results remain unchanged.

holds should be below the poverty line in order to receive the transfer, our working sample suggests that about 19% of the households were above the poverty line defined by INEI [11].

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 3,781 individuals living in 1,215 households enrolled in Juntos.

5.2 Outcome Variables

We focus on three different measures of labor supply behavior: participation (extensive margin) weekly hours worked (intensive margin) and working for paid activities. As described above, each of the outcome variables are defined for the week before the day of the interview (which usually takes place on Sundays). Labor participation is a dummy variable equals to one when the individual reported having worked or searching for a job any time during the seven days prior to the interview. To measure labor intensity, we take the total number of hours worked during the same week. These two variables are commonly used in empirical studies (Skoufias and di Maro, 2006; Maluccio, 2008; Fogel and Paes de Barros, 2010; Alzúa et al., 2010) and henceforth are also useful to make comparisons of adult labor supply responses to cash transfers across Latin American countries. 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).

Given that we have information of the number of hours worked in each day of the reference week, we are 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.

5.3 Descriptive Statistics

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 dummies of distance. The average individual is about 42 years old. With regards to the educational attainment, 18% of individuals report not having reached any regular basic educational level, 60% have (incomplete) primary level education, and 17% reported at least one year of secondary education. About 1% of individuals have at least one formal year of tertiary education and only 1% of individuals have completed tertiary education. We also include native language indicators in order to capture race heterogeneity. Individuals are 65% likely to report Quechua as their native language. Both the high percentage of Quechua speakers and the 85% of individuals living in rural areas suggest that our sample is mainly composed of indigenous people. Given the possibility of filters among the program regarding non-poor people receiving cash transfers,

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[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).

we include three poverty indicators: (i) non-poor, (ii) poor, and (iii) extremely poor. These indicators are calculated by INEI using a poverty line and the monthly per capita expenditure at the household level as a welfare measure. Around 21% of individuals in the whole sample are non-poor, while roughly 42% are extremely poor and 37% are poor. The bottom part of the table shows descriptive statistics of the outcome variables used in the analysis. Around 94% of individuals reported participating in the labor force. The number of hours worked is about 30 per week. Finally, 56% of individuals in our sample reported having worked for paid activities.

Table 2 also shows some heterogeneity between groups. However, these differences in characteristics such as education and native language seem negligible. In the following empirical analysis we include this set of variables and municipality fixed effects in order to control for these slight differences. In section 6 we present the results arising from the transfer proximity model described in the previous section.

Main regressions are estimated only for poor individuals to circumvent potential problems associated with the possibility of self-selection which can bias our results (e.g., individuals who are not poor can mislead the program targeting procedure in order to become eligibles). However, we also present evidence that when including non-poor individuals in our regressions, coefficients do not significantly differ.

6 Results

6.1 Main Results

Table 3 reports the resulting estimates for the equation of labor force participation. Each row indicates the distance between the cash transfer and the reference week. Columns (3), (6) and (9) are our preferred specifications since they include municipality fixed effects as well as individual covariates. Results from these columns suggest that there are no effects on the extensive margin of the labor supply (i.e. participation) even when splitting the sample by recipient and recipient's partner.

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 three possible confounding factors: (i) anticipation bias (increasing demand for leisure just before the transfer is made); (ii) time spent in transportation from the location of residence to the bank; and (iii) time spent in purchasing the goods or consuming the money once it has been withdrawn from the bank. Under the assumption that 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 the week before the beginning of 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[12].

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[12]It is worth noting, however, that the reduction in working hours occurs in the reference week. So if there

Now, we explore if the distance between the cash transfer and the reference week affects the decision of working for paid activities. The dependent variable is a dummy which is equal to one if the individual is engaged in a paid-job and is zero, otherwise. Results from this estimation are presented in Table 5. The estimated effects are insignificant but in the case of recipients, they are negative while in the case of their partners the estimates are positive. These results may reflect some rigidities to switch from unpaid jobs to paid jobs in the short run.

In addition, we test whether the reduction in hours devoted to working activities is concentrated in a particular day 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 reduce by roughly 1.3 hours in every day except for Sundays. In addition, we find that hours of work on Thursday reduce by 1 hour if payment occurs in the reference week.

These 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. However, when payment takes place one week before the reference week, the reduction in labor intensity is evenly distributed along reference week which is inconsistent with the hypothesis that our results are mainly driven by transportation from the household to the bank. Thus, the dummy "during the reference week" captures the reduction driven by transportation while the dummy "one week before" reflects the disincentives to work generated by the having received the cash transfer.

6.2   Heterogeneous Effects

The purpose of this section is to explore whether there are heterogeneous effects of the cash transfer on weekly hours of work [13]. We begin by splitting the sample according to married and not-married women. Results from these estimations are shown in Table 7. Interestingly, we find that if cash transfer is made in the week prior to the reference week, the reduction in hours of work is larger - 11 hours- for married women than for not-married. One potential explanation for this difference is that married women also rely on their husbands' income and this allow them to reduce their labor supply more than not-married women.

Next, we analyze if there is heterogeneity between young and old 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. The point estimate of the effect of being paid one week before the reference week is larger for young recipients - 12 hours- than for old recipients. From a theoretical point of view, this evidence is consistent with young people having younger children or higher discount rates[14] and therefore,

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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).

[13]We should mention that when comparing independent v.s. dependent beneficiaries and highly-educated v.s. low-educated recipients , we find no difference (results not reported) in the effects.

[14]Behavioral economists suggest that young people is usually more present-biased than old people.

reacting more than old people, to the same income shock.

Lastly, 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 than recipients with older children. The point estimate 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 taking care of their children.

Taken together, these heterogeneous effects provide evidence which is consistent with an non-labor income shock. They also shed some light on what mechanisms explain our main results. Now, we turn to question some assumptions made and perform some robustness checks to see what happens if these assumptions do not hold.

7   Robustness Analysis

When we identified the beneficiaries from Juntos, we excluded non-poor households. However, in our data some of them claimed that they were receiving the cash transfer from the program on a monthly basis. Although Juntos is targeted at the poor, it is possible that households who were poor when Juntos arrived at their municipalities escaped from poverty along the years. In Table 10 we present estimates from equation (1) but including non-poor beneficiaries (only recipients). We find that having been paid one week before the reference week reduces by 6 hours the labor supply of beneficiaries -see column (3). These estimated effects are slightly larger than those presented in Table 4. This additional evidence suggests that the reduction in hours of work is not driven by the time needed to withdraw the money given that non-poor beneficiaries are more likely to spend *less* time going from home to the bank. Moreover, this difference may suggest that the labor supply of non-poor beneficiaries is more elastic than that of poor beneficiaries.

A major threat to our identification strategy is that the dummies of distance between payment dates and interview dates may be capturing other variables not related to the cash transfer, but to the specific date of the payment. For instance, it could be 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 data from non-beneficiaries. If our dummies are correlated with variables that affect labor supply, they should also have an impact on the hours of work of non-beneficiaries. Thus, we estimate equation (1) but only including non-beneficiaries in our sample[15]. Table 11 presents results from this estimation. Not surprisingly, we find that none of our distance dummies are significant at any conventional level. This evidence suggests that our indicator variables are not correlated with omitted variables that may affect the labor supply. Based on these results, our identification strategy does not seem to be invalid.

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[15]We include spouses of household heads who did not report to be beneficiaries from Juntos but who lived in municipalities that the program has reached.

Recall - see section 3- that some municipalities may have more than two payment dates. In the previous analysis we have used the first payment date. Now, we re-estimate equation (1) with the set of four dummies but using the last payment date instead of the first. In Table 12, we present results from this estimation. The first three columns report results for only poor recipients and the other three include non-poor recipients in the sample. In column (3) we find that the effect of having being paid one week before is slightly smaller than when we used the first date of payment (column (6) in Table 4) but it is still highly significant. In column (6), we include non-poor recipients in the sample and the estimated effect is larger than in column (3). Also, it is remarkably similar to that of column (3) in Table 10 (when we used the first date of payment). Thus, 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.

Even after all these checks one may still argue that our results are being driven by the time recipients spend in going to the bank. In order to rule out this possibility, we split the sample according to the payment mechanism defined by Juntos at the village level. The payment mechanisms are: i) going to the bank (which may not be at your village of residence) at the date defined by Juntos or later and ii) going to the main square of your village to wait for an armored van and receive the cash on the same day defined by Juntos. In Table 13, we present the estimates of these regressions. As we can see, the estimates are not significant for those beneficiaries who went to the bank. In contrast, the effects of receiving the cash transfer one week before is large - about 9 hours- and highly significant for recipients who went to the main square of their village. This last piece of evidence tells us that the reduction in hours of work is due to an income shock and is not related to the time the beneficiary needs to go to the bank.

8   Concluding Remarks

It is well-known that welfare programs in developed countries have unintended effects on labor supply (Moffit 2002). In spite of this evidence, there have been few efforts to identify the impacts of CCTs on the labor supply of their beneficiaries.

In this paper we make a first attempt to estimate the effects of cash transfers on labor supply without using an experimental research design. Given the high flexibility of rural occupations (mostly agricultural), our approach consists of studying the behavior of beneficiaries in days near to the payment dates of Juntos. In particular, we find that having been paid one week before the reference week reduces the labor supply of female heads (cash recipients) by about 6 hours in the mentioned week.

Some interesting policy implications arise from our findings. First, CCTs could have larger effects on education and health if mothers that work less are encouraged to invest more time with their children. Second, changes in the frequency of payments may alter the magnitude of the estimated effects in this study. Third, it should be analyzed whether it is feasible to offer training or new technologies for agricultural activities (e.g. use of fertilizers) near the payment dates. Based on results from Duflo et al. (2011), these special offers could have large impacts on productivity given that households have extra time and money during these days.

Finally, we believe that our strategy could be used to analyze other interesting outcomes as

well. 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 see if recipients of the cash do not lose control over the money once she arrives 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. These are promising avenues for future research that may expand the discussion about the benefits and limitations of CCTs.

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Figure 1: Weekly Hours of Work according to distance from Payment
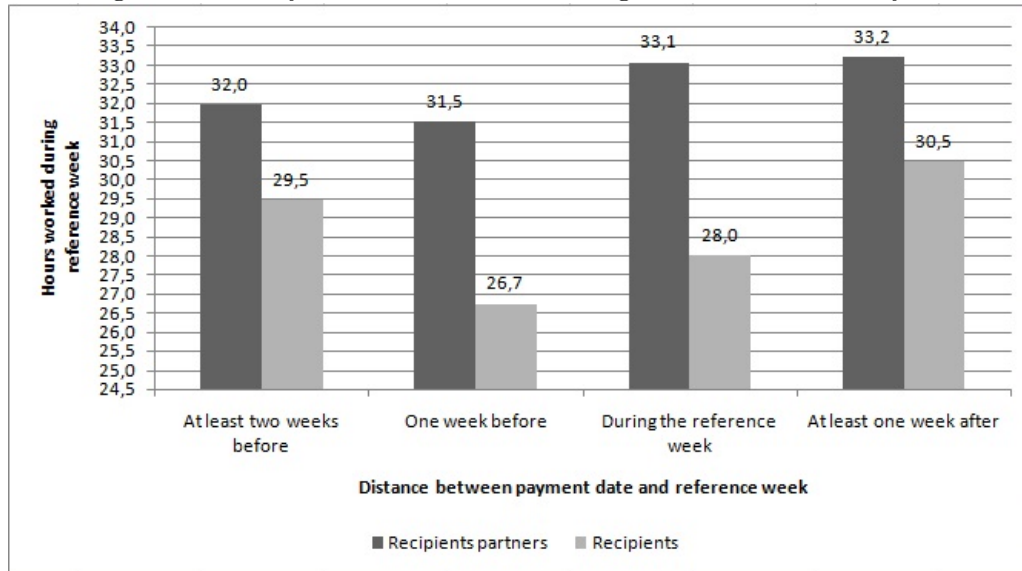
Table 1: Distribution of Payment and Interview Dates

Panel A: Dates of Payment	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: Dates of Interview	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)

Table 2: Descriptive Statistics

	Distance between cash transfer date and the reference week			
Variable	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)
Native Language: Spanish	0.30	0.28	0.33	0.27
	(0.46)	(0.45)	(0.47)	(0.44)
Native Language: Quechua	0.61	0.71	0.62	0.68
	(0.49)	(0.46)	(0.49)	(0.46)
Native Language: Other	0.09	0.01	0.05	0.05
	(0.28)	(0.09)	(0.22)	(0.22)
Poverty status: Extremely poor	0.40	0.47	0.43	0.47
	(0.49)	(0.50)	(0.50)	(0.50)
Poverty status: Poor	0.37	0.39	0.36	0.35
	(0.48)	(0.49)	(0.48)	(0.48)
Poverty status: Non poor	0.22	0.14	0.21	0.18
	(0.42)	(0.35)	(0.41)	(0.39)
Lives in rural areas	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	425	565

Table 3: Effects of distance between cash transfer and the reference week on labor force participation

	All			Recipients			Recipients' partner		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transfer was:									
One week before the reference week	-0.028*	-0.009	-0.010	-0.072**	-0.044	-0.043	0.015*	0.006	0.148
	(0.016)	(0.030)	(0.030)	(0.029)	(0.060)	(0.065)	(0.009)	(0.007)	(0.203)
During the reference week	-0.027	-0.011	-0.008	-0.054*	-0.046	-0.047	0.002	0.019	0.162
	(0.017)	(0.032)	(0.032)	(0.029)	(0.059)	(0.070)	(0.012)	(0.016)	(0.206)
At least one week after the reference week	-0.011	-0.007	-0.011	-0.029	-0.024	-0.035	0.010	-0.001	-0.319
	(0.014)	(0.024)	(0.024)	(0.024)	(0.047)	(0.050)	(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	0.186	0.008	0.284	0.293	0.005	0.280	0.399

<u>Note</u>: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

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Table 4: Effects of distance between cash transfer and the reference week on weekly hours of work

	All			Recipients			Recipients' partner		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transfer was:									
One week before the reference week	-0.558	-1.899	-1.756	-1.640	-4.192*	-5.618**	0.241	-0.425	-0.425
	(1.131)	(1.840)	(1.832)	(1.398)	(2.284)	(2.397)	(1.789)	(2.903)	(2.903)
During the reference week	-0.294	-1.234	-0.972	-0.790	-0.594	-1.836	0.196	-1.537	-1.537
	(1.164)	(1.849)	(1.835)	(1.414)	(2.213)	(2.367)	(1.872)	(3.017)	(3.017)
At least one week after the reference week	1.308	0.678	0.996	0.992	3.802*	2.573	1.614	-3.312	-3.312
	(1.073)	(1.825)	(1.806)	(1.300)	(2.206)	(2.295)	(1.729)	(2.918)	(2.918)
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	0.002	0.259	0.284	0.005	0.395	0.409	0.001	0.419	0.419

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

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Table 5: Effects of distance between cash transfer and the reference week on working for paid activities

	All			Recipients			Recipients' partner		
Transfer was:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
One week before the reference week	0.027	0.070	0.021	-0.019	-0.015	-0.013	0.004	0.028	0.028
	(0.035)	(0.065)	(0.036)	(0.037)	(0.062)	(0.041)	(0.016)	(0.024)	(0.024)
During the reference week	-0.020	-0.062	-0.046	-0.030	-0.086	-0.054	-0.026	-0.003	-0.003
	(0.037)	(0.065)	(0.035)	(0.037)	(0.064)	(0.041)	(0.021)	(0.026)	(0.026)
At least one week after the reference week	0.005	-0.029	-0.043	0.001	-0.081	-0.047	-0.003	0.001	0.001
	(0.034)	(0.065)	(0.035)	(0.035)	(0.065)	(0.042)	(0.016)	(0.019)	(0.019)
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	0.720	0.001	0.358	0.699	0.004	0.402	0.402

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 6: Effects of distance between cash transfer and the reference week on daily hours of work (recipients only)

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
Transfer was:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
One week before the reference week	0.828*	-0.732	-0.810*	-1.269**	-1.319***	-1.334***	-0.982*
	(0.452)	(0.494)	(0.491)	(0.498)	(0.479)	(0.514)	(0.535)
During the reference week	1.231**	-0.183	-0.760	-0.783	-0.994*	-0.644	0.297
	(0.500)	(0.531)	(0.527)	(0.520)	(0.534)	(0.548)	(0.552)
At least one week after the reference week	1.341***	0.342	0.225	0.019	-0.084	0.074	0.656
	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 7: Effects of distance between cash transfer and the reference week on weekly hours of work by Marital Status

	Married		Not Married	
Transfer was:	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 8: Effects of distance between cash transfer and the reference week on weekly hours of work by Group Age

	Young (Under 40)		Old	
Transfer was:	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 9: Effects of distance between cash transfer and the reference week on weekly hours of work by Children's age

Transfer was:	With Children aged 5 or less		With children aged 6 or more	
	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 10: Effects of distance between cash transfer and the reference week on weekly hours of work (recipients only, including non-poors)

Transfer was:	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 11: Effects of distance between cash transfer and the reference week on weekly hours of work (non-beneficiaries housewifes)

Transfer was:	(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

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 12: Effects of distance between cash transfer and the reference week on weekly hours of work using the last payment date (recipients only)

	Poors			Poors and Non-poors		
Transfer was:	(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)	(2.718)	(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	0.392	0.003	0.339	0.339

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.

Table 13: Effects of distance between cash transfer and the reference week on weekly hours of work by Payment Mechanism

Payment Mechanism	Bank		Armored Van	
Transfer was:	(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	Yes	Yes	Yes	Yes
Observations	369	369	458	458
R-squared	0.086	0.478	0.060	0.400

Note: Clustered standard errors at the village level in parentheses. Additional controls include: sex, marital status, age, education, native language indicators, poverty status dummies and a dummy variable that indicates whether or not the individual lives in a rural area. Finally we interact our dummies of interest with a dummy that is equal to one when the individual is the head of the household.