Does the benefits schedule of cash assistance programs affect the purchase of temptation goods?

Evidence from a natural experiment in Peru*

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Abstract

A critique of cash assistance programs is that beneficiaries may spend the money on "temptation goods" such as alcohol and tobacco. We exploit a change in the payment schedule of Peru's conditional cash transfer program to identify the impact of benefit receipt frequency on the purchase of temptation goods. We use annual household data among cross-sectional and panel samples to analyze the effect of the policy change on the share of the household budget devoted to six categories of temptation goods. Using a difference-in-differences estimation approach, we find that larger, less frequent payments increased the expenditure share of alcohol by 55-80% and sweets by 10-40%. Our study suggests that less frequent benefits scheduling may lead cash recipients to make certain types of temptation purchases.

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1 Introduction

A common critique of cash assistance programs is that beneficiaries may squander the money or use it in ways that reduce their welfare. A particular source of concern is that husbands will wrest the money from their wives and use it to feed their own vices, such as alcohol and tobacco (John, 2008; Wang, Sindelar and Busch, 2006). This concern has prompted some programs to give transfers preferentially to a female head of the household, who are thought to be more likely to invest in their children's human capital (Lundberg, Pollak and Wales, 1997). Behavioral economists have noted that, in addition to intra-household bargaining between spouses, cash transfers can spur *intra-personal* bargaining conflicts. Many individuals experience a short-run impatience that leads a present self to neglect the long-run consumption plans of past selves and the consequences of impulsive consumption for future selves (Laibson, 1997; O'Donoghue and Rabin, 1999). As a result, present-biased individuals are tempted to spend income on goods that benefit the present consumer but not his future incarnations. Banerjee and Mullainathan (2010) refer to these purchases as "temptation goods."

In this study, we consider whether the timing of income receipt promotes the purchase of temptation goods among beneficiaries of a conditional cash transfer (CCT) program in Peru. We narrow our focus to temptation goods for several reasons. First, as cash assistance programs have proliferated in developing countries, researchers and policymakers have begun to understand their impacts on the health and welfare of recipients. The consumption of temptation goods represents an unintended consequence that has rarely been incorporated into evaluations of program effectiveness, despite temptation spending being indicative of a wasteful and potentially welfare-reducing use of program funds. Second, the health and economic impacts of temptation purchasing likely fall disproportionately on low-income populations. Low-income groups face a long list of complex and competing demands for their mental resources. As a result, they may have limited cognitive "bandwidth" available to devote to willpower (Mullainathan and Shafir, 2013). Several studies find that cognitive performance decreases when a person is mentally taxed (Mani et al., 2013; Spears, 2011), and low-income families are most likely to face this mental strain. Third, from a theoretical standpoint, temptation purchasing in response to monthly transfers is inconsistent with the permanent income hypothesis, which states that current consumption should not respond to predictable sources of income. The existence of temptation purchasing would pose a challenge to standard economic theory that has implications for how economists model consumer choice.

Much of the evidence on temptation purchasing comes from payday or "first-of-the-month" effects. In many contexts, the timing of household purchasing behavior is sensitive to the timing of income receipt, often displaying signs of a regular cycle. Recipients tend to make larger or more frequent discretionary purchases around the time of receipt of a regular income stream. Researchers have documented this pattern among Social Security recipients and vehicle loan recipients in the U.S., paycheck recipients in the U.K, and pensioners in Japan (Stephens, 2003, 2006, 2008; Stephens and Unayama, 2011). In addition, several studies have found a monthly consumption cycle for recipients of food assistance in the U.S. (Wilde and Ranney, 2000; Shapiro, 2005; Hastings and Washington, 2010). These consumption cycles are highly suggestive that individuals have a short-run

impatience, or present bias (Huffman and Barenstein, 2005).[1] Patterns of cycling may have particularly serious consequences for low-income households, for example, increasing their risk of health problems as a result of food shortfalls at month's end (Seligman et al., 2014).

While research has pointed to temptation purchasing in high-income countries, the evidence in low-and middle-income countries tends to downplay its importance. Evans and Popova (2014) conduct a systematic review of the effects of cash transfer programs in low- and middle-income countries on alcohol and tobacco consumption. They identify 19 studies drawn largely from unpublished material, including eight randomized controlled trials. All but two show a negative or null effect of transfers on alcohol and tobacco consumption. The authors suggest several factors that may offset the income effect of transfers on temptation purchasing: cash transfers may induce a substitution effect that increases the value of health and schooling among recipients; social messaging from programs may lead to mental labeling of cash transfers for health and schooling; and money is often targeted to women who are be less likely to use alcohol and tobacco.

The findings from Evans and Popova (2014) appear to be robust to different measures of consumption, different estimation strategies, and different countries, although the existing literature does have certain limitations. Several studies suffer from weak methods, for example, being under-powered to detect an effect or using a pre-post design.[2] Several studies focus solely on consumption by children and adolescents, who are not the principal recipients of the transfers nor the primary consumers of temptation goods. As such, they may have limited scope to respond behaviorally to the transfers. At least one study measures outcomes using indicator variables for whether respondents consumed any temptation goods. We hypothesize that cash transfers are more likely to operate on the extensive margin for adults, whose consumption habits are well established, for example, making them more likely to purchase an extra pack of cigarettes than to initiate a smoking habit. Finally, the demand for temptation goods may be manifested through the consumption of goods aside from tobacco or alcohol, such as sweets, that have been far less studied.

In this study, we exploit a change in the payment schedule of Peru's CCT program to identify the impact of benefit receipt frequency on the purchasing practices of member households. Starting in January 2010, the payment schedule in the Juntos CCT program in Peru changed from once a month to once every two months. The total annual payment did not change. We hypothesize that larger, less frequent payments lead households to make more temptation purchases. The policy puts more money in the hands of households at one time, which may trigger two behavioral mechanisms that contribute to the purchase of temptation goods. First, present bias may make recipients who are flush with cash more likely to splurge on temptation goods, a conclusion supported by the literature on payday effects. Second, households are more likely to be in a state of heightened arousal at the time they receive the transfer, and consumers in a viscerally aroused state are more likely to over-estimate their preferences for consuming temptation goods. This tendency is reflected in the old adage never to shop on an empty stomach for fear of consuming more than needed. Behavioral

[1] Consumption cycles could be consistent with a rational choice model if prices fluctuate cyclically with demand, although at least one study has ruled out this possibility as driver of cyclical consumption patterns (Hastings and Washington, 2010).

[2] One study does not deal with alcohol or tobacco expenditures at all, and appears to have been inadvertently included in the systematic review (Bazzi, Sumarto and Suryahadi, 2012).

economists refer to the tendency to project one's current state onto one's predictions for the future as "projection bias" (Loewenstein, O'Donoghue and Rabin, 2003). Projection bias may lead hungry consumers to "over-consume" unhealthy goods and consumers in a state of craving to "over-consume" alcohol or tobacco (Read and van Leeuwen, 1998; Badger et al., 2007). Consumers are most likely to find themselves in these visceral states at the time that they receive the transfer.

We determine the impact of the payment schedule change using a difference-in-differences estimation strategy, before and after the policy change for Juntos recipient and non-recipient households. The control group consists of households in comparable low-income districts where Juntos was not available. Using household data from 2007 to 2012, we analyze the impact of the payment schedule change on the share of the household budget devoted to six categories of temptation expenditures: alcohol, tobacco, sweets and sugary foods, soft drinks, commercially prepared meals consumed at home, and food purchases at restaurants or markets. We derive a series of demand equations using a Quadratic Almost Ideal Demand System to study the impact of benefits scheduling on temptation purchasing. We test for temptation purchasing in a repeated cross-section of households and a panel. We include area-level fixed effects in the repeated cross-sectional analysis and household fixed effects in the panel analysis. Thus, in the panel sample, we identify the policy impact by analyzing the purchasing behavior of the same households over time before and after the policy change.

Two studies have addressed temptation purchasing among beneficiaries in the Juntos CCT program in Peru. Dasso and Fernandez (2013) use quasi-random variation in the payment dates for districts and survey interview dates of respondents in order to isolate the effect of having "cash in hand." They find that households who recently received a Juntos payment have higher consumption of sweets, soft drinks, and meals in restaurants, each measured as an indicator for any consumption. Consumption of alcohol did not change for those who had cash in hand. Interestingly, the effects on temptation purchasing are concentrated in 2010, the year immediately after the policy under study here went into effect.[3] As part of a broader evaluation of the Juntos program, Perova (2010) examines alcohol consumption among recipient households and a set of control households. Using a difference-in-differences estimator, she finds that Juntos decreased expenditures on alcoholic beverages by 0.15 Peruvian nuevos soles per month. Using an instrumental variables approach that accounts for selection into the program, the sign on the alcohol coefficient flips; Juntos increased expenditures on alcohol by 0.28 soles per month, a small but statistically significant amount. Our study builds on this literature by testing for temptation purchasing using a natural experiment and testing how a CCT program's design can encourage or discourage temptation purchasing.

We find that larger, less frequent payments increased the share of expenditures spent on alcohol and sweets and perhaps tobacco, but not those spent on the other four categories of temptation goods. The less frequent payment system increased alcohol expenditures about 55-80% and expenditures on sweets about 10-40%. We find evidence that the effect of the scheduling change is concentrated almost exclusively on the intensive margin. Our study

[3] We use the same data sources as Dasso and Fernandez (2013), although we have not been able to replicate their study sample. We contacted the authors but did not hear back. Consequently, we are not able to resolve this difference.

highlights the importance of benefits scheduling for the purchasing behavior of social welfare recipients. Policymakers may be able to curtail the degree to which public program recipients use their benefits on temptation goods simply by distributing payments more frequently over time.

2 Background

From 1980 to 2000, the Peruvian countryside was ravaged by a guerrilla war between Maoist insurgents and a government counterinsurgency. In the wake of the political violence, the Peruvian government searched for ways to build national solidarity and to assist affected areas. In 2005, Peru established the Juntos ("Together") program, inspired by and modeled after successful CCT programs in countries like Mexico, Brazil, and Colombia. A number of impact evaluations have found that CCT programs, which condition the receipt of cash on meeting specified criteria, lead to large improvements in the health, economic, and educational outcomes of beneficiaries (Ranganathan and Lagarde, 2012; Fiszbein and Schady, 2009; Baird et al., 2013). The objectives of Peru's program were twofold: poverty alleviation in the short run by providing cash to households and disruption of intergenerational poverty in the long run by developing human capital via improved access to schooling and health services (Perova and Vakis, 2009). Program enrollment has grown steadily from about 2,000 villages (70 districts) in 2005 to 28,000 villages (646 districts) in 2010 to more than 37,000 villages (1,083 districts) in 2013.

Cash assistance in Juntos, as with other CCTs, is tied to several conditionalities, including school attendance, infant vaccination, well-child visits, nutrition supplementation for infants, prenatal and postnatal care, and parental education about nutrition, health, and hygiene at health clinics. Program eligibility is determined in three stages: the selection of eligible districts, the selection of eligible households within eligible districts, and community-level validation of the beneficiary list (Perova and Vakis, 2009). Districts are selected based on exposure to violence during the guerrilla war, the amount of poverty and extreme poverty, the poverty gap, or average income shortfall relative to the poverty line, and the amount of child malnutrition (Perova and Vakis, 2009). Eligible households have a child under the age of 14 or a pregnant woman and are selected based on a proxy means test. Community members, local authorities, and officials from the Ministry of Education and Health then validate the households selected for inclusion and exclusion.

Juntos provides a fixed, lump-sum payment to eligible households that does not vary by household size or number of children. As a fraction of average household expenditures, Juntos is less generous than most other CCT programs in Latin America, with the exception of the programs in Honduras and Bolivia (Perova and Vakis, 2012). The Juntos transfer is equivalent to 10-15% of average monthly household expenditures, compared to 25% in Mexico and 30% in Colombia (Fernandez and Saldarriaga, 2013; Perova and Vakis, 2012). Through December 2009, Juntos recipient households received 100 soles per month. Starting in January 2010, the government moved from a monthly to a bimonthly payment schedule, such that Juntos households received 200 soles every two months. There are three payment mechanisms assigned by district: at a local branch of Banco de Nación, by armored van delivery to the village or district center, and at the offices of correspondent banks of Banco

de Nación. In 2010, 55.4% of beneficiaries picked up a check at Banco de Nación, 42.6% received their payment at the village or district center, and 2.0% at a correspondent bank of Banco de Nación. When the program moved to bimonthly payments, it substantially reduced the program's operating costs for payment disbursement and delivery.

Consumption of temptation-related behaviors in Peru is typical for a Latin American country. In 2010, the prevalence of tobacco use in Peru was about 18% for men and 5% for women (Ng et al., 2014), slightly higher than the regional average. In 2005, adult per-capita consumption of alcohol was about 7 liters, compared to 9 liters on average in the Americas (World Health Organization, 2011). In 2011, Peruvians consumed 56 liters of soft drinks per capita, slightly below its neighbors (e.g., 65 liters in Bolivia and Ecuador and 68 liters in Colombia), according to Euromonitor data.

3 Data

We use a difference-in-differences estimation strategy to determine the impact of the change in the CCT payment schedule on the budget share of certain categories of temptation goods. The policy change took effect on January 1, 2010. A challenge for the analysis is identifying a valid control group, as a key assumption of difference-in-differences models is that the average pre-treatment time trend is the same for the treatment and control groups. We constructed the control group from low-income households in districts that are not eligible for the CCT program.[4] In order to improve comparability, we estimated a propensity score and trimmed the sample to ensure common support of all covariates between the treatment and control groups, thereby meeting the so-called overlap condition. This condition ensures that treatment observations have comparison observations "nearby" in the propensity score distribution and is an important precondition for estimating causal effects (Heckman, Ichimura and Todd, 1997; Imbens, 2014). The estimated propensity score included the full set of covariates described below. We further restrict the sample to the highlands and rainforest regions of Peru where the Juntos program has been targeted,[5] districts that have median net income below 30,000 soles, and households that have net income below 45,000 soles. The latter two cutoffs are based on the income distributions for Juntos households and districts.

We focus our attention on six expenditure categories of temptation goods. These categories are: alcohol, tobacco, sweets, soft drinks, commercially prepared foods consumed at home, and food purchased outside of the home.[6] The latter two categories adopt an expansive definition of temptation goods, such that some purchases likely qualify as temptation purchases while others do not. As the policy's impacts from these expenditure categories also have relevance for our hypothesized mechanism, we include them in the analyses. Alcohol expenditures includes spending on all alcoholic products, including

[4] The Peruvian government originally considered 638 districts for the CCT program. We are working to access this list as an alternate basis for the control group, which we hope to include in a future version of this paper.

[5] This procedure drops all households from Lima and the coastal regions.

[6] Expenditures on food purchased outside of the home excludes food consumed at church, school, or other people's homes.

whisky, rum, *pisco* (a Peruvian brandy), beer, and wine. Tobacco expenditures includes spending on all tobacco products. Expenditures on sweets includes pastries, candies, and chocolates. Expenditures on soft drinks includes spending on all carbonated beverages, aside from mineral water. Expenditures on prepared foods includes spending on foods prepared by a commercial establishment but consumed at home, such as tamales or hamburgers. Expenditures on foods consumed outside of the home includes spending on food prepared by a commercial entity and purchased at a restaurant or market.

Our main dependent variable is the expenditure share by category. As alternate outcome measures, we consider an indicator of whether the household purchased any goods from a given category and the expenditure share conditional on any purchases within the category. These two outcomes allow us to differentiate between the policy's impacts on the intensive and extensive margins. It also takes into account the large number of "corner" solutions (i.e., zero expenditures) for many of our categories, including alcohol, tobacco, and beverages. We assume that consumers have weakly separable preferences across expenditure categories and time periods.

3.1 Encuesta Nacional de Hogares

This study uses data from Peru's Encuesta Nacional de Hogares (ENAHO), an annual survey of individuals and households collected by the National Institute of Statistics (Instituto Nacional de Estadística e Informática, or INEI). Geocoded microdata from in-person interviews are publicly available at the district level from 1997 onward.[7] The study has a probabilistic, stratified, multi-stage sampling design within each department. In urban areas, the primary sampling unit is an urban population center with 2,000 or more residents, and the secondary sampling unit is a cluster that has an average of 120 private residences. In rural areas, the primary sampling unit is an urban population center with 500 to 2,000 residents or a Rural Registration Area (AER, using the acronym in Spanish) that has on average 100 residences, and the secondary sampling unit in urban population centers is a cluster that has an average of 120 private residences. The study is designed for a level of inference at the region $\times$ urbanicity level.

We use the cross-sectional and panel samples of households in ENAHO. We use the repeated cross-sectional data collected from 2007 through 2012. Prior to 2007, the survey identified Juntos recipients only if the household responded in the affirmative to a general screening question about whether it had received government assistance, which may lead to systematic under-reporting. The panel sample covers the period from 2007 through 2011, although most households are replaced after two waves because the survey follows the place of residence rather than the household residing there. Juntos participation is determined in ENAHO by a question asking, "In the last 6 months, did you receive any public or private transfers, for example, Juntos program transfers?" The survey included information from 261 of the 638 districts (40%) enrolled in Juntos in 2009 and 159 of the 646 districts (25%) in 2010 (Dasso and Fernandez, 2013). Perova and Vakis (2012) compare administrative data from Juntos and survey-weighted responses about Juntos participation in ENAHO and find

[7] Peru has about 1,800 districts. Each district has at least 3,500 residents if located in the rainforest, 4,000 in the Andes highlands, and 10,000 in the coastal area.

that the percentage difference is 8% in 2008 and 1% in 2009.

ENAHO includes questions on household consumption of roughly 200 food and beverage items during the prior 15 days. Tobacco consumption is based on the prior 30 days. The food and beverage data are reported by the head of the household or the head's spouse. Specific questions include: whether anyone in the household obtained, consumed, purchased, or received the item; how the item was obtained (purchase, self-supply, donation, etc.); if bought, how often it was bought or obtained; in what quantity it was bought; where it was bought; and how much was the total amount of the purchase. For all expenditure categories, we restrict analysis to items that households purchased. All expenditures are annualized and deflated to 2009 terms using the consumer price index constructed for ENAHO.

3.2 Descriptive Statistics

Table 1 shows the descriptive characteristics among sample households. In our cross-sectional sample, the control group consisting of households from non-Juntos districts includes 20,529 observations, and the treatment group consisting of Juntos recipient households includes 9,717 households. The comparable numbers for the panel sample is about one-third of the size, 6,032 observations in the control group and 3,014 observations in the treatment group. Although we constructed the control group based on the characteristics of the treatment group, there are still significant differences between the groups. For example, 9% of households in the treatment group reside in the rainforest, compared to 40% in the control group drawn from non-Juntos districts.

The crucial assumption in a difference-in-differences estimator is that the outcome variables would have grown at the same rate between baseline and follow-up in both treatment and control groups had the policy change not taken place. This common trends assumption is not directly testable, but we observe pre-treatment behavior to give an indication of whether the assumption is likely to hold. Figure 1 shows the share of expenditures by temptation category and month for Juntos recipient households and the control group of households from non-Juntos districts. We make use of the fact that ENAHO quasi-randomly assigns districts to an interview month, in order to construct month-by-month means. We see that the outcome variables follow a roughly similar trajectory for Juntos recipient households and the control group of households from non-Juntos districts. In Figure A1, we show that the by-group trends in total expenditures follow an even more similar trend in pre-policy period. We formally test the trend difference during the pre-treatment period by interacting our treatment indicator with year dummies in the period before the policy change. We find that the trends are not significantly different for all outcome variables, except for food consumed outside of the home (Appendix Table 2). This gives us a measure of confidence that the common trends assumption holds for five of the six outcomes used in the analyses of the cross-sectional sample. Figure 1 also indicates that the share of expenditures varies greatly by category, but in all cases the share is relatively modest. While food consumed outside of the home constitutes 10-20% of total expenditures on average, tobacco constitutes less than 0.2%.

Another important assumption that we make in the analyses using the cross-sectional sample is that temporal changes in the composition of the sample do not vary between the treatment and control groups. To measure compositional changes, we compare pre-post

characteristics of the treatment and control groups. The results indicate that the changes in sample composition are small (Appendix Table A1). All but one of the control variables has a change over time of less than 0.2 standard deviations, implying that the samples are relatively well balanced over time.

4 Results

4.1 Policy Effects on Temptation Expenditures

In this section, we describe the impact of a change in the frequency of CCT payments on the share of expenditures spent on six categories of temptation goods. We conduct difference-in-differences analyses of changes in temptation expenditure shares before and after the policy change for Juntos recipient households and control households from non-Juntos districts. We hypothesize that larger, less frequent payments will increase the expenditure shares spent on the temptation goods. We use a model of consumer demand to determine the change in consumer expenditure patterns that result from implementation of the Juntos benefits scheduling policy. To do so, we follow the literature on demand systems that specifies expenditure shares as the dependent variable (Deaton, 1986). Let the expenditure share for the j^{th} good in household i and year t be:

$$w_{ijt} = \frac{p_{ijt}q_{ijt}}{X_{it}},$$

where $p_{ijt}q_{ijt}$ are the household's expenditures on good j and X_{it} are total expenditures.

We derive a series of Engel curves from a Quadratic Almost Ideal Demand System (QUAIDS) that is quadratic in the logarithm of total expenditures (Banks, Blundell and Lewbel, 1997). QUAIDS is a generalization of the Almost Ideal Demand System developed by Deaton and Muellbauer (1980) to give a first-order approximation of household expenditures. The model allows for aggregation across consumers and is consistent with the axioms of utility maximization under consumer theory. QUAIDS more flexibly models the relationship between total household expenditures and the shares of expenditures on certain categories of goods.

We run models with a repeated cross-sectional sample and a panel sample. Using the repeated cross-sectional sample, our main equation takes the form:

$$w_{ijt} = \alpha + \beta(T_{it} \times Post_t) + \gamma_1 T_{it} + \delta_1 \log X_{it} + \delta_2 (\log X_{it})^2 + \mathbf{Z}_{it}\zeta + \theta_t + \epsilon_{ijt} \qquad (1)$$

where T_{it} is the treatment indicator for household participation in the CCT program; $Post_t$ is an indicator for post-policy implementation, turned on from 2010 through 2012; $\mathbf{Z_{it}}$ is a vector of household- and area-level characteristics; θ_t are year fixed effects; and ϵ_{ijt} is a random error term. This formulation is similar to Deaton, Ruiz-Castillo and Thomas (1989) and adopts the standard convention that prices are the same for all households within each cluster. Our panel analyses are robust to violations of this assumption.

In the cross-sectional models, we control for a number of household and area-level covariates ($\mathbf{Z_{it}}$). These help to account for compositional differences between the treatment

and control groups. Covariates include household size, the percentage of household members who are adults (18 years or older), the age, sex, education (none, at least some primary, at least some secondary, or at least some tertiary), and native language (Spanish, Quechua, other) of the household head, the community type as measured by the community's population size and urbanicity (< 140 rural, 140-400 rural, < 401 urban, 401-4,000 urban), and the region of Peru (north highlands, central highlands, south highlands, and rainforest).[8]

In the models with the panel sample, we estimate regressions of the following form:

$$w_{ijt} = \alpha + \beta(T_{it} \times Post_t) + \gamma_1 T_{it} + \delta_1 \ln X_{it} + \delta_2 (\ln X_{it})^2 + \theta_t + \mu_i + \epsilon_{ijt} \tag{2}$$

where μ_i are household fixed effects. In these models, our estimation strategy uses within-household variation in temptation expenditure shares before and after the policy change, providing a better estimate of the changes due to the policy as compared to the cross-sectional models. This refinement comes at a cost, as the panel sample ($N = 9,046$) is much smaller than the cross-sectional sample ($N = 30,246$) and thus leads to less precisely estimated effects. In the panel models, the household fixed effects absorb all time-invariant household- and community-level characteristics. In all cross-sectional and panel models, we estimate heteroskedasticity-robust standard errors clustered at the district level.

The results from Equation 1 and Equation 2 are presented in Table 3. Estimates of the variable of interest, $T_{it} \times Post_t$, are displayed for each outcome by temptation expenditure category. In Panel A of Table 3, we analyze the impact on expenditure shares. The coefficients represent the change in the share of expenditures spent on each given category of expenditures. Using the cross-sectional sample, we find that the decreased frequency of payments increased the budget share spent on alcohol and decreased the budget share spent on soft drinks and commercially prepared food. The absolute changes in expenditure shares are small. In Figure 2, we quantify the magnitude of the policy's impacts by calculating the percentage change in each dependent variable. The increase in alcohol use amounts to 55%, and the decrease in soft drinks and prepared food are 12% and 20%, respectively. Using the panel sample, we find that the decreased frequency of payments marginally increased alcohol expenditure shares by 77%, and significantly increased expenditures on alcohol shares by 102% and sweets shares by 41%. The policy also increased with marginal significance the expenditures on prepared foods by 30%, and decreased with marginal significance the expenditure shares on food consumed outside of the home by 14%.

We next try to determine whether the effects are concentrated on the extensive (Panel B) or intensive (Panel C) margins. In Panel B of Table 3, we analyze the policy impact on an indicator for the purchase of any goods within the category. None of the coefficients reaches statistical significance at conventional levels. In Panel C of Table 3, we analyze the policy impact on conditional budget shares—that is, on the share of expenditures conditional on the purchase of any goods in the category—in order to measure the inframarginal effect. Using the cross-sectional sample, we find that the decreased frequency of payments significantly increased the conditional budget share for alcohol by 60% and sweets by 21%. Using the panel sample, the effects on conditional expenditure shares of alcohol and sweets are of similar magnitude: 68% for alcohol and 30% for sweets. We also observe a significant effect

[8] We also generated an index of housing quality, but excluded it from our analyses as it is highly collinear with community size.

on conditional tobacco shares of 104%. None of the other conditional outcomes changed by a significant amount.

Figure 2 provides a visual summary of the main results. There is a consistent, positive effect of the policy change on budget shares spent on alcohol and sweets, and a positive effect for tobacco shares in the panel models only. The policy does not seem to have affected the budget shares spent on soft drinks, commercially prepared foods, and food consumed outside of the home.

4.2 Decomposition of Changes in Budget Shares

We next decompose the total change in budget shares into the portions attributable to the extensive and intensive margins. Following McDonald and Moffitt (1980), we combine linear probability models of purchase or no purchase with OLS models of budget shares on the intensive margin to address selection into the purchase decision in two steps. First, we estimate a linear probability model of the probability of purchasing temptation goods during the recall period as a function of the covariates in Equation 1.[9] Second, we estimate the expected budget shares conditional on positive budget shares as a function of these same variables. We then recover the total derivative of our outcome with respect to our treatment variable using the McDonald-Moffitt procedure described next.

Let $E(y)$ be the expected value of outcome y; $E(y^*)$ be the expected value of y, conditional on y being greater than zero; and $F(z)$ be the probability that y is greater than zero. Then, $E(y) = F(z)E(y^*)$. The total derivative of $E(y)$ with respect to the treatment variable x is:

$$\frac{dEy}{dx} = F(z)\frac{dEy^*}{dx} + Ey^*\frac{dF(z)}{dx}. \tag{3}$$

We can disaggregate the total change in y into two parts as represented by the two terms on the right-hand side of the equation: 1) the first term is the change in the intensive margin, or the change in y of those with positive expenditures; and 2) the second term is the change in the extensive margin, or the change in the probability of $y > 0$ weighted by the expected value of y if above zero. We repeat this procedure 400 times per temptation category and compute the bootstrapped standard errors.

We show the results of the decomposition in Table 4. The first column of results gives the total change in unconditional expected budget shares from before and after the policy change. The second and third columns of results decompose this unconditional change into changes along the extensive and intensive margins: the change in the expected probability of purchase from before and after the policy change, and the expected change in budget shares, given expenditures are greater than zero. The fourth column expresses the fraction of the unconditional change that is attributable to changes in the intensive margin. We find that all of the unconditional change in alcohol and sweets spending occurs on the intensive margin, as does 89% of the unconditional change in tobacco spending. The remaining categories

[9] Hastings and Washington (2010) provide an empirical example of this decomposition and argue that a linear probability model is preferable to maximum likelihood estimation because nonlinear panel data models (e.g., Tobit models) have been shown to be biased and inconsistent when the number of time periods is small, and the present approach facilitates the interpretation of the decomposition as changes in the intensive and extensive margins.

of temptation goods did not experience significant increases in spending in the post-policy period, and the decomposition does not yield significant changes, aside from a significant increase in any purchasing of commercially prepared foods after the scheduling change.

4.3 Robustness of the Results

We run a series of sensitivity analyses to determine the robustness of our results. In Panels A and B of Table 5, we check whether the results are robust to alternative measures of the outcome variables: annualized expenditures and annualized expenditures conditional on any purchases of the good. These outcome measures also have the benefit of being directly interpretable. The results are generally in line with our main findings, such that expenditures on alcohol and sweets remain positive and significant in most of the models. Expenditures conditional on any purchases increased by 70 to 125 soles for alcohol and 10 to 33 soles for sweets. However, there are also some notable differences. Tobacco expenditures are not statistically significant across any of the models. In contrast, soft drink expenditures decreased by 27 to 48 soles among those households that purchased any soft drinks.

In Panel C of Table 5, we use an alternate rule for trimming the sample in order to achieve greater overlap of the propensity score distributions between the treatment group and control group.[10] We do so by dropping all observations in either group with propensity scores less than 0.5 or greater than 0.95. This rule reduces the cross-sectional sample by 41% to 17,797 observations and the panel sample by 39% to 5,477 observations. Despite the smaller sample, the regression results are very similar.

One concern might be that our estimates of the treatment effect incorporate secular changes that occurred in the years following the benefits scheduling change. In order to test this assertion, we estimate the immediate effects of the scheduling change in the year after it went into effect. Panel D Table 5 shows that the patterns are broadly similar when we restrict the post-policy period to 2010. In the cross-sectional sample, the effect on budget share spent on sweets achieves marginal statistical significance, whereas in the panel sample, the effect on the budget share spent on alcohol moves just beyond marginal significance.

Our decomposition indicates that expenditures on the intensive margin drive the policy's effects on alcohol, tobacco, and sweets. However, this result does not address whether consumers purchase these temptation goods more frequently, in larger quantities, or of a higher quality. ENAHO includes data on the frequency of purchases of foods consumed inside the home. Among our outcome variables, frequency data are available for alcohol, sweets, soft drinks, and commercially prepared foods. We look at the frequency of purchases per week for each of these categories in Panel E of Table 5. After the benefits scheduling change went into effect, the frequency of sweets purchases increased significantly albeit by less than once per week. The point estimate for the frequency of alcohol purchases is larger and positive, but not statistically significant. The increases in expenditure shares on sweets appears to be driven in part by purchase frequency, whereas the effects on alcohol seem to be driven more by the quantity or quality of alcohol purchased.

Finally, we use an alternate strategy to identify the intensive margin of treatment.

[10] Appendix Figure A2 shows the propensity score distributions using the max-min trim rule and the rule trimming 5% from each tail.

Exploiting variation in household size, we identify the effect of transfer size per capita on household expenditures on temptation goods in the cross-sectional sample. The model takes the form:

$$X_{ijt} = \alpha + \beta Transfer_{it} + \gamma_1 T_{it} + \delta_1 \log X_{it} + \delta_2 (\log X_{it})^2 + \mathbf{Z}_{it}\zeta + \theta_t + \epsilon_{ijt} \quad (4)$$

where $Transfer_{it}$ is the per-capita transfer amount and the dependent variable X_{ijt} represents the total expenditures on goods in temptation category j. Each additional 10 soles of per-capita transfers increases expenditures on alcohol and sweets by 0.7 soles, tobacco by 0.3 soles, soft drinks by 1.2 soles, commercially prepared food by 1.5 soles, and food consumed outside of the home by 28 soles (Panel F of Table 5). These results rely on a similar identification strategy to prior studies of temptation goods, and reinforce that the amount of the transfer is an important factor affecting the consumption decision, along with the frequency of the transfer.

5 Discussion

This study evaluates the consequences of a decreased frequency of payments to benefits recipients in Peru's CCT program. The advantages of evaluating this natural experiment include: district-by-district program eligibility permit a treatment/control design; the change in the frequency of payments did not alter the total transfer amount; the household data capture a rich set of temptation goods; and the policy under investigation suggests a concrete approach to tempering the amount of temptation purchasing undertaken by recipients.

We find that a decrease in the frequency of payments as a result of an exogenous policy change increased the consumption of certain temptation goods, notably alcohol and sweets. The changes appear to be concentrated on the intensive margin, as predicted. We find mixed support for an increase in the household budget spent on tobacco and no increase for soft drinks, commercially prepared food, and food consumed outside of the home. The increased consumption of alcohol contrasts with the prior literature documenting no effect of cash transfers on temptation purchases (Evans and Popova, 2014).

A limitation of the analysis is that expenditures are reported by a single individual in the household, which may lead to measurement error of our outcome variable. In descriptive analyses (not shown), we find that male respondents report significantly fewer expenditures on alcohol by 77% and on tobacco by 50%, relative to female respondents. This is highly indicative that female respondents systematically underreported expenditures on these temptation goods, likely because they were not fully aware of purchases made by male household members. Such mismeasurement of our dependent variables would decrease the precision with which the parameters are estimated, and it would mechanically bias the estimated coefficients toward zero. The relative effect sizes reported in Figure 2 would not be subject to this bias. As such, we place greater confidence in the magnitudes of the relative effect sizes than the absolute effect sizes. Our relative effect estimates indicate that alcohol expenditure shares increased by 55 to 80% following the policy change, and sweets expenditure shares increased by about 10 to 40%.

This study contributes to two important questions in the literature. One is applied in

nature: to what extent do cash transfers stimulate the consumption of temptation goods? We show that the share of the household budget on alcohol and sweets does increase, and this result has implications for the overall effectiveness of the CCT program in Peru. Another question is theoretical in nature: do individuals behave consistently with the permanent income hypothesis? The present study suggests that individuals are sensitive to the frequency of anticipated sources of income, a divergence from the predictions of the permanent income hypothesis. The observed behavior is consistent with alternative models of consumer behavior developed in the behavioral economics literature, such as a model of present-biased preferences.

Many types of temptation goods, including several of the ones under study here, are known to be risk factors for a variety of negative health outcomes. Behavioral scientists have long searched for interventions that might reduce the consumption of temptation goods, such as alcohol, tobacco, and sweets. Our study proposes and indirectly tests a particular approach to limiting the consumption of temptation goods. We find that less frequent payments lead households to purchase more alcohol and more sweets. The potential under-reporting in our sample challenges our ability to address the cost-effectiveness of the benefits scheduling change. In programs such as Juntos, where the transfers are delivered by hand, the administrative costs incurred by the government may well outweigh any negative consequences of increased temptation purchases. In settings where benefits are delivered through electronic transfers, as is becoming increasingly common, the administrative costs of spreading payments throughout the benefits schedule would be much smaller. In such cases, the health benefits from reduced temptation consumption may offset the added administrative costs, making a more compelling argument for such a policy. A next step is to investigate whether policies that *increase* the frequency of cash payments reduce the consumption of temptation goods, as our results indicate. In addition, analyses of individual-level data would help to parse which individuals in the household are responsible for temptation purchases and the intra-household allocation of resources. Both of these factors are important for understanding the welfare implications and health impacts of the change in benefits scheduling.

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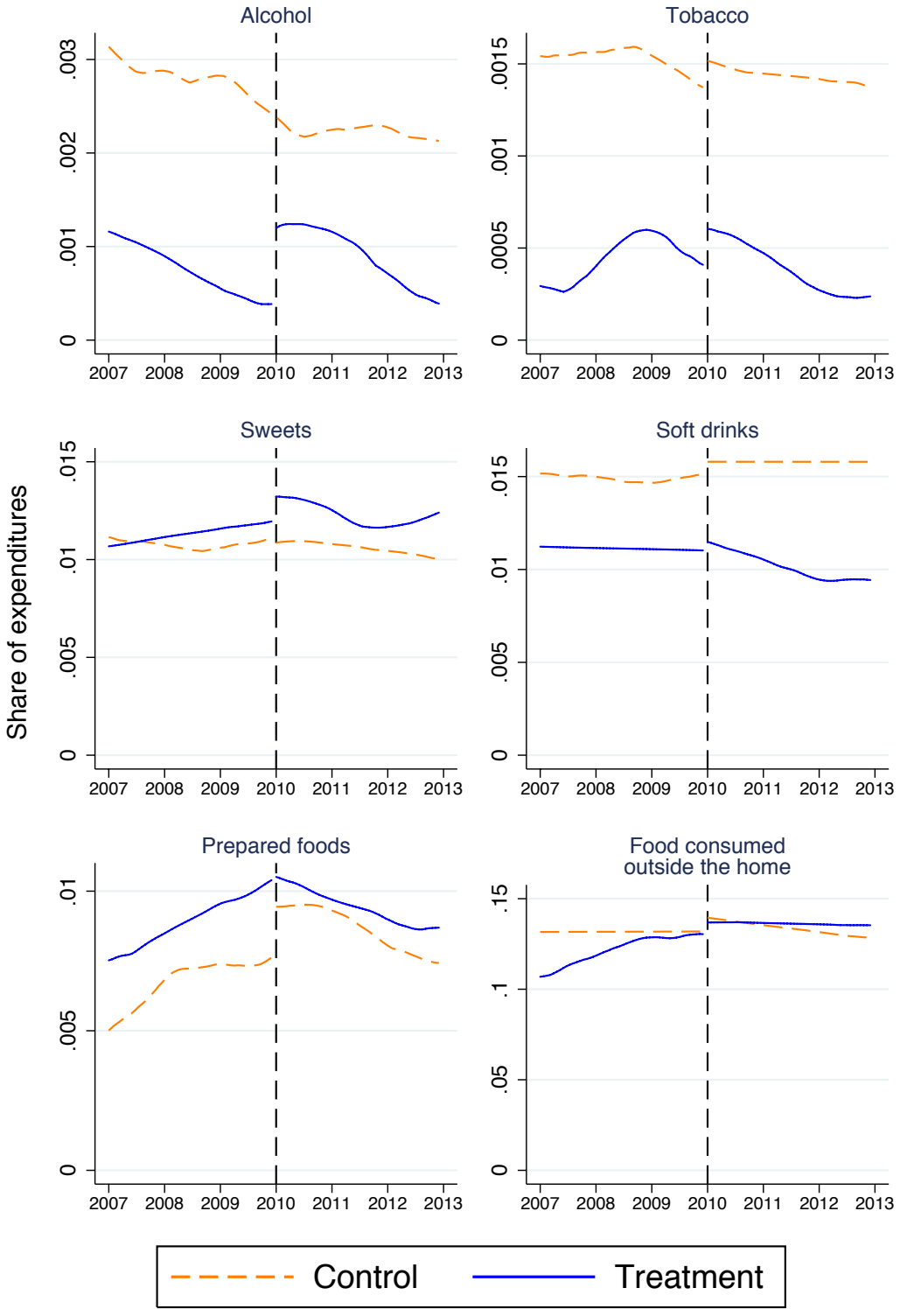
Table 1: Descriptive statistics among sample households

	Cross-section		Panel	
Variable	Control	Treatment	Control	Treatment
log(Expenditure)	8.557	8.410	8.516	8.365
	(1.012)	(0.702)	(1.007)	(0.700)
Household size	3.962	5.516	4.150	5.622
	(2.212)	(1.999)	(2.259)	(2.055)
Household members who are adult (%)	0.701	0.476	0.690	0.467
Head is male (%)	0.809	0.848	0.814	0.849
Education of head (%)				
None	0.098	0.122	0.093	0.118
At least some primary	0.538	0.632	0.578	0.623
At least some secondary	0.279	0.228	0.256	0.234
At least some tertiary	0.085	0.018	0.073	0.026
Native language of head (%)				
Spanish	0.619	0.309	0.624	0.314
Quechua	0.283	0.648	0.278	0.639
Other	0.098	0.043	0.098	0.047
Marital status of head (%)				
Married or cohabitating	0.725	0.842	0.740	0.845
Widowed, divorced, or separated	0.219	0.139	0.208	0.139
Single	0.056	0.019	0.052	0.016
Respondent is male (%)	0.276	0.175	0.267	0.174
District poverty (%)	0.060	0.099	0.060	0.101
Community type (%)				
Rural, 1-140 residents	0.163	0.189	0.157	0.204
Rural, 141-400 residents	0.480	0.673	0.515	0.684
Urban, 1-400 residents	0.168	0.102	0.161	0.079
Urban, 401-4,000 residents	0.188	0.036	0.167	0.033
Region (%)				
Highlands	0.601	0.911	0.610	0.908
Rainforest	0.399	0.089	0.390	0.092
Year (%)				
2007	0.162	0.096	0.156	0.107
2008	0.158	0.152	0.206	0.204
2009	0.159	0.165	0.222	0.214
2010	0.156	0.165	0.279	0.298
2011	0.183	0.205	0.137	0.177
2012	0.183	0.202	-	-
Number of observations	20,529	9,717	6,032	3,014
Number of districts	416	321	228	273

Note: This table displays means of variables and standard deviations of continuous variables in parentheses. The control group consists of households from non-Juntos districts.

Figure 1: Shares of expenditures, by month and category
(Cross-sectional sample)



Note: This figure fits a local polynomial smooth to the monthly trend for the expenditure share by category. Separate trends are plotted for the periods before and after the policy change.

Table 2: Test of common trends assumption
(Cross-sectional sample)

	Budget shares					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-0.0019**	-0.0006**	-0.0015	-0.0028**	0.0041***	-0.0021
	(0.0009)	(0.0003)	(0.0021)	(0.0014)	(0.0011)	(0.0089)
CCT × 2008	0.0003	0.0002	0.0012	0.0010	-0.0012	0.0125
	(0.0009)	(0.0003)	(0.0011)	(0.0013)	(0.0013)	(0.0090)
CCT × 2009	-0.0001	0.0002	0.0017	0.0001	0.0006	0.0171**
	(0.0009)	(0.0002)	(0.0013)	(0.0013)	(0.0014)	(0.0081)
Year, ref = 2007						
2008	-0.0004	0.0001	-0.0011	-0.0012	0.0016**	-0.0042
	(0.0009)	(0.0002)	(0.0008)	(0.0009)	(0.0006)	(0.0052)
2009	-0.0003	0.0000	-0.0003	-0.0014	0.0012*	-0.0084*
	(0.0008)	(0.0002)	(0.0008)	(0.0009)	(0.0006)	(0.0052)
Constant	0.0559*	-0.0009	0.1005*	-0.0327**	0.0334***	-0.2635***
	(0.0287)	(0.0035)	(0.0555)	(0.0140)	(0.0088)	(0.0955)
Full set of covariates	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	13,836	13,836	13,836	13,836	13,836	13,836
Number of districts	617	617	617	617	617	617

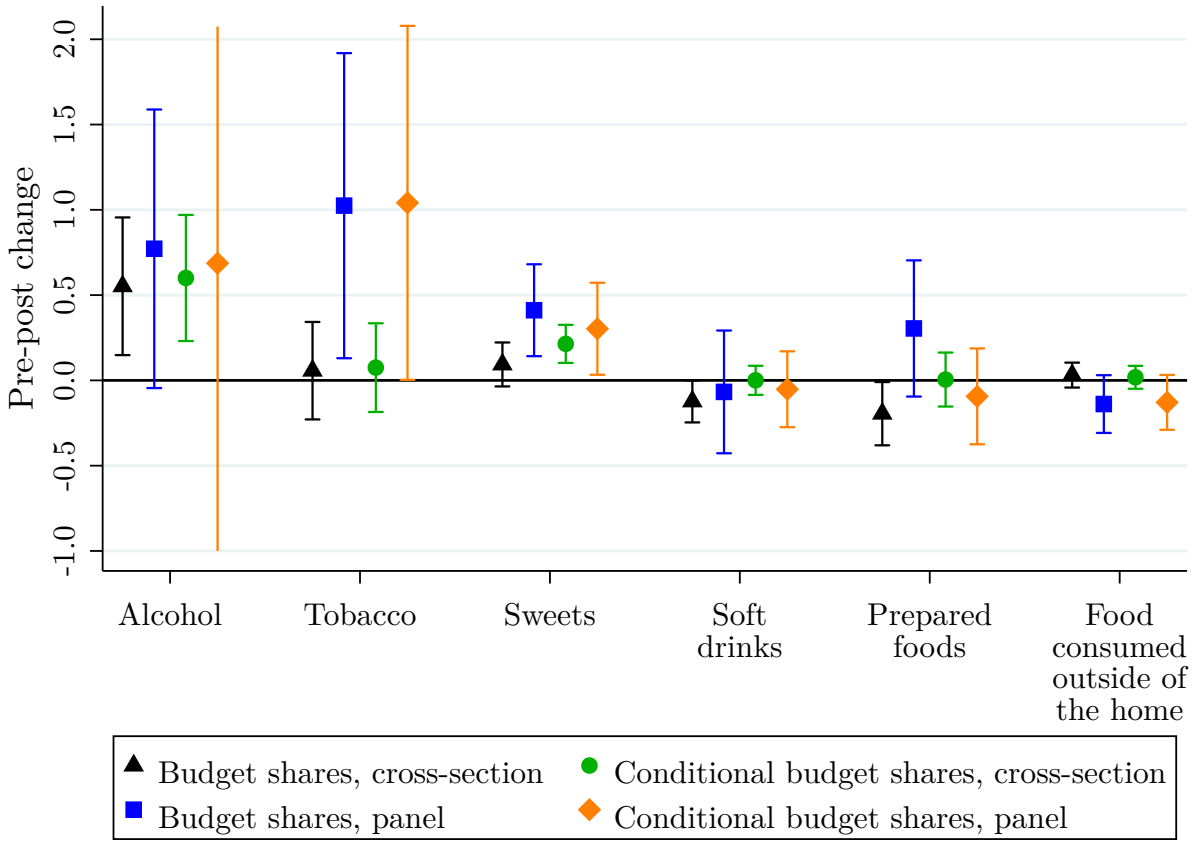
Note: This table displays the differences between CCT recipient households and non-CCT district controls in the pre-policy change period. The coefficients on the interaction of the treatment indicator and year is an indication of whether the two groups have common trends before the policy change. The regressions include all covariates from the models in Table A2. Robust standard errors are clustered at the district level.

Table 3: Policy impact on outcomes

	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
Panel A. Budget shares						
Cross-section	0.0011***	0.0001	0.0010	-0.0017**	-0.0016**	0.0041
	(0.0004)	(0.0002)	(0.0007)	(0.0009)	(0.0008)	(0.0049)
Panel	0.0017*	0.0011**	0.0045***	-0.0009	0.0025	-0.0185
	(0.0009)	(0.0005)	(0.0015)	(0.0026)	(0.0017)	(0.0115)
Panel B. Indicator for any expenditures						
Cross-section	-0.0034	0.0008	-0.0224	-0.0177	-0.0286*	0.0176
	(0.0053)	(0.0074)	(0.0156)	(0.0145)	(0.0170)	(0.0145)
Panel	0.0221	0.0266*	0.0445	0.0168	0.0279	-0.0084
	(0.0146)	(0.0157)	(0.0372)	(0.0350)	(0.0374)	(0.0325)
Panel C. Conditional budget shares						
Cross-section	0.0249***	0.0011	0.0045***	0.0000	0.0001	0.0034
	(0.0078)	(0.0019)	(0.0012)	(0.0015)	(0.0019)	(0.0065)
Panel	0.0323	0.0146**	0.0065**	-0.0018	-0.0021	-0.0249
	(0.0528)	(0.0074)	(0.0029)	(0.0040)	(0.0032)	(0.0158)

Note: This table displays the impact of the policy change on outcomes for the cross-sectional and panel samples. The cross-sectional models include fixed effects for year, region, and community type. The panel models include fixed effects for year and household. Robust standard errors, clustered at the district level, are shown in parentheses. Significance: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$. The full regression output is provided in Appendix Tables A2 to A7.

Figure 2: Relative change in outcomes following the policy change



Note: This figure shows the relative change in (conditional) budget shares in the post period, based on the regression results for the cross-sectional and panel samples in Table 1. The percentage change is calculated as the effect size divided by the mean of the dependent variable. ($\star$) indicates statistical significance at the 5% level.

Table 4: Decomposition of changes in budget shares

	Change in unconditional mean budget shares	Change in budget shares on extensive margin	Change in budget shares on intensive margin	Change in intensive/change in total
Alcohol	0.0010***	-0.0001	0.0012***	1.1386
	(0.0004)	(0.0002)	(0.0004)	
Tobacco	0.0001	0.0001	0.0001	0.8867
	(0.0002)	(0.0001)	(0.0001)	
Sweets	0.0019***	-0.0005*	0.0024***	1.2506
	(0.0006)	(0.0003)	(0.0005)	
Soft drinks	-0.0006	-0.0006	0.0000	-0.0072
	(0.0006)	(0.0004)	(0.0005)	
Prepared food	-0.0006	-0.0007**	0.0000	-0.0632
	(0.0006)	(0.0003)	(0.0006)	
Food consumed outside home	0.0057	0.0033	0.0024	0.4165
	(0.0047)	(0.0021)	(0.0042)	

Note: Bootstrapped standard errors with 400 repetitions are shown in parentheses. Significance: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$.

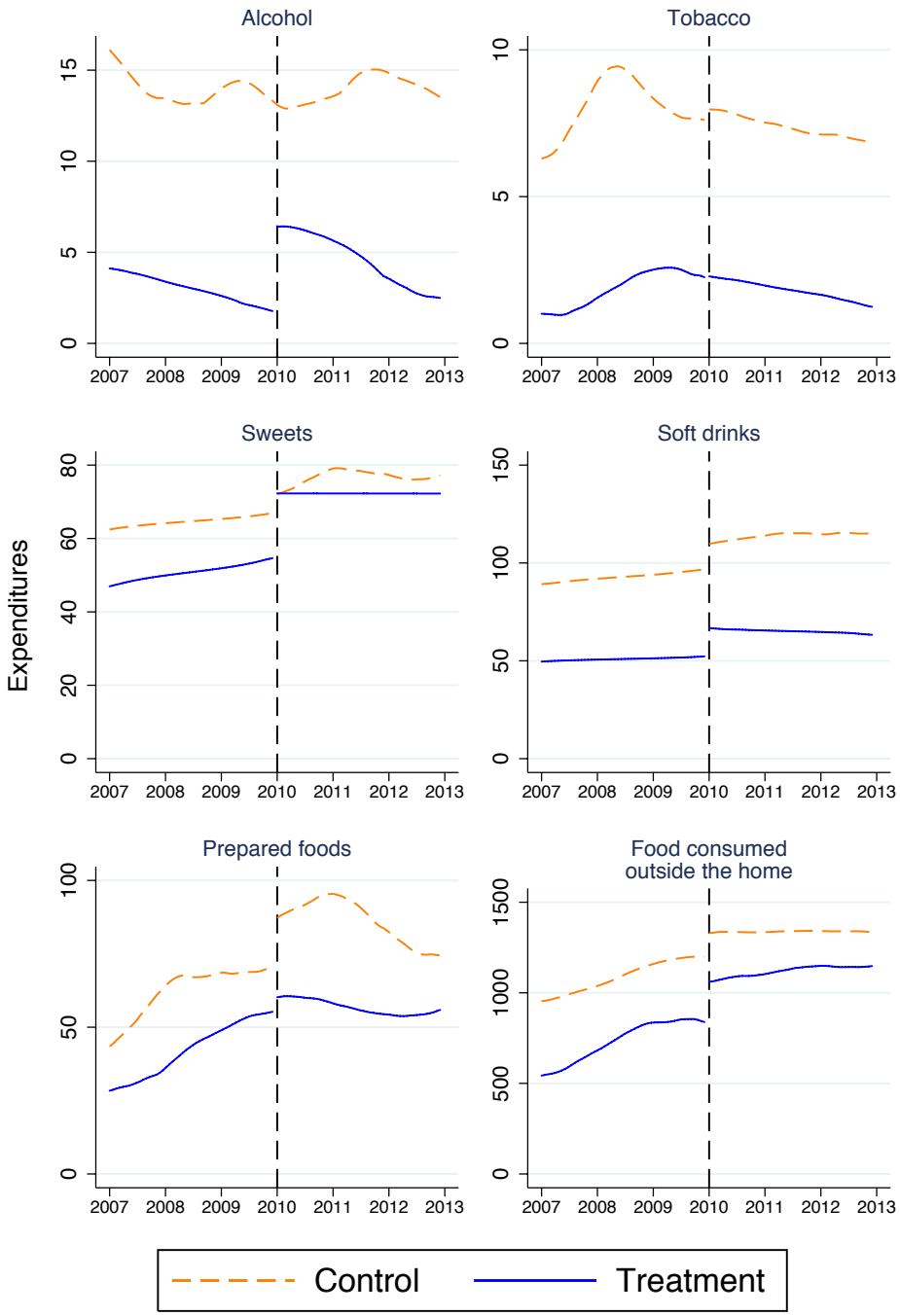
Table 5: Robustness checks

	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
Panel A. Expenditures						
Cross-section	1.337	0.044	2.100	-15.433***	-16.350**	-18.044
	(1.570)	(0.706)	(3.206)	(4.142)	(6.410)	(39.460)
Panel	8.568*	0.021	23.916***	-17.962*	15.804	-61.211
	(4.458)	(1.451)	(8.390)	(9.970)	(13.747)	(91.753)
Panel B. Conditional expenditures						
Cross-section	69.771**	1.404	10.191**	-26.752***	-27.902*	-2.621
	(33.008)	(8.031)	(4.579)	(7.737)	(16.140)	(52.121)
Panel	124.537	-25.291	32.898**	-48.194**	-16.297	-108.035
	(225.483)	(27.263)	(14.405)	(20.996)	(31.502)	(125.573)
Panel C. Budget shares, trimming propensity score at 5% and 95%						
Cross-section	0.0007*	0.0002	0.0009	-0.0014	-0.0020**	0.0017
	(0.0004)	(0.0002)	(0.0008)	(0.0010)	(0.0009)	(0.0059)
Panel	0.0017*	0.0014*	0.0056***	-0.0008	0.0009	-0.0185
	(0.0009)	(0.0008)	(0.0019)	(0.0026)	(0.0023)	(0.0156)
Panel D. Budget shares, immediate effects in 2010						
Cross-section	0.0015***	0.0003	0.0016*	-0.0005	-0.0008	-0.0050
	(0.0006)	(0.0002)	(0.0010)	(0.0012)	(0.0010)	(0.0068)
Panel	0.0016	0.0014**	0.0046***	0.0010	0.0027	-0.0189
	(0.0010)	(0.0006)	(0.0017)	(0.0020)	(0.0018)	(0.0125)
Panel E. Frequency of purchases per week						
Cross-section	0.2302	-	0.0764**	-0.0056	0.0470	-
	(0.1458)	-	(0.0350)	(0.0233)	(0.0736)	-
Panel	1.9361	-	0.1829**	-0.0263	0.0029	-
	(1.6272)	-	(0.0896)	(0.0604)	(0.1924)	-
Panel F. Expenditures						
Transfers per capita	0.0650***	0.0296***	0.0715**	0.1227***	0.1531***	2.7812***
	(0.0234)	(0.0108)	(0.0311)	(0.0391)	(0.0533)	(0.4552)

Note: The cross-sectional models include fixed effects for year, region, and community type. The panel models include fixed effects for year and household. Robust standard errors, clustered at the district level, are shown in parentheses. Significance: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$.

Appendix A Additional Figures and Tables

Figure A1: Expenditures, by month and category
(Cross-sectional sample)



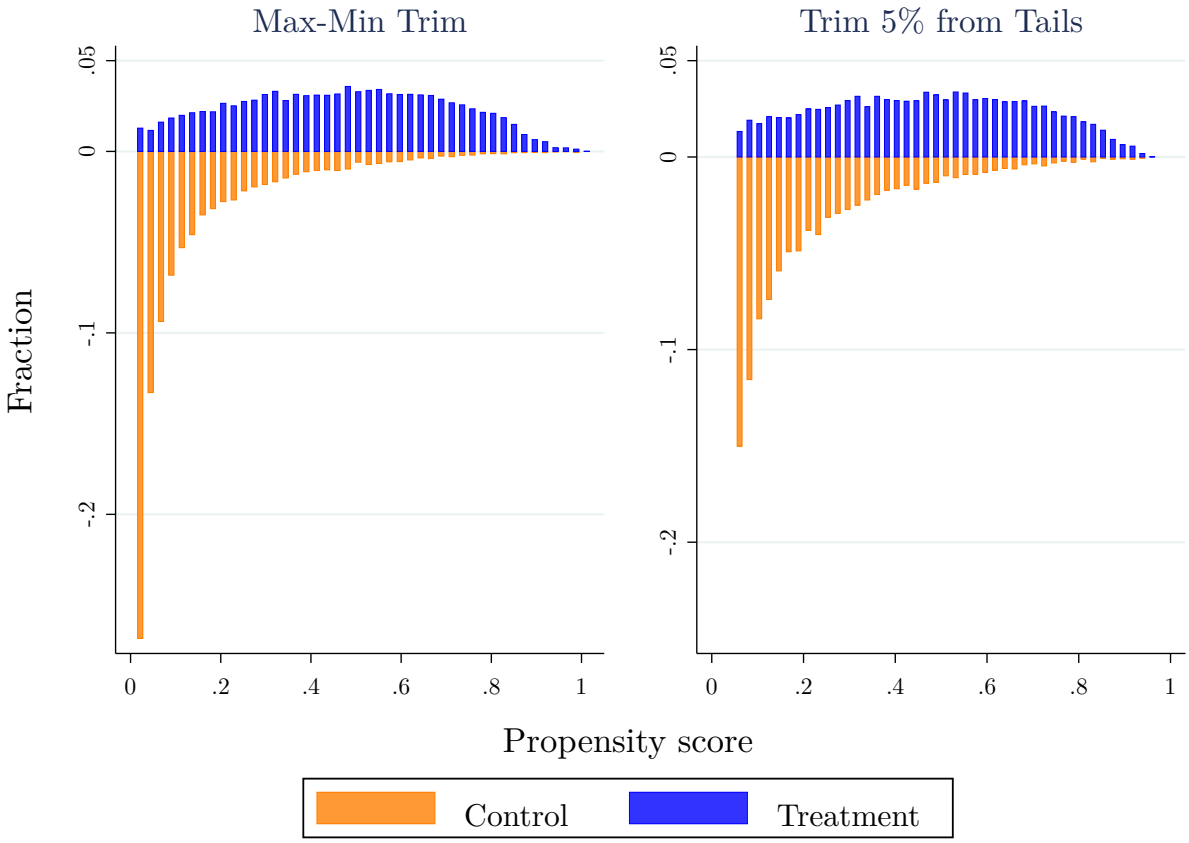
Note: This figure fits a local polynomial smooth to the monthly trend for expenditures by category. Separate trends are plotted for the periods before and after the policy change.

Table A1: Compositional changes
(Cross-sectional sample)

	Control			Treatment		
	Pre	Post	Diff/SD	Pre	Post	Diff/SD
log(Expenditures)	8.457	8.648	0.189	8.232	8.536	0.433
Household size	4.094	3.840	-0.115	5.649	5.422	-0.114
Household members who are adult	0.686	0.714	0.110	0.461	0.487	0.161
Head is male	0.815	0.803	-0.031	0.855	0.843	-0.033
Education of head						
None	0.095	0.101	0.021	0.119	0.123	0.012
Some primary	0.544	0.532	-0.024	0.635	0.630	-0.010
Some secondary	0.278	0.281	0.007	0.227	0.229	0.005
Some tertiary	0.084	0.087	0.011	0.019	0.017	-0.014
Native language of head						
Spanish	0.619	0.620	0.002	0.300	0.315	0.032
Quechua	0.283	0.283	0.000	0.668	0.633	-0.073
Other	0.099	0.098	-0.003	0.032	0.051	0.095
Marital status of head						
Married or cohabiting	0.739	0.712	-0.060	0.852	0.836	-0.044
Widowed, divorced, or separated	0.206	0.232	0.063	0.130	0.145	0.043
Single	0.055	0.056	0.006	0.018	0.020	0.010
Respondent is male	0.288	0.266	-0.049	0.208	0.152	-0.147
District poverty	0.063	0.056	-0.152	0.107	0.094	-0.315
Community type						
Rural, 0-140 residents	0.163	0.163	0.000	0.196	0.184	-0.031
Rural, 141-400 residents	0.487	0.475	-0.024	0.670	0.675	0.011
Urban, 0-400 residents	0.166	0.171	0.013	0.102	0.102	0.000
Urban, 401-4,000 residents	0.184	0.191	0.018	0.031	0.039	0.044
Region						
Highlands	0.599	0.603	0.008	0.929	0.899	-0.106
Rainforest	0.401	0.397	-0.008	0.071	0.101	0.107
Number of observations	9,827	4,009		10,702	5,708	

Note: This table displays the pre-post changes for non-CCT and CCT households. The magnitude of the compositional changes are expressed as the difference in means divided by the pooled standard deviation (Diff/SD).

Figure A2: Propensity score distributions
(Cross-sectional sample)



Note: This figure shows the propensity score distribution using a rule that trims observations in each group above the maximum or below the minimum in the other group (left) and a rule that trims the outer 5% from the tails of the distribution (right).

Table A2: Policy effects on budget shares in the post-treatment period (Cross-sectional sample)

	Budget shares					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-0.0019***	-0.0005*	-0.0002	-0.0022**	0.0032***	0.0082
	(0.0004)	(0.0003)	(0.0013)	(0.0009)	(0.0008)	(0.0057)
CCT × Post	0.0011***	0.0001	0.001	-0.0017**	-0.0016**	0.0041
	(0.0004)	(0.0002)	(0.0007)	(0.0009)	(0.0008)	(0.0049)
log(Expenditures)	-0.0067*	-0.0041	-0.0157	-0.0017	-0.0050*	-0.0338**
	(0.0036)	(0.0032)	(0.0096)	(0.0080)	(0.0028)	(0.0146)
log(Expenditures)^2	0.0004*	0.0002	0.0008	0.0000	0.0004**	0.0060***
	(0.0002)	(0.0002)	(0.0006)	(0.0005)	(0.0002)	(0.0009)
Household size	0.0000	0.0001***	-0.0006***	-0.0003**	-0.0012***	0.0093***
	(0.0001)	0.0000	(0.0001)	(0.0001)	(0.0001)	(0.0010)
% of adults in household	0.0006	0.0003	-0.0079***	-0.0016	-0.0168***	0.0870***
	(0.0005)	(0.0003)	(0.0012)	(0.0011)	(0.0012)	(0.0082)
Head is male	0.0035***	0.0017***	-0.0001	0.0043***	0.0013*	0.0243***
	(0.0006)	(0.0003)	(0.0008)	(0.0010)	(0.0007)	(0.0051)
Education, ref = none						
Some primary	-0.0004	-0.0001	0.0002	0.0004	-0.0008	-0.0121**
	(0.0008)	(0.0003)	(0.0007)	(0.0009)	(0.0006)	(0.0050)
Some secondary	-0.0006	0.0000	0.0000	0.0001	-0.0003	-0.0249***
	(0.0008)	(0.0003)	(0.0007)	(0.0009)	(0.0007)	(0.0054)
Some tertiary	-0.0004	-0.0002	-0.0004	-0.0026**	0.0007	-0.0512***
	(0.0009)	(0.0003)	(0.0009)	(0.0011)	(0.0010)	(0.0068)
Language, ref = Spanish						
Quechua	0.0004	-0.0006***	-0.0007	-0.0011	-0.0022***	0.0037
	(0.0004)	(0.0002)	(0.0006)	(0.0009)	(0.0006)	(0.0041)
Other	-0.0012*	-0.0003	-0.0006	0.0006	-0.0021**	0.0188***
	(0.0007)	(0.0005)	(0.0012)	(0.0017)	(0.0009)	(0.0059)
Marital status, ref = married						
Widowed, divorced, or	0.0018***	0.0010***	-0.0003	0.0019*	0.0012*	0.0596***
separated	(0.0006)	(0.0003)	(0.0008)	(0.0011)	(0.0007)	(0.0051)
Single	0.0013	0.0007**	0.0028	0.0018	0.001	0.0896***
	(0.0008)	(0.0003)	(0.0019)	(0.0013)	(0.0010)	(0.0078)
Respondent is male	0.0011***	0.0002	-0.0012***	0.0032***	0.0002	0.0187***
	(0.0004)	(0.0001)	(0.0004)	(0.0005)	(0.0004)	(0.0030)
District poverty	-0.0047	-0.0009	-0.0095*	-0.0217***	-0.0082*	0.1044***
	(0.0032)	(0.0017)	(0.0054)	(0.0081)	(0.0043)	(0.0401)
Constant	0.0308**	0.0187	0.0983**	0.0222	0.0334***	-0.1327**
	(0.0151)	(0.0133)	(0.0402)	(0.0344)	(0.0119)	(0.0649)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	30,246	30,246	30,246	30,246	30,246	30,246
Number of districts	737	737	737	737	737	737
Mean of dependent variable	0.0019	0.0012	0.0111	0.0137	0.0082	0.1320

Note: This table displays the impact of the policy change on budget shares for the cross-sectional sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A3: Policy effects on budget shares in the post-treatment period
(Panel sample)

	Budget shares					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT × Post	0.0017*	0.0011**	0.0045***	-0.0009	0.0025	-0.0185
	(0.0009)	(0.0005)	(0.0015)	(0.0026)	(0.0017)	(0.0115)
log(Expenditures)	-0.013	0.001	-0.0056	-0.0522	-0.0029	-0.0867
	(0.0160)	(0.0014)	(0.0098)	(0.0436)	(0.0047)	(0.0702)
log(Expenditures)^2	0.0008	-0.0001	0.0003	0.0029	0.0002	0.0113***
	(0.0009)	(0.0001)	(0.0006)	(0.0026)	(0.0003)	(0.0041)
Constant	0.0576	-0.0006	0.0388	0.2435	0.0157	0.0481
	(0.0683)	(0.0066)	(0.0411)	(0.1830)	(0.0198)	(0.2974)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,046	9,046	9,046	9,046	9,046	9,046
Number of clusters	501	501	501	501	501	501
Mean of dependent variable	0.0022	0.0011	0.0111	0.0140	0.0082	0.1337

Note: This table displays the impact of the policy change on budget shares for the panel sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A4: Policy effects on any category expenditures in the post-treatment period (Cross-sectional sample)

	Indicator for any expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-0.0203***	-0.0318***	0.1169***	0.016	0.1255***	0.0041
	(0.0059)	(0.0100)	(0.0167)	(0.0139)	(0.0204)	(0.0168)
CCT × Post	-0.0034	0.0008	-0.0224	-0.0177	-0.0286*	0.0176
	(0.0053)	(0.0074)	(0.0156)	(0.0145)	(0.0170)	(0.0145)
log(Expenditures)	-0.0519***	0.006	-0.0850***	-0.0771***	-0.2635***	0.1251***
	(0.0126)	(0.0161)	(0.0255)	(0.0237)	(0.0224)	(0.0306)
log(Expenditures)^2	0.0042***	0.0002	0.0152***	0.0147***	0.0233***	0.0075***
	(0.0008)	(0.0011)	(0.0017)	(0.0016)	(0.0015)	(0.0020)
Household size	-0.0009	0.0055***	-0.0216***	-0.0160***	-0.0188***	0.0080***
	(0.0008)	(0.0020)	(0.0021)	(0.0023)	(0.0022)	(0.0017)
% of adults in household	0.0009	0.0123	-0.2711***	-0.1116***	-0.4712***	0.0218
	(0.0070)	(0.0107)	(0.0183)	(0.0178)	(0.0192)	(0.0151)
Head is male	0.0410***	0.0559***	0.0088	0.0667***	0.0507***	0.0579***
	(0.0057)	(0.0074)	(0.0116)	(0.0115)	(0.0110)	(0.0108)
Education, ref = none						
Some primary	-0.0087*	-0.0107**	0.0072	0.0105	-0.0065	0.0128
	(0.0047)	(0.0053)	(0.0107)	(0.0099)	(0.0105)	(0.0104)
Some secondary	-0.0166***	-0.0191***	0.0098	0.0027	0.0007	0.0105
	(0.0052)	(0.0071)	(0.0125)	(0.0116)	(0.0134)	(0.0121)
Some tertiary	-0.008	-0.0429***	0.0062	-0.0581***	-0.0011	-0.0148
	(0.0075)	(0.0093)	(0.0163)	(0.0172)	(0.0164)	(0.0152)
Language, ref = Spanish						
Quechua	0.0134***	-0.0238***	0.011	0.0255**	-0.0501***	0.015
	(0.0051)	(0.0069)	(0.0125)	(0.0100)	(0.0125)	(0.0104)
Other	-0.0064	-0.0087	0.0455	0.0827***	-0.0624**	0.0447*
	(0.0077)	(0.0170)	(0.0310)	(0.0171)	(0.0247)	(0.0241)
Marital status, ref = married						
Widowed, divorced, or separated	0.0159***	0.0158**	-0.0076	0.0359***	0.0134	0.0961***
	(0.0056)	(0.0068)	(0.0109)	(0.0108)	(0.0103)	(0.0099)
Single	0.0108	0.0170*	0.0027	0.0559***	-0.0058	0.1191***
	(0.0074)	(0.0089)	(0.0167)	(0.0150)	(0.0135)	(0.0142)
Respondent is male	0.0153***	0.0115**	-0.0503***	0.0400***	-0.0487***	0.0023
	(0.0039)	(0.0051)	(0.0079)	(0.0079)	(0.0071)	(0.0070)
District poverty	-0.0387	0.0439	-0.0958	-0.1536	-0.1904*	-0.0823
	(0.0402)	(0.0682)	(0.1136)	(0.0977)	(0.1111)	(0.1000)
Constant	0.1591***	-0.0624	0.4091***	-0.0539	1.1572***	-1.0733***
	(0.0496)	(0.0653)	(0.1005)	(0.0959)	(0.0889)	(0.1219)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	30,246	30,246	30,246	30,246	30,246	30,246
Number of districts	737	737	737	737	737	737
Mean of dependent variable	0.0467	0.0799	0.5231	0.3937	0.3510	0.6974

Note: This table displays the impact of the policy change on an indicator variable for any expenditures of the category for the cross-sectional sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A5: Policy effects on any category expenditures in the post-treatment period (Panel sample)

	Indicator for any expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT × Post	0.0221	0.0266*	0.0445	0.0168	0.0279	-0.0084
	(0.0146)	(0.0157)	(0.0372)	(0.0350)	(0.0374)	(0.0325)
log(Expenditures)	-0.0573	-0.0033	0.0548	-0.1666**	-0.1853***	0.0969
	(0.0413)	(0.0395)	(0.0862)	(0.0798)	(0.0513)	(0.1094)
log(Expenditures)^2	0.0048*	0.0017	0.0058	0.0200***	0.0183***	0.0115*
	(0.0026)	(0.0025)	(0.0055)	(0.0050)	(0.0035)	(0.0067)
Constant	0.1839	-0.0102	-0.3607	0.3306	0.6005***	-0.9784**
	(0.1648)	(0.1578)	(0.3436)	(0.3271)	(0.1941)	(0.4468)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,046	9,046	9,046	9,046	9,046	9,046
Number of clusters	501	501	501	501	501	501
Mean of dependent variable	0.0464	0.0786	0.5186	0.3949	0.3597	0.6915

Note: This table displays the impact of the policy change on an indicator variable for any expenditures of the category for the panel sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A6: Policy effects on conditional budget shares in the post-treatment period (Cross-sectional sample)

	Conditional budget shares					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-0.0218***	-0.0009	-0.0062***	-0.0083***	-0.0011	0.0082
	(0.0078)	(0.0019)	(0.0022)	(0.0018)	(0.0017)	(0.0072)
CCT × Post	0.0249***	0.0011	0.0045***	0.0000	0.0001	0.0034
	(0.0078)	(0.0019)	(0.0012)	(0.0015)	(0.0019)	(0.0065)
log(Expenditures)	-0.3132***	-0.1925***	-0.1957***	-0.2891***	-0.1004***	-0.2022*
	(0.0777)	(0.0679)	(0.0385)	(0.0692)	(0.0333)	(0.1119)
log(Expenditures)^2	0.0165***	0.0105***	0.0105***	0.0152***	0.0054***	0.0134**
	(0.0044)	(0.0039)	(0.0022)	(0.0039)	(0.0019)	(0.0063)
Household size	0.0004	0.0004**	-0.0002	0.0006**	-0.0018***	0.0091***
	(0.0012)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0011)
% of adults in household	0.0032	0.0046**	-0.0063***	-0.0027	-0.0120***	0.1031***
	(0.0095)	(0.0023)	(0.0021)	(0.0026)	(0.0031)	(0.0106)
Head is male	0.0119**	0.0023	0.0015	0.0029	0.0034	0.0233***
	(0.0058)	(0.0018)	(0.0012)	(0.0018)	(0.0024)	(0.0063)
Education, ref = none						
Some primary	0.0010	0.0023	0.0018	-0.0010	-0.0021	-0.0259***
	(0.0133)	(0.0041)	(0.0011)	(0.0020)	(0.0023)	(0.0074)
Some secondary	0.0082	0.0040	0.0018	-0.0008	-0.0012	-0.0427***
	(0.0140)	(0.0040)	(0.0012)	(0.0021)	(0.0024)	(0.0077)
Some tertiary	0.0017	0.0048	-0.0005	-0.0039	-0.0004	-0.0708***
	(0.0158)	(0.0033)	(0.0016)	(0.0026)	(0.0028)	(0.0089)
Language, ref = Spanish						
Quechua	-0.0066	-0.0021	-0.0016*	-0.0056***	-0.0030**	0.0017
	(0.0053)	(0.0014)	(0.0009)	(0.0018)	(0.0013)	(0.0055)
Other	-0.0324***	-0.0011	-0.0070***	-0.0095***	-0.0004	0.0175*
	(0.0112)	(0.0022)	(0.0023)	(0.0033)	(0.0028)	(0.0100)
Marital status, ref = married						
Widowed, divorced, or separated	0.0028	0.0019	0.0002	-0.0012	0.0056**	0.0591***
	(0.0084)	(0.0020)	(0.0015)	(0.0021)	(0.0026)	(0.0064)
Single	0.0010	0.0010	0.0058*	-0.0029	0.0098**	0.0999***
	(0.0130)	(0.0027)	(0.0034)	(0.0026)	(0.0042)	(0.0094)
Respondent is male	0.0076	-0.0010	0.0001	0.0028***	0.0046***	0.0252***
	(0.0055)	(0.0011)	(0.0007)	(0.0008)	(0.0012)	(0.0039)
District poverty	-0.1238*	-0.0120	-0.0209**	-0.0516***	-0.0138	0.1816***
	(0.0657)	(0.0117)	(0.0089)	(0.0151)	(0.0108)	(0.0541)
Constant	1.5040***	0.8722***	0.9360***	1.3916***	0.4978***	0.8272
	(0.3421)	(0.2887)	(0.1697)	(0.3052)	(0.1474)	(0.5042)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,413	2,418	15,823	11,909	10,615	21,094
Number of districts	412	427	711	706	699	728
Mean of dependent variable	0.0414	0.0144	0.0212	0.0349	0.0233	0.1892

Note: This table displays the impact of the policy change on conditional budget shares for the cross-sectional sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A7: Policy effects on conditional budget shares in the post-treatment period (Panel sample)

	Conditional budget shares					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT × Post	0.0323	0.0146**	0.0065**	-0.0018	-0.0021	-0.0249
	(0.0528)	(0.0074)	(0.0029)	(0.0040)	(0.0032)	(0.0158)
log(Expenditures)	-0.6078**	-0.2096*	-0.1536**	-0.5648**	0.0147	0.0582
	(0.2735)	(0.1131)	(0.0734)	(0.2459)	(0.0345)	(0.2059)
log(Expenditures)^2	0.0335**	0.0109*	0.0082**	0.0307**	-0.0011	0.0009
	(0.0159)	(0.0061)	(0.0042)	(0.0140)	(0.0020)	(0.0115)
Constant	2.7718**	1.0023*	0.7308**	2.6084**	-0.0244	-0.3789
	(1.1661)	(0.5233)	(0.3208)	(1.0694)	(0.1480)	(0.9170)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	420	711	4,691	3,572	3,254	6,255
Number of clusters	196	202	491	475	460	490
Mean of dependent variable	0.0470	0.0141	0.0213	0.0354	0.0227	0.1934

Note: This table displays the impact of the policy change on conditional budget shares for the panel sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A8: Policy effects on expenditures in the post-treatment period (Cross-sectional sample)

	Expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-5.715***	-1.861**	1.722	-11.768***	15.413***	85.753*
	(1.691)	(0.797)	(4.192)	(4.533)	(5.599)	(44.190)
CCT × Post	1.337	-0.044	2.100	-15.433***	-16.350**	-18.044
	(1.570)	(0.706)	(3.206)	(4.142)	(6.410)	(39.460)
log(Expenditures)	-36.757***	-5.405***	-166.906***	-203.683***	-305.489***	-4405.561***
	(7.580)	(1.990)	(11.015)	(15.532)	(26.856)	(178.082)
log(Expenditures)^2	2.775***	0.468***	13.035***	16.377***	22.333***	342.820***
	(0.521)	(0.143)	(0.747)	(1.066)	(1.813)	(11.495)
Household size	-0.854*	0.492**	-4.948***	-2.106**	-12.382***	113.399***
	(0.480)	(0.226)	(0.706)	(1.002)	(1.297)	(9.087)
% of adults in HH	-1.963	4.052***	-61.138***	-34.704***	-146.208***	1026.629***
	(4.032)	(1.395)	(5.639)	(7.131)	(12.060)	(70.948)
Head is male	9.327***	5.299***	8.522***	15.568***	12.364**	22.173
	(2.791)	(0.978)	(3.038)	(3.911)	(5.445)	(40.140)
Education, ref = none						
Some primary	-3.765**	-0.852*	-0.006	-3.282	-8.696***	-83.569***
	(1.885)	(0.493)	(2.161)	(3.030)	(3.170)	(30.167)
Some secondary	-3.507*	-0.637	0.521	-0.850	-2.501	-196.636***
	(2.105)	(0.757)	(2.639)	(3.936)	(4.778)	(37.810)
Some tertiary	1.320	-0.961	10.405*	-26.421***	23.314**	-535.749***
	(4.456)	(1.395)	(5.360)	(7.698)	(10.978)	(59.894)
Language, ref = Spanish						
Quechua	-1.292	-1.638**	-1.917	-2.644	-12.347***	58.517*
	(2.006)	(0.688)	(2.676)	(5.447)	(4.695)	(35.529)
Other	-4.797	-1.014	-2.511	11.018	-9.574	263.253***
	(2.992)	(1.393)	(4.369)	(8.949)	(6.530)	(50.498)
Marital status, ref = married						
Widowed, divorced,	3.392	2.987***	3.048	3.635	5.993	322.909***
or separated	(2.525)	(1.032)	(2.828)	(3.606)	(5.899)	(38.424)
Single	3.260	3.256***	5.882	8.916*	-2.898	565.481***
	(3.546)	(1.257)	(5.582)	(4.650)	(6.803)	(56.950)
Respondent is male	4.979***	-0.119	-3.239	15.763***	7.822*	159.577***
	(1.587)	(0.603)	(2.013)	(3.084)	(4.152)	(24.732)
District poverty	-14.245	2.694	-71.195***	-130.705***	-12.134	1317.431***
	(14.325)	(7.614)	(23.598)	(43.288)	(37.132)	(304.557)
Constant	123.785***	6.655	612.668***	630.904***	1147.445***	12288.442***
	(28.673)	(7.644)	(41.139)	(60.316)	(103.856)	(692.589)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	30,246	30,246	30,246	30,246	30,246	30,246
Number of districts	737	737	737	737	737	737
Mean of dependent variable	10.662	5.863	68.615	88.954	66.864	1,136.840

Note: This table displays the impact of the policy change on expenditures for the cross-sectional sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A9: Policy effects on expenditures in the post-treatment period
(Panel sample)

	Expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT × Post	8.568*	0.021	23.916***	-17.962*	15.804	-61.211
	(4.458)	(1.451)	(8.390)	(9.970)	(13.747)	(91.753)
log(Expenditures)	-47.068*	-3.662	-118.636***	-155.437***	-223.113***	-5294.218***
	(27.257)	(3.170)	(18.625)	(29.572)	(62.062)	(651.908)
log(Expenditures)^2	3.568*	0.407*	9.757***	12.848***	17.073***	412.858***
	(1.860)	(0.224)	(1.261)	(2.020)	(4.307)	(40.410)
Constant	153.632	7.052	363.588***	464.250***	710.838***	16038.591***
	(98.081)	(11.214)	(67.636)	(106.690)	(213.565)	(2610.122)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,046	9,046	9,046	9,046	9,046	9,046
Number of clusters	501	501	501	501	501	501
Mean of dependent var.	10.639	5.756	65.463	86.256	62.891	1,124.384

Note: This table displays the impact of the policy change on expenditures for the panel sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A10: Policy effects on conditional expenditures in the post-treatment period (Cross-sectional sample)

	Conditional expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT	-84.702***	-6.650	-18.250**	-33.734***	18.879	119.702**
	(32.121)	(6.753)	(7.296)	(8.801)	(13.565)	(56.925)
CCT × Post	69.771**	1.404	10.191**	-26.752***	-27.902*	-2.621
	(33.008)	(8.031)	(4.579)	(7.737)	(16.140)	(52.121)
log(Expenditures)	-295.252**	-48.399**	-277.040***	-420.127***	-917.131***	-7179.875***
	(133.907)	(21.327)	(35.241)	(53.531)	(110.867)	(458.982)
log(Expenditures)^2	23.081***	4.143***	20.010***	30.238***	61.400***	510.482***
	(8.711)	(1.401)	(2.141)	(3.307)	(6.722)	(27.574)
Household size	-9.951	1.133	-4.737***	0.958	-21.393***	108.836***
	(9.060)	(1.066)	(1.170)	(1.806)	(2.716)	(10.671)
% of adults in HH	-10.338	39.135***	-57.255***	-22.208*	-182.381***	1100.617***
	(76.664)	(13.927)	(9.191)	(13.301)	(27.335)	(86.890)
Head is male	41.826	25.543***	17.153***	13.889*	33.418	34.074
	(41.486)	(7.602)	(5.314)	(7.356)	(20.504)	(52.937)
Education, ref = none						
Some primary	-40.748	4.625	2.212	-0.451	-6.897	-115.591**
	(36.940)	(5.555)	(3.986)	(6.867)	(10.654)	(45.519)
Some secondary	-9.583	13.286*	1.088	2.365	10.829	-274.679***
	(41.286)	(7.282)	(4.730)	(8.263)	(13.194)	(53.148)
Some tertiary	-17.714	42.597***	9.606	-28.680**	39.837	-717.566***
	(73.460)	(16.108)	(7.953)	(12.446)	(25.132)	(75.782)
Language, ref = Spanish						
Quechua	-85.228**	-1.627	-7.794*	-19.154*	-14.373	36.073
	(35.677)	(6.183)	(4.639)	(10.298)	(12.192)	(46.431)
Other	-89.955*	-1.008	-21.087***	-20.421	5.480	263.001***
	(49.038)	(7.380)	(7.002)	(15.599)	(20.319)	(75.914)
Marital status, ref = married						
Widowed, divorced,	-8.300	11.201	11.920**	-7.303	38.338*	336.511***
or separated	(34.214)	(8.989)	(5.429)	(6.480)	(22.337)	(50.811)
Single	20.666	19.081	19.266	-6.072	30.900	698.905***
	(56.637)	(13.950)	(11.722)	(8.600)	(27.199)	(73.273)
Respondent is male	27.475	-7.632	5.922*	20.015***	47.491***	220.324***
	(24.818)	(5.407)	(3.215)	(5.417)	(12.752)	(32.271)
District poverty	-197.918	-7.810	-89.509**	-193.676**	42.782	2169.278***
	(298.570)	(63.688)	(37.323)	(82.209)	(95.214)	(418.298)
Constant	1140.304**	82.801	1093.693***	1573.111***	3599.770***	23847.091***
	(539.880)	(85.895)	(146.760)	(223.418)	(467.705)	(1906.865)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Community type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	1,413	2,418	15,823	11,909	10,615	21,094
Number of districts	412	427	711	706	699	728
Mean of dependent variable	228.222	73.336	131.159	225.923	190.519	1,630.077

Note: This table displays the impact of the policy change on conditional expenditures for the cross-sectional sample. Robust standard errors, clustered at the district level, are shown in parentheses.

Table A11: Policy effects on conditional expenditures in the post-treatment period (Panel sample)

	Conditional expenditures					
	Alcohol	Tobacco	Sweets	Soft drinks	Prepared food	Food consumed outside of the home
CCT × Post	124.537	-25.291	32.898**	-48.194**	-16.297	-108.035
	(225.483)	(27.263)	(14.405)	(20.996)	(31.502)	(125.573)
log(Expenditures)	-187.422	-42.136	-324.745***	-55.862	-529.328	-1.08e+04***
	(229.686)	(185.032)	(83.895)	(115.545)	(421.100)	(1219.243)
log(Expenditures)^2	21.033	3.700	23.245***	7.724	37.717	739.557***
	(17.400)	(10.860)	(5.126)	(6.888)	(25.248)	(73.207)
Constant	322.958	147.037	1180.511***	103.450	1866.079	38872.952***
	(734.642)	(785.285)	(341.842)	(490.148)	(1740.115)	(5049.631)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	420	711	4,691	3,572	3,254	6,255
Number of clusters	196	202	491	475	460	490
Mean of dependent var.	229.135	73.237	126.236	218.440	174.835	1,626.088

Note: This table displays the impact of the policy change on conditional expenditures for the panel sample. Robust standard errors, clustered at the district level, are shown in parentheses.