Temptation Goods and Conditional Cash Transfers in Peru*

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Abstract

We examine whether beneficiaries of a CCT program in Peru change their food consumption patterns when they have the cash on their hands. Exploiting variation in interview dates and pay dates, we find that food expenditures go up by 10-20 percent when benefit recipients have the cash transfer on their hands. Moreover, beneficiaries with cash-on-hand are more likely to consume candies, chocolates, soft drinks and meals in restaurants. These findings are inconsistent with standard models of intertemporal choice such as the Permanent Income Hypothesis. However, models of borrowing constraints combined with commitment problems are consistent with our results.

Keywords: CCT, Temptation goods, Peru
JEL Codes: D91, I38, O10

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1 Introduction

In the last decades, many governments around the world implemented Conditional Cash Transfers (henceforth CCT) programs in order to reduce current and future poverty. These programs provide cash to poor households if they meet some conditions such as sending their kids to school or taking them to health centers on a regular basis (Fiszbein and Schady 2009). However, cash recipients are free to choose how to spend their extra money.

A growing and recent literature documents that many poor individuals exhibit self-control problems and present-biased preferences (see Bryan, Karlan and Nelson, 2010 for an excellent review). Such behavior can affect different intertemporal decisions which are crucial for escaping out of poverty. For example, it may influence how much poor farmers invest in fertilizers (Duflo, Kremer and Robinson 2011) or how much families save for health-protective technologies (Dupas and Robinson 2013). Evidently, time-inconsistent behavior is also observed in rich individuals but the very fact that the poor have fewer resources may exacerbate the consequences of even small *unplanned expenditures*. Indeed, Banerjee and Mullainathan (2010) theoretically show that consuming "temptation goods" can generate poverty traps. Examples of temptation goods could be donuts or beers because we derive utility at the moment of consuming them, but "we would like future selves not to spend money on them" (Banerjee and Mullainathan 2010).

This paper is the first attempt to evaluate whether cash transfers could increase the consumption of temptation goods among beneficiaries of "Juntos", a CCT program in Peru. Two previous studies estimate the effects of Juntos on consumption, education and nutrition. On the one hand, Perova and Vakis (2012), using instrumental variables and matching methods, find that the program increases overall consumption and school enrollment. On the other hand, Sanchez and Jaramillo (2012) show that Juntos reduces early malnutrition among children in treated households.

Unlike these papers, we do not rely on comparisons between treated and control households. Instead, we look directly at how beneficiaries change their consumption when they have the cash on their hands. Standard models of intertemporal behavior will predict no change in consumption after receiving the cash transfer because such transfers are known in advance. In particular, any increase in consumption will violate the Permanent Income Hypothesis (PIH). Thus, we begin our empirical analysis by testing whether food consumption jumps up when beneficiaries have the cash on their hands, relative to when they do not have it[1].

[1]We do not attempt to summarize this literature here but it should be noted that some

Then, -and more importantly- we focus on testing whether having the cash transfer makes beneficiaries shift their consumption toward "temptation goods". To do so, we exploit within-district differences in the interview dates of the Peruvian Household Survey (henceforth, ENAHO for its name in Spanish) and between-districts variation in the pay dates from Juntos. The combination of the timing of the interviews and the payment schedule exogenously generates that some beneficiaries are interviewed when they have the cash on their hands while others do not.

We departure from the large literature on CCTs (for reviews see Parker, Rubalcava and Teruel 2008; Fiszbein and Schady 2009), in which treatment effects of cash transfers are estimated by comparing the consumption of treated households with that of non-treated families[3]. For instance, several papers exploit the random assignment of PROGRESA, a well-known CCT program in Mexico[4]. Rubalcava, Teruel and Thomas (2004) find that the additional money from PROGRESA increases spending on food and investments in small livestock. Attanasio and Lechene (2011) show that there is a positive relation between food expenditures and income in the hand of women from PROGRESA. These two papers argue that the increase in food consumption is driven by the fact that the money is given to the female head of the household[5]. Finally, Angelucci and Attanasio (2013) find that cash transfers to women in urban Mexico increase high-protein food consumption.

Two papers outside the literature on CCTs have similar empirical strategies to ours. First, Shapiro (2005) examines monthly consumption patterns of food stamp recipients in the US and finds that the caloric intake of members of recipient households declines by 10 percent over the food stamp month. Using information on the exact date of the last benefit payment received by each recipient household and exploiting cross-sectional variation in the interview dates of the Continuing Survey of Food Intake by Individuals (CSFII),

authors find empirical evidence that supports the PIH (Browning and Collado, 2001; Hsieh, 2003; Shapiro and Slemrod, 1995) while others do not (Parker, 1999; Shapiro and Slemrod, 1995; Souleles, 1999; Stephens Jr. and Unayama, 2011) [2]. Most studies from this empirical literature have three common features: i) they focus on developed countries; ii) they use fiscal policy interventions and changes in the credit market to estimate the consumption response to income; and iii) they explain the reaction of consumption to predictable income changes by taking into account liquidity constraints: consumption will react to predictable changes in income because individuals cannot borrow against their future income.

[3]As shown by Angelucci and De Giorgi (2009), Bobba and Gignoux (2014), these comparisons are ignoring spillovers or externalities on non-treated households.

[4]This program is now known as OPORTUNIDADES.

[5]Schady and Rosero (2008) find that unconditional cash transfers to women in Ecuador also increase food consumption.

his results suggest that caloric intake declines by 0.45 percent per day after receipt of stamps. Second, Mastrobuoni and Weinberg (2009) use data from the US to analyze the shape of consumption profiles over the month for Social Security benefit recipients. Using variation in the interview dates of the CSFII, they find that individuals with less than $5,000 in savings have consumption that is 24 percent lower during the final few days of the pay cycle than it is during the first week. The empirical evidence of these two papers is hard to reconcile with the PIH but it could be explained with models of quasi-hyperbolic discounting and self-control problems.

We document two changes in the patterns of food consumption when beneficiaries have cash on their hands. First, we find that consumption increases by 10-20 percent during the days when benefit recipients have cash-on-hand. This jump in consumption is inconsistent with the Permanent Income Hypothesis, which predicts no change in consumption given that cash transfers are perfectly anticipated by beneficiaries. Second, our results show that having cash-on-hand makes it more likely to consume temptation goods such as chocolates, soft drinks and meals in restaurants. We think of these goods as tempting because they are more tasty, more expensive but perhaps less nutritive. These findings represent our contribution to the literature.

The rest of the paper proceeds as follows. Section 2 describes the basic features of Juntos. Section 3 gives the details of our data. Section 4 outlines the identification strategy. Section 5 presents our results. Section 6 offers concluding remarks.

2 Juntos: The Peruvian CCT program

In 2005, the Peruvian government launched a nation-wide CCT program named Juntos. This program seeks to reduce current and future poverty through cash transfers and investments in children's human capital (health and education). In its first year, Juntos was implemented in 70 districts, and its budget was close to US$ 45 million. By the end of 2013, 718,275 families were direct beneficiaries in 1,097 districts[6] and the program's budget was more than US$ 320 million [7].

The program eligibility was defined in two stages. First, eligible districts were chosen on the basis of five criteria: i) exposure to political violence

[6]These numbers were taken from the program's website on November 2014: http://www.juntos.gob.pe/index.php/usuarios/cobertura-geografica.

[7]This amount was taken from the Ministry of Finance's Budget Office website on November 2014: http://apps5.mineco.gob.pe/transparencia/Navegador/default.aspx . We used an exchange rate of 2.75 *Nuevos Soles* per US dollar.

during the 1980s; ii) poverty rates; iii) poverty gap; iv) child malnutrition; and v) extreme income poverty. These five indicators were combined into a single summary index that measures each district's needs for public spending and support. Districts were ranked according to this aggregate score and then Juntos set the roll-out of the program. Once eligible districts were identified, census data were collected in these areas to determine eligible households. Using these data, household eligibility was based on poverty. Finally, only households with children under 14 years old or at least one pregnant woman were selected.

The program does not impose any constraint on the use of the cash transfer, although all beneficiaries must meet the following conditions: i) children of age 6-14 years attend at least 85 percent school classes; ii) children of age 0-60 months get fully immunized and visit health centers 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 centers for post-natal care; vi) parents attend health clinics to receive information about nutrition, health and hygiene; vii) parents without ID (identification card) attend the program *Mi nombre* (My Name).

The conditions outlined above are very similar to those of other CCT programs (e.g., PROGRESA). However, Juntos was not randomly assigned so comparisons of treated and non-treated household would deliver inconsistent estimates of the impact of the program. Although there have been efforts to estimate the impact of Juntos on schooling, consumption and malnutrition (Perova and Vakis, 2012; Sanchez and Jaramillo, 2012), we follow a different objective in this paper and exploit differences in the ENAHO interview dates and the pay dates defined by Juntos.

Until December 2009, the amount of the transfer was 100 *Nuevos Soles* every month, which is equivalent to US$ 38. Since January 2010, each household receives 200 *Nuevos Soles* every other month. Once the family is enrolled in the program, transfers are given to the female head of the household according to the payment schedule defined by Juntos. All beneficiaries are informed in advance about the exact pay dates.

3 Data: ENAHO and Juntos payment schedule

Our primary data source is the household survey, known as ENAHO, conducted by the *Instituto Nacional de Estadística e Informática* in 2009 and

2010. In each year, this survey consists of a nation-wide representative sample that collects rich information at the individual and household level. In this study, we use data from the food expenditures record to construct our outcome variables.

We focus on food consumption for two reasons. On the one hand, food expenditures account for a large fraction of total expenditures in poor families. On the other hand, food consumption is reported for the two weeks preceding the ENAHO interview date. That is, interviewers ask household members whether they have consumed a large list of goods in the last two weeks. For instance, interviewers ask the following question: "in the last two weeks, from [day 1] to [day 14], have you consumed chicken?". We will refer to these two weeks before the interview as the "reference period". Interview dates vary within districts across households. Figures 1 and 2 display the distribution of interview dates in both years.

Our outcome variables are food expenditures and consumption indicators of "temptation goods". We build four sets of temptation goods: i) Sweets: candies and chocolates; ii) Alcohol: beer, whiskey, rum, *pisco*, a very popular brandy among Peruvians; iii) Soft drinks, and iv) Restaurants: roasted chicken, Chinese food and barbecue[8].

Are these set of goods really tempting? Maybe only for poor individuals who usually struggle to consume all the calories they need. Our choice is motivated by the empirical results of Jensen and Miller (2011) which indicate that consumers who face price subsidies in China shift their consumption toward more tasty food. That is, these goods are tempting in the sense that they are more tasty, more expensive and less nutritive. Also, Banerjee et al. (2013) use the following measure of consumption on temptation goods: "sum of monthly spending on meals or snacks consumed outside the home; pan, tobacco and intoxicants; and lottery tickets/gambling".

To correctly identify Juntos beneficiaries in ENAHO, we check whether the transfer conditions are consistently replicated in each surveyed household. In other words, we check that (i) the cash recipient is the mother (female head or household head's spouse); (ii) the amount of the transfer reported by the woman is equivalent to the actual transfer (100 or 200 *Nuevos Soles*); and (iii) the frequency of transfers is equivalent to the actual frequency (monthly or every other month). Around 98 percent of the cash recipients in our sample are women satisfying the mentioned conditions.

Our secondary data source is the payment schedule of Juntos at the village level for the years 2009 and 2010, provided by program's officials upon authors' request. This data set includes the exact pay date (day/month/year)

[8]Unfortunately, ENAHO does not include questions about consumption of cigarettes

for all villages enrolled in the program during these years (Figures 3 and 4 show the distribution of pay dates). That is, Juntos sets a particular day in every village so we have some within-district variation in the exact day of payment but all payments within a district are made in the same week. For example, in a given district, there could be two villages and each of them may have a different payment date: in village 1 the pay takes place on Tuesday while in village 2 the pay occurs on Friday.

However, ENAHO only provides information up to the district level so we cannot identify which households live in village 1 and who lives in village 2. In these cases, we define the district pay date as the first date (the earliest) of payments (in our example, the pay date would be Tuesday). This data limitation leads us to act as if we do not observe the exact day of payment but only the week of payment. Thus, pay dates in our data vary between districts rather than within-districts. In 2009, ENAHO reached 40 percent (261 out of 638) of the districts treated by Juntos, while in 2010 it reached 25 percent (159 out of 646). District identifiers are used to match the information of pay dates to the beneficiaries sample from ENAHO.

Our final sample contains information on 3,772 and 2,678 households in 2009 and 2010, respectively. Household characteristics (e.g., access to water/electricity, head's level of education) that will be included as controls are taken from ENAHO's housing and education records. In Tables 1 and 2, we present averages of covariates in the control group, and the associated difference between control and treatment groups. Specifically, we regress each covariate on the treatment dummy along with district fixed effects. All differences are small and most of them are statistically insignificant. These figures are reassuring because our specifications rely on within-district variation to estimate the change in consumption patterns.

4 Empirical Strategy

We do *not* attempt to estimate the average treatment effect of the program by comparing treated and non-treated households. Rather, the main purpose of the empirical analysis is to answer two related questions. First, we want to test whether food consumption jumps up when beneficiaries have the cash on their hands. Second, if such a jump is observed, we want to check whether it is related to an increase in the likelihood of consuming "temptation goods". We believe that answering these questions could be helpful for understanding the link between cash transfers and food consumption.

Our strategy exploits differences in ENAHO interview dates and Juntos pay dates to answer our questions of interest. On the one hand, interview

dates vary across households *within* a district and determine the "reference period" for each of them. On the other hand, Juntos pay dates do not vary within districts but *between* districts. The combination of pay dates and interview dates generates that some households receive the cash transfer within the reference period while others do not.

Thus, we compare beneficiaries who have the cash on their hands in the reference period to those who do not. We construct two groups depending on whether the cash transfer is received during the reference period or not. The first group includes households who have the cash transfer on their hands in the reference period. The second group consists of beneficiaries who do not receive the cash transfer in the reference period. For convenience, we refer to these two groups as the "treatment" and "control" group, respectively. We think of the "treatment" as having cash-on-hand in the reference period. But note that we are comparing beneficiaries so our "treatment" does not mean that some households are enrolled in the program and others do not.

We begin our empirical analysis by running the following regression:

$$\log c_{ijm} = \alpha_j + \alpha_m + \beta T_{ijm} + X_{ijm}^{'}\Gamma + \mu_{ijm} \tag{1}$$

where c_{ij} is food consumption of household i in district j in month m, α_j is a district fixed effect, α_m is a month fixed effect, T_{ijm} indicates whether the household belongs to the treatment group or not, X_{ijm} is a vector of covariates, and μ_{ijm} is the error term. The parameter of interest is β and captures the difference in consumption between the treatment and control group.

Then, we estimate the following equation for each set of temptation goods:

$$z_{ijm} = \lambda_j + \lambda_m + \delta T_{ijm} + X_{ijm}^{'}\Phi + \epsilon_{ijm} \tag{2}$$

where z_{ijm} is a binary variable which is equal to one if household i that lives in district j in month m consumed temptation goods and zero otherwise. All other variables have been defined in the previous paragraph. If parameters β (see equation (1)) and δ (see equation (2)) are positive, then we conclude that there is a jump in consumption when consumers have the cash on their hands and that this jump is driven by an increase in the consumption of "temptation goods".

Our identifying assumption is that, conditional on our controls, the distance between ENAHO interview dates and Juntos pay dates is randomly determined across households. To put it differently, we assume that the only difference between treatment and control groups is that the former had the cash on their hands two weeks before the interview date while the latter did not. Shapiro (2004), Mastrobuoni and Weinberg (2009), and Fernandez and

Saldarriaga (2014) rely on similar identification strategies to recover their parameters of interest.

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 district, they go first to richer families and later to families who are poorer. If this were the case, our estimates should be seen as a lower bound [9]. On the other hand, our treatment variable may capture other effects not related to the transfer but correlated with other unobservable variables that determine food consumption. To check that this is not the case, we conduct a falsification test using a sample of non-beneficiaries who live in districts treated by Juntos (the details of this procedure would be discussed in Section 5).

One additional concern would be that Juntos pay dates are systematically correlated with other pay dates. For instance, it could be that both Juntos and employers make their payments on the same days. This concern would be especially problematic if most of our individuals were wage-earners. However, beneficiaries from Juntos are self-employed workers (mostly in agriculture) who do not earn wages (see Fernandez and Saldarriaga 2014 for a more detailed analysis of the occupations of beneficiaries from Juntos). Thus, other pay dates that could be correlated with Juntos payment schedule do not represent a threat to our strategy.

5 Results

In Table 3, we present the estimates of the effects of receiving the cash transfer within the reference period. Each column is a separate regression: columns (1) and (2) are for the year 2009, and columns (3) and (4) correspond to the year 2010. The estimates suggest that food expenditures go up by 10-20 percent when payment occurs in the reference period. The fact that the amount of transfer in 2010 is two times the amount of the transfer in 2009 may account for the difference between the point estimate in columns (1)-(2) and (3)-(4). It is also worth noting that once we include controls in the regressions, the point estimates go down but remain significant.

We should bear in mind that increases in food expenditures do not necessarily lead to higher consumption. For instance, cash recipients may just buy food for the future and stock it in the house. If this were the case, then our results do not provide evidence against the PIH. Thus, by only looking at

[9] This is because households in the control group may be richer and therefore their level of consumption could be higher even in the absence of the treatment.

these results, we cannot claim that consumption increases when beneficiaries have the cash on their hands.

Table 4 presents evidence on the effects of receiving the cash transfer in the reference period on the consumption of temptation goods. Each column is a (separate) regression with a different dependent (discrete) variable. In Panel A, we find that receiving the cash in the reference period only affects the consumption of the last set of temptation goods: it increases the likelihood of eating at a restaurant by 1.5 percentage points. This estimated effect is rather large because in our sample the probability of eating at a restaurant is 2.28 percent in 2009. In Panel B, we present the results for the sample of households in 2010, when the amount of the cash transfer is larger than in 2009. We find that having cash-on-hand increases consumption of sweets, soft drinks and meals outside the home. The point estimates are 6.5, 6.4 and 5.5 percentage points, respectively. Again, these estimates reflect large jumps in the probability of consuming temptation goods because their baseline levels are very low. We do not see any increase in the consumption of alcohol. This lack of response in alcohol may be driven by the fact that women usually spend less on alcohol than men. Finally, given that eating at a restaurant necessarily increases consumption, we now feel more confident to say that consumption increases when beneficiaries have the cash on their hands.

What can explain these results? A natural explanation for this jump in consumption would be borrowing constraints: poor people have to wait until the pay date to make their food purchases. We do believe that borrowing constraints play a role in determining the expenditures pattern among poor households. However, we think that credit market imperfections *alone* cannot explain why poor people shift their consumption toward tempting goods when they have additional cash on their hands. This last result could be better explained by models of time-inconsistent consumers who face difficulties for saving[10].

Our results are in line with those of Barrera-Osorio et al. (2011). Exploiting a randomized experiment in Colombia, the authors find that postponing the cash transfer to the beginning of the school year (what they call the "savings treatment") is more effective at increasing enrollment than giving bi-monthly transfers ("standard treatment"). Therefore, their results suggest that poor households face challenges for saving money.

Moreover, the estimated change in consumption patterns may explain why Fernandez and Saldarriaga (2014) find that beneficiaries from Juntos

[10]The "temptation model" of Banerjee and Mullainathan (2010) is, according to Bryan, Karlan and Nelson (2010), a specific version of hyperbolic discounting

reduce their labor supply in the days following the cash transfer (i.e. beneficiaries work less because they spend additional time enjoying meals in restaurants).

In Table 5, we show the impact of having cash on hand on four sets of food: milk, eggs, beans, and fruits. Though the consumption of temptation goods increases, we find no change in the consumption of nutritive food. In all specifications, we cannot reject the null hypothesis that each coefficient is equal to zero. These results reinforce the idea that having cash on hand induces consumers toward more tasty and expensive food but does not increase the consumption of nutritive food. Moreover, the fact that the consumption of these goods does not increase suggests that borrowing constraints are not the main driver of our results.

One may think that cash recipients (recall they are women) increase consumption in order to keep the money away from their husbands, who could spend it on alcohol (Anderson and Baland 2002). If these strategic motives were present, then the interpretation of our results would be different. To check whether this strategic behavior is driving our estimates, we split the sample according to the sex of the household head and re-estimate equation (1). Households with a female head are most of the times single-parent households so recipients from these households should not have incentives to strategically increase consumption. Table 6 presents the estimated coefficients of having cash-on-hand on food expenditures. In both female-headed and male-headed households we do find a jump in consumption when beneficiaries have cash-on-hand. This evidence suggests that the jump in consumption is not driven by strategic motives that lead cash recipients to spend the money on food before their husbands spend it on alcohol.

A major threat to our identification strategy is that the binary variable of interest may be capturing other factors 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 food consumption is high for a different reason than the transfer (e.g. holidays). This potential correlation between dates and unobservable variables that affect consumption would invalidate our strategy. As a robustness check, we conduct a placebo test using samples of non-beneficiaries that live in districts reached by Juntos. If our dummy variable is correlated with variables that affect consumption, it should also have an impact on the food expenditures of non-beneficiaries. Thus, we estimate equation (1) but only including non-beneficiaries in our sample. More specifically, we include poor households who do not receive cash transfers from Juntos but live in districts where the program operates. Table 7 reports the results of these estimations. All coefficients are very small and

insignificantly different from zero. This last piece of evidence suggests that our dummy variable of interest is not correlated with omitted variables that may affect consumption. Based on this falsification test, our identification strategy does not seem to be invalid.

6 Concluding Remarks

Conditional Cash Transfer programs give monetary incentives to poor people to send their children to school. These transfers attenuate borrowing constraints that limit food expenditures or investments in children's human capital. Indeed, a large body of evidence indicates that families who receive cash transfers are better than those who do not receive them: they consume more food and their kids are more likely to be enrolled in school. However, cash recipients are free to choose what to buy with the additional money. If we believe that poor households may spend money on goods they wish to consume less, cash transfers could increase the consumption of "temptation goods".

Rather than estimating the treatment effect of CCTs on consumption, our approach consists of looking at the consumption patterns of beneficiaries when they have the extra money on their hands. Using data on beneficiaries from a CCT program in Peru, we document two related changes in food consumption. We find that food expenditures go up when beneficiaries have the cash transfer on their hands, which is inconsistent with the predictions of the PIH model. Then, we show that this jump in consumption is driven by an increase in consumption of chocolates, candies, soft drinks and meals in restaurants. Moreover, the consumption of "nutritive food" remains unchanged, which suggests that borrowing constraints are not the main driver of our results. We also conduct a placebo test to rule out the idea that our findings are being driven by potential supply shocks related to pay dates.

Some caveats are worth mentioning. First, we rely on food expenditures to document the change in consumption but this measure may suffer from measurement error. Also, we are unable to check whether caloric intake declines when beneficiaries have cash-on-hand. Second, because we do not observe the exact day of payment, our variable of interest may capture some noise. If the measurement error is random, our estimates would be biased toward zero. Third, our data do not allow us to distinguish the relative importance of borrowing constraints and commitment problems. Fourth, the external validity of these results is not guaranteed.

With these caveats in mind, we think of our findings as only representing a first step toward a more rigorous analysis of the interaction between cash

transfers and commitment problems. Our results do not imply that the welfare of beneficiaries is lower because they consume more tasty food. For instance, having meals in restaurants may increase the subjective well-being of beneficiaries more than eating at home. Moreover, these results do not imply that the *level* of consumption of temptation goods is higher among cash beneficiaries compared to non-treated individuals.

If future empirical work confirms our results, policy makers should take into account that cash transfers may increase consumption of more tasty but perhaps less nutritive food. They should also evaluate the possibility of encouraging savings among beneficiaries of CCTs. Finally, more attention should be paid to the timing of the transfers (e.g. monthly versus bi-monthly) in the design of these programs.

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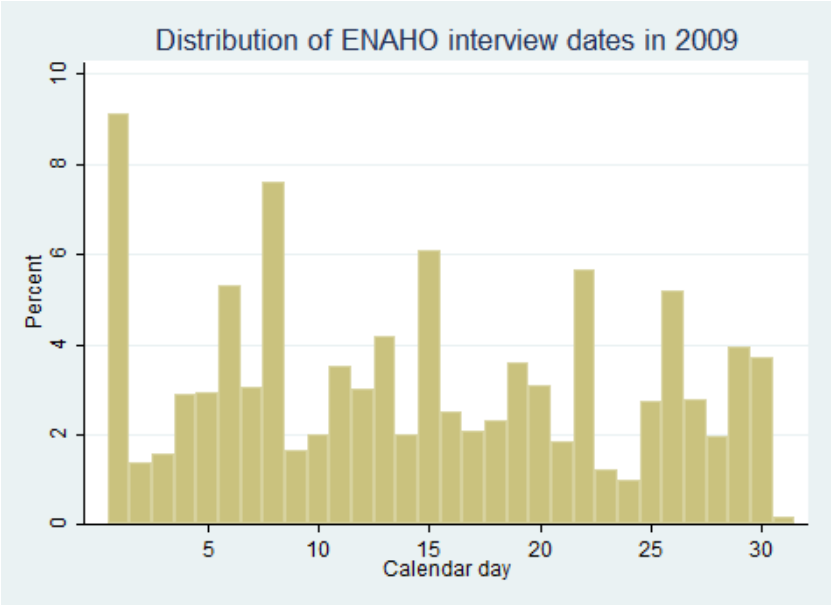
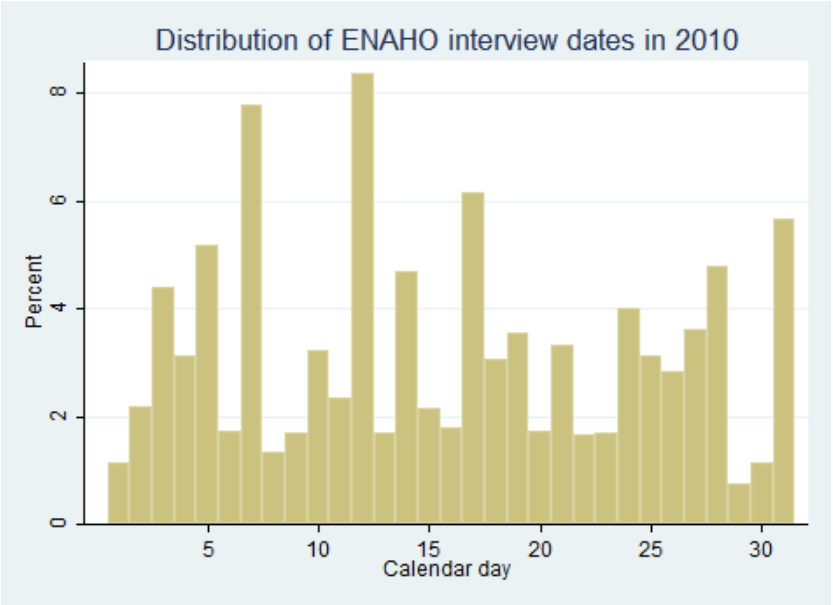
Figure 1: Distribution of ENAHO interview dates in 2009



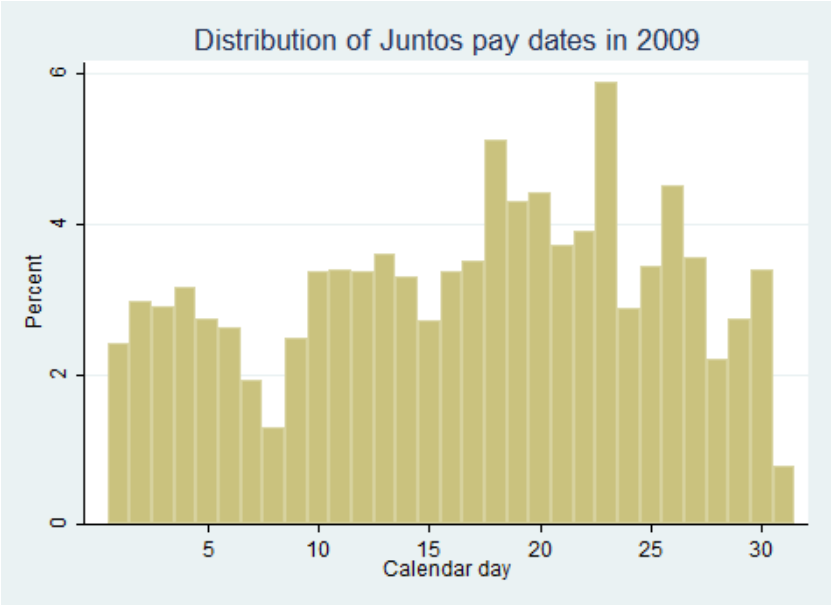
Source: ENAHO 2009

Figure 2: Distribution of ENAHO interview dates in 2010



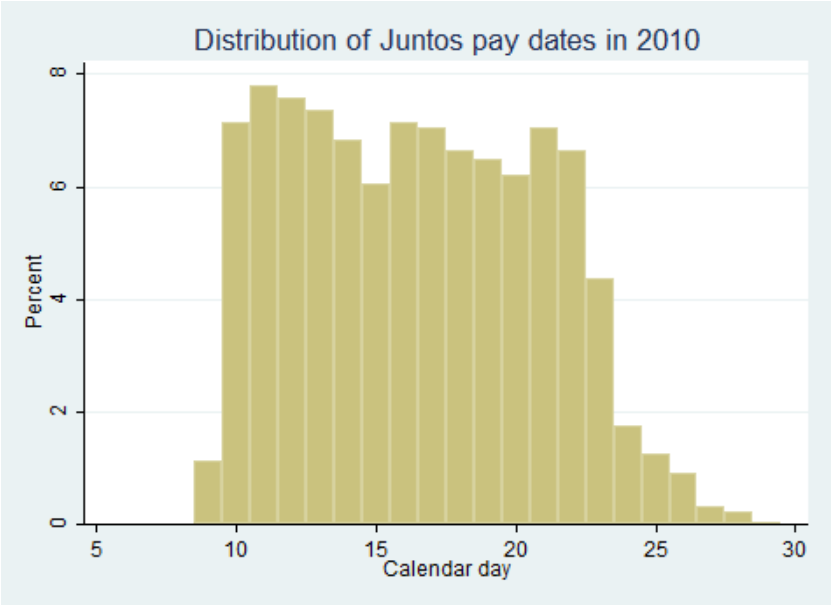
Source: ENAHO 2010

Figure 3: Distribution of Juntos pay dates in 2009



Source: Juntos payment schedule in 2009

Figure 4: Distribution of Juntos pay dates in 2010



Source: Juntos payment schedule in 2010

Table 1: Within-district Comparisons of Control and Treatment Groups. Year 2009

Variable	Control mean	Standard Error	Difference (T-C)	Standard Error
Head has primary education	0.52	(0.015)	-0.018	(0.034)
Head has secondary education	0.34	(0.014)	-0.012	(0.032)
Head has some college education	0.01	(0.005)	0.022*	(0.011)
Head's maternal language: Quechua	0.61	(0.013)	-0.050*	(0.029)
Head is female	0.50	(0.009)	0.004	(0.020)
Family size	3.57	(0.078)	0.214	(0.173)
Access to electricity	0.47	(0.029)	0.085	(0.065)
Access to water	0.29	(0.021)	0.033	(0.047)
Cooks with wood	0.87	(0.024)	-0.015	(0.054)
Wall is resistant	0.47	(0.016)	0.052	(0.036)
Roof is resistant	0.40	(0.021)	0.058	(0.047)
Observations	3,772			

Each row is a separate regression. The dependent variable appears on the first column. All regressions include an intercept (the control group mean), the treatment variable (1 if the household has cash-on-hand and 0 otherwise), and district fixed effects. The difference between control and treatment groups is the coefficient on the treatment variable. Standard errors are reported in parentheses.

* significant at the 10 percent level.

Table 2: Within-district Comparisons of Control and Treatment Groups. Year 2010

Variable	Control mean	Standard Error	Difference (T-C)	Standard Error
Head has primary education	0.48	(0.010)	0.016	(0.042)
Head has secondary education	0.34	(0.010)	-0.008	(0.040)
Head has some college education	0.02	(0.001)	-0.013*	(0.008)
Head's maternal language: Quechua	0.60	(0.008)	0.027	(0.035)
Head is female	0.13	(0.013)	-0.004	(0.055)
Family size	3.79	(0.076)	-0.113	(0.307)
Access to electricity	0.53	(0.017)	0.054	(0.072)
Access to water	0.37	(0.019)	-0.091	(0.079)
Cooks with wood	0.89	(0.013)	-0.017	(0.056)
Wall is resistant	0.55	(0.013)	-0.053	(0.055)
Roof is resistant	0.39	(0.012)	0.059	(0.050)
Observations	2,716			

Each row is a separate regression. The dependent variable appears on the first column. All regressions include an intercept (the control group mean), the treatment variable (1 if the household has cash-on-hand and 0 otherwise), and district fixed effects. The difference between control and treatment groups is the coefficient on the treatment variable. Standard errors are reported in parentheses.

* significant at the 10 percent level.

Table 3: Effects of having cash-on-hand on Food Expenditures

Dep. variable:	(log) Food Expenditures			
Year:	2009		2010	
	(1)	(2)	(3)	(4)
cash-on-hand	0.12***	0.10***	0.29***	0.21***
	(0.03)	(0.03)	(0.05)	(0.06)
Controls	No	Yes	No	Yes
Mean Food Expenditures	85.25		95.24	
Observations	3,751	3,751	2,678	2,678
R-squared	0.57	0.59	0.55	0.58

Note: Robust standard errors are shown in parentheses. All regressions include district and month fixed effects. Additional controls include: household head's characteristics (education, native language, sex), family size and house's characteristics (access to electricity, access to water, whether the food is cooked with wood, whether the wall is resistant, whether the roof is resistant, and whether is rural). Food expenditures are calculated for the reference period and expressed in *Nuevos Soles*.

*** significant at the 1 percent level.

Table 4: Effects of having cash-on-hand on Consumption of Temptation Goods

Dep. Variable:	Sweets	Alcohol	Soft Drinks	Restaurants
	(1)	(2)	(3)	(4)
Panel A: 2009				
cash-on-hand	0.017	-0.003	-0.027	0.016**
	(0.017)	(0.003)	(0.025)	(0.008)
Mean Dep. variable	0.12	0.004	0.39	0.022
Observations	3,772	3,772	3,572	3,772
R-squared	0.30	0.31	0.36	0.29
Panel B: 2010				
cash-on-hand	0.064**	0.007	0.064*	0.054***
	(0.030)	(0.006)	(0.037)	(0.012)
Mean Dep. variable	0.18	0.007	0.43	0.029
Observations	2,716	2,716	2,566	2,716
R-squared	0.35	0.31	0.38	0.40

Note: Robust standard errors are shown in parentheses. Each column is a separate regression. All regressions include district fixed effects and month fixed effects. Additional controls include: household head's characteristics (education, native language, sex), family size and house's characteristics (access to electricity, access to water, whether the food is cooked with wood, whether the wall is resistant, whether the roof is resistant, and whether is rural). Sweets include: candies and chocolates. Alcohol includes: whiskey, rum, *pisco* and beer. Restaurants include: roasted chicken, barbecue, and Chinese food.
* significant at the 10 percent level.
** significant at the 5 percent level.
*** significant at the 1 percent level.

Table 5: Effects of having cash-on-hand on Consumption of Nutritive Food

Dep. Variable:	Milk	Eggs	Legume	Fruits
	(1)	(2)	(3)	(4)
Panel A: 2009				
cash-on-hand	-0.03	-0.004	0.025	0.018
	(0.02)	(0.02)	(0.02)	(0.01)
Mean Dep. variable	0.30	0.28	0.31	0.72
Observations	3,772	3,772	3,767	3,772
R-squared	0.37	0.36	0.43	0.51
Panel B: 2010				
cash-on-hand	-0.027	0.029	0.046	0.036
	(0.03)	(0.03)	(0.03)	(0.03)
Mean Dep. variable	0.34	0.29	0.34	0.68
Observations	2,716	2,716	2,716	2,716
R-squared	0.33	0.38	0.42	0.49

Note: Robust standard errors are shown in parentheses. Each column is a separate regression. All regressions include district fixed effects and month fixed effects. Additional controls include: household head's characteristics (education, native language, sex), family size and house's characteristics (access to electricity, access to water, whether the food is cooked with wood, whether the wall is resistant, whether the roof is resistant, and whether is rural). Legume include: lentils, Canario bean, dried pea, broad bean, Lima bean. Fruits include: papaya, orange, mandarin, banana, apple, pineapple, grapes, and watermelon.

Table 6: Effects of having cash-on-hand on Food Expenditures by sex of the household head.

Dep. variable:	(log) Food Expenditures			
Year:	2009		2010	
Household Head is:	Female	Male	Female	Male
	(1)	(2)	(3)	(4)
cash-on-hand	0.09*	0.11**	0.27***	0.18**
	(0.054)	(0.056)	(0.093)	(0.084)
Observations	1,910	1,841	1,380	1,298
R-squared	0.59	0.61	0.59	0.59

Note: Robust standard errors are shown in parentheses. All regressions include district fixed effects and month fixed effects. Controls include: household head's characteristics (education, native language), family size and house's characteristics (access to electricity, access to water, whether the food is cooked with wood, whether the wall is resistant, whether the roof is resistant, and whether is rural).

* significant at the 10 percent level.

** significant at the 5 percent level.

*** significant at the 1 percent level.

Table 7: Effects of having "cash-on-hand" on Food expenditures. *Placebo* samples for 2009 and 2010.

Dep. variable:	(log) Food Expenditures			
Year:	2009		2010	
	(1)	(2)	(3)	(4)
"cash-on-hand"	0.01	-0.03	0.01	0.01
	(0.04)	(0.04)	(0.07)	(0.07)
Controls	No	Yes	No	Yes
Mean Food Expenditures	90.68		88.48	
Observations	4,353	4,353	2,256	2,256
R-squared	0.46	0.57	0.46	0.56

Note: Robust standard errors are shown in parentheses. All regressions include district fixed effects and month fixed effects. Controls include: household head's characteristics (education, native language, sex), family size and house's characteristics (access to electricity, access to water, whether the food is cooked with wood, whether the wall is resistant, whether the roof is resistant, and whether is rural). Food expenditures are calculated for the reference period and expressed in *Nuevos Soles*. In each year, the placebo sample includes non-treated households who live in treated districts.