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 exogenous variation in interview dates and pay dates, we find that food expenditures go up by 10-20 percent when beneficiaries have the cash transfer on their hands as opposed to when they do not have it. Additional evidence suggests that this increase is driven by higher consumption of candies, chocolates, soft drinks and meals in restaurants. Our findings are inconsistent with standard models of intertemporal choice such as the Permanent Income Hypothesis. However, 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 decade 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. However, cash recipients are free to choose how to spend their extra money.

The recent literature on commitment problems faced by the poor highlights that many households are tempted to over-spend on goods they wish to consume less (see Bryan, Karlan and Nelson, 2010 for a recent review). Thus, we wonder whether the interaction of cash transfers and commitment problems could increase the consumption of "temptation goods" among beneficiaries of "Juntos", a CCT program in Peru.

In particular, we analyze the consumption response to cash transfers among households enrolled in Juntos. Our approach differs from the previous literature which relies on comparisons between treated and non-treated households to estimate the impact of CCT on consumption. We do *not* attempt to estimate the average treatment effect of Juntos here. Instead, we are interested in examining whether food consumption jumps up when beneficiaries have the cash on their hands as oppose to when they do not have it. In other words, we compare beneficiaries who have the cash of the program on their hands to beneficiaries who do not.

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 variation across districts 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 document a change in the patterns of food consumption when beneficiaries have cash on their hands. First, we find that consumption jumps up by 10-20 percent during the days when benefit recipients have cash-on-hand. Second, our results suggest that this change in consumption is driven by increases in the consumption of 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 less nutritive. Moreover, the empirical evidence presented is inconsistent with the Permanent Income Hypothesis, which would predict no

change in consumption given that cash transfers are perfectly anticipated by beneficiaries. These findings represent our contribution to the literature.

The rest of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 describes the program. Section 4 gives the details of our data. Section 5 outlines the identification strategy. Section 6 displays our results. Section 7 offers concluding remarks (see the appendix for helpful definitions).

2 Literature Review

The early empirical literature that studied the consumption response to income transfers was motivated by the main theoretical prediction of the Permanent Income Hypothesis (henceforth, PIH): consumption should not react to anticipated income changes. We do not attempt to summarize this literature here but it should be noted that some 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) [1]. This empirical literature has 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.

More recently, in the literature of CCTs several authors have estimated the effects of cash transfers on consumption. Most of them exploit the random assignment of PROGRESA, a well-known CCT program in Mexico, and compare the consumption of treated households with that of non-treated families. Ruvalcaba, Teruel and Thomas (2004) find that the additional money from PROGRESA increases spending on child clothing, 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

[1] For recent surveys on this topic see Attanasio and Weber (2010) and Japelli an Pistaferri (2010).

fact that the money is given to the female head of the household[2]. Finally, Angelucci and Attanasio (2013) find that cash transfers to women in urban Mexico increase high-protein food consumption.

A couple of papers provide evidence from Juntos in Peru. 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. Unlike these papers, we do not rely on comparisons between treated and control households. We look directly at how beneficiaries change their consumption when they have the cash on their hands. Although we focus on a CCT program, our empirical approach is more closely related to two papers outside of this literature. We briefly summarize them in the next paragraphs.

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), 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 exogenous 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. That is, the food intake of people who reach retirement age with low savings declines over the month. 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.

In this study, we aim to provide evidence from a developing country by focusing on the consumption response to CCTs. Unlike Shapiro (2005) and Mastrobuoni and Weinberg (2009), we do not calibrate any structural model to estimate daily discount factors. We look at consumption of a set of goods which may play a key role in explaining the observed behaviors of the poor in terms of consumption and savings: "temptation goods". We follow the definition of Banerjee and Mul-

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

lainathan (2010): "temptation goods are defined to be the set of goods that generate positive utility for the self that consumes them, but not for any previous self that anticipates that they will be consumed in the future"[3]. For instance, Banerjee et al. (2013) show that access to microcredit reduces the consumption of meals outside the home, tobacco, and lottery tickets.

3 THE PROGRAM

In 2005, the Peruvian government launched a nation-wide CCT program, named Juntos. The program seeks to reduce current and future poverty through cash transfers and investments in children's human capital. Initially, Juntos was implemented in 70 districts and its budget was close to US$ 45 million. By the end of 2012, 650,000 families were direct beneficiaries in 1011 districts and the budget was more than US$ 360 million.

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 percent 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 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 they have been efforts to estimate the impact of Juntos on schooling,

[3]In order to clarify the concept of temptation goods we present another quotation from Banerjee and Mullainathan (2010): "In the core of our model of self-control is the assumption that there are two types of goods: goods that generate utility both when consumed but also before they are consumed (i.e. in anticipation) and goods which, to a much greater extent, generate utility only at the point of consumption. We may take great pleasure from smoking a cigarette today or eating a whole box of donuts, but knowing that we will consume them in the future even though they are bad for us does not make us happy (and indeed, may even serve to get us depressed)."

consumption and malnutrition (Perova and Vakis, 2011; 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 two months. 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. It should be noted that all beneficiaries are informed in advance about the exact pay dates. Now, we turn to describe our data.

4 DATA

Our primary data source is the ENAHO conducted by the Instituto Nacional de Estadística e Informática (INEI) in the years 2009 and 2010. The ENAHO is a nationwide representative survey that collects individual level information. We use information from the food expenditures record to construct our measure of consumption.

Two factors lead us to focus on food consumption. 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".

More specifically, we use two dependent variables: food expenditures and consumption of "temptation goods". Four sets of temptation goods are built: i) Sweets: candies and chocolates; ii) Alcohol: beer, whisky, rum, *pisco*[4], a very popular brandy among Peruvians; iii) Soft drinks, and iv) Restaurants: roasted chicken, Chinese food and barbecue.

Are these set of goods really tempting? Maybe only for poor individuals who usually struggle to consume all the calories they need. Our

[4]Wikipedia defines Pisco as: "...a colorless or yellowish-to-amber colored grape brandy produced in winemaking regions of Peru and Chile. Pisco was developed by Spanish settlers in the 16th century as an alternative to orujo, a pomace brandy that was being imported from Spain".

choice is motivated by the empirical results of Jensen and Miller (2011) who show 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[5].

To properly identify Juntos beneficiaries, we check whether the transfer conditions are consistently reproduced in each of the surveyed households. 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 two months). Around 98 percent of the cash recipients in our sample are women satisfying the mentioned conditions.

In 2009, the 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). Using ENAHO's sampling weights, Perova and Vakis (2011) find that the number of households who report receiving cash transfers from Juntos surveyed in the ENAHO is very close to the actual number of beneficiary households listed in the official records of Juntos.

Our secondary data source is the payment schedule of Juntos at the village level for the years 2009 and 2010 [6]. This data set includes the exact pay date (day/month/year) for all villages enrolled in the program during these years. 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, the 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. District identifiers are used to match the information of pay dates to

[5]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."

[6]Data on pay dates are publicly available in Juntos' website: http://www.juntos.gob.pe/cronograma_transportadora.php

the beneficiaries sample built up from the 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 the housing and education records of the ENAHO.

5 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 driven by an increase in the consumption of "temptation goods". We believe that answering these questions could be helpful for understanding the link between cash transfers and food consumption.

We exploit 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 (recall that the reference period is defined as the two weeks before the interview date). On the other hand, Juntos pay dates do not vary within a district but vary across districts. The combination of pay dates and interview dates generates that some households receive the cash transfer within the reference period while others receive it weeks later.

Thus, our identification strategy consists of comparing 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 consists of beneficiaries who are surveyed at most two weeks after they receive the cash transfer. In other words, it 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[7].

[7]Note that we compare beneficiaries so our treatment does not mean that some

We begin our empirical analysis by running the following regression:

$$\log c_{ij} = \alpha_j + \beta T_{ij} + X_{ij}^{'}\gamma + \mu_{ij} \tag{1}$$

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

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

$$z_{ij} = \lambda_j + \delta T_{ij} + X_{ij}^{'}\phi + \epsilon_{ij} \tag{2}$$

where z_{ij} is a binary variable which is equal to one if household i that lives in district j 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 (2013) 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 [8]. 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

households are enrolled in the program and others do not.

[8]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.

using a sample of non-beneficiaries who live in districts treated by Juntos. The details of this procedure would be discussed in Section 6.

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 firms 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 2013 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.

6 RESULTS

Table 1 reports summary statistics according to treatment status in our sample. We also report the p-value for the null hypothesis that the means are equal in the two groups. Table 1 shows that, on average, household heads have the same education level on control and treatment groups. For instance, in both groups 40 percent of the household head has primary education. In terms of native language, we see that 60 percent of the heads in our sample speak Quechua. Family size is on average less than 4 people in both groups but appears to be slightly larger in control households. Access to electricity and water (inside the home) is also more prevalent in the control group than in the treatment group. Regarding the safety/quality of the house, control households are more likely to have resistant walls and roof. Although there are significant differences in these observable characteristics, we do not see them large enough to introduce large biases. In all regressions we control for these variables and also include district and month fixed effects to reduce these concerns. In any case, the control groups seems to be better off than the treatment group. Thus, if consumption is higher in better off families, our estimates will be downward biased.

In Table 2, we present the estimates of the effects of receiving the cash transfer within the reference period (which is defined as the two weeks before the interview date, see the Appendix). 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 goes 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 (2) and (4). These coefficients suggest that the increase in food expenditures is equivalent to 89 percent and 70 percent of the cash transfer on 2009 and 2010, respectively[9]. It is also worthy to note that once we include the controls in the regressions, the point estimates go down, which make sense according to what we see in Table 1.

We should bear in mind that increases in food expenditures do not necessarily lead to higher consumption: cash recipients may just buy food for the future and stock it in the house. If consumption tracks expenditure, these results are inconsistent with the PIH: consumption does react to anticipated cash transfers. If consumption does not track expenditure, then our results do not provide evidence against the PIH. Thus, if we only look at these results, we cannot claim that consumption goes up when beneficiaries have the cash on their hands.

Table 3 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 consumption of temptation goods because their baseline levels are pretty low. We do not see any increase in the consumption of alcohol. This may be driven by the fact that women usually spend less on alcohol than men (recall that women receive the cash transfer). Finally, given that eating at a restaurant necessarily increases consumption, we now feel more confident to say that consumption goes up when beneficiaries have the cash on their hands.

In Table 4, 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

[9]These calculations are obtained when we regress food expenditures (in levels) on the treatment variable and the rest of controls.

nutritive foods. In all specifications, we cannot reject the null hypothesis that each coefficient is equal to zero. That is, even though some point estimate are positive, they are very small and statistically insignificant. These results reinforce the idea that having cash on hand induces consumers towards 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 go up suggests that borrowing constraints are not the main driver of our results.

What can explain these results? A natural explanation for this jump in consumption would be borrowing constraints: poor people have to wait to the pay date to make their food purchases. We do believe that borrowing constraints play a *major* 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 enrolment 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 (2013) find that beneficiaries from Juntos 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).

However, our findings differ from the empirical evidence provided by Banerjee, Duflo, Glennerster and Kinnan (2013). Using random variation in the assignment of micro-credit, they show that micro-credits decrease the consumption of temptation goods among beneficiaries. Their explanation is that micro-credit acts as a commitment device that help poor families to cut unnecessary spending. We agree with their story but think that cash transfers do not serve as a commitment device because beneficiary households do not have to repay the cash they receive.

One may think that cash recipients (recall they are women) increase

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

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 5 shows 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 the 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 6 reports the results of these estimations. All coefficients are very small and insignificantly different from zero. This 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.

7 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 human capital of children. Indeed, a large body of evidence indicates that fami-

lies 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 pattern of beneficiaries when they have the extra money on their hands. Using data on beneficiaries from a CCT program of 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. Then, we show that this jump in consumption is driven by an increase in consumption of chocolates, candies, soft drinks and meals in restaurants. Consumption of alcohol does not go up probably because cash transfers are given to women instead of men.

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. But if the measurement error is correlated with unobservable characteristics that affect food consumption our results would be inconsistent. 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. Moreover, 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.

If future empirical work confirms our results, policy makers should take into account that cash transfers may increase consumption of more tasty but less nutritive food. They should also evaluate the possibility of encouraging savings among beneficiaries of CCTs. The timing of the transfers would also deserve some attention in the design of

these programs.

8 APPENDIX

- CCT: Conditional Cash Transfers
- Juntos: CCT program in Peru
- ENAHO: Peruvian National Household Survey (Encuesta Nacional de Hogares)
- PIH: Permanent Income Hypothesis
- Reference period: two weeks before the ENAHO interview date
- Temptation goods: set of goods that generate positive utility for the self that consumes them, but not for any previous self that anticipates that they will be consumed in the future. In our data, these goods include the following: i) Sweets: candies and chocolates; ii) Alcohol: beer, whisky, rum, *pisco*; iii) Soft drinks, and iv) Restaurants: roasted chicken, Chinese food and barbecue.
- Treatment group: includes households who have the cash transfer on their hands in the reference period
- Control group: includes households who do not have the cash transfer on their hands in the reference period
- Placebo sample: includes poor households who do not receive cash transfers from Juntos but live in districts where the program operates.

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Table 1: Summary Statistics

Variable	Control	Treated	Difference	P-Value
Head has complete primary education	0.409	0.437	-0.028	0.160
	(0.491)	(0.496)		
Head has complete secondary education	0.339	0.337	0.002	0.450
	(0.473)	(0.473)		
Head has (some) college	0.021	0.015	0.006	0.069
	(0.144)	(0.125)		
Head's native language: Quechua	0.607	0.592	0.015	0.118
	(0.488)	(0.491)		
Head is female	0.116	0.121	-0.005	0.263
	(0.005)	(0.007)		
Family size	3.734	3.661	0.073	0.028
	(1.522)	(1.427)		
Access to electricity	0.523	0.541	-0.018	0.077
	(0.499)	(0.498)		
Access to water (inside home)	0.348	0.283	0.065	0.000
	(0.476)	(0.450)		
Cooks with wood	0.880	0.877	0.003	0.394
	(0.324)	(0.327)		
Walls are resistant	0.533	0.482	0.051	0.000
	(0.499)	(0.498)		
Roof is resistant	0.409	0.437	-0.028	0.013
	(0.491)	(0.496)		
Observations	4105	2383		

Note: Standard deviations are reported in parentheses

Table 2: Effects of having cash-on-hand on Food Expenditures

Dep. variable:	(log) Food Expenditures			
Year:	2009		2010	
	(1)	(2)	(3)	(4)
cash-on-hand	0.1264***	0.1028***	0.2910***	0.2149***
	(0.0359)	(0.0374)	(0.0562)	(0.0603)
Controls	No	Yes	No	Yes
District Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	3,751	3,751	2,678	2,678
R-squared	0.5755	0.5972	0.5556	0.5830

Note: Robust standard errors are shown in parentheses. 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).

Table 3: Effects of having cash-on-hand on Consumption of Temptation Goods

Dep. Variable:	(1) Sweets	(2) Alcohol	(3) Soft Drinks	(4) Restaurants
Panel A: 2009				
cash-on-hand	0.0173	-0.0036	-0.0278	0.0157**
	(0.0176)	(0.0034)	(0.0257)	(0.0080)
Observations	3,772	3,772	3,572	3,772
R-squared	0.3083	0.3135	0.3697	0.2992
Panel B: 2010				
cash-on-hand	0.0648**	0.0073	0.0641*	0.0549***
	(0.0302)	(0.0069)	(0.0379)	(0.0129)
Observations	2,716	2,716	2,566	2,716
R-squared	0.3513	0.3143	0.3829	0.4064

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: whisky, rum, *pisco* and beer. Restaurants include: roasted chicken, barbecue, and Chinese food.

Table 4: Effects of having cash-on-hand on Consumption of Nutritive Food

Dep. Variable:	(1) Milk	(2) Eggs	(3) Legume	(4) Fruits
Panel A: 2009				
cash-on-hand	-0.0331	-0.0049	0.0250	0.0185
	(0.0233)	(0.0230)	(0.0224)	(0.0198)
Observations	3,772	3,772	3,767	3,772
R-squared	0.3782	0.3668	0.4321	0.5186
Panel B: 2010				
cash-on-hand	-0.0276	0.0294	0.0467	0.0367
	(0.0371)	(0.0346)	(0.0349)	(0.0312)
Observations	2,716	2,716	2,716	2,716
R-squared	0.3395	0.3814	0.4233	0.4918

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 5: 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
cash-on-hand	0.0963*	0.1172**	0.2766***	0.1899**
	(0.0547)	(0.0561)	(0.0933)	(0.0846)
Observations	1,910	1,841	1,380	1,298
R-squared	0.5960	0.6195	0.5902	0.5994

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).

Table 6: 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	0.0191	-0.0312	0.0147	0.0118
	(0.0458)	(0.0415)	(0.0787)	(0.0717)
Controls	No	Yes	No	Yes
District Fixed Effects	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,353	4,353	2,256	2,256
R-squared	0.4665	0.5700	0.4658	0.5659

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). In each year, the sample includes non-treated households who live in treated districts.