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DISCUSSION PAPER SERIES

IZA DP No. 14452

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ABSTRACT

Transitory Income Changes and Consumption Smoothing: Evidence from Mexico*

We study how 3,534 beneficiaries of PROSPERA, Mexico's cash transfer program, smooth food consumption around the transfer payday, an anticipated and transitory income shock. We find that food consumption and food security do not change around the transfer payday, including for recipients with impatient or time-inconsistent preferences and households with higher than median transfer dependence. Conversely, health and employment shocks (unexpected and less transitory income changes) reduce food security. The transfer's relative illiquidity may act as a commitment device, helping time-inconsistent and less experienced debit card holders smooth consumption.

JEL Classification: D12, D91, E21, I12, I38

Keywords: consumption smoothing, permanent income hypothesis, payday

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1 Introduction

The permanent income hypothesis (PIH) predicts that transitory income changes should not affect consumption. Nevertheless, a large literature has documented that consumption tracks income even when the changes are transitory and anticipated (e.g., Jappelli and Pistaferri 2010). Theory and evidence suggest that financial market imperfections and self-control issues are two likely determinants of this phenomenon (e.g., Shapiro 2005; Mastrobuoni and Weinberg 2009).

A related issue is the *cost* of income fluctuations (Chetty and Looney 2006). First, households may resort to costly actions to keep consumption stable: from reducing child schooling, to depleting assets, to forgoing high-risk, high-return investment (e.g., Rosenzweig and Binswanger 1992; Jacoby and Skoufias 1997; Frankenberg et al. 1999, 2003; Chetty and Looney 2009; Mogues 2011). This behavior may cause households to forgo future higher consumption in exchange for a smoother current consumption. Second, income fluctuations and consumption smoothing may also entail cognitive and psychological costs, which may alter behavior and have long-term negative effects. The state of scarcity or the uncertainty associated with income fluctuations may impair cognition and decision-making (Mani et al. 2013; Carvalho et al. 2016; Lichand and Mani 2020). Poverty may worsen mental health, contributing to psychological poverty traps (Haushofer and Fehr 2014; Ridley et al. 2020). This may lead to higher risk tolerance and lower prevention behavior (Angelucci and Bennett 2021).

This paper studies the payday effects of PROSPERA's cash transfer, an anticipated and transitory income shock, on food consumption. PROSPERA, formerly known as PROGRESA/*Oportunidades*, was Mexico's anti-poverty program. The transfer is paid bimonthly at dates, known in advance, which vary between recipients. It is deposited into savings accounts linked to debit cards. The average transfer in our sample is USD80, approximately 110% of total weekly income (other than the transfer), but only 17% of

total annual income.[1] We randomly interview 3,534 households the week before or after receiving the transfer and compare the consumption behavior of households right before and after the payday, as in Stephens (2003) and the related literature. The random timing of the survey ensures that there are no systematic differences between the before and after groups. Moreover, the payday variation across recipients rules out price effects (Hastings and Washington 2010) or spurious correlations with recurring expenses (Gelman et al. 2014). To estimate the very short-term effects on food consumption, we measure food consumption the previous day and food security the previous week. We also collect data on finances, assets sales, employment and income, mental health, preventative healthy habits, cognition, and preferences to test if there is a payday effect on these families of outcomes.

We find that households smooth food consumption around the transfer payday: food consumption and food security do not change before and after the transfer payday. The differences are about 0.05 standard deviations (SD) and never statistically significant. These results hold also for impatient and time-inconsistent recipients, as well as households with high transfer dependence (i.e., a high transfer/income ratio).

Furthermore, we do not find evidence that smoothing consumption around the transfer payday is costly. Households do not sell their assets and have minimal increases in labor supply. Moreover, we do not find any reductions in cognitive performance, mental health, healthy habits, or any changes in time preferences.

Next, we compare these results with the effects of recent employment and health shocks, which are likely unexpected and less transitory income changes. Since the frequency and severity of these shocks may be correlated with unobserved determinants of consumption, we restrict this analysis to households that experienced at least one such shock in the previous year, and compare households with recent (the previous two weeks only) and less recent (the remainder of the previous year) shock occurrences. Unlike for the transfer

[1]This difference arises because the transfer is paid every other month, while labor earnings, the other main income source, are generally paid weekly or daily.

payment, we find that food security is 0.16SD lower in households that experienced a recent health and employment shock. Moreover, the two types of income fluctuations have differential effects for multiple families of outcomes: besides food security decreasing more, households with a recent health or employment shock have more negative labor supply, mental health, and healthy habits effects than households that have not received a cash transfer in almost two months.

The transfer is relatively illiquid. It is deposited into a savings account, unlike the other income sources. Free ATM withdrawals are limited and the median household lives 6.2 kilometers away from the nearest bank branch. We find that junk food consumption is between 10 to 40% lower the week before the payday for some recipients who live at least a thirty-minute walk from the nearest bank branch. This occurs to time inconsistent and relatively new recipients. This finding suggests that the relative illiquidity of the transfer may help some time-inconsistent transfer recipients to smooth consumption, as in Huffman and Barenstein (2004), Shapiro (2005), and Mastrobuoni and Weinberg (2009).

Finally, the difference between our results and studies that find a payday effect may be explained by differences in measurement, transfer dependence, and the timing of the payday. First, we measure food consumption, while other studies consider expenditures (Stephens 2006; Gelman et al. 2014; Carvalho et al. 2016; Olafsson and Pagel 2018). Drops in the latter do not necessarily reflect changes in the former (Aguiar and Hurst 2005). Second, the transfer we consider represents only 17% of income. Studies of payday effects around the receipt of Social Security Income found impacts for households with at least 70 or 80% transfer dependence (e.g., Stephens (2003); Mastrobuoni and Weinberg (2009)). Third, since the payday is uniformly distributed throughout the month, we avoid price effects (Hastings and Washington 2010) and spurious correlations between the timing of the recurrent expenses and the payday (Gelman et al. 2014), which would make consumption *seem* lower before a payday.

These findings contribute to our understanding of consumption smoothing by showing

that low-income households *can* behave in ways consistent with the PIH and that transitory and anticipated income changes may not entail socioeconomic, health, and cognitive costs. The findings also suggest that transfer liquidity may be a useful policy tool to reduce the excess sensitivity of consumption to anticipated and transitory income changes.

2 Setting and Data

PROSPERA, formerly known as PROGRESA/*Oportunidades*, was one of the largest and best-known cash transfer programs. It started in 1998 and ended in 2019. It covered approximately 6.8 million households and 28 million people, or about one fourth of Mexican households (Ministry of Social Development, 2017). PROSPERA beneficiaries receive the transfer every other month at pre-specified dates. Recipients have a payday calendar, so that they know each payday in advance. The paydays vary across participants and depend on the program roll-out in each locality and the timing of enrollment in the program.

We surveyed 3,534 PROSPERA beneficiaries from 52 peri-urban localities in six states, collecting socio-economic individual and household data.[2] The interviews took place between January 25, 2016 and February 25, 2016. Figure A1 shows the administrative records of the pay dates in our sample, which are evenly distributed across work days. This even distribution rules out price and cyclical expense effects (Hastings and Washington 2010; Gelman et al. 2014).

To study the effect of the cash transfer on consumption, we randomly assigned beneficiaries to be interviewed the week *before* or *after* their transfer payday. Specifically, we randomly assigned about 75% of the sample to be interviewed 8, 4 days, and 0 days before the payday (25% each) and the remaining 25% to be interviewed 4 days after the payday. Whenever interviews needed to be rescheduled (for reasons ranging from the respondent

[2]For budgetary purposes we surveyed PROSPERA beneficiaries in six central states of Mexico: Distrito Federal, Hidalgo, Estado de México, Michoacán, Morelos, and Veracruz.

being unavailable to the enumerators running out of time), the enumerators did so in the following days. Ultimately, 86.5% of households were surveyed 8, 4, and 0 days before the payday or 4 days after the payday. The remaining households were surveyed the one to three days after the initially scheduled date.

To identify the effect of the timing of the PROSPERA transfer, we assume that there are no systematic differences in potential outcomes between the before and after groups. Since both the day of the interview and the transfer payday are exogenous, this assumption seems realistic. It is, indeed, consistent with the evidence in Table A1, which shows that the predetermined characteristics of recipients and households surveyed before and after the payday are not systematically different.

Our main outcomes of interest are food security and food consumption. We also consider six families of outcomes: liquidity; finances and employment; cognition; healthy habits; mental health; and preferences.

Food consumption. We measure previous day's household food consumption by asking the respondents to recall the total quantity and monetary value of food consumed inside the household and the total value of the food consumed outside the household the previous day.[3] We consider three food categories in our analysis: total food consumption; consumption of perishables (fruits and vegetables and food of animal origin); and junk food consumption (junk food and snacks and non-alcoholic beverages). For these variables, the "after" payday group is households whose payday is either the day of the interview or the week after the interview. The "before" group is households whose payday occurred 1-7 day before the interview.

Food security. Food security is an index built using three questions that measure the number of days in the previous week in which the household did not face food scarcity.[4]

[3]PROSPERA beneficiaries are the survey respondents and are generally the household member in charge of buying and preparing food. We use a list of ten food categories: fruits and vegetables; desserts; cereals and grains; legumes; food of animal origin (chicken, meat, fish and shellfish, eggs, milk, yogurt, cheese); lard and vegetable oil; non-alcoholic beverages (soft drinks, syrup or powder to prepare flavored beverages, coffee and tea); junk food and snacks; bottled water.

[4]The three questions are: Last week, how many days did any member of this household ask for money

We then sum up the values of the three questions and standardize so that the index has zero mean and a standard deviation equalling 1 in the “after" group. We use this approach throughout when we standardize, so that all standardized outcomes are 0 in the “after" group. We group households as “before" or “after" the payday as we do for food consumption. However, this variable has a seven day recall. For households interviewed 1-6 days after the payday, the recall period encompasses the days right before/after the transfer. This introduces measurement error in the “after" group: the recall period for some households extends to the pre-payday days. We address this issue in section 4.

Disposable income. Labor income in the last three and seven days is the sum of income from all household members' primary and secondary jobs received the previous three and seven days and, therefore, differs from average labor income. Disposable income in the previous three and seven days is the sum of total household labor income, monetary savings, and the cash transfer (for households that received the transfer in the previous 3 and 7 days). For these variables, the “after" payday group is households whose payday is the week after the interview. The “before" group is households whose payday occurred either the day of the interview or 1-7 days before the interview.

Household finances and labor supply. Households' net savings are savings minus debt. We measure asset fire sales as the count of assets sold the previous week. Regarding labor supply and income, we consider seven different outcomes for adults and children separately: labor income in the last 3 and 7 days; the fraction of adults/children who were paid in the last 3 and 7 days; the fraction of adults/children who worked in the last 3 and 7 days; and hours worked in the last 3 days.[5]

Cognition, healthy habits, mental health, and preferences. We create three indices for cognitive function, healthy habits, and mental health. We are interested in habits that are not costly

or borrow money to eat? Last week, how many days was there insufficient food to eat and some adults in the households remained hungry? Last week, how many days was there insufficient food to eat and some children in the households remained hungry? For each question, we assign a 0 to 7 value according to the number of days in the week in which the household did not suffer from food insecurity.

[5]We did not measure hours worked in the previous 7 days.

or difficult to implement, such as hand washing and tooth brushing, but which can have large future health benefits. We also create a risk tolerance index, a patience indicator, and an indicator assessing whether the beneficiary exhibits time-consistent preferences (see Appendix B for details). For these variables, the "after" payday group is households whose payday is the week after the interview. The "before" group is households whose payday occurred either the day of the interview or 1-7 days before the interview.

3 Identification and Estimation

To study how the timing of the cash transfer affects our outcome variables Y_i, we estimate the parameters of the following equation:

$$Y_i = \alpha_0 + \alpha_1 P_i + \Omega' X_i + \epsilon_i \tag{1}$$

The indicator P_i equals 0 for households with a transfer payday in the last 7 days and 1 for households with a transfer payday in the next 7 days. This latter group has *not* received a transfer for almost two months. The predetermined variables X_i include: age and education of the PROSPERA beneficiary; marital status; number of male and female children (0-17 year of age), adult (18-64 years of age), and elderly (older than 65) household members; dummies for having experienced employment or health shocks in the previous 12 months and for whether the latest shock occurred within the previous two weeks; weekday indicators; and state fixed effects.

The coefficient α_1 identifies the effects of being about to receive the PROSPERA transfer relative to having just received it. This parameter is identified under the assumption that there are no systematic differences in the outcome determinants for households surveyed before and after their payday. This assumption is likely to hold since the timing of the survey is random.

We estimate the parameters of this equation by OLS, clustering the standard errors by

locality (Abadie et al. 2017). Since we consider multiple outcomes that belong to the same "family," we also control for the False Discovery Rate (FDR) within each family (Benjamini and Hochberg 1995), unless we created an index for the family of outcomes (as we did for cognition, healthy habits, and mental health).

4 Results

Figure 1 shows that disposable income decreases by around 1.2SD before the payday. This reduction is entirely driven by the cash transfer.

The top panel of Figure 2 shows our main finding: households smooth food consumption around the transfer pay date. Neither aggregate food consumption and its components the previous day, nor food security the previous week are lower for households about to receive the cash transfer. The coefficient estimates for these variables are around 0.05SD and statistically insignificant. We present a second estimate for food security that we obtained dropping households surveyed 1-3 days after the payday from the "after" group. We do this because the recall period for these households falls mainly in the days before payday. The results are qualitatively unchanged. Figure A2 shows that the distributions of these four variables are also similar before and after the pay date.[6]

The second panel of Figure 2 shows that net savings, asset sales, labor income, and employment also do not vary around the pay date.[7] The only statistically significant impact is on the fraction of adults who worked the previous 3 days, which increases by about 0.07SD, or 2 percentage points from 65%. This effect however, is no longer statistically significant once we adjust for multiple inference. Overall, the estimates for these outcomes are both statistically insignificant and small. These findings suggest that smoothing consumption around the transfer pay date is not achieved through dissaving, fire sales, income smoothing, or higher adult or child labor. Under this metric, smoothing food

[6] A Kolmogorov-Smirnov test rejects the hypothesis that the distributions are lower before the pay date for each variable.

[7] The lack of an effect on net savings is consistent with people considering the transfer disposable income.

consumption around the transfer payday does not appear to be costly for the households in our sample.

As mentioned earlier, income fluctuations may have "hidden" cognitive and health costs. We explore this hypothesis in the third panel of Figure 2. As the estimates show, cognition does not decrease when the recipient has not received the transfer for almost two months. This is consistent with Carvalho et al. (2016)'s findings about low income US households, but inconsistent with Mani et al. (2013)'s finding that scarcity impairs cognitive function. We speculate that this discrepancy in findings may be related to uncertainty about the specific timing and amount of the future income change. In our case and Carvalho et al. (2016)'s, households know when and how much money they will receive in the near future. Conversely, the state of scarcity in Mani et al. (2013) has a more uncertain resolution. Indeed, evidence suggests that uncertainty is cognitively taxing (Lichand and Mani 2020).

Next, the fourth panel of Figure 2 shows that being about to receive the transfer has small and *positive* health effects: mental health improves and healthy habits increase. These results may seem counter-intuitive, as they imply that mental health and healthy habits are higher when income is lower. Nevertheless, they are consistent with reward anticipation: people derive utility from anticipating future pleasant events and their anticipatory utility may be higher than the utility from the event itself (Loewenstein 1987; Caplin and Leahy 2001; Baumeister et al. 2007). These findings highlight an overlooked feature of cyclical income changes such as the PROSPERA transfer: the day in which households have not received a transfer the longest is also the day in which the future income increase is closest, thus encompassing both a "bad" and an anticipated "good" state of the world.

The remainder of the fourth panel shows that, while we find no change in time preferences, people have higher risk intolerance before the transfer payday. This finding is consistent with the hypothesis that facing negative shocks may be costlier when liquidity

is lower. In addition, the link between better mental health and higher risk intolerance is consistent with findings that providing mental health care to depressed adults decreases both their symptoms of depression and their risk tolerance (Angelucci and Bennett 2021).

Next, we discuss two sources of measurement error. Classical measurement error in the payday would attenuate the differences between the two groups of households, thus underestimating the true effects on our outcomes. Having administrative records of paydays minimizes these concerns. The second source of mis-measurement pertains to food security over the past 7 days and employment and labor income over the past 3 and 7 days. For households interviewed up to three days after the payday, the recall period for these variables encompasses the period before/after the payday.[8] Dropping these households from the analysis fully addresses this source of mis-measurement for the 3-day recall period, and attenuates it for the 7-day recall period.[9] To address both issues, we repeat the analysis dropping households whose reported payday is within plus or minus 3 days since the interview date. This reduces our sample size by 27%. The left panel of Figure A3 shows that the results are qualitatively unchanged.

As a further robustness check, we also show that our results are not driven by clustering the standard errors by locality. The right panel of Figure A3 shows that the results are qualitatively unchanged when we do not cluster our standard errors and use instead standard errors robust to heteroskedasticity (White 1980). The results are also unchanged if we use survey date fixed effects instead of weekday fixed effects in equation 1.

Tables A2–A5 reproduce the results from Figures 1 and 2 without standardizing the outcomes. They also show the estimated effects for specific subgroups by interacting the "Transfer in 1-7 days" indicator by a subgroup dummy. We consider three dimensions of heterogeneity: impatience, time inconsistent preferences, and household dependence on

[8]For example, consider a recipient with a Monday payday who is surveyed three days later. When she describes food security the previous seven days, she refers to a time period that includes days before and after the payday.

[9]We cannot drop the households whose payday occurred 4 to 6 days before the survey because we would drop all but 6 of the observations in our "after" group.

the transfer. The impatience indicator equals one for people whose behavior is consistent with having a low discount factor. The time inconsistent indicator equals one for people whose intertemporal choices differ when the sooner date is today or some time in the future.[10] We define high transfer dependence as having a higher than median transfer as a proportion of household income. The median transfer/income ratio is 12%. We create a "large transfer" indicator that equals one for households with a transfer/income ratio of at least 12%.

Overall, we find no heterogeneity by impatience and time inconsistent preferences. Conversely, our findings suggest that households with higher than median transfer dependence may smooth food consumption around payday through income and employment smoothing. While this group of households also smooths consumption around the transfer payday, Table A4 shows that the fraction of adults paid the previous week and who worked in the previous 3 days both increase by 4 percentage points, corresponding to 7 and 6% increases from the respective means.[11] In addition, Table A5 shows that cognitive performance increases before the transfer payday for households with a low transfer dependence but *not* for high transfer dependence households. This differential effect suggests that there may be positive anticipation effect on cognition for households with low transfer dependence. However, this positive effect may be offset by the cognitive costs of being in a state of scarcity for high transfer-dependence households.

We also estimated heterogeneous effects for the top transfer dependence decile (356 households). For these households, the transfer is approximately one third of total income. We note that food security is 0.2SD lower before the payday, suggesting that these households may not smooth food consumption around the payday. However, this effect is imprecisely estimated, likely because of the small sample size.

[10] See Appendix B for details.

[11] Neither coefficient is statistically significant after adjusting for the FDR, though. Moreover, we do not find statistically significant effect on labor income received the previous three and seven days.

5 Comparison with Employment and Health Shocks

Some households in our sample experienced the following health or employment shocks the previous year: employment or business loss; prolonged illness of a household member; death of a household member or relative. The associated income shocks are more likely to be unexpected and non-transitory than the PROSPERA transfer. Moreover, their resolution is likely more uncertain. We can, therefore, test whether households that experienced a recent health or employment shock consume less than other households. In addition, if uncertainty causes mental health, cognitive, and behavioral costs, the two types of shocks may affect these outcomes differently.

The likelihood of experiencing health or employment shocks may be endogenous to consumption if, for example, poorer and more vulnerable households are more susceptible to shocks. To address this issue, we consider households that experienced at least one such shock in the previous year (47% of our sample) and exploit plausibly exogenous variation in the timing of the last shock they experienced. We then compare outcomes for households that have experienced a shock recently (the previous 2 weeks) and less recently (the remainder of the previous year).

We first check that households in these two groups are similar. Appendix Table A6 shows that households that experienced employment and health shocks the previous year do not differ substantially from the remaining households: while some variables are statistically different in the two groups, these differences are small. Thus, our findings from this smaller group are likely generalizable to the whole sample. Households that experienced health and employment shocks more or less recently also have similar characteristics. The only exception is income, which is unsurprisingly lower for households that experienced recent shocks.

To study how consumption varies by different types of income changes, we estimate the parameters of the following equation for households that experienced a health or

employment shock the previous year:

$$Y_i = \alpha_0 + \alpha_1 P_i + \alpha_2 H_i + \Omega' X_i + \epsilon_i. \quad (2)$$

The indicator P_i equals 1 for households about to receive the PROSPERA transfer and the indicator H_i equals 1 for households that have experienced employment or health shocks in the previous 14 days. The variables X_i are predetermined characteristics (age, education, and marital status of the PROSPERA beneficiary; number of male and female children, adults, and elderly household members; PROSPERA transfer; and weekday and state dummies).

Our parameters of interest are α_1 and α_2. The coefficient α_1 identifies the effects of being about to receive the PROSPERA transfer. This parameter is identified under the assumptions that the transfer payday is exogenous. The parameter α_2 identifies the effect of having experienced an employment/health shock in the previous two weeks. The identification assumption is that, conditional on experiencing at least one such shock in the previous 12 months and on the X covariates, the timing of the latest shock is exogenous.

Figures 3 and 4 compare the effects of the two types of income change – being about to receive the transfer and having experienced a negative shock the previous two weeks – on all outcomes previously considered. We indicate when each pair of estimates is statistically different from each other.

Figure 3 shows that disposable income decreases less after an employment/health shock (about -0.15SD vs. -0.8SD) and that the drop in liquidity is driven by a reduction in labor income (-0.2SD vs. 0SD). Figure 4 shows that the two income changes have differential impacts. First, households that recently experienced a negative employment/health shock have 0.16SD less food security than households who experienced similar shocks earlier in the year and 0.2SD less food security than households about to receive their cash transfer. When we re-estimate the parameters of equation 2 dropping mis-classified

households, the differences are even starker: food security is 0.33SD lower for households that experienced a recent employment/health shock (compared with both households that did not experienced a recent shock and households about the receive the transfer).[12] Food consumption the day before does not decrease for households with recent employment/health shocks, possibly because, by the time they are surveyed, these households have resumed they regular food intake.

Second, the two types of income changes affect household labor supply and finances differently: net savings, labor income, and employment decrease for households that experienced an employment/health shock in the previous two weeks. Third, the two income changes have different effects on healthy habits and mental health: compared to households about to receive the transfer, households that suffered recent health/employment shocks have 0.2SD lower mental health and healthy habit indices, consistent with the hypothesis that likely more uncertain and less transitory income changes may have mental health and behavioral costs. However, cognition and time and risk preferences are not lower when experiencing health/employment shocks and the effects of the two income changes on these outcomes do not differ.

Overall, the two income changes we considered have different effects. Some of these differences, such as the better smoothing of the expected and transitory income change, and the resulting differences to finances and employment, are consistent with the PIH (Jappelli and Pistaferri 2010). Differences in the effects on mental health and healthy habits may be related to the more uncertain and permanent nature of the health and employment shocks and suggest that some income fluctuation may have additional costs besides the utility loss from lower consumption.

[12] To account for the seven day recall for food security, we drop households that experienced a shock fewer than seven days before. For these households, the recall period includes days before the shock occurred. This is analogous to what we did in Figure 2.

6 Mechanisms

The households in our sample may smooth food consumption around the transfer payday because the transfer is relatively illiquid, unlike labor income, which is generally paid in cash.[13] The transfer is deposited into a savings account with BANSEFI, a government-owned bank. While the account is linked to a debit card, PROSPERA beneficiaries use debit cards to pay for purchases infrequently (Bachas et al. 2021). This is a common phenomenon in Mexico: debit card holders with 5-9 years of education (the interquartile education range of our sample) use debit cards at point of sale on average 1.48 times per month and 58% of users report never using their debit card to make a purchase.[14] In addition, all but the first two ATM withdrawals are free. Moreover, while withdrawals from BANSEFI branches are free, about three quarters of the respondents live at least a 30 minute walk from the closest BANSEFI branch and only 16 percent of them own a car.[15] The distance from the branch, coupled with the limited free withdrawals from ATMs and the reluctance to use the debit cards to make purchases reduces the transfer's liquidity for most households. This may help households smooth consumption around the payday, especially the ones in need of a commitment device.

The data weakly support this conjecture. Consider distance from the closest BANSEFI branch as a proxy for *de facto* transfer liquidity: the transfer is supposedly less liquid for households who live farther from the nearest branch. To compare the consumption-smoothing behavior of households with more or less liquid transfers, we group households within a 30 minute walk from the nearest BANSEFI branch or that own a car and define them as being close to a bank. We define households farther from the nearest branch

[13] About 88% of employed people are paid daily or weekly. Most of the remaining 12% hold informal jobs in agriculture, construction, and services. The frequency of payment and job types suggests that most people are paid in cash.

[14] Data from 2018 ENIF (Encuesta Nacional de Inclusión Financiera). 55.5% of ENIF's respondents with 5-9 years of education report not using the card because they simply prefer using cash. Another 7.4% mentioned lack of trust, and yet another 7.4% said they did not know they could use the debit card to make purchases.

[15] The median distance from the nearest BANSEFI branch is 6.2 kilometers.

and without a car as being far from the bank. We then estimate versions of equation 1 with an added "close to bank" indicator and we let the coefficient α_1 vary for households close to and far from a bank. Table A7 reports these two coefficients and their difference. The findings suggests that consumption smoothing may be harder for some households, when they live close to a branch. Panel A shows that these households consume between 10 and 40% less junk food before the transfer payday, depending on their characteristics. Moreover, column (4) shows that junk food consumption before the payday decreases statistically more for households close to the bank than for households far from the bank, if the recipients are time inconsistent and relatively new debit card holders (and thus less likely to use the card to make purchases, if use increases with experience).[16]

These findings suggest that some recipients of more liquid transfers may yield more easily to temptation and increase consumption of unhealthy food right after having received the transfer. For these recipients, distance from the bank may act as a commitment device and help to prevent this behavior. These findings are conceptually consistent with Huffman and Barenstein (2004); Mastrobuoni and Weinberg (2009); Shapiro (2005). However, junk food represents only 8% of total food consumption and distance from a bank branch does not significantly affect overall food consumption smoothing.[17]

Households may smooth consumption in the presence of income volatility through informal resource-sharing networks, consistent with evidence from rural Mexico (Angelucci and De Giorgi 2009). In our case, monetary or in-kind transfers from family and friends before the transfer payday may help keep food consumption stable. However, only 5% of households report having received any such transfers in the previous 12 months (possibly because these networks are stronger in rural than urban areas). Therefore, social networks are unlikely to play an important role in smoothing consumption around the transfer pay date.

[16]The median household has owned a debit card for 26 months. We define lower than median households as relatively new card holders.

[17]We also fail to find differential effects by distance from the bank for fruits and vegetables and perishables.

Another potential mechanism may be related to household bargaining. Since transfer recipients are women, the transfer likely increases their bargaining power and hence the structure of household demand, provided that women's preferences systematically differ from the preferences of the remainder of the household. We rule out this possible pathway because we do not expect women's bargaining power (and hence household consumption) to vary around the payday.

Lastly, we compare our findings with the payday effect literature. Stephens (2003, 2006) and Olafsson and Pagel (2018) find that expenditures increase after a payday. However, food consumption may not vary when expenditures decrease (Aguiar and Hurst 2005). Shapiro (2005) and Mastrobuoni and Weinberg (2009) find that caloric intake increases after a payday for SNAP recipients (Shapiro 2005) or Social Security Income (SSI) recipients with no savings (Mastrobuoni and Weinberg 2009). However, food stamps and SSI are likely more liquid than the PROSPERA transfers. Moreover, the transfer dependence in our sample is 17%. This is slightly lower than the average transfer dependence for SNAP recipients, and much lower than the SS recipients, who are selected to have a transfer dependence of at least 0.8. We conjecture that the lower transfer liquidity and dependence in our sample may be important determinants of consumption smoothing.

7 Conclusion

We study food consumption smoothing around a transfer payday for a sample of low-income cash transfer recipients from peri-urban Mexico. Their behavior is consistent with the permanent income hypothesis: food security and food consumption do not change in response to anticipated and transitory income changes. This finding holds also for households with time-inconsistent or impatient recipients, as well as for households with a high transfer dependence.

We do not find that smoothing food consumption around the transfer payday is as-

sociated with large financial, employment, health, and cognitive changes. This finding suggests that smoothing consumption around the transfer payday may not be costly for the households in our sample.

Further, we find that food security decreases after unexpected and less transitory employment and health shocks, also consistent with the PIH. The two income changes also have differential effects on mental health and prevention behavior. These results suggest that some types of income fluctuations may have additional health costs, besides causing drops in consumption.

Lastly, our findings suggest that the relative illiquidity of the transfer may help some households smooth consumption around the payday. This limited liquidity may especially benefit financially inexperienced and time-inconsistent recipients.

The literature suggests that more frequent transfers (Shapiro 2005; Mastrobuoni and Weinberg 2009) or in-kind transfers (Huffman and Barenstein 2004) may help recipients smooth consumption in the presence of time-inconsistent preferences. Our results suggest that reducing the transfer liquidity may also improve consumption smoothing for some households.

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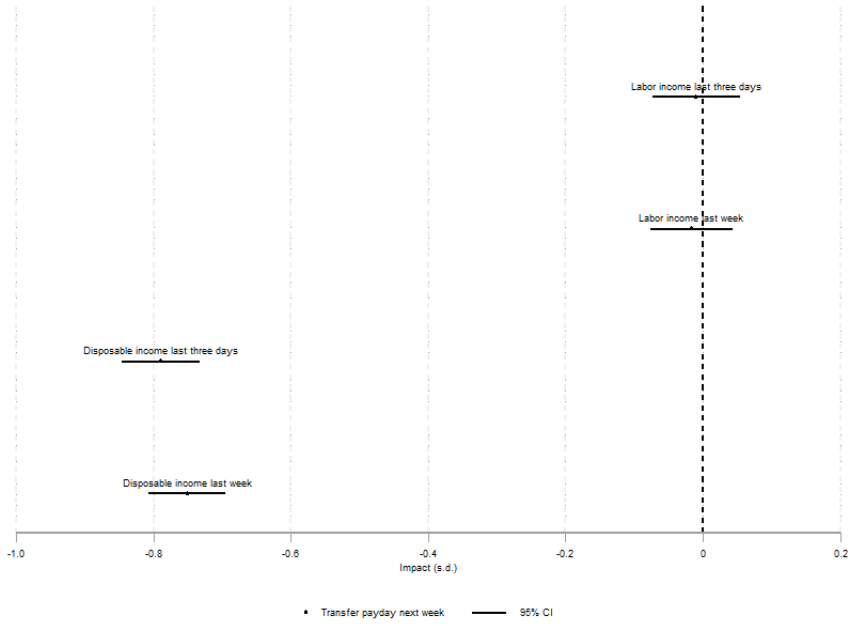
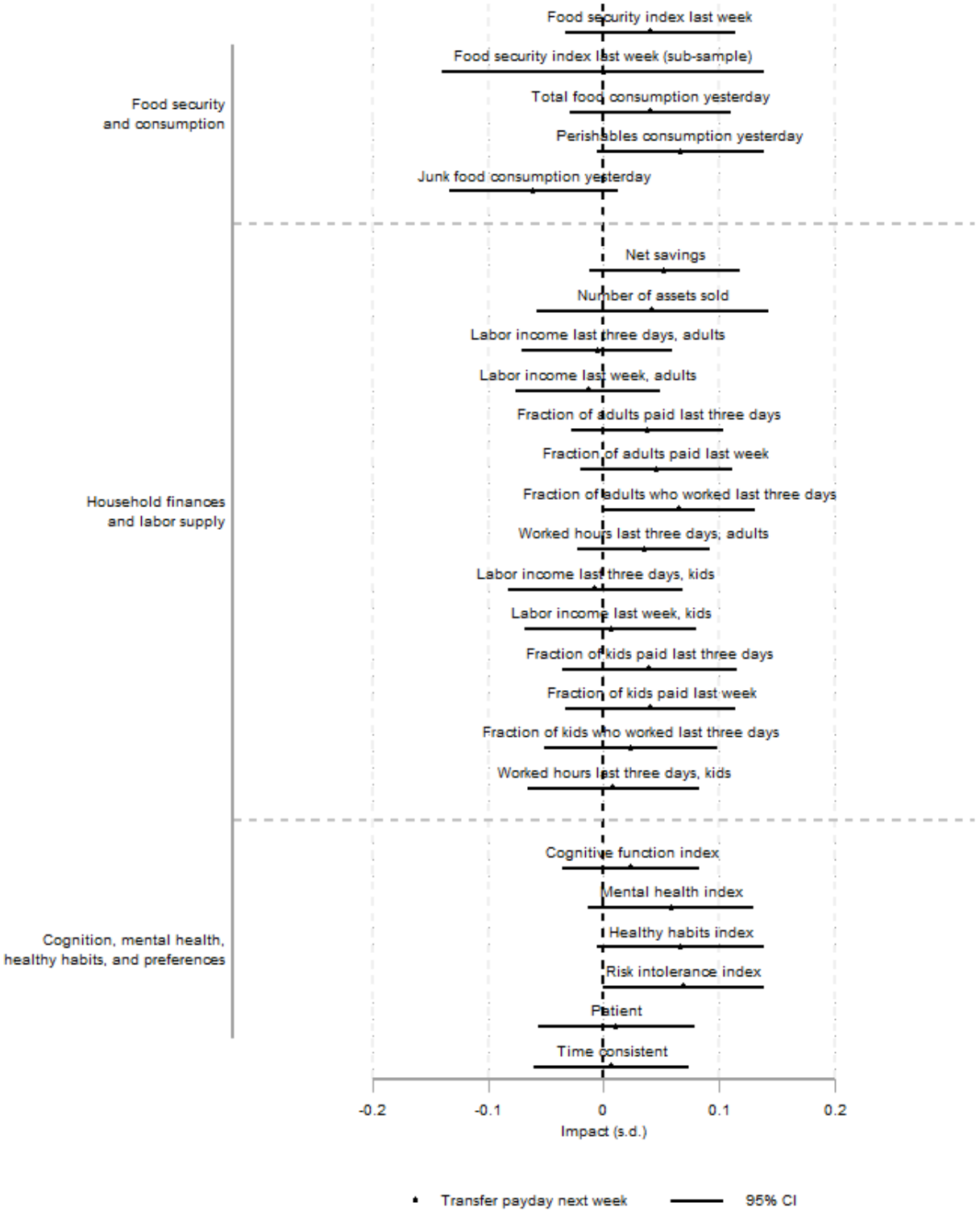
Figures

FIGURE 1: Differences in disposable income before transfer payday



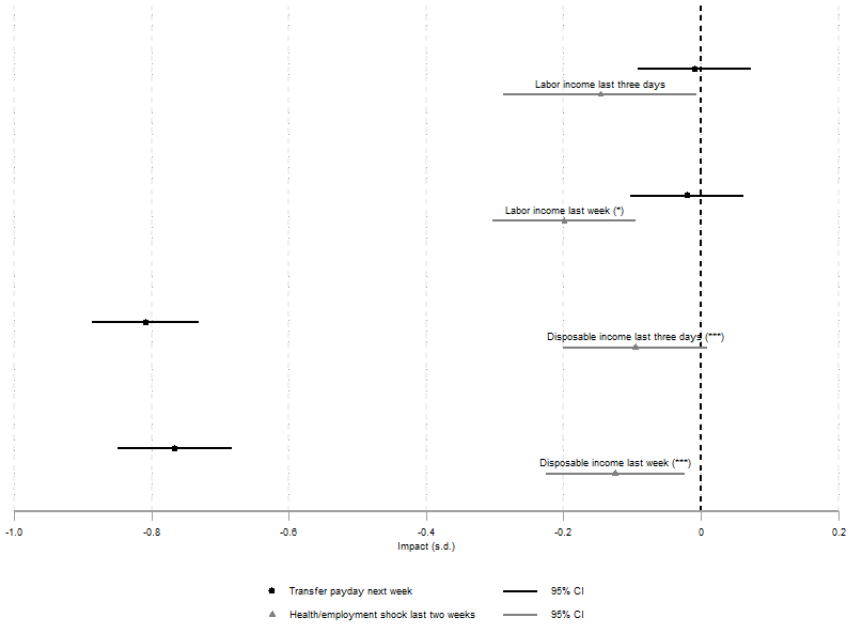
Note: Disposable income is the sum of labor income, monetary savings and the PROSPERA transfer (when applicable). Effect size in standard deviations of the group that just received the transfer.

FIGURE 2: Differences in outcomes before transfer payday



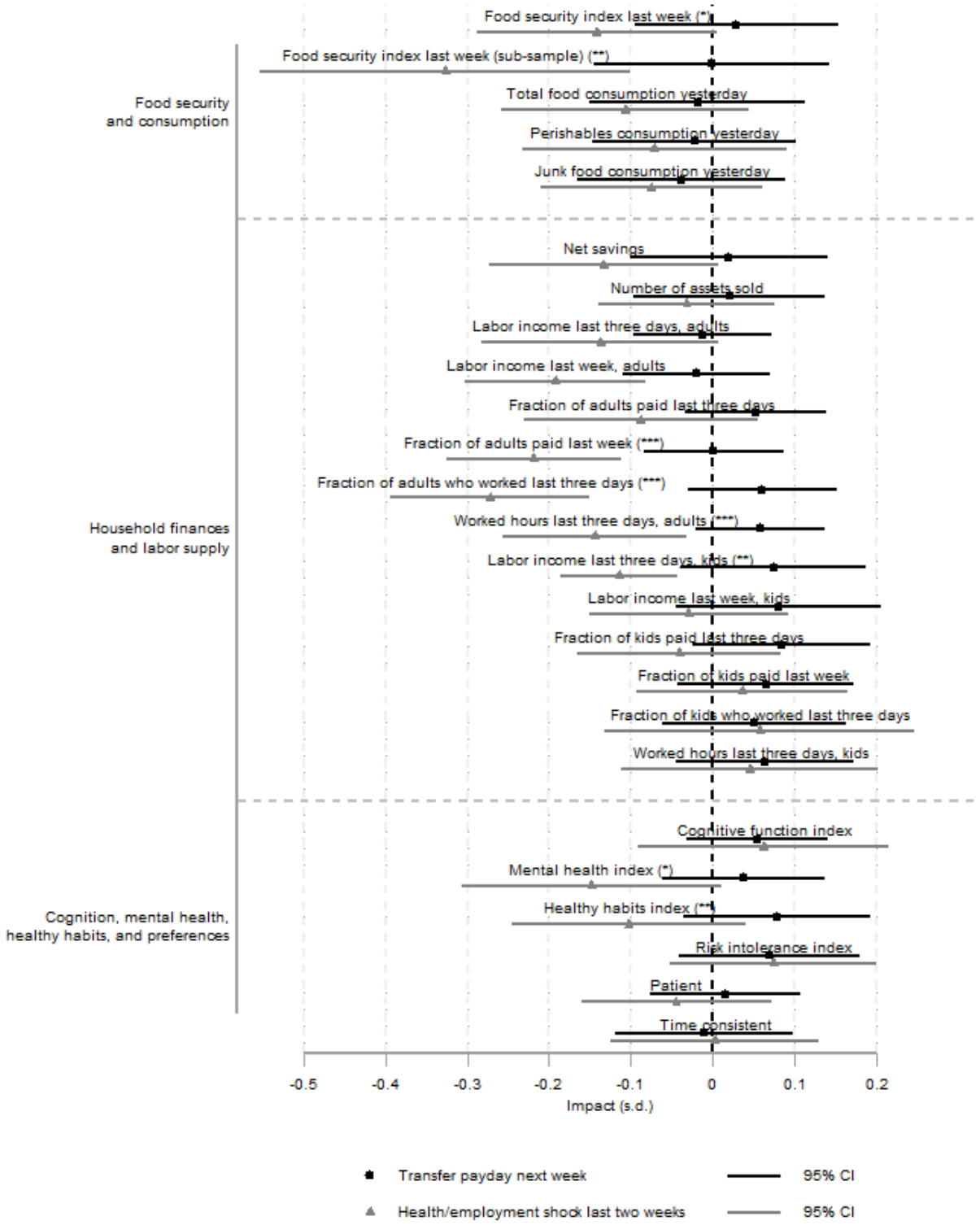
Note: Effect size in standard deviations of the group that just received the transfer.

FIGURE 3: Differences in disposable income before transfer payday and after recent health/employment shock



Note: *,**,*** transfer next week and recent health/employment shocks are statistically different from each other at the 90, 95, and 99 percent levels. Effect size in standard deviations of the group that just received the transfer.

FIGURE 4: Differences in outcomes before transfer payday and after recent health/employment shock



Note: *,**,*** transfer next week and recent health/employment shocks are statistically different from each other at the 90, 95, and 99 percent levels. Effect size in standard deviations of the group that just received the transfer.

Appendix A Additional Figures and Tables

FIGURE A1: Payday frequency over the survey period

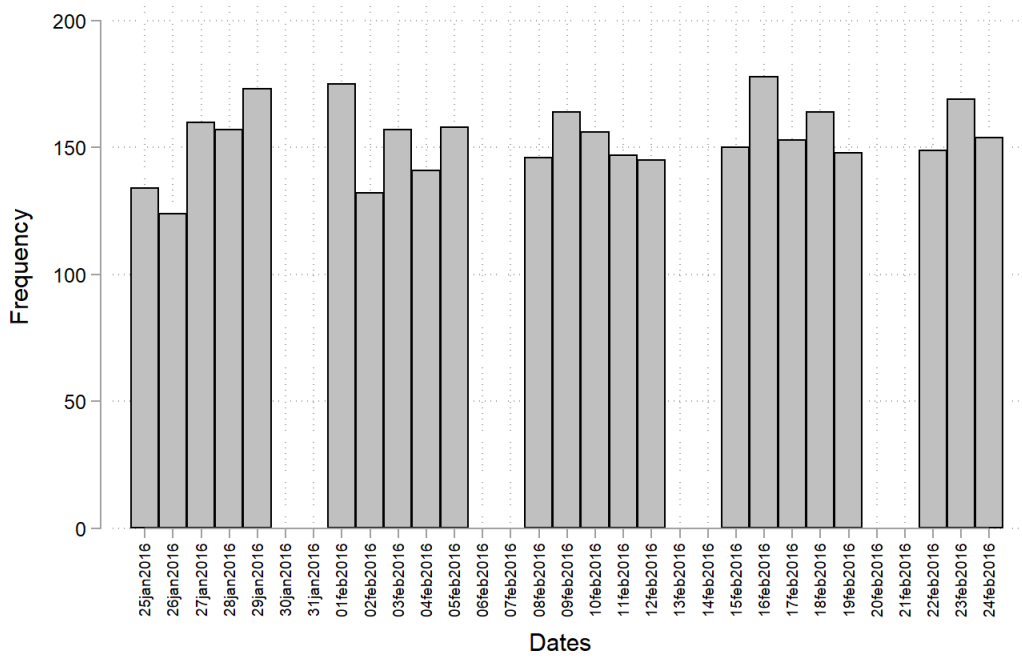
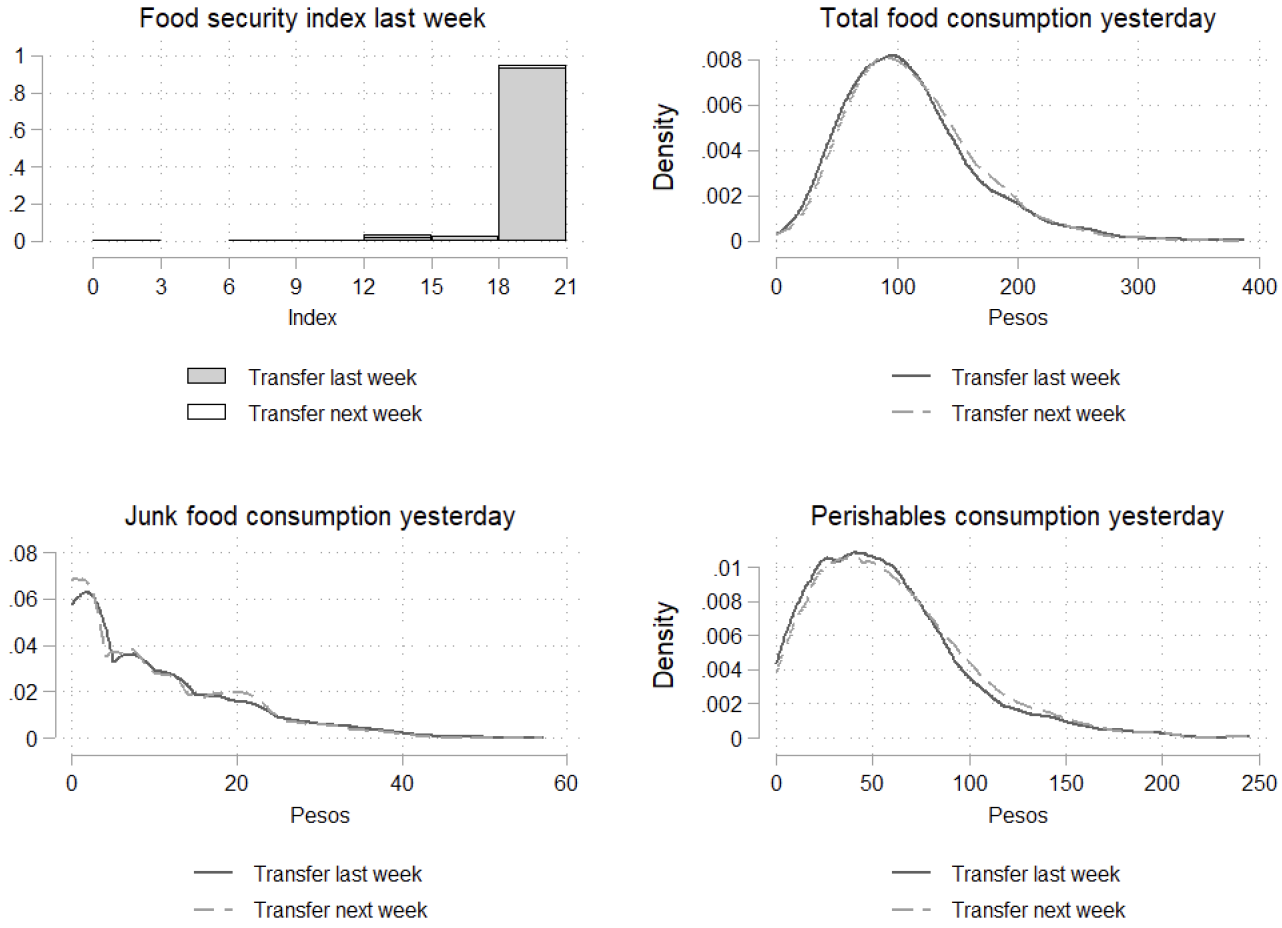
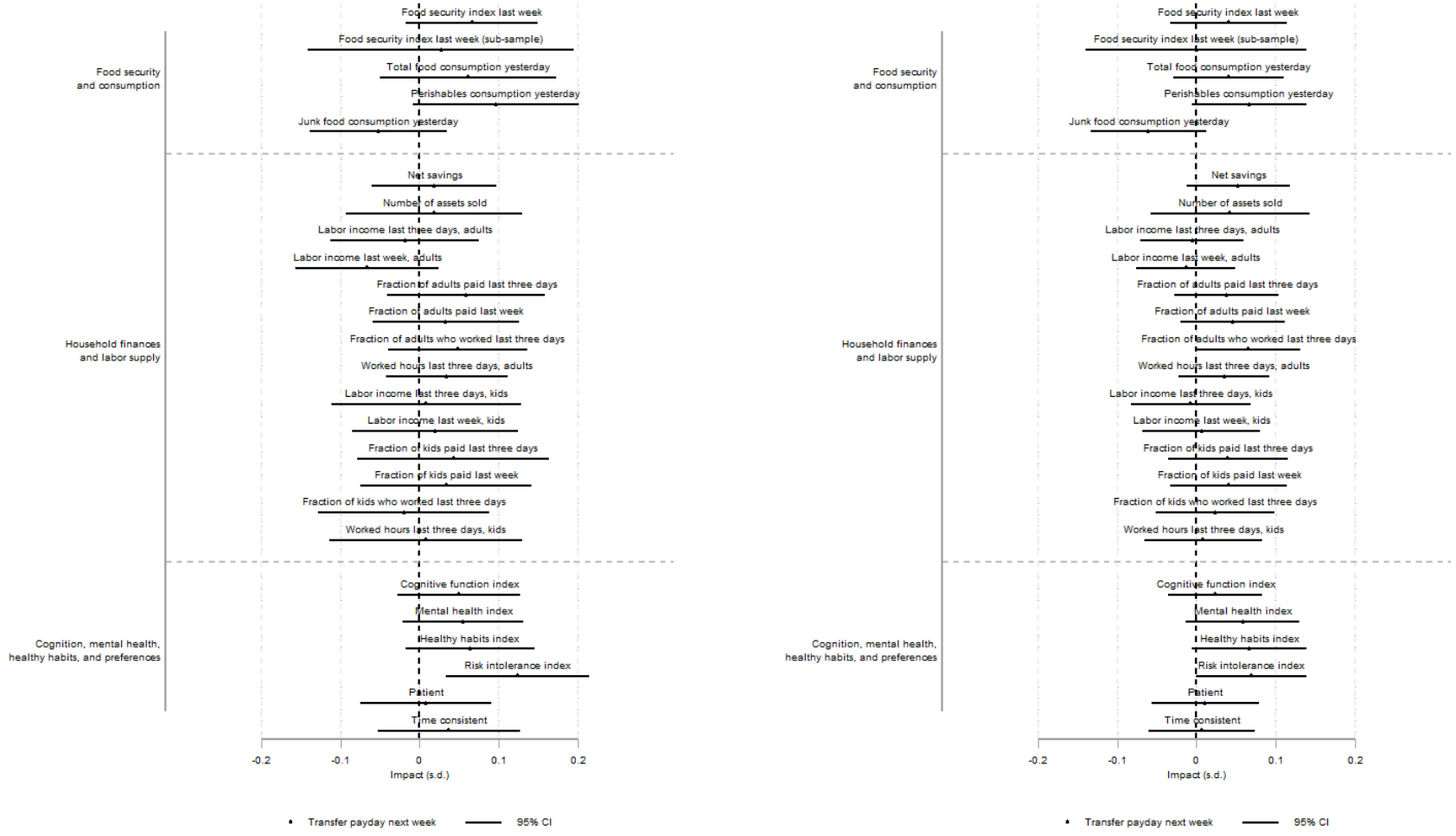


FIGURE A2: Distribution of food security and food consumption before and after the payday



Note: If we drop households whose payday occurs 3 days before the survey from the "after payday" group, the food security distribution is qualitatively unchanged.

FIGURE A3: Robustness checks: measurement error and clustering.



(A) Dropping surveys within 3 days from payday

(B) Robust and non-clustered standard errors

Note: Panel (A) drops households surveyed within 3 days from the payday. The sample size is 2,574. Panel (B) reports robust and non-clustered standard errors. The sample size is 3,534. Effect size in standard deviations of the group that just received the transfer.

TABLE A1: Means of predetermined variables and balance tests

	Comparing households that received transfer last 0-7 days and households that will receive transfer next 1-7 days	
	Received transfer last 0-7 days Mean [s.d.] (1)	Difference: transfer last 0-7 days - next 1-7 days (s.e) (2)
Couple headed	0.69	0.01
	[0.46]	(0.01)
Beneficiary's age	43.57	-0.24
	[11.76]	(0.32)
Beneficiary's schooling	6.72	-0.06
	[3.63]	(0.12)
Males aged 0-17	1.02	0.04
	[0.97]	(0.03)
Females aged 0-17	0.97	0.01
	[0.98]	(0.03)
Males aged 18-64	1.05	-0.02
	[0.80]	(0.03)
Females aged 18-64	1.33	-0.02
	[0.73]	(0.03)
Males aged >65	0.08	0.01
	[0.27]	(0.01)
Females aged >65	0.10	-0.01
	[0.31]	(0.01)
Prospera transfer	1,604.50	-17.46
	[995.85]	(30.65)
Health/employment shock last year	0.46	0.01
	[0.50]	(0.02)
Health/employment shock last two weeks	0.07	-0.01
	[0.26]	(0.01)
Total weekly income (except PROSPERA)	1,460.71 [1,035.35]	9.98 (27.63)
Test of joint significance (p-value)		0.1255
Sample size:		3,534

Notes: *, **, ***: statistically significance at the 90, 95, and 99 percent level. Columns 1 shows the average outcomes for households whose transfer payday was 0-7 day before the survey. Column 2 shows the difference in outcomes between households whose payday occurred in the last 0 to 7 days and households whose payday will occur within the next 1 to 7 days. We regress each outcome on a "Transfer in 1-7 Days" dummy, weekday dummies, and state dummies and shows the estimates of the "Transfer in 1-7 Days" coefficient. Standard errors clustered by locality in parentheses. Total weekly income includes all regular labor and non-labor income sources except for the PROSPERA transfer. The test of joint significance does not include the weekday and state dummies. One US dollar is equivalent to 19.9 Mexican *pesos* in 2021.

TABLE A2: Differences in disposable income before transfer payday

	Labor income last		Disposable income (Labor income + savings + transfer)	
	3 days	week	last 3 days	last week
	(1)	(2)	(3)	(4)
Panel A:	Effect of transfer in 1-7 days			
Transfer in 1-7 days	-7.33	-15.84	-1623.21*** ^^^	-1631.67*** ^^^
	(23.29)	(28.69)	(66.48)	(68.62)
Panel B:	Effect of transfer in 1-7 days by subgroup			
Transfer in 1-7 days	-18.28	-83.12*	-1187.75*** ^^^	-1252.43*** ^^^
	(50.84)	(44.77)	(116.59)	(126.39)
Transfer in 1-7 days x impatient	6.07	27.24	-18.1	2.67
	(51.56)	(60.06)	(144.77)	(145.19)
Transfer in 1-7 days x time inconsistent	56.00	24.74	2.00	-28.84
	(46.32)	(50.45)	(117.34)	(121.39)
Transfer in 1-7 days x large transfer	-52.85	52.19	-894.01*** ^^^	-789.39*** ^^^
	(44.45)	(38.98)	(110.02)	(114.27)
Mean for households that just received transfer	452.98	969.95	2,669.70	3,186.67
Sample size	3,534	3,534	3,532	3,532

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. The symbols ^, ^^, ^^^ mean statistical significance at the 90, 95, and 99 percent level, after correcting for the FDR (Benjamini and Hochberg 1995). Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Transfer in 1-7 days" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Standard errors clustered by locality in parentheses. Transfer in 1-7 days equals 1 for households that will receive the transfer within the following 1-7 days; impatient is an indicator for people preferring a larger amount of money later over a smaller amount sooner; time inconsistent is an indicator for people whose preference over a larger reward later over a smaller reward sooner vary depending on whether the sooner day is close to or far from the present; large transfer is an indicator for households with a greater than median transfer/income ratio. This median is 0.12.

TABLE A3: Differences in food security and food consumption before transfer payday

	Food security last week		Food consumption yesterday		
	(1)	(Sub-sample) (2)	Total (3)	Perishables (4)	Junk food (5)
Panel A:	Effect of transfer in 1-7 days				
Transfer in 1-7 days	0.04	-0.01	2.19	2.6	-0.62
	(0.04)	(0.08)	(2.55)	(1.83)	(0.37)
Panel B:	Effect of transfer in 1-7 days by subgroup				
Transfer in 1-7 days	0.08	0.02	0.35	0.96	-0.85
	(0.07)	(0.14)	(4.49)	(3.03)	(0.65)
Transfer in 1-7 days x impatient	-0.04	0.05	-5.85	-5.16	-0.53
	(0.09)	(0.15)	(3.96)	(3.51)	(0.7)
Transfer in 1-7 days x time inconsistent	0.01	0.03	-0.13	2.32	-0.46
	(0.08)	(0.12)	(3.69)	(2.39)	(0.78)
Transfer in 1-7 days x large transfer	-0.06	-0.11	7.11*	3.95	1.24**
	(0.09)	(0.13)	(3.57)	(2.7)	(0.61)
Mean for households that just received transfer	-0.04	-0.05	109.93	55.75	9.11
Sample size	3,531	1,483	3,534	3,534	3,534

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. The symbols ^, ^^, ^^^ mean statistical significance at the 90, 95, and 99 percent level, after correcting for the FDR (Benjamini and Hochberg 1995). Column (2) drops households surveyed 1 to 3 days before the payday from the "after payday" group (the omitted category). Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Transfer in 1-7 days" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Standard errors clustered by locality in parentheses. Transfer in 1-7 days equals 1 for households that will receive the transfer within the following 1-7 days; impatient is an indicator for people preferring a larger amount of money later over a smaller amount sooner; time inconsistent is an indicator for people whose preference over a larger reward later over a smaller reward sooner vary depending on whether the sooner day is close to or far from the present; large transfer is an indicator for households with a greater than median transfer/income ratio. This median is 0.12.

Table A4: Differences in finances and employment before transfer payday

			Adults						Children aged 5 to 17					
	Net savings	Number of assets sold	Labor income last 3 days	Labor income last week	Fraction paid last 3 days	Fraction paid last week	Fraction worked last 3 days	Worked hrs. last 3 days	Labor Income Last 3 days	Labor Income Last week	Fraction paid last 3 days	Fraction paid last week	Fraction worked last 3 days	Worked hrs. last 3 days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A:							Effect of transfer in 1-7 days							
Transfer in 1-7 days	278.56	0.01	-4.07	-12.79	0.01	0.01	0.02*	0.80	-0.85	0.94	0.01	0.01	0.01	0.05
	(170.19)	(0.01)	(22.52)	(29.77)	(0.01)	(0.01)	(0.01)	(0.54)	(5.33)	(6.35)	(0.01)	(0.01)	(0.01)	(0.31)
Panel B:							Effect of transfer in 1-7 days by subgroup							
Transfer in 1-7 days	219.08	0.01	-15.41	-75.42	-0.01	-0.03	-0.01	0.60	0.75	1.54	-0.01	0.01	-0.01	0.18
	(391.32)	(0.01)	(50.96)	(50.03)	(0.02)	(0.02)	(0.02)	(1.15)	(11.45)	(14.46)	(0.01)	(0.01)	(0.02)	(0.54)
Transfer in 1-7 days x impatient	121.19	0.01	3.98	18.1	0.02	0.03	0.01	-2.53**	-1.5	-0.39	0.01	0.01	0.01	0.06
	(448.09)	(0.01)	(51.2)	(59.39)	(0.02)	(0.03)	(0.02)	(1.24)	(8.44)	(12.46)	(0.01)	(0.01)	(0.01)	(0.56)
Transfer in 1-7 days x time inconsistent	-10.58	0.01	55.96	26.96	0.04	0.02	0.01	0.10	-3.12	-6.18	0.01	0.01	0.02	0.06
	(336.91)	(0.01)	(48.29)	(52.61)	(0.02)	(0.02)	(0.02)	(1.02)	(7.90)	(8.60)	(0.01)	(0.01)	(0.01)	(0.60)
Transfer in 1-7 days x large transfer	98.25	-0.01	-50.1	52.49	-0.01	0.04**	0.04**	1.02	-1.52	0.89	-0.01	-0.01	-0.01	-0.51
	(402.45)	(0.01)	(46.98)	(43.44)	(0.02)	(0.02)	(0.02)	(1.25)	(8.72)	(12.03)	(0.01)	(0.01)	(0.01)	(0.52)
Mean for households that just received transfer	-1,578.16	0.01	446.49	958.84	0.35	0.57	0.65	32.52	17.39	32.25	0.03	0.04	0.06	1.96
Sample size	3,366	3,375	3,269	3,269	3,269	3,269	3,269	3,269	2,785	2,785	2,785	2,785	2,785	2,785

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. The symbols ^, ^^, ^^^ mean statistical significance at the 90, 95, and 99 percent level, after correcting for the FDR (Benjamini and Hochberg 1995). Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Transfer in 1-7 days" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Standard errors clustered by locality in parentheses. Transfer in 1-7 days equals 1 for households that will receive the transfer within the following 1-7 days; impatient is an indicator for people preferring a larger amount of money later over a smaller amount sooner; time inconsistent is an indicator for people whose preference over a larger reward later over a smaller reward sooner vary depending on whether the sooner day is close to or far from the present; large transfer is an indicator for households with a greater than median transfer/income ratio. This median is 0.12. The sample size in columns 9-14 is smaller because some households do not have 5-17 year old children.

TABLE A5: Differences in cognition, mental health, healthy habits, and preferences before transfer payday

	Cognitive function index (1)	Mental health index (2)	Healthy habits index (3)	Risk intolerance index (4)	Patient (5)	Time consistent (6)
Panel A:			Effect of transfer in 1-7 days			
Transfer in 1-7 days	0.02	0.06*	0.07	0.07**	0.01	0.01
	(0.03)	(0.03)	(0.04)	(0.03)	(0.01)	(0.02)
Panel B:			Effect of transfer in 1-7 days by subgroup			
Transfer in 1-7 days	0.12***	0.03	0.01	0.04	0.01	0.01
	(0.04)	(0.07)	(0.08)	(0.06)	(0.02)	(0.02)
Transfer in 1-7 days	0.04	0.02	0.06	0.12		
x impatient	(0.06)	(0.08)	(0.09)	(0.08)		
Transfer in 1-7 days	-0.10*	-0.03	0.01	0.04		
x time inconsistent	(0.05)	(0.06)	(0.07)	(0.08)		
Transfer in 1-7 days	-0.13**	0.07	0.06	-0.06	0.01	-0.02
x large transfer	(0.05)	(0.07)	(0.08)	(0.07)	(0.03)	(0.03)
Mean for households that just received transfer	-0.01	-0.04	-0.04	-0.03	0.71	0.53
Sample size	3,375	3,372	3,369	3,199	3,373	3,375

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. The symbols ^, ^^, ^^^ mean statistical significance at the 90, 95, and 99 percent level, after correcting for the FDR (Benjamini and Hochberg 1995). Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Transfer in 1-7 days" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Standard errors clustered by locality in parentheses. Transfer in 1-7 days equals 1 for households that will receive the transfer within the following 1-7 days; impatient is an indicator for people preferring a larger amount of money later over a smaller amount sooner; time inconsistent is an indicator for people whose preference over a larger reward later over a smaller reward sooner vary depending on whether the sooner day is close to or far from the present; large transfer is an indicator for households with a greater than median transfer/income ratio. This median is 0.12.

TABLE A6: Comparing households by health/employment shock occurrence and timing - means and balance across groups

	Comparing households with and without health/employment shocks last year		Comparing households by timing of shock (distant vs. recent)	
	Households without shock last year	Difference (with - without shock)	Households with distant shock	Difference (recent - distant shock)
	Mean		Mean	
	[s.d.]	(s.e)	[s.d.]	(s.e)
	(1)	(2)	(3)	(4)
Couple headed	0.69	0.02	0.71	-0.04
	[0.46]	(0.02)	[0.46]	(0.03)
Beneficiary's age	43.57	0.62	43.9	0.49
	[11.76]	(0.54)	[12.03]	(0.80)
Beneficiary's schooling	6.72	-0.11	6.67	-0.24
	[3.63]	(0.19)	[3.56]	(0.26)
Males aged 0-17	1.02	-0.06	0.99	0.00
	[0.97]	(0.04)	[0.97]	(0.06)
Females aged 0-17	0.97	-0.02	0.96	0.12*
	[0.98]	(0.03)	[0.98]	(0.07)
Males aged 18-64	1.05	0.07**	1.08	-0.02
	[0.80]	(0.03)	[0.82]	(0.05)
Females aged 18-64	1.33	0.04*	1.35	0.06
	[0.73]	(0.02)	[0.75]	(0.05)
Males aged >65	0.08	0.03**	0.10	0.02
	[0.27]	(0.01)	[0.29]	(0.02)
Females aged >65	0.10	0.02	0.11	0.07**
	[0.31]	(0.01)	[0.32]	(0.03)
Prospera transfer	1,604.46	-49.33	1,577.70	-88.37
	[995.85]	(51.85)	[992.73]	(74.71)
Total weekly labor income	1,460.71	-6.44	1,457.21	-188.96***
	[1035.35]	(40.12)	[1,011.94]	(62.75)
Test of joint significance (p-value)		0.0010		0.0000
Sample size:		3,534		1,617

Notes: *, **, *** mean statistical significance at the 90, 95, and 99 percent level. Columns 1 and 2 compare households that did and did not experience any health or employment shocks in the previous year. Columns 3 and 4 restrict the analysis to households that experienced at least one such shock last year and compare them by the timing of the most recent shock. We define shocks as recent if they occurred within the previous 14 days or Distant if they occurred in the previous 15 to 365 days. Column 2 regresses each outcome on a "distant shock" dummy, weekday dummies, and state dummies for all households and reports the estimates of the "distant shock" coefficient. Column 4 regresses each outcome on a "recent shock" dummy, weekday dummies, and state dummies for households that experienced health/employment shocks in the previous year and reports the estimates of the "recent shock" coefficient. Standard errors clustered by locality in parentheses. We measure total weekly labor income by asking respondents about all household members' regular weekly income from their primary and secondary jobs. The test of joint significance does not include the weekday and state dummies. Dropping total weekly income from the set of regressors, the p-value become 0.0007 and 0.0279, respectively.

TABLE A7: Heterogenous effects of transfer payday by distance

	All	New recipient	Time inconsistent	New recipient and time inconsistent
	(1)	(2)	(3)	(4)
Panel A:		Junk food consumption yesterday		
Transfer in 1-7 days	-0.93	-1.41*	-1.63*	-3.69***
if bank close	(0.56)	(0.81)	(0.84)	(1.09)
Transfer in 1-7 days	-0.46	-0.3	-0.75	-0.05
if bank far	(0.49)	(0.54)	(0.60)	(0.61)
Double Ddifference	0.47	1.11	0.88	3.65***
(far-close)	(0.75)	(0.97)	(0.99)	(1.07)
Mean for households that	9.11	8.82	9.11	8.68
just received transfer [s.d]	[10.19]	[10.29]	[10.41]	[10.21]
Panel B:		Total food consumption yesterday		
Transfer in 1-7 days	1.98	2.01	-2.2	-5.3
if bank close	(3.83)	(4.66)	(4.98)	(7.16)
Transfer in 1-7 days	2.24	3.97	3.88	4.02
if bank far	(3.23)	(3.66)	(3.57)	(4.11)
Double difference	0.26	1.96	6.09	9.32
(far-close)	(4.82)	(5.57)	(5.98)	(8.12)
Mean for households that	109.93	110.79	110.14	111.46
just received transfer [s.d]	[54.89]	[57.32]	[52.96]	[55.52]
N	3,533	2,601	1,666	1,242

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. "Bank close" groups households that live within a 30 minute walk from the nearest BANSEFI branch or own a car. "Bank far" groups households that live at least a 30 minute walk from the nearest BANSEFI branch and do not own a car. "Double difference" is the difference between the estimates in rows 1 and 2. "New recipients" are households who own a debit card associated with their BANSEFI savings account for 28 months or less, the median time in our sample. All regressions control for predetermined respondent and household characteristics, weekday dummies, and state dummies. Standard errors clustered by locality.

Appendix B Mental health and preferences indexes

The **cognitive function index** is based on three tasks: forward and backward digit recall, and a battery of Raven's matrices. The aggregate cognitive index is created by standardizing the score of each individual test, adding up the sum of scores, and standardizing again the sum so that households that just received the transfer have a mean of zero and standard deviation of one.

The **mental health index** includes three dimensions. (1) A locus of control scale. We used 4 pairs of questions from the Rotter Scale (Rotter 1966).[18] (2) A (lack of) stress scale. To create this index, we used the Perceived Stress scale 4 (Cohen et al. 1994), changing the time interval from the previous month to the previous day. (3) A (lack of) depression scale using five questions to measure whether the respondent felt unhappy and unsatisfied with her life.[19] As before, we standardize each variable, sum it, and standardize again.

The **healthy habits index** considers the following variables measured the day prior to the survey date: minutes spent working out, minutes spent sleeping, number of times the respondent brushed her teeth, and an indicator variable for washing hands properly with soap and water. As before, we standardize each variable, sum it, and standardize again.

To construct the **risk tolerance index** we use an incentivized lottery-choice task to measure risk attitudes. In the lottery-choice task, subjects were asked to choose among five lotteries, which differed on how much they paid depending on whether a coin landed on heads or on tails. The lottery-choice task is similar to that used by Binswanger (1980), Eckel and Grossman (2002) and Garbarino et al. (2011). Based on a coin flip, each lottery had a 50-50 chance of paying either a lower or higher reward. The five (lower; higher) pairings were (200; 200), (180; 260), (150; 320), (115; 380) and (90; 440). The choices in the lottery task allow one to rank subjects according to their risk tolerance: subjects that are more risk tolerant will choose the lotteries with higher expected value. Given the low level of literacy of our sample, we opted for a visual presentation of the options, similar to Binswanger (1980). Each option was represented with pictures of Mexican pesos bills corresponding to the amount of money that would be paid if the coin landed on heads or

[18] Respondents must pick the sentence they most agree in each of the following four pairs. (1) "Everything that happens to me was caused by what I have done." or "Sometimes I feel like I have not enough control over the direction my life is taking.". (2) "When I make plans, I am almost sure I can make them work." or "It's not always good to plan too much because many things depend on the good or bad fortune." (3) "In my case, what I get has nothing to do with luck." or "Sometimes it is good to take decisions flipping a coin betting head or tail." (4) "Many times I have felt that I have little influence over the things that happen to me." or "It is impossible for me to believe that chances or luck play an important role in my life."

[19] The five questions are: Yesterday, did you feel unsatisfied with your life? Yesterday, did you feel happy? Yesterday, did you feel sad? Yesterday, did you feel happy with your way of being? Yesterday, did you feel your life was pleasant? Yesterday, did you feel your life had no meaning?

tails. We assigned values from 0 to 4 to the respondent's lottery choice, with 0 being the lowest expected value and variance and 4 being the highest expected value and variance. Therefore, the higher the index, the more risk-tolerant the respondent. We standardize this score by subtracting the mean and dividing by the standard deviation for households that just received a transfer.

To build the **patience indicator**, we measured the willingness to delay gratification by asking individuals to make incentivized choices between a smaller, sooner monetary reward and a larger, later monetary reward (Tversky and Kahneman 1986; Benzion and Rapoport 1989). Study participants were asked to choose between receiving 100 pesos in 1 week or 200 pesos in 1 month and 1 week. Those who chose the 100 pesos in 1 week were asked to make a second choice between 100 pesos in 1 week or 300 pesos in 1 month and 1 week. Those who had chosen again 100 pesos in 1 week were asked to make a third choice between 100 pesos in 1 week or 400 in 1 month and 1 week. Patient beneficiaries are those who are willing to wait for a greater reward in each of the three stages previously described.

Finally, for the **time-consistent indicator**, we asked a second set of questions about time preferences in which we varied the time frame used in the previous battery: we ask the respondent to choose 100 pesos to be paid in six months or 200 pesos after seven months, and then we vary the size of the reward to 300 and 400 if the respondent is willing to delay the payment. A respondent is classified as consistent if she is willing to wait for a larger prize when offered to choose between 100 and 200 pesos in both set of batteries, or if she takes the 200 pesos option in both batteries. We flip the signs of these indicators in equation 2.